CENTRE *for* ECONOMIC PERFORMANCE

CEP Discussion Paper No 878

July 2008

The Cost of Grade Retention

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E·S·R·C ECONOMIC & SOCIAL RESEARCH COUNCIL

Abstract
This paper uses administrative longitudinal micro data on the universe of Junior High school students in Uruguay to measure the effect of grade failure on students' subsequent school outcomes. Exploiting the discontinuity induced by a rule establishing automatic grade failure for pupils missing more than 25 days, I show that grade failure leads to substantial drop-out and lower educational attainment even after 4 to 5 years since the time when failure first occurred. Complementary evidence based on a change in the regime of grade promotion leads to very similar conclusions, suggesting that non-random sorting around the discontinuity point is unlikely to drive my results.

Keywords: grade retention, school drop-out, regression discontinuity, sorting
JEL Classification: I21, I22, J20

This paper was produced as part of the Centre's Labour Markets Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

Acknowledgements
I am grateful to Jerome Adda, David Card, Chang-Tai Hsieh, Thomas Lemieux, Alan Manning, Justin McCrary, Enrico Moretti, Barbara Petrongolo, Tommaso Valletti, Steve Pischke, and seminar participants at UC Berkeley, LSE, Paris-Jourdan, CEMFI, the Tinbergen Institute, the University of Toulouse, Bocconi, the University of Bologna, IFS, the Universidad de la Republica Montevideo and ANEP for many helpful comments and suggestions. I am also grateful to Federico Bachino, Darwin Caraballo, Santiago Cardozo, Fernando Filgueira, Laura Galletti, Cecilia Llambi, Estela Montado, Renato Opertti, Marcelo Pereira, Andres Peri, Alejandro Retamoso, Carmen Tornaria and especially to Hilda Surraco and Andrea Vigorito for many helpful discussions about the school system in Uruguay and help with the data. Diego Aboal and Cecilia Vera provided excellent research assistance. My thanks to ANEP for making the data available.

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Published by
Centre for Economic Performance
London School of Economics and Political Science
Houghton Street
London WC2A 2AE

ISBN 978-0-85328-285-3

1. Introduction

Grade repetition is particularly widespread in less developed countries, and often accompanied by low enrollment and high drop-out rates, the combination of the two often referred to as 'wastage'.[1] Figure 1 plots gross enrollment rates in secondary school on grade repetition rates in primary school in 65 countries.[2] Sub-Saharan Africa shows both the lowest enrollment rate (31%) and the highest repetition rate (around 20%). At the other end of the spectrum, Central Asia, Eastern and Western Europe and North America display repetition rates that vary between 1% and 2% and enrollment rates that vary between 86% and 112%. North-Africa, Middle East and South East Asia locate somewhere halfway - with repetition rates between 8% and 9% and enrollment rates between 62% and 66%. Latin America is not very dissimilar (see Urquiola and Calderon, 2006), with an average enrollment rate of 73% and an average repetition rate of 6%.

Figure 2 plots the average repetition rate in primary school over PPP per-capita GDP. The figure shows clearly that not only do repetition rates in primary school tend to be strongly negatively associated with low enrollment in secondary school but also with low levels of income per-capita.

The evidence in Figures 1 and 2 immediately raises the question of whether the correlation between grade repetition and subsequent school enrollment is by any means causal. Do repetition policies bear a responsibility for children's low educational attainment? In particular, do the hurdles that these policies create for the students' transition through the school system explain why a large fraction of students eventually drop out? Or is it the case that the correlation in Figure 1 is just spurious, due for example to the circumstance that where the demand for education is low, as in many developing countries, the efficiency of the system (as measured by grade promotion) is also low, perhaps due to low public investment in education? Does the poor quality of teachers and schools, teachers' absenteeism, lack of school infrastructures - often cited as major problems of school systems in developing countries - explain both high failure rates and students' incentives to abandon the system?[3] Or is it the case that, where poverty is widespread, students find it harder to progress through the

[1] In this text I use interchangeably the terms grade retention and grade repetition.
[2] Data come from UNESCO (2002) and refer only to those countries that report positive repetition rates, since there is no way in the data to ascertain if missing data on grade repetition are due to lack of data or lack of repetition.
[3] On teachers' absenteeism in Indian schools see Banerjee and Duflo (2006), Chaudhury et al. (2005). See Duflo (2001) on the effect of the supply of schools on enrolment.

system due to malnutrition, lack of financial resources (coupled with credit constrains) or the higher opportunity cost of attending school, hence leading to both repetition and drop-out?

The desirability of a grade retention policy is a controversial issue. Even in the USA (not in the figures), where grade retention is widespread, there is a heated debate on the merits and demerits of this policy.[4] In his 1998 State of the Union Address, President Clinton called for an end to 'social promotion' (i.e. the widespread practice of passing students failing to meet performance standards), and several school districts have followed suit. This debate reflects a substantial disagreement on whether, on the whole, grade repetition is beneficial to students and the society at large, and more fundamentally the circumstance that there are both costs and benefits associated to this policy. On the one hand, there is a widespread view among psychologists and part of the pedagogical profession that early grade failure does not lead to improvements in school achievement (McCoy and Reynolds, 1999), while raising drop-out (Jimerson et al., 2002), with negative - or at least non-positive - effects on socio-emotional adjustment (Jimerson et al., 1997).[5] Low self-esteem - possibly due to a student being disenfranchised or stigmatized by peers, teachers or family -, lower expectations on the part of the student or the environment around him, or the cost of having to readjust to a new class (and possibly a new teacher) as a result of repetition, might worsen a student's school outcomes. This might even result in a student dropping out of school if he is older than compulsory schooling age (or above compulsory schooling level), or even before this, when enforcement of compulsory schooling laws is lax, as in many developing countries.[6]

A different view emphasizes the benefits of grade repetition. Grade retention might reinforce a student's knowledge or discipline, with potential beneficial effects on his outcomes. Additional exposure to teaching, especially in early grades, might strengthen a student's background making him more apt - and hence presumably more likely - to pursue higher levels of education. Repetition might in addition help improve the quality of the match between the school and the student. This happens if a child's development makes him more apt to attend a certain grade at a later age, or if changing peers and possibly teachers leads to an increase in a child's productivity. According to this view, grade repetition is potentially an efficient mechanism to reallocate students to classes.

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[4] Although the Department of Education does not provide official figures on repetition, estimates for the USA (not in the figure) from the CPS show that around 12% of individuals aged 12-15 have repeated at least a grade (Cascio, 2005).

[5] For a less negative view of the effect of grade repetition see Alexander et al. (2003).

[6] The potential effect of grade failure on drop-out makes the problem of grade repetition less apparent - if drop-out happens immediately after grade failure - but the consequences of grade failure not less serious.

Possibly, however, the strongest argument in favor of grade repetition is that it acts as a deterrent against students' poor school performance. By inflicting a high penalty to underperformers, this policy creates an incentive for students to increase their school effort (see Jacob, 2005) on the incentive effect of high stakes exams on students' test scores and learning), although this might come at a cost, since students take longer to transit through the school system. Experiencing the penalty of repeating a grade might also make repeaters less likely to wanting to experience this again, hence creating an incentive to improve their school performance, possibly because of the increasing marginal cost of repeating an additional grade.

Although there is a rather copious body of research on the determinants of grade repetition, convincing quasi-experimental evidence on its effect on students' outcomes is still scarce, especially for developing countries and this paper aims at filling this gap.[7]

The main difficulty in identifying the effect of grade failure on subsequent school outcomes is that latent school outcomes - i.e. the ones which would be observed in the absence of grade retention - and the propensity to fail a grade are likely to be simultaneously determined. Similarly to the spurious cross-country correlation discussed above, characteristics of the pupil - such as his ability or motivation - together with characteristics of his environment - such as his family or neighborhood – and school are likely to affect both the probability that a student will fail a grade and his probability of being in school and, conditional on this, his school outcomes. In addition, current poor school performance might itself be a cause of both current grade failure and future poor school outcomes. Similarly, a student's decision to drop out of school during a certain school year might simply result in this student failing that grade, inducing some reverse causality between drop-out and grade retention. Because many of the variables simultaneously affecting grade failure and latent school outcomes are typically unobservable on the part of the researcher, and because of the potential reverse causality between drop-out and grade failure, simple (conditional or

[7] Evidence on the determinants of grade repetition illustrates a causal relationship between family socio-economic status (Oreopoulos et al., 2006 for evidence on parental education, Maurin and Goux, 2005 for evidence on residential overcrowding), educational inputs (Pischke, 2003) for evidence on the length of the school year) and early childhood interventions (Currie and Neidel, 2004) for evidence on Head Start) on the one hand, and grade retention (often measured as accumulated age-grade distortion) on the other. For less developed countries, there is also ample evidence that family background and school inputs are important determinants of grade failure (Gomes-Neto and Hanushek,, 1994 for Brazil and Patrinos and Psacharopoulos, 1996 for Bolivia and Guatemala). Evidence from Colombia based on a randomized school voucher program illustrates the positive effect of increased school choice and the ability to afford private education on promotion rates (Angrist et al., 2002). Conditional and unconditional cash transfers also appear to have a positive effect on grade promotion (Behrman et al., 2001), Schady and Araujo (2005) although potentially they might create distorted incentives if conditional on enrollment (Dubois et al., 2004).

unconditional) correlations between grade failure and school outcomes are unlikely to provide a good indication of the causal effect of grade failure on subsequent school outcomes. Because of the confounding effects listed above, most likely such correlations will tend to overestimate the impact of grade failure on subsequent school outcomes.

Two papers, both for the US, account explicitly for the potential endogeneity of failure rates. Jacob and Lefgren (2004) use the discontinuous relationship between test scores and promotion to assess the casual impact of grade repetition on achievement among Chicago public school students in third and sixth grade. Their results show a positive short-term effect of grade retention on third graders' achievement and no effect on sixth graders'. Eide and Showalter (2001) use the US high School and Beyond Survey to assess the impact of grade retention on drop-out rates and earnings later in life. Using the variation in age of entry into kindergarten across US states as an instrument for repetition - an instrument whose validity appears questionable - they conclude that for white students, grade repetition tends to lower the drop-out rate and increase earnings, although results cannot be told statistically apart from zero.

In order to circumvent this classical identification problem, in this paper I suggest using a rule in force in secondary Junior High school in Uruguay - a country with remarkably high repetition rates- that establishes automatic grade failure for those pupils missing more than 25 days during the school year. I exploit the discontinuity in grade advancement induced by this rule to asses the causal impact of grade failure on drop-out rates and school attainment later in life. Effectively, I compare individuals to the left and to the right to the discontinuity point in a given year, and follow their school progression over time. As discussed in the text, this design attempts to mimic random assignment of grade failure and - under some assumptions - allows one to answer the question of what effect grade failure has on a student's performance net of confounding factors or reverse causality.

Using administrative data on a sample of around 100,000 students in public non-vocational Junior High school (grades 7-9), I find pronounced and statistically significant negative effects of grade failure on later school outcomes. Not only does grade failure induce students to drop-out at the end of the school year when failure occurs, but its effects appear long lasting. Both failers and repeaters, i.e. those who fail a grade and stay on, stay shorter in school than non-failers and end up with less accumulated education, implying high costs of failing a grade.

One concern with this the regression estimates is that the variable whose discontinuous relationship with failure rates I use to identify, namely number of school days, is to some extent under the students' control (and potentially the school's control if records of missed days can be manipulated). This might be particularly problematic if students with otherwise better (worse) latent school outcomes end up disproportionately locating below (above) 25 days. In this case, one might end up overestimating the cost of grade failure, since one would erroneously attribute the poor performance of individuals above the threshold to the effect of the policy. Indeed, evidence of a discontinuity in the density of missed school days exactly at the 25 days threshold raises potential concerns of non random assignment around the threshold (Lee, 2005 and McCrary, 2007). Using a simple dynamic investment model of students' absenteeism, I show however that this feature of the data is not necessarily troublesome for identification. This is confirmed by the data. First, I show that the inclusion of the (admittedly few) observable controls available in the data set leads to point estimates that are very similar. Second, I present evidence based on a different identification strategy, namely differences-in-differences, that is based on a change in the regime of grade promotion and does not rely on random assignment around the discontinuity point. Reassuringly, this alternative estimation method leads to similar conclusions, suggesting that non-random sorting of individuals around the threshold is unlikely to be a major source of concern in this application.

Although in the analysis I largely focus on the consequences of this policy among those who happen to experience the penalty of failing a grade, the data show some clear behavioral responses even among those who do not fail, presumably associated to the deterrence effect of the policy. In the conclusions to this paper I try to contrast what I estimate being the costs of the policy to its benefits induced by the deterrence effect.

The structure of the paper is as follows. Section 2 provides background information on the school system in Uruguay. Section 3 presents the basic data. Section 4 discusses the specification of the regression model and the identification behind the empirical strategy proposed. Section 5 presents the regression results and Section 6 finally concludes.

2. The School System in Uruguay: Background

Uruguay is one of the smallest (176,220 km^2) countries in Latin America. Population in 2005 was about 3.4 million, approximately half of which in the metropolitan area of Montevideo, the capital city. Despite Uruguay being an early starter in the process of development, over the last century the country has grown at a very slow pace. While per-capita GDP in 1870 was approximately equal to the contemporaneous per-capita income in the USA, by the end of the last century this ratio was around 30% (Maddison, 2004). Nowadays the share of population below the poverty line is about 21% (CIA, 2005).

Uruguay boasts a long tradition of publicly provided education and social inclusion. Primary school was made compulsory in 1877, universal primary schooling was achieved in the 1950s and the literacy rate is among the highest in the region (97% for men and 98% for women). The school system is organized into three basic cycles: Primary Education (*Education Primaria*, grades 1-6, theoretical ages 6-11), Junior High (*Ciclo Básico,* grades 7-9, theoretical ages 12-14) and Senior High (*Education Secundaria*, grades 10-12, theoretical ages 15-17).[8] This is summarized in Chart 1. Primary and Junior High school are compulsory.[9] Junior and Senior secondary education are offered in both *Liceos*, i.e. non-vocational secondary schools, and in vocational colleges, *UTU*s (*Universidad del Trabajo del Uruguay,* literally the Uruguayan Employment University). In 2000, enrollment in *Ciclo Básico* was in the order of 115,154 students, around 87% of which in *Liceos*. Not dissimilar from many other Latin American countries, private fee-based education is widespread at all levels. This covers respectively 9% of primary school enrolment and about 15% of secondary school enrolment.

Even if Uruguay still ranks high in terms of educational outcomes compared to the rest of Latin America (with only Argentina, Chile and Peru showing higher average years of education), its education system is not problem free.[10] From 1960 to 2000 for example, while average years of education in the population over 25 in the USA has risen by around 4.5 years (from 8.7 to 12.2), in Uruguay this growth has been in the order of 2.1 years (from 5.1 to 7.2) (Barro and Lee, 2001).

While enrolment in primary school is timely, and completion of primary education almost universal, the system is unable to retain a large share of students in *Ciclo Básico* (Furtado, 2003; Da Silveira and Queirolo, 1998). Data from a specific education module administered in conjunction with the National Household Survey (*Encuesta Continua des Hogares*) of 2001 show that 42% of individuals aged 15 to 17, i.e. of post-compulsory school

[8] Since 2002, an additional year of compulsory pre-primary education for 5 years old has been introduced.
[9] Minimum working age over the period of analysis was 14 (hence lower than the minimum age required to complete compulsory education). The circumstance that minimum working age is lower than minimum compulsory age is not unique to Uruguay. Similar disciplines were in force in several US states at the beginning of the last century. Special provisions are made for working students in terms of night schools.
[10] See Bucheli and Casacuberta (2000). Starting from the mid 1990s and in recognition of these problems a very ambitious reform of the educational system has taken place (ANEP, 2000).

age, have not yet completed *Ciclo Básico*. In the age group 24-29, around 20% of individuals declare never having started *Ciclo Básico* and, among those who started this school cycle, around 16% declare not having completed it. This possibly suggests that students get discouraged or find their poor school ability revealed as they cross the system. One of the hurdles that students find during their progression through the school system is the high probability of failing a grade in both primary and secondary school. Around one in three children in *Ciclo Básico* fail each year.

Grade progression in *Ciclo Básico* depends on the student fulfilling two conditions: the first relates to attainment during the school year and the second to absenteeism. These rules apply to the school years 1996 and 1997 and are summarized in Chart 2. In 1998 the system was reformed, as explained later in the paper.[11]

At the end of each school year (that goes from March to December), students are assigned a mark for each of the 12 taught subjects based on their performance during the year. Students pass a subject if they get a mark above a given threshold. Those who fail a subject must eventually re-take it during subsequent exam sessions. The first opportunity for retaking exams is just before the beginning of the subsequent school year, in February, following a short remedial course. Subsequent re-take exams take place in July and December of each year. The first condition for grade promotion is that at the beginning of each school year (i.e. after the February re-take session) the student has no more than three accumulated 'debts' from any previous year that he has not in the meantime cleared.

The second condition that must be simultaneously fulfilled is that the student has accumulated no more than 20 missed school days during the year.[12] A student fulfilling both conditions is automatically entitled to grade advancement. Discretion on the part of the school is allowed provided the student has missed no more than 25 days of school (provided again he has no more than three pending subjects). Grade failure is automatic with more than 25 days of absence or with more than three pending subjects.[13] In the rest of the paper I

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[11] The discipline mentioned here (ANEP, 1996) refers to old *curriculum* (*Plan 86*). In 1996 a new curriculum was introduced (*Plan 96*) that changed both the content and the structure of teaching, the length of the school day (from 3.5 hours to 5 hours) and the rules determining promotion. As *Plan 96* was introduced experimentally in a few schools (*Liceos Pilotos*), the majority of students in 1996 and 1997 were still under the old Plan. Regression results below refer to both *Liceos Plan 86* and *Pilotos*. Results (not reported) are virtually unchanged if I only restrict to *Liceos Plan 86*.

[12] For the purpose of computing missed school days, any justified absence for medical reasons or any other "serious" reason counts half a day. Missed school days are approximated to the lowest integer for the purpose of grade promotion.

[13] It is not immediately clear why such a sharp rule relating grade promotion to absenteeism (in addition to performance) is in place. It is difficult to imagine this being generated by some efficient incentive contract. In addition, this is likely to penalize more disadvantaged children, with lower school attachment and potentially

exploit this rule in order to identify the effect of grade failure on students' subsequent school performance.

3. Data

For the purpose of this analysis I have assembled administrative micro data on students in (almost) all public non-vocational high schools (both Junior and Senior, i.e. grades 7 to 12) in the country. The data, which are described in detail in the Data Appendix, refer to the school years 1996 to 2001 and they report information on the institution and grade attended, whether the student passed or failed that grade, and number of missed school days. Because a unique identifier refers to each pupil, observations across years can be linked. In the following I restrict to individuals who transited through Junior High (grades 7-9) between 1996 and 1997 and I follow their school progression (in both Junior and Senior High) up to (potentially) 2001.

There are some important limitations to the data. First, they do not provide information on students' end of year marks or number of pending subjects, one of the variables affecting grade promotion. Second, the data exclude *UTU*'s and private institutions. Because of this, I can only measure whether a student is retained within the public non-vocational system but I am unable to distinguish those leaving the educational system *tout court* from those moving to private or vocational intuitions. I attempt to address the potential bias in the estimates induced by this margin of adjustment at the end of the paper. Third, since there is no information on promotion or failure in 1999 and 2001, I measure school progression as maximum grade attended (as opposed to completed).

Table 1 provides some descriptive statistics. The left panel of the table refers to the entire sample, the middle panel to those who fail, and the right panel to those who pass. Each observation refers to an individual-grade observation. Overall, there are 149,983 observations in the sample and 100,862 individuals. Each column of the table refers to a separate grade. The number of individuals falls from seventh grade (accounting for about 40% of the sample) to ninth grade (accounting for about 29% of the sample). This reflects the pyramidal structure of secondary schools in Uruguay. This in turn is due to school drop-out rather than to differential retention rates across grades. Average age distortion (age-grade-6) increases from

higher gains from exposure to teaching. Most likely this works as an anti-vagrancy device (see Katz and Goldin, 2003) for a similar rationale underlying compulsory schooling laws in 20th century US).

0.64 in seventh grade to 0.94 in ninth grades, a sign of a modest delay in school progression. Arguably, those in ninth grade are more likely to have started at an earlier age and not to have repeated before compared to those in seventh grade, so this number provides a lower bound for the delay in school progression in the absence of selective drop-out. Girls tend to be more represented the higher the grade, suggesting larger drop-out among boys. Missed school days are in the order of 18-20. Since the school year is approximately 180 days, this implies an average absenteeism rate in the order of 10-12%. Failure rates are in the order of 30%. Both repetition and absenteeism are the highest in ninth grade and the lowest in eighth grade.

Data on outcomes are right censored since some individuals will still be in school in 2001. Censored observations include both those who have not dropped out and are still in school because they could not have theoretically done so under normal progression (e.g. those in seventh grade in 1997) or those who are still in school because of later failure (e.g. those in eighth grade in 1997 who fail ninth grade in 1998). The data clearly illustrate the extent of censoring. The proportion of individuals still in the sample in year 2001 varies from 47% to 16% moving from seventh to ninth grade.

As an outcome variable at this stage, I examine a student's number of additional school grades attended by the end of the period of observation. This is the difference between maximum grade attended by 2001 and the grade where the student was observed in *Ciclo Básico* in 1996 or 1997. This variable potentially ranges from zero (for those who drop out immediately or repeaters who fail again and then drop out) to 5 (if individuals are observed in 1996 in seventh grade and progress smoothly to twelfth grade by 2001). Since, mechanically, grade failers have to be exposed to one extra year of schooling in order to potentially make up for the lost year due to repetition, I allow failers to be followed for one more year compared to non-failers. I do so by censoring observations to the year 2000 for passers and following failers until 2001. The number of additional grades attended falls from around 2 for those observed in seventh grade to 1.5 for those observed attending ninth grade. This is obviously a reflection of the fact that the number of grades left falls as the grade attended increases. However, this variable does not fall one to one with the initial grade attended. This is the reflection of those attending ninth grade being a selected sample of individuals (those with higher probability of staying on and progressing faster).

An analysis of the differences between grade failers and non-failers illustrates that, along all the observable dimensions, failers perform worse than non-failers. Compared to non-failers, failers are between 0.5 and 0.9 year older, and they tend on average to miss school 4 to 5 times more. Higher absenteeism among failers might be the result of failure and absenteeism being both correlated with the student's motivation, cost of - or return to - attending school, might be due to the circumstance that school days are an input in the child's education production function, or simply be the mechanical effect of the rule linking absenteeism to failure. Boys are much more likely to repeat a grade than girls (34% compared to 26%). This fact is not unique to Uruguay, although it is not obvious whether this is due to differential school ability, differential socio-emotional development, differential opportunity costs (e.g. boys having better labor market opportunities than girls) or other factors. When I move to outcomes, failers perform much worse than non-failers. If failers were identical to non-failers and grade failure only delayed progression, one would expect failers and non-failers to attain the same number of additional grades. In fact, the data show that failers accumulate on average between 2 and 1.5 less years of schooling compared to non-failers suggesting unequivocally worse outcomes for failers than non-failers. This difference is likely to underestimate the true gap in the number of additional grades attended by the end of their school career between failers and non-failers, since the proportion of censored observations is much lower for failers than non-failers (between 56 and 8 p.p. depending on grade). Obviously, though, the negative correlation between failure and later outcomes is not necessarily indicative of a negative effect of grade failure. As already stressed in the introduction, failers are likely to do worse than non-failers for reasons other than failure itself.

4. Specification and Identification

In this section I devise a regression strategy aimed at identifying the impact of grade failure on school outcomes that is based on the (fuzzy) discontinuity in failure rates implicit in the rule described above. By using a regression model, this allows me to control for the observed characteristics of students and their schools as well for the potential bias in the regression coefficients that stems from differential censoring in school outcomes across individuals originally observed in different grades (7, 8 or 9) or different years (1996 or 1997). In this section I also discuss the identification assumptions underlying the consistency of the proposed estimator.

Ignoring for simplicity other covariates, suppose that school outcomes Y depend additively on a continuous function $f(.)$ in the number of missed school days, D, and on grade failure, F:

(1) $Y = \beta_0 + \beta_1 F + f(D) + u$

where u is an error term. The error term potentially includes a student's past attainment as well as other unobserved determinants of performance. As already pointed out, the OLS estimate of equation (1) is biased if u is correlated with F due to unobserved heterogeneity of reverse causality.

In order to circumvent this problem, I suggest using the discontinuity in the failure rate at 25 missed school days as an instrument for failure rates in (1). Consistent with the rule, I assume that failure is a continuous function of missed school days $g(D)$ plus a dummy for 26 or more missed school days P=I(D>25):

(2) $F = \gamma_0 + \gamma_1 P + g(D) + v$

One can use (2) as a first stage equation for F and obtain and instrumental variable estimate of β_1 from (1). This is a classical (Fuzzy) Regression Discontinuity (RD) estimator. The identification assumption underlying the consistency of the IV estimate is that school progression varies continuously around 25 missed school days if not for the rule governing grade failure (Hahn et al., 2002).[14] For this IV estimate to carry a causal interpretation hence one requires that individuals do not sort around the discontinuity point based on unobserved determinants of the outcome variable (u), i.e. that assignment around the discontinuity is as good as random assignment.

Figure 3 plots the probability of failing a grade on the number of missed school days (equation (2) for those with a number of missed school days between 1 and 35. While the probability of failing a grade is less than 0.5% for those with 1 missed school day, this raises to around 51% at 25 days of absence. Again, this is due to the number of missed school days proxying for a student's unobserved motivation, the opportunity cost of (or returns to) attending school, might reflect the circumstance that worse performing students have a lower incentive to attend school because they expect to fail the grade anyway, or simply be due to the fact that school days enter as inputs in the education production function. One can notice acceleration in the failure rate after 20 days of missed school but no obvious discontinuity. What is potentially more interesting is that, after 25 missed school days, the probability of

[14] There are plenty of applications of the RD design to schooling data. Typically, procedures and regulations attaining to students', teachers' or schools' behavior lead to discontinuities in treatment. See for example Angrist and Lavy (1999), van der Klaauw (2002), Jacob and Lefgren (2004), Urquiola (2006).

grade failure jumps to around 95% and it grows only modesty afterwards. This is consistent with the rule establishing automatic grade failure after 25 days of absence (although it also suggests non-perfect enforcement).[15]

Since grade failure is a discontinuous function of missed school days, for failure rates to have an effect on school outcomes, one would expect outcomes to vary discontinuously at 25 missed school days too. Figure 4 analyzes the correlation between additional grades attended (up to 2001 for failers and up to 2000 for passers) and missed school days for those in *Ciclo Básico* between 1996 and 1997 (reduced form equation). The number of additional grades attended falls monotonically with number of missed school days in *Ciclo Básico.* While those with 1 missed school day accumulate on average 3.3 additional years of schooling, this number is about 0.4 for those with 35 days of absence. For the same reasons mentioned above, this negative correlation is expected. More interesting, though, is the sharp discontinuity in the outcome variable that is apparent between 25 and 26 missed school days. This is a fall of around half a year from around 1.1 at 25 days to 0.7 years at 26 days. Because this is the mirror image of the effect of missed school days on grade failure in Figure 3, and if one is willing to assume that assignment around the discontinuity point is as good as random assignment, this can be taken as evidence of grade failure having a causal negative effect on school outcomes.

However, in this case, where the running variable is potentially under the agent's control, one might be concerned that assignment around the discontinuity point is not random, invalidating the conditions required for consistency of the RD estimator (Lee, 2005; McCrary, 2007). For example, if pupils with better latent school outcomes are more able to sort strategically below the threshold, perhaps because unexpected shocks to their attendance rates outside their controls are less likely to hit them or because they have a greater ability to keep track of how many missed school days they accumulate day by day, the IV estimate will tend to overestimate the effect of grade failure on school outcomes. In addition, records of missed school days could be in principle manipulated by schools or teachers (see Jacob, 2005) for evidence of the potentially distortionary effect of incentives on teachers' behavior, and Jacob and Levitt, 2003 for evidence on teachers' cheating in response to incentives). This might be a concern if students with different latent outcomes are treated differently. Anecdotic evidence suggests that teachers and schools are not alien to record manipulation. This in turn might be due to a teacher being corrupt or if high failure rates act as a signal to

[15] Under special circumstances (i.e. children with special needs, or chronic diseases) the 25 days rule can be waived.

pupils, parents, colleagues or the head-teacher of the teacher's quality, which he would rather conceal and, in an attempt to do so, will favor some students. In addition, teachers or schools might decide to circumvent this rule in order to adjust class sizes across grades and avoid ending up with some very large classes and some very small classes in the same school. Again, this might in theory impact different students differentially. Teachers' behavior might lead to negative or positive sorting, with otherwise worse performing students being less or more likely to locate below or above the threshold. Negative sorting would occur for example if teachers tend to be more lenient towards students with lower chances of later progression due to compassion, the recognition that grade repetition might be of little help to them or simply the fact that, by passing worse performing students, teachers hope to get rid of them quickly as they progress further through the system (and indeed the evidence below on repeaters seems to suggest that this might be the case).[16]

Figure 5 reports the distribution of missed days between 21 and 35. Proportions are calculated relative to the entire sample (i.e. with missing days from 0 to 180). The Figure shows a monotonic fall in the density as the number of days increases, a spike at 25 days (from 0.6% at 24 days to 0.8% at 25 days), after which the probability of missing one extra school day remains very low (less than 0.2%).[17] The discontinuity in the density of the running variable is sometimes taken as an indication of the failure of the random assignment hypothesis, since this might be suggestive of students' (or schools') ability to manipulate the running variable in a fashion that is correlated with their latent outcomes. This is by now a standard test in applications involving an RD design (see for example DiNardo and Lee, 2004).[18]

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[16] Lee (2005) shows that localized random assignment can occur (and hence consistency of the RD estimator warranted) even in the presence of sorting, provided individuals have no ability to sort 'precisely' around the discontinuity point. This result is based on the assumption that the manipulation error is continuously distributed in the neighborhood of the discontinuity point. In this case, agents planning to be above (below) the threshold might be pushed below (above) it by the manipulation error. As explained by McCrary (2007) the partially manipulated running variable will be a convolution of a discontinuous variable (the manipulated score) and a continuous error term, hence being itself continuous.

[17] Another remarkable feature of the distribution of missed school days is the concentration of individuals at 20 days (4.3%, compared to 3.2% at 19 days and 1% at 21 days). This is shown in Figure A1 in the Appendix where I report the distribution of missed schools days between 1 and 35 (I report this distribution in a separate graph due to its different scale). Recall that among those with no more than three pending subjects, promotion is automatic at 20 missed school days or below and discretional above (provided the student misses no more than 25 days). This shows that teachers' behavior is unlikely to explain the spike at 20 (and by extrapolation at 25) days. Overall, if there is a cost associated to records' adulteration, and if teachers can pass students with 21-25 missed days anyway, one would not expect teachers to go through the trouble of adulterating students' records. Second and differently from the discontinuity at 25 days, Figure 3 shows that the gap in the failure rate between 20 and 21 missed school days is very modest (around 2 p.p.). Perhaps this is suggestive of the incentive effect of the policy: those at risk of failing due to absenteeism (above 20) exert extra effort to avoid the penalty.

[18] In the context of partial manipulation, Lee (2005) shows that, if the manipulation error is continuous, one will expect the density of the running variable to be continuous in the neighborhood of the discontinuity point. By

However, failure of this test in this application (where the support of the running variable is discrete) is not conclusive on the existence of sorting (although not inconsistent with it either).[19] This point is made formally in the Technical Appendix, where I present a dynamic model of school attendance with uncertainty. Each day, students decide whether to attend or not, conditional on the numbers of missed days accumulated so far, the cost and return of attending today, and their expectation of the future costs and returns. I show that, with ex-ante identical individuals (and hence no role for heterogeneity), one would expect precisely some discontinuity in the density of the running variable. In an attempt to avoid the stiff penalty associated to the rule, students will act conservatively, ending up in large numbers below the cut-off point. However, even in a world with ex-ante identical agents, ex-post some will be hit by bad luck and just cross the threshold. In this case, the variation around the discontinuity point will still identify the average treatment effect, despite the density function being discontinuous. In practice, sickness, bad weather, transport difficulties or family responsibilities might push individuals to be absent from school on a certain day despite their original plan to attend. If the variation around the discontinuity point is largely generated by these random shocks, one can legitimately use this variation as an instrument for grade failure in (1).[20]

In sum, in this application, the density test is unlikely to be very informative. The discontinuity in the density is not inconsistent with random sorting. To the extent that the variation around the threshold is largely generated by random shocks, then the RD estimates will lead to consistent estimates of the treatment effect. For this reason, in the following I present RD estimates of model (1) and I attempt to test for random assignment by including observable controls in the model and testing the sensitivity of the results to their inclusion

converse, if agents have perfect control of the running variable (i.e. manipulation is complete), one will expect a discontinuous density. McCrary (2007) makes a similar point in the case of perfect manipulation and points out that when manipulation is incomplete, the density test is a valid test for random assignment only if auxiliary conditions (namely monotonic manipulation) hold.

[19] In this case, both the manipulated running variable and the error term will be discrete, and their convolution is not necessarily a continuous random variable.

[20] Although it is effectively difficult to ascertain whether and to what extent the spike below the discontinuity point is due to random luck or instead to students' or schools' strategic behavior, a separate analysis of the density for those who eventually fail and those who eventually pass shows that (perfect) sorting is unlikely to occur in this application. These densities, that are reported in Figure A2, show that both passers and failers end up locating in large numbers at 25 days. The reason for this is simple: students do not know if they will eventually fail or pass a grade, since this will depend on final year marks plus possibly the results of the February re-sit session, both of which are still in part unknown at the time when the students decided whether to attend or not on a given day. If the running variable were subject to perfect manipulation, one would not expect failers to sort below the threshold. Ultimately, if there is a cost to school attendance, students will not want to incur this cost if they know that they are going to fail anyway. This picture also suggests that teachers' adulteration of records is unlikely to give reason of the spike at 25. Again, if there is a cost to adulterating records, one would expect teachers not to do so for the students who eventually fail.

(see Lee, 2005 for an interesting application; see also McEwan and Urquiola, 2005). Second, I present evidence based on a different identification strategy that does not rely on any assumption on the assumption of random assignment of students' unobservable characteristics around the discontinuity point. Both these strategies lead to results that are similar to the (unconditional) RD estimates, suggesting that non-random sorting around the discontinuity point is not a major source of concern in this application.

5. Estimates

5.1 RD Estimates: Missed School Days, Grade Failure and Final School Outcomes

In this section I present IV estimates of model (1) and (2). I measure the effect of the rule on failure rate in (1) as the estimated difference between the actual and the counterfactual failure rates at 25 days. I follow Lee and Card's (2006) suggestion in the context of Regression Discontinuity design with a discrete running variable by modeling $f(D)$ as a parametric (quadratic in this case) spline in D, whose shape and intercept I allow to vary on either side of the discontinuity point (see also Lemieux and Milligan,, 2007) for an application). Standards errors are clustered by number of missed school days.[21] I restrict to those in the neighborhood of the discontinuity point (21 to 35 missed school days) and I pool observations for 1996 and 1997, treating individuals who appear in the sample more than once as two separate observations.[22] I go back to the short term dynamics of grade failure later on in the paper.

Columns (1) to (4) of Table 2 present the OLS estimates of equation (2), where the dependent variable is 1 for individuals failing a grade and zero otherwise. Column (1) includes no controls if not the splines in missed school days. The jump in the failure rate at the discontinuity point is estimated to be 43 p.p. One can visualize this jump in Figure 3

[21] Because some observations refer to the same individuals, I should account for this in computing the standard errors. To check for this, I have rerun regressions in Tables 2 and 3 on individuals who are observed for the first time in the sample only (76,904 observations), again clustering the standard errors by number of missed school days. Point estimates are very similar and standard errors are also similar to the ones reported in Tables 2 and 3.

[22] The estimates mix first time attendees with (potentially multiple-times) repeaters. Ideally, one would want to distinguish between first time attendees and repeaters. Prior grade failure might affect the probability of staying on as well as pupils' and schools' behavior and hence subsequent grade failure. Unfortunately, because the data are left censored (in 1996, or for those first observed in 1997), these is no way to ascertain for all individuals in the sample whether they failed the year before and, in this case, how many times. In order to avoid treating censored and uncensored observations differently, I ignore for the time being the dynamics in grade advancement between 1996 and 1997 and I treat individuals observed in both years as two separate observations.

where I have superimposed to the data the estimated quadratic splines and the estimated gap at 25 days.

If sorting around the discontinuity point is a serious concern, one would expect observed covariates to vary discontinuously around 25 missed school days. This is a natural test for random assignment around the discontinuity point (Lee, 2005). Unfortunately the data set is not very rich in terms of covariates. I start by including school, grade and year fixed effects so that the estimates pick up the variation within groups defined by these variables, and abstract from differences in enforcement or behavior across these groups. Once I control for grade, year and school dummies in column (2), I find an effect that is slightly lower than the one found in column (1). It is the inclusion of school fixed effects that drives the difference between the two specifications: schools with higher average absenteeism are also those where the probability of failure is higher irrespective of absenteeism. Omission of school fixed effect hence tends to slightly overestimate the effect of the rule due to a compositional effect.

In column (3) I include sex, age and age-grade distortion (age-grade-6) dummies. Figure A3 shows that girls are less likely to be above the discontinuity point (the coefficient on the dummy for more than 25 missed school days when year, school and grade effects are included is -0.091, s.e. 0.045) and so are individuals with lower accumulated delay (although not significantly so, the coefficient is -0.115, s.e. 0.090). Since girls are also less likely to fail, everything else equal, this leads to an OLS coefficient in column (1) that is slightly overestimated. However, differences between column (1) and (3) are not statistically significant. Once all controls (school, year, grade, sex, age and age-grade) are included in column (4), the OLS estimate falls by around 10% (from 0.43 with no controls to 0.39 when all controls are included), suggesting some modest role for individual heterogeneity and compositional effects in explaining the results.

In the middle part of Table 2 I report reduced form estimates of the model. Here I report the same specifications as in row 1, where the dependent variable is now additional grades attended up to 2001 (censored to 2000 for non-failers). Again results are somewhat sensitive to the inclusion of additional controls and again the effect seems to be slightly dampened by the inclusion of all controls. While, with no controls (column (1)), I find that being at 26 instead of 25 missed school days leads to a fall in the number of additional grades of around 0.33 years, when all controls are included, the estimated coefficient falls to around 0.29 years (column (4)).

Instrumental variable estimates in the bottom part of the table show little variation depending on whether controls are included or excluded. In column (1), with no controls, the estimated effect of grade failure on additional grades attended is -0.78. When all controls are included, this leads to an estimated effect of -0.75.[23] [24] Given the high standard errors, differences across specifications are not statistically significant. In sum, the data show a negative effect of grade failure on subsequent school outcomes in the order of 0.78 years. A modest share of this correlation is due to compositional effects and the differential propensity of students with different observable characteristics (females) to locate on either side of the 25 days cut-off. Although there is some evidence of (positive) sorting, neither the inclusion of school, grade and year fixed effects nor controls for children's observed characteristics (gender, age and accumulated delay) alter substantially my conclusions.[25]

5.2 Short Term Dynamics

Having ascertained that grade failure has a negative impact on the number of additional years of school attended, I now investigate the dynamics of this process. Nothing so far allows us to understand why failers appear to lag behind non-failers. Is this due to drop-out or subsequent grade failure? And if drop-out contributes to explain this result, where does this precisely occur? Is drop-out just following grade failure or do instead grade failers tend to drop out of the system at a higher rate than non-failers even after a certain number of years? If it is instead later failure to explain these results, is this due to repeaters tending to exert less effort, possibly because of disenfranchisement or stigma, or is it that schools tend to discriminate against these students? Or is instead the case that lower educational attainment four to five years down the line among those who failed in *Ciclo Básico* is neither due to these students failing again nor to them dropping out earlier but to the circumstance that grade failers are more likely to temporarily exit the system and then renter, so that the estimated gap in

[23] OLS estimates of grade repetition on the same outcome variable are -1.014 (s.e. 0.031) with controls included.

[24] Table 2 pools individuals from different grades together. Table A1 in the appendix investigates the effect of the rule and the consequences of grade failure separately by grade. In order to save space, I only present regressions with controls. The effect of the rule on grade failure appears to be stronger in eighth grade (where repetition is lower) than in seventh or ninth grade. The effect of grade failure on additional grades attended is negative at each grade, although this is smaller for those who fail in eighth grade. Results by gender (not reported) also show pronounced differences. First stage estimates are larger for girls, suggesting that they are more likely to fail due to absenteeism rather than poor performance. Reduced form and IV estimates though are smaller in magnitude for girls, potentially suggesting that failure is less harmful for them.

[25] I have also run the regressions in Table 2 using in turn a linear and a cubic spline in number of missed school days. Results are very similar to those reported in Table 2.

educational attainment masks a higher probability of intermittent attendance (and hence more censoring) among failers?

In Table 3, I start by analyzing the dynamics of grade failure year by year. Similarly to column (4) of Table 2, all the regressions in the table include the whole set of controls (sex, age, initial age-grade distortion, grade, year and school dummies). Columns (1) to (5) show the survival probability at time t (t=1, 4) on the probability of failing at time t=0. One can see that grade failure is followed by high drop-out rates. Grade failers are on average 20 p.p. less likely to be in school after one year compared to non-failers. Over time, as non-failers drop out or end their school cycle, the two distributions tend to converge and after 4 years failers effectively to catch up with non-failers. Differences are in the order of -6 p.p. but statistically insignificant.

Column (5) reports the overall duration in the sample. This is a variable that ranges from 0 to 4 (for those observed in 1997) or 5 (for those observed in 1996). On average failers spend about 0.73 less years in the sample than non-failers, consistent with the notion that early drop-out rather than the compounded effect of grade failure explains why grade failers end up with lower educational attainment than non-failers.

As an additional outcome variable in column (6), I analyze the effect of grade failure at time t=0 on intermittent attendance. I measure this as the probability of being in the sample at any time between 2 and 5 periods after failure conditional on not being in the sample 1 year after. I find no evidence of failers being more likely to attend intermittently than non-failers. If anything the reverse is true, the estimated effect is -0.067, implying that drop-out is a more permanent phenomenon for failers than for non-failers (although this difference is statistically insignificant).

The following columns of the table report information on the number of additional grades attended by failers and non-failers, whether still in school or not, at any time t (=1,.., 4) following grade failure (t=0). Because, as said, attendance is measured in terms of the highest grade attended (rather than successfully completed), it does not make any difference to the result for the first period if failers drop out or not following grade failure. In either case maximum grade attended will be the one they failed. Some non-failers though can also drop out, so the difference in maximum grade attended the year after grade failure will be strictly less than one. As expected, grade failers have just below one year gap compared to the non-failers at time t=1 (-0.89). After two years failers partially catch up with non-failers. The estimated gap is -0.74. This is possibly the result of differential drop-out rates or differential failure rates among those who originally failed at time t=0 and those who passed. The data are unable to provide an answer to this (expect in the very short run as I discuss below). After 4 years the difference in maximum grades attended is -0.84. This is close to, but slightly lower than, the effect on the censored distribution reported in Table 2 (-0.75), implying some additional gain among failers in the last year of observation.

In sum, the data show an early disadvantage for grade failers in terms of grades attended that they never make up for. This is largely explained by drop-out. There appears to be some initial catching up between failers (who stay on) and non-failers but this is a rather transitory phenomenon, largely limited to the first period. Intermittent attendance is unlikely to give reason of these results.

5.3 Repeaters

In this section I compare the outcomes of failers who stay on (i.e. repeat a grade) with those who pass and stay on. By looking at those who repeat one, can check whether it is simply initial drop-out among grade failers to give reason of their differential outcomes or whether the effects of grade failure are long-lasting. While the estimates in the previous sections can be interpreted as 'intention to treat' estimates of the effect of grade repetition, estimates in this section refer to the 'effect of treatment on the treated'.

As already shown, a large proportion of failers tend to drop out just following grade failure. Since, among failers, stayers are likely to be selected in such a way to have higher chances of acquiring extra education, there are probably good reasons to believe that the school outcomes of repeaters only provide a lower bound estimate for the effect of grade repetition on the population of failers at large, i.e. had all failers stayed on.[26]

Column (1) of Table 4 presents the first stage estimates for this subpopulation. Compared to the estimates in Table 2, estimates among the compliers show a slightly larger effect of the rule on grade failure (0.43 versus 0.39). This is possibly due to the fact that, among those at 25 days who eventually stay on, school performance is better than among those who drop out, so that the probability of failing at 25 days is lower.

In column (2) I present the effect of grade failure at time t=0 on the probability of dropping out of school between time t=2 and time t=1, conditional on being in school at time t=1. Repeaters tend to be at higher risk of drop-out compared to non-repeaters even the year following grade repetition, suggesting long lasting (at least two periods) effects of grade

[26] Among failers, those who stay on have on average one less of day of absence in the previous year (26 versus 27) and 0.87 less years of accumulated delay (0.32 versus 1.19).

failure. Column (3) investigates the effect of grade failure on the probability of changing school. One might speculate that one way by which students tend to undo the negative effect of grade failure is by changing institution. This might be due to their selective choice of less demanding schools or to the fact that grade failure is symptomatic of the bad quality of the match between the student and the school. Consistent with this, one finds that grade failure leads to a higher probability of school change, this difference is estimated in the order of 11 p.p. In columns (4) and (5) I present the effect of grade repetition on duration and maximum grade achieved (censored to 2000 for non-repeaters). It is interesting to notice that the effect of grade failure remains negative and strong even among those who repeat. Point estimates imply that repeaters accumulate 0.67 less grades and stay in the sample on average 0.35 less years. These figures are slightly lower than those found for the population of failers at large, consistent with a positive selection of repeaters among the population of failers (plus the mechanic effect of repeaters staying on one extra year). In any case, repetition does not appear to improve school outcomes.

Column (6) investigates the short term dynamics of grade failure. Although the data do not allow us to investigate grade failure in any subsequent year (due to missing data on failure in 1999 and 2001) one can still look at failure rates one year after grade failure (in 1997 for those who failed in 1996 and in 1998 for those who failed in 1997). In principle, the effect of grade repetition on students' failure later in life might go in a number of directions. On the one hand, students might react to early grade failure by exerting more effort, or repetition might improve their attainment. However, the opposite might be true due for example to stigma or disenfranchisement. As said, schools on their part might take into account early grade repetition in deciding whether a student has to fail an additional grade or not. Schools might either reinforce this effect by making early repeaters repeat more (again if stigma is a relevant factor in the school's decision for example) or they might be indulgent towards early repeaters if they feel that early repetition would harm them or make them little good. Schools might also discriminate in favor of early repeaters if they do not have an incentive to retain failers (who stay on) too long in the system, putting the school resources under strain. The data illustrate a very strong negative effect of repetition on subsequent grade failure. Estimates in column (6) suggest that repeaters are around 29 p.p. less likely to fail again than non-repeaters. As an (admittedly imperfect) way of checking whether this is due to differential treatment on the part of schools or instead changes in students' behavior, in column (7) I run the same regression as in column (6) with the addition of current (i.e. t=1) missed school days (again, using a quadratic spline interacted with a dummy for 26 or more missed school days). The negative effect of repetition on subsequent failure tends to fall by about two thirds (from 0.29 to 0.09 p.p.). In column (8) I regress the number of missed school days at time t=1 on the discontinuity at time t=0. At time t=1 repeaters tend on average to miss 24 less days of school than non-repeaters. Effectively one of the reasons why repeaters tend to be less at risk of failing the same grade is that they exert more effort than non-repeaters. This is consistent with the idea that (among the selected sample of failers who stay on) grade repetition acts a discipline device.[27]

To wrap up, the results in this section illustrate that the negative effects of grade failure extend also to those (positively selected) individuals who decide to stay on and repeat a grade. Grade repetition does not lead to improvements in outcomes. Rather, the opposite is true. The results in Table 4 also shed some light on the catching up between grade failers and non-failers in the year just after grade failure occurs. Repeaters are less likely to fail again than non-repeaters and this to a large extent appears the result of repeaters exerting more effort. As said, though, this initial (partial) reduction of the gap is undone in later years.

5.4 Differences-in-Differences Estimates

In this section I propose an alternative identification strategy based on differences-in-differences. The rules governing grade promotion changed in 1998 (see Chart 2). Although the basic principles of the system were left unchanged, in April 1998 a new directive was issued by the central school authorities that allowed discretion on the part of schools in passing students with more than 20 missed school days independent of the number of missed school days over that limit (and provided again they had no more than three pending subjects) (ANEP, 1998). Differently from the rule in force in 1996 and 1997, hence, failure was not automatic for those above 25 missed school days. This change in the discipline happened in a period of repeated attempts on the part of the central authorities to reform grade promotion rules. In particular, an early project circulated to schools in 1997 (ANEP, 1997) was later repealed. This created a climate of insecurity among teachers and students regarding the rules that would have eventually governed grade promotion at the end of 1998, since the possibility of other directives was not to be ruled out. A simple inspection of the data illustrates what effect this reform had on promotion. In Figure A4 in the appendix I plot the average failure rate by number of missed school days (1-35) in 1998. Two observations are in order. First, as

[27] Data across grades in Table A2 again illustrate a weaker effect for those in eighth grade who also appear more likely than those failing seventh or ninth grade to later catch up with non-failers.

in previous years, failure increases with the number of missed school days and shows a discontinuous jump between 25 and 26 missed school days (from 46% to 71%). However, consistent with the new discipline, failure rates at 26 missed school days are sensibly below 100% and tend to increase afterwards, suggesting that schools became less strict in the application of the (old) rule. Anecdotic evidence from that period suggests a great amount of inertia on the part of schools in adopting the new discipline. Perhaps more interesting is the observation that the distributions of missed school days appears similar in 1996-1997 and in 1998. This can be seen in Figure A5 where I plot the frequency distribution of missed school days between 1 and 35 in 1998 *vis à vis* 1996/1997. A visual inspection of the Figure shows that the shape of the two distributions is very similar.[28] It appears that while the incentives linked to the rule in force in 1996/1997 remained largely in place in 1998, the rules governing grade promotion did not. Most likely, the uncertainty about the discipline that would have prevailed by the end of the school year pushed students to act conservatively, for fear that the old discipline might eventually be reinstated. If the students' incentives associated to the rule operated similarly in 1998 and before but the effect of missed school days on repetition changed over time, then one can use the differential time-variation in failure rates across individuals with the same number of missed school days to infer the effect of grade failure on school outcomes net of sorting. This is a simple differences (across time) in differences (across number of missed school days) estimator of the effect of grade failure on school outcomes. By differentiating across individuals with the same number of missed school days, this estimator should effectively control for sorting around the discontinuity point, provided this is the same across years.

Before presenting the regression results, it is useful to show graphically the effect of this regime change on students' outcomes. In Figure 6 I plot the time difference between 1998 and 1996/1997 in the failure rate. I standardize this series to its value at 20 missed school days. This is essentially the (standardized) difference between the series plotted in Figure A4 and in Figure 3. One can see a modest fall in the difference up to 25 missed school days, a negative jump between 25 and 26 missed school days and then some convergence. This is consistent with much lower stringency in the application of the rule in 1998. I have

[28] As a more formal check, I have run a grouped regression of the relative frequency of each cell defined by year (1996, 1997 and 1998) and number of missed school days (between 21 and 35) on a quadratic spline in the number of missed school days, a dummy for more than 25 missed school days, interactions of the two, year dummies, the interaction between a 1998 dummy and the quadratic splines, and interaction between a dummy for more than 25 missed school days and the 1998 dummy. This last interaction term is small (-0.1 p.p.) and insignificant at conventional levels (p-value 0.361), implying that the discontinuity in the density of missed school days is essentially the same in 1996/1997 and 1998.

superimposed to this series the difference between school outcomes of those observed in 1998 and those observed in 1996/1997. To make these outcomes comparable across years, I have computed the number of additional grades attended after three years (the maximum time lag for those observed in 1998). As above, I have artificially censored this variable for non-failers by computing the number of additional grades attended after 2 years for these individuals. Again, in the figure, I have standardized this series to its value at 20 missed school days. It is remarkable that these two series appear almost like the mirror image of one another. It is noticeable in particular that the reduction in failure rates at 26 missed school days that happened in 1998 relative to the previous years translates into a significantly higher number of grades attended for these students, suggesting again a negative effect of failure rate on later outcomes. Notice that this negative correlation appears to hold elsewhere along the distribution of missed school days.

To formalize the evidence in Figure 6, in Table 5 I report the results of an IV regression of the number of additional grades attended after 3 years (2 years for non-failers) on the probability of failing a grade instrumented by the interaction of number of missed school days with a dummy for 1998. Regressions control for additive dummies for missed school days and year dummies. As before, I report regressions without and with additional controls. Regressions refer again to observations with 21-35 missed school days.[29] Point estimates imply that grade failure is associated to between 0.58 and 0.65 less years of education, depending on whether controls are included or not, so smaller than the RD estimates, but not significantly different from them.[30] Reassuringly, an over-identification test reported at the bottom of the table shows that one cannot reject the hypothesis that the instruments are exogenous. In sum, results based on differences-in-differences that exploit the change in the discipline governing grade failure between 1996/1997 and 1998 lead to similar conclusions on the effect of grade failure on subsequent school outcomes.[31] If potentially they suggest some role for unobserved heterogeneity (in the form of positive sorting), this effect appears again to be modest.

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[29] Results hold also on different subsamples, including observations with 1-25 missed days (so away from the discontinuity point). Point0020estimates tend to become increasing large in absolute value when I restrict to individuals with lower absenteeism.

[30] When the dependent variable is number of additional grades attended after three years (as in Table 5), the RD estimate is -0.809 (s.e. 0.069) (controls included).

[31] Since, as said, age and sex are missing in 1998 and hence for individuals first observed in that school year, the values of these covariates for these individuals is completely subsumed by a dummy for missing age, sex (and age-grade distortion).

5.5 Endogenous mobility

Because of the nature of the data, that only refer to students in public non-vocational secondary schools, in the previous sections I might have erroneously classified students who move to vocational or private schools as drop-outs, and assigned them zero additional years of education while these students in fact pursue their studies elsewhere. This might potentially lead to downward biased estimates of the consequences of grade failure, if failers are more likely than passers to leave *Liceos* for schools outside the system. Indeed, I have shown above that repeaters are more likely to move across *Liceos* the year following grade failure, so mobility across different sub-systems might be a serious concern. In practice, though, there are two pieces of evidence suggesting that this is unlikely to be the case.

First, evidence from a follow-up phone survey of 660 individuals who dropped out of the first year of *Ciclo Básico* in *Liceos* in 1997 reported in ANEP (2000) shows that of these only around 1.6% had moved to a private institution and 15% had moved to a *UTU* in 1998. Since the average differential in drop-out between failers and non failers in grade seven is in of 42.2%, even in the unlikely event in which it is only grade failers to move, this would still imply a larger drop-out (of around 25 p.p.=0.42-0.15-0.02) among grade failers.

As a second check, I have used micro data from the 1999 National Learning Census (*Evaluation censal de aprendizajes en terceros años del ciclo medio*) that collects socio-economic characteristics and standardized test scores on all students (in both vocational and non-vocational schools and in both private and public institutions) in ninth grade in 1999. I have linked these data to the 1998 administrative records via a unique student identifier. This allows me to analyze the destination state of students enrolled in public non-vocational schools in 1998. I restrict to those who either failed ninth grade or passed eight grade in 1998, i.e. individuals potentially in ninth grade in 1999. Column (1) of Table 6 reports the proportion of these students who were registered in public non-vocational schools at the beginning of 1999 based on the administrative records. While 97% of grade passers were still in the system at the beginning of 1999, among grade failers this proportion is 67%. Previously, I have classified those not in school (3%=1-0.97 of grade passers and 33%=1-0.67 of grade failers) as drop-outs. The Census data in column (2) show that around 88% of passers were in public non-vocational schools during the Census period, while this proportion is around 46% for grade failers. One limitation of the Census is that it only refers to students at school in the Census reference period, so one cannot distinguish between actual drop-outs and those who were temporarily absent from school over that reference period. The difference between column (1) and column (2) hence can be ascribed to either students having in the meantime exited permanently the public non-vocational school system (true drop-outs or individuals who switched to a private or vocational school after the beginning of 1999) or students being temporarily absent during the Census reference period. Columns (3) and (4) show no appreciable differences among grade repeaters and passers in the proportion taking the exam in private or vocational schools. This proportion is in the order of 1.58% for repeaters and 0.76% for passers. If one is willing to assume that all individuals originally registered in the public non vocational system (column (1)) who do not appear in the Census (column (1) minus column (2)) were temporarily absent, and that the probability of being absent is the same in all types of institutions (public and private, vocational and non-vocational), this gives an estimate for the proportion enrolled in private or vocational schools in the order of 0.84% (0.76/(1-87.98/97.37)) among passers and 2.30% (1.58/(1-46.13/67.01)) among failers. This would lead to a 'correctly' estimated differential in drop-out between passers and failers of 28.9 p.p. (97.37+0.84-(67.01+2.30)) which is around 7% lower than the estimate based on administrative data (30.36 p.p.=97.37-67.01).[32] Overall, although there is some evidence of grade failers being more likely than grade passers to migrate to private and non-vocational schools, this is likely to induce only a negligible bias in the estimates above.

6. Discussion and Conclusions

School systems in poorer countries are typically characterized by remarkably high failure and drop-out rates and overall low school attainment. Uruguay is no exception to this pattern: failure rates in Junior High school are in the order of 30% and about a fourth of individuals aged 25-29 declare not having completed this level of compulsory education.

This paper uses administrative longitudinal micro data on the universe of students enrolled in public non-vocational Junior High school in Uruguay between 1996 and 1997 to assess the cost of grade failure as measured by its effect on students' subsequent school outcomes. Exploiting the discontinuity in promotion induced by a rule that establishes that a pupil missing more than 25 days during the school year will automatically fail that grade, I

[32] If, by converse, one takes the view that the Census data provide the correct estimate of enrolment in different types of schools, one would find an even larger gap in drop-out between failers and non failers (this is 40.95=87.9+0.59+0.17-46.13-0.43-1-.15).

show that grade failure leads to lower educational attainment (in the order of half to three fourths of a school grade) 4 to 5 years after failure.

A large part of this disadvantage is due to immediate drop-out: compared to non-failers, failers are at disproportionate risk of abandoning school the year following failure. However, even among those who stay on (and hence repeat), the effect of grade failure appears long lasting. In the short term, repeaters tend to partially make up for the missed grade. This is in great part due to repeaters exerting more effort compared to passers. This initial advantage however is lost after two years: ultimately, even among the presumably positive selected sample of grade failers who stay on, outcomes appear worse than otherwise identical individuals who did not fail in Junior High school. Intermittent attendance is unlikely to give reason of this result.

A major concern in the analysis is that the variable whose discontinuous relationship with failure rates I use in order to identify the causal effect of grade failure on subsequent school outcomes, namely missed school days, is largely under the students' control (as well as possibly under the schools' control, if school records can be manipulated). To the extent that students sort around the discontinuity point in a fashion that is correlated with their latent outcomes (i.e. the ones that would be observed in the absence of grade failure), and in particular if otherwise better (worse) performing students end up locating below (above) the 25 missed school days threshold, one might be concerned that the RD estimates pick up unobservable differences in students' characteristics on either side of the threshold, rather than the genuine effect of the policy. Evidence from the distribution of missed school days shows a clear clustering of individuals at 25 missed school days. Although - as shown in the paper - this is consistent with the incentives of the promotion rule and does not necessarily imply that random assignment around the discontinuity point is violated, one might still be concerned about this feature of the data. In order to try to deal with this potential source of inconsistency of the regression estimates, I propose two strategies. First, I include in the regressions a number of observable controls. To the extent that individuals with different latent outcomes are able to systematically sort on either side of the discontinuity point, one would expect estimates to be affected by the inclusion of students' observable characteristics. Although there is some evidence of manipulation (with girls being more likely to locate below the threshold, but no significant differences across individuals with different accumulated school delay), results appear to be essentially unaffected by the inclusion of these controls. Second, I use a regime change that took place during the school year 1998 to derive a differences (across years, 1998 and 1996/1997) in-differences (across individuals with different numbers of missed school days) estimate of the effect of grade failure on subsequent school outcomes that does not rely on random assignment around the 25 days threshold. This strategy leads essentially to similar results as the RD design. Similarly to what found when controls are included, there is some evidence of positive sorting, but this is rather inconsequential in terms of the main results of the paper.

One limitation of the data is that these refer only to individuals within the public non-vocational system. To the extent that grade failers are disproportionately at risk of migrating to private or vocational schools, I might be overestimating the negative effect of this policy on later school outcomes. Complementary evidence from two additional data sets suggests that this source of bias is negligible and unable to account for my results. Note, additionally, that because non-failers are at much higher risk of being in the sample by the end of the period, my estimates tend presumably to underestimate the negative gap in subsequent educational attainment between failers and non-failers.

In sum, the data show high costs of grade failure as measured by failers' worse subsequent school outcomes. Although this paper concentrates on the costs of grade failure, it must be emphasized that the evidence presented here suggests that the benefits of this policy due to its deterrence effect on students' underperformance might be non-negligible. This is probably the ultimate reason why repetition policies exist. Simple back of the envelope calculations show that for such an incentive effect to offset the cost of repetition, one would expect 22% of individuals to accumulate one extra year of schooling due to the threat effect of the rule.[33] In practice, it appears that one should have high incentive effects to compensate for what I estimate being the high costs of grade failure among the ones (30% of the population) who eventually incur the penalty.

Precisely because of this tradeoff, in the USA the emphasis now seems to have shifted towards policies the combine grade retention – so to preserve the incentive effect – with remedial interventions - to attenuate the negative consequences of repetition and potentially make failure less likely in the future (for the experience of the Chicago Public School - a front-runner in implementing these policies, see Roderick et al. (1999). Recent experimental evidence shows substantial gains from informal inexpensive remedial education among more disadvantaged children in India (Banerjee et al., 2006), suggesting that even in developing countries remedial education might be a viable alternative to widespread repetition.

[33] I estimate the effect of grade failure to lead on average to around 0.22 less years less of education (this is the effect of grade failure on outcomes, -0.747 – from Table 2, column (4), bottom row - times the proportion of failers, that is 30%). This corresponds to one extra year of education for all grade passers with at least 13 missed school days (and at least one extra year of education).

Chart 1
The Uruguayan School System

School Cycle	Grades	Theoretical ages	Compulsory	School type(s)
Educación Primaria	1-6	6-11	yes	*Escuelas*
Ciclo Básico	7-9	12-14	yes	*Liceos/UTU*
Educación Secundaria	10-12	15-17	no	*Liceos/UTU*

Notes. The chart summarizes the structure of the Uruguayan school system during the period of analysis.

Chart 2
Grade Promotion in *Ciclo Básico*

School year					
1996 & 1997			1998		
	Debts			Debts	
	<=3	>3		<=3	>3
Missed days			Missed days		
<=20	Pass	Fail	<=20	Pass	Fail
21-25	Pass at school discretion	Fail	21-25	Pass at school discretion	Fail
>25	**Fail**	Fail	>25	**Pass at school discretion**	Fail

Notes. The chart summarizes the rules governing grade promotion in *Ciclo Básico* in the school years 1996 and 1997 (left hand side panel) and 1998 (right hand side panel).

Table 1
Descriptive Statistics: Students in *Ciclo* Bás*ico* - 1996-1997
Public non-Vocational Schools Only

		All			Failers			Passers		
		Grade			Grade			Grade		
		7	8	9	7	8	9	7	8	9
1.	Age distortion	0.640	0.638	0.946	1.368	1.276	1.545	0.318	0.401	0.670
2.	% girls	0.523	0.560	0.562	0.431	0.507	0.501	0.563	0.580	0.590
3.	Missed school days	19.703	18.084	19.861	46.002	41.977	40.82	8.058	9.207	10.213
4.	Failed	0.307	0.271	0.315	1.000	1.000	1.000	0	0	0
5.	% censored (2001)	0.470	0.346	0.160	0.140	0.137	0.109	0.616	0.423	0.183
6.	Additional grades attended (censored)	1.991	1.910	1.523	0.534	0.567	0.486	2.636	2.409	2.000
	Observations	58,350	47,503	42,180	17,908	12,868	13,293	40,442	34,635	28,887

Notes. The table reports information on students in non-vocational public *Ciclo Básico* in 1996 and 1997. The left hand panel refers to the entire sample, the middle panel to those who failed a grade and the right panel to those who passed. For each panel separate information by grade is reported. Row 1 reports age distortion (age-grade-6), row 2 the proportion of girls, row 3 the number of missed school days during the school year, row 4 whether the student failed that grade, row 5 the proportion still in school in 2001 and row 6 the number of additional grades attended until the year 2001 if a student failed a grade or until 2000 if a student did not fail. Source: *Bases de datos de rendimiento a nivel de estudiantes en educación secundaria*, Administración Nacional de Educación Publica, Uruguay.

Table 2

Missed School Days, Grade Failure in *Ciclo* Bás*ico* (1996-1997) and Subsequent School Attainment

	(1)	(2)	(3)	(4)
	First stage Dependent variable: Grade failure t=0			
I(Missed $days_0$>25)	0.428	0.401	0.415	0.387
	(0.016)***	(0.014)***	(0.018)***	(0.018)***
	Reduced form Dependent variable: Additional grades			
I(Missed $days_0$>25)	-0.330	-0.288	-0.344	-0.289
	(0.033)***	(0.032)***	(0.033)***	(0.038)***
	Instrumental Variables Dependent variable: Additional grades			
Grade failure t=0	-0.771	-0.718	-0.831	-0.747
	(0.093)***	(0.087)***	(0.067)***	(0.079)***
School dummies		yes		yes
Grade dummies		yes		yes
Year dummies		yes		yes
Age dummies			yes	yes
Sex dummies			yes	yes
Age-Grade dummies			yes	yes
Observations	8,453	8,453	8,453	8,453

Notes. The top panel reports the OLS coefficients from a regression of a dummy equal one for grade failure on a dummy equal one for more than 25 missed school days in *Ciclo Básico* (first stage equation). The middle panel reports the coefficient of the number of additional grades attended by the end of the period (censored to the year 2000 for non-failers) on the dummy for more than 25 missed school days (reduced form equation). The bottom panel reports the IV estimates of the effect of grade failure on the number of additional grades attended, where grade failure is instrumented by a dummy for missed school days greater than 25. All specifications include a second order polynomial in the number of missed days interacted with a dummy for missed school days greater than 25. Standard errors in brackets are clustered by number of missed school days. ***, **, * denote respectively significant at 1%, 5% and 10% level. Sample refers only to individuals with between 21 and 35 missed school days. See also notes to Table 2.

Table 3
Grade Failure in *Ciclo* Bás*ico* (1996-1997) and Subsequent School Outcomes
All Controls Included
IV estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Survival at time t=		Duration	Intermittent attendance			Additional grades at time t=	
	1	2	3	4			1	2	3	4
Grade failure t=0	-0.205 (0.032)***	-0.176 (0.047)***	-0.258 (0.040)***	-0.064 (0.040)	-0.730 (0.108)***	-0.066 (0.046)	-0.886 (0.009)***	-0.743 (0.070)***	-0.862 (0.081)***	-0.839 (0.085)***
Observations	8,453	8,453	8,453	8,453	8,453	6,295	8,453	8,453	8,453	8,453

Notes. Entries are IV estimates of the effect of grade failure at time t=0 on the dependent variable (in the top row) where grade failure is instrumented by the discontinuity at 25 missed school days. Columns (1) to (4) refer to the probability of being in the sample in any given year (1,..., 4) following grade failure. Column (5) to duration. Column (6) to intermittent attendance. Columns (7) to (10) to the number of additional grades attended by the individuals in the sample in any given year (1,...4) following t=0. All regressions include a quadratic polynomial in number of missed school days interacted with a dummy for more than 25 missed school days, plus dummies for: age, grade, age minus grade, sex, year and school. See also notes to Table 2.

Table 4
Grade Failure in *Ciclo* Bás*ico* (1996-1997) and Subsequent School Outcomes
Repeaters only - All Controls Included

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Repeat t=0	In sample t=2	Change School t=1	Duration	Additional grades	Fail t=1	Fail t=1 (controls for missed days at t=1)	Missed days t=1
I(Missed days$_0$>25)	0.427 (0.010)***							
Grade Failure t=0		-0.059 (0.037)	0.106 (0.030)***	-0.353 (0.142)**	-0.673 (0.111)***	-0.286 (0.056)***	-0.093 (0.041)**	-24.402 (6.656)***
Observations	6,295	6,295	6,295	6,295	6,295	6,295	6,295	6,295

Notes. The table refers to individuals in school at time t=1 only. Column (1) reports the OLS estimate on a dummy for more than 25 missed school days at time t=0 on the probability of repetition (first stage). Columns (2)-(8) report IV estimates of the effect of grade repetition at time t=0 on a number of outcomes variables (as in Table 3). Column (2) refers to the probability of being in the sample 2 yeas after grade failure (t=2). Column (3) to the probability of changing school in the year following grade failure (t=1). Columns (4) and (5) to duration and (censored) additional grades attended. Column (6) and (7) to the probability of grade failure at time 1. Column (8) to the number of missed school days at time 1. See also notes to Table 3.

Table 5
Missed School Days, Grade Failure in *Ciclo* Bás*ico* (1996, 1997 versus 1998) and Subsequent School Outcomes
Differences-in-differences - IV Estimates

	(1)		(2)	
	Dependent variable: Additional grades			
Grade Failure t=0	-0.650 (0.205)***	-0.591 (0.190)***	-0.627 (0.199)***	-0.579 (0.185)***
School Dummies		yes		yes
Grade Dummies		yes		yes
Year Dummies		yes		yes
Age Dummies			yes	yes
Sex Dummies			yes	yes
Age-Grade Dummies			yes	yes
Overid-test [p-value]	11.150 [0.598]	8.406 [0.395]	8.026 [0.626]	6.061 [0.300]
Observations	15,024	15,024	15,024	15,024

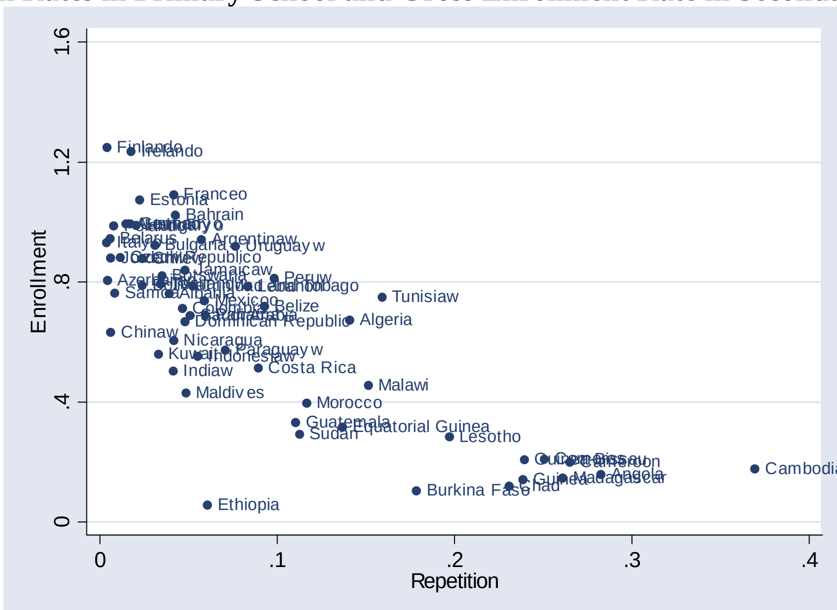
Notes. The table reports IV estimates of the effect of grade failure on the number of additional grades attended for those observed in *Ciclo Básico* in 1996, 1997 or 1998. Dependent variable is additional grades attended after 3 years (2 year for non-failers). Grade failure is instrumented by the interaction of missed school days with a dummy for the year 1998. All regressions control for additive year and missed school days dummies and they only refer to individuals with 21-35 missed school days. Robust standard errors reported. An over-identification test (p-value in brackets) is reported at the bottom of the table. See also notes to Table 2.

Table 6
Destination State of Grade Passers and Failers in 1999
Students Enrolled in Public Non-Vocational Schools in 1998 and Eligible for Enrollment in Ninth Grade in 1999

	(1)	(2)	(3)	(4)
	Administrative data	Census data		
	Registered in Public non-vocational school	Public non-vocational school	Private non-vocational school	Vocational school
Passers in 1998	97.37	87.98	0.59	0.17
Failers in 1998	67.01	46.13	0.43	1.15

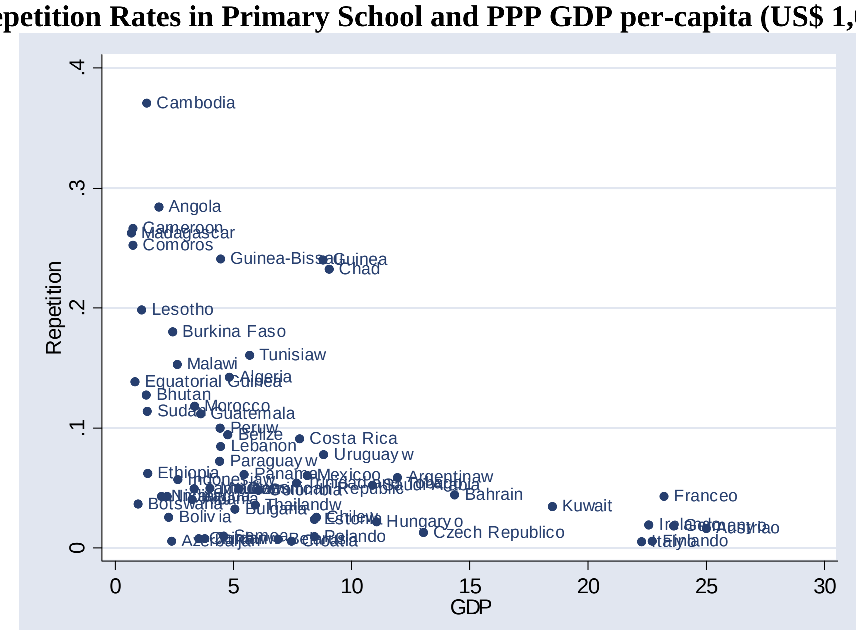
Notes. The table refers to individuals in public non-vocational *Ciclo Básico* in 1998 according to the *Bases de datos de rendimiento a nivel de estudiantes en educación secundaria, de la Administración Nacional de Educación Publica*. Passers are individuals in grade 8 in 1998 who were promoted to grade 9. Failers are those in grade 9 in 1998 who failed. Column (1) reports the proportion enrolled in grade 9 in public-non vocational schools in 1999 according to the same data. Entries in column (2) to (4) are the proportions of individuals observed in 1998 by type of school in 1999 based on the *Base de datos de la evaluación censal de aprendizajes en terceros años del ciclo medio 1999*, Administración Nacional de Educación Publica, Uruguay. Column (2) refers to individuals in public non-vocational schools, column (3) to individuals in private non-vocational schools and column (4) to individuals in vocational schools.

Figure 1
Repetition Rates in Primary School and Gross Enrollment Rate in Secondary School



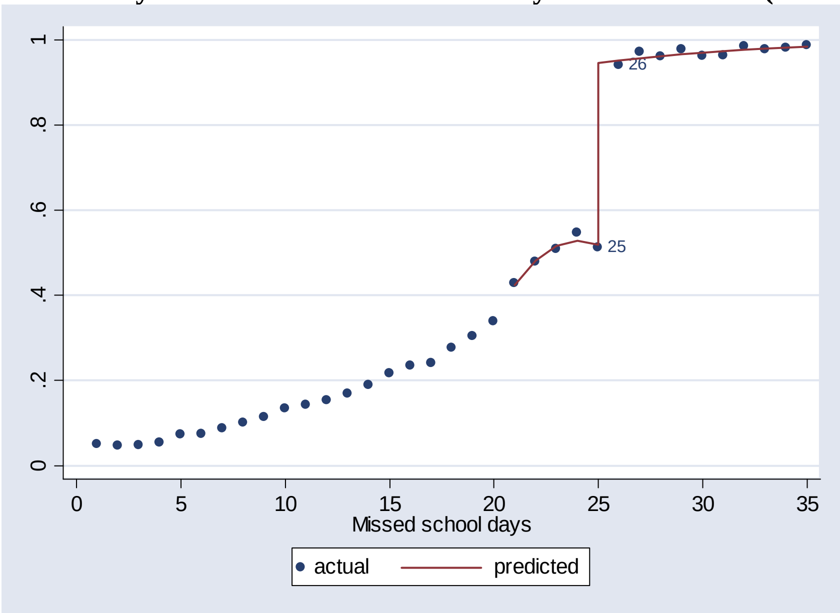
Notes. The graph plots repetition rates in primary school on the horizontal axis and gross enrollment in secondary school on the vertical axis. The sample refers to countries with income per-capita not greater than US$ 28,000. Source: UNESCO (2002).

Figure 2
Repetition Rates in Primary School and PPP GDP per-capita (US$ 1,000)



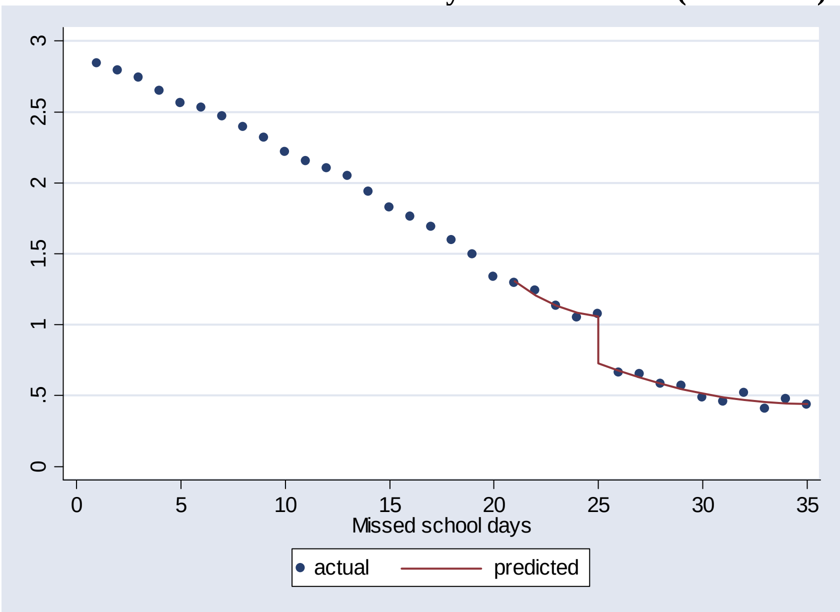
Notes. The graph plots (1999) PPP GDP per-capita on the horizontal axis and repetition rates in primary school on the vertical axis. See also notes to Figure 1.

Figure 3
Grade Failure by Number of Missed School Days in *Ciclo* Bás*ico* (1996-1997)



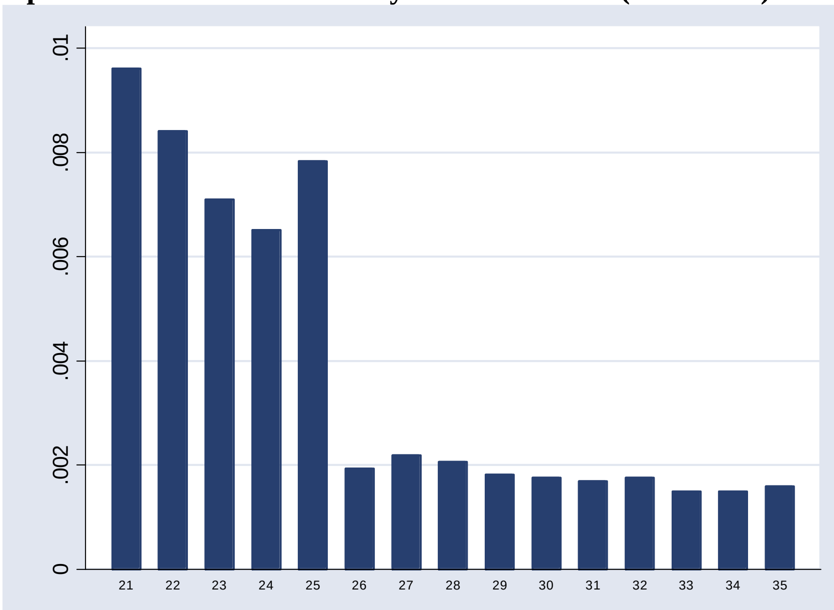
Notes. The figure reports the proportion of individuals failing a grade by number of missed school days in the year. The data refer to individuals in *Ciclo Básico* in 1996 or 1997 and with a number of missed school days between 1 and 35. A parametric spline between 21 and 35 missed school days is superimposed to the data. Source: *Bases de datos de rendimiento a nivel de estudiantes en educación secundaria*, Administración Nacional de Educación Publica, Uruguay.

Figure 4
Additional Grades Attended by 2001 (2000 for non-failers)
and Number of Missed School Days in *Ciclo* Bás*ico* (1996-1997)



Notes. The figure reports the number of additional grades attended by 2001 among those in *Ciclo Básico* in 1996 or 1997 as a function of the number of missed school days in *Ciclo Básico*. The series is censored to the year 2000 for those who did not fail. A parametric spline between 21 and 35 missed school days is superimposed to the data. See also notes to Figure 4.

Figure 5
Proportion of Missed School Days in *Ciclo Básico* (1996-1997) - 21-35



Notes. The figure reports the distribution of number of missed school days between 21-35 (as a proportion of missed days between 0 and 180). See also notes to figure 3.

Figure 6
Failure Rate and Additional Grades Attended
Difference between 1998 and 1996/1997

Notes. The figure reports the difference (between 1998 and 1996/1997) in difference (relative to 20 missed school days) in failure rates (solid line) and additional grades attended after 3 years - 2 years for grade passers – (dotted line) by number of missed school days (1-35) in *Ciclo Básico*.

Table A1
Missed School Days, Grade Failure in *Ciclo Básico* (1996-1997) and Subsequent School Attainment
Individuals with 21-35 missed school days
Estimates By Grade – All controls included

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Fail	Additional grades (censored)	Survival at time t=				Duration	Intermittent attendance	Additional grades at time t=			
			1	2	3	4			1	2	3	4
7th grade												
I(Missed $days_0$>25)	0.330 (0.027)***											
Grade Failure t=0		-0.799 (0.116)***	-0.323 (0.080)***	-0.188 (0.148)	-0.285 (0.044)***	-0.094 (0.081)	-0.841 (0.295)**	-0.034 (0.124)	-0.932 (0.017)***	-0.917 (0.109)***	-0.955 (0.124)***	-0.947 (0.116)***
Observations	3,136	3,136	3,136	3,136	3,136	3,136	3,136	2322	3,136	3,136	3,136	3,136
8th grade												
I(Missed $days_0$>25)	0.432 (0.033)***											
Grade Failure t=0		-0.463 (0.212)**	-0.165 (0.059)**	-0.089 (0.063)	-0.050 (0.056)	0.044 (0.105)	-0.353 (0.195)*	0.093 (0.076)	-0.931 (0.027)***	-0.734 (0.073)***	-0.673 (0.185)***	-0.514 (0.231)**
Observations	2,738	2,738	2,738	2,738	2,738	2,738	2,738	2117	2,738	2,738	2,738	2,738
9th grade												
I(Missed $days_0$>25)	0.396 (0.037)***											
Grade Failure t=0		-1.087 (0.241)***	-0.155 (0.070)**	-0.260 (0.107)**	-0.494 (0.181)**	-0.153 (0.077)*	-1.063 (0.328)***	-0.210 (0.087)**	-0.771 (0.031)***	-0.553 (0.130)***	-0.997 (0.173)***	-1.142 (0.252)***
Observations	2,579	2,579	2,579	2,579	2,579	2,579	2,579	1856	2,579	2,579	2,579	2,579

Notes. The table reports the same (first stage and IV) estimates as in Tables 2 and 3 separately by grade.

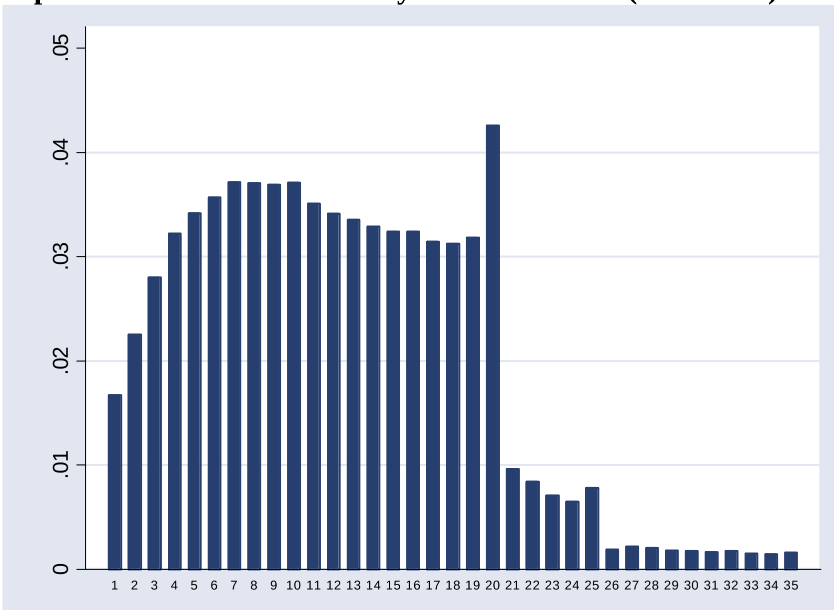
Table A2
Grade Repetition in *Ciclo* Bá*sico* (1996-1997) and Subsequent School Outcomes
Individuals with 21-35 Missed School Days at Time t=0 in School at Time t=1- All Controls Included
Estimates By Grade – All controls included

	(1) Repeat t=0	(2) In sample t=2	(3) Change School t=1	(4) Duration	(5) Additional grades	(6) Fail t=1	(7) Fail t=1 (controls for missed days at t=1)	(8) Missed days t=1
7th grade								
I(Missed days$_0$>25)	0.395							
	(0.032)***							
Grade Failure t=0		-0.016	0.001	-0.298	-0.665	-0.154	-0.062	-19.511
		(0.141)	(0.068)	(0.339)	(0.128)***	(0.102)	(0.062)	(18.976)
Observations	2,322	2,322	2,322	2,322	2,322	2,322	2,322	2,322
8th grade								
I(Missed days$_0$>25)	0.442							
	(0.025)***							
Grade Failure t=0		0.073	0.379	0.198	-0.308	-0.297	-0.053	-29.782
		(0.072)	(0.070)***	(0.227)	(0.241)	(0.113)**	(0.097)	(8.357)***
Observations	2,117	2,117	2,117	2,117	2,117	2,117	2,117	2,117
9th grade								
I(Missed days$_0$>25)	0.426							
	(0.037)***							
Grade Failure t=0		-0.168	-0.243	-0.795	-1.094	-0.448	-0.222	-14.755
		(0.090)*	(0.094)**	(0.247)***	(0.217)***	(0.128)***	(0.104)*	(9.890)
Observations	1,856	1,856	1,856	1,856	1,856	1,856	1,856	1,856

Notes. The table reports the same regressions as in Table 4, separately by grade.

Figure A1

Proportion of Missed School Days in *Ciclo Básico* (1996-1997) - 1-35

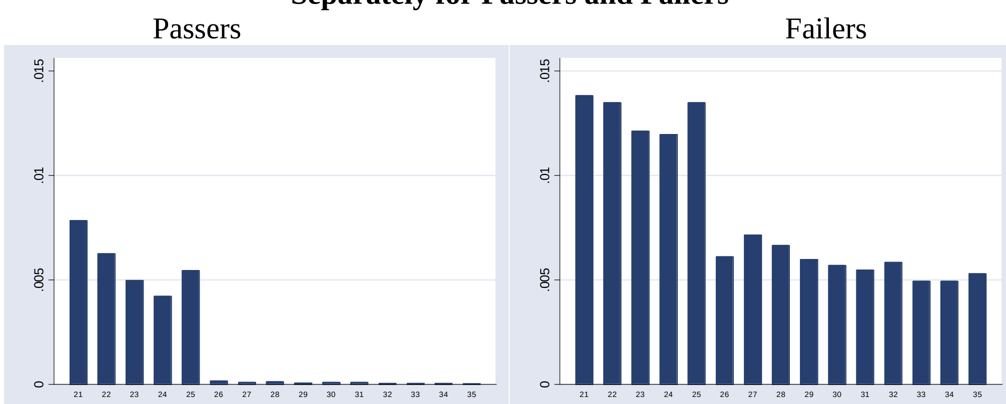


Notes. The figure reports the distribution of number of missed school days between 1-35 (as a proportion of missed days between 0 and 180). See also notes to figure 3.

Figure A2

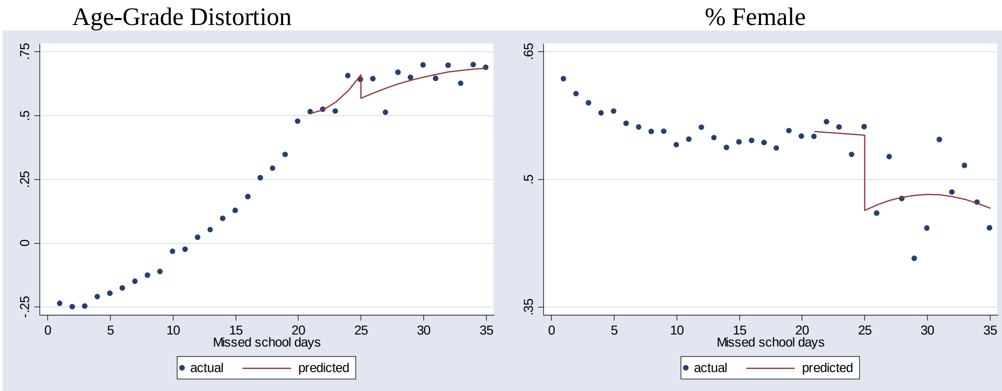
Proportion of Missed School Days in *Ciclo Básico* (1996-1997) - 21-35

Separately for Passers and Failers



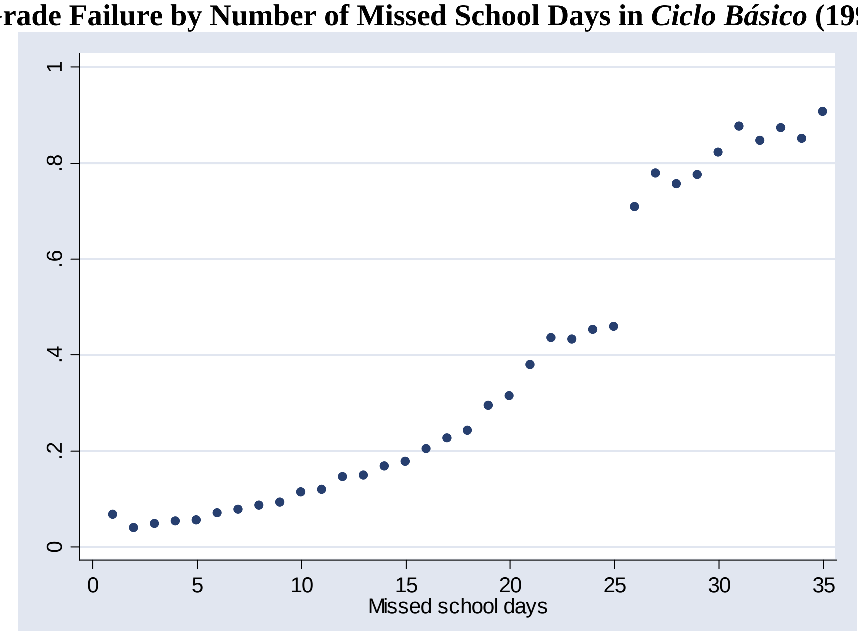
Notes. The figure reports the distribution of number of missed school days between 21-35 (as a proportion of missed days between 0 and 180) separately for those who passed the school year and those who failed. See also notes to figure A1.

Figure A3
Average Covariate Values by Number of Missed School Days in *Ciclo Básico* (1996-1997)



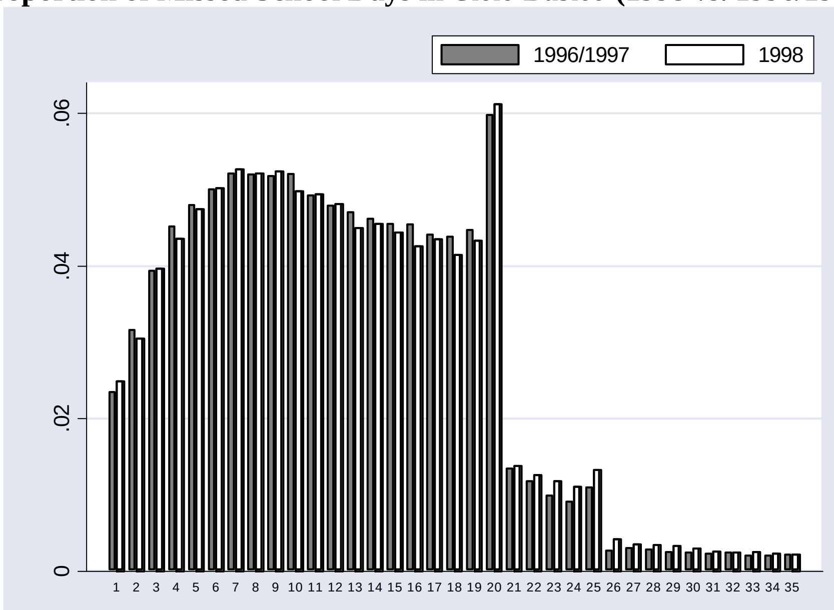
Notes. The figure reports average age-grade distortion (age-grade-6) and the proportion of females by number of missed school days.

Figure A4
Grade Failure by Number of Missed School Days in *Ciclo Básico* (1998)



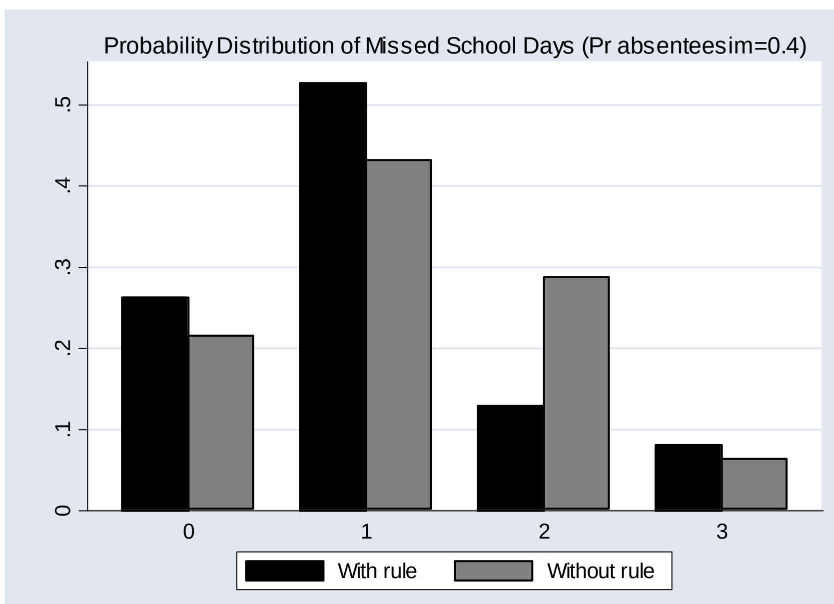
Notes. The figure reports the proportion of individuals failing a grade by number of missed school days in the year. The data refer to individuals in *Ciclo Básico* in 1998. See also notes to Figure 3.

Figure A5
Proportion of Missed School Days in *Ciclo Básico* (1998 vs. 1996/1997)



Notes. The figure reports the distribution of number of missed school days between 1-35 separately for the school years 1996/1997 and 1998.

Figure A6
Simulated Effect of the Rule on Absenteeism



Notes. The figure reports the distribution of missed school days generated by the model in the appendix. The school year is assumed to last three years. I report two distributions, one for the case where the individual gains from each additional day of school (No rule), and one where the individual fails (zero payoff) if he has more than 1 missed school day (With rule). See Appendix for details.

Data Appendix

The main data used in this paper come from the *Bases de datos de rendimiento a nivel de estudiantes en educación secundaria, de la Administración Nacional de Educación Publica* (ANEP). Micro data refer to the years 1996, 1997, 1998 and 2000 (no data is available for the year 1999) and contain information on grade and institution attended, final year result (if passed or failed), number of missed school days, age and sex (only up to 1997). Data for each year have a longitudinal component since they also report information on whether a student registered the following year. For example, data for 1998 also record if the student enrolled in 1999 (for which data are missing). The same applies to the year 2000, based on which one can derive information on enrollment in the year 2001. In this way – and despite administrative records for the years 1999 and 2001 not being available- one can (somewhat) follow individuals as they progress through the public non-vocational secondary school system from 1996 to 2001 (this obviously excludes new entrants or those who temporarily left the system).

Because a student's age is an important variable in my analysis and because promotion rules changed in 1998, below I only restrict to those individuals who transited through *Ciclo Básico* between 1996 and 1997 and I follow their school progression over time. Given the available data, I do not know whether those who registered in a certain grade in 1999 or 2001 eventually passed or failed that school grade. Because of this, I measure school progression in terms of additional grades attended (rather than completed).

Finally note that some schools happen not to be included in the sample although this problem tends to be less serious at the end of the period: the number of missing institutions is 56 in 1996, 59 in 1997, 13 in 1998 and 4 in 2000 (out of around 250 schools).

Technical Appendix

I present a stylized model of school attendance that captures the main features of the discipline governing grade promotion in Uruguayan schools. I assume that individuals with a number of missed school days greater than a given threshold will automatically fail a grade. In the presence of ex-ante identical individuals and unexpected (non-negligible) shocks to individuals' daily cost of (or returns to) school attendance, the model predicts a spike in the density of missed school days at the threshold and some non negligible mass above it. This suggests that –in principle- pure 'luck' might generate a discontinuity in the density of the variable missed school days (as the one observed in Figure 5), without this implying any violation of the random assignment hypothesis.

Assume that the school year lasts 3 days and that students missing more than 1 school day automatically fail. The payoffs from attending different number of days are reported below. A student needs to attend at least two days to pass. So the return from attending the fist school day only accrues to him on his second school day. Each additional school day above 2 yields a return.

School days	Missed school days	Payoff
0	3	0
1	2	0
2	1	2V
3	0	3V

Attendance is costly. Cost C_t is random and uncorrelated across periods. I write the student's problem as a dynamic optimization problem. In each period the student maximizes:

$$V_M^t = \text{Max}\{R_M^t(1)+E(V_M^{t+1}|d_t=1), R_M^t(0)+E(V_{M+1}^{t+1}| d_t=0)\}$$

where V_M^t is the continuation value at time t conditional on having missed M days of school so far, d_t is a dummy equal 1 if the individual attends school at time t and $R_M^t(d_t)$ is the net payoff from choosing action d_t conditional on having accumulated M days of absences so far. I have solved the problem backward starting from the last period and I have simulated the distribution of missed school by the end of the school year assuming that the distribution of C is normal and that average absenteeism rate is 0.4. I report two distributions: one generated in the presence of the above rule and one where no rule is in force. The distribution with no rule is obtained assuming that individuals go to school in each period if $V>C_t$ and the payoff if V for each additional school day. These two distributions are reported in Figure A6.

A few observations are in order. First, with no rule, the distribution of missed school days is continuous (bell shaped). Individuals will locate between 0 and 3 missed school days depending on the magnitude of the shocks hitting them. When the rule is introduced, the mass of the distribution below or at the threshold (0 or 1 missed school days) increases. The reason for this is simple. Individuals will exert extra effort in an attempt to avoid the penalty associated to missing more than one school day. Some will be lucky and just make it. Others might even 'overshoot'. In an attempt to avoid the penalty, they will act conservatively in the first two periods and accumulate no missed school days. If in the last period they are hit by a relatively good shock (i.e. low cost), it will be worth for them to attend. This increases the mass at 0 missed school days. By converse, the proportion of individuals at 2 missed school days falls, although this is still positive. These are individuals with 1 missed school day at the beginning of period who are hit by a bad shock at time 3. Notice that the proportion of those at 3 missed school days also increases. These are individuals who happen to miss the first two school days because of high costs. Under the rule, there is no incentive for them to attend an additional school day.

The model delivers some interesting implications. First, it shows clearly that the rule acts as a strong deterrent again underperformance. It effectively creates and incentive to elicit effort via reduced absenteeism. Second, the model illustrates clearly that, although there is some bunching at the threshold, hence leading to some discontinuity in the density of missed school days, assignment around the threshold is as good as random assignment. Children happen to be on either side simply due to good or bad luck (since ex-ante they are identical). In particular those at 2 missed days tried to game the system but were unsuccessful. This is a case where the discontinuity in the density test proposed by Lee (2005) and McCrary (2007) does not necessarily lead to a break in (one of) the identification assumptions required for consistency of the RD estimator. [34] Third, the model shows some perhaps unintended consequences of this policy. The proportion of those with a very high (3) number of missed school days increases. This is because the rule eliminates any incentive to attend on the last day among those who have already missed 2 days.

[34] Obviously this does not rule out that in the presence of heterogeneity, assignment around the discontinuity point might not be random, and that in that case the density might be discontinuous.

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