Policy Research Working Paper 9778

Cash Transfers and the Local Economy

Evidence from Brazil

François Gerard
Joana Naritomi
Joana Silva

WORLD BANK GROUP
Latin America and the Caribbean Region
Office of the Chief Economist
September 2021

Policy Research Working Paper 9778

Abstract

Cash transfers have been adopted worldwide and credited with significant reductions in poverty. However, their economy-wide effects continue to spark heated debates, particularly due to potential adverse effects on the labor market. This paper studies the impact of a flagship government-run program—*Bolsa Família* in Brazil—on local economies in a context where such concerns are particularly strong, as eligibility is means-tested. The study finds that an expansion of the program positively affected local economic activity using variation in the size of the reform across municipalities. The results are consistent with cash transfers stimulating local demand, despite means testing. These economy-wide effects substantially increase the marginal value of public funds of the reform, raising it above the value of a non-distortionary transfer.

This paper is a product of the Office of the Chief Economist, Latin America and the Caribbean Region. It is part of a larger effort by the World Bank to provide open access to its research and make a contribution to development policy discussions around the world. Policy Research Working Papers are also posted on the Web at http://www.worldbank.org/prwp. The authors may be contacted at jsilva@worldbank.org.

Cash Transfers and the Local Economy: Evidence from Brazil *

François Gerard
University College London

Joana Naritomi
London School of Economics

Joana Silva
World Bank & Catolica Lisbon

Originally published in the Policy Research Working Paper Series on *September 2021*.
This version is updated on *September 2024*.
To obtain the originally published version, please email prwp@worldbank.org.

Cash transfer programs have become a pervasive tool of social assistance in the developing world. It is well established that they reduce poverty and improve the lives of beneficiaries, but we still know little about their economy-wide effects (Kabeer and Waddington 2015, Niehaus and Suri 2024). Fears over "lazy welfare recipients" are generally unwarranted (Banerjee et al. 2017). Yet, the targeting of some of these programs, such as the use of means testing, can distort beneficiaries' labor supply decisions (Bergolo and

*f.gerard@ucl.ac.uk, J.Naritomi@lse.ac.uk, jsilva@worldbank.org. We thank Fernanda Brollo, Adriana Camacho, Mayara Felix, Claudio Ferraz, Giacomo de Giorgi, Gustavo Gonzaga, Nathan Hendren, Chris Moser, Paul Niehaus, Luis Henrique Paiva, Mounu Prem, Rodrigo Soares, Serguei Soares, Juan Carlos Suárez Serrato, Pablo Surico, Carlos Vegh and Guillermo Vuletin for helpful comments. Daniel M. Angel, Catarina Gaspar, Lorenzo Germinetti, Alejandra Martinez, Sebastian Melo, Maria Mittelbach, Samira Noronha, Lorenzo Pessina, Rafael Prado Proenca, Divya Singh and Francisco Weinholtz provided outstanding research assistance. We thank the LSE Inequalities Institute for financial support.

Cruces 2021), fueling concerns that these programs hurt the economy.[1] By contrast, there is evidence that cash transfers can stimulate local economic activity through multiplier effects (Egger et al. 2022). In practice, these effects can coexist in the case of means-tested cash transfers, but there is limited evidence on their aggregate impact to inform policy.[2]

This paper studies the aggregate effects of the largest means-tested cash transfer program in the world – *Programa Bolsa Família* (PBF) – on the local economy. PBF is the main social assistance program in Brazil,[3] targeting low-income families across the entire country, and it has been an influential model of cash transfer policy world-wide. We estimate the impacts of a 2009 reform that increased the number of PBF beneficiaries by 17% (or almost two million families) during a period of steady economic growth in Brazil. Our Difference-in-Differences research design exploits quasi-experimental variation in the size of the 2009 PBF expansion across municipalities. We find that higher PBF payments had positive aggregate effects on various measures of local economic activity and that the results are consistent with cash transfers stimulating local demand. These economy-wide impacts have relevant welfare implications: they substantially increase the marginal value of public funds of the reform, raising it above the value of a non-distortionary transfer.

A key feature of PBF for our research design is that many families who meet the per capita income criteria in Brazil's registry for social programs – *Cadastro Único* – do not become PBF beneficiaries, due to constraints on the program's size. The national number of slots for PBF beneficiaries is revised every three years and the 2009 revision induced a large expansion of the program. The methodology used for allocating slots across municipalities also changed at the time, which led to substantive and persistent differences in the additional numbers of PBF beneficiaries across municipalities. Our main Difference-in-Differences strategy exploits the timing of the 2009 PBF expansion and compares municipalities in the top 50% and bottom 50% of a measure of treatment intensity that isolates the variation in the size of the expansion coming from this change of methodology. Using administrative data, we find that the number of PBF beneficiaries rose sharply in 2009 for municipalities in the first group, but it remained stable for those in the second group. We estimate a differential increase in PBF payments of 13.4% between the two groups. We then examine how this variation in PBF outlays affected local economic activity.

We begin by studying impacts on formal employment. We leverage the granularity of the Brazilian matched employee-employer data to conduct a detailed analysis of the

[1] *The Economist* magazine published an article controversially titled "A land of useless workers" on June 10, 2023, in which "the structure of some welfare states, such as Brazil's Bolsa Família" is presented as a reason for low worker productivity in Latin America because it "makes operating informally more attractive."

[2] This debate is particularly heated in Brazil, with right-leaning politicians emphasizing negative effects on the labor supply of *Bolsa Família* beneficiaries, and left-leaning politicians highlighting positive effects on beneficiaries and local economies (we provide some illustrative quotes in Online Appendix A).

[3] As of 2012, PBF reached around a quarter of the Brazilian population.

effects of the 2009 PBF expansion and their underlying mechanisms. Formal employment is also a particularly interesting outcome to study. First, expanding formal employment has been a policy focus in Latin America, as it is more likely to provide workers with social security coverage and better working conditions, and it is associated with higher productivity (Ulyssea 2020). Second, formal jobs are the margin of economic activity most likely to be adversely affected by the means testing of PBF, because formal earnings are more-easily verifiable by the government. Moreover, an advantage of focusing first on a quantity is that we study an outcome that is unaffected by potential price effects.

We find that municipalities that received higher PBF payments experienced an *increase* in the number of private-sector formal jobs, which reached 2% by 2011. This result is robust across specifications, definitions of treatment intensity, and weighting schemes. It is also robust to using the Synthetic Difference-In-Differences (SDID) estimator (Arkhangelsky et al. 2021) and to aggregating the analysis at the level of geographic clusters of municipalities that could share economic spillovers, indicating that our findings are not due to reallocation effects across connected municipalities (Chodorow-Reich 2019).

The result is consistent with larger PBF outlays stimulating local demand. First, because we can match workers to the administrative PBF data at the individual level, we can show that about two thirds of the increase in formal employment is driven by workers who were never beneficiaries of PBF in the period of analysis. The literature also highlights that local multipliers will be stronger if the increase in spending is concentrated on locally-produced goods and services and does not lead to substantial increases in wages and prices (Nakamura and Steinsson 2014). We find that the increase in formal employment is concentrated in non-tradable industries, and we find no change in wages holding workers' composition fixed.[4] The average wage in fact decreases, but this is entirely due to a composition effect: the increase in formal employment is driven by low-wage workers.[5]

Next, we extend the analysis to other measures of economic activity. We provide survey evidence of an increase in employment and labor force participation. We find positive impacts using administrative data on banking activity (bank deposits, credits and loans), electricity use by households (a measure of consumption), electricity use by firms in non-tradable industries (another production input, besides labor), and vehicle registration (a physical asset). Finally, municipalities that received higher PBF payments experienced an increase in local GDP (as measured in disaggregated national accounts), which is due to increases in both the value added of firms in non-tradable industries and the revenue from

[4]There is no available data on the local price of non-tradables with extensive geographic coverage, but we find no changes in the local price of motor-vehicle fuels and cooking gas, which are available during our sample period. Overall, the more recent papers on the aggregate effects of cash transfers do not find relevant price effects (Niehaus and Suri 2024), except in remote areas (Filmer et al. 2021).

[5]Henceforth, we refer to 'low-wage workers' as those earning less than two minimum wages, a criterion used in Brazilian law to identify workers in need of income support (e.g., personal income tax exemption).

taxes levied on the sale of goods and services. In sum, our findings are consistent with a demand multiplier mechanism, where PBF transfers are spent locally, increasing the incomes of local goods and service providers, which further stimulates economic activity.

The increase in economic activity occurred even though the 2009 PBF expansion also induced behavioral responses consistent with concerns that means-tested cash transfer programs incentivize families to alter their behaviors to qualify for benefits. Specifically, we find that the 2009 PBF expansion led more families to be registered with income per capita below the eligibility thresholds in *Cadastro Único*.[6] This result implies that about a quarter of the estimated increase in PBF outlays was paid to families who changed their behavior to be eligible for PBF, a sizable negative fiscal externality.

Taken together, our findings indicate that cash transfers can have positive effects on the local economy, even if they create incentive for some households to remain poor. To assess magnitudes, we estimate a cost per formal job of US$9,799 per year (or 3.67 times the yearly minimum wage). This is comparable to the estimates in Corbi et al. (2019) of the impact of increases in local government budgets on formal employment in Brazilian municipalities. We also obtain an estimate for the output multiplier of 1.49 by building on the methodology relating output and employment multipliers in Chodorow-Reich (2019).[7] This is in line with the estimate in Pennings (2021) for permanent transfers in the U.S., but lower than the estimate in Egger et al. (2022) for a large one-off transfer in rural Kenya.

We end by highlighting the implication of our results for the welfare effects of the 2009 PBF expansion. Specifically, we compute its Marginal Value of Public Funds (MVPF), which is the ratio of the willingness-to-pay for the benefits of a policy to the net cost of funding it (Finkelstein and Hendren 2020). If we ignore the aggregate impacts on the local economy, we obtain a MVPF of .746 because of the negative fiscal externality on PBF payments. However, once we consider the positive impact on tax revenues and the willingness-to-pay for the increased economic activity, we show that the MVPF rises well above a value of 1, which corresponds to the benchmark of a non-distortionary policy.

This paper contributes to an extensive literature on cash transfers in developing countries, which focuses on the direct impacts on beneficiaries rather than on their broader effects in the economy (Niehaus and Suri 2024).[8] Our main contribution is threefold.

[6]This result is in line with the evidence in Bergstrom et al. (2022). It is consistent with several margins of behavioral responses: the 2009 PBF expansion increased families' incentives to register, to under-report their income, and to decrease their labor supply, particularly in the formal sector. It is challenging to quantify the impact of the 2009 PBF expansion on these various margins. Yet, we provide micro-evidence of negative formal labor supply responses to PBF benefit eligibility in Online Appendix C. Our aggregate results on formal employment may thus underestimate the strength of the local demand effects of PBF.

[7]We follow this approach, rather than using the impact on local GDP directly, because of the challenges in disaggregating GDP at the local level precisely (see Sections 3.5 and 5). This alternative approach gives us a larger output multiplier, although with a confidence interval that includes our preferred estimate.

[8]In a recent review paper, Gassmann et al. (2023) argue that "even though there has been increasing

First, we show that a means-tested cash transfer can increase local economic activity, leveraging the expansion of a nationwide government program that has been running for 20 years. Our findings complement the evidence in Egger et al. (2022), who estimate a large local multiplier from a one-time NGO transfer amounting to 15% of GDP in treatment villages. As the authors point out, their results do not imply that scaling up these cash transfers nationally would yield similar results. Evidence from rural settings may not apply to urban populations, which are increasingly covered by cash transfer programs in developing countries and make up the majority of PBF beneficiaries. Moreover, at scale, cash transfers are run by governments, are typically persistent, and involve local fiscal shocks that are often much smaller than those studied in Egger et al. (2022).[9] The use of means testing for targeting social transfers – with its potential adverse effects on the economy – is also bound to expand around the world, as countries develop and income becomes more verifiable across the income distribution (Jensen 2022). The policy variation that we study is, therefore, particularly relevant for informing policy debates.[10]

Second, we find that the impact of the 2009 PBF expansion is concentrated in non-tradable industries and that most of the formal employment gains are captured by non-beneficiaries, indicating a demand spillover mechanism.[11] Our study complements the evidence from previous work tracing the effects of cash transfers on non-beneficiaries. For instance, Angelucci and De Giorgi (2009) document increases in consumption among ineligible households living in the same villages as beneficiaries of the Mexican *Progresa* program. The effect operates through the insurance and credit markets, with no increase in local employment or output. Egger et al. (2022) do find income spillovers on non-beneficiaries and firms consistent with a demand mechanism, but the increase in output is not associated with any increase in employment. They argue that firms in rural Kenya severely under-utilize their factors of production, so that they have ample capacity to increase output without hiring new workers. Our evidence of positive employment effects is more in line with the literature on the multiplier effects of demand shocks in richer countries, which considers high unemployment rates as a key indicator of slack in an economy.[12] Relatedly, we show that the employment effect is stronger in municipalities with a history of excess capacity *in the labor market*, as measured by unemployment rates

interest in the multiplier effects of cash transfer programs, there is scant rigorous evidence."(page 2)

[9]By comparison, in 2008, PBF payments amounted to .8% of local GDP for the median municipality, and even for municipalities at the 90th percentile of the distribution, the ratio reached 'only' 4.4% of local GDP.

[10]Earlier work documented a positive association between PBF spending and economic activity in Brazil (e.g., Neri et al. 2013; Denes et al. 2018). Following our paper, other studies have found positive effects of cash transfers on local economic activity in Brazil (e.g., Cunha et al. 2022; Feler et al. 2023).

[11]Another strand in the literature shows that multiplier effects can arise from productive investments made by beneficiaries, increasing their own income above the value of the transfer (Sadoulet et al. 2001).

[12]For instance, in the model of Michaillat and Saez (2015), higher aggregate demand allows firms to find more customers, reducing the idle time of their employees, *and* increasing their labor demand.

several years prior to the 2009 PBF expansion. There might be less excess capacity *within firms* in more urban settings in middle-income countries compared to rural Kenya.[13]

Third, we highlight the welfare implications of our findings. The link between output multipliers and welfare effects must be examined carefully. For instance, even if we estimated a multiplier of the same magnitude as in Egger et al. (2022), the welfare implications would be different between the two settings for three main reasons. PBF is means-tested, and since we find evidence of behavioral responses to meet the eligibility criteria, the MVPF of the 2009 PBF expansion would be below 1 if we ignored the aggregate impacts on the local economy. The MVPF of the NGO transfer in Egger et al. (2022) would be equal to 1 under the same assumption. The willingness-to-pay for a given increase in economic activity would also be smaller in our context. The output gains in Egger et al. (2022) are driven by pure productivity gains, so that they should be valued at $1 per $1 in welfare terms. By constrast, we find increases in factors of production, such as labor, which come at an opportunity cost that must be accounted for in a welfare analysis (Sims and Wolff 2018). Finally, unlike Egger et al. (2022), we find a sizable increase in tax revenues because we study more formal economies where a greater share of economic activity is taxed. This effect decreases the net cost of the policy and thus increases its MVPF. Taking all these considerations into account, we show that the aggregate effects of the 2009 PBF expansion on economic activity can substantially alter its welfare evaluation.[14]

The paper also contributes to the literature on the aggregate effects of social policies more broadly.[15] For developing countries, several papers study the aggregate effects of public works employment programs in rural or urban labor markets (Imbert and Papp 2015; Muralidharan et al. 2023; Franklin et al. 2024). These programs create spillover effects through a different channel: they drive up private-sector wages and earnings by improving workers' outside option. Relatedly, Bandiera et al. (2017) show that a policy that transfers assets and skills to poor women engaged in casual wage labor induce them to switch to a different occupation, raising the wage of ineligible women who continue to supply labor in that market. Similar to our study, Bosch and Campos-Vazquez (2014) estimate the impact of a social policy that altered the incentives to operate in the formal economy, but they find negative aggregate effects on local formal employment. However, they study an in-kind transfer that is not tradable – providing health insurance to informal workers – and that is, therefore, less likely to generate sizable aggregate demand effects.

Finally, our results contribute to the literature on the effect of social policies on benefi-

[13] Despite this difference, like Egger et al. (2022), we estimate a sizable multiplier at a time of steady economic growth. Thus, our findings support the view that the existence of slack is a persistent feature of developing economies, rather than a feature of recessions as in richer countries (Michaillat and Saez 2015).

[14] Hackmann et al. (2022) use a similar approach to study a health insurance expansion in Germany.

[15] For richer countries, there is a large literature on the aggregate effects of unemployment insurance (e.g., Lalive et al. 2015), including their potential impact on aggregate demand in recessions (Kekre 2023).

ciaries' labor supply in developing countries. Recent studies argue that cash transfers do not discourage work (Banerjee et al. 2017) and might even help beneficiaries find better jobs (Baird et al. 2018). However, much of that discussion focuses on programs that only generate income effects (the targeting is based on proxy-means testing). Means-testing also generate substitution effects and several papers in the Latin American context found negative formal labor supply responses to cash transfers that change the relative returns of formal employment (e.g., Garganta and Gasparini 2015; Bergolo and Cruces 2021; Gerard and Gonzaga 2021). Our findings highlight the importance of considering aggregate effects to capture the impact of these policies on formal labor markets in full.

1 Institutional background and data

This section begins by introducing the *Programa Bolsa Família* (PBF) and institutional details that are relevant for our study period, which is from the beginning of 2007 to the end of 2011. We then describe the various datasets used in the empirical analysis.

1.1 *Bolsa Família* Program (PBF)

PBF was created in 2004, bringing together and expanding a set of existing social transfers. As of 2012, it reached around a quarter of the Brazilian population, for a cost of about 0.6% of GDP. Since its inception, the targeting of PBF has been based on families' income per capita. Families with income per capita below an extreme poverty threshold are eligible for an unconditional *Basic benefit* and *Variable benefits* per child conditional on the child's school attendance and health checks. Families with income per capita above the extreme poverty threshold but below a higher poverty threshold are only eligible for the conditional variable benefits. Benefits are paid to families on a monthly basis from the federal budget, typically into a bank account at *Caixa*, the main state bank in Brazil.

The relevant income per capita definition is the one recorded in *Cadastro Único*, which is a federal registry that is continuously updated. *Cadastro Único* was created at the same time as PBF and was the result of a similar consolidation effort. Yet, its purpose is to serve as basis for other social programs as well. For this reason, it aims to include families with income per capita below one-half of the minimum wage (R$255 in 2010), which is much higher than the two poverty thresholds for PBF eligibility (R$70 and R$140 in 2010).[16] The information in *Cadastro Único* is based on a standardized survey that asks families about income, housing, and assets, among other characteristics. Families can apply to their municipality to take the survey and be registered in *Cadastro Único*. Municipalities can also actively identify poor families and survey them, which was the main channel

[16] For reference, the exchange rate in 2010 was about R$2=US$1.

of registration in the first years of the system. Once a family is registered, its information must be updated following changes in, e.g., family income or size. At a minimum, families must update their information every two years for their registration to remain valid.

The income per capita in *Cadastro Único* is based on self-reported information. This leaves scope for discrepancies with families' true income per capita, but there are constraints on families' ability to misreport their income. For instance, the income questions come at the end of the *Cadastro Único* survey so that answers to previous questions about assets and social demographics help the interviewer gauge the veracity of the reported income. The law suggests that interviews should be done in the family's home to facilitate such verification. Audits can also be conducted following citizens' complaints or red flags arising from cross-checking the information in *Cadastro Único* with data from formal employment and social security records. This enforcement process is likely constrained by limited administrative capacity to cross-check information systematically and follow up on each case.[17] Nonetheless, the use of means-testing to determine eligibility creates incentives for families to remain poor or appear poor to the authorities.

A key feature of PBF for the empirical analysis is that families can be eligible without becoming beneficiaries because of constraints on the size of the program. First, the number of slots for PBF beneficiaries at the national level is set by the federal budget. It was initially set in 2003 and revised every three years. We focus on the 2009 revision, which saw the largest expansion in the national number of slots. Second, the national number of slots is divided in municipal quotas that were also revised every three years. These are not used as strict quotas for the program administration, but they determine the allocation of slots across municipalities. Slots that are not yet assigned to a family, or that reopen whenever a family leaves the program, are assigned to municipalities based on a priority ratio – the number of beneficiaries divided by the quota – and on unmet demand defined by the number of eligible families that are not receiving benefits (MDS 2008). Thus, eligible families can only become beneficiaries if there are available slots assigned to their municipality. Section 2 provides details on the rules used to compute the municipal quotas and how these rules changed at the time of the 2009 PBF expansion, which led to large differences in the additional numbers of PBF beneficiaries across municipalities.

1.2 Data

The analysis in this paper draws on several sources of data that we present here briefly.[18]

A. *Cadastro Único*. We use snapshots of *Cadastro Único* in December 2008 and August 2010. They include both family-level information (e.g., per capita income, family compo-

[17]For more details on eligibility and enforcement, see Lindert et al. (2007) and MDS (2010).

[18]The descriptive statistics reported below are also presented in Online Appendix Table B1.

sition, geographical location) and individual-level information (e.g., age, education).

B. PBF data. We use administrative data on the municipal quotas and on the universe of PBF benefits paid from 2004 to 2014. The payment sheet data include the amount received by each beneficiary in each month by benefit type. It allows us to calculate the number of PBF beneficiaries and the total PBF payments per municipality in each month.

C. Formal employment records (*RAIS*). We use the Brazilian matched employee-employer dataset, which covers the universe of formal employment spells in each year. For each worker, the data include information on municipality, industry, education, wage, gender, race, as well as hiring and separation dates. *RAIS* allows us to calculate municipal and individual formal labor market outcomes for each month in our period of analysis.[19]

We can link individuals across these administrative datasets using a unique ID number. This allows us to shed light on the mechanisms through which PBF affects the local economy. Indeed, we can distinguish between changes in local formal employment driven by PBF beneficiaries directly and spillover effects on individuals who never participated in the program. It also allows us to highlight some of the characteristics of PBF families. For instance, the August 2010 snapshot of *Cadastro Único* shows that PBF families mostly live in urban areas (70%), that they have low high-school completion rates (12%), and that their average income per capita falls below the extreme poverty line (R$54). Matching these families to the PBF payment sheets show that their average monthly benefit (R$95) was substantial compared to their average monthly income (R$206). It also shows that *Cadastro Único* includes many families that are not eligible for PBF, and that some families are eligible but are not beneficiaries. Specifically, 21% of extreme poor families, and 46% of families with income per capita between the extreme poverty line and the poverty line, were not PBF beneficiaries. Additionally, matching these families to *RAIS* shows that a relevant share of them interact with the formal labor market: about 35% of PBF families had at least one adult with positive formal employment over the following 12 months.

D. Other administrative data. We use other sources of administrative data at the municipal level to document changes in local economic activity over time: (i) data on total bank deposits (current accounts and savings) and total credits and loans reported by every bank branch in the country to the Brazilian Central Bank; (ii) data on electricity consumption for residential and commercial customers from 11 of the 27 Brazilian states;[20] (iii) data on the fleet of vehicles registered with the Department of Motor Vehicles (*DETRAN*).

[19] Formal workers are those with a signed working card (*carteira assinada*). Accurate information in RAIS is required for workers to access the benefits and labor protections afforded by the legal employment system.

[20] There are several electricity distributors in Brazil, and the data is not consolidated by a single source. To create a municipal panel data, we reached out to each of the providers by state. While some did not respond or declined, we successfully obtained data from major states like São Paulo, Pernambuco, and Bahia (in addition to Santa Catarina, Alagoas, Espírito Santo, Ceará, Goiáis, Mato Grosso do Sul, Paraná and Rio Grande do Sul).

E. Brazilian Census Bureau (*IBGE*) data. We use various surveys and datasets from *IBGE*. For instance, we use microdata from the 2000 Brazilian census to compute poverty rates, unemployment rates, and formality rates for each municipality. We use microdata from the 2010 Brazilian census and from annual household surveys (*PNAD*) to capture labor market outcomes beyond formal employment for our period of analysis. We also use municipal data on estimated population growth, GDP, and taxes on goods and services. Finally, we compute a few statistics using data from the 2008-2009 Consumer Expenditure Survey (*POF*) and from the 2010 Annual Surveys of Trade (*PAC*) and Services (*PAS*).

2 Empirical strategy

In this section, we lay out our empirical strategy to estimate the impact of PBF on local economic activity. It uses quasi-experimental variation in program size generated by the 2009 PBF expansion through a Difference-in-Differences (DD) research design.

2.1 The 2009 PBF expansion and the allocation of municipal quotas

We begin by describing the evolution of the number of slots at the national level and the 2009 PBF expansion. We also provide details on the allocation of these slots into municipal quotas and on the change in methodology in 2009 that we exploit in our empirical strategy.

A. National number of slots and the 2009 PBF expansion. The national number of slots was first set in 2003 following computations made by *IBGE* of the number of poor families in each of the 27 Brazilian states using microdata from the 2001 *PNAD* survey, which is representative at the state level.[21] The estimated figure of 11.2 million poor families was much larger than the number of beneficiaries of pre-existing social programs that could be transferred to PBF at the start of 2004. This is shown in Figure 1a, which displays the national numbers of slots and beneficiaries from 2004 to 2014. The number of PBF beneficiaries increased over time as more beneficiaries of other programs were transferred to PBF and municipalities registered new families in *Cadastro Único*. Yet, the national cap only became binding shortly after the 2006 revision, which reduced the national number of slots to 11.1 million based on the number of poor families in the 2004 *PNAD* survey.

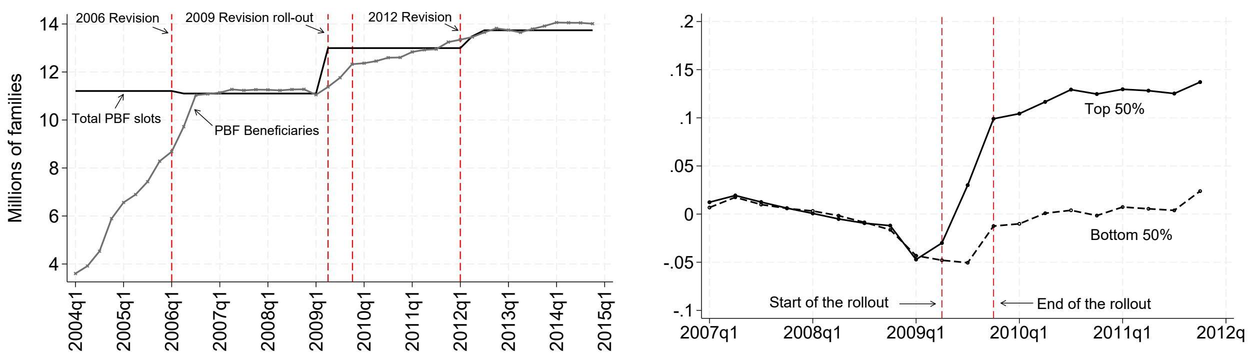
The national cap remained essentially binding until the 2009 revision, which led to a large expansion of PBF. The increase of 17% in the total number of slots at the time was not caused by deteriorating poverty rates. The Brazilian economy grew steadily in the 2000s and the number of poor families computed by *IBGE* in 2009 – using the 2006 *PNAD* survey – decreased compared to previous estimates. However, this number was scaled

[21] *IBGE* used poverty thresholds corresponding to R\$45 and R\$90 (i.e., one quarter and one half of the minimum wage) per capita to define extreme poverty and poverty, respectively (MDS, 2009b).

FIGURE 1: NUMBER OF PBF SLOTS AND BENEFICIARIES OVER TIME

(a) PBF slots and beneficiaries at the national level

(b) Relative change in the number of PBF beneficiaries by treatment intensity – raw data



Notes: Panel (a) displays the number of PBF slots (black line) and PBF beneficiaries (gray line) at the national level over the first decade of the program. The dashed vertical lines indicate the timing of the 2006, 2009, and 2012 revisions of the national number of slots. We include two lines in 2009 to mark the beginning and the end of the rollout of the large PBF expansion resulting from the 2009 revision. Panel (b) focuses on the 2009 PBF expansion and displays the average of the relative change in the number of PBF beneficiaries (compared to the period Jan 2007-March 2009), for municipalities in the top 50% and bottom 50% of our measure of treatment intensity.

up by a factor of 1.18 following the work of Soares (2009) showing that, due to income volatility, more families ended up regularly in a situation of poverty than the number of families observed below a given poverty line at any single time (MDS 2009b).

Figure 1a shows that the resulting increase in the national number of slots led to a large increase in the number of beneficiaries. It initially decreased in the first quarter of 2009, due to a "cleanup" of *Cadastro Único* through cross-checks with other administrative records. The number of beneficiaries then increased by more than 10% as the expansion was rolled out between the second and the fourth quarters of 2009, and continued to increase until it reached the national cap by 2011. Afterward, the government allowed the number of beneficiaries to exceed the national cap, and its revision in 2012 only caught up with the program's actual size at the time. The 2009 PBF expansion is thus the only revision that induced a sharp increase in the national number of PBF beneficiaries.

B. Allocation of municipal quotas and change in methodology in 2009. Each municipality is allocated a quota of the national number of slots based on municipal poverty measures computed by *IBGE*. As discussed in Section 1.1, these are not used as strict quotas, but they determine the allocation of any slot available at the national level across municipalities. The municipal quotas were thus instrumental in creating variation in the number of new PBF beneficiaries across municipalities following the 2009 PBF expansion.[22]

[22] The calculations underlying the allocation of municipal quotas are conducted by *IBGE* using data from the Census and other household surveys, so there is no clear room for political manipulation in this process. Brollo et al. (2020) show that there is evidence of political manipulation in other aspects of the program, i.e., in the enforcement of the school attendance conditionality. The quota allocation does not consider informa-

The government followed a similar strategy to allocate the national number of slots into municipal quotas when it set it in 2003 and when it first revised it in 2006. The number of poor families in each state – estimated from *PNAD* surveys (see above) – were apportioned across municipalities within the state using municipal poverty measures based on the 2000 census, the most recent source of data representative at the municipal level. Formally, the municipal quotas in 2003 and 2006 were calculated as follows:

$$Quota_{ms}^{2003} = \frac{Poor_{ms}^{2000}}{\sum_{k\in s} Poor_{ks}^{2000}} \cdot Poor_{s}^{2001} \tag{1}$$

$$Quota_{ms}^{2006} = \frac{Poor_{ms}^{2000} \cdot n_{ms}^{[2000,2003]}}{\sum_{k\in s}\left(Poor_{ks}^{2000} \cdot n_{ks}^{[2000,2003]}\right)} \cdot Poor_{s}^{2004} \tag{2}$$

where $Poor_s^{2001}$ and $Poor_s^{2004}$ are the number of poor families in each state based on the 2001 and 2004 *PNAD* surveys, respectively. The variable $Poor_{ms}^{2000}$ is the number of poor families in municipality m in state s based on the 2000 census. In 2006, this municipal poverty measure used for the apportionment was simply multiplied by $n_{ms}^{[2000,2003]}$, an estimate of population growth in each municipality between 2000 and 2003 (MDS 2012).

At the time of the 2009 PBF expansion, *IBGE* adopted a new methodology to compute the municipal poverty measure for allocating municipal quotas (MDS 2012). It used a statistical method developed by World Bank researchers to generate measures of poverty at low levels of spatial aggregation, when detailed household surveys are not representative of these local areas (Elbers et al. 2003). In a nutshell, the approach was to first estimate a prediction model for income per capita at the family level in the 2006 *PNAD* survey, using only survey variables also available in the 2000 census and municipal variables from other data sources (e.g., on local education, local GDP). *PNAD* surveys do not include all municipalities and are not representative at the municipal level. The model was then used to predict income per capita in 2006 for the families in the 2000 census, thus providing an estimate of the number of poor families in each municipality in 2006, $\widehat{Poor}_{ms}^{2006}$ (IBGE 2009). Using this measure of local poverty, the municipal quotas were calculated as:

$$Quota_{ms}^{2009} = \frac{\widehat{Poor}_{ms}^{2006}}{\sum_{k\in s} \widehat{Poor}_{ms}^{2006}} \cdot 1.18 \cdot Poor_{s}^{2006} \tag{3}$$

where the number of poor families in each state based on the 2006 *PNAD* survey is scaled up by 1.18 as explained earlier. *IBGE* returned to using census data to compute the municipal poverty measure for the 2012 revision (using the 2010 census). This alternative

tion related to program implementation, so this type of manipulation is not a concern for our analysis.

methodology for allocating slots across municipalities was thus only used in 2009.

2.2 Research design

Next, we present our research design, which exploits this change in methodology in 2009.

A. Quasi-experimental variation across municipalities. Figure 1b highlights that the change in methodology at the time of the 2009 PBF expansion provides us with substantial quasi-experimental variation in the number of PBF beneficiaries across municipalities. To isolate this source of variation, we compute counterfactual 2009 quotas as if the methodology used to update municipal quotas in 2006 had been maintained for the 2009 revision:

$$CountQuota_{ms}^{2009} = \frac{Poor_{ms}^{2000} \cdot n_{ms}^{[2000,2006]}}{\sum_{k \in s} \left(Poor_{ks}^{2000} \cdot n_{ks}^{[2000,2006]} \right)} \cdot 1.18 \cdot Poor_{s}^{2006}, \tag{4}$$

where $n_{ms}^{[2000,2006]}$ is an estimate of population growth in each municipality between 2000 and 2006 from *IBGE*. We then calculate the difference between the actual 2009 quota and the counterfactual 2009 quota, relative to the municipal population in 2006:

$$\Delta Quota_{ms}^{2009} = \frac{Quota_{ms}^{2009} - CountQuota_{ms}^{2009}}{Pop_{ms}^{2006}}, \tag{5}$$

which captures the *relative* change in quota in 2009 due to the change in methodology.[23]

Figure 1b displays the average change in the number of beneficiaries in each quarter between 2007 and 2011, compared to before the 2009 PBF expansion, for municipalities in the top 50% and bottom 50% of the distribution of $\Delta Quota_{ms}^{2009}$.[24] The two groups shared the same trend until the first quarter of 2009. Following the roll-out of the PBF expansion, the number of beneficiaries increased by more than 15% in the top-50% group during the remaining quarters of 2009. By contrast, it only returned to pre-2009 levels in the bottom-50% group. Therefore, the change in methodology to allocate municipal quotas led to large increases in the number of beneficiaries in some municipalities but not in others.

B. Difference-in-Differences strategy. We exploit this source of quasi-experimental variation in program size through a Difference-in-Differences (DD) strategy focusing on the period from January 2007 to December 2011 (before the 2012 revision). There were no

[23] Although the methodology used to estimate $\widehat{Poor}_{ms}^{2006}$ is described in several documents (see references in the text), we did not obtain sufficient information from *IBGE* to replicate their estimation procedure. Therefore, we cannot exploit any of its idiosyncrasies to isolate exogenous variation in $Quota_{ms}^{2009}$ directly.

[24] The full distribution is presented in Online Appendix Figure B1. Note that no beneficiary was forced to leave the program as a result of the update of municipal quotas in 2009, even in municipalities that experienced a reduction in their quota (these municipalities became less likely to be allocated *new* slots).

changes in quotas between 2007 and 2009, and the data used to compute the 2009 quotas – including the municipal poverty measure – were measured before 2007. Thus, the period from 2007 to the first quarter of 2009 serves as a useful pre-treatment period to provide supporting evidence for the identification assumption underlying our research design.

In the empirical analysis, we will estimate variants of the following specification:

$$y_{m,s,t} = \alpha_m + \phi_{t,s} + \sum_{t \neq t_0} \beta_t \cdot Treat_{m,s} + \sum_k \sum_{t \neq t_0} \gamma_{t,k} \cdot X^k_{m,s} + \varepsilon_{m,s,t}, \tag{6}$$

where $y_{m,s,t}$ is an outcome of interest at time *t* for municipality *m* in state *s*. Municipality fixed effects α_m control for time-invariant characteristics of municipalities. State-by-time fixed effects $\phi_{t,s}$ absorb any variation over time that is common across municipalities within a state, such as the increase in the sum of the municipal quotas within each state in 2009 from $Poor^{2004}_s$ to $1.18 \cdot Poor^{2006}_s$. The DD coefficients β_t capture any difference between municipalities with higher versus lower values of $Treat_{m,s}$ at time t compared with the last period before the 2009 PBF expansion t_0. The specification also allows for municipalities with different values of some predetermined variables $X^k_{m,s}$ to have different trends, which we discuss further below. We cluster the error term $\varepsilon_{m,s,t}$ by municipality, which is the unit of treatment assignment (Abadie et al. 2023).

In our analysis, we estimate specifications at the yearly level, but we will start by estimating specifications that exploits the higher frequency of some of our data to highlight the timing of the roll-out of the PBF expansion in 2009. For most of the analysis, we will also estimate impacts on the logarithm of an outcome, but we will use a specification in growth rates for some of our regressions following the macroeconomics literature on cross-region multipliers (Chodorow-Reich 2019). An advantage of this functional form is that it allows us to decompose the impact on an outcome into the impacts on specific sub-components, e.g., the impact on formal employment into the impact on employees of tradable and non-tradable industries, separately (see Section 3.4 for more details). It also allows us to obtain estimates scaling the impact of PBF on local economic activity per \$1 of PBF outlays, which is helpful to discuss magnitudes and implications (see Section 5).

In our preferred specification, $Treat_{m,s}$ corresponds to a dummy indicating whether a municipality belongs to the top 50% of the distribution of $\Delta Quota^{2009}_{ms}$ (as in Figure 1b). For exposition purposes, we refer to these municipalities as the *treatment* group hereafter, and municipalities in the bottom 50% of our measure of treatment intensity as the *control* group. We consider alternative treatment definitions as robustness checks.

Our main analysis sample includes 5,076 out of the 5,570 municipalities in Brazil at the time. To balance the panel, we restrict attention to municipalities that have at least one PBF beneficiary – and thus positive PBF payments – in every month from January 2007 to December 2011. Additionally, we exclude outliers, as Brazil had municipalities with population ranging from 828 inhabitants (Borá) to 11,016,703 inhabitants (São Paulo city) in 2006. Specifically, we exclude municipalities below the 1st percentile and above the 99th percentile of the 2006 population distribution, and we restrict attention to municipalities that have at least five private-sector formal employees every month from 2007 to 2011 (i.e., above the 1st percentile). We show in robustness checks that our results are similar if we include very large municipalities like the city of São Paulo. The restrictions over very small municipalities and minimum number of beneficiaries and formal employees are useful to minimize noise, keep a balanced panel, and exclude municipalities that may not have accurate data (e.g., implausibly low formal employment in some month).[25]

C. Identification assumption. We consider potential challenges to the identification assumption underlying our DD strategy, i.e., that municipalities in our treatment and control groups would have experienced a parallel trend in local economic activity if they had experienced a similar change in their municipal quota at the time the 2009 PBF expansion.

First, we weaken our identification assumption of a parallel trend between the *same* two groups of municipalities *for all* outcome variables by presenting results using the Synthetic Difference-In-Differences" (SDID) estimator of Arkhangelsky et al. (2021).[26] Second, our focus on the change in quota induced by the change in methodology shares similarities with the re-centering procedure proposed by Borusyak and Hull (2023) in the case of non-random exposure to an exogenous shock (in our case, the timing of the 2009 PBF expansion). Indeed, we are defining our measure of treatment intensity by adjusting the change in quota for the "expected treatment", i.e., if the government had maintained the same methodology to update the municipal quotas as in the 2006 revision.

Nevertheless, our measure of treatment intensity is not randomized, so our DD estimates remain vulnerable to biases if treatment effects are heterogeneous across municipalities (de Chaisemartin and D'Haultfoeuille 2022). We believe that the source of variation in program size that we exploit mitigates such concerns. Specifically, although municipalities with different program size are very different from each other, municipalities in our treatment and control groups are more comparable. This is shown in Table 1. Columns [1] and [2] present descriptive statistics for municipalities in the top 50% and bottom 50% of the distribution of the number of PBF beneficiaries per capita in 2008. The difference in program size is stark with municipalities in the top-50% group receiving, on average,

[25] We note that, because of data availability, our sample of municipalities is smaller for three sets of outcomes (employment and labor force participation, electricity consumption, and fuel prices). In each of these cases, we re-define our treatment dummy such that it compares municipalities in the top 50% and bottom 50% of our measure of treatment intensity in the relevant sub-sample of municipalities.

[26] The SDID estimator assigns different weights to control units for each outcome "so that the average (pre-treatment) outcome for the treated units is approximately parallel to the weighted average of control units" (Arkhangelsky et al. 2021).

about 3 times more PBF payments per capita and about 7 times more as a share of local GDP (2.9% vs. 0.4%). In line with PBF targeting goals, municipalities in the top-50% group had, on average, GDP per capita levels about 2.4 times lower, a lower private-sector formal employment rate (15.2% vs. 32.5%), and a higher unemployment rate (11% vs. 9.2%). They were also less likely to be defined as urban municipalities by *IBGE* (46.9% vs. 68.6%).

By contrast, these differences are smaller between columns [3] and [4], which consider municipalities in our treatment and control groups. The relative size of the program was comparable, although slightly larger in our control group (R$82.2 vs. R$92.5 per capita per year; 1.5% vs. 1.7% of local GDP). Treatment and control municipalities had also more comparable levels of GDP per capita (R$10,848 vs. R$11,880) and similar urban shares, formal employment rates, and unemployment rates. An important reason for these smaller differences is that, although the size of the program is geographically very concentrated, this is not the case for our measure of treatment intensity. Municipalities in the poorer areas of the North and the Northeast of Brazil have always more beneficiaries per capita. By contrast, since our measure of treatment intensity re-centers the change in quota around an expected treatment holding fixed the state-level number of poor families, our research design ensures that we have treatment and control municipalities within each state.[27]

TABLE 1: DESCRIPTIVE STATISTICS AT THE MUNICIPAL LEVEL

	Comparing municipalities by program size in 2008		**Comparing municipalities by treatment intensity**	
	Top 50% [1]	Bottom 50% [2]	Top 50% [3]	Bottom 50% [4]
Number of PBF beneficiaries per capita (2008)	0.120	0.047	0.078	0.090
PBF payments per capita (2008, in BRL)	131.8	42.9	82.2	92.5
PBF payments over GDP (2008)	0.029	0.004	0.015	0.017
GDP per capita (2008, in BRL)	6,673	16,056	10,849	11,881
Private-sector formal employment rate (2000)	0.152	0.325	0.242	0.234
Unemployment rate (2000)	0.110	0.092	0.098	0.103
Defined as urban municipality by IBGE	0.469	0.686	0.573	0.582
Population (2006)	18,509	29,706	28,008	20,207
Quota 2006	2,122	1,231	1,727	1,626
Population growth 2000-2006	1.077	1.109	1.054	1.132
Counterfactual quota 2009	2,333	1,581	1,967	1,948
Quota 2009	2,261	1,617	2,241	1,637
Measure of treatment intensity	-0.004	-0.001	0.015	-0.020
Number of municipalities	2,538	2,538	2,538	2,538

Notes: The table displays the average of variables measured prior to 2009 for the municipalities in our analysis sample. Columns [1] and [2] compare municipalities in the top 50% and bottom 50% of the distribution of program size (number of PBF beneficiaries per capita in 2008). Columns [3] and [4] compare municipalities in the top 50% and bottom 50% of our measure of treatment intensity.

Yet, Table 1 highlights two differences that remain sizable when we compare treatment and control municipalities. First, treatment municipalities were more populated on average (28,008 vs. 20,207 inhabitants). As a result, they had a larger municipal quota prior to

[27]Online Appendix Figure B2 presents a map of Brazil showing that we have contiguous municipalities with very different levels of treatment intensity all around the country.

the 2009 PBF expansion. Second, population grew, on average, faster in control municipalities from 2000 to 2006. Their quotas would have, therefore, increased relatively more than in treatment municipalities if the methodology used to update municipal quotas had not changed in 2009 (as captured by the counterfactual 2009 quota). In reality, because of the change of methodology, the quotas increased by 30.2% on average in treatment municipalities, but by only 0.7% in control municipalities. We account for these two differences in our preferred specification by including the logarithm of the 2006 quota and the counterfactual change in quotas ($\Delta CountQuota^{2009}_{m,s} = (CountQuota^{2009}_{m,s} - Quota^{2006}_{ms})\,/\,Pop^{2006}_{ms}$) as predetermined variables $X^k_{m,s}$. Thus, we allow municipalities that had different quotas at baseline, and municipalities that would have been impacted differently by the 2009 PBF expansion in absence of the change in methodology, to have different trends in local economic activity. Our results are robust to excluding these controls.

Finally, to address concerns that heterogeneous effects by transfer size could still bias our estimates of multiplier effects in the local economy, we also show that our results remain unchanged if we reweight municipalities such that the distribution of PBF payments per capita at baseline is the same between treatment and control municipalities.

3 Main results

We begin by showing that the variation induced by the 2009 PBF expansion and the change in methodology for allocating municipal quotas led to a large increase in PBF payments in treatment municipalities compared to control municipalities. Next, we estimate impacts on local economic activity. We first analyze impacts on formal employment. This is for three reasons: we have high-frequency administrative data on formal employment at the local level; formal employment is a quantity, so it is unaffected by any local price effect; and the detailed microdata allow us to shed light on mechanisms. We show impacts on a range of other measures of economic activity in the final part of the section.

3.1 Impact on total PBF payments

Figure 2a displays the average change in PBF payments in each quarter between 2007 and 2011, compared to before the 2009 PBF expansion, for municipalities in our treatment and control groups. As in Figure 1b, the two groups initially shared a similar trend, but the PBF expansion led to a larger increase in PBF payments in treatment municipalities, as it was rolled out in the last three quarters of 2009.[28] Figures 2c and 2e present regression results quantifying this impact. Figure 2c displays DD coefficients from estimating our preferred

[28]Total PBF payments did not return to pre-2009 levels in the control group, in contrast to the pattern in Figure 1b for PBF beneficiaries, because the level of the PBF benefits also increased over time.

specification in equation (6) at the quarterly level for the logarithm of PBF payments. For exposition purposes, we display linear combinations of our quarterly estimates averaging the estimated impacts in six time periods p, which is sufficient to trace the evolution of the outcome before the 2009 PBF expansion ($p = \{2007, 2008, 2009_{q1}\}$), during its rollout ($p = \{2009_{q2-q4}\}$), and in the following years ($p = \{2010, 2011\}$). Our estimates are precise and imply that treatment municipalities experienced a relative increase in PBF payments of about 0.14 log points by 2011. The impact appears during the rollout of the PBF expansion and is stable between 2010 and 2011, which is consistent with the raw data in Figure 2a. Figure 2e displays results from a specification in growth rates at the yearly level. The estimated impact reaches about 17% between 2010 and 2011. Finally, dashed lines in both figures show that our results are unchanged if we use the SDID estimator.

3.2 Impact on private-sector formal employment

Figures 2b, 2d and 2f display results for the number of private-sector formal employees, which mirror the results for PBF payments in Figures 2a, 2c and 2e. Figure 2b shows that treatment and control municipalities shared a common trend prior to the PBF expansion. Formal employment continued to evolve similarly between the two groups throughout 2009, but we can see in the raw data that it started to increase faster in treatment municipalities after the end of the rollout of the PBF expansion. As a result, treatment municipalities had experienced a relative gain in private-sector formal employment by 2011.

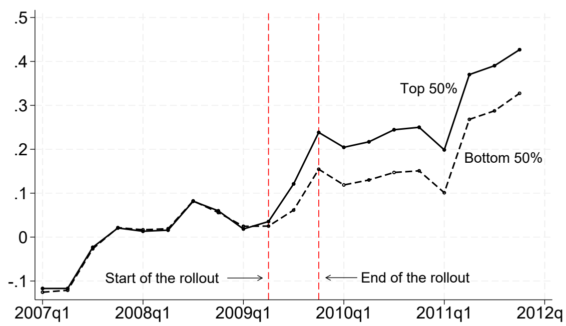
The DD estimates imply that treatment municipalities experienced a relative increase in private-sector formal employment that reached .024 log point by 2011 or 4.33% using the specification in growth rates. Point estimates are similar using the SDID estimator. The 2011 effect is significant at conventional levels, although confidence intervals are unsurprisingly wider for formal employment than for PBF payments. As in the raw data, the impact appears with a lag compared with the increase in PBF payments. This delayed response is consistent both with multiplier effects from the increase in resources spent in the local economy (e.g., firms may not expand labor demand until an increase in demand appears persistent) and with increases in formal labor supply among new beneficiaries (e.g., job-search investments may take time to yield returns). We explore mechanisms below.

3.3 Robustness

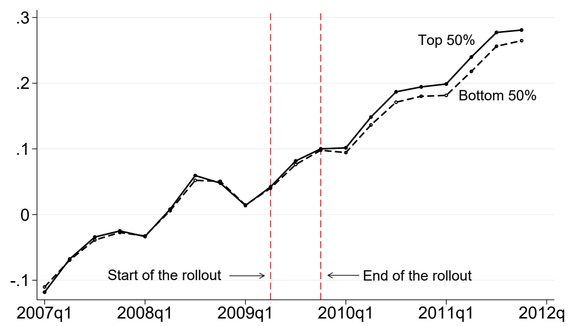
Before examining mechanisms, we present a series of robustness checks in Table 2. As a benchmark, column [1] summarizes the results in Figure 2 by reporting linear combinations of the DD estimates averaging the estimated impacts in 2010 and 2011 for the specifications in logarithms (top panel) and in growth rates (bottom panel). The estimates

FIGURE 2: IMPACT ON PBF PAYMENTS AND FORMAL EMPLOYMENT

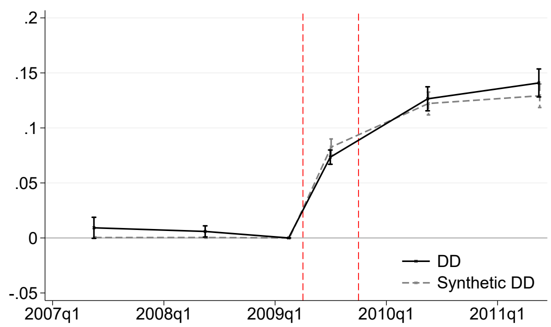
(a) Relative change in PBF payments by treatment intensity - raw data



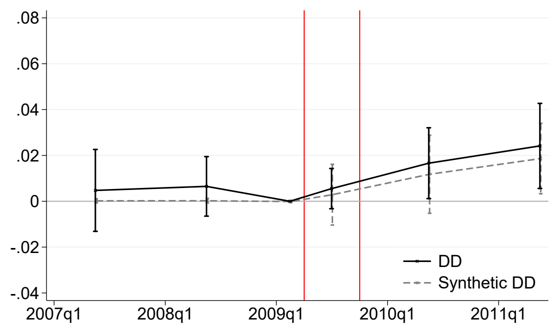
(b) Relative change in private-sector formal employment by treatment intensity - raw data



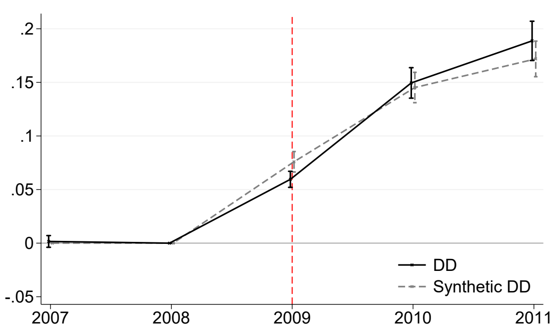
(c) PBF payments (log)



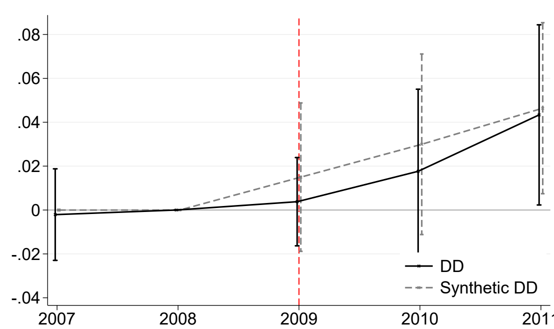
(d) Private-sector formal employment (log)



(e) PBF payments (growth)



(f) Private-sector formal employment (growth)



Notes: Panels (a) and (b) display the average of the relative change in PBF payments and in the number of private-sector formal employees (compared to the period Jan 2007-March 2009) for municipalities in the top 50% and bottom 50% of our measure of treatment intensity. Panels (c) and (d) display DD coefficients (solid black lines) and SDID coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (6) at the quarterly level for the logarithm of these outcomes. For exposition purposes, we display linear combinations of our quarterly estimates averaging the estimated impacts in six time periods p: before the 2009 PBF expansion ($p = \{2007, 2008, 2009_{q1}\}$), during its roll-out ($p = \{2009_{q2-q4}\}$), and in the following years ($p = \{2010, 2011\}$). Panels (e) and (f) display comparable results using a specification in growth rates at the yearly level. The vertical lines indicate the start and end of the rollout of the 2009 PBF expansion (panels a-d) or the year of the reform (panels e and f).

are in line with the patterns in Figure 2: an increase in PBF payments of 0.134 log point or 16.9%, and an increase in private-sector formal employment of .02 log point or 3%.

Columns [2], [3], and [4] consider alternative treatment definitions. In column [2], we keep only municipalities in the top 25% and bottom 25% of our measure of treatment intensity. The relative increases in PBF payments and private-sector formal employment become larger, which is consistent with the fact that we are exploiting a starker difference in treatment intensity.[29] Column [3] shows that the estimated effects are similar if we define treatment and control groups based on the simple change in the 2009 quota compared to the 2006 quota (rather than compared to the counterfactual 2009 quota). In column [4], we use $\Delta Quota_{ms}^{2009}$ linearly. Point estimates are consistent with those in column [1], considering a gap in average $\Delta Quota_{ms}^{2009}$ of .0342 between treatment and control groups.

In columns [5] and [6], we allow large municipalities to have a greater influence on our estimates. The sample in column [5] includes municipalities above the 99th percentile of the 2006 population distribution. The specification in column [6] weights municipalities by their 2006 population. In both cases, the results are essentially unchanged.

Columns [7] and [8] address potential challenges to our identification strategy. Column [7] shows that our results are robust to heterogeneous effects by transfer size: we re-weight municipalities such that the distribution of PBF payments per capita prior to the 2009 PBF expansion is the same between treatment and control groups. Our results are unchanged in column [8], where we exclude the controls X^k interacted with time fixed effects. Thus, the two differences between treatment and control municipalities in Table 1 highlighted in Section 2.2 do not appear correlated with differential trends in the outcomes.

Columns [9] and [10] consider a different identification concern. The literature on cross-region multipliers highlight that the impact in the recipient locality may underestimate or overestimate the overall economic activity generated. On the one hand, increases in local demand might "leak" to other areas, including control municipalities. On the other hand, workers may move in from other areas, including control municipalities. In column [10], we follow a common approach to address such concerns in the literature by replicating the analysis at a higher level of geographic aggregation (Chodorow-Reich 2019). Specifically, we aggregate the data and compute our measure of treatment intensity at the level of the "Immediate Geographic Regions" (*RGI*), which are defined by *IBGE* as groups of municipalities around urban centers that may supply goods and services to municipalities within the region.[30] Point estimates are of a similar magnitude as those in column [1], although they are slightly higher for private-sector formal employment and slightly lower for PBF payments.[31] This is consistent with the finding that local multipliers tend to provide lower-bounds for aggregate multipliers because the effect of demand leak-

[29]Online Appendix Figure B3 shows that these two groups also shared a common trend in total PBF payments and private-sector formal employment prior to the 2009 PBF expansion.

[30]The RGIs replaced the "microregions," which were defined based on data from 1980 (IBGE, 2017).

[31]Online Appendix Figure B4 presents the supporting graphs. The results of the municipal-level regressions in Table 2 are robust to clustering standard errors at the *RGI* level (see Online Appendix Table B2).

TABLE 2: IMPACT ON PBF PAYMENTS AND FORMAL EMPLOYMENT – SUMMARY AND ROBUSTNESS CHECKS

	Top 50% vs bottom 50% [1]	Top 25% vs bottom 25% [2]	Top 50% vs bottom 50% (actual quota change) [3]	Treatment intensity linearly [4]	Including largest cities [5]	Population weights [6]	DFL weights for PBF payments per capita [7]	No baseline controls [8]	Outcomes in per capita terms [9]	Aggregated at the *RGI* level [10]
Panel A. Log specification										
PBF payments	0.134***	0.217***	0.152***	4.568***	0.133***	0.135***	0.126***	0.107***	0.142***	0.102***
	(0.006)	(0.010)	(0.005)	(0.163)	(0.006)	(0.009)	(0.007)	(0.005)	(0.006)	(0.010)
Private-sector formal employment	0.020***	0.029***	0.021***	0.765***	0.021***	0.019***	0.022***	0.018***	0.029***	0.020***
	(0.008)	(0.013)	(0.008)	(0.223)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)
Public-sector employment	0.010	-0.024	0.011	0.098	0.012	0.009	0.009	0.007	0.018	0.007
	(0.012)	(0.042)	(0.014)	(0.437)	(0.012)	(0.014)	(0.012)	(0.013)	(0.012)	(0.009)
Panel B. Growth specification										
PBF payments	0.169***	0.280***	0.194***	5.923***	0.168***	0.165***	0.160***	0.134***	0.174***	0.128***
	(0.008)	(0.013)	(0.007)	(0.225)	(0.008)	(0.012)	(0.009)	(0.007)	(0.008)	(0.013)
Private-sector formal employment	0.030*	0.068**	0.030	0.785*	0.032*	0.029**	0.039**	0.033*	0.036**	0.036**
	(0.017)	(0.029)	(0.018)	(0.469)	(0.017)	(0.011)	(0.016)	(0.017)	(0.018)	(0.014)
Public-sector employment	0.068	0.169	0.229	8.049	0.075	-0.166	0.040	-0.093	0.057	-0.002
	(0.230)	(0.993)	(0.269)	(7.662)	(0.228)	(0.207)	(0.234)	(0.300)	(0.228)	(0.013)

Notes: The table reports linear combinations of our DD coefficients (with standard errors in parentheses), averaging the estimated impact in 2010 and 2011, for specifications in logarithms (panel A) and growth rates (panel B). We present results for the same two outcomes variables as in Figure 2, and for the number of public-sector employees. In column (1), we summarize the results in Figure 2 by using the same specification, sample, and definition of the treatment variable. The other columns present robustness checks: we keep only municipalities in the top 25% and bottom 25% of our measure of treatment intensity (column 2); we assign municipalities to treatment and control groups based on the simple change in their 2009 quota compared to their 2006 quota (column 3); we use our measure of treatment intensity linearly (column 4); we include the largest municipalities in our analysis sample (column 5); we weight municipalities by their 2006 population (column 6); we re-weight municipalities such that the distribution of PBF payments per capita prior to the 2009 PBF expansion is the same between treatment and control municipalities (column 7); we exclude the set of baseline controls interacted with time fixed effects (column 8); we express the outcomes in per capita terms (column 9); and we aggregate the data and compute our measure of treatment intensity at the level of the "Immediate Geographic Regions" (*RGI*).

ages typically dominates any migration effect (Chodorow-Reich 2019). Relatedly, column [9] shows that the results of our municipal-level regressions are unchanged if we express the outcomes in per capita terms, using *IBGE* estimates of local population in each year.[32]

Finally, we also present results for public employment in Table 2. We find no evidence of a differential impact in treatment municipalities. Thus, the increase in transfers to low-income families in our setting is not associated with an expansion of the government workforce, as in the case of the transfers to local governments in Corbi et al. (2019).

3.4 Anatomy of the impact on formal employment and mechanisms

To shed light on mechanisms, we leverage the granularity of our matched administrative data to study the types of formal jobs that were created and who obtained those jobs. Our results are consistent with the increase in cash-on-hand in treatment municipalities stimulating aggregate demand in the local economy and expanding labor demand.

The first piece of evidence is that the increase in formal employment is mostly driven by workers who were not directly impacted by the 2009 PBF expansion.[33] To show this, we select all workers who appeared in *RAIS* at any point over our sample period and we only keep those who were never PBF beneficiaries, i.e., those who were never part of a family that received PBF benefits during our sample period. We then estimate the contribution of these workers to the overall impact on private-sector formal employment. We use a DD specification in growth rates, where the dependent variable is the change in private-sector formal employment in the subgroup of interest, relative to the overall private-sector formal employment in the reference period.[34] Figure 3a compares the effect among these "never-beneficiaries" (black line) and the overall effect presented in Figure 2f (grey line). The estimates are comparable because both outcomes are expressed in percentage of total private-sector formal employment in 2008. The impact on never-beneficiaries follows the same pattern over time as the impact on all workers, reaching 2.96% by 2011, and thus accounting for about two thirds (63.2%) of the overall impact.[35] This result implies that

[32]We find no evidence of differential population growth in treatment municipalities using the same specifications as in column [1]. Point estimates are .000 (.002) for both the log and the growth specifications. This is consistent with evidence that large shocks in Brazil did not trigger much migration. Dix-Carneiro and Kovak (2019) show that workers adversely affected by import competition following the 1990s trade liberalization did not migrate to less affected regions, and Costa et al. (2016) do not find net migration responses to changes in local labor demand triggered by an increase in exports associated with the China shock.

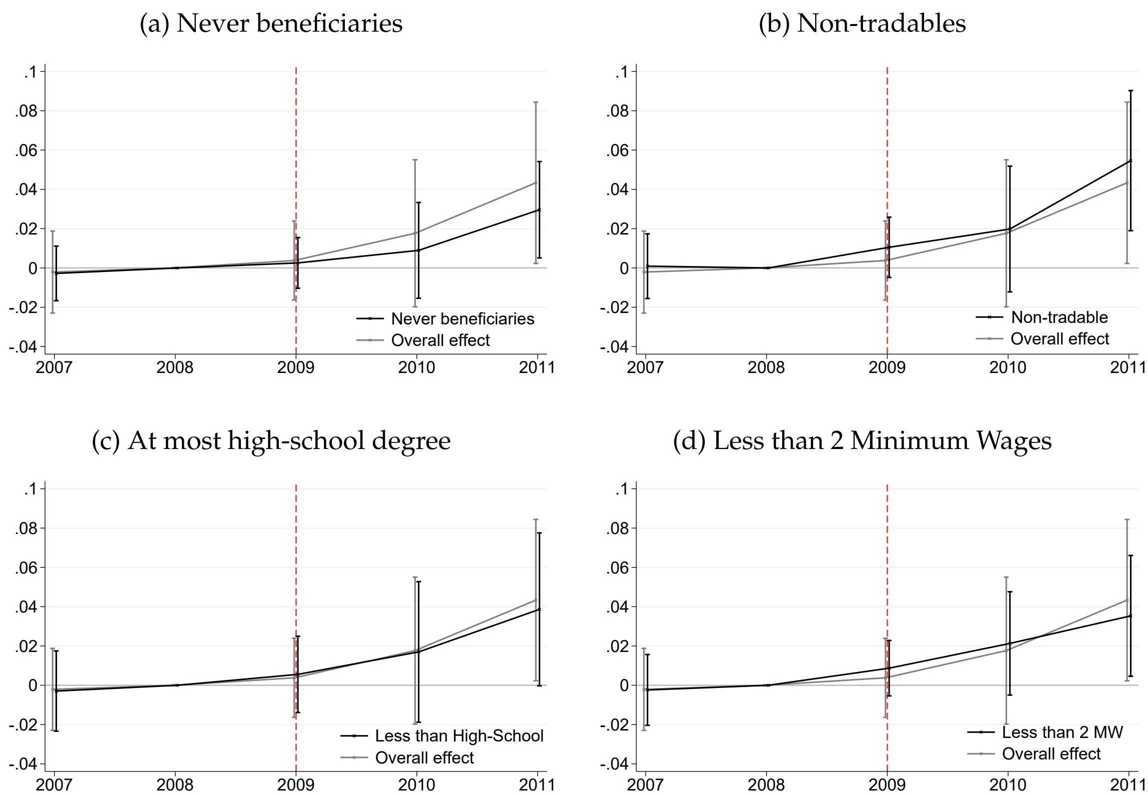
[33]The type of market externalities studied in, e.g., Lalive et al. (2015), through which decreases in job search among beneficiaries can increase job-finding among non-beneficiaries, could rationalize this specific result but not the overall increase in formal employment (considering all workers together).

[34]Specifically, for any outcome $a_{m,s,t} = b_{m,s,t} + c_{m,s,t}$, the overall effect on $(a_{m,s,t} - a_{m,s,t_0})/a_{m,s,t_0}$ is the sum of the effects on $(b_{m,s,t} - b_{m,s,t_0})/a_{m,s,t_0}$ and on$(c_{m,s,t} - c_{m,s,t_0})/a_{m,s,t_0}$. This equality does not necessary hold for SDID estimates because the weights on control units are outcome-specific.

[35]We provide complementary evidence in Online Appendix Figure B5a: treatment municipalities also

the relative increase in private-sector formal employment in treatment municipalities is not driven by an increase in formal labor supply among new beneficiaries.

FIGURE 3: ANATOMY OF THE IMPACT ON FORMAL EMPLOYMENT



Notes: The figure shows the contribution of specific groups of workers to the overall impact on private-sector formal employment. It displays DD coefficients (with their 95% confidence intervals) from specifications in growth rates at the yearly level, where the dependent variable is the change in private-sector formal employment in a specific group, relative to the overall private-sector formal employment in 2008 (black lines). For comparison, in each panel, we also reproduce the estimates for the overall impact on private-sector formal employment from Figure 2f (gray lines). We consider workers who were never part of a PBF family during the period of analysis (panel a), workers employed by firms in non-tradable industries (panel b), workers with no more than a high school degree (panel c), and workers earning less than twice the minimum wage (panel d). The vertical lines indicate the year of the PBF expansion.

The literature on local multipliers emphasizes that local demand effects will be stronger if the increase in resources is spent rather than saved and if the spending is concentrated on locally produced goods and services (Nakamura and Steinsson, 2014). We cannot estimate the impact of PBF on the spending pattern of beneficiaries, so we cannot provide direct evidence for these two conditions.[36] Yet, it is reasonable to argue that the first condition is likely satisfied in our context: the propensity to spend is typically high among poor

experienced an increase in private-sector formal employment among other workers who were arguably not directly impacted by the 2009 PBF expansion: those who were *already* PBF beneficiaries in 2007 and 2008.

[36]The only available data on the spending pattern of PBF beneficiaries during our sample period come from the Consumer Expenditure Survey (*POF*) conducted by *IBGE* in 2008-2009. Using these data and the definition of formality from Bachas et al. (2023), we find that PBF beneficiaries spend a large share of their expenditures in formal stores (63%), an additional condition for an increase in *formal* labor demand.

households (e.g., Krueger 2012; Johnson et al. 2006). We provide evidence in support of the second condition in Figure 3b. Using information on the industry code of each establishment, we follow the same approach as in Figure 3a to estimate the contribution of non-tradable industries to the overall impact on private-sector formal employment.[37] Point estimates are similar (even slightly higher) as those considering all industries. Therefore, non-tradable industries can account for the *whole* impact on formal employment.

For general equilibrium effects to imply large local multipliers, it must also be that demand effects do not lead to substantial increases in local prices and wages. We do not have access to detailed price data at the local level during our sample period,[38] but we can study impacts on formal wages because *RAIS* includes information on the wage of every formal employee in December in each year. Figure 4a displays DD and SDID estimates for average log wages, revealing a decrease in average wages following the 2009 PBF expansion. However, this pattern is entirely driven by a composition effect. Figure 4b shows that the impact on average wages is essentially zero if we restrict attention to workers who were formally employed in each of the five years in our study period (the composition of that sample is fixed). Moreover, Figures 3c and 3d show that the overall increase in private-sector formal employment following the 2009 PBF expansion is entirely driven by workers with lower education levels (i.e., with no more than a high-school degree) and low-wage workers.[39] An increase in labor demand without any increase in wages is consistent with the existence of excess supply in the labor market. For instance, it is worth noting that the minimum wage was binding during our study period (Engbom and Moser 2022).[40] Overall, the more recent papers on the aggregate effects of social protection transfers do not find relevant price effects (Niehaus and Suri 2024). Even for the cash transfers amounting to 15% of local GDP in Egger et al. (2022), consumer prices increased by only 0.1%-0.2%. Given this recent evidence and the absence of wage effects, it is reasonable to assume that the 2009 PBF expansion did not lead to large price effects.

Online Appendix Figure B8 provides additional evidence on the anatomy of the formal

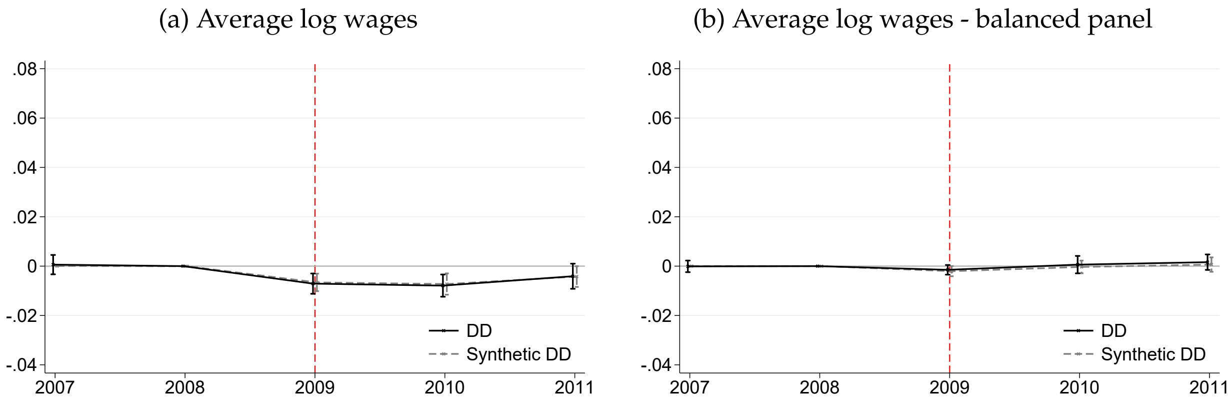
[37] We follow Dix-Carneiro and Kovak (2019) and classify industries (CNAE codes) related to services and commerce as non-tradables and those related to agriculture and manufacturing as tradables.

[38] The data used to compute price indices in Brazil only cover a few large metropolitan areas. The only price data available at the municipal level with extensive geographic coverage during our study period are retail prices for motor-vehicle fuels (gasoline, ethanol, diesel) and cooking gas. We find no evidence of any differential increase in those prices in treatment municipalities (see Online Appendix Figure B7). However, this evidence is only suggestive of a null price effect. These fuels are arguably tradable, and while their prices vary within the country, they may be less responsive to local demand than the price of non-tradables.

[39] Accordingly, we show in Online Appendix Figure B6 that the drop in average wages in Figure 4a essentially disappears if we focus on low-wage workers. Online Appendix Figure B5b also shows that treatment municipalities experienced an increase in private-sector formal employment among low-income workers in general, by considering all workers who were registered in *Cadastro Único* in 2008.

[40] Relatedly, Hackmann et al. (2022) find that a health insurance expansion in Germany increased aggregate employment, but had no effect on wages, in a labor market with binding wage floors.

FIGURE 4: IMPACT ON FORMAL WAGES



Notes: The figure displays DD coefficients (solid black lines) and SDID coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (6) at the yearly level for the average of log wages. Panel (a) displays estimates for the wage of private-sector formal workers employed in December of each year, which reflect a combination of treatment effects and compositional changes among formal employees. Panel (b) displays comparable estimates for a balanced panel of workers employed throughout the sample period to shut down any composition effect. The vertical lines indicate the year of the PBF expansion.

employment response to the 2009 PBF expansion. For instance, the impact is entirely driven by full-time workers; men and women contribute about two thirds and one third of the overall impact, respectively; and most of the impact is driven by an increase in formal employment among establishments that did not exist prior to 2009. Relatedly, Online Appendix Figure B9 shows that the 2009 PBF expansion led to a relative increase in the number of establishments with at least one formal employee in treatment municipalities.[41]

3.5 Broader impacts on local economic activity

This subsection provides evidence that the private-sector formal employment effects documented above reflect a broader increase in local economic activity. First, we use administrative data capturing other relevant dimensions of economic activity. Second, we use survey data to explore impacts on overall employment, including formal *and* informal employment. Third, we use national accounts data on taxes and GDP disaggregated at the municipal level, which aim to provide a more holistic measure of economic activity.

A. Other relevant dimensions of economic activity. Figures 5a and 5b begin by documenting significant increases in banking activity in response to the 2009 PBF expansion. DD estimates in Figure 5a show that treatment and control groups shared a common trend in total bank deposits between 2007 and 2008. However, the amounts held in checking and

[41] DD estimates for the number of establishments with at least one formal employee indicate a pre-trend between our two groups of municipalities, but we obtain a similar treatment effect using the SDID estimator, indicating that the positive effect after 2009 is not systematically correlated with that pre-trend.

savings accounts started to increase relatively more in treatment municipalities in 2009. This increase in banking activity is not mechanically driven by the higher PBF payments. It is the case that, for most PBF beneficiaries, their monthly benefits are deposited in a bank account at *Caixa*, the main state bank in Brazil. However, the results are similar if we exclude *Caixa* accounts (see Online Appendix Figure B10). Moreover, Figure 5b shows that credits and loans also increased in treatment municipalities after 2009. DD estimates feature a pre-trend between our two groups of municipalities, but we find comparable results for 2010 and 2011 using the SDID estimator, indicating that the relative increase in credits and loans after 2009 is not systematically correlated with that pre-trend.

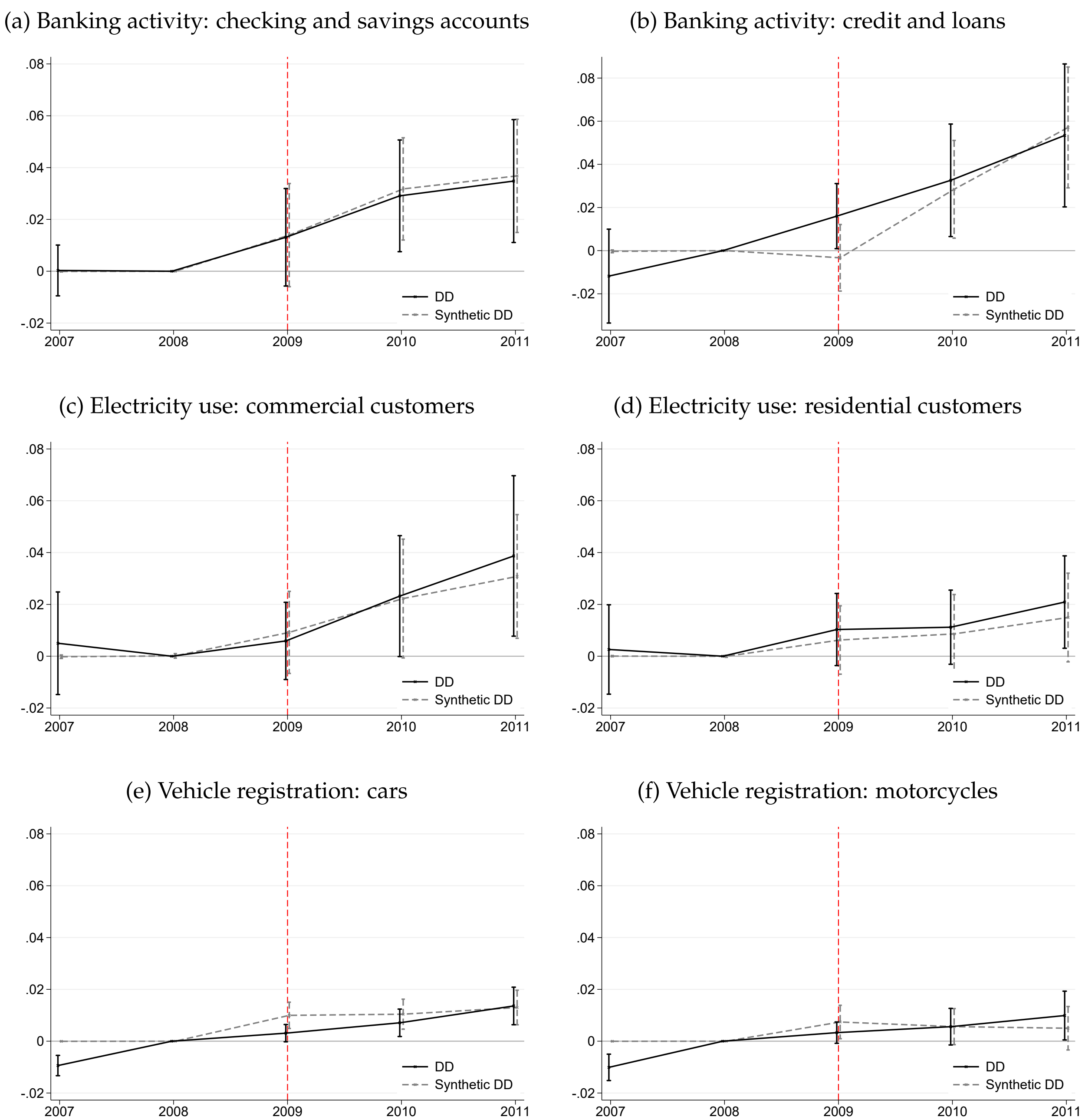
Figures 5c and 5d show that electricity use by commercial and residential customers also started to increase relatively more in treatment municipalities in 2009. For commercial customers, which correspond to firms in non-tradable industries (i.e., excluding agriculture and manufacturing), electricity consumption captures another variable production input, besides the number of formal employees. Electricity use by residential customers is a rare measure of non-durable consumption at the municipal level.

Figures 5e and 5f present results for the number of cars and motorcycles registered, which is the only form of physical asset with data available at the municipal level for our study period. DD estimates imply a relative increase for both variables in treatment municipalities after 2009, although they also reveal a differential pre-trend. This does not appear to be a concern in the case of cars because the results are robust to using the SDID estimator. However, SDID estimates are no longer significant at conventional levels by 2011 for motorcycles, so this result is more tentative.

B. Overall employment. Figure 6 provides evidence suggesting that overall employment also increased in treatment municipalities after the 2009 PBF expansion. Administrative data only capture formal employment, but a large share of the labor force works informally in Brazil and other developing countries. Therefore, we must rely on data from the annual *PNAD* surveys to investigate impacts on overall employment over time. A limitation of these surveys is that they are only meant to be representative at the state level. Therefore, the sampling scheme only covers around 15% of municipalities, and respondents are not meant to be representative of the population of these municipalities. Nevertheless, there is no reason to believe that the sampling scheme will be biased in a way that correlates with exposure to treatment after the 2009 PBF expansion.[42] In Figure 6, we thus present DD results following the specification in equation (6) using these data. *PNAD* surveys were not conducted in 2010, which was a census year, so we combine data from *PNAD* surveys for 2007, 2008, 2009, and 2011, together with data from the 2010 census in our analysis. We restrict attention to municipalities with some adult respondent

[42]We thank the editor and an anonymous referee for suggesting to add this analysis to the paper.

FIGURE 5: OTHER DIMENSIONS OF ECONOMIC ACTIVITY



Notes: The figure displays DD coefficients (solid black lines) and SDID coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (6) at the yearly level for the logarithm of various municipal outcomes. We consider the value of deposits in checking and savings accounts (panel a), the value of bank credits and loans (panel b), electricity use by commercial customers, which correspond to firms in non-tradable industries (panel c), electricity use by residential customers (panel d), the number of cars – and other 4-wheel vehicles such as SUVs – registered (panel e), and the number motorcycles – and other 2-wheel vehicles such as scooters – registered (panel f). The vertical lines indicate the year of the PBF expansion.

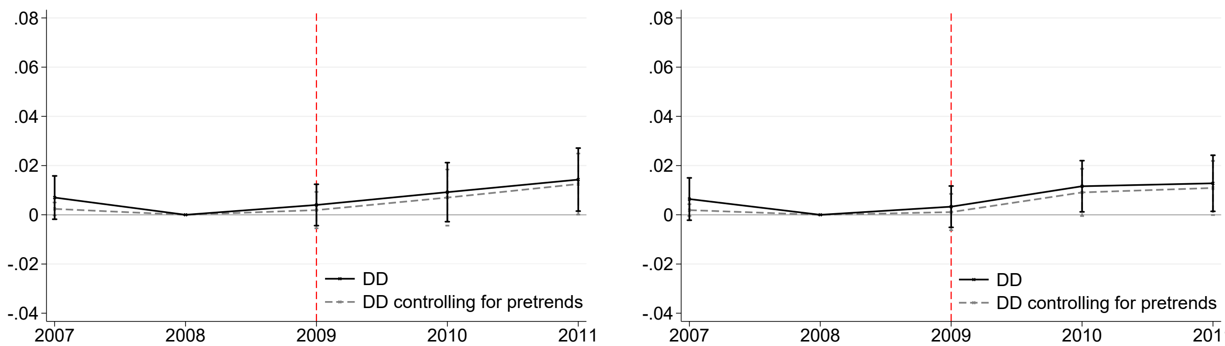
(age 18 to 60) in each year, resulting in a balanced panel of 744 municipalities. We run our regressions at the individual level to control for differences in individual characteristics correlated with employment outcomes (gender, race, education, urban area, age), which could be caused by changes in sample composition over time. For robustness, we also present results controlling for pre-trends in average outcomes at the municipal level.[43]

The outcomes in Figures 6a and 6b are dummy variables equal to one if the adult respondent is employed and if the adult respondent is in the labor force (i.e., employed or searching for a job), respectively. In both cases, we find a relative increase in treatment municipalities starting in 2009, which is robust to controlling for pre-trends. This evidence suggests that the increase in formal employment that we documented earlier in this section appears to be picking up an overall positive effect on local labor markets.

FIGURE 6: IMPACT ON EMPLOYMENT AND LABOR FORCE PARTICIPATION

(a) Employment

(b) Labor force participation



Notes: The figure displays DD coefficients (with their 95% confidence intervals) from estimating a similar specification as in equation (6) at the yearly level using microdata from the Brazilian labor force surveys (*PNAD*) for 2007, 2008, 2009, and 2011, together with microdata from the 2010 census. The outcome in panel (a) is a dummy variable indicating whether an individual reports any employment. The outcome in panel (b) is a dummy variable indicating whether an individual reports any employment or searching for jobs. We present estimates from running the regressions at the individual level, controlling for differences in individual characteristics (gender, race, education, urban area, age) correlated with employment outcomes (solid black lines) and further controlling for pre-trends in average outcomes at the municipal level (dashed gray lines). The vertical lines indicate the year of the PBF expansion.

C. GDP and taxes. Figure 7 shows that we also find an increase in economic activity in treatment municipalities after the 2009 PBF expansion using data on GDP and taxes on goods and services computed by *IBGE* every year. It is worth noting that GDP is computed using the production approach, adding the value added of all industries and all taxes levied on goods and services (value added taxes, excise taxes, other sales taxes, import taxes, and taxes on financial transactions). Its measurement is the result of extensive efforts by *IBGE* to produce estimates at the municipal level consistent with national accounts data at higher levels of aggregation. In particular, *IBGE* strives to include the entirety of economic activity in its measure of value added, including the informal sector.[44]

[43]We use this approach to net out any differential pre-trend in the outcomes because we cannot use the SDID estimator for this analysis (the individual level data are not longitudinal).

[44]Notably, since 2007, there has been a concerted effort to better account for informal activities. The 2007

Figures 7a and 7b begin by showing results for GDP and taxes using the log specification and presenting both DD and SDID estimates. Considering the sum of all taxes levied on goods and services separately is helpful since it is the one component of GDP that can be measured directly at the municipal level. Moreover, these taxes should respond positively to an increase in local economic activity. Estimating the other component of local GDP – the value added produced in the municipality – is always challenging and must necessarily rely on strong assumptions. Next, Figures 7c-7f present results using the specification in growth rates, which allows us to decompose the impact on overall GDP into the contribution of taxes and of the value added from non-tradable and tradable industries.

Figures 7a and 7b show that treatment municipalities experienced a relative increase in GDP and taxes after the 2009 PBF expansions. The growth specifications in Figures 7c and 7d confirm these findings. The increase in GDP reaches 1.8% by 2011; the increase in taxes reaches .4% of GDP or slightly above one fifth of the overall increase in GDP. Finally, Figures 7e and 7f highlight that the remaining increase in GDP can be entirely attributed to the value added created by non-tradable industries. We interpret the results on GDP and its sub-components as providing further evidence that treatment municipalities experienced a relative increase in economic activity that is consistent with the increase in cash-on-hand generating multiplier effects in the local economy.

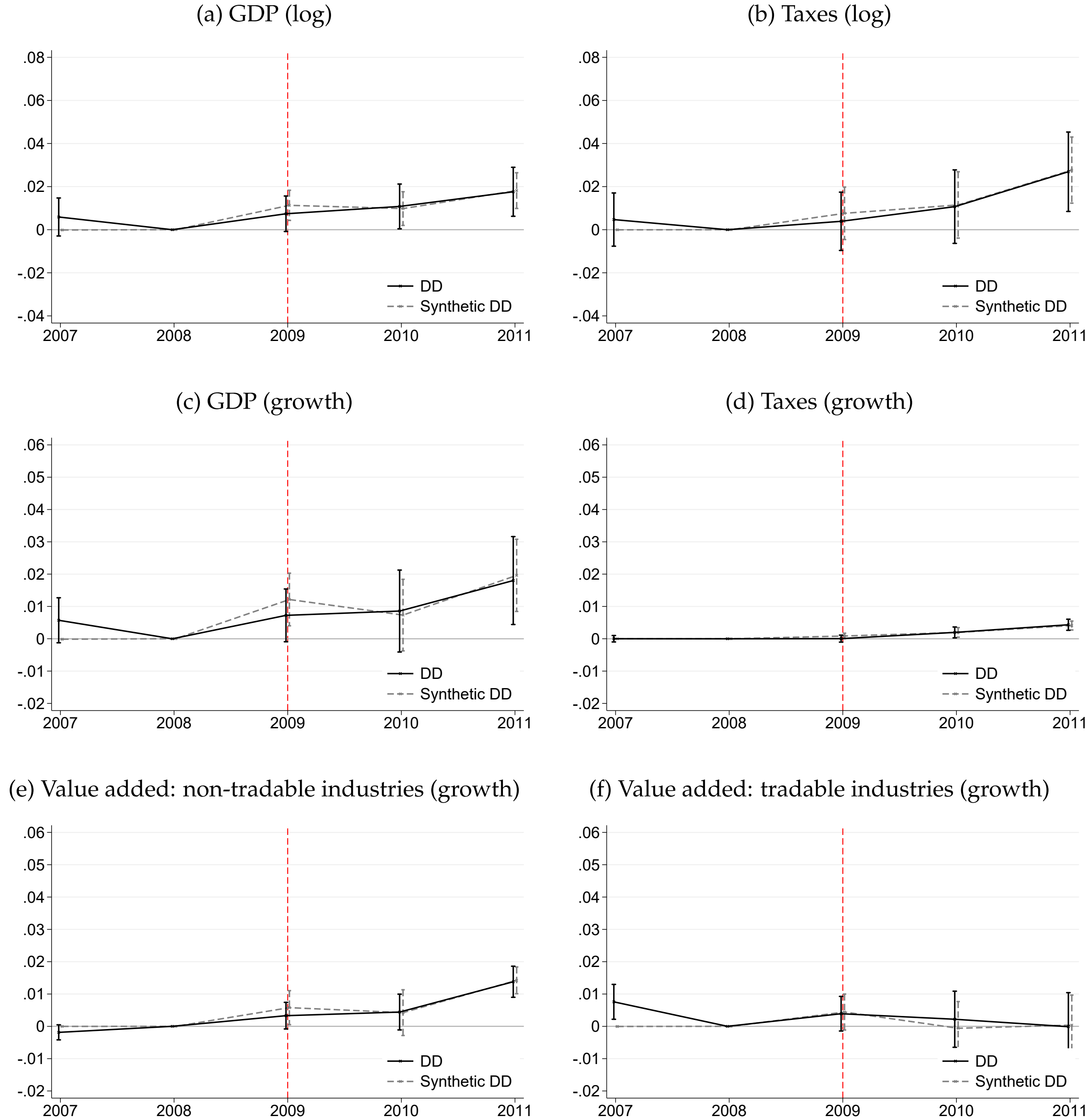
4 Behavioral responses to means testing

We show in this section that the 2009 PBF expansion also induced *negative* behavioral responses consistent with concerns that means-tested cash transfer programs incentivize families to alter their behaviors to qualify for benefits. Specifically, we find that the 2009 PBF expansion induced more families to be registered with income per capita below the eligibility thresholds in *Cadastro Único*.[45] Our analysis uses the two snapshots of *Cadastro Único* available over our study period and a variant of the specification in equation (6) comparing the growth in municipal outcomes between August 2010 and December 2008. In the next section, we combine this result and our evidence on the positive aggregate impact on local economic activity to evaluate the welfare effects of the 2009 PBF expansion.

revision of the national accounts system included a host of new data to better capture the value added of the informal economy and of non-profit economic activities. This is achieved by using, among other sources, household surveys, census data, data from the agricultural census, and data from various administrative systems including data from regulatory agencies that oversee utilities such as water, telecommunications, electricity, oil and gas. For instance, the output of industries that may be particularly informal such as construction is gauged based on the consumption of typical inputs, like cement. According to the national accounts system, the informal economy accounts for about 10% of GDP (see Hallak Neto et al. 2012). We note that data from formal employment contracts (*RAIS*) in particular are not used in the GDP calculations.

[45] Bergstrom et al. (2022) provides evidence of similar behavioral responses using a 2014 reform of PBF that increased the poverty and extreme poverty thresholds, as well as the benefit levels, nationally.

FIGURE 7: IMPACT ON GDP AND TAXES ON GOODS AND SERVICES



Notes: The figure displays DD coefficients (solid black lines) and SDID coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (6) at the yearly level using national accounts data disaggregated at the level of each municipality. Panels (a) and (b) display results for the logarithm of municipal GDP and taxes on goods and services. Panel (c) displays results for municipal GDP using the specification in growth rates; panels (d)-(f) decompose this impact into the contribution of taxes, of the value added from non-tradable industries, and of the value added from tradable industries. The estimates in panels (c)-(f) are all expressed in percentage of the municipal GDP in 2008. The vertical lines indicate the year of the PBF expansion.

We begin by shedding additional light on the impact of the 2009 expansion on PBF payments, using payment data in the month following each snapshot of *Cadastro Único*. In the first row of Table 3, column (1) shows that we obtain an estimate consistent with the results in the bottom panel of Table 2 using only these two months of data: a relative increase of 17% in treatment municipalities. Columns (2)-(4) decompose this effect into the contribution of three types of families. Column (2) shows that about three quarters of the increase in PBF payments is driven by families who were "already eligible" prior to the 2009 expansion, i.e., those who would have been eligible in 2010 based on their income per capita in *Cadastro Único* 2008. By contrast, column (3) shows that "previously ineligible" families – who were registered in *Cadastro Único* 2008 with income per capita too high to be eligible in 2010 – account for a very small share of the increase in PBF payments. Finally, column (4) shows that a sizable share of the overall effect comes from "newly registered" families, i.e., those who first registered in *Cadastro Único* after 2008.

TABLE 3: IMPACT ON PBF PAYMENT AND ELIGIBILITY BY BENEFICIARY GROUP

		Decomposing the overall effect by type of families		
	Overall effect [1]	Already eligible [2]	Previously ineligible [3]	Newly registered [4]
PBF Payments	0.170***	0.132***	0.004**	0.034***
	(0.007)	(0.006)	(0.001)	(0.002)
Number of eligible families	0.029***	0.012***	0.001	0.016***
	(0.003)	(0.002)	(0.000)	(0.002)

Notes: The table reports DD coefficients (with standard errors in parentheses) from estimating a variant of the specification in equation (6), comparing the growth in municipal outcomes between the two snapshots of *Cadastro Unico* in December 2008 and August 2010. Column [1] shows results for the overall change in PBF payments (first row) and in the number of families registered in *Cadastro Unico* as eligible for PBF (second row). Column [2]-[4] decompose this effect into the contribution of families who would have been eligible in 2010 based on their income per capita in *Cadastro Unico* 2008 (column 2), of those who were registered in *Cadastro Unico* 2008 with income per capita too high to be eligible in 2010 (column 3); and of those who first registered in *Cadastro Unico* after 2008 (column 4).

These impacts on PBF payments do not constitute evidence of behavioral responses. All families had an increased likelihood of receiving PBF benefits in 2010 in treatment municipalities *if* they were eligible in *Cadastro Único* after the start of the 2009 PBF expansion. It is the case, therefore, that the policy reinforced families' incentives to be registered as eligible in *Cadastro Único* in treatment municipalities. Yet, many families would have been eligible in 2010 even without any behavioral response. In treatment and control municipalities alike, many already eligible families would have remained eligible, some previously ineligible families would have experienced a drop in income per capita, and new families would have registered with income per capita below the eligibility thresholds.

We provide evidence of behavioral responses in the second row of Table 3 by showing that the 2009 PBF expansion increased the number of eligible families in treatment municipalities. Column (1) reports an estimated overall effect of 2.9%. Columns (2)-(4) show that this increase is driven by already eligible families – they were more likely to remain eligible in treatment municipalities – and by newly registered families – more families were induced to register with income per capita below the eligibility thresholds.[46] By contrast, we find no evidence of behavioral responses among previously ineligible families.[47]

In terms of magnitudes, an increase of 2.9% in the number of eligible families implies that, for every \$1 of extra PBF payment in treatment municipalities, \$.254 was paid to families who altered their behavior to qualify for PBF (based on the average PBF payment received by eligible families in treatment municipalities).

The relative increase in the number of eligible families could result from several margins of behavioral responses. Families with income per capita below the eligibility thresholds could become more likely to register, families could under-report their income per capita, or families could reduce their labor supply, particularly in the formal sector where income is more readily observed by the government. Among these three margins, labor supply responses are particularly relevant in the context of our study. The positive aggregate effects of the 2009 PBF expansion on local employment could occur despite negative labor supply responses among beneficiaries or, instead, be partly driven by positive responses among beneficiaries. Our aggregate results would underestimate the strength of local demand effects in the first case and overestimate it in the second case.

It is challenging to quantify the impact of the 2009 PBF expansion on the labor supply of families who became beneficiaries at the time (even if we focus on formal employment). One would have to find a suitable control group *within* the same municipality to net out local demand effects. Nevertheless, we show in Online Appendix C that concerns about negative labor supply responses are at least qualitatively relevant in our context. We use micro-level data and compare the formal labor supply of families eligible for different PBF benefit amounts through a Regression Discontinuity design around the PBF eligibility thresholds. We find no evidence that receiving higher PBF benefits increases formal employment outcomes. Point estimates are negative for both formal employment and formal earnings, which is consistent with the evidence on behavioral responses to means testing in developing countries (Bergolo and Cruces 2021; De Brauw et al. 2015). We interpret these results as suggesting that, if anything, our aggregate results on formal employment might underestimate the strength of the local demand effects of PBF.[48]

[46]This sizable role played by the registration margin is unlikely due to eligible families moving to areas where the program expanded more. We do not find evidence of differential changes in population (see footnote 32). Quotas weakly increased everywhere in the country and the quotas are binding only for new families (no families are 'kicked out' from the program if they keep meeting the eligibility criteria). Moreover, the increase in the number of eligible families in Bergstrom et al. (2022) is also partly due to newly registered families, but they study a national policy, so migration cannot be driving their results.

[47]In principle, all families have incentives to decrease their reported income in *Cadastro Único* to meet the eligibility criteria. However, if there is suspicion that a family is deliberately adjusting their reported income downward, they can be investigated and excluded from *Cadastro Único* (MDS, 2010).

[48]The magnitude of this bias is likely limited. Incentive effects are particularly strong at the eligibility thresholds, where a marginal increase in formal income could be perceived by families as carrying the

5 Implications

In this section, we assess the magnitude of the estimated effects of the 2009 PBF expansion on local economic activity through their implied cost per job and output multiplier. We also highlight how these aggregate effects impact the welfare evaluation of the policy.

5.1 Cost per job

The empirical literature on cross-region multipliers often focuses on the employment multiplier or the cost per job – the inverse of the employment multiplier – rather than the output multiplier because it is challenging to measure local GDP precisely (Chodorow-Reich, 2019). Following this literature (and in line with the summary results in Table 2), we regress the change in private-sector formal employment (FE) on the change in PBF payments (PBF) between 2008 (pre) and the average of 2010 and 2011 ($post$):

$$\frac{FE_{m,s,post} - FE_{m,s,pre}}{FE_{m,s,pre}} = \phi_s + \mu \cdot \frac{PBF_{m,s,post} - PBF_{m,s,pre}}{FE_{m,s,pre}} + \sum_k \gamma_k \cdot X^k_{m,s} + \varepsilon_{m,s}, \tag{7}$$

We scale the change in both variables in the same way so that μ has an employment multiplier interpretation (i.e., the increase in employment per \$1 of PBF payments). For identification, we instrument the (scaled) change in PBF payments by our treatment dummy and estimate equation (7) by 2SLS. The inclusion of the state fixed effects ϕ_s and the control variables $X^k_{m,s}$ ensures that we use the same variation as before for identification.

Table 4 presents the estimated employment multiplier and the estimated cost per job ($1/\mu$). The first row uses overall formal employment; the second row focuses on low-wage workers, who drive our results and for whom the estimates are more precise. Using this employment measure, we estimate that it costs \$9,799 of PBF payments to create one formal job or that \$100,000 generates 10.21 additional formal jobs. Results are comparable, albeit less precise, using the overall employment measure. A cost per job of \$9,799 at the yearly level, which corresponds to 3.67 times the yearly minimum wage at the time, is comparable to the preferred estimate in Corbi et al. (2019) of \$8,000.[49]

Table 4 also presents results that are consistent with the finding that multipliers tend to

risk of a discontinuous decrease in PBF benefits. The size of our estimates is thus likely to be local to the thresholds, and labor supply responses are likely smaller for the average PBF family. Moreover, we show in Online Appendix Figure B11 that the formal employment rate of already eligible families (and of previously ineligible families), which we can identify in both treatment and control municipalities, increased relatively more in treatment municipalities after the 2009 PBF expansion. Therefore, local demand effects dominated any labor supply response within that group, which accounts for most of the increase in PBF payments.

[49]These cost-per-job figures are smaller than typical cost-per-job estimates in the United States (Chodorow-Reich 2019), but there are clear differences in wage levels between Brazil and the United States. Our estimates are expressed in 2016 USD to make them directly comparable to those in Corbi et al. (2019).

TABLE 4: IMPLIED COST PER FORMAL JOB AND EMPLOYMENT MULTIPLIER

	Main Sample		By unemployment rate prior to the 2009 PBF expansion High (top 50%)		Low (bottom 50%)	
	Cost per job [1]	Jobs created (per \$100k) [2]	Cost per job [3]	Jobs created (per \$100k) [4]	Cost per job [5]	Jobs created (per \$100k) [6]
All jobs	12,261.85	8.16	5,593.35	17.88	103,155.41	0.97
	(6,913)	(4.60)	(2,304)	(7.36)	(488,606)	(4.59)
Low-wage jobs	9,799.10	10.21	6,342.02	15.77	22,410.63	4.46
	(3,340)	(3.48)	(1,974)	(4.91)	(18,722)	(3.73)

Notes: The table presents estimates (with their standard errors in parenthesis) of the cost per job – the inverse of the employment multiplier – and the number of jobs created for every \$100k in PBF outlays – the employment multiplier times \$100k – implied by the impact of the 2009 PBF expansion on private-sector formal employment. We report estimates using overall formal employment in the first row and focusing on low-wage workers (who drive our formal employment results) in the second row. The estimates in columns [1] and [2] consider all municipalities in our sample. Columns [3]-[6] present results for municipalities with higher (above median) and lower (below median) unemployment rates in the 2000 census, separately. Standard errors for the cost-per-job estimates are obtained using the delta method. All monetary values are in 2016 U.S. dollars (USD).

be higher when there is more slack in the economy (Chodorow-Reich 2019). The cost per job is smaller – and the employment multiplier is higher – if we estimate the specification in equation (7) separately for municipalities with higher vs. lower unemployment rates in the 2000 census (above vs. below the median).[50] Formal employment creation following the 2009 PBF expansion, which was a period of steady economic growth in Brazil, was thus concentrated in municipalities with a history of excess capacity in the labor market.

We interpret our estimates as *employment* multipliers because the evidence in Figure 6 suggests that the increase in formal employment following the 2009 PBF expansion was also associated with an increase in overall employment. Below, we analyze the implied output multiplier considering different assumptions on informal employment effects.[51]

5.2 Output multiplier

To quantify our findings in terms of output multiplier, we could simply estimate the specification in equation (7) replacing formal employment with the measure of municipal GDP computed by *IBGE*. This approach gives us a very large output multiplier: even if we only focus on the value-added created by non-tradable industries, we obtain a point estimate

[50]We note that these results are not affected by any pre-existing differences in private-sector formal employment rates. Indeed, for the results in columns (3)-(6), we re-weight municipalities such that the distribution of formal employment rates prior to the 2009 PBF expansion is the same between treatment and control groups, and is the same as the distribution in our main sample (used in columns 1 and 2).

[51]It is worth noting that the 2009 PBF expansion was funded by the general government budget and loans from international organizations (WorldBank September 17, 2010, MDS 2009a), with no contemporaneous tax increases (economic growth was increasing the tax base at the time). In such a case, Chodorow-Reich (2019) argues that a cross-region multiplier provides a "rough lower bound for a particular, policy-relevant type of national multiplier, the closed economy, no-monetary-policy-response, deficit-financed multiplier."

of 4.41 (the lower-bound of the 95% confidence interval is 1.27).[52] Therefore, given the strong assumptions necessarily embedded in any effort to disaggregate GDP at the local level, we prefer to use the methodology relating output and employment multipliers from Chodorow-Reich (2019). This approach also has the advantage of being robust to any price effects and allows us to evaluate the sensitivity of our estimate to different assumptions regarding the impact of the 2009 PBF expansion on the informal sector.

We start from the same production function: $Y = A \cdot (N \cdot L)^{1-\alpha}$, where N denotes hours per worker and L the number of effective units of labor. While the latter is equal to total employment E in Chodorow-Reich (2019), we introduce heterogeneity in productivity across three categories of workers (as in Cunha et al. 2022): $L = FE_{low} + \psi \cdot FE_{high} + \rho \cdot IW$, with FE_{low}, FE_{high}, and IW being the number of low-wage formal employees, high-wage formal employees, and informal workers, respectively. The factors ψ and ρ scale the relative productivity of high-wage formal employees and informal workers with respect to low-wage formal employees. We can then derive an expression for the output multiplier μ_Y as a function of the relevant formal employment multiplier $\mu_{FE_{low}}$:

$$\mu_Y = (1-\alpha)\left[\chi^E \frac{dE}{dFE_{low}}\frac{FE_{low}}{E} + \frac{FE_{low}}{L}\left(1 + \psi\frac{dFE_{high}}{dFE_{low}} + \rho\frac{dIW}{dFE_{low}}\right)\right]\frac{Y}{FE_{low}}\mu_{FE_{low}} \tag{8}$$

where χ^E denotes the elasticity of hours per worker to total employment.[53]

For calibration, we proceed as follows. We use the formal employment multiplier among low-wage workers in Table 4. We take $\chi^E = .12$, $(1-\alpha) = .666$, and $\rho = .55$ from Corbi et al. (2019). For total output (Y), we aggregate the municipal GDP estimates. We obtain the share of informal workers from the 2010 census ($IW/E = .493$). We take the number of low-wage private-sector formal employees from *RAIS* and compute their share ($FE_{low}/E = .274$) by combining the share of private-sector formal employees in the census with the ratio of low-wage vs. high-wage private-sector workers in *RAIS*. The remaining share of workers, which includes public employees, is pooled into the category of high-wage formal employees ($FE_{high}/E = .235$). We assume that the relative productivity of high-wage vs. low-wage formal employees can be captured by the ratio of their formal earnings among private-sector workers in *RAIS* ($\psi = 2.89$). Finally, we find no effect on other formal employees, so we have $\frac{dFE_{high}}{dFE_{low}} = 0$. For consistency, we compute all the above statistics in 2010 using only the municipalities in our main estimation sample.

Our benchmark calibration assumes no impact on the informal sector ($\frac{dE}{dFE_{low}} = 1$ and

[52]Using overall GDP, the point estimate is even larger (7.16), and it is much noisier (the lower-bound of the 95% confidence interval is 0.59).

[53]By comparison, the expression in Chodorow-Reich (2019) simplifies to $\mu_Y = (1-\alpha)\left(\chi^E + 1\right)\frac{Y}{E}\mu_E$. Online Appendix D derives the expression in equation (8) from the neoclassical production function.

$\frac{dIW}{dFE_{low}} = 0$). This is consistent with evidence from business cycle variation in developing countries that, while formal employment and total employment are strongly correlated, informal employment is essentially acyclical (see, e.g., Ohnsorge and Yu 2022). With this assumption, we obtain an output multiplier of 1.49 (s.e. 0.51). It is quantitatively important for our computation that we account for the finding that the increase in formal employment is concentrated among low-wage workers. The multiplier would reach 1.77 (s.e. 0.60) or 2.62 (s.e. 0.89) if we instead assumed that new jobs were as productive as the average job or as productive as the average formal job in the economy, respectively.

Existing estimates of local output multipliers in Brazil mainly consider the impact of government purchases rather than the impact of transfers to private households, and purchase multipliers are mechanically higher than transfer multipliers (Pennings 2021).[54] Corbi et al. (2019) consider the impact of increases in municipal government budgets and obtain multipliers ranging from 1.1 to 2.6 using a similar approach. Colonnelli and Prem (2022) computes purchase multipliers from 1.46 to 4.60 from anti-corruption spillovers in Brazil. For transfers multipliers, Egger et al. (2022) estimate a multiplier of 2.4 for a one-time transfer to poor families in rural Kenya, while Pennings (2021) estimate a multiplier of 1.5 for permanent transfers to old-age pensioners in the U.S.[55] Our estimate is lower than that of Egger et al. (2022), but we note that equation (8) assumes the productivity of each category of worker is fixed. Therefore, we may underestimate the multiplier if the 2009 PBF expansion led to similar productivity gains as observed in their setting.

Considering informality responses. Our benchmark calibration assumes no impact on the informal sector, but transfers to poor households might increase demand in both the formal and the informal sector alike. For instance, the output multiplier would increase to 1.94 (s.e. 0.66) if we assumed instead that one informal job was created for every two formal jobs (as in Corbi et al. 2019). We can also consider cases where informal employment and formal employment move in opposite directions following demand shocks. For instance, Ponczek and Ulyssea (2022) finds that the reduction in total employment in-

[54]For instance, in the New Keynesian model with rigid prices and wages, the multiplier from a permanent \$1 increase in government purchases and in transfers would be $1/(1-\alpha)$ and $\alpha^b/(1-\alpha)$, respectively. The parameters α and α^b correspond to the marginal propensity to spend on locally produced goods and services for the average household and for those households receiving the transfer, respectively. The numerators in these expressions capture the direct impact on the local economy, which is an increase in spending of 1 for government purchases but of only $\alpha^b \leq 1$ for transfers; the denominators capture the general equilibrium effects. These expressions also illustrate why the empirical literature on purchase multipliers typically uses a reference value of 1 and why the same benchmark does not apply to transfer multipliers.

[55]In a paper that followed our study, Feler et al. (2023) use a Bartik-style instrument that relies on the differential impact of national variation in PBF transfers between poorer and richer states in Brazil, and obtain a multiplier of 2.2. In another recent working paper, Cunha et al. (2022) obtain a multiplier in the range of 0.5-1.5 using a Bartik-style instrument that relies on the differential impact of national variation in top-up transfers to PBF payments during the Covid pandemic – thus, at a time of severe constraints on economic activity – between municipalities with higher vs. lower shares of the population receiving PBF.

duced by the unilateral trade liberalization adopted in Brazil in the 1990s was only a third of the decrease in formal employment, because informal employment increased. Assuming that the impact on total employment was only a third of the formal employment effect, the multiplier would decrease to .89 (s.e. 0.30). However, this assumption is likely too extreme in our case. The negative demand effects arising directly from the trade liberalization were heavily concentrated in the formal sector, i.e., domestic manufacturing faced increased import competition. By contrast, the direct demand effect of an increase in PBF transfers is likely to benefit both the formal and the informal sectors. We thus consider our benchmark calibration – grounded in the evidence from business-cycle variation in developing countries – as rather conservative.[56] Yet, we show next that even using an output multiplier of .89, the aggregate effects on the local economy that we estimate can have a considerable impact on the welfare evaluation of the 2009 PBF expansion.

5.3 Marginal Value of Public Funds

We end our analysis by evaluating the welfare effects of the 2009 PBF expansion. The primary aim of a cash transfer program such as PBF is not to stimulate the economy and, arguably, its merit should not be based on the size of its output multiplier. Moreover, the link between output multipliers and welfare effects must be examined carefully.

Specifically, we compute a marginal value of public funds (MVPF) for the 2009 PBF expansion. The MVPF of a policy corresponds to the ratio of the beneficiaries' willingness-to-pay (WTP) for its benefits to its net cost to the government (Finkelstein and Hendren 2020), where a value of 1 corresponds to the benchmark of a non-distortionary policy. The MVPF also informs welfare by specifying how much a government must value spending on a policy, compared to spending \$1 on their next best alternative, for the policy to be welfare improving. In other words, the welfare gain is: $dW = \omega \cdot MVPF - 1$, where ω captures the money-metric welfare gain from giving \$1 to the policy's beneficiaries, i.e., their "social welfare weight" divided by the shadow value of public funds.

We proceed in steps to highlight the implications of our findings for the MVPF of the 2009 PBF expansion, considering for simplicity the MVPF of a marginal change in benefits.

A. Considering the direct impact on PBF beneficiaries. A first step is to consider the direct impact from receiving the additional PBF benefits. For this purpose, it is useful to distinguish between infra-marginal beneficiaries who received the extra benefits without changing their behaviors and marginal beneficiaries who changed their behavior to become eligible (Finkelstein and Hendren 2020). For a cash transfer, we can assume that the first group values receiving \$1 in benefits at \$1: $WTP^I = 1$. By contrast, assuming that families make privately optimal decisions, the welfare effect is nil for marginal

[56] In fact, Feler et al. (2023) find positive effects on formal *and* informal employment in their recent study.

beneficiaries: $WTP^M = 0$. We reported in Section 4 that our estimates imply that, for every $1 of extra PBF payment in treatment municipalities, $.254 was received by marginal beneficiaries. We thus have: $MVPF_1 = \frac{.746 \cdot WTP^I + .254 \cdot WTP^M}{1} = .746$.[57] For reference, the comparable figure is .904 for the means-tested cash transfer in Uruguay studied in Bergolo and Cruces (2021); it would simply be 1 for the NGO transfer in Egger et al. (2022).

B. Adding the impact on tax revenues. The net cost to the government – the denominator of the MVPF – includes not only the direct cost of the policy but also any additional impact on tax revenues. This is a typical concern with transfer programs, especially when the use of means testing induces beneficiaries to reduce their labor supply. For instance, taking into account the negative tax revenue implications from the partial equilibrium reduction in formal employment, the MVPF in Bergolo and Cruces (2021) drops to .61. Yet, as we show in this paper, there can be relevant aggregate responses in the local economy generating positive impacts on tax revenue. Estimating a similar specification as in equation (7), we obtain a tax multiplier of: $\frac{dTax}{dPBF}$ =.58 (s.e. 0.25).[58] Adding the impact on tax revenues thus increases the MVPF of the 2009 PBF expansion to: $MVPF_2 = \frac{.746}{1-.58} = 1.777$, raising it *above* the value of a non-distortionary transfer.

C. Adding the WTP for the increase in local economic activity. The aggregate impacts of the 2009 PBF expansion on the local economy can also affect the numerator of the MVPF by increasing the WTP for the policy. Yet, these gains in economic activity are worth less than $1 per $1 in welfare terms because they come at an opportunity cost (e.g., increases in production inputs such as labor and electricity).[59] In fact, an increase in economic activity must not imply any first-order effect on welfare if all markets in the economy are at a socially efficient equilibrium. One source of distortion that induces a "wedge" between social marginal benefits and social marginal costs comes from taxes, which we have already accounted for through the denominator of the MVPF. Other distortions could also imply efficiency gains from increases in economic activity. Our finding of an increase in labor demand for lower-skilled workers with no increase in wages suggests the existence of excess supply in the labor market, and is consistent with the strong bunching at the minimum wage during our study period (Engbom and Moser 2022). De Loecker and Eeckhout (2018) estimate a high degree of market power in Brazilian product markets. As a first-order approximation, the WTP for increases in economic activity in these two mar-

[57] Specifically, we obtain this value of .254 by estimating a similar specification as in equation (7) providing us with an estimate of the increase in the number of eligible families per $1 in PBF benefits, which we multiply by the average PBF payment received by eligible families in treatment municipalities.

[58] The measure of local taxes from *IBGE* includes all taxes on goods and services but it does not include personal income taxes, so it would not capture changes in tax revenue from increases in labor income. This should not affect our results, however: the positive impact on formal employment is concentrated among low-wage workers who are exempt from personal income taxation in Brazil.

[59] Egger et al. (2022) do not find any increase in production inputs, so that the output gains appear to be driven by productivity gains, and the WTP for an additional $1 in the economy would be $1 in their case.

kets would be captured by the gap between wage and reservation wage and by the price markup, respectively.[60] Thus, to illustrate the importance of accounting for the aggregate effects of the 2009 PBF expansion, we combine the impacts on formal employment and output with estimates for these two wedges in the numerator of the MVPF formula.

We are not aware of any estimate of the relevant wedge for the labor market in Brazil. In a recent survey, Mui and Schoefer (2024) find an average 'reservation raise' – the reservation wage divided by the actual or potential wage – of .714 for unemployed workers in the U.S. This figure is in line with structural estimates in Engbom and Moser (2022), who find a ratio of the flow value of leisure to the average wage between .6 and .8 for workers at the bottom of the ability distribution. Therefore, using the value for the U.S., we obtain a WTP for the increase in formal employment of \$.107 per \$1 in PBF payment.[61] This raises the MVPF of the 2009 PBF expansions to $MVPF_3 = \frac{.746+.107}{1-.58} = 2.032$.

We obtain a WTP for the increase in output of \$.262 using an average markup of 1.288 estimated for firms in the Annual Surveys of Trade (*PAC*) and Services (*PAS*) following De Loecker and Eeckhout (2018).[62] Considering the efficiency gains in both markets thus increases the MVPF of the 2009 PBF expansion to $MVPF_4 = \frac{.746+.107+.262}{1-.58} = 2.656$.

D. Discussion. To provide some comparison, Figure 8 displays our four estimates above (dashed vertical lines) with MVPF estimates for four types of policies from the *Policy Impacts Library*: any type of cash-based transfers, unemployment insurance programs (as important cash-based social protection programs across countries), and two categories for which MVPFs are usually small (Job Training) and very large (Child Health and Education). Once we account for the positive aggregate effects of the 2009 PBF expansion, Figure 8 shows that its MVPF not only increases above that of a non-distortionary transfer (solid vertical line), but that it also becomes relatively high. It increases above most of the MVPF estimates for cash transfer, unemployment insurance and job training policies, and even above some estimates for Child Health and Education policies. The graph also highlights the paucity of estimates from developing countries.

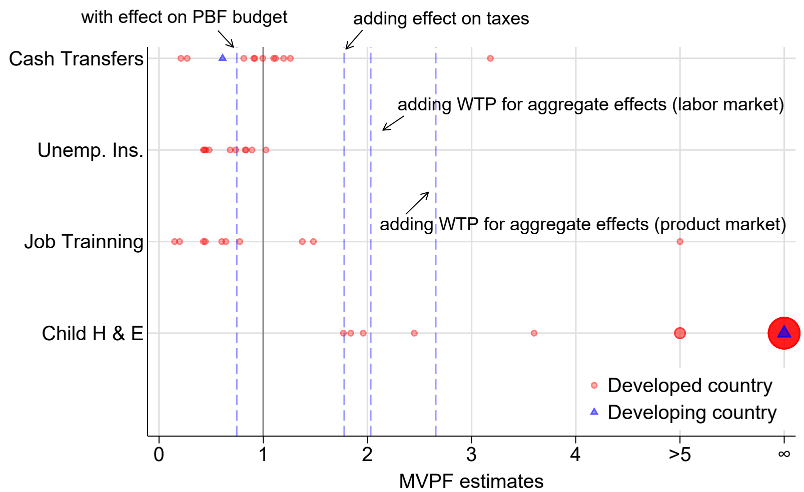
A relatively high MVPF implies that a policy yields a relatively high welfare return

[60] Sims and Wolff (2018) show that this first-order approximation carries out in a stylized general equilibrium model of the economy linking output multiplier and "utility" multiplier.

[61] The WTP is computed as: $\mu_{FE_{low}} \cdot w_{FE_{low}} \cdot (1 - .714)$, where $\mu_{FE_{low}}$ is the relevant formal employment multiplier and $w_{FE_{low}}$ is the average wage among low-wage formal employees in 2010.

[62] The WTP is the product of the estimated output multiplier (net of the tax multiplier to avoid double-counting) and the average markup (minus 1). Following the 'production approach', the average markup in De Loecker and Eeckhout (2018) is the sales-weighted average of a markup for each firm i in year t equal to: $\alpha_{it} \cdot (P_{it}Q_{it})/(P^V_{it}V_{it})$, where the first term is the output elasticity and the second term is the ratio of sales to expenditures on variable inputs. De Loecker and Eeckhout (2018) compute the firm-level markups by using their elasticity estimates from the U.S. for the same industry and year (assuming that "firms in the same sector have access to the same technology, yet they differ in TFP") and the ratio of sales to the Cost of Goods Sold in their data. We calculate this ratio in the Brazilian surveys in 2010. We do not use their mark-up estimate for Brazil because it is computed for listed firms only and includes manufacturing firms.

FIGURE 8: IMPLIED MARGINAL VALUE OF PUBLIC FUNDS



Notes: The figure compares estimates of the marginal value of public funds (MVPF) for the 2009 PBF expansion with MVPF estimates from *policyimpacts.org* (as of March 2024) for policies categorized as "Cash Transfers", "Unemployment Insurance", "Job Training" and "Child Health" or "Child Education" (focusing on those with $MVPF \geq 0$). Circles and triangles indicate estimates for policies in developed countries and developing countries, respectively. Because the last two ticks on the x-axis group studies with $MVPF > 5$ and $MVPF = \infty$, respectively, the size of the markers are proportional to the number of studies in the group. The dashed vertical lines indicate the four values of the MVPF of the 2009 PBF expansion discussed in the text. The solid gray vertical line marks the benchmark value for a non-distortionary transfer ($MVPF = 1$).

compared to other policies benefiting the same population. Yet, whether the government should allocate an additional dollar on this specific policy relative to other policies more generally depends also on welfare weights. Considering only the direct impact on PBF beneficiaries, the 2009 PBF expansion was welfare improving if the government valued spending \$.746 on the policy more than spending \$1 on their next best alternative or $\omega^{PBF} > 1.34$. This condition might already be satisfied given that PBF targets poor families, which likely carry a high social welfare weight. It weakens considerably once we add the aggregate impact on tax revenues: the minimum necessary welfare weight on infra-marginal beneficiaries drops to $\omega^{PBF} > .56$. Considering potential efficiency gains in the labor and product markets would further reduce the minimum necessary welfare weight on the policy's beneficiaries. However, this would depend on who benefits from these aggregate effects, as they likely carry a different welfare weight than PBF beneficiaries. Denoting those weights by ω^{AggL} and ω^{AggP} in the two markets, we have: $\omega^{PBF} > .56 - .144 \cdot \omega^{AggL} - .351 \cdot \omega^{AggP}$.[63] It is worth noting that, although the efficiency gains in the product market has a greater influence on the MVPF than the efficiency gains in the labor market, these gains benefit producers and low-wage formal employees, respectively. We thus may have $\omega^{AggL} > \omega^{AggP}$, although both groups likely carry lower welfare weights than PBF beneficiaries who are at the bottom of the income distribution.

[63] It follows from: $dW > 0 \Leftrightarrow 1.777 \cdot \omega^{PBF} + (2.032 - 1.777) \cdot \omega^{AggL} + (2.656 - 2.032) \cdot \omega^{AggP} > 1$.

E. Considering informality responses. The welfare implications from considering the aggregate impact on tax revenues only depend on the tax multiplier, so they are robust to any assumption regarding the impacts of the 2009 PBF expansion on the informal sector. The results from considering potential efficiency gains in labor and product markets assume no impact on the informal sector. Relaxing this assumption, the MVFP would be unchanged if there is no wedge in the informal labor and product markets. At the other extreme, we would still have $MVPF_4 = 2.149$ even if we assume that the reservation raise and the markup were the same in the formal and informal sectors and that the increase in total employment was only a third of the increase in formal employment.

6 Conclusion

Our paper sheds new light on the aggregate effects of cash transfer programs targeting poor households in developing countries. We document that an expansion of the largest cash transfer program in the world – *Programa Bolsa Família* (PBF) – increased local economic activity. This occurred despite the fact that the program is means-tested, and that a quarter of the additional benefits were paid to families who changed their behaviors to be eligible for the program expansion. In fact, we find a positive aggregate effect even for the margin of economic activity – formal employment – that should be most adversely impacted by means-testing. Our findings are consistent with spillovers effects of cash transfers through increases in local demand: the increase in economic activity is concentrated in non-tradable industries and most of the formal employment gains are captured by non-beneficiaries. These aggregate effects considerably improve the welfare effects of the policy, raising its MVPF well above the value of a non-distortionary transfer.

Importantly, whether the expansion of a cash transfer program like PBF provides the 'best bang for the buck' depends on the alternative uses of resources. Developing countries use other tools of social assistance: in-kind transfers, asset transfers, public employment programs, among others. Yet, despite a growing body of evidence on the impacts of such policies (Banerjee et al. 2024), it remains challenging to understand the relative returns of spending on these different programs. A useful step in that direction would be for researchers to adopt a unifying welfare metric when evaluating these programs, such as the MVPF. Comparing the willingness-to-pay for the direct impact of these policies to the net cost for the government would allow a systematic comparison across policies, to the extent that they target a similar population of beneficiaries. The spillover effects of these programs on non-beneficiaries introduces important incidence questions, however. Who gains from these external effects and what are their welfare weights?

How much a government should spend on social assistance programs also depends on the welfare cost of raising revenue, i.e., another use of resources is to reduce taxes. The evidence that cash transfers to poor households can increase local economic activity, improving the welfare effect of spending resources in that way, raises a natural question. Do taxes imply similar demand effects in the opposite direction, increasing the welfare cost of raising revenue? While the empirical literature on taxation in developing countries has greatly expanded in the last ten years, we still know little about the potential economy-wide effects of transferring resources from taxpayers to governments. Evidence from the U.S. indicate that, although tax changes for lower-income taxpayers can have sizable impacts on aggregate economic activity, tax changes for high-income groups have only small aggregate effects (Zidar 2019). Yet, it remains unclear how progressive tax systems in developing countries truly are, given the greater reliance on indirect taxation and the more limited enforcement capacity (Bachas et al. 2024). More research is needed to understand the potential demand effects of raising the resources that fund social assistance policies.

References

ABADIE, A., S. ATHEY, G. W. IMBENS, AND J. M. WOOLDRIDGE (2023): "When should you adjust standard errors for clustering?" *The Quarterly Journal of Economics*, 138, 1–35.

ANGELUCCI, M. AND G. DE GIORGI (2009): "Indirect effects of an aid program: How do cash transfers affect ineligibles' consumption?" *American Economic Review*, 99, 486–508.

ARKHANGELSKY, D., S. ATHEY, D. A. HIRSHBERG, G. W. IMBENS, AND S. WAGER (2021): "Synthetic Difference-in-Differences," *American Economic Review*, 111, 4088–4118.

BACHAS, P., L. GADENNE, AND A. JENSEN (2023): "Informality, consumption taxes, and redistribution," *Review of Economic Studies*, rdad095.

BACHAS, P., A. JENSEN, AND L. GADENNE (2024): "Tax Equity in Low-and Middle-Income Countries," *Journal of Economic Perspectives*, 38, 55–80.

BAIRD, S., D. MCKENZIE, AND B. ÖZLER (2018): "The effects of cash transfers on adult labor market outcomes," *IZA Journal of Development and Migration*, 8, 22.

BANDIERA, O., R. BURGESS, N. DAS, S. GULESCI, I. RASUL, AND M. SULAIMAN (2017): "Labor Markets and Poverty in Village Economies," *The Quarterly Journal of Economics*, 132, 811–870.

BANERJEE, A., R. HANNA, B. A. OLKEN, AND D. S. LISKER (2024): "Social protection in the developing world," .

BANERJEE, A. V., R. HANNA, G. E. KREINDLER, AND B. A. OLKEN (2017): "Debunking the stereotype of the lazy welfare recipient: Evidence from cash transfer programs," *The World Bank Research Observer*, 32, 155–184.

BERGOLO, M. AND G. CRUCES (2021): "The anatomy of behavioral responses to social assistance when informal employment is high," *Journal of Public Economics*, 193, 104313.

BERGSTROM, K., W. DODDS, AND J. RIOS (2022): "Welfare Analysis of Changing Notches: Evidence from Bolsa Família," *World Bank Policy Research Working Paper 10117*.

BORUSYAK, K. AND P. HULL (2023): "Nonrandom Exposure to Exogenous Shocks," *Econometrica*, 91, 2155–2185.

BOSCH, M. AND R. M. CAMPOS-VAZQUEZ (2014): "The Trade-Offs of Welfare Policies in Labor Markets with Informal Jobs: The Case of the “Seguro Popular" Program in Mexico,” *American Economic Journal: Economic Policy*, 6, 71–99.

BROLLO, F., K. KAUFMANN, AND E. LA FERRARA (2020): “The political economy of program enforcement: Evidence from Brazil,” *Journal of the European Economic Association*, 18, 750–791.

CHODOROW-REICH, G. (2019): “Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?” *American Economic Journal: Economic Policy*, 11, 1–34.

COLONNELLI, E. AND M. PREM (2022): “Corruption and firms,” *The Review of Economic Studies*, 89, 695–732.

CORBI, R., E. PAPAIOANNOU, AND P. SURICO (2019): “Regional transfer multipliers,” *The Review of Economic Studies*, 86, 1901–1934.

COSTA, F., J. GARRED, AND J. P. PESSOA (2016): “Winners and losers from a commodities-for-manufactures trade boom,” *Journal of International Economics*, 102, 50–69.

CUNHA, D., J. PEREIRA, R. A. PERRELLI, AND F. TOSCANI (2022): “Estimating the Employment and GDP Multiplier of Emergency Cash Transfers in Brazil,” *IMF Working Paper WP/22/55*.

DE BRAUW, A., D. O. GILLIGAN, J. HODDINOTT, AND S. ROY (2015): “Bolsa Família and household labor supply,” *Economic Development and Cultural Change*, 63, 423–457.

DE CHAISEMARTIN, C. AND X. D’HAULTFOEUILLE (2022): “Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: a survey,” *The Econometrics Journal*, 26, C1–C30.

DE LOECKER, J. AND J. EECKHOUT (2018): “Global Market Power,” *NBER Working Paper*, 24768.

DENES, G., B. K. KOMATSU, AND N. MENEZES-FILHO (2018): “Uma avaliação dos impactos macroeconômicos e sociais de programas de transferência de renda nos municípios brasileiros,” *Revista Brasileira de Economia*, 72, 292–312.

DIX-CARNEIRO, R. AND B. K. KOVAK (2019): “Margins of labor market adjustment to trade,” *Journal of International Economics*, 117, 125–142.

EGGER, D., J. HAUSHOFER, E. MIGUEL, P. NIEHAUS, AND M. WALKER (2022): “General equilibrium effects of cash transfers: experimental evidence from Kenya,” *Econometrica*, 90, 2603–2643.

ELBERS, C., J. O. LANJOUW, AND P. LANJOUW (2003): “Micro–level estimation of poverty and inequality,” *Econometrica*, 71, 355–364.

ENGBOM, N. AND C. MOSER (2022): “Earnings Inequality and the Minimum Wage: Evidence from Brazil,” *American Economic Review*, 112, 3803â47.

FELER, L., A. MENDES, W. MIYAMOTO, T. L. NGUYEN, AND S. PENNINGS (2023): “The Macroeconomic Effects of Cash Transfers: Evidence from Brazil,” *World Bank Policy Research Working Paper*.

FILMER, D., J. FRIEDMAN, E. KANDPAL, AND J. ONISHI (2021): “Cash Transfers, Food Prices, and Nutrition Impacts on Ineligible Children,” *The Review of Economics and Statistics*.

FINKELSTEIN, A. AND N. HENDREN (2020): “Welfare analysis meets causal inference,” *Journal of Economic Perspectives*, 34, 146–67.

FRANKLIN, S., C. IMBERT, G. ABEBE, AND C. MEJIA-MANTILLA (2024): “Urban Public Works in Spatial Equilibrium: Experimental Evidence from Ethiopia,” *American Eco-*

nomic Review, 114, 1382â1414.

Garganta, S. and L. Gasparini (2015): "The impact of a social program on labor informality: The case of AUH in Argentina," *Journal of Development Economics*, 115, 99–110.

Gassmann, F., U. Gentilini, J. Morais, C. Nunnenmacher, Y. Okamura, G. Bordon, and G. Valleriani (2023): "Is the Magic Happening? A systematic literature review of the economic multiplier of cash transfers," *World Bank Policy Research Working Paper*.

Gerard, F. and G. Gonzaga (2021): "Informal labor and the efficiency cost of social programs: Evidence from unemployment insurance in brazil," *American Economic Journal: Economic Policy*, 13, 167–206.

Hackmann, M., J. Heining, R. Klimke, M. Polyakova, and H. Seibert (2022): "Health Insurance as Economic Stimulus? Evidence from Long-Term Care Jobs," *Working Paper*.

Hallak Neto, J., K. Namir, and L. Kozovits (2012): "Setor e emprego informal no Brasil: análise dos resultados da nova série do sistema de contas nacionais-2000/07," *Economia e Sociedade*, 21, 93–113.

IBGE (2009): "Metodo de Construcao do Mapa de Pobreza Utilizando a PNAD 2006 e o Censo Demografico 2000," Tech. rep., Instituto Brasileiro de Geografia e Estatística - IBGE.

——— (2017): *Divisão regional do Brasil em regiões geográficas imediatas e regiões geográficas intermediárias : 2017*, Instituto Brasileiro de Geografia e Estatística - IBGE.

Imbert, C. and J. Papp (2015): "Labor market effects of social programs: Evidence from India's employment guarantee," *American Economic Journal: Applied Economics*, 7, 233–63.

Jensen, A. (2022): "Employment structure and the rise of the modern tax system," *American Economic Review*, 112, 213–234.

Johnson, D. S., J. A. Parker, and N. S. Souleles (2006): "Household expenditure and the income tax rebates of 2001," *American Economic Review*, 96, 1589–1610.

Kabeer, N. a. and H. Waddington (2015): "Economic impacts of conditional cash transfer programmes: a systematic review and meta-analysis," *Journal of Development Effectiveness*, 7, 290–303.

Kekre, R. (2023): "Unemployment Insurance in Macroeconomic Stabilization," *Review of Economic Studies*, 90, 2439–2480.

Krueger, A. B. (2012): "The rise and consequences of inequality in the United States," *Speech at the Center for American Progress*, 12.

Lalive, R., C. Landais, and J. Zweimüller (2015): "Market externalities of large unemployment insurance extension programs," *American Economic Review*, 105, 3564–96.

Lindert, K., A. Linder, J. Hobbs, and B. De la Brière (2007): "The nuts and bolts of Brazil's Bolsa Família Program: Implementing conditional cash transfers in a decentralized context," 709.

MDS (2008): "PORTARIA GM/MDS No 341: Dispõe sobre procedimentos operacionais necessários ao ingresso de famílias no Programa Bolsa Família," Tech. rep., Ministerio do Desenvolvimento Social (MDS).

——— (2009a): "Financiamento da Assistência Social no Brasil. Caderno SUAS IV," Tech. rep., Ministerio do Desenvolvimento Social (MDS) - Secretaria Nacional de Assistência

Social.

——— (2009b): "Nota tecnica 70: Atualizacao das estimativas municipais," Tech. rep., Ministerio do Desenvolvimento Social (MDS).

——— (2010): "Orientações para a Fiscalização e Controle Social do Programa Bolsa Família. Guias e Manuais: Ministerio do Desenvolvimento Social," Tech. rep., Ministerio do Desenvolvimento Social (MDS).

——— (2012): "Nota tecnica 152: Atualizacao das estimativas municipais," Tech. rep., Ministerio do Desenvolvimento Social (MDS).

MICHAILLAT, P. AND E. SAEZ (2015): "Aggregate Demand, Idle Time, and Unemployment," *Quarterly Journal of Economics*, 130, 507–569.

MUI, P. AND B. SCHOEFER (2024): "Reservation Raises: The Aggregate Labour Supply Curve at the Extensive Margin," *The Review of Economic Studies*, rdae021.

MURALIDHARAN, K., P. NIEHAUS, AND S. SUKHTANKAR (2023): "General equilibrium effects of (improving) public employment programs: Experimental evidence from India," *Econometrica*, 91, 1261–1295.

NAKAMURA, E. AND J. STEINSSON (2014): "Fiscal Stimulus in a Monetary Union: Evidence from U.S. Regions," *American Economic Review*, 104, 753–792.

NERI, M. C., F. M. VAZ, AND P. H. G. F. D. SOUZA (2013): "Efeitos macroeconômicos do Programa Bolsa Família: uma análise comparativa das transferências sociais," *Programa Bolsa Família: uma década de inclusão e cidadania. Brasília: Ipea*, 1, 193–206.

NIEHAUS, P. AND T. SURI (2024): "Cash Transfers," in *The Handbook of Social Protection: Evidence to Inform Policy in Low- and Middle-Income Countries*, ed. by R. Hanna and B. Olken.

OHNSORGE, F. AND S. YU, eds. (2022): *The Long Shadow of Informality: Challenges and Policies*, Washington, DC: World Bank.

PENNINGS, S. (2021): "Cross-region transfer multipliers in a monetary union: Evidence from social security and stimulus payments," *American Economic Review*, 111, 1689–1719.

PONCZEK, V. AND G. ULYSSEA (2022): "Enforcement of labour regulation and the labour market effects of trade: Evidence from Brazil," *The Economic Journal*, 132, 361–390.

SADOULET, E., A. DE JANVRY, AND B. DAVIS (2001): "Cash Transfer Programs With Income Multipliers: PROCAMPO in Mexico," *World Development*, 1043–1056.

SIMS, E. AND J. WOLFF (2018): "The Output and Welfare Effects of Government Spending Shocks Over the Business Cycle," *International Economic Review*, 59, 1403–1435.

SOARES, S. S. D. (2009): *Metodologias para estabelecer a linha de pobreza: Objetivas, subjetivas, relativas e multidimensionais*, Instituto de Pesquisa Econômica Aplicada (IPEA).

ULYSSEA, G. (2020): "Informality: Causes and consequences for development," *Annual Review of Economics*, 12, 525–546.

WORLDBANK, T. (September 17, 2010): "Brazil's Landmark Bolsa Família Program Receives US$200 Million Loan," *Press Release, World Bank*.

ZIDAR, O. (2019): "Tax cuts for whom? Heterogeneous effects of income tax changes on growth and employment," *Journal of Political Economy*, 127, 1437–1472.