Cash Transfers and the Local Economy: Evidence from Brazil *

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Cash transfers have been adopted worldwide and credited with significant reductions in poverty. However, their broader economic effects remain debated –especially when eligibility rules, such as means-testing, create unintended incentives to become or remain eligible. This paper examines the impact of a flagship means-tested program – *Bolsa Família* in Brazil – on local economies. Using variation across municipalities in the size of a reform that considerably raised the number of beneficiaries, we find that the expansion of the program increased local economic activity, including in the formal sector. The results suggest that the cash transfers increased local demand, despite behavioral responses to eligibility rules. We show that these economy-wide effects – by increasing tax revenues and potentially mitigating pre-existing distortions in the economy – can substantially increase the Marginal Value of Public Funds of the reform, raising it above the benchmark value of a hypothetical non-distortionary transfer.

Cash transfers have become a pervasive tool of social assistance in the developing world. It is well established that they reduce poverty and improve the lives of beneficiaries, but we still know relatively little about their economy-wide effects (Niehaus and Suri 2025). Fears of "lazy welfare recipients" are generally unwarranted (Banerjee et al.

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2017). Yet, the targeting of some of these programs – such as the use of means-testing – can distort beneficiaries' labor supply decisions (Bergolo and Cruces 2021), fueling concerns that these programs hurt the economy.[1] At the same time, there is evidence that cash transfers can increase local economic activity through demand effects (Egger et al. 2022). In practice, these effects can coexist in the case of means-tested cash transfers, but there is limited evidence on their net aggregate impact to inform the welfare implications of these policies.[2]

This paper addresses this gap by studying the aggregate effects of a flagship means-tested cash transfer program – Brazil's *Programa Bolsa Família* (PBF) – on local economies. We estimate the impacts of a 2009 reform that raised the number of beneficiaries by 17% (or almost two million families) during a period of steady economic growth in Brazil. Exploiting quasi-experimental variation across municipalities in the size of the reform, we find that the 2009 PBF expansion increased local economic activity, including in the formal sector, thereby raising tax revenues. The results suggest that the cash transfers increased local demand, and these effects arise despite evidence that some households altered their behavior to become or remain eligible for the means-tested benefits.

We then highlight the implications of our findings for the welfare effect of the 2009 PBF expansion by computing its Marginal Value of Public Funds (MVPF), which is the ratio of the willingness-to-pay (WTP) for the policy to its net cost to the government (Finkelstein and Hendren 2020).[3] The MVPF of a non-distortionary cash transfer would be equal to 1, as benefits would be valued at their cost. With means-testing, some households may adjust their behavior to qualify for benefits – and thus incur costs – reducing their WTP below the value of the transfer. Means-testing may further reduce the MVPF by increasing the net cost of the policy, if it discourages beneficiaries' formal employment, leading to losses in tax revenues. However, if cash transfers raise local demand, particularly in the formal sector, tax revenue may increase instead. An increase in economic activity could also raise the WTP for the policy, but the link between output and welfare must be examined carefully when expanding output involves opportunity costs. Specifically, an increase in economic activity will only be welfare improving *if* the level of economic activity was inefficiently low to begin with; otherwise, it would not imply any first-order effect on the WTP (Sims and Wolff, 2018). Considering both plausible sources of pre-existing dis-

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

[2] This debate is particularly heated in Brazil: right-leaning politicians emphasize the former effect and left-leaning politicians the latter (and the poverty reduction). We provide illustrative quotes in Online Appendix A.

[3] The MVPF informs welfare analysis by indicating how much the government must value spending on a policy in order for the policy to improve welfare.

tortions and the impact on tax revenues, we show that accounting for the economy-wide effects of the 2009 PBF expansion raises its MVPF from below to above the benchmark value of 1.

A key feature of PBF for our empirical strategy is that there is a national cap on the program's size, which means that many families who meet the per capita income eligibility criteria in Brazil's registry for social programs – *Cadastro Único* – do not receive benefits. The cap is revised every three years and the 2009 revision led to a large expansion of the program. The methodology used to allocate slots across municipalities also changed at the time, leading to substantive and persistent differences in the additional number of beneficiaries across municipalities. Our Difference-in-Differences strategy exploits the timing of the expansion, and compares municipalities in the bottom 50% and top 50% of a measure of treatment intensity that uses variation in the gap between the actual expansion and that which would have occurred under the older methodology. We find that the number of beneficiaries remained stable in the first group, which is thus our *control* group, but it rose sharply in 2009 for municipalities in the top-50% group, our *treatment* group. With this design, we then examine how the 2009 PBF expansion affected local economic activity.

We begin by studying impacts on formal employment. This is a particularly interesting outcome to study. First, the impacts of the policy on the formal sector are directly relevant for its MVPF. Tax revenues depend on the size of the formal sector; it is also the sector in which the level of economic activity is widely believed to be inefficiently low in the Latin American context (Levy, 2010), because of taxation, stricter regulations, and higher barriers to entry (Ulyssea, 2020; Dix-Carneiro et al., 2025). Second, and relatedly, expanding formal employment is a policy focus in many countries, as it is more likely to provide workers with social security coverage and better working conditions, and it is associated with higher productivity. Third, formal employment is the margin of economic activity most likely to be adversely affected by the means testing of PBF, since formal earnings are more-easily verifiable. Moreover, the granularity of the Brazilian linked employee-employer data allows us to unpack the effects of the 2009 PBF expansion and its underlying mechanisms.

We find that the 2009 PBF expansion *increased* the number of private-sector formal jobs in treated municipalities by 2%-3% on average.[4] The result is consistent with larger PBF outlays raising local demand, rather than reflecting an increase in labor supply among new beneficiaries who became less financially constrained to search for jobs (Baird et al.,

[4]This result is robust across specifications, treatment definitions, and weighting schemes. It is also robust to using the Synthetic Difference-in-Differences estimator (Arkhangelsky et al. 2021) and to aggregating the analysis at the level of geographic clusters that could share economic spillovers, indicating that our findings are not due to reallocation effects across connected municipalities (Chodorow-Reich 2019). Moreover, we find no change in public-sector employment, so the 2009 PBF expansion did not increase the government workforce.

2018). Because we can match workers to the administrative PBF data at the individual level, we show that about two-thirds of the increase in formal employment is driven by workers who were never beneficiaries of PBF during the period of analysis. The literature also highlights that local multipliers will be stronger if the increase in spending is concentrated on locally-produced goods and services and does not lead to substantial increases in wages and prices (Nakamura and Steinsson 2014). We find that the impact is concentrated in non-tradable industries and we find no change in wages holding workers' composition fixed.[5] The average wage in fact decreases, but this is entirely due to a composition effect: the increase in formal employment is driven by low-wage workers.[6]

Next, we show that the impact of the 2009 PBF expansion extends to the overall economy. We find that it raised local GDP (as measured in disaggregated national accounts) in treated municipalities.[7] The effect is driven by increases in the value added of firms in non-tradable industries and in the revenue from all taxes levied on the sale of goods and services in the municipality, which are directly affected by consumer demand overall and in the formal sector, respectively. All the results documented so far could reflect a formalization of the economy without any increase in overall employment, given the higher productivity of formal jobs. However, we also provide survey evidence that the impacts on total employment and labor force participation track the impact on formal-sector employment.

To assess magnitudes, we estimate a cost per formal job of US$9,799 in PBF payments per year (or 3.67 times the yearly minimum wage). This is comparable to the findings of Corbi et al. (2019) on the impact of increases in municipal budgets on formal employment in Brazil. We also obtain estimates for the output multiplier of the 2009 PBF expansion. For instance, the estimated increase in formal employment implies a multiplier between 1.46 and 1.95.[8] This is in line with Pennings (2021) for permanent transfers in the U.S., but

[5]There is no available data on the local price of non-tradables with extensive geographic coverage, but we find no changes in the local price of motor-vehicle fuels and cooking gas, which are available during our sample period. Overall, the more recent papers on the aggregate effects of cash transfers do not find relevant price effects, except in remote areas (Niehaus and Suri 2025; Filmer et al. 2021). Importantly, we document increases in measures of economic activity - e.g., the number of formal jobs - that are unaffected by potential price effects.

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

[7]We also provide evidence that the 2009 PBF expansion affected various dimensions of economic activity: banking activity, electricity use by households and firms in non-tradable industries, and vehicle registration.

[8]For these estimates, we use an approach from the literature inferring the impact on output from that on employment, which makes strong but transparent assumptions (Chodorow-Reich, 2019). As we discuss in Section 4.2, using the measure of local GDP, we obtain a very large multiplier of around 5. While some components of GDP – such as tax revenues – can be measured directly, statistical agencies must always rely on various imputation methods to disaggregate value added at the local level, which can introduce (non-classical) measurement error. Obtaining a multiplier of around 5 using the previous approach would

lower than the estimate in Egger et al. (2022) for a large one-off transfer in rural Kenya.

This increase in economic activity occurred despite behavioral responses consistent with concerns that means-tested transfer programs incentivize families to adjust their behavior to qualify for benefits. Specifically, we find that about 25% of the increase in benefit outlays caused by the 2009 PBF expansion went to families who altered their behavior to be registered with per capita income below the eligibility thresholds in *Cadastro Único*.[9]

We end by computing the MVPF of the 2009 PBF expansion. Even if we obtained a multiplier of the same magnitude as in Egger et al. (2022), the MVPF would differ between the two settings. First, PBF is means-tested and the behavioral responses that we estimate would imply an MVPF of about 0.75, without considering economy-wide effects. The MVPF of the transfer in Egger et al. (2022) would be equal to 1 under the same assumption. Second, unlike Egger et al. (2022), we find that the increase in local demand led to higher tax revenue, as we study a more formal economy. This effect reduces the net cost of the 2009 PBF expansion and, alone, more than doubles its MVPF.[10]

Third, the WTP for a given impact on economic activity would also be smaller in our context. Intuitively, an increase in economic activity improves welfare only if consumers value the additional goods and services above the opportunity cost of producing them. The output gains in Egger et al. (2022) are driven by pure productivity gains, so that they should be valued at \$1 per \$1 in welfare terms. By contrast, the increase in economic activity following the 2009 PBF expansion entails an opportunity cost, as it involves greater use of factors of production such as labor. Yet, the WTP remains positive if the increase occurred in markets where pre-existing distortions created a "wedge" between consumer value and production cost – that is, where economic activity was inefficiently low to begin with.

Such wedges may arise in both output and input markets, and the empirical evidence points to two relevant sources of distortion in our setting, which we use to illustrate the importance of accounting for the aggregate effects of the 2009 PBF expansion. First, a growing literature highlights that producers often sell their output at a markup over

require considerable increases in informal employment or hourly productivity, suggesting that the estimate based on local GDP could be biased upward.

[9]This result is in line with the evidence in Bergstrom et al. (2022). It is challenging to quantify the impact of the 2009 PBF expansion on the margins of behavioral responses consistent with this result: increases in registration, under-reporting of income, and decreases in labor supply, particularly in the formal sector. Yet, we provide micro-evidence of negative formal labor supply responses to PBF benefit eligibility in Online Appendix D. Our aggregate results on formal employment may thus underestimate the strength of the local demand effects of PBF.

[10]The measure of tax revenue raised in each municipality that we use includes all taxes on goods and services. Including tax revenue from increases in labor income would not affect our results: the positive impact on formal employment is concentrated among low-wage workers who are exempt from personal income taxation in Brazil.

marginal cost, including in Brazil (De Loecker and Eeckhout, 2018). Second, producers may pay for inputs above their opportunity cost. Specifically, our finding of an increase in labor demand for low-wage workers with no increase in wages suggests excess supply in the labor market – consistent with the strong bunching at the minimum wage during our study period (Engbom and Moser, 2022). Combining our results with estimates from the literature to quantify these wedges, we find that the MVPF of the 2009 PBF expansion rises to 2.5.

This paper contributes to an extensive literature on cash transfers in developing countries, which focuses on the direct impacts on beneficiaries rather than their broader effects on the economy (Niehaus and Suri 2025). Our main contribution is threefold.

First, we show that a means-tested cash transfer can increase local economic activity by leveraging the expansion of a nationwide government program that has been running for 20 years, and we highlight the welfare implications of these economy-wide effects.[11] Our findings complement the evidence in Egger et al. (2022), who estimate a large local multiplier from a one-time NGO transfer amounting to 15% of GDP in treatment villages. Evidence from rural settings may not apply to urban populations, which are increasingly covered by cash transfer programs in developing countries and make up the majority of PBF beneficiaries. Moreover, at scale, cash transfers are run by governments, are typically persistent, and involve fiscal shocks that are much smaller than those studied in Egger et al. (2022).[12] The use of means testing for targeting social transfers – with its potential adverse effects on labor supply – is also bound to expand around the world, as countries develop and income becomes more verifiable across the income distribution (Jensen 2022). The policy variation that we study is, therefore, particularly relevant for informing policy debates.[13]

Second, our study complements the existing evidence tracing the effects of cash transfers on non-beneficiaries.[14] For instance, Angelucci and De Giorgi (2009) document consumption spillovers of the *Progresa* program in Mexican villages, which operate through the insurance and credit markets, with no increase in local employment or output. Egger et al. (2022) do find income spillovers on non-beneficiaries consistent with a demand mechanism, but the increase in output is not accompanied by any increase in employment. Our evidence of positive employment effects on non-beneficiaries is more in line

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

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

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

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

with the literature on the multiplier effects of demand shocks in richer countries (e.g., Michaillat and Saez, 2015). The paper also contributes to the literature on the aggregate effects of social policies more broadly. For developing countries, the literature has documented labor market-wide effects of public works employment policies (Imbert and Papp 2015; Muralidharan et al. 2023; Franklin et al. 2024), graduation programs (Bandiera et al. 2017), and subsidized health insurance for informal workers (Bosch and Campos-Vazquez 2014).[15]

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

1 Institutional background and data

1.1 *Programa Bolsa Família (PBF)*

PBF is Brazil's main social assistance program, targeting low-income families nationwide, and it has been an influential model for cash transfer policy worldwide. Created in 2004, it consolidated and expanded existing programs. By 2012, it reached about one-quarter of the population with a budget of 0.6% of GDP. Targeting is based on families' per capita income (*pci*). Families below an extreme poverty threshold are eligible for an unconditional *Basic benefit* and *Variable benefits* per child, conditional on school attendance and health checks. Those above the extreme poverty threshold but below a higher poverty threshold are eligible only for the conditional variable benefits. Benefits are paid monthly from the federal budget, typically into a bank account at *Caixa*, the main state-owned bank.

The relevant *pci* definition comes from *Cadastro Único*, a continuously updated federal registry created alongside PBF. It serves as the basis for multiple social programs and aims to include the universe of families with *pci* below one-half of the minimum wage (R$255 in 2010), which is much higher than the two poverty thresholds for PBF eligibility (R$70

[15]These effects operate through different channels. Public works and graduation programs raise wages for non-beneficiaries who remain in the labor market. Providing an in-kind transfer that is not readily tradable to informal workers, creates disincentives to operate in the formal economy, but is less likely to generate large demand effects.

and R$140 in 2010).[16] The registry is based on a survey that asks families about income and assets, among other characteristics. Families can apply to their municipality to take the survey, or municipalities can actively identify and survey poor families – the main channel of registration initially. Once registered, families must update their information when circumstances change, such as changes in income or family size; they must provide a complete update at least every two years for their registration to remain valid.

The *pci* in *Cadastro Único* is based on self-reported information, which leaves scope for discrepancies, but several constraints limit misreporting. Income questions are placed at the end of the survey so that interviewers can gauge responses against earlier questions on assets and social demographics, and the law recommends conducting interviews at home to facilitate verification. Audits may follow citizens' complaints or red flags arising from cross-checking the information reported with data from formal employment and social security records. This enforcement process is likely constrained by administrative capacity to cross-check information systematically and follow up on each case.[17] Nonetheless, means-testing creates incentives for families to remain poor or appear poor to the authorities.

A key feature of PBF for the empirical analysis is that eligible families may not become beneficiaries due to program size constraints. First, there is a national cap on the number of beneficiaries, revised every three years. We focus on the 2009 revision, which led to PBF's largest expansion. Second, the national cap is divided into municipal quotas that do not serve as strict quotas but determine the allocation of slots across municipalities. Slots that are not yet assigned to a family, or that reopen whenever a family leaves PBF, are allocated to municipalities based on a priority ratio – the number of beneficiaries divided by the quota – and on unmet demand, defined as the number of eligible families not receiving benefits (MDS 2008). Thus, eligible families can only become beneficiaries if there are available slots assigned to their municipality. Section 2 details how municipal quotas were computed and how the methodology changed with the 2009 PBF expansion, which generated large differences in the additional number of beneficiaries across municipalities.

1.2 Data

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

A. *Cadastro Único*. We use snapshots of *Cadastro Único* in December 2008 and August 2010. They include both family-level information (e.g., per capita income, family composition, geographical location) and individual-level information (e.g., age, education).

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

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

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

C. Formal employment records (*RAIS*). We use the Brazilian linked employee-employer dataset, which covers all formal employment spells in each year. For each worker, the data include information on municipality, industry, education, wage, gender, race, as well as hiring and separation dates. *RAIS* allows us to calculate formal labor market outcomes at the municipal and individual levels, for each month in our period of analysis.[18]

We can link individuals across these administrative datasets using a unique ID number. This allows us to shed light on the mechanisms through which PBF affects the local economy: we can distinguish between changes in formal employment driven by PBF beneficiaries and spillover effects on individuals who never participated in the program. It also allows us to highlight some of the characteristics of PBF families (see Online Appendix Table B1). For instance, the August 2010 snapshot of *Cadastro Único* shows that they mostly live in urban areas (70%), that they have low high-school completion rates (12%), and that their average *pci* falls below the extreme poverty line (R$54). It also shows that *Cadastro Único* includes many families that are not eligible for PBF (16%), and that some families are eligible but are not beneficiaries (21% for extreme poor families and 46% for those with *pci* between the extreme poverty line and the poverty line). The payment sheet data highlight that the average monthly benefit of PBF families (R$95) was substantial compared to their average monthly income (R$206). Additionally, the *RAIS* data reveal that about 35% of these families had at least one adult in formal employment over the following 12 months.

D. Brazilian Census Bureau (*IBGE*) data. The analyses presented in the paper also rely on various surveys and datasets from *IBGE*. We use microdata from the 2000 Brazilian census to compute poverty rates, unemployment rates, and formality rates for each municipality. We use microdata from the 2010 Brazilian census and from annual household surveys (*PNAD*) to study labor market outcomes beyond formal employment. Moreover, we use municipal data on estimated population growth and GDP, which is our other main measure of aggregate economic activity besides (formal) employment.[19] GDP is

[18]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.

[19]In Online Appendix C, we use other sources of administrative data at the municipal level to document changes in various dimensions of local economic activity over time: (i) data on total bank deposits (current accounts and savings) and total credits and loans reported by every bank branch in the country to the Brazilian Central Bank; (ii) data on electricity consumption for residential and commercial customers of several electricity distributors; and (iii) data on the fleet of vehicles registered with the Department of Motor Vehicles (DETRAN).

computed using the production approach by adding the value added of all industries and the revenue from all taxes levied on goods and services. While tax revenues are measured directly (by summing the revenue raised in the municipality from value added taxes, excise taxes, other sales taxes, import taxes, and taxes on financial transactions), the value added estimates are the result of extensive efforts by *IBGE* to produce estimates at the municipal level consistent with national accounts data at higher levels of aggregation. *IBGE* strives to capture the entirety of economic activity in its measure of value added, including the informal sector.[20] Finally, we use microdata from the 2010 Annual Surveys of Trade (*PAC*) and Services (*PAS*) to compute an average mark-up in non-tradable industries and quantify a relevant pre-existing distortion in the Brazilian output market that enters the MVPF calculations.

2 Empirical strategy

2.1 The 2009 PBF expansion and the allocation of municipal quotas

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

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

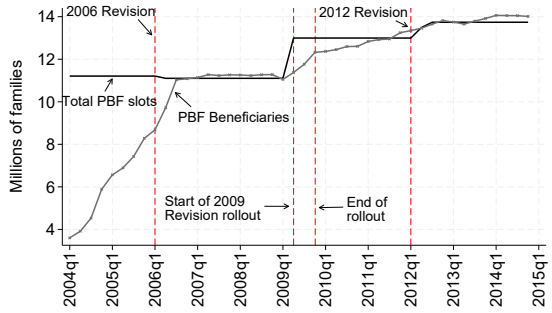
The cap remained essentially binding until the 2009 revision. The 17% increase in the

[20] Notably, the 2007 revision of the national accounts system included a host of new data to better capture the value added of the informal economy and of non-profit economic activities. This is achieved by using, among other sources, household surveys, census data, data from the agricultural census, and data from various administrative systems including data from regulatory agencies that oversee utilities such as water, telecommunications, electricity, oil and gas. For instance, the output of industries that may be particularly informal such as construction is gauged based on the consumption of typical inputs, like cement. According to the national accounts system, the informal economy accounts for about 10% of GDP (Hallak Neto et al. 2012).

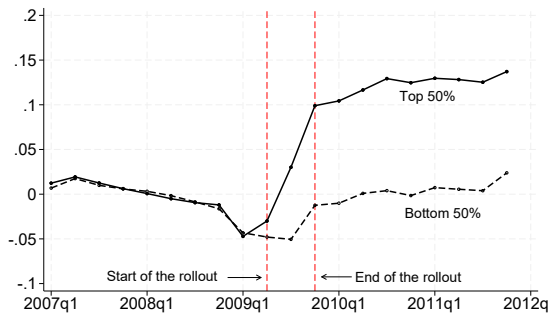
[21] *IBGE* used poverty thresholds corresponding to R$45 and R$90 (i.e., one quarter and one half of the minimum wage) per capita to define extreme poverty and poverty, respectively (MDS, 2009b).

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

(A) PBF SLOTS AND BENEFICIARIES AT THE NATIONAL LEVEL



(B) RELATIVE CHANGES IN PBF BENEFICIARIES



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

number of slots at that time was not driven by deteriorating poverty rates. Brazil's economy grew steadily in the 2000s, and the number of poor families computed by *IBGE* in 2009 – using the 2006 *PNAD* survey – was lower than previous estimates. However, this number was adjusted upward by a factor of 1.18 following the work of Soares (2009) showing that, due to income volatility, more families regularly found themselves in poverty than the number of families observed below a given poverty line at any single time (MDS 2009b).

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

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

pansion.[22]

The government followed a similar strategy to allocate the national number of slots into municipal quotas when it set it in 2003 and when it first revised it in 2006. The number of poor families in each state – estimated from *PNAD* surveys (see above) – was apportioned across municipalities within the state using municipal poverty measures based on the 2000 census, the most recent source of data representative at the municipal level. Formally, the 2003 quotas were computed as: $Quota_{ms}^{2003} = (Poor_{ms}^{2000} / \sum_{k \in s} Poor_{ks}^{2000}) \cdot Poor_{s}^{2001}$, where $Poor_{ms}^{2000}$ is the number of poor families in municipality m in state s from the 2000 census, and $Poor_{s}^{2001}$ is the number of poor families in each state from the 2001 *PNAD* survey. The quotas were revised in 2006 by first updating the number of poor families in each state using the 2004 *PNAD* survey and then adjusting the municipal poverty measures from the 2000 census by an estimate of population growth between 2000 and 2003 (MDS 2012). Formally, the 2006 quotas were thus calculated as: $Quota_{ms}^{2006} = (\widetilde{Poor}_{ms}^{2004} / \sum_{k \in s} \widetilde{Poor}_{ms}^{2004}) \cdot Poor_{s}^{2004}$, where $\widetilde{Poor}_{ms}^{2004} = n_{ms}^{[2000,2003]} \cdot Poor_{ms}^{2000}$.

IBGE adopted a new methodology in 2009 (MDS 2012). It used a statistical method developed by World Bank researchers to generate measures of poverty at low levels of spatial aggregation, when detailed household surveys are not representative of these areas (Elbers et al. 2003). In a nutshell, *IBGE* first estimated a prediction model for *pci* at the family level in the 2006 *PNAD* survey, using variables also available in the 2000 census and municipal variables from other data sources (e.g., on local education, local GDP). Next, the model was used to predict *pci* in 2006 for the families in the 2000 census, thus providing an estimate of the number of poor families in each municipality in 2006, $\widehat{Poor}_{ms}^{2006}$ (IBGE 2009). The 2009 quotas were then calculated as: $Quota_{ms}^{2009} = (\widehat{Poor}_{ms}^{2006} / \sum_{k \in s} \widehat{Poor}_{ms}^{2006}) \cdot 1.18 \cdot Poor_{s}^{2006}$, where the number of poor families in each state based on the 2006 *PNAD* survey is adjusted upward by 1.18 as explained earlier. *IBGE* returned to using census data to compute the municipal poverty measure for the 2012 revision (using the 2010 census). This alternative methodology for allocating slots across municipalities was thus only used in 2009.

2.2 Research design

A. Quasi-experimental variation across municipalities. Figure 1b shows that the change in methodology at the time of the 2009 PBF expansion provides us with substantial quasi-experimental variation in the number of PBF beneficiaries across municipalities.

To isolate this source of variation, we compute counterfactual 2009 quotas as if the methodology used to update municipal poverty measures in 2006 had been maintained

[22]The calculations underlying the allocation of municipal quotas are conducted by *IBGE* using data from the Census and other household surveys, so there is no clear room for political manipulation in this process.

for the 2009 revision: $\widetilde{Quota}_{ms}^{2009} = (\widetilde{Poor}_{ms}^{2006} / \sum_{k \in s} \widetilde{Poor}_{ms}^{2006}) \cdot 1.18 \cdot Poor_{s}^{2006}$, where $\widetilde{Poor}_{ms}^{2006} = Poor_{ms}^{2000} \cdot n_{ms}^{[2000,2006]}$, and $n_{ms}^{[2000,2006]}$ is an estimate of municipal population growth between 2000 and 2006 from *IBGE*. We then calculate the difference between the 2009 quota and this counterfactual, relative to the municipal population in 2006: $\Delta Quota_{ms}^{2009} = (Quota_{ms}^{2009} - \widetilde{Quota}_{ms}^{2009}) / Pop_{ms}^{2006}$. This measure of treatment intensity captures the *relative* change in quota in 2009 due to the change in methodology.[23]

Figure 1b displays the average change in the number of beneficiaries in each quarter between 2007 and 2011, compared to before the 2009 PBF expansion, for municipalities in the top 50% and bottom 50% of the distribution of $\Delta Quota_{ms}^{2009}$.[24] The two groups shared the same trend until the first quarter of 2009. By contrast, the number of beneficiaries increased by more than 15% in the top-50% group during the roll-out of the PBF expansion, while it only returned to pre-2009 levels in the bottom-50% group.[25] Hereafter, we refer to these two groups of municipalities as the *treatment* and *control* groups, respectively.

B. Difference-in-Differences strategy. We exploit this source of quasi-experimental variation to estimate the impact of an increase in program size on local economies. We adopt a Difference-in-Differences (DD) strategy and focus on the period from January 2007 to December 2011 (before the 2012 revision). There were no changes in quotas between 2007 and 2009, and the data used to compute the 2009 quotas – including the municipal poverty measures – were measured before 2007. Thus, the period from 2007 to the first quarter of 2009 serves as a useful pre-treatment period to provide supporting evidence for the common-trend assumption underlying our research design, as we explain below.

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

$$y_{m,s,t} = \alpha_m + \phi_{t,s} + \sum_{t \neq t_0} \beta_t \cdot Treat_{m,s} + \sum_{k} \sum_{t \neq t_0} \gamma_{t,k} \cdot X_{m,s}^{k} + \varepsilon_{m,s,t}, \tag{1}$$

where $y_{m,s,t}$ is an outcome of interest at time t for municipality m in state s. Municipality fixed effects α_m control for time-invariant characteristics of municipalities. State-by-time fixed effects $\phi_{t,s}$ absorb any variation over time that is common across municipalities within a state – such as the increase in the sum of the municipal quotas within each state in 2009 from $Poor_{s}^{2004}$ to $1.18 \cdot Poor_{s}^{2006}$. The DD coefficients β_t capture any difference

[23] In this sense, our research design shares similarities with the re-centering procedure in Borusyak and Hull (2023) for the case of non-random exposure to exogenous shocks. Indeed, our measure of treatment intensity adjusts for municipalities' "expected treatment," had the government maintained the methodology used in the 2006 revision. As a result, by holding fixed the state-level number of poor families, our research design ensures the presence of both treatment and control municipalities within each state, for instance.

[24] The full distribution is presented in Online Appendix Figure B1.

[25] No beneficiary was forced to leave the program following the 2009 quota update, even in municipalities that experienced a reduction in their quota; these municipalities only became less likely to be allocated *new* slots.

between municipalities in the treatment and control groups at time t compared with the last period before the 2009 PBF expansion. The specification also allows for municipalities with different values of some predetermined variables $X^k_{m,s}$ to have different trends. We cluster the error term $\varepsilon_{m,s,t}$ by municipality, i.e., the unit of treatment assignment (Abadie et al. 2023).

For most of the analysis, we estimate specifications at the yearly level, but we will start by estimating specifications that exploit the higher frequency of some of our data to highlight the timing of the roll-out of the PBF expansion in 2009. We also use a specification in growth rates for many of our regressions following the macroeconomics literature on cross-region multipliers (Chodorow-Reich 2019). An advantage of this functional form is that it allows us to decompose the impact on an outcome into the impacts on specific subcomponents, e.g., the impact on formal employment or GDP into the impact in tradable and non-tradable industries, separately (see Section 3.4 for more details). It also allows us to obtain estimates scaling the impact of PBF on local economic activity per $1 of PBF outlays, which is helpful for discussing magnitudes and implications (see Section 4).

Our main analysis sample includes 5,076 out of the 5,570 municipalities in Brazil at the time. To balance the panel, we restrict attention to municipalities that have at least one PBF beneficiary in every month from 2007 to 2011. Additionally, we exclude outliers, as Brazilian municipalities had populations ranging from 828 to 11,016,703 inhabitants in 2006. Specifically, we exclude municipalities below the 1st percentile and above the 99th percentile of the 2006 population distribution, and we restrict attention to those that have at least five private-sector formal employees every month (i.e., above the 1st percentile).[26]

C. Identification assumption. Our research design, which aims to estimate the impact of expanding PBF, approximates a setup in which treated units are compared to a group of "stayers" that receive no treatment. Indeed, Figure 1b shows that the average number of beneficiaries increased sharply in the treatment group following the 2009 PBF expansion, but remained relatively stable in the control group. Under this approximation, the identification assumption boils down to that of a standard binary DD design: that the two groups of municipalities would not have experienced different outcome trends *in the absence of treatment* – i.e., the increase in program size induced by the 2009 PBF expansion. This can be supported by standard pre-trend tests, as the two groups of municipalities did not experience meaningful changes in average program size prior to the 2009 expansion. In this case, the DD estimates capture the Average Treatment effect on the Treated (ATT), i.e., on municipalities that experienced an increase in program size because of the 2009

[26]Our results are robust to including the very large municipalities. The restrictions on very small municipalities and minimum number of beneficiaries and formal employees help minimize noise, keep a balanced panel, and exclude municipalities that may have inaccurate data (e.g., implausibly low formal employment in some month).

expansion.

Two points on identification merit further discussion. First, while pre-trend tests provide valuable supportive evidence, they do not ensure that the common-trend assumption holds in later periods. Nevertheless, we believe that our research design makes this assumption quite plausible. The concern is that other shocks could affect the two groups differentially after treatment. Arguably, this concern would be more severe if treated and control units were very different, as they would be more likely to respond differently to potential common shocks and to be exposed to distinct idiosyncratic shocks. However, by focusing on the change in quotas induced by the change in methodology, we can exploit stark variation in program size across comparable groups of municipalities. This wouldn't be the case, for instance, if the 2009 revision had simply expanded the program where it was already larger.

This is shown in Table 1. Columns [1] and [2] first indicate that municipalities in the top 50% and bottom 50% of the distribution of baseline program size – the number of PBF beneficiaries per capita in 2008 – differed substantially. On average, municipalities in the top-50% group received about three times more PBF payments per capita, were less urban, and had lower GDP per capita, lower private-sector formal employment rates, and higher unemployment rates. By contrast, columns [3] and [4] show that our treatment and control groups were far more comparable. Baseline program size was more similar, albeit slightly larger in the control group, and both groups had similar GDP per capita, urban shares, formal employment rates, and unemployment rates. A key reason for these smaller differences is that, while program size is geographically concentrated – larger in the poorer areas of the North and Northeast of Brazil – our measure of treatment intensity is not.[27]

TABLE 1: DESCRIPTIVE STATISTICS AT THE MUNICIPAL LEVEL

	Program size in 2008		**Treatment intensity**	
	Top 50%	Bottom 50%	Top 50%	Bottom 50%
	[1]	[2]	[3]	[4]
PBF beneficiaries per capita (2008)	0.120	0.047	0.078	0.090
PBF payments per capita (2008, BRL)	131.8	42.9	82.2	92.5
PBF payments over GDP (2008)	0.029	0.004	0.015	0.017
GDP per capita (2008, BRL)	6,673	16,056	10,849	11,881
Formal employment rate (2000)	0.152	0.325	0.242	0.234
Unemployment rate (2000)	0.110	0.092	0.098	0.103
Urban municipality (IBGE)	0.469	0.686	0.573	0.582
Population (2006)	18,509	29,706	28,008	20,207
Quota 2006	2,122	1,231	1,727	1,626
Population growth 2000–2006	1.077	1.109	1.054	1.132
Counterfactual quota 2009	2,333	1,581	1,967	1,948
Quota 2009	2,261	1,617	2,240	1,637
Treatment intensity	-0.004	-0.001	0.015	-0.020
Number of municipalities	2,538	2,538	2,538	2,538

Notes: The table displays averages of variables measured prior to 2009. Columns [1] and [2] compare municipalities in the top and bottom 50% by the number of PBF beneficiaries per capita in 2008; columns [3] and [4] compare them by treatment intensity.

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

Yet, Table 1 highlights two differences that remain sizable between treatment and control municipalities. First, treatment municipalities were more populated on average and, therefore, had a larger municipal quota at baseline. Second, population grew, on average, faster in control municipalities from 2000 to 2006. Consequently, their quotas would have increased relatively more than those in treatment municipalities if the methodology used to update municipal quotas had not changed in 2009. In reality, the quotas increased by 30.2% on average in treatment municipalities, but by only 0.7% in control municipalities.

We account for these two differences in our preferred specification by including the log of the 2006 quota and the counterfactual change in quotas, $(\widetilde{Quota}_{ms}^{2009} - Quota_{ms}^{2006}) / Pop_{ms}^{2006}$, as predetermined variables $X_{m,s}^{k}$. Thus, we allow municipalities with different quotas at baseline, and those that would have been impacted differently by the 2009 revision without the change in methodology, to follow different trends. We also present three robustness checks to strengthen the plausibility of the common-trend assumption. We show that our results do not depend on including these controls and remain unchanged if we ensure that treated and control units are similar across the entire distribution of baseline program size. We also weaken our assumption of a parallel trend between the *same* two groups of municipalities *for all* outcome variables by presenting results using the Synthetic Difference-in-Differences (SDD) estimator (Arkhangelsky et al., 2021).[28]

The second point is that the control group is not strictly composed of stayers that received no treatment. Program size naturally varies from year to year, as households exit and enter the program, and the 2009 PBF expansion likely affected program size in some control municipalities. More precisely, our identification assumption is thus that outcomes would have followed parallel trends *had the treatment group experienced changes in program size similar to those in the control group*. A potential concern is that our DD estimates could be biased if the effect of the "treatment doses" received by control municipalities varies with characteristics that differ systematically between the two groups. For instance, the same increase in program size could have different effects in municipalities with higher baseline sizes (e.g., new beneficiaries may have different propensities to spend locally) or higher unemployment rates (e.g., demand effects may be stronger). However, our research design mitigates this concern: control and treatment municipalities are comparable along key dimensions of heterogeneity (and we further improve their comparability as described above), and the distribution of treatment doses in the control group is centered around zero. We also show that our findings are robust to alternative treatment defini-

[28] It assigns different weights to control units for each outcome "so that the average (pre-treatment) outcome for the treated units is approximately parallel to the weighted average of control units" (Arkhangelsky et al. 2021). In the next section, we also show robustness checks indicating that our DD estimates are not driven by spillover effects across connected municipalities, another standard condition for DD designs to capture treatment effects.

tions. Specifically, we compare municipalities in the top 25% of $\Delta Quota_{ms}^{2009}$ to those in the bottom 25% – a control group with a different distribution of treatment doses – and we use $\Delta Quota_{ms}^{2009}$ linearly, a design that relies on stronger assumptions about the correlation between potential treatment effects and treatment doses.

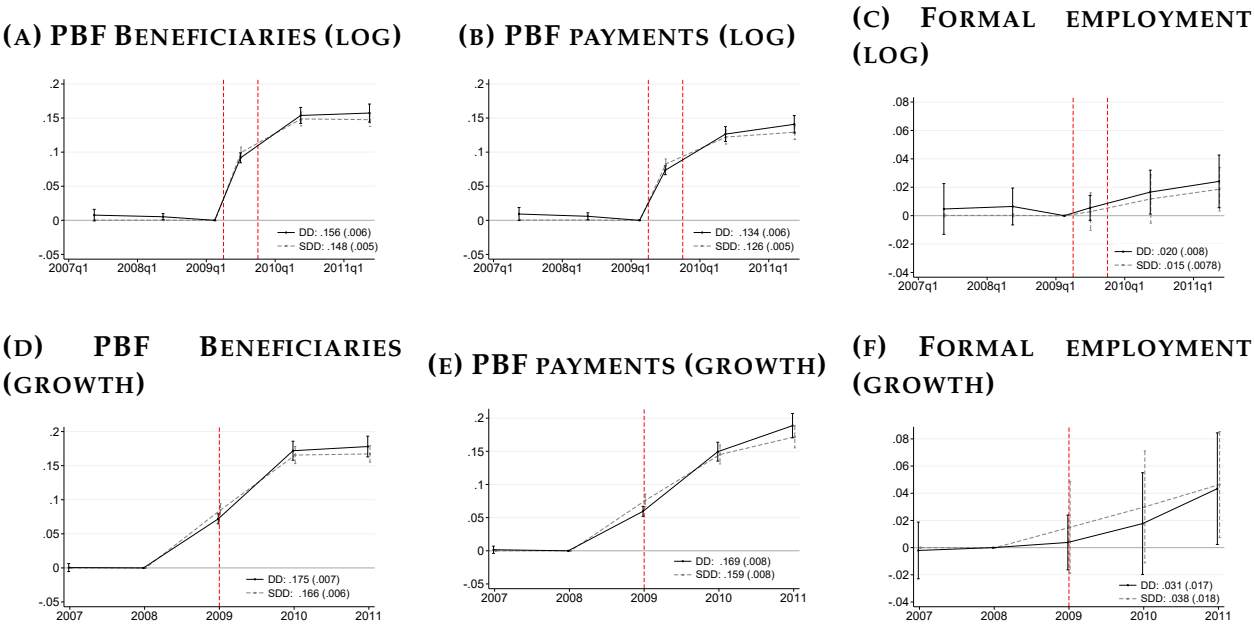
3 Empirical results

In this section, we use our DD strategy to estimate the impact of the 2009 PBF expansion on the number of beneficiaries and total PBF payments in treatment municipalities. We then examine the effects on local economic activity, beginning with formal employment. As discussed in the Introduction, this is a particularly important outcome to study. In addition, the granularity of the administrative data allows us to trace the economic impacts of the PBF expansion and shed light on underlying mechanisms. Next, we show that the effects on the overall local economy – GDP and its components (value-added and tax revenue), total employment – track those on formal employment. Finally, we provide evidence that the 2009 PBF expansion also induced behavioral responses consistent with concerns that means-tested cash transfers incentivize families to alter their behavior to qualify for benefits. We bring these results together to discuss welfare implications in the next section.

3.1 Impact on the number of PBF beneficiaries and total PBF payments

Figure 2a displays DD coefficients from estimating our preferred specification in equation (1) at the quarterly level for the log of the number of PBF beneficiaries. For exposition purposes, we display our quarterly estimates averaged over six time periods p, which are sufficient to trace the evolution of the outcome before the 2009 PBF expansion ($p = \{2007, 2008, 2009_{q1}\}$), during its roll-out ($p = \{2009_{q2-q4}\}$), and in the following years ($p = \{2010, 2011\}$). We also summarize the results by reporting the average of the 2010 and 2011 coefficients. Our estimates imply that treatment municipalities experienced a relative increase in the number of beneficiaries of about 0.15 log points. Figure 2d shows that the estimated impact reaches about 17% using a specification in growth rates at the yearly level. The impact appears during the roll-out of the expansion and remains stable between 2010 and 2011, consistent with the raw data in Figure 1b. Next, Figures 2b and 2e display results for total PBF payments, which closely mirror those for the number of beneficiaries (we present the raw patterns in Online Appendix Figure B3). Finally, dashed lines show that our results are robust to using the SDD estimator.

FIGURE 2: IMPACT ON PBF BENEFICIARIES, PBF PAYMENTS, AND PRIVATE-SECTOR FORMAL EMPLOYMENT



Notes: Panels (a)-(c) display DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the quarterly level for the log of these outcomes. For exposition purposes, we display our quarterly estimates averaged over six time periods p: before the 2009 PBF expansion ($p = \{2007, 2008, 2009_{q1}\}$), during its roll-out ($p = \{2009_{q2-q4}\}$), and in the following years ($p = \{2010, 2011\}$). We also report the average of the 2010 and 2011 coefficients in the lower-right corner of each graph (with standard errors in parentheses). Panels (d)-(f) display results using a specification in growth rates at the yearly level. The vertical lines indicate the start and end of the rollout of the 2009 PBF expansion (panels a-c) or the year of the reform (panels d-f).

3.2 Impact on private-sector formal employment

Figures 2c and 2f display results for the number of private-sector formal employees. The DD estimates imply that treatment and control municipalities shared a common trend prior to the PBF expansion. Formal employment continued to evolve similarly in both groups through 2009, but began to grow faster in treatment municipalities after the roll-out ended (this can be seen in the raw data in Online Appendix Figure B3). As a result, we estimate a relative gain in treatment municipalities that reached about 0.02 log point over 2010 and 2011 with the log specification (panel c), and about 3% with that in growth rates (panel f). Results are similar with the SDD estimator. The impact appears with a delay relative to the increase in PBF beneficiaries and payments, and is larger in 2011 than in 2010. This lag is consistent with both demand effects from the increase in resources spent in the local economy (e.g., firms may not expand labor demand until an increase in demand appears persistent) and increases in formal labor supply among new beneficiaries (e.g., job-search investments may take time to yield returns). We explore mechanisms below.

3.3 Robustness

Before turning to mechanisms, we emphasize that our main results in Figure 2 hold across a series of robustness checks, which are reported in Online Appendix Table B2.

First, the results are robust to considering alternative treatment definitions. When we keep only municipalities in the top 25% and bottom 25% of $\Delta Quota_{ms}^{2009}$, the DD coefficients increase for all three outcomes, consistent with the fact that we exploit a starker difference in treatment intensity. The estimated effects are similar when we define treatment and control groups based on the simple change in the 2009 quota compared to the 2006 quota (rather than compared to the counterfactual 2009 quota). Moreover, if we use $\Delta Quota_{ms}^{2009}$ linearly, the point estimates are consistent with our main results, considering a gap in average $\Delta Quota_{ms}^{2009}$ of .035 between treatment and control groups (see Table 1).

Moreover, the results are robust to allowing large municipalities to have a greater influence on our estimates, by including municipalities above the 99th percentile of the 2006 population distribution in the sample or by weighting municipalities by their 2006 population. They are also unchanged when we further ensure that our treatment and control groups are comparable in baseline program size, by re-weighting municipalities so that the 2008 distribution of PBF beneficiaries per capita is the same between the two groups. In addition, our results are unchanged if we exclude the controls X^k interacted with time fixed effects. Thus, the two differences between treatment and control municipalities highlighted in Section 2.2 do not appear to be correlated with differential trends in the outcomes.

Next, we analyze a different identification concern. The literature on cross-region multipliers highlights that the impact in the recipient locality may underestimate or overestimate the overall economic activity generated. On the one hand, increases in local demand may "leak" to other areas, including control municipalities. On the other hand, workers may move in from other areas, including control municipalities. We follow a common approach to address such concerns by replicating the analysis at a higher level of geographic aggregation (Chodorow-Reich 2019). We aggregate the data and compute our measure of treatment intensity at the level of the "Immediate Geographic Regions" (*RGI*), which are defined by *IBGE* as groups of municipalities around urban centers that may supply goods and services to municipalities within the region.[29] Point estimates are similar to those in Figure 2, although they are slightly higher for private-sector formal employment and slightly lower for PBF beneficiaries and PBF payments.[30] This is consistent with the finding that local multipliers tend to provide lower-bounds for aggregate multipliers because

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

[30]Online Appendix Figure B4 presents the supporting graphs. The results of the municipal-level regressions are also robust to clustering standard errors at the *RGI* level as shown in Online Appendix Table B2.

the effect of demand leakages typically dominates any migration effect (Chodorow-Reich 2019). Relatedly, the results of our municipal-level regressions are unchanged if we express the outcomes in per capita terms, using *IBGE* estimates of local population in each year.[31]

Finally, we present results for public-sector employment in Online Appendix Table B2. We find no evidence of a differential impact in treatment municipalities. Thus, the increase in transfers to low-income families is not associated with an expansion of the government workforce, as in the case of the transfers to local governments in Corbi et al. (2019).

3.4 Anatomy of the impact on formal employment and mechanisms

To shed light on mechanisms, we study the types of formal jobs that were created and who obtained them. Our results are consistent with the increase in cash-on-hand in treatment municipalities raising aggregate demand in the local economy, and expanding labor demand, rather than with an increase in (formal) labor supply among new beneficiaries who became less financially constrained to search for jobs (Baird et al., 2018).

The first piece of evidence is that the increase in formal employment is mostly driven by workers who were not directly impacted by the 2009 PBF expansion.[32] To show this, we select all workers who appeared in *RAIS* at any point over our sample period and we only keep those who were never part of a family that received PBF benefits during that period. We then estimate the contribution of these workers to the overall impact on private-sector formal employment. We use a DD specification in growth rates, where the dependent variable is the change in formal employment in a given subgroup, relative to overall formal employment in the reference period.[33] Figure 3a compares the effect among these "never-beneficiaries" to the overall effect presented in Figure 2f – they are both expressed as a percentage of total private-sector formal employment in 2008. The impact on never-beneficiaries follows the same pattern over time as the impact on all workers, reaching 2.96% by 2011, and thus accounting for about two thirds (63.2%) of the

[31] We find no evidence of differential population growth in treatment municipalities using the same specifications as in our main results. Summary estimates for 2010-2011 are .000 (.002) for both the log and the growth specifications. This is consistent with evidence that large shocks in Brazil did not trigger much migration. Dix-Carneiro and Kovak (2019) show that workers adversely affected by import competition following the 1990s trade liberalization did not migrate to less affected regions, and Costa et al. (2016) do not find net migration responses to changes in local labor demand triggered by an increase in exports associated with the China shock.

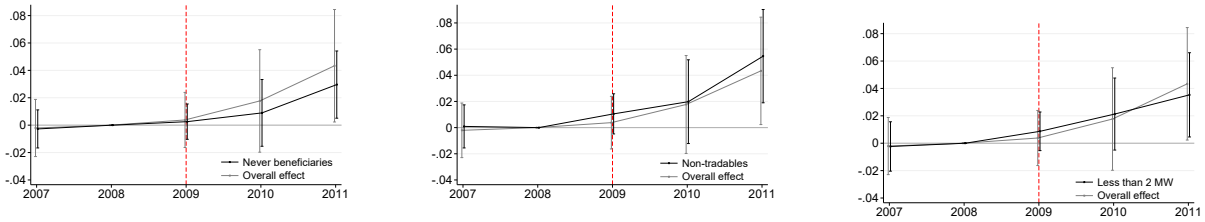
[32] The type of market externalities studied in, e.g., Lalive et al. (2015), through which decreases in job search among beneficiaries can increase job-finding among non-beneficiaries, could rationalize this specific result but not the overall increase in formal employment (considering all workers together).

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

overall impact.[34]

FIGURE 3: ANATOMY OF THE IMPACT ON FORMAL EMPLOYMENT

(A) NEVER BENEFICIARIES **(B) NON-TRADABLES** **(C) BELOW 2 MINIMUM WAGES**



Notes: The figure shows the contribution of specific groups of workers to the overall impact on private-sector formal employment. It displays DD coefficients (with their 95% confidence intervals) from specifications in growth rates at the yearly level, where the dependent variable is the change in private-sector formal employment in a specific group, relative to the overall private-sector formal employment in 2008 (black lines). For comparison, in each panel, we also reproduce the estimates for the overall impact on private-sector formal employment from Figure 2f (gray lines). We consider workers who were never part of a PBF family during the period of analysis (panel a), workers employed by firms in non-tradable industries (panel b), and workers earning less than twice the minimum wage (panel c). The vertical lines indicate the year of the PBF expansion.

The literature on local multipliers emphasizes that local demand effects will be stronger if the increase in resources is spent on locally produced goods and services (Nakamura and Steinsson, 2014). Using information on the industry code of each establishment, we thus estimate the contribution of non-tradable industries to the overall impact on private-sector formal employment in Figure 3b, following the same approach as in Figure 3a. Point estimates are similar to those considering all industries together. Therefore, non-tradable industries can account for the *entire* impact on formal employment.[35]

For demand effects to imply large local multipliers, it must also be that they do not lead to substantial increases in local prices and wages. We do not have access to detailed price data at the local level,[36] but we can study impacts on formal wages because *RAIS* includes information on the wage of every employee in December of each year. Figure 4a

[34]We provide complementary evidence in Online Appendix Figure B5a. Treatment municipalities also experienced a relative increase in private-sector formal employment among other workers who were arguably not directly impacted by the 2009 PBF expansion: those who were *already* PBF beneficiaries in 2007 and 2008.

[35]We follow Dix-Carneiro and Kovak (2019) and classify industries (CNAE codes) related to services and commerce as non-tradables and those related to agriculture and manufacturing as tradables. We cannot estimate impacts of household spending directly. The only available data on the spending pattern of households during our sample period come from the Consumer Expenditure Survey (*POF*) conducted by *IBGE* in 2008-2009. Using these data and the definition of formality from Bachas et al. (2023), we find that PBF beneficiaries spend a large share of their expenditures in formal stores (63%), which helps rationalize an increase in *formal* labor demand.

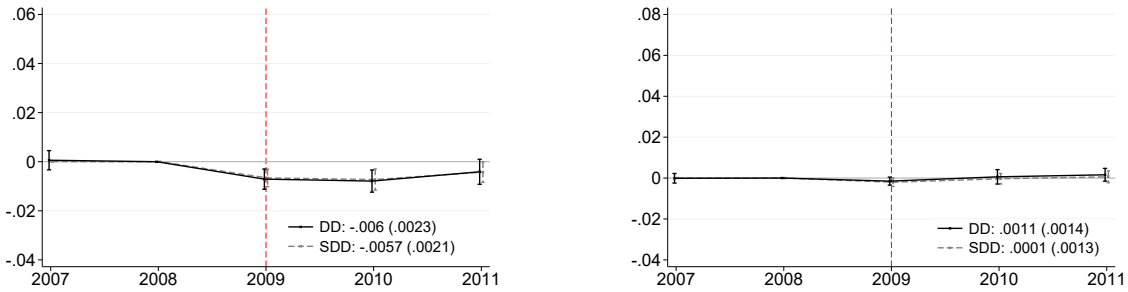
[36]The data used to compute price indices in Brazil only cover a few large metropolitan areas. The only price data available at the municipal level with extensive geographic coverage during our study period are retail prices for motor-vehicle fuels (gasoline, ethanol, diesel) and cooking gas. We find no evidence of any differential increase in those prices in treatment municipalities (see Online Appendix Figure B6). However, this evidence is only suggestive of a null price effect. These fuels are arguably tradable, and while their prices vary within the country, they may be less responsive to local demand than the price of non-tradables.

displays DD and SDD estimates for average log wages, revealing a decrease in average wages following the 2009 PBF expansion. However, this pattern is entirely driven by a composition effect. Figure 4b shows that the impact on average wages is essentially zero if we restrict attention to workers who were formally employed in each year in our study period (the composition of that sample is fixed). Moreover, Figure 3c shows that the overall increase in formal employment following the 2009 PBF expansion is entirely driven by low-wage workers.[37] An increase in labor demand without any corresponding increase in wages suggests the existence of excess supply in the labor market, consistent with the strong bunching at the minimum wage during our study period (Engbom and Moser 2022).[38]

Overall, the more recent papers on the aggregate effects of social protection transfers do not find relevant price effects (Niehaus and Suri 2025). Even for the cash transfers amounting to 15% of local GDP in Egger et al. (2022), consumer prices increased by only 0.1%-0.2%. Given this recent evidence and the absence of wage effects, it is reasonable to assume that the 2009 PBF expansion did not lead to large price effects.

FIGURE 4: IMPACT ON FORMAL WAGES

(A) AVERAGE LOG WAGES **(B) AVERAGE LOG WAGES - BALANCED PANEL**



Notes: The figure displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level for the average of log wages. Panel (a) displays estimates for the wage of private-sector formal workers employed in December of each year, which reflect a combination of treatment effects and compositional changes among formal employees. Panel (b) considers a balanced panel of workers employed throughout the sample period to shut down any composition effect. The vertical lines indicate the year of the PBF expansion.

Online Appendix Figure B9 provides additional evidence on the anatomy of the formal employment response to the 2009 PBF expansion. For instance, the impact is entirely driven by full-time workers; men and women contribute about two thirds and one third of the overall impact, respectively; and most of the impact is driven by an increase in formal

[37] Accordingly, we show in Online Appendix Figure B7 that the drop in average wages in Figure 4a essentially disappears if we focus on low-wage workers. Online Appendix Figures B5 and B8 also show that the 2009 PBF expansion raised formal employment in treatment municipalities among low-income workers in general, by considering all workers who were registered in *Cadastro Único* in 2008 and those with lower levels of education.

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

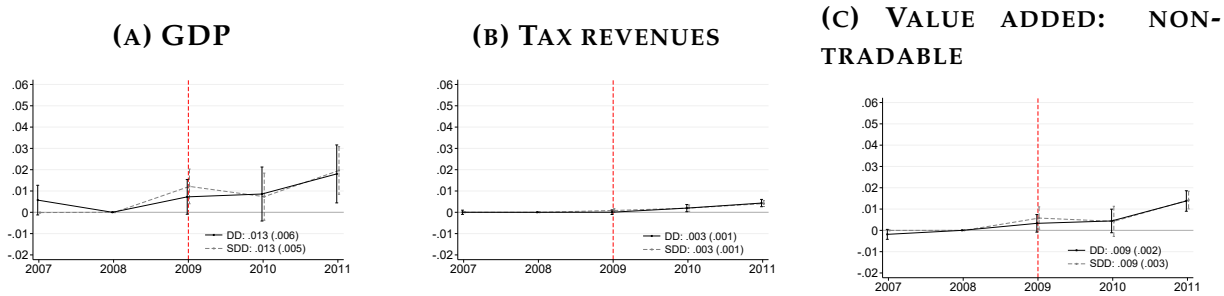
employment among establishments that did not exist prior to 2009. Relatedly, Online Appendix Figure B10 shows that the 2009 PBF expansion led to a relative increase in the number of establishments with at least one formal employee in treatment municipalities.

3.5 Impact on the overall economy

Next, we investigate the impact of the 2009 PBF expansion on the overall economy. Figure 5 displays DD estimates for the measure of local GDP computed by *IBGE*. We use the specification in growth rates to decompose the impact on GDP into the contribution of its sub-components, which are provided separately by *IBGE*: the revenues from all taxes levied on goods and services and the value added from non-tradable and tradable industries.

Figure 5a shows that the 2009 PBF expansion led to an increase in local GDP in treatment municipalities that reached 1.8% by 2011. Figure 5b shows that this effect is accompanied by an increase in tax revenues (0.4% of GDP by 2011), which is consistent with a rise in economic activity in the formal sector. Figure 5c shows that the increase in value added from non-tradable industries (1.4% of GDP by 2011) can account for the entire remaining increase in local GDP.[39] This is in line with the finding in Figure 3b that non-tradable industries account for the entire effect on formal employment.

FIGURE 5: IMPACT ON GDP, TAX REVENUES, AND VALUE ADDED



Notes: The figure displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level using national accounts data disaggregated by municipality. Panel (a) displays results for municipal GDP using the specification in growth rates. Panels (b) and (c) decompose this impact into the contribution of the revenues from all taxes levied on goods and services and of the value added from non-tradable industries, respectively (Online Appendix Figure B11 considers the value added from tradable industries). All estimates are expressed in percentage of the municipal GDP in 2008. The vertical lines indicate the year of the PBF expansion.

All the results documented so far could reflect a formalization of the economy without a corresponding increase in overall employment; indeed, the higher productivity of formal jobs could lead to an increase in GDP even if total employment remains unchanged.

[39] We present the null result for the value added from tradable industries in Online Appendix Figure B11. We also present the results for GDP and tax revenues using the log specification in Online Appendix Figure B12.

However, we also provide survey evidence in Figure 6 that the impacts on total employment and labor force participation track the effect on formal employment.

We must rely on data from the annual *PNAD* surveys to explore these impacts. A limitation of these surveys is that they are only meant to be representative at the state level. Therefore, the sampling scheme only covers around 15% of municipalities, the number of interviewed households per municipality is relatively small, and these households are drawn from a few census tracts within each sampled municipality. Nevertheless, there is no reason to believe that the sampling scheme will be biased in a way that correlates with exposure to treatment after the 2009 PBF expansion.[40] In Figure 6, we thus present DD results following the specification in equation (1) using these data. However, we run our regressions at the individual level to address the clustered sampling scheme within each municipality (*IGBE* does not consider the data to be representative at the municipal level) and to control for differences in individual characteristics correlated with employment outcomes (gender, race, education, urban area, age). These controls help mitigate potential biases from changes in sample composition over time (different households are sampled in each year). *PNAD* surveys were not conducted in 2010, which was a census year, so we combine data from *PNAD* surveys for 2007, 2008, 2009, and 2011, with data from the 2010 census. We restrict attention to municipalities with working-age respondents (age 18 to 60) in each year, resulting in a balanced panel of 744 municipalities. For robustness, we also present results controlling for pre-trends in average outcomes at the municipal level.[41]

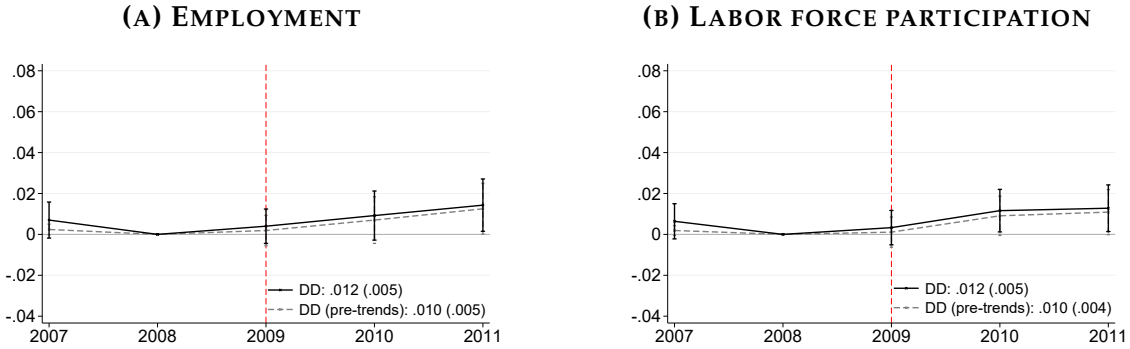
The outcomes in Figures 6a and 6b are dummy variables equal to one if the working-age respondent is employed and if they are in the labor force (i.e., they are employed or searching for a job), respectively. In both cases, we find evidence of a relative increase in treatment municipalities starting in 2009, which is robust to controlling for pre-trends.

Finally, we provide complementary evidence in Online Appendix C showing that the impact of the 2009 PBF expansion can be found across various relevant dimensions of economic activity (those we could study using available administrative data). Specifically, we show that the 2009 PBF expansion led to an increase in banking activity (total bank deposits, credits and loans), in electricity use by households (a measure of consumption), in electricity use by firms in non-tradable industries (another production input, besides labor), and in the number of cars and motorcycles registered (a physical asset).

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

[41] We use this approach to net out any differential pre-trend in the outcomes; given that we run this analysis at the individual level, we cannot use the SDD estimator (the individual level data are not longitudinal).

FIGURE 6: IMPACT ON OVERALL EMPLOYMENT AND LABOR FORCE PARTICIPATION



Notes: The figure displays DD coefficients (with their 95% confidence intervals) from estimating a similar specification as in equation (1) at the yearly level using microdata from *PNAD* surveys for 2007, 2008, 2009, and 2011, together with microdata from the 2010 census. The outcome in panel (a) is a dummy variable indicating whether a working-age respondent is employed. The outcome in panel (b) is a dummy variable indicating whether they are employed or searching for jobs. We present estimates from running the regressions at the individual level, controlling for differences in individual characteristics (gender, race, education, urban area, age) correlated with employment outcomes (solid black lines) and further controlling for pre-trends in average outcomes at the municipal level (dashed gray lines). The vertical lines indicate the year of the PBF expansion.

3.6 Behavioral responses to means testing

We end this section by showing that the 2009 PBF expansion also induced behavioral responses consistent with concerns that means-tested cash transfer programs incentivize families to alter their behavior to become or remain eligible for the benefits. Our analysis uses the two snapshots of *Cadastro Único* and a variant of the specification in equation (1) comparing the growth in municipal outcomes between August 2010 and December 2008.

We begin by shedding additional light on the impact of the 2009 expansion on the number of PBF beneficiaries and total PBF payments, using payment data from the month following each snapshot of *Cadastro Único*. In the first two rows of Table 2, column (1) shows that we obtain estimates consistent with those averaging 2010 and 2011 coefficients in Figure 2, using only these two months of data – increases of 18.9% and 17% for the number of PBF beneficiaries and total PBF payments, respectively. Columns (2)-(4) decompose these effects into the contribution of three types of families. Column (2) shows that about three quarters of these effects is driven by families who were "already eligible" prior to the 2009 expansion, i.e., those who would have been eligible in 2010 based on their *pci* in *Cadastro Único* 2008. By contrast, column (3) shows that "previously ineligible" families – who were registered in *Cadastro Único* 2008 with *pci* too high to be eligible in 2010 – account for a very small share of the increase in the number of beneficiaries and total PBF payments. Finally, column (4) shows that a sizable share of the overall effect comes from "newly registered" families, i.e., those who first registered in *Cadastro Único* after 2008.

These estimates do not in themselves constitute evidence of behavioral responses. All families in treatment municipalities were more likely to receive PBF benefits in 2010 *if*

TABLE 2: BEHAVIORAL RESPONSES TO MEANS TESTING

		Decomposing the overall effect by type of families		
	Overall effect	Already eligible	Previously ineligible	Newly registered
	[1]	[2]	[3]	[4]
PBF Beneficiaries	0.189***	0.147***	0.005***	0.038***
	(0.007)	(0.006)	(0.001)	(0.002)
PBF Payments	0.170***	0.132***	0.004**	0.034***
	(0.007)	(0.006)	(0.001)	(0.002)
Number of eligible families	0.029***	0.012***	0.001	0.016***
	(0.003)	(0.002)	(0.000)	(0.002)

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

they were registered as eligible in *Cadastro Único*. The policy therefore reinforced families' incentives to be registered as eligible in *Cadastro Único* in treatment municipalities. Yet, many families would have been eligible in 2010 even in absence of any behavioral response. In both treatment and control municipalities, many already eligible families would have remained eligible, some previously ineligible families would have experienced a drop in *pci*, and new families would have registered with *pci* below the eligibility thresholds.

The third row of Table 2 provides evidence of behavioral responses by showing that the 2009 PBF expansion increased the number of eligible families in treatment municipalities. Column (1) reports an overall effect of 2.9%. Columns (2)-(4) show that this increase is driven by already eligible families – they were more likely to remain eligible – and by newly registered families – more families were induced to register with *pci* below the eligibility thresholds.[42] By contrast, we find no evidence of behavioral responses among previously ineligible families.[43] We show in the next section – where we discuss welfare implications – that the size of these behavioral responses imply sizable losses in economic efficiency.

This finding could reflect several margins of responses: families with *pci* below the eli-

[42]This sizable role played by the registration margin is unlikely due to eligible families moving to areas where the program expanded. We do not find evidence of differential changes in population (see footnote 31). Quotas weakly increased everywhere and the quotas are binding only for new families (no families are 'kicked out' from PBF if they keep meeting the eligibility criteria). Moreover, Bergstrom et al. (2022) provides evidence of similar behavioral responses using a 2014 reform of PBF that increased the poverty and extreme poverty thresholds, as well as the benefit levels. The increase in the number of eligible families that they document is also partly due to newly registered families, but they study a national policy, so migration cannot be driving their results.

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

gibility thresholds could become more likely to register; families could under-report their *pci*; or families could reduce their labor supply, particularly in the formal sector where income is more readily observed by the government. Among these margins, labor supply responses are especially relevant for our study. The aggregate effects of the 2009 PBF expansion on local employment could arise despite negative labor supply responses among beneficiaries or, instead, be partly driven by positive responses among new beneficiaries who became less financially constrained to search for jobs. Our aggregate results would underestimate the strength of local demand effects in the first case and overestimate it in the second case.

It is challenging to quantify the impact of the reform on the labor supply of families who became beneficiaries at the time. One would have to find a suitable control group *within* the same municipality to net out local demand effects. Nevertheless, we show in Online Appendix D that concerns about negative labor supply responses are at least qualitatively relevant in our context. We use micro-level data and compare the formal labor supply of families eligible for different benefit amounts through a Regression Discontinuity design around the PBF eligibility thresholds. We find no evidence that receiving higher PBF benefits increases formal employment outcomes. Point estimates are negative for both formal employment and formal earnings, which is consistent with the evidence on behavioral responses to means testing in developing countries (Bergolo and Cruces 2021; De Brauw et al. 2015). These results thus suggest that, if anything, our aggregate results on formal employment might underestimate the strength of the local demand effects of PBF.[44]

4 Implications

In this section, we assess the magnitude of the estimated effects of the 2009 PBF expansion on local economic activity and we discuss their welfare implications. We start by examining its implied cost per job and output multiplier.[45] While this is useful to compare our

[44]The magnitude of this bias is likely limited, though. Incentive effects are particularly strong at the eligibility thresholds, where a marginal increase in formal income could be perceived by families as carrying the risk of a discontinuous decrease in PBF benefits. The size of our estimates is thus likely to be local to the thresholds, and labor supply responses are likely smaller for the average PBF family. Moreover, we show in Online Appendix Figure B13 that the formal employment rate of already eligible families, which we can identify in both treatment and control municipalities, increased relatively more in treatment municipalities after the 2009 PBF expansion. Therefore, local demand effects dominated any labor supply response within that group, which accounts for most of the increase in the number of PBF beneficiaries and total PBF payments.

[45]It is worth noting that the 2009 PBF expansion was funded by the general government budget and loans from international organizations (World Bank, 2009, MDS 2009a), with no contemporaneous tax increases (economic growth was increasing the tax base at the time). In such cases, Chodorow-Reich (2019) argues that a cross-region multiplier provides a "rough lower bound for a particular, policy-relevant type of national

estimates to the literature, it is important to note that the primary aim of a cash transfer such as PBF is not to stimulate the economy and, arguably, its merit should not be based on the size of its impact on the local economy. Moreover, the link between output multipliers and welfare effects must be examined carefully. We thus end by discussing how these aggregate effects impact the welfare implications of the policy.

For these different purposes, we express our DD estimates per $1 of PBF payments throughout the section. Specifically, following the empirical literature on cross-region multipliers (Chodorow-Reich, 2019), and in line with the results averaging 2010 and 2011 coefficients in Figure 2, we regress the change in some outcome of interest y on the change in PBF payments (PBF) between 2008 (pre) and the average of 2010 and 2011 ($post$):

$$\frac{y_{m,s,post} - y_{m,s,pre}}{y_{m,s,pre}} = \phi_s + \mu \cdot \frac{PBF_{m,s,post} - PBF_{m,s,pre}}{y_{m,s,pre}} + \sum_k \gamma_k \cdot X^k_{m,s} + \varepsilon_{m,s} \qquad (2)$$

We scale the changes in the outcome and in PBF payments in the same way so that μ has a "multiplier" interpretation, i.e., the increase in the outcome per $1 of PBF payments ($dy/dPBF$). We instrument the scaled change in PBF payments by the treatment dummy and estimate equation (2) by 2SLS. The inclusion of the state fixed effects ϕ_s and the control variables $X^k_{m,s}$ ensures that we use the same variation as in equation (1) for identification.[46]

4.1 Employment multiplier and cost per job

We start by estimating the formal employment multiplier of the 2009 PBF expansion – i.e., the increase in formal employment per $1 of PBF payments – and the implied cost per formal job – i.e., the inverse of the formal employment multiplier. The empirical literature on cross-region multipliers often focuses on the employment multiplier or the cost per job rather than the output multiplier – i.e., the increase in GDP per $1 spent – because it is challenging to measure local GDP precisely (Chodorow-Reich, 2019). We focus on formal employment for three reasons. First, as we discuss in Section 4.3, the employment multiplier in the formal sector is most relevant for the MVPF of the 2009 PBF expansion. Second, due to data limitations (see Section 3.5), we cannot estimate the impact on overall employment precisely. Third, this allows us to compare our estimates to those in Corbi

multiplier, the closed economy, no-monetary-policy-response, deficit-financed multiplier."

[46] With a slight abuse of notation, we use $dy/dPBF$ here and below to refer to the causal effect of the 2009 PBF expansion on outcome y scaled by its causal effect on PBF payments, which is what equation (2) and our instrument deliver. However, we do not imply that the 2009 PBF expansion only affects outcome y through the amount of cash disbursed in the locality through the PBF program. In other words, the exclusion restriction may not hold, for instance because of the fact that PBF is means-tested (see the discussion at the end of Section 3.6).

et al. (2019), who also focus their analysis on formal employment. Nevertheless, we also consider potential effects in the informal sector in our discussions in Sections 4.2 and 4.3.

Focusing on low-wage formal jobs – which drive the formal employment results (see Figure 3c) – we estimate that $100,000 in PBF payments generate $\mu_{FE_l} \cdot 100,000$ =10.21 (s.e. 3.48) low-wage formal jobs. This implies a cost per job of $1/\mu_{FE_l}$ =$9,799 (s.e. $3,340). A cost per job of $9,799 at the yearly level – equivalent to 3.67 times the annual minimum wage at the time – is comparable to the preferred estimate in Corbi et al. (2019) of $8,000.[47]

Our evidence of a sizable employment multiplier aligns with the literature on the effects of demand shocks in richer countries, which considers high unemployment rates as a key indicator of slack in an economy (Michaillat and Saez 2015). Relatedly, we show in Appendix Table B3 that the formal employment multiplier is higher – and the cost per job lower – in municipalities with above-median unemployment rates in the 2000 census. The formal employment creation induced by the 2009 PBF expansion – which was a period of steady economic growth in Brazil – was thus concentrated in labor markets with a history of excess capacity. This finding supports the view that the existence of slack is a persistent feature of developing economies, rather than a feature of recessions as in richer countries.

4.2 Output multiplier

We can use different approaches to obtain an output multiplier for the 2009 PBF expansion. First, we can estimate the specification in equation (2) for the measure of local GDP computed by *IBGE*. This yields a very large multiplier of around 5, combining the increase in the value added from non-tradable industries ($\mu_{VA_{NT}} = 4.41$; s.e. 1.61) with a tax multiplier of $\mu_{tax} = 0.58$ (s.e. 0.25).[48] While the revenue raised locally from all taxes levied on goods and services can be measured directly, estimating the value added produced in a municipality must necessarily rely on strong assumptions. In practice, few countries attempt to produce GDP measures at such a disaggregated level. Even the U.S. administration only first released county-level GDP data to the public in December 2019. These figures rely on various imputation methods to allocate state-level GDP to the local level every year. The measurement error introduced by these methods is likely higher in a country like Brazil with a large informal sector for which economic activity must be measured indirectly. Moreover, few studies use these disaggregated GDP measures even when they are available, including in the Brazilian context (see, e.g., Corbi et al., 2019; Colonnelli and

[47] Results are comparable, but less precise, using the overall formal employment measure ($1/\mu_{FE}$ = $12,262; s.e. $6,913). These cost-per-job figures are smaller than typical cost-per-job estimates in the United States (Chodorow-Reich 2019), but there are clear differences in wage levels between Brazil and the United States. Our estimates are expressed in 2016 USD such that they are directly comparable to those in Corbi et al. (2019).

[48] Using overall GDP, the point estimate is even larger and it is much noisier (μ_Y =7.16 with s.e. 3.35).

Prem, 2022). Therefore, to assess these magnitudes and compare them to the literature, we also use an alternative approach relating output and employment multipliers following Chodorow-Reich (2019), which relies on strong but transparent assumptions. It has the added benefit of allowing us to compute a multiplier for the formal sector of the economy – which we use in the welfare evaluation – and to assess how sensitive our estimate is to different assumptions about the impact of the 2009 PBF expansion on the informal sector.

We start from the production function: $Y = A \cdot (N \cdot L)^{1-\alpha}$, with hours per worker N and the number of effective units of labor L. While L equals total employment E in Chodorow-Reich (2019), we introduce heterogeneity in hourly productivity (ψ) across three worker types (Cunha et al. 2022): $N \cdot L = \sum_{J \in \{FE_l, FE_h, IW\}} N_J \cdot \psi_J \cdot J$, with FE_l, FE_h, and IW the number of low-wage formal employees, high-wage formal employees, and informal workers, respectively. We can then derive an expression for the output multiplier:

$$\mu_Y = (1-\alpha) \cdot (\chi + 1) \cdot \frac{Y}{E} \cdot (\kappa_{FE_l} \cdot \mu_{FE_l} + \kappa_{FE_h} \cdot \mu_{FE_h} + \kappa_{IW} \cdot \mu_{IW}) \tag{3}$$

where χ denotes the elasticity of hours per worker to employment and κ captures each worker type's productivity relative to the average worker in the economy.[49] This production function corresponds to a short-run perspective, where hours per worker adjust but capital does not. Alternatively, we can take a longer-run perspective, assuming that hours per worker remain fixed and capital scales up in proportion to effective labor. In that case, we simply have: $\mu_Y = \frac{Y}{E} \cdot (\kappa_{FE_l} \cdot \mu_{FE_l} + \kappa_{FE_h} \cdot \mu_{FE_h} + \kappa_{IW} \cdot \mu_{IW})$.[50]

For calibration, we proceed as follows. For low-wage formal workers, we use the formal employment multiplier estimated above. We take $\chi^E = 0.12$, $(1-\alpha) = 0.666$, and $\kappa_{IW} = 0.55 \cdot \kappa_{FE_l}$ from Corbi et al. (2019). For total output (Y), we aggregate the municipal GDP estimates. We obtain the share of informal workers from the 2010 census ($IW/E = 0.493$). We take the number of low-wage private-sector formal employees from *RAIS* and compute their share ($FE_l/E = 0.274$) by combining the share of private-sector formal employees in the census with the ratio of low-wage vs. high-wage private-sector workers in *RAIS*. The remaining workers – including public employees – are pooled into the category of high-wage formal employees ($FE_h/E = 0.235$). We assume that the relative productivity of high- vs. low-wage formal employees can be captured by the ratio

[49] Specifically, $\kappa_J = \frac{\psi_J \cdot N_J}{(N \cdot L)/E}$ for $J \in \{FE_l, FE_h, IW\}$. We note that we only need to know the ratios $\kappa_{FE_h}/\kappa_{FE_l}$ and $\kappa_{IW}/\kappa_{FE_l}$ – and each worker type's share of total employment – to pin down the three κ_J's.

[50] Online Appendix E derives the expression in equation (3). By comparison, the expression in Chodorow-Reich (2019) simplifies to $\mu_Y = (1-\alpha) \cdot (\chi + 1) \cdot \frac{Y}{E} \cdot \mu_E$. According to Chodorow-Reich (2019), we have $(1-\alpha)(\chi+1) \simeq 1$ for the U.S., so these short-run and longer-run perspectives provide similar estimates.

of their earnings among private-sector workers in *RAIS* ($\kappa_{FE_h} = 2.89 \cdot \kappa_{FE_l}$). Finally, we find no employment effect for other formal employees, so we have $\mu_{FE_h} = 0$. For consistency, we compute all the above statistics in 2010 using only the municipalities in our main estimation sample.

Our benchmark calibration assumes no employment effect in the informal sector ($\mu_{IW} = 0$). This is consistent with evidence from business cycle variation in developing countries that, while formal employment and total employment are strongly correlated, informal employment is essentially acyclical (see, e.g., Ohnsorge and Yu 2022). With this assumption, we obtain an output multiplier for the 2009 PBF expansion of 1.46 (s.e. 0.50) using the expression in equation (3), and of 1.95 (s.e. 0.66) using the longer-run perspective.[51]

Existing estimates of local output multipliers in Brazil mainly consider the impact of government purchases rather than the impact of transfers to private households, and purchase multipliers are mechanically higher than transfer multipliers (Pennings 2021).[52] Corbi et al. (2019) consider the impact of increases in municipal government budgets and obtain multipliers ranging from 1.1 to 2.6 using the same short-run approach as in Chodorow-Reich (2019). Colonnelli and Prem (2022) computes purchase multipliers from 1.46 to 4.34 for anti-corruption policies in Brazil, using an approach akin to our longer-run perspective (multiplying the average firm value added by their estimated increase in the number of firms). For transfers multipliers, Egger et al. (2022) estimate a multiplier of 2.4 for a one-time transfer to poor families in rural Kenya, while Pennings (2021) estimate a multiplier of 1.5 for permanent transfers to old-age pensioners in the U.S.[53] Our estimate is lower than that of Egger et al. (2022), but we note that equation (3) assumes that each worker type's hourly productivity is fixed. Therefore, we may underestimate

[51] It is quantitatively important that the employment effect is concentrated among low-wage formal workers. The multiplier would be 1.77 (short-run) and 2.38 (longer-run) if new jobs were as productive as the average job, or 2.72 (short-run) and 3.65 (longer-run) if new jobs were as productive as the average formal job in the economy.

[52] In the New Keynesian model with rigid prices and wages, the multiplier from a permanent \$1 increase in government purchases and in transfers is $1/(1-\alpha)$ and $\alpha^b/(1-\alpha)$, respectively; α and α^b capture the marginal propensity to spend on locally produced goods and services for the average household and for households receiving the transfer, respectively. The numerators in these expressions capture the direct impact on the local economy: an increase in spending of 1 for government purchases, but of only $\alpha^b \leq 1$ for transfers; the denominators capture the general equilibrium effects. These expressions also illustrate why the empirical literature on purchase multipliers typically uses a reference value of 1 and why the same benchmark does not apply to transfer multipliers. In our setting, α^b could be lower than in cases of lump-sum transfers (or programs using proxy-means testing) if new beneficiaries reduced their earnings because of the potential substitution effect induced by means-testing.

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

the multiplier if the 2009 PBF expansion also led to productivity gains as observed in their setting.

Our benchmark calibration assumes no impact on the informal sector, but transfers to poor households might increase demand in both the formal and the informal sectors. Specifically, the output multiplier would increase to 1.86 (short-run) and 2.49 (longer-run) if we assumed, as in Corbi et al. 2019, that one informal job was created for every two formal jobs.[54] We can also consider cases where demand shocks impact formal and informal employment in opposite directions. For instance, Ponczek and Ulyssea (2022) finds that the loss in total employment caused by the unilateral trade liberalization in Brazil in the 1990s was only a third of the loss in formal employment, because informal employment went up. Assuming a similar impact in our setting ($\mu_{IW} = -0.66 \cdot \mu_{FE_l}$) would reduce the multiplier to 0.92 (short-run) and 1.24 (longer-run). However, this assumption is likely too extreme in our case. The negative demand effects arising directly from the trade liberalization were heavily concentrated in the formal sector – domestic manufacturing faced increased import competition. By contrast, the direct demand effects of expanding PBF payments likely benefited both the formal and the informal sectors. We thus consider our benchmark calibration – grounded in the evidence from business-cycle variation – as rather conservative.

These calibrations suggest that the output multiplier estimated using the local GDP measures is rather high and so could be biased by measurement error. Even considering the longer-run perspective, rationalizing a multiplier of 5 would require a substantial increase in informal employment (three informal jobs per formal job created) or more than a twofold rise in productivity for low-wage formal workers. Therefore, to be conservative, we use the lower estimate of 1.46 when assessing the welfare implications of the 2009 PBF expansion.

4.3 Marginal Value of Public Funds (MVPF)

We end by highlighting that the aggregate effects of the 2009 PBF expansion can substantially change its MVPF, which is the ratio of the WTP for the policy to its net fiscal cost (Finkelstein and Hendren 2020). The MVPF informs welfare by indicating how much the government must value spending on a policy for it to improve welfare: the welfare gain is given by $dW = \omega \cdot MVPF - 1$, where $MVPF - 1$ captures the changes in social efficiency per \$1 of net government spending. An MVPF of 1 thus corresponds to the benchmark of a hypothetical non-distortionary policy, where benefits are valued at their cost. The parameter ω captures the money-metric welfare gain from giving \$1 to the population affected by the policy – in our case, both the new PBF beneficiaries and those impacted indirectly

[54] In fact, Feler et al. (2023) find increases in both formal and informal employment in their study.

via aggregate effects. Specifically, ω corresponds to their "social welfare weight" divided by the shadow value of public funds. An efficiency-reducing policy ($MVPF < 1$) could still improve welfare if it benefits groups with a high social welfare weight ($\omega > 1$).

We proceed in steps to compute the MVPF of the 2009 PBF expansion, and follow the literature by considering the MVPF of a marginal change in the size of the policy. We focus on an intuitive explanation here and provide more details in Online Appendix F.

A. Considering the direct impact on PBF beneficiaries. A first step is to consider the direct impact from receiving the additional PBF payments. For this purpose, it is useful to define $dPBF = dM + dB$, where we distinguish between 'infra-marginal' beneficiaries (dM) who received the extra benefits without changing their behavior, and 'marginal' beneficiaries (dB) who adjusted their behavior to qualify for the benefits (Finkelstein and Hendren 2020). For a cash transfer, we can assume that the first group values receiving \$1 at \$1: $WTP^{dM} = 1$. By contrast, under the assumption that households make privately optimal decisions, the welfare effect is nil for marginal beneficiaries: $WTP^{dB} = 0$.[55] Therefore, considering only the direct impact from receiving the additional PBF benefits, we would have $MVPF_1 = \frac{\frac{dM}{dPBF} \cdot WTP^{dM} + \frac{dB}{dPBF} \cdot WTP^{dB}}{1} = \frac{dM}{dPBF}$, which is the fraction of the additional PBF payments that accrue to infra-marginal beneficiaries.

By this metric, the behavioral responses documented in Section 3.6 imply sizable losses in efficiency. To quantify this, we first estimate $\frac{dB}{dPBF}$. We use a similar specification as in equation (2) and the two snapshots of *Cadastro Unico* in December 2008 (*pre*) and August 2010 (*post*). To capture the change in PBF payments due to the rise in the number of eligible families (dB), the outcome variable y is the number of eligible families multiplied by the average PBF payment received by eligible families in treatment municipalities. We estimate that, for every \$1 of additional PBF payments in treatment municipalities, \$0.25 (s.e. 0.02) went to marginal beneficiaries. This implies: $MVPF_1 = 0.75$. For comparison, the corresponding figure is higher – 0.90 – for the means-tested cash transfer in Bergolo and Cruces (2021); it would be 1 for the lump-sum NGO transfer in Egger et al. (2022).

B. Adding the impact on tax revenues. The net fiscal cost – the denominator of the MVPF – includes not only the direct cost of the policy but also any impact on tax revenues: $MVPF_2 = \frac{\frac{dM}{dPBF}}{1 - \frac{dTax}{dPBF}}$. This is a common concern with transfer programs, especially when the use of means testing induces beneficiaries to reduce their formal labor supply

[55] This general point is a standard application of the envelope theorem. For non-marginal changes in the size of the policy, we would have $0 < WTP^{dB} < 1$ (by revealed preferences). Thus, by considering a marginal change, we are underestimating the WTP of marginal beneficiaries. It is also worth nothing that, in a setting like ours where some eligible households are not beneficiaries, the result $WTP^{dB} = 0$ applies *on average* to households who changed their behavior to qualify for the benefits (see Online Appendix F). The logic is simple: these households incur a cost to receive the PBF benefits with some probability; at the margin, they must therefore be better off if they end up receiving the benefits, worse off otherwise, and their expected welfare gain is nil.

and, therefore, their taxable earnings. For instance, incorporating the implied loss in tax revenue from the partial equilibrium reduction in formal employment lowers the MVPF in Bergolo and Cruces (2021) to 0.61. As we discuss in Section 3.6, this type of partial equilibrium response could be at play in our setting. However, there can also be aggregate responses in the local economy – particularly in its formal sector – that increase tax revenue. The 2009 PBF expansion in fact raised formal employment on net. This effect in itself does not affect tax revenue in Brazil because it is concentrated among low-wage workers who are exempt from income taxation. Yet, we showed that the higher economic activity led to higher revenue from taxes levied in formal output markets – by \$0.58 per \$1 of PBF payments. This effect alone increases the MVPF of the 2009 PBF expansion to: $MVPF_2 = \frac{0.75}{1-0.58} = 1.78$, raising it *above* the benchmark value for a non-distortionary transfer.[56]

C. Adding the WTP for the increase in economic activity. The aggregate impacts of the 2009 PBF expansion on the local economy can also affect the numerator of the MVPF by increasing the WTP for the policy. This depends, however, on the net social value WTP^{dQ} of the increase in economic activity dQ, which may not necessarily be high. To see this, it is useful to make the first-order approximation: $WTP^{dQ} = k - h$, where k denotes the social marginal benefit of an increase in goods and services and h is its social marginal cost.[57] If all markets in the economy are at a socially efficient equilibrium, k and h correspond to the willingness-to-pay of consumers and to the willingness-to-supply of producers, respectively, and both are equal to the market price. Consequently, we have $k = h$ and an increase in economic activity has no first-order effect on social efficiency.

In reality, various distortions can create a positive "wedge" between the price faced by consumers and the opportunity cost of production in some markets ($k - h > 0$), causing output to be below its socially efficient level. Increasing output in these markets will therefore yields some efficiency gain, but the gain will be less than one-for-one if the increase in economic activity comes with an opportunity cost: $dGDP = dQ \cdot k > dQ \cdot (k - h)$, where GDP captures the value of economic activity (i.e., consumer price times quantity). An increase in economic activity would only be valued one-for-one if it involves no social cost. Egger et al. (2022) argue that this is the case in rural Kenya: firms under-utilize their factors of production, so they have ample capacity to increase output without any additional input.

The increase in economic activity comes with an opportunity cost in our setting (e.g.,

[56] There are also taxes on capital income in Brazil, but we cannot estimate the size of any overall increase in capital in the local formal sector or of the revenue from taxes levied on it. So, we prefer to be conservative and abstract from it altogether, with the understanding that our analysis likely provides a lower bound as a result.

[57] Sims and Wolff (2018) show that this first-order approximation holds in a stylized general equilibrium model of the economy linking the output multiplier to what they call the "utility" multiplier.

workers provide more labor). Nevertheless, the efficiency gains are likely positive because empirical evidence points to plausible sources of positive wedges in markets impacted by the 2009 PBF expansion.[58] Such wedges can arise in both output and input markets. To illustrate this, we can use the decomposition: $WTP^{dQ} = (k - s) + (s - h)$. First, distortions may drive a wedge between the price charged by output producers and their private marginal cost of production s. For instance, consistent with the growing literature highlighting that producers often sell their products above marginal cost, De Loecker and Eeckhout (2018) estimate sizable price markups in Brazil's formal output markets. Second, the private marginal cost of production may exceed the true opportunity cost h. Our finding of an increase in labor demand for low-wage formal workers with no corresponding increase in wages suggests excess supply in the labor market – consistent with the strong bunching at the minimum wage during our study period (Engbom and Moser 2022). This would imply a gap between the wage paid by employers and these workers' reservation wage.

We consider these two sources of distortion in turn to illustrate the importance of accounting for the aggregate effects of the 2009 PBF expansion, noting that we must rely on estimates from other studies to quantify their efficiency implications – in contrast to the case of taxes, for which we could provide estimates directly from our data.

Considering excess supply in the low-wage formal labor market introduces a new term in the numerator of the MVPF: $\frac{dFE_l}{dPBF} \cdot w_{FE_l} \cdot (1 - \frac{w_{FE_l}}{w_{FE_l}})$, where $\frac{w_{FE_l}}{w_{FE_l}}$ captures the "reservation raise," the ratio of the reservation wage to the actual wage. In a recent survey, Mui and Schoefer (2024) find an average reservation raise of 0.71 for unemployed workers in the U.S. We use this value because it is in line with structural estimates in Engbom and Moser (2022) of the ratio of the flow value of leisure to the average formal wage between 0.6 and 0.8 for lower-skilled workers in Brazil. Combining it with the formal employment multiplier estimated above, and the average wage among low-wage formal employees in 2010, we obtain a WTP for the increase in formal employment of \$0.11 per \$1 in PBF payments. This raises the MVPF of the 2009 PBF expansions to: $MVPF_3 = \frac{0.75+0.11}{1-0.58} = 2.03$.

Similarly, considering pricing power in the output market adds a new term in the numerator of the MVPF: $\frac{dQ}{dPBF} \cdot p \cdot \frac{\theta}{1+\theta}$, where θ denotes the markup, and $\frac{dQ}{dPBF} \cdot p$ is the pre-tax value of the increase in output in the market where producers exert such pricing power. We focus on non-tradable industries, which drive the increase in economic activity (see Sections 3.4 and 3.5), and estimate an average mark-up of 0.288 by combining the methodology in De Loecker and Eeckhout (2018) with microdata from the 2010 Annual Surveys of Trade (*PAC*) and Services (*PAS*).[59] This estimate – and that in De Loecker and

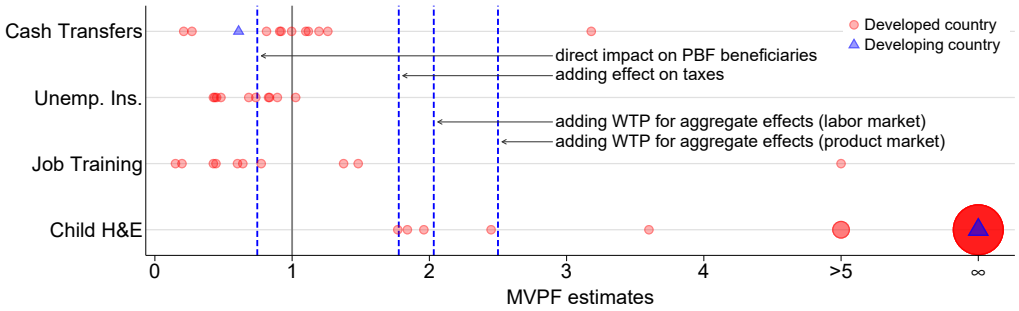
[58] We note that we have already accounted for the wedges between social marginal benefits and social marginal costs arising from taxation through the denominator of the MVPF (see Online Appendix F).

[59] Following the so-called "production approach," the average markup in De Loecker and Eeckhout

Eeckhout (2018) – is limited to formal firms. Therefore, to measure $\frac{dQ}{dPBF} \cdot p$ (and to be conservative), we use our lower estimate of the output multiplier of 1.46 (to which we subtract $\frac{dTax}{dPBF}$), which captures the impact in the formal sector (by contrast, the estimates based on the measure of local GDP from *IBGE* cover both formal and informal sectors). We obtain a WTP for the increase in output of \$0.20 per \$1 in PBF payments, which further raises the MVPF of the 2009 PBF expansion to: $MVPF_4 = \frac{0.75+0.11+0.20}{1-0.58} = 2.50$.

D. Discussion. To provide some comparison, Figure 7 displays our four estimates of the MVPF with estimates for four types of policies from the *Policy Impacts Library*: any type of cash-based transfers, unemployment insurance (a leading cash-based social insurance program), and two categories for which MVPFs are usually low (Job Training) and very high (Child Health and Education). Once we account for the aggregate effects of the 2009 PBF expansion, Figure 7 shows that its MVPF not only increases above that of a hypothetical non-distortionary transfer, but that it also becomes relatively high. It increases above most of the MVPF estimates for cash transfers, unemployment insurance programs and job training policies, and even above some estimates for Child Health and Education policies. The graph also highlights the paucity of estimates from developing countries.

FIGURE 7: IMPLIED MARGINAL VALUE OF PUBLIC FUNDS



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

A policy with a high MVPF yields a relatively high welfare return compared to other policies benefiting *the same population*. However, whether the government should allocate more resources to this policy, relative to others that may affect different populations, also

(2018) is the sales-weighted average of a markup for each firm i in year t equals to: $\alpha_{it} \cdot (P_{it}Q_{it})/(P_{it}^V V_{it})$, where the first term is the output elasticity and the second term is the ratio of sales to expenditures on variable inputs. De Loecker and Eeckhout (2018) compute the firm-level markups by using their elasticity estimates from the U.S. for the same industry and year (assuming that "firms in the same sector have access to the same technology, yet they differ in TFP") and the ratio of sales to the Cost of Goods Sold in data available for listed companies in Brazil. We use these elasticity estimates as well, but we calculate the ratio $(P_{it}Q_{it})/(P_{it}^V V_{it})$ in the Brazilian surveys in 2010. We do not use their estimate for Brazil because it is restricted to listed firms and includes manufacturing firms.

depends on welfare weights. Considering only the direct impact on PBF beneficiaries, the 2009 PBF expansion was welfare improving if the government valued spending \$0.746 on the policy more than spending \$1 on its next best alternative; that is, if $\omega^{PBF} > 1.34$. This condition might already be satisfied without considering the aggregate effects of the policy, given that PBF targets poor families who likely carry a high social welfare weight. The threshold decreases considerably once we add the impact on tax revenues: the minimum necessary welfare weight on infra-marginal beneficiaries drops to $\omega^{PBF} > 0.56$.

Including the efficiency gains in the labor and product markets further lowers the minimum necessary welfare weight on those affected by the policy. The extent of this reduction, however, depends on who benefits from these aggregate effects, as they likely carry different welfare weights than PBF beneficiaries. Denoting these weights ω^{AggL} and ω^{AggP} for the two markets, respectively, we have: $\omega^{PBF} > 0.56 - 0.14 \cdot \omega^{AggL} - 0.26 \cdot \omega^{AggP}$.[60] While the efficiency gain in the product market has a greater impact on the MVPF than the efficiency gain in the labor market, these benefits accrue to different groups: producers and low-wage formal employees, respectively. As a result, we likely have $\omega^{AggL} > \omega^{AggP}$, and both groups likely carry lower welfare weights than the poorer PBF beneficiaries.

E. Considering informality responses. These welfare implications of the 2009 PBF expansion are robust to any assumption regarding its impacts on the informal sector *if* there is no wedge between social marginal benefits and social marginal costs in that sector. Focusing on distortions affecting the formal sector is a natural starting point in a Latin American context. The main concern is that the size of the formal economy is inefficiently low in these countries (Levy, 2010), and the wedges that we consider matter most in the formal sector, which is subject to taxation, stricter regulations, and higher barriers to entry (Ulyssea, 2020). Nevertheless, it is worth noting that we would have $MVPF_4 = 2.12$ if we assume that reservation raises and markups were the same in the two sectors, and that the employment effect was only a third of the formal employment effect ($\mu_{IW} = -0.66 \cdot \mu_{FE_l}$).

5 Conclusion

We provided new evidence on the aggregate effects of cash transfers targeting poor households in developing countries and highlighted that these effects can significantly improve the welfare implications of these programs. Considering only the direct impact on PBF beneficiaries, we showed that the MVPF of the 2009 PBF expansion falls below the benchmark value of 1 for a non-distortionary transfer because PBF is means tested. However, the aggregate effects of the policy raise its MVPF well above that value, by increasing tax revenues and potentially mitigating pre-existing distortions in the economy.

Importantly, whether expanding a cash transfer program like PBF offers the 'best bang

[60] This follows from: $dW > 0 \Leftrightarrow 1.78 \cdot \omega^{PBF} + (2.03 - 1.78) \cdot \omega^{AggL} + (2.50 - 2.03) \cdot \omega^{AggP} > 1$.

for the buck' depends on the alternative uses of public resources. Developing countries employ various social assistance tools – including in-kind transfers, asset transfers, and public employment programs. Despite growing evidence on the impacts of such policies (Banerjee et al. 2024), comparing their relative returns remains challenging. A step forward would be for researchers to adopt a common welfare metric, such as the MVPF. Comparing the willingness-to-pay for the direct impact of these policies to their net cost to the government would allow a systematic comparison across policies targeting similar populations. However, the spillover effects of these programs on non-beneficiaries raise important incidence questions: who gains from these external effects and what are their welfare weights?

How much a government should spend on social assistance also depends on the welfare cost of raising revenue – another use of public resources is to reduce taxes. Evidence that cash transfers to poor households can increase economic activity – improving the welfare effect of these policies – raises a natural question: do taxes imply similar demand effects in reverse, increasing the welfare cost of revenue collection? While the empirical literature on taxation in developing countries has grown a lot over the last decade, we still know little about the potential economy-wide effects of transferring resources from taxpayers to governments. Evidence from the U.S. suggests that tax changes for lower-income taxpayers can have sizable impacts on economic activity, while changes for higher-income groups have only small aggregate impacts (Zidar 2019). More research is needed to understand the potential demand effects of funding social assistance policies in developing countries.

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Online Appendix

Cash Transfers and the Local Economy: Evidence from Brazil

by François Gerard, Joana Naritomi and Joana Silva

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

This Appendix provides examples of how relevant politicians discuss the effects of *Programa Bolsa Familia* (PBF) on the economy. 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 2018 - 2022 and 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**: UOL Notícias (April 14th, 2011).

"This is a crime, there are people who have been in the Bolsa Família program for nine years, they don't want to be employed because they would lose Bolsa Família. Bolsa Família serves families with up to five children, these kids will grow up, thinking what? Bolsa Família as well. You see girls in the Northeast, patting their bellies, pregnant, saying that they also get a birth grant, 'this one will be a refrigerator', and 'this one will be a washing machine'. And they don't want to work." (ChatGPT translation)

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**: *Roda Viva* TV Show (June 3rd, 2014).

"...What happens today is the fear of people who are beneficiaries of Bolsa Família to seek space in the formal job market, not to secure that job, not to stay in that job, and to lose both. (...) Brazil will be a better place when we, respecting the rights of those who receive Bolsa Família, who are

untouchable, celebrate because Brazil has grown, developed, these people have become qualified, celebrate that we have two or three or five million fewer families in Bolsa Família." (ChatGPT translation)

3. **Luis Inácio Lula da Silva**, 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.

"...public money spent on people, in health, education, income, and food, as shown by the data Teresa presented, should never again be treated as an expense, but rather as a great investment. I am in favor, but sometimes we take a billion, lend it to a company. It will build a business, which will create 200 jobs after completion, and will export almost nothing of what it imports. Now take a billion and give it to Bolsa-Família and see how many kilos of beans, meat, tripe, jerked beef, fish the person will buy. It's proven that Bolsa Família money stimulates trade, boosts the consumption of food, clothes, and hygiene products. Because people also have the right to access things. In fact, the expansion of income, combined with the valorization of wages and the democratization of credit, is at the root of the miracle that we have made." (ChatGPT translation)

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**: Folha de São Paulo newspaper (February 9th, 2016).

"Bolsa Família came in the context of food security (...) and has an understandable immediacy (...). In regions with depressed economies, these exit doors are already simpler to visualize today. That small commerce, the government purchases through family agriculture interacting with the issue of Bolsa Família, has already stirred a very rudimentary, very simple, but very effective economic dynamic for millions of people."(ChatGPT translation)

Online Appendix B: Additional Tables and Figures

TABLE B1: CADASTRO UNICO AND FORMAL EMPLOYMENT

	All families [1]	Families below the extreme poverty line [2]	Families below the poverty line [3]
Cadastro			
Number of individuals	72,553,248	44,853,444	18,757,388
Number of families	20,460,846	12,253,616	4,980,687
Share of families in PBF	0.62	0.79	0.54
Share of adults with high-school degree among PBF families	0.12	0.11	0.15
Avg. share urban among PBF families	0.70	0.66	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 monthly benefits	95.34	105.19	64.22
RAIS: Sep. 2010 - Aug. 2011			
Share of families with at least 1 adult in RAIS among PBF families	0.35	0.29	0.52
Number of adult-months in RAIS among PBF families	3.22	2.50	5.43
Average wage among PBF adults in RAIS (conditional on working)	746.31	737.58	759.64

Notes: 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*; the middle panel matches these families to their PBF payments in the following 12-month period; and the bottom panel matches these families to the formal employment data (*RAIS*) in the following 12-month period. Monetary values are reported in BRL of 2010.

TABLE B2: TABLE 2 WITH STANDARD ERRORS CLUSTERED BY MUNICIPALITY AND BY RGI

	Top 50% vs bottom 50% [1]	Top 25% vs bottom 25% [2]	Top 50% vs bottom 50% (actual quota change) [3]	Treatment intensity linearly [4]	Including largest cities [5]	Population weights [6]	DFL weights for baseline program size [7]	No baseline controls [8]	Outcomes in per capita terms [9]	Aggregated at the *RGI* level [10]
Panel A. Log specification										
PBF beneficiaries	0.156***	0.252***	0.177***	5.242***	0.155***	0.144***	0.147***	0.126***	0.162***	0.120***
s.e. clustered by municipality	(0.006)	(0.010)	(0.005)	(0.174)	(0.006)	(0.009)	(0.007)	(0.005)	(0.007)	
s.e. clustered by rgi	(0.008)	(0.012)	(0.007)	(0.203)	(0.008)	(0.010)	(0.008)	(0.007)	(0.008)	(0.011)
PBF payments	0.134***	0.217***	0.152***	4.568***	0.133***	0.128***	0.123***	0.107***	0.140***	0.102***
s.e. clustered by municipality	(0.006)	(0.010)	(0.005)	(0.163)	(0.006)	(0.009)	(0.006)	(0.005)	(0.006)	
s.e. clustered by rgi	(0.007)	(0.011)	(0.006)	(0.190)	(0.007)	(0.009)	(0.008)	(0.007)	(0.007)	(0.010)
Private-sector formal employment	0.020**	0.029**	0.021***	0.765***	0.020**	0.018***	0.023***	0.018**	0.027***	0.020**
s.e. clustered by municipality	(0.008)	(0.013)	(0.008)	(0.223)	(0.008)	(0.006)	(0.008)	(0.008)	(0.008)	
s.e. clustered by rgi	(0.008)	(0.014)	(0.009)	(0.232)	(0.008)	(0.006)	(0.008)	(0.008)	(0.008)	(0.009)
Public-sector employment	0.010	0.000	0.011	0.098	0.012	0.016	0.009	0.007	0.016	0.007
s.e. clustered by municipality	(0.012)	(0.022)	(0.014)	(0.437)	(0.012)	(0.013)	(0.013)	(0.013)	(0.012)	
s.e. clustered by rgi	(0.012)	(0.022)	(0.013)	(0.436)	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.009)
Panel B. Growth specification										
PBF beneficiaries	0.175***	0.287***	0.200***	6.031***	0.174***	0.167***	0.165***	0.141***	0.174***	0.131***
s.e. clustered by municipality	(0.007)	(0.013)	(0.006)	(0.202)	(0.007)	(0.011)	(0.008)	(0.006)	(0.007)	
s.e. clustered by rgi	(0.009)	(0.015)	(0.008)	(0.241)	(0.009)	(0.012)	(0.010)	(0.008)	(0.009)	(0.013)
PBF payments	0.169***	0.280***	0.194***	5.923***	0.168***	0.165***	0.155***	0.134***	0.168***	0.128***
s.e. clustered by municipality	(0.008)	(0.013)	(0.007)	(0.225)	(0.008)	(0.012)	(0.009)	(0.007)	(0.008)	
s.e. clustered by rgi	(0.010)	(0.016)	(0.009)	(0.263)	(0.010)	(0.013)	(0.010)	(0.009)	(0.009)	(0.013)
Private-sector formal employment	0.031*	0.068**	0.030*	0.786*	0.032*	0.029***	0.040**	0.033*	0.034*	0.036**
s.e. clustered by municipality	(0.017)	(0.029)	(0.018)	(0.469)	(0.017)	(0.011)	(0.017)	(0.017)	(0.018)	
s.e. clustered by rgi	(0.017)	(0.028)	(0.017)	(0.489)	(0.017)	(0.011)	(0.017)	(0.016)	(0.018)	(0.014)
Public-sector employment	0.068	-0.333	0.229	8.049	0.075	-0.166	0.077	-0.093	0.061	-0.002
s.e. clustered by municipality	(0.230)	(0.493)	(0.269)	(7.662)	(0.228)	(0.207)	(0.241)	(0.300)	(0.227)	
s.e. clustered by rgi	(0.249)	(0.545)	(0.239)	(8.009)	(0.248)	(0.223)	(0.258)	(0.332)	(0.247)	(0.013)

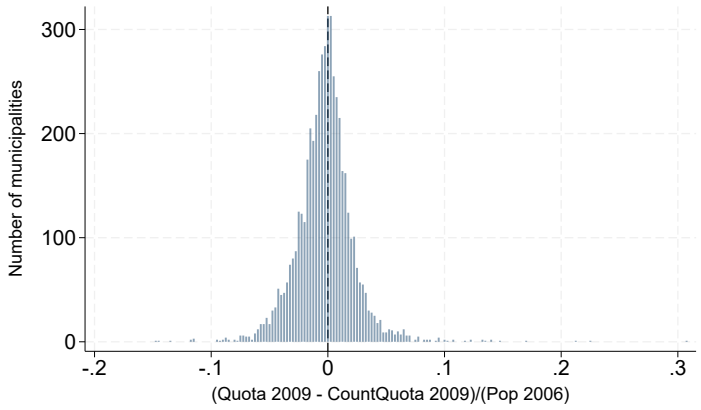
Notes: The table reports linear combinations of the DD coefficients (with standard errors in parentheses) that average the estimated impacts in 2010 and 2011, for specifications in log (panel A) and in growth rates (panel B). We present results for the same outcome variables as in Figure 2, and for the number of public-sector employees. In column (1), we summarize the results in Figure 2 by using the same specification, sample, and definition of the treatment variable. The other columns present robustness checks: we keep only municipalities in the top 25% and bottom 25% of our measure of treatment intensity (column 2); we assign municipalities to treatment and control groups based on the simple change in their 2009 quota compared to their 2006 quota (column 3); we use our measure of treatment intensity linearly (column 4); we include the largest municipalities in our analysis sample (column 5); we weight municipalities by their 2006 population (column 6); we re-weight municipalities such that the distribution of baseline program size is the same in the treatment and control groups (column 7); we exclude the set of baseline controls interacted with time fixed effects (column 8); we express the outcomes in per capita terms (column 9); and we aggregate the data and compute our measure of treatment intensity at the level of the "Immediate Geographic Regions" (*RGI*; column 10). The table includes for each estimate standard errors clustered by RGI in addition to the standard errors clustered by municipality.

TABLE B3: IMPLIED COST PER FORMAL JOB AND EMPLOYMENT MULTIPLIER

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

Notes: The table presents estimates (with their standard errors in parenthesis) of the cost per job – the inverse of the employment multiplier – and the number of jobs created for every $100k in PBF outlays – the employment multiplier times $100k – implied by the impact of the 2009 PBF expansion on private-sector formal employment. We report estimates using overall formal employment in the first row and focusing on low-wage workers (who drive our formal employment results) in the second row. The estimates in columns [1] and [2] consider all municipalities in our sample. Columns [3]-[6] present results for municipalities with higher (above median) and lower (below median) unemployment rates in the 2000 census, separately. Standard errors for the cost-per-job estimates are obtained using the delta method. All monetary values are in 2016 U.S. dollars (USD). We note that these results are not affected by any pre-existing differences in private-sector formal employment rates. Indeed, for the results in columns (3)-(6), we re-weight municipalities such that the distribution of formal employment rates prior to the 2009 PBF expansion is the same between treatment and control groups, and is the same as the distribution in our main sample (used in columns 1 and 2).

FIGURE B1: DISTRIBUTION OF OUR MEASURE OF TREATMENT INTENSITY

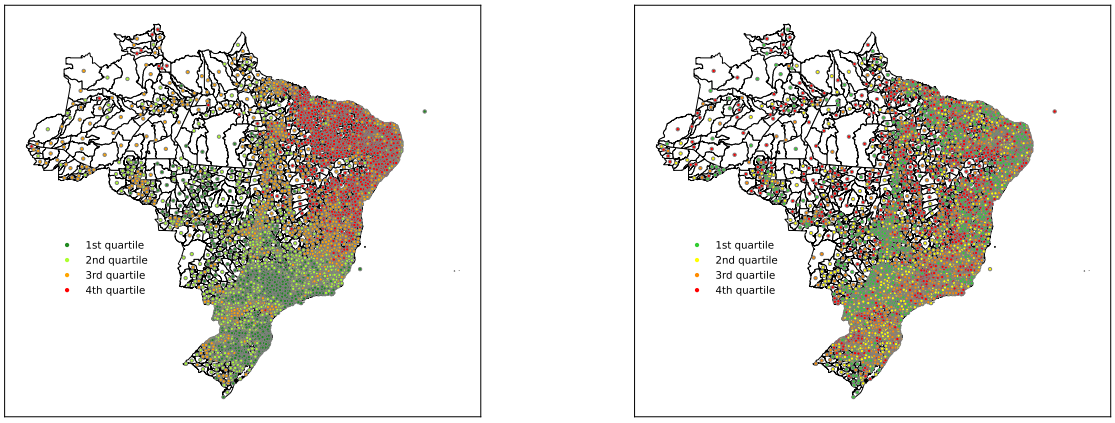


Notes: The figure displays the distribution of our measure of treatment intensity ($\Delta Quota_{ms}^{2009}$) across Brazilian municipalities..

FIGURE B2: GEOGRAPHICAL VARIATION IN THE SIZE OF PBF ACROSS MUNICIPALITIES PRE-REFORM AND IN OUR MEASURE OF TREATMENT INTENSITY

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

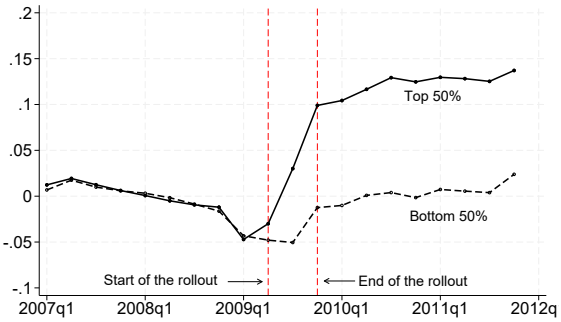
(b) Measure of treatment intensity ($\Delta Quota_{ms}^{2009}$)



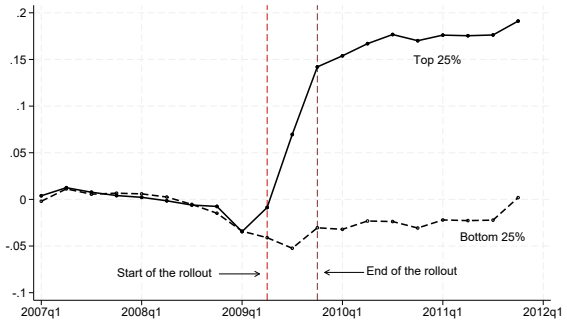
Notes: The figure compares the geographical variation in the size of PBF across municipalities prior to the 2009 PBF expansion and in our measure of treatment intensity, i.e., the relative change in municipal quota induced by the change in methodology in 2009. The map in panel (a) ranks municipalities 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 our measure of treatment intensity ($\Delta Quota_{ms}^{2009}$). Lines in the maps indicate the borders of each municipality; dots indicate the quartile in which the municipality belongs.

FIGURE B3: IMPACT ON PBF PAYMENTS AND FORMAL EMPLOYMENT - RAW DATA

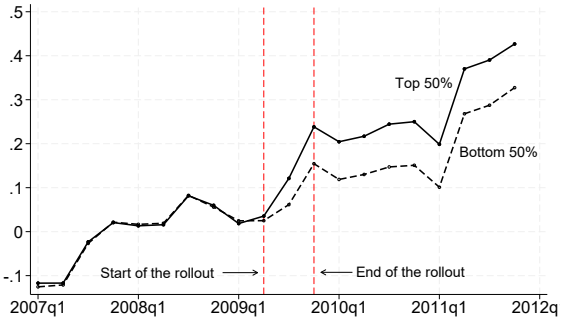
(a) Relative change in PBF beneficiaries - above vs. below p50



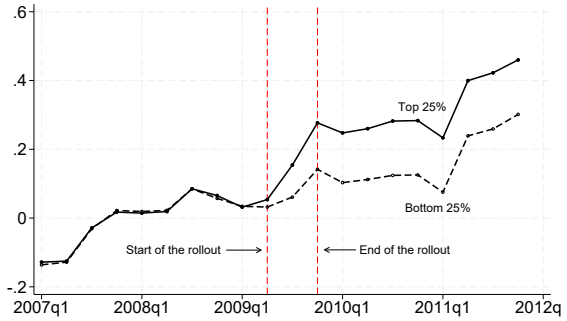
(b) Relative change in PBF beneficiaries - above p75 vs. below p25



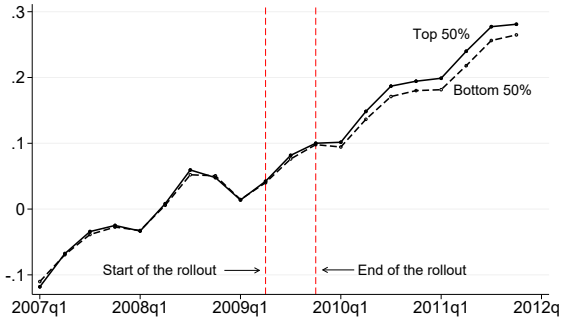
(c) Relative change in PBF payments - above vs. below p50



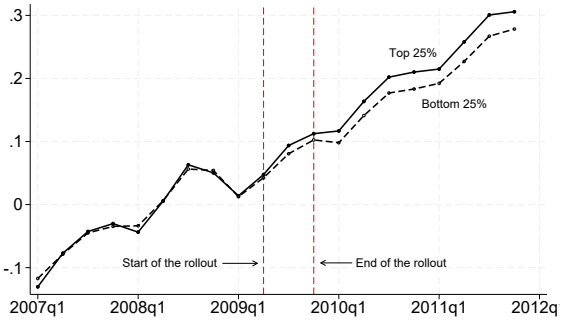
(d) Relative change in PBF payments - above p75 vs. below p25



(e) Relative change in private-sector formal employment - above vs. below p50

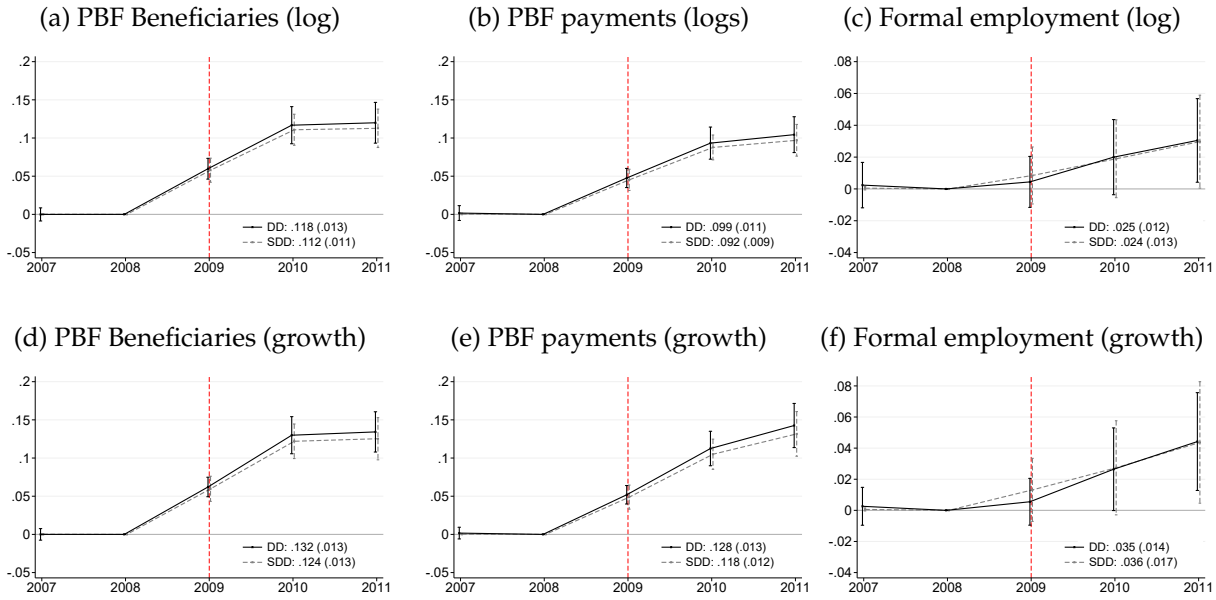


(f) Relative change in private-sector formal employment - above p75 vs. below p25



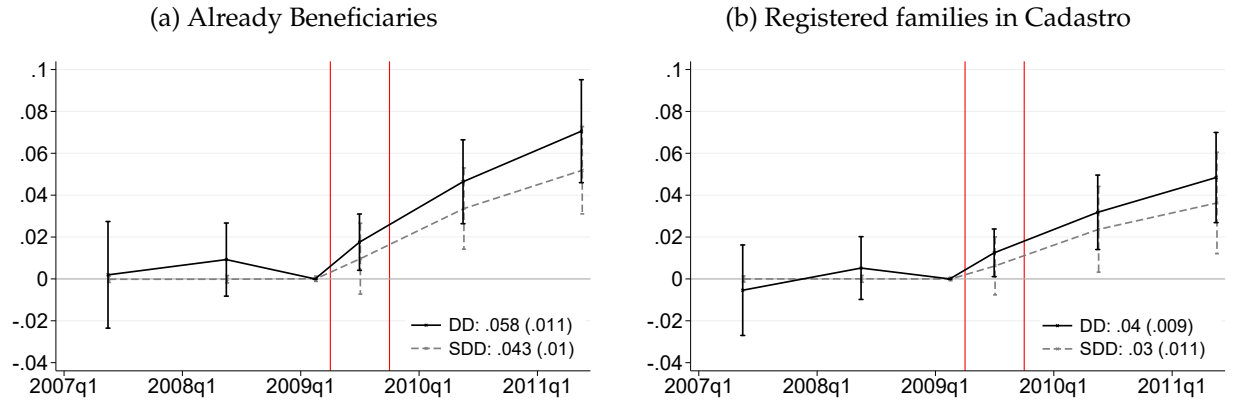
Notes: Panels (a) and (b) display the relative change (compared to the period Jan 2007-March 2009) in PBF beneficiaries for municipalities in the top 50% and bottom 50% of our measure of treatment intensity ($\Delta Quota_{ms}^{2009}$), and for municipalities in the top 25% and bottom 25% of our measure of treatment intensity, respectively. Panels (c) and (d) show the relative changes in PBF payments. Panels (e) and (f) show relative changes of number of private-sector formal employees. The vertical lines indicate the start and end of the rollout of the 2009 PBF expansion. There is an increase in PBF payments in 2011 because the value of the benefits increased in that year.

FIGURE B4: MAIN RESULTS AT THE *Regiões Geográficas Imediatas* (RGI) LEVEL



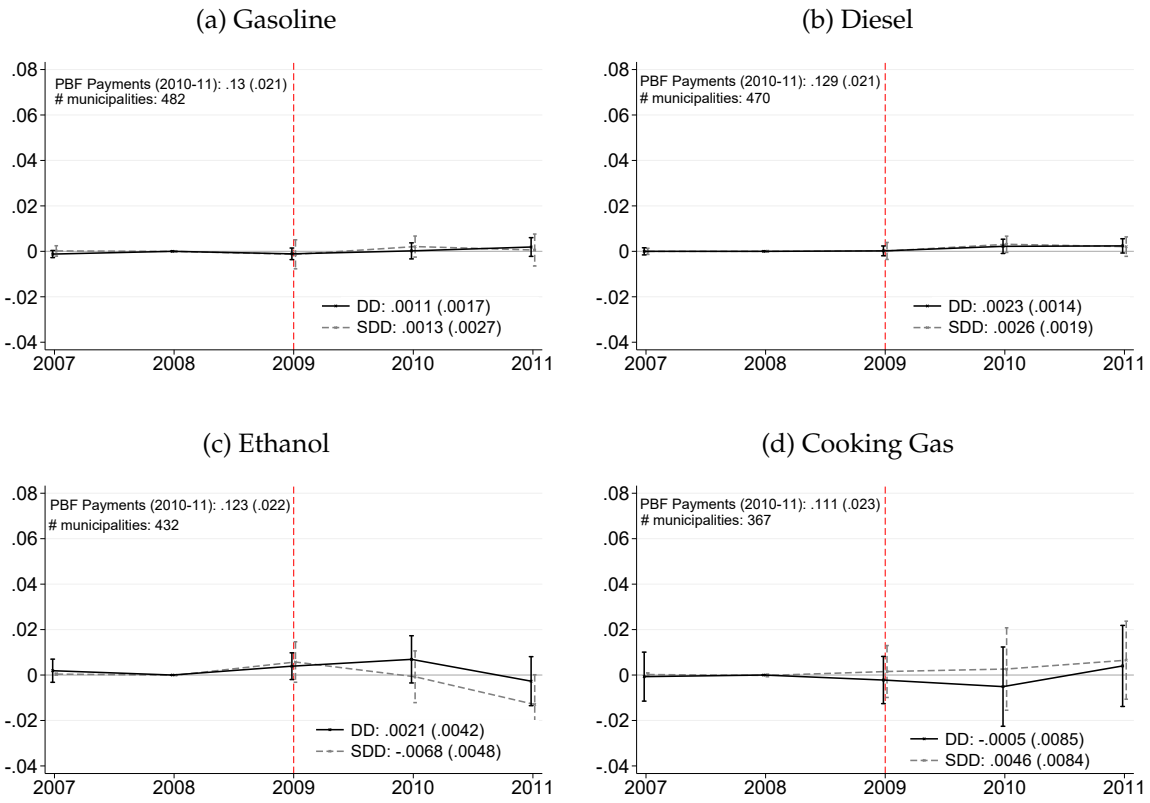
Notes: The figure present results aggregating the data used in Figure 2 at the RGI level, and re-defining treatment at the RGI level (top 50% and bottom 50% of the same measure of treatment intensity constructed at the RGI level). Panels (a)-(c) display DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the quarterly level for the log of these outcomes. For exposition purposes, we display linear combinations of our quarterly estimates averaging the estimated impacts in six time periods p: before the 2009 PBF expansion ($p = \{2007, 2008, 2009_{q1}\}$), during its roll-out ($p = \{2009_{q2-q4}\}$), and in the following years ($p = \{2010, 2011\}$). Panels (d)-(f) display results using a specification in growth rates at the yearly level. The vertical lines indicate the start and end of the rollout of the 2009 PBF expansion (panels a-c) or the year of the reform (panels d-f). We also report the average of the 2010 and 2011 coefficients on each graph (with standard errors in parentheses).

FIGURE B5: IMPACT ON FORMAL EMPLOYMENT FOR ALREADY BENEFICIARIES AND REGISTERED FAMILIES IN CADASTRO



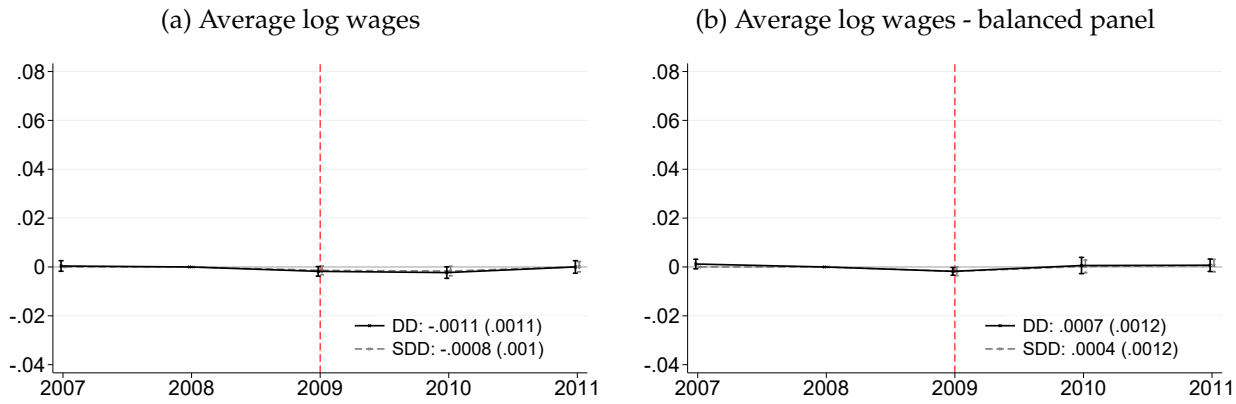
Notes: Panels (a) and (b) display DD coefficients (solid black lines) and SDID coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the quarterly level for the logarithm of private-sector formal employment for two separate groups: individuals who were already beneficiaries before the 2009 PBF expansion and all individuals registered in Cadastro Unico in December 2008 (including families that were not PBF beneficiaries), respectively. For exposition purposes, we display linear combinations of our quarterly estimates averaging the estimated impacts in six time periods p, which is sufficient to trace the evolution of the outcome before the 2009 PBF expansion ($p = \{2007, 2008, 2009_{q1}\}$), during its roll-out ($p = \{2009_{q2-q4}\}$), and in the following years ($p = \{2010, 2011\}$). The vertical lines indicate the start and end of the rollout of the 2009 PBF expansion.

FIGURE B6: IMPACT ON FUEL PRICES



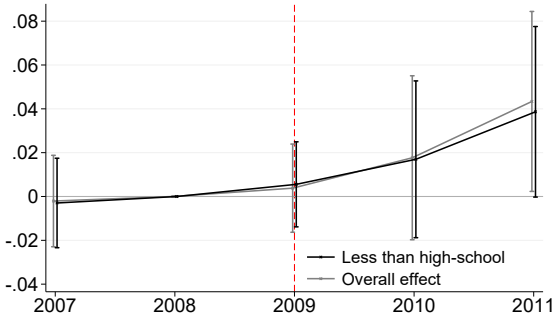
Notes: The figure displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level for the average of log retail prices for the following fuels: gasoline, diesel, ethanol and cooking gas. The data was obtained data from *Agência Nacional do Petróleo, Gás Natural e Biocombustíveis* (ANP) [National Agency of Petroleum, Natural Gas and Biofuels]. We restricted attention to prices for which we have at least 300 municipalities in the balanced panel. We re-defined as treatment municipalities those that fall in the top 50% of the distribution of our measure of treatment intensity ($\Delta Quota_{ms}^{2009}$) in the sample for which we have a balanced panel. In the panels, we display the DD estimate, the "first stage" effect on PBF payments for this subsample, and the number of municipalities in the panel.

FIGURE B7: IMPACT ON FORMAL WAGES, WORKERS EARNING LESS THAN 2 MINIMUM WAGES



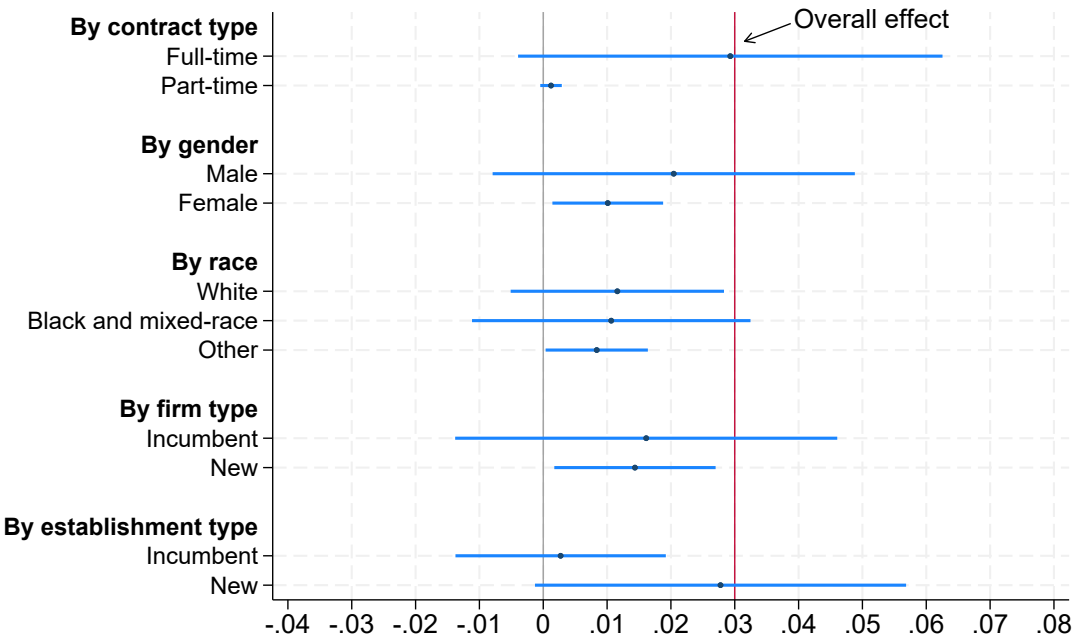
Notes: The figures displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level for the average of log wages. Panel (a) displays estimates for the wage of private-sector formal workers employed in December of each year earning less than two Minimum Wages to mitigate composition effects. Panel (b) displays comparable estimates for a balanced panel of workers earning less than two Minimum Wages employed throughout the sample period to shut down any remaining composition effect. The vertical lines indicate the year of the PBF expansion.

FIGURE B8: IMPACT ON FORMAL EMPLOYMENT FOR WORKERS WITH AT MOST HIGH-SCHOOL DEGREE



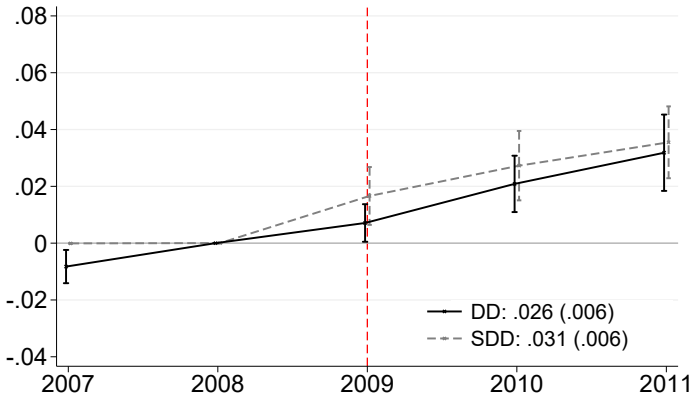
Notes: The figure shows the contribution of workers with no more than a high school degree to the overall impact on private-sector formal employment. It displays DD coefficients (with their 95% confidence intervals) from specifications in growth rates at the yearly level, where the dependent variable is the change in private-sector formal employment in a specific group, relative to the overall private-sector formal employment in 2008 (black lines). For comparison, in each panel, we also reproduce the estimates for the overall impact on private-sector formal employment from Figure 2f (gray line). The vertical lines indicate the year of the PBF expansion.

FIGURE B9: ANATOMY OF THE IMPACT ON FORMAL EMPLOYMENT



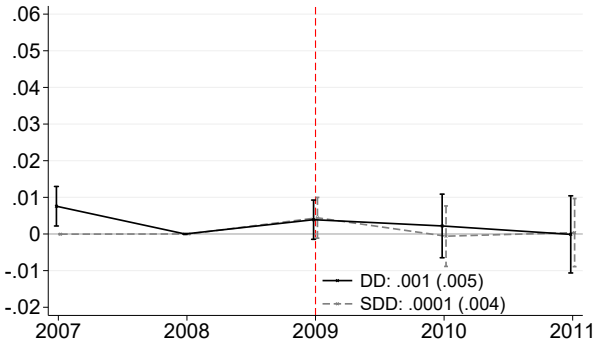
Notes: The figure displays the contribution of different subgroups of workers to the overall impact on private-sector formal employment. The reported coefficient is a linear combinations of our DD coefficients (with their 95% confidence intervals), averaging the estimated impact in 2010 and 2011 (as in Table B2). We estimate our preferred specification in equation (1), where the dependent variable is the change in private-sector formal employment in a specific group, relative to the overall private-sector formal employment in 2008. The point estimate for the overall effect is indicated by the red vertical line for reference. For each of the five decompositions presented in the figure, the sum of the coefficients add up to the overall effect.

FIGURE B10: IMPACT ON THE NUMBER OF ESTABLISHMENTS



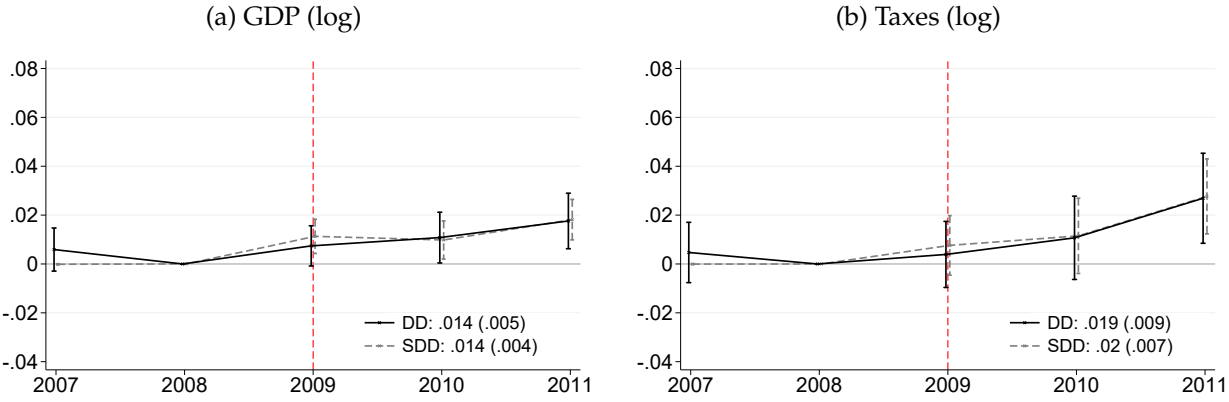
Notes: The figure displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level for the growth of the number of establishments in the Brazilian matched employee-employer data (i.e., with at least one formal employee). The vertical line indicates the year of the PBF expansion. The DD estimates indicate a pre-trend between our two groups of municipalities, but we obtain a similar treatment effect using the SDD estimator, indicating that the positive effect after 2009 is not systematically correlated with that pre-trend.

FIGURE B11: IMPACT ON VALUE ADDED: TRADABLE INDUSTRIES (GROWTH)



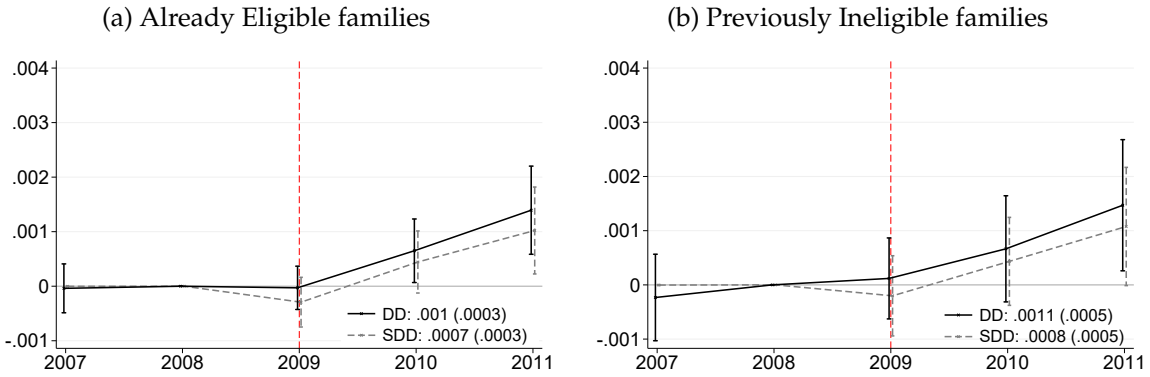
Notes: The figure displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level using national accounts data disaggregated at the level of each municipality. It displays the value added created by tradable industries, complementing Figure 5 in the paper. These estimates are all expressed in percentage of the municipal GDP in 2008. The vertical lines indicate the year of the PBF expansion.

FIGURE B12: IMPACT ON GDP AND TAXES ON GOODS AND SERVICES



Notes: The figure displays DD coefficients (solid black lines) and SDID coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level using national accounts data disaggregated at the level of each municipality. Panels (a) and (b) display results for the logarithm of municipal GDP and taxes on goods and services. The vertical lines indicate the year of the PBF expansion.

FIGURE B13: IMPACT ON FORMAL EMPLOYMENT RATE, ALREADY ELIGIBLE AND PREVIOUSLY INELIGIBLE FAMILIES IN *Cadastro Único* 2008



Notes: Panels (a) and (b) display DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level for the formal employment rate among two group of families: those who were "already eligible" prior to the 2009 expansion, i.e., those who would have been eligible in 2010 based on their income per capita in *Cadastro Único* 2008 (panel a) and those who were "previously ineligible" prior to the 2009 expansion, i.e., those who were registered in *Cadastro Único* 2008 with income per capita too high to be eligible in 2010. The vertical line indicates the year of the PBF expansion. Already eligible families accounted for most of the increased PBF payments following the 2009 PBF expansion. Yet, their formal employment rate increased to a similar extent to that of previously ineligible families – who accounted for only a very small share of the increased PBF payments – after the 2009 PBF expansion.

Online Appendix C - Aggregate Impacts of the reform - Other Dimensions of Economic Activity

In this Appendix, we show the impact of the 2009 PBF expansion on other measures of economic activity: banking activity (bank deposits, credit and loans), electricity use by households (a measure of consumption), electricity use by firms in non-tradable industries (another production input, besides labor), and vehicle registration (a physical asset). Together with the employment effects and GDP results that we document in the paper, these findings are consistent with a demand multiplier mechanism, in which PBF transfers are spent locally, increasing the incomes of local goods and service providers, which further stimulates economic activity.

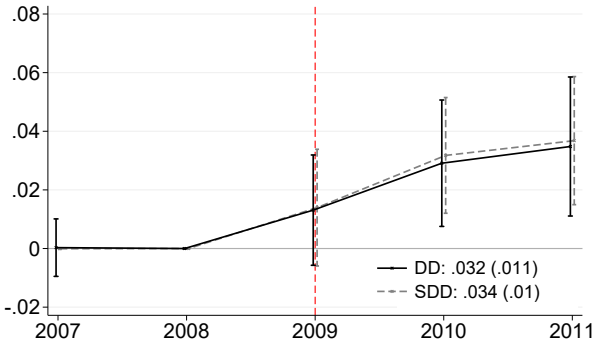
To study these additional outcomes, we complement the data used in the paper with additional sources of administrative data at the municipal level to document changes in local economic activity over time that are consistent with our main findings: (i) data on total bank deposits (current accounts and savings) and total credit and loans reported by every bank branch in the country to the Brazilian Central Bank; (ii) data on electricity consumption for residential and commercial customers for 60% of the Brazilian population;[1] (iii) data on the fleet of vehicles registered with the Department of Motor Vehicles (*DETRAN*).

Figures C1a and C1b begin by documenting significant increases in banking activity in response to the 2009 PBF expansion. DD estimates in Figure C1a show that treatment and control groups shared a common trend in total bank deposits between 2007 and 2008. However, the amounts held in checking and savings accounts started to increase relatively more in treatment municipalities in 2009. This increase in banking activity is not mechanically driven by the higher PBF payments. While it is the case that, for most PBF beneficiaries, their monthly benefits are deposited in a bank account at *Caixa*, the main state bank in Brazil, the results are similar if we exclude *Caixa* accounts (Figure C1c). Moreover, Figure C1b shows that credit and loans also increased in treatment municipalities after 2009. DD estimates feature a differential pre-trend between our two groups of municipalities, but we find comparable results for 2010 and 2011 using the SDD estimator, indicating that the relative increase in credit and loans after 2009 is not systematically correlated with that pre-trend.

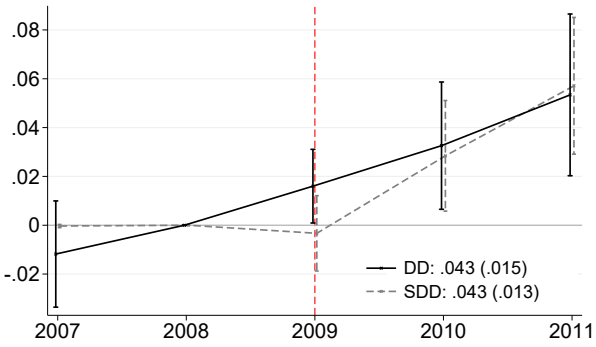
[1] We collected data from 11 of the 27 Brazilian states. There are several electricity distributors in Brazil, and the data are not consolidated by a single source. To create municipal panel data, we reached out to each of the providers by state. While some did not respond or declined, we successfully obtained data from major states like São Paulo, Pernambuco, and Bahia (in addition to Santa Catarina, Alagoas, Espírito Santo, Ceará, Goiáis, Mato Grosso do Sul, Paraná, and Rio Grande do Sul).

FIGURE C1: BANKING ACTIVITY

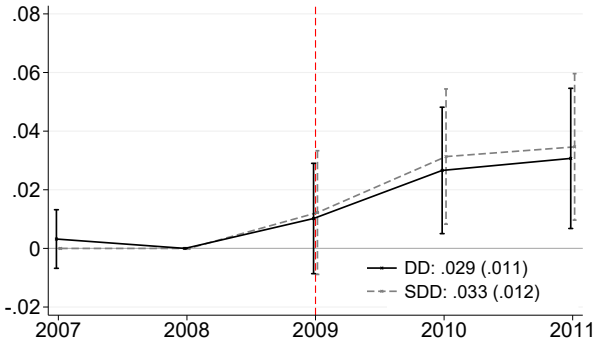
(a) Checking and savings accounts



(b) Credit and loans



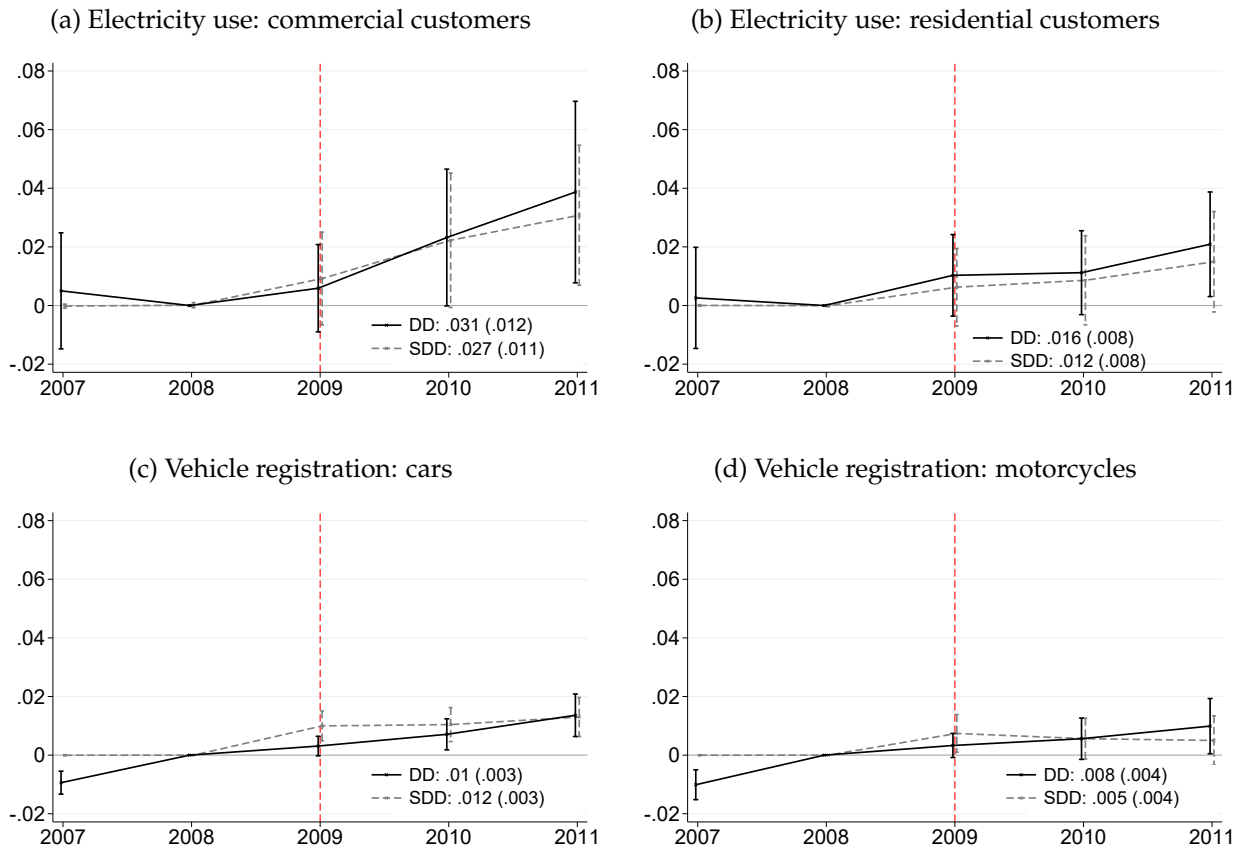
(c) Checking and savings accounts, excluding accounts held at Caixa



Notes: The figure displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level for the logarithm of various municipal outcomes. We consider the value of deposits in checking and savings accounts (panel a), the value of bank credit and loans (panel b), checking and savings accounts excluding accounts held at *Caixa* (panel c), the main state bank where most PBF beneficiaries receive their PBF benefits. The vertical lines indicate the year of the PBF expansion.

Figures C2c and C2d show that electricity use by commercial and residential customers also started to increase relatively more in treatment municipalities in 2009. For commercial customers, which correspond to firms in non-tradable industries (i.e., excluding agriculture and manufacturing), electricity consumption captures another variable production input, besides the number of formal employees. Electricity use by residential customers is a rare measure of non-durable consumption at the municipal level.

FIGURE C2: OTHER DIMENSIONS OF ECONOMIC ACTIVITY



Notes: The figure displays DD coefficients (solid black lines) and SDD coefficients (dashed gray lines) with their 95% confidence intervals from estimating our preferred specification in equation (1) at the yearly level for the logarithm of various municipal outcomes. We consider the value of deposits in checking and savings accounts (panel a), the value of bank credit and loans (panel b), electricity use by commercial customers, which correspond to firms in non-tradable industries (panel c), electricity use by residential customers (panel d), the number of cars – and other 4-wheel vehicles such as SUVs – registered (panel e), and the number motorcycles – and other 2-wheel vehicles such as scooters – registered (panel f). The vertical lines indicate the year of the PBF expansion.

Figures C2e and C2f present results for the number of cars and motorcycles registered, which are the only forms of physical assets with data available at the municipal level for our study period. DD estimates imply a relative increase for both variables in treatment municipalities after 2009, although they also reveal a differential pre-trend. This does not appear to be a concern in the case of cars because the results are robust to using the SDD estimator. However, SDD estimates are no longer significant at conventional levels by 2011 for motorcycles, so this result is more tentative.

In sum, several dimensions of economic activity increased following the 2009 PBF expansion. Taken together, these results corroborate that the policy change led to higher local economic activity in Brazil and are consistent with a demand-driven mechanism.

Online Appendix D - Micro-level evidence: impact of PBF benefits on labor supply

In this Online Appendix, we examine micro-level formal labor supply responses to PBF. The relative increase in the number of eligible families that we document in Section 4 of the paper could result from several margins of behavioral responses. Families with income per capita below the eligibility thresholds could become more likely to register, families could underreport their income per capita, or families could reduce their labor supply – particularly in the formal sector where income is more readily observed by the government – to remain or become eligible. Among these three margins, labor supply responses are particularly relevant in the context of our means-tested study.

Indeed, it remains an open question whether the local demand effects of PBF occur despite negative 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 them in the second case. As we mention in the paper, it is challenging to quantify the impact of the 2009 PBF expansion on the labor supply of families who became beneficiaries at the time, even if we focus on formal employment (which we can observe). Indeed, one would have to find a suitable control group within the same municipality to net out demand effects.

In this Appendix, we leverage cross-sectional variation in PBF eligibility to show that concerns about negative formal labor supply responses to means-testing are relevant in our context, which is in line with the literature in Latin America. For instance, Bergolo and Cruces (2021) find that households who became eligible for a means-tested cash transfer in Uruguay experienced a relative decrease in formal labor supply and a relative increase in both informal employment and unemployment. The results in this Appendix thus suggest that the positive aggregate effect on formal employment following the 2009 PBF expansion likely occurred despite negative formal labor supply responses among some beneficiaries.

A. Research design. We use 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 Único* rises above the extreme poverty line (R$70), the basic benefit may be "taxed away." We also exploit the discontinuity at the poverty line (R$140) used to determine eligibility for the variable benefits. Concretely, we compare families with income per capita just below and just above the eligibility cutoffs in August 2010, 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, 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 Único* and a loss of benefit eligibility. This disincentive was likely binding in the period that we study, which was characterised by economic growth in Brazil and an overall increase in formal employment. In fact, the evidence in Bergolo and Cruces (2021), where beneficiaries faced a similar macroeconomic environment and similar incentives, came from a reduction in formal employment growth and not in employment levels.

In principle, families above the cutoffs also have an incentive to decrease their formal employment to more easily adjust downward their income per capita in *Cadastro Único* to meet the eligibility criteria for (higher) PBF benefits. However, if there is suspicion that a family is deliberately adjusting their reported income downward, they can be investigated and excluded from *Cadastro Único* (MDS, 2010). Moreover, families have no guarantees that they will become PBF beneficiaries even if they become eligible given that the national number of slots was binding over the period of our study. The results in column (3) of Table 2 in the paper show that, even when the chance to become a PBF beneficiary increases in a municipality, previously ineligible families are no more likely to decrease their income per capita in *Cadastro Único* in treatment municipalities compared to control municipalities. In the RD analysis below, any incentive to decrease formal employment among families located above one of the 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 aggregate formal employment in the paper is driven by demand effects.

B. 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 Único* 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 Único* (see the discussion in Section 2 of the paper).

Figure D1 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 D1a and D1b (200 levels in each case). Figure D2 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:

$$Y_i = f(\widetilde{Z}_i) + \beta D_i + \varepsilon_i, \tag{1}$$

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 Único* , Z_0 is either the extreme poverty line or the poverty line, $f(.)$ is a linear function of the normalized income per capita $\widetilde{Z}_i = Z_i - Z_0$ (the slope is allowed to differ between positive and negative values of $\widetilde{Z}_i$) and D_i 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 D6.

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 Único* . Specifically, Figures D3 and D4 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 D1 also presents the $\widehat{\beta}$ coefficients from estimating the specification in equation (1) 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.

C. Results. We construct three main outcome variables following families for the 12-month period after the August 2010 snapshot of *Cadastro Único*: the total PBF benefits received, the total number of months observed in formal employment across all family members, and the total family income from formal employment. Figure D5 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 (1). Panel B in Table D1 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 (1), as well as municipality fixed effects.

We find clear evidence that families that are eligible for higher PBF benefits indeed received higher benefits in the next 12 months. We estimate an increase of R$437.9 in PBF benefits at the extreme poverty line and an increase of R$219.5 in PBF benefits at the poverty line, or increases of 95.9 percents and 184.8 percents, respectively. Adding controls does not affect our point estimates, but it greatly improves their precision.

We find no evidence that receiving higher PBF benefits increases formal employment outcomes. The point estimates are negative for both the number of months in formal employment and formal employment income. The discontinuity is not evident in Figure D5, 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 D5 do not contribute equally to the estimation given the non-smooth distribution of the running variable.

The corresponding RD estimates reported in Table D1 ("no controls") are economically large, but imprecisely estimated. Adding controls greatly improves precision again, and the RD estimates become statistically significant at conventional levels. The point estimates remain economically large. For instance, they imply an 8.7 percent reduction in the number of months in formal employment and a 9.3 percent reduction in formal employment income at the extreme poverty line; the corresponding figures are 9.8 percent and 13.5 percent at the poverty line. These effects most likely reflect a substitution effect. Income effects are typically zero in similar contexts (e.g., Banerjee et al. 2017), and another study 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).

We carried out two sets of robustness checks. First, we estimated the same RD specifications (with controls) assuming that the cutoffs were located at each of the R$0.25 income

per capita levels displayed in Figures D1a and D1b. Figure D2 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 months in formal employment at the poverty line. Second, Figure D7 shows that our point estimates are essentially unchanged if we vary the bandwidth size.

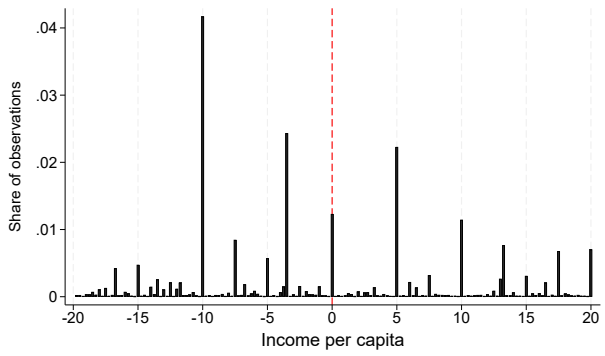
TABLE D1: IMPACT OF PBF BENEFITS ON BENEFICIARIES' FORMAL LABOR SUPPLY

	[1]	[2]	[3]	[4]
	Extreme Poverty Cutoff		Poverty Cutoff	
	RD estimate	(s.e.)	RD estimate	(s.e.)
A. Covariates (August 2010)				
Family size	-0.0665	[0.535]	0.0483	[0.441]
Number of rooms in dwelling	-0.0481	[0.0801]	-0.0722	[0.0717]
Living in rural area (dummy)	-0.00456	[0.00843]	0.00626	[0.00528]
Receives any pension or UI benefit (dummy)	-0.0109	[0.0432]	0.0732	[0.0634]
Share of women in household	-0.00127	[0.0149]	-0.00547	[0.0201]
Share of adults who completed high school	-0.00193	[0.0117]	0.0129	[0.0122]
B. Outcomes (September 2010-August 2011)				
PBF benefits				
No controls	437.9***	[90.01]	219.5***	[60.94]
Covariates + municipal fixed effects	422.9***	[34.52]	214.5***	[34.82]
Months in formal employment				
No controls	-0.667	[0.719]	-0.934	[1.351]
Covariates + municipal fixed effects	-0.440*	[0.235]	-0.695*	[0.368]
Income from formal employment				
No controls	-613.5	[552.6]	-1,141	[1,144]
Covariates + municipal fixed effects	-400.7**	[194.9]	-876.3***	[297.4]

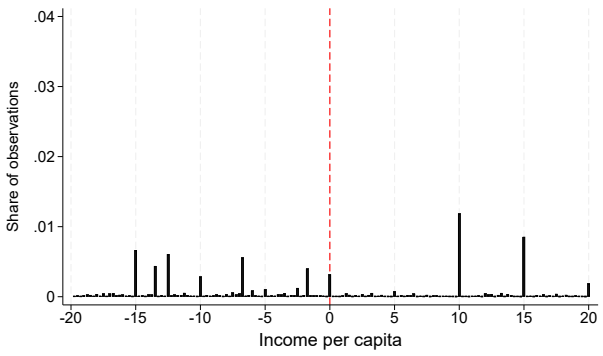
Note: The table presents results for the impact of PBF benefits on beneficiaries' formal labor supply. It reports regression discontinuity estimates (with standard errors in parentheses) that compare families with income per capita on different sides of either the extreme poverty line (columns [1] and [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 (1). 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 (1) ("No controls") and a specification that controls for the covariates in panel A as well as municipality fixed effects.

FIGURE D1: DISTRIBUTION OF MONTHLY INCOME PER CAPITA IN CADASTRO UNICO

(a) Extreme Poverty line (R$70)



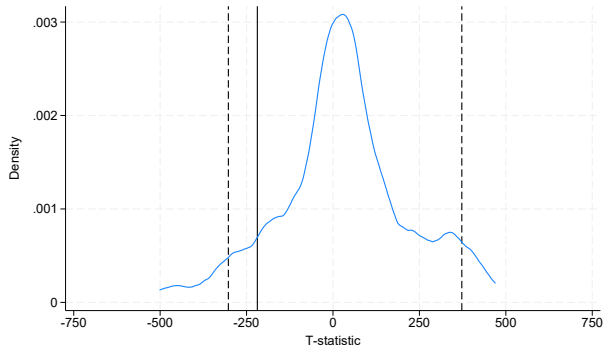
(b) Poverty line (R$140)



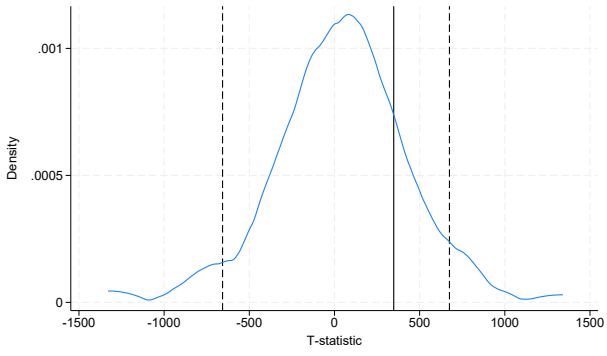
Notes: This figure plots the distribution of monthly income per capita in the August 2010 snapshot of Cadastro Unico, by bins of R$0.25 around the extreme poverty line (R$70) in panel (a) and around the poverty line (R$140) in panel (b). The red vertical line in each panel indicates the level of the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

FIGURE D2: DENSITY MANIPULATION TEST AROUND THE PBF ELIGIBILITY CUTOFFS

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

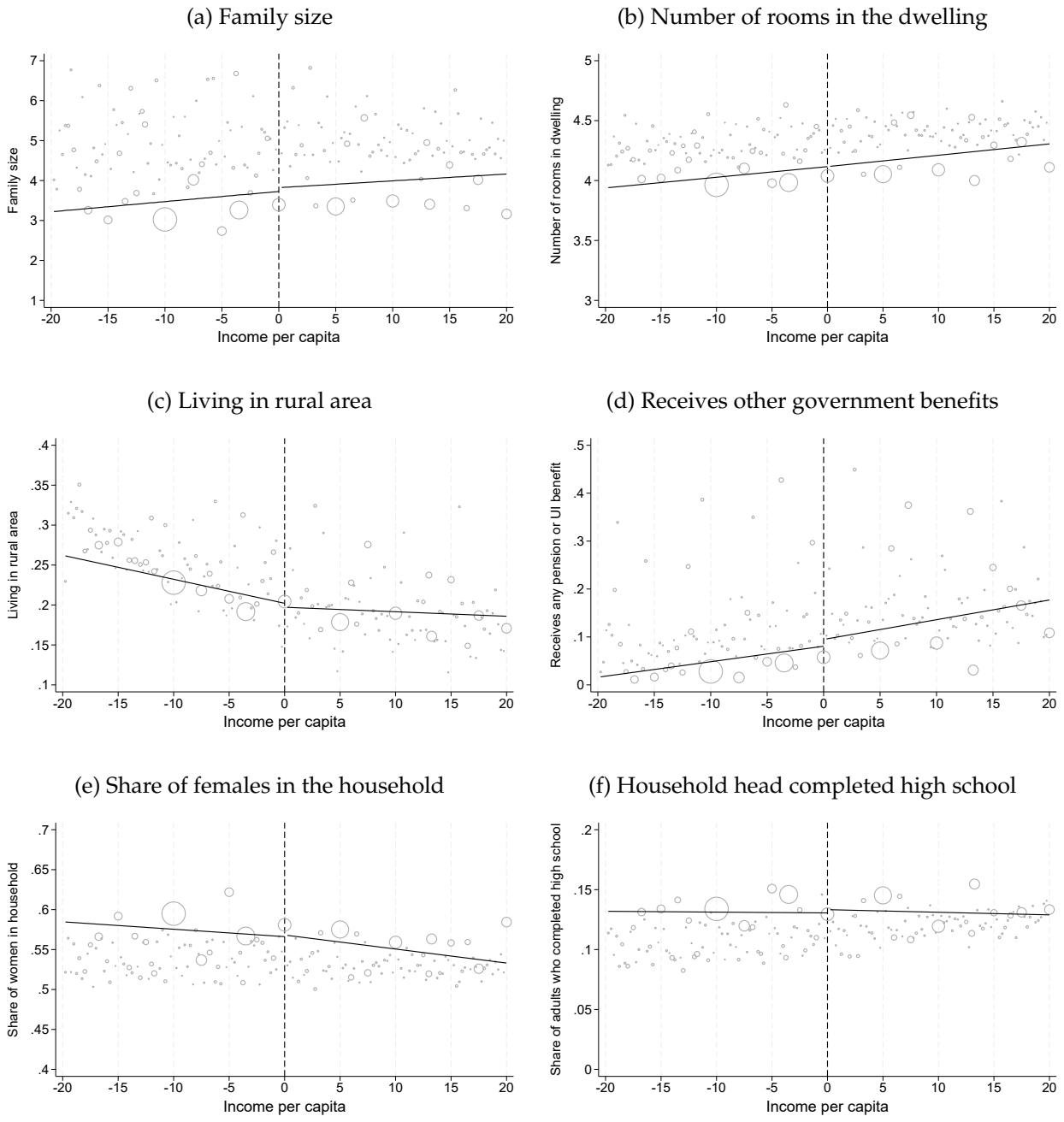


(b) Distribution of T-statistics for the test across income per capita levels around the poverty line (R$140)



Notes: 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 Figure D1a (around the extreme poverty line) and D1b (around the poverty line) in the paper (see, also, Cattaneo et al. 2018). Panels (a) and (b) plot the distribution of the t-statistics at each income per capita level. 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).

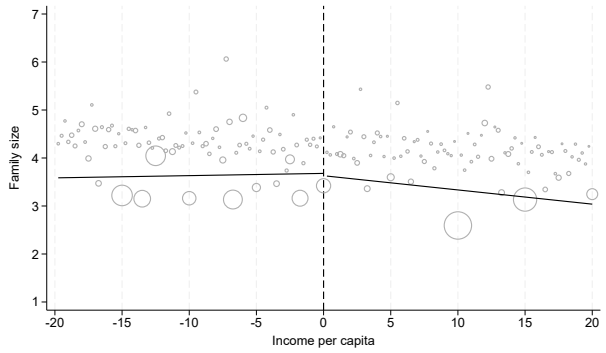
FIGURE D3: COVARIATES AROUND THE EXTREME POVERTY LINE



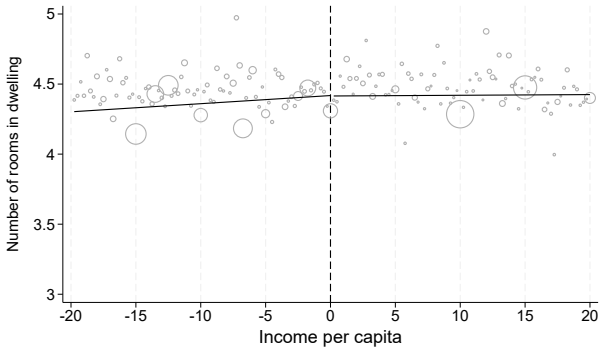
Notes: The figure displays averages of six pre-determined variables by income per capita bins of R$0.25 around the Extreme Poverty line. In each panel, it also displays the linear fit on each side of the cutoffs from estimating the regression discontinuity specification in equation (1). Income per capita 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 D4: COVARIATES AROUND THE POVERTY LINE

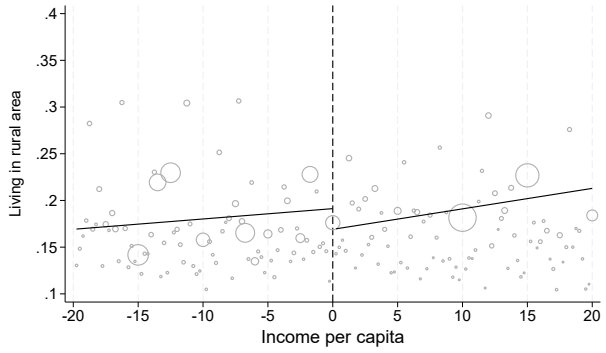
(a) Family size



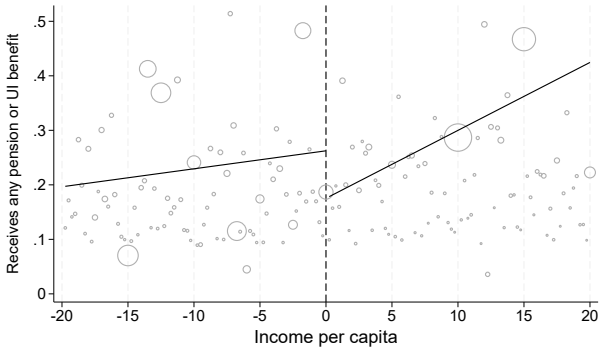
(b) Number of rooms in the dwelling



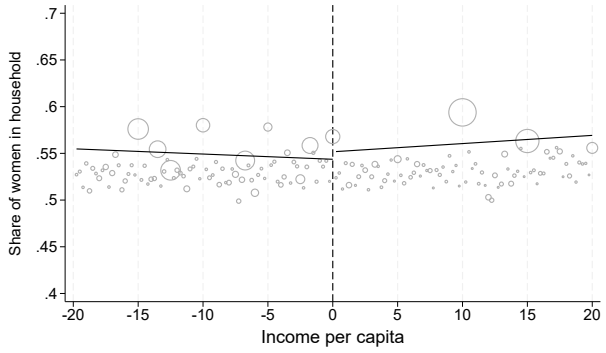
(c) Living in rural area



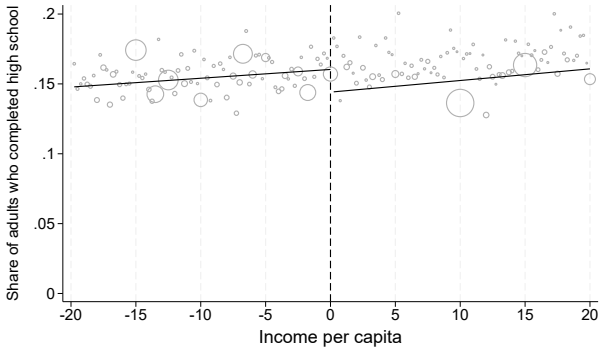
(d) Receives other government benefits



(e) Share of females in the household



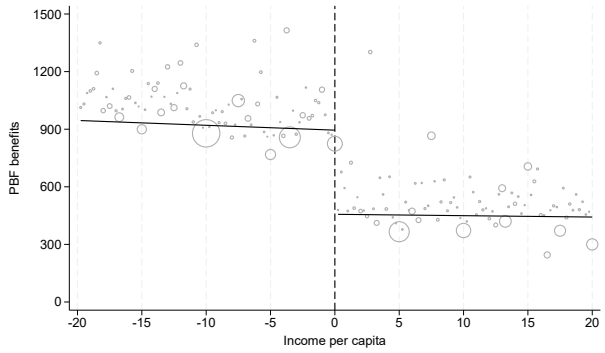
(f) Household head completed high school



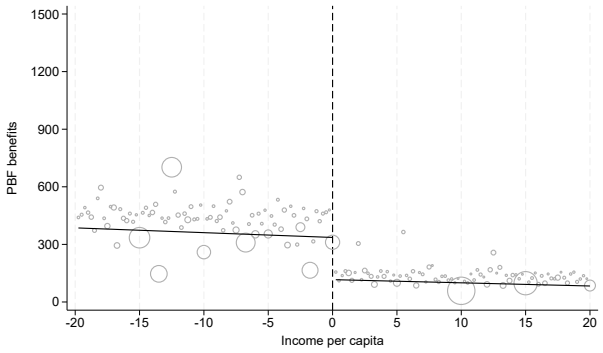
Notes: The figure displays averages of six pre-determined variables by income per capita bins of R$0.25 around the Poverty line. In each panel, it also displays the linear fit on each side of the cutoffs from estimating the regression discontinuity specification in equation (1). Income per capita is 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 D5: PBF BENEFITS AND BENEFICIARIES' FORMAL LABOR SUPPLY

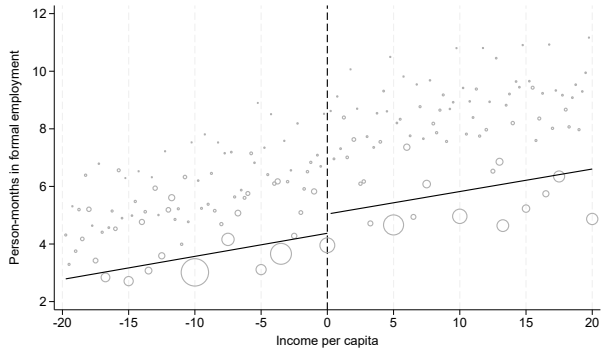
(a) PBF benefits (EP)



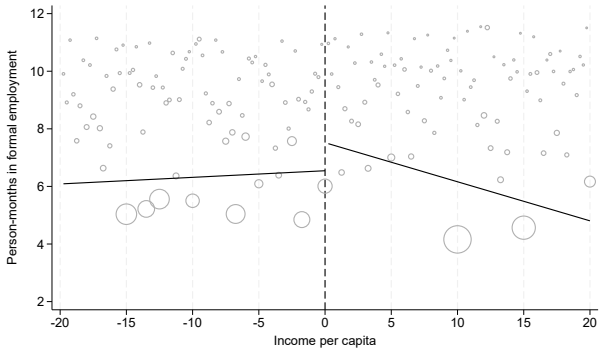
(b) PBF benefits (P)



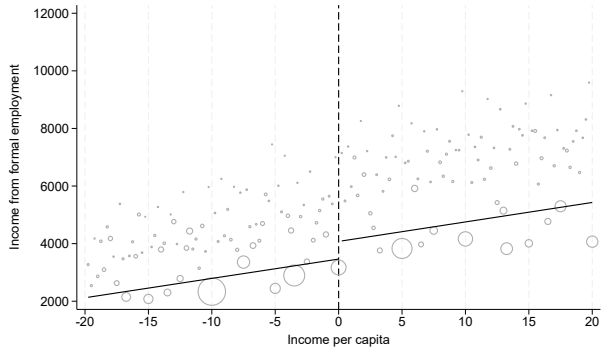
(c) Adult-months in formal employment (EP)



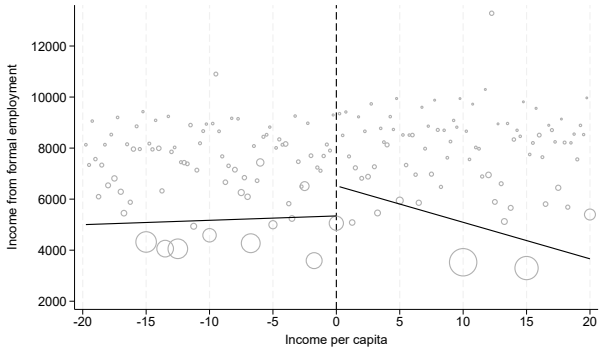
(d) Adult-months in formal employment (P)



(e) Income from formal employment (EP)



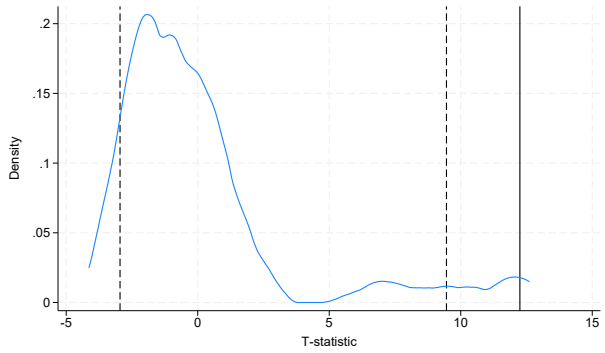
(f) Income from formal employment (P)



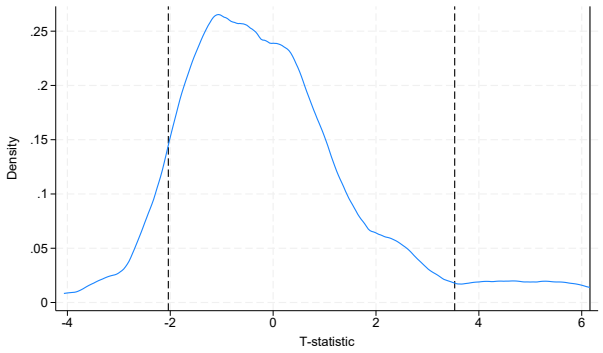
Notes: The figure displays averages of our three outcome variables by income per capita bins of R$0.25 around the Extreme Poverty (EP) line and around the Poverty (P) line. Each panel also displays the linear fit on each side of the cutoffs from estimating the regression discontinuity specification in equation (1). 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).

FIGURE D6: DISTRIBUTION OF T-STATISTICS FOR RD ESTIMATES ACROSS INCOME PER CAPITA LEVELS

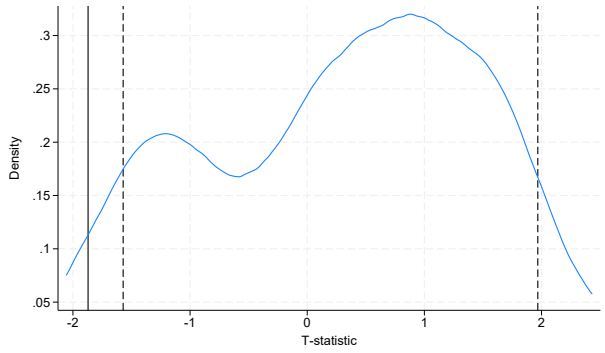
(a) PBF Benefits (EP)



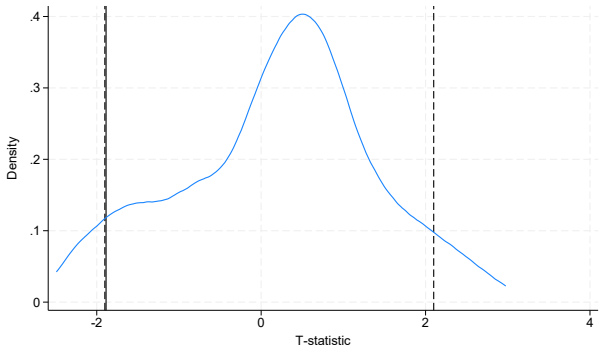
(b) PBF Benefits (P)



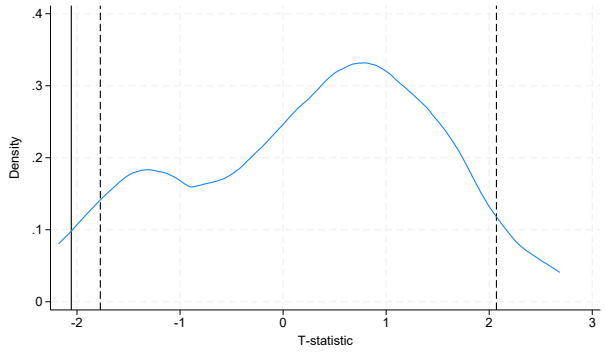
(c) Adult-months in formal employment (EP)



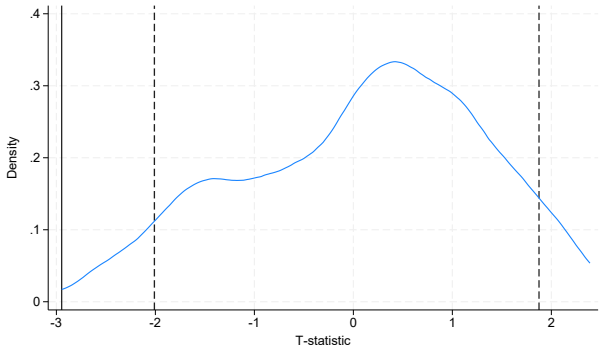
(d) Adult-months in formal employment (P)



(e) Income from formal employment (EP)



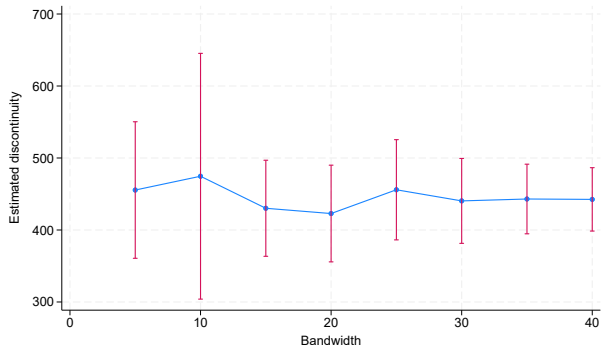
(f) Income from formal employment (P)



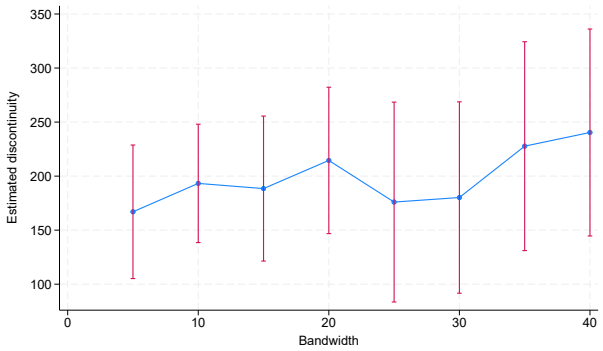
Notes: The figure displays the distribution of the T-statistics from estimating the same RD specifications (with controls) as in Figure D5 but assuming that the cutoffs were located at each of the R$0.25 income per capita levels displayed in Figures D1a and D1b.

FIGURE D7: ROBUSTNESS OF RD ESTIMATES WITH RESPECT TO THE BANDWIDTH SIZE

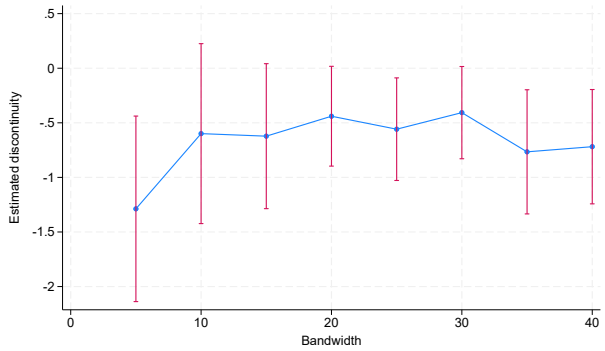
(a) PBF Benefits (EP)



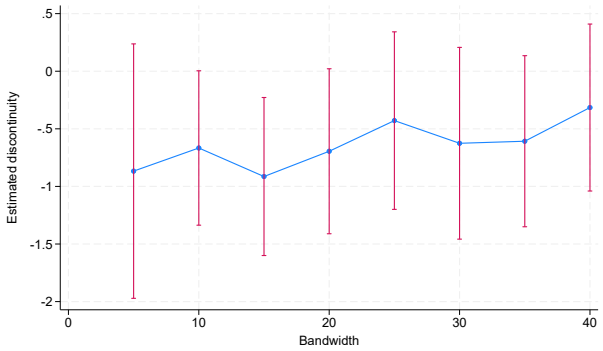
(b) PBF Benefits (P)



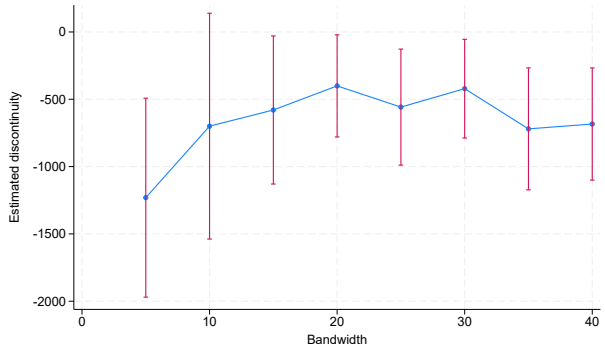
(c) Adult-months in formal employment (EP)



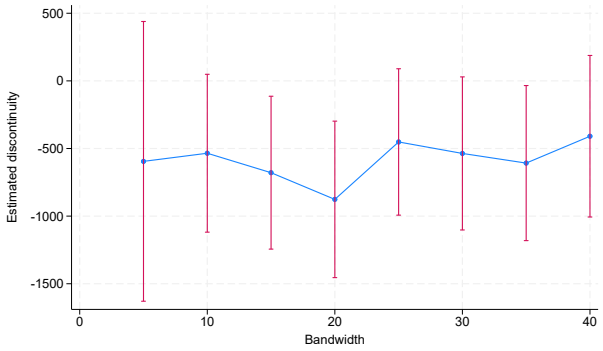
(d) Adult-months in formal employment (P)



(e) Income from formal employment (EP)



(f) Income from formal employment (P)



Notes: The figure displays RD estimates obtained by estimating the same specification as for the results in panel B in Table D1 (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.

Online Appendix E: Linking output and employment multipliers

In Section 4.2, we adapt the methodology relating output and employment multipliers in Chodorow-Reich (2019) to our results: an increase in *formal* employment concentrated among *low-wage* workers. This section derives the expression in equation (3) from the neoclassical production function for the economy: $Y = A \cdot (N \cdot L)^{1-\alpha}$, where N denotes hours per worker and L the number of effective units of labor. While L equals total employment E in Chodorow-Reich (2019), we introduce heterogeneity in hourly productivity (ψ) across three worker types (as in Cunha et al. 2022): $N \cdot L = N_{FE_l} \cdot \psi_{FE_l} \cdot FE_l + N_{FE_h} \cdot \psi_{FE_h} \cdot FE_h + N_{IW} \cdot \psi_{IW} \cdot IW$, with FE_l, FE_h, and IW the number of low-wage formal employees, high-wage formal employees, and informal workers, respectively.

The change in output Y following a change in cash transfer T is then:

$$\begin{aligned}
\frac{dY}{dT} =& A \cdot (1-\alpha) \cdot (N \cdot L)^{-\alpha} \cdot \frac{d(N \cdot L)}{dT} \\
=& (1-\alpha) \cdot \frac{Y}{N \cdot L} \cdot \left[\sum_{J \in \{FE_l, FE_h, IW\}} \psi_J \cdot \left(\frac{dN_J}{dT} \cdot J + N_J \cdot \frac{dJ}{dT} \right) \right] \\
=& (1-\alpha) \cdot (1+\chi) \cdot \frac{Y}{N \cdot L} \cdot \left[\psi_{FE_l} \cdot N_{FE_l} \cdot \frac{dFE_l}{dT} + \psi_{FE_h} \cdot N_{FE_h} \cdot \frac{dFE_h}{dT} + \psi_{IW} \cdot N_{IW} \cdot \frac{dIW}{dT} \right] \\
=& (1-\alpha) \cdot (1+\chi) \cdot \frac{Y}{E} \cdot \left[\frac{\psi_{FE_l} \cdot N_{FE_l} \cdot \frac{dFE_l}{dT} + \psi_{FE_h} \cdot N_{FE_h} \cdot \frac{dFE_h}{dT} + \psi_{IW} \cdot N_{IW} \cdot \frac{dIW}{dT}}{(N \cdot L)/E} \right] \\
=& (1-\alpha) \cdot (1+\chi) \cdot \frac{Y}{E} \cdot \left[\kappa_{FE_l} \cdot \frac{dFE_l}{dT} + \kappa_{FE_h} \cdot \frac{dFE_h}{dT} + \kappa_{IW} \cdot \frac{dIW}{dT} \right]
\end{aligned}$$

where χ denotes the elasticity of hours per worker to employment and κ captures each worker type's productivity relative to the average worker in the economy: $\kappa_J = \frac{\psi_J \cdot N_J}{(N \cdot L)/E}$ for $J \in \{FE_l, FE_h, IW\}$. The third line assumes that the change in hours is proportional to the change in employment as in Chodorow-Reich (2019): $\frac{dN_J}{dT} \cdot J = \frac{dJ}{dT} \cdot N_J \cdot \chi$, with $\chi = \frac{dN_J}{N_J} \cdot \frac{J}{dJ}$. Using the multiplier notation (μ) in the paper, the above expression becomes:

$$\mu_Y = (1-\alpha) \cdot (\chi + 1) \cdot \frac{Y}{E} \cdot (\kappa_{FE_l} \cdot \mu_{FE_l} + \kappa_{FE_h} \cdot \mu_{FE_h} + \kappa_{IW} \cdot \mu_{IW})$$

We note that we only need to know the relative productivities $\kappa_{FE_h}/\kappa_{FE_l}$ and $\kappa_{IW}/\kappa_{FE_l}$ – and each worker type's share of total employment – to pin down the level of the three

κ_j's. Indeed, one can write:

$$\mu_Y = (1-\alpha)\cdot(\chi+1)\cdot\frac{Y}{E}\cdot\kappa_{FE_l}\cdot\left(\mu_{FE_l}+\frac{\kappa_{FE_h}}{\kappa_{FE_l}}\cdot\mu_{FE_h}+\frac{\kappa_{IW}}{\kappa_{FE_l}}\cdot\mu_{IW}\right)$$

Moreover, we have:

$$\begin{aligned}\kappa_{FE_l} &= \frac{\psi_{FE_l}\cdot N_{FE_l}}{\left(N_{FE_l}\cdot\psi_{FE_l}\cdot FE_l + N_{FE_h}\cdot\psi_{FE_h}\cdot FE_h + N_{IW}\cdot\psi_{IW}\cdot IW\right)/E}\\ &= \frac{1}{\left(\frac{FE_l}{E}+\frac{\kappa_{FE_h}}{\kappa_{FE_l}}\cdot\frac{FE_h}{E}+\frac{\kappa_{IW}}{\kappa_{FE_l}}\cdot\frac{IW}{E}\right)}\end{aligned}$$

where the second line uses: $\frac{\kappa_{FE_h}}{\kappa_{FE_l}} = \frac{\psi_{FE_h}\cdot N_{FE_h}}{\psi_{FE_l}\cdot N_{FE_l}}$ and $\frac{\kappa_{IW}}{\kappa_{FE_l}} = \frac{\psi_{IW}\cdot N_{IW}}{\psi_{FE_l}\cdot N_{FE_l}}$

For the longer-run perspective, we consider the production function for the economy: $Y = Y_{FE_l} + Y_{FE_h} + Y_{IW}$ where $Y_J = A\cdot(K_J)^{\alpha}\cdot(L_J)^{1-\alpha}$, where $L_J = \psi_J\cdot N_J\cdot J$ is the number of effective hours of work by workers of type $J\in\{FE_l, FE_h, IW\}$ and K_J denotes the productive capital allocated to each workers' type.

We assume that capital can adjust to a transfer shock but not hours per worker. The change in output Y following a change in cash transfer T is then:

$$\begin{aligned}\frac{dY}{dT} &= \sum_{J\in\{FE_l,FE_h,IW\}} A\cdot\left[\alpha\cdot\frac{(K_J)^{\alpha}}{K_J}\cdot(L_J)^{1-\alpha}\cdot\frac{dK_J}{dT}+(1-\alpha)\cdot(K_J)^{\alpha}\cdot\frac{(L_J)^{1-\alpha}}{L_J}\cdot\frac{dL_J}{dT}\right]\\ &= \sum_{J\in\{FE_l,FE_h,IW\}} A\cdot(K_J)^{\alpha}\cdot(L_J)^{1-\alpha}\cdot\left[\alpha\cdot\frac{1}{K_J}\cdot\frac{dK_J}{dT}+(1-\alpha)\cdot\frac{1}{L}\cdot\frac{dL_J}{dT}\right]\\ &= \sum_{J\in\{FE_l,FE_h,IW\}}\frac{Y_J}{L_J}\cdot\frac{dL_J}{dT}\\ &= \frac{Y}{E}\cdot\left[\kappa_{FE_l}\cdot\frac{dFE_l}{dT}+\kappa_{FE_h}\cdot\frac{dFE_h}{dT}+\kappa_{IW}\cdot\frac{dIW}{dT}\right]\end{aligned} \tag{2}$$

where the third line assumes that capital scales up in proportion to effective labor (as stated in the text), the fourth line uses $\frac{1}{L_J}\cdot\frac{dL_J}{dT} = \frac{1}{J}\cdot\frac{dJ}{dT}$, and $\kappa_J = \frac{Y_J/Y}{J/E}$ still captures each worker type's productivity relative to the average worker in the economy. Using the multiplier notation (μ) in the paper, the above expression becomes:

$$\mu_Y = \frac{Y}{E}\cdot\left(\kappa_{FE_l}\cdot\mu_{FE_l}+\kappa_{FE_h}\cdot\mu_{FE_h}+\kappa_{IW}\cdot\mu_{IW}\right)$$

We note that, once again, we only need to know the relative productivities $\kappa_{FE_h}/\kappa_{FE_l}$ and $\kappa_{IW}/\kappa_{FE_l}$ – and each worker type's share of total employment – to pin down the level

of the three κ_j's. Indeed, one can write:

$$\mu_Y = \frac{Y}{E} \cdot \kappa_{FE_l} \cdot \left(\mu_{FE_l} + \frac{\kappa_{FE_h}}{\kappa_{FE_l}} \cdot \mu_{FE_h} + \frac{\kappa_{IW}}{\kappa_{FE_l}} \cdot \mu_{IW} \right)$$

Moreover, we have:

$$\begin{aligned} \kappa_{FE_l} &= \frac{Y_{FE_l}}{Y} \cdot \frac{E}{FE_l} \\ &= \frac{1}{1 + \frac{Y_{FE_h}}{Y_{FE_l}} + \frac{Y_{IW}}{Y_{FE_l}}} \cdot \frac{E}{FE_l} \\ &= \frac{1}{\frac{FE_l}{E} + \frac{\kappa_{FE_h}}{\kappa_{FE_l}} \cdot \frac{FE_h}{E} + \frac{\kappa_{IW}}{\kappa_{FE_l}} \cdot \frac{IW}{E}} \end{aligned}$$

where the third line uses: $\frac{\kappa_{FE_h}}{\kappa_{FE_l}} = \frac{Y_{FE_h}/FE_h}{Y_{FE_l}/FE_l}$ and $\frac{\kappa_{IW}}{\kappa_{FE_l}} = \frac{Y_{IW}/IW}{Y_{FE_l}/FE_l}$

Online Appendix F:
Welfare effect of Cash Transfers

This Appendix explains in more detail how we apply the Marginal Value of Public Funds (MVPF) framework to assess the welfare implications of the 2009 PBF expansion. The MVPF of a policy corresponds to the ratio of the WTP for the policy to its net fiscal cost (Finkelstein and Hendren 2020). The MVPF informs welfare by indicating how much the government must value spending on a policy for it to improve welfare: the welfare gain is given by $dW = \omega \cdot MVPF - 1$, where $MVPF - 1$ captures the changes in social efficiency per \$1 of net government spending. The parameter ω captures the money-metric welfare gain from giving \$1 to the population affected by the policy. Specifically, ω corresponds to their "social welfare weight" divided by the shadow value of public funds. An efficiency-reducing policy ($MVPF < 1$) could still improve welfare if it benefits groups with a high social welfare weight ($\omega > 1$).

To derive an expression from the MVPF of the 2009 PBF expansion, we start from the impact of the policy on social efficiency per \$1 of additional PBF benefits ($dPBF$):

$$\begin{aligned} &\frac{dM}{dPBF} \cdot (WTP^{dM} - 1) + \frac{dB}{dPBF} \cdot (WTP^{dB} - 1) + \frac{dQ}{dPBF} \cdot WTP^{dQ} \\ &= \underbrace{\frac{dM}{dPBF} \cdot WTP^{dM} + \frac{dB}{dPBF} \cdot WTP^{dB}}_{\text{Direct impact on PBF beneficiaries}} + \underbrace{\frac{dQ}{dPBF} \cdot WTP^{dQ}}_{\text{Changes in economic activity}} - 1 \end{aligned} \tag{3}$$

with

- $dPBF = dM + dB$, where we distinguish between 'infra-marginal' beneficiaries (dM) who received the extra benefits without changing their behavior, and 'marginal' beneficiaries (dB) who adjusted their behavior to qualify for the benefits; WTP^{dM} and WTP^{dB} is the average willingness-to-pay per \$1 in PBF benefits for the two types of beneficiaries, respectively;
- dQ is the change in economic activity induced by the PBF expansion, and WTP^{dQ} is the willingness-to-pay for that increase in economic activity.

As we explain in the paper, we can assume that infra-marginal beneficiaries value receiving \$1 in benefits at \$1 for a cash transfer ($WTP^{dM} = 1$), and assuming that families make privately optimal decisions, we have that the willingness-to-pay is nil for marginal

beneficiaries ($WTP^{dB} = 0$). Equation (3) thus simplifies to:

$$\begin{aligned} &\frac{dM}{dPBF} \cdot WTP^{dM} + \frac{dB}{dPBF} \cdot WTP^{dB} + \frac{dQ}{dPBF} \cdot WTP^{dQ} - 1 \\ =&\frac{dM}{dPBF} + \frac{dQ}{dPBF} \cdot WTP^{dQ} - 1 \end{aligned} \tag{4}$$

In turn, the net willingness-to-pay for a marginal increase in economic activity can simply be written as $WTP^{dQ} = k - h$, where k denotes the social marginal benefit of an increase in goods and services and h is its social marginal cost. If all markets in the economy are at a socially efficient equilibrium, k and h correspond to the willingness-to-pay of consumers and to the willingness-to-supply of producers, respectively, and both are equal to the market price. Consequently, we have $k = h$ and an increase in economic activity has no first-order effect on social efficiency. In reality, various distortions can create a positive "wedge" between the price faced by consumers and the opportunity cost of production in some markets ($k - h > 0$), causing output to be below its socially efficient level. Increasing output in these markets will therefore yields some efficiency gain.

To show this, we use the decomposition: $WTP^{dQ} = (k - s) + (s - h)$. First, the price paid by consumers k can exceed the private marginal cost for producers s. This could be driven by output taxes and price markups. Specifically, we define p as the price charged by producers, $k = (1 + t) \cdot p$ with t the output tax, and $p = (1 + \theta) \cdot s$ with θ the markup over marginal cost. We then have: $k - s = t \cdot p + \frac{\theta}{1+\theta} \cdot p$, and $\frac{dQ}{dPBF} \cdot (k - s)$ becomes:

$$\frac{dQ}{dPBF} \cdot (k - s) = \frac{dTax}{dPBF} + \frac{dVA}{dPBF} \cdot \frac{\theta}{1 + \theta}$$

where $dTax = dQ \cdot t \cdot p$ is the revenue from output taxes and $dVA = dQ \cdot p$ is the value of production net of output taxes (or $dVA = dGDP - dTax$ with $dGDP = k \cdot dQ$).

Second, the private marginal cost of production s may exceed the true opportunity cost of production h. If we assume the simple production function: $Q = f(V)$ for a composite variable input V, with unit cost paid by producers w, but true opportunity cost per unit of extra input $\underline{w}$. We then have: $s - h = \frac{w - \underline{w}}{f'(V)}$, and $\frac{dQ}{dPBF} \cdot (s - h)$ becomes:

$$\frac{dQ}{dPBF} \cdot (s - h) = f'(V) \cdot \frac{dV}{dPBF} \cdot \left(\frac{w - \underline{w}}{f'(V)} \right) = \frac{dV}{dPBF} \cdot w \cdot \left(1 - \frac{\underline{w}}{w} \right)$$

We can then derive the following expression for equation (4):

$$\begin{aligned}&\frac{dM}{dPBF}+\frac{dQ}{dPBF}\cdot WTP^{dQ}-1\\=&\frac{dM}{dPBF}+\frac{dVA}{dPBF}\cdot\frac{\theta}{1+\theta}+\frac{dV}{dPBF}\cdot w\cdot\left(1-\frac{\underline{w}}{w}\right)-(1-\frac{dTax}{dPBF})\end{aligned} \tag{5}$$

where we collect all the terms involving net costs to the government together at the end of equation (5). Finally, we can express equation (5) in terms of the net cost to the government – rather than per \$1 of additional PBF benefits – to obtain the definition of the MVPF:[2]

$$\frac{\frac{dM}{dPBF}+\frac{dVA}{dPBF}\cdot\frac{\theta}{1+\theta}+\frac{dV}{dPBF}\cdot w\cdot\left(1-\frac{\underline{w}}{w}\right)}{1-\frac{dTax}{dPBF}}-1=MVPF-1 \tag{6}$$

To apply this expression to our empirical setting, we focus on the average markup over marginal cost and the value added in non-tradable industries, which we show increased following the 2009 PBF expansion. We also focus on low-wage formal employment as input, which we show increased following the 2009 PBF expansion, and for which our evidence – an increase in labor demand with no corresponding increase in wages – suggest the existence of excess supply in the labor market – motivating the possibility of a reservation raise $\frac{\underline{w}}{w}$ below 1.

Remark 1: In footnote 55 on page 37, we write:

> *It is also worth nothing that, in a setting like ours where some eligible households are not beneficiaries, the result $WTP^{dB}=0$ applies* on average *to households who changed their behavior to qualify for the benefits (see Online Appendix F). The logic is simple: these households incur a cost to receive the PBF benefits with some probability; at the margin, they must therefore be better off if they end up receiving the benefits, worse off otherwise, and their expected welfare gain is nil.*

To see this, let's assume that qualifying for the benefits (if not mechanically eligible) involves a cost C_i, which varies across households according to some cdf $F(C_i)$ and that households who changed their behavior in order to be eligible only become beneficiaries and receive transfer T with probability p. Households at the margin of changing their behavior to qualify for the benefits must then satisfy: $p\cdot T=C_i$. Let's also define the number of new infra-marginal beneficiaries – those who became beneficiaries mechanically, i.e., without changing their behavior – as N^{dM} and the number of households who

[2]We abstract from potential impacts on input taxes in the derivation laid out in this section given our context, but any impact on input taxes would simply add to $\frac{dTax}{dPBF}$ in the denominator of equation (6).

became beneficiaries by changing their behavior as $N^{dB} \cdot p$, where N^{dB} is the number of households who changed their behavior in order to be eligible (and thus incurred cost C_i).

We can then write the MVPF considering only the direct impact from receiving the additional PBF payments (as on page 37 in the paper) as follows:

$$MVPF_1 = \frac{N^{dM} \cdot WTP^{dM}}{N^{dM} + N^{dB} \cdot p} + \frac{N^{dB} \cdot \overline{WTP}^{dB}}{N^{dM} + N^{dB} \cdot p}$$

where $\overline{WTP}^{dB}$ capture the average willingness-to-pay among households who changed their behavior to qualify for the benefits. As we wrote in the paper, for a cash transfer, we can assume that infra-marginal beneficiares value receiving \$1 at \$1: $WTP^{dM} = \frac{T}{T} = 1$. Given that we consider the MVPF of a marginal change in the size of the policy, we must also have that $N^{dB} = f(p \cdot T)$ and that $\overline{WTP}^{dB} = \frac{p \cdot T - C_i}{p \cdot T} = 0$. We thus have: $MVPF_1 = \frac{N^{dM}}{N^{dM} + N^{dB} \cdot p} = \frac{dM}{dPBF}$, the same expression as in the paper (on page 37).

Remark 2: We follow the literature by considering the MVPF of a marginal change in the size of the policy. Although standard in the literature, policy changes are often large, including the one we study. As written by Finkelstein and Hendren (2020): "*For large policies, the marginal cost of public funds remains a useful guide, but measuring willingness to pay can be less straightforward because it now requires incorporating some value of benefits to marginal recipients.* (...) *Fundamentally, we need an estimate of the marginal recipients' demand curve for the increase in public expenditure: that is, willingness to pay is the area under that demand curve. For a large increase in a public cash transfer, pre-existing recipients still value the transfer at its dollar value (a dollar is still worth a dollar), but for recipients who change their behavior in order to access the larger public cash transfer, we need to know their willingness to pay for that cash transfer, net of the utility cost of their behavioral change.* (...) *one standard approach (really just a short cut) is to count 50 percent of the increased transfer payments to marginal recipients; this 50 percent approximation follows from an assumption of linearity in the response function and the geometry of triangles (Plimpton 1800BC and Pythagoras 500BC). This approach is popular for its ease of implementation, if not necessarily, its realism; Finkelstein, Hendren, and Luttmer (2019) and Hendren and Sprung-Keyser (2020) are recent examples.*"

In sum, by assuming $WTP^{dB} = 0$, we are underestimating the WTP of households who changed their behavior to qualify for the benefits, as mentioned in footnote 55 on page 37.

To see this, we can use the simple model laid out in Remark 1. Prior to the policy, households at the margin of changing their behavior to qualify for the benefits were such that: $p^{pre} \cdot T = C_i$. After the reform, many households changed their behavior in order to be eligible, and the last households to do so are such that $p^{post} \cdot T = C_i$,

with $p^{post} > p^{pre}$, where we no longer assume that the change in the size of the policy is marginal. We now have that $N^{dB} = F(p^{post} \cdot T) - F(p^{pre} \cdot T)$ and that $\overline{WTP}^{dB} = \frac{E\left[p^{post} \cdot T - C_i | p^{pre} \cdot T \leq C_i \leq p^{post} \cdot T\right]}{p^{post} \cdot T} > 0$. So, while the welfare gain is still nil for the marginal households who changed their behavior in order to be eligible after the reform, the surplus is as big as a $\frac{p^{post} - p^{pre}}{p^{post}}$ for the households who were at the margin of changing their behavior to qualify for the benefits prior to the policy.

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