# Cash Transfers and Productive Inclusion: Evidence from Bolsa Família*

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**Abstract**

We study how cash transfers affect work and health. Exploiting an increase in the generosity of the world's largest cash-transfer program for the extremely poor, we show that the reform raised employment by 5 percent while sharply improving health: hospitalization fell 6 percent and mortality 14 percent, saving roughly 1,000 lives. These findings challenge the view that transfers reduce work. Instead, transfers can relax binding subsistence and health constraints, raise productivity, and expand labor supply. We formalize this mechanism in a model of productive inclusion and use it to evaluate welfare, yielding lessons for antipoverty policy design in low-income settings.

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# 1 Introduction

Cash transfers are among the most widely used policy tools to combat poverty worldwide (Gentilini, 2024). A central question in the public debate surrounding these programs is whether providing cash discourages work by reducing beneficiaries' incentives to supply labor. Existing evidence points to a striking pattern. In high-income countries, cash transfers tend to reduce labor supply, consistent with standard predictions (e.g., Cesarini *et al.*, 2017; Vivalt *et al.*, 2024). In contrast, in low- and middle-income settings, the estimated labor supply response is often positive (Bastagli *et al.*, 2019) – see Figure 1 for a summary of the evidence. This paper asks why labor supply responses tend to be positive in low-income settings? We argue that relaxing binding subsistence and health constraints can raise productivity and expand labor supply. We refer to this mechanism as productive inclusion.

We address this question by leveraging quasi-experimental variation due to the expansion of the world's largest cash transfer program. The reform introduced exogenous variation in transfer generosity by sharply increasing benefits for households whose income placed them below the extreme-poverty line. That is, the reform introduced a guaranteed income (negative income tax) taxed away at a 100% marginal tax rate for beneficiaries below the extreme poverty line, while leaving the schedule faced by beneficiaries above the extreme poverty unchanged. We implement a difference-in-differences design using rich administrative microdata covering the universe of low-income households in Brazil to estimate the labor supply and health responses to a large transfer. We find that the *Bolsa Família* expansion increased employment by 4.8 percent, with similar effects for men and women, and increased total earnings by 5 percent. Standard neoclassical models would instead predict a reduction in labor supply due to both income and substitution effects.

To interpret these responses and conduct welfare analysis, we develop a simple extension of the standard model to capture productive inclusion effects to rationalize the empirical findings and inform policy design. The model, inspired by long standing work on the impact of health constraints on productivity (Dasgupta & Ray, 1986; Mullainathan & Shafir, 2013), features a production technology in which workers cannot realize their full productivity when basic health and

subsistence needs are not met. Introducing a guaranteed income that meets some households' basic needs increases their productivity. This generates two positive labor supply channels. First, on the extensive margin there is a set of households who are able to earn significantly more thanks to their higher productivity and now find it worthwhile to bear the costs of participation in the labor market. Second, on the intensive margin, raising households' productivity induces substitution away from leisure raising earnings. Neither effect is present in a standard model that does not feature the productive inclusion channel.

To provide direct evidence on the health effects of the policy, we link our administrative data to the universe of public hospital admissions in Brazil.[1] We find large and economically meaningful improvements in health along multiple dimensions. First, the probability of hospitalization falls by 6 percent. Second, conditional on being hospitalized, the length of stay declines by 2 percent, indicating better health even among individuals who require inpatient care. Finally, although public hospitals are free at the point of use in Brazil, administrative records allow us to observe the fiscal cost of hospitalizations. We find that hospitalization costs fall by 10 percent for eligible individuals, implying that part of the fiscal cost of the transfer expansion is offset by reduced public health expenditures.

Most notably, we find that the probability of a death within the household declines by 14 percent. This finding both aligns with the broader pattern of health improvements that helps explain the positive employment response and stands on its own as compelling evidence that cash transfers can save lives. A back-of-the-envelope calculation suggests that the 2012 *Bolsa Família* expansion prevented approximately 1,000 deaths. To further illuminate the health channel and the role of basic needs, we exploit detailed diagnosis codes and show that hospital admissions decline most sharply for conditions closely linked to income constraints, including under-nutrition (17 percent), infectious diseases (20 percent), and digestive complications (9 percent). These conditions are plausibly affected by additional resources and are commonly associated with reduced ability to participate in the labor market.

[1] According to the Pesquisa Nacional de Saúde (IBGE, 2019), low income Brazilians rely exclusively on the public health system for all health services, with private insurance concentrated only among higher income households.

Finally, we provide evidence on channels that help explain how cash transfers can improve health. We leverage detailed *Cadastro Único* data on households' monthly expenditures and find that beneficiaries increase spending on medication by about 48 percent on average. This pattern is consistent with the idea that relaxing subsistence constraints enables households to afford essential care. Improved access to medications and better nutrition are complementary inputs into health and offer a plausible explanation for both the health gains and the stronger labor supply observed among eligible households. This mechanism aligns with the long-standing insight in Dasgupta & Ray (1986) that inadequate nutrition can limit individuals' ability to work and that relaxing subsistence constraints can generate productivity and welfare gains.

To benchmark the welfare gains from the reform against its fiscal cost, we follow (Finkelstein & Hendren, 2020) and compute the associated Marginal Value of Public Funds (MVPF). Since we argue that the reform augmented households' productivity, the associated changes in behavior can have first-order welfare effects, which we show can be bounded by the associated earnings changes. We begin by calculating the MVPF using only labor market outcomes, mirroring the type of evidence typically available in settings without linked administrative health data. Under this approach, we estimate that each additional dollar of net government spending generates about $3.9 in value for program beneficiaries. When we incorporate the documented health improvements, the implied MVPF becomes infinite in our accounting because reductions in public health expenditures more than offset the direct fiscal cost of the transfer expansion.

**Literature.** The main contribution of this paper is to bridge the labor and health literature on cash transfers and to extend standard participation models from the optimal tax literature to incorporate productivity-reducing health and subsistence constraints within a unified framework. Concretely, we contribute to three strands of literature. First, we add to the large literature studying the effects of cash transfer programs in both developed and developing countries (see Currie, 1994; Banerjee *et al.*, 2015; Crosta *et al.*, 2024, for reviews). This substantial body of work documents mixed labor supply responses to transfers, with studies in high-income settings typically finding negative employment effects, while evidence from low- and middle-income countries often points to neutral or positive responses. Negative labor supply effects are well understood within standard

economic frameworks and have been extensively discussed in the literature.

Our contribution is to provide new empirical evidence from a large guaranteed-income expansion and a unified framework that clarifies when transfers can raise employment rather than reduce it. When households face binding subsistence and health constraints, cash transfers can relax these constraints, increase effective productivity, and expand labor supply. We refer to this mechanism as *productive inclusion*. It is closely related to ideas in the poverty and behavioral economics literature, including scarcity and "mental bandwidth" constraints (e.g., Mullainathan & Shafir, 2013) and the productivity consequences of malnutrition (e.g., Dasgupta & Ray, 1986). These findings are consistent with the absence of discouragement effects of means tested transfers in low-income settings (Banerjee *et al.*, 2017), and have direct implications for the design of transfer programs.

Second, we contribute to a smaller but growing literature on the health effects of cash transfers (Blattman & Niehaus, 2014; Evans *et al.*, 2019; Stedman, 2023; Walker *et al.*, 2025). Existing studies provide important evidence that transfers can improve health outcomes, particularly those related to birth and maternal health in predominantly rural settings. We extend this literature by showing that cash transfers can reduce mortality and generate broad improvements in health, with associated fiscal savings in public health expenditures. These savings are large enough that the health benefits alone can offset the direct fiscal cost of the program. Relative to recent work such as Walker *et al.* (2025), our analysis goes beyond by examining the world's largest cash transfer program using comprehensive administrative data.[2] This setting allows us to document mortality effects across a wide age range and a broad set of diseases, and to study impacts in urban areas, where a large share of cash transfer beneficiaries worldwide reside and where effects may differ from those observed in predominantly rural contexts.

Finally, we contribute to the literature on individual income taxation, negative income tax reforms, and optimal tax design (e.g. Saez, 2002; Kleven &

[2]Despite its scale, Brazil's *Bolsa Família* has been relatively understudied, in part because its national scope makes it difficult to obtain credible quasi-experimental variation in transfer generosity (with few exceptions, such as Gerard *et al.* (2025); Bergstrom *et al.* (2025)). We exploit the 2012 guaranteed-income expansion, which provides a sharp and previously unexplored source of plausibly exogenous variation around the extreme-poverty line. To our knowledge, this is the first study to use individual-level administrative data to examine the health and mortality effects of Bolsa Família within a unified framework that also captures labor market responses.

Kreiner, 2006; Jacquet *et al.*, 2013; Piketty & Saez, 2013). While some frameworks in this tradition allow for endogenous productivity, the focus has typically been on incentives for investment in human capital accumulation (Lans Bovenberg & Jacobs, 2005; Best & Kleven, 2012; Stantcheva, 2017; Makris & Pavan, 2021) rather than relaxing productivity constraints at the bottom of the income distribution. We show that this abstraction can be quantitatively important in a low-income setting. By extending a standard framework to allow transfers to relax binding subsistence and health constraints, we generate productivity gains alongside incentive effects and reconcile positive employment responses to a large guaranteed-income reform with observed behavior. We discipline the model using quasi-experimental variation from a nationwide policy shock and use it to evaluate welfare. The results highlight that analyses of redistribution that abstract from productivity responses at the bottom of the income distribution may substantially understate both behavioral responses and the welfare gains from transfers.

The rest of the paper is organized as follows. Section 2 describes the institutional setting of the *Bolsa Família* program, the 2012 reform, and the administrative data sources. Section 3 develops the model of productive inclusion. Section 4 presents the empirical strategy and the main results on labor supply and health outcomes. Section 5 explores the mechanisms underlying the estimated effects. Section 6 computes the marginal value of public funds. Section 7 concludes.

# 2 Institutional Background and Data

## 2.1 Bolsa Família Program

Brazil's *Programa Bolsa Família* (PBF) is the main national safety-net program and, during our period of interest, the largest cash-transfer program in the world. It targets low-income families through a means test based on monthly income per capita (MIPC) and two federal thresholds: a poverty line (PL) and an extreme-poverty line (EPL). Eligibility and benefit levels also depend on household composition, especially the presence and age of children. Table 1 reports the exact thresholds and values for each type of PBF benefit.

At baseline, households with per-capita income below the EPL were entitled

to an unconditional *Basic* benefit of R$70 per *household* in 2011.[3] Families with children could also receive a *Variable I* benefit of R$32 per child younger than 16 and a *Variable II* benefit of R$38 per adolescent aged 16 to 18, conditional on school attendance and basic health requirements. These conditionalities may, in principle, complicate inference about behavioral responses to PBF. In our setting, however, the variation we exploit comes from an expansion of the program that operated independently of conditionality compliance — both our treatment and control groups faced the same conditional benefit schedule.

## 2.2 Institutional Details of the 2012 PBF Reform

In 2012, the administration of President Dilma Rousseff, elected on an explicitly anti-poverty platform, rolled out a major expansion of Bolsa Família with the stated goal of eradicating extreme poverty. The reform modified the transfer formula so that any family whose monthly income per capita remained below the EPL after standard PBF benefits would receive a top-up sufficient to bring its per-capita income exactly to the EPL. In effect, the expansion turned PBF into a guaranteed-income scheme with a 100% marginal tax rate for the extreme poor.[4]

**Eligibility Assignment.** The 2012 expansion of Bolsa Família generated a sharp and transparent source of quasi-experimental variation in transfer generosity. The reform classified households into three mutually exclusive groups based on total resources per capita, defined as monthly income per capita (MIPC) plus baseline Bolsa Família transfers (i.e., all standard PBF components, excluding the new guaranteed-income top-up). Families whose total resources per capita fell below the extreme-poverty line (EPL) received both the standard PBF benefits and a guaranteed-income top-up sufficient to reach the EPL. Households with total resources between the EPL and the poverty line (PL) continued to receive standard variable benefits but were not eligible for the top-up. Families above the PL remained outside the program altogether.

**Income Distribution.** Panel (a) of Figure 2 plots the cross-sectional distribution of MIPC in 2012. While eligibility is formally determined by total resources per

[3] In 2011, the exchange rate was approximately R$2 per USD.

[4] The reform was first passed through Provisional Measure (PM #570) in May 2012 and converted into Law 12.722/2012 in the same year.

capita, the figure illustrates the underlying income distribution prior to the application of transfers. A substantial share of households lies below the extreme-poverty line, indicating that the guaranteed-income component of the reform applied to a sizable share of the population.

**Benefit Schedule.** Panel (b) of Figure 2 shows how the eligibility rule maps into top-ups. The figure plots total guaranteed-income payments in 2014 against pre-top-up total resources per capita (MIPC plus baseline PBF transfers), using fine income bins. At the bottom of the distribution, transfers rise steeply because the guaranteed-income component mechanically fills the gap between a household's pre-top-up resources and the EPL. For example, households with total resources per capita of R$50 receive a R$20 top-up to reach the EPL of R$70. At the EPL itself, the schedule displays a sharp kink, after which top-up payments are phased out.

This pronounced "hockey-stick" pattern is a direct mechanical implication of the reform and reflects the nonlinearity it introduced into households' budget sets. As illustrated in Figure 3, the guaranteed-income component inserts a flat segment into the budget constraint of households below the EPL, implying a 100 percent implicit marginal tax rate over the range where the guarantee binds. This sharp policy-induced nonlinearity provides the core source of identifying variation exploited in the empirical analysis that follows.

**Budget Set.** From the perspective of household decision-making, the reform inserts a flat segment into the budget set of the extreme poor: additional earnings dollar-for-dollar reduce the transfer, implying a 100 percent marginal tax rate over that range. Figure 3 illustrates the resulting shift in the budget set. The policy variation mirrors closely the guaranteed-income environment analyzed in Section 3, providing a direct bridge between the theoretical framework and the policy experiment we exploit empirically.

## 2.3 Data

To study the effects of this large expansion of Bolsa Família, we assemble several administrative datasets that together cover nearly the entire low-income population of Brazil for more than a decade. Each dataset is structured as an individual-level panel with stable identifiers, allowing us to link PBF transfers, labor-market

outcomes, health events, and socio-economic characteristics with precision.

**Cadastro Único.** The backbone of our analysis is the federal social registry, *Cadastro Único* (CU), widely regarded as a comprehensive census of low-income households in Brazil. During our study period (2012–2020), the registry covers roughly 40 percent of the national population, and our access includes the full universe of records—approximately 80 million individuals per year. CU provides detailed information on education, self-reported labor and non-labor income, and location of residence. Because the registry also assigns a family identifier, we can recover household structure and construct consistent measures of household composition over time. CU attains unusually broad coverage because it serves as the administrative gateway for numerous social programs, including Bolsa Família.

Although eligibility for Bolsa Família is based on self-reported income in CU, several features of the data collection process enhance the reliability of this information. Enrollment and updates are conducted by trained enumerators who elicit income only at the end of the interview, after an extensive sequence of questions on assets, housing, and demographics, which allows caseworkers to assess the internal consistency of responses. Field protocols encourage home visits, giving enumerators direct visibility into living conditions and enabling them to resolve discrepancies when they arise. Families must also update their information at least every two years to remain active in the registry. Importantly, CU collects income independently of the labor-market outcomes we observe in other datasets, reducing the scope for mechanical correlations.[5]

Finally, whenever administrative capacity permits, the government cross-checks reported incomes against records from the formal labor market and the social-security system. Altogether, the possibility of verification and the design of the interview process both constrain large deviations from true income. While acknowledging incentives for under-report, in robustness checks we further examine concerns related to misreporting.

**Bolsa Família Payroll Microdata.** We then match the registry to monthly Bolsa Família payroll microdata from the Ministry of Social Development. These records cover the universe of PBF disbursements from 2007 to 2020. For each payment,

[5]Moreover, the results we will document reveal *increases* in earnings among beneficiaries, while means testing creates incentives for under-reporting earnings, suggesting that any biases in our estimates are towards zero.

we observe the recipient family, the exact month, and the full decomposition of the transfer into basic, child, adolescent, and guaranteed-income top-up components. This allows us to reconstruct, at a granular level of detail, the effective transfer schedule for each household and to measure the size and timing of the shock induced by the PBF expansion.

**RAIS.** Formal labor market outcomes come from the *Relação Anual de Informações Sociais* (RAIS), Brazil's matched employer–employee administrative database. We observe the full universe of formal employment relationships, including monthly wages, hours, occupation, firm identifiers, sector, and basic worker demographics. RAIS has been widely used to study the Brazilian labor market (see Dix-Carneiro (2014)), and unlike CU, its information is third-party reported by employers rather than workers.

**SUS.** Finally, we incorporate administrative health records from Brazil's Unified Health System (*Sistema Único de Saúde, SUS*) by integrating the two nationwide systems that capture severe health events and mortality: the Hospital Information System (SIH/SUS). SIH covers the universe of SUS-reimbursed inpatient admissions and in-hospital deaths over our study period (2008–2020). These records report admission and discharge dates, the primary diagnosis (coded using the International Classification of Diseases, 10th revision, ICD-10), procedures and other utilization measures, and whether the hospitalization ended in an in-hospital death. Although SUS care is free at the point of use, SIH also includes administrative information on reimbursed costs and length of stay, allowing us to construct standardized measures of hospitalization costs, and severity at the individual level. By linking these administrative health records to Cadastro Único and the Bolsa Família payroll, we can trace how changes in transfers translate into health events and survival at the individual level.

Together, these rich administrative datasets yield more than one billion person-year observations with detailed individual- and household-level information, providing a uniquely comprehensive setting to study how cash-transfer policy shapes heterogeneous behavior across multiple welfare-relevant margins.

## 2.4 Sample Construction

**Eligibility Assignment.** Our empirical analysis centers on the cohort of households recorded in *Cadastro Único* in 2012, the first year in which we have access to CU registries. We treat this cross-section as our baseline population and follow its members backward and forward in time using other administrative sources that cover earlier and later years. This approach aligns the definition of the population with the information set used to assign eligibility under the 2012 reform.

We can reconstruct pre-reform eligibility status using 2009 to 2011 Bolsa Família disbursement records. Individuals receiving the basic benefit, available only to those below the EPL, in all three years are classified as below the EPL. Those receiving only the variable benefit are classified as between the EPL and the PL. This provides a consistent measure of exposure to the transfer schedule in the pre-reform period and ensures comparability across years.

**Labor Market.** Using unique personal identifiers (CPF and NIS), we link each individual in the 2012 CU cohort to their historical and subsequent records in Bolsa Família disbursements, and RAIS formal employment records. When a household does not update its CU record in a given year, we carry forward the most recent demographic and household characteristics. In additional to labor market outcomes observed in RAIS our data also encompasses informal labor market outcomes reported in Cadastro Único. This procedure yields a balanced longitudinal dataset for the 2012 cohort, which forms the basis for the subsequent analysis.

**Health.** Analogously to the labor market datasets, we build a balanced dataset for the 2012 cohort with health outcomes. To that end, we deterministically merge SIH to individuals in Cadastro Único using stable demographic and residence identifiers. For SIH, we perform linkages using CEP (ZIP code) when available, combining year $\times$ CEP $\times$ date of birth $\times$ sex, and otherwise rely on linkages based on year $\times$ municipality of residence $\times$ date of birth $\times$ sex when CEP is missing or does not yield a unique match. Using the linked data, we construct person-year outcomes capturing (i) hospitalization costs and intensity (e.g., any admission, number of admissions, total inpatient days, total reimbursed cost), and (ii) in-hospital mortality from SIH.

## 2.5 Descriptive Statistics

Overall, our data cover more than 80 million unique individuals nested within approximately 22 million families over a twelve-year period. The rich family identifiers in *Cadastro Único* allow us to present descriptive statistics at both the individual and family levels.

**Individual-level.** Table 2 summarizes the characteristics of individuals in our 2012 baseline. The population is relatively young, with more than one third of members under 16 years of age, and only a small fraction above retirement age. Formal employment is about 13 percent, reflecting the income distribution of the CU population, with an additional 8 percent reporting informal work. Among formal workers, the majority hold low-wage, blue-collar jobs.

**Family-level.** Table 3 shows that household structure is split between two-adult and single-headed families, and the prevalence of children and adolescents is high. Nearly half of all families have per-capita income below the EPL, about one quarter fall between the EPL and PL, and the remainder lie above the PL. Just over half of families receive PBF benefits, reflecting the program's substantial coverage among low-income households.

# 3 Conceptual Framework

This section develops a simple framework that can account for the behavioral responses to cash transfers documented in low-income settings, such as the empirical environment studied in this paper. The key feature of the framework is that households' effective productivity may depend on whether basic subsistence and health needs are met, in addition to standard incentive considerations.

We proceed in two steps. As a benchmark, we first analyze a standard participation model in which productivity is independent of transfers, and higher benefits affect labor supply only through incentives. We then extend the model to allow transfers to relax binding subsistence and health constraints, introducing a channel through which transfers can raise productivity and labor supply. This extension provides a parsimonious structure for interpreting the empirical findings and for conducting the welfare analysis that follows.

### 3.1 Households

Households consist of $N$ members and are characterized by two sources of heterogeneity. First, their ability $A > 0$ governs their potential earnings and productivity, as in classical models of optimal income taxation with a single dimension of heterogeneity that focus on intensive-margin choices (Piketty & Saez, 2013). Second, households face a participation cost $q < q^{max}$ of working (Blundell & MaCurdy, 1999). These costs may be monetary, such as child care or transportation, time-related, such as commuting, or psychic. They may also be negative if households value employment *per se* or feel stigma from not working.

We follow Kleven & Kreiner (2006); Jacquet *et al.* (2013) and assume that participation costs are separable from consumption and effort in households' utility functions. Moreover, for simplicity, we assume that utility is quasi-linear in consumption, abstracting away from income effects:[6]

$$U(c, h; q) = c(h) - \nu(h) - q\mathbf{1}[h > 0] \tag{1}$$

Households receive disutility $\nu(h)$ from work effort $h$. This can be interpreted as the number of hours worked, but that literal interpretation is inconsistent with the formal labor market in Brazil, which features rigid hours constraints (Rosen, 1976; Altonji & Paxson, 1988). Instead, we interpret $h$ as the amount of effort devoted to generating income. For convenience, we assume that the disutility of work effort is isoelastic:

$$\nu(h) = \frac{1}{1 + 1/\varepsilon} h^{1+1/\varepsilon} \tag{2}$$

where $\varepsilon > 0$ denotes the elasticity of labor supply with respect to the net return to work. Households also have a minimum level of consumption $\bar{c}$ required to satisfy their basic needs. This minimum consumption plays no role in the benchmark model but features prominently in our productive inclusion model.

Households receive benefits from the government of $b_t(n_c, N, y)$ that depend on the number of children in the household $n_c$, the overall household size $N$, and earned income $y$. Households must finance their consumption from their

[6] Adding income effects will only strengthen the qualitative conclusions.

earnings and benefits, making their budget constraint

$$c(h) = y(h) + b_t(n_c, N, y(h)). \tag{3}$$

Before the guaranteed income reform ($t = 0$), households received two basic income transfers. First, all households with per capita earned income $y(h)/N$ below the extreme poverty line $\bar{b}$ received a lump-sum transfer of $\bar{b}$. Second, households below the poverty line received a conditional transfer of $b_c$ for each child in the household.[7] A household with $n_c$ children therefore faced a benefit schedule of

$$b_0(n_c, N, y) = \begin{cases} \bar{b} + n_c b_c & \text{if } y \leq N\bar{b} \\ n_c b_c & \text{if } y > N\bar{b} \end{cases} \tag{4}$$

The reform introduces a guaranteed income, which ensures that all households have per capita income of at least $\bar{b}$. To achieve this, the government gives households a "top-up" transfer: $b_{\text{top-up}} = N\bar{b} - y - b_0$ if $y + b_0 < N\bar{b}$. This means that the benefit schedule after the reform ($t = 1$) is:

$$b_1 = b_0 + b_{\text{top-up}} = \begin{cases} N\bar{b} - y & \text{if } y < N\bar{b} - b_0 \\ \bar{b} + n_c b_c & \text{if } N\bar{b} - b_0 \leq y < N\bar{b} \\ n_c b_c & \text{if } y \geq N\bar{b} \end{cases} = \max\{N\bar{b} - y, b_0\} \tag{5}$$

## 3.2 A Benchmark Model

In the benchmark model, households' productivity does not depend on whether they are able to satisfy their basic needs and so their earnings are $y(h) = Ah$ as in standard models (Piketty & Saez, 2013). We can characterize their choices in the following proposition:

**Proposition 1** (Basic Income in the Benchmark Model)**.** *In the benchmark model with a basic income benefit as given by* (4) *individuals choose participation and work effort conditional on participation to maximize* (1) *subject to* (3)*. Their choices are characterized as follows:*

[7] The benefit amounts differed for children above and below age 16 and were limited to five children, which we suppress here for simplicity.

- *For each level of $q$ there is a threshold $\underline{\tilde{A}}(q, b_0)$. Households with $A \leq \underline{\tilde{A}}(q, b_0)$ do not work. The threshold is $\underline{\tilde{A}}(q, b_0) = [q(1+\varepsilon)]^{\frac{1}{1+\varepsilon}}$.*
- *Those who work choose effort $\tilde{h}^*(A, q, b_0) = A^{\varepsilon}$ and have earnings of $\tilde{y}^*(A, q, b_0) = A^{1+\varepsilon}$.*
- *Households with intermediate productivity ($\underline{\tilde{A}}(q, b_0) < A \leq \bar{\tilde{A}}(q, b_0) = (\bar{y} - b_0)^{\frac{1}{1+\varepsilon}}$, where $\bar{y} = \bar{c} + q$) work but do not meet their basic needs. They are working poor.*

*Proof.* The proof is presented in Appendix B.1. □

As we show in Section 3.4, the benchmark model unambiguously predicts that a guaranteed income reduces earnings, contrary both to the broader evidence from anti-poverty programs reviewed above and to our evidence from Brazil's 2012 reform. A simple extension of the benchmark model can instead rationalize the evidence and shed light on the effects of the guaranteed income in Brazil.

### 3.3 The Productive Inclusion Model

In the productive inclusion model, households that cannot meet their basic needs have diminished abilities. As discussed in detail in Section 5, this productive inclusion effect is consistent with a variety of microfoundations, including psychological costs of reduced "bandwidth" (Mullainathan & Shafir, 2013), physical diminishment due to malnutrition (Dasgupta & Ray, 1986), or occupational upgrading (Banerjee & Newman, 1993).

To capture this, we directly model the loss of productivity from having unmet basic needs. To meet their basic needs households require earnings $\bar{y}$ such that $\bar{y} + b_t(\bar{y}) = \bar{c}$. However, the marginal product of work effort is lower while households are not meeting their basic consumption needs of $\bar{c}$. We model the productivity loss as

$$y'(h) = \begin{cases} A\left(\frac{Ah}{\gamma(\bar{y}-b_0)}\right)^{\gamma-1} & \text{if } y(h) + b_t \leq \bar{y} \\ A & \text{if } y(h) + b_t > \bar{y} \end{cases} = A \times \min\left\{\left(\frac{Ah}{\gamma(\bar{y}-b_0)}\right)^{\gamma-1}, 1\right\} \tag{6}$$

where $\gamma \geq 1$ governs the rate at which meeting more of the household's basic needs improves their productivity. The benchmark model arises when $\gamma = 1$.

With these modifications, we can characterize household choices in the productive inclusion model similarly to the benchmark model in Proposition 1:

**Proposition 2** (Basic Income in the Productive Inclusion Model)**.** *In the productive inclusion model with marginal output given by* (6) *and a basic income benefit as given by* (4)*, individuals choose participation and work effort conditional on participation to maximize* (1) *subject to* (3)*. Their choices are characterized as follows:*

- *For each level of $q$ there is a threshold $\underline{A}(q, b_0)$. Households with $A \leq \underline{A}(q, b_0)$ do not work. The participation threshold is*

$$\underline{A}(q, b_0) = \begin{cases} \gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{\gamma-1}{\gamma}} \left( q \frac{1+\varepsilon}{1-\varepsilon(\gamma-1)} \right)^{\frac{1}{1+\varepsilon} - \frac{\gamma-1}{\gamma}} & \text{if } q \leq \tilde{q}(b_0) \\ \left( (1+\varepsilon) \left[ (\gamma-1)(\bar{y} - b_0) + q \right] \right)^{\frac{1}{1+\varepsilon}} & \text{if } q > \tilde{q}(b_0) \end{cases} \tag{7}$$

  *where $\tilde{q}(b_0) = \frac{\bar{c} - b_0}{\varepsilon\gamma} [1 - \varepsilon(\gamma - 1)]$ and $\bar{y} = \bar{c} + q$.*

- *Households with intermediate productivity — $\underline{A}(q, b_0) < A \leq \bar{A}(q, b_0)$ — and $q \leq \tilde{q}(b_0)$ work but do not meet their basic needs: they are working poor. They choose effort $h^* = \left( A^\gamma [\gamma(\bar{y} - b_0)]^{-(\gamma-1)} \right)^{\frac{\varepsilon}{1-\varepsilon(\gamma-1)}}$ and have earnings of $y^* = \left( A^\gamma [\gamma(\bar{y} - b_0)]^{-(\gamma-1)} \right)^{\frac{1+\varepsilon}{1-\varepsilon(\gamma-1)}} / \gamma$. The threshold for meeting basic needs is*

$$\bar{A}(q, b_0) = [\gamma(\bar{y} - b_0)]^{\frac{1}{1+\varepsilon}}. \tag{8}$$

- *Households with high productivity — $A > \bar{A}(q, b_0)$ — work and meet their basic needs. They choose effort $h^* = A^\varepsilon$ and have earnings of $y^* = A^{1+\varepsilon} - (\gamma - 1)(\bar{y} - b_0)$.*

*Proof.* The proof is presented in Appendix B.2. □

Panel A in Figure 4 shows the choices made by different households as characterized in Proposition 2.

## 3.4 Effects of Introducing a Guaranteed Income

This section characterizes both models' predictions on the effects of the switch from the basic income schedule (4) to the guaranteed income (5), and shows that

the two models deliver markedly different predictions. Starting with the benchmark model, we first characterize household choices under the guaranteed income and then show how the introduction of the guaranteed income affects these choices.

**Proposition 3** (Guaranteed Income in the Benchmark Model)**.** *In the benchmark model with a guaranteed income that meets basic consumption needs $N\bar{b} > \bar{c}$, as given by* (5)*, households choose participation and work effort conditional on participation to maximize* (1) *subject to* (3)*. Their choices are characterized as follows*

- *For each level of $q$ there is a threshold $\underline{\tilde{A}}(q, b_1)$. Households with $A \leq \underline{\tilde{A}}(q, b_1)$ do not work. The threshold is $\underline{\tilde{A}}(q, b_1) = \left[(1+\varepsilon)\left(q + N\bar{b} - b_0\right)\right]^{\frac{1}{1+\varepsilon}}$.*
- *Those who work choose effort $\tilde{h}^*(A, q, b_1) = A^{\varepsilon}$ and have earnings of $\tilde{y}^*(A, q, b_1) = A^{1+\varepsilon}$.*
- *There are no working poor.*

*Proof.* The proof is presented in Appendix B.3. □

Things are markedly different in the productive inclusion model. For many people, the guaranteed income will mean that they can cover their basic needs with the guaranteed income: $N\bar{b} \geq \bar{y}$. They are those with sufficiently small participation costs:

$$\bar{y} = \bar{c} + q \leq N\bar{b} \leftrightarrow q \leq N\bar{b} - \bar{c} = \bar{q}(b_1)$$

For those with large participation costs, the guaranteed income does not cover their basic needs and they need to earn more than the guaranteed income to meet those needs. They therefore face the same problem as under the basic income and must earn $y = \bar{y} - b_0$ to meet their basic needs. We can characterize household choices in the following proposition:

**Proposition 4** (Guaranteed Income in the Productive Inclusion Model)**.** *In the productive inclusion model with a guaranteed income that meets basic consumption needs $N\bar{b} > \bar{c}$, as given by* (5)*, households choose participation and work effort conditional on participation to maximize* (1) *subject to* (3)*. Their choices are characterized as follows*

- *For each level of $q$ there is a threshold $\underline{A}(q, b_1)$. Households with $A \leq \underline{A}(q, b_1)$ do not work. The threshold is given by*

$$\underline{A}(q, b_1) = \begin{cases} \left[(1+\varepsilon)\left(N\bar{b} - b_0 + q\right)\right]^{\frac{1}{1+\varepsilon}} & \text{if } q \leq \bar{q}(b_1) \\ \left[(1+\varepsilon)\left(N\bar{b} - \bar{c} + \gamma(\bar{y} - b_0)\right)\right]^{\frac{1}{1+\varepsilon}} & \text{if } q > \bar{q}(b_1) \end{cases} \tag{9}$$

  *where $\bar{q}(b_1) = N\bar{b} - \bar{c}$.*

- *Those who work choose effort $h^*(A, q, b_1) = A^{\varepsilon}$ and have earnings of $y^*(A, q, b_1) = A^{1+\varepsilon} - \mathbf{1}[q > \bar{q}(b_1)](\gamma - 1)(\bar{y} - b_0)$.*

- *There are no working poor.*

*Proof.* The proof is presented in appendix B.4 □

Panel B of Figure 4 shows households' choices under the guaranteed income. As in the benchmark model, the productive inclusion model predicts that the guaranteed income eliminates the working poor. The two models differ sharply, however, in their predictions for effort decisions. The benchmark model unambiguously predicts that labor force participation decreases and that there is no effect on the intensive margin. By contrast, the productive inclusion model, consistent with our empirical findings in Section 4.3, predicts that some households start working and that others increase earnings on the intensive margin.

Figure 5 shows the predictions of the productive inclusion model graphically. It overlays households' choices under the basic income and under the guaranteed income, with three different effects highlighted by the shaded areas. The model also allows us to study the welfare effects of the reform. Since the reform has direct productivity effects, there are first-order welfare gains from behavioral changes that may need to be accounted for. In appendix B.5 we delineate the different ways households are affected by the reform and show how to bound households' willingness to pay for the reform.

# 4 Impacts of the 2012 Bolsa Família Reform

Our empirical strategy compares individuals below the extreme poverty cutoff to individuals just above it, before and after the 2012 expansion of Bolsa

Família. This dynamic difference-in-differences design isolates the causal effect of the guaranteed-income component by exploiting sharp discontinuities in benefit eligibility.

## 4.1 Policy-Induced Variation in Transfers

We begin by documenting the policy variation that underlies our identification. The key comparison is the evolution of transfers for baseline-eligible and baseline-ineligible households around the time of the reform. Because eligibility is determined by income, an inherently endogenous variable, we fix eligibility status using pre-reform administrative data as described in section 2.4.

**Top-up.** Figure 6a plots the average monthly top-up received by eligible and ineligible households. Prior to 2012, neither group received any top-up payments. Following the reform, transfers for the eligible group rise sharply, while the ineligible group exhibits only a modest increase. The presence of a small positive amount for the latter reflects the fact that eligibility is fixed at baseline: some initially ineligible households subsequently fall below the EPL. For this reason, all subsequent estimates should be interpreted as intent-to-treat effects.

**Total transfers.** Figure 6b shows that total PBF transfers display a clear discontinuous shift for treated households relative to controls after the reform. Total transfers include not only the top-up but also the standard PBF components, such as the basic and variable benefits. Because households above the EPL are not eligible for the basic benefit, though they may still receive the variable benefit, ineligible households receive positive but comparatively smaller amounts in the pre-reform years. The gap in total transfers between the two groups widens sharply in the year of the reform and remains sizable for several years, gradually narrowing as some initially eligible households move above the EPL and some initially ineligible households fall below it. This pronounced and persistent divergence in transfers provides the core source of identifying variation for our difference-in-differences design. In the next section, we exploit this policy-induced variation to estimate the causal effects of the expansion.

## 4.2 Identification Strategy

We estimate the effects of the 2012 expansion of *Bolsa Família* using a difference-in-differences event-study design that compares individuals whose baseline household income per capita placed them below the extreme-poverty line with those just above it. Formally, we estimate

$$Y_{it} \;=\; \sum_{s \neq 2011} \beta_s \; \mathbf{1}\{t = s\} \times D_{j(i)} \;+\; \theta_i \;+\; \delta_t \;+\; \varepsilon_{it}, \tag{10}$$

where $Y_{it}$ is the outcome for individual $i$ in year $t$; $\theta_i$ and $\delta_t$ denote individual and year fixed effects; and $\varepsilon_{it}$ is the error term. Because eligibility is determined at the family-level, we cluster standard errors at the 2012 family code following the guidance in Bertrand *et al.* (2004); and Cameron & Miller (2015). We define eligibility based on pre-reform income per-capita, and define,

$$D_{j(i)} = \mathbf{1}(\text{income}_i < \text{R\$ } 70)\,,$$

with R$ 70 representing the statutory extreme-poverty threshold. Because $D_{j(i)}$ is fixed ex-ante, it delivers a time-invariant measure of exposure and shields the design from behavioral income adjustments around the reform. The coefficients $\beta_s$ therefore trace the intention-to-treat effect, i.e., how initial extreme-poverty status shapes subsequent outcomes regardless of later income mobility.

**Validity of the Design.** Identification requires that, conditional on fixed effects, eligibility is uncorrelated with time-varying unobservables that affect outcomes. Equivalently, eligible and ineligible households would have followed parallel trends absent the reform. The event-study estimates below show no evidence of differential pre-trends prior to the 2012 *Bolsa Família* expansion, lending strong support to this assumption.

## 4.3 Labor Market Responses

In this section, we present the causal effects of the Bolsa Família expansion on a broad set of labor market outcomes. We show that, following an exogenous increase in transfers, both employment and earnings per worker rise. These positive labor supply responses are difficult to reconcile with standard neoclassical

models, yet they align closely with the productive inclusion mechanism highlighted in Section 3: when households face subsistence constraints, alleviating basic needs can unlock latent productivity and enable higher labor supply. We then complement the labor market results with reduced-form evidence on health improvements. Taken together, the patterns are consistent with a simple model of productive inclusion in which extremely poor households must first satisfy essential needs before they can fully realize their labor supply potential.

**First stage.** We begin by confirming that the reform generated a sizable and discontinuous increase in transfers for households identified as eligible ex ante. To do so, we estimate a first-stage regression using top-up transfers as the outcome in equation (10). Figure 9a shows a sharp divergence in top-up receipts between eligible and ineligible households immediately after the reform. This discontinuity provides a transparent source of quasi-experimental variation that we then use to study the labor market consequences of an exogenous shift in benefit receipt.

**Employment.** Figure 7 presents the reduced-form effects on formal employment observed in RAIS. Prior to the reform, formal employment for eligible and non-eligible individuals evolves in a similar way, which supports the validity of the research design. Immediately following the expansion of *Bolsa Família*, formal employment rises sharply for the eligible group, suggesting a strong labor market response to the cash transfer shock.

A natural question is whether the increase in formal employment reflects a shift away from informal work or whether total labor supply rises. Although informal employment is inherently difficult to measure, the *Cadastro Único* survey provides individual-level data of informal work for the population of interest. Figure 9b shows that total employment, defined as the sum of formal and informal work, also increases after the reform. The patterns for total and formal employment differ in important ways. Total employment rises and then stabilizes at a sustained but moderate positive level, which is consistent with the idea that once households satisfy their basic needs, they maintain a higher level of labor supply relative to the pre-reform period. Formal employment, in contrast, displays a steeper and more persistent increase. This pattern suggests that the initial rise in labor supply is driven by a relaxation of subsistence constraints, while

the subsequent growth in formal employment could reflect a gradual shift from informal to formal work as individuals gain the stability and resources needed to access formal job opportunities.

**Transition patterns.** To further understand the nature of the employment response, Table 4 decomposes the pooled treatment effect by individuals' baseline employment status in 2012, prior to the reform. We classify each individual into one of three mutually exclusive categories based on their pre-reform labor market status: formally employed, informally employed, or unemployed. We then estimate the same pooled difference-in-differences specification separately for each subgroup, maintaining the individual and year fixed effects used in the main analysis.

Three main findings emerge. First, among individuals who were formally employed at baseline, the program increases formal employment by 12.5 percent relative to the control mean, with a much smaller increase in informal work. This pattern suggests that the transfer helps workers remain attached to the formal sector. Second, among individuals who were unemployed at baseline the program increases informal employment by 30.1 percent while slightly reducing formal employment. Similarly, among workers who were informally employed at baseline, the program reduces informal employment by 27.5 percent mostly through transitions to unemployment. This suggests the expansion increased churn between the informal sector and unemployment.

Third, the average effects in Table 4 suggest some substitution from formal work to informal work (informal workers are 6.5% less likely to become formal and formal workers are 4.3% more likely to become informal on average over the entire 2012–2020 period), while the dynamics shown in Figures 7 and 9b show clear evidence of longer-run increases in the share of formal relative to informal work. This suggests that while the short-run effects of the reform are a relative increase in informal work, in the longer run, productive inclusion leads to the formalization of labor (Banerjee & Newman, 1993). These heterogeneous responses help reconcile the overall increase in employment documented above with the model of productive inclusion developed in Section 3.

**Earnings per worker.** Panel 9c shows that earnings per worker rise following the expansion of *Bolsa Família*, a pattern that mirrors the increase in total employ-

ment documented in Panel 9b.[8] The Brazilian labor market offers limited scope for adjustment along the hours margin, and Table 5 confirms that conditional on being employed, hours worked remain essentially unchanged. This indicates that the increase in earnings is not driven by changes in labor supply at the intensive margin.

The most plausible explanation is instead an increase in worker productivity. The cash transfer allows households to meet basic needs and reduce economic stress, while the health improvements documented in Section 4.4 further enhance individuals' capacity to work. Both mechanisms are consistent with the model in Section 3, in which relaxing subsistence constraints raises productivity and leads to higher earnings.

## 4.4 Health Improvements

The model in Section 3 suggests that cash transfers can relax subsistence constraints and, in doing so, improve not only labor market outcomes but also fundamental dimensions of individual well-being, including health. To examine this mechanism directly, we link our administrative datasets to the universe of public hospital admissions in Brazil.[9] For each hospitalization, we observe the diagnosis at admission, the associated cost of the hospital stay, and the outcome at discharge, including mortality. Consistent with the model's predictions and with the labor market responses documented above, we find substantial improvements in health outcomes along several dimensions.

**Mortality.** We find that the additional cash transfer reduces mortality substantially. The PBF expansion lowers the annual death by roughly 0.022 percentage points, which corresponds to a decline of about 14 percent relative to the baseline death probability in the family. This effect implies that the program saves 1.4 lives

[8] As discussed in Section 2, outcomes from *Cadastro Único* (CU) are observed only from 2012 onward, which prevents us from estimating long pre-trends for CU-based variables. Reassuringly, for outcomes measured in datasets with longer pre-reform coverage, namely RAIS and SUS, we find stable pre-trends prior to the expansion (see Figures 7 and 8). For CU outcomes, we observe sharp changes immediately after the reform followed by stabilization, a pattern that is consistent with a causal response to the *Bolsa Família* expansion.

[9] Brazil's public health system (*Sistema Único de Saúde*, SUS) is universally accessible and free at the point of care. According to the *Pesquisa Nacional de Saúde* (IBGE, 2019), low income Brazilians rely exclusively on SUS for all health services, with private insurance concentrated among higher income households.

per thousand families. Scaling it by the number of beneficiary families, it saves overall 1,000 lives.[10] The estimates are highly significant at the 1 percent level and show no evidence of pre-trends, as shown in Figure 8. The gradual accumulation of the effect over time, evident in Figure 8, is consistent with health improvements that materialize progressively as households sustain higher levels of basic consumption. Mortality is the most fundamental measure of health, and the fact that a relatively inexpensive cash transfer can generate such improvements has important implications per-se, well beyond the labor market consequences.

**Main health outcomes.** Panel A of Table 6 shows that the expansion of cash transfers reduces both mortality and hospitalization rates, providing additional evidence of meaningful health improvements. Specifically, eligible individuals are 6 percent less likely to be hospitalized. The table also indicates that households increase monthly medication expenditure by approximately 48 percent.[11] This pattern is consistent with the idea that the additional income enables individuals to meet basic medical needs that were previously unaffordable. Together, these results reinforce the view that relaxing subsistence constraints improves health in ways that complement the labor market effects documented above.

**Diagnosis at admission.** Consistent with this interpretation, Panel B of Table 6 examines diagnoses at hospital admission and shows that eligible individuals become less likely to be hospitalized for conditions related to sub-nutrition. We also find declines in hospitalizations due to infectious, respiratory and digestive diseases. These patterns align with the idea that the additional transfers allow households to meet essential needs, strengthen their underlying health status, and reduce vulnerability to preventable illnesses.

**Costs of hospitalization.** For individuals who are hospitalized, the data allow us to observe both the length of stay and the associated public cost of each admis-

[10] Because individuals can die only once, the most granular unit that allows for meaningful time variation in mortality outcomes is above the individual level. Consistent with the literature, our main specification therefore analyzes mortality using outcomes aggregated from the individual level (Otero & Munoz (2022); Walker *et al.* (2025)). In our setting, the natural unit of aggregation is the family, and we conduct the main mortality analysis at the family level, as shown in Figure 8. For completeness, we also replicate the analysis at the individual level and obtain qualitatively similar results.

[11] Despite public hospitals in Brazil being free of charge, medication purchased outside of hospitals is fully paid out-of-pocket, even if prescribed by a SUS doctor. This is different than, for example, the UK NHS which features a small, flat co-pay for medication.

sion. Although patients do not pay out of pocket, the hospital system records the fiscal cost of all treatments. Health improvements appear not only through the reduction in hospitalization rates but also along the intensive margin. Following the 2012 *Bolsa Família* expansion, eligible individuals spend about 6 percent fewer days in the hospital, and the average cost of hospitalization falls by roughly 10 percent. These fiscal savings are important, and incorporated into the cost-benefit analysis in the next section.

## 4.5 Robustness

We assess the robustness of our findings to alternative definitions of the comparison group and concerns about strategic income misreporting. Across all specifications, the estimated effects remain stable, reinforcing the credibility of the research design and the substantive conclusions of the main analysis.

**Validity of the control.** Our main identification strategy compares individuals whose baseline income placed them below the extreme-poverty (EP) line with those just above it. A potential concern is that individuals well above the cutoff may differ systematically from those just below it, so that their post-reform trajectories may not provide a credible counterfactual for marginally ineligible households.

For this reason, our preferred specification restricts the control group to individuals with baseline income between the EP line and the poverty line (PL), improving comparability between treated and control units. Table 7 shows that the results are qualitatively similar across alternative control definitions, including specifications that compare households below EP to those above EP or above PL. The sign, significance, and the main economic conclusions remain stable across designs.

**Strategic misreporting.** Another concern is that households narrowly above the EP cutoff may understate income in order to qualify for the guaranteed-income component, potentially contaminating the control group. We address this by implementing a donut design that excludes households with baseline income between the EP and PL thresholds. Comparing households below EP to those above PL removes precisely the observations most likely to be affected by such behavior.

Table 7 shows that the estimated effects are qualitatively unchanged under this alternative specification. If anything, any manipulation would tend to bias estimates upward in the baseline design that includes households just above the EP cutoff, who are most likely to engage in such behavior. The fact that estimates are similar or smaller when these households are excluded from the control group further alleviates the strategic misreporting concern.

**Control variables.** The main specification presented in Figure 9 includes individual and year fixed effects. Table 5 shows that the results are very similar if we instead use family and year fixed effects in Column 2 or year $\times$ municipality fixed effects in Columns 3 and 4. Estimates for log earnings and log hours are based on narrower samples by construction, but the broader pattern of positive employment and earnings effects remains unchanged.

# 5 Mechanisms

Our findings to this point run counter to the neoclassical prediction: across specifications, transfers increase employment rather than reduce it. This section studies the channels through which the 2012 *Bolsa Família* expansion may affect recipients' health, and we link these mechanisms to the employment gains documented earlier. Before turning to these channels, we discuss alternative mechanisms.

## 5.1 Alternative Mechanisms

Beyond the productive inclusion mechanism discussed above, it is important to consider alternative channels that could, in principle, generate the labor market responses we observe. This subsection examines these possibilities using the rich individual-level data and the large policy shock.

**Multiplier.** Another potential explanation is a local multiplier effect. The idea is that when many residents of a given location receive additional income, demand for local goods and services may rise and stimulate local labor markets. Gerard *et al.* (2025) measures this phenomenon using municipality-level variation, while Egger *et al.* (2022) document it in an experimental setting. We take advantage of

the rich data and granular variation to test this hypothesis by running individual-level regressions with location fixed effects. We define location, following Gerard *et al.* (2025), at the municipality level. Table 5 shows that the employment effects remain similar once these fixed effects are included. This pattern is difficult to reconcile with a pure multiplier story. If the reform had stimulated local economic activity more broadly, ineligible individuals in the same municipality should also have experienced improved labor market outcomes, which would reduce the difference between eligible and ineligible groups. A natural explanation for the limited role of general equilibrium effects in our setting is that the reform we study modified only one component of Bolsa Família and affected a subset of beneficiaries, whereas the policy variation in Gerard *et al.* (2025) captures the rollout of the entire transfer program at the local level.

**Childcare.** Another possible mechanism is childcare. Using a different source of variation, Leite Mariante (2025) argues that the employment effects of cash transfers are concentrated among women, consistent with improved childcare facilitating labor supply in that setting. Our evidence points in a different direction. As shown in Table 8, the employment effects are positive for both heads of household and spouses, and the magnitudes are fairly similar. If anything, the effects are somewhat larger for spouses, who are overwhelmingly male in our sample. This pattern goes against a childcare-based explanation, which would predict a disproportionately stronger response among women.[12]

This difference is consistent with the nature of the policy shock and the population affected. The 2012 *Bolsa Família* expansion modified only one component of an existing transfer program and targeted households at the very bottom of the income distribution. For this group, the guaranteed-income top-up is unlikely to be sufficient to finance formal childcare arrangements, and childcare is plausibly not the binding constraint on labor supply. Instead, extremely poor households face more fundamental barriers related to health, nutrition, and basic subsistence. In this context, increased labor supply is more plausibly driven by the relaxation of subsistence constraints affecting both spouses, while childcare considerations

[12]Beyond household heads and their spouses we have studied the effects on children in beneficiary households. We find a negative short-run employment effect for children which is consistent with them getting more education, as in de Janvry *et al.* (2006). Further exploring this channel is a promising avenue for future work.

are likely to play a larger role in settings with more generous transfers and households less constrained by basic needs.

**Violence.** Another potential channel is a reduction in violence-related harm. One possibility is that improved economic conditions might reduce exposure to violent incidents. Although our data are not ideally suited to study this mechanism, we can examine violence-related admissions in the hospital records. We classify all hospitalizations associated with violence and estimate the effect of the transfer on the probability of such an admission. Panel B of Table 6 shows that the estimated effect is small and statistically insignificant.

A null result could be plausible because criminal behavior and involvement in violence reflect complex decisions and social dynamics that may not shift meaningfully in response to an increased cash transfer. Moreover, basic intuition yields ambiguous predictions about crime participation: improved well-being might reduce exposure to risky environments, yet meeting basic needs could also strengthen individuals in ways that do not necessarily diminish the likelihood of violence-related incidents. Given these considerations, it is not unreasonable that the expansion of *Bolsa Família* does not generate detectable changes in violence-related health outcomes.

## 5.2 Productive inclusion

The model in Section 3 suggests a simple interpretation: when basic needs are unmet, extremely poor households may be constrained in their ability to work. By guaranteeing a minimum level of resources for households below the extreme-poverty line, the 2012 *Bolsa Família* expansion can relax this constraint, allowing health and productivity to improve and labor supply to rise. Consistent with this mechanism, Section 4.4 documents sizable improvements in mortality, hospitalization, and preventable disease. We now provide more direct evidence on the margins through which these health improvements arise.

### 5.2.1 Medication Expenditure

Consistent with the broader evidence on health improvements, and with the idea that additional income enables households to meet basic medical needs, we find that the PBF expansion leads to a substantial rise in medication expenditure.

*Cadastro Único* records monthly spending on medications, which allows us to track this response at the individual level. Panel 10a plots raw averages for eligible and ineligible households before and after the reform. Eligible households display a clear and immediate increase in medication spending once the transfer is introduced. In contrast, the control group exhibits a modest decline in spending after 2014, which coincides with the onset of the 2015 economic crisis and is consistent with reduced purchasing power among low-income households not receiving the guaranteed income.

We next estimate the effect of the expansion using the main empirical specification in Equation 10. To facilitate interpretation, we scale the estimated coefficients by baseline medication expenditure, so the post-estimation results can be read as percentage changes relative to the pre-reform mean. Figure 10b plots the event-study coefficients. Medication expenditure for the two groups evolves in parallel prior to the reform, which supports the validity of the design. Following the expansion of *Bolsa Família*, treated households exhibit a sharp and sustained increase in medication spending. The effect reaches roughly 60 percent of baseline expenditure, a sizable response that is consistent with the idea that relaxing subsistence constraints enables households to afford essential medications. This mechanism likely contributes to the broader health improvements documented in Section 4.4.[13]

**Nutrition.** Medication expenditure is unlikely to account for the full set of health effects generated by the transfer. Consistent with the idea that additional income enables households to satisfy other basic needs, we also find suggestive evidence of improvements in nutrition. Panel B of Table 6 shows that, among hospitalized individuals, admissions related to sub-nutrition fall by roughly 38 percent after the expansion. Better nutrition and increased access to medications reinforce one another and help explain both the health gains and the stronger labor supply observed among eligible households. This mechanism is consistent with the long-standing idea, articulated in Dasgupta & Ray (1986), that inadequate nutrition can limit individuals' capacity to work and that relaxing subsistence constraints can generate productivity and welfare improvements.

[13]Prior work in other settings has found that smaller transfers often generate limited or no detectable health effects (see Baird *et al.* (2019); Haushofer & Shapiro (2018)).

# 6 Marginal Value of Public Funds

Having documented the effects of the transfer expansion and explored the underlying mechanisms, we now turn to a transparent framework for assessing the welfare implications of this policy change. Following Finkelstein & Hendren (2020), we compute the marginal value of public funds (MVPF), defined as the ratio of the willingness to pay of affected households to the net fiscal cost to the government. The latter accounts for behavioral responses that alter tax revenue or program spending. The MVPF provides a simple and interpretable measure of the welfare gain generated per unit of government expenditure. It also facilitates comparisons between the *Bolsa Família* expansion and alternative interventions targeted at similar populations.

Because the reform relaxes productivity constraints for a subset of beneficiaries, it can raise productivity and change effort and earnings choices in ways that have first-order welfare effects. The envelope theorem therefore need not apply to all beneficiaries: if it did, these behavioral responses would have no first-order welfare effects, and willingness to pay would simply equal the additional benefits received. We therefore account for these welfare effects explicitly. In appendix B.5, we return to the model in Section 3 and show that willingness to pay can be bounded as

$$WTP \in \left(\Delta b, \Delta b + \Delta y - P\tfrac{\varepsilon}{1+\varepsilon}\Delta y_{\mathcal{P}}\right] \tag{11}$$

where $\Delta b$ is the average increase in benefits received; $\Delta y$ is the average increase in earnings; $\mathcal{P}$ is the set of households whose participation decision changes as a result of the reform; $P$ is the fraction of households whose participation decision changes; and $\Delta y_{\mathcal{P}}$ is the average earnings change among these households, $\mathsf{E}\left[\Delta y_i | i \in \mathcal{P}\right]$. The derivation of the bound, the specific data inputs, and the full numerical implementation of each component are detailed in appendix B.5. Table 9 reports the welfare calculation, decomposing the two main channels through which the expansion affects recipients.

We first consider only the gains that materialize through labor market outcomes. This mirrors the type of evidence typically available in settings where researchers do not have access to linked administrative health data, and it helps clarify how much is missed when the analysis relies exclusively on earnings re-

sponses. Using only labor market benefits, we estimate an MVPF of 3.9. This implies that each additional dollar of net government expenditure generates about 3.9 dollars of value for program beneficiaries. Focusing only on labor market outcomes, our estimate is close to that in Gerard *et al.* (2025), who also find an MVPF of similar magnitude based on earnings responses alone. This parallel is notable given the different sources of variation exploited across the two studies and underscores that labor market gains can generate substantial welfare benefits even when attention is restricted to earnings. At the same time, incorporating health impacts is central to a full accounting of the welfare consequences of transfer programs.

When we incorporate the health improvements documented in Section 4.4, the conclusions change markedly. The implied MVPF becomes infinite because the behavioral responses induced by the policy reduce public health expenditures by more than the direct fiscal cost of the transfer. In the terminology introduced by Hendren & Sprung-Keyser (2020), an infinite MVPF signals a policy that pays for itself within the accounting exercise and is therefore preferable to any policy with a finite MVPF.[14] Figure 11 summarizes the MVPF analysis with and without accounting for health impacts.

From the perspective of recipients, the dominant labor market benefit is the increase in earnings, while on the health side the most consequential gain is the reduction in mortality. To express the reduction in mortality in monetary terms, we draw on the standard practice in economics of assigning a statistical value of life (VSL). Several approaches exist for calculating a VSL, but we follow the revealed preference methodology used by Berry *et al.* (2020) and Kremer *et al.* (2011), who estimate VSLs for populations with income levels similar to those studied here. In our main exercise, we adopt the midpoint of the range of statistical values of life reviewed by Berry *et al.* (2020). However, the precise valuation of lives saved is not pivotal for the welfare analysis in our setting, since the reform generates fiscal savings that exceed its direct cost. As a result, the MVPF is infinite regardless of how the additional welfare gains from reduced mortality are valued.

[14]Under the standard MVPF formula in the last row of Table 9, a policy that pays for itself would yield a negative MVPF. We follow Hendren & Sprung-Keyser (2020) in labeling this case as an infinite MVPF to convey more clearly its welfare ranking relative to policies with positive and finite MVPFs.

On the fiscal side, we begin by computing the mechanical cost of the reform, which reflects the additional transfers provided to eligible households. From this amount, we subtract the behavioral responses that generate savings for the public budget. Several channels contribute to these offsets. First, as individuals increase their labor supply, they become less reliant on *Bolsa Família* in subsequent years, reducing program outlays. Second, improvements in health reduce the likelihood of hospitalization and shorten the duration and cost of hospital stays when they occur, which lowers public health expenditures. Third, the rise in employment increases payroll tax contributions. Although beneficiaries are exempt from income taxation given their low earnings, they nonetheless contribute through mandatory payroll taxes. To translate these components into aggregate fiscal effects, we scale the individual-level costs and savings by the number of beneficiaries and by the years over which the reform operates. This procedure yields the net fiscal cost used in the MVPF calculations.

# 7 Conclusion

This paper studies the labor market, health, and welfare effects of a large expansion of Brazil's Bolsa Família program that introduced a guaranteed income for the extreme poor. Exploiting sharp, plausibly exogenous variation around the extreme-poverty line and rich administrative microdata covering the universe of low-income households, we document three main findings. First, the expansion increased formal employment by 14 percent. The effects are positive for both heads of household and spouses, with somewhat larger estimates for spouses. Second, the expansion improved health outcomes, including hospitalization, public health expenditures, and mortality. Third, these health gains translated into substantial fiscal savings, so that once health is incorporated, the program appears to pay for itself in our MVPF accounting.

These findings challenge the canonical view that cash transfers discourage work. In our setting, the opposite occurs. The evidence points to a mechanism in which transfers relax binding subsistence and health constraints, raising productivity and enabling greater labor supply. We formalize this mechanism in a model of productive inclusion that extends standard participation frameworks to allow income to feed back into productivity. The model rationalizes the positive

employment responses observed in low-income settings and provides a disciplined structure for welfare analysis. From a welfare perspective, focusing only on labor market outcomes yields substantial gains, in line with prior work. Incorporating health effects, however, fundamentally alters the welfare assessment, as fiscal savings from improved health exceed the program's direct cost, underscoring that ignoring health responses may substantially mischaracterize the welfare consequences of transfer policy.

More broadly, our findings suggest that the impacts of cash transfers depend critically on the economic environment in which they operate. In settings where households are constrained by basic needs, transfers can function as productivity-enhancing investments rather than pure redistribution. This perspective helps reconcile the divergent labor supply responses documented across high- and low-income contexts and highlights the importance of incorporating health and subsistence constraints into models used for policy design.

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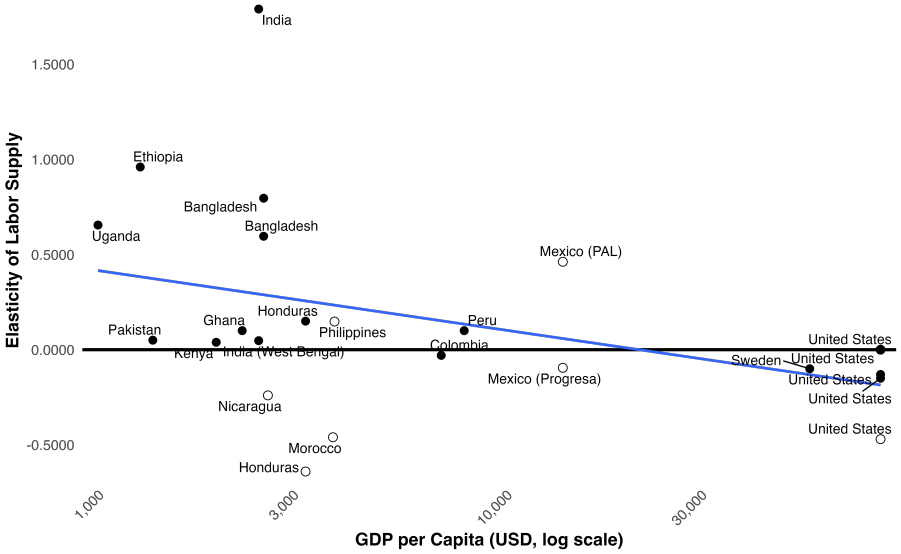
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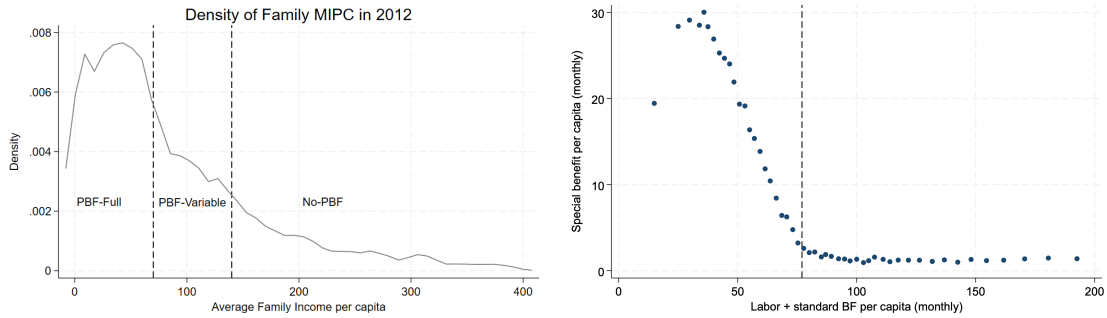
# Figures and Tables

Figure 1: Labor Supply Elasticities Around the World




*Note:* This figure provides a meta-analysis of labor supply responses to cash transfers around the world. Each dot represents an elasticity estimate from a different paper. Hollow markers denote estimates that are not statistically significant, whereas filled markers denote statistically significant estimates. The fitted line reports the relationship between a country's GDP per capita and the estimated labor supply response. Some studies report elasticities directly; for others, we calculate elasticities from the underlying employment or hours estimates. More details on the meta-study methodology appear in Appendix A.

Figure 2: Exposure to the 2012 Reform

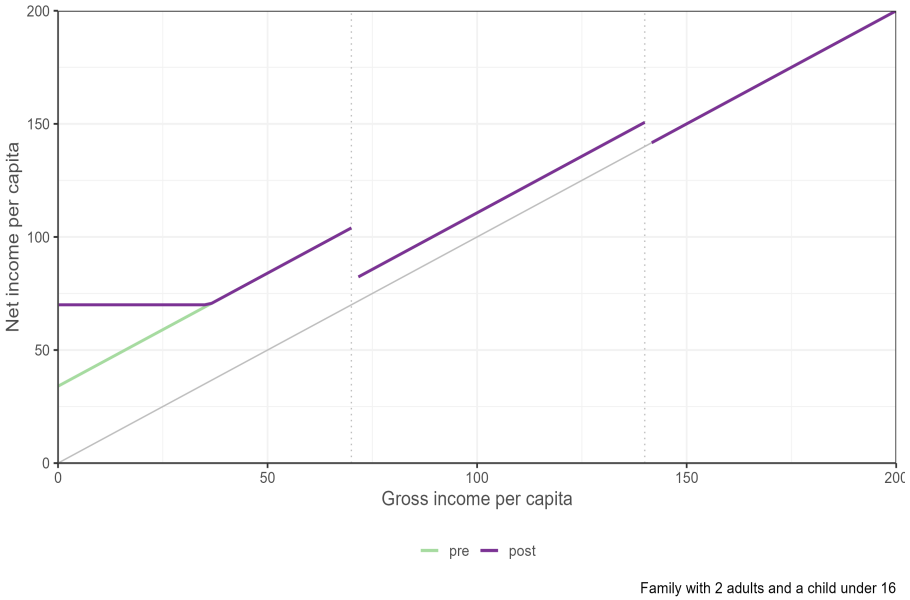



(a) Density of 2012 MIPC

(b) Benefit Schedule

*Note:* Panel (a) plots the kernel density of monthly income per capita (MIPC) in 2012 and indicates the locations of the extreme-poverty line (EPL) and the poverty line (PL). Households are classified into three mutually exclusive groups, labeled in the figure: PBF-Full (MIPC $<$ EPL), PBF-Variable (EPL $\leq$ MIPC $<$ PL), and No-PBF (MIPC $\geq$ PL). Panel (b) plots total Bolsa Família guaranteed-income transfers in 2014 against pre-top-up total resources per capita, using fine income bins. The pre-top-up is computed using a combination of two datasets: Cadastro Único for MIPC, and Bolsa Família disbursement data for other baseline transfers. The vertical line marks the extreme-poverty line (EPL).

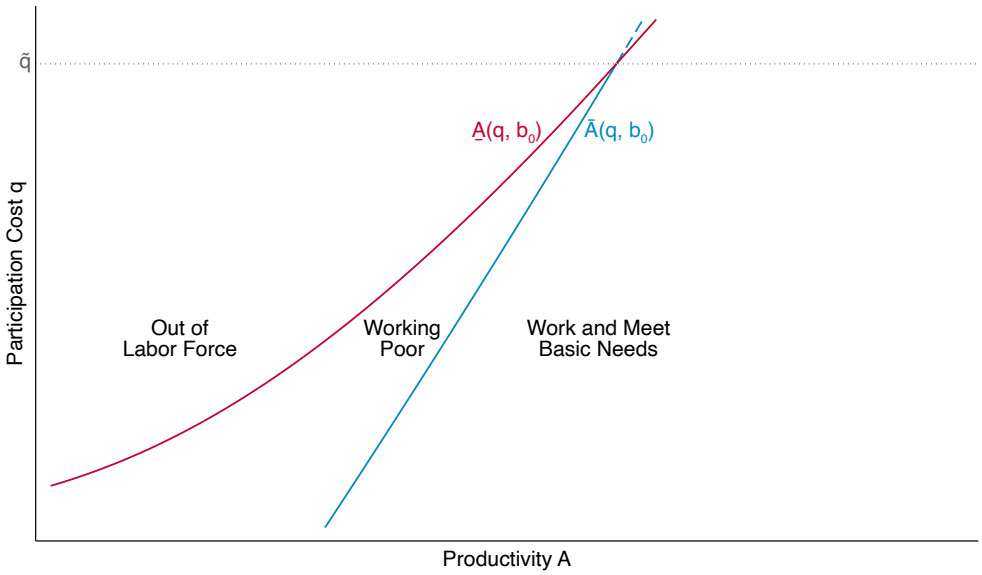
Figure 3: Budget Set Before and After the Expansion



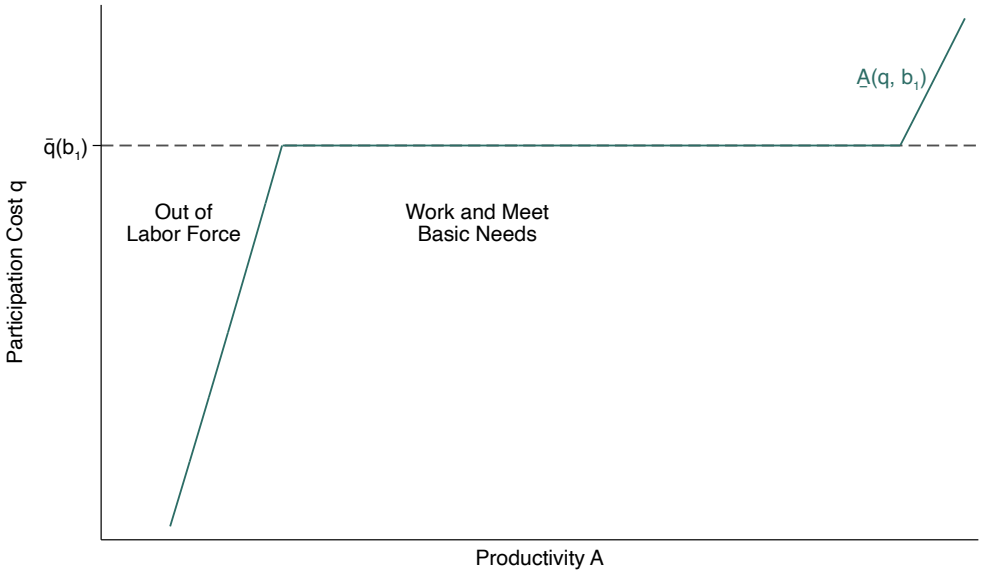

*Note:* The figure shows pre- and post-reform net income as a function of gross labor income for a family with two adults and one child younger than 16. Prior to the reform, net income increases with earnings, incorporating the basic and child benefits. After the 2012 expansion, families with MIPC below the EPL receive a top-up that makes net income flat up to the EPL, implying a 100 percent marginal tax rate over that range.

Figure 4: Household Choices in The Productive Inclusion Model

**PANEL A: CHOICES UNDER THE BASIC INCOME**




**PANEL B: CHOICES UNDER THE GUARANTEED INCOME**




*Note:* The panels illustrate household choices under the productive inclusion model. Panel A corresponds to the basic-income schedule and Panel B to the guaranteed-income schedule. Household choices vary with ability and participation costs, which determine whether households remain out of work, work while below basic needs, or work while meeting basic needs.

Figure 5: Introducing the Guaranteed Income

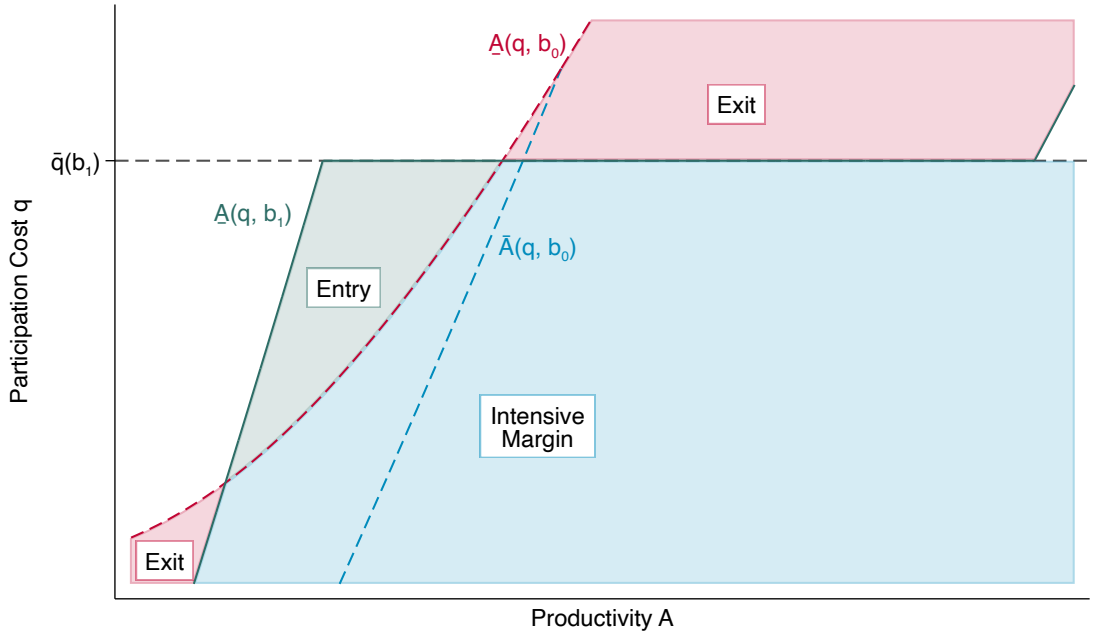
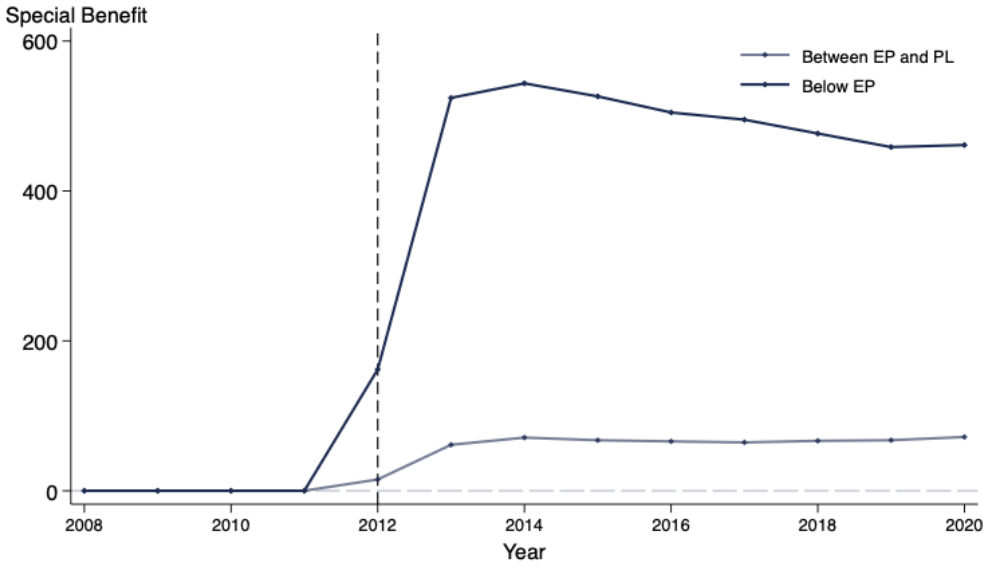
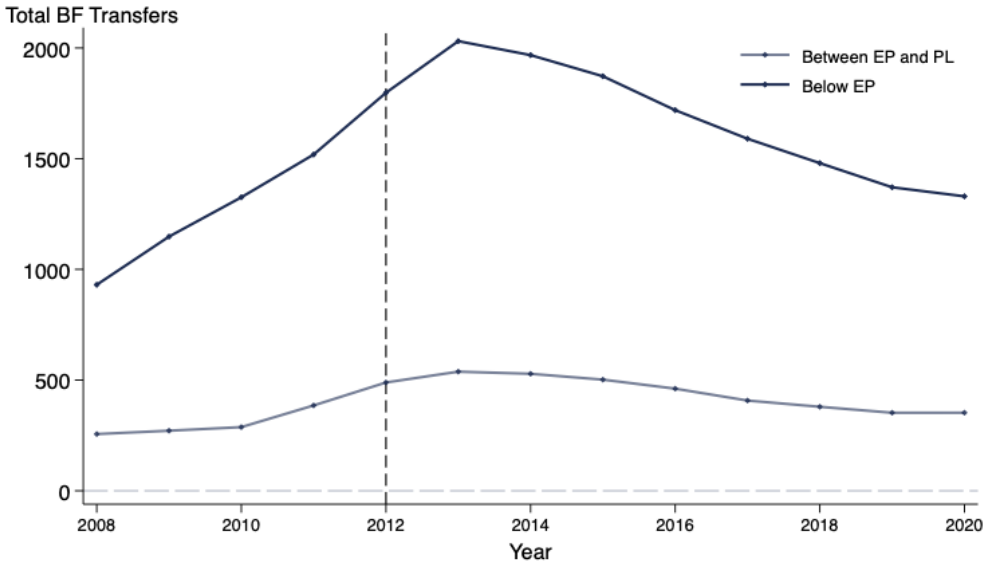


Figure 6: PBF Disbursements

(a) Guaranteed-income top-up

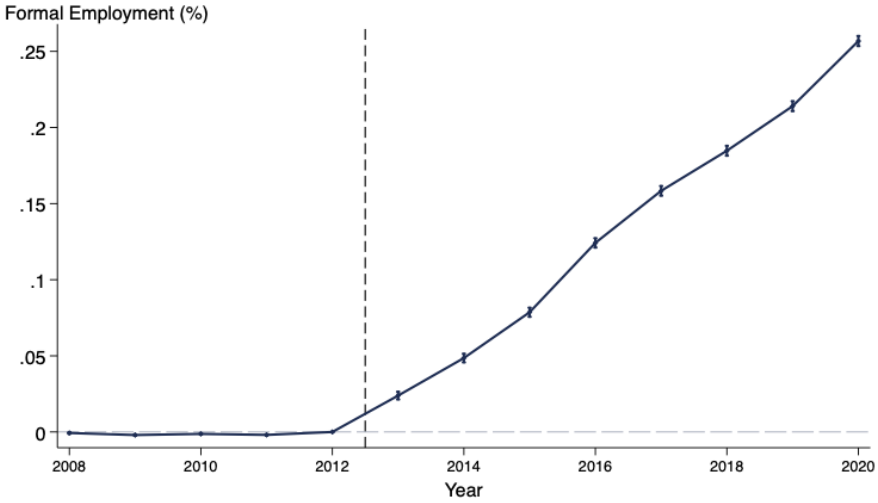



(b) Total Bolsa Família transfers



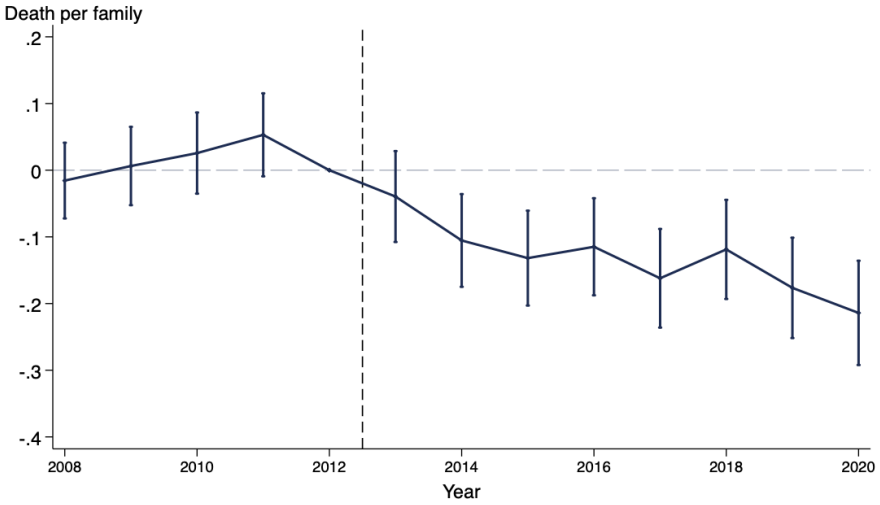

*Note:* This figure reports average monthly transfers by year for baseline-eligible individuals, defined as those below the EPL in 2009 to 2011, and baseline-ineligible individuals between the EPL and the PL. Panel A shows the guaranteed-income top-up introduced in 2012, and Panel B shows the total Bolsa Família payments. The vertical line marks the year of the expansion.

Figure 7: Employment Effect



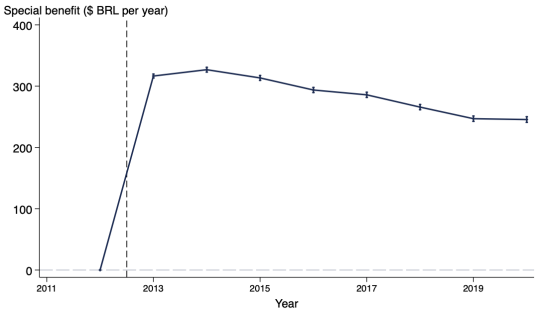

*Note:* This figure plots event-study estimates of the effect of the 2012 *Bolsa Família* expansion on the probability of formal employment. Coefficients correspond to the relative change in employment for individuals whose baseline income placed them below the extreme-poverty line, relative to those just above it. Estimates present the % effects by dividing the level effect by the baseline formal employment rate. Estimates include individual and year fixed effects, and standard errors are clustered at the 2012 family level.

Figure 8: Mortality Effect

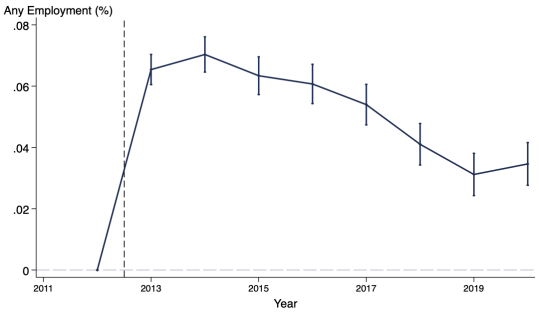



*Note:* This figure plots event-study estimates of the effect of the 2012 *Bolsa Família* expansion on family mortality. The estimates are conducted at the family level, with the outcome equal to the total number of deaths in the family. Coefficients correspond to the relative change in mortality for families whose baseline income placed them below the extreme-poverty line, relative to those just above it. Percentage effects are computed by dividing the level effect by the baseline mortality rate. Estimates include family and year fixed effects, and standard errors are clustered at the 2012 family level.

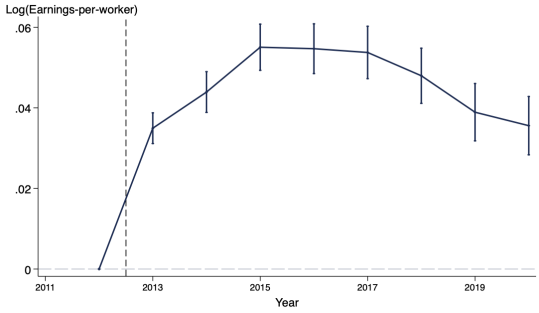
Figure 9: Margins of Response




(a) First Stage



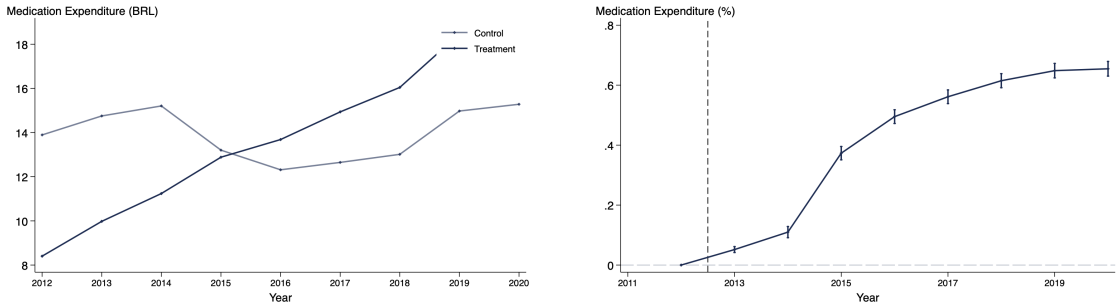

(b) Any employment




(c) Earnings

*Note:* This figure reports event-study estimates for the main labor market outcomes. Each panel refers to a different outcome using the same regression specification, as in Equation 10. Standard errors are clustered at the 2012 family level.

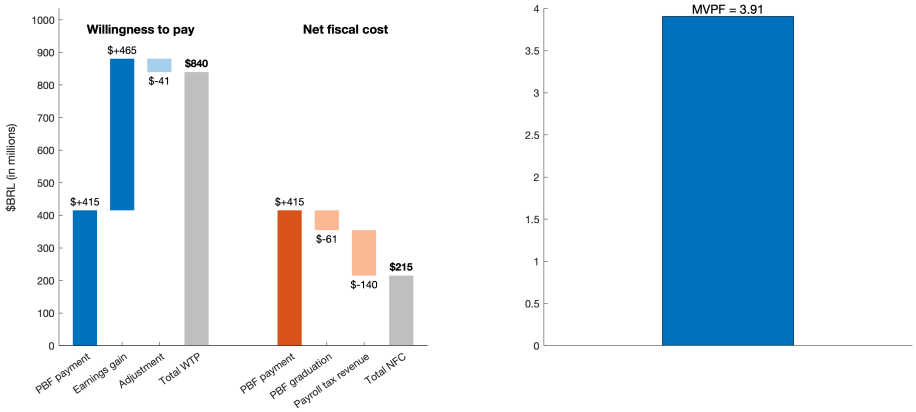
Figure 10: Medication Expenditure



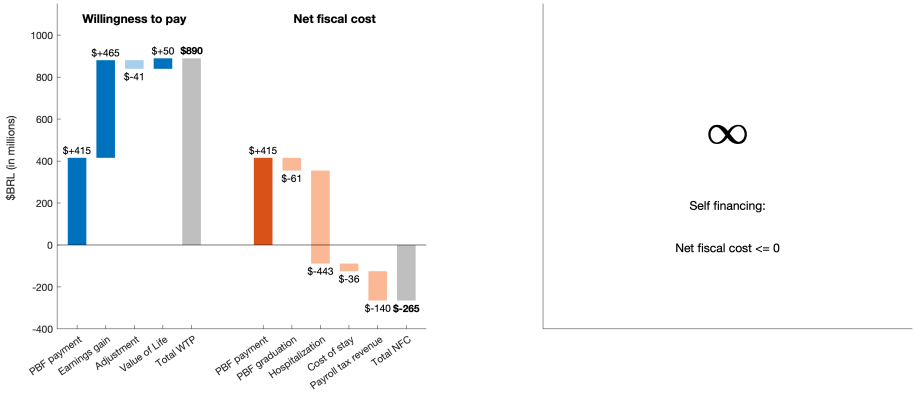

(a) Medication Expenditure - Raw Average (b) Medication Expenditure - Effect

*Note:* This figure reports the effect of the 2012 *Bolsa Família* expansion on monthly medication expenditure. Panel A plots raw averages for baseline-eligible and baseline-ineligible households. Panel B presents event-study estimates obtained from Equation 10, with the treatment effect in levels scaled by baseline expenditure to express results in percentage terms. The event-study specifications include individual and year fixed effects, and standard errors are clustered at the 2012 family level.

Figure 11: MVPF




(a) MVPF (no health)




(b) MVPF (including health)

*Note:* The figure reports the marginal value of public funds (MVPF) for the program. Panel A excludes health outcomes, while Panel B includes both labor-market and health benefits. Willingness to pay combines earnings gains and, when included, monetized health benefits using the value of a statistical life. Net fiscal cost sums mechanical program costs, behavioral fiscal responses, health-related savings, and additional payroll tax revenue, with negative values indicating reductions in government spending. The MVPF is defined as willingness to pay divided by net fiscal cost. When net fiscal cost is non-positive, the MVPF is reported as infinity.

Table 1: Benefits and Threshold Adjustments (in current R$)

| | 2011 | 2014 | 2016 | 2018 |
|---|---|---|---|---|
| **Panel A: Eligibility thresholds (per capita)** | | | | |
| Poverty line (PL) | 140 | 154 | 170 | 178 |
| Extreme-poverty line (EPL) | 70 | 77 | 85 | 89 |
| **Panel B: Benefit values** | | | | |
| Basic benefit (per family) | 70 | 77 | 85 | 89 |
| Variable I (per child $< 16$) | 32 | 35 | 39 | 41 |
| Variable II (per teen 16 to 18) | 38 | 42 | 46 | 48 |

*Note:* This table reports the statutory thresholds and benefit values used to compute Bolsa Família transfers over time. Eligibility is determined using per-capita income reported in the federal social registry (Cadastro Único).

Table 2: Individuals' Characteristics (2012)

| Variable | Mean | SD |
|---|---|---|
| **Panel A. All individuals** | | |
| *A1. Age distribution (%)* | | |
| Below 16 | 33.8 | 47.3 |
| Btw 16 and 17 | 5.3 | 22.5 |
| Btw 18 and 24 | 13.7 | 34.4 |
| Btw 25 and 34 | 14.2 | 34.9 |
| Btw 35 and 54 | 22.7 | 41.9 |
| Btw 55 and 64 | 5.6 | 23.1 |
| 65 and older | 4.6 | 21.0 |
| *A2. Sociodemographics (%)* | | |
| Male | 45.1 | 49.8 |
| Head of household | 30.7 | 46.1 |
| Spouse | 13.7 | 34.4 |
| Informal Worker | 9.2 | 29.0 |
| Formal Worker | 13.2 | 33.8 |
| **Panel B. Formal Workers** | | |
| Blue collar (%) | 76.1 | 42.7 |
| N months | 7.73 | 4.37 |
| Av. monthly earnings | 1513.57 | 1142.51 |

*Note:* This table reports means and standard deviations for the 2012 *Cadastro Único* universe. Labor market variables are measured using RAIS. "Formal worker" indicates having at least one RAIS job during the year.

Table 3: Families' Characteristics (2012)

| Variable | Mean | SD |
|---|---|---|
| **Panel A. All families** | | |
| *A1. Household composition (%)* | | |
| Single-headed | 37.9 | 48.5 |
| 2 adults | 39.1 | 48.8 |
| No children $< 16$ | 39.8 | 48.9 |
| 2 children $< 16$ | 19.1 | 39.3 |
| No teen | 83.8 | 36.8 |
| *A2. Sociodemographics (%)* | | |
| Below EPL | 46.7 | 49.9 |
| Btw EPL and PL | 23.6 | 42.5 |
| Above PL | 29.7 | 45.7 |
| A formal worker | 33.5 | 47.2 |
| PBF recipient | 55.2 | 49.7 |
| **Panel B. PBF recipients** | | |
| Av. annual transfer | 1434.48 | 770.22 |
| Monthly income per capita (excl. PBF) | 65.34 | 112.94 |
| Monthly income per family (excl. PBF) | 233.50 | 329.69 |

*Note:* Household-level characteristics for the 2012 baseline sample. Income categories are based on pre-transfer monthly income per capita (MIPC) relative to the extreme-poverty line (EPL) and poverty line (PL). "PBF recipient" indicates any Bolsa Família payment in 2012.

Table 4: Employment Transitions by Baseline Status

| | (1) Informal Emp | (2) Formal Emp |
|---|---|---|
| **Panel A.** Pooled DD | | |
| Post × Eligible | $-0.007^{***}$ | $0.143^{***}$ |
| | ( 0.001) | ( 0.001) |
| **Panel B.** Separate regressions by baseline status (2012) | | |
| Baseline unemployed | $0.288^{***}$ | $-0.013^{***}$ |
| | ( 0.001) | ( 0.001) |
| Baseline informal | $-0.268^{***}$ | $-0.065^{***}$ |
| | ( 0.002) | ( 0.003) |
| Baseline formal | $0.043^{***}$ | $0.135^{***}$ |
| | ( 0.001) | ( 0.003) |
| Individual + Year FE | ✓ | ✓ |
| *N* (Panel A) | 214,375,863 | 214,375,863 |
| *N* (Unemp) | 128,973,117 | 128,973,117 |
| *N* (Informal) | 48,184,929 | 48,184,929 |
| *N* (Formal) | 37,217,817 | 37,217,817 |

*Note:* This table decomposes the pooled treatment effect by individuals' pre-reform employment status in 2012. The three baseline categories—formally employed, informally employed, and unemployed—are mutually exclusive and exhaustive among card holders. Estimates are expressed as a share of the control-group mean within each outcome column. The baseline average for informal is 0.2784 and for formal employment is 0.1598. Panel A reports the pooled DD coefficient from the full sample. Panel B reports estimates from separate regressions, each restricting the sample to the indicated baseline subgroup. All specifications include individual and year fixed effects. Standard errors clustered at the 2012 family level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Robustness Across Fixed Effects

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Formal Emp | 0.143*** | 0.143*** | 0.133*** | 0.133*** |
| | ( 0.001) | ( 0.001) | ( 0.001) | ( 0.001) |
| Any Emp | 0.048*** | 0.048*** | 0.040*** | 0.040*** |
| | ( 0.001) | ( 0.001) | ( 0.001) | ( 0.001) |
| Log Earnings | 0.050*** | 0.061*** | 0.059*** | 0.061*** |
| | ( 0.001) | ( 0.001) | ( 0.001) | ( 0.001) |
| Log Hours | 0.001*** | 0.001*** | 0.000** | 0.001*** |
| | ( 0.000) | ( 0.000) | ( 0.000) | ( 0.000) |
| Individual FE | ✓ | × | ✓ | × |
| Family FE | × | ✓ | × | ✓ |
| Year FE | ✓ | ✓ | × | × |
| Year x Muni FE | × | × | ✓ | ✓ |

*Note:* This table reports intention-to-treat estimates of the 2012 *Bolsa Família* expansion on labor-market outcomes. Each column corresponds to a distinct set of fixed effects indicated in the bottom panel. Standard errors, clustered at the 2012 family level, are shown in parentheses.

Table 6: Health Improvements

| | Level effect | % effect |
|---|---|---|
| **Panel A. Main** | | |
| Prob hospitalization | -0.00234*** | -0.057*** |
| | ( 0.00011) | ( 0.003) |
| Death in the family | -0.00022*** | -0.144*** |
| | ( 0.00002) | ( 0.014) |
| Medication expenditure | 6.07704*** | 0.483*** |
| | ( 0.11747) | ( 0.009) |
| **Panel B. Diagnosis at admission** | | |
| Nutrition | -0.00002*** | -0.168*** |
| | ( 0.00000) | ( 0.041) |
| Infectious | -0.00052*** | -0.200*** |
| | ( 0.00002) | ( 0.008) |
| Respiratory | -0.00024*** | -0.089*** |
| | ( 0.00003) | ( 0.010) |
| Digestive | -0.00046*** | -0.091*** |
| | ( 0.00003) | ( 0.007) |
| Aggression | -0.00000 | -0.379 |
| | ( 0.00000) | ( 0.692) |
| Skin | -0.00001 | -0.021 |
| | ( 0.00001) | ( 0.017) |
| **Panel C. Length and costs** | | |
| Days hospitalized | -0.00392*** | -0.020*** |
| | ( 0.00121) | ( 0.006) |
| Cost of hospitalization | -4.39446*** | -0.099*** |
| | ( 0.30483) | ( 0.007) |



*Note:* This table reports intention-to-treat estimates of the 2012 *Bolsa Família* expansion on health outcomes, including hospitalization, mortality, medication expenditures, diagnostic categories at admission, and hospitalization costs. The first column reports effects in levels. Each cell reports the coefficient from a separate regression including individual and year fixed effects. The only exception is "death in the family," which is estimated at the 2012 family level and therefore in-

Table 7: Robustness to Alternative Control Groups

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Benefit | Formal Emp | Log Earnings | Log Hours |
| Below EP vs Between EP and PL | 314.996*** | 0.143*** | 0.050*** | 0.001*** |
| | ( 0.292) | ( 0.001) | ( 0.001) | ( 0.000) |
| Below EP vs Above EP | 304.606*** | 0.145*** | 0.078*** | 0.002*** |
| | ( 0.268) | ( 0.001) | ( 0.000) | ( 0.000) |
| Below EP vs Above PL | 301.219*** | 0.145*** | 0.088*** | 0.003*** |
| | ( 0.274) | ( 0.001) | ( 0.000) | ( 0.000) |
| Individual FE | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ |

*Note:* This table reports robustness checks examining alternative definitions of the comparison group. Each row corresponds to a different control group relative to households below the extreme poverty (EP) line. Columns display estimates for each outcome. Standard errors, clustered at the 2012 family level, are shown in parentheses.

Table 8: Heterogeneity in Household Position

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Head - Formal Emp | 0.101*** | 0.099*** | 0.137*** | 0.137*** |
| | ( 0.001) | ( 0.008) | ( 0.009) | ( 0.009) |
| Head - Any Emp | 0.030*** | 0.034*** | 0.046*** | 0.046*** |
| | ( 0.001) | ( 0.004) | ( 0.004) | ( 0.004) |
| Spouse - Formal Emp | 0.176*** | 0.173*** | 0.075*** | 0.075*** |
| | ( 0.001) | ( 0.007) | ( 0.008) | ( 0.008) |
| Spouse - Any Emp | 0.065*** | 0.067*** | 0.030*** | 0.030*** |
| | ( 0.001) | ( 0.003) | ( 0.004) | ( 0.004) |
| Individual FE | ✓ | × | ✓ | × |
| Family FE | × | ✓ | × | ✓ |
| Year FE | ✓ | ✓ | × | × |
| Year x Muni FE | × | × | ✓ | ✓ |

*Note:* This table reports difference-in-differences estimates for heterogeneity in household position, across alternative fixed-effects specifications. Rows report treatment effects on formal and any employment for heads of household and spouses in the main sample. All effects are expressed as a share of the control-group mean. Columns vary the set of fixed effects included: individual or family, and year or year-by-municipality, as indicated at the bottom of the table. Standard errors, reported in parentheses, are clustered at the family level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9: Marginal Value of Public Funds

| | | |
|---|---|---|
| **MVPF** | $\infty$ | $\frac{b}{c}$ |
| **MVPF**$_{\text{Labor}}$ | 3.906 | $\frac{w+x}{c_L}$ |
| **MVPF**$_{\text{Health}}$ | $\infty$ | $\frac{w+z}{c}$ |
| **Willingness to pay** | 890 | $b = w + x + z$ |
| *Transfer* | 415 | $w$ |
| *Labor* | 424 | $x$ |
| *Earnings gain* | 465 | $x_1\, x_2\, n_i\, n_y\, 10^{-6}$ |
| Effect on Earnings | 0.050 | $x_1$ |
| Baseline earnings | 2,085 | $x_2$ |
| *Adjustment* | − 41 | $-P\, \frac{\varepsilon}{1+\varepsilon}\, \Delta y_{\mathcal{P}}\, n_i\, n_y\, 10^{-6}$ |
| $(frac.\ switching)$ | 0.115 | |
| $\varepsilon$ (labor supply elasticity) | 0.5 | |
| $\Delta y_{\mathcal{P}}$ (earnings change, switchers) | 240 | |
| Avg. earnings informal (ctrl) | 5,460 | |
| Avg. earnings formal (ctrl) | 10,319 | |
| *Health* | 50 | $z = z_1 + z_3$ |
| *Death* | 50 | $z_3 = z_{31}\, z_{32}\, n_i\, n_y\, 10^{-6}$ |
| Effect on death (–) | 0.00022 | $z_{31}$ |
| Lives saved | 999 | $z_{31}\, n_i\, n_y$ |
| Value of a statistical life | 50,000 | $z_{32}$ |
| **Net Fiscal Cost** | -265 | $c = w + y + s - t$ |
| *Mechanical* | 415 | $w = (w_1/w_2)\, n_i\, n_y\, 10^{-6}$ |
| Special transfer per family | 400 | $w_1$ |
| Individuals per family | 4.3 | $w_2$ |
| *Behavioral* | -680 | $p = y + s - t$ |
| *Reliance on PBF* | -61 | $y = y_1\, y_2\, n_i\, n_y\, 10^{-6}$ |
| Effect on BF graduation | -0.19 | $y_1$ |
| Basic benefit | 70 | $y_2$ |
| *Hospitalization* | -443 | $z_1 = z_{11}\, z_{12}\, n_i\, n_y\, 10^{-6}$ |
| Effect on probability of hospitalization | -0.0835 | $z_{11}$ |
| Cost of hospitalization (conditional) | 1,190 | $z_{12}$ |
| *Cost of hospital stay* | -36 | $s = s_1 s_2 s_3 n_i n_y 10^{-6}$ |
| Effect on cost of stay | -0.147 | $s_1$ |
| Cost of hospitalization (conditional) | 1,190 | $s_2$ |
| Probability of hospitalization | 0.047 | $s_3$ |
| *Payroll tax collection* | 140 | $t = t_1\, t_2\, t_3\, n_i\, n_y\, 10^{-6}$ |
| Effect on Earnings | 0.050 | $t_1$ |
| Baseline earnings | 2,085 | $t_2$ |
| Payroll tax rate | 0.30 | $t_3$ |
| **Scaling** | | |
| N individuals (eligible) | 496,049 | $n_i$ |
| N years | 9 | $n_y$ |

*Note:* This table summarizes the components used to compute the marginal value of public funds (MVPF) for the 2012 *Bolsa Família* expansion. The top panel reports the headline MVPF values for the labor-only and full calculations. The subsequent panels detail the underlying estimates, mechanical and behavioral fiscal effects, and scaling parameters. Monetary quantities are expressed in millions of current reais unless otherwise indicated, and the formulas in the right column show how each component enters the MVPF calculation.

# A Summary of Elasticity Estimates

Table 10: Elasticity Estimates of Earnings with Respect to Transfers

| Study Citation | Country | Elasticity | GDP per Capita (USD) |
|---|---|---|---|
| Balboni *et al.* (2021) | Bangladesh | 0.1630 | 2,551.02 |
| Bandiera *et al.* (2017) | Bangladesh | 0.0771 | 2,551.02 |
| Banerjee *et al.* (2021) | India (West Bengal) | 1.8500 | 2,480.79 |
| Banerjee *et al.* (2015) | Ethiopia | 0.9800 | 1,272.02 |
| | Ghana | 0.7700 | 2,260.29 |
| | Honduras | 1.4700 | 3,231.66 |
| | India | 4.8900 | 2,480.79 |
| | Pakistan | 0.3400 | 1,365.28 |
| | Peru | 1.0600 | 7,906.59 |
| Blattman *et al.* (2013) | Uganda | 0.2700 | 1,002.31 |
| Seim (2017) | Sweden | –0.1700 | 55,516.84 |
| Egger *et al.* (2022) | Kenya | 0.0730 | 1,952.30 |
| Gertler *et al.* (2012) | Mexico | 0.0750 | 13,790.02 |
| Golosov *et al.* (2023) | United States | –0.2615 | 82,769.41 |
| Guarin *et al.* (2023) | Colombia | 0.0060 | 6,947.36 |
| Imbens *et al.* (2001) | United States | –0.138 | 82,769.41 |
| Balakrishnan *et al.* (2024) | United States | –0.7400 | 82,769.41 |
| | United States | –0.3600 | 82,769.41 |
| Vivalt *et al.* (2024) | United States | –0.1500 | 82,769.41 |
| | United States | –0.3000 | 82,769.41 |

Table 11: Elasticity Estimates of Labor Supply with Respect to Transfers

| Study Citation | Country | Elasticity | GDP per Capita (USD) |
|---|---|---|---|
| Balboni *et al.* (2021) | Bangladesh | 0.5960 | 2,551.02 |
| Bandiera *et al.* (2017) | Bangladesh | 0.7960 | 2,551.02 |
| Banerjee *et al.* (2021) | India (West Bengal) | 0.0475 | 2,480.79 |
| Banerjee *et al.* (2017) | Honduras | –0.6400 | 3,231.66 |
| | Morocco | –0.4600 | 3,771.45 |
| | Philippines | 0.1480 | 3,804.87 |
| | Mexico (PAL) | 0.4620 | 13,790.02 |
| | Nicaragua | –0.2400 | 2,612.87 |
| | Mexico (Progresa) | –0.0951 | 13,790.02 |
| Banerjee *et al.* (2015) | Ethiopia | 0.9600 | 1,272.02 |
| | Ghana | 0.1000 | 2,260.29 |
| | Honduras | 0.1500 | 3,231.66 |
| | India | 1.7900 | 2,480.79 |
| | Pakistan | 0.0500 | 1,365.28 |
| | Peru | 0.1000 | 7,906.59 |
| Blattman *et al.* (2013) | Uganda | 0.6550 | 1,002.31 |
| Seim (2017) | Sweden | –0.1000 | 55,516.84 |
| Egger *et al.* (2022) | Kenya | 0.0390 | 1,952.30 |
| Gertler *et al.* (2012) | Mexico | | 13,790.02 |
| Golosov *et al.* (2023) | United States | 0.0000 | 82,769.41 |
| Guarin *et al.* (2023) | Colombia | –0.0300 | 6,947.36 |
| Imbens *et al.* (2001) | United States | | 82,769.41 |
| Balakrishnan *et al.* (2024) | United States | –0.4700 | 82,769.41 |
| | United States | 0.000029 | 82,769.41 |
| Vivalt *et al.* (2024) | United States | –0.1300 | 82,769.41 |
| | United States | –0.1500 | 82,769.41 |

## A.1 Computation of Elasticities Study-by-Study

### 1. "Why Do People Stay Poor?" (Balboni *et al.* (2021))

We treat the one-off livestock grant, median value 9000 BDT (p. 789), as the transfer. Earnings growth for households initially above the poverty line is 1462 BDT over eleven years, i.e. 133 BDT yr (Table IV). Baseline annual earnings for the ultra-poor are 5559 BDT (Table I). We compute the percentage gain in earnings and divide by the percentage increase in assets, yielding an earnings elasticity of $0.163$. The annual increase in labor hours is 87 h (Table IV); dividing the 8.8 % rise in hours by the 14.7 % rise in (annualized) transfer gives a labor-supply elasticity of $0.596$.

### 2. "Labor Markets and Poverty in Village Economies" (Bandiera *et al.* (2017))

The program cost per household is 1121 BDT (Table IX). Ultra-poor women's earnings rise by 21 % after four years (Table 3, panel b). Using baseline annual earnings of 411 BDT (same table) the transfer equals 2.73 $\times$ baseline income; the earnings elasticity is therefore $0.21/2.73 = 0.077$. Total hours worked rise 17 % (panel a, col. 7), giving a labor-supply elasticity of $0.17/2.73 = 0.062$.

### 3. "Long-Term Effects of the Targeting the Ultra Poor Program" (Banerjee *et al.* (2021))

We use the direct-transfer component of \$739 PPP per household (Online App. §C). Monthly income ITTs and control means come from Figure 1, panel C. For each survey wave we convert the monthly ITT to an annual flow and divide by \$739. The earnings elasticities are 0.422 (18 mo), 0.682 (3 yr), 3.329 (7 yr) and 2.972 (10 yr). Productive-time ITTs (Table 1, col. 8) are expressed in minutes day; converting to annual hours and dividing by \$739 yields labor-supply elasticities between 0.028 and 0.081.

### 4. "Debunking the Stereotype of the Lazy Welfare Recipient" (Banerjee *et al.* (2017))

Transfer-to-consumption ratios by program are taken from Figure 2; ITTs and control means for hours worked come from Table 3. For each country we compute

$$\varepsilon = \frac{\Delta H / H_0}{T/C},$$

where $T/C$ is the transfer share of consumption. The resulting hour-elasticities (Honduras, Morocco, Philippines, Mexico–PAL, Nicaragua, Mexico–Progresa) range from $-0.64$ to $+0.46$. No comparable earnings measures are reported.

**5. "A Multifaceted Program Causes Lasting Progress for the Very Poor" (Banerjee *et al.* (2015))**

Direct costs delivered to beneficiaries (Online App. Table S7) serve as $T$. Monthly ITTs on the four income components at Endline 2 (Online App. Table S5f-2) are summed and annualized for $\Delta E$. Minutes of productive activity at Endline 1 (Online App. Table S5e-1) give $\Delta H$. Dividing each $\Delta E$ or annualized $\Delta H$ by its country-specific $T$ produces the earnings and labor elasticities reported in the text.

**6. "Generating Skilled Self-Employment in Developing Countries" (Blattman *et al.* (2013))**

The one-off grant is \$382 (p. 699). Monthly cash-earnings ITTs (Table III) are annualized; dividing by \$382 gives earnings elasticities of 0.24 (two-year) and 0.30 (four-year). Weekly-hours ITTs are converted to annual hours and divided by \$382, yielding labor elasticities of 0.56 and 0.75.

**7. "The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries" (Seim (2017))**

Table 3 reports $-1.066$ SEK in earnings per 100 SEK of prize wealth, hence $\varepsilon = -0.0107$. Table 4 gives $-1.282$ hours per million-SEK prize; dividing yields $-1.28 \times 10^{-6}$ SEK. The life-cycle adjustment in Section VII implies a lifetime earnings elasticity of $-0.17$.

**8. "General Equilibrium Effects of Cash Transfers" (Egger *et al.* (2022))**

Program description gives the mean lump-sum transfer of 87000 KES = 1871 USD PPP. The total-effect ITT on annual household income is 135.7 USD (Table I, panel C); dividing gives an earnings elasticity of 0.073. Weekly-hours ITT of 1.41 h (Table II) is annualized and divided by the transfer to yield a labor elasticity of 0.039.

**9. "Investing Cash Transfers to Raise Long-Term Living Standards" (Gertler *et al.* (2012))**

Monthly agricultural-income ITT of 2.358 MXP (Table 4) represents a 9.58 % gain over the 24.624 MXP control mean. The average transfer per adult-equivalent is 31.44 MXP month (Table 10 note). Elasticity is $0.0958/1.277 = 0.075$.

**10. "How Americans Respond to Idiosyncratic and Exogenous Wealth Shocks" (Golosov *et al.* (2023))**

The marginal propensity to earn (flow basis) is $-0.50$ (Abstract). Because both numerator and denominator are dollars per year, this is directly the earnings elasticity. For lump-sum wealth, Table 3 shows $-\$2.30$ earnings per \$100 prize, i.e. $\varepsilon = -0.023$. Employment effects in Table 4 convert analogously to $-3.7 \times 10^{-7}$ per dollar, which is essentially $0$.

**11. "Reparations as Development? Evidence from the Colombian Armed Conflict" (Guarin *et al.* (2023))**

Payments average 29.575 monthly minimum-wages, $\approx$3.3 $\times$ annual pre-transfer income (Table I). Three years post-payment, earnings rise 2.05 % and formal employment falls 1.6 p.p. (=9.8 %). Dividing by 3.3 gives earnings and participation elasticities of $+0.006$ and $-0.030$, respectively (Figures III–IV).

**12. "Estimating the Effect of Unearned Income on Labour Earnings: Evidence from U.S. Lotteries" (Imbens *et al.* (2001))**

Specification VIII of Table 4 reports an MPE of $-0.124$. Adjusting for the finite 20-year annuity horizon ($\hat{\lambda} = 0.90$) gives $-0.124/0.90 = -0.138$, the earnings elasticity used.

**13. "Household Responses to Guaranteed Income: Evidence from Compton, California" (Balakrishnan *et al.* (2024))**

The ITT transfer is \$5400/year (p. 3 fn 2). Monthly ITTs on household income and respondent earnings (pp. 3–5) are annualized and divided by \$5 400, giving earnings elasticities of $-0.74$ and $-0.36$. Weekly-hours and participation ITTs (Table 4) are treated analogously, yielding labor elasticities of $+0.00029$ /hour and $-0.47$.

**14. "The Employment Effects of a Guaranteed Income: Experimental Evidence from Two U.S. States" (Vivalt *et al.* (2024))**

Monthly transfer net of claw-back is \$950 (\$1000 stipend minus \$50 forgone welfare; Section 2). Annualizing (\$11400) and dividing the ITT changes reported in Table 3 produces earnings elasticities ranging from $-0.15$ to $-0.30$. Weekly-hours and participation ITTs in Table 4 convert to labor elasticities of $-0.006$/hour and $-3.4 \times 10^{-6}$ (probability points per dollar).

# B Proofs

## B.1 Proof of Proposition 1

*Proof.* We solve the model backwards by first characterizing the optimal intensive margin choice if the household chooses to work and then comparing utility between working the optimal amount and not working. If the household works, they set the marginal utility of consumption equal to the marginal disutility of supplying work effort:

$$c'\left(\tilde{h}^*\right) = \nu'\left(\tilde{h}^*\right) \leftrightarrow A = h^{1/\varepsilon} \leftrightarrow \tilde{h}^* = A^{\varepsilon}$$

Turning to the extensive margin, households participate if their utility is higher from participating and earning $\tilde{y}^* = y\left(\tilde{h}^*\right) = A\tilde{h}^* = A^{1+\varepsilon}$ than from not participating. That is,

$$\begin{aligned} U\left(c\left(\tilde{h}^*\right), \tilde{h}^*\right) &> U\left(c\left(0\right), 0\right) \\ Ah^* + b_0 - q - \nu\left(h^*\right) &> b_0 \\ A &> \left[q\left(1+\varepsilon\right)\right]^{\frac{1}{1+\varepsilon}} = \underline{\tilde{A}}\left(q, b_0\right) \end{aligned}$$

Finally, we can characterize who are the working poor as those for whom working does not satisfy basic needs:

$$c\left(\tilde{h}^*\right) - q \leq \bar{c} \leftrightarrow A\tilde{h}^* + b_0 - q \leq \bar{c} \leftrightarrow A \leq \bar{\tilde{A}}\left(q, b_0\right) = \left(\bar{y} - b_0\right)^{\frac{1}{1+\varepsilon}}$$

where $\bar{y} = \bar{c} + q$. □

## B.2 Proof of Proposition 2

*Proof.* As before, we proceed by studying the intensive margin conditional on participation and then work backwards to study the participation decision.

### B.2.1 Intensive Margin

**Working Poor** The households optimize by setting the marginal output equal to the marginal disutility of effort:

$$\begin{aligned}
\nu'(h^*) &= y'(h^*) \\
h^{\frac{1}{\varepsilon}} &= A^{\gamma}\left[\gamma(\bar{y}-b)\right]^{-(\gamma-1)} h^{\gamma-1} \\
h^* &= \left(A^{\gamma}\left[\gamma(\bar{y}-b_0)\right]^{-(\gamma-1)}\right)^{\frac{1}{\frac{1}{\varepsilon}-(\gamma-1)}} && (12) \\
y^* = y(h^*) &= A^{\frac{\gamma(1+\varepsilon)}{1-\varepsilon(\gamma-1)}} \gamma^{\frac{-\gamma}{1-\varepsilon(\gamma-1)}} (\bar{y}-b_0)^{\frac{(1-\gamma)(1+\varepsilon)}{1-\varepsilon(\gamma-1)}} \\
&= A^{\frac{\gamma(1+\varepsilon)}{1-\varepsilon(\gamma-1)}} \left[\gamma(\bar{y}-b_0)\right]^{-\frac{(\gamma-1)(1+\varepsilon)}{1-\varepsilon(\gamma-1)}} \gamma^{-1} && (13) \\
U(c(h^*), h^*) &= y^* + b_0 - q - \nu(h^*) \\
&= \frac{1-\varepsilon(\gamma-1)}{\gamma(1+\varepsilon)} \left(A^{\gamma}\left[\gamma(\bar{y}-b_0)\right]^{-(\gamma-1)}\right)^{\frac{1+\varepsilon}{1-\varepsilon(\gamma-1)}} + b_0 - q && (14)
\end{aligned}$$

**Unconstrained** For those who more-than satisfy their basic needs, equating marginal disutility of effort and marginal output implies

$$\begin{aligned}
h^{\frac{1}{\varepsilon}} &= A \\
h^* &= A^{\varepsilon} && (15) \\
y^* = y(h^*) &= A^{1+\varepsilon} - (\gamma-1)(\bar{y}-b_0) && (16) \\
U(c(h^*), h^*) &= y^* + b_0 - q - \nu(h^*) \\
&= \frac{A^{1+\varepsilon}}{1+\varepsilon} - (\gamma-1)(\bar{y}-b_0) + b_0 - q && (17)
\end{aligned}$$

**Distinguishing the two groups** The working poor are those for whom $y'\left(\bar{h}\right) \leq \nu'\left(\bar{h}\right)$, while the unconstrained are those for whom $y'\left(\bar{h}\right) > \nu'\left(\bar{h}\right)$. These unconstrained households have high enough productivity that even after meeting their basic needs, their marginal disutility of effort is below their marginal output. That is, their productivity $A$ is above a threshold $\bar{A}(q, b_0)$ given by:

$$\begin{aligned}
\nu'\left(\bar{h}\right) &< A \\
A &> \left(\gamma \frac{\bar{y}-b_0}{A}\right)^{\frac{1}{\varepsilon}} \\
A^{1+\frac{1}{\varepsilon}} &> \left(\gamma(\bar{y}-b_0)\right)^{\frac{1}{\varepsilon}} \\
A &> \left[\gamma(\bar{y}-b_0)\right]^{\frac{1}{1+\varepsilon}} = \bar{A}(q, b_0) && (18)
\end{aligned}$$

We can see this visually in Figure 12. It shows three households with different

levels of their potential ability $A$. Household $A_1$ is unconstrained. Household $A_3$ is working poor, and household $A_2$ is the marginal working poor household with $A_2 = \bar{A}(q, b_0)$.

### B.2.2 Extensive Margin

Having characterized the households' intensive-margin choices, we can characterize their extensive margin choices by comparing their utility at their optimally chosen effort at the intensive margin conditional on working, to their utility from not working, $U(c(0), 0) = b_0$.

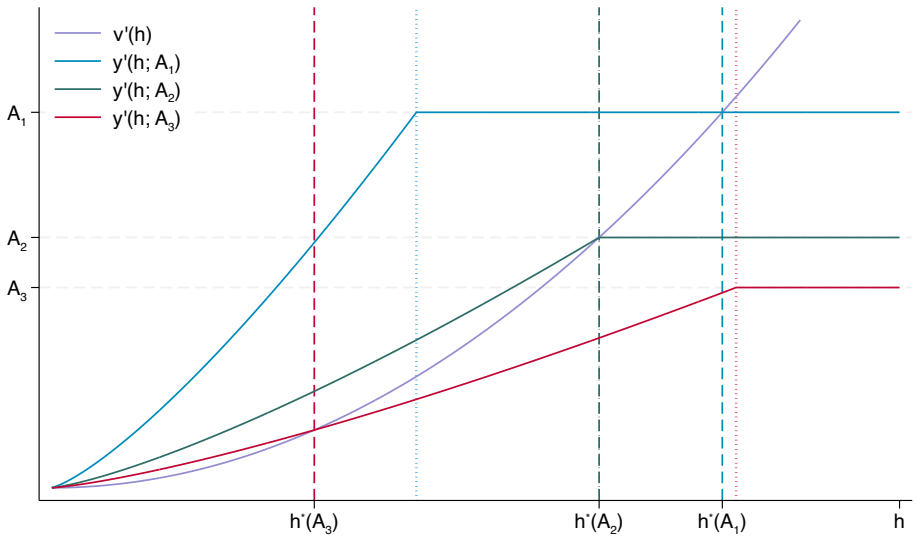
**Working Poor** Starting with the working poor, and using (14) for their utility if they work,

$$\begin{aligned}
U(c(h^*), h^*) &> U(c(0), 0) \\
q &< \left(A^\gamma \left[\gamma(\bar{y} - b_0)\right]^{-(\gamma-1)}\right)^{\frac{1+\varepsilon}{1-\varepsilon(\gamma-1)}} \frac{1-\varepsilon(\gamma-1)}{\gamma(1+\varepsilon)} \\
A^\gamma \left[\gamma(\bar{y} - b_0)\right]^{-(\gamma-1)} &> \left(q\frac{\gamma(1+\varepsilon)}{1-\varepsilon(\gamma-1)}\right)^{\frac{1-\varepsilon(\gamma-1)}{1+\varepsilon}} \\
A &> \left[\gamma(\bar{y} - b_0)\right]^{\frac{\gamma-1}{\gamma}} \left(q\frac{\gamma(1+\varepsilon)}{1-\varepsilon(\gamma-1)}\right)^{\frac{1-\varepsilon(\gamma-1)}{\gamma(1+\varepsilon)}} \\
&= \gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{\gamma-1}{\gamma}} \left(q\frac{1+\varepsilon}{1-\varepsilon(\gamma-1)}\right)^{\frac{1}{1+\varepsilon}-\frac{\gamma-1}{\gamma}} = \underline{A}^c(q, b_0) \quad (19)
\end{aligned}$$

**Unconstrained** Similarly for the unconstrained, using (17), those who work are those for whom

$$\begin{aligned}
U(c(h^*), h^*) &> U(c(0), 0) \\
q &< \frac{A^{1+\varepsilon}}{1+\varepsilon} - (\gamma-1)(\bar{y} - b_0) \\
A &> \left((1+\varepsilon)\left[(\gamma-1)(\bar{y} - b_0) + q\right]\right)^{\frac{1}{1+\varepsilon}} = \underline{A}^u(q, b_0) \quad (20)
\end{aligned}$$

Figure 12: Intensive-Margin Choices Under Productive Inclusion




**Combining the two groups** The participation constraint of the working poor is the relevant one whenever $\underline{A}^c(q, b_0) < \bar{A}(q, b_0)$. This happens when

$$\gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{\gamma-1}{\gamma}} \left[ q \frac{1+\varepsilon}{1-\varepsilon(\gamma-1)} \right]^{\frac{1}{1+\varepsilon} - \frac{\gamma-1}{\gamma}} < \gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{1}{1+\varepsilon}}$$

$$\left( \frac{q}{\bar{y} - b_0} \frac{1+\varepsilon}{1-\varepsilon(\gamma-1)} \right) < 1$$

$$q < \frac{\bar{c} - b_0}{\varepsilon\gamma} [1 - \varepsilon(\gamma - 1)] = \tilde{q}(b_0) \tag{21}$$

Combining these, we can write the combined participation constraint as

$$\underline{A}(q, b_0) = \begin{cases} \gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{\gamma-1}{\gamma}} \left( q \frac{1+\varepsilon}{1-\varepsilon(\gamma-1)} \right)^{\frac{1}{1+\varepsilon} - \frac{\gamma-1}{\gamma}} & \text{if } q \leq \tilde{q}(b_0) \\ \left( (1+\varepsilon) [(\gamma-1)(\bar{y} - b_0) + q] \right)^{\frac{1}{1+\varepsilon}} & \text{if } q > \tilde{q}(b_0) \end{cases} \tag{22}$$

□

## B.3 Proof of Proposition 3

*Proof.* As before, we solve the model backwards by first characterizing the optimal intensive margin choice if the household chooses to work and then comparing utility between working the optimal amount and not working. If the household works, continue exerting effort until their marginal utility of consumption

equals their marginal disutility of supplying work effort.

$$c'\left(\tilde{h}^*\right) = \nu'\left(\tilde{h}^*\right) \leftrightarrow A = h^{1/\varepsilon} \leftrightarrow \tilde{h}^* = A^{\varepsilon}$$

With a guaranteed income, the marginal consumption from working more is zero until the guaranteed income is exhausted and so no household whose optimal effort leads to earnings below the guaranteed income threshold chooses to work. Households with earnings $y < N\bar{b} - b_0$ are below the guaranteed income threshold. To reach the guaranteed income threshold requires work effort of $\hat{h} = \left(N\bar{b} - b_0\right)/A$. Households with $\tilde{h}^* \leq \hat{h}$ do not work, regardless of their participation cost $q$. Turning to the extensive margin, As we just argued, no households that work choose to earn below the guaranteed income threshold and so all workers receive the basic income not the guaranteed income. However, if they stopped working, they would receive the guaranteed income $N\bar{b}$, which is larger than the basic income $b_0$. They participate if their utility is higher from participating and earning $\tilde{y}^* = y\left(\tilde{h}^*\right) = A\tilde{h}^* = A^{1+\varepsilon}$ than from not participating. That is,

$$\begin{aligned} U\left(c\left(\tilde{h}^*\right), \tilde{h}^*\right) &> U\left(c\left(0\right), 0\right) \\ Ah^* + b_0 - q - \nu\left(h^*\right) &> N\bar{b} \\ A &> \left[\left(q + N\bar{b} - b_0\right)\left(1+\varepsilon\right)\right]^{\frac{1}{1+\varepsilon}} = \underline{\tilde{A}}\left(q, b_1\right) \end{aligned}$$

Finally, we can show that there are no longer any working poor, they all exit the labor force. As we argued above, all workers earn above the guaranteed income threshold and receive only the basic income. For a worker to be working poor, they must both have sufficient productivity to participate, $A > \underline{\tilde{A}}\left(q, b_1\right)$ and have low enough productivity not to earn enough to satisfy their basic needs. The latter condition requires $\tilde{y}^*\left(A, q, b_0\right) + b_0 - q < \bar{c}$, which requires $A < \left(q + \bar{c} - b_0\right)^{\frac{1}{1+\varepsilon}} = \bar{\tilde{A}}\left(q, b_0\right)$. However, because by assumption the guaranteed income covers basic consumption needs $N\bar{b} > \bar{c}$, $\bar{\tilde{A}}\left(q, b_0\right) < \underline{\tilde{A}}\left(q, b_1\right)$ and so there are no working poor. □

## B.4 Proof of Proposition 4

*Proof.* As discussed in section 3, the reform moves households to the benefit schedule

$$b_1 = \max\{N\bar{b} - y, b_0\} \tag{23}$$

For many people, the guaranteed income will mean that they can cover their basic needs with just the guaranteed income: $N\bar{b} \geq \bar{y}$. They are those with suffi-

ciently small participation costs:

$$\bar{y} = \bar{c} + q \leq N\bar{b} \leftrightarrow q \leq N\bar{b} - \bar{c} = \bar{q}(b_1)$$

Since these households don't require any work effort to meet their basic needs, they are fully productive with their first unit of effort: $\bar{h} = 0$.

Meanwhile, for those with $q > \bar{q}(b_1)$, meeting basic needs requires earnings net of benefits above $N\bar{b}$ which requires earnings above $N\bar{b}$ and so the amount of effort required to meet basic needs is unchanged: $\bar{h}(A, q; b_0) = \bar{h}(A, q; b_1)$.

### B.4.1 Intensive Margin

Here we characterize household decisions on the intensive margin and then work backwards to characterize extensive margin decisions.

**below $\bar{q}(b_1)$:** Households choose work effort by setting marginal output equal to the marginal disutility of work effort:

$$\begin{aligned}
\nu'(h^*) &= y'(h^*) \\
h^{1/\varepsilon} &= A \\
h^* &= A^{\varepsilon} && (24) \\
y^* = y(h^*) &= Ah^* = A^{1+\varepsilon} && (25) \\
U(c(h^*), h^*) &= \max\{y^* + b_0, N\bar{b}\} - q - \nu(h^*) \\
&= \begin{cases} N\bar{b} - q - \frac{1}{1+\frac{1}{\varepsilon}} A^{1+\varepsilon} & \text{if } A \leq \left(N\bar{b} - b_0\right)^{\frac{1}{1+\varepsilon}} \\ \frac{1}{1+\varepsilon} A^{1+\varepsilon} + b_0 - q & \text{if } A > \left(N\bar{b} - b_0\right)^{\frac{1}{1+\varepsilon}} \end{cases} && (26)
\end{aligned}$$

**above $\bar{q}(b_1)$:** Since these households still have the same $\bar{h}$, their intensive margin decisions are governed by the same set of conditions. The working poor are those with $A \leq \bar{A}(q, b_0)$ as given by (18) and they choose effort (12) to generate income (13) and have utility (14). The unconstrained are those with $A > \bar{A}(q, b_0)$ as given by (18) and they choose effort (15) to generate income (16) and have utility (17).

### B.4.2 Extensive Margin

**below $\bar{q}(b_1)$:** As before, households compare their utility from working and choosing effort optimally $U(c(h^*), h^*)$ to their utility from staying out of the labor force $U(c(0), 0) = N\bar{b}$. For households with low productivity ($A \leq \left(N\bar{b} - b_0\right)^{\frac{1}{1+\varepsilon}}$), $c(h^*) = N\bar{b}$ and so it never makes sense to work if their consumption won't increase.

For more productive households ($A > \left(N\bar{b} - b_0\right)^{\frac{1}{1+\varepsilon}}$), they work whenever

$$\begin{aligned} N\bar{b} &< \frac{1}{1+\varepsilon} A^{1+\varepsilon} + b_0 - q \\ A &> \left[(1+\varepsilon)\left(N\bar{b} - b_0 + q\right)\right]^{\frac{1}{1+\varepsilon}} = \underline{A}_1(q, b_1) \end{aligned} \tag{27}$$

**above** $\bar{q}(b_1)$**:** There are two groups to consider. First, the working poor with $A \leq \bar{A}(q, b_0)$, and second the unconstrained with $A > \bar{A}(q, b_0)$.

There are no working poor. To see this, note that for there to be working poor they would have to prefer working to not working and they would need to earn less than their basic needs. But they can meet their basic needs by not working. Just to double-check, we outline the condition where they work, and we obtain a contradiction. To work they would need:

$$\begin{aligned} N\bar{b} &< \frac{1-\varepsilon(\gamma-1)}{\gamma(1+\varepsilon)} \left(A^{\gamma}\left[\gamma(\bar{y} - b_0)\right]^{-(\gamma-1)}\right)^{\frac{1+\varepsilon}{1-\varepsilon(\gamma-1)}} + b_0 - q \\ A &> \gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{\gamma-1}{\gamma}} \left(\frac{1+\varepsilon}{1-\varepsilon(\gamma-q)}\left(N\bar{b} - b_0 + q\right)\right)^{\frac{1}{1+\varepsilon} - \frac{\gamma-1}{\gamma}} = \underline{A}^c(q, b_1) \end{aligned} \tag{28}$$

For there to be such households we need $\underline{A}^c(q, b_1) < \bar{A}(q, b_0)$ for them. This requires

$$\begin{aligned} \gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{\gamma-1}{\gamma}} \left(\frac{1+\varepsilon}{1-\varepsilon(\gamma-q)}\left(N\bar{b} - b_0 + q\right)\right)^{\frac{1}{1+\varepsilon} - \frac{\gamma-1}{\gamma}} &< \gamma^{\frac{1}{1+\varepsilon}} (\bar{y} - b_0)^{\frac{1}{1+\varepsilon}} \\ \frac{1+\varepsilon}{1-\varepsilon(\gamma-1)} &< \frac{\bar{c} - b_0 + q}{N\bar{b} - b_0 + q} \end{aligned}$$

but this is a contradiction since the LHS is bigger than 1 and the RHS is less than 1 whenever $N\bar{b} > \bar{c}$.

This means that all the households with $q > \bar{q}(b_1)$ who work are unconstrained. For them to work it must be the case that

$$\begin{aligned} U(c(h^*), h^*) &> U(c(0), 0) \\ N\bar{b} &< \frac{A^{1+\varepsilon}}{1+\varepsilon} - (\gamma-1)(\bar{y} - b_0) + b_0 - q \\ \frac{A^{1+\varepsilon}}{1+\varepsilon} &> \gamma(\bar{y} - b_0) + N\bar{b} - \bar{c} \\ A &> \left[(1+\varepsilon)\left(N\bar{b} - \bar{c} + \gamma(\bar{y} - b_0)\right)\right]^{\frac{1}{1+\varepsilon}} = \underline{A}_2(q, b_1) \end{aligned} \tag{29}$$

**Combining the two groups:** Putting this together, we have

$$\underline{A}\left(q, b_1\right) = \begin{cases} \left[(1+\varepsilon)\left(N\bar{b} - b_0 + q\right)\right]^{\frac{1}{1+\varepsilon}} & \text{if } q \leq \bar{q}\left(b_1\right) \\ \left[(1+\varepsilon)\left(N\bar{b} - \bar{c} + \gamma\left(\bar{y} - b_0\right)\right)\right]^{\frac{1}{1+\varepsilon}} & \text{if } q > \bar{q}\left(b_1\right) \end{cases} \tag{30}$$

□

## B.5 Bounding Willingness to Pay

There are 9 types of households defined by their pre- and post-reform labor supply decisions. For each of them, we can show that their willingness to pay lies between the change in their received benefit and the sum of their additional benefit and their change in earnings. Figure 13 reproduces figure 5 but with the nine groups labeled. We go through them individually below.

**Group I.** These households do not work before or after and have $q > \bar{q}\left(b_1\right)$. For them, earnings are 0 before and after the reform, and benefits increase from $b_0$ to $N\bar{b}$. Utility before the reform is $b_0$ and after the reform is $N\bar{b}$. As a result, their willingness to pay is

$$\Delta U^I = \Delta b \quad (= \Delta b + \Delta y)$$

**Group II.** These households do not work before or after the reform and have $q \leq \bar{q}\left(b_1\right)$. By the same logic as group I, their willingness to pay is

$$\Delta U^{II} = \Delta b \quad (= \Delta b + \Delta y)$$

**Group III.** These households are working poor before the reform and do not work after the reform. They have $\underline{A}\left(q, b_0\right) \leq A \leq \underline{A}\left(q, b_1\right)$ and $q \leq \bar{q}\left(b_1\right)$. Their welfare before the reform is $U_{pre}^{III} = \frac{1-\varepsilon(\gamma-1)}{(1+\varepsilon)} y_{pre}^{III} + b_0 - q$ and their welfare after the reform is $N\bar{b}$. Their willingness to pay is

$$\begin{aligned} \Delta U^{III} &= \Delta b + \frac{1-\varepsilon\left(\gamma-1\right)}{1+\varepsilon}\Delta y + q \\ &\in \left[\Delta b, \Delta b + \frac{1}{1+\varepsilon}\Delta y\right] \end{aligned}$$

To see the bounds note that since these households participated under the basic benefit, $-\frac{1-\varepsilon(\gamma-1)}{(1+\varepsilon)}\Delta y \geq -\frac{1-\varepsilon(\gamma-1)}{(1+\varepsilon)}\Delta y - q > 0$ and $\frac{1-\varepsilon(\gamma-1)}{1+\varepsilon} < \frac{1}{1+\varepsilon}$ since $\gamma > 1$.

**Group IV.** These households do not work before the reform and start to work after the reform. They have $q \leq \bar{q}\left(b_1\right)$ and $\underline{A}\left(q, b_1\right) \leq A \leq \underline{A}\left(q, b_0\right)$. Their welfare

before the reform is $b_0$. After the reform they work and continue to receive a benefit of $b_0$, enjoying welfare of $U^{IV}_{post} = \frac{1}{1+\varepsilon}A^{1+\varepsilon} + b_0 - q$. Their willingness to pay is

$$\begin{aligned}\Delta U^{IV} &= \frac{1}{1+\varepsilon}\Delta y - q \\ &\in \left[\Delta b, \Delta b + \frac{1}{1+\varepsilon}\Delta y\right)\end{aligned}$$

To see the bounds note that for the households to be willing to work after the reform it must be the case that $\frac{1}{1+\varepsilon}\Delta y - q > 0$.

**Group V.** These households work both before and after the reform. They are working poor before the reform but not after. They have $q < \bar{q}(b_1)$ and $\max\{\underline{A}(q, b_0), \underline{A}(q, b_1)\} \leq A \leq \bar{A}(q, b_0)$. They receive the basic benefit $b_0$ before and after the reform and so their willingness to pay is

$$\begin{aligned}\Delta U^{V} &= \Delta y - \Delta\nu(h^*) \\ &= \frac{1-\varepsilon(\gamma-1)}{1+\varepsilon}\Delta y + \frac{\varepsilon(\gamma-1)}{1+\varepsilon}y^{V}_{post} \\ &\in (\Delta b, \Delta b + \Delta y)\end{aligned}$$

To see the bound note that $0 < \Delta\nu < \Delta y$. It is positive since the reform increases households' marginal output $y'_{post}(h) > y'_{pre}(h)$ and the household's first order condition for effort choices then implies that $\nu'_{post}(h) > \nu'_{pre}(h)$ and hence that $\nu_{post}(h) > \nu_{pre}(h)$ since $\nu(\cdot)$ is strictly convex. It is below $\Delta y$ since the household must be at least as well off after the reform as before since their productivity has increased, expanding their choice set.

**Group VI.** These households are working poor before the reform and do not work after the reform. They have $\underline{A}(q, b_0) \leq A \leq \underline{A}(q, b_1)$ and $q > \bar{q}(b_1)$. Analogous reasoning to group III shows that

$$\begin{aligned}\Delta U^{VI} &= \Delta b + \frac{1-\varepsilon(\gamma-1)}{1+\varepsilon}\Delta y + q \\ &\in \left[\Delta b, \Delta b + \frac{1}{1+\varepsilon}\Delta y\right]\end{aligned}$$

**Group VII.** These households were unconstrained before the reform and do not work after the reform. They have $q > \bar{q}(b_1)$ and $\max\{\underline{A}(q, b_0), \bar{A}(q, b_0)\} \leq A \leq$

$\underline{A}(q, b_1)$. By analogous reasoning to gtoups III and VI, we have that

$$\begin{aligned}\Delta U^{VII} &= \Delta b + \frac{1 - \varepsilon(\gamma - 1)}{1 + \varepsilon}\Delta y + q \\ &\in \left[\Delta b, \Delta b + \frac{1}{1 + \varepsilon}\Delta y\right]\end{aligned}$$

**Group VIII.** These households are unconstrained both before and after the reform, but have diminished productivity at low levels of hours both before and after the reform: They have $q > \bar{q}(b_1)$ and $A > \underline{A}(q, b_1)$. They do not change their earnings decisions or benefit receipt and so for them

$$\Delta U^{VIII} = \Delta b = \Delta b + \Delta y \quad (= 0)$$

**Group IX.** These households are unconstrained both before and after the reform, but have their basic needs met after the reform and consequently increase earnings. They have $q \leq \bar{q}(b_1)$ and $A > \bar{A}(q, b_0)$. They do not change their effort choices but meeting their basic needs after the reform increases their earnings by $\Delta y = (\gamma - 1)(\bar{y} - b_0)$. Their willingness to pay is hence

$$\Delta U^{IX} = \Delta y \quad (= \Delta b + \Delta y)$$

**Combining all groups.** Combining these we can characterize the upper bound on willingness to pay. For groups that do not change their extensive margin decision, the upper bound is $\Delta b + \Delta y$. For groups that do change their extensive margin decision, the upper bound is $\Delta b + \frac{1}{1+\varepsilon}\Delta y$. Denoting the set of households who change their extensive margin decision (groups III, IV, VI, and VII above) as $\mathcal{P}$ and the fraction of households in this set as $P$, we have that total willingness to pay of the affected households is

$$WTP \in \left(\Delta b, \Delta b + \Delta y - P\tfrac{\varepsilon}{1+\varepsilon}\mathsf{E}\left[\Delta y_i | i \in \mathcal{P}\right]\right] \tag{31}$$

which is the bound we use in equation (11) in section 6.

**Implementing the upper bound.** We now describe how we compute each component of the upper bound on willingness to pay used in Table 9. We need three objects: the average increase in earnings $\Delta y$, the fraction of households that change their extensive margin decision $P$, and the average earnings change among those households $\Delta y_{\mathcal{P}}$.

**Average earnings increase** $(\Delta y)$**.** The average earnings increase is computed as the product of the estimated proportional effect on earnings and average base-

line earnings:

$$\Delta y = \hat{\beta}_{\text{earnings}} \times \bar{y},$$

where $\hat{\beta}_{\text{earnings}} = 0.050$ is the estimated treatment effect on log earnings from Table 5 (column 1, individual and year fixed effects) and $\bar{y} = 2{,}085$ BRL/year is the average annual earnings in the eligible population. This gives $\Delta y = 0.050 \times 2{,}085 = 104$ BRL per person per year.

**Fraction switching extensive margin** $(P)$**.** The set $\mathcal{P}$ consists of individuals who change their employment status (between employed and unemployed) due to the reform. We observe three baseline groups—unemployed, informally employed, and formally employed—and two employment outcomes (informal and formal employment). For each group, we measure the net probability of switching between employment and unemployment using the treatment effects on informal and formal employment from the interaction specification underlying Table 4 (a single regression interacting the post-treatment indicator with baseline employment status). For the baseline unemployed, the net flow into employment is the increase in informal employment minus any offsetting decrease in formal employment. For the baseline informally employed, the net flow out of employment is the absolute decrease in informal status minus the offsetting increase in formal employment. For the formally employed, the net flow out of employment is the absolute decrease in formal status minus the offsetting increase in informal employment. Weighting by population shares $\pi_g$ (computed from the subgroup sample sizes in Table 4):

$$\begin{aligned} P = {} & \underbrace{\frac{128{,}973{,}117}{214{,}375{,}863}}_{0.602} \times \underbrace{(0.138 - 0.036)}_{\text{unemp}\to\text{emp}} + \underbrace{\frac{48{,}184{,}929}{214{,}375{,}863}}_{0.225} \times \underbrace{(0.334 - 0.043)}_{\text{inf}\to\text{unemp}} \\ & + \underbrace{\frac{37{,}217{,}817}{214{,}375{,}863}}_{0.174} \times \underbrace{(0.013 - 0.078)}_{\text{net } \Delta\text{emp, formal}} \\ = {} & 0.061 + 0.065 + (-0.011) = 0.115. \end{aligned}$$

That is, approximately 11.5 percent of the population changes their extensive margin employment status as a result of the reform on net.

**Earnings change among switchers** $(\Delta y_{\mathcal{P}})$**.** For each baseline group, the earnings change associated with switching is determined by the type of transition and the earnings level in the destination (or origin) employment type. An individual who transitions from unemployment to informal employment gains $\bar{y}_{\text{inf}}$ per year; one who transitions from unemployment to formal employment gains $\bar{y}_{\text{form}}$; one

who leaves informal employment loses $\bar{y}_{\text{inf}}$; and so on. The average earnings change among switchers is therefore:

$$\Delta y_{\mathcal{P}} = \sum_{g \in \{U,I,F\}} \pi_g \left( \hat{\beta}^g_{\text{inf}} \times \bar{y}_{\text{inf}} + \hat{\beta}^g_{\text{form}} \times \bar{y}_{\text{form}} \right),$$

where $\bar{y}_{\text{inf}}$ and $\bar{y}_{\text{form}}$ are the average annual earnings of informally and formally employed individuals in the control group prior to the reform. Using the self-reported earnings from *Cadastro Único* for informal workers ($\bar{y}_{\text{inf}} = 5{,}460$ BRL/year) and formal wages from RAIS for formal workers ($\bar{y}_{\text{form}} = 10{,}319$ BRL/year), both conditional on positive earnings in the pre-reform control group and annualized from monthly values, we obtain:

$$\begin{aligned}
\Delta y_{\mathcal{P}} &= 0.602 \times (0.138 \times 5{,}460 + 0.036 \times 10{,}319) \\
&\quad + 0.225 \times (-0.334 \times 5{,}460 + 0.043 \times 10{,}319) \\
&\quad + 0.174 \times (0.013 \times 5{,}460 + (-0.078) \times 10{,}319) \\
&\approx 240 \text{ BRL/year}.
\end{aligned}$$

The positive value reflects the fact that the dominant transition is from unemployment into employment, which carries a positive earnings gain.

**Assembling the upper bound.** Combining these components and setting $\varepsilon = 0.5$, which is the midpoint of the range reviewed in the labor supply elasticity literature (Saez *et al.*, 2012), the adjustment to the WTP upper bound is:

$$P \frac{\varepsilon}{1+\varepsilon} \Delta y_{\mathcal{P}} = 0.115 \times \frac{0.5}{1.5} \times 240 \approx 9.2 \text{ BRL per person per year.}$$

Scaling to the full eligible population ($n_i = 496{,}049$ individuals over $n_y = 9$ post-reform years) and dividing by $10^6$ to express in millions of BRL:

$$\text{Adjustment} = 9.2 \times 496{,}049 \times 9 \times 10^{-6} \approx 41 \text{ million BRL.}$$

The upper bound on the earnings component of willingness to pay is therefore $\Delta y - P\frac{\varepsilon}{1+\varepsilon}\Delta y_{\mathcal{P}} = 465 - 41 = 424$ million BRL, where 465 million is the gross earnings gain ($\hat{\beta}_{\text{earnings}} \times \bar{y} \times n_i \times n_y \times 10^{-6}$). This is the value reported in Table 9.

Figure 13: Nine Types of Households

