On Government Effectiveness: Organizational and Governance Limitations on the Delivery of Education

On Government Effectiveness: Organizational and Governance Limitations on the Delivery of Education

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On Government Effectiveness:
Organizational and Governance Limitations on the Delivery of Education

Abstract

These essays discuss organizational and governance issues that limit the effectiveness of governments, with an emphasis on the provision of public education in Brazil. Chapter 1 provides evidence of one benefit of environments that recognize performance, which government organizations often fail to do. I show that recognizing a high-achieving student increases the performance of the recognized student and his high-achieving classmates. The results are consistent with the explanation that the award changes classmates' perceptions of themselves, which promotes a high-performance mindset (i.e., shaping students' perceptions about ability, goals within reach and interests). Chapter 2 studies political transitions in an environment where politicians control the appointment of bureaucrats. We show that a change in the political party of local governments adversely affects the quality of education, measured by student achievement. Party turnover leads to disruptions in the operation of the school, increasing the replacement rate of teachers and headmasters and leading to the termination of school programs. This finding highlights the costs of allowing political control over bureaucracies: the control results in disruption of personnel and programs. Chapter 3 shows a negative association between incidences of corruption in educational funds in municipalities and student achievement in municipal schools. The results suggest that both mismanagement and lack of resources due to corruption matter for the quality of public education.

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Introduction

These essays discuss organizational and governance issues that limit the effectiveness of governments, with an emphasis on the provision of public education in Brazil. Chapter 1 highlights one benefit of environments that recognize performance, which government organizations often fail to do. Awards that confer public recognition for outstanding performance can impact ex-post behavior by changing beliefs, norms or interests. I investigate whether the public recognition of students' accomplishments impacts their own and their peers' subsequent academic performance. I exploit Brazil's Math Olympiad "Honorable Mention" award that recognizes the top 4% of participants in a national competition involving 18 million students annually. I use a regression discontinuity design comparing classrooms with narrow winners and losers of the award. I find that the award improves the future educational outcomes of both the winner and her classmates. The spillovers on classmates are economically meaningful – one-fifth of the magnitude of the effects on the winner herself – and have long-run consequences: the enrollment in selective colleges of classmates of a narrow award winner increases by 10%. Proximity to the winner, both physical and in terms of ability, appears to be a key mediating channel. Taken together, the evidence is consistent with the idea that students' (and potentially teachers') behavioral changes explain the spillovers, at least in part due to changes in classmates' perceptions of themselves (i.e., shaping their perceptions about ability, goals within reach or interests). Chapter 2 examines the intersection of human resources practices in local governments and political constraints. We study how political party changes in mayoral elections in Brazil affect the provision of public education in a setting where politicians control the appointment of school personnel.

Exploiting a regression discontinuity design for close elections, we find that municipalities with a new party in office (close winners) have test scores that are 0.07 standard deviations lower than comparable municipalities with no change in the ruling party (close losers). Party turnover leads to a sharp increase in the replacement rate of headmasters and teachers, and a decrease in measures of the quality of school personnel in schools controlled by the municipality. The results show that party turnover in Brazilian municipalities negatively impacts the quality of public education measured by student outcomes, highlighting an additional cost of allowing political control over bureaucracies: frequent disruption in the provision process. Chapter 3 examines whether corruption and mismanagement affect the quality of public education. We use data from the auditing of Brazil's local governments to construct objective measures of corruption and mismanagement involving educational block grants transferred from the central government to municipalities. Using variation in the incidence of corruption across municipalities and controlling for student, school, and municipal characteristics, we find a significant negative association between corruption and the school performance of primary school students. Students residing in municipalities where corruption in education was detected score 0.35 standard deviations less on standardized tests, and have significantly higher dropout and failure rates. Using a rich data set of school infrastructure and teacher and principal questionnaires, we also find that school inputs such as computer labs, teaching supplies, and teacher training are reduced in the presence of corruption. To summarize, the essays document how certain organizational and governance aspects of the operation of governments are a roadblock to the full effectiveness of governments' organizations and programs.

Chapter 1

Recognizing Performance: How Awards Affect Winners' and Peers' Performance in Brazil

> We think that recognizing ordinary people who performed extraordinary acts of kindness and service is the best way anyone can think of to promote those values and to make everyone who watches think, "I could be that person too. I could do those kinds of things too"
>
> – Ron Rand, President of the Congressional Medal of Honor Foundation[1]

1.1 Introduction

Recognizing performance is a ubiquitous part of economic life. In education, it often takes the form of prizes and certificates, while in the labor market, 37% of US jobs have some form of pay for performance.[2] Much research has examined how the recognition

[1] https://americanwaymagazine.com/leaving-legacy

[2] Using Panel Study of Income Dynamics (PSID), Lemieux, McCleod and Parent (2009) estimate that the incidence of performance pay jobs is 37% on average between 1976-1998

affects the recognized individual.[3] However, as the president of the Congressional Medal of Honor foundation puts it in the opening quote, the recognition of an accomplishment is often intended to influence a broader audience. Does the recognition of someone's accomplishments also affect the much larger group of individuals who witness the success? Several ideas – ranging from aspiration theory to role model and morale effects – predict that the recognition of an individual's accomplishments spills over to the performance of the peers either positively or negatively (Chung, 2000, Ray, 2006, Lazear, 1989). Yet the magnitude and scope of such spillovers remain largely unknown.

In this essay, I study these spillovers in the context of education. I investigate how the public recognition of students' accomplishments impacts their own and their classmates' subsequent academic performance. To do so, I exploit a natural experiment on performance recognition provided by a large national math competition in Brazil. My setting is well suited to studying this question because I can measure individual-level performance and because there is a well-defined social group—a classroom of students—that witnesses the award.

Specifically, I study the Honorable Mention award in Brazil's Math Olympiad. The Math Olympiad (MO) is a large annual competition, involving 89% of Brazil's public schools. The Honorable Mention is awarded to the top 4% of participants—approximately 30,000 students out of 800,000 per year. The award has two important conceptual features for the question of interest. First, there is no monetary prize, only a certificate. Second, it is a public recognition. Award winners' names are disclosed on the Math Olympiad website, and school staff enters the classrooms to announce the winner.

I combine multiple sources of administrative data to track students and their classmates nationwide. I use administrative data on classroom assignments of all K-12 schools in Brazil to identify the participants' classmates at the time of the award. I then track the performance of the participants and their classmates in future years regardless of their future schools

[3] A few examples documenting ex-ante consequences are Angrist and Lavy (2009) and Kremer, Miguel, and Thornton (2009). Diamond and Persson (2016) and Dee, Dobbie, Jacob, and Rockoff (2016) examine ex-post consequences. For a comprehensive recent survey of the literature in education see Fryer (2016)

or classroom assignments. To assess the consequences for students at different parts of the ability distribution, I use several measures of academic performance, including subsequent participation in the Math Olympiad itself, school dropout data and test scores, and Brazilian SAT scores and college enrollment.

To recover the parameter of interest, I exploit a regression discontinuity design and a unique feature of my setting. The ideal experiment requires observing two equally accomplished students, only one of whom gets recognized; my empirical design approximates this.[4] Specifically, I compare two classrooms, in each of which a participant in the Math Olympiad scored close to the award threshold—one narrowly winning, the other narrowly losing. The unique feature of the setting is that the Math Olympiad organizers do not disclose the rank or score of non-winners, so near-winners don't know they were close to winning. The comparison between a comparable winner and non-winner allows me to recover the informational impact of recognition, which is a potential channel through which the recognition spills over to others.

Using data on five million students in 170,000 classrooms in schools all over Brazil, I show that the award increases the recognized students' and their classmates' subsequent academic performance. I divide the analysis into three parts. First, I find positive and meaningful spillover to the winners' classmates. The award increases participation and scores in the Math Olympiad and has long-term consequences, as it increases the classmates' enrollment in selective colleges by around 10%. To make a rough estimate of the impact on student's future earnings, I combine this causal impact of the award with non-causal estimates of the college wage premium in Brazil. The impact of the award on college enrollment is equivalent to an increase in the average classmate's annual earnings of about 39 reales (2005 CPI), 0.5% of per capita income in Brazil.[5] Since the winner has, on average,

[4]If, instead, I compare all recognized and non-recognized students, regardless of whether they were equally accomplished, subsequent performance differences might be a result of pre-award differences in performance rather than the causal impact of the award (Lee and Lemieux, 2010).

[5]College wage premium and other relevant statistics come from Ferreira, Firpo, and Messina (2014) and Binelli, Meghir, and Menezes-Filho (2008). Section 1.5 describes in detail the assumptions made, relevant statistics used and caveats with this exercise.

30 classmates, the aggregate effect of the spillover is substantial. For each award given, the overall increase in classmates' annual earnings is 1170 reales. This is equivalent to expanding the conditional cash transfer in Brazil to one additional beneficiary.[6]

Second, I find that the spillovers on classmates' performance are smaller and less persistent than the impact on the winner. The increase in classmates' subsequent Math Olympiad performance is about 1/5 of the impact on the winner. Moreover, the effect on classmates lasts for one year, while for the winner the impact persists for at least two years. The differences in magnitude and persistence suggest that the mechanisms that explain the spillovers are a subset of those that explain the impact on the winner.

In the third step, I shed light on the mechanism by investigating whether dimensions of proximity to the winner matter for the classmates' performance improvement. Physical proximity is one that does. I provide evidence that the winner's continued presence in the classroom is a key mediating factor driving the spillover results: grade-mates from other classrooms do not experience a performance improvement. The award also affects the likelihood that a classmate of a participant—that is, of a winner or narrow loser—will continue to be in the same classroom as that participant, with a larger spillover on performances when that happens (although noisily estimated). While this is consistent with changes in teachers' and participant' behavior, I find evidence suggestive that at least in part the spillovers are driven by classmates' behavioral response. Specifically, I find that classmates experience a performance improvement even when they are no longer in the winner's classroom in the subsequent year. Moreover, similarity in terms of ability between the winner and the classmates also matters. The spillovers are found for students in the top quartile of the pre-award test-score distribution and on outcomes particularly relevant to those students, such better performance in the Math Olympiad and on the Brazilian SAT and greater enrollment in selective colleges. The validity of my interpretation depends on whether alternative mechanisms can account for my findings equally well. I do not, however, find support for alternative explanations. In particular, I show that the award does

[6] 1170 reales represents around 15% of per capita income in Brazil

not affect the quality of teachers or students who are assigned to the subsequent classroom of the winner.

Several countries use awards to recognize student achievement.[7] Past literature has documented several benefits of such policies, such as an improvement in educational outcomes prior to the award (Kremer et al., 2009) and benefits that accrue to the recognized student (Ebenstein, Lavy and Roth, 2017). Taken together, my results suggest that recognizing a particular student's high performance enhances effort by shaping students' perceptions of themselves (changing their perceptions about ability, goals within reach and interests), which promotes a high-performance mindset among high achievers, highlighting an additional benefit from environments that recognize performance.

This finding alters the cost-benefit calculus of the Math Olympiad program and potentially of similar tournaments. I estimate that the recognized individual experiences only 50% of the total performance improvement that the award generates, while the remaining 50% occurs among high-achieving peers.[8] Moreover, the scope of the spillovers on the peers' performance suggests the importance of certain details in the design of tournaments. In particular, the benefits occur mostly to peers in the winner's vicinity (in terms of ability and presence in the winner's classroom). One implication is that the organizational structure matters; the partition of students into classrooms and the density of classmates around the participants' level of ability matter for the spillover results.[9]

This essay relates to two bodies of work. First, it relates to a literature that studies tournaments and non-monetary rewards. Past work has documented the consequences of

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[7]For instance, Colombia, India, Kenya, the United States, and others have introduced policies that allocate vouchers for secondary school and college and other non-monetary awards based on student achievement.

[8]This estimate is based on the impact on the Math Olympiad Performance in the subsequent year

[9]The analysis, however, is not sufficient to answer the question of whether to increase the number of awards. The degree to which marginal increases in the number of awards will improve students' overall outcomes, therefore, hinges either on information frictions or on relative concerns driving students' effort. The resulting trade-offs of an additional award in each of these cases are very different. In the information friction case, the trade-off is between motivating the top vs. discouraging the bottom. In the relative concerns case, it is between motivating the marginal and harming the infra-marginal, as the motivational value of the award is given by its scarcity. Moreover, due to the regression discontinuity design, the results should be interpreted as a net increase in the performance of peers of winners relative to peers of losers. It is possible that the results are driven by a decrease in the performance of the losers' peers.

recognition for the performance of the recognized individual in a variety of settings —in education (Diamond and Persson, 2016), in the workplace (Neckerman, Frey and Cueni, 2014) and in on-line platforms (Gallus, 2016). I complement this literature by showing that public recognition increases performance also for the high-achieving peers for whom the "certificate effect" is absent (i.e., peers cannot use the award as a signal to others of their ability)[10]

Second, my work contributes to research that lies at the intersection of peer effects and role models. There is a very active literature documenting the importance of peer effects in schooling behavior (Fryer and Torelli (2010), Lyle (2007), Carrell, Fullerton, and West (2009), Austen-Smith and Fryer (2005), Sacerdote (2001)). Few papers, however, look at how policies that target a particular student interact with peer effects. Two exceptions in this literature, and those most closely related to my results, are Bursztyn and Jensen (2015) and Sequeira, Spinnewijn, and Xu (2016). Both, as I do, examine the consequences of public recognition of a student's performance on her peers' performance. I complement previous findings by studying a setting where key elements of a role model effect are present—recognition happens in a group setting and is informative about goals within reach—which leads to a distinct result: public recognition has positive consequences for the peers' performance.

Specifically, Sequeira et al. (2016) study a merit scholarship in India. The authors find that the award increases a winner's perceived return to schooling, but doesn't impact her peers' perceived returns, only their self-reported interests. In contrast, I focus on actual academic achievement and behavior as outcomes and find that the award has positive effects. Differences in our results may be driven by the fact that I study effects on peers in a natural group setting (170 thousand classrooms with 5 million classmate peers), while they study a total of 1000 peers from anywhere in the winners' networks. Bursztyn and Jensen (2015) study peers in a natural group setting, a classroom. The authors shows

[10]One possibility is that the award changes the reputation of the school, resulting in a "certificate effect" for the entire school. I show, however, that the benefits are exclusive to the winner's classmates, not experienced by students in other classrooms in the same grade, which goes against the school reputation explanation of the results.

how public recognition of students—specifically, showing the top-performing students' names on a leaderboard—backfires and discourages effort. While my results are not in contrast with theirs, as they study the ex-ante effect of recognition, their findings do suggest that in low-study cultures, a student's success can cause social sanctions and decrease peers' performance. One conceptual difference is that Bursztyn and Jensen study the effect of revealing the relative positions of students within a small group (remedial education students), an innately zero-sum action, while I study the effect of recognizing success within a larger group (nationwide). In my setting, the award reveals information about a type of goal that is within students' reach.[11]

The remainder of the paper is organized as follows. Section 1.2 describes the Math Olympiad and its relation to the educational system in Brazil. Section 1.3 presents the several data sources used for the analysis. Section 1.4 outlines the empirical strategy and identification assumptions. Section 1.5 presents the results. Section 1.6 presents robustness checks. In section 1.7, I assess alternative mechanisms to explain my findings, state some limitations of my results, and discuss the policy implications of my study. Section 1.8 concludes.

1.2 Institutional Context

This section describes the Math Olympiad, the Honorable Mention Award and how it relates to schools and classrooms in Brazil. I emphasize the aspects of the award and its context that are likely to encourage students' and potentially teachers' effort.

The Brazilian Math Olympiad in Public Schools (Math Olympiad) is a large annual competition that targets public schools exclusively. It is organized by *the Institute of Applied and Pure Mathematics (IMPA)*, a Brazilian federal government research institute. All public schools in Brazil that serve 6^{th} to 12^{th} grade are eligible to participate.[12] The students

[11] My setting may also be a high-study culture relative to that of Bursztyn and Jensen. However, 10% of the classrooms in my sample are in schools in the bottom quartile of the national distribution of public schools.

[12] There is another, much smaller Math Olympiad that is open to private schools.

compete in three different levels depending on their grade: 6th-7th graders, 8th-9th graders and 10th-12th graders. Roughly 89% of the 65,000 eligible schools participate. Every year there are around 18 million student participants. The competition is advertised on popular TV channels and in every public school.

The competition is organized in two phases. The first phase is implemented entirely at the school level. The Math Olympiad organizers send each school the exam and instructions on how to grade it. [13]. The schoolteachers grade all the exams and send a list of the top 5% of students in their school to the Math Olympiad organizers. The scores and ranks in this phase serve only to determine the students who qualify for the second phase. Every year around 800,000 students advance to the second phase. The prizes and awards are entirely determined by the second phase of the Math Olympiad.

The second phase is jointly implemented by a regional coordinator and the central agency.[14] The exams are graded at the regional center and then sent to the national office. The national office produces the national ranking based only on the scores of the students in the second phase of the Math Olympiad. They then assign medals (gold, bronze and silver) to the top 2,300 students and honorable mention to the subsequent 30,000 students[15].

The award used throughout this paper is the Honorable Mention award, which has three components. First, the student receives a certificate congratulating him on the accomplishment. No monetary prize or training is provided to the Honorable Mention winners. This is important because it attenuates a potential resource channel that often comes with awards

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[13]The school chooses a regular school day and administers the exam to all enrolled students. The share of students who participate varies by school. Based on field interviews, there are two types of schools: schools that enroll all students in the MO, reserving half of a school day for the exam, and schools that enroll certain classrooms and not others.

[14]The regional coordinator takes care of the exam implementation and part of the grading. There is approximately one regional coordinator per state, and all are professors of mathematics in public universities chosen by the central agency. They are responsible for clustering the schools into groups based on their location and assigning each cluster an Application Center where the students take the exam. The exam consists of 20 open-ended questions and lasts 3 hours. The content is not entirely connected to the regular curriculum. Instead, it focuses on deep understanding of basic mathematical concepts.

[15]The assignment of the medals also depends on the student's state - as there are minimum numbers of silver and bronze medals given to each state. The assignment of honorable mention, however, depends only on the position in the national ranking.

that directly impact students' performance. Second, the winners' names are disclosed on the Math Olympiad website, ordered by rank. As I mentioned, non-winners get no information about their score or rank. This implies that an important component of the award is the information treatment. By winning the award, students are informed that they are in the top 4% of the participants in the 2nd phase of the Math Olympiad.[16] It is important to emphasize that the pool of 2nd phase participants is a sample that roughly consists of the top 5% of students from each public school in the country. Therefore, the award is a unique source of information about the students' positions in the national distribution, as students don't usually participate in national exams until they are about to graduate from high school[17]. The third component is a ceremony where the award winners are celebrated. This ceremony is not a formal or mandatory event. Some regional coordinators organize ceremonies; others do not. Beyond these Math Olympiad-sponsored ceremonies, there is much anecdotal evidence that schools and municipalities organize celebrations themselves. The information shock and the ceremony, when there is one, leverage the encouragement channel.

There are two reasons that make the Honorable Mention award the appropriate variation to answer the question of interest. First, due to the large number of Honorable Mention awards, there is more density around this cutoff than other medal cutoffs. For example, there are 10 times more students right at the cutoff of Honorable Mention than there are at the Bronze medal cutoff. This feature is important for being able to statistically identify an impact on peers that is potentially small in magnitude. Second, unlike the Honorable Mention award, the medals come with a prize. Medal winners receive an annual scholarship, participate in math training, are assigned an adviser who is a professor of mathematics from

[16] One can imagine that students can infer their score by checking the solution for the exam. However, the exams are not comparable across years, which makes it hard for students to learn their position in the distribution. For example, in the data the cutoff varies by 10 to 30 points on a 1-120 point scale. Moreover, the organization does not disclose statistics about the scores of past winners, just their rank and identity.

[17] Alternative sources of information would be other years of the Math Olympiad or similar competitions in other subjects. Other competitions, however, usually focus on a population of students who score higher in the distribution than those who score around the Honorable Mention cutoff. For example, the Brazilian Olympiad of Physics in Public Schools has only 3,500 awards, vs. 33,000 awards that are given in the Math Olympiad http://www.sbfisica.org.br/ obfep/.

a public university, and can participate in a national ceremony attended by the president of Brazil. Therefore, the medal discontinuity is a bundle of interventions which confounds the award impact and the resources that automatically come with it. The existence of such prizes, however, helps to leverage the public recognition aspect of the award as it comes from a nationally known and highly competitive tournament.

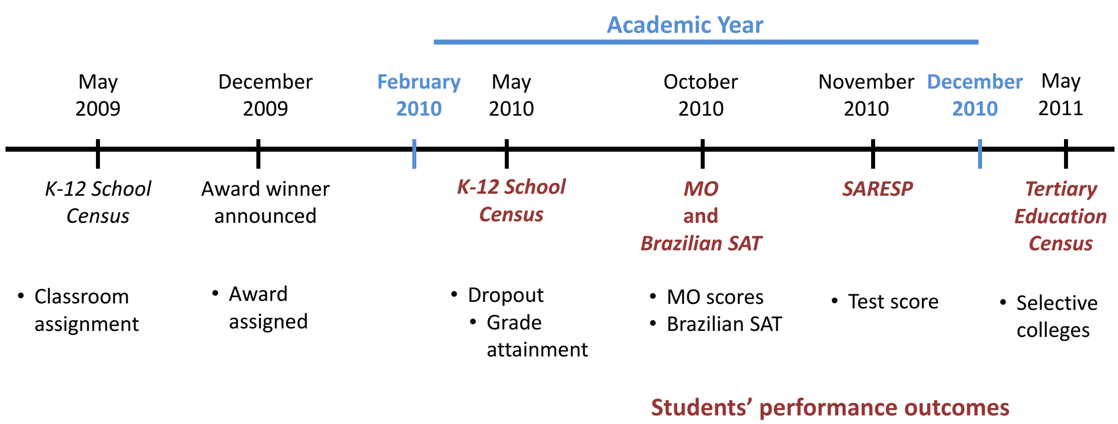
Several characteristics of the school system are important for interpreting the results. Teachers and classmates are likely to know about the award winner. They can check the Math Olympiad website as well as learn about the award in school. The Honorable Mention certificates are sent to the schools, and the school staff is responsible for distributing the certificates in the classrooms. There are around 31 students per classroom. The students in a given classroom take all subjects together for at least one year. It is also common for the same class to remain together for several years. In the data, around 50% of the students stay in the same classroom as the winner in the following year.

1.3 Data

This section describes multiple sources of data that fulfill three purposes. The first set of data allows me to identify the participants (narrow losers and narrow winners) and their peers - students who were classmates with the participant at the time the award winner was announced. I then describe all the performance outcomes that I use to assess performance throughout the ability distribution and overtime. Finally, I describe the auxiliary variables that I use as a control and to elucidate the mechanisms behind the results.

I use two data sources to construct the sample of participants close to the threshold and their classmates. Both are based on information that refers to, and was reported during, a period prior to the assignment of the award. I use the administrative data of the Math Olympiad from 2009 to 2012 to construct the score margin and the award cutoff for all participants in the Math Olympiad. As mentioned in the context section, the score margin is based entirely on the 2^{nd} phase of the Math Olympiad and can be interpreted in standard deviation units of the 2^{nd} phase exam[18]. I use information from the School Census of K-12 Education to recover the identity of the classmates of the Math Olympiad participants. This census is an annual survey filled out by schools in Brazil. A large share of the educational budget is determined based on the enrollment figures in this census, and in recent years, the government has begun auditing the census information; thus misreporting can have consequences. I therefore believe this survey to be accurate and reliable.

Figure 1.1: *Timeline*



Notes: This figure presents the timeline of the Math Olympiad and the outcome variables. The Math Olympiad varied over the years the month in which the award winners are announced. I report the average month of disclosure - empirically it varied from end of November to first half of February. MO stands for Math Olympiad.

I use several educational outcomes in order to have a comprehensive assessment of impacts on students' performance throughout the ability distribution. I designate the year the student takes the exam and is recognized as t. Most outcomes are measured in the following year $(t+1)$. The exact time within the 1-year range is presented in Figure 1.1. The Math Olympiad is my primary source of academic performance. I use participation in the Math Olympiad 2nd phase exam and the students' performance on the MO 2nd phase exam (from now on, MO Exam) as measures of MO performance.[19] This set of outcomes has the

[18] The original scores are not comparable across years. In order to use several cohorts in the same specification, I standardized the annual scores to have mean zero and standard deviation one. Therefore, the score margin used throughout the paper is in standard deviation units of the Math Olympiad score.

[19] Throughout this paper, the phrase "MO Exam" refers to the MO 2nd phase exam. I do not use information

advantage of being available for all grades and for consecutive years for the same student. I complement the Math Olympiad outcomes with a variety of student-level performance variables. The data sources and outcome variables are as follows: from the Census of K-12 Education, I construct measures of whether students dropped out of school and students' grade attainment. From the administrative data of the *Brazilian college entrance examination*, I use measures of students' participation and score performance on the Brazilian SAT (henceforth called the SAT)[20]. From the administrative data of the *Secretary of Education in the state of Sao Paulo - SEEDUC-SP*, I use Math and Language test scores on a low-stakes standardized exam.

To assess the medium- to long-run consequences of the award, I use additional outcomes. First, I use the Math Olympiad outcomes measured two years after the award ($t+2$). This allows me to measure the degree of persistence of the impacts. In addition, I use tertiary education outcomes, which come from the Brazil Census of Post-secondary Education[21]. The outcome variables are: enrollment in any tertiary education and enrollment in selective colleges with different degrees of selectivity. I construct a measure of college selectivity by ranking all colleges by the SAT score of the average admitted student.[22]

To assess the interdependencies between peers and participants inside the classroom, I leverage individual-level information from different sources. First, I use variables that are assessed prior to the award. I use the gender and race reported in the Census of Primary and Secondary Education at time t. For students' performance, I use student-level performance on a national standardized exam two years prior to the award ($t-2$). This is from *Prova Brasil*, administered by INEP. Prova Brasil is a bi-annual standardized exam available since

about the scores in the first phase of the Math Olympiad. As mentioned above, the awards are assigned based only on the students' performance on the 2nd phase exam, which has centralized grading and is therefore less prone to manipulation.

[20]Both the Census of K-12 and the Brazilian SAT are administered by Instituto Nacional de Estudos e Pesquisas Educacionais Anisio Teixeira- INEP http://portal.inep.gov.br/home

[21]Administered by Instituto Nacional de Estudos e Pesquisas Educacionais Anisio Teixeira- INEP http://portal.inep.gov.br/home

[22]Admission to college in Brazil is to a major-college combination rather than just to a college. To simplify for readers who are not used to the Brazilian context, I treat each college-major combination as a separate college.

2007 and administered to 9^{th} and 5^{th} grade students in all public schools with at least 20 students in one of the grades.[23] Second, I use classmates' school choices and measures of educational inputs in years after the award to shed light on the scope of the impact and mechanisms. The data source for these variables is the Census of K-12 education. I use the following outcome variables: whether the student transferred to another school, the student's subsequent classroom assignment and several measures of the classroom's composition in terms of students' and teachers' characteristics.

I use school-level information as controls. To obtain a unique measure of school quality for students in all grades, I combine information from Prova Brasil for primary schools and the SAT for secondary schools.[24] I use the position of the school in the quartile of the national distribution as a measure of school quality. Precisely every school in one of the four quartile-based bins.

1.3.1 Sample Selection

I take a number of steps to restrict the sample used in the empirical analysis. My starting point is the universe of 1.5 million 6^{th}-11^{th} grade participants in the 2009-2012 Math Olympiad (and approximately 45 million of their classmates). To identify individuals across data sets, I follow the procedure explained in detail in the appendix. I restrict my sample to students who were uniquely identified using this procedure (based on information that refers to and was reported during a period prior to the award). this restriction excludes *30%* of participants (and their classmates).

[23] This data was not used as an outcome for a few reasons: first, it is not available for 11^{th} grade, which is the grade that all the analysis, including the comprehensive set of outcomes, refers to. Second, unlike the other sources, it is given bi-annually rather than annually. Lastly, since there is data from the *Secretary of Education from the State of Sao Paulo* about standardized test scores, the Prova Brasil, which refers to the same type of outcome, would add little to the analysis.

[24] Including SAT data is necessary as it is the only source of national data for the universe of secondary schools. For students enrolled in 6^{th}-9^{th} grade, I rank their schools based on the average score on the *Prova Brasil* Exam in the year prior to the award.[25]. I divide the schools into quartiles based on their rank and use their position in terms of quartiles as the measure of school quality. For students enrolled in secondary school, I do the same procedure but instead of using *Prova Brasil*, I use the performance on the Brazilian SAT, as it is the only national test available for secondary schools.

The second restriction concerns students who score right at the cutoff point. Due to the discreteness of the score scale, there are a number of students who score right at the cutoff point. The Math Olympiad organizers use tie-breaking criteria that depend on the degree of difficulty of the questions. Since the criterion for ranking the students at the cutoff is constructed *ex-post* and is different from the criterion for ranking the remainder of students, I leave the students who score right at the cutoff point out of the sample. This restriction excludes an additional *.3%* of the initial sample of participants.

The third restriction is due to the definition of the running variable (participant's score in the Math Olympiad in the year of the award). In the ideal experiment, we would recognize one student in a classroom and observe students' performance relative to the control classroom where no award was given. In practice, there are classrooms with multiple award winners and classrooms with a narrow loser together with a winner. To make the treatment interpretation as close as possible to the ideal experiment, the running variable (and so the treatment) is defined for the highest- scoring student in the classroom. This excludes an additional *26%* from the initial sample.[26]

The final sample consists of 700 thousand (43% of the initial sample) of Math Olympiad participants enrolled in 6th-11th grades. Close score participants account for 170,000 narrow winners and narrow losers, with a total of 5 million classmates. I explain the close scores definition in the next section. Section 1.3.2 presents summary statistics comparing participant vs. classmates as well as classrooms within the RD sample vs. the full sample.

Sample limitations. All the results reported in this paper are based on the final sample described above: Math Olympiad participants enrolled in 6th-11th grades who satisfy the three sample restrictions. There are two exceptions where I use a more limited sample.

First, when I use the comprehensive set of outcomes (performance outcomes that do not refer to the Math Olympiad), I restrict the sample to 11th grade students only. Unlike the Math Olympiad outcomes, these additional outcomes are not all available for all grades.[27]

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[26]The share of students around the cutoff who were 1st highest, 2nd highest and 3rd highest in the classroom were respectively 76%, 14% and 4%.

[27]Dropout, Grade Attainment and Math Olympiad are available for all relevant grades 6th to 11th; Low stakes

For example, students only take the SAT in the last year of high school. Since all outcomes are available for the 11th grade, I present results only for the 11th grade. I make it explicit in the Tables and Figures when the sample is restricted to 11th graders only. Second, on top of being restricted to 11th graders, the results based on heterogeneity of pre-award students' performance impose an additional restriction. Pre-award students' performance is only available for one of the years of the Math Olympiad, 2011.[28]

1.3.2 Summary statistics

This section provides summary statistics for the participants (close to the award threshold) and their classmates, and compares classrooms within the RD sample and the overall sample. The regression discontinuity design that I implement relies on the assumption that relevant factors that determine the outcomes vary smoothly around the threshold. I test this assumption in Section 1.4. While average levels of students' characteristics and performance are not relevant for internal validity, they are still relevant for external validity and interpretation of the results.

Characteristics of participants (who score close to the award threshold) and their classmates. In the appendix, Table 1.1, Panel B compare participants and their classmates within the RD sample. Participants have better educational outcomes compared to classmates; for example, they are 3 times more likely to enroll in selective colleges. A participant is also slightly more likely to be white and male than a classmate is, but the differences are small. For example, 46% of participants are female, while 52% of classmates are female.

Classrooms characteristics in RD window vs. in full sample. In the appendix, Table 1.1, Panel A compares classrooms in the RD sample with the full sample. The regression discontinuity method estimates a Local Average Treatment Effect (LATE). This means that the results are representative only for students around the threshold. The summary statistics

test score is available for 9th and 11th grade; SAT and college is available for the 11th graders only.

[28]I use *Prova Brasil* 2009 as a source of pre-award ($t-2$) test score information for 9th graders who after two years are in the 11th grade.

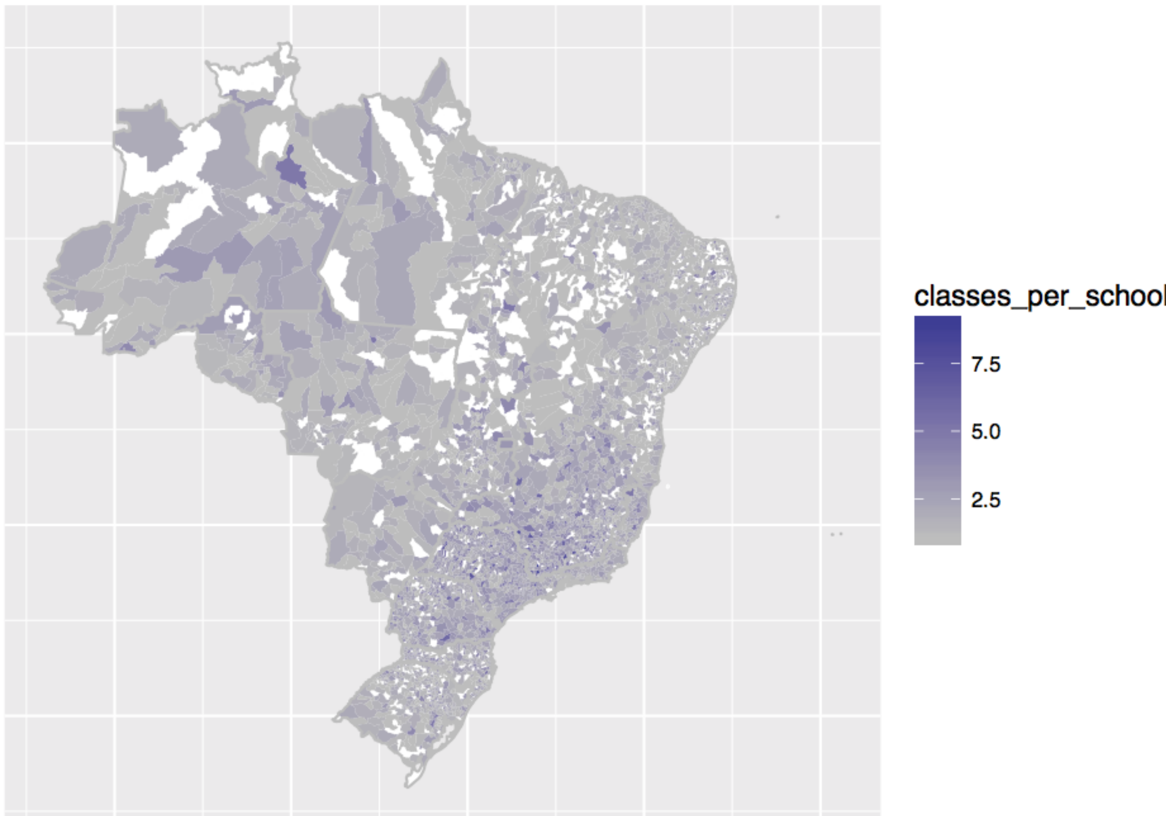
Table 1.1: *Summary Statistics*

Panel A		RD sample	Full sample
% of schools in quartiles of the test score distribution of all schools	Q1	0.10	0.18
	Q2	0.20	0.24
	Q3	0.29	0.25
	Q4	0.34	0.22
# of students enrolled in the school		780.07	738.51
% of students participating in SAT		0.52	0.49
Number of award in past 2 years		4.15	2.88
# of classrooms		170,335	688,655

Panel B	RD Sample	
	Participant	Classmates
% female	0.46	0.52
% white	0.61	0.52
% that participate in MO exam	0.13	0.02
Among MO takers, % that score above the median	0.84	0.51
% of SAT takers	0.80	0.55
Among SAT takers, % scoring above the median in SAT	0.80	0.40
Among SAT takers, % enrolled in top 30th selective college	0.28	0.11
# of students	170,335	5,114,922

Notes: This table report in Panel A, summary statistics for the RD sample vs Full sample and in Panel B, summary statistics of the participant and her classmates. In Panel A, first 4 lines report % of schools in each of the samples that are in each of the quartiles of the national score distribution. N of students enrolled in the school, is the total number of students at the year t% of students participating in SAT, is the share of 12th graders that participate in the SAT. Panel B, first two lines represent the gender and race of the student. The following are measures of students' performance measured at $t+1$ but all referring to the control group.

Figure 1.2: *Average Number of Classrooms per School in the RD sample*



Notes: This figure presents the number of classroom per school in the RD sample and the geographic location in the map of Brazil of the corresponding municipality where the school is located. In the map, municipalities are colored in white if there is no classroom in the RD sample in that municipality. Municipalities colored in light gray represents municipalities where there is is average one classroom per school, up to dark purple 7.5 classrooms per school. The map refers to the first MO-cohort in the sample, 2009.

are useful for understanding the profile of students for whom the results are representative. The classrooms in the RD sample come from better schools than in the full sample, but there is a reasonable number of schools in all quartiles of the national distribution of school quality. For example, 10% of schools in the RD sample come from the bottom quartile. The schools in the RD are also larger and more likely to have had a student who won the award in the past two years.

1.4 Empirical strategy

To estimate the impact of the award, I use a regression discontinuity design, comparing students who had an award winner in their classroom to a control group - students in a classroom in which the participant earned a score similar to that of the award winner, but narrowly lost the award. I discuss the challenges addressed by this research design, the specification choices made, the assumptions it implies, and lastly, threats to the identification strategy.

There are two main empirical challenges that this research design addresses. First, students who receive the award are usually stronger to begin with. I therefore narrow the comparison to classrooms where participants scored close to the threshold but may not have been recognized with an award because of limits in the total number of awards given every year. The second challenge is that an award can determine the peers and classmates of a recognized student, making the network endogenous to the treatment. In my setting, this is not a issue because the classmates are defined as the participants' classmates at time t, prior to the announcement of the award winners.

As emphasized, participants' assignment to treatment is determined by their score on the 2nd phase Math Olympiad Exam relative to the award threshold. The threshold is determined by the lowest-scoring participant to receive the award, considering the category in which the student competes.[29] 98% of students who score greater than the award threshold receive the award. I therefore implement a sharp discontinuity design.

I follow standard methods for regression discontinuity analysis (as in Lee and Lemieux (2010)). My main specification restricts the data to a small window around the threshold and estimates an ordinary least square (OLS) regression using a flexible linear specification (as in equation 2.1). I present results for three different chosen bandwidths. First, for

[29] For example, all students in a given year competing at a certain level experience the same cutoff. Only in 2012 did some school characteristics start to matter for the allocation of the Honorable Mention Award. In 2012, the Math Olympiad established a minimum number of awards to be awarded in each of the 27 states, as well as a maximum number of awards that could be assigned to students enrolled in selective schools (e.g., schools that use exams in their admission process).

each outcome I implement the procedure proposed by Imbens and Kalyanaraman (2011) to estimate the optimal bandwidth and report the results for that bandwidth. This results in different samples for different outcomes. In order to keep the sample consistent across different estimations, I report two other bandwidths. I report the results using the minimum optimal bandwidth considering all outcomes and the average optimal bandwidth.[30] I define a close score as one where the difference between the participant's score and the award cutoff falls within the different bandwidths: the average optimal bandwidth(.82 s.d. of Math Olympiad exam [Main]); the minimum optimal bandwidth (.62 s.d. of Math Olympiad exam); or the individual outcome optimal bandwidth.

For the main results of the paper, I present two additional robustness checks to this main specification. First, I report how the results vary for alternative bandwidths of .32, .42, .52, .72 and .82 standard deviations. Second, I relax the linearity assumption and estimate a non-parametric local linear regression.

Terminology: Participant and Classmates Throughout the paper, the terms *participant* and *classmates* each refer to a specific group of students. A participant is a narrow winner or narrow loser who scores close to the award threshold. Due to the sample selection procedure, there is exactly one *participant* per classroom in the RD sample. There may be other students in the participant's classroom who also participated in the Math Olympiad, but I do not refer to those students as participants. Classmates are students in the same classroom as the participant at year t. Classmates could have participated in the Math Olympiad or not. Regardless, they are included as classmates.

The specification is always at the individual level, estimated separately for the participant and for the classmates. The specification for the participant is:

$$y_{ck} = \alpha + \beta Award_{ct} + \lambda Score_{ct} + \delta Award_{ct} \times Score_{ct} + \gamma_c^{grade} + \gamma_t + \varphi X_{ct} + \varepsilon_{ct}; \qquad (1.1)$$

[30]all educational performance outcomes were used to define these bandwidths: Dropout, Grade Attainment, Standardized test score, Brazilian SAT, enrollment in selective colleges and Math Olympiad

estimated for participants whose scored margin of loss or win is less than the selected bandwidth, where y_{ck} denotes the outcome y for a participant in classroom c at time $k = (t, t+2]$ assessed after the award is announced at year t. In principle, there could be many winners per classroom, c. As explained in the Sample Selection Section, the variables are defined for the participants who earned the highest score in the classroom. Therefore, there is only one narrow winner or loser per classroom, c. The model includes control variables all determined at year t. γ_c^{grade} and γ_t are a set of grade fixed effects and Math Olympiad cohort-year fixed effects; X_{ct} are controls for school quality. $Score_c$ is the participant's score normalized to be zero at the award threshold. It is therefore the running variable. The variable $Award_c$ is a indicator function equal to one if the participant scored above the award cutoff ($Score_c \geq 0$). The coefficient of interest, β, captures the effect of the award on outcome y.

The specification for the classmates of the participant is analogous:

$$y_{ick} = \alpha + \beta Award_{ct} + \lambda Score_{ct} + \delta Award_{ct} \times Score_{ct} + \gamma_c^{grade} + \gamma_t + \varphi X_{ct} + \varepsilon_i; \qquad (1.2)$$

Where y_{ick} denotes the outcome y for a classmate i in a classroom c at time $k = (t, t+2]$ assessed after the award being announced at year t. 1.2 is estimated at the classmate level. For each classroom, there are around 30 observations (number of classmates). Since the award treatment is at the classroom level, I cluster the standard errors at the classroom level.

The main outcome y for the participant and for classmates measures the student's subsequent performance in the Math Olympiad assessed at $t+1$. As explained in the data section, I complement this measure with a comprehensive set of 9 outcomes all assessed at $t+1$, spanning outcomes relevant for students throughout the ability distribution. I present the results of each individual regression as well as on a summary measure following Kling, Liebman, and Katz (2007).[31] I run seemingly unrelated regressions (SUR) to compute an

[31]The results are similar if instead of including all 9 outcomes I make an intermediary aggregation into 4 components - progress in school index (=1, did not dropout; =2, grade attainment; =0 otherwise), Standardized test score , SAT index (=1, if enrollment in SAT; =2, whether the student scored in the top 50th percentile; =3, whether the student scored in the top 30th percentile; 0, otherwise), MO index (=1, whether the student qualified to the Math Olympiad, =2, whether the student show up to the exam, =3, whether the student scored

effect size $\hat{\beta}$:

$$\hat{\beta} = \frac{1}{K} \sum \frac{\hat{\beta}_j}{\hat{\sigma}_j} \tag{1.3}$$

In equation 1.3, $\hat{\beta}_j$ are the point estimates obtained for estimating equation 1.2 for each of the outcomes. $\hat{\sigma}_j$ are the standard deviations of the outcome for the control group, and K is the number of outcomes included in the summary measure (In this case $K = 9$). I use bootstrapping to obtain standard errors for $\hat{\beta}$.

Validity of Identification Assumption. The identification assumption is that relevant factors that determine outcomes vary smoothly around the threshold; thus any discontinuity after the award is the result of the treatment assignment. While it is not possible to directly test this assumption, we provide sugestive evidence that the assumption holds in this setting. First, we find no evidence of manipulation around the award cutoff. As I emphasized previously, the award threshold is unknown ex-ante, and empirically varied over the years by 10 to 30 points on a scale of 1-120 points. This makes any manipulation unlikely. The density is indeed smooth around the award threshold as reported in the appendix. Using the Frandsen (2016) test, I do not reject the hypothesis the density is smooth around the threshold.[32]

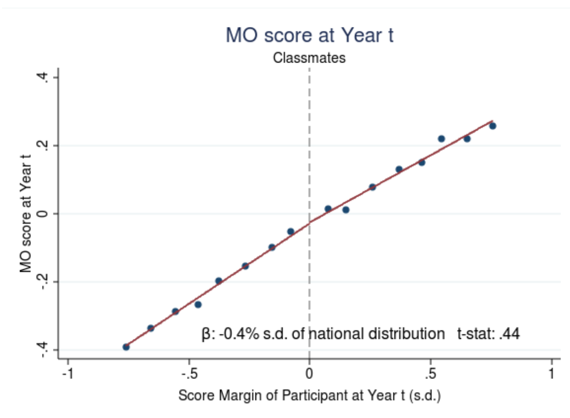
I also test for smoothness in pre-award characteristics and lagged outcomes. Figure 1.3 report the results and the corresponding table with additional tests are in the appendix. Since there are some outcomes available for all grades, and some available only for 11th grade, I test for discontinuity separately for both samples. I use school-level characteristics and student characteristics and lagged outcomes. Out of the 52 tests, 0 are significant at 5%, and 5 are significant at 10%. Overall this lends support to the identification assumption of continuity of pre-award characteristics around the award threshold.

in the top 30th percentile; =0, otherwise)

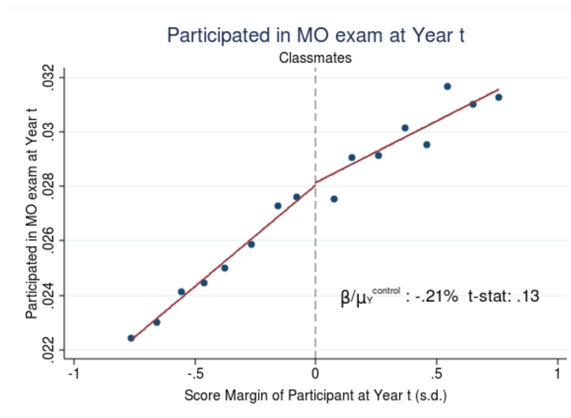
[32]The Frandsen test is an alternative to McCrary that is appropriate when: i) the running variable is discrete, and ii) the discontinuity is located in a segment of the density where the linearity assumption used in McCrary is not satisfied. Since in my setting the running variable is discrete and the threshold is closer to the upper tail, I use the Frandsen test.

Figure 1.3: *RD plots for pre-award outcomes* ***(PLACEBO)***

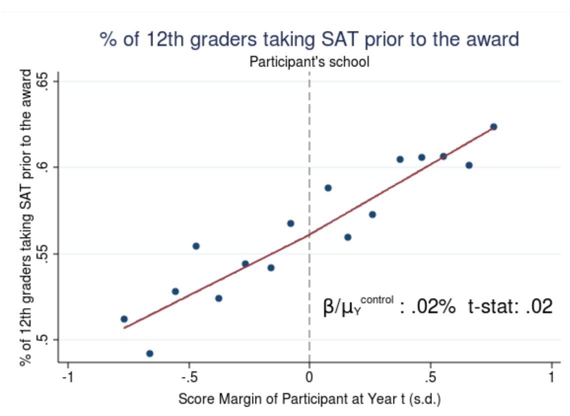
(a) *Classmates*



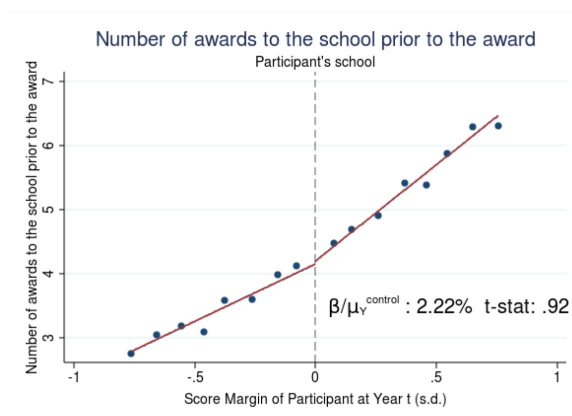
(b) *Classmates*



(c) *School*



(d) *School*



Note: This figure plots pre-award student's characteristic variables as a function of score margin of participant at year t (running variable).

1.5 Results

My main result shows that awards lead to higher performance on the part of the participant and her classmates. The spillovers on classmates are economically meaningful. It increases measures of students' participation and score in the Math Olympiad. The award has potentially long-term consequences as the award increases classmates' enrollment in selective colleges. Second, I show that the impact on classmates is smaller and less persistent than the impact on the recognized student. Finally, I exploit proximity, physical and in terms of characteristics, between a classmate and the recognized student to shed light on the mechanism behind the spillover results. I find that the presence of the winner in the classroom and similarity in terms of ability mediate the spillover results. I argue that the evidence is consistent with students' (and potentially teachers') behavioral changes explaining the classmates' performance improvement. Precisely, at least in part the performance spillovers are due to changes in classmates' perceptions about ability, goals within reach and interests - which I denominate a high-performance mindset.

1.5.1 Effect of award on participant's academic performance

This section examines whether the award impacts the participant. In what follows, I show that the award increases the participant's subsequent academic performance. While the focus of this paper is on the peers, studying the consequences for the participant serves two purposes: it serves as a benchmark of the magnitude of the award impacts on classmates and also helps shed light on the mechanism. The encouragement mechanism that seems to explain the spillovers on classmates is also relevant for the participant. It is therefore reassuring that the award also impacts the participant's subsequent performance.

Before presenting the results, it is important to clarify a few conceptual differences in interpreting the impact on the participant compared to the impact on classmates. First, the participant is at the very top of the ability distribution and many margins are not relevant for her. Second, the winner can use the award to signal his ability, a possibility that is not available for classmates. While in other countries receiving a award may result in having greater access to better colleges, in the Brazilian case most colleges, especially selective ones, have purely exam-based admission. However, could use the award as a signal to access better labor market opportunities. Third, for each participant there are 30 classmates and therefore the results for participants are less precise. I present below results on outcomes that are relevant to top students. The appendix shows results for all performance outcomes. The results are in general consistent with these three features I emphasize.

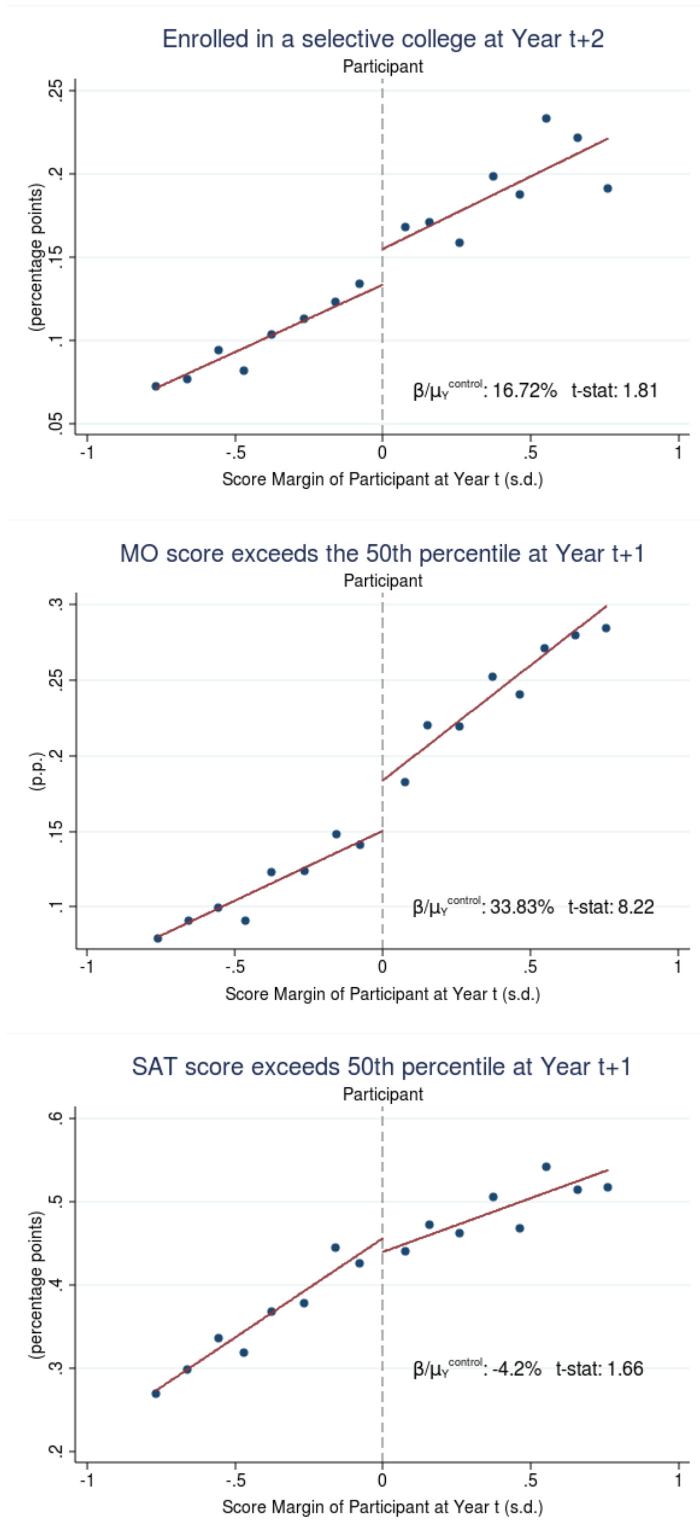
There are three educational outcomes that are likely relevant to top students: the Math Olympiad in subsequent years, tertiary education and the SAT. The participation in all these measures is an active decision of the student, and the participation itself is an outcome of interest. I therefore present results on outcomes that are well defined (non-missing) for the entire sample - report impacts on participation margins, and on the likelihood the student score above different thresholds. Most participants (80%) in the control group take the SAT at $t+1$, half of these (40%) enroll in college at $t+2$, and finally only 12% participate in the MO Exam at $t+1$.

Table 1.4, Panel A report impacts on MO participation and probability of exceeding different percentiles.[33] Students that do not participate in the MO are coded as having a score of 0 (e.g. as if he did not exceed any percentile). The award increases participation in the Math Olympiad by 27% relative to the control group mean, and also increases the share of students scoring above the different thresholds. For example, it increases by 68% the share of students scoring in the top decile of the national MO score distribution. Finding an impact on the Math Olympiad is reassuring as it lends support that the empirical strategy is indeed capturing the impact of the award. Given the participation levels, the Math Olympiad outcomes are also the least likely to suffer from a ceiling effect (when the levels of the outcome are so high that there is little room for improvements).

For 11th graders, which represent 12% of the full sample, it is possible to estimate the impact on other performance outcomes. Table 1.4, Panel B reports the impact on

[33]I don't present the impact on raw scores because participation is affecting the marginal score. I discuss in detail the choice of the variables in the next section.

Figure 1.4: *Student's Post-Award Educational Outcomes (Participant)*



Note: This figure plots post-award participant's educational outcomes variables as a function of score margin of the participant at year t (running variable).

tertiary education at $t+2$, and Panel C reports the impact on SAT performance at $t+1$. Due to the sample restriction the estimates are imprecise, but it is overall consistent with the award having greater impact as the outcomes have greater relevance to top students (from Panel C to Panel A). Most students already participate in the SAT (80%) and among those 60% already score in the top 30th of the national distribution. The award therefore does not impact participation on SAT, the magnitude on the coefficient is insignificantly negative.[34] There is a positive impact on the share of students scoring at the very top of the SAT distribution as well as increases in enrollment in selective colleges. These results are however statistically weak, and not robust using other bandwidths. Figure 1.4 presents the regression discontinuity plots for attending selective colleges, getting above the 50th percentile in the subsequent Math Olympiad, and getting above the 50th percentile in the SAT. Overall there is a robust impact on Math Olympiad related outcomes and at most statistically weak impacts on likelihood of enrolling in selective colleges.

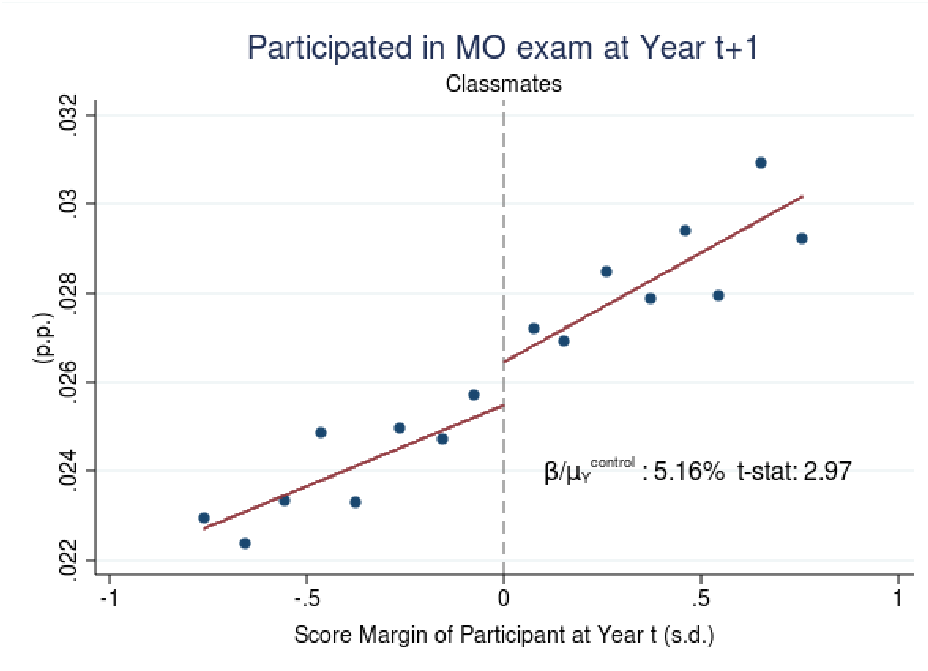
1.5.2 The spillovers to classmates

In this section I investigate whether the award impacts classmates. I first present the effects on classmates' performance in the Math Olympiad at $t+1$. This helps make the case that the impacts I document are indeed a consequence of the award; it also allows me to benchmark the results with those of the paper closest to this one Sequeira et al. (2016). I then investigate the scope and persistence of the spillover effects.

Figure 1.5 reports results for classmates' participation in the Math Olympiad at Year $t+1$. The outcomes are grouped in .1 standard deviation bins around the award threshold. The figure shows that even within our relatively small bandwidth there is a general increase in students' participation (y-axis) as the score of the participant in t increases (x-axis). The increase in participation observed just above the threshold appears to be a level shift, with very similar slopes on both sides of the threshold. The award is associated with an increase

[34]This is consistent with the idea that the award might be helping winners to get access to job opportunities and postponing plans to apply to college. The magnitude is however very small and statistically not significant.

Figure 1.5: *Participation in Math Olympiad Exam at* $t+1$ *(Classmates)*



Notes: This figure plots Classmates' participation in the Math Olympiad at year $t+1$ as a function of score margin of the participant at year t (running variable).

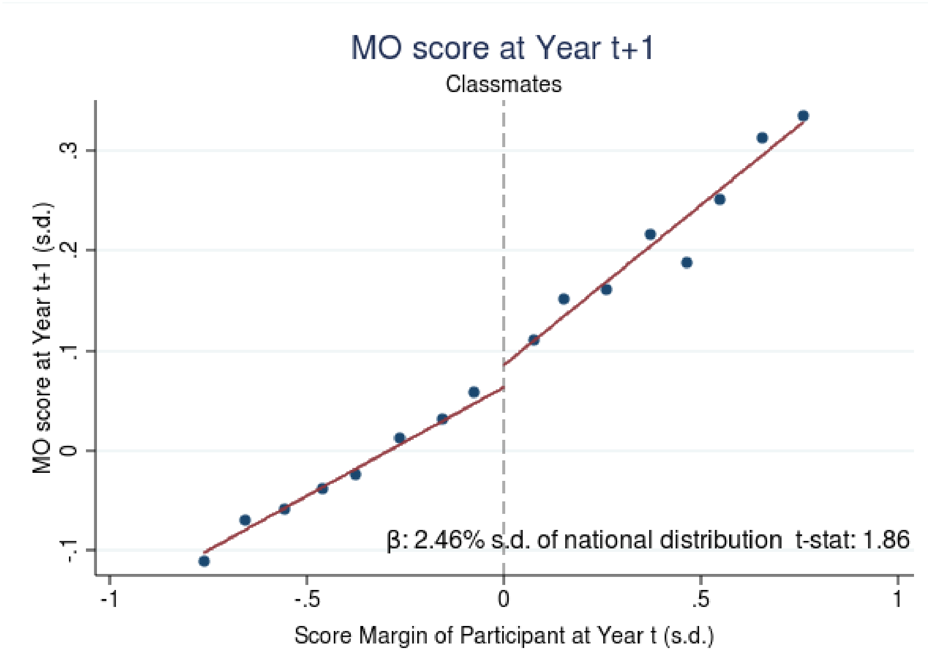
in participation of 5.26% relative to the control group mean.

The appendix reports the corresponding regression results (Table A.3). The first and second column report results without and with controls, for the smallest bandwidth. The following columns include controls and vary the bandwidth. The estimates are stable across the different specifications, and are all statistically significant at 5% level.

Benchmark for Magnitude of Effects: vs. Participant. The Math Olympiad outcomes offer the most precise estimates of the impact of the award.[35] They are therefore the best outcomes to compare the magnitude of the impact on participant and classmates. The award increase classmates' participation by .13 percentage points, while it increases participant's participation by 3.48 p.p. The absolute impact is therefore 27 times larger for the participant. However, only 2.5% of classmates in the control group participate in the Math Olympiad at $t+1$ (compared to 13% of control participants). Therefore, relative to the respective control

[35]this is due to the fact that the sample is restricted to 11th graders for the other outcomes

Figure 1.6: *Math Olympiad Score at $t+1$ (Classmates)*



Notes: This figure plots classmate's score in the Math Olympiad at year $t+1$ (in standard deviation units of national distribution) as a function of score margin of the participant at year t (running variable).

group mean, the impact on classmates is about 1/5 of the impact on the participant.

Figure 1.6 presents results on whether the award impacts classmates' Math Olympiad scores at $t+1$. The award is associated with a 2.93% increase in scores, in terms of standard deviations of the Math Olympiad national distribution. One should be cautious in interpreting this as evidence of greater learning, as the award might change the composition of students taking the Math Olympiad. While it is likely that the marginal student for whom the award influenced participation has lower ability than those that would have participated in the absence of the award, one cannot be certain without further evidence. For a sub-sample of students we can provide an empirical test for whether the award impacted the composition of MO participants[36]. In the appendix, Table A.4 tests for the impact of the award on the pre-award test score of students who participate in the MO exam at Year

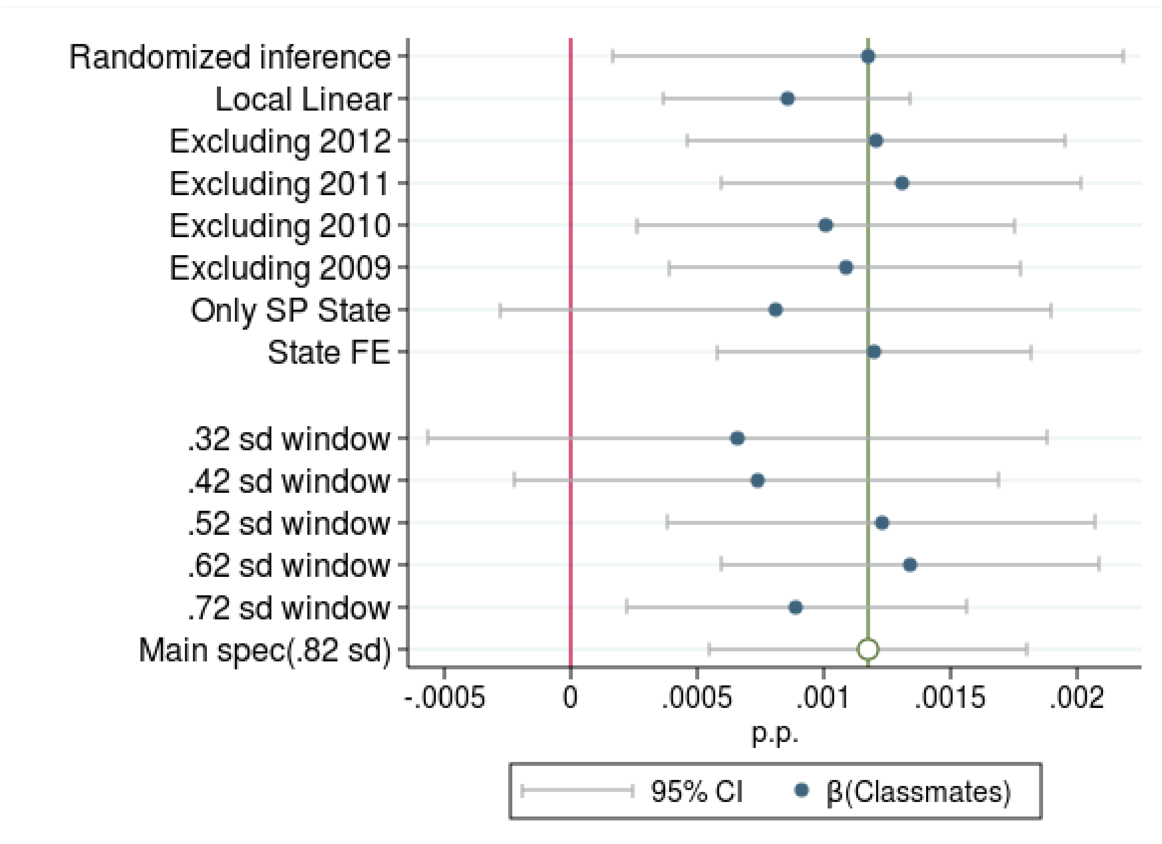
[36]The sub-sample is determined due to data availability. Precisely, the students who participated in the State of *Sao Paulo* standardized exam two years prior to the award

$t+1$. As expected the award decreases the average ability of students who participated at $t+1$. This lends support to the argument that the award not only increases classmates' participation, but also their learning. I follow Angrist, Bettinger, and Kremer (2006) strategy to derive non-parametric bounds on the impact of the award on MO scores. Since the selection bias is negative, selection-contaminated comparisons provide a lower bound on the likely impact of the award on achievement. For the upper bound I make a correction on the treatment sample which consists of including only the upper part of the distribution of scores, assuming that the students that wouldn't have participated in the absence of the award score at the bottom of the distribution. The bounds for the impact on classmates are tight, vary between 0.0293 up to 0.0341 standard deviations.

Both the participation and learning margins, capture important consequences of awards more generally, and therefore it is important to measure the consequences of awards for both margins. In the analysis that follows, many of the outcomes also have participation as a student's choice, and so together with the actual test performance the participation is an outcome of interest. Instead of documenting the existence of selection for each outcome I follow Angrist, Bettinger and Kremer (2006) and present the results on participation as well as on the probability of exceeding different percentiles of the corresponding score distribution. It seems unlikely that students that score at the very top were at the margin of not participating in the MO and SAT, for example. To shed light on whether there are impacts on learning margins, I report the probability of exceeding the median, as well as each of the four subsequent deciles. The results on how the award impact the Math Olympiad (that I just presented) using this alternative procedure are reported in the appendix Table A.5. Whenever I have to reduce dimensionality, in what follows, I use the probability of exceeding the median (Table A.5, column 2)[37].

[37] Alternatively one could use two other ways to reduce dimensionality: i) summary measure with all this variables. However, since the standard deviations are mechanically smaller as the percentiles grows, this would result in greater weighs to, for example, exceeding 90th percentile, which is not necessarily desirable. ii) Use a probit. The probit has the inconveneince in a RD strategy as the corresponding estimates would not represent what we see in the RD plots. Reporting one of the outcomes (MO score exceeding the median) and doing robustness with other outcomes was my preferred choice

Figure 1.7: *Sensitivity to Alternative Specifications (Classmates)*



Notes: This graph report impact of award using alternative specifications. Outcome variable in all specification is equal to 1 if classmate's MO score at Year $t+1$ exceeds the 50th percentile of national score distribution (0 otherwise). Outcome variable is well defined (1 or 0) for the entire sample.

Robustness checks. So far the results are robust to adding controls and to three different bandwidths. Figure 1.7 shows that the main results are robust to additional specification and sensitivity checks.

Figure 1.7 report the impact of the award on whether the classmates' MO score exceeded 50th percentile of the national distribution for a variety of specifications. I present 12 variations in total and all the results are within the 95% confidence interval. The set of alternative specifications and the explanation for testing those are as follows. First, the results are robust to a permutation exercise. I implement a battery of placebo tests in the control group region and a randomized permutation test (Chetty et al. 2009) to further reject that my results could be capturing other spurious noise unrelated to the award. Figure 1.7 reports the corresponding standard errors from this analysis, and I present the distribution results of the permutation exercises in the Appendix. Second, I estimate using local linear specification, instead of flexible linear, to test for whether the results were driven by the linear functional form. Estimates leaving out each MO-year cohort and including State FE, to understand if the effect was driven by a exact year or location where implementation was more successful (or failed). Estimates using only Sao Paulo State, this is due to the fact that standardized test score is only available for Sao Paulo, and it is important to understand if the results looks similar if compared to the rest of the country (as other outcomes are available nationally wide). Finally, I present the results for 5 other bandwidths, in .1 standard deviations intervals from my the bandwidth.

Impact on outcomes that are relevant to students throughout the ability distribution

Does the award impact all classmates equally no matter their level of ability? In this section I show that the spillovers are found only for top students, having little effect for other students.

On average only 5% of the classmates participate in the Math Olympiad. Even if the award has negative consequences on students at the bottom or at the middle of score distribution, I would likely not detect these impacts by studying Math Olympiad outcomes.

To understand the consequences of the award for students in different parts of the ability distribution, I implement two strategies[38]. First, I estimate the impact on a variety of outcomes that are relevant to students in different parts of the ability distribution. Second, I conduct heterogeneous analysis based on pre-award test score. This comes however with a constraint, as the new outcomes are not all available for all grades[39]. Since all outcomes are available for the 11th grade, I present results for the 11th grade.

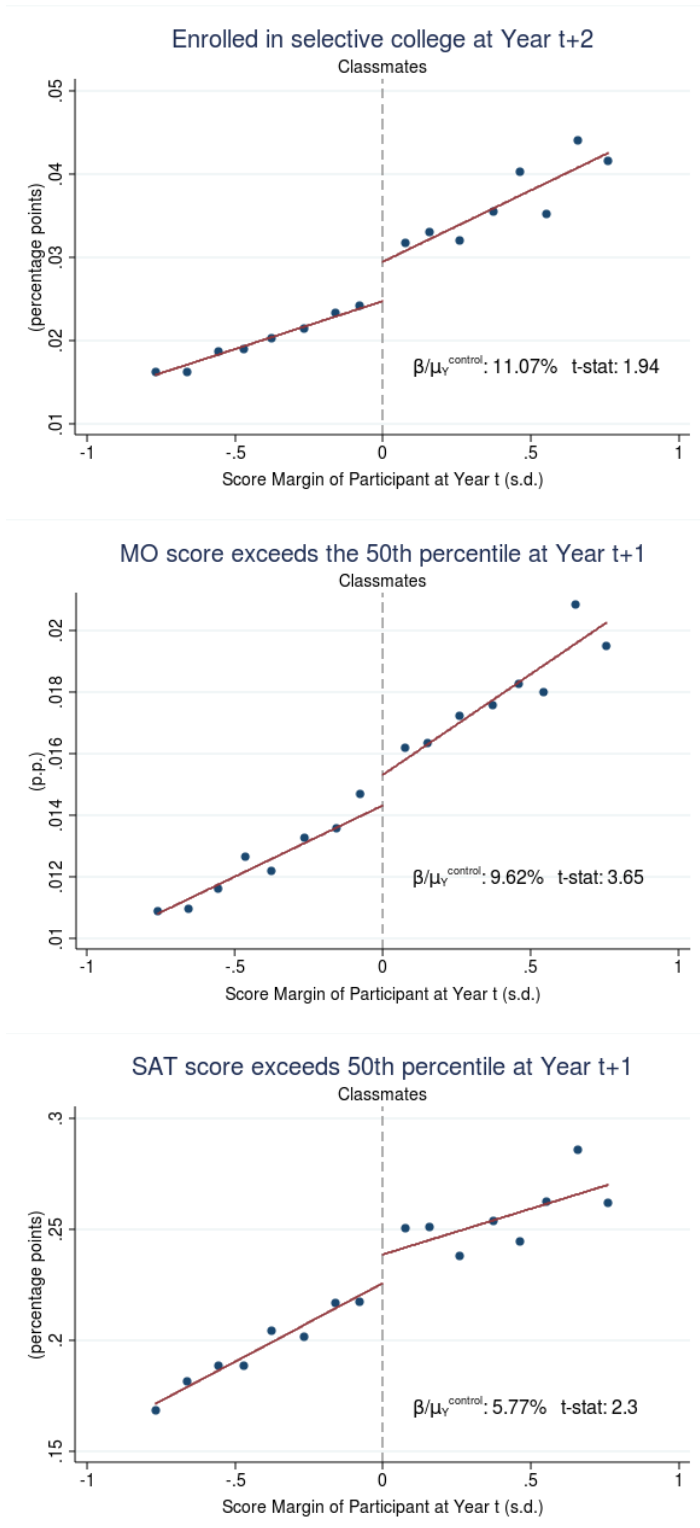
In the appendix, Table A.6 presents the impact of the award on SAT performance. Similar to the previous analysis on Math Olympiad outcomes, I present the impact on the likelihood of participation, as well as on the classmates' probability of exceeding different score percentiles. The award increases participation in the SAT by .89 percentage points and the likelihood that the student score in the top 20th percentile of the score distribution by .46 p.p.. It seems unlikely that many students that end up scoring in the top 20th percentile of the score distribution would not have participated in the SAT in the first place. This lends further support to the argument that the award impacts not only participation but also students' learning. Figure 1.8 presents the corresponding RD plot associated with the SAT results.

Benchmark for Magnitude of Effects: Private secondary school vouchers Angrist, Bettinger and Kremer (2006) study the impact of providing secondary school vouchers in Colombia on participation and performance in the Colombian SAT. The similarity of the context and the outcomes used makes this estimate the best one to benchmark the impact of the award to a alternative policy in education. The authors find that the voucher increased the probability the student score exceeded the 50th percentile by 4.3 p.p. (from a baseline level of 44.5%). This represents a increase of 9.8% relative to the control group mean. As

[38]A alternative strategy to get at impacts at different parts of the distribution which is more robust to outliers than studying impacts on the average, would be to implement a quantile regression. However, all the performance measures which are affected by the award (SAT and Math Olympiad) suffers from endogenous participation, and are also only available for a segment of the ability distribution. MO scores are available for the top 3%, SAT score is available to top 55%. Implementing a quantile regression in such cases would report a incomplete story as it would miss a large portion of the ability distribution.

[39]Dropout, grade Attainment and Math Olympiad are available for all relevant grades 6th to 11th; Low stakes test score is available for 9th and 11th grade, SAT and college is available for the 11th grade only

Figure 1.8: *Student's Post-Award Educational Outcomes (Classmates)*



Notes: This figure plots post-award classmates' educational outcomes variables as a function of score margin of the participant at year t (running variable).

reported in the appendix the impact of the award on the peers increases the probability the classmate score in the top 50th percentile by .92 p.p. The raw impact is about 1/5 of the impact of providing the voucher to the student. Relative to the control group mean the comparison of the respective samples, the award impact is about 1/2 of the impact of providing the voucher. The voucher had a value of roughly US$ 190 (about 8.3% of per capita GDP in Colombia at the time). The impact that I document here is therefore large, especially taking into account that the estimate is only capturing the ex-post effect of awards, rather than the entire effect of awards.

In the appendix, Table A.7 reports the award impact on a comprehensive set of outcomes measured at $t+1$, spanning outcomes relevant throughout the ability distribution. The outcomes are: no dropout, grade attainment, scores on the low-stakes state of Sao Paulo standardized test, participation in the SAT, SAT Score in top 50^{th} percentile, SAT score in top 30^{th}, qualified to Math Olympiad, participation in Math Olympiad exam, Math Olympiad Score in top 30^{th} percentile. The inclusion of dropout, grade attainment and test score outcomes are straightforward. The choice regarding the selection of outcomes measuring performance in the SAT and the Math Olympiad deserves a detailed explanation. Participation in the SAT and MO are active choices of students. To measure performance on these measures I therefore include not only their participation but also whether their scores exceeded different percentiles. As shown in Table A.6 in the appendix the results on the SAT measure are almost identical if I use neighboring percentiles. The results, therefore, do not depend on the exact definition of outcomes.

Overall there are a few interesting patterns worth emphasizing. First, the magnitude of the impact is larger for outcomes that are more relevant to top students such as SAT and Math Olympiad (and later on I will show that the same holds for college enrollment as well). This pattern is also evident for different percentiles of the SAT (Table A.6). For example, the award increase the share of students scoring in top 50th percentile by 4% and it increases students scoring in top 10th percentile by 9.5%. Margins that are relevant for students at the middle and bottom of the distribution, such as dropout, grade attainment and test scores are not statistically impacted, and magnitudes are positive and small. Second, the lack of impact on standardized test scores is particularly interesting. There are two potential explanations for why there is no impact on test scores. First, this is a low-stakes exam for the student while other performance measures presenting greater impact are all high-stakes for the student. Second, this is the only test score measure in which participation is not a active decision of the student. While I have presented evidence that the award impacts learning as well as participation, the lack of result on test scores suggests that the learning impact is small, or at least not detectable in a standardized low-stakes exam.

The second exercise is to test empirically whether top students are indeed driving the performance result. For a sub-sample of students, I have pre-award test score measured at Year $t-2$.[40]. Table 1.2 reports the heterogeneity analysis result. I find that students in the top quartile of the pre-award score distribution are driving the increase in the summary measure. There is some weak evidence that the award might be detrimental to the bottom. The award negatively impacts students below the median, but this finding is not robust for the different bandwidths.

Taken together, the evidence suggests that spillovers are concentrated on classmates in the top of the ability distribution: i) the award only affects margins that are relevant for top students, such as MO, SAT and (as I show in the next section) College enrollment; ii) The heterogeneity analysis shows the increase in overall performance for the full sample is driven by students initially at the top of the ability distribution.

Persistence of Impact

Does the impact of awards on student achievement persist over time? I implement two exercises to answer this question. First, I compare the impact of the award on Math Olympiad performance at $t+1$ vs. $t+2$. The impact is short lived, as there is no impact at $t+2$. Second, I test for whether there is a impact on enrollment in tertiary education. I find a positive impact on classmates' enrollment in selective colleges.

[40]See data section for a detailed explanation of data availability

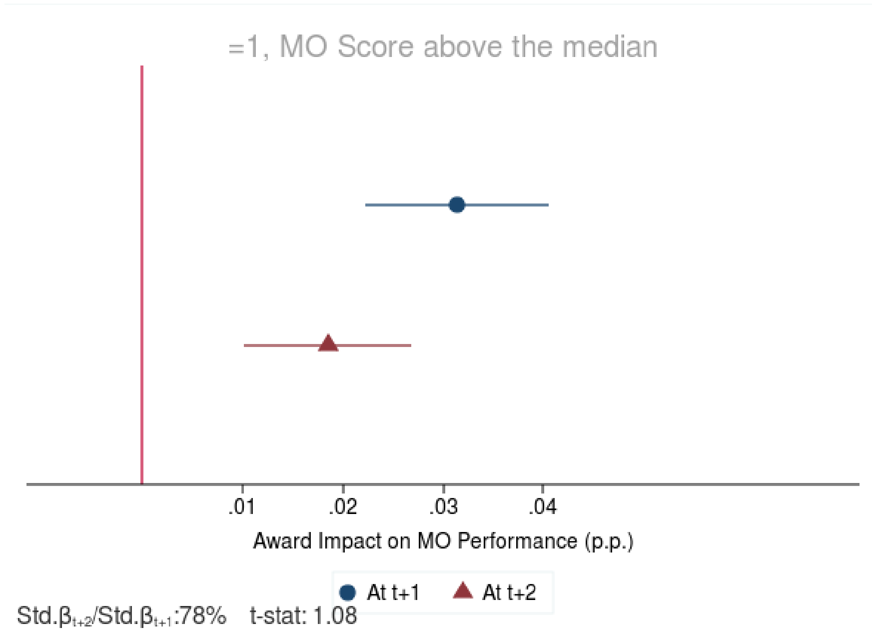
Table 1.2: *Impact of Award on Summary Measure at t+1 by classmates pre-award test score performance (Classmates)*

	Summary measure at t+1			
Award	-0.0264	-0.0098	-0.0406 *	-0.0313
	(0.0263)	(0.0272)	(0.0237)	(0.0238)
Award x Above Median	0.0852 **		0.0552	
	(0.0429)		(0.0379)	
Award x in Top Quartile		0.111 **		0.079 *
		(0.0556)		(0.0477)
linear combination: coef.	0.0588	0.1012 *	0.0146	0.0477
linear combination: s.e.	(0.0429)	(0.0549)	0.032444	0.047534
% of pop in each subgroup	57%	30%	56%	29%
Bandwidth selection	0.62	0.62	0.82	0.82
Students (Obs.)	32000	32000	43349	43349
Classrooms (Clusters)	2083	2083	2854	2854
Sample restriction	11th grade sample			

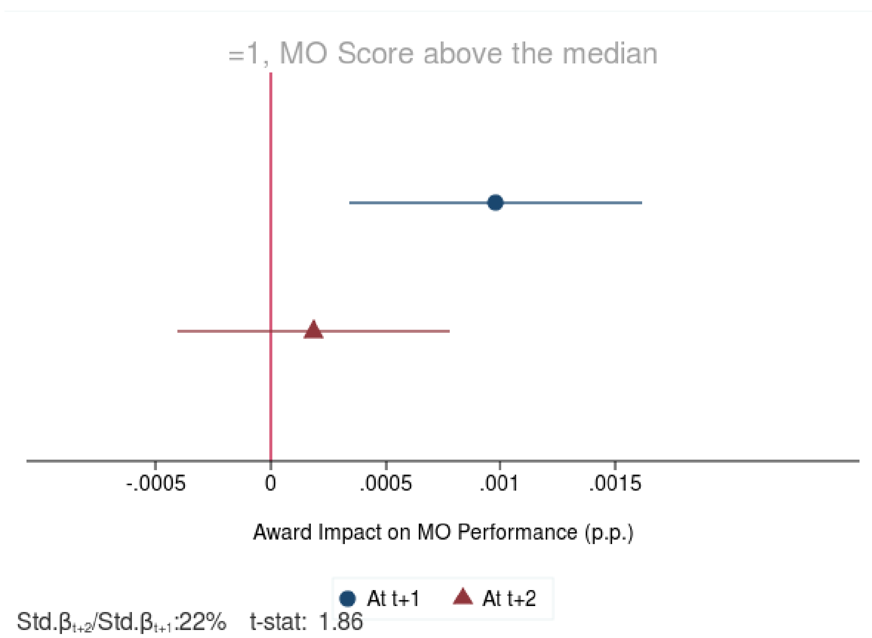
Notes: Sample includes 11th graders only. The table displays heterogeneous effects of the award impact on the summary measure at $t+1$ by pre-award score performance.

Figure 1.9: *Award Impact assigned at Year t on MO performance at $t+1$ and $t+2$*

(a) *Participant*



(b) *Classmates*



Notes: Sample restricted to be a balanced sample: includes 6^{th}-10^{th} grades, 2009-2012 (excludes 11th grade). Restriction imposed for keeping the same sample in $t+1$ and $t+2$ specifications.

Figure 1.9 reports the treatment effect on the performance in Math Olympiad one and two years after the award has been assigned for a balanced panel of students. The impact on the participant persists; after two years, the impact is still statistically significant at 5% and the magnitude is 77% of the impact after one year. The impact on classmates, however, lasts for one year only. The impact after two years is not statistically significant and the magnitude is only 4% of the one year impact.

Another dimension of persistence is whether there are consequences for post-secondary education outcomes. I test for whether there is an impact on enrollment in tertiary education after two years of the award - which is the relevant outcome for the 11th grade cohort that takes the SAT at year $t + 1$.

Data on tertiary education outcomes has a limitation which is relevant for interpreting the results. I only observe enrollment in tertiary education if the student participates in the Brazilian SAT. The admission process in Brazilian colleges are entirely exam-based, and most colleges use the SAT in some stage of its admission process. For example, 45% of all students admitted into any post-secondary institution participated in the SAT in 2012.[41] Therefore, a candidate who are considering applying for higher education would likely take the SAT. I observe the enrollment status as long as the student participate in the SAT, regardless of whether the SAT is used in the admission process of the institution in which he enrolled. Since the award impacts enrollment in the SAT, there will be a potential mechanical impact on college enrollment even if in practice students are not changing college decisions. To minimize this issue, I report the impact on overall tertiary education enrollment as well as on enrollment in colleges that have a higher level of selectivity[42]. Students that enroll in a selective college are unlikely to be at the margin of participating in the SAT, and so an increase in enrollment in selective college is almost certain to capture a greater enrollment in college. I construct a measure of college selectivity based on the average SAT score of

[41]This number refers to 2012 and the importance of SAT for college admission has grown over the years

[42]the admission to college in Brazil is to a major-college combination rather than just to a college. To simplify for the reader who are not used to the Brazilian context I'm calling any college-major combination a different college

enrolled student.

In the appendix, Table A.8 presents the impact of the award on enrollment in tertiary education (and Figure 1.8 the corresponding RD plot). The award increases by .31 the likelihood the student enroll in a selective college (based on column 4 where the outcome is whether the student enrolled in a college that exceeds the 70th selectivity percentile). This represents an increase of 11% relative to the control group mean.

Benchmark for Magnitude of Effects: Increase in earning using college wage premium estimates. The impact of the award on classmates' probability of enrolling in a selective college allows me to construct a rough estimate of the increase in total earnings that results from one additional award given (relative to the control classroom). To do so, I combine the causal impact of the award on enrollment in college with relevant statistics and non-causal estimates from other sources - Ferreira et al. (2014) and **?**. Specifically, I use college wage premium estimates from a Mincer equation (non-causal) assessed in 2012, the average dropout rate in selective colleges in Brazil, and average per capita income in 2012. Based on these estimates, the impact of the award is equivalent to an increase in the average classmate's annual earnings of about 39 reales (2005 CPI), 0.5% of per capita income in Brazil.[43] Since the winner has, on average, 30 classmates, the aggregate effect of the spillover is substantial. For each award given, the overall increase in classmates' annual earnings 1170 reales, equivalent to expanding the main conditional cash transfer program in Brazil to one additional beneficiary.[44]

To sum up, although the impact on classmates has smaller magnitude and is less

[43]Ferreira et al. (2014) estimate the college wage premium and household per capita income in 2012. The ratio between college graduates wage earnings relative to individuals with secondary school is about 2.85 and household monthly per capita income is 670 reales (2005 CPI). Estimates of selective college dropout rate comes from Binelli et al. (2008). The authors report a dropout rate of 20% for students enrolled in public universities which are often the most selective ones. My estimates show that the impact of the award on the winner's classmates likelihood of enrolling in selective colleges is .0031 percentage points. Therefore, the increase in wage earnings for the average classmate = 39 reales =(Impact of award on classmates' enrollment in selective colleges) x [Difference in wage from college graduates and individuals with secondary education] x [Probability of graduating] = $.0031 \times [12 \times (2.71828)^{7.629} \times (1 - .35)] \times [.80]$. In the absence of college wage premium for selective colleges I make a conservative assumption that the premium is the same for selective and non-selective college. The predicted impact on earnings is therefore a lower bound of what one should expect.

[44]around 15% of per capita income in Brazil (2005 CPI).

persistent than the impact on the participant, it is still economically relevant. The differences (magnitude and persistence) suggest that the mechanism that explain the spillovers are a subset of the ones that explain the impact on the recognized student. One possibility is that the labeling of success results in a "certificate effect" and a "self-signaling effect" for the recognized individual, while it only has a "self-signaling effect" for the classmates. For example, the students' may work as a self-signaling by changing students' perceptions about ability, goals within reach and interests. In the next section, I discuss and assess empirical support of different mechanisms explaining the spillover results. The evidence is consistent with the "self-signaling effect" explaining at least in part the spillover results.

1.5.3 What explains the spillovers to classmates?

I first present evidence consistent with students' and (potentially teachers') behavioral changes explaining the spillovers. Exploiting proximity between a classmate and the recognized individual, in terms of ability and physical proximity, I argue that the spillovers are at least in part driven by classmates' changes in their perceptions about ability, goals within reach or interests - which I denominate a change to a high-performance mindset. I then take a step back and discuss alternative mechanisms. I find evidence against alternative stories; For example, the award had no impact on measures of peers' quality or of teachers' quality that were assigned to the recognized individual's classroom. In this section I will focus entirely on performance in the Math Olympiad as the outcome of interest. This is due to three reasons: First, Math Olympiad outcomes are available for all grades which makes my sample 10 times larger than the equivalent sample for SAT, for example. Second, it is the only performance outcome where I observe the same student for several years. Third, in the causal chain between the award and the consequences for the various students' performance, it is likely that the Math Olympiad should be one of the first margins that are affected. The Math Olympiad is in fact the margin in which I find the strongest results.

If teachers' and participant's behavioral changes are driving the spillovers, then the impact should be larger for classmates that are physically close to the winner. I study

Table 1.3: *Heterogeneity of Award Impact on Classroom Assignment at Year $t+1$ by Pre-award Performance*

Panel A. Classmates	Student is at participant's classroom at t+1	
	(1)	(2)
Award	0.0084**	0.0086**
	(0.0040)	(0.0040)
Award x MO Performance at t		-0.0070
		(0.0068)
Students (obs.)	4,164,486	4,164,486
Classrooms (Clusters)	170,232	170,232
Dep. variable control mean	.5044	.5044
Bdw selection	.82	.82
Linear combination:β		.001689
Linear combination: s.e.		.007648
Controls	Yes	Yes
Sample Restriction: Student passed the grade	Yes	Yes

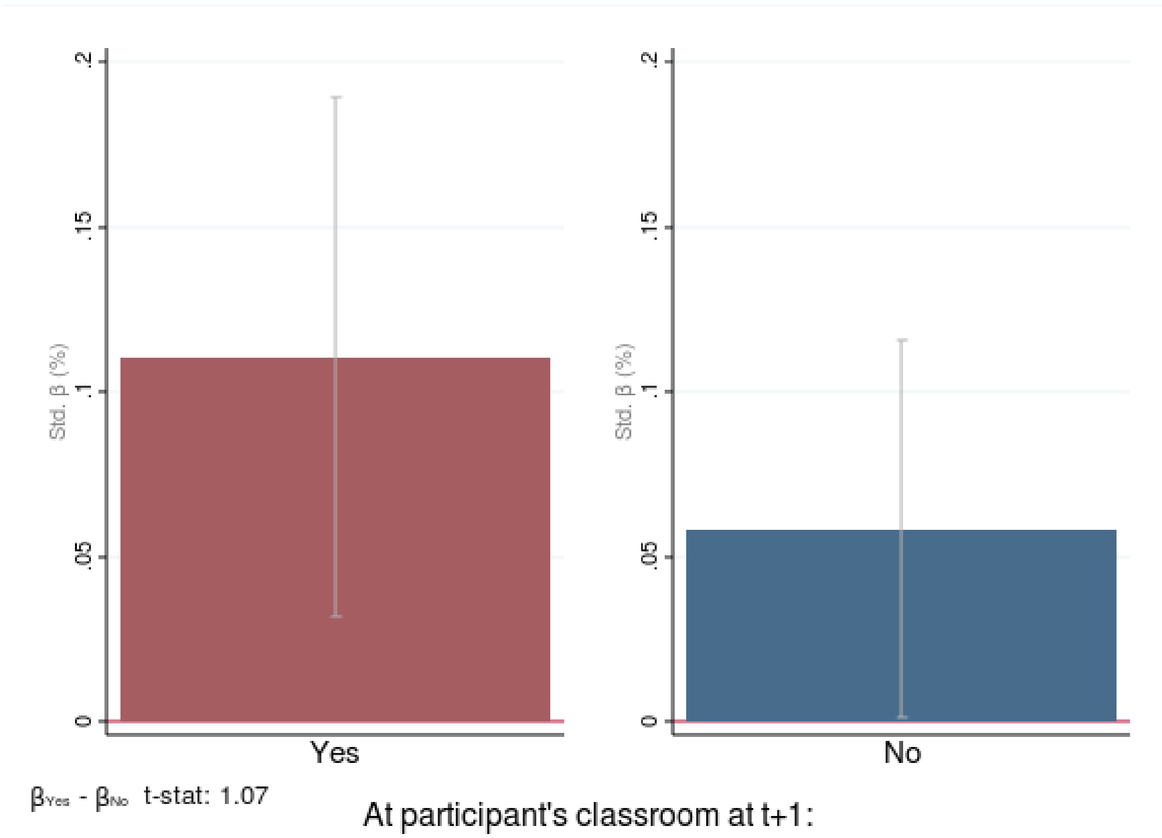
Notes: The table displays heterogeneous effects of the award impact on the summary measure at $t+1$ by pre-award performance in the MO.

whether physical proximity between a classmate and the participant matter for the spillovers. Table 1.3 presents the impact of the award on the likelihood the student continue in the participant's classroom at $t+1$. The award positively impact the likelihood the students continues in the classroom.[45] Relative to the control group mean of 50.5% probability of continue in the classroom, it increase the probability by 1.7%. This is evidence that the award increased the likelihood a classmate remained physically close to the participant. It is however unclear whether the continued presence in the classroom indeed explains the classmates' performance improvement. Figure 1.10 presents suggestive evidence that it does.

Since the award impacts the likelihood of continuing classmates, any heterogeneous analysis on whether the student continues in the classroom must be interpreted with cautious. For example, if the award impacts more high-ability students (relatively to poor performing ones) to continue in the classroom then, this bias the results in the direction of

[45]This table also present the results on whether the likelihood of continuing in the same classroom is different for low and high ability students. I discuss the findings of the heterogeneous analysis in Section 1.6.1

Figure 1.10: *Impact of award on MO performance at $t+1$ by Classroom assignment at $t+1$ (Classmates)*



Notes: This figure reports heterogeneous impact by classroom assignment at $t+1$. The outcome is equal to 1 if the student's MO score at Year $t+1$ exceeds the 50th percentile of national score distribution (0 otherwise). Outcome variable is well defined (1 or 0) for the entire sample.

finding a greater impact for those who continue in the same classroom. If this is the case, a positive association between continue in the same classroom and performance would not be the causal impact of being around the winner, but instead a result of top students being more likely to continue, and as shown, they are also more likely to experience a performance improvement due to the award. However, Table 1.3 present evidence that the bias is likely negative: Table 1.3 shows that students who had participated in the Math Olympiad in previous years are, if anything, less likely to continue in the same classroom compared to the average classmate. I therefore present heterogeneous analysis by whether the student continue in the same classroom.

Figure 1.10 presents the results on whether the impact on classmates' performance in the Math Olympiad varies by whether the student continue in the participant's classroom at $t+1$. There are two patterns worth emphasizing. First, students who continue in the participant's classroom experience a improvement in performance that corresponds to $2\times$ the impact on students who do not continue in the same classroom. Observe that I'm restricting the sample to all students who pass the grade, and therefore this is not driven by student who dropout or fail the grade who would naturally not be in the participant's classroom at $t+1$. Despite the difference in magnitudes, the impact on classmates who continue in the classroom, and who do not, is not statistically different. Figure A.3 shows, for a balanced sample, that the overall relationship between continue in the classroom and experiencing greater performance improvement also holds for future years. For example, students that continue in the classroom at $t+1$ experience greater performance at $t+1$, but not at $t+2$. If, however, they continue at $t+2$ as well then they again experience greater levels of performance $t+2$. The results at $t+2$ however are also not statistically significant. These are suggestive that having continued exposure to the winner in the classroom leverages the impact on the award. The second patterns is that even students who do not continue in the classroom also experience a positive impact of the award on their performance. There is, therefore, a residual impact even for students who are not classmates of the winner at $t+1$. These results suggest that although teachers' and participant's behavioral changes potentially amplified the spillovers, at least in part the spillovers are driven by classmates' behavioral response.

The change in classmates behavior may have a variety of reasons. One possibility is that the awards lead to some classmates to change perceptions about their ability, goals within reach or interests. In order to test for whether this set of explanations has empirical support in the data, I rely on past literature. Chung's (2000) presents a theoretical framework of a "Role Model effect" - Precisely, it studies how the success of a student spills over to the peers by changing peers' behavior (due to her peers being updated about goals within reach or interests). In the framework, the change in behavior is the greatest depending on the following three conditions: Observability of successful individual's effort-ability; Similarity in terms of ability between peers and the successful individual; Social similarity between peers and the successful individual. In what follows, I discuss the intuition behind the conditions as well as the empirical tests associated with each of the conditions.

Observability of successful individual's effort-ability A student is more likely to update about the fact that the success of the recognized individual is relevant for him, if he can compare himself to the recognized individual in terms of ability. Moreover, the signal will be of greater precision if the student can also attribute the success to effort or ability. These two factors (observing effort and relative ability) are both facilitated if the student is in the same classroom of the recognized individual. Table 1.4 presents the impact of the award on school choice and MO performance at $t+1$ for the participant, her classmates and her grademates (grademates excludes participant's classmates). In the first panel, I report the impact on whether the student exceeded the median score in the Math Olympiad. Participant and classmates are more likely to score above the median as a result of the award. The award does not increase the likelihood that a grademate score above the median. Moreover, we can statistically reject that the impact on classmates is the same as the impact on grademates (t-stat:2.77). The presence of the participant in the classroom is therefore necessary for the performance improvement. This provides empirical support for the idea that observability of the successful individual's effort-ability is a important condition for the

Table 1.4: *Impact of Award assigned at Year t on School Choice and MO performance at $t+1$*

	=1, if MO score is above the median		
	(1)	(2)	(3)
	Participant	Classmates	Grademates
Award	0.0321***	0.0012***	0.0001
	(0.0047)	(0.0004)	(0.0002)
Classrooms (Clusters)	148,705	148,705	148,705
Dep. Variable Mean	.11	.013	.011
Bdw selection	.82	.82	.82
Controls	Yes	Yes	Yes
	Transfer to other school		
	(1)	(2)	(3)
	Participant	Classmates	Grademates
Award	-0.0092**	-0.0089***	-0.0074***
	(0.0042)	(0.0026)	(0.0025)
Classrooms (Clusters)	148,705	148,704	148,705
Dep. Variable Mean	.19	.21	.22
Bdw selection	.82	.82	.82
Controls	Yes	Yes	Yes

Notes: The specification is equivalent to equation 2.1. All regressions estimated at the classroom level. Column 1, refers to outcome for the participant. In Column 2 the outcome refers to the classmates. Since it is estimated at the classroom level, the outcome used is the average of each variable (transfer to school or performance in MO) for the participant's classroom excluding the participant herself. In Column 3 the outcome refers to the grade-mates (excluding classmates). Since it is estimated at the classroom level, the outcome used is the average of each variable (transfer to school or performance in MO) for grade-mates excluding the participant's classroom.

performance spillovers.

Similarity in terms of ability between peers and the successful individual. The success of a high-achieving student is likely to be informative about the chances of succeeding for another high-achieving student. Therefore, the students' behavioral response should be larger if the student is similar in ability to the recognized individual. The results presented in the previous section is consistent with this. The impact of the award is concentrated in outcome margins that are relevant to the top of the score distribution, precisely performance in the Brazilian SAT, in the Math Olympiad and enrollment in selective colleges. I also show that the impact on a summary measure which includes all available outcomes is driven by students that score in the top quartile of the score distribution (see Table 1.2).

Social similarity between peers and the successful individual. Social similarity between the student and the recognized individual can increase the salience of the information, resulting in greater updating for students that are socially similar along different dimensions. Table 1.5 presents the results on whether the impact on the classmates' Math Olympiad performance at $t+1$ varies by measures of social proximity to the participant. I present three measures of social proximity: classmates and participant have the same gender, classmates and participants have the same race, classmates and participants were in the same classroom at t-1 (as a measure of the length of their relationship). Columns 4, 5 and 6 present the results respectively for each of these dimensions. The impact of the award is not statistically different if the classmate is socially close to the winner - based on any of the three dimensions. In the appendix, Table **??** shows that this is also the case even if we restrict the sample to include only the respective minority group (female or non-white).[46] While this is not supportive of the "role model effect", it is also not evidence against. First, it is unclear which characteristic is salient in the context. For example, females and non-white are a minority among participants but still well represented relative to their overall representation in the classroom. Second, the estimation of heterogeneous effects requires the estimation of several additional parameters, and therefore, it suffers from power issues. In Table 1.5 columns 1, 2 and 3 I estimate heterogeneous effects with a more parsimonious specification not including all the appropriate interactions. The levels of the estimated coefficient are however different, and therefore, this simplification does not represent a good approximation of the more flexible one.

My interpretation of the results is that the award encourages students' and potentially teachers' effort. Moreover, taken together the classmates' performance result are at least in part explained by classmates' change in perceptions about ability, goals within reach and interests - which I label a change to a high performance mindset. It could be argued that

[46] I discuss the results reported in Table 1.5 columns 1, 2 and 3; as well as the heterogeneous analysis by gender and race below when I provide further tests on whether the results are driven by preference or beliefs.

Table 1.5: *Impact of Award assigned at Year t on Performance in MO at $t+1$ by classmates' social proximity to the participant (Classmates)*

Classmates	MO score above the median at t+1					
	(1)	(2)	(3)	(4)	(5)	(6)
Award	0.0004	0.0012**	0.0004	0.0010**	0.0016**	0.0012***
	(0.0003)	(0.0005)	(0.0003)	(0.0004)	(0.0007)	(0.0004)
Award x Same gender as participant	0.0016***			0.0003		
	(0.0003)			(0.0006)		
Award x Same race as participant		0.0007*			0.0000	
		(0.0004)			(0.0008)	
Award x Same classroom as participant at t-1			0.0017***			0.0000
			(0.0003)			(0.0006)
Students (obs.)	5,114,922	2,511,911	4,815,437	5,114,922	2,511,911	4,815,437
Classrooms (Clusters)	170,331	105,319	165,697	170,331	105,319	165,697
Dep. variable control mean	.01223	.01374	.01251	.01223	.01374	.01251
Bdw selection	.82	.82	.82	.82	.82	.82
Linear combination:β	.001991	.001865	.002111	.001325	.001631	.001197
Linear combination: s.e.	.0003488	.000489	.0003809	.0004383	.000572	.0005268
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Full interaction	No	No	No	Yes	Yes	Yes

Notes: The table displays heterogeneous effects of the award impact on on students' performance in the Math Olympiad at $t+1$ by classmates' social proximity the participant (assessed prior to the award). Panel A report results for the participant, and in Panel B, for the classmates.

the award impact students' performance for other reasons. Section 1.6.1 provides evidence against several alternative mechanisms.

1.6 Discussion

1.6.1 Alternative mechanisms

My results are consistent with the award impacting behavior of students and teachers in the classroom, as long as they are close to the winner, physically or in terms of ability. In the following I investigate the extent to which my results could be explained by resource based explanations.

The production of education depends on the effort of teachers and students but also, it depends on a variety of inputs - teacher's quality, quality of peers, availability of textbooks to mention a few. The award may have changed the extent to which these other inputs are available to the winners' classroom and this is what I call resource-based explanation. I present below evidence that the award did not impact the provision of two of the most important inputs in education, teachers' quality and peers' quality. In the appendix I provide additional empirical tests for other resource-based explanations for the results. In the online appendix, Table **??** shows that the award did not increase overall enrollment to the winners' school. A increase in enrollment would suggest that the school as a whole received greater attention from parents and perhaps greater funding. I also present evidence that the award does not impact the likelihood the winner transfer to a private school. Greater access to private schools, which are often of better quality, could explain the participant's performance improvement.

Teacher quality in the winner's classroom do not change. When a winner is announced in the school, it is possible that the best teachers choose to teach the winner. Testing for this is difficult as teacher's quality is known to be hard to measure and often uncorrelated with observable characteristics. Considering this, the best test for whether best teachers are sorting into the winners' classroom is to understand whether the other classrooms in the winner's grade-school do worse in following years. Table 1.4 shows that grade-mates are not negatively impacted by the award at $t+1$. If the pool of teachers are fix to the school, this is evidence against greater teacher sorting. In the online appendix, Table **??**, I present additional tests showing that the award does not impact the characteristics of the teacher that are assigned to the winners' classroom at $t+1$.

Student composition in the winner's classroom do not change. It is possible that the award increased the amount of tracking, changing the composition of students in the winner's classroom at $t+1$. If this is indeed the case, the increase in students' performance would be a result of having better peers, which could impact directly their learning, and also change the level of instructions, which has implications for students performance, especially at the top. Table 1.3 had already shown that the likelihood that a high ability student continue in the winners classroom as a results of the award, is not statistically larger than the average student. In fact, if anything, the likelihood to continue classmates for high-abilities seems smaller. This is evidence against tracking.

1.7 Conclusion

In this paper I exploit a natural experiment in education policy in Brazil to estimate how recognition of the accomplishment of a high-performing student affects the ex-post performance of the recognized student and her classmates. To do so, I draw on a variety of administrative data sources, including measures of students' performance throughout the ability distribution, from middle-school dropout up to performance on the SAT, as well as educational outcomes with long-run implications, such as enrollment in selective colleges.

The study has three main findings. First, the award increases the participant's subsequent performance in the Math Olympiad and increases her probability of enrolling in a selective college (although college results are statistically weak). Second, the impact of the award spills over to the participant's classmates. The impact on classmates is substantial and has long-run consequences, as the award impact classmates' enrollment in selective colleges by 10%. Third, I show that the spillover results are specific to the participant's vicinity, in terms of ability and physical proximity. Spillovers are found for students in the top quartile of the score distribution and are mediated by the continued presence of the participant in the classroom. My interpretation of the results is that the award encourages students' and potentially teachers' effort. Moreover, taken together the classmates' performance result are at least in part explained by classmates' change in perceptions about ability, goals within reach and interests - which I label a change to a high performance mindset.

One implication of these findings is that policies that recognize the accomplishment of a student, such as scholarships, certificates and grades, should take into account the local spillovers they generate. In particular, the results shows that the spillovers are local to students in the winners' vicinity, and are a effective policy to promote the high performance of other high-achieving peers. One limitation of this study, however, is that I cannot distinguish the importance of different aspects of success: prestige and information. Another limitation is due to the very nature of the variation I'm using - a natural experiment with limited degrees of freedom to distill the different mechanisms. While I'm confident that the spillovers are driven by behavioral responses in the classroom, separating exactly the contribution of different agents - teachers, winner or classmates - is beyond the scope of this paper. In future work, I hope to address these shortcomings, as well as examine the further implications of recognition for classroom dynamics.

Chapter 2

Political Turnover, Bureaucratic Turnover, and the Quality of Public Services

2.1 Introduction

Countries differ in the extent to which politicians have discretion or control over the bureaucracy, in particular the extent to which politicians control the appointment and turnover of public employees within the bureaucracy. One of the first cross-country datasets on bureaucratic structure (Evans and Rauch, 1999) documents that in many East Asian countries, as well as in India and in Argentina, only the top chiefs and vice-chiefs in the core administrative agencies of the country are appointed by the president (or its equivalent). On the other end of the spectrum, in Israel, Haiti, Nigeria, and Brazil, almost all of the top 500 positions in the core government agencies are politically appointed by the president. Furthermore, political control over the bureaucracy can extend beyond the highest positions in the administration. In the country we study, Brazil, the president, state governors, and mayors make anywhere from 15,000 to 105,000 appointments to the federal, state, and local bureaucracy, respectively, once they enter office.[1]

A potential cost of having civil service positions at the discretion of politicians may arise from the fact that this kind of discretion links together bureaucratic turnover and political turnover. Given that the bureaucracy is the central agency responsible for the provision of public services, what is the effect of political turnover, and any subsequent disruptions to the bureaucracy, on the provision of public services?

We study this question in the context of public education provision by local governments in Brazil. We focus on this particular public service and context for several reasons. First, education is a key public service and it is a significant factor in macroeconomic growth and individual earnings (Barro, 1991; Card, 2001). In Brazil, education expenditures constitute 6% of GDP (World Bank Indicators, 2012). Second, local governments are the main providers of primary education in Brazil and spend 30% of their budget on education provision. Furthermore, local politicians have considerable discretion over the public education system and the appointment of public school personnel, such as headmasters and teachers. This allows us to analyze the research question of interest in this context: What is the effect of a change in the political party in power at the municipal level on the provision of public education in an environment where the municipal government has considerable influence over the education bureaucracy?

To estimate the causal effect of political turnover on education quality, we rely on a regression discontinuity design that uses close elections as an exogenous source of variation in political party turnover. We use this identification strategy because a comparison of outcomes in municipalities that experience a change in the ruling party to those that do not may give biased estimates of the impact of political party turnover. For instance, in a municipality with an incompetent ruling party, quality of public services are likely low and the constituency is likely to vote for a change in the ruling party during elections. To identify the causal impact of political party turnover, we compare outcomes in municipalities where

[1]See Evans (1995) for presidential political appointees and the survey of bureaucratic structure (*Pesquisa de Informações Básicas Estaduais/Municipais*) conducted by the Brazilian Census Bureau (*IBGE*) in 2012 for state and municipal political appointees.

the incumbent party barely loses (and, hence, there is political party turnover) to outcomes in municipalities where the incumbent political party barely wins (and, hence, there is no political party turnover). The identification assumption is that in municipalities with close elections, political turnover is essentially as good as randomly assigned and indeed we find evidence in support of this identification assumption.

Political party turnover reduces the quality of education in Brazilian municipalities. We find that party turnover lowers test scores, as measured one year after the election, by .05–.08 standard deviation units in terms of the individual-level distribution of test scores. We also find that party turnover increases the replacement rate of headmasters and teachers by 28 and 11 percentage points, respectively, one year after the election. We explore the heterogeneity in our results with respect to municipal-level income since prior work by Bursztyn (2016) has found that low-income voters in Brazil do not prioritize investments in public education. The effect of political turnover on the replacement rate of school personnel is approximately two to three times larger in low-income municipalities. Political parties appear to exercise considerably more discretion over school personnel in low-income areas.

Political party turnover reduces test scores and increases the replacement rate of school personnel regardless of whether the winning party is ideologically to the left or to the right. This finding implies that the effect of party turnover on test scores and personnel replacements is not driven by general shifts in political ideology in the particular elections we study.[2]

Does the disruption in the assignment of school personnel cause the negative impact of political turnover on students' test scores or does party turnover lead to other changes in the municipality that then drive the negative effect on test scores? To understand this better, we exploit the fact that the municipal government does not control all schools to

[2]If in the particular elections we study, 2008 and 2012, there were overwhelming shifts from the right to the left, for example, one could argue that our estimated effect of political party turnover on educational provision is picking up the effect of an ideological shift. Given that previous work has shown a link between party ideology and adoption of policies/economic outcomes (Pettersson-Lidbom, 2008), this would be a valid concern. However, by showing that the effect of political party turnover on outcomes is independent of the ideology of the winning political party, we can rule out such an argument and provide evidence that we are indeed estimating the effect of a change in *any* political party.

conduct a "placebo" exercise. We find that for local schools not controlled by the municipal government, i.e. non-municipal schools, a change in the political party of the municipal government does not impact the replacement rate of school personnel or student test scores. This finding rules out an effect of political turnover on student achievement due to any shocks that are common to the entire municipality, such as municipal-level changes in income or crime. Instead, the placebo exercise shows that political turnover negatively impacts student outcomes due to the discretion of the municipal government over the municipal education bureaucracy and the resulting disruptions in the assignment of school personnel.

In addition to the placebo exercise, we present two other pieces of evidence consistent with party turnover impacting student achievement through the politically caused disruption in the school. First, school personnel in municipalities with a new political party are more likely, compared to those in municipalities with no party change, to answer negatively to a series of survey questions regarding the offering of school programs for students, the availability of and participation in teacher training and teacher council meetings, and the degree of collaboration between school personnel. Given that high teacher turnover rates are linked to lower test scores possibly due to disruptions in the organizational cohesion of the school (Ronfeldt, Loeb, and Wyckoff, 2013), it is likely that politically caused changes in the assignment of school personnel disrupt school operations and management and, hence, lower test scores.[3] Second, we rule out an alternative explanation for how political turnover may affect students: changes in financial resources. One could argue that when new parties comes to power, their candidate is less experienced or they undergo a transition period in raising revenue or managing financial resources – and this may impact the quality of public education. However, we do not find evidence that party turnover impacts the access to or the allocation of education resources at the municipality or school-level. Taken together, the placebo exercise, the surfacing of problems in school operation and management, and the

[3]Some examples of how school personnel turnover may disrupt the organizational cohesion of the school are: loss of school-specific human capital, interrupted school programs, and lessened collaboration among school personnel.

lack of evidence that education resources are impacted suggest that party turnover affects student achievement through the (politically caused) disruption in the school.

Prior literature has highlighted patronage and short-horizoned incentive structures as potential costs of political control over the bureaucracy (Weber, 1922; Rauch, 1995); our paper highlights another cost of such bureaucratic structure.[4] By tying the turnover of service delivery personnel to the turnover of politicians, political discretion over the bureaucracy means that political turnover will *disrupt* the process of public service provision. One component of this disruption is closely linked with patronage: newly-elected politicians may use their discretion over the bureaucracy to award public employment based on political affiliation rather than merit (Folke, Hirano, and Snyder, 2011; Colonnelli, Prem, and Teso, 2016). In fact, in our setting, we suspect some patronage is at play since municipalities with a new party in power have less experienced headmasters and less educated teachers. However, independent of this patronage component of disruption, the linking of political and bureaucratic turnover creates instability in the process of public service provision. In our study, political turnover and the subsequent turnover of school personnel disrupt school programs, teacher training, and relationships within the school. Of course, political control over the bureaucracy has potential benefits as well, such as allowing politicians to form cohesion between the executive and the administration (Gulzar and Pasquale, 2016).[5] But, this benefit of political discretion over the bureaucracy is often mentioned in relation to high-level bureaucrats. It is less clear why cohesion between politicians and low-level personnel involved in public service delivery (such as school headmasters and teachers) would ease policy implementation. Our study highlights that, within a system where the

[4]The cost we uncover is economically meaningful. The negative effect of political party turnover on test scores in Brazil (which the evidence suggests is due to political discretion over the bureaucracy) is approximately one-third of the impact of some of the most successful education interventions, such as providing smaller classrooms or incentivizing teachers through performance pay (Krueger, 1999; Muralidharan and Sundararaman, 2011a).

[5]Another potential benefit of political discretion over the bureaucracy is that it allows politicians to provide incentives and accountability to bureaucrats. Raffler (2016) directly studies this potential benefit using a randomized control trial in Uganda. In addition, there is a literature on how politicians respond to electoral incentives, for instance, by reducing corruption (Ferraz and Finan, 2011b). Presumably, this requires the cooperation of bureaucrats and the administration.

bureaucracy is not shielded from the political process, political turnover disrupts the process of public service delivery and has a negative net impact on a welfare relevant outcome: student test scores.

The remainder of the paper is structured as follows. Section 2.2 describes the relevant institutional details of Brazilian municipal governments, the education system, and the link between the political process and the education system. Section 2.3 describes the data sources used and the steps we take to select our sample. Section 2.4 outlines the empirical strategy, discusses the identification assumption, and provides evidence in support of the identification assumption. Section 2.5 shows the main results of the effect of political party turnover on student achievement, the effect of political party turnover on the replacement of school personnel, and the connection between these two findings. Section 2.6 sheds light on the mechanisms by which political turnover translates to worse outcomes for students. Section 2.7 concludes.

2.2 Context

We use party changes in mayoral elections in Brazil to study the effect of political party turnover on the provision of a key public service, education. This section provides relevant details on municipal elections and municipal governments in Brazil. It also describes the education system and the link between municipal governments and the education system.

2.2.1 Brazilian Municipalities

There are 5,563 Brazilian municipalities (as of 2008). Municipalities are highly decentralized, autonomous, and responsible for key public services such as education, health, transportation, and sanitation.[6] Mayors are elected in municipal elections that are held every four

[6]Brazil is highly decentralized in terms of the provision of public services. However, in terms of raising revenue, municipalities rely mostly on transfers from the higher (state and federal) levels of government (Gardner, 2013).

years on the same day across the country.[7]

Municipal employment is a large part of public sector employment and has been growing in recent years. Municipal employment was 47% of public employment in 2002 and 52.6% of public employment in 2010 (Instituto de Pesquisa Econômica Aplicada, 2011). The appointment of personnel to municipal employment takes two forms. Approximately 68% of municipal employees are civil servants (Relação Anual de Informações Sociais, 2010). They have passed a civil service exam (*concurso público*) and have tenure. The remainder of municipal employees are hired on contract. The use of contract workers is meant to allow municipalities more flexibility and control so that personnel can be hired faster or with particular qualifications that are missing from the pool of those who have passed the civil service exam. However, the mayor must be able to provide justification for hiring contract workers and may be investigated if misconduct is detected.[8]

2.2.2 Brazilian Education

One of the main responsibilities of municipal governments is the provision of public education. Under Brazil's Law of Educational Guidelines (Law 9394) municipalities are responsible for basic education (early childhood and elementary education), while states and the federal governments are responsible for providing higher levels of education. Municipalities can also provide middle schools so long as they fulfill their responsibilities toward basic education foremost. We focus on primary education (elementary and middle schools) due to the availability of test score data. Overall, 14% of primary schools are private schools, less than 1% are controlled by the federal government, 18% are controlled by states, and 68% are controlled by municipalities.[9] For municipal schools, the municipal government

[7] Mayors are term-limited: they can hold office for two consecutive terms. Political parties are, of course, not term-limited.

[8] For instance, mayors in 86 cities in the state of Paraíba had criminal and civil complaints filed against them for hiring 20,000 contract workers under the guise of exceptional public interest in 2012 [http://www.diariodosertao.com.br/noticias/paraiba/79267, accessed March 2014].

[9] The vast majority of students in Brazil, 76.8% are enrolled in public schools (Brazilian National Household Survey, 2011).

serves as the school district. However, the funding of education comes primarily from higher levels of government. Most of the funds for education, especially those funds that ensure the daily operations of schools, come from a federal fund called FUNDEF, a non-discretionary fund that pays a fixed rate per enrolled student. Thus, the funding of the daily operations of schools is unlikely to be affected by political cycles or political alliances.[10]

The municipality is responsible for all decisions regarding the daily operations of the school: distribution of school lunches, providing school transportation, and the hiring, paying, and training of school personnel (teachers, headmasters, and administrators). Similar to the municipal bureaucracy more generally, 66% of teachers have passed an exam and have job security (although they can be transferred across schools). The remainder of teachers are hired on contract, at the discretion of the municipal government, and do not have job security. The mayor's office is allowed to hire teachers on contract to fill vacancies or find people with the appropriate qualifications.

Furthermore, approximately 60% of headmasters in municipal schools are politically appointed, as opposed to being selected through a competitive process or being elected by the school community. In Brazil, the position of headmaster is considered a "position of trust" (*cargo de confiança*), which means that politicians (can and do) appoint someone they trust to this position and hold considerable discretion over it. There are several reasons why local politicians may care about the school headmaster position. First headmasters are the managers of schools and the municipal government may want to provide incentives and accountability to such managers. Second, headmasters play a key role in enforcing the conditionality of the *Bolsa Familia* conditional cash transfer program. School-aged children must be in attendance for 85% of school-days in order for their family to receive this transfer and headmasters have discretion over whether school absences count towards non-compliance (Brollo, Kaufmann, and La Ferrara, 2015). And lastly, the headmaster position may be used to reward political supporters.[11]

[10]This is important in our setting given that we are studying the effect of political party turnover on education. Nonetheless, we investigate the effect of party turnover on education resources in Section 2.6.3.

[11]The headmaster position may be used to reward political supporters directly (i.e. patronage) or indirectly.

2.3 Data

We combine electoral outcomes for local governments with data on several aspects of public education. We first provide a brief timeline of when elections take place and when data is collected and then describe each of the data sources used in more detail.

2.3.1 Timeline

We focus on the 2008 and 2012 elections because some of our key outcome variables (student test scores and teacher assignments), first become available in 2007. As the timeline shows in Figure 2.1, municipal elections are held in October (every four years) and the mayor takes office in January of the following year.[12] The academic year begins in March and ends in December. We use two main sources to measure the quality of education provision: the School Census (*Censo Escolar*), which is conducted annually in May, and the nation-wide, standardized exam *Prova Brasil*, which is proctored every two years in November.

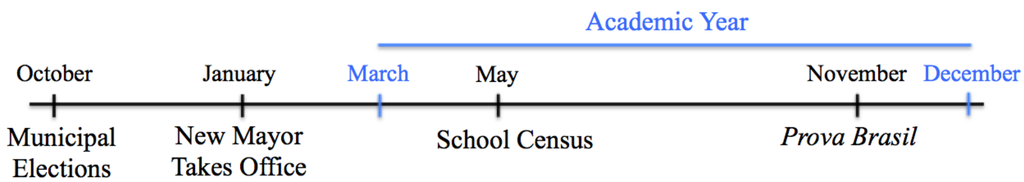
2.3.2 Electoral Data

The electoral data come from the Brazilian Superior Electoral Court (*Tribunal Superior Eleitoral, TSE*), which oversees all local, state, and federal elections in Brazil. We use electoral data from 2004, 2008, and 2012 to determine the incumbent party, the winning party, and each party's vote share in the 2008 and the 2012 municipal elections. This allows us to compute the running variable in our regression discontinuity design: the incumbent political party's vote margin, defined as the vote share of the the incumbent political party minus the vote share of the incumbent party's strongest opponent.

Since school management in Brazil involves an abundance of resources for food, transportation, and textbook programs, there is some anecdotal evidence that the headmaster position is used as a way to provide contracts to political supporters in the process of acquiring school supplies. See, for example, the following interview with the outgoing secretary of education for the state of Rio de Janeiro: http://oglobo.globo.com/sociedade/educacao/o-pais-nao-tem-mais-tempo-perder-discutindo-obvio-diz-wilson-risolia-14892991, accessed October 2016.

[12]Federal and state elections also take place every four years, but they are staggered to occur two years apart from municipal elections.

Figure 2.1: *Timeline of Election and Data Collection*



Notes: This timeline shows the timing of local elections and data collection. Municipal elections in Brazil are held in October every four years on the same day in all municipalities. The mayor takes office in January of the following year. The academic year runs from March to December. The School Census is collected annually in May and allows us to identify schools and measure the replacement rate of teachers. The Prova Brasil exam is a nation-wide, standardized exam and occurs every two years in November. We use Prova Brasil to measure student achievement, as well as the replacement rate of headmasters. Therefore, the measure of teacher replacement should be thought of as an evaluation of the education system 5 months after a new party has come to power and the measures of student achievement and headmaster replacement should be thought of as evaluations of the education system 11 months after a new party has come to power.

2.3.3 Education Data

The data on education comes from two sources made available by the National Institute for Research on Education (*Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira, INEP*). The first is the School Census (*Censo Escolar*), an annual survey of every school in Brazil (private and public). A large share of the educational budget is determined based on the enrollment figures in this census. Hence, the federal government frequently checks and audits the information in this census and misreporting has serious consequences. Therefore, this survey is a reliable source of information. We use the School Census from 2007, 2009, 2011, and 2013 to build a panel of schools with the following information: characteristics of the school (such as the quality of its infrastructure and whether the school is located in an urban or rural area), school-level dropout rates, school-level enrollment figures, school-level student characteristics (such as gender and whether the location of birth and residency are urban or rural), school-level teacher characteristics (such as gender, age, and education), and the movement of individual teachers. This last measure is one of our main outcome variables and is computed by comparing teacher rolls from the year before the election and the year after the election. More precisely, we compute the share of teachers that are new to the school by taking the pool of teachers in a given school the year after the election and checking to see if those teachers were present in the same school the year before the election. We also compute the share of teachers that have left a school by taking the pool of teachers in a given school the year before the election and checking to see if those teachers are present in the same school the year after the election.[13] The School Census is conducted in May and, therefore, any outcome measure from the Census should be thought of as an assessment of the education system five months after the new party has been in power.

Our second source of education data is *Prova Brasil*, a nation-wide, standardized exam administered every two years since 2007 to all 4th and 8th graders in public schools that have at least 20 students enrolled in that particular grade-level. We use *Prova Brasil* data from 2007, 2009, 2011, and 2013 (the most recent year with available data) to measure student achievement and the movement of headmasters. For each student, we average her math and Portuguese language test scores. To ease interpretation, we then standardize student test scores according to the individual-level distribution of test scores for students in municipalities that did not experience political party turnover in the most recent election cycle. When students take the exam, all students, the proctoring teachers, and the headmaster of the school complete a survey. We use the student surveys to obtain demographic characteristics of students (race, gender, and family background), which we use as controls in some specifications. We use the headmaster survey to construct our measure of headmaster replacement. The survey asks headmasters "How many years have you been a headmaster in this school?" We consider new headmasters to be those who report being the headmaster of their current school for less than two years. The exam is administered in mid-November and, therefore, any outcome measure from *Prova Brasil* should be thought of as an assessment of the education system eleven months after the new party has been in power.

[13]We cannot say whether teacher who have left did so voluntarily or were fired/transferred.

2.3.4 Municipal Characteristics and Political Ideology

We supplement our core election and education data with municipal characteristics from the census (*Instituto Brasileiro de Geografia e Estatística, IBGE*). We use this source to gather information on municipal population and municipal median income. We also use municipality-level public finance data, drawn from Ministry of Finance (*Ministerio da Fazenda*) to obtain data on municipal-level educational resources. Finally, we use data from *Atlas Político – Mapa do Congresso* to identify party ideology as belonging the left, center, or right.

2.3.5 Sample Selection and Summary Statistics

We take a number of steps to select municipalities into our sample. We start with 5,553 municipalities.[14] We consider only municipalities where political parties compete in *regular* elections. This means we drop 147 and 111 municipalities in 2008 and 2012, respectively, that had irregular elections due to, for instance, the death of a candidate or possible detection of fraud ahead of election-day. We also drop municipalities that can potentially go to 2nd round elections. Second-round elections can only occur if the municipality is above the 200,000 population threshold *and* no candidate wins the majority of the votes. Given that the average municipal population in Brazil is 33,000, this restriction drops a small number of municipalities: 124 and 132 municipalities in 2008 and 2012, respectively.[15]

Since the incumbent party's vote margin is the running variable in our regression discontinuity design, the incumbent political party must run for re-election to be included in our estimation sample. This is the case in approximately half of the municipalities. There are 35 political parties in Brazil and it is not uncommon for a political party to support the candidate of another party in a particular election instead of running its own candidate. Overall, we are left with 2,500 municipalities in 2008 and 3,114 municipalities in 2012. These municipalities constitute our sample.

[14] We lose ten municipalities because we are not able to match their electoral data to their education data.

[15] In the Appendix, we show that our results do not change if we include municipalities that could potentially go to 2nd round and use a fuzzy RD on the incumbent party's vote margin from the first round of elections as the running variable.

Appendix Table A.11 shows some descriptive statistics of the data. The unit of observation in this table is a municipality-election cycle. Column 1 shows municipal and school characteristics for all municipalities and Column 2 shows these same characteristics for municipalities in our sample. Our sample of municipalities is similar to Brazilian municipalities overall, with the exception that municipalities in our sample are smaller in terms of population and, therefore, have fewer and smaller schools. Column 3 of Appendix Table A.11 shows descriptive statistics for municipalities in our sample that have at least one school that participates in the *Prova Brasil* (PB) exam. A school must have at least 20 students enrolled in the 4^{th} or 8^{th} grade to participate in the national exam for that particular grade-level. This means that schools with *Prova Brasil* data are large schools and are more likely to be located in urban areas. The variables measured from the School Census (for instance, teacher replacement or dropout rates) are available for all schools in our sample (Column 2). Any measures that come from the *Prova Brasil* exam (student test scores or headmaster replacement) are available only for larger, more urban schools (Column 3).

2.4 Empirical Strategy

To estimate the effect of political party turnover on educational outcomes, we rely on a regression discontinuity design (RDD) for close municipal elections in Brazil. This section describes the details of our RDD identification strategy and provides evidence in support of the identification assumption.

2.4.1 Identification Strategy

To identify the effect of a change in the political party, we compare outcomes in municipalities where the incumbent party barely loses (thus there is political party turnover) to outcomes in municipalities where the incumbent political party barely wins (and there is no political party turnover). That is, we use a sharp regression discontinuity design for close elections.

Our main specification is a linear regression for close elections, where "close" is defined according to the optimal bandwidth selection of Calonico, Cattaneo, Farrell, and Titiunik

(2016). We estimate the effect of political party turnover on outcomes of interest by estimating the following equation at the individual-level or the school-level, depending on the outcome, for municipalities with close elections:

$$\begin{aligned} Y_{jmt+1} =&\alpha + \beta\mathbb{1}\{IncumbVoteMargin_{mt} < 0\} + \gamma IncumbVoteMargin_{mt} + \\ &\delta\mathbb{1}\{IncumbVoteMargin_{mt} < 0\} \times IncumbVoteMargin_{mt} + X'_{jmt}\Lambda + \epsilon_{jmt}, \end{aligned} \tag{2.1}$$

where Y_{jmt+1} is the outcome variable of interest (individual-level test scores or school-level headmaster/teacher replacements) in municipality m, measured one year after the election (election time t is either 2008 or 2012). The running variables of the RD is the incumbent vote margin, $IncumbVoteMargin_{mt}$, and it is computed as the vote share of the the incumbent political party minus the vote share of the incumbent party's strongest opponent. The treatment variable is $\mathbb{1}\{IncumbVoteMargin_{mt} < 0\}$, which is an indicator variable equal to one if the incumbent political party lost the election and, hence, the municipality experienced political party turnover. X_{jmt} is a set of controls that includes school-level baseline test scores and individual-level demographics (when the outcome variable is test scores), school-level characteristics, and an election-cycle dummy to control for a general time trend between the two election cycles.[16] Standard errors are clustered at the municipality level.

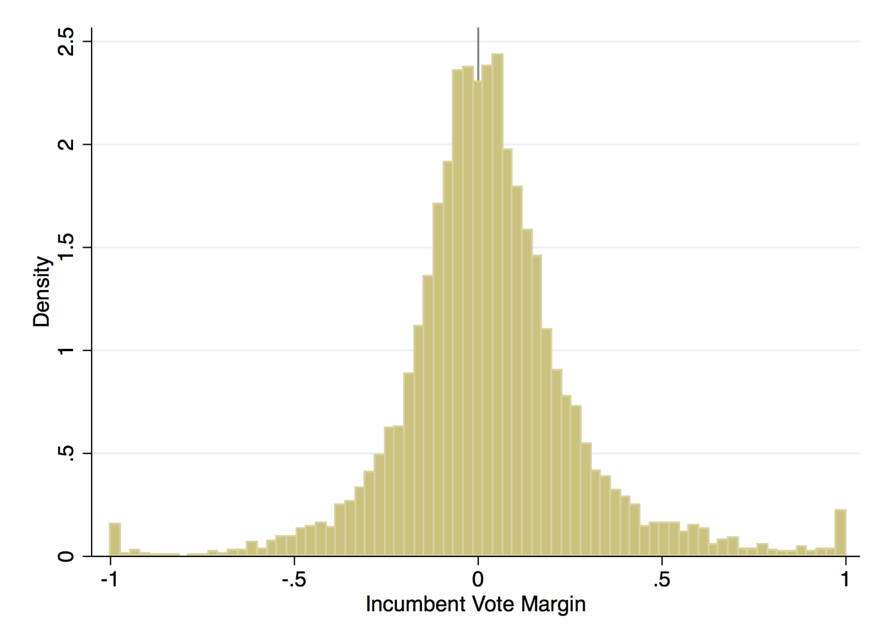
2.4.2 Identification Assumption

For Equation (2.1) to estimate the causal effect of political party turnover, the key identification assumption is that potential outcomes are continuous around the cutoff $IncumbVoteMargin = 0$ and, thus, any discontinuity in outcomes at the cutoff is the result of political party turnover. Essentially, the identification assumption is that in competitive elections, whether the incumbent political party wins or loses is "as good as" randomly assigned. To provide support for this identification assumption, we show that there is no evidence of sorting of the running variable $IncumbVoteMargin$ around the zero threshold and there is no evidence of

[16]We do not have a panel of students. We observe 4th and 8th graders every two years. We have a panel of schools and, therefore, control for the baseline, school-level average test score of the school we observe a particular student in.

discontinuity in covariates at the zero threshold.

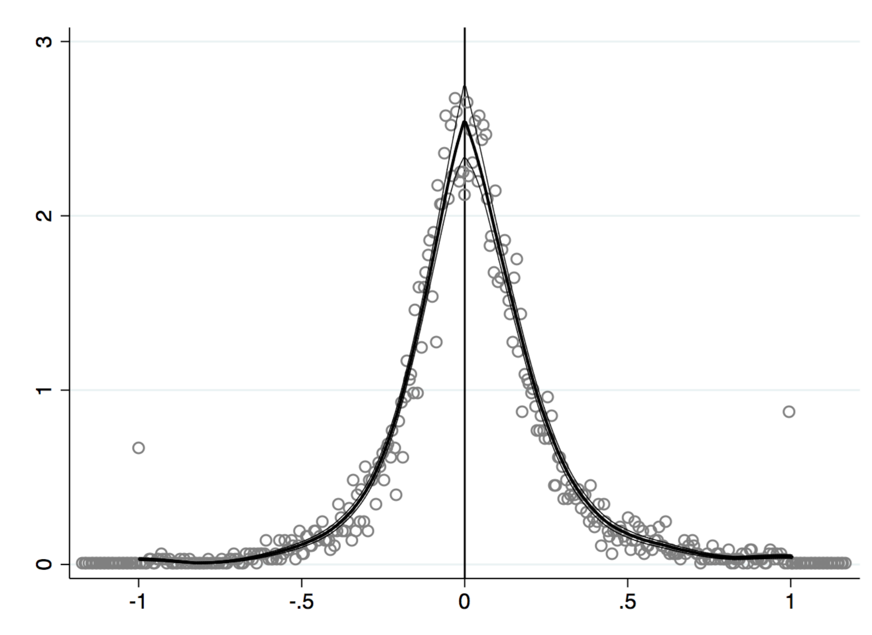
Figure 2.2: *Distribution of Incumbent Vote Margin*



Notes: This histogram shows the distribution of the running variable in the RDD, $IncumbVoteMargin$, in our sample of municipalities in the 2008 and 2012 election cycle. $IncumbVoteMargin$ is computed as the vote share of the incumbent political party minus the vote share of the incumbent party's strongest opponent.

Figure 2.2 shows the distribution of the running variable in our RDD, $IncumbVoteMargin$, for municipalities in our sample in both elections cycles. Municipalities with $IncumbVoteMargin < 0$ are those where the incumbent party lost its re-election bid and, hence, the municipality experienced political party turnover in the respective election cycle. Municipalities with $IncumbVoteMargin > 0$ are those where the incumbent party won re-election and, hence, the municipality did not experience political party turnover in the respective election cycle. The distribution of $IncumbVoteMargin$ seems fairly smooth around the $IncumbVoteMargin = 0$ threshold. In fact, a formal test for manipulation of the running variable fails to reject the null hypothesis that $IncumbVoteMargin$ is continuous at the zero threshold. Figure 2.3 shows this formal test, the McCrary Test (McCrary, 2008). The estimated discontinuity at the zero threshold is $-.0019$ (log difference in height) with a standard error of .0607.[17]

Figure 2.3: *McCrary Test for Manipulation of Incumbent Vote Margin*



Notes: This figure shows the McCrary Test for manipulation of the running variable in the RDD, $IncumbVoteMargin$. The test fails to reject the null hypothesis that $IncumbVoteMargin$ is continuous at the zero threshold. The estimated discontinuity is -.0019 (log difference in height) with a standard error of .0607.

Further evidence that lends support to our identification assumption is that we do not find evidence of discontinuity in covariates at the $IncumbVoteMargin = 0$ threshold. Columns 1 and 2 in Appendix Table A.12 show the mean value of 43 variables at baseline (one year prior to the election) for municipalities that did not have party turnover and municipalities that did have party turnover the year of the election in a close election. "Close" is defined as $|IncumbVoteMargin| < .09$ in this table.[18] This bandwidth corresponds to

[17]Further confirming our finding of no manipulation in the running variable is a study done by Eggers, Fowler, Hainmueller, Hall, and Snyder (2015). They analyze data from 40,000 close races in many different electoral settings, including Brazilian mayors in 2000-2008. They find no systematic evidence of sorting or imbalance around electoral thresholds and confirm that the relevant actors do not have precise control over election results in these settings (with the exception of U.S. House of Representative in the 2[nd] half of the 20[th] century).

[18]Approximately 40% of the municipalities in our sample fall within this bandwidth. Local elections in Brazil

the winning party receiving at most 54.5% of the votes and the losing party receiving at least 45.5% of the votes if there were two parties running in the elections.[19] The balance of covariates is not sensitive to the chosen bandwidth. Column 3 shows the p-value corresponding to the coefficient on $\mathbb{1}\{IncumbVoteMargin < 0\}$ in Equation (2.1) with the corresponding variable at baseline used as the outcome variable. As the p-values in Column 3 suggest, among 43 covariates, there is only one that displays a discontinuity at the $IncumbVoteMargin = 0$ threshold. Importantly, there is no discontinuity in our outcomes of interest (test scores and replacement rate of school personnel) at baseline. The absence of a discontinuity at the relevant threshold for baseline characteristics lends credibility to our identification assumption that political party turnover is "as good as randomly assigned."[20]

2.5 Results

Our main results, which we present below, show that political party turnover reduces students' test scores. The negative effect of political political party turnover on student achievement is not driven by selection or shifts in party ideology and persists up to three years after the election, at which time there is another election. Additionally, political party turnover increases the replacement rate of school personnel. This replacement occurs soon after the election (within a year) and seems to have a political component: political party turnover induces replacement of headmasters amongst politically appointed headmasters

are quite competitive.

[19]There are between 1-12 candidates/parties running in mayoral elections with an average of 2.7 and a median of 2 candidates.

[20]An additional threat to the validity of our empirical strategy is the possibility of manipulation of vote shares in close elections in a way that correlates with our outcomes of interest *but* does not result in sorting of the running variable around the threshold or a jump of covariates at the threshold. For instance, incompetent incumbent parties may be the least successful at manipulating close elections in their favor *and* the least effective at provision of public services. Therefore, municipalities where incumbent parties barely lose may have particularly bad public education. To address this concern, we check whether mean baseline characteristics shown in Appendix Table A.12 are systematically different in municipalities with and without party turnover in close elections – essentially a comparison of means instead of checking for a discontinuity in the $IncumbVoteMargin$ at the zero threshold (what Appendix Table A.12 shows). As Appendix Table A.31 shows, among 43 covariates, there are 6 variables with a significant mean difference across control and treated municipalities. Therefore, it is unlikely that such a threat to our identification is valid.

and municipalities that experience a change in the political party have lower quality school personnel (in terms of experience and education). Finally, we use a placebo exercise to provide evidence that political party turnover impacts student achievement due to political discretion over the education bureaucracy.

We show the RD plots using the optimal bandwidth for each outcome. Since we have several outcomes of interest and the optimal bandwidth is different for each of these outcomes, we also show the corresponding regression tables using the optimal bandwidth for the particular outcome under study and two other bandwidths (0.07 and 0.11) in an effort to keep the estimation sample fixed and, also, to show that our point estimates are not sensitive to the using bandwidth.

2.5.1 Political Turnover and Student Achievement

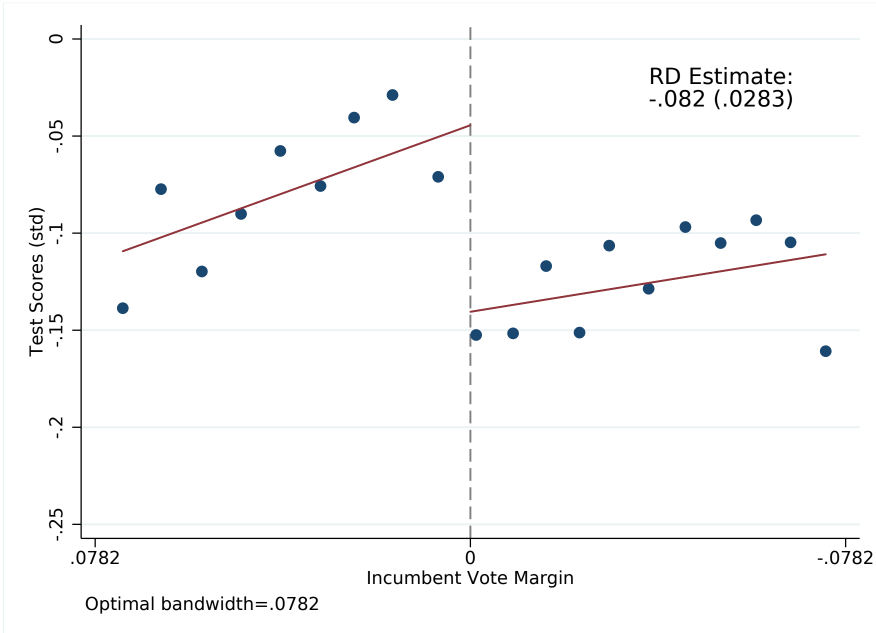
We estimate Equation (2.1) separately for 4^{th} and 8^{th} graders because all municipalities offer elementary schools but not all municipalities offer middle schools (usually the larger municipalities offer both elementary and middle schools).

Effect on 4^{th} Graders. Figure 2.4 shows 4^{th} grade test scores one year after the election (in 2008 or 2012) in municipalities with close elections.[21] Test scores for 4^{th} graders are lower in municipalities where a new political party has barely won (right hand side of the figure) compared to municipalities where the incumbent political party has barely stayed in power (left hand side of the figure). As Appendix Table A.13 shows, municipalities with a new party in office have test scores that are 0.08 standard deviations lower than comparable municipalities with no change in the political party. The estimated effect of political party turnover is robust to the inclusion of individual-level demographic controls, school-level controls, a dummy for the 2012 election cycle, and varying the estimation bandwidth.

Effect on 8^{th} Graders. The same pattern holds for 8^{th} grade test scores one year after the election, as shown in Figure 2.5. Eighth graders' test scores are lower in municipalities where

[21] Test scores are standardized based on the national distribution of test scores. Municipal schools are, on average, of lower quality compared to other public (state and federal) schools. Hence, the mean standardized test score for 4^{th} graders in municipal schools is less than zero.

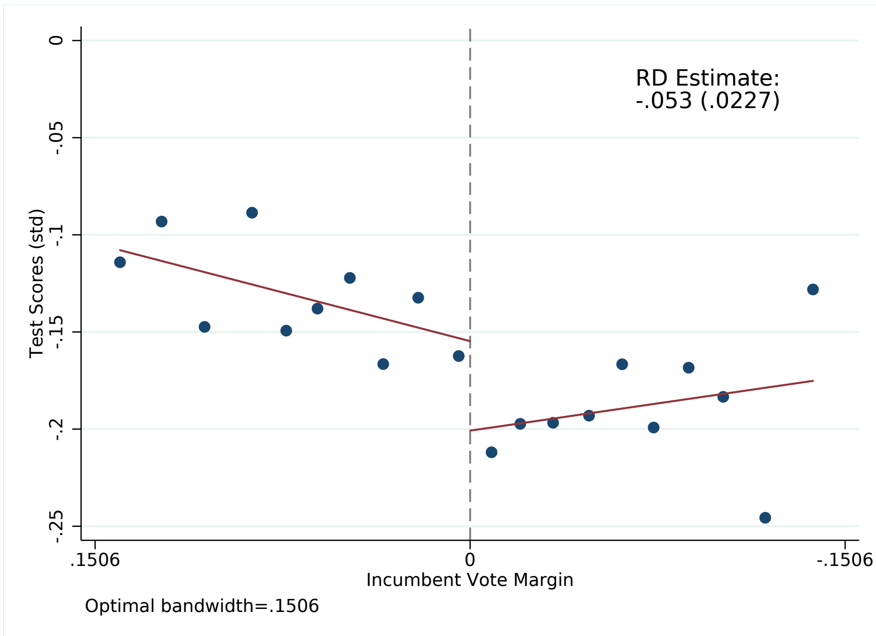
Figure 2.4: *Political Turnover and 4^{th} Grade Test Scores*



Notes: This figure shows the mean of individual-level 4^{th} grade test scores by bins of $IncumbVoteMargin$ (the size of each bin is 1.5 percentage points). Municipalities with $IncumbVoteMargin$<0 experienced a change in the political party of the mayor. Municipalities with $IncumbVoteMargin$>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 4^{th} grade test scores at baseline (the year before the respective election) is included as a control.

a new political party has barely won compared to municipalities where the incumbent political party has barely stayed in office. Appendix Table A.14 is the corresponding table and shows that test scores are 0.05 standard deviation units lower in municipalities with a new party in office. Again the effect of political party turnover on test scores for students in 8^{th} grade is robust to the inclusion of controls and varying the estimation bandwidth. One potential issue with test scores for 8^{th} graders is that the optimal bandwidth is very large: 0.151. This is presumably the case because there are fewer municipal middle schools. Nonetheless, municipalities with $|IncumbVoteMargin| < 0.151$ constitute 60% of the municipalities in our sample. Reassuringly, even when we restrict the estimation bandwidth to smaller bandwidths (Columns 3-6 in Appendix Table A.14), bandwidths that are closer to the optimal bandwidth for 4^{th} grade test scores, we still find a negative effect

Figure 2.5: *Political Turnover and 8^{th} Grade Test Scores*



Notes: This figure shows the mean of individual-level 8^{th} grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 8^{th} grade test scores at baseline (the year before the respective election) is included as a control.

of political party turnover on 8^{th} grade test scores.

Ruling out Selection. A particular explanation for the relationship between political party turnover and test scores observed so far may be that new parties often come to power on a platform to broaden access education. Hence, when new parties come to power, they systematically increase access to education *or* manage to reduce the dropout rate in a way that brings marginal students into the education system and, therefore, lowers test scores. Appendix Table A.15 shows the effect of political party turnover on the composition of students one year after the election. In terms of observable characteristics, students are similar in municipalities where the incumbent party (barely) lost and those where the incumbent party (barely) won. Furthermore, we estimate the effect of political party turnover on school-level dropout rates. One benefit of this measure is that it is available for all schools (as compared to information from *Prova Brasil*, which is available only for larger schools). Appendix Figure A.14 and Appendix Table A.32 show these results. Municipalities with political party turnover have 12% higher dropout rates compared to municipalities without political party turnover. However, this estimate is not statistically significant. Importantly, we do not find evidence that political party turnover *decreases* the dropout rate and, hence, gives rise to a relationship between political turnover and test scores that is due to selection. If anything, our estimate of the effect of political party turnover on test scores is an underestimate given that party turnover has a slight positive effect on dropout rates (assuming that students at the bottom of the distribution are the most likely to dropout).

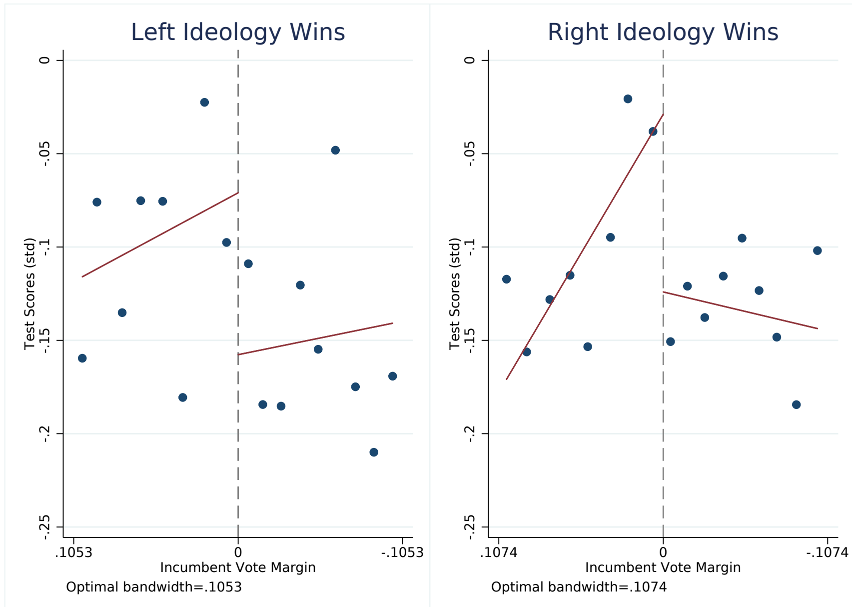
Heterogeneity with Respect to Party Ideology. Figures 2.6 shows the effect of party turnover on 4th grade test scores separately for municipalities where a left-leaning political party (barely) wins and those where a right-leaning political party (barely) wins.[22] Political party turnover reduces test scores regardless of the ideology of the winning party. Thus, the effect of political party turnover on test scores cannot be explained by general shifts in ideology that have been shown to impact the adoption of policies and economic outcomes in previous work (Pettersson-Lidbom, 2008).[23]

Persistence. Does the effect of political party turnover on test scores persist? This is an important question not only from a welfare perspective, but also to understand potential mechanisms. If political party turnover reduces student achievement initially but puts students on a better trajectory, then we would expect test scores to decrease the year after the election but begin to improve over time. Using the 2008 election, we can trace out the effect of political party turnover on test scores one, three, and five years after the election. We do not have a panel of students. Instead, we estimate the effect of party turnover in

[22]Appendix Figure A.10 shows the same analysis for 8th graders and the results are similar.

[23]There are municipalities that go from a left-leaning party to a right-leaning party and municipalities that move in the other direction in both election cycles. Thus it is not the case that there is persistence in the ideology of governing parties for a given municipality over time. This lack of persistence in ideology allows us to talk about "shifts" in ideology.

Figure 2.6: *Political Turnover and 4th Grade Test Scores in Municipalities where the Winning Party was from the Left vs. the Right*



Notes: This figure shows the mean of individual-level 4th grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 4th grade test scores at baseline (the year before the respective election) is included as a control. Party ideology is classified as belonging to the left vs. the right according to Atlas Político – Mapa do Congresso.

2008 on 4th graders in 2009, 4th graders in 2011 (who were in the 2nd grade when the 2008 election took place), and 4th graders in 2013 (who were in kindergarten when the 2008 election took place). Appendix Table A.16 shows how a change in the political party in 2008 affects 4th graders' test scores over time. The effect of political party turnover is most precisely estimated one year after the election.[24] Yet, as time passes, there is still a lingering negative effect of political party turnover on test scores. Although the estimated effect is

[24] Appendix Table A.13 and Appendix Table A.16 are different. The first table pools together the 2008 and 2012 elections and considers the effect of political turnover on test scores one year after the election (i.e. test scores in 2009 and in 2013). The second table shows the effect of political turnover in 2008 on test scores in 2009 in Columns 1-2.

not significantly different than zero in later years, we cannot reject that the effect of party turnover on test scores in 2009 is different than the effect in 2011 or 2013.[25]

Interpretting the Magnitude. The cost of political party turnover for students in municipal primary schools is large. Previous literature has shown that the conditional cash transfer program in Brazil, *Bolsa Familia*, which covered over 11 million families (about one fourth of Brazil's population) and cost 4 billion U.S. dollars per year in 2007, has increased enrollment, lowered dropout rates, and raised grade promotion, but has had *no* effect on student test scores – potentially due to the increases in enrollment rates (Glewwe and Kassouf, 2012; De Brauw, Gilligan, Hoddinott, and Roy, 2015). Quantifying the monetary value of our point estimate using interventions in the same context is difficult given the lack of an impact of the largest education policy in Brazil, *Bolsa Familia*, on test scores.[26] Hence, we look to another (similar) setting to benchmark our results. Angrist, Bettinger, Bloom, King, and Kremer (2002) finds that providing vouchers for private schools increases test scores by .2 standard deviation units at a total cost of $195 per student. If the municipal governments in our sample tried to offset the effect of political party turnover for one cohort of affected students (who experienced party turnover in 4^{th} grade and then again in 8^{th} grade) by carrying out a an intervention similar to that of Angrist et al. (2002), they would need to spend: $25 million U.S. dollars.[27] This calculation underestimates the cost of political party turnover on student achievement as it does not take into account the effect of party turnover in municipalities with non-close elections.

[25]Appendix Table A.33 shows the same results for 8^{th} graders. Because there are fewer municipal middle schools, we have significantly less observations (both in terms of individual students and in terms of clusters) when we limit our analysis to the 2008 election cycle. The negative effect of political party turnover on 8^{th} grade test scores is negative and persistent; however, the standard errors are large and the estimates are noisy.

[26]Mexico's conditional cash transfer program, *Progresa*, which was implemented as a randomized control trial unlike *Bolsa Familia* and, therefore, offers the opportunity for a more systematic analysis, has also been shown to have increased enrollment, with no significant impacts on test scores (Behrman, Sengupta, and Todd, 2000).

[27]This calculation is made using the following assumptions. We assume that raising one students' test scores in our setting would cost $195 multiplied by how our point estimate compares to that of Angrist et al. (2002): 0.08/0.2=.4. We then count the number of students in treated municipalities from our main regression: Appendix Table A.13, Column 1 and Appendix Table A.14, Column 1. In total, there are 324,885 students who experienced a change in the political party in a close election in 2008 and 2012. We arrive at $22 million by making the following calculation: $(.08/.2)\times195\times324{,}885=25{,}341{,}030$.

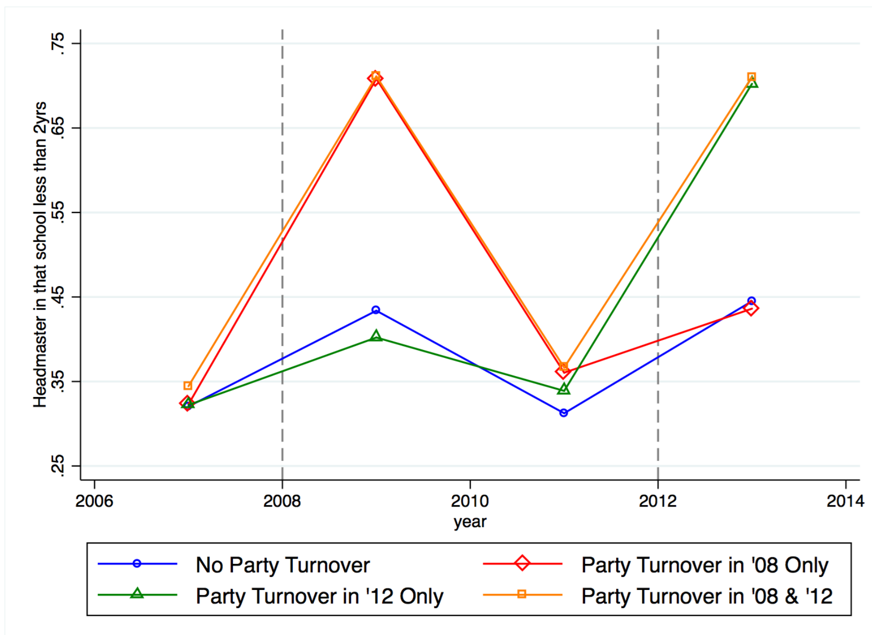
2.5.2 Political Turnover and School Personnel

Headmaster Replacements. Figure 2.7 shows how political party turnover affects headmaster replacements in all municipalities (not just those with close elections). This figure plots the share of headmasters that are new to their current school for schools in 4 different kinds of municipalities: municipalities that did not experience a change in the political party neither in 2008 nor in 2012, ones that experienced a change only in 2008, ones that experienced a change only in 2012, and ones that experienced a change in both election cycles. When a new party takes office, there is a sharp increase in the share of schools with new headmaster the following year. This event-study analysis is striking, yet it may be that when an incumbent party gets voted out of office with a large margin, the new party comes to power on a mandate to change the education system and, therefore, there is a sharp increase in the replacement rate of headmasters. So we estimate the effect of political party turnover on headmaster replacements for municipalities with close elections. Figure 2.8 shows the share of schools with a new headmaster one year after the election in municipalities where a new political party (barely) wins compared to municipalities where the incumbent political party (barely) stays in power. Appendix Table A.17 shows the corresponding regression results: political party turnover leads to an increase of 28 percentage points in the replacement rate of headmasters (64% of the mean headmaster replacement rate).[28]

Headmaster Characteristics. Using the *Prova Brasil* headmaster questionnaire, we explore how political party turnover affects the characteristics of headmasters in treated and control municipalities. Appendix Table A.18 shows that headmasters in municipalities that (barely) experience political party turnover are less experienced as headmasters (by 1.8 years or 35% of the mean years of headmaster experience) and slightly less likely to have graduate

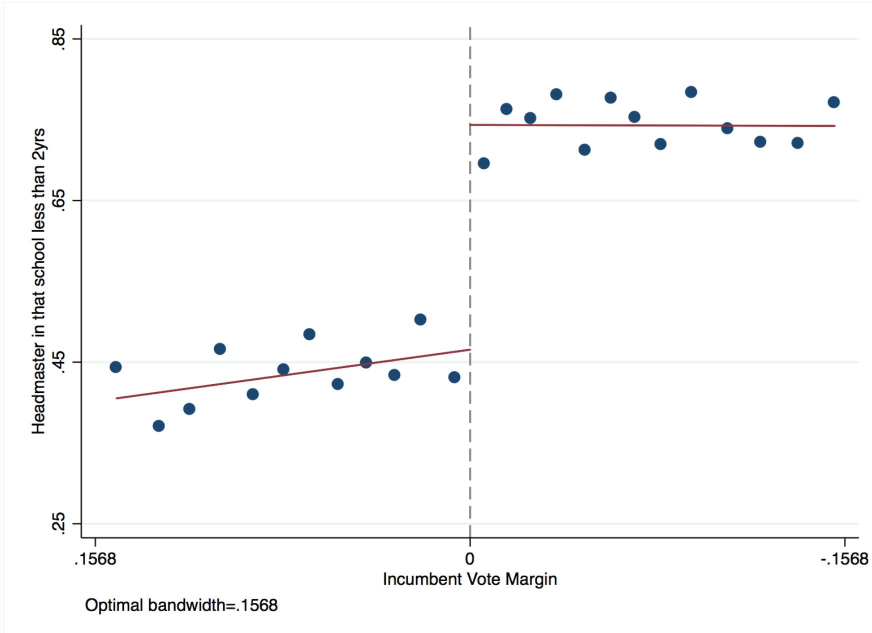
[28]The event-study analysis shows that political turnover increases headmaster replacements the year after the election. To illustrate the timing of headmaster replacements with causal estimates, Appendix Figure A.15 and Appendix Table A.34 show how political party turnover in 2008 affects headmaster replacements one, three, and five years after the election for municipalities that had close elections in 2008. In municipalities with a (barely) new political party, there is a sharp increase in the share of schools with a new headmaster only the year after the election. So it seems that the replacement of headmasters occurs soon after the new political party takes office in January.

Figure 2.7: *Political Turnover and Headmaster Replacement - Event Study*



Notes: This figure shows the share of schools with a new headmaster in municipalities that: did not experience party turnover in either election cycle, experienced party turnover only in 2008, experienced party turnover only in 2012, or experienced party turnover in both election cycles. New headmasters are those that report being the headmaster of their current school for less than two years on the Prova Brasil headmaster questionnaire.

Figure 2.8: *Political Turnover and Headmaster Replacement*



Notes: This figure shows the share of schools with a new headmaster by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. New headmasters are those that report being the headmaster of their current school for less than two years on the Prova Brasil headmaster questionnaire.

training (the equivalent of a masters degree).

In this context, another important headmaster characteristic is a headmaster's type of appointment. Headmasters in Brazil are chosen mainly by: selection through a competitive process (such as taking a civil service exam), election by the school community (i.e. parents and teachers), political appointment, or a combination of these (for instance, in Rio, the school community can vote among a few candidates who have passed the civil service exam).[29] The headmaster questionnaire asks the headmasters "How did you get to the headmaster position in this school?" Based on this question, we categorize the method

[29]There is heterogeneity within municipalities in terms of the mechanism by which the headmaster is chosen. We have not been able to fully understand where this heterogeneity comes from – although we suspect there is some historical dependence. Understanding this heterogeneity and its impact on the quality of public service provision would certainly make for interesting future research.

by which the headmaster was chosen as: selection, election, or political appointment.[30] In municipal schools, the most common method for choosing the headmaster is political appointment: 65% of headmasters (that we can categorize) respond that they are political appointees. We divide headmasters into two types: those who are political appointees and those who are not political appointees (i.e. they were selected or elected). Then we construct a categorical variable to indicate whether the headmaster in school s, at time t, in municipality m is a new headmaster *and* politically appointed:

$$y_{smt} = \left\{ \begin{array}{c} No\ Change\ in\ Headmaster\ (base) \\ Headmaster\ is\ new,\ not\ Political \\ Headmaster\ is\ new,\ and\ Political \end{array} \right\}$$

We use this categorical variable as the outcome in a multinomial logistic regression similar to our main estimation equation, Equation (2.1). Appendix Table A.19 shows the results from this regression with the referent (base) category as those schools where there is no change in the headmaster. Political party turnover significantly increases the relative risk of experiencing a politically appointed headmaster change by a factor of 3.67, or $e^{1.301}$. Although political party turnover also increases the relative risk of experiencing headmaster replacement for non-politically appointed headmasters, the magnitude is considerably smaller (a factor of 1.52, or $e^{.418}$) and the coefficient is only marginally statistically significant. Overall, political party turnover induces headmaster replacement mostly amongst politically appointed headmasters, which is in line with new political parties appointing new, politically appointed headmasters to schools under the control of the municipality.[31]

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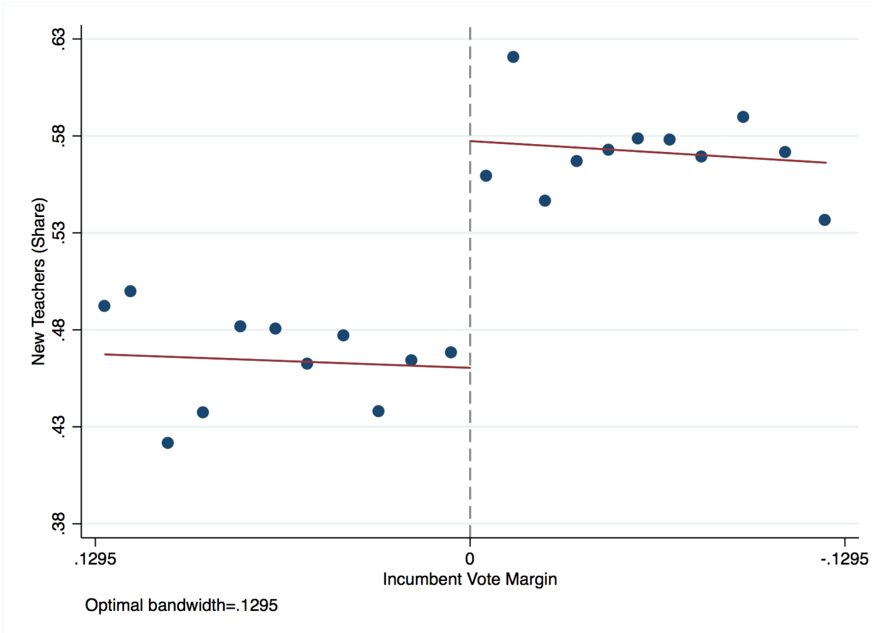
[30]More precisely, the survey responses are: selection (8%), election only (18%), selection and election (7.5%), technical appointment (15%), political appointment (31%), other kinds of appointment (15%), and other means (6%). Based on our analysis of school characteristics and conversations with the Former Secretary of Education in Rio, we categorize *any* kind of appointment (technical appointment, political appointment, and other appointment) as political appointment. However, our results are similar if consider political appointees strictly as those headmasters who choose political appointment on the survey.

[31]Anecdotally, such headmasters are often teachers within a school who are promoted to the headmaster position. Since they do not reach the headmaster position via civil service examination, they do not have job tenure as headmasters. Thus, when the political party that appointed them leaves office, they often go back to being a teacher.

Figure 2.9: *Political Turnover and New Teachers*

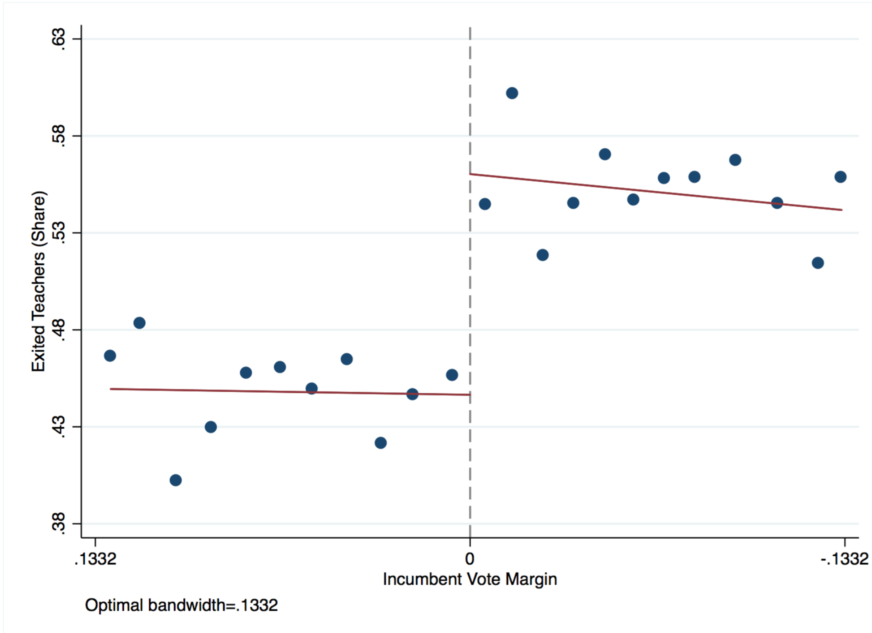


Notes: This figure shows the share of teachers that are new to a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that are new to a school is computed using the School Census and corresponds to the share of teachers in a school who are in that school at time t (one year after the respective election) but were not in that same school at time $t-2$ *(the year before the respective election).*

Teacher Replacements. Figure 2.9 shows that schools in municipalities with a (barely) new political party have a higher share of teachers that are new to the school one year after the election. Figure 2.10 shows that schools in municipalities with a (barely) new political party also have a higher share of teachers that have left the school one year after the election. The corresponding regressions are shown in Appendix Table A.20. Political party turnover increases the share of teachers that are new to a school by approximately 111 percentage points and increases the share of teachers that have left the school by approximately the same amount. Thus, it is not the case that new teachers enter the school once a new political party takes office and there is an inflation in the size of the teaching staff. Rather it seems that there is "reshuffling" of teachers across schools.[32] In fact, the number of teachers per

[32]Baseline teacher turnover is very high: as Appendix Table A.20 shows, the average share of teachers that

Figure 2.10: *Political Turnover and Teachers that have Left*



Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that have left a school is computed using the School Census and corresponds to the share of teachers in a school who were in that school at time $t-2$ (the year before the respective election) but are no longer in that same school at time t (one year after the respective election).

school is not different in municipalities with and without political turnover (Appendix Table A.21, Column 1).

Unfortunately, we cannot repeat the event-study analysis that we did for headmasters with teachers because the School Census did not track teachers in 2005, hence, we cannot compute the share of teachers that are new to a school/have left a school in 2007. Instead,

are new to a school is 46% in our sample of control municipalities. There are two reasons for such a high rate. First, this rate is computed over a two year period. The second reason has to do with the way that the market for teachers is organized in Brazil. Once teachers pass the civil service exam, they are called to work at any school with a vacancy. This school is often not the teacher's preferred location. Every year, there is an "internal selection process" (*concurso remoçã*) which allows teachers to choose a different school than the one they were initially assigned to. Thus a 46% teacher turnover rate is not uncommon in Brazil. In fact, we found several newspaper articles that document similar high turnover rates throughout Brazil. "Secretary of Education of São Paulo, Maria Helena Guimarães de Castro stated [teacher] turnover of 40% in the state system:" http://gestaoescolar.org.br/formacao/rotatividade-professores-483054.shtml, accessed October 2016.

Appendix Figure A.4 shows how political party turnover in 2008 affects teacher turnover one, three, and five years after the election to gain a better sense of how the effect of political party turnover propagates.[33] The corresponding table, Appendix Table A.35, shows that one year after a new party (barely) enters office, there is a sharp increase in the replacement rate of teachers. Three years after the election, the replacement rate of teachers is still higher in treated municipalities, so there is some persistence in the effect of party turnover on teacher assignments. However, the estimated coefficient is not statistically significant and the magnitude is half of the estimated coefficient for the effect immediately after the election. By 2013, at which time there has been another election, there is no effect of political party turnover in 2008 on teacher replacements.

Teacher Characteristics. The School Census contains demographic information on teachers: their age, gender, education-level, and type of contract (starting in 2011). Using this information, we test whether the composition of the pool of teachers in municipalities with and without political party change is different. Appendix Table A.21 shows that the share of teachers with a B.A. is 7.3 percentage points (or 15% of the mean value) lower in municipalities that (barely) experience political party turnover.[34]

Heterogeneity with Respect to Party Ideology. Figure 2.11 shows the effect of political party turnover on headmaster replacements separately for municipalities where a left-leaning party (barely) wins and those where a right-leaning party (barely) wins. Similar to the heterogeneity analysis for test scores, political party turnover increases the replacement rate of headmasters regardless of the ideology of the winning party. The corresponding figures for teacher replacements are shown in Appendix Figures A.6 and A.7 and show similar results. Thus, the effect of political party turnover on the replacement rate of school

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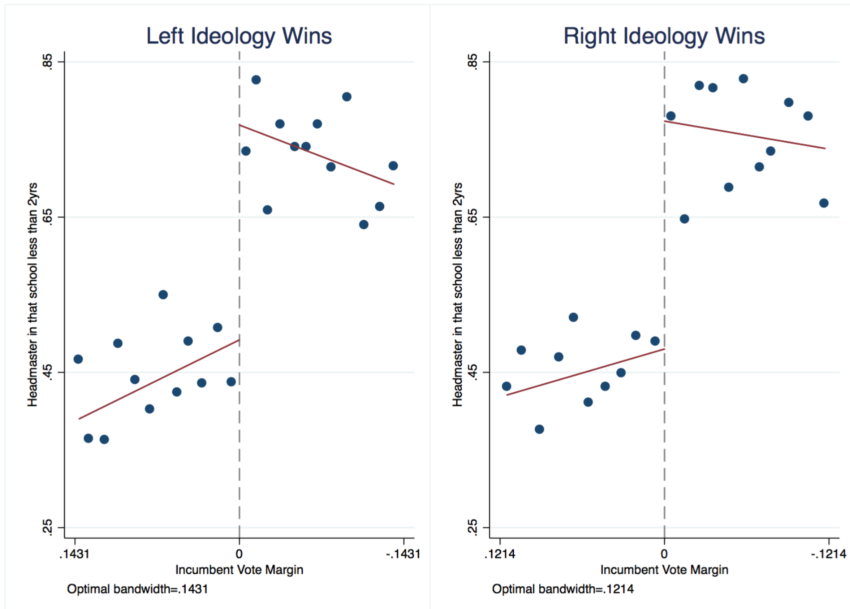
[33]This figure shows teacher turnover in terms of the share of teachers that are new to a school and Appendix Figure A.5 shows teacher turnover in terms of the share of teachers that have left a school. Both figures show similar patterns.

[34]However, this does not mean that over time the education level of teachers in Brazil is declining. In fact, between 2007-2013, the share of teachers with a B.A. increased from 37% to 63%. Starting in the late 1990s/early 2000s laws began to pass that required a B.A. in pedagogy for teachers and as older generations of teachers retire, the share of teachers with a B.A. is increasing.

Figure 2.11: *Political Turnover and Headmaster Replacement in Municipalities where the Winning Party was from the Left vs. the Right*



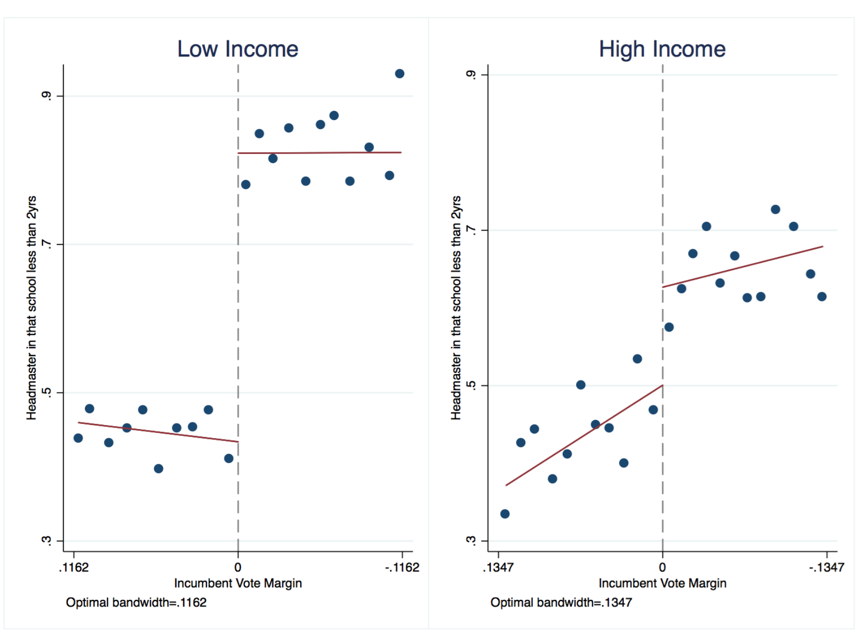
Notes: This figure shows the share of schools with a new headmaster by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. New headmasters are those that report being the headmaster of their current school for less than two years on the Prova Brasil headmaster questionnaire. Party ideology is classified as belonging to the left vs. the right according to Atlas Político – Mapa do Congresso.

personnel cannot be explained by general shifts in ideology.

Heterogeneity with Respect to Municipal Income. Anecdotal evidence suggests that parents do protest against politically motivated replacement of headmasters.[35] Prior work (Bursztyn, 2016) has shown that low-income voters in the same context prefer direct transfers to investments in public education spending. Hence, it is possible that parental resistance occurs more in high-income areas and dampens the discretion of politicians over the assignment of school personnel. We divide our sample of municipalities into the subset of municipalities with below median income and the subset of municipalities with above

[35]See for example: http://www.saocarlosagora.com.br/cidade/noticia/2013/04/30/41314/vereadores-afirmam-que-cargo-de-diretor-de-escola-e-de-livre-escolha-do-prefeito, accessed October 2016.

Figure 2.12: *Political Turnover and Headmaster Replacement in Low- and High-income Municipalities*



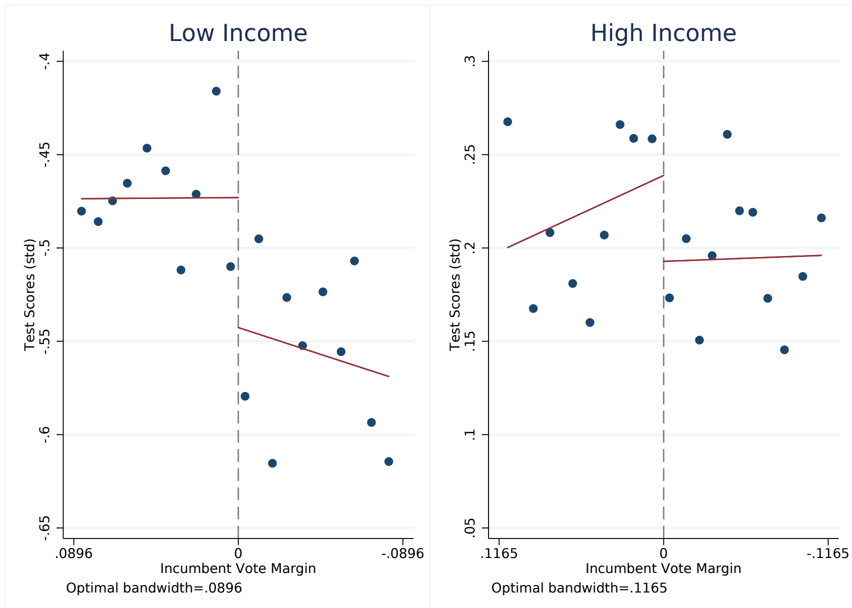
Notes: This figure shows the share of schools with a new headmaster by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. New headmasters are those that report being the headmaster of their current school for less than two years on the Prova Brasil headmaster questionnaire. Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

median income and estimate the effect of political turnover on replacement of school personnel separately for low- and high-income municipalities.[36] Figure 2.12 (Appendix Table A.22) shows that political party turnover increases the rate of headmaster replacement by 39 percentage points in low income areas and by 13 percentage points in high income areas. This difference is statistically significant. The effect of political party turnover on teacher replacements is also more pronounced in low-income municipalities (Appendix Figures A.8 and A.9 and Appendix Table A.36). The heterogeneity in the effect of political party turnover on assignment of school personnel suggests that political discretion over

[36]Our measure of income is the median of monthly household income within a municipality in 2000.

school personnel is higher in low-income municipalities.[37]

Figure 2.13: *Political Turnover and 4^{th} Grade Test Scores in Low- and High-income Municipalities*



Notes: This figure shows the mean of individual-level 4^{th} grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 4^{th} grade test scores at baseline (the year before the respective election) is included as a control. Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

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[37]Despite this heterogeneity in the effect of political party turnover on school personnel replacements with respect to income, Figure 2.13 and Appendix Table A.23 show that political party turnover reduces test scores in low (Panel A of the table) and high (Panel B) income areas. Although the estimated coefficients are more precisely estimated in low income areas, we cannot reject that the effect of political party turnover is the same in low and high income municipalities. Results for 8^{th} graders are shown in the appendix and conclusions are similar (Appendix Figure A.16 and Appendix Table A.37). One could argue that the negative impact of political party turnover on test scores should be larger in low-income areas if the relevant mechanism by which political party turnover impacts students is through personnel replacements. However, test scores in low-income areas are already very low and, presumably, more difficult to reduce even further.

2.5.3 Political Discretion over the Education Bureaucracy

So far, we have shown that a change in the political party of the mayor impacts the provision of public education in schools controlled by the municipality. In this section, we use local schools that are not controlled by the municipal government to perform a placebo exercise. We show that changes in the party of the mayor do not impact the rate of replacement of school personnel or student test scores in these *non-municipal* schools.[38]

Municipal governments control 68% of primary schools. The remainder of *public* primary schools are controlled by the state.[39] Most public elementary schools are controlled by the municipality, most public high schools are controlled by the state, and public middle schools are split half and half between municipal and state governments. When we consider the effect of changes in the mayor's party on headmaster replacement and student test scores in non-municipal schools, the set of non-municipal schools is comprised of state and federal schools (since only public schools participate in the *Prova Brasil* exam). When we consider teacher replacements as an outcome, the set of non-municipal schools is comprised of state, federal, and private schools (since all schools participate in the School Census).

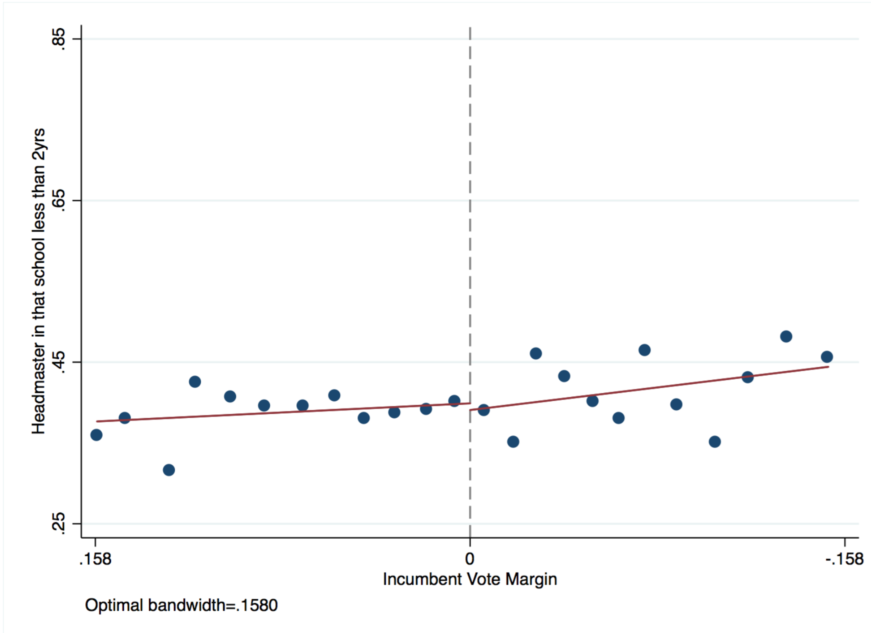
School Personnel in Non-municipal Schools. Figure 2.14 and Appendix Table A.24 show that when a new mayoral political party (barely) comes to power, there is no change in the share of non-municipal schools with a new headmaster. Appendix Figure A.11 and Appendix Table A.25 shows the same results for the share of teachers that are new to non-municipal schools.[40] The share of teachers that are new to non-municipal schools is slightly higher, 1.1 percentage points, in municipalities with a new political party in power. However, this increase is noisily estimated and is one-tenth of the increase in the same measure for municipal schools. The fact that we observe a small effect, although

[38] State and federal elections are held every four years as well, but with a 2-year gap from municipal elections. Thus we do not have political turnover in higher levels of government that coincide with our treatment of local political party turnover.

[39] The federal government controls less than 1% of primary schools. There are also private primary schools (14%).

[40] Appendix Figure A.12 show the results graphically for the share of teachers that have left non-municipals school.

Figure 2.14: *Political Turnover and Headmaster Replacement in Non-municipal Schools*



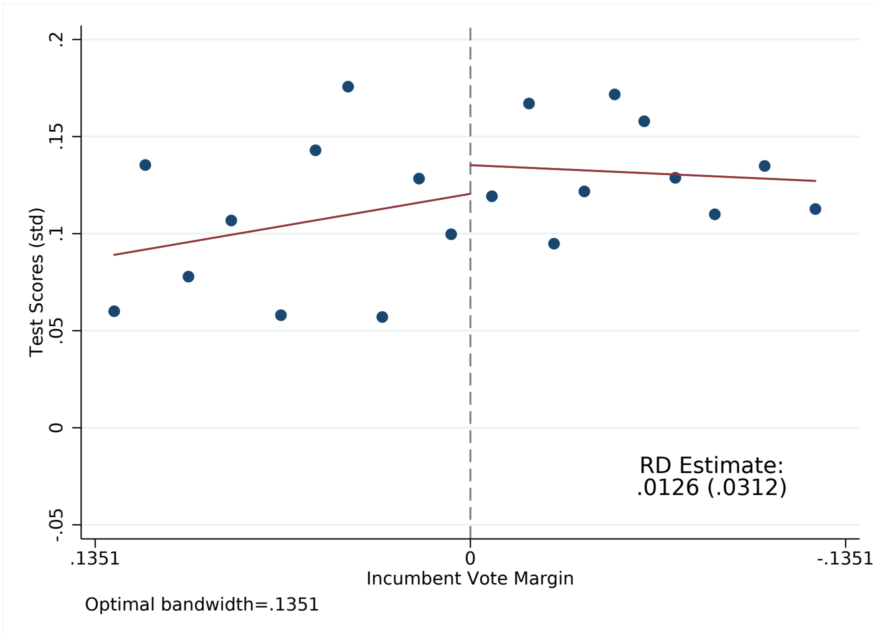
Notes: This figure shows the share of non-municipal schools with a new headmaster by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. New headmasters are those that report being the headmaster of their current school for less than two years on the Prova Brasil headmaster questionnaire. The set of non-municipal schools for this outcome is comprised of state and federal schools, since only public schools participate in the Prova Brasil exam.

not statistically significant, on teacher replacements in non-municipal schools is likely due to the fact that the teacher market for municipal and non-municipal schools is somewhat integrated and the disruption to the teacher market for municipal schools spills over into the market for teachers in non-municipal schools.[41] Overall, we see that changes in the mayor's political party have little to no effect on teacher and headmaster replacements in non-municipal schools.

Student Achievement in Non-municipal Schools. Appendix Figure 2.15 and Appendix Table A.26 show the effect of political party turnover in mayoral elections on 4^{th} grade

[41]In fact, 22% of teachers in non-municipal schools also teach in municipal schools. In Brazil, teachers may teach in more than 1 school since the school-day is only half of a day. In our sample, teachers teach in 1.3 schools on average.

Figure 2.15: *Political Turnover and 4th Grade Test Scores in Non-municipal Schools*



Notes: This figure shows the mean of individual-level 4th grade test scores for students in non-municipal schools by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 4th grade test scores at baseline (the year before the respective election) is included as a control. The set of non-municipal schools for this outcome is comprised of state and federal schools, since only public schools participate in the Prova Brasil exam.

test scores in non-municipal schools.[42] When a new mayoral political party (barely) comes to power, there is no statistically significant decrease in test scores for students in non-municipal schools. Importantly, we can formally reject that the effect of mayoral political party turnover on 4th grade test scores in municipal and non-municipal schools is the same with an estimated difference in coefficients of 0.095 and a p-value of .017.

One important issue is that municipal schools are worse quality schools than non-municipal schools: in 2007, for example, the average test score in municipal schools was .085

[42]We show the corresponding analysis for middle schools (i.e. 8th graders) in Appendix Figure A.17 and Appendix Table A.38. The results are similar: political party turnover in mayoral elections does not significantly reduce 8th grade test scores. Although we cannot formally reject that the effect of party turnover for 8th grade test scores is the same in municipal and non-municipal schools.

standard deviation units lower than in non-municipal schools. So it may be that political party turnover only reduces student achievement in low-quality schools. We check the heterogeneity of the effect of political party turnover on student achievement in municipal schools with respect to school quality. We divide our sample of municipal schools into low-quality schools (average school-level baseline test scores below median) and high-quality schools (average school-level baseline test scores above median). Appendix Figure A.13 and Appendix Table A.27 show the effect of political party turnover on test scores in low-quality municipal schools (Panel A of the table) and high-quality municipal schools (Panel B of the table). We see that the effect of political party turnover is negative in both low- and high-quality municipal schools. Although the coefficients are more noisily estimated in high-quality schools, we cannot reject that the effect of political turnover is the same in low- and high-quality schools. Therefore, the fact that we do not see an effect of political party turnover on student achievement in non-municipal schools cannot be explained by differences in school quality.[43]

What the Placebo Shows. Political party turnover in mayoral elections does not translate into disruptions in the assignment of school personnel or deteriorations in student achievement in non-municipal schools. The absence of an effect of mayoral party changes on test scores in non-municipal schools is not due to the fact that non-municipal schools are of better quality. These findings rule out an effect of political party turnover on education provision due to any changes caused by party turnover that affect the entire municipality (such as municipal-level changes in crime or income). Instead, the findings of this placebo show that political turnover in Brazilian municipalities negatively impacts student outcomes through political discretion over the municipal education system, the key difference between municipal and non-municipal schools. The findings of this section also provide suggestive evidence that political party turnover impacts student achievement through the replacement

[43]We show the result of the heterogeneity analysis with respect to baseline test scores for 8th graders in municipal schools in Appendix Figure A.18 and Appendix Table A.39. Since there are fewer municipal middle schools to begin with, we lose power when we divide the sample of 8th graders based on baseline test scores. However, there is no evidence that the negative effect of political party turnover on 8th grade test scores is driven by low-quality schools.

of school personnel: when political party turnover is not accompanied by a disruption in the school, there is no negative effect of political turnover on student achievement. However, the municipal government controls aspects of municipal education provision besides appointment of headmasters and hiring/transferring of teacher. For instance, the municipal government also controls education administrators and the disbursement of funds. Therefore, we cannot claim that the placebo exercise provides conclusive evidence that political turnover affects student achievement *only* through the politically caused replacement of school personnel: other aspects of education provision, which are also under the control of the municipal government, may be affected by political party turnover as well. In the next section, we explore some other potential mechanisms by which political party turnover may affect student achievement.

2.6 Mechanisms

How does political turnover and political discretion over the education bureaucracy translate into lower student achievement? The most obvious mechanism, given our findings so far, is the replacement of school personnel. In this section, we explore to what extent three other mechanisms (quality of school personnel, school operations, and education resources) contribute to the negative impact of party turnover on test scores. Lower quality of school personnel and signs of problems with the operation and management of the school are two mechanisms that we find evidence for. We do not find evidence that political turnover impacts the access to and allocation of education resources at the municipality or school-level.

2.6.1 School Personnel Quality

As discussed in Section 2.5.2 (Appendix Tables A.18 and A.21) school personnel in municipalities where a new political party (barely) comes to power are of worse quality (in terms of observable characteristics). Headmasters in municipalities with political party turnover are 1.8 years less experienced as headmasters. One additional year of headmaster experience is correlated with a .001 standard deviation unit improvement in test scores.[44] The share of teachers in a school with a B.A. located in a municipality with political party turnover is 7.3 percentage points lower compared to schools in municipalities with no political party turnover. A decrease of 7.3 percentage points in the share of teachers with a B.A. within a school is correlated with a .017 standard deviation decrease in test scores. Therefore the loss of headmaster experience and teacher education may explain 0.0188 standard deviation units of the (0.05–0.08 standard deviation unit) reduction in test scores due to political turnover.

2.6.2 School Operations

Ronfeldt et al. (2013) associate high teacher turnover with lower test scores for elementary school students in New York City. They suggest that there are disruptive effects of teacher turnover (beyond changing the distribution of teacher quality) such as: reduced school-specific human capital, disrupted school programs, and lessened teacher collaboration. Using the *Prova Brasil* surveys completed by headmasters, we find that political party turnover increases the share of headmasters who report negatively on a series of questions about how their school operates. Appendix Table A.28 shows these results. Headmasters in municipalities with political turnover report holding fewer teacher council meetings and are less likely to report: having a coordinated curriculum within the school, having a curriculum that was developed jointly by the teachers and headmaster, receiving textbooks on-time, receiving the correct textbooks, offering programs for dropouts and failing students, and holding teacher training. They also report that less teachers participate in training conditional on holding teacher training. Appendix Table A.29 reports the same results for questions regarding the operation of the school that were asked of teachers.[45] The results

[44]The correlations in this subsection are estimated using the municipalities in our sample with close elections that did not have political turnover as to avoid including the causal effect of political party turnover in the correlations.

[45]However, the *Prova Brasil* teacher survey is filled out by the teacher who happens to be proctoring the exam. So it is unclear who the sample of respondents are for the *Prova Brasil* teacher survey.

are similar. Moreover, teachers in municipalities with party turnover report negatively about their relationship with the headmaster and other teachers, but these point estimates are statistically insignificant. These patterns are consistent with political turnover (and potentially the subsequent replacement of school personnel) disrupting school programming and lessening collaboration between school personnel. These disruptions in school operations may partially explain how political party turnover impacts student achievement.

2.6.3 Education Resources

Education funding in Brazil is mostly non-discretionary and comes from a federal program (FUNDEF) that pays a fixed rate per student.[46] Municipalities are mandated to spend an additional 10% of their total revenue on education. If the combination of the federal transfers and the amount spent by municipalities themselves does not amount to a minimum (pre-established) amount per pupil, the federal government complements educational resources to reach the set minimum.

We find that political party turnover does not affect the number of students enrolled (results not shown). So the non-discretionary component of municipal-level educational funding is likely not affected by political party turnover (or at least not supposed to be in theory). Yet, if new parties are less experienced in raising revenue or managing the disbursement of funds or if political turnover systematically changes the alignments between municipal and higher levels of government, then political turnover may impact education because of access to or allocation of educational funds. Appendix Table A.30, Panel A, shows that municipalities with and without political party turnover in close elections are similar in terms of their total expenditures, expenditures on education, and share of expenditures spent on education (as reported by the Ministry of the Economy, *Ministerio da Fazenda/STN*, database).[47] This suggests that changes in mayoral parties do not impact

[46]Menezes-Filho and Pazello (2007a) provide a detailed description of FUNDEF.

[47]Currently, we only have municipality public finance data for 2009. We are working on expanding this analysis the to the 2012 election cycle as well.

education funds *at the municipality level*. However, the municipal government itself could re-allocate funds across municipal schools in a way that results in lower average test scores for the municipality. The *Prova Brasil* headmaster survey asks headmaster whether the school has experienced financial difficulties. Appendix Table A.30, Panel B shows that political party turnover does not seem to impact school-level financial resources (as reported by the headmaster). Therefore, we do not find evidence that political turnover impacts the access to and allocation of education resources at the municipality or school-level.

2.7 Conclusion

Using close mayoral elections as a source of variation in political party turnover, we document that student achievement is reduced and school personnel are replaced when the political party of the mayor in Brazil changes. We then use the set of local, non-municipal schools that are not under the discretion of the municipal government to conduct a placebo exercise: changes in the party of the mayor do not impact student achievement or the assignment of school personnel in non-municipal schools. Therefore, political party turnover negatively impacts student outcomes due to political discretion over the municipal education bureaucracy. The analysis of the mechanisms suggests that political turnover translates into lower student achievement due to the politically caused disruption in the assignment of personnel. We conclude that in an environment where the education bureaucracy is not shielded from the political process, political party turnover can adversely affect the quality of a welfare relevant outcome: student test scores.

Previous work has documented several potential costs of political discretion over the bureaucracy. The use of public service positions for patronage (Weber, 1922; Folke et al., 2011), the loss of autonomy (Rasul and Rogger, 2016), and short-horizoned incentive structures (Rauch, 1995) are some of the potential costs that the literature has studied. Our work highlights another potential cost of political discretion over the bureaucracy: by tying the turnover of public employees to political turnover, political discretion disrupts the process of public service delivery. One component of this disruption may be the (widely-studied) use of patronage, instead of merit, in making personnel decisions. Another component of this disruption, which our work points to, is the instability it creates in the process of public service delivery.

There are also potential benefits of political control over the administration. For instance, political discretion allows politicians: to align the incentives between the executive and the administration (Gulzar and Pasquale, 2016), provide accountability to public employees (Raffler, 2016), and fight bureaucratic entrenchment. In our current study, we are not able to explore the potential benefits of political control over the bureaucracy. A natural next step for research in this area would be to examine any potential benefits to society – and any potential private gains to politicians – of political control over personnel decisions in the bureaucracy.

Chapter 3

Corrupting Learning: Evidence from Missing Federal Education Funds in Brazil[1]

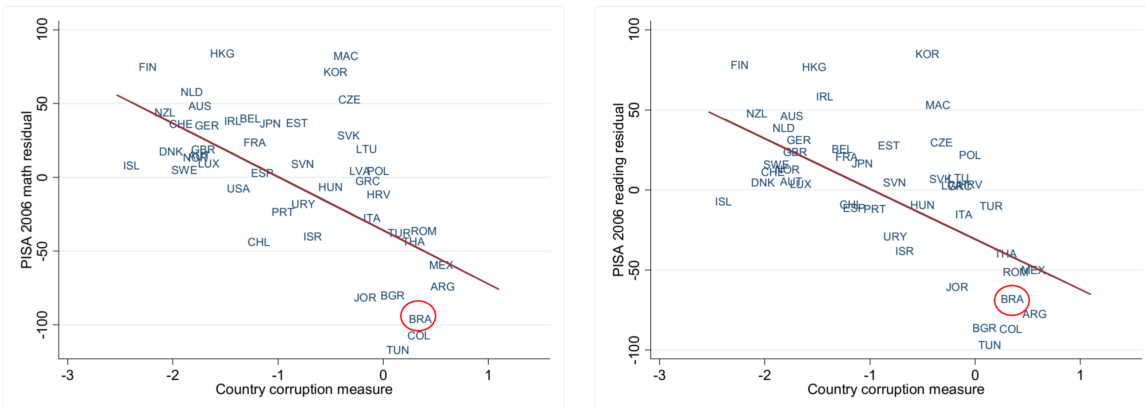
3.1 Introduction

The focus of most education reforms around the world has been to provide more resources to public schools. But whether more resources lead to improvements in student outcomes is highly disputed, due in large part to the difficulties in evaluating these types of policies.[2] The evaluation of policies aimed at providing more resources to schools is complicated for at least two reasons. First, those involved in the educational process may respond to the policy in ways that might dampen its effects – local officials may cut back on educational funding from local taxes or other revenue sources, teachers may feel less of a need to compensate for the lack of resources and in so doing provide less effort in the classroom or, similarly,

[1]Co-authored with Claudio Ferraz (PUC-Rio) and Frederico Finan (UC Berkeley)

[2]See for instance Hanushek (1996), Hedges, Laine, and Greenwald (1994), Glewwe and Kremer (2006).

parents might decide to provide less inputs at home.[3] Second, resources transferred from higher level offices can be expropriated by the local government or school-level officials. In the presence of leakages, reported transfers to schools do not translate into school inputs.[4]



PANEL A: MATH PANEL B: READING

Figure 3.1: *Test Score and Corruption*

Notes: The scatter plots in panels A and B depict the relationship between the residuals from a regression of performance on the PISA exams in 2006 on expenditure on primary education per capita as a share of 2005 GDP per capita, and the World Bank corruption index (Kaufmann, Kraay, and Mastruzzi (2005)). The data used for these graphs can be found: http://www.pisa.oecd.org.

Evidence from cross-country data supports the idea that leakages can reduce educational quality. As seen in Figure 3.1, there is a strong negative association between a country's corruption level and its performance on international standardized exams.[5] But there are several reasons why one should be cautious about interpreting this relationship as causal. First, there are many institutional and cultural differences across countries that determine both its level of corruption and the quality of education. Moreover, as it has been

[3]See Todd and Wolpin (2003), Das, Dercon, Habyarimana, Krishnan, Muralidharan, and Sundararaman (2011), Pop-Eleches and Urquiola (2011).

[4]Although the divergence of public sector resources is more common in developing countries (Reinikka and Svensson (2004)), there is also evidence that bureaucrats in developed countries also use creative accounting to divert funds (Baicker and Staiger (2005)).

[5]Figure 3.1 plots the relationship between the performance on the PISA international exams in 2006, after accounting for expenditures on primary schooling per pupil, and a country's corruption index. The PISA examination is available in 2006 for 56 countries when we include only those countries for which we also have information on spending in primary education per pupil. The corruption index is from Kaufmann, Kraay, and Mastruzzi (2009); we invert the sign of the corruption control index.

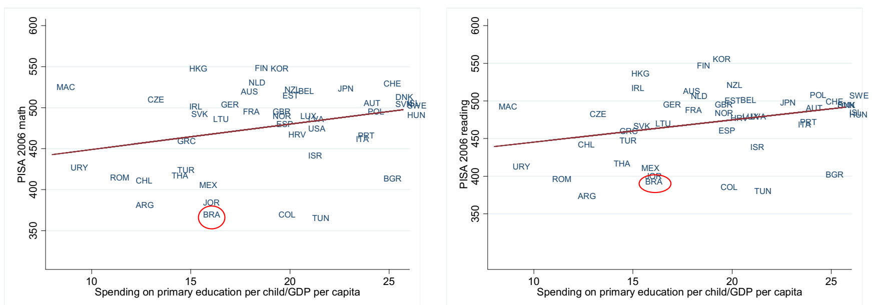
well documented, subjective cross-country measures of corruption are prone to important shortcomings Svensson (2005). Thus, despite its importance, empirical evidence on the effects of leakages from educational funds on student outcomes remains remarkably sparse.

This paper examines the extent to which money matters in education by looking at whether missing resources due to corruption affect student outcomes. We use data from public schools in Brazil where locally-provided primary education is mostly funded by block grants from the central government. Brazil provides an ideal case to examine the effects of corruption in education. Despite significant expenditures on primary schooling per pupil, students' performances on the PISA examination ranks among the worst in the world (see panels A and B of Figure 3.2). Even within Brazil, the association between spending per pupil and academic performance among primary school children in public schools is weak (see panels A and B of Figure 3.3). Finally, based on both official government audits and media reports, corruption involving education grants has become an overarching concern in Brazil.

To overcome the data constraints that have limited cross-country analysis, we build a novel dataset based on audit reports to quantify local-level corruption and mismanagement associated with grants earmarked for education.[6] This data set, which represents one of the first large-scale attempts to measure corruption in education at a local level, has several advantages over the existing literature.[7] First, we have corruption information about not only educational grants, but also transfers made in other sectors such as health and urban infrastructure. Because we can distinguish between corruption in education and corruption in other sectors, we can test whether our estimates reflect leakages from educational funds or simply capture the effects of overall corruption in the municipality. Second, the effects of corruption are identified separately from the effects of mismanagement practices in education. Corrupt politicians may have low management skills or hire poor managers,

[6]The data were constructed based on the audit reports used in Ferraz and Finan (2011a), but exploiting the detailed reports from the educational grants.

[7]See Reinikka and Svensson (2004) for estimates of local capture of educational grants using expenditure tracking surveys.



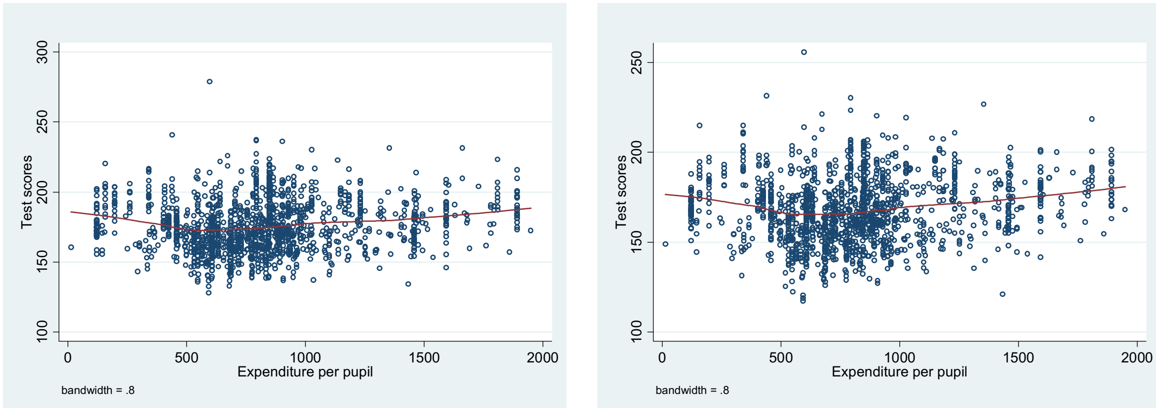
PANEL A: MATH PANEL B:READING

FIGURE 2: TEST SCORES AND SPENDING IN PRIMARY SCHOOL PER PUPIL IN 2005

Notes: The scatter plots in panels A and B depict the relationship between a country's performance on the PISA exams in 2006 and its expenditure on primary education per child as a share of GDP per capita in 2005. The data used for these graphs can be found: http://www.pisa.oecd.org.

Figure 3.2: *Test Scores and Spending in Primary School per Pupil in 2005*

Notes: The scatter plots in panels A and B depict the relationship between a country's performance on the PISA exams in 2006 and its expenditure on primary education per child as a share of GDP per capita in 2005. The data used for these graphs can be found: http://www.pisa.oecd.org.



Panel A: Mathematics

Panel B: Portuguese

Figure 3.3: *Distribution of Test Scores for Mathematics and Portuguese by Corruption*

Notes: Panels A and B display kernel densities of 2005 test scores aggregated at the school-level by subject matter. The densities were estimated separately depending on whether the school resided in a municipality where corruption was detected in education. The densities were estimated using the Epanechnikov kernel, with an optimally computed bandwidth.

both of which may negatively affect educational outcomes. Our data allow us to distinguish between these different types of irregularities.[8]

We link municipal-level corruption measures to data on the educational achievement of primary school students across 1488 public schools located in 365 municipalities throughout Brazil. We use the variation in corrupt practices across municipalities to estimate the effects of “missing resources" on dropout rates, failure rates, and student achievement in a national standardized exam. We find that the educational outcomes of students residing in municipalities where corruption was uncovered are significantly lower than those of students residing in municipalities where no corruption was detected. For instance, test scores on a standardized Language and Math exam among 4^{th} graders are 0.30 standard deviations lower in corrupt municipalities. Corruption is also associated with higher dropout and failure rates among primary school children. We use complementary data sources to show that educational inputs are indeed lower in municipalities with more corruption. Based on Brazil’s school census, we find that the percentage of teachers who had received pedagogical training is 10.7 percentage points lower compared with non-corrupt municipalities. Schools

[8]This is related to the distinction made by Bandiera, Prat, and Valletti (2009) on active and passive waste.

in corrupt municipalities are also less likely to have a computer lab. From independent principals' and teachers' surveys, we also find that both teachers and principals of schools in municipalities where corruption was detected are much more likely to report the lack of resources and teaching supplies as being serious problems.

We undertake a series of robustness tests to make sure our results are indeed driven by "missing resources". First, we account for a large number of factors that are correlated with both corruption and test scores. These factors include not only the standard socio-economic characteristics that have been showed to be associated with corruption (e.g. GDP per capita, urbanization, population size, and income inequality), but also many of the local institutional characteristics that allow the population to hold school managers accountable (e.g. presence of parent-teacher associations, elections for school principals, and the degree of community participation in school maintenance, etc). Second, we show that the results are robust to the control corruption measures detected in other sectors (e.g. health and infrastructure). Controlling for corruption in sectors other than education is likely to proxy for many of the unobservable characteristics that are both correlated with corruption in education and determine student achievement. It will also capture any indirect effects that corruption in other sectors might have on student achievement.[9] Third, using the audit reports we also construct a measure of mismanagement of education resources. This allows us to disentangle the effects of corruption from the effects of mismanagement. Finally, we conduct a placebo test in which we examine whether corruption in education funds in the municipality affects the schooling outcomes of children attending private school. We do not find any evidence that public sector corruption is associated with the dropout and failure rates of children attending private school, suggesting that children are neither sorting into private schools nor that differences in education performance are driven by municipal-level

[9]Given that we control for all these potential determinants of corruption, a subsequent question is what variation is used to identify the effects of corrupt practices on schooling outcomes. We present evidence showing that there is large variation in corrupt practices induced by how the Federal Government monitors and audits intergovernmental transfers in education. In particular, municipalities that have a larger share of educational funds from FUNDEF (Fundo de Manutenção e Desenvolvimento do Ensino Fundamental e de Valorização do Magistério), which is a program with weak monitoring, have more corruption.

unobserved characteristics.

Our findings contribute to the literature that examines whether resources matter for education. We show that a reduction in the availability of resources driven by corruption has negative effects on student outcomes. This is consistent with recent experimental and quasi-experimental evidence showing that increases in school inputs affect student outcomes (e.g. Muralidharan and Sundararaman (2011b), Clark (2009), Duflo, Hanna, and Ryan (2010)). It is also consistent with Harbison and Hanushek (1992) who find that, when schools lack even the most basic resources such as infrastructure, textbooks, and teachers with completed secondary education, resources can have positive effects.

This study contributes, more broadly, to the literature on the consequences of corruption. While there is general consensus among academics and policy makers that corruption harms economic development, with few exceptions, the evidence is based on cross-country comparisons using subjective or self-reported measures of corruption (e.g. Mauro (1995)). Our study complements a growing literature showing that leakages from public funds create inefficiencies in the delivery of public goods and services.[10]

Our results are most closely related to Bjorkman (2007) and Reinikka and Svensson (2011); both use variation from an information campaign to measure the effects of a reduction in corruption on student outcomes. But our study differs in several respects. First, we provide evidence on the mechanisms linking corruption to student outcomes. We use a rich dataset of school infrastructure and teacher and principal questionnaires to show how school inputs, such as teachers with a higher education degree, computer labs, resources and teaching supplies, and teacher training are reduced in the presence of corruption. Second, we exploit the richness of the audit reports to build different measures of corruption and mismanagement.

The paper proceeds as follows. Section 3.2 provides an overview of Brazil's public

[10]See Chaudhury, Hammer, Kremer, Muralidharan, and Rogers (2006) who provide evidence on the widespread teacher and health worker absenteeism in developing countries; Olken (2006) who examines corruption in redistributive programs; Niehaus and Sukhtankar (2011) who examine leakages from public employment programs.

education system and the corruption program that conducted the audit reports. In Section 3.3, we describe the data, including how our corruption measures were coded. Section 3.4 describes our conceptual framework and outlines our empirical strategy. Section 3.5 presents our results, and Section 3.6 concludes.

3.2 Background

3.2.1 Decentralization and Block Grants for Education

Brazil transfers over US$2.2 billion in educational grants to municipal governments and spends 4.1 percent of its GDP on public education per year. Unfortunately, these expenditures have not led to significant improvements in academic performance. For instance, on the 2006 Programme for International Student Assessment (PISA) test among 15 year-old students, Brazil ranked 54th among 57 countries in mathematics and ranked 49th among 56 countries in reading. Brazil also placed well below Mexico and Argentina, both of which spend on average similar amounts on primary education.

Brazil's local governments bare much of the blame for this poor performance. The constitution dictates that state and municipal governments share the responsibility for the provision of primary and secondary education. In practice, however, most state governments manage secondary schools, while municipal governments manage primary schools (*ensino fundamental*). By 2005, approximately 85 percent of all first to fourth grade primary schools were managed by municipal governments.[11] In order to guarantee adequate investments in education, Brazil's constitution mandates that at least 25 percent of all state and municipal revenues are spent for educational purposes. Local governments are thus responsible for building schools, providing adequate infrastructure, distributing school lunches and providing school transportation, training teachers, and paying salaries.

To cover these costs, the federal government transfers to states and municipalities large

[11]See Madeira (2007) for details on the school decentralization process and its impact in the state of São Paulo.

sums of resources in the form of block grants.[12] Also, a new financing scheme named FUNDEF was created in 1997 to equalize the amount of resources available for education across regions.[13] It consists of a state fund to which state and municipal governments contribute 15 percent of specific taxes and transfers. The fund, which totaled US$13.7 billion in 2005, is then redistributed to state and municipal governments on the basis of student enrollment. The federal government supplements local governments in states where per student allocations fall below an established spending floor. The FUNDEF constitutes a large share of resources available to mayors, but the use of resources is not completely unrestricted. For instance, the rule stipulates that at least 60 percent of FUNDEF revenues must be spent on teachers' salaries.

Despite being the largest block grant, FUNDEF is effectively left unmonitored.[14] The principal reason for this lack of oversight has to do with the fact that no government agency was ever assigned to monitor the resources.[15] According to the laws that govern FUNDEF, each municipality is required to transfer 15 percent of its revenue to a state fund that is then redistributed to municipalities based on the share of primary school students enrolled relative to total state enrollment. Since redistribution takes place within states, similar municipalities across different states receive different amounts of FUNDEF funds. If a minimum spending per pupil is not met then the federal government complements the amount municipalities receive. Because all three spheres of government (municipal, state and federal levels) contribute to this fund, the law does not specify which tier of

[12]The largest block grant, called Fundo de Participação dos Municípios, was created in the 1960s and distributes resources to municipalities based on their population and the state's income per capita.

[13]See Gordon and Vegas (2005) and Menezes-Filho and Pazello (2007b) for a detailed description of FUNDEF.

[14]In a report by Transparência Brasil, based on audits executed by the Controladoria Geral da União (CGU), the federal government controller's office, estimated that approximately 13% to 55% of FUNDEF's total budget between 2001 and 2003 was lost to fraud. Transparência Brasil (2005).

[15]Municipalities are required to establish local councils comprised of municipal government representatives, teachers, and parents to monitor the funds. Unfortunately, these councils have been mostly ineffective. They have either been captured by local mayors or do not meet regularly enough to effectively monitor the use of these resources Transparência Brasil (2005). Yet, it is not too surprising that these local councils are unable to fulfill their role as an effective watchdog since local governments are under the control of elites and powerful mayors that often divert resources for their own benefits.

government should be responsible for monitoring it. As a result, prior to the introduction of the audit program in 2003, the spending of these funds went largely unmonitored.[16] Because of the lack of oversight associated with FUNDEF funds, we would expect corruption in education to be higher in places that receive a higher share of their educational resources from FUNDEF. We test this prediction in the next section.

3.2.2 Embezzlement and Misuse of Educational Block Grants

Cases of mayors diverting resources from these educational block grants are numerous. During 2005 alone, there were at least 26 news stories about the misuse of FUNDEF resources in the Brazilian press.[17] Some examples are helpful to illustrate how ubiquitous the problem has become. In the municipality of Placas, in the North of Brazil, the ex-mayor could not account for US$1.25 million of FUNDEF funds between 2003 and 2004. Moreover, when auditors asked the new mayor that took office in 2005 for documents and receipts, he said that all documents disappeared from the archives during the government transition.[18] The new mayor of Camaragibe, state of Pernambuco, also had a surprise when he took office in January 2005. He discovered that US$400,000 from the FUNDEF account was transferred by the ex-mayor to a private bank account.[19]

It appears that embezzlement even extends to stealing teacher's wages, sparking frequent conflicts. In May 2009, approximately 90 percent of municipal school teachers in Itabuna, Bahia received less than half of their monthly salaries, after approximately US$100,000 "disapeared" from the FUNDEF account.[20] In the municipality of Senador Alexandre Costa,

[16]In 2010, the Federal Auditors' Court (TCU) ruled that the legislation creating FUNDEF did not assign any entity to monitor the use of its resources and that it was not the responsibility of the National Fund for Educational Development (FNDE), the branch responsible for making all educational transfers of federal resources to municipalities.

[17]See www.deunojornal.org.br/busca.php?assunto=463

[18]"Dinheiro do FUNDEF é o maior alvo de desvios", O Globo 06/25/2006.

[19]"Desvio do FUNDEF atrasa salários de professores", O Globo 03/27/2005.

[20]See "Professores de Itabuna recebem só metade do salario", in the Blog *Pimenta na Muqueca*, accessed in 05/04/2009.

Maranhão, teachers did not receive their 13th monthly salary and bonus payment because the mayor had diverted all of the funds from FUNDEF. By April 2007, despite the school year having started in early February, all municipal schools were still closed and without electricity due to the lack of payments made.[21] In Gonçalves Dias, Maranhão, 129 municipal teachers did not receive their salaries during 9 months in 2004. They went on strike and it was only in December that the municipal government paid part of their earnings. The new mayor, who inherited the debt, negotiated to pay only 40 percent of the back pay in exchange for having the new salaries paid on time.[22]

Mayors have engaged in other forms of coercion as well. For instance, in the municipality of Traipu, a geography teacher and local representative of the teachers' union, was transferred from an urban school where she taught geography to high school students to a rural school to teach small children after she denounced the mayor's misuse of educational grants. In the municipality of Viçosa, Alagoas students who participated in protests were forbidden to use the municipal bus that transports students to the only secondary school, which was located in the neighboring municipality.[23] The small city of Satuba, Alagoas provides an extreme case of coercion. In June 2003, a teacher started a campaign to denounce the mayor for embezzling funds. Soon after, he was found tortured and killed.

While mayors have found ways of coercing teachers, this does not suggest that all cases of corruption go unpunished. In 2005 the Federal Police arrested 8 mayors and 4 ex-mayors in the state of Alagoas with charges of diverting US$1 million from the FUNDEF.[24] The ex-mayor of Cocal, in the state of Piauí, was also arrested for diverting US$1.2 million from the FUNDEF. He had already been impeached from public office in 2008 under corruption

[21] Taken from a public complaint made by a citizen from Senador Alexandre Costa on a public email sent to Arlindo Chinaglia, the President of the National Congress, in April 2007.

[22] According to Francisco Carlos Custódio, the municipal Secretary of Education for Gonçalves Dias: "Many teachers were angry with the situation, but accepted the offer because they were afraid of not receiving their future salaries." (*Desvio do FUNDEF atrasa salários de professores*, O Globo 03/27/2005).

[23] See the report "Irregularidades na utilização de recursos públicos - Alagoas", written by the NGO *Ação Educativa*, available at http://www.acaoeducativa.org.br.

[24] See O Globo, "Dinheiro do FUNDEF é o maior alvo de desvios", 25/06/2006.

allegations.[25] In December 2008, after a long investigation, the Federal Police arrested 9 mayors, 7 municipal secretaries and 64 public servants for diverting resources from education and health funds in 16 municipalities in the state of Bahia. The police estimated that approximately US$11.5 million was embezzled.[26] In April 2009, the Federal Police arrested four ex-mayors and 17 other persons in the municipalities of Montes Altos, São Pedro da Água Branca, as well as Governor Edison Lobão, in the south of Maranhão. All of which were accused of diverting R$6.5 million from educational grants in 2008.[27]

Given its prevalence in the education sector, corruption can severely impact a student's ability to learn in a variety of ways. First, when teacher salaries are delayed or not paid in full due to corruption, this can affect teacher motivation or the functioning of the school. Second, school quality is also compromised when funds intended for new classrooms or school supplies are diverted. Insufficient school inputs may not only have a direct effect on a student's ability to learn but can also affect a teacher's ability to teach. Third, corruption also occurs in the provision of school lunches. For children of poor households, these meals can represent an important source of daily calories. If corruption reduces the ability to retrieve these calories, then enrollment or regular attendance may suffer.

In sum, Brazil's local governments receive large sums of resources through educational block grants.A significant share of these resources is misused and diverted, thus affecting educational quality. Brazil's local governments provide an ideal setting to examine whether corruption at the local government level affects educational outcomes. Next, we describe Brazil's anti-corruption program and how we used its audit reports to build measures of misuse and diversion of resources from educational block grants.

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[25]See O Globo "PI: ex-prefeito é preso por desvios de fundo do Fundeb e do FUNDEF", Correio Braziliense, 01/30/2009.

[26]See A Tarde, "Prefeitos envolvidos na Operação Vassoura-de-Bruxa devem ser ouvidos até sexta".

[27]See Estado de S.Paulo, "PF prende quatro ex-prefeitos e mais 17 pessoas no MA",04/28/2009.

3.3 Data

Our empirical analysis combines three different data sources. First, we use information contained in the audit reports of Brazil's anti-corruption program to construct our measures of corruption and mismanagement in the education sector. Second, we collect information on various schooling outcomes and student characteristics, which we aggregate at the school level. Third, we assemble a dataset containing information about the socio-economic characteristics of the municipality. Because the identifying variation is at the level of the municipality, accounting for differences across municipalities will be important for our analysis.

3.3.1 Building Measures of Corruption and Mismanagement of Educational Funds

Widespread corruption scandals in municipalities have led to a growing concern over the misuse of federal funds. In May 2003, the federal government started an unprecedented anti-corruption program based on the random auditing of municipal government expenditures. The program, which is implemented through the *Controladoria Geral da União* (CGU), aims at discouraging the misuse of public funds among public administrators and fostering civil society participation in the oversight of public expenditures. The program began by auditing 26 randomly selected municipalities, one in each state of Brazil. It has since expanded to auditing 50 and later 60 municipalities per lottery, from a sample that includes all Brazilian municipalities with less than 450,000 inhabitants. The lotteries, which are held on a monthly basis at the Caixa Econômica Federal in Brasilia, are drawn in conjunction with the national lotteries. To ensure a fair and transparent process, the CGU invites representatives of the press, political parties, and civil society to witness the lottery.[28]

Once a municipality is chosen, the CGU gathers information on all federal funds transferred to the municipal government from 2001 onwards. Approximately 10 to 15 CGU

[28]See Ferraz and Finan (2008) for a more detailed description of these audits.

auditors are then sent to the municipality to examine accounts and documents, to inspect for the existence and quality of public work construction, and delivery of public services. Auditors also meet with members of the local community, as well as municipal councils in order to collect direct complaints about any malfeasance.[29] After approximately one week of inspections, the auditors submit a report containing, for each inspected area (e.g. education, health, urban infrastructure), a list of government programs audited, the total amount of federal funds transferred, and a detailed list describing each irregularity found.[30] At the time of this study, audit reports were available for approximately 790 municipalities which were randomly selected through the first 16 lotteries of the anti-corruption program. From these 16 lotteries, we randomly selected the municipalities from 10 lotteries to measure corruption and mismanagement in education, health, and urban infrastructure, which are the three largest sources of federal transfers for municipalities.[31] Thus, in total, we construct indicators of corruption and mismanagement for 366 municipalities.

In order to build our measures of corruption and mismanagement, we read the report for each municipality and classify the irregularities listed by the auditors into several pre-established categories. We define three types of corruption practices: *diversion of public funds*, *over-invoicing*, and *irregular public procurements*. We classify *diversion of resources* as any irregularity involving the embezzlement of public funds. This typically occurs in one of two situations: 1) federally-transferred resources simply “disappear" from municipal bank accounts; and 2) the municipality claimed to have purchased goods and services that were never provided, which is determined when there is no proof of purchase and community members confirm that the goods were in fact not delivered. We classify *over-invoicing* as any irregularity in which auditors determined that the goods and services were purchased at a value above market price. We classify the irregularity as an *irregular public procurement* when

[29]These auditors are hired based on a public examination, and prior to visiting the municipality receive extensive training on the specificities of the sampled municipality. Also, there is a supervisor for each team of auditors.

[30]For some irregularities, the amount of resources diverted are estimated by the auditors.

[31]As a result, we do not have data from lotteries 8, 11-13, and 15.

there is an illegal call-for-bids and the contract is awarded to a “friendly firm". These firms are usually connected directly to the mayor and/or his family or some cases do not exist. Most cases of corruption involving illegal public procurements include any combination of: i) use of non-existing firms in the bidding process; ii) use of fake receipts to pay for goods and services; iii) over-invoicing of prices to increase the amount paid for the goods and services.

Drawing on the classifications described above, we define three measures of corruption. First, an indicator for whether auditors detected any corruption in education. Second, we count the number of irregularities associated with corruption and divide by the number of service items audited. Third, we estimate the value of resources diverted (when information is available) and divide it by the amount of resources in education that were audited.[32] While the second and third measures capture the extent of corruption, corruption in education was only detected in 35 percent of municipalities, suggesting that the extensive margin may capture most of the relevant variation in the data. So while we present results using all three measures of corruption, most of our analysis will focus on the corruption indicator.

In addition to documenting the cases of corruption, we also construct measures of mismanagement. These are irregularities that are uncovered by the auditors, but do not involve any incidence of fraud. Administrative irregularities, however, may still affect the quality of education if they create inefficiencies in the allocation of school inputs. Some examples are useful to illustrate this measure. Municipalities that receive funds from the FUNDEF program are required to establish an active and independent community council to monitor the use of these funds. Auditors uncovered several cases where the council simply did not function. It either never met or was led by a mayor’s family member. Although this irregularity is not an act of corruption, the lack of a well functioning council prevents the effective use and monitoring of resources by civil society. Another common form of mismanagement is the use of resources that are mandated for other purposes. For instance,

[32]Because some of the irregularities associated with corruption have missing values, the share of corruption is underestimated.

Table 3.1: *Corruption in the Education Sector*

	N	mean	sd	p25	p50	p75
Proportion of municipalities with corruption in education	365	0.35	0.48	0.00	0.00	1.00
Proportion of items in education found to be corrupt	365	0.12	0.25	0.00	0.00	0.15
Proportion of items in education found to be corrupt conditional on some corruption	128	0.35	0.32	0.13	0.25	0.50
Share of resources audited in education that were found to be corrupt	365	0.03	0.12	0.00	0.00	0.01
Share of resources audited in education found to be corrupt conditional on some corruption	128	0.08	0.19	0.00	0.03	0.07
Proportion of municipalities with corruption in some area other than education	365	0.50	0.50	0.00	1.00	1.00
Proportion of items audited found to be associated with mismanagement	365	2.00	1.83	0.75	1.46	2.71
Proportion of municipalities with corruption involving a school feeding program	343	0.15	0.36	0.00	0.00	0.00
Proportion of municipalities with corruption involving teachers and school supplies	305	0.28	0.45	0.00	0.00	1.00
Proportion of municipalities with corruption involving other aspects of education	364	0.03	0.18	0.00	0.00	0.00

Notes: This table reports descriptive statistics on the various measures of corruption. Column 1 reports the sample size. Column 2 reports the mean and column 3 reports the standard deviation. Columns 4-6 report the 25th, 50th, and 75th percentiles of the distribution. The data used to compute these statistics come from the audit reports.

mayors have to spend at least 60 percent of resources from FUNDEF on teacher salaries. In some municipalities, auditors discovered that these resources were used to pay for the salaries of other public servants or to purchase gasoline for municipal cars. Again, even though this does not constitute the diversion of resources for private gains, it may affect the allocation of resources intended for education. Finally, public procurements require at least three firms to participate in the call-for-bids. Even in the case where the public good or service was provided (and is thus not considered corruption) the lack of competition in the bidding process might have led the government to overspend, thus creating distortions in the allocation of resources. For mismanagement, most irregularities are not associated with values (e.g. lack of a council to monitor the use of funds) and virtually every municipality has some incident of mismanagement. Thus, we can only build measures of mismanagement by counting the total number of irregularities.

Table 3.1 presents summary statistics of the corruption measures. Corruption in the area of education was discovered in 35 percent of municipalities. Among these municipalities, 35 percent of service items in education were found to be subject to corruption and 8 percent of resources were diverted. Corruption in other sectors was also discovered in 50 percent of the municipalities, and on average 2 irregularities per service item were found to be associated with some type of mismanagement.

3.3.2 Data on Schooling Outcomes and Municipal Characteristics

We have two main sources of schooling data, both of which are aggregated at the school level. The data on test scores and student characteristics come from a national standardized examination of 4th and 8th graders called *Prova Brasil*. In 2005, the Federal government conducted a standardized exam in the subjects of Mathematics and Portuguese given to all 4th graders enrolled in a public school with at least 20 students. In addition to the exam, the program conducted a survey designed to measure the child's socio-economic conditions. The survey includes not only information about the child, such as gender, age, and race, but also information about their parents and home environment: such as the education of the parents, whether the child lives with both parents, size of the family, whether the household owns a computer and other assets. The wealth of information contained in the survey allows us to control for a host of characteristics that are likely to affect student achievement.

Our second principal data source comes from the 2005 and 2006 school census, referring to information from the 2004 and 2005 school year respectively. The census measures the basic conditions of schools in Brazil. It contains information about approval rates, dropout rates, and failure rates by school. There is also information regarding school conditions such as whether the school has sanitation, or computer and science labs, as well as information about teachers, namely years of experience and what proportion have a degree or a credential.

Table 3.2 and Table 3.3 provide summary statistics based on information from these surveys, as well as basic socio-economic information about the municipality. We see that the proportion of children with parents with at least high school degree is on average 16 percent. On average 15 percent of children have a computer at home. The average dropout rate for schools in our sample is 4 percent, while failure rates are at 10 percent. Only 19 percent of schools have a computer lab and 4 percent of schools have a science lab.

From the 2007 *Prova Brasil*, we also have responses from a principal's survey and a teacher's survey. These surveys, which were conducted separately, asked whether the following four items were a serious concern at school: 1) lack of financial resources 2) lack

Table 3.2: *Summary Statistics*

	N	mean	sd	p25	p50	p75
Panel A: Student characteristics						
Standardized exam in Mathematics - 4th grade	1488	175.80	18.25	162.23	174.01	188.22
Standardized exam in Portuguese - 4th grade	1488	168.09	18.07	155.41	167.10	180.15
% males	1488	0.50	0.09	0.45	0.50	0.56
% white	1488	0.31	0.15	0.21	0.29	0.39
% of mothers with a high school degree	1488	0.17	0.09	0.10	0.15	0.21
% of fathers with a high school degree	1488	0.15	0.08	0.09	0.14	0.19
% children that live with both parents	1488	0.61	0.12	0.54	0.62	0.70
% families with 6 or more inhabitants	1488	0.26	0.13	0.17	0.24	0.34
% families with a home computer	1488	0.15	0.10	0.08	0.13	0.20
% families with electricity at home	1488	0.92	0.09	0.89	0.94	0.97
% families with running water at home	1488	0.84	0.14	0.79	0.88	0.93
% of children who are 8 years old or younger	1488	0.01	0.02	0.00	0.00	0.02
% of children who are 9 years old	1488	0.05	0.05	0.01	0.04	0.07
% of children who are 10 years old	1488	0.36	0.18	0.22	0.35	0.49
% of children who are 11 years old	1488	0.25	0.10	0.18	0.24	0.31
% of children who are 12 years old	1488	0.12	0.07	0.07	0.11	0.16
Panel B: School Characteristics						
Dropout rates	1488	0.04	0.07	0.00	0.02	0.06
Failure rates	1488	0.10	0.09	0.03	0.09	0.16
% of teachers with a teaching credential	1488	0.43	0.36	0.05	0.42	0.75
School has a computer lab	1488	0.19	0.39	0.00	0.00	0.00
School has a science lab	1488	0.04	0.19	0.00	0.00	0.00
School has sanitation	1488	0.03	0.17	0.00	0.00	0.00
Director's survey						
Lack of financial resources is a serious concern	1488	0.55	0.50	0.00	1.00	1.00
Lack of schooling supplies is a serious concern	1488	0.40	0.49	0.00	0.00	1.00
Lack of teachers is a serious concern	1488	0.23	0.42	0.00	0.00	0.00
Disciplinary problems is a serious concern	1488	0.63	0.48	0.00	1.00	1.00
Training courses are provided to teachers	1488	0.49	0.50	0.00	0.00	1.00
Teacher's survey						
Lack of financial resources is a serious concern	1488	0.56	0.50	0.00	1.00	1.00
Lack of schooling supplies is a serious concern	1488	0.51	0.50	0.00	1.00	1.00
Lack of teachers is a serious concern	1488	0.26	0.44	0.00	0.00	1.00
Disciplinary problems is a serious concern	1488	0.63	0.48	0.00	1.00	1.00

Notes: This table reports descriptive statistics for the variables used in the analysis. Column 1 reports the sample size. Column 2 reports the mean and column 3 reports the standard deviation. Columns 4-6 report the 25th, 50th, and 75th percentiles of the distribution. The variables presented in Panels A and B are computed for the 1488 schools that reside in the 365 municipalities for which information on corruption exists.

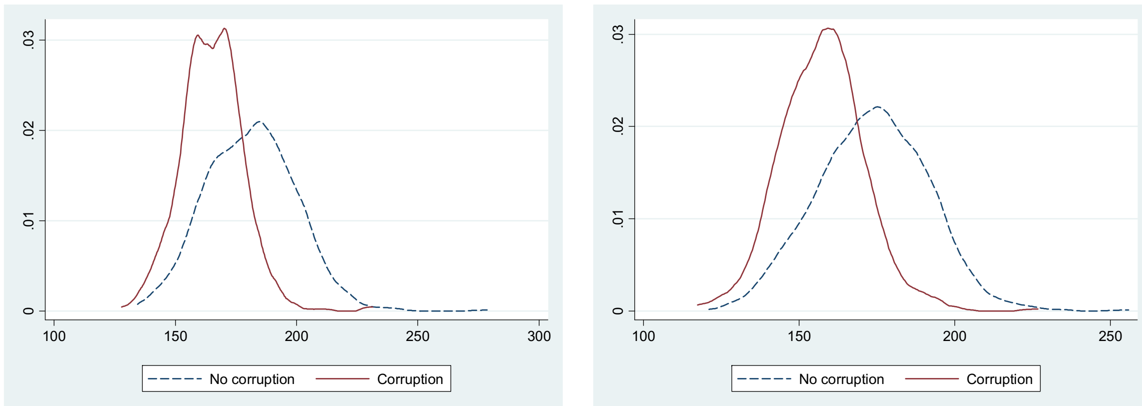
Table 3.3: *Summary Statistics - Municipal Characteristics*

	N	mean	sd	p25	p50	p75
Panel C: Municipal Characteristics						
% population urban	365	0.61	0.23	0.44	0.62	0.80
Gini	365	0.57	0.06	0.54	0.57	0.61
GDP per capita	365	8707.74	22821.08	2545.43	4678.03	8544.47
Expenditure in primary school per child	365	942.20	487.67	656.48	856.68	1106.40
Dropout rates among private schools	188	0.01	0.03	0.00	0.00	0.00
Failure rates among private schools	188	0.02	0.04	0.00	0.00	0.02
Election is held for principal	365	0.10	0.30	0.00	0.00	0.00
Average number of state schools that elect its principal	365	0.43	1.25	0.00	0.00	0.00
Average number of state schools in the municipality	365	1.68	3.31	0.00	1.00	2.00
PTA is active in the municipality	365	0.48	0.50	0.00	0.00	1.00
Municipality has a intergovernmental consortium	365	0.26	0.44	0.00	0.00	1.00
Municipality has an education council	365	0.69	0.46	0.00	1.00	1.00
Schools receive support from private sector	365	0.07	0.25	0.00	0.00	0.00
Municipality uses participatory budgeting	365	0.71	0.45	0.00	1.00	1.00
The community helps in the maintenance of the school	365	0.15	0.36	0.00	0.00	0.00
The school participated in an awareness campaign for the community	365	0.41	0.49	0.00	0.00	1.00

Notes: This table reports descriptive statistics for the variables used in the analysis. Column 1 reports the sample size. Column 2 reports the mean and column 3 reports the standard deviation. Columns 4-6 report the 25th, 50th, and 75th percentiles of the distribution. The variables presented in Panels A and B are computed for the 1488 schools that reside in the 365 municipalities for which information on corruption exists.

of school supplies 3) lack of teachers to teach the courses 4) disciplinary problems among the student body. In both the teacher's and principal's surveys, 55 percent of the schools cite lack of resources and school supplies as serious concerns. Only 23 percent cite lack of teachers as an important concern.

Combining the test score data with the information from the audit reports, Figure 3.4 plots the distribution of test scores by whether or not corruption in education was detected in the municipality. Consistent with the cross-country evidence, we find that the distributions of scores for both math and language in corrupt municipalities is to the left of the distributions of scores in municipalities where corruption was not found. On average, test scores are 15 points lower in municipalities where some corruption in education was detected. We later investigate the robustness of this relationship.



Panel A: Mathematics

Panel B: Portuguese

Figure 3.4: *Distribution of Test Scores for Mathematics and Portuguese by Corruption*

Notes: Panels A and B display kernel densities of 2005 test scores aggregated at the school-level by subject matter. The densities were estimated separately depending on whether the school resided in a municipality where corruption was detected in education. The densities were estimated using the Epanechnikov kernel, with an optimally computed bandwidth.

3.4 Empirical Strategy

To estimate the effects of corruption on student achievement, let us assume that the academic achievement $A_{i,s,m,t}$ of an individual i attending school s in municipality m in grade g is determined by the following reduced-form equation:

$$A_{i,s,m,g} = \delta A_{i,s,m,g-1} + \gamma_g (Y_{m,g} - C_{m,g})$$

where $Y_{m,g}$ is the amount of the education funds per school, and $C_{m,t}$ is the amount per school that is diverted.[33] The parameter γ_g measures the effect of schooling resources on student performance, which may vary by grade level, and δ captures how much learning decays from one grade to the next. Under this value-added specification, a student's achievement at the end of the fourth grade is given by the following expression:

$$A_{i,s,m,4} = \sum_{g=1}^{4} \delta^{4-g} \gamma_g (Y_{m,g} - C_{m,g}) + \delta^4 A_{i,s,m,0}.$$

[33]We are assuming that the municipality's education budget is distributed evenly across schools, which is why we drop the schooling subscript, s. Thus, we do not consider the possibility that corruption affects one school disproportionately more than another.

Given that our measure of corruption captures the average amount of diversion in education over a three-year period, and we assume that corruption does not vary much across grades, we can rewrite the equation above as:

$$A_{i,s,m,4} = \beta C_m + \delta^4 A_{i,s,m,0} + \sum_{g=1}^{4} \delta^{4-g} \gamma_g Y_{m,g}$$

where $\beta = -\sum_{g=1}^{4} \delta^{4-g} \gamma_g$. After averaging across students within a school, we arrive at the our estimation equation:

$$A_{s,m,4} = \alpha + \beta C_m + Z'_m \theta_1 + X'_{s,m} \theta_2 + \epsilon_{s,m} \tag{3.1}$$

where $A_{s,m,4}$ is the average student achievement of fourth graders in school s in municipality m, C_m is the level of corruption in education that was detected in the municipality, and $X_{s,m}$ is a vector of predetermined student characteristics (e.g. gender, age, race, etc.) and family characteristics (e.g. parent's education, assets, etc.) that will account for differences in the initial student achievement, $A_{s,m,0}$, of the student body. To proxy for $\sum_{g=1}^{4} \delta^{4-g} \gamma_g Y_{m,g}$, we control for total expenditure in primary school which is included in the vector Z_m along with a set of other municipal characteristics. The variable $\epsilon_{s,m}$ denotes a random error term that is clustered at the school level. Given the value-added specification and under the assumption that $E[C_m \epsilon_{s,m} | X_m Z_{s,m}] = 0$, the coefficient β captures the discounted cumulative effects of corruption on student performance since the first grade.

Given our identification assumption, there are three broad classes of factors that are likely to affect our ability to interpret the causal effects of corruption on student achievement. First, as we know from the cross-country literature, corruption is not only negatively correlated with economic development, but test scores are also on average lower among countries that are less economically developed. In wealthier places, households will invest more in their children's education both because they have more financial resources to do so, and because the returns to education might be higher due to different types of economic activities. In our regressions, we account for a municipality's level of economic development using municipal GDP per capita. We also control for other socio-economic characteristics that have been shown to be associated with corruption, such as urbanization, population size, and income inequality.[34] Second, local institutions that hold school managers accountable to the population are likely to improve school performance and reduce corrupt practices in education. We use detailed institutional data to control for the presence of parent-teacher associations, elections of school principals, and the degree of community participation in school maintenance. Third, the education policies of a municipality also reflect the preferences of the mayor. Mayors who care more about education will presumably be less willing to divert money away from education. To account for the mayor's preferences towards education, we control for several characteristics of the municipality and the mayor: the amount of ostensible spending per pupil, whether the municipality has an intergovernmental consortium in education, whether a school council exists, as well as the mayor's gender and schooling level.

In Table 3.4, we examine how these various characteristics correlate with our measures of corruption in education. Each column uses a different measure of corruption and for each one we estimate both a basic OLS model and a non-linear model accounting for corner solutions. Overall, the results suggest that municipalities with a larger urban population and greater inequality are associated with more corrupt practices, while municipalities that hold elections for school principals, that have a school council, and where the mayor holds a college degree are associated with less corruption.[35]

Given that our most robust specification controls for all these potential determinants of corruption, a natural question becomes: what is the variation that allows us to identify the effects of corrupt practices on schooling outcomes? The identifying variation comes from how the Federal Government monitored and audited intergovernmental transfers in education prior to the introduction of the CGU audit program. As we discussed in Section 3.2, municipalities fund their expenditures in education through a variety of sources. These

[34]See for instance Ades and Di Tella (1999), Glaeser and Saks (2006), Glaeser, Scheinkman, and Shleifer (2003), La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1999), Reinikka and Svensson (2004), and Treisman (2000).

[35]The results for the share of audited resources with corruption in education are less precisely estimated.

Table 3.4: *Determinants of Corruption in Education*

Dependent variable:	Corruption in education		Proportion of items with corruption in education		Share of audited resources with corruption in education	
	OLS	Probit	OLS	Tobit	OLS	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)
School finance:						
% education revenue from FUNDEF transfers	0.501	0.574	0.195	0.209	0.036	0.044
	[0.200]**	[0.277]**	[0.096]**	[0.103]**	[0.024]	[0.022]**
Socio-economic characteristics						
Share of urban population	-0.413	-0.417	-0.224	-0.178	-0.006	-0.017
	[0.135]***	[0.145]***	[0.075]***	[0.055]***	[0.010]	[0.011]
Gini coefficient	0.745	0.883	0.286	0.326	0.055	0.061
	[0.417]*	[0.456]*	[0.255]	[0.178]*	[0.044]	[0.037]
Log GDP per capita	-0.041	-0.034	0.026	0.003	-0.011	-0.006
	[0.037]	[0.040]	[0.033]	[0.013]	[0.004]**	[0.003]**
Log population	0.043	0.050	0.015	0.016	0.001	0.002
	[0.033]	[0.039]	[0.020]	[0.015]	[0.003]	[0.003]
School institutions						
Share of schools with elections	-0.017	-0.089	-0.006	-0.033	-0.001	-0.007
	[0.010]*	[0.037]**	[0.005]	[0.015]**	[0.001]	[0.003]**
Share of schools with a PTA	0.000	0.009	0.002	0.006	-0.001	0.000
	[0.011]	[0.014]	[0.006]	[0.006]	[0.002]	[0.001]
% schools community helps in maintenance	-0.011	-0.033	-0.021	-0.025	-0.001	-0.002
	[0.021]	[0.030]	[0.016]	[0.012]**	[0.003]	[0.002]
% schools participate in an awareness community	0.014	0.014	0.018	0.012	0.003	0.002
	[0.019]	[0.023]	[0.017]	[0.010]	[0.003]	[0.002]
Preferences towards education						
Log spending primary education per kid (×100)	-7.632	-9.502	-0.318	-2.125	0.004	-0.331
	[5.004]	[5.291]*	[2.680]	[1.961]	[0.561]	[0.401]
Intergovernmental consortium in education	0.052	0.065	0.014	0.018	0.007	0.006
	[0.057]	[0.063]	[0.032]	[0.024]	[0.007]	[0.005]
Education council exists	-0.093	-0.103	-0.073	-0.055	-0.004	-0.009
	[0.056]*	[0.058]*	[0.031]**	[0.024]**	[0.007]	[0.005]*
Mayor is a male	-0.142	-0.161	-0.122	-0.094	-0.002	-0.008
	[0.095]	[0.107]	[0.080]	[0.056]*	[0.010]	[0.009]
Mayor has a college degree	-0.136	-0.148	-0.036	-0.050	-0.004	-0.007
	[0.051]***	[0.053]***	[0.030]	[0.021]**	[0.006]	[0.004]*
Number of observations	366	366	366	366	361	361
R-squared	0.18		0.11		0.06	
Pseudo R-squared		0.17		0.15		0.69

Notes: This table reports the association between municipal characteristics and different measures of corruption in education. Column (1), (3) and (5) present OLS results, while columns (2), (4) and (6) present the marginal effects of non-linear models that account for the discrete or censured dependent variable. The dependent variable used in each regression is listed at the top of each column. All regressions exclude municipalities that report zero revenues from the FUNDEF program. The regressions shown in columns (5) and (6) exclude municipalities where the estimated share of corruption is above 1. Robust standard errors are displayed in brackets. Significantly different than zero at 99 ($***$), 95 ($**$), 90 ($*$) percent confidence.

funding sources, however, are subjected to different degrees of monitoring under Federal law. Consequently, municipalities receiving the same amount of educational resources can experience, for arguably exogenous reasons, quite different degrees of monitoring.

To see how variation in the degree of monitoring might affect corruption, consider the case of FUNDEF. As we mentioned previously, despite the fact that FUNDEF represents almost 80 percent of the intergovernmental transfers used for education, these funds are essentially unmonitored. Thus, we would expect more corruption in education in places that receive a higher share of their educational funding from FUNDEF. In the first row of Table **??** we find that municipalities with a larger share of revenues from FUNDEF have, on average, more corrupt practices: a one standard deviation increase in the share of educational funds from FUNDEF increases the probability of corruption in education by 8 percentage points, or 23 percent. This result is consistent with the idea that the decentralization of responsibilities financed by intergovernmental transfers, rather than local revenue collection, allow local officials to ignore the consequences of mismanagement because they are less accountable to local taxpayers.[36]

Even after controlling for the observable characteristics described above, we might still be worried that municipalities with less corruption may offer more public goods and other amenities that might affect student achievement. For instance, mayors who care about education may also care about improvements in health, and may also refrain from corruption in the health sector. Because the health of a child is also likely to affect his academic achievement, our estimates may also be capturing the effects of less corruption in the health sector. It may also be the case that families that value education may choose to live in municipalities with less corruption. In these situations, we will over-estimate the negative effects of corruption on education.[37]

To address these concerns, we present several robustness checks. First, we re-estimate

[36] See Fan, Lin, and Treisman (2009); Fisman and Gatti (2002)

[37] If parents in corrupt municipalities compensate for the lack of schooling inputs then we would underestimate the negative effects of corruption.

Equation (3.1) controlling for corruption detected in other sectors (e.g. health and infrastructure). Controlling for corruption in sectors other than education is likely to proxy for many of the unobservable characteristics that are both correlated with corruption in education and determine student achievement. It will also capture any indirect effects that corruption in other sectors might have on student achievement. Second, using the audit reports we also construct a measure of mismanagement of education resources. This allows us to disentangle the effects of corruption from the effects of mismanagement.

Our third main robustness check uses private schools as a placebo test. Here, we re-estimate Equation (3.1) using educational outcomes of children who attend private school as the dependent variable. Under this specification, we would expect $\hat{\beta} = 0$, since corruption in public expenditures should not affect private school outcomes. Similarly, we also test whether the effects of corruption on educational outcomes differ in municipalities with private schools by estimating the following equation:

$$A_{sm} = \alpha + \beta C_m + \eta_1 P_m + \theta(P_m \times C_m) + Z'_m \delta + X'_{s,m} \gamma + \epsilon_{sm}$$

where P_m is an indicator for whether a private school exists in the municipality. If in corrupt municipalities more able students are sorting into private schools, then we would expect the interaction effect between corruption and the existence of a private school to be negative, i.e. $\theta < 0$.

3.5 Results

In this section we present the main empirical results of the paper. We begin by presenting estimates of the relationship between schooling outcomes and corruption in education. We then show that our estimates are robust across various specifications, including ones that control for the effects of mismanagement and corruption in other sectors. In the final part of the section, we explore the mechanisms that link corruption to poor schooling achievement.

3.5.1 The effects of corruption practices on educational outcomes

Table 3.5 reports estimates of the association between corruption and various schooling outcomes measured in 2005. The results are OLS estimates of a series of regression models based on Equation (3.1). Our base specification, which is reported in the odd columns, adjusts for several key school characteristics (e.g. gender, race, age, parent's education, household wealth, student-teacher ratio) which are likely to affect the education production function. In the even columns, we augment this base specification to also include various characteristics of the municipality (e.g. GDP per capita, population, Gini).[38]

Panel A presents estimates using as our measure of corruption the proportion of audited education items found to involve corruption. Across the various schooling measures, the negative effects of corruption are substantive. For instance, a 30 percentage point (or approximately one standard deviation) increase in corruption is associated with a 0.10 standard deviation decrease in test scores (columns 2 and 4), and a 0.6 percentage point increase in both dropout and failure rates (columns 6 and 8). These point estimates, while economically meaningful, are also highly robust to the inclusion of important controls that account for differences in labor market opportunities, such as GDP per capita and urbanization rates.

In Panel B, we present estimates using the share of resources in education found to be corrupt as an alternative measure of corruption. In reading the audit reports, it is difficult to calculate a dollar amount for every irregularity. Yet despite the imprecision associated with this measure, the results in Panel B convey a similar story. In columns 2 and 4 of Panel B, the estimates imply that a 5 percentage point increase in corruption is associated with a 0.04 standard deviation decrease in test scores. The share of audited resources found to be corrupt is also positively associated with both dropout and failure rates, but imprecisely

[38]While a more robust specification would also include state-fixed effects, 9 out of the 25 states have 1 or fewer municipalities with corruption in education, which constitutes close to 40 percent of observations in our sample. Thus when we incorporate state fixed-effects, we are either losing the contribution of certain states that do not have any variation or introducing a lot of measurement error given that our averages for the "treatment group" are based on a single observation. Nevertheless, when relying on within state variation, the point estimates, while less precise, are still negative.

Table 3.5: *The Effect of Corruption on Schooling Outcomes*

Dependent variable:	Mathematics		Portuguese		Dropout rates		Failure rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corruption in education	-0.286	-0.283	-0.264	-0.277	0.027	0.027	0.018	0.022
	[0.076]***	[0.080]***	[0.071]***	[0.070]***	[0.005]***	[0.005]***	[0.009]**	[0.009]**
Local institutional quality								
Corruption in other sectors	-0.199	-0.185	-0.162	-0.141	0.007	0.006	0.001	0.001
	[0.068]***	[0.068]***	[0.064]**	[0.063]**	[0.004]*	[0.004]	[0.007]	[0.007]
Judiciary district	0.133	0.146	0.179	0.202	-0.004	-0.007	-0.006	-0.003
	[0.093]	[0.092]	[0.083]**	[0.084]**	[0.005]	[0.005]	[0.007]	[0.008]
Share of council that supports the mayor	-0.178	-0.145	-0.133	-0.107	-0.015	-0.014	-0.022	-0.028
	[0.190]	[0.187]	[0.168]	[0.162]	[0.012]	[0.012]	[0.020]	[0.019]
Practices participatory budgeting	0.024	-0.005	0.074	0.044	0.009	0.009	-0.001	0.001
	[0.079]	[0.076]	[0.071]	[0.067]	[0.005]*	[0.005]*	[0.010]	[0.010]
School institutions								
School elects the principal		0.136		0.081		0.004		0.005
		[0.079]*		[0.062]		[0.004]		[0.008]
School has active PTA		0.014		0.057		-0.003		-0.001
		[0.045]		[0.036]		[0.003]		[0.005]
School receives help from community		0.056		0.035		-0.003		-0.002
		[0.050]		[0.041]		[0.003]		[0.006]
School participates in community awareness campaigns		0.012		0.005		-0.001		0.006
		[0.035]		[0.035]		[0.003]		[0.004]
Preferences towards education								
Municipality has an intergovernment consortium in education		0.136		0.161		-0.004		0.001
		[0.078]*		[0.067]**		[0.005]		[0.008]
Education council exists		-0.031		-0.002		0.003		0.002
		[0.077]		[0.064]		[0.005]		[0.007]
Mayor is a male		0.082		0.02		0.011		-0.01
		[0.124]		[0.125]		[0.008]		[0.014]
Mayor has a college degree		0.07		0.062		-0.008		0.015
		[0.072]		[0.066]		[0.005]		[0.008]*
Student and municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of schools	1488	1468	1488	1468	1488	1468	1488	1468
R-squared	0.54	0.55	0.6	0.61	0.31	0.32	0.16	0.17

Notes: This table reports the effects of corruption on various education outcomes. Each column presents the results of an OLS regression where the dependent variable is listed at the top of each column. For the results reported in Panels A, C, and D, the number of observations is 1488 schools. Whereas, for Panel B, the number of observations is 1479, due to missing values in the amount of resources audited. Student characteristics included proportion of male children, proportion of white children, the schooling of the mother, schooling of the father, the proportion of kids with both parents living at home, family size, proportion of households with a computer, proportion of families with running water, proportion of families with electricity, and age dummies. Municipal characteristics included share of population that resides in urban areas, Gini coefficient, Log GDP per capita in 2004, and log population. Robust standard errors clustered at the municipality are displayed in brackets. Significantly different than zero at 99 ($***$), 95 ($**$), 90 ($*$) percent confidence.

estimated.

In Panel C, we present a third alternative corruption measure: an indicator for whether or not corruption in education was detected. The result suggests that children residing in municipalities where corruption was detected fare much worse on the standardized exams than those with similar observable characteristics but residing in municipalities where no corruption was revealed. Based on the estimates presented in column 1, corruption in education is associated with a significant decrease of 0.35 standard deviations in test scores (robust standard error = 0.076).

While columns 1-4 suggest that corruption may have affected learning, the results in columns 5-8 indicate that corruption may also affect a child's educational attainment. Dropout rates are 2.9 percentage points higher in municipalities where corruption was detected, representing almost a 65 percent increase from the average. Failure rates are also higher in corrupt municipalities (see column 7 and 8), thus consistent with the effects on test scores.

While all three alternative measures of corruption produce similar results, the measures presented in panels A and B have the potential advantage of capturing the effects of corruption along the intensive margin. However, given that only 35 percent of municipalities have some practice of corruption in education, the relevant variation in the data may simply be reflected in the extensive margin. In Figure 3.5, we plot the relationship between test scores and the proportion of items audited associated with corruption. As we see from this figure, the effects of corruption, while decreasing, are statistically similar once the proportion of items is larger than 0.1. While this relationship might appear puzzling, it is likely to reflect the fact that this measure does not capture the amount of resources diverted. Thus, committing one big act of corruption versus many small acts of corruption may affect education similarly. This explanation is consistent with what we see in Panel B when we plot the relationship between test scores and the share of resources involving corruption. Here, we find a much more consistently negative relationship between corruption and test scores, although as we mentioned previously, this variable is measured with much more noise.[39] For these reasons, in the remainder of the analysis, we use the indicator for whether or not corruption in education was detected as our main measure of corruption.

Overall, the results presented in Table 3.5 suggest that the effects of corruption on education outcomes are quite severe. Our findings are however comparable to those presented by Reinikka and Svensson (2011), who find that a 30 percentage point increase (or approximately one standard deviation) in corruption is associated with a 0.10 standard deviation decrease in test scores. Because their measure of leakage is continuous, we can compare this effect with our point estimates in Panel B of Table 3.5. When based on the share of resources associated with corruption, our estimates imply that an increase in corruption of 20 percentage points (or approximately one standard deviation) is associated with a reduction in test scores of 0.14 (for Math) and 0.18 (for Portuguese) standard deviations. Although our estimates appear slightly larger, recall that our measure of leakage represents an average over a 3 year period. Thus, our effects are in fact slightly smaller than those reported by Reinikka and Svensson (2011) whose measure of leakage is based on a single year.

3.5.2 Robustness Checks

Controlling for Institutional Quality, School Institutions, and Educational Preferences

In Table 3.6 we re-estimate our main specification controlling for corruption in other sectors, as well as other measures of institutional quality at the local level (e.g. whether members of the community participates in the budgetary process or has a judiciary district). This specification is useful for two reasons. First, it identifies the effects of corruption specifically in education, rather than potentially estimating a proxy for more general corruption. Second, by controlling for corruption in other sectors and other measures of institutional quality, we are accounting for many of the unobserved differences between municipalities that do and do not engage in corruption more generally. For instance, returns to education are often

[39] Given these figures, it is perhaps not surprising that the correlation between the proportion of items found to be corrupt and the share of resources found to be corrupt is only 0.29.

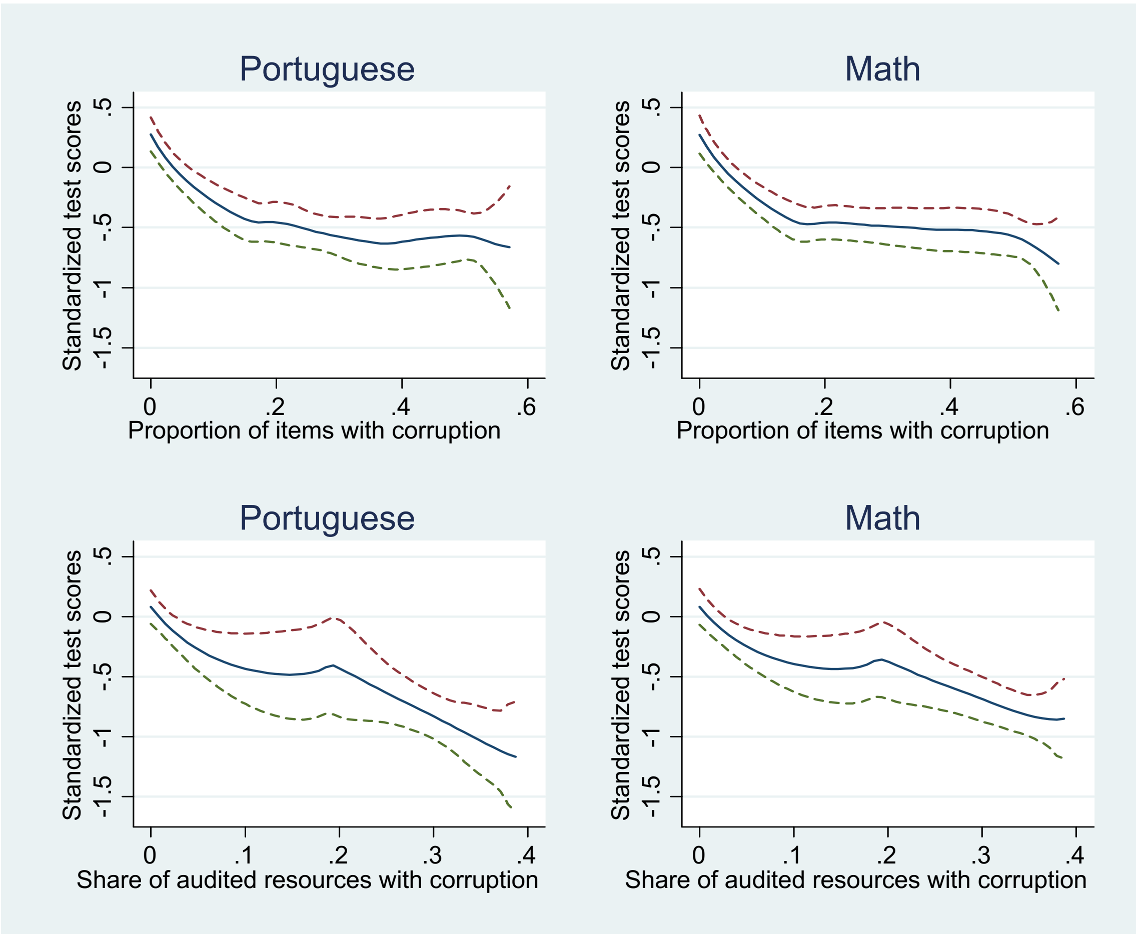


Figure 3.5: *Association Between Test Scores for Mathematics and Portuguese and Corruption in Education*

Notes: Each graph shows the results of a locally weighted regression with a quartic Kernel (Fan, 1992). The dependent variable is the 4th grade standardized test score for either Portuguese or Math and the independent variable is the respective measure of corruption (proportion of items found with corruption or share of audited resources found with corruption). The bandwidth is equal to one-third of the range of the independent variable. The lines in dashes show the 95 percent confidence intervals calculated with 100 bootstrapped replications where the standard error is clustered by municipality. The estimation dropped 5 outliers with extremely high corruption.

lower in places that are more prone to corruption, since these areas tend to be economically depressed and more reliant on local patronage practices. With this specification, we are, for example, able to capture any potential differences in the returns to education that were not necessarily accounted for by controlling for income levels.

Corruption in other sectors also has a strong negative correlation with educational outcomes. For instance, in column 1 of Table 3.6, corruption in other sectors is associated with a 0.2 standard deviation decline in test scores, which is comparable to our main effects. This estimate reflects the fact that corruption in other sectors may not only be serving as an important proxy for other institutional characteristics of the municipality that adversely affect test scores, but is also capturing the negative indirect effects that corruption in sectors, such as health and sanitation, can have on test scores. We also see that even after controlling for whether corruption in other sectors was detected, our estimates remain virtually unchanged. Overall, these results suggest that our estimates are robust to unobservable factors that affect both schooling outcomes and a municipality's propensity to engage in corruption more broadly.[40]

In the even columns of Table 3.6, we re-estimate the main regression model controlling for a series of variables intended to capture differences in either preferences or local institutions specific to education. These variables either directly capture the efficacy of local schools and parent organizations (active PTA, existence of a school council) or serve as proxies for the general level of civic engagement in the municipality (e.g. principal is elected, municipality uses participatory budgeting).[41] Our original results are again robust to controlling for these additional local institutions.

[40]Even after controlling for corruption in other sectors, we cannot of course rule out the possibility that other forms of unobserved heterogeneity are biasing our results.

[41]Intergovernmental consortiums are entities managed by civil society. They group municipalities to implement a certain action that individual municipalities are not capable of doing alone. They have autonomous management and financing and are commonly used to provide public services, e.g. management of a public hospital. Participatory budgeting is a type of participatory democracy, in which citizens are formally given the opportunity to discuss and prioritize public spending projects, and in some cases even decide how to allocate parts of the municipal budget.

Table 3.6: *The Effect of Corruption on Schooling Outcomes - Robustness*

Dependent variable:	Mathematics		Portuguese		Dropout rates		Failure rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corruption in education	-0.286	-0.283	-0.264	-0.277	0.027	0.027	0.018	0.022
	[0.076]***	[0.080]***	[0.071]***	[0.070]***	[0.005]***	[0.005]***	[0.009]**	[0.009]**
Local institutional quality								
Corruption in other sectors	-0.199	-0.185	-0.162	-0.141	0.007	0.006	0.001	0.001
	[0.068]***	[0.068]***	[0.064]**	[0.063]**	[0.004]*	[0.004]	[0.007]	[0.007]
Judiciary district	0.133	0.146	0.179	0.202	-0.004	-0.007	-0.006	-0.003
	[0.093]	[0.092]	[0.083]**	[0.084]**	[0.005]	[0.005]	[0.007]	[0.008]
Share of council that supports the mayor	-0.178	-0.145	-0.133	-0.107	-0.015	-0.014	-0.022	-0.028
	[0.190]	[0.187]	[0.168]	[0.162]	[0.012]	[0.012]	[0.020]	[0.019]
Practices participatory budgeting	0.024	-0.005	0.074	0.044	0.009	0.009	-0.001	0.001
	[0.079]	[0.076]	[0.071]	[0.067]	[0.005]*	[0.005]*	[0.010]	[0.010]
School institutions								
School elects the principal		0.136		0.081		0.004		0.005
		[0.079]*		[0.062]		[0.004]		[0.008]
School has active PTA		0.014		0.057		-0.003		-0.001
		[0.045]		[0.036]		[0.003]		[0.005]
School receives help from community		0.056		0.035		-0.003		-0.002
		[0.050]		[0.041]		[0.003]		[0.006]
School participates in community awareness campaigns		0.012		0.005		-0.001		0.006
		[0.035]		[0.035]		[0.003]		[0.004]
Preferences towards education								
Municipality has an intergovernment consortium in education		0.136		0.161		-0.004		0.001
		[0.078]*		[0.067]**		[0.005]		[0.008]
Education council exists		-0.031		-0.002		0.003		0.002
		[0.077]		[0.064]		[0.005]		[0.007]
Mayor is a male		0.082		0.02		0.011		-0.01
		[0.124]		[0.125]		[0.008]		[0.014]
Mayor has a college degree		0.07		0.062		-0.008		0.015
		[0.072]		[0.066]		[0.005]		[0.008]*
Student and municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of schools	1488	1468	1488	1468	1488	1468	1488	1468
R-squared	0.54	0.55	0.6	0.61	0.31	0.32	0.16	0.17

Notes: This table reports the effects of corruption on various education outcomes. Each column presents the results of an OLS regression where the dependent variable is listed at the top of each column. Our measure of corruption is an indicator for whether corruption was detected in education. Our measure of other corruption is an indicator for whether corruption was detected in sectors other than education. Student characteristics included proportion of male children, proportion of white children, the schooling of the mother, schooling of the father, the proportion of kids with both parents living at home, family size, proportion of households with a computer, proportion of families with running water, proportion of families with electricity, and age dummies. Municipal characteristics included share of population that resides in urban areas, Gini coefficient, GDP per capita in 2004, and expenditure per child in primary school. Robust standard errors clustered at the municipality are displayed in brackets. Significantly different than zero at 99 ($***$), 95 ($**$), 90 ($*$) percent confidence.

Table 3.7: *Private Schools*

Dependent variable:	Dropout rates for private schools (1)	Failure rates for private schools (2)	Share of students enrolled in a private school (3)	Mathematics (4)	Portuguese (5)	Dropout rates (6)	Failure rates (7)
Corruption in education	-0.005	-0.008	0.004	-0.28	-0.249	0.023	0.019
	[0.003]	[0.006]	[0.009]	[0.101]***	[0.098]**	[0.006]***	[0.013]
Corruption in education × Municipality has a private school				0.003	-0.017	0.002	-0.003
				[0.100]	[0.097]	[0.008]	[0.012]
Student characteristics	Yes	Yes	No	Yes	Yes	Yes	Yes
Municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of schools	1185	1185	331	1488	1488	1488	1488
R-squared	0.07	0.14	0.04	0.53	0.59	0.31	0.19

Notes: This table reports whether there is a differential effects of corruption in municipalities with a private school. Each column presents the results of an OLS regression where the dependent variable is listed at the top of each column. In columns 1 and 2, the dependent variables are dropout and failure rates of children in private schools. In columns 3-6, the dependent variables are the education outcomes for children attending municipal schools (as in the previous tables). Our measure of corruption is an indicator for whether corruption was detected in education. Student characteristics included proportion of male children, proportion of white children, the schooling of the mother, schooling of the father, the proportion of kids with both parents living at home, family size, proportion of households with a computer, proportion of families with running water, proportion of families with electricity, and age dummies. Municipal characteristics included share of population that resides in urban areas, Gini coefficient, GDP per capita in 2004, and expenditure per child in primary school. Robust standard errors clustered at the municipality are displayed in brackets. Significantly different than zero at 99 ($***$), 95 ($**$), 90 ($*$) percent confidence.

Does the misuse of public funds affect educational performance in private schools?

In Table 3.7, we present alternative tests for whether unobserved differences between corrupt and non-corrupt municipalities are affecting our results. In Columns 1 and 2, we estimate the effects of corruption in education on the dropout and failure rates of children attending private schools.[42] Because our measure of corruption is based on the misuse of funds intended for public schools, we should not expect the measure to predict educational outcomes of private-school children. The results in columns 1 and 2 do in fact show that the effects of corruption on private schooling outcomes are small and statistically insignificant.

Although we do not find that corruption affects the dropout and failure rates of private school children, an alternative explanation for our results is that corruption influenced the selection of students into public and private schools. We test this hypothesis in columns 3-7. In column 3, we estimate whether children are more likely to enroll in private schools in

[42]Unfortunately, standardized Mathematics and Portuguese exams are only conducted on students attending public schools.

municipalities with corruption. In columns 4-7, we estimate whether the effects of corruption on educational outcomes are more pronounced in municipalities where a private school exists. If in municipalities with corruption high ability students are more likely to attend private schools, then we should expect the effects of corruption to be more pronounced among municipalities with a private school. But as we see in columns 3-7, corruption does not predict enrollment rates among private schools and its effects are not more pronounced in municipalities with a private school. This suggests that differential sorting does not explain our findings.

Corruption or mismanagement?

Another possible concern is that our estimates capture the effects of not only the diversion, but also the mismanagement of educational resources. If corruption and mismanagement of educational funds are positively correlated, then our estimates are overstated. Table 3.8 shows that this is not the case. In columns 1-4, we re-estimate the full specifications presented in Table 3, controlling for the share of audited items in education associated with mismanagement practices. Our findings in columns 1 and 2 suggest that test scores are in fact negatively correlated with the incidence of mismanagement. A one standard deviation increase in the incidence of mismanagement is associated with a 0.14 standard deviation decrease in math scores. Yet despite this negative correlation, the magnitude of the effect is small relative to the size of the effects of corruption. The incidence of mismanagement in a municipality would have to increase from the 1st percentile to the 99th percentile of the distribution in order to achieve the same effects as those of corruption. Overall, the estimated coefficients across the various educational outcomes suggest substantive effects of corruption, even after accounting for the negative effects of mismanagement.

3.5.3 Mechanisms linking corruption to educational outcomes

Thus far, we have presented estimates of a reduced-form relationship between corruption in education and student achievement. As discussed in Section 3.4, corruption can affect

Table 3.8: *Effect of Corruption on Schooling Outcomes Accounting for Mismanagement*

Dependent variable:	Mathematics	Portuguese	Dropout rates	Failure rates
	(1)	(2)	(3)	(4)
Corruption in education	-0.265	-0.251	0.024	0.02
	[0.081]***	[0.077]***	[0.005]***	[0.009]**
Mismanagement	-0.041	-0.046	0.001	-0.004
	[0.018]**	[0.017]***	[0.001]	[0.004]
Student characteristics	Yes	Yes	Yes	Yes
Municipal characteristics	Yes	Yes	Yes	Yes
Number of schools	1486	1486	1486	1486
R-squared	0.53	0.59	0.31	0.17

Notes: This table reports the effects of corruption on various education outcomes, controlling for mismanagement and corruption in other sectors. Each column presents the results of an OLS regression where the dependent variable is listed at the top of each column. Our measure of corruption is an indicator for whether corruption was detected in education. Our measure of mismanagement is the share of audited service items that found to be associated with poor management practices. Student characteristics included proportion of male children, proportion of white children, the schooling of the mother, schooling of the father, the proportion of kids with both parents living at home, family size, proportion of households with a computer, proportion of families with running water, proportion of families with electricity, and age dummies. Municipal characteristics included share of population that resides in urban areas, Gini coefficient, GDP per capita in 2004, and expenditure per child in primary school. Robust standard errors clustered at the municipality are displayed in brackets. Significantly different than zero at 99 ($***$), 95 ($**$), 90 ($*$) percent confidence.

Table 3.9: *The Effect of Corruption on Schooling Inputs*

Dependent variable:	Percentage of teachers with a higher education degree		Proportion of schools with a computer lab		Proportion of schools with a science lab		Proportion of schools with sanitation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption in education	-0.113	-0.114	-0.06	-0.059	-0.008	-0.005	-0.008
	[0.053]**	[0.053]**	[0.026]**	[0.029]**	[0.013]	[0.014]	[0.016]
Initial input in 2001	N	Y	N	Y	N	Y	N
Student characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of schools	1488	1488	1488	1150	1488	1150	1488
R-squared	0.26	0.26	0.13	0.13	0.08	0.08	0.02

Notes: This table reports the effects of corruption on various schooling inputs. Each column presents the results of an OLS regression where the dependent variable is listed at the top of each column. Our measure of corruption is an indicator for whether corruption was detected in education. Student characteristics included proportion of male children, proportion of white children, the schooling of the mother, schooling of the father, the proportion of kids with both parents living at home, family size, proportion of households with a computer, proportion of families with running water, proportion of families with electricity, and age dummies. Municipal characteristics included share of population that resides in urban areas, Gini coefficient, GDP per capita in 2004, and expenditure per child in primary school. Robust standard errors clustered at the municipality are displayed in brackets. Significantly different than zero at 99 ($***$), 95 ($**$), 90 ($*$) percent confidence.

student performance through various channels. The direct channel we examine here is the reduction of school inputs and/or infrastructure. In Table 3.9, we explore whether schooling inputs are lower in municipalities where corruption was detected using data from the 2005 school census.

Column 1 examines whether corrupt municipalities are less likely to have received pedagogical training. One common form of corruption uncovered in the audits was the diversion of funds intended for teacher training. The results in column 1 confirm this hypothesis. In municipalities where corruption was detected, the percentage of teachers who are trained is 11.3 percentage points (standard error 0.061) lower compared to non-corrupt municipalities. Given that 44 percent of teachers receive training, this estimate represents a 25 percent decline. Schools in corrupt municipalities are also less likely to have a computer lab (coefficient=-0.060; standard error =0.026), but we find no effects on the likelihood of having a science lab (coefficient=-0.008; standard error =0.013). We do not find any evidence that schools in corrupt municipalities have less access to sanitation, but this

Table 3.10: *Problems that Schools Face Based on Teacher and Principal Survey*

Survey repondent:	Teacher				Principal				
Dependent variable:	Insufficient resources	Insufficient teaching supplies	Lack of teachers	Disciplinary problems among students	Insufficient resources	Insufficient teaching supplies	Lack of teachers	Disciplinary problems among students	Provided teacher training
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Corruption in education	0.072	0.066	-0.004	0.007	0.045	0.106	-0.014	-0.032	-0.106
	[0.034]**	[0.032]**	[0.031]	[0.029]	[0.034]	[0.035]***	[0.030]	[0.031]	[0.047]**
Student characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of schools	1488	1488	1488	1488	1488	1488	1488	1488	1488
R-squared	0.02	0.05	0.04	0.01	0.05	0.08	0.02	0.02	0.04

Notes: Each column presents the results of an OLS regression where the dependent variable is listed at the top of each column. Our measure of corruption is an indicator for whether corruption was detected in education. In columns 1-4, the data come from a survey conducted with a teacher. In columns 5-9, the data come from a survey conducted with the principal. Student characteristics included proportion of male children, proportion of white children, the schooling of the mother, schooling of the father, the proportion of kids with both parents living at home, family size, proportion of households with a computer, proportion of families with running water, proportion of families with electricity, and age dummies. Municipal characteristics included share of population that resides in urban areas, Gini coefficient, GDP per capita in 2004, and expenditure per child in primary school. Robust standard errors clustered at the municipality are displayed in brackets. Significantly different than zero at 99 ($***$), 95 ($**$), 90 ($*$) percent confidence.

might be a margin where corruption is harder to hide. Also, from the results presented in the even columns, our estimates are robust to controlling for school inputs in 2001.

Table 3.10 provides further evidence that schools have fewer resources in municipalities where corruption in education was detected. Table 9 presents estimates based on a series of linear probability models, where the dependent variable is specified at the top of each column. Each dependent variable is constructed based on a series of questions asking teachers and principals whether the school faced the following non-mutually exclusive problems: 1) insufficient resources; 2) insufficient teaching supplies; 3) lack of teachers; 4) disciplinary problems among the students. In columns 1-4, we present estimates based on information from a teacher's survey, whereas the estimates presented in columns 5-8 are based on responses to the same question, but asked separately to the school principal.

Despite the fact that the two surveys were conducted separately, both teachers and principals of schools in municipalities where corruption was detected are much more likely to report a lack of resources as a serious problem. For instance, in corrupt municipalities, teachers are 7.2 percentage points (standard error=0.034) more likely to indicate a lack of teaching supplies (see column 2), whereas school directors are 10.6 percentage points (standard error=0.035) more likely to complain about a lack of teaching supplies (see column 6). While corruption would be expected to lead to fewer resources, one would not necessarily expect corruption to affect disciplinary problems among students or even a lack of teachers (at least in the short run). The data do in fact bear this out. In columns 3-4 and 7-8, we do not find any association between corruption in education and whether the school faces disciplinary problems among its students or a lack of teachers. Using information from the principal's survey, we investigate whether schools in corrupt municipalities are less likely to offer pedagogical training. As reported in column 9, we find that teachers of schools in corrupt municipalities are 10.6 percentage points less likely to have gone through teacher training. This result is consistent with the finding presented in Table 8.

3.6 Conclusions

Improving school quality remains a challenge faced by most developing countries. But how to improve quality is still a highly debated question. In this paper, we present evidence that leakages from educational resources can be an important constraint on school quality. Using a novel dataset of corruption in education and schooling outcomes across public schools in Brazil, we find that student test scores on a national standardized exam and pass rates are significantly lower, and dropout rates are significantly higher in municipalities where corruption is prevalent.

Consistent with the idea that corruption reduces schooling inputs, we find that schools in municipalities found to be corrupt have less school infrastructure and teachers that have received training. Moreover, both teachers and principals report the lack of resources as a principal concern in corrupt municipalities. Thus, our results contradict a large literature suggesting that additional resources do not affect schooling outcomes. We conclude that, in environments where basic schooling resources are lacking, money does matter for educational achievement. To the extent that the quality of education affects long-run economic growth, our results suggest a direct channel through which corruption affects long-run economic development Hanushek and Woessmann (2009).

Our findings have important policy implications. First, they suggest that efforts to increase school quality in developing countries need to incorporate policies that aim at reducing leakages. Introducing a system to monitor the use of educational funds, including block grants, should be of central concern to governments. Moreover, it can be a cost-effective way to improve schooling outcomes. Second, in addition to corruption, we find that the mismanagement of resources have detrimental effects on students' performance. These findings complement the work of Bandiera et al. (2009) who show that passive waste in public service might be as important as active waste in generating public-sector inefficiencies. Thus, reforms aimed at improving the capabilities of local bureaucracies may help reduce inefficiencies in the use of public funds.

Although our results focus on the direct effects of corruption-induced leakages, the negative effects of corruption on schooling may not simply represent a shift in the school budget constraint. If, for instance, corruption also affects the allocation of school inputs – perhaps to avoid detection – then corruption can lead to important distortionary effects as well Shleifer and Vishny (1993). Future research should address these additional costs of corruption.

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Appendix A

Appendix to Chapter 1

A.1 Supplementary Tables

Table A.1: *Smoothness in Pre-Award Characteristics*

RD bandwidth: h=.82

	All grades			11th grade		
	β	t-stat	$\mu^{control}$	β	t-stat	$\mu^{control}$
Panel A. School level	(1)	(2)	(3)	(4)	(5)	(6)
# of Medals and HM awards in the past 2 years	0.0454	0.52	3.565	0.1384	0.48	4.216
# of Honorable Mention awards in the past 2 years	0.0512	0.70	3.295	0.1159	0.48	3.913
North Region	0.0000	0.00	0.0669	-0.0072	0.96	0.0746
Northeast Region	0.0009	0.24	0.199	0.0042	0.35	0.249
Southeast Region	0.0012	0.21	0.462	-0.0154	0.99	0.413
South Region	0.0020	0.44	0.183	0.0032	0.25	0.173
Central west Region	-0.0041	1.32	0.0879	0.0153*	1.70	0.0904
=1, if school is managed by municipal government	0.0090*	1.80	0.308	-0.0020	0.48	0.0127
% of 12th graders participating in SAT	-0.0003	0.07	0.512	-0.0035	0.45	0.527
=1 if selective school	0.0017	0.89	0.0183	0.0044	0.56	0.0415
Average SAT score	0.0051	0.98	-0.285	0.0092	1.51	-0.282
Average 9th grade Prova Brasil score	-0.3162	1.25	495.6	-2.1905*	1.83	493.2
Total school enrollment	-1.5948	0.30	775.6	-1.6642	0.10	918.1
Rural school	-0.0008	0.29	0.0826	-0.0004	0.06	0.0417
Panel B. Classmates level						
Grade attainment at t	-0.0009	0.75	0.906	-0.0009	0.29	0.920
=1, if Participated at MO Exam at t	-0.0001	0.25	0.0243	0.0001	0.091	0.0211
=1, if MO score in top 30th percentile at t	0.0003	1.5	0.00382	-0.0005	0.714	0.00561
Female	0.0006	0.5	0.522	0.0034	0.872	0.571
White	0.0039	1.15	0.507	0.0004	0.041	0.487
Panel C. Participant level						
Grade attainment at t	-0.0018	1.13	0.972	0.0026	0.53	0.971
=1, if Participated at MO Exam at t-1	0.0027	0.55	0.131	0.0218*	1.85	0.0968
=1, if MO score in top 30th percentile at t-1	0.0023	0.64	0.0620	0.0139	1.34	0.0579
Female	-0.0016	0.28	0.474	-0.0112	0.70	0.419
White	0.0012	0.17	0.585	0.0077	0.38	0.545

Notes: This table report tests for smoothness in pre-award characteristics. 11th grade sample size: 21366; All grades sample size: 170,355. Column 1 and 4 reports β of main specification estimated with the bandwidth $h = .82$: $Y_{ij} = \alpha + \beta Award_j + \gamma_1 ScoreMargin_j + \gamma_2 Award_j \times ScoreMargin_j + X_j + \epsilon_{ij}$. The Y_{ij} are specified in each of the lines of the table and are at different level: school and participant and classmates level. The Column 2 and 5 report the t-statistics associated with the estimate β. Column 3 and 6 report the mean of the control group.

Table A.2: *Impact of Award on Performance Outcomes (Participant)*

Panel A	Math Olympiad at Year t+1					
	Participated in MO	Probability of exceeding MO score percentiles of national distibution: 50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)
Award	0.0348***	0.0348***	0.0335***	0.0322***	0.0286***	0.0191***
	(0.0045)	(0.0044)	(0.0043)	(0.0041)	(0.0037)	(0.0030)
Students (obs.)	170,335	170,335	170,335	170,335	170,335	170,335
Dep. Variable control mean	0.128	0.107	0.0959	0.0793	0.0577	0.0281
Std. beta	0.272	0.325	0.349	0.406	0.496	0.680
Panel B	Tertiary Education at Year t+2					
	Enroll in tertiary education	Probability of attending a college that exceeds selectivity percentile: 50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)
Award	-0.0075	0.0038	0.0022	0.0224*	0.0195**	0.0134*
	(0.0152)	(0.0137)	(0.0129)	(0.0117)	(0.0099)	(0.0071)
Students (obs.)	21,346	21,346	21,346	21,346	21,346	21,346
Dep. Variable control mean	0.366	0.223	0.180	0.128	0.077	0.030
Std. beta	-2.05%	1.70%	1.23%	17.46%	25.28%	45.39%
Panel C	SAT at Year t+1					
	Participate in SAT	Probability of exceeding SAT score percentiles of national distibution: 50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)
Award	-0.0103	-0.0195	-0.0216	-0.0101	0.0179	0.0310**
	(0.0159)	(0.0161)	(0.0161)	(0.0159)	(0.0151)	(0.0121)
Students (obs.)	21,346	21,346	21,346	21,346	21,346	21,346
Dep. Variable control mean	0.8	0.642	0.573	0.464	0.313	0.121
Std. beta	-1.29%	-3.04%	-3.77%	-2.18%	5.72%	25.60%
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bdw selection	h=.82					

Notes: Sample for Panel A includes all grades, sample for Panel B and C inlcudes only 11th graders. The table displays the award impact on participant performance using a regression discontinuty design specified at equation 2.1. The regressions are all estimated on a sample within $h = .82$ s.d. above and below the award threshold. Results for the other bandwidths are in the appendix. In addition to the *Award* variable, the specification includes the following covariates (coefficients not reported): $Score_c$, $Award_c \times Score_c$. All columns include controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of national school quality distribution. Panel A reports the award impact on Math Olympiad performance at $t + 1$. In column 1, the outcome Participate in MO is equal to 1 if the student show up to the MO Exam at $t + 1$; Columns 2 to 6 report if the student score exceeded different percentiles of the MO national distribution. The outcome in Column 2, for example, is equal to 1 if the student score exceeded the 50th percentile of the national MO score distribution. Panel C, follows the same structure of outcomes but for the participation and performance in the SAT at $t + 1$ Panel B, report the impact on tertiary education enrollment. Column 1, Enroll in tertiary education is equal to 1, if the student enrolled in any tertiary education. Columns 2 up to 6, measure whether the student enroll in colleges with different degrees of selectivity. The outcome in column 2 for example is equal to 1 if the college the student enrolled exceeds the 50th percentile in terms of selectivity. All outcomes are well defined, assuming values 1 or 0, (not missing) for the entire sample. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1%

Table A.3: *Impact of Award assigned at Year t on Math Olympiad (MO) Performance at Year $t+1$ (Classmates)*

Panel B. Classmates	Participated in MO exam at Year t+1			
	(1)	(2)	(3)	(4)
Award	0.0014***	0.0014***	0.0013***	0.0011**
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Students (obs.)	3,540,290	3,540,290	5,114,922	4,899,094
Classrooms (Clusters)	117,882	117,882	170,331	163,337
Dep. variable control mean	.024	.024	.024	.024
Bdw selection	h= .62 (Min)	h= .62 (Min)	h= .82 (Avg)	h= .79 (optimal)
Controls	No	Yes	Yes	Yes

Panel B. Classmates	MO score at Year t+1			
	(1)	(2)	(3)	(4)
Award	0.0362**	0.0409***	0.0293**	0.0367**
	(0.0166)	(0.0147)	(0.0124)	(0.0152)
Students (obs.)	89,872	89,872	127,331	84,033
Classrooms (Clusters)	54,213	54,213	77,191	50,554
Dep. variable control s.d.	.82	.82	.81	.82
Bdw selection	h= .62 (Min)	h= .62 (Min)	h= .82 (Avg.)	h= .57 (optimal)
Controls	No	Yes	Yes	Yes

Notes: The table displays award impact on classmates performance in the MO at $t+1$ using a regression discontinuity design specified at equation 1.2. The regressions are all estimated on a sample within h s.d. above and below the award threshold. I report various bandwidths of size h: column 1 and 2, $h = .62$; column 3, $h = .82$, column 4 $h = .79$. In addition to the *Award* variable, the specifications include the following covariates (coefficients not reported): $Score_c$, $Award_c \times Score_c$. The models reported in columns 2, 3 and 4 contain controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of the national distribution of school quality distribution. Panel A report impact on *Participate in MO* which is equal to 1 if the student show up to the MO Exam at $t+1$. This outcomes is well defined, assuming values 1 or 0, (not missing) for the entire sample. Panel B reports award impact on MO score at $t+1$. Standard errors are clustered at the classroom level. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1%

Table A.4: *Impact of award on composition of students who Participate in MO exam at $t+1$ (Classmates)*

Classmates	Std. Math+ELA scores (s.d.) at t-2		
	(1)	(2)	(3)
Award	-0.2936*	-0.2944*	-0.2727**
	(0.1625)	(0.1561)	(0.1295)
Students (obs.)	1,174	1,174	1,578
Classrooms (Clusters)	793	793	1,075
Dep. variable control s.d.	1.1	1.1	1.1
Bdw selection	h=.62	h=.62	h=.82
Controls	No	Yes	Yes
Sample Restriction: Students participated in MO at t+1	Yes	Yes	Yes

Notes: The table displays award impact on the composition of classmates participating in the MO exam at $t+1$ using a regression discontinuity design specified at equation 1.2. The regressions are all estimated on a sample within h s.d. above and below the award threshold. I report various bandwidths of size h: column 1 and 2, $h = .62$; column 3, $h = .82$. In addition to the *Award* variable, the specifications include the following covariates (coefficients not reported): $Score_c$, $Award_c \times Score_c$. The model reported in columns 2 and 3 contains controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of the national distribution of school quality distribution. To get at change in composition of test MO participants, the sample is restricted to students who participate in the MO. The outcome variable is a standardized low-stakes test score assessed at $t-2$. This is only available for a sub-sample (11^{th} graders, in the State of Sao Paulo). Standard errors are clustered at the classroom level. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1%.

Table A.5: *Impact of Award assigned at Year t on MO Performance at $t+1$ (Classmates)*

	Math Olympiad at Year t+1					
	Participate in MO exam	Probability of exceeding MO score percentiles of national distibution				
		50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Award	0.0013***	0.0012***	0.0011***	0.0008***	0.0006***	0.0002*
	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0001)
Students (obs.)	5,114,922	5,114,922	5,114,922	5,114,922	5,114,922	5,114,922
Classrooms (clusters)	170331	170331	170331	170331	170331	170331
Bdw select	0.82	0.82	0.82	0.82	0.82	0.82
Panel B						
Award	0.0014***	0.0013***	0.0013***	0.0010***	0.0008***	0.0004**
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0001)
Students (obs.)	3,540,290	3,540,290	3,540,290	3,540,290	3,540,290	3,540,290
Classrooms (clusters)	117882	117882	117882	117882	117882	117882
Bdw select	0.62	0.62	0.62	0.62	0.62	0.62
Panel C						
Award	0.0011**	0.0009**	0.0013***	0.0008***	0.0006***	0.0003**
	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0001)
Students (obs.)	4,899,094	4,439,637	3,540,290	5,631,938	4,171,519	3,672,852
Classrooms (clusters)	163337	147928	117882	187788	138796	122418
Bdw select	0.79	0.75	0.59	0.88	0.68	0.65
Control	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Variable control mean	0.0239	0.0122	0.00969	0.00694	0.00431	0.00174
Std. beta	0.054	0.098	0.114	0.115	0.139	0.115

Notes: Sample includes 11$^{\text{th}}$ graders only. The table displays the award impact on classmates Math Olympiad performance at $t+1$ using a regression discontinuity design specified at equation 1.2. The regressions are all estimated on a sample within h s.d. above and below the award threshold. Panel A, report the results for $h = .82$, Panel B report results for $h = .62$ and Panel C, for various bandwidths - the optimal bandwidth for each individual outcome. In addition to the *Award* variable, the specification includes the following covariates (coefficients not reported): $Score_c$, $Award_c \times Score_c$. All columns include controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of national school quality distribution. In column 1, the outcome Participate in MO is equal to 1 if the student show up to the MO Exam at $t+1$; Columns 2 to 6 report if the student score exceeded different percentiles of the MO national distribution. The outcome in Column 2, for example, is equal to 1 if the student score exceeded the 50th percentile of the national MO score distribution. All outcomes are well defined, assuming values 1 or 0, (not missing) for the entire sample. Standard errors are clustered at the classroom level. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1%

Table A.6: *Impact of Award assigned at Year t on SAT Performance at $t+1$ (Classmates)*

	SAT at Year t+1					
	Participate in SAT	Probability of exceeding SAT score percentiles of national distibution				
		50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Award	0.0089	0.0092**	0.0085**	0.0086**	0.0046*	0.0019
	(0.0060)	(0.0046)	(0.0041)	(0.0035)	(0.0027)	(0.0016)
Observations	675,221	675,221	675,221	675,221	675,221	675,221
Classrooms (clusters)	21346	21346	21346	21346	21346	21346
Bdw selection	0.820	0.820	0.820	0.820	0.820	0.820
Panel B						
Award	0.0127*	0.0102*	0.0078	0.0078*	0.0039	0.0019
	(0.0072)	(0.0055)	(0.0049)	(0.0042)	(0.0032)	(0.0018)
Students (obs.)	471,656	471,656	471,656	471,656	471,656	471,656
Classrooms (clusters)	14917	14917	14917	14917	14917	14917
Bdw selection	0.620	0.620	0.620	0.620	0.620	0.620
Panel C						
Award	0.0138*	0.0085**	0.0088**	0.0086**	0.0035	0.0019
	(0.0070)	(0.0042)	(0.0039)	(0.0035)	(0.0029)	(0.0017)
Students (obs.)	485,727	816,077	736,985	675,221	585,723	585,723
Classrooms (clusters)	15373	25812	23323	21346	18499	18499
Bdw selection	0.652	0.967	0.902	0.814	0.763	0.773
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Variable control mean	0.55	0.22	0.17	0.11	0.06	0.02
Std. Beta	0.016	0.041	0.051	0.078	0.078	0.114

Notes: Sample includes 11^{th} graders only. The table displays the award impact on classmates SAT performance at $t+1$ using a regression discontinuity design specified at equation 1.2. The regressions are all estimated on a sample within h s.d. above and below the award threshold. Panel A, report the results for $h = .82$, Panel B report results for $h = .62$ and Panel C, for various bandwidths - the optimal bandwidth for each individual outcome. In addition to the *Award* variable, the specification includes the following covariates (coefficients not reported): $Score_c$, $Award_c \times Score_c$. All columns include controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of national school quality distribution. In column 1, the outcome *Participate in SAT* is equal to 1 if the student participated in the SAT at $t+1$; Columns 2 to 6 report if the student score exceeded different percentiles of the SAT national distribution. The outcome in Column 2, for example, is equal to 1 if the student score exceeded the 50th percentile of the national SAT score distribution. All outcomes are well defined, assuming values 1 or 0, (not missing) for the entire sample. Standard errors are clustered at the classroom level. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1%

Table A.7: *Impact of Award assigned at Year t on Educational Outcomes at $t+1$ (Classmates)*

Classmates		(1)		(2)		(3)		LHS control	
	summary measure (s.d)	0.0186 *	(0.0095)	0.015 *	(0.0081)			μ	β/μ
MO	Score in top 30th percentile	0.0008	(0.0010)	0.0004	(0.0009)	0.0004	(0.0009)	1%	4%
	Participated in MO exam	0.0022	(0.0015)	0.0016	(0.0013)	0.0013	(0.0013)	2%	7%
	Qualified to MO	0.0010	(0.0024)	-0.0001	(0.0020)	-0.0008	(0.0022)	6%	0%
SAT	Score in top 30th percentile	0.0078 *	(0.0042)	0.0086 **	(0.0035)	0.0086 **	(0.0035)	11%	8%
	Score in top 50th percentile	0.0112 *	(0.0059)	0.0103 **	(0.0050)	0.0103 **	(0.0050)	27%	4%
	Participated in SAT	0.0127 *	(0.0072)	0.0089	(0.0060)	0.0138 **	(0.0070)	55%	2%
Test Score	Std. Math+ELA scores (s.d.)	0.0129	(0.0358)	0.0133	(0.0301)	0.0128	(0.0321)		1%
School progress	grade attainment	0.0043	(0.0058)	0.0071	(0.0049)	0.0055	(0.0047)	78%	1%
	no dropout	0.0036	(0.0037)	0.0029	(0.0031)	0.0024	(0.0031)	89%	0%
Bandwidth selection		h=.62		h=.82		(.62 < h < .89)			

Notes: Reports β from main specification specified at equation 1.2. Number of clusters for test score specification is 2717, for the remaining (MO, SAT and Progress in school) is 21346. Test score comes from SARESP only availaable for the State of Sao Paulo. Include controls: Grade FE, MO cohort FE and Quartile of school quality. Standard errors are clustered at the classroom level. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1%

Table A.8: *Impact of Award assigned at Year t on Tertiary Education at $t+2$ (Classmates)*

	Enroll in tertiary education	Probability of attending a college that exceeds selectivity percentile				
		50th	60th	70th	80th	90th
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Award	0.0073**	0.0040*	0.0038*	0.0031*	0.0027**	0.0020***
	(0.0037)	(0.0024)	(0.0020)	(0.0016)	(0.0012)	(0.0007)
Students (obs.)	675,221	675,221	675,221	675,221	675,221	675,221
Classrooms (clusters)	21346	21346	21346	21346	21346	21346
Bdw selection	0.820	0.820	0.820	0.820	0.820	0.820
Panel B						
Award	0.0044	0.0023	0.0030	0.0024	0.0015	0.0017**
	(0.0044)	(0.0028)	(0.0024)	(0.0019)	(0.0014)	(0.0008)
Students (obs.)	471,656	471,656	471,656	471,656	471,656	471,656
Classrooms (clusters)	14917	14917	14917	14917	14917	14917
Bdw selection	0.620	0.620	0.620	0.620	0.620	0.620
Panel C						
Award	0.0045	0.0041*	0.0040**	0.0029*	0.0025**	0.0020**
	(0.0034)	(0.0023)	(0.0019)	(0.0016)	(0.0012)	(0.0008)
Students (obs.)	816,077	705,911	790,612	736,985	736,985	585,723
Classrooms (clusters)	25812	22321	25002	23323	23323	18499
Bdw selection	0.976	0.866	0.963	0.916	0.873	0.767
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Variable control mean	0.158	0.062	0.044	0.028	0.015	0.005
Std. beta	0.046	0.065	0.086	0.111	0.186	0.428

Notes: Sample includes 11$^{\text{th}}$ graders only. The table displays the award impact on classmates enrollment in tertiary education at $t+2$ using a regression discontinuity design specified at equation 2.1. The regressions are all estimated on a sample within h s.d. above and below the award threshold. Panel A, report the results for $h = .82$, Panel B report results for $h = .62$ and Panel C, for various bandwidths - the optimal bandwidth for each individual outcome. In addition to the *Award* variable, the specification includes the following covariates (coefficients not reported): $Score_c$, $Award_c \times Score_c$. All columns include controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of national school quality distribution. In column 1, the outcome *Enroll in tertiary education* is equal to 1 if the student enrolled in tertiary education at $t+2$; Columns 2 to 6 report if the student enrolled in a college which exceeded the different percentiles of the degrees of selectivity (selectivity is defined by the average SAT of students enrolled in that college). The outcome in Column 2, for example, is equal to 1 if the student enrolled in a college which exceeded the 50th percentile of degree of selectivity. All outcomes are well defined, assuming values 1 or 0, (not missing) for the entire sample. Standard errors are clustered at the classroom level. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1%

Table A.9: *Impact of Award assigned at Year t by Classroom Assignment at* $t+1$

Panel A. Classmates	MO score is above the median	
	(1)	(2)
Award	0.0006**	0.0009**
	(0.0003)	(0.0004)
Award x at participant's classroom at t+1	0.0010	
	(0.0007)	
Award x at participant's classroom at t+1		0.0008
		(0.0007)
Students (obs.)	5,114,922	4,164,486
Classrooms (Clusters)	170,331	170,232
Dep. variable control mean	.01223	.01487
Bdw selection	.82	.82
Linear combination:β	.001597	.001643
Linear combination: s.e.	.0005966	.0005959
Controls	Yes	Yes
Sample Restriction: Student passed the grade	No	Yes

Notes: The table displays heterogeneous effects of the award impact on the summary measure at $t+1$ by whether the student continue to be classmates' of the participant at $t+1$. The specification is equivalent to equation 2.1 and includes full set of interactions with "at participant's classroom at $t+1$". The regressions are all estimated on a sample within $h=.82$ s.d. above and below the award threshold. All columns include controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of national school quality distribution. The outcome is whether the student score in the MO at $t+1$ exceeded the 50th percentile. In column 1 and 2, both use the same variable for heterogeneity analysis (whether the student continue to be classmates' of the participant at $t+1$). Column 2 is analogous with column 1, but restricts the sample to include only students who pass the grade. Standard errors are clustered at the classroom level. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1% .

Table A.10: *Impact of Award assigned at Year t by Classroom Assignment at $t+1$ and $t+2$*

Panel A. Classmates	MO score is above the median			
	(1)	(2)	(3)	(4)
	at t+1	at t+2	at t+1	at t+2
Award	0.0005	-0.0000	0.0014***	-0.0002
	(0.0005)	(0.0005)	(0.0005)	(0.0004)
Award x at participant's classroom at t+1	0.0012	0.0004		
	(0.0008)	(0.0008)		
Award x at participant's classroom at t+2			-0.0006	0.0011
			(0.0009)	(0.0009)
Students (obs.)	3,118,997	3,118,997	3,118,997	3,118,997
Classrooms (Clusters)	148,775	148,775	148,775	148,775
Dep. variable control mean	.01587	.01416	.01587	.01416
Bdw selection	.82	.82	.82	.82
Linear combination:β	.001677	.0003654	.0007434	.0009462
Linear combination: s.e.	.0006685	.0006181	.0008113	.0008185
Controls	Yes	Yes	Yes	Yes
Sample Restriction: balanced panel	Yes	Yes	Yes	Yes

Notes: The table displays heterogeneous effects of the award impact on the performance in the MO at $t+1$ by classroom assignment at $t+1$ and $t+2$. The specification is equivalent to equation 2.1 and includes full set of interactions with "at participant's classroom at $t+1$". The regressions are all estimated on a sample within $h = .82$ s.d. above and below the award threshold. All columns include controls. The controls are: grade fixed effect, MO cohort-year fixed effect, indicator variables for each of the quartiles of national school quality distribution. Columns 1 and 2, both use the same variable for heterogeneity analysis (whether the student continue to be classmates' of the participant at $t+1$). In column 1, the outcome is whether the student score in the MO at $t+1$ exceeded the 50th percentile, and in column 2, the same Math Olympiad performance outcome assessed at $t+2$. Columns 3 and 4, both use the same variable for heterogeneity analysis (whether the student continue to be classmates' of the participant at $t+2$). In column 3, the outcome is whether the student score in the MO at $t+1$ exceeded the 50th percentile, and in column 4, the same Math Olympiad performance outcome assessed at $t+2$.Sample restricted to be balance accross all specifications. This implies excluding 2012 MO-cohort and 11th graders. Levels of significance: $*$ 10%, $**$ 5%, and $***$ 1% .

Table A.11: *Selection of Municipalities and Schools into the Sample*

	(1) All Municipalities		(2) Sample Municipalities		(3) Sample Municipalities & School takes PB	
	Mean	SD	Mean	SD	Mean	SD
Municipal Characteristics						
Municipality population	33,290.76	197,908.57	20,201.30	27,236.13	21,180.96	27,771.40
Ruling party from left	0.26	0.44	0.26	0.44	0.26	0.44
Winning party from left	0.30	0.46	0.28	0.45	0.28	0.45
Ruling party from right	0.56	0.50	0.55	0.50	0.55	0.50
Winning party from right	0.53	0.50	0.53	0.50	0.53	0.50
School Characteristics						
Number of schools per municipality	17.85	29.62	14.88	20.54	4.96	6.51
Share urban	0.34	0.47	0.31	0.46	0.73	0.45
Share connected to grid	0.85	0.36	0.86	0.35	0.99	0.11
Share connected to water network	0.45	0.50	0.43	0.50	0.80	0.40
Share connected to sewage system	0.21	0.41	0.18	0.39	0.41	0.49
Share with regular trash collection	0.45	0.50	0.44	0.50	0.85	0.35
Share with Internet	0.29	0.45	0.27	0.45	0.64	0.48
Number of teachers per school	9.67	11.42	8.79	10.17	18.83	11.27
Teacher age	37.26	6.64	37.13	6.56	38.27	3.97
Share of female teachers	0.81	0.28	0.82	0.27	0.85	0.15
Share of teachers with B.A.	0.50	0.41	0.51	0.40	0.70	0.29
Share of teachers who took *Concurso*	0.64	0.38	0.63	0.38	0.76	0.26
Number of students per school	190.37	252.30	163.43	214.72	378.56	245.30
Share of female students	0.47	0.09	0.47	0.09	0.48	0.04
Share of student with urban residence	0.32	0.42	0.29	0.40	0.64	0.39
Number classrooms per school	8.42	8.92	7.66	7.96	15.69	8.44
Students/class per school	18.51	7.38	17.72	7.15	23.41	4.91
Number of 4th graders per school	23.33	35.78	20.34	30.72	49.84	38.31
Number of 8th graders per school	10.66	30.96	8.83	26.65	24.07	41.44
N (municipality-election cycle)	11,106		5,966		5,608	

This table shows descriptive statistics for: all municipalities, municipalities in our sample, and municipalities in our sample with at least one school that participates in the *Prova Brasil* exam. Our sample is selected by dropping: municipalities with irregular elections, municipalities that could potentially go to second round elections, and municipalities where the incumbent political party did not run for re-election. Furthermore, schools that participate in the *Prova Brasil* exam are schools with at least 20 students enrolled in the relevant grade-level. Hence the sample of schools for which we have *Prova Brasil* data for is also "selected." The unit of observations is a municipality-election cycle.

Table A.12: *Descriptive Statistics and Test for Discontinuity in Baseline Characteristics, $|IncumbVoteMargin|$<.09*

	(1) No Party Turnover	(2) Party Turnover	(3) P-value
Number of Municipalities	1,233	1,195	.
Municipal Characteristics			
Population	18,299.92	20,095.88	0.72
Ruling party from left	0.25	0.23	0.78
Winning party from left	0.25	0.30	0.04
Ruling party from right	0.57	0.57	0.36
Winning party from right	0.57	0.52	0.57
School Characteristics			
Share urban	0.26	0.28	0.50
Share connected to grid	0.83	0.84	0.30
Share connected to water network	0.39	0.41	0.84
Share connected to sewage system	0.15	0.16	0.79
Share with regular trash collection	0.37	0.40	0.70
Share with Internet	0.17	0.20	0.21
Number of school staff	15.13	16.24	0.78
Number of teachers per school	7.58	8.05	0.95
Teacher age	36.57	36.60	0.44
Share of female teachers	0.82	0.82	0.17
Share of teachers born in same municipality	0.69	0.69	0.41
Share of teachers with B.A.	0.43	0.44	0.48
Share of teachers who took *Concurso*	0.66	0.68	0.20
Share of teachers who are temporary	0.33	0.31	0.20
Number of classrooms taught per teacher	1.87	1.90	0.25
Number of schools taught per teacher	1.29	1.29	0.50
Share of teachers who teach only in municipal schools	0.93	0.92	0.99
Teacher experience (only in PB)	12.46	12.40	0.88
Share of female headmasters (only in PB)	0.85	0.85	0.27
Headmaster age (only in PB)	40.91	41.44	0.70
Headmaster education experience (only in PB)	14.23	14.59	0.28
Headmaster experience (only in PB)	4.99	5.39	0.69
Number of students per school	152.24	160.96	0.74
Share of female students	0.46	0.47	0.82
Share of students born in same municipality	0.62	0.63	0.72
Share of student with urban residence	0.25	0.27	0.64
Share of students who use school transportation	0.26	0.27	0.11
Number classrooms per school	7.02	7.41	0.73
Students/class per school	17.97	18.08	0.53
Number of 4th graders per school	18.55	20.16	0.93
Number of 8th graders per school	7.62	8.23	0.65
Outcomes of Interest at Baseline			
4th grade test scores (only in PB)	-0.16	-0.12	0.10
8th grade test scores (only in PB)	-0.18	-0.16	0.22
Dropout rate	0.04	0.04	0.85
New headmaster (only in PB)	0.36	0.33	0.80
Share of teachers who are new to the school	0.51	0.52	0.68
Share of teachers who have left the school	0.50	0.51	0.48

This table shows descriptive statistics for municipalities that did not have political party turnover and municipalities that did have political party turnover in close elections, $|IncumbVoteMargin|$<.09, in Columns 1-2. Column 3 tests for a discontinuity in baseline characteristics at the $IncumbVoteMargin$=0 threshold: This column reports the p-value corresponding to the coefficient on $\mathbb{1}\{IncumbVoteMargin < 0\}$ in our main specification, Equation 2.1, with the corresponding variable at baseline used as the dependent variable.

Table A.13: *Political Turnover and 4^{th} Grade Test Scores*

Outcome:	Individual 4^{th} Grade Test Scores (standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.082***	-0.064**	-0.091***	-0.075***	-0.067***	-0.055**
	(0.028)	(0.026)	(0.029)	(0.027)	(0.024)	(0.022)
School-level baseline scores	0.869***	0.739***	0.864***	0.737***	0.861***	0.732***
	(0.014)	(0.014)	(0.015)	(0.015)	(0.012)	(0.012)
N	325,554	325,554	295,170	295,170	429,979	429,979
R-squared	0.218	0.252	0.213	0.248	0.218	0.252
Controls	No	Yes	No	Yes	No	Yes
Clusters	1669	1669	1538	1538	2101	2101
Using Bandwidth	0.0782	0.0782	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.0782	0.0782	0.0782	0.0782	0.0782	0.0782

This table reports the coefficient on political party turnover from regressing individual-level 4^{th} grade test scores on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. Test scores are from the *Prova Brasil* exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. All specifications control for school-level, average test scores for 4^{th} graders at baseline (one year before the respective election). Controls include school-level controls (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet), individual-level controls (an indicator variable for gender, whether the student is white, and whether the student sees their mother reading), and a 2012 election-cycle indicator.

Table A.14: *Political Turnover and 8^{th} Grade Test Scores*

Outcome:	Individual 8^{th} Grade Test Scores (standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.054**	-0.042*	-0.050*	-0.046	-0.059**	-0.049**
	(0.023)	(0.023)	(0.030)	(0.029)	(0.025)	(0.025)
School-level baseline scores	0.789***	0.729***	0.783***	0.725***	0.783***	0.722***
	(0.012)	(0.013)	(0.016)	(0.017)	(0.013)	(0.014)
N	245,302	245,302	126,855	126,855	191,169	191,169
R-squared	0.162	0.174	0.158	0.170	0.157	0.169
Controls	No	Yes	No	Yes	No	Yes
Clusters	1602	1602	965	965	1335	1335
Using Bandwidth	0.151	0.151	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.151	0.151	0.151	0.151	0.151	0.151

This table reports the coefficient on political party turnover from regressing individual-level 8^{th} grade test scores on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. Test scores are from the *Prova Brasil* exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. All specifications control for school-level, average test scores for 8^{th} graders at baseline (one year before the respective election). Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet), individual-level controls taken from the *Prova Brasil* questionnaire filled out by students (an indicator variable for gender, whether the student is white, and whether the student sees their mother reading), and a 2012 election-cycle indicator.

Table A.15: *Political Turnover and Student Composition*

Outcome:	Female	White	Asset Index	Mother's Education	Mother Reads	Parental Support Index	Works Outside	Previously Failed	Previously Dropped Out
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.007	-0.011	0.059	0.085	-0.0002	-0.002	-0.006	-0.005	-0.002
	(0.006)	(0.013)	(0.215)	(0.097)	(0.011)	(0.038)	(0.004)	(0.012)	(0.004)
N	933,305	933,305	933,143	933,305	933,305	933,143	933,305	691,559	700,956
R-squared	0.001	0.015	0.055	0.010	0.008	0.003	0.002	0.016	0.005
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	2294	2294	2294	2294	2294	2294	2294	2287	2287
Mean Dep Var	0.381	0.231	-1.358	4.091	0.628	-0.0834	0.134	0.369	0.0818
Using Bandwidth	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110
Optimal Bandwidth	0.133	0.103	0.117	0.137	0.133	0.112	0.134	0.0972	0.114

This table reports the coefficient on political party turnover from regressing each of the student characteristic variables on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. The Asset Index is constructed as follows. We standardize the responses to a series of questions – regarding the number of household items (colored TV, radio, fridge, bathroom), whether or not the student has other items (a VCR/DVD player, a freezer, a vacuum cleaner, a computer, Internet), and how often a domestic worker comes to the student's house – by subtracting the overall mean and dividing by the standard deviation of all student responses for each question. We then add all these standardized question responses to arrive at the "Asset Index." The Parental Involvement Index is constructed as follows. We standardize responses to a series of questions – regarding how often the students' parents attend Parent-Teacher Council meetings, whether parents talk to the student about what happens in school, and whether parents incentivize the student to do homework, read, and attend school – by subtracting the overall mean and dividing by the standard deviation of all student responses for each question. We then add all these standardized question responses to arrive at the "Parental Involvement Index." Student characteristics are from the *Prova Brasil* questionnaire filled out by students. Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.16: *Political Turnover in 2008 and 4^{th} Grade Test Scores 1, 3, and 5 Years After the Election*

Outcome:	Individual 4^{th} Grade Test Scores (standardized)					
	2009		2011		2013	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.113**	-0.115***	-0.093	-0.092	-0.080	-0.061
	(0.046)	(0.041)	(0.063)	(0.056)	(0.064)	(0.055)
School-level scores in 2007	0.827***	0.696***	0.796***	0.675***	0.765***	0.616***
	(0.027)	(0.028)	(0.028)	(0.032)	(0.029)	(0.028)
N	138,089	138,089	124,158	124,158	121,986	121,986
R-squared	0.179	0.209	0.164	0.199	0.149	0.203
Controls	No	Yes	No	Yes	No	Yes
Clusters	728	728	728	728	728	728
Using Bandwidth	0.0700	0.0700	0.0700	0.0700	0.0700	0.0700
Optimal Bandwidth	0.0772	0.0772	0.104	0.104	0.102	0.102

This table reports the coefficient on political party turnover from regressing individual-level 4^{th} grade test scores on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth, separately for each year t, where t is one year, three years, and five years after the 2008 election. Test scores are from the *Prova Brasil* exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. All specifications control for school-level, average test scores for 4^{th} graders at baseline (one year before the respective election). Controls include school-level controls (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet), individual-level controls (an indicator variable for gender, whether the student is white, and whether the student sees their mother reading), and a 2012 election-cycle indicator.

Table A.17: *Political Turnover and Headmaster Replacements*

Outcome:	Headmaster is new to the school (as Headmaster)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.278***	0.277***	0.273***	0.272***	0.271***	0.270***
	(0.027)	(0.026)	(0.040)	(0.039)	(0.032)	(0.032)
N	15,011	15,011	7,517	7,517	11,196	11,196
R-squared	0.099	0.103	0.090	0.096	0.096	0.100
Controls	No	Yes	No	Yes	No	Yes
Clusters	2648	2648	1562	1562	2139	2139
Mean Dep Var	0.435	0.435	0.454	0.454	0.446	0.446
Using Bandwidth	0.157	0.157	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.157	0.157	0.157	0.157	0.157	0.157

This table reports the coefficient on political party turnover from regressing an indicator variable for whether the school has a new headmaster on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. New headmasters are those that report being the headmaster of their current school for less than two years on the *Prova Brasil* headmaster questionnaire. Controls include school-level controls (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.18: *Political Turnover and Headmaster Characteristics*

Outcome:	Female	Age	B.A.	Graduate Training	Salary	Hours Worked	Experience in Education	Experience as Headmaster
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.019	-0.230	-0.004	-0.044**	9.107	-0.097	-0.133	-1.756***
	(0.018)	(0.406)	(0.014)	(0.022)	(76.810)	(0.274)	(0.222)	(0.257)
N	11,112	10,989	10,853	10,773	11,019	11,170	11,161	11,176
R-squared	0.033	0.055	0.052	0.254	0.275	0.323	0.149	0.046
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	2142	2141	2132	2130	2141	2144	2136	2142
Mean Dep Variable	0.820	41.62	0.901	0.767	2056	38.69	14.14	5.047
Using Bandwidth	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110
Optimal Bandwidth	0.137	0.139	0.142	0.113	0.117	0.145	0.166	0.134

This table reports the coefficient on political party turnover from regressing each of the headmaster characteristic variables on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. The headmaster characteristics are from the *Prova Brasil* headmaster questionnaire. Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.19: *Political Turnover and Politically Appointed Headmasters*

	(1) Headmaster New & not Political	(2) Headmaster New & Political	(3) Headmaster New & not Political	(4) Headmaster New & Political
$\mathbf{1}\{IncumbVoteMargin < 0\}$	0.418*	1.301***	0.413*	1.303***
	(0.232)	(0.170)	(0.231)	(0.168)
N	10,662	10,662	10,662	10,662
Controls	No	No	Yes	Yes
Clusters	2119	2119	2119	2119
Using Bandwidth	0.110	0.110	0.110	0.110

This table reports the coefficient on political party turnover from a multinomial logistic regression with y_{smt} as the categorical outcome variable and the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables as the right hand side variables, for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. y_{smt} is equal to 0 (the referent category) if the headmaster of a school is not a new headmaster, equal to 1 if the headmaster is a new headmaster *but* not a political appointee ("Headmaster New & not Political"), and equal to 2 if the headmaster is a new headmaster *and* a political appointee ("Headmaster New & Political"). New headmasters are those that report being the headmaster of their current school for less than two years on the *Prova Brasil* headmaster questionnaire. Politically appointed headmasters are those who report being some type of "appointee" on the *Prova Brasil* headmaster questionnaire. Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.20: *Political Turnover and Teacher Replacements*

Outcome:	Share of Teachers New to the School						Share of Teachers that have Left the School					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.117***	0.119***	0.112***	0.113***	0.101***	0.102***	0.114***	0.115***	0.106***	0.107***	0.098***	0.099***
	(0.018)	(0.018)	(0.022)	(0.022)	(0.019)	(0.019)	(0.018)	(0.018)	(0.022)	(0.022)	(0.019)	(0.019)
N	38,065	38,065	21,885	21,885	32,883	32,883	38,808	38,808	21,885	21,885	32,883	32,883
R-squared	0.026	0.032	0.027	0.031	0.030	0.035	0.024	0.028	0.025	0.028	0.027	0.031
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Clusters	2297	2297	1509	1509	2056	2056	2327	2327	1509	1509	2056	2056
Mean Dep Var	0.464	0.464	0.464	0.464	0.459	0.459	0.448	0.448	0.449	0.449	0.444	0.444
Using Bandwidth	0.130	0.130	0.0700	0.0700	0.110	0.110	0.133	0.133	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.130	0.130	0.130	0.130	0.130	0.130	0.133	0.133	0.133	0.133	0.133	0.133

This table reports the coefficient on political party turnover from regressing the share of teachers the are new to the school or the share of teachers that have left a school on the running variable of the RDD (*IncumbVoteMargin*), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. The share of teachers that are new to a school is computed using the School Census and corresponds to the share of teachers in a school who are in that school at time t (one year after the respective election) but were not in that same school at time $t-2$ (the year before the respective election). The share of teachers that have left a school is also computed using the School Census and corresponds to the share of teachers in a school who were in that school at time $t-2$ (the year before the respective election) but are no longer in that same school at time t (one year after the respective election). Controls include school-level controls (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.21: *Political Turnover and Teacher Characteristics*

Outcome:	N Teachers (1)	Age (2)	Female (3)	B.A. (4)	Graduate Training (5)	Temporary Contract (6)	Contract Type Missing (7)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.196	-0.400	-0.015	-0.073***	-0.023*	0.042	0.010*
	(0.279)	(0.364)	(0.014)	(0.023)	(0.014)	(0.034)	(0.006)
N	39,642	39,642	39,642	39,642	39,642	20,945	20,945
R-squared	0.507	0.060	0.068	0.295	0.200	0.121	0.024
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	2304	2304	2304	2304	2304	1523	1523
Mean Dep Var	7.859	37.31	0.815	0.485	0.155	0.344	0.0184
Using Bandwidth	0.110	0.110	0.110	0.110	0.110	0.110	0.110
Optimal Bandwidth	0.0922	0.144	0.0995	0.0917	0.0884	0.0915	0.169

This table reports the coefficient on political party turnover from regressing each of the teacher characteristic variables on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. The teacher characteristics are from the School Census and are averaged at the school-level. Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.22: *Political Turnover and Headmaster Replacement in Low- and High-income Municipalities*

Outcome:	Headmaster is new to the school (as Headmaster)					
Panel A	Low Income Municipalities (Below Median Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.389***	0.389***	0.371***	0.371***	0.379***	0.378***
	(0.038)	(0.037)	(0.047)	(0.045)	(0.039)	(0.038)
N	6,703	6,703	4,294	4,294	6,447	6,447
R-squared	0.151	0.154	0.160	0.168	0.156	0.159
Controls	No	Yes	No	Yes	No	Yes
Clusters	1073	1073	754	754	1030	1030
Mean Dep Variable	0.447	0.447	0.447	0.447	0.445	0.445
Using Bandwidth	0.116	0.116	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.116	0.116	0.116	0.116	0.116	0.116
Panel B	High Income Municipalities (Above Median Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.126***	0.127***	0.131**	0.134**	0.115**	0.122**
	(0.043)	(0.043)	(0.064)	(0.063)	(0.048)	(0.048)
N	5,870	5,870	3,223	3,223	4,749	4,749
R-squared	0.052	0.053	0.032	0.036	0.046	0.048
Controls	No	Yes	No	Yes	No	Yes
Clusters	1272	1272	808	808	1109	1109
Mean Dep Variable	0.433	0.433	0.464	0.464	0.449	0.449
Using Bandwidth	0.135	0.135	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.135	0.135	0.135	0.135	0.135	0.135

This table shows the same analysis as in Table A.17 separately for low-income (Panel A) and high-income (Panel B) municipalities. Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

Table A.23: *Political Turnover and 4th Grade Test Scores in Low- and High-income Municipalities*

Outcome:	Individual 4th Grade Test Scores (standardized)					
Panel A	Low Income Municipalities (Below Median Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.060	-0.038	-0.069*	-0.053	-0.061*	-0.047
	(0.037)	(0.035)	(0.039)	(0.037)	(0.032)	(0.031)
School-level baseline scores	0.737***	0.667***	0.738***	0.669***	0.726***	0.654***
	(0.025)	(0.024)	(0.024)	(0.023)	(0.022)	(0.022)
N	148,635	148,635	127,443	127,443	188,065	188,065
R-squared	0.111	0.152	0.112	0.152	0.109	0.151
Controls	No	Yes	No	Yes	No	Yes
Clusters	802	802	718	718	987	987
Using Bandwidth	0.0812	0.0812	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.0812	0.0812	0.0812	0.0812	0.0812	0.0812

Panel B	High Income Municipalities (Above Median Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.038	-0.031	-0.101**	-0.089**	-0.067*	-0.060*
	(0.034)	(0.032)	(0.042)	(0.037)	(0.035)	(0.031)
School-level baseline scores	0.733***	0.642***	0.732***	0.637***	0.744***	0.653***
	(0.020)	(0.018)	(0.026)	(0.025)	(0.020)	(0.019)
N	266,709	266,709	167,727	167,727	241,914	241,914
R-squared	0.108	0.143	0.102	0.138	0.112	0.147
Controls	No	Yes	No	Yes	No	Yes
Clusters	1180	1180	820	820	1114	1114
Using Bandwidth	0.120	0.120	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.120	0.120	0.120	0.120	0.120	0.120

This table shows the same analysis as in Table A.13 separately for low-income (Panel A) and high-income (Panel B) municipalities. Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

Table A.24: *Political Turnover and Headmaster Replacements in Non-municipal Schools*

Outcome:	Headmaster is new to the school (as Headmaster)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.008	-0.016	0.002	-0.019	0.027	0.008
	(0.027)	(0.025)	(0.039)	(0.036)	(0.032)	(0.030)
N	7,762	7,762	4,050	4,050	5,780	5,780
R-squared	0.001	0.023	0.001	0.029	0.000	0.025
Controls	No	Yes	No	Yes	No	Yes
Clusters	2321	2321	1374	1374	1858	1858
Mean Dep Var	0.389	0.389	0.387	0.387	0.395	0.395
Using Bandwidth	0.158	0.158	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.158	0.158	0.158	0.158	0.158	0.158

This table shows a similar analysis to that of Table A.17 with the key difference that the estimation sample for this table is *non-municipal* schools. The set of *non-municipal* schools for this outcome is comprised of state and federal schools, since only public schools participate in the *Prova Brasil* exam.

Table A.25: *Political Turnover and Teacher Replacements in Non-municipal Schools*

Outcome:	Share of Teachers New to the School						Share of Teachers that have Left the School					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.011	0.015	0.037**	0.034**	0.021	0.022	0.015	0.015	0.039**	0.034**	0.023*	0.020
	(0.013)	(0.013)	(0.019)	(0.017)	(0.015)	(0.014)	(0.012)	(0.012)	(0.018)	(0.017)	(0.014)	(0.013)
Observations	13,819	13,819	7,449	7,449	10,774	10,774	14,427	14,427	7,449	7,449	10,774	10,774
R-squared	0.001	0.018	0.003	0.015	0.001	0.018	0.001	0.023	0.003	0.021	0.002	0.025
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Clusters	2466	2466	1521	1521	2064	2064	2551	2551	1521	1521	2064	2064
Mean Dep Variable	0.477	0.477	0.475	0.475	0.475	0.475	0.460	0.460	0.455	0.455	0.458	0.458
Using Bandwidth	0.147	0.147	0.0700	0.0700	0.110	0.110	0.156	0.156	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.147	0.147	0.147	0.147	0.147	0.147	0.156	0.156	0.156	0.156	0.156	0.156

This table shows a similar analysis to that of Table A.20 with the key difference that the estimation sample for this table is *non-municipal* schools. The set of *non-municipal* schools for this outcome is comprised of state, federal, and private schools (since all schools participate in School Census

Table A.26: *Political Turnover and 4th Grade Test Scores in Non-municipal Schools*

Outcome:	Individual 4th Grade Test Scores (standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.013	0.024	-0.005	0.024	0.007	0.025
	(0.031)	(0.029)	(0.044)	(0.040)	(0.035)	(0.033)
School-level baseline scores	0.805***	0.707***	0.806***	0.707***	0.816***	0.716***
	(0.016)	(0.016)	(0.020)	(0.020)	(0.018)	(0.018)
N	152,086	152,086	89,753	89,753	126,439	126,439
R-squared	0.157	0.191	0.154	0.188	0.158	0.192
Controls	No	Yes	No	Yes	No	Yes
Clusters	1161	1161	755	755	1015	1015
Using Bandwidth	0.135	0.135	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.135	0.135	0.135	0.135	0.135	0.135

This table shows a similar analysis to that of Table A.13 with the key difference that the estimation sample for this table is *non-municipal* schools. The set of *non-municipal* schools for this outcome is comprised of state and federal schools, since only public schools participate in the *Prova Brasil* exam.

Table A.27: *Political Turnover and 4th Grade Test Scores in Low- and High-quality Municipal Schools*

Outcome:	Individual 4th Grade Test Scores (standardized)					
Panel A	Low Quality Schools (Below Median Baseline Test Scores)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.082**	-0.059*	-0.078**	-0.051	-0.075**	-0.052*
	(0.033)	(0.031)	(0.039)	(0.037)	(0.033)	(0.031)
School-level baseline scores	0.867***	0.725***	0.860***	0.715***	0.860***	0.716***
	(0.026)	(0.025)	(0.032)	(0.029)	(0.026)	(0.025)
N	187,409	187,409	122,528	122,528	181,445	181,445
R-squared	0.074	0.122	0.074	0.121	0.074	0.122
Controls	No	Yes	No	Yes	No	Yes
Clusters	1186	1186	818	818	1150	1150
Using Bandwidth	0.113	0.113	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.113	0.113	0.113	0.113	0.113	0.113

Panel B	High Quality Schools (Above Median Baseline Test Scores)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.056*	-0.053	-0.112***	-0.106***	-0.068**	-0.069**
	(0.034)	(0.033)	(0.039)	(0.035)	(0.032)	(0.029)
School-level baseline scores	0.775***	0.674***	0.775***	0.672***	0.784***	0.683***
	(0.024)	(0.024)	(0.031)	(0.031)	(0.025)	(0.024)
N	234,508	234,508	162,053	162,053	229,476	229,476
R-squared	0.081	0.119	0.079	0.118	0.082	0.121
Controls	No	Yes	No	Yes	No	Yes
Clusters	1338	1338	969	969	1319	1319
Using Bandwidth	0.113	0.113	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.113	0.113	0.113	0.113	0.113	0.113

This table shows the same analysis as in Table A.13 separately for low-quality (Panel A) and high-quality (Panel B) *municipal* schools. Low-quality schools are those below the median in the school-level distribution of test scores at baseline (the year before the respective election). High-quality schools are those above the median in this distribution.

Table A.28: *Political Turnover and School Problems (as Reported by Headmaster)*

Outcome:	Teacher Council Meetings (1)	Coordinated Curriculum (2)	Curriculum Together (3)	Textbooks On-time (4)	Textbooks Correct (5)	Program for Dropouts (6)	Program for Tutoring (7)	Program for Failing Students (8)	Teacher Training Held (9)	Teacher Training Participation (10)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.130**	-0.027**	-0.047**	-0.090***	-0.059**	-0.052*	-0.076***	-0.035*	-0.150***	-0.026**
	(0.066)	(0.011)	(0.021)	(0.026)	(0.025)	(0.027)	(0.025)	(0.019)	(0.031)	(0.013)
N	7,058	7,058	7,058	7,058	7,058	7,058	7,058	7,058	7,058	4,228
R-squared	0.035	0.036	0.155	0.029	0.023	0.086	0.055	0.123	0.024	0.488
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	1819	1819	1819	1819	1819	1819	1819	1819	1819	1521
Mean Dep Var	2.557	0.963	0.776	0.776	0.794	0.621	0.835	0.845	0.607	0.639
Using Bandwidth	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110
Optimal Bandwidth	0.160	0.119	0.142	0.115	0.138	0.113	0.132	0.139	0.120	0.117

This table reports the coefficient on political party turnover from regressing each of the outcome variables (survey responses) on the running variable of the RDD (*IncumbVoteMargin*), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. The survey responses are from the *Prova Brasil* headmaster questionnaire. Teacher Council Meetings refers to the number of teacher council meetings that have been held in the school this year (ranges from 0-3). Coordinated Curriculum refers to whether the school has a teaching plan (*Projeto Pedagógico*). Curriculum Together refers to whether the headmasters and teachers developed the teaching plan together. Textbooks On-time refers to whether the school received its textbooks at the beginning of the school-year and Textbooks Correct refers to whether the appropriate textbooks were (eventually) received. Each of the Program variables refers to whether the school offers that particular program for students. Teacher Training Held refers to whether the school held any teacher training sessions. Finally, Teacher Training Participation refers to the share of teachers who participated in the teacher training sessions (conditional on the school holding at least one such session). Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.29: *Political Turnover and School Problems (as Reported by the Proctoring Teacher)*

Outcome:	Teacher Council Meetings (1)	Coordinated Curriculum (2)	Curriculum Together (3)	Relationship w/ Headmaster (Index) (4)	Relationship w/ Teachers (Index) (5)	Collaborative Environment (6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.042	-0.014**	-0.037**	-0.235	-0.059	-0.028
	(0.064)	(0.007)	(0.017)	(0.311)	(0.157)	(0.031)
N	23,409	23,409	23,409	23,409	23,409	23,409
R-squared	0.025	0.021	0.055	0.007	0.022	0.327
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	2087	2087	2087	2087	2087	2087
Mean Dep Var	2.337	0.969	0.800	0.705	0.387	3.677
Using Bandwidth	0.110	0.110	0.110	0.110	0.110	0.110
Optimal Bandwidth	0.146	0.135	0.144	0.190	0.130	0.121

This table reports the coefficient on political party turnover from regressing each of the outcome variables (survey responses) on the running variable of the RDD (*IncumbVoteMargin*), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. The survey responses are from the *Prova Brasil* teacher questionnaire, which is administered to teachers who proctor the exam. Teacher Council Meetings refers to the number of teacher council meetings that have been held in the school this year (ranges from 0-3). Coordinated Curriculum refers to whether the school has a teaching plan (*Projeto Pedagógico*). Curriculum Together refers to whether the headmasters and teachers developed the teaching plan together. The Relationship with Headmaster Index is constructed as follows. We standardize the responses to a series of questions – regarding whether the teacher trusts the headmaster, whether the teacher believes the headmaster motivates her, is committed to the school, innovates, cares about the students, cares about the school personnel, and cares about the school as a whole, and whether the teacher respects the headmaster/feels respected by the headmaster – by subtracting the overall mean and dividing by the standard deviation of all teacher responses for each question. We then add all these standardized responses to arrive at the "Relationship w/ Headmaster Index." The Relationship with Teacher Index is constructed as follows. We standardize the responses to a series of questions – regarding whether the teachers share ideas and whether the teachers work together – by subtracting the overall mean and dividing by the standard deviation of all teacher responses for each question. We then add all these standardized responses to arrive at the "Relationship w/ Teacher Index." Finally, Collaborative Environment refers to how collaborative the teacher feels the school is (on a scale of 1-5, where 5 is very collaborative). Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.30: *Political Turnover and Education Resources*

Panel A	Municipal Level Financial Resources								
Outcome:	Total Expenditures			Education Expenditures			Share of Expenditures on Education		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.6524	-0.3477	-0.2203	-0.0929	-0.0129	0.0944	-0.0120	-0.0071	-0.0076
	(2.0928)	(2.2166)	(1.8907)	(1.0661)	(1.1811)	(0.9950)	(0.0132)	(0.0189)	(0.0149)
N	1,188	919	1,305	1,215	919	1,305	1,543	919	1,305
Mean Dep Variable	14.29	14.41	14.17	7.145	7.187	7.129	0.488	0.488	0.486
Using Bandwidth	0.0982	0.0700	0.110	0.102	0.0700	0.110	0.136	0.0700	0.110
Optimal Bandwidth	0.0982	0.0982	0.0982	0.102	0.102	0.102	0.136	0.136	0.136

Panel B	School Level Financial Resources		
Outcome:	"Does your school experience financial problems?"		
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.023	0.038	0.027
	(0.024)	(0.028)	(0.023)
N	10,813	7,389	11,011
R-squared	0.013	0.014	0.013
Controls	Yes	Yes	Yes
Clusters	2105	1563	2139
Mean Dep Variable	0.601	0.608	0.601
Using Bandwidth	0.108	0.0700	0.110
Optimal Bandwidth	0.108	0.108	0.108

This table reports the coefficient on political party turnover from regressing each of the variables on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. Panel A shows municipal-level regressions, using data from the Ministry of the Economy. Currently, we only have this data for 2009 so the analysis in Panel A is using only the 2008 election-cycle. Total Expenditures refer to a municipality's total budget in 2009 and Education Expenditures refer to how much the municipality spent on education in 2009. Both of these variables are in 2000 prices (in Reais) and are scaled by a factor of 1,000,000. Share of Expenditures on Education is the share of the budget spent on education in 2009. Panel B shows school-level regressions, using data from the *Prova Brasil* headmaster questionnaire (for both election-cycles). Controls in Panel B include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

A.2 Supplementary Figures

Table A.31: *Descriptive Statistics and Comparison of Means for Baseline Characteristics,* $|IncumbVoteMargin| < .09$

	(1) No Party Turnover	(2) Party Turnover	(3) P-value (1)-(2)
Number of Municipalities	1,233	1,195	.
Municipal Characteristics			
Population	18,299.92	20,095.88	0.22
Ruling party from left	0.25	0.23	0.43
Winning party from left	0.25	0.30	0.00
Ruling party from right	0.57	0.57	0.74
Winning party from right	0.57	0.52	0.02
School Characteristics			
Share urban	0.26	0.28	0.23
Share connected to grid	0.83	0.84	0.57
Share connected to water network	0.39	0.41	0.20
Share connected to sewage system	0.15	0.16	0.61
Share with regular trash collection	0.37	0.40	0.23
Share with Internet	0.17	0.20	0.00
Number of school staff	15.13	16.24	0.15
Number of teachers per school	7.58	8.05	0.19
Teacher age	36.57	36.60	0.91
Share of female teachers	0.82	0.82	0.80
Share of teachers born in same municipality	0.69	0.69	0.97
Share of teachers with B.A.	0.43	0.44	0.57
Share of teachers who took*Concurso*	0.66	0.68	0.38
Share of teachers who are temporary	0.33	0.31	0.38
Number of classrooms taught per teacher	1.87	1.90	0.57
Number of schools taught per teacher	1.29	1.29	0.89
Share of teachers who teach only in municipal schools	0.93	0.92	0.25
Teacher experience (only in PB)	12.46	12.40	0.66
Share of female headmasters (only in PB)	0.85	0.85	0.56
Headmaster age (only in PB)	40.91	41.44	0.03
Headmaster education experience (only in PB)	14.23	14.59	0.03
Headmaster experience (only in PB)	4.99	5.39	0.02
Number of students per school	152.24	160.96	0.29
Share of female students	0.46	0.47	0.45
Share of students born in same municipality	0.62	0.63	0.47
Share of student with urban residence	0.25	0.27	0.25
Share of students who use school transportation	0.26	0.27	0.48
Number classrooms per school	7.02	7.41	0.20
Students/class per school	17.97	18.08	0.73
Number of 4th graders per school	18.55	20.16	0.14
Number of 8th graders per school	7.62	8.23	0.31
Outcomes of Interest at Baseline			
4th grade test scores (only in PB)	-0.16	-0.12	0.23
8th grade test scores (only in PB)	-0.18	-0.16	0.51
Dropout rate	0.04	0.04	0.14
New headmaster (only in PB)	0.36	0.33	0.13
Share of teachers who are new to the school	0.51	0.52	0.90
Share of teachers who have left the school	0.50	0.51	0.60

This table shows descriptive statistics for municipalities that did not have political party turnover and municipalities that did have political party turnover in close elections, $|IncumbVoteMargin|$<.09, in Columns 1-2. Column 3 tests whether the mean of each variable is significantly different for municipalities that did not have political party turnover (Column 1) and municipalities that did have political party turnover (Column 2).

Table A.32: *Political Turnover and Dropout Rates*

Outcome:	School-level Dropout Rates					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.0039	0.0033	0.0049	0.0050	0.0031	0.0031
	(0.0036)	(0.0033)	(0.0034)	(0.0032)	(0.0033)	(0.0030)
Baseline dropout rate	0.3423***	0.3130***	0.3399***	0.3139***	0.3380***	0.3060***
	(0.0248)	(0.0231)	(0.0284)	(0.0263)	(0.0207)	(0.0194)
N	31,742	31,742	26,492	26,492	39,661	39,661
R-squared	0.1446	0.1651	0.1502	0.1681	0.1391	0.1614
Controls	No	Yes	No	Yes	No	Yes
Clusters	2029	2029	1783	1783	2412	2412
Mean Dep Var	0.0337	0.0337	0.0323	0.0323	0.0335	0.0335
Using Bandwidth	0.0836	0.0836	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.0836	0.0836	0.0836	0.0836	0.0836	0.0836

This table reports the coefficient on political party turnover from regressing school-level dropout rates on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth. The school-level dropout rate is measured by the School Census and refers to the dropout rate for all students within a school (in all grade levels). All specifications control for school-level, dropout rate at baseline (the year before the respective election). Controls include school-level controls taken from the School Census (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.33: *Political Turnover in 2008 and 8th Grade Test Scores 1, 3, and 5 Years After the Election*

Outcome:	Individual 8th Grade Test Scores (standardized)					
	2009		2011		2013	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.043	-0.053	-0.053	-0.075	-0.111	-0.131*
	(0.049)	(0.050)	(0.066)	(0.059)	(0.075)	(0.067)
both_score_8_std08_2007	0.791***	0.732***	0.819***	0.736***	0.648***	0.570***
	(0.027)	(0.027)	(0.033)	(0.034)	(0.037)	(0.037)
Observations	50,338	50,338	49,142	49,142	49,229	49,229
R-squared	0.152	0.162	0.159	0.178	0.103	0.124
Controls	No	Yes	No	Yes	No	Yes
Clusters	432	432	432	432	432	432
Using Bandwidth	0.0700	0.0700	0.0700	0.0700	0.0700	0.0700
Optimal Bandwidth	0.122	0.122	0.120	0.120	0.110	0.110

This table reports the coefficient on political party turnover from regressions of individual-level 8th grade test scores on the running variable of the RDD ($IncumbVoteMargin$), political party turnover ($\mathbb{1}\{IncumbVoteMargin < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin|$<Using Bandwidth, separately for each year t, where t is one year, three years, and five years after the 2008 election. All specifications control for school-level, average test scores for 8th graders at baseline (one year before the respective election). Controls include school-level controls (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet), individual-level controls (an indicator variable for gender, whether the student is white, and whether the student sees their mother reading), and a 2012 election-cycle indicator. Test scores are from the *Prova Brasil* exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party.

Table A.34: *Political Turnover in 2008 and Headmaster Replacement 1, 3, and 5 Years After the Election*

Outcome:	Headmaster is new to the school (as Headmaster)					
	2009		2011		2013	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.274***	0.271***	-0.056	-0.054	-0.056	-0.064
	(0.050)	(0.051)	(0.042)	(0.042)	(0.056)	(0.055)
N	4,882	4,882	3,966	3,966	3,794	3,794
R-squared	0.090	0.091	0.002	0.005	0.005	0.014
Controls	No	Yes	No	Yes	No	Yes
Clusters	1082	1082	995	995	969	969
Mean Dep Variable	0.438	0.438	0.348	0.348	0.665	0.665
Using Bandwidth	0.110	0.110	0.110	0.110	0.110	0.110
Optimal Bandwidth	0.128	0.128	0.152	0.152	0.0785	0.0785

This table shows the coefficient on political party turnover in 2008 from regressing an indicator variable for whether the school has a new headmaster on the running variable of the RDD ($IncumbVoteMargin_{2008}$), political party turnover ($\mathbb{1}\{IncumbVoteMargin_{2008} < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin_{2008}|$<Using Bandwidth, separately for each year t, where t is one year, three years, and five years after the 2008 election. New headmasters are those that report being the headmaster of their current school for less than two years on the *Prova Brasil* headmaster questionnaire. Controls include school-level controls (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.35: *Political Turnover in 2008 and Teacher Replacements 1, 3, and 5 Years After the Election*

Outcome:	Share of Teachers New to the School						Share of Teachers that have Left the School					
	2009		2011		2013		2009		2011		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.098***	0.097***	0.042	0.040	0.006	0.005	0.093***	0.092***	0.036	0.035	0.016	0.015
	(0.033)	(0.032)	(0.038)	(0.038)	(0.039)	(0.039)	(0.034)	(0.034)	(0.038)	(0.038)	(0.035)	(0.035)
N	12,637	12,637	12,637	12,637	12,637	12,637	12,637	12,637	12,637	12,637	12,637	12,637
R-squared	0.019	0.023	0.005	0.014	0.001	0.007	0.017	0.021	0.004	0.010	0.001	0.006
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Clusters	944	944	944	944	944	944	944	944	944	944	944	944
Mean Dep Variable	0.489	0.489	0.446	0.446	0.526	0.526	0.465	0.465	0.445	0.445	0.499	0.499
Using Bandwidth	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110	0.110
Optimal Bandwidth	0.163	0.163	0.145	0.145	0.153	0.153	0.152	0.152	0.145	0.145	0.145	0.145

This table shows the coefficient on political party turnover in 2008 from regressing the share of teachers the are new to the school or the share of teachers that have left a school on the running variable of the RDD ($IncumbVoteMargin_{2008}$), political party turnover ($\mathbb{1}\{IncumbVoteMargin_{2008} < 0\}$), and the interaction of these two variables for the set of municipalities with $|IncumbVoteMargin_{2008}|$<Using Bandwidth, separately for each year t, where t is one year, three years, and five years after the 2008 election. The share of teachers that are new to a school is computed using the School Census and corresponds to the share of teachers in a school who are in that school at time t but were not in that same school at time $t-2$. The share of teachers that have left a school is also computed using the School Census and corresponds to the share of teachers in a school who were in that school at time $t-2$ but are no longer in that same school at time t. Controls include school-level controls (whether: the school is located in an urban or rural area, the school is connected to the electric grid, the school is connected to the water network, the school is connected to the sewage system, the school's trash is regularly collected, and the school has Internet) and a 2012 election-cycle indicator.

Table A.36: *Political Turnover and Teacher Replacements in Low- and High-income Municipalities*

Outcome:	Share of Teachers New to the School						Share of Teachers that have Left the School					
Panel A	Low Income Municipalities (Below Median Income)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.113***	0.113***	0.130***	0.130***	0.117***	0.117***	0.115***	0.115***	0.127***	0.127***	0.114***	0.113***
	(0.024)	(0.024)	(0.028)	(0.028)	(0.024)	(0.024)	(0.024)	(0.024)	(0.028)	(0.028)	(0.025)	(0.024)
N	24,003	24,003	16,008	16,008	24,337	24,337	25,052	25,052	16,008	16,008	24,337	24,337
R-squared	0.036	0.041	0.032	0.036	0.035	0.040	0.032	0.037	0.029	0.033	0.032	0.037
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Clusters	965	965	707	707	975	975	1001	1001	707	707	975	975
Mean Dep Variable	0.447	0.447	0.453	0.453	0.447	0.447	0.434	0.434	0.439	0.439	0.433	0.433
Using Bandwidth	0.108	0.108	0.0700	0.0700	0.110	0.110	0.114	0.114	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.108	0.108	0.108	0.108	0.108	0.108	0.114	0.114	0.114	0.114	0.114	0.114
Panel B	High Income Municipalities (Above Median Income)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.058***	0.064***	0.063*	0.064**	0.057**	0.064**	0.058***	0.063***	0.049	0.050	0.055**	0.061**
	(0.022)	(0.021)	(0.033)	(0.032)	(0.026)	(0.026)	(0.022)	(0.022)	(0.031)	(0.030)	(0.025)	(0.024)
N	12,321	12,321	5,877	5,877	8,546	8,546	11,148	11,148	5,877	5,877	8,546	8,546
R-squared	0.015	0.026	0.014	0.027	0.013	0.024	0.014	0.023	0.014	0.027	0.013	0.024
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Clusters	1380	1380	802	802	1081	1081	1277	1277	802	802	1081	1081
Mean Dep Variable	0.495	0.495	0.497	0.497	0.498	0.498	0.475	0.475	0.476	0.476	0.478	0.478
Using Bandwidth	0.168	0.168	0.0700	0.0700	0.110	0.110	0.144	0.144	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.168	0.168	0.168	0.168	0.168	0.168	0.144	0.144	0.144	0.144	0.144	0.144

This table shows the same analysis as in Table A.20 separately for low-income (Panel A) and high-income (Panel B) municipalities. Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

Table A.37: *Political Turnover and 8^{th} Grade Test Scores in Low- and High-income Municipalities*

Outcome:	Individual 8^{th} Grade Test Scores (standardized)					
Panel A	Low Income Municipalities (Below Median Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.032	-0.027	-0.034	-0.027	-0.015	-0.007
	(0.028)	(0.028)	(0.037)	(0.037)	(0.031)	(0.031)
School-level baseline scores	0.687***	0.659***	0.663***	0.633***	0.687***	0.655***
	(0.020)	(0.021)	(0.032)	(0.031)	(0.024)	(0.024)
N	143,725	143,725	74,190	74,190	113,464	113,464
R-squared	0.081	0.092	0.072	0.084	0.082	0.093
Controls	No	Yes	No	Yes	No	Yes
Clusters	936	936	549	549	770	770
Using Bandwidth	0.154	0.154	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.154	0.154	0.154	0.154	0.154	0.154

Panel B	High Income Municipalities (Above Median Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.072*	-0.050	-0.079	-0.072	-0.122***	-0.099**
	(0.038)	(0.035)	(0.050)	(0.050)	(0.043)	(0.041)
School-level baseline scores	0.775***	0.725***	0.767***	0.718***	0.763***	0.710***
	(0.027)	(0.025)	(0.030)	(0.030)	(0.029)	(0.027)
N	103,705	103,705	52,665	52,665	77,705	77,705
R-squared	0.108	0.128	0.100	0.120	0.103	0.123
Controls	No	Yes	No	Yes	No	Yes
Clusters	677	677	416	416	565	565
Using Bandwidth	0.151	0.151	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.151	0.151	0.151	0.151	0.151	0.151

This table shows the analysis in Table A.14 separately for low-income (Panel A) and high-income (Panel B) municipalities. Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

Table A.38: *Political Turnover and 8th Grade Test Scores in Non-municipal Schools*

Outcome:	Individual 8th Grade Test Scores (standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.017	-0.011	-0.030	-0.013	-0.031	-0.023
	(0.018)	(0.018)	(0.026)	(0.025)	(0.021)	(0.020)
Baseline Scores	0.760***	0.697***	0.753***	0.688***	0.762***	0.699***
	(0.010)	(0.010)	(0.013)	(0.013)	(0.011)	(0.011)
N	381,972	381,972	222,724	222,724	316,167	316,167
R-squared	0.106	0.125	0.106	0.125	0.107	0.126
Controls	No	Yes	No	Yes	No	Yes
Clusters	2155	2155	1409	1409	1888	1888
Using Bandwidth	0.136	0.136	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.136	0.136	0.136	0.136	0.136	0.136

This table shows a similar analysis to that of Table A.14 with the key difference that the estimation sample for this table is *non-municipal* schools. The set of *non-municipal* schools for this outcome is comprised of state and federal schools, since only public schools participate in the *Prova Brasil* exam.

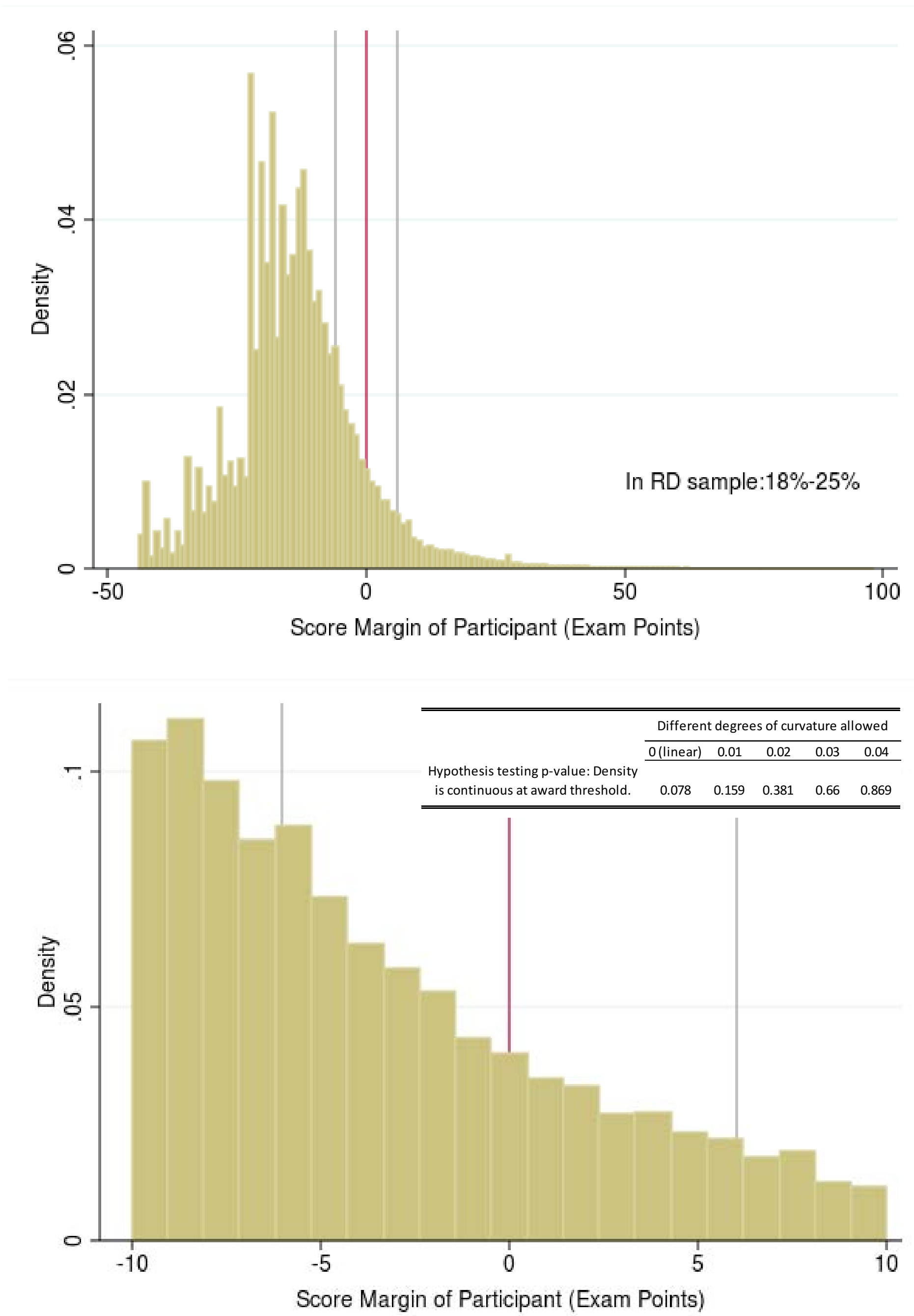
Table A.39: *Political Turnover and 8th Grade Test Scores in Low- and High-quality Municipal Schools*

Outcome:	Individual 8th Grade Test Scores (standardized)					
Panel A	Low Quality Schools (Below Median Baseline Test Scores)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.054*	-0.048	-0.033	-0.025	-0.047	-0.038
	(0.032)	(0.032)	(0.039)	(0.039)	(0.034)	(0.035)
School-level baseline scores	0.674***	0.626***	0.653***	0.603***	0.669***	0.622***
	(0.033)	(0.034)	(0.041)	(0.041)	(0.035)	(0.035)
N	99,103	99,103	59,639	59,639	91,279	91,279
R-squared	0.040	0.054	0.036	0.050	0.040	0.054
Controls	No	Yes	No	Yes	No	Yes
Clusters	811	811	533	533	744	744
Using Bandwidth	0.122	0.122	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.122	0.122	0.122	0.122	0.122	0.122

Panel B	High Quality Schools (Above Median Baseline Test Scores)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	-0.038	-0.018	-0.080*	-0.071*	-0.082**	-0.064*
	(0.035)	(0.035)	(0.043)	(0.042)	(0.036)	(0.035)
School-level baseline scores	0.861***	0.788***	0.853***	0.779***	0.839***	0.762***
	(0.028)	(0.029)	(0.037)	(0.038)	(0.030)	(0.031)
N	105,075	105,075	62,711	62,711	90,880	90,880
R-squared	0.086	0.103	0.085	0.102	0.078	0.095
Controls	No	Yes	No	Yes	No	Yes
Clusters	841	841	548	548	762	762
Using Bandwidth	0.128	0.128	0.0700	0.0700	0.110	0.110
Optimal Bandwidth	0.128	0.128	0.128	0.128	0.128	0.128

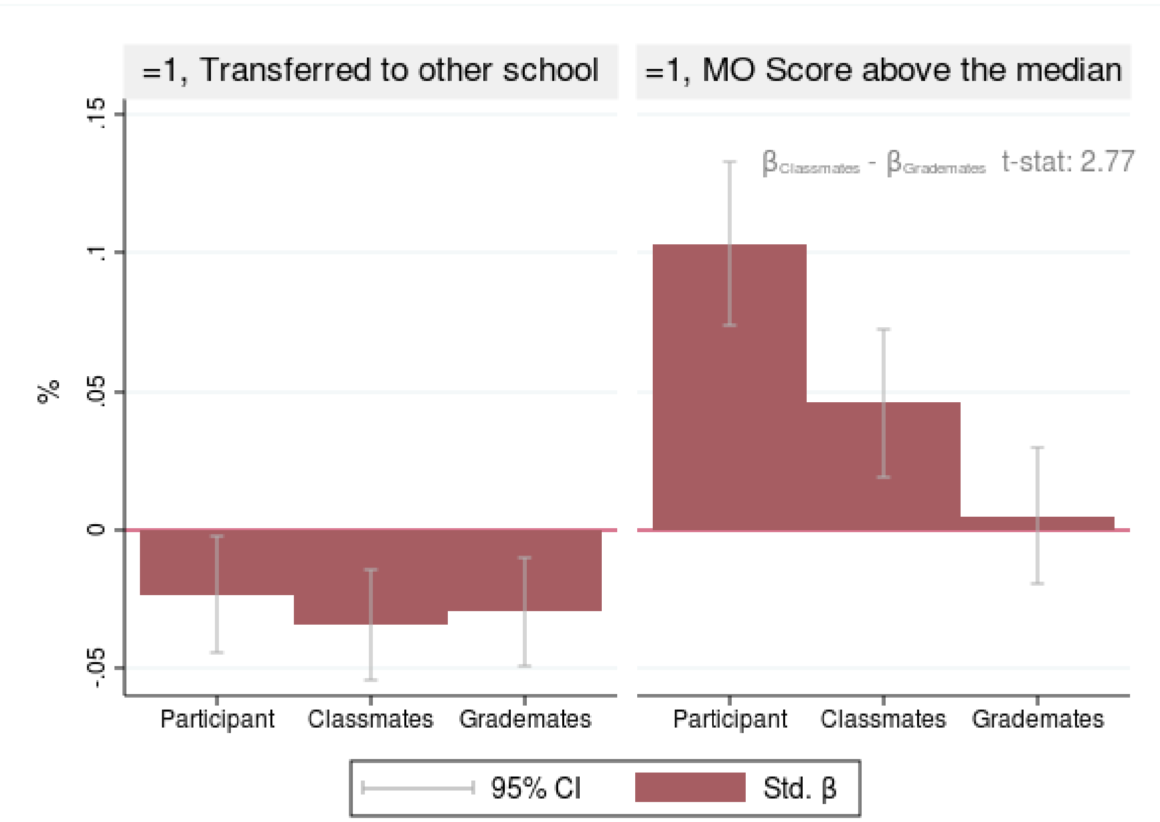
This table shows the same analysis as in Table A.14 separately for low-quality (Panel A) and high-quality (Panel B) *municipal* schools. Low-quality schools are those below the median in the school-level distribution of test scores at baseline (the year before the respective election). High-quality schools are those above the median in this distribution.

Figure A.1: *Density at Award Cutoff*



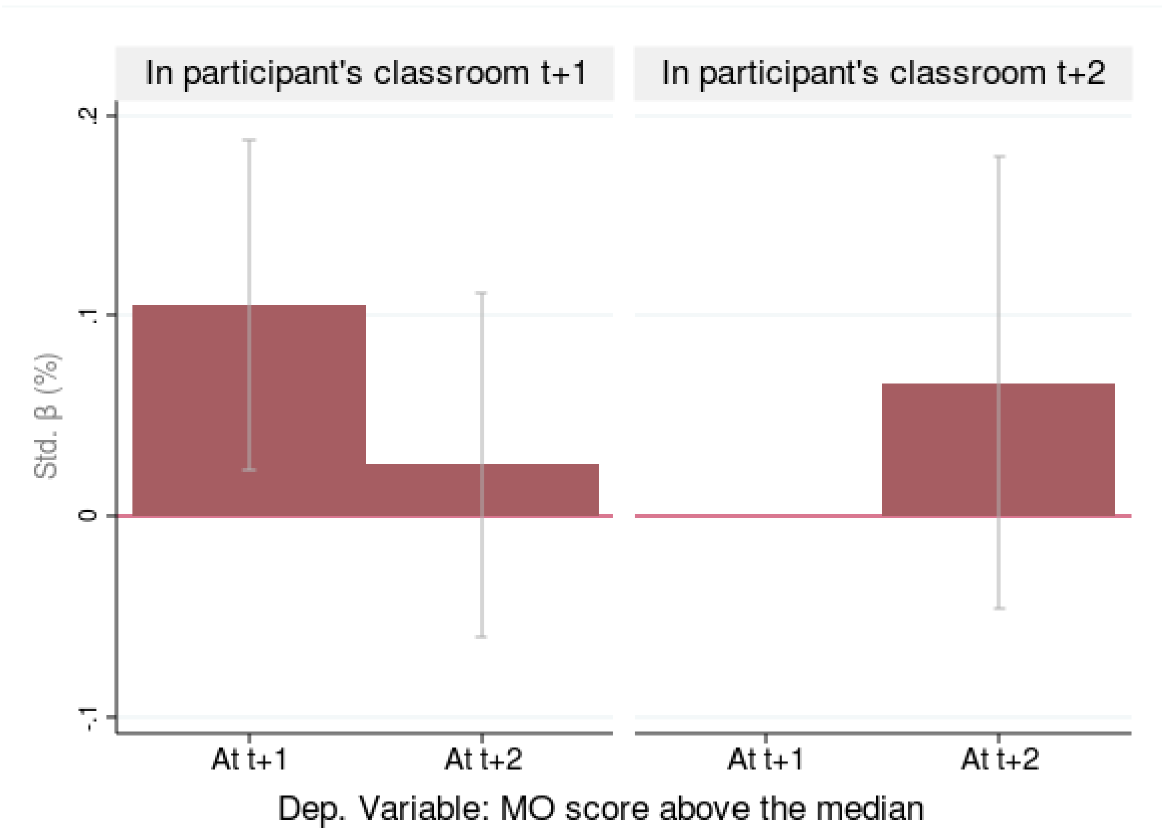
This figure reports the density of MO participants on the running variable, i.e. score margin of Math Olympiad participants. The score margin is in original points of the exam scale. Exam scale was a discrete scale from 0 to 120 points. Top figure reports density for full exam scale. Bottom figure reports density around the regression discontinuity bandwidth (18% to 25% of full exam scale sample). Bottom figure also report the results of the test for whether the density is continuous around the award threshold (similar to McCrary test) allowing for different degrees of curvature in the density function.

Figure A.2: *Impact of award assigned at t on School Choice and MO Performance at $t+1$*



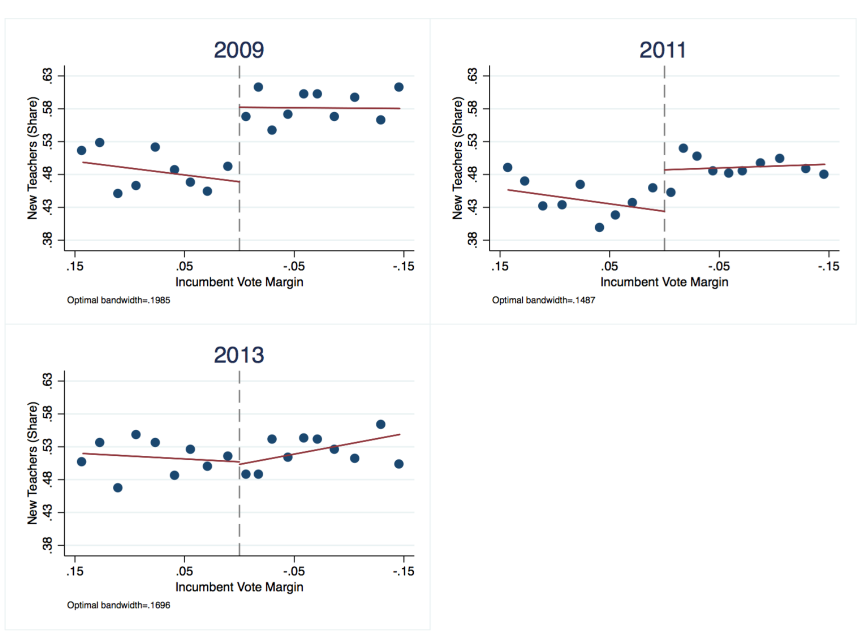
Notes: Outcome on the left is equal to 1 if the students transfer to another school. Outcome on the left is equal to 1 if MO score exceeds the 50th percentile of national distribution. All regressions estimated at the classroom level but for different samples. Classmates regression outcome is the mean of the outcome for all students in the participant's classroom excluding the participant herself. Grade-mates' regression outcome is the mean of the outcome for all students in the participant's grade excluding the participant's classroom. I explain in the text why it is necessary to take the mean rather than estimating at the individual level. The corresponding regression is reported in Table 1.4

Figure A.3: *Impact of award on MO performance at $t+1$ and $t+2$ for students that continue classmates with the participant (Classmates)*



Notes: This figure reports heterogeneous impact by classroom assignment at years $t+1$, figure on the left, and $t+2$, figure on the right. The outcome is equal to 1 if the student's MO score exceeds the 50th percentile of national score distribution (0 otherwise), first and third bar refers to outcome assessed at $t+1$, and second and forth bar refers to outcome assessed at $t+2$. Outcome variable is well defined (1 or 0) for the entire sample. The corresponding regression is reported in Table A.10

Figure A.4: *Political Turnover in 2008 and New Teachers 1, 3, and 5 Years After the Election*



Notes: This figure shows the share of teachers that are new to a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for each year t, where t is one year, three years, and five years after the 2008 election. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor in 2008. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor in 2008. The share of teachers that are new to a school is computed using the School Census and corresponds to the share of teachers in a school who are in that school at time t but were not in that same school at time $t-2$.

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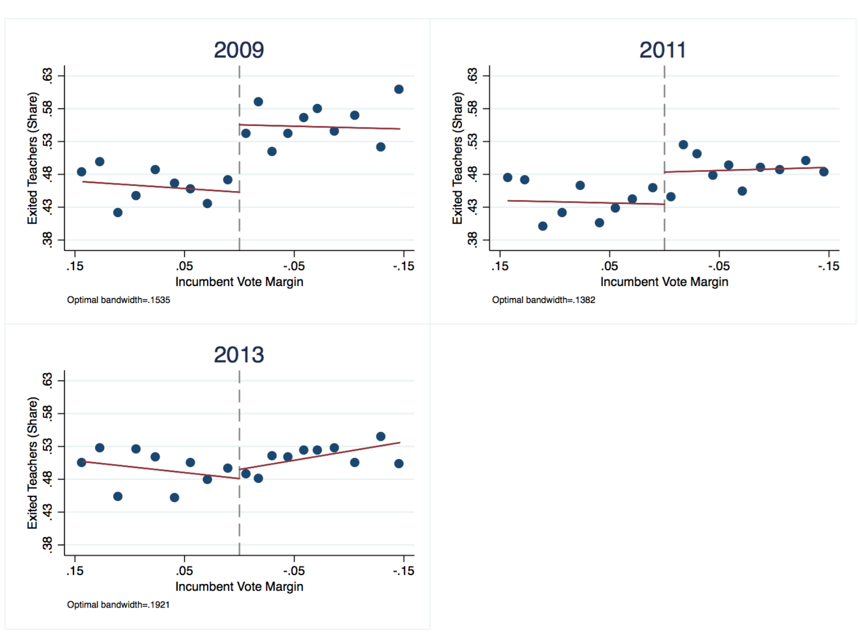
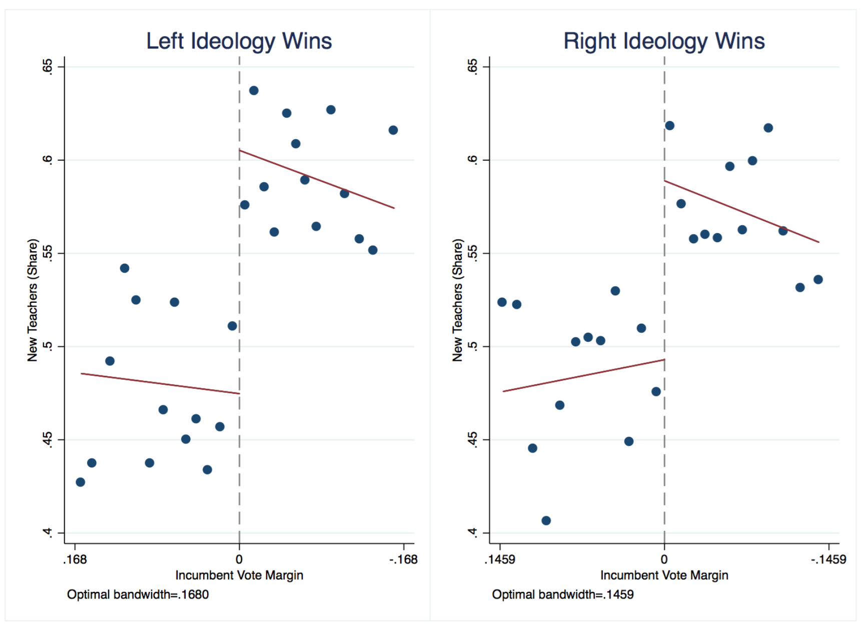
Figure A.5: *Political Turnover in 2008 and Teachers that have Left 1, 3, and 5 Years After the Election*



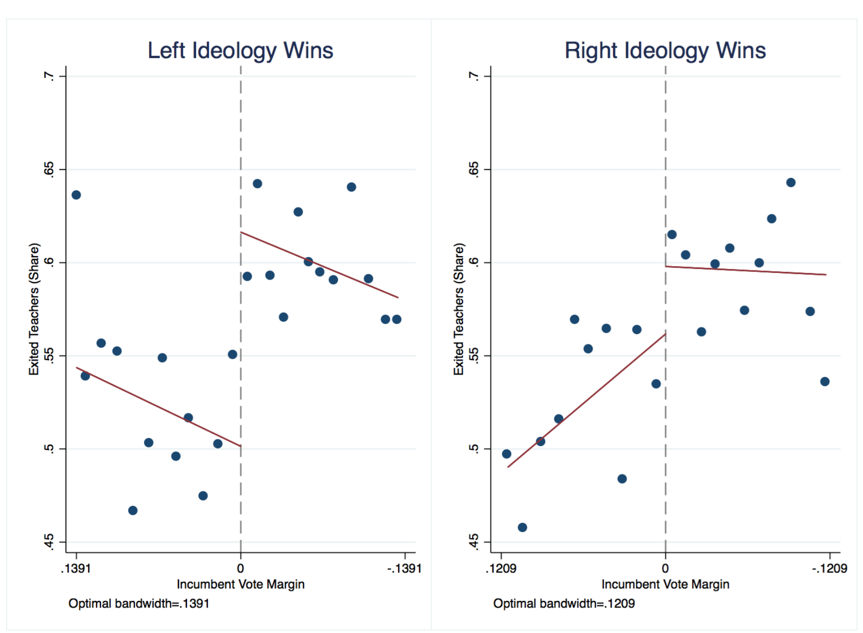
Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for each year t, where t is one year, three years, and five years after the 2008 election. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor in 2008. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor in 2008. The share of teachers that have left a school is computed using the School Census and corresponds to the share of teachers in a school who were in that school at time $t-2$ *but are no longer in that same school at time t.*

Figure A.6: *Political Turnover and New Teachers in Municipalities where the Winning Party was from the Left vs. the Right*



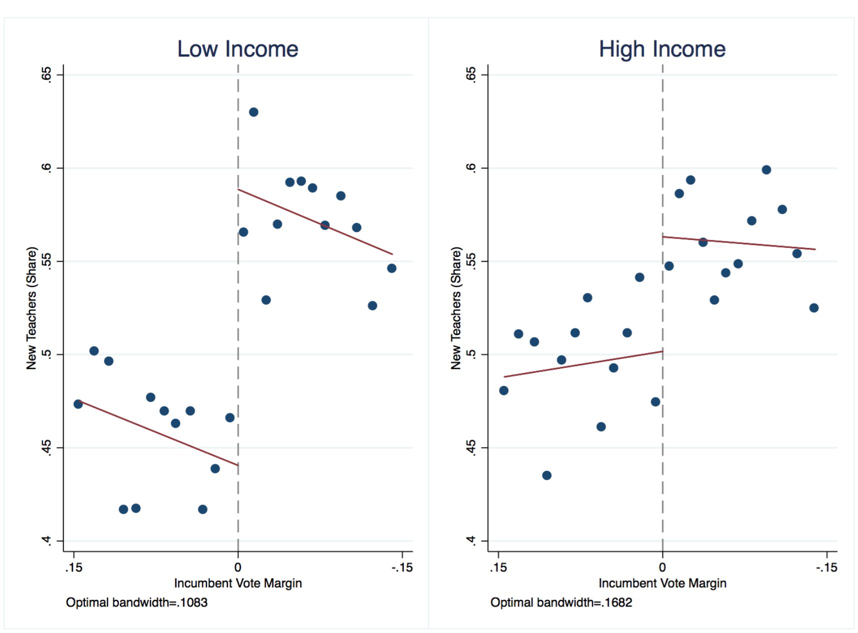
Notes: This figure shows the share of teachers that are new to a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that are new to a school is computed using the School Census and corresponds to the share of teachers in a school who are in that school at time t (one year after the respective election) but were not in that same school at time $t-2$ (the year before the respective election). Party ideology is classified as belonging to the left vs. the right according to Atlas Político – Mapa do Congresso.

Figure A.7: *Political Turnover and Teachers that have Left in Municipalities where the Winning Party was from the Left vs. the Right*



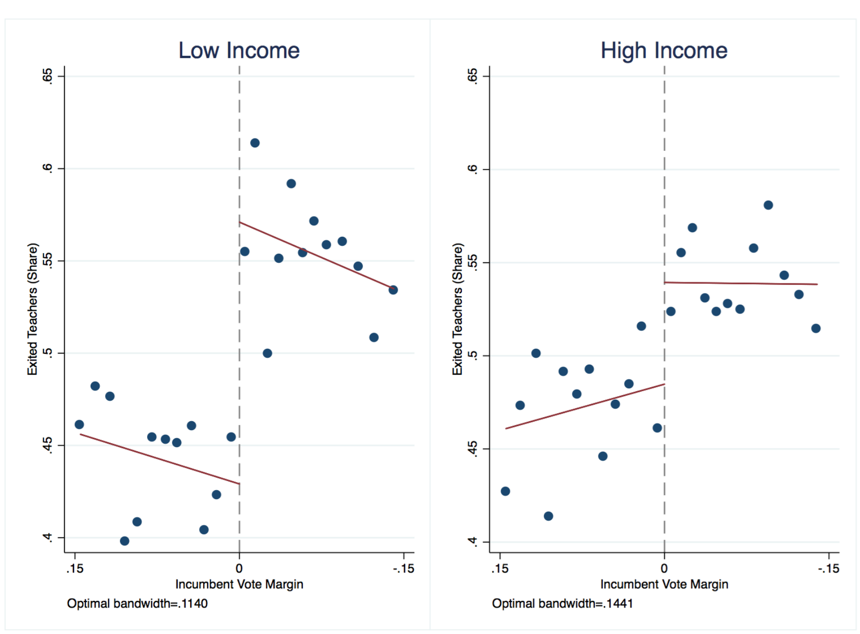
Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that have left a school is computed using the School Census and corresponds to the share of teachers in a school who were in that school at time $t-2$ (the year before the respective election) but are no longer in that same school at time t (one year after the respective election). Party ideology is classified as belonging to the left vs. the right according to Atlas Político – Mapa do Congresso.

Figure A.8: *Political Turnover and New Teachers in Low- and High-income Municipalities*



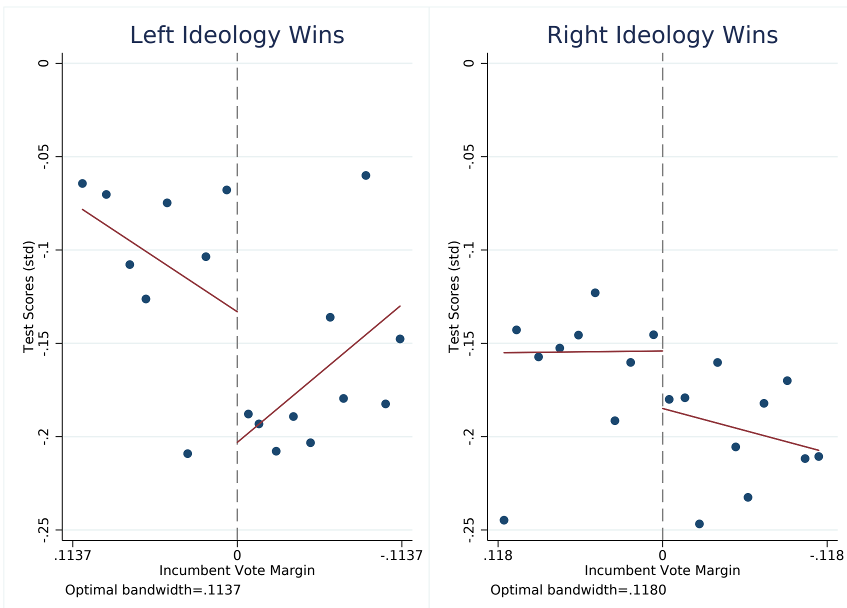
Notes: This figure shows the share of teachers that are new to a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that are new to a school is computed using the School Census and corresponds to the share of teachers in a school who are in that school at time t (one year after the respective election) but were not in that same school at time $t-2$ (the year before the respective election). Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

Figure A.9: *Political Turnover and Teachers that have Left in Low- and High-income Municipalities*



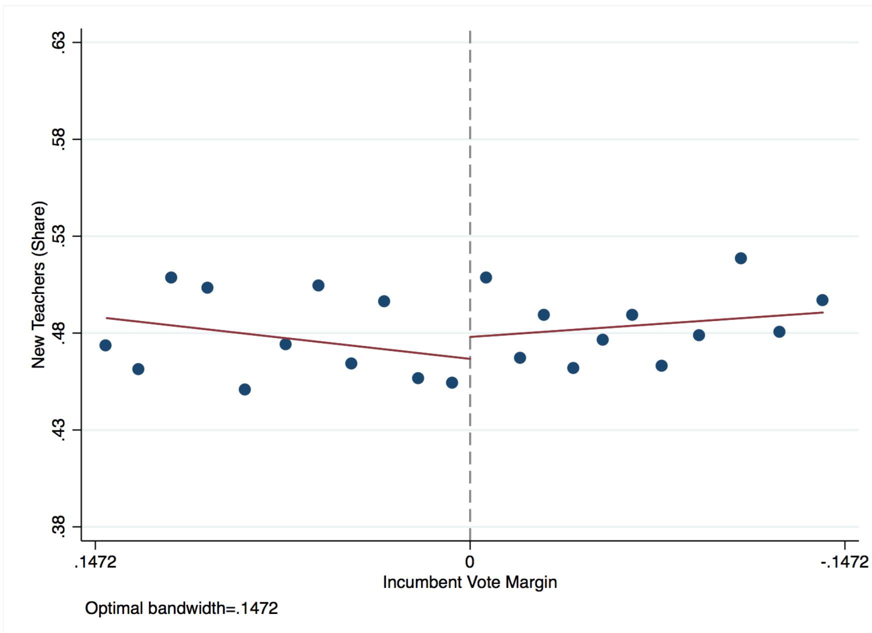
Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that have left a school is computed using the School Census and corresponds to the share of teachers in a school who were in that school at time $t-2$ (the year before the respective election) but are no longer in that same school at time t (one year after the respective election). Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

Figure A.10: *Political Turnover and 8th Grade Test Scores in Municipalities where the Winning Party was from the Left vs. the Right*



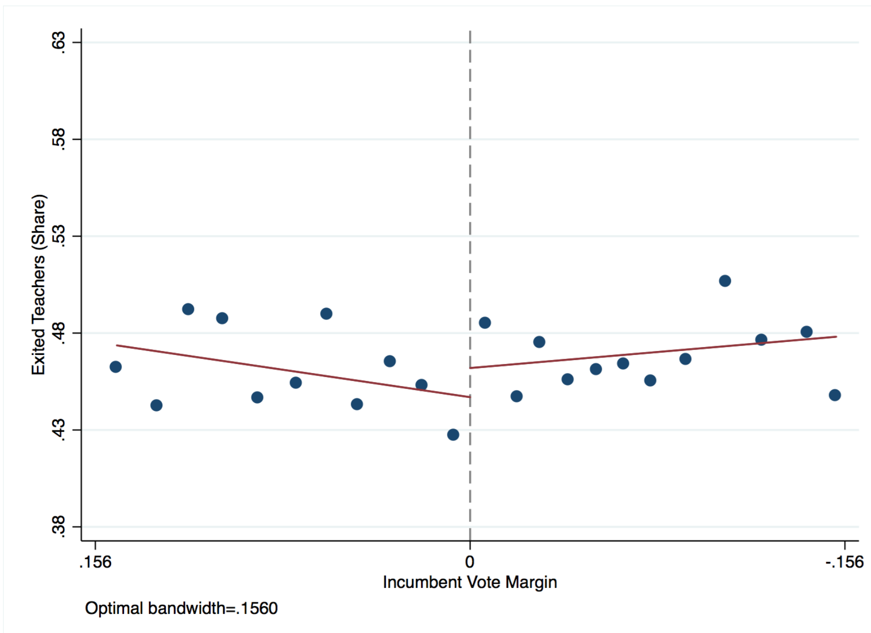
Notes: This figure shows the mean of individual-level 8th grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 8th grade test scores at baseline (the year before the respective election) is included as a control. Party ideology is classified as belonging to the left vs. the right according to Atlas Político – Mapa do Congresso.

Figure A.11: *Political Turnover and New Teachers in Non-municipal Schools*



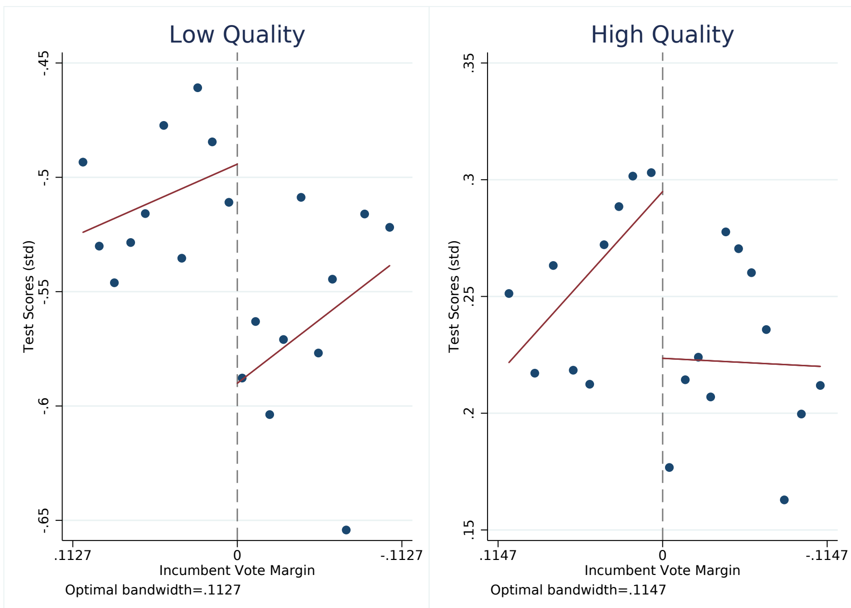
Notes: This figure shows the share of teachers that are new to non-municipal schools by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that are new to a school is computed using the School Census and corresponds to the share of teachers in a school who are in that school at time t (one year after the respective election) but were not in that same school at time $t-2$ (the year before the respective election). The set of non-municipal schools for this outcome is comprised of state, federal, and private schools.

Figure A.12: *Political Turnover and Teachers that have Left in Non-municipal Schools*



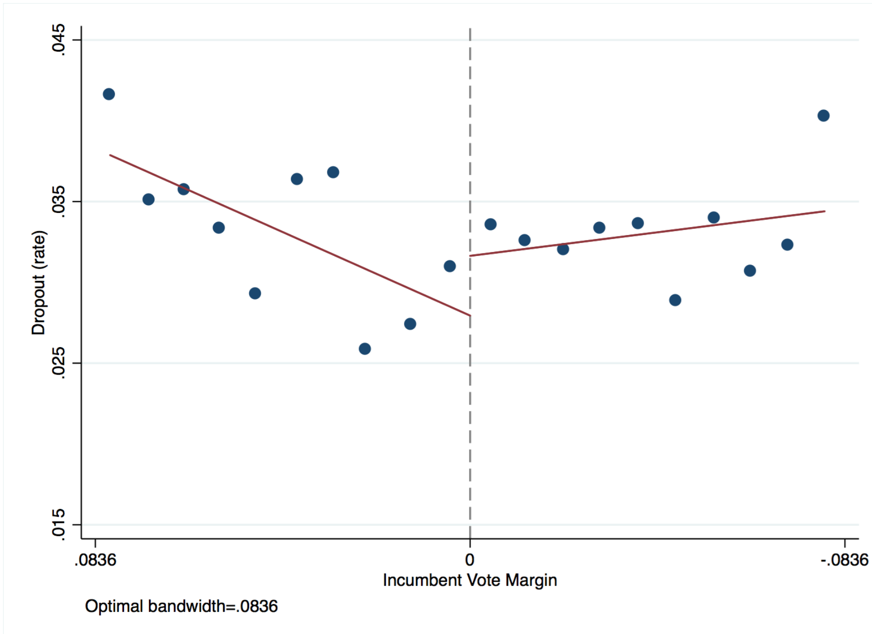
Notes: This figure shows the share of teachers that have left non-municipal schools by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The share of teachers that have left a school is computed using the School Census and corresponds to the share of teachers in a school who were in that school at time $t-2$ (the year before the respective election) but are no longer in that same school at time t (one year after the respective election). The set of non-municipal schools for this outcome is comprised of state, federal, and private schools.

Figure A.13: *Political Turnover and 4^{th} Grade Test Scores in Low- and High-quality Schools*



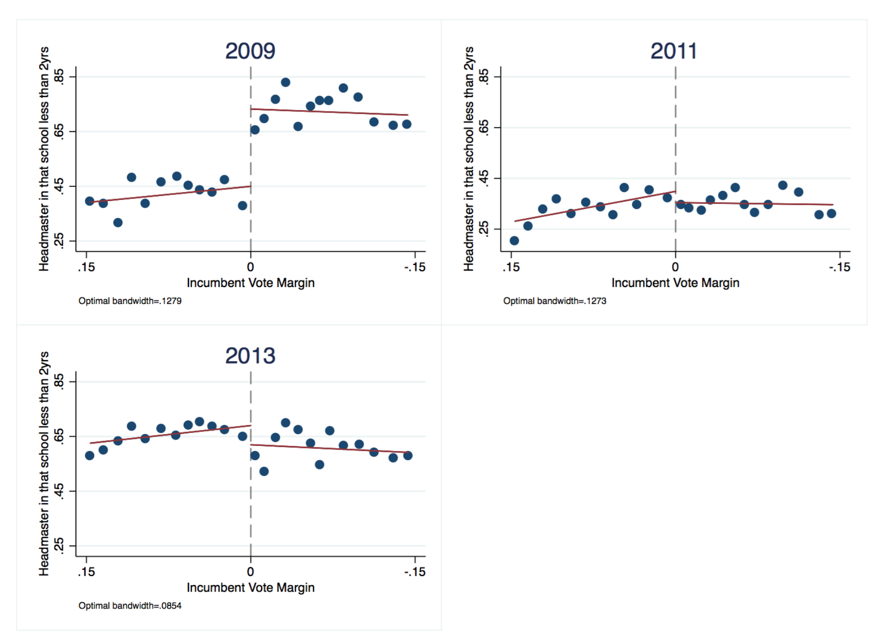
Notes: This figure shows the mean of individual-level 4^{th} grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for low- and high-quality municipal schools. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 4^{th} grade test scores at baseline (the year before the respective election) is included as a control. Low-quality schools are those below the median in the school-level distribution of test scores at baseline (the year before the respective election). High-quality schools are those above the median in this distribution.

Figure A.14: *Political Turnover and School-level Dropout Rates*



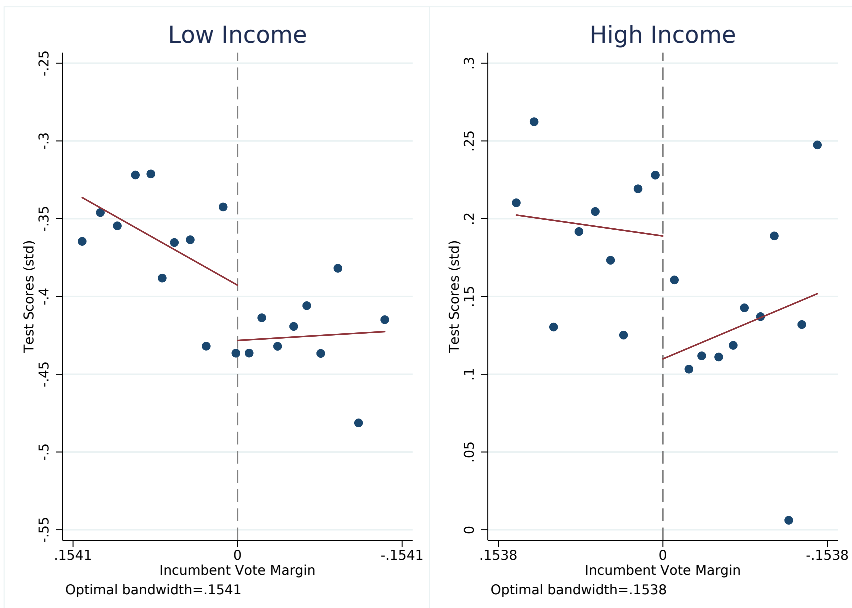
Notes: This figure shows the mean of school-level dropout rates by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. The school-level dropout rate is measured by the School Census and refers to the dropout rate for all students within a school (in all grade levels). The school-level dropout rate at baseline (the year before the respective election) is included as a control.

Figure A.15: *Political Turnover in 2008 and Headmaster Replacements 1, 3, and 5 Years After the Election*



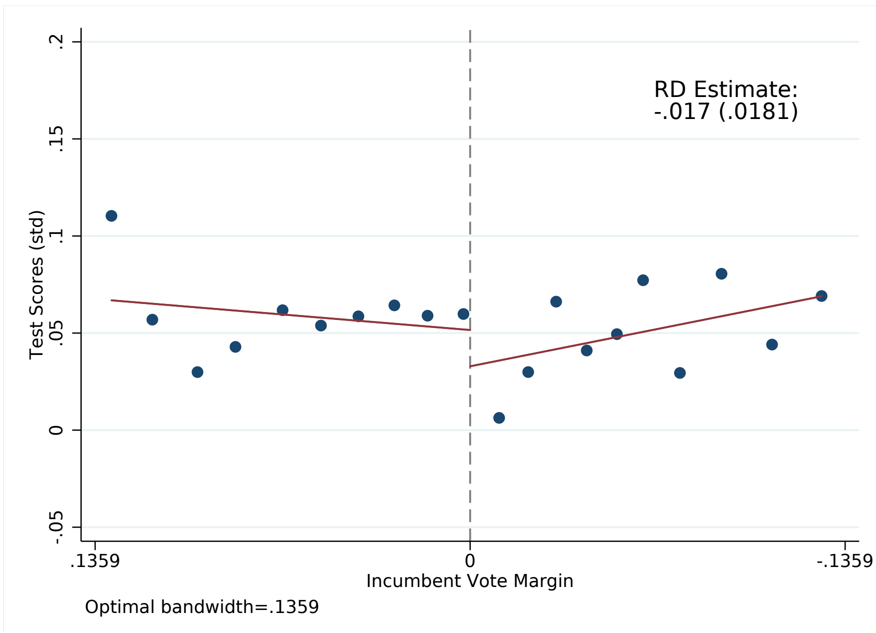
Notes: This figure shows the share of schools with a new headmaster by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for each year t, where t is one year, three years, and five years after the 2008 election. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor in 2008. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor in 2008. New headmasters are those that report being the headmaster of their current school for less than two years on the Prova Brasil headmaster questionnaire.

Figure A.16: *Political Turnover and 8^{th} Grade Test Scores in Low- and High-income Municipalities*



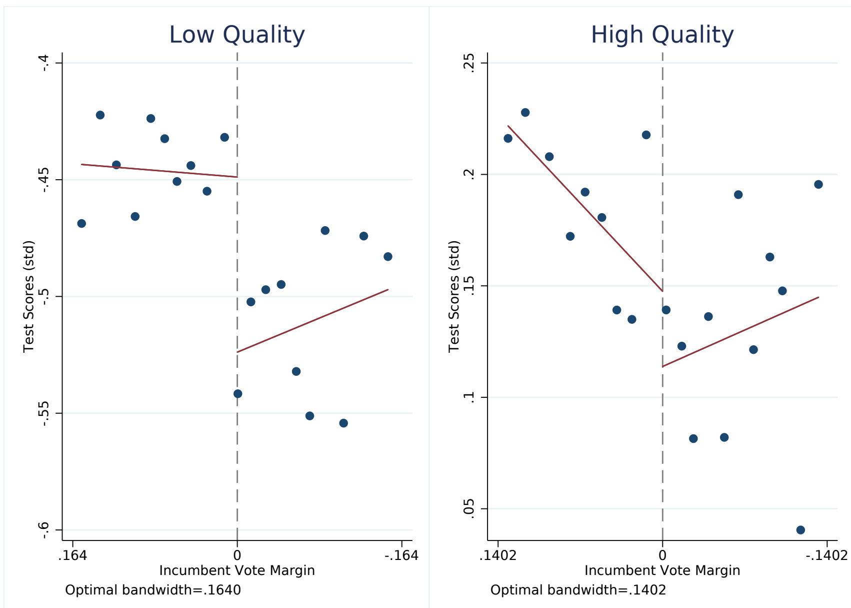
Notes: This figure shows the mean of individual-level 8^{th} grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 8^{th} grade test scores at baseline (the year before the respective election) is included as a control. Low-income municipalities are those below the median in the municipal-level distribution of median monthly household income as measured in the 2000 Census. High income municipalities are those above the median in this distribution.

Figure A.17: *Political Turnover and 8th Grade Test Scores in Non-municipal Schools*



Notes: This figure shows the mean of individual-level 8th grade test scores for students in non-municipal schools by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 8th grade test scores at baseline (the year before the respective election) is included as a control. The set of non-municipal schools for this outcome is comprised of state and federal schools, since only public schools participate in the Prova Brasil exam.

Figure A.18: *Political Turnover and 8^{th} Grade Test Scores in Low- and High-quality Schools*



Notes: This figure shows the mean of individual-level 8^{th} grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for low- and high-quality municipal schools. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change in the political party of the mayor. Test scores are from the Prova Brasil exam and are standardized based on the distribution of individual-level test scores in municipalities with no change in the ruling party. Average, school-level 8^{th} grade test scores at baseline (the year before the respective election) is included as a control. Low-quality schools are those below the median in the school-level distribution of test scores at baseline (the year before the respective election). High-quality schools are those above the median in this distribution.