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Performance: Evidence from Brazil**

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Abstract

This paper examines the effect of the introduction of automatic grade promotion on student performance in 1,993 public primary schools in the Brazilian state of Minas Gerais. A difference-in-difference approach that exploits variation over time in the adoption of the policy allows the identification of the treatment effect of automatic promotion. I find a negative and significant effect of about 6% of a standard deviation. Under plausible identifying assumptions the estimates can be interpreted as the disincentive effect on student effort associated with the introduction of automatic promotion.

Keywords: Grade retention, automatic promotion, incentives for effort.

JEL numbers: I28, I21, O15, H52

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1. INTRODUCTION

Grade retention,¹ the practice of holding back students in the same grade for an extra year if they fail to achieve promotion requirements, either in the form of a performance measure or in the form of minimum attendance, is used in many developing and in some developed countries. It is particularly widespread and pronounced in African and Latin American countries, where repetition rates are often as high as 30% (UNESCO 2008).² Historically grade repetition had a prominent role in Brazil and repetition rates in Brazilian primary schools reached 24% in 1st grade and 14% in 4th grade in 2005.³

Retaining students has important consequences both for the individual as well as for the school system overall. Overall, every repeater has the same effect on school resources as enrolling an additional student at that grade and subsequent grades and either leads to compromising per pupil school inputs e.g. through larger class size or to a pressure on public finances through the additional demand for teachers, classrooms, desks and other inputs.⁴

Opponents of grade repetition contend that it negatively impacts on the retained individual by stigmatizing them and harming their self-esteem, by impairing established peer relationships and generally alienating the individual from school, which may in turn negatively impact on academic achievement and increase probability of dropping-out of school (Holmes 1989). Furthermore, repeating grades delays entrance of students into the labour market which poses substantial monetary cost on students over the life-cycle. In contrast, proponents argue that repetition can improve academic achievement by exposing low performing students to additional teaching and allowing them to catch up on the curriculum and the content of teaching. This is particularly important if school absence for reasons such as illness in a specific school year is the reason for retention. Grade retention may also help to make classes more homogeneous in achievement and therefore easier to teach by improving the match between peers in the classroom.

Studies that try to tackle the selection problem and deal with the potential endogeneity of grade failure leave a rather inconclusive picture of the direction of the effect. Eide and Showalter (2001) present evidence from the 'High School and Beyond' survey on US high-

¹ In the literature also referred to as *grade repetition*.

² 40 out of 43 African countries for which data is available in 2006 use grade retention (and for which average repetition rates exceed 4% in primary school) and 18 out of 23 Latin American and Caribbean countries.

³ Data available at <http://stats.uis.unesco.org/unesco/ReportFolders/ReportFolders.aspx>. UNESCO Institute for Statistics, Data Centre, January 2008.

⁴ A very rough estimate of the annual cost of repetition on public finances in Brazil using average expenditure per pupil at primary schools in 2006 of \$554 (in constant 2005 US\$) and 18,661,000 students enrolled at primary school and an average repetition rate over all grades of 18.7% (not accounting for loss of students due to drop-out etc.) amounts to approximately 1.9 billion US\$ (all data from UNESCO 2008).

school students to assess the impact of grade retention on the probability of dropping out of high-school. They report a reduction of drop-out rates and increased labour market earnings for white students, but their IV estimates are not statistically significant at conventional levels. Dong (2009) uses a control-function approach and reports positive effects of retention on academic performance of a representative sample of kindergarten children in the U.S.

Jacob and Lefgren (2004) use a regression-discontinuity design exploiting the discontinuous relationship between test scores and promotion to estimate the causal effect of repetition for 3rd and 6th grade public school students in Chicago. They find that repetition increases achievement for third-graders in the short-run, but find no effect on 6th graders math achievement (and a negative effect for reading). Using a similar identification strategy Jacob and Lefgren (2009) examine the long-run effects of repetition on high school completion focussing again on Chicago public school students. They find that repetition has no effect on the likelihood for high school completion of 3rd grade students, but negatively affects the probability of 8th grade students to complete high school.

Manacorda (2011) provides quasi-experimental evidence for a middle-income country analysing the effect of retention on student drop-out at junior high school in Uruguay.⁵ He uses the discontinuity of the promotion rule induced by minimum attendance requirements to investigate the long-run consequences of grade failure. His findings point to long-lasting negative effects on educational attainment and an increase in drop-out rates as a result of grade retention.

Using variation across schools in test score variation Glick and Sahn (2010) show that repeaters are more likely to leave school before completing primary school in Senegal.

Considering the mixed empirical results on the effect on repeaters, the use of public resources and the undesirable consequences for public finances, the persistence of grade retention regimes in many countries is puzzling. This is particularly the case for developing countries where repetition rates are often very high and pressure on public resources is even higher. Even when assuming positive effects on achievement of repeaters, this leaves an equity question to the policy maker, as the cost associated with wide-spread retention may be levied on all students in terms of an increase in class-size and a reduction of per-pupil resources, but benefits are limited to repeaters.⁶ Furthermore, repetition increases the age variation in the

⁵ The only available attempts to assess grade retention on student outcomes for Brazil are by Gomes-Neto & Hanushek (1994) and Ferrão, Beltrão & dos Santos (2002). They nevertheless do not account for endogeneity of grade failure.

⁶ Leaving potential positive spillover effects from “treated” repeaters on the peer in the classroom unconsidered.

classroom and repeaters may also directly lead to negative externalities on their peer students (Lavy, Paserman and Schlosser 2011).

A possible explanation for the persistence of repetition regimes may be based on the deterrent effect of grade retention.⁷ Grade retention induces students to exert more effort as it potentially inflicts the high costs of repetition on low performers. This incentive effect of grade retention may be particularly important on mean outcomes on the school level, as it is not restricted to repeaters only, but may create incentives for a much wider range of students. While empirical work on grade retention focuses on the ex-post effect on repeaters, there exists – to the author’s knowledge – no research on the ex-ante effect of the repetition regime on academic outcomes of a wider set of students. In this analysis I look at a change in the promotion regime to evaluate the impact of the incentive associated with the threat of potential retention on educational achievement of 4th grade students in Brazilian primary schools. In the economics of education literature incentives have been discussed in relation to merit-based scholarships and direct cash incentives for performance (Kremer, Miguel & Thornton 2009, Henry & Rubinstein 2001, Angrist & Lavy 2009, Leuven, Oosterbeek and van der Klaauw 2009, Dynarski 2003), programmes that allocate private school vouchers to students conditional on maintaining satisfactory academic levels (Angrist et al. 2002, Angrist, Bettinger & Kremer 2006) or in relation to cash transfers conditional on satisfactory school attendance.⁸

In this study I contribute to the understanding of the ex-ante effects of retention on academic achievement and the persistence of grade retention regimes by using a unique policy experiment that allows disentangling the incentive effect from other channels through which retention may impact on student outcomes. Rather than estimating the effect of repetition on repeaters, this analysis estimates the effect of the incentive of grade retention to exert effort. More specifically, I study the effect of the introduction of automatic promotion that removes the incentive associated with the deterrent effect of grade retention in the context of public primary school students in Brazil. I exploit credible exogenous variation in the timing of the adoption of automatic promotion in the Brazilian state of Minas Gerais to estimate the effect of removing the deterrent of grade retention on standardized math test scores.

⁷ Manacorda (2008) is the first to point out such a deterrence effect of retention in the literature. A related argument of a deterrence effect is discussed by Angrist et al. (2002) in relation to school vouchers and by Jacob (2005) in relation to high stakes testing in the US.

⁸ As in programmes like Bolsa Família (formerly Bolsa Escola) in Brazil (Bourguignon, Ferreira & Leite 2003) or PROGRESA in Mexico (Schultz 2004).

I find that the introduction of automatic promotion significantly reduces academic achievement measured by math test scores of 4th graders by 6% of a standard deviation. Under plausible assumptions I argue for the interpretation of the results as the disincentive effect of the introduction of automatic promotion.

The remainder of the paper is organized as follows. Section 2 provides information on the school system in Brazil and in the state of Minas Gerais. Section 3 presents the data. Section 4 describes the natural experiment and outlines the assignment of schools to treatment. Section 5 introduces the empirical strategy. The results, their interpretation and falsification exercises are presented in section 6, and section 7 concludes.

2. THE SCHOOL SYSTEM IN BRAZIL AND MINAS GERAIS

Primary school is compulsory in Brazil for children between the ages of 7 to 14 and consists of eight years of schooling (MEC 1996).⁹ Public schooling is free at all ages and enrolment in primary and secondary school is open to students of all ages.

The Brazilian educational system has undergone substantial changes during the last two decades and has achieved considerable progress in expanding access to education. Starting from a primary school net enrolment rate of only 85% in 1991, Brazil achieves today almost universal primary school enrolment with a net rate of 95% (UNESCO 2009). Primary school completion and youth literacy rates have improved notably, but the country continues to suffer from high repetition and drop-out rates.¹⁰

The national conditional cash transfer programme *Bolsa Família*, formerly *Bolsa Escola*, which is a means-tested monthly cash transfer to poor households conditional on school enrolment and regular attendance among other conditions, plays a significant role for the rise in school enrolment and attendance of school age children (de Janvry, Finan & Sadoulet 2006).¹¹

This analysis focuses on the state of Minas Gerais, the second most populous state in Brazil with an estimated population of about 19 million (IBGE 2007). Minas Gerais contributes 10% to the Brazilian GDP and is among the most developed states in Brazil (OECD 2005).

⁹ The school entry age has been lowered recently to 6 years and primary school has been extended to 9 years.

¹⁰ The overall repetition rate in primary schools in Brazil in 2006 was 18.7% and the total drop-out rate for primary school 19.5% (UNESCO 2009).

¹¹ The conditionalities of *Bolsa Família* require a minimum school attendance of 85% and extend to the fulfilment of basic health care requirements such as vaccinations of the children and pre and postnatal medical consultations for pregnant women. Monthly per capita income in the household cannot exceed R\$120 (US\$57 in 2006) to remain eligible for the transfer. See Lindert et al. 2007 for a comprehensive description of the programme.

The education system of Minas Gerais is among the most developed and in national performance tests students continue to be among the top 5 states (INEP 2007).

According to state legislation, the State Secretariat of Education (SEE) has extensive authority to plan, direct, execute, control and evaluate all educational activities in Minas Gerais. Based on far-reaching decentralization of education in Brazil, the SEE transfers authority to a large extent to Regional Authorities for Education (Superintendências Regionais de Ensino: SREs) and directly to the municipalities. SREs and municipalities therefore play a major role in the provision of schooling and the implementation of educational policies.¹² Municipal schools account for more than half (56%) of all primary schools and state schools, that are directly under the control of the SEE, account for 22% of all schools. Outside the public education system private schools play an important role and account for the remaining 22%.¹³

3. DATA DESCRIPTION

This study uses data from two sources. Information on school characteristics comes from the annual Brazilian school census that is conducted by the National Institute for the Study and Research on Education (INEP) under the control of the Federal Ministry of Education (MEC). The Brazilian school census compiles data annually from all primary and secondary schools in Brazil. The exceptionally rich data includes information on the location and administrative dependence of schools, physical characteristics (quantity of premises and class rooms, equipment and teaching material), the participation in national, state and municipal school programmes, the number of teachers and administrative staff, average class-size, detailed information on student flows (number of students in each grade according to age, repetition, drop-out and student transfer rates) among other information. Summary statistics for the public schools used in this analysis are presented in panel A of table A1 in the annex.

The school census also contains the information on the regime of grade promotion adopted in each school (grade retention versus automatic promotion), which is used to establish treatment and control groups.

The second part of the data comes from the State System of the Evaluation of Public Education (Sistema Mineiro de Avaliação da Educação Pública: SIMAVE), which includes

¹² The installation of FUNDEF, a federal fund established in 1996 with the aim of redistributing state and municipal resources back to (mainly) municipalities according to student numbers contributed to the improvement of the control of municipalities over educational decisions. See de Mello & Hoppe (2005) for an analysis of FUNDEF.

¹³ There are also 28 federal schools in Brazil which are under the direct control of the federal government; the single federal school in Minas Gerais has not been included in the dataset.

the programme for the evaluation of primary and secondary schools (Programa de Avaliação da Educação Básica: PROEB).

The main outcome variable is student achievement in state schools in Minas Gerais measured by math test scores in 2003 and 2006. All classes and all students in 4th grade of each school are examined and participation of schools and pupils is compulsory. The cognitive test scores are standardized to a mean of 500 and a standard deviation of 100. In total 246,959 students have been tested in 1,993 state schools in Minas Gerais. I use the repeated cross-section of test score data from 2003 and 2006 for this analysis. The students in the dataset have, as generally students in public schools, a deprived socioeconomic background. Almost half (45.6%) of the families with children at state schools in Minas Gerais qualify for Bolsa Família and can be considered poor. Information on sex, date of birth, racial background and on the socio-economic family background also is available from an adjunct questionnaire. Chart B of table A1 presents summary statistics on these variables.

4. THE GENERAL EDUCATION ACT OF 1996: THE CASE OF A QUASI- EXPERIMENT

4.1 Policy background

The General Education Act of 1996 (Lei de Diretrizes e Bases da Educação Nacional: LDB) paved the way for the introduction of automatic promotion policies in Brazil. Federal Law No 9.394/1996, which came into effect in 1998, regulates the responsibility for education between the federal, state and municipal level and facilitated federal and state programmes to control the grade promotion regime (Pino & Koslinski 1999). Section 3 of Art. 32 §1&2 formally distinguishes two alternatives for educational authorities to organize student progression: besides the conventional annual grade repetition regime the option of automatic promotion was introduced, a system in which students progress automatically to the next grade at the end of the school year. Between these two extremes, a mixture of both regimes was also permitted. In the mixed regime, schools define “learning cycles” that stretch over several – most commonly three - school years. During the initial years of the cycle students are promoted automatically. In the final year of a cycle students that do not meet the minimum requirements set in the curriculum are retained. The idea behind these learning cycles is to allow students an individual studying pace (Mainardes 2004). If students fall behind their classmates they have a longer period to catch up on the curriculum. This particularly aims at reducing the long-run impact of negative temporary shocks, such as school days lost to sickness or adverse family events. With the mixed regime schools that

have adopted automatic promotion in learning cycles, grade retention is not entirely eliminated, but limited to the final years of the cycles. The LDB furthermore sets fundamental criteria on how to organize promotion under any one regime: In every school year a minimum attendance of 75% of all school days must be fulfilled as a general requirement for promotion, so that grade retention is still permitted in exceptional cases where students fail to achieve 75% minimum attendance rule.

According to the legal framework of the LDB the decision on the promotion regime and its exact specifications is taken on the state level. Automatic promotion was introduced at an early stage by the states of São Paulo, Minas Gerais and Paraná, and to some extent in the state of Pernambuco and by the federal resolution SE No 4, 1/98 in all federal schools in Brazil. Only very recently a federal resolution has been passed to disallow retention for the first three school years in all schools in Brazil to take effect from 2011.

In the state of Minas Gerais the new regime has been established by state resolution No. 8.086 in 1997. It stresses the autonomy of each public school in the decision whether to continue with the annual repetition regime or to introduce automatic promotion. I use the variation in the repetition regime this freedom in the choice of the regime creates over time and across schools for identification of the effect of the policy change. In the year 2000 1,449 out of 1,993 state schools had established automatic promotion with two initial three-year cycles. At the beginning of the school year 2004 the remaining 544 state schools switched to automatic promotion.

4.2 Assignment to treatment

I take schools that adopted automatic promotion at the beginning of the year 2004 as the treatment group and schools with automatic promotion (which have adopted automatic promotion since the year 2000) as the control group. I focus on two cohorts of 4th graders, which I call the test cohorts 2003 and 2006 for which test scores are available. Chart 1 presents an overview of these two cohorts and the change in the organization of promotion for the control and treatment group.

When using this division into treatment and control group for comparison a sound understanding of the assignment process that leads to this division is essential. In the case of state schools in Minas Gerais the 46 regional authorities for education were asked to propose a plan of implementation of automatic promotion for the schools under their administration. The decision for early adoption of the policy was made by each SRE in agreement with the state secretariat. The second wave was initiated by the SEE in an attempt to make automatic

promotion universal for all schools. As the adoption of the policy is not randomized across schools in an experimental setting, treatment and control schools may not be balanced in the distribution of school and mean pupil characteristics. Although the identification strategy employed in this analysis does not rely on the distribution of covariates being balanced, it is generally reassuring to find school and mean pupil characteristics of treatment and control group to be very similar. Table A1, chart A and B present descriptive characteristics of treatment and comparison schools for 2003 and 2006. T-tests (and Chi-square for categorical variable) for the equality of means between treatment and comparison group, accounting for clustering on the SRE level, reveal only very few small but statistically significant differences. As sample size is partly reflected in the t-statistics, it is more useful to look at the

$$\text{normalized difference } \textit{norm-diff} = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{S_{x,0}^2 + S_{x,1}^2}}$$

scale-free measure of the balancing properties of the covariates (Imbens & Wooldridge 2009). The normalized difference is small for all covariates and never exceeds the absolute value 0.25,¹⁴ suggesting that treatment and control schools are in fact extremely similar in terms of their physical school characteristics. Even more importantly, the normalized differences for mean student characteristics, which may indicate compositional differences of the student populations, are all very small and are far below the suggested rule-of-thumb value of |0.25| in both years. Apart from mean age, which differs slightly as expected,¹⁵ no other variable reveals any considerable difference at the mean. The overlap in the covariate distributions can also be examined by looking at the distribution of the propensity score for the treatment and control group. Figure 1 shows the propensity score for the probability of treatment for the treatment and control group revealing substantial overlap in the multivariate distribution of covariates and a relatively similar pattern of the distribution of the propensity score for the treatment and control group.¹⁶

In addition, I estimate a linear probability model to determine whether there are systematic differences between schools that have adopted automatic promotion at different points in time. The results are presented in table A5. The coefficients on the set of school characteristics are generally small and only very few are statistically significant. When

¹⁴ This is a rule of thumb suggested in Imbens & Wooldridge 2009 to check the unconfoundedness assumption for the use of linear regression in estimating average treatment effects.

¹⁵ Mean age is expected to differ as part of the treatment, which will be clarified in a later.

¹⁶ A formal test under the null for the equality of the distribution (Kolmogorov-Smirnov) of the propensity score is nevertheless rejected.

including SRE controls even fewer variables show a significant effect and it is difficult to establish any systematic pattern.

Given the similarity of treatment and control schools with respect to the distribution of school characteristics and the student composition, it is plausible to consider the assignment of schools to treatment and control groups as conditionally random.

5. EMPIRICAL STRATEGY

To estimate the treatment effect of the policy change I use a difference-in-difference (DiD) estimator exploiting the variation in treatment status of schools over time, identifying an average treatment effect on individuals at schools assigned to treatment. The double difference approach is capable of removing biases resulting from permanent latent differences between treatment and control as well as biases resulting from common trends over time. The estimation in a regression setup allows including additional regressors on the individual and school level to improve precision and to test for the presence of omitted-school specific trends, in particular related to potential changes in the student composition. Identification requires that trends in student outcomes at treated and control schools would not be systematically different in the absence of treatment.

Under this identifying assumption, I estimate the effect of the introduction of automatic promotion on test scores of 4th graders by the following regression model:

$$Y_{ist} = \beta_0 + d_s + d_t + \gamma d_{st} + \beta_1 Z_{it} + \beta_2 X_{st} + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is the test score for individual i in school s at time t , d_s is a school dummy which captures school-specific time invariant effects, d_t is a time dummy which captures the common time trend of control and treatment group, d_{st} is the time/treatment-status interaction term containing information on treatment status of the schools, that varies over time. γ in equation (1) is the coefficient of interest and reflects the average treatment effect of the introduction of automatic promotion on test scores of 4th graders. Z_{it} is a set of covariates controlling for individual characteristics. X_{st} denotes a set of exogenous covariates for class and school characteristics, including average socioeconomic characteristics of students, detailed school characteristics,¹⁷ the participation in federal, state and municipal educational

¹⁷ Specifically, the covariates include initial (1st grade) enrolment, number of teachers at school, number of total staff (besides teaching staff), dummy variables describing the type of the premises used for the school, dummies for the availability and number of teaching material (e.g. overhead projectors, personal computers, TV and video sets etc.), the availability of computer and science labs, school kitchen, the quality of sanitary units, number of

programmes¹⁸, teacher characteristics and other.¹⁹ ε is a stochastic error term. Although non-random assignment of schools to treatment may lead to a correlation between assignment status and outcomes, this does not violate the common trend assumption as long as any differences that lead to the adoption of the policy are captured by the school-fixed effects. The common trend assumption may nevertheless be violated if selection into treatment was based on pre-treatment trends in school characteristics that differ between treatment and control. If, for example, schools with high performing pupils and low repetition rates adopt automatic promotion test scores and treatment status are correlated for reasons other than the treatment impact of automatic promotion. Unfortunately I do not have pre-treatment test score data to test directly for the common trend assumption. I nevertheless can investigate whether selection into treatment is based on pre-treatment differences in repetition rates. Table A2 reveals that pre-intervention repetition rates (from the 1997 school census before automatic promotion was introduced at any school) were virtually identical across treatment and control schools, so that there is little concern for self-selection of schools into treatment based on high or low repetition rates. As I have pointed out earlier, the first wave of the policy adoption was initiated on the SRE level, which furthermore limits the potential for individual schools to select into treatment based on trends in test scores. The second wave was then determined by the decision of the CEE made for all remaining schools, so that there is virtually no scope for selection on a pre-treatment trend basis.

As the treatment regressor varies at the school level and test scores of students in the same school are likely correlated, for example because they share the same learning environment and are from the same neighbourhood, conventional standard errors may be misleading as they do not account for the grouped error structure. The robust standard errors reported therefore allow for clustering on the school level (Donald & Lang 2007).

6. ESTIMATION RESULTS

class rooms in- and outside the school and dummies for whether the school provides all 8 years of primary education.

¹⁸ These programmes include National Minimum Income Programme, Free School Lunch programme, the provision of public school transportation, TV escola (a national education TV programme), other educational TV programmes, computer literacy programmes, and other state and municipal school programmes.

¹⁹ This will also allow accounting for eligibility specific effects (Ashenfelter 1978). This way the above time invariant composition assumption can be relaxed to accommodate for the case where treatment and control group are expected to differ in covariates that may affect the outcome variable.

6.1 Main results

The basic idea of the difference-in-difference strategy can be illustrated by a simple 2-by-2 table. Table 1 shows the levels and differences in test scores between treatment and control groups and the changes over time. The first row reports means before treatment (year=2003), when control schools were already under the automatic promotion regime and the treatment schools were still under the annual grade repetition regime and the mean difference for the two groups. The entries in the first column reveal that schools that have already adopted automatic promotion have a mean score that is 7.05% of a standard deviation lower than schools that had not yet adopted the new regime in 2003. After the adoption of automatic promotion by schools of the treatment group this difference almost completely disappears and students at both groups have very similar average test scores and the difference in means is not statistically significant. Likewise, schools in the control group have very similar mean test scores over time with a difference that is not significantly different from zero. The lower right entry reports the simple difference-in-differences estimates, which can be interpreted as the causal effect of treatment under the above identifying assumptions. The adoption of automatic promotion leads to a decrease in test scores of 6.65% of a standard deviation. Almost the entire fraction of the difference-in-difference outcome originates from the pre-treatment difference between control and treatment schools. After the adoption of automatic promotion in treatment schools the difference between treated and control schools almost completely disappears.

This first difference-in-difference result can be amended in a regression framework according to equation (1) to improve precision of the estimates and to be able to control for covariates and check the sensitivity of the estimates to their inclusion. Table 2 presents the estimates for different sets of controls. All specifications include school fixed effects and year dummies. School fixed effects capture stable unobserved characteristics of the schools and year dummies pick up common trends in the test scores that are not related to treatment. Specification (1) of table 3 includes no additional controls, in specification (2) school characteristics are included as controls, specification (3) controls for school and peer characteristics and specification (4) also includes individual level covariates. The estimates in all specifications reveal a stable negative effect of around 6% of a standard deviation and are very precisely estimated (1% level of significance). Adding school level and peer controls reduces the negative effect, but the reduction is relatively small. Controlling additionally for individual characteristics delivers an estimated effect of virtually the same size as in specification (1). The results reveal that the regime change from annual grade retention to

automatic promotion has a significant negative impact on educational attainment on 4th graders in state schools in Minas Gerais. In the next section I will discuss the interpretation of the results.

6.2 Interpretation of the results and the disincentive effect of automatic promotion

To understand the ex-ante treatment effect of grade retention, one needs to understand how this effect is related to the incentive to exert effort. In table 3, column 1 reports the difference-in-difference estimates of the treatment on repetition rates for grades 1-4. This is done by following the theoretical exam cohorts of 2003 and 2006 over grades 1-4. The bottom entry for column 1 shows how the introduction of automatic promotion reduces the repetition rate in 4th grade by 0.086. Prior to the policy change, about 10% of all students in treatment schools repeated 4th grade, but only about 2% did so after the introduction of automatic promotion.²⁰ In this analysis I am interested in understanding the effect of the drop of the repetition rate on educational achievement of 4th graders that arises from the elimination of the threat of repetition. The two cohorts of students at treatment schools face very different incentive structures. The 2003 cohort is subject to grade retention, while the 2006 cohort does not face the threat of potential grade retention. For the interpretation of the estimates as disincentive effect, any possible channel of effects of the policy reform on outcomes - other than the disincentive effect - has to be precluded. Most importantly, potential changes in the composition of students in treatment and control schools over time could have an effect on mean educational outcomes that is not due to the change in the setting of incentives.

Table 5 reports estimates on a range of mean socioeconomic variables on the school level on a treatment dummy. For each outcome variable I have fitted a separate regression including school fixed effects and year dummies. Only the coefficients on the mean number of fridges per household and on mean age are statistically significant. All other indicators of the socioeconomic composition are not affected by the introduction of automatic promotion, which is reassuring. While the coefficient for the mean number of fridges per household is very small and may be due to some spurious correlation, the significant reduction in mean age by about one month is more relevant and it is important to understand the source of this reduction in age and its consequence for the interpretation of the result.

²⁰ Repetition rates stay above zero because repetition is still possible when failing to achieve 75% minimum school attendance.

This reduction in age is caused by the difference in the inflow of repeaters in 4th grade at the treatment schools before and after treatment. Whereas treatment schools still received an inflow of repeaters from 4th grade of the previous year at the beginning of the year 2003, there was no such inflow of repeaters in 2006, which leads to the reduction in mean age, as repeaters are on average one year older. Table 6, column 2 shows the DiD estimate of the policy change on the net inflow of students from 1st to 4th grade and from 1st to 3rd grade in column (1). Whereas the coefficient in column (1) is very small, negative and not statistically significant, the coefficient for the net inflow of students including the inflow of repeaters from the previous year at the beginning of 4th grade is sizeable, positive and very precisely estimated (column (2)). Looking at the direct effect of the inflow of the repeaters on mean age of the cohort shows that this almost exactly explains the age effect estimated in table 5.²¹ As this means that the composition is altered due to the treatment it is important to understand the potential bias of the compositional change on mean achievement.

Even assuming a positive effect of repetition on educational outcomes of repeaters²², it is very plausible to assume that average performance of repeaters is still below the mean performance of non-repeaters in the test cohort, as repeaters are selected as the lowest performers in 4th grade in the preceding year.²³ How does this differential inflow affect the outcome variable of interest? As there was an inflow of such low performing students in 2003, but not in 2006 the results for the disincentive effect of automatic promotion are, if anything, possibly biased downwards and the reported coefficients in table 2, chart A need to be regarded as a lower bound of the true effect. Unfortunately, there is no direct information in the student questionnaire on whether and when students were retained. I can nevertheless use individual age to single out repeaters to some extent. A regression sensitivity analysis that includes individual age as a control variable may give a basic idea about the size of the bias of the differential inflow of repeaters. Adding individual age to specification (1) leads to an increase in the negative effect of about 20% to -7.97% of a standard deviation compared to 6.65 % without controlling for age, reported in chart B of table 2. Controlling for individual age in specification (2) and (3) leads to a very similar increase of 20% of the effect to -7.33% and -6.77%, respectively. An alternative way of investigating the importance of the bias for all specifications is to restrict the estimations to students that have never repeated by excluding all students outside the target age range of 4th graders. By doing students from the

²¹ Assuming that they are about one year older the inflow of repeaters at 4th grade leads to a decrease of mean age of the cohort of 36 days compared to the estimated effect on mean age of 39 days.

²² And a direct effect related to age, as repeaters are one year due to repeating the grade.

²³ This is confirmed by the findings elsewhere; see Manacorda (2008) for example.

additional inflow at 4th grade from the sample are removed, leaving a sample with students that have never repeated.²⁴ Chart A of table 4 reports the results for the same specifications as in table 3, but restricts the sample to students in the target age range for 4th graders. By restricting the sample in this way the coefficients exceed the estimates of the original full sample in all specifications by around 30%. The estimated effect is a further 11-16% larger compared to the estimates in chart B of table 2. Restricting the sample to repeaters (chart B, table 4) reveals a negative effect that is considerably smaller and no longer statistically significant. The number of excluded students is nevertheless larger than what could be explained by excluding 4th grade repeaters only. Removing overage students from the sample also removes students that have repeated at 3rd grade. As repetition is equally possible in all schools at 3rd grade, the additional increase in the estimates is therefore not necessarily related to treatment. The increase rather suggests that the incentive of grade retention may have a different impact on previous repeaters compared to students that have never repeated a grade. The cost of repetition is likely highest for students that have not previously repeated. In contrast marginal cost of being retained again is diminishing for previous repeaters, as they may already have suffered stigmatization and have already been separated from their original peer group. The difference in results for the restricted sample therefore may not only reflect the correction for the differential inflow of repeaters at 4th grade, but may also more generally reflect heterogeneous effects on repeaters and non-repeaters.

A more comprehensive analysis of the sensitivity of the estimates to the inclusion of age controls is provided by table A3 in the annex. I present different specifications of equation (1) with and without controlling for individual age for the full sample (chart A) and the age restricted sample in chart B. The results support the previous findings. Adding individual age as control (columns 4, 6 and 8) strengthens the negative effect in the full and restricted sample for all the different specifications.

Besides the direct effect on the composition, there may be indirect effects of having repeaters in the class room on their peers. Repeaters may impose a negative externality on their peers because their achievement is lower or because they may be more disruptive in class. Lavy, Paserman and Schlosser (2011) elaborate on the extent of ability peer effects associated with repeaters and show that academic performance and behaviour of repeaters may be responsible for the negative effect. By adding peer age in the DiD specification I can control for potential

²⁴ Nevertheless I cannot distinguish repeaters from students that have enrolled late at first grade. With rather strict enforcement of the enrolment age in Minas Gerais and the incentives to parents to enrol their children based on Bolsa Família conditions, late enrolment is very limited.

peer effects from the differential inflow at 4th grade. Adding peer age as control only moderately increases in size the coefficients in specification 1 and 2 in chart C of table 3. Columns 2 and 5 in charts A and B of table A3 reveal that the inclusion of peer age only has a minor effect when controlling for other peer variables and does not strengthen the estimates of the treatment effect suggesting that there is no noteworthy bias on the. If anything, a negative peer effect of repeaters, as suggested in the literature, would lead to a downward bias of the disincentive effect.

Conditioning on individual age and restricting the sample to non-repeaters reveals that the differential inflow at 4th grade changes the composition of students in a way that underestimates the true impact of the disincentive effect by not taking into account the net inflow of repeaters into 4th grade in 2003. The size of the downward bias ranges between 20 and 30%. The estimates for the restricted sample should nevertheless be considered with caution, as the disincentive of automatic promotion may have differential impact on previous repeaters and non-repeaters.

Under the above identifying assumptions and excluding other channels that could drive the results, I interpret the above estimates as the effect of the disincentive of the introduction of automatic promotion and the original estimates of around 6% as a lower bound for the true effect. I will discuss the relevance of other potential channels of effects of the policy change on academic achievement in detail later.

6.3 Could the estimated effects be caused by channels other than the disincentive effect?

The interpretation of the above estimates as disincentive effect requires the exclusion of any other channel through which the introduction of automatic promotion affects academic achievement. Similarly to the above presented alteration of the student composition in treatment and control schools over time that is related to 4th grade retention there could be other causes for compositional changes. The following section also provides some tests for other effects of the policy change that could impact on achievement.

6.3.1 Introduction of automatic promotion at 2nd grade

Because of the introduction of automatic promotion at treatment schools at 2nd grade of the cohort of interest, this potentially may also have an impact on the composition of students. Table 1 shows how repetition rates from 1st to 4th grade of the theoretic test cohorts are affected by the policy introduction. The estimates for first and third grade show no effect of the treatment as expected. Rates at first grade are unaffected with the change of the repetition

regime only in the subsequent year and third grade rates are unaffected as the final year of the cycle (3rd grade) remains with grade retention for both cohorts in treatment and control group. The estimate for the impact on the second grade reveals how the policy introduction lowers the repetition rate by almost 12% at second grade in 2004. The potential threat to the interpretation of the results arises from the fact that by introducing automatic promotion at 2nd grade for the 2006 exam cohort, this cohort may be “contaminated” by low performers that would have been removed in the absence of treatment. The mean repetition rate for 2nd grade at treatment schools drops from 12.8% (2003 exam cohort) to 3.1% (treatment cohort). Rather than looking at 2nd grade repetition only, in- and outflows in each grade up to the end of 3rd grade have to be taken into account when examining the effect of the adoption of automatic promotion on the student composition. Looking at the overall student flows of the test cohorts reveals that the negative selection has largely cancelled out up to when the test cohort enters 4th grade in 2006. In particular, repetition at the consecutive 3rd grade plays an important role here. The first column of table 7 reports the effect of the policy introduction on the net flow taking into account in- and outflows over grades 1-3. The net inflow due to the introduction of automatic promotion is very close to zero and not statistically significant. This is mainly based on two factors: Focussing only on treatment schools, table A4 shows that repetition rates at 3rd grade actually increased by about 4.3% for the 2006 exam cohort, which filters out a substantive fraction of the low-performers already. Furthermore, 3rd grade repetition rates for the two cohorts have to be compared with caution, as these may have a different impact on removing low-performers from the previous year depending on the inflow into 3rd grade at the beginning of the year. Considering net-flows, the 2003 and 2006 cohorts are nearly unaffected in terms of their composition at the beginning of 4th grade. As mentioned earlier, the socioeconomic composition between the cohorts (table 5) is almost completely unaltered due to treatment, which supports the findings, that the policy introduction does not change the composition of students up to 4th grade.

This is also corroborated by the fact that almost the entire fraction of the DiD result arises from the ex-ante difference between treatment and control group in 2003, rather than from the difference after treatment. The results for the simple difference over time of the control schools and the difference between control and treatment schools after treatment in 2006 are very small and not significant at conventional levels.

6.3.2 Effect of the policy change on drop-out rates

Elsewhere in the literature the effect of retention on student drop-out has been studied how (see Jacob and Lefgren 2004, 2009, Manacorda 2011). If the introduction of automatic promotion has an effect on drop-out rates in grades prior to 4th grade, this may change unobserved student characteristics that cannot be controlled for. To test for an effect of the policy change on drop-out rates I estimate its effect on drop-out rates in a DiD specification similar to equation (1) as $y_{s,t} = \beta_0 + d_s + d_t + \gamma d_{s,t} + \varepsilon$ (2) using aggregated data from schools. Column (2) of table 3 reports the coefficients for each grade. Drop-out rates in 2nd grade are unaffected by the policy change.²⁵ The treatment nevertheless has a small effect on drop-out rates at 3rd grade, by reducing the drop-out rate by half a percent. This is equivalent to a mean reduction of 0.31 students per school/cohort and presumably negligible in its potential impact on student outcomes.

6.3.3 Effect of the policy change on school transfer rates

Another potential source for a compositional change is related to the possibility of students to change their school. Parents expecting a negative effect of automatic promotion on their children may want to move their children to a school with grade retention. In Minas Gerais the possibility for switching public schools is very limited, as enrolment is based on residence and parents cannot choose freely between different public schools. Given very substantial fees at private schools it is also unlikely that parents move their child to a private school to circumvent a specific grade promotion regime. To test for any effect of the policy change on between-school mobility I estimate the effect of the policy adoption on student transfer rates using the same framework as in the previous section. Column (3) of table 3 shows that there is no significant effect of the regime change on transfer rates for any grade including grades 2, 3 and 4, and an effect of differential student mobility on the outcome variable can be ruled-out.

6.3.4 Systematic test taking behaviour

Although participation in PROEB is mandatory on the school and individual level, some students fail to attend the test.²⁶ If the propensity to show up at the exam is related to the

²⁵ 1st grade repetition rates are also unaffected as predicted, because the policy change only takes effect after 1st grade. This is a relevant observation as it shows that there are no anticipatory effects from schools to the introduction to the policy change.

²⁶ The participation rate for the 2003 and 2006 wave of PROEB is around 95% as participation is strictly enforced and absence is only permitted in case of illness.

capacity of the student and to the treatment status of the school, this might bias the estimates. This might be induced by strategic behaviour of school administrators or teachers trying to manipulate the mean test scores of their school in the PROEB exam. If this is systematically linked to treatment status this could bias the estimates. I use information from the official student numbers in each school from the school census and compare these to the number of students participating in PROEB. I estimate the above regression (2) using the difference between the two figures as outcome variable. Column (4) of table 3 presents the results from the regression. The coefficients are very small (.12 students) and are not significant so that there is no evidence for systematic absence from the exam or manipulation by schools related to treatment status.

6.3.5 Effect of the policy change on class size

Besides compositional effects the potential reallocation of resources within schools that are induced by the policy change need to be considered. With a reduction in retention rates, class-size may be affected. There is a comprehensive literature on the effect of class size on student performance and the overall picture about class-size effects remains rather unclear.²⁷ To rule out that the estimates are biased by an indirect effect of the policy on class-size I test for an effect of treatment on class-size for each grade for the cohort of interest in the above framework. Column (4) of table 3 reports the results for the DiD regressions. There is no significant effect of the policy change on class-size in any grade, so that estimates on test scores are unlikely biased by effects of the treatment on class-size.

Even under the assumption that the introduction of automatic promotion releases other school resources that could be allocated to 4th grade students (for which there is no evidence in the present analysis) this would lead to underestimating the true impact of the disincentive created by automatic promotion.

The fact that none of the above estimates (for repetition rates, drop-out rates, class-size, transfer rates) reveal any significant effect for first grade estimates is in itself an important falsification exercise. All these estimates are based on a pseudo-treatment as the first grade of the 2006 exam cohort was not yet affected by the policy introduction that took place only at 2nd grade. This also shows that there are no anticipatory effects of the schools in respect to the imminent introduction of automatic promotion that may affect student outcomes at a later stage.

²⁷ See Hoxby (2000) and Angrist & Lavy (1999) for two prominent studies on class size effects.

7. CONCLUSIONS

Existing empirical work on grade retention has to date focused on analysing the direct effect of retention on repeaters. The focus on the ex-post effect may nevertheless neglect an important effect of the grade retention regime on incentives to exert effort on a much larger range of students. The introduction of automatic promotion removes the incentive previously linked to the threat of retention and I use exogenous variation in the implementation of the policy over time in state primary schools in the Brazilian state of Minas Gerais to obtain causal estimates of the disincentive effect from the introduction of automatic promotion measured by the impact on standardized math test scores.

Using a difference-in-difference approach I find a negative effect of 0.06 of a standard deviation, significant at the 1% level. Controlling for individual age strengthens the negative effect by about 20%, which gives an idea about the size of the bias associated with the differential inflow of repeaters into 4th grade before and after treatment.

The estimated disincentive effect of the introduction of automatic promotion appears to be of moderate size. Considering the potential for an accumulative nature of the negative effect over several grades, the overall impact of the automatic promotion regime may lead to considerable loss of academic achievement over the eight years of primary school.

The estimates of the disincentive effect related to removing the deterrent of repetition close a gap in the literature on the effects of grade retention and help to explain the persistence of repetition regimes in many countries. Grade retention reduces internal flow efficiency at schools and is a costly policy, but creates a positive effect on academic achievement through the deterrence of retention. The findings are also important because they reveal that a large fraction of students is affected by the grade promotion regime. Rather than focusing only on the effect on repeaters, attempts to assess the cost and benefit of retention therefore need to take into account as well the effects on non-repeaters.

The results are particularly relevant because of the universal introduction of automatic promotion in all primary schools in Brazil coming into effect by federal legislation in 2011. Although the Brazilian experience may not be completely transferable to other countries with often lower repetition rates, the findings may nevertheless be relevant for the discussion of automatic promotion, often referred to as social promotion in the United States, in these countries and may provide policy makers with a more complete picture of the potential effects of changes in the repetition regime.

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FIGURE 1: HISTOGRAM FOR THE PROPENSITY SCORE (YEAR 2003)

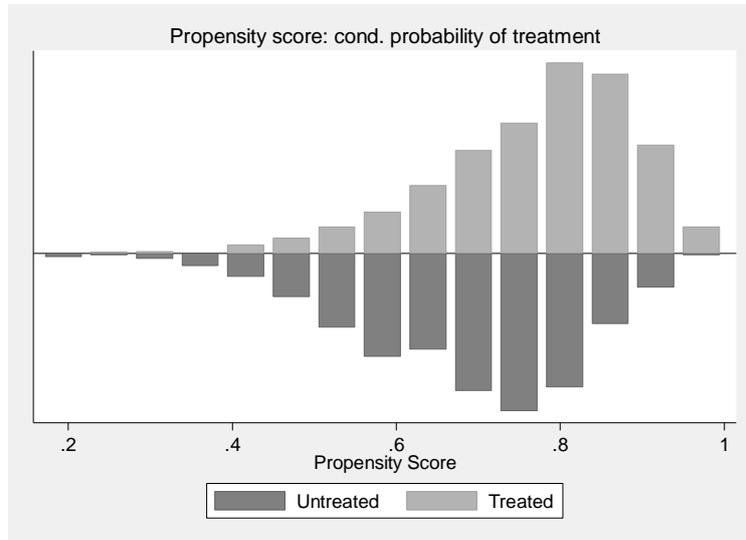


CHART 1: TREATMENT SCHEME

					PROEB test
		1 st grade	2 nd grade	3 rd grade	4 th grade
Grade	exam cohort				
Treatment schools	2003 year	1	1	1	1
	2006 year	1	0	1	0
	2000 year	2000	2001	2002	2003
	2003 year	2003	2004	2005	2006
Control schools	2003 year	0	0	1	0
	2006 year	0	0	1	0
	2000 year	2000	2001	2002	2003
	2003 year	2003	2004	2005	2006

Notes: Testing takes place for all pupils at the end of 4th grade. The cohorts are denoted according to the year in which they are tested through PROEB. 1 denotes grades with grade retention, 0 denotes grades with automatic promotion.

TABLE 1: TEST SCORE MEANS IN TREATMENT AND CONTROL SCHOOLS BEFORE AND AFTER THE ADOPTION IN THE TREATMENT SCHOOLS

	Before treatment	After treatment	Change in mean test scores
Control schools	498.48 (1.55)	498.99 (1.51)	-0.51 (2.01)
Treatment schools	505.53 (2.71)	499.39 (2.59)	6.14 (2.97)
Difference in mean test scores	-7.05 (3.12)	-0.40 (2.99)	-6.65 (3.22)

Notes: Mean outcomes for treatment and control before and after treatment. Standard errors, adjusted for clustering within SREs, are reported in parenthesis.

TABLE 2: MAIN ESTIMATION RESULTS AND SENSITIVITY TO AGE CONTROLS

Dependent variable: PROEB math test scores				
Observations: 244,081, number of clusters: 1,993				
	(1)	(2)	(3)	(4)
Chart A				
Treatment effect	-6.65*** (2.03)	-6.13*** (2.00)	-5.67*** (1.98)	-6.24*** (2.04)
R-squared	0.00	0.01	0.01	0.17
Chart B – adding individual age control				
Treatment effect	-7.97*** (2.16)	-7.33*** (2.11)	-6.77*** (2.04)	-6.24*** (2.04)
R-squared	0.05	0.04	0.04	0.17
Chart C – adding peer age control				
Treatment effect	-6.99*** (2.06)	-6.46*** (2.02)	-5.67*** (1.98)	-6.24*** (2.04)
R-squared	0.00	0.01	0.01	0.17
School fixed effects	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
School level controls	no	yes	yes	yes
Peer characteristics controls	no	no	yes	yes
Individual characteristics controls	no	no	no	yes

Notes: *** denotes significance at 1%. Robust standard errors, adjusted for clustering within schools, are reported in parenthesis. Specification (1) contains year dummies and school fixed effects, specification (2) additionally controls for a rich set of school characteristics (physical characteristics of the school and the class rooms, teaching material, teacher characteristics, participation in educational programmes etc.), specification (3) additionally controls for peer socio-economic characteristics at the school level and specification (4) also controls for individual characteristics.

TABLE 3: EFFECT OF THE ADOPTION OF AUTOMATIC PROMOTION ON STUDENT FLOWS AND CLASS-SIZE

Dependent variable:				
	Repetition rate	Drop-out rate	Transfer-rate	Class-size
	(1)	(2)	(3)	(4)
Grade 1	-0.010 (0.010)	0.001 (0.003)	-0.005 (0.007)	0.692 (0.363)
Grade 2	-0.118*** (0.010)	-0.002 (0.003)	0.002 (0.006)	0.828 (0.422)
Grade 3	-0.019 (0.011)	-0.005** (0.002)	-0.002 (0.003)	0.502 (0.416)
Grade 4	-0.086*** (0.007)	-0.005 (0.004)	-0.007 (0.004)	0.252 (0.336)

Number of schools: 1993, years 2000-2006, average cohort size: 61.24

Notes: *** denotes significance at 1%, ** denotes significance at 5%. The coefficients report the effect of introducing automatic promotion on the dependent variables for 1st to 4th grade using data from the school census 2000-2006 following the theoretical test cohorts. For each grade a separate regression has been fitted estimating the effect corresponding to equation (1) as $y_{st} = \beta_0 + d_s + d_t + \gamma d_{st} + \varepsilon_{st}$. The regression estimates are weighted by school cohort size and include year dummies (d_t) and school fixed effects (d_s). Robust standard errors, adjusted for clustering within 46 SREs, are reported in parenthesis.

TABLE 4: ESTIMATION RESULTS FOR RESTRICTED AGE RANGES

Dependent variable: PROEB test scores				
Number of clusters: 1,993				
	(1)	(2)	(3)	(4)
Chart A – students in target age range for 4 th grade				
Treatment effect	-8.67*** (2.33)	-8.07*** (2.26)	-7.50*** (2.23)	-7.22*** (2.25)
R-squared	0.00	0.01	0.01	0.13
Observations	149,223			
Chart B – repeaters (outside target age range)				
Treatment effect	-3.78 (2.53)	-3.42 (2.50)	-2.89 (2.50)	-3.29 (2.42)
R-squared	0.01	0.01	0.00	0.08
Observations	88,657			
School fixed effects	yes	yes	yes	yes
Year dummy	yes	yes	yes	yes
School level controls	no	yes	yes	yes
Peer characteristics controls	no	no	yes	yes
Individual characteristics controls	no	no	no	yes

Notes: *** denotes significance at 1%. The above samples exclude students that are below the target age range. Robust standard errors, adjusted for clustering within schools, are reported in parenthesis. Specification (1) only includes year dummies and school fixed effects, specification (2) additionally controls for a rich set of school characteristics (physical characteristics of the school and the class rooms, teaching material, teacher characteristics, participation in educational programmes etc.), specification (3) additionally controls for peer socio-economic characteristics at the school level and specification (4) also controls for individual characteristics.

TABLE 5: EFFECT OF THE ADOPTION OF AUTOMATIC PROMOTION ON THE SOCIO-ECONOMIC COMPOSITION

Dependent variables:	Coefficient	Robust standard error
Proportion of white students	0.010	(0.008)
Proportion of mixed students	-0.009	(0.012)
Proportion of black students	0.003	(0.005)
Proportion of Asian students	-0.004	(0.003)
Proportion of indigenous students	-0.000	(0.003)
Mean age (in years)	-0.106***	(0.022)
Mean male students	-0.005	(0.007)
HH wealth index	0.007	(0.025)
Bathroom mean	0.018	(0.009)
TV mean	0.004	(0.007)
Video mean	0.007	(0.012)
Radio mean	-0.010	(0.012)
Fridge mean	0.016**	(0.007)
Freezer mean	-0.014	(0.019)
Washing machine mean	0.012	(0.010)
Car mean	0.011	(0.009)
Computer mean	-0.004	(0.008)
Books mean	0.026	(0.014)

n=1993

Notes: denotes ** significance at 5% level, *** significance at 1% level. All estimates refer to school means or proportions at the school level. All data is taken from the socio-economic questionnaire of PROEB. For each dependent variable the effect is estimated separately in a regression corresponding to equation (1) as $y_{st} = \beta_0 + d_s + d_t + \gamma d_{st} + \varepsilon_{st}$. The regression estimates are weighted by school cohort size and include a year dummies (d_t) and school fixed effects (d_s). Robust standard errors, adjusted for clustering within SREs, are reported in parenthesis. All estimates are weighted by school cohort size.

TABLE 6: EFFECT OF THE ADOPTION OF AUTOMATIC PROMOTION ON PARTICIPATION IN PROEB

Dependent variable: difference between official student numbers and Proeb participation numbers			
Coefficient	R-squared within	R-squared between	R-squared overall
0.119 (0.976)	0.594	0.020	0.038

Notes: The coefficient reports the effect of the introduction of automatic promotion on the difference of the number of students according to the school census and the PROEB test. The effect is estimated by a regression corresponding to equation (1) as $y_{st} = \beta_0 + d_s + d_t + \gamma d_{st} + \varepsilon_{st}$. The estimates are weighted by school cohort size and include year dummies (d_t) and school fixed effects (d_s). Robust standard errors adjusted for 46 clusters (on SRE level) are reported in parenthesis.

TABLE 7: EFFECT OF POLICY ADOPTION ON STUDENT NET FLOW

	(1)	(2)
	Student net inflow up to 1 st – 3 rd grade	Student net inflow including 4 th grade
Coefficient	-0.010 (0.015)	0.079*** (0.014)
R-squared	0.55	0.59

Notes: *** denotes significance at 1%. The coefficients report the effect of introducing automatic promotion on net flow (including in/outflow due to repetition using data from the school census 2000-2006. A separate regression has been fitted estimating the effect corresponding to equation (1) as $y_{st} = \beta_0 + d_s + d_t + \gamma d_{st} + \varepsilon_{st}$ for the two models. Model (1) refers to net flows including 2nd and 3rd grade, model (2) refers to net flows including 2nd, 3rd and 4th grade. The regression estimates are weighted by school cohort size and include a year dummy (d_t) and school fixed effects (d_s). Robust standard errors adjusted for 46 clusters (on SRE level) are reported in parenthesis.

9. ANNEX

TABLE A1: MEAN CHARACTERISTICS OF TREATMENT AND CONTROL GROUPS IN 2003 AND 2006
CHART A: PHYSICAL CHARACTERISTICS AND SCHOOL PROGRAMME PARTICIPATION

	2003						2006					
	Control		Treatment		P-value	Norm-diff	Control		Treatment		P-value	Norm-diff
n=1993	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Rural school	0.885	(0.319)	0.890	(0.313)	0.788	-0.011	0.885	(0.319)	0.895	(0.307)	0.544	-0.023
State property	0.918	(0.275)	0.880	(0.325)	0.020	0.089	0.907	(0.290)	0.881	(0.324)	0.106	0.060
Municipal property	0.062	(0.242)	0.093	(0.290)	0.035	-0.082	0.066	(0.249)	0.090	(0.287)	0.098	-0.063
Private property	0.020	(0.141)	0.067	(0.161)	0.414	-0.220	0.024	(0.154)	0.027	(0.163)	0.696	-0.013
School ownership	0.901	(0.298)	0.866	(0.340)	0.040	0.077	0.907	(0.290)	0.881	(0.324)	0.106	0.060
Rented school	0.014	(0.118)	0.020	(0.140)	0.393	-0.033	0.010	(0.100)	0.09	(0.138)	0.164	-0.469
Donated school	0.085	(0.278)	0.114	(0.317)	0.068	-0.069	0.082	(0.275)	0.100	(0.300)	0.261	-0.044
Shared school	0.197	(0.398)	0.238	(0.426)	0.060	-0.070	0.197	(0.398)	0.197	(0.398)	10.00	0.000
Principal office	0.875	(0.331)	0.858	(0.350)	0.324	0.035	0.865	(0.342)	0.874	(0.332)	0.625	-0.019
Admin. office	0.909	(0.287)	0.937	(0.243)	0.036	-0.074	0.950	(0.219)	0.952	(0.214)	0.845	-0.007
Teacher room	0.813	(0.390)	0.811	(0.392)	0.912	0.004	0.827	(0.379)	0.843	(0.363)	0.382	-0.030
School kitchen	0.829	(0.377)	0.824	(0.381)	0.808	0.009	0.821	(0.384)	0.821	(0.384)	0.997	0.000
Refectory	0.374	(0.484)	0.440	(0.497)	0.010	-0.095	0.412	(0.493)	0.464	(0.499)	0.046	-0.074
Food storage	0.839	(0.368)	0.856	(0.352)	0.368	-0.033	0.732	(0.443)	0.718	(0.450)	0.533	0.022
Computer lab	0.237	(0.426)	0.149	(0.356)	0.000	0.159	0.302	(0.460)	0.193	(0.394)	0.000	0.180
Science lab	0.165	(0.372)	0.142	(0.349)	0.205	0.045	0.143	(0.350)	0.120	(0.325)	0.176	0.048
Other lab	0.022	(0.147)	0.021	(0.145)	0.920	0.005	0.020	(0.141)	0.018	(0.133)	0.767	0.010
Toilets outside	0.054	(0.227)	0.094	(0.291)	0.006	-0.108	0.0443	(0.206)	0.070	(0.254)	0.045	-0.079
Toilets inside	0.980	(0.141)	0.967	(0.178)	0.150	0.057	0.992	(0.089)	0.981	(0.136)	0.101	0.068
Freezer	0.899	(0.301)	0.920	(0.271)	0.144	-0.052	0.907	(0.290)	0.914	(0.280)	0.633	-0.017
Filtered water	0.845	(0.362)	0.825	(0.380)	0.299	0.038	0.881	(0.324)	0.858	(0.350)	0.183	0.048

Table A1 cont.

Video cassettes	1.757	(1.000)	1.763	(1.190)	0.911	0.000	1.501	(0.936)	1.551	(1.368)	0.444	-0.030
TV sets	2.038	(1.066)	2.025	(1.324)	0.541	0.024	2.107	(1.021)	2.108	(1.353)	0.980	-0.001
Projectors	0.851	(0.544)	0.858	(0.535)	0.815	-0.009	0.867	(0.545)	0.876	(0.530)	0.741	-0.012
Printers	1.790	(2.200)	1.517	(2.040)	0.002	0.110	2.370	(2.295)	1.948	(1.961)	0.000	0.140
Pentium computers	2.757	(4.866)	1.919	(3.915)	0.000	0.151	3.050	(5.053)	2.239	(4.435)	0.001	0.121
386/486 computers	0.656	(2.486)	0.428	(1.469)	0.014	0.079	1.032	(3.109)	0.737	(2.249)	0.022	0.077
Perm. class rooms	10.069	(4.857)	10.060	(4.343)	0.320	0.035	10.651	(5.439)	10.533	(4.403)	0.625	0.017
Prov. class rooms	0.193	(0.581)	0.150	(0.456)	0.092	0.058	0.165	(0.459)	0.172	(0.516)	0.794	-0.010
Class rooms	9.487	(5.188)	9.205	(4.195)	0.223	0.042	10.010	(5.255)	9.760	(4.198)	0.282	0.037
Total staff	49.535	(30.385)	46.612	(25.796)	0.037	0.073	51.553	(31.624)	48.281	(25.749)	0.021	0.080
Teachers	32.177	(20.045)	30.243	(16.885)	0.035	0.074	32.441	(20.651)	30.207	(16.330)	0.014	0.085
Min inc. program	0.598	(0.491)	0.545	(0.498)	0.043	0.076	0.970	(0.171)	0.967	(0.176)	0.778	0.012
TV escola	0.732	(0.443)	0.751	(0.432)	0.401	-0.031	0.495	(0.500)	0.564	(0.496)	0.008	-0.098
Other education TV	0.237	(0.426)	0.304	(0.460)	0.004	-0.107	0.117	(0.321)	0.150	(0.358)	0.062	-0.069
PROINFO	0.199	(0.400)	0.126	(0.332)	0.000	0.140	0.171	(0.377)	0.124	(0.330)	0.009	0.094
State programmes	0.314	(0.465)	0.206	(0.404)	0.000	0.175	0.247	(0.432)	0.225	(0.418)	0.309	0.037
Munic. programmes	0.091	(0.287)	0.102	(0.303)	0.449	-0.026	0.175	(0.380)	0.170	(0.376)	0.787	0.009
School transport	0.527	(0.500)	0.487	(0.500)	0.118	0.057	0.722	(0.448)	0.668	(0.471)	0.025	0.083
Initial enrolment	72.630	(52.326)	69.245	(50.132)	0.197	0.047	38.276	(30.966)	41.202	(32.187)	0.077	-0.066
Classes in 1 st grade	2.537	(1.640)	2.359	(1.542)	0.030	0.079	1.682	(1.259)	1.766	(1.308)	0.215	-0.046
Classes in 2 nd grade	2.410	(1.420)	2.425	(1.581)	0.854	-0.007	1.793	(1.295)	1.963	(1.324)	0.013	-0.092
Classes in 3 rd grade	2.348	(1.428)	2.470	(1.567)	0.125	-0.058	2.163	(1.449)	2.123	(1.420)	0.587	0.020
Classes in 4 th grade	2.334	(1.460)	2.469	(1.571)	0.080	-0.063	2.082	(1.289)	2.076	(1.314)	0.919	0.003

Notes: The binary variables of school characteristics and programme participation are coded 0 for not present (no participation) and 1 for present (participation). All data is from the Brazilian school census 2003 and 2006. The p-value is reported from a test on the equality of the mean between the treatment and control groups (independent samples). As the sample size is sufficiently large the result for using a classical t-test or taking into account the binary values and the underlying binomial distribution deliver very similar results. As the group size and with it the variances between the groups differ, approximate t using individual sample variances instead of the pooled variance and Welch's approximation of the degrees of freedom have been used.

The normalized difference is computed as $norm-diff = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{S_{X,0}^2 + S_{X,1}^2}}$, where S^2 denotes the sample variance of X_i .

TABLE A1: MEAN CHARACTERISTICS OF TREATMENT AND CONTROL GROUPS IN 2003 AND 2006
 CHART B: SOCIO-ECONOMIC BACKGROUND AND AVERAGE COHORT CHARACTERISTICS AT SCHOOL LEVEL

	2003						2006					
	Control		Treatment		P-value	Norm-diff	Control		Treatment		P-value	Norm-diff
	Mean	SD	Mean	SD			Mean	SD	Mean	SD		
Male	0.492	(0.500)	0.495	(0.500)	0.414	-0.004	0.490	(0.500)	0.498	(0.500)	0.028	-0.011
Age (in months)	135.43	(14.53)	132.01	(12.72)	0.000	0.177	134.53	(13.73)	132.22	(12.46)	0.000	0.125
% white pupils	0.302	(0.459)	0.333	(0.471)	0.000	-0.047	0.316	(0.465)	0.318	(0.466)	0.479	-0.003
% mixed pupils	0.340	(0.474)	0.354	(0.478)	0.000	-0.021	0.420	(0.494)	0.428	(0.495)	0.021	-0.011
% black pupils	0.118	(0.323)	0.121	(0.326)	0.125	-0.007	0.155	(0.362)	0.146	(0.363)	0.000	0.018
% Asian pupils	0.034	(0.181)	0.034	(0.181)	0.935	0.000	0.045	(0.206)	0.043	(0.203)	0.250	0.007
% indig. pupils	0.042	(0.200)	0.043	(0.203)	0.324	-0.004	0.046	(0.210)	0.046	(0.209)	0.001	0.000
Bathroom	1.267	(0.560)	1.270	(1.291)	0.000	-0.002	1.263	(0.583)	1.276	(0.585)	0.001	-0.016
TV	1.323	(0.792)	1.321	(0.773)	0.762	0.002	1.495	(0.788)	1.487	(0.775)	0.171	0.007
Video	0.370	(0.483)	0.374	(0.484)	0.134	-0.006	0.606	(0.689)	0.605	(0.687)	0.816	0.001
Radio	1.468	(0.796)	1.440	(0.790)	0.000	0.025	1.360	(0.752)	1.341	(0.738)	0.000	0.019
Fridge	0.954	(0.468)	0.978	(0.468)	0.000	-0.036	0.995	(0.493)	1.002	(0.466)	0.054	-0.010
Freezer	1.945	(0.228)	1.940	(0.237)	0.003	0.015	1.937	(0.242)	1.932	(0.252)	0.002	0.014
Clothes washer	0.746	(0.435)	0.781	(0.414)	0.000	-0.058	0.924	(0.601)	0.944	(0.583)	0.000	-0.024
Car	0.621	(0.855)	0.660	(0.862)	0.000	-0.032	0.641	(0.814)	0.672	(0.810)	0.000	-0.027
Computer	0.170	(0.376)	0.176	(0.381)	0.022	-0.011	0.254	(0.435)	0.258	(0.437)	0.229	-0.006
Books	21.867	(27.328)	22.070	(27.612)	0.311	-0.005	20.870	(27.850)	20.471	(27.664)	0.037	0.010

Notes: All data is taken from the socio-economic questionnaire of PROEB 2003 and 2006. The p-value is reported from a test on the equality of the mean between the treatment and control groups (independent samples). As the sample size is sufficiently large the result for using a classical t-test or taking into account the binary values and the underlying binomial distribution deliver very similar results. As the group size and with it the variances between the groups differ, approximate t using individual sample variances instead of the pooled variance and Welch's approximation of the degrees of freedom have been used.

The normalized difference is computed as $norm - diff = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{S_{X,0}^2 - S_{X,1}^2}}$, where S^2 denotes the sample variance of X_i .

TABLE A2: MEAN PRE-INTERVENTION REPETITION RATES IN 1997

1997						
n=1,993	Control		Treatment		P-value	Norm-diff
	Mean	SD	Mean	SD		
Pre-treatment repetition rate 1 st grade (1997)	0.08	(0.108)	0.08	(0.106)	0.98	-0.001
Pre-treatment repetition rate 2 nd grade (1997)	0.16	(0.208)	0.17	(0.200)	0.37	-0.034
Pre-treatment repetition rate 3 rd grade (1997)	0.05	(0.053)	0.05	(0.143)	0.63	-0.014
Pre-treatment repetition rate 4 th grade (1997)	0.06	(0.072)	0.06	(0.093)	0.21	-0.044

Notes: All data is from the Brazilian school census 1997. The p-value is reported from a test on the equality of the mean between the treatment and control groups (independent samples). As the sample size is sufficiently large the result for using a classical t-test or alternatively taking into account the binary values and the underlying binomial distribution deliver very similar results. As the group size and with it the variances between the groups differ, approximate t using individual sample variances instead of the pooled variance and Welch's approximation of the degrees of freedom have been used.

The normalized difference is computed as $norm - diff = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{S_{X,0}^2 + S_{X,1}^2}}$, where S^2 denotes the sample variance of X_i .

TABLE A3: SENSITIVITY OF ESTIMATES TO INDIVIDUAL AND PEER AGE CONTROLS FOR DIFFERENT AGE RANGES

Dependent variable: PROEB test scores								
Chart A – all pupils								
	Peer controls		Peer and individual controls				Individual controls	
	excl. peer age	incl. peer age	excl. ind. and peer age	incl. age, excl. peer age	incl. peer age, excl. ind. age	incl. peer and ind. age	excl. ind. age	incl. ind. age
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment effect	-5.65*** (1.98)	-5.67*** (1.98)	-6.02*** (1.98)	-6.47*** (2.04)	-5.78*** (1.99)	-6.24*** (2.04)	- 6.76** *	-6.87*** (2.08)
R-squared	0.01	0.01	0.12	0.17	0.12	0.17	0.12	0.16
Observations: 244,081								

Chart B – students in target age range for 4 th grade								
	Peer controls		Peer and individual controls				Individual controls	
	excl. peer age	incl. peer age	excl. ind. and peer age	incl. age, excl. peer age	incl. peer age, excl. ind. age	incl. peer and ind. age	excl. ind. age	incl. ind. age
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
treatment effect	-7.47*** (2.23)	-7.50*** (2.23)	-7.30*** (2.25)	-7.46*** (2.25)	-7.11*** (2.25)	-7.22*** (2.25)	- 7.73** *	-7.96*** (2.26)
R-squared	0.01	0.01	0.13	0.13	0.13	0.13	0.12	0.12
Observations: 149,223								

Notes: *** denotes significance at 1%. All estimates include controls for school characteristics (physical characteristics of the school and the class rooms, teaching material, teacher characteristics, participation in educational programmes etc.) The specifications (1) include additionally controls for peer socio-economic characteristics, specifications (2) control for peer and individual characteristics, specifications (3) control for individual characteristics. The row below specifies further controls for individual and peer age in the estimation.

TABLE A4: EFFECT OF POLICY ADOPTION ON 3RD GRADE REPETITION RATE FOR TREATMENT SCHOOLS

3 rd grade repetition	
Coefficient	0.0428*** (0.0099)
R-squared	0.019

Notes: *** denotes significance at 1%. The coefficient reports the effect of introducing automatic promotion on 3rd grade repetition rate for the cohort of interest of treatment schools.

TABLE A5: LINEAR PROBABILITY MODEL OF ASSIGNMENT TO TREATMENT

School provides all years of fundamental education	0.061**	(0.030)	0.067**	(0.027)
School characteristics				
rural school	0.004	(0.039)	0.001	(0.036)
proper school building	0.070	(0.099)	0.100	(0.099)
church building	-0.152**	(0.070)	-0.128	(0.076)
teacher home	-0.156	(0.175)	-0.289	(0.170)
building of other school	0.013	(0.061)	-0.016	(0.053)
farm building	-0.111	(0.120)	-0.037	(0.113)
other building	-0.107	(0.059)	-0.119**	(0.053)
state property	-0.271	(0.240)	-0.130	(0.233)
municipal property	-0.380	(0.233)	-0.210	(0.229)
private property	-0.348	(0.237)	-0.227	(0.227)
school property	-0.061	(0.070)	-0.076	(0.059)
rented property	0.034	(0.109)	0.045	(0.097)
shared school	-0.038	(0.023)	-0.052**	(0.021)
principal office	0.007	(0.033)	-0.023	(0.030)
secretarial office	-0.067	(0.042)	-0.083**	(0.040)
school library	-0.008	(0.030)	0.016	(0.027)
reading room	-0.029	(0.052)	-0.087	(0.046)
teacher room	-0.004	(0.030)	-0.001	(0.027)
video library	-0.031	(0.041)	-0.050	(0.039)
TV room	0.006	(0.024)	0.021	(0.022)
school kitchen	0.016	(0.029)	-0.018	(0.027)
cafeteria	0.008	(0.024)	0.011	(0.021)
refectory	-0.045**	(0.021)	-0.023	(0.020)
stationary	-0.025	(0.024)	0.003	(0.022)
computer laboratory	0.017	(0.042)	0.012	(0.039)
science laboratory	0.011	(0.031)	0.003	(0.029)
other laboratory	0.018	(0.061)	-0.005	(0.053)
toilets outside school	-0.088**	(0.036)	-0.053	(0.033)
toilets inside school	0.023	(0.055)	-0.013	(0.052)
toilets ready for special needs	0.108	(0.083)	0.144	(0.076)
school ready for special needs	-0.062	(0.071)	-0.022	(0.062)
industrial oven	0.021	(0.089)	0.056	(0.088)
home oven	-0.022	(0.030)	-0.009	(0.029)
wood oven	0.052	(0.048)	0.016	(0.037)
freezer	-0.052	(0.038)	-0.056	(0.034)
filtered water	0.051**	(0.026)	-0.007	(0.024)
internet access	0.017	(0.033)	-0.012	(0.030)
public energy supply	-0.044	(0.247)	-0.049	(0.162)
solar energy	-0.229***	(0.071)	-0.100	(0.086)
using 220 volt	-0.020	(0.040)	-0.008	(0.037)
using 110 & 220 volt	-0.063**	(0.029)	-0.010	(0.029)
public water supply	-0.025	(0.089)	0.061	(0.077)
artesian well	-0.082	(0.086)	0.003	(0.076)
cistern water	-0.003	(0.073)	-0.021	(0.063)
no running water	-0.093	(0.250)	-0.075	(0.267)

	spring water	-0.060	(0.105)	-0.034	(0.093)
	public sewerage	0.055	(0.303)	0.125	(0.240)
	septic tank	-0.070	(0.303)	0.098	(0.241)
	no sewerage	0.121	(0.319)	0.135	(0.250)
Number of	video tapes	-0.012	(0.014)	-0.010	(0.013)
	TV sets	-0.006	(0.012)	-0.004	(0.012)
	printers	-0.001	(0.009)	0.002	(0.008)
	overhead projectors	-0.043**	(0.021)	-0.015	(0.020)
	Pentium computers	0.008	(0.004)	0.004	(0.004)
	386/486 computers	0.013**	(0.006)	0.013**	(0.006)
	permanent class rooms	-0.015	(0.009)	-0.007	(0.008)
	provisory class rooms	0.019	(0.023)	0.015	(0.022)
	class rooms in school	0.017	(0.010)	0.013	(0.009)
	class rooms away from school	0.030	(0.015)	0.023	(0.013)
School size proxies	student enrolment 1st year	-0.000	(0.000)	0.000	(0.000)
	total number of staff	0.000	(0.003)	0.002	(0.003)
	total number of teachers	0.001	(0.004)	-0.001	(0.004)
	number of teachers in classes 1-4	-0.003	(0.002)	-0.003	(0.002)
School programme participation	Minimum Income Programme	0.012	(0.020)	0.004	(0.022)
	TV escola	-0.007	(0.024)	0.012	(0.023)
	Education TV	-0.040*	(0.022)	-0.013	(0.021)
	Parameters in Action	-0.045	(0.028)	-0.025	(0.027)
	FNDE	0.005	(0.025)	-0.025	(0.025)
	Ouvebem	-0.061**	(0.028)	-0.025	(0.026)
	Reabvis	-0.025	(0.027)	-0.007	(0.025)
	School Transport Programme	0.074***	(0.027)	0.021	(0.027)
	National Library Programme	-0.025	(0.021)	-0.006	(0.019)
	State computer programme	0.038	(0.062)	0.043	(0.059)
	Municipal computer programme	0.213	(0.179)	0.153	(0.170)
	Proinfo	0.023	(0.039)	0.058	(0.036)
	other state programme	0.104***	(0.025)	0.008	(0.024)
	other municipal programme	-0.039	(0.032)	-0.068**	(0.031)
	Free School Lunch	0.047	(0.170)	0.007	(0.181)
	Free Public School Transport	-0.035	(0.024)	0.007	(0.022)
	Constant	0.634	(0.504)	0.807	(0.440)

Observations	1993	
SRE dummies	No	Yes
R-squared	0.09	0.28

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%, standard errors are in parenthesis. Specification (2) includes regional school administration dummies (SRE). Most of the physical characteristics describing the schools are indicator variables on the presence at school. Similarly, indicator variables inform about participation in education programmes. The programme *Parameters in Action* is a federal programme for the professional development of teachers; *FNDE* denotes a maintenance and development programme for education by the National Fund for the Development of Education, *Ouvebem* is a national campaign for the importance of the sense of hearing, *Reabvis* is a national campaign on visual rehabilitation, *PROINFO* is a federal computer literacy programme. Table A3 reports the coefficients from two specifications of a linear model of the effect of school characteristics on the probability for treatment. The dependent variable is an indicator that equals zero for being in treatment group and equals 1 for being in the control group. In specification (1) only very few coefficients are significant at the 1% and 5% level of significance, of which some disappear when including regional dummies (specification (2)). None of the coefficients of the linear model produces values outside the unit-interval and a logit specification delivers very similar results to the linear specification diminishing doubts on the suitability of the linear specification (not reported).