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Abstract *

This paper studies the effects of changes in local public education budgets on individual schooling attainment and migration, as well as on local labor market outcomes. I leverage the introduction of FUNDEF, a large federal program that redistributed public education finance across Brazilian municipalities in the late 1990s, as a source of exogenous variation. Using a cohort-exposure design, I find that, at the individual level, doubling the program-related public education budget led to a 1.4 percentage point increase in the likelihood of completing primary school, and a 0.5 percentage point decrease in the likelihood of staying in the local labor market among exposed cohorts, on average. The mobility effects are concentrated among individuals educated in municipalities that received a positive budget shock as a result of the program, which were also characterized by relatively worse local labor market conditions. At the local labor market level, difference-in-differences estimates suggest that higher public education budgets were associated with *lower* employment rates and average wages, suggesting that the "brain drain" effect depressed local labor demand in the long run.

JEL classifications: I2, O15, R23.

Keywords: school spending, schooling attainment, migration.

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1 Introduction

A growing body of literature documents that increased school spending positively impacts educational and labor market outcomes (Jackson, 2020). Most prior studies, however, overlook the spatial dimension of labor markets. This is relevant because, in most countries, there are few restrictions on migration. Moreover, various studies find a positive selection of educated individuals into migration (Dahl, 2002)—particularly from less to more developed regions (e.g., Young, 2013)—suggesting that increased school spending, by enhancing schooling, may also lead to increased migration from lagging regions. Recent research focusing on the effects of school construction, in particular, has found either positive effects on migration (Akresh et al., 2023), or that the availability of migration destinations helps explain heterogeneous effects on labor market outcomes (Hsiao, 2023). This paper studies the migration effects of changes in public education budgets and their implications for local labor markets.

To capture exogenous changes in local education budgets, I use FUNDEF, a large federal policy enacted in the late 1990s in Brazil. The program redistributed significant resources earmarked for primary and middle school education across municipalities within states. Because of the resource allocation rules and the timing of the policy announcement and implementation, the municipality-level changes in education resources produced by the policy in its first year were unpredictable and uncorrelated with local policy preferences (Bertoni et al., 2023; Estevan, 2015; Menezes-Filho and Pazello, 2006).

Using census data and the variation of the year of introduction of the program (1997) as a quasi-exogenous shock to public school budgets, I implement a cohort-exposure difference-in-differences design. This design takes advantage of the fact that an individual's age at the time of implementation mediates their exposure to the program. This approach was introduced by Duflo (2001) to study the effects of a large school construction program in Indonesia and has been employed by multiple subsequent studies on the long-term effects of this program (e.g., Akresh et al. 2023; Duflo 2004; Hsiao 2023). It has also been used in the U.S. context to study the effects of school spending (Jackson et al., 2016). In the case of FUNDEF, individuals who were of middle-school age or younger in 1998 were potentially exposed, whereas individuals who were older than middle-school age were not. I use policy-based predicted changes in education spending rather than the actual changes to avoid potential unobserved confounders related to the actual spending decision.

I start by showing that the program led to an increase in the educational attainment of the individuals exposed to the policy. The "FUNDEF shock"—i.e., the size of the policy-related changes in local public education budgets—led to higher educational attainments among the cohorts that were, in principle, exposed to the policy, compared to the cohorts that

were not. A policy-driven 100 percent increase in the education budget in the individual's municipality of education was associated with a 1.4 percentage points higher likelihood of completing at least primary school. The effects on completing middle school or higher educational levels were small and not statistically significant.

I then examine the effects of the program on migration. I find that, on average, doubling the program-related public education budget led to a 0.5 percentage point lower likelihood of remaining in the same local labor market in which they were educated among individuals exposed to the program relative to those who were not. Both the schooling and the migration effects increased with years of exposure.

The impact of schooling and migration varies based on whether individuals received their education in municipalities that financially benefited from the program or those that contributed funds. In municipalities that gained a budgetary boost through FUNDEF—typically characterized by poorer labor market conditions and lower educational standards—exposed cohorts exhibited significant migration effects, despite the program having a comparatively modest effect on their educational achievements. Conversely, in municipalities that experienced a negative education budget shock—which had, on average, better labor market conditions—individuals experienced larger effects on their schooling attainment, but this did not result in a statistically significant change in their propensity to migrate.

In the last part of the study, I explore the effects of changes in the local public education budget on regional-level schooling and labor market outcomes. Using a difference-in-differences design, I find that FUNDEF had a small positive impact on primary educational attainment of the local labor force and was associated with worsening average local wages and employment rates. Taken together, these results suggest that, while higher public education budgets led to greater schooling attainment among individuals exposed to these changes, many of them, particularly in lagging regions, did not find suitable employment opportunities and decided to emigrate. This, in turn, negatively affected labor demand in their municipalities of education.

This paper contributes to the literature on the effects of school spending on educational and labor market outcomes. Recent work has found that increases in education investments lead to higher educational attainment (Hyman, 2017; Jackson and Mackevicius, 2024; Lafortune et al., 2018; Rothstein and Schanzenbach, 2022) and better labor market outcomes (Jackson et al., 2016) in the U.S. context.[1] This paper highlights an important mechanism mediating the link between education investments and labor market outcomes, namely, the effect of these investments on individuals' likelihood of migrating to more productive regions.

[1] See Jackson (2020) for a review.

A related literature studies the geographic sorting of workers by skills, and how it affects econometric estimates of returns to schooling. More educated workers in the United States tend to migrate to places where the returns to education are larger (Dahl, 2002; Heckman et al., 1996), and where better amenities can be found (Dahl, 2002; Diamond, 2016). This generates an upward bias in OLS estimates of returns to education in local labor markets (Dahl, 2002). Recent related research has highlighted how the connection between mobility and educational attainment can affect our estimates of the labor market returns to investments in education infrastructure, such as the widely studied Sekolah Dasar INPRES school construction program in Indonesia (Akresh et al., 2023; Hsiao, 2023). My paper complements this literature by showing that shifts in local education budgets can have a migration effect. This implies that estimates of the returns to schooling based on changes in education spending may conflate returns to schooling with returns to migration.

Finally, I make a contribution to the literature on education as a driver of economic growth. While at the country level, multiple studies have failed to find a connection between human capital and growth (Pritchett, 2006), or have found it only in a subset of countries (Krueger and Lindahl, 2001), at the local level, the literature has documented a strong connection between initial schooling levels and subsequent growth in population and/or wages (Glaeser et al., 1995; Shapiro, 2006; Da Mata et al., 2007; Gennaioli et al., 2014; Chauvin et al., 2017). This may suggest that education spending can be an effective long-run local development strategy. This paper shows that such investments not only can be ineffective at improving the labor market conditions of residents but they can also lead to worsened outcomes if workers with high earning potential disproportionately leave their place of education. Education investments are likely justified given their positive effects on multiple other outcomes, including crime rates (Lochner and Moretti, 2004), health and mortality (Lleras-Muney, 2005), fertility rates, and the stability of marriages (Oreopoulos and Salvanes, 2011), to name just a few. However, their prospective effects on local economic development are not unambiguously positive.

The remainder of this paper is organized as follows. Section 2 describes the FUNDEF program and related facts about the context in which the policy was implemented. Section 3 discusses the data, measures, and empirical strategy. Section 4 presents and discusses the individual-level effects of the program, which constitute the main results of the paper. Section 5 focuses on the effect of the program at the regional level. Section 6 concludes.

2 The FUNDEF Program and Its Context

The Fund for the Maintenance and Development of Fundamental Education and the Valorization of Teaching—FUNDEF (Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério, in Portuguese)—was enacted in July of 1998 with the goal of improving the distribution and spending efficiency of basic and middle-school education within states. The reform aimed to address school-funding disparities, which had increased after a constitutional reform that took place 10 years earlier. The 1988 Constitution mandated that state and municipal governments invest at least 25% of their total revenues in public education. This led to a growing gap in the public education budget and the per-student education expenditure across high-revenue and low-revenue subnational governments (Gordon and Vegas, 2005; Estevan, 2015).

FUNDEF targeted school years 1 through 8, of which years 1 through 4 were considered primary education (educação básica) and years 5 through 8 were considered middle school education (ensino médio).[2] The municipal and state governments were the primary providers of schooling at these levels at the time, each running their own school network.[3] This is shown in Table 1, which presents a breakdown of enrollment in the year 1997 for the grades affected by the program by school network. In that year, over 34 million students were enrolled in Fundamental Education. Around 90% of students were enrolled in public schools, either state or municipal (the share of federal schools in enrollment was negligible). Within public school enrollment, about 40% was in municipal networks and 60% in state networks.

The reform consisted in a redistribution, within states, of funding across school networks (including the networks run by municipalities and the network run by the corresponding state government). FUNDEF kept in place the 1988 constitution's requirement of devoting a minimum of 25% of local governments' revenues to public education, but introduced the mandate that three-fifths of these resources (i.e., 15% of total revenues) were to be transferred to a state-level fund. This fund was subsequently redistributed to the municipal and state school networks in proportion to their prior year's share in *state-level* enrollment in school years 1 through 8 (Menezes-Filho and Pazello, 2006). In addition, the reform introduced a minimum level of spending per student. States with insufficient education budgets became entitled to receive federal transfers to be able to meet this benchmark.[4]

[2]Primary education was extended from 4 to 5 years to include kindergarten in 2003. In 2006, FUNDEF was replaced by FUNDEB (Fund for the Development of *Basic* Education and Appreciation of the Teaching Profession), which expanded the coverage of the fund to high-school education.

[3]Brazil had 5,507 municipalities and 26 states at the time the policy was implemented.

[4]The impact of these transfers on the overall policy was relatively small. In 1998, a total of 8 out of 26

Table 1: Enrollment in Fundamental Education in Brazil in 1997, by Network

Grades	Enrollment	School network			
		Municipal	State	Federal	Private
1st.	6,575,734	58.2%	33.9%	0.0%	7.9%
2nd.	5,154,094	46.7%	43.6%	0.1%	9.6%
3rd.	4,724,389	41.6%	48.1%	0.1%	10.3%
4th.	4,113,911	38.9%	49.8%	0.1%	11.2%
5th.	4,510,872	21.5%	68.0%	0.1%	10.4%
6th.	3,630,218	19.8%	68.0%	0.1%	12.0%
7th.	2,993,337	18.0%	68.2%	0.2%	13.6%
8th.	2,526,833	16.4%	68.1%	0.2%	15.4%
Primary (1-4)	20,568,128	47.6%	42.8%	0.1%	9.5%
Middle school (5-8)	13,661,260	19.3%	68.1%	0.1%	12.5%
Fundamental (1-8)	34,229,388	36.3%	52.9%	0.1%	10.7%

Source: Brazilian Education Census of 1997.

The reform also mandated that 60% of the resources were to be spent on teachers' wages, while the remaining funds could be used for eligible operation and maintenance activities (De Mello and Hoppe, 2005).

With respect to the initial impact of the reform, prior research has shown that the introduction of FUNDEF increased both the share of municipal networks in local education expenditures and the aggregate magnitude of these expenditures. The program had a "decentralization" effect, in that it transferred resources from state to municipal school networks, because municipalities had higher enrollment relative to revenues than the states did (Menezes-Filho and Pazello, 2006). In spite of this, per-student transfers increased in real terms (De Mello and Hoppe, 2005), and total municipal expenditure in education increased by an average of 8% (Menezes-Filho and Pazello, 2006), with larger relative increases observed in poorer municipalities (Cruz and Rocha, 2018). The program does not appear to have crowded out resources from other sources of financing (Gordon and Vegas, 2005).

FUNDEF also impacted teacher hiring decisions, with effects diverging depending on the direction of the shock. Municipalities whose education budget increased due to the program increased teachers' wages, whereas municipalities that experienced budget cuts hired more teachers while reducing wages (Cruz, 2018). The program, on the other

states received federal top-up transfers, which amounted to 3.7% of the total balance of the funds. By 2002, there were only 5 recipient states, with transfers accounting for 1.8% of the total funds (De Mello and Hoppe, 2005).

hand, had a relatively minor impact on the stage of education at which the funds were invested. Most municipalities were already spending 60% or more of their mandated education budget (equivalent to 15% of their total budget) on Fundamental Education. The program did lead to a small initial reduction in expenditures on pre-school education (Menezes-Filho and Pazello, 2006).[5]

The program was deployed during a period in which access to education was expanding nationally. In terms of net enrollment rates, even though Brazil was lagging behind relative to other middle-income countries by the beginning of the 2000s (De Mello and Hoppe, 2005), it had experienced an unprecedented expansion at all education stages starting in the early 1990s (Menezes-Filho, 2001; De Barros et al., 2006).

Appendix Figure A.1 shows the percentage of the adult population in each educational attainment category at the beginning and at the end of the decade following the implementation of FUNDEF. The share of the population with primary education or less decreased from 60% to 42%. Meanwhile, the share with high-school education increased from 20% to 29%, and the share with college education or higher from 7% to 13%. Prior research has shown that FUNDEF played a role in the increases in enrollment, particularly at the primary and middle school levels (Gordon and Vegas, 2005; De Mello and Hoppe, 2005; Menezes-Filho and Pazello, 2006; Cruz and Rocha, 2018).[6]

3 Data and Empirical Strategy

3.1 Data and Measures

The data used in this analysis come from multiple sources. The enrollment data come from the Brazilian School Census, and the data on taxes and transfers used to calculate the resources contributed to and received from FUNDEF are from the National and State Treasuries (Secretaria do Tesouro Nacional, STN). The rest of the outcome variables and controls are constructed from the microdata of three rounds of the decennial population censuses published by the Brazilian Institute of Geography and Statistics (IBGE) in 1991,

[5]The program also led to an increase in the re-election probabilities of mayors in municipalities that increased education expenditures due to the program (Assunção and Estevan, 2022).

[6]Other social programs introduced during the 2000s, particularly conditional cash transfers that required low-income families to enroll their children in school (Bolsa Escola and Bolsa Familia), also played a role. Existing evaluations suggest that their contribution to enrollment in fundamental education was negligible, largely because the beneficiaries of these programs already had their children enrolled in school (Schwartzman, 2005). However, they may well have had an impact—starting in the mid-2000s—at the high-school level, where enrollment was smaller. On the demand side, Bolsa Familia included stipends for youth aged 15 to 17 to attend school, while simultaneously FUNDEB expanded coverage of supply-side subsidies to high school (OECD, 2011).

2000, and 2010. Appendix Table A.4 presents summary statistics of the main individual-level variables used in the analysis.

To capture the impact of FUNDEF on local education finance, I use the program-induced proportional change in the local educational budget, following Estevan (2015). This municipality-level variable, which I refer to as the "FUNDEF shock," measures the gap between the funds received from FUNDEF in the first year of implementation of the program for all education networks operating in the jurisdiction (municipal or state-level) and the funds contributed to the program in the same year.[7] The gap is expressed as a share of the funds contributed to the program.

Formally, the municipality-level "FUNDEF Shock" is defined as:

$$FS_j = \sum_{e \in \{m,s\}} \eta^e_{j,97} \left(\frac{I^e_{j,98} - O^e_{j,98}}{O^e_{j,98}} \right) \tag{1}$$

where the units of observation are school networks operating in municipality j, denoted by the superscript $e = \{m, s\}$, which can be municipal (m) or state-level (s). The weight $\eta^e_{j,97}$ is the share of the school network e in the municipality-level enrollment in public education in 1997. The term in parentheses is the program-induced proportional change in education transfers to the school system e, where $I^e_{j,98}$ is the money that the system received from FUNDEF in 1998, and $O^e_{j,98}$ the money that it contributed to the program's state-level fund.

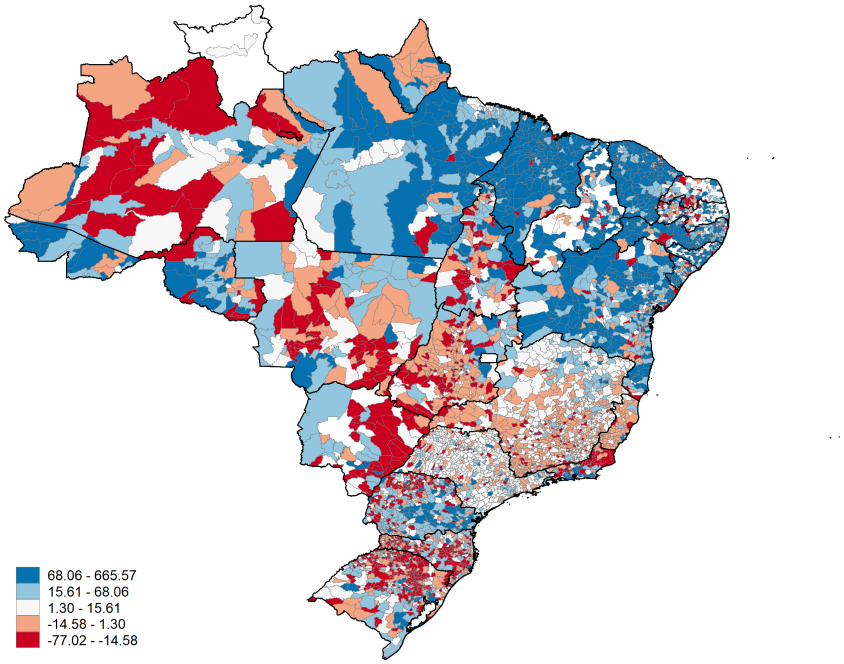
I use only the variation of the first year of the program to address potential distortions related to municipalities inflating enrollment figures to capture additional FUNDEF funds. Indeed, there is evidence showing that some municipalities did engage in this behavior in subsequent years. However, the 1998 transfers were based on data collected in 1997, before the allocation rules of the program had been announced (Estevan, 2015), and thus local authorities were unable to manipulate them.

Figure 1 depicts the geographic distribution of the FUNDEF shock. It shows that states were heterogeneous in terms of the intensity of the redistribution induced by FUNDEF across their municipalities. While in a couple of states positive and negative budget shocks were relatively small, in others the program induced sizable education budget changes at the municipal level. Appendix Figure A.2 (left) presents the distribution of the FUNDEF shock across all municipalities in the country. The average Brazilian municipality received, in the first year of FUNDEF, 28 percent more money than it contributed to its state's fund.

[7] The contributions consist of a FUNDEF-mandated 15% share of the resources received from four taxes and transfers (FPM/FPE, IPIExp, LC87/96, and ICMS), which the Constitution of 1988 had established as a regular source of revenues for subnational governments.

For municipalities that were net beneficiaries of the policy (i.e., those that experienced a positive budget shock), the shock represented an average 55 percent gain over their contributions, whereas for municipalities that were net donors (i.e., those experiencing negative budget shocks), the shock amounted to an average 16 percent loss relative to their contributions.

Figure 1: Geographic Distribution of Budget Changes Induced by the First Year of FUNDEF



Notes: FUNDEF-induced shocks to local public education budgets, estimated according to equation 1. All values are multiplied by 100 to interpret them as percentage of the resources contributed to the state-level FUNDEF fund.

3.2 Empirical Strategy

To capture the reduced-form effect of FUNDEF on individual outcomes, I take advantage of the fact that the exposure to the program varies by year-of-birth cohort and by how the program affected resources for public education in the municipalities where the individual went to school. This approach has been previously used by Duflo (2001) and Jackson et al. (2016), among others. Specifically, in my baseline specification, I estimate:

$$Y_{ija} = \beta_0 + \sum_{a=3}^{26} (FS_j \times d_{ia})\, \beta_{1,a} + C_i' \beta_{1,i} + \sum_{a=3}^{26} (C_j \times d_{ia})\, \beta_{2,a} + \beta_j + \beta_a + \epsilon_{ija} \qquad (2)$$

where the dependent variable Y_{ija} is the outcome of interest measured in 2010 for individual i, educated in municipality j, and aged a in 1998 (i.e., born in year $1998 - a$). FS_j is the FUNDEF shock in municipality j (equation 1), d_{ia} is a dummy that takes the value one if individual i was aged $a \in [3, 26]$ in 1998, C_i is a vector of individual-level controls, C_j are municipality-level controls, and β_j and β_a are municipality and cohort fixed effects, respectively. In the preferred specification, I use sex and self-declared race as individual demographic controls, since educational attainment and the propensity to migrate can differ along these characteristics. As a municipal-level control, I use the share of the population that was 14 or younger in 1997 in the municipality of education (estimated with 2000 census data). Municipalities with disproportionately larger school-age populations may have been both more likely to benefit from FUNDEF and less likely to reach all of their school-age population with their education spending.

Since 12 cohorts are, in principle, exposed to the program (those aged 3 through 14 in 1998), I use an equal-sized set of cohorts that were, in principle, not exposed as controls (those aged 15 through 26 in 1998). Individuals aged 26 in 1998 are the reference group. The youngest cohort included in the estimation was 15 in 2010, and the oldest was 38. This ensures that all individuals were, at the time the outcome was measured, old enough to have finished middle school.

To estimate the average effect of the program across exposed cohorts, I estimate the following variant of equation 2:

$$Y_{ija} = \beta_0 + \beta_1 (FS_j \times T_i) + \beta_2 C_i + \beta_3 (C_j \times T_i) + \beta_j + T_i + \epsilon_{ija} \tag{3}$$

where T_i is a dummy that takes the value one if individual i was aged 14 or younger in 1998, and the value zero if they were aged 15 or older.

In this difference-in-differences setup, the causal interpretation of the estimates $\hat{\beta}_{1,a}$ from equation 2 and $\hat{\beta}_1$ from equation 3 relies on the assumption that, in the absence of the program, the gaps in Y_{ija} across exposed and non-exposed cohorts would not have been systematically different between individuals who studied in regions with high program incidence and individuals who studied in regions with low program incidence. In Section 4, I provide evidence that suggests that this assumption holds in our context. Furthermore, using policy-based predicted changes in education spending rather than the actual changes avoids potential unobserved confounders related to the actual spending decision.

3.3 Other Social Programs As Potential Confounders

A potential threat to identification comes from the fact that, during the period of analysis, FUNDEF was not the only social program promoting education in Brazil. For example, in 1996, the Federal Government implemented the Programa de Erradicação do Trabalho Infantil (PETI) and, in 1998, it also implemented the Programa de Garantia de Renda Mínima (PGRM), which was later integrated into the Bolsa Escola, the flagship federal conditional cash transfer program implemented in 2001. At the end of 2003, those programs were integrated into the larger Bolsa Família program. Since these programs promoted enrollment by stimulating the demand for education and were targeted primarily at low-income children, my results could mistakenly attribute the observed effects to increases in schooling spending, when they are in reality shaped by demand-side policies.

Appendix Figure A.3 depicts, for the cohorts born between 1981 and 2003, the year in which they became eligible and the year in which they stopped being eligible for the most important education-related programs during the period of study. It shows that, while eligibility for many of these programs did not overlap with eligibility for FUNDEF across cohorts and time, in a few programs (in particular PETI, PGRM, and some local programs) it did, at least for some cohorts in the late 1990s. This cohort-level eligibility overlap does not necessarily imply individual exposure overlap. This is because programs differed in the territories in which they were present and in the magnitude of the intervention in each territory, so that individuals from the same cohort could have been, on average, exposed to different government interventions depending on where they were located at the time and on the specific design of each program.

To address this concern, I empirically investigate whether exposure to FUNDEF can predict exposure to demand-side programs. To this end, I use data from the 2000 census, which contains information at the household level on benefits received from these programs. I estimate equation 3 using as the dependent variable an indicator that takes the value one if the person lived in a household that was a beneficiary of any education-related social program. The results are shown in Table 2. I report results for all municipalities, and separately for municipalities that received a positive and a negative budget shock to education due to FUNDEF. Column one reports the estimates of the effects of FUNDEF on the likelihood of being a beneficiary of a different social program without controls, column two includes individual demographic controls, and column three controls for the share of the population aged 14 or younger in 1997. Reassuringly, all point estimates are small and not statistically significant, suggesting that other education-related social programs did not systematically affect the same individuals as FUNDEF in the late 1990s.

Table 2: Effects of the Initial FUNDEF Shock on the Likelihood of Being a Beneficiary of an Education-Related Social Program in 2000

	(1)	(2)	(3)
All municipalities	0.001	0.001	-0.000
	(0.001)	(0.001)	(0.001)
Positive Budget Shocks	0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)
Negative Budget Shocks	-0.003	-0.002	-0.002
	(0.008)	(0.009)	(0.009)
Demographic controls	No	Yes	Yes
14 or younger share in population in 1997	No	No	Yes

Notes: Individual-level regressions using data from the 2000 census. The sample is restricted to individuals aged 3 to 26 in 1998. The dependent variable is an indicator that takes the value one if the person lives in a household that was, in 2000, a beneficiary of any education-related social program. Robust standard errors clustered at the municipality of education level in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

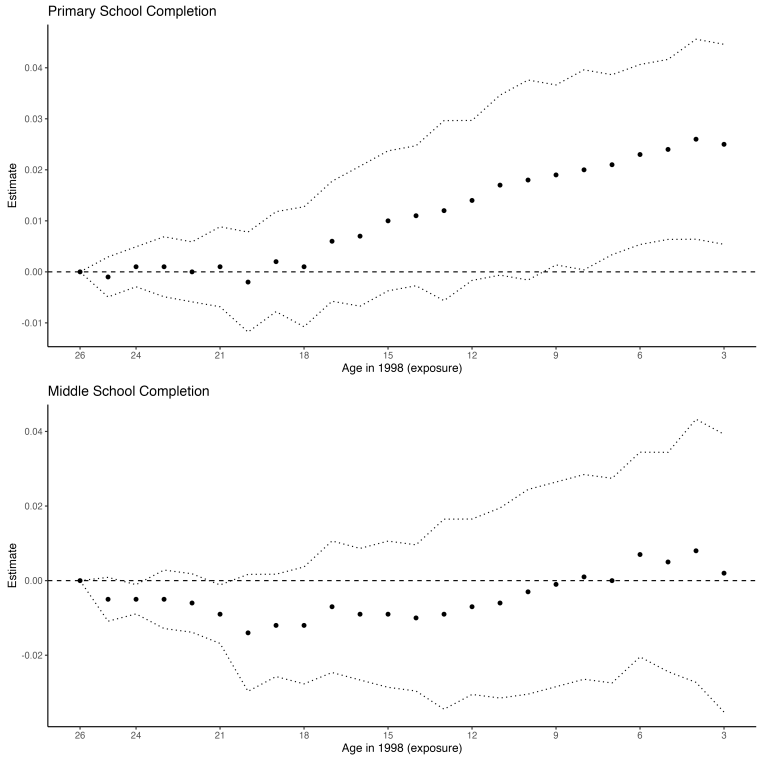
4 Main Results

4.1 Schooling Attainment

I start by looking at the effects of FUNDEF on educational attainment. Prior research has shown that the program had an initial positive effect on enrollment (De Mello and Hoppe, 2005; Cruz and Rocha, 2018), so we would expect it to also have a positive effect on the years of schooling completed. Because the Brazilian population census of 2010, the year in which outcomes are measured, did not record the exact number of years of schooling for individuals, my analysis is based on attainment at the two levels categories that FUNDEF initially targeted: primary and middle school.

Figure 2 reports estimates of the effects of FUNDEF-induced changes in local education budgets on individual cohorts' likelihood of completing primary education (top graph) and middle school (bottom graph). These effects are captured by the coefficient on the interaction of the cohort dummy with the FUNDEF shock ($\hat{\beta}_{1,a}$ in equation 2). The figure plots estimates of these effects for cohorts aged between 3 and 25 in 1998, relative to the cohort aged 26 in that year.

Figure 2: Effects of FUNDEF on the Probability of Completing Primary Education and Middle School, by Cohort



Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 3 to 26 in 1998. All regressions include municipality of education fixed effects, and control for sex, self-declared race, and the municipal-level share of children aged 0 to 14 in the population in 1997. Dashed lines denote 95% confidence intervals based on robust standard errors clustered at the municipality of education level.

If FUNDEF had an effect on individual educational attainment, we should observe positive and significant estimates in the cohorts that were exposed to the program, and not in the others. Because the program targeted primary and middle school, we would expect to see an effect in the cohorts that, in 1998, were in the ages that corresponded to those educational stages (7 to 10 and 11 to 14 years old, respectively) or younger. The younger the individual was in 1998, the longer exposure to the program they were likely to have. Individuals aged 6 or younger at the time were in principle exposed to the program for the full duration of their primary and middle school years. In addition, although individuals older than 14 in 1998 were, in theory, not exposed to the program, in practice some of them were exposed due to late school entry and grade repetition.[8] The likelihood of still

[8]In Brazil, as in many other developing countries, the incidence of late school entry and the repetition rates are high (Estevan, 2015). This implies that a subset of individuals in the cohorts who were old enough to have finished middle school by 1998 were still eligible to attend school and thus could have benefited from

being enrolled in school years 1 through 8 for this population is higher at younger ages. Intuitively, one could think of the X axis in Figure 2 as broadly capturing exposure to the program.

The results indicate that FUNDEF had a positive effect on primary school attainment. The effects increase with exposure to the program and are statistically significant at the 95% level for all cohorts aged 10 or younger in 1998. Positive point estimates—even if not statistically significant—are observed starting with individuals aged 17, suggesting that some older cohorts might have benefited from the program, plausibly due to grade repetition. A program-induced doubling of the transfers-related local education budget (a 100 percent increase relative to the money contributed to the state-level fund) was associated with an increase in the likelihood of completing primary school of between 0.6 and 2.5 percentage points, depending on the cohort, relative to those aged 26 in 1998. For the average municipality, which received a 28% FUNDEF shock, this corresponds to effects ranging from 0.17 to 0.7 percentage points.

In contrast, I find no evidence that FUNDEF had an effect on middle-school completion in the average municipality. Figure 2 (bottom graph) shows a small and not statistically significant estimate across cohorts.

Next, I estimate the average effects on educational outcomes across all exposed cohorts. Estimates of the coefficients from equation 3 are reported in Table 3. I find that, on average, doubling the FUNDEF-related public education budget led to an increase of 1.4 percentage points (or 0.0412 standard deviations) in the likelihood of completing primary education among individuals principally exposed to the program relative to those who were not (column 1). This represents a modest improvement relative to the 86 percent average rate of primary school completion in 2010 (Appendix Table A.4). For a municipality that received the average FUNDEF shock, it corresponds to an increase of 0.39 percentage points (0.011 standard deviations). The point estimate for the effects on the probability of completing middle school (column 2) was much smaller and not statistically significant.

These estimates are in the same order of magnitude as estimates of the effects of school spending on educational attainment in the United States. A recent meta-analysis of design-based studies of the impacts of K-12 public school spending on student outcomes estimates that, on average, an increase in school spending of $1,000 per pupil sustained over four years leads to an increase in educational attainment of 0.057 standard deviations (Jackson and Mackevicius, 2024).

To validate my empirical strategy, I replicate this analysis using a placebo treatment group. Specifically, I estimate equation 3 classifying as "exposed" cohorts aged 15 to 26 in

FUNDEF.

Table 3: Effects of FUNDEF on Educational Attainment

	Primary completion (1)	Middle-school completion (2)
FUNDEF x Exposed	0.014***	0.004
	(0.004)	(0.007)
Exposed	-0.179***	-0.103***
	(0.017)	(0.034)
Sex	0.027***	0.065***
	(0.002)	(0.003)
Ethnicity	-0.011***	-0.031***
	(0.000)	(0.001)
Share of children 0-14 in 1997 population	-0.667***	-0.598***
	(0.046)	(0.088)
Observations	5,289,021	5,059,035
R^2	0.055	0.097

Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 3 to 26 in 1998. Individuals aged 3 to 14 in 1998 are classified as exposed to the program; those aged 15 to 26 are the control group. All regressions include municipality-of-education fixed effects. Robust standard errors, clustered at the municipality-of-education level, are shown in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

1998, and including an equal-sized set of older cohorts (those aged 27 to 38 in that year) as non-exposed controls. The reference group in this regression is the cohort aged 38. Because the placebo treatment group was in principle not exposed to FUNDEF, we would expect the estimates of $\hat{\beta}_1$ in this sample to be statistically indistinguishable from zero. The presence of significant coefficients would point to a failure of the parallel trends assumption. The results of this regression for the schooling attainment outcomes are reported in Appendix Table A.5 (columns 1 and 2). Reassuringly, the point estimates are small and not statistically significant.

4.2 Migration

I now turn to exploring the effects of FUNDEF on migration. This is motivated, first, by the fact that internal mobility in Brazil—as in many other developing countries—is not legally restricted and is relatively high.[9] Individual beneficiaries can thus choose to migrate to other locations looking for opportunities that better match their qualifications (Andrews,

[9]Even though in Brazil, internal mobility had slowed down relative to the prior three decades, it was still high over the period of interest. Between 2000 and 2010, 10.35% of the adult population changed microregions of residence (Chauvin et al., 2017).

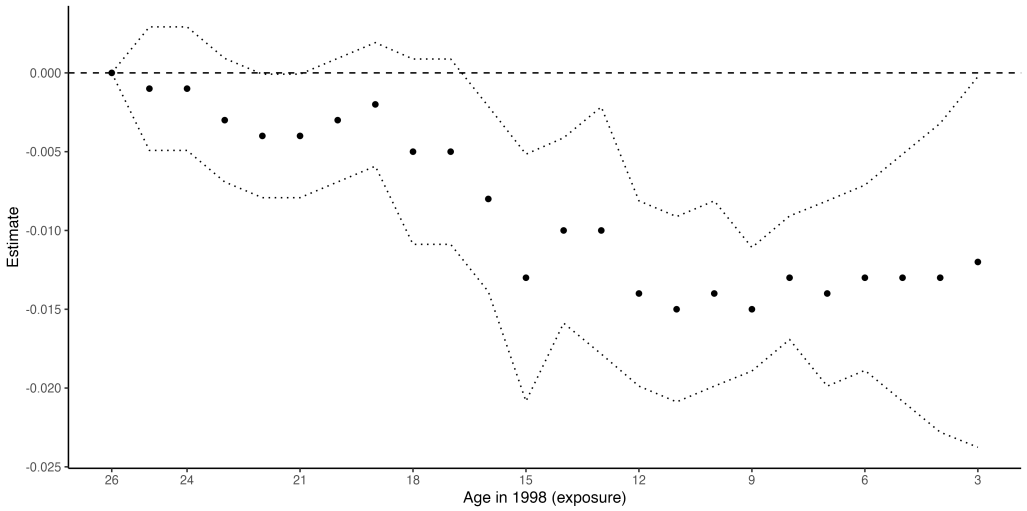
2022; Abel and Deitz, 2012). If labor demand for educated workers is unevenly distributed across the national geography, educated workers in places with low demand for skills will have the incentive to leave. Moreover, because migration is costly (Morten and Oliveira, 2024), infra-marginal individuals may be closer to the margin of migrating as they get educated and their potential income increases.

A second motivation is that the literature has extensively documented a connection between education and the geographic sorting of workers. Educated individuals are relatively more mobile (Notowidigdo, 2019), and when they migrate, they are more likely to go to larger (Combes et al., 2008; Glaeser and Resseger, 2010), more distant (Wozniak, 2010), and more educated places (Berry and Glaeser, 2005; Diamond, 2016). Austin et al. (2018) document that, in the United States, migrants are on average more educated than the non-migrant population in their place of origin. Appendix Figure A.4 reports the distribution of educational attainment in Brazil in 2010 by internal migrant status. It shows that, consistent with the patterns discussed in the literature, the Brazilian migrant population had, on average, higher educational attainment than the non-migrant population: while among the former, 59% had middle school or higher education, this number was 56% among the latter.

Figure 3 presents cohort-level estimates of the effect of FUNDEF ($\hat{\beta}_{1,a}$ in equation 2) on the likelihood of living in the same municipality of education by 2010. I find that individuals who were exposed to FUNDEF were more likely to migrate than the reference group. The effect of doubling program-related education funds ranged from a 0.8 to 1.3 percentage points decrease in the likelihood of staying in the same municipality. As a reference, 84 percent of the population aged 15 to 38 in 2010 lived in the same municipality where they were living when they were middle-school age or younger. The size of the effect increases with exposure to the program and, as in the case of educational attainment, there appears to be a smaller but still significant effect on cohorts that were old enough to just have left middle school, which could reflect grade repetition. These effects tend to disappear as the cohorts get older, consistent with the size of the effect being larger for individuals who were exposed to the program for longer.

Because in Brazil multiple municipalities can be part of the same local labor market, these results may reflect mere changes of residence within the same region—for example, driven by education-related income growth—rather than migration. To address this concern, I estimate the effects of FUNDEF on the likelihood of living, in 2010, in a municipality within the same local labor market as the municipality of education. To approximate the boundaries of local labor markets, I use "microregions." These are groupings of contiguous and economically integrated municipalities defined by the Brazilian Institute of Statistics

Figure 3: Effects of FUNDEF on the Probability of Staying in the Municipality of Education, by Cohort



Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 3 to 26 in 1998. All regressions include municipality of education fixed effects and control for sex, self-declared race, and the municipal-level share of children aged 0 to 14 in the population in 1997. Dashed lines denote 95% confidence intervals based on robust standard errors clustered at the municipality of education level.

(IBGE, 2002). I use the time-consistent boundary definition from Chauvin (2018), which corrects for municipality-level boundary changes over the period of interest, following the method proposed by Kovak (2013). Cohort-level estimates of the effect of FUNDEF are reported in Appendix Figure A.5. I find a pattern of results very similar to those in Figure 3, with slightly smaller effects ranging from 0.4 to 1.1 percentage points for a 100 percent increase in the FUNDEF shock. This is consistent with the interpretation that individuals who were more exposed to the program during school age were subsequently more likely to migrate to different labor markets.

Table 4 reports estimates of the average effects across exposed cohorts, relative to those who were, in principle, not exposed, estimated using equation 3. I find that, on average, a FUNDEF-related doubling of the school budget led to a 0.8 percentage point decrease in the likelihood of remaining in the municipality of education (column 1), and a 0.5 percentage point decrease in the likelihood of remaining in the same labor market (column 2).

These migration effects could reflect the relocation of individuals who seek to continue their studies elsewhere. High schools and universities are less geographically ubiquitous than primary and middle schools, particularly those of relatively higher quality. This implies that for many students wishing to continue their education after middle school, the only alternative may be to move to a different location where these opportunities are

Table 4: Effects of FUNDEF on the Probability of Staying in the Place of Education

	Stayed in municipality (1)	Stayed in microregion (2)	Student in 2010 (3)
FUNDEF x Exposed	-0.008***	-0.005***	-0.000
	(0.002)	(0.002)	(0.003)
Exposed	0.130***	0.089***	0.389***
	(0.011)	(0.014)	(0.013)
Sex	-0.008***	-0.003***	0.022***
	(0.001)	(0.001)	(0.001)
Ethnicity	0.004***	0.005***	-0.012***
	(0.001)	(0.001)	(0.001)
Share of children 0-14 in 1997 population	0.157***	0.086***	0.243***
	(0.028)	(0.028)	(0.032)
Observations	5,289,021	5,289,021	5,289,021
R^2	0.042	0.041	0.120

Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 3 to 27 in 1998. Individuals aged 3 to 14 in 1998 are classified as exposed to the program, those aged 15 to 26 are the control group. All regressions include municipality of education fixed effects. Robust standard errors clustered at the municipality of education level in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

available. To test this possibility, I estimate the effects of FUNDEF on the likelihood of being enrolled in an educational institution in 2010. The results are reported in Table 4, column 3. I find small and statistically insignificant effects, suggesting that the migration effects of FUNDEF are not driven by young people traveling to finish their formal studies.

Lastly, Appendix Table A.5 (columns 3, 4, and 5) reports estimates of the effects of FUNDEF on the two migration outcomes and on 2010 school enrollment for the placebo treatment group. The point estimates are small and not statistically significant in all three cases, supporting the causal interpretation of the estimates in Table 4.

4.3 Heterogeneity by the Direction of the Shock

FUNDEF was a redistribution program and, as a result, the direction of the shock differed across municipalities. While some municipalities were net beneficiaries of the program—in the sense that they received more than they contributed to their state-level fund—and experienced increases in their education budgets, others were net contributors and experienced budget cuts. The effects of having a larger budget may not be the same if this comes from receiving more funds than others, than if it comes from losing less funds than others. To explore these differences, I estimate equation 3 separately for each of these two cases. The

results are reported in Table 5. Column 1 reproduces the results obtained with the whole sample as a reference, column 2 presents the results for individuals whose municipality of education received a positive budget shock as a result of the program, and column 3 does the same for individuals whose municipality of education exhibited a negative budget shock.

Table 5: Effects of FUNDEF on Educational Attainment and Migration by Type of Shock

	All Municipalities (1)	Positive Budget Shock (2)	Negative Budget Shock (3)
Panel A: Schooling outcomes			
Primary completion	0.014***	0.013***	0.048**
	(0.004)	(0.004)	(0.022)
Middle-school completion	0.004	0.001	0.081**
	(0.007)	(0.006)	(0.038)
Panel B: Migration outcomes			
Stayed in municipality of education	-0.008***	-0.006**	0.015
	(0.002)	(0.003)	(0.024)
Stayed in microregion of education	-0.005***	-0.005**	0.010
	(0.002)	(0.002)	(0.017)
Student in 2010	-0.000	0.003	-0.025
	(0.003)	(0.002)	(0.016)
Observations	5,289,021	3,766,987	1,522,034

Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 3 to 27 in 1998. Individuals aged 3 to 14 in 1998 are classified as exposed to the program, those aged 15 to 26 are the control group. Column 2 includes only individuals whose municipality of education was a net beneficiary of FUNDEF, and column 3 those whose municipality of education was a net donor to the program. All regressions include municipality of education fixed effects. Robust standard errors, clustered at the municipality of education level, are shown in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

Panel A in Table 5 reports the results on schooling attainment. I find that the effect on primary school completion was positive and statistically significant for individuals educated in both types of municipalities, but noticeably larger for individuals educated in municipalities that were net donors to the program.[10] A 100 percent larger FUNDEF education budget was associated with a 1.3 percentage points higher likelihood of completing primary education in municipalities where the budget shock was positive, and with a 4.8 percentage points higher likelihood in municipalities with a negative budget shock. A similar pattern is observed for the likelihood of completing middle school. In this

[10]Note that in these municipalities a positive FUNDEF shock represents a smaller budget cut in relative terms.

case, the small, non-significant effect in the full sample of municipalities masks significant heterogeneity. While the effect in municipalities that experienced a positive budget shock was very close to zero, there was a positive and significant effect in municipalities that received a negative budget shock, where a 100 percent increase in the FUNDEF-related budget was associated with an 8.1 percentage points higher middle-school completion rate. In sum, this evidence indicates that the positive schooling effects of FUNDEF were largely driven by net-contributor municipalities. In a context where, as discussed in Section 2, access to education was expanding nationally, individuals educated in municipalities that experienced a larger education budget reduction attained lower education levels.

Panel B in Table 5 presents the results for the migration-related outcomes. In contrast with the schooling effects, the effects on the likelihood of remaining in the locality of education are largely driven by individuals whose municipality of study was a net beneficiary of the program. The effects are of similar direction, size, and statistical significance in municipalities with a positive budget shock as in the sample that includes all municipalities, both for the likelihood of remaining in the same municipality of education and for the likelihood of remaining in the same labor market. Meanwhile, the effect is positive and statistically non-significant for both outcomes in the case of municipalities that experienced a negative budget shock. The effects on the likelihood of being enrolled in school in 2010 remain not statistically significant for both sub-samples, but the point estimates are of different signs. Moreover, the point estimate for municipalities that experienced a negative budget shock is negative and large, suggesting that in this group of municipalities, individuals may have been less likely to continue their education by 2010, consistent with the schooling results in Panel A.

Overall, these results are consistent with the interpretation that the migration effects of FUNDEF are, at least in part, the result of the lack of demand for educated workers in the municipalities whose local education budget benefited from the program. As shown in Table A.6, regions that experienced positive budget shocks due to FUNDEF had noticeably worse labor market conditions in the year 2000 than regions that experienced negative budget shocks, including lower wages, employment, and participation rates, and higher informality and unemployment rates. Thus, individuals that increased their schooling as a result of the program in economically lagging municipalities were more likely to leave, even if the schooling effects were not as large as in municipalities with more economic opportunities.

4.4 Robustness

Table 6 reports the robustness of the results to a set of variations in the specification and the shock. Column 1 reproduces the baseline results as a reference.

A first concern relates to the role of pre-program municipal enrollment rates. Because municipalities that had a larger share in the state-level public enrollment received proportionally more resources from FUNDEF, one would expect exposure to the program in 1998 to be positively correlated with enrollment rates in 1997. Having higher initial enrollment rates, in turn, implies higher educational attainment in 2010. To address this concern, I use 1997 municipal enrollment rates as a control instead of the share of individuals aged 14 or younger in the population in column 2. All results remain qualitatively the same, and almost all maintain a similar size and level of statistical significance, except for the results on primary school completion, which are larger but less precisely measured than in the main specification. In column 3, I control for both enrollment rates and the share of middle-school-age or younger individuals in the population. The results of this specification are virtually the same as in column 1, suggesting that the 1997 enrollment rates are not an important omitted variable in the main analysis.

Another concern, raised by Kosec (2014), is that the 1998 municipal revenues may be affected by omitted variables (e.g., macroeconomic fluctuations) that also directly affect the outcome variables. To address this concern, she employs a measure of the FUNDEF shock based on revenue data from 1997, the year prior to the start of the program. Estevan (2015) uses a similar approach to estimate a "predicted" impact of FUNDEF, specifically computing:

$$FS_j^{pred} = \sum_{e \in \{m,s\}} \eta_{j,97}^e \left(\frac{I_{j,97}^e - O_{j,97}^e}{O_{j,97}^e} \right) \tag{4}$$

where $O_{j,97}^e$ corresponds to 15% of the actual 1997 revenues, and $I_{j,97}^e$ is a simulated FUNDEF transfer, based on enrollment shares and the projected total value of each state-level FUNDEF fund in 1997.

In column 4 of Table 6 I compute the estimates using the predicted FUNDEF shock from equation 4 instead of the actual FUNDEF shock from equation 1 used in the rest of the analysis. Most results remain of very similar size and statistical significance as in the baseline specification, except for the schooling results in the case of municipalities that received negative budget shocks, whose point estimates remain of similar size but are less precisely measured.

Table 6: Robustness Tests

	Baseline Results	Enrollment instead of young pop.	Enrollment and young pop.	Predicted FUNDEF shock	Excluding High-school aged in 1997
	(1)	(2)	(3)	(4)	(5)
Panel A: All municipalities					
Schooling outcomes					
Primary completion	0.014***	0.024*	0.014***	0.011***	0.019***
	(0.004)	(0.013)	(0.004)	(0.003)	(0.007)
Middle-school completion	0.004	0.013	0.004	0.003	0.004
	(0.007)	(0.015)	(0.007)	(0.006)	(0.011)
Migration outcomes					
Stayed in municipality of education	-0.008***	-0.010***	-0.008***	-0.006**	-0.010***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Stayed in microregion of education	-0.005***	-0.006***	-0.005***	-0.004**	-0.007***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Student in 2010	-0.000	-0.004	-0.000	0.000	-0.001
	(0.003)	(0.006)	(0.003)	(0.002)	(0.003)
Panel B: Positive Budget Shocks					
Schooling outcomes					
Primary completion	0.013***	0.025	0.013***	0.010***	0.018***
	(0.004)	(0.015)	(0.004)	(0.003)	(0.006)
Middle-school completion	0.001	0.012	0.001	0.001	0.001
	(0.006)	(0.017)	(0.006)	(0.005)	(0.010)
Migration outcomes					
Stayed in municipality of education	-0.006**	-0.009***	-0.006*	-0.005*	-0.010***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
Stayed in microregion of education	-0.005**	-0.006***	-0.005**	-0.003*	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Student in 2010	0.003	-0.001	0.003	0.000	0.003
	(0.002)	(0.006)	(0.002)	(0.003)	(0.003)
Panel C: Negative Budget Socks					
Schooling outcomes					
Primary completion	0.048**	0.044*	0.048**	0.044	0.082***
	(0.022)	(0.022)	(0.022)	(0.030)	(0.031)
Middle-school completion	0.081**	0.077*	0.080**	0.062	0.120**
	(0.038)	(0.040)	(0.038)	(0.058)	(0.051)
Migration outcomes					
Stayed in municipality of education	0.015	0.016	0.014	0.006	0.014
	(0.024)	(0.027)	(0.025)	(0.035)	(0.028)
Stayed in microregion of education	0.010	0.010	0.009	0.004	0.011
	(0.017)	(0.018)	(0.017)	(0.026)	(0.018)
Student in 2010	-0.025	-0.022	-0.024	-0.021	-0.035**
	(0.016)	(0.019)	(0.017)	(0.022)	(0.017)

Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 3 to 27 in 1998. Individuals aged 3 to 14 in 1998 are classified as exposed to the program, those aged 15 to 26 are the control group, except in column 5, where it is composed of people aged 19 to 30 in 1998. All regressions include municipality of education fixed effects. Robust standard errors, clustered at the municipality of education level, are shown in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

Lastly, as discussed in Section 4, the relatively high incidence of grade repetition in Brazil implies that cohorts that were in principle too old to have benefited from FUNDEF may have been at least partially exposed to the program. This implies that some individuals that were partially "treated" are included in the control group. To address this concern, in column 5 of Table 6 I exclude from the sample individuals who were high-school age in 1998 (ages 15 through 18), and redefine the control group accordingly to include individuals aged 19 to 30 in that year. I obtain results that are very close in magnitude, direction, and statistical significance as in the main specification. The one notable exception is the effect on the likelihood of being enrolled in school in 2010, for the case of municipalities that experienced a negative budget shock due to FUNDEF. In this specification, the negative effect becomes statistically significant at 95%, which is consistent with the more pronounced effects on primary and middle-school completion previously found for these municipalities.

5 Regional-Level Effects

This section examines the regional-level impacts of FUNDEF on schooling and labor market outcomes. Given the individual-level results discussed in the prior section, the program could have had either positive or negative effects at the local labor market level. Indeed, the fact that FUNDEF did increase primary education attainment among individuals who were exposed to the policy does not necessarily imply that we will observe an increase in the shares of adults with at least primary education in the impacted regions. The migration effects reported in Section 4.2 imply that the increase in schooling attainment among local children is likely to translate, down the road, into smaller, or even null increases in the supply of more educated adult workers in the locality. The program could also have affected local labor demand. If, despite the migration effects, the skills upgrading of the local labor force was large enough, it could have led to increases in productivity and the demand for local goods and services, with a corresponding expansion in the demand for labor. If, on the contrary, the "brain drain" effect dominated, the relative loss of higher-education and higher-income workers could have depressed local labor demand.

5.1 Regional Specification

To explore the regional-level impacts of the program empirically, I construct variables at the local labor market level (microregions) using census microdata, sampling weights, and

time-invariant regional boundaries.[11] Descriptive statistics for these variables for the 2000 and 2010 censuses are reported in Appendix Table A.6.[12] Regions with higher incidence of the program did see rising regional education levels in the 2000s. Figure 4 shows the simple correlation between the regional-level FUNDEF shock and the growth of aggregate education levels. It uses as local education measures the share of the "educated" in the adult population for three different categories of educational attainment: primary school, middle school, and high school. The incidence of the program has a strong correlation with growth in the share of adults who completed primary education, consistent with the individual-level results. On average, a 100 percent increase in the education budget—which corresponds to 1.63 standard deviations—was associated with a 7.7 percentage points increase in the share of individuals with at least primary education in the adult population—equivalent to a 0.44 standard deviations reduction. The program had a weaker correlation with higher education attainment measures.

This correlation, however, does not necessarily imply that the local growth in the share of the educated population was caused by FUNDEF. In order to assess the causal impact of the program at the regional level, I turn to the following difference-in-differences regression:

$$Y_r = \alpha_0 + \alpha_1 \text{Post} + \alpha_2 FS_r + \alpha_3(\text{Post} \times FS_r) + \alpha_4 C_r + \alpha_5(\text{Post} \times C_r) + \epsilon_r \qquad (5)$$

where Y_r is the outcome of interest in region r, $Post$ is a dummy variable that takes the value 1 for the year 2010 (post-treatment) and zero for the year 2000 (pre-treatment),[13] and C_r is a vector of regional-level time-invariant controls, which are allowed to have time-variant effects. I cluster standard error at the mesoregion level (the next geographic level above the microregion) to address potential spatial autocorrelation concerns.[14]

The use of controls in this case is important, because the program's redistribution rule was based on local governments' transfer revenues and local schools' enrollment, and both of these variables are potentially correlated with pre-existing trends in the economic performance of local labor markets. A first-order concern comes from the fact that different areas of Brazil have very different levels of economic development and growth prospects. To

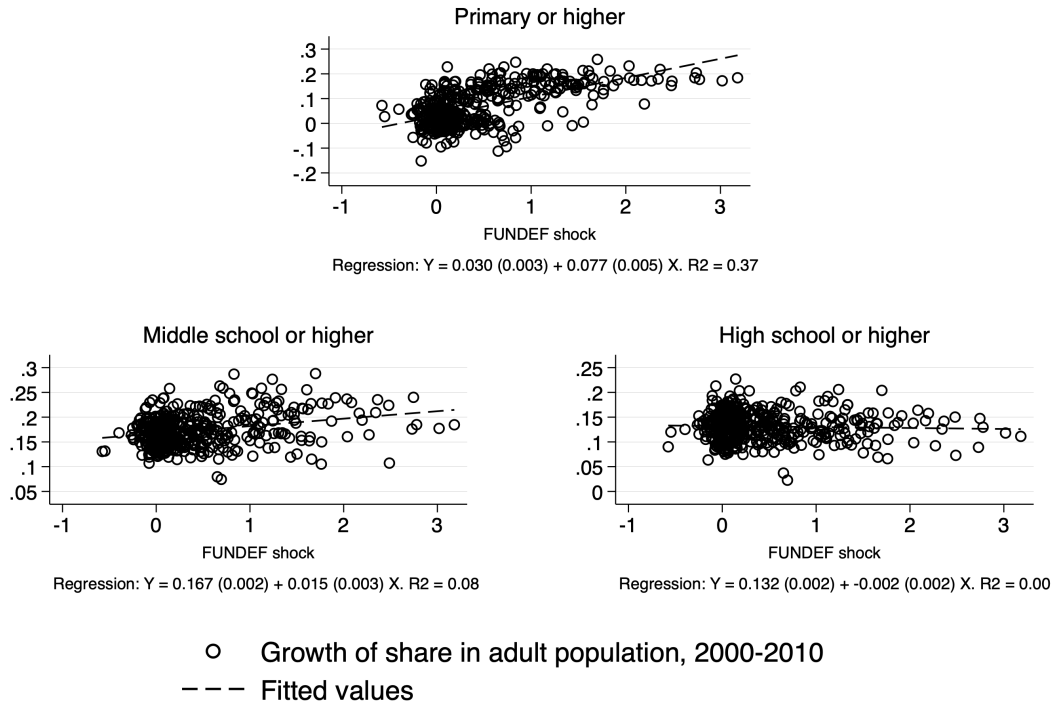
[11] All the regional analysis is performed at the time-consistent microregion level because, as previously discussed, this geographic unit captures better than municipalities the boundaries of local labor markets (which oftentimes encompass more than one municipality).

[12] All regional outcomes are estimated excluding teachers, in order to avoid the potential confounding effect of program-mandated wage increases for these workers.

[13] Note that FUNDEF was implemented two years before this "pre" period. However, by 2000 all the cohorts impacted in the program were still at school, and were unlikely to affect regional-level outcomes measured for adults.

[14] I employ the same methodology used for microregions to adjust the boundaries of mesoregions, ensuring their comparability over time. This process yields 136 clusters (i.e., time-consistent mesoregions).

Figure 4: Correlation between the FUNDEF Shock and Regional-Level Measures of Changes in Educational Attainment



Note: Observations are microregions in which all municipalities have data on FUNDEF shock (N=456).

avoid comparing local labor markets that are located in areas with very dissimilar economic trends (e.g., those from the generally poorer North and the richer South macroregions), the vector of controls includes fixed effects for the five macro-regions defined by the IBGE.[15]

Even within these macroregions, there can be important heterogeneity in growth prospects across local labor markets. To address this concern, I include two exogenous controls. First, I use the average historical summer and winter temperatures to capture variation in local natural conditions. Temperatures can shape crops' composition and the vulnerability to natural disasters in rural areas (Aragón et al., 2021), and they can affect learning, health and productivity (Colacito et al., 2019; Park et al., 2021) as well as the quality of living (i.e., the natural "amenities") in urban areas (Chauvin et al., 2017). Second, I control for the geographic area of the microregions. Large microregions are typically more rural and relatively less populated and developed, and the provision of development-promoting infrastructure and services is relatively more costly there. Lastly,

[15]The North macroregion is left out of the regression as the reference group.

lagging regions tend to have a relatively less vibrant private sector, and the local public sector—whose hiring and firing decisions are not necessarily driven by the rules of supply and demand of competitive labor markets—can play a disproportionate role in the demand for educated labor (Jofre-Monseny et al., 2020). I thus control for the pre-program (1991) share of government in local employment.

FS_r in equation 5 is a regional-level version of the FUNDEF shock. While the municipal-level measure of the shock from Equation 1 is useful to capture the exposure of a particular individual to FUNDEF, it does not adequately capture the incidence of the program in a particular local economy. This is because, as mentioned earlier, local labor markets in Brazil often incorporate two or more geographically proximate municipalities. Thus, to study the effects of FUNDEF on the aggregate outcomes of local economies, I construct a microregion-level shock, namely:

$$FS_r = \sum_{j \in r} \varsigma_{j,97} \times FS_j \tag{6}$$

where $\varsigma_{j,97}$ is the share of municipality j in region r's school-age population in 1997. Appendix Figure A.2 (right) reports the distribution of this measure across local labor markets, and Appendix Table A.6 also reports means and standard deviations for the full set of regions and by direction of the shock. For the average region, the program produced, in its first year, a 43 percent increase in the education budget relative to the money contributed to the state's fund. Among regions where the budget shock was positive, the average budget increase was 54 percent, and among regions where the shock was negative, the average drop was 10 percent.

The key identifying assumption in this design is the existence of "parallel trends," namely, that conditional on the controls and in the absence of the program, changes in Y_r would not have been systematically different between high-incidence and low-incidence regions. To explore the validity of this assumption, I perform a "placebo test," in which I estimate the effects of FUNDEF using regression 5 on pre-program outcomes. Specifically, I use the year 2000 as the "post" period and 1991 as the "pre-period." Because the cohorts impacted by the program were still in school by 2000, finding significant effects in this regression would suggest the presence of pre-trends. The results are reported in Appendix Table A.7 and show that, conditional on covariates, the FUNDEF shock is reassuringly uncorrelated with the 1990s trends in the outcomes of interest.

5.2 Schooling Outcomes

Table 7 reports estimates of the coefficient $\hat{\alpha}_3$ in equation 5, which measures the average treatment effect of increased local public education investments on the beneficiary regions' outcomes of interest. Note that these estimates reflect both the direct effect of the increased relative supply of local educated labor—i.e., the program's effect on moving a share of the local population from a low education category to a high education category—and any general equilibrium—e.g., any effects of local education levels on labor demand and subsequent migratory adjustments (Moretti, 2011).

Table 7: Regional-Level Effects of FUNDEF on Educational and Labor Market Outcomes

	(1)	(2)	(3)	(4)
Panel A: Schooling Outcomes				
Primary completion	0.018***	0.013**	0.012**	0.010*
	(0.006)	(0.006)	(0.005)	(0.005)
Middle-school completion	0.001	-0.003	-0.003	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Panel B: Labor Market Outcomes				
Ln of employment	-0.010	-0.023	-0.020	-0.014
	(0.017)	(0.017)	(0.016)	(0.016)
Employment rate	-0.020***	-0.019***	-0.019***	-0.019***
	(0.006)	(0.005)	(0.005)	(0.005)
Wage (residualized)	-0.024	-0.037**	-0.037**	-0.028*
	(0.017)	(0.015)	(0.016)	(0.015)
Microregion fixed effects	Yes	Yes	Yes	Yes
Average summer and winter temperatures	No	Yes	Yes	Yes
Municipality area	No	No	Yes	Yes
Government share in employment, 1991	No	No	No	Yes

Notes: Regional-level regressions using data from the 2000 and 2010 censuses. Robust standard errors clustered at the mesoregion level are in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

Panel A in Table 7 reports the results for schooling outcomes. The difference-in-differences estimation yields smaller estimates than the simple OLS. A 100 percent increase in FUNDEF-related budget was associated with a 1.8 percentage points increase in the share of individuals with primary education or higher in the specification where I only control for macro-region fixed effects. Introducing additional controls yields smaller point estimates. In my most conservative specification, where I use all the controls described in Section 5.1, the estimated effect is of 1 percentage point, significant at the 10 percent level.

I find no significant effects on the share of the adult population living in the municipality that completed middle school education. Appendix A.8 reports these estimates calculated separately for municipalities with program-driven positive and negative budget shocks. I find small and non-significant impacts on education in each group separately, suggesting that the aggregate primary-schooling effects come from comparing across the two groups.

Overall, the results suggest that, while the additional public education budget made *individuals* from affected cohorts more likely to graduate from primary school (and middle school in municipalities that were net donors of FUNDEF), the fact that the program also had a migration effect resulted in small and non-significant labor supply shifts in these schooling categories *at the local labor market level*.

5.3 Labor Market Outcomes

Because the effect of FUNDEF on the local supply of workers with higher levels of schooling was small, the effects on local labor markets are also likely to have been small. If the demand for relatively more educated workers is downward-sloping, one would observe small increases in employment and a reduction in wages. In addition, the program could have affected local labor demand. On the one hand, a more educated labor force could have led to positive demand shifts due to human capital spillovers (Moretti, 2004), with positive effects on both employment and wages. On the other hand, if more educated workers represent a large share of the local consumption of goods and services produced by the less educated, their emigration could have depressed the local aggregate labor demand, affecting wages and employment negatively.

Estimates of the reduced-form effects of FUNDEF on regional-level labor market outcomes are reported in Panel B of Table 7. The results show that, on average, labor market outcomes worsened in regions that benefited from FUNDEF. I first look at the effect on total employment and find it to be negative but not statistically significant. This could reflect a reduction in the number of workers—in line with the migration effect discussed in Section 4.2—as well as worsened employment opportunities for the workers who stayed. Next, I consider local employment rates (i.e., the share of the working-age population that had a remunerated job at the time of the census). I find a negative effect, statistically significant at the 1 percent level. In my preferred specification, a program-induced one hundred percent larger public education budget in the microregion was associated with a 1.4 percentage points reduction of the average employment rate. When I break down the results by subgroups of municipalities according to the type of shock (Appendix Table A.8), I find that this negative effect is only significant in local labor markets that experienced positive budget shocks due to the program.

Lastly, I look at the effects on average local wages. In order to account for differences in the composition of local human capital that can affect average wages, I use as my wage measure the residuals of an individual-level regression of the average wage on education attainment and age categories indicators (see Appendix A for details). I find that a shock-induced doubling of the education budget was associated with a 2.8 percentage points wage decrease, significant at the 10 percent level.

Overall, the evidence suggests that the increases in public fundamental education funding through FUNDEF adversely affected the local labor market demand 12 years later. This aligns with the migration effects identified in the individual-level analysis (Section 4.2). Higher emigration rates among affected cohorts likely slowed the growth of the locally available labor force and altered its composition. Prior studies have shown that individuals who become internal migrants tend to be positively selected on observable characteristics[16], and sometimes also on non-observables.[17] Thus, the "brain drain" induced by the program resulted in a diminished pool of high-earning and high-consuming workers in the affected local economies, curtailing the growth of local labor demand.

6 Conclusion

This paper studies the effects of changes in local public education spending in Brazil induced by FUNDEF, a large federal program that redistributed public education budgets across municipalities within states. I find that individuals exposed to the program showed increases in both their schooling attainment and their propensity to migrate to other local labor markets, compared to those who were not exposed. Doubling the educational budget associated with the program resulted in an average 1.4 percentage point increase in the likelihood of completing primary school, and a 0.5 percentage point decrease in the likelihood of remaining in their locality of education after reaching high-school age.

The impact of the program on schooling and migration varied depending on whether the municipalities were net recipients or donors. Individuals educated in municipalities that experienced a positive budget shock from FUNDEF—initially characterized by weaker labor markets and lower education levels—were the primary drivers of the observed migration effects, despite experiencing relatively smaller improvements in schooling attainment. Conversely, those from municipalities experiencing a negative budget adjustment—typically

[16] See (Dos Santos Júnior et al., 2005) and (Freguglia and Menezes-Filho, 2012) for evidence from Brazil

[17] The evidence about spatial sorting on non-observables is mixed. For instance, (Combes et al., 2008) find it to be quantitatively important in France, whereas (Baum-Snow and Pavan, 2012) and (De la Roca and Puga, 2017) find little evidence of sorting on unobserved characteristics, as captured by individual fixed effects.

characterized by better economic opportunities—saw more substantial gains in schooling attainment but did not exhibit a significant migration response. At the regional level, FUNDEF had a small positive effect on the share of adult individuals with primary school education, but it was also associated with lower employment rates and wages.

Taken together, these findings suggest that program-induced increases in public education budgets helped individuals attain higher education but also to migrate, and this "brain drain" negatively impacted local demand and labor market outcomes. This underscores the need to factor in migratory responses when analyzing the outcomes of local education investments, as these responses can shape both individual gains and the broader effects on local labor markets.

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Appendix

A Data Appendix

Table A.1: Databases Used

Database	Provider	Years	Source
Population census microdata sample	IBGE	1991, 2000, 2010	loja.ibge.gov.br/populacao/amostra
Municipality areas	IBGE	2010	https://mapas.ibge.gov.br/bases-e-referenciais/bases-cartograficas/malhas-digitais.html
Municipality average historial temperatures	IPEA	2010	http://www.ipeadata.gov.br
Evolution of municipality borders across census years	IBGE	1980-2010	www.ibge.gov.br/home/geociencias/geografia/default_evolucao.shtm
National consumer price index	IBGE	1980-2010	ww2.ibge.gov.br/home/estatistica/indicadores/precos/inpc_ipca/defaultseriesHist.shtm
Brazilian School Census	INEP	1997, 1998	http://portal.inep.gov.br/microdados
Brazilian national and state treasuries budget dataset	SNT	1997, 1998	tesouro.fazenda.gov.br

Table A.2: Individual-Level Variables Definitions

Variable	Years	Description / comments
Primary school completed	2010, 2000	Indicator that takes the value one if the person has completed primary schoolor higher at the time of the census, and zero otherwise.
Middle-school completed	2010, 2000	Indicator that takes the value one if the person has completed middle schoolor higher at the time of the census, and zero otherwise.
Stayer in municipality	2010, 2000	Individual that declares that its time of residence in their current municipality is greater than the number of years that have passed since they stopped being school age.
Stayer in microregion	2010, 2000	Individual that declares that its time of residence in their current microregion is greater than the number of years that have passed since they stopped being school age.
Student in 2010	2010, 2000	Individual that is enrolled in an educational institution of any level in the year 2010.

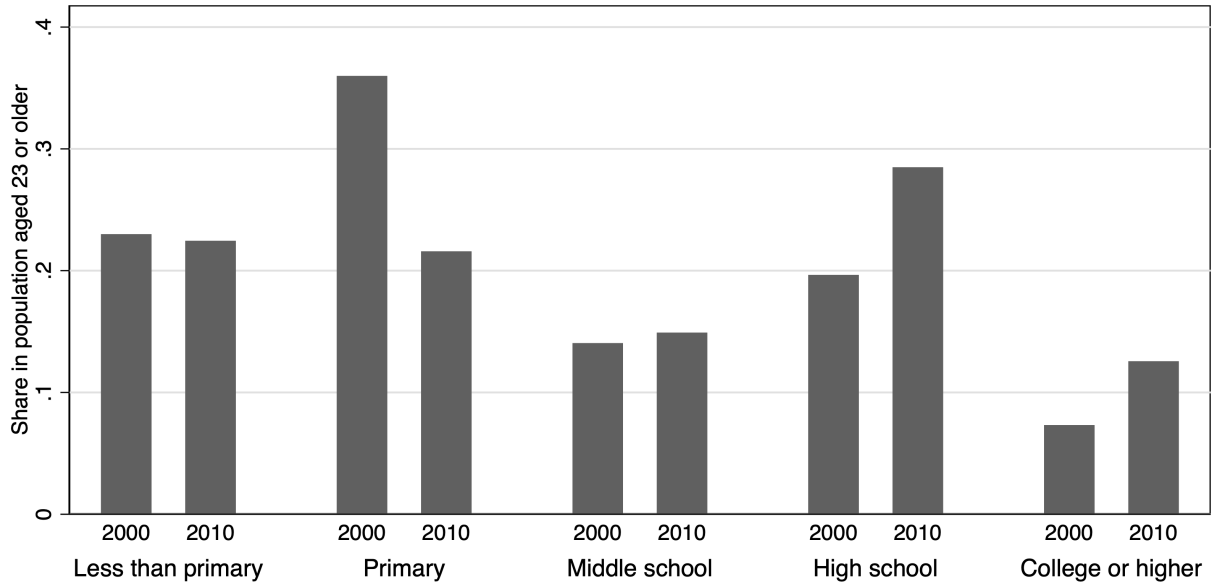
Table A.3: Regional-Level Variables Definitions

Variable	Years	Description / comments
Microregion	1991, 2000 2010	Conglomerates of adjacent municipalities that constitute a local labor market, as defined by the IBGE. Boundaries are adjusted to make them time-consistent, following a procedure similar to that in (Kovak, 2013).
Municipal FUNDEF shock	1998	Change in the municipal-level fundamental education budget induced by FUNDEF, expressed as a fraction of the resources contributed the fund by local governments (equation 1).
Prediced municipal FUNDEF shock	1997	Predicted change in the municipal-level fundamental education budget induced by FUNDEF, expressed as a fraction of the predicted contribution to the fund by local governments (equation4).
Regional FUNDEF shock	1998	Weighted sum of the FUNDEF shock from the municipalities belonging to the microregion, using the share of each municipality in the region's school-age population as weights (equation 6)
Primary completion rate	1991, 2000 2010	Share of the population 15 years or older that have completed primary school.
Middle-school completion rate	1991, 2000 2010	Share of the population 15 years or older that have completed middle school.
Employment	1991, 2000 2010	Number of individuals reported being employed and receiving a positive wage in the main occupation.
Employment rate	1991, 2000 2010	Employed individuals as a share of the working age population.
Average log wage residual	1991, 2000 2010	Average of the log wage residual at the region level. The residuals are calculated regressing the monthly labor income in the main occupation on individual characteristics including age categories, schooling categories, sex and race. It is calculated for adult individuals reporting positive wage.
Average temperatures	2010	Weighted average of municipal-level summer and winter average historical temperatures, where the weights are the share or each municipality's area in the microregion's total area.
Microregion area	2010	Sum of the areas of all municipalities that belong to a given microregion in square km.
Government share in employment	1991	Share of workers with positive wage that work in an industry classified as government at the two-digits level.

Notes: All monetary values are expressed in 2010 reais. Variables are converted from prior currencies to reais and deflated using the national consumer price index (INCP) provided by the IBGE. The original INPC deflators are adjusted to account for inconsistencies derived from a dual-currency period in 1994, following the method proposed by Corseuil and Foguel (2002). The reference period changed between the censuses up to 1991 (when it was defined as the prior 12 months before the survey) and the censuses of 2000 and after (when it was defined as the prior week before the survey). Civil service employees and employers are excluded from the computations of the regional-level aggregate labor- market variables.

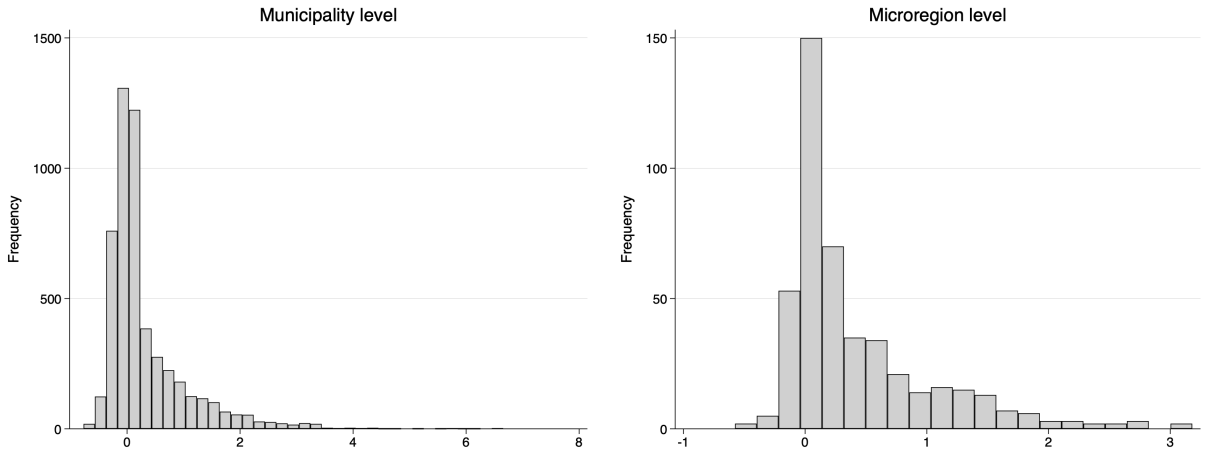
B Additional Figures

Figure A.1: Percentage of Brazilian Population Aged 23 or Older in Each Educational Attainment Category in 2000 and 2010



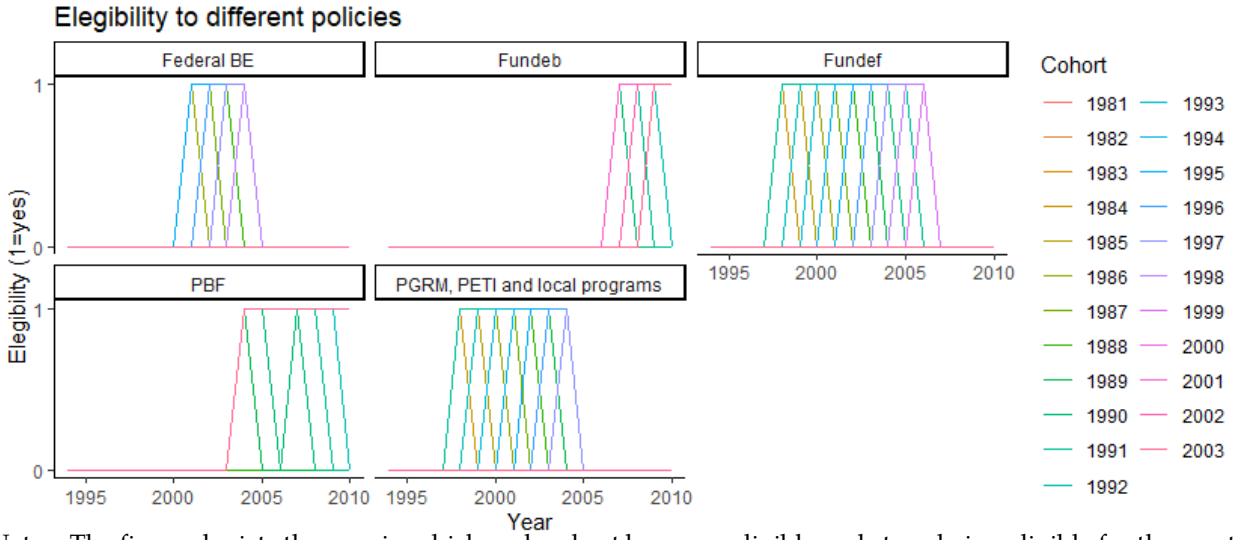
Source: Population censuses of 2000 and 2010.

Figure A.2: Distribution of Budget Changes Induced by the First Year of FUNDEF (i.e., "FUNDEF Shock")



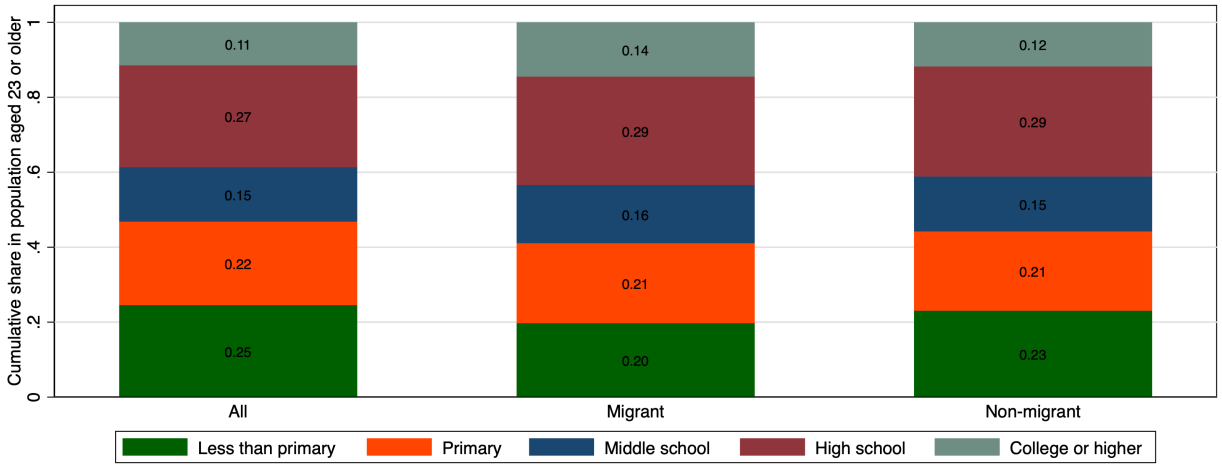
Sources:

Figure A.3: Eligibility of Each Cohort for Education-Related Social Programs



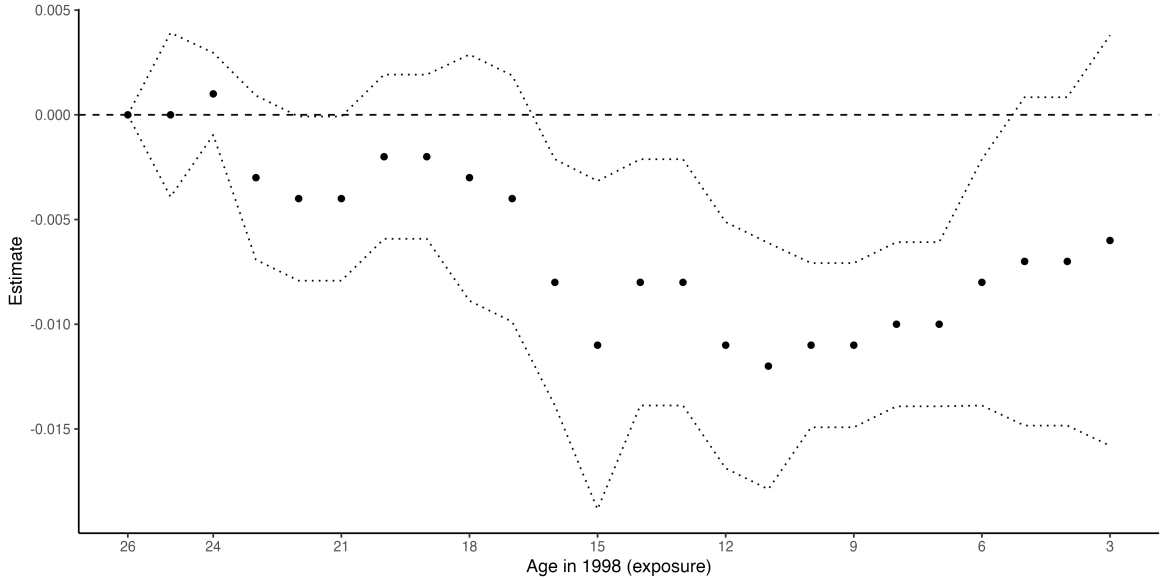
Notes: The figure depicts the year in which each cohort becomes eligible and stops being eligible for the most important education-related programs during the period of study.

Figure A.4: Percentage of Brazilian Population Aged 23 or Older in Each Educational Attainment Category in 2010, by Migration Status



Source: Population census of 2010.

Figure A.5: Effects of FUNDEF on the Probability of Staying in the Microregion of Education, by Cohort



Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 3 to 26 in 1998. All regressions include municipality of education fixed effects, and control for sex, self-declared race, and the municipal-level share of children aged 0 to 14 in the population in 1997. Dashed lines denote 95% confidence intervals based on robust standard errors clustered at the municipality of education level.

C Additional Tables

Table A.4: Individual-Level Summary Statistics in 2010

	All municipalities		Positive Budget shock		Negative Budget shock	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(5)	(6)
Probability of living in municipality of education	0.84	0.37	0.84	0.37	0.84	0.37
Probability of living in microregion of education	0.87	0.33	0.87	0.33	0.88	0.32
Probability of having completed primary	0.86	0.35	0.85	0.36	0.89	0.32
Probability of having completed middle-school	0.67	0.47	0.66	0.47	0.72	0.45
Probability of having completed high-school	0.49	0.50	0.48	0.50	0.54	0.50
Monthly wage	1219.73	2788.30	1214.95	2796.57	1241.31	2750.52
Employment rate	0.68	0.47	0.67	0.47	0.71	0.46
Participation rate	0.74	0.44	0.73	0.44	0.77	0.42
Informality rate	0.26	0.44	0.26	0.44	0.25	0.43
Unemployment rate	0.06	0.24	0.06	0.24	0.07	0.26

Source: Author's calculations from 2010 population census using sampling weights.

Table A.5: Placebo Regressions of Effects of FUNDEF on Educational Attainment and Migration

	Schooling		*Migration*		
	Primary completion (1)	Middle-school completion (2)	Stayed in municipality (3)	Stayed in microregion (4)	Student in 2010 (5)
FUNDEF x Exposed	0.005	-0.007	-0.001	-0.002	-0.000
	(0.004)	(0.007)	(0.003)	(0.003)	(0.001)
Exposed	-0.137***	0.002	-0.013	-0.009	0.075***
	(0.019)	(0.031)	(0.017)	(0.017)	(0.005)
Sex	0.029***	0.053***	0.007***	0.006***	0.019***
	(0.002)	(0.004)	(0.001)	(0.001)	(0.001)
Ethnicity	-0.020***	-0.042***	0.002	0.003***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Share of children 0-14 in 1997 population	-0.569***	-0.322***	0.045	0.032	0.078***
	(0.051)	(0.080)	(0.043)	(0.036)	(0.012)
Observations	3,993,103	3,944,981	3,993,103	3,993,103	3,993,103
R^2	0.083	0.126	0.039	0.039	0.018

Notes: Individual-level regressions using data from the 2010 census. The sample is restricted to individuals aged 15 to 38 in 1998. In this placebo regression, individuals aged 15 to 26 in 1998 are classified as exposed to the program, those aged 27 to 38 are the control group. All regressions include municipality of education fixed effects. Robust standard errors clustered at the municipality of education level in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

Table A.6: Regional-Level Summary Statistics

	All municipalities		Positive Budget shock		Negative Budget shock	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2000						
FUNDEF shock	0.43	0.61	0.54	0.62	-0.1	0.1
Population (in 1000s)	317.72	815.62	334.9	865.02	224.68	456.86
Primary graduates share	0.59	0.17	0.57	0.17	0.69	0.11
Middle-school graduates share	0.25	0.11	0.24	0.12	0.29	0.07
High-school graduates share	0.15	0.07	0.15	0.08	0.17	0.05
College graduates share	0.02	0.02	0.02	0.02	0.03	0.01
Monthly wage	911.35	363.05	885.69	378.32	1050.37	219.71
Employment rate	0.51	0.09	0.5	0.09	0.56	0.07
Participation rate	0.59	0.1	0.58	0.1	0.63	0.07
Informality rate	0.59	0.14	0.6	0.14	0.57	0.11
Unemployment rate	0.13	0.04	0.13	0.04	0.11	0.04
Panel B: 2010						
Population (in 1000s)	356.14	893.79	375.82	946.71	249.52	511.95
Primary graduates share	0.66	0.11	0.65	0.11	0.71	0.08
Middle-school graduates share	0.42	0.11	0.42	0.11	0.46	0.08
High-school graduates share	0.28	0.09	0.27	0.09	0.3	0.07
College graduates share	0.05	0.03	0.05	0.03	0.06	0.02
Monthly wage (2010 reais)	592.01	208.44	572.02	214.2	700.26	128.92
Employment rate	0.57	0.11	0.56	0.11	0.65	0.09
Participation rate	0.62	0.11	0.61	0.11	0.69	0.08
Informality rate	0.55	0.15	0.55	0.15	0.51	0.11
Unemployment rate	0.08	0.03	0.08	0.03	0.06	0.03

Source: Author's calculations from 2010 population census. I first use sampling weights to calculate municipality-level measures, and then calculate descriptive statistics across municipalities.

Table A.7: Placebo Regressions of Regional Effects of FUNDEF on Educational and Labor Market Outcomes

	2000 outcomes (placebo)
Panel A: Schooling Outcomes	
Primary completion	0.001
	(0.004)
Middle-school completion	-0.004
	(0.003)
Panel B: Labor Market Outcomes	
Ln of employment	0.007
	(0.023)
Employment rate	-0.008
	(0.007)
Wage (residualized)	-0.023
	(0.016)
Microregion fixed effects	Yes
Average summer and winter temperatures	Yes
Municipality area	Yes
Government share in employment, 1991	Yes

Notes: Regional-level regressions using data from the 1991 and the 2000 censuses. Robust standard errors clustered at the mesoregion level in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.

Table A.8: Regional-Level Effects of FUNDEF on Educational and Labor Market Outcomes by Type of Shock

	All Municipalities (1)	Positive Budget Shock (2)	Negative Budget Shock (3)
Panel A: Schooling Outcomes			
Primary completion	0.010*	-0.001	-0.011
	(0.005)	(0.005)	(0.042)
Middle-school completion	-0.003	-0.006	0.010
	(0.004)	(0.004)	(0.024)
Panel B: Labor Market Outcomes			
Ln of employment	-0.014	-0.007	-0.013
	(0.016)	(0.017)	(0.124)
Employment rate	-0.019***	-0.015**	-0.042
	(0.005)	(0.006)	(0.035)
Wage (residualized)	-0.028*	-0.020	0.045
	(0.015)	(0.018)	(0.077)
Microregion fixed effects	Yes	Yes	Yes
Average summer and winter temperatures	Yes	Yes	Yes
Municipality area	Yes	Yes	Yes
Government share in employment, 1991	Yes	Yes	Yes

Notes: Regional-level regressions using data from the 2000 and the 2010 censuses. Robust standard errors clustered at the mesoregion level in parentheses. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively.