

Old and Informal: The Labor Market Effects of Social Pensions in Brazil

Rodrigo Toneto*

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Abstract

How can informal economies protect those excluded from contributory systems without undermining incentives to formalize? As many of these countries undergo rapid demographic transitions, policymakers struggle to design sustainable and inclusive pension schemes. This paper examines the impact of expanding social pensions on local labor markets in Brazil. Leveraging administrative data on benefits and employment spells, as well as the quasi-random rollout of pension agency branches, I find that higher social pension take-up increases private formal employment without affecting average wages. I can trace the local employment result back to individual responses by combining household survey data with administrative bank credit records. Four interrelated factors help explain the aggregate effect: increased household net income following pension receipt, a high marginal propensity to consume among beneficiaries, non-homothetic preferences that redirect spending toward formal retailers, and improved access to payroll-deducted loans.

*Queen Mary University of London and Institute for Applied Economic Research (IPEA - Brazil).
Email: r.toneto@qmul.ac.uk

1 Introduction

The population aged 65 and over is projected to at least double in most low- and middle-income countries (LMICs) over the next 30 years (United Nations, 2019; OECD, 2023). This demographic shift poses challenges for the design of social protection systems, particularly in contexts where high labor informality simultaneously narrows the tax base and leaves most older individuals ineligible for contributory pensions. In response, non-contributory pension schemes —publicly funded transfers provided regardless of prior contributions, now implemented in over 100 countries (Banerjee et al., 2022) — have gained importance and are expected to absorb a growing share of public resources. While these schemes protect those excluded from contributory systems, they may also weaken incentives to work in the formal sector (Levy and Schady, 2013; Ulyssea, 2020). On the other hand, by providing a stable source of income and easing liquidity constraints among former informal workers, they could stimulate formal employment through local demand effects (Corbi et al., 2019; Egger et al., 2022; Gerard et al., 2024). Understanding how these opposing forces shape labor market dynamics is thus essential for designing sustainable and inclusive social security systems in highly informal economies.

This paper examines how expanding social pension payments affects local labor markets, tracing the link between aggregate employment dynamics and household-level responses. I empirically evaluate this question in the context of Brazil, a developing economy with pervasive informality, which allocates roughly 10% of its GDP to its pension system. Using two comprehensive administrative datasets on the universe of pensioners and formal employment from 2007 to 2018, I leverage the quasi-random establishment of new pension agency offices to identify the causal effects of increased pension registration. The availability of an office leads to increased pension take-up, primarily through the expansion of non-contributory benefits. This, in turn, raises formal private employment, particularly in the non-tradable sector. I then combine household expenditure surveys with administrative credit records to unpack how pension income shapes household behavior in ways that propagate into local labor market effects. The results are consistent with an income-driven formalization process.

To address concerns about endogenous placement, I exploit a policy reform that created exogenous variation in the timing of agency openings across municipalities. In 2011, Brazil’s National Institute of Social Security (INSS) launched a national initiative to expand its agency network. I digitize internal planning documents to recover the list of municipalities officially designated for expansion and link this information to administrative records on the timing of agency openings. According to the INSS, target municipalities were those that, based on institutional criteria, should have received an agency by 2010 but had not. My identification strategy leverages the staggered implementation of this policy across otherwise similar municipalities to recover the causal impact of expanded pension coverage on local labor markets. In my preferred specification, I use the estimator proposed by Callaway and Sant’Anna (2021) to compare municipalities treated earlier with those treated later or never treated, addressing potential biases from staggered adoption and treatment effect heterogeneity.

The presence of a local agency increases social pension take-up by 4.9%. The resulting increase in pension transfers leads to a 5.1% rise in private-sector formal employment,

concentrated in the non-tradable sector. There is no increase in public employment, ensuring that the employment gains are not driven by direct hiring within the public sector. Nor is there any significant effect on average wages, consistent with the view that weak demand, rather than labor-supply frictions, previously constrained formal job growth. The estimated cost per job is approximately USD 6,100, aligning closely with prior studies of fiscal multipliers in Brazil (Corbi et al., 2019; Gerard et al., 2024). Results are robust across multiple difference-in-differences estimators, comparison groups, and the inclusion of baseline covariates.

One potential concern is that the observed effects may be confounded by broader policy actions undertaken by politically active mayors, which could coincide with the arrival of pension agencies. I address this by showing that the timing of agency openings is largely outside the control of a single actor. Moreover, I document that the opening of an agency is not accompanied by changes in a wide array of municipal-level quality indicators, suggesting that the expansion process was orthogonal to other local policy shifts. Another concern is that the estimated effects might not be driven by increased pension payments *per se*, but rather by broader access to other social benefits that may have become more accessible with the arrival of an agency. However, I show that conditional cash transfer disbursements did not increase following the opening of an agency; in fact, they declined in subsequent years. This pattern suggests that, by providing social insurance to previously informal workers and stimulating local demand and employment, an inclusive safety net reduces reliance on targeted poverty alleviation programs.

To investigate the mechanisms behind this employment response, I combine household expenditure surveys with administrative bank-level credit data. Using the nationally representative Household Budget Survey (POF), I exploit age-based eligibility thresholds to causally estimate the impact of receiving a pension on household income composition, consumption patterns, and access to credit. Pension benefits stand out as a stable income source, which makes them eligible as collateral for payroll-deducted loans offered at subsidized interest rates. I utilize bank-level data to investigate how the establishment of a pension agency impacts the stock of consumer credit at the municipal level.

The results indicate four complementary factors that contribute to explaining the observed aggregate increase in formal employment. First, households experience sizable gains in net income following the receipt of pensions, despite reductions in labor income. Second, households that benefit from social pensions have a high marginal propensity to consume. Third, consumption shifts toward formal retailers, consistent with non-homothetic demand for formal goods. Finally, pensions expand household access to consumer credit, further reinforcing demand for formal sector goods and services.

This paper contributes to the literature on informality and the design of social insurance. In my setting, more than half of all jobs were informal, and 76% of pensions are either non- or partially contributory. A large body of research has examined how different social insurance arrangements affect individual incentives to formalize (Joubert, 2015; Cruces and Bergolo, 2013; Canelas and Nino Zarazua, 2022; McKiernan, 2021; Finamor, 2022; Joubert and Kanth, 2022; Delalibera et al., 2023). A central concern is that generous non-contributory pensions may reduce the perceived value of formal employment by weakening the link between contributions and future benefits (Levy and Schady, 2013; Ulyssea, 2020). This paper demonstrates that in contexts characterized by slack labor markets and

where social pensions reach liquidity-constrained households, the aggregate employment effects of income support — operating through local demand and credit markets — can outweigh potential disincentives to formalize. Taken to the national level, these findings imply that if the labor supply elasticity to taxation in highly formal regions is sufficiently low, redistributive transfers from formal to informal regions — via social pensions — can serve as an effective mechanism to reduce spatial labor market disparities.

The paper also contributes to a growing literature on the local effects of government transfers in low- and middle-income countries (LMICs). While much of the fiscal multiplier literature focuses on advanced economies and spending on infrastructure or public employment (e.g., [Nakamura and Steinsson, 2014](#); [Serrato and Wingender, 2016](#); [Auerbach et al., 2019](#)), recent work has begun to explore the employment effects of income transfers in LMICs ([Kraay, 2012](#); [Corbi et al., 2019](#); [Egger et al., 2022](#); [Gerard et al., 2024](#)). I add to this literature by studying the effects of expanding social pensions—arguably the most widespread form of income transfer globally, and one that is projected to absorb a growing share of public budgets in LMICs. Unlike temporary or conditional programs, pensions are large, permanent, and predictable, targeting older adults who often lack access to contributory schemes due to lifetimes spent in informal employment. Using administrative data from Brazil and a staggered roll-out of pension agency branches, I show that higher pension coverage increases formal private employment. Beyond documenting formal employment gains, I trace the full transmission mechanism — from income receipt to consumption, preferences, and credit — thus deepening our understanding of how social transfers propagate through the economy.

In doing so, the paper also contributes to the literature on income-driven structural change. I show how stable, verifiable income flows reshape household economic behavior—raising consumption, shifting demand toward formal outlets, and expanding access to credit. These behavioral margins, in turn, aggregate into local formal employment growth, particularly in the non-tradable sector. The results align with research on non-homothetic preferences and the role of income growth in driving formalization and sectoral reallocation ([Matsuyama, 2019](#); [Buera and Kaboski, 2009](#); [Comin et al., 2021](#); [Bachas et al., 2023](#); [Fan et al., 2023](#); [Gollin and Kaboski, 2023](#)), particularly the expansion of services emphasized by [Buera and Kaboski \(2012\)](#) and [Fan et al. \(2023\)](#). My findings also echo arguments that insufficient demand can constrain productivity growth and structural transformation in developing economies made by [Murphy et al. \(1989\)](#) and [Goldberg and Reed \(2023\)](#). By linking household-level responses to aggregate labor market effects, this paper provides evidence on how social policy can shape the trajectory of structural transformation in low- and middle-income economies.

This paper builds on evidence that social pensions improve household welfare. For example, in the context of South Africa, [Duflo \(2000\)](#) shows that pension income improved child health and household resources. More recently, [Huang and Zhang \(2021\)](#) show that pension receipt in China increases household income, food consumption, and self-reported health¹. While this literature has documented individual-level impacts, it has paid less attention to general equilibrium effects. This paper fills this gap by showing how so-

¹See also [Case and Deaton \(1998\)](#); [Duflo \(2003\)](#); [de Carvalho Filho \(2008\)](#); [Ardington et al. \(2009\)](#); [Kaushal \(2014\)](#); [Galiani et al. \(2016\)](#); [Ceni \(2017\)](#); [Becerra \(2017\)](#) for additional evidence on the household-level effects of pensions on household welfare, labor supply, and intra-household dynamics.

cial pensions can scale up to influence aggregate labor market outcomes through local demand channels, bridging household behavior and macro-level employment dynamics. This perspective aligns with recent work by [Hackmann et al. \(2023\)](#), who demonstrate that subsidies for aging-related sectors can stimulate local labor markets, and resonates with broader policy debates surrounding aging populations and the employment implications of demographic change ([Hou et al., 2024](#)).

The paper proceeds as follows: Section 2 discusses the institutional context and data. Section 3 presents the empirical strategy. Section 4 presents the results for the aggregate-level impact of the pension expansion. Section 5 provides evidence on how pensions affect formal employment growth through household decisions. Section 6 concludes.

2 Context and data

2.1 Pensions system in Brazil

The Brazilian pension system combines contributory and non-contributory schemes with distinct rules for urban and rural areas. In the contributory scheme, there are specific regulations for certain public service careers. The system also provides coverage for workers who are permanently incapacitated because of illness or accident. Additionally, spouses or children can access survivor pensions if their partner or parent has died. The duration of these benefits varies based on the age of the dependent and the number of months the deceased insured person had contributed.

Before the implementation of the online platform "INSS Digital" in late 2017, the process of obtaining an INSS pension in Brazil involved several steps, requiring physical presence and substantial paperwork. Applicants had to visit an INSS agency to initiate their application. The procedure began with scheduling an appointment, which could be done either by phone through the INSS service line or by visiting the agency directly. During the appointment, applicants had to present various documents, including identification, proof of contribution periods, and other relevant paperwork depending on the type of pension sought (e.g., retirement by age, disability, survivor's pension).

This process often involved multiple visits to the agency, as initial appointments were used to verify the documentation and provide a formal list of any additional documents needed. Applicants were required to fill out detailed forms and submit them along with their documents for processing. The INSS officials would then review the submission, a process that could take several weeks. During this period, the potential beneficiaries could track the status of their request by revisiting the agency or through follow-up phone calls.

For rural workers or those seeking disability pensions, additional documents, such as medical reports or certifications from rural workers' unions, were necessary, adding to the complexity and length of the process. The in-person nature of the process, coupled with the bureaucratic requirements, often led to significant barriers to accessing the system.

Given the in-person nature of the application process and the multiple appointments typically required, accessing pensions can be particularly costly in the absence of a local agency. To illustrate the magnitude of these costs, consider Iúna, the median municipality in our treated sample in terms of baseline benefit levels, located in the relatively wealthy state of Espírito Santo. Reaching the nearest agency required either a 2.5-hour drive or a 4-hour and 40-minute bus trip, with transportation expenses— whether bus fares or fuel costs — ranging from one-third to one-half of the monthly minimum wage in 2010.

In this paper, I use the cost reduction associated with establishing a new social security facility in a given municipality as a source of variation in pension take-up. Here, cost can be broadly understood to include monetary, time, and informational costs associated with obtaining benefits.

It is worth noting that INSS agencies operate exclusively within the pension system and are not involved in the administration or delivery of other social programs, such as *Bolsa Família* or unemployment insurance. These benefits fall under the jurisdiction of separate ministries and federal agencies—such as the Ministry of Social Development and the Ministry of Labor—and rely on entirely different eligibility systems and service networks. As such, INSS agencies do not provide information, referrals, or administrative support for programs outside the pension framework.

Understanding the economic context is also essential to thinking about the potential local employment effects. In 2011, prior to the expansion program, the median ratio of pension beneficiaries to formal employees in target municipalities was 2.2. This elevated ratio reflects the structure of Brazil’s non-contributory pension schemes, which enable rural and informal workers, as well as individuals with long periods of unemployment, to retire with a pension. At the national level, the ratio was 1.7 in the same year. These figures highlight the pivotal role of the pension system as a reliable source of income in Brazil’s smaller municipalities, where labor markets are characterized by informality, seasonal rural work, home production, and high unemployment rates.

In what follows, I detail the policy that expanded the agency network, allowing me to exploit quasi-experimental variation in the timing and location of new openings.

2.2 Expansion Plan - PEX

According to pension system regulations, the decision to open an agency should be based on the municipality’s total population, potential demand for benefits, and the distance to an existing facility. In practice, however, these criteria are weighted in such a way that it becomes difficult to predict which municipalities will be selected. Therefore, simply comparing cities with or without an agency would still be affected by selection bias. To address this issue, I would ideally compare a municipality that was due to receive an agency but did not, with one that was targeted and did receive an agency.

In Brazil’s 2012–2015 multi-year strategic plan (PPA 2012–2015), launched in 2011, the National Institute of Social Security (INSS) set a goal to expand its network of pension agencies. The objective was to extend coverage to municipalities that met a combination

of criteria, including a population threshold and/or significant distance from the nearest existing agency, with a focus on treating municipalities with a population above 20,000 inhabitants. With this expansion, the government aimed to bring services to 1,685 out of 5,570 Brazilian municipalities by the end of 2016. The INSS granted me access to documents that define the list of the 529 targeted municipalities and the corresponding agency opening dates. From these data, I observe that 288 agencies were built between 2012 and 2016, while 241 were ultimately not delivered by the end of the period. Figure 1 shows the roll-out of the policy from 2012 until 2016.

The construction of new pension agencies depended on both municipal initiative and central government capacity. On the one hand, municipalities needed to actively express demand by preparing and submitting all required documentation. On the other hand, the federal government determined the timing of construction based on the availability of budgetary resources. This dual mechanism — local demand conditional on compliance with technical criteria and centralized supply subject to budget constraints — shaped the pace and geography of the expansion. As shown in Figure 2, there is no clear geographical pattern in the target municipalities or in those that were effectively treated. Out of the 26 Brazilian states, only one did not receive any agency.

On the supply side, the central government prioritized agency construction requests solely based on the order in which municipalities completed the application process. Each year, new projects were initiated according to the budget allocated to the INSS for agency construction. As shown in Figure 3, annual construction budgets closely mirror the roll-out pattern depicted in Figure 1. Importantly, the agency construction budget is set by the federal Budget Office and lies outside the control of both municipalities and the INSS itself, for whom infrastructure delivery is only one among several competing spending mandates. Even in 2012—the year with the highest allocation—agency construction accounted for less than 1% of total INSS expenditures.

On the demand side, municipalities seeking to receive an agency were required to donate a plot of land that met specific technical criteria: a minimum area of 1,000 square meters, flat topography, and a central, easily accessible location. The process involved several administrative stages. First, the mayor proposed a suitable plot; next, INSS engineers conducted a site inspection to verify its compliance with construction standards. If deemed adequate, the mayor then submitted a land donation bill to the municipal council. Once the bill was approved and the donation formally authorized, the municipality could be registered in the construction queue.

Importantly, while municipalities had partial control over the administrative aspects of the application—such as completing paperwork or securing eligible land—they had no control over whether the central government would fund their agency in a given year. This results in quasi-random variation across municipalities in both whether and when the agency was ultimately delivered.

The accountability report for the PEX policy in 2012 highlights several challenges faced by the federal government in meeting its agency expansion targets. Out of the 220 planned agencies for that year, only 118 were actually inaugurated.²

²See the 2012 Accountability Report: <https://www.gov.br/previdencia/pt-br/assuntos/prestacao-de-contas/relatorios-anuais/relatorio-de-gestao-2012.pdf>.

The report attributes this shortfall to a combination of fiscal, administrative, and logistical constraints. These included lower-than-anticipated federal budget availability for agency construction, the inadequacy or non-compliance of municipal land plots, and significant delays in the legal transfer of land ownership from municipalities to the federal government. Additional obstacles stemmed from procurement inefficiencies—particularly related to the contracting of engineering and technical services—and from adverse weather conditions that disrupted construction timelines.

2.3 Data

These are the main data sets used in this research:

- **RAIS** The RAIS (Relação Anual de Informações Sociais) is an employer-employee administrative dataset collected annually by the Brazilian Ministry of Labor. It provides comprehensive data on formal employment across all sectors of the Brazilian economy. The RAIS dataset includes detailed information on employees, such as their demographic characteristics, job tenure, wages, and occupation. It also covers firm-level details like industry classification and location.
- **INSS payment sheets:** administrative records maintained by the INSS, updated using the same type of information (social security numbers) as the widely used RAIS. These records provide detailed information on the total payments and benefits disbursed in each category of pension. The payment sheets cover all Brazilian municipalities and encompass yearly data from the year 2007 to 2018.
- **Municipality Target Lists for PEX Expansion:** For each state, the INSS defined a list of municipalities selected for the expansion program (PEX), documented in internal reports and administrative records.³ I compiled and digitized these materials to construct a dataset identifying targeted municipalities across states.
- **Agency Opening Dates:** The list of the exact dates on which each new INSS agency began operating.
- **Bolsa Família Disbursements:** Monthly data on total disbursements and beneficiary counts of the *Programa Bolsa Família* at the municipal level, publicly available from the Ministry of Social Development. This dataset allows me to assess whether access to targeted conditional cash transfers changed following the expansion of pension agencies.
- **Municipal Quality Indices (IGD, TAFE, TAAS):** These indices measure the quality of municipal implementation of social programs. The *Índice de Gestão Descentralizada* (IGD) reflects overall administrative performance in managing the

³These documents also contain information on unrelated internal discussions and operational matters within the INSS, which are not publicly disclosed. As a result, accessing them required formal authorization and confidentiality agreements.

social registry and monitoring conditionalities. The TAFE and TAAS indices specifically measure the quality of school attendance monitoring and health service follow-up (e.g., prenatal visits and vaccination coverage), respectively. While these indicators are publicly available only from 2015 onwards, I obtained access to a longer time series (2008–2018) with the department responsible for paying municipalities according to their performance.

- **CENSO 2010** The 2010 Census dataset in Brazil offers detailed information at the household level. It allows me to recover municipal-level covariates such as informality levels, age profiles, income, and education.
- **POF** The POF (Pesquisa de Orçamentos Familiares) is a household budget survey conducted periodically by the Brazilian Institute of Geography and Statistics (IBGE). I use the 2008-2009 wave of the survey that encompassed a nationally representative sample of 55,970 households. It provides detailed information on household income, expenditure, and consumption patterns, including spending on categories such as food, housing, transportation, education, and healthcare. The survey also includes data on income sources, asset ownership, and the place of acquisition for each good or service consumed. This feature enables the classification of expenditures by store type. Following the methodology of [Bachas et al. \(2023\)](#), I classify purchases as occurring in formal or informal establishments based on the reported place of acquisition.
- **Estban** The Central Bank of Brazil produces the Estban municipal balance sheet dataset. It contains monthly information on the balance sheet positions of key credit and deposit accounts, disaggregated at the municipality level for private and public banks. The dataset includes the stock of credit operations by type (e.g., consumption, housing, corporate) and reflects the monthly position as reported by financial institutions.

To clearly establish the timing of events, employment and benefits data refer to December of each year from 2008 to 2018. A municipality is considered treated in a given year if its first pension agency opened within that year; all agency openings occur before October.

3 Empirical Strategy

The primary goal of this research is to identify the employment effect of an increase in pension earnings within local labor markets. A simple correlation between total pension payments and employment levels and composition could suffer from significant endogeneity issues. For example, a sluggish labor market with fewer employment opportunities in dynamic sectors could lead to increased pension take-up (reverse causality). Conversely, more densely populated local labor markets may simultaneously offer greater access to public services and economic diversification, creating omitted variable bias. To address these endogeneity concerns, I exploit quasi-experimental variation from the establishment of pension agencies in specific municipalities.

Staggered differences-in-differences approach: The variation in the timing of the opening (or non-opening) of pension agencies in municipalities targeted by the PEX program allows me to identify the causal effect of expanding pension system coverage on labor market outcomes. Specifically, I compare the evolution of outcomes over time in municipalities that were treated early with those treated later and with those never treated. Each municipality i is assigned to a treatment cohort defined by the year $G_i = g \in 2012, \dots, 2016$ when the pension agency opened. Municipalities that never received an agency or received one after the analysis period are assigned $G_i = \infty$.

The applied literature conventionally relies on the two-way fixed-effects (TWFE) estimator in settings like this. In this context, it implies estimating the following equation:

$$y_{it} = \gamma_i + \lambda_t + \sum_{e \neq g-1} \beta_e \times D_{it}^e + \varepsilon_{it} \quad (1)$$

where y_{it} represents the outcome of interest for municipality i in year t . The model includes municipality fixed effects (γ_i) to control for time-invariant local characteristics and year-fixed effects (λ_t) to account for common shocks that affect all municipalities. The key independent variables are a set of indicators, D_{it}^e , which take the value one if municipality i has already been treated for e periods by time t . The coefficient β_t measures differences in outcomes e periods after the agency's arrival in treated municipalities and those not yet treated by that time. Standard errors are clustered at the municipality level to account for serial correlation within municipalities ⁴

A fast-growing econometric literature documents that using conventional two-way fixed effect (TWFE) regressions might produce misleading estimates of treatment effect⁵. The central issue is that, in the presence of heterogeneous effects across groups and time periods, the implied weighting scheme is not innocuous and can lead to biased estimations. In settings with staggered treatment adoption and heterogeneous cohort sizes, as is the case here, [Sun and Abraham \(2021\)](#) discuss that TWFE can be particularly misleading.

My preferred estimator is [Callaway and Sant'Anna \(2021\)](#). Their method addresses the issue of forbidden comparisons while maintaining a less stringent parallel trend assumption ([Marcus and Sant'Anna, 2021](#); [De Chaisemartin and d'Haultfoeuille, 2023](#)). It also easily accommodates covariates ⁶. Still, my results are robust to different estimation methods.

The estimator proposed by [Callaway and Sant'Anna \(2021\)](#) identifies the average treat-

⁴While Brazil's micro-regions approximate commuting zones, they are relatively large territorial units—each encompassing, on average, about 11 municipalities. Most already had at least one agency in place before the expansion. Since treatment varies at the municipality level, this is also the relevant unit for identification.

⁵see [De Chaisemartin and d'Haultfoeuille \(2023\)](#) and [Roth et al. \(2023\)](#) for a comprehensive review of the new available methods

⁶Additionally, [Callaway \(2023\)](#) demonstrate how these measures compare with different strategies suggested in the literature. Specifically, the [Callaway and Sant'Anna \(2021\)](#) estimator can be expressed as an imputation estimator, similar to the approaches by [Borusyak et al. \(2024\)](#) and [Gardner \(2022\)](#). Moreover, the [Callaway and Sant'Anna \(2021\)](#) never-treated estimator can be mapped into regression approaches, such as those developed by [Sun and Abraham \(2021\)](#) and [Wooldridge \(2021\)](#).

ment effect for units in cohort g e periods after treatment. It is defined as:

$$\widehat{ATT}(g, e) = \frac{1}{N_g} \sum_{i:G_i=g} (Y_{i,g+e} - Y_{i,g-1}) - \frac{1}{N_{g'}} \sum_{i:G_i=g', g'>g+e} (Y_{i,g+e} - Y_{i,g-1}), \quad (2)$$

where $Y_{i,t}$ denotes the outcome of unit i at time t , G_i is the period in which unit i first receives treatment, and N_g and $N_{g'}$ are the number of units in the treated and not yet treated cohorts, respectively. The first term captures the average outcome change for units treated in period g between the pre-treatment baseline ($g-1$) and e periods after treatment. The second term estimates the same outcome change for units that will only be treated after period $g+e$. This estimator identifies the effect of treatment for cohort $G_i = g$ under a parallel trends assumption, which states that the average change in the outcome between periods $g-1$ and $g+e$ for cohorts not yet treated by time $g+e$ serves as a valid counterfactual for what cohort g would have experienced in the absence of treatment.

Identification: The model will correctly identify the causal impact of agency arrival assuming that, conditional on municipality fixed effects — which capture time-invariant factors such as bureaucratic capacity and land availability — and time fixed effects — which account for year-to-year changes in central government fiscal constraints — the timing of agency arrival is orthogonal to trends in the outcome variable. In the following section, I show that conventional event study plots lend support to this assumption by revealing no systematic differences across groups in pre-treatment trends.

The main threat to my identification strategy would be the presence of time-varying unobservable municipal-level characteristics that affect the labor market and change precisely when the agency arrives. Specifically, one could worry that the agency’s arrival coincides with broader local government efforts to improve public services, and that these broader initiatives—rather than the agency itself—drive the observed effects on labor market outcomes or pension take-up.

I argue this is unlikely to be the case. To open a new agency, a municipality must first secure a suitable plot of land in the city center that meets specific technical criteria. Once the land is available, the municipality enters a queue to receive funding. Each year, the central government allocates funds according to its annual budget and decides how many municipalities from the queue will move forward with construction. If selected, the construction begins and typically takes between 10 and 16 months; if not, the municipality must wait until the following year. This multi-step process—dependent on local land availability and annual federal budget constraints—introduces variation in the timing of agency openings that is largely outside the control of local governments. Therefore, while municipalities may share similar observable characteristics, the exact timing of treatment is plausibly exogenous to other simultaneous policy efforts. Consistent with this interpretation, I show that a range of municipal-level quality indices do not exhibit coordinated changes around the timing of agency openings—reinforcing the view that the arrival of a new branch was not part of a broader, synchronized initiative to improve local public service delivery.

A different potential concern relates to the implied exclusion restriction. For my inter-

pretation of the results to be valid, it must be the case that the only channel through which the agency affects the labor market is by reducing the cost of pension registration, thereby increasing access to benefits. One could worry that the establishment of an agency might itself mobilize local labor resources—either through construction or public hiring—directly influencing employment. In the next section, I show there is no evidence supporting this concern. Finally, the arrival of an agency does not appear to affect the take-up of other social programs outside its administrative scope, which helps rule out the possibility that the physical presence of the state broadly improves access to social policies. It is also worth noting that treatment is absorbing, as no agency closures were observed during the analysis period.

Descriptive statistics in Table 1 compare baseline characteristics between municipalities treated at different times and those never treated. The first two columns report mean values of key variables—such as total population, share of the population receiving pension benefits, private-sector employment share, average wages, total number of establishments, for eventually treated and never-treated groups. This baseline similarity reinforces the assumption that subsequent differences in outcome trends can be attributed to the timing of treatment, rather than to pre-existing differences between municipalities.

4 Main Results

4.1 Main Results

The goal of this section is to show how variations in social pension payments within a municipality affect its labor market. To identify these effects causally, I exploit an exogenous shift in benefit levels induced by the opening of a local pension agency. For this strategy to be valid, the arrival of the agency must increase pension take-up.

Figure 4 and Panel A of Table 2 present the dynamic and average treatment effects under the main specification described in Equation 2. I begin by showing that pension payments respond to agency openings. I then examine the effects on private formal employment, with a particular focus on the non-tradable sector, and conclude with results on average wages.

The cost reduction in benefits access induced by the opening of an agency is expected to increase take-up, particularly among liquidity-constrained workers. This is precisely what Panel (a) of Figure 4 shows. The event study plot depicts the evolution in the logarithm of total payments of social pensions after the opening of an agency, estimated using the [Callaway and Sant’Anna \(2021\)](#) method. Roughly 80% of the new pensions granted are from the non-contributory or the partial contributory system, reinforcing that the take-up margin affected by the physical presence of the agencies is for low income households.

First, it is important to highlight the absence of pre-trends. Municipalities treated at different times (and those that were never treated) exhibited similar trends for more than five years before the agency’s arrival. In the post-treatment period, there is a significant

increase in pension payments directed to treated municipalities. The aggregate increase in benefit payments after the agency’s arrival is 4.9%, as shown in Panel A of Table 2. On average, the establishment of a local agency results in 288 new benefits and an additional US\$1.3 million in annual transfers (2016 values), equivalent to 7% of the average private wage bill before treatment.

Given the documented increase in benefit flows, I next examine how this translates into labor market changes. Panel (b) of the same figure presents the event study for the logarithm of total private formal employment. Column 2 of Table 2 shows that the average treatment effect on employment across the post-treatment period is 5.1%, which corresponds to approximately 201 additional formal jobs per treated municipality. These findings are consistent with a growing body of literature documenting positive employment multiplier effects from government transfers in general, and cash transfers in particular (Corbi et al., 2019; Egger et al., 2022; Gerard et al., 2024).

As discussed in Section 3, the median ratio of private-sector jobs to pension beneficiaries in the target municipalities was 2.2 prior to the roll-out of the policy. This relatively high ratio helps rationalize the employment elasticity estimated in Column 2 of Table 2.

One relevant question is to understand how the increase in pension transfers affects labor allocation across sectors. Panel (c) of Figure 3 displays the evolution of employment in the non-tradable sector, while Column 3 of Table 2 confirms that this sector concentrated the employment gains, with a statistically significant increase of 6.1% following the arrival of the agency.⁷ These results are consistent with the notion that the non-tradable sector is more responsive to local income shocks and with the idea that non-homothetic preferences lead to disproportionately higher demand for services as income rises.

Importantly, there are no significant changes in average wages. The estimated coefficient in the main specification is -0.002 and is statistically indistinguishable from zero, as shown in Column 4 of Table 2 and Panel (d) of Figure 4. The lack of wage response, despite a rise in formal employment, suggests that the labor supply curve in the formal sector is relatively elastic at the prevailing wage. One interpretation is that the local labor market exhibits substantial slack: firms are able to hire additional workers without bidding up wages, either because of high unemployment, a binding minimum wage, or the presence of underemployed workers in the informal sector. This pattern is consistent with the view that labor demand—not labor supply—was the main constraint to formal employment growth in this context.

Panel B of Table 2 shows results using the never-treated comparison group in the Callaway and Sant’Anna (2021) estimator. The fact that both Callaway and Sant’Anna (2021) specifications yield very similar results is reassuring with respect to a core identifying assumption: that trends in the outcome variables did not systematically differ across municipalities treated at different times. This suggests that, among target municipalities—both treated and never treated—there were no systematic differences in unobservable efforts, such as local development initiatives or policy priorities, that could have driven divergent labor market trajectories. In the next section, I proceed with a variety of robustness checks that corroborate with the interpretation of the results discussed here.

⁷No significant effects are observed in the tradable sector.

4.2 Robustness Checks

A potential concern for identification is that households may respond to the announcement of an agency, rather than its actual opening, by seeking out existing agencies in nearby municipalities. If the announcement of an agency increases awareness of pension eligibility, pensioners might begin registering in other municipalities before the opening of the agency in their own town. However, because pension payments are assigned to the municipality of residence in administrative records, such early registrations would appear as anticipation effects in the data. To test this, I estimate an event study that sets the treatment window to one year prior to the agency’s opening. As shown in Figure A.1, pension payments do not increase before the agency arrives. Payments begin to rise only after the agency becomes operational, confirming that the treatment is driven by actual openings.

By testing for anticipation, I also address concerns related to the exclusion restriction: that the construction of the agency itself could temporarily boost employment and trigger local multiplier effects unrelated to pension income. If that were the case, the observed increase in formal employment might reflect short-term infrastructure investment rather than the flow of pension transfers. As shown in Figure A.1, Panel (b), formal employment only begins to respond after the agency becomes operational.

Relatedly, one might worry that the surge in demand driving the multiplier effects in private employment is not driven by pensioners’ income but rather by an increase in public employment due to the operation of the agency itself. This seems unlikely, as administrative records indicate that the average establishment employs just 2.7 workers. Consistent with this, Table A.1 in the appendix shows no significant change in public employment following the agency’s opening.

One natural concern is that the arrival of a federal agency would increase the presence and the administrative capacity of the state, thereby lowering information or registration barriers to access other social programs, such as the *Programa Bolsa Família*, the country’s largest conditional cash transfer program. If that were the case, the observed effects on employment or local demand might not be attributable solely to pension income flows, but more broadly to increased state capacity. Figure A.2, Panel (a), addresses this concern by showing that the number of Bolsa Família beneficiaries does not increase following the agency’s arrival.

If anything, the evidence suggests the opposite: expanding access to a distinct social program (social pensions) that can stimulate formal employment leads to a reduction in reliance on targeted poverty alleviation benefits in the medium run. This pattern is not mechanically guaranteed. First, families can accumulate pensions and the cash transfer benefits (social pension income is excluded from Bolsa Família’s means test). On the labor market side, families can strategically remain informal to conceal income sources and retain eligibility for Bolsa Família. Yet the observed decline in beneficiary counts suggests that households are not avoiding formal employment in order to maintain access to transfers. Rather, it indicates that when offered stable income and improved job opportunities, families are willing to forgo targeted benefits. This supports the interpretation that general equilibrium income effects—driven by increased formalization and

labor demand—outweigh any potential disincentives arising from substitution effects.

Panels (b) through (d) of Figure A.2 focus on local administrative quality. Panel (b) presents the IGD (Índice de Gestão Descentralizada), a composite index that determines performance-based funding for municipalities and captures efforts in managing the social registry and monitoring conditionalities for targeted social programs. Panel (c) displays the TAFE index, which captures the quality of school enrollment and attendance monitoring at the municipal level. Panel (d) presents the TAAS index, which measures the quality of health service follow-up, including coverage of child vaccinations and prenatal care visits. These indicators are routinely used to measure the quality of local implementation of social policy. Because municipalities are financially rewarded based on the quality of these indicators, any broad local initiative to expand social assistance funding would likely involve improving them, especially since these efforts are relatively low-cost and lie within municipal control. Table A.2 shows that none of these variables responds significantly to the agency opening, and the magnitude of the point estimates is close to zero. The fact that IGD, TAFE, and TAAS remain flat reinforces the interpretation that the agency expansion process was not part of a broader administrative initiative that could have independently influenced local labor markets.

Finally, the choice of differences-in-differences estimator should be informed by the suitability of the identifying assumptions for each context. As discussed in Section 3, I believe the assumptions of the [Callaway and Sant’Anna \(2021\)](#) estimator are appropriate for my setting. However, the robustness of my estimates across various methods reinforces the validity of the main findings. Table A.2 demonstrates that results are consistent across alternative estimators—including traditional two-way fixed effects (TWFE), [Roth and Sant’Anna \(2023\)](#), and [Sun and Abraham \(2021\)](#). Across specifications, the estimated effects on formal private employment range from 4.1% to 5.5%, with the main specification yielding a mid-range estimate of 5.1%. This is also true for the different outcome variables.

The table also presents results when controlling for baseline characteristics, including 2010 population size, the share of the population receiving pension benefits, and the share employed in the formal private sector (Panel D). If anything, the estimates become slightly larger, suggesting that baseline differences across municipalities do not drive the results in the main specification.

4.3 Cost per Job

Taking advantage of the robustness of my results across different estimation methods, I use the TWFE specification in this section to estimate the implied cost per job via Two-Stage Least Squares (2SLS). A key advantage of using TWFE instead of the Callaway and Sant’Anna (CS) estimator is that it allows for the direct computation of the Wald ratio and its standard errors via *2SLS*. The quantity of interest is the number of additional jobs created per extra *Real* transferred. Formally, I estimate:

$$FormalPrivateEmployment_{it} = \gamma_i + \lambda_t + \theta TotalPayments_{it} + \eta_{it} \quad (3)$$

where I instrument $TotalPayments_{it}$ using an indicator variable equal to one if unit i is treated in period t . This approach captures the average increase in formal private employment in treated municipalities resulting from the average post-treatment increase in pension payments, relative to non-treated units. This procedure is closely related to methodologies used in the literature, such as Chodorow-Reich (2019); Corbi et al. (2019); Gerard et al. (2024).

The cost per job is computed as $\frac{1}{\theta}$. I find that each additional US\$6,100 transferred to a municipality generates one extra job, with standard errors equal US\$ 3,043 obtained using the delta method. The same Wald ratio can be obtained by dividing the coefficients from my preferred specification. This implies a cost per job of US\$5,967, which follows closely the one estimated using 2SLS.

This estimate is consistent with Corbi et al. (2019) US\$ 8,000 found for regional transfers and aligns with the cost between US\$ 6,300 and US\$ 5,900 estimated by Gerard et al. (2024) for *Bolsa Família* in a subsample of municipalities similar to the ones studied here.

5 Mechanisms

This section investigates the channels through which pension income influences local labor market outcomes. Two broad mechanisms may account for the observed employment effects. First, increased income could relax household liquidity constraints, allowing beneficiaries or their family members to search more effectively for formal jobs—particularly in settings where job search entails upfront costs. Therefore, it affects employment levels through an increase in labor supply. Second, higher household income may stimulate aggregate demand, particularly for local non-tradable goods and services, thus driving employment through a Keynesian multiplier effect that boosts employees’ labor demand.

The sectoral pattern of employment and wage responses shown earlier provides preliminary support for the demand channel: formal job gains are disproportionately concentrated in the non-tradable sector, with no meaningful wage effect. However, it is also plausible that the service sector, given its lower capital intensity, can more easily absorb additional labor supply, and, when bound by the minimum wage, do so without changes in wages. In what follows, I present evidence that systematically supports the interpretation that demand-side forces, rather than supply-side adjustments, are the primary drivers of the employment response.

To further distinguish between these channels, I conduct two empirical exercises. First, I use household budget survey data (POF) to estimate the causal impact of pension income on total household income, labor earnings, and consumption. Second, I use administrative financial data to test whether access to pension income increases the uptake of targeted consumption credit.

5.1 Household-Level Evidence

A necessary condition for either increased labor supply or a surge in labor demand to explain the observed effect is that pensions meaningfully affect household income. In economies with high informality and frequent employment turnover, pensions provide not only additional resources but also a stable and predictable stream of income. A large literature documents that informal earnings in Brazil are not only lower but also substantially more volatile than formal wages (e.g., [Gomes et al., 2020](#); [Engbom et al., 2022](#)).

Administrative data from treated municipalities confirm this pattern: pension payments exhibit lower volatility than formal sector earnings. Table 3 reports, for pensioners and for formal employees, the coefficient of variation for both the number of recipients and the average earnings among those with positive income. This illustrates how both the size of the recipient pool and the earnings they receive vary over time, highlighting pensions as a more stable income source. The stability is relevant for both channels. On the supply side, predictable income may reduce the urgency of accepting low-quality informal work, allowing for longer or more selective job searches. On the demand side, stable income facilitates smoother consumption profiles and improves creditworthiness, thereby amplifying the aggregate demand response.

To provide causal evidence on the mechanisms underlying the observed employment effects, I examine whether pension income leads to changes in household income composition and consumption behavior, with particular attention to expenditures on non-tradable goods and purchases in formal retail outlets. The analysis is based on the 2008–2009 wave of a nationally representative household budget survey (POF).

Eligibility for Brazil’s main pension programs changes discretely according to age, gender, and employment history, generating sharp variation in the likelihood of receiving a pension. Female rural workers become eligible for a non-contributory pension at age 55, while male rural workers qualify at age 60. Individuals with at least 15 years of formal contributions but insufficient tenure for full retirement eligibility can begin receiving a partially contributory pension at age 60 (women) or 65 (men), with benefits determined by their contribution history. Finally, informal workers with fewer than 15 years of contributions become eligible at age 65 for the *Benefício de Prestação Continuada* (BPC), a means-tested transfer equal to the minimum wage.⁸ Together, these three schemes account for more than 80% of retirement benefits in the municipalities in my analysis sample.

Empirical strategy—household-level analysis. Assignment to pension receipt is not independent of household consumption and labor market decisions. Unobserved factors such as individual effort, job quality, prior labor market history, and household needs may jointly influence the likelihood of retirement and the outcomes of interest. As a result, simple comparisons between treated and untreated households—those receiving and not receiving pensions—are likely to be biased. To address this concern, I exploit

⁸Although BPC eligibility is officially restricted to households with per capita income below one-quarter of the minimum wage, the test excludes several income sources and is weakly enforced in practice, rendering the effective coverage broader.

variation in pension receipt induced by age-based eligibility thresholds, using age as an instrumental variable (IV) to identify the causal effect of pension income on household income composition and consumption behavior.⁹ Since individuals receive pensions but the unit of observation is the household, I define the treatment assignment using the age of the oldest household member as the relevant eligibility criterion.

The validity of the IV strategy relies on two key assumptions. First, the exogeneity condition requires that the running variable (age) is not subject to manipulation around the eligibility cutoff. Second, the exclusion restriction requires that, conditional on smooth functions of age and other covariates, crossing the pension eligibility threshold affects household outcomes only through pension receipt. In other words, households whose oldest member is just above the threshold should not differ in labor market behavior or consumption solely because of age, except through access to pension income. This assumption is plausible given that no other institutional changes or social entitlements are triggered at the same ages, and household economic decisions are expected to evolve smoothly with household average age in the absence of pension eligibility. Furthermore, as shown in Table 4, households with the oldest members just below and just above each threshold are similar across a range of predetermined characteristics, lending additional support to the identification strategy.

In the *2SLS* framework employed here, I exploit the discontinuous increase in pension eligibility at specific age thresholds to instrument actual pension receipt. Let $Pension_i = 1$ if individual i receives any pension income. The first stage estimates the relationship between pension receipt and a binary indicator for crossing the relevant age cutoff, controlling flexibly for age trends and predetermined household characteristics. This isolates exogenous variation in treatment status attributable to institutional eligibility rules.

The first-stage equation is specified as:

$$Pension_i = \gamma_0 + \gamma_1 \mathbb{I}(Age_i \geq a^*) + \Psi(Age_i - a^*) + State_i + \mathbf{X}_i' \Gamma + \eta_i,$$

the indicator $\mathbb{I}(Age_i \geq a^*)$ captures the shift in eligibility induced by institutional rules, and $\Psi(\cdot)$ is a linear function of age, allowing for different slopes on either side of the cutoff, where a^* is the relevant eligibility threshold (55, 60, or 65) for each subgroup. $State_i$ denotes state fixed effects (reflecting the POF's sampling design representative at the state level), and \mathbf{X}_i is a vector of predetermined household covariates.¹⁰

The second-stage equation is:

$$Y_i = \delta_0 + \delta \widehat{Pension}_i + \Psi(Age_i - a^*) + \mathbf{X}_i' \Delta + State_i + \epsilon_i,$$

where Y_i is the outcome of interest—such as total household income, labor income, or consumption—and δ captures the local average treatment effect of pension receipt.

⁹This empirical setting corresponds to a fuzzy regression discontinuity design, which can equivalently be estimated via a local IV approach around the eligibility cutoffs (Abadie and Cattaneo, 2018). Given the discrete nature of the running variable, the IV terminology provides a more precise description of the identification strategy.

¹⁰ \mathbf{X}_i includes years of education, an indicator for illiteracy, gender, race, and a predicted income measure constructed by regressing household income net of pensions on predetermined household characteristics. These controls account for baseline income heterogeneity unrelated to treatment

Because each age threshold applies only to a specific subgroup (e.g., rural women or urban men), separate regressions by eligibility group would suffer from limited power. To address this, I pool observations across gender, location, and cutoff definitions into a unified regression framework, interacting controls as needed to preserve the integrity of the identification strategy. This approach improves precision while allowing for heterogeneous first-stage effects across subpopulations ¹¹

Household-Level Results. Table 5 confirms that crossing an age-based pension eligibility threshold significantly increases the probability of receiving pension income. Table 6 reports the corresponding *2SLS* estimates for household income and expenditure outcomes. Receiving a pension increases pension income by approximately R\$10,650 per year, relative to a control group mean of R\$1,881 - more than a fivefold increase. This is because few households qualify for fully contributory retirement; most beneficiaries begin receiving pensions only upon reaching the statutory age cutoff under the non-contributory or partially contributory schemes. This translates into a R\$7,609 increase in total household income, roughly a 55% rise over the control mean of R\$13,733.

Consistent with the existing literature on pension income and household labor supply (e.g., [de Carvalho Filho, 2008](#)), I find no evidence of increased labor earnings. In fact, the estimate for the marginal effect in household labor income is negative (R\$-4,191). This suggests that pensions do not crowd in labor supply or increase earnings among other household members. Instead, they reduce reliance on labor income. This suggests that the aggregate effect I observed at the municipal level is unlikely to be driven by the recipient household’s increase in labor supply.

Still, total income rises because pension transfers are large enough to offset the negative labor market response, leading to higher consumption. Total household expenditure increases by R\$6,678—about 52% of the control group’s baseline consumption. This implies a marginal propensity to consume (MPC) of roughly 0.88, indicating that almost all of the additional pension income is spent rather than saved. This pattern reinforces the view that non-contributory and partially contributory pensions mainly benefit liquidity-constrained households with limited access to savings or credit.

The consumption response is concentrated in formal consumption¹²: the share of total spending in formal retail outlets increases by 12%. This provides causal evidence for what [Bachas et al. \(2023\)](#) describes as the Informality Engel Curve, which posits that as households become richer, a larger share of their consumption shifts toward formal

¹¹The second-stage equation becomes:

$$Y_i = \delta_0 + \delta \widehat{Pension}_i + \sum_{g=1}^G [\Psi_g(Age_i - a_g^*) \cdot \mathbb{I}(i \in g)] + \mathbf{X}_i' \Delta_g + State_i + \epsilon_i,$$

where G includes rural women ($53 \leq Age_i \leq 59$), urban women ($56 \leq Age_i \leq 62$), rural men ($56 \leq Age_i \leq 62$), and a pooled group of all individuals aged 63 to 69. Each group is defined over a specific age window around their respective pension eligibility thresholds ($a_g^* \in \{55, 60, 65\}$). The instrumented variable $\widehat{Pension}_i$ is constructed using discontinuities in eligibility at ages 55, 60, and 65, each interacted with its corresponding group and age band. Age trends ($Age_i - a_g^*$) and the control vector are also interacted with group dummies to flexibly account for heterogeneity.

¹²Formal consumption is defined following [Bachas et al. \(2023\)](#), using information on the place of acquisition reported in the POF to distinguish between formal and informal retail outlets.

establishments.

By leveraging non-homotheticities in demand, social pensions that provide stable income to previously liquidity-constrained households can influence the transition from informal to formal economies. This mechanism sheds light on the structural path through which economies move toward higher levels of formalization and tax collection.

Table 7 further disaggregates the consumption response by category. The largest increases are observed in health services (R\$698, or a 227% increase over the control mean of R\$307), bank services (R\$972, or a 141% increase), and home utilities (internet, water, sewage, electricity, television, telephone), which see an increase of R\$396, or 30%. These shifts suggest that pension income enables households to catch up on essential services and improve access to formal financial institutions, as suggested by the increase in bank expenditures—most likely driven by credit repayment.

Overall, the results provide robust evidence that access to pensions meaningfully increases household resources, reshapes the composition of income, and induces large consumption responses—particularly in formal and non-tradable sectors. These findings reinforce the interpretation that the observed municipality-level employment effects are driven by household-level demand expansion following pension receipt. The strong response in bank-related spending is particularly notable, as it aligns with the fact that pensions provide households not only with stable income but also with a source of collateral enabling greater engagement with the formal financial system. I explore this amplification mechanism in more detail in the following section.

5.2 Credit Market Evidence

In highly informal economies, credit markets are often rationed because lenders struggle to verify volatile or undocumented earnings, leaving households unable to smooth consumption or finance durable goods. When workers shift from irregular income to a stable, predictable pension, this reliable cash flow can serve as credible collateral, relaxing credit constraints and expanding access to formal loans. Many governments leverage this feature by offering pension-backed credit schemes—effectively transforming pensions into an instrument that both enhances household liquidity and amplifies the demand-side stimulus of the transfer.

Brazil offers a particularly salient case. Retirees automatically qualify for *crédito consignado*, a payroll-deductible loan that is repaid via direct debits from pension benefits. Unlike traditional consumer loans, it features lower interest rates and relaxed collateral requirements, as repayment is guaranteed through direct debits from a beneficiary’s monthly pension. Eligibility is typically automatic for public pensioners, with regulated interest rate caps and fixed maximum deduction rates (generally limited to 30% of the monthly benefit). In the event of a borrower’s death, remaining loan balances are absorbed by credit insurance rather than passed on to surviving family members.¹³

¹³Under Brazilian regulation, *crédito consignado* loans are required to include life insurance protection. If a pensioner dies before the loan is repaid, the outstanding balance is generally covered by the insurer and not collected from the estate.

Comparable mechanisms operate elsewhere. The Philippines’ Government Service Insurance System (GSIS) and Social Security System (SSS) offer pension-backed loans; Colombia’s *Libranza* system provides payroll-deducted credit to pensioners and public employees; and Argentina’s ANSES offers subsidized loans whose installments are withheld from benefits. Related evidence from the United States shows that collateralizable income gains, such as those triggered by minimum-wage increases, lead low-income workers to expand secured borrowing and raise consumption (Aaronson et al., 2012). Taken together, these cases highlight a common mechanism: when income shocks arrive in a form that can be collateralized, they unlock formal credit for liquidity-constrained households and magnify the consumption effects of social programs.

To assess whether pension income affects credit access in a manner consistent with the proposed mechanism, I examine whether the arrival of a pension agency leads to changes in the stock of consumption credit at the municipal level. I replicate the main difference-in-differences strategy used for labor market outcomes, estimating both event-study dynamics and average treatment effects. As shown in the event study in Figure 5, there is no evidence of differential trends in the pre-treatment period, and a clear increase in consumption credit begins immediately following the arrival of the pension agency. Table 8 reports the corresponding average treatment effects: the stock of consumption loans increases by approximately 8% relative to pre-treatment levels. In contrast, there is no statistically significant effect on other types of credit, including housing credit and business loans.

These results support the interpretation that pension income acts not only as a direct transfer but also as a facilitator of broader consumption through improved access to liquidity. Retirees who were previously excluded from formal credit markets due to income volatility become eligible for low-cost credit once they begin receiving stable pension payments. This credit channel reinforces the demand-side responses observed in formal employment and service-sector activity.

Importantly, the credit findings also lend support to the validity of the identification strategy employed throughout the paper. If the opening of a pension agency coincided with broader municipal-level economic policies, we would expect to observe increases in multiple credit categories, particularly business and housing loans. Instead, the credit response is narrowly concentrated in consumption loans—the only category directly tied to pension eligibility. This targeted pattern is consistent with the interpretation that observed labor market and consumption effects are driven by an expansion in the number of pension recipients, rather than by broader policy interventions that could be simultaneously affecting labor and credit markets.

6 Conclusion

Designing effective social protection systems in highly informal economies remains a major challenge for policymakers and a long-standing topic of debate among economists. Increasing formalization is critical to managing demographic transitions, as the formal sector typically bears the financial burden of contributory pension systems. Recent ev-

idence from [Egger et al. \(2022\)](#) and [Gerard et al. \(2024\)](#) suggests that targeted cash transfers can stimulate local economic activity by increasing household demand. This paper provides the first empirical evidence on the local multiplier effects of the most widespread form of transfer globally: pensions.

In developing countries, pensions not only raise average household incomes but also stabilize earnings over time. Exploiting an expansion in pension coverage, I show that private formal employment increases, particularly in the non-tradable sector. The absence of a significant effect on the average wage suggests an elastic labor supply and slackness in the use of labor. This aligns with a demand-constrained development path where workers are willing to work at current wage levels, but firms lack the capacity to absorb them into the formal sector due to low aggregate demand. By combining household survey data with administrative bank-level credit records, I identify factors driving the formal employment effect: an increase in disposable income, high marginal propensities to consume, non-homothetic demand shifts toward formal goods, and improved access to credit via more stable income streams. These mechanisms highlight both behavioral responses and credit market frictions as key to understanding the local employment impact of social transfers.

A natural concern is the extent to which these results generalize beyond the municipalities in my sample. With over 5,000 municipalities, Brazil features substantial regional heterogeneity: its wealthiest regions approach the income levels of France, while its poorest resemble those of Chad. The municipalities in this study are similar to the median Brazilian municipality in terms of size and income. Given the wide support of Brazil’s income per capita distribution, they are plausibly representative of local labor markets in many developing countries. More importantly, the paper clarifies the conditions under which pensions are most likely to generate employment effects—namely, in informal labor markets where pensions constitute a substantial and stable income gain for liquidity-constrained informal workers.

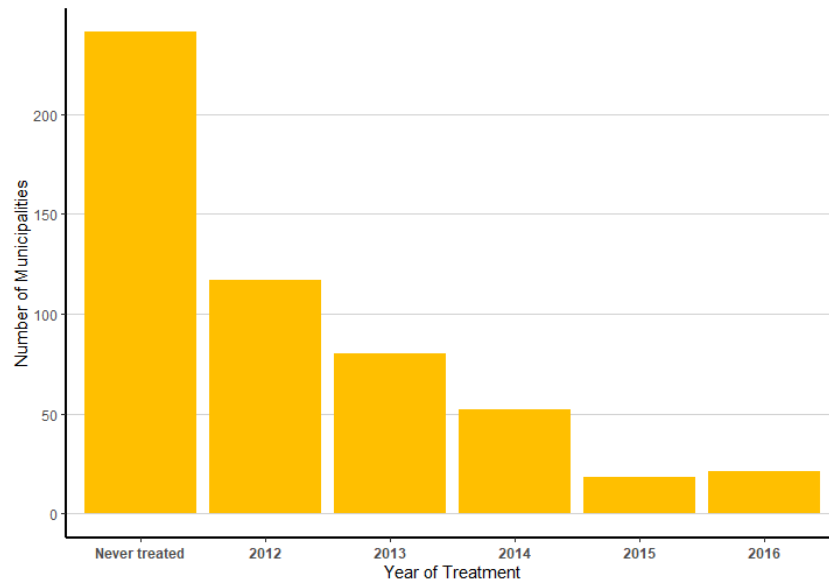
The substantial heterogeneity in formalization rates across local labor markets within an economy not only raises questions about the external validity of my results but also offers insights into how such transfers might be financed. While I do not directly address the taxation side of this question, the findings suggest that social pensions can be designed to function as an equalizing mechanism for formalization rates across regions. If taxes are primarily collected in areas with higher formalization rates and relatively inelastic labor supply—where taxation is less likely to discourage formal employment—and then redistributed to regions with lower formalization and more elastic labor supply, the resulting increase in local demand in the recipient areas may offset potential disincentives in the more heavily taxed regions. These results offer a new perspective on the broader role of non-contributory pension programs, highlighting their potential to reduce inter-regional disparities in formalization while raising important questions about the optimal design of financing mechanisms.

This paper offers new insights for the design of social insurance in economies characterized by high informality. A key area for future research is to quantify the trade-off between the aggregate efficiency costs of raising taxes on formal workers and the demand stimulus generated by transferring income to liquidity-constrained households. Another promising avenue is to further explore the extent to which cash transfers can alleviate credit market

frictions in developing economies, and how improved credit access interacts with the consumption of formal goods and services. It also seems promising to examine how programs that shift income across demographic groups influence consumption patterns and interact with structural transformation. Finally, the effect may vary by gender, as women are often responsible for caring for older household members. Understanding how these transfers to older individuals affect women's labor supply is an interesting avenue for future research.

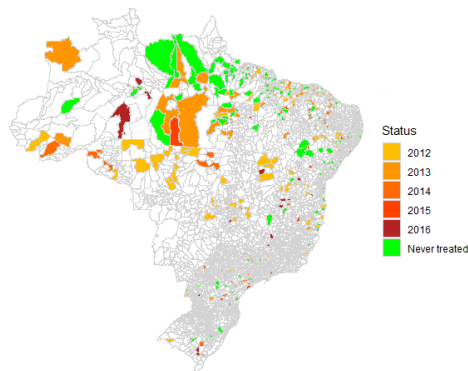
Main Figures and Tables

Figure 1: Agency arrival date



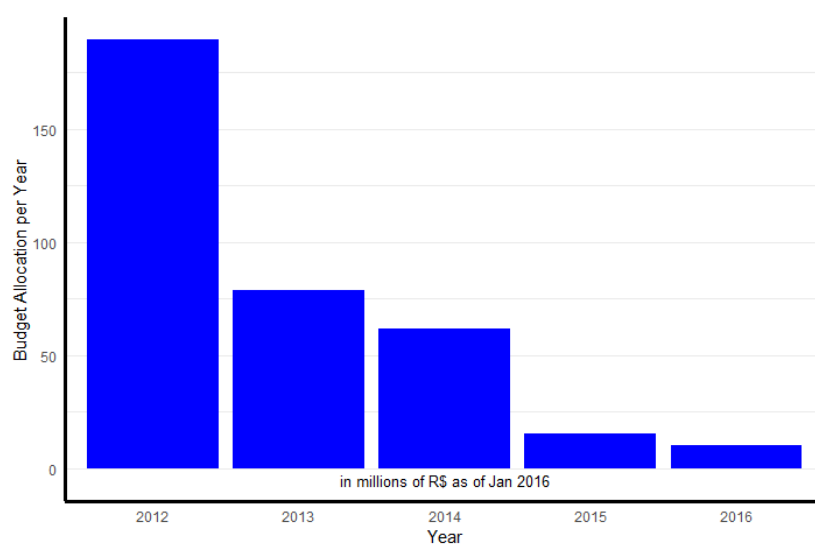
Note: The plot shows the size of each cohort, defined by the year when each municipality received an agency.

Figure 2: Geographical distribution of targeted municipalities



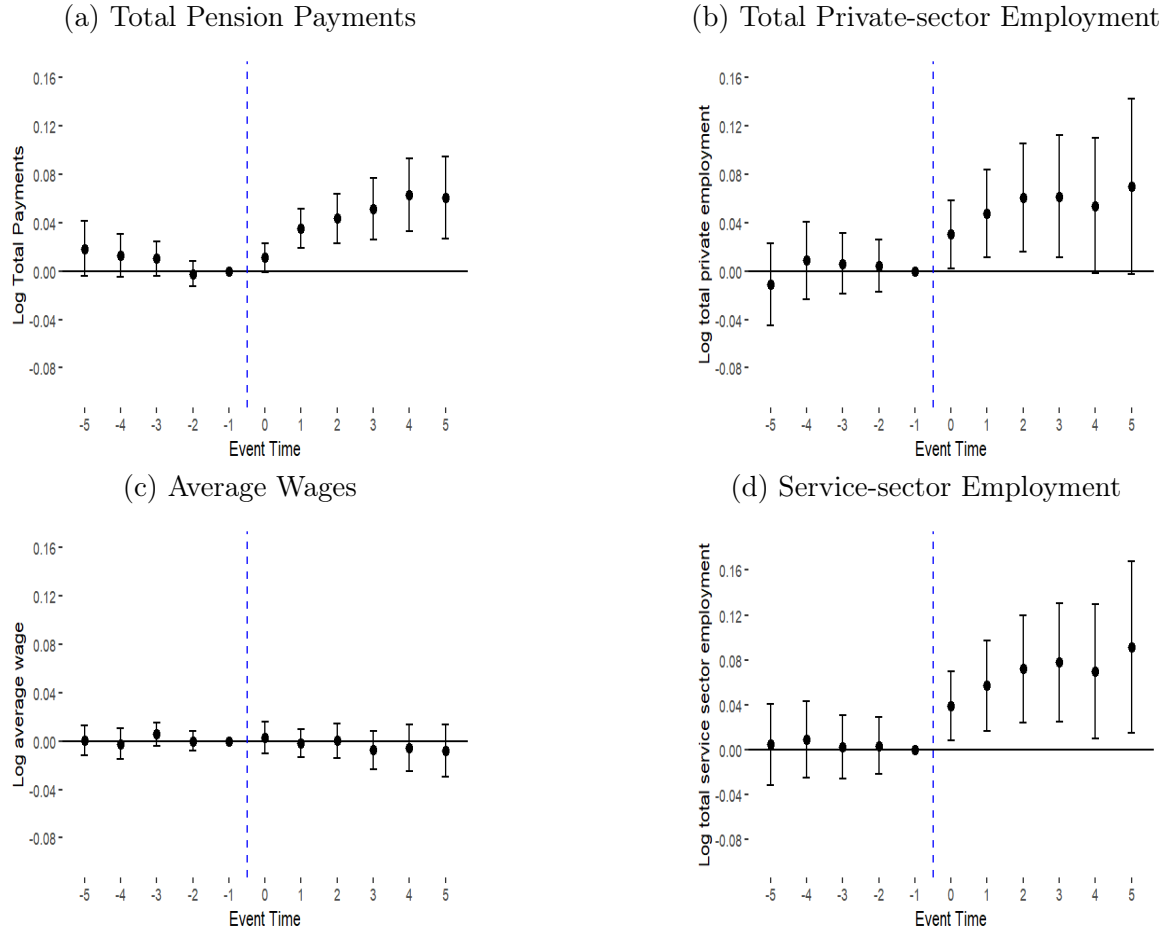
Note: The map displays the municipalities that were target by the policy according to their year of treatment.

Figure 3: Central Government Budget for Agency Construction



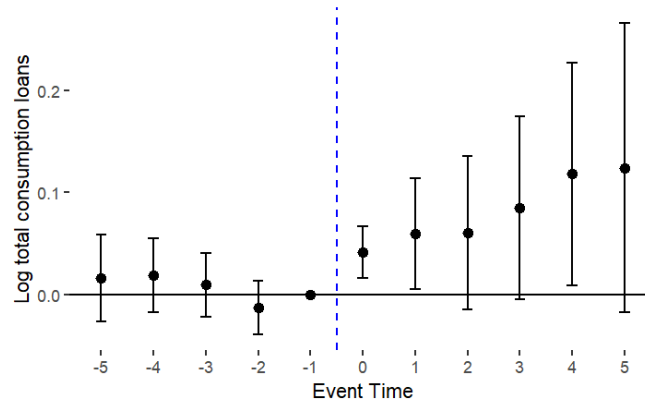
Note: The plot shows the budget dedicated to agency construction in each calendar year.

Figure 4: Impact of Agency Arrival - Main Results



Note: The plots depict the impact of the opening of an INSS agency on various economic indicators in a municipality. The first plot shows the impact on the log of total amount of pension payments (a). The second plot illustrates the effect on the log of total private-sector formal employment (b). The third plot shows the impact on the log of average wages (c). The fourth plot depicts the effect on the log of total service-sector employment (d). The fifth plot shows the impact on the log of number of firms with more than 50 employees (e). The sixth plot illustrates the effect on the employment share at the top 10% biggest firms (f), with the y-axis representing the the share. In all plots, the x-axis indicates the event time in years, with the agency's opening normalized at time zero. The points represent the estimated effects, and the vertical lines indicate the 95% confidence intervals for these estimates. Standard errors are clustered at the municipal level.

Figure 5: Impact of Agency Arrival on Consumption Credit



Note: This figure plots the event-study estimates of the impact of the opening of an INSS agency on the log of total consumption credit in a municipality. The x-axis indicates event time in years, with zero marking the year of agency arrival. Points represent estimated coefficients and vertical lines indicate 95% confidence intervals. Standard errors are clustered at the municipality level.

Table 1: Descriptive Statistics

| Variable | All Sample | Eventually Treated | Never Treated | p-value (F test) |
|---------------------------------|------------|--------------------|---------------|------------------|
| Population | 36923 | 34777 | 40185 | 0.03 |
| Share Beneficiaries | 0.15 | 0.16 | 0.13 | 0.05 |
| Share Private Formal Employment | 0.12 | 0.10 | 0.13 | 0.16 |
| Average Wage | 1098 | 1065 | 1137 | 0.06 |
| Number of Firms | 799 | 799 | 798 | 0.72 |
| Number of Observations | 529 | 288 | 241 | |

Note: This table shows descriptive statistics for baseline characteristics across different samples. The first column presents the average value of each variable for the whole sample, the second column shows the average for those treated between 2012 and 2016, and the third column provides the average for those never treated. The final column reports the p-value of a joint F-test of the coefficients from a linear regression of each variable against a set of cohort dummies, testing whether the timing of treatment explains any differences in baseline information across cohorts. Population data is sourced from the 2010 census, while other variables are baseline values calculated for 2011.

Table 2: Main Results

| | <i>Log</i> Total Pension Payments | <i>Log</i> Total Private Employment | <i>Log</i> Service Sector Employment | <i>Log</i> Average Wage |
|--------------------|--------------------------------------|--|---|----------------------------|
| <i>Panel A:</i> | | | | |
| CS Not-Yet Treated | 0.0488*** (0.0112) | 0.0508*** (0.0202) | 0.0609*** (0.0215) | -0.0023 (0.0063) |
| <i>Panel B</i> | | | | |
| CS Never Treated | 0.0497*** (0.0124) | 0.0553*** (0.0220) | 0.0669*** (0.0238) | -0.0033 (0.0062) |
| Observations | 6012 | 6012 | 6012 | 6012 |

Note: The table reports the Average Treatment Effects on the Treated (ATT) of the agency arrival on various labor market outcomes, estimated using the [Callaway and Sant'Anna \(2021\)](#) approach under different comparison group assumptions. Each column corresponds to a different outcome variable in logarithmic form. The sample excludes the top and bottom 5% of municipalities in terms of total population based on the 2010 Census. Panel A presents results using not-yet-treated municipalities as the control group, while Panel B uses only never-treated municipalities. Standard errors are clustered at the municipality level. Significance levels are denoted as: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Mechanisms

Table 3: Average Coefficient of Variation across Target Municipalities

| | Formal Private Sector | Pensions |
|-------------------|-----------------------|----------|
| Number of Earners | 0.253 | 0.173 |
| Average Earnings | 0.337 | 0.222 |

Notes: The table displays, for the target municipalities, the average coefficient of variation (CV) of the total number of benefits and private formal contracts and the average coefficient of variation of the average earnings across years using RAIS and pensions payment sheets. To compute the average earnings we deflate all the values to 2016 prices.

Table 4: Descriptive Statistics for Different Age Groups Using POF Data

| | Age Groups | | | |
|----------------------|------------|-------|-------|-------|
| | 53–54 | 56–59 | 61–64 | 66–69 |
| Years of Schooling | 4.821 | 4.267 | 3.160 | 2.579 |
| Illiteracy | 0.213 | 0.270 | 0.353 | 0.447 |
| Non-white | 0.615 | 0.615 | 0.623 | 0.632 |
| Household Size | 3.641 | 3.485 | 3.370 | 3.228 |
| Number of Rooms | 2.107 | 2.078 | 2.039 | 1.994 |
| Number of Bathrooms | 1.164 | 1.150 | 1.116 | 1.133 |
| Connection to Water | 0.767 | 0.767 | 0.752 | 0.760 |
| Connection to Sewage | 0.365 | 0.336 | 0.322 | 0.309 |
| Urban Household | 0.745 | 0.729 | 0.716 | 0.724 |

Notes: The table displays descriptive statistics for different age groups using POF data. Columns show mean values for the indicated age ranges. Variables refer to the characteristics of the oldest household member and their household conditions.

Table 5: First Stage – Pension Eligibility and Receipt

| | P(Pension = 1) |
|--------------------------|-----------------------|
| Rural women ≥ 55 | 0.200*** (0.066) |
| Rural men ≥ 60 | 0.343*** (0.045) |
| Urban women ≥ 60 | 0.058 (0.048) |
| Over 65 | 0.123*** (0.020) |
| Mean value control group | 0.432 |
| Effective Observations | 4,060 |
| Weak Instrument F-Test | 50.1 |

Notes: The table reports 2SLS first-stage results. The dependent variable is an indicator for whether the household receives pension income. Each row corresponds to the estimated increase in pension receipt at a relevant eligibility cutoff defined by age, gender, and rural/urban status. Standard errors are reported in parentheses. The number of observations for each regression is 4,060. The Weak Instrument F-test is reported in the last row. Significance levels are denoted as: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 6: Results - Household level

| | Pension Income | Total Income | Labor Income | Expenditure | Share Formal Stores |
|----------------------------|--------------------|-------------------|------------------|-------------------|---------------------|
| $P(\widehat{Pension} = 1)$ | 10650*** (1012) | 7609*** (1482) | -4191* (1494) | 6678*** (2260) | 0.1205** (0.042) |
| Mean values control group | 1881 | 13733 | 10867 | 12625 | 0.7167 |
| Effective Observations | 4,060 | 4,060 | 4,060 | 4,060 | 4,060 |

Notes: The table reports 2SLS estimation results. The dependent variable for the first column is Pension Income, the second column is Total Income, and the third column is Labor Income. The fifth column shows the total Expenditures and the sixth column reports the change in the share of Expenditures in Formal Stores. Standard errors are reported in parentheses. The number of observations for each regression is 10,924. The Weak Instrument F test is reported in the first column. Significance levels are denoted as: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 7: Results - Expenditure Categories

| | Health Services | Services | Prepared Food | Home Utilities | Bank services |
|----------------------------|-----------------|---------------|----------------|----------------|-----------------|
| $P(\widehat{Pension} = 1)$ | 698*** (229) | 203** (80) | -268* (166) | 396** (181) | 972*** (299) |
| Mean values control group | 307 | 243 | 658 | 1303 | 687 |
| Effective Observations | 4,060 | 4,060 | 4,060 | 4,060 | 4,060 |

Notes: The table reports 2SLS estimation results. The dependent variable in each column corresponds to the expenditure in the respective category: Health Services, other Services, Prepared Food, Home Utilities, and Bank Services. The reported coefficient represents the estimated effect of receiving a pension ($P(\widehat{Pension} = 1)$) on expenditures. Standard errors are reported in parentheses. The number of observations for each regression is 5,244. Significance levels are denoted as: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 8: Bank Loans

| | <i>Log Consumption Credit</i> | <i>Log Other Credit</i> |
|--------------------|-------------------------------|-------------------------|
| CS Not-Yet Treated | 0.0805** (0.0367) | 0.0383 (0.0862) |
| Observations | 6012 | 6012 |

Note: The table reports the Average Treatment Effects on the Treated (ATT) of the agency arrival on different credit outcomes, estimated using the [Callaway and Sant'Anna \(2021\)](#) approach with not-yet-treated municipalities as the control group. The outcomes are measured in logarithmic form. The sample excludes municipalities in the top and bottom 5% of the 2010 population distribution. Standard errors are clustered at the municipality level. Significance levels are denoted as: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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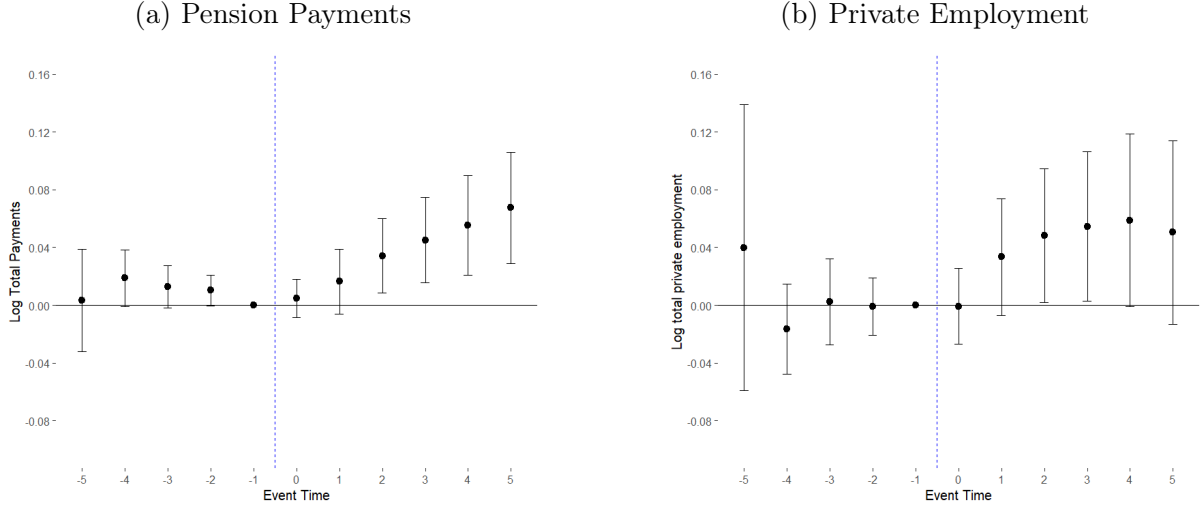
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Appendix

A: Robustness Checks

Figure A.1: No Anticipation Effects: Pension Payments and Employment



Note: The plots depict dynamic treatment effects from an event study specification in which the event time is normalized to one period before the actual opening of the INSS agency (to test for anticipation). Panel (a) shows the impact on the log of total pension payments; Panel (b) shows the impact on the log of total private formal employment. In both plots, the x-axis represents event time in years, and the points display the estimated coefficients. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the municipal level.

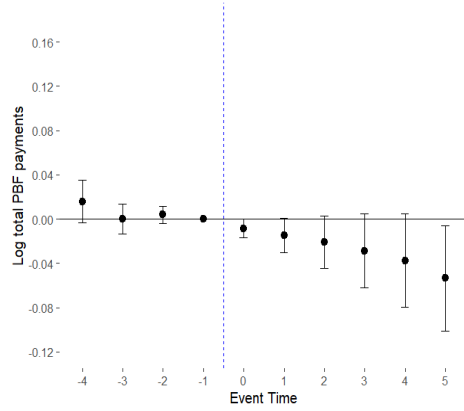
Table A.1: Public Employment

| | (2) | (3) | (4) |
|-------------------|----------|----------|------------|
| Public Employment | 0.0283 | 0.0294 | 0.0218 |
| | (0.0499) | (0.0523) | (0.0535) |

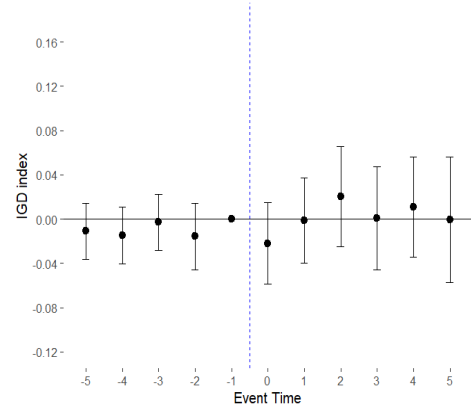
Note: The table depicts the Average Treatment Effects on the treated of the agency arrival using different specifications of the [Callaway and Sant'Anna \(2021\)](#) estimator. Each column shows the results using an inverse hyperbolic sine transformation due to some municipalities reporting 0 public employment in some years. The sample excludes the 5% outliers in terms of total population as measured by the 2010 Census. Column 1 represents [Callaway and Sant'Anna \(2021\)](#) not yet treated, Column 2 represents [Callaway and Sant'Anna \(2021\)](#) using only never treated units as comparison group, Column 3 runs the same model as (1) controlling for baseline mean of total population, the share of the population receiving pension benefits, the share of the population employed as private employees, average wages, the total number of establishments, and the number of establishments with more than 50 employees. Standard errors are clustered at the municipal level. Significance levels are denoted as: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure A.2: Impact of Agency Arrival on Social Program Indicators and Municipal Quality

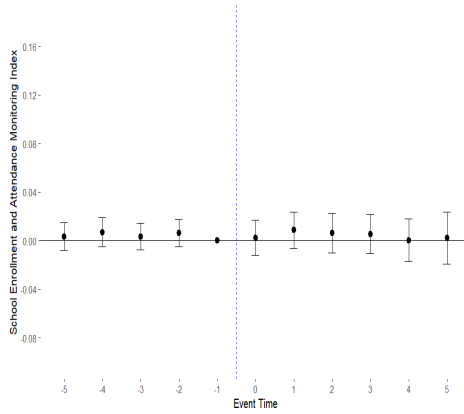
(a) Total Bolsa Família Beneficiaries



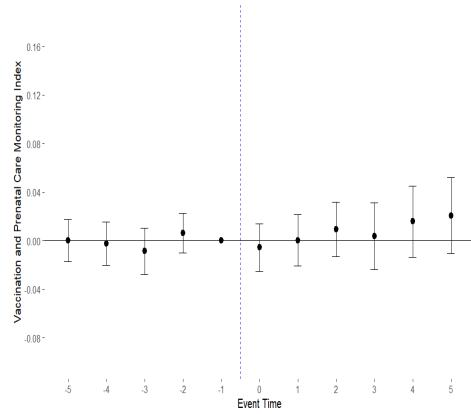
(b) IGD Composite Index



(c) TAFE Index



(d) TAAS Index



Note: This figure presents the estimated dynamic effects of INSS agency openings on key indicators related to social policy implementation. Panel (a) shows the effect on the log of total Bolsa Família payments. Panel (b) presents the impact on the *Índice de Gestão Descentralizada* (IGD), a composite index measuring the quality of municipal administration, including the quality of health and education outcomes monitoring, as well as how well municipalities manage the Bolsa Família Program. Panel (c) displays effects on the School Enrollment and Attendance Monitoring Index. Panel (d) shows the Vaccination and Prenatal Care Monitoring Index. All panels plot event-time coefficients, with time zero normalized to the year of agency arrival. The points represent estimated coefficients, and the vertical lines denote 95% confidence intervals. Standard errors are clustered at the municipal level.

Table A.2: Impact of Agency Arrival on Social Program Indicators and Municipal Quality

| | <i>Log Bolsa Família</i> | <i>IGD Composite</i> | <i>TAFE Index</i> | <i>TAAS Index</i> |
|--------------------|--------------------------|----------------------|--------------------|--------------------|
| <i>Panel A:</i> | | | | |
| CS Not-Yet Treated | -0.0303** (0.0137) | 0.0060 (0.0177) | 0.0063 (0.0070) | 0.0065 (0.0102) |
| <i>Panel B:</i> | | | | |
| CS Never Treated | -0.0311** (0.0151) | 0.0050 (0.0178) | 0.0062 (0.0072) | 0.0061 (0.0104) |
| Observations | 6012 | 6012 | 6012 | 6012 |

Note: This table presents the estimated average treatment effects of INSS agency openings on key indicators of social policy implementation. Column (1) shows the effect on the log of total Bolsa Família beneficiaries. Column (2) presents the impact on the *Índice de Gestão Descentralizada* (IGD), a composite index that measures the quality of municipal administration, including health and education monitoring, as well as the management of the Bolsa Família program. Columns (3) and (4) show, respectively, the effects on the School Enrollment and Attendance Monitoring Index (TAFE) and the Vaccination and Prenatal Care Monitoring Index (TAAS). Panel A uses not-yet-treated municipalities as the comparison group, while Panel B uses only never-treated municipalities. Standard errors are clustered at the municipality level. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.3: Robustness Results — Alternative Estimators and Specifications

| | <i>Log Total Pension Payments</i> | <i>Log Total Private Employment</i> | <i>Log Service Sector Employment</i> | <i>Log Average Wage</i> |
|-----------------------|---------------------------------------|---|--|-----------------------------|
| <i>Panel A:</i> | | | | |
| TWFE | 0.0381*** (0.0116) | 0.0411** (0.0186) | 0.0507*** (0.0194) | -0.0006 (0.0062) |
| <i>Panel B:</i> | | | | |
| Roth & Sant’Anna (RS) | 0.0649*** (0.0091) | 0.0532*** (0.0194) | 0.0670*** (0.0205) | -0.0091 (0.0055) |
| <i>Panel C:</i> | | | | |
| Sun & Abraham (SA) | 0.0488*** (0.0116) | 0.0552*** (0.0209) | 0.0642*** (0.0223) | 0.0032 (0.0063) |
| <i>Panel D:</i> | | | | |
| Baseline Controls | 0.0481*** (0.0117) | 0.0516*** (0.0212) | 0.0626*** (0.0257) | 0.0047 (0.0065) |
| Observations | 6012 | 6012 | 6012 | 6012 |

Note: The table presents robustness checks for the estimated Average Treatment Effects on the Treated (ATT) of the agency arrival on various labor market outcomes. Each column reports results for a different outcome variable in logarithmic form. Panel A uses a traditional two-way fixed effects (TWFE) estimator. Panel B presents estimates using the [Roth and Sant’Anna \(2023\)](#), which is the most efficient estimator when assuming the timing is random. Panel C uses the estimator proposed by [Sun and Abraham \(2021\)](#). Panel D augments the main specification with baseline covariates: the 2010 municipal population, the share of residents receiving pension benefits, and the share of private-sector employment. The sample excludes municipalities in the top and bottom 5% of the 2010 population distribution. Standard errors are clustered at the municipality level. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.