Equalizing the Great Equalizer: The Long-run Consequences of School Funding Reforms*

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Abstract

In 1998, Brazil implemented a major reform to increase the funding of elementary schools and equalize it across municipalities. Using linked administrative data and a new panel of municipality accounts combined with Treasury data and the annual Census of Education, we study the long-run educational and labor market outcomes of this reform. The poorest and most affected municipalities saw an immediate increase of 20% in their educational spending, and a close to 10% increase in their overall spending. Comparing new cohorts of students (affected by the reform) vs. older cohorts across more or less affected municipalities, we find a gradual increase in the number of years of schooling, an increase in high school and college graduation rates, as well as a rise in wages in the formal sector (+5%). These results highlight the critical role of education funding policies in raising wage levels and shaping the wage distribution in middle-income countries.

JEL Codes: J15, J38, J23, J31.

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1 Introduction

What is the role of education funding policies in lifting educational attainment and in reducing wage inequality in middle-income countries? Brazil offers one of the most spectacular example of rapid changes in the skills composition of the labor force in recent decades. Between 1999 and 2015, the share of workers with a high school degree rose from 26% to 47% in the formal labor force and from 14% to 31% in the informal labor force (Derenoncourt et al., 2021). This increase coincides with a remarkable decline in wage inequality: from 1995 to 2012, the Gini coefficient for the wage distribution in Brazil fell nearly 20% (Ferreira et al., 2022).

Yet we do not have a precise quantitative understanding of the role played by major changes in school funding reforms in the evolution of educational attainment and wage inequalities. This project attempts to fill this gap.

This project quantifies the role of the 1998 FUNDEF school funding reform of the elementary system in long-run educational and labor market outcomes. The goal of the 1998 reform was to equalize per-pupil educational spending for children aged 7 to 14 across states, and across municipalities within states. This reform was implemented in a context of a decade long increase in per-pupil spending disparities in public primary schools. These large inequities were inherited from the 1988 Constitution that mandated each municipality and state to fund primary schools by allocating a fixed proportion of their tax revenue to schools. Poorer municipalities had fewer tax revenues, and therefore a smaller envelope to spend on primary education; municipalities with strong demographic dynamics had also lower expenditures per student than municipalities with low population dynamics.

The 1998 school equalization reform has to key aspects. First, it equalizes per-pupil spending across municipalities within states *via* the creation of a state-level fund that pooled a fraction of municipalities and states' revenues that was then redistributed to municipalities according to their number of students. This state-level fund is called *Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério* (FUNDEF), literally meaning the fund for the maintenance and development of elementary education and the enhancement of teaching. Second, it imposes a per-pupil spending floor to promote adequacy across states.

The reform primarily benefitted poorer and rural municipalities, at the expense of larger cities that ended up being unaffected by the reform, or negatively affected by the reform. We show that the poorest and most affected municipalities saw an immediate increase of 20% in

their per-pupil spending in 1998, and close to a 10% increase in their overall spending. Our estimates show a gradual increase in the number of years of schooling, an increase in high school and college graduation rates, as well as in wages in the formal sector (+5%). We also show that the reform was effective at reducing the likelihood to benefit from any conditional cash transfer program (-2%). To our knowledge, our article provides the first causal evidence on how a school equalization reform affects wage inequality and poverty in middle-income countries.

We make two main contributions.

First, we provide an in-depth analysis of the causal effect of the 1998 school equalization reform – a large natural quasi-experiment – on school and labor market outcomes, as well as on poverty in a middle-income country. To capture the long-run effects of reform, we follow long-run educational and labor market outcomes in linked administrative data and we employ a difference-in-differences design that compares new cohorts of students who are affected by the reform vs. the older ones across more or less affected municipalities.

We start by documenting the role of school funding reforms on student outcomes. There is a long literature documenting their effects, but it is largely focused on the US, where there might be small and declining returns to investment in schooling. Although Hanushek (1997) and Hanushek (2003), in two meta-analyses, concludes there is no relationship between school resources and student achievement, Jackson (2020) argues that this relationship has been misinterpreted from a statistical point of view. He points out that these meta-analyses were conducted on correlational studies, prone to omitted variable bias and characterized by a lack an exogenous source of variation in school spending. By contrast, when studies causally identify the effect of school spending on outcomes – leveraging state court-ordered school finance reforms that changed the within-state distribution of funding in the US – they unambiguously find that money matters in education (see e.g., Card and Payne (2002); Jackson et al. (2016); Lafortune et al. (2018); Biasi (2023)). In this project, we exploit arguably exogenous and large spending shocks created by the introduction of a state-level fund to equalize resources across municipalities in Brazil in the mid-1990s, and show – in line with the recent US literature – that school spending matters. We show that older cohorts of students who are born before 1984 (and are therefore unaffected by the reform) were on similar trends in terms of number of years of schooling, probability to complete high school and college in more affected vs. unaffected municipalities. Starting for cohorts born in 1985 (i.e. cohorts that have one year of exposure to the reform), our educational outcomes exhibit a gradual increase for students born in municipalities affected by the reform vs. those in

unaffected ones. We find that the first fully exposed cohort (i.e. children born in 1991) has gained 0.13 years of schooling. This result shows that an extra 10% in spending is associated with 0.065 extra years of schooling. Using a similar design, we find that the first fully exposed cohort (born in 1991) is respectively 1% more likely to complete high school and 2% more likely to complete college than students in unaffected municipalities in the control group. An extra 10% in spending is associated with 0.5% in high school graduation rates, and 1% college graduation rates. Interestingly, the positive effects of the reform are greater for cohorts born after 1991, even if they are fully exposed to the reform – as the 1991 cohort is. This can be due to the fact that the positive effects of the reform take time to fully materialize, as the reform may affect long-run investment decisions that take time to be fully effective (such as teacher's hiring decisions, investment in infrastructure, etc.).

After documenting the role of school funding reforms on student outcomes, we turn to long-run poverty and labor market outcomes – a question on which the literature is much scarcer since it is empirically hard to track the outcomes of students differentially exposed to a school funding reform in the long-run. The richness of the Brazilian data is unique in this regard. We use data on the universe of the private formal workforce in 2019 combined with the full registry of cash transfers beneficiaries that contains the place of birth to compare newly affected cohorts to old unaffected cohorts within municipalities. We find that the first fully exposed cohort (born in 1991) has wages 5% higher as a result of the reform when they grew up in a city that was positively treated by FUNDEF vs. in an unaffected city. So, in our case a 10% increase in per-pupil spending is associated with 2.5% higher formal wages. These effects are statistically and economically significant, but lower, however, than in Jackson et al. (2016) studying the effects of the 1970-1990 school finance reforms in the US and in which a 10% increase in per-pupil spending is associated with 7% higher wages. Why are the effects on wages lower than Jackson et al. (2016)'s estimates? I would at first think that returns to education would be higher in a middle-income country as Brazil.I think it would be nice to develop this discussion a bit more, and think of possible causes – structure of the labor market in BR x US? Differences in how schools actually spent the money (connect to optimal spending)? We also find that the first fully exposed cohort (born in 1991) is nearly 2% less likely to be part of a conditional cash transfer program following the reform.

Perhaps one of the most influential papers to document the effect of school spending on long-run outcomes is Duflo (2001) who uncovers the role of school construction in Indonesia between 1973 and 1978. During that period, Indonesia underwent what was at the time the fastest primary school construction program ever undertaken. Using intercensal cross-

sectional survey data and a difference-in-differences approach that exploits differences in the number of schools constructed across regions and differences in the number of years of exposure across cohorts, she finds that each primary school constructed per 1,000 children led to an average of up to 0.19 years of education, as well as up to 2.7 percent increase in wages. This paragraph seemed to me a bit out of place to me where it was (after the next one), I felt like it didn't dialogue with the surrounding paragraphs so I moved it here. I think adding the rest of the the literature (Ree et al. (2018), Duflo et al. (2015), Das et al. (2013)) will help too.

Second, we hope to contribute in a future version of this paper by diving into the heterogeneous effects of school expenditures. We'll contribute to the literature on the mechanisms through which education spending affect future educational and labor market outcomes by analyzing the effect of different types of school expenditures. Our first step is to construct a database on the different categories of education spending around the time of the FUNDEF reform: we're able to disentangle teachers' wage bill at the municipality level from the rest of educational expenditures (i.e. staff wage bill, and infrastructure) by combining the information on teachers' wages contained in the linked employer-employee data to our municipalities accounts' database. We're able to recover the number of teachers employed in each municipality, their level of education, and their wages. As a second step, and in a future version of this paper, we hope to be able to analyze the effect of teachers' quantity vs. teachers' quality, class size vs. investment in infrastructure at the municipality level. There is a long-lasting debate on optimal input spending in the education literature, and the previous research on school funding reform admittedly hasn't been able to tackle this question directly (Jackson et al., 2016; Lafortune et al., 2018). With such data we hope therefore to fill an important gap in the literature, with major policy implications.

In sum, our article sheds new light on the role of school funding reforms in shaping structural change and wage inequality in developing countries. We push the literature forward by using the rich Brazilian data environment to help ellucidate: i) the mechanisms through which school finance reforms can compress educational and wage gaps in middle-income countries; ii) the heterogeneity of the effect of these reforms across students from high vs. low-income family backgrounds and across racial groups; iii) the ultimate labor market effects of such reforms.

The paper is organized as follows. Section 2 provides the institutional background of the Brazilian education system, as well as the legislation of the 1998 education spending reform. Section 3 presents our data sources. Section 4 defines our treatment variables, and

research designs separately for our analyses of municipality-level outcomes and individual-level outcomes. Section 5 presents our results on the effect of the implementation of the equalization funds on education spending, as well as on individual-level outcomes such as on school quantity, poverty and labor market outcomes. Section 6 concludes.

1.1 Related Literature

We contribute to three strands of the literature. First, we add evidence on the effects of a large school finance reform – a type of reform that has mainly been studied in the context of the United States with both of the so-called “equity" and “adequacy" reforms. An older literature going back to the influential Coleman (1968) report has questioned the efficacy of the increase in spending from such reforms in improving educational outcomes. Jackson (2020) and Jackson and Persico (2023) argue, however, that interpreting those studies – based on correlational observation – as showing no such impact is statistically misguided, and that the best evidence we have strongly suggests otherwise. In particular, more recent papers have tried to credibly identify the causal effects of such reforms in the US, leveraging the plausible exogeneity in the timing of their implementation.

Looking at court-ordered school finance reforms between 1971 and 2010, Jackson et al. (2016) analyze the impacts of increased school spending on educational and long-term economic outcomes. Their estimates suggest strong impacts of money in education: a 10% increase in per pupil spending leads an additional 0.31 years of education and an extra 7 p.p in the probability of completing high school, as well as 7.7% higher wages and a reduction in the probability of being in poverty of 2.6 p.p among all children, with even higher estimates for low-income children.

Lafortune et al. (2018) report similar results when focusing on the so-called “adequacy" reforms. They find that these funding equalization reforms significantly increased test scores in poorer districts but had no discernible effects on rich ones, indicating that their outcome equalization impact was done via “catching up" instead of “levelling down". They also find that the reforms led to a reduction in the slope of the relationship between test scores and income (a measure similar to Hoxby (1998) and Card and Payne (2002)). When looking at a similar slope (between per pupil spending and income), Biasi (2023) shows that school finance reforms also led to its reduction, which had the significant effect of increasing intergenerational mobility, especially for the bottom of the income distribution. A thorough review and meta-analysis of this literature for the US is avaiable in Jackson and Mackevicius

(2024).

Still within the US, some recent papers have started to empirically address the optimal spending of educational inputs, highlighting the mechanisms through which school finance reforms can be effective. Baron (2022) leverages close elections in funding referenda in Winconsin to analyze the seperate impacts of increasing operational expenditure (e.g. teacher compensation and class size) and capital expenditure (e.g. new buildings and renovations). He finds that improvements in operational inputs lead to sizeable positive effects in test scores, enrollment, and a reductions in dropout rates, whereas increases in capital outlays show no significant impact on those variables. Relatedly, Baron, Hyman et Vasquez (2024) look at the effects on crime of two natural experiments in Michigan: a school finance reform which increased only operational spending, and close elections in capital spending referenda. They find that for both types of increased funding, impacted students have substantially lower probabilities of being arrested as an adult.

Finally, Biasi, Lafortune and Schönholzer (2024) explore the heteregenous impacts of different types of increased capital expenditure in US states: by exploiting close elections in school funding referenda with explicitly specified use of funds, they are able to estimate the impact on test scores for different categories of capital spending, finding that there's a large heterogeneity in the impact of capital outlays on test scores. In particular, they find that expenditures such as HAVC, classroom space and STEM equipment have positive effects on test scores, whereas other like athletic facilities and busses do not.

By contrast with this US literature, we bring evidence on the effect of school finance reforms in a middle-income country like Brazil, where presumably the marginal effects of an additional dollar of expenditure should be larger. We also continue to push forward the growing literature on optimal allocation of educational resources, helping to pinpoint the specific mechanisms that contribute to the effectiveness of school finance reforms. This is a topic of significant importance for economic policy – especially in low- and middle-income countries with scarcer resources –, and on which the literature is still far from reaching a consensus.

Second, we contribute to the literature that has analyzed the long-run consequences of changes in school inputs on long-run outcomes in developing countries. The seminal work of Duflo (2001) analyses the effect of school construction in Indonesia on educational attainment and wages. We push the literature forward by bringing more granularity of the data so that we're able to study the effects on the labor markets beyond the effect on wages (effect on the dynamics of wages, effect on co-workers' wages etc.)

Finally, we complement the vast literature that exists in Latin America on school policies. The bulk of the evidence concerns changes in the demand for schooling. Many papers—notably in Chile—have studied the role of vouchers on school choice. We contribute by adding evidence on policies that affect the supply of schooling. XX Cite here all papers that talk about FUNDEF often published in BR outletsXX. To our knowledge, we provide the first comprehensive study of FUNDEF on long-run outcomes.

2 Institutional background and the FUNDEF reform

2.1 Institutional background

Structure of the Brazilian educational system. The system of education in Brazil is structured around two pillars[1]: i) basic education – which encompasses preschool, primary school, and secondary school; and ii) higher education. This paper focuses on primary education. Primary education has two levels: level 1 for grades 1-4th (i.e. ages 7 to 10)[2] and level 2 for grades 5-8th (i.e. ages 11 to 14)[3]. The 1988 Constitution instituted education as a universal right: primary education is free at public institutions, and compulsory. The system is highly centralized: the 1996 National education law established a common curriculum in primary education[4]. In 1997, before the FUNDEF reform, the bulk of primary schools are public (90%), while the remaining 10% of schools are private (see Table 1). This share is stable over time, until today.

Provision of public schools. Municipal governments provide and fund all preschools, and some elementary schools, the "municipal elementary schools". State governments share the responsibility of providing elementary schooling with municipalities, and fund "state elementary schools". As a result, state and municipal elementary schools coexist and run separate schools in each municipality. State governments are also in charge of providing secondary education, while the federal government funds and provides tertiary education.

Enrollment in public schools. In 1997, more than 30 millions students were enrolled in public primary schools (see Table 1), of which 60% (i.e. nearly 19 million) in level 1, and the remaining 40% in level 2 (i.e. 11 million). While level 1 is mainly provided by municipalities, children mainly attend state schools for level 2. Table 1 shows that, in 1997, 53% of level 1 students were enrolled in municipal elementary schools, while 78% attended state schools for level 2. Interestingly, enrollment varies considerably around the time of the reform[5]: Figure

[1] As defined in the 1996 National education law, the "Lei de Diretrizes e Bases da Educação" available here.

[2] Before 2005, primary education started at age 7; in 2005, a reform gradually expanded primary education to children aged 6. All primary schools had to comply with this new law by 2010. In practice, the vast majority of them had complied by 2008. Today, compulsory schooling is therefore applicable to children aged 6-14, while it was only compulsory for children aged 7-14 at the time of the implementation of the FUNDEF reform.

[3] Level 1 of primary education in Brazil corresponds to "Ensino Fundamental I", or EF1; Level 2 corresponds to "Ensino Fundamental II", or EF2.

[4] The 1996 "Lei de Diretrizes e Bases da Educação" also instituted a national curriculum for secondary education, increased the duration and number of teaching days, established minimum standards for teacher education and made the nine-year primary education compulsory; see here and Gordon and Vegas (2005) pp5-6.

[5] See also de Mello and Hoppe (2005).

1 shows that the total number of children enrolled in public elementary school increased by 13.1% between 1995 and 2000, from 28.5 million to 32.3 million, with the sharpest increase happening between 1997 and 1998. This increase may be a direct effect of the FUNDEF reform, as the funding mechanism (see subsection 2.2) incentivized municipalities to increase enrollment at the local level by directly conditioning funding to the number of children enrolled (see also Gordon and Vegas (2005)) . Finally, this increase in enrollment coincides with a shift in the provision of elementary education, with an expansion of enrollment in municipal schools and a decline in those provided by states. By 2003, 66% of level 1 students were enrolled in municipal elementary schools, while 54% attended state schools for level 2. This corresponds to 79% of all children aged 7-14.

2.2 The 1998 school funding reform (FUNDEF)

Large inequities in per-pupil spending before 1998. The first school finance reform of the modern period in Brazil was embedded in the 1988 Constitution that stipulated that each municipality and state had to fund primary schools by allocating 25% of their tax revenue to schools. The ratification of the Constitution led to an increase in the total funding of Brazilian elementary schools, but also to important inequities across the territory.

These inequities stem from two mechanisms, that often reinforced each other within the same municipalities: (i) poorer municipalities had fewer tax revenues, and therefore a smaller envelope to spend on primary education; (ii) municipalities with strong demographic dynamics (what is meant by that? younger/increasingly young population?) had also lower expenditures per student than municipalities with low population dynamics. I think it would be nice to quote some discussions from the period, maybe from legislastors during the passing of the reform (couldn't find anything so far tho).

Figure 2a shows large inequities in per-pupil spending across states in 1997: São Paulo, the highest spending state, spent 10 times more than Alagoas, the lowest spending state. While São Paulo was spending on average R$7,466 per student enrolled in municipal schools in 2019 terms (i.e. a bit less than $2,000 per year per student in current dollars), Alagoas was spending R$766 per student (or a bit less than $200 per year per student).

Even within states, there were large inequities across municipalities in per-pupil spending before the FUNDEF reform. Figure 2b highlights the case of Rio Grande do Sul, a rich state in the South of Brazil. The city of Porto Alegre (the state capital, with 1.3m inhabitants) spent on average R$10,047 per student, that is, 15 times more than Trindade do Sul – the lowest

spending municipality in that state, a city of 6,000 inhabitants.

The 1998 school funding reform. The goal of the 1998 school finance reform reform was to reduce inequality in educational spending inherited from the 1988 school funding reform. The reform has two key aspects: (i) it equalizes per-pupil spending across municipalities within states *via* the creation of a state-level fund that pooled a fraction of municipalities and states' revenues that was then redistributed to municipalities according to their number of students. This state-level fund is called FUNDEF[6]; (ii) it imposes a per-pupil spending floor to promote adequacy across states: the federal government topped-up the state-level fund by the necessary funds to reach the per-pupil spending floor.

Taken together, these two aspects of the reform resulted in an increase in per-pupil education spending for primary schools in poor municipalities and poor states. Figure 3a shows that per-pupil spending increased by 15% between 1997 and 1998 for municipal schools, increasing the average per-pupil spending from R$5,204 to R$5,994 (in constant 2019 R$ terms). Figure 3b shows that poor municipalities benefitted the most from the 1998 reform: the reform more than doubled per-pupil spending for the bottom 8% of municipalities.

Contributions to state-level funds. Municipal, state and federal governments had to contribute to the FUNDEF state-level funds.

Municipalities and states had to contribute a fraction of their tax revenues to the fund. Only contributing revenues entered the formula while non-contributing revenues were set aside when computing their contributions. Appendix Figure A2 shows that contributing revenues represented on average 62% of total revenues for municipalities. Importantly, the contributing revenues were made of taxes that could not be influenced or manipulated by municipalities, since they all correspond to taxes collected at the state or federal level and then transferred to municipalities.

Municipalities had to contribute to the state-level fund each year according to the following formula:

$$\text{Municipality Contribution}_m = 15\% \times (\text{IPI}_m + \text{ICMS}_m + \text{LC 87/96}_m + \text{FPM}_m)$$

With IPI_m a tax on industrial products proportional to exports collected by states then transferred to municipalities; ICMS_m, a tax on consumer goods, similar to VAT, collected by

[6]FUNDEF stands for "Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério" literally meaning the fund for the maintenance and development of elementary education and the enhancement of teaching.

states before being transferred to municipalities; LC $87/96_m$, an interstate commerce taxes designed to complement ICMS_m, collected by the federal government before being transferred to municipalities; and FPM_m, a transfer from federal government to municipalities mandated by the 1988 Constitution. We provide more details on these taxes in Appendix A.

States had to contribute according to a similar formula:

$$\text{State Contribution}_s = 15\% \times (\text{IPI}_s + \text{ICMS}_s + \text{LC } 87/96_s + \text{FPE}_s)$$

With FPE_s being a transfer from federal government to states.

Federal governments contributed to the state-level fund only if the sum of the municipal and state contributions was below a per-pupil spending floor fixed annually. In practice, seven states out of 27 benefitted from the federal top-up in 1998.[7]

Transfers from state-level funds. All the contributions from the municipal, state and federal governments are the redistributed back to municipal and state governments, according to a formula that is proportional to enrollment in the respective municipal and state schools. We cross-checked in particular that the transfers reported in our data perfectly match the simulated amounts obtained by using the formulas for both transfers to municipalities and states in Appendix Figure A3.[8] Furthermore, the 1998 reform imposed that at least 60% of the net FUNDEF transfer (i.e. the difference between contributions sent to FUNDEF and the transfers received from FUNDEF) must be used to pay teachers in the primary education system. Two questions: (i) This requirement of net FUNDER transfer to be majority pay it out to teachers implied in higher wages? (ii) If so, did could have induce high-quality teacher (or at least those willing to pay the cost of moving) to move from control to net beneficiaries municipalities?

The education spending floor. Since the 1988 Constitution, municipal and state governments must spend at least 25% of their overall tax revenues on education. After the 1998 FUNDEF reform, municipal and state governments must therefore spend at least the equivalent of 15% of their contributing revenues in education, the other 10% coming from non-contributing revenues.

[7] See Gordon and Vegas (2005), p.17.
[8] See Appendix A for more details on the formulas used for the FUNDEF transfers.

3 Data sources

We combine a wide variety of data to paint a comprehensive picture of the effect of school finance reforms on educational outcomes and labor market outcomes. We first combine data on municipal and state finances on both revenues and expenditures. This allows us to precisely define which municipalities and states are more or less effected by the financial shock created by the 1998 reform, and its impact on their expenditures. Further down our analysis, we use these financial accounts to dive into the mechanisms through which educational expenditures can affect educational and labor market outcomes. Second, we collect a wide array of datasets to document the effect of the reform on education, poverty and labor market outcomes. Further details – including how we accessed the data – are provided in Appendix B.

A new panel on municipal and states finances, 1989-2020. We collect, harmonize and combine different data sources to recover the information on both revenues and expenditures for the 5,500 municipalities in Brazil and its 27 states. We construct a new panel of municipalities and states, from 1989 to today.

For municipalities, we use the FINBRA data (1989-), a dataset comprised of public accounts information annually and mandatorily provided by municipalities to the National Treasury, which gives us with details on both revenues collected and expenditures by broad categories starting in 1989. Because we're interested in the financial shock received by municipalities in the wake of FUNDEF, we supplement the FINBRA data with a series of data sources:

1. Treasury data (1998-) that gives the information on all the contributing taxes that enter the formula to compute the municipalities' contributions. Specifically, this external data source allows us to (i) cross-check that the information on IPI, XX and XX in the FINBRA and the treasury data are consistent. We show perfect correlation between the two in Appendix XXX; (ii) add the IPM tax to the database as this specific tax that enters the calculation of FUNDEF contributions was missing in the FINBRA. The treasury data therefore allows us to precisely simulate the contributions due to FUNDEF by each municipality. Doing so, we're also able to cross-check that the actual amounts of contributions sent to FUNDEF by municipalities are in line with the simulated contributions (see Appendix Figure XX).

2. Newly digitized data on FUNDEF allocation keys that gives us the information on how much money from the state-level fund each municipality receives as a FUNDEF

transfer. We collected the allocation keys from the ministry of education archives and digitize them from 1998 to today (see Appendix Figure XX). Since the allocation keys depend on enrollment numbers in primary education that are contained in the Census of Education (see below), we cross-checked that the enrollment numbers underpinning the allocation keys are perfectly consistent with the enrollment numbers we processed from the Census of Education (see Appendix Figure XX).

3. Newly digitized data on the federal minimum spending floors that vary by year, and rural vs. urban areas. This allows to recover which states benefit from the federal top-up, and to better simulate the net FUNDEF transfers received by municipalities. We collect this data from Ministry of Education archived reports.

We're also interested in the level of educational spending by municipalities, as well as by the different categories of spending to understand the input optimality of educational spending. We therefore supplement the FINBRA data with:

1. The Census of Education (1995-) that provides us with the information on enrollment, and allow us to compute a per-pupil education spending at the municipality level. The Census of Education is publicly available every year at the school by grade level between 1995 and 2004. Starting in 2005, this information is available at the individual level. We aggregate the information on enrollment at preschools and primary schools (EF1 and EF2[9]) to compute per-pupil spending at the municipality level across all years.

2. The matched employer-employee database RAIS (1985-) that gives us the information on the number of teachers, their wages, their level of education and age. This information is available at the XX school level XX in the employer-employee data. We combine the information on total teacher's wage bill at the municipality level, with the total education expenditure contained in FINBRA to disentangle teachers' wage bill from other types of expenditures. We use the information on the number of teachers combined with the enrollment figures in the Census of Education and the number of schools contained in XX XX to recover an average class size at the municipality level. Finally, we're able to capture the average education level and tenure of teachers (i.e. a proxy for teachers' quality) at the municipality level.

For states, states archived accounts provide us with their revenues and expenditures side. We're in the process of gathering them, as well as data on states' audits that allow us to

[9]Level 1 (Ensino Fundamental 1) and Level 2 (Ensino Fundamental 2) of primary education, respectively.

cross-check that the actual amounts reported in the official state accounts are in line with the actual amounts reported in states' audits. As for municipalities, we augment this data source with treasury data on FUNDEF contributing taxes and cross-check that the actual and simulated contributions are in line (see Appendix Figure XXX). We're also able to cross-check that the actual and simulated FUNDEF transfers are in line (see Appendix Figure XXX) when we use as allocation keys the enrollment numbers in state schools provided in the Census of Education.

Data on school quantity and quality. We study the effect of the reform on school quantity by leveraging the information on educational attainment contained in the Census of Education (1995-). Because the data is available at the school by grade level from 1995 to 2004, and then at the individual level afterwards, we easily construct an average educational attainment variable at the municipality level from 1995.

We study the effect of the reform on school quality by leveraging data on test scores: (i) at age 14, i.e. right after the end of primary education, by using the information contained in the SAEB survey (1995-). This survey provides information on XXX at the XX level; (ii) at age 18, or right before entering tertiary education by using the information contained in the restricted access Exame Nacional do Ensino Médio (ENEM) database. The ENEM is a national-level university entrance exam taken by students at the final year of High School; some caveats on using it as a measure of school quality therfore apply, since the FUNDEF might have affected not only the quality of education received by students who take the ENEM, but also the composition of students who end up choosing to do so (Jackson and Persico, 2023) . After 2007, Censo Escolar has information at the school-level on physical infrastructure. Do we believe that school quality can measured as such? Gadenne (2017): "combine the eight variables related to the quality of the infrastructure that are measured consistently over the period (number of municipal schools with computers, with internet, with a sports facility, a library, television/video equipment, and connected to the sewage and electricity systems) using principal component analysis to construct a quality index."

Data on poverty. We study the long-run effects of the reform on poverty, by using the restricted access to the registry of conditional cash transfers recipients (Cadastro Único). This restricted access dataset starts in 2010 and provides the information on conditional cash transfers records and their beneficiaries at the individual level.

Data on labor market outcomes. We leverage two data sources to quantify the long-run effects of the reform on labor market outcomes:

1. Censuses data: The 1991 and 2010 Censuses allow us to assess the effect of FUNDEF on Child labor. We plan to use the 2022 Census when it is released to study the effects on the probability of employment, and on the probability of employment in different sectors (formal, informal salaried, self-employment), as well as the effect on informal wages. Importantly, the Census data contains the information on the place of birth of the person, an information that we use as a proxy for the municipality where the person went to school.

2. The matched employer-employee data (RAIS): its panel nature allows us to analyze not only the effects of the reform on average wages but also on wage dynamics in the formal sector.

4 Identification strategies

I think it would be nice if there was a "null result" in terms of observational variation in education spending and education outcomes, similar to the original Coleman report (Jackson et al. (2016) do it Section IV.B). I feel like it illustrates well the need for credible causal estimates, which the paper intends to give, and what a good opportunity the FUNDEF reform is to get that.

We know that 60% of FUNDEF net transfers should go to teachers wages in primary education; The other 40% are at the discretion of the mayor/governor? The reform especified any other use? If so, that's one the first things to look at.

For our analysis of the effect of the reform on municipality-level outcomes (such as municipalities' spending), our main identification strategy exploits the heterogeneous change in educational spending at the local level caused by the creation of the state-level equalization funds. We compare strongly affected municipalities vs. those for which the FUNDEF reform did not affect education spending in a difference-in-differences design.

For our analysis of the effect of the reform on individual-level outcomes (such as education quantity, quality and subsequent labor market outcomes), our main identification strategy exploits the change in educational spending over time across cohorts of students within municipalities (in the spirit of, e.g., Saez et al. (2019)). We leverage a measure of cohort exposure that compares cohorts based on years of exposure to the policy, and use both variation in

municipalities' dosage and variation in cohort exposure in a difference-in-differences design.

4.1 Identification strategy at the municipality level

Definition of the municipalities' dosage measure. The 1998 reform created heterogeneous changes in education spending S_m in primary schools at the local level. We relate these quasi-random reform-induced changes in school spending – rather than all variation in spending – to remove the confounding effects of unobserved factors that may impact both school spending and student outcomes.

Education spending at the municipality level can be written as:

$$\begin{aligned} S_m &= 25\% \cdot (NCR_m + CR_m) \\ &= 25\% \cdot NCR_m + 10\% \cdot CR_m + \text{Net FUNDEF transfer}_m \end{aligned}$$

With NCR_m being revenues that do not contribute to FUNDEF (i.e. non-contributing revenues); CR_m are revenues that do contribute to FUNDEF; and Net FUNDEF transfer$_m$ is the net transfer received by municipalities, and is simply calculated as the difference between the transfer received and the contribution make to the state-level fund (see Subsection 2.2). Using our new panel of municipalities' finances, we're able to compute the net FUNDEF transfer for each year at the municipality level.

Our main treatment variable classifies municipalities into three distinct groups, according to the intensity of the FUNDEF shock on their budget. We compute a dosage measure for each municipality that quantifies the effect of a 1% increase in spending induced by FUNDEF in the first year of the reform. This measure writes:

$$\text{Spending Dosage}_m = \frac{\text{Net transfer in 1998}_m}{\text{Education spending in 1997}_m} \times 100$$

This dosage measure goes from -1 to +1 (or more in case the net FUNDEF transfer is greater than the 1997 education spending), and is centered around 0. Figure 5

We use this measure to define three quantiles of dosage that will determine our three groups of municipalities: Possibility of using continuous treatment instead of quantiles, à la Callaway, Goodman-Bacon and Sant'Anna (NBER 2024)?

1. Net beneficiaries municipalities are those that benefit from the 1998 FUNDEF reform and are in the top quartile of the distribution of the municipalities' dosage measure. Figure 4a displays the per-student education spending over time for net beneficiaries in green, with and without the net FUNDEF transfer. The shaded area in green shows the

importance of the net FUNDEF transfer. Net beneficiaries municipalities are those that spend the least in education (compared to the two other groups) before the reform. We estimate that the FUNDEF reform was able to increase per-student spending among these municipalities from R$1,100 to R$1,650.

2. Net neutral municipalities (or control municipalities) are those that are comprised between the 1st and the 3rd quartile of the distribution of the municipalities' dosage measure. On average among this group of municipalities, the FUNDEF reform left their education spending unaffected, at R$1,750 per student (see yellow lines and shaded area in Figure 4a).

3. Net contributors municipalities are those that looses from the 1998 FUNDEF reform and are in the bottom quartile of the distribution of the municipalities' dosage measure. Interestingly, contributors municipalities are those that spend the most per-student before the reform. We estimate that the FUNDEF reform was able to reduction in per-student spending from R$4,000 to R$3,200 (see red lines and shaded area in Figure 4a).

The importance of the net FUNDEF transfer on total expenditures – although mechanically smaller than on education spending (which represents on average 20% of total spending) – can be seen in Figure 4b. The 1998 reform limited the spending increase among relatively rich municipalities, accelerated it among poorer municipalities.

Our balance table is displayed in Table 2.

We define an alternative treatment variable using another municipality's dosage measure that captures the effect of increasing spending by R$1 per student around the time of the reform.

$$\text{Alternative Spending Dosage}_m = \frac{\text{Net transfer in 1998}_m}{\text{Enrollment in 1997}_m}$$

We present the distribution of this alternative dosage measure in Appendix Figure B1, and the corresponding balance table in Appendix Table B1. It paints a consistent picture with our baseline dosage measure.

Research design. We rely on a difference-in-differences strategy to assess the effect of the policy on outcomes measured at the municipality level.

We restrict for now our analysis of the net beneficiaries vs. the control group, and set aside the net contributors – those municipalities that are "negatively treated" by the reform.[10] The model writes:

[10]This is because net contributors are following a very different (and increasing) per-pupil pre-trend (See

$$\mathrm{Y}_{mt} = \alpha + \sum_{k} \beta_k \cdot \mathrm{D}_m \cdot \mathbb{1}\{k = t\} + \delta_m + \delta_t + \varepsilon_{mt},$$

With Y_{mt} the log educational spending of municipality m in year t, D_m equals 1 if the municipality belongs to the net beneficiaries group, and 0 if it belongs to the control group. Our coefficient of interest is β_k which measures the effect of reform k years after base year 1997. We include municipality fixed effects δ_m to control for fixed differences in outcomes between municipalities. The year fixed effects δ_t control trends in outcomes that are common to all municipalities. Standard errors are clustered at the municipality level.

4.2 Identification strategy at the individual level

Data and proxy for the municipality where an individual went to school. We switch from the municipality-level design to the individual once to study the effects of the reform on education, poverty and labor market outcomes. In what follows, we use the linked employer-employee data matched with the registry of conditional cash transfers recipients (Cadastro Único) and to our municipality accounts' database. Our sample of analysis consists of all adults employed in a formal job in December 2019 aged 21 to 44 years old (i.e. born between 1975 and 1998).

We proxy the municipality where an individual grew up and went to school to by the place of birth. One difficulty here is that the place of birth is not directly observable in the linked employer-employee data. It is, however, in the registry of conditional cash transfers recipients – which partly overlaps with adults present in the linked employer-employee data. We therefore match these two datasets to recover the municipality of birth for approximately 30% of our sample in the linked employer-employee data. For the remaining 70% of adults we proxy the municipality of birth by the municipality of job spell. One caveat here is that only 40% of individuals work in the same municipality where they were born – so that our proxy for the municipality of birth is prone to bias. If it is the case that on average high-skilled individuals born in net beneficiaries municipalities tend to move to control municipalities (i.e. the "never-takers") then our results presented in Section 5 are attenuated. Specifically, we will underestimate the benefits of the reform on educational outcomes and labor market

Figure 4a). These municipalities are concentrated in São Paulo and Minas Gerais, and may have responded to the policy by overcompensating the negative net FUNDEF transfer. We're currently working on better understanding the public finance response of that group, before we're able to include it in our analysis. Eventually, we hope that this approach will allow us to both evaluate the response of municipalities that won and lost from the reform, relative to an unaffected control group. We'll therefore be able to shed light on whether the effects are symmetric, and if not, why.

outcomes. If the never-takers move to net contributors municipalities instead of control ones, then the magnitude of this bias is smaller. This downward bias is magnified if it is also true some some lower-skilled born in control municipalities move to work in net beneficiaries municipalities ("always-takers"). Because we do have the exact municipality of birth for all conditional cash transfer recipients, we believe that our results are less prone to this bias (which would be an upward bias in this case – i.e. we would overestimate the reduction in poverty due to the reform).

Perhaps we could get an idea of migration across 1991 and 2010 using Census data. It's was one of Card's points. If we believe population do not move much (IDK about internal migration in Brazil during this time), we may believe that the effects are persistent (on educ. attai. and labor market outcomes). In a future version of the paper, we plan to leverage the micro-files of the 2022 Census (soon to be relased) that contains the municipality of birth at the individual-level so that we can benchmark our results obtained with the linked-employer-employee data with these ones. Access to the 2022 Census will also allow us to study a broader set of labor market outcomes, such as the probability to participate in the labor force, or to be employed in the informal sector.

Definition of the cohort exposure measure. Because the FUNDEF reform is implemented in 1998 for primary education, all students born in 1984 or before (i.e., who were 14 or older in 1998) were too old to be affected by the reform. Children born in 1985 (respectively 1986, ..., 1991) were affected by the reform for one year (respectively 2, ..., 6 years). All children born in 1992 and after were fully affected by the reform (i.e., benefitted from increased funding for the full 7 years of their primary education).

Research design. In the following difference-in-differences design, outcomes are measured at the student level i, and the treatment variable interacts the cohort exposure measure described above with the binary treatment variable that classifies municipalities into net beneficiaries vs. control municipalities. We estimate the following model:

$$\mathrm{Y}_i = \alpha + \sum_k \beta_k \cdot \mathrm{D}_{m(i)} \cdot \mathbb{1}\{k = c(i)\} + \mathrm{X}_i'\Gamma + \delta_{m(i)} + \delta_{c(i)} + \varepsilon_i$$

With Y_i the outcome for individual i; $D_{m(i)}$ is an indicator that takes the value 1 if that person i was born in a net beneficiary municipality, 0 if not; $c(i)$ indicates the cohort (year of birth) of individual i. Our coefficient of interest is β_k that measures effect of reform k years relative to the last not exposed cohort (i.e., 1984). We include a set of individual-level controls

(race, gender) X_i, as well as municipality (δ_m) and cohort fixed effects δ_c. The cohort fixed effects control for trends in outcomes that are common to all municipalities. Standard errors clustered at the municipality of birth level.

As a robustness check, it might be nice to see if there are any migration/family income trends between treatment and control municipalities in the period. Lafortune et al. (2018) mention the possibility that a SFR might reduce the value of living in a high-income municipality, making high-income families to move to low-income municipalities and possibly increasing educational achievement in those cities. It would be nice to confirm that there are no such trends.

5 Results

Might be nice to compare results with other succesful education reform programs in Brazil/LatAm, to assess the relative efficacy of FUNDEF

5.1 Effects of the 1998 reform on municipalities' spending

Figure 6 presents our results from our municipality level difference-in-differences design. Prior to the reform, educational spending in the net beneficiaries municipalities and the net neutral municipalities (control group) evolve in parallel. Starting right in 1998, the first year of the reform, there is an immediate, sharp increase in educational spending of the net beneficiaries of 20% above and beyond what's happening in the control group.

This is a strong first stage for a school finance reform.

In the future, we're planning to document the effect of the reform on other several first-stage outcomes such as enrollment and enrollment of the correct age. We're also planning to document the public finance response of municipalities (and in particular the public finance response of net contributors).

5.2 Effects of the 1998 reform on school quantity

Interesting number to have: what's the implied needed increase in per-pupil spending throughout all school-age years for low-income children to equalize the educational attainement gap between poor and rich families? How feasible is that?.

Figure 7 presents our results of the effect of the reform on the number of years of schooling, using our difference-in-differences design on individual outcomes. It shows that for cohorts born up to nine years before the first cohort (born in 1984) is exposed, the number of years

of schooling is the same in the net beneficiaries and in the control group. Starting for the cohort born in 1984, there is a gradual increase in the number of years of schooling in the net beneficiaries vs. the control municipalities. The first fully exposed cohort (born in 1991) has gained 0.13 years of schooling. Our results show that an extra 10% in spending is associated with 0.065 extra years of schooling.

Figure 8a presents our results of the effect of the reform on the probability of completing high school. The first fully exposed cohort (born in 1991) is 1% more likely to complete high school than unaffected municipalities in the control group. Our results are consistent with the idea that an extra 10% in spending is associated with a 0.5% increase in high school graduation rates.

Similarly, Figure 8b presents our results on the probability of completing college. The first fully exposed cohort (born in 1991) is nearly 2% more likely to complete college than unaffected municipalities in the control group, i.e. an extra 10% in spending is associated with a 1% in college graduation rates.

In the future, we'll document the effect of the reform on school quality, and on other outcomes such as teachers' wages, teachers' qualifications and class size.

5.3 Effects of the 1998 reform on poverty

Figure 9 (maybe address pre-trends?) presents shows that that the reform affected long-run poverty rates. The first fully exposed cohort (born in 1991) is nearly 2% less likely to be part of a conditional cash transfer program following the reform.

5.4 Effects of the 1998 reform on labor market outcomes

Finally, Figure 10 shows that the first fully exposed cohort (born in 1991) has wages 5% higher (how does that translate to present value? what's the benefit-cost comparison for affected individuals?) as a result of the reform when they grew up in a city that was positively treated by FUNDEF vs. in an unaffected city. These effects are statistically and economically significant, but lower, however, than in (Jackson et al., 2016) in which a 20% increase in per pupil spending is associated with 14% higher wages.

6 Conclusion

In 1998, Brazil implemented a major reform to increase the funding of elementary schools and equalize it across municipalities. Using linked administrative data and a new panel of

municipality accounts combined with Treasury data and the annual census of education, we study the long-run educational and labor market outcomes of this reform. The poorest and most affected municipalities saw an immediate increase of 20% in their educational spending, and a close to 10% increase in their overall spending. Comparing new cohorts of students (affected by the reform) vs. older cohorts across more or less affected municipalities, we find a gradual increase in the number of years of schooling, an increase in high school and college graduation rates, as well as a rise in wages in the formal sector (+5%). These results highlight the critical role of education funding policies in raising wage levels and shaping the wage distribution in middle-income countries.

In a future version of this paper, we plan on better understanding the heterogeneity of the nature of education spending across municipalities and contribute to the literature on optimal input spending. We're also in the process of evaluating the effects of the reform on test scores. Finally, as the micro-files of 2022 Census are released, we'll be able to study the effects of the reform on the probability of being employed, and the probability of being employed in different sectors (formal vs. informal).

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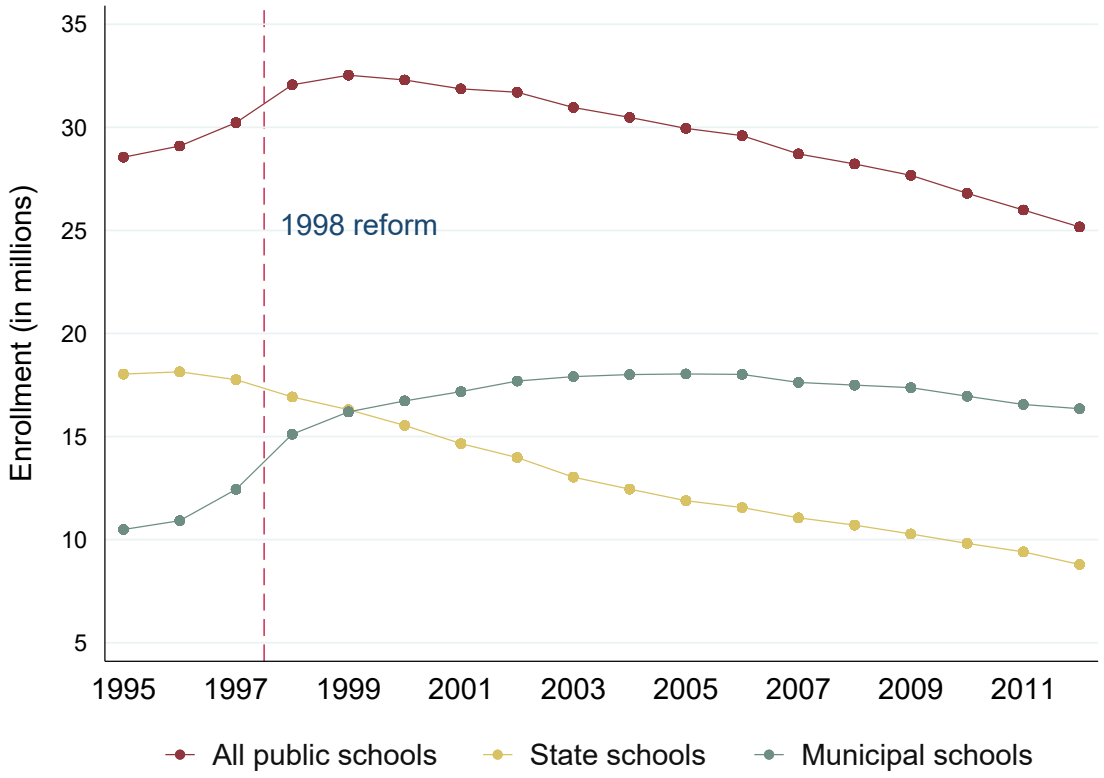
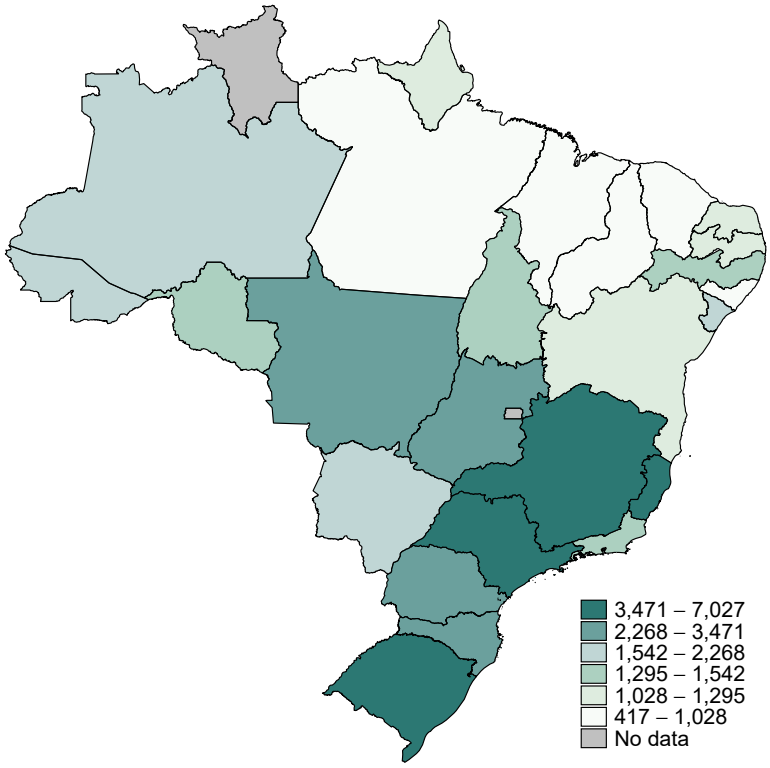
Figure 1: Total enrollment in public elementary education



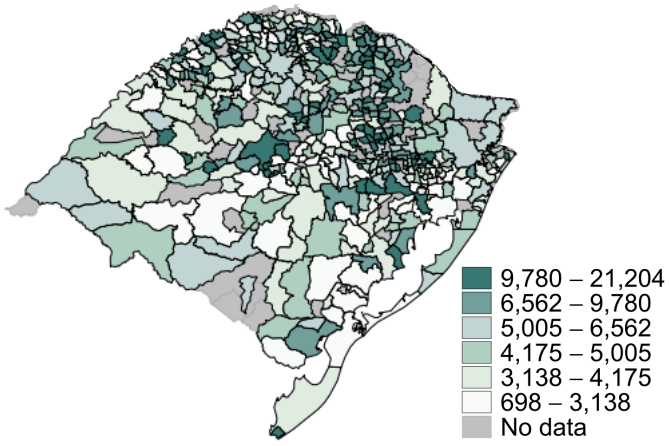
Source: Census of Education.
Note:

Figure 2: Inequities in per-pupil spending across and within states, 1997

(a) Across states



(b) Within Rio Grande do Sul, across municipalities

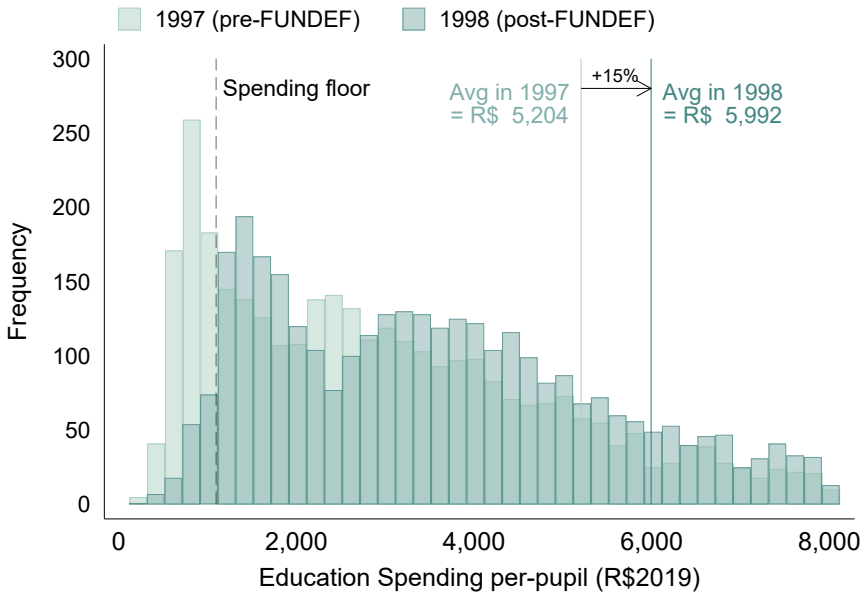


Sources: Municipality accounts combined with Census of Education, 1997.

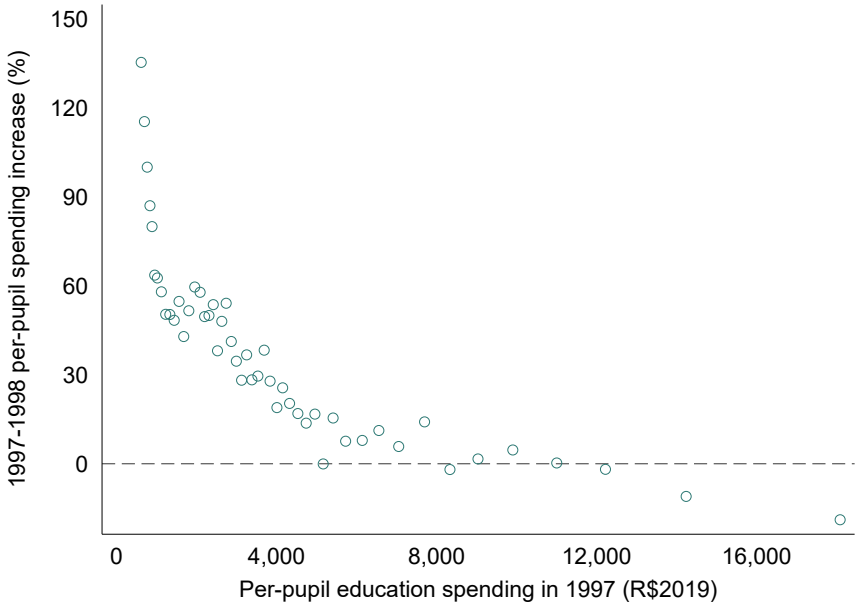
Note: spending is calculated at the municipality level using municipality accounts, and therefore refers to spending in municipal schools. It is reported here in constant 2019 R$, deflated using the INPC series. Because the municipality accounts data do not provide a breakdown between spending in preschool and in primary education, spending here encompasses both spending in preschools and primary schools. In panel (a) spending at the municipality level is then aggregated at the state-level. Per-pupil spending by state is obtained by dividing the aggregated municipal spending in education by the total number of students enrolled in municipal schools in that state.

Figure 3: Changes in 1997-1998 education spending at the municipality level

(a) Distribution of per-pupil spending across municipalities in 1997 and 1998



(b) 1997-1998 per-pupil spending increase across municipalities against 1997 initial levels

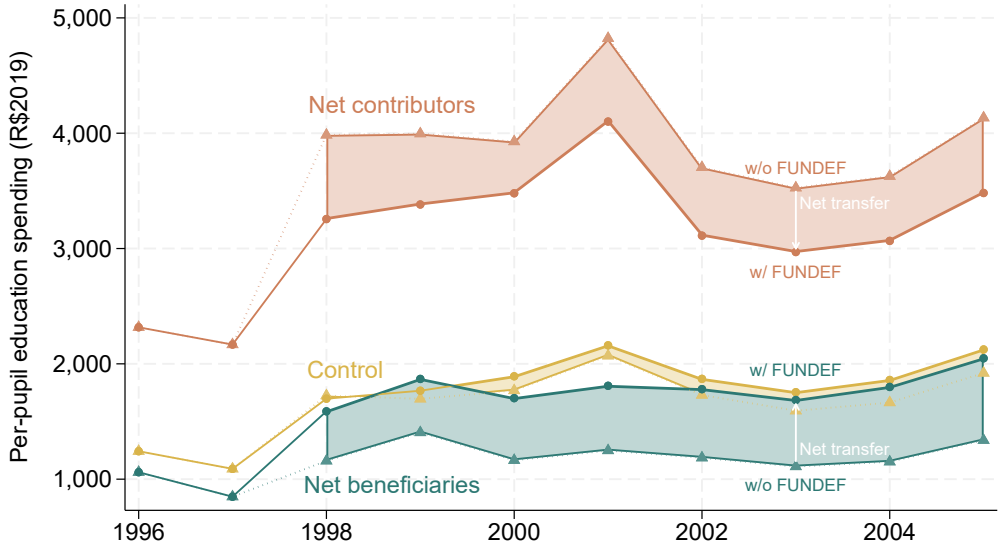


Sources: Municipality accounts combined with Census of Education, 1997.

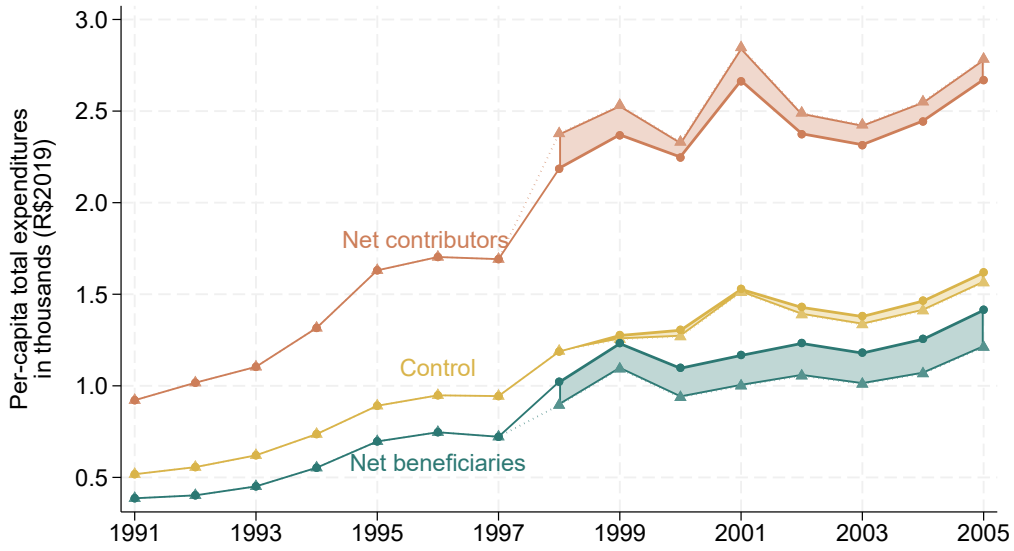
Note: spending is calculated at the municipality level using municipality accounts, and therefore refers to spending in municipal schools. It is reported here in constant 2019 R$, deflated using the INPC series. Because the municipality accounts data do not provide a breakdown between spending in preschool and in primary education, spending here encompasses both spending in preschools and primary schools.

Figure 4: Changes in municipalities' education and total spending following the 1998 reform

(a) Changes in education spending



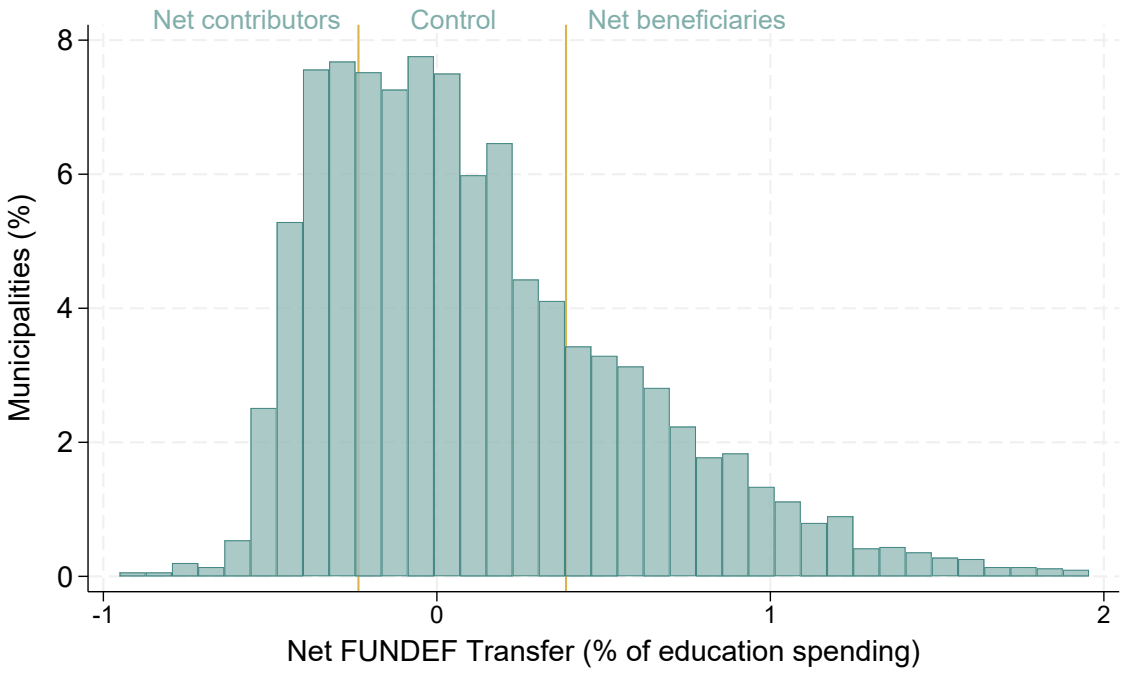
(b) Changes in total expenditures



Sources: Our new panel of municipalities' finances (see section 3) combining FINBRA, Treasury data, Census of Education, newly digitized data on FUNDEF allocation keys.

Note: spending is calculated at the municipality level using municipality accounts, and therefore refers to spending in municipal schools. It is reported here in constant 2019 R$, deflated using the INPC series. Because the municipality accounts data do not provide a breakdown between spending in preschool and in primary education, spending here encompasses both spending in preschools and primary schools.

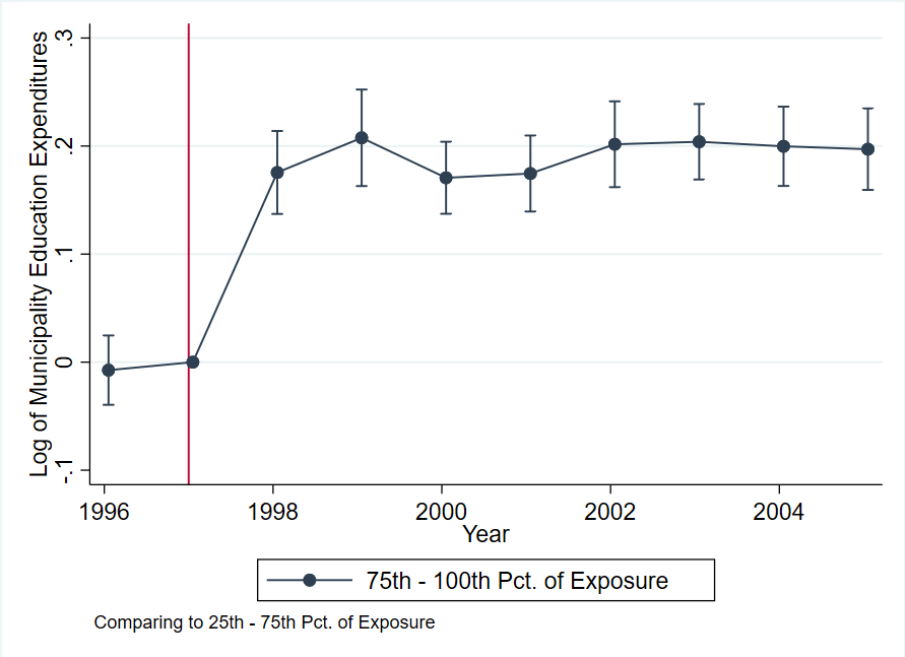
Figure 5: Distribution of our baseline municipalities' dosage measure



Source: Our new panel of municipalities' finances (see Section 3) combining FINBRA, Treasury data, Census of Education, newly digitized data on FUNDEF allocation keys.

Note: Net beneficiaries municipalities are that are situated in the top quartile of the distribution of the municipalities' dosage measure; net neutral municipalities (or control municipalities) are those that are comprised between the 1st and the 3rd quartile of the distribution of the municipalities' dosage measure; net contributors are those that are in the bottom quartile of the distribution of the municipalities' dosage measure. See Subsection 4.1 for more details.

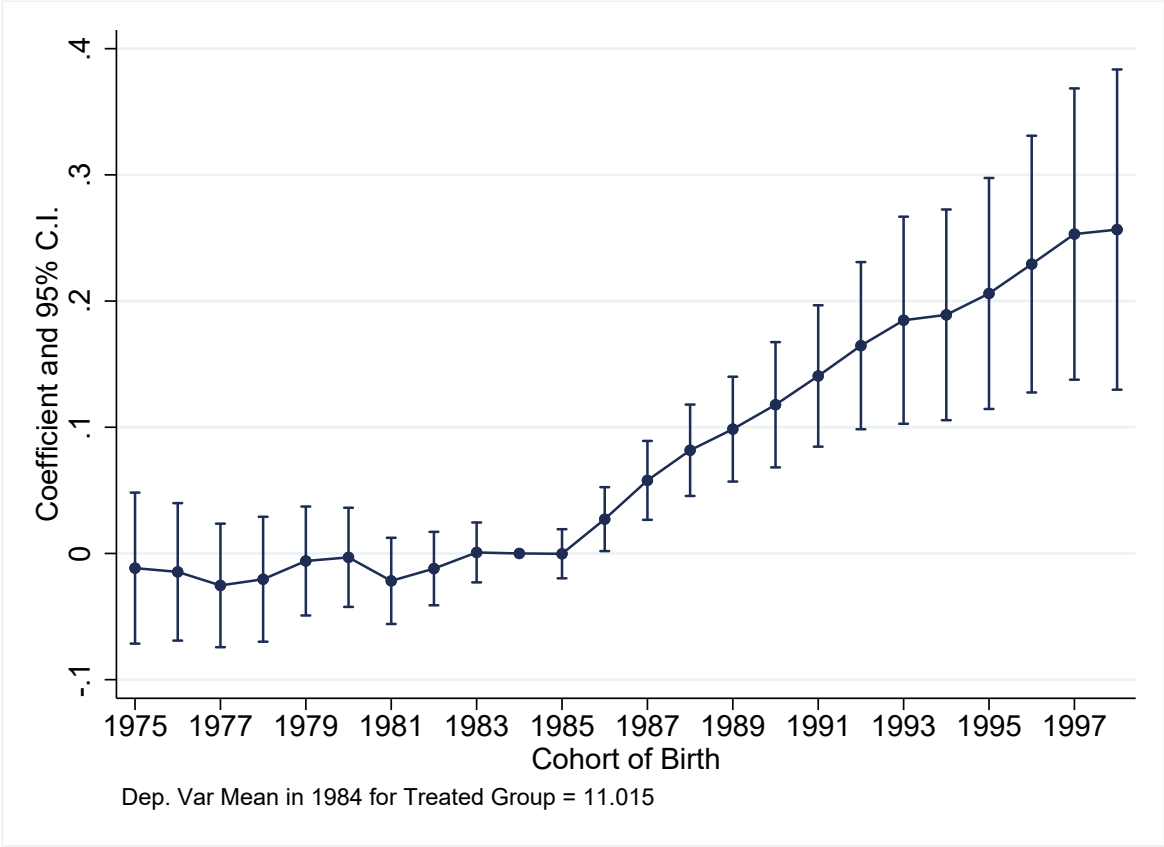
Figure 6: Effect of the 1998 school funding reform on municipalities' education spending



Source: Our new panel of municipalities' finances (see Section 3) combining FINBRA, Treasury data, Census of Education, newly digitized data on FUNDEF allocation keys.

Note: Net beneficiaries municipalities are that are situated in the top quartile of the distribution of the municipalities' dosage measure; net neutral municipalities (or control municipalities) are those that are comprised between the 1st and the 3rd quartile of the distribution of the municipalities' dosage measure; net contributors are those that are in the bottom quartile of the distribution of the municipalities' dosage measure. See Subsection 4.1 for more details.

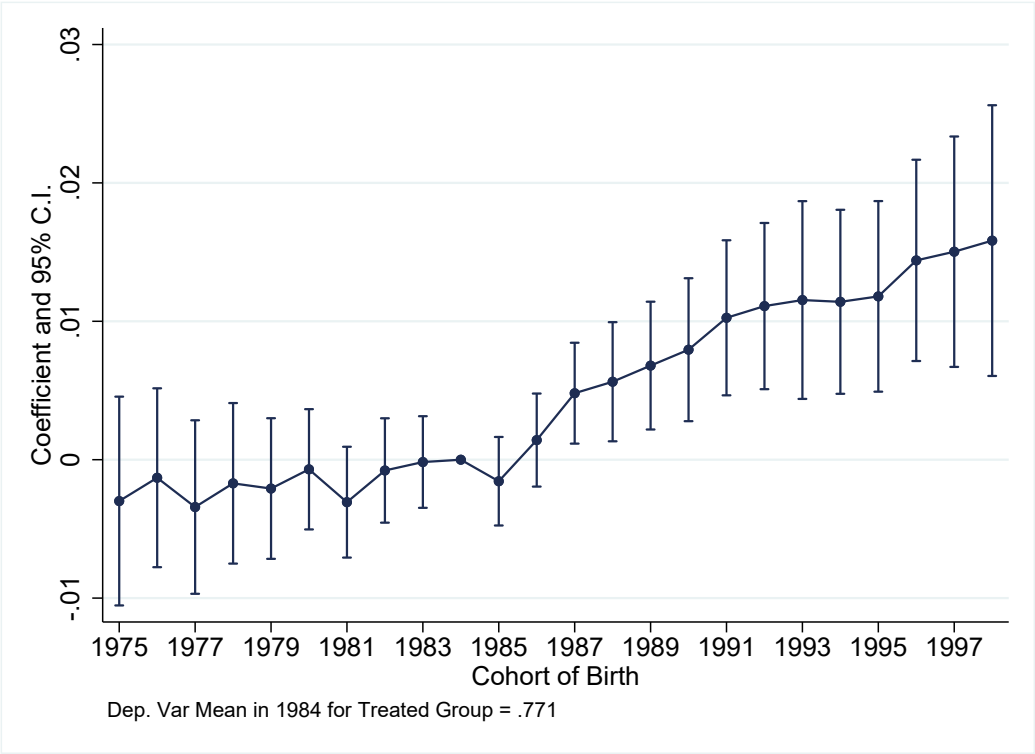
Figure 7: Effect of the 1998 school funding reform on number of years of schooling



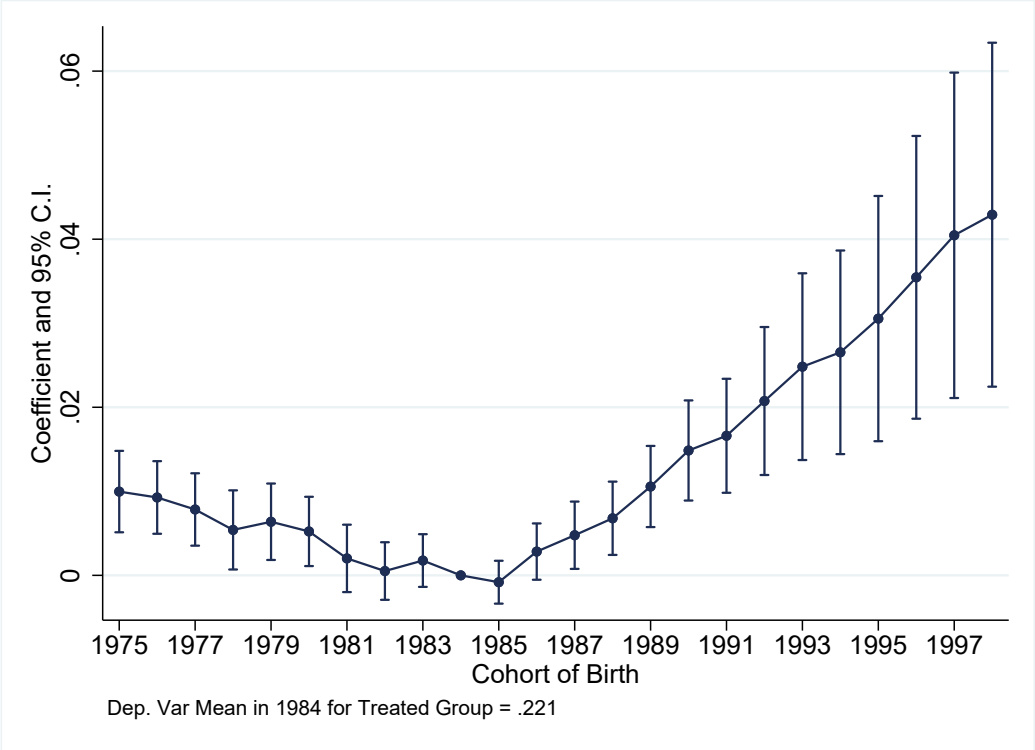
Sources: Matched employer-employee data (2019) matched to our new panel of municipalities' finances (see section 3).

Figure 8: Effect of the 1998 school funding reform on probability to complete high school and college

(a) Effect on probability to complete high school

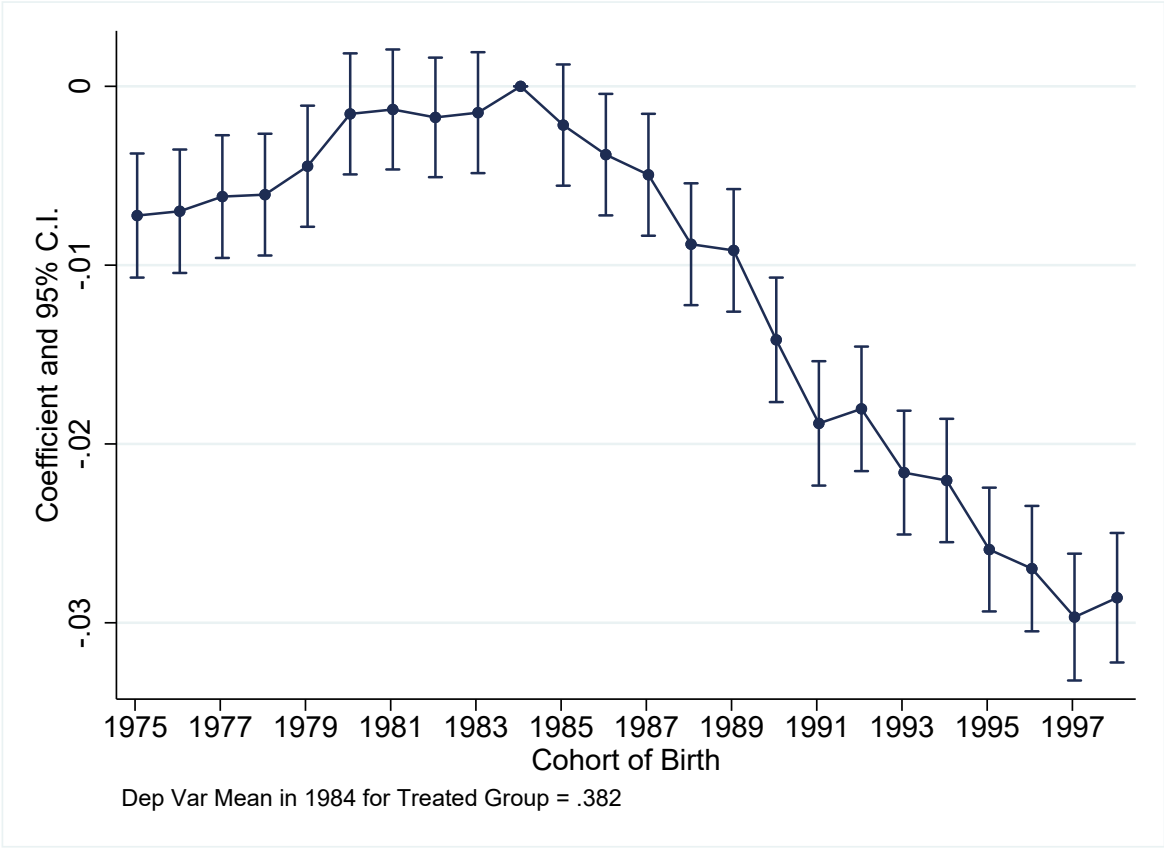


(b) Effect on probability to complete college



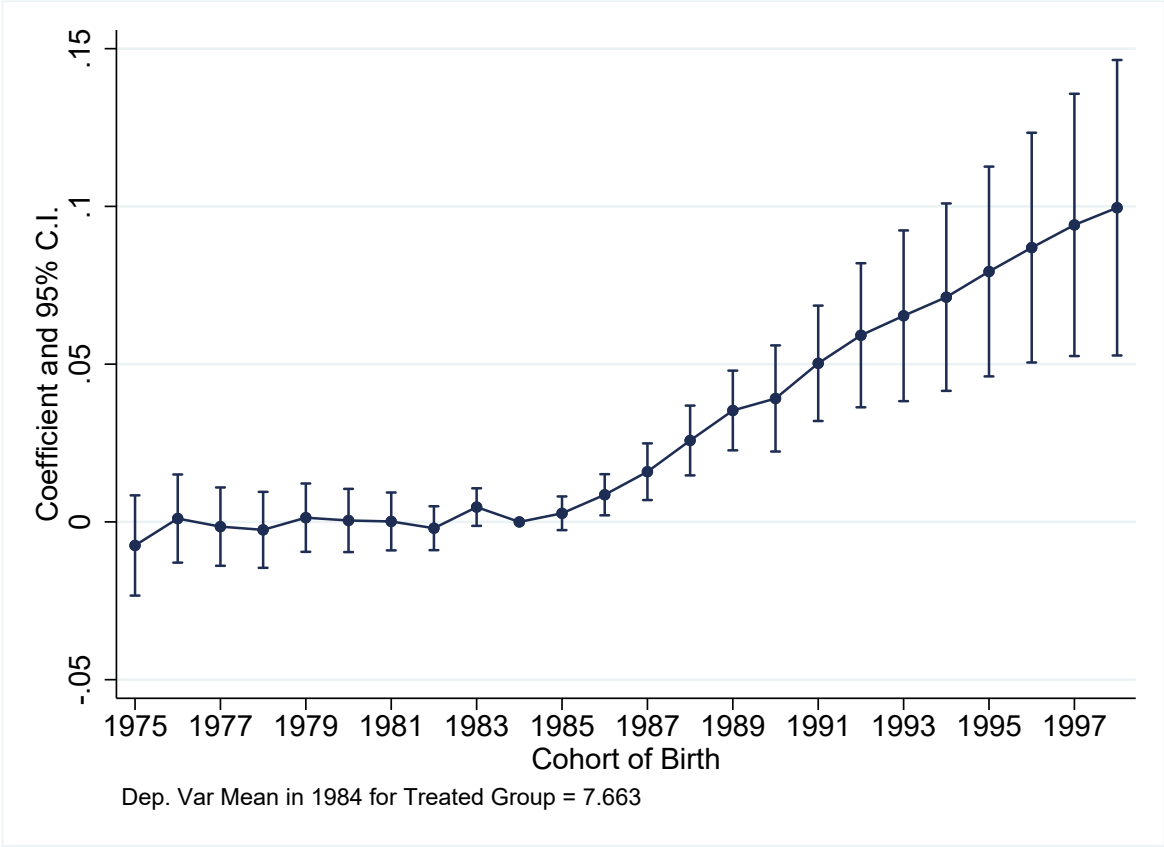
Sources: Matched employer-employee data (2019) matched to our new panel of municipalities' finances (see section 3).

Figure 9: Effect of the 1998 school funding reform on probability to receive a conditional cash transfer (Bolsa Família)



Sources: Matched employer-employee data (2019) matched to our new panel of municipalities' finances (see section 3).

Figure 10: Effect of the 1998 school funding reform on log wages in the formal sector



Sources: Matched employer-employee data (2019) matched to our new panel of municipalities' finances (see section 3).

Table 1: Descriptive statistics on Brazilian elementary system, 1997

	All i.e. ages 7-14	Level 1 i.e. ages 7-10	Level 2 i.e. ages 11-14
Elementary school			
Number of students (millions)	34.23	20.57	13.67
In public schools (%)	0.89	0.91	0.88
Municipal schools (%)	*0.36*	*0.48*	*0.19*
State schools (%)	*0.53*	*0.43*	*0.68*
Federal schools (%)	*0.00*	*0.00*	*0.00*
In private schools (%)	0.11	0.10	0.13

Source: Census of Education, 1997.

Sample: All children enrolled in level 1 or level 2 of primary school.

Notes: Even though primary education is meant to enroll students aged 7 to 14 years old, many children – especially in the 1990s – were enrolled in primary education at later ages. These children are included in this table – no matter how old they are, provided that they are enrolled in primary education.

Table 2: Balance table across our treatment and control groups of municipalities, 1997

	All cities	Net contributors	Control	Net beneficiaries
Population				
Estimated population from IPEA	37,720	15,374	37,296	62,315
Number of kids of elementary school age	6,420	2,464	6,359	10,737
Elementary school system (grades 1-8)				
Total number of students	7,706	2,914	7,706	12,788
Median number of students	2,545	816	2,520	4,326
Share in municipal schools	0.342	0.181	0.301	0.582
Share in state schools	0.627	0.796	0.661	0.377
Share in federal schools	0.004	0.000	0.004	0.006
Share in private schools	0.059	0.066	0.057	0.057
Elementary school system (grades 1-4)				
Total number of students	4,359	1,560	4,238	7,563
Median number of students	1,607	444	1,486	3,010
Share in municipal schools	0.476	0.290	0.449	0.712
Share in state schools	0.496	0.686	0.514	0.253
Share in federal schools	0.007	0.000	0.006	0.008
Share in private schools	0.054	0.066	0.054	0.049
Elementary school system (grades 4-8)				
Total number of students	3,348	1,354	3,468	5,225
Median number of students	887	371	996	1,185
Share in municipal schools	0.135	0.045	0.089	0.312
Share in state schools	0.823	0.931	0.867	0.621
Share in federal schools	0.001	0.001	0.001	0.001
Share in private schools	0.075	0.068	0.067	0.093
Region				
North	0.019	0.034	0.003	0.038
Northeast	0.212	0.139	0.083	0.549
Southeast	0.392	0.229	0.590	0.129
South	0.291	0.392	0.283	0.224
Midwest	0.086	0.206	0.041	0.059
Municipality Accounts				
Total expenditures (R$2019)	44,446,410	65,506,009	14,594,176	85,413,878
Per-capita expenditures (R$2019)	1,177	1,372	1,255	784
Total education spending (R$2019)	10,557,499	15,801,993	3,928,713	19,096,687
Total revenues (R$2019)	51,429,068	74,761,223	16,206,544	101246784
Per-capita revenues (R$2019)	1,323	1,561	1,413	855
Total ICMS revenues (R$2019)	15,024,828	26,725,748	4,960,073	24,261,320
First Year FUNDEF (1998)				
Contributions to FUNDEF	3,489,478	5,634,937	1,590,753	5,298,543
Transfers from FUNDEF	4,118,191	3,920,615	1,074,291	10,628,493
Net FUNDEF Transfer	916,697	-1,150,632	-421,538	5,374,482
Net Transfer (% of education spending)	0.117	0.104	-0.077	0.492
Observations	3,077	757	1,514	758

Source: Our new panel of municipalities' finances (see Section 3) combining FINBRA, Treasury data, Census of Education, newly digitized data on FUNDEF allocation keys.
Note: For a definition of the municipalities that are net contributors, net beneficiaries and those that received net neutral transfers, see Subsection 4.1.

Appendix A FUNDEF: context, contributions and transfers

A.1 Context

The 1998 school funding reform was voted in a context of large inequities across and within states. Appendix Figure shows the large inequities at play across states, and complement the map provided in Figure A1.

A.2 Contributions to FUNDEF

States and municipalities had to contribute a fraction of their tax revenues to the "FUNDEF" state-level funds created by the 1998 school funding reform. To do so, the revenue side of states and municipalities was decomposed into contributing and non-contributing revenues. Appendix Figure A2 shows that on average across all municipalities, the contributing revenues were representing 62% of all tax revenues. This figure also shows wide heterogeneity in this share across municipalities.

Federal government also had to contribute to the state-level s fund in each year t if the total amount of contributions per pupil was below a certain threshold.

Overall, the resources contained in each state-level fund are:

$$\text{Fund}_{st} = \text{Federal Top up}_{st} + \text{State Contributions}_{st} + \sum_{k \in \mathcal{S}_s} \text{Munic Contributions}_{kt}$$

A.3 Transfers from FUNDEF

All the contributions from the municipal, state and federal governments are the redistributed back to municipal and state governments, according to a formula that is proportional to enrollment in the respective municipal and state schools.

Municipalities received FUNDEF transfers from state s in year t according to the following formula:

$$\text{Transfer}_{mt} = \gamma_{mt} \cdot \text{Fund}_{s(m)t}; \quad \gamma_{mt} = \frac{\text{MSE}_{mt}}{\sum_{k \in \mathcal{S}_{s(m)}} \text{MSE}_{kt} + \sum_{k \in \mathcal{S}_{s(m)}} \text{SSE}_{kt}}$$

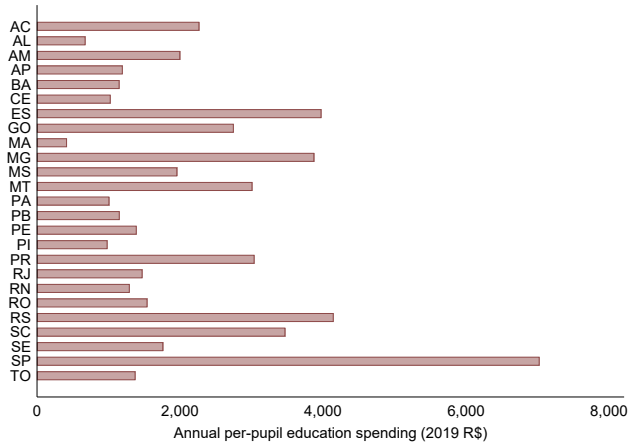
States received FUNDEF transfers from state s in year t according to the following formula:

$$\text{Transfer}_{st} = \gamma_{st} \cdot \text{Fund}_{st}; \quad \gamma_{st} = \frac{\sum_{k \in \mathcal{S}_s} \text{SSE}_{kt}}{\sum_{k \in \mathcal{S}_s} \text{MSE}_{kt} + \sum_{k \in \mathcal{S}_s} \text{SSE}_{kt}}$$

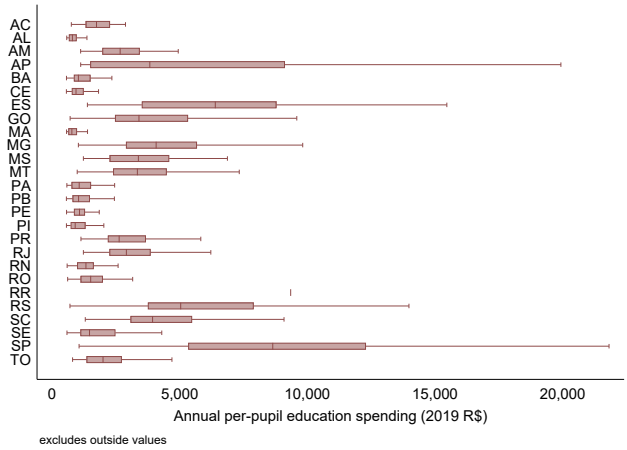
With MSE_{mt} being municipal school enrollment in municipality m, SSE_{mt} state school enrollment in municipality m, $s(m)$ the state to which municipality m belongs, and $\mathcal{S}_s$ the set

Figure A1: Large inequities in states' revenues per capita (2019 R$), 1997

(a) Histogram



(b) Boxplot

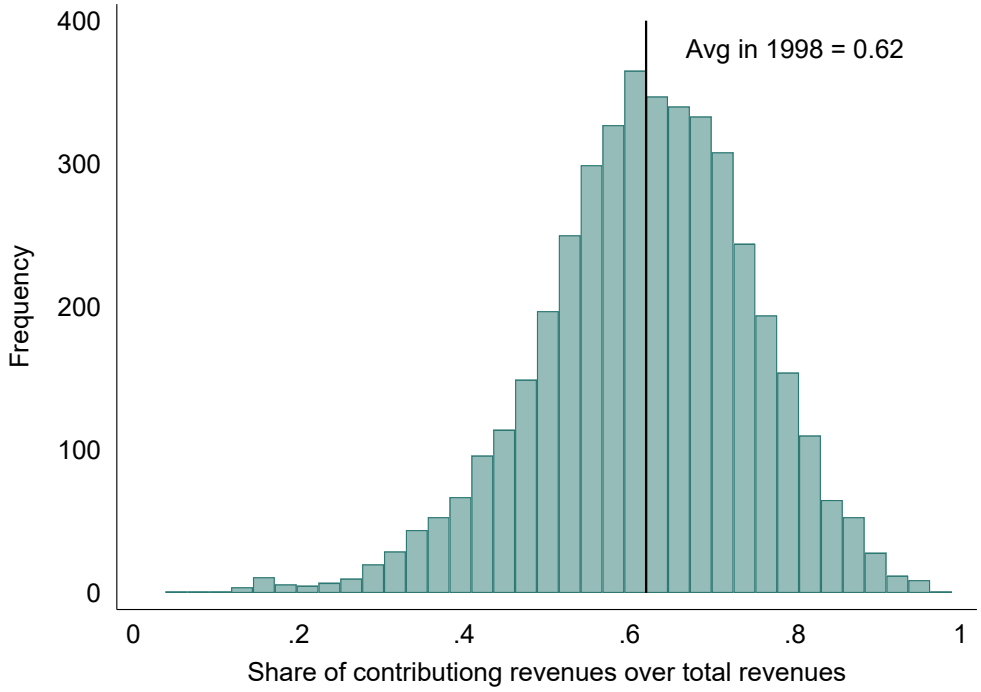


Sources: Municipality accounts combined with Census of Education, 1997.
Note: spending is calculated at the municipality level using municipality accounts, and therefore refers to spending in municipal schools. It is reported here in constant 2019 R$, deflated using the INPC series. Because the municipality accounts data do not provide a breakdown between spending in preschool and in primary education, spending here encompasses both spending in preschools and primary schools. In both panels, spending at the municipality level is then aggregated at the state-level. Per-pupil spending by state is obtained by dividing the aggregated municipal spending in education by the total number of students enrolled in municipal schools in that state.

of municipalities in state s. We refer to γ_{mt} and γ_{st} as the FUNDEF coefficients for municipality m and state s in year t.

In 1998 and 1999, the redistribution is directly proportional to the share of students enrolled in grades 1 to 8 in the municipality over the total number of students enrolled in grades 1 to 8 at the state level. This formula is slightly amended in 2000 and for the subsequent

Figure A2: Distribution of the share of contributing revenues in total revenues across municipalities, 1998



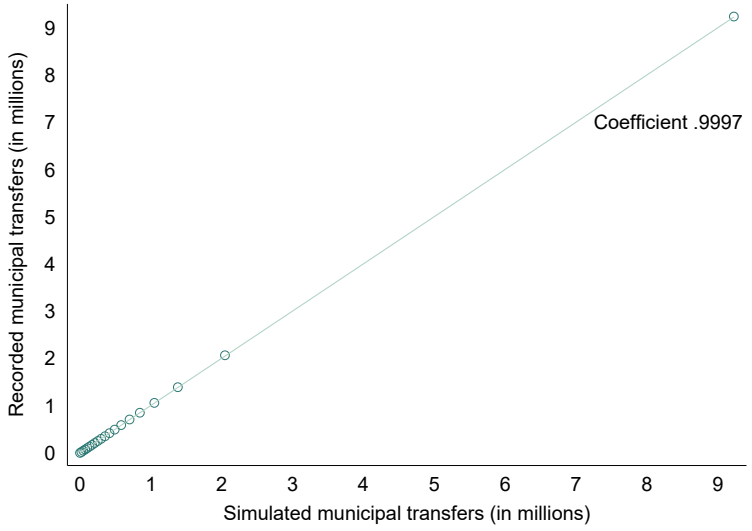
Sources: Municipality accounts, 1998.

years to directly take into account the number of students enrolled in special education.

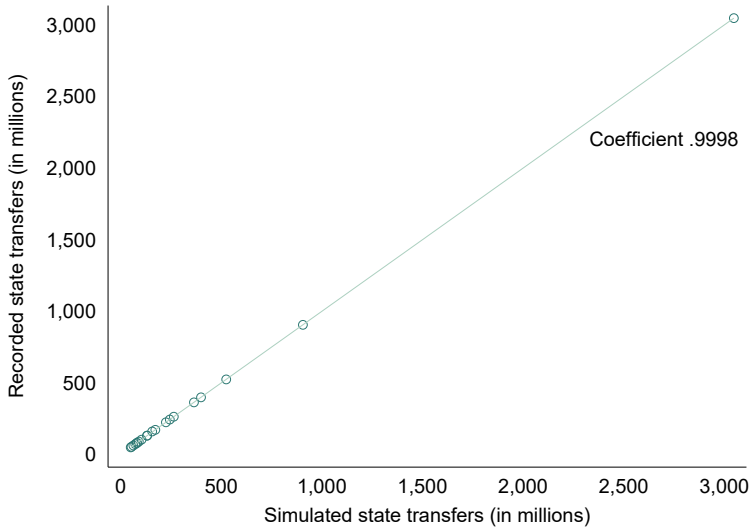
As shown on Appendix Figure A3, we cross-checked that the transfers reported in our data perfectly match the simulated amounts obtained by using the formulas at the state and municipality levels.

Figure A3: Actual vs. simulated FUNDEF transfers, 1998

(a) Transfers to municipalities



(b) Transfers to states



Sources: Municipality accounts combined with Treasury data on FUNDEF transfers, 1998.
Note: The transfer amounts at the state or municipality levels are reported in millions R$, in constant 2019 R$, deflated using the INPC series.

Appendix B Data sources

B.1 Municipality accounts database 1989-2012

Content. Municipalities' financial records are reported in a dataset called FINBRA. This dataset contains information on revenues, expenditures, and transfers involving each municipality. We use it both to assess how much money is spent on education, but also to manually calculate FUNDEF contributions using tax revenue data. Furthermore, it allows us to observe FUNDEF transfers directly during the years in which the policy was in place. It contains data from 1989 all the way up to today but we only constructed it until 2012. It becomes more and more detailed throughout time starting to disentangle money spent on the different levels of education (Preschool, EF, and High School) in 2002. It also includes how contributions to FUNDEF are actually being made and from which taxes they are coming from starting in 2002. These make it possible for us to test if initial assumptions we have of the data prove to be true in later years. For instance, we are able to verify if most of the money municipalities spend on education goes to EF education which we assumed to be true up until 2002 and verify that to be the case in later year.

The portion of text below was removed: why? "Starting in 1996, these accounts allow us to disentangle the budget allocated to wages versus school infrastructure. We combine the municipality financial accounts with linked employer-employee data (described below) to identify the number of teachers hired and their wage bill, further enabling us to decompose the wage bill between teachers and staff."It was written by someone before me and I do not believe it is true which is why I removed it.

Access, and documentation. Access to this data and its documentation is available in the Brazilian treasury website here.

Data processing. Report here any data imputations we made in the master data, assumptions used, etc. Data used to cross check our calculations etc.

Descriptives. Report here any relevant descriptive statistics using this dataset that would give confidence in the data construction process. ie show e.g. any results / graphs of our cross checks.

B.2 Treasury Transfers database (1997-2012)

Content The Brazilian Treasury has another dataset in which it reports all of the constitutionally mandated transfers made to states and municipalities. It contains the values for FUNDEF, FPE, FPM, IPI-exp and LC 87/96, transfers for every municipality in the country starting in 1997 and going up to 2020. We choose to only process it up to 2012 for the time being. This Treasury data is by far the most complete data source we have access to containing essentially all municipalities in the country every single year which expands our analysis to the universe of municipalities in the country. It also allows us to run consistency checks for these specific transfers comparing them to the values present in FINBRA. The values are extremely consistent accross datasets which enables us to complete wrongly reported zeros or missing values commonly present in FINBRA data.

(please describe precisely the cross-checks that have been conducted. Please also include the results/graphs of these cross checks here).

Access. The data and its documentation can be accessed here.

Data processing.

Descriptives.

B.3 Institute of Applied Economic Research (IPEA) data 1990-2012

Content The Brazilian Institute of Applied Economic Research compiles data from several different government sources throughout the years. From it we are able to gather important demographic and public finances information. More precisely, we use their population estimates for each municipality in a given year which later allows us to create variables such as total spending per capita. Moreover, the public finances data serves as an additional consistency check for the values present in FINBRA and the Treasury Transfers data. Fortunately the values are extremely consistent across the three sources. More importantly, the IPEA data contains information on education spending for more years than FINBRA does allowing us to go further back in time and have longer pre-trends in our event study graphs.

Access. http://ipeadata.gov.br

B.4 Census of Education Add years for which the dataset has been constructed).:

Content. The Census of Education is published annually by the National Institute Educational Studies and Research ("Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira") (INEP). This institute depends on the Ministry of education, and is in charge of evaluation educational systems as well as the quality of education in Brazil. From 1995-2005, it contains enrollment records at the school-by-grade level. Starting in (2005 the data is available at the individual level. Starting in 2009, the data can be merged at the individual level using a personal identifier with the administrative data.

Remains to be processed. Only data on childhood education (?), EF1 and EF2 enrollment has been processed so far. High school enrollment has not.

Access. The micro-files of the census of education are available from 1995 to 2023 here.

B.5 Brazilian census Add years for which the dataset has been constructed).:

Content Performed every 10 years by the Brazilian Institute of Geography and Statistics (IBGE), the Brazilian census contains valuable information on the universe of the brazilian population. We have access to the 1991, 2000, 2010, and 2022 (soon to be released) population census. These include individual-level data, including gender, race, educational attainment, labor market outcomes, and migration profiles, among several other demographics. Moreover, for a representative sample of the population a larger survey is performed giving even more insight into those individuals. With the census we are able to evaluate long term outcomes of the reform. needs completing. What will we use it for in the end?

B.6 FUNDEF allocation coefficients (1998-2006)

Content The Brazilian Ministry of Education produced yearly tables on the allocation coefficients used to redistribute the FUNDEF funding. As described previously in section A.3, these coefficients depend on the enrollment in each municipality both on municipal and state schools, varying by municipality and year. These tables were published in the official journal of the Federal Government (Diário Oficial da União), and through an arduous procedure we were able to locate, digitize, and process them. Later we were able to merge them to our other datasets using municipalities' names and their unique identifier IBGE codes. By doing

that we verified using the enrollment data from the Census of Education that the reported coefficients truly matched the artificial ones we created by hand. We also verified that they produced very consistent transfer values by multiplying the coefficients with the total value of the state-level fund and comparing it to the values present in FINBRA and the Treasury Transfers data.

B.7 Ministry of Education FUNDEF report (1998-2001)

Content In 2002 the Brazilian Ministry of Education produced a report on FUNDEF in which it made available the total amount of the fund for each state in each year. This coupled with the allocation coefficients allow us to create a synthetic version of the FUNDEF transfers using only the policy's formulas. The synthetic transfers produced this way are extremely consistent with the transfer present in FINBRA and the Treasury Transfers data. Furthermore, the report contains information on how much each state contributed, and how much each state received from FUNDEF. With this info we are able to evaluate whether states are loosers or winners from the reform in a given year. Lastly, the report tells us how much the federal government had to top off in every single year, an information not available in any other datasource.

B.8 SAEB and ENEM Add years for which the dataset has been constructed). Please fo two separate paragraphs on SAEB and ENEM:

Content We utilize two different standardized tests to measure student learning. The first is SAEB, which started beeing applied in public schools in 1995 and evaluates the proficiency of students mainly in mathematics, and portuguese, but also other subjects. These subjects change throughout the years but the two main ones appear in all versions of the test. This test is administered every two years to 4th and 8th graders. While it started in 1995, comprehensive coverage of all public schools began in 2003. The second test is ENEM, primarily designed as a high school graduation assessment. It started in 1998, mainly covering public high schools. In 2009, ENEM underwent significant changes, expanding its role to also serve as a university entrance examination. This dataset includes not only the scores from each section of the exam but also responses to an extensive questionnaire about the students' socioeconomic backgrounds. With it we are able to gather a lot of information on students and alco match them to their high schools in the Census of Education helping us understand where they went to school and if they were affected by FUNDEF.

B.9 Annual Assessment of Social Information (RAIS) Add years for which the dataset has been constructed). :

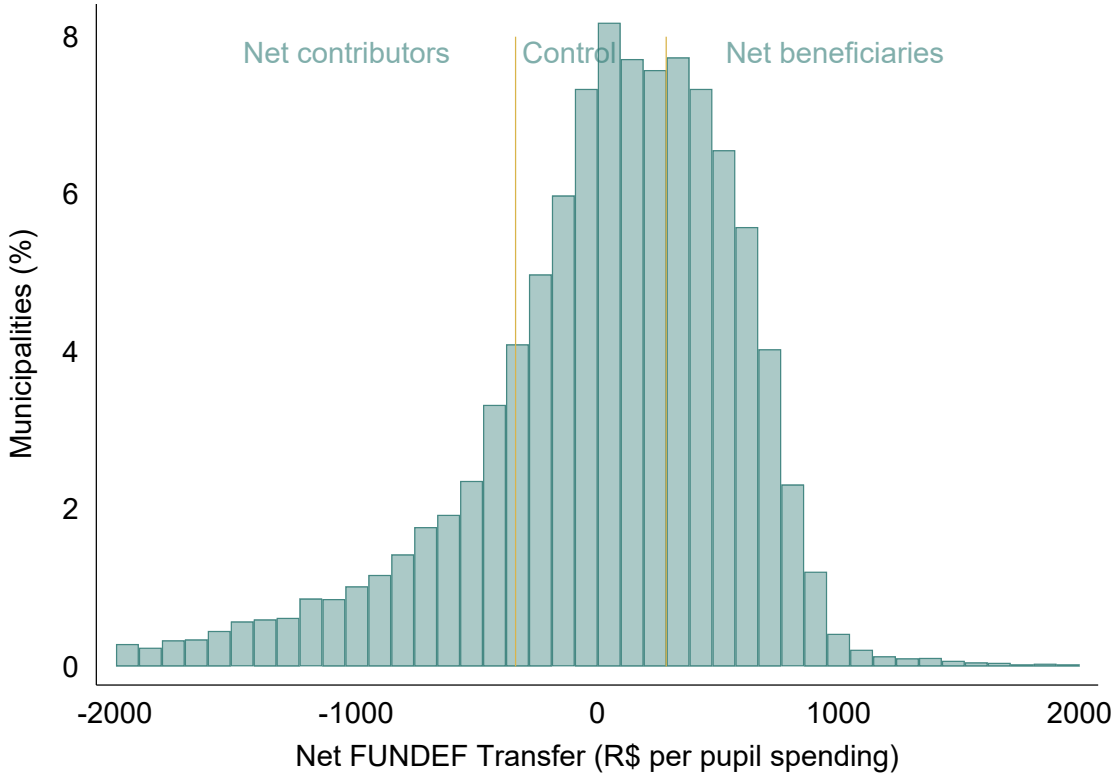
Content RAIS is a restricted access dataset from the Brazilian Ministry of Labor that contains matched employer-emplyee administrative records. This is considered to be a high-quality census of the Brazilian formal labor market. It also includes information on gender, race, and educational attainment. The data is available from 1985-2021. We use these data to measure the employment and wages of individuals in the formal sector.

B.10 Registry for social programs Add years for which the dataset has been constructed). :

Content This dataset provides a comprehensive mapping of low-income families, encompassing approximately 35% of the total population. It specifically targets families whose monthly income per person does not exceed half the minimum wage. We employ this dataset to devise poverty metrics and to pinpoint individuals who are beneficiaries of the conditional cash transfer program. Additionally, it contains information on reported income, which we utilize to supplement the data on formal income reported by individuals. It also includes information on gender, race, and educational attainment. We have this data starting in 2012.

Appendix B Additional details on municipalities' dosage measure

Figure B1: Distribution of alternative dosage measure to the reform at the municipality level



Source: Our new panel of municipalities' finances (see Section 3) combining FINBRA, Treasury data, Census of Education, newly digitized data on FUNDEF allocation keys.
Note: For a definition of the municipalities that are net contributors, net beneficiaries and those that received net neutral transfers, see Subsection 4.1.

Table B1: Balance table across our treatment and control groups of municipalities using alternative dosage measure, 1997

	All cities	Net contributors	Control	Net beneficiaries
Population				
Estimated population from IPEA	37,720	54,664	13,601	71,051
Number of kids of elementary school age	6,420	9,177	2,325	12,200
Elementary school system (grades 1-8)				
Total number of students	7,706	11,136	2,787	14,529
Median number of students	2,640	2,274	1,613	5,057
Share in municipal schools	0.627	0.709	0.697	0.390
Share in state schools	0.342	0.239	0.284	0.561
Share in federal schools	0.004	0.002	0.009	0.005
Share in private schools	0.059	0.078	0.042	0.063
Elementary school system (grades 1-4)				
Total number of students	4,359	5,999	1,569	8,528
Median number of students	1,680	1,315	923	3,557
Share in municipal schools	0.476	0.352	0.441	0.671
Share in state schools	0.496	0.597	0.542	0.286
Share in federal schools	0.007	0.001	0.015	0.006
Share in private schools	0.054	0.077	0.039	0.054
Elementary school system (grades 5-8)				
Total number of students	3,348	5,138	1,218	6,001
Median number of students	920	933	647	1,452
Share in municipal schools	0.135	0.081	0.072	0.310
Share in state schools	0.823	0.865	0.904	0.614
Share in federal schools	0.001	0.002	0.000	0.001
Share in private schools	0.075	0.081	0.051	0.097
Region				
North	0.019	0.034	0.003	0.038
Northeast	0.212	0.139	0.083	0.549
Southeast	0.392	0.229	0.590	0.129
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Midwest	0.086	0.206	0.041	0.059
Municipality Accounts				
Total expenditures (R$2019)	44,446,410	65,506,009	14,594,176	85,413,878
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hspace0.5cmPer-capita revenues (R$2019)	1,323	1,561	1,413	855
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First Year FUNDEF (1998)				
Contributions to FUNDEF	3,489,478	5,634,937	1,590,753	5,298,543
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Net FUNDEF Transfer	916,697	-1,150,632	-421,538	5,374,482
Net Transfer (% of education spending)	0.117	0.104	-0.077	0.492
Observations	3,077	757	1,514	758

Source: Our new panel of municipalities' finances (see Section 3) combining FINBRA, Treasury data, Census of Education, newly digitized data on FUNDEF allocation keys.

Note: Net beneficiaries municipalities are that are situated in the top quartile of the distribution of the municipalities' dosage measure; net neutral municipalities (or control municipalities) are those that are comprised between the 1st and the 3rd quartile of the distribution of the municipalities' dosage measure; net contributors are those that are in the bottom quartile of the distribution of the municipalities' dosage measure. See Subsection 4.1 for more details.