Anchors in the Storm: Can Emergency Cash Transfers Protect Human Capital During Economic Crises?

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Abstract

Can emergency unconditional cash transfers (UCTs) protect educational investments and human capital accumulation during economic crises? While UCTs mitigate immediate economic hardship, evidence on their capacity to safeguard educational outcomes during emergencies remains limited. This study investigates Brazil's Auxílio Emergencial program—one of the developing world's largest emergency cash transfer programs—and its impact on educational attendance during the COVID-19 pandemic. Using household survey microdata and a regression discontinuity design that exploits exogenous variation in program eligibility, I estimate causal effects on educational attendance among demographic groups within vulnerable single-mother households. Eligibility increased attendance by 16.0 percentage points for young men aged 18-24, with effects driven primarily by those who had dropped out and re-engaged with secondary virtual education. The effects are concentrated among men and are not statistically significant for women. The mechanism operates by reducing economic pressure on households, enabling continued educational participation among younger members while preventing primary earners from engaging in low-quality or informal employment.

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1   Introduction

Can emergency unconditional cash transfers (UCTs) safeguard educational investments during economic crises? While UCTs effectively mitigate immediate economic hardship (Bastagli et al., 2016; Cejudo et al., 2022; Gentilini et al., 2021), evidence on their capacity to protect human capital accumulation during emergencies remains scarce. Research on cash transfers and educational outcomes has focused mainly on Conditional Cash Transfers (CCTs), and consistently suggests that CCTs outperform UCTs in preventing school dropout and improving educational outcomes under normal economic conditions (Bastagli et al., 2016; Handa et al., 2018; Hanna and Olken, 2018; Barrera-Osorio et al., 2011; Snilstveit et al., 2015; Parker and Todd, 2017; Baird et al., 2014). Nevertheless, little has been discussed about the protective role UCTs might have in emergency settings when conditionality is not enforceable and vulnerable households face acute income shocks.

This paper examines whether generous emergency UCTs prevent educational dropout among vulnerable households. I leverage Brazil's Auxílio Emergencial (AE) program, an emergency cash transfer implemented during the COVID-19 pandemic, to identify the causal impact of unconditional transfers on school attendance. Using a regression discontinuity design, I exploit the program's household per capita income eligibility threshold of approximately half the minimum wage, focusing on single-mother households near this cutoff. Single-mother households constituted the primary target group during Brazil's pandemic response. When eligible, they received transfers equivalent to the minimum wage per month for three months, followed by reduced payments of one-fourth of the minimum wage for five additional months. I examine whether this financial support protected educational participation when families faced severe economic disruption and analyze the mechanisms through which emergency UCTs safeguard educational investments.

The program increased educational attendance in the short term among young men from vulnerable households by 16.0 percentage points. This local intention-to-treat (ITT)

estimate represents a lower bounds for the local average treatment effect (LATE), as imperfect compliance at the eligibility threshold attenuates estimated treatment effects.

The findings for young men are robust to multiple sensitivity analyses, including specifications with varying covariate inclusion, alternative bandwidth selection procedures, and a Difference-in-Discontinuity (DiDC) estimator that controls for potential confounders at the cutoff. For children, girls, boys, and young women, the effects are statistically indistinguishable from zero.

The program also decreased the time households allocate to low-quality or informal employment, with this effect mainly driven by main income earners such as household heads and prime-age adults (25-64).

1.1 Literature review

The existing literature shows that UCTs can influence household decisions beyond immediate consumption, including educational investments and labor market participation (Kilburn et al., 2017; Chakrabarti and Handa, 2023; Haushofer and Shapiro, 2013). Nevertheless, policymakers have systematically preferred implementing CCTs, not only because evidence indicates they tend to be slightly more effective[1] (Baird et al., 2014; Bastagli et al., 2016; Handa et al., 2018; Hanna and Olken, 2018; Akresh et al., 2013), but also because one of the most widespread concerns about cash transfers, particularly UCTs, is whether households would spend this additional money on "temptation goods" such as alcohol or tobacco (Banerjee and Mullainathan, 2010; Evans and Popova, 2017; Haushofer and Shapiro, 2013).

This concern centers on whether household members might alter their consumption patterns toward goods that impose negative externalities on other family members. Although multiple studies have found no significant increases—and in some cases, significant decreases—in alcohol or tobacco consumption following unconditional cash provision (Banerjee and Mullainathan, 2010; Evans and Popova, 2017; Bobonis, 2009; Al Izzati et al., 2020), policymaker apprehensions persist.[2] These persistent concerns, combined with evidence favoring conditional programs, have shaped policy adoption patterns. Following

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[1](Baird et al., 2014) analyze over 75 studies covering UCTs and CCTs from 35 different programs and their impact on educational outcomes, finding that both conditional cash transfers (CCTs) and unconditional cash transfers (UCTs) improve the odds of being enrolled in and attending school compared to no cash transfer program. The effect sizes for enrollment and attendance are larger for CCTs compared to UCTs, but the difference is not statistically significant. Akresh et al. (2013) found that CCTs and UCTs similarly increase enrollment among children traditionally favored for schooling (boys, older children, higher ability children), but CCTs significantly outperform UCTs in enrolling "marginal children" (girls, younger children, lower ability children) who receive fewer parental investments, highlighting conditionality's critical role in reaching disadvantaged children.

[2]Evans and Popova (2017) conducted a meta-analysis of 50 estimates from 19 studies and found that studies report either no significant impact or a significant negative impact of transfers on alcohol and tobacco expenditures, suggesting cash transfers are not used for these goods, irrespective of region or program design.

the successful implementation of Progresa/Oportunidades in Mexico, CCTs became the dominant antipoverty intervention across Latin America and other developing regions over the past 25 years (Laguinge et al., 2025; Parker and Todd, 2017).[3] Meanwhile, UCT implementation and evaluation have remained limited.

However, the adoption of UCTs as emergency response mechanisms has surged during recent economic crises. The COVID-19 pandemic catalyzed unprecedented global deployment of unconditional transfers, with governments implementing UCT programs reaching 17% of the world's population—734 programs across 186 countries according to Gentilini et al. (2021). Latin American countries exemplified this trend, with Cejudo et al. (2022) documenting 122 cash transfer programs implemented by 27 governments in the region.

These emergency programs differed markedly in generosity and duration. Brazil's AE was among the region's most extensive: eligible single mothers received transfers equal to one minimum wage (about 82.3% of mean income) per month for three months, followed by reduced payments of roughly 25% of the minimum wage for five additional months. Other programs offered more modest support. Peru's Bono Familiar Universal provided a single transfer equivalent to 82% of the monthly minimum wage (90.8% of mean income), while Argentina's Ingreso Familiar de Emergencia delivered about 49% of the monthly minimum wage (less than 45% of mean income) across three payments. Colombia's emergency cash transfer (VAT compensation) amounted to roughly 8% of the monthly minimum wage (around 12% of mean income) every five to eight weeks, and Costa Rica's three-month Bono Proteger totaled about 44% of the monthly minimum wage (38.3% of mean income).

While existing research documents positive educational effects from UCTs during crises (Kilburn et al., 2017; Chakrabarti and Handa, 2023; Haushofer and Shapiro, 2013), evidence on substantially more generous emergency UCTs—here defined as monthly payments exceeding 50% of mean income—remains limited. Most prior studies examine relatively modest interventions: Londoño-Vélez and Querubín (2022) evaluated Colombia's VAT compensation (equivalent to PPP US$55.6) and found small positive effects on parental educational investment (0.032 standard deviations); Aggarwal et al. (2024) documented that comparable transfers modestly increased education spending, enrollment, and reduced school absences by 1.46 days annually in Malawi and Liberia; and Banerjee et al. (2020) showed positive effects from early Universal Basic Income pilots in India and Namibia. This body of evidence highlights an important limitation: we lack systematic evaluations of how emergency and UCTs of larger magnitude might protect human capital accumulation during acute household crises. Substantial transfers could meaningfully reduce the opportunity costs that lead families to withdraw children or young adults from

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[3]Evaluations of CCT programs consistently show they reduce child labor and increase school enrollment and attendance, with enrollment gains ranging from 0.5 percentage points in Jamaica to 12.8 percentage points in Nicaragua (Ibarrarán et al., 2017).

education during economic shocks, as discussed in the theoretical framework section 2.[4]

A few studies of UCT programs in emergency settings reveal how cash transfers protect educational investments. Kilburn et al. (2017) demonstrate that UCTs increase school enrollment by alleviating household financial constraints, operating primarily through income effects that enable families to afford essential educational expenses. Household spending patterns confirm this mechanism: Banerjee et al. (2020) found that parents allocated transfer funds toward schooling materials, while Londoño-Vélez and Querubín (2022) documented educational investments including books, school supplies, and mobile internet access for remote learning during COVID-19.

The relationship between transfer magnitude and educational impact receives empirical support from Kilburn et al. (2017), who demonstrate that programs providing at least 20% of pre-program household consumption generate substantially larger enrollment effects than programs with smaller amounts. Meta-analyses by Baird et al. (2014); Snilstveit et al. (2015); García and Saavedra (2017) corroborate this finding, indicating that educational outcomes scale positively with transfer size.[5]

This research addresses the evidence gap on the educational effects of large-value emergency UCTs by analyzing how Brazil's AE affected educational participation during the COVID-19 crisis in vulnerable single-mother-led households. The analysis employs PNADC microdata from the *Instituto Brasileiro de Geografia e Estatística* (IBGE), a nationally representative survey of Brazilian households. I exploit the program's prioritization of single-mother households and income eligibility threshold to implement a regression discontinuity design that identifies the local causal effect of eligibility on school attendance.

The remainder of the paper proceeds as follows. Section 2 develops a theoretical framework extending the traditional schooling model to show how UCTs can protect educational investment during emergencies. Section 3 describes Brazil's social protection system and AE's implementation during the pandemic. Sections 4 and 5 present the data sources, descriptive statistics, and empirical strategy. Section 6 presents the main results, with robustness checks reported in Section 7. Section 8 discusses these findings and Section 9 concludes.

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[4]This distinction is particularly relevant given evidence suggesting that transfer size can shape educational outcomes and other intertemporal household decisions (Baird et al., 2014; Balboni et al., 2022).

[5]The importance of adequate transfer size extends beyond education to broader poverty alleviation strategies. Balboni et al. (2022) demonstrate that households require a minimum threshold of income to escape poverty and accumulate financial wealth, suggesting that transfers below this critical level may prove insufficient to generate meaningful behavioral changes if they fail to enable investment in physical or human capital beyond essential survival needs.

2 Theoretical Framework

2.1 Cash transfers' effect on schooling in normal economic contexts

Following Parker and Todd (2017), consider a two-period model where individuals allocate time among leisure, work, and school as youths (period 1), and between leisure and work as adults (period 2). The model assumes diminishing marginal utility of consumption and leisure in each period and diminishing marginal productivity of schooling on the second-period wage rate. Consumption and leisure are normal goods, and for simplicity, schooling provides no direct utility but solely serves as a technology for transferring resources from the first to the second period and increasing the second-period wage rate. The lifetime utility function is:

$$U_1(C_1, L_1) + \beta U_2(C_2, L_2) \tag{1}$$

where C_t denotes consumption in period p, L_t denotes leisure, and β is the discount factor.

Time constraints in each period are:

$$S + t_1 + L_1 \leq T \tag{2}$$
$$t_2 + L_2 \leq T \tag{3}$$

where S represents time in school, t_i is time spent working, and T is the total time endowment.

The lifetime budget constraint is:

$$C_1 + \frac{C_2}{(1+r)} = CT + p_s S + (T - L_1 - S)W_1 + \frac{(T - L_2)W_2(S)}{(1+r)} \tag{4}$$

where CT represents unconditional transfers to the individual, p_s is the per-unit subsidy for schooling (conditional on attending), W_1 is the child wage rate, $W_2(S)$ is the adult wage rate (increasing in S), and r is the interest rate.

The optimality condition that holds in any interior solution is:

$$MU_{C_1}(W_1 - p_s) = \beta MU_{C_2} t_2 W_2'(S) \tag{5}$$

The left side represents the marginal cost of schooling: direct monetary costs (net of subsidies) and foregone earnings from youth employment. The right side captures the marginal benefit through increased adult earnings.

CCTs reduce the opportunity cost of school attendance by subsidizing education relative to alternative uses of youths' time such as work. This generates both income and

price effects, whereas UCTs produce only income effects. The price effect, which lowers the shadow price of schooling, unambiguously increases educational investment. By contrast, the impact of UCTs on schooling depends on several factors: the marginal effect of education on future wages, adult labor supply (t_2), the discount rate, and household knowledge of these potential returns. Heterogeneity in any of these parameters can thus generate differential responses to UCTs across individuals and households (Parker and Todd, 2017).[6]

Under this model, CCTs have theoretically unambiguous positive effects on schooling through the price mechanism, while UCTs face theoretical ambiguity. Because schooling generates no direct utility in this model (serving only as an intertemporal transfer technology), the income effect alone does not guarantee increased educational investment. This supports empirical findings that UCTs are typically less effective than CCTs for educational outcomes under normal economic conditions.

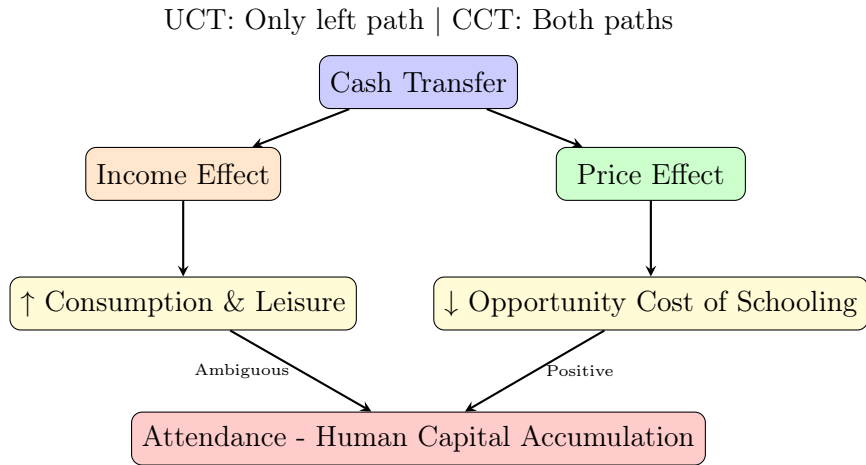UCT: Only left path | CCT: Both paths

Figure 1: Cash Transfer Effects on Educational Attendance in Non-emergency Settings. Sources: Author's own elaboration.

2.2 Can UCTs Have a Protective Effect on Human Capital Accumulation During Crises?

During economic crises, vulnerable households face severe income shocks that might alter their educational investment decisions. These shocks increase the opportunity cost of schooling as families struggle to maintain basic consumption. Under financial stress, current income becomes a critical determinant of intertemporal decisions, inducing more present-oriented behavior. A key question is whether UCTs in crisis settings can protect household investments in human capital accumulation. Evidence of such protection would

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[6]For example, adult males typically supply more market hours than adult females, implying that the marginal benefit of schooling—measured through lifetime earnings—is higher for men than women at any given education level S. This mechanism may partially explain the gender gaps in educational attainment documented in the following sections.

suggest that a) short-term income shocks affect long-term educational decisions, a mechanism overlooked in the model presented in Section 2.1 or b) indicate that the income effect of UCTs on human capital accumulation is positive rather than ambiguous.

Conventional schooling models cannot capture the first mechanism because they treat household discount rates as exogenous and assume that schooling provides no direct utility. The standard framework can be extended to address this limitation by making the discount rate an explicit function of current income and household characteristics:

$$r_p = f(Y_p, X_p) \tag{6}$$

where r_p represents the household discount rate in period p, Y_p denotes current total household income, and X_p captures other household characteristics. The key relationships are $\frac{\partial r_p}{\partial Y_p} < 0$ and $\frac{\partial^2 r_p}{\partial Y_p^2} > 0$. The first derivative indicates that income declines increase discount rates, making families more present-biased and potentially reducing long-term human capital formation as they prioritize current income from labor over the future returns of educational investments. The second derivative captures that this sensitivity diminishes at higher income levels, where education costs represent a smaller proportion of available income. Thus, discount rates respond more strongly to income changes among lower-income households.[7]

Define total income for individual i in period p as $Y_{ip} = W_{ip} + UCT_{ip}$, where W_{ip} represents labor income and UCT_{ip} denotes cash transfer income. Under the relationships described above, it follows that $\frac{\partial r_p}{\partial UCT_p} < 0$ and $\frac{\partial^2 r_p}{\partial UCT_p^2} > 0$ for a given W_{ip}.

If these relationships hold in crisis settings, UCTs would be expected to buffer the impact of income shocks on educational decisions through income stabilization. Consider that households take coordinated decisions on how to allocate the time of adults between work and leisure and the time of younger members between school, work, and leisure. Vulnerable households experiencing negative shocks move toward the lower portion of the income distribution where $\left| \frac{\partial r_p}{\partial Y_p} \right|$ is largest. In this region, income losses would substantially increase discount rates due to the convexity of $r(Y)$, inducing families to choose suboptimal schooling levels for their younger members that sacrifice long-term returns for immediate income. UCTs would counteract this effect by maintaining household income at levels associated with lower discount rates, preventing premature withdrawal of younger members from school. The convexity further implies that transfers should yield greater protective benefits when income is most constrained.

While household discount rates cannot be directly observed, this mechanism can be tested indirectly by evaluating whether UCTs significantly affected educational attendance. Finding a protective effect on schooling would support either: (a) the endogenous

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[7]A functional form consistent with these properties is $r_p = \frac{k}{Y_p^n} + g(X_p)$ for $n > 0$, where marginal effects $|\frac{\partial r}{\partial Y}| = \frac{nk}{Y^{n+1}}$ are larger when Y is small.

discount rate mechanism described above, or (b) the simpler interpretation that the income effect of UCTs on human capital accumulation is positive rather than ambiguous as predicted by the standard model.
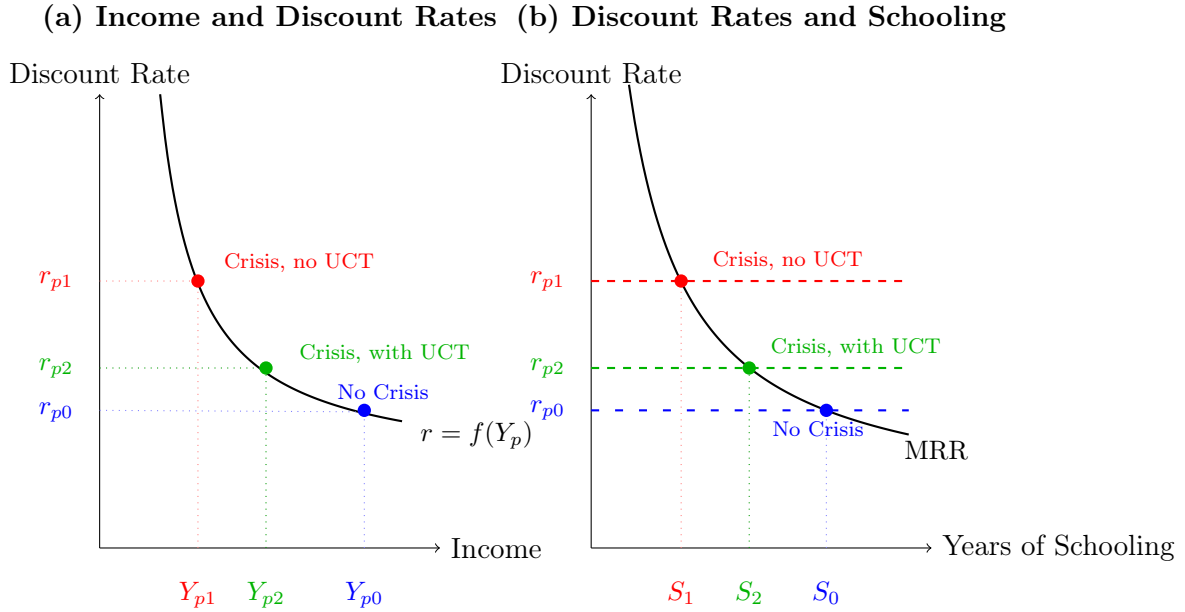
(a) Income and Discount Rates (b) Discount Rates and Schooling

Figure 2: Ilustration of the UCT Protection Mechanism: Non-Linear Relationship Between Income, Discount Rates, and Schooling Decisions. Panel (a) illustrates the convex relationship $r = f(Y)$ where $\frac{\partial r}{\partial Y} < 0$ and $\frac{\partial^2 r}{\partial Y^2} > 0$, showing that discount rates exhibit greater sensitivity to income changes at lower income levels. Panel (b) shows the downward-sloping marginal rate of return (MRR) curve, illustrating how lower discount rates are associated with higher levels of schooling (and expected long-term income). The convexity of both curves explains why UCTs might be particularly effective during crises: the same transfer yields larger reductions in r when baseline income is low, which translates into larger gains in schooling.
Sources: Author's own elaboration.

3 Institutional Context: Auxílio Emergencial

Launched in April 2020, AE became one of the world's largest emergency cash transfer programs, supporting 67 million individuals—over 30% of Brazil's population—by September 2020. The program provided monthly payments in two phases: three initial transfers of R\$600 for standard beneficiaries (57% of the minimum wage) and R\$1,200 for vulnerable single-mother households (115% of the minimum wage), followed by five additional payments of R\$300 (29% of the minimum wage) for all recipients (Lara Ibarra and Campante Vale, 2023; Falcão, 2022).[8]

The program targeted vulnerable populations through three eligibility criteria: per capita household income below R\$522.5 (half the minimum wage in 2020), a maximum of two beneficiaries per household, and prioritization of single-mother households meeting

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[8]Brazil documented the first COVID-19 cases in Latin America and had the world's second-highest case count by mid-2020 (Barone et al., 2021).

the income condition. Single mothers below the program eligibility threshold received double the standard transfer amount, and no other household members could receive additional transfers from the program.

3.1 Beneficiary Identification and Coverage

Building on Brazil's experience with the Bolsa Família program and its comprehensive social registry system (Cadastro Único), most single mothers who were eligible for AE received transfers automatically through existing Cadastro Único classifications or prior Bolsa Família enrollment. Beneficiaries not already in Cadastro Único were identified through a digital application platform where potential recipients applied for benefits, which were granted when submitted information aligned with existing social protection registries (Lara Ibarra and Campante Vale, 2023). Administrative records indicate that approximately 90% of eligible single-mother households successfully received transfers, facilitated by Brazil's relatively high financial inclusion, particularly through innovations such as PIX (Lara Ibarra and Campante Vale, 2022).[9]

COVID-19's outbreak in March 2020 prompted all 26 Brazilian states and the Federal District to suspend in-person education, affecting over 35.2 million children and adolescents—approximately 17% of the national population (Barberia et al., 2021). These widespread school closures, which lasted 40 weeks on average—twice the global average—were implemented as social distancing measures but created additional hardships for low-income families who lost access to school-based services while facing increased childcare demands. The educational disruption was particularly acute given Brazil's digital divide: only 81% of households had home internet access, severely limiting effective remote learning participation among vulnerable populations.

At the tertiary education level, most universities transitioned to virtual learning strategies. Nevertheless, public federal institutions made this transition more slowly than state-level institutions, as the federal government maintained an anti-closure stance. In response to limited internet access among certain households, some institutions implemented emergency programs to facilitate connectivity and device lending. Brazil's higher education system—comprising tuition-free public universities alongside private institutions accessible through programs such as PROUNI (scholarships for low-income students) and FIES (student financing)—serves a student population disproportionately drawn from higher socioeconomic strata, though federal affirmative action policies have expanded access for vulnerable populations since 2012.

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[9]Brazil's Central Bank launched PIX, an instant payment system, in November 2020. Unlike existing electronic transfer methods (TED and DOC), PIX enabled real-time transfers 24/7 at no cost to individuals, significantly expanding financial inclusion and bringing over 71 million previously unbanked individuals into the formal financial system (International Monetary Fund, 2023). The AE program combined existing social registries with on-demand applications via online, telephone, and face-to-face channels, registering 96 million applicants by late May 2020, with a 61% acceptance rate.

Concurrently, Brazil's labor market experienced severe contraction, with 7.3 million jobs lost by year-end and unemployment rising from 11.9% in 2019 to 13.5% in 2020, as the informal sector—where many low-income families work—accounted for two-thirds of all job losses.

This analysis examines whether AE transfers enabled vulnerable households to sustain human capital accumulation during the crisis by protecting children's and young adults' engagement with emergency virtual schooling and tertiary education, thereby mitigating dropout driven by financial constraints and allowing families to cope not only with the direct costs of schooling but also with additional household burdens related to school closures, such as increased caregiving demands.

4   Data and Descriptive Statistics

This study employs microdata from the *Pesquisa Nacional por Amostra de Domicílios Contínua* (PNAD-C), Brazil's continuous national household survey that monitors labor market dynamics and socioeconomic development. The PNADC provides nationally representative data on demographics, income, labor market outcomes, and educational attendance. The primary analysis uses quarterly cross-sectional data from 2020, while robustness checks employ longitudinal data from the same survey spanning 2019–2020.

The analysis restricts attention to individuals in single-mother households to ensure treatment homogeneity. All such households below the program threshold were eligible for identical monthly transfers equivalent to approximately 115% of the minimum wage.

I construct the analytical sample by identifying single-mother households through a sequential process. First, I identify female household heads who: (1) report single marital status, (2) have at least one child under 18 years of age, and (3) have no cohabiting partner within the household. I then include all individuals residing in these single-mother-headed households. This procedure generates a sample of 26,175 individuals from 11,855 households in 2020, representing 7.9% of households in the PNAD-C 2020 sample, with 38.7% falling below the AE eligibility threshold. As discussed in Section 5, the effective analytical sample is smaller, as the RD regression uses only observations within a selected bandwidth around the eligibility cutoff.

Brazil's PNAD-C survey employs a five-quarter continuous rotation scheme that tracks households for 15 months. While the public microdata lacks unique individual or household identifiers, it provides variables that enable constructing such identifiers. I construct the panel dataset by matching individuals and households whose first interview occurred in 2019 and whose fifth interview fell in the second, third, or fourth quarter of 2020. This matching procedure permits tracking the same units before and after the onset of the public health emergency and the initiation of AE transfers. With it, I implement a Differences-in-Discontinuities (DiDC) analysis to test the robustness of the main results

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after controlling for potential confounders at the discontinuity. I also implement a transition analysis that evaluates how units changed their educational attendance status and labor market participation, including entry into and exit from informal and formal jobs.

4.1 Limitations

The primary limitation of the data is that household receipt of AE transfers is not directly observed—only eligibility is observed. Nevertheless, single-mother households were the best-targeted and most-reached group, with administrative records estimating an effective take-up of approximately 90% (Lara Ibarra and Campante Vale, 2022). Consequently, as explained in Section 5, the estimates correspond to the local intention-to-treat effect (LITT), which represents a lower bound of the local average treatment effect (LATE), since eligible households that did not receive transfers would attenuate the estimated effect.

A second important limitation is that both attendance and income variables are self-reported in PNAD-C data. Self-reported income raises concerns about potential strategic misreporting to qualify for social programs. However, several factors mitigate this concern: the surveys used for this research were conducted prior to program implementation in 2019 and after implementation in 2020, reducing the likelihood that households perceived the survey as linked to program targeting. Moreover, the actual targeting mechanism operated independently of these surveys and relied primarily on administrative records from Cadastro Único rather than self-reported survey data. To support the validity of the methodology, Section 3 demonstrates that there is no evidence of manipulation around the program's eligibility threshold.

Self-reported attendance also poses limitations, as eligible households might exhibit greater social desirability bia, even though the program was unconditional. This limitation and its implications are further discussed in Section 8.

4.2 Descriptive statistics

Table 1 presents descriptive statistics for household head characteristics. The eligible and non-eligible groups differ substantially in pre-treatment characteristics, making it clear that these populations are not directly comparable without the regression discontinuity design.

Examining household heads first, eligible mothers are younger (36.9 vs 40.6 years), have more children (2.14 vs 1.35), and lead larger households (3.70 vs 2.95 members). They exhibit lower educational attainment, with 42% having incomplete primary education compared to 24% among non-eligible heads. Eligible heads show lower labor force participation (55% vs 78%) and employment rates (36% vs 71%), alongside substantially lower per person income at the household level(325 vs 1,277 BRL). Geographic concen-

tration also differs markedly, with eligible households more prevalent in the historically less developed Northeast region (49% vs 30%) and less likely to be urban (77% vs 89%).

Table 1: Pre-treatment Descriptive Statistics: Household Head Characteristics

	Total			Eligible ($\leq$ 522.5)			Non-Eligible ($>$ 522.5)			Diff.
Variable	Mean	SD	N	Mean	SD	N	Mean	SD	N	(E-NE)
Demographic Characteristics										
Age	39.23	8.60	7440	36.92	8.09	2727	40.56	8.60	4713	-3.64***
Urban	0.84	0.37	7440	0.77	0.42	2727	0.89	0.32	4713	-0.12***
Household Structure										
Number of children	1.64	0.89	7440	2.14	1.06	2727	1.35	0.61	4713	0.79***
Household members	3.23	1.28	7440	3.70	1.41	2727	2.95	1.12	4713	0.74***
Education										
Incomplete primary	0.30	0.46	7440	0.42	0.49	2727	0.24	0.43	4713	0.19***
Complete primary	0.09	0.28	7440	0.11	0.31	2727	0.08	0.27	4713	0.03***
Incomplete secondary	0.08	0.27	7440	0.10	0.30	2727	0.07	0.26	4713	0.03***
Complete secondary	0.32	0.47	7440	0.30	0.46	2727	0.34	0.47	4713	-0.03***
Incomplete tertiary	0.07	0.26	7440	0.04	0.20	2727	0.09	0.28	4713	-0.04***
Complete tertiary	0.13	0.34	7440	0.03	0.17	2727	0.19	0.40	4713	-0.17***
Maximum education level[a]	3.67	1.53	7440	3.05	1.40	2727	4.02	1.49	4713	-0.98***
Economic Variables										
Labor force participation	0.70	0.46	7440	0.55	0.50	2727	0.78	0.41	4713	-0.23***
Employed	0.58	0.49	7440	0.36	0.48	2727	0.71	0.45	4713	-0.35***
Number employed in HH	0.83	0.76	7440	0.49	0.66	2727	1.02	0.75	4713	-0.53***
Per capita income (thousands)	0.93	1.14	7440	0.32	0.13	2727	1.28	1.31	4713	-951.9***
Geographic Distribution										
Region: North	0.14	0.35	7440	0.16	0.37	2727	0.13	0.34	4713	0.03***
Region: Northeast	0.37	0.48	7440	0.49	0.50	2727	0.30	0.46	4713	0.19***
Region: Southeast	0.26	0.44	7440	0.20	0.40	2727	0.29	0.46	4713	-0.09***
Region: South	0.14	0.34	7440	0.07	0.26	2727	0.17	0.38	4713	-0.10***
Region: Central-West	0.09	0.29	7440	0.08	0.27	2727	0.10	0.30	4713	-0.02***

Source: Own construction, PNAD-C. Sample: household heads in single-mother households.
[a] Maximum education level: categorical variable from 0 (incomplete primary) to 6 (complete tertiary).
Joint significance test for covariate balance: F-statistic = 23.95 (df = 4), p-value = 0.005.
* p<0.10, ** p<0.05, *** p<0.01. Difference = Eligible mean - Non-eligible mean.

Table A.1 presents household-level averages across all members and shows the same marked differences between eligible households and non-eligible ones: they have younger members on average (19.5 vs 25.0 years), a slightly higher proportion of men (34.3% vs 30.8%), more children (2.1 vs 1.4 on average), lower labor force participation (44.5% vs 64.0%) and lower number of people employed in the household (0.49 vs 1.02).

These structural differences preclude direct comparison between eligible and non-eligible groups to assess program effectiveness. To estimate the causal effects of AE on educational and labor market outcomes, Section 5 presents the regression discontinuity framework that exploits the discontinuity at the eligibility threshold to assess such effects.

5 Empirical Framework and identification

The ideal experimental design to evaluate the effect of an emergency UCT on educational attendance would randomly assign treatment among comparable households and follow individuals over time. Under such randomization, a simple mean difference would yield an unbiased estimate of the average treatment effect through the following specification:

$$A_{ij} = \alpha + \tau D_{ij} + \boldsymbol{\beta} \boldsymbol{Z_{ij}} + \varepsilon_{ij} \quad \text{with} \quad D_{ij} \perp \varepsilon_{ij} \tag{7}$$

where A_{ij} represents the outcome of interest (e.g. a dummy variable indicating if the individual i goes to school in household j), D_{ij} is the treatment indicator, $\boldsymbol{Z_{ij}}$ is an optional vector of pre-treatment covariates, and ε_{ij} is the idiosyncratic term. The condition $D_{ij} \perp \varepsilon_{ij}$ indicates that under randomization treatment assignment is orthogonal to the regression error (that is, uncorrelated with unobserved determinants of the outcome that might be related with the included regressors).[10]

However, AE was not randomly assigned. The program was implemented as an emergency intervention making all households below a specified per capita income threshold eligible for transfers. I exploit this eligibility threshold—set at half the minimum wage (522.5 BRL in monthly per capita income)—as a source of exogenous variation.

As noted in Section 7, no other program uses this same eligibility threshold, and there is no evidence of prior policies or interventions varying discontinuously at this threshold before the program's implementation. I define the running variable as $R_{ij} = M_{ij} - 522.5$, where M_{ij} represents household per capita income in Brazilian reais. This normalization assigns eligibility to households with $R_{ij} < 0$, while those with $R_{ij} \geq 0$ remain ineligible. The arbitrary nature of this income cutoff provides quasi-random variation in treatment assignment for households near the threshold. Small differences in reported income around the cutoff are unlikely to correlate with unobserved determinants of educational attendance, as households had limited ability to manipulate their eligibility status.[11]

Under the potential outcomes framework, let $A_{ij}(1)$ and $A_{ij}(0)$ denote attendance outcomes for individual i in household j with and without treatment, respectively. The observed outcome is

$$A_{ij} = T_{ij} \times A_{ij}(1) + (1 - T_{ij}) \times A_{ij}(0) \tag{8}$$

where treatment assignment follows $T_{ij} = \mathbf{1}\{R_{ij} < 0\}$. If the conditional expectation

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[10]Under a valid randomization this vector should not affect the estimation of the interest coefficient τ but might be useful to obtain more precise estimates and control for random differences between groups after the randomization.

[11]For surveys conducted prior to the program's implementation, households had no knowledge of program eligibility criteria. Once implemented, although the threshold became public knowledge, most eligible households were identified through Cadastro Único administrative records. Households not in these records applied through a web application independent of the survey used in this study.

functions for potential outcomes are continuous at the threshold, the regression discontinuity design identifies the local average treatment effect (LATE)[12]:

$$\tau = E[A_{ij}(1) - A_{ij}(0)|R_{ij} = 0] = \lim_{x\downarrow 0} \mu(x) - \lim_{x\uparrow 0} \mu(x) \tag{9}$$

where $\mu(x) = E[A_{ij}|R_{ij} = x]$ represents the conditional expectation of the outcome variable given the running variable. The parameter τ can be estimated using local polynomial regression within a data-driven bandwidth around the threshold (Cattaneo et al., 2020), comparing outcomes of households just below and above the eligibility cutoff.

Regression discontinuity designs exploit the quasi-experimental variation generated by eligibility thresholds to identify local causal effects under plausible identifying assumptions[13]. The key identifying assumptions are that potential outcomes are continuous at the threshold and that individuals cannot precisely manipulate their position relative to the cutoff (Calonico et al., 2014). While continuity of potential outcomes is untestable, the plausibility of the design can be assessed empirically through density tests for manipulation and covariate balance checks around the threshold.

The primary limitation of this approach is that it estimates a local average treatment effect (LATE) in the neighborhood of the cutoff, which limits extrapolation to populations farther from the eligibility threshold. Section 5.1 presents diagnostic tests that examine whether households can strategically manipulate their reported income and whether pre-treatment covariates exhibit discontinuities at the threshold.

5.1   Estimation Strategy

I implement the regression discontinuity design using local polynomial regression within a data-driven optimal bandwidth around the eligibility threshold. The main estimation equation is:

$$A_{ij} = \alpha + \tau T_{ij} + \sum_{k=1}^{p} \beta_k R_{ij}^k + \sum_{k=1}^{p} \gamma_k T_{ij} \times R_{ij}^k + \boldsymbol{\delta}\mathbf{Z}_{ij} + \theta_r + \varepsilon_{ij} \tag{10}$$

for observations within the bandwidth $|R_{ij}| < h$, where T_{ij} is the eligibility indicator, R_{ij} represents the running variable (per capita income minus the threshold), and p denotes the polynomial order. The main specification employs a $p = 1$ polynomial (local linear regression) in the running variable and its interactions with the treatment indicator. This approach follows Calonico et al. (2020), who show that first-order polynomials yield robust bias-corrected inference when the conditional expectation function is sufficiently smooth.

The specification incorporates pre-treatment covariates $\mathbf{Z}_{ij}$, including individual char-

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[12]For a detailed formal development, see Kolesár and Rothe (2018).

[13]For an in-depth discussion of nonparametric inference using local polynomial regressions, see Calonico et al. (2014, 2015, 2018, 2019, 2020); Cattaneo et al. (2020); Calonico et al. (2022).

acteristics (sex, age, educational attainment of the household head, and area of residence), and regional fixed effects θ_r to control for systematic geographic variation. Standard errors are clustered at the regional level to account for within-region correlation. While not strictly necessary for identification in a valid RDD, these controls improve precision (Calonico et al., 2019), and the main results remain substantively unchanged with or without their inclusion.

The coefficient τ captures the local intent-to-treat effect (LITT) of program eligibility on the outcome variable. Since eligibility rather than actual participation is observed, this estimate provides a lower bound of the true treatment effect, as discussed in Section 4. The main specification employs an MSE-optimal bandwidth selected using data-driven procedures to balance bias-variance trade-offs (Cattaneo et al., 2020). Robustness checks use separate MSE-optimal bandwidths for each side of the cutoff.

5.2 Validation of Identifying Assumptions

The identification strategy relies on two key assumptions: continuity of potential outcomes and the absence of precise manipulation around the eligibility threshold. Figure 3, Table A.2, and Figure A.1 provide empirical support for these assumptions.

Figure 3 presents two formal validation tests. First, the manipulation test using local polynomial density estimation yields a bias-corrected p-value of 0.51, failing to reject the null hypothesis of no manipulation around the threshold. Second, placebo tests applying the main specification with pre-treatment covariates as outcomes reveal no statistically significant discontinuities at the eligibility threshold, providing evidence supporting the continuity of potential outcomes assumption.[14]

Nevertheless, this figure shows that eligible households around the threshold tend to have a higher probability of being in a rural area than ineligible households. Table A.2 presents traditional covariate balance tests using a linear OLS specification for twelve pre-treatment variables at a bandwidth of 50 BRL. Only two variables exhibit statistically significant differences between units below and above the threshold: eligible households have slightly fewer members (3.6 versus 3.7 on average) and lower rates of urban residence (76 percent versus 86 percent). The joint significance test fails to reject the null hypothesis of covariate balance. The difference in household size is negligible, and both urban residence and household size are controlled for in the main specification.
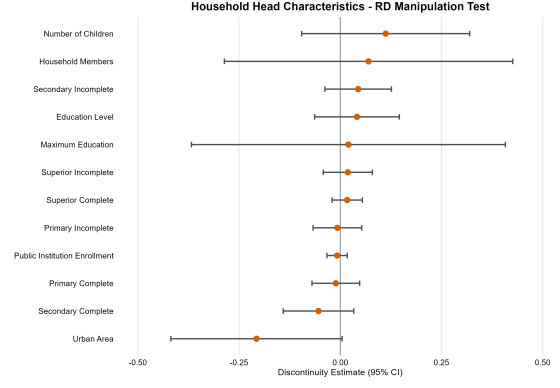
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[14]Figure A.1 provides a visual examination of the running variable distribution around the eligibility threshold. The kernel density estimate exhibits no visible discontinuity at the threshold and transitions smoothly from below to above the cutoff, with no evidence of jumps or bunching.
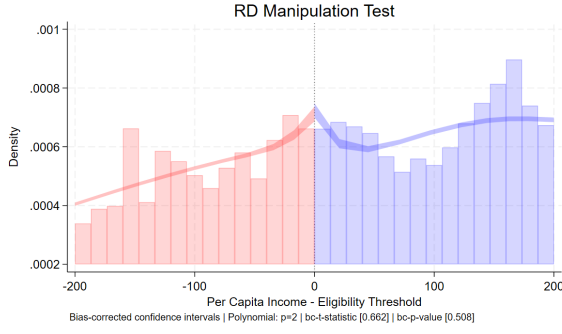
(a) Manipulation Test Using Local Polynomial Density Estimation

(b) RD placebo test on pre-treatment covariates

Figure 3: Regression Discontinuity Validation Tests. Panel (a) shows the manipulation test using local polynomial density estimation. Panel (b) presents RD coefficient estimates for the effect of eligibility on pre-treatment covariates using the same specification as the main analysis at the household level.
Sources: Author's own elaboration.

6 Results

The theoretical framework in Section 2 predicts that the AE program alleviates immediate liquidity constraints that affect households' intertemporal discount rates by reducing the opportunity cost of decreasing current household labor supply when investing in human capital. Consequently, younger members of eligible households should exhibit higher educational attendance compared to members of ineligible households near the eligibility threshold.

6.1 Effect of AE on Human Capital Accumulation

To test this prediction, I implement the estimation strategy described in Section 5. Figure 4 presents the local intent-to-treat effect (LITT) of AE on younger members of single-mother-led households. The results indicate that AE positively affected human capital accumulation, as measured by attendance at educational institutions, with heterogeneous effects by age and gender. For young men aged 18-24, the program increased educational attendance by 16.0 percentage points near the eligibility threshold, as shown in Figure 5. This effect was concentrated among young men; estimates for children under 18 and women aged 18-24 were not statistically different from zero.[15]

The heterogeneous effects by gender are also consistent with previous research conducted by Barrera-Osorio et al. (2011) in Colombia that showed that a CCT on attendance

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[15]Appendix B presents a heterogeneity analysis by demographic subgroups within single-mother-led households and RD plots using local linear (p=1) and local quadratic (p=2) polynomial specifications.

had much smaller gains for girls: they experienced no statistically significant change in attendance across the experiment's treatment arms.
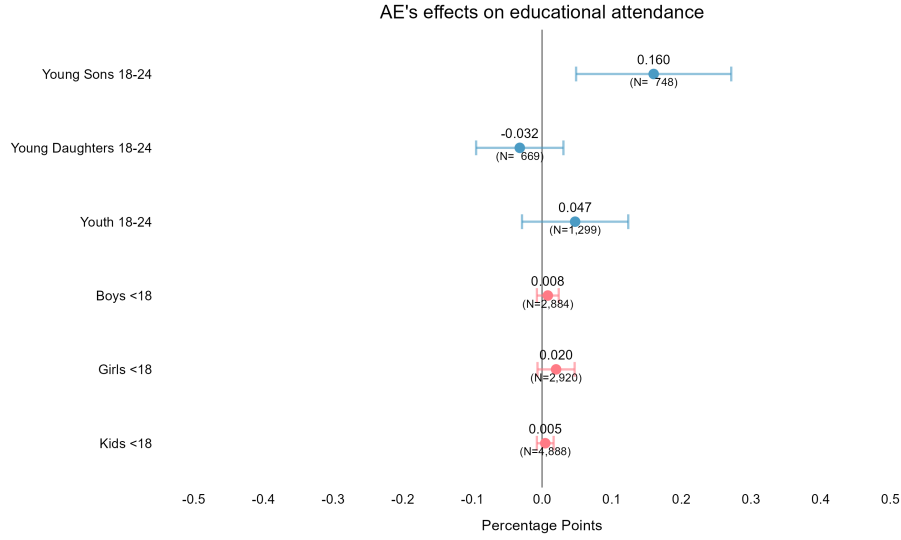

Figure 4: EA effects on Educational Attendance of Young People and Children
Source: Own construction using data from Single-mother-led households, PNACD (2020).
Note: The unit of analysis is households; the outcome variable is the household-level average attendance rate for each demographic group (e.g., average attendance among young sons within the household). Estimates use local polynomial regression with bias-corrected inference following Calonico et al. (2018, 2020). The running variable is household per capita income centered at the eligibility threshold. Standard errors are clustered at the regional level. Household-level controls include: household size, urban location, proportion of male members, and household head education. Bandwidth selection follows the MSE-optimal criterion. Polynomial orders: p=1 (local linear estimation), q=2 (bias correction).
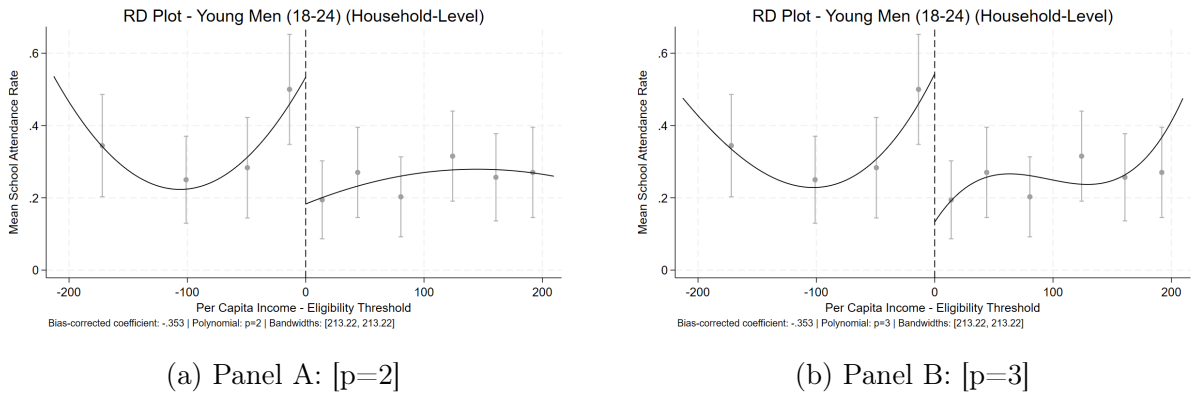

(a) Panel A: [p=2]

(b) Panel B: [p=3]

Figure 5: EA effects on Educational Attendance of Young People and Children
Source: Own construction using data from Single-mother-led households, PNACD (2020).
Note: The unit of analysis is households; the outcome variable is the household-level average attendance rate for each demographic group (e.g., average attendance among young sons within the household). Estimates use local polynomial regression with bias-corrected inference following Calonico et al. (2018, 2020). The running variable is household per capita income centered at the eligibility threshold. Standard errors are clustered at the regional level. Household-level controls include: household size, urban location, proportion of male members, and household head education. Bandwidth selection follows the MSE-optimal criterion. Polynomial orders: p=1 (local linear estimation), q=2 (bias correction).

6.2 Decomposing Effects on School Attendance

One relevant question is whether the program protected the human capital accumulation of young men by allowing previously enrolled students to continue attending school, by enabling non-enrolled students to begin new educational investments or both, as this requires resources such as computers and internet access, along with costs associated with educational materials, transportation, and forgone labor income.

To test this hypothesis, I exploit the PNAD-C panel data by matching households and individuals observed in 2019 and revisited in the second, third, or fourth quarter of 2020. The panel comprises 18,774 individuals within 6,686 single-mother households, representing 7.6 percent of the total panel sample (the same proportion as in the 2020 cross-sectional data). I construct two binary outcome variables: one indicating whether an individual started school attendance (transitioning from non-attendance in 2019 to attendance in 2020) and another indicating whether an individual stopped attending (transitioning from attendance in 2019 to non-attendance in 2020).

Figure 6a shows that program eligibility increased the probability of starting to attend school for young sons (18-24) who were previously not enrolled by 10.3 percentage points at the eligibility threshold. That is, in the context of the pandemic, they began attending educational institutions virtually. In contrast, the effects on this transition for young daughters (18-24) and boys (<18) were not statistically significant. For girls (<18), the program reduced the probability of starting a virtual course by 3.9 percentage points.

Likewise, Figure 6b shows that program eligibility reduced the probability of stopping attendance by 9.6 percentage points for young men (18-24) but increased this probability by 8.6 percentage points for young daughters (18-24). In contrast, the program had no statistically significant effect on the probability of withdrawing from school for children under 18.

So what are these young men studying that started attending in 2020? 84.51 percent declared they were attending a high-school equivalent education, while the remaining 15.49 percent declared attending a tertiary level institution, which seems to suggest that the transfer enabled these young men to return to school after dropping out before finishing it.

These results suggest that the effects of AE on human capital accumulation were mainly driven not only by young men for which the probability of withdrawing was reduced but also by the ones that had not previously completed their secondary education and decided to do it as a result of the UCT during the pandemic. Consequently, it is important to consider the role of access to virtual education, as the effects might be driven not only by the reduction of liquidity constraints in beneficiary households but also by the expanded availability and accessibility of virtual programs that were less well-known or accessible before the pandemic outbreak.

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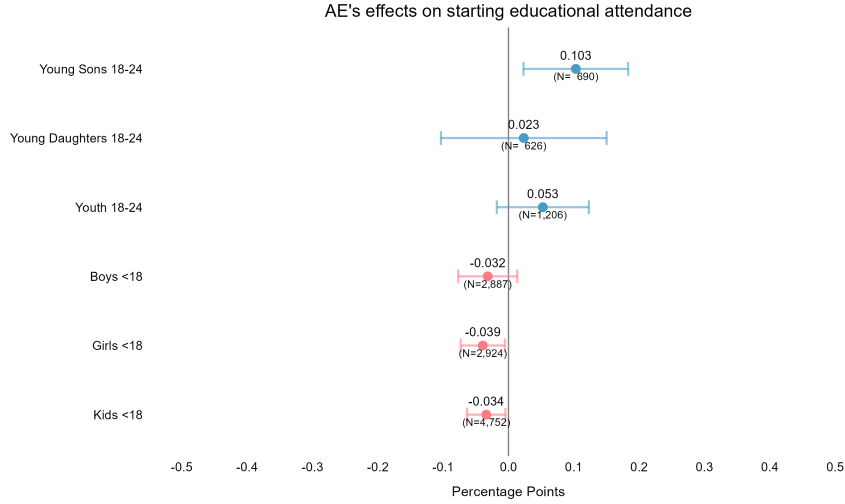
(a) Started Attending School
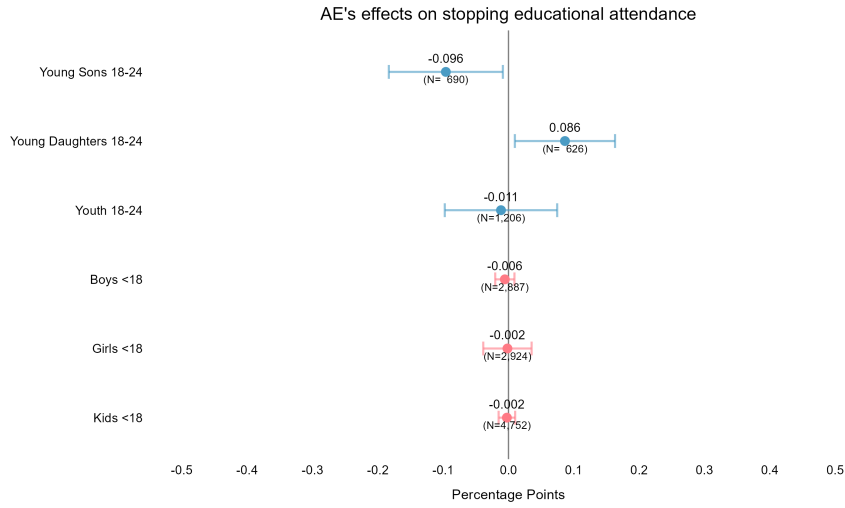
(b) Stopped Attending School

Figure 6: EA Effects on Schooling Transitions Among Children in Single-Mother House-holds. Panel (a) shows the effect on starting school attendance (transitioning from not attending in 2019 to attending in 2020). Panel (b) shows the effect on stopping school attendance (transitioning from attending in 2019 to not attending in 2020).
Source: Own construction using household-level panel data from single-mother-led households, PNAD-C (2019-2020).
Note: The eligibility threshold for Auxilio Emergencial is 522.5 BRL per capita. Analysis conducted at the household level using mean transition rates among children of the household head. Estimates use local polynomial regression discontinuity with bias-corrected inference following Calonico et al. (2014, 2020). Standard errors are clustered at the regional level. Household-level controls include household size, urban location, and proportion of male children. Bandwidth selection follows the MSE-optimal criterion. Polynomial orders: p=1 (local linear estimation), q=2 (local quadratic bias correction).

Why do young women in treated households have a higher probability of dropping out? 65 percent of the ones that dropped out were in tertiary education in 2019, so one plausible hypothesis is that the quality issues of virtual learning could have incentivized them to stop learning and wait until universities opened again or were better adapted for

virtual teaching. However, these hypotheses are not tested in the paper.

## 6.3 Effect of AE on Labor Market Outcomes

The theoretical framework in Section 2 implies that unconditional cash transfers could mitigate the effects of socioeconomic shocks on household intertemporal human capital accumulation. It also discusses how a cash transfer can lead to reduced labor market participation within households if leisure is considered a normal good. Moreover, because the program operates at the household level, this effect might not only occur among youth but may lead to a cross-substitution effect on time allocation of adults, assuming that work activities of children and adults are substitutes within the household (Parker and Todd, 2017).

As shown in Figure B.1, AE did not have statistically significant effects on the labor market participation or employment of children. Child labor is around 5 percent for both treated and untreated groups. Nevertheless, this result seems less trustworthy as families are likely not to report child labor. On the other hand, Figure 7 presents the effects of AE on labor force participation and on the desire to work additional hours or obtain additional employment. The results show that program eligibility reduced the probability of labor market participation by 23.3 percentage points for single mothers (household heads) and by 25.2 percentage points for adults aged 25–64 years. For young women, the point estimate of the reduction exceeded 13.0 percentage points, though this estimate is imprecise and the bias-corrected confidence interval includes zero. Similarly, Figure 7b shows that program eligibility reduced the probability of desiring to work additional hours or obtain an additional job by 11 percentage points for household heads and 10 percentage points for adults aged 25–64 years. The point estimate for young women is also at 10 percentage points, but it is not statistically significant at the 95% confidence level.

Figures 4 and 7 show that the groups experiencing reduced labor market participation are not the same groups exhibiting increased human capital accumulation, a pattern consistent with coordinated household decision-making. The positive attendance effects are driven by young individuals (mainly men) in a life stage where human capital accumulation generates higher long-run returns, while the reductions in labor market participation are driven by single-mother household heads and adults aged 25–64, who are likely the primary income earners in households facing a labor market crisis. These results are also consistent with the two-period model proposed in Section 2. Notably, young men, who drive the attendance effects, show no significant changes in labor market outcomes, suggesting that access to virtual education enabled them to enroll in school without altering their labor market behavior (whether employed or not working).[16]

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[16]Figure B.1 presents the heterogeneous effects of program eligibility on labor force participation, em-
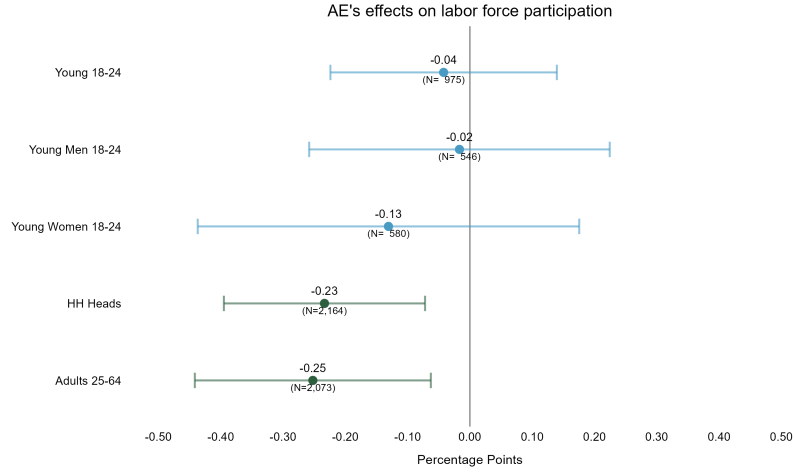
(a) Labor Market Participation
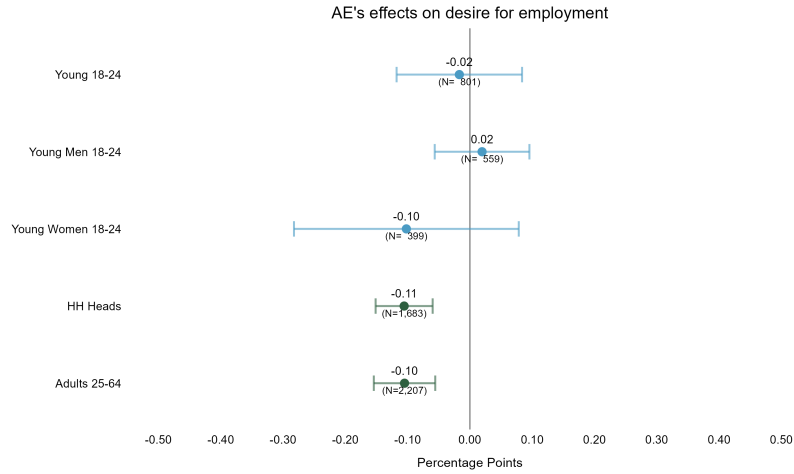
(b) Desired for additional job

Figure 7: EA Effects on Labor Market Participation. Panel (a) shows the effect on labor marquet participation. Panel (b) shows the effect on declaring the desire for working more hours or an additional job.

Source: Own construction using data from single-mother-led households, PNAD-C (2019-2020).

Note:The eligibility threshold for Auxilio Emergencial is 522.5 BRL per capita. Estimates use local polynomial regression, adjusting for mass points in the running variable. Bias-corrected estimates are reported following Calonico et al. (2018, 2020). Standard errors are clustered at the regional level. Controls include gender, household head status, presence of children, household size, urban location, and household head education. Bandwidth selection follows the MSE-optimal criterion (mserd). Polynomial orders: p=1 (local linear estimation), q=2 (bias correction).

ployment, desire for additional work, and unemployment. These results show that the labor market effects are concentrated among adults and household heads, and that the employment effects mirror those for labor force participation. The effects on unemployment are positive but not statistically distinguishable from zero, suggesting that the program induced labor force withdrawal rather than creating job-finding difficulties.

6.4 Testing Labor Market Mechanism Using Panel Data

As in Section 6.2, this section exploits panel data constructed from the PNAD-C by applying the empirical strategy discussed in Section 5 to outcome variables describing transitions in workers' formality status and labor force participation. Figure 8a shows the program had no statistically significant effects on workers' transitions to formal positions, although the point estimate for young men is 10 percentage points. In contrast, Figure 8b shows that the program significantly reduced the probability that eligible household heads and adults transitioned to informal positions between 2019 and 2020 by 9 percentage points. [17]

These findings suggest that cash transfers protected beneficiaries from accepting low-quality jobs during the emergency, as informal employment is associated with lower productivity, limited benefits, and weaker social protection mechanisms.

Figure 8c indicates the program had no statistically significant effects on labor force entry (transitioning from non-participation in 2019 to participation in 2020), but the point estimate for young men was a 12 percentage point increase. In contrast, Figure 8d shows the program increased the probability of labor force exit by 13.3 percentage points for adults and 12.0 percentage points for household heads. The transfer not only fostered human capital accumulation among young household members but also enabled primary earners to avoid low-quality informal jobs.

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[17]Following the World Bank (2025) definition for productive informality, this analysis classifies as informal those workers who hold salaried jobs in firms with fewer than five employees, are self-employed without education beyond high school, or are unpaid family workers.

(a) Transition to formal position   (b) Transition to informal position

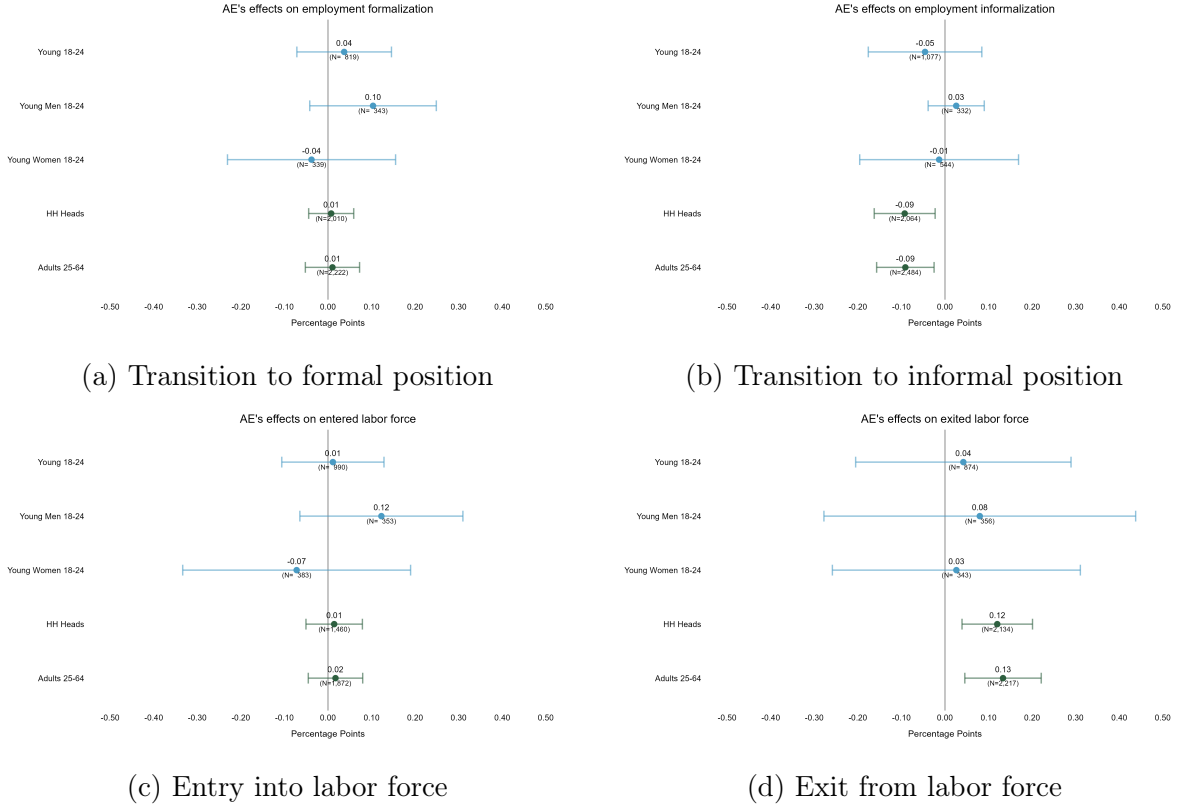(c) Entry into labor force   (d) Exit from labor force

Figure 8: EA Effects on Labor Market Transitions. Panel (a) shows the effect on transitions to formal employment. Panel (b) shows the effect on transitions to informal employment. Panel (c) shows the effect on labor force entry. Panel (d) shows the effect on labor force exit.
Source: Own construction using data from single-mother-led households, PNAD-C (2019-2020).
Note:The eligibility threshold for Auxilio Emergencial is 522.5 BRL per capita. Estimates use local polynomial regression, adjusting for mass points in the running variable. Bias-corrected estimates are reported following Calonico et al. (2018, 2020). Standard errors are clustered at the regional level. Controls include gender, household head status, presence of children, household size, urban location, and household head education. Bandwidth selection follows the MSE-optimal criterion (mserd). Polynomial orders: p=1 (local linear estimation), q=2 (bias correction).

7 Robustness Checks

7.1 Difference-in-Discontinuities Design (DiDC)

A potential concern with the Regression Discontinuity Design is that estimates at the cutoff may be biased if previous interventions also vary around the eligibility threshold. For instance, if single-mother households below the same threshold received other socioeconomic support prior to AE implementation, the estimated program effect would be confounded by these pre-existing policies.

To the best of my knowledge, no other policy varied at this threshold prior to AE implementation. To test this, I implement a Difference-in-Discontinuities design (DiDC) following Picchetti et al. (2024), using the PNAD-C panel data exploited in Sections 6.2 and 6.4. The DiDC method eliminates the effects of time-invariant confounders at the discontinuity by exploiting temporal variation in outcomes around the threshold.

The DiDC estimand differences RD treatment effects across pre-treatment (2019) and post-treatment (2020) periods:

$$\tau_{DiDC} = [E[Y_1|R \geq c] - E[Y_1|R < c]] - [E[Y_0|R \geq c] - E[Y_0|R < c]],\qquad (11)$$

where subscripts denote post- and pre-treatment periods, R represents the running variable (per capita income), and c is the eligibility cutoff.

Identification requires four assumptions: (i) *RDD continuity* of potential outcomes in the running variable—ensuring that discontinuities at the threshold reflect treatment rather than discontinuous shifts in other determinants; (ii) *treatment probability discontinuity* at the cutoff—providing the necessary variation in treatment assignment that identifies the local treatment effect; (iii) *time-invariant confounders* stable across periods—the key DiDC assumption that allows temporal differencing to eliminate time-constant confounding factors at the discontinuity; and (iv) *functional form restrictions* on the data-generating process around the threshold—ensuring that derivatives of conditional expectations behave predictably, which guarantees the zero asymptotic bias property when combined with time-invariance.

Following Picchetti et al. (2024), I estimate this parameter using local polynomial regression of outcome differences (post minus pre) around the threshold, employing the robust bias-corrected inference procedures described in Section 5.Under time-invariant data-generating processes on both sides of the threshold, DiDC achieves zero asymptotic bias (Picchetti et al., 2024). [18]

Figure 10 presents the Difference-in-Discontinuities (DiDC) estimates of the AE's effect on school attendance for young men, where the point estimate remains consistent with the cross-sectional results detailed in Sections 6.1 and 6.2: eligibility is associated with an increase of 13.9 percentage points in attendance for young men. However, the estimate is insignificant at the 95% confidence level, reflecting the efficiency loss inherent in DiDC's differencing operation—which incorporates outcome variances from both periods—combined with the smaller sample size resulting from attrition in the panel data.

The consistency between DiDC and cross-sectional RDD estimates suggests that no time-invariant confounders operated at the eligibility threshold prior to AE implementation. Given this absence of confounding, the main cross-sectional specification is preferred for inference. The cross-sectional approach offers three advantages: it leverages the larger sample, avoids potential bias from panel attrition and self-selection, and circumvents the efficiency loss inherent in DiDC's differencing operation.

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[18]This "RDD of differences" approach is preferred over the "difference of RDDs" method because it directly accounts for within-individual correlation across periods.

DiDC Estimates of AE's Effect on Delta Attendance (BC Estimate, CL SE)

Panel Data | Outcome: Change in Attendance (2020 - 2019) | Specification: Cluster With Controls
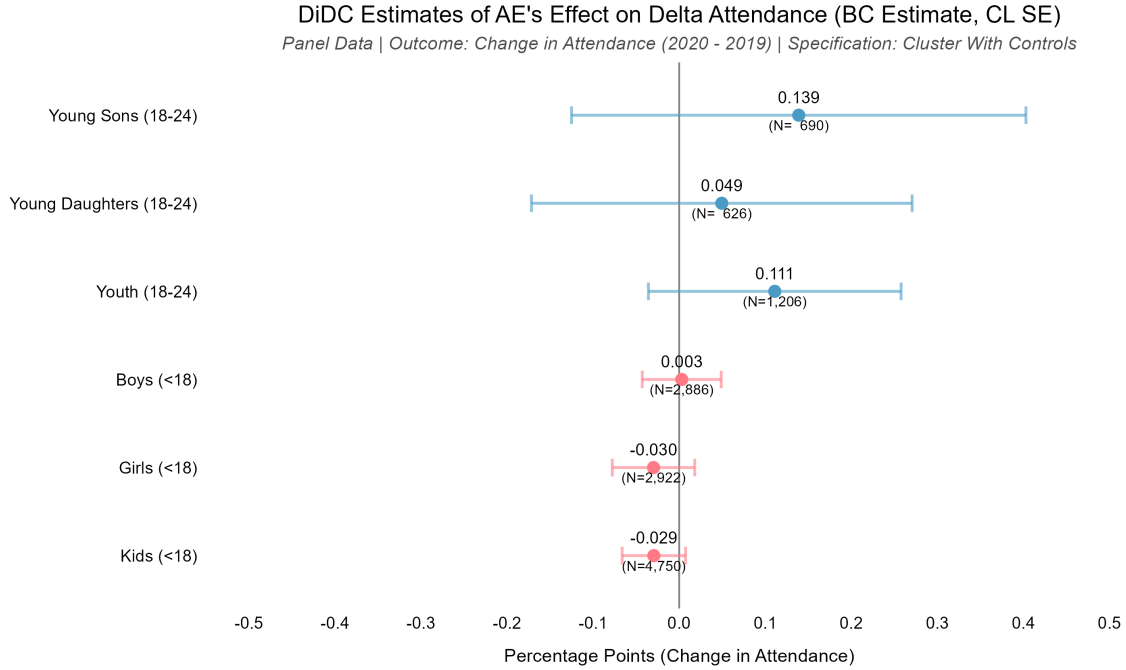
Figure 9: DIDC using MSE approach

Figure 10: DiDC Estimates of AE Effects on Attendance <small>Source: Own construction using panel data from single-mother-led households, PNAD-C (2019-2020).</small>
<small>Note: Eligibility threshold for Auxilio Emergencial: 522.5 BRL per capita. Estimates use local polynomial regression of outcome differences with bias-corrected robust inference. Shaded areas represent 95% confidence intervals.</small>

7.2 Robustness to Alternative Specifications

The results presented in Section 6 implement nonparametric local polynomial regressions using the `rdrobust` command (Calonico et al., 2014) with MSE-optimal bandwidth selection (Calonico et al., 2020) and the presented point estimates and confidence intervals are the Bias-Corrected ones. These results remain robust to varying or withdrawing covariates from the estimation and to alternative polynomial degrees used in the estimation procedure.

The main specification employs local linear regression ($p = 1$) with local quadratic bias correction ($q = 2$) and includes pre-treatment covariates such as number of household members, area of residence, and household head education level. Standard errors are clustered at the region level. As shown in Figure 11, the estimates of the effect on educational assistance are robust to alternative specifications without controls and to the alternative `msetwo` bandwidth selector, which allows for two different MSE-optimal bandwidths below and above the cutoff.[19]

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[19]The `mserd` method offers the advantage of imposing a common bandwidth constraint that can improve efficiency when treatment effects are symmetric around the cutoff, while `msetwo` provides greater flexibility by allowing asymmetric bandwidths, which may better capture heterogeneous treatment effects or differences in data density across the threshold. All specifications adjust for mass points in the running variable and report bias-corrected estimates with robust standard errors following Calonico et al. (2018, 2020).

Effects on Educational Attendance: Method Comparison

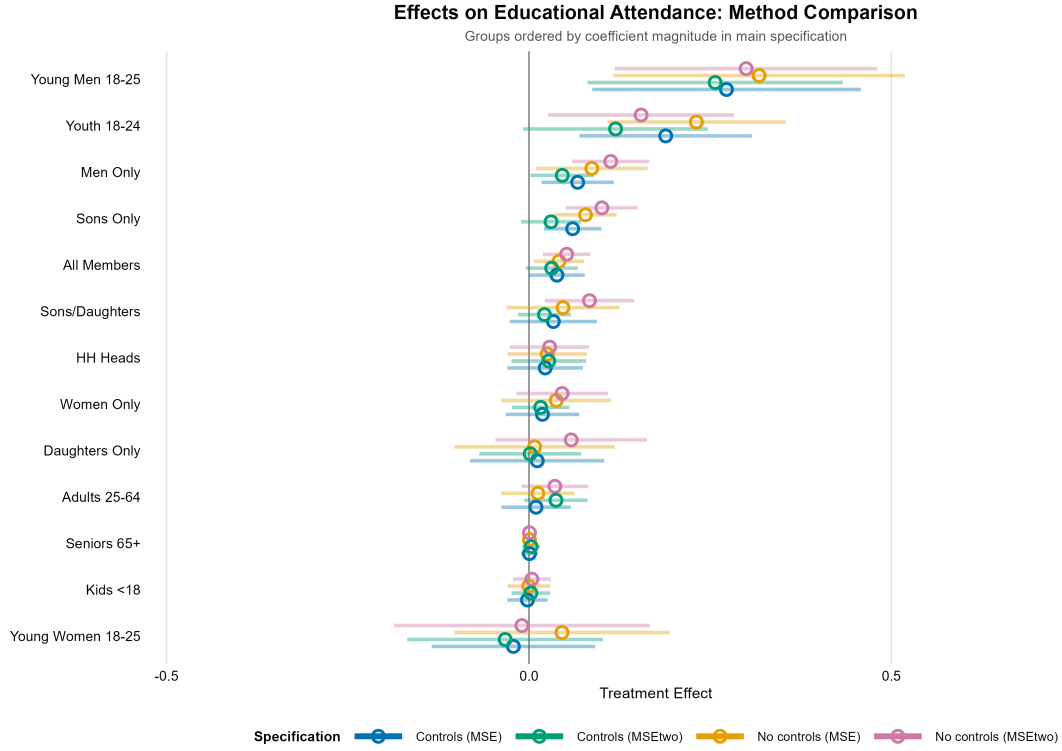Groups ordered by coefficient magnitude in main specification

Figure 11: AE Effects on Educational Attendance by Estimation Method
Note: Source: Own construction using data from single-mother-led households, PNADC (2020). Eligibility threshold for AE: 522.5 BRL per capita.

The main results remain robust to alternative specifications. As showed in Annex B, estimates are stable when employing second-degree ($p = 2$) local polynomial regressions.

7.3   Placebo tests

Placebo tests using an alternative threshold of 300 BRL in 2020 reveal no statistically significant discontinuities in attendance (Figure 12). A placebo test using the 2019 eligibility threshold of 499 BRL similarly shows no significant effects on attendance for any subgroup. Combined with the evidence in Section 5.2, these findings validate our identification strategy and indicate that the observed discontinuities at the eligibility threshold result from program eligibility.
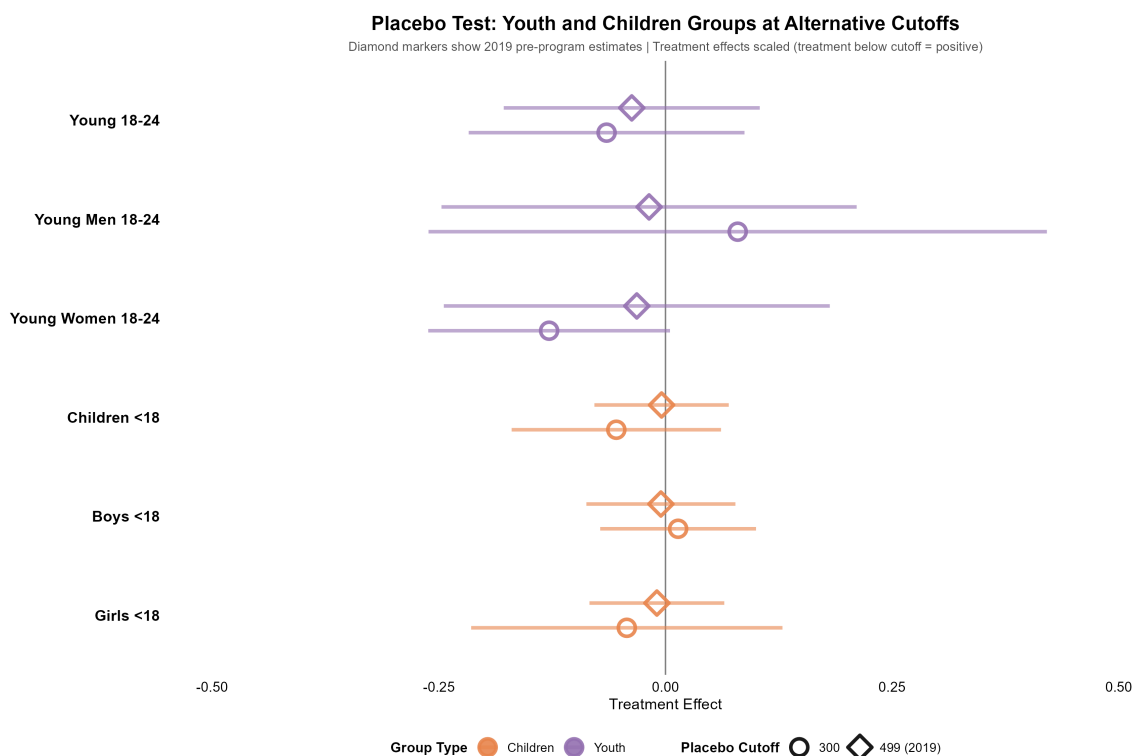
Placebo Test: Youth and Children Groups at Alternative Cutoffs

Diamond markers show 2019 pre-program estimates | Treatment effects scaled (treatment below cutoff = positive)

Figure 12: Pacebo test - AE Effects on 300 and 800 BRL cutoffs

Note: Source: Own construction using data from Single-mother-led households, PNACD (2020). Note: Eligibility threshold for Auxilio Emergencial: 522.5 BRL per capita.

8 Discussion and Policy Implications

The empirical findings indicate that the AE unconditional cash transfer program fostered human capital accumulation in Brazil during the pandemic by altering household time allocation between schooling and work. The positive effects on educational attendance were concentrated among young men aged 18–24 in single-mother-led households, specifically those who were previously unenrolled and entered secondary-level education, suggesting they resumed education they may have previously dropped. Conversely, the labor market effects were concentrated among household heads (single mothers) and adults aged 25–64.

Because most higher education institutions offered virtual instruction during the pandemic, the increased attendance among eligible households likely reflects both the direct income effect of the transfer and improved access to virtual programs that were previously unavailable or unaffordable, especially for youths with incomplete secondary education. Although these schooling improvements were available to all households, AE may have enabled eligible families to afford the associated direct costs, such as computers or in-home Wi-Fi connections, or simply made the opportunity to engage with virtual classes more visible to them.

This attendance pattern represents a direct human capital investment, as virtual

schooling entails costs including enrollment fees, internet access, and necessary equipment. Nevertheless, these estimates are local causal effects, focused on vulnerable single-mother households near the eligibility threshold. Therefore, they should not be extrapolated to households at significantly different income levels, with different family structures (such as married couples or father-headed families), or within countries with distinct institutional and educational contexts.

The results demonstrate that labor market participation, employment, and willingness to seek additional work decreased among primary earners in eligible households. The program thus altered labor market dynamics within recipient households. While labor supply decreased among eligible households overall, young women, adults, and household heads experienced higher probabilities of securing formal employment contracts, permanent positions, and high-skill occupations—all characteristics associated with employment quality. The program also reduced the probability of transitioning into informal employment for these groups.

The estimated effects on household-level labor market participation support the proposed mechanism: income transfers mitigate crisis-induced increases in household discount rates that would otherwise discourage long-term human capital investment. The program enabled families to reduce immediate income-generating activities while facilitating new educational investments, consistent with the income stabilization channel proposed in the theoretical framework.

These results demonstrate that the program protected primary earners from accepting low-quality jobs due to financial constraints and fostered human capital formation among young men. The gender disparity in effects is puzzling: while the program increased attendance for young men by more than 10 percentage points, it had null effects on young women's attendance, despite their already lower baseline attendance rates.

These findings have relevant policy implications at both household and economy-wide levels. For vulnerable households, income shocks can substantially reduce long-term human capital formation as the opportunity cost of studying relative to working increases. The larger effects observed among young adults aged 18–25 indicate that emergency transfers may be especially effective in preventing human capital losses at key educational transitions, when opportunity costs of continued schooling are highest.

However, the concentration of these positive effects among males, and the failure to increase already low attendance rates for young women, should inform future policy design. This gender gap should be interpreted cautiously: the effect is primarily driven by secondary school dropouts, a group for whom women have a lower initial probability of belonging. Furthermore, the discrepancy may reflect preferences for in-person learning or increased caregiving overloads for young women in their households. Nevertheless, addressing the barriers that prevent women from participating equally in human capital formation is essential for reducing labor market gaps in Brazil and the region, as reduced

educational attainment perpetuates income and social mobility hurdles for women.

From a macroeconomic perspective, preventing educational disruption during widespread crises preserves aggregate human capital stocks that drive long-term growth. Since the pandemic simultaneously affected millions of vulnerable households, even modest individual-level effects can translate into economically significant aggregate accumulation of human capital.

This research has several limitations that must be considered when interpreting these findings. First, the regression discontinuity design identifies local treatment effects around the eligibility threshold, limiting external validity for households at different income levels and in contexts different from Brazil during the Covid-19 pandemic. Second, the analysis captures short-term protective effects during the transfer period but cannot assess long-term effects after program termination. Third, while the study documents attendance preservation, it cannot measure potential learning losses from school disruptions or variations in remote instruction quality during the pandemic.

Despite these constraints, the evidence demonstrates that emergency UCTs of sufficient magnitude—in this case, transfers initially equivalent to full minimum wages—can foster educational investments and reduce engagement in low quality work activities. This finding challenges the conventional view that UCTs necessarily underperform CCTs for educational outcomes in the short run, suggesting instead that transfer generosity and implementation context fundamentally shape program effectiveness, as documented in recent influential literature (Balboni et al., 2022).

9 Conclusion

This study demonstrates that emergency unconditional cash transfers can foster human capital accumulation for young adults during economic crises and prevent primary household earners from accepting informal or low-quality employment due to financial constraints. Using a Regression Discontinuity (RD) design to exploit one of the world's largest emergency cash transfer programs during the COVID-19 pandemic, I find that eligibility increased educational attendance by 16.0 percentage points for young adults in vulnerable single-mother households. These effects are driven by young men who had not completed their secondary studies. The mechanism operates through a reduction in economic pressure on the household, which enables young adults to resume their secondary education while simultaneously allowing household heads to avoid low-quality employment and increase their probability of obtaining formal, stable, and high-skill positions.

These findings demonstrate that large-scale unconditional cash transfers can preserve human capital investments when vulnerable families face severe income disruptions during crises. While these estimates represent local intention-to-treat effects around the eligibility threshold, they provide policy-relevant evidence for emergency social protection

design. The results suggest that substantial unconditional transfers serve a dual function during crises: providing immediate household relief while protecting the long-term development prospects of vulnerable populations by preventing educational disruption at critical junctures.

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Appendix

A Descriptive Statistics

Table A.1: Pre-treatment Descriptive Statistics: Household-Level Averages (All Members)

	Total			Eligible (≤ 522.5)			Non-Eligible (> 522.5)			Diff.
Variable	Mean	SD	N	Mean	SD	N	Mean	SD	N	(E-NE)
Demographic Characteristics										
HH avg: Proportion male	0.32	0.24	7440	0.34	0.23	2727	0.31	0.24	4713	0.04***
HH avg: Age	22.95	7.09	7440	19.49	5.74	2727	24.95	7.03	4713	−5.47***
HH avg: Urban	0.84	0.37	7440	0.77	0.42	2727	0.89	0.32	4713	−0.12***
Household Structure										
HH avg: Number of children	1.64	0.89	7440	2.14	1.06	2727	1.35	0.61	4713	0.79***
HH avg: Household members	3.23	1.28	7440	3.70	1.41	2727	2.95	1.12	4713	0.74***
Education										
HH avg: Educational attendance	0.50	0.23	7440	0.55	0.23	2727	0.47	0.23	4713	0.08***
HH avg: Incomplete primary	0.55	0.28	7440	0.67	0.25	2727	0.48	0.27	4713	0.19***
HH avg: Complete primary	0.04	0.11	7440	0.04	0.11	2727	0.04	0.12	4713	0.00*
HH avg: Incomplete secondary	0.14	0.20	7440	0.12	0.18	2727	0.15	0.21	4713	−0.03***
HH avg: Complete secondary	0.17	0.21	7440	0.14	0.19	2727	0.19	0.23	4713	−0.05***
HH avg: Incomplete tertiary	0.05	0.13	7440	0.02	0.09	2727	0.06	0.14	4713	−0.04***
HH avg: Complete tertiary	0.06	0.15	7440	0.01	0.06	2727	0.09	0.18	4713	−0.08***
HH avg: Max education level[a]	3.67	1.53	7440	3.05	1.40	2727	4.02	1.49	4713	−0.98***
Economic Variables										
HH avg: Labor force part.	0.57	0.39	7440	0.45	0.41	2727	0.64	0.36	4713	−0.20***
HH avg: Employed	0.45	0.40	7440	0.26	0.36	2727	0.55	0.38	4713	−0.29***
HH avg: Number employed	0.83	0.76	7440	0.49	0.66	2727	1.02	0.75	4713	−0.53***

Source: Own construction, PNAD-C (2020, Q2-Q4). Household-level averages across all members.
[a] Maximum education level: categorical variable from 0 (incomplete primary) to 6 (complete tertiary).
* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Difference = Eligible mean - Non-eligible mean.
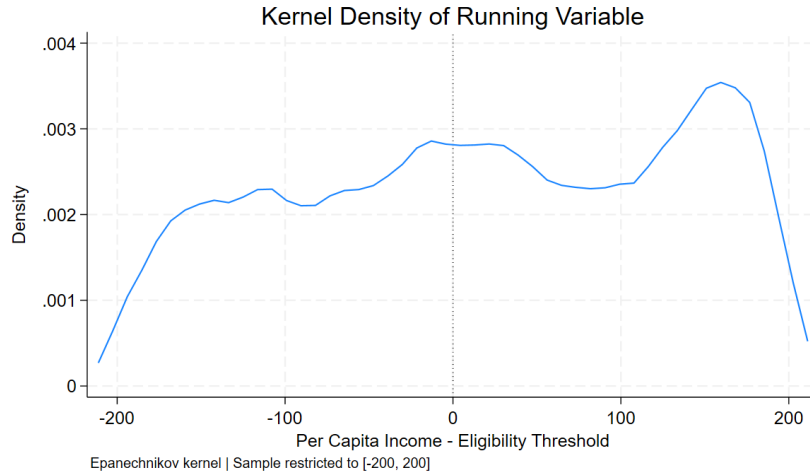Joint significance test for covariate balance: F-statistic = 35.43 (df = 4), p-value = 0.002.

Kernel Density of Running Variable

Epanechnikov kernel | Sample restricted to [-200, 200]

Figure A.1: Kernel Distribution of Per Capita Household Income Aroung the elegibility Threshold

Note: This figure provides a visual examination of the distribution of the running variable around the eligibility threshold. The plot employs an Epanechnikov kernel.

Table A.2: Balance Test: Pre-treatment Covariate Means by Eligibility Status of All Household Members

Variable	Overall Mean	Overall SD	Eligible Mean	Eligible SD	Noneligible Mean	Noneligible SD	Difference
Male	0.35	0.48	0.34	0.47	0.37	0.48	-0.036* (0.021)
Number of children	1.83	0.77	1.87	0.70	1.77	0.87	0.107* (0.064)
Household members	3.70	1.22	3.64	1.20	3.78	1.25	-0.140*** (0.054)
Urban	0.80	0.40	0.76	0.42	0.86	0.35	-0.094*** (0.017)
Maximum education	1.56	0.60	1.57	0.61	1.55	0.59	0.020 (0.026)
Incomplete primary	0.65	0.48	0.66	0.47	0.63	0.48	0.033 (0.021)
Complete primary	0.04	0.19	0.03	0.18	0.04	0.20	-0.009 (0.009)
Incomplete secondary	0.12	0.33	0.12	0.33	0.13	0.33	-0.005 (0.015)
Complete secondary	0.14	0.35	0.13	0.34	0.16	0.36	-0.024 (0.015)
Incomplete tertiary	0.03	0.17	0.03	0.17	0.03	0.16	0.004 (0.007)
Complete tertiary	0.02	0.13	0.02	0.13	0.02	0.13	0.001 (0.006)
Maximum education level	3.28	1.40	3.25	1.42	3.33	1.38	-0.082 (0.061)
Joint significance test			$\chi^2 = 9.42$, df $= 4$, p $= 0.214$				
Observations (total)	17,243						
Observations (in bandwidth)	2,128		1,216		912		
Bandwidth	50 BRL						

Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Eligible = Below threshold (income < 522.5 BRL); Non-eligible = Above threshold.

Source: Own calculations using PNAD-C microdata for quarters 2, 3, and 4 of 2020.

Note: The joint significance test is based on a regression of the eligibility indicator on all covariates with region-clustered standard errors; the chi-squared statistic is computed as F-statistic $\times$ degrees of freedom.

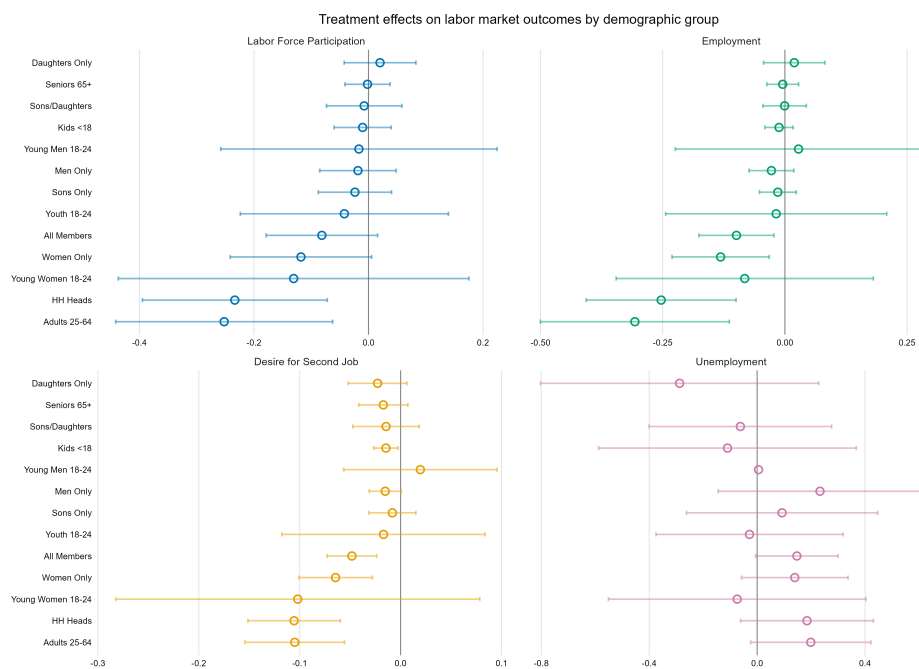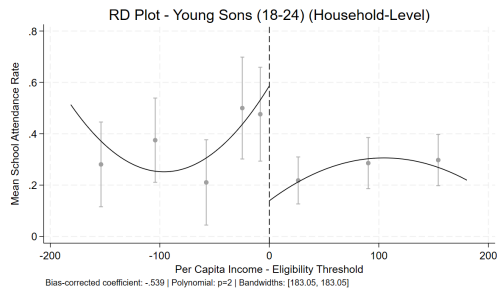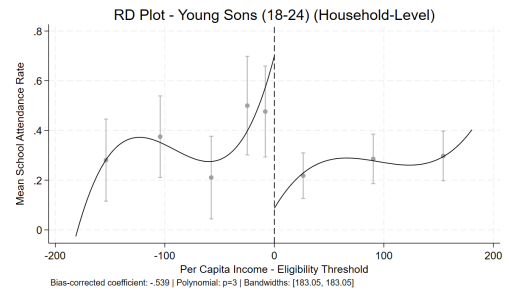# B Regression Discontinuity Estimates and plots

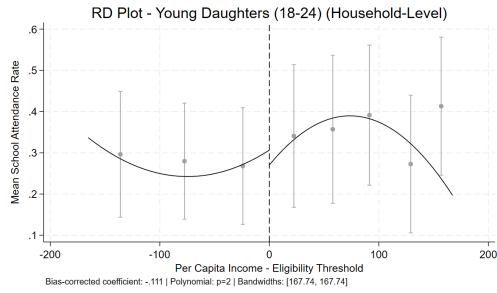Figure B.1: EA effects on Labor Market Outcomes by Group

Note: Source: Own construction using data from Single-mother-led households, PNACD (2020). Note: Eligibility threshold for Auxilio Emergencial: 522.5 BRL per capita.
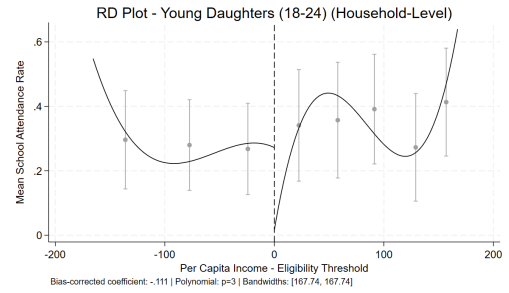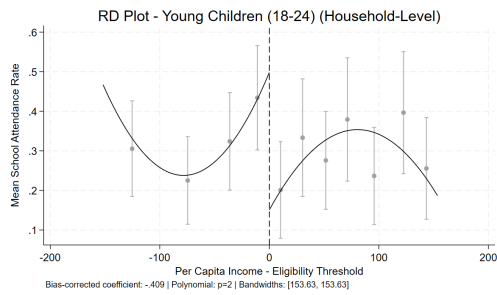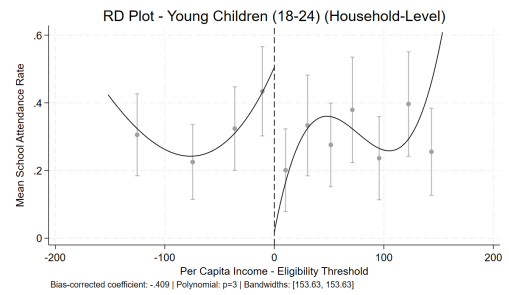
(a) Young Men 18-24 (p=2)
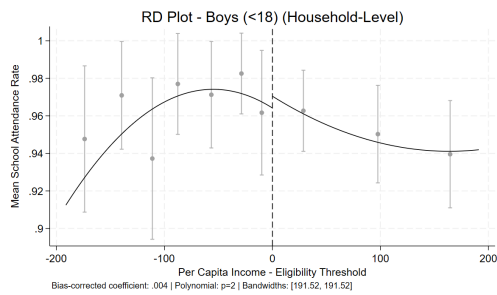
(b) Young Men 18-24 (p=3)

(c) Young Women 18-24 (p=2)
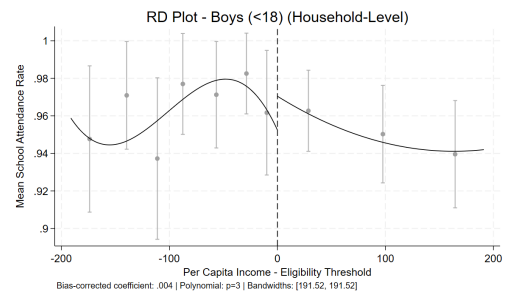
(d) Young Women 18-24 (p=3)

(e) Youth 18-24 (p=2)

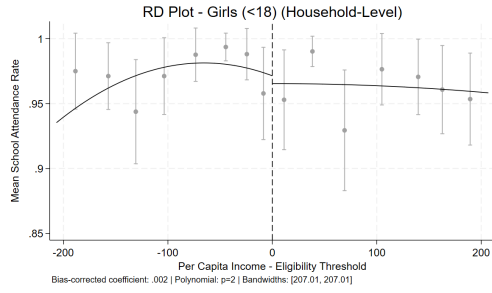(f) Youth 18-24 (p=3)

(g) Boys Under 18 (p=2)
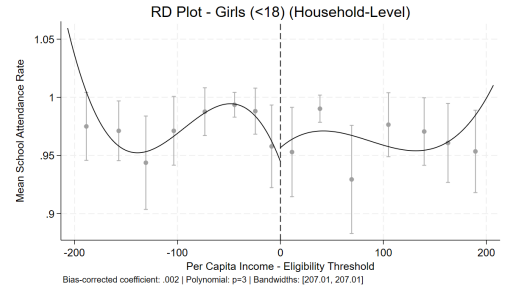
(h) Boys Under 18 (p=3)

Figure B.2: Regression Discontinuity Estimates by Polynomial Order. Left column displays RD plots using local quadratic regression (p=2) and the right column displays RD plots using p=3. Each panel presents the estimated discontinuity in school attendance at the eligibility threshold using MSE-optimal bandwidth selection with quantile-spaced binning.

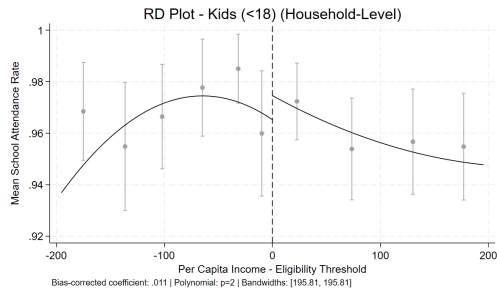**Source:** Own construction using PNACD (2020) data.

RD Plot - Girls (<18) (Household-Level)

Bias-corrected coefficient: .002 | Polynomial: p=2 | Bandwidths: [207.01, 207.01]

(a) Girls Under 18 (p=2)

RD Plot - Girls (<18) (Household-Level)

Bias-corrected coefficient: .002 | Polynomial: p=3 | Bandwidths: [207.01, 207.01]

(b) Girls Under 18 (p=3)

RD Plot - Kids (<18) (Household-Level)

Bias-corrected coefficient: .011 | Polynomial: p=2 | Bandwidths: [195.81, 195.81]

(c) Children Under 18 (p=2)

RD Plot - Kids (<18) (Household-Level)

Bias-corrected coefficient: .011 | Polynomial: p=3 | Bandwidths: [195.81, 195.81]

(d) Children Under 18 (p=3)

Figure B.3: Regression Discontinuity Estimates by Polynomial Order. Left column displays RD plots using local quadratic regression (p=2) and the right column displays RD plots using p=3. Each panel presents the estimated discontinuity in school attendance at the eligibility threshold using MSE-optimal bandwidth selection with quantile-spaced binning.

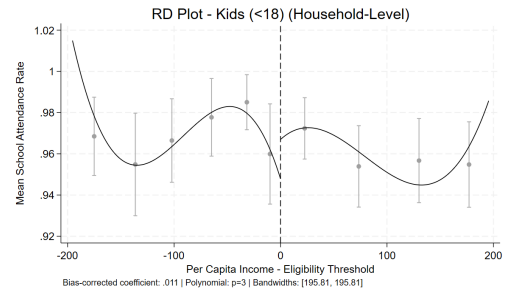**Source:** Own construction using PNACD (2020) data.