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The Effects of Universal Primary Education on Attendance: Evidence from Burkina Faso*

Georges Vivien Houngbonon †

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Abstract

Universal primary education (UPE) programs typically increase the supply of education, but at the expense of quality. Therefore, its effects on school attendance is not necessarily positive. This paper evaluates the impact of PDDEB, Burkina-Faso's UPE program which mainly increases the number of public primary schools by 50 per cent between 2002 and 2006. Using household surveys and a constrained logistic regression, our methodology compares the trend in attendance rates between exposed and non-exposed birth cohorts. We find that the PDDEB significantly increased attendance in first and second grades of primary school, particularly for girls and children living in initially school-deprived areas. However, there was a significant early dropout after the second grade, mainly for girls. These findings suggest that the increase in education supply induced by the PDDEB have been at the expense of lower quality at higher grades.

Keywords: Universal Primary Education, School Attendance, Africa

JEL Classification: I26, I28, O12

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†Paris School of Economics, 48 Boulevard Jourdan, 75014 Paris. Mail: gvivienh@gmail.com

1 Introduction

Development economist and policymakers have long been interested in the effects of educational inputs on schooling by the poor.[1] Yet, there is still very limited evidence about the effect of school infrastructures in particular on schooling. Duflo (2001) provides one of the compelling evidence of the effect of school construction on educational attainment in Indonesia, but much less is known in the African context where institutions and social norms regarding education might differ.

The last decades in Africa have witnessed the implementation of Universal Primary Education (UPE) programs. These programs typically increase the supply of education through school construction, and sometimes reduce the cost of schooling through the abolition of certain school fees. While these policies are predicted to increase school attendance, the reduction in school fees may also limit the ressources available for improving the quality of education. As a result, more parents may find schooling less attractive than the outside opportunity of sending their kids into the labor market. The overall impact of the UPE program is therefore an empirical question. However, to date, very few studies have investigated the impact of these programs on schooling.

This paper takes advantage of PDDEB[2], a UPE program implemented in Burkina-Faso between 2002 and 2006, to assess the effect of school construction on attendance. Between 2002 and 2006, this program increased the number of primary schools by 50 per cent, with a corresponding rise in the education budget share from 7 to 11 per cent.[3] We focus in particular on the effects of this policy on attendance in the first grade of primary school. The evaluation relies on five household surveys conducted in Burkina-Faso with a specific section on education.

The identification consists in comparing the schooling trend between birth cohorts exposed to the program and birth cohorts that did not. Schooling trend is measured by the share of a cohort's members attending a given grade of primary school. Primary schools in Burkina-Faso are open to children between 6 and 12 years old, therefore, cohorts that are more than 12 years old in 2002, the starting year of the program, are not expected to be exposed to its effects. Put it differently, cohorts born

[1] See Birdsall (1985) on the effect of teachers' income on school attainment, Case & Angus (1996) on the effects of school fees in Ghana and Case & Angus (1999) on the impact of school size on educational outcomes.

[2] PDDEB: Programme Décennal de Développement de l'Education au Burkina-Faso.

[3] In Burkina-Faso, the budget share of education increases from 7 percent in 2001 to 11 percent in 2003, according to official figures released in "Tableau de Bord 2010-2011".

before 1990 are expected not to benefit from the program and therefore constitute the control group in the evaluation. We estimate the trend in attendance rate over this group and compare it to the trend observed for cohorts exposed to the program.

We find that the PDDEB increased significantly attendance in first grade of primary school particularly for younger children. This effect is larger for girls; thereby closing significantly the gender gap in school attendance. Moreover, the impact of the program is much larger on children living in areas which were initially deprived. However, there have been dropouts from the second and third grades, particularly for girls.

The findings of this paper contribute to the literature on the determinants of schooling. Duflo (2001) provides evidence of a positive effect of school construction on school attainment in Indonesia, but not in the African context. Deininger (2003) and Nishimura *et al.* (2008) provide some evidence in the African context, particularly in Uganda, but they focus only on the effect of abolition of school fees because the Ugandan UPE program does not involve large school construction as in Burkina-Faso. Harounan *et al.* (2013) recently investigate the effects of school construction on schooling and test scores in Burkina-Faso but the scope of their study was limited to a specific program design to enhance girls enrolment. This paper complement this literature by providing evidence of the impact of a nation-wide school construction program in the context of Africa.

The rest of the paper is organised as follows. Section 2 briefly reviews the literature on the demand for education and the empirical assessment of an UPE program. Section 3 presents the UPE program in Burkina-Faso, while section 4 describes the dataset, provides some descriptive statistics and explains the empirical strategy to identify the impact of the program. Section 5 presents the results along with their robustness checks and section 6 concludes.

2 Related literature

Due to credit constraints and externalities, investment in education can be limited by both demand and supply constraints. Early models of investment in education, whether in static form (Becker, 1962) or dynamic (Ben-Porath, 1967), typically assumes unlimited borrowing and saving opportunities. However, this assumption has been challenged by credit market failures whereby some individuals, mostly the poor, are credit constrained and could not invest in human capital in spite of higher

rates of return (See Jacoby (1994), Jacoby (1997), Edmonds (2006) and Attanasio & Kaufmann (2009)). Moreover, as education is embodied in human being it is difficult to be used as collateral in the credit market. Removing these contraints would alleviate the burden on the demand for education. One way of removing these constraints is through the abolition of school fess.

Empirical studies of the effects of school fees abolition are somehow scarce. Deininger (2003) investigates the impact of fees abolition in Uganda and finds positive effect on attendance, particularly for girls and children living in rural areas. He basically compares the share of children enrolled at primary school before and after the abolition of school fees, controlling for children and households' characteristics. This methodology does not account for the natural upward trend in primary school enrolment as the expected return and the willingness to pay for education increase with economic development. Although he compares the estimates to the one obtained for secondary school enrolment, it is not clear why primary and secondary school enrolment would have followed the same trend absent the abolition of primary school fees. For instance, entry into secondary school is subjected to a test, contrary to entry into primary school.

Nishimura *et al.* (2008) extends the evaluation of Deininger (2003) to include the impact of fees abolition on delayed enrolment, thanks to households data collected 5 years after the implementation of the fees abolition. While their dataset allow the evaluation of the fees abolition over a longer time period, they arrive at the same conclusions as Deininger (2003), that is fees abolition in Uganda raises primary school attendance and reduced delayed enrolment. However, they rely on the same methodology as Deininger (2003) so that their findings are still subjected to the same limitations. This paper overcomes these limitations by estimating the trend in schooling across birth cohorts. Doing so allows us comparing the trend between exposed and non-exposed cohorts.

To the extent that education generate positive externalities, it would be under supplied by private firms. This is why several governments have invested in large public school construction. Duflo (2001) evaluates the long run effects of these supply-side programs in the case of Indonesia and finds that school construction increases the number of years of schooling. Yet, the short run effects of these policies might differ according to the institutional framework, whereas evidence from Africa is still scarce.

Harounan *et al.* (2013) is, to our knowledge, the recent paper that investigates the

effect of school construction in Africa and particularly in Burkina-Faso. However, the scope of their study was limited to a specific school construction program targeted at improving girls schooling. In this paper we adopt a broader perspective by evaluating the impact of a nationwide school construction program.

3 Background on the PDDEB

3.1 Context

Following the adoption of the Millennium Development Goals, the Government of Burkina Faso launched in September 2002 a decennial UPE program labelled PDDEB. This program was introduced amidst a shortage of schools under demographic pressure. Indeed, the years 90s were characterised by substantial economic downturn and social unrest in most of West African countries, especially in Burkina-Faso. The increasing burden of public debt led to the implementation of the so-called "Structural Adjustment Programs" which mainly consist in austerity policies. Under these programs, the government of Burkina-Faso had to cut spendings not only by reducing the number of civil servants through massive layoff, but also by cutting social expenditures in education and health. In addition, investments in infrastructures such as schools, hospitals, roads, electric power, and communication facilities were reduced.

Meanwhile, there was an increasing number of primary school aged children, driven by high fertility rate. The massive layoff combined with the lack of job opportunity does not allow the vast majority of the poor households to send their children to private schools. Other economic shocks such as currency devaluation in 1994 worsened the economic conditions of the households since the country is a net importer of foods. These economic and social events have played a role in decreasing primary school attendance in Burkina-Faso during the 90s (See figure 1). In this context, the Universal Primary Education program has been advocated as a way to increase school attendance and reduce gender gap in line with the Millennium Development Goals.[4]

[4]Target A, Goal n°2.

3.2 Components

The PDDEB was implemented in two rounds throughout the whole country starting from the academic year 2002-2003.[5] Due to lack of data, this paper focuses on the first round which lasted from 2002 to 2006. The intensity of the program varies according to two groups of areas (provinces): a group of 20 provinces, labeled "Provinces Prioritaires" and denoted "PP areas" in the remaining of the paper, received more investment in school construction than a second group made of the remaining provinces. Provinces belonging to the PP areas were selected as the ones with primary school enrolment below the country average. The PDDEB was targeted to both demand and supply constraints on education.

On the demand side, tuition fees were abolished for all first grade students, including repeaters, and they were provided with school supplies for free.[6] In addition, Parents Teacher Association (PTA) fees, which are additional fees paid by the parents on top of tuition fees, were abolished for girls in the "PP areas". Before the PDDEB, tuition and PTA fees were the major financial resources of public schools. Tuition fees are used to pay teachers' salary; while PTA fees mainly serve to repair classrooms and schools. Moreover, school supplies, previously paid by parents were provided by the government free of charge to all first grade students enrolled in both public and private schools.

Table 1 in the appendix shows that schooling fees paid by parents have not changed in 2002. Data on households schooling expenditures are not available after 2002. However, qualitative information obtained from official sources suggest that schooling fees abolition was not effectively implemented before 2007 and more particularly in some remote areas from the capital city Ouagadougou.[7] This can be explained by the fact that the abolition measure was not legally enforced and that school fees were the main source of revenues for schools maintenance costs. Using households survey data, table 2 shows that the share of households not sending their children to school due to high cost of schooling has not decreased until 2006. Figure 2 shows that the number of free books distributed to students doubles from 2004, but it is only from 2007, the second round of the program, that this number rises markedly to reach 4 books per students in 2011.

[5]Each of the phases lasts four years and the program covered the period from 2002 to 2010.

[6]School supplies encompass students' books and other materials used at school. In the first phase, it was mainly made of students' books.

[7]Officials from the Ministry of Basic Education and Literacy as well as from the Bureau of Statistics.

On the supply side, the government undertook the construction of schools and classrooms, along with the recruitment of additional teachers, particularly in "PP areas". Figure 2 shows that the number of schools increased by 50% from 4,700 in 2002 to 6,960 in 2006. This trend is confirmed by the figures in table 2 which show a significant drop in the share of households not sending their children to school due to distance. Moreover, the school construction component was effectively targeted to "PP areas" as shown in table 3. Besides, figures on the average number of classrooms per school by phases indicate that it went from 3.4 during the first phase to 3.7 during the second phase of the program. As a result, one shall expect the program to affect mostly lower grades during its first phase.

Finally, the program was disseminated through large awareness raising campaign conducted by the government and NGOs mainly through radio and television air programs. Although there is no hard evidence of the effectiveness of this component of the program, many local and national newspapers have reported about it. This component, by breaking the cultural norms that might have prevented girls' schooling, is likely to have an effect on their entry into the first grade. Moreover, the government has established a specific Bureau dedicated to the promotion of girls' schooling. Overall, free provision of school supplies school construction are the components of the PDDEB expected to affect schooling decision.

4 Empirical framework

4.1 Datasets

This study uses both administrative as well as household surveys datasets.[8] The administrative data were gathered by the Ministry of Basic Education of Burkina-Faso. These are school-level data that provide comprehensive information about enrolment, number of classrooms and schools, number of students' books provided by the government for free. It covers the whole period of the implementation of the PDDEB, from 2002 to 2010 and the number of students enrolled in the first grade of government schools is available from 1963 to 2010.

The household dataset is made of five repeated cross-section surveys conducted by

[8]I thank Abdouramane Karim Sere from the Ministry of Education of Burkina-Faso for having provided the administrative datasets, and Alexandre Ouedraogo from the statistical office of Burkina and Denis Cogneau from the Paris School of Economics for having provided the household datasets.

the National Bureau of Statistics.[9] The five rounds of surveys correspond respectively to the academic years 1993-1994; 1997-1998; 2002-2003; 2004-2005 and 2006-2007.[10] Altogether, these five surveys provide information on 279,492 individuals and cover the period from 1992 to 2006.

Our sample is made of cohorts of individuals born between 1960 and 2000 (included). It basically excludes under-five children in 2006 as they are still very unlikely to attend school, and individuals born before 1960 in order to avoid statistical noises that may be generated by social unrest before the independence of the country.

The households surveys dataset provides information about individuals' level of education, their year of birth, gender and place of residence. More specifically, each survey provides information on whether they are attending the first grade during the survey or during the previous year of the survey. It also provides information about whether they attended first grade of primary school once in their life. For some birth cohorts, these information are available across all five surveys. It allows comparing the evolution of school attendance throughout the academic years.

The dataset also provides information about the level of education and demographic characteristics of head of households. However, information on grade repetition, birth order, place of birth, and parents' education level are not available, thereby limiting a thorough analysis of the heterogeneous effects of the program. In addition, data on households' expenditures were not collected during the last two rounds in 2005 and 2007. Thus, statistics on the evolution of schooling expenditures could not be computed for these years.

4.2 Descriptive statistics

The outcome variable of interest is attendance in the first grade of primary school. Two types of measurement can be used for this variable. The first is a dummy variable indicating whether an individual is attending the first grade during a sur-

[9]The survey implementation periods are presented as follow:
1st round: from 25th October 1994 to 25th January 1995
2nd round: from 25th April to 3rd July 1998
3rd round: from 10th May to 15th July 2003
4th round: from 15th August to 15th October 2005
5th round: from 19th February to 30th March 2007.

[10]In Burkina-Faso, the academic year typically spans from October of year Y to June of year Y+1, and is denoted Y/Y+1. For a matter of convenience, an academic year will be denoted by its starting year in the rest of this paper. For instance, the academic year 2002-2003 will be referred to as the year 2002.

vey. It has been used to compute the share of children between 5 and 11 years old attending the first grade. This variable could provide biased measurement of attendance because of seasonality. For instance, a survey conducted at the beginning of the academic year may show a large share of individuals attending the first grade ; whereas the same survey conducted at the end of the academic year may provide a lower share of individuals attending the first grade due to early drop-out. This issue can also arise if the survey is conducted during the holidays as in the case of the fourth round survey conducted from the 15th August to the 15th October 2005.

The second type of measurement overcomes this issue by generating a dummy for individuals that have ever attended the first grade of primary school as of a given academic year. Not only is this measurement cleaned from seasonality, but also it extends the measurement of attendance to all individuals in the sample. Econometric estimations rely on this measurement.

The percentage of a cohort's members having attended the first grade can be computed for each survey separately. However, it can be biased as it is not computed from census data. Figure 3 shows that there is a large variation in the estimated shares of entry in the first grade according to the survey. As a result, the estimated percentage of a cohort's members having attended the first grade using a single survey may be fraught with bias. This bias may stem from sampling variation or attrition of cohorts' members due to migration or death (especially for older cohorts). As some birth cohorts have been sampled several times in the different surveys, it may be possible to provide a better estimate of the percentage of a cohort's members having attended the first grade by using a weighted average. Yet, the fact that only some cohorts have been sampled several times could also bias this new estimate. Therefore, the estimation will only rely on the percentage of a cohort's members having attended the first grade estimated using each survey separately.

This variable can be computed for two academic years from a single survey since information about whether an individual was attending school in the previous academic year of the survey is available. Finally, the outcome variable is labelled as "having attended the first grade as of Y", where Y is the academic year. For instance, the 2007 survey provides information on the school attendance status of each individual for the academic years 2006 and 2005, allowing the computation of the outcome variable for both academic years. Though a particular focus is given to the attendance in the first grade, the analysis will be extended to attendance in higher grades, in order to check whether there are some drop-outs after the first grade.

Before going into the econometric estimates, figure 4 provide a descriptive statistic about the evolution of attendance rates across the years of the surveys. It shows a large increase in the percentage of children attending the first grade between 2002 and 2006. Indeed, the percentage of children attending the first grade has increased by 3.3 percentage points over 5 years from 1997 to 2001; while it increases by 6.7 percentage points over the same duration from 2002 to 2006. the difference tends to suggest that the PDDEB has increased first garde attendance.

However, this conclusion can be misleading for two reasons. First, the trend in attendance cannot be effectively estimated as the program might have induced a change in the age of entry into the first grade. The computation of attendance rate relies on children between 5 and 11 years old. However, the program might have reduced delayed enrolment such that older children enrolled from the beginning and only younger children are likely to enrol afterwards. The second reason is that some children are still likely to attend the first grade and there is no way to disentangle their attendance rate from this analysis. Therefore, relying on these conclusions would underestimate the impact the program on the younger children.

As a result, the identification strategy will rather rely on the share of a cohort's members having attended the first grade. A discussion of this identification strategy follows.

4.3 Empirical Strategy

The estimation of the effect of the PDDEB relies on a two-stage constrained logistic regression model. The two stages are motivated by the fact that we distinguish two groups of cohorts: a control group made of individuals that were not exposed to the program and a treated group made of individuals that were exposed. Individuals exposition to the program depends on their age at the beginning of the program in 2002, more generally on their year of birth.

According to figure 5, the share of individuals with more than 14 years old that enter the first garde is almost nil. Therefore, these individuals who made up the cohorts born before 1988 (included) are not exposed to the program. They represent our control group. The younger cohorts, born after 1988, represent the treated group. Figure 5 suggests that the choice of 1988 is not endogenous, because cohorts born before 1988 are not likely to enter the first grade, whether before or after the launch of the program in 2002.

The first stage estimates the shares of entry in the first grade for all cohorts in the control group. This estimation yields a "trend" in the share of entry according to the year of birth. Formally, let's E_i denotes the dummy outcome variable equals 1 if individual i born in year Y_i has attended the first grade as of a given academic year. The first stage equation writes:

$$E_i = \alpha + \sum_{j=1}^{d} \beta_j Y_i^j + \mu_i \tag{1}$$

d is the order of the polynomial that fits the shares of entry with the year of birth. Figure 6 presents alternative polynomials for this fit. It turns out that the cubic polynomial provides a better fit. A linear polynomial is not consistent with the fact that the share of a cohort that could attend the first grade cannot be greater than 1. Quartic and quintic polynomials predicts an explosion or a drop in the share of entry into first grade, which is contradictory. Therefore, the polynomial order d will be set to 3 in the main estimation. We also provide robustness results for a quadratic polynomial.

μ_i corresponds to the residuals of the model.

Equation (1) is estimated using a logistic model on the control group, that is on individuals born before 1988. The logistic model is chosen in order to ensure the internal consistency of the estimates and to be able to generate odds-ratio, easier to interpret.

At the second stage, the estimates of the first stage equation are used to estimate the following constrained equation with a logistic model:

$$E_i = \hat{\alpha} + \sum_{j=1}^{d} \hat{\beta}_j Y_i^j + \sum_{y=1986}^{2000} \delta_y D_{iy} + \varepsilon_i \tag{2}$$

$\hat{\alpha}$ and $\hat{\beta}_j$ are the estimated coefficients from the first stage regression. D_{iy} is a dummy variable taking 1 if the individual i is born in year y; and 0 otherwise. Its coefficient δ_y captures the impact of the program on each cohort born after 1986. Actually cohorts born between 1986 and 1988 are included in this component of the model to serve as a placebo test. There corresponding coefficients are not expected to be significant, under our identification assumption. ε_i corresponds to the residuals. Equation (2) is estimated over the sample of cohorts born between 1960 and 2000.

We investigate the heterogeneous impact of the program with respect to gender, place of residence and grades. More precisely, equations (1) and (2) have been estimated respectively on the whole sample, girls only, individuals living in "PP areas" and for higher grades.

5 Results

This section presents the main findings of the paper and tests their robustness. Given that the econometric models have been estimated using a logistic regression, we only report Odds-Ratios (OR), that is ratios of probabilities that an individual born in a given year is likely to attend the first grade. They give by how much attendance rate, measured as the percentage of a cohort's members having attended the first grade, is above or below its counterfactual estimated from the control group. When the OR is higher than 1 for a given cohort, attendance rate is said to be above its counterfactual.

5.1 Main results

Table 4 presents the estimates of the impact of the PDDEB on first grade attendance, for the whole sample. The availability of households surveys every two years allows estimating the impact of the program throughout the period of implementation, 2002-2006. In addition, our methodology allows the impact evaluation for each birth cohort. In doing so, we present the results according to two groups of cohorts. The first group will be labelled "older cohorts" and comprises cohorts born before 1994, that is individuals that are more than 12 years old in 2006. The second group labelled "younger cohorts" includes cohorts born after 1994, that is individuals that are less than 12 years. The idea being that older cohorts could no longer enter first grade after 2006 and that we are able to observe the full impact of the program on these cohorts. On the contrary, younger cohorts are still likely to enter first grade after 2006 so that we can only estimate a lower bound of the impact of the PDDEB on their entrance into first grade.

We find a significant and robust positive effect of the program on first grade attendance by the older cohorts. In particular, cohorts born between 1990 and 1994, that is children who were 8 to 12 years old at the beginning of the program enter the first grade due to the program. On average, the magnitude of the effect is such

that these children were 1.5 time more likely to attend the first grade than before the program. This magnitude has remained constant between 2003 and 2002, suggesting that they were induced to enter the first garde right after the launching of the program in 2003.[11] Interestingly, odds ratios estimated in 2001 and 2002, before the beginning of the program, for these cohorts are not statistically significant. In other words, their attendance rate was not higher than predicted by the cubic trend before the beginning of the program. This result lends support to the finding that the significant effects observed after 2002 are due to the program. In addition, cohorts born before 1990, and particular those born before 1988 (more than 14 years old in 2002), are not affected by the program, a result which validates the choice of cohorts born before 1988 to build the control group.

The findings on the younger cohorts provide additional support for the positive effect of the program on first grade attendance. Estimates presented in table 4 show that first grade attendance rate is higher for these cohorts than predicted from the control group. In addition, this difference is becoming larger over time. For instance, attendance rate of children born in 1995 was 1.5 time higher in 2003, and became 2.2 times higher in 2006. Figure 7 in the appendix depicts the impact of the program on first grade attendance in 2006.

Table 5 presents the same results, but on the sample of girls. It turns out that the magnitude of the effects of the program is larger for girls, particularly for younger cohorts.[12] Likewise, table 6 shows that the impact of the program is larger for children living in PP areas, where the rate of attendance was initially lower than the country average. Moreover, we assess the effect of the program on attendance in the second and third grades of primary school to check whether there has been dropouts. Table 7 presents the outcomes of the estimation for attendance rates in 2006. It tuns out that attendance rate has increased in the second grade but not in the third grade, particularly for girls. This finding suggests early dropout from primary school, particularly for girls.

5.2 Robustness checks

The main results presented above rely on the definition of the control group and the assumption that attendance rates across cohorts can be modelled as a cubic

[11] The program was launched in 2002, but became effective from the 2003 academic year.

[12] Seemingly unrelated regressions suggest that the coefficients in tables 4 and 5 are statistically different.

polynomial. In this section, we relax these assumptions by changing the birth year threshold that characterises the control group, and by using a quadratic polynomial to estimate attendance rates across birth cohorts. In addition, alternatives mechanisms such as economic recovery may explain the estimated rise in attendance rates. To rule out these potential explanations, we implement a placebo test which evaluates whether there has been a particular increase in attendance in secondary school, a level of education which should not be affected by the program.

Table 8 presents the outcome of the estimation according to the choice of the cutoff year of birth that defines the control group and the type of polynomial. More specifically, the control groups are respectively defined as comprising individuals born before 1986, 1987 and 1988. The positive and significant impact of the program on first grade attendance still persists. However, the cubic polynomial tends to overestimate the impact of the program compared to the quadratic one.

Table 9 presents the results of the placebo test. This test consists in checking whether the increase in first grade attendance rate is driven by a particularly favourable economic conditions. In that case we expect that attendance in secondary school should have increased as well. However, the estimates in this table indicate the contrary. There was no significant rise in attendance in first grade of secondary school for cohorts likely to be affected, that is individuals that were more than 12 years old in 2002. Therefore, the main results could not have been driven by a generally favourable economic conditions.

6 Conclusion

This paper finds that the large school construction induced by the PDDEB has increased first and second grade attendance and reduced delayed enrolment as well as gender and spatial inequalities in education. These findings suggest that the incentives provided by the increase in education supply have not been dampened by a potential fall in quality of education, defined for instance as students to teachers ratio. However, early dropout occurs from the third grade of primary school, particularly for girls, suggesting that quality might have fallen at higher grades. These results accord well with the findings by Duflo (2001) in Indonesia and Harounan *et al.* (2013) in Burkina-Faso.

Our results suggest that increasing the supply of education may be a way to raise school attendance, but careful attention needs to be paid to quality, particularly at higher grades. The welfare loss associated with early dropouts could be attenuated by balancing expenditures in school construction with expenditures in improving the quality of education.

This paper investigates the overall reduced-form effect of the program on attendance. A way forward would be to distinguish between the effect induced by change in the number of schools from the one induced change in quality. Moreover, we would like to investigate how the effect of these types of program depends on labor market conditions as well as their long run effects on wages and other dimensions of human development such as health.

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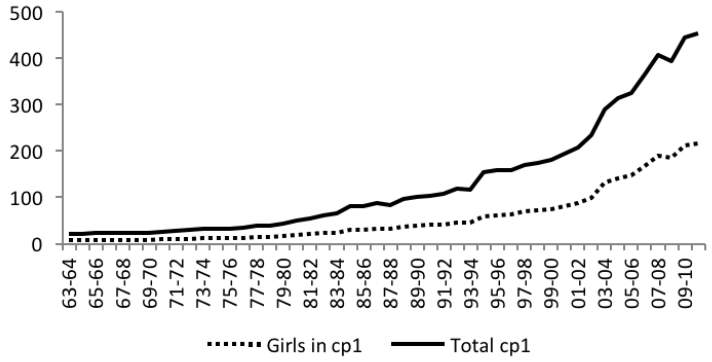
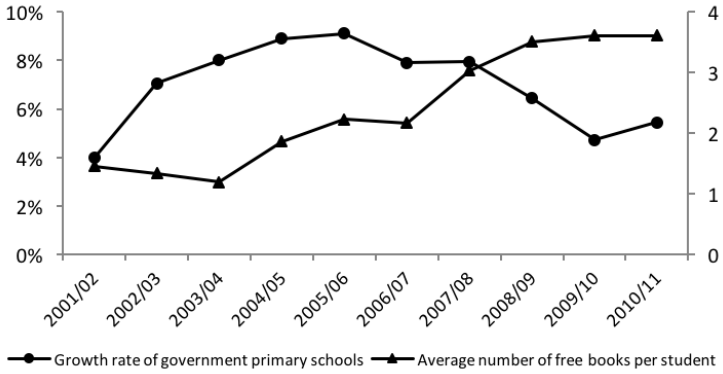
Figure 1: Enrolment in first grade from 1963 to 2010 (Thousands)



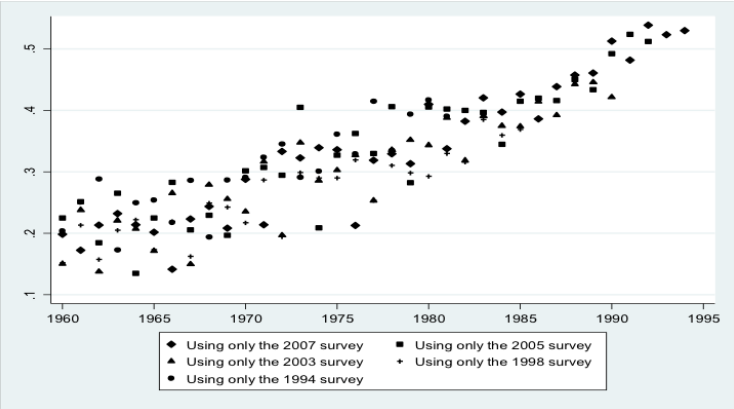
Source: Administrative data from the Ministry of Education (academic years on the x-axis).

Figure 2: School construction and Books distribution



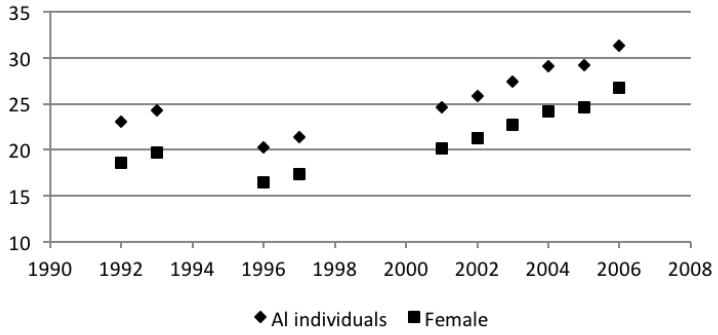
Source: Administrative data from the Ministry of Education (academic years on the x-axis).

Figure 3: Estimated share of cohorts' members having attended the first grade



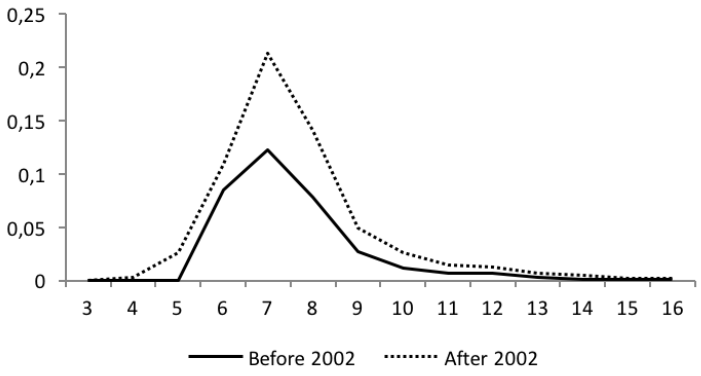
Source: Households Surveys (year of birth on the x-axis).

Figure 4: Percentage of children attending the first grade during an academic year



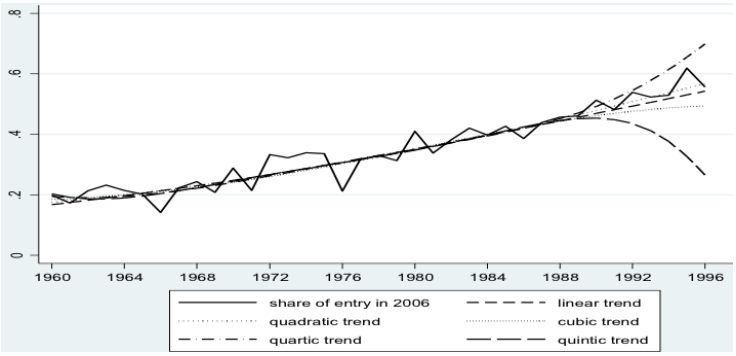
Source: Households Surveys (relevant academic years on the x-axis). Each percentage has been estimated over the sample of children between 5 and 11 years old.

Figure 5: Share of children attending the first grade



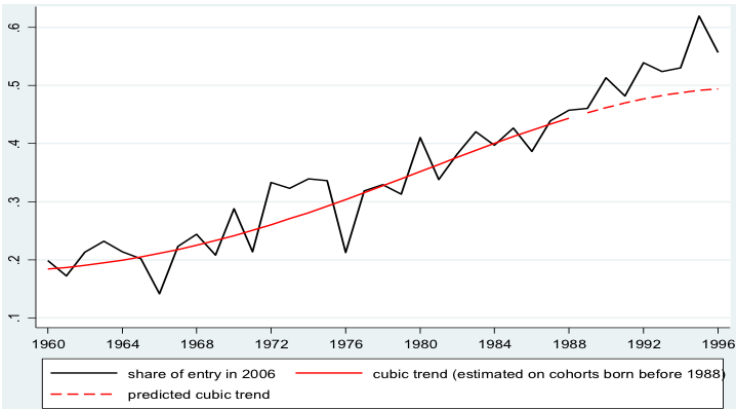
Source: Households Surveys (age on the x-axis). This figure shows that whether before or after 2002, individuals of more than 14 years old are not likely to attend the first grade. This threshold will be used as the cut-off point in the estimation.

Figure 6: Estimated trends in the share of cohorts' members having attended the first grade



Source: Households Surveys (year of birth on the x-axis). Each trend is first estimated on cohorts born before 1988 using a logistic function; and then predicted for the following cohorts. It provides a first insight into the best trend that could fit the data.

Figure 7: Impact of the program in 2006



Source: Households Surveys (year of birth on the x-axis). The predicted trend starts from 1989. The deviation from the trend for cohorts born from 1990 can be associated to the impact of the program.

	Average in 1993	Difference in 1997	Difference in 2002
Nominal expenditures per student			
Tuition in first grade	1727.50	-216.89	388.66
PTA fees in first grade in *«PP areas»*[(1)]	--	1259.09	31.91
School supplies in primary school	3766.61	-76.83	-218.72*
Total	6415.26	-479.85	201.88

Significant at 1%(***), 5%(**) and 10%(*).
(1): PTA fees data were not available in 1994.
Sample: The averages have been computed among households whose children are at most in government's primary schools. The sample excludes those households sending their children to private schools or whose children are attending a higher grade than primary school. The sample size is specific to each computation. The average sample size is 184 households (or individuals).

Table 1: Education expenditures in local currency (CFA francs)

		Variation with respect to the previous year		
Reason for not attending:	Average in 1997	2002	2004	2006
High schooling cost	0.512	0.072***	0.104***	-0.047***
No School/Too Far	0.451	-0.129***	-0.060***	0.018*
Not necessary or Too young/old	0.558	0.019***	0.014**	0.092***
Primary school attendance	0.213	0.036***	0.058***	0.040***

Significant at 1%(***), 5%(**) and 10%(*).
Sample: Individuals from 5 to 17 years old, i.e. eligible to primary school, who are not attending primary school during the survey.
Except the last row on primary school attendance, each row presents the percentage of individuals not attending school because of a specific reason. For the first three rows, the percentage is computed on children from those households who did not find school attendance "unnecessary".
Given that individuals within a household typically have the same reasons for not attending; the number of primary school age children not attending school has been added in a linear probability model to compute the percentages.

Table 2: Checking the effectiveness of the program using the most important reason for not attending school

	2002	2003	2004	2005	2006
In *«PP areas»*	1804	2047 (13.5)	2275 (11.1)	2541 (11.7)	2807 (10.5)
In other Provinces	3224	3380 (4.8)	3634 (7.5)	3904 (7.4)	4153 (6.4)

Percentage increase from the previous year in parentheses.
Administrative data source: Ministry of Basic Education and Literacy. Data are not available before 2002, as the collection of detailed data on schools starts with the program in 2002.

Table 3: Evolution of the number of primary schools (including government and private schools)

	Dependent variable: Having attended the first grade as of :					
	2006	2005	2004	2003	2002	2001
Year of birth	1.008	1.008	1.008	1.008	1.008	1.008
Square of Year of birth	1.003	1.003	1.003	1.003	1.003	1.003
Cubic of year of birth	1.000	1.000	1.000	1.000	1.000	1.000
Born in 1986	0.933	1.091	1.391	1.399	1.410*	1.410*
	(0.0751)	(0.157)	(0.311)	(0.318)	(0.280)	(0.280)
Born in 1987	1.122	1.318*	1.325	1.333	1.243	1.244
	(0.0904)	(0.190)	(0.296)	(0.303)	(0.247)	(0.247)
Born in 1988	1.175**	1.380**	1.479*	1.469*	1.490**	1.490**
	(0.0947)	(0.199)	(0.330)	(0.334)	(0.296)	(0.296)
Born in 1989	1.164*	1.367**	1.352	1.341	1.473*	1.461*
	(0.0937)	(0.197)	(0.302)	(0.305)	(0.292)	(0.290)
Born in 1990	1.411***	1.640***	1.683**	1.659**	1.311	1.288
	(0.114)	(0.236)	(0.376)	(0.377)	(0.260)	(0.256)
Born in 1991	1.230**	1.438**	1.887***	1.856***	1.438*	1.414*
	(0.0991)	(0.207)	(0.422)	(0.422)	(0.285)	(0.281)
Born in 1992	1.536***	1.791***	1.789***	1.697**	1.270	1.211
	(0.124)	(0.258)	(0.400)	(0.386)	(0.252)	(0.241)
Born in 1993	1.447***	1.652***	1.878***	1.795**	1.461*	1.344
	(0.117)	(0.238)	(0.420)	(0.408)	(0.290)	(0.267)
Born in 1994	1.496***	1.681***	1.684**	1.535*	1.313	0.994
	(0.120)	(0.242)	(0.376)	(0.349)	(0.260)	(0.197)
Individuals still likely to enter the first grade after 2006 (Less than 12 years old)						
Born in 1995	2.188***	2.430***	1.798***	1.503*	0.774	0.369***
	(0.176)	(0.350)	(0.402)	(0.341)	(0.154)	(0.0732)
Born in 1996	1.728***	1.874***	1.702**	0.944	0.234***	0.127***
	(0.139)	(0.270)	(0.380)	(0.215)	(0.0464)	(0.0252)
Born in 1997	1.974***	1.956***	0.851	0.414***	0.0474***	0.0330***
	(0.159)	(0.282)	(0.190)	(0.0941)	(0.00941)	(0.00655)
Born in 1998	1.720***	1.409**	0.273***	0.147***	0.0115***	0.00896***
	(0.138)	(0.203)	(0.0611)	(0.0334)	(0.00229)	(0.00178)
Constant	0.217***	0.185***	0.167***	0.166***	0.161***	0.161***
	(0.0175)	(0.0266)	(0.0374)	(0.0378)	(0.0320)	(0.0320)
Observations	30,398	30,398	39,213	39,213	43,237	43,237

Significant at 1%(***); 5%(**); 10%(*). Robust standard errors clustered at the cohort level are in parentheses.

The dependent variables have been computed using the following surveys: 2007 for entry as of 2005 and 2006; 2005 for entry as of 2003 and 2004; and finally 2003 for entry as of 2002 and 2001.

Estimation: Constrained logistic regression with three constraints. Odds-Ratios (OR) are reported. The OR provides the ratio of chance that an individual born in a given year is more likely to enter the first grade than usual. For instance, in the first column of the results, individuals born in 1995 are 2.18 times more likely to attend the first grade than usual as of 2006. When the OR is higher than one, the probability of entry is above the trend; and the reverse holds when the OR is below one.

Placebo coefficients are in the shaded cells.

The cubic trend is estimated over the sample of cohorts born between 1960 and 1987 (more than 15 years old in 2002)

The overall estimation is implemented over the sample of individuals born between 1960 and 2000.

Table 4: Impact of the PDDEB on first grade attendance

	Dependent variable: Having attended the first grade as of :					
	2006	2005	2004	2003	2002	2001
Year of birth	0.982	0.982	0.982	0.982	0.982	0.982
Square of Year of birth	1.006	1.006	1.006	1.006	1.006	1.006
Cubic of year of birth	1.000	1.000	1.000	1.000	1.000	1.000
Born in 1986	0.787**	0.936	1.243	1.252	1.428**	1.428**
	(0.0794)	(0.0745)	(0.192)	(0.198)	(0.201)	(0.201)
Born in 1987	0.843*	1.003	1.341*	1.351*	1.274*	1.274*
	(0.0850)	(0.0798)	(0.207)	(0.214)	(0.179)	(0.179)
Born in 1988	0.965	1.148*	1.415**	1.401**	1.618***	1.618***
	(0.0973)	(0.0913)	(0.218)	(0.222)	(0.228)	(0.228)
Born in 1989	1.147	1.364***	1.537***	1.511***	1.520***	1.520***
	(0.116)	(0.109)	(0.237)	(0.239)	(0.214)	(0.214)
Born in 1990	1.350***	1.590***	1.727***	1.715***	1.394**	1.377**
	(0.136)	(0.126)	(0.266)	(0.271)	(0.196)	(0.194)
Born in 1991	1.299***	1.545***	1.973***	1.961***	1.591***	1.567***
	(0.131)	(0.123)	(0.304)	(0.310)	(0.224)	(0.221)
Born in 1992	1.718***	2.029***	2.000***	1.868***	1.357**	1.323**
	(0.173)	(0.161)	(0.309)	(0.295)	(0.191)	(0.186)
Born in 1993	1.610***	1.867***	2.035***	1.925***	1.496***	1.378**
	(0.162)	(0.149)	(0.314)	(0.304)	(0.211)	(0.194)
Born in 1994	1.672***	1.889***	2.054***	1.908***	1.452***	1.149
	(0.169)	(0.150)	(0.317)	(0.302)	(0.204)	(0.162)
Individuals still likely to enter the first grade after 2006 (Less than 12 years old)						
Born in 1995	2.731***	3.123***	2.476***	2.065***	1.010	0.494***
	(0.276)	(0.248)	(0.382)	(0.327)	(0.142)	(0.0695)
Born in 1996	2.305***	2.504***	2.261***	1.262	0.376***	0.201***
	(0.233)	(0.199)	(0.349)	(0.199)	(0.0529)	(0.0283)
Born in 1997	2.862***	2.922***	1.112	0.549***	0.0781***	0.0527***
	(0.289)	(0.232)	(0.172)	(0.0867)	(0.0110)	(0.00742)
Born in 1998	2.608***	2.151***	0.465***	0.282***	0.0136***	0.00905***
	(0.263)	(0.171)	(0.0717)	(0.0446)	(0.00191)	(0.00127)
Constant	0.157***	0.132***	0.117***	0.116***	0.113***	0.113***
	(0.0158)	(0.0105)	(0.0181)	(0.0184)	(0.0159)	(0.0159)
Observations	15,762	15,762	20,103	20,103	22,253	22,253

Significant at 1%(***); 5%(**); 10%(*). Robust standard errors clustered at the cohort level are in parentheses.

The dependent variables have been computed using the following surveys: 2007 for entry as of 2005 and 2006; 2005 for entry as of 2003 and 2004; and finally 2003 for entry as of 2002 and 2001.

Estimation: Constrained logistic regression with three constraints. Odds-Ratios (OR) are reported. The OR provides the ratio of chance that an individual born in a given year is more likely to having attended the first grade than usual. For instance, in the first column of the results, a boys born in 1995 are 2.73 times more likely to having attended the first grade than usual as of 2006. When the OR is higher than one, the probability of entry is above the trend; and the reverse holds when the OR is below one.

Placebo coefficients are in the shaded cells.

The cubic trend is estimated over the sample of female cohorts born between 1960 and 1987 (more than 15 years old in 2002)

The overall estimation is implemented over the sample of female cohorts born between 1960 and 2000.

Table 5: Impact of the PDDEB on first grade attendance by girls

	Dependent variable: Entry into the first grade as of :					
	2006	2005	2004	2003	2002	2001
Year of birth	0.982	0.982	0.982	0.982	0.982	0.982
Square of Year of birth	1.005	1.005	1.005	1.005	1.005	1.005
Cubic of year of birth	1.000	1.000	1.000	1.000	1.000	1.000
Born in 1986	0.539***	0.677***	1.567***	1.591***	1.770***	1.770***
	(0.0997)	(0.0457)	(0.213)	(0.228)	(0.216)	(0.216)
Born in 1987	0.965	1.252***	1.821***	1.849***	1.710***	1.710***
	(0.178)	(0.0846)	(0.248)	(0.265)	(0.208)	(0.208)
Born in 1988	1.219	1.582***	1.778***	1.698***	2.136***	2.136***
	(0.226)	(0.107)	(0.242)	(0.243)	(0.260)	(0.260)
Born in 1989	1.197	1.554***	1.682***	1.686***	2.620***	2.551***
	(0.222)	(0.105)	(0.229)	(0.242)	(0.319)	(0.311)
Born in 1990	1.233	1.599***	2.542***	2.548***	2.020***	1.911***
	(0.228)	(0.108)	(0.346)	(0.365)	(0.246)	(0.233)
Born in 1991	1.773***	2.300***	3.075***	2.900***	2.108***	1.998***
	(0.328)	(0.155)	(0.418)	(0.415)	(0.257)	(0.243)
Born in 1992	2.127***	2.760***	2.940***	2.721***	2.372***	2.152***
	(0.394)	(0.187)	(0.400)	(0.390)	(0.289)	(0.262)
Born in 1993	2.710***	3.413***	3.014***	2.945***	2.547***	2.135***
	(0.501)	(0.231)	(0.410)	(0.422)	(0.310)	(0.260)
Born in 1994	2.289***	2.780***	2.912***	2.667***	2.210***	1.590***
	(0.423)	(0.188)	(0.396)	(0.382)	(0.269)	(0.194)
Individuals still likely to enter the first grade after 2006 (Less than 12 years old)						
Born in 1995	3.805***	4.594***	3.340***	2.779***	1.793***	0.813*
	(0.704)	(0.311)	(0.454)	(0.398)	(0.218)	(0.0990)
Born in 1996	2.972***	3.541***	3.665***	2.291***	0.460***	0.207***
	(0.550)	(0.239)	(0.499)	(0.328)	(0.0560)	(0.0252)
Born in 1997	3.926***	4.474***	1.848***	0.868	0.153***	0.0569***
	(0.726)	(0.302)	(0.252)	(0.124)	(0.0187)	(0.00694)
Born in 1998	3.868***	3.595***	0.785*	0.292***	0.0248***	
	(0.715)	(0.243)	(0.107)	(0.0418)	(0.00302)	
Constant	0.117***	0.0905***	0.0865***	0.0852***	0.0629***	0.0629***
	(0.0217)	(0.00612)	(0.0118)	(0.0122)	(0.00766)	(0.00766)
Observations	8,632	8,632	11,236	11,236	11,454	10,975

Significant at 1%(***); 5%(**); 10%(*). Robust standard errors clustered at the cohort level are in parentheses.

Estimation: Constrained logistic regression with three constraints. Odds-Ratios (OR) are reported. The OR provides the ratio of chance that an individual born in a given year is more likely to enter the first grade than usual.

The placebo coefficients are in the shaded cells.

The cubic trend is estimated over the sample of female cohorts born between 1960 and 1987 (more than 15 years old in 2002).

The overall estimation is implemented over the sample of individuals living in the PP region and born between 1960 and 2000.

The PP region comprises all school deprived regions in 2002. The school expansion was larger in these regions thanks to the PDDEB. More specifically, they are regions of which more than half of the provinces are eligible as PP areas.

Table 6: Impact of the PDDEB on first grade attendance by children living in PP areas

	Dependent variable: having attended the …					
	1st grade		2nd grade		3rd grade	
	(1)	(2)	(1)	(2)	(1)	(2)
Year of birth	1.008	0.982	0.994	0.987	1.013	1.015
Square of Year of birth	1.003	1.006	1.005	1.005	1.002	1.002
Cubic of year of birth	1.000	1.000	1.000	1.000	1.000	1.000
Born in 1986	0.933	0.787**	1.035	1.030	1.342	1.680
	(0.0751)	(0.0794)	(0.0917)	(0.128)	(0.433)	(0.906)
Born in 1987	1.122	0.843*	1.269***	1.061	1.559	1.589
	(0.0904)	(0.0850)	(0.112)	(0.132)	(0.503)	(0.856)
Born in 1988	1.175**	0.965	1.358***	1.196	1.609	1.691
	(0.0947)	(0.0973)	(0.120)	(0.149)	(0.519)	(0.912)
Born in 1989	1.164*	1.147	1.368***	1.370**	1.543	1.778
	(0.0937)	(0.116)	(0.121)	(0.171)	(0.498)	(0.958)
Born in 1990	1.411***	1.350***	1.680***	1.608***	1.766*	1.934
	(0.114)	(0.136)	(0.149)	(0.200)	(0.570)	(1.043)
Born in 1991	1.230**	1.299***	1.535***	1.565***	1.508	1.707
	(0.0991)	(0.131)	(0.136)	(0.195)	(0.487)	(0.920)
Born in 1992	1.536***	1.718***	2.001***	1.957***	1.780*	1.863
	(0.124)	(0.173)	(0.177)	(0.244)	(0.574)	(1.004)
Born in 1993	1.447***	1.610***	1.904***	1.709***	1.511	1.440
	(0.117)	(0.162)	(0.169)	(0.213)	(0.488)	(0.776)
Born in 1994	1.496***	1.672***	2.052***	1.739***	1.426	1.229
	(0.120)	(0.169)	(0.182)	(0.217)	(0.460)	(0.662)
Born in 1995	2.188***	2.731***	3.106***	2.768***	1.780*	1.563
	(0.176)	(0.276)	(0.275)	(0.345)	(0.575)	(0.843)
Born in 1996	1.728***	2.305***	2.565***	2.110***	1.160	0.918
	(0.139)	(0.233)	(0.227)	(0.263)	(0.374)	(0.495)
Born in 1997	1.974***	2.862***	2.867***	2.433***	0.803	0.630
	(0.159)	(0.289)	(0.254)	(0.303)	(0.259)	(0.339)
Born in 1998	1.720***	2.608***	2.118***	1.642***	0.334***	0.219***
	(0.138)	(0.263)	(0.188)	(0.205)	(0.108)	(0.118)
Constant	0.217***	0.157***	0.202***	0.115***	0.128***	0.0552***
	(0.0175)	(0.0158)	(0.0179)	(0.0144)	(0.0412)	(0.0298)
Observations	30,398	15,762	30,307	15,723	30,307	15,723

The overall estimation is implemented over the sample of: (1) all individuals and (2) Only girls born between 1960 and 2000.

Significant at 1%(***); 5%(**); 10%(*). Robust standard errors clustered at the cohort level are in parentheses.

Estimation: Constrained logistic regression with three constraints. Odds-Ratios (OR) are reported. The OR provides the ratio of chance that an individual born in a given year is more likely to having attended the first grade than usual.

Figures in the shaded cells are the point estimates on cohorts still likely to attend the corresponding grade.

Table 7: Impact on higher grades attendance as of 2006

	Cubic trend			Quadratic trend		
	Dependent variable: Having attended the first grade as of 2006					
	(1)	(2)	(3)	(1)	(2)	(3)
Year of birth	0.999	1.008	1.017	1.044	1.043	1.041
Square of Year of birth	1.004	1.003	1.002	1.000	1.000	1.000
Cubic of year of birth	1.000	1.000	1.000			
Born in 1986	0.877*	0.933	0.993	1.114	1.115	1.117
	(0.0653)	(0.0751)	(0.114)	(0.193)	(0.198)	(0.205)
Born in 1987	1.069	1.122	1.181	1.313	1.313	1.314
	(0.0797)	(0.0904)	(0.136)	(0.227)	(0.233)	(0.240)
Born in 1988	1.138*	1.175**	1.221*	1.341*	1.339*	1.337
	(0.0848)	(0.0947)	(0.140)	(0.232)	(0.238)	(0.245)
Born in 1989	1.148*	1.164*	1.191	1.288	1.285	1.280
	(0.0855)	(0.0937)	(0.137)	(0.223)	(0.228)	(0.234)
Born in 1990	1.422***	1.411***	1.419***	1.506**	1.500**	1.492**
	(0.106)	(0.114)	(0.163)	(0.261)	(0.266)	(0.273)
Born in 1991	1.270***	1.230**	1.214*	1.259	1.252	1.243
	(0.0946)	(0.0991)	(0.139)	(0.218)	(0.222)	(0.227)
Born in 1992	1.629***	1.536***	1.482***	1.497**	1.486**	1.472**
	(0.121)	(0.124)	(0.170)	(0.259)	(0.264)	(0.269)
Born in 1993	1.581***	1.447***	1.362***	1.334*	1.321	1.306
	(0.118)	(0.117)	(0.156)	(0.231)	(0.235)	(0.239)
Born in 1994	1.691***	1.496***	1.370***	1.295	1.281	1.263
	(0.126)	(0.120)	(0.157)	(0.224)	(0.227)	(0.231)
Individuals still likely to enter the first grade after 2006 (Less than 12 years old)						
Born in 1995	2.566***	2.188***	1.943***	1.764***	1.742***	1.713***
	(0.191)	(0.176)	(0.223)	(0.306)	(0.309)	(0.314)
Born in 1996	2.110***	1.728***	1.484***	1.288	1.269	1.245
	(0.157)	(0.139)	(0.170)	(0.223)	(0.225)	(0.228)
Born in 1997	2.520***	1.974***	1.634***	1.349*	1.326	1.297
	(0.188)	(0.159)	(0.188)	(0.234)	(0.235)	(0.237)
Born in 1998	2.303***	1.720***	1.368***	1.067	1.047	1.021
	(0.172)	(0.138)	(0.157)	(0.185)	(0.186)	(0.187)
Constant	0.243***	0.217***	0.195***	0.164***	0.163***	0.162***
	(0.0181)	(0.0175)	(0.0224)	(0.0284)	(0.0290)	(0.0297)
Observations	30,398	30,398	30,398	30,398	30,398	30,398

Significant at 1%(***); 5%(**); 10%(*). Robust standard errors clustered at the cohort level are in parentheses.

Estimation: Constrained logistic regression with three constraints. Odds-Ratios (OR) are reported. The OR provides the ratio of chance that an individual born in a given year is more likely to enter the first grade than usual. For instance, in the first column of the results, individuals born in 1995 are 2.56 times more likely to enter the first grade than usual as of 2006.

The placebo coefficients are in the shaded cells.

Sample of estimation: Individuals born between 1960 and 2000.

Sample of estimation of the trend: Individuals born between 1960 and (1): 1986; (2): 1987; and (3): 1988. This corresponds to individuals with more than 16, 15 and 14 years old in 2002 respectively.

Table 8: Robustness according to the order of the polynomial

Dependent variable: Having attended the first grade of secondary school as of 2006	
Year of birth	1.059
Square of Year of birth	1.000
Cubic of year of birth	1.000
Born in 1982	1.100
	(0.0717)
Born in 1983	1.175**
	(0.0765)
Born in 1984	0.970
	(0.0632)
Born in 1985	0.961
	(0.0626)
Born in 1986	0.801***
	(0.0522)
Born in 1987	1.019
	(0.0664)
Born in 1988	1.060
	(0.0690)
Individuals still likely to enter the first grade after 2006 (Less than 18 years old)	
Born in 1989	1.019
	(0.0664)
Born in 1990	0.958
	(0.0624)
Constant	0.0891***
	(0.00580)
Observations	18,015
Significant at 1%(***); 5%(**); 10%(*). Robust standard errors clustered at the cohort level are in parentheses.	
Estimation: Constrained logistic regression with three constraints. Odds-Ratios are reported.	
Sample of estimation: Individuals born between 1960 and 1990.	
Sample of estimation of the trend: Individuals born between 1960 and 1984.	

Table 9: Placebo test using the attendance in the first grade of secondary school