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University of Brasília
Department of Statistics

**Impact Analysis of Fare-Free Public Transit Programs
on Service Tax Collection**

Rafael de Acypreste Monteiro Rocha

Bachelor dissertation submitted to the
Department of Statistics at the University
of Brasília, as part of the requirements to
obtain the Bachelor's Degree in Statistics.

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*Às pequenas cidadãs comuns tais como
“a merendeira [que] desce, o ônibus sai
Dona Maria já se foi, só depois é que o sol nasce.”*

Adaptado de “A ordem natural das coisas”, de Damien Seth / Emicida.

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[Portuguese version]

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Abstract

Fare-Free Public Transportation (FFTP) means that passengers do not pay for the service directly. This policy increases the utilization of public transportation, serves as an instrument of social inclusion, and helps to reduce traffic congestion, pollution, and greenhouse gas emissions. This research aims to evaluate the impact of the Fare-free policy on the municipality's service tax collection. To accomplish such main objective, a causal inference framework is used, with the Differences-in-Differences (DiD) technique serving as the method of analysis. The municipalities which adopted the FFPT policy between 2003 and 2019 were evaluated in Brazil. The principal finding of this investigation revealed an influence attributable to the Fare-Free Public Transportation policy, manifesting as an average 10.1% (95% confidence interval: [3.6%, 16.6%]) augmentation in ISS (tax on services) tax revenue, which constitutes the overall average treatment effect on the treated (ATT). Subsequent metrics corroborate this positive trend, albeit with marginally varying magnitudes. Further research incorporating a larger time span — consequently, amplified sample size — is advisable when additional data becomes available. Also, elucidating the determinants of policy adoption can strengthen the validity of the estimated causal effects.

Keywords: Differences-in-Differences, Fare-Free Public Transportation, Causal Inference.

Resumo

Tarifa-Zero no Transporte Público (TZ) significa que os passageiros não pagam diretamente pelo serviço. Essa política aumenta a utilização do transporte público, serve como instrumento de inclusão social e ajuda a reduzir o congestionamento do trânsito, a poluição e a emissão de gases de efeito estufa. O objetivo desta pesquisa é avaliar o impacto da política de gratuidade na arrecadação do imposto sobre serviços do município. Para atingir esse objetivo principal, utiliza-se uma estrutura de inferência causal, com a técnica de Diferenças-em-Diferenças (DiD) servindo como método de análise. Foram avaliados os municípios que adotaram a política de TZ entre 2003 e 2019 no Brasil. O principal resultado desta investigação revelou uma influência atribuível à política de Transporte Público Gratuito, manifestando-se como um aumento médio de 10,1% (intervalo de confiança de 95%: [3,6%, 16,6%]) na receita tributária do Imposto sobre Serviços (ISS), que constitui o efeito médio geral do tratamento sobre os tratados. As métricas subsequentes corroboram essa tendência positiva, embora com magnitudes marginalmente variáveis. É aconselhável efetuar mais investigação do efeito da TZ, incorporando um período de tempo mais alargado — consequentemente, uma amostra de maior dimensão — quando estiverem disponíveis dados adicionais. Além disso, a elucidação dos factores determinantes da adoção de políticas pode reforçar a validade dos efeitos causais estimados.

Palavras-chave: Diferenças-em-Diferenças, Tarifa-Zero no Transporte Público, Inferência Causal.

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1 Introduction

“Fare-Free Public Transportation” (FFPT) is a funding model in which the cost of public transit (PT) is covered by taxes or other sources, rather than being collected from passengers through fares. Several cities and models of total or partial subsidized public transit are present in the United States, Europe, Australia, and China (Kębłowski, 2020). As of April 2024, 106 out of 5,570 municipalities in Brazil¹ had already adopted such a model (Santini, 2024). Therefore, an impact analysis of the FFPT on public transit helps to understand the policy’s effects on the municipality’s fiscal health.

In general, the primary objective of this policy is to increase the utilization of public transit, which benefits the economically disadvantaged, specifically those who have limited mobility (Cats, Susilo and Reimal, 2017, p. 1095). This policy may also reduce the use of private cars (Brown, Hess and Shoup, 2003); consequently, traffic congestion, pollution, and greenhouse gas emissions would be cut, even though, some argue that the largest share of new users will be pedestrians and cyclists, rather than drivers (Bull, Muñoz and Silva, 2021; Cats, Susilo and Reimal, 2017, p. 1095; Štraub, 2020). If this is the case, traffic safety would be the main positive externality (Kębłowski, 2020, p. 2816).

Once PT users do not use cars daily, they offer a service for drivers — like less traffic congestion — and should spend less or nothing on this transportation (Costa Gonçalves and Santini, 2023a, p. 113; Kębłowski, 2020, p. 2816). Furthermore, public transit serves as an instrument of social inclusion, as it allows individuals to access jobs, education, and health services that would otherwise be inaccessible. The recent reduction in public transit use comes with lower transportation policy funding through fare collection — a phenomenon occurring not only in Brazil (Costa Gonçalves and Santini, 2023a; Štraub, 2020), which urges policy alternatives.

¹The Federative Republic of Brazil is structured as a union of 26 distinct subnational entities and their respective subdivisions into municipalities, alongside a singular Federal District that uniquely encompasses the roles of both a federative unit and a municipality. This complex political organization ensures that each component — whether it be the Union, the federative units (States), the subnational entities (municipalities), or the Federal District—exercises both executive and legislative authority. However, the judicial branch is exclusively reserved for the Federal Union, the individual federative units, and the Federal District, delineating a clear separation of powers within the nation’s governance framework.

The costs associated with the policy raise doubts and fears among policymakers, who worry about its financial sustainability and the increased demand for public transit. Fare-Free means that public transit can be used without a ticket because the system is fully subsidized. Some transportation engineers and economists are concerned about the risk of crowding externalities (Bull, Muñoz and Silva, 2021), and of “useless” trips (Kębłowski, 2020, pp. 2807–14) made solely because they are free, despite the distinct Fare-Free designs. In a contrary line of reasoning, some researchers suggest that FFPT frees up the portion of the family budget allocated for transportation, allowing it to be used for other local economic activities. This, in turn, boosts the municipality’s economy and tax collection.

Against this backdrop, the study examines the variability of Service Tax — known as “Imposto Sobre Serviços de Qualquer Natureza” (ISS or ISSQN) in Portuguese — as the response variable influenced by the adoption of the FFPT policy. The ISS was instituted by the Complementary Federal Law No. 116, dated July 31, 2003, with its taxable event being the provision of services delineated in the law’s appendices². In Brazil, municipalities have the jurisdiction to levy this tax, which forms a significant portion of their fiscal revenues.

Therefore, the primary goal of this research is to evaluate the impact of the Fare-Free policy on the municipality’s fiscal circumstances. Specifically, the study aims to assess the medium- and long-term fiscal sustainability, as well as civil rights considerations, of implementing such a policy. The present inquiry pertains to the determination of causal effects, wherein traditional statistical and economic methodologies as cost-benefit analysis, conventional time-series, econometric models, and case studies prove insufficient. While the former trio may facilitate the identification of correlations and potential predictions of future trends, they fall short in establishing causality due to potential biases and inaccuracies introduced during variable selection (Cinelli, Forney and Pearl, 2024). Conversely, case studies offer valuable insights into the contextual underpinnings and

²The appendices list 40 activities subject to ISS, including health, education, and information technology services.

motivations behind policy implementation, yet they do not provide a robust mechanism for assessing the economic consequences of said policies. Consequently, to accomplish such objective, a causal inference framework is used. The municipalities that adopted the FFPT policy between 2003 and 2019 were evaluated³.

Thence, the impact of FFPT on tax revenues is estimated using the Differences-in-Differences (DiD) method as a bridge to causal inference assertions. This is a quasi-experimental approach that mitigates the constraints in observational studies when a randomized controlled trial (RCT) is not feasible. It facilitates the estimation of a policy's causal effect by contrasting the experiences of the treatment group with a control group, both pre- and post-policy implementation. Provided the model's assumptions hold true (see Chapter 2 and Section 2.3), selection biases are absent, allowing for an accurate estimation of the causal effect. The DiD technique operates by calculating the within-group differences pre- and post-intervention and then determining the difference between these two figures, hence the term 'differences-in-differences'.

With its assumptions upheld, the DiD model becomes identifiable, allowing for the estimation of the average treatment effect on the treated, even in scenarios where RCTs are impractical. The findings of this evaluation could inform the discourse on public policy, particularly within the framework of local transit systems plagued by substandard conditions and high costs. To accomplish such goal, the following tasks are necessary: i) organize the database of variables related to transportation policies; ii) identify the most suitable DiD causal model to assess the implications of the Zero-Fare policy on municipal tax revenue; iii) compare the municipalities that have adopted the Zero-Fare policy, establishing municipalities that can serve as a counterfactual; and iv) investigate the indirect economic benefits, such as increased economic activity, which may positively impact the municipality's overall fiscal health.

In Chapter 2, a literature review on FFPT worldwide experiences and DiD methods as a source of causal interpretation is employed. In Section 2.3, the Difference-in-

³The number of cities included in the case study may increase depending on the policy adoption and availability of data. Notably, the political profile of the government at the time of adoption appears to have no bearing on the decision to implement a Fare-Free system (Kębłowski, 2020).

Difference method (assumptions and structures) is detailed and the data collection is explained. Materials and Methods are presented in Chapter 3. Chapter 4 presents the main findings, tests the model's assumptions, and implements some sensitivity analysis. Some discussions and conclusions lay in the Chapter 5.

2 Literature Review

To the best of our knowledge, the intersection of Fare-Free Public Transit initiatives and state revenue generation remains underexplored. A causal inquiry is of particular interest in this context. Existing literature provides a foundation of theoretical and methodological frameworks that could facilitate such an analysis.

2.1 Fare-Free Policy

The evolution of urban transportation systems towards a model centered on private vehicle usage incurs significant land use and necessitates substantial public investment in infrastructure maintenance. Transitioning to a more sustainable, rational, and low-carbon model presents challenges, as it requires altering entrenched user behaviors and regulatory frameworks (Costa Gonçalves and Santini, 2023b; Štraub, 2020). The current infrastructure is ill-equipped to support such a shift (Gabaldón-Estevan *et al.*, 2019).

Notably, Fare-Free Public Transit for specific demographic groups, such as seniors or students, generally garners public approval (Brown, Hess and Shoup, 2003). In this sense, any general policy aimed at enhancing public transit utilization must account for the potential escalation in operational costs, including labor, vehicle capacity, and energy expenditures (Gabaldón-Estevan *et al.*, 2019). These issues tend to be exacerbated during peak travel times⁴. Consequently, the implementation of such policies should be synchronized with infrastructural enhancements to support increased demand (Štraub, 2020).

In contrast, the implementation of FFPT may streamline social interactions and invigorate economic activities (Gabaldón-Estevan *et al.*, 2019). Such a policy could counteract the trend of diminishing public transit patronage, as evidenced by multiple studies Gabaldón-Estevan *et al.* (2019), Costa Gonçalves and Santini (2023b), Brown, Hess

⁴Even though it is not always the case, as affirmed by Bull, Muñoz and Silva (2021).

and Shoup (2003), and Štraub (2020). Moreover, transportation barriers disproportionately exacerbate employment conditions for economically marginalized groups. Notably, subsidies in public transit have been shown to facilitate job-seeking behaviors, thereby potentially enhancing labor market engagement, although the evidence supporting this last affirmation is not robust⁵, as indicated by Phillips (2014). This effect is particularly pronounced in areas with limited local employment opportunities, necessitating extensive commutes to employment centers.

The prevailing theory posits that public transit fares impose a significant financial burden on the incomes of the working class (Gabaldón-Estevan *et al.*, 2019). Consequently, the abolition of such fees is anticipated to enable these individuals to reallocate their expenditures towards a broader array of services and goods. Additionally, the attractiveness of FFPT may catalyze an influx of new residents, potentially augmenting local tax revenues. For the economically disadvantaged, the subsidization of public transit may inadvertently function as a redistributive mechanism, disproportionately benefiting those with limited mobility (Costa Gonçalves and Santini, 2023a; Kębłowski, 2020).

The efficacy of these benefits is more pronounced among users sensitive to high transportation costs, who regularly utilize public services. Conversely, users with lower cost sensitivity may perceive a diminution in transportation barriers (Gabaldón-Estevan *et al.*, 2019). This dynamic is instrumental in the modal transition from private vehicles (Brown, Hess and Shoup, 2003; Cats, Susilo and Reimal, 2017) to public transit, a pivotal conduit for achieving environmental sustainability. Prolonged investigations into the shift from automotive to public transit usage could shed light on this phenomenon⁶.

Moreover, Fare-Free systems typically operate within urban boundaries, exerting a pull on migration patterns (Cats, Susilo and Reimal, 2017). A recent empirical analysis by Bull, Muñoz and Silva (2021) noted a marked increase in off-peak travel post-FFPT implementation, predominantly for leisure activities. This suggests that FFPT policies

⁵Evidence on labor market outcomes remains inconclusive, as demonstrated by the case study of Tallinn, Estonia, reported by Cats, Susilo and Reimal (2017).

⁶As indicated by Bull, Muñoz and Silva (2021), initial findings likely represent a conservative estimate of modal transition, with public transit adoption expected to escalate progressively.

may be more efficacious in creating new journeys rather than altering the travel modes of current commuters. Nonetheless, should significant inter-city integration be present, the potential exclusionary effects of such segregation must be addressed. These considerations are also imperative when the intended outcome is the reduction of fossil fuel emissions. Therefore, it is important to measure the impact of an FFTP policy adoption on the fiscal health of the local government.

2.2 Causal Inference through Observational Studies

Causal effects are fundamental to the impact evaluation of any public policy (Batista and Domingos, 2017). A policy is developed and applied to attain specific objectives. Then, policymakers should review it after implementation to confirm its effects, particularly if the original goals were met or if any unexpected externalities were developed. Thus, the key causal question is the counterfactual: what would have happened if the policy had not been implemented? The answer to this issue is the policy's causal effect, which cannot be directly observed, however. As a result, the approaches used to assess causal effects seek to recover this unobservable variable.

The gold standard for measuring the causal effect of a policy is the randomized controlled trial. When applied, an RCT ensures that the treatment is randomly assigned to the units. Therefore, the treated and untreated (control) groups differ only by the treatment, and its effect can be assessed. In this manner, the control group acts as a counterfactual. However, randomization is rare in public policies, even if it is feasible. Sometimes, the policy has already been implemented, and only observational data is accessible. In most cases, RCT is not possible because of political, ethical, or cost questions. Fare-Free is such a case: once there is no national transportation plan, policy adoption is a local administration option, where several unknown factors matter.

To tackle these causal issues, quasi-experimental designs such as differences-in-differences, matching, or synthetic control have been used (Angrist and Pischke, 2009; Batista and Domingos, 2017). Quasi-experimental designs aim to “isolate” the causal

effect from confounders and selection bias *strictu sensu* (Batista and Domingos, 2017). Confounders are variables that affect both the treatment and outcome variables (Pearl, 2018). Without simultaneously accounting for confounding factors, it is impossible to determine a causal relationship between variables, as the measure may also be influenced by these variables. Confounding is widely regarded as the primary challenge in causal inference and is the inspiration behind the adage “correlation does not imply causation” (Hernan and Robins, 2020, p. 83). The increase in a municipality’s positive GDP growth has a significant impact on tax collection and promotes the construction of FFPT, for example. The estimation of causal effects is subject to bias if it is not mathematically adjusted. Randomized controlled trials can mitigate confounding effects by design, while observational studies typically estimate causal effects by controlling for confounding variables.

In that regard, the issue of why a municipality has adopted Fare-Free Public Transit is of concern, as the selection process is not random. For example, during the COVID-19 pandemic, several municipalities witnessed private companies abandoning their public transit concessions. To fill the void, some municipalities assumed the service and provided it Fare-Free. This has led to a bias in the selection of subjects under analysis, as private companies’ bankruptcy influenced the FFPT adoption. Consequently, the treated group may not be representative of the population, and some statistical control must be employed before generalizing the conclusions (Pearl and Mackenzie, 2018, ch. 5).

Selection bias *strictu sensu* occurs when common effects of the treatment and outcome variable are taken into account (Hernan and Robins, 2020, p. 99). This type of bias can arise when individuals self-volunteer to participate in a non-randomized study. In the case of FFPT, selection bias may be introduced by conditioning on the level of employment or wages, as the Fare-Free policy tends to increase employment and wages (Piazza, 2017). Additionally, increased tax collection through FFPT may improve state service acquisitions, leading to higher levels of employment. It is also a crucial assumption that RCTs address, although selection bias may still occur in these studies when some units leave the study — i.e., when there is censoring in the study.

Although there are distinctions between confounding and selection bias *strictu sensu*, econometricians and statisticians often refer to them generically as “selection bias”. This is because both types of bias are related to the selection process, whether it be the selection of individuals with observed data under analysis (selection bias *strictu sensu*) or the selection of individuals subject to the treatment (confounding) as stated by Hernan and Robins (2020, p. 103). In the case of FFTP, where policy adoption is discretionary and data covers all municipalities, the primary concern is confounding. Consequently, in the Section 2.3, selection bias and confounding are used interchangeably when there is no risk of confusion.

Along with these two warnings, the structure of the FFTP’s possible effect on tax collection requires two comparisons. To start, it is important to consider how the policy affects the fiscal situation in municipalities with and without free public transit. Additionally, one might want to examine how the policy impacts the fiscal situation in municipalities pre- and post-adoption. The first analysis compares data across different sections, while the second one examines data over time.

However, none of them automatically provides an exact counterfactual (Batista and Domingos, 2017). Counterfactual estimation is required when there is no selection bias. In cross-sectional analysis, selection bias may be caused by the influence of the reasons for a municipality’s adoption of the FFPT on the outcome variable. Conversely, it also can impact time-series analysis as concurrent time-varying variables may also influence the outcome of interest.

Difference-in-Differences approach effectively neutralizes constant differences between groups, including unobservable factors, by design. However, other sources of selection bias may arise depending on the case specifics. The model requires adherence to the parallel trends assumption, which posits that, absent treatment, the treated group’s trajectory would mirror that of the untreated group. While this cannot be directly verified, indirect methods can offer reasonable estimates. Additionally, two further assumptions are critical: first, the absence of treatment anticipation, ensuring subjects do not alter their behavior prior to the actual intervention; and second, the Stable Unit Treatment

Value Assumption (SUTVA), which assumes that one unit’s treatment does not influence another’s potential outcomes. These assumptions are elaborated upon subsequently in Section 2.4.

2.3 Potential Outcome Framework

The assessment of the causal impact of a treatment variable on an outcome variable involves comparing the observed effect when the treatment is administered against the hypothetical scenario where the treatment was not applied, known as the counterfactual. However, this counterfactual is inherently unobservable. For instance, it is not feasible for the FFTP to be both implemented and not implemented simultaneously within any given municipality.

The crux of the debate, therefore, lies in identifying a comparable unit (or units) that can effectively represent the counterfactual. Within the framework of a randomized controlled trial, the counterfactual is embodied by the control group, which is selected through randomization (Angrist and Pischke, 2009, p. 15; Hernan and Robins, 2020). This method is considered the gold standard as it engenders a high level of confidence that any observed differences in outcomes between the treatment and control groups are attributable solely to the treatment. In cases where randomization is impracticable, alternative statistical methodologies must be employed for impact evaluation.

Before exploring these alternative methodologies, it is essential to establish a mathematical framework for causal inference. Utilizing the notation for potential outcomes⁷ (Roth *et al.*, 2023, p. 2221), let us denote $Y_{i,t}(0, 0)$ as the outcome of interest for a unit that has not received the treatment during two distinct periods, where i represents the unit under study and $t \in \{1, 2\}$ signifies the period. Conversely, $Y_{i,t}(0, 1)$ represents the outcome for a unit that was untreated in the first period ($t = 1$) but received treatment in the subsequent period. For the sake of brevity, the potential outcomes notation

⁷Pearl (2018) applies some distinct notations and causal inference architecture, even with a lot in common with “potential outcomes”, mostly used in Economics.

will be simplified to $Y_{i,t}(0) \equiv Y_{i,t}(0,0)$ and $Y_{i,t}(1) \equiv Y_{i,t}(0,1)$ ⁸. In instances where the treatment is dichotomous, the resultant outcome is delineated by the following equation:

$$Y_{i,t} = D_{i,t} \cdot Y_{i,t}(1) + (1 - D_{i,t}) \cdot Y_{i,t}(0). \quad (2.3.1)$$

Here, $D_{i,t} = 1$ signifies the administered treatment during period t , with the observation of only one potential outcome⁹.

The primary concern lies in calculating the average effect of the treatment on the subjects who received it. The fundamental equation for estimating the Average Treatment Effect on the Treated (τ_2 or ATT) for the period $t = 2$ is expressed as:

$$\tau_2 = \mathbb{E}[Y_{i,2}(1) - Y_{i,2}(0)|D_{i,2} = 1]. \quad (2.3.2)$$

Within this context, $\{Y_{i,2}(0)|D_{i,2} = 1\}$ epitomizes the counterfactual scenario for the treated cohort in the second period, assuming the absence of treatment, which remains inherently unobservable.

Therefore, (2.3.2) can be rewritten to find anything — $\{Y_{i,2}(0)|D_{i,2} = 0\}$ is the natural candidate — to act as a counterfactual:

$$\tau_2 = \mathbb{E}[Y_{i,2}(1)|D_{i,2} = 1] - \mathbb{E}[Y_{i,2}(0)|D_{i,2} = 1] + \overbrace{\mathbb{E}[Y_{i,2}(0)|D_{i,2} = 0] - \mathbb{E}[Y_{i,2}(0)|D_{i,2} = 0]}^{=0},$$

which can be rearranged to:

$$\tau_2 = \mathbb{E}[Y_{i,2}(1)|D_{i,2} = 1] - \mathbb{E}[Y_{i,2}(0)|D_{i,2} = 0] + \underbrace{\mathbb{E}[Y_{i,2}(0)|D_{i,2} = 0] - \mathbb{E}[Y_{i,2}(0)|D_{i,2} = 1]}_{\text{selection bias}}. \quad (2.3.3)$$

⁸It is important to note that in this initial framework, groups that have always received treatment ($Y_{i,t}(1,1)$) and those that were previously treated but are no longer receiving treatment ($Y_{i,t}(1,0)$) are not considered. In practice, if the treatment's effects are consistent over time, groups that are always treated can serve as a control.

⁹It is imperative to note that the causative variable is presumed to temporally precede the outcome, hence the simultaneous period t for both treatment and outcome variables suggests the treatment's initiation and the outcome's culmination within the same period, a concept feasible due to the discrete temporal sequence.

The last term on the right side represents the unquantifiable selection bias. Fundamentally, this implies that should the potential outcomes for both the treated and untreated cohorts be identical — that is, $\mathbb{E}[Y_{i,2}(0)|D_{i,2} = 1] = \mathbb{E}[Y_{i,2}(0)|D_{i,2} = 0]$ — then the untreated cohort, henceforth referred to as the control group, is postulated to adopt the counterfactual position. Consequently, the mean difference between the observed outcomes is indicative of the causal treatment effect on the treated, denoted as τ_2 . The proposal here is to adopt the Difference-in-Differences methodology to calculate such value, provided its prerequisites are met.

2.4 Difference-in-Differences

The Difference-in-Differences (DiD) methodology serves as a quasi-experimental design for estimating the treatment’s average causal impact on a treated group relative to a control group. This design is applicable when the treatment is administered concurrently or at varying time intervals (Cunningham, 2024, ch. 9; Roth *et al.*, 2023). Its principal aim is to ascertain the causal effect in the absence of a randomized controlled trial, particularly when treatment allocation is non-random or endogenous, according to the terminology used in Economics (Bermudez, Bladimir Carrillo and Branco, Danyelle Santos, 2024, p. 330). Without randomization, there may be pre-existing differences between the treated and control groups that influence the outcome of interest, which DiD seeks to address.

Concisely, this approach involves calculating the pre- and post-treatment differences within each group, followed by the intergroup differences. To mitigate omitted variable bias (OVB) arising from confounders or other forms of selection bias, multiple regression techniques are employed as outlined in the Section 2.4.2. When OVB is effectively controlled through a comprehensive set of covariates, the DiD estimator is rendered unbiased and consistent, thereby facilitating the accurate estimation of the causal effect.

Additional prerequisites must be satisfied for the validity of the Difference-in-Difference approach, notably the “parallel trends” assumption. This postulates that, in the absence of any intervention, the trajectories of both the treated and untreated groups

would have evolved in tandem (Roth *et al.*, 2023, p. 2221). This premise, however, is inherently unverifiable since it pertains to a hypothetical scenario concerning the potential outcomes of the treated cohort. In addition, observing parallel trends prior to the intervention does not establish them as either a necessary or sufficient condition for post-intervention parallelism. Formally, the population-level expression of the parallel trends assumption is encapsulated in Equation (2.4.1):

$$\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_{i,2} = 1] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_{i,2} = 0]. \quad (2.4.1)$$

The presumption of parallel trends is implicitly embedded within the framework of linear regression analysis, specifically the Ordinary Least Squares (OLS) estimator. This is because the OLS estimator intrinsically adopts the trajectory of the control group as a surrogate for the counterfactual scenario of the treated group (Cunningham, 2024, ch. 9.2.3). Should this assumption prove untenable, the resultant model estimations would be deemed inaccurate. Essentially, this necessitates the presence of a control group that closely mirrors the trajectory that the treated group would have followed in the absence of any intervention¹⁰. Nonetheless, this too cannot be empirically verified. An indirect method to gauge the plausibility of this assumption involves examining the pre-treatment trends within both cohorts (Bermudez, Bladimir Carrillo and Branco, Danyelle Santos, 2024, p. 336).

In the conventional Difference-in-Differences methodology, variables that could potentially introduce selection bias yet remain constant over time are naturally attenuated. This attenuation occurs as the methodology inherently controls for these fixed effects by computing the differential of the observed variable for the same unit across two distinct time periods. Consequently, the DiD approach effectively neutralizes the influence of time-invariant confounding variables (Cunningham, 2024, ch. 9.2.2). For instance, in the FFTP scenario, any geographical or legislative factors that remain constant

¹⁰A less stringent variant of this assumption exists for time-varying treatments, termed ‘variance weighted common trends’, which allows for the nullification of divergent trends through weighting (Cunningham, 2024, ch. 9.6.4).

over time and could affect the adoption of a policy are effectively controlled for in the estimation process, irrespective of their direct measurement.

However, this equilibrium can be disrupted by certain elements, as noted by Roth *et al.* (2023, p. 2229). Specifically, confounders that fluctuate over time pose a significant challenge if they are correlated with regional economic conditions that may affect the implementation of Fare-Free policies. Moreover, the functional form used in the application of OLS can alter the assumed stability of trends. As an example, the method of quantifying tax revenue could infringe upon this assumption, whereas utilizing the logarithmic value of tax revenue might not, and the inverse may also be true. The prevalent strategy to address this concern involves adjusting for a vector of covariates, \mathbf{X}_i , to simulate random assignment of the treatment (Roth *et al.*, 2023, p. 2229), that is detailed in Section 2.4.2.

Given the pivotal nature of the parallel trend premise, Roth and colleagues (2023, p. 2236) advocate for a sensitivity analysis to assess the impact of any deviations on the primary outcomes. They employ “event-study” diagrams to ascertain the integrity of this assumption, applying varied lagged values of treatment to circumvent the adverse implications of “two-way fixed effects” (TWFE) modeling (Roth *et al.*, 2023, p. 2235). This is done in Section 4.4.2. An exception arises when the intervention influences time-sensitive covariates, thereby rendering them “bad” controls (Roth *et al.*, 2023, p. 2232). Consequently, a sensitivity analysis is imperative to confirm the resilience of the DiD framework.

Another assumption is the Stable Unit Treatment Value Assumption (SUTVA). This requisite posits that the potential outcomes for any given unit are unaffected by the particular treatments applied to other units. This implies an absence of interference or “externalities” (Cunningham, 2024, ch. 4.1.5). This assumption is bifurcated into two components: firstly, the uniformity of treatments, which, in the context of a Fare-Free initiative, translates to a consistent policy application across all inhabitants of the municipality; secondly, the independence of policy effects, indicating that the adoption of a policy within one municipality does not directly influence the potential outcomes, such

as the GDP of other municipalities.

The SUTVA assumption is deemed viable within the context of the present analysis for two primary reasons. In the case of the first component, the feasibility of the Fare-Free program is considered in scenarios where it is universally applied across the entire populace. In the second component, the plausibility of the assumption is supported by the relatively dispersed arrangement of municipalities that have implemented the FFTP, coupled with the minimal economic interdependence observed amongst them.

Furthermore, the identification of causal effects is contingent upon a third assumption termed “no-anticipation” (Roth *et al.*, 2023, p. 2222). This presupposes the absence of behavioral modifications prior to the application of a treatment, formally expressed as $\{Y_{i,1}(0) = Y_{i,1}(1)\}$ for all individuals i where $D_{i,2} = 1$. Analyzing the specific scenario at hand, it is inferred that the prospective effects on outcomes, such as tax revenue, should remain uninfluenced by the populace before the enactment of a Fare-Free policy. Consequently, this appears to be a non-issue within the Fare-Free context. Additionally, akin to numerous statistical models, the estimation process is predicated on the sampling of independent clusters — municipalities, in this instance — from a larger super-population (Roth *et al.*, 2023, p. 2219).

Given the stipulated conditions and in the absence of omitted variable bias, the DiD model is ascertainable, as delineated by Roth (2023, p. 2222). The estimation proceeds from the foundational parallel trend assumption in the Equation (2.4.1), articulated as follows:

$$\begin{aligned} \mathbb{E}[Y_{i,2}(0)|D_{i,2} = 1] &= \mathbb{E}[Y_{i,1}(0)|D_{i,2} = 1] + \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_{i,2} = 0] \\ &\stackrel{\text{no-anticipation}}{=} \mathbb{E}[Y_{i,1}(1)|D_{i,2} = 1] + \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_{i,2} = 0] \quad (2.4.2) \\ &\stackrel{\text{unconfoundedness}}{=} \mathbb{E}[Y_{i,1}|D_{i,2} = 1] + \mathbb{E}[Y_{i,2} - Y_{i,1}|D_{i,2} = 0]. \end{aligned}$$

This equation is predicated on the average manifestation of the no-anticipation effect, where $\mathbb{E}[Y_{i,1}(0)|D_{i,2} = 1] = \mathbb{E}[Y_{i,1}(1)|D_{i,2} = 1]$. Under the unconfoundedness assumption, the DiD estimator encapsulates the differential of the observed mean — see Equation

(2.3.1) — outcomes between the treated and control cohorts across both temporal phases — represented by $\mathbb{E}[Y_{i,1}|D]$ and $\mathbb{E}[Y_{i,2}|D]$. Consequently, Equation (2.4.2) elucidates the methodology for estimating the counterfactual. By substituting this into Equation (2.3.2), one derives the DiD estimator as:

$$\begin{aligned}\tau_2 &= \mathbb{E}[Y_{i,2}(1) - Y_{i,2}(0)|D_{i,2} = 1] \\ &= \mathbb{E}[Y_{i,2} - Y_{i,1}|D_{i,2} = 1] - \mathbb{E}[Y_{i,2} - Y_{i,1}|D_{i,2} = 0].\end{aligned}\tag{2.4.3}$$

This represents the aggregate of the effective Difference-in-Differences means for the population (Roth *et al.*, 2023). However, this framework is designed to account for two periods, which may not be applicable in scenarios where the treatment is administered at varying intervals. This is addressed in the following Section.

2.4.1 Difference-in-Differences and Time-varying Treatment

The adoption of treatments that vary over time can be addressed through Difference-in-Differences analysis. In the realm of social sciences, experimental designs are less prevalent compared to other fields, leading to treatments being implemented at disparate times. Consequently, for a unit receiving treatment at a particular time, there exist three potential control groups: a) units that have never been treated; b) units that have already been treated; and c) units that have not yet been treated. This scenario was observed in the implementation of Fare-Free Public Transit in Brazilian municipalities, a consequence of its federalist structure. Therefore, the implementation of the policy in each unit is contingent on its unique political and fiscal conditions.

Nonetheless, the canonical DiD 2×2 model (two periods \times two groups), as presented in Section 2.4, is not suitable for time-varying models (Goodman-Bacon, 2021, pp. 254–5) when measuring the average treatment effect on the treated. The primary reason is that it considers all possible 2×2 combinations, one of which involves comparing late-treated units with early-treated units acting as the control group, a scenario that is not plausible. Hence, the model must be adapted to accommodate time-varying treatments.

The concept of time-varying is formalized by defining periods as $t = 1, 2, \dots, T$, where the treatment can be implemented at any time $t > 1$. The time of implementation is indexed by $G_i = \min\{t : D_{i,t} = 1\}$ (Roth *et al.*, 2023, p. 2223). If the treatment is never implemented, then $G_i = \infty$, which is denoted by a binary variable, defined as an indicator function $C_i = \mathbb{1}\{G_i = \infty\} = 1$. In the potential outcomes framework, the outcome variable is denoted by $Y_{i,t}(\mathbf{0}_{g-1}, \mathbb{1}_{T-g+1})$, or simply $Y_{i,t}(g)$, when the treatment is implemented at time g . For a unit that never receives treatment, the outcome variable is represented by $Y_{i,t}(\mathbf{0}_T)$, denoted by $Y_{i,t}(\infty)$.

The intervention being scrutinized is dichotomous, and we will consider only municipalities exhibiting an irreversible nature; that is, subsequent to its implementation, the policy remains in effect indefinitely. It fundamentally constructs a framework around the group-time average treatment effect on the treated (ATT), articulated as $ATT(g, t) = \mathbb{E}[Y_{i,t}(g) - Y_{i,t}(\infty) | D_{i,t} = 1, G_i = g]$, wherein G_i denotes the cohort to which unit i belongs, having been administered the treatment commencing in period g . This methodology was conceptualized in Callaway and Sant'Anna (2021a). To illustrate, $ATT(2014, 2016)$ signifies the impact of the FFTP in 2016 contingent upon its adoption by the municipality in 2014. Presuming the parallel trend assumption¹¹ and the absence of anticipatory behavior, the ATT is discerned through the disparity between the outcomes of the treated and those of the control group (those awaiting treatment or never treated) during periods t and $g - 1$ (Roth *et al.*, 2023):

$$ATT(g, t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1} | G_i = g] - \mathbb{E}[Y_{i,t} - Y_{i,g-1} | G_i = g'] \quad \text{for any } g' > t.$$

This can be extended to encompass all $\mathbb{G}_{comp} = \{g : g' > t\}$ as delineated by

$$ATT(g, t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1} | G_i = g] - \mathbb{E}[Y_{i,t} - Y_{i,g-1} | G_i \in \mathbb{G}_{comp}]. \quad (2.4.4)$$

The empirical estimation of ATT is derived from its sample counterpart (Roth *et al.*,

¹¹Refer to Section 2.4.2 for the scenario where parallel trends are valid solely upon conditioning on covariates.

2023, p. 2226):

$$\widehat{ATT}(g, t) = \frac{1}{N_g} \sum_{i:G_i=g} [Y_{i,t} - Y_{i,g-1}] - \frac{1}{N_{G_{comp}}} \sum_{i:G_i \in G_{comp}} [Y_{i,t} - Y_{i,g-1}], \quad (2.4.5)$$

where N represents the count of units within each respective sample.

The heterogeneous effects of the FFTP policy are revealed through individual causal parameters. However, the numerous interactions between g and t may complicate the interpretation of the average treatment effect on the treated for each permutation. To simplify, the methodology computes a weighted average of these ATTs, following the guidelines of Callaway and Sant'Anna (2021a) and Roth *et al.* (2023). This approach estimates the mean effects for each interval (l) after treatment begins, highlighting the policy's varying impacts over time and offering an insight into its outcomes.

Probably, the economic and political milieu surrounding the implementation of Fare-Free policies necessitates an averaging model for a specified duration post-intervention, articulated as:

$$ATT_l^w = \sum_g w(g) \cdot ATT(g, g + l). \quad (2.4.6)$$

Here, $w(g)$ signifies the weight assigned to the ATT during the interval g , which is uniformly defined across cohorts or in accordance with the prevalence of each time span l (Roth *et al.*, 2023, p. 2227). Alternatively, the policy's effect can be averaged over a specific year of implementation, revealing diverse effects across different cohorts and providing a comprehensive analysis. Other less common comparisons are also possible and may be applied in this context. The employed aggregations are presented in Section 2.4.3. In the inference phase, bootstrapping techniques are employed to construct confidence intervals for the ATTs, as outlined in Callaway and Sant'Anna (2021a, p. 216). Finally, this foundational model is adaptable for the inclusion of covariates.

2.4.2 Difference-in-Differences and Parallel Trends Accounting for Covariates

The pronounced variation among Brazilian municipalities in aspects such as area, gross product, and demography may impinge upon the assumption of parallel trends. This variance has the potential to introduce bias within the Difference-in-Differences model. To mitigate this risk, the model's extension to encompass covariates enhances its robustness (Roth *et al.*, 2023, p. 2229). To succinctly put it, we presume that parallel trends are exhibited solely by municipalities that share similar attributes. The assumption of parallel trends, conditional on covariates, is encapsulated by the following equation:

$$\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_{i,2} = 1, \mathbf{X}_i] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_{i,2} = 0, \mathbf{X}_i] \quad (\text{almost surely}), \quad (2.4.7)$$

where \mathbf{X}_i represents a vector of covariates prior to treatment. This paradigm is further extrapolated to accommodate models with time-varying treatments as expounded by Callaway and Sant'Anna (2021a).

A critical consideration when incorporating covariates is the prerequisite of having both treated and untreated municipalities within a particular covariate set, i.e. $\mathbf{X}_i = x$, for some i across both cohorts. This requirement is encapsulated in the “strong overlap assumption”. In a formal sense, this postulates that for an infinitesimal $\varepsilon > 0$, the probability $\varepsilon < P(D_{i,t} = 1|X_i) < 1 - \varepsilon$ must hold for every \mathbf{X} within the sample (Roth *et al.*, 2023, p. 2230). In Section 3.2, a partial analysis is shown. Furthermore, the Equation (2.4.3) undergoes a reformulation with the integration of covariates, delineated as:

$$\begin{aligned} \tau_2 &= \mathbb{E}[Y_{i,2}(1) - Y_{i,2}(0)|D_{i,2} = 1, \mathbf{X}_i] \\ &= \mathbb{E}[Y_{i,2} - Y_{i,1}|D_{i,2} = 1, \mathbf{X}_i] - \mathbb{E}[Y_{i,2} - Y_{i,1}|D_{i,2} = 0, \mathbf{X}_i]. \end{aligned} \quad (2.4.8)$$

Upon delineating the theoretical framework and its foundational premises, the subsequent phase involves an empirical analysis for estimation and inference. Within the domain of empirical scrutiny for a nonparametric model, three methodologies stand out as underscored by Callaway and Sant'Anna (2021a, pp. 205–6): Outcome Regression

(OR), Inverse Probability Weighting (IPW), and Doubly Robust (DR) estimators. The DR paradigm amalgamates IPW and OR, commencing with the computation of the generalized propensity score, succeeded by an outcome regression. A key requirement is the accurate specification of either the outcome evolution or the propensity score model for the never-treated — or not-yet-treated, or both, depending on the assumptions — units (Sant’Anna and Zhao, 2020a, p. 105).

Therefore, doubly robust has been selected for the present study owing to its resilience to errors in model specification (Callaway and Sant’Anna, 2021a, p. 212). The model for propensity scores characterizes the likelihood of a unit being administered treatment within a specific timeframe, given the covariates. By designating the never-treated and the not-yet-treated cohorts as the control group¹², the model is operationalized through logistic regression (Sant’Anna and Zhao, 2020a, pp. 108–9), and the probability of initial treatment adoption is articulated as $p_{g,T}(\mathbf{X}) = P(G_g = 1 | \mathbf{X}, G_g + C = 1)$, or in a more condensed form, $p_g(\mathbf{X})$ ¹³.

The theoretical approach of the Difference-in-Differences methodology is essential for the examination of the FFTP policy’s causal impact on municipal tax revenues. From the empirical point of view, the integration of covariates is consistent under the doubly robust estimation methodology, which is explicated in Callaway and Sant’Anna (2021a). The DR estimation is articulated as follows:

$$ATT(g, t) = \mathbb{E} \left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(\mathbf{X})C}{1 - p_g(\mathbf{X})}}{\mathbb{E} \left[\frac{p_g(\mathbf{X})C}{1 - p_g(\mathbf{X})} \right]} \right) (Y_t - Y_{g-1} - m_{g,t}(\mathbf{X})) \right]. \quad (2.4.9)$$

The term $m_{g,t}(\mathbf{X}) = \mathbb{E}[Y_t - Y_{g-1} | \mathbf{X}, C = 1]$ denotes the expected outcome regression for the population that has never or not yet been treated (Callaway and Sant’Anna, 2021a, p. 205; Sant’Anna and Zhao, 2020a, p. 104).

For all these reasons, the DiD model’s estimation can be executed by employing

¹²In Callaway and Sant’Anna (2021a), scenarios where units yet to be treated serve as the control group are also elaborated.

¹³From now on, it is assumed that the index i is implicit for the sake of brevity.

a two-step double robust estimator — once more, contingent upon the validity of the parallel trends assumption — as proposed by Callaway and Sant’Anna (2021a)¹⁴. The initial step involves the estimation of the following weights for the treated and the control groups, respectively:

$$\widehat{w}_g^{treat} = \frac{G_g}{\mathbb{E}_n[G_g]}, \quad \widehat{w}_g^{control} = \frac{\frac{\widehat{p}_g(\mathbf{X}; \widehat{\pi}_g)C}{1 - \widehat{p}_g(\mathbf{X}; \widehat{\pi}_g)}}{\mathbb{E}_n \left[\frac{\widehat{p}_g(\mathbf{X}; \widehat{\pi}_g)C}{1 - \widehat{p}_g(\mathbf{X}; \widehat{\pi}_g)} \right]},$$

where the operator $\mathbb{E}_n[Z] = n^{-1} \sum_{i=1}^n Z_i$ denotes the empirical mean of a variable Z . Additionally, the term $\widehat{p}_g(\cdot; \widehat{\pi}_g)$ represents the estimated propensity score that is derived using a logistic regression model (Callaway and Sant’Anna, 2021a, p. 212). The second step of the estimation process is encapsulated by:

$$\widehat{ATT}(g, t) = \mathbb{E}_n \left[(\widehat{w}_g^{treat} - \widehat{w}_g^{control})(Y_t - Y_{g-1} - \widehat{m}_{g,t}(\mathbf{X}; \widehat{\beta}_{g,t})) \right], \quad (2.4.10)$$

where $\widehat{m}_{g,t}(\mathbf{X}; \widehat{\beta}_{g,t})$ expresses the outcome regression, which is obtained through a linear regression model, as indicated by Callaway and Sant’Anna (2021a, p. 212). That is the empirical version of the Equation (2.4.9).

In conclusion, the process of deducing the confidence interval relevant to asymptotic inference is effectively executed by employing a direct multiplier bootstrapping approach. This technique is comprehensively explicated in Callaway and Sant’Anna (2021a, pp. 212–215). Collectively, these procedures facilitate the computation of the causal influence of the FFPT on tax revenues, thereby enabling the subsequent deduction concerning the policy’s impact on the fiscal dynamics of Brazilian municipalities.

¹⁴In the original model, the δ parameter, indicates a less stringent nonanticipation assumption. However, it is not considered herein. For an in-depth discussion, refer to Callaway and Sant’Anna (2021a, p. 204).

2.4.3 Aggregating Average Treatment Effects

Various aggregative frameworks may be constructed based on the Equation (2.4.6). The initial synthesis of average effects pertains to the calculation of treatment impacts within a particular cohort over time. This synthesis facilitates the comprehension of the heterogeneity effect among cohorts that commenced participation simultaneously. The parameter delineated by Callaway and Sant'Anna (2021a) is articulated as follows:

$$\theta_{sel}(g) = \frac{1}{T - g + 1} \sum_{t=g}^T ATT(g, t). \quad (2.4.11)$$

Here, $\theta_{sel}(g)$ represents the mean effect throughout all subsequent periods post-treatment for group g . It is imperative to acknowledge that each temporal instance is accorded equivalent significance. Additionally, a singular coefficient encapsulating comprehensive effects can be deduced as:

$$\theta_{sel}^O = \sum_{g \in \mathbb{G}} \theta_{sel}(g) P(G = g | G \leq T). \quad (2.4.12)$$

This cumulative parameter is the general-purpose parameter proposed by Callaway and Sant'Anna (2021a, p. 211) for those seeking a singular summarizing figure. It equates to the mean treatment effect experienced by all entities subjected to treatment at a unique temporal juncture and is the parameter most closely aligned with the traditional DiD estimate within a 2×2 framework.

The DiD model further provides a dynamic interpretation via an aggregation predicated on the duration of treatment exposure. Initially, let $e = g - t$ denote the elapsed time since the initiation of treatment. Therefore, the aggregation is:

$$\theta_{es}(e) = \sum_{g \in \mathbb{G}} \mathbb{1}\{G + e \leq T\} P(G = g | G + e \leq T) ATT(g, g + e). \quad (2.4.13)$$

The comprehensive summation is given by:

$$\theta_{es}^O = \frac{1}{T-1} \sum_{e=0}^{T-2} \theta_{es}(e). \quad (2.4.14)$$

The aggregation under discussion embodies complexities, as delineated by Callaway and Sant'Anna (2021a, pp. 208–10). Variability in treatment exposure duration across units—particularly, the longer exposure of initially treated units—complicates the cumulative effect analysis. The dynamic weights in Equation (2.4.13), which are dependent on the duration of treatment across units, necessitate careful adjustment. Comparisons across varying treatment durations not only reflect differences in treatment effects but also alterations in group composition, potentially leading to erroneous conclusions if the dynamic weights are not properly calibrated.

Consequently, the authors define a temporal event e' such that $0 \leq e \leq e' \leq T-2$, and the structure of balanced aggregation for lucid interpretation is delineated thereafter:

$$\theta_{es}^{bal}(e; e') = \sum_{g \in \mathbb{G}} \mathbf{1}\{G + e' \leq T\} ATT(g, g + e) P(G = g | G + e' \leq T). \quad (2.4.15)$$

The specified equation represents the mean impact subsequent to e iterations, ensuring that each group-time entity is equal and has undergone treatment for a minimum of e' durations. Consequently, the aggregate effect can be deduced as follows:

$$\theta_{es}^{O, bal}(e') = \frac{1}{e' + 1} \sum_{e=0}^{e'} \theta_{es}^{bal}(e, e'). \quad (2.4.16)$$

Finally, an alternative aggregation method computes the average treatment effect at a given calendar time t across all entities that have received treatment by that time, which is articulated as:

$$\theta_c(t) = \sum_{g \in \mathbb{G}} \mathbf{1}\{t > g\} ATT(g, t) P(G = g | G \leq T). \quad (2.4.17)$$

This equation yields the mean effect for all entities treated up to time t . The comprehen-

sive version of this aggregation is expressed by:

$$\theta_t^O = \frac{1}{T-1} \sum_{t=2}^T \theta_c(t). \quad (2.4.18)$$

Each aggregation technique facilitates an interpretation of the joint impact, which must be approached with prudence, particularly in scenarios characterized by significant heterogeneity in treatment effects. The FFPT case exemplifies such a scenario, as evidenced in the Section 4.3. Conversely, these diverse aggregation methods provide a nuanced analysis of effects, thereby enhancing the evaluation of policy implications.

3 Materials and Methods

This section delineates the computational resources, datasets, and statistical procedures employed in the empirical investigation. The dataset is organized in a longitudinal panel structure, comprising variables pertinent to Brazilian municipalities spanning from 2003 to 2019. This dataset serves as the foundation for evaluating the causal effects of the Fare-Free Public Transit initiative on the fiscal outcomes of municipalities, specifically tax revenue generation. The methodological framework adopted is the Difference-in-Differences (DiD) approach, which has been augmented to incorporate dynamic treatment effects and ensure the assumption of parallel trends is met, conditional on observed covariates. The inferential analysis is facilitated by the implementation of a doubly robust estimation technique, culminating in an aggregated assessment that elucidates the overall fiscal implications of the transportation policy.

3.1 R packages

The main packages used in the analysis — and its summary descriptions — are the following:

1- *Difference-in-Differences (did)* (Callaway and Sant’Anna, 2021b): The **did** R package contains tools for computing average treatment effect parameters in a Difference-in-Differences setup allowing for: a) More than two time periods; b) Variation in treatment timing (i.e., units can become treated at different points in time); c) Treatment effect heterogeneity (i.e, the effect of participating in the treatment can vary across units and exhibit potentially complex dynamics, selection into treatment, or time effects); d) The parallel trends assumption holds only after conditioning on covariates.

2- *Doubly Robust Difference-in-Differences* (Sant’Anna and Zhao, 2020b): The **DRDID** R package implements different estimators for the Average Treatment Effect on the Treated (ATT) in Difference-in-Differences setups where the parallel trends assumption holds after conditioning on a vector of pre-treatment covariates. It is used through

the DiD package.

3.2 Data

In the present analysis, the Fare-Free public transit policy is examined as a quasi-experimental design. Given the non-randomized nature of the intervention, it is imperative to account for potential confounding variables to ascertain the causal impact of fare exemption on tax revenue. If all needed information is available, then potential outcomes can be treated as random, conditional on covariates. The data to fit the models needs to be in the “panel data” format (longitudinal data, in Statistics jargon). In summary, the data must have information over the units (municipalities) and time (years) and no restriction on time series correlation is necessary, in principle (Callaway and Sant’Anna, 2021a, p. 203).

Therefore, data pertaining to Brazilian municipalities will be utilized for this study. The potential confounders variables will serve as control covariates (\mathbf{X}_i) within the Difference-in-Differences analytical framework. The dataset encompasses municipal records spanning from 2003 to 2019, thus presenting a hybrid of cross-sectional and time series data. Details regarding the implementation year of the FFPT policy can be found in Santini (2024)¹⁵, while the tax revenue data has been procured from Instituto de Pesquisa Econômica Aplicada (IPEA) (2020). From the Brazilian Institute of Geography and Statistics (IBGE), the datasets are: Municipal Gross Domestic Product (GDP), population projections, and land area.

3.3 Methods

The assessment of the Fare-Free Public Transit (FFTP) policy’s impact on tax revenue necessitates the application of the Differences-in-Differences (DiD) approach, as delineated in Section DID (Section 2.4). This methodology is necessitated due to the im-

¹⁵Accessed on 05/04/2024

practicality of a randomized controlled trial (RCT). Given the temporal variation in policy adoption, a time-varying treatment analysis was conducted, as explicated in Section 2.4.1. Control groups were constituted by municipalities that have never implemented the policy or have not yet done so, limiting the sample to states with at least one municipality that has enacted the policy. The inception year of the FFTP served as the criterion for group classification.

Data compilation, as detailed in Section 3.2, was executed utilizing official institutional websites. Section 4.1 elucidates the primary data characteristics, providing essential insights into the FFTP, tax collection, and control variables — employed to mitigate selection bias—with foundational discussions presented in Section 2.4.2. Economic and demographic variables were logarithmically transformed to minimize data format discrepancies. Additionally, monetary variables were adjusted for inflation using the Broad Consumer Price Index (IPCA)¹⁶ to reflect December 2020 values. The R-packages, as introduced in Section 3.1, underpin the principal methodological framework, with function outputs tailored to align with the findings and discussions in the results in Chapter 4 and discussions in Chapter 5.

The primary model’s estimation, pursuant to Equation (2.4.5), represents the initial phase in evaluating potential aggregations, as discussed in Section 2.4.3. This phase encompasses group, dynamic (whether balanced or not), and calendar time aggregations to ensure the robustness of the results, safeguarding against the possibility of spurious findings contingent upon the structural design of the model. These aggregations were facilitated by the functionalities provided within the referenced R packages, with all inferential statistics reported at a 95% confidence interval. To further fortify against selection bias, diverse sets of covariates were employed, alongside an examination of the parallel trends assumption via an event-study analytical framework.

¹⁶Brazil’s CPI.

4 Results

The results are presented in the following sections. The first section details the exploratory data analysis, which includes the staggered treatment assignment of the Fare-Free Public Transit policy. The subsequent sections delineate the causal inference analysis, encompassing the estimation of the Average Treatment Effect on the Treated and the aggregated treatment effects. The final section provides a comprehensive analysis of the policy's fiscal implications on Brazilian municipalities.

4.1 Exploratory Data Analysis

Therefore, the units subject to treatment or acting as control are in Figure 1. The implementation of staggered treatment assignments frequently characterizes quasi-experimental methodologies. This approach is notable in the adoption of the Fare-Free policy. Within the timeframe of 2003 to 2019, a total of 27 municipalities elected to adopt this policy. Given that the enactment of such a policy is subject to discretionary application, the proposed methodological framework must be employed contingent upon the dependability of its underlying assumptions.

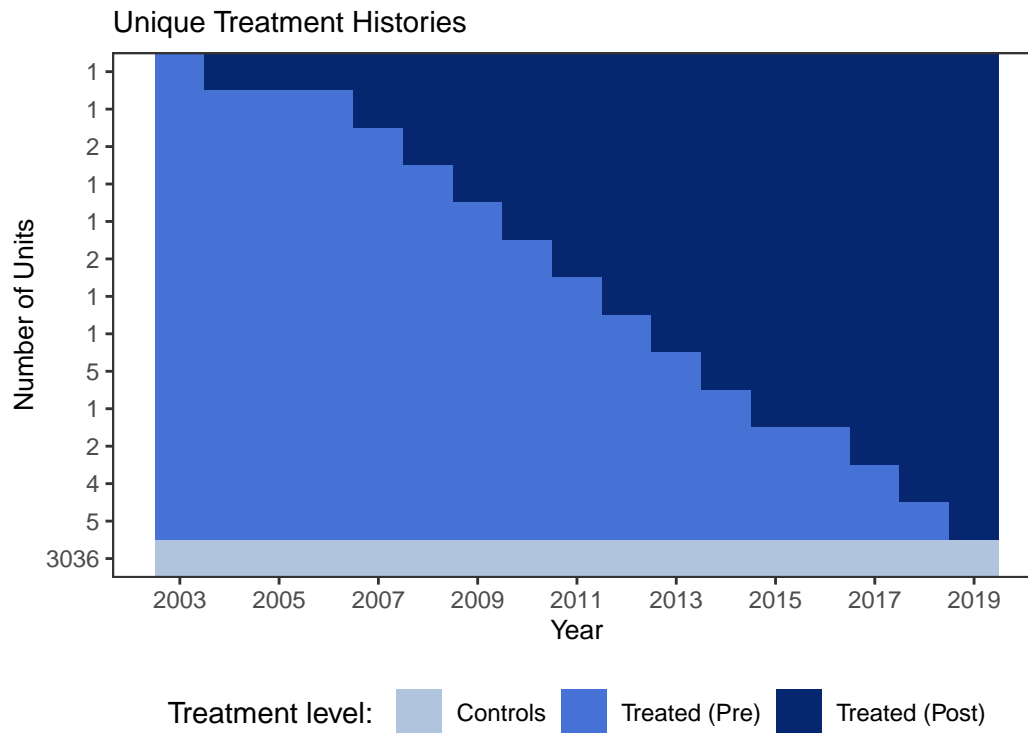


Figure 1: Staggered treatment assignment for the Fare-Free Public Transit policy.

In the assessment of causative factors influencing an outcome variable, it is anticipated that a variation in value, either ascending or descending, will be observed. This expectation is predicated on the hypothesis that the cause under evaluation exerts a measurable effect on the outcome variable in question. The average by a cohort of the treatment effect on the ISS revenue is presented in Figure 2. The term ‘cohort’ refers to a pool of municipalities that have concurrently implemented the Fare-Free within the same calendar year.

Observationally, these cohorts have demonstrated a trend of progressive growth in ISS revenue in the years succeeding their adoption of the FFTP, maintaining an upward trajectory or reversing a prior decline. This pattern indicates a period of adjustment and eventual stabilization at an elevated operational plateau, reflecting the assimilation of the new public transit methodology.

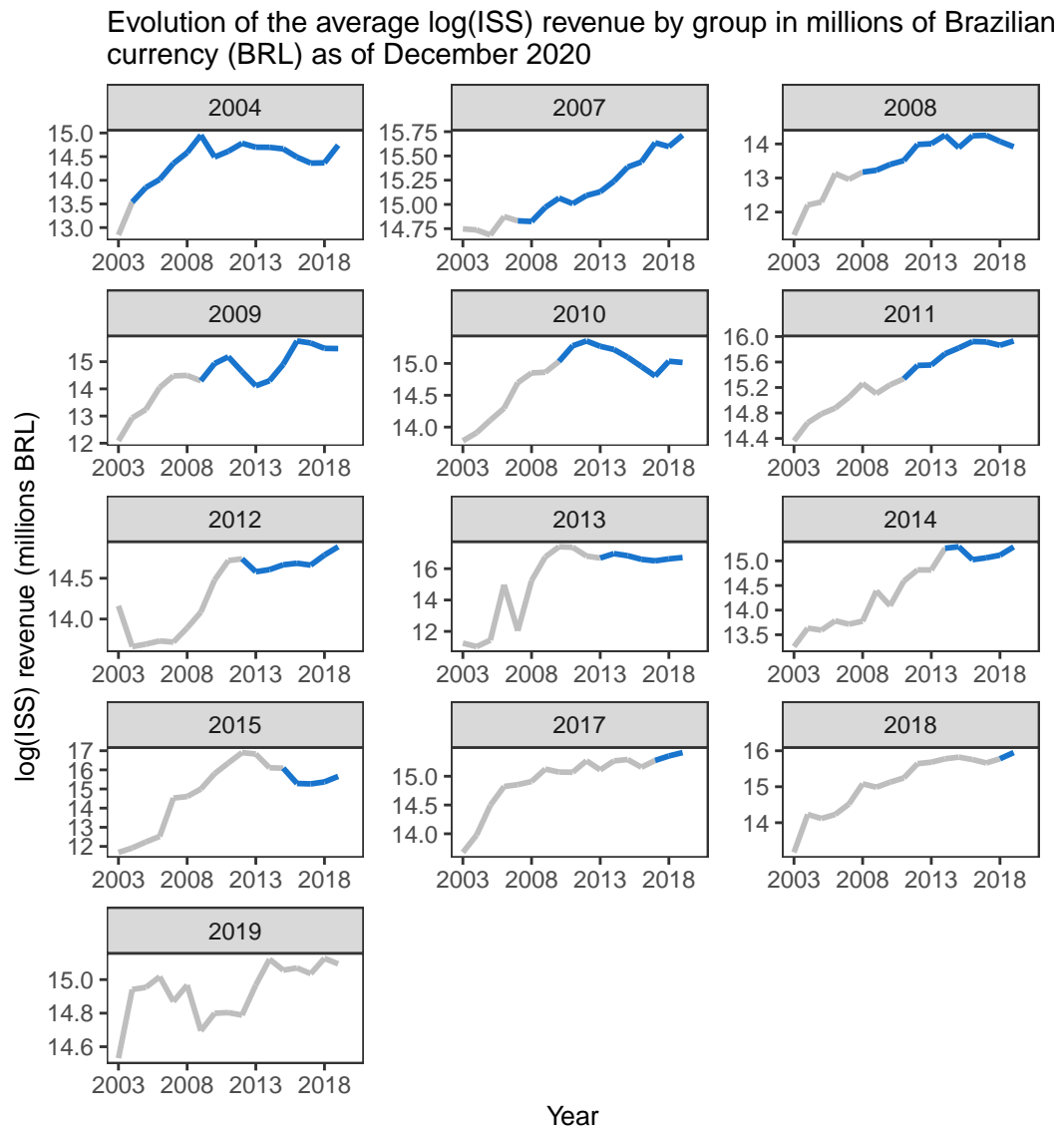


Figure 2: Log(ISS) revenue by cohort. The treatment period is in blue. Vertical axes vary according to the subplot.

The overlapping of variable information sets constitutes an additional assumption within the model framework (see Section 2.4.2). This necessitates verification through a descriptive analytical approach. The density plot, as depicted in Figure 3, illustrates that the distributions of the response variables and covariates largely coincide. Accounting for time-varying treatment conditions, descriptive statistics for the reference year of 2010, are presented irrespective of the actual treatment starting point. The graphical representation reveals that the logarithmic transformation of the territorial area exhibits a highly similar distribution across both treated and untreated groups. The remaining three variables, also transformed logarithmically, display minor distributional discrepancies; however, the

extent of overlap and the similarity in the design of the curves remain noticeable.

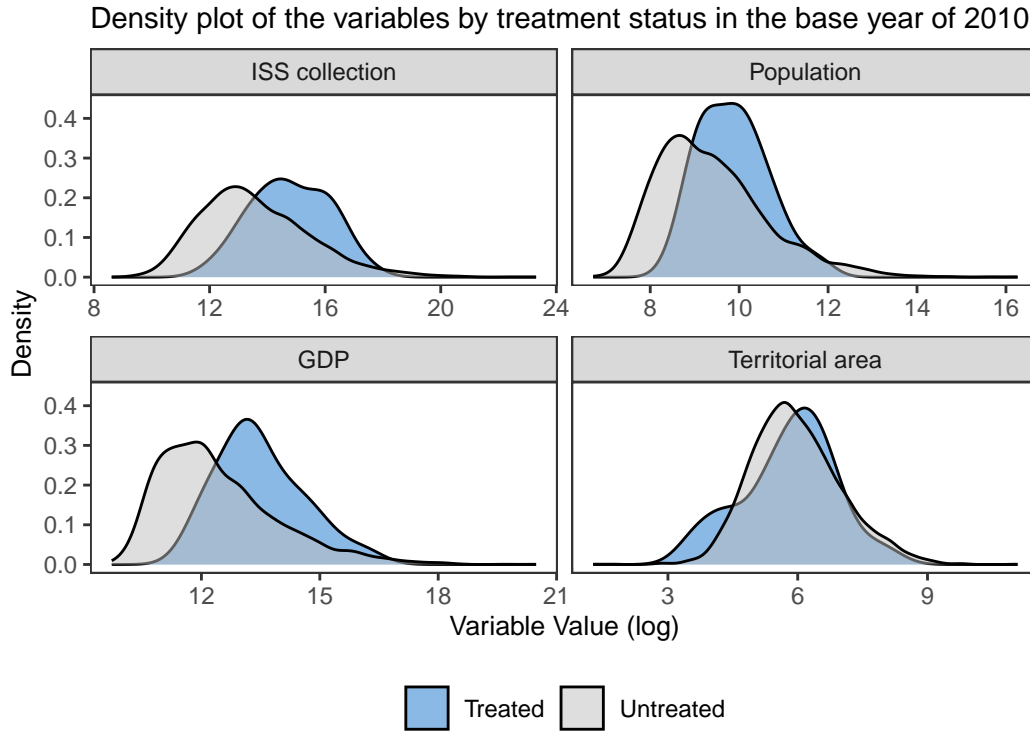


Figure 3: Density plot of the response variables and covariates.

Furthermore, the mean and median values, referenced in Table 1 and Table 2 respectively, highlight the compositions between the treated and control groups. While the mean differences for the logarithms of ISS collection, population, and GDP are statistically significant, the order statistics portray a more congruent pattern.

Table 1: Descriptive statistics of the mean response variables and covariates by treatment status.

Variable (log)	Treated, N = 27 ¹	Untreated, N = 3,037 ¹	Difference ²	95% CI ^{2,3}
ISS collection	14.80 (1.33)	13.60 (1.89)	1.2	0.67, 1.7
Population	9.87 (0.78)	9.39 (1.23)	0.48	0.17, 0.79
GDP	13.50 (1.10)	12.42 (1.49)	1.1	0.64, 1.5
Territorial area	5.79 (1.09)	5.96 (1.04)	-0.17	-0.60, 0.26

¹Mean (SD)

²Welch Two Sample t-test

³CI = Confidence Interval

Table 2: Descriptive statistics of median response variables and covariates by treatment status.

Variable (log)	Treated, N = 27 ¹	Untreated, N = 3,037 ¹
ISS collection	14.68 (13.74, 15.89)	13.33 (12.25, 14.74)
Population	9.89 (9.25, 10.35)	9.24 (8.47, 10.07)
GDP	13.26 (12.80, 14.19)	12.11 (11.31, 13.21)
Territorial area	5.99 (5.21, 6.45)	5.87 (5.25, 6.60)

¹Median (IQR)

Given that these variables exhibit a certain level of asymmetry, even on a logarithmic scale, these ordered summaries provide additional insight into the overlapping conditions. Consequently, the current dataset may satisfy several of the Difference-in-Differences model's presuppositions.

4.2 Application

Two pivotal inquiries in assessing the validity of a causal model via observational data pertain to the determinants of treatment allocation and the treated units' knowledge of the outcome variable. Selection bias introduces noise into the estimation of causal effects, necessitating meticulous scrutiny. The dependency of treatment adoption on antecedent conditions of the outcome variable amplifies the need for vigilance in affirming the model's presuppositions.

In the present case, the proposition of a Fare-Free Public Transit policy is conducted by a municipal mayor articulation on the local legislature's endorsement, contingent upon a demonstration of fiscal viability. Given that budgetary projections are speculative, the actual fiscal condition of the municipality does not constitute a primary impediment. Furthermore, the collection of the Service Tax mirrors economic activity, making a contrived effort to inflate it to justify FFTP implementation unlikely.

Moreover, the decision-making process encompasses all local political entities. Debates surrounding FFTP typically engage civil organizations, non-governmental organizations, public transit users, and others, indicating that policy enactment is seldom the purview of a solitary individual or confined to a select group of municipalities. In the

absence of specific legislative mandates for this type of public transit policy, the collective nature of the decision-making precludes the existence of an independent cause within this research milieu that would render all treated entities as unique and thus create insoluble selection bias dilemmas.

Within the context of the reciprocal relationship between treatment and outcome variables, the enactment of FFTP policy does not appear to be a direct consequence of fluctuations in Service Tax revenue. Additionally, since the impact of FFTP on tax revenues remains undetermined, increased tax income is not the primary impetus behind the policy's introduction. Given that public transit expenses often represent a significant portion of household budgets, the political capital gained by the implementing authority may warrant such fiscal commitments. Other motivations outlined in Section 2.1 could also hold significance.

4.3 Tax on Service (ISS) Revenue

The Service Tax (ISS) is intrinsically tied to economic activities, making it vulnerable to shifts in the allocation of funds previously earmarked for public transit costs. It has been suggested that the income elasticity of services is considerable (Fuchs, 1965). This implies that a family's consumption of services and goods tends to increase as its disposable income rises, exemplified by savings from transportation expenses. If services exhibit greater income elasticity than goods, an increase in service consumption patterns and, consequently, ISS tax revenue is anticipated¹⁷.

If this hypothesis is validated, implementing the Fare-Free Transportation Policy could lead to a surge in consumption that exceeds organic growth, thus enhancing tax revenues beyond the existing trend. This potential uptick in consumption could improve individual welfare and strengthen the financial stability of municipal governments via increased tax collections. Nevertheless, the sufficiency of this increase to counterbalance the related expenditures warrants additional empirical scrutiny.

¹⁷Should goods demonstrate greater elasticity than services upon the release of income, a heightened condition of tax on goods would be observed.

4.3.1 Group Treatment Effects

Aggregating groups based on the year of Fare-Free Public Transit adoption entails calculating the mean causal impact on the dependent variable for entities that implemented the policy concurrently. Additionally, the aggregate average treatment effect on the treated is derived as a weighted average across all group values. This dual-aggregated metric more closely approximates the estimator used in conventional “two-way” fixed effects regression models, as referenced by Callaway and Sant’Anna (2021a, p. 211). The model base is given in the Section 2.4.2, which results are aggregated by the process delineated by Equation (2.4.11), while the comprehensive summary of the ATT is computed following Equation (2.4.12). The not yet treated units were also included in the analysis, as part of the control units.

The summary outcomes are displayed in Figure 4, where five groups demonstrated statistically significant positive effects, and four exhibited negative ones. The results are also presented in Table 3. The overall summary is statistically positive and significant, suggesting that the enactment of FFTP is associated with an 10.1% [3.6%, 16.6%] increase in ISS tax revenue, potentially due to enhanced economic activity. It is worth evaluating such results along with the fact that early adopters present more years of results to be averaged.

Notably, earlier adopters show larger effects, yet the divergent outcomes among groups signal a potential dependency on the business cycle and local conditions. Also, the unstable group composition may influence the results. It is recommended that subsequent research address these aspects. Also, the precision of these estimates is likely to improve as additional municipalities adopt the policy.

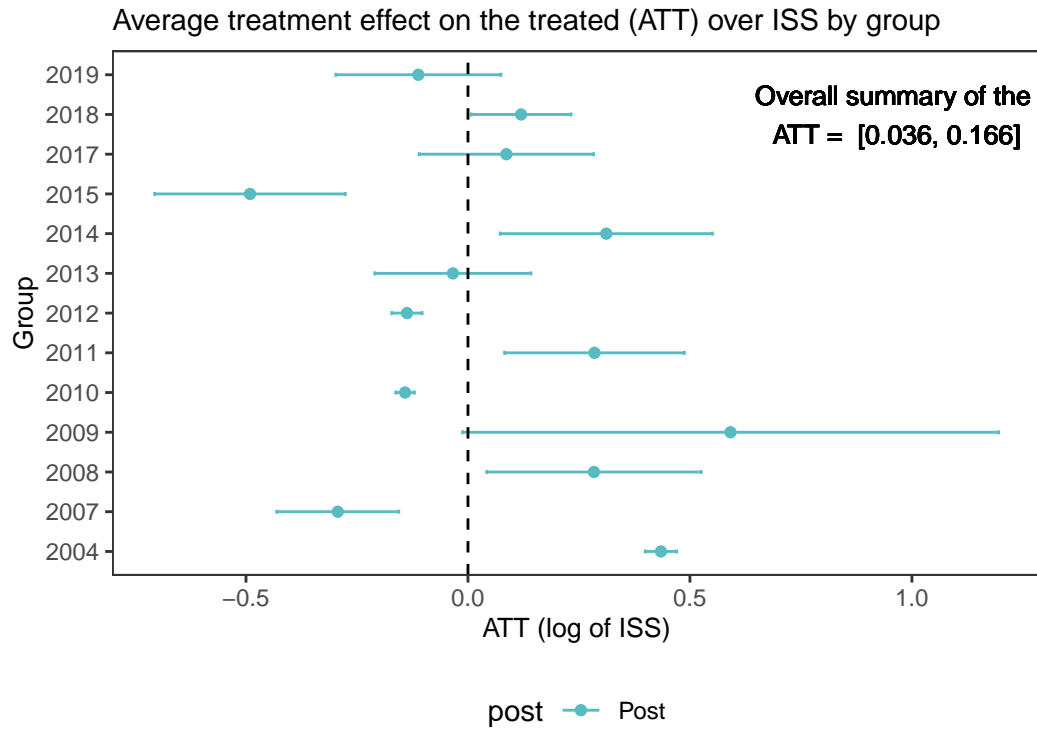


Figure 4: DiD for the average effect of the treatment by group (GDP, population, and area logarithms as covariates).

Table 3: DiD for the average effect of the treatment by group (GDP, population, and area logarithms as covariates).

Event time	Estimate (ATT)	Std. Error	95% Lower Bound	95% Upper Bound	Number of units
2004	0.435	0.015	0.399	0.470	1 *
2007	-0.293	0.057	-0.431	-0.156	1 *
2008	0.284	0.100	0.042	0.525	2 *
2009	0.591	0.249	-0.012	1.195	1
2010	-0.142	0.009	-0.163	-0.121	1 *
2011	0.285	0.083	0.083	0.487	2 *
2012	-0.137	0.014	-0.172	-0.103	1 *
2013	-0.034	0.073	-0.210	0.142	1
2014	0.312	0.099	0.073	0.551	5 *
2015	-0.491	0.089	-0.706	-0.276	1 *
2017	0.087	0.081	-0.110	0.283	2
2018	0.120	0.046	0.007	0.232	4 *
2019	-0.112	0.077	-0.298	0.074	5

Note:

Signif. codes: ‘*’ confidence band does not cover 0.

4.3.2 Dynamic Treatment Effects

The variability of treatment effects may be contingent upon the underlying causal structure. For example, a policy might exhibit lagged effects if its operational mechanism necessitates behavioral modifications among its recipients. Conversely, a treatment could engender an immediate and singular alteration in the outcome variable, devoid of subsequent variations. Thus, an aggregation attuned to the duration of pre- and post-treatment exposure could elucidate the accommodation of dynamic effects.

The temporal influence of FFTP adoption calculated using Equation (2.4.13) is illustrated in Figure 5. At this juncture, the zero-time marker denotes the immediate effect of adoption, while a length of -1 signifies the interval just prior to policy implementation. Preliminary analysis suggests that estimations of the effect are predominantly stable pre-treatment and turn positive post-treatment. A discernible pattern within the graph indicates that the impact of FFTP on ISS revenue collection intensifies progressively, culminating in a peak average value approximately seven to ten years subsequent to adoption. Moreover, there appears to be no deviation from the parallel trend assumption before the introduction of the treatment, as evidenced by the absence of significant disparities between the treatment and control cohorts (as represented by the red dots).

A summative assessment of the treatment effect calculated by Equation (2.4.18) yields a positive outcome (19.7% [4.8%, 34.5%]), signifying a beneficial influence of FFTP on ISS tax accrual. Nonetheless, this aggregation is subject to certain limitations. Given that exposure duration is contingent upon the group and the corresponding temporal progression, the estimates presented are inherently imbalanced — that is, the effects associated with exposure length are assessed solely within entities that have attained such a temporal milestone. To rectify this imbalance, a balanced approach to dynamic effect aggregation may be employed.

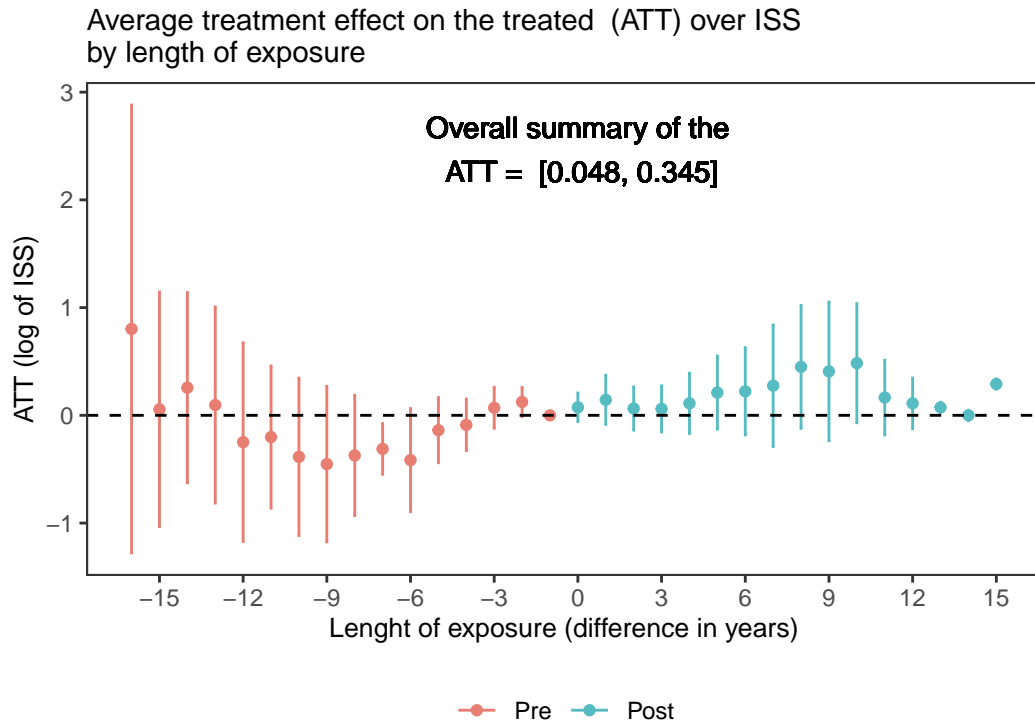


Figure 5: DiD for the average effect of the treatment by length of exposure (GDP, population, and area logarithms as covariates).

4.3.2.1 Balanced Dynamic effects

The dynamic effects of the FFPT policy on the Income from Service Tax revenue are estimated in a balanced manner delineated by Equation (2.4.15) and depicted in the Figure 6. This analysis is confined to municipalities that have implemented the policy for a minimum of five years. Such an approach is advocated in instances of group heterogeneity (Callaway and Sant’Anna, 2021b), averaging the same units across all periods preceding and succeeding the treatment.

Conversely, the dynamics closely resemble those observed when considering all treated units. Prior to the implementation of FFPT, the treated municipalities exhibited negative/null variations in ISS tax collection relative to control groups. Post-treatment, this trend inverted, resulting in positive differentials. The overarching summary — there were 15 units with complete information — indicates an increase in ISS tax revenue by 15.3% [0.2%, 30.4%], albeit with marginal statistical significance.

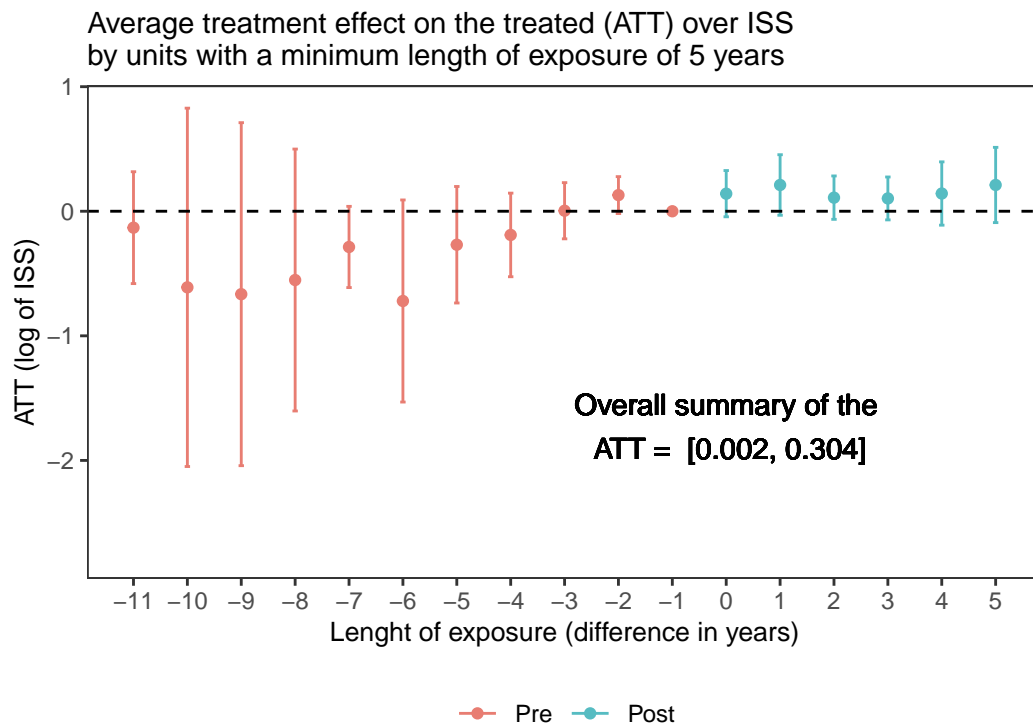


Figure 6: DiD for the average effect of the treatment by length of exposure in balanced aggregation (GDP, population, and area logarithms as covariates).

4.3.3 Calendar-time treatment effects

In conjunction with the aforementioned aggregations, examining the effect in each year of adoption could shed light on the variability of the treatment effect contingent on the year of analysis. The Difference-in-Differences framework neutralizes the influence of unit-specific variables that remain constant over time or affect all units uniformly. The “calendar effect” then measures the impact of year-specific economic conditions on the FFPT policy’s influence on ISS collection. Notable distinct effects suggest that economic cycles may elucidate the variable effects. These calendar effects given by Equation (2.4.17) are illustrated in Figure 7.

In this particular model, the average treatment effect is 21.3% [5.2%, 37.4%], despite the absence of significant discrepancies in treatment effects across the years studied (all positive).

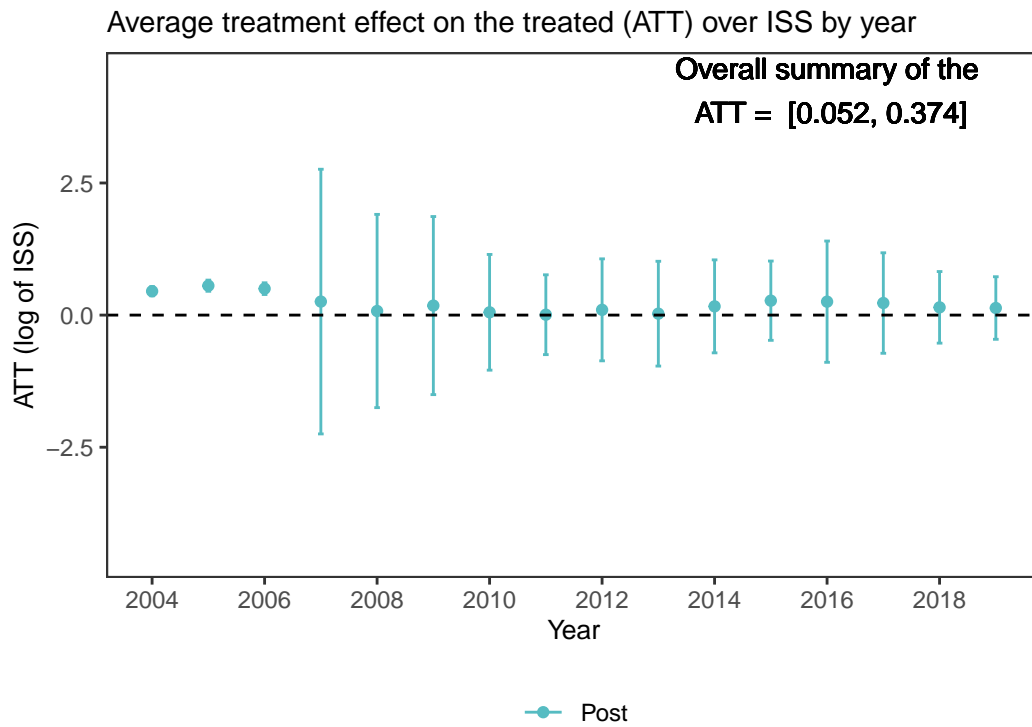


Figure 7: DiD for the average effect of the treatment by calendar time (GDP, population, and area logarithms as covariates).

A last caveat is that the calendar-time treatment effects depends on the size of the groups. The summary statistics are in Table 4.

Table 4: Summary of the DiD for the average effect of the treatment by calendar time (GDP, population, and area logarithms as covariates).

Event time	Estimate (ATT)	Std. Error	95% Lower Bound	95% Upper Bound	Number of units	
2004	0.452	0.011	0.365	0.539	1	*
2005	0.556	0.014	0.451	0.662	1	*
2006	0.500	0.014	0.390	0.611	1	*
2007	0.256	0.320	-2.248	2.760	2	
2008	0.077	0.234	-1.751	1.906	4	
2009	0.180	0.215	-1.505	1.865	5	
2010	0.053	0.140	-1.042	1.148	6	
2011	0.007	0.097	-0.748	0.762	8	
2012	0.100	0.123	-0.865	1.064	9	
2013	0.027	0.127	-0.964	1.018	10	
2014	0.165	0.112	-0.714	1.045	15	
2015	0.273	0.096	-0.477	1.023	16	
2016	0.254	0.147	-0.893	1.402	16	
2017	0.228	0.122	-0.722	1.179	18	
2018	0.147	0.087	-0.530	0.824	22	
2019	0.134	0.076	-0.458	0.726	27	

Note:

Signif. codes: ‘*’ confidence band does not cover 0.

4.4 Sensitivity Analysis

In this Section, a sensitivity analysis was conducted to evaluate the model's robustness. In the Section 4.4.1, distinct sets of covariates are tested. In the Section 4.4.2, the parallel trends assumption is under (although limited) scrutiny.

4.4.1 Evaluating Distinct Covariates Sets

The structure of covariates may be deemed essential to validate the assumption of parallel trends. In the absence of such a structure, the effects estimated could be confounded, leading to biased outcomes. Alternative model configurations could be explored to determine the implications of alterations in the covariate structure. These findings are delineated in Table 5. The absence of covariates results in group and dynamic effects that are statistically indistinguishable from zero. This indicates that variables such as population and GDP are influential factors in the ISS collection outcomes when contrasting treated and untreated groups, given that their distributions do not fully coincide (refer to Figure 3).

Table 5: Estimated average treatment effect by different covariates structures.

Covariate(s)	Lower bound	Upper bound
Group		
Intercept only	-0.013	0.142
Population and Area	0.000	0.151 *
GDP and Population	0.033	0.162 *
GDP, Population and Area	0.036	0.166 *
Dynamic		
Intercept only	-0.008	0.224
Population and Area	0.030	0.267 *
GDP and Population	0.038	0.330 *
GDP, Population and Area	0.048	0.345 *
Calendar-time		
Intercept only	0.015	0.304 *

Population and Area	0.039	0.337	*
GDP and Population	0.040	0.367	*
GDP, Population and Area	0.052	0.374	*

Signif. codes: ‘*’ confidence band does not cover 0.

All models include an intercept.

A comparison between the models with complete and partial covariates reveals a convergence of values, although the model with the full complement of covariates registers marginally higher values. This convergence bolsters the reliability of the model’s findings. Consequently, a comprehensive covariate set, which aligns with the theoretical framework, ensures the maintenance of the parallel trend assumption and the exclusion of confounding variables.

4.4.2 Parallel trends: an event-study

The assumption of parallel trends constitutes a foundational premise in the evaluation of causal inferences, despite its inherent unverifiability within empirical contexts. A methodological approach to approximate the validation of this assumption is the implementation of an event-study analysis, which scrutinizes the consistency of parallel trends prior to the intervention. This is exemplified in Figure 8, wherein the grey data points demarcate the pre-intervention phase, revealing negligible discrepancies antecedent to the adoption of treatment. Conversely, post-treatment estimations corroborate the findings delineated in antecedent sections.

Event-studies serve as a pivotal methodology for estimating dynamic treatment effects utilizing panel data, wherein the zero point demarcates the inaugural year of intervention. Within this framework, temporal combinations antecedent and subsequent to treatment adoption are encapsulated in the regression model. Miller (2023) posits that an absence of trend is anticipated prior to the treatment. Deviations from this expected pattern may indicate the presence of confounding variables, potentially rendering the model specious. Such anomalies also intimate potential issues with the assumption of parallel trends.

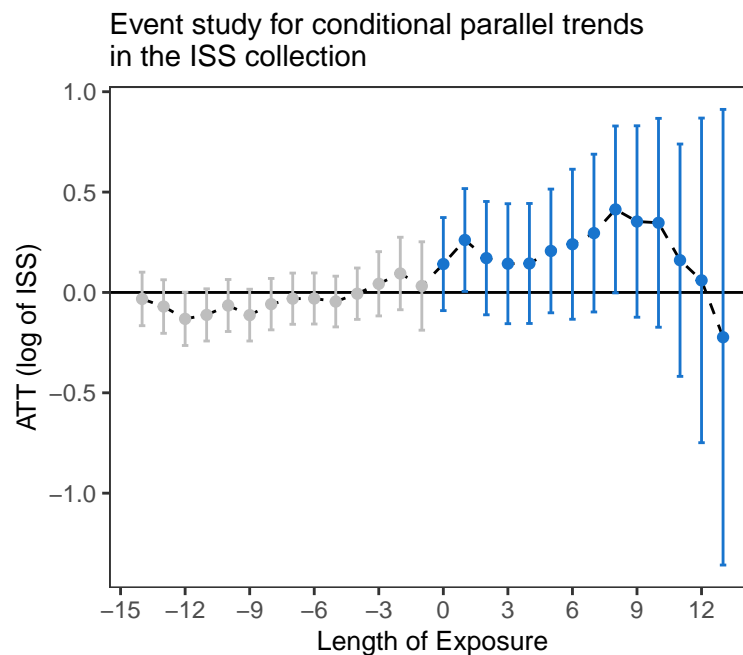


Figure 8: Event-study for the average effect of the treatment on the ISS collection (GDP, population, and area logarithms as covariates).

The parallel trend assumption endeavors to authenticate that the outcomes observed are not attainable independently of the policy implementation. While the assumption is intrinsically linked to the post-treatment parallel trend — rendering it empirically unverifiable — the analysis of the pre-treatment phase can provide some evidentiary support. Absent pre-treatment convergence, the plausibility of post-treatment parallel trends sustaining becomes questionable.

5 Discussion and Conclusion

The feasibility of an experimental design involving the random implementation of Fare-Free Public Transit (FFPT) is compromised due to its inherently political nature, which is contingent upon the agenda of local governance. Consequently, the assessment of causal relationships necessitates an observational study design. The Differences-in-Differences (DiD) analytical framework provides a robust structure for drawing causal inferences, provided that the underlying assumptions are met. The validity of causal effect identification is contingent upon the satisfaction of parallel trends, the Stable Unit Treatment Value Assumption (SUTVA), and the absence of anticipation effects. The incorporation of covariates allows to make causal affirmations, ensuring the robustness of the model's assumptions.

Upon the application of the DiD analytical framework, the empirical evidence suggests a statistically significant positive impact of the Fare-Free Public Transit policy on the collection of the Service Tax (ISS). The computed average treatment effect the treated (ATT) across the cohort of municipalities that concurrently instituted the fare exemption reveals an augmentation of ISS revenues by an estimated 10.1% — the 95% confidence interval is [3.6%, 16.6%]. This estimator aligns closely with the conventional two-by-two DiD model. The set of covariates is given by the logarithms of GDP, population, and area.

Upon examination of the dynamic effects contingent relative to the duration of policy adoption, the assessed ATT was 19.7% [4.8%, 34.5%]. The substantial confidence interval suggests that, notwithstanding the limited sample size, the ramifications of the FFPT effect on tax collection may exhibit temporal variability. Subsequent metrics corroborate this positive trend, albeit with marginally varying magnitudes. The observed fiscal enhancement is ostensibly linked to a reallocation of household spending from public transit fares to the consumption of services and goods. The redirection of financial resources from fare-based funding to alternative mechanisms is imperative for the sustained

implementation of this public policy.

However, it is imperative to acknowledge the limitations inherent in this methodological approach, particularly when applied to a limited sample size, as is the case in our study. With only 27 municipalities adopting the FFPT within the studied timeframe, the confidence intervals for group-specific or temporal analyses often yield inconclusive results, despite more robust aggregated data. Consequently, further research incorporating a larger sample size is advisable when additional data becomes available. For instance, as of May 2024, the number of municipalities that have adopted the FFPT has expanded to 108. This expansion is anticipated to provide more robust estimators for both overall and dynamic analyses. Furthermore, an exhaustive examination of the modifications within the tax legislation could significantly enhance the study. This is particularly pertinent if the newly proposed bill permits a phased enactment. Such an analysis would provide a comprehensive understanding of the incremental changes and their potential impacts on the application of the tax law.

Further exploration into the motivations behind the adoption of FFPT is necessary. The observed behaviors exhibited by the municipalities with the highest and lowest performance metrics provide a foundational basis for understanding the underlying causes of the pronounced variability observed between these cohorts. This comparative analysis is essential for elucidating the factors that contribute to the disparities in municipal performance outcomes. Moreover, given that selection bias is a significant assumption in the Difference-in-Differences model, elucidating the determinants of policy adoption can strengthen the validity of the estimated causal effects. Typically, qualitative research methods are better suited to explore the decision-making processes, the knowledge base of the policymakers, and the permissible sources of selection to being treated. Future research endeavors should consider the disaggregation of service sectors as a focal point of study. This approach is pivotal to elucidate the nuanced impacts of the FTTP on distinct service industries.

Enhancing access to public transit serves as a pivotal strategy for combating social exclusion, diminishing greenhouse gas emissions from personal vehicles, and bolstering

urban mobility. Moreover, the allocation of public funds to this essential service reinforces the concept of right to city. Therefore, subsequent research could provide valuable insights into the fiscal viability of such initiatives, ensuring their long-term sustainability and effectiveness.

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