
Long-Run Local Spillovers of Conditional Cash Transfers: Evidence from
Brazil's Bolsa Família

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Abstract

This dissertation examines whether large-scale conditional cash transfers generate persistent economic spillovers. It studies Brazil's Programa Bolsa Família and evaluates the long-run effects of the program's 2009 expansion through 2019. The central question is whether more exposed municipalities experienced durable gains in local economic activity, formal employment, tax revenue, and selected welfare outcomes.

The empirical strategy uses a municipality-year panel from 2007 to 2019 and estimates an event-study difference-in-differences model. Municipalities are classified by exposure to the 2009 expansion, comparing those with larger beneficiary increases to those with smaller increases within the same state. The model includes municipality fixed effects, state-year fixed effects, and baseline program controls interacted with year indicators.

A key result for interpretation is that more exposed municipalities remained on a higher path of beneficiaries and payments, although the differential declined over time. Therefore, the long-run estimates capture outcomes under a persistent but attenuated difference in program intensity, not only delayed effects of a temporary 2009 shock. The evidence indicates persistent positive effects on GDP per capita and private-sector formal employment. Product-tax revenue increases mainly in the short run, while income-tax effects are positive but imprecise. The sectoral analysis builds on short-run evidence that Bolsa Família's local employment effects were concentrated in non-tradable activities, such as commerce and local services. By separating employment into non-tradable and tradable activities, such as agriculture and manufacturing, this dissertation tests whether that demand-driven pattern persisted. The results suggest that non-tradables remain important, but long-run employment gains were not confined to them, since tradable employment also increased.

Non-income results are mixed. Health expenditure shows limited evidence of persistent level effects. Female homicides decline in the growth specification, while male homicides show no comparable effect. This contrast may indicate a gender-related mechanism, consistent with Bolsa Família's design of directing benefits preferably to women.

JEL-codes: H53, I38, R11, C23

Key-words: Bolsa Família, conditional cash transfers, local multipliers, difference-in-differences.

Resumo

Esta dissertação examina se transferências condicionadas de renda geram efeitos econômicos locais persistentes. O estudo analisa o Programa Bolsa Família e avalia os efeitos da expansão de 2009 até 2019. A questão é se municípios mais expostos apresentaram ganhos duradouros em atividade econômica, emprego formal, arrecadação e bem-estar.

A estratégia empírica utiliza um painel município-ano de 2007 a 2019 e estima um estudo de eventos com diferenças em diferenças. Os municípios são classificados conforme sua exposição à expansão de 2009, comparando maiores e menores aumentos de beneficiários dentro do mesmo estado. O modelo controla efeitos fixos municipais e de estado-ano, além de diferenças iniciais do programa.

Um resultado central é que os municípios mais expostos permaneceram em trajetória mais elevada de beneficiários e pagamentos, embora esse diferencial tenha diminuído. Assim, as estimativas de longo prazo capturam resultados sob diferença persistente, mas atenuada, na intensidade do programa, e não apenas efeitos de um choque temporário em 2009. As evidências indicam efeitos positivos persistentes sobre PIB per capita e emprego formal privado. A arrecadação sobre bens e serviços aumenta no curto prazo, enquanto os efeitos sobre imposto de renda são positivos, mas imprecisos.

A análise setorial parte da evidência de curto prazo de que os efeitos locais do Bolsa Família sobre o emprego se concentraram em atividades não transacionáveis. Ao separar emprego não transacionável, como comércio e serviços, e transacionável, como agricultura e manufatura, testa-se se esse padrão associado à demanda local persistiu. Os resultados sugerem que essas atividades continuam importantes, mas os ganhos de longo prazo não se limitaram a elas.

Os resultados não monetários são mistos. O gasto em saúde mostra evidência limitada de efeitos. Os homicídios femininos diminuem na especificação em crescimento, enquanto os masculinos não apresentam efeito comparável. Esse contraste pode indicar um mecanismo de igualdade de gênero.

Códigos-JEL: H53, I38, R11, C23

Palavras-chave: Bolsa Família, transferências de renda condicionadas, multiplicadores locais, diferenças em diferenças.

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Chapter 1. Introduction

Over the last two centuries, views on poverty have evolved from treating it as a “necessary evil” to recognizing it as a constraint for economic growth, leading modern policy to balance protection (preventing incomes from falling below critical thresholds) and promotion (building human capital) (Ravallion, 2015). Building on this shift, cash transfer programs have become one of the main policy instruments used to combat poverty across both developing and developed economies.

A large and growing body of empirical evidence documents the direct effects of cash transfers at the household level. These programs have been shown to reduce poverty, improve educational attainment, enhance health outcomes, and increase consumption and investment among beneficiaries (Bastagli et al., 2016). Beyond these direct impacts, however, cash transfers may also generate broader economic effects by altering local demand, employment, firm behaviour, and fiscal outcomes. These indirect effects, commonly referred to as spillovers, represent a key dimension for understanding the full impact of social policy.

Egger et al. (2022) provide one of the first pieces on the subject, demonstrating that large one-time cash grants towards poor households generate neutral effects on local prices and significant positive spillovers for local businesses revenue, although the effect on firms’ profits or firms’ market entry remain inconclusive. On the other hand, Delius and Sterck (2024) show that the use of small multiple-time digital cash transfers (that were only accepted by specific licensed shops) leads to higher local prices (and also increased revenue and profits for licensed businesses), indicating a potential inefficiency due to the limited competition.

Understanding the broader economic implications of cash transfers is essential for evaluating their full socioeconomic benefits. In this context, Brazil’s Programa Bolsa Família (PBF), implemented in 2004, provides a particularly useful case study, as one of the world’s largest and longest-running conditional cash transfer (CCT) programs, benefiting, even, in 2019, around one fifth of Brazil’s population (Brollo, 2024). Even more, its national scope together with administrative microdata and municipality-level panels, enables credible empirical designs. Altogether, Brazil’s case presents an exceptional opportunity to explore the spillover effects of cash transfers.

Using data on Brazil’s PBF, Gerard et al. (2024) explore the impact of an increase in, an already established, unrestricted long-term cash transfer in the local economy. To do that,

the authors analyse the 2009 national quota-increase in PBF¹, which led to a large expansion of the national number of slots for beneficiaries. Besides the increase in slots, the methodology of allocation between municipalities also changed, creating a substantial difference in the number of increase of recipients of the program between municipalities. Applying a difference-in-differences (DID) analysis, the authors reach to the conclusion that the program's shock impacted positively the municipalities GDP, tax revenue and formal labour without increasing wages (which, according to the authors, may indicate a no rise in prices) in the short run. The paper also finds a strong concentration of those impacts in the non-tradable sector. Additionally, the paper also estimates the Marginal Value of the Public Funds (MVPF) of the PBF, that is the benefits (willingness-to-pay) over the net cost ratio of the program (Finkelstein & Hendren, 2020), having a greater than 1 ratio (benefits are greater than costs).

Despite recent progress, the empirical literature on spillover effects remains incomplete, especially when it comes to persistence over time. While existing studies provide evidence of positive local economic responses in the short run, it is less clear whether these effects are sustained in the long run, whether they reflect temporary adjustments or more durable changes in local economic activity, and how they evolve as program exposure changes. Addressing these questions is central to evaluating whether cash transfers generate lasting transformations in local economies or primarily short-lived demand shocks.

Utilizing the same Gerard et al. (2024)' municipality-level DID approach, this dissertation aims to evaluate the long-term impact of CCT on the local economy by extending their analysis from 2009 until 2019 (when the PBF was shortly substituted by a unconditional cash transfer due to COVID-19). Additionally, based on the mandatory health check-ups of the recipients' children and the fact that most recipients are the mother of the family, this research also studies the impacts of the transfers on health expenditure and female homicide.²

Therefore, this essay aims to answer the following questions: Did municipalities more exposed to the 2009 quota shock in PBF experience persistent gains in the local economy? Did the pattern of gains suggested by the literature in the short-term maintain in the long

¹ The program worked on a quota system where the families could only become beneficiaries if, besides proving low income, there was a vacant slot in their municipality.

² Domestic violence data in the municipal level is only available from 2009 onwards, the reason why female homicide is used instead.

run? Did the transfers positively impact other non-income welfare outcomes?

Our research goals are the following: i) to frame and synthesize the literature on cash transfers focusing on conditioned one's and their effects on local and aggregate level; ii) to analyse the effects of an increase in the PBF in the municipalities' GDP, tax revenue and formal employment in the long-term; iii) to check which activities are more impacted by the increase in the PBF; iv) to analyse if other non-income welfare measures are positively impacted.

This study is relevant to quantify the whole impact, in the long run, of cash transfers programs with particular emphasis on indirect spillover effects on local employment, prices, business activity and other non-income welfare measures. Its focus on the spillover effects of the transfers in the long-term fills a key gap in the literature. Reliably estimating the indirect impact of PBF can show policymakers the whole benefit of the program, and whether the tax feedback and local multipliers make the program fiscally sustainable in the long run.

This dissertation is organized into five chapters in addition to this Introduction. Chapter 2 reviews the literature on anti-poverty policies, cash transfers, and their direct and indirect effects, and then situates Bolsa Família within the Brazilian institutional context. Chapter 3 presents the empirical strategy, detailing the identification approach, the event-study difference-in-differences model, and the sectoral framework used in the analysis. Chapter 4 reports the empirical results, beginning with the evolution of the treatment differential and then examining local economic activity, the sectoral composition of employment effects, and non-income welfare outcomes. Chapter 5 discusses the main findings and their broader implications. The final chapter concludes.

Chapter 2. Research background

This chapter reviews the concept and structure of cash transfers, as well as the evidence on its direct and indirect effects. It also aims to differentiate the different designs of possible programs. Lastly, it does a historical background of the PBF and review its specific literature.

2.1 Anti-poverty policies

As Ravallion (2015) argues, nowadays, anti-poverty policies are framed around two pillars: protection and promotion. That means, the goal of those policies is to make sure people's income do not fall below critical thresholds (even after crisis), and the policies are efficient (through schooling, basic health, etc.) in creating opportunities and developing human capital to incentivize people to escape poverty. However, policies are constrained by information and incentive constraints. If incomes were perfectly observable and behavior invariant, guaranteeing a minimum income would be straightforward, but limited administrative capacity and possible labour disincentives forces design trade-offs. Also, the initial level of development of the country matters, as poorer countries have a bigger informal sector, the use of self-targeting or indicator-based targeting systems are more adequate. Further, policies that apply only to formal-sector workers carry added efficiency costs as it may incentivize informality.

Therefore Ravallion (2015) presents antipoverty measures along a spectrum: on one end, tightly targeted transfers (proxy-means tests, self-targeting), on the other, more universal schemes (basic income). There are also special designs such as Employment Guarantee Schemes that, even though it is not targeted (anyone can assess it), it imposes an opportunity cost on participants (people need to work on unskilled labour and receive the minimal rates) that grants a costless and non-interventionist targeting.

2.2 Cash transfers

Cash transfers have become one of the main social policies across the globe in combating poverty. Indeed, before COVID-19, more than 130 countries had permanent cash transfers on their social programs (Bastagli et al., 2016). Design, however, varies along two dimensions: conditionality and targeting. For conditionality the program can be either unconditional or conditional to certain behaviours. The specific conditional behaviours vary from program to program but are normally focused on developing human capital for future

generations (e.g. school attendance and health check-up for the children). For targeting, the transfer can be universal to all residents or targeted to a specific group, considering asset, income, gender, number of children, etc. These different designs are seen into well-known programs: Brazil's Bolsa Família is targeted and conditional, directing benefits to low-income households that keep children in school and comply with basic health check-ups; South Africa's Child Support Grant is targeted but unconditional, reaching low-income caregivers without behaviour requirements; Alaska's Permanent Fund Dividend is universal and unconditional, distributing an equal payment to residents regardless of income or household characteristics.

Fiszbein & Schady (2009) presents three main reasons for conditionality on cash transfers. The first is that families may underinvest in education and health because of myopia and imperfect information that shifts their utility from the private optimum. The second is that conditions create a greater support from non-beneficiaries as it is seen as a benefit for good behaviours. Lastly, even if the families hold perfect information and get to the private optimum, that might not be enough to reach social optimality and, thus, conditions are needed.

For targeting, Akerlof (1978) argues that "tagging" (to identify groups that are on average needy) and giving them a distinct transfer can deliver higher support to the poor with lower efficiency costs than a uniform negative income tax, because non-needy taxpayers are not granted the transfer in the first place. At the same time, tagging is not free. The trade-off consists of the administrative costs of identifying who is eligible and the possible change in behaviour some individuals may do in order to receive the transfer. The optimal extent of tagging requires, therefore, designers to weigh administrative burdens and behavioural responses against the efficiency gains from excluding non-needy taxpayers from transfers.

Besides that, targeting may impact the labour market, as cash transfers can disincentivize labour in two ways. First, in Becker's (1965) time-allocation model, an increase in non-labour income, *ceteris paribus*, shifts in parallel the opportunity curve to the right. This income effect decreases hours worked. Second, when transfers are means-tested and phase out with earnings, they raise the implicit marginal tax rate on work, lowering the net return to additional hours. This substitution effect makes work relatively less attractive and distorts labour supply.

To minimize the labour supply disincentives of the cash transfers, universal cash transfers are suggested due to their labour neutrality. A universal cash transfer acts similarly

to a negative lump-sum tax, that is taxes that are independent from individuals' action. Due to that independence, lump-sum taxes only affect people's behaviour through the income effect and does not change marginal incentives to work (Atkinson & Stiglitz, 2015). Thus, in theory universal cash transfers have a lower disincentive on work when compared to targeted transfers (that create a substitution effect).

2.3 Cash transfer and the poor

Haushofer and Shapiro's (2016) experiment in Kenya suggest that unconditional cash transfers (UCTs) are effective anti-poverty tools. The randomized trial shows that the transfer reliably increases consumption, asset holdings, and psychological well-being in the short run of the recipients, without triggering the temptation goods (such as alcohol and tobacco) or local price effects.

In the long run the results are even more promising. Analysing the U.S. Mothers' Pension program (1911–1935), Aizer et al. (2016) find that boys in accepted households lived, on average, nearly one year longer than boys from rejected applications. The authors argue this longevity gain is partly caused by improved young adult outcomes among beneficiaries. They estimate the program caused a 50% reduction in undernutrition, a 13% increase in income, and 0.4 additional years of schooling. Altogether, their analysis shows that cash transfers, in the long run, may increase life expectancy and welfare among the children of the participants during their lifetime.

Even so, a recurring controversy is that cash transfers may decrease the price of leisure and, therefore, discourage labour supply. In fact, Cesarini et al. (2017) using Swedish lottery data, show that large, exogenous wealth shocks reduce labour supply. Nonetheless, when applied to a government cash transfer program, Banerjee et al. (2017) cross-program re-analysis of seven programs shows no meaningful reduction in work (neither in rates nor hours) among recipients. Thus, the policy-debate claims that transfers are laziness-inducing, fuel inflation and/or shift spending towards temptation goods are not supported by the evidence.

In fact, similarly with the target cash transfers, Jones & Marinescu's (2022) article based on Alaska universal fund presents an empirical evidence to support the idea that the universal fund does not impact labour supply as well. Hence, universal cash transfers are theoretically and empirically backed by their low interference in workforce when compared to means-tested one's.

However, as evidenced by Kozminski & Baek (2017), universal cash transfers can actually worsen income inequality and, thus, is not advisable as a redistribution policy. Yet, Berman (2018) shows empirical evidence that it's still a useful tool for combating poverty and extreme poverty.

The fact that there are no conditions to be monitored gives a cost advantage to UCTs when compared to the CCTs. However, as highlighted by Baird et al (2011), the CCTs' different design, focused on shifting behaviour tied to human capital, is effective. Indeed, the authors' experiment in Malawi, whereas adolescent girls were randomized to CCTs and UCTs arms, showed a CCTs advantage in improving school enrolment (being even more cost-effective than the cheaper design towards that goal).

Cash transfers may be effective in combating gender inequality. Orozco & Gammage (2017) article review on Mexico's conditional cash transfer program (Progressa-Oportunidades-Prospera) show how the transfer has a great impact on the empowerment of women's younger generation by increasing years of schooling more on girls than in boys. Nonetheless, care roles may persist for older generations, even though improved child health created by the program likely reduces mothers' time caring for sick children, conditionalities and higher school attendance can reallocate domestic and caregiving tasks toward mothers, with studies documenting declines in women's leisure time and little change for men, underscoring the need to pair transfers with childcare and labour-market services to translate educational gains into broader economic empowerment. Apart from the inconclusive result on time, intimate partner violence studies also show that the violence can either be raised, decreased or stay the same depending on the context (cultural background of the family, place of living, etc.) (Leite et al., 2019).

Bastagli et al. (2016) large multi-country review similarly find that CCTs consistently increase school enrolment and health-service use, while effects on learning outcomes are more modest, reflecting the fact that conditions chiefly move inputs (attendance, check-ups) rather than instructional quality. Millán et al. (2019) raises three main possibilities for that: when there is no conjunct investment in education, higher enrolment crowds classrooms and raises teacher–pupil ratios; the students drawn in by the program may also have lower expected returns to schooling (being less prepared or motivated, from poorer households, or with less time for study); CCTs often target disadvantaged areas, average teaching quality and school resources tend to be weaker. However, according to the Milan et al.' analysis, children enrolled in Colombia's Familias en Acción were 8–16% more likely to graduate from

high school than comparable nonparticipants. As the authors conclude, that may indicate another positive impact of the program (more people going into higher education, for instance) in improving human capital quality, even though it does not seem to impact the academic performance of the participants.

2.4 Spillovers and indirect effects

Cash transfers can reshape local economies beyond direct recipients by shifting demand, prices, employment, and firm behaviour. Here, spillovers denote effects on non-recipients that arise via general-equilibrium adjustments in local markets. More broadly, indirect effects encompass all outcomes outside the program's immediate objectives (intended or unintended), including but not limited to spillovers.

Mechanically, a cash transfer acts as an exogenous income shock, impacting the local economy in multiple ways. One way is through demand. Egger et al. (2022) show that when local supply is elastic and markets are competitive, the increase in demand tends to lead to adjustments on the quantity, raising output and employment rather than prices or profits. At the municipal level, Gerard et al. (2024) also find that these impulses concentrate in non-tradable services and translate into significant gains in formal employment and economic activity for non-recipients.

However, an increase in demand does not necessarily guarantee a corresponding expansion in output. When markets are not sufficiently competitive, or when program design generates a segmented (two-tier) market with differentiated prices, the adjustment may occur primarily through higher prices and profits rather than increased production.

Delius and Sterck (2024) study the importance of the program design. In rural Kenya, they examine a restricted, unconditional electronic transfer structured as food vouchers that could be redeemed only at a set of pre-approved, licensed shops. Such designs are typically defended on behavioural grounds, limiting purchases to “essential” goods and discouraging temptation ones. Yet, the restrictive nature of the program created a second market whereas licensed shops could increase prices over their marginal cost, getting non-normal profit. The licensing requirement acted as an artificial barrier to entry, in which probable competitors were excluded unless they obtained the license, so the residual demand facing approved shops became less elastic. That way, licensed shops were able to capture economic rents from the program, making the program less cost efficient.

Filmer et al. (2018) also show the impact of non-competitive market on the outcome

of the program. In the Philippines, they compare price dynamics for nationally traded goods with perishable, locally traded goods before and after the program's rollout. Prices for local perishables (supplied mainly by oligopolistic producers) rose significantly, while nationally traded goods did not, and these increases translated into worse nutrition among non-beneficiaries, evidenced by higher stunting rates. Nonetheless, those effects were only significant in areas with higher program saturation, and for beneficiaries, income gains largely offset the local price pressures. As the authors argue, these results point to a potential negative spillover effect of the cash transfers in lower-income settings, where problems like stunting is more prevalent, relative to more studied middle-income contexts such as Mexico or Brazil.

Yet, it is not clear if those negative spillovers may happen in other poor countries with more competitive markets. For example, Sileshi et al. (2024) study on the nutritional spillover effect of an in kind transfer (which consisted in nutritional, agricultural and financial training plus seeds transfer) in Madagascar, in villages characterized by smallholder agricultural production based on small family crops, finds positive within-village spillovers on non-beneficiaries families nutrition, having an improvement in woman's and children's nutritional score. To the best of our knowledge the nutritional spillovers of cash transfers in competitive frameworks is still sparse, future research may address that.

Cash transfers can also operate on the supply side of local economies. In their evaluation of Zambia's Child Grant Programme, an unconditional cash transfer to households with at least one child under three, Daidone et al. (2014) document sizeable increases in beneficiaries' investment in productive assets and agricultural inputs. These investments translated into higher crop output and, at the community level, stimulated local economic activity. Beneficiary households shifted a larger share of food consumption toward market purchases rather than home production, signalling deeper market participation.

Papineni et al. (2025) extend this results by evaluating the spillover effect on the supply. By studying an unconditional cash transfer paid to women in ultra-poor households in rural Nigeria, they find that the program increased women's entry into entrepreneurship among recipients and, via local spillovers, among non-recipient women living in the same villages. These effects persist beyond the last payments, entrepreneurship gains remain detectable one year after the final payment, and local GDP positive effects become statistically significant only in that post-program year.

Altogether, as Gassmann et al. (2023) systematic literature review demonstrates,

through their indirect effects, cash transfers serve more than just a redistribution of income and reduction of poverty policy. By easing the household constraint and increasing the purchase power of recipients, transfers create a multiplier effect in the local economy. Although the multiplier effect has empirically always been positive, due to multiple different methodologies, its magnitude may differ, and it is not possible to make a meta-analysis. Overall, the evidence indicates that cash transfers can increase local economic activity rather than merely reallocate resources, but the size of these gains is highly contingent on programme design and market conditions (See in Annex a reading table for this literature review).

2.5 Brazil's case

As argues Burgos (2024), social protection in Brazil is deeply connected with “familism”, a characteristic of Southern European welfare regimes that moved into Latin America. In this concept, the family is positioned as the central provider for social welfare, and the State assumes a subsidiary role. This is officially recognized by the Brazil's own constitution, in article 229, which determines the family as responsible for the care of children, teenagers, and the elderly. Taking this into account, the PBF targets the family unit, taking the woman of the family as the preferential manager of the benefit, in order to break the intergenerational cycle of poverty.

The program has passed through multiple transformations throughout the years. Those changes are shortly explained in the next section. After that, the subsequent section presents the results and impact of PBF over more than 20 years of history.

2.5.1 PBF's institutional background

It is important to note that the PBF was preceded by some anti-poverty measures in Brazil. However, the PBF represented a change in the fragmented approach to poverty alleviation. Departing from structurally disconnected aid programs, the PBF introduced a unified, large-scale CCT model that integrated immediate income relief with long-term human capital investment. Before 2003, Brazil's social safety net was characterized by a mix of uncoordinated initiatives spread across different ministries, resulting in overlapping beneficiaries and diverging administrative protocols. The anti-poverty measures were mainly constituted of: *Bolsa Escola*, managed by the Ministry of Education (Lei No 10.219, 2001); *Bolsa Alimentação*, focusing on nutritional risks under the Ministry of Health (Medida

Provisória No 2.206-1, 2001); the Auxílio-Gás subsidy administered by the Ministry of Mines and Energy (Decreto No 4.102, 2002); and the Cartão Alimentação, an initiative linked to the early Fome Zero strategy (Lei No 10.689, 2003). This fragmentation resulted in significant inefficiencies, forcing families to navigate disparate bureaucracies and eligibility criteria.

Recognizing these administrative complexities, the PBF was created to unify these different parts into a single, robust program via Lei No 10.836 (2004). This legislation, which converted Medida Provisória No 132 (2003) into federal law, formally created the PBF, cancelling the older separate programs.

The original legislation established a targeted benefit structure based on distinct poverty thresholds: a monthly unconditional basic benefit of R\$ 50.00 for families in extreme poverty, and a monthly conditional variable benefit of R\$ 15.00 for poor families, based on the number of children or pregnant women. These variable benefits are strictly conditional on the family following human capital requirements, specifically school attendance for children and regular health checks. Originally, the conditional transfer was capped at a maximum of 3 children, penalizing larger families.

A key structural feature of the PBF, which has significant implications for empirical analysis and program coverage, was that families could meet the eligibility criteria without necessarily becoming beneficiaries due to hard constraints on the program's size. The number of beneficiary slots at the national level was not open-ended but was set by the federal budget and historically revised every three years (MDS, 2010, 2013). This national cap was subsequently divided into municipal quotas. While these are not utilized as strict administrative caps, they effectively determine the allocation of available slots across municipalities. Slots that are not yet assigned, or that reopen when a family leaves the program, are allocated to municipalities based on a priority ratio, calculated as the number of current beneficiaries divided by the quota, and the level of unmet demand, defined by the number of eligible families not yet receiving benefits (MDS, 2008). Consequently, an eligible family can typically only access the program if there are available slots assigned to their specific municipality. However, the rigidity of this mechanism evolved significantly, that evolution will be further analysed in Chapter 3.

This quota system emphasizes the importance of the 2009 revision, which Gerard et al. (2024) identify as the largest expansion in the national number of slots in the program's history. This expansion was not merely a budgetary increase but involved a significant

methodological shift in how municipal quotas were calculated. The government updated the "municipal estimates of poor families" by integrating data from the 2006 National Household Sample Survey (PNAD) and the 2000 Census to recalibrate poverty estimates, replacing older methodologies. This administrative adjustment created substantial variation in the additional number of PBF beneficiaries across municipalities. A more detailed explanation of this change in methodology and its impact to different municipalities will be presented in Chapter 3.

Following this period of consolidation and expansion, a major legislative evolution occurred in 2012 with the acting of Lei No 12.722 (2012), which instituted the Ação Brasil Carinhoso. This legislation signalled a conceptual shift from merely alleviating poverty to overcoming extreme poverty, particularly for families with young children. The crucial innovation of Brasil Carinhoso was the removal of the benefit ceiling for families with children aged 0 to 6 years, authorizing a supplementary transfer calculated to fill the exact gap between the family's income and the extreme poverty line. This mechanism ensured that, theoretically, no beneficiary family with young children would remain statistically in extreme poverty.

This trajectory faced a significant break in 2020 when the PBF regular execution was suspended to accommodate the Auxílio Emergencial which provisionally increased PBF transfers and suspended conditions to mitigate the economic impact of the COVID-19 pandemic (Lei No 13.982, 2020). In 2021, the program was formally extinguished and replaced by the Programa Auxílio Brasil (Lei No 14.284, 2021). The new program introduced a phase-out mechanism where a family with an income per person greater than the poverty line (and lower than 2.5 times it), could receive the benefit for 24 months. Also, in 2022, the complementary law *Lei* N° 14.342 (2022), add a minimum benefit of R\$ 400 for all families. The return of the PBF was subsequently enacted by Lei No 14.601 (2023) in 2023, with small changes (see in annex a table with the historical changes cited here).

2.5.2 The PBF's Impact

For the direct impact of the PBF, the program's impact on health has been significant on the poor. Regarding child survival rate, as denoted by Ramos et al. (2021) propensity score model, the PBF participation leads to a 17% reduction in mortality for children aged 1 to 4 years, with even stronger protective effects observed among preterm children, children of Black mothers, and those residing in the poorest municipalities. Yet, the nutritional outcome

is mixed. As showed by Falcão et al. (2025), the program decrease the likelihood of stunting by 17% in children under five, and also positively decrease the rate of obesity in vulnerable children (with less educated mothers) by 12%. However, with children from mothers with 8 years or more of education, it increases the likelihood of obesity by 9%, leading to a negative impact in children's health.

In terms of the effect in education, the PBF has impacted both in short and long term. According to Glewwe & Kassouf (2012), using school census data from 1998 to 2005, found that the Bolsa Escola/PBF increased enrolment after three years of benefit by approximately 5.5% for children in grades 1–4 and 6.5% for those in grades 5–8. Those estimates are calculated on the average treatment effect (include both participant and non-participants), so by assuming no spillovers and knowing the rate of participation in data is around 30%, the effect of participant children in grade 1-4 (5-8) was around 17% (20%). The program also decreased the dropout rate and increased the promotion rate of the children.

These short-term gains have translated into lasting structural changes. As showed by Britto et al. (2025)³, boys and girls exposed to the program's conditionalities between ages 10 and 15 attained 1.3 (14%) and 1.9 (19%) more years respectively of education compared to non-beneficiaries, boys (girls) achieved high school completion rates 15% (21%) higher, and both genders were between 9 to 11 percentage points more likely to complete a college degree, which considering the groups base rate represented a more than 100% increase. Another interesting take is that the results for partially treated children (older children with less years of benefit and not impacted by conditionalities, only by the income effect, ages between 16 and 21) were weaker, with effects on years of education smaller than 5%.

That positive result in increasing human capital also converted into economic gains. Britto et al. (2025) results on formal jobs, show an increase in the probability of holding at least one formal job among ages 23 and 25 between 8 and 14 p.p. for fully treated and 5p.p. for partial ones. Fully treated participants also showed an increase income to around 2300 reais/year (30%) for men and 1450 reais/year for women. That rise in revenue transferred to an increase of chance in social mobility, with fully treated having a 4 p.p. (30%) lower likelihood of being part of the program in the adulthood.

Besides those direct effects there are also effects from the increase in demand

³ Paper assumes no spillover effects, and rate of participation in data is around 50%, so they use the formula *Effect on beneficiaries = Total effect/~50%*.

generated by the program. As Gerard et al. (2024) suggests the PBF stimulates labour supply and economic activity. This labour market stimulation contributes to a broader local economic multiplier effect, for every \$1 transferred by the program, an estimated MVPF of \$1.49 is generated (considering a concentration of those newly created jobs in low wage), largely driven by increased demand in the non-tradable sectors. The authors also claim that the result would be even greater if considering the informal sector. Other researched indirect effects of the program consist of a reduction in crime in school neighbourhoods after an expansion of the PBF (Chioda et al., 2016) and a reduction in the vulnerability to clientelism, as beneficiaries are more likely to reject vote-buying offers (Phillips, 2025).

Chapter 3. Methodology

This dissertation estimates the long-run effects of differential exposure to the 2009 expansion of PBF on local economic activity, the sectoral composition of gains, and non-income welfare outcomes. As explained in Section 2.5.1, until 2012 the PBF had a municipal quota system based on estimates of the number of families below the poverty line. Under this system, even if a family could prove that it was below the poverty line, that would not guarantee entry into the program, since the municipality where that family lived also needed to have a spot available under its quota. In 2009, this quota was expanded due to the work of Soares (2009), who showed that, because of income volatility, the real number of people below the poverty line was greater than what could be observed in a specific period of time (MDS, 2009, as cited in Gerard et al., 2024). Besides the expansion itself, since the last federal census had been conducted in 2000, the government also adopted a new poverty estimation methodology created by Elbers et al. (2003). This methodology made it possible to use the PNAD 2006 data, which were available only at the state level rather than at the municipal level, and, by comparing similar variables shared by the 2000 Census and the 2006 PNAD, multiplying by population growth from 2006 to 2009, and by 1.18, the factor proposed by Soares (2009), estimate municipal poverty in 2009 (MDS, 2012, as cited in Gerard et al., 2024).

These changes in methodology, however, generated different increases in quotas across municipalities. Exploiting that variation, we create a dummy variable, our treatment variable, for the municipalities in the top 50% of the increase in beneficiaries per state⁴. That way, we can apply a difference-in-differences model to estimate the impact of that differential variation.

The empirical strategy, thus, is an event-study difference-in-differences model estimated on a municipality-by-year panel from 2007 to 2019. For outcome y_{mst} in municipality m , state s , and year t , the main specification is (for y_{mst} as municipal GDP, tax-revenue or formal employment change):

⁴ Gerard et al. (2024) uses the top 50% of the quota increase instead of the real increase in beneficiaries but that data is not publicly available.

$$\begin{aligned}
y_{mst} = & \alpha_m + \phi_{st} + \sum_{t \neq 2008} \beta_t (\text{Treat}_m^{2009} \times 1[\text{Year} = t]) \\
& + \sum_k \sum_{t \neq 2008} \gamma_{k,t} (X_m^k \times 1[\text{Year} = t]) + \varepsilon_{mst} \quad (1)
\end{aligned}$$

The specification includes Municipality fixed effects α_m to control for time-invariant differences across municipalities (such as the geography), and state-by-year fixed effects ϕ_{st} to absorb shocks common to all municipalities within a state each year, including statewide macroeconomic conditions and program evolution.

The treatment variable is a predetermined indicator for whether a municipality received a high 2009 quota shock, which is held fixed over time. The model estimates a separate treatment effect β_t for each year relative to 2008, producing a dynamic event-study profile.

To reduce the risk that the β_t coefficients reflect pre-existing differences in municipal trajectories rather than the consequences of the 2009 expansion, we condition the analysis on predetermined municipal characteristics measured before the reform, denoted by X_m^k , and allow their association with outcomes to vary year by year. In our baseline specification, X_m^k includes all predetermined programme-related variables used to capture baseline exposure to Bolsa Família and the counterfactual evolution of beneficiaries under the pre 2009 rule. We do this because municipalities entered Bolsa Família with different initial programme intensity and, under the earlier allocation regime, would also have faced different mechanically predicted growth. If we ignored these baseline differences, the event study could mistakenly attribute to the 2009 methodological break outcome dynamics that were already implied by where municipalities started in the programme or by how quotas would have evolved even without the change.

Concretely, we include the initial programme scale and the municipality's predicted quota growth under the pre-2009 rule and we interact each of these baseline covariates with year indicators. This choice means that municipalities with higher baseline exposure are not forced to follow the same evolution as municipalities with lower baseline exposure in every year. Instead, the data are allowed to reflect that these groups may follow systematically different time paths, and we let those predictable differences appear through year-specific coefficients on the interacted baseline covariates. These year-specific coefficients are not the focus of the analysis. Their role is to absorb differential time patterns that are plausibly linked

to pre-reform programme position.

With this structure in place, the treatment effects are identified from deviations beyond those predictable trajectories. After accounting for time-invariant municipal characteristics and for shocks shared by all municipalities within a state each year, we ask whether treated municipalities experience additional changes relative to control municipalities that cannot be explained simply by baseline programme intensity or by the program growth they would have been expected to receive under the earlier rule. Finally, we cluster standard errors at the municipality level so that inference remains valid when unobserved factors are correlated within municipalities over time.

We use the year-by-year treatment effects from the event study to study not only short-run impacts, but also whether the effects of exposure to the 2009 expansion persist over time. We define the long-run analysis in terms of persistence because the institutional environment does not remain fixed after 2009. Gerard et al. (2024) describe the 2009 revision as the only episode that produced a sharp national increase in the aggregate number of beneficiaries, while the 2012 revision is characterized as a catch-up to the program's realized size rather than a new sharp national break. Even so, allocation rules and implementation can evolve after 2012, so we interpret coefficients after 2012 as persistence of exposure to the 2009 shock under subsequent program adjustments, not as effects under an unchanged assignment regime.

To keep this interpretation grounded in the data, we estimate a dose-event study by applying the same specification to Bolsa Família payments. This gives us, for each year through 2019, the difference in transfer intensity between treated and control municipalities implied by the 2009 shock. We then read the outcome dynamics alongside this dose path. If the transfer difference remains large, persistence in outcomes is naturally interpreted as operating through sustained differences in transfer intensity. If the transfer difference narrows over time, but outcomes remain separated, this points to longer lasting consequences of the initial expansion that go beyond contemporaneous differences in transfers.

For disciplined long horizon inference, we summarize the event study profile using pre-specified horizon averages. We define the short-run effect as the average of the estimated coefficients for 2010 and 2011. We define the long-run persistence effect as the average of the estimated coefficients from 2014 through 2019. We report these summaries together with the full event study path and use them to assess whether effects persist and whether the long-

run differs from the short-run ones.

All additional analyses follow the same identification strategy and the same discipline in interpreting dynamics. In the sectoral analysis, we estimate the model separately for tradable and non-tradable formal employment to assess whether the pattern of gains persists over longer horizons. In the non-income analysis, we apply the framework to public health expenditure and female homicide outcomes.

3.1 Sectoral analysis

The sectoral analysis tests whether the long-run effects of exposure to the 2009 PBF expansion are concentrated in non-tradable activities and whether non-tradables can account for the aggregate employment effect in the long run. Sectors are classified using a fixed mapping from industry codes into tradable and non-tradable groups held constant over 2007–2019. Following Gerard et al. (2024), non-tradables are defined as activities primarily serving local demand, such as commerce and locally provided services, while tradables are defined as activities whose output is more plausibly sold outside the municipality, such as agriculture and manufacturing.

Let Emp_{mt}^{All} be total formal employment and Emp_{mt}^{NT} non-tradable formal employment in municipality m and year t , with 2008 as the omitted baseline year. Define the aggregate scaled change as

$$C_{mt}^{All} = (Emp_{mt}^{All} - Emp_{m,2008}^{All}) / Emp_{m,2008}^{All} \quad (2)$$

and the non-tradable contribution as

$$C_{mt}^{NT} = (Emp_{mt}^{NT} - Emp_{m,2008}^{NT}) / Emp_{m,2008}^{All} \quad (3)$$

The event-study specification in the main text is estimated separately for C_{mt}^{All} and C_{mt}^{NT} , producing year-specific coefficients β_t^{All} and β_t^{NT} . Long-run persistence is summarized by

$$LR^{All} = \frac{1}{6} \sum_{t=2014}^{2019} \beta_t^{All} \text{ and } LR^{NT} = \frac{1}{6} \sum_{t=2014}^{2019} \beta_t^{NT} \quad (4)$$

Using a similar approach to Gerard et al. (2024), but for the long-run, non-tradables

are interpreted as accounting for the aggregate effect when LR^{NT} is close to LR^{All} , this is evaluated with the restriction test $H_0: LR^{All} - LR^{NT} = 0$.

3.2 Descriptive analysis

In Table 3.1, we present descriptive statistics for the main variables used in the empirical analysis. Our working database is an annual municipality-by-year panel covering Brazilian municipalities from 2007 to 2019. To keep the discussion focused and the table readable, we report only the core measure associated with each substantive dimension of the analysis and omit mechanically transformed variants of the same underlying series. Thus, when a variable later appears in normalized form, such as per capita or per 100,000 inhabitants, these measures are constructed from the same underlying information rather than treated as separate variables.

We assemble the panel by combining several administrative and official statistical sources. We obtain Bolsa Família information from the administrative records made available by the Ministério do Desenvolvimento e Assistência Social. We construct the female homicide measure from the Sistema de Informações sobre Mortalidade. We derive formal labour-market variables from RAIS microdata. We assemble municipal health expenditure from public accounts data provided by the Secretaria do Tesouro Nacional, combining FINBRA for the earlier years with SICONFI for the later period. We obtain federal income tax collection from Receita Federal do Brasil, while municipal GDP, tax on goods and services and the population denominators used to normalize selected variables come from IBGE SIDRA.

Because these sources differ in temporal coverage and completeness, the effective sample is not identical across variables. For this reason, Table 3.1 is constructed using a variable-specific balanced-sample rule. For each variable, we retain only those municipalities with non-missing information for that same variable throughout 2007–2019. This procedure allows us to preserve the broadest informative sample for each series, rather than imposing a single common balanced sample driven by the most incomplete source. The municipality count reported in the table should therefore be interpreted as the effective cross-sectional support underlying each variable in the subsequent analysis.

Table 3.1 Descriptive statistics of the main variables

Variable			Mun.	Mean	SD	Min	Max	Source
Bolsa Família beneficiary families, monthly mean			5,564	2,344.37	7,958.31	1.00	490,938.58	MDS
Bolsa Família transfers, annual total (BRL)			5,564	3,921,907.29	13,170,026.13	0.00	889,404,705.00	MDS
Female homicides, municipality of residence			5,570	0.79	4.22	0.00	197.00	SIM
Formal private employment			4,217	3,181.61	41,257.92	0.00	4,139,999.00	RAIS
Tradable formal private employment			4,217	793.52	5,111.62	0.00	508,517.00	RAIS
Non-tradable formal private employment			4,217	2,084.20	33,136.88	0.00	3,506,662.00	RAIS
Municipal health expenditure per capita (BRL)			4,434	568.71	370.32	0.00	38,530.86	STN
Income tax per capita (BRL)			5,209	213.31	794.85	0.07	38,547.91	RFB
Tax on goods and service per capita (BRL)			5,541	1,242.67	2,669.09	4.07	118,420.54	IBGE
Municipal GDP per capita (BRL)			5,563	16,895.82	19,562.50	1,560.12	815,697.80	IBGE

Note: Mun. reports the number of municipalities retained for each variable under the variable-specific balanced-sample rule applied over 2007–2019. MDS = Ministério do Desenvolvimento e Assistência Social, Família e Combate à Fome. SIM = Sistema de Informações sobre Mortalidade. RAIS = Relação Anual de Informações Sociais. STN = Secretaria do Tesouro Nacional, combining FINBRA and SICONFI. RFB = Receita Federal do Brasil. IBGE = Instituto Brasileiro de Geografia e Estatística. Population is used as the denominator in normalized variables, but it is not shown as a separate row here because the compact table reports only the core outcome and input series.

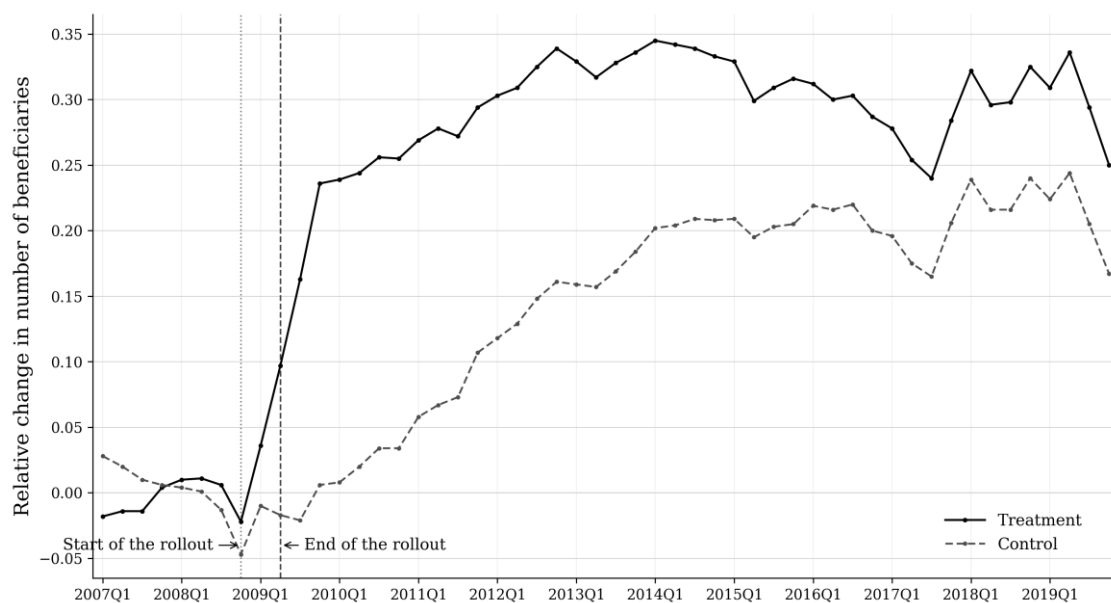
Chapter 4. Empirical Results

This chapter presents the empirical results of this dissertation. It first examines the evolution of beneficiary incidence in treated and control municipalities, then analyses the effects on GDP per capita, tax revenue per capita, and formal employment. It also examines the sectoral composition of the employment response and extends the analysis to health expenditure and female homicides. Together, these results show whether greater exposure to the 2009 Bolsa Família expansion was associated with persistent differences in local outcomes through 2019.

4.1 Evolution of the treatment differential in PBF beneficiaries

Before turning to the effects of the 2009 reform on local economic outcomes, it is essential to document how the number of Bolsa Família beneficiaries evolved in municipalities with greater and lower exposure to the quota reallocation. The treatment is defined using the 2009 quota shock, but the empirical results that follow are estimated over a much longer horizon. For that reason, it is important to know first if the shock generated a large difference in beneficiaries' increase, and second whether this difference persisted over time.

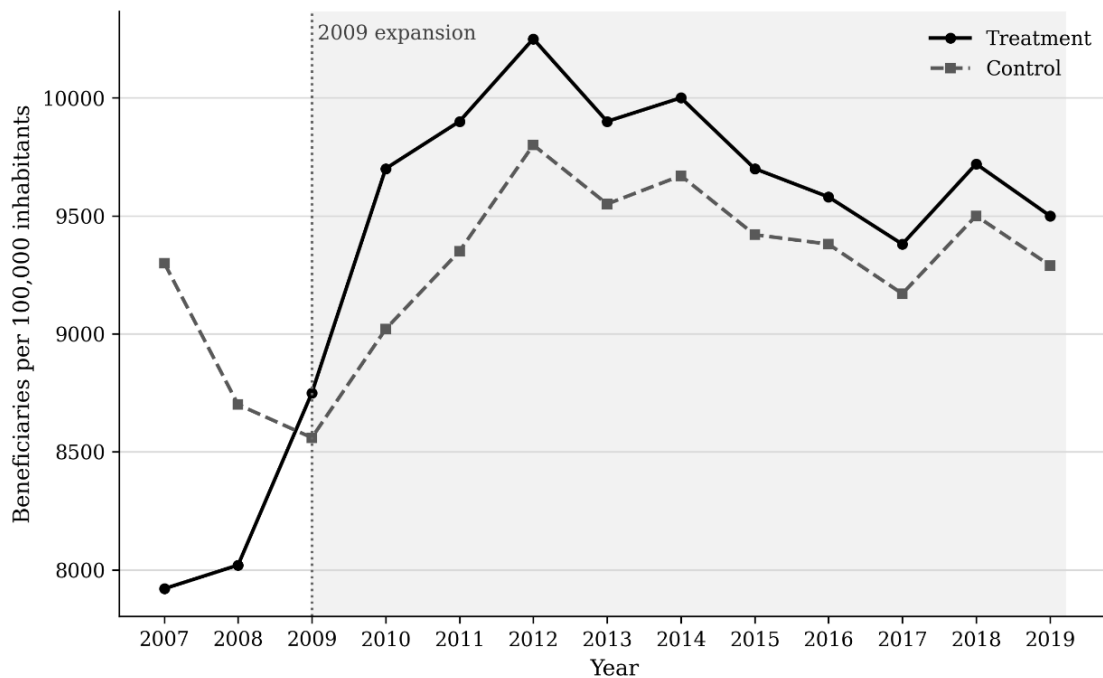
Figure 4.1 Relative change in the number of PBF beneficiaries, by treatment intensity



Reference: Own elaboration.

Figure 4.1 addresses the first of these questions. It plots the relative change in the number of beneficiaries, at quarterly frequency, for municipalities in the top and bottom halves of the treatment-intensity distribution. Prior to the rollout, the two series remain close to zero and follow broadly similar movements. Once the 2009 expansion begins, however, they diverge sharply. Municipalities in the top 50 per cent of the treatment-intensity distribution exhibit a much larger increase in beneficiaries during the implementation period. The figure therefore provides direct descriptive evidence that the 2009 reform generated a differential increase in realized program exposure across municipalities.

Figure 4.2 Mean PBF beneficiaries per 100,000 inhabitants in treated and control



Reference: Own elaboration.

The quarterly evidence is informative about timing, but it is not sufficient for the purposes of the dissertation. The central question here concerns persistence: whether the difference created during the rollout was transitory or whether it remained in place over the following years. Figure 4.2 addresses this issue by plotting the annual mean number of beneficiaries per 100,000 inhabitants in treated and control municipalities from 2007 to 2019. This normalization is the relevant one for the subsequent analysis, since it places beneficiary incidence on a per-capita basis and therefore makes the comparison more consistent with the later outcome measures.

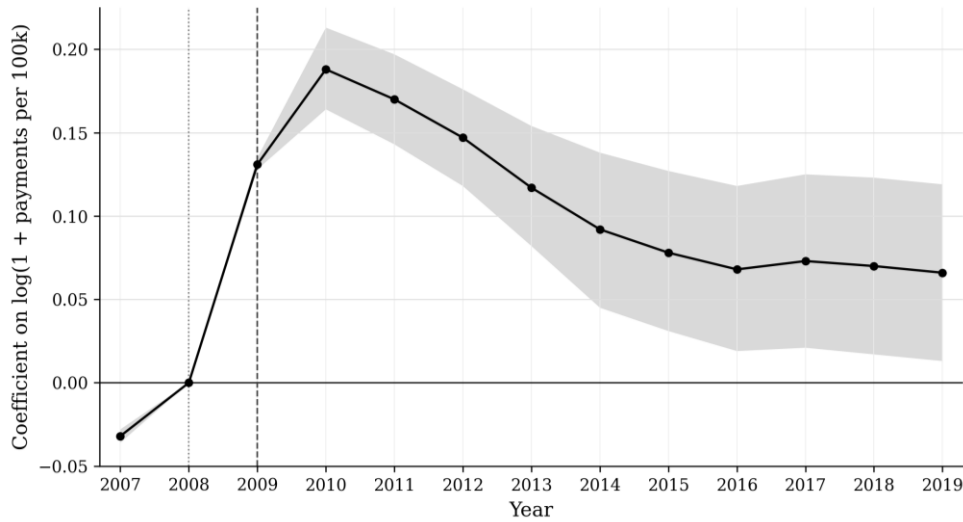
Two features of Figure 4.2 are especially important. First, prior to the reform, treated municipalities display lower beneficiary incidence than control municipalities. This ordering reverses in 2009, due to the change in methodology for the quota allocation. Second, after the reversal, treated municipalities follow a systematically higher path throughout the remainder of the sample. The gap widens in the immediate post-reform years, remains substantial through the middle of the period, and, although it narrows, it does not disappear by 2019. In other words, the differential created around the 2009 expansion is not limited to the implementation period, it remains visible over the long run.

This persistence is central for the interpretation of the estimates reported in the next sections. If the reform had generated only a brief divergence in beneficiary incidence, the later coefficients for GDP, tax revenue, employment, and other outcomes would be interpreted mainly as long-run consequences of a one-off shock. The descriptive evidence presented here suggests a different configuration. The initial divergence created during the rollout is followed by a persistent difference in beneficiary incidence between treated and control municipalities. As a result, the post-2012 outcome estimates should not be read solely as delayed effects of a temporary increase in beneficiaries in 2009. Rather, they reflect long-run differences in outcomes observed in a setting where municipalities more exposed to the reform continued to display higher program intensity, although at a lower differential than in the immediate post-reform years.

At the same time, Figures 4.1 and 4.2 also indicate that the treatment differential changes in magnitude over time. The increase is strongest during the rollout and in the years immediately following it, while the subsequent gap is smaller but still positive. This point is relevant because it implies that the later estimates do not correspond to the same intensity of exposure observed in 2009 and 2010. The long-run analysis therefore concerns persistence under an attenuated, though still non-zero, treatment differential. This distinction will matter in the discussion chapter, where the results will be interpreted in terms of both the persistence of the original 2009 shock and the persistence of differential exposure after the reform.

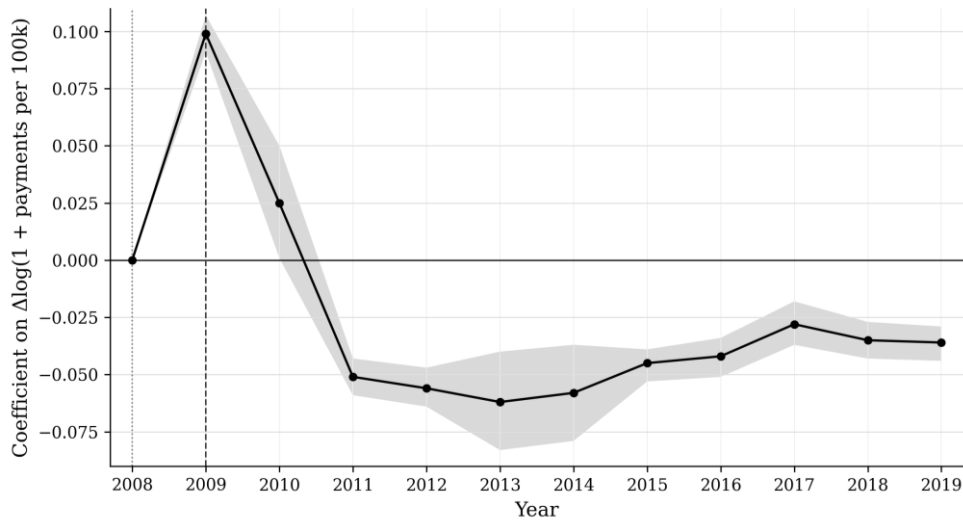
We also examine the evolution of PBF payments per 100,000 inhabitants, which provide a closer measure of treatment intensity than beneficiary counts alone. While the beneficiary figures document the persistence of differential program exposure, the payment figures show whether that differential is also visible in transfer outlays.

Figure 4.3 Event-study estimates for PBF payments: log specification



Reference: Own elaboration.

Figure 4.4 Event-study estimates for PBF payments: growth specification



Reference: Own elaboration.

Figures 4.3 and 4.4 report the event-study estimates for PBF payments per 100,000 inhabitants in the log and growth specifications. In the log specification, the coefficients become strongly positive after 2009, reach their highest magnitude in the immediate post-reform years, and then decline gradually over time, while remaining positive through the end

of the sample. The short-run average is 0.179 log points, which corresponds to about 19.6 percent, and the long-run average is 0.074 log points, or about 7.7 percent. This pattern indicates that municipalities more exposed to the 2009 reform remained on a higher path of PBF payments per capita throughout the period studied.

In the growth specification, the pattern is different. The coefficient is strongly positive in 2009, at 0.099, and remains positive in 2010, but becomes negative thereafter. The short-run average, defined over 2010 and 2011, is -0.013, while the long-run average is -0.041. Taken together, the two specifications indicate that the payment differential was concentrated in the initial post-reform adjustment and then persisted primarily as a higher level of transfers rather than as continued faster growth in payments year after year.

These payment results complement the evidence based on beneficiary counts. The two sets of figures point in the same direction. Municipalities more exposed to the reform experienced both a larger increase in beneficiaries during the rollout and a higher path of payments per capita thereafter. This is important for the interpretation of the subsequent outcome results, since it indicates that the treated-control differences in GDP, tax revenue, and employment are observed in a setting where the treatment differential remained visible not only in beneficiary incidence, but also in the monetary intensity of the program.

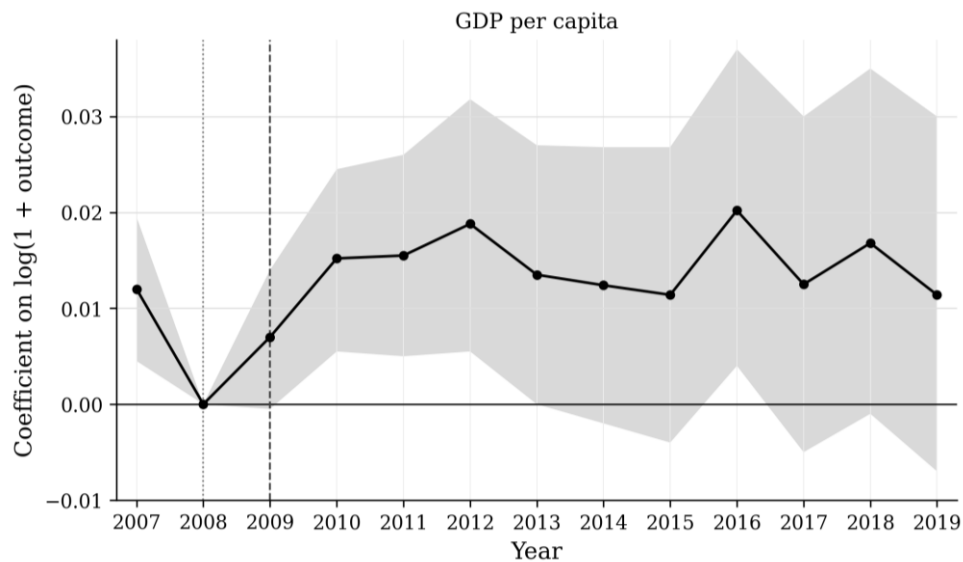
4.2 Effects on local economic activity

Having established in the previous subsection that municipalities more exposed to the 2009 reform experienced a persistent increase in beneficiary incidence, we now turn to the main economic outcomes. The focus is on three variables: municipal GDP per capita, tax revenue per capita, and private-sector formal employment per 100,000 inhabitants. Taken together, these outcomes provide a broad view of local economic adjustment. GDP per capita captures overall local economic activity, tax revenue per capita provides evidence on fiscal externalities, and formal employment measures whether the expansion was associated with changes in the quantity of local labour demand.

This subsection reports two complementary specifications for each outcome. The first uses the logarithm of the outcome, so that the event-study coefficients capture proportional differences in levels between treated and control municipalities relative to the omitted baseline year, 2008. The second uses the annual change in the logarithm of the outcome, so that the coefficients capture differences in year-to-year growth. The distinction is substantively important. In the present setting, a positive coefficient in the log specification

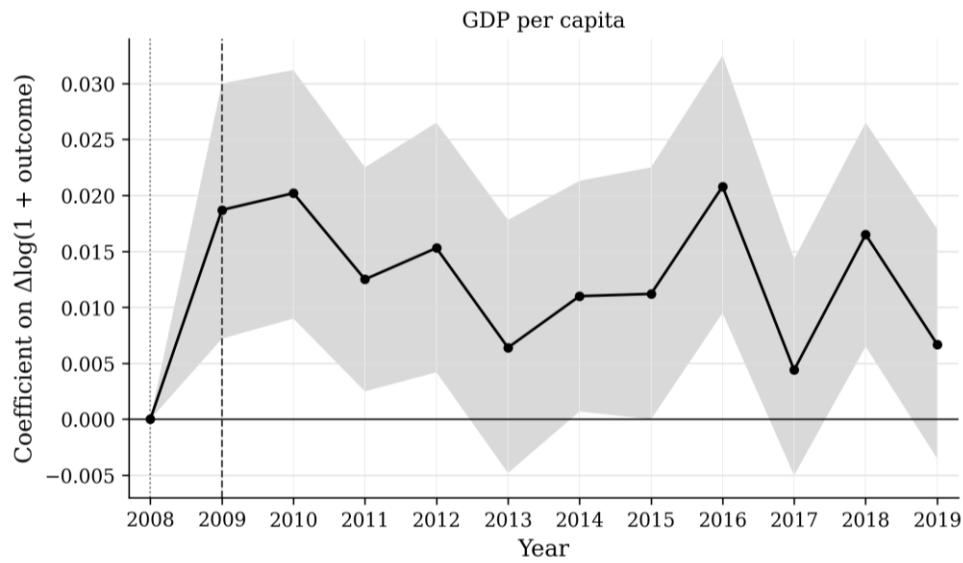
indicates that treated municipalities remain on a higher outcome path after the reform. By contrast, a positive coefficient in the growth specification indicates that treated municipalities are still experiencing faster annual expansion. These two objects need not move together. Following a large initial increase, an outcome may remain persistently above the control group in levels even if subsequent differential growth becomes small, disappears, or fluctuates around zero. For that reason, the log specification provides a direct measure of post-treatment differences in outcome levels, whereas the growth specification is informative about the timing and dynamics of adjustment.

Figure 4.5 Event-study estimates for GDP per capita: log specification



Reference: Own elaboration.

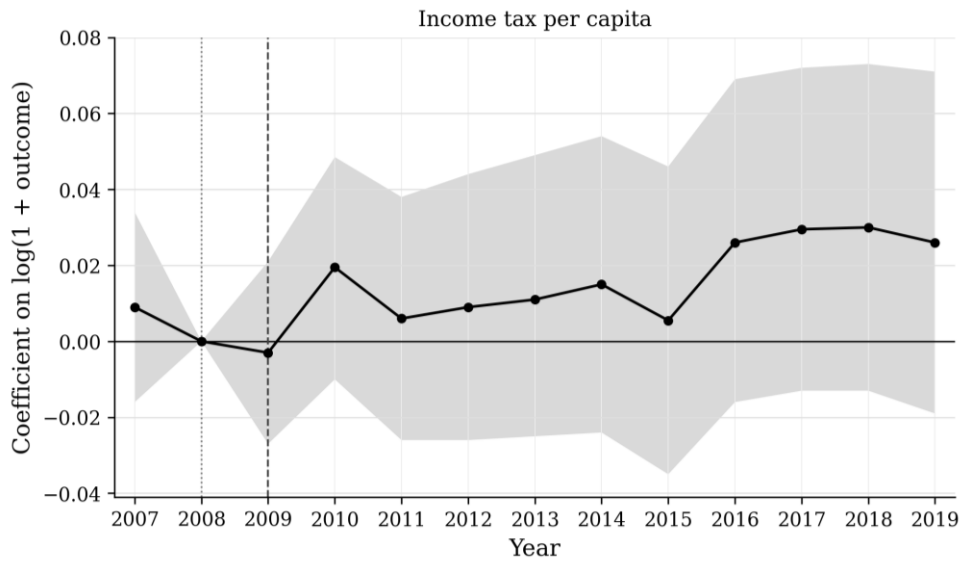
Figure 4.6 Event-study estimates for GDP per capita: growth specification



Reference: Own elaboration.

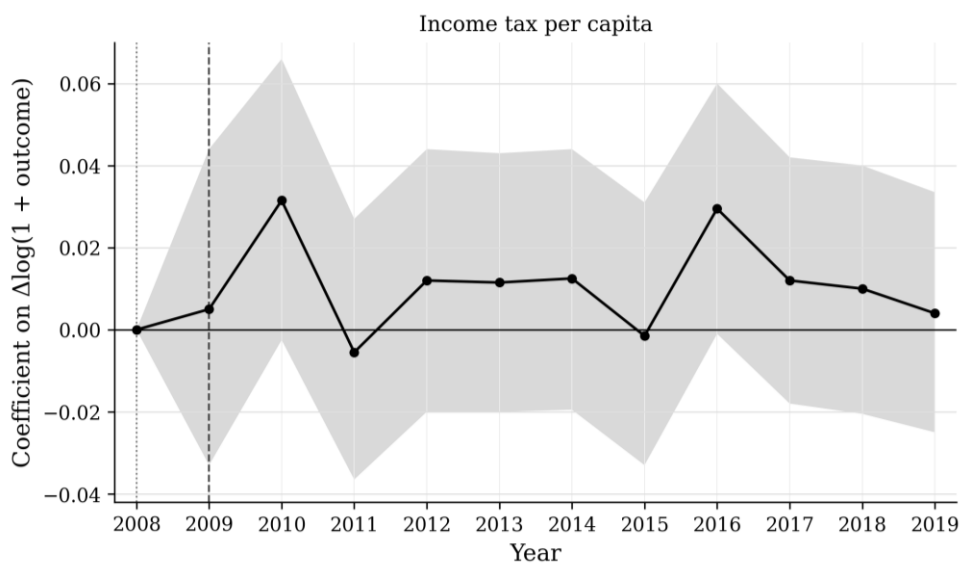
The estimates for GDP per capita suggest a modest but persistent positive response in levels. In the log specification, the coefficients become positive from 2009 onward and remain so throughout the post-treatment period. Their magnitude is generally stable, with point estimates around one to two log points for most post-reform years. Although the confidence intervals are not tight enough to support strong year-by-year claims, the overall pattern is consistent with treated municipalities remaining on a somewhat higher GDP per capita path after the reform. The corresponding growth specification yields a different, but complementary, picture. Differential GDP growth is positive in the early post-reform years, particularly between 2009 and 2012, but later estimates become smaller and less stable. Read jointly, the two specifications indicate clearer post-treatment differences in levels than in annual growth rates.

Figure 4.7 Event-study estimates for income tax per capita: log specification



Reference: Own elaboration.

Figure 4.8 Event-study estimates for income tax per capita: growth specification

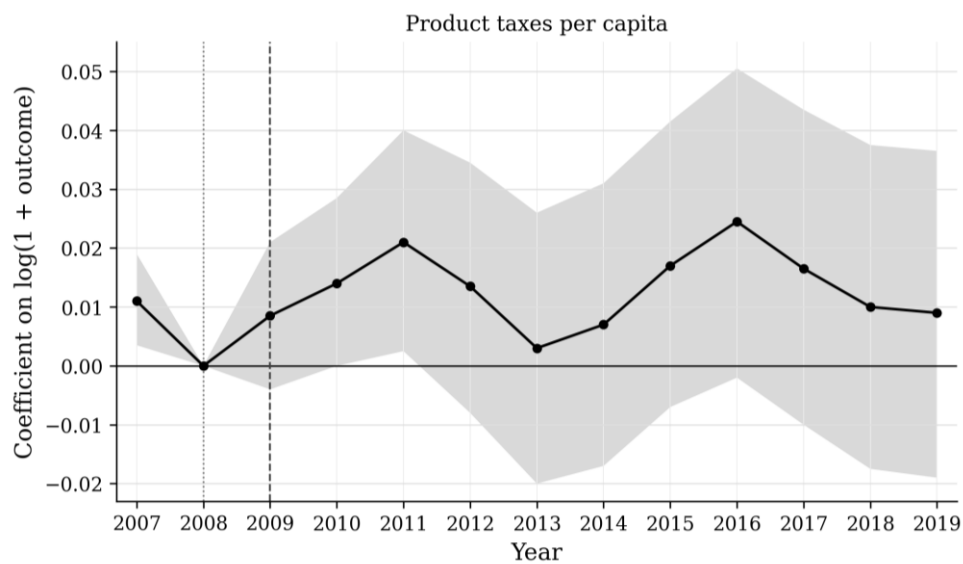


Reference: Own elaboration.

The results for income tax per capita are more cautious. In the log specification, the post-treatment coefficients are generally positive after 2010 and become somewhat larger in the later part of the sample. At the same time, the estimates are imprecise and the confidence intervals remain wide throughout, so the evidence is weaker than for GDP. The growth specification yields a similar pattern. Differential tax growth is close to zero in most years,

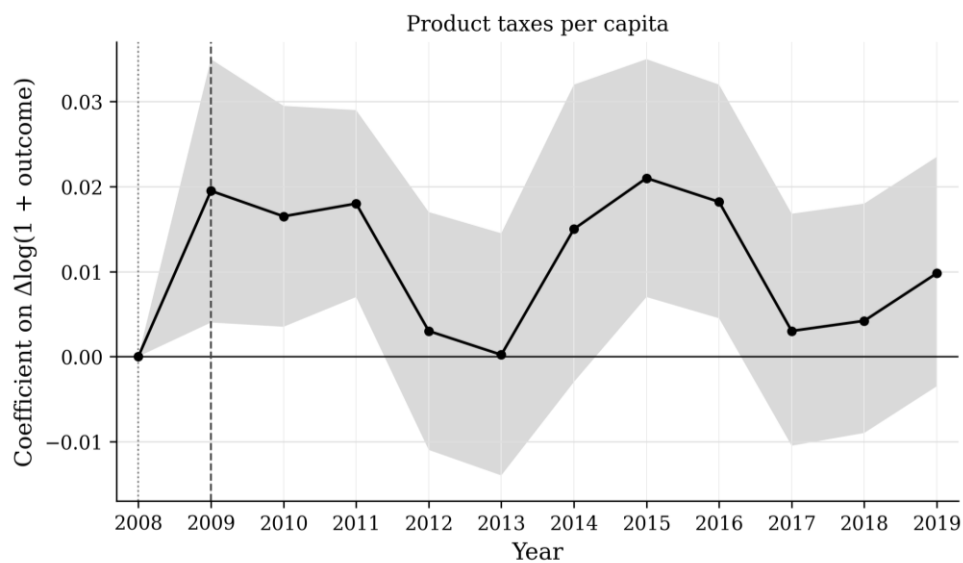
with only a few isolated positive movements. Taken together, the two tax specifications show weaker and less precise post-treatment differences than those observed for GDP.

Figure 4.9 Event-study estimates for tax on goods and service per capita: log specification



Reference: Own elaboration.

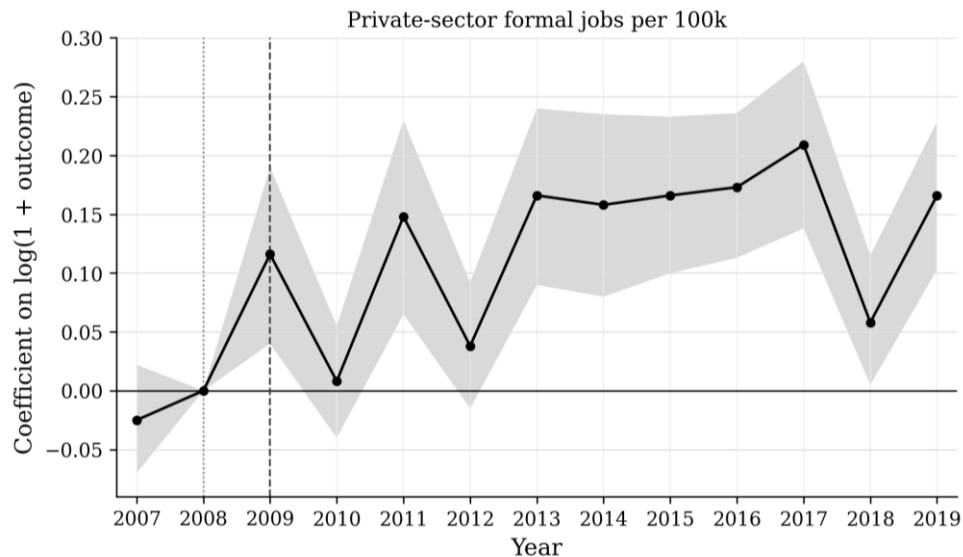
Figure 4.10 Event-study estimates for tax on goods and service per capita: growth specification



Reference: Own elaboration.

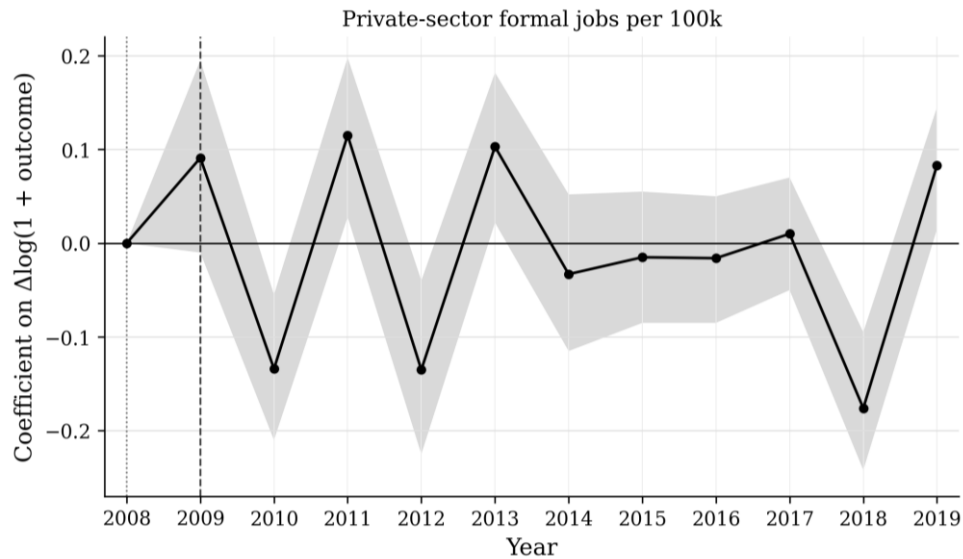
The results for tax on goods and service per capita display a pattern that differs from both GDP and income tax. Relative to GDP, the evidence is less coherent and provides weaker support for a sustained post-treatment divergence. Relative to income tax, however, the pattern is not simply weaker, but somewhat different. In the log specification, the post-treatment coefficients are positive in most years, which suggests that treated municipalities may have experienced somewhat higher levels of product-tax revenue per capita after the 2009 expansion. Yet this pattern is not as clearly persistent as the one observed for GDP. In the growth specification, the coefficients are positive only in some periods and remain close to zero in others, indicating episodic rather than sustained differential growth. Taken together, the two specifications suggest that product taxes occupy an intermediate position: the evidence is less convincing than for GDP, but somewhat more recurrent than the one found for income tax, while still too irregular and imprecise to support a strong conclusion about a stable long-run effect.

Figure 4.11 Event-study estimates for private-sector formal employment: log specification



Reference: Own elaboration.

Figure 4.12 Event-study estimates for private-sector formal employment: growth specification



Reference: Own elaboration.

Private-sector formal employment presents the clearest evidence of a persistent level effect. In the log specification, the coefficients turn positive after 2009 and remain positive throughout the remainder of the sample, with larger magnitudes from 2011 onward. The point estimates indicate that treated municipalities experienced a sustained increase in formal private employment per capita relative to the control group. By contrast, the growth specification for formal employment is considerably more volatile (the estimated coefficients alternate between positive and negative values). Taken together, the two specifications again show clearer differences in levels than in annual growth rates.

Altogether, the level and growth specifications point to a common pattern. For GDP per capita and formal employment, the post-treatment differences are more evident in levels than in growth rates. Tax revenue per capita follows the same qualitative direction, but with substantially greater uncertainty. This distinction between level effects and growth effects is important for the remainder of the chapter. Since Section 4.1 showed that the treated-control difference in beneficiary incidence remained positive after the reform, the economic results reported here are observed in a setting where the difference in program exposure also persisted over time.

Table 4.1 Short-run and long-run effects on local economic activity

Outcome	<i>Log SR</i>	<i>Log LR</i>	<i>Growth SR</i>	<i>Growth LR</i>	<i>N municipalities</i>	<i>Municipality-years (log)</i>	<i>Municipality-years (growth)</i>
GDP per capita	0.015*** (0.005)	0.014* (0.008)	0.016*** (0.005)	0.012*** (0.004)	5,563	72,319	66,756
Income Tax per capita	0.013 (0.015)	0.022 (0.020)	0.013 (0.015)	0.011 (0.013)	5,337	69,381	64,044
Product Tax per capita	0.018** (0.008)	0.014 (0.012)	0.017*** (0.006)	0.012*** (0.004)	5,563	72,319	66,756
Private-sector formal jobs	0.077* (0.030)	0.155*** (0.030)	-0.009 (0.032)	-0.025 (0.025)	4,220	54,860	50,640

Notes: $SR = \frac{1}{2}(\beta_{2010} + \beta_{2011})$ and $LR = \frac{1}{6}\sum_{t=2014}^{2019} \beta_t$. The log specification uses $\log(1 + \text{outcome})$, and the growth specification uses $\Delta \log(1 + \text{outcome})$. Standard errors in parentheses are computed from the full variance-covariance matrix of the corresponding linear combinations and are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Reference: Own elaboration.

A compact summary of these patterns is provided in Table 4.1, which reports the pre-specified short-run and long-run averages for each outcome under both the log and growth specifications. The short-run measure captures the immediate post-reform response, while the long-run measure summarizes the average effect over the later period of the sample. Presenting the estimates in this form serves two purposes. First, it avoids placing excessive weight on individual year-to-year fluctuations in the event-study coefficients. Second, it allows a direct comparison across outcomes and specifications. The table shows that the evidence of persistence is stronger in the level specification, particularly for GDP per capita and private-sector formal employment, whereas the growth specification is stronger for product taxes. Income Tax per capita follows the same broad direction, but with considerably less precision. Also, applying the same analysis for the mean wages on the private sector did not show a pattern in the log or growth specification.

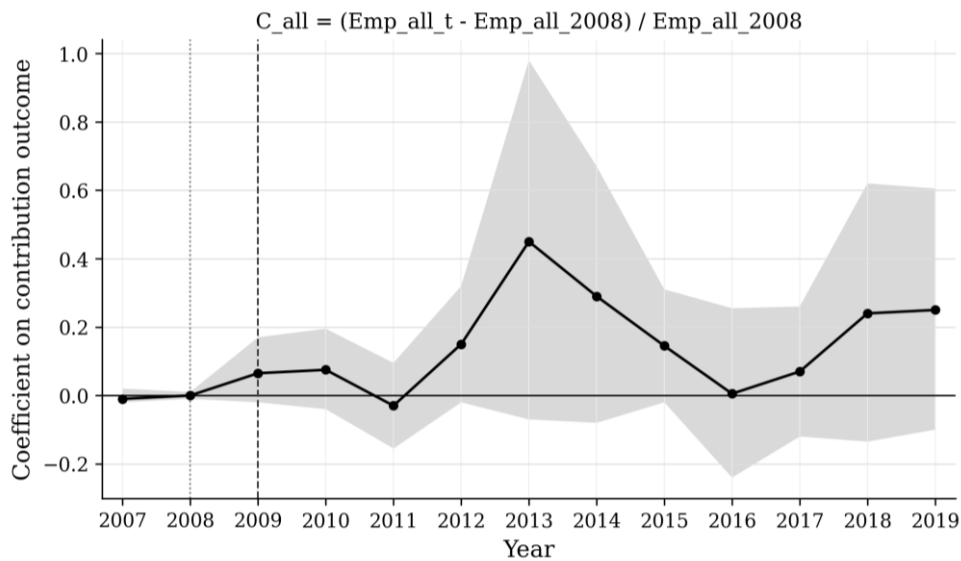
The overall evidence in this subsection therefore suggests that municipalities more exposed to the 2009 expansion experienced positive differences in local economic activity

over the subsequent decade, especially in GDP per capita, product taxes per capita and private-sector formal employment. At the same time, the sustained difference in beneficiaries was not accompanied by equally clear evidence of sustained differential growth.

4.3 Sectoral composition of the employment response

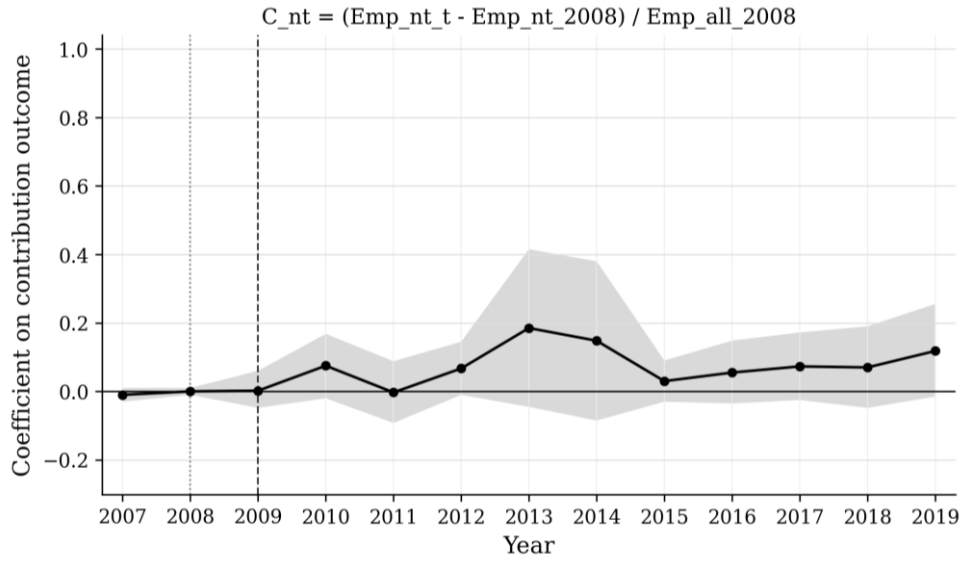
We next examine whether the aggregate employment response documented in the previous subsection can be accounted for by the non-tradable sector. To do so, we follow the contribution-based specification described in the methodology chapter. In this setup, we define the aggregate employment contribution measure C_{mt}^{All} and the non-tradable employment contribution measure C_{mt}^{NT} , both normalized by baseline aggregate employment in 2008. This allows us to compare the evolution of the aggregate employment effect with the contribution of non-tradable employment over time.

Figure 4.13 Event-study coefficients for aggregate employment contribution, C_{mt}^{All}



Reference: Own elaboration.

Figure 4.14 Event-study coefficients for non-tradable employment contribution, C_{mt}^{NT}



Reference: Own elaboration.

Figures 4.13 and 4.14 report the event-study coefficients for C_{mt}^{All} and C_{mt}^{NT} . In both cases, the coefficients are close to zero before the reform and become positive in the post-treatment period. The aggregate contribution measure, C_{mt}^{All} , shows positive coefficients in most years after 2012, with larger point estimates in the middle and later part of the sample. The non-tradable contribution measure, C_{mt}^{NT} , also becomes positive after the reform and remains above zero in most post-treatment years, although at lower magnitudes than C_{mt}^{All} in much of the sample. This pattern suggests that non-tradables account for an important part of the aggregate employment response, but that the estimated contribution path remains below the aggregate path in point estimates.

At the same time, the figures also show that the confidence intervals are wide, especially in the later years of the sample. This is important for the interpretation of the results. Although the point estimates for C_{mt}^{All} are generally above those for C_{mt}^{NT} , the degree of precision is limited, so the difference between the two series cannot be assessed from the figures alone. For that reason, we turn to the formal hypothesis test reported in the table below.

Table 4.2 Hypothesis test of whether non-tradables account for the long-run aggregate employment effect

Test	$LR^{All} - LR^{NT}$	SE	95% CI	p-value	Decision
$H_0: LR^{All} - LR^{NT} = 0$	0.082	0.075	[-0.066, 0.230]	0.277	Do not reject null

Notes: The table reports the Gerard et al. (2024) contribution test based on the normalized employment-change variables defined in the methodology chapter. LR^{All} and LR^{NT} denote the averages of the annual event-study coefficients from 2014 to 2019 for aggregate employment and the non-tradable contribution, respectively. Standard errors are computed from the full variance-covariance matrix of the corresponding linear combinations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Reference: Own elaboration.

Table 4.2 formalizes the comparison between the aggregate employment contribution and the non-tradable contribution in the long run. The estimated difference, $LR^{All} - LR^{NT}$, is 0.082, with a standard error of 0.075 and a p-value of 0.277. The null hypothesis of equality is therefore not rejected. Under this setup, this means that we do not reject the null that the long-run tradable contribution is equal to zero, or equivalently, that the non-tradable contribution is statistically indistinguishable from the aggregate long-run employment effect.

However, the equality test may have limited power because inference on the difference between the two estimated effects is based on the variance of a linear combination of coefficients, which depends on their variances and covariance. Therefore, the absence of statistical significance for the difference should be interpreted as imprecision in the estimated contrast, rather than as evidence that the effects are equal (Wooldridge, 2016). To examine whether the aggregate employment effect may be largely driven by non-tradable activities, we re-estimate the event-study model using tradable private jobs per 100,000 inhabitants as the dependent variable. To assess whether the aggregate employment effect may be driven by non-tradable activities, we re-estimate the event-study model using tradable private jobs per 100,000 inhabitants as the dependent variable. This is a more restrictive test, because it asks whether the effect on tradable employment is itself positive, rather than whether a contrast across sectoral effects is different from zero (Wooldridge, 2016). Table 4.3 presents the results.

Table 4.3 Short-run and long-run effects on Tradable private jobs

Outcome	<i>Log SR</i>	<i>Log LR</i>	<i>Growth SR</i>	<i>Growth LR</i>	<i>N municipalities</i>	<i>Municipalit y-years (log)</i>	<i>Municipalit y-years (growth)</i>
Tradable private jobs	0.113*** (0.039)	0.160*** (0.044)	0.034 (0.040)	0.010 (0.034)	4,220	54,860	50,640

Notes: $SR = \frac{1}{2}(\beta_{2010} + \beta_{2011})$ and $LR = \frac{1}{6}\sum_{t=2014}^{2019} \beta_t$. The log specification uses $\log(1 + \text{outcome})$, and the growth specification uses $\Delta \log(1 + \text{outcome})$. Standard errors in parentheses are computed from the full variance-covariance matrix of the corresponding linear combinations and are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Reference: Own elaboration.

Indeed, tradable private employment presents positive and statistically significant coefficients in the log specification in both the short run and the long run, with estimates of 0.113 and 0.160, respectively. This indicates that municipalities more exposed to the 2009 PBF expansion experienced persistently higher levels of tradable private employment. Although the coefficients in the growth specification are not statistically significant, this pattern is also present for total private employment and therefore does not appear to be specific to the tradable sector. For this reason, the main implication of these results is that the employment response was not confined to non-tradable activities. Rather, tradables also seem to have contributed to the aggregate effect, even if the available estimates do not allow a precise comparison of the relative importance of each sector.

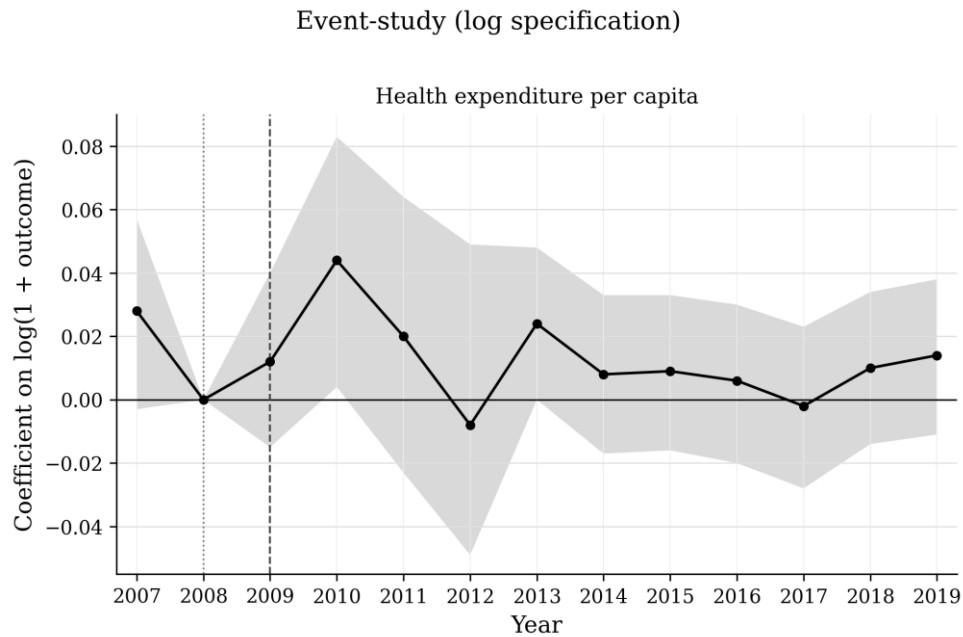
4.4 Non-income welfare outcomes

We next examine whether the effects of the 2009 expansion extended beyond local economic activity to non-income welfare outcomes. The analysis focuses on two variables: municipal health expenditure per capita and female homicides per 100,000 inhabitants. These outcomes capture different dimensions of municipal adjustment. Health expenditure per capita provides evidence on public spending, while female homicides per 100,000 inhabitants capture a broader social outcome. In order to visualize the existence (or not) of a gender equality mechanism of the program male homicides per 100,000 inhabitants is also examined.

As in the previous subsections, we report both the log and growth specifications. In

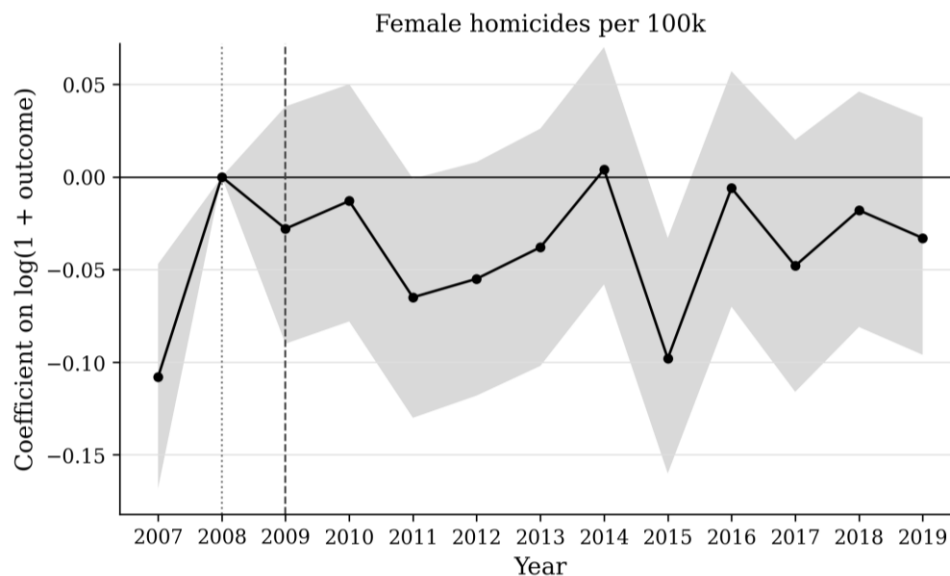
the present setting, the distinction remains important. The log specification captures proportional differences in levels between treated and control municipalities relative to the omitted baseline year, 2008, while the growth specification captures differences in year-to-year changes. The two specifications therefore provide complementary information about post-treatment dynamics.

Figure 4.15 Event-study estimates for health expenditure per capita: log specification



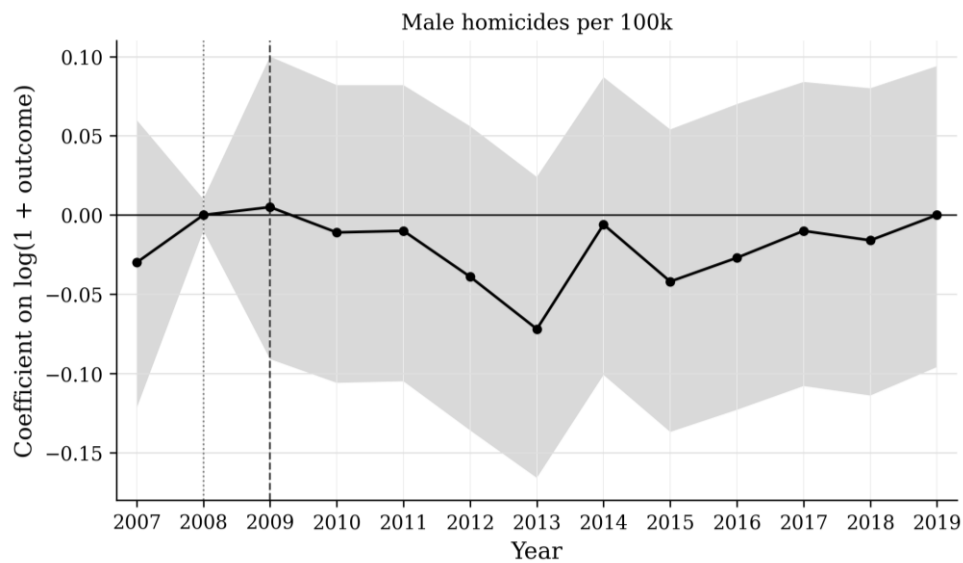
Reference: Own elaboration.

Figure 4.16 Event-study estimates for female homicides: log specification



Reference: Own elaboration.

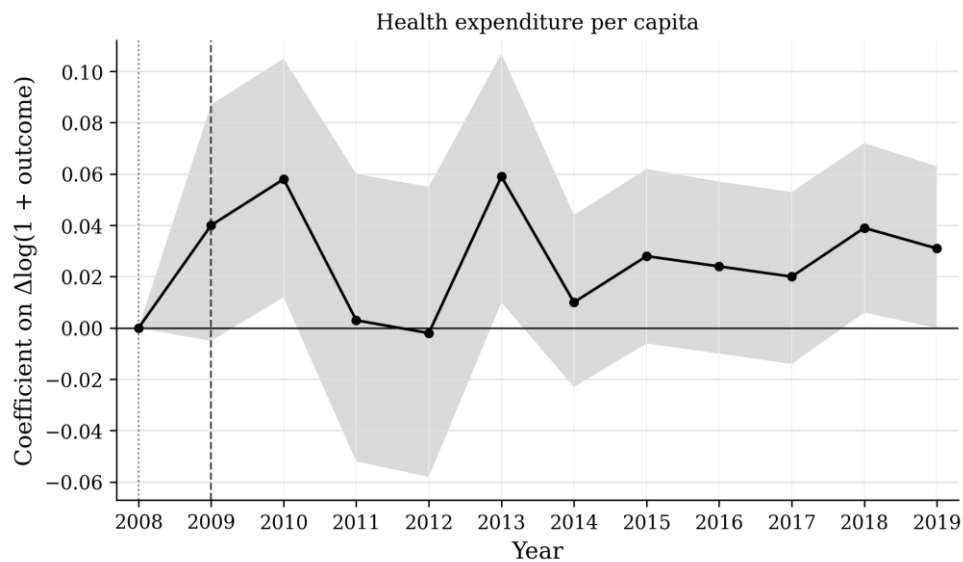
Figure 4.17 Event-study estimates for male homicides: log specification



Reference: Own elaboration.

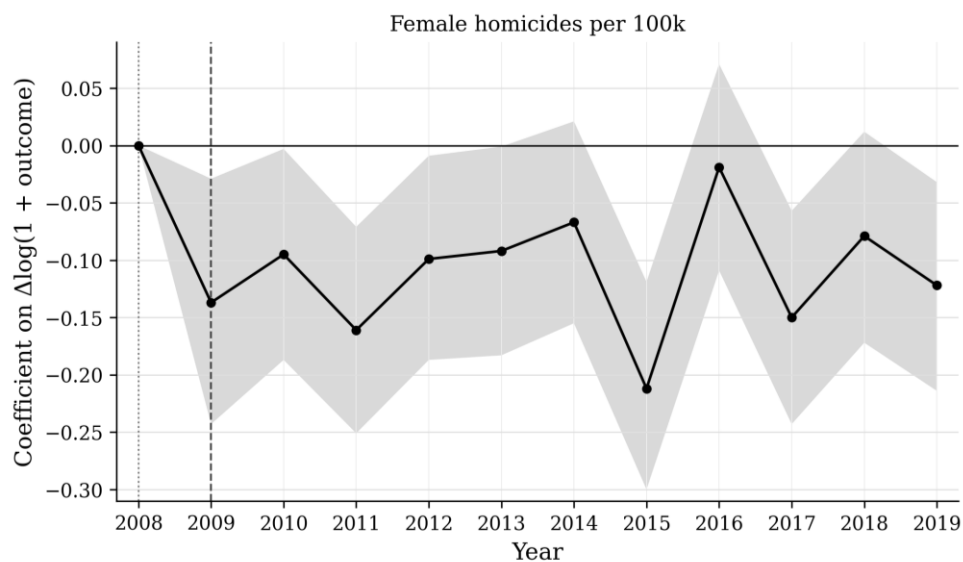
Figures 4.15, 4.16 and 4.17 report the log-specification estimates for health expenditure per capita, female homicides per 100,000 inhabitants and male homicides per 100,000 inhabitants. For health expenditure per capita, the post-treatment coefficients in the short run are positive but they become close to zero shortly after. Although there are isolated positive point estimates in some years, the confidence intervals remain wide, and the long-run estimate is close to zero. For female homicides per 100,000 inhabitants, the coefficients are negative in most post-treatment years, but they are also imprecisely estimated. The male homicides per 100,000 inhabitants present the same pattern. Taken together, the log specification provides limited evidence of persistent post-treatment differences in these non-income outcomes.

Figure 4.18 Event-study estimates for health expenditure per capita: growth specification



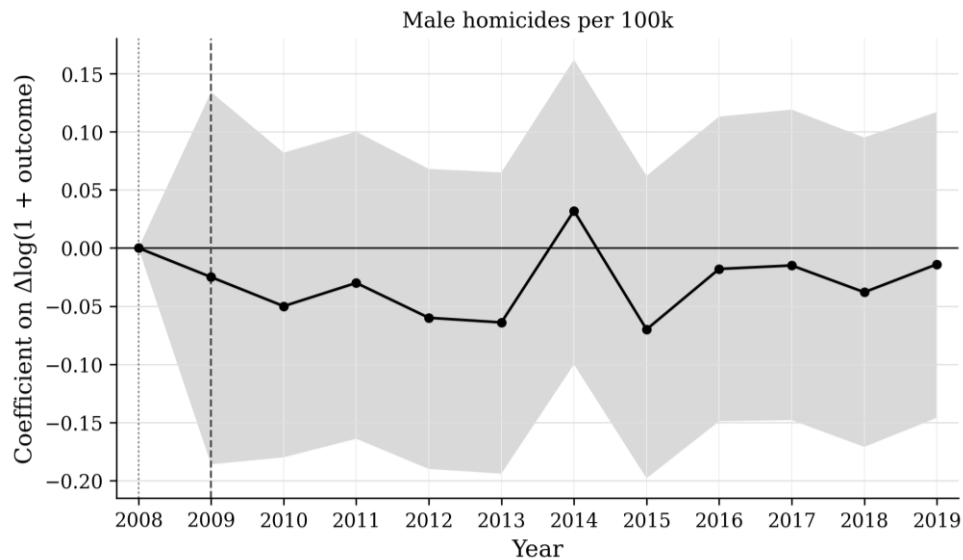
Reference: Own elaboration.

Figure 4.19 Event-study estimates for female homicides: growth specification



Reference: Own elaboration.

Figure 4.20 Event-study estimates for male homicides: growth specification



Reference: Own elaboration.

Figures 4.18, 4.19 and 4.20 present the corresponding growth-specification estimates. For health expenditure per capita, the coefficients are positive throughout the post-treatment period and are more stable than in the log specification. The growth estimates suggest a positive differential in annual changes, particularly from 2012 onward. For female homicides per 100,000 inhabitants, the coefficients are negative in all post-treatment years and considerably larger in magnitude than in the log specification. Although there is some fluctuation over time, the pattern is consistently negative. While the male homicides per 100,000 inhabitants presents a similar non-significant pattern presented in the log setup.

Table 4.4 Short-run and long-run effects on non-income welfare outcomes

Outcome	Log SR	Log LR	Growth SR	Growth LR
Health expenditure per capita	0.032* (0.017)	0.002 (0.012)	0.031 (0.019)	0.025 (0.016)
Female homicides per 100,000 inhabitants	-0.040 (0.028)	-0.033 (0.025)	-0.128*** (0.036)	-0.108*** (0.032)
Male homicides per 100,000 inhabitants	-0.011 (0.042)	-0.017 (0.038)	-0.040 (0.054)	-0.020 (0.048)

Notes: $SR = \frac{1}{2}(\beta_{2010} + \beta_{2011})$ and $LR = \frac{1}{6}\sum_{t=2014}^{2019} \beta_t$. The log specification uses $\log(1 + \text{outcome})$, and the growth specification uses $\Delta \log(1 + \text{outcome})$. Standard errors in parentheses are computed from the full variance-covariance matrix of the corresponding linear combinations and are clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Reference: Own elaboration.

Table 4.4 summarizes these results using the pre-specified short-run and long-run averages. For health expenditure per capita, the log specification yields a significant positive estimate in the short run and a close to zero in the long run, whereas the growth specification yields positive point estimates in the short-run and in the long-run but still statistically insignificant at conventional levels. For female homicides per 100,000 inhabitants, the point estimates are negative in both specifications, but the contrast across specifications is clear. In the log specification, both the short-run and long-run estimates are negative but imprecisely estimated. In the growth specification, both horizon averages are negative and statistically significant. For male homicides per 100,000 inhabitants, the points estimates are all negative but poorly estimated.

Overall, the results for the non-income welfare outcomes are more mixed than those for the economic outcomes examined in the previous subsections. For health expenditure per capita, the evidence differs across specifications, with little indication of persistent differences in levels but positive short run impact and only a point estimation in the growth specification. For female homicides per 100,000 inhabitants, the estimates are negative in both specifications, with clearer evidence in the growth specification. For male homicides per 100,000 inhabitants, there is no strong evidence of an impact.

Chapter 5. Discussion

As explained in Section 4.1, the 2009 expansion generated a treated-control difference in beneficiary and payments incidence that persisted through the end of the sample, although with lower intensity than in the immediate post-reform years. This point is central for the interpretation of the long-run estimates. The later coefficients should not be read simply as delayed effects of a one-off shock in 2009, nor as if treatment intensity had remained constant over time. Rather, they reflect long-run outcome differences in a setting where municipalities more exposed to the reform continued to display higher program exposure, although in attenuated form. Knowing that, this chapter discusses the main economic results, compares them with the findings of Gerard et al. (2024), and examines the sectoral and non-income dimensions of the longer-run response. In order to help the discussion, table 5.1 summarises the main results.

Table 5.1 Summary of main empirical results: log and growth specifications

Outcome	Log SR	Log LR	Growth SR	Growth LR	Main interpretation
PBF beneficiaries per 100k	0.213***	0.136***	0.010*	-0.032***	Treated municipalities remained on a higher payment path, but the differential weakened over time.
GDP per capita	0.015***	0.014*	0.016***	0.012***	Positive and persistent effect, with evidence in both levels and growth.
Income tax per capita	0.013	0.022	0.013	0.011	Positive point estimates but imprecisely estimated in both specifications.
Tax on goods and services per capita	0.018**	0.014	0.017***	0.012***	Stronger evidence in the growth specification than in long-run levels.
Private-sector employment	formal 0.077*	0.155***	-0.009	-0.025	Strong persistent level effect, but no evidence of sustained faster employment growth.
Tradable employment	private 0.113***	0.160***	0.034	0.010	Tradable employment also increased in levels, so employment gains were not confined to non-tradables.
Health expenditure per capita	0.032*	0.007	0.031	0.025	Some short-run evidence in levels, but no robust long-run effect.
Female homicides per 100,000 inhabitants	-0.040	-0.033	-0.128***	-0.108***	Negative effects are clearer in the growth specification than in levels.
Male homicides per 100,000 inhabitants	-0.011	-0.017	-0.040	-0.020	No robust evidence of a comparable effect.

Notes: $SR = \frac{1}{2}(\beta_{2010} + \beta_{2011})$ and $LR = \frac{1}{6}\sum_{t=2014}^{2019} \beta_t$. The log specification uses $\log(1 + \text{outcome})$, and the growth specification uses $\Delta \log(1 + \text{outcome})$. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Reference: Own elaboration.

5.1 Local economic activity in the long run

Our extension points in the same direction as Gerard et al. (2024), but the comparison becomes sharper once the estimated outcome effects are related to the corresponding difference in PBF payments. Gerard et al. (2024) report a 13.4 percent increase in PBF payments, a 2.0 percent increase in private-sector formal employment, and a 1.8 percent increase in GDP by 2011. Relative to the payment differential, these estimates correspond to about 0.15 percent more employment and 0.13 percent more GDP for each 1 percent difference in payments. They also report positive effects on taxes on good and

service in the log specification, although they do not summarize them as a direct percentage increase in taxes comparable to the employment and GDP results.

In our study, the short-run payment effect corresponds to about a 19.6 percent increase in PBF payments, while the short-run employment effect corresponds to about an 8.0 percent increase in private-sector formal employment. This implies about a 0.41 percent increase in employment for each 1 percent difference in payments, a considerably larger effect than the one reported by Gerard et al. (2024). For GDP per capita, the short-run effect is about 1.5 percent, which implies about a 0.08 percent increase in GDP per capita for each 1 percent difference in payments, leading to a result similar to that of Gerard et al. (2024). For taxes on goods and services, the short-run coefficient is 0.018 log points, or about 1.8 percent, which implies about a 0.09 percent increase in taxes on goods and services for each 1 percent difference in payments.

In the long run, however, the results change. The payment differential declines to about 7.7 percent, while private-sector formal employment remains about 16.8 percent above the control path and GDP per capita remains about 1.4 percent higher. This corresponds to about a 2.18 percent increase in employment and about a 0.18 percent increase in GDP per capita for each 1 percent difference in payments, showing an increase in the local economy effect over the longer term. For taxes on goods and services, however, the effect is not sustained with the same clarity. The long-run coefficient remains positive, at 0.014 log points, or about 1.4 percent, which would correspond to about a 0.18 percent increase for each 1 percent difference in payments, but this estimate is not statistically significant. Income taxes, which were not analysed by Gerard et al. (2024), move in the same qualitative direction as employment and GDP in the short run, but they do not provide equally strong evidence of persistence over the longer horizon (no coefficient is statistically significant).

Yet, the interpretation must be qualified by the persistence of differences in PBF payments across municipalities. Because treated municipalities continued to receive higher payments over time, the positive and significant coefficients in the log specification cannot be interpreted unambiguously as evidence of a long-run program effect. Part of this result may simply reflect the fact that treated municipalities remained on a higher transfer path, which in turn may have sustained higher outcome levels. This interpretation seems particularly plausible for private employment.

With this in mind, one of the main focuses of Gerard et al. (2024) is the positive tax response generated by the program, which they use in their welfare analysis. In our results,

nonetheless, this positive effect is statistically significant only in the short run. This raises doubts about whether the tax response reflects a broader and sustainable economy-wide adjustment, or whether it is more closely related to a monetization effect in the years immediately following the expansion.

In line with Gerard et al. (2024), the wage results do not show an effect. The estimates do not reveal a clear pattern in either the log or growth specifications over the full period. This suggests that the positive differences observed in GDP and formal employment were not accompanied by equally clear wage pressure, which is consistent with the absence of large price effects.

GDP, however, points to a somewhat different conclusion. In addition to positive coefficients in the log specification, GDP also presents a positive and significant coefficient in the growth specification (and a stable plot). This pattern is harder to attribute solely to the payment differential, since payments themselves display negative growth after the initial adjustment. Taken together, these results suggest that, although part of the long-run effect may reflect the persistence of transfer differences, there is also evidence consistent with a more durable transformation of local economic activity.

5.2 Sectoral composition of the employment response

The sectoral evidence is where our results depart most clearly from Gerard et al. (2024). In the original paper, the increase in formal employment following the 2009 expansion is concentrated in non-tradable industries, and the authors argue that non-tradables can account for the whole employment effect. This result is central to their interpretation of the reform as operating through local demand spillovers.

In our extension, the Gerard-style contribution exercise still points in that direction, but less sharply. The estimated path for the non-tradable contribution is positive after the reform and remains below the aggregate employment contribution in point estimates. In the long run, the formal test yields $LR^{All} - LR^{NT}$ is 0.082, with a p-value of 0.277, so the null that the non-tradable contribution equals the aggregate effect is not rejected. Taken on its own, this result is consistent with the view that non-tradables may account for the aggregate employment response.

However, the additional evidence on tradable employment qualifies that interpretation. When we estimate the model directly for tradable private jobs, the coefficients in the log specification are positive and statistically significant in both horizons, with

estimates of 0.113 in the short run and 0.160 in the long run. This is difficult to reconcile with a strong version of the Gerard et al. (2024) result in which the employment response is concentrated only in non-tradables. At a minimum, it suggests that the longer-run employment response was not confined to that sector.

The most reasonable reading of these results is therefore more cautious than in the original paper. The Gerard-style contribution test does not reject that non-tradables account for the aggregate effect, but neither does it establish that result decisively. Once the direct estimates for tradable employment are taken into account, the longer-run evidence points to a broader sectoral pattern than the one emphasized by Gerard et al. (2024). In that sense, our results preserve the importance of non-tradables, but they no longer support the conclusion that the employment gains were concentrated exclusively in activities tied more closely to local demand.

5.3 Non-income welfare outcomes

The non-income results can be interpreted considering two features of the program itself. First, Bolsa Família was designed as a transfer paid preferably to women (Lei N° 10.836, 2004, art. 2, § 14), especially mothers, and this choice has often been justified by the idea that directing income to women can strengthen their position within the household and favor expenditures more closely related to family welfare (World Bank, 2004). In that sense, the negative coefficients for female homicides, especially in the growth specification, point in the same direction as a possible gender-related channel of the program. In Chapter 4, the log coefficients for female homicides are negative but imprecisely estimated, while the growth coefficients are negative and statistically significant in both the short run and the long run, with estimates of -0.128 and -0.108, respectively. This pattern suggests that municipalities more exposed to the 2009 expansion experienced a more favorable trajectory in this outcome over time, even if the evidence for a persistent level difference is weaker.

The comparison with male homicides allows a more direct interpretation of this result. While the estimates for female homicides are negative in both specifications and statistically significant in the growth setup, the corresponding coefficients for male homicides are also negative in sign but remain small and statistically insignificant in both horizons. This pattern is therefore consistent with a gender-related channel of the program, rather than with a general reduction in homicides. At the same time, the estimates capture the overall effect of greater exposure to the reform on this outcome, rather than isolating the specific mechanism through which it operated. For that reason, the results are best read as evidence

consistent with a gender-related channel, strengthened by the fact that the contrast appears more clearly for female than for male homicides.

The health-expenditure results point to a different mechanism. Bolsa Família links transfers to health conditionalities, including health monitoring for children and prenatal and postpartum care for pregnant women (Portaria Interministerial N° 2.509, 2004). One possible result would therefore be that greater program exposure could reduce later health costs if checkups allow health problems to be identified earlier. Our estimates do not point in that direction. In Chapter 4, the log coefficients for health expenditure are positive and significant in the short run and essentially flat in the long run, while the growth specification don't show any significant result it points to a increase in expenditure as well. This suggests that municipalities more exposed to the expansion experienced an immediate increase in health expenditure and may have faced faster subsequent growth rather than savings in this margin. A plausible interpretation is that the mandatory monitoring itself raised the use of health services and, with it, the associated public cost.

Chapter 6. Conclusions

This dissertation examined the long-run local effects of differential exposure to the 2009 expansion of Bolsa Família, extending the horizon of analysis from the short-run focus in Gerard et al. (2024) to 2019. The central motivation of the study was the gap identified in the literature: while the direct effects of cash transfers on poverty, education, health, and household welfare are now well established, the indirect and spillover effects of these programs on local economies, especially over longer horizons, remain much less understood. This gap is particularly relevant for targeted and conditional programs such as Bolsa Família, since the existing literature emphasizes both the potential of cash transfers to generate local demand spillovers and the possibility that means testing may create efficiency costs through behavioural responses. In this sense, the dissertation aims to contribute not by revisiting whether cash transfers matter for beneficiaries, but by asking whether differential exposure to a large program expansion generated persistent differences in local economic activity and in broader welfare outcomes.

The main conclusion is that municipalities more exposed to the 2009 reform experienced persistent positive differences in local economic activity through 2019, although the form of that persistence varies across outcomes. The results showed that the treated-control difference in beneficiary incidence and in PBF payments did not vanish after the rollout. Instead, it narrowed over time but remained positive through the end of the sample, which is essential for interpreting the later estimates. GDP per capita provides the strongest evidence of a longer-run program effect, since it remains positive and statistically significant in both the log and growth specifications, even when the growth of PBF payments is already negative. This makes the GDP result less easily attributable only to the persistence of higher transfers. Private-sector formal employment, in turn, is more clearly characterized by persistence in levels. Product taxes move in the same direction in the short run, but the evidence is less stable over longer horizons, and income tax remains too imprecisely estimated to sustain stronger conclusions. Taken together, these results suggest that the 2009 expansion did not merely generate a temporary local adjustment. It left treated municipalities on persistently higher economic paths, even if the underlying pattern differs according to the outcome considered.

These findings connect directly with the arguments developed in the literature. First, they reinforce the view that cash transfers should not be understood only as redistributive instruments operating at the household level. As discussed by Egger et al. (2022), Gerard et

al. (2024), and reviewed by Gassmann et al. (2023), transfers can also generate broader local multiplier effects through demand. The evidence in this dissertation is consistent with that mechanism in the Brazilian case. Municipalities more exposed to the expansion did not merely display higher beneficiary incidence; they also showed stronger economic outcomes over time. This is important because it supports the idea that a targeted transfer program can operate not only through protection, in the sense stressed by Ravallion (2015), but also through broader local adjustment that may sustain economic activity beyond direct recipients. At the same time, the results also qualify that interpretation. Since the payment differential itself persisted over time, the positive long-run log coefficients cannot be interpreted as pure legacy effects of a one-off shock. Part of the observed persistence likely reflects the fact that treated municipalities remained on a higher transfer path.

The sectoral evidence is broadly consistent with the importance of non-tradables stressed by Gerard et al. (2024), but it does not replicate as clearly the concentration of the employment response in that sector. Although the Gerard-style contribution test does not reject the null that non-tradables account for the aggregate effect, the direct estimates for tradable private employment are positive and statistically significant in both the short and long run. The results therefore suggest that, in our extension, the employment response was not confined to non-tradable activities, even if those activities continued to play an important role.

The non-income results broaden the scope of the dissertation beyond local output and labour demand. For health expenditure, the evidence suggests not a persistent level difference, but faster subsequent growth in spending among more exposed municipalities. This pattern is consistent with the institutional design of Bolsa Família, which links transfers to health conditionalities for children and pregnant women. Rather than generating fiscal savings through prevention, the program may have increased the use of health services and, with it, public expenditure. For violence, the inclusion of male homicides alongside female homicides made the interpretation more direct. Female homicides display negative and statistically significant coefficients in the growth specification, while male homicides do not show comparable significant effects. This pattern is consistent with a gender-related channel of the program, which is plausible given the institutional choice to direct benefits preferably to women. The evidence does not isolate the exact mechanism, but it does suggest that the program's longer-run effects may have extended beyond income and employment into intra-household and social dimensions of welfare. This result also dialogues with the Papineni et

al.' (2025) work, showing that CCTs, besides women's empowerment, may also increase women's safety .

Overall, the dissertation supports four main conclusions. First, differential exposure to the 2009 Bolsa Família expansion generated persistent positive differences in local economic activity through 2019. Second, the strongest longer-run evidence appears in GDP per capita and especially private-sector formal employment, while the tax results are more uneven and less conclusive. Third, the sectoral pattern remains partly consistent with a local-demand mechanism, but the longer-run employment response is broader than the concentration in non-tradables emphasized in Gerard et al. (2024). Fourth, the program may also have non-income welfare outcomes, particularly through a gender-related channel. In that sense, the dissertation strengthens the argument that conditional cash transfers should be evaluated not only as anti-poverty tools for direct recipients, but also as policies with potentially important local spillovers whose effects can persist beyond the initial expansion.

At the same time, the dissertation has important limitations. The first is the absence of the administrative municipal quota data used by Gerard et al. (2024). Because these quota files are not publicly available, the treatment variable in this dissertation had to be constructed directly from realized beneficiary changes rather than from the original administrative allocation rules. The second is the lack of access to Cadastro Único snapshots, which are also not publicly available. Gerard et al. (2024) use those snapshots to calculate the behavioural responses generated by the program (how many families changed their behaviour in order to qualify for the transfer) and use that to build the welfare calculation behind the Marginal Value of Public fund (see more in Finkelstein & Hendren, 2020) that as argued by the authors makes an inter-programs comparison an easier task. Without access to those data, it was not possible here to estimate a new Marginal Value of Public Fund that incorporated the longer-run evidence produced in this dissertation. For that reason, although the results speak directly to the broader question of whether the program may have generated fiscal and economic spillovers large enough to matter for welfare evaluation, they do not yet permit a full recalculation of the marginal value of public funds using the extended time horizon.

These limitations also point directly to the most promising avenues for future research. A priority would be to request access to the administrative quota files and Cadastro Único snapshots in order to build a new Marginal Value of Public Fund for the 2009 expansion using the longer-run evidence documented here. That would make it possible to

connect the long-run local effects on GDP, taxes, and employment to a welfare framework comparable to the one in Gerard et al. (2024), but no longer limited to the short-run horizon. A second priority would be to deepen the study of mechanisms. On the economic side, future work could investigate more precisely why GDP retains stronger long-run evidence than some of the tax outcomes. On the social side, the gender-related findings deserve much more direct investigation. The difference between female and male homicides suggests that Bolsa Família may have operated through a gender equality channel, but the exact pathways remain open. Future work could examine whether those effects are related to women's bargaining power, changes in household vulnerability, or other dimensions of social protection linked to the design of the program. Future research could also investigate further the impact of the program on health expenditure as if it's notable it should be accounted for in future expansions.

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Annexes

Annex A. Supporting tables

A.1 Literature Review on the indirect effects

Reference	Research Goal	Main concepts	Sample	Methodology	Variables	Main Conclusions
Papineni et al. (2025)	GE and spillovers of UCT to women in rural Nigeria.	UCT to women; entrepreneurship; spillovers; multiplier.	Nigeria; 52 villages; 1,463 EV HH; 2015–2018.	HH-level randomization; panel OLS FE; RD cutoff check; spatial SEs.	Enterprise, profits, yields, consumption, food security, empowerment.	Entrepreneurship +20 pp (treated), +13 pp (non-beneficiary women in program village). Consumption/food security increase. Demand-driven; partial multiplier ≥ 0.32 based on female micro-entrepreneurship alone.
Filmer et al. (2018)	Do CCTs raise prices and harm ineligibles' nutrition?	CCT; tradables vs non-tradables; saturation; child nutrition.	Philippines; 130 villages RCT; 2,555 HH; 2011; price/unit-value data.	Village RCT; OLS FE; saturation interactions; validation with PSA/FIES.	Food prices; protein intake; HAZ/WAZ; stunting.	Protein prices increase 6–8% (high saturation). Ineligibles: HAZ -0.4 SD and stunting increase by 11 pp. Tradables $\approx$ no change.
Daidone et al. (2014)	24-month UCT impacts on consumption, production, labour.	UCT; productive investment; NFE; labour reallocation.	Zambia; 90 CWACs; 2,298 HH after attrition; ZMK 60/mo.; 2010–2012.	Community RCT; DiD with covariates; clustered SEs; SD/IPW as needed.	Inputs/land; outputs; livestock; NFE; savings; labour; income shares.	Consumption, inputs/land, harvest value increase. Livestock and NFE increase. Savings increase. Shift from agricultural wage to NFE/own farm. No child-labour change.
Orozco & Gammage (2017)	Synthesize impacts on poverty and women's empowerment.	CCT; gender; time poverty; co-responsibilities; agency.	Mexico; 150+ studies since late 1990s.	Literature + administrative-data review; domain synthesis.	Health; education; income/poverty; labour/time-use; empowerment/social capital.	Strong health/education gains. Poverty reduction. Time use is ambiguous. Care roles may persist. May benefit more when together with other social policies
Gassmann et al. (2023)	Systematic review of cash-transfer multipliers.	Multipliers; spillovers; methods: econometrics, LEWIE, CGE, SAM.	23 studies on cash transfer	PRISMA-style review; classify methods, contexts, outcomes.	Income/production multipliers; GDP effects; transfer efficiency.	Multipliers generally >0 , often >1 . Context/method dependent. Supply elasticities, MPC, permanence matter. Need standardization and data capacity.
Delius & Sterck (2024)	Effects of restricted digital transfers on firms and prices.	Restricted vouchers; market power; entry barriers.	Refugee settlements, Kenya; 400k recipients; 252 licensed shops; surveys of shops/HH.	Micro Theoretical model; PSM.	Revenues; profits; prices; license status; exposure.	Licensed shops: revenues +175%, profits +154%; higher prices. Parallel, less-competitive market. Avoid restrictive designs that enrich intermediaries.
Egger et al. (2022)	Measure GE and spillovers from large UCT.	Multiplier; spillovers; prices; slack.	Rural Kenya; 653 villages; $\sim 10,500$ HH; shock $\approx 15\%$ local GDP.	Large-scale RCT; village saturation; firm/HH/price surveys.	HH consumption; non-recipient consumption; firm revenue; prices; multiplier.	Multiplier ≈ 2.5 . Non-recipients' consumption increase. Firm revenues increased but not the profits. Prices maintained flat ($<1\%$).
Gerard et al. (2024)	Economy-wide effects on labor markets and activity.	Means-tested transfer; non-tradables; MVPF.	Brazil; 5,000+ municipalities; 2009 expansion (+17% slots).	Municipal DiD; FE; robustness; admin datasets (jobs, banking, GDP).	Formal jobs; wages; banking; electricity; vehicles; GDP; taxes.	Formal jobs increase 2%, concentrated in non-tradables. Wages flat. Positive effects on Banking/electricity/vehicles. Increase in local GDP. Output multiplier ≈ 1.49 ; MVPF >1 (considering all aggregate effects).
Sileshi et al. (2024)	Estimate direct and spillover FNS impacts and pathways in rural Madagascar.	Spillovers; FNS; ESR; PSM; WDDS; IYCDDS; FIES; diversification; postharvest losses.	715 women (15–49) with child 6–23m; 304 treated, 304 controls, 107 nonbeneficiaries; SE Madagascar; Apr–May 2023.	Cross-sectional endline; ESR + PSM; spillovers via nonbeneficiaries-in-treated; pathway regressions (crop/veg diversity, postharvest loss).	FIES; WDDS; IYCDDS; channels: field-crop diversity, vegetable diversity, postharvest-loss reduction.	Direct: WDDS +0.70; FIES +0.13 pp; IYCDDS +0.27. Spillovers: WDDS +0.25–0.38; IYCDDS +0.14–0.33; none for FIES; ≈ 36 –50% of direct. Pathways: increase crop/veg diversity; decrease postharvest losses; no income-diversification effect.

HAZ — Height-for-Age Z-score; **WAZ** — Weight-for-Age Z-score; **TLU** — Tropical Livestock Units; **RCT** — Randomized Controlled Trial; **RD** — Regression Discontinuity; **ANCOVA** — Analysis of Covariance; **SUTVA** — Stable Unit Treatment Value Assumption; **PSM** — Propensity Score Matching; **IPW** — Inverse Probability Weighting; **IHS** — Inverse Hyperbolic Sine transform; **Oster bounds** — Selection-on-unobservable robustness; **Lee bounds** — Sample-selection trimming robustness; **LEWIE** — Local Economy-Wide Impact Evaluation; **CGE** — Computable General Equilibrium; **SAM** — Social Accounting Matrix; **PSA** — Philippine Statistics Authority; **FIES** — Family Income and Expenditure Survey; **RAIS** — Brazilian formal-jobs registry; **ILO** — International Labour Organization; **MPC** — Marginal Propensity to Consume; **PET** — Share of nearby households receiving cash transfers; **PEV** — Share of extremely vulnerable households; **FNS** — Food and Nutrition Security; **ESR** — Endogenous Switching Regression; **WDDS** — Women's Dietary Diversity Score; **IYCDDS** — Infant and Young Child Dietary Diversity Score; **FIES** — Food Insecurity Experience Scale. **Reference:** Own elaboration.

A.2 Institutional Background of the PBF

Year	Legal Inst.	Eligibility Threshold	Basic Benefit / Floor	Key Structural Changes
2004	Lei 10.836	Extreme: R\$ 50 Poverty: R\$ 100	R\$ 50.00	Program Creation. Unification of previous grants into a single strategy.
2008	Lei 11.692	Extreme: R\$ 60 Poverty: R\$ 120	R\$ 62.00	Creation of extra benefit: Variable Benefit for Youths (16–17 years) introduced to fight high school dropout.
2012	Lei 12.722	Extreme: R\$ 70 Poverty: R\$ 140	R\$ 70.00	Brasil Carinhoso: Benefit covers the entire gap to the poverty line for families with children 0–6.
2020	Lei 13.982	N/A (Emergency)	R\$ 600.00	Auxílio Emergencial: Conditionalities suspended. Benefit value increased due to pandemic.
2021	Lei 14.284	Extreme: R\$ 105 Poverty: R\$ 210	R\$ 130.00	Auxílio Brasil: Return of conditionalities. Introduction of a phase-out mechanism
2022	Lei 14.342	Extreme: R\$ 105 Poverty: R\$ 210	R\$ 400.00 (floor)	Minimum Benefit: Create a floor, which favours smaller families
2023	Lei 14.601	R\$ 218.00 (Unified)	R\$ 600 (Floor)	Reinstatement: Increase floor and adapt the phase-out mechanism

Reference: Own elaboration.

