Conditional Cash Transfers, Strategic Informality and Child Labor*

Pedro Guerra[1] and Jorge Herrera-Juárez[1]

[1] Department of Economics, University of Minnesota

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PRELIMINARY VERSION (DO NOT SHARE)

Abstract

Conditional cash transfers (CCTs) provide means-tested support subject to requirements such as school attendance. Focusing on Brazil's *Bolsa Família* program, we use household survey data to document some behavioral responses from informal workers around the eligibility threshold and a close relationship between child labor and school attendance. We incorporate these decisions into an overlapping-generations model calibrated to Brazil. We find that large eligibility expansions increase informality more than benefit expansions, while labor-tax financing amplifies both responses. Higher thresholds admit new recipients, including households that qualify by becoming informal, whereas larger benefits mainly affect existing recipients, some of whom reduce hours. These differences may help explain mixed empirical findings on the effects of CCTs on informality. We show that under consumption-tax financing, removing schooling requirements reduces both attendance and child labor, and that replacing the CCT with a budget-matched universal basic income raises long-run output but generates initial welfare losses that outweigh future gains.

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1 Introduction

Conditional cash transfers (CCTs) are means-tested transfers subject to nonpecuniary conditionalities, such as children's school attendance and health checkups. These programs seek to reduce current poverty and its transmission across generations. Their design, however, creates important trade-offs in economies with widespread informality and child labor. Income targeting may encourage adults to work informally to obtain or retain benefits. Schooling requirements encourage human capital investment and can raise future productivity, but also impose costs on families. Families may rely on child labor to generate current resources, although working during childhood may itself reduce future productivity. These choices affect both the returns of the policy and the tax revenue available to finance it.

This paper studies these trade-offs using Brazil's *Bolsa Família* Program (BFP). We address two main questions. First, how do expansions in benefits and eligibility affect informality, and how do these responses depend on the tax used to finance them? Second, how do informality and schooling requirements change the macroeconomic and welfare effects of CCTs?

To address these questions, we proceed in two steps. First, using household survey data, we document empirical evidence of "strategic informality" around the BFP eligibility threshold—specifically, a concentration of informal households just below the cutoff, a drop in the probability of informal employment above it, and a jump in hours worked among informal workers. Additionally, we show that household decisions regarding child labor and school attendance are deeply interdependent, emphasizing that the opportunity cost of child labor is a key determinant for the design of conditional cash transfers. Together, these empirical patterns demonstrate the necessity of jointly analyzing labor formality choices alongside human capital decisions. Second, to evaluate these behavioral responses and quantify their macroeconomic and welfare consequences, we develop and calibrate an overlapping-generations model to Brazilian data for 2016.

We build a simple model that clarifies the trade-offs behind the strategic informality response.

If the government fully observes the income of the formal workers but not the informal ones, a household that is ineligible at its formal earnings can lower observed income by becoming informal or reducing hours and qualify to receive the transfers. Informality entails a productivity penalty, while fewer hours reduce earnings but also labor disutility. In this context, the size of the benefits will be crucial: small benefits may not justify these adjustments; intermediate benefits can make informality more attractive; and sufficiently large transfers can make formal employment with fewer hours preferable to qualify. This possibility of different responses guides our quantitative analysis: alongside aggregate informality, we examine how households adjust employment sector and hours to obtain or retain benefits.

With our OLG model we explore different potential policy reforms financed via different tax sources. The quantitative results show that increasing the eligibility threshold can generate substantially more informality than increasing benefits. Under consumption-tax financing, doubling the threshold raises informality by 18.86 percentage points, compared with 3.20 points when benefits double, from a baseline rate of 32.67%. This difference reflects the number and type of households reached by each reform. A higher cutoff allows some households to qualify at their current income, while others qualify by changing their employment sector or hours. When we hold households' assets, human capital, and life-cycle stage at their baseline values, most new recipients under the doubled threshold qualify by moving from formal to informal employment. In contrast, larger benefits mainly increase payments to existing recipients, some of whom reduce hours and retain BFP.

Financing the same expansions through labor-income taxes produces larger informality responses because it also changes the relative return to formal work. The effect also varies with reform size: several benefit increases leave informality nearly unchanged, while some threshold increases reduce it relative to a smaller expansion. Thus, neither a larger benefit nor a higher cutoff implies a uniform response in informality across households or policy changes. This dependence on the policy instrument, reform size, and financing offers a possible explanation for the mixed empirical

evidence on the effects of BFP on formal employment (de Brauw et al., 2015; de Holanda Barbosa and Corseuil, 2013; Fruttero et al., 2020; Gerard et al., 2021).

We then evaluated alternative transfer policies, and found that reducing informality does not necessarily improve welfare. We compare transfer regimes in economies with and without informality, using consumption taxes to balance the government budget. With informality, eliminating the CCT or replacing it with a budget-matched universal basic income reduces informality and raises long-run output. However, both reforms impose initial welfare losses especially among low earners and BFP recipients. In our comparison across generations, these losses outweigh future welfare gains. The incentive to work informally therefore needs to be considered alongside the protection that income targeting provides to low-income households.

Removing the schooling condition of the CCT while retaining income targeting reduces both attendance and child labor. Household comparisons show that many families that initially were combining school and work, stopped both activities, retained BFP, and saved more. Avoiding schooling costs can help explain these choices, but the future productivity loss from child labor also depends on whether the child attends school. Schooling and work therefore need not move in opposite directions. Removing the conditionality generates small welfare gains for current households and long-run entrants with informality. In a formal-only economy simulation, current households gain, but long-run entrants lose slightly. Informality can therefore change the welfare evaluation of schooling requirements as well as income targeting.

2 Related Literature

We contribute to three main strands of literature: quantitative studies of cash transfer policies, research on children's human capital and child labor, and empirical work on BFP and adult labor-market outcomes.

First, our paper relates to quantitative comparisons of conditional and unconditional cash transfers.

Ferreira et al. (2025) compare CCTs and UBI in a life-cycle model calibrated to Brazil, emphasizing human capital accumulation and intergenerational linkages. Cespedes-Reynaga (2025) studies the effects of CCTs on schooling, child labor, and the intergenerational transmission of poverty. Related work on UBI highlights the importance of human capital, fiscal financing, and distributional effects (Daruich and Fernández, 2024; Luduvice, 2024). We add endogenous informality to the comparison between conditional, unconditional means-tested, and universal transfers. When eligibility depends on observed income, sector choices affect both access to benefits and tax payments. By comparing transfer regimes in economies with and without informality, we show that this margin can change the welfare evaluation of targeting and schooling requirements, both in the short and long run.

Second, our model builds on research linking parental resources and investments to children's outcomes (Caucutt and Lochner, 2020; Lee and Seshadri, 2019). Mullins (2025) shows that accounting for how cash assistance affects parental time and monetary investments in children can substantially change the costs and benefits of transfer policies. Structural and empirical studies also document the effects of CCTs on schooling decisions (Attanasio et al., 2012; Glewwe and Kassouf, 2012). However, increased attendance need not imply less child labor: Chitolina et al. (2016) find that a BFP expansion increased both attendance and the likelihood of combining school and work. In a finding closely related to our mechanism, Peruffo and Ferreira (2017) show that a schooling requirement can initially increase child labor when transfers do not fully cover schooling costs. We study these household resource trade-offs together with adult informality. Our framework therefore accounts for the schooling requirement's direct cost and its effects on future earnings, while allowing parents to adjust their sector of employment and the income observed by the government.

Finally, our paper relates to the empirical literature on BFP and adult labor-market outcomes. The evidence of the effects of BFP on formal and informal employment is mixed. de Brauw et al. (2015) find a reallocation of household labor hours from formal to informal work, while de Holanda Barbosa and Corseuil (2013) find no detectable effect on occupational choice between the two sectors.

In contrast, Fruttero et al. (2020) find increased entry into formal employment, and Gerard et al. (2021) document an increase in local formal employment following a program expansion. These studies examine different interventions: some compare recipients with nonrecipients, while others exploit changes in eligibility rules or program expansions across municipalities. Our empirical analysis provides evidence consistent with strategic informality near the eligibility threshold. The model offers a mechanism that may help explain the different findings by separating benefit increases from eligibility expansions and considering their size and financing. Our results show that the sectoral response to one type of reform need not carry over to another.

The remainder of the paper is organized as follows. Section 3 describes the institutional background of *Bolsa Família*, and Section 4 presents the empirical evidence on informality, schooling, and child labor. Section 5 introduces a simple model to explain the main mechanisms. Section 6 presents the quantitative model, and Section 7 discusses its calibration and fit. Section 8 studies BFP expansions and compares alternative transfer regimes, including their macroeconomic and welfare effects. The final section concludes.

3 Institutional Background: The *Bolsa Família* Program

The *Bolsa Família* Program (BFP) is Brazil's conditional cash transfer program and one of the largest CCTs in the world. The program was introduced in 2003[1] with the central objective to reduce poverty in the short run through direct income transfers while encouraging investments in children's human capital through education and health conditionalities. These include school attendance, vaccination, nutritional monitoring, and prenatal care.

Eligibility for *Bolsa Família* is based on monthly family income per capita. In its initial design, the program distinguished between families in extreme poverty and families in poverty. In 2004, the extreme poverty line was set at R$50 (US$17.09) per capita per month, while the poverty line was

[1] BFP started in 2003, and was formally created by Law No. 10,836 in January 2004, consolidating several existing federal transfer programs, including *Bolsa Escola, Bolsa Alimentação, Cartão Alimentação* and *Auxílio-Gás*.

set at R$100 (US$34.17) per capita per month.[2] Just the families below the extreme poverty line were eligible for a monthly basic benefit of R$50 per family. In addition, families in both poverty or extreme poverty could receive variable benefits depending on household composition, including R$15 (US$5.13) per child aged 15 or younger. Thus, the original structure combined a basic transfer targeted to the poorest households with additional benefits tied to family composition. Over time, both the income thresholds and benefit amounts increased, and two additional benefits were introduced: one in 2007 for adolescents aged 16 or 17 and another in 2012 to bring recipient families' income, including the other BFP benefits, to at least the extreme-poverty line. Table 1 summarizes these changes and the differences in benefits by household composition. Appendix A provides further details and illustrates the resulting transfer schedules.

Table 1: Evolution of *Bolsa Família* eligibility thresholds and benefits

	Thresholds (R$)		**Bolsa Família Benefits (R$)**			
Year	**Extreme Poverty Line**	**Poverty Line**	**Basic per Family***	**Variable Child / Preg. / Nursing**	**Variable Adolescent**	**Variable to Overcome EP****
2004	50	100	50	15	–	–
2006	60	120	50	15	–	–
2007	60	120	58	18	30	–
2008	60	120	62	20	30	–
2009	70	140	68	22	33	–
2011	70	140	70	32	38	–
2012	70	140	70	32	38	Variable
2014	77	154	77	35	42	Variable
2016	85	170	85	39	46	Variable
2018	89	178	89	41	48	Variable
2019	89	178	89	41	48	Variable

Notes: * Given to families with per capita income below the extreme poverty line. ** Difference to guarantee family per capita income equals the extreme poverty line.
Source: Brazilian Law Decree N° 5.209 and subsequent amendments.

This benefit structure is directly related to the mechanisms studied in this paper. On one hand, the transfer relaxes household resource constraints and makes compliance with the conditionalities more attractive, increasing incentives to invest in children's human capital and potentially discour-

[2]US dollar equivalents use the annual average exchange rate for the corresponding year, from the U.S. Federal Reserve (FRED series AEXBZUS). The Brazilian real depreciated substantially against the dollar during the 2010s, so comparisons of dollar amounts across years also reflect exchange-rate movements and do not directly measure changes in domestic purchasing power.

aging child labor. However, its effect on child labor is not necessarily negative, because families may use children's earnings to finance schooling costs (Peruffo and Ferreira, 2017). On the other hand, because eligibility depends on observed income, the program may distort labor supply and sectoral choices. In particular, households close to the eligibility threshold may have incentives to reduce observed income in order to qualify for benefits. In an economy with a large informal sector, this can occur not only through fewer hours worked, but also through strategic informality, since informal work allows households to hide part of their income from the government.

4 Empirical Evidence

In this section, we use microdata from the Brazilian Institute of Geography and Statistics (*IBGE*) alongside official government decrees to examine the interactions among the *Bolsa Família* Program (BFP), labor informality, child labor, and household labor supply. Specifically, we draw on the Monthly Employment Survey (PME), the Continuous National Household Sample Survey (PNADC),[3] and regulatory eligibility thresholds to test for behavioral responses near the program cutoff. Furthermore, we analyze how cash transfer receipts shape schooling and child labor decisions across the household income distribution.

4.1 Strategic Informality Behavior

First, we examine whether households strategically adjust their labor formality status to qualify for *Bolsa Família*. Because formal earnings are fully observable to the government through tax and social security records, workers can exploit the unobservability of informal income to underreport earnings and claim benefits. Theoretically, this incentive generates two testable empirical predictions near the eligibility threshold: (i) local bunching of informal workers just below the cutoff, and (ii) a sharp drop in the probability of informal employment immediately above it. Using mi-

[3]The PME covers 1980 through February 2016, collecting household-level employment, educational, and demographic data. It was succeeded by the PNADC, which provides comparable data on a quarterly basis.

crodata from the PME (January 2004–February 2016)[4] and the PNADC (March 2016–2019), we empirically evaluate these predictions. Additionally, we study a potential response in the intensive margin by studying how hours worked change around the threshold.

We compute the per capita household income and determine the formality status of employed individuals according to the following criteria: Households are classified as "formal" if at least one member possesses a signed work card (*carteira de trabalho*), or contributes to the official social security system, making their income easily verifiable via administrative registers; or as "informal" if no working-age member holds a formal job. Because the official *Bolsa Família* poverty line ($\bar{y}_t$) was adjusted multiple times throughout our period of analysis (see Table 1), we standardize the self-reported per capita household income in every period ($y_{i,t}$) to allow for comparability across time. We calculate the distance to the active poverty threshold for each family as $D_i = y_{i,t} - \bar{y}_t$, where $\bar{y}_t$ is the active threshold at time t. This standardization centers the program eligibility cutoff exactly at zero across all policy regimes. Finally, following the bunching estimation approach of Saez (2010), we construct empirical densities of this normalized income. We divide the running variable D_i into discrete, non-overlapping bins of a width of R$20.[5] For each bin, we aggregate the survey expansion weights to estimate the true population-level density of households at that income level. By plotting these densities separately for formal and informal households, we can isolate the strategic informality bunching behavior.

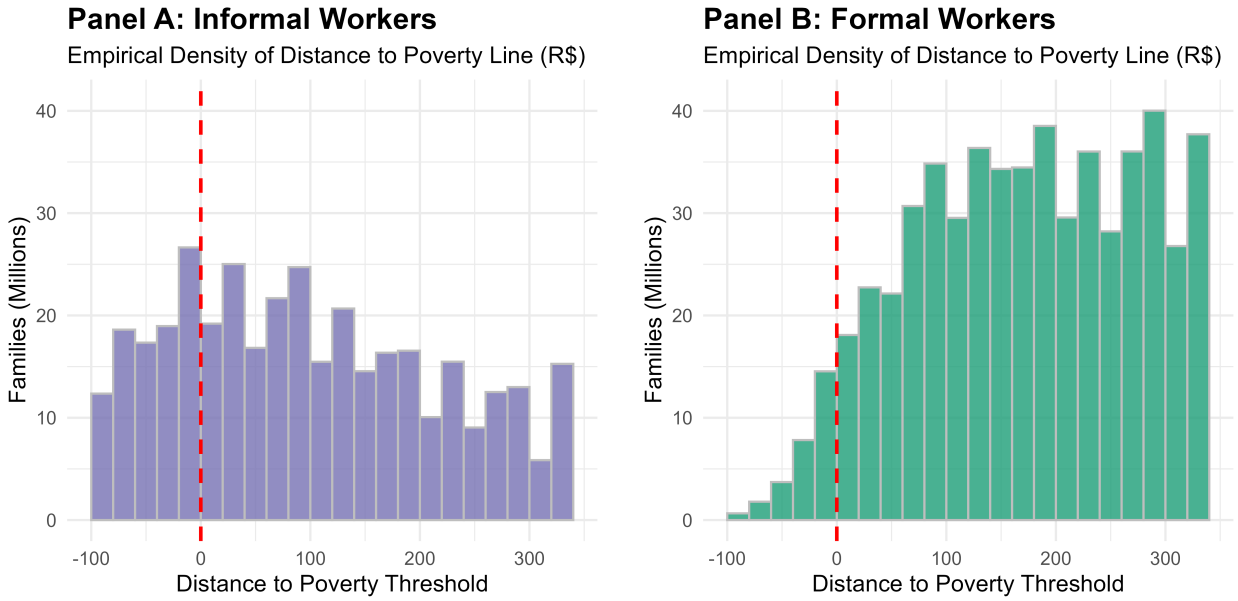
Figure 1 presents the empirical densities of the distance to the active *Bolsa Família* poverty line. A striking contrast emerges between the two panels. In Panel A (Informal Workers), there is a visible accumulation of mass —or bunching— to the left of the zero threshold across the different regimes. This could signal that a disproportionate number of informal households report incomes just low enough to qualify for the program. Conversely, Panel B (Formal Workers) reveals a smooth, continuous distribution around the cutoff, suggesting that the eligibility threshold does

[4]The *Bolsa Família* Program was implemented in 2003.

[5]As a robustness check, we also build the panels for bins of a width of R$10 and R$25. They are available in Appendix B.

not distort the density of formal households.

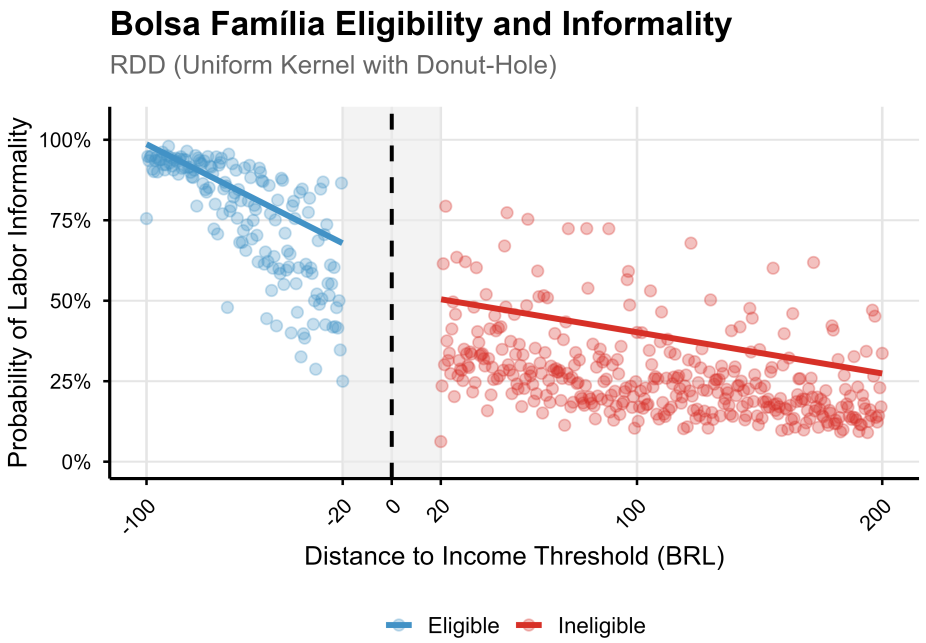
Figure 1: Strategic Informality Around the *Bolsa Família* Qualifying Threshold



Two institutional features of our data strengthen the interpretation of these findings as strategic informality. First, because these surveys rely on self-reported income, respondents are prone to rounding errors or reporting specific salient values (e.g., multiples of the minimum wage). However, if this bunching were driven entirely by survey-reporting frictions or "heaping," we would expect to observe similar patterns in both panels. The fact that the spike is isolated to informal households confirms an asymmetric reporting response. Second, it is critical to note that the PME and PNADC are independent household surveys conducted by IBGE; they are not the official administrative registry (Cadastro Único) used by the government to disburse benefits, meaning that underreporting income to an IBGE interviewer yields no direct financial gain. The presence of bunching even within an anonymous, non-consequential survey suggests that households either internalize their manipulated "eligible" income or exhibit a pervasive reporting bias aimed at aligning their survey answers with their institutional status.

We now estimate the impact of eligibility for the *Bolsa Família* Program (BFP) on labor market outcomes, namely the probability of being informal and hours worked, using a local polynomial

Figure 2: Probability of Informality Around the *Bolsa Família* Eligibility Threshold



Regression Discontinuity Design (RDD) for the PME and PNADC data[6]. Because self-reported household income near the qualifying threshold is subject to strategic underreporting —as established before— a standard RDD would yield biased estimates due to local manipulation. We address this endogenous sorting by implementing a "donut-hole" RDD, which excludes observations nearest the cutoff y^* within a ±R$20 window. Still, all the estimates from the RDD should be interpreted as evidence of a discontinuity rather than causal estimates. The estimation sample is restricted to individuals with positive monthly per capita household income between y^* - R$100 and y^* + R$200, isolating the policy-relevant region where cash transfers directly alter labor incentives[7].

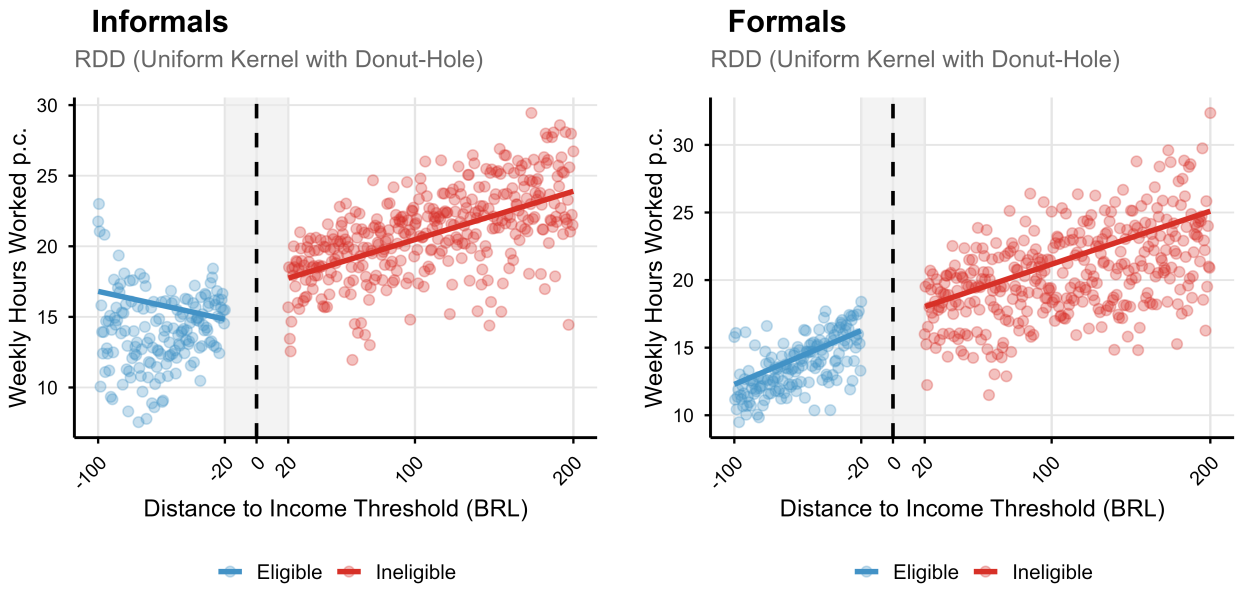
The RDD estimates for the probability of labor informality[8] show a highly statistically significant discontinuity at the eligibility cutoff. Crossing the boundary from eligible (below the threshold)

[6]As before, we use the data from the PME from 2003 to 2015 and of the PNADC from 2016 to 2019, standardized using the distance of the household per capita income from the BFP qualifying threshold for that particular period.

[7]We also run the estimations with "donut-holes" of R$30, R$10, and R$0 (no hole) as well as with alternative dependent variables of the probability of informality. The results are similar and are shown in Appendix D.

[8]As before, a household is defined as "formal" if at least one member is formally employed; or "informal" if no working-age member holds a formal job.

Figure 3: Weekly Hours Worked Around the *Bolsa Família* Eligibility Threshold



to ineligible (above the threshold) status decreases the probability of informal employment by 7.1 percentage points significant at the 1% level. Conversely, program eligibility imposes an implicit tax on formal employment, encouraging low-income households to remain in the informal sector to avoid losing the benefit. The estimation is illustrated in Figure 2 and the table with the estimated results is available in Appendix C.

The RDD estimates for weekly hours worked per capita[9], separated by formality status, are illustrated in Figure 3. We detect a significant discontinuity at the eligibility threshold for informal workers but not for the formal ones. Crossing the boundary from eligible (below the threshold) to ineligible (above the threshold) status increases in 2.7 the weekly hours worked for informal workers, significant at the 1% level. This suggests an intensive-margin response, where informal workers strategically reduce their hours to keep household income below the eligibility threshold.[10]

Taken together, these empirical findings demonstrate that household labor supply responds to *Bolsa*

[9]The weekly hours worked per capita are the sum of all the hours usually worked by every member in a household, divided by the number of working age individuals in the household. This allows us to consider potential substitution or complementary responses in the intensive margin by family members, for example.

[10]The table with the main estimations is available in Appendix C. For robustness, we also run the estimations with "donut-holes" of R$30, R$10, and R$0 (no hole). The results hold and are shown in Appendix D.

Família incentives along both the extensive (sectoral choice) and intensive (hours worked) margins. In developing labor markets, formal employment usually entails fully verifiable earnings via tax reports and rigid contractual working hours, constraining flexible labor supply adjustments. In contrast, informal employment provides both partial income unobservability[11] and schedule flexibility, enabling households to strategically adjust their labor supply to earn an observable income below the qualifying threshold, preserving benefit eligibility.

4.2 Child Labor and School Attendance

We now consider the role of child labor and the trade-off between schooling and labor for adolescents in low-income households. We use official data from *PNADC*, which includes a dedicated section on the characteristics of child labor in Brazil.[12]

Figure 4 shows the share of children not regularly attending school across age groups and household income per capita levels. Similarly, Figure 5 shows the child labor rate, using the official definition, for different ages and household income per capita levels.[13] The official definition of child labor follows International Labour Organization (ILO) criteria and classifies child labor as follows[14]: i) children aged 0-13 who perform any economic activity or self-consumption production, ii) children aged 14-15 who perform any economic activity without legal protections or work involving long hours, or self-consumption production, or any dangerous activity, iii) children aged 16-17 who perform any economic activity for more than 44 hours per week, work in a dangerous activity, or work in informality.

The figures reveal two main patterns: first, both school non-attendance and child labor rates remain

[11] Informality usually provides partial rather than full income unobservability, since governments frequently cross-check self-reported incomes using digital infrastructures and face-to-face evaluations.

[12] From 2016 onward, the *PNADC* included a particular section in the fifth-visit survey about work and time usage of children aged 5-17. We employ this section of the survey from 2016 to 2019, excluding the years 2020 to 2022, since this section was not asked due to the Covid-19 pandemic.

[13] Both the school non-attendance and child labor rates refer to the average of these rates from 2016 to 2024, excluding the years 2020 to 2022. The averages are calculated using each survey's population weights.

[14] For the full definition and construction of the child labor series, see the PNADC Technical Note 1/2020, IBGE (2021).

Figure 4: School Non-attendance Rates

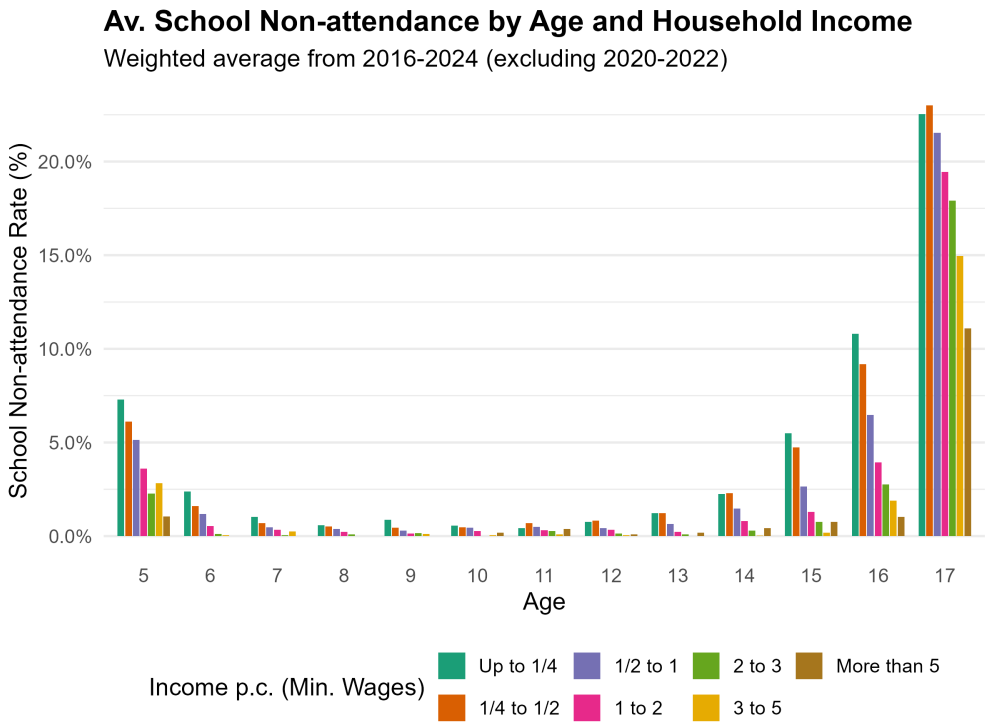
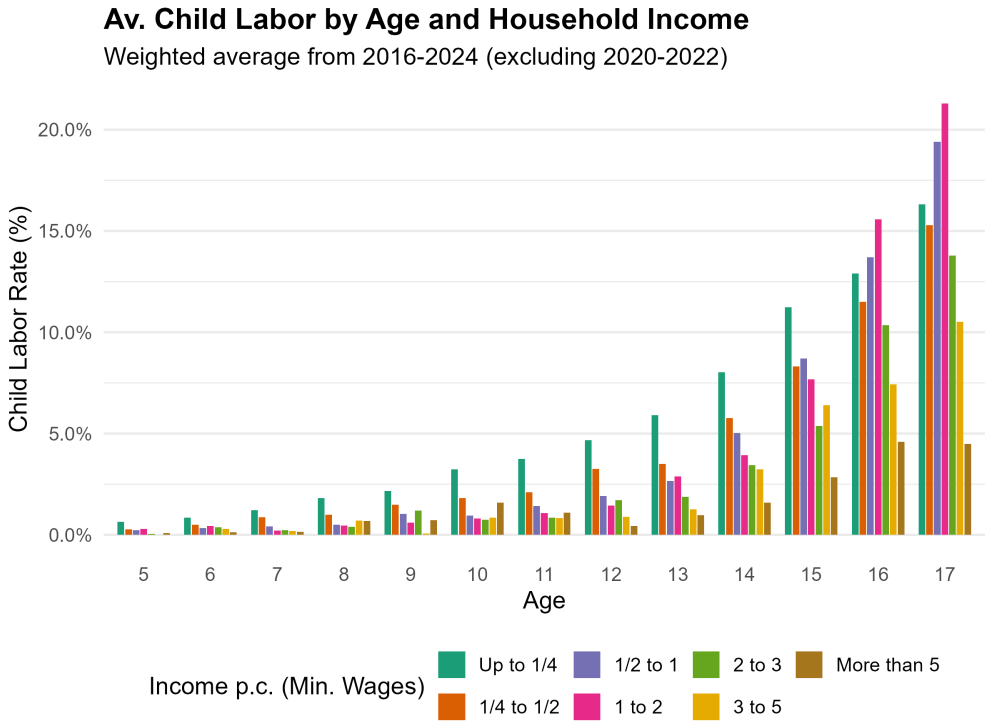


Figure 5: Child Labor Rate



very low and close to 0% for most ages but increase dramatically at ages 15, 16 and 17, consistent with the human capital theory —the opportunity cost of schooling rises as older children obtain higher wages—; and second, children in poorer households are more likely to stop attending school and start working, consistent with a relatively higher marginal utility of consumption in poorer households.

We argue that this trade-off between child labor and education is crucial for any policy that tries to break the intergenerational transmission of poverty, such as CCTs. To test if the household decisions regarding human capital accumulation and child labor are taken jointly or independently, we employ a Bivariate Probit framework. This approach corrects for endogeneity by allowing the unobserved determinants of both outcomes to correlate, effectively capturing the simultaneous nature of these choices. Additionally, we construct a conservative school dropout proxy that combines current school nonattendance with evidence of an accumulated schooling deficit.[15] The schooling-deficit condition is intended to distinguish persistent nonattendance from a temporary one.

We estimate the latent propensities for a child to drop out of school (y_1^*) and to participate in child labor (y_2^*) as a function of observable characteristics:

$$y_1^* = \beta_1' X + \epsilon_1 \tag{1}$$

$$y_2^* = \beta_2' X + \epsilon_2 \tag{2}$$

The vector X includes household real income per capita[16], age, gender, rural/urban residency status, and BFP recipient indicator, alongside state and year fixed effects to control for regional

[15]Defining dropout simply as nonattendance would be excessively broad because current nonattendance may reflect a temporary interruption or school completion. The PNADC does not directly identify whether an individual has permanently dropped out of school. Therefore, we consider dropouts as children who are not currently attending school and their completed years of schooling are below the age-specific benchmark, defined as age minus six. Because this measure is an indirect proxy rather than an observed transition out of school, it should be interpreted cautiously.

[16]We incorporate the real income per capita employing the official deflator series from the *IBGE* with a log transformation, $\log(Real_Income_p.c._{t,i} + 1)$.

heterogeneity and macroeconomic shocks. The error terms ϵ_1 and ϵ_2 are assumed to follow a standard bivariate normal distribution where $Cov(\epsilon_1, \epsilon_2) = \rho$. A statistically significant ρ indicates that unobservable factors jointly influence both outcomes, rendering independent models biased.

The results of the Bivariate Probit model reveal a highly significant error correlation: $\rho = 0.410$ ($p < 0.01$)[17]. This firmly establishes that the unobservable shocks driving a household to pull a child out of school are deeply intertwined with the shocks pushing that child into the labor market. Modeling one of these decisions without the other would ignore the complex reality of the household decision making process. The coefficients indicate that older or male children, as well as those living in rural areas, have a larger probability of dropping out of school and start working. Additionally, the baseline estimates suggest that participation in the *Bolsa Família* program significantly increases both the likelihood of school dropout ($\beta = 0.623$) and child labor ($\beta = 0.094$). However, these estimates are heavily confounded by selection bias. Because *Bolsa Família* is a narrowly targeted policy designed for households in extreme poverty, these baseline coefficients do not represent a causal failure of the program. Instead, they reflect the fact that beneficiaries are inherently the most vulnerable, structurally disadvantaged children in the dataset, who are more inclined to drop out of school and start working.

To distinguish the behavioral response of the cash transfer from this severe targeting bias, we construct a Propensity Score Matching (PSM) model. We pair treated and untreated children using nearest-neighbor matching with a stringent 0.05 caliper, balancing the sample on log real per capita income, age, gender, rural residency, and temporal fixed effects.[18] This procedure yields a perfectly balanced matched sample of 4,608 observations (2,304 treated, 2,304 control), effectively neutralizing selection bias arising from poverty targeting (all standardized mean differences ≤ 0.09). We then apply a second bivariate probit model to this matched sample, introducing categorical age cohorts (7–9, 10–12, 13–15, and 16–17 years) interacted with the *Bolsa Família* indicator. This

[17]The full table with the results is shown in Appendix E.

[18]To be able to match the observations, we had to omit the state fixed effects, since many states have very few or no observations for some income and age groups.

interaction explicitly tests whether the program's educational conditionalities succeed or fail at distinct developmental milestones.

Table 2: Matched Bivariate Probit: Age Cohort Interactions (Ages 7-17)

Variables	**(1) Conservative Dropout** *Coefficient (Std. Error)*	**(2) ILO Child Labor** *Coefficient (Std. Error)*
Main Effects		
Bolsa Família (Base: Ages 7–9)	-0.438	-0.697***
	(0.393)	(0.242)
Age 10 to 12	-0.033	0.008
	(0.284)	(0.171)
Age 13 to 15	0.463**	0.351**
	(0.234)	(0.149)
Age 16 to 17	1.117***	0.813***
	(0.223)	(0.144)
Interaction Terms		
Bolsa Família × Age 10 to 12	0.044	0.548*
	(0.530)	(0.292)
Bolsa Família × Age 13 to 15	0.547	0.828***
	(0.419)	(0.266)
Bolsa Família × Age 16 to 17	1.299***	0.853***
	(0.401)	(0.255)
Controls		
Log(Real Income p.c. + 1)	-0.134***	0.079***
	(0.028)	(0.028)
Female (1=Yes)	0.416***	-0.420***
	(0.080)	(0.061)
Rural (1=Yes)	0.174***	0.689***
	(0.067)	(0.060)
Year Fixed Effects	Yes	Yes
Error Correlation (ρ intercept)	0.439*** (0.103)	
Observations (Matched)	4,608 (2,304 treated, 2,304 control)	
Log-likelihood	-2060.7	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Base reference category for age interactions is the 7 to 9 cohort.

The matched interaction model results in Table 2 reveal a stark, age-dependent divergence in program efficacy. For the baseline cohort of young children (ages 7 to 9), *Bolsa Família* successfully and significantly reduces the probability of engaging in child labor ($\beta = -0.697$, $p < 0.01$). However, this effect deteriorates entirely as adolescents age. For 16-to-17-year-olds, the cash transfer is

associated with a massive relative surge in both severe school dropout ($\beta = 1.299$, $p < 0.01$) and child labor ($\beta = 0.853$, $p < 0.01$). Furthermore, the matched model uncovers persistent gender disparities: young women are less likely to engage in child labor ($\beta = -0.420$) but significantly more vulnerable to school dropout ($\beta = 0.416$), pointing to the systemic substitution of labor for other activities like domestic care work. Finally, the matched error correlation remains positive and highly significant ($\rho = 0.439$, $p < 0.01$), confirming that the household shocks pushing an adolescent out of school simultaneously push them into the labor force.

Two central conclusions emerge from this analysis. First, household decisions regarding child labor and school attendance are deeply joint and interdependent—a reality especially pronounced among low-income families. Consequently, theoretical models evaluating human capital accumulation in emerging economies should explicitly model both choices simultaneously rather than treating schooling in isolation. Second, CCT programs such as *Bolsa Família* should internalize in their structural design the opportunity cost and the direct costs of education. Because the economic trade-offs of schooling incorporate both costs, policy benefits must be generous enough to effectively protect against adolescent school dropout and early labor market entry.

Overall, our empirical analysis demonstrates that targeted conditional cash transfer (CCT) programs can generate unintended distortions in household labor supply and human capital investments within emerging economies characterized by large informality and child labor. Two primary mechanisms drive these findings. First, strict income eligibility cutoffs induce strategic informality near the threshold. Second, transfers that fail to compensate the opportunity cost of schooling will also fail to retain children in school, especially in secondary or post-secondary levels. To formalize these joint behavioral responses, the following section presents a theoretical simple model that endogenizes household sectoral, schooling and child labor choices, illustrating the main mechanisms through which CCT policies operate.

5 A Simple Model

To illustrate how conditional cash transfers affect human capital investment and households' choices between formal and informal employment, we begin with a simple two-period model with child labor. The goal of the model is to clarify the key tradeoffs faced by households when transfers are means-tested and conditional on investments in children. In particular, the model highlights how human capital accumulation, child labor, and strategic informality respond to the design of the CCT policy.

Assume two periods, $t = 1, 2$, and two agents: a parent, indexed by p, and a child, indexed by k. Adult agents can work in one of two sectors, formal or informal, denoted by $s_t^i \in \{F, I\}$. The parent enters the first period with assets a_1 and human capital h^p. She chooses consumption c_1^p, hours worked l_1^p, sector of employment s_1^p, savings for herself a_2^p, transfers to the child a_2^k (which will be the initial assets of the child in $t = 2$), whether to invest in the child's human capital $HC \in \{0, 1\}$, and whether to use child labor $CL \in \{0, 1\}$. Investing in the child's human capital captures actions such as school attendance and health checkups, which are the conditionalities required to receive the CCT. Child labor provides current income to the household and can affect the child's future human capital.

The parent's decisions over human capital investment and child labor determine the child's stock of human capital in the second period:[19]

$$\log h^k = \log \bar{h} + \alpha_1 HC + \alpha_2 CL + \alpha_3 HC \times CL. \tag{3}$$

The parameter $\bar{h}$ is the child's baseline level of human capital, obtained when the parent does not invest and the child does not work. The parameter $\alpha_1 > 0$ captures the log return to human capital

[19]Even though our empirical analysis suggests age-dependent heterogeneous effects in schooling and child labor, we do not explore such heterogeneity in this paper. This is a deliberate choice to keep the model tractable, while still studying the key mechanisms and trade-offs associated with CCTs.

investment when the child does not work, while $\alpha_2 < 0$ captures the log human capital cost of child labor when the parent does not invest. The interaction term α_3 allows these effects to be non-additive. When the child works, the log return to investment is $\alpha_1 + \alpha_3$; when the parent invests, the log effect of child labor is $\alpha_2 + \alpha_3$. Thus, $\alpha_3 < 0$ means that child labor reduces the effectiveness of investment, while $\alpha_3 > 0$ means that investment mitigates the adverse effect of child labor. A positive interaction does not necessarily make child labor beneficial, since its effect also depends on α_2.[20]

In the second period, the child becomes independent and forms a new household with no children. Given human capital h^k and assets a_2^k, the child chooses consumption, hours worked, and sector of employment, (c_2^k, l_2^k, s_2^k). The parent also chooses second-period consumption, hours worked, and sector of employment, (c_2^p, l_2^p, s_2^p), taking as given her own assets a_2^p and human capital h^p. After the second period, the economy ends.

Preferences are given by $u(c_t^i, l_t^i) = \ln(c_t^i) + \mu \ln(1 - l_t^i)$. The parent discounts second-period utility by β and values the child's second-period utility with altruism weight δ. A lower δ reduces the importance of the child's future welfare relative to the parent's own consumption and leisure.

Income depends on human capital, hours worked, and the sector of employment:

$$y_t^i = \begin{cases} w_t h^i l_t^i, & \text{if } s_t^i = F, \\ (1 - \psi(h^i)) w_t h^i l_t^i, & \text{if } s_t^i = I. \end{cases} \tag{4}$$

Both sectors face the same wage per efficiency unit, w_t, but informal employment entails a productivity penalty $\psi(h^i)$. We assume that this penalty is increasing in human capital, so that informality is relatively more costly for higher-human-capital workers.

[20] Emerson and Souza (2011) estimate the effect of working earlier in life on adult earnings in Brazil. They show that child labor can affect adult outcomes both through schooling and through effects that remain after controlling for schooling, and that these effects vary with the age at which work begins. This motivates allowing a flexible interaction between child labor and human capital investment.

Formal income is fully observed by the government, while informal workers can hide a share χ of their earnings:

$$y_t^{i,\text{obs}} = \begin{cases} y_t^i, & \text{if } s_t^i = F, \\ (1-\chi)y_t^i, & \text{if } s_t^i = I. \end{cases} \tag{5}$$

Informality therefore reduces gross earnings but also lowers taxable and observed income.

The government taxes observed income at rate τ_t^y. The CCT policy is means-tested and depends on observed income and the investments in the child's human capital. In the first period, the parent receives the transfer only if observed income is below the income eligibility threshold ($y_t^{i,\text{obs}} \leq \bar{y}$) and if she invests in the child's human capital ($HC = 1$). In the second period, both the parent and the child receive transfers only if their observed income is below the income eligibility threshold.[21]

The parent's problem is

$$V(a_1, h^p) = \max_{c_1^p, l_1^p, s_1^p, HC, CL, \{c_2^i, l_2^i, s_2^i, a_2^i\}_{i=p,k}} \Big\{ u(c_1^p, l_1^p) + \beta\big[u(c_2^p, l_2^p) + \delta u(c_2^k, l_2^k)\big] \Big\} \tag{6}$$

subject to

$$c_1^p + a_2^p + a_2^k + \kappa HC = y_1^p - \tau_1^y y_1^{p,\text{obs}} + \phi CL + a_1 + \omega_1 \mathbf{1}\{y_1^{p,\text{obs}} \leq \bar{y}\}\mathbf{1}\{HC = 1\}, \tag{7}$$

$$c_2^i = y_2^i - \tau_2^y y_2^{i,\text{obs}} + (1+r)a_2^i + \omega_2 \mathbf{1}\{y_2^{i,\text{obs}} \leq \bar{y}\}, \qquad i \in \{p, k\}, \tag{8}$$

$$\log h^k = \log \bar{h} + \alpha_1 HC + \alpha_2 CL + \alpha_3 HC \times CL, \tag{9}$$

$$c_1^p > 0, \qquad l_1^p \in [0, 1), \qquad HC, CL \in \{0, 1\}, \tag{10}$$

$$a_2^i \geq 0, \qquad c_2^i > 0, \qquad l_2^i \in [0, 1), \qquad i \in \{p, k\}, \tag{11}$$

with total and observed labor income defined above.

The first constraint is the parent's first-period budget constraint. The household uses resources

[21] This will be consistent with Brazil's BFP in which the transfer in a household with no children under 18 and no pregnant or lactating mother, will only be conditional on the per capita household income being below the poverty line. See Section 3 for more details on the BFP transfers.

to consume, save for the parent, transfer assets to the child, and pay a cost κ of investing in the child's human capital. Resources come from labor income net of taxes, initial assets, the income ϕ generated by child labor, and the CCT transfer. The first-period transfer is received only when the parent is income-eligible and complies with the conditionality by choosing $HC = 1$. The second constraint describes second-period consumption for both the parent and the child. Each agent receives labor income net of taxes, asset returns, and a transfer if observed income is below the eligibility threshold. Unlike in the first period, this transfer does not depend on human capital investment because neither household has a dependent child.

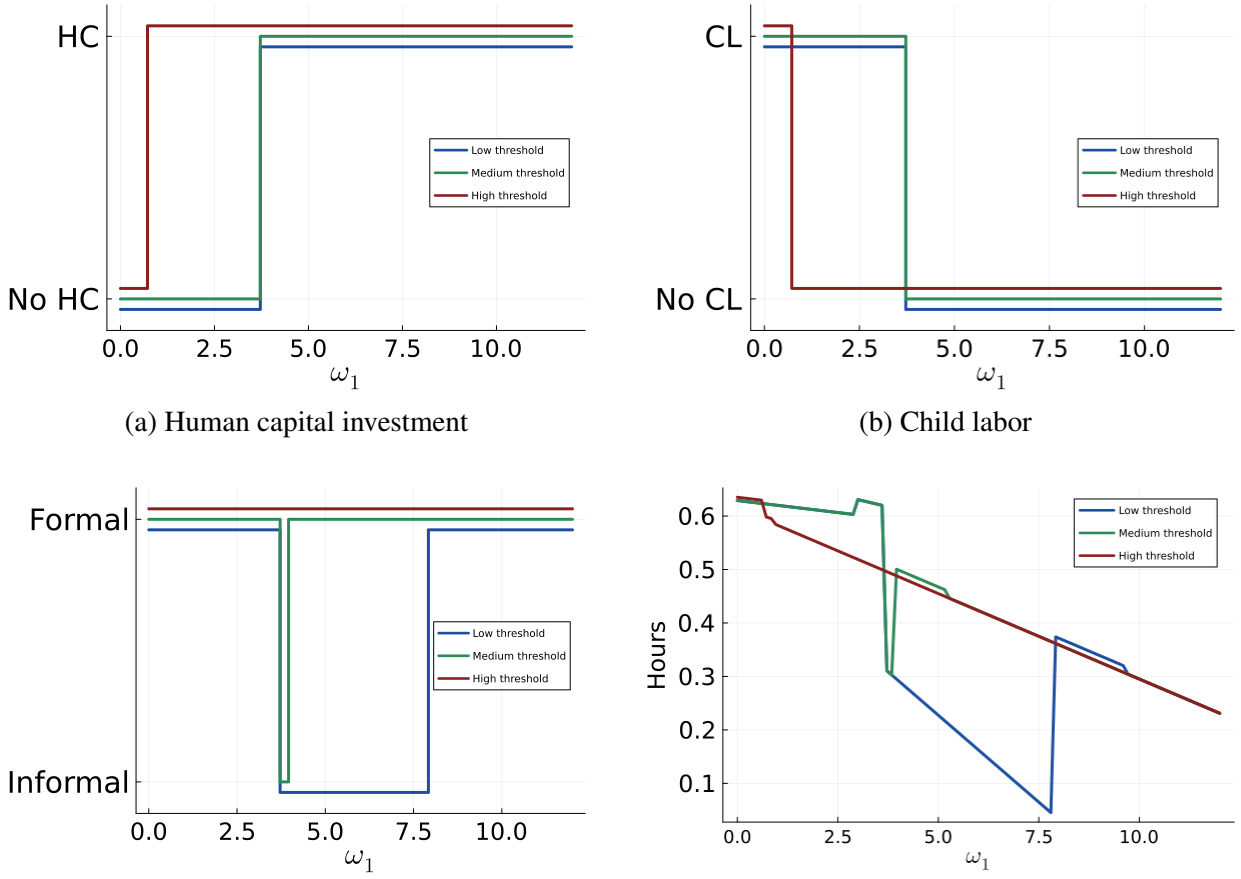
5.1 Main mechanisms

This simple environment illustrates the interaction between human capital investment, child labor, and strategic informality under a means-tested CCT. Human capital investment has a current cost, which families may partly finance with children's earnings. Informal employment reduces productivity but also lowers observed income, potentially allowing the household to qualify for transfers. Households can also adjust observed earnings through hours worked. Changes in benefits or eligibility therefore affect both the resources available to finance schooling and the incentives governing sector and hours choices.

Figure 6 follows one household as benefits increase under three eligibility thresholds in the first period. Panels (a) and (b) show that, in these examples, larger benefits induce human capital investment and eliminate child labor. These changes occur at smaller benefit amounts under the high threshold. This illustrates how the income test affects the household's response to the schooling requirement, holding the human-capital technology fixed.

Panel (c) illustrates the possibility of strategic informality. Under the low and medium thresholds, the household chooses formality when benefits are small, informality at intermediate benefit levels, and formality again when benefits become sufficiently large. Access to transfers can make informal work attractive despite its productivity cost. This response is not uniform across eligibility rules:

Figure 6: Toy model: Main mechanisms



(a) Human capital investment

(b) Child labor

(c) Sectoral choice

(d) Hours worked

under the high threshold, the household remains formal throughout the plotted range.

Panel (d) shows that hours also respond, including when sector choice remains unchanged. Under the high threshold, parental hours decline as benefits increase even though the household continues working formally. Under the low threshold, hours decline over the range of informal employment, but jump upward when the household returns to formality. Although hours remain below their initial level, this return to formality does not coincide with a further hours reduction. Sector and hours choices therefore adjust jointly, rather than following a single pattern as benefits increase.

These examples guide the quantitative exercises. We examine how benefit and threshold expansions affect informality under different financing rules, and whether sector and hours adjustments

allow households to obtain or retain BFP. We also compare the macroeconomic and welfare effects of alternative transfer designs in economies with and without informality.

6 Quantitative Model

6.1 Demography, timing, and period length

We consider a stationary overlapping-generations economy populated by a continuum of households. The life cycle consists of childhood, three working stages, and retirement:

$$\begin{aligned} j = 0: &\quad \text{childhood, ages } 5\text{–}17, \\ j = 1: &\quad \text{young workers, ages } 18\text{–}30, \\ j = 2: &\quad \text{working parents, ages } 31\text{–}43, \\ j = 3: &\quad \text{older workers, ages } 44\text{–}56, \\ R: &\quad \text{retirement, beginning at age } 57. \end{aligned}$$

Childhood and each working stage last 13 years. Retirement lasts $N_R = 19$ years, consistent with life expectancy in Brazil, and the value of the remaining lifetime consumption and saving decisions is summarized by V^R.

We begin modeling children at age 5, consistent with the starting age of child-labor measurement in the Brazilian data. We do not model decisions during ages 0–4 separately. Their effects, together with family background, early health, and other pre-market characteristics, are summarized by a child-type shock observed by the parent.

When an adult enters stage $j = 2$, she has a dependent child in stage $j = 0$ and observes the child's type. She then chooses whether to invest in the child's human capital and whether the child works. At the beginning of the parent's stage $j = 3$, the child becomes an independent adult and enters stage $j = 1$ with the human capital generated during childhood. The parent can then transfer assets

to the newly independent child. This timing preserves the main intergenerational decisions while keeping the model tractable.

Adults retire at age 57, after completing stage $j = 3$. This age is close to the average age of 58 at which new retirement benefits were granted under Brazil's Social Security rules in 2016, the year we consider in our calibration. Choosing age 57 keeps the three working stages equal in length while remaining close to this empirical benchmark. During retirement, households receive pension benefits and finance consumption using these benefits and the assets carried from their final working stage.

Households choose consumption, labor supply, and sector once per working stage and keep them constant during its 13 years. Monetary flows are expressed in annual units, while assets and inter vivos transfers are stocks. Retirement decisions are annual.

6.2 Preferences

An adult household has period utility

$$u(c, l) = \frac{c^{1-\gamma_c}}{1-\gamma_c} - \mu \frac{l^{1+\gamma_\ell}}{1+\gamma_\ell},$$

where c is annual consumption, $l \in [0, 1]$ is labor supply as a fraction of available time, $\gamma_c > 0$ is the coefficient of relative risk aversion, $\gamma_\ell > 0$ is the inverse Frisch elasticity of labor supply, and $\mu > 0$ is the weight on the disutility of labor. Households discount future utility using the model-period discount factor $\beta \in (0, 1)$.[22] Parents are altruistic toward their children in the Barro–Becker sense, with altruism parameter $\delta \geq 0$.

[22] Each working stage lasts 13 years, so $\beta = \beta_a^{13}$, where β_a is the annual discount factor. Value functions use a common utility normalization, described together with the annual accounting conventions in Appendix F.

6.3 Childhood human capital accumulation

The parent chooses two binary variables for the child during ages 5–17: human-capital investment, $HC \in \{0, 1\}$, and child labor, $CL \in \{0, 1\}$. The child's adult human capital at age 18 is

$$\log h^k = \alpha_1 HC + \alpha_2 CL + \alpha_3 HC \times CL + \varepsilon^k,$$

where ε^k is the child's inherited type, which we assume the parent observes before choosing HC and CL. The coefficients α_1, α_2, and α_3 have the interpretation described in Section 5.

We allow for intergenerational persistence in child endowments by assuming that the child type follows an AR(1) process across generations:

$$\varepsilon^k = \rho_\varepsilon \varepsilon + \nu^k, \qquad \nu^k \sim \mathcal{N}(0, \sigma_\nu^2),$$

where ε is the parent's inherited type, ρ_ε captures intergenerational persistence, and ν^k is an idiosyncratic shock. We assume that human capital investment entails an annual resource cost κ, while child labor generates annual resources ϕ.

6.4 Production, factor prices, and informality

Final output is produced competitively with physical capital and effective labor:

$$Y = AK^\alpha H^{1-\alpha}, \qquad \alpha \in (0, 1).$$

We do not model a separate informal representative firm. Instead, we model informality as a household labor-market status that changes the number of efficiency units supplied by a worker. Aggregate effective labor is

$$H = \int h_i l_i \left[\mathbf{1}\{s_i = F\} + (1 - \psi(h_i))\mathbf{1}\{s_i = I\}\right] di,$$

where h_i is the worker's human capital, $s_i \in \{F, I\}$ denotes formal or informal work, and $\psi(h)$ is the productivity cost of informal labor. We impose $0 < \psi(h) < 1$, so that informal labor is less productive than formal labor but still supplies positive efficiency units.

The annual wage per efficiency unit of labor and the net interest rate are

$$w = (1-\alpha)AK^{\alpha}H^{-\alpha}, \qquad r = \alpha AK^{\alpha-1}H^{1-\alpha} - \delta_K,$$

where δ_K is physical-capital depreciation.

We assume that the productivity penalty from informality is weakly increasing in human capital. This captures the idea that high-human-capital workers may lose more from informality because informal jobs often provide less access to capital, scale, contracts, training, and stable employment relationships[23]. To keep the model parsimonious, we use a bounded specification that is linear in normalized log human capital:

$$\psi(h) = \psi_L + (\psi_H - \psi_L)x(h), \qquad 0 \le \psi_L \le \psi_H < 1,$$

with $x(h) = \min\left\{1,\ \max\left[0,\ \frac{\log h - \log \underline{h}}{\log \overline{h} - \log \underline{h}}\right]\right\}$, where $\underline{h}$ and $\overline{h}$ are fixed lower and upper bounds for the human-capital support.

Therefore, ψ_L is the informality cost at the lower bound of the human-capital support, while ψ_H is the informality cost at the upper bound. The difference $\psi_H - \psi_L$ governs how strongly the informality cost rises with human capital.

Given w, *actual* labor income is

$$y(h,l,s) = \begin{cases} whl, & s = F, \\ w(1-\psi(h))hl, & s = I. \end{cases}$$

[23] See the discussion in Ulyssea (2018) and the reduced-form formulation in Ayres et al. (2025).

The government observes $\tilde{y}(h, l, s)$ labor income, which equals $y(h, l, s)$ under formality and $(1 - \chi)y(h, l, s)$ under informality, where $\chi \in [0, 1)$ is the fraction of informal income hidden from the government.

6.5 Government, taxes, and the CCT policy

The government levies a progressive labor-income tax on observed income, denoted by $T_y(\tilde{y})$, a proportional tax on capital income at rate τ_a, and a proportional consumption tax at rate τ_c. G denotes government expenditures other than pension payments and CCT benefits. In the baseline economy, G adjusts residually to balance the government budget. The CCT policy is means tested, where $\bar{y}$ denotes the observed-income threshold to qualify to receive the transfers.[24]

Transfers depend on family composition:

$$B_j(\tilde{y}, a, HC) = \omega_1 \mathbf{1}\{\tilde{y} + ra \leq \bar{y}\}\mathbf{1}\{HC = 1\}, \quad \text{for } j = 2,$$

$$B_j(\tilde{y}, a) = \omega_0 \mathbf{1}\{\tilde{y} + ra \leq \bar{y}\}, \quad \forall j \in \{1, 3\},$$

where $\omega_1 > \omega_0 > 0$ and ra is asset income. The term ω_1 is the transfer received by a working parent with a dependent child if the household is income-eligible and complies with the non-pecuniary conditionality, while ω_0 is the transfer for poor households outside the dependent-child stage. This reduced-form family-structure schedule is consistent with the fact that *Bolsa Família* benefits depend on household composition.

6.6 Household problems

Assets a and a' are held at the beginning and end of a working stage, respectively. The budget constraints accumulate initial assets by R_{13} and annual flows by F_{13}.[25]

[24] For tractability, we summarize the eligibility rules with a single observed-income threshold rather than modeling separate extreme-poverty and poverty lines. This abstraction preserves the key margin for the model, while avoiding additional policy kinks that would complicate the analysis without necessarily changing the main mechanism.

[25] $R_{13} = [1 + (1 - \tau_a)r]^{13}$ and $F_{13} = \sum_{n=0}^{12}[1 + (1 - \tau_a)r]^n$. Appendix F derives these expressions.

Stage $j = 1$ (young independent worker). A young adult with assets a, human capital h, and inherited type ε solves

$$V_1(a, h, \varepsilon) = \max_{c,l,s,a'} \left\{ u(c, l) + \beta \mathbb{E}\left[V_2(a', h, \varepsilon^k) \mid \varepsilon \right] \right\},$$
$$\text{s.t.} \quad (1 + \tau_c) F_{13} c + a' = R_{13} a + F_{13} \Big[y(h, l, s) - T_y\big(\tilde{y}(h, l, s)\big) + B_1\big(\tilde{y}(h, l, s), a\big) \Big],$$
$$c > 0, \quad a' \geq 0, \quad l \in [0, 1], \quad s \in \{F, I\}.$$

Stage $j = 2$ (working parent). After observing the child's type ε^k, the parent solves

$$V_2(a, h, \varepsilon^k) = \max_{c,l,s,a',HC,CL} \left\{ u(c, l) + \beta V_3^M(a', h, h^k, \varepsilon^k) \right\},$$
$$\text{s.t.} \quad (1 + \tau_c) F_{13} c + a' = R_{13} a + F_{13} \Big[y(h, l, s) - T_y\big(\tilde{y}(h, l, s)\big) + B_2\big(\tilde{y}(h, l, s), a, HC\big) + \phi CL - \kappa HC \Big],$$
$$\log h^k = \alpha_1 HC + \alpha_2 CL + \alpha_3 HC \times CL + \varepsilon^k,$$
$$c > 0, \quad a' \geq 0, \quad l \in [0, 1], \quad s \in \{F, I\}, \quad HC, CL \in \{0, 1\}.$$

Stage $j = 3$ (older worker and inter vivos transfer). For convenience, we split the stage-3 problem into two subproblems: morning and afternoon. In the morning, the child becomes independent and the parent chooses an asset transfer $\hat{a}$:

$$V_3^M(a, h, h^k, \varepsilon^k) = \max_{\hat{a}} \left\{ V_3^N(a - \hat{a}, h) + \delta V_1(\hat{a}, h^k, \varepsilon^k) \right\},$$
$$\text{s.t.} \quad 0 \leq \hat{a} \leq a.$$

In the afternoon, taking this transfer $\hat{a}$ as given, the parent solves

$$V_3^N(a-\hat{a},h) = \max_{c,l,s,a'} \left\{ u(c,l) + \beta V^R(a', \tilde{y}_3^{ann}) \right\},$$

$$\text{s.t.} \quad (1+\tau_c)F_{13}c + a' = R_{13}(a-\hat{a}) + F_{13}\Big[y(h,l,s) - T_y\big(\tilde{y}(h,l,s)\big) + B_3\big(\tilde{y}(h,l,s), a-\hat{a}\big)\Big],$$

$$c > 0, \quad a' \geq 0, \quad l \in [0,1], \quad s \in \{F, I\}.$$

The household enters retirement with assets a'.

6.7 Retirement

After age 56, households retire for N_R years. They do not work, receive deterministic pensions, and face no survival risk. We summarize their remaining utility through the continuation value V^R, following Gourinchas and Parker (2002).

Let a_4 denote assets at retirement and $\tilde{y}_3^{ann}$ denote annual observed labor income in the last working stage. We use the simplified pension rule of Delalibera et al. (2025), as adopted by Ferreira et al. (2025):

$$b_{ss}(\tilde{y}_3^{ann}) = \max\left\{ \min\left\{ \nu_{ss}\tilde{y}_3^{ann}, y_{\max} \right\}, y_{\min} \right\},$$

where ν_{ss} is the replacement rate, and $y_{\min}$ and $y_{\max}$ are the pension floor and cap. Using last-stage income rather than the full contribution history avoids adding a state variable to the working-life problems.

With annual discount factor β_a and the same annual interest rate r, the retirement problem is

$$V^R(a_4, \tilde{y}_3^{ann}) = \max_{\{c_n, a_{n+1}\}_{n=0}^{N_R-1}} \frac{1-\beta_a}{1-\beta} \sum_{n=0}^{N_R-1} \beta_a^n \frac{c_n^{1-\gamma_c}}{1-\gamma_c},$$

$$\text{s.t.} \quad (1+\tau_c)c_n + a_{n+1} = [1+(1-\tau_a)r]a_n + b_{ss}(\tilde{y}_3^{ann}), \quad n = 0, \ldots, N_R - 1,$$

$$a_0 = a_4, \quad a_{N_R} = 0, \quad a_{n+1} \geq 0, \quad c_n > 0.$$

The factor $(1-\beta_a)/(1-\beta)$ expresses retirement utility in the same units as the working-stage value functions.

For tractability, we assume that retirees do not receive *Bolsa Família*. According to administrative data, approximately 95% of benefit recipients are younger than age 56, so our working stages cover the ages most relevant for program receipt.

6.8 Recursive stationary equilibrium

Let λ denote the collection of household-state distributions for each working stage and retirement year, including those before and after the stage-3 inter vivos transfer. The corresponding population-weighted measures for working households and retirees are denoted by λ^W and λ^R, respectively, with the working-age population normalized to one.

Definition 1 (Recursive stationary competitive equilibrium). Given the tax, pension, and *Bolsa Família* rules, a recursive stationary competitive equilibrium consists of value functions $\{V_1, V_2, V_3^M, V_3^N, V^R\}$, the corresponding household policy functions, factor prices (w, r), aggregate quantities, government spending G, and distributions (λ^W, λ^R), such that:

1. *Households optimize.* Given prices and policy rules, the value functions and household policies solve the recursive problems defined above.

2. *Firms optimize.* Production and factor prices satisfy

$$Y = AK^{\alpha}H^{1-\alpha}, \qquad w = (1-\alpha)\frac{Y}{H}, \qquad r = \alpha\frac{Y}{K} - \delta_K.$$

3. *Individual and aggregate behavior are consistent.* Capital and effective labor supplied by households equal the quantities used in production:

$$K = \int a(x)\, d\lambda^W(x) + \int a(x)\, d\lambda^R(x),$$

$$H = \int h(x)l(x)\Big[\mathbf{1}\{s(x) = F\} + (1 - \psi(h(x)))\mathbf{1}\{s(x) = I\}\Big] d\lambda^W(x),$$

where x denotes the household working/retirement state and $a(x)$ denotes its contribution to aggregate capital. For stage-3 workers, λ^W uses the distribution after the inter vivos transfer.

Total consumption includes working households and retirees:

$$C = C^W + C^R = \int c^W(x)\, d\lambda^W(x) + \int c^R(x)\, d\lambda^R(x).$$

4. *The final-good market clears.*

$$Y + Y^{CL} = C + \delta_K K + G + X^{HC},$$

where child-produced resources and schooling expenditures are

$$Y^{CL} = \phi \int_{\{x:\, j(x)=2\}} CL(x)\, d\lambda^W(x), \qquad X^{HC} = \kappa \int_{\{x:\, j(x)=2\}} HC(x)\, d\lambda^W(x).$$

Both integrals cover households with dependent children ($j(x) = 2$), and stationarity implies that investment replaces depreciated capital.

5. *The government budget balances.*

$$\mathcal{T}_y + \tau_a r K + \tau_c C = \mathcal{B} + SS + G,$$

where $\mathcal{T}_y$ is aggregate labor-tax revenue, including pension contributions; $\mathcal{B}$ is aggregate Bolsa Família spending; and SS denotes spending on pensions.

6. *The household distributions are stationary.*

$$(\lambda^W, \lambda^R) = \mathcal{P}(\lambda^W, \lambda^R),$$

where $\mathcal{P}$ maps the collection of household distributions into the distributions induced by household policies, the human-capital transition, and the demographic structure.

7 Calibration and Identification

We calibrate the model to Brazil in 2016. Monetary quantities are expressed in annual-minimum-wage units. We set parameters externally using policy rules, microdata, and estimates from the literature, and jointly estimate the remaining parameters by the simulated method of moments (SMM), following McFadden (1989) and Pakes and Pollard (1989).

7.1 Externally calibrated parameters

We use standard values for the preference curvatures and calculate the capital share and depreciation rate from the Penn World Table. The share of informal income hidden from the government follows Ayres et al. (2025), while intergenerational type persistence is set using the Brazilian intergenerational income-elasticity estimate of Britto et al. (2026). We measure the schooling cost using education expenditures per attending child in the 2017–2018 Household Budget Survey (POF), expressed in 2016 prices.

Policy parameters follow statutory rules and administrative data for the calibration year. Labor-income taxation combines capped pension contributions and progressive income taxes. The consumption-tax rate follows Azevedo and Fasolo (2015), whose effective tax estimate serves as a benchmark rather than a direct estimate for 2016. For BFP, we convert the per-capita poverty line into a household threshold using a representative beneficiary family size of approximately four people. Households with dependent children receive the administrative average benefit per beneficiary family, while eligible households outside that stage receive the basic benefit.

We estimate the human-capital coefficients using two survey-weighted regressions from the 2016 PNADC. We combine the association between adult schooling and log hourly earnings with the

associations between children's completed schooling, attendance, child labor, and their interaction. These estimates are associations, not causal effects. In the model, they imply that child labor reduces human capital both with and without schooling, with a larger proportional reduction without schooling. Appendix G.1 reports the parameter table, sources, and estimation details.

7.2 Parameters estimated by SMM

We jointly estimate the remaining eight parameters, $(\beta_a, \mu, \delta, A, \psi_L, \psi_H, \sigma_\nu, \phi)$, by SMM using nine moments. The capital-output ratio comes from the Penn World Table, while the remaining targets come from the 2016 annual PNADC.

The capital-output ratio, mean labor supply, mean earnings, and dispersion of log earnings primarily discipline β_a, μ, A, and σ_ν, respectively. Aggregate informality and its education gap inform the informality costs ψ_L and ψ_H. Given the externally calibrated schooling cost and human-capital coefficients, school nonattendance primarily disciplines altruism δ, while the child-labor rate primarily disciplines child-produced resources ϕ. The joint school-and-work rate provides an additional restriction on how the value of the child's future human capital interacts with the current return to child labor. Appendix G.3 reports the parameter estimates and details of the target construction.

Table 3 compares the model with its targets.

Table 3: Model Fit – Targeted Moments

Targeted moment	Model	Data
Capital-output ratio	4.94	4.46
Mean adult labor supply	26.99%	35.70%
Mean adult earnings	2.58	2.30
Standard deviation of adult log earnings	1.04	0.88
Aggregate informality rate	32.67%	36.16%
Informality rate gap by education: low minus high education	30.33 pp	30.85 pp
School nonattendance rate, ages 5–17	4.74%	4.26%
Child labor rate, ages 5–17	5.76%	5.74%
Joint school-and-work rate, ages 5–17	4.32%	4.83%

The model closely matches aggregate informality, its education gap, and the three child-related moments, which are central to our policy exercises. The main discrepancies concern adult labor supply and earnings dispersion: labor supply is understated and dispersion is overstated. The capital-output ratio and mean earnings also exceed their targets.

7.3 Validation moments

We assess the model using untargeted moments of program participation, earnings, and life-cycle outcomes. Table 4 reports BFP spending and household coverage, the formal–informal hourly-earnings gap, and residual earnings dispersion.

Table 4: Validation Moments

Moment	Model	Data
Bolsa Família spending relative to GDP	0.56%	0.47%
Bolsa Família household coverage rate	18.79%	14.27%
Formal–informal hourly-earnings gap, all workers	0.62	0.61
Standard deviation of residual log annual earnings	0.68	0.68

Notes: None of the moments in this table enters the SMM objective. The empirical household coverage rate is calculated using the 2016 annual PNADC. The *Bolsa Família* spending-to-GDP ratio combines administrative program expenditure with aggregate GDP. The formal–informal earnings gap is the difference in mean log hourly earnings between formal and informal workers.

The model overstates program coverage and spending, but closely matches the formal–informal hourly-earnings gap. Residual earnings dispersion is also close, although differences in regression controls make this an auxiliary comparison. Appendix G.4 explains its construction.

Table 5 compares labor-market outcomes across the three working stages, which are also untargeted.

Labor supply is understated and earnings dispersion overstated in every stage. Mean earnings are too high, especially among young workers. The model also fails to reproduce the rise in informality in the final stage. This discrepancy is consistent with the simplified pension rule, which links benefits to final-stage observed earnings and strengthens incentives to work formally

Table 5: Life-Cycle Validation Moments

	Ages 18–30		Ages 31–43		Ages 44–56	
Moment	Model	Data	Model	Data	Model	Data
Mean labor supply	26.10%	35.27%	24.92%	36.03%	29.94%	35.75%
Mean annual earnings	2.46	1.66	2.32	2.51	2.97	2.77
Standard deviation of log earnings	1.00	0.75	1.00	0.89	1.10	0.97
Informality rate	39.96%	36.28%	34.94%	33.97%	23.12%	39.11%

Notes: Empirical moments are calculated using the 2016 annual PNADC.

before retirement, subject to the pension floor and cap.

8 Counterfactual exercises

We use the calibrated model to study two questions. First, how do expansions of *Bolsa Família* affect informality, and how do households adjust sector and hours to obtain or retain benefits? Second, how do income targeting and the schooling condition affect macroeconomic outcomes and welfare, with and without informality? We keep structural parameters fixed throughout. In each comparison, government consumption remains at its value in the corresponding baseline equilibrium.

8.1 BFP expansions and strategic informality

We expand BFP by multiplying either both benefit amounts, ω_0 and ω_1, or the income threshold, $\bar{y}$, by a factor between one and two. Benefit expansions preserve the ratio between payments and leave the threshold unchanged, while threshold expansions leave payments unchanged. We solve a new stationary equilibrium for each reform.

We consider two financing instruments: a common multiplier on the progressive labor-income tax schedule or an adjustment in the consumption tax. Pension contributions and capital-income taxes remain fixed. This distinction matters because labor-income taxes directly affect the return to hiding income, whereas consumption-tax financing leaves that schedule unchanged.

Figure 7 shows that both policy design and financing matter. Under consumption-tax financing, doubling the threshold raises informality by 18.86 percentage points, compared with 3.20 points when benefits double, from a baseline rate of 32.67%. Labor-tax financing produces larger increases, bringing informality to 59.03% under the doubled threshold and 51.27% under doubled benefits. With consumption-tax financing, the tax rate rises from 25.30% to 26.58% when benefits double and to 27.93% when the threshold doubles.

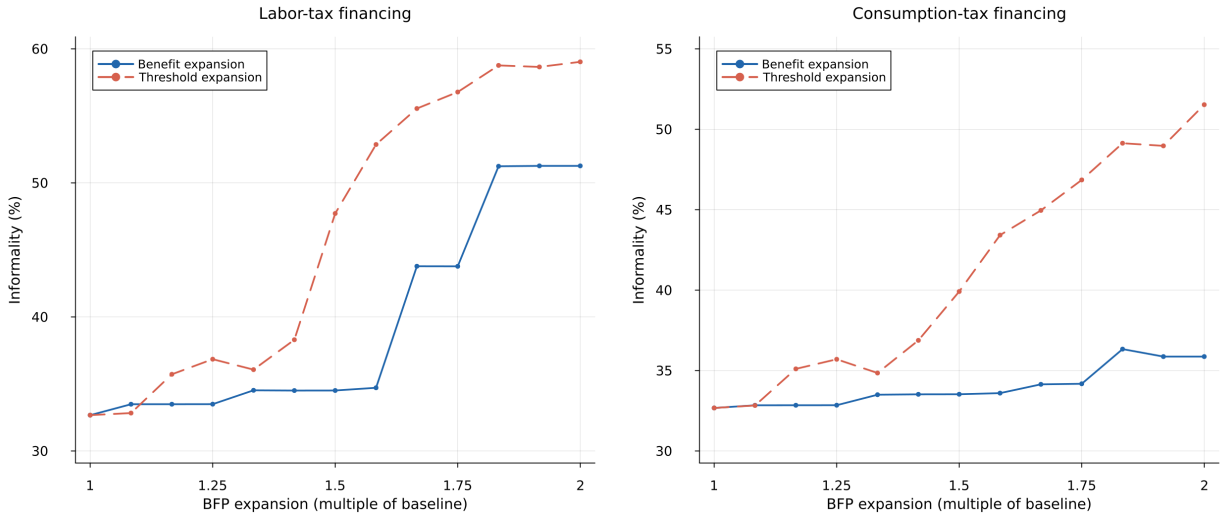


Figure 7: *Bolsa Família* expansions and aggregate informality

Notes: Each point uses the reform's stationary distribution. Benefit expansions scale both payments; threshold expansions scale only the income cutoff. Government consumption is fixed. The left panel adjusts progressive labor-income taxes, and the right panel adjusts consumption taxes.

These patterns offer a possible explanation for the mixed empirical findings on BFP and informality. The response depends jointly on financing, whether the reform changes payments or eligibility, and the size of the expansion. Several benefit increases leave informality nearly unchanged, while some threshold increases reduce it relative to a smaller expansion, even though larger expansions raise it relative to the baseline. Studies of different reforms may therefore find different responses without contradicting one another. The model does not establish why particular empirical estimates differ, but shows why BFP expansions need not produce the same informality response.

To investigate strategic informality, we first distinguish changed household choices from changes in the population's composition. We compare baseline and reform choices at the same assets,

human capital, and life-cycle stage, then account for the reform's stationary distribution. Under consumption-tax financing, changed choices at baseline states raise informality by 15.98 percentage points when the threshold doubles, but reduce it by 0.50 points when benefits double. Thus, the threshold response mainly reflects different choices, whereas the aggregate benefit response mainly reflects changes in the distribution. Appendix H.1 reports the decomposition. We focus on consumption-tax financing below to avoid combining BFP responses with changes in labor-income taxation.

The two reforms reach different households. Larger benefits make eligibility more valuable without changing the income cutoff, mainly increasing payments to existing recipients. A higher threshold also admits previous nonrecipients at their original income and can make eligibility attainable for others through changes in sector or hours. Table 6 separates new recipients who pass the income test through the higher cutoff, changes in prices, or changes in their choices, and reports hours adjustments among initial recipients.

Table 6: BFP receipt under doubled benefits and threshold

	Benefits doubled	Threshold doubled
Panel A: New recipients		
Qualify through the cutoff alone	0.00	4.53
Qualify after also updating prices	0.00	0.23
Require sector or hours changes	0.16	25.59
Total new recipients	0.16	30.35
Panel B: Initial recipients		
Retain BFP and reduce hours	2.49	< 0.01
Retain BFP and keep or increase hours	15.58	18.79
Retain BFP with zero hours	0.44	0.00
Lose BFP	0.28	0.00
Total initial recipients	18.79	18.79

Notes: All entries are percentages of baseline working-age households. Both reforms use consumption-tax financing and are evaluated at baseline household states. In Panel A, the first test changes only the cutoff, retaining baseline prices, sector, and hours. Among those still ineligible, the second updates prices while retaining sector and hours. The remaining new recipients require sector or hours changes. Categories are mutually exclusive and sum to total new recipients. Panel B partitions initial BFP recipients by reform receipt and hours. Its categories sum to the initial-recipient share. Sums may differ because of rounding.

When the threshold doubles, the higher cutoff alone admits 4.53% of baseline working-age households, while 25.59% require changes in sector or hours to qualify. All households in the latter group move from formal to informal employment and their original income exceeded even the expanded threshold. By contrast, doubling benefits brings only 0.16% of baseline households into receipt. The larger threshold response therefore reflects substantial movement into informality among previous nonrecipients, not simply eligibility created mechanically by the higher cutoff.

Hours provide another way to adjust observed income. As in the simple model, households weigh the productivity loss from informality against the earnings and labor-disutility consequences of reducing hours. When benefits double, 2.49% of baseline working-age households reduce hours worked, remain informal, and retain BFP. For about 65% of these households, keeping their original hours at reform prices would make them ineligible, because the higher equilibrium interest rate raises the asset income included in the income test. Under the doubled threshold, all initial recipients retain BFP and virtually all keep or increase their hours. Appendix H.1 details these eligibility checks and cases in which sector and hours change together.

These comparisons provide model-based evidence consistent with strategic informality: many new recipients under the threshold expansion obtain access through informal employment, while some existing recipients respond to larger benefits through hours adjustments.

8.2 Transfer regimes, macroeconomic outcomes, and welfare

We next compare the baseline CCT with three alternatives: no BFP; a means-tested transfer with the same benefits and income threshold but no schooling condition; and a budget-matched UBI. Under no BFP, transfers are eliminated and fiscal savings allow a reduction in consumption taxation. Removing the schooling condition allows eligible families to receive transfers regardless of attendance. The UBI pays the same amount to each working or retired household, calculated by dividing baseline CCT spending by the total mass of households and keeping that payment fixed.

We run each comparison in the calibrated economy with informality and in a formal-only economy with its own CCT reference equilibrium. These comparisons evaluate policies within each environment, not the welfare gain from eliminating informality. Reforms are unexpected and permanent. We compute the new stationary equilibrium and the transition from the baseline, with consumption taxes balancing the budget throughout. Other tax rules and government consumption remain fixed at baseline levels.

Table 7: Macroeconomic effects of alternative transfer regimes

	With informality			Formal only		
Outcome	No BFP	Means-tested	UBI	No BFP	Means-tested	UBI
Market output	1.65	-0.06	1.57	0.58	-0.13	0.55
Capital stock	3.47	0.03	3.44	1.19	-0.18	1.14
Mean adult human capital	-0.04	-0.08	-0.05	-0.02	-0.07	-0.01
Informality rate (pp)	-13.03	0.10	-13.04	0.00	0.00	0.00
School attendance (pp)	-1.65	-2.93	-2.64	0.00	-1.51	-0.01
Child labor (pp)	-2.63	-2.10	-3.29	0.01	-0.97	0.11

Notes: Changes relative to the CCT stationary equilibrium in each environment. Entries are percentage changes except rows marked pp, which are percentage-point changes. Means-tested retains the baseline benefits and income test but removes the schooling condition. Consumption taxes adjust, while other tax rules and government consumption remain fixed.

Table 7 summarizes the stationary effects. Removing BFP or replacing it with UBI reduces informality by approximately 13 percentage points and raises output by 1.65% and 1.57%, respectively. Both reforms remove the incentive to hide income to obtain transfers. Movement toward formality reduces the associated productivity loss, while higher saving increases capital. Output gains are smaller in the formal-only economy, illustrating how employment choices affect the macroeconomic evaluation of transfer policies. Appendix H.2 reports the complete macroeconomic results.

These production gains do not by themselves establish whether households are better off. We measure welfare using consumption-equivalent variation (CEV), reporting effects for households alive at reform, including retirees, and for stage-1 entrants in the new stationary equilibrium. We also report a discounted utilitarian welfare measure, aggregating household utility for the initial

population and future entering cohorts, with the latter discounted using β. All measures include parental altruism. Appendix H.2 specifies the cohort coverage and welfare calculation.

Table 8: Welfare under alternative transfer regimes

Alternative to CCT	Alive at reform (%)	Long-run entrant (%)	Discounted aggregate (%)
Panel A: Economy with informality			
No Bolsa Família	-1.31	0.74	-0.72
Means-tested, no schooling condition	0.10	0.14	0.12
Budget-matched UBI	-0.93	1.05	-0.35
Panel B: Formal-only economy			
No Bolsa Família	-0.69	0.24	-0.41
Means-tested, no schooling condition	0.05	-0.05	0.03
Budget-matched UBI	-0.57	0.34	-0.29

Notes: Consumption-equivalent changes relative to the CCT in each environment. Alive at reform includes all three working stages and retirees. Long-run entrant refers to the stage-1 cohort entering the new stationary equilibrium. The discounted aggregate combines households alive at reform with cohorts entering through year 104, discounting future cohorts using the factor β. Each cohort's welfare includes its full remaining lifetime and parental altruism.

Table 8 shows that no BFP and UBI generate long-run gains but initial losses. Existing recipients lose their transfers under no BFP or receive smaller payments under UBI, while later cohorts benefit from the adjusted capital stock, prices, and taxes. With informality, discounted aggregate welfare falls by 0.72% under no BFP and 0.35% under UBI. Distributional results in Appendix H.3 show that initial losses are concentrated among low earners and BFP recipients. Thus, removing the incentives created by income targeting improves some long-run aggregates but imposes losses that remain important under discounted welfare.

Removing only the schooling condition produces a different response. With informality, attendance falls by 2.93 percentage points and child labor by 2.10 points. Comparing choices at the same baseline parent states, 48.53% of families initially combining school and work choose neither activity under the reform. The corresponding share is 53.17% in the formal-only economy. These are low-earning families that almost all retain BFP, keep parental hours unchanged, and save more. Appendix H.2 provides the household comparisons.

Avoiding schooling costs while retaining BFP may reduce the value of the current resources provided by child labor. Those resources cover only about 6% of schooling costs, so stopping both activities releases resources despite the loss of child earnings. The human-capital technology offers another possible explanation: child labor causes a larger proportional productivity loss without schooling. Our comparisons do not separate the contributions of these mechanisms, but establish that many of the same families stop both activities. Removing a schooling requirement therefore need not increase child labor.

The ability to avoid schooling costs generates small welfare gains for current households in both environments, but the long-run effects differ. Removing the condition raises long-run entrant welfare by 0.14% with informality and lowers it by 0.05% in the formal-only economy. The discounted aggregates are small gains of 0.12% and 0.03%, respectively. Informality changes access to BFP and therefore which families incur schooling costs in order to receive the benefits, affecting the balance between current savings and future human-capital returns.

The formal-only pattern is qualitatively consistent with Ferreira et al. (2025): their means-tested policy without the schooling requirement produces larger gains for households alive at implementation than their CCT, but worse outcomes for future generations as education declines. Their policies start from a common economy without transfers and are matched on initial spending, whereas we reform an existing CCT, so the magnitudes are not directly comparable. Our reversal of the long-run ranking when informality is available suggests that accounting for this employment margin can change the welfare evaluation of schooling requirements as well as income targeting.

9 Conclusion

We study how informality and child labor affect the welfare and macroeconomic consequences of conditional cash transfers. Brazilian household data suggest bunching of informal households below the *Bolsa Família* eligibility threshold and a close relationship between child labor and school attendance. We incorporate these decisions into an overlapping-generations model calibrated to

Brazil to examine policy expansions and alternative transfer regimes.

The effect of BFP expansions on informality depends jointly on their design, size, and source of financing. Large eligibility expansions generate stronger increases in informality than benefit expansions, while labor-tax financing amplifies the responses to both reforms. A higher cutoff allows some households to qualify without changing their behavior, while others obtain benefits through changes in employment and hours. Benefit expansions mainly increase transfers to existing recipients, some of whom reduce hours while retaining benefits. These different responses offer a possible explanation for the mixed empirical findings on BFP and informality.

Under consumption-tax financing, removing BFP or replacing it with a budget-matched UBI reduces informality and raises long-run output in the economy with informality. However, both reforms reduce welfare for households alive at implementation, with losses concentrated among low earners and existing recipients. Although the long-run entering cohort gains, these gains do not offset the earlier losses under our discounted welfare measure. Removing the incentives created by income targeting therefore comes at a cost to households that rely on targeted transfers.

Removing only the schooling condition produces smaller changes. School attendance and child labor both fall. Our household comparisons show that many families initially combining school and work stop both activities, retain BFP, and save more. Under our calibration, this reform generates small welfare gains for current households and long-run entrants when informality is available. In the formal-only economy, current households gain but long-run entrants lose. This difference suggests that informality changes the balance between schooling costs and future human-capital returns. Transfer policies should therefore be evaluated together with their financing and their effects across households and generations. Informality, which is widespread in many economies with CCT programs, also matters because it affects access to transfers and households' responses to policy changes.

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Appendix

A Evolution of Bolsa Família eligibility and benefits

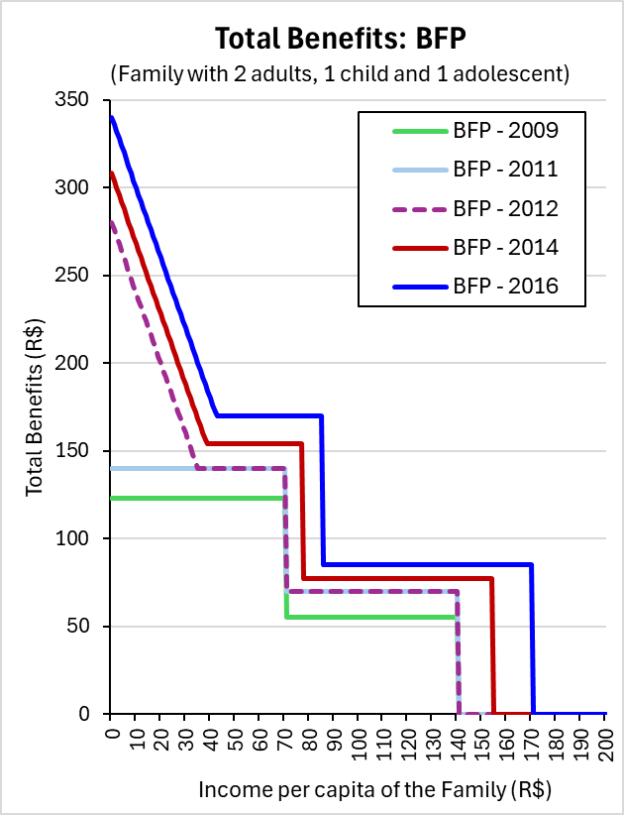
In addition to increases in the thresholds and benefit amounts, the program introduced a separate benefit for adolescents and a complement designed to overcome extreme poverty. The adolescent benefit extended variable payments to families with children aged 16 or 17. The extreme-poverty complement covered the remaining income gap after accounting for the basic and variable benefits, with its amount depending on family size. Consequently, families with lower income received larger complements.

These institutional details imply that Bolsa Família generates a nonlinear budget set. To illustrate this point, consider a family with two adults, one child aged 15 or younger, and one adolescent aged 16 or 17. Before the introduction of the benefit to overcome extreme poverty, the transfer schedule was approximately a step function. For example, under the benefit amounts introduced in 2009, a family below the extreme-poverty line received the basic benefit plus the child and adolescent variable benefits, totaling R$123 (US$61.57). A family with income per capita between the extreme-poverty and poverty lines did not receive the basic benefit, but still received the child and adolescent benefits, totaling R$55 (US$27.53). Once income per capita exceeded the poverty line, the family became ineligible and received no transfer. Under the benefit amounts introduced in 2011, the same family would receive R$140 (US$83.72) below the extreme-poverty line, R$70 (US$41.86) between the extreme-poverty and poverty lines, and zero above the poverty line.

The introduction of the variable benefit to overcome extreme poverty changed the shape of the schedule at the bottom of the income distribution. For the same family of four, the rules in place at the end of 2012 provided a complement when family income, including the other BFP benefits, remained below R$70 (US$35.83) per capita. Abstracting from statutory rounding, a family with zero pre-transfer income would therefore receive approximately R$280 (US$143.33) in total

benefits. As pre-transfer income increased, the additional benefit declined until the fixed benefits alone were enough to bring income to the extreme-poverty line. The same logic applies to later years. In 2014, when the extreme-poverty line increased to R$77 (US$32.75), a family with zero pre-transfer income would receive approximately R$308 (US$131.00). Following the increase to R$85 (US$24.40) in 2016, this amount reached approximately R$340 (US$97.59). Hence, the program combines discrete eligibility thresholds with income-dependent transfers, creating incentives that may affect both labor supply and reported income. Figure 8 illustrates these schedules for selected years.

Figure 8: *Bolsa Família* benefits for a family with two adults, one child, and one adolescent



B Strategic Informality Across Alternative Bandwidths

We re-estimate the empirical income densities relative to the *Bolsa Família* Program (BFP) eligibility threshold using alternative bandwidths of R$10 (Figure 9) and R$25 (Figure 10). Both specifications yield a consistent pattern: a substantial density of informal workers reports an income just below the qualifying threshold, whereas the eligibility boundary exerts no observable effect on the reported income of formal workers. This finding is consistent with strategic informality, wherein informal sector workers underreport income to qualify for the conditional cash transfer, since the government cannot directly observe their income. As expected, narrowing the bandwidth to R$10 increases data noise, a standard feature in survey data where self-reported income is subject to rounding and heaping at salient values. Reducing the bandwidth to R$25 reduces the variation but makes the increase before the threshold seem less relevant. Nonetheless, this bar is still the bandwidth with the largest number of represented households.

Figure 9: Strategic Informality Around the *BFP* Qualifying Threshold **(Bandwidth: R$10)**

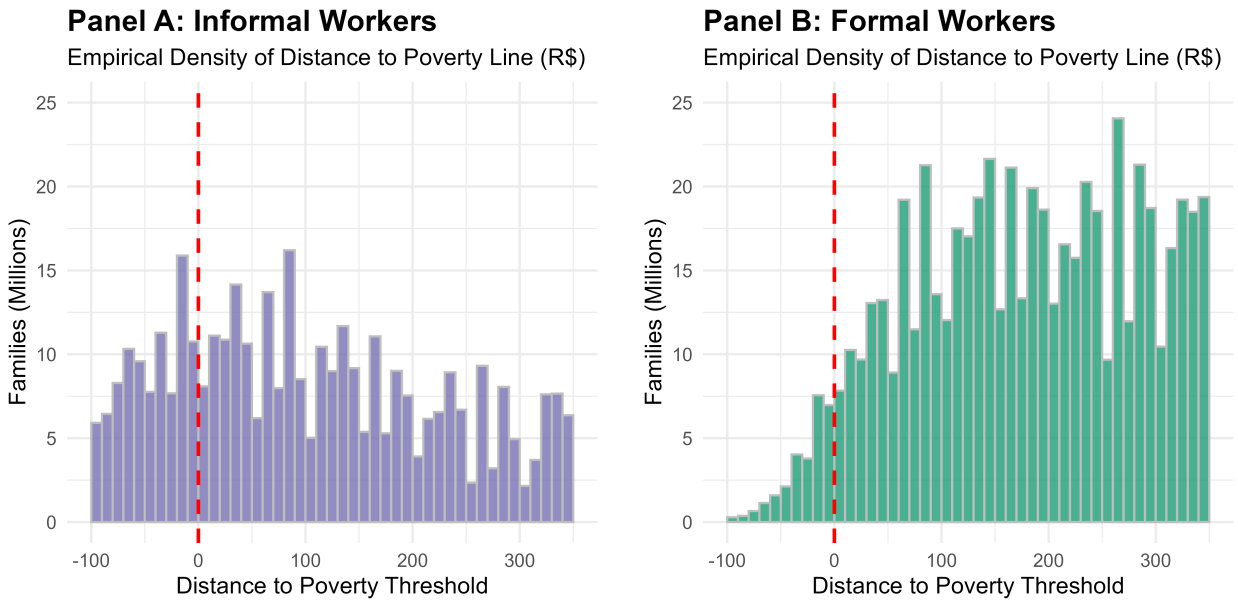
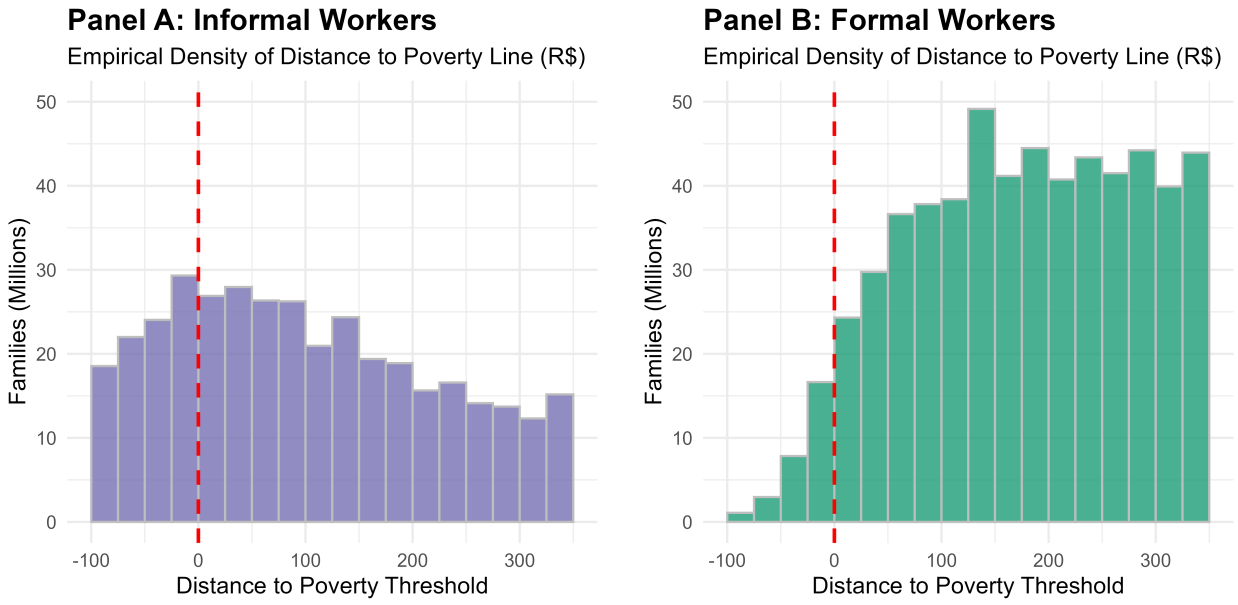


Figure 10: Strategic Informality Around the *BFP* Qualifying Threshold **(Bandwidth: R$25)**



C Main Result Tables of the RDD with "Donut-Hole"

Table 9: Donut-Hole RDD: *Bolsa Família* Eligibility and Family Informality

	Dep. Variable: Probability of Informality
RD Effect (Conventional)	-0.071***
	[40.302]
Robust Inference	
95% Bias-Corrected C.I.	[0.277, 0.306]***
z-statistic	40.302
p-value	0.000
Bandwidths (h)	
Left Window (Eligible)	100.00
Right Window (Ineligible)	200.00
Observations	
Effective Obs. (Left / Right)	194,886 / 909,179
Total Obs. (Left / Right)	338,672 / 1,577,363
Kernel Type	Uniform
VCE Method	HC1 (Robust)
Donut-Hole Adjustment	Yes (±R$20)

Notes: Local polynomial Sharp Regression Discontinuity estimates. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (based on the robust p-value).

Table 10: Donut-Hole RDD: *Bolsa Família* Eligibility and Hours Worked by Sector

	Informal Workers	**Formal Workers**
Dep. Variable:	Weekly Hours p.c.	
RD Effect (Conventional)	2.715***	-0.007
Robust Inference		
95% Bias-Corrected C.I.	[-3.408, -2.213]***	[-0.396, 0.395]
z-statistic	-9.218	-0.003
p-value	0.000	0.997
Bandwidths (h)		
Left Window (Eligible)	100.00	100.00
Right Window (Ineligible)	200.00	200.00
Observations		
Effective Obs. (Left / Right)	35,294 / 555,849	159,592 / 353,330
Total Obs. (Left / Right)	75,077 / 1,052,635	263,595 / 524,728
Kernel Type	Uniform	Uniform
VCE Method	HC1 (Robust)	HC1 (Robust)
Donut-Hole Adjustment	Yes ($\pm$R\$20)	Yes ($\pm$R\$20)

Notes: Local polynomial Sharp Regression Discontinuity estimates. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (based on the robust p-value).

D Alternative Specifications of the RDD with "Donut-Hole"

To evaluate the sensitivity of our baseline regression discontinuity design (RDD), Table 11 reports robustness checks across alternative donut-hole windows (±R$30, ±R$10, and ±R$0) and informality definitions. Columns 1–3 employ our definition of informality that assigns an informal label to a household if any of its members is employed in the informal sector ("Family Informality"). Columns 4–6 examine "Individual Informality", which relies strictly on the survey respondent's employment status regardless of other household members.

Across all specifications, crossing the Bolsa Família Program (BFP) eligibility threshold yields a statistically significant reduction in the probability of informality. Although the magnitude of this drop increases under wider exclusion zones (±R$30), overly large donut holes risk comparing fundamentally distinct populations—namely, extremely low-income households reliant on informal work versus higher-income households with structural access to formal jobs. Conversely, retaining all observations near the threshold (±R$0) still produces a significant 6.6 percentage point decline. However, as noted in the main text, estimates without a donut hole remain vulnerable to local sorting and income underreporting.

Table 12 presents corresponding robustness estimates for per capita weekly hours worked, disaggregated by sector. The primary findings remain robust: informal workers exhibit a statistically significant increase in per capita weekly hours worked upon crossing the eligibility cutoff, ranging from 0.8 hours with no exclusion window to 4.3 hours under a ±R$30 window. In contrast, formal workers display no meaningful response, exhibiting a negligible shift (0.03 hours) only when the donut hole is completely removed. This sectoral asymmetry aligns with institutional features of the labor market, as informal workers enjoy schedule flexibility, whereas formal employees face legally binding employment contracts and rigid work hours.

Collectively, these findings reinforce the "strategic informality" hypothesis: informal workers actively adjust their labor supply along both the extensive (sectoral choice) and intensive (hours

worked) margins to preserve BFP eligibility. Nevertheless, these estimates capture localized discontinuities rather than pure causal treatment effects. Because households can endogenously select into informality and manipulate reported income around the legally established cutoff, these results should be interpreted as empirical suggestive evidence of strategic behavioral responses rather than exogenous program impacts.

Table 11: Robustness Check: Varying Donut-Hole Sizes for Probability of Informality

	Family Informality			**Individual Informality**		
	±R$30	**±R$10**	**±R$0 (None)**	**±R$30**	**±R$10**	**±R$0 (None)**
Dep. Variable: Probability of Informality						
RD Effect	-0.153***	-0.079***	-0.066***	-0.126***	-0.063***	-0.054***
Robust Inference						
95% Bias-Cor. C.I.	[0.320, 0.364]	[0.071, 0.088]	[0.021, 0.032]	[0.325, 0.368]	[0.072, 0.088]	[0.021, 0.032]
z-statistic	30.374	18.866	8.952	31.526	19.265	9.030
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Bandwidths (h)						
Left Window	100.00	100.00	100.00	100.00	100.00	100.00
Right Window	200.00	200.00	200.00	200.00	200.00	200.00
Effective Obs.						
Left (Eligible)	165,546	246,361	281,222	165,546	246,361	281,222
Right (Ineligible)	839,430	950,309	985,744	839,430	950,309	985,744
Kernel Type	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
VCE Method	HC1	HC1	HC1	HC1	HC1	HC1

Notes: Local polynomial Sharp Regression Discontinuity estimates. The robust inference 95% confidence intervals are bias-corrected. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (based on the robust p-value).

Table 12: Robustness Check: Varying Donut-Hole Sizes by Employment Sector

	Informal Workers			Formal Workers		
	±R$30	**±R$10**	**±R$0 (None)**	**±R$30**	**±R$10**	**±R$0 (None)**
Dep. Variable: Weekly Hours p.c.						
RD Effect	4.300***	1.523***	0.850***	0.099	-0.243	0.031***
Robust Inference						
95% Bias-Cor. C.I.	[-5.193, -2.978]	[-1.335, -0.740]	[-0.748, -0.397]	[-0.345, 0.931]	[-0.208, 0.262]	[0.460, 0.781]
z-statistic	-7.231	-6.832	-6.408	0.900	0.226	7.590
p-value	0.000	0.000	0.000	0.368	0.821	0.000
Bandwidths (h)						
Left Window	100.00	100.00	100.00	100.00	100.00	100.00
Right Window	200.00	200.00	200.00	200.00	200.00	200.00
Effective Obs.						
Left (Eligible)	25,711	52,320	66,110	139,835	194,041	215,112
Right (Ineligible)	527,389	578,482	593,978	312,041	371,827	391,766
Kernel Type	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
VCE Method	HC1	HC1	HC1	HC1	HC1	HC1

Notes: Local polynomial Sharp Regression Discontinuity estimates. The robust inference 95% confidence intervals are bias-corrected, which may center differently from the conventional point estimates. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (based on the robust p-value).

E Results of Bivariate Probit

Table 13: Baseline Bivariate Probit: School Dropout and ILO Child Labor (Ages 7-17)

Variables	**(1) School Dropout** *Coefficient (Std. Error)*	**(2) ILO Child Labor** *Coefficient (Std. Error)*
Bolsa Família (1=Yes)	0.623***	0.094***
	(0.002)	(0.002)
Log(Real Income p.c. + 1)	-0.073***	0.028***
	(0.000)	(0.000)
Age	0.255***	0.186***
	(0.000)	(0.000)
Female (1=Yes)	-0.001***	-0.331***
	(0.000)	(0.000)
Rural (1=Yes)	0.094***	0.744***
	(0.001)	(0.000)
Year Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Error Correlation (ρ)	**0.410*** (0.001)**	
Log-likelihood	-57,923,305	
Observations (Weighted)	Approx. 1.25 Million	

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Standard errors rounded to nearest thousandth; values smaller than 0.001 displayed as (0.000).

F Period length and annual accounting

Working households make decisions once every 13 years. Consumption, labor supply, sector, and childhood choices are constant within each working stage. Annual accounting determines the utility and asset accumulation associated with these choices, without introducing additional annual decisions during working life.

Utility normalization. Let W_j denote lifetime utility obtained by adding discounted annual utilities, before normalization. Define

$$S_{13} = \sum_{n=0}^{12} \beta_a^n = \frac{1-\beta}{1-\beta_a}, \qquad \beta = \beta_a^{13}.$$

With constant consumption and labor supply, utility within a working stage is $S_{13}u(c,l)$. The value functions in the main text are $V_j = W_j/S_{13}$. Therefore,

$$W_j = \max\left\{S_{13}u(c,l) + \beta\mathbb{E}W_{\text{next}}\right\} \quad \Longleftrightarrow \quad V_j = \max\left\{u(c,l) + \beta\mathbb{E}V_{\text{next}}\right\}.$$

The same normalization applies to the parent and child values in the inter vivos transfer problem and to the retirement continuation value. This explains the factor $1/S_{13} = (1-\beta_a)/(1-\beta)$ in the retirement objective. Dividing all values by this common positive constant leaves household choices and consumption-equivalent welfare comparisons unchanged. It does not remove annual discounting within a stage.

Working-stage budgets. Let q denote annual household resources after labor taxes, including transfers and, in stage 2, child earnings net of schooling costs. The implied annual asset path satisfies

$$a_{n+1} = [1 + (1-\tau_a)r]a_n + q - (1+\tau_c)c, \qquad n = 0, \ldots, 12.$$

Here, a_0 is the initial asset position, net of the inter vivos transfer in stage 3, and $a_{13} = a'$. Iterating gives

$$a' = R_{13}a_0 + F_{13}\big[q - (1+\tau_c)c\big],$$

where

$$R_{13} = [1 + (1-\tau_a)r]^{13}, \qquad F_{13} = \sum_{n=0}^{12}[1 + (1-\tau_a)r]^n.$$

Thus, annual earnings, taxes, transfers, consumption expenditure, schooling costs, and child earnings enter the working-stage budget through F_{13}. Total consumption over the stage is $13c$, whereas $F_{13}c$ is its accumulated financial value. The latter is not the consumption argument in utility.

Policy units and retirement. Earnings, tax brackets, contribution caps, benefits, and eligibility thresholds are expressed in consistent annual units. Assets are stocks and require no conversion for period length.

BFP eligibility uses annual observed labor income and asset income evaluated at stage entry, net of the inter vivos transfer in stage 3. The resulting benefit remains constant within the stage. Assets carried into retirement satisfy $a_4 = a'$ and require no conversion. The pension base is annual observed labor income in the final working stage, $\tilde{y}_3^{ann} = \tilde{y}(h, l, s)$.

G Calibration

G.1 Externally calibrated parameters

Table 14 reports the externally calibrated parameters and their sources.

Preferences, production, and heterogeneity. We set the preference curvatures to $\gamma_c = \gamma_\ell = 2$, the capital share to $\alpha = 0.4$, and annual depreciation to $\delta_K = 0.0503$. The last two parameters are calculated from the Penn World Table. We set the share of informal income hidden from the government to $\chi = 0.6$, following Ayres et al. (2025). Intergenerational type persistence is set to

Table 14: Externally calibrated parameters

Non-policy parameters			Policy parameters		
Parameter	Value	Source	Parameter	Value	Source
N^R	19 years	IBGE	τ_a	15.00%	Tax rules
γ_c	2	Standard	τ_c	25.30%	Azevedo and Fasolo (2015)
γ_ℓ	2	Standard	τ^{pen}	9.00%	Pension rules
ρ_ε	0.50	Britto et al. (2026)	ν	87.00%	Ferreira et al. (2025)
α_1	0.1477	PNADC 2016	$\bar{y}^{pen}$	R$5,189.82	Pension rules
α_2	−0.0168	PNADC 2016	y^{max}	R$5,189.82	Pension rules
α_3	0.0098	PNADC 2016	y^{min}	R$880.00	Pension rules
κ	0.1024	POF 2017–2018	ω_1	R$175.06	Administrative data
χ	0.60	Ayres et al. (2025)	ω_0	R$85.00	BFP rules
α	0.40	PWT	$\bar{y}$	R$170.00	BFP rules
δ_K	5.03%	PWT			

Notes: Monetary policy values are expressed in 2016 reais per month. The eligibility threshold $\bar{y}$ is reported per person and converted into a household threshold as described in the text. The schooling cost κ is expressed in annual-minimum-wage units. Depreciation is annual.

$\rho_\varepsilon = 0.5$, using the Brazilian intergenerational income-elasticity estimate of Britto et al. (2026).

Schooling costs. We measure κ using the 2017–2018 Household Budget Survey (POF). The expenditure basket includes transportation, books, uniforms, enrollment fees, school materials, and regular-school tuition. We annualize expenditures, convert them to December 2016 prices, and divide weighted total spending by the weighted number of children attending school. This yields $\kappa = 0.1024$ in model units. Tuition is included because the measure captures realized spending across public- and private-school users.

Policy parameters. We set the capital-income tax rate to $\tau_a = 0.1500$ and the consumption-tax rate to $\tau_c = 0.2530$. The latter is an effective tax estimate from Azevedo and Fasolo (2015), which we use as a benchmark rather than a direct estimate for 2016.

Total taxes on observed labor income combine pension contributions and progressive income tax-

ation:

$$T_y(\tilde{y}) = \tau^{pen} \min\{\tilde{y}, \bar{y}^{pen}\} + \bar{T}_y(\tilde{y}),$$

where τ^{pen} is the pension contribution rate, $\bar{y}^{pen}$ is the contribution cap, and $\bar{T}_y$ follows the 2016 statutory income-tax brackets. We abstract from income-tax deductions. Appendix G.2 reports the complete schedule.

Pension bounds and BFP parameters follow the institutional rules and administrative data reported in Table 14. We convert the program's per-capita poverty line into a household threshold using a representative beneficiary family size of approximately four people. The benefit for households with dependent children is based on the administrative average per beneficiary family, while eligible households outside that stage receive the basic benefit.

Human-capital coefficients. We estimate α_1, α_2, and α_3 using two survey-weighted regressions from the 2016 annual PNADC. For adults aged 18–56, we estimate the association between schooling and log hourly earnings. For children aged 5–16, we estimate the association between completed schooling, attendance, child labor, and their interaction:

$$\log w_i = \varrho S_i + X_i'\Gamma + \mu_s + u_i,$$
$$S_i = d_1 HC_i + d_2 CL_i + d_3(HC_i \times CL_i) + Z_i'\Pi + \mu_s + \lambda_a + v_i.$$

Here, S_i is completed years of schooling, HC_i indicates school attendance, and child labor CL_i includes economic activity and production for own consumption. Both regressions control for gender, race, urban or rural residence, and state fixed effects μ_s. The adult regression includes age and age squared, while the child regression includes age fixed effects λ_a. Thus, the child coefficients compare schooling progression among children of the same age. We exclude age 17 from the child regression because nonattendance may reflect completed secondary education rather than a lack of schooling investment.

We map these estimates into log-human-capital units as $\alpha_j = \varrho d_j$, for $j = 1, 2, 3$, obtaining

$$\alpha_1 = 0.1477, \qquad \alpha_2 = -0.0168, \qquad \alpha_3 = 0.0098.$$

These are associations, not causal estimates. Child labor is associated with lower human capital among both nonattending and attending children, since $\alpha_2 < 0$ and $\alpha_2 + \alpha_3 < 0$. The positive interaction coefficient implies a smaller proportional human-capital penalty from child labor when the child attends school. The child moments targeted in the calibration retain the model's full age range of 5–17.

G.2 Labor-income tax schedule

We report the tax schedule in monthly 2016 reais. Pension contributions are proportional to observed labor income up to the contribution cap, with $\tau^{pen} = 0.09$ and $\bar{y}^{pen} = \text{R\$}5{,}189.82$.

The progressive income-tax component uses the following bracket thresholds, rounded to whole reais:

$$y_1 = 1904, \qquad y_2 = 2827, \qquad y_3 = 3751, \qquad y_4 = 4665.$$

For observed labor income $\tilde{y} \geq 0$, the schedule is

$$\bar{T}_y(\tilde{y}) = \begin{cases} 0, & \tilde{y} \leq y_1, \\ 0.0750(\tilde{y} - y_1), & y_1 < \tilde{y} \leq y_2, \\ 0.0750(y_2 - y_1) + 0.1500(\tilde{y} - y_2), & y_2 < \tilde{y} \leq y_3, \\ 0.0750(y_2 - y_1) + 0.1500(y_3 - y_2) + 0.2250(\tilde{y} - y_3), & y_3 < \tilde{y} \leq y_4, \\ 0.0750(y_2 - y_1) + 0.1500(y_3 - y_2) + 0.2250(y_4 - y_3) + 0.2750(\tilde{y} - y_4), & \tilde{y} > y_4. \end{cases}$$

Each marginal rate applies only to income within its bracket. We apply this schedule directly to

observed labor income, without deductions.

G.3 SMM estimates and target construction

Table 15 reports the eight parameters jointly estimated by SMM using nine moments.

Table 15: Estimated Parameters

Parameter	Description	Estimated value
β_a	Annual discount factor	0.9814
μ	Weight on the disutility of labor	21.2417
δ	Parental altruism toward the child	0.9492
A	Aggregate total factor productivity	2.0363
ψ_L	Informality productivity cost at low human capital	0.0041
ψ_H	Informality productivity cost at high human capital	0.1876
σ_ν	Innovation standard deviation in the intergenerational human-capital process	0.8343
ϕ	Household resource gain from child labor	0.0060

Adult moments. The capital-output ratio comes from the Penn World Table. The remaining targets use the 2016 annual PNADC. Adult labor supply is weekly hours divided by a weekly non-sleep time endowment of 112 hours, corresponding to 16 hours per day. Earnings are expressed in annual-minimum-wage units.

For the education gap in informality, high education in the data means completed high school or more. In the model, we rank workers by human capital and choose the cutoff so that the high-human-capital group has the same population share as high-education workers in the PNADC, approximately 59.7%. The low-minus-high informality gap therefore compares groups of the same relative size.

Child moments. We target school nonattendance, child labor, and joint school attendance and work among children aged 5–17. Child labor includes economic activity and production for own consumption.

With the schooling cost κ and the three human-capital coefficients fixed externally, these moments

jointly discipline altruism δ and child-produced resources ϕ. Altruism affects the value of the child's future human capital, while ϕ affects the current return to child labor. The joint school-and-work rate adds a restriction on how these incentives interact. We estimate ϕ internally because the model's resource gain does not correspond directly to a single observed category of child remuneration.

G.4 Residual earnings dispersion

We compare residual earnings dispersion using weighted regressions of log annual earnings. PNADC controls include schooling, age, age squared, gender, race, urban status, and state fixed effects. Model controls are the high-human-capital indicator and working-stage fixed effects. The weighted standard deviations of the residuals are 0.6776 in the model and 0.6779 in the data. Because the controls differ, this comparison is an auxiliary diagnostic. We target unconditional earnings dispersion in the SMM because it has the same definition in the data and model.

H Counterfactual Analyses

H.1 Additional results on BFP expansions and informality

Let $I(D, s)$ denote informality given distribution D and sector and hours choices s. Superscripts B and R denote the baseline and reform. The decomposition used in the main text is

$$I(D^R, s^R) - I(D^B, s^B) = \left[I(D^B, s^R) - I(D^B, s^B)\right] + \left[I(D^R, s^R) - I(D^B, s^R)\right].$$

The first term compares baseline and reform choices at the baseline distribution. Reform choices reflect the new stationary prices, taxes, benefit amounts, eligibility threshold, and continuation values. The second term replaces the baseline distribution with the reform distribution while retaining the reform choices at each state.

Table 16: Informality and sector changes: consumption-tax financing

	Benefit expansion		Threshold expansion	
	1.5×	2×	1.5×	2×
Panel A: Informality changes (pp)				
Total stationary change	0.85	3.20	7.24	18.86
Fixed-state behavioral component	0.05	-0.50	-0.87	15.98
Distributional component	0.80	3.69	8.11	2.88
Panel B: Sector switches at baseline states (%)				
Formal to informal	0.02	0.16	5.72	25.59
Informal to formal	0.00	0.25	5.87	9.65

Notes: Panel A reports percentage-point changes relative to the baseline informality rate of 32.67%. The fixed-state component compares baseline and reform choices using the baseline distribution. The distributional component captures the additional change from using the reform's stationary distribution. Panel B reports sector switches as percentages of all baseline working-age households, including all three working stages. These switches require positive hours before and after the reform. Panel A also includes movements into and out of employment, which are not displayed in Panel B. Differences in sums reflect rounding.

Table 16 reports this decomposition and sector switches under consumption-tax financing. The comparisons include all three working stages. Sector switches require positive hours before and after the reform. Movements into and out of employment are included in the aggregate decomposition but not in the displayed sector-switching rows.

For the eligibility classification in Table 6, we consider baseline nonrecipients who receive BFP under reform choices. We first change the cutoff while retaining baseline prices and choices. Among those still failing the income test, we update prices while retaining baseline sector and hours. Those who remain ineligible require sector or hours changes. This ordering gives mutually exclusive categories; it is an accounting classification, not an order-independent causal decomposition. Actual receipt also satisfies the schooling condition whenever it applies.

Table 17 examines the last group. At reform prices, we test the reform sector with baseline hours and the baseline sector with reform hours. "Both required" means that neither change alone passes the income test, while their combination does. These tests do not rule out eligibility at other, untested hours choices.

Table 17: Sector and hours changes that create income eligibility

Multiplier	Sector alone sufficient	Hours alone sufficient	Both required	Total
Panel A: Benefit expansions				
$1.25\times$	0.00	0.00	0.00	0.00
$1.50\times$	0.00	0.00	0.02	0.02
$1.75\times$	0.00	0.00	0.15	0.15
$2.00\times$	0.00	0.00	0.16	0.16
Panel B: Threshold expansions				
$1.25\times$	0.15	0.00	0.45	0.59
$1.50\times$	5.41	0.75	0.31	6.47
$1.75\times$	9.67	0.00	5.12	14.79
$2.00\times$	24.67	0.00	0.93	25.59

Notes: Entries are percentages of all baseline working-age households under consumption-tax financing. The table includes new BFP recipients who remain income-ineligible without changing sector or hours, after accounting for the new cutoff and prices. Tests use reform prices and baseline household states. Sector alone combines the reform sector with baseline hours; hours alone combines reform hours with the baseline sector. Both required means neither change alone passes the income test. No household in these groups qualifies under both separate tests, so the displayed categories exhaust the total. The hours-only group under the 1.50 threshold expansion consists almost entirely of households leaving employment. Actual receipt satisfies any applicable schooling condition. Differences in sums reflect rounding.

Under doubled benefits, new recipients requiring behavioral changes need both their sector and hours changes to qualify. Under the doubled threshold, changing sector alone is sufficient for most. At the 1.50 threshold multiplier, the hours-only group consists almost entirely of households leaving employment, rather than reducing positive hours.

Table 17 focuses on new recipients. A separate check examines whether hours reductions preserve eligibility among existing recipients. Under doubled benefits, households representing 1.61% of the baseline working-age population reduce positive hours, remain informal, and retain BFP, but would fail the income test at their original hours and reform prices. The higher equilibrium interest rate raises their asset income. This subgroup accounts for approximately 65% of the hours-reducing informal recipients discussed in the main text.

We also compare choices across reform sizes, rather than only against the baseline as in the table. Households representing 0.98% of the baseline working-age population work informally when

the threshold is approximately 1.92 times baseline. At twice the baseline threshold, they become formal, reduce hours by 25%, and retain BFP. At the doubled-threshold prices, formal employment at their previous hours would make them ineligible. Their final hours equal their baseline hours, so a baseline-to-double comparison misses this joint sector–hours adjustment. These comparisons concern alternative stationary reforms, not successive dates along a transition.

Table 18 reports the decomposition under labor-tax financing. When benefits double, only 1.18% of formal-to-informal switchers become new BFP recipients. Most switches therefore do not coincide with gaining benefits, consistent with labor taxation providing an additional incentive to work informally. The labor-tax multiplier reaches 2.87 under doubled benefits and 3.18 under the doubled threshold.

Table 18: Informality and employment changes: labor-tax financing

	Benefit expansion		Threshold expansion	
	$1.5\times$	$2\times$	$1.5\times$	$2\times$
Panel A: Informality changes (pp)				
Total stationary change	1.84	18.60	15.05	26.36
Fixed-state behavioral component	0.69	14.85	6.73	25.19
Distributional component	1.14	3.74	8.32	1.16
Panel B: Employment changes at baseline states (%)				
Formal to informal	0.67	14.82	13.30	34.81
Informal to formal	0.00	0.00	5.87	9.65
Informal to nonemployment	< 0.01	< 0.01	0.73	0.00
Nonemployment to informal	0.03	0.03	0.03	0.03

Notes: Definitions follow Table 16. Panel A reports percentage-point changes, and Panel B reports percentages of all baseline working-age households. A common multiplier adjusts the progressive labor-income tax schedule. Pension contributions, consumption and capital-income tax rates, and the residual government expenditure component remain fixed. Formal and informal employment require positive hours; nonemployment means zero hours. Differences in sums reflect rounding.

H.2 Additional results on transfer regimes

Macroeconomic outcomes. Table 19 reports the complete stationary comparisons. Each reform is evaluated relative to the CCT equilibrium in the corresponding environment. Consumption taxes

adjust, while government consumption and the other tax rules remain fixed.

Table 19: Macroeconomic effects of alternative transfer regimes

	With informality			Formal only		
Outcome	No BFP	Means-tested	UBI	No BFP	Means-tested	UBI
Market output	1.65	-0.06	1.57	0.58	-0.13	0.55
Capital stock	3.47	0.03	3.44	1.19	-0.18	1.14
Mean adult labor supply	0.38	-0.23	0.15	0.96	-0.11	0.87
Effective labor input	0.46	-0.12	0.35	0.18	-0.09	0.17
Mean adult human capital	-0.04	-0.08	-0.05	-0.02	-0.07	-0.01
Aggregate consumption	1.06	-0.06	0.96	0.39	-0.06	0.36
Mean adult earnings	1.53	-0.20	1.41	0.51	-0.09	0.49
Wage per efficiency unit	1.06	-0.08	1.06	0.32	0.00	0.32
Interest rate (pp)	-0.13	0.01	-0.13	-0.04	0.00	-0.04
Informality rate (pp)	-13.03	0.10	-13.04	0.00	0.00	0.00
School attendance (pp)	-1.65	-2.93	-2.64	0.00	-1.51	-0.01
Child labor (pp)	-2.63	-2.10	-3.29	0.01	-0.97	0.11
School only (pp)	1.29	0.00	1.25	0.16	-0.31	0.18
Work only (pp)	0.31	0.84	0.61	0.17	0.23	0.30
School and work (pp)	-2.94	-2.94	-3.89	-0.16	-1.20	-0.19
Neither activity (pp)	1.34	2.09	2.03	-0.18	1.28	-0.29

Notes: Changes relative to the CCT stationary equilibrium in each environment. Entries are percentage changes except rows marked pp, which are percentage-point changes. Means-tested retains the baseline benefits and income test but removes the schooling condition. The consumption tax adjusts; other tax schedules and the residual government expenditure component remain fixed. Effective labor includes hours, human capital, and the informal-sector productivity penalty. Formal-only informality is zero under every regime by construction.

Schooling and child labor. We compare the baseline CCT with the means-tested transfer without its schooling condition, applying each regime's stationary decision rules to the same baseline stage-2 household states. Assets, parental human capital, and child types are held fixed, while prices and continuation values correspond to each regime. These are comparisons of stationary choices, not household responses at the first date of the transition.

Table 20 describes families that initially combine schooling and child labor and then choose neither activity after the reform. Their mean baseline adult earnings are 28.69% of mean parental earnings in the economy with informality and 12.27% in the formal-only economy. Almost all retain BFP,

Table 20: Household responses to removing the schooling condition

	With informality	Formal only
Panel A: Parents initially combining school and work		
Stop both activities (%)	48.53	53.17
Panel B: Among parents who stop both activities (%)		
Receive BFP in the baseline	100.00	99.04
Receive BFP under the reform	99.28	99.04
Keep parental hours unchanged	98.02	99.36
Increase next-stage assets	98.20	98.58

Notes: The reform removes the schooling condition while retaining the baseline benefit schedule and income threshold. Consumption taxes balance the government budget. Comparisons apply each regime's stationary decision rules to the same baseline stage-2 household states. Panel A reports the percentage of parents whose children initially combine schooling and work who choose neither activity under the reform. Panel B reports characteristics of this subgroup.

leave parental hours unchanged, and increase next-stage assets.

Avoiding schooling expenditure releases more resources than the household loses from child labor. In annual model units, the schooling cost is $\kappa = 0.1024$ and child labor generates $\phi = 0.0060$, with $\phi/\kappa \simeq 0.0584$. Most of the resulting budget savings are carried as assets into the next working stage rather than used for higher current consumption.

The human-capital interaction also changes the consequences of child labor. Holding the child's type fixed, its proportional effect on human capital is $\exp(\alpha_2+\alpha_3)-1$ with schooling and $\exp(\alpha_2)-1$ without schooling. The estimated coefficients imply reductions of approximately 0.71% and 1.67%, respectively. These calculations describe mechanisms consistent with the joint decline, but do not measure their separate contributions.

Welfare calculation. For households alive at reform, we evaluate welfare from their baseline states, accounting for the paths of prices and taxes as the economy adjusts to the new stationary equilibrium. This includes all three working stages and retirees. Long-run entrant welfare refers to a stage-1 household entering the reform's stationary equilibrium.

The discounted aggregate includes the initial population and stage-1 cohorts entering in years $13, 26, \ldots, 104$. A cohort entering k model periods after the reform receives discount weight β^k, while the initial population receives weight one. We use 104 years, the transition horizon for the means-tested and UBI reforms, as a common cutoff for including future cohorts. This common cutoff determines which entering cohorts are included, not when their welfare evaluation ends. Each included cohort's welfare covers its full remaining lifetime, including continuation beyond the cutoff. Parental altruism remains in all value functions.

We aggregate utility before calculating a single CEV, rather than averaging household or group-specific CEVs. The compensation changes baseline consumption proportionally while holding baseline labor choices fixed.

H.3 Welfare effects across the earnings distribution

Table 21 reports welfare by earnings quartile and baseline BFP receipt for households in all three working stages. Quartiles use gross adult labor earnings before taxes, transfers, and income hiding. For households alive at reform, groups are defined before the policy change. Long-run results compare corresponding quartiles in each regime's stationary earnings distribution.

Initial losses under no BFP and UBI are concentrated among low earners and BFP recipients. With informality, baseline recipients lose 4.66% under no BFP and 3.96% under UBI. In the long run, the first two working-age earnings quartiles gain under these policies, while the third and fourth lose. Positive aggregate long-run welfare therefore coexists with losses for some earnings groups.

Figure 11 presents the effects for households alive at reform using finer earnings groups.

Table 21: Welfare by earnings group: all working stages

	Alive at reform			Long run		
Group	No BFP	Means-tested	UBI	No BFP	Means-tested	UBI
Panel A: Economy with informality						
All	-1.40	0.10	-1.02	0.88	0.12	1.18
First quartile	-3.66	0.20	-2.99	2.15	0.20	2.51
Second quartile	-0.45	0.06	-0.15	2.31	0.11	2.34
Third quartile	-0.15	0.05	0.10	-1.43	0.08	-0.92
Fourth quartile	0.26	0.04	0.35	-0.32	0.03	-0.08
Baseline BFP recipients	-4.66	0.22	-3.96	–	–	–
Baseline nonrecipients	-0.13	0.06	0.13	–	–	–
Panel B: Formal-only economy						
All	-0.73	0.05	-0.61	0.29	-0.06	0.38
First quartile	-1.87	0.11	-1.67	0.69	-0.66	0.83
Second quartile	-0.18	0.02	-0.09	0.21	0.72	0.27
Third quartile	-0.12	0.02	-0.03	0.01	0.00	0.12
Fourth quartile	-0.02	0.01	0.01	-0.03	-0.02	0.00
Baseline BFP recipients	-4.49	0.25	-4.22	–	–	–
Baseline nonrecipients	-0.26	0.02	-0.16	–	–	–

Notes: Entries are consumption-equivalent percentage changes. The population includes stages 1–3, ages 18–56, and excludes retirees. Quartiles use gross adult labor earnings before taxes, transfers, and income hiding. BFP receipt groups are reported only at implementation, where baseline membership has a direct interpretation. Aggregate CEV is calculated from combined household welfare, rather than by averaging the group-specific CEVs. All policies use consumption-tax financing.

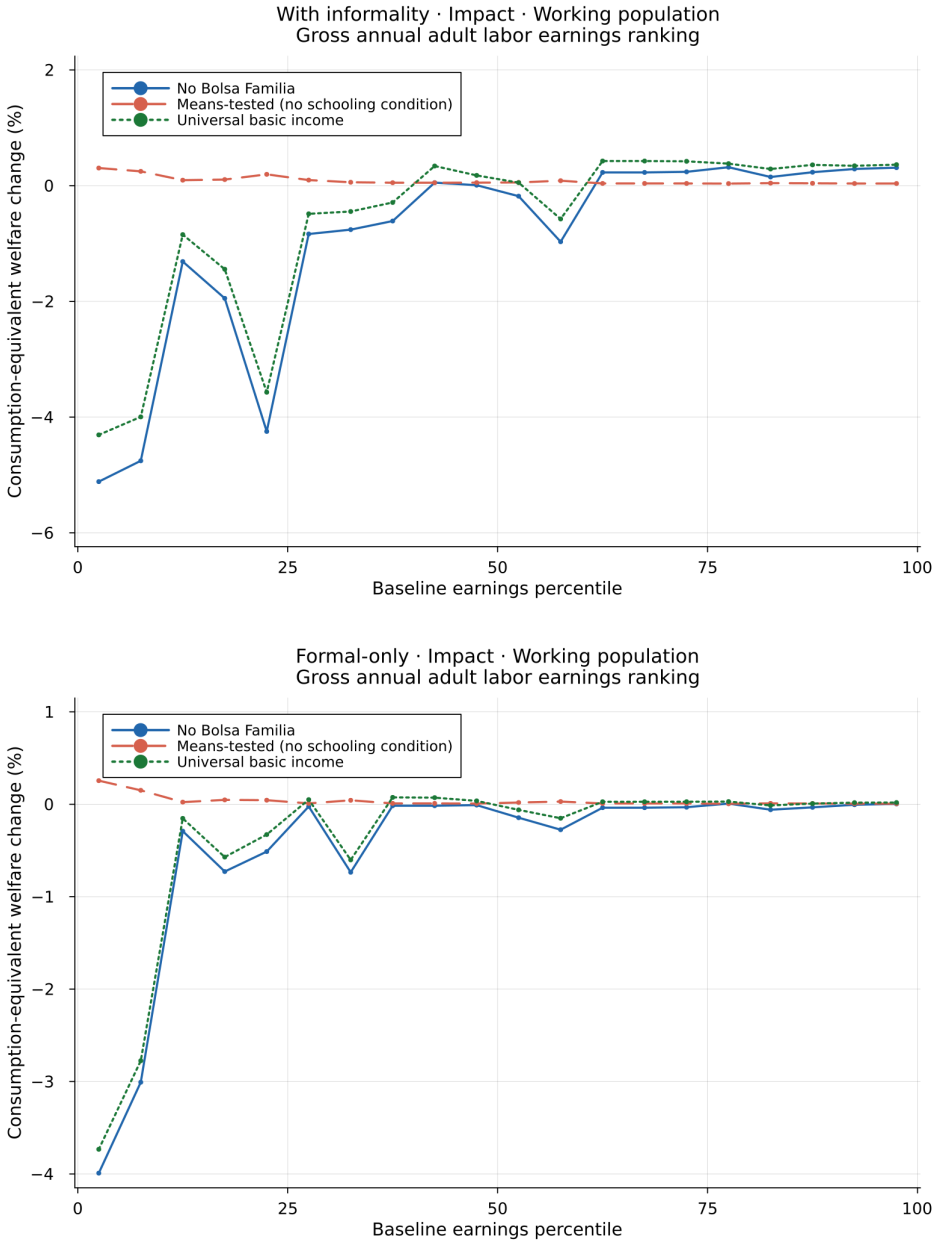


Figure 11: Welfare by baseline earnings rank: households alive at reform

Notes: All working stages, ages 18–56. Each point is a group CEV for a five-percent population bin ranked by baseline gross adult labor earnings. The upper panel allows informality; the lower panel is formal only. Values include the full remaining lifetime along the transition, not only utility in the first period. Vertical scales differ.

H.4 Welfare by earnings group: stage 1

Table 22 repeats the distributional comparison for stage-1 households. Initial losses remain concentrated among low earners and BFP recipients, but some long-run results differ. With informality, the highest earnings quartile gains under no BFP and UBI, whereas the corresponding quartile across all working stages loses.

Table 22: Welfare by earnings group: stage 1

	Alive at reform			Long run		
Group	No BFP	Means-tested	UBI	No BFP	Means-tested	UBI
Panel A: Economy with informality						
All	-1.42	0.11	-1.03	0.74	0.14	1.05
First quartile	-3.78	0.22	-3.09	1.73	0.27	2.13
Second quartile	-0.56	0.06	-0.23	4.52	0.09	4.61
Third quartile	0.31	0.04	0.48	-4.75	0.07	-4.17
Fourth quartile	0.29	0.04	0.34	0.26	0.02	0.30
Baseline BFP recipients	-5.30	0.24	-4.59	–	–	–
Baseline nonrecipients	-0.14	0.07	0.13	–	–	–
Panel B: Formal-only economy						
All	-0.71	0.05	-0.59	0.24	-0.05	0.34
First quartile	-1.87	0.12	-1.67	-0.40	-0.82	-0.22
Second quartile	-0.15	0.02	-0.05	1.40	0.95	1.42
Third quartile	-0.02	0.01	0.05	0.05	-0.02	0.18
Fourth quartile	0.01	0.01	0.02	0.02	-0.03	0.04
Baseline BFP recipients	-8.18	0.50	-7.84	–	–	–
Baseline nonrecipients	-0.29	0.03	-0.18	–	–	–

Notes: Entries are consumption-equivalent percentage changes. Quartiles use gross adult labor earnings before taxes, transfers, and income hiding. BFP receipt groups are reported only at implementation, where baseline membership has a direct interpretation. Aggregate CEV is calculated from combined household welfare, rather than by averaging the group-specific CEVs. All policies use consumption-tax financing.