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# Public Education Investment and Local Labor Markets:

## Evidence from a Large Federal Program in Brazil

Juan Pablo Chauvin

Department of Research and Chief Economist

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Research Department, Inter-American Development Bank

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**Abstract**

Do education investments improve regional labor market outcomes? In principle, education could lead to higher local productivity, but potential benefits to local economies could be muted if the educated workers leave in search of better opportunities, or if shifts in the supply of skills outpace demand growth. I use a large program that redistributed public education finance across Brazilian municipalities (FUNDEF) as a source of exogenous variation to empirically study the effects of expansions in public education expenditure on attainment and labor market outcomes at the individual and the local economy levels. The program was successful at improving educational attainment levels for individuals and regions, specially at the primary school level. For individuals, education led to higher wages -mainly by enabling workers to migrate to more productive places- but my estimates of returns to schooling turn negative when I control for region-of-work characteristics. For regions, the program worsened wages and other labor market outcomes but not employment, suggesting that the increased supply of educated workers outpaced demand growth.

*JEL Codes:* I2, J3, O15, O18, R11, R23

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# 1. Introduction

Policymakers often turn to education as a strategy to promote economic development in their localities in the medium and long run. However, the existing literature gives us reasons to be skeptical of this approach. While researchers have indeed established that more educated cities tend to grow faster (Glaeser et al. 1995; Glaeser and Shapiro 2003; Shapiro 2006; Gennaioli et al. 2014), it is unclear that investing in local education will necessarily result in higher local education levels, because educated individuals may leave the city if they find better work opportunities elsewhere (Abel and Deitz, 2012). This local "brain drain" may be more pronounced in low and middle-income countries, where differences in economic opportunities across rich and poor cities are oftentimes larger than those among rich and poor countries (Acemoglu and Dell 2010). Even if frictions prevent the educated population from migrating, the labor market effects of local education expansion remain ambiguous. Education can both increase the individual productivity and generate productivity spillovers, boosting labor demand (Moretti 2004.) But if the growth in the supply of educated workers outpaces demand growth, it can also push down the equilibrium local wage for this group. This paper studies empirically the effects of expanding local education on the economic outcomes of individuals and of places in the context of Brazil.

In order to capture exogenous expansions of local education investment, I use FUNDEF, a large federal policy enacted in the late 1990s. FUNDEF effectively redistributed sizable resources earmarked for primary education and middle school 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 non-predictable and uncorrelated with the local policy preferences (Estevan 2015; Menezes-Filho and Pazello 2006.)

I start by showing that FUNDEF led to an increase in the educational attainment of the individuals exposed to the policy. Following Duflo (2001), I take advantage of the fact that age at the time of implementation mediates individual's exposure to the program. Those who were of middle-school age or younger in 1998 were potentially exposed, whereas individuals that were older than middle-school age were not. I demonstrate 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-mandated one percent increase in the baseline education budget in the individual's municipality of education was associated with a 2.4 higher likelihood of completing at least primary school. The equivalent

figures are 1 for middle-school and 0.4 for high-school.[1]

Individuals exposed to FUNDEF were also more likely to migrate after finishing their education. One percentage point education budget increase was associated with a 1.2 percentage points increase in the likelihood of being a migrant in 2010 for this group, relative to those who were not exposed.

Higher local public education expenditures also led to higher wages for the beneficiaries of the policy. A one percent larger shock was associated with an average 1.7% increase in hourly wages for individuals who were in principle exposed to the program, relative to those who were not. The effects was completely driven by male workers, for whom hourly wages increased by 2.8%. The effect on female wages was statistically non-distinguishable from zero. The program also increased informality rates and unemployment, mainly among women. Its effects on labor force participation was mixed: positive along the intensive margin, and negative along the extensive margin, with minor differences across genders.

The program's impact on individual wages appears to be more related to migration than to productivity effects. I estimate a direct effect of the program on hourly wages of 5.2% for migrants, but only of 0.8% for non-migrants. The difference is highly statistically significant. Using exposure to FUNDEF as an instrument for individual educational attainment, I find average returns of 1.9% for middle school attainment, and of 0.8% for high school attainment. However, when I control for region-of-work fixed effects, these estimates become *negative* 0.6 and 1.01, respectively. These findings are consistent with prior research showing that a large fraction of the wage effects of migration are explained by the characteristics of the destination place, rather than of the individual (Clemens 2013; De la Roca and Puga 2017.) Interestingly, the gender differences in the effects on labor market outcomes are not explained by differential effects on the probability of migrating, suggesting that other mechanisms -such as male-biased joint mobility decisions (Chauvin 2018)- may be at play.

To study the effects of education expansion at the local labor market (microregion) level, I use a standard difference in differences regression set-up. After conditioning on long-term trends in local labor market outcomes, regional program intensity is uncorrelated with 1990s trends in the share of primary school educated in the working-age population, suggesting that the approach is valid in this context. This is not the case for measures of higher education attainment (i.e. middle school and high school.)

I find that FUNDEF had a positive impact on aggregate educational attainment, particularly

[1]FUNDEF may have also increased the probability of graduating from college, but the period between the year in which the policy was implemented (1998) and the year in which the outcomes are measured (2010) is insufficient to have precise measures of this effect.

at the primary school level. A program-induced one percentage point in the local public education budgets (corresponding to 1.63 standard deviations) was associated with a 7.5 percentage points increase in the the share of (at least) primary-educated in the adult population (corresponding to 0.44 standard deviations.)

In spite of increasing the share of primary-educated, FUNDEF was associated with worsening average local wages, labor force participation, formality rates, and unemployment. The evidence suggests that this is because local supply of educated labor outpaced local demand. Using program exposure to instrument for changes in the share of primary educated in the adult population, I find that a one percentage point higher share was associated with a 0.3 decrease in the average hourly wage net of observable individual characteristics and a positive -although not statistically significant- change in employment.

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) and better labor market outcomes (Jackson et al. 2016) in the U.S. context. The connection between education resources and learning outcomes is empirically weaker (Hanushek 2003.) This paper highlights an important mechanism mediating the connection between education investments and labor market outcomes, namely, the effect of these investments on the individual likelihood of migrating to more productive regions.

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 U.S. tend to migrate to places where the returns to education are larger (Heckman et al. 1996; Dahl 2002), 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). In addition to documenting similar patterns in a developing country context, my work points to large gender differences in selection. Increases in local public schools budgets in Brazil raised both educational attainment and the probability of migrating for both men and women, but while males obtained significantly better labor market outcomes, females did not.

My work also relates to the literature on the effectiveness of place-based policies. Economists have been skeptical of growth-promotion investments targeting specific cities or regions, because mobility responses may undermine potential benefits of these policies for locals (Glaeser and Gottlieb 2008; Kline and Moretti 2014). In recent work Austin et al. (2018) take a more favorable view specifically with respect to place-based policies targeting local labor demand, arguing that joblessness is a more acute social problem than low income in the U.S., and these policies are likely

more effective at alleviating it than "people-based" policies. My paper treats local education investments as place-based policy targeting labor supply, and shows that in Brazil these investments led to better outcomes for individuals but not for places. Migration was a key mediator, as individuals that obtained a higher education left to places with better economic opportunities. Existing work in the U.S. context has also found a prominent role of migration adjustments in determining the local effects of academic R&D activities (Abel and Deitz, 2012) and the establishment of new colleges (Andrews 2017.)

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). Local governments around the world motivate education expenditures as long-run development strategies. This paper shows that these investments not only can be ineffective at improving the labor market conditions of residents, but they can lead to worsened outcomes if the supply of qualified labor is not met by corresponding demand increases. 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. But their prospective effects on local economic development are not unambiguously positive.

The remainder of this paper proceeds 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 and how I use the variation introduced by the program to identify the effects of increases in education attainment on individual and on local labor market outcomes. Section 4 presents and discusses the evidence of the effects of FUNDEF on individual educational attainment, migration, and labor market outcomes. 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 Sustainment and Development of Fundamental Education and Appreciation of Teaching - FUNDEF (Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério), was enacted in July of 1998 with the goal of improving the distribution and spending

efficiency for basic and middle-school education within states. The 1988 Constitution had mandated that state and municipal governments invest at least 25% of their total revenues in public education. This rule brought about large differences 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), which the reformed aimed to correct.

The reform targeted school years 1 through 8, of which years 1 through 4 were considered primary education (educação básica) and years 5 through 8 middle school education (ensino médio).[2] It kept in place the 25% minimum requirement, 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, which then redistributed it to the municipal and state school systems according to their share in state-level enrollment for schooling years 1 through 8 (Menezes-Filho and Pazello 2006.) The reform also introduced a minimum level of spending per student. States with insufficient education budget became entitled to receive federal transfers to be able to meet this benchmark.[3] In addition, the reform mandated that 60% of the resources were to be spent in teachers' wages, while the remaining funds could be used for eligible operation and maintenance activities (De Mello and Hoppe 2005.)

The introduction of FUNDEF increased both the total resources locally spent on education and the share of municipal systems in these spendings. The program had a "decentralization" effect, in that it transferred resources from state to municipal public education systems 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 about 8% (Menezes-Filho and Pazello 2006.) The program does not appear to have crowded out resources from other sources of financing (Gordon and Vegas 2005.)

The program had a relatively minor impact on the level of education in 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) in Fundamental Education. The program did lead to a small initial reduction in expenditures in pre-school education (Menezes-Filho and Pazello 2006.) 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.

[2]Primary education was extended from 4 to 5 years to include kindergarten in 2003.

[3]The impact of these transfers in the overall policy was relatively small. In 1998, a total of 8 out of 26 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 to 1.8% of the total funds (De Mello and Hoppe 2005.)

At the time of the introduction of the policy, the vast majority of students were enrolled in public education. Table 1 provides a break down of enrollment in the year 1997 for the grades affected by the program by school system. 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).[4] Within public school enrollment, about 40% was in municipal systems and 60% in state systems.

**Table 1:** Enrollment in Fundamental Education in Brazil in 1997, by system

| Grades | Total enrollment | School System | | | |
|---|---|---|---|---|---|
| | | Municipal | State | Federal | Private |
| 1 | 6,575,734 | 58.2% | 33.9% | 0.0% | 7.9% |
| 2 | 5,154,094 | 46.7% | 43.6% | 0.1% | 9.6% |
| 3 | 4,724,389 | 41.6% | 48.1% | 0.1% | 10.3% |
| 4 | 4,113,911 | 38.9% | 49.8% | 0.1% | 11.2% |
| 5 | 4,510,872 | 21.5% | 68.0% | 0.1% | 10.4% |
| 6 | 3,630,218 | 19.8% | 68.0% | 0.1% | 12.0% |
| 7 | 2,993,337 | 18.0% | 68.2% | 0.2% | 13.6% |
| 8 | 2,526,833 | 16.4% | 68.1% | 0.2% | 15.4% |
| Primary Education (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 Education (1-8) | 34,229,388 | 36.3% | 52.9% | 0.1% | 10.7% |

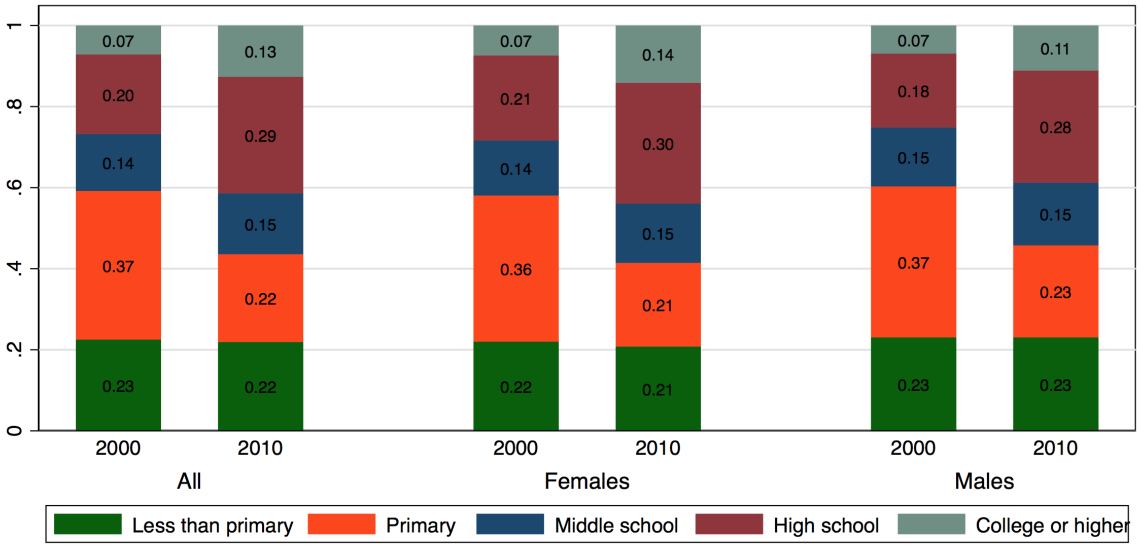
**Source:** Brazilian Education Census of 1997.

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 in education at all levels starting in the early 1990s (Menezes-Filho 2001; De Barros et al. 2006.) Figure 1 shows the percentage of the adult population in each educational attainment category at the beginning and at the end of the decade. The share of the population with primary education or less went 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%. Females expanded their favorable schooling gap relative to males. By the end of the decade, 44% of adult women had achieved at least high-school education, compared to 39% of adult men. Existing research has shown that FUNDEF played a role in the increases in enrollment,

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

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).[5]

**Figure 1:** Percentage of Brazilian population age 23 or older in each educational attainment category




**Source:** Population censuses of 2000 and 2010.

The 2000s was also a decade of improving labor market conditions, especially for the *least educated* population. Figure 2 shows decade-long changes in labor market outcomes for adults wage earners aged 23 through 64 in five different education groups. During this decade, employment rates increased by 5 percentage points for workers with less than primary education. The increases were less pronounced at higher educational categories and were only of 1.5 percentage points for workers with a college degree or higher. This aggregate pattern is largely driven by females (Appendix Figure A.1.) Among males, the increase in employment rates was generally smaller and was more pronounced among higher education groups.

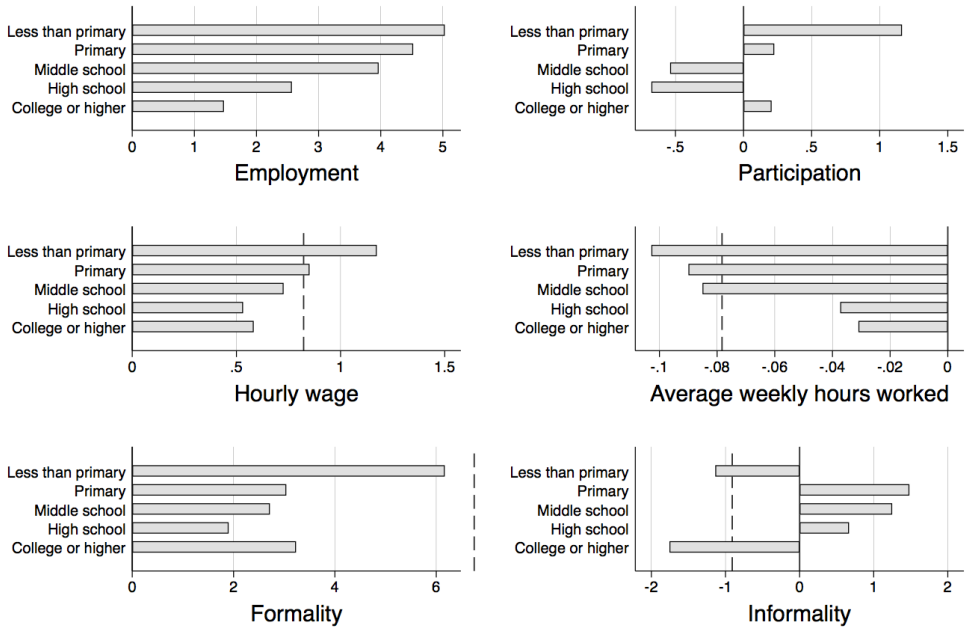
While part of the low-education employment growth reflects a recovery from unusually high unemployment rates in the 1990s,[6] a dominating force behind employment growth comes from

[5]Other social programs introduced during the 2000s, and in particular conditional cash transfers that required low-income families to enroll their children in school (Bolsa Escola and Bolsa Família), could also have had 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 Família included stipends four youth aged 15 to 17 to attend school, while simultaneously FUNDEB expanded coverage of supply-side subsidies to high-school (OECD 2011.)

[6]During that decade, national unemployment rates grew sharply in Brazil. These trends were particularly severe among semi-skilled and low-skilled workers (Reis 2006.)

changes in female labor force participation (Corseuil et al. 2010.)[7] Prior studies have found a strong positive association between education levels and female labor force participation in Brazil during the 1980s and 1990s (Scorzafave and Menezes-Filho 2001; Soares and Izaki 2002). During the 2000s, participation also increased significantly among low-education females (Appendix Figure A.1.) Participation rates actually decreased among males over this period, particularly at lower education levels. These changes were apparent mostly along the extensive margin. The intensive margin of participation (average weekly hours worked) decreased over the period, at rates that were similar for both genders and more pronounced at lower education levels.

**Figure 2:** Changes in labor market outcomes 2000-2010 by educational attainment category




**Note:** Restricted to wage-earning population aged 23 through 64. Dashed lines denote population averages. All estimates are own calculations from microdata using sample weights. See the data appendix C for details on the measurement of each variable. **Source:** Population censuses of 2000 and 2010.

Much of the employment growth in this decade went towards formal jobs, but an important share of the increase in female labor force participation was also absorbed by the informal sector. Changes in formality rates were most pronounced at both the lowest and the highest education categories. For both of these groups, aggregate formality rates increased *and* aggregate informality rates decreased over the period (Figure 2.) These aggregates, however, mask important gender heterogeneity. While formal employment increased for both males and females, informal employment

[7]Prior studies have also argued that the increase in female labor force participation was partly a response to increased unemployment among male heads of household (Fernandes and de Felicio 2005.)

dropped for males (particularly among the least educated) but *increased* for females (Appendix Figure A.1). Gender differences are more pronounced at middle-education levels (primary, middle school, and high school), where growth in female informal employment drives the overall increase in informality rates over the period.

Lower-education workers also saw notable wage increases in the 2000s. Adult wage-earners with less than primary education saw their average hourly wage more than double over the 2000s in real terms. For workers with a college education, the increase was close to 60%. The accelerated growth in both wages and employment during this decade is consistent with a net increase in labor demand, particularly among low-education groups.[8]

## 3. Measures, data and identification

This paper's empirical strategy relies on the redistribution of public education finance across municipalities introduced by FUNDEF. Because the program led to an increase in enrollment (De Mello and Hoppe 2005; Cruz and Rocha 2018) the program should also have had a measurable effect on the average educational attainment of individuals after school age and can potentially be used as a source of exogenous variation.

I study the effect of FUNDEF on educational attainment and on labor market outcomes at the individual and at the regional level. This section starts by describing the measures used and their data sources. It then turns to discussing the baseline empirical specifications employed, as well as the identifying assumptions.

### 3.1. Measures and data

To capture the impact of FUNDEF on local education finance I use the program-induced proportional change in local educational budget, following Estevan (2015). This municipality-level variable measures, for all education systems operating in the jurisdiction (municipal or state-level), the gap between the funds received from FUNDEF in the first year of implementation of the program[9] and

[8]Prior studies have documented a relative increase in demand of low-education workers during the preceding decade, linking it with the national trade liberalization policy. Gonzaga et al. (2006) show that, following the early 1990s liberalization, employment shifted from high-skilled to low-skilled sectors (although the share of high-skilled in both sectors increased.) The skills wage differential dropped over this period. Dix-Carneiro and Kovak (2017) find that employment loss in regions that were hit the hardest by trade liberalization in the 1990s became more severe -rather than mean-reverting- during the 2000s.

[9]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. There is evidence showing that some muncipalities 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.)

the funds contributed to the program (15% of the total revenues) in the same year.[10] This gap is expressed as a share of the funds contributed to the program.

Formally, the municipality-level "FUNDEF Shock" measure is defined, for municipality $j$, 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 observations are school systems, denoted by the superscript $e = \{m, s\}$, which can be municipal ($m$) or state-level ($s$). The main weight is the share of the municipal system $e$ located in municipality $j$ in the state-level enrollment in public education in 1997 ($\eta^e_{j,97}$). The term in parenthesis is the program-induced percentage change in education transfers, where $I^e_{m,98}$ is the money that the municipal system received from FUNDEF in 1998, and $O^e_{m,98}$ the money that it contributed to the program's state-level fund.[11]

While this measure is useful to capture the exposure of a particular individual to FUNDEF, it doesn't adequately capture the incidence of the program in a particular local economy. This is because local labor markets in Brazil oftentimes incorporate two or more geographically proximate municipalities. Thus, in order to study the effects of FUNDEF on the aggregate outcomes of local economies - which I refer to as "regions" throughout the paper - I use a regional-level shock, namely:

$$FS_r = \sum_{j \in r} \varsigma_{j,97} \times FS_j \tag{2}$$

where $\varsigma_{j,97}$ is the share of municipality $j$ in region $r$'s school-age population.

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 (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

[10] Local education budgets come from four taxes and transfers (FPM/FPE, IPIExp, LC87/96 and ICMS), as determined in the constitution of 1988. The FUNDEF policy applies to the money related to these sources.

[11] A concern raised by Kosec (2014) is that the 1998 revenues may be affected by omitted variables (e.g. macroeconomic fluctuations) that also affect directly 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:

$$FS^{pred}_j = \sum_{e \in \{m,s\}} \eta^e_{j,97} \left( \frac{I^e_{j,97} - O^e_{j,97}}{O^e_{j,97}} \right)$$

where $O^e_{j,97}$ corresponds to 15% of the actual 1997 revenues, and $I^e_{j,97}$ is a simulated FUNDEF transfer, based on enrollment shares and simulated total value of each state-level FUNDEF fund in 1997. I replicate all the analyses using this alternative measure, and I obtain virtually identical results.

proposed by Kovak (2013).

The data used in this analysis comes from multiple sources. The enrollment data comes from the Brazilian School Census. 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) and were compiled by Estevan (2015). The school-age population shares, as well as well as most of the outcome variables and controls, are constructed from the microdata of the decennial population censuses published by the IBGE. Appendix Figure A.2 shows the distribution of the FUNDEF shock measured at the municipal level and at the regional level. Appendix tables A.1 through A.5 report summary statistics and correlations for individual and regional-level variables. Appendix C offers a detailed description of each variable and their sources.

### 3.2. Identification of individual effects

The first part of the analysis focuses on the effect of education on individual's educational attainment and labor market outcomes. An important limitation is that 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. Therefore my analysis is based on educational attainment categories.

To capture the direct effect of FUNDEF on individual outcomes, I follow Duflo (2001), and 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. Specifically, in my baseline specification I estimate:

$$Y_{ijb} = \beta_0 + \sum_{a=l}^{h} (FS_j \times d_{ia})\, \beta_{1,a} + \beta_2 FS_j + \sum_{a=l}^{h} (E_{j,97} \times d_{ia})\, \beta_{3,a} + \beta_4 C_{j,97} + \beta_k + \beta_r + \epsilon_{ijb} \qquad (3)$$

where the dependent variable $Y_{ijb}$ is the outcome of interest measured in 2010 for individual $i$, educated in municipality $j$ and born in year $b$. $FS_j$ is the FUNDEF shock in municipality $j$ (equation 1), $d_{ia}$ is a dummy that takes the value one if individual $i$ was age $a \in [l, h]$ in 1998, $E_{j,97}$ are fundamental education enrollment rates in municipality $j$ in 1997, $C_j$ is a vector of municipality of origin controls (ten age-group shares in the total population of municipality $j$ in 1997), $\beta_k$ is a cohort of birth fixed-effect, and $\beta_r$ a region of work fixed effect (only used in of the some specifications in which $Y_{ijb}$ are individual labor market outcomes.)

The set of cohorts included in each regression ($a \in [l, h]$) vary depending on the outcome variable. The youngest cohort $l$ is chosen to ensure that the individuals included in the analysis were old-enough in 2010 for the outcome variable to be adequately measured. For instance, if the outcome

variable is a dummy for having attained high-school education or higher, I use $l = 6$ so that the youngest cohort included was age 18 in 2010 (age 17 corresponds, in theory, to the last year of high school). For labor market outcomes I use $l = 3$ to ensure that all cohorts included were of working age in 2010. In all individual specifications I restrict the analysis to younger cohorts (up to age 40 in 2010). My baseline specification uses $h = 27$, and uses the cohort aged 28 in 1998 as the reference group.

For the coefficients $\beta_{1,a}$ to be given a causal interpretation, a given cohort's exposure to the program should be independent of the error term $\epsilon_{ijb}$ conditional on the controls. Exposure to the program is a function of the individual's year of birth and the individual's municipality of education. Year of birth is exogenous. Municipality of education may, in principle, be endogenous if families with school-aged children selectively migrated towards beneficiary reasons. In practice, this appears unlikely because FUNDEF benefited regions where education opportunities were lagging relative to others. However, I replicate the analysis using region of birth[12] instead of region of education as a robustness check.

Given that this is a difference-in differences set up, the causal interpretation relies also on the assumption that, in the absence of the program, the changes in $Y_{ijb}$ 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.

### 3.3. Identification of regional effects

The second part of the analysis turns to the effects of investments in public education on aggregate local education attainment levels and labor market outcomes. The unit of observation is the microregion. My preferred specification for all reduced-form analysis is the standard difference-in differences regression set up, namely:

$$Y_r = \alpha_0 + \alpha_1 Post + \alpha_2 FS_r + \alpha_3 (Post \times FS_r) + \alpha_4 \left(Post \times C_r\right) + \epsilon_r \tag{4}$$

where $Y_r$ is the outcome of interest in region $r$, $Post$ is a dummy that takes the value 1 for the year 2010 (post treatment) and zero for the year 2000 (pre treatment), $FS_r$ is the regional-level shock from equation 2, and $C_r$ is a vector of regional-level lagged changes in local labor market conditions (measured in the 1980-1991 decade.)[13]

[12] I do not observe region of birth directly in the data, but I can infer it for most individuals using their migration and residence data.

[13] Use 1980-1991 to measure pre-trends because the alternative (1991-2000) includes three years in which the

The key identifying assumption is that of "parallel trends", namely, that in the absence of the program changes in $Y_r$ would not have been systematically different between high-incidence and low-incidence regions, conditional on the controls. The use of lagged trends controls in this case is important, because the program targeted low-enrollment municipalities, and having low enrollment is likely correlated with pre-existing trends in the share of educated workers in the labor force, and in labor market outcomes.

# 4. Individual-level results

This section focuses on the effects of FUNDEF at the individual level. I start by exploring the effects on educational attainment. Second, I turn to the effects on the likelihood of migrating. Third, I look at labor market outcomes effects, and how they differ among men and women, and among migrants and non-migrants. Finally, I explore to what extent labor market outcomes can be explained by characteristics of the place of work, as opposed to individual-level characteristics.

## 4.1. Effects on individual educational attainment

I first turn to the effects of FUNDEF on individual educational attainment. Figure 3 plots the estimated coefficients $\hat{\beta}_{1,a}$ for cohorts $a \in [l, h]$ from a linear probability estimation of equation 3. These measure the effect of the exposure to the program on the likelihood of having a given educational attainment in 2010 for each cohort relative to the cohort aged 28 in 1998. The figure looks at four left-hand-side dichotomous measures of education attainment, namely, primary, middle school, high school and college. All measures take a value of one if the individual has achieved *at least* that attainment level in 2010.

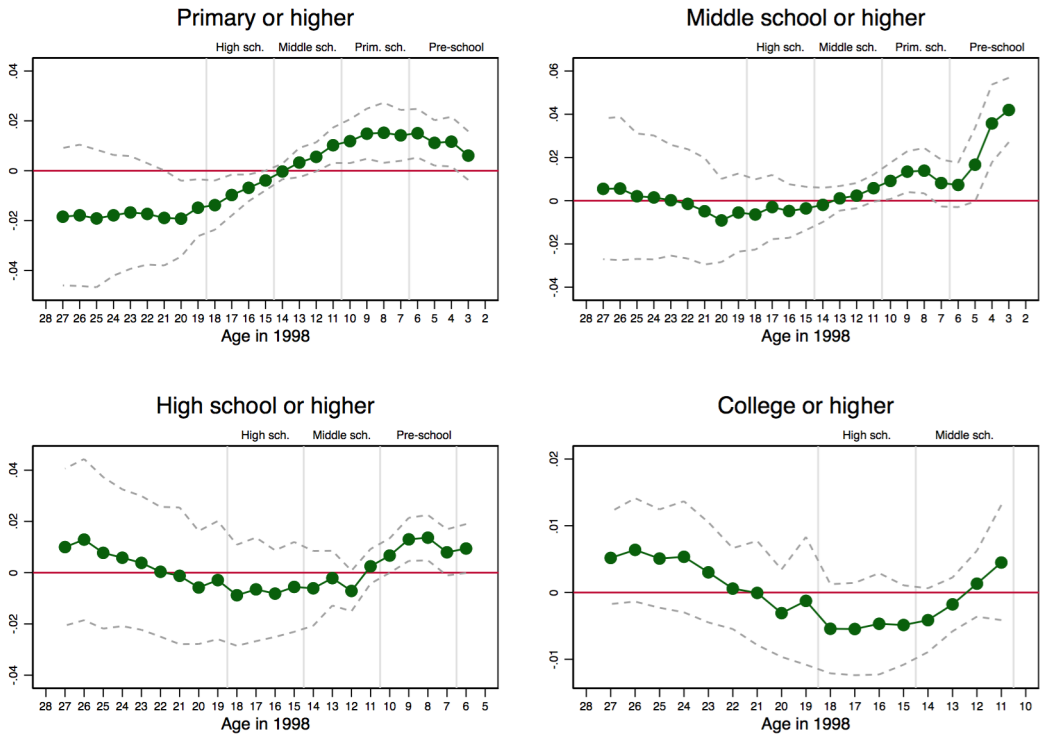
If the program had an effect on individual educational attainment, we should observe it in the cohorts that were exposed to the program, and see no effects in the cohorts that were not exposed. Figure 3 shows the education level that corresponded to each cohort's age in 1998. Recall that the program targeted primary and middle school. This implies that the cohorts that were enrolled in these educational levels, as well as younger cohorts, were in theory exposed to the program, and older cohorts were not. Consequently, the $x$ axes of the graphs in Figure 3 capture exposure to the program, the younger the individual, the greater the exposure. Individuals aged 6 or less in 1997 entered primary school when the program was already in place, and were in principle fully exposed.

According to these estimates, FUNDEF had a positive effect on primary school attainment,

program was already in place.

particularly among younger cohorts. A program-induced one percent higher local education budget in the municipality of education led to an increase of around 2 percentage points in the likelihood of having completed at least primary education for most cohorts that were at least partially exposed to the program, relative to the cohort aged 28 in 1998. I also find positive effects in individuals whose age in 1998 corresponded to the first two years of middle-school. This could be potentially explained by late school entrance and high repetition rates.[14]

**Figure 3:** Effects of FUNDEF on probability of reaching a specific educational attainment in 2010 by cohort




**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

FUNDEF also had a positive effect on middle school and high school completion. This is consistent with existing work showing that investment at lower education levels can increase enrollment and attainment at higher levels (Hyman 2017.) The effect is present and statistically significant for the cohorts that were, in theory, enrolled in primary school at the time that the program started.

[14]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 that were old enough to have finished middle school by 1998 were still eligible to attend school and thus could have benefited from FUNDEF.

In the case of middle school attainment, it is noticeably larger and significant for the cohorts that were yet to enter primary school in 1998. In both cases, the effect is lower and not statistically significant for individuals that were ages 6 and 7 in 1998, suggesting that these cohorts had more difficulty in completing high-school than others. A potential explanation is that these cohorts faced a more challenging economic environment than others, given that they would have been in the last and second-to-last years of high school during the 2009 recession.

The trend of the coefficients across cohorts suggest that the program may have also increased the probability of graduating from college, but the available sample is insufficient to estimate this effect precisely. Individuals that were old-enough to have left college in 2010 were already enrolled in middle school in 1998, which implies that I can use for estimation only the four cohorts that were, in principle, the least exposed to the policy.

The figures show that the identification strategy is reasonable in this context. Both in the case of middle school and of high school, the cohorts that were in theory not exposed to the program had an educational attainment that was not statistically different than that of the comparison group in 2010.[15]

I obtain measures of the average effect of the program on the individual educational attainment by comparing all the cohorts that were, in principle, exposed to the program, to all the cohorts that were not. Specifically, I estimate the following variant of equation 3:

$$Y_{ijb} = \beta_0 + \beta_1 \left(FS_j \times T_i\right) + \beta_2 FS_j + \sum_{a=l}^{h} \left(E_{j,97} \times d_{ia}\right) \beta_{3,a} + \beta_4 C_{j,97} + \beta_k + \epsilon_{ijb} \tag{5}$$

where $T_i$ is a dummy that takes the value one if individual $i$ was age 14 or younger in 1998, and the value zero if they were age 15 or older. Estimates of $\hat{\beta}_1$ are reported in Table 2.

The results suggest that, on average, one percent increase in the education budget in the individual's municipality of education led to a 2.4 percentage points higher likelihood of having completed at least primary school in 2010 for individuals who were in theory exposed to the program, relative to those who were not. The effects on the probability of completing at least middle and high school were of 1 and 0.4 percentage points, respectively. I don't find a significant average effect on the probability of completing college. Finding smaller effects at higher education levels is what we would expect given the program's target groups. By means of comparison, Duflo (2001) finds that a large school construction program in Indonesia induced about 6% of the population to complete at

[15] In the case of the primary education measure, I do find a significant negative effect for cohorts aged 16 through 20 in 1998. This may reflect an omitted variable that was correlated with municipal FUNDEF exposure and negatively affected primary school enrollment in these cohorts in prior years.

least primary education. She also finds a smaller effect on middle-school completion, and a negative effect on high-school completion.

FUNDEF appears to have had a stronger educational attainment effects among males than among females. Columns 2 and 3 in Table 2 report separate estimations for each gender from a seemingly unrelated regressions (SUR) model. Column 4 presents the results of tests of differences between the male and female coefficients. The point estimates are larger for males than for females in all attainment categories, although the difference is not statistically significant for middle school. Interestingly, in the gender-specific regressions I obtain statistically significant effects for college education attainment, which are positive for men, and negative for women. Appendix Figure A.3 presents estimations of cohort-specific effects by gender.

**Table 2:** Effects of FUNDEF on probability of reaching of reaching a specific educational attainment

| | All | By gender | | |
|---|---|---|---|---|
| | | Male | Female | Test |
| | (1) | (2) | (3) | (4) |
| | | | | *(F-stat and p-val.)* |
| Panel A: Lowest education attained | | | | |
| Primary school or higher | 0.024*** | 0.026*** | 0.022*** | 17.67 |
| | (0.001) | (0.001) | (0.001) | 0.000 |
| Middle school or higher | 0.010*** | 0.010*** | 0.009*** | 1.61 |
| | (0.001) | (0.001) | (0.001) | 0.204 |
| High school or higher | 0.004*** | 0.005*** | 0.002** | 4.69 |
| | (0.001) | (0.001) | (0.001) | 0.030 |
| College or higher | 0.000 | 0.003*** | -0.003*** | 29.14 |
| | (0.001) | (0.001) | (0.001) | 0.000 |
| Cohort of birth dummies | Yes | Yes | Yes | Yes |
| Enrollment rates times cohort of birth dummies | Yes | Yes | Yes | Yes |
| Demographic structure controls | Yes | Yes | Yes | Yes |

**Note:** The table reports the coefficients on the treatment variable in equation 5. Regressions are at the individual level. Robust standard errors clustered at the municipality of education level in parentheses. Column 4 reports results of adjusted Wald tests of hypotheses of the type $H_0 : \beta_{males} - \beta_{females} = 0$ on SUR models' coefficients in columns 2 and 3. *** p<0.01, ** p<0.05, * p<0.1.

## 4.2. Effects on likelihood of migrating

Increases in local provision of education may lead to increased out-migration. If labor demand for educated workers is unevenly distributed in the national geography, newly-educated workers in places with low demand for skills will have the incentive to leave looking for opportunities that better match their qualifications. Moreover, if migration is costly (Morten and Oliveira 2016), individuals

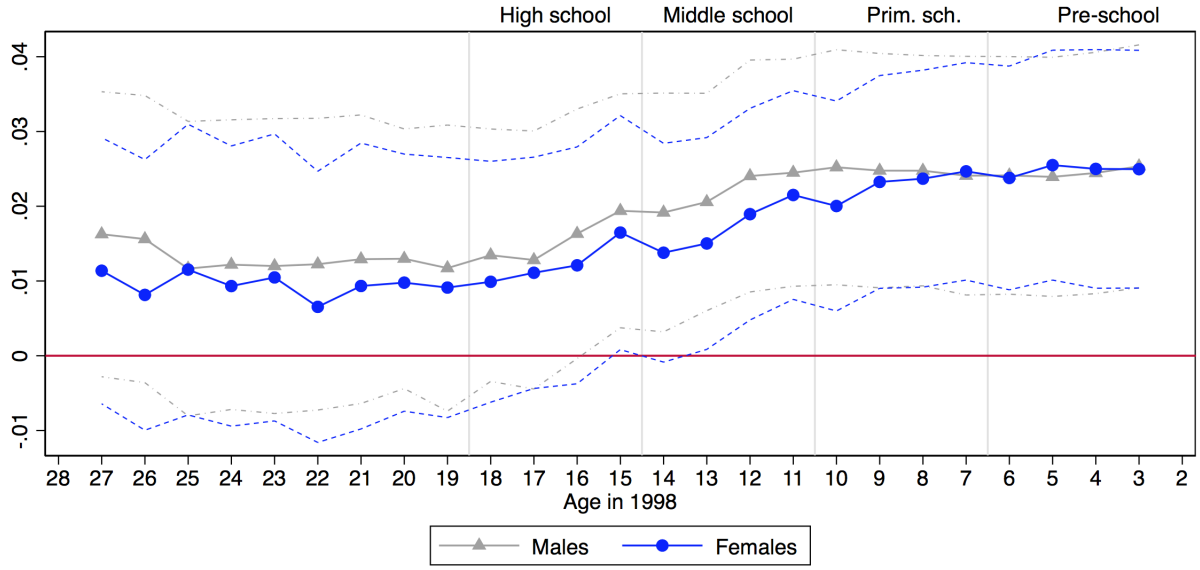
may be closer to the margin of migrating as they get educated and their potential income increases. Austin et al. (2018) document that, in the U.S., prime-age male migrants are on average more educated than the non-migrant population in their place of origin.

The effects of education on migration will, in turn, mediate the effects of education expansion on aggregate local labor market outcomes. Higher levels of education may make individuals more productive and give them access to higher paying jobs, but if the supply of educated workers grows faster than the demand, the local private returns to education may be minimal or even negative. In that case, migration may allow individuals to obtain higher returns from their education, and potentially improve the returns of local non-migrant educated workers by alleviating excess supply.

If the effect of education on local labor market outcomes comes primarily from productivity spillovers, then improving local levels of education can lead to in-migration through increased labor demand. And the education profile of immigrants will, in turn, shape the aggregate education levels of the local economy further.

My empirical results suggest that individuals who were exposed to FUNDEF were more likely to migrate than the reference group. Figure 4 depicts the estimates for $\hat{\beta}_{1,a}$ in a linear probability estimation of equation 3, where the outcome of interest is a dummy for being a migrant. Here, migrant is defined as a person that in 2010 was living in a region different than the one where their municipality of education was located. The coefficients are statistically significant only for the cohorts that were, in theory, at least partially exposed to the program. Some of the point estimates appear to be higher, on average, for males, but gender differences are not statistically significant in this specification.

**Figure 4:** Effects of FUNDEF on probability of being a migrant in 2010 by cohort and gender




**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

Table 3 reports measures of the effect of the program on two measures of migration. The first measure defines migrant as someone that in 2010 was living in a municipality different than their municipality of education (even if it was in the same microregion). The second measure includes only migrants that in 2010 lived in a different microregion (the same definition as in Figure 4). Columns 1 through 3 report estimates of the reduced-form effect of the program ($\hat{\beta}_1$ in equation 5.) On average, one percentage point increase in the FUNDEF shock was associated with a 1.2 percentage points higher probability of migrating to a different municipality, and a 0.8 percentage points higher probability of migrating to a different microregion for the beneficiaries of the policy. These average (across cohorts) results also confirm that there are no measurable gender differences in the program's migration effects.

To explore the extent to which the effects of the program on migration operates through its effect on individual educational attainment, I estimate the following model using 2SLS:

$$Y_{ijb} = \beta_0 + \beta_1 \times d_{i,sch} + \sum_{a=l}^{h} \left(E_{j,97} \times d_{ia}\right) \beta_{3,a} + \beta_4 C_i + \beta_5 C_{j,97} + \beta_k + \beta_r + \epsilon_{ijb} \tag{6}$$

where $d_{i,sch}$ is a dummy that takes the value one if individual $i$ has attained the level of schooling

$sch = \{p, ms, hs, c\}$ in 2010,[16] and $C_i$ is a vector of individual-level characteristics, including sex and race (variation on individual age is already captured by the cohort of birth dummies). I instrument for $d_{i,sch}$ using the interactions of the FUNDEF shock with the cohort of birth identifiers, $FS_j \times d_{ia}$. The estimates are reported in columns 5 through 7 of Table 3.

The results suggest that increases in the probability of a higher education attainment lead to increases in the likelihood of migrating. The estimated effects are larger for primary education, and when explaining migration across municipalities (as opposed to migration across microregions). When using higher measures of educational attainment (middle school and high school) as the instrumented explanatory variable, I also find large and significant effects on migrating to a different municipality, and relatively smaller effects on migrating to a different microregion.

**Table 3:** Individual effects of FUNDEF on migration

| | Reduced-form effects of FUNDEF on migration | | | | 2SLS estimates of effects of education on migration | | |
|---|---|---|---|---|---|---|---|
| | All | By gender | | | By educational attainment | | |
| | | Males | Females | Test | Prim. | Mid-sch. | High-sch. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | | *(F-stat and p-val.)* | | | |
| Probability of migrating | 0.012*** | 0.012*** | 0.012*** | 0.42 | 0.461*** | 0.531** | 0.226** |
| to a different municipality | (0.000) | (0.000) | (0.000) | 0.51 | (0.121) | (0.240) | (0.110) |
| Probability of migrating | 0.008*** | 0.007*** | 0.009*** | 0.18 | 0.326*** | 0.357** | 0.139* |
| to a different microregion | (0.001) | (0.001) | (0.003) | 0.42 | (0.076) | (0.148) | (0.077) |

**Note:** Columns 1 through 3 reports linear probability estimates of $\hat{\beta}_1$ in equation 5. Column 4 reports results of adjusted Wald tests of the hypothesis $H_0 : \beta_{males} - \beta_{females} = 0$ on SUR models' coefficients in columns 2 and 3. Columns 5 through 7 report estimates of $\hat{\beta}_1$ in equation 6 where the instruments are the interactions of the FUNDEF shock in the municipality of education and the cohort fixed-effects. All regressions use weighting based on sample design. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

### 4.3. Effects on individual labor outcomes

I turn now to the analysis of the effects of FUNDEF on individual labor market outcomes. Figure 5 shows estimates of the cohort-specific effects ($\hat{\beta}_{1,a}$ in equation 3) on wages and labor force participation. The figure at the top presents results for hourly wages net of observable individual characteristics. The two figures at the bottom measure labor force participation. To capture the

[16] The levels of schooling are defined as having at least a given educational attainment, where attainment can be primary ($p$), middle school ($ms$), high school ($hs$) or college ($c$).

extensive margin of labor force participation, I use a dummy that takes a value one if the individual is either formally employed, informally employed, or unemployed in 2010. To measure the intensive margin, I use the average number of paid hours worked per week. All regressions are based on a sample that includes individuals aged 15 through 40 in 2010, except for those that were enrolled in school in that year. Appendix C provides further details on measurement.

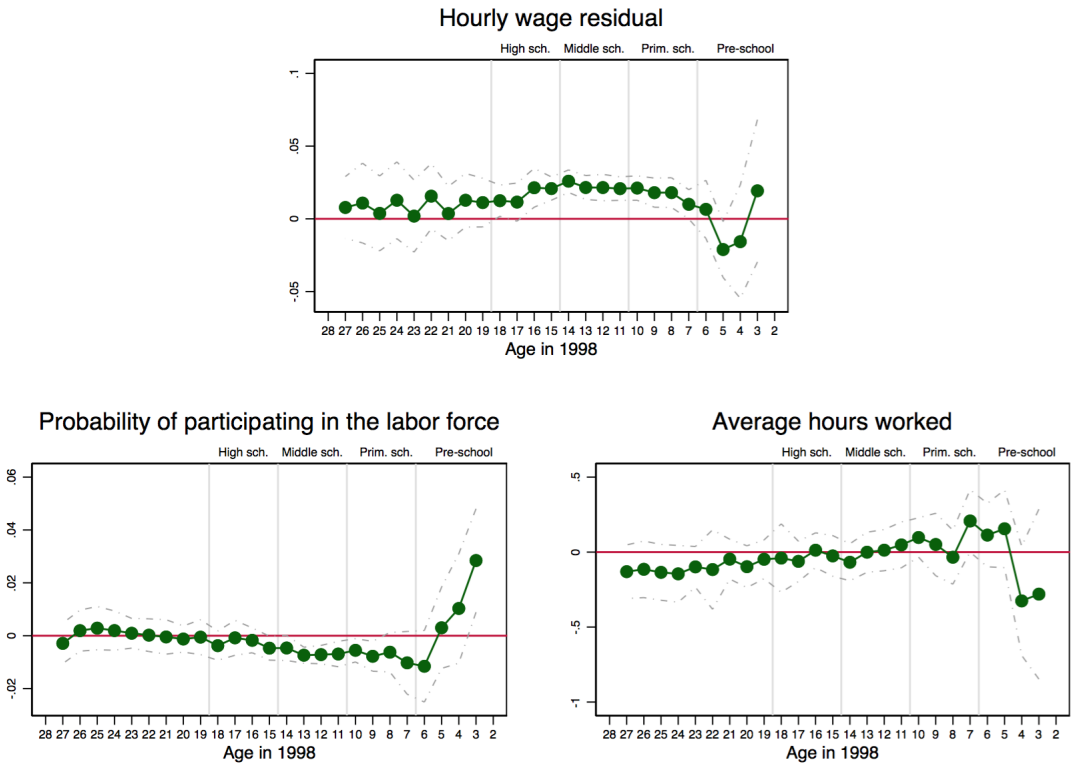
In contrast with the first-stage estimates discussed in section 4, when looking at labor market outcomes there is no expectation that the coefficients for cohorts that were in theory not exposed to FUNDEF should necessarily be zero. This is because all cohorts work in the same labor markets. Workers from different cohorts may be complements or substitutes in production, or increases in education in a subset of workers may lead to positive or negative spillovers on others.

FUNDEF had a positive impact on wages for most cohorts exposed to the program. As shown in the top graph in Figure 5, the only exceptions are the cohorts aged 4 through 7 in 1998. The cohort aged 4 in 1998 turned working age in 2009, a recession year, which may explain this pattern. Significant effects on individuals older than middle-school age in 1998 could be due to spillovers or, more plausibly, by late school entrance and high repetition rates. Similar results are obtained for alternative wage measures, and are reported in Appendix Figure A.4.

Table 4 provides measures of the average effects (across exposed cohorts) based on equation 5. One percent increase in the education budget in the individual's municipality of education led, on average, to a 1.7% increase in hourly wages and a 1.9% increase in monthly wages for individuals who were in theory exposed to the program, relative to those who were not. Jackson et al. (2016) also find positive effects of K-12 education spending on wages in the U.S. context.

I find a sharp gender difference in the wage effect. While a one percent FUNDEF shock led to a 2.8% increase in male hourly wage, the effect on female wages was statistically non-distinguishable from zero. Such gender differences are only significant in the generations that experienced a positive wage effect, as shown in Appendix Figure A.5. These results are consistent with Chauvin (2018), who finds that over the 1990s and 2000s, males wages grew when local labor demand increased while female wages did not.

**Figure 5:** Effects of FUNDEF on wages and labor force participation




**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

FUNDEF had, on average, a negative effect on labor force participation. A one percent FUNDEF sock was associated with an average 0.5 percentage points reduction in the probability of participating in the labor force for the exposed cohorts. The effect was notoriously different for the cohorts that were fully exposed to the program (ages 6 and younger in 1998), for whom exposure is associated with increased labor force participation. The gender differences in the effects also vary across cohorts. While, on average, women exposed to the program decreased their participation more than men exposed to the program (Table 4), among the younger cohorts women had a larger participation increase (Appendix Figure A.5).

In contrast, I find a significant positive effect on the intensive margin of participation (average hours worked) for both men and women. Again, this net positive effect has a very different explanation for each gender. As depicted in Appendix Figure A.5, the positive effect among males reflects a decline in participation of the older generation (those not affected by the program). Among females, it reflects an increase in the number of hours worked of the generations affected by FUNDEF.

**Table 4:** Effects of FUNDEF on individual labor market outcomes

| | All | By gender | | | By migrant | | |
|---|---|---|---|---|---|---|---|
| | | Male | Female | Test | Non-mig. | Migrant | Test |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | | | | *(F-stat and p-val.)* | | | *(F-stat and p-val.)* |
| Hourly wage | 0.017*** | 0.028*** | 0.003 | 56.44 | 0.008*** | 0.052*** | 117.59 |
| | (0.002) | (0.003) | (0.004) | 0.000 | (0.002) | (0.004) | 0.000 |
| Monthly wage | 0.019*** | 0.031*** | 0.003 | 88.32 | 0.009*** | 0.055*** | 182.03 |
| | (0.001) | (0.003) | (0.004) | 0.000 | (0.002) | (0.003) | 0.000 |
| Labor force participation | -0.005*** | -0.004*** | -0.006*** | 3.53 | -0.007*** | 0.004*** | 35.88 |
| | (0.001) | (0.001) | (0.001) | 0.060 | (0.001) | (0.002) | 0.000 |
| Weekly hours worked | 0.116*** | 0.156*** | 0.115** | 0.52 | 0.090*** | 0.109 | 0.07 |
| | (0.028) | (0.036) | (0.049) | 0.469 | (0.031) | (0.068) | 0.796 |
| Formality | -0.004*** | -0.002 | -0.009*** | 16.83 | -0.006*** | 0.003 | 16.80 |
| | (0.001) | (0.001) | (0.002) | 0.000 | (0.001) | (0.002) | 0.000 |
| Informality | 0.003*** | 0.002 | 0.005*** | 3.81 | 0.005*** | -0.003* | 12.86 |
| | (0.001) | (0.001) | (0.001) | 0.051 | (0.001) | (0.002) | 0.000 |
| Unemployment | 0.001* | -0.000 | 0.004*** | 11.05 | 0.002* | 0.001 | 0.35 |
| | (0.001) | (0.001) | (0.001) | 0.001 | (0.001) | (0.001) | 0.552 |
| Cohort of birth dummies | Yes | Yes | Yes | | Yes | Yes | |
| Enrollment x cohort controls | Yes | Yes | Yes | | Yes | Yes | |
| Demographic structure controls | Yes | Yes | Yes | | Yes | Yes | |
| Region of work fixed effect | Yes | Yes | Yes | | Yes | Yes | |

**Note:** The table reports the coefficients on the treatment variable in equation 5. Regressions are at the individual level. Robust standard errors in parentheses. All regressions use weighting based on sample design. Columns 4 and 7 report results of adjusted Wald tests of hypotheses of the type $H_0 : \beta_{males} - \beta_{females} = 0$ on SUR models' coefficients in columns 2 and 3, and columns 5 and 6, respectively. *** p<0.01, ** p<0.05, * p<0.1.

Conditional on participating in the labor force, the program decreased the probability of becoming formally employed. A one percentage point higher FUNDEF shock was associated with an average 0.4 percentage points reduction in the probability of formal employment. Part of this change was absorbed by an increased probability of informal employment (0.3 percentage points), and part by an increased probability of unemployment (0.1 percentage point.)

Virtually all of these other negative effects on employment outcomes are driven by women. I find no measurable effect on males' formality or unemployment rates. In the women-only sample, the program is associated with a 0.9 drop in the probability of formal employment, matched with a 0.5 percentage points increase in the probability of informal employment and a 0.4 increase in the probability of unemployment.

## 4.4. Mechanisms

A possible explanation for the positive effect of FUNDEF on average wages and other labor market outcomes is that higher education attainment made workers more productive, leading to higher

incomes. Prior research has found large returns to education in developing countries. Duflo (2001) showed that a large school construction program in Indonesia in the 1970s led to an increase in the average years of schooling of the population exposed to the program, and wage returns to a year of schooling in the range of 6.8 to 10.6 percent in a sample restricted to males.[17]

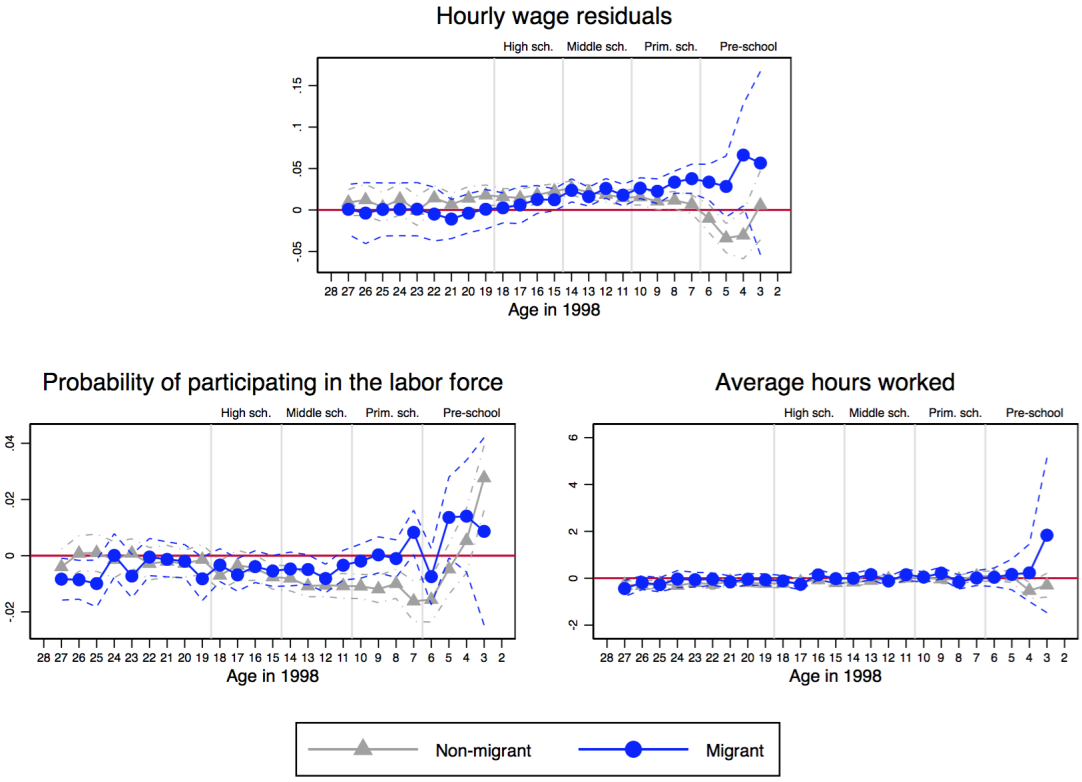
An alternative explanation is that educated workers obtained higher incomes because they became able to move to more productive places. Internal mobility in Brazil is relatively high,[18] and we have already seen that the program led to out-migration among its beneficiaries (Section 4.4.) Moreover, multiple studies have attributed at least part of the wage premium of migrants to the characteristics of their destination place. Glaeser and Maré (2001) showed that moving to cities gives workers both a static and a dynamic wage effect, so that there urban wage premium accrues over time for workers who live in MSAs, and remains with them after they leave. De la Roca and Puga (2017) found similar results in Spain, where workers who move to larger cities have a discrete increase in wages upon migrating, and accumulate human capital at a faster pace than workers that stayed in smaller cities. Clemens (2013), using the U.S. visa lottery as a source of exogenous location for employees of a software firm, found large wage differences between programmers that stay in India and those who migrate to the U.S., which seems to be derived exclusively from the location.

In an effort to tell apart these alternative explanations, I start by estimating the effects of FUNDEF on labor market outcomes separately for migrants and non-migrants. Figure 6 displays the results of the cohort-specific regression (equation 3) for wages and labor force participation. I find that, among the cohorts exposed to the program, wages increased for migrants and decreased for non-migrants. The gap widens in younger generations, who were in principle more exposed to the program. I find very similar patterns using alternative measures of wages (Appendix Figure A.6.) In most cohorts, I also find a larger effect on the extensive margin of labor force participation among migrants than among non-migrants.

[17] Although Indonesia was not as close to achieving universal primary education in the 1970s as Brazil was in the 1990s, the INPRES school construction program allocation rule also prioritized regions that had the highest non-enrolled school-age population (Duflo 2001.)

[18] 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 (**?**.)

**Figure 6:** Effects of FUNDEF on wages and labor force participation by migrant status




**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

Across cohorts, I find that the effects of the program on labor market outcomes were systematically better for migrants than for non-migrants. Columns 5 and 6 in Table 4 report estimates of the average treatment effects on the exposed cohorts from equation 5, calculated separately for migrants and non-migrants. Column 7 in the same table reports tests of differences of the coefficients of the two groups. I find that a one percent increase in the education budget in the municipality of education led, on average, to a 0.8% increase in individual hourly wages for non-migrants, and to a 5.2% increase for migrants. The difference in the coefficients is highly statically significant. Migrants also had a positive effect on labor force participation (as opposed to a negative effect for non-migrants) and a negative effect on informality (which contrasts with a positive effect for non-migrants.)

Interestingly, the sharp gender differences in labor market outcomes' effects discussed in Section 4.3 appear to be largely orthogonal to the differences between migrants and non-migrants. For females and non-migrants FUNDEF implied worse labor market outcomes relative to males and migrants, respectively. But as discussed in Section 4.2, the effect of the program on the likelihood to migrate was not statistically different between men and women. In other words, it appears

that females obtained lower labor market effects from FUNDEF not *because* they migrated less, but *in spite* of migrating at similar rates. A possible explanation for this result is the presence of male-biased joint mobility decisions. Chauvin (2018) finds that married couples in Brazil during this period were more likely to migrate in response to better labor market prospects for men than for women. Tied-migrant women, consequently, were more likely than men to locate in regions with weak job prospects for their human capital levels. This may be an important hurdle for the ability of women to turn their increasing education levels into better job market outcomes, specially given that the majority of married women in Brazil have a partner that has a lower educational attainment (Ganguli et al. 2014.)

The fact that I find significantly better effects on labor market outcomes for migrants could, in turn, be explained by differences in individual characteristics or by differences in characteristics of the place of work of migrants and non-migrants.

A large literature has studied the connection between education and the geographic sorting of workers. Educated individuals are relatively more mobile (Notowidigdo 2013), 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.) In addition, return migrants (De la Roca 2017) and migrants to smaller cities (Combes et al. 2012) tend to be negatively selected.

I find that, during the period of interest, migrants in Brazil did have higher observable human capital characteristics. In 2010, the migrant population had, on average, higher educational attainment than the non-migrant population (Appendix Figure A.7.) While among the former 59% had middle school or higher education, this number was 56% among the latter. I also find a larger migrant-non migrant gap in the wage effect when I use wage measures that do not control for observable individual characteristics (Figure A.6). These patterns are consistent with previous literature documenting that internal migrants in Brazil, as in many other context, are positively selected (Dos Santos Júnior et al. 2005; Freguglia and Menezes-Filho 2012.)

In addition to this selection on observables, migrants could also be selected on characteristics that are hard to observe or unobservable.[19] It is possible that the observable and unobservable characteristics that drive sorting are strongly correlated and accounted for by the controls. Prior literature has shown that, while migrants to larger cities in the U.S. and Spain are positively selected on schooling and other observed characteristics, there is little evidence of sorting on unobserved

[19]For instance, looking at data from Project STAR, a well-studied experiment that randomly assigned kindergarten students in Tennessee to classrooms with different characteristics in the mid 1980s, Chetty et al. (2011) find that the likelihood of living out of state as adults was positively associated with kindergarten test scores.

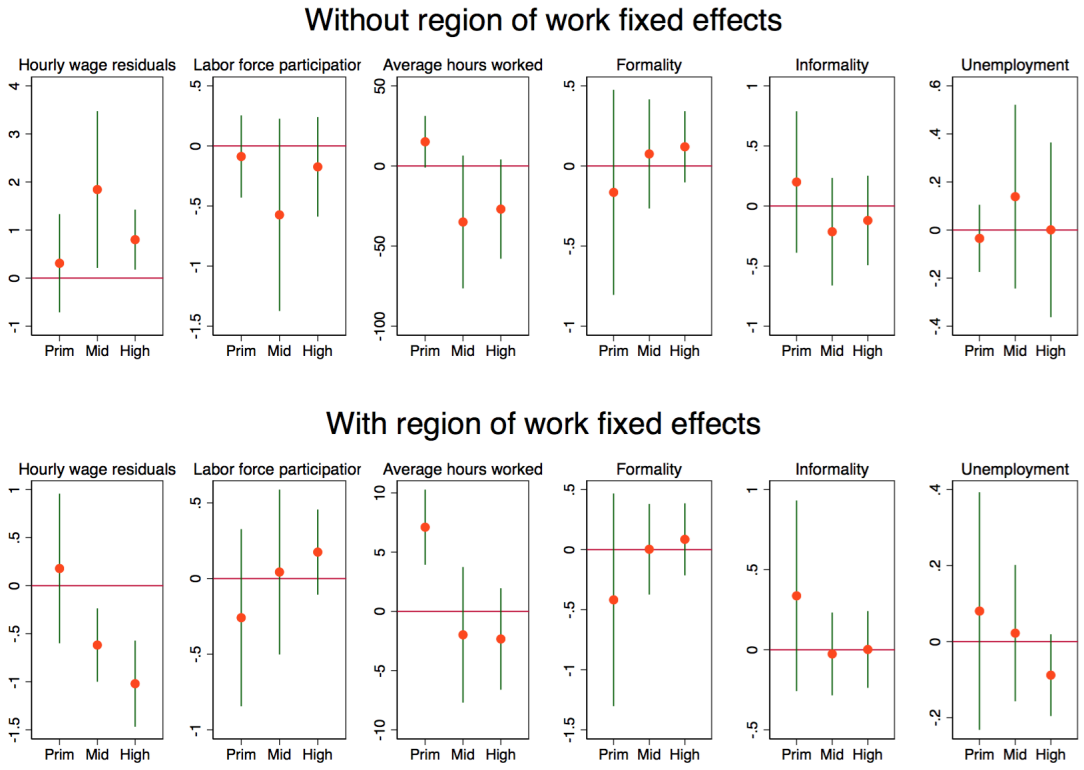
characteristics, as captured by individual fixed effects (Baum-Snow and Pavan 2012; De la Roca and Puga 2017). However, if these unobserved characteristics are not accounted for by my controls, my estimates of the effects of FUNDEF on the wage of migrants may have an upward bias.

The census data does not allow me to control for individual-level fixed effects. Instead, I look at what happens to the returns to education when I account for characteristics of the place of work of migrants. To this effect, I produce 2SLS estimates of the returns to educational attainment (coefficient $\hat{\beta}_1$ in equation 6) using two different specifications. In the first specification I do not control for place of work fixed effects. The returns captured by these estimates reflect both any increase in individual productivity and any gains from relocating to more productive places. In the second specification, I control for region of work fixed effects, shutting down the variation coming from potential productivity differences across localities. Figure 7 reports the results of these estimations for the three attainment levels affected by FUNDEF. In addition to wages, it includes 2SLS estimates to other labor market outcomes of interest.

The results are consistent with the interpretation that the positive connection between exposure to FUNDEF and individual wages is derived from the productivity of the places where the beneficiaries worked in 2010, rather than from increases in the productivity of the individual workers. In the specification without fixed effects I estimate positive and significant returns to middle school attainment and to high school attainment. However, when I control for time-invariant characteristics of the place of work, I obtain *negative* and statistically significant estimates. If unobserved individual characteristics were the key drivers of the positive wage effects of education attainment, it would be hard to explain why the estimates of these effects turn negative with the introduction of region of work fixed effects.

The only other outcome for which I observe a statistically significant effect of educational attainment is the average hours worked per week. Achieving primary school or higher attainment is associated with an weekly increase of 15 work hours worked in the specification that does not control for region of work characteristics, and of 7 hours in the specification that does. I do not find equivalent effects for middle school and high school achievement.

**Figure 7:** Effects of education attainment on labor market outcomes (2SLS estimates)




**Note:** The figure reports 2SLS coefficients on different levels of education attainment using the interaction of cohort fixed-effects and the intensity of FUNDEF transfers in the municipality of education as instruments. Markers denote coefficients and lines denote 95% confidence intervals. **Sources:** See data appendix.

Finding negative returns to education in a period where employment was increasing suggests that the growth in the supply of educated workers outpaced demand growth (Pritchett 2001). This is in line with Andrade and Menezes-Filho (2005) who find that, during the 1980s and 1990s, the increase in the relative supply of middle-education workers in Brazil outpaced growth in their relative demand, while demand for high-education workers remained stable, and the relative supply of the least educated workers decreased, driving the relative wage increase.[20]

My returns to education estimates could be biased if the program affected not only the quantity but also the quality of local education. A long-standing literature has documented that school quality can affect both returns to education and educational attainment levels (Card and Krueger 1992; Heckman et al. 1996; Deming et al. 2014.) Hanushek and Woessmann (2012) find that differences in quality of education explain why Latin America trailed other world regions in terms

[20]In this work low-education workers as defined as having less than primary school, the middle-education workers as having at least primary and up to high-school, and the high-education workers as having at least one year of college.

of economic development, in spite of having higher initial attainment levels.

Whether the effects of local public education investments on the outcomes of interest come from changes in quantity or quality of education is much harder to identify. The literature has failed to find a systematic relationship between additional resources and the quality of schooling (Hanushek 1997, 2003.) In theory, the program could have deteriorated educational quality, introducing an downward bias. In its initial years of implementation, FUNDEF was associated with both increases in total enrollment and decreases in the total number of schools -as state-run schools closed- leading do higher average class size. Moreover, municipalities had some discretion on the nature of their education investments, and whether these emphasized quantity or quality may be endogenous.[21]

However, existing evaluations of the effects of FUNDEF find that, on the net, the program had a *positive* effect on quality through increasing the total number of public teachers, their wages, the availability of funds for their training (Menezes-Filho and Pazello 2007).[22] These findings suggest that quality is unlikely to be behind the negative returns estimates.

## 5. Regional-level results

I turn now to the analysis of regional-level effects. I start by assessing the effect of FUNDEF on aggregate local educational attainment. Second, I discuss the effects of the program on migration, and how in turn they may shape the education composition of the local labor force. Finally, I explore the effects of the program on aggregate local labor market outcomes.

### 5.1. Effects on regional educational attainment levels

The fact that FUNDEF did increase educational attainment among individuals that were exposed to the policy does not necessarily imply that we will observe an increase in the local education levels of the places that benefited from the program. In a context where there is free internal mobility, as in Brazil, individual beneficiaries may choose to migrate to other locations in search of better economic opportunities (Andrews 2017; Abel and Deitz 2012). Moreover, if the program did increase local education levels, and that in turn increased local productivity and labor demand (as in Moretti 2004) it could have attracted workers from different regions. In that case the education

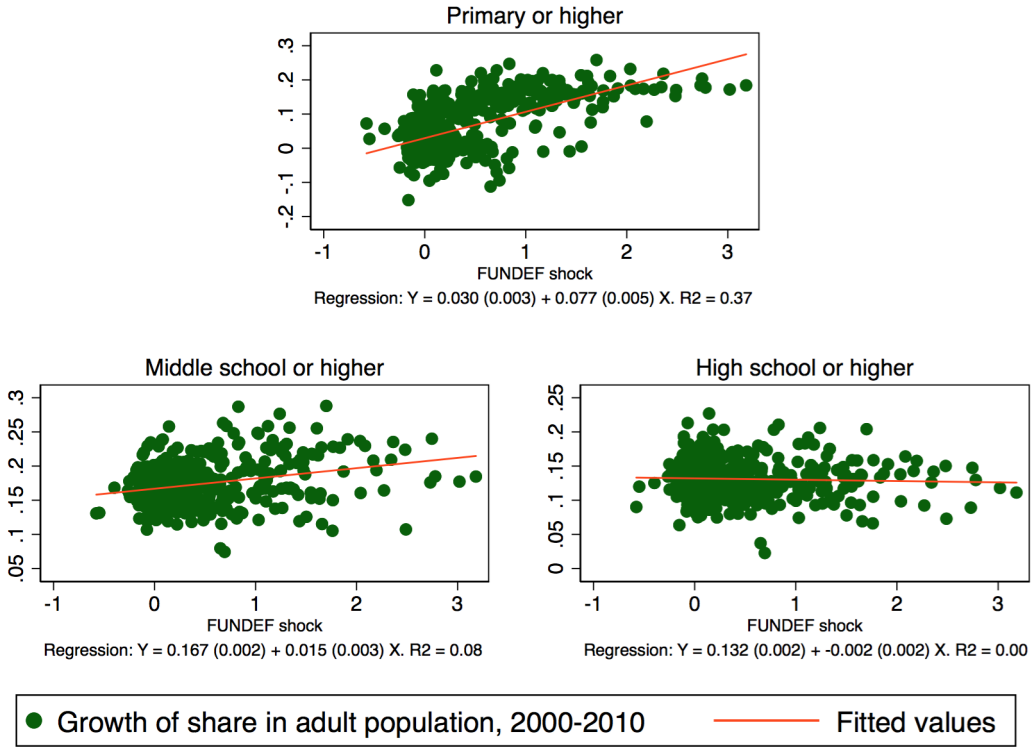
[21] Katrina Kosec (2014) finds that municipalities with higher median income and higher inequality spent less of the program's revenues in expanding public school enrollment. Rather, they were more likely to invest in public infrastructure with general-public use (e.g. roads and parks).

[22] This contrasts with recent experimental evidence from Indonesia, which finds that increases in teacher wages led to higher teacher satisfaction but had no impact on learning outcomes of students (de Ree et al. 2018.)

attainment of immigrants may have, in turn, contributed to shaping the aggregate education levels of the local economy.

Regions with higher incidence of the program did see rising regional education levels in the 2000s. Figure 8 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 or higher, middle-school or higher, and high-school or higher. The program appears to have been particularly effective at increasing the share of adults completing primary education. On average, a one percentage point 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 primary (or higher) education in the adult population -equivalent to a 0.44 standard deviations reduction-. The program had a weaker correlation with higher education attainment measures.

**Figure 8:** Effects of FUNDEF on growth of the share of educated people among adults




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

In order to explore to what extent this relationship can be interpreted as causal, I turn to a difference-in-differences regression set-up. Table 5 reports the coefficients on the interaction between

the FUNDEF shock and the "after" period (2010) dummy in equation 4. The difference in differences technique identifies the average treatment effect on the treated. In this context, assuming that the parallel trends assumptions holds conditional on controls, the coefficient $\alpha_3$ in equation 4 is an estimate of the average treatment effect of increased local public education investments on the beneficiary regions' outcomes of interest. Note that this estimate reflects both the direct effect of 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 effects -e.g. effects of local education levels on labor demand an subsequent migratory adjustments (Moretti, 2011)-.

The difference in differences estimation yields estimates that are very close to the coefficients of the simple OLS regression. A one percentage point larger increase in FUNDEF transfers was associated with a 7.5 percentage points increase in the share of individuals with primary education or higher. In this case, the point estimates are fairly similar for the sample restricted to males than for the sample restricted to females. The estimates for higher education levels are smaller, specifically of 1.3 percentage points for the share of middle school or higher, and of a non-significant negative 0.4 percentage points for the share of high school or higher. At the middle-school margin, the effect is driven by the female population, and at the high-school margin the negative effect is driven by the male population.

**Table 5:** Effects of FUNDEF on local education attainment

| | Change in share of educated in adult population | | | | | |
|---|---|---|---|---|---|---|
| | 2000-2010 | | | 1991-2000 (placebo test) | | |
| | Primary | Mid-school | High-school | Primary | Mid-school | High-school |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| All individuals | 0.075*** | 0.013** | -0.004 | 0.004 | -0.030*** | -0.023*** |
| | (0.008) | (0.006) | (0.004) | (0.005) | (0.004) | (0.003) |
| Males only | 0.081*** | 0.005 | -0.010** | 0.002 | -0.030*** | -0.022*** |
| | (0.009) | (0.006) | (0.004) | (0.005) | (0.004) | (0.003) |
| Females only | 0.070*** | 0.021*** | 0.003 | 0.006 | -0.030*** | -0.025*** |
| | (0.007) | (0.007) | (0.005) | (0.005) | (0.004) | (0.003) |
| Effective F statistic | 117.31 | 9.75 | 0.52 | | | |
| Weak instrument test critical value | 23.11 | 23.11 | 23.11 | | | |
| Changes in formality rates and wages in the 1980s | Yes | Yes | Yes | Yes | Yes | Yes |

**Note:** The table reports the coefficients on the *Post* × *Treatment* interaction in equation 4. Regressions are at the microregion level (N=456). Robust standard errors clustered at the mesoregion level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

To explore the validity of the parallel trends assumption, replicate the same analysis using 2000 as the "post" period and 1991 as the "pre-period". The results are reported in columns 4 through

6 of Table 5. After controlling for local labor market trends in the 1980s, the FUNDEF shock appears to be largely uncorrelated with the 1990s trends in the share of the primary educated in the population. However, the same is not true in the case of the 1990s tends in the shares of middle school and high school educated population. Regions with high FUNDEF transfers were also regions in where the shares of adults with higher education levels was declining during the 1990s.

The fact that the parallel trends assumption appears to hold - conditional on controls - for primary education but not for higher education levels is puzzling. As discussed in Section 3, FUNDEF targeted low-enrollment regions, and it is reasonable to expect that program intensity correlates with prior local trends on educational attainment. Many Brazilian regions that saw deteriorating labor market conditions during the 1980s and 1990s, experienced an economic recovery during the 2000s. Deteriorating conditions in the prior decades may have in turn led to lower enrollment rates in 1998. This motivates the use of 1980s trends controls in my preferred specification. But while conditioning on these variables accounts for the correlation of the program intensity with pre–trends in the share of primary education, it fails to do the same for the cases of middle school and high school education. A possible explanation for this difference relates to the effects of the program on the immigration of educated workers, which I explore next.

### 5.2. FUNDEF, migration, and the educational attainment levels of regions

A likely source of endogeneity of the shares of middle-school and high-school educated is the potential effect of the program on the *demand* for qualified workers. Andrabi et al. (2013) argue that regions with initially low education levels face subsequent low supply of local population qualified to teach. The authors document, in the context of Pakistan, that the construction of government girls' secondary schools was associated with a higher likelihood of private schools presence in the following years. The introduction of FUNDEF in 1998 increased the availability of funds specifically earmarked for teacher wages in beneficiary regions, and migrants may have filled at least part of the unmet demand. Controlling for the volume and education of composition of migrants during the 1990s, makes the FUNDEF shock uncorrelated with pre-trends in shares of middle school and high-school educated (Appendix Table A.6). Moreover, in Brazil - as in Pakistan - women play a prominent role as teachers, which may explain why the correlation of the program with the growth in the shares of middle- and high school educated in the 2000s is noticeably larger for females than for males.

To further explore the role of migration in the composition of local human capital following FUNDEF, Table 6 reports difference in differences estimates of the effect of the program on popula-

tion growth for different education attainment groups. A long-standing literature has documented a strong connection between initial education *levels* and subsequent population growth in U.S. cities (Glaeser et al. 1995; Glaeser and Shapiro 2003; Shapiro 2006). Chomitz et al. (2005) and Chauvin et al. (2017) find a similar correlation in Brazil. However, I find a small and statistically non-significant connection between FUNDEF-induced local public education investments and aggregate population growth in microregions during the 2000s.

**Table 6:** Effects of FUNDEF on regional population

| | Log of population | | | Shares of education group in population | | |
|---|---|---|---|---|---|---|
| | All | Males | Females | All | Males | Females |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Panel A: Effects on aggregate population | | | | | | |
| All adult population | 0.015 | 0.013 | 0.018 | | | |
| | (0.028) | (0.027) | (0.029) | | | |
| Panel B: Effects on population by education group | | | | | | |
| Less than primary | -0.085*** | -0.087*** | -0.085*** | -0.026*** | -0.025*** | -0.026*** |
| | -0.025 | -0.024 | -0.026 | -0.006 | -0.006 | -0.006 |
| Primary or higher | 0.191*** | 0.218*** | 0.168*** | 0.075*** | 0.081*** | 0.070*** |
| | (0.037) | (0.038) | (0.036) | (0.008) | (0.009) | (0.007) |
| Middle school or higher | 0.256*** | 0.266*** | 0.250*** | 0.013** | 0.005 | 0.021*** |
| | (0.046) | (0.048) | (0.045) | (0.006) | (0.006) | (0.007) |
| High school or higher | 0.247*** | 0.258*** | 0.242*** | -0.004 | -0.010** | 0.242*** |
| | (0.050) | (0.052) | (0.049) | (0.004) | (0.004) | (0.049) |
| College or higher | 0.354*** | 0.230*** | 0.419*** | -0.011*** | -0.009*** | -0.013*** |
| | (0.066) | (0.068) | (0.073) | (0.001) | (0.001) | (0.001) |

**Note:** The table reports the coefficients on the $Post \times Treatment$ interaction in equation 4. Regressions are at the microregion level (N=456). Robust standard errors clustered at the mesoregion level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

But while overall mobility towards FUNDEF-intensive regions was not systematically different than regions with low program intensity, the changes in the education composition were. The overall population with less than primary education shrank in beneficiary regions, while the population in the other educational categories increased. The increases in primary and middle-school educated population - the schooling levels targeted by the program - outpaced growth in other education categories, and the share of these education groups grew following the program, while the shares of the other groups shrank. The fact that the share of primary-educated grew the most in spite of the fact that FUNDEF was associated with a *negative* net migration in this education category (Appendix Table A.7), is consistent with the individual-level findings showing that the program's largest impact on educational attainment was at this level (Section 4).

Gender differences by education group are consistent with the interpretation that FUNDEF had

a direct impact on the *demand* of workers with intermediate education. While program intensity in the region was associated with a significant increase in the share of middle school and high school educated among women, the effect was small and non-significant for middle school, and negative for high-school in the case of men. Furthermore, the effects of the program on migration is positive and significant for females, and close to zero and non-significant for males in these two education categories (Appendix Table A.7.)

### 5.3. FUNDEF and regional labor market outcomes

I turn now to the effects of FUNDEF on labor market outcomes at the regional level. Table 7 summarizes the results of regional-level regressions exploring six aggregate labor market outcomes. Panel A reports difference in differences estimates of the reduced-form effects of the program on the outcomes (equation 4.) Panels B and C explore the effects of changes in local education attainment levels on local labor market outcomes, estimating regressions of the form:

$$\Delta_{2000s} Y_r = \gamma_0 + \gamma_1 \Delta_{2000s} Prim_r + \gamma_2 \left(\Delta_{1980s} C_r\right) + \epsilon_r \tag{7}$$

where $\Delta$ denote decade-long changes, $Y_r$ is the regional-level outcome of interest, $Prim_r$ is the share of primary-educated in region $r$, and $\Delta_{1980s} C_r$ are the same lagged trends controls used in my estimates of equation 4. Panel B reports OLS estimates of $\hat{\gamma}_1$, and Panel C reports 2SLS estimates of the same coefficient using the regional-level FUNDEF shock (equation 2) as instrument.

My choice of explanatory variable y informed by the findings in prior sections. Changes in local education levels at the primary education margin capture the level at which the program had the strongest impact, and are uncorrelated with observable pre-trends conditional on controls. In Table 5, where I report the first-stage results discussed in Section 5.1, I also include the results of the test for weak instruments of Montiel Olea and Pflueger (2013). The regional FUNDEF shock is a strong instruments for this explanatory variable, but not for measures of changes in local education at higher levels.

The results show that, on average, labor market outcomes worsened in regions that benefited to FUNDEF. A program-induced one percentage point larger public education budget was associated with a 3.1% reduction of the average hourly wage, after controlling for individual characteristics. This is in spite of the fact that the program had a direct positive effect through its mandated increases in teachers' wages.[23] The program was also associated with lower participation (in the

[23] Following the introduction of the FUNDEF, teacher's salaries rose by an average of 13%, and in the poor north east increases were as high as 60% (OECD 2011.)

extensive and the intensive margins), higher informality rates, and higher unemployment. The decline in wages was stronger among males, and the increase in informal employment among females.

**Table 7:** Effects of FUNDEF on local labor market outcomes

| | Employed population (1) | Hourly wage res. (2) | Particip. (3) | Hours worked (4) | Formal (5) | Informal (6) | Unemp. (7) |
|---|---|---|---|---|---|---|---|
| Panel A: Reduced-form relationship | | | | | | | |
| All individuals | -0.029 | -0.031* | -0.018*** | -0.486*** | -0.024*** | 0.005 | 0.019*** |
| | (0.034) | (0.018) | (0.005) | (0.176) | (0.005) | (0.006) | (0.003) |
| Males only | -0.123*** | -0.048** | -0.012** | -0.439*** | -0.019*** | 0.002 | 0.017*** |
| | (0.040) | (0.020) | (0.005) | (0.161) | (0.006) | (0.007) | (0.003) |
| Females only | -0.009 | -0.007 | -0.023*** | -0.487** | -0.038*** | 0.021*** | 0.016*** |
| | (0.042) | (0.016) | (0.006) | (0.229) | (0.005) | (0.007) | (0.005) |
| Panel B: Effects of changes in the share of primary educated , OLS | | | | | | | |
| All individuals | -0.005 | -0.490*** | -0.211*** | -0.106*** | -0.235*** | 0.098** | 0.138*** |
| | (0.104) | (0.097) | (0.030) | (0.032) | (0.037) | (0.046) | (0.027) |
| Males only | 0.115 | -0.573*** | -0.073** | -0.070*** | -0.153*** | 0.044 | 0.109*** |
| | (0.104) | (0.097) | (0.033) | (0.026) | (0.033) | (0.043) | (0.025) |
| Females only | 0.093 | -0.290*** | -0.336*** | -0.119** | -0.396*** | 0.290*** | 0.106*** |
| | (0.151) | (0.102) | (0.047) | (0.055) | (0.057) | (0.069) | (0.037) |
| Panel C: Effects of changes in the share of primary educated , 2SLS | | | | | | | |
| All individuals | 0.079 | -0.380* | -0.226*** | -0.225*** | -0.293*** | 0.049 | 0.244*** |
| | (0.195) | (0.219) | (0.052) | (0.060) | (0.054) | (0.072) | (0.051) |
| Males only | 0.090 | -0.522** | -0.146** | -0.195*** | -0.217*** | 0.006 | 0.211*** |
| | (0.186) | (0.215) | (0.065) | (0.049) | (0.056) | (0.073) | (0.046) |
| Females only | 0.400 | -0.031 | -0.294*** | -0.226** | -0.502*** | 0.276*** | 0.226*** |
| | (0.270) | (0.226) | (0.057) | (0.094) | (0.072) | (0.093) | (0.068) |

**Note:** Panel A reports the coefficients on the $Post \times Treatment$ interaction in equation 4. Panel B reports OLS estimates of the coefficient on the change in the share of individuals with at least primary school in the adult population in equation 7. Panel C reports 2SLS estimates of the same coefficient using the regional-level FUNDEF shock (equation 2) as instrument. Regressions are at the microregion level (N=456). Robust standard errors clustered at the mesoregion level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

The evidence is consistent with the interpretation that local supply of educated labor outpaced local demand. Worsened labor market outcomes could be partially explained by negative selection among the non-migrants. However, while the IV estimates show a negative 0.3 percentage points effect on the hourly wage, the point estimate on employment is positive (although not statistically significant), suggesting a downward-sloping relatively inelastic demand. Local employment grew primarily in the informal sector and among women.

# 6. Conclusion

This paper explores the effects of public education investments on individual and regional labor market outcomes. Using Brazil's FUNDEF as a source of exogenous variation in local public education budgets I find generally positive effects at the individual level and negative effects at the regional level.

FUNDEF had a positive effect on individual educational attainment. Cohorts that were in principle exposed to the program had, on average a 2.4 percentage points higher likelihood of attaining at least primary education relative to cohorts that were not exposed. The reform was less effective in increasing education attainment at other margins, with a 1 percentage point effect on middle school attainment, and a 0.4 percentage point effect on high school attainment.

The program also had a positive effect on individual wages, which was concentrated among individuals that migrated outside their region of education. One percent increase in the education budget in the individual's municipality of education led, on average, to a 1.7% increase in hourly wages and a 1.2 percentage points increase in the likelihood of migrating of individuals who were (in theory) exposed to the program. The wage effect was 5.2% for migrants, and only 0.8% for non-migrants. I estimate positive average returns to educational attainment for middle school and high school in the order of 1.9% and 0.8%, respectively, but these estimates become *negative* when I control for region of work fixed effects, suggesting that the bulk of the wage effect comes from characteristics of migrants' destination regions.

The results unveil large gender differences in the individual effects of local education spending. While the average wage effect for males was of 2.8%, the equivalent for women was close to zero. This gap is not explained by gender differences in migration elasticities. Joint location decisions that favor male over female labor market prospects may account for these patterns, but further research is required to better understand the mechanisms at play.

FUNDEF also led to higher educational attainment at the regional level, specially at the primary education education margin. But the increase in the share of educated workers was associated with worsening local labor market outcomes. The results on wages and employment suggest that growth in demand for educated labor was not large enough to absorb the program-related supply shifts.

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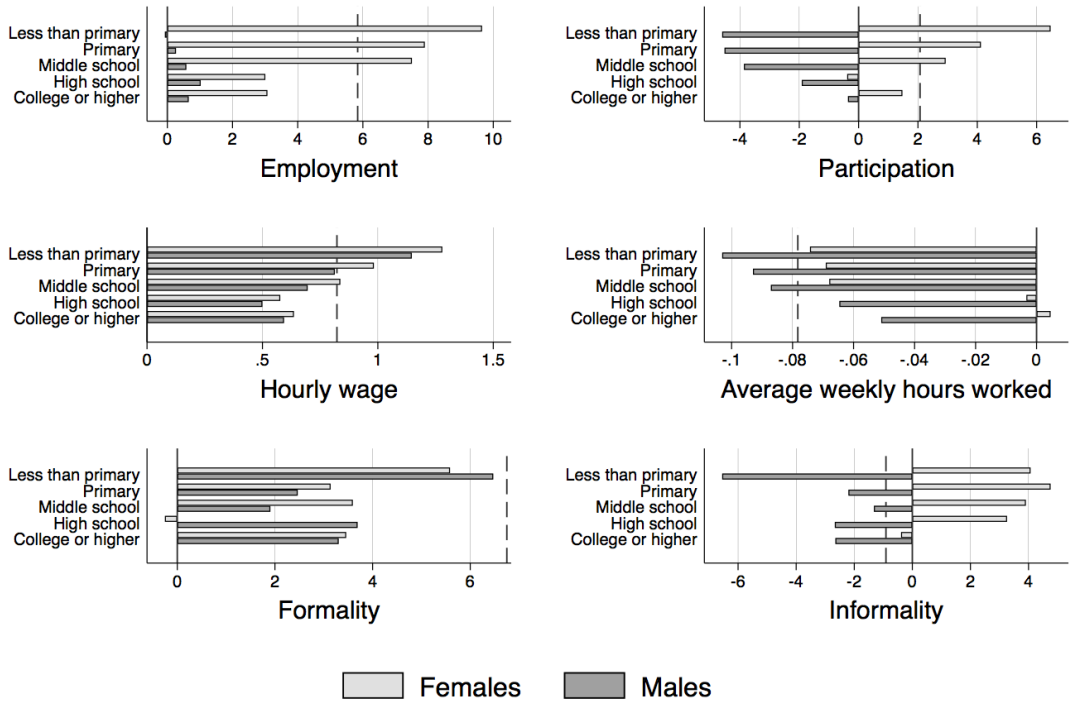
# Appendix

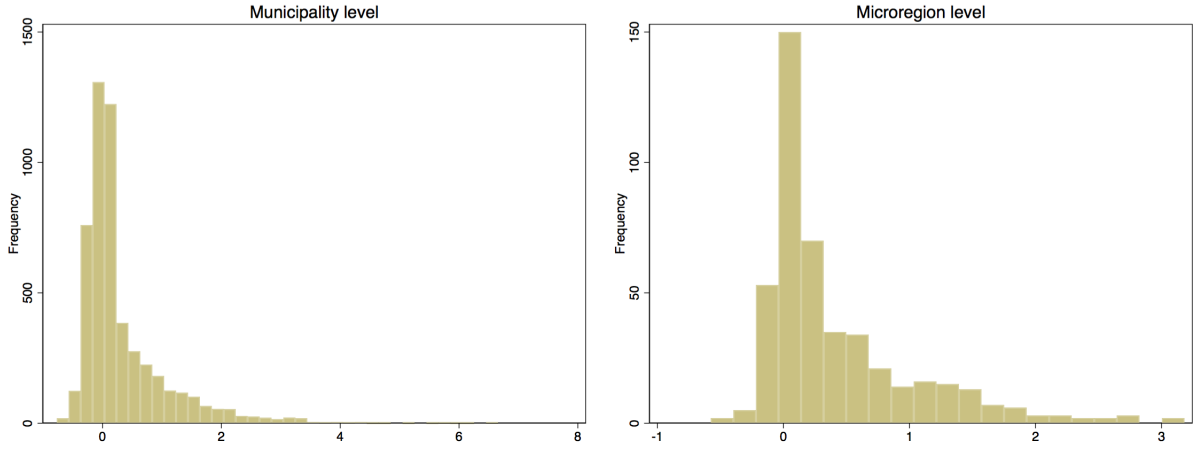
## A. Figures

**Figure A.1:** Changes in labor market outcomes 2000-2010 by educational attainment category and gender



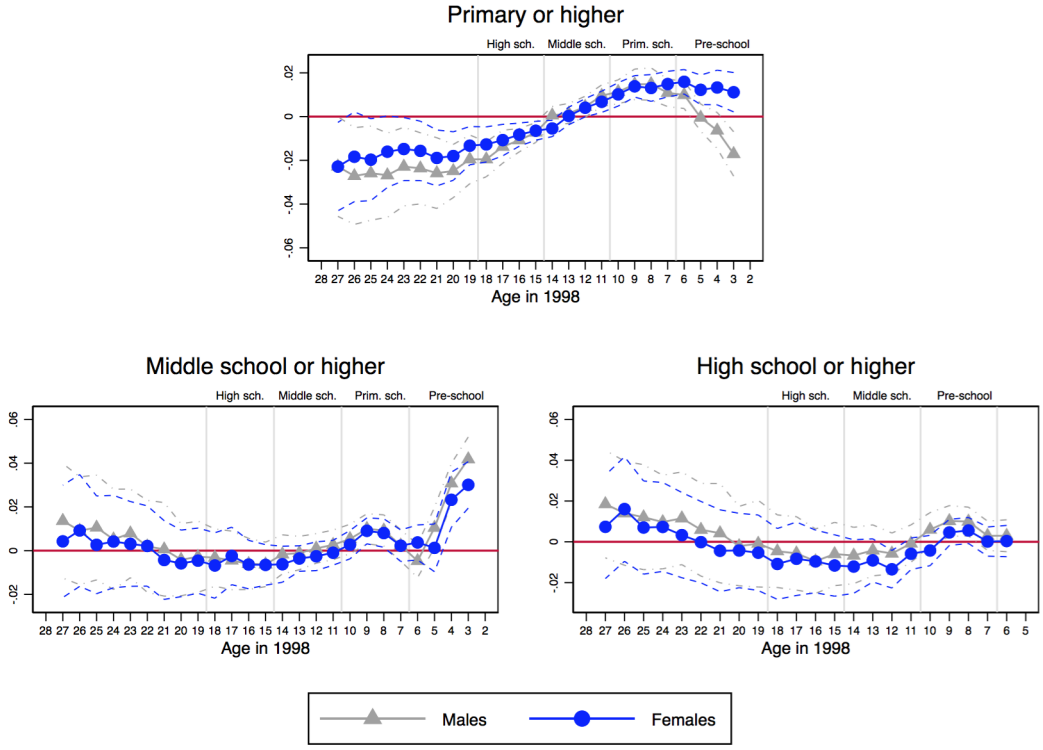

**Note:** Restricted to wage-earning population aged 23 through 64. Dashed lines denote population averages. All estimates are own calculations from microdata using sample weights. See the data appendix C for details on the measurement of each variable**. Source:** Population censuses of 2000 and 2010.

**Figure A.2:** Distribution of FUNDEF shock across localities




**Sources:** See data appendix.

**Figure A.3:** Effects of FUNDEF on individual's probability of reaching a specific educational attainment in 2010 by cohort and by gender

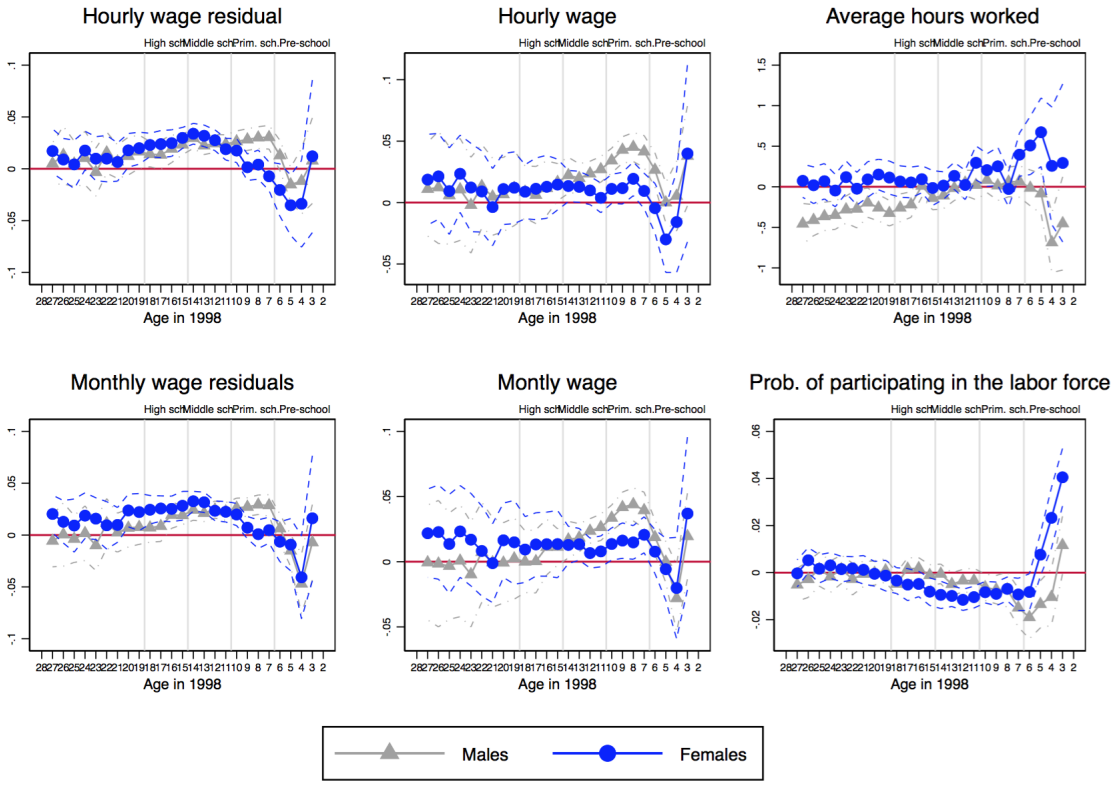



**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

**Figure A.4:** Effects of FUNDEF on wages using alternative measures

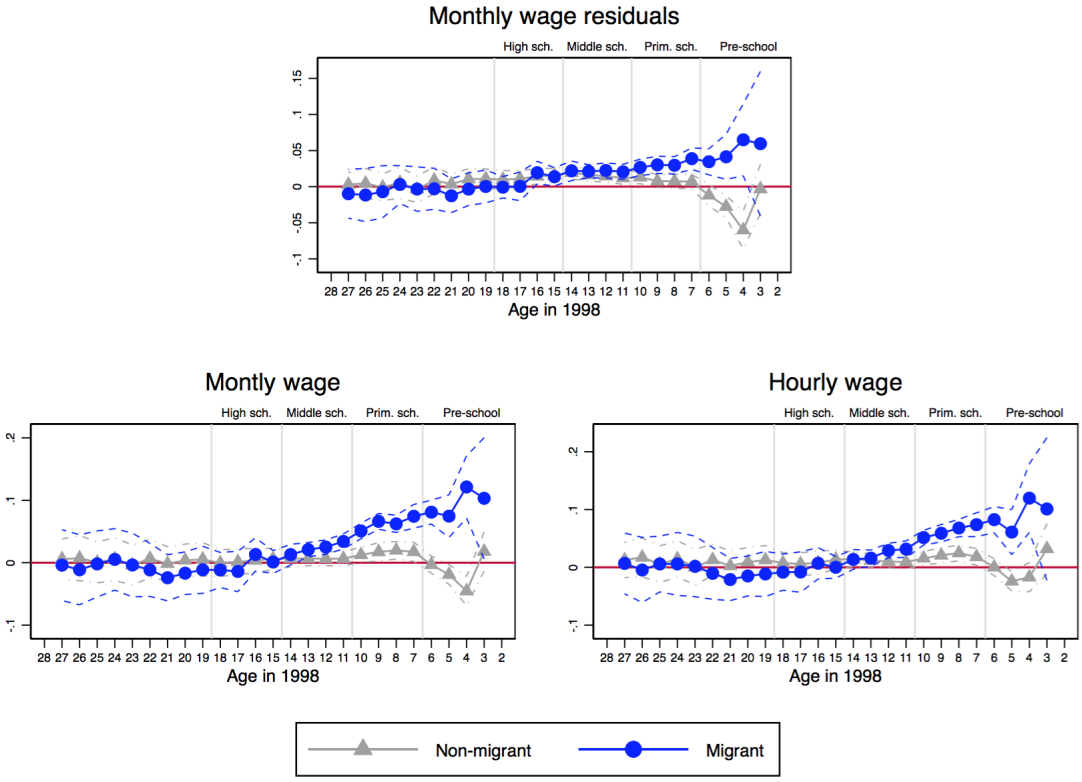

**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

**Figure A.5:** Effects of FUNDEF on wages and labor force participation by gender



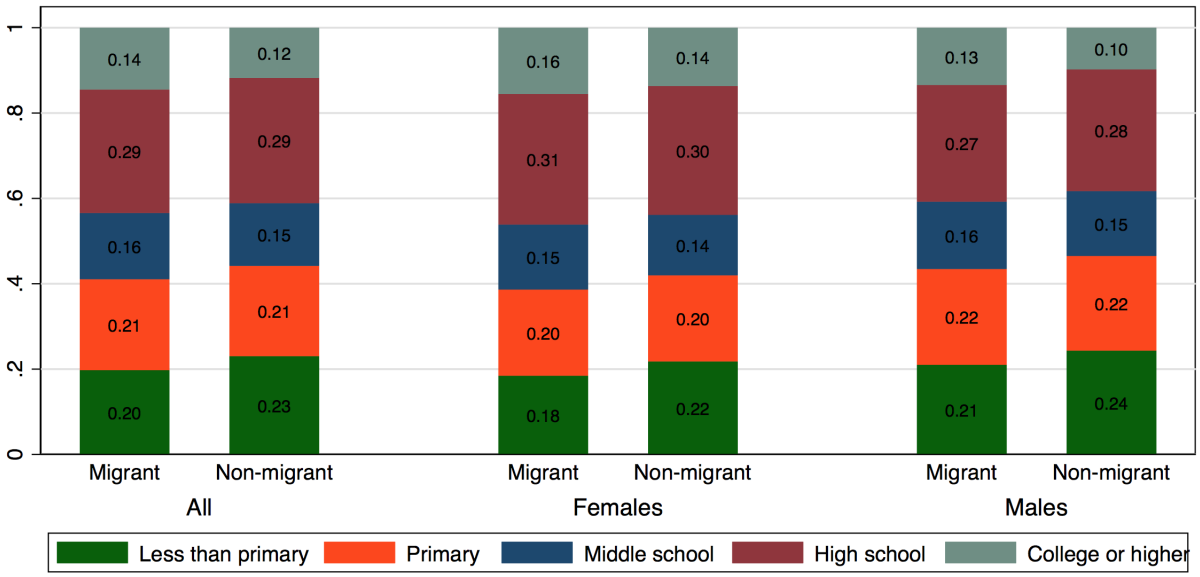

**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

**Figure A.6:** Effects of FUNDEF on wages by migrant status using alternative measures




**Note:** The markers represent the coefficient on the interaction of the FUNDEF treatment variable and each cohort dummy in equation 3. Dashed lines are 95% confidence intervals, with standard errors clustered at the municipality of education level.
**Sources:** See data appendix.

**Figure A.7:** Percentage of Brazilian population age 23 or older in each educational attainment category by migrant status, 2010




**Source:** Population censuses of 2000 and 2010.

# B. Tables

**Table A.1:** Individual summary statistics, 2010

| | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| Less than primary | 0.15 | 0.36 | 0 | 1 |
| Primary | 0.20 | 0.40 | 0 | 1 |
| Middle school | 0.18 | 0.38 | 0 | 1 |
| High school | 0.37 | 0.48 | 0 | 1 |
| College or higher | 0.10 | 0.30 | 0 | 1 |
| Hourly wage | 7.389 | 21.924 | 0.002 | 8,660.505 |
| Less than primary | 4.966 | 17.704 | 0.003 | 3,464.204 |
| Primary | 4.570 | 11.347 | 0.002 | 3,464.204 |
| Middle school | 5.472 | 13.830 | 0.002 | 2,771.363 |
| High school | 6.675 | 16.389 | 0.002 | 4,618.936 |
| College or higher | 18.146 | 44.516 | 0.003 | 8,660.505 |
| Monthly wage | 1,082 | 2,208 | 1 | 800,000 |
| Less than primary | 662 | 920 | 1 | 102,010 |
| Primary | 674 | 1,267 | 1 | 600,000 |
| Middle school | 808 | 1,853 | 1 | 800,000 |
| High school | 996 | 1,570 | 1 | 400,000 |
| College or higher | 2,673 | 4,346 | 1 | 750,000 |
| Probability of being a migrant | 0.12 | 0.32 | 0 | 1 |
| Less than primary | 0.13 | 0.33 | 0 | 1 |
| Primary | 0.14 | 0.35 | 0 | 1 |
| Middle school | 0.14 | 0.35 | 0 | 1 |
| High school | 0.12 | 0.33 | 0 | 1 |
| College or higher | 0.17 | 0.38 | 0 | 1 |
| Employment rate | 0.66 | 0.47 | 0 | 1 |
| Less than primary | 0.56 | 0.50 | 0 | 1 |
| Primary | 0.59 | 0.49 | 0 | 1 |
| Middle school | 0.65 | 0.48 | 0 | 1 |
| High school | 0.72 | 0.45 | 0 | 1 |
| College or higher | 0.87 | 0.33 | 0 | 1 |
| Number of weekly hours worked | 41.3 | 14.0 | 1 | 140 |
| Less than primary | 40.8 | 15.2 | 1 | 140 |
| Primary | 42.0 | 15.0 | 1 | 140 |
| Middle school | 42.2 | 14.5 | 1 | 140 |
| High school | 41.7 | 13.2 | 1 | 140 |
| College or higher | 39.1 | 12.3 | 1 | 140 |

(Continues...)

... (Table A.1 continued)

| | | | | |
|---|---|---|---|---|
| Probability of participating in the labor force | 0.73 | 0.44 | 0 | 1 |
| Less than primary | 0.62 | 0.48 | 0 | 1 |
| Primary | 0.66 | 0.47 | 0 | 1 |
| Middle school | 0.72 | 0.45 | 0 | 1 |
| High school | 0.80 | 0.40 | 0 | 1 |
| College or higher | 0.92 | 0.28 | 0 | 1 |
| Probability of being formally employed * | 0.54 | 0.50 | 0 | 1 |
| Less than primary | 0.39 | 0.49 | 0 | 1 |
| Primary | 0.40 | 0.49 | 0 | 1 |
| Middle school | 0.51 | 0.50 | 0 | 1 |
| High school | 0.62 | 0.49 | 0 | 1 |
| College or higher | 0.73 | 0.44 | 0 | 1 |
| Probability of being informally employed* | 0.36 | 0.48 | 0 | 1 |
| Less than primary | 0.51 | 0.50 | 0 | 1 |
| Primary | 0.50 | 0.50 | 0 | 1 |
| Middle school | 0.39 | 0.49 | 0 | 1 |
| High school | 0.27 | 0.45 | 0 | 1 |
| College or higher | 0.23 | 0.42 | 0 | 1 |
| Probability of being unemployed* | 0.10 | 0.29 | 0 | 1 |
| Less than primary | 0.10 | 0.29 | 0 | 1 |
| Primary | 0.11 | 0.31 | 0 | 1 |
| Middle school | 0.10 | 0.31 | 0 | 1 |
| High school | 0.10 | 0.31 | 0 | 1 |
| College or higher | 0.04 | 0.21 | 0 | 1 |

**Source:** Own calculations from 2010 population census using sampling weights.

* Probability conditional on participating in the labor force.

**Table A.2:** Regional summary statistics, 2010

| | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| Shocks (1997) | | | | |
| *Municipality level* | | | | |
| Fundef shock | 0.33 | 0.75 | -0.77 | 6.66 |
| Predicted FUNDEF shock | 0.43 | 0.99 | -0.73 | 10.61 |
| *Regional level* | | | | |
| Fundef shock | 0.43 | 0.61 | -0.58 | 3.18 |
| Predicted FUNDEF shock | 0.56 | 0.82 | -0.23 | 4.61 |
| Main variables | | | | |
| Total population (1,000s) | 291.76 | 769.38 | 4.95 | 13757.32 |
| Working-age population (1,000s) | 147.34 | 409.72 | 2.26 | 7443.07 |
| Males | 71.73 | 191.99 | 1.27 | 3528.61 |
| Females | 75.62 | 217.84 | 0.99 | 3914.46 |
| Migrant share | 0.25 | 0.12 | 0.03 | 0.71 |
| Males | 0.24 | 0.12 | 0.03 | 0.69 |
| Females | 0.25 | 0.12 | 0.03 | 0.72 |
| Average hourly wage | 2.70 | 1.19 | 0.70 | 7.40 |
| Males | 2.87 | 1.33 | 0.72 | 8.04 |
| Females | 2.40 | 1.04 | 0.36 | 6.69 |
| Average montly wage | 437.77 | 200.43 | 66.67 | 1217.12 |
| Males | 495.33 | 238.87 | 127.71 | 1417.59 |
| Females | 327.77 | 165.27 | 23.98 | 1002.02 |
| Employment rate | 0.54 | 0.08 | 0.22 | 0.81 |
| Males | 0.77 | 0.12 | 0.26 | 2.04 |
| Females | 0.30 | 0.13 | 0.04 | 0.72 |
| Average weekly hours worked | 42.73 | 3.26 | 32.36 | 53.96 |
| Males | 44.76 | 3.30 | 34.10 | 55.88 |
| Females | 38.33 | 3.35 | 25.47 | 50.79 |
| Participation rate | 0.58 | 0.09 | 0.27 | 0.82 |
| Males | 0.81 | 0.09 | 0.33 | 0.96 |
| Females | 0.34 | 0.14 | 0.04 | 0.74 |
| Formality rate | 0.31 | 0.17 | 0.01 | 0.82 |
| Males | 0.31 | 0.18 | 0.01 | 0.80 |
| Females | 0.35 | 0.15 | 0.02 | 0.87 |
| Informality rate | 0.62 | 0.17 | 0.16 | 0.97 |
| Males | 0.64 | 0.19 | 0.19 | 0.98 |
| Females | 0.55 | 0.14 | 0.11 | 0.95 |
| Unemployment rate | 0.07 | 0.05 | 0.00 | 0.26 |
| Males | 0.05 | 0.04 | 0.00 | 0.22 |
| Females | 0.10 | 0.08 | 0.00 | 0.77 |

**Source:** Own calculations with population censuses. Outcomes calculated for individuals aged 15-64. N=456.

**Table A.3:** Regional summary statistics, 2000

| | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| Main variables | | | | |
| Total population (1,000s) | 317.72 | 815.62 | 12.95 | 12790.27 |
| Working-age population (1,000s) | 157.65 | 429.59 | 5.77 | 6859.49 |
| Males | 76.85 | 200.68 | 3.16 | 3216.30 |
| Females | 80.80 | 229.00 | 2.61 | 3643.20 |
| Migrant share | 0.19 | 0.08 | 0.03 | 0.62 |
| Males | 0.19 | 0.08 | 0.03 | 0.62 |
| Females | 0.19 | 0.08 | 0.03 | 0.62 |
| Average hourly wage | 2.56 | 0.96 | 0.92 | 6.57 |
| Males | 2.71 | 1.09 | 0.94 | 7.21 |
| Females | 2.29 | 0.78 | 0.78 | 5.77 |
| Average montly wage | 911.35 | 363.05 | 258.32 | 2375.99 |
| Males | 529.16 | 227.27 | 159.84 | 1417.59 |
| Females | 359.74 | 137.20 | 92.74 | 980.08 |
| Employment rate | 0.51 | 0.09 | 0.22 | 0.74 |
| Males | 0.71 | 0.11 | 0.26 | 0.99 |
| Females | 0.32 | 0.09 | 0.09 | 0.61 |
| Average weekly hours worked | 44.09 | 2.95 | 34.76 | 53.96 |
| Males | 46.48 | 3.04 | 38.84 | 55.88 |
| Females | 39.10 | 3.20 | 25.47 | 50.79 |
| Participation rate | 0.59 | 0.10 | 0.27 | 0.78 |
| Males | 0.78 | 0.10 | 0.33 | 0.91 |
| Females | 0.40 | 0.10 | 0.16 | 0.66 |
| Formality rate | 0.28 | 0.13 | 0.04 | 0.62 |
| Males | 0.28 | 0.14 | 0.03 | 0.64 |
| Females | 0.28 | 0.12 | 0.02 | 0.65 |
| Informality rate | 0.59 | 0.14 | 0.30 | 0.88 |
| Males | 0.63 | 0.15 | 0.31 | 0.93 |
| Females | 0.52 | 0.11 | 0.28 | 0.85 |
| Unemployment rate | 0.13 | 0.04 | 0.03 | 0.26 |
| Males | 0.09 | 0.04 | 0.01 | 0.22 |
| Females | 0.20 | 0.05 | 0.06 | 0.51 |
| 1980s trends controls | | | | |
| $\Delta_{80-91}$ Formality rate | 0.03 | 0.07 | -0.18 | 0.29 |
| $\Delta_{80-91}$ Average montly wage | 0.05 | 0.16 | -0.50 | 0.70 |

**Source:** Own calculations with population censuses. Outcomes calculated for individuals aged 15-64. N=456.

**Table A.4:** Correlations among regional-level variables, 2010

| **Variables** | | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** | **10** | **11** | **12** | **13** | **14** | **15** |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **1** | Regional FUNDEF shock | 1 | | | | | | | | | | | | | | |
| **2** | Regional predicted FUNDEF shock | 0.99 | 1 | | | | | | | | | | | | | |
| **3** | Total population | 0.01 | -0.04 | 1 | | | | | | | | | | | | |
| **4** | Working-age population | 0.00 | -0.05 | 1.00 | 1 | | | | | | | | | | | |
| **5** | Share of migrants in working-age pop. | -0.19 | -0.19 | 0.10 | 0.11 | 1 | | | | | | | | | | |
| **6** | Average log hourly wage | -0.38 | -0.41 | 0.33 | 0.34 | 0.66 | 1 | | | | | | | | | |
| **7** | Average log wage | -0.42 | -0.45 | 0.31 | 0.32 | 0.64 | 0.98 | 1 | | | | | | | | |
| **8** | Employment rate | -0.40 | -0.41 | 0.17 | 0.18 | 0.55 | 0.65 | 0.68 | 1 | | | | | | | |
| **9** | Participation rate | -0.40 | -0.41 | 0.22 | 0.23 | 0.53 | 0.73 | 0.77 | 0.94 | 1 | | | | | | |
| **10** | Average montly hours worked | -0.43 | -0.43 | 0.01 | 0.01 | 0.13 | 0.08 | 0.25 | 0.40 | 0.42 | 1 | | | | | |
| **11** | Formality rate | -0.47 | -0.50 | 0.25 | 0.26 | 0.51 | 0.74 | 0.73 | 0.67 | 0.68 | 0.31 | 1 | | | | |
| **12** | Informality rate | 0.44 | 0.48 | -0.27 | -0.28 | -0.44 | -0.76 | -0.76 | -0.57 | -0.67 | -0.29 | -0.96 | 1 | | | |
| **13** | Unemployment rate | 0.05 | 0.05 | 0.11 | 0.10 | -0.33 | 0.06 | 0.19 | -0.28 | 0.07 | -0.09 | -0.03 | -0.26 | 1 | | |
| **14** | Change 1980-1991 in formality rate | -0.08 | -0.10 | -0.25 | -0.25 | 0.02 | -0.15 | -0.13 | -0.01 | -0.04 | 0.07 | -0.12 | 0.13 | -0.05 | 1 | |
| **15** | Change 1980-1991 in average log wage | 0.00 | -0.02 | 0.12 | 0.12 | 0.16 | 0.26 | 0.21 | 0.16 | 0.17 | 0.01 | 0.19 | -0.19 | 0.02 | 0.03 | 1 |

Note: Own-calculations with population census data.

**Table A.5:** Correlations among regional-level variables, 2000

| **Variables** | | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** | **10** | **11** | **12** | **13** | **14** | **15** |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **1** | Regional FUNDEF shock | 1 | | | | | | | | | | | | | | |
| **2** | Regional predicted FUNDEF shock | 0.99 | 1 | | | | | | | | | | | | | |
| **3** | Total population | 0.01 | -0.04 | 1 | | | | | | | | | | | | |
| **4** | Working-age population | 0.00 | -0.05 | 1.00 | 1 | | | | | | | | | | | |
| **5** | Share of migrants in working-age pop. | -0.13 | -0.13 | 0.05 | 0.05 | 1 | | | | | | | | | | |
| **6** | Average log hourly wage | -0.48 | -0.53 | 0.39 | 0.39 | 0.42 | 1 | | | | | | | | | |
| **7** | Average log wage | -0.51 | -0.55 | 0.36 | 0.37 | 0.46 | 0.99 | 1 | | | | | | | | |
| **8** | Employment rate | -0.51 | -0.53 | 0.15 | 0.16 | 0.36 | 0.69 | 0.73 | 1 | | | | | | | |
| **9** | Participation rate | -0.55 | -0.57 | 0.23 | 0.23 | 0.40 | 0.76 | 0.78 | 0.96 | 1 | | | | | | |
| **10** | Average montly hours worked | -0.46 | -0.45 | 0.05 | 0.05 | 0.45 | 0.47 | 0.56 | 0.61 | 0.63 | 1 | | | | | |
| **11** | Formality rate | -0.53 | -0.57 | 0.23 | 0.24 | 0.25 | 0.82 | 0.82 | 0.74 | 0.80 | 0.50 | 1 | | | | |
| **12** | Informality rate | 0.52 | 0.56 | -0.27 | -0.27 | -0.25 | -0.78 | -0.78 | -0.59 | -0.72 | -0.45 | -0.96 | 1 | | | |
| **13** | Unemployment rate | -0.04 | -0.06 | 0.19 | 0.18 | 0.04 | 0.06 | 0.03 | -0.36 | -0.10 | -0.08 | 0.07 | -0.36 | 1 | | |
| **14** | Change 1980-1991 in formality rate | -0.08 | -0.10 | -0.26 | -0.26 | 0.03 | -0.21 | -0.17 | -0.02 | -0.05 | 0.08 | 0.00 | 0.02 | -0.07 | 1 | |
| **15** | Change 1980-1991 in average log wage | 0.00 | -0.02 | 0.13 | 0.12 | 0.19 | 0.30 | 0.29 | 0.12 | 0.16 | 0.03 | 0.19 | -0.20 | 0.09 | 0.03 | 1 |

Note: Own-calculations with population census data.

**Table A.6:** Effects of FUNDEF on local education attainment controlling for 1990s' migration composition

| | Change in share of educated in adult population | | | |
|---|---|---|---|---|
| | 2000-2010 | | 1991-2000 (placebo test) | |
| | Mid-school | High-school | Mid-school | High-school |
| | (1) | (2) | (3) | (4) |
| All individuals | 0.041*** | 0.020*** | -0.001 | -0.003 |
| | (0.007) | (0.005) | (0.005) | (0.003) |
| Males only | 0.034*** | 0.013*** | -0.002 | -0.003 |
| | (0.007) | (0.005) | (0.005) | (0.003) |
| Females only | 0.049*** | 0.027*** | -0.001 | -0.004 |
| | (0.008) | (0.006) | (0.005) | (0.003) |
| Changes in formality rates and wages in the 1980s | Yes | Yes | Yes | Yes |
| 1990s migration as a share of local population | Yes | Yes | Yes | Yes |
| Share of high-school educated in 1990s migrants | Yes | Yes | Yes | Yes |

**Note:** The table reports the coefficients on the treatment variable in equation 4. Regressions are at the microregion level (N=456). Robust standard errors clustered at the mesoregion level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

**Table A.7:** Effects of FUNDEF on migrant population by educational attainment group

| | All | Males | Females |
|---|---|---|---|
| | (1) | (2) | (3) |
| Less than primary | -0.206*** | -0.214*** | -0.196*** |
| | (0.042) | (0.044) | (0.041) |
| Primary or higher | -0.045* | -0.025 | -0.064*** |
| | (0.026) | (0.030) | (0.024) |
| Middle school or higher | 0.044** | 0.027 | 0.062*** |
| | (0.022) | (0.027) | (0.022) |
| High school or higher | 0.047* | 0.014 | 0.076*** |
| | (0.026) | (0.030) | (0.025) |
| College or higher | 0.201*** | 0.096* | 0.287*** |
| | (0.043) | (0.050) | (0.047) |

**Note:** The table reports the coefficients on the treatment variable in equation 4. Regressions are at the microregion level (N=456). Robust standard errors clustered at the mesoregion level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

# C. Data appendix

## C.1. Databases Used

| Acronym | Database | Years | Source |
|---|---|---|---|
| PC | IBGE - Population census microdata sample | 1991 (5%). | IBGE microdata made available by the Centro de Estudos da Metrópole web.fflch.usp.br/centrodametropole |
| | | 2000 (5%) 2010 (5%) | IBGE microdata loja.ibge.gov.br/populacao/amostra |
| IPEA1 | IPEA - Municipality areas | 2010 | www.ipeadata.gov.br |
| IPEA2 | IPEA - Climate data | 2002 | www.ipeadata.gov.br |
| IBGE1 | IBGE - Municipality Borders GIS files | 2010 | https://mapas.ibge.gov.br/bases-e-referenciais/bases-cartograficas/malhas-digitais.html |
| IBGE2 | IBGE - Evolution of municipality borders over census years | 1872-2010 | www.ibge.gov.br/home/geociencias/geografia/default_evolucao.shtm |
| IBGE3 | IBGE - National consumer price index | 1980-2010 (montly) | ww2.ibge.gov.br/home/estatistica/indicadores/precos/inpc_ipca/defaultseriesHist.shtm |
| SC | INEP - Brazilian school census | 1997,1998 | http://portal.inep.gov.br/microdados |
| TRES | STN - Brazilian National and State Treasuries | 1997,1998 | tesouro.fazenda.gov.br |

## C.2. Individual-level variables definitions

| Variable | Samples | Description / comments |
|---|---|---|
| Montly Wage | PC 1980, 1991, 2000 and 2010; IBGE3. | Monthly labor income in main occupation in the reference period, in 2010 reais.* ** |
| Monthly wage residual | PC 2000 and 2010. | Residuals of an individual-level regression of the log of wage on individual characteristics including age categories, schooling categories, sex and race. All regressions use sample weights provided in the IBGE microdata samples.* ** |
| Weekly hours worked | PC 2000 and 2010. | Usual number of hours worked at main job during the reference week (variables V0653 in the 2010 census and V0453 in the 2000 census.)* ** |
| Hourly wage | PC 2000 and 2010. | Montly wage divided by 4.33, and then by the weekly hours worked.* ** |
| Hourly wage residual | PC 2000 and 2010. | Residuals of an individual-level regression of the log of the hourly wage on individual characteristics (same procedure as in the monthly wage residuals calculations)* ** |
| Participant | PC 2000 and 2010. | Individual that is either formally employed, informally employed or unemployed.** *** |
| Formally employed | PC 1980, 1991, 2000 and 2010. | Individual that worked over the period of reference with a signed work card or as civil-service employee.** *** |
| Informally employed | PC 2000 and 2010. | Individual that worked over the period of reference as a private sector or domestic employee without a signed work card, or was self-employed.** |

| Variable | Samples | Description / comments |
|---|---|---|
| Employed | PC 2000 and 2010. | Individual either formally or informally employed. |
| Unemployed | PC 2000 and 2010. | Individual that declared that they looked for employment but were not employed over the period of reference.** |
| Migrant | PC 2000, 2010. | Individual that declares that its time of residence in their current municipality is less or equal to the year they finished schooling (numerical response in variable V0416 in 2000 and V0624 in 2010). |

* 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.

## C.3. Region-level variables definitions

| Variable | Samples | Description / comments |
|---|---|---|
| **Main outcome variables** | | |
| Microregion | PC 1980, 1991, 2000, and 2010; IBGE2. | Time-consistent boundary of microregion. The definitions are constructed in two steps, following a procedure similar to that described in Kovak (2013). First, I construct time-consistent municipality boundaries (known in the literature as minimum-comparable areas - MCAs) by joining municipalities with common ancesters for the period 1980-2010, based on the official IBGE municipality family tree (see source IBGE2 in subsection C.1). IPEA provides a similar definition for the period 1872-2007 (Reis et al. 2007) but in this source MCAs are more aggregated than needed for accurate comparisons in recent decades. Second, I generate time-consistent microregions by aggregating MCAs that share common ancesters also for the period 1980-2010. |
| FUNDEF shock (muni) | SC, TRES, PC 2000 | 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.) |
| FUNDEF shock (region) | | 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 2.) |
| Migrant population | PC 2000, 2010. | Total population of adults that were living in a different microregion in the year of the prior census |
| Working-age pop. | PC 2000, 2010. | Total population aged 15 through 64. |

| Variable | Samples | Description / comments |
|---|---|---|
| Average log hourly wage residual | PC 2000, 2010. | Average of the log of the hourly wage residual at the region level, for adult individuals reporting positive wage. The same estimation is used for other wage aggregates. |
| Employment rate | PC 2000, 2010. | Employed individuals as a share of the working age population. |
| Participation rate | PC 2000, 2010. | Individuals that participate in the labor force as a share of the working-age population. |
| Formality rate | PC 2000, 2010. | Share of formally employed in participant population. |
| Informality rate | PC 1980, 1991, 2000 and 2010. | Share of informally employed in participant population. |
| Unemployment rate | PC 1980, 1991, 2000 and 2010. | Share of unemployed in participant population. |