Cash Transfers and Formal Labor Markets: Evidence from Brazil *

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June 2021

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Abstract

Cash transfer programs have expanded widely in developing countries and have been credited for sizable reductions in poverty. However, their potential disincentive effects on beneficiaries' labor supply have spurred a heated policy debate. This paper studies the impact of a large-scale program (*Bolsa Familia* in Brazil) on local labor markets in a context where such concerns could be particularly strong: eligibility is *means-tested* and we focus on the *formal* labor market, where earnings are more easily verifiable. Yet, we find that an expansion of *Bolsa Familia* increased local formal employment, using variation in the size of the reform across municipalities. The evidence is consistent with multiplier effects of cash transfers in the local economy, which dominate potential negative effects on formal labor supply among beneficiaries.

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Cash transfer programs are the main type of safety net in middle-income countries, covering about 30 percent of the poor (Honorati et al. 2015). It is well established that these programs reduce poverty and improve the lives of beneficiaries in several important dimensions.[1] However, their potential impacts on the labor market have spurred a heated policy debate. Fears over "lazy welfare recipients" that often fuel controversy over the future of these programs are generally unwarranted (Banerjee et al. 2017). Yet, concerns remain that the targeting of some of these programs, such as the use of means testing, could distort beneficiaries' incentives work, especially in the formal economy where earnings are more easily verifiable (Levy 2008; for recent evidence, see Bergolo and Cruces 2021). By contrast, some studies argue that cash transfers can help beneficiaries find better jobs by unlocking liquidity constraints (Baird et al. 2018), or that they can stimulate labor markets through increases in labor demand due to multiplier effects in the local economy (e.g., Neri et al. 2013, Egger et al. 2019). In practice, these effects can coexist, and there is limited evidence on their relative importance to inform the policy debate.[2]

This paper contributes to the literature by studying the effects of a large-scale means-tested cash transfer program – *Programa Bolsa Familia* (PBF) in Brazil – on aggregate formal employment in local labor markets. This is the margin of employment most likely to be affected by disincentive effects from means testing. Formal job creation has also been a primary focus of economic policies in Latin America over the past decades (Levy 2008), as formal jobs are more likely to provide workers social security coverage and better working conditions (e.g., Perry et al. 2007) and are associated with higher output and total factor productivity (e.g., Ulyssea 2020). We use longitudinal administrative data covering the universe of formal workers and PBF beneficiaries, along with the benefits disbursed through the program. With these data, we estimate the impacts of a reform that increased the total number of PBF beneficiaries by more than 17 percent (or almost two million additional families) in 2009. We find that higher PBF payments *increased* local formal employment, by exploiting variation in the size of the expansion across Brazilian municipalities. The evidence is consistent with multiplier effects of cash transfers in the local economy, which dominate potential negative effects on formal labor supply among PBF beneficiaries.

Our main empirical strategy relies on the fact that, because of budgetary constraints, PBF is not an entitlement program. The total number of beneficiaries is set nationally and

[1]See Bastagli et al. (2016), Fiszbein and Schady (2009), and Garcia and Saavedra (2017) for systematic literature reviews and Viana et al. (2018) for studies on the *Bolsa Familia* program in Brazil.

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

specific rules guide the allocation of slots across municipalities. The 2009 reform increased the national cap and changed the methodology used to allocate slots. In our analysis, we use a Difference-in-Differences design that exploits the variation in the intensity of the reform across municipalities caused by this change in methodology. The number of beneficiaries increased sharply in 2009 for municipalities in the top 50 percent of our measure of treatment intensity, and it was more than 10 percent higher in 2010-2011 compared with 2007-2008. By contrast, it was similar between these two periods for municipalities in the bottom 50 percent of our measure of treatment intensity. We estimate a differential increase in total PBF payments of 13.6 percent between the two groups.

Our main finding is that this differential increase in total PBF payments led to an increase in the number of private-sector formal jobs that reached 2 percent by 2011, two years after the reform. The effect on total payroll is slightly smaller (1.7 percent) because the increase in formal employment is concentrated in lower-skill occupations. We find no effect on public employment. The results are robust across various specifications and definitions of treatment intensity, and the formal employment effect is larger when we compare groups of municipalities with a larger differential increase in PBF payments (those in the top 75 percent versus the bottom 25 percent of our measure of treatment intensity).

The results are consistent with larger PBF outlays stimulating local labor demand through a multiplier effect. Indeed, we find a similar increase in formal employment among workers who were never part of the program.[3] We provide three additional pieces of evidence of an overall increase in activity in the local economy. First, we show that our results are not simply driven by a reallocation of formal employment across neighboring municipalities within a greater local economy. The results are similar if we replicate the same analysis at a higher level of aggregation than the municipality (i.e., micro-regions). Second, we estimate effects on municipal gross domestic product (GDP) and taxes paid in the municipality. The results are in line with our formal employment results, with increases in local GDP and local taxes reaching 1.7 and 2.7 percent by 2011, respectively. Third, we document a clear increase in the municipal GDP attributed to non-tradable industries after the reform. Point estimates for the relative increase in formal jobs are also larger for non-tradables versus tradables, although our estimates become noisy when we explore heterogeneity across industries.[4]

In the last section of the paper, we explore whether the overall increase in formal em-

[3]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).

[4]Non-tradable industries are more informal (Dix-Carneiro and Kovak 2019), such that a similar relative change in formal employment could be consistent with a larger overall impact on non-tradable industries.

ployment occurs despite negative effects of PBF on beneficiaries' formal labor supply or are partly driven by positive responses among beneficiaries.[5] We compare formal employment outcomes for families that are eligible for different benefit amounts within a local labor market through a Regression Discontinuity (RD) design. We exploit income per capita cutoffs that determine eligibility for benefits per child conditional on school attendance and health checks ("poverty line") and for an additional unconditional benefit ("extreme poverty line"). We find no evidence that benefits increase formal labor supply. In fact, our results are consistent with recent evidence in the Latin American context that substitution effects created by means testing can induce beneficiaries to reduce their formal labor supply to retain eligibility (Bergolo and Cruces 2021).

Taken together, our results indicate that cash transfer programs can have positive effects on formal labor markets and the local economy at large, even if they create potential disincentive effects on beneficiaries' labor supply. To put the magnitudes in perspective, we estimate the implied cost per formal job, following the literature on the impact of local government spending on local labor markets.[6] We obtain a point estimate of US$5,600 per year, or about 2.1 times the minimum wage at the time. This is much lower than estimates for the United States,[7] but closer to the estimates in Corbi et al. (2019) for the impact of inter-governmental transfers on formal employment in Brazilian municipalities (US$8,000). Direct transfers to poor families with likely high marginal propensity to consume may imply larger multipliers than transfers to local governments. Interestingly, we also find that the cost per formal job is much lower in municipalities with lower informality rates at baseline. This is consistent with the possibility that more jobs are at the margin of formalization in those labor markets. By contrast, the cost per formal job is closer to U.S. estimates in more informal labor markets. This is consistent with the possibility that our analysis misses economic activity generated in the informal sector in those labor markets, and that it takes larger demand shocks to create formal jobs where informality is pervasive.

This paper contributes to the literature on the aggregate effects of social transfers on

[5] A recent literature has argued that there is no micro-evidence that cash transfers discourage recipients from working (e.g., Banerjee et al. 2017). However, many of the programs discussed in this literature only generate income effects because targeting is performed through proxy-means tests that are reassessed infrequently and do not include labor market outcomes. By contrast, means-tested programs can generate both income and substitution effects as workers may change their labor supply, particularly in the formal sector where earnings are more easily verifiable, to obtain or retain eligibility.

[6] This literature often focuses on the "cost per job" rather than the "GDP multiplier," in part because local GDP is measured more noisily (Zidar, 2019; Chodorow-Reich, 2019).

[7] For instance, Chodorow-Reich et al. (2012), Suárez Serrato and Wingender (2016), and Zidar (2019) obtain estimates of US$30,000. Our 95 percent confidence interval is relatively wide, however, going up to US$26,833.

local labor markets. While most of the literature focuses on the effects of social protection policies in developed countries (e.g., Hagedorn et al. 2013, Lalive et al. 2015, Chodorow-Reich and Karabarbounis 2016, Marinescu 2017), a smaller, but growing, literature studies developing countries. Angelucci and De Giorgi (2009) highlight the importance of considering the effect of cash transfers on the overall local economy. They find evidence of indirect effects of the *Progresa* program in Mexico (which is not means-tested) on the consumption of ineligible households living in the same villages. The effect operates entirely through the insurance and credit markets in their setting, however, while we provide evidence of multiplier effects impacting the local labor market. A possible reason for this difference is that their setting consists of poor rural villages, while most PBF families live in urban areas. Bosch and Campos-Vazquez (2014) estimate the aggregate effects of extending health insurance coverage to workers outside the formal sector in Mexico. They find reductions in local formal employment, driven by negative (substitution) effects on formal labor supply among beneficiaries. In-kind transfers, especially in the form of health insurance coverage, may not lead to similar multiplier effects in the local economy as cash transfers.[8] Imbert and Papp (2015) and Muralidharan et al. (2017) estimate the aggregate effects of a public employment program on local labor markets in India. Such programs deliver benefits in the form of (manual) employment, and therefore operate through a different channel, driving up private-sector wages and earnings by improving workers' outside option (Ravallion 1987, Basu et al. 2009). These papers also focus on rural agricultural labor markets, whereas we study urban and rural formal labor markets, spanning different types of sectors and skills. More recently, Egger et al. (2019) study how a one-off nongovernmental transfer affects local recipients and rural economies in Kenya. They find large multiplier effects on consumption and assets but no effects on employment. Our analysis studies a persistent expansion of a large-scale government welfare program that has been running for more than 15 years.

Our results also contribute to the extensive literature on the effect of social transfers on beneficiaries' labor supply in developing countries. Recent studies argue that cash transfers do not discourage work among beneficiaries (see Baird et al. 2018 for a review of the literature). However, much of that discussion focuses on programs that use proxy-means testing to define eligibility based on geographic location and household assets (e.g., Banerjee et al. 2017), such that these programs only generate income effects. We study a context in which disincentive effects are likely stronger: under means testing, labor supply could be affected by substitution effects as well, particularly in the formal sector where earnings

[8] Health insurance cannot be exchanged for cash, and it may improve families' health care rather than imply large changes in their budget constraint, depending on their health care spending at baseline.

are third-party reported to the government. Several other papers in the Latin American context have found formal labor supply responses to cash transfer programs that changed 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 also accounting for aggregate effects to capture the impact of these policies on formal labor markets in full. This point is particularly policy-relevant: as countries develop and income becomes more verifiable across the income distribution (Jensen 2019), the use of means testing for targeting social transfers is bound to expand around the world.

The paper is organized as follows. Section 1 describes the institutional background and data. It also documents the extent to which PBF beneficiaries participate in the formal labor market. Section 2 lays out our main empirical strategy. Section 3 presents the results, and Section 4 provides additional analyses to explore mechanisms, including formal labor supply responses among PBF beneficiaries. Section 5 concludes.

1 Institutional details and data

This section provides important background information for the empirical analysis. We begin by briefly introducing the *Bolsa Familia* program (PBF) and some of its relevant institutional details for our period of analysis, which is from the beginning of 2007 to the end of 2011. We then describe the various data sets that we use. Finally, we use these data to provide some descriptive statistics on PBF beneficiaries and their participation in the formal employment sector.

1.1 *Bolsa Familia* Program (PBF)

PBF is the largest conditional cash transfer program in the world and the main social program for the poor in Brazil. It was created in 2004, bringing together and expanding existing social transfers (Bolsa Escola, Bolsa Alimentação, and Auxílio Gás). As of 2012, it reached 13.9 million families or around a quarter of the Brazilian population, for a cost of about 0.6 percent of GDP. Pioneered by Brazil and Mexico, conditional cash transfer programs have expanded widely over the past 20 years and became the main type of safety net in middle-income countries, covering 30 percent of the poor (Honorati et al. 2015). Nowadays, they exist in almost every country in Latin America, as well as in Bangladesh, Cambodia, Indonesia, Malawi, Morocco, Pakistan, and South Africa, among other countries.

There are two important features of PBF for our study. First, it is means tested. Second, due to budgetary restrictions, it is not an entitlement program.

A. Means testing. Eligible families are classified into "poor" and "extreme poor" categories based on two income per capita thresholds.[9] Extreme poor families are eligible for an unconditional monthly benefit (the "basic benefit") and monthly benefits per child conditional on the child's school attendance and health checks (the "variable benefits").[10] Poor families are only eligible for the conditional variable benefits.[11] Thus, in terms of the eligibility criteria, PBF is more similar to welfare programs in richer countries; most developing countries rely instead on proxy-means testing. For example, in Mexico and Colombia, families are assigned a poverty index score computed from information collected through a national survey conducted every three to four years (see, e.g., Camacho and Conover 2011). One reason for these differences in targeting between richer and poorer countries comes from differences in how easily governments can observe families' income. Indeed, income tends to become more verifiable as countries develop (Jensen 2019). Brazil is thus an interesting policy case as it lies in the middle of the development path: it has a large informal economy, but it also has very developed administrative data systems, including detailed records of formal employment income.

At the core of the targeting of PBF is *Cadastro Unico*, the "census of the poor," which is a federal registry that is continuously updated. Municipalities must register families through interviews, verify the information provided by them, and ensure that it is updated following changes, for example, in family income or family size. This process is based on a questionnaire that asks questions about income, housing, and assets, among other characteristics. Importantly, *Cadastro Unico* is not used exclusively for PBF. Many social programs with different eligibility criteria use this registry. For this reason, *Cadastro Unico* aims to include all families with income per capita below one-half of the minimum wage (R$255 in 2010), which is much higher than the poverty threshold (R$140 in 2010). As a result, it includes families that are not eligible for PBF. To be eligible for any government benefit that uses *Cadastro Unico*, families must have a valid registration (complete and up-to-date); at a minimum, a valid registration must be updated every two years.

The income per capita definition in *Cadastro Unico* is computed based on information reported by families. During the interview, the applicant reports the value for each of

[9] These income thresholds are defined at the national level. During the period of analysis, they were revised once in 2009: the extreme poverty threshold was increased from R$60 to R$70 (∼US$35), and the poverty threshold from R$120 to $140 (∼US$70).

[10] The variable benefits include specific amounts for pregnant women, lactating women, children younger than age 15 years, and adolescents ages 15 to 18.. The number of variable benefits that a family can receive is capped by the total amount and a maximum number of children.

[11] During the period of analysis, the basic benefit increased from R$58 to R$62 in 2008. Variable benefits per child increased from R$18 to R$20 in 2008. Variable benefits per adolescent increased from R$30 to R$33 in 2009.

seven income categories – earnings in the last month, earnings in the last 12 months, donations, pensions, unemployment benefits, alimony, and other income – for each household member. Income per capita is computed in three steps. The first step takes the minimum between the average monthly income in the last 12 months and the last month's income for each individual. The second step adds all other income categories to this value. The third step sums the individual monthly income across all household members and divides it by the number of household members, including all adults and children registered under the same household (names and ID numbers are recorded for each household member).[12]

There are two main strategies to prevent income misreporting. First, the income questions come at the end of the questionnaire so that answers to the previous questions about assets and social demographics help the interviewer gauge the veracity of the total reported income. The law suggests as best practice that registration should be done in the family's home to facilitate verification of answers to the questionnaire.[13] Second, audits can be conducted following citizens' complaints or red flags arising from cross-checking the information in *Cadastro Unico* with data from formal employment records and the Brazilian Social Security System. Government employees may visit families to update their information or require them to update their information at the local administration office. Nonetheless, the large informal sector in the Brazilian economy, and limited capacity to cross-check information systematically and follow up on each case, leave scope for discrepancies between the information in *Cadastro Unico* and families' true income per capita. We return to this discussion in Section 4.2. For more details on eligibility and enforcement, see Lindert et al. (2007).

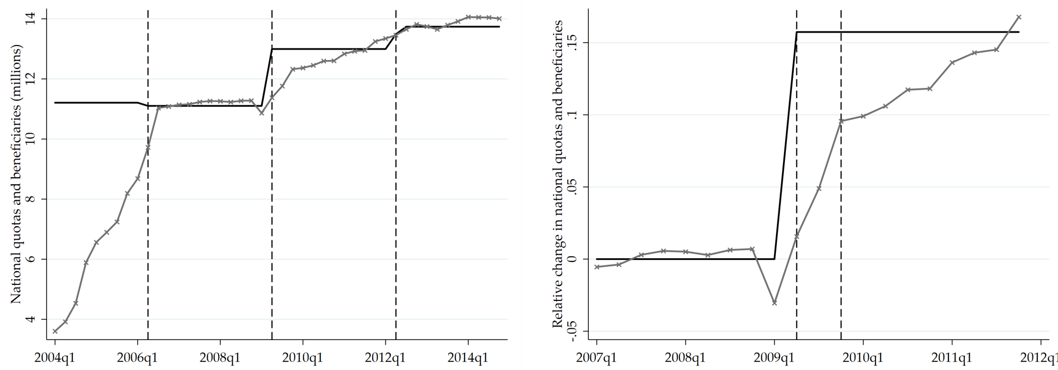
B. Benefits are not an entitlement. Eligible families are not entitled to become PBF beneficiaries. The federal budget finances the program and sets the total number of slots for PBF beneficiaries at the national level accordingly. Figure 1a displays the total number of slots and the total number of beneficiaries over time. The total number of slots was set in 2003 (before the start of the program) at 11.2 million. It was then revised in 2006 (11.1 million), 2009 (13 million), and 2012 (13.7 million). In the first few years, as beneficiaries of other welfare programs were transferred to PBF, and municipalities were registering new families in *Cadastro Unico*, the number of beneficiaries gradually increased. It only reached the national cap in mid-2006, after the first reform. The national cap was then binding until the 2009 reform when the total number of slots increased by about 15 percent. The expansion was rolled out progressively between the second and fourth quarters of 2009, as shown in Figure 1b. The total number of beneficiaries decreased in the first quarter of the

[12] This process refers to the procedure used during our period of analysis.

[13] See link to *Portaria* 177, article 5, which states that registration should be performed in the family's home as a default option.

FIGURE 1: TOTAL NUMBER OF PBF SLOTS AND BENEFICIARIES OVER TIME

(a) From 2004 to 2014 (levels) (b) Around the 2009 reform (relative changes)



Note: This figure presents the evolution of the total number of PBF slots and beneficiaries at the national level, in levels since the creation of PBF in 2004 (panel a) and in relative changes around the 2009 reform (panel b). Vertical lines in panel (a) indicate the timing of reforms of the program, which updated the total number of slots and municipal quotas (in 2006, 2009, and 2012). Vertical lines in panel (b) indicate the start and the end of the rollout of the 2009 reform studied in the paper.

year, due to a "cleanup" of *Cadastro Unico* through cross-checks with other administrative records. It then increased by more than 10 percent as the expansion was rolled out, and continued to increase until it reached the national cap by the end of 2011.[14] In this paper, we focus on the impacts of the 2009 PBF expansion highlighted in Figure 1b.

The total number of slots is divided across municipalities based on the "municipal quotas" that were set in 2003, and revised in 2006, 2009, and 2012. These are not strictly speaking quotas, but they determine the allocation of slots across municipalities. In particular, 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 PBF beneficiaries divided by the municipal quota (lowest values first) — and on local demand defined by the number of eligible families in the municipality that are registered in *Cadastro Unico* and not yet receiving PBF benefits. Therefore, eligible families can only become PBF beneficiaries if there are available slots assigned to their municipality.

Section 2 provides more details on the rules used to allocate the municipal quotas as they generate the variation at the core of the empirical analysis. Specifically, the 2009 PBF expansion was also associated with a change of methodology, which led to large differences in the additional numbers of PBF beneficiaries across municipalities.

[14]After that, the federal government let the number of beneficiaries exceed the cap. The increase in the total number of slots in 2012 only caught up with the actual number of beneficiaries at the time, which in 2014 was temporarily allowed to exceed the cap by 0.3 million, but has since converged to the 2012 cap.

1.2 Data

The analysis in this paper draws on three main administrative data sets.

A. Cadastro Unico. First, we use snapshots of *Cadastro Unico*, the census of the poor described in the previous subsection, in December 2008 and August 2010. They include family-level information on per capita income, family composition, and geographical location. Each individual in the family is identified with an ID number that can be matched to the other two administrative records below.

B. Bolsa Familia payment sheets. Second, we use administrative data from the former Brazilian Ministry of Social Development (MDS) on the universe of PBF benefits paid. The data identify the amount received by each family in each month by type of benefit (basic benefit and variable benefits). We use monthly data from January 2007 to December 2011, which allows us to calculate, for example, the number of PBF beneficiaries and the total PBF payments per municipality for each month in our period of analysis.

C. Relação Anual de Informações Sociais (RAIS). Third, we use the Brazilian matched employee-employer data set from the former Brazilian Ministry of Labor (MTE), which covers the universe of formal employment spells in each year. For each worker, the data include information on occupation, wage, race, gender, industry, municipality, as well as hiring and termination dates. *RAIS* allows us to calculate municipal and individual formal labor market outcomes for our period of analysis.[15] For instance, we can compute the number of formal private-sector jobs, overall and by worker category, in each municipality in a given month. We can also match individuals in *RAIS* and *Cadastro Unico* to compute how many adults in each family are formally employed in a given month.

We use other sources of data to complement these administrative data. The total number of PBF slots and the municipal quotas were made publicly available by the former Ministry of Social Development. We use microdata from the 2000 Brazilian census to compute poverty rates and informality rates for each municipality. Finally, we use municipal data on estimated population growth, GDP, and taxes paid from the Brazilian Census Bureau (*IBGE*).

1.3 Descriptive statistics on PBF beneficiaries

Table 1 displays descriptive statistics for three groups of families using the snapshot of *Cadastro Unico* from August 2010: all families, families below the extreme poverty line,

[15] 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. Employing firms face fines for failure to report. RAIS omits interns, domestic workers, and other minor employment categories, along with those without signed work cards, including the self-employed.

and families between the poverty line and the extreme poverty line. In 2010, the poverty line was R$140 per capita per month (∼US$70), and the extreme poverty line was R$70 (∼US$35).

Table 1 first highlights that *Cadastro Unico* includes many families that are not PBF beneficiaries. Around 61 percent of all families in *Cadastro Unico* in August 2010 were PBF beneficiaries. PBF is not an entitlement, such that families can be eligible without becoming beneficiaries. For instance, 21 percent of families below the extreme poverty line in *Cadastro Unico* in August 2010 were not PBF beneficiaries. Comparing the demographic characteristics of PBF families in Table 1 (discussed below) with the same characteristics for all families in *Cadastro Unico* in these income per capita brackets (see Online Appendix Table A.1) shows that PBF families have lower socioeconomic status on average than families that were eligible but were not beneficiaries.

The top panel in Table 1 also shows that PBF families mostly live in urban areas, have low high-school completion rates, and have low income levels. In particular, their average monthly income per capita in *Cadastro Unico* in August 2010 was R$53.9, and it was only R$36.2 among the extreme poor. The middle panel in Table 1 shows that the average monthly benefits received by PBF families (R$101.93; calculated over the following 12-month period) were substantive compared with their average monthly income (R$206.3). Extreme poor families have lower income levels and are eligible for both the unconditional basic benefit and the conditional variable benefits, such that they received higher benefits than poor families in absolute terms (R$112.45 versus R$68.66) and relative to their income levels (83 percent versus 17 percent).

The bottom panel in Table 1 sheds some light on PBF families' participation in the formal labor market, by matching all adult household members in *Cadastro Unico* in August 2010 to their formal employment spells in *RAIS* over the following 12-month period. This exercise reveals that although lower-income families are in general more likely to work informally in middle-income countries (Perry et al. 2007), a relevant share of PBF families interact with the formal labor market. Yet, their attachment to the formal labor force remains limited. For instance, 24 percent of extreme poor families and 31 percent of poor families had at least one adult formally employed for at least one month in the 12-month window. On average, extreme poor families had a total of 2.45 adult-months in formal employment over the period; this figure is twice as large among poor families, highlighting the positive correlation between formal employment and income. Finally, conditional on working formally, adults in PBF families earned more than the minimum wage on average (R$510 per month in 2010). However, because the income definition for eligibility is the minimum between the current monthly income and the average income over a 12-

month period, working in a formal job does not necessarily disqualify a family from PBF benefits as long as their per capita income remains below the eligibility threshold.

TABLE 1: PBF BENEFICIARIES AND FORMAL EMPLOYMENT

	All families	Families below the extreme poverty line	Families below the poverty line and above the extreme
Cadastro			
Number of families	20,564,520	12,254,032	4,980,800
Number of individuals	72,765,336	44,860,344	18,759,020
Number of individuals in PBF	52,237,520	38,921,380	12,104,991
Share of families in PBF	0.61	0.79	0.54
Share of adults who completed high-school among PBF families	0.12	0.11	0.15
Avg. share urban among PBF families	0.70	0.67	0.82
Average family size among PBF families	3.9	3.8	4.1
Average per capita income among PBF families	53.9	36.2	101.4
Average total income among PBF families	206.3	135.6	411.6
Payment sheets: Sep. 2010 - Aug. 2011			
Average montly benefits	101.93	112.45	68.66
RAIS: Sep. 2010 - Aug. 2011			
Share of families with at least 1 adult in RAIS among PBF families	0.23	0.24	0.31
Number of adult-months in RAIS among PBF families	3.11	2.45	5.14
Average wage among PBF adults in RAIS (conditional on working)	728.30	715.22	745.11

Note: The table presents summary statistics for families registered in *Cadastro Unico* in August 2010. We use the information on monthly per capita income (excluding PBF benefits) to classify families as extreme poor (below R$70) or poor (between R$70 and R$140). The top panel only uses information from the snapshot of *Cadastro Unico*, the middle panel matches these families to PBF payment sheets in the following 12-month period, and the bottom panel matches these families to the formal employment data (*RAIS*) over the same period. Here we report statistics for PBF beneficiaries (based on the PBF payment sheets in August 2010); Online Appendix Table A.1 reports the same statistics for all families in each income group. Monetary values are reported in nominal terms.

2 Empirical strategy

To estimate the impact of PBF on local formal labor markets, we exploit the timing of the 2009 PBF expansion and variation in the number of new PBF beneficiaries across municipalities due to changes in the methodology used to allocate municipal quotas. We lay out our empirical strategy in this section. First, we provide details on the allocation of municipal quotas. Second, we present our difference-in-differences (DD) research design.

2.1 Allocation of PBF quotas across municipalities

Each municipality is assigned a quota of the national slots based on poverty estimates performed by IBGE. These municipal quotas are not strictly speaking quotas, but they determine the allocation of any PBF slot available at the national level (when new slots are created or an existing slot is made available after a family leaves the program) across municipalities.[16]

A. Initial methodology. When the government set the initial number of national slots in 2003, and when it first revised this number in 2006, it followed a similar strategy to assign the municipal quotas. First, IBGE calculated the number of poor families in each state based on the poverty threshold[17] using microdata from the National Household Sample Survey (PNAD), a yearly household survey that is representative at the level of the 27 Brazilian states, but not at more disaggregated geographical levels. Specifically, IBGE used PNAD 2001 and PNAD 2004 for the 2003 and 2006 quotas, respectively. Second, it assigned quotas across municipalities within a state using microdata from the 2000 Brazilian census, the only source of data representative at the municipal level at the time. IBGE calculated the number of households in each municipality with total income below twice the minimum wage. Compared with 2003, it simply updated these figures in 2006 using an estimate of the population growth in each municipality between 2000 and 2003 (MDS 2012).

Formally, the 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 $Poor_{ms}^{2000}$ is the number of poor families in each municipality m in state s based on the 2000 census; $Poor_{s}^{2001}$ and $Poor_{s}^{2004}$ are the number of poor families in each state based on PNAD 2001 and PNAD 2004, respectively; and $n_{ms}^{[2000,2003]}$ is an estimate of the population growth in each municipality between 2000 and 2003. By construction, the number of poor families in each state equals the sum of the quotas across all the municipalities in the state.

B. Change of methodology in 2009. At the time of the 2009 PBF expansion, the IBGE changed its methodology to assign municipal quotas.

The first step remained essentially unchanged. IBGE used PNAD 2006 to calculate the

[16]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 for existing PBF beneficiaries (to keep their *variable benefits*). The quota allocation does not consider information related to the implementation of the program, so this type of manipulation is not a concern for our analysis.

[17]The poverty threshold that was used to compute national slots and state quotas corresponded to half of the minimum wage per capita.

number of poor families in each state. However, as shown in Figure 1, the national number of slots was increased compared with previous years, and it actually became greater than the total number of poor families across all states. Concretely, this higher number of slots was obtained by scaling up the number of poor families by a factor of 1.18 to account for "transitory poverty" (see Soares 2009 and MDS 2009).[18]

The second step was fundamentally different. Instead of relying on the number of poor families in each municipality based on the 2000 census, IBGE adopted a "poverty map" methodology developed by World Bank researchers (Elbers et al. 2003). The methodology essentially worked as follows (MDS 2012). IBGE developed a statistical model to predict the number of poor families in each municipality in the 2000 census based on prior data on municipal-level variables, as well as household-level variables from PNAD, although PNAD is not representative at the municipal level. The idea was to capture an overall welfare measure, including information on education, health, and other indicators, to compute poverty shares. It then used the model and data from 2006 (municipal-level variables and PNAD) to predict the number of poor families in each municipality in 2006, $\widehat{Poor}_{ms}^{2006}$ (IBGE 2019). The municipal quotas were then calculated as before:

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

IBGE returned to using census data to construct the municipal quotas in 2012 (based on the 2010 census). Thus, this poverty map methodology was only used in 2009.

2.2 Research design

In our empirical analysis, we exploit the timing of the 2009 PBF expansion and differences in the relative size of the expansion across municipalities induced by the change of methodology described above through a difference-in-differences strategy.

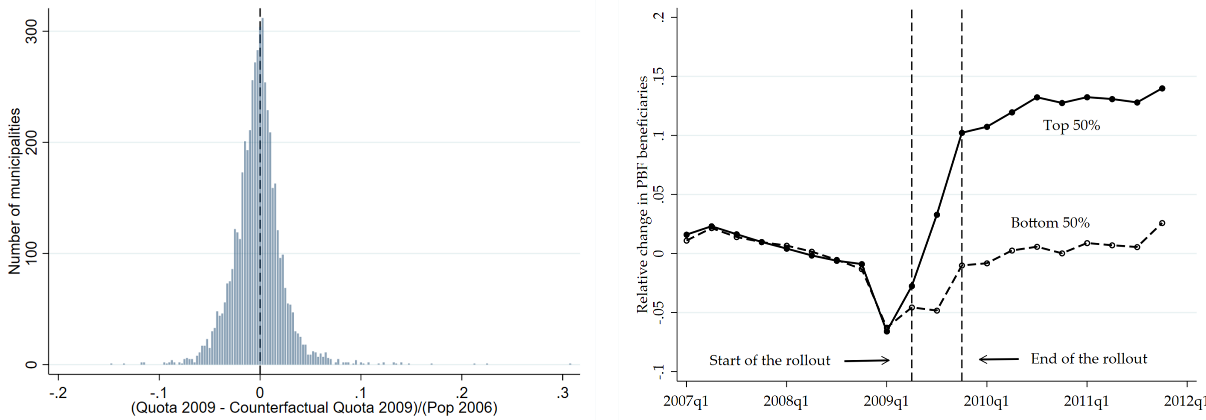
A. Cross-sectional variation in municipal quotas. To capture the variation across municipalities induced by this change of methodology, for our research design, we calculate the difference between the actual 2009 quotas and counterfactual quotas computed as if the previous methodology had been maintained. Specifically, following the approach used to update the municipal quotas in 2006, we define the counterfactual quotas as fol-

[18] The concept of "transitory poverty" is that more families may regularly fall below the poverty line than the number of families observed below the poverty line at any single time (Soares 2009).

FIGURE 2: VARIATION IN THE SIZE OF THE 2009 EXPANSION ACROSS MUNICIPALITIES

(a) Distribution of the relative difference between actual and counterfactual 2009 quotas ($\Delta Quota_{ms}^{2009}$)

(b) Relative change in PBF beneficiaries by reform size (above/below the median of $\Delta Quota_{ms}^{2009}$)



Note: The figure presents the variation in the size of the 2009 PBF reform across municipalities induced by the change in the methodology used to update municipal quotas. Panel (a) displays the distribution of $\Delta Quota_{ms}^{2009}$ across municipalities, that is, the difference between their actual 2009 quota and their counterfactual 2009 quota (computed as if the previous methodology had been maintained) relative to their population in 2006. Panel (b) displays the average of the relative change in the number of PBF beneficiaries for municipalities in the top 50 percent and bottom 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$.

lows:

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

where $n_{ms}^{[2000,2006]}$ is the estimated population growth between 2000 and 2006 from IBGE. The state-level constant, $1.18 \cdot Poor_{s}^{2006}$, is simply the sum of the 2009 quotas across the municipalities in each state.[19] We then rank the municipalities based on the difference between the actual and counterfactual quotas relative to their population in 2006:

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

This variable captures the *relative* change in quota in 2009 coming from the change in methodology, that is, abstracting from any change coming from population growth.

Figure 2a displays the distribution of $\Delta Quota_{ms}^{2009}$ across municipalities, which is more or less centered around zero. We note that no beneficiary was forced to leave the program as a result of the update of municipal quotas in 2009, even in municipalities for which

[19] Although the methodology used to assign municipal quotas in 2009 is described in several documents (see references in the text), we did not obtain the statistical model used by IBGE. Therefore, we cannot recover $\widehat{Poor}_{ms}^{2006}$ or exploit idiosyncrasies of that model to isolate exogenous variation in $\widehat{Poor}_{ms}^{2006}$.

quotas were reduced in 2009 (these municipalities became less likely to be allocated *new* PBF slots). Figure 2b shows that this variation led to systematic differences in the evolution of the number of PBF beneficiaries across municipalities. It displays the average changes in the number of PBF beneficiaries in each municipality between 2007 and 2011 relative to the average in the pre-reform period for two groups of municipalities: those in the top and bottom 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$. The two groups shared the same trend before the reform, including the 5 percent drop in the number of beneficiaries in the first quarter of 2009 highlighted in Figure 1b. The number of PBF beneficiaries increased rapidly in the following three quarters of 2009 for municipalities in the top 50 percent group, by more than 15 percent. In contrast, the number of PBF beneficiaries only returned to pre-2009 levels in the bottom 50 percent group. The 2009 reform thus led to large increases in the number of PBF beneficiaries, and thus in the total amount of PBF payments, in some municipalities, but not in others.

We present two maps of Brazil to compare the geographical variation in the size of the program across municipalities prior to the reform and in the relative change in quota induced by the change in methodology in 2009. Figure 3a displays the number of PBF beneficiaries as a share of the population in each municipality in 2008. There is a clear geographical pattern in line with PBF targeting goals: poorer areas in the North and the Northeast have relatively more PBF beneficiaries. By contrast, Figure 3b shows that there is no such clear geographical pattern for the distribution of $\Delta Quota_{ms}^{2009}$: in many areas, contiguous municipalities fall in nonadjacent quartiles of the distribution of $\Delta Quota_{ms}^{2009}$.

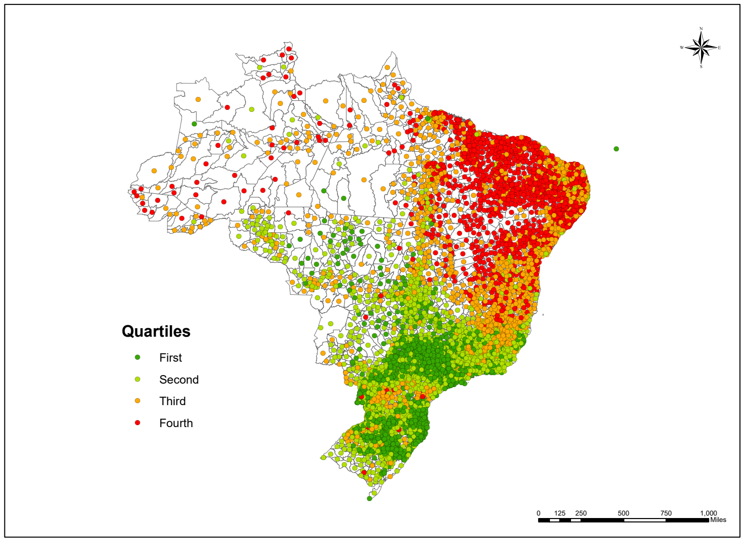
B. Difference-in-differences strategy. In our analysis, we restrict attention to the five-year period from January 2007 (after the 2006 quota reform; the most recent data used to compute the 2009 quotas were also from 2006) to December 2011 (before the 2012 quota reform). We estimate variants of the following difference-in-differences specification:

$$\begin{aligned} y_{m,s,t} =& \alpha_m + \theta_p + \phi_{p,s} + \sum_{p \neq 2009q1} \beta_p \cdot \mathbb{1}\{t \in p\} \cdot Treat_{m,s} \\ &+ \sum_{k} \sum_{p \neq 2009q1} \gamma_{p,k} \cdot \mathbb{1}\{t \in p\} \cdot X_{m,s}^{k} + \varepsilon_{m,s,t}, \end{aligned} \tag{6}$$

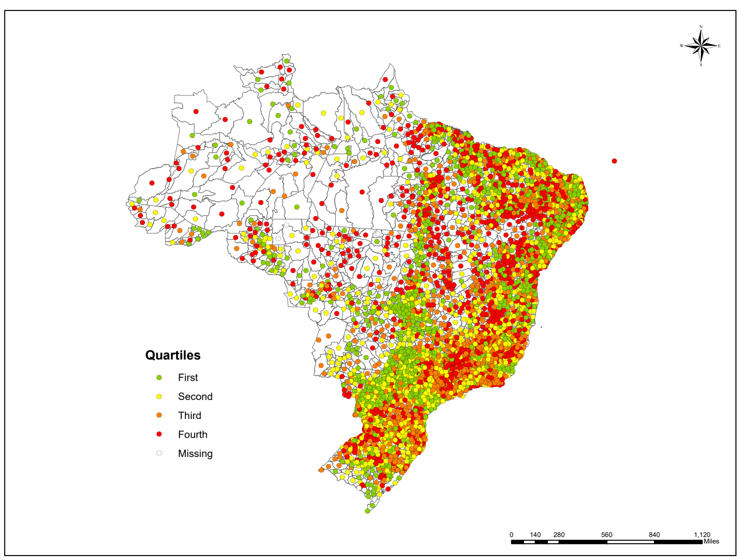
where $y_{m,s,t}$ is an outcome of interest in month *t* for municipality *m* in state *s*. Our main outcomes of interest are the logarithm of the number of PBF beneficiaries, of the total PBF payments, and of the number of private-sector formal employees. Municipality fixed effects α_m control for time-invariant characteristics of municipalities. We bin time in six periods, $p = \{2007, 2008, 2009_{q1}, 2009_{q2-q4}, 2010, 2011\}$, to trace the evolution of the outcome before the 2009 reform, during its roll-out in 2009, and in the following years. Period

FIGURE 3: GEOGRAPHICAL VARIATION IN THE SIZE OF PBF ACROSS MUNICIPALITIES PRE-REFORM AND IN THE SIZE OF THE 2009 EXPANSION

(a) Number of PBF beneficiaries as a share of the population in 2008



(b) Relative difference between actual and counterfactual 2009 quotas ($\Delta Quota_{ms}^{2009}$)



Note: The figure compares the geographical variation in the size of the program across municipalities prior to the reform and in the relative change in quota induced by the change in methodology in 2009. The map in panel (a) ranks municipalities by quartiles of the number of PBF beneficiaries as a share of their population in 2008. The map in panel (b) ranks municipalities by quartiles of $\Delta Quota_{ms}^{2009}$, that is, the difference between their actual 2009 quota and their counterfactual 2009 quota (computed as if the previous methodology had been maintained) relative to their population in 2006. Lines in the maps indicate the borders of each municipality; dots indicate the quartile in which the municipality belongs.

fixed effects θ_p control for common shocks across municipalities. State-by-period fixed effects $\phi_{p,s}$ absorb any variation over time that is common across municipalities within a state, such as the update of the total quota across all municipalities of the state from $PoorState_s^{2004}$ to $1.18 \cdot PoorState_s^{2006}$. Our difference-in-differences coefficients β_p capture any difference for municipalities with high versus low values of $Treat_{m,s}$ in a given time period p compared with the first quarter of 2009 (just before the start of the rollout of the reform).

In our preferred specification, $Treat_{m,s}$ corresponds to a dummy indicating whether a municipality belongs to the top 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$. We also consider alternative definitions of $Treat_{m,s}$: comparing municipalities in the top 75 percent versus in the bottom 25 percent of the distribution of $\Delta Quota_{ms}^{2009}$, using $\Delta Quota_{ms}^{2009}$ linearly, or using the relative change in quota compared with the 2006 quota ($Quota_{ms}^{2006}$) rather than compared with the counterfactual 2009 quota ($CountQuota_{ms}^{2009}$) to rank municipalities.

The specification in equation (6) allows municipalities with different values of some predetermined variables $X_{m,s}^k$ to have different trends. In our preferred specification, for instance, these variables include the 2006 quota ($Quota_{ms}^{2006}$), to capture differential trends related to the baseline size of the program, and the counterfactual change in the quota in 2009 ($\Delta CountQuota_{m,s}^{2009} = CountQuota_{m,s}^{2009} - Quota_{ms}^{2006}$), to capture differential trends related to population growth.[20] We include these controls because of baseline differences between municipalities in the top and bottom 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$ documented below (see Table 2). However, we show that our results are robust to excluding these controls in the next section. Finally, we cluster the error term $\varepsilon_{m,s,t}$ by micro-regions, which are groups of contiguous municipalities sharing local similarities as defined by IBGE.

C. Descriptive statistics by groups of municipalities. Before turning to the main results, Table 2 provides summary statistics of baseline characteristics for municipalities in the analysis sample (4,981 out of 5,570 municipalities). 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 (São Paulo city) in 2006. Specifically, we exclude very small municipalities (below the 1st percentile of the 2006 population distribution) and very large municipalities (above the 99th percentile), and we restrict attention to municipalities that have at least

[20] Given that we control for state-by-period fixed effects, the only variation left in $\Delta CountQuota_{m,s}^{2009}$ comes from differences across municipalities in the estimated population growth between 2003 and 2006.

five private-sector formal employees every month from 2007 to 2011 (above the 1st percentile of the distribution of that variable). As we show in the next section, the 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 employment in some month).

Prior to the 2009 reform, municipalities in the top 50 percent of the distribution of $\Delta Quota^{2009}_{ms}$ (our *treatment* group) were on average slightly larger than municipalities in the bottom 50 percent (our *control* group) in terms of population, quotas, number of beneficiaries, and private-sector formal employment. By contrast, rates of labor market informality, as measured by the share of informal employees and self-employed workers in the 2000 census, were quite similar across groups. Table 2 also shows that population grew a lot faster from 2000 to 2006 in control municipalities than in treated municipalities. Thus, if the methodology had not changed in 2009, and the same approach used to update quotas in 2006 had been implemented in 2009 (i.e., generating the *counterfactual* quotas), the quotas would have actually increased relatively more in control municipalities (19.8 percent) than in treated municipalities (14.1 percent), bringing the total number of quotas closer between the two groups in 2009. However, because of the change of methodology, the quotas increased by 30.2 percent on average in treated municipalities, but by only 0.7 percent in control municipalities. By taking the difference between the actual and counterfactual quotas in 2009 to rank municipalities, we use variation in the size of the reform across municipalities net of differences in population growth.[21]

3 Main results

We now present our main difference-in-differences results for the impact of PBF on local formal labor markets, as well as robustness checks. We explore the mechanisms driving these results in the next section.

3.1 Impacts on local formal labor markets

We begin by providing some support for the main identification assumption underlying our empirical strategy. All the information used for the update of the municipal quotas in

[21]The average 2009 quota and the average counterfactual quota in 2009, considering all municipalities in the first column in Table 2, are slightly different because we exclude some municipalities from the analysis sample as discussed in the previous paragraph. Without these sample restrictions, the two averages would be the same as the two variables allocate the same total number at the national level across municipalities.

TABLE 2: SUMMARY STATISTICS FOR MUNICIPALITIES BY SIZE OF $\Delta Quota_{ms}^{2009}$

	[1]	[2]	[3]
	All municipalities	Municipalities above the p50 of Δ Quota	Municipalities below the p50 of Δ Quota
Population 2006	24,148	28,057	20,237
Quota 2004	1,677	1,736	1,617
Quota 2006	1,652	1,702	1,602
Population growth 2000 - 2006	1.09	1.06	1.13
Counterfactual Quota 2009	1,930	1,942	1,919
Quota 2009	1,914	2,215	1,613
Δ Quota 2009	-0.002	0.015	-0.019
Number of Beneficiaries 2007	1,726	1,816	1,635
Total PBF montly payments 2007	117,903	124,409	111,394
Number of formal private-sector employees 2007	2,532	3,200	1,863
Average private-sector formal wage 2007	762	774	751
Informality rate 2000	0.54	0.54	0.55
Number of Municipalities	4,981	2,491	2,490

Note: The table displays the average of baseline characteristics for municipalities in our analysis sample (4,981 out of 5,570 municipalities). Column [1] includes all municipalities in the main sample. Columns [2] and [3] restrict attention to municipalities in the top 50 percent and bottom 50 percent, respectively, of the distribution of $\Delta Quota_{ms}^{2009}$. Informality rates are calculated using the 2000 census.

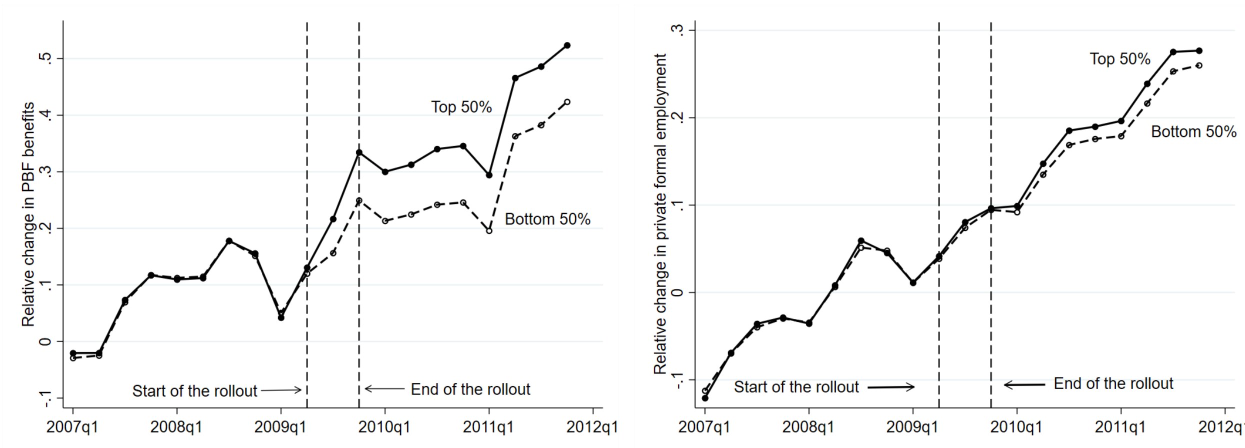
2009 was from before 2007. Therefore, we can use the period preceding the rollout of the 2009 reform within our window of analysis (2007-2009q1) to evaluate the plausibility of a parallel-trend assumption between municipalities in the top and bottom 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$. Figure 2b already showed that municipalities in the top and bottom 50 percent groups shared a common trend prior to the 2009 reform in terms of the number of PBF beneficiaries. Figures 4a and 4b show that this is also the case in terms of the total PBF payments and the number of formal private-sector employees, respectively.

Figures 2b, 4a and 4b also preview our results. Figure 2b showed that the rollout of the 2009 reform led to a larger relative increase in the number of PBF beneficiaries for municipalities in the top 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$. Figure 4a shows that this was also associated with a larger relative increase in total PBF payments. Formal employment continued to evolve similarly between the two groups of municipalities throughout 2009, as shown in Figure 4b. However, after the end of the rollout, private-sector formal employment started to increase faster in municipalities in the top 50 percent group. As a result, municipalities that experienced a larger increase in PBF beneficiaries and total PBF payments had also gained relatively more formal private-sector jobs by 2011.

Figure 5 presents our main results. It displays the difference-in-differences coefficients $\widehat{\beta_p}$ from estimating our preferred specification for the logarithm of the total PBF payments (panel a) and of the number of formal private-sector employees (panel b). Figure 5a shows that municipalities in the top 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$ experienced a relative increase in total PBF payments that reached about 14 percent by 2011 and is highly

Figure 4: Raw patterns in the data by size of $\Delta Quota_{ms}^{2009}$

(a) Total PBF payments (b) Private-sector formal employees



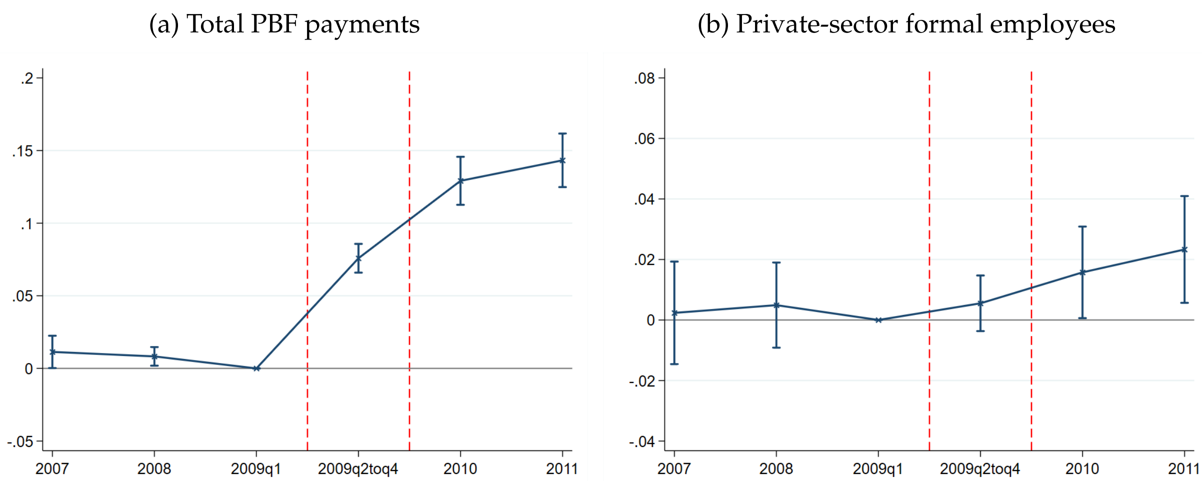
Note: The figure displays the average of the relative change in total PBF payments (panel a) and in the number of private-sector formal employees (panel b) for municipalities in the top 50 percent and bottom 50 percent of the distribution of $\Delta Quota_{ms}^{2009}$. The vertical lines indicate the start and end of the rollout of the 2009 reform.

significant. The estimated effect increases gradually during the rollout of the reform and is relatively stable between 2010 and 2011. Figure 5b shows that these municipalities also experienced a relative increase in private-sector formal employment after the reform, reaching about 2 percent by 2011. The estimated effect is statistically significant at conventional levels, although the confidence intervals are unsurprisingly wider in this case. The effect appears with a slight lag compared with the increase in PBF payments, as seen in the raw data in Figure 4. This delayed response is consistent with multiplier effects from the increase in resources spent in the local economy expanding labor demand (e.g., firms may not adjust their workforce until an increase in demand appears persistent) and with increases in formal labor supply among new beneficiaries (e.g., investments in job search may take time to yield returns). We explore both mechanisms in the next section.

We note that the coefficient estimates are close to zero prior to the reform in both panels of Figure 5, supporting again our main identification assumption.

We summarize our results in Table 3 by reporting the $\widehat{\beta_p}$ coefficients post-reform from a sparser specification that combines 2010 and 2011 in one post-reform period. The estimates from our preferred specification in column [1] are in line with the patterns in Figure 5: an increase in total PBF payments of 13.6 percent and an increase in private-sector formal employment of 2 percent. In the next rows in Table 3, we consider additional outcome variables. We find that the total payroll for formal private-sector employees increased by 1.7 percent after the reform. This slightly smaller increase in payroll than in employment comes from the fact that the formal employment effect is concentrated among lower-paid

FIGURE 5: IMPACT OF THE 2009 REFORM – MAIN RESULTS



Note: The figure reports the $\widehat{\beta_p}$ coefficients (with their 95% confidence intervals) from estimating our preferred specification in equation (6), which defines $Treat_{m,s}$ as a dummy variable indicating whether a municipality belongs to the top 50 percent of the distribution of $\Delta Quota^{2009}_{ms}$. The outcomes in panels (a) and (b) are the logarithm of total PBF payments and the logarithm of the number of private-sector formal employees, respectively. The vertical lines indicate the start and end of the rollout of the 2009 reform.

occupations, as discussed in the next section. We also find that the formal employment effect is almost identical if we focus on full-time workers only (1.9 percent). Thus, the results are not associated with a change in the relative share of full-time versus part-time jobs. Finally, we find that the reform had no statistically significant effect on public employment.[22]

3.2 Robustness checks

The remaining columns in Table 3 present a series of robustness checks. Columns [2], [3], and [4] show that our results are robust to considering alternative treatment variables. Column [2] uses a dummy indicating whether a municipality belongs to the top 25 percent of the distribution of $\Delta Quota^{2009}_{ms}$ (keeping only municipalities in the top and bottom 25 percent). The effects on both total PBF payments and private-sector formal employment are larger in this case, which is consistent with the fact that we are exploiting a larger difference in reform size across municipalities.[23] By contrast, the effect on public employment is essentially nil. We thus find no evidence of an impact of the pro-

[22]The outcomes are in logarithms for all the variables in Table 3, except for public employment. In that case, we use an inverse hyperbolic sine transformation to keep the same balanced panel across all our regressions (public employment is nil for less than 0.85 percent of observations). The results are unchanged if we use the logarithm of public employment instead (results available upon request).

[23]Online Appendix Figure B.1 shows that these two groups also shared a common trend in total PBF payments and private-sector formal employment prior to the 2009 reform.

TABLE 3: IMPACT OF THE 2009 REFORM – SUMMARY OF MAIN RESULTS AND ROBUSTNESS CHECKS

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Above p50 vs below p50	Above p75 vs below p25	Above vs. below p50 of actual quota change	Linear Δ Quota 2009	Including large municipalities	Population weights	No baseline controls	Controling for pre-trends
Total PBF payments	0.136***	0.223***	0.155***	4.663***	0.135***	0.130***	0.109***	0.135***
	(0.009)	(0.012)	(0.008)	(0.255)	(0.009)	(0.010)	(0.008)	(0.008)
Private-sector formal employment	0.020***	0.031**	0.025***	0.766***	0.019***	0.018***	0.017**	0.019***
	(0.007)	(0.013)	(0.008)	(0.212)	(0.007)	(0.006)	(0.007)	(0.007)
Private-sector formal payroll	0.017*	0.029*	0.027***	0.731***	0.016*	0.015*	0.015*	0.013
	(0.009)	(0.016)	(0.010)	(0.258)	(0.009)	(0.008)	(0.009)	(0.009)
Full-time formal private sector employment	0.019**	0.031**	0.027***	0.811***	0.019**	0.018***	0.016**	0.018**
	(0.008)	(0.013)	(0.009)	(0.212)	(0.007)	(0.006)	(0.007)	(0.007)
Public Employment	0.010	0.002	0.007	0.060	0.009	0.015	0.009	0.014
	(0.015)	(0.028)	(0.013)	(0.471)	(0.014)	(0.014)	(0.016)	(0.010)
Number of obs	298,860	149,460	298,860	298,860	302,040	298,860	298,860	298,860
Municipal FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
State-by-period FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period interactions with Log Quota 2006	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*no*	*yes*
Period interactions with the counterfactual Δ Quota 2009	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*no*	*yes*
Period interacted with pre-trends	*no*	*no*	*no*	*no*	*no*	*no*	*no*	*yes*

Note: The table summarizes our main results and presents a series of robustness checks. It reports the $\widehat{\beta_p}$ coefficients post-reform (with standard errors in parentheses) from estimating a sparser version of the specification in equation (6), which combines 2010 and 2011 in one post-reform period. Column [1] summarizes our main results, using a similar specification and definition of the variable $Treat_{m,s}$ as in Figure 5. Column [2] compares groups of municipalities with a larger difference in reform size, i.e., those in the top 75 percent versus bottom 25 percent of $\Delta Quota^{2009}_{ms}$. Column [3] categorizes municipalities based on their relative change in quota in 2009 compared to the 2006 quota ($Quota^{2006}_{ms}$) rather than compared to the counterfactual 2009 quota ($CountQuota^{2009}_{ms}$). Column [4] uses $\Delta Quota^{2009}_{ms}$ linearly. Columns [5] and [6] use the same specification as in column [1], but include municipalities above the 99th percentile of the 2006 population distribution in the sample and re-weight municipalities by their 2006 population, respectively. Column [7] excludes the control variables interacted with time periods from the main specification in column [1]. Column [8] adds the pre-trend in the outcome as control variable interacted with time periods to the specification in column [1]. Total PBF payments, private-sector formal employment, private-sector formal payroll and full-time private-sector formal employment are in logs. For public employment, we use an inverse hyperbolic sine transformation to keep the same balanced panel across all regressions in each column (public employment is nil for less than 0.85 percent of observations).

gram expansion on the size of the government workforce. Column [3] uses a treatment dummy categorizing municipalities based on their relative change in quota in 2009 compared with the 2006 quota ($Quota_{ms}^{2006}$) rather than compared with the counterfactual 2009 quota ($CountQuota_{ms}^{2009}$). The effects are slightly higher in this case because the treatment group includes municipalities that experienced a relatively higher population growth. Column [4] uses $\Delta Quota_{ms}^{2009}$ linearly, and the coefficients are consistent with our preferred specification.

Columns [5] and [6] 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 very similar to our preferred specification.

Finally, columns [7] and [8] show the results for alternative specifications. In column [7], we exclude the set of baseline controls X^k interacted with time periods. In column [8], we add pre-trends of the outcomes ($log(Y_{m,s,2009q1}) - log(Y_{m,s,2007q1})$) to the set of controls X^k. In both cases, the results are again consistent with our preferred specification.

4 Mechanisms

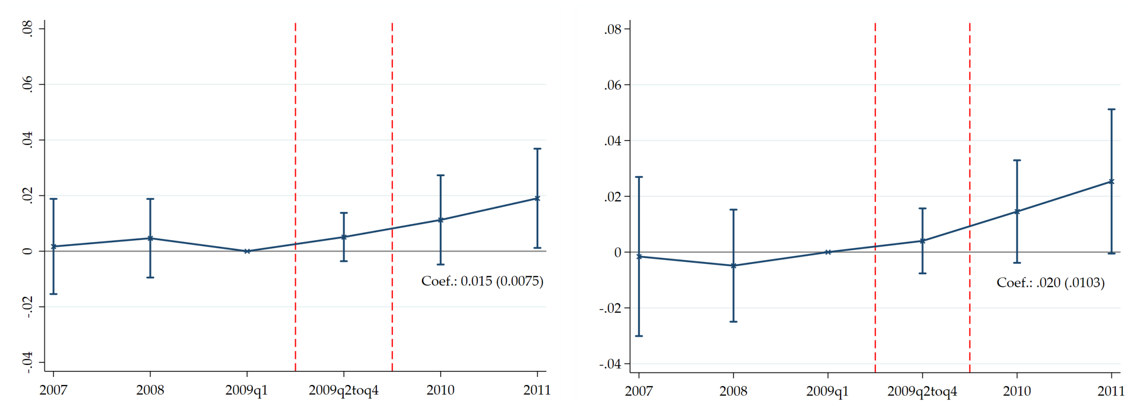
We now turn to an examination of the mechanisms behind the increase in formal employment resulting from the 2009 PBF expansion. On their own, standard income and substitution effects among the beneficiaries of a means-tested program such as PBF would predict a decrease in formal labor supply (see, e.g., Bergolo and Cruces 2021). However, the increase in local resources from PBF payments could also generate multiplier effects in the local economy expanding labor demand, or the PBF benefits could unlock liquidity constraints in job search among beneficiaries (Baird et al. 2018), generating a positive income effect on their own formal labor supply. This section explores both mechanisms.

4.1 Local demand effect

A. Impact on non-beneficiaries. To study whether PBF payments affect the local economy beyond their impact on PBF beneficiaries themselves, we investigate whether workers who have no direct link to the program experienced an increase in formal employment as well. We exploit the fact that we can link individuals in *RAIS*, in *Cadastro Unico*, and in the PBF payment sheets. Specifically, we select all workers who appeared in *RAIS* at any point over our sample period. We then only keep those who were never PBF beneficiaries, that is, those who never part of a family that received PBF benefits during our sample period. Finally, we compute the number of workers in this sample who were formally employed in each month by municipality. Figure 6a shows that our difference-in-differences results are similar in this sample.[24]

FIGURE 6: IMPACT OF THE 2009 REFORM - DEMAND EFFECTS

(a) Never beneficiaries (b) Micro-region



Note: The figure reports the $\widehat{\beta_p}$ coefficients (with their 95% confidence intervals) from estimating the same specification and definition of the variable $Treat_{m,s}$ as in Figure 5. Panel (a) displays estimates for private-sector formal employment among workers who were never beneficiaries of PBF. Panel (b) displays estimates for private-sector formal employment aggregating the data at the level of the micro-region and comparing micro-regions that experienced higher (top 50 percent) versus lower (bottom 50 percent) relative changes in quota due to the change of methodology in 2009. The vertical lines indicate the start and end of the rollout of the 2009 reform.

To complement this finding and to better understand which workers were most affected by the PBF expansion, we explore the heterogeneity of our results across other worker categories in Table 4. Using detailed information on occupations, we first classify workers into higher-skilled and lower-skilled occupations.[25] Columns [1] and [2] show that the increase in formal employment is entirely driven by lower-skilled jobs. Columns [3] and [4] show that the relative increases in formal employment are similar for men and women. This would imply larger increases in levels among men, however, because men account for a larger share of formal employment.[26] Finally, columns [5] and [6] show

[24]This sample of workers is likely deferentially selected between our two groups of municipalities given that one group experiences an increase in the number of PBF beneficiaries and PBF beneficiaries are systematically excluded from the sample. However, any differential selection is constant over time (workers are excluded from the sample even if they receive PBF benefits in one month only). Moreover, the estimates pre-reform in Figure 6a are close to zero, providing support for a common-trend assumption in that sample.

[25]We use the correspondence table developed by Helpman et al. 2017 to convert the Brazilian classification of occupations at 4 digits into the following five categories: Professional and Managerial, Skilled White Collar, Skilled Blue Collar, Unskilled White Collar, and Unskilled Blue Collar. We then group the first three categories as skilled and the two last categories as low-skilled.

[26]We report the average number of formal employees in the control group in the post-reform period in each column of Table 4.

that the relative increase is larger for black and mixed-race (*pardo*) workers than for white workers, although the estimates are less precise in this case.[27] This is consistent with the fact that black and mixed-race workers are over-represented in lower-paid formal jobs in Brazil.

B. Additional evidence on local demand effects. Given that the increase in formal employment affects non-beneficiaries, a natural explanation for our findings would be that the increase in PBF payments led to an overall increase in demand in the local economy, which in turn led to an increase in labor demand. This mechanism is consistent with Corbi et al. (2019) who find that an increase in inter-governmental transfers to Brazilian municipalities also led to an increase in private-sector formal employment.

We provide three additional pieces of evidence for an overall increase in activity in the local economy. First, as in other papers on local multipliers in the literature (e.g., Suárez Serrato and Wingender 2016), we show that our results are not simply driven by a reallocation of formal employment across neighboring municipalities within a greater local economy. Following Corbi et al. (2019), we aggregate the data at the level of the micro-region and perform the exact same analysis as above comparing micro-regions that experienced higher (top 50 percent) versus lower (bottom 50 percent) relative changes in quota due to the change of methodology in 2009. Figure 6b shows that our point estimates are essentially unchanged, although they become less precise. This is consistent with several recent studies that have documented the lack of worker mobility in response to labor demand shocks in Brazil.[28]

The increase in formal employment that we documented could occur through the creation of new formal jobs or through the formalization of jobs that were previously informal in the same local labor market.[29] The informality literature highlights that job formalization in itself (i.e., even without job creation) is also associated with real increases in economic activity (Ulyssea 2020). Nevertheless, to support the conclusion that our results capture relevant changes in local economic activity, we also present results for two additional outcomes. IBGE produces yearly estimates of municipal GDP and taxes paid in the municipality every year. Figures 7a and 7b show estimates from a similar difference-in-differences specification as before, but at the yearly level (2008 is the reference year),

[27] We follow other papers in the literature in aggregating black and mixed-race workers together, as wage gaps compared with white workers are similar for both groups (see, e.g., Gerard et al., 2021).

[28] For instance, Dix-Carneiro and Kovak (2019) show that workers who were 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.

[29] Unfortunately, there is no available data in Brazil that would allow us to extend our analysis to the informal sector. The yearly labor force surveys in Brazil are not representative at the level of the municipality or the micro-region, only at the state level.

Table 4: Impact of the 2009 reform – Heterogeneity by worker/industry characteristics

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Higher Skill Occupations	Lower Skill Occupations	Men	Women	White	Black and Mixed	Tradables	Non-Tradables
Private-sector formal employment	0.004	0.036***	0.020**	0.022***	0.011	0.019	0.008	0.013
	(0.007)	(0.011)	(0.008)	(0.008)	(0.011)	(0.014)	(0.012)	(0.009)
Average formal employees in control group post	997	1183	1490	733	1368	733	939	1285
Municipal FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
State-by-period FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period interactions with Log Quota 2006	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period interactions with the counterfactual Δ Quota 2009	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*

Note: The table presents heterogeneous effects of the 2009 reform on private-sector formal employment by worker/industry characteristics. It reports the $\widehat{\beta_p}$ coefficients post-reform (with standard errors in parentheses) from estimating the same specification as in Table 3. Columns [1]-[6] present results for higher-skill versus lower-skill occupations (using the categorization in Helpman et al. 2017), for men versus women, and for white versus black and mixed-race workers, respectively. Columns [7] and [8] present results for tradable and non-tradable industries, respectively, using the categorization in Dix-Carneiro and Kovak 2019. In each column, the table also reports the average number of formal employees in the control group in the post-reform period. In all cases, we use an inverse hyperbolic sine transformation of the outcome to ensure that we maintain the same balanced panel throughout the analysis. As a reference, the main results for total PBF payments and private-sector formal employment using the inverse hyperbolic sine transformation are very similar to the ones using logarithms in column [1] of Table 3: 0.136 (s.e. 0.009) and 0.020 (s.e. 0.007), respectively.

using these two variables. The results are in line with our formal employment results, showing gradual increases in local GDP and local taxes starting in 2009, reaching 1.7 and 2.7 percent by 2011, respectively.

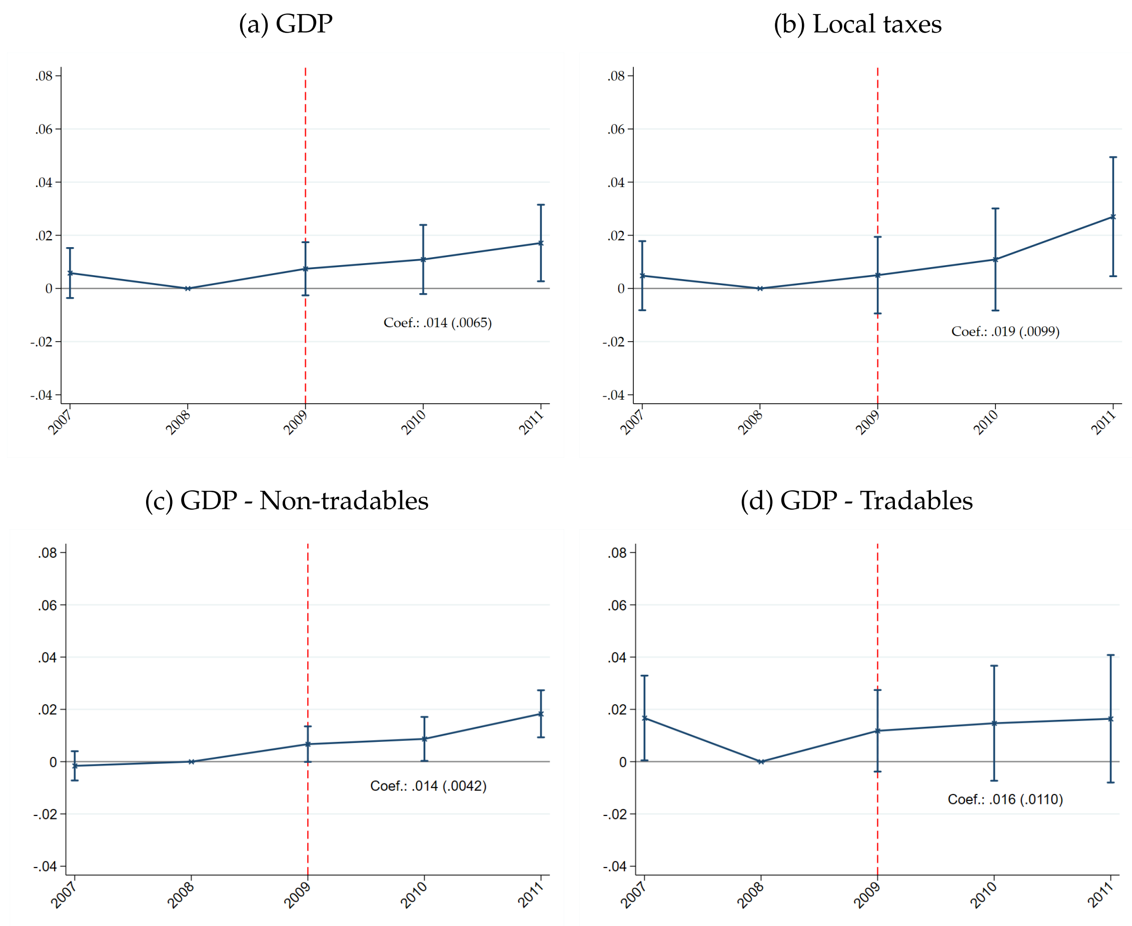
The literature on local multipliers (see, e.g., Nakamura and Steinsson, 2014) emphasizes that increases in government spending at the local level are more likely to lead to stronger local demand effects if the increase in local resources is spent rather than saved and if the spending is concentrated on locally produced goods. We do not have data on the spending pattern of PBF beneficiaries, so we cannot provide direct evidence for these two conditions. It is reasonable to argue, however, that the first condition is likely satisfied in our context: the propensity to spend will be particularly high among poor households, and thus among PBF beneficiaries (e.g., Krueger 2012; Johnson et al. 2006). We shed light on the second condition in two ways. First, IBGE decomposes its yearly estimates of municipal GDP into non-tradable industries (that is, services) and tradable industries (that is, agriculture and manufacturing). Figure 7c shows a clear increase in the municipal GDP attributed to non-tradable industries after the reform, which is precisely estimated and of similar magnitude as the effect on total GDP. By contrast, Figure 7d shows that we do not find conclusive evidence for an effect on tradable industries. Second, using information on the industry code of each establishment, we show in the last two columns of Table 4 that our point estimates for the effect on private-sector formal employment are larger for jobs in non-tradable versus tradable industries,[30] although estimates become noisy when we explore heterogeneity across industries in the data. We note that the point estimates in these two exercises would also imply larger increases for non-tradable industries in levels, because non-tradable industries account for a larger share of GDP and employment. Additionally, non-tradable industries tend to be more informal than tradable industries (Dix-Carneiro and Kovak 2019), such that the same relative change in formal employment would be consistent with a larger overall impact on non-tradable industries.

C. Implied cost per formal job. To quantify the results, the empirical literature on the impact of local government spending on local labor markets often focuses on the cost per job rather than the GDP multiplier, in part because local GDP is measured more noisily (Zidar, 2019; Chodorow-Reich, 2019). As in Corbi et al. (2019), we thus discuss our results through the lens of the implied cost per formal job in Table 5.

Our preferred estimates for the impact of the 2009 PBF expansion on the total PBF payments and the number of formal private sector jobs are 13.6 and 2 percent, respectively, as replicated at the top of column [1] in Table 5. In the next row, we report the

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

FIGURE 7: IMPACT OF THE 2009 REFORM - LOCAL ECONOMIC ACTIVITY



Note: The figure reports the $\widehat{\beta_p}$ coefficients (with their 95% confidence intervals) from estimating a similar specification as in Figure 5 at the yearly level (2008 is the reference year). Panels (a), (b), (c) and (d) display results for the logarithm of municipal total GDP, the logarithm of taxes paid in the municipality, the logarithm of municipal GDP attributed to non-tradable industries (that is, services), and the logarithm of municipal GDP attributed to tradable industries (that is, agriculture plus manufacturing) (variables that are only available at the yearly level), respectively. The vertical line indicates the year of the reform.

implied cost per formal job — that is, the number of additional formal jobs divided by the increase in PBF payments — for the median municipality, together with 95 percent confidence intervals based on 1,000 bootstrapped replications.[31] Our estimate of US$5,600 at the yearly level,[32] which corresponds to about 2.1 times the minimum wage at the time, is much smaller than typical cost-per-job estimates in the United States. For instance, Chodorow-Reich et al. (2012), Suárez Serrato and Wingender (2016), and Zidar (2019) all

[31]Specifically, we apply our point estimates to the average of the total PBF payments and of the number of formal private-sector jobs post-reform (2010-2011) in control municipalities to compute the number of additional formal jobs and the increase in PBF payments that they would have experienced if they had been in the treatment group. We then report the ratio between these two variables for the median municipality in that sample. Confidence intervals are obtained from 1,000 bootstrapped replications of our point estimates.

[32]Our cost-per-job estimates are in 2016 US$ to be directly comparable to those in Corbi et al. (2019).

report estimates around US$30,000, although we note that our confidence intervals are relatively wide, including a value of up to US$26,833. There are clear differences in wages between Brazil and the United States, such that we would not expect similar values in the two countries. Our point estimate is much closer to the preferred estimate in Corbi et al. (2019) of US$8,000. It is possible that direct transfers to poor families with likely high marginal propensity to consume in our context generate larger demand effects than transfers to local governments.

Corbi et al. (2019) compute a local multiplier based on their estimate of the cost per formal job following the methodology relating output and employment multipliers in Chodorow-Reich (2019).[33] Using their calibration, our point estimate implies a local multiplier around 2.8 compared with around 2 in Corbi et al. (2019). The presence of a large informal sector, however, challenges the applicability of the methodology in Chodorow-Reich (2019) in the Brazilian context. On the one hand, the impact of government spending could be even larger if it also led to the creation of new informal jobs, which we do not measure. Relatedly, Corbi et al. (2019) propose to adjust their estimate by a factor of 1.2 to account for increases in informal economic activity. On the other hand, new formal jobs could result from the formalization of existing informal jobs. The gain in output could still be sizable in that case, but it would be smaller than if it resulted from the creation of a new job.[34]

We illustrate this point more concretely in Table 5, by replicating the same analysis as in column [1], but separately for municipalities with rates of labor market informality (as measured by the 2000 census) below the median in column [2] versus above the median in column [3]. The point estimates for the relative impacts on total PBF payments and private-sector formal employment are comparable in the two columns. However, high-informality municipalities have lower levels of formal employment to begin with and are also typically poorer such that they receive higher PBF payments. As a result, the same point estimates imply a cost per formal job that is very different between the two groups of municipalities. The cost per formal job in more formal labor markets (US$2,200) is much smaller (less than a minimum wage). This is consistent with the possibility that there are more jobs at the margin of formalization in those labor markets, such that job formaliza-

[33] This methodology uses the relationship between output multiplier μ_Y and employment multiplier μ_E (i.e., the inverse of the cost per job) implied by a neoclassical production function: $\mu_Y = (1-\chi)(1+\xi)(Y/E)\mu_E$, where χ is the share of capital in the production function, ξ is the elasticity of hours per worker to total employment, and Y/E is income or output per worker.

[34] Using our estimates for the impact of the PBF expansion on local GDP directly, we obtain multipliers with very wide 95 percent confidence intervals: from 1.8 to 20.9. Because local GDP is measured more noisily (see the discussion in, e.g., Zidar 2019), we focus on formal employment to discuss the magnitude of our effects.

tion might play a bigger role in that case. By contrast, the cost per formal job is much larger in more informal labor markets (US$20,417), bringing it closer to U.S. estimates. This is consistent with the possibility that our analysis misses economic activity generated in the informal sector in those labor markets, and that it takes much larger transfers to create a formal job in contexts where informality is pervasive.[35]

TABLE 5: IMPACT OF THE 2009 REFORM – COST PER FORMAL JOB

	[1]	[2]	[3]
	Main Sample	Low Informality	High Informality
Total PBF payments	0.136***	0.132***	0.136***
	(0.009)	(0.012)	(0.008)
Private-sector formal employment	0.020***	0.018**	0.023*
	(0.007)	(0.008)	(0.013)
Implied cost per formal job created (US$2016)	5600	2200	20417
[95% confidence interval]	[3176; 26833]	[1086; +∞)	[9322; +∞)
Number of obs	298,860	149,460	149,400
Municipal FE	*yes*	*yes*	*yes*
State-by-period FE	*yes*	*yes*	*yes*
Period interactions with Log Quota 2006	*yes*	*yes*	*yes*
Period interactions with the counterfactual Δ Quota 2009	*yes*	*yes*	*yes*

Note: The table presents estimates of the cost per formal job implied by the impact of the 2009 reform. Column [1] uses all municipalities and columns [2] and [3] explore heterogeneity across municipalities by labor market informality: they present results for municipalities with rates of labor market informality (as measured in the 2000 census) below the median versus above the median, respectively. In each column, the table first reports the $\widehat{\beta_p}$ coefficients post-reform (with standard errors in parentheses) from estimating the same specification as in Table 3 for total PBF payments and private-sector formal employment. The table then reports the implied cost per formal job — that is, the number of additional formal jobs divided by the increase in PBF payments — for the median municipality, together with 95% confidence intervals based on 1,000 bootstrapped replications of our point estimates. The upper bounds of the confidence intervals are undefined in columns [2] and [3].

4.2 Micro-level evidence on the impact of PBF on beneficiaries' formal labor supply

We showed that the 2009 PBF expansion increased local formal employment and that this increase was not concentrated among PBF beneficiaries. It remains an open question, however, whether the local demand effects of PBF occur despite negative formal labor supply responses among beneficiaries (e.g., driven by standard income and substitution effects) or, instead, are partly driven by positive responses among beneficiaries (e.g., recent research argues that cash transfers could help beneficiaries find better jobs) (Banerjee et al. 2017; Baird et al. 2018). Our aggregate results would underestimate the strength of local demand effects in the first case and overestimate it in the second case.

[35] The confidence intervals for the costs per formal job are very wide and the upper bounds are undefined in columns [2] and [3]. Yet, if we replicate a similar exercise using the variables presented in Figure 7, we obtain point estimates for the local GDP and tax multipliers that are much larger in municipalities with lower levels of labor market informality, but again with wide confidence intervals.

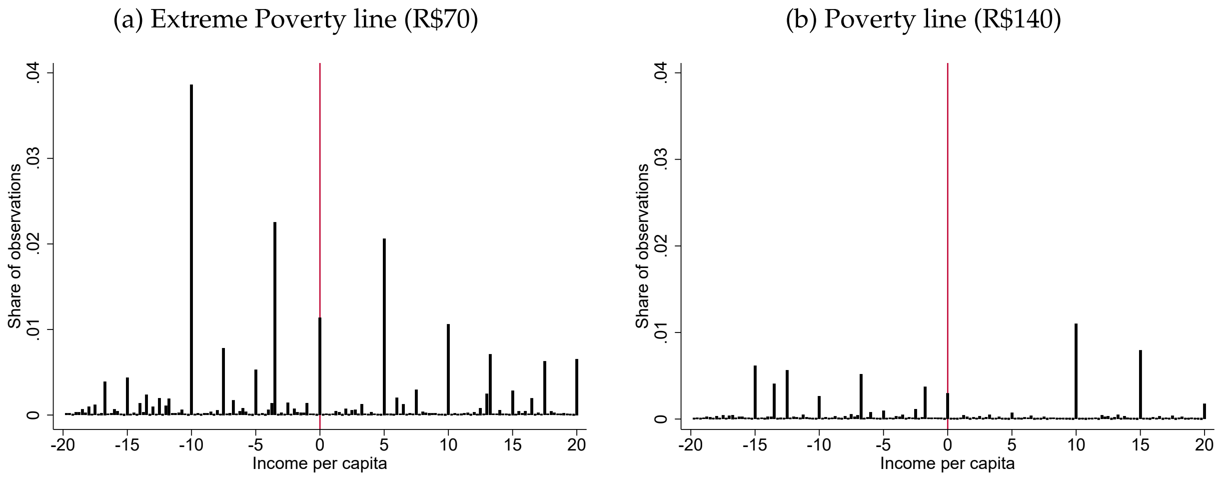
To make progress on this question, we exploit micro-level data and compare the formal labor supply of families eligible for different PBF benefit amounts through a Regression Discontinuity (RD) design. We use the fact that if a family's income per capita in *Cadastro Unico* rises above the extreme poverty line, the basic benefit may be "taxed away." We also exploit the discontinuity at the poverty line used to determine eligibility for the variable benefits. Concretely, we compare families with income per capita just below and just above the eligibility cutoffs according to the August 2010 snapshot of *Cadastro Unico*, and look at their formal employment outcomes in the following 12-month period. Focusing on this time period is useful as it is a stable period in terms of the institutional features of PBF (no changes in municipal quotas, eligibility thresholds, or benefit levels).

Families below the cutoffs are eligible for higher PBF benefits (and receive higher benefits as we show below), potentially giving rise to income effects. Families below the cutoffs have less of an incentive to increase their formal employment as it may trigger a readjustment upward of their income per capita in *Cadastro Unico* and a loss of benefit eligibility. The period that we study was characterised by economic growth in Brazil, associated with an overall increase in formal employment. In a similar macroeconomic environment, Bergolo and Cruces (2021) provide evidence that means testing reduced formal employment growth among beneficiaries of a transfer program in Uruguay.

In principle, families above the cutoffs could also have incentives to decrease their formal employment. This may allow them to more easily adjust downward their income per capita in *Cadastro Unico* to cross the poverty line and become eligible for PBF. However, as discussed in Section 1.1, PBF is not an entitlement, such that families have no guarantees that they will become PBF beneficiaries even if they become eligible. Moreover, the big increase in the national number of slots following the 2009 expansion had already taken place by August 2010 (see Figure 1). Any new slots might also not be assigned to their municipality. A reduction in reported income per capita from above to below the extreme poverty line would entitle a family to the basic benefit. Receiving it would not be immediate, however, as *Cadastro Unico* would first have to be consolidated at the federal level (see MDS 2004). Moreover, if a family reports a reduction in income to *Cadastro Unico* from above to below the extreme poverty line, the municipality can visit their home to gauge whether their living conditions are consistent with extreme poverty (see MDS 2011).

In the analysis below, any incentive to decrease formal employment among families located above one of these cutoffs would bias our estimates toward finding a positive formal labor supply response. To the extent that we do not find any positive effect, the existence of such a bias would only strengthen our conclusion that the increase in local formal employment in the previous section is driven by demand effects.

FIGURE 8: DISTRIBUTION OF MONTHLY INCOME PER CAPITA IN CADASTRO UNICO



Note: This figure plots the distribution of monthly income per capita in the August 2010 snapshot of Cadastro Unico, by bins of R$0.25 around the extreme poverty line (R$70) in panel (a) and around the poverty line (R$140) in panel (b). The red vertical line in each panel indicates the level of the relevant cutoff (income per capita is normalized to the cutoff in each panel).

A. Empirical strategy. A challenge in exploiting the discontinuities at the extreme poverty line and at the poverty line through an RD design is that the distribution of income per capita in *Cadastro Unico* may feature strategic bunching of families just below these cutoffs, with the risk of creating differential selection of families on the two sides of the cutoffs. In practice, however, it is not straightforward for families to target a specific income per capita in *Cadastro Unico* (see the discussion in Section 1).

Figure 8 displays the distribution of income per capita in August 2010 by bins of R$0.25 around the extreme poverty line (panel a) and around the poverty line (panel b). A striking pattern is that the distribution presents many mass points, which are large at round numbers (but not only at round numbers), including at the eligibility cutoffs. The excess mass at the two eligibility cutoffs is smaller than at other surrounding income per capita levels. Thus, it is not clear that the excess mass at the two eligibility cutoffs is driven by any strategic bunching or is particularly unusual in this distribution.

To evaluate this concern more systematically, we implemented the manipulation test proposed in Cattaneo et al. (2020) at each of the R$0.25 income per capita levels displayed in Figures 8a and 8b (200 levels in each case). Online Appendix Figure B.2 presents the value of the test statistic at each income per capita level. It shows that this test detects a significant discontinuity at most income per capita levels, confirming that the distribution of our running variable is not smooth. Moreover, the test statistics at the extreme poverty and poverty lines are not outliers in that respect: they fall neither in the top 5 percent nor in the bottom 5 percent of the distribution across income per capita levels. We take away from this analysis that families may not be strategically located just below these cutoffs.

This discussion also highlights that our setting does not lend itself to the nonparametric methods used to estimate treatment effects in the RD literature, which typically rely on the smoothness of the distribution of the running variable. In this context, we thus adopt a more parametric approach, in which we estimate the following specification:

$$Y_i = f(\widetilde{Z}_i) + \beta D_i + \varepsilon_i, \tag{7}$$

where Y_i is an outcome of interest for family i, Z_i is their income per capita in the August 2010 snapshot of *Cadastro Unico*, Z_0 is either the extreme poverty line or the poverty line, $f(.)$ is a linear function of the normalized income per capita $\widetilde{Z}_i = Z_i - Z_0$, and D_f is a dummy variable that takes value 1 if income per capita is less than or equal to the respective eligibility cutoff ($\widetilde{Z}_i \leq 0$). The coefficient β captures a discontinuity in the outcome at the eligibility cutoffs. Standard errors are clustered at the income per capita level. Our main results use a bandwidth of R$20 on either side of the cutoffs, but we present robustness checks for different bandwidth sizes. We also show how our estimates compare if we perform the same analysis assuming that the cutoffs were hypothetically located at each of the R$0.25 income per capita levels displayed in each of the panels in Figure 8.

Before discussing the results, we provide some support for our empirical strategy. We show that families located on different sides of the two cutoffs do not appear to be systematically different based on observables recorded in the August 2010 snapshot of *Cadastro Unico*. Specifically, Online Appendix Figures B.3 and B.4 show that there is no visible discontinuity at the cutoffs in terms of (i) family size, (ii) number of rooms in the dwelling, (iii) whether the family lives in a rural area, (iv) whether the family receives any retirement or unemployment benefits, (v) the share of females in the household, and (vi) whether the household head completed high school. Panel A in Table 6 also presents the $\widehat{\beta}$ coefficients from estimating the specification in equation (7) using each of these characteristics as outcome of interest. It confirms the absence of any significant discontinuity in these observable characteristics at the cutoffs. Therefore, we find no evidence of any systematic differential selection at the extreme poverty line or at the poverty line.

B. Results. We construct three main outcome variables following families for the 12-month period after the August 2010 snapshot of *Cadastro Unico*: the total PBF benefits received, the total number of months observed in formal employment across all adults in the family, and the total income from formal employment across all adults in the family. Figure 9 displays the averages of these variables by income per capita bins of R$0.25 around the two cutoffs. It also displays the linear fit on each side of the cutoffs, and the discontinuity at the cutoffs, from estimating the specification in equation (7). Panel B in Table 6 presents the corresponding RD estimates ("no controls"). It also displays estimates of $\widehat{\beta}$ from including the predetermined family characteristics considered in panel A as controls when estimating the specification in equation (7), as well as municipality fixed effects.

We find clear evidence that families that are eligible for higher PBF benefits based on their income per capita in August 2010 indeed received higher benefits in the next 12 months. We estimate an increase of R$467.9 in benefits for families at the extreme poverty line, and an increase of R$234.9 in benefits for families at the poverty line. Adding controls does not affect our point estimates, but it greatly improves their precision.

We find no evidence that receiving higher PBF benefits increases families' formal employment outcomes. The point estimates are actually negative for both the number of adult-months in formal employment and formal employment income. The discontinuity is not evident in Figure 9, in part because of the large variance of these two variables across income per capita levels and in part because all the dots in Figure 9 do not contribute equally to the estimation given the non-smooth distribution of the running variable.[36] The corresponding RD estimates reported in Table 6 ("no controls") are economically large, but they are imprecisely estimated. Adding controls greatly improves precision again, and the RD estimates become statistically significant at conventional levels. The point estimates become smaller, but they remain economically large: they imply an 8.4 percent reduction in the number of adult-months in formal employment at both cutoffs, a 9.2 percent reduction in formal employment income at the extreme poverty line, and a 12.5 percent reduction at the poverty line.

We end by presenting two sets of robustness checks. First, we estimated the same specifications as for the results in panel B in Table 6 (with controls) assuming that the cutoffs were located at each of the R$0.25 income per capita levels displayed in Figures 8a and 8b. Online Appendix Figures B.5 and B.6 present the value of the T-statistic for the estimated $\widehat{\beta}$ at each income per capita level. The figures show that the estimates that we obtain at the extreme poverty and poverty lines are clear outliers compared with estimates obtained at other income per capita levels, with maybe the exception of the number of adult-months in formal employment at the poverty line. Second, Online Appendix Figure B.7 shows that our point estimates are essentially unchanged if we vary the bandwidth size.

The above estimates are only local to the cutoffs, and the RD design is imperfect given

[36]The linear fits are not centered with respect to the dots in Figure 9 because the lower dots tend to be those with mass points in the distribution of the running variable (see Figure 8).

Table 6: Impact of PBF benefits on beneficiaries' formal labor supply

	[1]	[2]	[3]	[4]
	Extreme Poverty Cutoff		**Poverty Cutoff**	
	RD estimate	(s.e.)	RD estimate	(s.e.)
A. Covariates (August 2010)				
Family size	-0.0931	(0.544)	0.0572	(0.481)
Number of rooms in dwelling	-0.000339	(0.111)	0.00446	(0.103)
Living in rural area (dummy)	0.00436	(0.0173)	0.0219	(0.0195)
Receives any pension or UI benefit (dummy)	-0.0144	(0.0464)	0.0848	(0.0806)
Share of women in household	-0.000983	(0.0165)	-0.00693	(0.0198)
Household head completed high school	-0.00269	(0.0126)	0.0162	(0.0114)
B. Outcomes (September 2010-August 2011)				
PBF benefits				
No controls	467.9***	(96.23)	234.9***	(65.19)
Covariates and municipality fixed effects	451.8***	(36.72)	229.4***	(37.23)
Adult-months in formal employment				
No controls	-0.610	(0.668)	-0.768	(1.226)
Covariates and municipality fixed effects	-0.403**	(0.204)	-0.580*	(0.310)
Income in formal employment				
No controls	-549.8	(508.0)	-962.2	(1,030)
Covariates and municipality fixed effects	-355.5**	(167.9)	-738.0***	(248.4)

Note: The table presents results for the impact of PBF benefits on beneficiaries' formal labor supply. It reports regression discontinuity estimates (with standard errors in parentheses) that compare families with income per capita on different sides of either the extreme poverty line (columns [1] and [2]) or the poverty line (columns [3] and [4]) in the August 2010 snapshot of Cadastro Unico (families with income per capita below the cutoff are eligible for higher PBF benefit amounts). Panel A tests for any discontinuity in pre-determined covariates recorded in the August 2010 snapshot of Cadastro Unico using the specification in equation (7). Panel B tests for any discontinuity in the total amount of PBF benefits received, in the number of adult-months in formal employment, and in the total income from formal employment in the following 12-month period. For each outcome, it reports estimates using the specification in equation (7) ("No controls") and a specification that controls for the covariates in panel A as well as municipality fixed effects.

the distribution of the running variable. Nevertheless, we interpret these results as suggesting that, if anything, our aggregate results in the previous section likely underestimate the strength of the local demand effects induced by the *Bolsa Familia* program. If we were to take the negative effects at face value, they are most likely reflecting a substitution effect. Income effects are typically zero in similar contexts (e.g., Banerjee et al. 2017), and another study in our context using children's age as variation in benefit amount among PBF beneficiaries — generating only income effects — find no change in formal or informal employment (Barbosa and Corseuil 2014). The negative effects are in line with the results in Bergolo and Cruces (2021) who study labor supply responses to cash transfers generating both income and substitution effects.

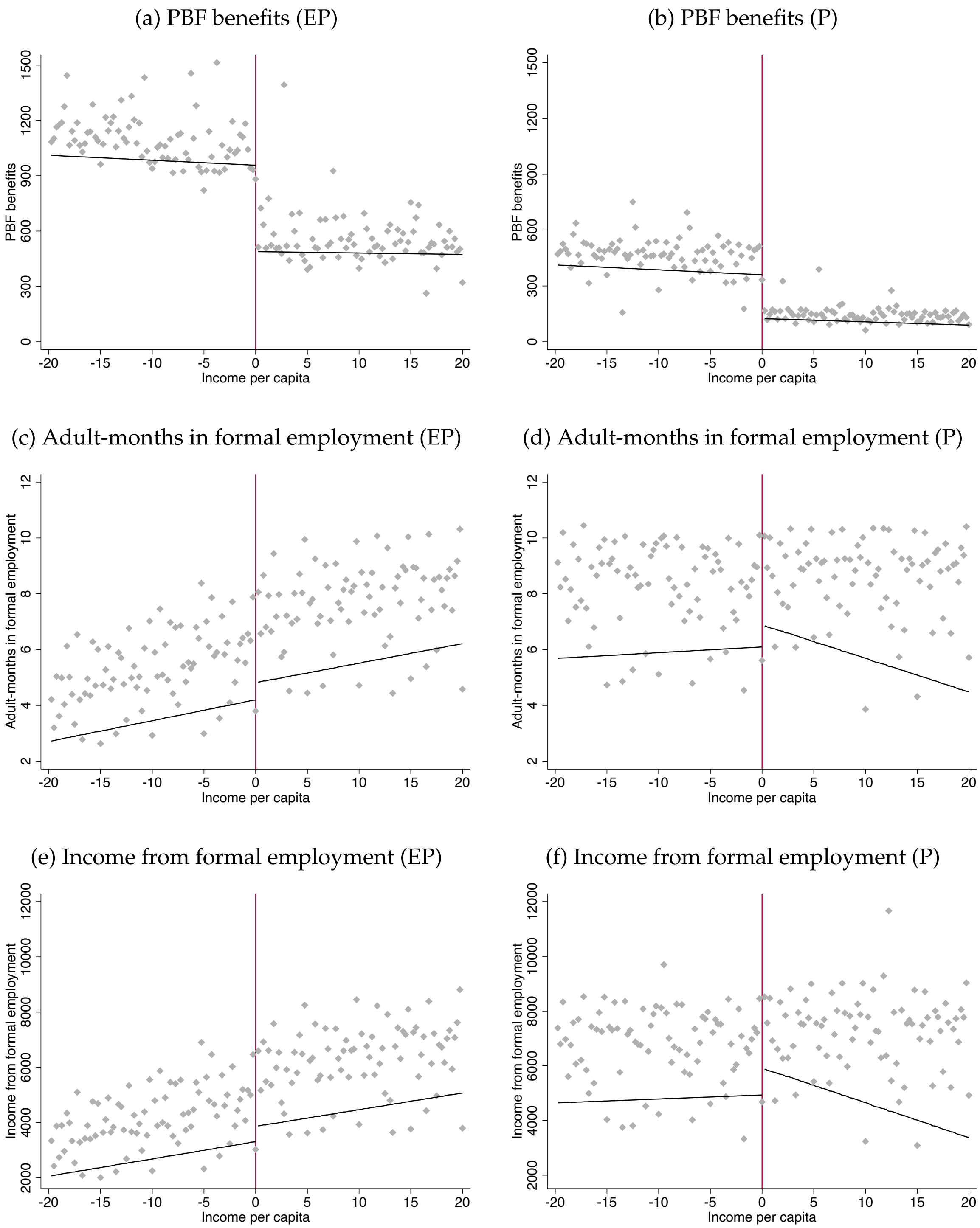
5 Conclusion

We have provided evidence on the aggregate effect of cash transfers on formal labor markets in a developing country context. Cash transfer programs are the main type of safety net in middle-income countries, and the COVID-19 crisis accelerated the creation of new programs and the expansion of existing ones across the developing world (Gentilini et al. 2020). The perception that such programs may have negative effects on labor markets, particularly in the formal sector, is quite potent in the policy debate and can shape the future of these programs. We found that an expansion of one of the largest cash transfer program in the world – *Bolsa Familia* in Brazil – led to an increase in formal employment in localities where the transfers increased the most after the 2009 reform. This occurred despite the fact that the program may in fact reduce formal labor supply among its beneficiaries.

The results are consistent with a large local multiplier effect and are in line with recent evidence on the impact of government spending on local formal employment in Brazil in Corbi et al. (2019). The smaller cost per formal job that we found in the case of *Bolsa Familia* is consistent with larger demand effects from direct transfers to poor families (with high marginal propensity to consume) than from transfers to local governments (as in their setting). We also found that it costs substantially more to create a formal job in labor markets where informality is more pervasive. By contrast, more workers may be at the margin of formalization in more formal labor markets, implying a smaller cost per formal job. This heterogeneity within Brazil, a country that is well known for its wide regional disparities, is informative about the external validity of our findings. Yet, local multipliers could be larger in other countries where the targeting of cash transfers does not rely on means testing, and thus does not disincentivize participation in the formal economy.

This paper focused on formal jobs, a critical segment of the labor market associated with greater social security coverage, better working conditions, and higher output and total factor productivity (e.g., Perry et al. 2007; Ulyssea 2020). Yet, the high cost per formal job that we estimated for more informal labor markets suggests that our analysis missed economic activity generated in the informal economy. Thus it would be valuable for future research to build evidence on the overall aggregate effects of cash transfers, including the informal economy. More research is also needed to understand the size of potential distortions on the funding side to obtain a complete picture of the general equilibrium effects of cash transfer programs on labor markets in middle-income countries.

FIGURE 9: PBF BENEFITS AND BENEFICIARIES' FORMAL LABOR SUPPLY



Note: The figure displays averages of our three outcome variables by income per capita bins of R$0.25 around the Extreme Poverty (EP) line and around the Poverty (P) line. Each panel also displays the linear fit on each side of the cutoffs from estimating the regression discontinuity specification in equation (7). Income per capita is based on information from the August 2010 snapshot of Cadastro Unico. The outcome variables are the total amount of PBF benefits received (panels a and b), the number of adult-months in formal employment (panels c and d), and the total income from formal employment (panels e and f) in the following 12-month period. The red vertical line in each panel indicates the level of the relevant cutoff (income per capita is normalized to the cutoff in each panel).

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Online Appendix

Cash Transfers and Formal Labor Markets: Evidence from Brazil

by François Gerard, Joana Naritomi and Joana Silva

Appendix A: Political controversy about the effects of *Bolsa Familia* on labor markets

This Appendix displays examples of how relevant politicians discuss the effects of Bolsa Familia on labor markets. The first two highlight negative impacts on beneficiaries' labor supply, while the last two emphasize positive impacts on beneficiaries and the local economy.

1. **Jair Bolsonaro**, president of Brazil since 2018, former federal congressman:

"Isso é um crime, tem gente que está aí há nove anos no Bolsa Família, não quer ser empregado porque perde o Bolsa Família. O Bolsa Família atende famílias de até cinco filhos, essa garotada vai crescer, o quê, pensando no quê? Bolsa Família também. Você vê meninas no Nordeste, bate a mão na barriga, grávidas, e falam o seguinte, que tem também o auxílio natalidade, "essa aqui vai ser uma geladeira", e "esse aqui vai ser uma máquina de lavar". E não querem trabalhar". **Source**: Interview to UOL Notícias (April 14th, 2011).

2. **Aécio Neves**, federal congressman, former Senador and Governor of Minas Gerais, and former leading opposition candidate for president in the 2014 elections:

"...O que acontece hoje é o temor das pessoas que são beneficiárias do Bolsa Família de buscarem o espaço no mercado de trabalho formal, não se garantirem naquele emprego, não ficarem naquele emprego, e perderem os dois. (...) O Brasil vai ser um lugar melhor quando nós, respeitando os direitos daqueles que recebem o Bolsa Família, eles são intocáveis, nós comemorarmos porque o Brasil cresceu, se desenvolveu, essas pessoas se qualificaram, comemorarmos que temos duas ou três ou cinco milhões a menos de famílias no Bolsa Família." **Source**: Interview to *Roda Viva* TV Show (June 3rd, 2014).

3. **Luis Inácio Lula da Silva**, former president of Brazil:

"... dinheiro público aplicado em gente, em saúde, educação e renda e comida, pelos dados que a Tereza apresentou nunca mais pode ser tratado como se fosse gasto, mas sim um grande investimento. Eu sou favorável, mas as vezes a gente pega um bilhão, empresta para uma empresa. Ela vai fazer uma empresa, que vai gerar 200 empregos depois de pronta, e vai exportar quase nenhum gasto do que ela importa. Agora pegue um bilhão e dê no Bolsa-Família para ver quantos quilos de feijão, de carne, de buchada, de jabá, de peixe a pessoa vai comprar. Tá provado que o dinheiro do Bolsa Família movimenta o comércio, impulsiona o consumo de alimentos, roupas e produtos de higiene. Porque as pessoas também tem direito de ter acesso as coisas. Na verdade, a ampliação da renda, combinada com a valorização dos salários e a democratização do crédito, está na raiz do milagre que nós fizemos." **Source**: Instituto Lula's website.

4. **Ciro Gomes**, former congressman, governor of Ceará, and presidential candidate:

"O Bolsa Família veio num contexto de segurança alimentar (...) e tem um imediatismo compreensível (...). Nas regiões de economia deprimida, essas portas de saída já são mais simples de serem visualizadas hoje. Aquele pequeno comércio, as compras governamentais através da agricultura familiar interagindo com a questão do Bolsa Família já tem agitado uma dinâmica econômica muito rudimentar, muito simples, mas muito efetiva para milhões de pessoas." **Source**: Interview to Folha de São Paulo newspaper (February 9th, 2016).

Online Appendix B: Additional Tables and Figures

TABLE B1: CADASTRO UNICO AND FORMAL EMPLOYMENT

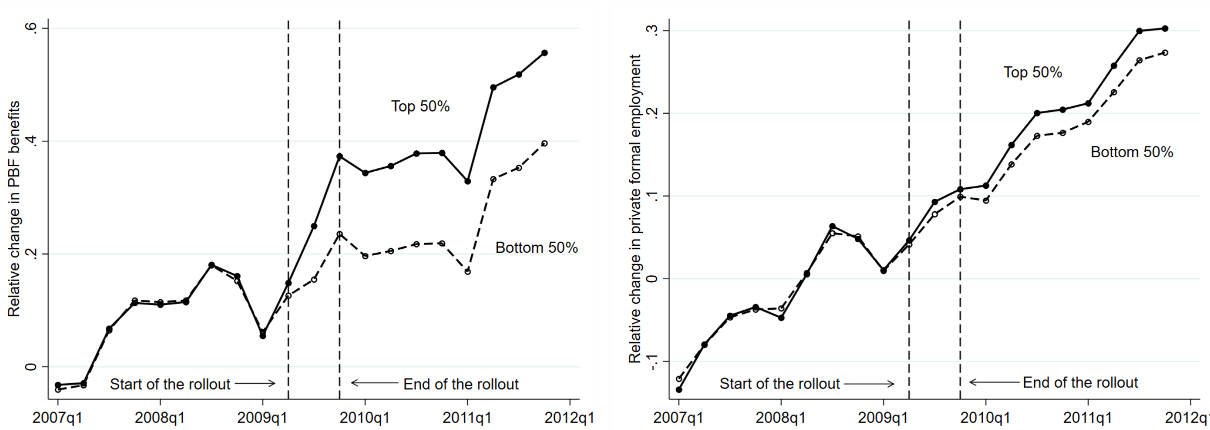
	All families	Families below the extreme poverty line	Families below the poverty line and above the extreme
Cadastro			
Number of families	20,564,520	12,254,032	4,980,800
Number of individuals	72,765,336	44,860,344	18,759,020
Share of adults who completed high-school	0.12	0.11	0.14
Avg. share urban	0.73	0.67	0.81
Average family size	3.6	3.7	3.8
Average per capita income	86.9	35.4	101.8
Average total income	271.0	129.3	383.9
RAIS: Sep. 2010 - Aug. 2011			
Share of families with at least 1 adult in RAIS	0.38	0.31	0.53
Number of adult-months in RAIS	3.11	2.45	5.14
Average wage in RAIS (conditional on working)	779.35	746.95	781.31

Note: The table presents summary statistics for families registered in *Cadastro Unico* in August 2010. We use the information on monthly per capita income (excluding PBF benefits) to classify families as extreme poor (below R\$70) or poor (between R\$70 and R\$140). The top panel only uses information from that snapshot of *Cadastro Unico* and the bottom panel matches these families to the formal employment data (*RAIS*) in the following 12-month period. Monetary values are reported in nominal terms.

FIGURE B1: RAW PATTERNS IN THE DATA BY SIZE OF $\Delta Quota^{2009}_{ms}$: P75 VS P25

(a) Total PBF benefits

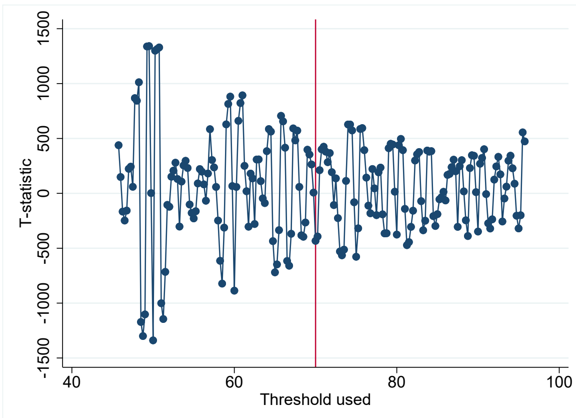
(b) Private-sector formal employees



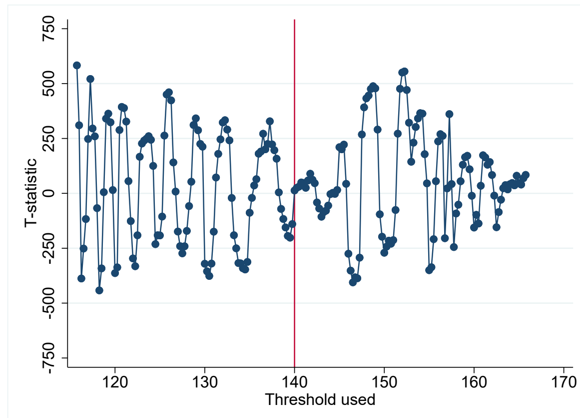
Note: The figure displays the average of the relative change in total PBF payments (panel a) and in the number of private-sector formal employees (panel b) for municipalities in the top 75 percent and bottom 25 percent of the distribution of $\Delta Quota^{2009}_{ms}$. The vertical lines indicate the start and the end of the roll-out of the 2009 reform.

FIGURE B2: DENSITY MANIPULATION TEST AROUND THE PBF ELIGIBILITY CUTOFFS

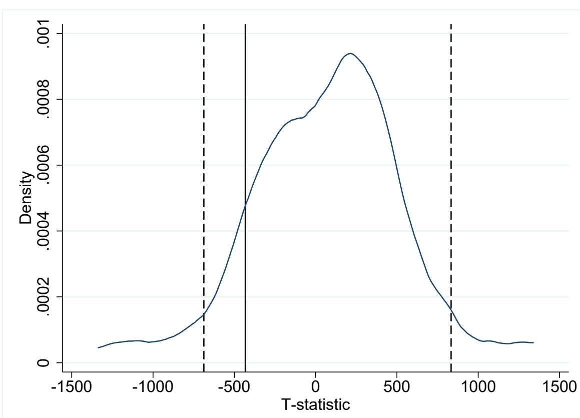
(a) T-statistic for the density manipulation test by income per capita level (R$.25 bin) around the extreme poverty line (R$70)



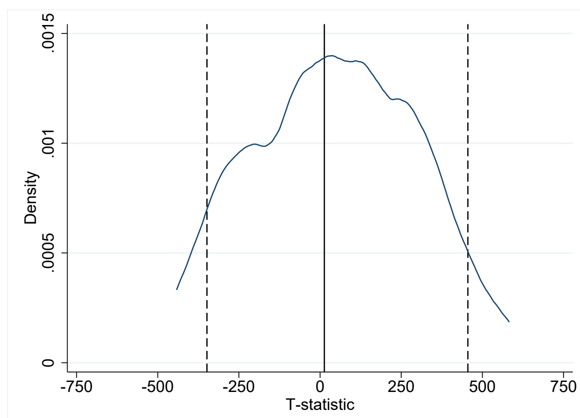
(b) T-statistic for the density manipulation test by income per capita level (R$.25 bin) around the poverty line (R$140)



(c) Distribution of T-statistics for the test across income per capita levels around the extreme poverty line (R$70)

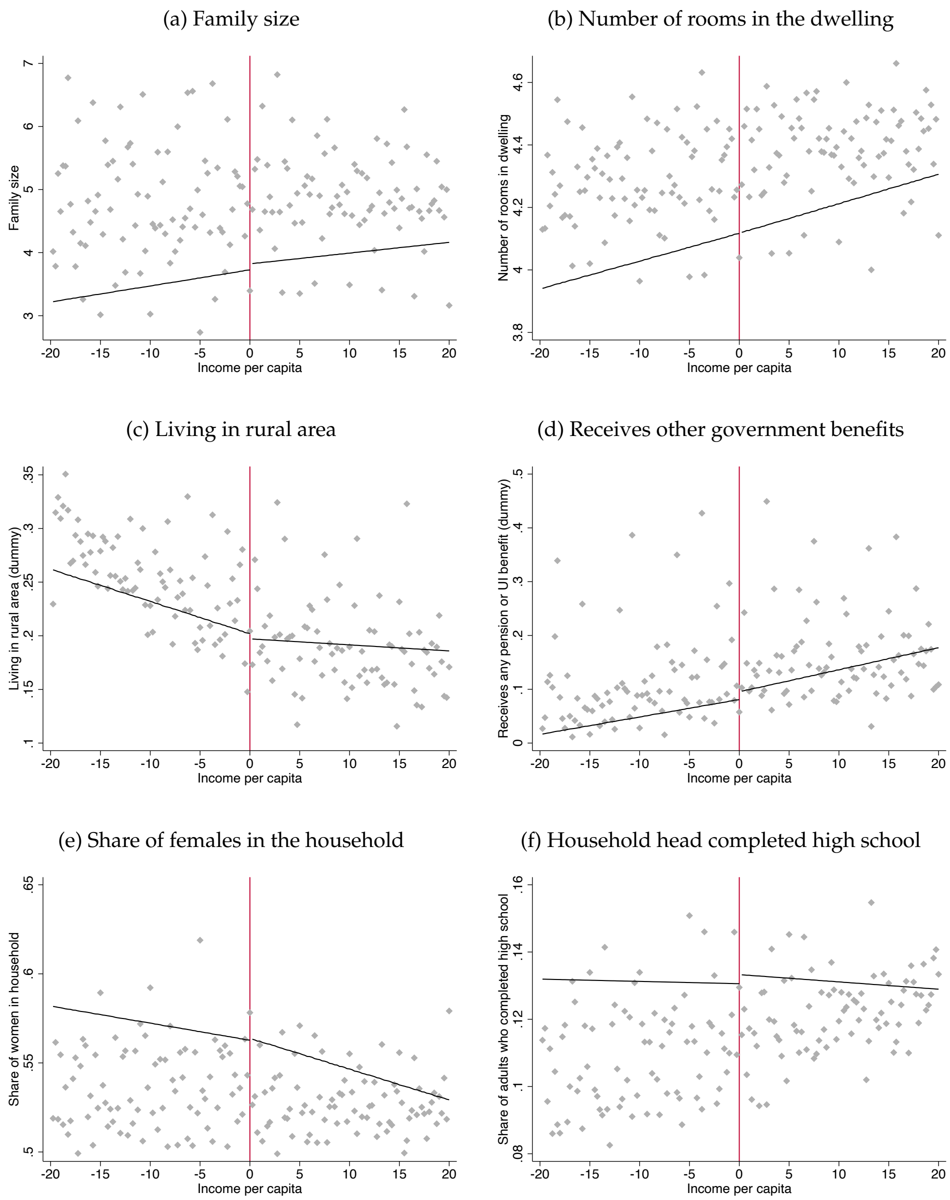


(d) Distribution of T-statistics for the test across income per capita levels around the poverty line (R$140)



Note: The figure presents results from implementing the density manipulation test proposed in Cattaneo et al. (2020) at each of the R$0.25 income per capita levels displayed in Figures 8a (around the extreme poverty line) and 8b (around the poverty line) in the paper (see, also, Cattaneo et al. 2018). Panels (a) and (b) plot the value of the t-statistics at each income per capita level. The solid lines indicate the location of the extreme poverty line in panel (a) and of the poverty line in panel (b). Panels (c) and (d) display the distribution of the T-statistics across the income per capita levels in panels (a) and (b), respectively. The dashed lines indicate the 5th and the 95th percentiles of the distribution. The solid lines highlight the value of the T-statistic at the extreme poverty line in panel (a) and at the poverty line in panel (b).

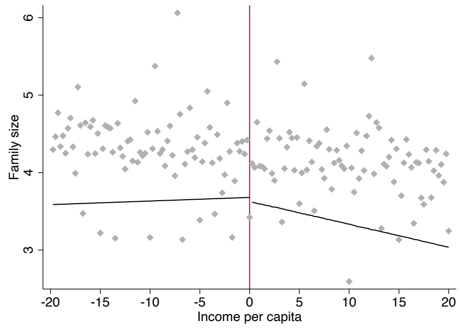
FIGURE B3: COVARIATES AROUND THE EXTREME POVERTY LINE



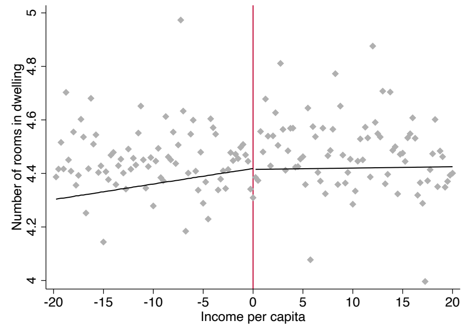
Note: The figure displays averages of six pre-determined variables by income per capita bins of R$0.25 around the Extreme Poverty line. In each panel, it also displays the linear fit on each side of the cutoffs from estimating the regression discontinuity specification in equation (7). Income per capita is based on information from the August 2010 snapshot of Cadastro Unico. The pre-determined variables are (a) family size, (b) number of rooms in the dwelling, (c) whether the family lives in a rural area, (d) whether the family receives any retirement or unemployment benefit, (e) the share of females in the household, and (f) whether the household head completed high school. The red vertical line in each panel indicates the level of the relevant cutoff (income per capita is normalized to the cutoff in each panel).

FIGURE B4: COVARIATES AROUND THE POVERTY LINE

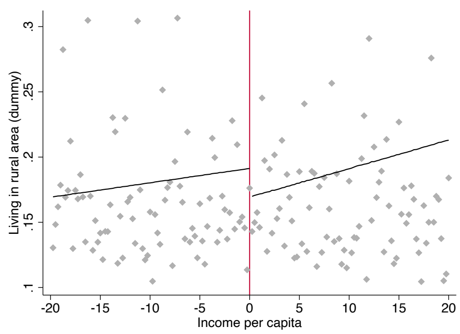
(a) Family size



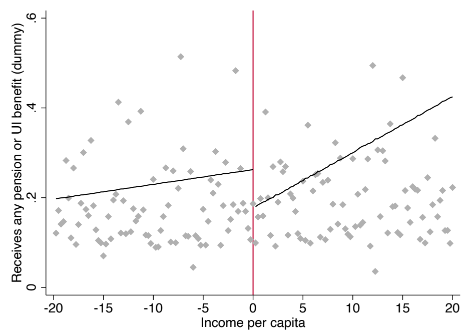
(b) Number of rooms in the dwelling



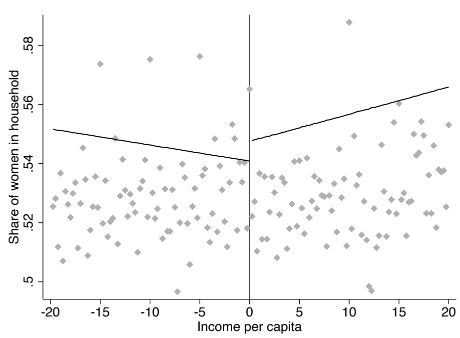
(c) Living in rural area



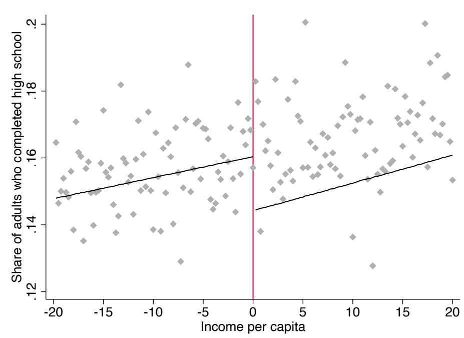
(d) Receives other government benefits



(e) Share of females in the household



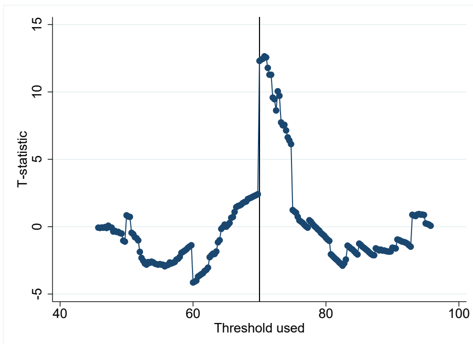
(f) Household head completed high school



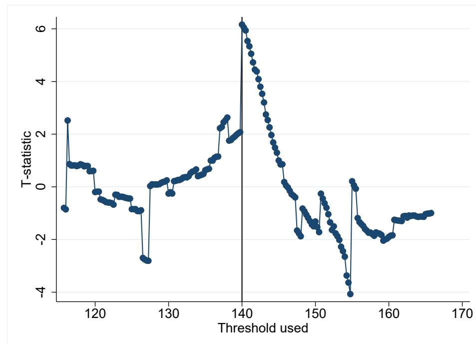
Note: The figure displays averages of six pre-determined variables by income per capita bins of R$0.25 around the Poverty line. In each panel, it also displays the linear fit on each side of the cutoffs from estimating the regression discontinuity specification in equation (7). Income per capita is based on information from the August 2010 snapshot of Cadastro Unico. The pre-determined variables are (a) family size, (b) number of rooms in the dwelling, (c) whether the family lives in a rural area, (d) whether the family receives any retirement or unemployment benefit, (e) the share of females in the household, and (f) whether the household head completed high school. The red vertical line in each panel indicates the level of the relevant cutoff (income per capita is normalized to the cutoff in each panel).

FIGURE B5: T-STATISTICS FOR RD ESTIMATES BY INCOME PER CAPITA LEVEL

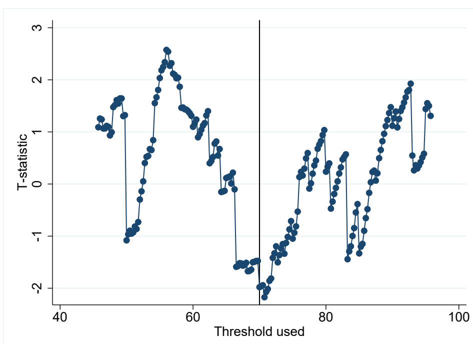
(a) PBF Benefits (EP)



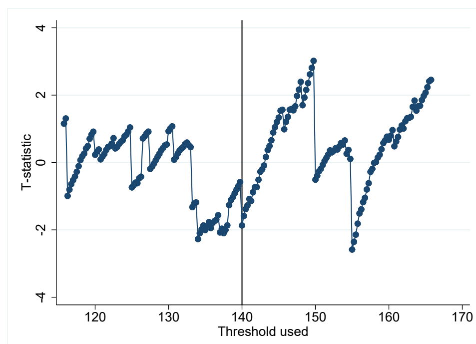
(b) PBF Benefits (P)



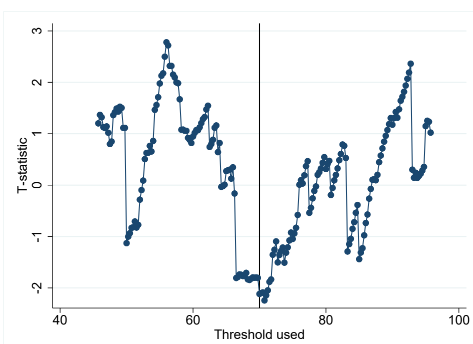
(c) Adult-months in formal employment (EP)



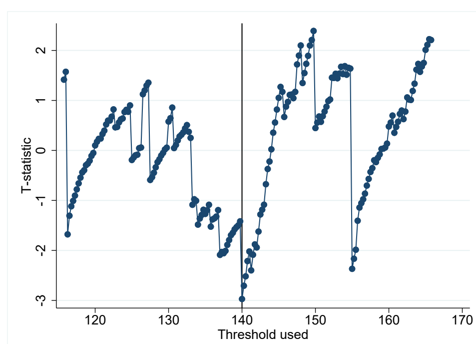
(d) Adult-months in formal employment (EP)



(e) Income from formal employment (EP)



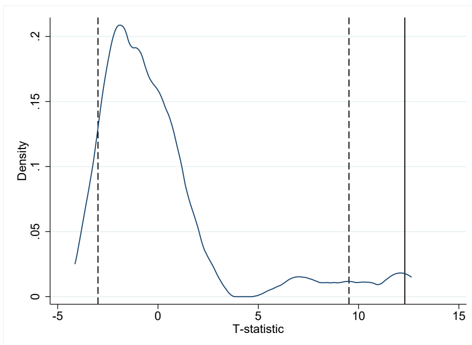
(f) Income from formal employment (EP)



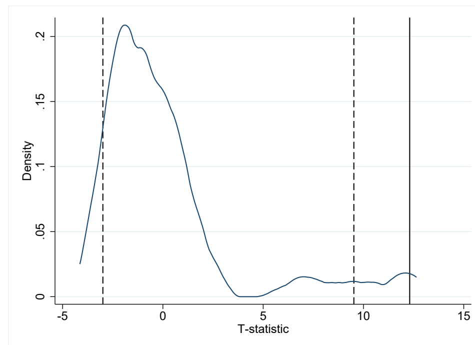
Note: The figure plots T-statistics for RD estimates obtained by estimating the same specification as for the results in panel B in Table 6 in the paper (with controls), but assuming that the cutoffs were located at each of the R$0.25 income per capita levels displayed in Figures 8a (around the Extreme Poverty line – EP) and 8b (around the Poverty line – P) in the paper. The solid lines indicate the location of the extreme poverty line (panels a, c, and e) and of the poverty line (panels b, d, and f).

FIGURE B6: DISTRIBUTION OF T-STATISTICS FOR RD ESTIMATES ACROSS INCOME PER CAPITA LEVELS

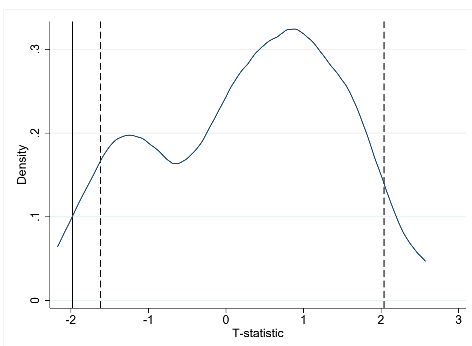
(a) PBF Benefits (EP)



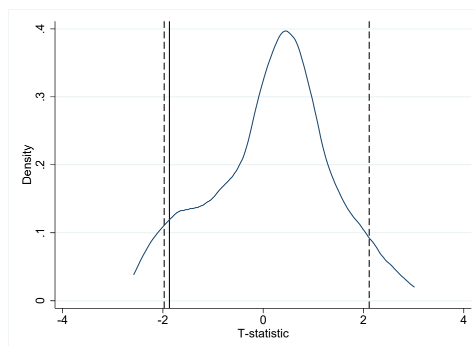
(b) PBF Benefits (P)



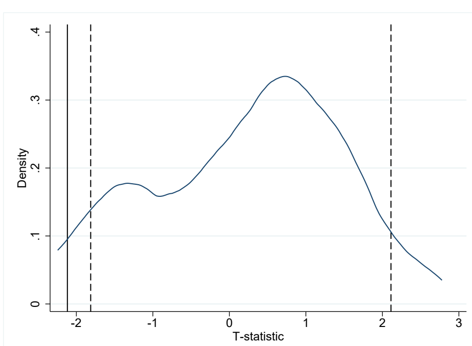
(c) Adult-months in formal employment (EP)



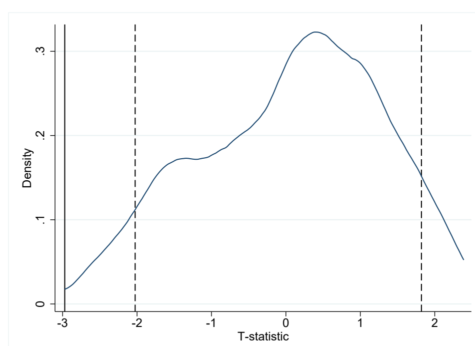
(d) Adult-months in formal employment (P)



(e) Income from formal employment (EP)



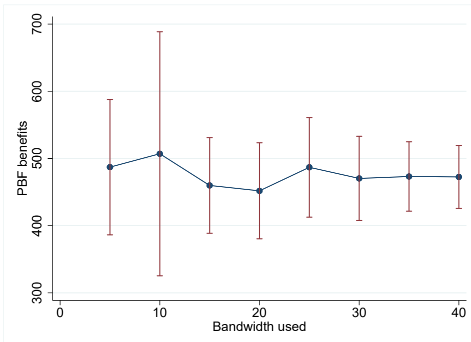
(f) Income from formal employment (P)



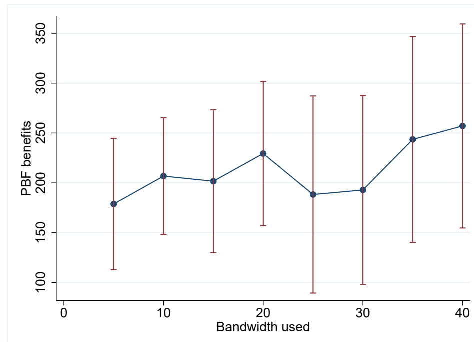
Note: Each panel in the figure displays the distribution of the T-statistics in the corresponding panel of Figure B5. The dashed lines indicate the 5th and the 95th percentiles of the distribution. The solid lines highlight the value of the T-statistic at the extreme poverty line (panels a, c, and e) and at the poverty line (panels b, d, and f).

FIGURE B7: ROBUSTNESS OF RD ESTIMATES WITH RESPECT TO THE BANDWIDTH SIZE

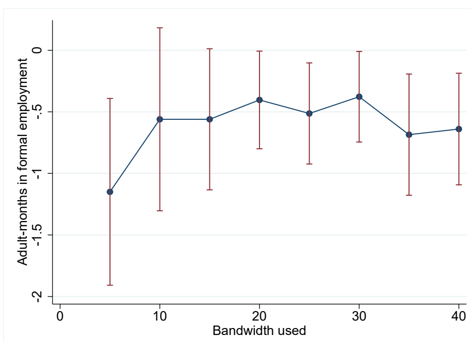
(a) PBF Benefits (EP)



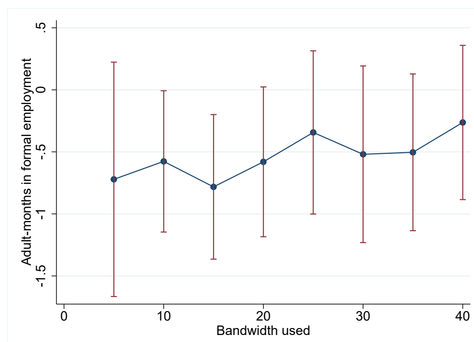
(b) PBF Benefits (P)



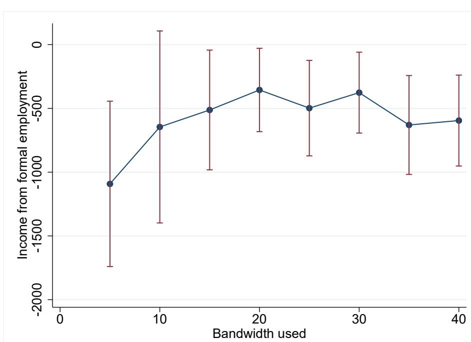
(c) Adult-months in formal employment (EP)



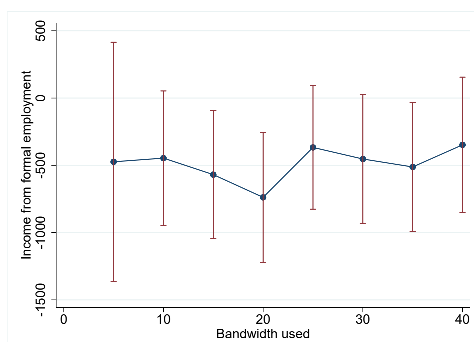
(d) Adult-months in formal employment (P)



(e) Income from formal employment (EP)



(f) Income from formal employment (P)



Note: The figure displays RD estimates obtained by estimating the same specification as for the results in panel B in Table 6 in the paper (with controls), but using different bandwidth sizes. Panels (a), (c), and (e) present RD estimates for the extreme poverty (EP) line. Panels (b), (d), and (f) present RD estimates for the poverty (EP) line.

References

Cattaneo, M. D., Jansson, M., and Ma, X. (2018). Manipulation testing based on density discontinuity. *The Stata Journal*, 18(1):234–261.

Cattaneo, M. D., Jansson, M., and Ma, X. (2020). Simple local polynomial density estimators. *Journal of the American Statistical Association*, 115(531):1449–1455.