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Public spending and pupils' achievement: Evidence from public elementary schools in Brazil

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Abstract

This paper tests whether an increase in per-pupil spending can increase pupils' achievement in municipal schools in Brazil. This relationship is known to be difficult to unravel because the two variables are confounded by multiple factors associated with the municipalities, the pupil, and their family. We follow over 600 thousand individuals longitudinally to estimate the effects of per-pupil spending on math test scores controlling for pupils' fixed effect. We also estimate effects on the probability of concluding elementary education in at most 11 years. On both specifications, we use only the fraction of municipal spending associated with Fundeb transfers, a state-level fund that redistributes revenue earmarked for education to municipalities, which the municipal government have limited room to interfere. Our results show a pattern of positive effects of per-pupil spending on pupils' achievement in both of the analysed dimensions. Heterogeneity analysis suggests lower effects for well-off children and higher for black, brown, or indigenous children. These results have important implications for educational policies aiming to increase educational quality and equity. Money seems to matter for educational quality in a middle-income country.

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Introduction

Economic theory emphasizes education as a driver of productivity, innovation, and social mobility (SCHULTZ, 1961; LUCAS, 1988; NELSON; PHELPS, 1966; AGHION et al., 2009), which points to the question of how to improve educational results. The relationship between public educational spending and the achievement of pupils has long been debated. However, empirical findings on whether additional resources improve learning remain mixed.

The debate on whether school resources improve pupil achievement has been shaped largely by US evidence, from the Coleman Report (COLEMAN, 1968) and Hanushek's early reviews (HANUSHEK, 1986, 2003) to more recent quasi-experimental designs showing consistent positive effects of finance reforms and resource shocks (JACKSON; JOHNSON; PERSICO, 2016; BARON, 2022; JACKSON; MACKEVICIUS, 2023). Although this literature helped settle the question that 'money matters' in the US context, its external validity for middle- and low-income countries remains limited, as educational systems differ substantially in institutional capacity and resource allocation levels.

This paper examines the effect of per-pupil spending on achievement in Brazilian municipal elementary schools. Brazil is a large middle-income federation with more than 5,000 municipalities, where the public sector is the main provider of primary education. Exploiting rich pupil-level longitudinal data, we contribute to this literature by: (i) estimating the effect of spending on standardized test scores (SAEB) using pupil fixed-effects; (ii) considering the effect of spending on a novel indicator that follows pupils over 11 years of elementary education and evaluate if hey concluded elementary education.

Evidence from developing countries is scarcer and more fragmented. Randomized controlled trials typically test discrete inputs, such as textbooks or equipment, often resulting in null effects, indicating that these inputs do not affect the daily experience of pupils (GLEWWE; KREMER; MOULIN, 2009; GANIMIAN; MURNANE, 2016). However, isolated variations in specific inputs do not reflect well what happens when educational spending increases in real life. Broader spending reforms are harder to study due to data constraints and endogeneity of finance decisions.

In Brazil, where municipalities are the main providers of basic education, existing studies using municipal panels or resource windfalls (e.g., Fundef/Fundeb reforms, oil royalties) find mixed or weak effects on test scores, although some positive impacts on promotion and achievement (AMARAL, 2011; HADDAD; FREGUGLIA; GOMES, 2016; SIMIELLI; ZOGHBI, 2017; MONTEIRO, 2015; CHAN; KARIM, 2023). These limitations highlight the need for longitudinal evidence at the pupil level that can credibly identify causal impacts of spending in developing contexts. This paper contributes to that gap by exploiting exogenous Fundeb variation and rich Brazilian microdata to estimate spending effects on a sample of more than 600 thousand pupils distributed across the entire

Brazilian territory.

To address endogeneity, we use only the portion of spending financed by Fundeb, a state-level fund that redistributes earmarked tax revenues across municipalities. Because local governments have limited discretion over Fundeb allocations, this provides plausibly exogenous variation in per-pupil spending. Furthermore, we show that the result is robust to an IV specification, which isolates an arguably exogenous component of the Fundeb funding formula. Finally, we consider the effects of municipal educational spending on students from state-administered schools, which should not be affected by municipal spending. The results are null in this test, pointing out that the positive estimates are driven by municipal educational spending and not by other correlated shocks occurring in the same municipality.

Our findings diverge from those of the rest of the Brazilian literature, which has tended to report null or negligible effects. We document a clear pattern of positive impacts for both text scores and the probability of finishing elementary education, with some specifications pointing to higher effects for low socioeconomic status and black, brown or indigenous students.

These results imply three contributions. First, school resources do matter for educational outcomes in a middle-income setting. Second, we find suggestive evidence that money is more effective in increasing achievement among historically disadvantaged groups. Third, focusing solely on test scores underestimates the broader benefits of spending, as longer-term educational trajectories, associated with socioemotional development, also respond to increases in spending.

The remainder of the paper is structured as follows. Section 2 discusses the Brazilian context; Section 3 describes data; Section 4 outlines the empirical strategy; Section 5 presents results and discussion.

1 Public Education in Brazil since the Federal Constitution of 1988

In Brazil, the Federal Constitution of 1988 and the Basic Guidelines for Education Act (LDB, the acronym in Portuguese) of 1996 delegate to municipal and state governments the responsibility to provide free public education. The constitution establishes that municipalities and state must spend 25% of their revenue on "maintenance and development of education"[1] (or MDE). While ensuring

[1]MDE is a legal definition introduced by the LDB and includes eight different types of spending: remuneration and training of teaching staff and other education professionals; acquisition, maintenance, construction and conservation of facilities and equipment necessary for teaching; use and maintenance of goods and services linked to education; statistical surveys, studies and research aimed primarily at improving the quality and expanding education; carrying out supporting activities necessary for the functioning of education systems; granting scholarships to students from public and private schools; amortization and financing of credit operations intended to comply with the provisions of the paragraphs of this article; acquisition of teaching materials and maintenance of school transport programs.

that a comprehensive share of municipal and education budgets is devoted to public education, the law allows substantial inequality of educational spending across municipalities and states, because the level of spending is determined by their own tax collecting capacity, which differs widely from state to state and from municipality to municipality.

Fostering educational equity, the law 11,494 9,524 of 2007 created the Fundeb (Fund for Maintenance and Development of Basic Education and Teachers). The law established a fund for each of the 26 states and one for the Federal District, amounting to a total of 27 funds. Each municipal and state government contributes with 20% a bundle of its own tax revenues to this fund, and receives a share of the fund equivalent to the number of enrollments, weighted according to the grade and the type of school[2].

The federal government also contributes to the funds with an amount that corresponds to 10% of the total sum of the 27 combined funds. Those additional federal resources are distributed to the funds with the lower value per pupil in a iterative way. First it transfers resources to the state with the lowest per-pupil value until it equalizes the level of the state with second lowest value. After that another round of equalization is carried out, now transferring money to the states with the first and second lowest per-pupil values until they match the value of the third lowest. That is done until the available resources are depleted. Thereby Fundeb increases the national minimum per-pupil spending value. Of the total amount of Fundeb received by a municipality or state government, 60% must be spent on active teachers' salaries, while the remaining resources from Fundeb and also from other sources, must be spent on MDE.

Municipalities are mandated to spend at least 25% of their revenue on education, considering the Fundeb transfers they received. To do that, they must spend the whole transfer received from Fundeb, an additional 5% of the tax and transfers linked to Fundeb, as well as 25% of the remaining revenuesit may have. As a result, some municipalities offset part of the educational spending after receiving the Fundeb transfers, which result in flypaper effect (CRUZ; SILVA, 2020). In practice the identified flypaper effect shows that they are reducing what would be spent outside of Fundeb.

2 Data

This paper uses data from multiple sources to characterize both the educational outcomes of the pupils and the financing of municipal public education. In this section, the data are described in two parts: pupil-level and municipal-level data.

[2]The weights should reflect different costs of education relating to grade, type of education (regular, special, vocational etc.), length of the school day (part-time or full-time school schedule), and locality (rural or urban). However, they are not based on a precise cost assessment.

2.1 Pupil-level data

The empirical strategy explores the richness of Brazilian educational data from the Instituto Nacional de Estudos e Pesquisas Educacionais (INEP), an autarchy linked to the Ministry of Education. Basic education in Brazil has two main data sources: SAEB and School Census. Those two data sets were used, respectively, to measure pupils' performance in standardized tests at the end of primary and middle school and to measure the quality of their trajectory at elementary and middle school. Both were used with pupil identifiers, allowing us to employ longitudinal econometric analysis.

2.1.1 SAEB: The Brazilian System of Basic Education Evaluation

SAEB is the Brazilian System of Basic Education evaluation[3]. It biannually applies multiple standardized exams to the entire population of public schools. In this paper, we focus on the mathematics and reading exams embedded in SAEB, applied to measure pupil proficiency at the end of primary school (fifth grade) and middle school (ninth grade). The exam is a multiple choice test and the test score is obtained through the use of item response theory, making it comparable across years. SAEB also has questionnaires answered by the pupil that covers information such as characteristics of the pupil, household, as well as parents' attitudes towards the pupil's education.

Exploring the fact that SAEB is applied every two years in both fifth and ninth grades, we restricted the sample to those pupils that where evaluated by both exams. We identify students who attended SAEB‘s fifth-year exam and longitudinally follow them to check if they reappear in the dataset doing SAEB‘s ninth-year exam. The common case would be for students to do the first exam and 4 years later do the second, but some students do them 6 or 8 years after because of irregular educational trajectories. This results in loss of a relevant part of the sample due to attrition. We also drop the observation of pupils that were missing information on the covariates we included in the econometric model.

Limiting the sample to those pupils may raise questions regarding the representativeness of this sample to the Brazilian public-school pupils population, because those pupils that manage to attend both saeb exams may be on the right tail of the distribution of test scores. Although this is a plausible objection, we note that the sample mean of SAEB scores is close to the population mean. However, even if they are not representative of the whole Brazilian population, the exercise is still a valid contribution to understanding the effect of education spending on pupils' test scores. In terms of the trade-off between internal validity and external validity, we exchanged some external validity to get a better identification, since using this limited sample helped us to control for relevant possible confounding factors in the econometric exercise.

Our final SAEB sample has 658.432 pupils, with two observations each, one at the moment

[3] SAEB is an acronym for *Sistema de Avaliação da Educação Básica.*

they passed the fifth-grade SAEB exam and another 4, 6 or 8 years later when they passed the ninth-grade exam. In this sense, we observe 3 cohorts of pupils, defined in the year they passed they fifth-grade exam: the 2011 cohort, the 2013 cohort, and the 2015 cohort. Table 1 presents descriptive statistics from both the sample and the population.

Table 1: Descriptives SAEB Sample and Population

Variable	Sample Mean	Population Mean	Difference
Math test-scores 5th year	224.37 (46.63)	215.83 (48.62)	8.54***
Reading test-scores 5th year	210.4 (46.39)	202.33 (49.49)	8.07***
Socioeconomic Index	5.16 (1.14)	5.12 (1.17)	0.05***
Female	0.55 (0.5)	0.48 (0.5)	0.07***
Black, brown or indigenous	0.57 (0.49)	0.58 (0.49)	-0.01***
Father completed primary education	0.57 (0.49)	0.54 (0.5)	0.03***
Computer at home	0.58 (0.49)	0.56 (0.5)	0.02***
Mother completed primary education	0.6 (0.49)	0.54 (0.5)	0.05***
Lives with both parents	0.74 (0.44)	0.69 (0.46)	0.06***
Retained	0.15 (0.36)	0.29 (0.46)	-0.14***
Parents support pupils education	0.93 (0.26)	0.89 (0.32)	0.04***
Dropped out	0.04 (0.19)	0.09 (0.28)	-0.05***

Source: INEP/MEC

Notes: Standard Deviation in parenthesis. Data from the first observation of the pupils, by the time of the fifth year exam. "Parents support pupils education" is a dummy that equals 1 if the pupil reported, in three different questions, that their parents support him to study at home, to read, and to be present at school.

We are interested in the mean of SAEB scores, but also in how it varies according to socioeconomic status (SES) and race. As a SES measure, we used the SES indicator of Alves, Soares, and Xavier (2014)[4]. This indicator is calculated through an application of item response theory to the pupils' answer of the SAEB socioeconomic questionaire, and is a scalar that varies between 0 and 10, with 10 being the highest socioeconomic status and 0 being the lowest. To identify race, we use the self-identified race question of SAEB.

2.1.2 The Brazilian School Census

The Brazilian School Census is the main instrument to monitor non-tertiary education in Brazil, used for both research and policy purposes [5]. Covers the whole population of Basic Education

[4]This indicator is more commonly used at the school level. However, since in the econometric estimation we are also interested in within-schools inequality, we used it at the pupil-level. It was only possible because Maria Teresa Alves kindly shared it with us at the pupil-level.

[5]The Fundeb rules, described in the Context section, allocate resources to municipalities based on the number of pupils enrolled in public schools, information collected in the School Census.

pupils every year, collecting information about the student, the classes, the teachers, the school principals, and the school infrastructure.

In INEP's restricted data room the pupil-level identified data is available, already in a panel data format, covering 2007 until the most recent released year[6]. This data set also has variables that identify whether the student was approved or retained, evaded, or dropped out of school. This allows researchers to analyze the entire school trajectory of these students, building novel longitudinal indicators.

In this paper, we use the regular trajectory indicator from FONSECA et al. (2024)[7]. The Regular Trajectory Indicator is a categorical variable that classifies pupils into four categories based on the characteristics of their trajectory in elementary education. The building block of the analysis is the computation of two variables: one dummy variable equal to 1 if the pupil enrolled in elementary school with the correct age[8], and a variable that counts the number of *successful school years* the pupil had in the 9-year window of the analysis. A pupil has a *successful school year* if he is approved in year t and is enrolled in the next grade in year t + 1. Based on those two variables, the intercurrencies are counted. An intercurrence can be either an unsuccessful school year or not enrolling in elementary education at the correct age. Every pupil is followed up through 11 years for its trajectory to be categorized.

The four categories are *regular trajectory*, *trajectory with few irregularities*, *irregular trajectory* or *discontinued trajectory*. A pupil is classified with a *regular trajectory* if he does not have any intercurrency in the 9-year trajectory. This is a pupil who enrolled in elementary education at the correct age and, 9 years later, enrolled in secondary education. A pupil is classified with *trajectory with few irregularities* if he has one or two intercurrencies, with a *irregular trajectory* if he had three or more intercurrencies but was still enrolled in the last year of the 9-year window. If he drops out and does not return to school in the 9-year window of the analysis, he is classified with a *discontinued trajectory*. Table describes the 4 categorical values of the indicator.

Based on this categorical variable, we compute a dummy variable to use as the dependent variable in the econometric estimation, which equals 1 if the pupil had regular or a few irregularities trajectories and 0 otherwise. You can interpret this dummy as being equal to 1 if the pupil completed elementary education in at most 11 years, having at most two intercurrencies. Our data set covers 5 entry-year cohorts, defined in the year the pupils enrolled in elementary education: 2007, 2008, 2009, 2010, 2011. The cohorts are followed from 2007 to 2021. Although ideally all pupils would have a regular trajectory, the more broad definition allowing for a few intercurrencies is justified on

[6]By the time we are writing this manuscript, it was 2024. For the project we used data from 2007 until 2020.

[7]They authours calculated the indicator for the 2008, 2009, 2010, and 2011 cohorts, and kindly allowed us to use it in this paper.

[8]In Brazil, children are mandated by law to enroll at elementary school by the age of 6 years.

Table 2: Trajectory Categories

Category	Description	Examples for cohort that enrolled in 2007
Regular Trajectory (Concluded regularly)	Concluded elementary education in 9 years with no intercurrencies.	1. Enrolled with the adequate age or younger and in 2015 got approved at the last year of elementary education.
Trajectory with few irregularities (Delayed conclusion)	Concluded elementary in at most 11 years and had at most two intercurrencies.	2.1. Enrolled with adequate age, was retained in 2012 but managed to succesfully conclude in 2016. 2.2. Enrolled one year late and concluded in 2016. 2.3. Enrolled one year late, was retained in 2012, but managed to succesfully conclude in 2016
Irregular Trajectory (Did not conclude)	Didn't conclude elementary education 11 years after starting it	3. Enrolled but didn't conclude it by 2017.
Discontinued Trajectory	Enrolled in elementary education, but disappeared from the dataset	4.1. Enrolled but dropped out in 2010 and never enrolled again.

the ground that having an irregular or discontinued trajectory is a relevant marker of educational deprivation.

After dropping students who had missing in the covariates used in the econometric analysis, we end with a sample of 907,623 students. Table 3 shows the descriptive statistics for each group. The highest share of the sample had a regular trajectory or a trajectory with few irregularities, but around 13% of the sample did not complete elementary education in the 11-year period. It also shows that better trajectories are associated with higher socioeconomic status and a lower share of black brown or indigenous students. In addition, it is more common for them to live with their parents, not be working, and have a computer at home. Lastly, their parents are both more present at home and have higher educational attainment, measured by having a primary education degree.

Table 3: Descriptives by Trajectory Category

	Regular Trajectory	Trajectory with few irregularities	Irregular Trajectory	Discontinued Trajectory
Pupils #	488955	292451	113109	13108
Share	0.54	0.32	0.12	0.01
Socioeconomics Index	5.33 (1.17)	5.11 (1.22)	4.89 (1.27)	5.02 (1.29)
Women	0.55 (0.5)	0.45 (0.5)	0.36 (0.48)	0.53 (0.5)
Black, brown or indigenous	0.55 (0.5)	0.63 (0.48)	0.72 (0.45)	0.65 (0.48)
Pupil also works	0.1 (0.3)	0.17 (0.38)	0.27 (0.44)	0.21 (0.41)
Retained	0.09 (0.28)	0.43 (0.5)	0.63 (0.48)	0.32 (0.47)
Dropped out	0.03 (0.16)	0.08 (0.27)	0.18 (0.38)	0.12 (0.33)
Lives with both parents	0.74 (0.44)	0.68 (0.46)	0.62 (0.49)	0.65 (0.48)
Mother completed primary education	0.62 (0.49)	0.52 (0.5)	0.43 (0.5)	0.48 (0.5)
Dad completed primary education	0.6 (0.49)	0.54 (0.5)	0.46 (0.5)	0.5 (0.5)
Parents support pupils education	0.94 (0.23)	0.89 (0.31)	0.81 (0.39)	0.85 (0.36)
Computer at home	0.62 (0.23)	0.55 (0.31)	0.48 (0.39)	0.51 (0.36)

2.2 Municipal Data

2.2.1 Public finance data

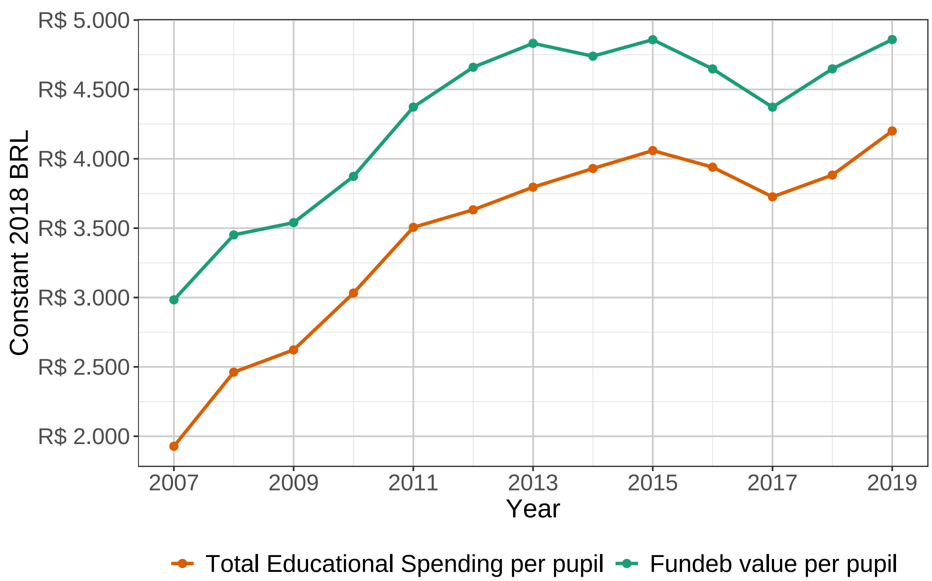
Brazil is a federation of 5,570 municipalities in 26 states and one federal district. Fiscal information from all levels of government is reported to the National Treasury's SICONFI system, which is publicly available through the FINBRA database.[9]

FINBRA provides detailed data on municipal revenues and expenditures, including education spending. While it distinguishes between kindergarten and elementary/middle school, this breakdown is noisy because many schools cover multiple stages and reporting practices vary.[10] We therefore use total municipal basic education spending, although our analysis focuses on elementary education.

The data set also includes transfers from Fundeb, which we use in the econometric analysis, as well as state-level Fundeb resources for an instrumental variable specification (see methodology). To compute per-pupil spending, we combine FINBRA with enrollment data from the School Census. We also use FINBRA to track other constitutional transfers, such as oil royalties.

We deflated the data to 2018 values using IPCA[11]. Figure 1 presents the evolution of the total municipal expenditure per pupil and the total value of Fundeb per pupil. From it we can see that both indicators follow a similar positive trend, with Fundeb being approximately 1000 BRL lower than total spending. There is a negative dip from 2015 to 2017 driven by the economic crisis Brazil had in those years, which also points to the fact that educational funding in Brazil is procyclical.

Figure 1: Education spending over time



Our empirical strategy uses two different datasets, which have reasonable overlap. For the test

[9]SICONFI stands for *Sistema de Informações Contábeis e Fiscais do Setor Público Brasileiro* (Accounting and Fiscal Information System of the Brazilian Public Sector). FINBRA stands for *Finanças do Brasil* (Finance of Brazil).

[10]The difficulty arises because the same school often serves kindergarten, primary, and middle levels.

[11]IPCA is the inflation index most commonly used in Brazil. Calculated by IBGE, it is based on the retail price of a representative set of goods and services consumed by Brazilian families with monthly earnings between 1 and 40 minimum salaries.

score analysis, we build a panel of SAEB pupils, which result in losing part of the sample because we require that the pupil take the exam in both fifth and ninth year, which is often not the case. For the school trajectory analysis, we use data from Censo Escolar which cover all students, but we lose a part of the sample because we cross it with SAEB to access the socioeconomic questionaire. Also, for the Censo Escolar, we use a 9-year in the econometric analysis. In table 4 we present the mean and standard deviation of the two analytical samples we use, as well as for the entire population. It shows that the SAEB sample have slightly higher mean spending Fundeb value per pupil, and that the trajectory sample value is lower for both. However, the trajectory sample is not directly comparable since it is a 9-year mean taken from 2015 to 2019.

Table 4: Per-pupil spending for population and analysed samples

Group	Total Educational Spending per pupil	Fundeb value per pupil
Population	4602.97 (1665.79)	3875.47 (647.69)
SAEB sample	4751.81 (1664.05)	3943.17 (633.77)
Trajectory sample	4427.42 (792.27)	3592.72 (587.86)

Source: SICONFI-FINBRA/National Treasury

2.2.2 Additional municipal data

Additional municipal data was obtained at IBGE, the Brazilian official statistic institute, and DATASUS, a dataset linked to the Brazilian public health system. From IBGE, we obtained municipal population estimates for all analyzed years, as well as GDP per capita data and the sectoral composition of municipalities. From DATASUS, we collected municipal populational estimates by age group.

3 Empirical Strategy

3.1 Test score model

To estimate the effect of municipal education spending on pupils' math and reading test scores, we use a two-way fixed effects model, exploiting within-pupil variation in test scores and exposure to spending. Spending is likely endogenous, since municipalities that invest more may also differ in unobserved ways. For example, a mayor who prioritizes education could both raise spending and introduce quality-improving policies unrelated to spending, such as appointing qualified staff to the education office.

To avoid those endogeneity channels we use only the fraction of educational spending financed by Fundeb transfers, which the municipal government cannot directly influence. Even though

Cruz and Silva (2020) estimates 30% elasticity of Fundeb transfers to educational spending, this missing 70% comes from the fact that municipalities reduce other sources of educational spending financed by other sources, meaning that even though Fundeb transfers are not integrally converted into educational spending, municipalities total educational spending is never lower than the amount received thorugh Fundeb. Therefore, the amount of money received through Fundeb is a good proxy for municipal educational spending. We consider a regression model with the following specification:

$$TS_{i,t} = C_i + \delta_t + \beta \ln(Fundeb_{m(i),t}) + M'_{m(i),t}\phi + P'_{i,t}\gamma + \epsilon_{i,t} \tag{1}$$

i and t index the pupil and the year she attended the exam. $TS_{i,t}$ is either math or reading test scores, measured by fifth and ninth-year SAEB exam; C_i and δ_i are pupil and time fixed effects; $\ln(Fundeb)_{m(i),t}$ is the natural log of the value of Fundeb transfer received by the municipality divided by the number of enrolled pupils in municipal elementary education; $M_{m(i),t}$ is a vector of municipal level control variables: the log of the estimated municipal population to control for demographic patterns that could influence both Fundeb transfers and pupils results, log of revenue per capita to control for the fiscal capacity of the municipality which could be correlated to its efficiency in obtaining educational results as well as the general level of economic activity, log of total enrollment in the municipality and the log of total enrollment on public pre-schools to control for any possible enrollment trend that could bias our identification strategy; $P_{i,t}$ is a vector of pupil-level time-varying control variables: whether the child has a computer at home, whether the child repeated a grade, whether the child has ever dropped out off school; $\epsilon_{i,t}$ is an error term, which we cluster at the municipal level.

β is the populational parameter of interest, representing the average response of test score to a variation in the Fundeb spending per pupil. β is measuring the effect of a variation in per-pupil spending on the variation of pupils' test scores between the fifth and ninth years exams. We estimate this model by pooling three cohorts of pupils observed in two moments in time each. More specifically, it measures how many additional points in the SAEB scale a 100% variation in per-pupil spending causes. Although the two exams are different, all the pupils in the same cohort are doing the same exams, in a way that the test-score variation is comparable.

Although the Fundeb transfer is more exogenous than the total spending per pupil, it could still violate the exogeneity hypothesis if we consider that the municipal government can strategically enroll students to increase the amount received (KOSEC, 2014), and that this capacity is correlated with the capacity it has to increase the test scores. Another possible source of bias comes from patterns of state economic activity. If a municipality that has a large share of a state's GDP passes through an economic boom, Fundeb transfers may be affected. If this happens, assuming that economic growth has effects on pupil achievement, Fundeb transfers would be biased upward. To protect our identification from those concerns, we implement an instrumental variable (IV) approach exploring Fundeb transfer rules to instrument the amount received per pupil similar to the one used in Cruz and Silva (2020). We estimate by two-stage least squares the following two equations:

$$\ln(Fundeb_{m(i),t}) = C_i + \delta_t + \eta \frac{E_{m(i),t-1}}{E_{s(i),t-1}} + M'_{m(i),t}\beta + P'_{i,t}\gamma + \epsilon_{i,t} \tag{2}$$

$$TS_{i,t} = C_i + \delta_t + \beta \ln(\widehat{Fundeb})_{m(i),t} + M'_{m(i),t}\beta + P'_{i,t}\gamma + \epsilon_{i,t} \tag{3}$$

The instrument used is $\frac{E_{m(i),t-1}}{E_{s(i),t-1}}$, the lagged ratio of municipal school enrollment at the municipal level to the total enrollment of public school at the state level which we will call relative enrollment from now on. Importantly, we additionally control for the log of total enrollment in the municipality and the log of total enrollment on public pre-schools to control for any possible enrollment trend that could bias our identification strategy. We also include party of the mayor fixed effects, controlling for the possibility that mayors form certain parties can get additional resources by connections with the state or federal government, and that those resources are correlated with the lagged relative enrollment. In this model, β measures the effect of a variation in Fundeb per-pupil transfers caused specifically by the variation in the lagged relative enrollment on the variation of pupils' test scores between the fifth- and ninth-year exams.

We estimate two additional models that allow for heterogeneous effects according to race and socioeconomic status (SES). We use the same econometric approach, using both the TWFE and the IV estimators, but including an interaction term of the dependent variable of interest with a racial dummy for the former and a SES indicator for the latter. The SES indicator is calculated by applying the item response theory in the SAEB pupil survey (SOARES; ALVES, 2024) [12]. It is a cardinal variable that varies between 0 and 10, with 10 being the highest SES. $blackindigenous_{i,t}$ is a dummy variable equal to 1 if the pupil self-identifies as black, brown, or indigenous [13].

In the SES heterogeneity model, the coefficient of the interaction term measures how the effect varies for each additional socioeconomic status point, as measured by the SES indicator. In the racial heterogeneity model, the coefficient of interaction measures the difference between the mean effects on pupils who self-identify as black, brown, and indigenous and students who self-identify as white or yellow [14]. In the case of SES heterogeneity, a negative coefficient implies that educational expenditure increases educational equity, that is, reduces the gap between students of different

[12] Kindly shared with us at the pupil-level by Maria Teresa Alves.

[13] Even though we consider indigenous pupils in this dummy variable, they account for a really small fraction of the sample.

[14] In Brazil, the way we collect racial information is based on the notion of color-race. In this system, yellow would be equivalent to Asian. For an overview of the Brazilian system of racial classification check Guimarães (2012)

socioeconomic status. In the case of racial heterogeneity, educational spending increases equity if the coefficient is positive.

For the IV to adequately estimate the local treatment effects, we rely on the usual 2SLS identifying assumption: monotonicity condition and exclusion restriction. The monotonicity condition is self-evident, since the Fundeb resource distribution rules don't have any relation to test scores. The exclusion restriction is stronger, but we also argue that it is satisfying it. It states that there cannot exist any time-varying unobserved variable correlated at the same time with the variation of our instrumental variable and of pupils' achievement, which means that we are assuming that the variations in relative enrollment in the year before are not determined by an effort of the mayor to increase its redistribution transfers in the next year. We argue that, conditional on our control variables and fixed effects, our instrument is exogenous for two main reasons. First, municipalities cannot affect enrollment in the rest of the state, meaning that the denominator of the instrument is exogenous. Second, because a mayor in year t cannot affect its enrollment in the year before and any populational or enrollment trend that could bias the estimate is controlled for with the log of the population, the log of total enrollment, and the log of total enrollment in public pre-schools.

The instrument is also uncorrelated with municipal economic activity that could be affecting both Fundeb transfers and pupils' test scores. One possible source of bias would be if higher-achieving pupils actively sort into municipalities they expect to increase per-pupil spending based on the observation of enrollment in the year before, in which case the exclusion restriction would be violated. However, the restriction of our sample to pupils that attended the exam in the same municipality in the 4-year interval between the fifth and ninth year together with the inclusion of pupil fixed effects control for this.

3.2 Trajectory model

To estimate the effects of municipal public education spending on pupils' achievement measured by the probability that the pupil concludes elementary education in at most 11 years, we use a two-way fixed effects estimator with municipal and cohort fixed effects. Since we only observe each pupil once in this specification, we pool the cross-section of 5 cohorts to estimate the model in a pooled cross-section setting.

$$Concluded_i = \alpha_{m(i)} + \sigma_c + \eta Fundeb_{m(i),c} + C_i' \gamma + \psi_i \tag{4}$$

i and c index the pupil and the cohort. $Concluded_i$ is a dummy variable that equals one if the pupil, 11 years after enrolling in elementary school, managed to conclude elementary education having at most two intercurrencies, as defined in Subsection 2.1.2. $\alpha_{m(i)}$ and σ_c is municipal and cohort fixed effects; $Fundeb_{m(i),c}$ is the 9-year mean of the natural log of the value of the Fundeb transfer received by the municipality divided by the number of students enrolled in the municipal elementary education, considering the first 9[15] years of a given cohort trajectory. We use this treatment indicator to consider spending in each year the pupil was studying at municipal public schools. $C'_{i,c}$ is a vector of pupil and household characteristics; ψ_i is an error term, which we cluster at the municipal level.

The same reasoning for using Fundeb transfers instead of reported educational spending in the test-score model applies here. Also, since we are not able to control for pupil fixed effects, we rely on a larger vector of variables to control for possible endogenous characteristics: whether the pupil self-identifies as a woman; whether the pupil self-identifies as black, brown, or indigenous; the pupil-level SES indicator; whether the pupil lives with his parents; whether the pupil mother completed elementary education; whether the pupil dad completed elementary education; whether he identifies their parents as supportive for his education. At the municipal level, we control for mean GDP per capita and mean net taxes revenue per capita to account for municipal economic activity heterogeneity between municipalities that could confound the estimation.

However, in this setting, we are also vulnerable to the strategic enrollment of students by the municipal government. For this reason, we apply an instrumental variable approach similar to the one used for the test scores model.

$$\ln(Fundeb_{m(i),c}) = \alpha_{m(i)} + \sigma_c + \eta \frac{E_{m(i),c-1}}{E_{s(i),c-1}} + C'_i\gamma + \psi_i \tag{5}$$

$$Concluded_i = \alpha_{m(i)} + \sigma_c + \eta \ln(\widehat{Fundeb}_{m(i),c}) + C'_i\gamma + \psi_i \tag{6}$$

The instrument used is the lagged average relative enrollment. To keep consistency with the endogenous dependent variable, we calculate it by taking the mean of the relative enrollment in the year before of each year considered by each cohort. For example, for the cohort that started elementary education in 2007 in a given municipality, the mean will consider relative enrollment in 2006 all the way to 2014.

The populational parameter of interest is η, measuring the local average effect of the mean exposure of Fundeb educational spending a pupil had in her 9 year trajectory on her probability of concluding elementary education in at most 11 years. That is, the effect of 1% in the mean exposure to educational spending on this probability, measured in percentage points (pp). The estimated effect is local in the sense that it is only identified in relation to the variation in relative enrollment.

Similarly to the test score model, identification relies on a monotonicity condition and exclusion restriction. Monotonicity is valid since our dependent variable measures based on the progression

[15] Although pupils are monitored for up to 11 years, those that manage to conclude in 9 are only observed in the first 9. Therefore, using 11 years would introduce noise in the estimation.

of the pupil on elementary education, which do not affect relative enrollment of the municipality in relation to the state. The exclusion restriction states that there is no unobserved characteristic at the pupil level and no time-varying unobserved characteristic at the municipal level that affects both the variation in mean relative enrollment and in the probability of being coded as 1 in our regular trajectory indicator. Exclusion is harder to argue in the absence of pupil fixed effects, but the inclusion of a set of pupil-level control variables attenuates possible bias regarding the correlation between the distribution of observed characteristics that predict educational attainment and the distribution of Fundeb transfers. Once again, we rely on the assumption that municipalities have limited room to affect relative enrollment and that using the lagged relative enrollment makes it more difficult.

We estimate two additional models that allow for heterogeneous effects by race and SES. The coefficients of the interaction have the same interpretation of the test-score model. A negative coefficient for the SES and/or a positive coefficient for the race interaction implies that educational spending reduces inequality.

4 Results

4.1 Main Results

4.1.1 Test Scores

Table 5 presents the estimated effects of variation in Fundeb transfers on math and reading test scores. Columns 1 and 4 report the results of the estimate of Equation 6 for ninth-grade SAEB scores in math and reading, using municipal fixed effects instead of pupil fixed effects.[16] Columns 2 and 5 show the results of Equation 6 with pupil fixed effects, while columns 3 and 6 present the second stage of the instrumental variable approach (Equation 8), which uses relative enrollment as an instrument for Fundeb transfers.

To aid in interpretation, we consider the sample mean of R$3,943 for Fundeb transfers per pupil, with a standard deviation of R$634. For ninth-year test scores, the sample mean is 262.5 points in math and 261.7 in reading, both with a standard deviation of 47.4. We also adopt the benchmark used in Jackson and Mackevicius (2024), considering an increase of 1,000 international dollars in annual per-pupil funding - equivalent to R$2,197 in the 2018 values using the PPP conversion rate.[17]

Looking first at columns 1 and 4, the estimates without pupil fixed effects indicate that they have no statistically significant effect on ninth-grade SAEB scores in either subject, consistent with

[16]The number of observations in these columns is less than half of those in Column 2, due to the inclusion of time-invariant pupil-level control variables that are missing for some students.

[17]We apply the 2018 PPP conversion rate of 2.197 BRL per dollar.

previous findings by Haddad, Freguglia, and Gomes (2016). Once pupil fixed effects are introduced in columns 2 and 5, the estimated coefficients increase to 11.97 for math and 9.44 for reading, both significant at the 1% level. These results suggest that an increase 1% in Fundeb transfers per pupil is associated with a 0.12-point gain in math and a 0.094-point gain in reading test scores. An increase in a standard deviation in per-pupil transfers (approximately 16% of the mean) is estimated to raise math and reading scores by 0.04 and 0.03 standard deviations, respectively. When translated into the international benchmark of a US$1,000 increase in per pupil funding, these effects correspond to gains of 0.14 and 0.11 standard deviations in math and reading scores. Compared to the global distribution of educational interventions in low- and middle-income countries (EVANS; YUAN, 2022), these impacts place the math effect above the 70th percentile and the reading effect above the 40th percentile.

Turning to the IV-TWFE specification in columns 3 and 6, the estimated coefficients rise to 36.72 for math and 25.89 for reading. These imply that an increase of 1% in Fundeb funding per pupil leads to gains of 0.367 points in math and 0.259 points in reading. A one standard deviation increase in funding corresponds to a 0.13 standard deviation rise in math scores and 0.09 in reading. Under the international benchmark of a US$1,000 per-pupil increase, the effects are even larger: 0.43 and 0.31 standard deviation improvements in math and reading, respectively. These effects are significant when compared internationally, with the math estimate exceeding the 90th percentile and the reading estimate above the 70th percentile of the global distribution of education interventions in comparable settings (EVANS; YUAN, 2022).

Table 5: Effects on math and reading test scores

Outcome:	Math test scores	Math test scores	Math test scores	Reading test scores	Reading test scores	Reading test scores
Estimator:	Municipal TWFE (1)	Pupil TWFE (2)	Pupil IV-TWFE (3)	Municipal TWFE (4)	Pupil TWFE (5)	Pupil IV-TWFE (6)
$\ln(FUNDEB)$	4.76 (3.20)	11.97*** (2.62)	36.72*** (7.13)	1.56 (2.89)	9.44*** (2.06)	25.89*** (5.54)
Fixed-Effects:						
Municipality	Yes	No	No	Yes	No	No
Pupil	No	Yes	Yes	No	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Mayor's party	Yes	Yes	Yes	Yes	Yes	Yes
Observations	659,372	1,318,752	1,318,752	659,372	1,318,752	1,318,752
R2	0.22	0.86	0.86	0.18	0.87350	0.87322
Dep. Var. mean	262.49	243.42	243.42	261.72	236.05	236.05

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Municipality) standard-errors in parentheses
Note: This table presents the results of the estimation of equation 6, in columns 1, 2, 4, and 5, and equation 8 in columns 3 and 6. The first stage of the 2SLS estimates are omitted. Column 1 and 4 restricts data only to the ninth-year observation to check how the estimate behaves when municipal fixed effects are controlled for instead of pupil fixed effects. Column 3 and 6 are the results for our preferred specification, using lagged relative enrollment as an instrument for Fundeb transfers.

4.1.2 Pupils school trajectory:

Table 6 presents the results for the effects of the nine-year municipal average of Fundeb per-pupil spending on pupils' probabilities of completing elementary education in at most 11 years. Similarly to the test score model, we present both the results of Equation 9, with municipal and year fixed effects, at column 1 ; as well as from Equation 11, when we instrument Fundeb per-pupil spending by the lagged average relative enrollment[18], in column 2.

Since we estimated a model where the dependent variable is a dummy and the independent variable is a continous indicator log transformed, we can interpret the coefficient as a semi-elasticity that measures how many percentage points a 1% variation in Fundeb increases in the probability of concluding elementary education in at most 11 years. To make the interpretation more tangible, we consider the mean value of the Fundeb average of 9 years of 3,593 and the 588 standard deviation. We also consider the benchmark of 1000 dollars, converted to R$2,197 in 2018 values using the PPP conversion rate.

The results are positive and statistically significant for all specifications. Looking at columns 1 , we see that the TWFE estimator points out that an increase 1% in the nine-year municipal mean of Fundeb per-pupil spending increases the probability of having this type of elementary school completion by 0.18p.p. This means that one standard deviation increase in the nine-year municipal average of Fundeb per-pupil spending would increase this probability by 2.95 p.p., while a 1000 dollar increase would have an 11 and 33.6 p.p. effect.

Considering the IV-TWFE results, column 2 shows that the semi-elasticity is 0.35, respectively. In this case, one standard deviation increase in average funding would increase the probability by 5.73, while the 1000 dollar increase would have an effect of 21.4 p.p..

4.2 Robustness tests:

We estimate 2 additional regressions for both math and reading test scores to test the robustness of our findings. Considering that large municipalities can affect the state-level fund and possibly confound the estimate, we take out of the sample municipalities that represent more than 10% of the state GDP. Estimating similar effects to the baseline specification helps confirm that large municipalities are not endogenously driving the results.

We also run placebo tests by estimating the same model using test scores of pupils enrolled in state-administrated schools. If the test score results are driven by anything else happening at the municipal level besides the increase in Fundeb transfers, such as increased spending in security or health provision, we would expect it to also affect those students. If the results are null for those schools, we have additional evidence that the observed positive effects are driven by something happening at municipal administered schools associated with increased Fundeb transfers.

[18]As discussed in session 4.2, we calculate it by taking the mean of the relative enrollment in the year before of each year considered by each cohort. For example, for the cohort that started elementary education in 2007 in a given municipality, the mean will consider relative enrollment in 2006 all the way to 2014.

Table 6: Effects on pupils trajectory

Outcome:	Concluded Elementary	Concluded Elementary
Estimator:	Municipal TWFE	Municipal IV-TWFE
	(1)	(2)
$\ln(meanFUNDEB)$	0.18*** (0.04)	0.55*** (0.18)
Fixed-Effects:		
Municipality	Yes	Yes
Year	Yes	Yes
Observations	907,623	907,623
R2	0.10	0.10
Dep. Var. mean	0.86	0.86

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Municipality) standard-errors in parentheses
Note: This table presents the results of the estimation of equation 9 in columns 1 and equation 10 in columns 2. The first stages of the 2SLS estimates are omitted. Column 2 is the result for our preferred specification, using the 9-year mean lagged relative enrollment as an instrument for the 9-year mean Fundeb transfers.

Table 7 presents the results of those additional estimates. Columns 1 and 3 show that the result is robust to a sample limited only to municipalities that represent less than 10% of the total state GDP. Columns 2 and 4 show that the result is null when we use the test scores of pupils enrolled at schools administered by the state government. Those results provide evidence in favor of the robustness of our results.

For the results related to the probability that the pupil concludes primary education in at most 11 years, we run the same two tests excluding relatively high GDP municipalities from the sample and using state school students as placebo, as well as an additional placebo test with private school students [19]. Table 8 column 1 shows that the results hold for the limited sample of municipalities, having a significantly higher point estimate. However, the two placebo tests show significant positive effects in 10%, although considerably lower compared to the results of municipal schools.

The positive coefficients in the placebos might be explained by the fact that in this trajectory data set the pupils are classified as municipal state or private school according to which type of school he was for the most years in the 11-year span. In this sense, it could be the case that they

[19] It is not possible to test for those with the test-score dataset because only a small sample of private schools participate in SAEB standardized tests.

were positively affected by increases in Fundeb while they were at municipal school, after which they changed to a state or private school.

Table 7: Robustness tests for math and reading test scores

Outcome:	Math test scores	Math test scores	Reading test scores	Reading test scores
Robustness test:	Limited sample (1)	State schools placebo (2)	Limited sample (3)	State schools placebo (4)
$\ln(FUNDEB)$	40.52*** (8.633)	14.46 (22.64)	29.76*** (6.450)	-3.336 (22.09)
Fixed-Effects:				
Pupil	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Mayor's party	Yes	Yes	Yes	Yes
Observations	964622	715430	964622	715430
R2	0.86	0.85	0.87	0.86
Dep. Var. mean	245.11	250.38	237.49	242.28

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Municipality) standard-errors in parentheses
Note: This table presents results of the 2SLS estimation of equation 8. In columns 1 and 3 we estimate the model restricting the sample to municipalities that represents less than 10% of total state GDP. Columns 2 and 4 presents the placebo estimates considering the outcomes of pupils enrolled at state administered schools.

4.3 Heterogeneity according to SES and race

Figure 9 presents the estimates of the heterogeneous effects of Fundeb transfers per pupil by race and socioeconomic status (SES) on the math and reading test scores. Panel A introduces the interaction between Fundeb and race, while Panel B interacts Fundeb with SES.

First, analyzing the pupil TWFE results, column 1 shows that pupils who self-identify as black, brown, or indigenous benefit more from increased Fundeb transfers compared to their white or yellow peers when math test scores are considered. However, although the coefficient is in the same direction for reading test scores, in column 3, the confidence interval is too large, making the result not statistically significant. For the IV-TWFE the interaction coefficient is too noisy, making it hard to draw any conclusion from it.

Panel B in Column 1 shows SES-based heterogeneity for the pupil TWFE model: the negative interaction term (2.05) implies that lower-SES pupils experience higher gains from increased spending when math test scores are considered as outcome. Once again, results are insignificant for reading test scores as well as for the IV-TWFE models, as seen in columns 2, 3, and 4.

Considering the probability of concluding elementary education in at most 11 years, the Municipal TWFE estimator, in column 1, points that effect is higher for black, brown or indigenous pupils. However, when the IV-TWFE estimator is considered, the interaction sign flips, which is puzzling. Considering SES-based heterogeneity, both estimators do not identify any statistically significant result.

Table 8: Robustness tests for pupils trajectory

Outcome:	Concluded Elementary	Concluded Elementary	Concluded Elementary
Robustness test:	Limited sample (1)	State schools placebo (2)	Private schools placebo (3)
$\ln(meanFUNDEB)$	0.66*** (0.20)	0.21* (0.12)	0.26* (0.14)
Fixed-Effects:			
Municipality	Yes	Yes	Yes
Year	Yes	Yes	Yes
Observations	751,400	705,229	98,938
R2	0.10	0.08	0.08
Dep. Var. mean	0.86	0.93	0.95

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Municipality) standard-errors in parentheses
Note: Note: This table presents results of the 2SLS estimation of equation 10. In column 1 we estimate the model restricting the sample to municipalities that represents less than 10% of total state GDP. Column 2 presents the placebo estimates considering the outcomes of pupils enrolled at state administered schools, while column 3 only considers pupils enrolled in private schools.

Overall the heterogeneity results are inconclusive although they seem to suggest that the effects are heterogenous for different type of students in terms of SES and race.

4.4 Discussion

Our estimates suggest that school resources have positive average effects on pupil achievement, as measured by the within-pupil difference between fifth and ninth-grade SAEB test scores and the probability of the pupil having a qualitatively better trajectory in elementary school.

Compared to the Brazilian literature reviewed in Section 2, where there are mixed results but authors tend to interpret it as pointing out that educational spending and pupils' achievement are unrelated, our results differ by presenting a clear pattern of positive educational spending effects on pupils' achievement. The main difference between this paper and the rest of the Brazilian literature comes from the longitudinal analysis that allows us to include pupils' fixed effects (on the test scores estimation) and rule out composition effects.

Considering an EPF in its structural form helps us to explain why this feature of our empirical analysis may be driving the difference in results. The structural EPF points out that pupils' achievement depends on school and teachers' characteristics, time-invariant characteristics of

Table 9: Effects on math and reading test scores by race and SES

Outcome:	Math test scores	Math test scores	Reading test scores	Reading test scores
Estimator:	Pupil Level TWFE (1)	Pupil Level IV-TWFE (2)	Pupil Level TWFE (3)	Pupil Level IV-TWFE (4)
Panel A:				
$\ln(FUNDEB)$	10.59*** (2.589)	40.14*** (8.17)	8.51*** (2.14)	27.32*** (6.67)
$\ln(FUNDEB) * Race$	2.35** (1.01)	-5.66 (3.83)	1.58 (1.03)	-2.35 (5.56)
Panel B:				
$\ln(FUNDEB)$	22.34*** (5.54)	50.02* (21.38)	12.27** (4.32)	26.67* (13.26)
$\ln(FUNDEB) * SES$	-2.05* (1.09)	-2.67 (3.79)	-0.56 (0.85)	-0.16 (2.22)
Fixed-Effects:				
Pupil	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Mayor's party	Yes	Yes	Yes	Yes
Observations	1,318,752	1,318,752	1,318,752	1,318,752
R2	0.86	0.86	0.873	0.87
Dep. Var. mean	243.42	243.42	236.05	236.05

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Municipality) standard-errors in parentheses
Note: This table presents the results of the estimation of equation 6, in columns 1 and 3, and equation 8 in columns 2 and 4. In panel A we add the interaction of our treatment outcome with racial dummy and panel B adds the interaction with the SES indicator. The first stage of the 2SLS estimates are omitted. Column 2 and 4 are the results for our preferred specification, using lagged relative enrollment as an instrument for Fundeb transfers.

the pupil and their household, pupil achievement on the past and privately provided educational inputs. Educational spending should impact pupil achievement by changing school and teacher characteristics.

The econometric analysis of these articles was performed at the municipal level[20]. Under a municipal level panel, identification comes from the correlation of the spending variation with the variation in pupils' achievement. That is, how the variation in the mean of school and teacher characteristics of municipal schools correlates with the municipal mean of pupils' achievement.

All of those papers estimated models of the reduced-form type, that is, allowing for parental reactions. However, if the author does not control for time-invariant children and household characteristics, and those variables are correlated with both educational achievement and educational spending, this research design would deliver biased estimates of the effect of educational spending on pupils' achievement, since the estimated correlation is confounded by the variation on the municipal mean of pupils and the characteristics of their household.

The municipal-level analysis implicitly assumes that, on the mean, the municipal mean of children and household characteristics of the pupils attending the ninth-year SAEB exam in a given municipality is not varying over time, which is a strong assumption. This is the condition for the interpretation of the inclusion of municipal fixed effects as controlling for those time-invariant characteristics. It is possible that this composition is varying between the analyzed years and that

[20]With the exception of Haddad, Freguglia, and Gomes (2016) that analyzed a panel at the school level. Even though we discuss limitations of the municipal level analysis, the same caveats apply to a school level analysis.

Table 10: Effects on regular trajectory by race and SES

Outcome:	Concluded Elemantary	Concluded Elementary
Estimator:	Municipal TWFE	Municipal IV-TWFE
	(1)	(2)
Panel A:		
$\ln(meanFUNDEB)$	0.17*** (0.04)	0.56** (0.18)
$\ln(meanFUNDEB) * Race$	0.0088* (0.0035)	-0.0180* (0.0090)
Panel B:		
$\ln(meanFUNDEB)$	0.17*** (0.04)	0.52** (0.17)
$\ln(meanFUNDEB) * SES$	0.0021 (0.0018)	0.0036 (0.0044)
Fixed-Effects:		
Municipality	Yes	Yes
Year	Yes	Yes
Observations	907,623	907,623
R2	0.10	0.10
Dep. Var. mean	0.86	0.86

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Clustered (Municipality) standard-errors in parentheses
Note: This table presents the results of the estimation of equation 9 in columns 1 and equation 10 in column 2. The first stage of the 2SLS estimates are omitted. In panel A we add the interaction of our treatment outcome with racial dummy and panel B adds the interaction with the SES indicator. Column 2 is the result for our preferred specification, using the 9-year mean lagged relative enrollment as an instrument for the 9-year mean Fundeb transfers.

this variation is leaving endogenous pupils' characteristics uncontrolled, biasing the estimates.

In the research design employed in this paper, we rule out this source of endogeneity by using a pupil-level panel, where the effect identification comes from the correlation of the variation of educational spending with the variation in the test score of the same pupil between the fifth and ninth year SAEB exam. This allows us to rule out the possibility that pupils' time-invariant characteristics are biasing the estimates.

Now that the difference in the empirical strategy is clear, we can explain why this difference may explain a downward bias in the rest of the Brazilian literature. For our explanation to be true, this downward bias has to come from a correlation of the municipal composition of pupils with characteristics correlated with lower educational achievement studying at the municipal schools with per-pupil spending. One possibility is that municipalities that are spending more per pupil can enroll more children in their schools[21], and those new pupils have characteristics negatively correlated with pupils' achievement. This negative correlation is plausible, since it is possible that those pupils that were not studying had parents with lower educational attainment and lower support for education. Results of Cruz and Rocha (2018), showing that municipalities that received net positive transfers from FUNDEB increased enrollment and that this increase is higher for children whose parents had fewer years of schooling, help to corroborate this argument.

Conclusion

This paper studied the effect of public education spending on the educational achievement of municipal public school pupils in Brazil, focusing on standardized test scores and the probability of completing elementary education in at most 11 years. By exploiting pupil identifiers and longitudinal data, we provided estimates that improve on previous approaches in developing countries. Our results point to positive effects of educational spending on pupils' achievement, with suggestive evidence of stronger effects for disadvantaged groups.

While these findings are encouraging, they come with important caveats. Our analysis focused on a restricted group of pupils who took SAEB exams, which may limit external validity. Survivorship bias could mean that our estimates understate the benefits of spending for more vulnerable pupils. Moreover, we examined the portion of municipal spending linked to Fundeb transfers, which differs from general education budgets in its rules and predictability. This may help explain why we find positive effects whereas other types of windfall resources, such as oil royalties, do not translate into improvements in terms of pupils outcomes(CASELLI; MICHAELS, 2013; MONTEIRO, 2015; CHAN; KARIM, 2023).

Despite these limitations, the results carry clear policy implications. Increasing per-pupil education spending can raise overall achievement and help reduce inequality, especially for lower-SES and minority pupils. Moreover, by focusing only on test scores, much of the literature may have understated the role of spending in shaping longer-run outcomes such as grade progression and socio-emotional development. Our findings reinforce the importance of Fundeb as a redistributive mechanism and support policies that expand and equalize education financing, such as the 2020 reform that increased federal transfers and introduced SES weights into the allocation formula.

In short, resources matter for learning, they matter most for disadvantaged pupils, and they matter for more than test scores alone. Strengthening equitable, well-targeted education funding remains a promising strategy for improving both efficiency and equity in Brazilian basic education.

[21] Even though the enrollment of new pupils would mechanically reduce per-pupil spending, it is possible that the total increase in educational spending more than compensates for this reduction.

The FUNDEB formula, which redistributes revenues earmarked for educational spending based on the number of enrolled pupils in municipal schools, supports this mechanism.

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