Grade Failure, Drop out and Subsequent School Outcomes: Quasi-Experimental Evidence from Uruguayan Administrative Data

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This paper uses administrative longitudinal micro data on about 100,000 Uruguayan students in public non-vocational Junior High school (grades 7-9) to identify the effect of grade failure on students' subsequent school outcomes. Exploiting the discontinuity in promotion rates induced by a rule that establishes that a pupil missing more than 25 days during the school year will automatically fail that grade I show that grade failure leads to substantial drop out and lower educational attainment after 4 to 5 years since the time when failure first occurred. Complementary evidence based on a change in the regime of grade promotion provides additional support for this conclusion.

Keywords: grade retention, school drop out, age-grade distortion, regression discontinuity.
JEL codes: I21, I22, J20

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Grade repetition is particularly widespread in Latin America in both primary and Secondary school. In Brazil for example repetition rates in primary school are in the order of 20%. Nicaragua, Peru and Uruguay follow suit with repetition rates between 8% and 10%. In lower secondary education repetition rates are the highest in Brazil (17%) followed by Uruguay (12%).[1] Similar or higher repetition rates can be found in other regions of the world. Repetition rates are of similar magnitude in North Africa and the Middle East. In Tunisia for example repetition rates in Junior secondary are 21% and similar rates can be observed in Morocco. By far repetition rates are the highest in Sub-Saharan Africa. In Burundi for example repetition in Junior secondary is 37% and in South Africa is 13%. Although repetition is not a phenomenon unique to developing countries, rates in developed countries are generally much lower. In Italy and Germany, two developed countries where repetition occurs, for example, repetition rates in lower secondary education are between 2% and 3%. The USA is probably an exception among developed countries. Although the US Department of Education does not provide official figures on repetition, estimates from the CPS for the 1990s show that around 12% of individuals aged 12-15 have repeated at least a grade (Cascio (2005)). This despite widespread (but declining) 'social promotion', i.e. the practice of passing students failing to meet performance standards.

In many developing countries grade retention policies are often accompanied by low enrollment and high drop out, the combination of the two often referred to as 'wastage' (for evidence on Latin America see Urquiola and Calderon (2004)). Figure 1 plots gross enrollment rates in secondary school on average (across grades) repetition rates in primary school for 61 countries. 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. 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

[1] Data refer to 2002 and come from the World Bank EdStats (http://www1.worldbank.org/education/edstats/index.html).

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 American countries locate just to the right of these countries, with an average enrollment rate of 73% and an average repetition rate of 6%.[2]

Figure 2 provides additional evidence on one of the factors correlated with repetition. This figure plots the average repetition rate in primary school over PPP per capita GDP in 1999. The figure shows clearly that not only do repetition rates in primary school tend to be strongly negatively related to enrollment in secondary school but also with GDP 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 low educational attainment of the population in developing countries? In particular, do the hurdles that this policy creates for the 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 possibly in many developing countries, the efficiency of the system (as measured by grade promotion) is also low, perhaps due to low public expenditure on education? Do poor teachers' quality and lack of school infrastructures - often cited as two main problems of the school systems in developing countries - explain both high failure rates and students' incentives to abandon the system? Or is it the case that where poverty is widespread students find it harder to progress through the system due to malnutrition, lack of financial resources (coupled with credit constrains), residential overcrowding, or the higher opportunity cost of attending school, hence leading to both repetition and drop out.

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[2] Results (not reported) are very similar if one uses repetition in secondary school as opposed to repetition in primary school.

This paper aims at estimating the causal effect of grade failure on students' subsequent school outcomes and rates using administrative micro data for Uruguay from 1996 to 2000.

The desirability of a grade retention policy in schools is a controversial issue and one to which there is no obvious and unequivocal response. There is large disagreement among education experts and the general public on whether on the whole grade repetition is beneficial to students and the society at large. 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 any improvement 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)).[3] 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 it when enforcement of compulsory schooling laws is lax, as in many developing countries. Whether due to disenfranchisement, stigmatization or pure rational calculation,[4] 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.

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 might strengthen a student's background making him more apt - and hence presumably more likely - to pursue higher levels of education. Experiencing the penalty of repeating a

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

[4] Grade repetition increases the time required to achieve a desired level of education and hence the horizon over which the returns to education accrue to the individual. In a simple human capital model, assuming that grade repetition is uncorrelated with future school and labor market outcomes other than through the extra year required to achieve a desired level of education, grade repetition should lower an individual's optimal level of education.

grade might also make a student less likely to want to experience this again hence creating an incentive for him to improve his school performance, possibly because of the increasing marginal cost of repeating an additional grade. Repetition might also potentially help improve the quality of the match between the school and the student. This might happen 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.

Possibly, however, the strongest argument in favor of grade repetition is that this might act 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, although this might come at a cost, since students take longer to transit through the school system, possibly in part compensated by the productivity gains of repetition and the savings due to lower attendance at higher grades when failure results in drop out.[5]

Although there is a rather copious body of research on the determinants of grade repetition, convincing evidence on its effect on students' outcomes is still scarce, especially for developing countries and this paper aims at filling this gap.[6] The main difficulty in identifying the effect of grade

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[5] The debate follows very closely an only apparently unrelated discussion among economists and social scientists at large, on the effect of public punishment of crime on individuals' well being and the well being of the society at large. The questions are very similar. Is there a rehabilitation effect of punishment (Ehrlich (1981))? Does punishment deter crime (Kessler and Levitt (1999), Lee and McCrary (2005))? What is the optimal level of punishment (Polinsky and Shavell (2000))? One major difference between these two phenomena is that while externalities from crime (to the detriment of the victims) are often cited as a rationale for policy intervention in the area of justice, such rationale is less clear cut when school underperformance becomes the target of the policy, unless one believes that there are negative externalities associated to underperformers remaining in a certain class.

[6] Evidence from more developed countries on the determinants of grade repetition illustrates a causal relationship between family socio-economic status (Oreopoulos et al. (2003) 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 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). 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)).

failure on a student's subsequent school outcomes is that a student's 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. 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 all likely to affect both the probability that a student will fail a grade and this student's 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 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 rates and subsequent educational attainment on the one hand and grade retention on the other hand. Because many of the variables simultaneously affecting grade failure and latent school outcomes are typically unobservable on the part of a researcher and because of the potential reverse causality between drop out and grade failure, simple (conditional or 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 would tend to overestimate the impact of grade failure on subsequent school outcomes.

In order to circumvent the classical identification problem highlighted above, in this paper I suggest using a rule in force in secondary Junior High school in Uruguay that establishes automatic grade failure for those pupils missing more than 25 days during the school year. This rule induces a sharp discontinuity in the relationship between the probability of passing a grade and the number of missed school days at 26 days of absence. 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 in the students' population and under some assumptions allows us to answer the question of what effect grade failure has on a student's performance net of any other confounding factors or reverse causality.

For the purpose of this exercise I use administrative data on a sample of around 100,000 students in public non-vocational Junior High school (grades 7-9) in Uruguay between 1996 and 1997. I am able to follow these students' progression in the public non-vocational secondary (both Junior and Senior, i.e. up to grade 12) school system until 2001.

The estimates below show 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 a shorter time in school than non failers and end up with less accumulated education.

Although, as said, the evidence on the causal effect of repetition on performance is scarce, others have attempted to tackle this issue. 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. Eide and Showalter (2001) use the US high School and Beyond Survey to assess the impact of grade retention (by grade 10) 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.

The empirical estimates in this paper suggest by converse a significantly negative effect of grade failure on later school outcomes, largely working through higher drop out. In the conclusions to the paper I attempt to reconcile my results with the previous evidence quoted above.

The structure of the paper is as follows. Section 1 provides background information on the school system in Uruguay. Section 2 presents the basic data. Section 3 presents evidence on the correlation between grade failure, absenteeism and school outcomes. 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.

I. 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 reside 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, in 1920 this figure was about 50% and by the end of the last century this 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 Uruguayan school system is organized into three basic cycles: Primary Education (*Education Primaria*, grades 1-6), Junior High (*Ciclo Básico*, grades 7-9) and Senior High (*Education Secundaria*, grades 10-12).[7] [8] School starting age is 6 and Primary and Junior high school (or the equivalent vocational course of studies) are compulsory. Minimum working age is 14 (hence lower

[7] Since 2002 an additional year of primary education for students aged 5 has been made compulsory.
[8] A parallel path to Ciclo Básico after Primaria leads directly to vocational training (*Educación Professional*).

than the minimum age required to complete compulsory education).[9] Junior and Senior secondary education is offered in both *Liceos*, similar to the French *Lycées*, i.e. non vocational secondary schools typically leading to university enrolment, and in vocational colleges, *UTU*s (*Universidad del Trabajo del Uruguay*, literally the Uruguayan Employment University) that offer both Secondary and Tertiary level education. The rest of tertiary education is offered in Universities and teacher training colleges (*Magisterios*). In 2000, enrollment in *Ciclo Básico* in *Liceos* was in the order of 115,154 students. Around 14,700 were attending *Ciclo Básico* in *UTUs*.[10] 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, as said, Uruguay still ranks high in terms of its 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.[11] Over the last decades growth in educational attainment of the population has been rather modest. 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), growth in Uruguay has been in the order of 2.1 years (from 5.1 to 7.2) (Barro and Lee (2001)).

Data from a specific education module administered in conjunction with the National Household Survey (*Encuesta Continua des Hogares*) of 2001 illustrate a long delay in the transition through the school system and widespread drop out before completion of the compulsory school cycle.[12] For example, between the ages of 15 and 17, when theoretically all youths should have

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[9] Recently, minimum working age has been increased to 15. 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 continuation (night) schools.

[10] Source www.utu.edu.uy. Enrolment in basic professional courses in UTU, (excluding *Ciclo Básico*) was in the order of 20,500 students.

[11] See also 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, the main ingredients of the reform being: school construction, curricula redesign and teacher training (ANEP (2000)).

[12] There is a consensus among experts of the Uruguayan educational system that the democratization of Secondary education and the ensuing increase in access to this level of education that took place over the 1980s-1990s did not translate

completed *Ciclo Básico,* the proportion with less than completed *Ciclo Básico* is about 42%. In the age group 24-29 around 20% of individuals declare never having started Junior high school. 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. Very notable is the difference in completion rates between repeaters and non-repeaters. Completion rates in *Ciclo Básico* amount to 87% for those with normal progression but only to 63% for those who repeat at least a grade. The data illustrate hence a very clear correlation between grade repetition and subsequent school outcomes at the individual level. As already hinted at in the introduction, though, it is premature to conclude from these figures that grade repetition is itself responsible for such poor outcomes. In the rest of the paper I will try to devise a strategy that attempts to isolate the causal effect of grade failure on subsequent school outcomes net of other confounding factors and the potential endogeneity of grade failure with respect to subsequent school outcomes.

II. DATA

In this section I present information on an administrative data set referring to a large sample of individuals in *Ciclo Básico*. Before presenting the data, it is useful to provide a few more details on the rules disciplining grade progression in *Ciclo Básico.* These details refer to the school years 1996 and 1997.[13] The discipline changed somewhat in 1998. I will return to this later in the paper when I exploit the differential intensity of the rule across years as an additional source of identification.

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into an equal increase in educational attainment due to the inability of the system to retain these students before completion of the school cycle (Furtado (2003), Da Silveira and Queirolo (1998b)).

[13] The discipline mentioned here 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*, literally Pilot High Schools), the majority of students in 1996 and 1997 were still under the old Plan. Regression results below refer to both Liceos *Pilotos* and those in *Plan 86*. Results (not reported) are virtually unchanged if I only restrict to *Liceos Plan 86*. This information comes from ANEP, *Circular* 2306, 1996.

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. In theory, grade advancement depends on the student fulfilling two conditions. First, it is required that at beginning of each school year the student owes no more than three subjects from any previous year, i.e. has no more than three accumulated fails that he has not in the meantime cleared. Those who fail a subject can re-take it during subsequent re-take sessions, provided they have no more than six accumulated debts, in which case they fail automatically. 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. Grade promotion depends on the number of debts after the February re-take session. So if a student ends the school year in December with four debts but he clears one of these debts in the following February re-take session he will still be allowed to progress to the next grade. Hence, a student with no more than three pending subjects is allowed to progress but will eventually need to pass exams in all the pending subjects before graduating. Students with more than three debts after the February re-take session will have to repeat that grade.

The second condition that must be simultaneously fulfilled is that the student has missed no more than 20 school days during the year.[14] 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.

Hence, in practice, repetition is warranted with no more than three pending subjects and at most 20 missed school days. Students with between 20 and 25 missed days of school and no more than three pending subjects can pass at the school discretion. Over 25 days of absence or with more than three pending subjects grade failure is unavoidable.

[14] 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.

For the purpose of this analysis I have assembled administrative micro data on students in (almost) all public non vocational high schools in the country. The data refer to the school years 1996, 1997, 1998 and 2000 (no data is available for the year 1999). The data report information on the institution and grade attended in a school year and whether the student passed or failed that grade. Data for each year have a longitudinal component since they also report information on whether a student registered the following year. Data on passing or failure refer to the beginning of the following school year after the February re-take session. If the student remains in the system in the following year, one also knows in what grade he registers next (but not in what institution). This means that for those observed in the year 1998 one is also able to see whether they registered in 1999 and if they did in which grade. The same applies to the year 2001 for which one can derive information on attendance based on year 2000 data. Because a unique identifier refers to each pupil, one can link observations across years. In this way – and despite administrative records for the years 1999 and 2001 not being available- one can follow individuals as they progress through the public non-vocational secondary school system from 1996 to 2001. Rather important for the purposes of this analysis, the data also provide information on the number of recorded missed school days during the school year (1996, 1997, 1998 and 2000) for each student. Finally, data for 1996 and 1997 (but not for later years) report also information on a student's sex and age.

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* sometime 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.[15] Because of this, in the following I measure school progression in terms of number of years in the sample and maximum grade attended (rather than maximum grade passed).

There are some important limitations to the data. First, 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 (whether Junior or Senior high) but I am unable to distinguish those leaving the educational system *tout court* from those moving to private institutions or to public vocational intuitions. Second, 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). This might be a problem if grade failers are more likely to move school than non failers. Third, students' progression in the school system can only be measured with error since data for 1999 and 2001 only refer to the grade attended by those who were registered in the previous year (1998 and 2000 respectively) and hence exclude those who enter for the first time or re-enter the system in 1999 or 2001. If repeaters are more likely to attend intermittently, again my estimates might overstate the gap in educational achievement between grade failers and grade passers.

None of these problems though is likely to seriously affect my results. First, students are largely assigned *ex-oficio* to a public school depending on their residence and school catchment areas (Da Silveira and Queirolo (1998a)), and changing school within the public non-vocational education system is far from easy. Second, evidence from a follow-up phone survey of 660 individuals who dropped out of first year of *Ciclo Básico* in a *Liceo* in 1997 and reported in ANEP (2000) shows that only around 1.6% had moved to a private institution and 15% had moved to a *UTU* by 1998.[16] What these results suggest is that misclassification of school movers as drop outs is unlikely to be a serious problem in my

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[15] In principle this information could be recovered in 1999 conditional on being enrolled in 2000 but this obviously refers to a selected sample.

[16] 60% of these individuals had not re-registered. Another 4.5% had moved to another public *Liceo*. The residual 19% was registered in informal education (most likely professional education).

data, although this is obviously no proof of this misclassification not affecting selectively more grade failers than grade passers.

A final weakness of the data at hand is they do not provide information on students' school outcomes (e.g. final marks and/or number of pending subjects) other than whether they fail or pass (in 1996, 1997, 1998 and 2000), whether they stay on and, conditional on this, on the grade attended each year. This makes it hard to investigate what channels lead to possibly different outcomes for grade failers compared to non failers. Progression following grade failure might depend on both a student's and a school's changes in opportunities, constraints or behavior. Grade repetition for example might affect a pupil's outcomes through repeated exposure to teaching, changes in effort or improved match with teachers or peers. Schools on their side might stigmatize or favor early repeaters even conditional on their year performance. In the following I will try where possible to shed some light on the role of these different mechanisms.

Table 1 provides some descriptive statistics based on the administrative records. The left part of the table refers to the entire sample, the middle part to those who fail a grade and the right part to those who pass. Each observation refers to an individual-grade observation. Overall, there are 148,038 observations in the sample and 100,862 individuals, implying that I have on average just below 1.5 observations per individual. 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 school system in Uruguay. This in turn is largely due to school drop out rather than - as shown below- to differential retention rates across grades. Average age increases from around 12.5 in seventh grade to 14.8 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 who never reach ninth grade, so this number provides a lower bound estimate of 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. There is some variation in number of missed school days or repetition rates across grades, the first varying between 18 and 20, and the second varying between 27% and 31%. Since the school year is approximately 180 days, this suggests an average absenteeism rate in the order of 10-12%. Both repetition and absenteeism are the highest in ninth grade and the lowest in eighth grade.

As an outcome variable at this stage I examine a student's 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.* Additional grades attended potentially range from zero (for those who drop out immediately or repeaters who fail again) to 5 (if individuals are observed in 1996 in seventh grade and progress smoothly to twelfth grade in 2001). The variable is right censored since some individuals will still be in school in 2001 and because this variable can at most take value 4 for those observed in 1997. 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. Additional grades attended increase from 2.3 for those observed in seventh grade to 1.6 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, because this variable does not fall one to one with the grade attended in *Ciclo Básico*, as it would be the case if drop out happened at the same rate after each grade, this also suggests a larger drop out rate at earlier grades. This is most likely the reflection of those attending ninth grade being a selected sample of individuals (those with higher probability of staying on).

Rather than looking at the number of additional grades the student has gone through one can examine the number of additional years the student has spent in school. Because progression is not automatic even at later grades, one would expect duration to be longer than the number of additional grades. Duration is a variable that ranges from zero (for those who drop out immediately) to 5 (if individuals first observed in 1996 are observed continuously up to 2001). This variable is again right censored since some individuals will still be in school at the end of the period. Again, the extent of censoring will depend on the year of first observation since those observed in 1997 can at most stay in the sample for an additional 4 years. Average duration in the sample is about 3, suggesting that some individuals drop out before 2001, so that censoring is far from universal (in which case duration would be around 4.5), consistent with the observation that the proportion of censored individuals is strictly less than one. Rather important, duration is longer than additional grades attended. This is an indication of slow progression later in school.

An analysis of the differences between grade failers and non failers illustrates that along the observable dimensions failers perform worse than non-failers. Compared to non-failers, failers are in between 0.5 and 0.9 year older and they tend on average to miss 4 to 5 times more school days. Rather interestingly, 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 current 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, grade failure only delayed progression in the school system and there were no censoring (i.e. one could follow individuals until the end of their school career), one would expect failers and non-failers to attain the same number of additional grades. In the case of complete censoring (i.e. if everybody were still at school in the last period), one would expect the difference in grade attended to be equal to minus one. In fact the data show that failers attend on average between 2.5 and 1.5 less years in school than non failers suggesting unequivocally worse outcomes for failers than non failers. This difference is likely to underestimate the true gap between failers and non failers in the number of additional grades attended by the end of their school career since the proportion of censored observations is much lower for failers than non failers (between 56 and 8 percentage points depending on grade).

Another way of looking at the data is to allow failers to be followed for one more year compared to non failers. In this case, in the absence of differential drop out rates and if grade progression is similar across groups, one would expect maximum grade attended to be equal for failers and non failers. If failers tend to drop out earlier than non failers, as shown this being the case, one would expect this difference to be negative. If drop out happens largely in the early years, i.e. away from the censoring point, then it should make little difference whether one follows failers for one extra year or not. To check for this I have computed a series for grade failers that allows them to be observed one year longer than grade passers. In practice I artificially censor the sample by following non failers up to 2000 (and failers up to 2001). Data reported in the table show clearly that the censored series is below the uncensored one for non failers (for failers this is obviously the same). Differences between grade failers and passers now range between 2 and 1.5 grades. An analysis of duration and number of additional grades attended shows rather clearly that non failers tend to stay longer and to accumulate more years of education. Failers tend in general to accumulate only half a grade more while non failers accumulate between 2 and 3 more additional years depending on which grade they were originally observed in. Duration is obviously higher for non failers (between 2.5 and 3.5) suggesting some accumulated delay of around half a year due probably to further grade repetition. For failers the difference between duration and number of additional grades is around 1 year, so higher than for non-failers. This possibly suggests that failers are more likely to repeat an additional grade. I will try to disentangle these two forces later on when I present my regression results.

III. GRADE FAILURE AND SCHOOL OUTCOMES: BASIC EVIDENCE

Although Table 1 is suggestive of a strong correlation between grade failure and subsequent school outcomes in the administrative sample of pupils in public non-vocational *Ciclo Básico*, as already said, this is by no means an indication of a causal relationship between the former and the latter variable. I have shown that along a number of observable dimensions failers are different from non failers and, as already discussed, reverse causality might also be a contributing factor explaining the negative correlation apparent in the data.

In order to ascertain this casual effect in the rest of the paper I exploit the discontinuity in grade failure induced by the rules disciplining grade promotion that I have discussed in the previous section. Figure 3 plots the probability of failing a grade on the number of missed school days for those with a number of missed school days between 1 and 35. I exclude individuals with zero missed school days (around 20% of the sample) and individuals with more than 35 missed school days of absences (about 11% of the sample) since for reasons that will become clearer later I will not use them in my regressions. The picture shows some interesting features. First, repetition is a positive function of number of absences. 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. This might be an indication of number of missed school days proxying for a student's unobserved motivation or opportunity of attending school, or might mask the fact that those performing worse might have a lower incentive to attend school because they expect to fail the grade anyway. Rather interestingly, there seems to be some acceleration in the failure profile after 20 days of missed school. This is consistent with the rule that establishes some discretion on the part of the schools for those with more than 20 (but at most 25) missed days of school. What is potentially more interesting is that after the 25 missed school days the probability of repetition jumps to around 95% and it stays flat afterwards. This is consistent with the rule establishing automatic grade failure after 25 days of absence.

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 26 missed school days too. Figure 4 analyzes the correlation between additional grades attended (up to 2001) and missed school days for those who cross *Ciclo Básico* sometimes between 1996 and 1997. As said, there is no way to measure a student's maximum grade achieved so one has to be somewhat cautions in interpreting the results from this variable. Additional grades attended (the solid line) falls monotonically with number of missed school days in *Ciclo Básico.* While those with 1 missed school day accumulate on average almost 3.3 additional years of schooling, this quantity is about 0.4 for those with 35 days of absence. If missed school days proxy for a pupil's ability, motivation or opportunity to attend school, this 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 the greatest jump in the outcome variable, with a fall of just less than half a year from around 1.1 at 25 days of absence to 0.7 years for those with 26 days. Because this is the mirror image of the effect of missed school days on grade failure in Figure 3 and because it is hard to imagine that maximum education achieved should display a discontinuity at 26 missed school days in *Ciclo Básico* for reasons other than the effect of the rule on grade failure, this can be taken as evidence of grade failure having a causal negative effect on school outcomes.

As said, maximum grade achieved is censored and censoring is potentially a more serious problem for grade failers. Rather than plotting the gap between maximum grade and current grade attended, I have computed the same series by censoring it artificially to the year 2000 for non failers. This series is plotted in Figure 4 as a dotted line. The censored distribution is systematically below the uncensored one to the left of 25 missed school days and the difference tends to fall as the number of missed school days increases, consistent with the observation that the failure rate increases as the number of missed school days rises. However, at around 25 days of missed school days the difference between the two series is virtually zero, so that the jump in the number of additional grades attended between 25 and 26 missed school days remains in the order of 0.4. Effectively this suggests that censoring is unlikely to be a problem around the discontinuity point, i.e. that most of the individuals around that point will have dropped out by 2001. This is confirmed in Figure 5 where I plot average duration in the sample by number of missed school days in *Ciclo Básico.* Again duration falls with number of missed school days in *Ciclo Básico.* Average duration is about 3.6 for those who missed one school day in *Ciclo Básico* and it falls to around 1.3 for those with 35 days of missed school. Again there seems to be a sharp discontinuity in average duration between 25 and 26 missed school days. This is again the greatest jump in duration in the sample, with a fall of just less than half a year from around 2 at 25 days of absence to 1.6 for those with 26 days.

In sum, Figures 3-5 show a clear discontinuity in grade repetition in *Ciclo Básico* at 26 missed school days. This is consistent with the rule disciplining grade promotion. I also find evidence that school duration and additional grades attended present the same (but opposite in sign) discontinuity as the one observed on failure rates. Because it is hard to imagine that students' progression in the school system depends systematically on whether they missed 25 or 26 school days in *Ciclo Básico* if not for the effect of the promotion rule in *Ciclo Básico,* one can take this evidence as suggestive that failure affects negatively subsequent school outcomes. I will go back to alternative explanations for the discontinuity in the outcome variables later on in the paper.

IV. SPECIFICATION AND IDENTIFICATION

In this section I devise a regression strategy aimed at assessing the impact of grade failure on school outcomes that is based on the discontinuity in failure rates that I have illustrated above. By using a regression model, this allows us to control for the observed characteristics of students and their schools and for the potentials biases in the regression coefficients that stem 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 assumption underlying the consistency of the proposed estimator.

Suppose that school outcomes depend additively on a continuous function of number of missed school days in *Ciclo Básico* and on grade failure. I ignore for the time being other covariates. If *Y* denotes the outcome variable, D the number of missed school days and F grade failure, this is equivalent to postulating that:

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

where $f(.)$ is a continuous function over the support of D and u an error term. This error term potentially includes a student's past attainment as well as other unobserved determinants of performance. As already pointed out, the OLS estimates of equation (1) are likely to be biased. This happens if, conditional on $f(D)$, the error term is correlated with F. As already pointed out in the introduction this might be due to unobserved co-determinants of grade failure and later school outcomes or possibly to reverse causality.

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

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

where $P=I(D>=26)$ is a dummy for 26 or more missed school days in *Ciclo Básico.* By substituting (2) into (1) one can derive the reduced form equation:

(3) $$Y = \theta_0 + \theta_1 P + h(D) + e$$

If P is uncorrelated with u in (1), the IV estimate of β_1 will be consistent. The identification assumption underlying the consistency of the IV estimate is that school progression varies continuously around 26 missed school days if not for the rule governing grade failure.[17]

Before presenting the estimates of models (1) to (3) it is useful to highlight the likely implications that the missed-days rule has on pupils' and schools' behavior to see how the differences between those to the left and those at 26 days can generate some variation in repetition rates that is

[17] A similar empirical strategy (fuzzy regression discontinuity) is used – among others – by Angrist and Lavy (1999) in their analysis of the effect of class size on pupils' attainment.

arguably exogenous to the outcome equation (1). What is crucial to this discussion is that missed school days is to a large extent a choice variable on the part of students, which might invalidate its variation around the 26 days threshold as a valid instrument.

To fix ideas, assume that pupils maximize their school outcomes at minimum cost. If each school day has some cost but it also enters as an input into the student's education production function, and pupils have perfect control on their consumption of school days, then one will find that the variation in number of school days across pupils will only reflect differences in returns to or costs of one extra day of school. This is arguably a variation that one does not want to use to infer the effect of grade failure on school outcomes, since most likely differential returns to and costs of an additional day of schooling across students will also be correlated with their future school outcomes. I try to account for this variation in the regressions controlling for a smooth function of missed school days. If a rule like the one that I have described above is introduced, and the cost of attending one extra day of school is high or the returns from it low, one would see some concentration of students at 25 missed days. A number of pupils might still decide to miss more than 25 days, in which case they would have an incentive to miss the entire school year, i.e. not to attend at all. If the decision to withdraw is taken sometimes over the year, one will find that each student who drops out will accumulate a number of missed days equal to 25 plus whatever the number of days still needed to complete the school year at the time this decision is taken. Again, those to the left and to the right of the discontinuity point will be intrinsically different and this would hardly make the discontinuity a good source of identification.

In fact, missed school days are also likely to be partly outside pupils' control. Sickness, bad weather, transport difficulties or family responsibilities might prevent a student from attending school on a certain day. Similar, the unexpectedly high cost of attending school on a specific day due for example to the unplanned circumstance that a teacher might want to monitor a student's performance on that day might deter a student from attending despite his original plans. What this suggests is that shocks to attendance rates (or to the variables that influence it) are likely to be partly unexpected. A student with less than 25 days of accumulated absence who intends or expects to pass the year will remain in school and decide whether to miss a specific day or not taking into account how many missed school days he has accumulated so far and his expectations of missing extra school days in the future due to either bad luck or the expected net gain from it. This implies that most pupils at 26 missed school days are likely to have suffered from unexpected shocks despite their expectation or desire to pass the year. Had a pupil decided to withdraw sometimes during the year one would expect to find him rather away from the discontinuity point. Similarly, had he expected to fail the grade due to poor performance, one would also likely not find him in the proximity of 26 missed school days, since the chances of passing the grade by failing less than 26 missed school days are low for this student and the rule hence has no bite on him.

If this is the case, differences between those with 25 and 26 missed school days will be to a large extent random, i.e. they will be due to differences between two otherwise similar pupils (presumably with low school attachment) one of whom was lucky enough not to experience a bad shock and the other was unlucky and just overcame the 25 days threshold. This is arguably the variation in school days around the discontinuity point that one wants to exploit to identify the causal effect of grade failure. In this case one can legitimately interpret the evidence in Figure 3 as the casual effect of the rule on failure rate. One can also legitimately use the variation around the discontinuity point as an instrument for grade failure in (1). This strategy would also presumably control for the fact that some students end up failing a grade because of their earlier decision to drop out during the year. For the reasons outlined above these students will presumably be substantially above the 26 days threshold. Because I am using those in the neighborhood of 26 missed school days, one would not expect these students to be in large numbers in that neighborhood. In this case the IV estimates will have a LATE interpretation, in the sense that they will identify the effect of failure rates on outcomes among those with high absenteeism rate.

Figure 6 reports the distribution of missed days of school for the observations with between 1 and 35 missed school days. Based on the above discussion, one would expect some bunching at both 20 and 25 missed school days. Data refer to the proportion in the whole sample, i.e. with any number of missed school days. The Figure shows an obvious bunching of observations at 20 days (4.3%), that corresponds to the maximum number of missed school days at which those with no more than three debts progress automatically to the next grade. The probability of missed school days falls to less than 1% for any school day above 20. There is an additional bunching then at 25 days of absence (0.8%), after which the probability of missing one extra school day remains very low (less than 0.2%). Recall that 25 days of absence is the threshold above which repetition is automatic irrespective of school outcomes.

At least three considerations emerge from an analysis of Figure 6. First, the circumstance that there is some bunching at the critical points is consistent with the effective threat effect of the rule.[18] Second, bunching seems to be particularly pronounced at 20 missed school days, where, recall, there is no discontinuity in failure rates (although there is some acceleration). This suggests that students regard crossing the 20 missed school days as significantly jeopardizing their chances of promotion. This feature of the data is potentially useful as a falsification exercise for my identification assumption, as I will show below. Third, there is a non negligible proportion of individuals (13.5%) who have more than 25 missed school days. Figure A1 reports the distribution of missed school days for those above 25. I report this in a separate graph because the scale of the graph in Figure 6 does not allow us to appreciate changes in the distribution of missed school days for those above the threshold. It is remarkable that the probability of each missed school day above 25 is higher the closer the pupil is to 26 missed school days. Although this is no incontrovertible proof, this bunching to the right of 25 is

[18] Note though that this result does not allow us to say whether in the absence of the rule pupils would have missed on average more or less days. While on the one hand the rule might deter potential shirkers from missing more than the prescribed number of days, this might also create an incentive for otherwise regular attendees to locate exactly at the threshold, hence missing more days that they would have otherwise done.

consistent with some students attempting to stick to the 25 days limit and somewhat being unable to do so.

Obviously, although there might be some random variation in the assignment of students to the left and to the right of the discontinuity point that one wants to exploit for identification, there is no guarantee that students on either side are identical in terms of latent outcomes. The apparent discontinuity in the distribution of the variable 'missed school days' is potentially (but not necessarily) indicative of the failure of the random assignment (i.e. continuity in unobservables) hypothesis around the discontinuity point that is required for a consistent estimate of the parameter of interest in an RD context (McCrary (2005); for an application of this test see also DiNardo and Lee (2004)). Students' ability or willingness to sort around the discontinuity point might vary in a fashion that is correlated with latent outcomes. For example, if pupils with better latent school outcomes are more able to sort strategically just at the 25 missed school days 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. Similarly, records of missed school days could be manipulated by schools or teachers. Anecdotic evidence suggests that teachers and schools are not alien to this type of strategic behavior. This might happen if teachers have a preference for some students or if teachers or schools have a return to passing or failing a pupil (see also Jacob and Levitt (2003) for evidence on teachers' cheating as a response to incentives). This in turn might be due to a teacher being corrupt or if high failure rates act as a signal to pupils, parents, colleagues or the head-teacher of this teacher's quality, which the teacher 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 impact on different students differentially. Ex-ante it is not obvious which direction the bias in the IV estimates would go, since teachers' behavior might lead to positive or negative sorting of students around the discontinuity point. This last case for example would happen if teachers passed a student with low chances of later progression due to compassion, the recognition that grade repetition might be of little help to this student 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.

Effectively a better look at Figure 3 shows that the failure rate appears to increase monotonically up to 24 days of absence but falls slightly (by about 3 percentage points) at 25 before jumping to about one at 26. There is some bunching of students with low probability of failure at 25 days of absence. This suggests in turn that students with 25 missed school days have possibly better school outcomes than those with 24 days. One reason why this might be the case is that teachers record 25 missed school days for those with more than 25 but whom they want to pass anyway due to their otherwise good school performance (most likely records adulterations has a cost so teachers will not tend to underestimate missed school days more than necessary) or perhaps to the fact that better performing students are more able to locate exactly at 25 missed school days. I will try to deal with these issues at my best in the regressions below.

V. REGRESSION RESULTS

V.a Missed School Days and Grade Failure

In this section I present estimates for the first stage equation (2). The objective is to estimate the gap in grade failure at 26 missed school days due to the operation of the rule. To do so I need to estimate the actual and counterfactual failure rate at 26 missed school days. The counterfactual failure rate is by definition not observable since those at 26 missed school days are subject to the rule. Following Card and Lee's (2004) suggestion in the context of regression discontinuity when the support for the treatment variable is discrete, I estimate this counterfactual failure rate at 26 days by extrapolating the estimated failure rate below 26 missed school days. I measure the effect of the rule on failure rate as the estimated difference between the actual failure rate and this counterfactual failure rate. In practice, in the regressions I model *f*(*D*), the continuous function in the number of missed school days, as a parametric spline and I allow both the shape and the intercept of this spline to vary between observations with less than 26 missed school days and those with 26 or more missed school days.

In this and the following regressions I restrict to those with 1 to 35 missed school days. I also pool observations for 1996 and 1997 and treat individuals who appear in the sample more than once as two separate observations. The estimates mix first time attendees with (potentially multiple-times) repeaters. Ideally, one would want to be able 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 conditional on staying on. 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. I will go back to the short term dynamics of grade failure later on.

In order to account for the fact that the counterfactual at 26 missed school days is estimated based on an extrapolation and hence potentially affected by specification error, I also follow Card and Lee's (2004) suggestion and I cluster standards errors by number of missed school days.[19]

Columns (1) ands (2) of Table 2 present the results of the first stage regression. Column (1) includes no controls if not the splines in missed school days. Here I constraint this spline to be a second order polynomial in the number of missed school days. The jump in the failure rate at the discontinuity point is estimated to be 38 percentage points. Column (2) includes additional controls. If sorting around

[19] 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 only on individuals who are observed for the first time in the sample (76,904 observations) again clustering the standard errors by number of missed school days. Point estimates are very similar and standard errors are generally lower than those reported in Tables 2 and 3, suggesting that the estimated standard errors reported in the paper are unlikely to be severely biased.

the discontinuity point is a serious concern, one would expect the OLS estimate of γ_1 to be affected by the inclusion of variables that affect directly a student's school outcomes. This is a test for random assignment around the discontinuity point (Lee (2004)). Unfortunately the data set is not very rich in terms of covariates. I include the following controls: dummies for year of observation, grade where failure occurred, age, sex and initial age-grade distortion (age minus grade). By including grade and year dummies I only exploit the variation in missed school days around the discontinuity point for individuals in the same year and grade and potentially control for the fact that grade failure and the intensity of the rule are correlated across years and grades. By controlling for initial age-grade distortion I attempt to condition for a pupil's past school history that might itself affect failure rates. In doing so I implicitly assume that those with the same accumulated delay in school progression so far (independent on when they failed a grade or even if they temporarily dropped out) should perform similarly if not for their level of absenteeism in the current year. Finally, I include school dummies. These wash out heterogeneity across schools in the application of the rule as well as differences in students' socio–economic characteristics and teachers' and schools' characteristics that are common to those in the same school. These are appropriate controls to include in the regressions if assignment to schools is largely outside a student's control, as it appears to be the case in the Uruguayan public system. When all controls are included the estimated effect remains virtually unchanged (37 percentage points) suggesting that the observed covariates are unlikely to be correlated with the probability of being on either side of the threshold and hence lending some support to the notion that sorting is unlikely to severely affect my results. As a way to confirm visually the circumstance that the discontinuity in missed school days is uncorrelated with the observed covariates in Figure A2 I plot average age-grade distortion on number of missed school days. Although those with more accumulated delay also tend to display on average higher distortion, there is no appreciable discontinuous change in this variable between 25 and 26 missed school days.

As already pointed out, a better look at the data in Figure 3 shows some acceleration in the probability of grade failure after 20 missed school days and some subsequent deceleration before jumping to around 1 after 25 missed school days. Regressions that pool individuals with 1 to 25 missed school days are unlikely to fit the data in the 21-25 range very well because most of the mass of the distribution is to the left of 21 missed school days, which will tend to drive the fit of the model. This can be clearly seen in Figure A3, top panel, in the appendix where I have superimposed to the actual series the predicted series from column (1), Table 2. One can see that the fit of the model is far from perfect in the range 21-25 missed school days. Because of the concavity in failure rates after 20 missed schools, the predicted series tends to underestimate the gap in failure rates between 25 and 26 missed school days. The potential advantage of taking all individuals in the range 1 to 25 though is that one bases the extrapolation on observations not in the immediate neighborhood of the discontinuity point. In this sense, the estimates are less likely to suffer from potential sorting and in particular from the possible bunching of otherwise better performing students exactly at 25 missed school days. As an alternative specification, in the bottom part of Table 2 I present regression results where I allow the spline in number of missed days to further change its shape and intercept between 20 and 21 missed school days. I do so by adding a dummy for missed school days below 21 and interacting it further with a second order polynomial in number of missed school days. In practice the identification of the discontinuity at 26 missed school days is only based on observations between 21 and 35 missed school days only (although when controls are included I constraint their effect to be monotonic over the whole range of missed school days 1 to 35). As expected from a simple inspection of Figure 3, point estimates increase substantially. When no controls are included, the estimated gap at 26 missed school days is 0.47. The inclusion of controls in this specification makes again little difference to the results. Point estimates in column (2) show an estimated gap of 0.45. Figure A3, bottom panel, in the appendix shows that this model fits the data remarkably well and illustrates that the estimated gap is higher when a separate spline is used for the observations in the range 21-25 because of the concavity in the failure rates over that range.

In the Appendix I present results that are based on different parametric assumption about the shape of *f*( ). In particular I use parametric splines of first and third order. Estimates in Table A1 show that the results are different when a linear spline over the range 1-25 missed school days is used. In particular the regression tends to lead to a larger estimated effect of the rule on failure rates (0.50). When a spline only in the range 21-25 is used the estimate is in line with the one in Table 2 (0.37). Regressions based on a cubic spline lead to opposite results. When a linear spline is used the point estimates is lower than in Table 2 (0.31) while the reverse happens when a separate spline is used in the range 21-25 (0.43).

In sum, point estimates of the gap in failure rates at 26 missed school days appear to be somewhat sensitive to whether one uses observations between 1 and 25 missed school days or only observations between 21 and 25 missed school days to estimate the counterfactual failure rate at 26 missed school days in the absence of the rule. The estimated impact of the rule on failure rate ranges from 0.31 to 0.50. In any event, results are always positive and strongly significant.

V.b Grade Failure and Final School Outcomes

I now move on to estimating the effect of grade failure on the number of additional grades attended using the IV strategy proposed above. As in Figure 4, this variable is artificially adjusted to account for differential censoring between failers and non failers. Column (3) Table 2 presents point estimates of the reduced form equation where the dependent variable is regressed on the discontinuity at 26 missed school days without any additional controls if not for a second order spline in missed school days interacted with the dummy for missed school days greater than 25. The reduced form in column (3) shows that the estimated gap in additional grades attended at 26 missed school days in *Ciclo Básico* is -0.16. This suggests that on average those with 26 or more missed school days in *Ciclo Básico* end up with about a sixth of a year less in educational attainment than those with less than 26 missed school days. Because the failure rate is estimated to increase at the discontinuity point by about 38 percentage points (column (1)), the implied effect of failing a grade in *Ciclo Básico* on the number of additional grades attended is about -0.42 (=-0.16/0.38), i.e. slightly less than half a grade less in educational attainment. This IV coefficient is shown in column (5).

Columns (4) and (6) present reduced form and IV estimates with the inclusion of controls. As before, I include dummies for year of observation, grade where failure occurred, age, sex, initial age-grade distortion (age minus grade) and school dummies. The inclusion of some of these controls might account for potential biases in the IV estimates and provide an indication that the random assignment hypothesis is violated in my data. First, as before, I include grade and year dummies potentially controlling for the fact that grade failure and subsequent school outcomes might be correlated across years and grades for reasons other than the rule. The inclusion of these dummies also controls for the potential bias in the IV estimates that is generated by the fact that observations referring to individuals in different grades and years might be differently affected by censoring. As already pointed out, given that the last year of observation is 2001, observations in the sample in the year 1997 can only at most accumulate 4 more grades as opposed to those in 1996 who can accumulate up to 5 more school grades. Similarly, given that the highest grade in secondary school is twelfth grade, those in seventh grade in 1996, say, can accumulate up to 5 more grades while those in ninth grade can accumulate up to 3 more grades. To the extent that the intensity of the rule varies across grades and years, then one might end up with a biased estimate of the effect of grade failure on the outcome variable. For example, if the rule tends to have a stronger effect in 1996 than in 1997, one might end up overestimating the effect of grade failure on future outcomes, because those in the sample in 1996 are mechanically more likely to attend additional grades in secondary school.

Again, in order to account for previous grade repetition, I include controls for initial age-grade distortion. Finally, by including school dummies I control for the fact that potential outcomes and the probability of grade failure due to the promotion rule might be correlated across schools. This would be for example the case if poor teachers' quality increases the chances of failing a grade as well as the subsequent progression in school. When all controls are included, the reduced form estimate falls by about 8 percentage points (from -0.16 to -0.24) and the IV estimate falls to about -0.64. If anything, the inclusion of controls makes the estimated effect larger in absolute value suggesting some negative selection of students around the discontinuity point (those with worse potential outcomes are more likely to be to the left of 25 missed school days) hence suggesting that the regressions with no controls tend to underestimate the effect of grade failure on later educational attainment.[20]

If one uses just the observations in the range 21-25 to extrapolate the gap in educational attainment at 26 missed school days, the estimated coefficient for the reduced form equation more than doubles. As column (1) in the bottom part of Table 2 shows, regressions with no controls show that the estimated gap is around -0.38 implying that on average those with 26 missed school days accumulate less than a year in education compared to those with 25 missed school days. The implied IV estimate is -0.83, suggesting that grade failers tend on average to lose just below a year relative to non-failers. When controls are included the IV estimates shows again a negative and significant effect of grade failure on extra education acquired. The point estimate is around -0.91, around 8 percentage points less than the corresponding IV estimate with no controls. In any case, point estimates are not statistically different from each other. Figure A4 plots that actual and estimated reduced form equations based respectively on a single quadratic spline between 1 and 25 missed school days and a spline between 21 and 25 missed school days. Again one can notice that the estimated gap is lower when a single spline over the range 1-25 is used for identification compared to the case when a spline only over the range 21-25 is used. The reason for this is that the first regression ignores the apparently abnormally high (i.e. off trend) performance of those with exactly 25 missed school days. The second regression instead fits

[20] By comparison, OLS estimates of grade failure on the number of additional grades attended lead to a point estimate of -1.327 (s.e. 0.049) with no controls and -1.173 (s.e. 0.032) with controls, suggesting a negative correlation between failure and the error term in equation (1).

the data better but in doing so it also fully fits the series at 25 missed school days. The convexity in the series between 21 and 25 missed school days tends to lead to higher estimates of the gap.

In the Appendix I present reduced form and IV results that are based on different parametric assumption about the shape of *f*( ). Estimates in Table A1 show that not only do different specifications affect the estimated first stage, but this is also true for the reduced form equation and hence the IV estimates. However, differences in specifications appear to lead to similar proportional differences in the estimated coefficients of the reduced form and first stage equations, so that the implied IV estimates do not differ greatly from those in Table 2. Estimates with a linear spline and the full set of controls lead to an estimated IV effect of between -0.67 and -0.87. Estimates with a cubic spline lead to an estimated IV coefficient of between -0.75 to -0.81.

Table 2 pools all individuals from different grades together. One might wonder if the effect of grade failure on later educational attainment varies depending on the grade where failure occurs. Table 3 hence 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 since I have already shown that these tend to make little difference to my results. The left hand side of the table uses a common spline over the support 1-25 missed school days while the right hand side uses two separate splines for observations in the range 1-20 and 21-25. The effect of the rule appears to be stronger in eighth grade (where repetition is lower) than in seventh or ninth grade. Results are again sensitive to whether one uses all the observations in the range 1-25 to extrapolate the value at point 26 or only observations in the range 21-25. When one moves to the instrumental variable estimates it appears that the effect of grade failure is negative at each grade, although this is smaller for those who fail in eighth grade. It appears that failing at the beginning or at the end of *Ciclo Básico* tends to have a relatively stronger effect. IV estimates imply between 0.71 and 1 less additional grades attended for failure in seventh grade, depending on which specification one uses for the spline. Failure in ninth grade appears to lead to a loss of between 0.65 and 1.1 years again depending on the specification used. For those who fail in eighth grade the associated penalty is between 0.32 and 0.65 but coefficients cannot be told apart from zero at conventional levels when a single spline for observations in the range 1-25 is used.

In sum, the data lead to an estimated effect of failure rate in *Ciclo Básico* on the number of additional grades attended that range from -0.42 to -0.91, i.e. in between approximately half and one less grade attended. The inclusion of controls tends in general to lead to larger estimates (in absolute value). So does the inference based on observations only in the range 21-35 missed school days compared to those in the range 1-35. If one truly believes that using the whole support in the range 1-25 is an adequate way of controlling for endogenous sorting, what the result suggests is that possibly endogenous sorting around the discontinuity point leads to overestimating the negative impact of grade failure on later school outcomes, consistent with positive sorting (otherwise better performing students being more likely to locate below 26 missed school days). Results based on the addition of observables though seem to imply the opposite, since their inclusion tends to lead to larger estimates in absolute value. The most plausible explanation is that on average there is some negative sorting around the discontinuity, with otherwise worse (better) performing students locating to the left (right) of 26 missed school days. At the same time, however, there is a disproportionate clustering of individuals with better latent outcomes exactly at 25 missed school days. It is reassuring though that whatever the point estimates is, the estimated effect is consistently negative and statistically significant at conventional levels.

V.c Short Term Dynamics

Having ascertained that grade failure has a negative impact on the 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 occurs? Is it 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 later failure to explain these results, is this due to repeaters tending to exercise less effort, possibly because of the disenfranchisement or stigma associated to their condition 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 due to neither these students failing nor dropping out more but to the circumstance that grade failers are more likely to temporarily exit the system and then renter, so that the estimated gap in educational attainment masks a higher probability of intermittent attendance (and hence more censoring) among non failers?

In Table 4 I start by analyzing the dynamics of grade failure year by year. Similarly to columns (2), (4) and (6) of Table 2, all the regression in the table include the whole set of controls (age, sex, initial age-grade distortion and school dummies). I again model the effect of missed school days as a second order spline that is allowed to change its shape and intercept to the left and the right of 26 missed school days. Results with a different spline for those just between 21 and 25 missed school days are reported in Table A2 in the appendix.

Columns (1) to (5) show the survival probability at time t+1 (t=0, 4). The coefficient in the table is the effect of grade failure in *Ciclo Básico* (t=0) instrumented by the discontinuity at 26 missed school days. One can see that grade failure is followed by high drop out rates. Grade failers are on average 25 percentage points 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, although even after 4 to 5 years failers have not completely caught up with non failers. Although the results at time 5 appear generally not in line with the pre-trends, some caution must be used in making inference based on these data because these only refer to around half of the sample (those observed in 1996).

Column (6) reports the overall duration in the sample. This as said is a variable that ranges from 0 to 4 (for those observed in 1997) or 5 (for those observed in 1996). If there was no intermittent attendance, this variable would be the sum of the coefficients in columns (1)-(5). On average failers spend about 0.8 less years in the sample than non failers.

As an additional outcome variable I analyze the effect of grade failure on intermittent attendance. I am particularly interested in checking if those who drop out following grade failure re-enter the system at any later time, i.e. if drop out is just a temporary phenomenon. This variable mixes true intermittent attendance with the fact that some schools not observed at the beginning of the period (1996-1997) are included in the sample in 1998 or 2000. In column (7) I report the probability of being in the sample after between 2 and 5 periods after failure conditional on not being in the sample 1 year after. The data show evidence of failers being less likely to attend intermittently than non failers. The estimated effect is about -7 percentage points implying that drop out is a more permanent phenomenon for failers than non failers.

The subsequent rows of the table report separate estimates by grade where failure occurred. Similar to the results on educational attainment, I find a less pronounced effect of early grade failure on later educational outcomes for those who failed in eighth grade compared to those who failed in seventh or ninth grade. Point estimates suggest a shorter stay among failers compared to non failers in the order of 0.35 to 0.94 years depending on whether failure occurred in eighth or seventh grade. Results based on a spline for observations in the range 21-25 in Table A2 show qualitatively similar results. If anything, the estimated impact of failure on survival, duration and intermittent attendance is larger, consistent with a similarly larger estimated negative effects on maximum grade attended.

Having ascertained that failure rates tend to affect negatively the probability of being in school in any subsequent year, overall duration and intermittent attendance, I now study school progression of those who originally failed compared to those who did not. Because individuals can accumulate some delay later in school, a simple inspection of the data in columns (1) to (6) does not provide a clear indication of the school progression of failers versus non failers year by year. The following columns of the table report information on the number of additional grades achieved by failers and non failers, whether still in school or not, at any time t (=1,.., 5). Because, as said, attendance is measured in term of 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 their 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 consistent with positive but low drop out rates among non failers. Rather interestingly, after two years failers partially catch up with non failers. The estimated gap is -0.64. This implies that the proportion of those passing the grade at time t=1 and staying on between time t=1 and t=2 (and ignoring re-entry) is greater for those who failed at time t=0 compared to those who did not. After time 2 the differences between failers and non failers widens up again. This is possibly the result of differential drop out rates or possibly the effect of further grade failure among those who originally failed at time t=0. The data are unable to provide an answer to this (expect in the very short run as I discuss below). After 5 years, the difference in maximum grades attended is -0.79. This is close to, but as expected lower than, the effect on the censored distribution reported in Table 2 (-0.64). Compared to the estimated effect of grade failure in *Ciclo Básico* on duration (column (6)), the data illustrate the differential school progression between the two groups, as measured by the difference in the additional years spent in school and the additional grades attended, i.e. the accumulated delay in school, remains unchanged by the end of the period compared to the year just following grade failure.

Data across grades 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. Trends for those in seventh or ninth grade are rather similar. Results based on a spline between 21-25 missed school days and reported again in Table A2 in the appendix lead to very similar conclusions.

In sum, the data show an early disadvantage for grade failers in terms of grades attended whom they hardly ever make up for. There appears to be some initial catching up between failers and non failers but this is a rather transient phenomenon largely limited to the first period following grade failure after which differentials widen up again. Intermittent attendance is unlikely to give reason of this result. These results hold particularly true for those in seventh or ninth grade. As already noticed above, at the end of the period of observation the proportion of non failers is much larger than the proportion of failers, implying that observations on failers are at much higher risk of being right censored. In turn this suggests that the results in Table 2 are likely to underestimate – and potentially rather severely - the magnitude of the (negative) effect of grade failure on school outcomes.

One limitation of the data, as said, is information on grade failure in 1999 and 2001 is missing, hence I am unable to say whether in any given year following time 0 failers face a different risk of repetition than non failers. In this sense the data do not allow us to investigate what role later failure and later drop out separately play in explaining these trends. In the next section I will try to shed some light on these different channels for the year just following grade failure.

V.d Repeaters

In order to try to shed some light on the subsequent failure rate of early failers and passers in this section I compare the outcomes of early failers who stay on (i.e. repeat a grade) to those who pass. 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 though, there is large non compliance among non failers since a large proportion tends 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 (ignoring general equilibrium effects or externalities of some kind) 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.[21]

Table 5 presents regression results for the sub-population of repeaters and compares it to the population of non failers who stay on the following year. Again regression results based on a spline for observations only in the range 21-25 are presented in the appendix, Table A3.

Column (1) presents the first stage estimates for this subpopulation. Compared to the estimates in Table 2, estimates among the compliers show in general a larger effect of the rule on grade failure. If repeaters are those with higher latent outcomes among the population of failers what this possibly suggests is that enforcement of the rule is stricter among those with better latent outcomes. Possibly this is due to the circumstance that schools tends to apply this rule with less vigor to those with prior accumulated delay.

In column (2) I present the effect of grade repetition on the probability of dropping out of school between time t=1 and time t=0 (i.e. one minus the hazard rate). It is remarkable that 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 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. In principle school changes within the public system are not easy. Consistent with this one finds no effect of grade failure on school change, although, as shown below, there is substantial heterogeneity across grades.

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[21] Indeed a regression of missed school days at time t_0 on a dummy for drop out, a dummy for missed school days larger than 25 and the interaction of the two (plus controls as above) leads to a coefficient on the interaction term of 2.178 (s.e. 0.502) suggesting that failers with relatively more missed school days are more likely to drop out than passers with relatively more school days.

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 negative effect of grade failure remains negative and strong even among those who repeat a grade. Point estimates imply that repeaters accumulate 0.62 less grades and stay in the sample on average 0.42 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. In particular it asks whether there is any state dependence or unobserved heterogeneity that generate a correlation between grade failure across subsequent years, so that repeaters tend systematically to fail more or less than non repeaters. Although the data do not allow us to investigate grade failure in any subsequent year (due to missing data 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, as already hinted at in the introduction, students might react to early grade failure by exerting more effort or repetition might improve their attainment. However, the opposite might be true due potentially 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 have an incentive not to retain failers 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 36 percentage points less likely to fail again than non repeaters. A simple way of checking whether this is due to differential treatment on the part of schools or instead changes in students' behavior consists in running the same regression as in column (6) with the addition of current (i.e. time 1) number of missed school days. I do so by including in the model a quadratic spline in number of missed days at time t=1, a dummy for 26 or more missed school days and interactions of the two. Column (7) shows that conditioning for missed school days at time t=1 the negative effect of repetition on failure rates later in school tends to fall by about two thirds. Conditional on absenteeism at time t=1, repeaters are still less likely to fail a grade than non repeaters but this effect is now estimated to be in the order of 14 percentage points. Column (8) clearly illustrates why this correlation tends to become weaker. Here I regress number of missed school days at time t=1 on the discontinuity at time t=0. It is remarkable that at time t=1 repeaters tend on average to miss 24 less days of school than non repeaters. What this suggests is that a factor largely explaining 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.

Differences across grades show in general rather consistent patterns with those mentioned above. One finds again a weaker effect of repetition in eighth grade. It is perhaps worth mentioning that the negative effect of grade failure on the probability of being in the sample two years after grade failure conditional on having repeated is particularly low for those repeating ninth grade. This would suggest that those who repeat ninth grade tend to drop out of non vocational public schools after they have completed *Ciclo Básico.* Rather interestingly the data also reveal some heterogeneity in the probability of changing school following grade failure across grades. Although I find no effect in seventh grade, the effect is positive in eighth grade and negative in ninth grade. This might be a suggestion that failers in the second year of *Ciclo Básico* use selective choice of school as a way to absorb part of the negative effect of grade failure. That might explain why the effect of grade failure is less pronounced for these individuals although one might wonder why the same does not happen among those failing seventh grade. As for those in ninth grade, the negative result is due most likely to the circumstance that those ending *Ciclo Básico* change institution once they register in the first year of upper Secondary education (tenth grade) while repeaters will have to wait a year before possibly doing so.

Results with a spline for the observations in the range 21-25 in Table A3 lead to similar conclusions. Rather interestingly, regressions in column (7) where grade failure at time t=1 is regressed on grade repetition plus controls for missed school days at time t=1 show that grade failure at time t=1 is completely (rather than by about two thirds) explained by students' behavioral changes. According to these estimates it appears that the lower risk of subsequent failure among early failers is exclusively due to these students' increased effort.

To wrap up, the results in this section illustrate that the negative effects of grade failure extend also to those (arguably 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 5 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 in an attempt to pass. As said, though, this initial partial reduction of the gap is undone in later years.

V.e Estimates Away from the Discontinuity Point

In this section I propose an alternative identification strategy based on differences in differences. As already hinted at above, the rule governing grade promotion changed somewhat in 1998. Although the basic principles of the system were left unchanged, at the beginning of the school year 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)[22]. 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 was later abrogated by the directive in question.[23] This created a climate of insecurity among teachers and students regarding the rules that would have eventually governed grade promotion at the end of the school year 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 A5 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 in the 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 some (but not all) schools abandoned the practice of failing children above 25 missed school days in 1998 or that schools were on average 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 A6 in the appendix where I plot the frequency distribution of missed school days in 1998 *vis à vis* the distribution in 1996/1997 (conditonal on the number of missed school days being between 1 and 35). A visual inspection of the Figure shows that the shape of the two distributions is very similar.[24] Taking together the evidence in Figures A5 and A6, it appears that while the incentives linked to the

[22] This is directive ANEP, 2320/98, April 21 1998.
[23] This is ANEP, *Acta* 70 *Res.* 96, 1997.
[24] As a more formal check, I have run a multinomial logit regression of number of missed school days (between 1 and 35) between 1996 and 1998 on a dummy for 1998. A Chi-2 test that the year 1998 dummies are jointly different from zero leads to a rejection of the null (chi2 (34)=138.44). This should be no surprise since given the very large sample size (176,453 observations).

rule in force in 1996/1997 remained largely in place in 1998, the rules governing grade promotion did not. Most likely the climate of insecurity around the discipline in force 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 that 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 different 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. The advantage of this estimator with respect to the one based on the discontinuity at 25 missed school days is that, to the extent that sorting around the discontinuity point is a source of concern and provided that sorting is similar across years, by differentiating across individuals with the same number of missed school days in different years one should be able to control for this source of unobserved heterogeneity. If the two estimation strategies lead to similar results, this is suggestive of sorting being unlikely to be a major problem in the estimates above.

Before presenting the regression results, it is useful to show graphically the effect of this regime change on students' outcomes. In Figure 7 I plot the time difference between 1998 and 1996/97 in the failure rate at any level of missed school days (between 1 and 35). I standardize this series to its value at 20 missed school days. This is essentially the (standardized) difference between the series plotted in Figure A5 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 less stringency in the application of the 25 missed school days rule in 1998. I have superimposed to this series the difference between the subsequent school outcomes of those observed in 1998 and those observed in 1996/97. To make these outcomes comparable across years, I have computed the number of additional grades attended after three years since the time the individual is observed failing a grade (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 7, in Table 6 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 dummies for missed school days and for year dummies. As before, I report regressions without and with additional controls. Standard errors are again clustered by number of missed school days. The top part of the table refers to regressions on all observations with 1-35 missed school days while the bottom part refers to observations with missed school days in the range 21-35. It is remarkable that the estimated coefficients are very similar to the ones in Table 2. Point estimates imply that grade failure is associated to between 0.58 and 0.96 less years of education. Results are robust to the sample used. The inclusion of additional controls makes some difference to the results when the entire sample is used, leading to smaller estimates of the negative effect of grade failure on subsequent school outcomes.[25] Although one might be concerned about this, differences in the estimated coefficient on the restricted sample (those with 21-35 missed school days) are virtually unchanged, suggesting little correlation between observed pupils' and schools' characteristics and the instrument.

[25] Since, as said, age is missing in 1998 and hence for individuals first observed in this school year, in this regression I add a dummy for missing age and missing grade-age distortion.

In sum, results based on differences in differences that exploit the regime change in the discipline governing grade failure between 1996/97 and 1998 lead to very similar conclusions on the effect of grade failure on subsequent school outcomes. This can be taken as corroborating evidence of the results in the previous sections, suggesting that students' non-random sorting around the discontinuity point is unlikely to be a major source of bias.

V.f Falsification Exercise

As an additional and final check for my estimates above, in this section I exploit the incentives created by the promotion rules in force in 1996/1997 for individuals to cluster at 20 missed school days (i.e. away from the discontinuity point). Recall from the previous discussion that promotion is automatic for students with no more than three pending subjects provided they have no more than 20 missed school days. Students hence will have an incentive not to cross this threshold. This is confirmed by the data (Figure 3).The circumstance that the distribution of the running variable number of missed school days appears discontinuous not only at 26 missed school days (where repetition is also discontinuous) but also at 21 missed school days (where there is no apparent discontinuity in promotion) is rather helpful for the purposes of my estimation.

As already discussed, the discontinuity in the distribution of the variables missed school days at 26 is potentially invalidating the consistency of the regression discontinuity estimates. In particular, if non-random sorting is such that individuals with better (worse) latent outcomes locate systematically below (above) the 26 missed school days threshold, this implies that the figures in Tables 2 to 5 tend to overestimate its impact of failure on later students' outcomes. However, if students tend to non-randomly sort around the 26 missed school days threshold – either because of their differential ability or because of differential treatment on the part of schools of teachers - one would expect a similar behavior around the 21 missed school days. Because there is apparently no discontinuity in promotion at the 21 days threshold, one can check whether actual outcomes vary discontinuously around that threshold. In particular if later outcomes worsen as a student crosses this threshold, this would suggest that students to the right and to the left of this threshold are systematically different for reasons other than failure rate and one should hence be very cautious in interpreting the results above as the (negative) effect of failure on later outcomes. In practice, this is a test for the exclusion restriction underlying the consistency of the instrumental variable estimates above. Already, a visual inspection of Figures 4 and 5 illustrates that this is unlikely to be a major problem, since there is no apparent fall in the outcome variables at the 21 days threshold.

This is confirmed in Table 7 where I present the estimated coefficient of regressions of grade failure (first stage estimates) and additional grades attended (reduced form estimates) on a dummy for 21 or more missed school days. All regressions refer to observations in the range 1-35 missed school days and again refer to the school years 1996 and 1997. These coefficients come from the regression reported in the bottom part of Table 2 where I allow both the shape and the intercept of the outcome variable to vary both between 20 and 21 missed school days and between 25 and 26 missed school days. Again standard errors are clustered by number of missed school days. I report regressions without and with controls. When no controls are included, there is evidence of a slight discontinuity in failure rates at 21 missed school days. The point estimate is 0.03 but the estimates are insignificant at conventional levels. When controls are included the estimate falls to about 0.002. Essentially the regression results show no evidence of a discontinuity in failure rates between 20 and 21 missed school days. The reduced form estimates show instead some discontinuity in the variable additional grades attended. When no controls are included the point estimates imply that those to the right of 21 missed school days gain on average a third of a school year relative to those to the left of this point (estimate is 0.32, and statistically significant at conventional levels). The point estimate falls to 0.19 and is again significant when all the controls are included. In sum, the data show some discontinuity in the outcome variable. This is such though to suggest that those at or to the right of 21 missed school days - if anything- perform better than those to its left. Potentially this might be the result of schools or teachers manipulating the records of missed school days in such a way to compensate underperformers or failers who stay on exerting more effort or being temporarily strengthened by grade repetition. If one is willing to extrapolate these results to those around the 26 missed school days threshold, this suggests that the point estimates above – if anything – tend to underestimate the negative effect of grade failure on later outcomes. So the estimates above have to be taken as conservative estimates.

VI. CONCLUSIONS

School systems in many developing countries are characterized by remarkably high failure and drop out rates and overall low school attainment. Uruguay is no exception to this: repetition rates in public schools in the compulsory school cycle (grades 1-9) are in the order of 20-25% and about a fourth of individuals aged 25-29 fail to have completed compulsory education.

This paper uses administrative micro data on around 100,000 students in Junior High school (grades 7 to 9) in Uruguay between 1996 and 1997 to assess the causal impact of grade failure on drop out and later 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 show that grade failure leads to between two thirds and one year less additional school grades attended by the year 2001. Results are robust to different specifications for the counterfactual school outcomes in the absence of grade failure.

A large part of the disadvantage for grade failers manifests through immediate drop out: compared to non failers, failers are at disproportionate risk of abandoning school the year when failure occurs. However, even among those who stay on (and hence repeat the grade they failed) the effects of grade failure appear 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 (in terms of lower absenteeism) compared to non failers. 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. If anything, non failers are more likely than failers to leave temporarily the system and then re-enter.

A major concern regarding my conclusion 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 26 missed school days threshold, one might be concerned that my estimates pick up unobservable differences in students' characteristics on either side of the threshold rather than the genuine effect of this policy. Evidence from the distribution of missed school days shows a clear clustering of individuals at 25 missed school days. Although this is consistent with the incentives of the promotion rule and not necessarily a sign of the continuity assumption in the distribution of unobservable characteristics required for consistency of the regression discontinuity estimator being violated, there are good reasons to be concerned about this feature of the data (McCrary (2005)). In order to try to deal with this potential source of inconsistency of the regression estimates, I propose a number of empirical 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 a discontinuity in students' observable characteristics at 26 missed school days (Lee (2004)). Second, I use observations not in the immediate neighborhood of the discontinuity point. In this way I hope to net out the bias in the estimates due to otherwise better performing students exactly locating at 25 missed school days. Third, I use a similar discontinuity in students' incentives to cluster at 20 (in addition to 25) missed school days implicit in the rules governing grade promotion. Because the failure rate shows no discontinuity between 20 and 21 missed school days, one can use this feature of the data to test for the exclusion restriction underlying the consistency of the IV estimates. Last, I use a regime change that took place during the school year 1998 to derive estimates away from the discontinuity point. During this school year, students with more than 25 missed days could pass at the discretion of the school. I use this regime change to derive a difference (across years, 1998 and 1996/1997) in difference (across individuals with different numbers of missed school days) estimate of the effect of grade failure on subsequent school outcomes.

Although my empirical estimates are somewhat sensitive to these different identification strategies, a single response emerges, namely that the effect of grade failure on school outcomes remains consistently negative and of reasonably similar magnitude. Generally speaking – and with the exception of results based on observations not in the immediate neighborhood of the discontinuity point – these checks suggest, if anything, negative sorting around the discontinuity point, with otherwise worse performing students being more likely to locate below the threshold, and hence being less likely to fail a grade. This would in turn suggest that the effect of failure on subsequent school outcomes is conservatively estimated. One possible explanation for this finding is that schools or teachers tend to be more lenient towards otherwise weaker students, due either to the recognition that these students might benefit less from grade failure, to teachers' self interest or compassion. Another explanation is that in the short run repeaters tend to exert more effort or they benefit from repetition in terms of increased attainment so that repetition is unlikely to occur more than once.

One major limitation of the data at hand 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 exaggerating the negative effect of this policy on later school outcomes. Although there is no direct way of checking this with my data, complementary evidence from a phone survey quoted in ANEP (2000) suggests that the majority of school drop outs leaves the system permanently, implying that movements to schools outside the system are unlikely to be of great importance. A second limitation of the data is that these only provide information on maximum grade attended (as opposed to maximum grade completed). Although there is no way to check this directly in the data, it is hard to believe that results would be reverted if information on actual grade completion were available. This would imply previous grade failers being disproportionately more likely to complete the final grade they attended compared to non failers.

Note additionally that because non failers are at much higher risk of being in the sample by the end of the period, this tends to underestimate the negative gap in subsequent educational attainment between failers and non failers. This again suggests that the effects of grade failure on school outcomes in the medium term estimated in this paper are possibly rather conservative estimates.

In sum, the data show strong negative causal effects of grade failure on a students' subsequent school outcomes. Back of the envelope calculations show that at least about a third of early drop out can be imputed to grade failure.[26] It must be emphasized though that the paper remains agnostic on whether overall this is a good policy from the perspective of the social planner. This would require quantifying the private and social costs and benefits of grade failure, including possibly the deterrence effect of such policy on students' underperformance. The evidence presented in the paper suggests that these deterrence effects might not be negligible.

A second important qualification of my conclusions regards the external validity of the estimates. As said, recent evidence for the US on the effect of repetition appears to suggest positive or at least non negative effects of this policy on school attainment of primary school children, at least in the short term (Jacob and Lefgren (2004)). There are a number of potential reasons why my results differ from the empirical estimates for the USA, in addition to the obvious differences in the two school systems in question. First, my analysis concentrates on students in Junior high school: the effect of repetition might be very different for students of different ages and in different school grades. Second,

[26] These are conservative estimates obtained by multiplying the average failure rate (30%) times the estimated effect of failure on the survival probability one year after (-0.25, row 1, column 1 in Table 4) and dividing it by the average hazard rate (0.26).

my data do not allow us to estimate the effect of repetition on attainment, since information on final year marks or test scores is not reported. In principle I cannot rule out that attainment improves following repetition, although the evidence that repeaters end up staying shorter in school and attending on average less grades appears hard to reconcile with improved attainment. Indeed, not differently from Jacob and Lefgren (2004), my estimates show improvements in performance (measured by lower probability of repeating again) among first time repeaters. As said, though, this is largely attributable to greater effort exerted by these students when they repeat a grade rather than presumably increased attainment. Because my data set allows me to follow individuals over 4 to 5 years after failure occurred, I am also able to assess the medium term effects of this policy. I show that the incentives to exert more effort among repeaters are rapidly dissipated and the medium term effect of grade failure is negative even among those who stay on. A final major difference between previous work and my analysis is that by concentrating on teenagers in a developing country I am able to study directly the effect of grade failure on immediate drop out, a phenomenon that is unlikely to happen among primary school children in more developed countries. As shown, this margin of adjustment is of great importance among Uruguayan teenagers and presumably similar effects are to be expected in other developing countries.

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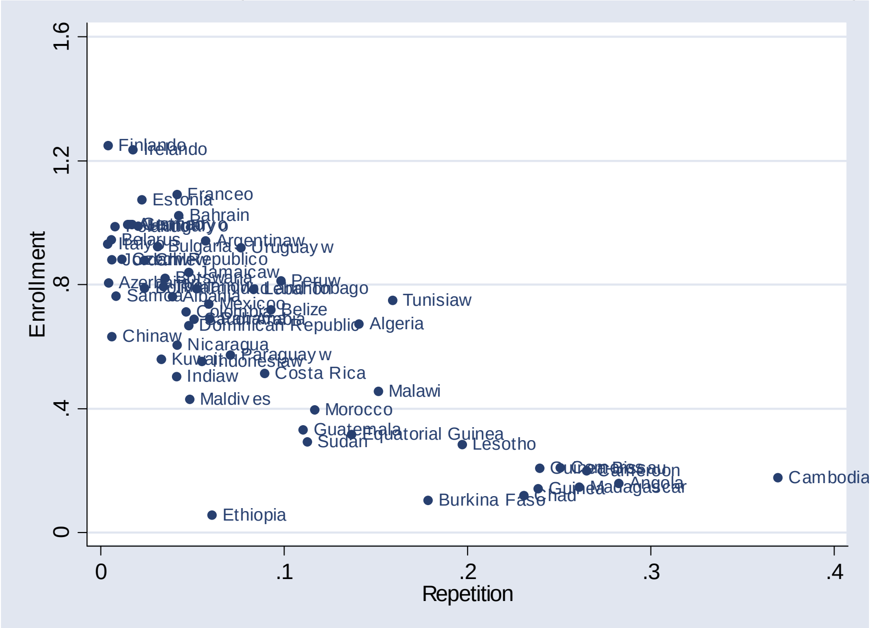
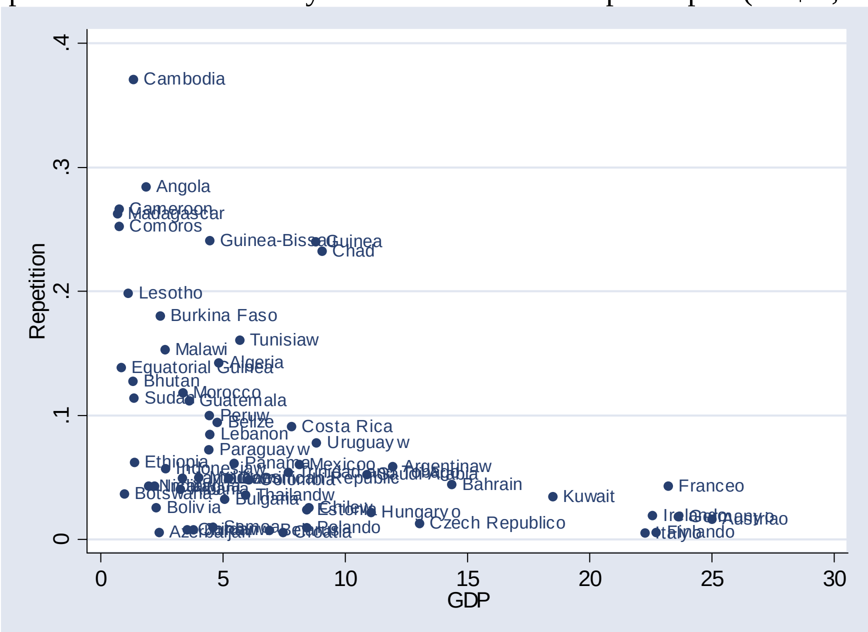
Figure 1
Repetition Rates in Primary School and Gross Enrollment Rate in Secondary School



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 country with positive and non missing information on repetition rates and with an income per capita not greater than US$ 28,000. Source: UNESCO (2002).

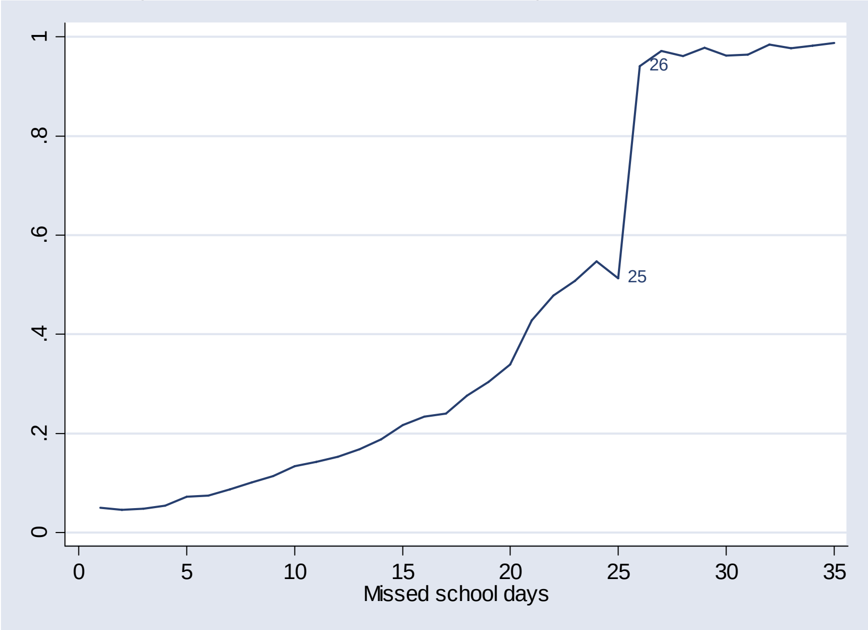
Figure 2
Repetition Rates in Primary School and PPP GDP per Capita (US$ 1,000)



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

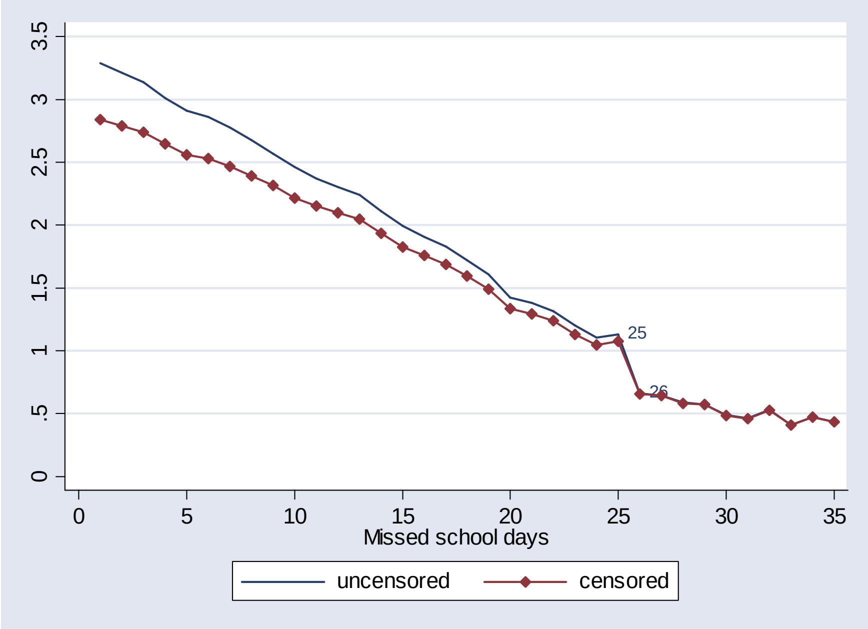
Grade Failure by Number of Missed School Days in *Ciclo* Bás*ico* (1996-1997)



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.

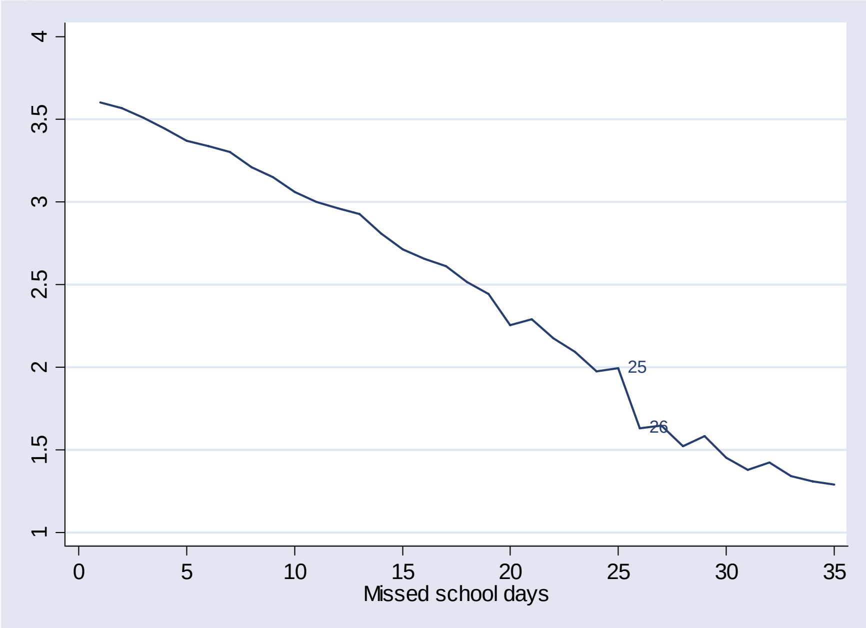
Figure 4

Additional Grades Attended by 2001 and Number of Missed School Days in *Ciclo* Bás*ico* (1996-1997)



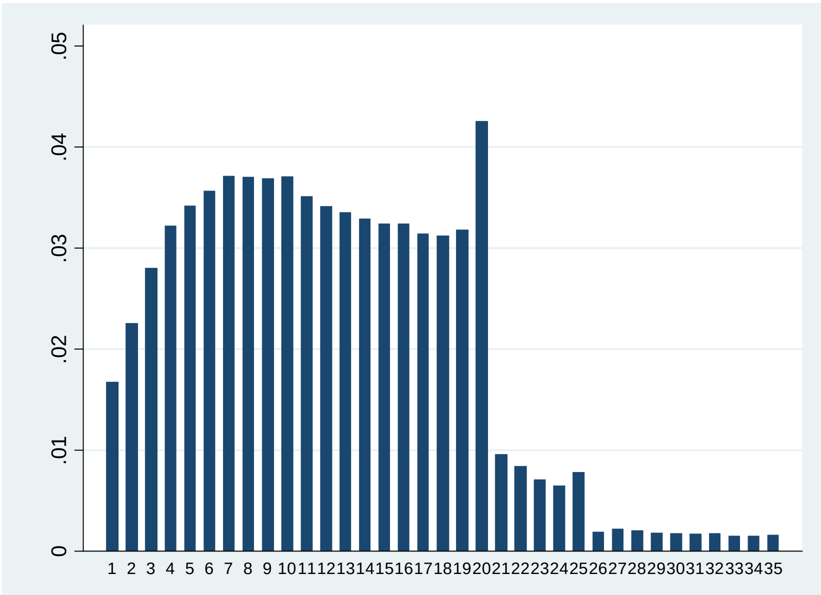
The figure reports the number of additional grades attended by 2001 (solid line) among those in *Ciclo Básico* in 1996 or 1997 as a function of the number of missed school days in *Ciclo Básico.* The dotted line reports the same series censored to the year 2000 for those who did not fail.

Figure 5
Duration in Sample (1966-2001) and Number of Missed School Days in *Ciclo* Bás*ico* (1996-1997)



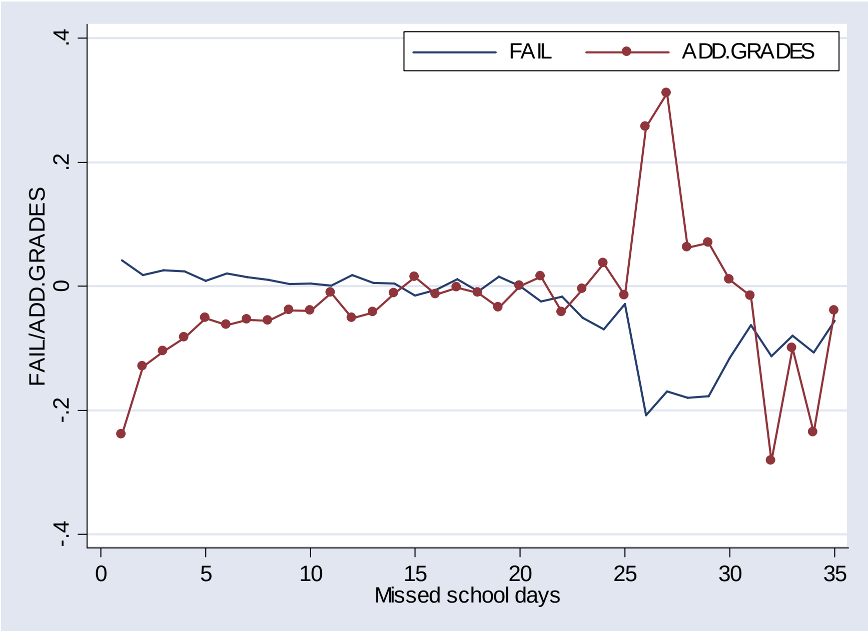
The figure reports the average duration in the sample until 2001 among those in *Ciclo Básico* in 1996 or 1997 as a function of the number of missed school days in *Ciclo Básico*.

Figure 6
Proportion of Missed School Days in *Ciclo Básico* (1996-1997)
1-35



The figure reports the distribution of number of missed school days over the support 1-35.

Figure 7
Failure Rate and Additional Grades Attended
Difference between 1998 and 1996/1997



The figure reports the difference (between 1998 and 1996/1997) in difference (relative to values at 20 missed school days) in failure rates and additional grades attended after 3 years (2 years for grade failers) by number of missed school days in *Ciclo Básico*.

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

		All			Failers			Passers		
			Grade			Grade			Grade	
		VII	VIII	IX	VII	VIII	IX	VII	VIII	IX
1.	Age	12.583	13.575	14.843	13.285	14.197	15.421	12.271	13.344	14.578
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.	Fail grade	0.307	0.271	0.315	1.000	1.000	1.000	0	0	0
5.	Additional grades attended by 2001	2.305	2.097	1.568	0.534	0.567	0.486	3.089	2.665	2.067
6.	% censored (2001)	0.470	0.346	0.160	0.140	0.137	0.109	0.616	0.423	0.183
7.	Duration	2.912	2.754	2.207	1.353	1.489	1.413	3.602	3.225	2.573
8.	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 descriptive 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 fail a grade and the right panel to those who do not fail a grade. For each panel I report information separately by grade. Row 1 reports average age, 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 number of additional grades attended until the year 2001, row 6 the proportion still in school in 2001, row 7 the duration in the sample until 2001, row 8 the number of additional grades attended until 2001 if a student failed a grade (as in row 4) or until 2000 if a student did not fail.

Table 2

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Grade failure		Additional grades			
	Spline 1-25					
I(Missed days>25)	0.377	0.369	-0.158	-0.237		
	(0.022)***	(0.025)***	(0.052)***	(0.046)***		
Grade Failure					-0.418	-0.642
					(0.145)***	(0.113)***
	Spline 21-25					
I(Missed days>25)	0.466	0.446	-0.385	-0.406		
	(0.017)***	(0.016)***	(0.041)***	(0.042)***		
Grade Failure					-0.826	-0.911
					(0.082)***	(0.075)***
Controls		yes		yes		yes

Columns (1) and (2) report the OLS coefficient of a regression of grade failure on a dummy for more than 25 missed school days in *Ciclo Básico* (first stage equation). Columns (3) and (4) report 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 same dummy (reduced form equation). Columns (5) and (6) report 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. Regressions refer only to observations with 1 to 35 missed school days. All specifications include a second order polynomial in the number of missed days interacted with a dummy for missed school days greater than 25. For each specification the table reports results without and with additional controls. Additional controls are: dummies for age, sex, age-grade distortion, grade, year and school. The bottom part of the table reports the corresponding regression coefficients with the addition of a dummy for missed school days less than 21 and interactions of this dummy with the second order polynomial in the number of missed school days. Standard errors in brackets are clustered by number of missed school days. ***, **, * denote respectively significant at 1%, 5% and 10% level. Number of observations 105,433.

Table 3

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

Separate Estimates by Grade

Controls Included

	(1)	(2)	(3)	(4)	(5)	(6)	
	Spline 1-25			Spline 21-25			Observations
	Failure	Additional grades		Failure	Additionalgrades		
I(Missed days>25)*I(VII)	0.332 (0.027)***	-0.234 (0.054)***		0.409 (0.030)***	-0.375 (0.058)***		
Grade Failure*I(VII)			-0.714 (0.149)***			-1.003 (0.126)***	40,486
I(Missed days>25)*I(VIII)	0.386 (0.030)***	-0.127 (0.086)		0.510 (0.028)***	-0.344 (0.082)***		
Grade Failure*I(VIII)			-0.323 (0.208)			-0.648 (0.138)***	34,201
I(Missed days>25)*I(IX)	0.376 (0.027)***	-0.246 (0.055)***		0.408 (0.038)***	-0.462 (0.046)***		
Grade Failure*I(IX)			-0.653 (0.161)***			-1.133 (0.150)***	30,746

The table reports the same regressions as in Table 2 separately by grade (VII, VIII and IX). Only specifications with the whole set of controls reported. See notes to Table 2.

Table 4
Grade Failure in *Ciclo* Bás*ico* (1996-1997) and Subsequent School Outcomes
Spline 1-25 – Controls Included
IV estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Survival at time t=					Duration	Intermittent attendance	Additional grades at time t=				
	1	2	3	4	5			1	2	3	4	5
Grade failure	-0.251 (0.022)***	-0.184 (0.037)***	-0.166 (0.029)***	-0.127 (0.061)**	-0.172 (0.082)**	-0.814 (0.128)***	-0.066 (0.031)**	-0.876 (0.011)***	-0.639 (0.079)***	-0.654 (0.113)***	-0.709 (0.133)***	-0.791 (0.145)***
Observations	105,433	105,433	105,433	105,433	47,470	105,433	94,973	105,433	105,433	105,433	105,433	105,433
Grade Failure*I(VII)	-0.366 (0.046)***	-0.198 (0.087)**	-0.208 (0.038)***	-0.094 (0.064)	0.031 (0.066)	-0.939 (0.217)***	-0.061 (0.063)	-0.904 (0.019)***	-0.720 (0.098)***	-0.712 (0.118)***	-0.707 (0.145)***	-0.817 (0.170)***
Observations	40,486	40,486	40,486	40,486	18,295	40,486	37,438	40,486	40,486	40,486	40,486	40,486
Grade Failure*I(VIII)	-0.186 (0.049)***	-0.180 (0.125)	-0.001 (0.054)	0.023 (0.107)	-0.196 (0.077)***	-0.345 (0.164)***	0.032 (0.029)	-0.907 (0.021)***	-0.558 (0.099)***	-0.423 (0.209)*	-0.324 (0.268)	-0.373 (0.274)
Observations	34,201	34,201	34,201	34,201	15,449	34,201	31,604	34,201	34,201	34,201	34,201	34,201
Grade Failure*I(IX)	-0.160 (0.043)***	-0.168 (0.070)**	-0.241 (0.113)**	-0.109 (0.047)**	0.037 (0.111)	-0.679 (0.184)***	-0.140 (0.057)**	-0.790 (0.025)**	-0.503 (0.082)***	-0.626 (0.110)***	-0.693 (0.166)***	-0.700 (0.162)***
Observations	30,746	30,746	30,746	30,746	13,726	30,746	25,931	30,746	30,746	30,746	30,746	30,746

Numbers in the table are IV estimates of the dependent variable on grade failure at time t=0 where grade failure is instrumented by the discontinuity at 26 missed school days. All regressions control for a second order polynomial in number of missed school days interacted with a dummy for 26 or more missed school days plus the whole set of controls. See notes to Table 2 for further details. Columns (1) to (5) reports probability of being in the sample in any given year (1,..., 5) following grade failure. Column (6) reports average duration. Column (7) reports intermittent attendance. Columns (8) to (12) reports the average increase in number of grades attended by the individuals in the sample in any given year (1,...5) following grade failure. See text for details.

Table 5
Grade Repetition in *Ciclo* Bás*ico* (1996-1997) and Subsequent School Outcomes
Repeaters Only
Spline 1-25 – Controls

	(1) Repeat	(2) In sample t=2	(3) Change School t=1	(4) Duration	(5) Additional grades	(6) Fail t=1	(7) Fail t=1	(8) Missed days t=1	Observations
I(Missed days>25)	0.430 (0.023)***								
Grade Failure		-0.061 (0.028)**	0.027 (0.022)	-0.424 (0.141)***	-0.616 (0.115)***	-0.362 (0.068)****	-0.139 (0.033)***	-24.070 (6.043)***	94,977
I(Missed days>25)*I(VII)	0.417 (0.030)***								
Grade Failure*I(VIII)		-0.071 (0.071)	-0.004 (0.027)	-0.465 (0.229)*	-0.664 (0.153)***	-0.242 (0.091)**	-0.091 (0.055)	-21.814 (14.207)	37,438
I(Missed days>25)*I(VIII)	0.432 (0.025)***								
Grade Failure *I(VII)		0.034 (0.035)	0.182 (0.053)***	0.058 (0.176)	-0.272 (0.204)	-0.479 (0.094)***	-0.157 (0.054)***	-36.573 (6.527)***	31,606
I(Missed days>25)*I(IX)	0.429 (0.027)***								
Grade Failure *I(IX)		-0.116 (0.065)*	-0.419 (0.098)***	-0.555 (0.146)***	-0.806 (0.149)***	-0.409 (0.067)***	-0.191 (0.043)***	-15.234 (5.657)**	25,933
Missed Days t=1							yes		

The table refers to repeaters, i.e. those who fail a grade and stay on the following year. Column (1) reports the OLS estimate on a dummy for more than 25 missed school days on repetition. Columns (2)-(8) report IV estimates. Column (2) reports the probability of being in the sample 2 yeas after grade failure. Column (3) the probability of changing school in the year following grade failure. Columns (4) and (5) duration and (censored) additional grades attended. Column (6) and (7) the probability of failing at time 1.Column (8) the number of missed school days at time 1.

Table 6

Missed School Days, Grade Failure in *Ciclo* Bás*ico* (1996, 1997 and 1998) and Subsequent School Outcomes

IV Estimates

Difference in Difference

	(1)	(2)
	Additional grades	
	1-35	
Grade Failure	-0.963	-0.579
	(0.119)***	(0.109)***
	21-35	
Grade Failure	-0.650	-0.579
	(0.085)***	(0.115)***
Controls		yes

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 observations in 1998. All regressions control for year and missed school days dummies. The top part of the table refers to observations with 1 to 35 missed school days (176,453 observations) and the bottom part to those with 21-35 missed school days. See also notes to Table 2.

Table 7

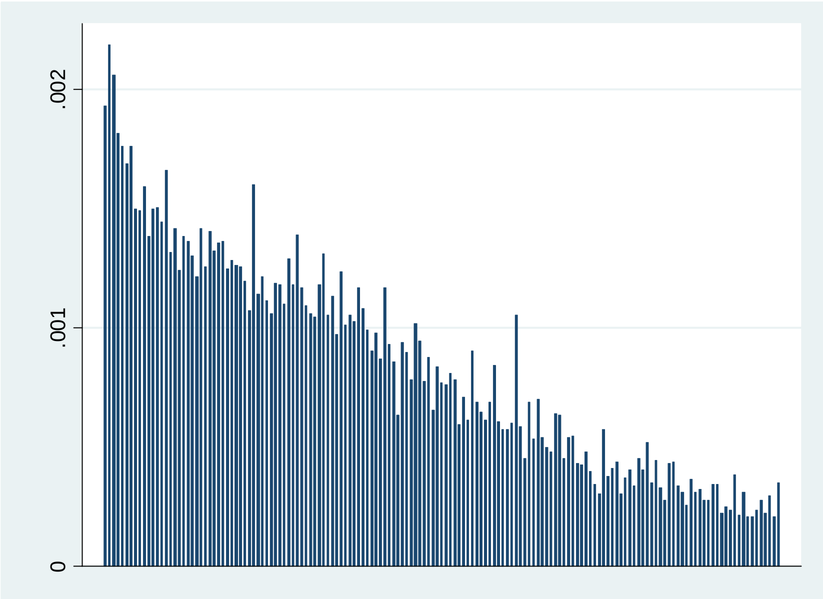
Missed School Days, Grade Failure in *Ciclo* Bás*ico* (1996-1997) and Subsequent School Outcomes

Falsification test

	(1)	(2)	(3)	(4)
	Grade failure		Additional grades	
I(Missed days>20)	0.026	0.002	0.317	0.192
	(0.018)	(0.014)	(0.067)***	(0.048)***
Controls		yes		yes

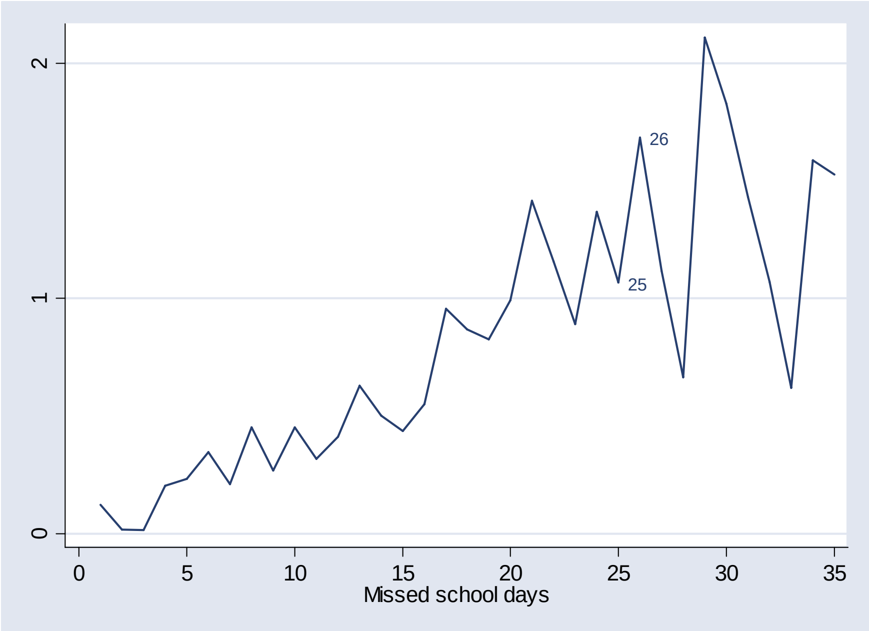
The table reports a coefficient on a dummy for missed school days greater than 20 from the same regressions as in the bottom part of Table 2, columns (1) to (4).

Figure A1
Proportion of Missed School Days in *Ciclo Básico* (1996-1997)
26-180



The figure reports the distribution of number of missed school days over the support 26-180.

Figure A2
Age-Grade Distortion and Number of Missed school Days in *Ciclo Básico* (1996-1997)

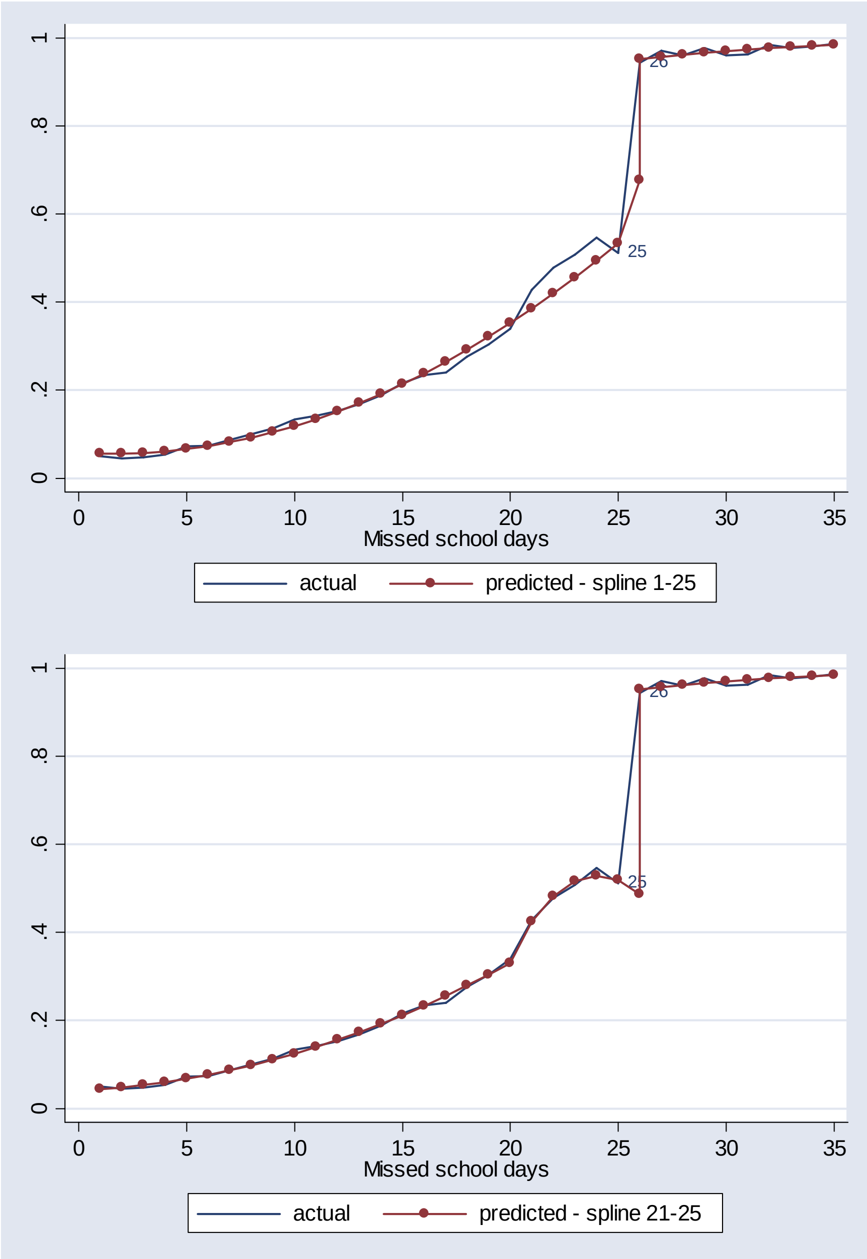


The figure reports the average age-grade distortion by number of missed school days. The data refer to individuals in *Ciclo Básico* in 1996 or 1997 and with a number of missed school days between 1 and 35.

Figure A3

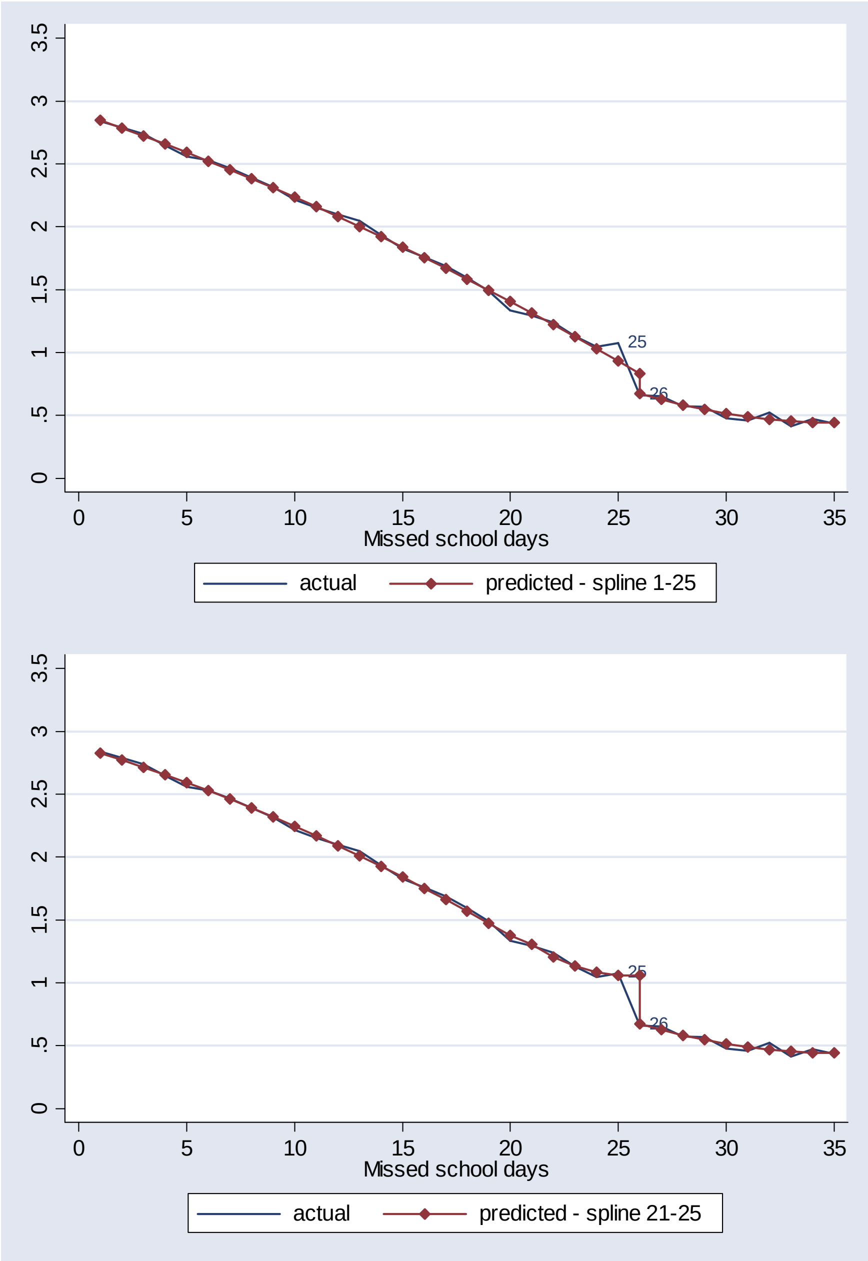
Grade Failure by Number of Missed School Days in *Ciclo Básico* (1996-1997)

Actual and Predicted



The figure reports the actual (as in Figure 3) and estimated proportion of individuals failing a grade by number of missed school days. The data refer to individuals in *Ciclo Básico* in 1996 or 1997 and with a number of missed school days between 1 and 35. Predicted series are obtained by fitting separate quadratic splines to the left and the right of the discontinuity point. The top graph fits a spline to the left of the discontinuity based on observations in the range 1-25. The bottom panel fits a spline based on observations in the range 21-25.

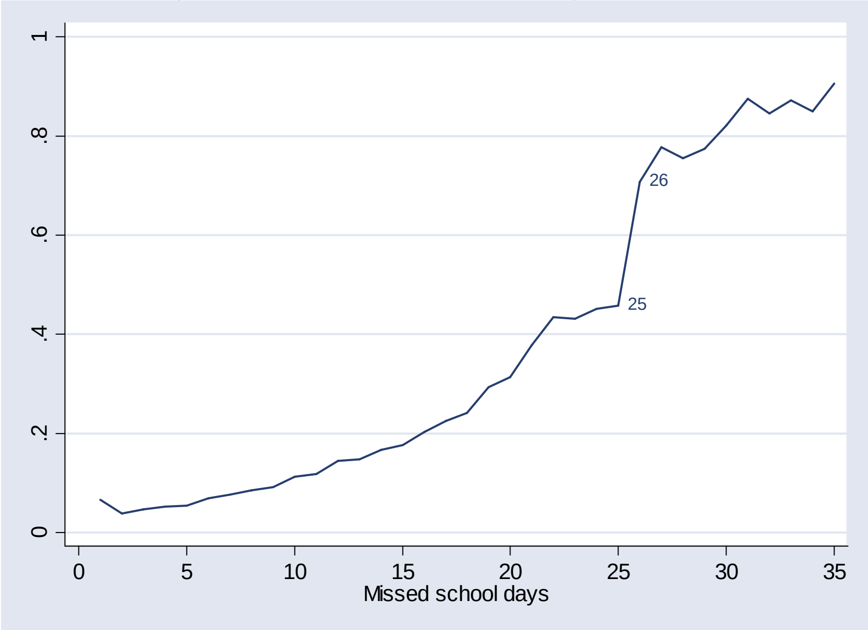
Figure A4
Additional Grades Attended by 2001 and Number of Missed School Days in *Ciclo* Bás*ico* (1996-1997)
Actual and Predicted



The figure reports actual (as in Figure 4) and estimated (censored) additional grades attended by number of missed school days. The data refer to individuals in *Ciclo Básico* in 1996 or 1997 and with a number of missed school days between 1 and 35. See also notes to figure A3.

Figure A5

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

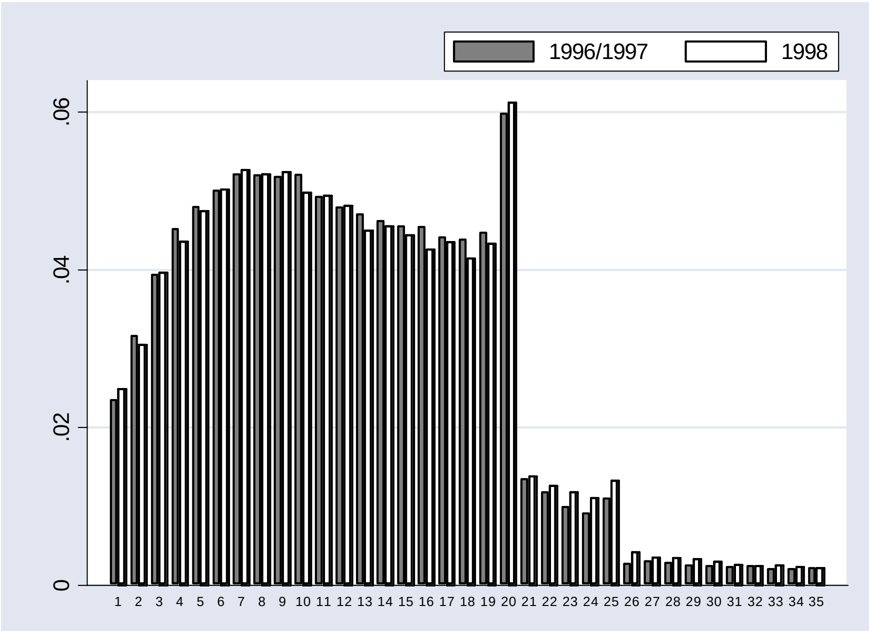


The figure reports the proportion of individuals failing a grade by number of missed school days. The data refer to individuals in *Ciclo Básico* in 1998 and with a number of missed school days between 1 and 35.

Figure A6

Proportion of Missed School Days in *Ciclo Básico* (1998 vs. 1996/97)

1-35



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

Table A1
Missed School Days, Grade Failure in *Ciclo Básico* and Additional Grades Attended
First and Third Order Parametric Splines
Regressions with controls only

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear			Cubic		
	Failure	Additional grades		Failure	Additional grades	
	Spline 1-25					
I(Missed days>25)	0.496	-0.331		0.311	-0.232	
	(0.021)***	(0.033)***		(0.037)***	(0.060)***	
Grade Failure			-0.668			-0.746
			(0.054)***			(0.129)***
	Spline 21-25					
I(Missed days>25)	0.370	-0.321		0.428	-0.530	
	(0.026)***	(0.041)***		(0.023)***	(0.050)***	
Grade Failure			-0.868			-0.807
			(0.061)***			(0.252)***

The table reports the same regressions as in Table 2 (with controls only) where the quadratic spline in number of missed school days is in turn replaced by a linear spline (columns (1) to (3)) and a cubic spline (columns (4) to (6)).

Table A2
Grade Failure in *Ciclo Básico* and Subsequent School Outcomes
Spline 21-25– Controls included
IV estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Survival at time t=					Duration	Intermittent attendance	Additional grades at time t=				
	1	2	3	4	5			1	2	3	4	5
Grade failure	-0.291	-0.304	-0.232	-0.073	0.008	-0.912	-0.163	-0.912	-0.863	-0.965	-0.991	-1.001
	(0.027)***	(0.031)***	(0.030)***	(0.050)	(0.065)	(0.085)***	(0.053)***	(0.015)***	(0.042)***	(0.067)***	(0.094)***	(0.104)***
Observations	105,433	105,433	105,433	105,433	47,470	105,433	94,973	105,433	105,433	105,433	105,433	105,433
Grade Failure*I(VII)	-0.336	-0.180	-0.224	-0.067	0.295	-0.888	-0.149	-0.929	-0.939	-1.010	-0.982	-1.074
	(0.096)***	(0.125)	(0.041)***	(0.061)	(0.072)***	(0.217)***	(0.057)**	(0.035)***	(0.095)***	(0.110)***	(0.109)***	(0.136)***
Observations	40,486	40,486	40,486	40,486	18,295	40,486	37,438	40,486	40,486	40,486	40,486	40,486
Grade Failure*I(VIII)	-0.240	-0.205	-0.046	0.023	-0.100	-0.509	-0.029	-0.954	-0.834	-0.803	-0.717	-0.714
	(0.048)***	(0.035)***	(0.040)	(0.084)	(0.058)*	(0.118)***	(0.029)	(0.033)***	(0.052)***	(0.137)***	(0.180)***	(0.188)***
Observations	34,201	34,201	34,201	34,201	15,449	34,201	31,604	34,201	34,201	34,201	34,201	34,201
Grade Failure*I(IX)	-0.185	-0.331	-0.500	-0.239	0.021	-1.241	-0.029	-0.756	-0.666	-1.011	-1.209	-1.231
	(0.043)***	(0.079)***	(0.136)***	(0.045)***	(0.082)	(0.206)***	(0.029)	(0.022)***	(0.072)***	(0.092)***	(0.162)***	(0.155)***
Observations	30,746	30,746	30,746	30,746	13,726	30,746	25,931	30,746	30,746	30,746	30,746	30,746

The Table reports the same regressions as in Table 2 where additionally a dummy for missed school days less than 21 and its interaction with a quadratic spline in number of missed school days is included in the model.

Table A3
Grade Repetition in *Ciclo Básico* and Subsequent School Outcomes
Repeaters only – Outcomes in year 1
Spline 21-25– Controls
IV estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
	Repeat	In sample t=1	Duration	Change school t=1	Additional grades	Fail t=1	Fail t=1	Missed days t=1	Observations
I(Missed days>25)	0.483***								
	(0.011)***								
Grade Failure		-0.151***	-0.370***	-0.025	-0.779***	-0.205***	-0.031	-19.968***	94,977
		(0.039)***	(0.132)***	(0.056)	(0.079)***	(0.042)***	(0.027)	(5.161)***	
I(Missed days>25)*I(VII)	0.487								
	(0.040)***								
Grade Failure*I(VIII)		-0.139	-0.323	-0.009	-0.770	-0.105	-0.036	-15.271	
		(0.060)**	(0.203)	(0.058)	(0.111)***	(0.057)*	(0.049)	(11.801)	37,438
I(Missed days>25)*I(VIII)	0.532								
	(0.021)***								
Grade Failure *I(VII)		-0.018	0.037	0.182	-0.498	-0.274	-0.009	-23.995	
		(0.029)	(0.137)	(0.053)***	(0.161)***	(0.060)***	(0.031)	(5.993)***	31,606
I(Missed days>25)*I(IX)	0.405								
	(0.047)***								
Grade Failure *I(IX)		-0.271	-0.965	-0.419	-1.159	-0.312	-0.058	-32.536	
		(0.094)***	(0.239)***	(0.098)***	(0.146)***	(0.074)***	(0.048)	(8.613)***	25,933
Missed Days t=0							yes		

The Table reports the same regressions as in Table 3 where additionally a dummy for missed school days less than 21 and its interaction with a quadratic spline in number of missed school days is included in the model.