

Longer school days do not imply better outcomes.

A natural experiment on the educational, occupational and income effects of lengthening primary school schedules

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1. Introduction

In 1971, longer school days were decreed for around a half of the primary schools of the city of Buenos Aires, Argentina. The policy comprised all the neighborhoods of the city and the schools were pretty randomly chosen to apply the new policy. As a consequence, very infrequent conditions of a natural experiment were created. In 2006 and 2007, we interviewed a sample of near 400 people of the 1971 cohort, thirty years after their 1977 graduation in schools with and without longer school days. We analyzed their educational, occupational and income outcomes, trying to identify the role of longer school days on them.

2. The policy and its context

2.1. The educational system in Argentina and Buenos Aires in 1970. Since the end of the XIX century the Argentine educational system has been traditionally governed by the principles of free and universal access, laity in the public schools and, up to the late seventies, seven years of compulsory primary education¹. Although constitutionally in the hands of the provinces, until the late seventies and early eighties, the federal state continued running primary schools in most of the provinces. The private sector –confessional or lay- was also authorized to run primary and secondary schools. The case of the city of Buenos Aires was peculiar. As the capital of Argentina, its administration was in the hands of the federal government, and the same happened with their schools. Enrollment rates in Argentina have been traditionally high when compared to other Latin American countries, and much higher in the city of Buenos Aires than in the rest of the country. At the federal level, enrollment rates for primary, secondary and tertiary education were at the end of the seventies of 93.9%, 63.3% and 18.9%, respectively.

2.2. The policy of lengthening school days in the city of Buenos Aires in 1970. The policy consisted in the introduction of a double shift (DS) or full time schooling² in the primary schools of the city of Buenos Aires, the capital of Argentina. It began very gradually, in 1960, and was drastically extended to almost fifty percent of the schools in the late sixties and early seventies. The

¹ At the end of the seventies also the first year of pre-school education was decreed as compulsory, and in the early nineties the compulsory education was extended up to the tenth year.

² The traditional length of primary schools' schedule in Argentina has been between four and four and a half hours, either in the mornings (more common) or in the afternoons. This system is known as simple schedule or day (*jornada simple*). Accordingly, the new system was named full schedule or day (*jornada completa*) or double shift (*doble jornada*). The length of the school day in the new system has been near 8:30 hours, including around 2 hours for lunch.

new curriculum was fully put in place in 1971³. For school administration purposes the city of Buenos Aires is divided into twenty-one School Districts (SD). Even when middle classes predominate in the city, there are important socio-economic differences among both, the quarters and the SD. The policy was originally conceived to achieve both, educational and social purposes (CNE, 1968 and 1971) and was evenly applied in all the SD, in such a way that in the early seventies the proportion of DS primary schools in every SD was near 50% of the total.

From the social point of view, the idea was to give an answer to the uneven consequences of the increasing women participation in the labor force. While richer households were able to pay for nurses or other domestic help, the poorer were not. For that reason, the first DS schools were organized in poorer SD. This policy was changed in the late sixties, when the educational purposes began to dominate over the social ones. This change was clearly shown in the parallel change of emphasis of the admission criteria. In 1968, it was mandated to give preference (1) to the familiar, social and economic needs of the candidates; (2) to the proximity of the address of the student to that of the school and, (3) to the students with sisters or brothers in the school. In 1971, on the contrary, it was clearly established that the main and unavoidable condition to be admitted was to live in the surroundings of the school. Only after the fulfillment of this requisite, and in order to give the final admission, the school should consider the following additional social criteria: (1) the household's socioeconomic precariousness; (2) both parents working and no domestic help and, (3) the number of siblings. The 1971 reform of the admission criteria is important from the point of view of the selection bias of our experiment. Additionally, it is very well known that the address and the siblings-in-the-school criteria have been the predominant ones in the city of Buenos Aires since time out of mind.

Academic contents were very precisely defined for the new DS schools. The additional time was assigned to three different kinds of activities: a) reinforcement of the academic contents already in place, particularly language and mathematics; (b) foreign language, typically English and, (c) a very varied set of activities, mainly craftsmanship. Although originally minded to teach to the children abilities useful for the labor market, many of them looked old-fashioned from the very beginning.

3. Longer school days, enduring effects of education and natural experiments: a review of the literature

As far as the authors know there are no previous research done with the two main purposes and methods of this paper, i.e., to assess a natural experiment of the enduring effects of longer school days on educational, employment and income achievements. So we review the literature on three different issues: the effects of instructional time on educational outcomes; the enduring, lifetime effects of education and, finally, natural experiments performed on education. It is a bit surprising that the big majority of the literature on instructional time belong to the sixties, the seventies or the eighties of the last century as if the discussion about the effectiveness of the instructional time had disappeared

³ See Ministerio de Cultura y Educación (1970).

since then. However, we note at the end of the revision a recent revival of instructional time as an educational policy.

3.1. Longer school days (allocated/scheduled time) and educational outcomes. This approach has a long tradition in educational sciences. Up to the middle of the last century it was mostly inspired in the behaviorist and even the Taylorist schools of thought (CIPPEC, 2006). Carroll (1963) was perhaps the first to adopt a more pedagogically oriented approach. Most of the literature since him have agreed that increasing the allocation of instructional time has positive but small impacts on educational achievements, and that these impacts tend to be higher the lower is the countries' GDP and the students' socio-economic status (SES). One of the oldest and most comprehensive revisions of this literature is Cotton (1989). She distinguishes different varieties of allocated time: school time (number of school days and hours per day); classroom time (hours spent at classrooms); instructional time (the portion of classroom time spent teaching students particular knowledge, concepts or skills); engaged time or time-on-task (portions of time during which students are paying attention to a learning task and attempting to learn); and academic learning time (ALT) ⁴. Cotton emphasizes the importance of keeping in mind this taxonomy, because comparisons such as the ratio of school time to instructional time and the ratio of classroom time to time-on-task have produced shocking findings. Only about a half of the typical school day has been actually used for instruction and the students have been engaged in learning activities only about half of their in-class time⁵. Cotton's concludes⁶ that there are (a), a small positive relationship between allocated time and students' achievements; (b) a stronger, but still small relationship between time-on-task and achievements; (c) a strong and positive association between ALT and students' achievements and attitudes; (d) greater achievements and enhanced attitudes when time-on-task is interactive with the teacher; (e) more beneficial increases in allocated or engaged time to lower-ability students, while higher-ability students benefits very slightly, if at all, from those increases; (f) more beneficial increases of time-on-task in highly structured subjects, such as mathematics and foreign languages.

Writing only one year after Cotton, Berliner (1990) also begins his revision reminding the multi-dimensionality of instructional time and emphasizing that the popularity of research on allocated or scheduled time is just a consequence of the easiness to measure it⁷. Contrary to the majority of the literature⁸, Berliner

⁴ ALT refers to that portion of engaged time that students spend working on tasks at an appropriate level of difficulty for them and experiencing high levels of success.

⁵ The rest of the time was typically expended in classroom procedural matters, transitions, disciplinary matters, dead time or off-task activities. After reviewing McMeekin (1993), Thrupp (1998) and Martinic (2002), CIPPEC arrives to the same conclusions regarding the scarcity of effective classroom time, adding that the problem is more serious in regions like Latin America and in poor socio-economic environments.

⁶ Cotton (1989) revised 57 research studies -mainly from developed countries- concerned with the relationship between the educational time factors cited above and the student outcomes of achievement and attitudes. Twenty-nine are primary sources and 28 secondary (reviews, syntheses, and meta-analyses).

⁷ He also points out that most of the contemporary arguments on the role of allocated instructional time on students' achievements were an outgrowth of the Coleman report (Coleman et al., 1966), with its skeptical view regarding the impact on educational outcomes of increased school resources of almost any kind, including time.

⁸ In a shorter revision, Pittman et al. (1986) had coincided with Berliner's first conclusion.

concludes that (a), despite the difficulties of working with a "molar" variable like raw instructional time in developed countries, the effect of quantity of instruction on achievement is clear and of great relevance to policy debates about education and, (b) the effects of quantity and quality of schooling are much clearer in lesser developed countries. He adds that Hyman, Wright and Reed (1975) is one of the few studies on the enduring effects of quantity of schooling on the overall quality of the life one leads. Sixteen years after Berliner, Bellei (2006) concludes from his revision⁹ that most of the American studies on the subject coincided (a), on the existence of a positive and statistically significant relationship between instructional time and academic achievements of the students; (b) the modes size of that relationship; (c) that it is stronger for students with initially low academic achievements; (d) tends to be curvilinear, showing diminishing returns to scale to the increase in instructional time. Bellei argues that the methodological limitations of most of most of the reviewed are huge and come from the small and non-randomly selected samples; the limited range of the independent variable; the cross-sectional nature of most of the studies; the short periods of time involved, generally less than one year and, finally and perhaps the most important, that is not clear to what extent the reviewed studies controlled for other factor that could bias the estimates. Finally, Fuller and Clarke (1994), quoted by CIPPEC (2006), reviewed literature specifically referred to developing countries and conclude that the effect of instructional time on educational outcomes were stronger there. CIPPEC (2006) did not find many studies on Latin America, but all of them coincide with the results obtained by Fuller and Clarke (1994)¹⁰.

School term's length. A close family of studies has analyzed the effects of the length of the school year on educational, labor market and income outcomes. In his own study, Bellei (2006) evaluates with a natural experiment methodology the Chilean "Full School Day-Program", designed to increase the yearly high-school instructional time from 955 to 1,216 hours. Every year, an additional group of high-schools has been integrated into the program, thereby "potentially establishing a natural experiment". The selection of the schools was not randomly, but decided by the government according to some criteria. Bellei uses a differences-in-differences approach and argues that it provides an unbiased estimate of the causal effect of the program on students' academic achievement, as measured by standardized tests. His main findings indicate that the program had positive effects on students' achievements, both in language (between 0.05 and 0.07 standard deviations) and math (around 0.07 sd¹¹), and stronger in rural and municipal schools than in urban and private schools. Marcotte (2005) also performed a natural experiment on the effects of instructional time on Maryland's primary schools test scores. He found that natural variation in snowfall over time, which affects the number of effective school days, was significantly -although very feebly- related to how well students within a school fared on the exams¹². According to Pischke (2007),

⁹ In addition to other already quoted papers he revises Jencks et al. (1972); Bloom (1976); Wiley (1976); Borg (1980); Fisher et al. (1980); Frederick and Walberg (1980); Karweit and Slavin (1981); Brown and Saks (1986, 1987) and Link and Mulligan (1986).

¹⁰ Cardoso (Uruguay, n/d); Cervini (Argentina, 2001); Ministerio de Educaci3n de Chile (2003); ANEP (Uruguay, 2003) and Bellei (Chile, 2006).

¹¹ Standard deviation.

¹² However, the differences found were very small, only 1 to 2 percent fewer students tested in harsh winters performed satisfactorily in math than did students examined after mild winters.

most of the studies on the length of the school term, including his own paper, do not find important and significant effects on future earnings, although it has positive and significant impacts on educational outcomes like avoiding grade repetition.

3.2. Enduring effects of education. The classical reference here is Hyman, Wright and Reed (1975), who analyzed responses to knowledge questions in public opinion surveys between 1947 and 1974¹³. Based on the fact that the higher the respondents' level of educational attainment, the more often were correct the responses given, the authors concluded that "education produces large, pervasive, and enduring effects on knowledge and receptivity to knowledge" (Hyman et al., 1975: 109). This effect of education was successfully controlled by sex, religion, ethnicity, geographical origin, age, socioeconomic background, and current occupational status. Wolfle (1980) emphasizes, however, that the study's principal weakness was the authors' inability to control for early intelligence or propensity to learn, in contrast to a vast literature that have demonstrated that intelligence has a strong effect upon socioeconomic achievements and that it is likely that analyses of educational effects which do not include IQ variables suffer severe, although unknown, specification errors¹⁴. Using a causal model of the enduring effects of education, including the estimated effects of intelligence measures, he concludes that previous studies have seriously overestimated the enduring effects of education¹⁵. A very important point added by Wolfle is that education does increase general intelligence, in such a way that the indirect effect of education on vocabulary through adult IQ is five times the size of the direct effect¹⁶.

More recently, in the age of the methodologies of instrumental variables and natural experiments, Duflo (2001) studied the educational and labor market outcomes of the construction of 61.000 primary schools in a very short period (1974-1978), in Indonesia. Measuring the effects twenty years after the program, in 1995, she found that, (a) it increased 0.25 to 0.40 the average years of education; (b) it improved by 12 percent the probability that an affected child would complete primary school and, (c) it raised wages ranging from 3% to 5.4%¹⁷. Combining both effects she estimated economic returns to education ranging from 6.8% to 10.6%¹⁸. She warns about the risks of generalizing her results to other contexts because a number of factors like the strong emphasis

¹³ In these surveys, people of different ages and educational attainment were polled on their knowledge of a wide variety of issues, from identifying prominent public figures to responding to questions on vocabulary.

¹⁴ His point is very relevant because it is very uncommon nowadays to include intelligence measures in studies of the determinants of educational outcomes. Meghir and Palme (2003, 2004) are some of the few exceptions.

¹⁵ However, he recognizes that his results are conditional until confirmed by longitudinal studies in which intelligence scores are obtained for a representative sample of children, and their subsequent levels of educational, intellectual, and verbal achievements are measured.

¹⁶ "This is the interpretation I would like to draw-that education's primary effect is a generalized development of adult cognitive skills, not necessarily the retention of specific bits of knowledge" (Wolfle, 1980, p.113).

¹⁷ She thinks that the increase in wages she found proves that there was a combined effect of quality and quantity changes in education leading to an increase in human capital.

¹⁸ She also argues that her 2SLS estimates are similar to OLS estimates and also similar to most estimates reported for developed countries, but smaller than estimates reported in Psacharopoulos (1994) for developing economies.

on education in Indonesia at the time of the program; the possibility of general equilibrium effects of the program on the returns to education¹⁹; and the fact that the program induced variation only at the primary school level, while returns to secondary education may be different. Additionally, individuals whose education level changed because of the program may experience returns to education that differ from the population average because, for instance, only individuals with high expected returns answered quickly to the enrollment opportunities open with the program. Duflo recognizes that the program increased the *quantity* of education and that it is sometimes feared that deterioration in the quality of education might result from this type of program, offsetting any gain in quantity. Also based on a natural experiment, Meghir and Palme (2003 and 2004) evaluated, around forty years later, the impact on educational attainment and earnings of a major school reform that took place in the 1950s in Sweden. The reform had many common elements with those in other European countries at that time, and included an increase in the years of compulsory schooling, a new national curriculum and the abolition of selection by ability into academic and non-academic streams at the age of 12. The authors found that the reform increased both the educational attainment and the earnings of those whose fathers had just compulsory education. However, the earnings of those with more educated parents declined - possibly because of a dilution of quality at the top end of the education levels. Although this study is a benchmark in the research of long lasting effects of education it seems to have some weaknesses. First, it was not possible to separate the effects of the increase in the quantity of education- years of schooling- from the *qualitative* ones -like the new curriculum or the elimination of selection by ability at 12. Second, the effects, even when positive and significant, were very small as well (see Table 1)²⁰.

3.3. *Natural and randomized experiments in education.* It is very welcome this century's blossoming of a new family of natural and random experiments on the educational and labor market effects of different educational policies. It has renewed the hopes of a better understanding of this very relevant question, after the disappointing results of the vast educational production function research program that followed the challenge posed by Coleman et al. (1966)²¹. However, although very probably more accurate than the previous research program, the most important common trait of this new vintage is that the effects of the measured educational policies on educational outcomes are modest, perhaps very modest, as can be seen in Table 1²². Unfortunately, only a few of them have studied the enduring effects of educational policies.

¹⁹ Given the fact that measuring the returns twenty years after the program, in an environment where the education levels were higher than when the program began, individuals' returns may be lower than they would be in other developing countries.

²⁰ As a benchmark for the magnitude of these effects, Anders Björklund (2000) estimated the wage premium per additional year of education to be 4.6 percent for Sweden (Meghir and Palme, 2004).

²¹ Two relevant contributions to get a balance of the educational production function research program are Akerlof and Kranton (2002) and Glewwe (2002).

²² Piketty argues that the class size is perhaps the most clear at the time of assessing the superiority of natural experiments.

Table 1. Compared results of natural (N) and random (R) experiments on outcomes of educational policies. Primary (P) and Secondary (S) education

Authors (date)	Independent Variable (s) Duration of the experiment	Results
A. Returns to education		
1. Duflo (2001, N, Primary (P) and Secondary (S)).	Program of school construction in Indonesia. Duration: 20 years.	6.8% to 10.6 % for primary education and 6.0% to 11.6% for secondary education.
2. Meghir and Palme (2003, 2004, N, S).	Sweden vast school reform (1950s): increase in the years of compulsory schooling; new national curriculum; abolition of selection by ability at the age of 12. Duration: around 30 years.	If all the changes in earnings were due to changes in the quantity of education, the results imply returns of 6.0% for low ability individuals, 11.6% for those with high abilities and 8.4% overall. If other variables played a role those returns are upper bounds.
B. Wages		
3. Duflo (2001, N, P&S).	Idem 1.	3% to 5.4% increase in wages.
4. Krueger (1999, R, P).	Reduction of class size (Tennessee STAR Project). Duration: 4 years.	The 0.22 sd improvement in test scores resulting from smaller class sizes imply an improvement of 1.7% and 2.4% average male and female earnings, respectively.
5. Meghir and Palme (2003, 2004, N, S).	Idem 2.	Overall, 1.42% increase in earnings. For those with unskilled fathers the reform increased earnings by an also very small 3.4%.
C. School completion / repetition		
6. Duflo (2001, N, P&S).	Idem 1.	12% increase in the probability of primary school completion.
7. Goux and Maurin (2007, N, S).	Neighbors' peer effects. France. Duration: cross-section, educational performance up to lower secondary.	1) Adolescents living in neighborhoods with a higher proportion of peers born at the beginning of the year have a higher probability of repetition by about 0.13 and 0.16 sd. 2) Adolescent's educational advancement negatively influenced by the proportion of non-educated families living in the neighborhood.
D. Years of education		
8. Duflo (2001, N, P&S).	Idem. 1.	Increase of 0.25 to 0.4 of a year.
9. Meghir and Palme (2003, 2004, N, S).	Idem. 2.	Increase by 0.298 of a year, entirely due to the increase in the educational attainment of those with unskilled fathers.
E. Standardized test scores		
10. Krueger (1999, R, P).	Idem 4.	Performance of students in smaller classes increased by 4 percentile points the first year and by 1 percentile point per year in subsequent years. Test scores in smaller classes rose by about 0.22 sd. Class size had a larger effect for minority students.
11. Banerjee et al. (2005, R, P).	Additional teaching support to lagging children in India.	0.14 / 0.28 sd increase in test scores compared to non-treated peers. One year after the program, initial gains faded to

	Duration: 2 years.	about 0.10 sd.
12. Banerjee et al. (2005, R, P).	Computer-assisted learning program: 2 hours per week of shared computer time. India. Duration: 2 (and more) years.	0.35 and 0.47 sd increase in math scores in the first and second year of the program, respectively. After that, the increase tends to fall.
13. Marcotte (2005, N, P&S).	Longer school year as measured by harshness of winters. Maryland (USA). Duration: cross-section, same year.	Only between 1 and 2 percent fewer students performed satisfactorily after harsh winters than did students examined after mild winters.
14. Hoxby and Rockoff (2005, N, P).	Comparison between "lotteried-out" and "lotteried-in" students in the applications to charter schools in Chicago (USA). Duration: 2 years (average)	Compared to their lotteried-out fellow applicants, students who apply to and attend charter schools an average of two years starting in the elementary grades score about 6 national percentile rank points higher, both math and reading.
15. Bellei (2006, N, S).	"Full-School-Day Program" in Chile: increase from 955 to 1216 hours per year in secondary schools. Duration: 2 years.	The program had a positive effect on students' achievement, both in language (between 0.05 and 0.07 sd) and math (around 0.07 sd). Stronger effects were attained in rural and municipal schools than in urban and private schools.
14. Berlinsky et al. (2006, N, P).	Preprimary schooling. Argentina, as measured by a vast preprimary classrooms construction program. Argentina. Duration: 4/5 years.	One year of preprimary school increases average third grade test scores by 8 percent of a mean or by .23 of the sd of the distribution of test scores. Preprimary school attendance positively affects student's self-control in the third grade as measured by behaviors such as attention, effort, class participation, and discipline.
16. Piketty and Valdenaire (2006, N, P&S).	Class size exogenously determined by the policy of putting a ceiling of 30 students per classroom. Duration: 6 years.	The reduction of one pupil per primary class allows an increase in the range of 0.3-0.4 points in math's test scores (and 0.7 in less socio-economically endowed contexts). Results for secondary schools are weaker, with increases of only 0.2 in test scores in lower secondary and only 0.05 in higher secondary schools.
17. Glewwe et al. (2007, R, P).	Random provision of textbooks to primary schools in Kenya. Duration: 4 years.	No increase in test scores, contrary to the results of the previous literature. Textbooks increased scores for students with high initial academic achievement. Students with weaker academic backgrounds did not benefit from the textbooks. Many of them could not read the textbooks, which are written in English, most students' third language.

A brief summary of the results shown the Table is as follows. Independent variables with positive effects on educational, labor and/or income outcomes are schools construction (Berlinsky, Duflo); the social structure of the neighborhood where the students live (Goux); more years or days of schooling (Meghir and Palme; Marcotte); and class-size or exposure to teachers intensity (Banerjee et al.). Regarding the dependent variables, the obtained rate of return

is similar to that found in previous studies; effects on wages are small; years of education increase by less than half year and the effects on scores of standardized tests are weak. Some of the studies, as Banerjee et al. and Hoxby and Rockoff, emphasize the importance of quality over “quantity” of education.

3.4. Recent revival of instructional time as an educational policy. The already quoted, recent works of Meghir and Palme (2003 and 2004), Banerjee et al. (2005), Bellei (2006) and CIPECC (2006)²³ reveal a sort of revival of the instructional time as an educational policy, perhaps due to the fact that it is a strategy relatively simple to implement. However, impacts of additional instructional time seem up to now really modest. Still seems true what Cotton (1989) pointed out: "Significant increases in the quantity of schooling would be required to bring about even modest increases in achievement. The costs associated with extending the school days or years are, therefore, not justifiable".

4. The design of the experiment and the database

4.1. General approach. We used the kind of natural experiment normally applied to observational studies. As it is well known, in this methodological framework, the estimation of the effects of the treatment may be biased because of the existence of confounding factors, and the comparison of sample means among groups is not optimal. To avoid this problem, we use the propensity score²⁴ matching to reduce the bias in the estimation of the outcomes, i.e., comparing the outcomes using treated and control subjects who are as similar as possible (Becker and Ichino, 2002). Since this method is not sufficient to estimate the Average Effect of Treatment on the Treated (ATT), we used the Kernel matching approach, in which all units of the treatment group are matched with a weighted average of all units of the control group, with weights that are inversely proportional to the distance between the propensity scores of treated and controls.

4.2. Data base and sample. The database comes from an ad hoc survey applied to the 1971 cohort, whose selection is methodologically very relevant. Since it was the first cohort to attend the primary schools of the city of Buenos Aires after the generalization of the DS policy, selection bias problems are minimized. The survey included items such as educational attainment at all levels, information about subjects' parents, current SES and labor status. The questionnaire included both, closed and open questions. To design the sample (Table 2), two variables were taken into account: the UBN (unsatisfied basic needs)²⁵ and the 1980 primary enrollment of the schools' districts.

²³ CIPPEC (2006) review some of the policy-oriented papers that analyze increased instructional time from different (mainly positive) points of view, including Huxti (1992); Pereyra (1992 a and b); Slavin (1996); Martinic (1998 and 2002); Aguerro (1998), and more recently Feldfeber et al. (2003); Boissiere (2004) and Llach et al. (2006). They also refer to some critical studies like Kurweit (1985) and National Education Commission (1994).

²⁴ Defined by Rosenbaum and Rubin (1983) as the conditional probability of receiving a treatment given the pre-treatment characteristics.

²⁵ Unsatisfied basic needs (UBN) is a compound index of social indicators like housing quality, employment status and educational attainment.

Table 2. Characteristics of the sample

			Total	Shift		Parent's SES			Students' SES		
				Simple	Double	High	Medium	Low	High	Médium	Low
Gender	Male	Obs.	185	86	99	22	73	90	61	59	65
		%	48.7%	50.0%	47.6%	53.7%	50.0%	46.6%	56.0%	41.8%	50.0%
	Female	Obs.	195	86	109	19	73	103	48	82	65
		%	51.3%	50.0%	52.4%	46.3%	50.0%	53.4%	44.0%	58.2%	50.0%
Total	Observations	380	172	208	41	146	193	109	141	130	
	%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	

We interviewed people who finished the primary school in 1977 (a) in double shift (DS) schools, where the policy was implemented in 1971 (treatment group), and (b), in simple shift (SS) schools with similar characteristics in pre-treatment variables such as socioeconomic status (SES) and geographic proximity (control group).

5. Results: more education does not imply better education

5.1. *Descriptive statistics and mean differences.* The main outcomes analyzed in the study and the mean differences between treatment and control groups are presented in Table 3^{26 27}.

Table 3. Outcomes' variables

	Obs.	Mean	Std. Dev.	Treatment group's mean (I)	Control group's mean (II)	(I-II)	
Students' SES (1: low, 2: medium, 3: high)	380	1.95	0.79	2	1.88	0.12	*
Changes of school (1: yes, 0:no)	380	0.57	0.50	0.62	0.51	0.11	**
Repetition in primary school (1: yes, 0:no)	380	0.06	0.23	0.05	0.07	-0.02	0
Conclusion of high school (1: yes, 0: no)	380	0.90	0.30	0.94	0.86	0.08	***
Repetition in high school (1: yes, 0:no)	375	0.21	0.40	0.22	0.19	0.03	0
Private or public high school (1: public, 2: private)	376	1.26	0.44	1.23	1.29	-0.06	*
Type of shift in high school (0:simple, 1: double)	380	0.21	0.41	0.23	0.19	0.04	0
High school career (1: "bachiller" 2: commercial, 3: technical, 4: other)	376	1.86	0.76	1.87	1.84	0.03	0
Tertiary (post-secondary) studies (1: yes, 0:no)	343	0.83	0.38	0.83	0.83	0.00	0
Conclusion of the first tertiary study (1: yes, 0: no)	285	0.76	0.43	0.77	0.75	0.02	0
Timely conclusion of the first tertiary study (1: yes, 0:no)	216	0.67	0.47	0.65	0.70	-0.05	0
Second tertiary study (1: yes, 0:no)	285	0.27	0.44	0.28	0.25	0.03	0
Conclusion of the second tertiary study (1: yes, 0: no)	76	0.58	0.50	0.64	0.48	0.16	*
Timely conclusion of the second tertiary study (1: yes, 0:no)	44	0.71	0.46	0.69	0.73	-0.04	0
Work during the studies (1: yes, 0:no)	380	0.65	0.48	0.68	0.62	0.06	0
Work related to the career (1: yes, 0:no)	247	0.61	0.49	0.66	0.55	0.11	**
Number of hours worked during the studies	244	6.80	2.17	6.80	6.80	0.00	0
Type of work (1:permanent, 2: temporary, 3: occasional work)	247	1.22	0.47	1.18	1.27	-0.11	0
Postgraduate studies (1: yes, 0:no)	67	0.96	0.21	0.92	1	-0.08	**

²⁶ Descriptive statistics of both, pre-treatment and treatment variables, are shown in Appendix 1.

²⁷ Some other characteristics of both groups are: (a) more than half of the students changed their primary school; (b) most of those who attended a double shift primary school changed to simple shift high school; (c) 76% of the students who started tertiary studies concluded them, and 70% of them did so in time; (d) many surveyed people worked during their studies and that work was related to what they were studying.

Type of postgraduate studies (1: post degree, 2: master, 3: PhD)	64	1.41	0.61	1.51	1.28	0.03	*
Conclusion of the postgraduate studies (1: yes, 0: no)	64	0.80	0.41	0.74	0.86	0.12	°
Knowledge of a foreign language (1: yes, 0:no)	380	0.87	0.33	0.90	0.84	0.06	**
Presence of children in the current household (1:yes, 0:no)	380	0.78	0.42	0.78	0.77	0.01	°
Number of children in the current household	380	1.55	1.11	1.50	1.60	-0.10	°
Quality of work of the person surveyed (1: stable, 2: occasional)	331	1.02	0.14	1.02	1.02	0.00	°
Unemployed status of the person being surveyed (1:yes, 0:no)	380	0.02	0.15	0.03	0.02	0.01	°
Changes of job since the person began working	378	3.63	3.98	3.17	4.18	-1.01	***
Unemployed status since the person began working	374	0.66	1.32	0.56	0.77	-0.21	*
Quality of work of the household head (1: worker, 2: occasional)	327	1.02	0.12	1.02	1.01	0.01	°
Unemployed status of the household head (1: yes, 0:no)	380	0.02	0.01	0.03	0.02	0.01	°
Net monthly income of the person being surveyed (1: less than \$323, 2: \$324 -\$484; 3: \$485-\$635, 4:\$636-\$806, 5: \$807-\$968, 6:\$969-\$1290, 7: >\$1290)	295	3.23	2.05	3.19	3.27	-0.08	°
Educational level of the spouse (2: lowest, 10: highest)	261	6.80	1.98	7.04	6.51	0.53	**
Educational level of the household head (2: lowest, 10: highest)	380	7.25	2.07	7.33	7.15	0.18	°

* Significant at 10%. ** Significant at 5%. ***Significant at 1%. °Not significant.

Some of the relevant results regarding variables that cannot be considered to have the same mean in both groups are the following. (a) Conclusion of high school is higher in the treatment group (TG) and changes of jobs are higher in the control group (CG), both significant at 1%; (b) changes of school, knowledge of a foreign language, a job related to the career and the educational level of the spouse are higher in TG, while postgraduate studies are more frequent in CG, all of them significant at 5%; (c) conclusion of the second tertiary study is higher in the TG, significant at 10%.

Tables 4 to 6 show some of the main, statistically significant mean differences in outcomes' variables for low, medium and high SES students, respectively. In the case of low SES students, conclusion of high school remains higher for TG and significant at 1%, and the same happens with knowledge of a foreign language, but now significant at 1%. These differences between TC and CG are higher than for the whole sample. On the other hand, the number of children and the educational level of the household head are higher in the CG and significant at 5%.

Table 4. Low SES students' outcomes

	Treatment group's mean (I)	Control group's mean (II)	(I-II)	
Conclusion of high school	0.87	0.63	0.23	***
Private or public high school	1.26	1.43	-0.17	**
Second Tertiary study	0.17	0.05	0.12	*
Work during the studies	0.37	0.25	0.12	*
Type of work	1.12	1.38	0.26	*
Knowledge of a foreign language	0.85	0.65	0.20	***
Presence of children in the current household	0.75	0.84	-0.09	*
Number of children in the current household	1.31	1.90	-0.59	***
Educational level of the household head	5.43	5.00	0.43	**

*significant at 10%, ** significant at 5%, *** significant at 1%, ° it is not significant

Table 5. Medium SES students' outcomes

	Treatment group's mean (I)	Control group's mean (II)	(I-II)	
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Repetition in high school	0.18	0.07	0.11	**
Type of work	1.14	1.25	0.11	*
Knowledge of a foreign language	0.91	0.97	-0.06	*
Changes of job since the person began working	2.78	4.58	-1.8	**
Unemployed status since the person began working	0.53	0.90	-0.37	*
Educational level of the household head	7.55	7.94	-0.39	*
<i>*significant at 10%, ** significant at 5%, *** significant at 1%, ° it is not significant</i>				

Table 6. High SES students' outcomes

	Treatment group's mean (I)	Control group's mean (II)	(I-II)	
Changes of school	0.63	0.36	0.27	***
Conclusion of the second tertiary study	0.80	0.43	0.37	**
Postgraduate studies	0.89	1.00	-0.12	**
Type of postgraduate studies	1.63	1.29	0.33	**
Conclusion of the postgraduate studies	0.71	0.94	-0.23	**
<i>*significant at 10%, ** significant at 5%, *** significant at 1%, ° it is not significant</i>				

In the case of medium SES students there are no outcome variable whose mean differences between TG and CG are significant at 1%. Repetition in high school is higher in the TG, while changes of job are more frequent in the CG, both significant at 5%. The former difference is higher in medium SES students than in the whole sample. Finally, in the case of high SES students presented in Table 6, the main variables that cannot be considered to have the same mean are changes of school in the primary level (higher in the TG), conclusion of a second tertiary study (higher in the CG) and conclusion of postgraduate studies (higher in the CG), all of them significant at 5%.

5.2. *Educational, occupational and income outcomes.* With the exception of students' nationality and gender, pre-treatment variables cannot be considered to have the same mean (Appendix 1). It follows that mean differences between TG and CG in the outcome variables cannot be considered as the result of the treatment per se. In order to control for these pre existing differences we adopt the propensity score matching with the Kernel method.

In the following tables we present the estimation of the average effect of policy on the treatment group (Attk). On the other hand, to estimate the impact of the double shift, we eliminated the outliers in the control group. In other words, we did not include in the sample individuals whose propensity score is lower than the minimum probability observed in the treatment group.

Regarding the main educational outcomes, the DS had a positive and significant effect in (a) the conclusion of high school: the treatment improved 21% the graduation rate, and this is one of the most relevant results of our research; (b) the access to, and the conclusion in time of, a second tertiary study and, (c) the probability to have a work related to the studies. On the other hand, the educational variable where the treatment appears as having a negative and significant effect is the access to, and the conclusion of, postgraduate studies. Other positive and significant effects of the DS were to change jobs less frequently, to have more children and the educational level of the spouse.

We also calculated the effects of the treatment desegregated in the three levels of students' SES. In Tables 8 to 10 we only show statistically significant effects.

Table 7. Effects of the double shift: average effects and standard errors

	Attk	Boot se
Pupils' SES	0.20	0.17
Changes of school	0.16*	0.10
Primary school repetition	-0.10	0.10
Conclusion of high school	0.21*	0.09
High school repetition	0.03	0.09
Private or public high school	-0.07	0.10
High school shift type	0.10*	0.06
Tertiary studies	-0.04	0.06
Conclusion of the first tertiary study	0.06	0.12
Year in which the first tertiary study was interrupted	-0.16	0.32
Conclusion in time of the first tertiary study	-0.16	0.13
Second tertiary study	0.15*	0.05
Conclusion of the second tertiary study	0.03	0.26
Conclusion in time of the second tertiary study	0.51*	0.14
Work during the studies	0.13*	0.09
Work related to the studies	0.21	0.15
Number of hours worked during the studies	-0.06	0.43
Type of work	-0.07	0.18
Postgraduate studies	-0.09*	0.05
Type of postgraduate studies	0.29	0.22
Conclusion of the postgraduate studies	-0.21*	0.10
Knowledge of a foreign language	0.09	0.09
Presence of children in the current household	0.02	0.07
Number of children in the current household	0.03	0.19
Quality of work of the person surveyed	-0.03	0.06
Unemployed status of the person being surveyed	0.00	0.00
Changes of job since the person began working	-1.43*	0.51
Unemployed status since the person began working	-0.29	0.27
Quality of work of the household head	-0.03	0.05
Unemployed status of the household head	0.00	0.00
Net monthly income	0.02	0.65
Educational level of the spouse	0.93*	0.50
Educational level of the household head	0.73*	0.41
<i>*significant</i>		

Notes: a) Attk: Average effect of treatment with Kernel matching.
b) Boot se: Bootstrapping of standard errors.

Table 8. Effects of the double shift for the low SES group: average effects and standard errors

	Attk	Boot se
Conclusion of high school	0,28*	0,15
Private or public high school	-0,21*	0,14
Type of shift in high school	0,16*	0,07

Second tertiary study	0,19*	0,07
Educational level of the household head	0,54*	0,34
*significant		

Notes: see Table 7.

In the group of low SES students, the DS improved 28% high school graduation rates –again, a very relevant result- and increased 19% the probability to have access to a second tertiary study. In the group of medium SES students, the DS had a positive a significant effect in the following educational variables: (a) tertiary studies (an increase of 41%); (b) a second tertiary study and, (c) the conclusion of the second tertiary study (whose graduation rate improved a 47%). On the other hand, the treatment appears as having had a negative and significant effect in the following educational variables: (a) repetition in the primary level (a very low increase of 4%); (b) repetition in high school (a more relevant increase of 14%), and (c), the knowledge of a foreign language. In the highest pupil SES, the double shift had a negative effect in the number of children who began a postgraduate study and its graduation rate. Also, we can see a positive effect in the net monthly income.

Table 9. Effects of the double shift for the medium SES group: average effects and standard errors

	Attk	Boot se
Changes of school	0,31*	0,10
Repetition in primary school	0,04*	0,03
Repetition in high school	0,14*	0,06
Tertiary studies	0,41*	0,24
Year in which the first tertiary study was interrupted	1,27*	0,77
Second tertiary study	0,22*	0,09
Conclusion of the second tertiary study	0,47*	0,12
Work related to the study	0,38*	0,15
Number of hours worked during studies	-1,48*	0,62
Knowledge of a foreign language	-0,10*	0,04
Number of children	0,45*	0,19
*significant		

Notes: see Table 7.

Table 10. Effects of the double shift for the high SES group: average effects and standard errors

	Attk	Boot se
Changes of school	0,48*	0,09
Private or public high school	0,16*	0,08
Conclusion in time of the second tertiary study	0,63*	0,24
Postgraduate studies	-0,13*	0,07
Conclusion of postgraduate studies	-0,30*	0,16
Net monthly income	1,61*	0,92
*significant		

Notes: see Table 7.

6. Discussion and policy implications

We have shown in this paper that the introduction of longer school days in 50% of the primary schools of the city of Buenos Aires in 1971 has modestly improved only some of the educational attainments of that year's cohort and have not had general occupational or income effects, all of them measured thirty nine or forty years after cohort's graduation. The general meaning of our result coincides with most of the literature reviewed in this paper, even the one performed with the more demanding methodology of natural experiments. Modest effects are common not only to lengthening the school days or the school year, but to all the educational policies reviewed too. Confronting this situation it is possible to hypothesize two broader conclusions. On the one hand, it is increasingly evident that in order to understand the educational process a very deep revision of the educational production function approach is needed. This revision should give a much more important to issues like the quality of education, their contents and what happens in the educational situation per se, every day, every week, every month, inside the classrooms. Of course, this is also very relevant regarding educational policies. Taking into account the high costs of generalizing a double shift policy, very probably unaffordable in developing or emerging countries, it seems crucial to address the look to the quality, the contents and the educational procedures involved in the extra school time. This only qualifies, but do not deny, the claim made by Piketty and Valdenaire (2006), who argue that a targeted allocation of resources to poorer -or the poorest- schools and students could have a significant impact in reducing educational inequalities, and that this effectiveness will be much greater if it concentrates from the early childhood onwards. This is a challenge, since it is referred to one of the most criticized consuming-resources educational policies, i.e., class size. So the debate seems still open in order to get better answers to the "eternal" question of how improve the quality of education for the poor.

Appendix 1
Descriptive statistic on pre treatment and treatment variables

	Obs.	Mean	Std. Dev.	Treatment group's mean (I)	Control group's mean (II)	(I-II)	
Pre-Treatment Variables							
Nationality (1: argentine, 0: foreigner)	380	1.00	0.07	1.00	1.00	0.00	°
Age	372	41.52	0.70	41.44	41.62	-0.17	***
Gender (1: men, 0: women)	380	0.49	0.50	0.48	0.50	-0.02	°
School SES (1: low, 2: medium, 3: high)	380	2.00	0.75	2.09	1.90	0.19	***
Parent's SES (1: low, 2: medium, 3: high)	380	1.60	0.68	1.69	1.49	0.19	***
Type of School (1: men and women, 2: women, 3:men)	380	1.40	0.71	1.62	1.13	0.49	***
Number of students	375	25.56	5.73	27.83	22.79	5.05	***
Number of sections	380	1.67	1.16	1.90	1.40	0.13	***
Father's educational level (1: lowest, 10: highest)	380	4.47	2.10	4.68	4.20	0.51	**
Mother's educational level (1: lowest, 10: highest)	370	5.08	2.37	5.17	4.67	0.48	**
Treatment Variables							
Foreign language as a subject (1: yes, 0:no)	380	0.52	0.50	0.93	0.02	0.90	***
Cultural activities (1: yes, 0: no)	380	0.67	0.47	0.67	0.67	0.00	°
Place where the cultural activities take place (0: nowhere, 1: at school, 2: at home, 3: at home and at school)	380	1.31	0.97	1.32	1.31	0.01	°
Presence of Lunch service (1: yes, 0: no)	378	0.59	0.49	1.00	0.08	0.92	***
Assistance to the lunch service (1: yes, 0:no)	221	0.66	0.48	0.70	0.07	0.62	***
*significant at 10%. ** significant at 5%. *** significant at 1%. °not significant							

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