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“Social Promotion in Primary School: Immediate and Cumulated Effects on Attainment”

Margaret Leighton, Priscila Souza and Stéphane Straub

Toulouse School of Economics

Social Promotion in Primary School: Immediate and Cumulated Effects on Attainment*

Margaret Leighton[†], Priscila Souza[‡] and Stéphane Straub[§]

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Abstract

Does social promotion perpetuate shortfalls in student achievement, or can low-achieving students catch up with their peers when they are pushed ahead? Using data from Brazilian primary schools, this paper presents evidence of substantial catch up among socially promoted students. After documenting sorting across schools in response to the policy, in particular away from gated-promotion private schools, we show that social promotion cycles has no significant effect on municipality enrolment figures or on the percentage of students

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†University of St Andrews, corresponding author: mal22@st-andrews.ac.uk

‡Climate Policy Initiative/Pontifical Catholic University of Rio de Janeiro

§Toulouse School of Economics

dropping out mid-year. Cohorts of students exposed to episodes of social promotion display higher rates of age-appropriate study than their peers who faced the threat of repetition each year: by age eleven, 5.6 fewer students out of 100 have fallen behind in their studies, while 5.1 fewer students out of 100 are two or more years delayed. These gains, which arise mechanically during the period of social promotion, are highly persistent over time – even through educational stages which are typically high-stakes. This evidence suggests that, absent the social promotion policy, retention rates in Brazilian primary schools are inefficiently high: many promoted students successfully pass gateway exams after being pushed ahead, and go on to complete junior primary school on time.

JEL codes: I21, I28, I25

1 Introduction

Brazil's large-scale experimentation with social promotion provides a unique opportunity to study the impact of repetition policies on primary school children. To combat the accumulation of students in the early grades of primary school, a policy of social promotion 'cycles' was introduced in the late 1990s. This policy, which encouraged the definition of groups of grades during which students would advance automatically to the next level without the threat of repeating, was adopted and implemented in different ways across the country. In some states, all schools were required to adopt the policy under similar guidelines; in others the decision was heavily decentralised. Many school jurisdictions that adopted the policy later abandoned it, and vice versa; across jurisdictions, the length and number of cycles also varied.

This paper exploits within-municipality variation in promotion policies to estimate how cycles of social promotion affect primary-aged children's progress through school. We first demonstrate that the introduction of social promotion cycles has no significant effect on municipality enrolment figures, but that students do sort across schools in response to changes in school policies. School-level adoption of social promotion is associated with an increase in enrolment at younger ages (nine and ten), and decreases at older ages (age twelve). While we cannot track individual students, we document that this arises in part due to net outflows from private schools in those municipalities where social promotion has been adopted among public schools: municipality-wide adoption of the cycles policy in public schools is associated with a 2-7% decrease in private school enrolments, depending on the age group.

Although adoption of the policy is associated with a significant decrease in repetition rates, the policy has no effect on the number of students dropping out mid-year. In contrast, exposure to the social promotion policy substantially decreases the share of students who are delayed one or more years in their studies. While this should occur deterministically during the social promotion cycle, the effect persists for at least five years - long after most students would again risk retention. Each year of exposure to the policy decreases the share of students who are delayed by between 1-2%. Nearly identical point estimates are found when estimating the effect of exposure to social promotion on the share of students who are delayed by two or more years, suggesting that the policy is particularly effective at keeping potential "serial repeaters" on track and engaged with the school system. These effects are highly persistent over time, and cumulate with further exposure to the policy.

This paper makes three contributions. First, we estimate how primary school students' grade progression at different ages is affected by exposure to social promotion, both in the current year and for up to six years previously. This allows us to study not only the short- and medium-term effects of promotion on attainment, but also how these effects cumulate.

Furthermore, we carry out our study in a country with very high baseline repetition rates. This sheds light on the extent to which recent studies in the United States can inform education policies in such settings. In contrast to much of this recent literature, our results indicate that social promotion has a positive net effect on student achievement in Brazil. Our results therefore suggest that current repetition rates in Brazil are too high, and that attainment would be improved by relaxing promotion standards.

Finally, we build on previous work by including the universe of Brazilian primary schools in our analysis.[1] Doing so allows us to offer insights into the overall impact of the policy, while also offering the first (to our knowledge) evidence of student sorting in response to the introduction of social promotion. While net municipal enrolment was not affected by municipality-wide adoption of the policy, we observe substantial changes at the school level. Schools with a social promotion policy in place attract younger students, particularly those aged nine and ten, and repel older students (specifically twelve-year-olds, who are the oldest age-group we look at). This is an important finding for studies which consider this policy at the school level, as the resulting changes in student composition pose a threat to identification.

The effects of grade repetition on the educational path of students are notoriously difficult to identify. Traditional repetition protocols tended to rely heavily on teacher or parental evaluations, and were therefore heavily influenced by unobserved student characteristics. The increasing use of standardised tests, particularly in the United States, has generated a new wave of research on grade repetition.[2] The discontinuity in repetition probabilities created by test score-based passing thresholds has generated populations of retained and promoted students who are more similar than ever before, paving the way for the application of more rigorous empirical methods.

While results from these programmes are mixed, they offer some support for grade repetition, particularly early in schooling. Using regression discontinuity around the retention cut off, Jacob and Lefgren (2004) find that retention under the Chicago

[1] We do exclude federal schools, which represent less than 0.0005% of all primary schools.

[2] In the late 1990s, Chicago moved away from a system of social promotion by introducing high stakes exams in grades three and six, accompanied by remedial summer school for students with poor exam performance. The state of Florida has implemented a similar programme.

programme had positive effects on subsequent test scores for students facing retention in grade three, but no significant effects for students facing retention in grade six. Applying a learning model to the same data, Roderick and Nagaoka (2005) reach a more pessimistic conclusion: retention was neutral for students in grade three, but actually harmful to students retained in grade six. In Florida, Greene and Winters (2007) find that retention in the third grade increased grades two years later.

Despite these recent advances, research on the longer-term effects of grade repetition remains rare. In a recent working paper, Schwerdt et al. (2015) estimate the impact of retention in third grade on outcomes up to graduation for students in Florida. The authors find that retention in third grade, along with an associated remedial education package, increases achievement significantly, but also find that these gains fade out over the subsequent six years. While they find that retention in grade three decreases the probability of future retention, it has no effect on the probability of graduating from high school. Allen et al. (2009)'s meta-analysis of recent North American studies supports this fade-out finding more generally.

The findings discussed so far are based primarily on data from the United States, and it is not obvious how they would translate to different contexts. Studies from both developed and developing countries find that repetition increases drop-out risk, suggesting some findings are robust across contexts.[3] In Brazil, as in many developing countries, repetition rates are high throughout primary school: the average first

[3]Using data from grade eight students in Chicago, Allensworth (2005) finds that dropout increased among retained students, while for the larger group of non-retained students, dropout decreased. André (2009) and Glick and Sahn (2010) conclude that grade repetition among primary school students in Senegal increases the risk of dropping out. Manacorda (2012) presents similar findings from Uruguay.

grade repetition rate in our sample is 16% (28% if you include mid-year quits among repeaters), and it remains close to 10% throughout primary school. While the North American studies suggest repetition can have a positive effect on learning, there has been little research on what the appropriate repetition threshold should be.

Koppensteiner (2014) considers the effect of introducing social promotion cycles on the test scores of 4th grade students in state-run schools in the Brazilian state of Minas Gerais. Comparing early and late adopters of the state social promotion policy, he finds that the move to social promotion in 2nd and 4th grade decreased test scores. In contrast to the national average, however, state schools in Minas Gerais had low baseline repetition rates: 6% in 2000, when the programme was first introduced. Menezes-Filho et al. (2008) study urban state schools across the country, and estimate how policy status affected test scores, drop-out and promotion rates in 2005. They find that drop-out rates decreased, particularly for eighth graders, and that test scores decreased for eighth graders, but not for fourth graders. Carvalho and Firpo (2014), in contrast, find that the introduction of cycles had no significant effect on the test scores of students in grades four and eight across the achievement quantiles. Interestingly, the authors find that the *un-adoption* of the cycles policy improved mathematics scores of fourth grade students across all achievement quantiles, while it had no effect on eighth grade students' mathematics scores.

The remainder of paper proceeds as follows. In Section 2 we provide background information on the education context of Brazilian primary schools, and describe the cycles policy in detail. Section 3 describes the data. Section 4 presents our empirical strategy. Sections 5 present our main results and robustness exercises. Section 6 discusses the findings, while Section 7 concludes. Additional details on the data and the cycles policy, as well as robustness exercises around the primary empirical specifications, are in the Appendix.

2 Background

2.1 Education in Brazil

High-quality education is crucial both for individual success and for a country's economic and social development. Deficiencies in both the quantity and the quality of Brazilian public education have long been seen as an one of the major obstacles to growth and social inclusion in the country. High age-grade gaps have historically plagued the Brazilian educational system, and remain particularly predominant in poor and rural areas.

Although historically an important issue in Brazil, the number of school-age children out of school had declined rapidly over the past two decades. While in 1992 13.4% of children between the ages of seven and fourteen were out of school, this number fell to 3.5% in 2001, 2.3% in 2007 and 1.5% in 2013 (PNAD/IBGE).[4] Cardoso and Verner (2006) confirm this trend. Reporting on a survey of twelve- to eighteen-year-olds living in favelas of Fortaleza, the authors find that, even among this high-risk population, almost all twelve-year-olds attend school. Attendance rates start to fall at age thirteen for boys (down to 80 percent), while they remain high

[4]PNAD is the National Household Sample Survey (Pesquisa Nacional por Amostra de Domicílios) from the Brazilian Institute of Geography and Statistics (IBGE-Instituto Brasileiro de Geografia e Estatística). The survey collects annual data on the characteristics of the population with a sample size of over 150,000 households.

among girls until age seventeen. Nationwide a smaller, but still impressive, decline in the out-of-school rate can be seen among older students compared to primary-school-aged children. For those between the ages of fifteen and seventeen, the rate fell from 40.3% in 1992 to 15.8% in 2013 (PNAD/IBGE).

The proportion of students who are too old for the classes they are attending has also decreased. As can be seen in Table 1, in 1982, 72% of first graders were too old for that grade. This problem spread over all grades, so that by seventh grade 80% of students were not at the appropriate grade for their ages. The issue has been drastically reduced, but it is still important. In 2010, 15% of first graders and 28% of seventh graders were above the target age for those grades. A number of policies have contributed to this improvement in educational outcomes, including the *Bolsa* conditional cash transfer programs, introduced in the early 2000s. Glewwe and Kassouf (2012) use data from the Brazilian school census to study the impacts of *Bolsa Escola* (later *Bolsa Familia)* on enrolment, dropout and promotion rates. They find positive effects of the program on all three indicators.

While enrolment rates and age-grade misalignment have both improved substantially in recent years, they remain significant obstacles to education quality in Brazil. Both of these problems are linked to the high levels of repetition predominant across the country. Following the 2012 PISA evaluation, the OECD (2013) noted that repetition rates in Brazil remain among the highest in surveyed countries. While there was a decline in grade repetition during primary school between the 2003 and 2012 evaluations, repetition rates in secondary school increased over the same period.

Grade repetition has been the object of study in Brazil for many years. Using

Table 1: **Percent of students in a grade not appropriate for their age**

Year	**Grade 1**	**Grade 2**	**Grade 3**	**Grade 4**	**Grade 5**	**Grade 6**	**Grade 7**
1982	71.9	76.5	77.2	76.6	76.6	80.2	79.8
1991	59.5	62.6	63.3	62.7	62.7	68.6	67.4
1996	40.0	44.1	46.4	46.6	46.6	53.2	49.2
2006	17.5	24.6	27.5	28.5	28.5	35.5	34.1
2007	18.3	23.7	27.2	28.2	28.2	34.4	32.1
2008	15.3	19.3	20.3	22.2	22.2	27.8	25.8
2009	15.4	21.5	22.5	23.0	23.0	29.5	27.5
2010	14.5	21.4	24.0	24.4	24.4	30.7	28.3

Source: PNAD.

microdata collected by the World Bank from 1981-1985 in Northeast Brazil, Gomes-Neto and Hanushek (1994) are able to follow individual students over several years. The authors find that poor academic performance and the absence of higher grades at a student's current school are both strong determinants of grade repetition. They show that students do increase their knowledge when they repeat grades, but suggest that repetition is a costly way of achieving these small gains. More recently, Koppensteiner (2014) finds that the shift towards social promotion cycles in Minas Gerais was accompanied by a decrease in 4th grade test scores, suggesting that repetition is indeed promoting student achievement.

2.2 The Cycles Policy

Until the early nineties, Brazilian schools followed the practice of allowing the repetition of students at every grade level. Students could not only repeat in every grade, but could also be retained several years in a row at the same level. Starting in 1997, a number of Brazilian municipalities and states adopted a system of 'learning cycles': groupings of school grades during which promotion is automatic. For example, if the first 8 years of primary school are grouped into 2 cycles of 4 years, then students will pass 1st, 2nd and 3rd grade automatically (subject to a minimum attendance rate), but may repeat the 4th grade. They will also pass the 5th, 6th and 7th grade, but may be retained in the 8th grade. This policy is called 'Continued Progression,'[5] referred to in this paper as the cycles policy.

This policy of learning cycles was nationally recognised in the Law of Guidelines and Foundations for Education[6] enacted by the Federal Government in 1996. This law granted additional autonomy to Municipalities and States to organise the schooling system. Although municipal schools in São Paulo had experimented with cycles as early as 1992, the first large-scale adoption of the policy was by the state of São Paulo in 1997. The Federal District and several of the 26 Brazilian States followed at various times, including Amazonas, Ceará, Espírito Santo, Mato Grosso, Minas Gerais, Paraná, Pernambuco, Rio de Janeiro and Rondônia.

Adoption of the policy took a number of different forms. While in some states and municipalities the adoption of cycles was mandatory, in others the system of cycles was only recommended. Therefore, in some states and municipalities schools could choose whether to adopt the social promotion system. While many states did not adopt the policy at all, others adopted it for some years and then retracted it. We will return to the policy and present some descriptive statistics on its adoption in Section 3.2.

[5] Progressão continuada.

[6] Lei de Diretrizes e Bases da Educação (LDB).

3 Data

3.1 Construction of the Panel

The data which provides the starting point of this paper is the *Censo Escolar*, an annual census of schools in Brazil below the tertiary level. The survey, carried out in May, covers both private and public schools, and has been running continuously since 1995. The data are publicly available from the *Instituto Nacional de Estudos e Pesquisas Educacionais* (INEP, the national education research institute). From 1995-2006 the *Censo Escolar* measured school-level variables, whereas from 2007 on data is presented at the student level, with associated school and teacher files.[7] The *Censo Escolar* survey varies from year to year; however the general topics remain fairly consistent over time. The survey sections include basic information, physical and instructional features of the school, teachers and staff, numbers of classes and students, and student flows from the previous year (retained, passed, dropped-out, and in some years transferred).

For the purposes of our study, we merge the data from the *Censo Escolar* into two panels, one at the school level, and one at the municipality level. Each panel begins with the first data on the cycles policy, in 1999, and runs until 2006. The two panels are presented individually below.

[7] School identifiers, as well as student-level identifiers, are encrypted in the publicly-available data. This encryption prevents the identification of individual schools, but also the linkage of schools across years.

3.1.1 School-level Panel

The *Censo Escolar* surveyed 248,257 schools in 2004. We restrict our attention to students in grades 1-8,[8] and to the schools in which they are enrolled. Excluding schools which teach only secondary school or pre-school leaves us with 166,505 schools. Of these, 116,209 are under municipal jurisdiction, 31,178 are under state jurisdiction, 19,078 are private schools, and the remaining 40 are federally-run. Table 2 gives the mean number of schools per municipality across the panel, both overall and by administrative jurisdiction.

Table 2: **Mean number of primary schools per municipality**

Year	**Total**	**Municipal**	**State**	**Private**	**Federal**
1999	33.192	23.750	6.208	3.224	0.009
2000	32.838	23.547	6.027	3.256	0.008
2001	31.854	22.711	5.837	3.297	0.008
2002	30.901	21.828	5.724	3.342	0.008
2003	30.282	21.247	5.635	3.393	0.007
2004	29.814	20.905	5.517	3.386	0.007
2005	29.115	20.484	5.239	3.384	0.007
2006	28.449	19.915	5.128	3.399	0.007

Source: Censo Escolar, authors' calculations. Mean values across municipalities. Municipal, State, Private and Federal refer to administrative authority of the schools from 1999-2006.

Not all primary schools offer both junior and senior primary classes. Table 3 lists the mean number of students enrolled in each grade, conditional on enrolment being

[8] We use the word 'grade' as an analog to the Brazilian term *serie*, with corresponding levels 1-8. During our panel, a new 9-year *ano* grade-level system began to be rolled out. The extra year (*ano 1*) essentially advanced primary school enrolment by one year. Throughout this study, we abstract from any differences in the two systems beyond their duration, and convert the *ano* grades (1-9) to their *serie* equivalent, where *ano* 2 = *serie* 1. The grade *ano 1* is excluded from the analysis. We will refer to these school years collectively as 'primary school'.

positive. As can be seen in the last column of the table, there are approximately three times as many schools offering junior primary grades as senior primary grades. Senior primary schools therefore enrol more students.

Table 3: **Mean number of students enrolled per school**

Variable	Mean	Std. Dev.	N
Grade 1	36.045	49.51	1233454
Grade 2	31.226	43.653	1214054
Grade 3	30.159	42.665	1180755
Grade 4	30.31	43.487	1114851
Grade 5	89.472	90.005	418137
Grade 6	81.211	76.407	389640
Grade 7	76.745	74.320	368378
Grade 8	73.184	71.726	347244

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2006.

The school level panel is highly unbalanced, due to schools opening, closing or registering as inactive. A total of 216,429 primary schools appear at least once in our 8-year panel; 14,227 are only active in a single year, while less than half, 129,942 schools, are present throughout.[9] Because our school-level regressions contain fixed effects, schools which are active only in a single year will drop out of our panel; we do not make further restrictions and retain the remaining schools for analysis.

3.1.2 Municipality-level Panel

Separately, we aggregate the school-level variables at the municipal level. We aggregate the data by summing the observations across schools. This is done in such

[9] To relate this number to the example given above, this means that 129,942 of the 166,505 schools active in 2004 were open throughout our panel, while the remainder were open for some part of the 8 years under study.

a way that it is as if the municipality had only one school, with all the students and resources pooled together. Data are then merged to create an 8-year panel with municipality-years as the unit of observation. The 1999-2006 municipality panel is highly balanced: compared to 2006, there are 4 fewer municipalities in 2001-2004, and 57 fewer in 1999-2000. We exclude these 57 municipalities from our study, retaining a final sample of 5507 municipalities.

We augment the municipality panel with census data on municipal population and gross domestic product from the *Instituto de Pesquisa Economica Applicada* (IPEA). Additional data regarding ages of children surveyed in the year-2000 census were acquired from the *Sistema IBGE de Recuperação Automática* (SIDRA).[10]

3.2 Policy Variable

Data on adoption of social promotion cycles are available in the *Censo Escolar* in two forms.[11] In 1999, and again from 2003 to 2006, the data include individual schools' reported "total number of cycles and duration of each cycle".[12] From 2009 onward, schools are simply asked whether or not elementary school is organised in cycles.[13] While the questionnaires from 2000-2002 also contain the cycles module,

[10]Data come from the section on education, accessed through: www.sidra.ibge.gov.br/bda/popul.

[11]Over the course of our panel, there are approximately 30,000 school-year observations which return a missing policy. A few examples from the data lead us to believe these are either clerical errors or misunderstandings, and are meant to indicate absence of cycles. One such example is the state of Minas Gerais where, between 2002 and 2003, the number of schools responding to the question falls by 61%, while the share of schools adopting cycles rises from 41% to 99%. This situation persists in 2004, before reverting to pattern much more similar to that observed in 2002. Coding these missing values as zeros also makes the school-level policy consistent with the municipal aggregation: weighted share of schools in the municipality reporting using cycles.

[12]From the 1999 *Censo Escolar* questionnaire: *Número Total de Ciclos e Duração de cada Ciclo.*

[13]From the 2009 *Censo Escolar* questionnaire: *Ensino Fundamental organizado em ciclos.*

the data are absent from the publicly available data files. Cycles data from these years were provided to us on request; however, these supplementary data are only yes/no. For the 1999-2006 panel, therefore, we have a consistent binary measure of cycles adoption, but no consistent details on duration or timing of these cycles: we therefore restrict ourselves to a binary adoption variable.[14] An overview of the number of cycles used by adopting schools, and of the length of the first cycles, can be found in Appendix B.2.

Summarising the adoption of cycles policies at the municipal level requires an aggregation which is less natural than that done for school outcomes. We first calculate the weighted share of schools within a given municipality which report using cycles: this corresponds to the probability that a randomly selected student is enrolled in a cycles-using school.[15] We then simplify this to a binary variable by defining a threshold share of adopting schools above which a municipality is coded as adopting the policy. Our intent here is compare municipalities where a majority of schools are using cycles to those where this is not the case. The distribution of the share of schools using cycles within a given municipality is highly bimodal (see Figures 2 and 3 in Appendix B), motivating our use of a binary specification. We therefore define cycles use in a municipality as equal to one if the share of weighted share of schools reporting use of a cycles policy is greater than 75%.[16]

[14] Menezes-Filho et al. (2008) use the cycles policy variable in a similar way: schools are coded as having the policy, or not. The authors also offer suggestive evidence that controlling for number and length of cycles strengthens the estimated effect of the policy, with fewer, longer cycles reducing drop-out rates.

[15] Weights are calculated based on each school's enrolment of students in grades 1-8.

[16] We also compute a series of alternate thresholds, retained for robustness exercises in Section C.5. The alternate measures are summarised in Table 35, Appendix B.2.

The annual means of our policy variable are shown in Table 4, both overall and for schools operated by each of the two primary public jurisdictions. The fairly stable mean prevalence of the cycles policy masks substantial volatility in the policy's adoption. As can be seen in Table 5, about 5% of municipalities move in or out of the policy every year: these municipalities go from either almost complete adoption to abandonment of the policy - or vice versa. A more detailed summary of the prevalence of the cycles policy across the five regions of Brazil, and how this changes over time, is given in Appendix B.3.

Table 4: **Cycle prevalence: municipality means**

Year	**Overall**	**Municipal**	**State**
1999	0.192	0.209	0.421
2000	0.178	0.225	0.372
2001	0.174	0.223	0.376
2002	0.166	0.232	0.370
2003	0.160	0.229	0.356
2004	0.146	0.237	0.334
2005	0.189	0.238	0.406
2006	0.154	0.225	0.328

Source: Censo Escolar, authors' calculations. Mean values across municipalities. Overall prevalence is the binary variable described in Section 3.2, while Municipal and State are equal to 1 in municipalities where policy adoption in municipality-run or state-run schools, respectively, exceed the 75% threshold.

Table 5: **Movement in and out of cycle use**

Year	**Change**	**Adopt**	**Unadopt**
1999	.	[0.238]	.
2000	0.071	0.029	0.042
2001	0.048	0.022	0.026
2002	0.043	0.017	0.025
2003	0.035	0.015	0.021
2004	0.057	0.021	0.035
2005	0.075	0.059	0.016
2006	0.063	0.014	0.049

Source: Censo Escolar, authors' calculations. Mean values across municipalities. Adopt is equal to 1 for municipalities whose binary cycles policy prevalence variable passed from 0 in the previous year to 1 in the current year (and vice-versa for unadopt). Change is equal to 1 if either adopt or unadopt are equal to 1. Since no data are available in 1998, the change in policy is not defined for 1999. The adoption figure in square brackets gives the share of municipalities using cycles in 1999. For further details, see Section 3.2.

Of the 5,507 municipalities in our panel, approximately one fifth (1,179) change policy status between 1999 and 2006. Given that the parameters of interest will be identified off these municipalities, it is worth looking at them in some detail before proceeding.[17] Figure 1 shows the location of these municipalities on a map of Brazil. Consistent with the overall adoption pattern of the policy, none of the municipalities are located in the North or in the South.[18] While there is some presence in the Northeast, the majority of the policy-changing municipalities are located in the Southeast and the Centre-West. Within these two areas, the distribution of these municipalities does not display any obvious geographical pattern.

Table 6 presents some descriptive statistics comparing municipalities that have a

[17]Not all adopting municipalities are counted in this 1179. Of the 4,328 municipalities who do not change policy status, 405 use social promotion.

[18]Given the density of municipalities in the South region, this is difficult to detect in Figure 1. For further details, see Appendix B.

policy change with those which do not. Municipalities with a policy change are on average slightly smaller, have higher municipal GDP per capita, have fewer municipal schools, fewer primary school teachers, and had a lower repetition rate in 1995.

Table 6: **Comparison of municipalities with a policy change to those without**

	Pop	**GDP**	**Schools**			**Teachers**	**95 Rep**
			Municipal	State	Private		
change=0	3.1e+04	4.021	1.705	0.337	0.051	11.341	0.185
	(2.0e+05)	(4.378)	(1.517)	(0.429)	(0.076)	(3.132)	(0.119)
change=1	3.1e+04	4.719	0.986	0.304	0.047	10.709	0.121
	(1.2e+05)	(5.264)	(0.912)	(0.297)	(0.069)	(2.959)	(0.120)

Source: Censo Escolar (2001, 1995) and INEP, authors' calculations. Mean values across municipalities; standard deviations in parentheses. Pop is population; GDP is per capita in yr-2000 Real; schools and teachers are counted per thousand capita, grades 1-8 only; 95 Rep is the repetition rate in 1995.

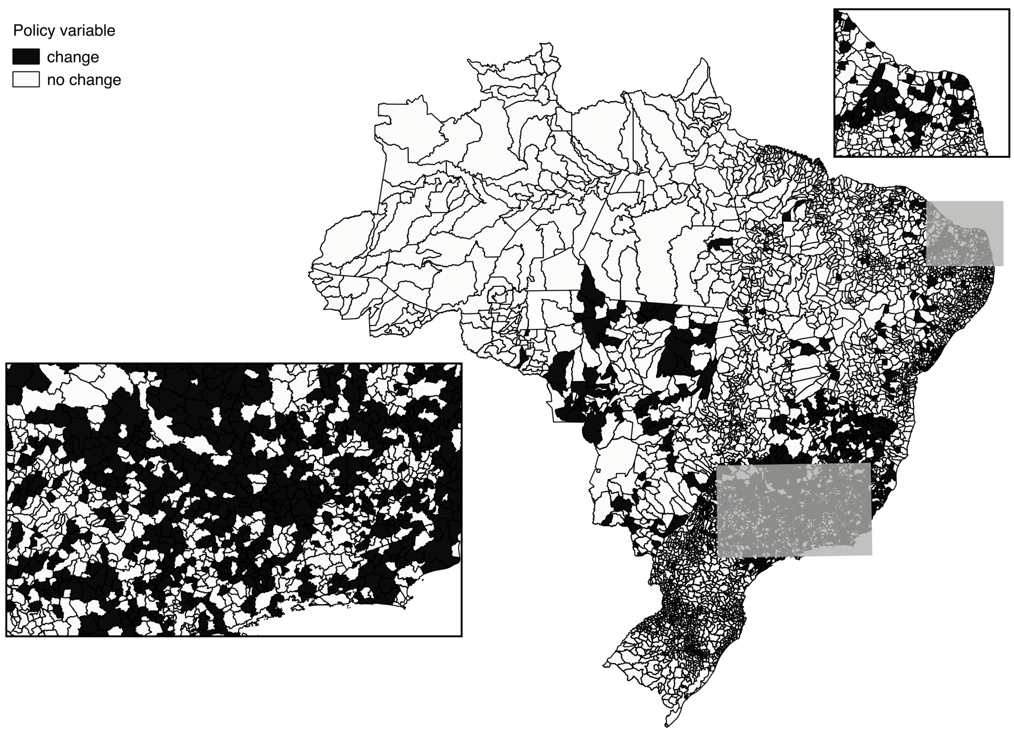
3.3 Outcome Variables

Our primary outcome measures are enrolment, grade attainment of those who are enrolled, and flows of students from one year to the next. The definition of these variables, and some summary statistics, are presented below.

3.3.1 Enrolment

We follow Glewwe and Kassouf (2012) by using the natural logarithm of student numbers as our primary measure of enrolment. Table 7 presents summary statistics of school-level age-specific enrolments, in levels and in natural logs. We maintain

Figure 1: **Location of municipalities with a change in policy status**



Source: authors' calculations, using municipality shapefile from IBGE and policy data from the *Censo Escolar*.

two other measures of enrolment for comparison: enrolment in levels, and enrolment as a share of relevant age category from the 2000 census (see Appendix C).

Table 7: **School-level mean enrolments: levels and natural logarith**

	Level	**Std. Dev.**	**Ln**	**Std. Dev.**	**N**
Age 6	6.645	12.833	1.19	1.066	474989
Age 7	18.628	28.59	2.108	1.285	1147125
Age 8	22.546	34.779	2.262	1.318	1208863
Age 9	22.651	34.84	2.258	1.327	1221614
Age 10	22.29	34.585	2.217	1.346	1253105
Age 11	22.394	32.961	2.213	1.383	1251465
Age 12	23.948	38.664	2.142	1.47	1017822

Source: Censo Escolar, authors' calculations. Mean values across schools, conditional on positive enrolment at that age level, from 1999-2006.

3.3.2 Passing, Repetition and Drop-out

Each wave of the *Censo Escolar* collects data on the student flows from the previous year. Specifically, schools are asked to report how many students from each grade repeated or were promoted at the end of the year, and how many dropped out before the end of the year.[19] In order to convert these student counts into rates, we divide the counts by the number of students enrolled in each grade, reported in the *Censo Escolar* of the previous year.

Table 8 gives school-average repetition, pass and dropout rates, both overall and by policy status, averaged across the years of our panel. Note that the difference in repetition rates between schools which have adopted the cycles policy and those

[19] In some waves there are additional categories: conditional or unconditional pass, transferred out of the school, joined the school part-way through the year, etc. We focus on these three because they are both the most interesting to us, and those which are most consistently measured across years.

that have not varies considerably across grade levels. The equivalent table at the municipal level (see Table 9) presents a similar picture: if anything, the differences between repetition rates and drop-out in adopting and non-adopting municipalities are larger than the inter-school differences. The variation over time for a subset of these statistics is presented in Appendix A.2 (see Table 28).

Table 8: **School-level student flows by policy status**

	Cycles	**Gr. 1**	**Gr. 2**	**Gr. 3**	**Gr. 4**	**Gr. 5**	**Gr. 6**	**Gr. 7**	**Gr. 8**
Repeat		0.19	0.15	0.11	0.09	0.12	0.10	0.08	0.07
	No	0.22	0.16	0.12	0.09	0.12	0.10	0.08	0.06
	Yes	0.05	0.13	0.06	0.09	0.10	0.09	0.07	0.08
Pass		0.70	0.77	0.80	0.83	0.77	0.81	0.82	0.85
	No	0.67	0.76	0.79	0.83	0.77	0.80	0.82	0.86
	Yes	0.86	0.82	0.88	0.86	0.79	0.82	0.81	0.84
Drop		0.11	0.08	0.08	0.08	0.10	0.09	0.09	0.08
	No	0.11	0.09	0.09	0.09	0.10	0.09	0.09	0.08
	Yes	0.07	0.05	0.05	0.05	0.10	0.09	0.10	0.10

Source: Censo Escolar, authors' calculations. Mean values across schools from 1999-2005.

Table 9: **Municipality-level student flows by policy status**

	Cycles	**Gr. 1**	**Gr. 2**	**Gr. 3**	**Gr. 4**	**Gr. 5**	**Gr. 6**	**Gr. 7**	**Gr. 8**
Repeat		0.16	0.14	0.10	0.09	0.13	0.11	0.08	0.07
	No	0.18	0.15	0.11	0.10	0.14	0.11	0.08	0.07
	Yes	0.04	0.08	0.04	0.09	0.07	0.07	0.05	0.08
Pass		0.73	0.77	0.81	0.82	0.73	0.77	0.79	0.82
	No	0.70	0.75	0.79	0.81	0.71	0.76	0.78	0.82
	Yes	0.88	0.86	0.90	0.85	0.83	0.84	0.85	0.82
Drop		0.08	0.06	0.06	0.06	0.12	0.11	0.11	0.10
	No	0.09	0.07	0.07	0.07	0.13	0.11	0.11	0.11
	Yes	0.04	0.03	0.03	0.03	0.08	0.08	0.08	0.09

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2005.

3.3.3 Grade Delay

Measuring grade attainment presents one major limitation: grade information is available only for those students enrolled in school. Since students who are not enrolled are likely to be lower-achieving than those who are, our estimates of grade attainment should be thought of as an upper bound. The fact that most children of primary school age are enrolled in school at this time attenuates this issue. We measure grade attainment as the share of students who are delayed in their studies. We define grade delay in a strict sense by coding a student as delayed if she is older than the target age for the grade she is in. By this strict definition, an eight-year-old studying in grade one would be classified as delayed. We also calculate a less strict measure, allowing a year of tolerance: an eight-year-old studying in grade one is not considered delayed by this measure, but a nine-year-old in grade one is.[20]

Table 10 presents the annual grade attainment of a single birth-cohort of students, illustrating the early onset and large extent of primary school grade delay. The table shows municipal mean enrolment numbers and grade level of children born in 1994, from 2000 (when the children were six years old) to 2006 (when they were twelve).[21] Children are deemed to be "on time" with their studies if they are in grade one when they are seven years old, in grade two when they are eight, and so on. The "on time" students are highlighted in bold in Table 10; those to the left of the bold figure are

[20] We also compute the average grade of a birth cohort, see Appendix C.3.2.

[21] We restrict our analysis to students six to twelve years old who are enrolled in grades that are no more than two years ahead of the age-appropriate level. Data are available on children 'younger than six' enrolled in grade 1, but we omit these (very few) individuals because we cannot precisely determine their age. Prior to 2003, student ages were not reported for ages younger than two years below the age-appropriate grade level (e.g. number of six-year-olds is reported in grades 1 and 2, but not in higher grades). From 2003-2006 we maintain this truncation for consistency.

delayed, while those to the right are advanced for their age.

Table 10: **Annual enrolment and grade of 1994-born cohort**

Year	**Gr. 1**	**Gr. 2**	**Gr. 3**	**Gr. 4**	**Gr. 5**	**Gr. 6**	**Gr. 7**	**Gr. 8**
2000	82.38	1.78	.	.	.	.	.	.
2001	**456.16**	34.49	1.61	.	.	.	.	.
2002	241.65	**345.12**	29.45	1.63	.	.	.	.
2003	73.68	219.17	**308.06**	28.21	1.45	.	.	.
2004	32.29	83.73	196.34	**289.20**	26.43	1.40	.	.
2005	17.38	42.56	80.24	187.87	**273.99**	25.10	1.35	.
2006	8.59	23.43	45.30	83.15	193.66	**247.70**	21.53	1.41

Source: Censo Escolar, authors' calculations. Mean values across municipalities.

Finally, we generate a 'placebo' measure of grade attainment which should not to be affected by social promotion: the share of a cohort studying at a grade level above the target for their age. These students, who were either enrolled early or skipped a grade during their studies, are presumably among the top of the ability distribution for their cohort. There is no reason to expect the number of these students to be affected by social promotion, as they were unlikely to be at risk of repeating a grade in the first place. Table 11 gives an overview of student delays, averaged over the course of our panel.

4 Estimation Strategy

4.1 Estimating Equation

Our interest in this paper lies in identifying the effect of the cycles policy on students' progress through school. We measure progress with four outcome variables: two

Table 11: **Share of cohort with grade delay**

	1 Year		**2 Years**		**Advanced**	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age 8	0.398	0.203	0	0	0.058	0.076
Age 9	0.495	0.212	0.159	0.150	0.049	0.069
Age 10	0.548	0.218	0.251	0.197	0.040	0.062
Age 11	0.600	0.217	0.316	0.226	0.033	0.055
Age 12	0.642	0.205	0.358	0.233	0.028	0.045

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2006. Delay defined as number of years behind the target of first grade at age seven.

student flows (repetition and mid-year dropout), total enrolment and age-grade delay. Student flows are only observed at the grade level, and are measured as the share of initial enrolment in a given grade. Enrolment and grade level are observed by age as well as by grade. We estimate the effect of social promotion on these measures by age, as this measure is robust to changes in grade composition induced by the policy. A first approach to the problem would be to estimate equations of the form:

$$Y_{it}^{\tau} = \alpha + \beta D_{it} + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it}, \tag{1}$$

where Y_{it}^{τ} is the outcome of interest in unit i at time t for students τ (with τ representing either the grade-level or age group, as appropriate); D_{it} is a dummy for the policy; $\mathbf{X}_{it}$ is a vector of time-varying characteristics at the unit level; θ_i is the unit fixed effect and ϵ_{it} is the unobserved error term. Y_{it}^{τ} will be, for instance, the repetition rate of 3rd grade students in municipality i, or the total enrolment of ten-year-olds in school i. In each case we are interested in estimating the coefficient β: the effect of exposure to the policy in the current year on the outcome under consideration.

While there is movement in and out of the policy every year, the adoption of cycles is highly persistent year-to-year (see Table 5). It is unlikely that the history of the policy in a given school or municipality will be too important for young students enrolling in school for the first time.[22] For older students, however, who have been in school for several years, past exposure to the policy could affect their progress through school today. Given the persistence of the policy, if we fail to control for this past exposure we run the risk of overestimating the effect of the policy in the current year: students exposed today were likely also exposed last year.

As a second approach, we therefore estimate an analog of Equation 1 including lagged values of the policy. For outcomes by grade, we select the number of lags in order to coincide with the years students would have been in school, had they been progressing at the target rate.[23] For instance, for the outcomes of grade two students, we include the current and lagged value of the policy, while for grade three students an additional lag is added. When considering outcomes by age, where early enrolment is potentially a result of interest, we include lagged values of the policy back to age six. Since we are constrained in the number of years for which we have data on the cycles policy, we include a maximum of five lagged values of the policy at the upper grades and ages.[24]

[22]There could of course be some impacts: the school could be better adjusted to the policy if they have had it for several years, or students may enrol earlier or later as a result of the policy.

[23]This approach does not, unfortunately, include all years that all students in the group in question have been at school: if a student in second grade repeated last year, he is currently in his third year of school and therefore one year of his policy history is not controlled for. On the other hand, including additional lags decreases the number of years on which our data can be estimated. We adopt this approach as a compromise between the two.

[24]We limit our age-based analysis to students aged seven to twelve, with the constraint binding

This second approach can be summarised in the following equation, where $\mathbf{D}_i$ is a vector containing both current and past policy dummies:

$$Y_{it}^{\tau} = \alpha + \mathbf{D}_i'\beta + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it}. \tag{2}$$

More explicitly, taking as example an outcome which we examine by age, we estimate the series of equations given in (3) below.

$$\begin{aligned}
Y_{it}^{7} &= \alpha + \beta_0^7 D_{it} + \beta_1^7 D_{it-1} + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it} \\
Y_{it}^{8} &= \alpha + \beta_0^8 D_{it} + \beta_1^8 D_{it-1} + \beta_2^8 D_{it-2} + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it} \\
Y_{it}^{9} &= \alpha + \beta_0^9 D_{it} + \beta_1^9 D_{it-1} + \beta_2^9 D_{it-2} + \beta_3^9 D_{it-3} + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it} \\
Y_{it}^{10} &= \alpha + \beta_0^{10} D_{it} + \beta_1^{10} D_{it-1} + \beta_2^{10} D_{it-2} + \beta_3^{10} D_{it-3} + \beta_4^{10} D_{it-4} + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it} \\
Y_{it}^{11} &= \alpha + \beta_0^{11} D_{it} + \beta_1^{11} D_{it-1} + \beta_2^{11} D_{it-2} + \beta_3^{11} D_{it-3} + \beta_4^{11} D_{it-4} + \beta_5^{11} D_{it-5} + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it} \\
Y_{it}^{12} &= \alpha + \beta_0^{12} D_{it} + \beta_1^{12} D_{it-1} + \beta_2^{12} D_{it-2} + \beta_3^{12} D_{it-3} + \beta_4^{12} D_{it-4} + \beta_5^{12} D_{it-5} + \mathbf{X}_{it}'\gamma + \theta_i + \epsilon_{it}
\end{aligned} \tag{3}$$

The coefficients of interest, the $\hat{\beta}_0^{\tau}...\hat{\beta}_5^{\tau}$, provide estimates of the effect of current and past exposure to the policy.

4.2 Unit of Analysis

Our finest unit of observation, both for our outcome variables and for the policy itself, is at the level of the school. The identifying assumption for school-level regressions

only for twelve-year-olds. Since we look at grade-specific outcomes up to grade eight, this limit is hit for most of upper primary school.

is that there are no unobserved, time-varying factors at the school level that are correlated both with policy adoption and with the outcomes we measure. Given that our student outcomes are measured only on those students enrolled, the possibility of students choosing their school based on the policy is a real concern.

Anecdotal evidence suggests that parents were displeased with the policy, and were concerned that students would under-perform if they did not face the threat of repetition. To the extent that this is true, more motivated parents may shift their children to schools which have not adopted the policy – possibly even sending their children to private schools to achieve this. Such a trend would be consistent with evidence from the United States, where Dong (2010) finds significant positive selection into schools with repetition in kindergarten.

In Section 5.1 below, we demonstrate that this concern is well-founded: while there is no net change in enrolment in municipalities which adopt the policy, individual schools that implement social promotion see substantial changes in student numbers. The altered student composition which results from such movements makes it impossible to interpret any associated changes in student performance as resulting solely from the introduction of social promotion.

Aggregating our data at the municipality level allows us to address this issue. Moving children to schools in a different municipality would be extremely costly for most families. While those living on a municipal boundary may do so in response to the policy, it is unlikely that this practice would be widespread. The identifying assumption at the municipality level is that changes in policy adoption are uncorrelated with any time-varying unobserved municipal characteristics which are, in turn, correlated with our outcome variables. While we cannot test this assumption directly, in Appendix B.1 we estimate policy adoption using both our baseline time-varying controls, and some time-invariant characteristics interacted with year. While some of our included controls do correlate with policy adoption (particularly, education level of upper primary school teachers), we find no systematic relation between social promotion policies and time-interacted political affiliation (mayor's party aligned with presidents, or mayor and governor being from the same party) or pre-policy repetition rates.

Two limitations of our municipal policy variable are worth raising before we turn to the results. First, it is a noisy aggregation. There may be some schools in the municipality who do not adopt cycles, even if the municipality is coded as an adopter. These non-adopters are unlikely to be randomly drawn. Private schools, for instance, rarely implement social promotion cycles. Similarly, public schools who defy the municipal norm are likely to have special characteristics. Second, adopting a policy of social promotion cycles does not mean promoting students automatically in each grade. As described in Section 2.2, some grades were commonly included in cycles, while promotion from others were rarely – if ever – accorded automatically.

Both of these issues introduce measurement error into our policy variable. We expect the first to dilute any effect of the policy, as some schools are opting out, and bias our coefficient estimates towards zero. The second prevents us from interpreting the effect of the policy as the impact of social promotion at any specific grade level (with the possible exception of grade 1). Our policy variable indicates that the vast majority of schools within the municipality were implementing social promotion at *some* grade levels. Although we will look at the effect this has on students across all primary school grades, only a fraction of these students will be directly effected in the current year. Any measured effect of policy status on grade seven students will therefore be the average effect across grade seven students at schools that were implementing social promotion in grade seven that year, and across those at schools that were implementing social promotion that year in some grades, but not in grade seven. Assuming that the effect of the policy will be strongest when it promises social promotion for the student group in question, this will also bias our coefficient estimates towards zero.[25]

5 Results

The following subsections present results from regressions described in Equations 1 and 2. All regressions include fixed effects (at either the school or municipality level), state-year interaction dummies, and a collection of time-varying controls. For school-level regressions, these controls include: a dummy for location[26] (rural or urban), a dummy for jurisdiction (state, municipal or private), total number of teachers, and number of teachers at the primary level. Standard errors are clustered at the level of the fixed effect.[27] For regressions using students aggregated by grade, the number and education of teachers teaching either grades 1-4 or 5-8 are also included.

[25]The policy may well impact students who are not in cycle grades as well. For instance, a student may feel she has a greater incentive to work this year if the next grade is inside a cycle.

[26]Approximately 2% of schools experience a location status change during the panel. It is not clear whether the schools themselves moved, the surrounding area developed, or if these are clerical errors.

[27]See, for example, Cameron and Miller (2015).

The additional controls included in municipal regressions are similar: the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, and the number and education scores of teachers teaching grades 1-4 and 5-8.[28] Summary statistics for controls at the school and municipal levels can be found in Appendix A.3 (see Tables 29 and 30).

5.1 Enrolment

Tables 12 to 15 present series of regressions with the natural log of total enrolment at each age as the dependant variable. Tables 12 and 13 present estimates of Equation 1: the effect of this year's promotion policy on enrolment, ignoring past policies. Results at the school level (Table 12) suggest that, while the adoption of social promotion does not affect the enrolment of seven- or eight-year-olds, it increases the numbers of nine- and ten-year-olds significantly, while lowering the number of twelve-year-olds. Results at the municipal level (Table 13), in contrast, show no significant effects of social promotion on enrolment at any age.[29]

Tables 14 and 15 present estimates of Equation 2, augmenting the previous regressions with lagged policy values. A similar contrast emerges in these augmented regressions as was previously observed in Tables 12 and 13: while Table 15 suggest that municipal enrolment is impervious to past and present promotion polices, Table

[28]Given that the qualification level and number of teachers could potentially be endogenous to the chosen promotion policy, we also replicate our main results omitting teacher controls. This omission does not significantly change our results (see Appendix C, Table 44).

[29]These regressions are replicated using levels rather than natural logs in Section C.1: see Tables 38 and 37.

14 reveals substantial movement of students across schools in response to the policy. These movements display a trend consistent with that displayed in Table 12: enrolment of young students increases with both current and past use of social promotion, while enrolment of twelve-year-olds decreases.[30] One possible explanation for this sorting pattern would be if families whose children progress through to higher grades are more averse to the cycles policy, while families of children who struggle at school view the policy - and schools that adopt it - more favorably.

Table 12: **Schools: log total enrolments (no lags)**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	0.00546	0.00431	0.0132***	0.0466***	0.000598	-0.0190***
	(0.00365)	(0.00297)	(0.00304)	(0.00351)	(0.00290)	(0.00318)
Observations	1147125	1208863	1221614	1253105	1251465	1017822

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), total number of teachers, number of teachers at the primary level and state-year interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Where are these students coming from, and where are they going? In Table 16, we estimate the effect of municipal promotion policies on the number of students enrolled in private schools, conditional on that municipality having a positive enrolment in private school in that year. Adoption of a social promotion policy is associated with a reduction in the number of students enrolled in private school at all ages, with that

[30] Particular caution is necessary when interpreting age-specific school-level results. Because not all schools teach all grades, the interpretation of the coefficients on the vector of lagged policy values must be carefully considered, particularly at higher grades. While we know the history of policy adoption for all schools, students who enrol at a senior primary school for the first time in grade 5 have not been affected by the policy history at that school; the history of promotion policies at those schools are therefore unlikely to have affected them significantly.

Table 13: **Municipalities: log total enrolments (no lags)**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	-0.00729	0.00368	-0.00100	0.00141	-0.00475	-0.00434
	(0.00779)	(0.00372)	(0.00410)	(0.00345)	(0.00319)	(0.00356)
Observations	44009	44027	44027	44027	44027	38531

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: **Schools: log total enrolments**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	0.0000795	0.00102	0.00792**	0.0235***	0.00633	-0.0177***
	(0.00411)	(0.00361)	(0.00399)	(0.00490)	(0.00579)	(0.00566)
L.Cycles	-0.00512	0.00876**	0.00548	0.0147***	-0.0138***	-0.0156***
	(0.00376)	(0.00353)	(0.00394)	(0.00486)	(0.00525)	(0.00517)
L2.Cycles		0.00601*	0.00857**	0.0150***	0.00430	-0.0133**
		(0.00334)	(0.00391)	(0.00487)	(0.00557)	(0.00541)
L3.Cycles			0.0113***	0.0134***	-0.00928	-0.0217***
			(0.00370)	(0.00490)	(0.00617)	(0.00618)
L4.Cycles				0.00758*	0.000402	-0.0138**
				(0.00442)	(0.00560)	(0.00577)
L5.Cycles					-0.00286	0.00103
					(0.00507)	(0.00524)
Observations	960116	831636	675264	539830	390857	366025

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), total number of teachers, number of teachers at the primary level and state-year interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: **Municipalities: log total enrolments**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	-0.00586	0.00441	0.00428	0.00186	-0.0183***	-0.00226
	(0.00870)	(0.00425)	(0.00602)	(0.00537)	(0.00620)	(0.00597)
L.Cycles	-0.00822	-0.00129	-0.00746	0.0000786	-0.00308	-0.0119*
	(0.00852)	(0.00422)	(0.00550)	(0.00556)	(0.00635)	(0.00642)
L2.Cycles		0.00374	-0.00587	0.00221	0.00601	0.00303
		(0.00537)	(0.00657)	(0.00595)	(0.00765)	(0.00675)
L3.Cycles			0.00443	-0.00104	0.0100	-0.00158
			(0.00499)	(0.00647)	(0.00739)	(0.00687)
L4.Cycles				-0.00356	0.0101	-0.0000293
				(0.00528)	(0.00685)	(0.00605)
L5.Cycles					-0.00349	0.00156
					(0.00591)	(0.00526)
Observations	38513	33029	27526	22021	16519	16519

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

reduction being substantial and statistically significant at all ages except seven (the first year of primary school) and ten (the year in which on-time students complete junior primary school). This finding contrasts with anecdotal evidence that social promotion policy was unpopular with parents: the widespread adoption of social promotion among a municipality's public schools results in a shift away from gated-promotion private schools. It should be noted that these are net effects: no doubt some parents did shift their children into private schooling in response to the policy. The net effect is dominated by other parents, possibly those with children struggling to meet passing requirements in their private school, who moved their children into the public system.[31]

Table 16: **Change in log private school enrolment**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	-0.0239	-0.0364**	-0.0389**	-0.0242	-0.0669***	-0.0500***
	(0.0178)	(0.0177)	(0.0180)	(0.0178)	(0.0206)	(0.0193)
Observations	18759	18757	18585	18424	17821	16678

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression, where the natural log of private school enrolment numbers is the dependant variable. Only municipality-years with a positive enrolment number are included. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A comparison of these municipal and school results suggests that students are indeed relocating across schools in response to the policy. While municipalities who adopt the policy display no change in enrolment, individual schools do. This finding,

[31] We also estimate the impact of municipal promotion policies on the share of private school students who are delayed. The overall effect is small and statistically insignificant.

while interesting in itself, makes us wary of pursuing any school-level analysis of student outcomes. Given the magnitude of student movements shown in Table 14, the composition of the student body at policy-adopting schools may evolve substantially following the implementation of social promotion. Assuming those students who select away from grade repetition have different average characteristics from those who do not, any analysis of school-level outcomes could be heavily biased. In the sections that follow we will present only municipal-level results, unless otherwise noted and justified.

Given the potentially enduring effects of social promotion on the grade level, subject mastery and motivation of students, it is likely that the promotion policies students have faced in the past will affect their current school experience in a number of ways. Furthermore, as promotion policies are highly persistent from year to year, current policy status will be strongly correlated with past policies, biasing the estimates of current promotion policies when past policies are not controlled for. A comparison of Tables 12 and 14 substantiates this concern. While the estimated effect of contemporary promotion policies on enrolment displays a similar trend across the two tables, point estimates are substantially smaller (and standard errors generally larger) when policies are controlled for. Due to this, and to our direct interest in estimating the persistence of any effects over time, we will limit ourselves in the following sections to estimating Equation 2.

5.2 Student Flows

Since the flows themselves affect the composition of each grade, lagged values of the policy are more delicate to interpret in grade-specific regressions than in age-specific regressions. Given the compositional effects of grade repetition, we cannot interpret the coefficient on the lagged policy variable as the effect of exposure to the policy for students when they were in the previous grade. Rather, these coefficients capture the average effect of exposure to the policy last year for students who are currently in enrolled in a given grade. This prevents us from using our estimates to compare the long-term impact of exposure to the policy at different grades.

5.2.1 Repetition Rate

Table 17 presents results from a series of municipal-level estimates of Equation 2 with grade-specific repetition rate as the dependant variable. This table gives us an indication of how the cycles policy was implemented. Contemporary social promotion policies decrease repetition rates in grades one, two and three, but not in grade four: a grade which is excluded from social promotion cycles (see Table 34 in Appendix B.2). Repetition rates are again lowered in grade five, with little effect thereafter besides some evidence for an increase in grade eight. The largest effect of the policy is seen in grade one, a grade which would almost universally be included in a social promotion cycle: municipalities which implement the policy retain 5 percentage points fewer first grade students than those which have no such policy in place. The policy coefficient in grades two, three and five is smaller, at around 2 percentage points. This could be due both to baseline repetition rates being lower in those grades, and to the fact that not all social promotion policies would include those grades within a cycle.

The last five rows of Table 17 present the effect of lagged exposure to the policy on repetition rates at given grades. While we hesitate to interpret individual coefficients, several are of particular interest. First, the second row of Column 2 indicates that social promotion in first grade does raise grade two repetition rates the following year. Given that many promotion cycles would have ended in second grade, with socially promoted students facing the threat of retention for the first time, this positive coefficient is expected. The small magnitude of this effect, which is one tenth the size of the initial boost in promotion, is somewhat surprising. Secondly, it is worth noting the effect of policy history on repetition rates in grade four (Column 4). Grade four is of particular interest due to it being the threshold year for entrance into senior primary school. If past exposure to social promotion resulted in a spike in repetition rates in that grade, we would be concerned that the attainment gains from the policy are largely mechanical: students pushed ahead by the policy simply fail at the earliest opportunity. We see some evidence for this in row 3, with exposure to the policy two years ago increasing the risk of repeating grade four; however, the magnitude of this effect is small.

5.2.2 Drop-out Rate

Results from equivalent estimates with drop-out rates as the dependant variables are presented in Table 18. Despite the large decrease in repetition rates, municipal-wide adoption of social promotion cycles has almost no effect on mid-year drop-out rates.

The one exception is in first grade, where exposure to the policy decreases dropout rates by about 0.3 of a percentage point. Given that grade one students drop out at an average rate of 11% across our sample (see Table 9), this is a small but not insignificant change.

The general absence of a significant change in dropouts contrast with previous research: across a diversity of context, researchers have found a positive association between repetition and drop out rates.[32] As Allensworth (2005) demonstrates, however, the effects of repetition on dropout can vary dramatically between repeaters and non-repeaters, and the absence of an overall effect can dissimulate compensating changes in these two groups. If students are promoted ahead of their abilities, they may face a discouraging mismatch between their abilities and the course material.[33] If this mismatch is severe, students may feel hopeless and dropout. On the other hand, repetition itself can be discouraging, and precipitate school-leaving. Given that social promotion could theoretically affect promoted students in either direction, we cannot draw firm conclusions. It is noteworthy, however, that there was in fact no overall effect: the observed gains in grade attainment were achieved without driving students more out of the classroom.

[32]See Allensworth (2005), André (2009), Glick and Sahn (2010) and Manacorda (2012).

[33]See, for instance, Pritchett and Beatty (2012) on learning profiles.

Table 17: **Municipality student flows: repeated**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	-0.0500***	-0.0171***	-0.0214***	-0.00300	-0.0215***	0.00429	-0.00164	0.00834*
	(0.00279)	(0.00236)	(0.00229)	(0.00235)	(0.00358)	(0.00443)	(0.00403)	(0.00456)
L.Cycles		0.00498**	-0.00528***	-0.000611	-0.00717**	0.00751	0.000389	0.0215***
		(0.00221)	(0.00193)	(0.00256)	(0.00314)	(0.00480)	(0.00430)	(0.00536)
L2.Cycles			0.00654***	0.00662***	0.00365	-0.000487	0.00293	0.00468
			(0.00208)	(0.00231)	(0.00364)	(0.00553)	(0.00549)	(0.00574)
L3.Cycles				0.00320	0.00547*	0.000682	0.00788*	0.00721
				(0.00211)	(0.00313)	(0.00496)	(0.00470)	(0.00551)
L4.Cycles					0.0131***	-0.000305	0.00160	0.000209
					(0.00336)	(0.00469)	(0.00449)	(0.00481)
L5.Cycles						-0.00416	-0.00601*	-0.00885**
						(0.00378)	(0.00345)	(0.00404)
Observations	38515	33016	27519	22017	16514	11011	11011	11011

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: **Municipality student flows: dropped**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	-0.00276*	-0.000335	0.000579	-0.000926	-0.000963	-0.00183	-0.000913	0.000611
	(0.00146)	(0.00132)	(0.00141)	(0.00106)	(0.00223)	(0.00312)	(0.00320)	(0.00368)
L.Cycles		0.000370	-0.00165	-0.000926	-0.00296	-0.00337	-0.00153	0.000288
		(0.00116)	(0.00120)	(0.000961)	(0.00232)	(0.00381)	(0.00374)	(0.00412)
L2.Cycles			-0.000113	-0.0000795	-0.00244	-0.00487	0.00500	0.00663
			(0.00126)	(0.00128)	(0.00262)	(0.00482)	(0.00406)	(0.00504)
L3.Cycles				-0.00165	0.00161	-0.000789	0.00133	-0.00362
				(0.00112)	(0.00256)	(0.00384)	(0.00430)	(0.00450)
L4.Cycles					0.00137	0.000335	0.00123	-0.00125
					(0.00220)	(0.00363)	(0.00397)	(0.00376)
L5.Cycles						0.00636**	0.00443	-0.00172
						(0.00276)	(0.00301)	(0.00336)
Observations	38515	33016	27519	22017	16514	11011	11011	11011

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.3 Grade Attainment

Table 19 presents results from a series of age-specific regressions where the dependent variable is the share of the enrolled cohort delayed by one or more grades. Seven-year-olds are excluded from the sample since, as grade one is the target grade for seven year olds, it is not possible for them to be both enrolled in school and delayed, according to our measures. Across the age range we study, past and present exposure to the social promotion policy has a substantial and significant negative effect on the share of the cohort delayed in their studies. The effect of the policy is strikingly consistent and persistent: each year of exposure reduces the share of delayed students by a little more than 1%, on average, with higher values for the youngest children. Significantly, the size of this decrease does not decay substantially with time, and cumulates with further exposure to the policy.

Table 20 replicates these findings using a looser measure, where students are considered delayed only if they are two or more years behind the target grade for their age. A very similar pattern emerges, with only slightly smaller effect sizes, as that in Table 19. The similar size of the estimates across the two measures of delay is noteworthy, as it suggests that the biggest gains from social promotion accrue to students who would otherwise have been retained twice. This points to a subset of the student population for whom repetition is particularly damaging: once retained, they are likely to be retained again. If pushed ahead, however, they stay ahead.[34]

[34] As a placebo test, we also regress the policy variable on a measure which should be unrelated: the share of a cohort studying ahead of their age level. We find no systematic effects of the policy on this measure (see Table 40 in Appendix C.3.1)

Table 19: **Municipalities: share of age group delayed one or more grades**

	(1) Age 8	(2) Age 9	(3) Age 10	(4) Age 11	(5) Age 12
Cycles	-0.0225***	-0.0163***	-0.0160***	-0.0132***	-0.00327
	(0.00389)	(0.00379)	(0.00329)	(0.00347)	(0.00354)
L.Cycles	-0.0237***	-0.0114***	-0.0135***	-0.0104***	-0.0114***
	(0.00378)	(0.00370)	(0.00333)	(0.00383)	(0.00362)
L2.Cycles	-0.0104***	-0.0161***	-0.0149***	-0.00751*	-0.00421
	(0.00367)	(0.00408)	(0.00357)	(0.00447)	(0.00429)
L3.Cycles		-0.00619*	-0.00811**	-0.0124***	-0.0108**
		(0.00344)	(0.00352)	(0.00450)	(0.00465)
L4.Cycles			-0.00528	-0.0128***	-0.00948**
			(0.00333)	(0.00435)	(0.00416)
L5.Cycles				-0.00238	-0.00319
				(0.00396)	(0.00363)
Observations	33029	27526	22021	16519	16519

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: **Municipalities: share of age group delayed two or more grades**

	(1) Age 9	(2) Age 10	(3) Age 11	(4) Age 12
Cycles	-0.0135***	-0.0114***	-0.0138***	-0.00570**
	(0.00193)	(0.00201)	(0.00291)	(0.00277)
L.Cycles	-0.0168***	-0.00991***	-0.0145***	-0.0121***
	(0.00194)	(0.00208)	(0.00310)	(0.00282)
L2.Cycles	-0.0131***	-0.0110***	-0.0145***	-0.0105***
	(0.00183)	(0.00230)	(0.00351)	(0.00288)
L3.Cycles	-0.00137	-0.00460**	-0.00908***	-0.00513*
	(0.00186)	(0.00214)	(0.00326)	(0.00289)
L4.Cycles		-0.00115	-0.00498	-0.00275
		(0.00203)	(0.00319)	(0.00281)
L5.Cycles			-0.00177	-0.00198
			(0.00280)	(0.00259)
Observations	27526	22021	16519	16519

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6 Extentions

The substantial effects of social promotion on grade progression the most striking findings of the paper. Not only are the effects non-trivial, they are also both sustained and cumulative. To the best of our knowledge, this is the first evidence on the medium-term effect of introducing social promotion, and it is decidedly positive. Cohorts exposed to social promotion, despite facing the threat of repetition later in their schooling, have permanently higher grade attainment through to the end of primary school. The fact that we find no evidence for increases in drop-out rates or decreases in enrolment due to the policy suggests that these figures are not simply inflated by artificially promoted students quitting at higher rates than their better-performing peers.

6.1 Heterogeneity of Impact

Which municipalities are responding most to the policy? Many municipal characteristics of interest are absorbed into the fixed effect. To compensate for this, we explore how our estimated parameter values change as we split our sample according to different criteria, and re-estimate our main results on each sample. For brevity, we present results only for strict grade delay of ten year olds.

Table 21 presents results for subsamples of the data, split into above- and below-median values of: first grade pre-policy repetition rates (from 1995; columns (3) & (4)); municipal population (in 1999; columns (5) & (6)); and municipal per capita GDP (in 1999; columns (7) & (8)). Because not all the municipalities in our sample existed in 1995, we also re-run our baseline results on the sample of municipalities for which we have 1995 repetition rates (column (2)), for comparability with columns (3) & (4). The relevant column from full sample main results is given in column (1).

It is also possible that the success of social promotion in keeping students on track will depend on the educational resources available inside the municipality. Table 22 extends the approach applied above to per capita values of total number of schools, number of municipal schools, number of state schools, and number of primary school teachers. While all of these variables do change slightly over time, this "within" variation is tiny compared to the variation across municipalities.

While the sign and size of coefficient estimates remains quite constant across most of these different specifications, the persistence of the effect drops off rapidly in some of the subsets. Most notable among these are municipalities with a high number of municipal schools per capita (Table 22, column (3)), but also small municipalities, poor municipalities, and those with high initial first grade repetition rates (Table 21, columns (3), (6) & (8)).

Table 21: **Municipalities: share of age group delayed one or more grades by subset**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baselines		1995 repetition rates		Population		GDP per capita	
	All	1995	High	Low	High	Low	High	Low
Cycles	-0.0160***	-0.0128***	-0.0127**	-0.0138***	-0.0162***	-0.0155***	-0.0154***	-0.0163***
	(0.00329)	(0.00336)	(0.00568)	(0.00410)	(0.00350)	(0.00528)	(0.00438)	(0.00498)
L.Cycles	-0.0135***	-0.0123***	-0.00804	-0.0141***	-0.00640*	-0.0197***	-0.0128***	-0.0136***
	(0.00333)	(0.00342)	(0.00602)	(0.00418)	(0.00341)	(0.00556)	(0.00426)	(0.00519)
L2.Cycles	-0.0149***	-0.0146***	-0.0136**	-0.0145***	-0.0136***	-0.0152**	-0.0180***	-0.00969*
	(0.00357)	(0.00366)	(0.00655)	(0.00448)	(0.00345)	(0.00618)	(0.00447)	(0.00585)
L3.Cycles	-0.00811**	-0.00800**	0.000749	-0.0124***	-0.00745**	-0.0101	-0.00549	-0.0109*
	(0.00352)	(0.00360)	(0.00601)	(0.00451)	(0.00359)	(0.00647)	(0.00438)	(0.00588)
L4.Cycles	-0.00528	-0.00307	-0.00341	-0.00252	-0.00563	-0.00474	-0.00408	-0.00646
	(0.00333)	(0.00336)	(0.00536)	(0.00422)	(0.00360)	(0.00571)	(0.00421)	(0.00540)
Observations	22021	19878	9929	9949	11012	11009	11010	11011

Dependent variable is the share of 11-year-olds who are one or more years delayed in their studies. Each column presents results from a fixed effect municipality-level regression on a subset of the data, as follows: (1) full dataset; (2) municipalities which existed in 1995 (baseline results to compare with cols (3-4)); above-median (3) and below-median (4) repetition rates in 1995; above-median (5) and below-median (6) municipal population; above-median (7) and below-median (8) municipal GDP per capita.

Standard errors in parentheses, clustered by municipality. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 22: **Municipalities: share of age group delayed one or more grades by subset**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Schools		Municipal schools		State schools		Primary teachers	
	High	Low	High	Low	High	Low	High	Low
Cycles	-0.0118**	-0.0188***	-0.0116**	-0.0191***	-0.0127**	-0.0194***	-0.0178***	-0.0152***
	(0.00585)	(0.00400)	(0.00565)	(0.00404)	(0.00508)	(0.00428)	(0.00492)	(0.00447)
L.Cycles	-0.00964*	-0.0154***	-0.00678	-0.0168***	-0.0173***	-0.00985**	-0.00847*	-0.0169***
	(0.00562)	(0.00413)	(0.00551)	(0.00419)	(0.00498)	(0.00447)	(0.00499)	(0.00455)
L2.Cycles	-0.0126**	-0.0153***	-0.00679	-0.0182***	-0.0169***	-0.0126**	-0.0147**	-0.0147***
	(0.00641)	(0.00436)	(0.00661)	(0.00428)	(0.00517)	(0.00502)	(0.00601)	(0.00441)
L3.Cycles	-0.00514	-0.00888**	-0.00980	-0.00630	-0.00639	-0.00939**	-0.00911	-0.00739*
	(0.00650)	(0.00421)	(0.00675)	(0.00411)	(0.00555)	(0.00458)	(0.00609)	(0.00425)
L4.Cycles	-0.00583	-0.00508	-0.00106	-0.00705*	-0.00726	-0.00342	-0.00800	-0.00313
	(0.00628)	(0.00392)	(0.00648)	(0.00385)	(0.00502)	(0.00450)	(0.00555)	(0.00411)
Observations	11008	11013	11008	11013	11009	11012	11008	11013

Dependent variable is the share of 11-year-olds who are one or more years delayed in their studies. Each column presents results from a fixed effect municipality-level regression on a subset of the data, as follows: above-median (1) and below-median (2) number of schools per capita; above-median (3) and below-median (4) number of municipal-run schools per capita; above-median (5) and below-median (6) number of state-run schools per capita; above-median (7) and below-median (8) primary school teachers per capita.

Standard errors in parentheses, clustered by municipality. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6.2 Comparisons with Previous Studies

6.2.1 Student Outcomes in Minas Gerais

In the work most closely related to our own, Koppensteiner (2014) studies the effect of introducing the cycles policy on test scores among students at state-run school in Minas Gerais. Studying such a restricted subset of schools has one important advantage: while the length and timing of cycles varied considerably among those schools adopting the policy across Brazil, Koppensteiner describes an implementation among state schools in Minas Gerais that left little school-level discretion. This decreases the measurement error in the policy variable substantially, since all schools were applying social promotion and repetition cycles along the same schedule, and allows for a more precise interpretation of the results. The shortcoming of looking only at state-run primary schools is that it prevents meaningful aggregation at the municipal level, and therefore is sensitive to students sorting themselves across schools.

Although not the focus of the paper, Koppensteiner also estimates of the effect of the policy on student flows for the two cohorts he studies. While he also finds significant, negative effects of the policy on repetition rates, in contrast to our findings these effects only appear in 2nd and 4th grades. In Appendix C.6 we replicate our student flow results using only state schools in Minas Gerais. While we find very similar estimates for 2nd and 4th grade (see Table 46), our finding of large and significant decreases in repetition rates in grade 1 (where we still see the largest effect) and grade 3 remain at odds with his results.

What could account for these differences? While the tables reported in Appendix C.6 restrict our sample to state schools in Minas Gerais, we are nevertheless estimating our model on different data sets. While Koppensteiner uses data from 2000-2006, almost identical to our own panel, he follows only two theoretical cohorts over that timeframe. In contrast, we estimate our model on all children enrolled in grades 1-8 between 1999-2006. As Koppensteiner observes, the absence of effect of the policy on repetition rates in first grade arises because the cohort in question was only treated with the policy from second grade: it should therefore be interpreted as an absence of anticipatory effects, rather than an absence of causal impact of the policy (see footnote 29, page 285). The difference in our estimates of the effect of the policy on repetition rates in grade 3 remains puzzling; however, differences in the cohorts on which the analysis was done are likely responsible.

6.2.2 The *Bolsa* Programs

How does the cycles policy compare with the *Bolsa* program? Glewwe and Kassouf (2012) study the impact of the *Bolsa escola* / *Bolsa familia* conditional cash transfer programme on enrolment, drop out and passing rates, both at the municipal and school levels. In their basic school-level model without lagged values of the policy, they find that the program increased enrolment by 2.8 percentage points for students in grades 1-4, and 3.2 percentage points for students in grades 5-8. While our enrolment regressions are at the age level rather than the grade level, we can nevertheless approximate a comparison by averaging the coefficient in Table 14 for children ages seven to ten (target ages for junior primary) and eleven-twelve (target ages from grades 5 and 6). Doing so, we find a 1.9 percentage points increase in enrolment for the junior ages, while the negative effect at age twelve dominates giving an average decrease of 0.4 percentage points. At the municipal level, Glewwe and Kassouf do not find any effect of the existence of the *Bolsa* program in the municipality on enrolment in younger grades, though they find a 4 percentage point increases in grades 5-8. We find no effect of cycles on municipality-level enrolment at ages six to twelve.[35]

Glewwe and Kassouf also find significant school-level decreases in dropout rates, and increases in promotion rates, due to the presence of the *Bolsa* program. They find that dropout rates decrease by 0.3% across primary grades, while promotion rates increase by 0.5% (in grades 1-4) and 0.3% (in grades 5-8). These estimates compare to our estimates of the effect of the cycles policy (again, averaged across grades) of 0.2% (grades 1-4) and 0.6% (grades 5-8) *increases* in dropout rates, and 3.3% (grades 1-4) and 1.1% (grades 5-8) increases in promotion rates.[36] The authors find no significant effect of the existence of the program in the municipality on promotion or dropout rates at the municipal level; we find little effect on dropout (besides an increase in first grade), but substantial increases in promotion rates at all grades except for grade 4.

While a thorough comparison of the two programs is beyond the scope of this study, it is interesting to note that the magnitude of the effects on the outcomes discussed above are in fact quite similar. The *Bolsa* programs increased enrolment and reduced dropout more noticeably than the cycles program, at least at the school level, while the cycles program increased promotion rates significantly more, in keep-

[35]We do not control for the *Bolsa* programs in our main regressions because data on the program are only available from 2001. Furthermore, by 2004 nearly every school had students on the program (see Table 50). In Appendix C.7 we replicate our main regression results while controlling for the program, and find no significant changes to our results due to the program.

[36]See Tables 52 and 51 in Appendix C.7. Two coefficients below the 10% significance level are included in these averages, though they are close to zero.

ing with the goals of each program. Nevertheless, the *Bolsa* program – somewhat surprisingly – also increased promotion rates.

7 Conclusion

Grade repetition has historically been a popular, but poorly understood, remedial education policy. In this paper, we exploit extensive variation in repetition policies in Brazil to study how the introduction of periodic social promotion affected grade attainment and annual grade progression of primary school children.

We find that the policy did indeed reduce repetition rates, particularly in younger grades, and brought about compensatory increases in promotion rates. Past exposure to the cycles policy increases repetition rates in subsequent years; however, these effects are modest and do not compensate for the reductions observed in earlier grades. We find no convincing evidence that the social promotion policy either reduced dropout, with the exception of a small reduction in first grade, or increased enrolment. Our results do suggest that considerable sorting takes place between schools in response to the policy, with social promotion attracting students in junior grades and driving them away at higher grade levels.

The policy also affected students' progress through school. We find that cohorts exposed to social promotion experience less grade-delay than their peers who face the threat of repetition every year. This improvement in age-grade matching arises mechanically from social promotion; however, the gains achieved due to the policy are highly persistent over time, and cumulate with further exposure. These gains are substantial, and appear to be driven primarily by children who would otherwise have become "serial repeaters." After being exposed to the policy from age seven, by age eleven 5.6 percentage points fewer children are one or more years delayed for their age, while 5.1 percentage points fewer children are delayed two or more years.

These results strongly suggest that, absent the social promotion policy, repetition rates in Brazilian primary schools are too high. Students progress more rapidly through primary school, and are less likely to fall behind in their studies, when exposed to episodic social promotion than when they face the threat of repetition every year. Our findings imply that a significant fraction of students who are pushed ahead by the policy – despite low achievement – manage to make up their learning shortfall before the subsequent retention year.

References

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Appendix: not for publication

A Data

A.1 Enrollment

A.1.1 Descriptive Statistics

Tables 23, 24 and 25 present municipality student enrolments in natural logs, levels and as a share of birth cohort.

Table 23: **Municipality: ln total enrolments by age**

Stats	Age 6	Age 7	Age 8	Age 9	Age 10	Age 11	Age 12
Mean	3.248	5.251	5.492	5.512	5.527	5.528	5.518
SD	1.697	1.169	1.146	1.143	1.138	1.136	1.135

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2006.

Table 24: **Municipality: total enrolments by age**

Stats	Age 6	Age 7	Age 8	Age 9	Age 10	Age 11	Age 12
Mean	81.7	484.4	617.7	627.2	633.1	635.2	631.3
SD	398.0	2542.2	3267.7	3266.6	3257.9	3275.1	3252.1

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2006.

A.1.2 Enrolled Share

To calculate the share of a birth cohort that is enrolled at any point in time, we combine student data from the *Censo Escolar* with age-specific counts from the year

Table 25: **Municipality: share enrolled by age**

Stats	Age 6	Age 7	Age 8	Age 9	Age 10	Age 11	Age 12
mean	0.2	0.9	1.0	1.1	1.1	1.0	1.0
sd	0.2	0.3	0.2	0.2	0.2	0.2	0.2

Source: Censo Escolar and INEP, authors' calculations. Mean values across municipalities from 1999-2006.

2000 Brazilian census. We do so by summing the counts of students of each age across grades, and dividing this total by the count of children of that same age from the census. While the census data allow us to have an external measure of total cohort size, several issues emerge.

First, the census reports ages of children in blocks, rather than by year. We have counts, specifically, of children aged five to six, seven to nine, and ten to fourteen. To deal with this, we use $(\frac{1}{m} * N)$ as the denominator for our enrolled share, where m is the number of ages aggregated (for the count of five and six-year-olds, $m = 2$), and N is the total count.[37]

The second issue is one of data availability. The youngest children for whom we have census data were five years old in 2000 (born in 1995), and therefore enter our analysis at the very earliest in 2001 when they turn six. For cohorts born after 1995, we cannot calculate the share of the cohort enrolled in school, because we do not have an estimate of cohort size which is exogenous to enrolment. Given that data on cycles begins in 1999, this constitutes a substantial loss of data.

Finally, although the approach outlined above should, in theory, provide an un-

[37]Note that this amounts to assuming that half of the sum of, e.g. five- and six-year-olds is an unbiased estimate of the number of five-year-olds or the number of six-year-olds.

biased estimate of the share of a birth cohort enrolled at any given time, there are substantial disparities between the population counts reported in the *Censo Escolar* and in the census. As can be seen in Table 26, enrolment figures are systematically larger than counts of children of the same age from the census, starting from age eight. The most likely explanation for this is over-reporting by schools of annual enrolment figures. Indeed, since the introduction of the *Fundo de Manutenção e Desenvolvimento do Ensino Fundamental* (FUNDEF) in 1998, transfers to municipalities for spending on primary education were tied to the number of enrolled students reported in the *Censo Escolar*. Further evidence for such an explanation comes from the fact that, when unique student identifiers were introduced the the *Censo Escolar* in 2007 - effectively making it more difficult to over-report - student numbers fell significantly.

Table 26: **Enrolled share over time by birth cohort**

Year	**1995**	**1994**	**1993**	**1992**	**1991**	**1990**	**1989**	**1988**
1999	.	.	0.20	0.86	1.04	1.00	1.04	1.04
2000	.	0.16	0.87	1.02	1.04	1.00	1.02	1.03
2001	0.17	0.87	1.04	1.04	1.05	0.99	1.01	.
2002	0.91	1.06	1.06	1.05	1.05	0.99	.	.
2003	1.08	1.08	1.07	1.05	1.04	.	.	.
2004	1.09	1.08	1.06	1.04	.	.	.	.
2005	1.09	1.07	1.05	.	.	.	.	.
2006	1.08	1.05	.	.	.	.	.	.

Source: Censo Escolar and INEP, authors' calculations. Mean values across municipalities.

We cannot test this theory directly, but we can compare municipalities which adopt cycles with those who do not. While we do not have an unbiased second estimate of cohort size, as a first check we can compare the maximal enrolment figure for a given cohort - that is, the largest number of students enrolled in any year - to our census cohort estimate. Table 27 compares the percent difference in enrolment of the 1994 cohort for municipalities with and without cycles. Note that this percent difference is constant across years (the census estimate and the maximal enrolment are time-invariant), however municipalities move between the two groups depending on their current cycles policy. While this test is in no way definitive, it gives us some confidence that the two groups of municipalities are not wildly different.

Table 27: **Percent difference between maximum enrolment and census for students born in 1994**

Year	**Cycles**	**Std. Dev.**	**No cycles**	**Std. Dev.**
1999	0.167	0.643	0.166	0.285
2000	0.167	0.637	0.166	0.282
2001	0.146	0.234	0.172	0.433
2002	0.148	0.231	0.172	0.435
2003	0.147	0.228	0.172	0.435
2004	0.163	0.617	0.168	0.289
2005	0.165	0.618	0.167	0.289
2006	0.168	0.641	0.166	0.287

Source: Censo Escolar and INEP, authors' calculations. Mean values across municipalities.

A.2 Student Flows

Table 28 shows how passing and repetition rates have varied over time for grade 1 students.

Table 28: **Municipality-level average grade 1 student flows**

Year	Pass	Drop	Repeat
1999	0.714	0.109	0.154
2000	0.699	0.114	0.157
2001	0.729	0.086	0.157
2002	0.740	0.069	0.156
2003	0.731	0.067	0.158
2004	0.737	0.069	0.167
2005	0.752	0.058	0.164

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2006.

A.3 Control Variables

Summary statistics for the list of school covariates are given in Table 29, while municipal covariates are given in Table 30. All variables are taken directly from the *Censo Escolar* and IPEA, except for *Training of teachers* (at levels 1-4 and 5-8). This last variable is an index of mean education levels of teachers teaching at the specified grade levels, coded such that 0 represents less than primary education, 1 is completed primary education, 2 is completed secondary education, and 3 is any form of tertiary training. Summary statistics on the number and education levels of teachers in the school panel are calculated conditional on having at least one teacher teaching at that level.

B The Cycles Policy

Table 29: **Time-varying school controls**

Variable	**Mean**	**Std. Dev.**	**N**
Urban	0.415	0.493	1372731
Municipal	0.704	0.456	1372731
State	0.186	0.389	1372731
Total teachers	12.122	16.92	1372731
Primary teachers	9.242	12.143	1372731
Teachers teaching 1-4	5.099	6.186	1281859
Training of teachers 1-4	2.061	0.592	1281859
Teachers teaching 5-8	14.941	11.453	428136
Training of teachers 5-8	2.679	0.402	428136

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2006.

Table 30: **Time-varying municipal controls**

Variable	**Mean**	**Std. Dev.**	**N**
Schools: number	31.092	66.018	44056
Schools: urban	12.907	59.471	44056
Schools: municipal	21.902	31.86	44056
Schools: state	5.784	20.134	44056
Schools: private	3.399	28.181	44056
Population	31874.608	190634.984	44056
Ln population	9.366	1.129	44056
Ln municipal gdp	10.566	1.392	44056
Municipal gdp	227680.017	2601262.8	44056
Teachers teaching 1-4	148.103	631.03	44056
Training of teachers 1-4	2.244	0.375	44055
Teachers teaching 5-8	144.922	768.349	44056
Training of teachers 5-8	2.654	0.334	44028

Source: Censo Escolar, authors' calculations. Mean values across municipalities from 1999-2006.

B.1 Predicting cycle adoption

Our empirical strategy relies on the assumption that there are no variables which have a time-varying effect on the coefficients of interest. While we cannot test this assumption directly, we explore it in the following ways. First, we predict municipality-level promotion policies using the baseline controls present in our main regressions. These characteristics are likely to affect both municipal policies and student outcomes; and as can be seen in Table 31, some do. This is not a problem in itself, however this can shed light what other (unobserved) factors might be a concern. The education score of upper primary school teachers is highly significant: a 1 point increase in the average education score (appx 3 standard deviations) is associated with 2 percentage points more schools in that municipality adopting the policy. If hiring more highly educated upper primary school teachers is correlated with some more fundamental (and time-varying) municipal characteristic, this would be cause for concern. Given that even this measured effect is relatively modest in size, we proceed under the assumption given above.

Table 32 also predicts the adoption of the policy, this time augmenting our baseline controls with time-interacted pre-policy repetition rates and political affiliations of the mayor and state governor.[38] We do not find evidence of significant variation over time in the effect of these controls on promotion policies; however, we nevertheless replicate our main results with these alternate sets of controls (see Appendix C.8).

[38]Mayor-president affiliations are derived from a list of parties in congress who supported the central government (available on request). Mayor-governor affiliations are based on the actual parties to which the current mayor and current governor belong.

Table 31: **Policy adoption: standard controls**

	(1) Cycles
Number of urban schools	-0.000152
	(0.000570)
Number of municipal schools	-0.000390*
	(0.000202)
Number of state schools	0.00172**
	(0.000817)
Number of private schools	0.0000285
	(0.000656)
Municipal population	6.88e-08
	(0.000000172)
Log municipal population	0.00826
	(0.0163)
Log municipal gdp	-0.0191
	(0.0116)
Municipal GDP in 000 yr-2000 Real	6.55e-09
	(9.70e-09)
Number of teachers grades 1_4	0.0000701*
	(0.0000359)
Ed score of teachers grades 1_4	0.0107
	(0.00736)
Number of teachers grades 5_8	-0.0000253
	(0.0000414)
Ed score of teachers grades 5_8	0.0213***
	(0.00825)
Observations	44027

Dependent variable is the weighted share of schools in a municipality using social promotion. Standard errors in parentheses, clustered by municipality. Additional controls (not shown): state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 32: **Policy adoption: additional controls with year interactions**

	(1) Mayor's party aligned with president	(2) Mayor and governor same party	(3) 1995 repetition rate
1999	0.0128	-0.0191*	0.0207
	(0.00901)	(0.0104)	(0.0466)
2000	-0.0109	0.00167	0.0832
	(0.00784)	(0.00921)	(0.0552)
2001	0.0136*	-0.00206	0.0462
	(0.00798)	(0.00670)	(0.0596)
2002	0.00538	0.00625	0.0511
	(0.00852)	(0.00733)	(0.0542)
2003	0.00960	-0.000191	0.122*
	(0.00820)	(0.00659)	(0.0645)
2004	0.0115	0.00335	0.0115
	(0.00806)	(0.00712)	(0.0667)
2005	0.00173	0.00502	0.212***
	(0.00805)	(0.00810)	(0.0685)
0.0207 2006	-0.00777	0.00678	(0.0466)
	(0.00760)	(0.0111)	
Observations	39755		

Dependent variable is the weighted share of schools in a municipality using social promotion. Table reports coefficients on column*row interacted variables in a single regression.

Standard errors in parentheses, clustered by municipality. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.2 Empirical description of adoption

While we do not have data on the duration and number of cycles for all years of our panel, we do have this information for some years. Tables 33 and 34 list raw descriptive statistics on the number of cycles and the duration of the first cycle, for all schools giving a non-empty answer to these questions in the 1999 *Censo Escolar*.

Table 33: **Number of cycles**

	Number	Per cent
0	99	0
1	14,641	45
2	14,778	46
3	966	3
4	1,819	6
8	2	0
14	1	0
Total	32,306	100

Source: Censo Escolar 1999, raw frequencies. Observations are at the school level.

Table 34: **Length of first cycle**

	Number	Per cent
0	60	0
1	3,933	13
2	8,385	28
3	4,640	15
4	12,938	43
5	58	0
6	1	0
8	12	0
Total	30,027	100

Source: Censo Escolar 1999, raw frequencies. Observations are at the school level.

Figure 2 shows that the share of schools in a municipality which use cycles is

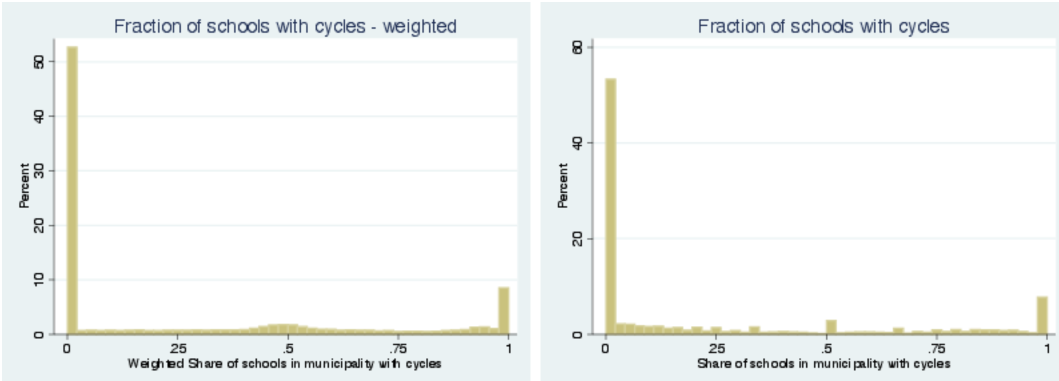

Figure 2: **Distribution of cycle frequency: municipal means**

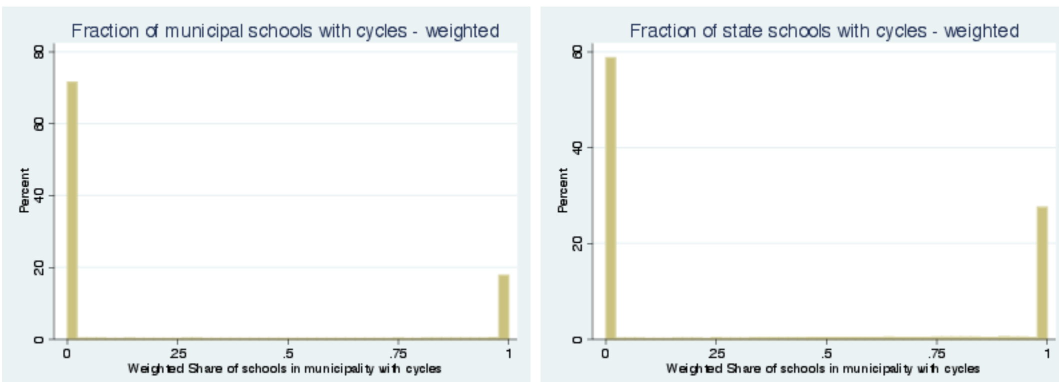

Figure 3: **Distribution of cycles: municipal vs. state schools (weighted)**

highly bimodal. Note that the data are aggregated over all years in our sample. Most municipalities make no use of cycles; those that do, however, commonly adopt entirely. This bimodality is even more pronounced when looking at school jurisdictions individually, as can be seen in Figure 3.

Table 35 lists the share of municipalities coded as 'adopters' for varying thresholds. Our primary results are shown to be robust to these different thresholds in Appendix C.5.

Table 35: **Share of municipalities adopting cycles, by threshold of use**

	10 %	**25%**	**33%**	**50%**	**66%**	**75%**	**90%**
1999	0.502	0.441	0.408	0.313	0.226	0.192	0.151
2000	0.478	0.419	0.385	0.293	0.206	0.178	0.134
2001	0.472	0.416	0.381	0.287	0.201	0.174	0.131
2002	0.467	0.414	0.382	0.286	0.197	0.166	0.120
2003	0.445	0.391	0.364	0.265	0.186	0.160	0.115
2004	0.429	0.384	0.354	0.258	0.176	0.146	0.104
2005	0.424	0.387	0.368	0.284	0.216	0.189	0.149
2006	0.371	0.338	0.320	0.238	0.176	0.154	0.117

Source: Censo Escolar, authors' calculations. Mean values across municipalities.

B.3 Geographic Variation

The popularity of cycles policies varies considerably across regions. A brief description of the general patterns follows: these overviews are based on a visual inspection of the distribution of municipality-level adoption rates for the years 1999, 2001, 2003 and 2005, for state-run and municipality-run schools separately.

Cycles in the North Municipal schools in the North have low or zero cycle adoption rates over the period. Rates are similarly low in state schools, with a few exceptions: state schools in Roraima report some cycle use (with a few municipalities registering a 100% adoption), while state schools in Tocantins have a range of adoption rates in 1999, diminishing to zero by 2003.

Cycles in the Northeast Municipal schools in the Northeast have low or zero cycle adoption rates, with the exception of Rio Grande do Norte which displays a strong bimodal distribution of municipalities: some adopt at near-census rates, while others avoid the policy entirely (rates peak in 2001-2003). Cycles are more prevalent among state schools. While half of the states have low or zero adoption, Ceará and Bahia display a 'messy' bimodal distribution (with some interior mass) from 1999-2003, and Pernambuco has such an adoption pattern in 2005 only (with no cycles prior to this). State schools in Rio Grande do Norte have a messy bimodal adoption pattern in 1999, which strengthens to a strong level of adoption in 2005 (most municipalities at 100%, and no mass at zero).

Cycles in the South Both municipal schools and state schools in the south adopted cycles at trivial rates, with the stark exception of municipal schools in Paraná. Municipal schools in Paraná display a distinctly bimodal adoption rate: most municipalities either fully adopt, or do not adopt cycles at all.

Cycles in the Southeast Municipal schools in the Southeast display a strongly bimodal distribution of adoption rates (Espírito Santo deviates slightly from this trend in 2005, with more interior points). State schools in general all adopted cycles. Exceptions to this are Minas Gerais in 2001 and 2003, and Espírito Santo in 2005, which are bimodal.

Cycles in the Centre-West The Centre-West region does not seem to follow a common trend. In Goiás, no schools adopted cycles at any point. In Mato Grosso do Sul, from 1999-2003, state schools all had cycles, while municipal schools mostly didn't, with some exceptions (including several with 100% adoption). In 2005, these rates fall to zero in both dependencies. Both municipal and state schools in Mato

Grosso display bimodal adoption rates throughout the time period, with non-trivial interior mass among state schools.

B.4 Weighting

Our primary cycles variable is computed using the share of schools in a given municipality in a given year who report using cycles, weighted by the primary school enrolment of those schools in that year. Table 36 presents a summary statistic comparison between weighted and unweighted measures.

Table 36: **Cycle prevalence: comparison of weighted and unweighted measures**

		Weighted			**Unweighted**	
Year	**Overall**	**Municipal**	**State**	**Overall**	**Municipal**	**State**
1999	0.317	0.212	0.423	0.242	0.190	0.416
2000	0.298	0.229	0.376	0.243	0.208	0.375
2001	0.293	0.233	0.372	0.240	0.212	0.372
2002	0.289	0.246	0.360	0.242	0.224	0.360
2003	0.274	0.239	0.338	0.234	0.220	0.340
2004	0.264	0.238	0.313	0.239	0.228	0.321
2005	0.286	0.241	0.364	0.248	0.232	0.364
2006	0.243	0.225	0.283	0.219	0.218	0.286

Source: Censo Escolar, authors' calculations. Mean values across municipalities.

C Robustness

C.1 Total Enrollment in Levels

Tables 37 and 38 replicate the natural log enrolment regressions (see Tables 15 and 14 in Section 5.1), this time in levels rather than natural logs. A comparison of the means and standard deviations of these variables is given in Appendix A.1. The municipality regressions suggest that a history of cycles may have some positive impact on enrolment at ages eleven and twelve, while contemporaneous cycles have a negative effect at age eight. School-level regressions in Table 38 present much the same story as the equivalent table in natural logs: increases in enrolment at junior primary ages seven to ten, and decreases at age twelve. Note that the school-level regression are conditional on enrolments at that age being positive. This maintains the same sample as the natural log enrolment regression, and prevents schools which do not offer higher grades from displaying 'negative' effects on enrolment as they reduce age-for-grade mismatches.[39]

C.2 Enrolled Share of Cohort

Increasing enrolment is in fact an indirect measure of a more fundamental goal: achieving universal enrolment of primary-school aged children. Using counts of children of different ages from the 2000 census, we replicate our municipality-level regressions using the enroled share of a birth cohort. This process, and some of the issues which arise from the measure, are described in Appendix A.1.2. Results from

[39]Consider a primary school that offers only grades 1-4: unless children are very delayed in their schooling, there should be no twelve-year-olds at that school.

Table 37: **Municipalities: total enrolments**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	-5.599	-4.639	-2.029	-2.578	-3.937*	1.248
	(14.38)	(2.993)	(2.999)	(1.943)	(2.135)	(2.162)
L.Cycles	-2.867	0.196	-0.715	0.594	-1.232	-0.338
	(7.457)	(2.996)	(3.270)	(2.123)	(2.291)	(2.774)
L2.Cycles		2.697	-1.453	3.995	6.258**	-2.139
		(2.522)	(2.589)	(2.526)	(2.740)	(2.881)
L3.Cycles			1.378	1.964	9.836*	3.683
			(2.619)	(2.924)	(5.199)	(3.530)
L4.Cycles				1.356	7.395**	7.092*
				(2.612)	(3.543)	(3.707)
L5.Cycles					-2.016	3.475
					(3.162)	(4.055)
Observations	38531	33029	27526	22021	16519	16519

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 38: **Schools: total enrolments (conditional on positive enrolment at that age)**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	0.357***	0.00423	0.186*	0.446***	0.113	-0.207
	(0.106)	(0.101)	(0.108)	(0.117)	(0.170)	(0.169)
L.Cycles	-0.109	0.197**	0.0859	0.256**	-0.318*	-0.251
	(0.100)	(0.0946)	(0.105)	(0.118)	(0.166)	(0.155)
L2.Cycles		0.0110	0.0439	0.141	0.245	0.0298
		(0.0930)	(0.101)	(0.116)	(0.172)	(0.166)
L3.Cycles			0.248**	0.122	-0.152	-0.172
			(0.0972)	(0.114)	(0.164)	(0.164)
L4.Cycles				0.169	-0.0121	-0.321**
				(0.105)	(0.153)	(0.145)
L5.Cycles					0.0600	0.242*
					(0.142)	(0.140)
Observations	960116	831636	675264	539830	390857	366025

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), total number of teachers, number of teachers at the primary level and state-year interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

age-specific regressions with enroled share of birth cohort as the dependant variable are presented in Table 39: as in our primary result, we do not find any significant pattern of effect.

Table 39: **Municipalities: share of cohort enroled**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	-0.0195	0.0131	-0.00291	0.0363	-0.0263**	0.00460
	(0.0322)	(0.0125)	(0.0123)	(0.0327)	(0.0111)	(0.00895)
L.Cycles	0.00168	-0.0237	0.00722	0.00280	0.0260	-0.00733
	(0.00875)	(0.0313)	(0.0136)	(0.0102)	(0.0285)	(0.0104)
L2.Cycles		0.00185	-0.0300	0.0124	0.00423	0.0503
		(0.00881)	(0.0354)	(0.0135)	(0.0106)	(0.0444)
L3.Cycles			0.00142	-0.0315	0.0177	-0.00450
			(0.00865)	(0.0370)	(0.0132)	(0.0113)
L4.Cycles				0.00512	-0.0129	-0.0108
				(0.00910)	(0.0343)	(0.0222)
L5.Cycles					0.00419	-0.0241
					(0.00893)	(0.0339)
Observations	16507	16507	16510	16511	16516	16516

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.3 Attainment measures

Table 40 presents results of a regression of past and present policy adoption of the share of an age cohort studying at a grade level too advanced for their age (for example, a seven-year-old studying in grade two). If the social promotion policy is implemented as expected – that is, by promoting poorly performing students who would otherwise have been retained – we would not expect the policy to significantly affect the number of students who are exceptionally advanced. Table C.3.1 reassures us that this is indeed the case.

C.3.1 Advanced students

Table 40: **Municipalities: share of age group ahead of grade**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	0.00203	0.000575	-0.000213	-0.000248	-0.000499	-0.00187
	(0.00217)	(0.00132)	(0.00147)	(0.00127)	(0.00175)	(0.00145)
L.Cycles	0.000627	0.0000211	-0.000442	-0.00145	-0.00303*	-0.00186
	(0.00196)	(0.00131)	(0.00159)	(0.00141)	(0.00177)	(0.00169)
L2.Cycles		-0.00106	0.000371	0.00126	0.00281*	-0.00205
		(0.00118)	(0.00157)	(0.00129)	(0.00157)	(0.00156)
L3.Cycles			-0.00263	-0.00424**	0.00244	0.000882
			(0.00175)	(0.00195)	(0.00237)	(0.00248)
L4.Cycles				-0.00190	0.000372	0.000425
				(0.00172)	(0.00221)	(0.00206)
L5.Cycles					-0.00212	0.000222
					(0.00182)	(0.00181)
Observations	38513	33029	27526	22021	16519	16519

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.3.2 Grade attainment

To calculate the mean grade attainment for a given birth cohort, we simply multiply the number of students of that year in each grade by the grade level, and divide by the total number of students born in that year. Equation (4) formalizes this approach, where g is a grade level, and n_g the number of students enroled at that level.

$$E^{a}_{ijt} = \frac{\sum_{1...8}^{g}(n_g * g)}{\sum_{1...8}^{g} n_g} \tag{4}$$

Table 41 gives the municipality average grade of students, by age, for each year of the sample. Note that the target grade for seven-year-olds is grade 1, and that average grade would increase by 1 each year if all students were promoted. If all student advanced on schedule, the mean grade for twelve-year-olds would be 6. While twelve-year-olds remain, in 2006, more than one year behind, there is steady improvement in this measure over the 8 years of the panel: the mean grade in 2006 is higher than in 1999 at all ages above seven.

Tables 42 and 43 present a series of regressions with the mean grade level at each age as the dependant variable. Note that this variable is calculated based only on those students enroled in school, therefore the minimum value is achieved when all students of that age who are enroled are in grade 1.

Tables 42 and 43 show that the social promotion policy had a significant and lasting impact on the average grade level of each birth cohort. The absence of an effect at age seven is unsurprising: students are normally enrolled in primary

Table 41: **Municipality-average grade attainment by age**

Year	age 6	age 7	age 8	age 9	age 10	age 11	age 12
1999	1.02	1.10	1.60	2.24	2.95	3.61	.
2000	1.02	1.08	1.62	2.29	2.99	3.71	4.39
2001	1.03	1.09	1.66	2.36	3.08	3.79	4.54
2002	1.03	1.08	1.68	2.42	3.17	3.90	4.63
2003	1.02	1.09	1.70	2.46	3.24	4.00	4.75
2004	1.02	1.09	1.70	2.48	3.27	4.07	4.84
2005	1.02	1.09	1.68	2.47	3.28	4.10	4.90
2006	.	1.09	1.67	2.46	3.29	4.12	4.94

Source: Censo Escolar, authors' calculations. Mean values across municipalities.

Table 42: **Municipalities: average grade (no lag)**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	0.00000660	0.0232***	0.0322***	0.0338***	0.0282***	0.0166***
	(0.00193)	(0.00369)	(0.00433)	(0.00451)	(0.00489)	(0.00535)
Observations	44009	44027	44027	44027	44027	38531

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 43: **Municipalities: average grade**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	0.00201	0.0233***	0.0294***	0.0300***	0.0305***	0.0154*
	(0.00228)	(0.00438)	(0.00529)	(0.00543)	(0.00733)	(0.00802)
L.Cycles	0.000395	0.0240***	0.0278***	0.0295***	0.0326***	0.0349***
	(0.00203)	(0.00429)	(0.00545)	(0.00568)	(0.00758)	(0.00786)
L2.Cycles		0.00931**	0.0296***	0.0349***	0.0391***	0.0272***
		(0.00405)	(0.00562)	(0.00594)	(0.00838)	(0.00860)
L3.Cycles			0.00464	0.00964	0.0316***	0.0243**
			(0.00529)	(0.00643)	(0.00881)	(0.0100)
L4.Cycles				0.00606	0.0209**	0.0153*
				(0.00604)	(0.00880)	(0.00869)
L5.Cycles					0.00237	0.0106
					(0.00782)	(0.00773)
Observations	38513	33029	27526	22021	16519	16519

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

school for the first time at this age, and therefore have not yet faced the possibility of repetition. From age eight onwards, both contemporary social promotion and past exposure to social promotion increase average grade level. By looking at the coefficients along the diagonal, we can see that the positive effect of past policy exposure is highly persistent over time. The coefficients are also quite similar in magnitude, regardless the age of exposure, ranging from approximately 0.01 to 0.04, with a mean around 0.03. In other words, exposure to the policy during one year increases the average grade attainment of the enrolled cohort permanently, with 3 children out of 100 at a grade level higher than they would be without the policy.

C.4 Omitting teacher controls

Table 44 replicates our main results omitting the four time-varying teacher controls used in our baseline.

C.5 Alternate Cycles Measures

As described in Section 3.2, we aggregate individual schools' adoption of the cycles policy to create a municipal-level variable. In our primary specification, we code a municipality as having adopted the policy if at least 75% of all schools in the municipality (weighted by enrolment) have done so. While the bi-modality of policy adoption rates within municipalities suggests this is reasonable, we explore several other thresholds to be sure this is the case. Table 45 presents a series of regressions where the share of delayed ten-year-olds is the dependant variable. In the regressions, the threshold for coding a municipality as having cycles varies from 10% of schools

Table 44: **Municipalities: share of age group delayed one or more grades, teacher controls omitted**

	(1) Age 8	(2) Age 9	(3) Age 10	(4) Age 11	(5) Age 12
Cycles	-0.0227***	-0.0163***	-0.0159***	-0.0132***	-0.00311
	(0.00389)	(0.00379)	(0.00328)	(0.00347)	(0.00355)
L.Cycles	-0.0237***	-0.0115***	-0.0133***	-0.0103***	-0.0113***
	(0.00378)	(0.00370)	(0.00333)	(0.00383)	(0.00362)
L2.Cycles	-0.0104***	-0.0161***	-0.0149***	-0.00768*	-0.00425
	(0.00368)	(0.00408)	(0.00357)	(0.00447)	(0.00430)
L3.Cycles		-0.00624*	-0.00816**	-0.0124***	-0.0107**
		(0.00344)	(0.00353)	(0.00451)	(0.00465)
L4.Cycles			-0.00523	-0.0128***	-0.00924**
			(0.00334)	(0.00437)	(0.00416)
L5.Cycles				-0.00267	-0.00336
				(0.00397)	(0.00364)
Observations	33041	27534	22027	16521	16521

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

having adopted, to 90%. Table 35 in Appendix B gives the share of municipalities coded as using cycles for each of these thresholds.

Not surprisingly, the effect of cycle adoption on repetition rates increases as we strengthen our definition of municipality-level adoption: fewer and fewer schools are implementing cycles, and therefore the policy is affecting fewer and fewer students. Above the 50% threshold results are strongly similar across specifications.

Table 45: **Share of 10 y.o. delayed one or more grades: alternate cycles measures**

	(1) 10%	(2) 25%	(3) 33%	(4) 50%	(5) 66%	(6) 75%	(7) 90%
Cycles	-0.00528**	-0.00631**	-0.00774***	-0.0113***	-0.0156***	-0.0160***	-0.0139***
	(0.00266)	(0.00260)	(0.00261)	(0.00263)	(0.00300)	(0.00329)	(0.00354)
L.Cycles	-0.00976***	-0.00974***	-0.0105***	-0.00886***	-0.0133***	-0.0135***	-0.0154***
	(0.00248)	(0.00260)	(0.00256)	(0.00257)	(0.00302)	(0.00333)	(0.00368)
L2.Cycles	-0.00980***	-0.00928***	-0.00911***	-0.0126***	-0.0129***	-0.0149***	-0.0178***
	(0.00257)	(0.00245)	(0.00253)	(0.00256)	(0.00326)	(0.00357)	(0.00405)
L3.Cycles	-0.00489*	-0.00533**	-0.00666**	-0.00335	-0.00970***	-0.00811**	-0.00554
	(0.00268)	(0.00256)	(0.00263)	(0.00271)	(0.00342)	(0.00352)	(0.00409)
L4.Cycles	-0.00147	0.000550	0.000465	-0.00214	-0.00380	-0.00528	-0.00907**
	(0.00243)	(0.00235)	(0.00239)	(0.00239)	(0.00302)	(0.00333)	(0.00392)
Observations	22021	22021	22021	22021	22021	22021	22021

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.6 Restricting to Minas Gerais

In order to make our findings comparable to those of Koppensteiner (2014), we extend our school-level regressions to study drop-out and repetition rates. For comparability, we restrict our analysis to the state of Minas Gerais. We estimate these regressions both for all schools, and for state-run schools only, as this last is the sample Koppensteiner studies. In all cases we do not include lagged values of the policy variable.

Tables 46 and 47 display results from regressions with repetition rates and dropout rates as outcome variables, for the state school sample. For comparison with our main results, Tables 48 and 49 display results from similar regressions using all schools in Minas Gerais. For repetition rates, the coefficients on the policy variable are in both cases substantially larger than those in our country-wide regressions, although the sign and precision of the estimates is maintained. The effect of the policy on dropout rates is also somewhat larger than in our baseline specification, though the magnitude remains modest.

Table 46: **School-level student flows in Minas Gerais state schools (no lags): repeated**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4
Cycles	-0.167***	-0.111***	-0.0718***	-0.0882***
	(0.00475)	(0.00389)	(0.00394)	(0.00362)
Observations	17562	17688	17876	18066

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Sample restricted to state schools in Minas Gerais. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), the number and education of teachers teaching grades 1-4 and year.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 47: **School-level student flows in Minas Gerais state schools (no lags): dropped**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4
Cycles	-0.00299	-0.00262	-0.00188	-0.00261
	(0.00253)	(0.00194)	(0.00201)	(0.00205)
Observations	17562	17688	17876	18066

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Sample restricted to state schools in Minas Gerais. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), the number and education of teachers teaching grades 1-4 and year.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 48: **School-level student flows in Minas Gerais (no lags): repeated**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4
Cycles	-0.163***	-0.0729***	-0.0807***	-0.0315***
	(0.00307)	(0.00267)	(0.00241)	(0.00224)
Observations	81411	80995	80426	78806

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Sample restricted to the state of Minas Gerais. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), the number and education of teachers teaching grades 1-4 and year.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 49: **School-level student flows in Minas Gerais (no lags): dropped**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4
Cycles	-0.00546***	-0.00560***	-0.00839***	-0.00503***
	(0.00192)	(0.00166)	(0.00170)	(0.00173)
Observations	81411	80995	80426	78806

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Sample restricted to the state of Minas Gerais. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), the number and education of teachers teaching grades 1-4 and year.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.7 Bolsa Escola

During our panel, the *Bolsa escola* (later *Bolsa familia*) conditional cash transfer program was rolled out across Brazil. Data on the presence of the program at a given school was first collected in 2001: by 2004, nearly every school was responding positively (see Table 50 in Appendix A.3). Glewwe and Kassouf (2012) show that *Bolsa escola* increased enrolment and promotion rates and reduced dropout. If the adoption of the cycles policy is correlated with availability of the *Bolsa* program - for instance, if some municipalities are 'early adopters' - this could confound our estimates.

Table 50: **Share of municipalities with *Bolsa* students**

Year	Mean
1999	.
2000	.
2001	0.469
2002	0.961
2003	0.983
2004	0.995
2005	0.995
2006	0.999

Source: Censo Escolar, authors' calculations. Mean values across municipalities.

To explore this possibility, we re-estimate our enrolment, promotion and dropout equations, controlling for the presence of the program in that municipality. While we present a set of school-level results for comparability (see Tables 51 and 52), we do not replicate the full set of results for schools. Given that students can sort themselves across schools in response to both the cycle policy and the Bolsa program, it would be difficult or impossible to interpret any differences that emerged. As the survey simply asked whether *Bolsa escola* exists at the school, but not how many students were eligible or enrolled, we follow Glewwe and Kassouf (2012) by using presence of the program as a binary indicator. We aggregate this at the municipality level, with *Bolsa* equal to one if any schools report the program.

Because data on *Bolsa escola* were first collected in 2001, the sample on which we estimate these equations is smaller than our baseline sample. To compare estimates with and without the Bolsa control, we first re-estimate our primary specification using only the subsample of municipalities with a valid Bolsa observation. We then estimate the same equation, with the addition of the Bolsa dummy variable.

Tables 53 and 54 present the results on enrolment. Compared to our baseline specification (see Table 15), restricting the sample to the Bolsa years does change our estimates. While our baseline specification shows no effect of the cycles policy on the natural log of enrolment, Table 53 suggests that, restricting to the sample to Bolsa years, cycles may have a negative effect on the enrolment of older children. When we compare these estimates to Table 54, however, we see that the addition of a dummy variable for *Bolsa escola* has only negligible effects on our estimated cycles coefficients: the effect is purely due to the sample restriction.

Tables 55 and 56 repeat this approach for passing rates, while Table 57 and 58 do the same for dropouts. In both cases, we observe only the slightest changes in estimates when controlling for the *Bolsa* program.

Table 51: **School-level student flows (no lags): promoted**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	0.0713***	0.0220***	0.0211***	0.0191***	0.00438***	0.0260***	-0.00436***	0.0248***
	(0.00170)	(0.00169)	(0.00156)	(0.00163)	(0.00158)	(0.00189)	(0.00165)	(0.00308)
Obs	1033310	1017858	990673	932982	353542	328867	310258	292118

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), total number of teachers, number of teachers at the primary level, number and education score of teachers teaching grades 1-4 (for columns (1)-(4)) or teaching grades 5-8 (for columns (5)-(8)), and state-year interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 52: **School-level student flows (no lags): dropped**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	0.00303***	-0.0019**	0.00541***	-0.0024***	0.00847***	-0.00011	0.0121***	0.00415***
	(0.000905)	(0.000725)	(0.000804)	(0.000798)	(0.00104)	(0.000934)	(0.00113)	(0.00105)
Obs	1033310	1017858	990673	932982	353542	328867	310258	292118

Standard errors in parentheses, clustered by school. Each column presents results from a fixed effect school-level regression. Controls (not shown): dummy for location (rural or urban), a dummy for jurisdiction (state, municipal or private), total number of teachers, number of teachers at the primary level, number and education score of teachers teaching grades 1-4 (for columns (1)-(4)) or teaching grades 5-8 (for columns (5)-(8)), and state-year interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 53: **Municipalities: ln total enrolments (no lag) - Bolsa sample**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	-0.0125	0.00411	0.00435	0.00653	-0.00867**	-0.00581
	(0.00988)	(0.00429)	(0.00510)	(0.00406)	(0.00391)	(0.00396)
Observations	33012	33029	33029	33029	33029	33029

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Sample is restricted to those municipality-years with *Bolsa* data. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 54: **Municipalities: ln total enrolments (no lag)**

	(1) Age 7	(2) Age 8	(3) Age 9	(4) Age 10	(5) Age 11	(6) Age 12
Cycles	-0.0125	0.00409	0.00433	0.00651	-0.00868**	-0.00582
	(0.00988)	(0.00428)	(0.00510)	(0.00406)	(0.00391)	(0.00396)
bolsa	0.00963	0.00752**	0.00993**	0.00644*	0.00349	0.00660**
	(0.00640)	(0.00367)	(0.00388)	(0.00338)	(0.00355)	(0.00332)
Observations	33012	33029	33029	33029	33029	33029

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 55: **Municipality student flows (no lags) - Bolsa sample: promoted**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	0.0412***	0.00315	0.0211***	-0.00007	0.0263***	0.0221***	0.0205***	0.0157***
	(0.00515)	(0.00435)	(0.00446)	(0.00425)	(0.00431)	(0.00435)	(0.00436)	(0.00506)
Obs	27517	27514	27519	27520	27522	27520	27515	27509

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Sample is restricted to those municipality-years with *Bolsa* data. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 56: **Municipality student flows (no lags): promoted**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	0.0412***	0.00313	0.0211***	-0.00009	0.0262***	0.0221***	0.0205***	0.0156***
	(0.00516)	(0.00435)	(0.00446)	(0.00425)	(0.00431)	(0.00435)	(0.00435)	(0.00506)
bolsa	0.00264	0.00424	0.00465	0.00408	0.00326	0.00310	0.00452	0.00565
	(0.00415)	(0.00338)	(0.00342)	(0.00369)	(0.00331)	(0.00333)	(0.00322)	(0.00357)
Obs	27517	27514	27519	27520	27522	27520	27515	27509

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 57: **Municipality student flows (no lags) - Bolsa sample: dropped**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	0.000607	0.00190	0.000398	-	-	-	-	-
				0.00112	0.00213	0.004**	0.00099	0.007***
	(0.00166)	(0.00135)	(0.00142)	(0.00130)	(0.00182)	(0.00181)	(0.00187)	(0.00193)
Obs	27517	27514	27519	27520	27522	27520	27515	27509

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Sample is restricted to those municipality-years with *Bolsa* data. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 58: **Municipality student flows (no lags): dropped**

	(1) Grade 1	(2) Grade 2	(3) Grade 3	(4) Grade 4	(5) Grade 5	(6) Grade 6	(7) Grade 7	(8) Grade 8
Cycles	0.000609	0.00190	0.000407	-	-	-	-	-
				0.00112	0.00213	0.004**	0.00099	0.007***
	(0.00166)	(0.00135)	(0.00142)	(0.00130)	(0.00182)	(0.00181)	(0.00187)	(0.00193)
bolsa	-	-	-	-	-	-	-	-
	0.00057	0.00104	0.00197	0.00006	0.00001	0.00028	0.0005	0.0046***
	(0.00150)	(0.00118)	(0.00128)	(0.00129)	(0.00164)	(0.00152)	(0.00167)	(0.00161)
Obs	27517	27514	27519	27520	27522	27520	27515	27509

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, and state-time interactions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.8 Inclusion of time interactions

In Appendix B.1 we demonstrated that neither our standard controls, nor some additional controls, had economically economically significant effects on the share of schools in a municipality who use the social promotion policy. Table 59 replicate our main results, using the baseline controls augmented with year-interacted political variables and 1995 repetitions rates (note that the inclusion of 1995 repetition rates restricts the sample to those municipalities which existed in that year). The additional controls and sample adjustment in Table 59 reduce the magnitude of some parameter values slightly, but does not change our main conclusions.

Table 59: **Share of age group delayed one or more grades: additional controls**

	(1) Age 8	(2) Age 9	(3) Age 10	(4) Age 11	(5) Age 12
Cycles	-0.0217***	-0.0133***	-0.0126***	-0.0126***	-0.00512
	(0.00394)	(0.00383)	(0.00336)	(0.00361)	(0.00364)
L.Cycles	-0.0245***	-0.0121***	-0.0122***	-0.00871**	-0.0131***
	(0.00386)	(0.00379)	(0.00342)	(0.00407)	(0.00378)
L2.Cycles	-0.00845**	-0.0153***	-0.0146***	-0.00568	-0.00298
	(0.00375)	(0.00393)	(0.00366)	(0.00470)	(0.00458)
L3.Cycles		-0.00566	-0.00804**	-0.0128***	-0.0101**
		(0.00352)	(0.00360)	(0.00465)	(0.00476)
L4.Cycles			-0.00280	-0.0127***	-0.0103**
			(0.00336)	(0.00449)	(0.00429)
L5.Cycles				-0.00150	-0.00434
				(0.00419)	(0.00378)
Observations	29816	24848	19878	14911	14911

Standard errors in parentheses, clustered by municipality. Each column presents results from a fixed effect municipality-level regression. Controls (not shown): the number of schools in the municipality, the number of schools by location and jurisdiction, population and municipal GDP in natural logs and in levels, the number and education scores of teachers teaching grades 1-4 and 5-8, state-time interactions, as well as year dummies interacted with mayor-president affiliation, mayor-governor affiliation and 1995 repetition rate.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.