Essays on Education and Political Economy

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Abstract

In this dissertation, I use the tools of applied microeconomics to study the provision of public services. The first chapter focuses on the provision of public education in Brazil. The second and third chapters study the provision of policing and education, respectively, in the United States.

In the first chapter, which is joint work with Diana Moreira and Laura Trucco, we study how a change in the political party of the mayor in Brazil affects the provision of public education. We find that students in municipalities with a new party in office have significantly lower test scores. We also find that school personnel (headmaster and teacher) turnover is higher in municipalities with a new party in power, but only in schools that are controlled by the municipal government. We then provide evidence that political turnover negatively impacts student outcomes through political discretion of the municipal government over the education bureaucracy.

In the second chapter, which is joint work with Frederik Schwerter, we study the relationship between fatal police use of force and trust in the police in the US. We find evidence that minority attitudes toward the police are negatively correlated with police use of force against minorities. White attitudes toward the police do not display such a relationship with respect to police use of force against whites (or minorities). We conclude that *only* minority attitudes toward the police respond to police use of force against their in-group; this response is targeted towards policing institutions and does not "spill over" to attitudes towards other formal or informal institutions.

In the final chapter, which is joint work with Natalie Bau, we study affirmative action policy in the US. We use a 2003 Supreme Court ruling that made affirmative action in college admissions constitutional to study the impact of affirmative action in higher education on educational achievement *prior* to college. Using SAT data and administrative school records, we find that the implementation of affirmative action narrowed the achievement gap between minority (black and Hispanic) and white high school students on standardized test scores, course grades, and the likelihood of taking advanced courses.

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To my grandfather, my Baba Ezzat

Introduction

In this dissertation, I empirically examine the provision of public services. In the first chapter, I study how the organizational structure of the government impacts the provision of public education in Brazil. In the other two chapters, I study how government actions and policies impact the provision of policing and education, respectively, in the United States.

In the first chapter, we study how political party turnover in mayoral elections in Brazil affects the provision of public education. Exploiting a regression discontinuity design for close elections and using administrative education data, we find that municipalities with a new party in office have test scores that are 0.05–0.08 standard deviations lower than comparable municipalities with no change in the political party. Party turnover leads to a sharp increase in the replacement rate of headmasters and teachers in schools controlled by the municipality. In contrast, we show that turnover in the political party of the mayor does not impact the replacement rate of school personnel or student test scores for local (non-municipal) schools that are not controlled by the municipal government. These findings suggest that political turnover in Brazilian municipalities negatively impacts student outcomes through political discretion over the municipal education bureaucracy. Political turnover can adversely affect the quality of public service provision in environments where the bureaucracy is not shielded from the political process.

In the second chapter, we study police use of force in the United States. A series of fatal police use of force cases have recently been in the national spotlight: Michael Brown in Missouri, Walter Scott in South Carolina, Freddie Gray in Baltimore, etc. Regardless of the legal justification surrounding each case, there has been a strong reaction from the public.

We study the relationship between fatal police use of force and trust in the police, in the government, and in other formal and informal institutions – and how this relationship varies by race. We use survey data to assess confidence in institutions and the FBI Supplementary Homicides data to measure police use of force. Using two different empirical strategies (a cross-sectional approach and an event-study analysis), we find evidence that trust in police by minorities is lower when and where minorities are more likely to be the target of fatal police encounters. On the other hand, there is no robust evidence that whites' attitudes toward the police are responsive to fatal police use of force against minorities or whites. There is no link between police use of force and trust in other formal institutions (such as trust in the local or national government, military, Supreme Court, banks, public schools, Congress, and the President) or non-institutional measures of trust (such as trust in neighbors, co-workers, local shop clerks, people from different racial/ethnic groups, and trust in general). Our results suggest that minority attitudes toward the police are correlated with police behavior; the same correlation does not hold for white attitudes.

In the final chapter, we study the incentive effects of affirmative action policy in the United States. Race-based affirmative action policies are widespread in higher education. Despite the prevalence of these policies, there is little evidence on whether affirmative action policies in higher education affect students *before* they reach college. We exploit the 2003 Supreme Court ruling in *Grutter v. Bollinger*, which overturned Texas, Louisiana, and Mississippi's affirmative action bans, to study the effect of race-based affirmative action on high school students' outcomes. We first use a panel of race-by-state-by-year SAT scores to show that minorities' math SAT scores improved relative to whites in the affected states following the ruling.To understand the drivers of this result, we then examine the evolution of the racial achievement gap using administrative data from a large, urban Texan school district. We find that the reinstatement of affirmative action narrowed the achievement gap between minority (black and Hispanic) and white high school students on standardized test scores, course grades, and the likelihood of taking advanced courses. Survey data further suggest that students' behavior and aspirations responded to the policy reversal.

Chapter 1

Political Turnover, Bureaucratic Turnover, and the Quality of Public Services[1]

1.1 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

[1]Co-authored with Diana Moreira and Laura Trucco

bureaucracy, respectively, once they enter office.[2]

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

[2]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.[3]

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

[3]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 *et al.*, 2013), it is likely that politically caused changes in the assignment of school personnel disrupt school operations and management and, hence, lower test scores.[4] 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

[4]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.[5] 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 *et al.*, 2011; Colonnelli *et al.*, 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).[6] 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 bureaucracy is not shielded from the

[5]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, 2011).

[6]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, 2011). Presumably, this requires the cooperation of bureaucrats and the administration.

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 1.2 describes the relevant institutional details of Brazilian municipal governments, the education system, and the link between the political process and the education system. Section 1.3 describes the data sources used and the steps we take to select our sample. Section 1.4 outlines the empirical strategy, discusses the identification assumption, and provides evidence in support of the identification assumption. Section 1.5 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 1.6 sheds light on the mechanisms by which political turnover translates to worse outcomes for students. Section 1.7 concludes.

1.2 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.

1.2.1 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.[7] Mayors are elected in municipal elections that are held every four years on the same day across the country.[8]

[7]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).

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

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.[9]

1.2.2 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.[10] For municipal schools, the municipal government 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

[9]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].

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

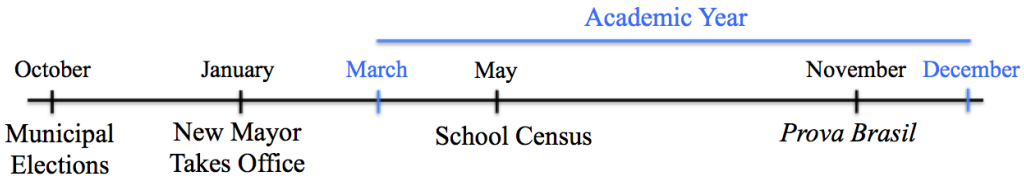
that ensure the daily operations of schools, come from a federal fund called FUNDEF, a non-discretionary fund that pays a fixed rate per enrolled student. Thus, the funding of the daily operations of schools is unlikely to be affected by political cycles or political alliances.[11]

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 *et al.*, 2015). And lastly, the headmaster position may be used to reward political supporters.[12]

[11]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 1.6.3.

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



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.

Figure 1.1: *Timeline of Election and Data Collection*

1.3 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.

1.3.1 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 1.1, municipal elections are held in October (every four years) and the mayor takes office in January of the following year.[13] 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.

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.

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

1.3.2 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.

1.3.3 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.[14] 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 4^{th} and 8^{th} 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.

1.3.4 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-

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

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.

1.3.5 Sample Selection and Summary Statistics

Table 1.1: *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.

We take a number of steps to select municipalities into our sample. We start with 5,553

municipalities.[15] 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.[16]

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.

Table 1.1 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 Table 1.1 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 4th or 8th 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

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

[16] 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.

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).

1.4 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.

1.4.1 Identification Strategy

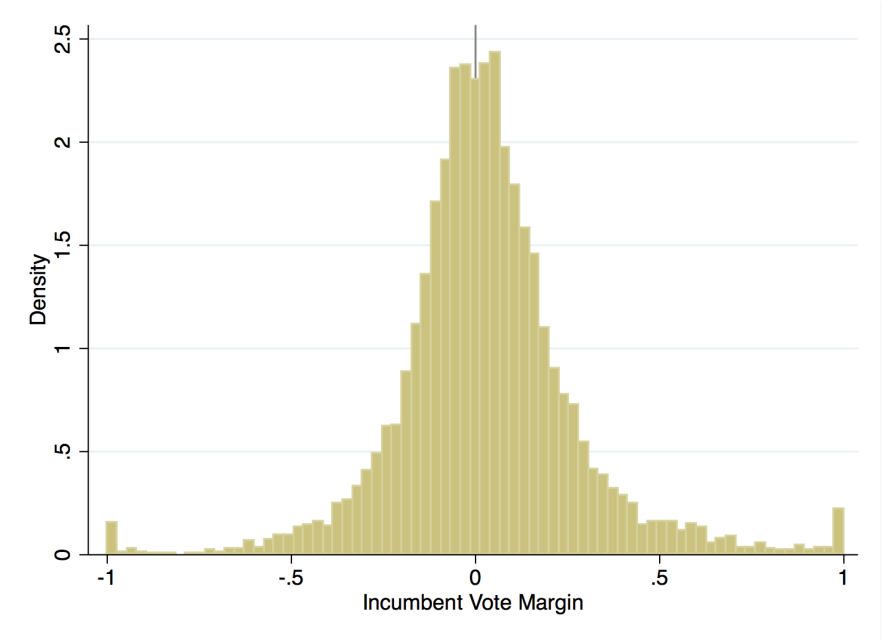
To identify the effect of a change in the political party, we compare outcomes in municipalities where the incumbent party barely loses (thus there is political party turnover) to outcomes in municipalities where the incumbent political party barely wins (and there is no political party turnover). That 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 *et al.* (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:

$$\begin{aligned} Y_{jmt+1} =& \alpha + \beta \mathbb{1}\{IncumbVoteMargin_{mt} < 0\} + \gamma IncumbVoteMargin_{mt} + \\ & \delta \mathbb{1}\{IncumbVoteMargin_{mt} < 0\} \times IncumbVoteMargin_{mt} + X'_{jmt}\Lambda + \epsilon_{jmt}, \end{aligned} \tag{1.1}$$

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.[17] Standard errors are clustered at the municipality level.



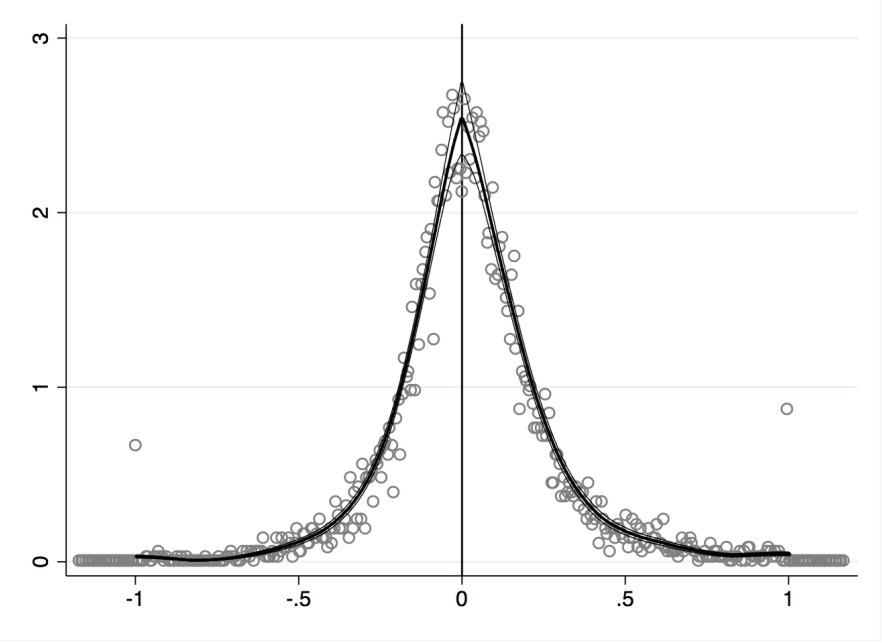
Notes: This histogram shows the distribution of the running variable in the RDD, IncumbVoteMargin, in our sample of municipalities in the 2008 and 2012 election cycle. IncumbVoteMargin is computed as the vote share of the incumbent political party minus the vote share of the incumbent party's strongest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

Figure 1.2: *Distribution of Incumbent Vote Margin*

[17] 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.

1.4.2 Identification Assumption

For Equation (1.1) 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 discontinuity in covariates at the zero threshold.



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.

Figure 1.3: *McCrary Test for Manipulation of Incumbent Vote Margin*

Figure 1.2 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 1.3 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.[18]

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 Table 1.2 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.[19] This bandwidth corresponds to 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.[20] 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 (1.1) 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

[18]Further confirming our finding of no manipulation in the running variable is a study done by Eggers *et al.* (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 2nd half of the 20th century).

[19]Approximately 40% of the municipalities in our sample fall within this bandwidth. Local elections in Brazil are quite competitive.

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

Table 1.2: *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 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 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 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 the main specification, Equation 1.1, with the corresponding variable at baseline as the dependent variable.

the relevant threshold for baseline characteristics lends credibility to our identification assumption that political party turnover is "as good as randomly assigned."[21]

1.5 Results

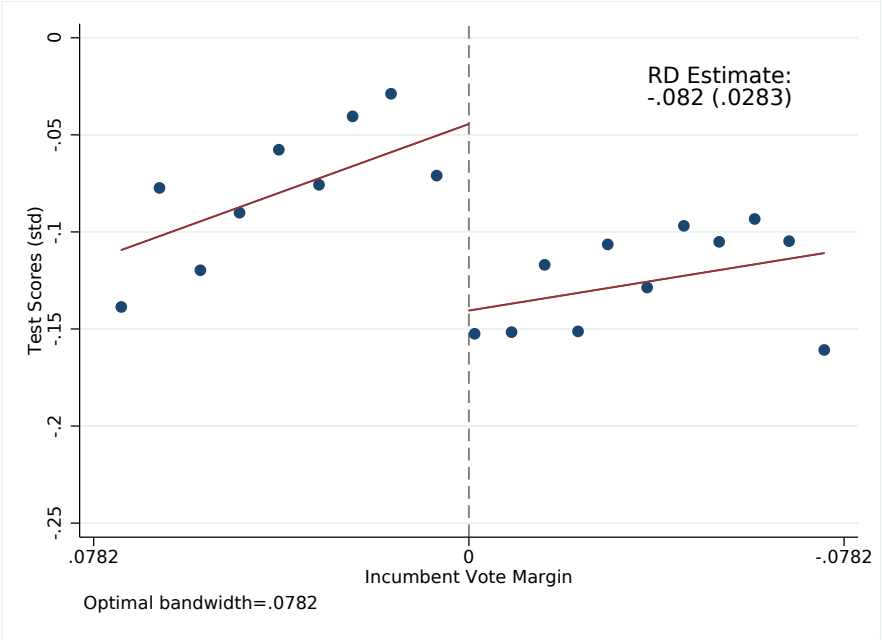
Our main results, which we present below, show that political party turnover reduces students' test scores. The negative effect of political political party turnover on student achievement is not driven by selection or shifts in party ideology and persists up to three years after the election, at which 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 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.

[21] 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 Table 1.2 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 Table 1.2 shows). As Appendix Table A.1 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.

1.5.1 Political Turnover and Student Achievement

We estimate Equation (1.1) separately for 4th and 8th graders because all municipalities offer elementary schools but not all municipalities offer middle schools (usually the larger municipalities offer both elementary and middle schools).



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). 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.

Figure 1.4: *Political Turnover and 4th Grade Test Scores*

Effect on 4th Graders. Figure 1.4 shows 4th grade test scores one year after the election (in 2008 or 2012) in municipalities with close elections.[22] Test scores for 4th 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

[22]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 4th graders in municipal schools is less than zero.

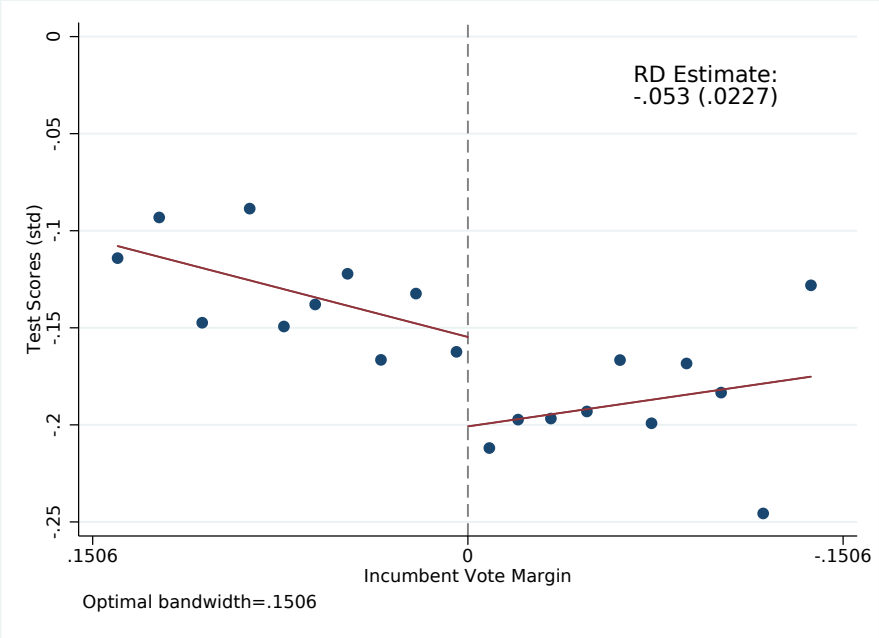
(left hand side of the figure). As Table 1.3 shows, municipalities with a new party in office have test scores that are 0.08 standard deviations lower than comparable municipalities with no change in the political party. The estimated effect of political party turnover is robust to the inclusion of individual-level demographic controls, school-level controls, a dummy for the 2012 election cycle, and varying the estimation bandwidth.

Table 1.3: *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.

Effect on 8^{th} Graders. The same pattern holds for 8^{th} grade test scores one year after the election, as shown in Figure 1.5. Eighth graders' test scores are lower in municipalities where a new political party has barely won compared to municipalities where the incumbent political party has barely stayed in office. Table 1.4 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



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). 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.

Figure 1.5: *Political Turnover and 8th Grade Test Scores*

robust to the inclusion of controls and varying the estimation bandwidth. One potential issue with test scores for 8th 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 Table 1.4), bandwidths that are closer to the optimal bandwidth for 4th grade test scores, we still find a negative effect of political party turnover on 8th 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,

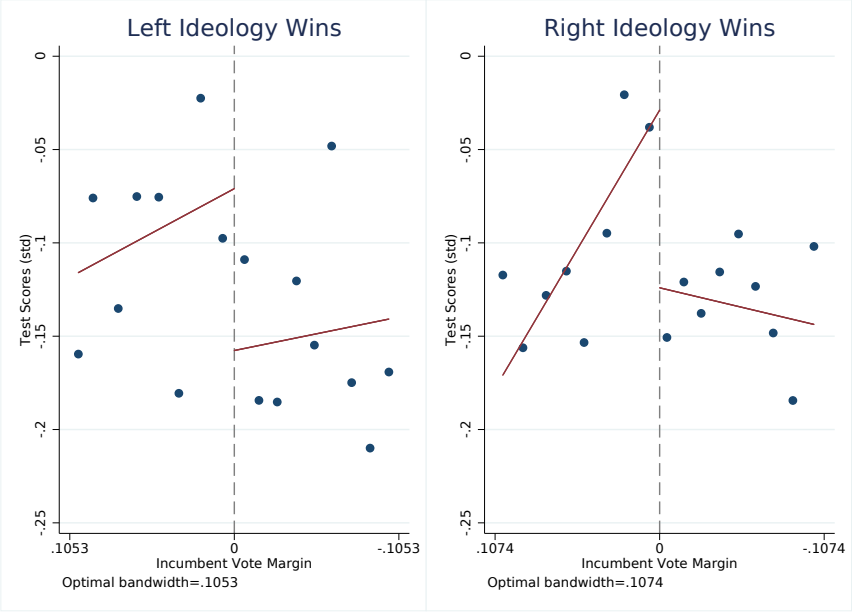
Table 1.4: *Political Turnover and 8th Grade Test Scores*

Outcome:	Individual 8th 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 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. 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 8th 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.

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. Table 1.5 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.1 and Appendix Table A.2 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).



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 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 4^{th} 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 1.6: *Political Turnover and 4^{th} Grade Test Scores in Municipalities where the Winning Party was from the Left vs. the Right*

Heterogeneity with Respect to Party Ideology. Figures 1.6 shows the effect of party turnover on 4^{th} grade test scores separately for municipalities where a left-leaning political

Table 1.5: *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.

party (barely) wins and those where a right-leaning political party (barely) wins.[23] 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).[24]

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 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). Table 1.6 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.[25] Yet, as time passes, there is still a lingering negative effect of political party turnover on test scores. Although the estimated effect is 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.[26]

[23] Appendix Figure A.3 shows the same analysis for 8th graders and the results are similar.

[24] 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.

[25] Table 1.3 and Table 1.6 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.

[26] Appendix Table A.3 shows the same results for 8th 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 8th grade test scores is negative and persistent; however, the standard errors are large and the estimates are noisy.

Table 1.6: *Political Turnover in 2008 and 4th Grade Test Scores 1, 3, and 5 Years After the Election*

Outcome:	Individual 4th 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 4th 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 4th 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.

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 *et al.*, 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

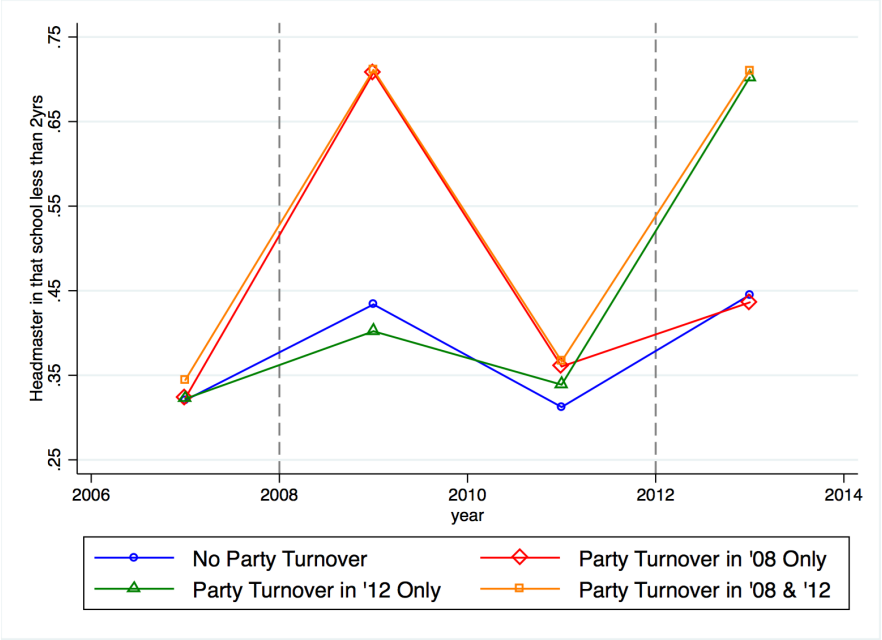
education policy in Brazil, *Bolsa Familia*, on test scores.[27] Hence, we look to another (similar) setting to benchmark our results. Angrist *et al.* (2002) finds that providing vouchers for private schools increases test scores by .2 standard deviation units at a total cost of $195 per student. If 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 4th grade and then again in 8th 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.[28] 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.

1.5.2 Political Turnover and School Personnel

Headmaster Replacements. Figure 1.7 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

[27]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 *et al.*, 2000).

[28]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: Table 1.3, Column 1 and Table 1.4, 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 $22 million by making the following calculation: (.08/.2)$\times$195$\times$324,885=25,341,030.



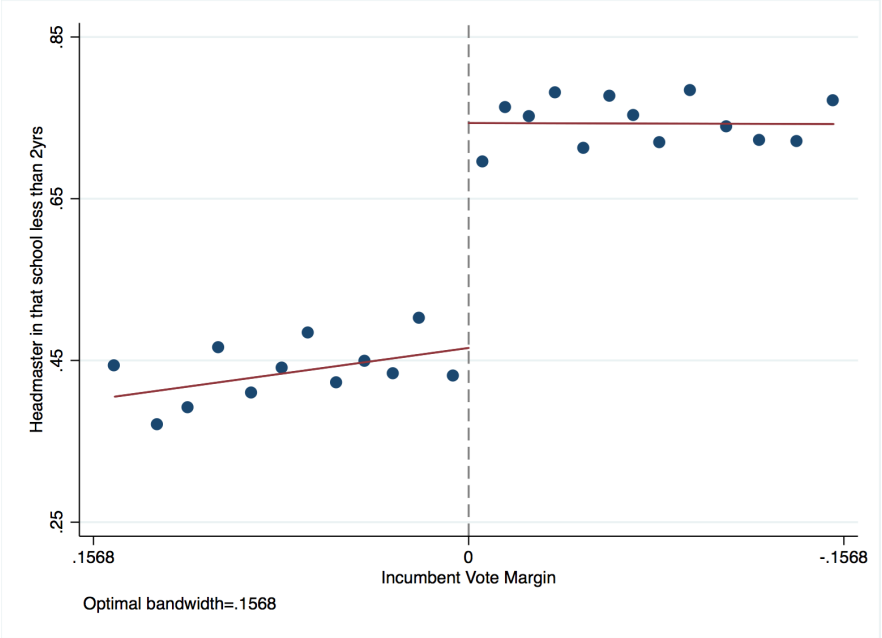
Notes: This figure shows the share of schools with a new headmaster in municipalities that: did not experience party turnover in either election cycle, experienced party turnover only in 2008, experienced party turnover only in 2012, or experienced party turnover in both election cycles. New headmasters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

Figure 1.7: *Political Turnover and Headmaster Replacements (Event Study)*

political party turnover on headmaster replacements for municipalities with close elections. Figure 1.8 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. Table 1.7 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).[29]

Headmaster Characteristics. Using the *Prova Brasil* headmaster questionnaire, we explore how political party turnover affects the characteristics of headmasters in treated and

[29]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.4 and Appendix Table A.4 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.



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.

Figure 1.8: *Political Turnover and Headmaster Replacements*

control municipalities. Table 1.8 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 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).[30] The headmaster questionnaire asks the headmasters "How did you get to the

[30]There is heterogeneity within municipalities in terms of the mechanism by which the headmaster is chosen.

Table 1.7: *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.

headmaster position in this school?" Based on this question, we categorize the method by which the headmaster was chosen as: selection, election, or political appointment.[31] 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

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.

[31]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.

Table 1.8: *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.

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 (1.1). Table 1.9 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.[32]

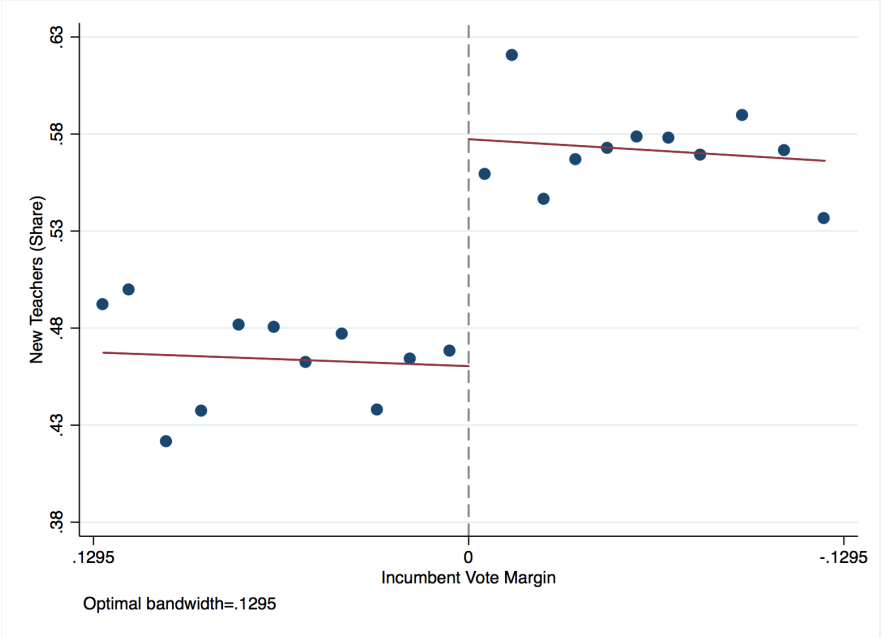
Table 1.9: *Political Turnover and Politically Appointed Headmasters*

	(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.

Teacher Replacements. Figure 1.9 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 1.10 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 Table 1.10. 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

[32] 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.



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).

Figure 1.9: *Political Turnover and New Teachers*

"reshuffling" of teachers across schools.[33] In fact, the number of teachers per school is not different in municipalities with and without political turnover (Table 1.11, Column 1).

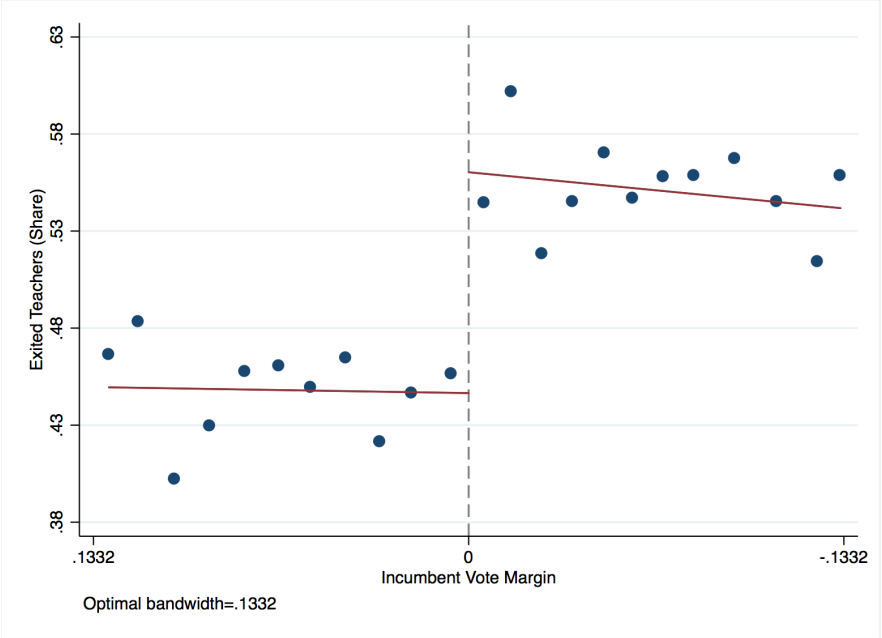
Unfortunately, we cannot repeat the event-study analysis that we did for headmasters with teachers because the School Census did not track teachers in 2005, hence, we cannot compute the share of teachers that are new to a school/have left a school in 2007. Instead,

[33]Baseline teacher turnover is very high: as Table 1.10 shows, the average share of teachers that 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.

Table 1.10: *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.



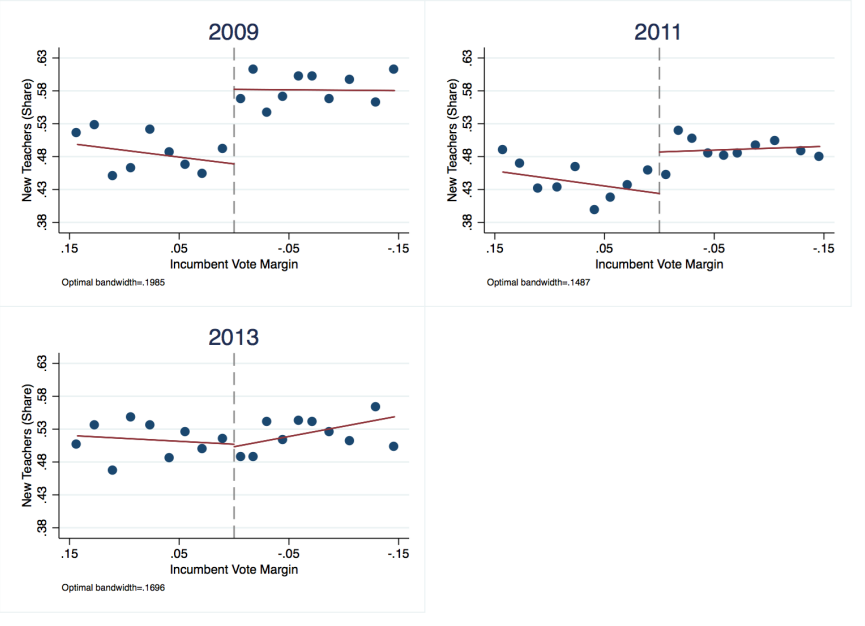
Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience 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).

Figure 1.10: *Political Turnover and Teachers that have Left*

Figure 1.11 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.[34] The corresponding table, Appendix Table A.5, 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

[34]This figure shows teacher turnover in terms of the share of teachers that are new to a school and Appendix Figure A.2 shows teacher turnover in terms of the share of teachers that have left a school. Both figures show similar patterns.

turnover in 2008 on teacher replacements.



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 1.11: *Political Turnover in 2008 and New Teachers 1, 3, and 5 Years After the Election*

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. Table 1.11 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.[35]

[35]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.

Table 1.11: *Political Turnover and Teacher Characteristics*

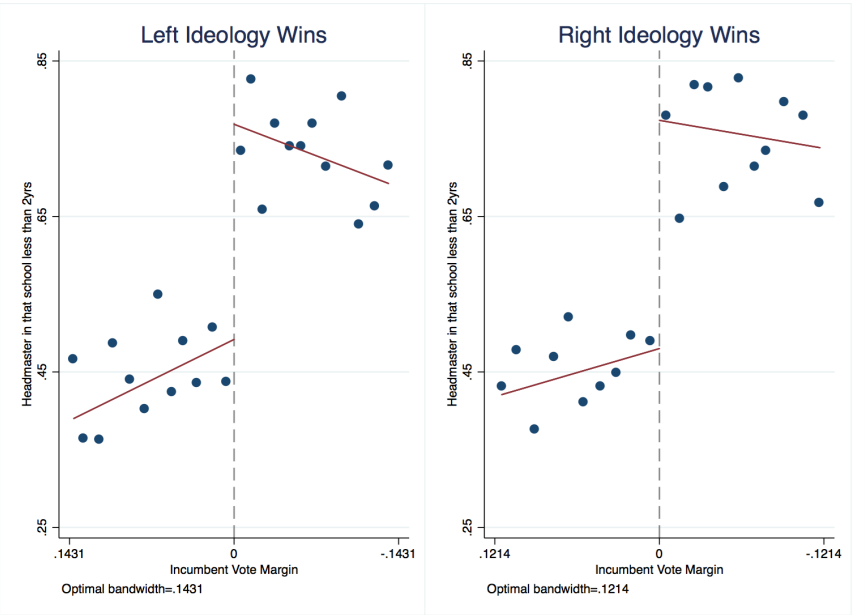
Outcome:	N Teachers (1)	Age (2)	Female (3)	B.A. (4)	Graduate Training (5)	Temporary Contract (6)	Contract Type Missing (7)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.196	-0.400	-0.015	-0.073***	-0.023*	0.042	0.010*
	(0.279)	(0.364)	(0.014)	(0.023)	(0.014)	(0.034)	(0.006)
N	39,642	39,642	39,642	39,642	39,642	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.

Heterogeneity with Respect to Party Ideology. Figure 1.12 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.5 and A.6 and show similar results. Thus, the effect of political party turnover on the replacement rate of school 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.[36] 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

[36]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.

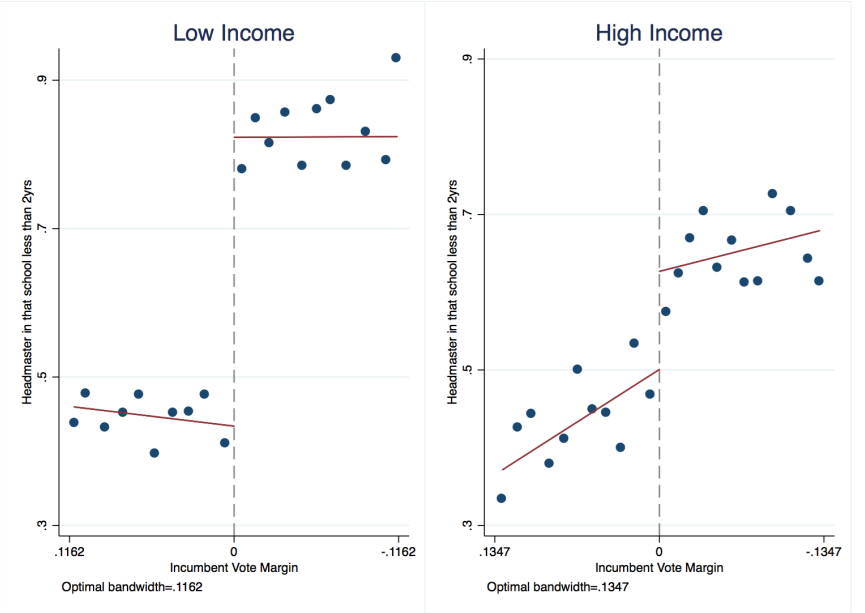


Notes: This figure shows the share of schools with a new headmaster by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<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.

Figure 1.12: *Political Turnover and Headmaster Replacements in Municipalities where the Winning Party was from the Left vs. the Right*

of municipalities with below median income and the subset of municipalities with above median income and estimate the effect of political turnover on replacement of school personnel separately for low- and high-income municipalities.[37] Figure 1.13 (Table 1.12) 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.7 and A.8 and Appendix Table A.6). The heterogeneity in the effect of political party turnover

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



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.

Figure 1.13: *Political Turnover and Headmaster Replacements in Low- and High-income Municipalities*

on assignment of school personnel suggests that political discretion over school personnel is higher in low-income municipalities.[38]

Table 1.12: *Political Turnover and Headmaster Replacements in Low- and High-income Municipalities*

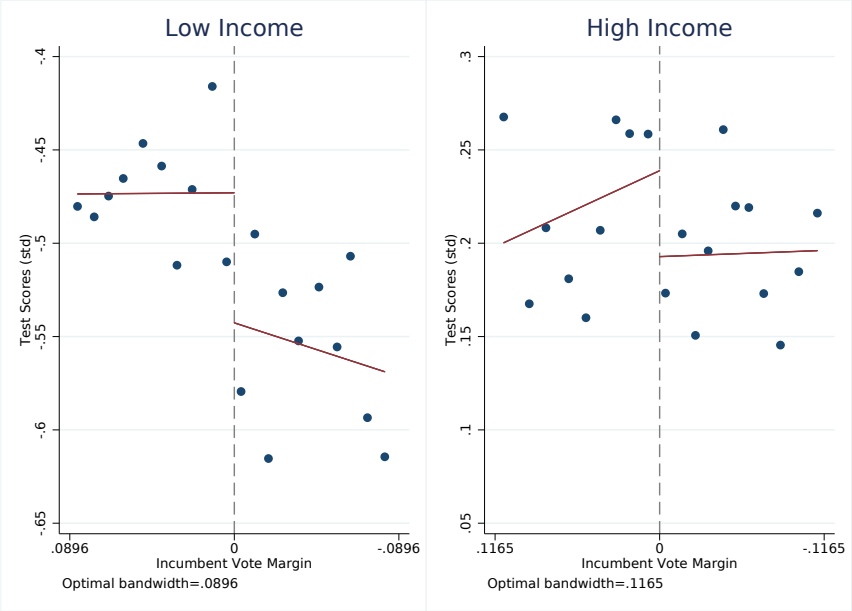
Outcome:	Headmaster is new to the school (as Headmaster)					
Panel A	Low Income Municipalities (Below Median Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{IncumbVoteMargin < 0\}$	0.389***	0.389***	0.371***	0.371***	0.379***	0.378***
	(0.038)	(0.037)	(0.047)	(0.045)	(0.039)	(0.038)
N	6,703	6,703	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 1.7 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 1.13: *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 1.3 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.



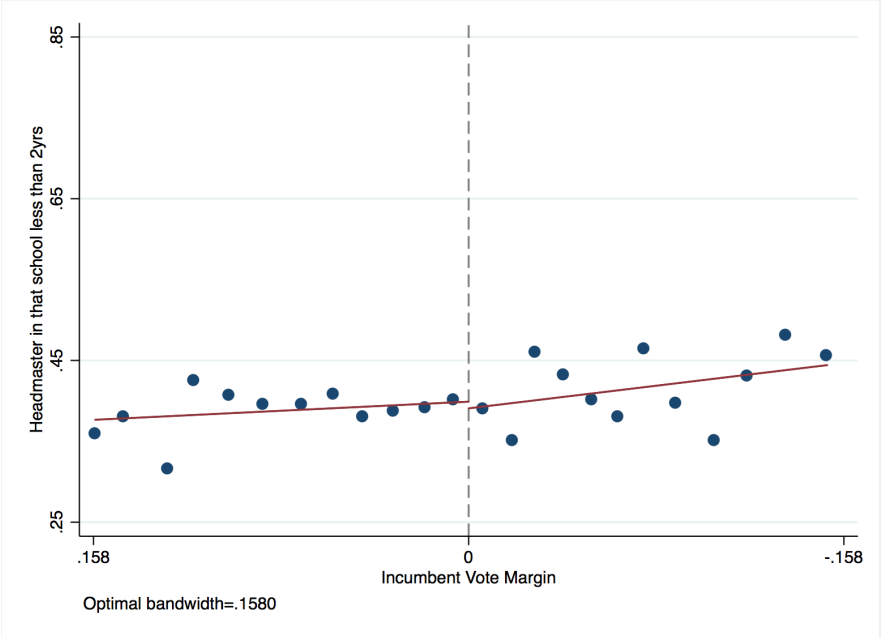
Notes: This figure shows the mean of individual-level 4^{th} grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the 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.

Figure 1.14: *Political Turnover and 4^{th} Grade Test Scores in Low- and High-income Municipalities*

1.5.3 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

[38]Despite this heterogeneity in the effect of political party turnover on school personnel replacements with respect to income, Figure 1.14 and Table 1.13 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.9 and Appendix Table A.7). 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.



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.

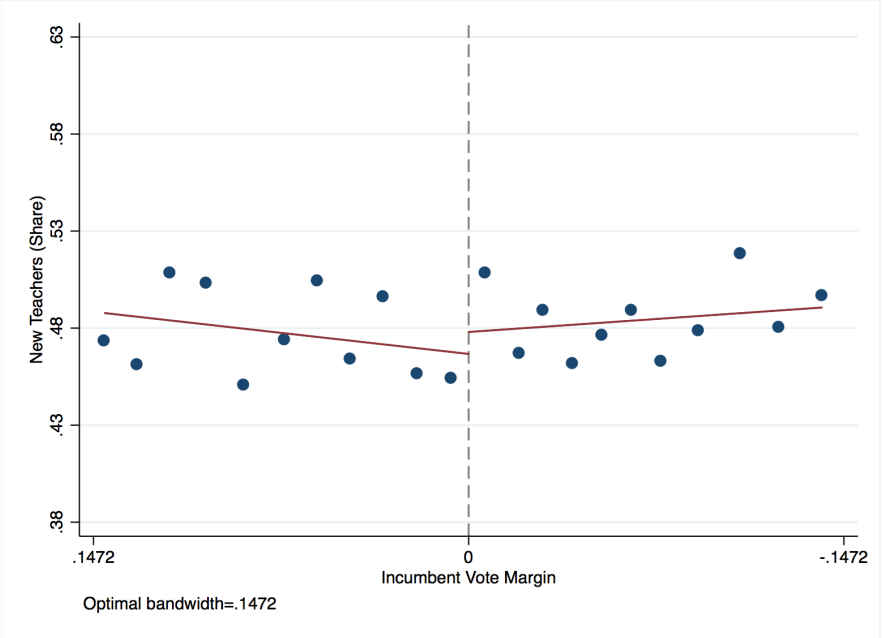
Figure 1.15: *Political Turnover and Headmaster Replacements in Non-municipal Schools*

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.[39]

Municipal governments control 68% of primary schools. The remainder of *public* primary schools are controlled by the state.[40] 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

[39]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.

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



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 1.16: *Political Turnover and New Teachers in Non-municipal Schools*

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 1.15 and Table 1.14 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. Figure 1.16 and Table 1.15 shows

Table 1.14: *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 1.7 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.

the same results for the share of teachers that are new to non-municipal schools.[41] 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 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.[42] 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. Figure 1.17 and Table 1.16 show

[41] Appendix Figure A.10 show the results graphically for the share of teachers that have left non-municipals school.

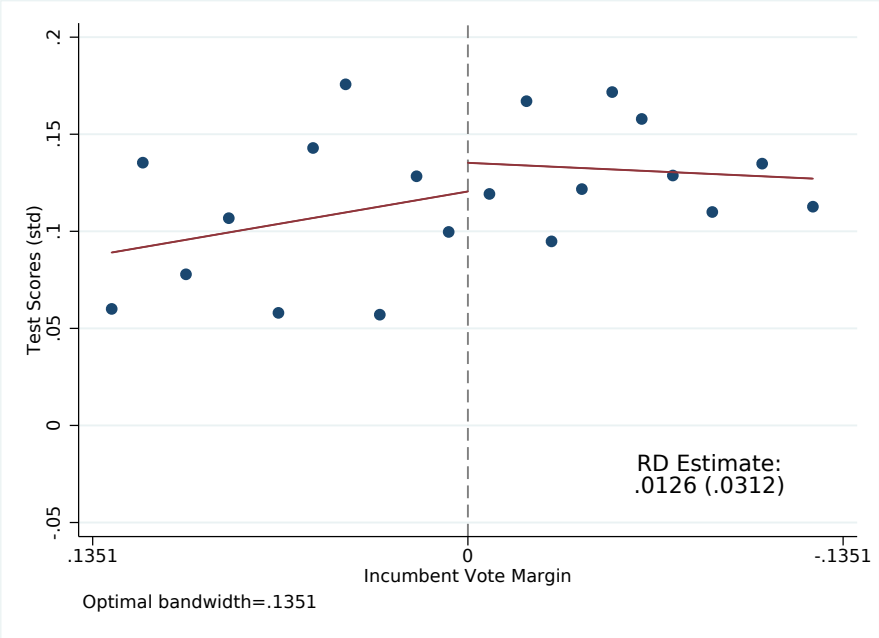
[42] In fact, 22% of teachers in non-municipal schools also teach in municipal schools. In Brazil, teachers may teach in more than 1 school since the school-day is only half of a day. In our sample, teachers teach in 1.3 schools on average.

Table 1.15: *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 1.10 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

the effect of political party turnover in mayoral elections on 4th grade test scores in non-municipal schools.[43] 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.



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.

Figure 1.17: *Political Turnover and 4th Grade Test Scores in Non-municipal Schools*

One important issue is that municipal schools are worse quality schools than non-

[43]We show the corresponding analysis for middle schools (i.e. 8th graders) in Appendix Figure A.11 and Appendix Table A.8. 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.

Table 1.16: *Political Turnover and 4^{th} Grade Test Scores in Non-municipal Schools*

Outcome:	Individual 4^{th} 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 1.3 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.

municipal schools: in 2007, for example, the average test score in municipal schools was .085 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). Figure 1.18 and Table 1.17 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.[44]

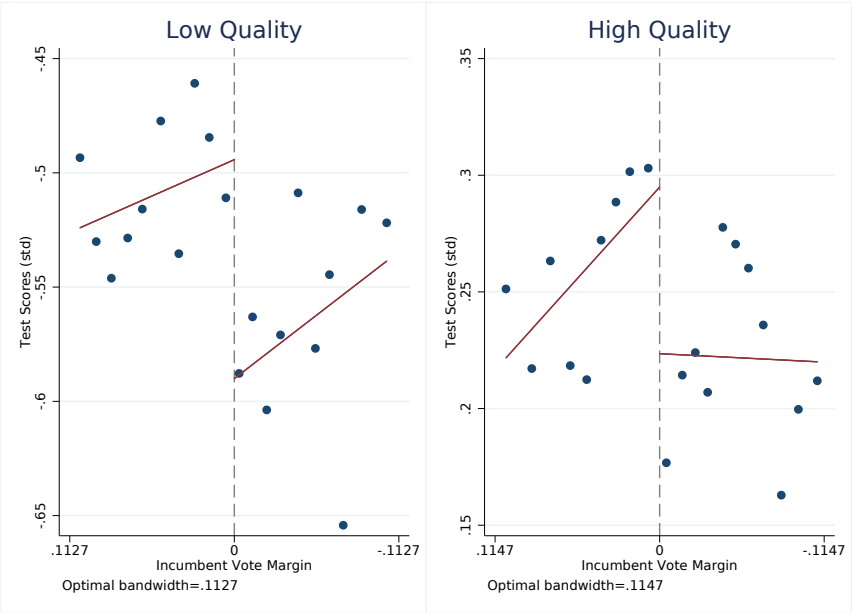
Table 1.17: *Political Turnover and 4th Grade Test Scores in Low- and High-quality Municipal Schools*

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 1.3 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.

What the Placebo Shows. Political party turnover in mayoral elections does not trans-

[44]We show the result of the heterogeneity analysis with respect to baseline test scores for 8th graders in municipal schools in Appendix Figure A.12 and Appendix Table A.9. 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.



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 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 4th 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 1.18: *Political Turnover and 4th Grade Test Scores in Low- and High-quality Schools*

late 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 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.

1.6 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.

1.6.1 School Personnel Quality

As discussed in Section 1.5.2 (Tables 1.8 and 1.11) 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.[45] 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.

1.6.2 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. Table 1.18 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. Table 1.19 reports the same results for questions regarding the operation of the school that were asked of teachers.[46] The results are similar. Moreover, teachers

[45]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.

[46]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.

Table 1.18: *Political Turnover and School Problems (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 1.19: *Political Turnover and School Problems (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.

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.

1.6.3 Education Resources

Education funding in Brazil is mostly non-discretionary and comes from a federal program (FUNDEF) that pays a fixed rate per student.[47] 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

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

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. Table 1.20, 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).[48] This suggests that changes in mayoral parties do not impact 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. Table 1.20, 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.

1.7 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

[48]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.

Table 1.20: *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.

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 2

Police Use of Force and Trust[1]

2.1 Introduction

Recently in the United States, a series of fatal police use of force cases have been in the national spotlight. In July 2014, amateur video footage captured New York City Police Department officers putting Eric Garner in a "chokehold" while arresting him; Garner was later pronounced dead at the hospital. In August 2014, unarmed teen Michael Brown was fatally shot by police in Ferguson, Missouri. In April 2015, Walter Scott was shot by police as he was fleeing the scene of a daytime traffic stop in North Charleston, South Carolina. In July 2016, Alton Sterling was shot several times at point-blank by officers who had him pinned to the ground and immobile in Baton Rouge, Louisiana. The next day, Philando Castile was shot and killed in his car by a police officer in Falcon Heights, Minnesota while his girlfriend and her four-year-old daughter were also in the car. Such cases spurred investigations by local and federal authorities that examined the legal justification of each individual case but also broader patterns of excessive use of force by police.

Regardless of the legal justification surrounding each case, there have been strong reactions from the public. Both peaceful protests and civil disorder followed the aforementioned incidents. A national movement, Black Lives Matter, has risen to prominence in part due to

[1]Co-authored with Frederik Schwerter

the public outrage surrounding policing issues.[2] In the most extreme reaction, two police officers were shot and killed in New York City by a man who called his attack "retribution for the deaths of Mr. Garner and Mr. Brown."[3,4] Public attitude toward the police was highlighted in the report released by the Department of Justice after its investigation of the Ferguson Police Department: "Since the August 2014 shooting death of Michael Brown, the lack of trust between the Ferguson Police Department and [residents] has become undeniable" (DOJ, 2015). Although the report concedes that the causes of this distrust towards the police are not completely clear and are the subject of debate, it proposes one potential explanation for the deep divisions between the public and the police in Ferguson: unnecessarily aggressive policing practices. This is precisely the issue we examine in this paper.

We study the link between police use of force and public attitudes toward the police. Understanding whether (and how) police use of force and attitudes toward the police are related is important for several reasons. First, the police can and often do compel obedience through the threat or actual use of force (Tyler, 2003). However, if a collateral consequence of police use of force is that it undermines police-community relations, then such a consequence must be taken into account when considering different policing tactics. Second, there is the perception that there are racial disparities in police use of force.[5] Almost all of the recent, high-profile cases of fatal police use of force, including all of the incidents mentioned in the first paragraph, involve white police officers killing black

[2]https://www.theguardian.com/world/2015/jul/19/blacklivesmatter-birth-civil-rights-movement, accessed March 2017.

[3]https://www.nytimes.com/2014/12/21/nyregion/two-police-officers-shot-in-their-patrol-car-in-brooklyn.html, accessed March 2017.

[4]It should be noted that the the man responsible for killing these two police officers had an extensive history with the police, had been arrested multiple times, and had suffered from mental health problems. So his actions cannot be generalized to the response from the broader public: https://nyti.ms/2nrvmQ0, accessed March 2017.

[5]Putting aside public perception, whether there are actual racial differences in police use of force is studied in Fryer (2016a).

civilians.[6] Therefore, understanding whether police use of force erodes trust in the police and whether the response is more pronounced for minority groups will shed some light on the fraught relationship between law enforcement agencies and minorities.

Whether (and how) police use of force and public perception of the police are linked is not evident. First, prior research, for example Skogan (2016), has shown that civilians who *directly* experience aggressive policing techniques are less trusting of police. However, the majority of Americans do not directly interact with the police and, furthermore, very few people experience the threat or use of force during police interactions. According to the Police-Public Contact Survey, which is a nationally representative survey offering detailed information about contacts with the police, 16% of Americans had face-to-face contacts with the police in 2008. Of those who had contacts with the police only 1.3% reported that the police threatened or used force during the interaction. For the majority of Americans, their attitudes towards the police cannot be shaped by their one-on-one interactions with police officers. Therefore, whether people use the experiences of others in society with the police when shaping their attitudes of police is an open question. Even conditional on the existence of a link between indirect exposures to police use of force and individuals' attitudes toward the police, it is not clear which way this relationship would go (i.e. positive or negative) and it may depend on how incidents of police violence are interpreted. If the use of force by police is deemed justifiable to the circumstances of a particular incident, then the public may update their beliefs positively about the police since they are keeping communities safe. On the other hand, if the use of force in a particular incident is deemed as excessive or unnecessary, the public response may be very different.

To study the relationship between police use of force and public perception of police, we combine data on instances of fatal police use of force and data on public opinion regarding the police – as well as other formal and informal institutions. We focus on *fatal* police use of (as opposed to use of force more generally) because fatal instances are the most

[6]The fatal shooting of Dylan Noble in June 2016 in Fresno, California is an example of a high-profile incident of fatal police use of force against a white civilian [http://www.latimes.com/local/lanow/la-me-ln-fresno-police-shooting-video-dylan-noble-autopsy-cocaine-20160802-snap-story.html, accessed March 2017].

extreme incidents of police use of force, are more likely to be covered by local and national media outlets, and are, therefore, the most likely to generate a response from the public. Additionally, even though nation-wide data on fatal police use of force is not complete, it is far better than national data on non-lethal police use of force.

We combine two empirical methods: an event-study approach using the Ferguson incident and a cross-sectional approach using variation in fatal police use of force across U.S. counties. In the event-study, we use the shooting of Michael Brown in Ferguson, Missouri as the event; this event received widespread media coverage and sparked a national debate in the U.S. about police use of force, the militarization of the police, and the relationship between law enforcement officers and racial minorities. Using data from the Gallup Poll, we plot the time series for a question on "confidence in police" and find that right after the events in Ferguson, there is a large drop in minority's (black, Hispanic, and Native American) confidence in police and this fall in confidence persists until 2016 (the latest year with available data). However, there is no movement in white's confidence in police after the events of Ferguson. The existing gap in confidence in police between minorities and whites more than doubled after Ferguson. The same stark pattern does not exist for confidence in other formal or informal institutions, with the exception of the Criminal Justice System: There is a decline in confidence in the Criminal Justice System for minorities relative to whites after Ferguson.[7] Overall, the event-study analysis suggests that the Ferguson incident, and the controversy surrounding it, had a deteriorating effect on attitudes towards law enforcement agencies, but only for minorities.

Most cases of fatal (or non-fatal) police use of force do not make the national spotlight the way that the shooting of Michael Brown in Ferguson did. However, they may still affect attitudes toward the police locally.[8] Our second empirical approach is to use variation in

[7]This can be due to minority's perception that the Department of Justice or other criminal justice agencies did not do enough to either prevent or prosecute cases like Ferguson or the fact that the events in Ferguson sparked a national discourse on racial disparities in the Criminal Justice System more broadly, beyond the police. For instance, the issue of "modern-day debtor's prisons" attracted activism following the events in Ferguson: http://www.npr.org/sections/codeswitch/2015/02/08/384332798/civil-rights-attorneys-sue-ferguson-over-debtors-prisons, accessed March 2017.

[8]Incidents of fatal police use of force that do not get nationwide attention can still reach local communities.

fatal police use of force across the United States to measure the correlation between police use of force and attitudes toward the police. We combine data from the FBI's Supplementary Homicides Report on the killing of civilians by police officers[9] with detailed survey data on trust from the Social Capital Community Benchmark Survey.

The cross-sectional analysis yields four patterns in the data. First, and perhaps not surprisingly, minority's trust in the local police is 16% lower than whites. Men, younger people, less educated people, poorer people, and people living in urban areas (as opposed to suburbia or rural areas) have lower trust in the police. After controlling for these other correlates of trust in the police, minority trust in the police is still 13% lower compared to whites. Second, trust in local police is lower in counties where there have been more police killings in the last few years;[10] this pattern is not differential by race. Our third, and perhaps more surprising, finding comes from analyzing police killings of distinct racial groups. We find that minorities are less likely to trust the police in counties where minorities are more often the target of fatal police use of force. For whites, their attitude toward the police is not correlated with fatal police use of force against minorities. Furthermore, whites' attitudes toward the police is not robustly correlated with fatal police use of force against whites. Put differently: minority attitudes toward the police are correlated with fatal police use of force against their in-group at the county level, but the same robust correlation does not exist for whites. Lastly, no other measures of trust (generalized trust, trust in neighbors, trust in co-workers or local shopkeepers, trust in the local government, or trust in the national government) show the same patterns with regards to police use of force.

The cross-sectional approach provides suggestive evidence that minorities attitudes of

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First, even in the absence of national coverage, local news outlets and social networks spread the news of such incidents within an area. Additionally, fatal incidents of police use of force may be a proxy for police behavior during routine interactions with a local community, in addition to being "news-worthy events."

[9] Although the SHR data has numerous shortcomings, it is the most consistent source of national data on police killings. We discuss the shortcomings (and the implications of these shortcomings for our findings) in more detail in Section 2.2.

[10] We measure police killings in a county over the last 10 years. However, the results are robust to varying the window used to measure the history of police killings. Also, the number of police killings within a county over time are highly correlated.

the police is responsive to police behavior towards minorities but whites attitudes of the police is not responsive to how the police treats whites or minorities. Nevertheless, even after controlling for individual-level demographics and county-level characteristics, there may still be other factors that lead to differentially lower trust for minorities compared to whites and, at the same time, police killings of minorities. Two points may address this omitted variable bias concern. First, the findings of the cross-sectional analysis are in line with the findings of the event-study analysis: only minorities lower their confidence in the police after the events of Ferguson. Second, measures of social capital are highly correlated – even after controlling for demographic characteristics, generalized trust is highly correlated with trust in formal (local police, government) and informal (neighbors, co-workers, etc.) institutions. The fact that the variation in fatal police use of force across U.S. counties is correlated with the variation in trust in police but no other measure of trust, is suggestive that there is indeed a link between police use of force and attitudes towards the police.

Moving away from concerns of omitted variable bias, there is also a potential issue of reverse causality. Does fatal police use of force targeted at minorities lead to a relative decline in trust in the police for minorities *or* do minorities in certain areas who have relatively lower trust in the police (for whatever reason) act in a way during interactions with the police that results in fatal police use of force? We do not take a strong stance on this issue because our cross-sectional empirical strategy does not provide a convincing answer to it. We simply document a link between minority attitudes and minority police killings and we believe both directions of causality are interesting. Nonetheless, the event-study analysis suggests that the chain of causality moves (at least in the case of Ferguson) from police behavior to public attitudes. It should be noted that if the chain of causality goes from minority attitudes (and implied behavior) towards police behavior, then it has to be the case that minorities with low trust in the police are more aggressive or non-compliant in their interactions with police officers compared to whites with low trust in the police. This, however, contradicts anecdotal accounts that minorities often try to be cautious in their interactions with the police given their perception of police bias in use of force against

them.[11]

Overall, it seems that minorities feel alienated from policing institutions in areas where police often use fatal force against the minority community. Trust in the police matters because negative attitudes toward the police can perpetuate a cycle of police use of force through both reducing cooperation with law enforcement agencies *and* compliance with laws. First, the police rely on public cooperation to effectively prevent and fight crime (Tyler, 2003). Second, "Procedural Justice" theory suggests that people comply with the constraints of the law not just because of the threat or use of force by legal authorities, but also because they "buy into" the legitimacy of legal institutions such as the police and the courts. Declining confidence in the police can lead to declining feelings of obligation to obey the police, the courts, and the law (Tyler, 1998). Hence, police-minority relations based on mistrust can make minority communities more prone to crime, further aggressive law enforcement tactics, and, hence, ever worse relations between police and minorities. One of our findings, however, suggests one way to break this perpetual cycle. The negative correlation between minority trust in police and minorities killed by the police is more pronounced in areas where the racial composition of the police does not represent the racial composition of the population it serves. Therefore, one way to improve minority attitudes toward to police in areas where tensions between police and minority communities are high is to make the police racially representative of the community it serves.

The remainder of the paper is organized as follow. Section 2.2 describes the data sources used and the steps taken to construct our police use of force explanatory variable. Section 2.3 shows how attitudes toward the police responded to the events in Ferguson (event-study analysis), as well as how such attitudes are correlated with police use of force across U.S. counties (cross-sectional analysis). We also explore some of the heterogeneity in the cross-sectional analysis in this section. Section 2.4 concludes with a discussion of the findings and

[11]See, for example, "A Conversation With My Black Son" [https://www.nytimes.com/2015/03/17/opinion/a-conversation-with-my-black-son.html, accessed March 2017] or "'The Talk:' How Parents Of All Backgrounds Tell Kids About the Police [http://www.npr.org/2014/09/05/346137530/the-talk-how-parents-of-all-backgrounds-tell-kids-about-the-police, accessed March 2017].

direction for future research.

2.2 Data

2.2.1 Data on Public Opinion

Gallup Poll

To measure attitudes toward the police, and other institutions, we use two data sources. Our first source is the Gallup Poll, which is the only data source – to our knowledge – that asks about attitudes toward police consistently over time. Each year since 2001, the Gallup Poll Social Series polls a random sample of approximately 1,000–1,500 American adults on their confidence in different American institutions, including the police.[12] They also ask a series of demographic questions on the respondent's race, ethnicity, gender, education, income, etc. We supplement this Gallup series with one survey from CBS News, which was conducted in October of 2014 and had a similar polling methodology and the exact same questions on confidence in American institutions as the annual Gallup Poll. We combine the Gallup and CBS polls to study the impact of the shooting death of Michael Brown in Ferguson (and the ensuing nationwide discussions of race and policing) on attitudes toward the police. Specifically, we have an (almost-) annual time-series of responses to the question "Now I am going to read you a list of institutions[13] in American society. Please tell me how much confidence you, yourself, have in each one?" for different institutions from 2001 to 2016. The answer to this question ranges from 1, very little confidence, to 4, a great deal of confidence. The time-series is "almost"-annual because we have two surveys in 2014, which

[12]Specifically, Gallup interviews U.S. adults aged 18 and older living in all 50 states and the District of Columbia using a dual-frame design, which includes both land-line and cellphone numbers. Gallup samples land-line and cellphone numbers using random-digit-dial methods. Gallup purchases samples for this study from Survey Sampling International (SSI). Gallup chooses landline respondents at random within each household based on which member had the next birthday. Each sample of national adults includes a minimum quota of 50% cellphone respondents and 50% landline respondents, with additional minimum quotas by time zone within region. Gallup conducts interviews in Spanish for respondents who are primarily Spanish-speaking.

[13]The institutions asked about consistently between 2001-2016 are: banks, church or organized religion, Congress, Criminal Justice System, police, President, public schools, medical system, military, and Supreme Court.

makes the event-study analysis sharper. Zooming in closer to the events of Ferguson, which took place in August 2014, we have one survey from Gallup Poll right before, in June 2014, and one survey from CBS News right after, in October 2014.

Social Capital Survey

The Gallup data is suitable for an event-study analysis. However, because it surveys 1,000-1,500 people per year, the geography of the data is very sparse. In order to compare attitudes toward the police across U.S. counties with more or less police use of force, we use data from the Social Capital Community Benchmark Survey (Social Capital Survey, for short). The collection of this survey data was lead by Robert Putnam's organization and the stated purpose was to establish a benchmark for a systematic measurement of "social capital," or trust.[14] The two waves of the survey were conducted in 2000 (N=30,000) and 2006 (N=12,000); each survey wave had a nationally representative component, as well as components that focused on 41 metropolitan areas in 2000 and 22 metropolitan areas in 2006. We pool both waves and all components of the data together. The surveys ask detailed questions about non-institutional measures of trust (trust in people in the neighborhood, coworkers, people at church, local shops owners, white/black/Asian/Hispanic people, and people in general) and trust in formal institutions (police in the community, local government, national government). The answers to these trust questions range from 1–"trust them not at all" to 4–"trust them a lot." The survey also asks a wide range of demographic questions that, importantly, identify the race/ethnicity and location (county) of respondents. To understand the relationship between trust and policing, we match the Social Capital Survey to data on fatal police use of force at the county level.

[14]Social capital is defined as the "social networks and the associated norms of reciprocity and trustworthiness (Putnam, 2000).

2.2.2 Data on Fatal Police Use of Force

Our data on fatal police use of force comes from the FBI's Supplementary Homicides Reports (SHR), which is part of the Uniform Crime Reporting (UCR) program. SHR provides data on all homicides in the United States, including "justifiable homicides" by police officers. This means that a police officer killed a civilian and at the time that the agency reported the incident to the FBI, which occurs on a monthly basis, the lethal use of force was deemed justifiable by the corresponding police agency. Unfortunately, we cannot identify "unjustifiable" incidents of fatal police use of force in the SHR. These are incidents in which officers may have been charged, criminally or in civil proceedings. However, the vast majority of police killings are deemed justifiable.[15]

Besides not accounting for unjustifiable police killings, another flaw of the SHR is that reporting into the UCR program (which the SHR is a part of) is voluntary. However, according to the FBI, as of 2010, law enforcement agencies active in the UCR Program represented 97.4% of U.S. population and UCR coverage is higher in urban areas.[16] Still, the SHR data certainly underestimates the number of civilians killed by police officers both because not all departments report into the system and because unjustifiable police homicides are not accounted for. The other two sources for such information are the Bureau of Justice Statistics' Arrest-Related Deaths (ARD) program and the Centers for Disease Control and Prevention's National Vital Statistics System (NVSS). The ARD is (or was meant to be) a national census of persons who died either during the process of arrest or while in the custody of state or local law enforcement personnel. However, the Bureau of Justice Statistic itself determined that the collection of this data varied widely across states and was not reliable and, therefore, the program was suspended in 2014.[17,18] The NVSS codes

[15]See interview with University of South Carolina criminologist Geoff Alpert: https://www.usatoday.com/story/news/nation/2014/08/14/police-killings-data/14060357/, accessed March 2017.

[16]https://ucr.fbi.gov/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/aboutucrmain, accessed March 2017.

[17]https://www.bjs.gov/index.cfm?ty=tp&tid=82, accessed March 2017.

[18]Another reason this dataset is not suitable for our study is because we need data of police killings prior to

deaths due to "legal intervention," which captures police killings as well as legal executions. The NVSS is problematic because it relies on the medical coroner or examiner to explicitly mention police involvement before categorizing a case (that is not an execution) as death due to legal intervention. There is no standardized training or certification process for examiners and, therefore, the consistency of reporting widely varies across the U.S. (Loftin *et al.*, 2003). Furthermore, Loftin *et al.* (2003) found that the SHR estimate of justifiable homicides committed by US police officers was 29% larger than the NVSS estimate. Overall, despite its flaws, the FBI's SHR records remain the most complete official nationwide account of people killed by the police.[19]

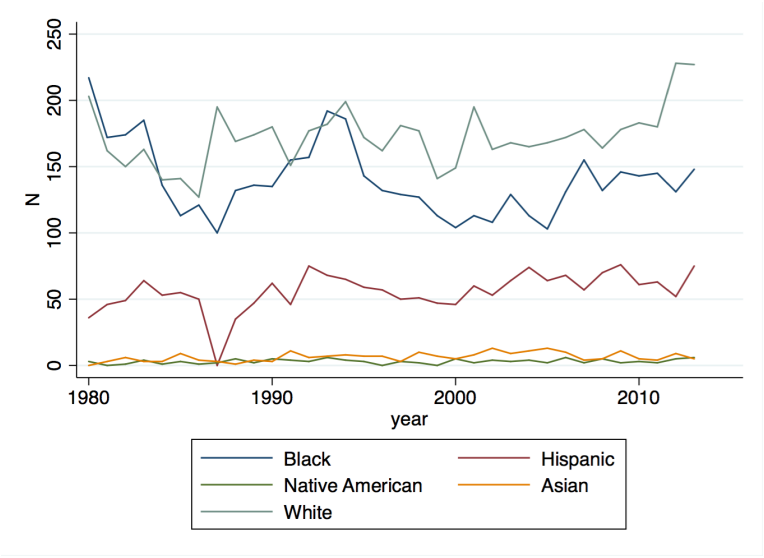
The SHR provides detailed, incident-level information on the victim(s), the offender(s), and the circumstances surrounding the homicide. We use the circumstance "felon killed by police officer" to identify instances of fatal police use of force.[20] Figure 2.1 shows the trends in the number of people killed by police between 1980[21] to 2013 (the latest year with available data at the time of data analysis). During this period, on average, there were 376 (justifiable) police killings per year across the U.S. This number peaked in 1994 with 462 police killings. We can identify the race/ethnicity of the person killed in the SHR data. Figure 2.1 also shows the trend in police killings separately for blacks, Hispanics, Native

2000: the first wave of the Social Capital Survey was conducted in 2000 but the ARD began its collection after that, in 2003.

[19]Since 2014, a number of media outlets and nonprofit organization have began collecting police killings through media searches, public records, social media, and/or crowdsourcing. For example, The Guardian's "The Counted" project counted 1,092 people killed by police in 2015. The Washington Post's "Fatal Force" project counted 991 people killed by police in the same year. "Mapping Police Violence" counted 1,152 people killed by police in 2015. All of these sources report considerably higher numbers than the SHR number, which is usually around 400. Unfortunately, we cannot utilize these innovative data sources because we need more historical records of fatal police use of force. Our hope is that the SHR data captures cross-sectional variation in fatal police use of force even if it does not capture the absolute number of fatal encounters with the police.

[20]The civilian is referred to as a felon due to the determination that the officer involved was justified to use lethal force. Two Supreme Court decisions from the 1980s, *Tennessee v. Garner* and *Graham v. Connor*, have set the framework for when deadly force by police is justifiable. Police officers are allowed to use lethal force under two circumstances: to protect their life or the life of another innocent party or to prevent the suspect from escaping – but only if the officer has cause to believe the suspect is dangerous. In addition, each individual police department may have additional standards that govern its officers' use of force.

[21]The first year with SHR data available is 1975. However, the quality of the data between 1975-1979 is lower than data beginning in 1980.

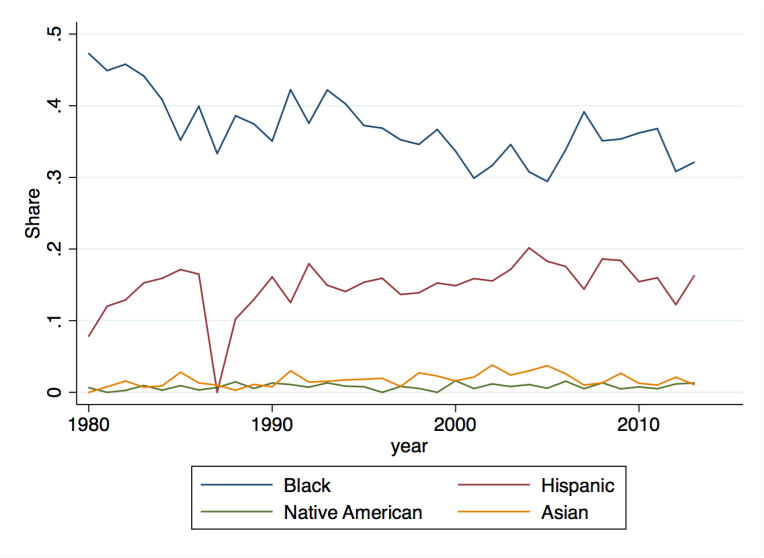


This graph plots the number of blacks, Hispanics, Native Americans, Asians, and (non-Hispanic) whites that are killed in a "justifiable" homicide by police officers from 1980 to 2013. The data is from the Supplementary Homicides Reports of the FBI. Data on ethnic origin (i.e. Hispanic origin) was not collected in 1987, hence, the dip in the Hispanic count for that year.

Figure 2.1: *Trend in Police Killings*

Americans, Asians, and whites. Figure 2.2 shows the trend in the share of people killed by police who are black, Hispanic, Native American, and Asian (the residual is whites killed by police). In 1987, the UCR did not track Hispanic origin; therefore, there is a dip in the number and share of people killed by police who are Hispanic. The share of police killings that are black has declined since 1980, while the share that is Hispanic has slightly increased. The increase for Hispanics may reflect broader demographic changes in the U.S.

We observe the police agency responsible for the "justifiable" police homicide in the SHR data. This allows us to identify the county in which the incident took place for the purpose of studying the cross-sectional correlation between trust in local police and police use of force. Since 1980, in 90.6% of county-years, there are no instances of "justifiable" police homicides reported to the SHR. The distribution of the number of police killings in the remaining county-years, ones that report at least one "justifiable" police homicide, is reported in Figure 2.3. The vast majority of counties that have at least one police killing

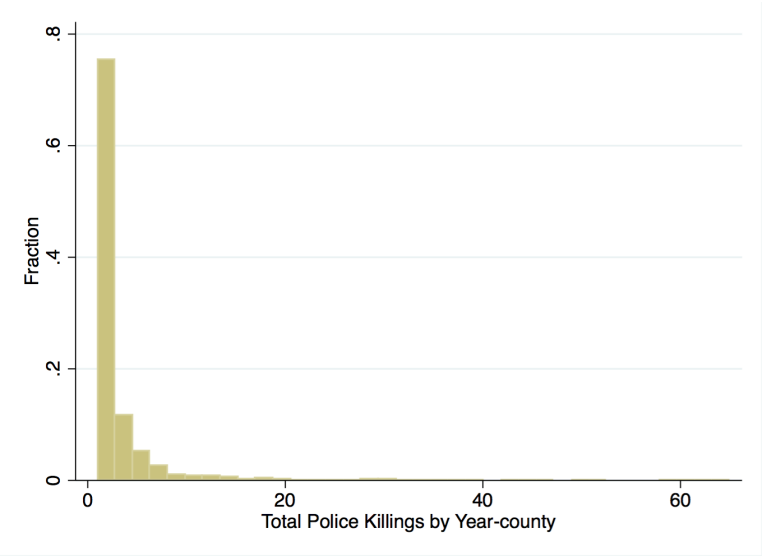


This graph plots the share of black, Hispanic, Native American, or Asian "justifiable" police homicides from 1980 to 2013. The data is from the Supplementary Homicides Reports of the FBI. Data on ethnic origin (i.e. Hispanic origin) was not collected in 1987, hence, the dip in the Hispanic share for that year.

Figure 2.2: *Trend in Racial Composition of Police Killings*

in a year have only 1-2 such incidents. However, the distribution is skewed to the right with Los Angeles County often reporting anywhere between 40–65 incidents per year. Los Angeles County has had, on average, 40 instances of "justifiable" police homicide per year, the highest average amongst all counties. New York County has the second highest average, with 17.5 such instances per year.

A very important limitation of the data is the following: if a police agency does not report any "justifiable" police homicide for a year, it is coded as having zero "justifiable" police homicides but there is no way to know whether there were truly no such incidents or whether there was such an incident but it was not reported. This caveat is particularly important for analysis of attitudes toward the police because it could certainly be the case that factors such as local police accountability measure influence both police reporting behavior *and* attitudes toward the local police. Two points address this concern. First, we do a robustness check where we limit the sample of analysis to states that consistently report to the UCR and SHR (Arizona, California, North Carolina, Oklahoma, Oregon, and Texas).



This histogram shows the distribution of "justifiable" police homicides in county-years with at least one such incident between 1980 to 2013. The vast majority of county-years (90.6%) have do not report any incidents of "justifiable" police homicides during this period. The data is from the Supplementary Homicides Reports of the FBI.

Figure 2.3: *Distribution of Police Killings Across County-years*

Second, the most likely scenario of systematic bias in reporting behavior is that places with the lowest trust in the police are the least likely to report their data; this would actually mean that the patterns we uncover in the data underestimate the relationship between fatal police use of force and trust.

2.2.3 Measuring Fatal Police Use of Force Across Counties

It is not obvious how each instance of fatal police use of force is perceived by the public. Do people evaluate each instance as a stand-alone event (in which case trust would be a function of the raw number of people killed by police)? Or do people evaluate each instance relative to the potential pool of instances? Since we do not know what model people use to form their opinion of the police, we construct several different variables to measure police use of fatal force across U.S. counties: the raw number of people killed by the police and the rate at which people are killed. For the raw number, we compute the total number of

people killed by the police in the last 10 years within a county. The last 10 years refers to 1990-1999 for those Social Capital Survey respondents surveyed in 2000 and 1996-2005 for those respondents surveyed in 2006.[22] We also compute the rate at which people are killed by dividing the number of people killed by the police by the county population in 2000, normalized by 100,000; this gives us a measure of police killings in a county per 100,000 people. We correlate these two explanatory variables, the raw number of people killed by police in county c over the last 10 years and the rate of police killings in county c over the last 10 years, with attitudes towards the police:

- $TotalPoliceKillings = TotalPoliceKillings_{c,10}$
- $TotalPoliceKillingsRate = \frac{TotalPoliceKillings_{c,10}}{Population_{c,2000}}$

Additionally, we construct measures of police killings of minorities. Again, because it is not clear how each instance of a minority killed by the police is interpreted, we construct several measures. First, we compute the raw number of minorities killed by police in each county over the last 10 years. We define minority as those who are black, Hispanic, or Native American/Alaskan.[23] Second, we compute the rate at which minorities are killed by the police by dividing the raw number of minorities killed by the total minority population in a county in 2000, again normalized by 100,000. Lastly, we also compute the minority share of police killings because a minority killed by the police may be interpreted differently in a place where no non-minorities are killed vs. in a place where many non-minorities are killed. To do this, we divide the number of minorities killed by the police in a county over the last 10 years by the total (minority and white) number of people killed by the police in a county over the last 10 years. This share is well-defined for the set of counties that have at least one police killing in the past 10 years. We correlate these additional explanatory

[22]Our results are not sensitive to using alternative windows to compute the number of police killings, such as police killings in the last 5, 15, or 20 years; this is perhaps not surprising given that police killings within a county are correlated over time.

[23]We do not categorize Asians as minority since the Asian-American community does not have the same tense relationship with the police as other racial minority groups do. Regardless, there are so few Asians killed by the police that we believe not including Asians in the minority count is inconsequential.

variables, the raw number of minorities killed by police in county c over the last 10 years, the rate of minority police killings in county c over the last 10 years, and the minority share of police killings in county c over the last 10 years, with attitudes towards the police:

- $MinorityPoliceKillings = MinorityPoliceKillings_{c,10}$
- $MinorityPoliceKillingsRate = \frac{MinorityPoliceKillings_{c,10}}{MinorityPopulation_{c,2000}}$
- $MinorityPoliceKillingsShare = \frac{MinorityPoliceKillings_{c,10}}{MinorityPoliceKillings_{c,10}+WhitePoliceKillings_{c,10}}$

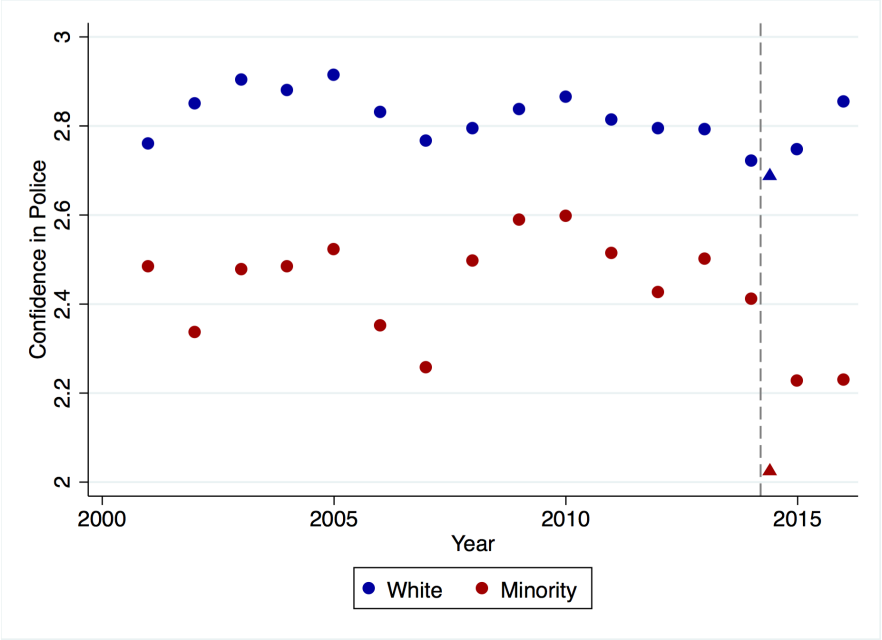
2.3 Empirical Analysis

2.3.1 Event-study Analysis

Figure 2.4 plots the time-series of confidence in the police from the Gallup Poll[24] separately for minorities (blacks, Hispanics, and Native Americans/Native Hawaiians) and whites (which includes non-Hispanic whites and Asians).[25] The dotted vertical line is approximately when Ferguson took place, August 9, 2014. The points immediately to the left of the dotted line are from a survey conducted in the first week of June of 2014 (2 months before Ferguson) and the points immediately to the right of the dotted line are from a survey conducted on October 15-16, 2014 (2 months after Ferguson.) As the figure shows, right after Ferguson, minority confidence in the police declined significantly; while for whites, there was almost no movement in confidence in the police. More precisely, in June of 2014, the gap in confidence between minorities and whites was $-.31$ on a 1–4 scale; by October 2014, this gap had more than doubled to $-.66$. By the summer of 2015, minority confidence in police had somewhat recovered but still significantly lower than its pre-Ferguson level; and in 2016, minority confidence was still lower than it was prior to Ferguson.

[24]More precisely, this times series comes from the annual Gallup Poll data conducted every summer and one CBS News Poll from October 2014. We refer to it as the Gallup time-series for simplicity.

[25]On average, Asians have confidence in the police that is closer to whites' confidence in the police. Therefore, we group Asians together with whites. However, there are only 323 Asians in the entire Gallup sample.

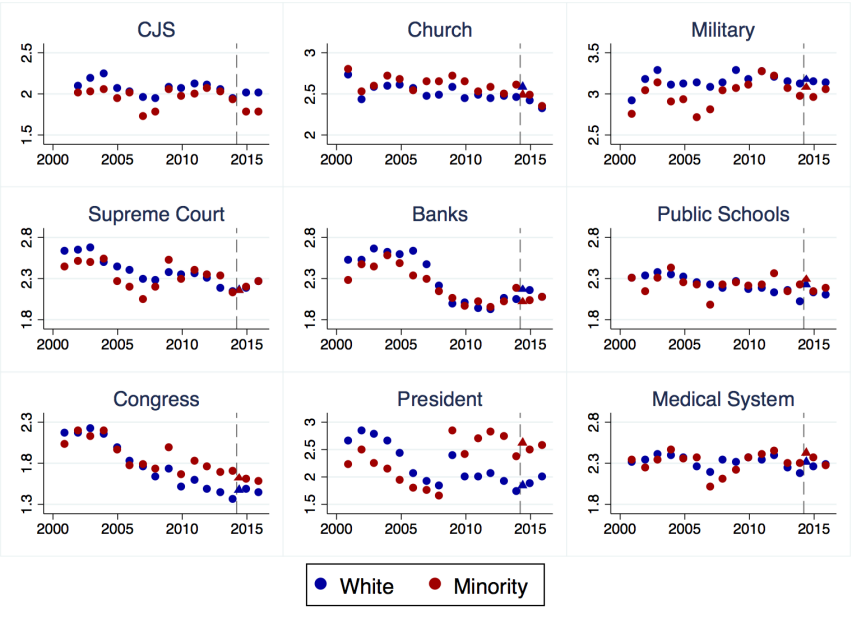


This graph plots the mean confidence in the police separately for minorities (blacks, Hispanics, Native Americans/Native Hawaiians) and whites. The confidence measure comes from either the Gallup Poll (round marks) or the CBS News Poll (triangular marks): "Now I am going to read you a list of institutions in American society. Please tell me how much confidence you, yourself, have in each one?" The answers range from 1, very little, to 4, a great deal. The dotted line roughly corresponds to August 2014, when Michael Brown was killed by police in Ferguson, MO. The Gallup Poll is conducted annually in the summer. The Gallup Poll right before Ferguson was conducted in June 2014. The CBS News Poll was conducted only once in October of 2014. The Gallup Poll and the CBS News Poll had identical methodologies and asked the exact same confidence questions. Each round of polling surveyed approximately 1,000 people.

Figure 2.4: *Confidence in Police Before and After Ferguson*

As Figure 2.4 shows, the decrease in confidence in police for minorities relative to whites after Ferguson is the largest such drop since 2001. In the mid-2000s, there was also a dip in confidence in the police for minorities relative to whites, although not as large as the decrease after Ferguson. Around 2005-2006, there were a series of high-profile cases of police use of force that involved black victims: In October 2004, Frank Jude was severely beaten by a group of ten off-duty Milwaukee police officers; In October 2005, Robert Davis was detained, arrested, and beaten by four police officers in New Orleans and the case was filmed by an Associated Press producer who was assaulted by a fifth officer; In November 2006, Sean Bell was shot 50 times, and killed, by plainclothes and undercover NYPD officers

the morning before his wedding.



This graph plots the mean confidence in each institution separately for minorities (blacks, Hispanics, Native Americans/Native Hawaiians) and whites. The confidence measure comes from either the Gallup Poll (round marks) or the CBS News Poll (triangular marks): "Now I am going to read you a list of institutions in American society. Please tell me how much confidence you, yourself, have in each one?" The answers range from 1, very little, to 4, a great deal. The dotted line roughly corresponds to August 2014, when Michael Brown was killed by police in Ferguson, MO. The Gallup Poll is conducted annually in the summer. The Gallup Poll right before Ferguson was conducted in June 2014. The CBS News Poll was conducted only once in October of 2014. The Gallup Poll and the CBS News Poll had identical methodologies and asked the exact same confidence questions. Each round of polling surveyed approximately 1,000 people. Confidence in the CJS was only asked in the Gallup Poll – therefore, this series only has two post-Ferguson periods of observation.

Figure 2.5: *Confidence in Other Institutions Before and After Ferguson*

Figure 2.5 shows the time-series of confidence in other institutions by race. There is no statistically significant decline in the confidence of minorities relative to whites after Ferguson in other American institutions, with the exception the Criminal Justice System (CJS). However, it should be noted that the CBS News Poll in October 2014 did not ask about confidence in the CJS; therefore, there are only two post-Ferguson periods of observations for the CJS. Nevertheless, it is not surprising that the events of Ferguson, which brought racial disparities in policing, the courts, and the justice system more broadly to the forefront of the national debate, would have an impact on confidence in the Criminal Justice System

as well as confidence in the police.

2.3.2 Cross-sectional Analysis

Confidence in the police plummeted after Ferguson. However, most cases of fatal (or non-fatal) police use of force do not make the national spotlight in the same way that the shooting death of Michael Brown in Ferguson, and the ensuing unrest, did. However, non-high-profile instances may still affect attitudes toward the police locally. We analyze whether the results from the event-study are generalizable: what is the relationship between fatal police use of force and attitudes towards at the police at the local level? And is there a differential pattern for minorities, as the the event-study suggests? In this subsection, we use data from the FBI SHR on "justifiable" police homicides and the Social Capital Survey to correlate fatal police use of force and attitudes toward the police at the local (county) level.

We first document some general patterns about attitudes toward the police for different subgroups. We then examine the relationship between trust in police and county-level fatal police use of force, as well as police use of force targeted at minorities. We also show that no other measure of trust (trust in institutions besides the police or non-institutional trust measures) is correlated with police use of force. Lastly, we present the heterogeneity of the results with respect to how racially representative of the population the local police force is.

Basic Patterns

As Table 2.1, Column 1 shows, not surprisingly, minorities have 16% lower trust in the police compared to whites. Also not surprisingly, older people, women, richer people, more educated people, and those living in suburban or rural areas (as opposed to urban areas) have higher trust in the police. After controlling for these other correlates of trust in the police, minorities still have 13% lower trust in the police compared to whites.

Table 2.1: *Basic Patterns in Trust in Local Police*

	Trust in Police						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Minority	-0.552***						-0.446***
	(0.023)						(0.024)
Age		0.009***					0.008***
		(0.000)					(0.000)
Female			0.086***				0.095***
			(0.010)				(0.009)
Income (less than 30K is omitted)							
=between 30K and 75K				0.157***			0.104***
				(0.013)			(0.013)
=more than 75K				0.239***			0.134***
				(0.016)			(0.013)
Education (HS or less is omitted)							
=some college					0.072***		0.033***
					(0.014)		(0.012)
=college grad					0.239***		0.154***
					(0.017)		(0.015)
=post graduate training					0.240***		0.115***
					(0.017)		(0.015)
Location (urban is omitted)							
=suburban						0.142***	0.060***
						(0.022)	(0.018)
=rural						0.105***	0.017
						(0.032)	(0.025)
Constant	3.468***	2.909***	3.296***	3.217***	3.240***	3.301***	2.881***
	(0.011)	(0.022)	(0.017)	(0.019)	(0.018)	(0.020)	(0.026)
Observations	35,535	35,535	35,535	35,535	35,535	35,535	35,535
R-squared	0.073	0.035	0.003	0.011	0.014	0.005	0.107
Clusters	1618	1618	1618	1618	1618	1618	1618

This table shows the relationship between trust in the local police and different demographic characteristics. Trust in the police is from a question in the Social Capital Survey that asks: "How much you can trust the police in your local community?" The answers range from 1, "trust them not at all," to 4, "trust them a lot." Minority is equal to one for blacks, Hispanics, and Native Americans/Alaskans and equal to zero for non-Hispanic whites and Asian Americans. Standard errors are clustered at the county level.

Total Police Killings

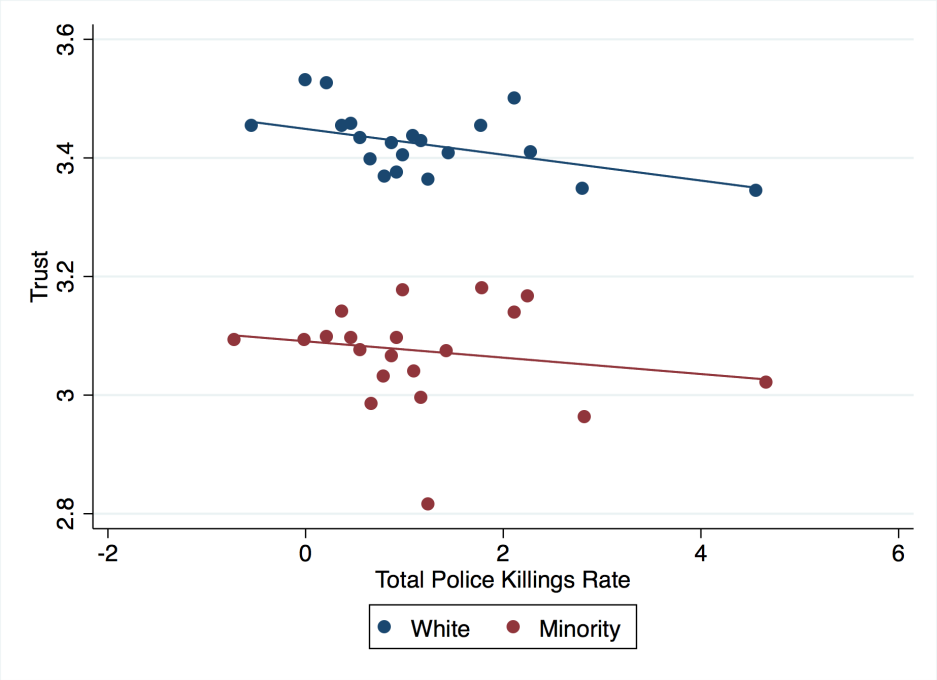
Table 2.2 shows the relationship between trust in local police and fatal police use of force at the county level. Fatal police use of force is measured either as the raw number of people killed by the police over the last 10 years in a county or the rate of such killings at the county-level. As Table 2.2 Columns 1 and 4 show, both measures of fatal police killings are negatively correlated with trust in the police. In the subsequent columns, we include correlates of trust in the police from Table 2.1, such as age, gender, income, education, and suburban/rural indicators, as demographic controls. Lastly, we also control for the population of the county and the minority share of the population as controls to account for the fact that police killings are likely to be higher in larger counties and/or counties with more minorities.

Table 2.2: *Trust in Local Police and Total Police Killings*

	Trust in Police					
	(1)	(2)	(3)	(4)	(5)	(6)
TotalPoliceKillings	-0.0007***	-0.0005***	0.0003			
	(0.0002)	(0.0002)	(0.0005)			
TotalPoliceKillingsRate				-0.0478***	-0.0362***	-0.0206**
				(0.0072)	(0.0070)	(0.0088)
Constant	3.3629***	2.6154***	2.7323***	3.4038***	2.6593***	2.7327***
	(0.0151)	(0.0275)	(0.0335)	(0.0149)	(0.0285)	(0.0346)
Observations	35,468	35,468	35,468	35,468	35,468	35,468
R-squared	0.0029	0.0657	0.0710	0.0077	0.0686	0.0718
Demographics	No	Yes	Yes	No	Yes	Yes
Minority Share	No	No	Yes	No	No	Yes
Population	No	No	Yes	No	No	Yes
Clusters	1596	1596	1596	1596	1596	1596

This table shows the relationship between trust in the local police and different measures of *total* police killings. Trust in the police is from a question in the Social Capital Survey that asks: "How much you can trust the police in your local community?" The answers range from 1, "trust them not at all," to 4, "trust them a lot." *TotalPoliceKillings* is measured as the total number of people killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005). *TotalPoliceKillingsRate* is *TotalPoliceKillings* divided by the population of the county in 2000 (normalized by 100,000). Demographic controls are: age, gender, education, income, and indication of urban location. The minority share and population controls are county-level measures taken from the 2000 Census. Standard errors are clustered at the county level.

Is this pattern similar for minorities and whites? Figure 2.6 show that although trust



This figure shows the binned scatterplot of trust in local police and the rate of total police killings separately for minorities (blacks, Hispanics, Native Americans/Alaskans) and whites.The trust measure comes from a question on the Social Capital Survey that asks "How much you can trust the police in your local community?" and is measured on a scale of 1 ("trust them not at all") to 4 ("trust them a lot"). The survey was conducted in 2000 (N=30,000) and 2006 (N=12,000). The total police killing rate is measured as the total number of people killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005) divided by the population of the county in 2000 (normalized by 100,000). Demographic characteristics (age, gender, education, income, and indication of urban location), total population in 2000, and the minority share of the population are included as controls.

Figure 2.6: *Trust in Police and Total Police Killings (Rate)*

is lower in areas with a higher rate of police killings, there is not a differential correlation for minorities. Table 2.3 shows the same result but using both the raw number of total killings and the rate of police killings. Although when the raw number of police killings is used as an explanatory variable, there is a differential (and surprisingly positive) impact on trust in police by minorities, this is not robust to the inclusion of controls. Furthermore, the rate measure of police killings as an explanatory variable does not exhibit a differential impact for minorities. Overall, it seems that both minorities and whites in counties with more police killings have lower trust in the police.

Table 2.3: *Trust in Local Police and Total Police Killings, by Minority Status*

	Trust in Police					
	(1)	(2)	(3)	(4)	(5)	(6)
Minority	-0.5655***	-0.3527***	-0.3903***	-0.5662***	-0.3589***	-0.4091***
	(0.0227)	(0.0527)	(0.0720)	(0.0278)	(0.0550)	(0.0707)
TotalPoliceKillings	-0.0004***	-0.0004***	-0.0002			
	(0.0001)	(0.0001)	(0.0004)			
Minority × *TotalPoliceKillings*	0.0005***	0.0007***	0.0007			
	(0.0001)	(0.0002)	(0.0009)			
TotalPoliceKillingsRate				-0.0246***	-0.0258***	-0.0210**
				(0.0069)	(0.0065)	(0.0090)
Minority × *TotalPoliceKillingsRate*				0.0180	0.0263**	0.0100
				(0.0113)	(0.0128)	(0.0139)
Constant	3.4736***	2.8456***	2.8730***	3.4928***	2.8695***	2.8821***
	(0.0113)	(0.0289)	(0.0335)	(0.0121)	(0.0306)	(0.0337)
Observations	35,468	35,468	35,468	35,468	35,468	35,468
R-squared	0.0739	0.1098	0.1102	0.0748	0.1104	0.1108
Demographics	No	Yes	Yes	No	Yes	Yes
Minority Share	No	No	Yes	No	No	Yes
Population	No	No	Yes	No	No	Yes
Clusters	1596	1596	1596	1596	1596	1596

This table shows the relationship between trust in the local police and different measures of *total* police killings separately for minorities and whites. Trust in the police is from a question in the Social Capital Survey that asks: "How much you can trust the police in your local community?" The answers range from 1, "trust them not at all," to 4, "trust them a lot." Minority is equal to one for blacks, Hispanics, and Native Americans/Alaskans and equal to zero for non-Hispanic whites and Asian Americans. *TotalPoliceKillings* is measured as the total number of people killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005). *TotalPoliceKillingsRate* is *TotalPoliceKillings* divided by the population of the county in 2000 (normalized by 100,000). Demographic controls are: age, gender, education, income, and indication of urban location. The minority share and population controls are county-level measures taken from the 2000 Census. In specifications with controls, the control variables are fully interacted with *Minority* status. Standard errors are clustered at the county level.

Minority Police Killings

So far, we have shown that trust in the local police is lower in counties with more police killings. We now consider *minorities* killed by the police in a county over the last 10 years as an explanatory variable. Of course, total police killings in a county and minority police killings in a county are highly correlated (total police killings is minorities killed plus non-minorities killed by the police). But given that Table 2.3 and Figure 2.6 show that minority and white attitudes toward the police have a similar correlation with respect to total police killings, we would like to understand how minorities and whites' attitudes correlate with minority police killings.

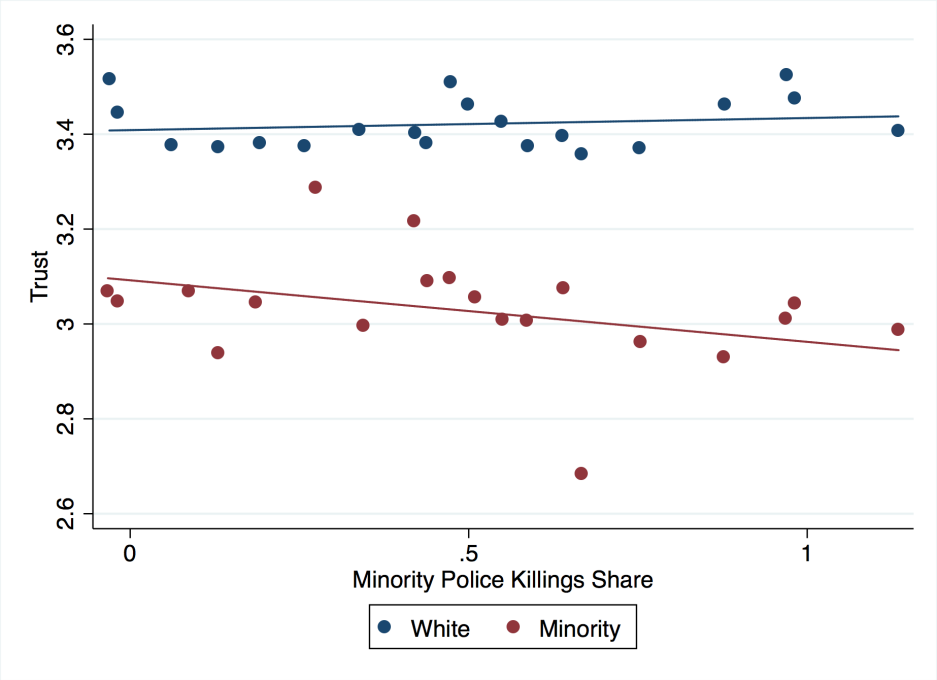
We consider three different measures of fatal police use of force against minorities: the raw number of minorities killed by the police over the last 10 years in a county, the share of police killings in the last 10 years in a county that are minority, and the rate of minority police killings. In considering the correlation between these different measures of minority police killings and attitudes toward the police, we control for total police killings since we have already shown that total police killings are negatively correlated with trust in the police. As Table 2.4 shows, regardless of the explanatory variable used to measure minority police killings and the controls used, minorities have lower trust in the police in counties where there are more minority police killings. Interestingly, whites' trust in the police is not correlated with minority police killings. Figures 2.7 and 2.8 show how the correlation for attitudes toward the police and minority police killings is different for minorities and whites. In counties with a higher share or rate of minority police killings, minorities have lower trust in the police; however, for whites, their level of trust in the police is comparable in counties with low and high measures of minority police killings.

Whites' attitudes toward the police is not correlated with how often police target minorities in their use of force. But is whites trust in the police correlated with police killing of whites? Looking at the coefficient of *MinorityPoliceKillingsShare* in Table 2.4 sheds light on this question because the minority share of police killings is one minus the white share of police killings. Therefore, the middle rows in Table 2.4 suggest that whites'

Table 2.4: *Trust in Local Police and Minority Police Killings*

	Trust in Police								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Minority	-0.5821***	-0.3836***	-0.4222***	-0.5004***	-0.3090***	-0.4048***	-0.5161***	-0.3127***	-0.3462***
	(0.0232)	(0.0506)	(0.0668)	(0.0358)	(0.0636)	(0.0846)	(0.0233)	(0.0513)	(0.0663)
MinorityPoliceKillings	0.0011	0.0015	0.0012						
	(0.0016)	(0.0017)	(0.0016)						
Minority × *MinorityPoliceKillings*	-0.0061**	-0.0077***	-0.0076***						
	(0.0024)	(0.0026)	(0.0028)						
MinorityPoliceKillingsShare				0.0246	0.0279	0.0392			
				(0.0396)	(0.0389)	(0.0371)			
Minority × *MinorityPoliceKillingsShare*				-0.1518***	-0.1431***	-0.1705***			
				(0.0564)	(0.0546)	(0.0488)			
MinorityPoliceKillingsRate							-0.0057	-0.0051	-0.0038
							(0.0049)	(0.0047)	(0.0049)
Minority × *MinorityPoliceKillingsRate*							-0.0202**	-0.0166**	-0.0189**
							(0.0083)	(0.0084)	(0.0088)
Constant	3.4755***	2.8486***	2.8740***	3.4625***	2.8602***	2.8935***	3.4811***	2.8553***	2.8777***
	(0.0115)	(0.0291)	(0.0337)	(0.0246)	(0.0385)	(0.0491)	(0.0119)	(0.0292)	(0.0333)
Observations	35,468	35,468	35,468	24,038	24,038	24,038	35,468	35,468	35,468
R-squared	0.0744	0.1105	0.1110	0.0854	0.1165	0.1172	0.0752	0.1107	0.1111
Total Police Killings	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Minority Share	No	No	Yes	No	No	Yes	No	No	Yes
Population	No	No	Yes	No	No	Yes	No	No	Yes
Clusters	1596	1596	1596	471	471	471	1596	1596	1596

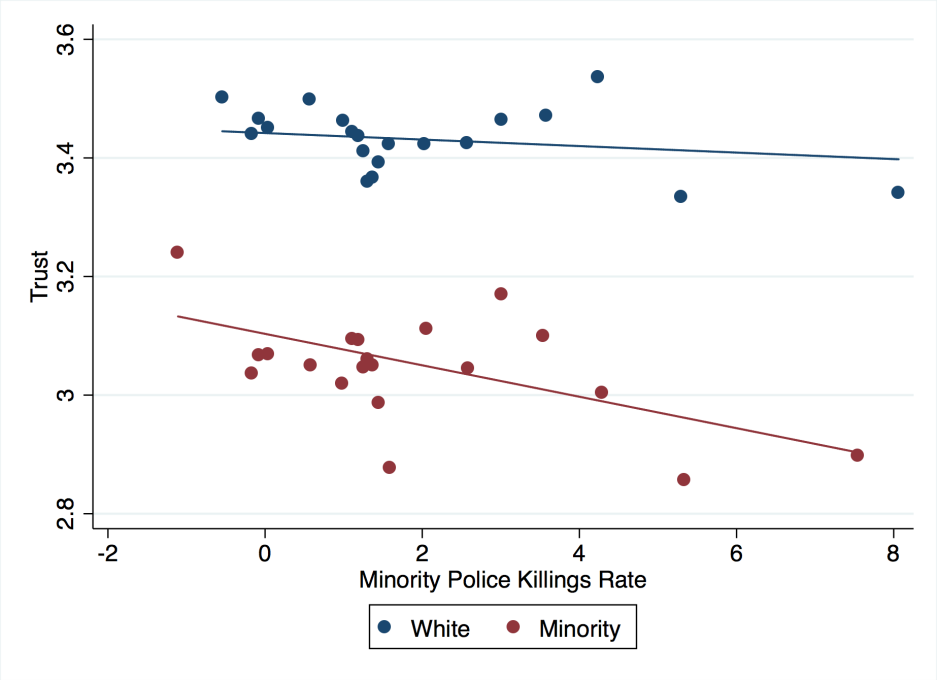
This table shows the relationship between trust in the local police and different measures of *minority* police killings separately for minorities and whites. Trust in the police is from a question in the Social Capital Survey that asks: "How much you can trust the police in your local community?" The answers range from 1, "trust them not at all," to 4, "trust them a lot." Minority is equal to one for blacks, Hispanics, and Native Americans/Alaskans and equal to zero for non-Hispanic whites and Asian Americans. *MinorityPoliceKillings* is measured as the number of minorities killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005). *MinorityPoliceKillingsShare* divides *MinorityPoliceKillings* by the total number of people (minorities and whites) killed by the police within the county over the same time period. *MinorityPoliceKillingsRate* is *MinorityPoliceKillings* divided by the minority population of the county in 2000 (normalized by 100,000). Total police killings is the total number of people killed by the police within the county over the last 10 years. Demographic controls are: age, gender, education, income, and indication of urban location. The minority share and population controls are county-level measures taken from the 2000 Census. In specifications with controls, the control variables are fully interacted with *Minority* status. Standard errors are clustered at the county level.



This figure shows the binned scatterplot of trust in local police and the minority share of police killings separately for minorities (blacks, Hispanics, Native Americans/Alaskans) and whites.The trust measure comes from a question on the Social Capital Survey that asks "How much you can trust the police in your local community?" and is measured on a scale of 1 ("trust them not at all") to 4 ("trust them a lot"). The survey was conducted in 2000 (N=30,000) and 2006 (N=12,000). The Minority Police Killing Share is measured as the number of minorities killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005) divided by the total number of people (minority and white) killed in a "justifiable" police homicide over the same time period. Demographic characteristics (age, gender, education, income, and indication of urban location) and total police killings over the last 10 years in the county are included as controls.

Figure 2.7: *Trust in Police and Minority Police Killings (Share)*

trust in the police is not correlated with the share of police killings that are white (otherwise the coefficient on *MinorityPoliceKillingsShare* would be positive and significant. Table 2.5 explicitly shows the relationship between attitudes towards the police for whites (and for minorities) with respect to the raw number of white police killings, the white share of police killings, and the rate of white police killings. The evidence for whites is mixed: whites who live in areas with more white police killings as measured by the raw number of white police killings and the share of police killings that are white do not have lower trust in the police. However, there is a negative correlation between whites' trust in the police and the



This figure shows the binned scatterplot of trust in local police and the rate of minority police killings separately for minorities (blacks, Hispanics, Native Americans/Alaskans) and whites.The trust measure comes from a question on the Social Capital Survey that asks "How much you can trust the police in your local community?" and is measured on a scale of 1 ("trust them not at all") to 4 ("trust them a lot"). The survey was conducted in 2000 (N=30,000) and 2006 (N=12,000). The Minority Police Killing Rate is measured as the number of minorities killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005) divided by the minority population of the county in 2000 (normalized by 100,000). Demographic characteristics (age, gender, education, income, and indication of urban location) and total police killings over the last 10 years in the county are included as controls.

Figure 2.8: *Trust in Police and Minority Police Killings (Rate)*

rate of white police killings. Overall, the relationship between white attitudes toward the police and police killings targeted at whites is not robust and is sensitive to the explanatory variable used to measure police killings of whites. This is in contrast to the evidence for minorities, which showed a robust relationship between minority attitudes and minority police killings.

Table 2.5: *Trust in Local Police and White Police Killings*

	Trust in Police								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Minority	-0.5803***	-0.3810***	-0.4166***	-0.6522***	-0.4521***	-0.5752***	-0.5786***	-0.3711***	-0.4007***
	(0.0233)	(0.0507)	(0.0673)	(0.0372)	(0.0612)	(0.0846)	(0.0258)	(0.0535)	(0.0715)
WhitePoliceKillings	-0.0009	-0.0012	-0.0009						
	(0.0016)	(0.0017)	(0.0016)						
Minority × *WhitePoliceKillings*	0.0054**	0.0069**	0.0067**						
	(0.0025)	(0.0027)	(0.0029)						
WhitePoliceKillingsShare				-0.0246	-0.0279	-0.0392			
				(0.0396)	(0.0389)	(0.0371)			
Minority × *WhitePoliceKillingsShare*				0.1518***	0.1431***	0.1705***			
				(0.0564)	(0.0546)	(0.0488)			
WhitePoliceKillingsRate							-0.0226***	-0.0211***	-0.0194***
							(0.0059)	(0.0060)	(0.0066)
Minority × *WhitePoliceKillingsRate*							0.0211**	0.0225**	0.0191*
							(0.0106)	(0.0112)	(0.0111)
Constant	3.4752***	2.8481***	2.8736***	3.4872***	2.8882***	2.9327***	3.4882***	2.8625***	2.8836***
	(0.0116)	(0.0292)	(0.0336)	(0.0263)	(0.0411)	(0.0499)	(0.0115)	(0.0299)	(0.0336)
Observations	35,468	35,468	35,468	24,038	24,038	24,038	35,468	35,468	35,468
R-squared	0.0743	0.1104	0.1108	0.0854	0.1165	0.1172	0.0749	0.1106	0.1108
Total Police Killings	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Minority Share	No	No	Yes	No	No	Yes	No	No	Yes
Population	No	No	Yes	No	No	Yes	No	No	Yes
Clusters	1596	1596	1596	471	471	471	1596	1596	1596

This table shows the relationship between trust in the local police and different measures of *white* police killings separately for minorities and whites. Trust in the police is from a question in the Social Capital Survey that asks: "How much you can trust the police in your local community?" The answers range from 1, "trust them not at all," to 4, "trust them a lot." Minority is equal to one for blacks, Hispanics, and Native Americans/Alaskans and equal to zero for non-Hispanic whites and Asian Americans. *WhitePoliceKillings* is measured as the number of whites (and Asians) killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005). *WhitePoliceKillingsShare* divides *WhitePoliceKillings* by the total number of people (minorities and whites) killed by the police within the county over the same time period. *WhitePoliceKillingsRate* is *WhitePoliceKillings* divided by the white population of the county in 2000 (normalized by 100,000). Total police killings is the total number of people killed by the police within the county over the last 10 years. Demographic controls are: age, gender, education, income, and indication of urban location. The minority share and population controls are county-level measures taken from the 2000 Census. In specifications with controls, the control variables are fully interacted with *Minority* status. Standard errors are clustered at the county level.

Table 2.6: *Other Measures of Trust and Minority Police Killings*

	(1) General Trust	(2) Neighbors	(3) Co-workers	(4) Local Shopkeepers	(5) Whites	(6) Blacks	(7) Hispanics	(8) Local Government	(9) National Government
Minority	-0.1934***	-0.5391***	-0.6002***	-0.4439***	-0.2113***	-0.3474***	-0.0451	0.0669	0.1816***
	(0.0647)	(0.0623)	(0.0600)	(0.0504)	(0.0474)	(0.0547)	(0.0489)	(0.0608)	(0.0542)
MinorityPoliceKillingsRate	0.0002	-0.0100**	-0.0011	-0.0050*	-0.0085***	-0.0080***	-0.0044	-0.0127***	0.0000
	(0.0050)	(0.0046)	(0.0036)	(0.0028)	(0.0028)	(0.0027)	(0.0027)	(0.0047)	(0.0032)
MinorityxMinorityPoliceKillingsRate	0.0016	0.0041	0.0055	0.0049	0.0049	0.0030	0.0070	0.0049	-0.0018
	(0.0073)	(0.0071)	(0.0067)	(0.0068)	(0.0054)	(0.0053)	(0.0060)	(0.0083)	(0.0069)
Constant	1.5504***	2.5845***	3.0974***	2.5123***	2.8652***	2.7910***	2.7382***	2.1786***	1.9652***
	(0.0421)	(0.0347)	(0.0322)	(0.0317)	(0.0220)	(0.0238)	(0.0242)	(0.0330)	(0.0289)
Observations	24,908	24,908	24,908	24,908	24,908	24,908	24,908	24,908	24,908
R-squared	0.1295	0.2225	0.1444	0.1333	0.0881	0.1072	0.0738	0.0455	0.0153
Total Police Killings	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Minority Share	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Population	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	1324	1324	1324	1324	1324	1324	1324	1324	1324

This table shows the relationship between different measures of trust and the rate of minority police killings separately for minorities and whites. All measures of trust come from the Social Capital Survey. The exact questions are as follows. General Trust asks "most people can be trusted" and the answer options are 1–"you cannot be too careful' and' 2–"People can be trusted." For people in your neighborhood, people you work with, people who work in the stores where you shop, white people, African Americans or blacks, and Hispanics or Latinos the question is "How much can you trust [...]?" and the answers range from 1, "trust them not at all," to 4, "trust them a lot." For local and national government, the question is "How much of the time do you think you can trust the [...] government to do what is right?" and the answers range from 1, "hardly ever," to 4, "just about always." Minority is equal to one for blacks, Hispanics, and Native Americans/Alaskans and equal to zero for non-Hispanic whites and Asian Americans. *MinorityPoliceKillingsRate* is the number of minorities killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005) divided by the minority population of the county in 2000 (normalized by 100,000). Total police killings is the total number of people killed by the police within the county over the last 10 years. Demographic controls are: age, gender, education, income, and indication of urban location. The minority share and population controls are county-level measures taken from the 2000 Census. In specifications with controls, the control variables are fully interacted with *Minority* status. Standard errors are clustered at the county level.

Other Measures of Trust

The Social Capital Survey asks about trust in the police, but also about: trust in general, trust in people in the neighborhood, trust in people the respondent works with, trust in people who work in the stores where the respondent shops, trust in white people, trust in black people, trust in Hispanics, trust in the local government, and trust in the national government. We would like to understand how police use of force correlates with these measures of trust. It may be that low trust in the police in areas with many minority police killings "spills over" to lower trust in the government (since the government may seem partially responsible or complacent in such instances as was the case with the Criminal Justice System after Ferguson). However, as Table 2.6 shows, non-institutional measures of trust or trust in national or local government do not exhibit the same patterns with respect to police use of force targeted at minorities. It seems that minority's response to fatal police use of force is narrowly targeted at the police.

Heterogeneity Analysis

We also analyze whether some subgroup of minorities have a stronger response to fatal police use of force. We do not find any heterogeneity with respect to age or gender.[26] However, one interesting heterogeneity in the results is shown in Table 2.7. The first column shows the relationship between minority attitudes and minority police killings in counties where the share of minorities in the police force is greater than or equal to the share of minorities in the county: there is no significant correlation. The second column shows the same relationship in counties where the share of minorities in the police force is less than the share of minorities in the population: there is a marginally significant correlation. Although we do not have enough power to reject that the coefficient on $Minority \times MinorityPoliceKillingsRate$ in the first column is significantly different than the same coefficient in the second column, the results here are suggestive that having a racially unrepresentative police force may exacerbate tensions between minority communities and the police in areas where minorities

[26]Results not shown.

Table 2.7: *Heterogeneity w.r.t How Racially Representative the Local Police is*

	Min Share of Officers $\geq$ Min Share in Population	Min Share of Officers < Min Share in Population
Minority	-0.3039	-0.4028***
	(0.2357)	(0.0741)
MinorityPoliceKillingsRate	-0.0256**	-0.0054
	(0.0105)	(0.0068)
Minority $\times$ *MinorityPoliceKillingsRate*	0.0048	-0.0175*
	(0.0190)	(0.0101)
Constant	2.7134***	2.9310***
	(0.0880)	(0.0357)
Observations	4,645	27,086
R-squared	0.1298	0.1133
Total Police Killings	Yes	Yes
Demographics	Yes	Yes
Minority Share	Yes	Yes
Population	Yes	Yes
Clusters	133	843

This table shows the relationship between trust in the local police and the rate of minority police killings separately for minorities and whites in two different sub-samples: Column 1 shows this relationship in counties where the minority share of full-time police officers in the county is greater than or equal to the minority share of the county population in 2000; Column 2 shows this relationship in counties where the minority share of officers is less than the minority share of the population. Trust in the police is from a question in the Social Capital Survey that asks: "How much you can trust the police in your local community?" The answers range from 1, "trust them not at all," to 4, "trust them a lot." Minority is equal to one for blacks, Hispanics, and Native Americans/Alaskans and equal to zero for non-Hispanic whites and Asian Americans. *MinorityPoliceKillingsRate* is the number of minorities killed in a "justifiable" police homicide within a county over the last 10 years (i.e. 1990-1999 for respondents in 2000 and 1996-2005 for respondents in 2005) divided by the minority population of the county in 2000 (normalized by 100,000). Total police killings is the total number of people killed by the police within the county over the last 10 years. Demographic controls are: age, gender, education, income, and indication of urban location. The minority share and population controls are county-level measures taken from the 2000 Census. In specifications with controls, the control variables are fully interacted with *Minority* status. Standard errors are clustered at the county level.

are often the target of fatal police encounters.

2.4 Discussion

Minorities have lower trust in the police compared to whites. Our paper suggests one potential factor for this gap: fatal police use of force. It seems that minority attitudes towards the police is responsive to fatal police use of force against their in-group. Whites' attitudes towards the police does not display the same pattern with regards to fatal police use of force against minorities or even whites. This differential pattern in response to fatal police use of force may be due to how instances of police use of force are interpreted: a minority civilian being killed by police officers may remind the community of the historical injustices or current racial disparities in society; a white civilian being killed by police officers may be interpreted as an instance of police doing their job and keeping the community safe. Future work should investigate how instances of police use of force against different racial groups are internalized.

Chapter 3

Race-based Affirmative Action and Student Effort[1]

3.1 Introduction

Affirmative action policies that weigh race or ethnicity as one factor in the college admission process are widespread in higher education in numerous countries, including the United States, Canada, Brazil, and India. In the U.S., affirmative action policies in public universities have repeatedly been challenged by court cases at the sub-national and national level,[2] and eight states have banned race-based affirmative action at all public universities. Despite the importance of race-based affirmative action policies, and the controversy surrounding them, there has been relatively little research on whether or how affirmative action policies affect students *prior* to reaching college.

Theoretically, the effects of affirmative action policies that favor minority students in the college admissions process on human capital investment prior to college entry are ambiguous. On the one hand, affirmative action policies may lead secondary school

[1]Co-authored with Natalie Bau

[2]Such cases include: *Regents of the University of California v. Bakke* in 1979, *Hopwood v. Texas* in 1996, *Grutter v. Bollinger* and *Gratz v. Bollinger* in 2003, *Fisher v. University of Texas* in 2013, *Schuette v. Coalition to Defend Affirmative Action* in 2014, and, most recently, *Fisher v. University of Texas* in 2015.

minority students to invest less in their human capital by lowering the threshold for college admissions (Coate and Loury, 1993). On the other hand, affirmative action policies may incentivize minority students to work harder by increasing the probability that their hard work will translate into college admission (Fryer and Loury, 2005). Since the theoretical effects of affirmative action are ambiguous, we turn to empirical methods to determine which of these theories best describes the behavior of minority students.

To investigate the effects of affirmative action[3] on the human capital investment of high school students, we exploit a natural experiment that induced a policy reversal in Texas, Louisiana, and Mississippi. In 2003, the Supreme Court decision in *Grutter v. Bollinger* ruled that a race-conscious admissions process that does not amount to a quota system is constitutional. This effectively reversed an earlier, lower court ruling that had prohibited the use of race in the admissions process in public universities in these three states. Therefore, we can examine how affirmative action policies affect student outcomes prior to college entry by comparing white and minority (black and Hispanic) students' outcomes before and after the 2003 court ruling.[4]

We first use a panel of state-by-race-by-year SAT scores to examine how minorities' and whites' SAT scores evolved following the reinstatement of affirmative action in public universities. Using difference-in-differences and triple-differences identification strategies, we find that minorities' math SAT scores improved in the treated states following the court ruling, and they improved relative to whites' math SAT scores by 0.07 standard deviations.

We then expand on these results using administrative data for repeated cross-sections of eleventh graders from a large, urban school district in Texas. With these data, we estimate how the within school-year racial achievement gap changed for 11^{th} graders following the introduction of affirmative action.[5] We find that after 2003, the achievement gap between

[3]For simplicity, unless otherwise noted, we use "affirmative action" to refer to race-based affirmative action in the college admissions process.

[4]The Texas "Top 10% Rule," which guarantees admission to any Texas public university to high school students graduating in the top 10% of their class, was held constant throughout our study period.

[5]We focus on eleventh graders because this group is preparing for college applications, and is therefore

minority and white high school students narrows for standardized test scores and course grades. Furthermore, the relative improvement in grades occurs despite minority students enrolling in more advanced courses after the policy change. The reduction in the racial achievement gap following the 2003 policy reversal is large and economically meaningful. The racial achievement gap narrows by .17 standard deviations for standardized test scores (20% of the within-school-year gap between minorities and whites) and by .07 standard deviations for course grades (19% of the within-school-year gap). Importantly, we compare minority and white students within the same school-year. Therefore, our estimates of the reductions in the achievement gap after 2003 cannot be attributed to a general improvement of poorly performing schools that are predominantly composed of minority students. Moreover, using placebo tests that move the policy change earlier in time, we find no evidence that our results are driven by differential time trends across races. We also do not observe any changes in the achievement gap between Asian-American and white students following the 2003 court ruling: this finding is consistent with the fact that Asian-American students are not favored by race-based affirmative action policies.

Finally, to better understand the mechanisms underpinning these results, we analyze survey data from high school seniors across Texas collected before and after the policy change. Our analysis suggests that students' behavior and aspirations respond to the policy change: minority students are more likely to spend time on their homework and they are more likely to apply to their first-choice college after 2003 compared to white students. We do not find that parental behavior or frequency of discussions about college applications with guidance counselors change after the policy change. Overall, our estimates provide evidence that race-based affirmative action in higher education can reduce the average racial achievement gap in student outcomes in high school.

Broadly our results relate to a large literature that studies the effects of affirmative action policies. This literature has focused primarily on affirmative action policies in higher education and their impact on college application behavior, college admissions, and

focused on admissions, but has not already sent out applications.

college graduation. This extensive literature includes Bowen and Bok (1998), Card and Krueger (2005), Arcidiacono (2005), Sander (2004), and Rothstein and Yoon (2008). Our main contribution is to a much smaller literature about the implication of affirmative action for student behavior *prior* to college admissions.

In this literature, Antonovics and Backes (2014) study California's ban on affirmative action and conclude that SAT scores and high school GPA changed little after the ban on race-based affirmative action. However, students' responses to the introduction of affirmative action and the banning of affirmative action may be asymmetric and, therefore, our study of the effect of the introduction of affirmative action is complementary to Antonovics and Backes (2014). Ferman and Assunção (2005) study the effects of race-based university admissions quotas in Brazil on high school students' test scores. They find that aggressive university quotas for black students in public high schools undercut the incentives of this group and had a negative effect on these students' test scores. However, the quotas they study are very aggressive. In fact, other groups of students, such as black students in private schools or mixed race students in public or private schools, which had relatively less aggressive quotas implemented in their favor, did not respond to the policy change. Therefore, the adverse effects on test scores they find may be particular to extreme cases of affirmative action.

Perhaps the papers most closely related to this one are Cotton *et al.* (2015) and Hickman (2013). Cotton *et al.* (2015) combines a theoretical framework with a field experiment. Their model assumes the existence of two demographic groups of students who have different learning costs and allows for a period of investment in human capital running up to a matching game between colleges and students. They model affirmative action as the admissions board basing placement decisions partially on demographic status. Overall, their model predicts that affirmative action increases the disadvantaged group's investment in human capital on average. They conduct a field experiment which confirms the predictions of their model: they pay middle school students based on their relative performance on a national math exam, using grade-cohort as the demographic delimiter, and find that

affirmative action increases the human capital investment of the disadvantaged group, as well as their proficiency. In contrast, Hickman (2013) structurally estimates the college admissions market and generates counterfactuals under race-neutral admissions. These counterfactuals suggest that eliminating race-based affirmative action would greatly reduce pre-college human capital investments by minorities. Our analysis is consistent with the findings of Cotton *et al.* (2015) and Hickman (2013) and confirms that affirmative action can increase minority students' human capital investment prior to the college matching process. We complement the findings of these papers by studying a real policy change using data that reveals the investments of high school students using their behavior on multiple dimensions, such as test scores, grades, and difficulty of courses.

Finally, our study relates to another strand of research on the effects of "color-blind" affirmative action on student effort. Cortes and Zhang (2011) study the incentive effects of the Top 10% Rule, which guarantees admission to a public university for Texas students in the top 10% of their high school graduating class. Cortes and Zhang (2011) find that the plan incentivized students to increase their effort in high school. While these results are consistent with ours, the Top 10% Rule and race-based affirmative action are quite different. First, unlike race-based affirmative action, the Top 10% Rule is manipulable since students can switch schools to help ensure better outcomes (Cullen *et al.*, 2013). Second, unlike race-based affirmative action, the Top 10% Rule has an explicit tournament structure with clear cutoffs. Therefore, it is unclear how similar the incentive effects of these policies will be.

In future drafts, we will expand upon the findings here. Theoretically, the response to affirmative action depends on a student's ability, the distribution of her competitors, and how she compares to her competitors overall. In other words, high ability students and low ability students may respond differently to affirmative action. The model of Cotton *et al.* (2015) predicts that affirmative action will decrease human capital accumulation for the highest ability minority students while increasing human capital accumulation by intermediate and low ability students. Therefore, while we find that affirmative action reduces the

racial achievement gap on average, it is important to investigate the distributional effects of the 2003 policy change. To this end, we have recently acquired lagged outcomes for our repeated cross-sections of 11^{th} graders. Using this information, we plan to estimate heterogeneous effects of affirmative action based on a student's position in the distribution *before* the affirmative action policy was reinstated. This exercise will allow us to quantify the effect of affirmative action beyond the average treatment effect and estimate the response for different groups of students more precisely.

Finally, we are in the process of linking individual-level records from the Texas Education Agency for all students in Texas to college administrative data from the Texas Higher Education Coordinating Board and to employment data from the Texas Workforce Commission. This will allow us to: (i) estimate the effect of using race-based affirmative action in college admissions on *all students* in Texas; (ii) estimate the effect of affirmative action on college applications, college enrollment, and college graduation for those students who continue on to higher education within the state;[6] (iii) leverage the comprehensive nature of the dataset to investigate the heterogeneity in response to the policy across the state; and (iv) study the effect of affirmative action on long-run employment outcomes, which will be a new contribution to the affirmative action literature. Although the comprehensive nature of the TEA data allows us to track *all* Texas students throughout their educational careers and employment records, our current study using data from one school district in Texas offers some advantages and is, therefore, also of importance. In particular, the TEA data measures student performance primarily using state-wide standardized tests; however, the Texas-wide TAAS changed to a different test, TAKS, in 2003, making comparisons before and after 2003 difficult. In our school district data, we observe a different standardized test that allows us to sidestep the use of the Texas-wide standardized tests. In addition, we observe students' course grades, which are unavailable in the TEA data. While we work to complete this more extensive study, we believe our current results are also of importance and provide evidence on the incentive effects of affirmative action.

[6] Approximately 90% of Texas high school students who continue onto college do so within the state.

The remainder of the paper will introduce the context in more detail in Section 3.2, and discuss the data in Section 3.3. In Section 3.4, we will present our results using both the nation-wide SAT and Texan administrative data. Section 3.5 concludes and discusses directions for future work.

3.2 Context

In 1996, the U.S. Court of Appeals for the Fifth Circuit, which has jurisdiction over Texas, Louisiana and Mississippi, ruled in *Texas v. Hopwood* that universities may not use race as a factor in deciding which applicants to admit. In the wake of this ruling, the Texas legislature passed the "Top 10% Rule" in 1997, which guaranteed admissions to *any* public university in Texas to those students graduating in the top 10% of their class. This law was passed as a means to promote diversity in universities by ensuring college access to high-achieving students from across Texas' somewhat segregated high schools. Then, in June 2003, the Supreme Court ruled in *Grutter v. Bollinger* that a race-conscious admissions process that does not amount to a quota system is constitutional. This Supreme Court decision repealed the ban on using race as a factor in the admissions process in Texas. Thus, Texas public universities were unable to use race explicitly in the admissions process prior to 2003 and were able to do so again after 2003.

We use this policy reversal to assess the effect of the introduction of race-based affirmative action on high school students' performance.[7] During our period of interest, there were no changes in the Top 10% Rule. However, the Top 10% Rule may affect the external validity of our results, since race-based affirmative action policy may interact with the clear admissions cutoffs under the Top 10% Rule. For instance, students in the top decile of their class may not respond to affirmative action since they are already guaranteed admission to Texas public universities. Nonetheless, while the Top 10% Rule may affect the external validity

[7]The first policy change in 1996 combines a ban on race-based affirmative action and the introduction of the Top 10% Rule, which is akin to a "race-blind" affirmative action policy in a setting with somewhat racially segregated high schools. Therefore, the 1996 policy change does not provide a clean experiment for estimating the effects of an affirmative action ban on student incentives.

of our results, we believe that this policy experiment is still of interest. First, Texas is a large state containing nearly 10% of the United States' population. From a welfare point of view, understanding the effects of Texas' affirmative action policies is important. Second, while the Top 10% Rule may affect our estimates at the top of the distribution, it is unlikely to affect the incentives of the median student. To the extent that our estimates are driven by the responses of students in the middle of the distribution (and preliminary quantile regressions suggest that this is indeed the case), our estimates are likely to be informative for other contexts.

On the day that the *Grutter v. Bollinger* decision was issued, UT Austin's president stated that the Texas flagship campus intended to return to considering race in the admissions process. Only the University of Texas Board of Regents could authorize the actual implementation of such a change and, in August 2003, the Board of Regents voted to allow all its campuses to return to considering race. The Texas Tech University Board of Regents also outlined a plan in October 2003 to include race as an element in admitting prospective students. Thus, from the onset of the 2003 Supreme Court ruling, it was clear that the state flagship university, UT Austin, and other public universities in Texas would begin to consider race in the admission process.

Due to the existence of the Top 10% Rule, Texas public universities first admit students who qualify for automatic admission. Students who are not eligible for automatic admission (i.e. are not in the top decile of their graduating class) are admitted based on a "holistic" review process which, after 2003, included consideration of race and ethnicity. While some portion of public university classes are admitted under the Top 10% Rule, the holistic admissions process still plays an important role in determining students' admission status. UT Austin, which has the highest percentage of freshmen admitted under the Top 10% Rule, admitted two-thirds of its entering freshmen class under automatic admission around 2003. The remainder of admitted freshmen were admitted through the holistic review process (Office of the President, 2008).

Figure 3.1 shows the trend in the racial composition of UT Austin's fall enrollment

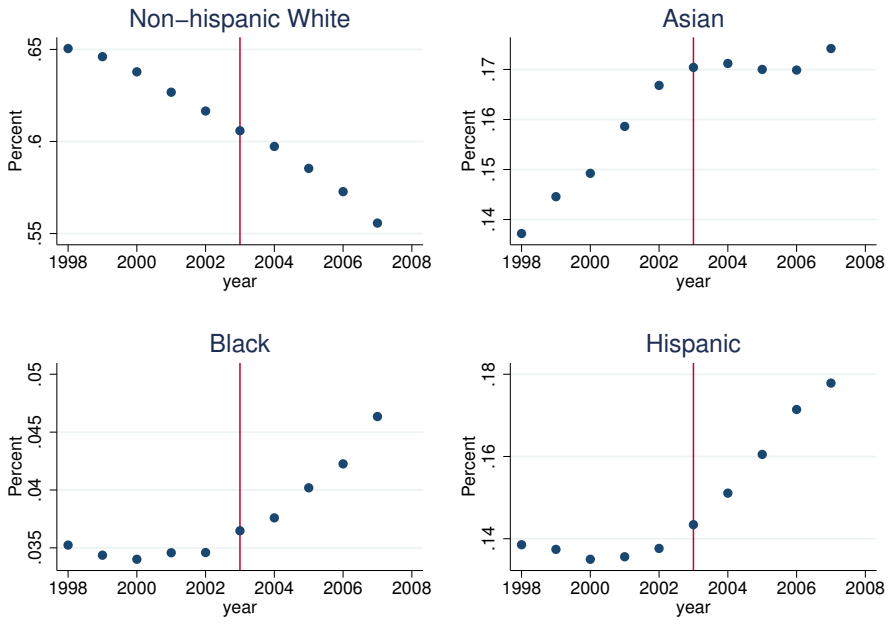


Figure 3.1: *Racial Composition of UT Austin*

around the 2003 policy change using data from the Integrated Postsecondary Education Data System (IPEDS). As this figure illustrates, the percentage of blacks and Hispanics in the UT Austin student body increased after 2003. This came at the cost of a decrease in the portion of white and Asian students.[8] Enrollment data from other UT campuses shows a similar pattern, although there is more noise when all the campuses are pooled together, possibly due to the demographic changes in Texas throughout this period.

Overall, the 2003 Supreme Court ruling reintroduced the use of race-based affirmative action in college admissions in Texas. Shortly after, universities expressed interest in considering race as one factor in the admissions process, and university enrollment figures show an increase in racial and ethnic diversity in the student body. Even if students were not directly aware of the court ruling, our conversations with administrators in Texas suggest that guidance counselors and school administrators were aware of the policy and did try to communicate this policy to their students.

[8]The raw number of students enrolled shows a similar pattern.

3.3 Data

In this section, we describe our three data sets: (1) the panel of race-by-state-by-year SAT scores, (2) the administrative data from a large urban school district, and (3) the survey data from the Texas Higher Opportunity Project (THEOP).

3.3.1 SAT Data

To analyze the effects of the re-instatement of affirmative action on SAT scores, we collected data on mean math and verbal SAT scores and number of test-takers at the state-by-race-by-year level from 1998 to 2010 from the College Board's publicly available reports. Following the college board's classifications, the racial groups are "American Indian," "Asian," "Black," "Hispanic," "Mexican American," "Puerto Rican," "White," and "Other." For the purposes of our study of affirmative action, we define American Indians, Blacks, Hispanics, Mexican Americans, and Puerto Ricans as minorities. Since Asians do not typically receive affirmative action, we do not classify them as minorities. Summary statistics of the SAT panel data are reported in Table 3.1. These summary statistics reveal a substantial racial achievement gap, with average math and verbal scores for whites of 513 and 505 respectively and for minorities of 444 and 443.

3.3.2 Administrative Data

Our administrative data comes from a large, urban school district in Texas. These data consist of repeated cross-sections of individual-level data for all 11^{th} graders in the school district between 1997 and 2010. The data contain information on students' demographics (race/ethnicity, gender, and zip code), standardized test scores, courses and course grades, attendance rates, and whether the student dropped out of school in the 11^{th} grade. For our standardized test results, we focus on the norm-referenced Stanford Achievement Test (hereafter, Stanford), a low-stakes achievement test that the school district has administered

Table 3.1: *Summary Statistics for the Panel of SAT Data*

Panel A: Summary Statistics

	Full Sample		Whites		Minorities	
	Mean	SD	Mean	SD	Mean	SD
Take SAT total	2,897.95	8,654.96	14,737.66	19,745.31	1,114.54	3,449.97
Verbal score total	505.15	39.58	528.09	19.31	443.48	23.24
Math score total	512.93	44.10	532.76	19.65	443.63	25.01

Panel B: Total Numbers

	N
All cells	5,943
Whites	663
Minorities	3,291

This table reports summary statistics for the panel of state-by-race-by-year SAT data assembled from the College Board reports. The racial groups consist of "American Indians," "Asians," "Blacks," "Hispanics," "Mexican Americans," "Puerto Ricans," "Whites," and "Others." The data span 1998-2010. Reported average test scores are weighted by the number of test-takers in the race-by-state-by-year cell. Averages over cells are weighted by the number of total test-takers in the cell.

since 2000.[9]

Summary statistics in Table 3.2 provide an overview of the students in our administrative data. The majority of students in our school district are black or of Hispanic descent: in a typical campus, 85% of students are black or Hispanic, and the remaining students are white and Asian. As the columns pertaining to "Entire Sample" in Table 3.2 show, students in our school district rank approximately in the 50^{th} percentile of the national distribution for the Stanford test and, on average, earn a 76 (out of 100) in their enrolled courses. Thirty seven percent of students are enrolled in at least one Advanced Placement (AP), Pre-AP, or honors course and 3% of students drop out of high school in the 11^{th} grade. The attendance rate indicates that students are present for 90% of the days they are enrolled in school.

These aggregate measures of performance mask the racial achievement gap. As shown in the remaining columns of Table 3.2, black and Hispanic students have lower achievement than white students along all dimensions. Black and Hispanic students score significantly

[9]We focus on the Stanford test rather than the Texas-wide standardized tests because the Texas-wide tests changed from TAAS to TAKS in 2003, and more importantly, the sample of 11^{th} graders who took the exam changed. Prior to 2003, only 11^{th} graders who had previously failed the TAAS were required to take the exam (exit-level). After 2003, all 11^{th} graders were required to take the TAKS.

Table 3.2: *Summary Statistics for Administrative Data*

Panel A: Summary Statistics

	Entire Sample		Whites		Blacks		Hispanics		Asians	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Campus Minority Share	0.85	0.20								
Stanford Percentile	48.27	26.38	71.58	25.07	42.00	23.62	43.97	23.52	70.80	26.02
Course Grades	75.87	12.32	80.78	10.72	74.47	12.09	74.88	12.42	82.94	10.19
Prob. Advanced Courses	0.37	0.48	0.63	0.48	0.32	0.47	0.30	0.46	0.75	0.43
Prob. Dropout	0.03	0.17	0.01	0.12	0.03	0.18	0.04	0.18	0.01	0.10
Attendance Rate	0.90	0.13	0.93	0.11	0.90	0.14	0.90	0.14	0.95	0.10

Panel B: Total Numbers

	N
Total Campuses	81
All Students	153,008
Whites	20,703
Blacks	51,247
Hispanics	74,604

This table presents summary statistics by race for the key variables of interest in our administrative data from a large, urban school district in Texas. The dataset spans 1997 to 2010 and consists of repeated cross-sections of 11^{th} graders. Students were coded as taking an advanced course if they enrolled in at least 1 honors, advanced, or advanced placement (AP) course. Dropout is measured as dropout in the 11^{th} grade, so dropout levels in this data will be lower than those implied by final high school graduation rates.

lower on the Stanford standardized test in terms of national percentile ranking compared to white students, have lower grades in their courses, are less likely to be enrolled in advanced courses, and are more than twice as likely as white students to drop out of high school in the 11^{th} grade. Because black and Hispanic students have similar educational attainment and affirmative action in college admissions applies similarly to both groups, we pool black and Hispanic students together as "minority students" and compare these minority students to whites in our analysis of the Texas data.[10]

3.3.3 Texas Higher Education Opportunity Project Data

We complement our administrative and SAT data with survey data from the Texas Higher Education Opportunity Project (THEOP). THEOP surveyed high school seniors from a random sample of 105 public high schools in Texas in 2002 and in 2004 regarding their

[10]Results are similar if we estimate coefficients for black and Hispanic students separately.

Table 3.3: *Summary Statistics for THEOP Survey Data*

Panel A: Summary Statistics						
	Full Sample		Whites		Minorities	
	Mean	SD	Mean	SD	Mean	SD
Time (minutes) Spent on Homework	64.54	56.69	56.06	53.60	70.56	56.26
Applied to First Choice College	0.65	0.48	0.70	0.46	0.60	0.49
Parental Involvement Index (0-15)	5.98	3.87	5.94	3.78	6.18	3.96
Discussed College App. w. Counselor	0.67	0.47	0.65	0.48	0.70	0.46

Panel B: Total Numbers	
	N
Total Students	13,938
Whites	6,406
Minorities	7,532
Students in 2002	11,098
Students in 2004	2,840

Table 3.4: *Summary Statistics for THEOP Survey Data*

This table presents summary statistics for the Texas Higher Education Opportunity Project (THEOP) survey data for two cohorts of seniors, one in 2002 and one in 2004. For the measure of how many minutes per day students spend on homework, students were asked how many hours per day they spent on their homework and were given the options zero heros, less than 1 hour, 1 to 2 hours, 3 to 4 hours, and 5+ hours. We convert these to minutes so that 0 hours is 0 minutes, less than 1 hour is 30 minutes, 1 to 2 hours is 90 minutes, and so on. The parental involvement index is also constructed using several questions that ask "How often do your parents ... (i) give you special privileges because of good grades, (ii) try to make you work harder if you get bad grades, (iii) know when you are having difficulty in school, (iv) help with your school work, and (v) talk with you about problems in school." Students' responses range from "very rarely" (1) to "almost all the time" (4). We sum across the answers to these questions to construct the "parental involvement index" in a way that a higher index corresponds to more involvement along these dimensions.

demographics, college perceptions, parental involvement, and other activities in high school. Unfortunately, the two waves of the survey are not identical: for instance, the first wave asks about student-teacher interactions, while the second wave does not. The set of questions that are consistent across the two waves allow us to compare the following outcomes, relevant to this study, for Texas seniors one year before and one year after the implementation of affirmative action: time spent on homework outside of school (in minutes), whether the student applied to their first choice college, a series of questions about parental behavior which we combine to construct a "parental involvement index" ranging from 5 to 20,[11] and whether the student discussed the college application process with his/her guidance counselor. Table 3.4 reports summary statistics for these data. The timing of the survey allows us to compare high school seniors right after the 2003 court ruling to high school seniors right before. We use this survey to provide suggestive evidence on students' and parents' response to affirmative action policy with the caveats that this survey only exists for two time periods and the sample size in 2004 is small.

3.3.4 Future Data Sources

In addition to the administrative data from the school district and the THEOP survey data, we have recently gained access to individual-level administrative records on all Texas high school students from the Texas Education Agency. This data is linked to (in-state) college administrative data, as well as unemployment records and wage data. This comprehensive dataset will allow us to analyze and trace the effect of affirmative action on *all* Texas students during high school, throughout the college application process, in college, and later on in the workforce. Results using this extensive dataset are currently in preparation.

[11] More precisely, we use a series of questions that ask, "How often do your parents ... (i) give you special privileges because of good grades, (ii) try to make you work harder if you get bad grades, (iii) know when you are having difficulty in school, (iv) help with your school work, and (v) talk with you about problems in school." Students' responses range from "very rarely" (1) to "almost all the time" (4). We sum across the answers to these questions to construct the "parental involvement index" in a way that a higher index corresponds to more involvement along these dimensions.

3.4 Effects of Affirmative Action on Students' Outcomes

In this section, we report our estimates of the effect of the re-instatement of affirmative action in our three different data sets. We first report the effect of affirmative action on minorities' SAT scores in difference-in-differences, as well as triple differences regressions that treat whites as a control group. We then use the administrative data from a large, urban Texas school district to estimate the effects of the re-instatement on the within-school-year racial achievement gap on standardized tests, course grades, advanced course enrollment, and drop out. Finally, we use the Texas Higher Education Opportunity Project Data to estimate the differential effect of affirmative action on minorities' responses to questions about effort (time spent on homework), college application behavior, and parental and guidance counselor involvement.

3.4.1 National Results

Empirical Strategy. To assess the effects of affirmative action on the average minority and non-minority students' SAT scores, we use our panel of SAT data to estimate the following difference-in-differences regression separately for non-Asian minorities, whites, and Asians:

$$y_{krt} = \beta_0 + \beta_1 I(Treated\_State_k) \times I(Post2003_t) + \alpha_k + \alpha_t + \alpha_r + \varepsilon_{krt}. \tag{3.1}$$

where k indexes a state, t indexes a year, and r indexes a racial group. Then, y_{krt} is either the mean test score or the log number of test-takers for group r in state k and year t, $I(Treated\_State_k)$ is an indicator variable equal to 1 if k is a treated state (Texas, Louisiana, and Mississippi), $I(Post2003_t)$ is an indicator variable equal to 1 if the year is greater than 2003, α_t is a year fixed effect, α_k is a state fixed effect, and α_r is a race fixed effect. We present results separately for Asians as well since they are a racial minority but were not affected by the change in affirmative action policies. When our outcome is test-scores, we weight the race-by-state-by-year cells by the number of test-takers. In all cases, we cluster our standard errors at the state-level.

In addition to these difference-in-differences results, we estimate the following triple-

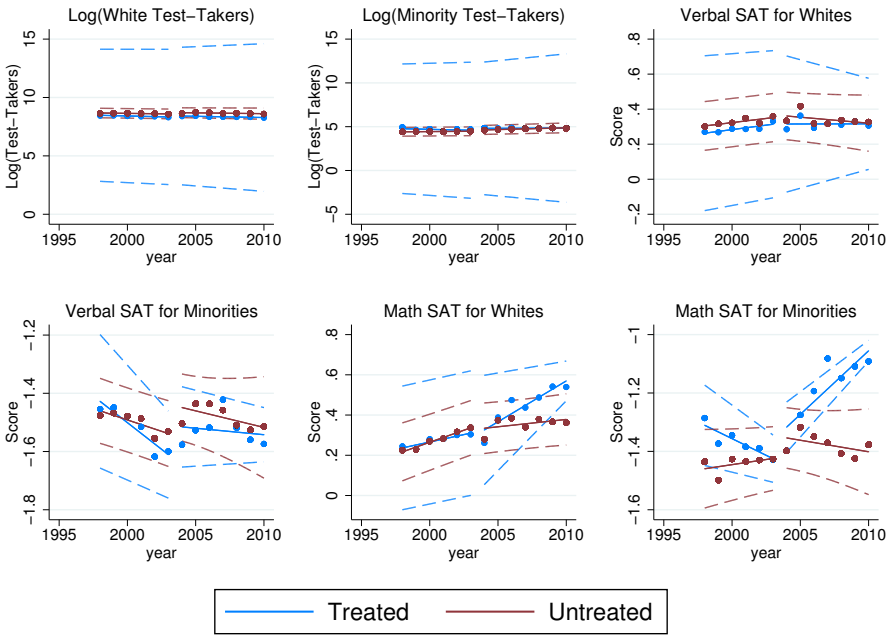


Figure 3.2: *Trends in SAT*

differences specification on the full sample

$$y_{krt} = \beta_0 + \beta_1 I(Treated\_State_k) \times I(Post2003_t) \times I(Minority_r) + \alpha_{kr} + \alpha_{rt} + \alpha_{kt} + \varepsilon_{krt}. \quad (3.2)$$

Here, $I(Minority_r)$ is an indicator variable equal to 1 if r belongs to a minority group, α_{kr} is a state-by-race fixed effect, α_{rt} is a race-by-year fixed effect, and α_{kt} is a state-by-year fixed effect. As before, we also run a version of this specification replacing $I(Minority_r)$ with an indicator variable for being Asian as a placebo test.

Results. Figure 3.2 provides the graphical analogues to the difference-in-differences regression results in Table 3.5, graphing average log number of test takers and test scores for minorities in treated and untreated states over time. As the figure shows, trends in the log number of minority and white test-takers are flat over the study period, indicating that selection into taking the SAT is unlikely to bias estimates of changes in test scores. Similarly, the panels of the figure in which the verbal SAT score is the outcome suggest that these scores did not change substantially for either whites or minorities in treated states relative

to untreated states after 2003. However, the final two panels suggest that affirmative action indeed had an effect on minority students' outcomes relative to whites. In the last panel, a *negative trend* in SAT scores for minority students is reversed after 2003, and math SAT scores for minorities in treated states grew and outpace those in untreated states following 2003.

Table 3.5 reports the coefficients from equation 3.1 (Columns (1)-(8)) and equation 3.2 (Columns (9)-(12)). The coefficient estimates are consistent with Figure 3.2. Columns (1) and (2) show that there was no change in the number of white or minority test-takers in treated states following 2003. Columns (3) and (4) show that math scores for both minorities and whites improved in treated states following 2003, but minorities' test scores improved by almost twice as much. This may be because whites' effort was also positively affected by the ban, as would be the case if whites' increased their effort in response to intensifying competition. In contrast, there is no effect on test scores for Asians (Column (5)), and verbal SAT scores did not change for whites, minorities, or Asians (Columns (6)-(8)). This is what we expect if the results are driven by the change in affirmative action policies since Asians do not benefit from affirmative action.

In the last three columns, we report the results of the triple-differences regressions for the log number of test-takers, math, and verbal SAT scores. We find that the number of test takers and verbal scores did not increase for minorities relative to whites in the treated states after 2003, but minorities' SAT math scores improved relative to whites in treated states by 0.07sd ($p < 0.01$). These results suggest that the reinstatement of affirmative action helped close the racial achievement gap in treated states. Having found this effect in aggregate SAT data, we next test whether this effect replicates in individual-level administrative data from a large school district in Texas.

3.4.2 Texas-Specific Results

Empirical Strategy. We use a differences-in-differences empirical strategy to identify changes in the achievement gap between minority and white high school students due to affirmative

Table 3.5: *Affirmative Action and SAT Outcomes*

	(1) Log(SAT Takers)	(2) Log(SAT Takers)	(3) Math	(4) Math	(5) Math	(6) Verbal	(7) Verbal	(8) Verbal	(9) Log(SAT Takers)	(10) Math	(11) Verbal
$I(Treated\_State_k) \times I(Post2003_t)$	-0.081	-0.108	0.079***	0.144***	0.013	0.001	-0.023	-0.003			
	(0.099)	(0.146)	(0.020)	(0.028)	(0.036)	(0.020)	(0.037)	(0.061)			
$I(Treated\_State_k) \times I(Post2003_t) \times I(Minority_r)$									-0.017	0.073***	0.023
									(0.089)	(0.019)	(0.020)
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State × Race FE	N	N	N	N	N	N	N	N	Y	Y	Y
State × Year FE	N	N	N	N	N	N	N	N	Y	Y	Y
Race × Year FE	N	N	N	N	N	N	N	N	Y	Y	Y
Sample	Whites	Minorities	Whites	Minorities	Asians	Whites	Minorities	Asians	All	All	All
N	663	3,291	663	3,090	663	663	3,080	663	3,954	3,753	3,743
Clusters	51	51	51	51	51	51	51	51	51	51	51
Adjusted R^2	0.990	0.857	0.965	0.825	0.938	0.967	0.778	0.921	0.992	0.997	0.997

This table reports estimates of the effect of the reinstatement of affirmative action on minority students' outcomes in difference-in-differences and triple-differences regressions. The outcomes of interest are log number of SAT test-takers and math and verbal SAT scores. The data spans 1998-2010, and an observation is at the race-by-year-by-state level. Test score regressions are weighted by the number of test-takers. Standard errors are clustered at the state level.

action in the administrative data. We compare the within-school achievement gap between minority and white students after the reintroduction of affirmative action in 2003 to the achievement gap before 2003. In our main specification, we estimate the following regression for student i, in school s, at time t using administrative data from our school district:

$$y_{ist} = \beta_0 + \beta_1 I(Minority_i) + \beta_2 I(Minority_i) \times I(Post2003_t) + \alpha_{st} + \varepsilon_{ist}, \tag{3.3}$$

where y_{ist} is student outcomes in high school in terms of standardized test scores, course grades, and course selection in the 11^{th} grade, $I(Minority_i)$ is an indicator variable equal to 1 if a student is black or Hispanic and 0 if the student is white, $I(Post2003_t)$ is an indicator variable equal to 1 if a student is observed after 2003 and 0 otherwise, and α_{st} are campus-year fixed effects.[12] Standard errors are clustered at the campus-year level. We include α_{st} to account for campus-year specific shocks that could result in changes in the racial achievement gap narrowing independently of affirmative action policy.[13] Accounting for variation at the school-level also accounts for course offerings, grading procedures, the quality of guidance counselors, and other factors that determine educational attainment and are determined at the campus level rather than the district level.

Results. Table 3.6 reports the estimates from equation 3.3. Column 1 shows a significant gap between minority and white students in the within-school Stanford score in terms of standard deviations: minorities score .81 standard devations lower on the Stanford test than white students in the same school. After the reinstatement of affirmative action in 2003, the racial achievement gap in Stanford narrows by .17 standard devations ($p < 0.01$), or 21% of the within-school gap. Column 2 shows a similar pattern for course grades: the racial achievement gap in grades improves by .07 standard devations ($p < 0.01$) or 19% of

[12]We do not include a $I(Post2003_t)$ indicator in the regression as it is subsumed by the campus-year fixed effects.

[13]For instance, if schools that are predominantly comprised of minority students are improving over time (perhaps due to school accountability policies), then we would observe a reduction in the achievement gap between minority and white high school students over time that cannot be attributed to affirmative action policy. With the inclusion of campus-by-year fixed effects, we account for this by comparing minority and white students *within the same school*.

the within-school gap. The improvement in grades does not come at the cost of "taking easier courses:" column 3 shows that minority students are 4 percentage points ($p < 0.05$) more likely to enroll in at least one advanced course (honors, Pre-AP, or AP course) after affirmative action policy is reinstated compared to whites.

Table 3.6: *Affirmative Action and Minority-white Achievement Gap*

	(1) Stanford Test	(2) Course Grades	(3) Advanced Course	(4) Dropout
I(Minority)	-0.812***	-0.369***	-0.289***	0.009***
	(0.033)	(0.018)	(0.011)	(0.002)
I(Minority)×I(Post2003)	0.173***	0.073***	0.044**	-0.003
	(0.042)	(0.025)	(0.017)	(0.003)
Campus-year FE	Y	Y	Y	Y
Mean Whites Pre-2003	1.023	0.450	0.582	0.011
N	91,578	118,270	118,386	146,554
Clusters	475	573	573.000	653
Adjusted R^2	0.356	0.140	0.167	0.078

This table presents difference-in-difference estimates of the effect of being a minority student post 2003 in the Texas administrative data. The dataset consists of repeated cross-sections of 11^{th} graders from 1997 to 2010. Asians are excluded from the regression. All regressions include campus-by-year fixed effects. Standard errors are clustered at the campus-year level.

Overall, Table 3.6 shows that on multiple dimensions (standardized test scores, grades, and the difficulty of courses), minority high school students' performance improves relative to whites' after the implementation of affirmative action policy in a statistically significant and economically meaningful way. Since our identification strategy relies on comparing the achievement of minorities and non-minorities over time, we are concerned that a general improvement of minorities' outcomes over time could bias our results. We offer three pieces of evidence that a general improvement of minorities' outcomes over time is not driving our results. First, we consider whether the dropout rate changes for minorities relative to whites after 2003. Since students who drop out are unlikely to be on the margin of attending 4-year universities, it may be indicative of other underlying time trends if the dropout rate also improves along with grades and test scores. As column 4 of Table 3.6 shows, we do not find

that the dropout rate of minority students changes relative to whites after 2003. Although minority students are approximately twice as likely as white students in the same school to drop out in the 11^{th} grade before 2003, there is no significant change in this relative dropout rate after affirmative action is put in place. Therefore we observe an effect of affirmative action on outcomes that matter for college admissions, such as test scores and courses, but not for outcomes where affirmative action is much less relevant, such as the dropout rate.

Additionally, to rule out general changes in the achievement gap over time that may bias our results, we conduct two placebo tests. In the first placebo test, we assign the policy change to earlier years, controlling for the true policy effect. If our results are biased by pre-trends, we should see significant effects on outcomes before the policy was reinstated. In our second test, we compare the outcomes of Asians to whites after 2003. Since Asians do not benefit from affirmative action, if our natural experiment is valid, we do not expect $I(Asian_i) \times I(Post2003_t)$ to have a significant coefficient. We discuss these results below.

Placebo Policies. To detect any pre-trends in minority students' outcomes, we assign years prior to 2003 to be "placebo" cutoffs and estimate the effect of these placebo cutoffs controlling for the effect of the true policy change. More formally, we estimate

$$y_{ist} = \beta_0 + \beta_1 I(Minority_i) + \beta_2 I(Minority_i) \times I(Post2003_t) + \beta_3 I(Minority_i) \times I(PostPlaceboYear_t) + \alpha_{st} + \varepsilon_{ist}, \quad (3.4)$$

where $I(PostPlaceboYear_t)$ indicates whether a student is observed after the placebo cutoff. We can vary the placebo cutoff to be any year from 2000 to 2002 for the Stanford test[14] and from 1997 to 2002 for grades and courses. If the changes in the racial achievement gap began earlier than 2003, then we should observe a positive and significant β_3 coefficient. The results for Stanford test appear in Table 3.7, for course grades in Table 3.8, and for selection of courses in Table 3.9. Column 1 in Table 3.7 replicates our main specification for the effect of the 2003 policy change on the Stanford test. In each subsequent column, we add a different $I(PostPlaceboYear_t)$ variable, starting with 2000. None of the placebo

[14]The Stanford began being administered in our school district in 2000.

interactions prior to 2003 are significant, and they are typically small in magnitude relative to the estimates for the true policy change. Similarly, column 1 in Table 3.8 replicates our main specification for the effect of affirmative action policy in 2003 on the racial gap in course grades. Again, there is no evidence that the change in the racial achievement gap began prior to the year of the policy change. In terms of course selection, there is also no evidence of pre-trends as shown in Table 3.9. These results suggest that our estimates of the effect of affirmative action are not driven by pre-trends in minority students' outcomes.

Table 3.7: *Test for Pre-trends in Stanford Test Scores*

	(1) Stanford Test	(2) Stanford Test	(3) Stanford Test	(4) Stanford Test
I(Minority)×I(Post2003)	0.173***	0.163***	0.123**	0.135**
	(0.042)	(0.046)	(0.048)	(0.061)
I(Minority)×I(Post2000)		0.039		
		(0.076)		
I(Minority)×I(Post2001)			0.103	
			(0.063)	
I(Minority)×I(Post2002)				0.050
				(0.068)
Campus-year FE	Y	Y	Y	Y
N	91,578	91,578	91,578	91,578
Clusters	475	475	475	475
Adjusted R^2	0.356	0.356	0.356	0.356

This table presents tests for pre-trends in Stanford scores which may bias the estimates of the effect of affirmative action. In addition to the difference-in-difference specification (column 1), we assign placebo policy changes to 2000 (column 2), 2001 (column 3), and 2002 (column 4). The dataset consists of repeated cross-sections of 11^{th} graders from 2000 to 2010. Asians are excluded from the regression. All regressions include campus-by-year fixed effects. Standard errors are clustered at the campus-year level.

Placebo Minorities. We now investigate whether Asian students improved relative to white students after 2003. We re-estimate equation 3.3, but we replace minorities with Asians. The sample now only contains whites and Asians. Since Asians do not benefit from affirmative action, we do not expect their outcomes to improve (or decline) relative to whites after 2003. As Table 3.10 shows, although Asian-American students generally outperform white students, this gap does not change after the implementation of affirmative action

Table 3.8: *Test for Pre-trends in Course Grades*

	(1) Course Grades	(2) Course Grades	(3) Course Grades	(4) Course Grades	(5) Course Grades	(6) Course Grades	(7) Course Grades
I(Minority)×I(Post2003)	0.073***	0.069***	0.072***	0.074***	0.055*	0.043	0.052
	(0.025)	(0.026)	(0.027)	(0.029)	(0.029)	(0.032)	(0.042)
I(Minority)×I(Post1997)		0.028					
		(0.053)					
I(Minority)×I(Post1998)			0.003				
			(0.042)				
I(Minority)×I(Post1999)				-0.003			
				(0.036)			
I(Minority)×I(Post2000)					0.031		
					(0.034)		
I(Minority)×I(Post2001)						0.042	
						(0.034)	
I(Minority)×I(Post2002)							0.024
							(0.042)
Campus-year FE	Y	Y	Y	Y	Y	Y	Y
N	118,270	118,270	118,270	118,270	118,270	118,270	118,270
Clusters	573	573	573	573	573	573	573
Adjusted R^2	0.140	0.140	0.140	0.140	0.140	0.140	0.140

This table presents tests for pre-trends in grades which may bias the estimates of the effect of affirmative action. In addition to the difference-in-difference specification (column 1), we assign placebo policy changes to 1997 (column 2), 1998 (column 3), and 1999 (column 4), 2000 (column 5), 2001 (column 6), and 2002 (column 7). The dataset consists of repeated cross-sections of 11^{th} graders from 1997 to 2010. Asians are excluded from the regression. All regressions include campus-by-year fixed effects. Standard errors are clustered at the campus-year level.

Table 3.9: *Test for Pre-trends in Course Enrollment*

	(1) Advanced Course	(2) Advanced Course	(3) Advanced Course	(4) Advanced Course	(5) Advanced Course	(6) Advanced Course	(7) Advanced Course
I(Minority)×I(Post2003)	0.044**	0.041**	0.040**	0.043**	0.027	0.020	0.035
	(0.017)	(0.018)	(0.019)	(0.020)	(0.020)	(0.022)	(0.028)
I(Minority)×I(Post1997)		0.020					
		(0.036)					
I(Minority)×I(Post1998)			0.016				
			(0.025)				
I(Minority)×I(Post1999)				0.003			
				(0.023)			
I(Minority)×I(Post2000)					0.031		
					(0.022)		
I(Minority)×I(Post2001)						0.034	
						(0.022)	
I(Minority)×I(Post2002)							0.011
							(0.027)
Campus-year FE	Y	Y	Y	Y	Y	Y	Y
N	118,386	118,386	118,386	118,386	118,386	118,386	118,386
Clusters	573	573	573	573	573	573	573
Adjusted R^2	0.167	0.167	0.167	0.167	0.167	0.167	0.167

This table presents tests for pre-trends in enrolling in advanced courses which may bias the estimates of the effect of affirmative action. The outcome is an indicator variable for enrolling in at least one honors, advanced, or advanced placement course. In addition to the difference-in-difference specification (column 1), we assign placebo policy changes to 1997 (column 2), 1998 (column 3), and 1999 (column 4), 2000 (column 5), 2001 (column 6), and 2002 (column 7). The dataset consists of repeated cross-sections of 11^{th} graders from 1997 to 2010. Asians are excluded from the regression. All regressions include campus-by-year fixed effects. Standard errors are clustered at the campus-year level.

policy in 2003. This result is consistent with the idea that affirmative action should only have an effect on students for whom affirmative action policy applies. Even if affirmative action policy had an impact on white and Asian high school students, its effect should not be differential as these two groups are treated the same for purposes of affirmative action.[15] This is confirmed in the data.

Table 3.10: *Affirmative Action and Asian-white Achievement Gap*

	(1) Stanford Test	(2) Course Grades	(3) Advanced Course	(4) Dropout
I(Asian)	-0.059	0.137***	0.106***	-0.002
	(0.065)	(0.024)	(0.026)	(0.002)
I(Asian)×I(Post2003)	0.071	-0.007	0.021	-0.007**
	(0.075)	(0.030)	(0.034)	(0.003)
Campus-year FE	Y	Y	Y	Y
Mean Whites Pre-2003	1.023	0.450	0.582	0.011
N	18,029	23,344	23,334	27,049
Clusters	391	461	461	556
Adjusted R^2	0.267	0.151	0.125	0.108

This table replicates the analyses in table 3.6, but replaces the indicator variable for minority (black or Hispanic status) with an indicator variable for Asian. The dataset consists of repeated cross-sections of 11^{th} graders from 1997 to 2010. Minorities are excluded from the regression. All regressions include campus-by-year fixed effects. Standard errors are clustered at the campus-year level.

3.4.3 Suggestive Evidence on Mechanisms

So far, we have shown that the achievement gap between minority and white students in terms of test scores, course grades, and course selection narrowed after the introduction of affirmative action policy in 2003. How did this reduction come about? Perhaps high school students changed their behavior or effort. Alternatively, teachers may have become more lenient toward minorities after the policy change or teachers may have focused more on improving minority students' outcomes. The relative improvement in the standardized test

[15] Kane (1998) has shown that racial preferences in admissions are given only at the most elite 20% of colleges and universities and, even at these colleges, the impact of racial preferences on the typical white (or Asian) applicant's admission probability is small.

scores is unlikely to be explained by teachers grading minorities more leniently, but this does not rule out the possibility that they focused more attention on improving minorities' learning. Similarly, the change in affirmative action policy may have led parents or guidance counselors to become more involved with students. To determine what drives minority students' improved outcomes, we analyze students' responses from the THEOP survey.

As mentioned previously, the THEOP survey asked high school seniors across Texas about their demographics, college application behavior, and high school activities in 2002 and then again in 2004. Unfortunately, the two waves of the survey are not identical. The set of questions that are consistent across the two waves allow us to compare the following outcomes for Texas seniors one year before and one year after the implementation of affirmative action: time spent on homework outside of school (in minutes), whether the student applied to his/her first choice college, a series of questions about parental behavior which we combine to construct a "parental involvement index," and whether the student discussed the college application process with his/her guidance counselor. For each of these outcomes, we run the following regression:[16]

$$y_{it} = \beta_0 + \beta_1 I(Minority_i) + \beta_2 I(Post2003_t) + \beta_3 I(Minority_i) \times I(Post2003_t) + \varepsilon_{it}, \quad (3.5)$$

where $I(Post2003_t)$ is an indicator equal to 1 for seniors surveyed in 2004. Table 3.11 shows these results. As column 1 shows, after the implementation of affirmative action, minority high school seniors spend 8% more time on homework outside of school relative to white students (a relative increase of approximately 5 minutes per day). Minority students are also 5 percentage points more likely to apply to their first choice college after the policy change compared to whites. We do not see any changes in the parental involvement index or the likelihood of discussing college applications with guidance counselors for minorities relative to white students after affirmative action is put in place. Overall Table 3.11 provides suggestive evidence that student behavior (such as time spent on homework) and college

[16]In this analysis, we cannot include campus fixed effects because we do not know the campus the student belongs to.

aspirations did respond to the introduction of affirmative action policy.

Table 3.11: *Affirmative Action and Student and Parent Behavior*

	(1) Time on Homework	(2) Applied to First Choice College	(3) Parental Involvement	(4) Guidance From Counselor
I(Minority)	12.446***	-0.107***	0.122*	0.047***
	(1.016)	(0.011)	(0.073)	(0.009)
I(Post2003)	26.070***	-0.145***	1.759***	0.191***
	(1.912)	(0.017)	(0.128)	(0.014)
I(Minority)×I(Post2003)	5.439**	0.047**	0.172	-0.025
	(2.496)	(0.023)	(0.166)	(0.018)
Mean Whites Pre-2003	51.585	0.732	10.635	0.614
N	13,452	9,993	13,558	13,699
Adjusted R^2	0.061	0.024	0.038	0.026

This table presents differences-in-differences analyses using survey data from two cohorts, both in their senior year, of the Texas Higher Education Opportunity Project (THEOP). The earlier cohort was surveyed in 2002 and the later cohort was surveyed in 2004. For the measure of how many minutes per day students spend on homework, students were asked how many hours per day they spent on their homework and were given the options zero heros, less than 1 hour, 1 to 2 hours, 3 to 4 hours, and 5+ hours. We convert these to minutes so that 0 hours is 0 minutes, less than 1 hour is 30 minutes, 1 to 2 hours is 90 minutes, and so on. The parental involvement index is also constructed using several questions that ask "How often do your parents ... (i) give you special privileges because of good grades, (ii) try to make you work harder if you get bad grades, (iii) know when you are having difficulty in school, (iv) help with your school work, and (v) talk with you about problems in school." Students' responses range from "very rarely" (1) to "almost all the time" (4). We sum across the answers to these questions to construct the "parental involvement index" in a way that a higher index corresponds to more involvement along these dimensions. Standard errors are heteroskedasticity robust.

3.5 Conclusion and Next Steps

In this paper, we study the effects of a 2003 U.S. Supreme Court ruling that effectively reinstated race-based affirmative action policies in public universities in Texas, Louisiana, and Mississippi. We find that the policy reduced the racial achievement gap on math SAT scores in the treated states. Additionally, comparing minority (black and Hispanic) and white students in the same schools in a large, urban school district in Texas, we find that this reinstatement substantially reduced the racial gap in standardized test scores, grades, and likelihood of enrolling in at least one advanced course. Our results are consistent with experimental work by Cotton *et al.* (2015) and the structural estimates of Hickman (2013), both of which find that affirmative action incentivizes greater human capital investment by minority high school students. We complement these findings by studying the effects of a real policy change that targeted students based on race. In addition, our large effect sizes suggest that policy debates that ignore the pre-college incentive effects of affirmative action policies ignore a significant benefit of these policies. Given the role the racial

achievement gap may play in determining gaps in long-term outcomes (Neal and Johnson, 1996), reductions in the achievement gap may translate into substantial reductions in the wage gap.

Using survey data, we examine how students' behavior, in addition to their outcomes, respond to the affirmative action policy. We find that minority students spend more time on their homework and are more likely to apply to their first choice college after the policy change. This is consistent with the idea that minority students respond to the affirmative action policy by changing their college aspirations and adjust their effort accordingly. We also speculate that these results are consistent with work by Hoxby and Avery (2012) and Hoxby and Turner (2013), which show that qualified, disadvantaged students are less likely to apply to highly selective four-year institutions. If affirmative action leads minority students to perceive admission to a selective school as more attainable, it may change both their application behavior and their pre-college human capital investment.

This paper presents our preliminary results using SAT data and data from one large, urban Texas school district. In the future, we will expand on these results. Using data from the Texas Education Agency, we will be able to observe students' outcomes in college and in the labor market. Thus, we will be able to see if affirmative action policy actually did affect minority students' likelihood of college admission and which students were most likely to be affected (and test if these are the students who respond the most to the policy change). Moreover, we can see how these students fared once they matriculated to college and entered the labor force.

Second, using lagged student outcomes from before affirmative action was reinstated, we will be able to see which part of the student distribution was most affected by the policy change. Since some commentators have argued that affirmative action only benefits already-advantaged minorities, this will provide us with important evidence on the distribution of the benefits of the policy. Additionally, estimating heterogeneous effects by location in the achievement distribution will allow us to test if *some* students at the top of the distribution decrease their effort, as some models predict (Cotton *et al.*, 2015). Finally, these

heterogeneous effect estimates will serve as an additional robustness test for our main results since we expect affirmative action policies to affect the effort of students who would benefit the most from such policies.

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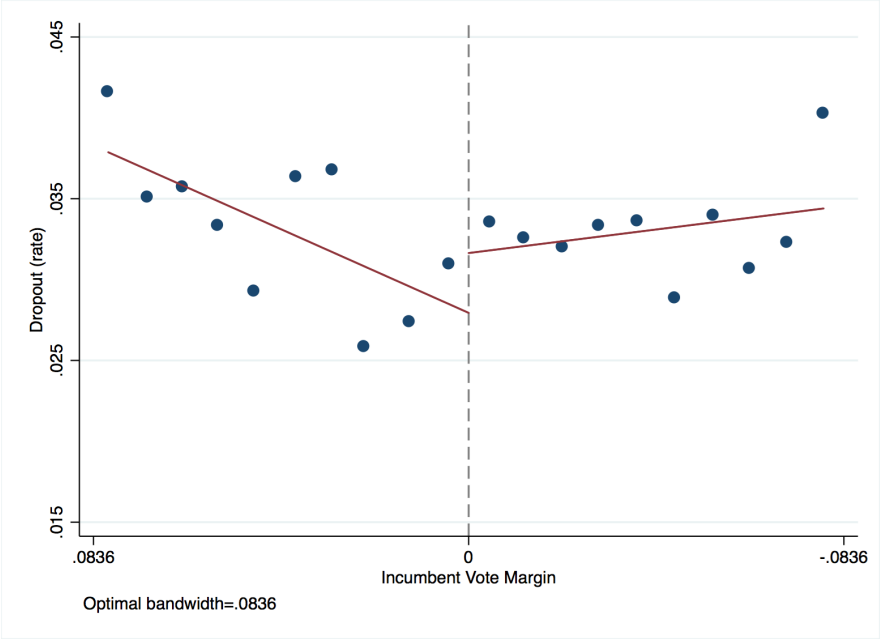
Appendix A

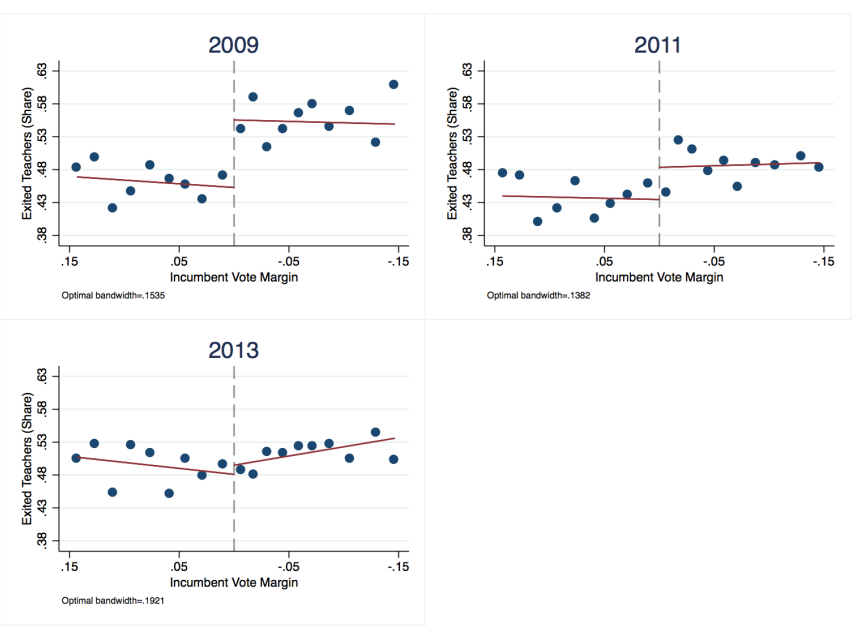
Appendix to Chapter 1

Figure A.1: *Political Turnover and School-level Dropout Rates*



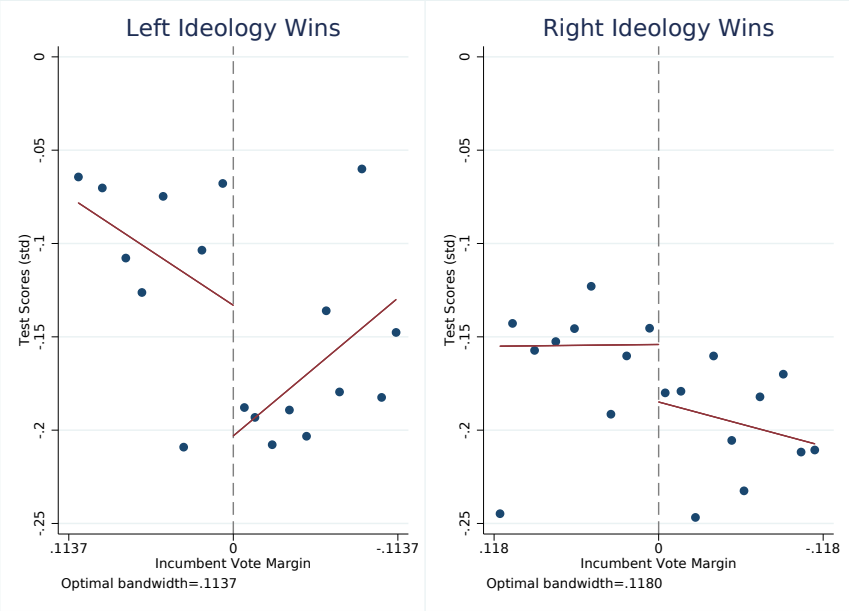
Notes: This figure shows the mean of school-level dropout rates by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did not experience a change 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.2: *Political Turnover in 2008 and Teachers that have Left 1, 3, and 5 Years After the Election*



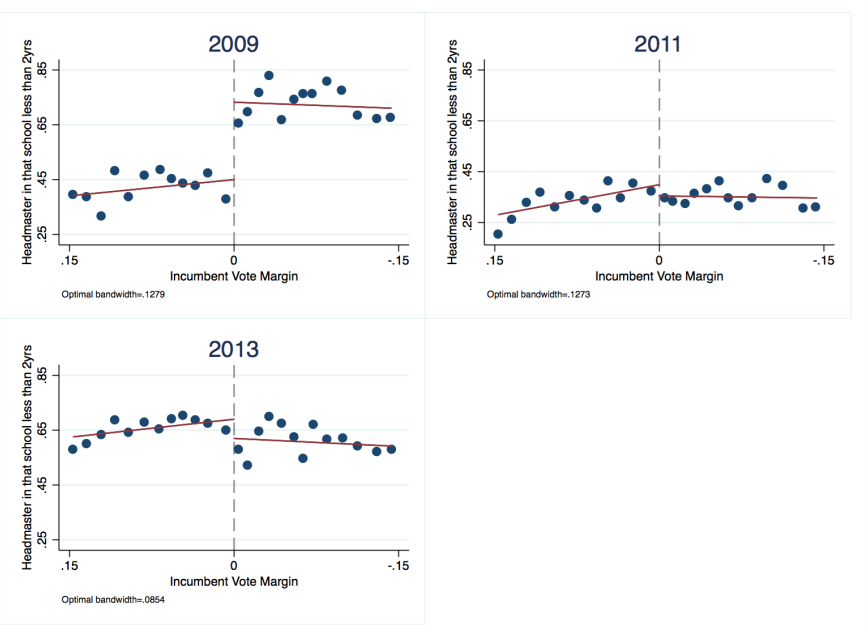
Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for each year t, where t is one year, three years, and five years after the 2008 election. Municipalities with IncumbVoteMargin<0 experienced a 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.3: *Political Turnover and 8^{th} Grade Test Scores in Municipalities where the Winning Party was from the Left vs. the Right*



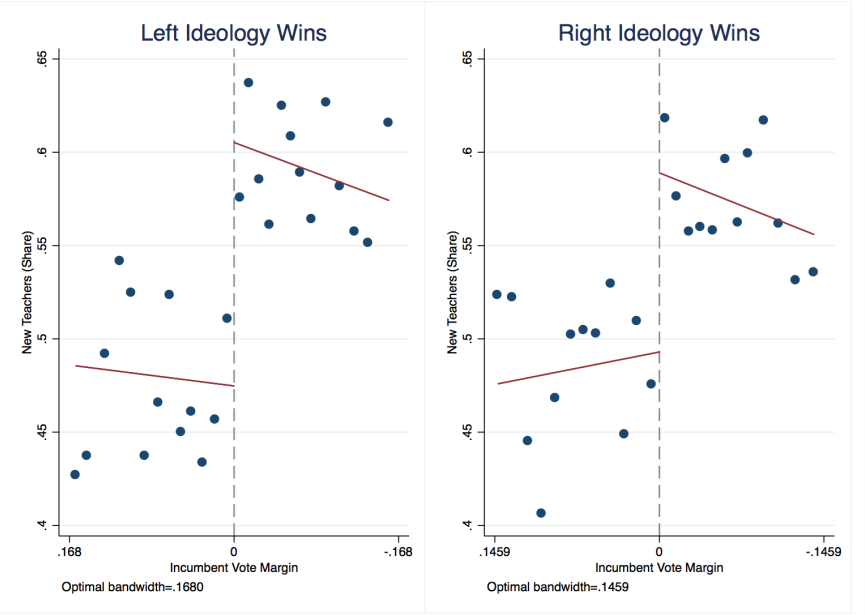
Notes: This figure shows the mean of individual-level 8^{th} grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with 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 8^{th} 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.4: *Political Turnover in 2008 and Headmaster Replacements 1, 3, and 5 Years After the Election*



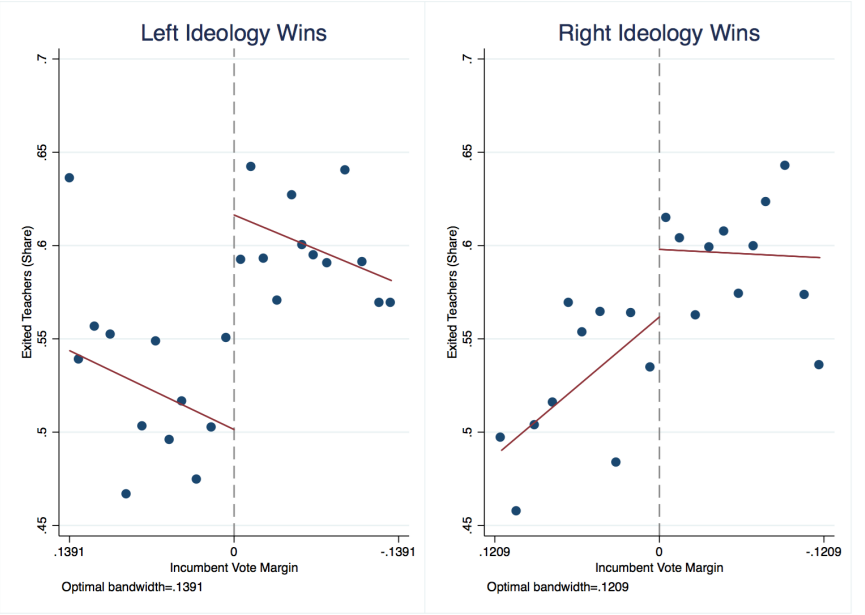
Notes: This figure shows the share of schools with a new headmaster by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for each year t, where t is one year, three years, and five years after the 2008 election. Municipalities with IncumbVoteMargin<0 experienced a change 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.5: *Political Turnover and New Teachers in Municipalities where the Winning Party was from the Left vs. the Right*



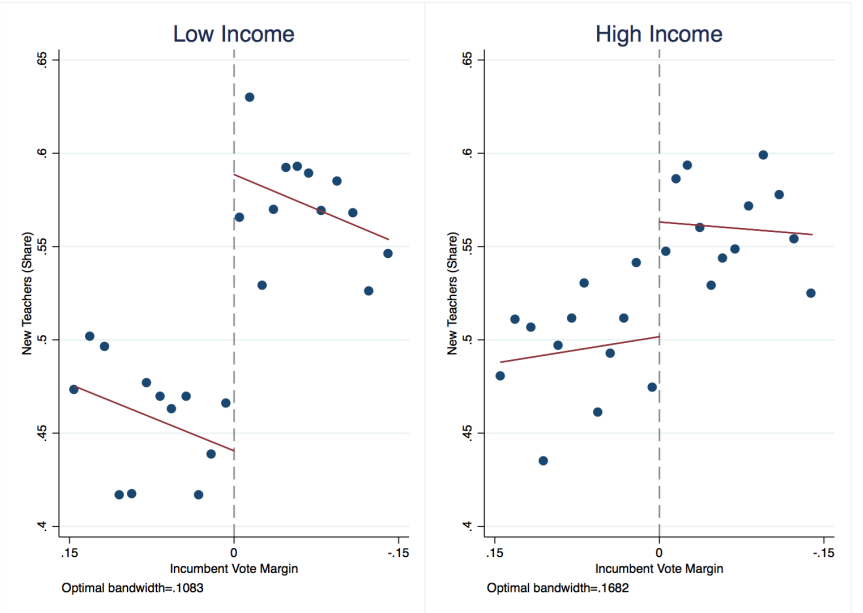
Notes: This figure shows the share of teachers that are new to a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<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.6: *Political Turnover and Teachers that have Left in Municipalities where the Winning Party was from the Left vs. the Right*



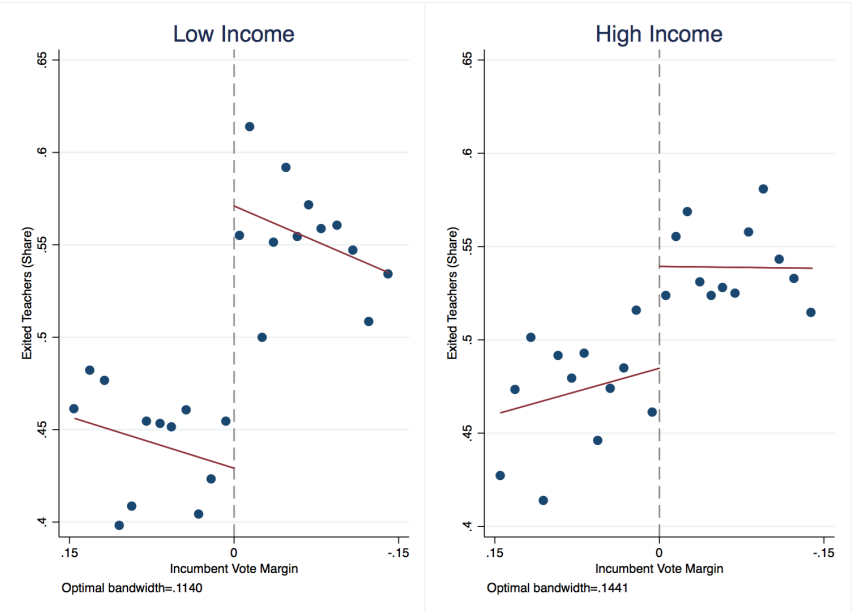
Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities where the winning party was from the left and those where the winning party was from the right. Municipalities with IncumbVoteMargin<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.7: *Political Turnover and New Teachers in Low- and High-income Municipalities*



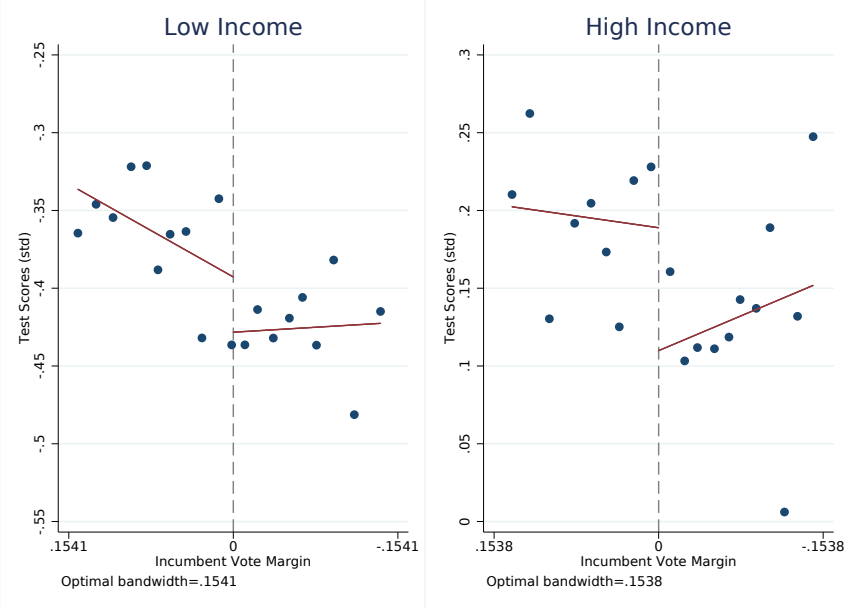
Notes: This figure shows the share of teachers that are new to a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. 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.8: *Political Turnover and Teachers that have Left in Low- and High-income Municipalities*



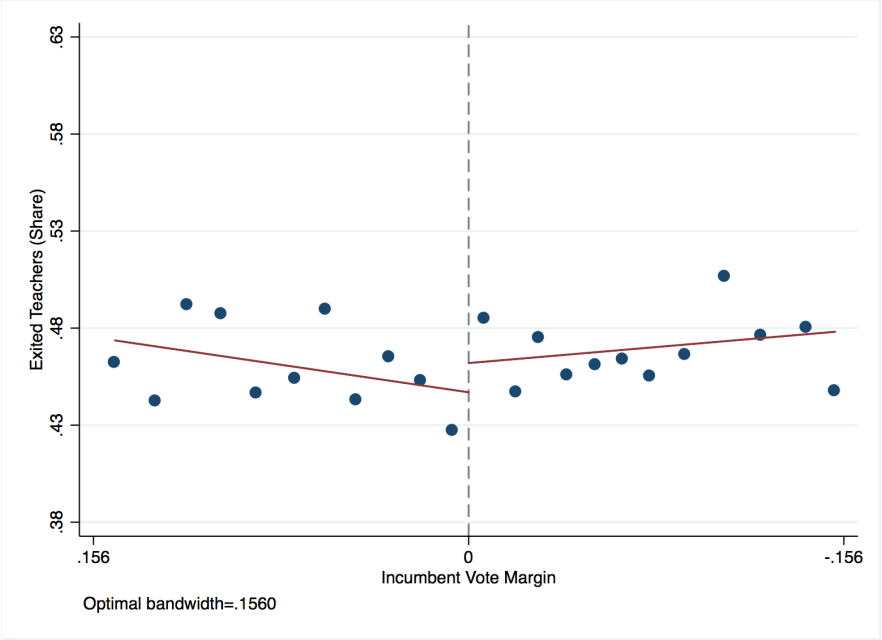
Notes: This figure shows the share of teachers that have left a school by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities 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.9: *Political Turnover and 8th Grade Test Scores in Low- and High-income Municipalities*



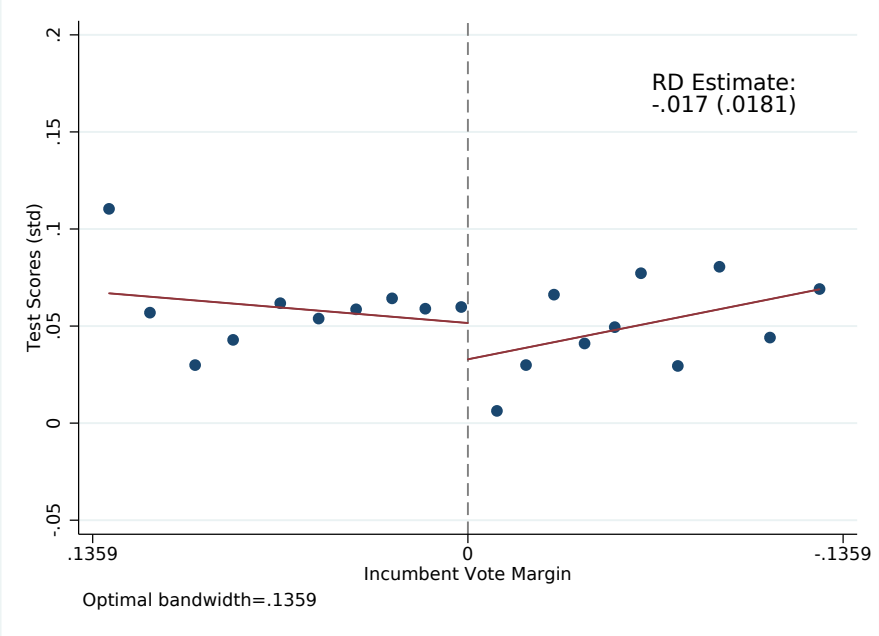
Notes: This figure shows the mean of individual-level 8th grade test scores by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points) separately for municipalities with high and low income. Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. 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 8th 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.10: *Political Turnover and Teachers that have Left in Non-municipal Schools*



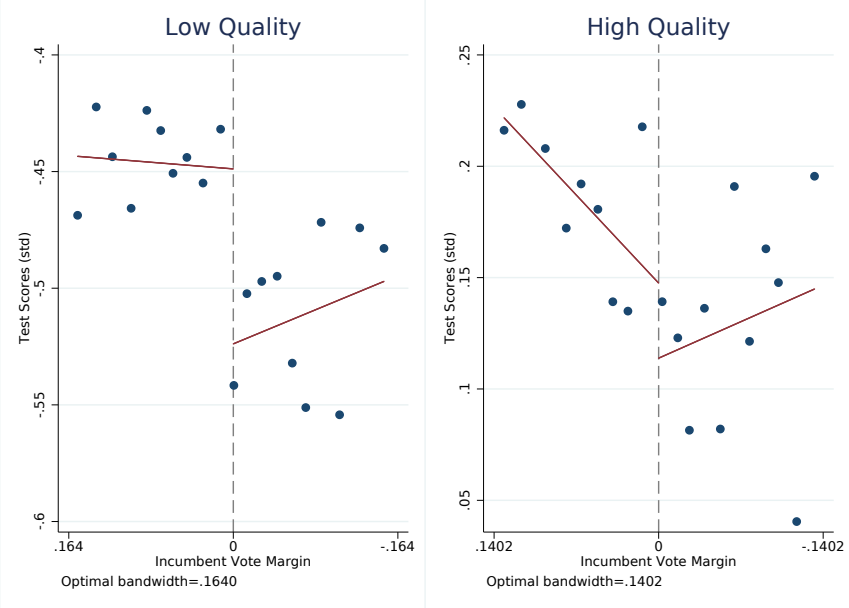
Notes: This figure shows the share of teachers that have left non-municipal schools by bins of IncumbVoteMargin (the size of each bin is 1.5 percentage points). Municipalities with IncumbVoteMargin<0 experienced a change in the political party of the mayor. Municipalities with IncumbVoteMargin>0 did 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.11: *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.12: *Political Turnover and 8th Grade Test Scores in Low- and High-quality Schools*



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 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 8th 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.

Table A.1: *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.2: *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.3: *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.4: *Political Turnover in 2008 and Headmaster Replacements 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.5: *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.6: *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 1.10 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.7: *Political Turnover and 8th Grade Test Scores in Low- and High-income Municipalities*

Outcome:	Individual 8th 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 1.4 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.8: *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 1.4 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.9: *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 1.4 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.