State Capacity and Social Insurance Delivery: Evidence from Brazil

Gabriel T. Lemos*

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Abstract

I study how expanding the delivery capacity of social insurance affects benefit take-up, targeting, and welfare. Beginning in 2009, the Brazilian social security agency opened nearly 500 new offices in mid-sized municipalities, expanding its network by 50.6 percent. I exploit a population-based eligibility threshold and staggered office openings in a difference-in-differences design using administrative data on more than 68 million benefit claims and municipality-level measures of economic activity. Opening a local office increases new benefit recipiency by 5.2 percent and social security transfers by 8.8 percent, consistent with lower take-up frictions. Marginal applicants are disproportionately self-employed, unemployed, and affected by severe disabilities, suggesting that reduced administrative barriers expanded access among the relatively disadvantaged. The resulting increase in local disposable income also generates sizable local general equilibrium effects, including higher tax revenue, formal employment in non-tradables, and consumer lending, with the strongest effects in less-developed municipalities. Accounting for these fiscal externalities more than doubles the estimated marginal value of public funds, to 2.76. These findings show that weak delivery capacity is an important barrier to social insurance access in developing countries and that expanding it can improve both distributional and local economic outcomes.

1. Introduction

Increasing social protection is a central challenge in developing countries. Social insurance, and social security in particular, is an important component of the social safety net because it provides income support while being less burdensome for the government given its contributory nature. Yet many workers in developing countries are not covered by public insurance, which requires enrollment through continued contributions to qualify for benefits. For informal and self-employed workers in particular, enrollment is often *de facto* voluntary since contributions cannot be easily enforced in the absence of reliable earnings records (Banerjee et al., 2024).

*MIT Sloan. E-mail: gtlemos@mit.edu

A further challenge is that frictions in claiming social insurance benefits are widespread and might significantly reduce the value of enrollment in the first place. In practice, even eligible workers may anticipate substantial barriers to accessing the insurance for which they contribute. Incomplete take-up of social benefits can result from lack of knowledge about eligibility as well as administrative burdens in the application process, such as extensive paperwork (Currie, 2004).[1] These frictions are especially severe where the state has weak capacity to manage and deliver social insurance. In fact, previous work has suggested that weak state capacity is a major binding constraint on social insurance delivery in developing countries (Banerjee et al., 2021), yet there is still limited evidence on how expanding that capacity affects take-up, enrollment, and welfare.

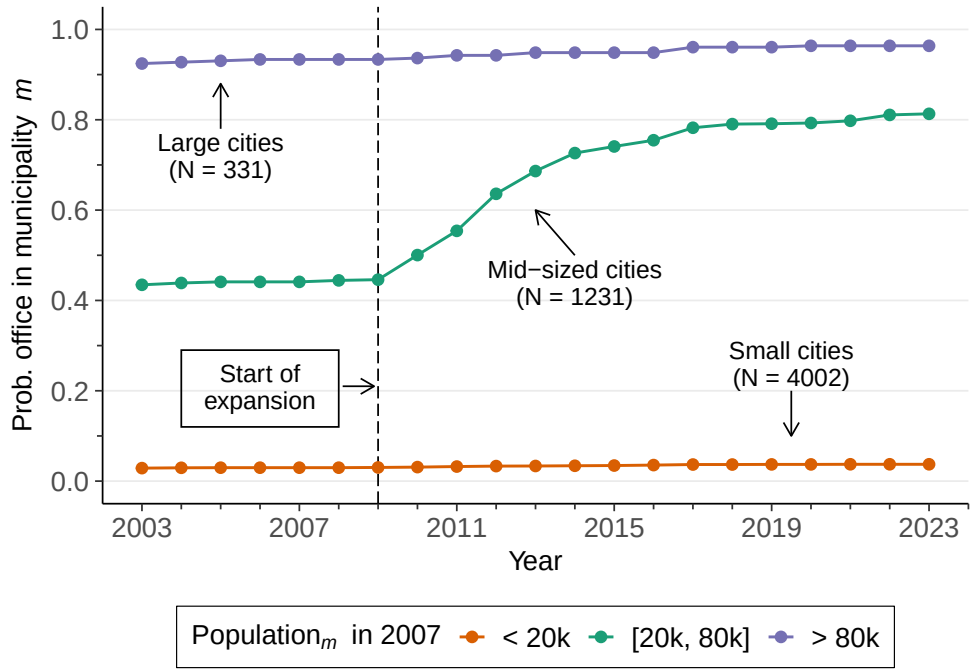
Importantly, the welfare effects of reducing take-up frictions are theoretically ambiguous. On the one hand, these frictions could provide valuable self-targeting of pensions towards needier applicants (Nichols and Zeckhauser, 1982; Kleven and Kopczuk, 2011). On the other hand, reducing these frictions may improve welfare if marginal applicants misperceive the value of claiming benefits (Finkelstein and Notowidigdo, 2019). The resulting welfare effect also depends on the relative fiscal externalities generated by additional take-up, which may be negative and potentially large. More broadly, if lower frictions increase take-up, the relevant fiscal externalities should also incorporate any general equilibrium effects induced by the transfers themselves. Yet evidence on the fiscal externalities of reducing take-up frictions remains limited even in partial equilibrium, since most existing studies rely on experimental settings.

In this paper, I provide the first large-scale evidence on how expanding the delivery capacity of social insurance affects benefit take-up, targeting, and welfare. I study the expansion of social security offices in Brazil, depicted in Figure 1. By 2008, less than one thousand of Brazil's more than five thousand municipalities had a local social security office. Between 2009 and 2023, the federal government opened almost 500 new offices in mid-sized municipalities, expanding the network by 50.6 percent. I exploit the staggered timing of office openings and the arbitrary targeting of the expansion in a difference-in-differences design. I combine this design with novel administrative data on more than 68 million social security claims and municipality-level data on local economic activity to study how social insurance's delivery capacity affects take-up, targeting, and local economic outcomes. Importantly, I show that general equilibrium effects are an essential component of the fiscal externality, with important welfare implications for the design of social insurance systems.

A central challenge in identifying the effects of state capacity is that local state capacity is typically endogenous to economic outcomes. The Brazilian setting is particularly useful to address this concern because the expansion of social security offices targeted only municipalities with more than 20,000 residents by 2007, generating quasi-experimental variation in local delivery capacity. The setting is also attractive because social security is the country's largest federal transfer program, accounting for approximately 9 percent of GDP. In addition, the Brazilian Social Security Agency (INSS) administers a broad range of benefits, including old-age pensions, survivors insurance, disability insurance, maternity leave, sickness leave, and a means-tested pension for the elderly and for people with disabilities.

[1] Stigma has also been suggested as an important channel to explain incomplete take-up of social benefits.

Figure 1: Expansion of Social Security Offices in Brazil (2009-2023)



Note: This figure shows the expansion of state capacity in Brazil through increased social security office coverage in mid-sized municipalities. Municipalities are grouped by their 2007 population, and the y-axis plots the share of municipalities in each group with a social security office in each year from 2003 to 2023 (x-axis).

I present the main results in three steps. First, opening a social security office increases benefit take-up among municipal residents. The number of new recipients rises by 5.2 percent, while the stock of social insurance recipients increases by 1.7 percent. The increase in new recipients is positive and statistically significant in every post-opening year, which suggests that the effect is not driven simply by intertemporal substitution in claiming. Consistent with a reduction in take-up frictions, office openings reduce claim processing time by about 10 percent, reduce travel time to the claiming office by 0.425 hour relative to a baseline of roughly one hour, and reduce the judicialization of maternity-leave and survivors insurance claims. As a result, social security transfers to the municipality increase by 8.8 percent on average and by 13.2 percent six or more years after the office opens. Since social security pensions amounted to 13.5 percent of GDP in treated municipalities on average before the expansion, this implies a sizable increase in local disposable income. Finally, social security contributions by residents of the municipality increase by 4.6 percent.

Notably, these effects are concentrated in municipalities with lower pre-existing participation in social insurance. Splitting treated municipalities by whether baseline transfers and contributions per capita were above or below the median, I find that the increase in transfers is 15.6 percent in lower-transfer municipalities, compared with 3.9 percent in higher-transfer municipalities. Likewise, social security contributions increase by 10.5 percent in municipalities with lower baseline contributions, while the effect is not statistically significant in municipalities with higher baseline contributions. These patterns suggest that expanding delivery capacity reduces cross-region inequalities in social insurance participation.

Second, I find suggestive evidence that reducing take-up frictions mostly improves the targeting of pensions, in the sense that the marginal applicants induced to claim seem to be rela-

tively needier on observable characteristics. The number of claims rises by 3.4 percent among formal workers, compared with 5.0 percent among the self-employed and 9.2 percent among individuals who were not employed when they started to contribute. I also find stronger effects among applicants with more severe health conditions: claims associated with high-severity disabilities or illnesses increase by 9.5 percent, compared with 5.4 percent and 5.8 percent for low- and medium-severity cases. Across benefit types, the largest increases occur for maternity-leave benefits (8.4 percent) and means-tested pensions (6.5 percent). Consistent with this pattern, opening a social security office also increases the share of total transfers accounted for by maternity-leave and sickness-leave benefits by 14.6 percent, suggesting that reduced take-up frictions are particularly important for more time-sensitive claims. At the same time, the share of claims from individuals aged 55 or older falls by 1.4 percent, while the share from individuals aged 30 to 54 rises by 1.0 percent, suggesting that the targeting effects are nuanced rather than pointing to an increase in targeting efficiency across the board.

Finally, I study the general equilibrium effects of opening a social security office. Opening an office does not directly provide any productive public infrastructure, since these offices only process and manage claims for social security benefits. However, the large increase in disposable income generated by higher take-up[2] suggests that office openings may nonetheless affect the local economy. For pension claimants, one might expect a negative labor supply response through an income effect. But to the extent that local economies have slack and aggregate demand is constrained, the increase in disposable income may instead stimulate local economic activity. Gerard et al. (2025) show that increases in conditional cash transfers raised local economic activity in Brazilian municipalities during the same period as my sample, suggesting that a similar demand-driven mechanism may operate in this context.

The evidence on tax revenue, labor market outcomes, and financial development is consistent with a demand-driven growth channel. I find that tax revenue from goods and services increases by 2.9 percent, that the number of corporate and personal income taxpayers per capita rises by 6.2 percent, and that overall tax collection increases by 3.8 percent. For the labor market analysis, I redefine treatment at the microregion level[3] to account for spillovers across municipalities within the same local labor market, and I find an increase of 2 percent and 4 percent on the number of establishments and formal labor contracts in the non-tradable private sector, respectively. In contrast, I find no effect on the tradable private sector, which supports the view that the expansion operates through local demand. Finally, consistent with local financial development, I find that the number of bank branches operating in the municipality increases by 4.4 percent and that consumer loans per capita rise by 13.5 percent.

Importantly, the local economic effects of the delivery capacity expansion are not shared uniformly across treated municipalities, but are instead concentrated in those with lower baseline levels of economic development. Among treated municipalities in my main sample, there

[2] Transfers rise by 13.2 percent six or more years after an office opens, and these transfers account for 13.5 percent of local GDP in treated municipalities on average.

[3] More specifically, Brazilian municipalities are grouped into microregions, which are approximately 500 in total. Prior research has suggested that labor markets in Brazil are defined at the microregion (and occupation) level, rather than at the municipality level (Felix 2024). In this exercise, I define a municipality as treated by the policy change if a social security office is opened at any municipality within the same microregion, where I restrict all municipalities to be those which did not have any office by 2008.

is substantial heterogeneity in pre-opening GDP per capita. Municipalities below the median had an average GDP per capita of R$4,343.23 two years before office opening, compared with R$14,946.22 among municipalities above the median, even though the average population size was very similar across the two groups (30,492.93 and 30,916.45, respectively).

I find that both groups experience similar increases in social security transfers, equal to 9.1 percent in lower-income municipalities and 10.5 percent in higher-income municipalities. However, only poorer municipalities exhibit broader local economic effects: the number of formal labor contracts in the private sector increases by 10.9 percent, tax revenue from goods and services rises by 6.4 percent, average wages increase by 2.4 percent, and bank deposits grow by 13.8 percent. In contrast, the corresponding effects in richer municipalities are statistically indistinguishable from zero. This pattern is consistent with a demand-driven mechanism that operates primarily in places with greater economic slack.

Does the reduction in take-up frictions increase the willingness-to-contribute to social security? Despite 45.4 percent of the Brazilian workforce being either informal or self-employed in 2010, only 22.6 percent of these workers contributed to social security. One possible reason for this incomplete enrollment is that workers place a higher value on alternative uses of their resources. Prior research has documented that liquidity constraints can bind insurance take-up decisions (Cole et al., 2013), but workers may also optimally choose not to contribute when social insurance is perceived as bad value for money (Olken et al., 2026). Understanding whether take-up frictions reduce the perceived value of social insurance, and whether lowering those frictions raises enrollment, is therefore important for the design of social insurance systems, and the Brazilian office expansion provides a compelling setting to study this question.

Interpreting the estimated effect on social security contributions is not straightforward. Because office openings also generate broader general equilibrium effects on the local economy, the 4.6 percent increase in contributions cannot on its own be interpreted as evidence of a higher willingness-to-contribute. In particular, it could simply reflect the increase in formal private-sector employment described above. To address this concern, I re-estimate the effect on social security contributions while including contemporaneous controls for local economic activity, such as income tax collection, the number of formal labor contracts, GDP, and social security transfers. Even after controlling simultaneously for all of these variables, the estimated effect on social security contributions remains 3.2 percent and is still statistically significant at the 10 percent level. This finding suggests that the increase in social security contributions cannot be fully explained by current economic activity, and is therefore consistent with reduced take-up frictions increasing the willingness-to-contribute to social security.

Finally, does the expansion in delivery capacity of social insurance increase welfare? To answer this question, I implement the marginal value of public funds (MVPF) framework of Finkelstein and Notowidigdo (2019) and extend it to incorporate general equilibrium fiscal externalities through higher tax revenue. I allow the MVPF to vary across groups defined by pension type and worker status at affiliation, so that welfare depends on the number of marginal and inframarginal applicants, the size of application misperceptions, the reduction in application costs, average benefit levels, and the fiscal externalities generated by transfers. To quantify the latter, I estimate the cross-region elasticity of taxes with respect to transfers us-

ing two-stage least squares, instrumenting transfers with the office expansion treatment. I find an elasticity of 0.396, implying that a 10 percent increase in social security transfers raises taxes from goods and services, income taxes, and social security contributions by 3.96 percent. Multiplying this elasticity by the average ratio of taxes to transfers yields a tax-revenue multiplier of 0.621, broadly consistent with the 0.58 estimate in Gerard et al. (2025).

The welfare conclusions depend strongly on whether workers are allowed to misperceive the value of claiming benefits. In a neoclassical model with no misperceptions, the estimated MVPF is 0.28 and rises to 0.68 once general equilibrium fiscal externalities are incorporated, implying that expanding delivery capacity still entails a net resource cost to the government. By contrast, in the behavioral model that allows for misperceptions, the MVPF is 1.15 and rises to 2.76 after accounting for general equilibrium effects. Under this interpretation, improving delivery capacity is welfare enhancing, and incorporating general equilibrium fiscal externalities more than doubles the estimated value of the policy relative to its net fiscal cost.

Taken together, these results show that expanding the delivery capacity of social insurance has important effects along several dimensions. The targeting results indicate that reducing take-up frictions allows relatively needier individuals to claim benefits, implying that the standard self-targeting logic is incomplete in this setting: for disadvantaged individuals, application barriers and imperfect information about eligibility may be especially costly, as in Deshpande and Li (2019). In addition, the increase in social security transfers is large enough to raise local disposable income substantially, generating meaningful general equilibrium effects.

This paper contributes to a large literature on social insurance in developing countries. A growing body of work emphasizes that efforts to expand coverage are often constrained not only by (lack of) demand for insurance, but also by limited state capacity to register and serve potential beneficiaries (Banerjee et al., 2021, 2024). Banerjee et al. (2021) provide experimental evidence from Indonesia showing that registration assistance can substantially raise attempted enrollment, but weak administrative capacity prevents many households from successfully enrolling. I provide the first quasi-experimental evidence on a large-scale expansion in the delivery capacity of social insurance in a large developing country, and I show how it affects take-up, targeting, and local economic activity. This paper also relates to a second strand of the literature that studies whether low enrollment in social insurance reflects frictions or low valuation. Prior work shows that take-up of social benefits can be highly sensitive to administrative and psychological frictions (Bhargava and Manoli, 2015), while recent work in developing countries emphasizes that households may also choose not to enroll because the willingness-to-pay for insurance is low (Cole et al., 2013; Olken et al., 2026). I contribute to this literature by providing suggestive evidence that take-up frictions themselves may depress the perceived value of social insurance, thereby lowering the willingness-to-contribute.

Finally, this paper contributes to the literature on the effects of application costs and information frictions on the take-up and targeting of social benefits. Recent evidence suggests that application costs are often more important than information frictions in limiting take-up, while the implications for targeting efficiency are somewhat context-dependent (Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019; Castell et al., 2025; Rafkin et al., 2026). I contribute to this literature by studying a large-scale quasi-experimental expansion in the delivery capacity

of social insurance in a developing country, and by tracing its effects on take-up, targeting, and local economic activity.

To the best of my knowledge, Gupta (2026) is the only other paper examining take-up and targeting in a developing-country setting, and it also finds evidence that reducing take-up frictions can improve targeting efficiency. My paper also complements Deshpande and Li (2019): while they study the closure of Social Security field offices in the United States, I study the opening of social security offices in Brazil. It is also related to Toneto (2025), which examines the labor market effects of office openings in Brazil. In contrast, Toneto (2025) relies on only 288 office openings and does not have benefit-level data, and therefore cannot study take-up or targeting directly. By using data on 484 office openings, I am able to provide a more comprehensive picture of the effects of expanding delivery capacity, including heterogeneity in general equilibrium effects across municipalities and the welfare implications of the policy.

Outline — The rest of the paper is organized as follows. Section 2 discusses the institutional background and the data. Section 3 presents the empirical strategy and describes the expansion of social security offices. Section 4 presents the main results on take-up, targeting, and local economic activity, including heterogeneity analyses and robustness checks. Section 5 examines the broader implications, including the effects on willingness-to-contribute to social security and on welfare. Finally, Section 6 concludes.

2. Background and Data

This section provides institutional background on social insurance in Brazil and describes the administrative and municipality-level data used in the analysis.

2.1. Institutional Background

Brazil is a developing country with a relatively large welfare state and a broad system of publicly provided social protection. In addition to unemployment insurance, conditional cash transfers to low-income households, and universal public healthcare, the country operates a large social security system that provides old-age pensions, survivors insurance, disability insurance, maternity leave, sickness leave, and a means-tested pension for elderly individuals and people with disabilities. These programs are administered by the Brazilian Social Security Agency (*Instituto Nacional do Seguro Social* or INSS).

Eligibility to social security pensions requires contributions to the system during the work life. Formal-sector workers are required to contribute through 20% payroll taxes and an additional 9-11% individual contributions. For informal and self-employed workers, contributions are also mandated by law, but in practice they are voluntary since enforcement is limited in the absence of reliable earnings records. Informal and self-employed workers can contribute either with a 5-11% contribution on the national minimum wage or with a 20% contribution on self-reported earnings.

Social security benefits are relatively generous since they are lower-capped on the national

minimum wage, which is periodically adjusted above the yearly inflation level. In 2010, more than 50% of Brazilian workers received less than 1.5 minimum wage, and approximately 70% received less than 2 minimum wages. Social benefits administered by the INSS fall into six major categories: old-age pensions, disability insurance, survivors insurance, sickness leave, maternity leave, and a means-tested pension known as BPC-LOAS. These categories differ substantially in both contribution requirements and fiscal importance, while all categories provide lifelong-pensions with the exception of sickness leave and maternity leave.

In 2010, old-age pensions accounted for 18.5 percent of claims but 50.6 percent of transfers, survivors insurance for 8.8 percent of claims and 22.9 percent of transfers, disability insurance for 4.3 percent of claims and 11.1 percent of transfers, sickness leave for 48.5 percent of claims and 6.2 percent of transfers, and means-tested pensions for 8.0 percent of claims and 8.2 percent of transfers. The BPC-LOAS program is non-contributory and serves elderly individuals above age 65 and people with disabilities who do not qualify for contributory pensions.

An important feature of the Brazilian setting is that, for most of the period I study, claiming benefits required in-person visits to social security offices.[4] Applicants had to start the benefit claiming process at a local INSS field office, where they submitted documentation supporting eligibility and where the INSS would cross-check information against its administrative records on contribution histories. For disability- and illness-related benefits, the process also involved medical examinations conducted inside the office. Claims were then processed by regional INSS offices, and applicants were commonly required to return to the local office to provide additional documentation before a final decision was made. Therefore, accessibility to a local social security office was an important determinant of application costs in Brazil, as individuals in municipalities without an INSS office had to travel to another municipality to apply, submit documents, undergo medical evaluation, or respond to administrative requests. Administrative capacity constraints also generated substantial delays: the average processing time in 2008 was 148 days. Additionally, judicial disputes as a result of benefit denials were also common, with 8.4 percent of benefits claimed in 2008 involving some form of judicialization.

The role of physical access was especially important because online claiming only became relevant around 2018 and 2019. Appendix Figure A5 shows that digital claiming started in 2018 and remained limited for much of the period covered by the main analysis. Thus, during most of the office expansion, social security offices were the main point of contact between claimants and social insurance delivery. Finally, INSS offices served the sole purpose of administering social security claims and providing medical examinations, as well as enrollment of new contributors to social insurance. Thus, these offices did not provide any infrastructure to tax collection and fiscal capacity in the municipality, for example.

2.2. Data

I employ novel administrative records on social security claims at the benefit-level, office-level information, and municipality-year level data on local economic outcomes.

The benefit-level data is the *Sistema de Informações the Benefícios* (SUIBE), which contains

[4] Appendix Figure A4 depicts an usual INSS office.

information on all 68 million social security benefits claimed between 2007 and 2020. These records include the type of pension claimed, the recipient's municipality of residence, date of birth, date of claiming, date of benefit dispatch, years of contribution to social security, work status at affiliation (whether formally employed, informal/rural worker, self-employed, or unemployed), municipality where the pension was claimed, and, for health-related claims, ICD codes on the underlying condition.

I create a municipality-year level dataset from 2007 to 2020 using the information from the benefit-level data. Importantly, the municipality refers to the municipality of residence of the claimant, not to the municipality where the pension was claimed in an INSS office. I combine this dataset with office-level administrative data on all municipalities with a social security office and its opening date. These data allow me to construct the timing of the expansion in local delivery capacity and to link office openings to claiming outcomes. They also make it possible to study mechanisms related to access, such as distance to the nearest office and distance to the office where the claim was filed.

Next, I create a municipality-year dataset from several additional sources. Formal labor market outcomes come from the publicly-available *Relação Anual de Informações Sociais* (RAIS) dataset, which provides information on all formal labor contracts and establishments, including wages, occupation, sector, and municipality. Financial outcomes come from the *Estatística Bancária Mensal* (ESTBAN) dataset, which contains information on banks and bank branches as well as deposits, loans by type, and related measures of local financial activity at the municipality-year level. I also use municipality-level data on GDP and population estimates from the *Instituto Brasileiro de Geografia e Estatística* (IBGE), social security transfers from the INSS (including by pension category[5]), and social security contributions and income tax outcomes from the Brazilian Tax Authority. In addition, I use the 2010 Brazilian Census to provide summary statistics on all municipalities, including baseline social insurance participation.

The resulting dataset is an annual municipality-level panel spanning 2007 to 2021 for all 5,570 Brazilian municipalities, while the outcomes derived from the benefit-level dataset are only available through 2020. For the empirical exercises described below, I restrict the municipalities to the main sample in three steps. First, I require all municipalities to have existed since 2001, which drops 10 municipalities. Second, I only keep municipalities which did not have a social security office by 2008, which drops 975 municipalities. Finally, I remove municipalities whose population in 2007 was an outlier relative to the distribution. That is, I drop municipalities from the resulting sample whose population in 2007 was either below the 0.5% quantile or above the 99.5% quantile of the distribution. This step drops 46 municipalities, resulting in a sample of 4,539 municipalities and includes 484 office openings, covering 97.4 percent out of of the 497 municipalities that obtained their first office during the expansion period.

[5] Importantly, the information on social security tranfers by pension type is only available form 2010 onwards. For 2007-2009, I assume that the share of transfers among pension types is the same as the shares in 2010, and then proxy the pension-type transfer outcome.

3. Empirical Strategy

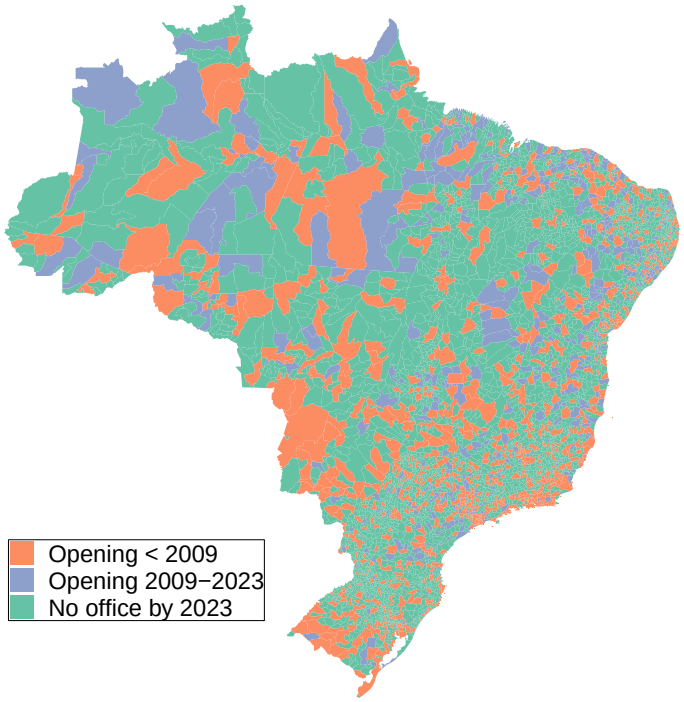
This section describes the expansion of social security field offices in Brazil and the empirical strategy used to estimate its effects. I first discuss the institutional details of the expansion and the sources of variation in office openings across municipalities. I then present the difference-in-differences design used to identify the effects of increased local delivery capacity on social insurance take-up and other outcomes.

3.1. The Expansion of Social Security Offices

By 2008, the geographic coverage of social security offices in Brazil remained limited. Only 975 out of 5,570 municipalities had an INSS field office, which meant that residents of smaller municipalities had to travel elsewhere to claim benefits. Weighting by the population size, this meant that 67.9% of residents in Brazilian municipalities with less than 80 thousand residents did not have a local INSS office. This limited local presence implied sizable access barriers, especially because benefit applications required in-person claiming.

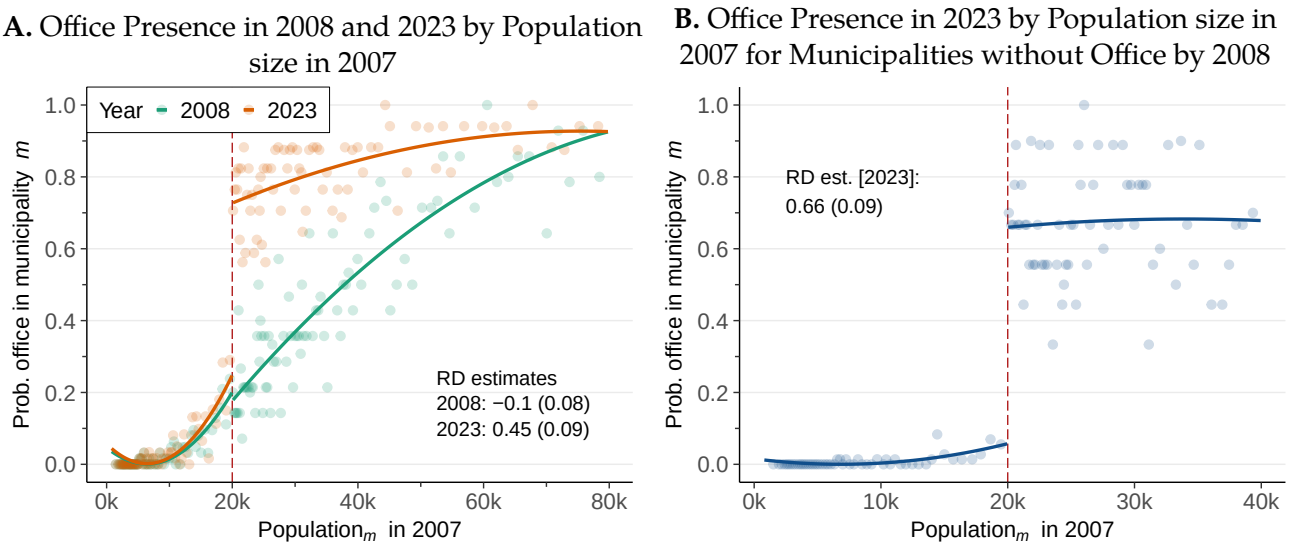
In 2009, the INSS launched a plan to expand its network of field offices with the goal of increasing access to social security services in smaller municipalities (this plan was called *Plano the Expansão da Rede de Atendimento* or PEX). The expansion proceeded gradually between 2009 and 2023, during which 494 municipalities obtained their first office. Relative to the 975 mu-

Figure 2: Map of Social Security Office Presence by Municipality



Note: This figure presents a map of Brazilian municipalities grouped by their status relative to social security office coverage. Municipalities in orange had an INSS office since before 2009, municipalities in blue got their first office from 2009 to 2023 as a result of the expansion program, and municipalities in green never had an office by 2023.

Figure 3: The Expansion Created a Discontinuity of Office Presence By Population Size in 2007



Note: This figure plots the share of municipalities with an INSS office against municipality population in 2007. Panel A shows office presence in 2008 and 2023 for the full sample of municipalities, while Panel B restricts the sample to municipalities without an office in 2008 and shows office presence in 2023. The solid lines are local polynomial fits estimated separately on each side of the 20,000-population cutoff using the Calonico et al. (2014) RD procedure with a quadratic polynomial and triangular kernel. The reported RD estimates correspond to the discontinuity at the cutoff, with robust bias-corrected confidence intervals.

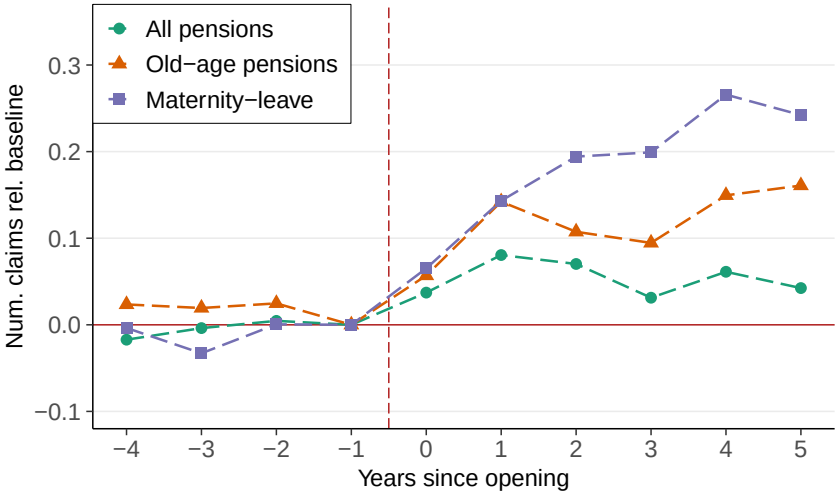
nicipalities with an office in 2008, this represented a 50.6 percent increase in coverage. Figure 2 depicts a map of Brazilian municipalities grouped by whether they already had an office by 2008, whether they got their first office through this expansion, or whether they never got an office. Notably, this policy expanded office coverage among all regions of the country.

An important feature of the social security office expansion was that it explicitly targeted municipalities above a population threshold. Municipalities with at least 20,000 residents in 2007 were eligible for the program, whereas smaller municipalities were generally not targeted. As a result, the policy disproportionately expanded office presence in mid-sized municipalities, generating a sharp discontinuity in the probability of having an office by the end of the expansion period, which can be seen in Figure 3.

However, the rollout of offices was staggered over time rather than implemented all at once. This gradual rollout reflected administrative and budgetary constraints. Municipal governments were required to provide land for the new office, while the INSS had to secure funding, oversee construction, and hire the personnel necessary to operate each unit. These constraints meant that not all eligible municipalities received an office immediately, and some municipalities with more than 20 thousand residents by 2007 never received one during this period. Appendix Table A2 presents summary statistics of treated municipalities by timing of office opening and also for municipalities which were eligible but did not get an office. Overall, this table suggests that the differential timing of the expansion was correlated with distance relative to the state capital, as municipalities treated in 2009-2011, 2012-2014, and 2015-2023 had on average 3.9, 4.2, and 6 hours of distance relative to their state capitals, respectively. As a result, municipalities treated earlier were in fact closer to INSS offices to begin with, and they also had relatively higher GDP per capita.

Figure 4 presents the trends of number of new recipients in treated municipalities relative

Figure 4: Evidence from Raw Data that the Expansion Increased Take-up of Social Security Benefits: Trends in New Recipients Relative to Baseline



to the pre-opening year. These raw data provide suggestive evidence that the opening of a social security office increased the number of new recipiens of social insurance in treated municipalities, and motivate the difference-in-differences strategy explained below to estimate the causal effects of expanding local social insurance delivery capacity.

3.2. Empirical Strategy

To estimate the effects of expanding local social insurance delivery capacity, I employ a difference-in-differences strategy that exploits the staggered timing of social security office openings across municipalities. The main idea is to compare outcomes in municipalities before and after they receive their first INSS office to outcomes in municipalities that have not yet been treated or are never treated within the sample.

The main specification is the event-study regression given by equation (1):

$$y_{m,t} = \alpha_m + \delta_{s(m),t} + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}_{(t - e_m = k)} + \lambda' X_{m,t} + \varepsilon_{m,t} \tag{1}$$

where $y_{m,t}$ is the outcome of interest in municipality m and year t, α_m are municipality fixed effects, $\delta_{s(m),t}$ are state-by-year fixed effects, and e_m is the year in which municipality m receives its first INSS office ($e_m = +\infty$ for never treated municipalities). I include time-varying effects of predetermined municipality characteristics as $\lambda' X_{m,t} = \sum_\ell \sum_{j \neq 2007} \lambda_j^\ell \cdot \mathbb{1}_{(j=t)} \cdot X_m^\ell$, which allow differential trends by interacting year dummies with predetermined municipal characteristics measured before the expansion, specifically the population size in 2006 and population growth between 2001 and 2006. The coefficients β_k for $k \in \{\leqslant -5, -4, -3, -2, 0, 1, 2, 3, 4, 5, \geqslant 6\}$[6] trace the dynamic effects of office openings relative to the pre-treatment period.

I estimate this specification using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). This approach is appropriate in this setting as treatment timing is stag-

[6] Note that I bin all pre-periods before -5 at -5 and all post-periods after +6 at +6 for treated municipalities, such that the estimating sample includes all (non-missing) information of treated municipalities, rather than restricting the panel around the -5 to +6 window.

gered and treatment effects may be heterogeneous across cohorts and over event time, such that two-way fixed-effects estimators might be biased.

The identifying assumption for β_k to recover the causal effect of opening a social security office is that, absent office openings, treated municipalities would have followed parallel trends relative to not-yet-treated and never-treated municipalities conditional on controls. The inclusion of municipality fixed effects absorbs time-invariant differences across municipalities, while state-by-year fixed effects account for any shocks common to municipalities within the same state, including state-level policy changes and regional business-cycle variation. Also, the flexible control using baseline population size and growth allows for differential trends among municipalities of different size, which helps improve the precision of the fixed effects since treated and never treated municipalities have different average population levels.

As described in Section 2, the main sample includes all municipalities that did not have an INSS office by 2008 and excludes extreme outliers in the 2007 population distribution. This restriction avoids contaminating the design with municipalities that had offices prior to the

Table 1: Summary Statistics at the Municipality-level: All Municipalities and Main Sample

	All Municipalities		Main Sample	
	No office	Office	No office	Office
	(1)	(2)	(3)	(4)
Panel A. Demography and the local economy (2008)				
Population	10114.350	100874.367	9800.133	30705.192
GDP/Pop	9063.147	11401.371	9036.619	8105.560
Hours to closest INSS office	0.815	0.356	0.816	1.038
INSS transfers/Pop	601.456	937.872	603.786	714.550
INSS transfers/GDP	0.095	0.126	0.095	0.135
Income tax collection/Pop	85.626	222.157	81.878	80.228
Social security contributions/Pop	221.261	369.718	220.372	212.328
Tax collection net-of-benefits/Pop	526.516	1502.655	514.305	497.371
Num. new benefit recipients/Pop	0.027	0.025	0.027	0.023
Num. formal labor contracts/Pop	0.108	0.150	0.107	0.107
Panel B. Census data (2010)				
Avg. household earnings per capita	497.224	631.848	496.784	469.291
% With secondary school education	0.296	0.381	0.295	0.310
% Working	0.460	0.487	0.460	0.446
% Formally employed	0.364	0.445	0.363	0.360
% Informally employed	0.309	0.266	0.309	0.321
% Self-employed	0.293	0.264	0.294	0.284
% Contributing to social security	0.491	0.555	0.490	0.459
% Informal empl. contributing to SS	0.139	0.134	0.139	0.116
% Self-employed contributing to SS	0.236	0.267	0.236	0.196
Observations	4101	1469	4055	484

Note: This table presents municipality-level summary statistics separately for municipalities that never receive an INSS office during the sample period and municipalities that eventually receive one. Columns 1 and 2 use all municipalities, while Columns 3 and 4 restrict attention to the main sample, defined as municipalities without an INSS office by 2008 and excluding outliers in 2007 population. Variables in Panel A are measured in 2008, before the expansion begins, and variables in Panel B are measured using the 2010 Census.

policy change and might have gone through decreased claims as a result of the opening of new offices in neighboring cities. The resulting sample contains 4,539 municipalities, which consists of 81.5 percent of all Brazilian municipalities, and it contains 484 out of the 497 first-office openings observed during the expansion period. Appendix Figure A1 depicts the histogram of population size in 2007 of municipalities without an INSS by 2008, before and after removing the outliers. For most outcomes, treatment is defined at the municipality level because the opening of a field office directly changes local access to the claiming process for residents of that municipality. However, for labor market outcomes I also redefine treatment at the microregion level to allow for spillovers across municipalities within the same local labor market.

Table 1 presents key summary statistics at the municipality-level for municipalities with and without a social security office in the full sample of all municipalities and in the main sample. Although municipalities in the main sample that later receive an office are larger and somewhat farther from the nearest INSS office, they look quite comparable on several economic margins, including income tax collection per capita, social security contributions per capita, tax collection net of benefits per capita, and formal labor contracts per capita. The same is true for the census variables. In 2010, municipalities that later receive an office have similar rates of formal employment, self-employment, and secondary-school attainment, while differences in household earnings and social security participation are small. Overall, relative to the full sample with all municipalities, the main sample substantially improves comparability between treated and never treated municipalities.

A potential violation of the parallel trends assumption would arise if treated municipalities were on differential economic trends relative to never treated municipalities. To explore this possibility, in Appendix Figure A2 I plot the trends of never treated and treated municipalities among several variables from 2005 to 2010 and show that both groups were on similar trends before the start of the expansion in social insurance delivery capacity.

4. Main Results

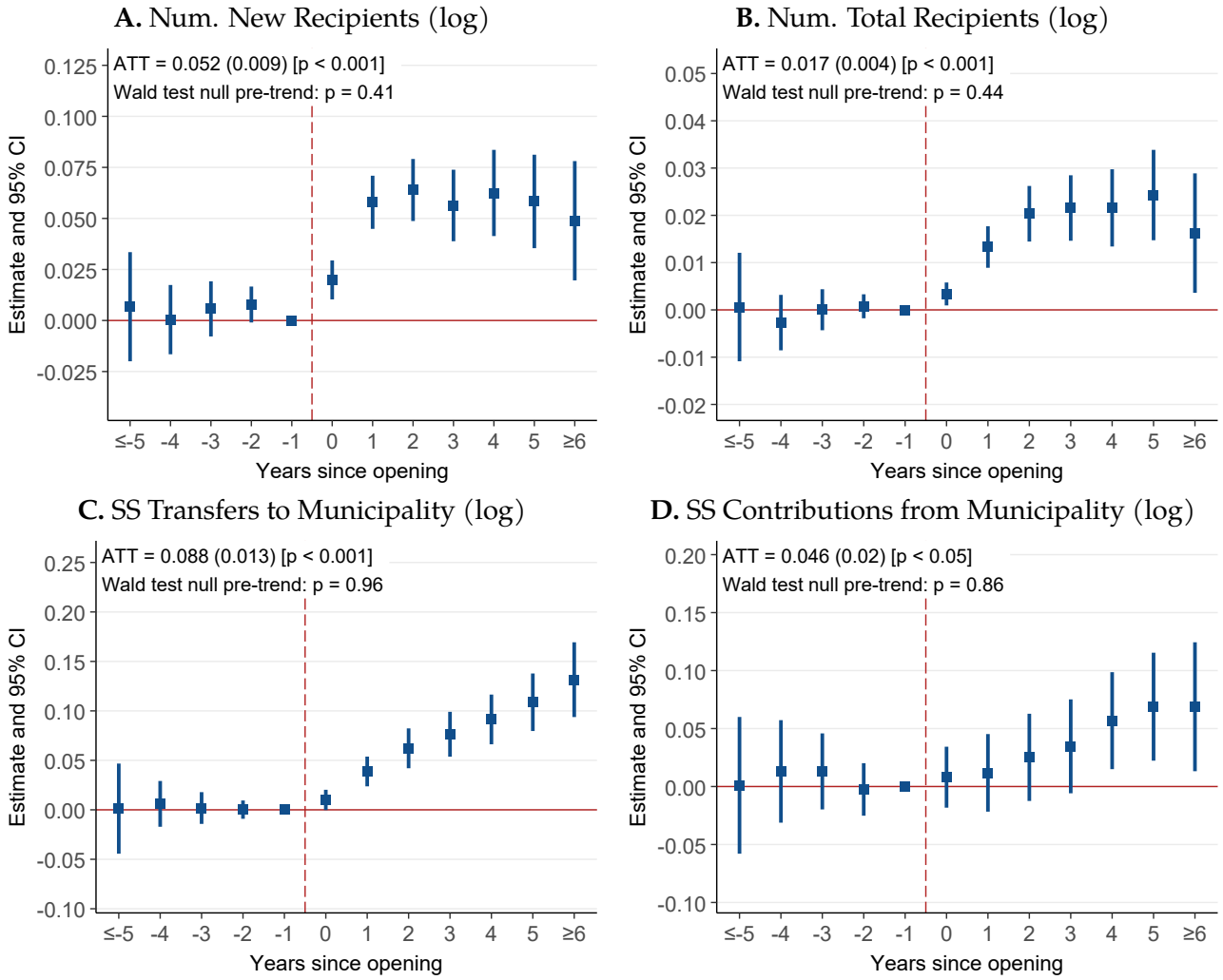
This section presents the main empirical results on the effects of expanding the local delivery capacity of social insurance. I start by examining the effects on benefit take-up, social security transfers, and social security contributions. I then explore the observable characteristics of marginal recipients to understand how the reduction in take-up frictions affects the composition of beneficiaries. Finally, I analyze the local economic effects due to increased local social security transfers.

4.1. Effect on Take-up, Transfers, and Social Security Contributions

I find that opening a social security office increases benefit take-up among municipal residents. Figure 5 Panel A shows that the number of new benefit recipients rises by 5.2 percent on average after an office opens.[7] The event-study coefficients are positive in every post-opening year and display no evidence of differential pre-trends (the p-value for the test of joint nullity of all

[7] Appendix Table A3 presents the dynamic treatment effects for the main results.

Figure 5: Effect on Take-up, Transfers, and Social Security Contributions

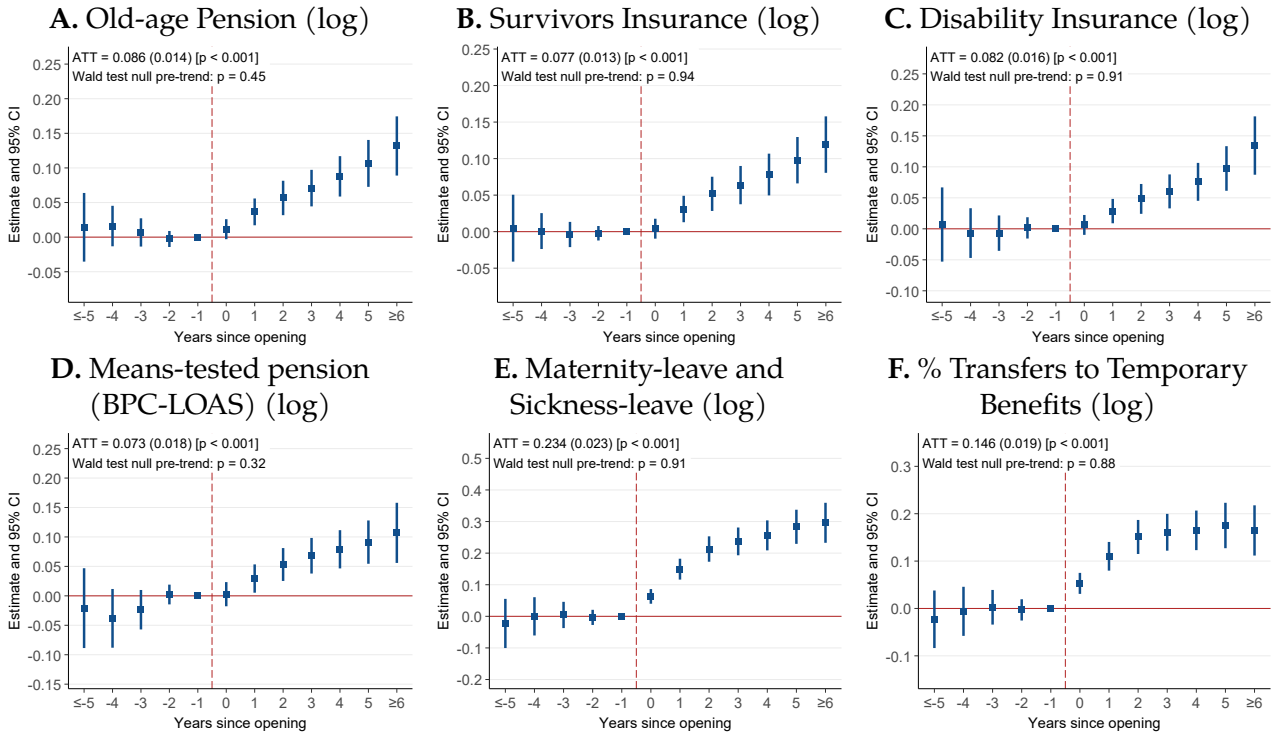


Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

pre-treatment coefficients is equal to 0.41), which suggests that the increase is not driven by differential trends across treated and control municipalities. Also, the fact that the estimated effect is approximately the same at all post-opening periods suggests that the estimated effect should not be interpreted as a temporary reshuffling of claims across periods, which could be the case if workers simply started claiming social insurance sooner. The increase in new recipients also translates into a larger stock of beneficiaries: the total number of benefit recipients in the municipality (Panel B) rises by 1.7 percent, further suggesting that the opening of an office does not simply accelerate claims that would have occurred anyway.

The increase in take-up created a large increase in transfers to the local economy (Panel C). Social security transfers to the municipality increase by 8.8 percent on average, with effects that build over time and reach 13.2 percent six or more years after the office opening. Figure 6 presents the effect on transfers by pension type and shows that there is not much heterogeneity on the margin of which transfers were most affected among old-age pensions, survivors insur-

Figure 6: Effect on Social Security Transfers by Pension Type



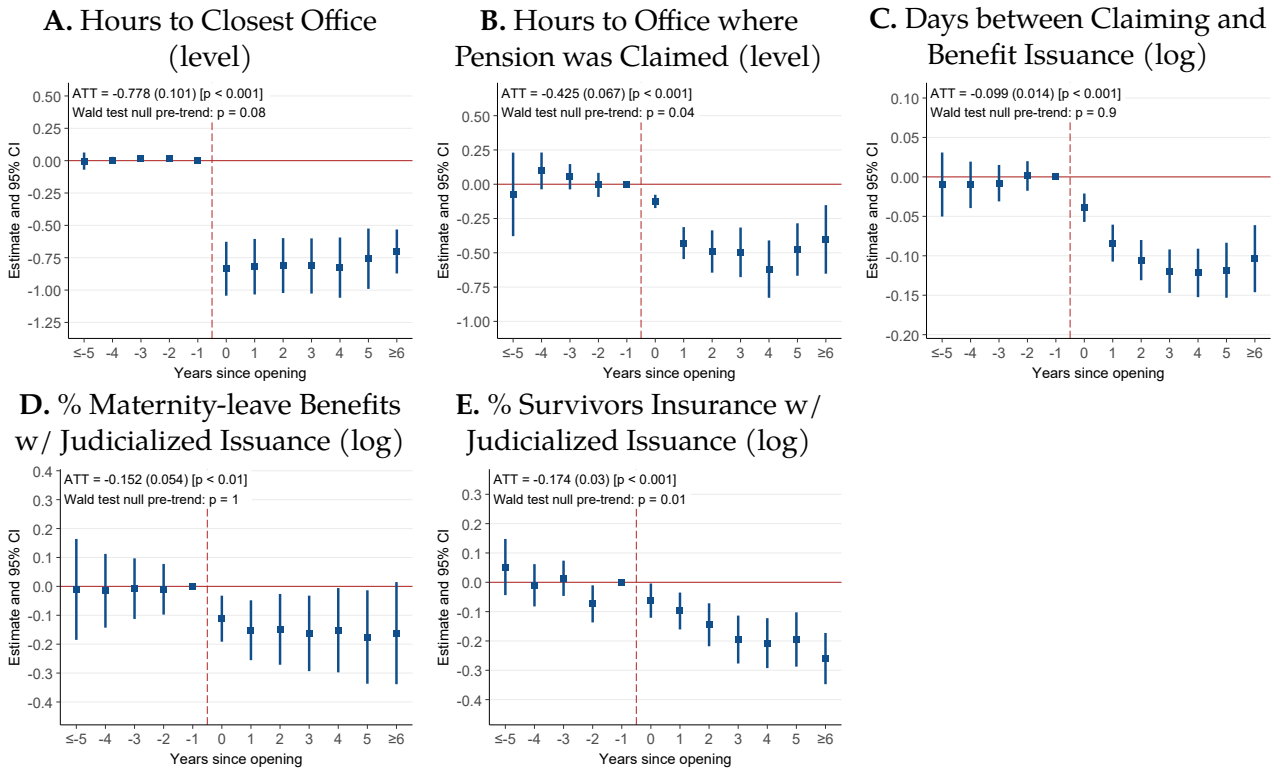
Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

ance, disability insurance, and means-tested pension. However, the share of local transfers that go towards temporary benefits (i.e. maternity leave and sickness leave) considerably increases by 14.6 percent. Importantly, social security transfers represented a large share of local income in treated municipalities before the expansion, such that the estimated effect implies a substantial increase in disposable resources received by residents.

Finally, opening a social security office also increases social security contributions paid by residents of the municipality by 4.6 percent (Figure 5 Panel D). However, interpreting this result is not straightforward. It could be the case that reducing take-up frictions raises the perceived value of social insurance and, in turn, increases the incentive to remain enrolled or to start contributing. At the same time, because I show in Section 4.3 that office openings also affect local economic activity more broadly, it could also be explained solely by changes in labor market conditions. Lastly, it is hard to rule out the possibility that opening an office only increases the salience of the social insurance system, but not necessarily its perceived valuation.

Mechanisms — I provide further evidence that take-up frictions do indeed reduce as a result of opening an INSS office. Figure 7 shows that opening a field office reduces travel time to the nearest office by about 0.78 hour. In addition, the average distance to the office where a pension is actually claimed falls by roughly 0.43 hour, indicating that residents use the newly available local office rather than continuing to rely on offices in other municipalities.

Figure 7: Mechanisms: Distance to INSS Office, Days of Processing Claim, and Judicialization

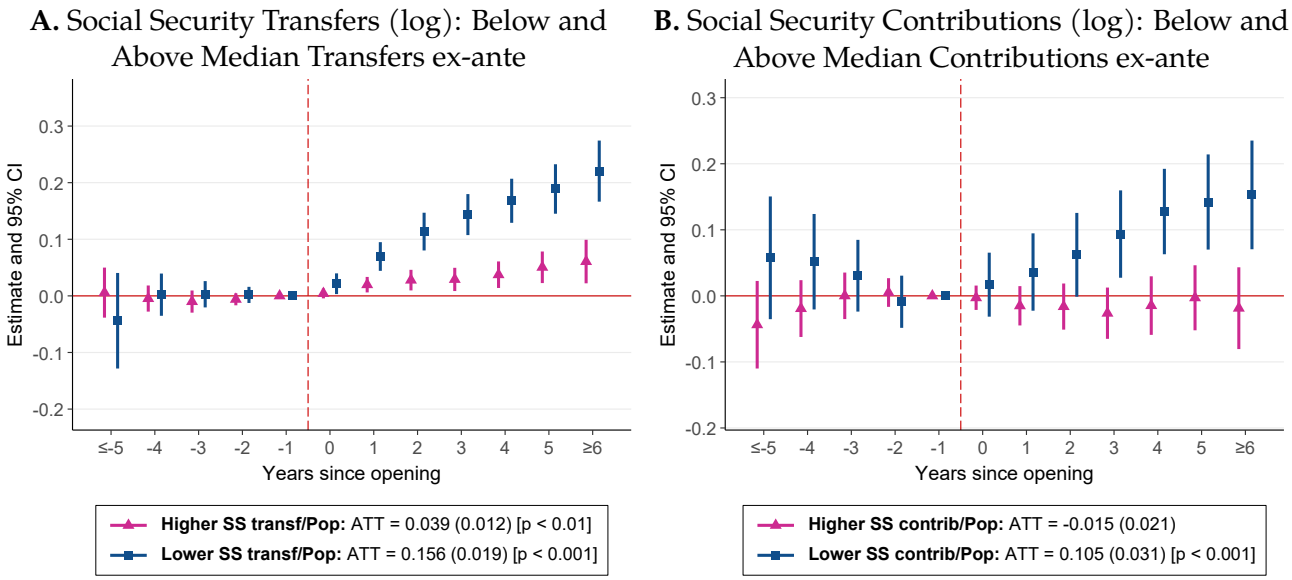


Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

Openings also improve administrative processing. The average number of days between claiming and benefit issuance falls by about 10 percent, implying that local expansion alleviates part of the lengthy delays in the claims process. This could be explained both by the reduced travel cost, as claimants are commonly required to return to the field office to provide additional documentation, as well as by increased staff capacity in managing the stock of claims to be analyzed. Moreover, the share of maternity leave and survivors insurance benefits issued through judicial disputes declines once a local office opens, which suggests that the policy reduces administrative failures or barriers that would otherwise lead claimants to rely on courts to obtain benefits.

Heterogeneity by Baseline Inequality in Social Insurance Participation — I find that the take-up effects are not evenly distributed across municipalities. I separate treated municipalities between those with higher and lower baseline participation in the social insurance system, as measured by transfers per capita and contributions per capita two year before the opening of an office. Figure 8 shows that the increase in transfers is substantially larger in municipalities with below-median transfers per capita before the expansion: transfers rise by 15.6 percent in these municipalities, compared with only 3.9 percent in municipalities above the median. Similarly, social security contributions rise by 10.5 percent in municipalities with below-median baseline contributions, while the estimated effect is close to zero and statistically insignificant

Figure 8: Heterogeneity: Effect on Transfers by Ex-ante Transfers p.c. and Effect on Social Security Contributions by ex-ante Contributions p.c.



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

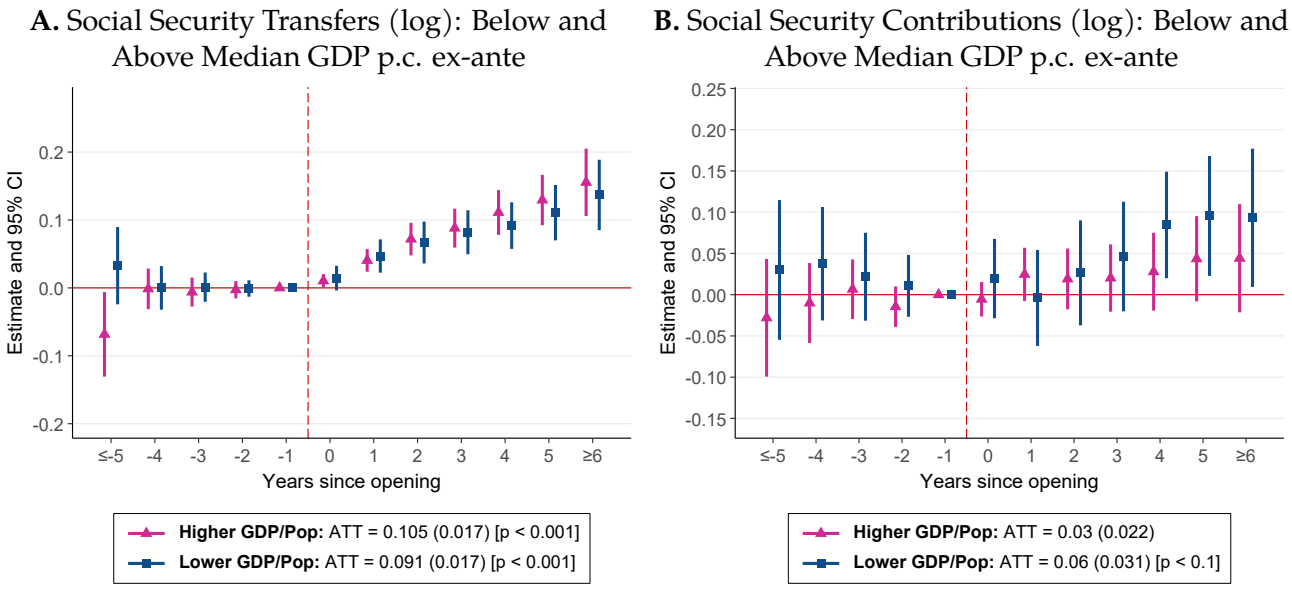
in municipalities with higher baseline contributions. These patterns suggest that the expansion of delivery capacity reduced cross-region inequality in social insurance participation by generating stronger gains in places that were initially less connected to the system.

Heterogeneity by Baseline Economic Development — The effects of expanding social security delivery capacity may differ depending on local economic conditions at baseline. On the one hand, poorer municipalities may experience larger effects if administrative barriers are more binding in these places to begin with. On the other hand, residents from richer municipalities may be better positioned to respond if they are more informed and if they have more previous social security contributions and thus are more likely to be eligible to pensions. To examine this heterogeneity, I split treated municipalities in the main sample according to whether GDP per capita two years before office opening was above or below the median.

Table A4 shows that there is a low of within-treatment variation in baseline economic development, despite both groups having very similar population size on average. Municipalities below the median had average GDP per capita of R$4,343.23 two years before office opening, compared with R$14,946.22 among municipalities above the median. Poorer treated municipalities also had lower baseline household earnings, lower formal employment, lower social security contributions, and substantially lower pre-existing participation in social insurance.

Figure 9 shows that the increase in social security transfers is similar across poorer and richer treated municipalities in percentage terms. The estimated effect is 9.1 percent in municipalities below the median of baseline GDP per capita and 10.5 percent in municipalities above the median, with both effects statistically significant. This result suggests that the direct effect

Figure 9: Heterogeneity: Effect on Transfers and Social Security Contributions by Baseline Economic Development



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

of expanding delivery capacity on benefit take-up is not confined to poorer or richer places, but rather office openings raise transfers broadly across different local economic environments. In contrast, the effects on social security contributions are noisier but appear somewhat stronger in poorer municipalities. Contributions increase by 6.0 percent in municipalities below the median of baseline GDP per capita, compared with 3.0 percent in municipalities above the median, although the estimate for richer municipalities is not statistically distinguishable from zero.

Taken together, these findings suggest that the take-up effects of office openings are broadly similar across municipalities with different levels of baseline development. However, note that even when transfer increases are comparable across groups, the downstream effects on the local economy through employment, tax revenue, and financial development might still depend on the amount of slack in the local economy. That is, baseline economic development may matter less for whether delivery capacity raises transfers (the "first-stage") and more for how those additional transfers propagate through the local economy.

4.2. Effect on Targeting

Next, I explore the effects of the increase in delivey capacity on the targeting of pensions by looking into observable characteristics of marginal applicants. I follow Deshpande and Li (2019) and provide results both for the effect on number of new recipients by group and also for the effect on the proportion of new recipients who are from a specific group. The targeting effect is of first-order importance because welfare consequences of reducing application frictions depend not only on how many additional individuals claim benefits, but also on who

Table 2: Estimates of the Effect of Openings on Applicant Characteristics

	Count (log)			Proportion (log)		
	Point estimate	Standard error	Avg. baseline counts	Point estimate	Standard error	Avg. baseline share
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Pension type						
Old-age pension	0.038***	(0.013)	157.05	−0.013	(0.012)	0.23
Survivors insurance	0.023**	(0.009)	69.28	−0.029***	(0.01)	0.1
Disability insurance	0.002	(0.024)	25.87	−0.048**	(0.023)	0.03
Sickness-leave	0.056***	(0.014)	282.68	0.005	(0.011)	0.36
Maternity-leave	0.084***	(0.022)	131.41	0.033*	(0.018)	0.18
Means-tested pension	0.065***	(0.018)	58.52	0.013	(0.017)	0.09
Panel B. Worker type						
Formal	0.034**	(0.014)	197.03	−0.018	(0.014)	0.25
Rural / Informal	0.028*	(0.015)	316.27	−0.024*	(0.013)	0.47
Self-employed	0.05***	(0.018)	67.35	−0.001	(0.017)	0.08
Not employed	0.092***	(0.014)	143.55	0.04***	(0.011)	0.2
Panel C. Age at claiming						
< 30	0.063***	(0.014)	189.1	0.012	(0.008)	0.26
30-54	0.061***	(0.011)	281.65	0.01*	(0.006)	0.37
$\geqslant 55$	0.038***	(0.009)	253.61	−0.014*	(0.008)	0.37
Panel D. Years of contribution to social security						
$\leqslant 1$	0.043***	(0.01)	324.72	−0.009	(0.006)	0.47
2-9	0.053***	(0.014)	186.01	0.002	(0.011)	0.24
$\geqslant 10$	0.072***	(0.011)	214.08	0.02**	(0.009)	0.29
Panel E. Benefit size						
1 minimum wage	0.045***	(0.01)	514.71	−0.007*	(0.004)	0.75
$>$ 1 minimum wage	0.029**	(0.013)	210.11	−0.023*	(0.013)	0.25
Panel F. Severity of illness / disability						
Low	0.054***	(0.017)	107.59	−0.013	(0.012)	0.36
Medium	0.058**	(0.028)	94.11	−0.011	(0.023)	0.29
High	0.095***	(0.016)	103.56	0.029***	(0.011)	0.35
Panel G. Illness / disability type						
Musculoskeletal	0.044*	(0.024)	54.07	−0.024	(0.017)	0.16
Injuries and external causes	0.053***	(0.015)	86.52	−0.015	(0.011)	0.28
Mental and behavioral disorders	0.035	(0.026)	30.13	−0.03	(0.021)	0.1
Neurological and sensory	0.093***	(0.022)	18.75	0.028	(0.018)	0.07
Cardiovascular and respiratory	0.074***	(0.02)	27.96	0.009	(0.016)	0.09
Cancer	0.068***	(0.021)	19.89	0	(0.019)	0.07
Infectious, digestive, and other	0.091***	(0.018)	67.95	0.024**	(0.012)	0.23

Note: This table reports average treatment effects on the treated (ATTs) estimated using the same difference-in-differences strategy as in the event-study figures above, based on equation (1) and the heterogeneity-robust estimator of Sun and Abraham (2021). For each outcome, I estimate the full event-study specification and report the ATT as the average of the post-treatment coefficients. Columns 1 and 4 report point estimates, Columns 2 and 5 report standard errors clustered at the municipality level, and Columns 3 and 6 report the average baseline level of the outcome among treated municipalities. Outcomes in Columns 1–3 are the log number of claims in each category, while outcomes in Columns 4–6 are the log share of claims in that category relative to total claims in the municipality-year. All regressions include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined controls, and are estimated on the main sample. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

those individuals are and whether they have any misperceptions about claiming.

Table 2 presents the main targeting results and sugests substantial heterogeneity across worker groups. The number of claims rises by 3.4 percent among formal workers, compared with 5.0 percent among the self-employed and 9.2 percent among individuals who were not employed when they first affiliated with social security. These differences suggest that local office openings matter more for groups with weaker attachment to formal labor markets and also lower prior engagement with the contributory social insurance system.

The composition of claims also shifts across benefit types. The largest increases occur for maternity leave benefits, whose number of new recipients rise by 8.4 percent, and for means-tested pensions, which rise by 6.5 percent. In contrast, the increase is smaller for old-age pensions and survivors insurance, and close to zero for disability insurance in the count specification. This pattern is consistent with the view that application frictions are especially important for benefits that are more time-sensitive or that serve more vulnerable populations, since it is arguably more difficult for a mother with a newborn child to deal with the costly application to social insurance than for a worker who is about to retire. In line with this interpretation, the share of transfers accounted for by temporary benefits such as maternity-leave and sickness-leave also rises after office openings, as shown in Figure 6.

The evidence on health-related claims points in a similar direction. Claims associated with high-severity disabilities or illnesses[8] increase by 9.5 percent, compared with 5.4 percent and 5.8 percent for low- and medium-severity cases, respectively. These results suggest that the marginal claimants induced by the expansion are not less needy. If anything, office openings appear to bring into the system individuals facing relatively severe health conditions.

The age profile of claimants also changes, although the pattern is more nuanced and difficult to map to the relative neediness of marginal applicants. Claims rise for all age groups, but the proportional increase is larger among younger and middle-aged individuals than among older claimants. Consistent with this, the share of claims from individuals aged 55 or older declines modestly, while the share from those aged 30 to 54 rises. This result does not imply that older individuals benefit less in absolute terms, but rather that the expansion disproportionately activates claiming among working-age people.

In terms of years of contribution to social security, the marginal applicant is someone with relatively more attachment to the system. The share of claimants with 10 or more years of contributions increases by 2 percent. This result suggests that the the reduction in take-up frictions is not benefiting only those workers who were marginally attached to the system, but also those with large contribution histories to social security.

Finally, I look into the effect on the number of new recipients by the benefit size (equal or above the national minimum wage), as this is my best proxy for earnings in the benefit-level dataset. I find that the share of new recipients receiving above the minimum wage falls by 2.9 percent. This is consistent with the marginal claimant being a worker with relatively lower contributions to social security, such that they qualify only to lower pensions.

Table 3 contains the targeting estimates separated by each worker status at affiliation to social security. It provides a more nuanced view on the effects of the policy change on the targeting efficiency of social insurance. Among formal workers, I find that take-up only increases for those who seem relatively needier in terms of severity of disability or illness, as well as benefit size. In comparison, among self-employed workers, I find that recipients with low

[8] I construct the severity categories using the granular ICD code information from the benefit-level dataset. First, I create a severity index. Using data on deaths by ICD chapter (21 categories) at the municipality–year level, I measure morbidity-based severity as the share of deaths attributable to each ICD chapter within a 5-year rolling window for each municipality. I then combine this local severity index with the ICD composition of social benefits to compute an average severity measure for each municipality-year. Finally, I create categories of low, medium, and high severity based on the quantiles of the severity index in the full dataset, and then I aggragate to the municipality-year level.

Table 3: Estimates of the Effect on Applicant Characteristics by Worker Affiliation – Number of New Recipients (log)

		Formal worker		Informal/Rural worker		Self-employed		Not employed	
		Est.	SE	Est.	SE	Est.	SE	Est.	SE
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Pension type									
Old-age pension		0.034	(0.032)	0.001	(0.023)	0.118***	(0.024)	0.155***	(0.031)
Survivors insurance		0.002	(0.023)	0.014	(0.016)	0.05*	(0.028)	0.076**	(0.03)
Disability insurance		0	(0.033)	0.048	(0.033)	0.065*	(0.036)	−0.017	(0.031)
Sickness-leave		0.033**	(0.017)	0.087***	(0.026)	0.069***	(0.026)	0.079***	(0.022)
Maternity-leave		0.066**	(0.032)	0.135***	(0.027)	0.104***	(0.031)	0.041	(0.033)
Means-tested pension		−0.123	(0.093)	−0.038	(0.119)	−0.026	(0.089)	0.081***	(0.018)
Panel B. Age and years of contribution to social security									
Age	Contribution								
	$\leqslant 1$	0.023	(0.027)	0.079***	(0.025)	0.041	(0.036)	0.085***	(0.02)
< 30	2-10	0.041*	(0.024)	0.142***	(0.038)	0.092***	(0.035)	0.058**	(0.029)
	> 10	−0.014	(0.042)	0.063	(0.055)	−0.005	(0.07)	0.03	(0.049)
	$\leqslant 1$	0.031	(0.027)	0.086***	(0.021)	0.067**	(0.032)	0.073***	(0.022)
30-55	2-10	0.029	(0.02)	0.105***	(0.03)	0.098***	(0.027)	0.079***	(0.028)
	> 10	0.055**	(0.025)	0.117***	(0.032)	0.122***	(0.028)	0.122***	(0.031)
	$\leqslant 1$	−0.028	(0.03)	−0.018	(0.021)	0.03	(0.031)	0.084***	(0.019)
> 55	2-10	0.021	(0.031)	0.06**	(0.03)	0.129***	(0.036)	0.051	(0.034)
	> 10	0.004	(0.026)	0.052**	(0.023)	0.108***	(0.024)	0.167***	(0.031)
Panel C. Severity of illness / disability									
Low		0.005	(0.02)	0.069**	(0.029)	0.102***	(0.034)	0.109***	(0.025)
Medium		0.054	(0.033)	0.102**	(0.041)	0.071*	(0.041)	0.065**	(0.031)
High		0.059***	(0.021)	0.128***	(0.029)	0.105***	(0.031)	0.139***	(0.028)
Panel D. Benefit size									
1 minimum wage		0.045**	(0.018)	0.021	(0.016)	0.08***	(0.023)	0.097***	(0.016)
> 1 minimum wage		0.006	(0.017)	0.126***	(0.038)	0.065***	(0.022)	0.055***	(0.019)

Note: This table reports average treatment effects on the treated (ATTs) estimated using the same difference-in-differences strategy as in the event-study figures above, based on equation (1) and the heterogeneity-robust estimator of Sun and Abraham (2021). For each outcome, I estimate the full event-study specification and report the ATT as the average of the post-treatment coefficients. Each column corresponds to a separate regression for the log number of new recipients in the indicated category and worker-affiliation group. Standard errors, reported in parentheses, are clustered at the municipality level. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined controls, and are estimated on the main sample. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

severity rise as much as those with high severity. Also, these estimates also suggest a moderate increase of 7.9 percent and 8.5 percent among workers who are relatively young and with relatively low contributions (aged less than 30 years old and with at most 1 year of contribution) among informal, rural, and unemployed workers at affiliation.

Overall, the targeting evidence is suggestive of improved access for relatively disadvantaged and underserved groups. The strongest increases occur among the self-employed, the not-employed, claimants with more severe health conditions, and recipients of maternity-leave and means-tested benefits. However, this does not mean that the self-targeting view is without merit, since I can only identify the effect on the marginal beneficiary, while the self-targeting view suggests we should look into the *average* beneficiary. Still, in this setting, administrative barriers appear to deter many individuals who are plausibly among the groups most in need of timely access to benefits.

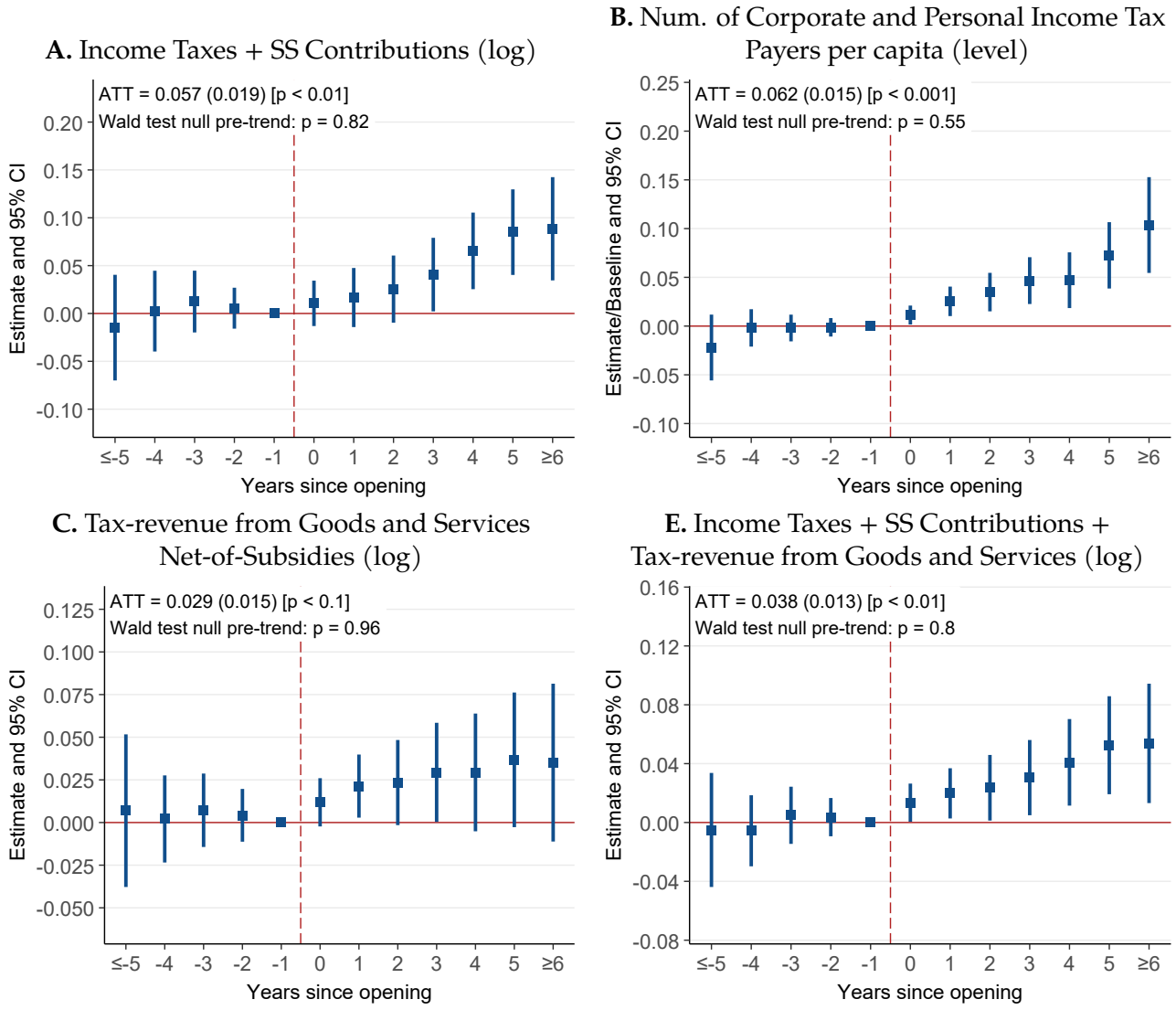
Table 4: Heterogeneity: Effect on Applicant Characteristics by Baseline Economic Development – Number of New Recipients (log)

	Lower GDP/Pop			Higher GDP/Pop		
	Point estimate	Standard error	Avg. baseline counts	Point estimate	Standard error	Avg. baseline counts
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Pension type						
Old-age pension	0.03**	(0.015)	163.32	0.042**	(0.019)	150.97
Survivors insurance	0.021*	(0.012)	67.65	0.027**	(0.012)	70.86
Disability insurance	0.02	(0.035)	15.8	0.004	(0.03)	35.64
Sickness-leave	0.077***	(0.021)	170.29	0.04**	(0.016)	391.74
Maternity-leave	0.029	(0.029)	179.49	0.143***	(0.029)	84.76
Means-tested pension	0.092***	(0.026)	56.89	0.056**	(0.023)	60.11
Panel B. Worker type						
Formal	0.048**	(0.023)	72.02	0.025*	(0.015)	318.33
Rural / Informal	0.002	(0.016)	452.36	0.046**	(0.022)	184.22
Self-employed	0.092***	(0.027)	24.42	0.018	(0.021)	109.01
Not employed	0.079***	(0.02)	103.96	0.108***	(0.017)	181.97
Panel C. Age at claiming						
< 30	0.041**	(0.018)	198.92	0.091***	(0.018)	179.57
30-54	0.06***	(0.016)	208.45	0.069***	(0.015)	352.69
$\geqslant 55$	0.03***	(0.01)	245.82	0.043***	(0.014)	261.17
Panel D. Years of contribution to social security						
$\leqslant 1$	0.032***	(0.012)	363.02	0.057***	(0.015)	287.56
2-9	0.082***	(0.022)	111.12	0.032*	(0.017)	258.68
$\geqslant 10$	0.068***	(0.015)	179.3	0.079***	(0.015)	247.83
Panel E. Severity of illness / disability						
Low	0.073***	(0.025)	66.44	0.039**	(0.019)	147.53
Medium	0.106***	(0.039)	57.07	0.025	(0.036)	130.04
High	0.093***	(0.025)	71.43	0.099***	(0.02)	134.74
Panel F. Benefit size						
1 minimum wage	0.021*	(0.011)	584.97	0.066***	(0.014)	446.53
$>$ 1 minimum wage	0.047**	(0.021)	68.48	0.018	(0.014)	347.53
Num. new recipients	0.04***	(0.011)	653.45	0.067***	(0.013)	794.07

Note: This table reports average treatment effects on the treated (ATTs) estimated separately for treated municipalities below and above the median of baseline GDP per capita in the main sample. Estimates are obtained using the same difference-in-differences strategy as in the event-study figures above, based on equation (1) and the heterogeneity-robust estimator of Sun and Abraham (2021). For each outcome, I estimate the full event-study specification within each subsample and report the ATT as the average of the post-treatment coefficients. Columns 1 and 4 report point estimates, Columns 2 and 5 report standard errors clustered at the municipality level, and Columns 3 and 6 report the average baseline level of the outcome among treated municipalities in the corresponding subsample. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined controls. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Heterogeneity by Baseline Economic Development — Table 4 presents the targeting results by baseline economic development. I do not find meaningful heterogeneity in this approach. In poorer treated municipalities, the increase in claims is relatively stronger among self-employed workers and means-tested pension recipients, while richer municipalities exhibit larger increases in some categories of formal and maternity-related claims. Still, the broader interpretation is the same in both settings: reducing local delivery frictions seems to induce relatively needier applicants to receive social insurance benefits.

Figure 10: Effect on Tax Revenue from the Municipality

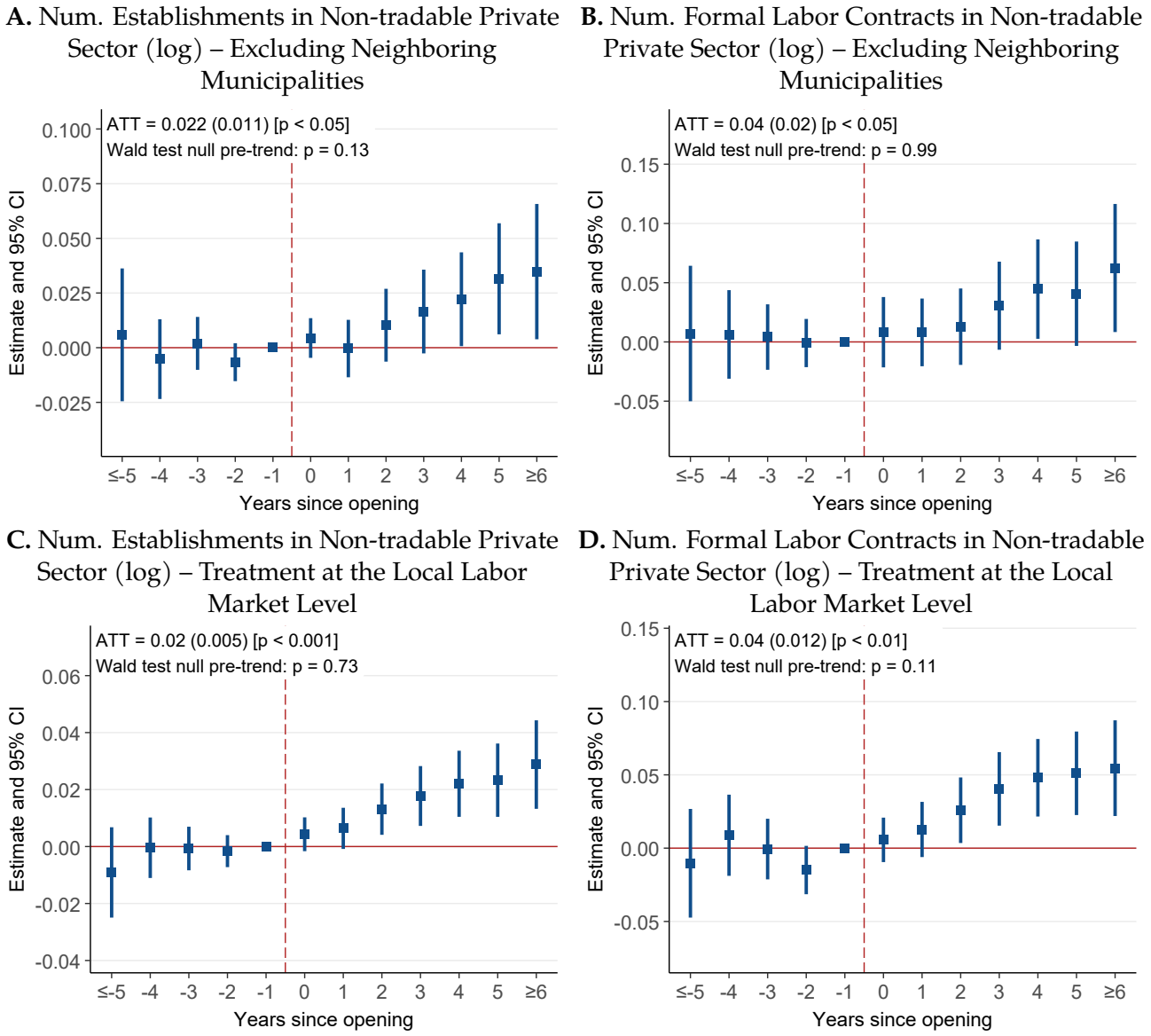


Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

4.3. Effect on the Local Economy

Opening a social security office does not directly provide productive infrastructure or local public goods. It should also not be viewed as a direct increase in the fiscal capacity of the municipality since these offices only manage social security claims and do not monitor contributions nor income tax reporting. The primary direct effect of increased delivery capacity is to reduce barriers to claiming benefits, thereby increasing transfers to local residents and raising local disposable income. This channel is quantitatively important in my setting: before the expansion, social security transfers accounted for 13.5 percent of GDP in treated municipalities on average, and office openings raise those transfers by 13.2 percent after 6 or more years. The natural question is therefore whether this increase in transfer income also generates broader lo-

Figure 11: Effect on Labor Market Outcomes

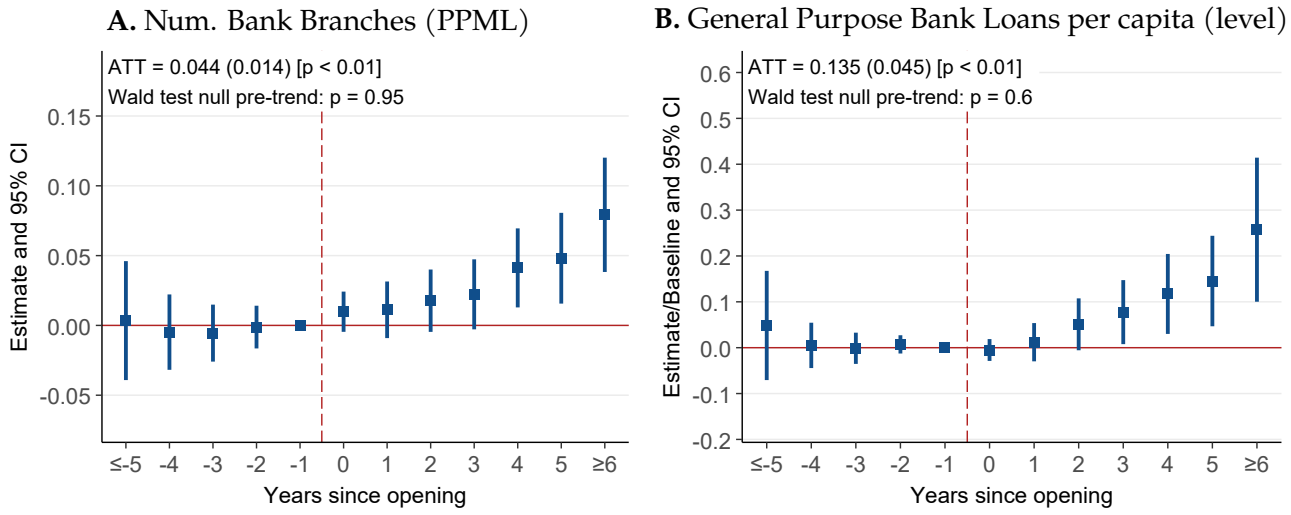


Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

cal economic effects. I look primarily into three dimensions of the local economy: tax revenue, formal labor markets, and local access to finance.

The tax revenue results are presented in Figure 10, which shows that office openings increase total tax collection by 3.8 percent, raise the number of corporate and personal income taxpayers per capita by 6.2 percent, and increase the sum of income taxes and social security contributions by 5.7 percent. Tax revenue from goods and services also rises by 2.9 percent. Taken together, these findings suggest that the transfer expansion stimulated taxable economic activity rather than merely increasing passive household income. That is, an increase to the delivery capacity of social insurance had an indirect effect of increasing the fiscal capacity of the government as well.

Figure 12: Effect on Local Access to Finance



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

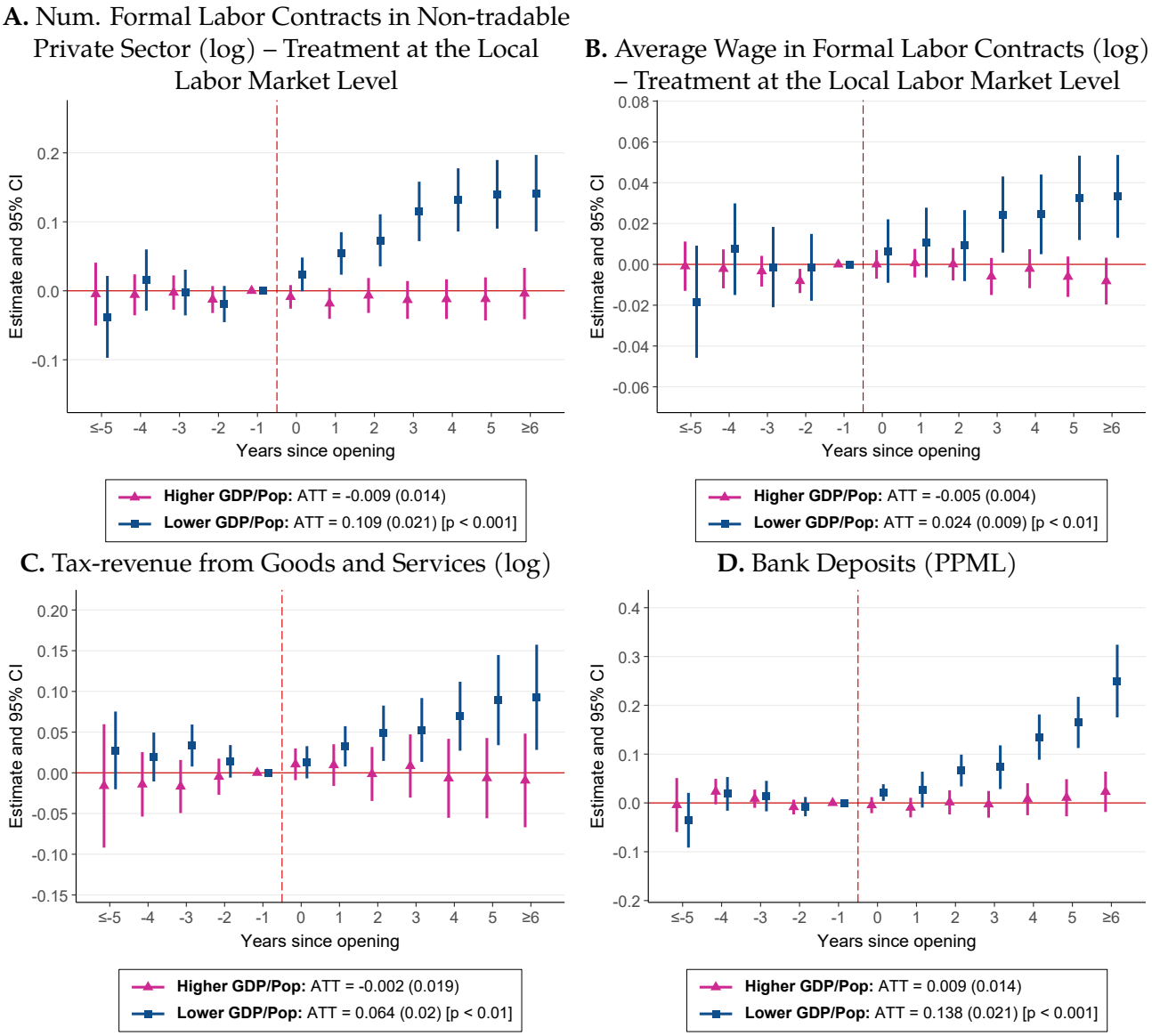
The labor market estimates are depicted in Figure 11 and point in the same direction. Since workers and firms may operate across municipal boundaries, a municipality-level treatment definition may understate labor market effects, such that in Panels A and B I exclude never treated municipalities in treated microregions, whereas in Panels C and D I redefine treatment at the microregion level to capture effects within local labor markets. Both approaches yield the same results. Office openings increase the number of establishments in the non-tradable private sector by 2.0 percent and the number of formal labor contracts in that sector by 4.0 percent. In contrast, Appendix Figure A8 shows no meaningful effect on establishments or formal labor contracts in the tradable private sector. This asymmetry is what would be expected under a local demand-driven growth channel where higher transfer income raises spending on goods and services produced locally, while leaving tradable activity largely unchanged.

Figure 12 presents the evidence for the local access to finance and shows that office openings raise the number of bank branches operating in the municipality by 4.4 percent and increase consumer loans per capita by 13.5 percent. These effects are also consistent with a broader local expansion in economic activity: higher and more stable transfer income may increase households' demand for financial services while also making these municipalities more attractive to financial intermediaries.

Heterogeneity by Baseline Economic Development — The effect on local economic activity is highly heterogeneous by baseline economic development with all effects being concentrated in poorer places. Figure 9 shows that poorer and richer municipalities experience very similar percentage increases in social security transfers, such that heterogeneity in the effect on transfers cannot explain the heterogeneity in these downstream outcomes.

Figure 13 shows that, in municipalities below the median of baseline GDP per capita, of-

Figure 13: Heterogeneity: Effect on the Local Economy by ex-ante GDP per capita



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

fice openings increase formal labor contracts in the non-tradable private sector by 10.9 percent, raise average wages by 2.4 percent, increase tax revenue from goods and services by 6.4 percent, and increase bank deposits by 13.8 percent. Appendix Figure A3 similarly shows larger effects in poorer municipalities on total tax revenue, non-tradable establishments, bank branches, and bank loans. In contrast, the corresponding estimates for richer municipalities are small and statistically indistinguishable from zero across all outcomes. This pattern reinforces the demand-driven interpretation, since this mechanism would operate more strongly where local economies have greater slack and where additional income is more likely to translate into spending on local non-tradable goods and services.

In sum, these findings suggest that the fiscal externality of reducing take-up frictions can-

not be assessed in partial equilibrium alone (which would likely yield negative estimates due to income and substitution effects of labor supply). I find that expanding delivery capacity not only increases transfers, but also appears to stimulate local labor markets, tax collection, and financial development, but only in poorer municipalities. As a result, the net fiscal cost of reducing administrative barriers may be substantially smaller than what one would infer from the direct increase in benefit payments alone.

4.4. Robustness Checks

I provide several robustness checks to the main results to show that these are not driven by a particular estimator, by the definiton of the control group, or by the choice of population window used to construct the sample. Across a broad set of alternative specifications, the core patterns remain intact: office openings increase the number of new recipients, social security transfers, tax revenue, non-tradable-sector activity, and local financial access, while the estimated increase in social security contributions remains positive though somewhat less precise in some specifications.

Table 5 reports three sets of robustness exercises. Panel A examines alternative estimation approaches. Relative to the main specification, the two-way fixed effects estimator yields somewhat smaller coefficients, but the qualitative conclusions are unchanged: the effect remains positive for new recipients, transfers, contributions, tax revenue, establishments, labor contracts, and bank branches. For example, the estimated effect on new recipients falls from 5.2 percent in the baseline to 4.6 percent using the TWFE estimator, while the effect on transfers falls from 8.8 percent to 6.6 percent. The effect on social security contributions also remains positive at 3.0 percent.

A possible concern one might have is that treated and never treated municipalities are in very different microregions, where treated microregions were on a differential growth pattern. To alleviate this concern, I present an alternative specification in Table 5 Panel A where I restrict the sample to treated microregions only. I also expressing outcomes in per-capita terms, and remove the time-varying population controls. All of these robustness checks yield the same results. In fact, the specification without population controls delivers estimates that are, somewhat larger for several outcomes including the number of new recipients.

Next, Table 5 Panel B addresses concerns about spillovers across nearby municipalities. Because residents may work and consume goods and services outside of their municipality of residence, a local office opening could affect neighboring areas and thereby contaminate the comparison group. Also, neighboring municipalities might also be affected by a reduction in take-up frictions, as the new office could be closer and less congested than the previously closest office. To assess this issue, I first redefine treatment at the local labor market level, which is the specification I used above for the labor market outcomes. Under this definition, only the effects on social security transfers (apart from the labor market outcomes from above) remain positive, yet much smaller than the main specification. I then exclude never-treated municipalities located inside treated microregions. The resulting estimates are slightly smaller for some outcomes, but the increase in new recipients, transfers, and the broader tax measure

remains economically meaningful. These results suggest that the baseline results are not driven by negative spillovers onto neighboring control municipalities. If anything, there are positive spillovers to labor market outcomes and social security transfers.

Finally, Table 5 Panel C shows that the results are also robust to restricting the main sample to municipalities whose population in 2007 is in a given window around the 20,000-resident cutoff that motivated the expansion. Using broader and narrower windows in 2007 population yields very similar estimates for the main outcomes. For the number of new recipients, the estimated effect ranges narrowly from 5.2 to 5.6 percent, and for social security transfers, from 8.5 to 10.3 percent. The effect on non-tradable labor contracts at the local labor market level also remains stable, ranging from 3.9 to 4.6 percent, while the effect on bank branches remains around 3.8 to 4.2 percent. Appendix Figure A9 complements these results by plotting the full event-study coefficients for alternative population windows. The dynamic treatment effects are visually similar across window choices, with no systematic evidence of differential pre-trends and with post-opening effects that closely track the baseline estimates for new recipients, transfers, contributions, tax revenue, labor contracts, establishments, and bank branches. This is especially reassuring because it indicates that the results do not depend on a specific sample definition around the population threshold, but instead reflect a broader and stable relationship between office openings and local outcomes.

Taken together, the robustness exercises strengthen the interpretation that the estimated effects capture the causal impact of expanding the local delivery capacity of social insurance. Alternative estimators, spillover adjustments, and sample windows all point to the same conclusion: opening a social security office increases take-up and transfers, and these increases are accompanied by broader effects on local economic activity.

Table 5: Robustness Checks and Spillovers for the Main Results

	Num. new recipients (log)	SS transfers (log)	SS contributions (log)	Income taxes + SS contributions (log)	Tax rev. goods /serv. + Inc. tax + SS contrib. (log)	Establishments in non-trade. priv. sector in LLM (log)	Labor contracts in non-trade. priv. sector in LLM (log)	Num. bank branches (PPML)	Num. munic.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Main specification	0.052***	0.088***	0.046**	0.057***	0.038***	0.02***	0.04***	0.044***	4539
s.e. munic. level	(0.009)	(0.013)	(0.02)	(0.019)	(0.013)	(0.005)	(0.012)	(0.014)	
s.e. microreg. level	(0.009)	(0.013)	(0.018)	(0.017)	(0.013)	(0.008)	(0.013)	(0.016)	
***Panel A.* Robustness to specification**									
TWFE specification	0.046***	0.066***	0.03*	0.037**	0.03**	0.011	0.018	0.033**	4539
	(0.01)	(0.015)	(0.018)	(0.018)	(0.013)	(0.009)	(0.017)	(0.013)	
Keeping only	0.048***	0.068***	0.062***	0.069***	0.034**	0.071**	0.019	0.053***	4539
treated microregions	(0.01)	(0.013)	(0.021)	(0.02)	(0.014)	(0.034)	(0.07)	(0.014)	
Outcomes in	0.045***	0.081***	0.039*	0.05***	0.031**	0.013**	0.033***		4539
per-capita terms	(0.009)	(0.013)	(0.02)	(0.02)	(0.014)	(0.006)	(0.012)		
No population	0.08***	0.021*	0.014	0.019	0.015	0.025***	0.027**	0.075***	4539
controls $\mathbf{X}_{m,t}$	(0.008)	(0.011)	(0.017)	(0.016)	(0.011)	(0.005)	(0.012)	(0.013)	
***Panel B.* Spillovers**									
Treatment defined	0.002	0.025**	−0.018	−0.014	0.002			0.004	4539
at LLM-level	(0.005)	(0.012)	(0.012)	(0.011)	(0.009)			(0.012)	
Excl. never treated	0.04***	0.061***	−0.02	0	0.036**			0.028	4539
in treated microreg.	(0.011)	(0.017)	(0.026)	(0.025)	(0.017)			(0.018)	
***Panel C.* Robustness to population bandwidth in 2007**									
5k-50k	0.053***	0.09***	0.035	0.048**	0.042***	0.015**	0.046***	0.042***	3186
	(0.01)	(0.013)	(0.022)	(0.021)	(0.015)	(0.006)	(0.013)	(0.016)	
2k-60k	0.056***	0.103***	0.041**	0.052***	0.039***	0.021***	0.039***	0.038**	4428
	(0.009)	(0.013)	(0.021)	(0.02)	(0.014)	(0.005)	(0.012)	(0.015)	
8k-40k	0.052***	0.085***	0.038	0.046**	0.036**	0.017**	0.044***	0.037**	2248
	(0.01)	(0.014)	(0.024)	(0.023)	(0.016)	(0.007)	(0.014)	(0.017)	
4k-55k	0.056***	0.093***	0.043**	0.054***	0.04***	0.014**	0.046***	0.038**	3611
	(0.009)	(0.013)	(0.021)	(0.02)	(0.014)	(0.006)	(0.013)	(0.015)	

Note: This table reports robustness checks for the main estimates. The first row reproduces the baseline estimates from the main event-study specification using the Sun and Abraham (2021) estimator on the main sample of municipalities without an INSS office by 2008. Columns 6 and 7 are estimated at the local labor market level, while all other outcomes are at the municipality level. Panel A varies the specification: TWFE, restricting to treated microregions, using per-capita outcomes, and omitting the population controls $\mathbf{X}_{m,t}$. Panel B examines spillovers by redefining treatment at the local labor market level and by excluding never-treated municipalities in treated microregions. Panel C changes the 2007 population window used to define the sample. Standard errors are in parentheses; for the baseline row, I report clustering at both the municipality and microregion levels. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5. Implications

This section examines the broader implications of the expansion in social insurance delivery capacity. I first study whether the increase in social security contributions documented in Section 4 reflects a higher willingness-to-contribute to social insurance, rather than only a by-product of the local economic expansion induced by higher transfers. I then estimate the fiscal externalities generated by the additional transfers as the elasticity of taxes with respect to transfers. Finally, I combine the evidence on take-up, targeting, and fiscal externalities to quantify the welfare effects of the policy through the marginal value of public funds (MVPF) framework of Finkelstein and Notowidigdo (2019).

5.1. Does the Willingness-to-pay for Social Insurance Increase?

A natural interpretation of the positive effect on social security contributions is that reducing claiming frictions increases the perceived value of social insurance, thereby raising the willingness-to-contribute among workers for whom contributions are *de facto* voluntary. This is especially important in the Brazilian context, since 45.4 percent of the workforce was either informal or self-employed, yet only 22.6 percent of these workers contributed to social security in 2010. In principle, low enrollment among such workers may reflect standard liquidity constraints or low willingness-to-pay for insurance. But it may also reflect the fact that workers anticipate substantial barriers when they later need to claim benefits. If so, improving delivery capacity may affect not only the take-up of benefits among current contributors, but also the ex ante demand for insurance itself.

Interpreting the reduced-form effect on social security contributions is not straightforward, however, because office openings also affect local economic activity. These general equilibrium effects could mechanically raise observed social security contributions even if workers' underlying willingness-to-contribute were unchanged. For example, if the office expansion stimulates local employment and shifts workers into formal jobs, contributions would increase because formal contributions are mandatory. More generally, a higher local tax base or higher earnings could raise contributions without requiring any change in insurance valuation.

Importantly, I cannot directly test from whom the increased contributions are coming from as I do not have individual-level data on social security contributions. This test would be the most appropriate to disentangle the mechanisms at play. Still, I provide suggestive evidence on whether the increase in contributions is fully explained by contemporaneous economic conditions, by re-estimating the effect of office openings on social security contributions while controlling for time-varying measures of local economic activity. Table 6 reports the results. The baseline estimate without contemporaneous controls is 0.046. Adding controls separately for formal labor contracts in the non-tradable private sector, formal labor contracts at the local labor market level, GDP estimate, population, or social security transfers leaves the estimated effect essentially unchanged, with point estimates ranging from 0.043 to 0.047. When I control for corporate and personal income taxes, the estimated effect equals 0.032 and is statistically significant at the 10 percent level. Likewise, controlling for all contemporaneous controls si-

Table 6: Estimate of the Effect of Openings on SS Contributions (log) with Contemporaneous Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.046**	0.044**	0.043**	0.043**	0.046**	0.047**	0.032*	0.032*
	(0.02)	(0.019)	(0.02)	(0.019)	(0.02)	(0.02)	(0.019)	(0.018)
log(Formal labor contracts		0.113***						0.058***
in non-tradable priv. sector)$_t$		(0.008)						(0.008)
log(Formal labor contracts			0.15***					0.07***
priv. sector in LLM)$_t$			(0.023)					(0.022)
log(GDP estimate)$_t$				0.3***				0.208***
				(0.017)				(0.014)
log(Population estimate)$_t$					0.061			−0.019
					(0.064)			(0.056)
log(SS transfers)$_t$						−0.006		−0.021**
						(0.009)		(0.009)
log(Income taxes)$_t$							0.15***	0.127***
							(0.006)	(0.006)
Observations	67892	67731	67892	67891	67892	67714	67038	66832

Note: This table re-estimates the effect of office openings on social security contributions with contemporaneous controls for local economic activity. All specifications include municipality and state-by-year fixed effects and the baseline controls. Standard errors are clustered by municipality.

multaneously, the estimated effect still equals 0.032 and remains statistically significant at the 10 percent level. Appendix Table A5 reports the same exercise using lagged controls, which yields the same results.

These results suggest that the rise in social security contributions is not solely a mechanical consequence of contemporaneous changes in local economic activity. Even after flexibly accounting for the main channels through which the increased transfers might affect the local economy, office openings still increase contributions by residents of treated municipalities. I interpret this finding as suggestive evidence that reducing take-up frictions increases the willingness-to-contribute to social insurance, since it is difficult to reconcile the persistence of the contribution effect with a purely mechanical formalization channel.

This interpretation is also consistent with the mechanisms documented earlier. Office openings reduce travel time to the claiming office, shorten processing times, and lower judicialization for some benefit categories. These changes make benefits more accessible and more predictable, increasing the expected value of contributing to the system. In that sense, delivery capacity affects insurance demand not only through eligibility and benefit generosity, but also through the enforceability and credibility of the insurance contract.

5.2. Tax Revenue Multipliers

The reduced-form evidence in Section 4 indicates that opening a social security office raises several forms of tax revenue. Income taxes and social security contributions increase by 5.7 percent, the number of income taxpayers per capita rises by 6.2 percent, taxes on goods and services increase by 2.9 percent, and overall tax collection rises by 3.8 percent. These effects imply that the fiscal externality of expanding delivery capacity may be substantially smaller than what would be inferred from transfer increases alone. To quantify this channel, I estimate the

elasticity of tax revenue with respect to social security transfers using two stages least squares. Specifically, the first-stage and second-stage equations are given by (2) and (3) below:

$$\log(\text{SS Transfers}_{m,t}) = \alpha_m + \delta_{t,s(m)} + \beta \cdot \mathbb{1}_{(t-e_m \geqslant 0)} + \lambda' X_{m,t} + u_{m,t} \tag{2}$$

$$\log(Y_{m,t}) = \alpha_m + \delta_{t,s(m)} + \mu \cdot \log(\widehat{\text{SS Transfers}}_{m,t}) + \gamma' X_{m,t} + \nu_{m,t} \tag{3}$$

where all elements are unchanged relative to the baseline difference-in-differences specification, with the exception that I group all post-opening years into a single treatment dummy $\mathbb{1}_{(t-e_m \geqslant 0)}$. The identifying assumptions for this instrumental variables strategy are relevance and exclusion. First, the opening of a social security office should increase social security transfers, which we have already concluded to be true from Section 4. Second, the office openings should affect local tax revenue only through their effect on social security transfers. This assumption seems plausible in this setting given that the INSS offices do not provide productive infrastructure or direct inputs into local production, do not employ many individuals, and do not enforce tax collection of any sort. Rather, these offices only process and manage benefit claims. A possible violation would occur if the opening of a social security office was inter-

Table 7: Tax Revenue Elasticities with respect to Social Security Transfers and Multipliers

	Post-period	SS contributions	Income taxes + SS contrib.	Tax rev. goods/ serv. + Inc. taxes + SS contrib.	First-stage F-statistic	N. Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
$\overline{\left(\frac{\text{Tax}}{\text{Transfers}}\right)}$		0.36	0.47	1.567		
$\frac{\text{d log Tax}}{\text{d log Transfers}}$	All	0.457	0.56*	0.451**	50.429	66914
		(0.292)	(0.296)	(0.217)		
	2 years	0.23	0.264	0.281	17.972	63162
		(0.28)	(0.272)	(0.196)		
	4 years	0.462**	0.562**	0.392**	36.631	63149
		(0.228)	(0.23)	(0.172)		
	6+ years	0.43**	0.533**	0.396**	97.474	64239
		(0.217)	(0.222)	(0.169)		
$\frac{\Delta\text{Tax}}{\Delta\text{Transfers}}$	All	0.164	0.263*	0.706**		
		(0.105)	(0.139)	(0.34)		
	2 years	0.083	0.124	0.44		
		(0.101)	(0.128)	(0.307)		
	4 years	0.166**	0.264**	0.614**		
		(0.082)	(0.108)	(0.27)		
	6+ years	0.155**	0.25**	0.621**		
		(0.078)	(0.104)	(0.265)		

Note: This table reports IV estimates of tax elasticities with respect to social security transfers and the implied tax-revenue multipliers. Log transfers are instrumented with an indicator for the post-opening period after a new social security office opens."All", "2 years", "4 years", and "6+ years" denote the post-opening period that is being included in the estimating sample for treated municipalities, while for never treated municipalities all periods are always included. All regressions include municipality and state-by-year fixed effects and the baseline controls; standard errors in parentheses are clustered by municipality.

preted as a signal that the government would provide additional infrastructure to that municipality. This violation of the exclusion seems unlikely given that the INSS operated the expansion independently, and to the best of my knowledge there were no other federal or municipal policies that followed the field office coverage expansion.

Table 7 reports the tax revenue elasticity results. In the full post-treatment sample, the elasticity of income taxes plus social security contributions with respect to transfers is 0.457, and the elasticity of taxes on goods and services is 0.560. The implied elasticity for total tax revenue is 0.451, with a first-stage F-statistic of 50.4. Focusing on longer-run effects, the elasticity of total tax revenue is 0.392 four or more years after opening and 0.396 six or more years after opening. Thus, a 10 percent increase in social security transfers raises total tax revenue by about 4 percent in the medium run.

To map these elasticities into fiscal multipliers which are informative to the MVPF implementation below, I multiply the elasticities by the average ratio of taxes to transfers in the baseline of treated municipalities. This yields a tax-revenue multiplier of 0.706 in the full post-treatment sample, 0.614 four or more years after opening, and 0.621 six or more years after opening. I use the six-year estimate as my preferred benchmark because the corresponding first-stage F-statistic of 97.474 in this specification is the highest. The resulting multiplier implies that each additional R$1.00 transferred through higher social security take-up generates approximately R$0.62 back in additional tax revenue. This estimate is economically large and broadly consistent with the evidence in Gerard et al. (2025), who study the local effects of conditional cash transfers in Brazil and estimate a tax-revenue multiplier of 0.58. The similarity is notable because the policy variation here comes not from an increase in benefit eligibility, but from a reduction in administrative barriers that raises claims and transfers. The results therefore suggest that, in local economies with slack, the fiscal externality of reducing take-up frictions can be substantial because the induced transfers stimulate economic activity.

The multiplier is also heterogeneous across municipalities. Appendix Table A6 shows that elasticities are substantially larger in municipalities with lower baseline GDP per capita than in richer municipalities. This pattern mirrors the heterogeneity in Section 4, where poorer municipalities exhibit strong effects on non-tradable employment, wages, tax revenue, and bank deposits, whereas richer municipalities show little local economic response despite similar increases in transfers. Together, these findings support a demand-driven interpretation in which transfer-induced spending stimulates local economic activity only where demand is likely to be constrained.

5.3. Marginal Value of Public Funds

I next turn to the welfare consequences of expanding delivery capacity of social insurance. I implement the MVPF framework of Finkelstein and Notowidigdo (2019)[9], while extending the fiscal externalities to allow for general equilibrium effects. The main advantage of this framework is that it makes transparent how welfare depends on the targeting of marginal applicants,

[9] Specifically, I use the MVPF for the combined intervention that provides information and reduces application costs.

the value of benefits to those applicants, the resource costs of delivering benefits, and the fiscal externalities generated by the policy.

I allow welfare effects to differ across worker types at affiliation and across pension types. This is important because the empirical results in Section 4 show strong heterogeneity in who is induced to claim: effects are larger among the self-employed and those not employed at affiliation than among formal workers, and they are especially pronounced for maternity-leave, means-tested pensions, and high-severity disability-related claims. Accordingly, I index applicants by worker type $i \in \{F, I, S, U\}$ and pension type $p \in P$, and I allow each cell to have its own number of inframarginal recipients, marginal response to treatment, average benefit level, application-cost reduction, and misperception of benefits.

In the neoclassical version of the model, marginal applicants are assumed to correctly perceive the value of claiming benefits, so the welfare gain from additional take-up comes only through the reduction in costs of applying. In the behavioral version, marginal applicants may understate the value of claiming because they misperceive their eligibility or the expected benefit stream. Under this interpretation, reducing administrative barriers can increase welfare both by lowering application costs and by allowing individuals to access benefits they previously undervalued. The empirical motivation for allowing such misperceptions is strong in this setting since social insurance is contributory and thus failing to claim in this setting could possibly reflect mistakes.

The MVPF is given by equation (4):

$$\text{MVPF} = \frac{\sum_{i \in \{F,I,S,U\}} \sum_{p \in \mathcal{P}} \left[\overbrace{-\varepsilon_{ip} \bar{B}_{ip} \frac{dN_{ip}}{dT}}^{\uparrow \text{Misperceived benefits for marg. recipients}} - \overbrace{\left(N_{ip} + \frac{dN_{ip}}{dT} \right) \frac{dc_i}{dT}}^{\downarrow \text{Application costs for marg. and inframarg. recipients}} \right]}{\sum_{i \in \{F,I,S,U\}} \sum_{p \in \mathcal{P}} \left[\underbrace{\left(\bar{B}_{ip} + g \right) \frac{dN_{ip}}{dT}}_{\substack{\uparrow \text{Transfers and} \\ \uparrow \text{Processing costs}}} - \underbrace{\bar{B}_{ip} \frac{\Delta \text{Taxes}}{\Delta \text{Transf.}} \frac{dN_{ip}}{dT}}_{\substack{\uparrow \text{Tax collection} \\ \text{in GE}}} \right]} \tag{4}$$

To quantify welfare, I combine several ingredients. First, I use the treatment effects by worker type and pension type to estimate the number of marginal recipients dN_{ip}/dT induced by office openings, as well as the number of recipients who are inframarginal N_{ip}. Second, I calculate average benefit values $\bar{B}_{ip}$ using the observed benefit composition and, for long-duration benefits such as old-age, survivors, disability, and means-tested pensions, I calculate the discounted sum of benefits using life expectancy estimates from the IBGE and an interest rate associated with an yearly discount factor of 0.995. Third, I measure application-cost reductions dc_i/dT using the observed decline in travel time and the reduction in claim-processing delays, combined with assumptions on time costs by worker type as in Finkelstein and Notowidigdo (2019). Fourth, I calibrate misperceptions ε_{ip} for each group, where I also follow the approach from Finkelstein and Notowidigdo (2019). Fifth, I incorporate the fiscal cost of higher transfers and administrative processing g (i.e. the costs for the INSS to assess if each application is deemed eligible or not). Finally, I subtract the general equilibrium fiscal externality using the

Table 8: MVPF Estimates under Different Assumptions

	Neoclassical model		Behavioral model	
	$\frac{\Delta Tax}{\Delta Transf.} = 0$	$\frac{\Delta Tax}{\Delta Transf.} \geqslant 0$	$\frac{\Delta Tax}{\Delta Transf.} = 0$	$\frac{\Delta Tax}{\Delta Transf.} \geqslant 0$
Panel A. Main estimates				
	0.28	0.68	1.15	2.76
Panel B. Estimates by baseline economic development				
Low ex-ante GDP per capita	0.32	0.44	1.23	1.7
High ex-ante GDP per capita	0.39	0.39	1.29	1.29

Note: This table reports estimated marginal values of public funds (MVPF) for office openings under alternative assumptions about misperceptions and general-equilibrium fiscal externalities. In the neoclassical model, misperceptions are set to zero; in the behavioral model, they are calibrated from application costs relative to benefit values. Columns with $\Delta Tax/\Delta Transf. \geqslant 0$ incorporate the estimated tax-revenue multiplier, while columns with $\Delta Tax/\Delta Transf. = 0$ shut down general-equilibrium fiscal effects. Panel B splits municipalities by baseline GDP per capita.

tax-revenue multiplier $\Delta Tax/\Delta Transfers$ estimated in Section 5.2.[10]

Table 8 presents the MVPF results. In the neoclassical model, the MVPF is 0.28 when general equilibrium tax effects are ignored and rises to 0.68 once the tax-revenue multiplier is incorporated. Thus, even after accounting for the positive fiscal externality, the policy remains below one in the absence of misperceptions, implying a net resource cost to the government larger than the welfare benefit to recipients under this benchmark. The conclusions change sharply in the behavioral model. When marginal applicants misperceive the value of claiming benefits, the MVPF is 1.15 even without general equilibrium fiscal externalities, and it rises to 2.76 after incorporating the tax-revenue multiplier. Under this interpretation, the delivery-capacity expansion is welfare enhancing, and the general equilibrium effects are quantitatively important since accounting for them more than doubles the estimated MVPF relative to the behavioral model without tax externalities.

These magnitudes imply that the welfare effect of reducing take-up frictions depends critically on local fiscal spillovers as well as whether workers are assumed to be behaviorally biased, as highlighted in Finkelstein and Notowidigdo (2019). In addition, the welfare gains are not uniform across municipalities with lower and higher baseline economic development, as

[10] I assume each worker type i has its own opportunity cost of time given by the daily wage of each group, as measured by the average wage in the 2010 Census for treated municipalities. I assume that total costs of application for cell (i, p) are a combination of travel costs for i, waiting costs for i, social security contribution costs for p, and benefit processing costs for (i, p). All of these calculations are very conservative. Otherwise, the implied misperceptions for all (i, p) would be close to -100%. In particular, the travel cost is calculated as a bus fare of R$2.70 plus twice the daily wage. The waiting cost is 1/4 the daily wage, where I assume 4 hours are spent inside the office. The social security contribution costs conservatively assumes that the worker has to pay additional contributions before qualifying for benefits. For old-age pensions, maternity leave, sickness leave, and disability insurance, I assume that the worker has to contribute 11% of the minimum wage for 12 months. Surviviors insurance and means-tested pensions are not triggered by additional contributions from the claimant, so I assume no further contributions for these pension types. Finally, the benefit processing cost assumes that, while the worker has already claimed a pension and is waiting for the decision by the INSS, she spends 1 hour every week dealing with bureaucracy, with the corresponding cost in terms of foregone labor income. For the change in application costs dc_i/dT, I assume only that it equals the (negative of the) travel cost calculated above. Misperceptions are given by ε_{ip} = total cost of applying for (i, p) -1. Finallt, the porcessing costs per application g are conservatively calibrated as the total number of pensions dispatched by INSS workers in 2009 divided by the total labor costs of the INSS in 2009, which equals R$ 457.00.

shown in Table 8 Panel B. In poorer municipalities, the neoclassical MVPF rises from 0.32 to 0.44 when tax externalities are included, while the behavioral MVPF rises from 1.23 to 1.70. In richer municipalities, the corresponding estimates are 0.39 in both neoclassical specifications and 1.29 in both behavioral specifications, since the tax revenue multiplier estimated for these municipalities is not statistically significant. This heterogeneity reinforces the idea that delivery-capacity expansions are especially valuable where both administrative barriers and local economic slack are high. It also shows the importance of considering the correlation between misperceptions and fiscal externalities. If general equilibrium effects are shut down, the MVPF of reducing take-up frictions is marginally higher for richer municipalities relative to poorer municipalities both in the neoclassical and behavioral models.[11]. In comparison, the MVPF of reducing take-up frictions is larger in poorer municipalities once we account for the general equilibrium effects.

6. Conclusion

This paper provides new evidence that the delivery capacity of the state is a central determinant of social insurance access and effectiveness. I show that the expansion of social security offices in Brazil increased take-up, reduced claiming frictions, and generated meaningful local economic spillovers, with especially large effects in less developed municipalities.

These results have two broader implications. First, I find that administrative barriers were screening out relatively needier applicants in the Brazilian setting. Second, because improved delivery capacity increased local economic activity and tax revenue in poorer municipalities, its welfare effects are considerably larger than a partial-equilibrium analysis would suggest.

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[11] This reflects the fact that residents in richer municipalities have a higher opportunity cost of time due to higher average wages, such that the estimated reduction in travel costs is higher in richer municipalities.

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A Appendix

Figure A1: Histogram of Population Size in 2007 for Municipalities without an Office by 2008

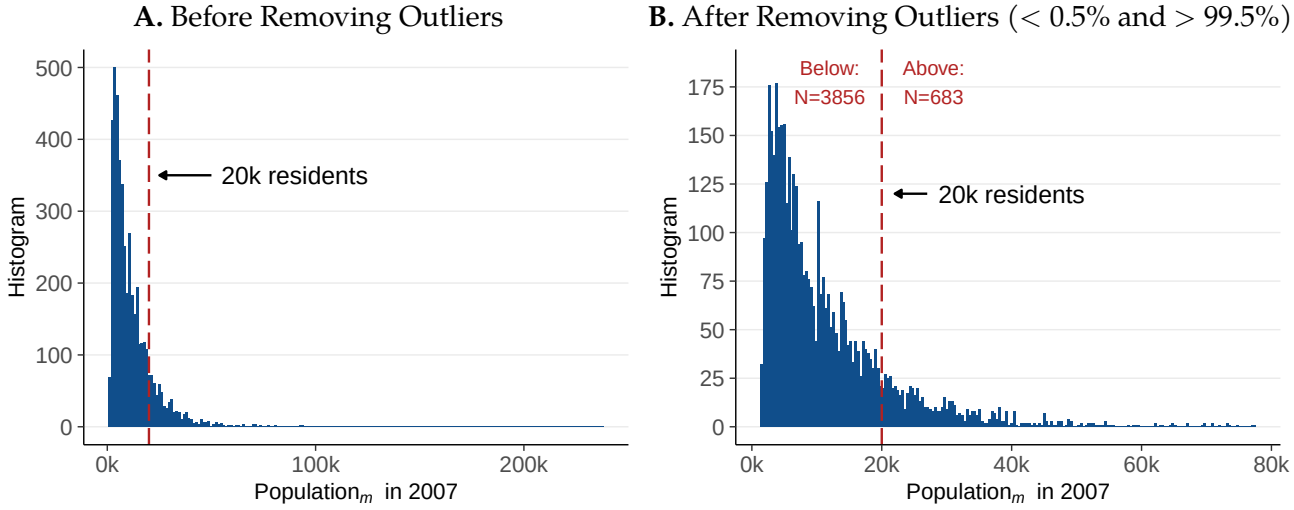


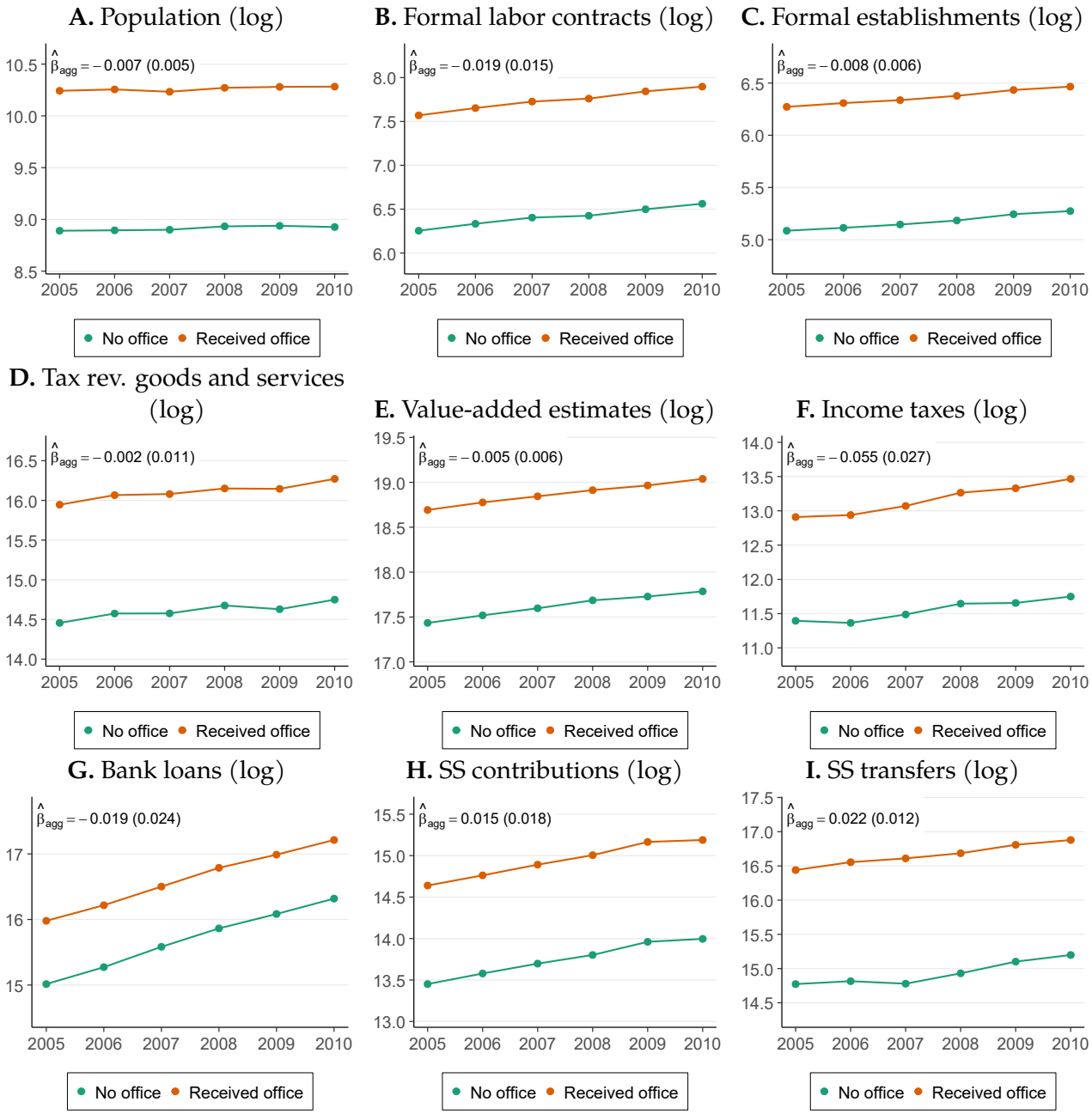
Table A1: Expanded Summary Statistics at the Municipality-level: All Municipalities, Main Sample, and Cutoff Sample

	All Municipalities		Main Sample		Cutoff Sample	
	No office	Office	No office	Office	No office	Office
	(1)	(2)	(3)	(4)	(5)	(6)
***Panel A.* Demography and the Local Economy (2008)**						
Population	10114.350	100874.367	9800.133	30705.192	12428.861	28741.702
GDP/Pop	9063.147	11401.371	9036.619	8105.560	8426.098	8191.945
Distance to state capital (hours)	4.596	4.082	4.608	4.578	4.671	4.609
Hours to closest office	0.815	0.356	0.816	1.038	0.865	1.033
INSS transfers/Pop	601.456	937.872	603.786	714.550	646.323	727.135
INSS transfers/GDP	0.095	0.126	0.095	0.135	0.111	0.137
Income tax collection/Pop	85.626	222.157	81.878	80.228	76.009	79.805
Income tax collection/GDP	0.006	0.013	0.005	0.007	0.006	0.006
Social security collection/Pop	221.261	369.718	220.372	212.328	203.760	210.912
Social security collection/GDP	0.025	0.029	0.025	0.023	0.024	0.023
Tax collection net-of-benefits/Pop	526.516	1502.655	514.305	497.371	490.327	487.543
FPM transfers/Pop	626.879	251.022	617.000	289.558	417.760	294.406
FPM transfers/GDP	0.105	0.040	0.104	0.059	0.084	0.060
PBF transfers/Pop	80.675	67.526	80.974	88.613	86.921	89.210
PBF transfers/GDP	0.019	0.014	0.019	0.022	0.022	0.022
Num. formal labor contracts/Pop	0.108	0.150	0.107	0.107	0.103	0.108
Num. formal establishments/Pop	0.032	0.036	0.032	0.027	0.029	0.028
% Any bank	0.491	0.966	0.493	0.917	0.605	0.912
Bank loans/Pop	798.581	2256.013	803.014	1237.333	982.328	1277.038
Bank deposits/Pop	666.913	2317.573	668.870	1181.561	811.125	1202.148
***Panel B.* Benefit recipients data (2008)**						
Num. new recipients/Pop	0.027	0.025	0.027	0.023	0.026	0.023
% New recipients old-age pension	0.245	0.196	0.245	0.230	0.240	0.230
% New recipients survivors insurance	0.098	0.094	0.098	0.098	0.098	0.098
% New recipients disability insurance	0.043	0.046	0.043	0.038	0.041	0.038
% New recipients sickness-leave	0.362	0.425	0.361	0.328	0.344	0.327
% New recipients maternity-leave	0.176	0.141	0.177	0.200	0.194	0.201
% New recipients means-tested pension	0.076	0.098	0.076	0.108	0.082	0.106
Avg. Benefit in MWs	1.181	1.370	1.179	1.220	1.180	1.216
Avg. age at claiming	45.145	44.634	45.138	44.395	44.734	44.392
Avg. days of processing	217.287	181.853	216.906	232.016	221.696	232.595
Avg. claimant's contribution years	7.157	7.479	7.162	7.019	7.172	7.024
***Panel C.* Census data (2010)**						
Avg. HH earnings p.c.	497.224	631.848	496.784	469.291	470.421	470.746
% Receiving any social benefits	0.282	0.245	0.283	0.262	0.279	0.264
% Migrant from another municipality	0.354	0.356	0.352	0.337	0.334	0.331
% Primary school education	0.981	0.981	0.981	0.975	0.979	0.975
% Secondary school education	0.296	0.381	0.295	0.310	0.294	0.310
% College education	0.055	0.086	0.055	0.056	0.054	0.056
Avg. Age	31.977	31.203	31.980	30.015	31.251	30.123
% Working	0.460	0.487	0.460	0.446	0.447	0.446
% Formally employed	0.364	0.445	0.363	0.360	0.355	0.359
% Informally employed	0.309	0.266	0.309	0.321	0.319	0.321
% Self-employed	0.293	0.264	0.294	0.284	0.292	0.284
% Contributing to social security	0.491	0.555	0.490	0.459	0.473	0.460
% Informal empl. contributing to SS	0.139	0.134	0.139	0.116	0.131	0.117
% Self-empl. contributing to SS	0.236	0.267	0.236	0.196	0.223	0.199
Num. observations	4101	1469	4055	484	2729	457

Table A2: Summary Statistics at the Municipality-level on the Timing of the Expansion

	Main Sample	INSS Office Opening in 2009-2011	INSS Office Opening in 2012-2014	INSS Office Opening in 2015-2023	No INSS Office and > 20k residents in 2007
	(1)	(2)	(3)	(4)	(5)
***Panel A.* Demography and the Local Economy (2008)**					
Population	30705.192	29387.572	30137.749	33332.900	32698.991
GDP/Pop	8105.560	9953.270	7582.460	6827.566	10980.599
Hours to state capital	4.578	3.895	4.240	6.020	4.566
Hours to closest office	1.038	0.670	0.994	1.564	1.133
INSS transfers/Pop	714.550	785.858	712.868	631.455	623.025
INSS transfers/GDP	0.135	0.121	0.141	0.141	0.116
Income tax collection/Pop	80.228	112.545	62.790	72.944	192.188
Income tax collection/GDP	0.007	0.009	0.006	0.006	0.009
Social security collection/Pop	212.328	303.381	181.007	159.022	300.870
Social security collection/GDP	0.023	0.027	0.022	0.020	0.024
Tax collection net-of-benefits/Pop	497.371	859.088	320.815	381.547	1698.951
FPM transfers/Pop	289.558	293.100	289.728	284.930	279.502
FPM transfers/GDP	0.059	0.049	0.061	0.068	0.059
PBF transfers/Pop	88.613	72.724	94.229	97.564	91.034
PBF transfers/GDP	0.022	0.016	0.024	0.026	0.023
Num. formal labor contracts/Pop	0.107	0.138	0.097	0.089	0.113
Num. formal establishments/Pop	0.027	0.033	0.026	0.021	0.023
% Any bank	0.917	0.952	0.886	0.933	0.852
Bank loans/Pop	1237.333	1800.734	1097.911	811.000	785.130
Bank deposits/Pop	1181.561	1596.528	1057.768	906.063	1010.894
***Panel B.* Benefit recipients data (2008)**					
Num. new recipients/Pop	0.023	0.025	0.023	0.021	0.021
% New recipients old-age pension	0.230	0.211	0.229	0.254	0.224
% New recipients survivors insurance	0.098	0.095	0.100	0.095	0.093
% New recipients disability insurance	0.038	0.044	0.037	0.030	0.034
% New recipients sickness-leave	0.328	0.395	0.319	0.261	0.317
% New recipients maternity-leave	0.200	0.155	0.203	0.248	0.217
% New recipients means-tested pension	0.108	0.099	0.112	0.111	0.114
Avg. Benefit in MWs	1.220	1.296	1.202	1.160	1.256
Avg. age at claiming	44.395	44.510	44.463	44.132	43.924
Avg. days of processing	232.016	209.044	223.676	274.995	238.115
Avg. claimant's contribution years	7.019	7.101	6.958	7.032	7.213
***Panel C.* Census data (2010)**					
Avg. HH earnings per capita	469.291	546.895	450.290	410.196	453.361
% Receiving any social benefits	0.262	0.248	0.271	0.262	0.255
% Migrant from another municipality	0.337	0.361	0.327	0.325	0.328
% Primary school education	0.975	0.979	0.974	0.970	0.975
% Secondary school education	0.310	0.333	0.309	0.287	0.305
% College education	0.056	0.067	0.054	0.044	0.052
Avg. Age	30.015	30.875	30.038	28.934	29.180
% Working	0.446	0.484	0.434	0.423	0.430
% Formally employed	0.360	0.432	0.347	0.296	0.354
% Informally employed	0.321	0.290	0.330	0.342	0.297
% Self-employed	0.284	0.251	0.287	0.319	0.313
% Contributing to social security	0.459	0.532	0.447	0.394	0.451
% Informal empl. contributing to SS	0.116	0.117	0.115	0.119	0.126
% Self-empl. contributing to SS	0.196	0.243	0.187	0.156	0.178
Num. observations	484	145	219	120	229

Figure A2: Trends of Local Economic Outcomes Prior to the Expansion: Treated and Never Treated Municipalities in Main Sample



Note: This figure shows pre-expansion trends in local economic outcomes for treated and never treated municipalities in the main sample. The sample includes the 4,539 municipalities without a social security office by 2008 and without population outliers. Treated municipalities are those that later received their first office during the expansion period. Outcomes are plotted by year for 2005–2010 and are expressed in logs. Each panel reports the estimated differential linear pre-trend between treated and never treated municipalities using 2005 as the baseline period, along with its standard error.

Table A3: Estimates of Dynamic Treatment Effects for Main Outcomes

	Num. new recipients (log)	SS transfers (log)	SS contributions (log)	Income taxes + SS contributions (log)	Tax rev. goods /serv. + Inc. tax + SS contrib. (log)	Establishments in non-trade. priv. sector in LLM (log)	Labor contracts in non-trade. priv. sector in LLM (log)	Num. bank branches (PPML)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\hat{\beta}_{\leqslant -5}$	0.007	0.001	0.001	−0.015	−0.005	−0.009	−0.01	0.003
	(0.014)	(0.023)	(0.03)	(0.028)	(0.02)	(0.008)	(0.019)	(0.022)
$\hat{\beta}_{-4}$	0	0.006	0.013	0.002	−0.006	0	0.009	−0.005
	(0.009)	(0.012)	(0.023)	(0.022)	(0.012)	(0.005)	(0.014)	(0.014)
$\hat{\beta}_{-3}$	0.006	0.002	0.013	0.012	0.005	−0.001	−0.001	−0.006
	(0.007)	(0.008)	(0.017)	(0.016)	(0.01)	(0.004)	(0.011)	(0.01)
$\hat{\beta}_{-2}$	0.008	0	−0.003	0.006	0.004	−0.002	−0.015	−0.001
	(0.004)	(0.005)	(0.012)	(0.011)	(0.007)	(0.003)	(0.008)	(0.008)
$\hat{\beta}_0$	0.02***	0.01	0.008	0.011	0.013**	0.004	0.006	0.01
	(0.005)	(0.005)	(0.013)	(0.012)	(0.007)	(0.003)	(0.008)	(0.007)
$\hat{\beta}_1$	0.058***	0.039***	0.012	0.017	0.02**	0.006	0.013	0.011
	(0.007)	(0.008)	(0.017)	(0.016)	(0.009)	(0.004)	(0.01)	(0.01)
$\hat{\beta}_2$	0.064***	0.062***	0.025	0.025	0.024**	0.013***	0.026**	0.018
	(0.008)	(0.01)	(0.019)	(0.018)	(0.011)	(0.005)	(0.011)	(0.011)
$\hat{\beta}_3$	0.056***	0.076***	0.035	0.041**	0.03**	0.018***	0.04***	0.022
	(0.009)	(0.012)	(0.021)	(0.02)	(0.013)	(0.005)	(0.013)	(0.013)
$\hat{\beta}_4$	0.062***	0.091***	0.057***	0.065***	0.041***	0.022***	0.048***	0.041***
	(0.011)	(0.013)	(0.021)	(0.02)	(0.015)	(0.006)	(0.013)	(0.014)
$\hat{\beta}_5$	0.058***	0.109***	0.069***	0.085***	0.053***	0.023***	0.051***	0.048***
	(0.012)	(0.015)	(0.024)	(0.023)	(0.017)	(0.007)	(0.015)	(0.017)
$\hat{\beta}_{\geqslant 6}$	0.049***	0.132***	0.069**	0.089***	0.054***	0.029***	0.055***	0.079***
	(0.015)	(0.019)	(0.028)	(0.028)	(0.021)	(0.008)	(0.017)	(0.021)
ATT	0.052***	0.088***	0.046**	0.057***	0.038***	0.02***	0.04***	0.044***
	(0.009)	(0.013)	(0.02)	(0.019)	(0.013)	(0.005)	(0.012)	(0.014)
Wald test (null pre-trend)	p = 0.41	p = 0.96	p = 0.86	p = 0.82	p = 0.8	p = 0.73	p = 0.11	p = 0.95
Baseline mean	724.815	28777344.685	9530039.043	12962696.211	41711255.387	330.588	602.332	2.852
N. municipalities	4538	4539	4531	4528	4528	4539	4539	2720
N. observations	63532	67907	67892	67071	67008	68085	67921	40800

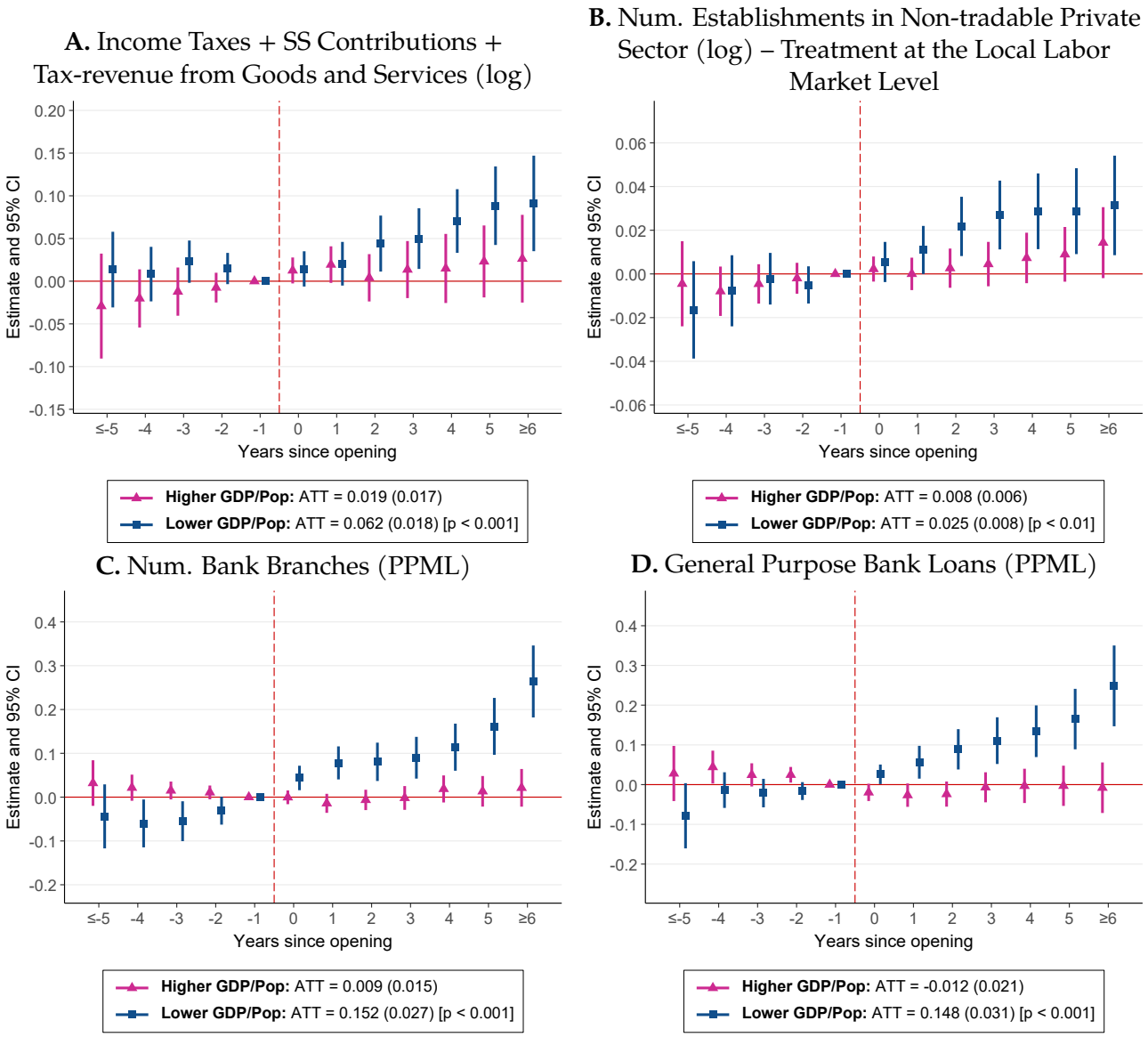
Table A4: Heterogeneity by Baseline Economic Development: Summary Statistics for Municipalities Below and Above Median ex-ante GDP per capita in the Main Sample

	All Treated Municipalities	Lower GDP per capita	Higher GDP per capita
	(1)	(2)	(3)
Panel A. Demography and the Local Economy (2008)			
Population	30705.192	30493.930	30916.455
GDP/Pop	9644.726	4343.229	14946.224
INSS transfers/Pop	904.473	844.934	964.011
INSS transfers/GDP	0.145	0.208	0.081
Tax revenue goods and services/Pop	850.335	223.382	1477.289
Income tax collection/Pop	100.565	14.474	187.013
Social security contributions/Pop	281.292	99.479	463.858
Social security contributions/GDP	0.027	0.023	0.031
Tax collection net-of-benefits/Pop	541.230	-477.015	1563.701
FPM transfers/Pop	290.645	294.558	286.699
PBF transfers/Pop	118.742	164.172	73.123
Num. new benefit recipients/Pop	0.024	0.021	0.026
Num. formal labor contracts/Pop	0.123	0.063	0.184
% Any bank	0.944	0.901	0.988
Bank loans/Pop	2632.826	1079.321	4186.330
Panel B. Census data (2010)			
Avg. Household earnings per capita	469.291	315.316	623.265
% Secondary school education	0.310	0.262	0.359
% Working	0.446	0.378	0.515
% Formally employed	0.360	0.242	0.477
% Informally employed	0.321	0.390	0.251
% Self-employed	0.284	0.318	0.250
% Contributing to social security	0.459	0.334	0.584
% Informal empl. contributing to SS	0.116	0.110	0.123
% Self-empl. contributing to SS	0.196	0.115	0.277
Observations	484	242	242

Table A5: Estimate of the Effect of Openings on SS Contributions (log) with Lagged Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ATT	0.046**	0.045**	0.044**	0.046**	0.046**	0.046**	0.041**	0.024*	0.04**	0.023*
	(0.02)	(0.02)	(0.02)	(0.019)	(0.02)	(0.02)	(0.019)	(0.013)	(0.019)	(0.013)
log(Formal labor contracts		0.102***							0.057***	0.025***
in non-tradable priv. sector)$_{t-1}$		(0.008)							(0.008)	(0.005)
log(Formal labor contracts			0.142***						0.076***	0.036***
priv. sector in LLM)$_{t-1}$			(0.022)						(0.022)	(0.012)
log(GDP estimate)$_{t-1}$				0.241***					0.174***	0.056***
				(0.016)					(0.014)	(0.008)
log(Population estimate)$_{t-1}$					0.065				0.147***	0.037
					(0.05)				(0.047)	(0.025)
log(SS transfers)$_{t-1}$						−0.002			−0.014*	−0.005
						(0.008)			(0.007)	(0.004)
log(Income taxes)$_{t-1}$							0.127***		0.107***	0.038***
							(0.006)		(0.006)	(0.003)
log(SS contributions)$_{t-1}$								0.559***		0.528***
								(0.007)		(0.007)
Observations	67892	67677	67892	67891	67892	67602	66666	67882	66362	66352

Figure A3: Heterogeneity: Effect on the Local Economy by ex-ante GDP per capita



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

Table A6: Heterogeneity of Tax Revenue Elasticities with respect to Social Security Transfers and Multipliers by Baseline Economic Development

	Post-period	SS contributions	Income taxes + SS contrib.	Tax rev. goods/ serv. + Inc. taxes + SS contrib.	First-stage F-statistic	N. Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Higher GDP per capita						
$\left(\frac{\text{Tax}}{\text{Transfers}}\right)$		0.565	0.765	2.616		
$\frac{\text{d log Tax}}{\text{d log Transfers}}$	All	0.32	0.429*	0.256	62.033	63286
		(0.238)	(0.251)	(0.192)		
	2 years	0.185	0.187	0.097	14.735	61302
		(0.261)	(0.267)	(0.204)		
	4 years	0.169	0.277	0.149	31.751	61298
		(0.207)	(0.215)	(0.182)		
	6+ years	0.297	0.412*	0.209	77.961	61912
		(0.225)	(0.233)	(0.176)		
$\frac{\Delta\text{Tax}}{\Delta\text{Transfers}}$	All	0.181	0.328*	0.671		
		(0.135)	(0.192)	(0.502)		
	2 years	0.104	0.143	0.254		
		(0.147)	(0.204)	(0.534)		
	4 years	0.096	0.212	0.39		
		(0.117)	(0.164)	(0.476)		
	6+ years	0.168	0.315*	0.548		
		(0.127)	(0.178)	(0.461)		
Panel B. Lower GDP per capita						
$\left(\frac{\text{Tax}}{\text{Transfers}}\right)$		0.156	0.177	0.526		
$\frac{\text{d log Tax}}{\text{d log Transfers}}$	All	0.421	0.532	0.586*	26.599	63303
		(0.422)	(0.415)	(0.316)		
	2 years	0.103	0.224	0.48	7.888	61535
		(0.452)	(0.423)	(0.311)		
	4 years	0.732*	0.841**	0.686**	13.535	61526
		(0.412)	(0.407)	(0.288)		
	6+ years	0.473	0.566*	0.548**	54.195	62002
		(0.294)	(0.295)	(0.24)		
$\frac{\Delta\text{Tax}}{\Delta\text{Transfers}}$	All	0.066	0.094	0.308*		
		(0.066)	(0.074)	(0.166)		
	2 years	0.016	0.04	0.253		
		(0.07)	(0.075)	(0.163)		
	4 years	0.114*	0.149**	0.361**		
		(0.064)	(0.072)	(0.151)		
	6+ years	0.074	0.1*	0.288**		
		(0.046)	(0.052)	(0.126)		

Figure A4: Example of a Social Security Office from the Expansion



Figure A5: Online Claiming of INSS Benefits Started in 2018

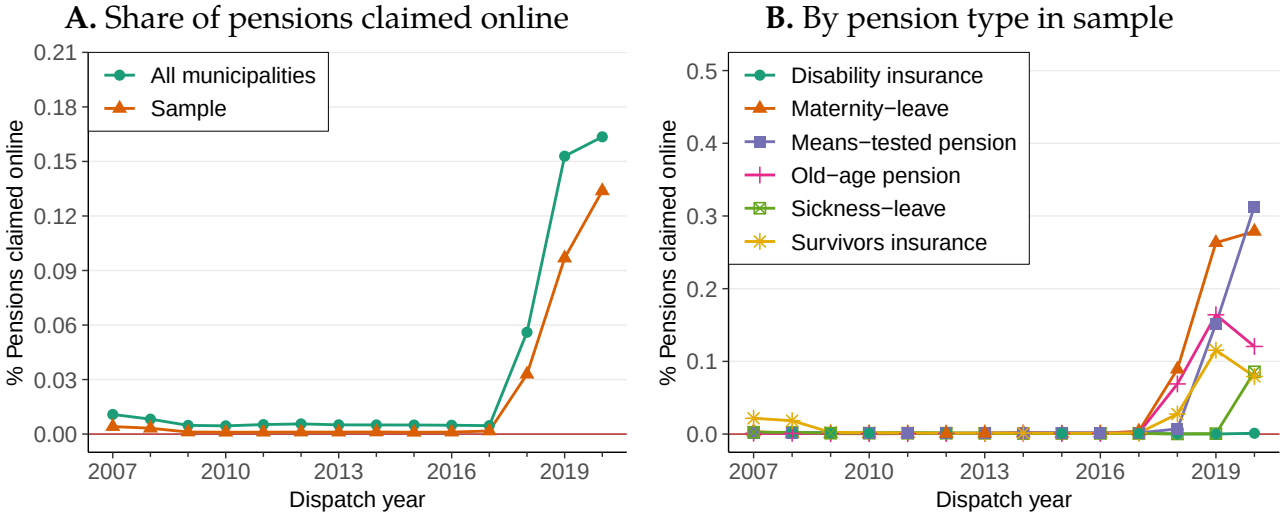
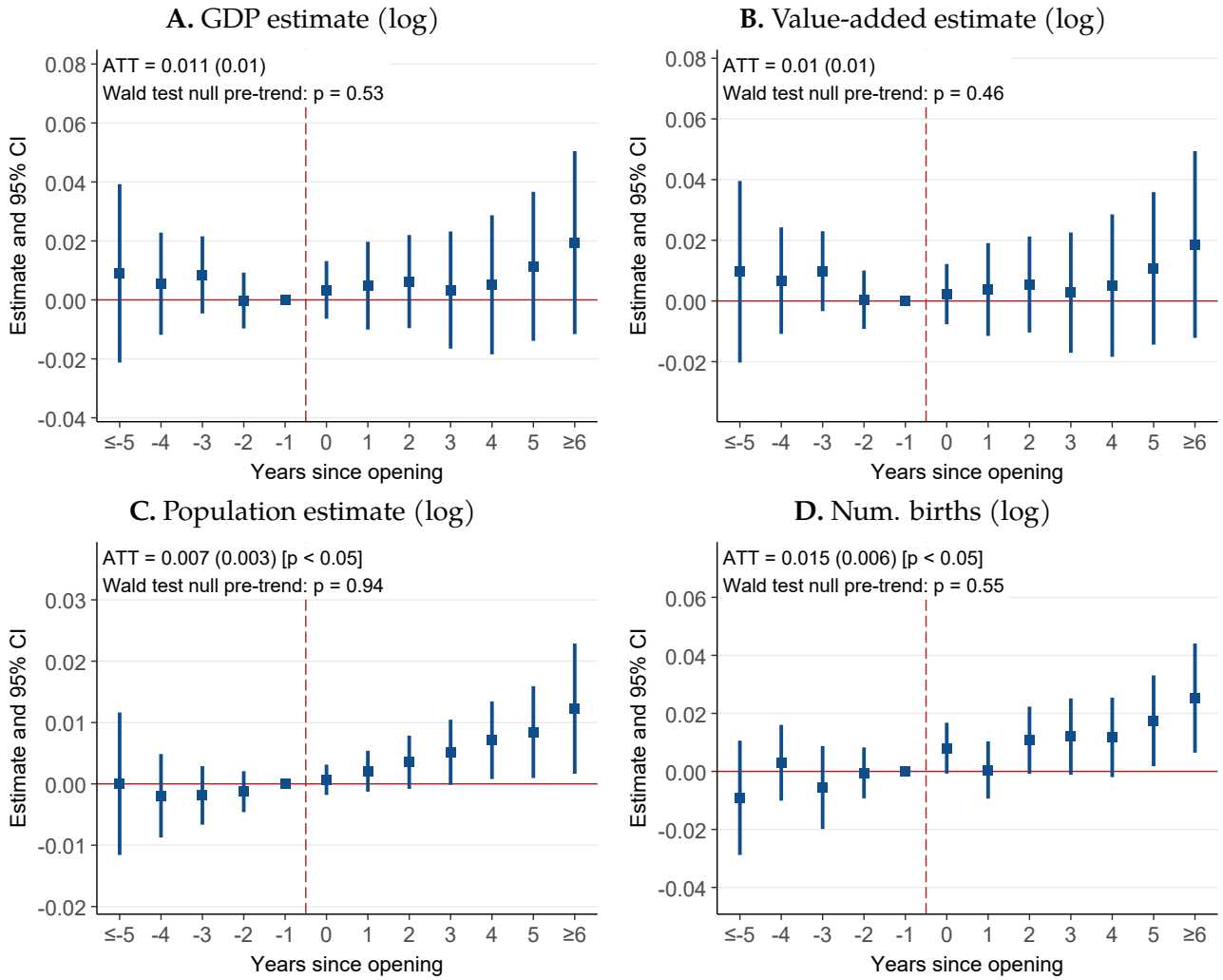
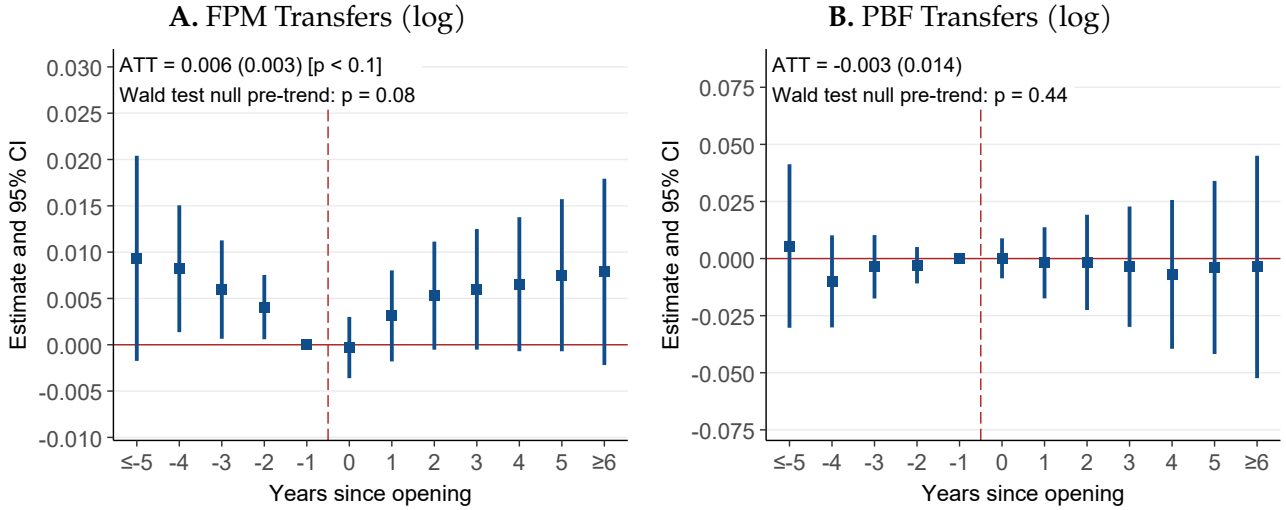


Figure A6: Effect on GDP and Value-added estimates, Population estimate, and Number of Births



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

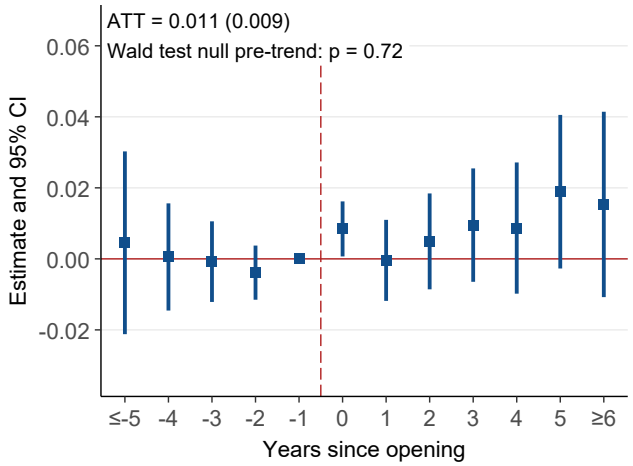
Figure A7: Effect on Other Federal Transfers: FPM and PBF



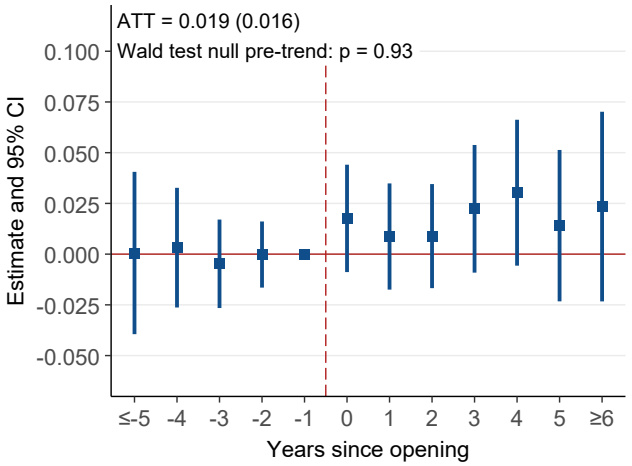
Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

Figure A8: Effect on Labor Market Outcomes

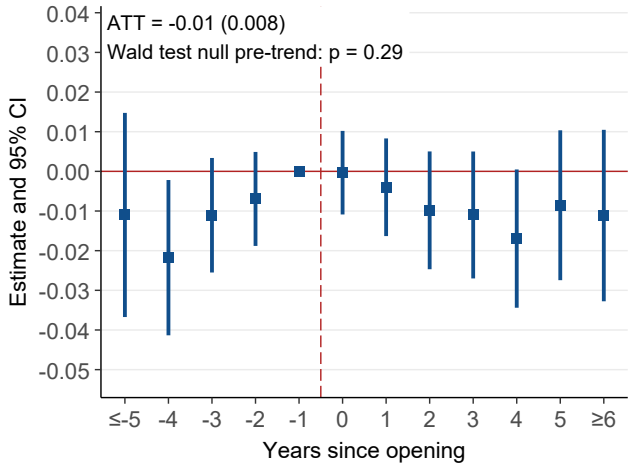
A. Num. Establishments in Non-tradable Private Sector (log)



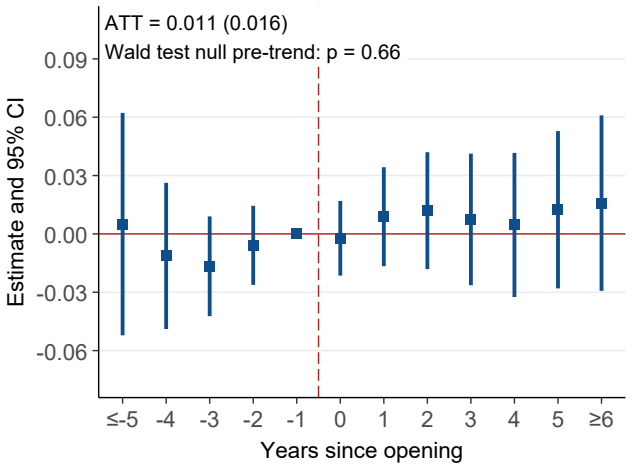
B. Num. Formal Labor Contracts in Non-tradable Private Sector (log)



C. Num. Establishments in Tradable Private Sector (log) – Treatment at the Local Labor Market Level

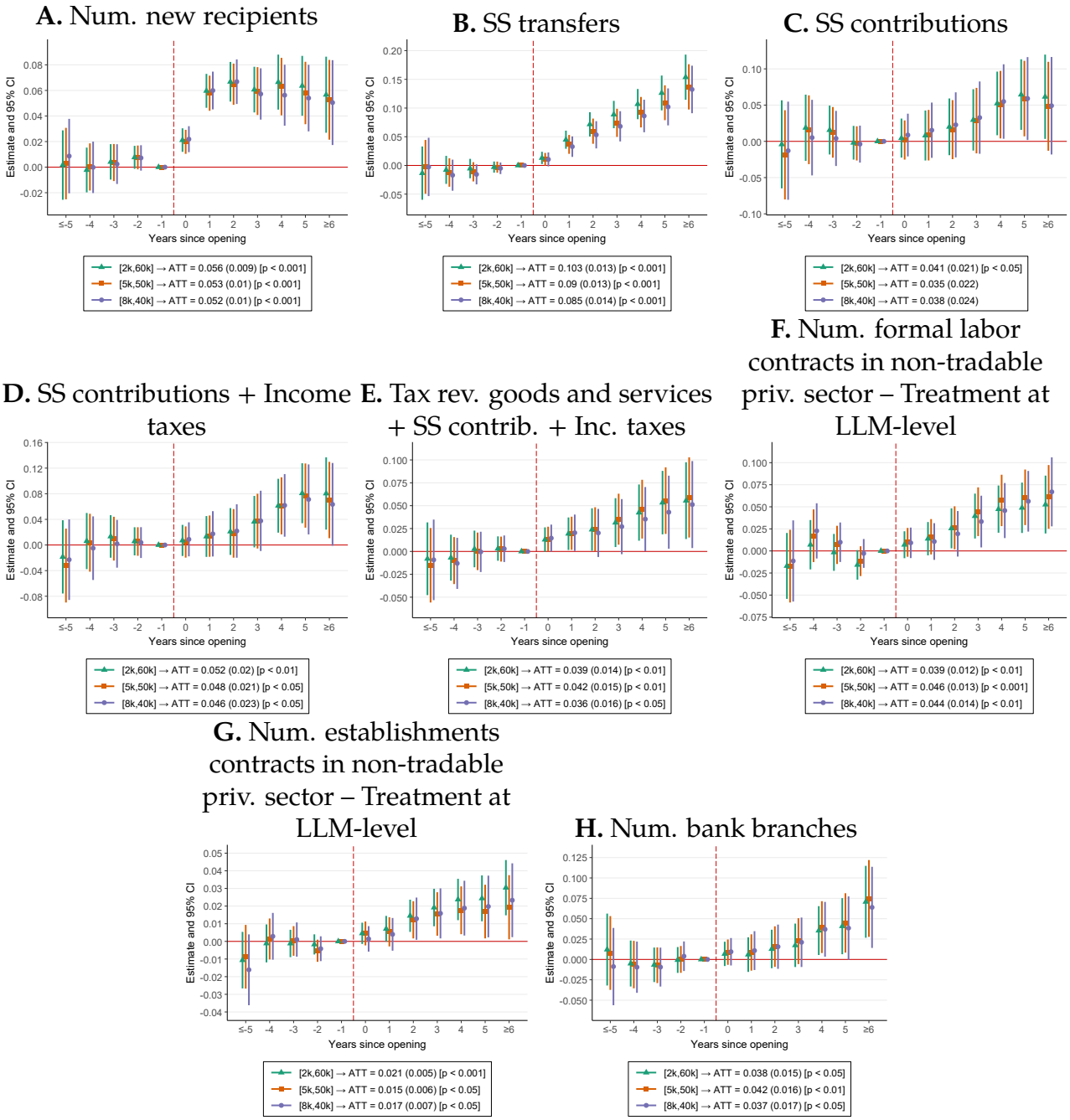


D. Num. Formal Labor Contracts in Tradable Private Sector (log) – Treatment at the Local Labor Market Level



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021). Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.

Figure A9: Robustness of Main Results to Population Window Around the 20k Residents Cutoff – Event studies



Note: This figure reports dynamic treatment effects from estimating equation (1) using the heterogeneity-robust event-study estimator of Sun and Abraham (2021) in other samples, where I restrict municipalities around the 20,000 residents threshold. Coefficients are shown relative to the year before office opening, which is omitted as the reference period. The figure also reports the average treatment effect on the treated (ATT), computed as the average of post-treatment coefficients, and the p-value of a Wald test of the null hypothesis that all pre-treatment coefficients are jointly equal to zero. All specifications include municipality fixed effects, state-by-year fixed effects, and time-varying effects of predetermined population controls. Standard errors are clustered at the municipality level, and 95 percent confidence intervals are shown.