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Do Longer School Days Have Enduring Educational, Occupational, or Income Effects? A Natural Experiment in Buenos Aires, Argentina

In 1971 longer school days were decreed for around half of the primary schools in the city of Buenos Aires, Argentina. The policy covered all the city neighborhoods, and the schools were chosen probably at random. An unusual opportunity for a natural experiment was thus created. In 2006 and 2007 we interviewed a sample of 380 alumni of the 1971 cohort, thirty years after their 1977 graduation from schools with and without longer days. We tried to identify how the length of their school days affected their education, occupation, and income.

The next section provides a fuller description of the aforementioned policy. The subsequent section, devoted to a review of the literature, is longer than usual. We thought it was important to review and to compare both the older literature on the relationship between the length of school schedules and academic results and the newer literature devoted to renewing the educational production function approach using random or natural experiments. Cross-references between different literatures are rare, but from our point of view, they can promote a better understanding of the issues dealt with here.

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The authors are very grateful to both IAE-Universidad Austral and Fundación Ethos, whose support to this project was decisive. The authors also wish to thank Walter Sosa Escudero, Ernesto Schargrodsky, Flavia Roldán, Milagros Nores, and the participants in seminars at IAE-Universidad Austral and the Argentine National Academy of Education for their helpful comments.

The third section presents the methodology and the characteristics of the database, and the fourth section shows the main results of the experiment. We then conclude with a discussion of the results and some of their policy implications.

The Policy and Its Context

In this section we describe the policy that gave origin to the natural experiment and the context in which it took place.

Educational System in Argentina and Buenos Aires in the 1970s

Since the end of the nineteenth century, the Argentine educational system has been governed by the principles of free and universal access, included laity in the public schools, and, up to the late 1970s, provided for seven years of compulsory primary education.[1] Although primary education was constitutionally in the hands of the provinces, the federal government continued running some primary schools in most provinces until the late 1970s and early 1980s. The private sector—both religious and secular—was also authorized to run primary and secondary schools. The case of the city of Buenos Aires was peculiar. As the capital of Argentina, until 1996 its administration was in the hands of the federal government, and the same happened with its schools. Enrollment rates in Argentina have been traditionally high when compared to other Latin American countries. In 1970 school expectancy from primary to tertiary education was 10.3 years at the federal level, and even higher and close to the average of developed countries (11.5 years) in the city of Buenos Aires (Llach 2005).

Policy of Lengthening School Days in the City of Buenos Aires: Creation of a Natural Experiment

The policy introduced a double shift (DS), or full-time attendance, into the primary schools of the city of Buenos Aires.[2] This approach began as a pilot

1. At the end of the 1970s, the first year of preschool education was also decreed to be compulsory, and in the early 1990s, compulsory education was extended up to the tenth year.

2. The traditional length of a primary school's schedule in Argentina has been between four and four-and-one-half hours, either in the mornings (more common) or in the afternoons. This system is known as a simple schedule or day (*jornada simple*). Accordingly, the new system was called a full schedule or day (*jornada completa*) or double shift (*doble jornada*). The length of the school day in the new system was nearly eight-and-one-half hours, including around two hours for lunch.

experiment in 1957 proposed by the professor Carlos Florit, general inspector of schools of the National Council of Education (*Consejo Nacional de Educación*).[3] During the 1960s, the number of DS schools increased very gradually, but in 1971 it was drastically expanded to encompass almost 50 percent of the primary schools of the city of Buenos Aires.[4] The policy was originally conceived to achieve both educational and social purposes (Consejo Nacional de Educación 1968, 1971) and was evenly applied in all the school districts in such a way that, in the early 1970s, the proportion of DS primary schools in every school district was around 50 percent of the total.

ADMISSION CRITERIA AND THE SELECTION BIAS ISSUE. Even in areas where the middle class predominates in the city, there were, and still are, important socioeconomic differences among the neighborhoods and school districts. From a social perspective, the idea of the new policy was to provide a solution to the uneven consequences of the increasing participation of women in the labor force. While richer households could pay for nurses or other domestic help to look after children before or after their single school shift, the poorer households could not. For that reason, the first DS schools were in poorer school districts. This policy was changed in the late 1960s, when educational goals began to supersede social ones. This change was clearly manifested in the parallel modification of admission criteria. In 1968 DS schools were ordered to give priority to

—familial, social, and economic needs of the candidates;

—proximity of the student's address to that of the school; and

—students with sisters or brothers in the school.

Additional changes in criteria occurred in 1971, when it was clearly established that the main and unavoidable condition for admission was living near the school. Only after that was fulfilled could the school consider the following additional social criteria: the family's unfavorable socioeconomic conditions, both parents working without domestic help, and the number of siblings.

The 1971 reform of the admission criteria was critical in reducing the selection bias of our research. In addition, it is very well known that the address and the siblings-in-the-school criteria always have been predominant in the city of Buenos Aires. Taken together, these criteria implied both that the freedom to choose the school was very limited and that the students were not mainly sorted by socioeconomic criteria. Although these factors do not preclude the

3. Feldfeber, Gluz, and Gómez (2003).
4. See Ministerio de Cultura y Educación (1970).

possibility of selection bias—mainly from parents choosing or avoiding DS schools—they reduce it considerably.

Academic content was very precisely defined for the new DS schools. Extra time was assigned to the following activities:

—reinforcement of the academic content already in place, particularly language and mathematics (35 percent);

—one-on-one teacher-assisted studying (25 percent);

—learning a foreign language, typically English (12.5 percent);

—gym and instruction on health and saving habits (7.5 percent); and

—crafts and job training (20 percent).

Although originally intended to teach students useful skills for the labor market, much of the craft and job training seemed outdated from the very beginning, so that the quality of the additional school time invested in this activity was not very good.

REFORM OF TEACHERS' CAREERS AND IMPLEMENTATION OF THE EXPERIMENT. In conformance with Law 18.614, an experimental new regime for full-time and part-time teachers was established during the 1970 school year.[5] The experiment was implemented in secondary schools and was intended to replace the system of "lesson hours" in full-time and part-time contracts with teachers in some institutions.[6] The main purpose was to increase the teacher's dedication to his or her pupils and avoid "taxi teachers," who taught classes in many different institutions in order to earn enough money for a decent living. Such geographical dispersion was detrimental to the pedagogical function and educational effectiveness.

Resolution 288 from the Ministry of Culture and Education, dated 12 March 1970, established which secondary schools would participate in this "micro-experiment." On 18 March 1970, through Resolution 324, the rules for designating the staff of teachers in such institutions were established, according to article 9 of Law 18.614. In principle, the same teachers that were already in the schools would stay, or they would be relocated if they requested this.[7] By the end of the year 1970, it was decided to extend the experiment to other institutions due to the positive results obtained during the first year. After the

5. This was known as Proyecto 13, and the teachers would have either a full-time schedule consisting of thirty-six hours or a part-time schedule, which could consist of thirty, twenty-four, or eighteen hours. Teachers had to perform extracurricular tasks for no less than four hours and no more than 50 percent of the teacher's time in the school.

6. In Spanish these are called *horas cátedra.*

7. This process applied only to secondary schools, since the problem of "taxi teachers" existed at that level and not with the primary schools.

Fourth National Meeting of Ministries of Education, the provinces agreed to enforce in 1971 the new structure and the new curricula and methodology at both primary and secondary levels of the different institutions. In this context, as directed by Resolution 1.885 of the Ministry of Culture and Education (dated 10 September 1970), it was decided that the double shift should be implemented in the different schools. Annex 2 of the resolution designated each of the public institutions where it would be implemented in the city of Buenos Aires, which corresponded to half of the public schools in every school district. As far as the authors know, the schools were assigned to the DS program at random. Since teachers and directors or principals were (and still are) assigned to schools through a very rigid, bureaucratic procedure, once a school was chosen, its teachers and directors or principals were part of the new program by default; they would be relocated and replaced only if they refused to participate.

In the city of Buenos Aires, the experiment was implemented in ninety-seven primary schools. Thirty years after the implementation, eight of them do not appear in the list of schools, thirteen are now on a single shift, and the remaining seventy-six continue as DS schools. In some cases, there has been a change in the name; in one instance, a school that had belonged to district 19 had been reassigned to the more recently established district 21.[8]

Longer School Days, Enduring Effects of Education, and Natural Experiments: Review of the Literature

As far as the authors know, no previous research has been done with the same purposes and methods of this paper, that is, to assess the enduring educational, occupational, and income effects of longer school days. For that reason, we separately 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 in education. It is a little surprising that the vast majority of the literature on the effectiveness of instructional time was written between 1960 and 1990, as if the issue has disappeared since then. However, as we note at the end of this review, there recently has been a revival of interest in instructional time as a component of educational policy.

8. A complete list of the schools can be found in appendix A.

Longer School Days (Allocated or Scheduled Time) and Educational Outcomes

This topic has a long history in the literature of educational science (CIPPEC 2006). Carroll (1963)—perhaps the first to adopt a pedagogically oriented approach—and most of the literature since then 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 the countries' GDP and the students' socioeconomic status (SES). Cotton (1989)—one of the oldest and most comprehensive reviews of this literature—distinguishes different varieties of allocated time: school time, classroom time, instructional time (the portion of classroom time spent teaching students), engaged time or time on task, and academic learning time (ALT).[9] Cotton emphasizes the importance of keeping in mind this taxonomy because only about half of the typical school day is actually used for instruction, and the students are engaged in learning activities for only about half of their in-class time.[10] She concludes that

—there is a small, positive relationship between allocated time and students' achievements;

—there is a stronger, but still small, relationship between time on task and achievements;

—there is a strong and positive association between ALT and students' achievements and attitudes;

—lower-ability students benefit more from increases in allocated or engaged time, whereas higher-ability students only benefit very slightly, if at all; and

—benefits are greater in highly structured fields of study, such as mathematics and foreign languages.[11]

Only one year after Cotton's study, Berliner (1990) also emphasized the multidimensionality of instructional time and noted that the popularity of research on scheduled time was largely a consequence of the ease in measuring it.[12] Contrary to findings in the majority of the literature, Berliner concludes

9. 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.

10. After reviewing McMeekin (1993), Thrupp (1998), and Martinic (2002), CIPPEC (2006) arrives at 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 socioeconomic environments.

11. Cotton (1989) reviewed fifty-seven research studies—mainly from developed countries—concerned with the relationship between the educational time factors cited above and students' outcomes.

12. He also pointed out that most of the studies on the effects of allocated instructional time on students' achievements were an outgrowth of the Coleman report (Coleman and others 1966), with its skeptical view of the return of any kind of educational investments on educational outcomes.

that in developed countries, the effects of quantity of instruction on achievement are clear and of great relevance for educational policies, and that the effects of quantity and quality of schooling are even clearer in less developed countries.[13] Nineteen years after Berliner, Bellei (2009) concludes from his review that most of the American studies on the subject agree

—on the existence of a positive and statistically significant relationship between instructional time and academic achievements of the students;

—on the modest size of that relationship;

—that the effect is stronger for students with initially low academic achievement; and

—that it tends to be curvilinear, showing diminishing returns to scale with the increase in instructional time.[14]

Finally, Fuller and Clarke (1994) also conclude that the effects of instructional time on educational outcomes are stronger in developing countries, including those in Latin America (see CIPPEC 2006).[15]

SCHOOL TERM LENGTH. A close family of studies has analyzed the educational, occupational, and income effects of the length of the school year. In his own study, Bellei (2009) uses a natural experimental methodology to evaluate 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 random but decided by the government according to certain criteria.[16] His main findings indicate that the program had positive effects on students' achievements, both in language (between 0.05 and 0.07 standard deviation) and mathematics (around 0.07 standard deviation), and had a stronger effect

13. In a shorter review, Pittman, Cox, and Burchfiel (1986) concur with Berliner's first conclusion. Berliner adds that the study of Hyman, Wright, and Reed (1975) is one of the few to consider the enduring effects of quantity of schooling on overall quality of life.

14. In addition to other already quoted papers, Bellei reviews Jencks and others (1972), Bloom (1976), Wiley (1976), Borg (1980), Fisher and others (1980), Frederick and Walberg (1980), Karweit and Slavin (1981), Brown and Saks (1986, 1987), and Link and Mulligan (1986). He also adds that the methodological limitations of most of the reviewed studies are huge and that it is not clear to what extent they were subject to other factors with the potential to affect the findings.

15. Latin American cases are analyzed in Cardoso (2004; Uruguay); Cervini (2001; Argentina); Ministerio de Educación de Chile (2003); Administración Nacional de Educación Pública (2003; Uruguay), and Bellei (2009; Chile).

16. 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.

in rural and municipal schools than in urban and private schools. Marcotte (2007) also performed a natural experiment on the effects of instructional time on Maryland's primary school test scores. He found that the natural variation in snowfall over time, which influenced the number of effective school days, has a small but significant effect on students' exam performance.[17] According to Pischke (2007), most of the studies on the effects of length of the school term, including his own, find that they are positive and significant only regarding educational outcomes like avoiding repetition, but not with regard to test scores, future earnings, or employment.

Enduring Effects of Education

The classical reference here is Hyman, Wright, and Reed (1975), who analyzed responses to general knowledge questions in public opinion surveys between 1947 and 1974.[18] Based on the fact that the higher the respondents' level of educational accomplishment, the more often the correct responses were given, the authors conclude that "education produces large, pervasive, and enduring effects on knowledge and receptivity to knowledge" (p. 109). However, Wolfle (1980) emphasizes that it is likely that all analyses of educational effects that do not include IQ variables suffer severe, although unknown, specification errors.[19] 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 education.[20]

More recently, in the age of the methodologies of instrumental variables and natural experiments, Duflo (2001) studies the educational and labor market outcomes of construction of 61,000 primary schools in Indonesia in a very short period (1974–78). Measuring the effects in 1995, twenty years after the program, she finds that it increased the average years of education by 0.25 to

17. Only 1 to 2 percent fewer students tested in harsh winters performed satisfactorily in math than did students examined after mild winters.

18. 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.

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

20. Wolfle recognizes, however, 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. He adds the very important point that education does increase general intelligence in such a way that its indirect effect on vocabulary through adult IQ is five times the size of the direct effect.

0.40 of a year; it improved by 12 percent the probability that an affected child would complete primary school; and it raised wages from 3 to 5.4 percent.[21] Combining these effects, Duflo estimates economic returns to education ranging from 6.8 to 10.6 percent.[22] She also warns about the risks of generalizing her results to other contexts because of a number of factors, such as the strong emphasis on education in Indonesia at that time, the possibility of general equilibrium effects of the program on the returns to education, and the fact that the program induced variations only at the primary school level, while returns to secondary education might have been different.[23] Duflo recognizes that the program increased the quantity of education and that there is some concern 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, the research of Meghir and Palme (2003, 2004) evaluates, approximately forty years later, the impact on educational attainment and earnings of a major school reform that occurred in Sweden in the 1950s. The reform had many elements in common with those occurring 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 nonacademic streams at the age of twelve. The authors find that the reform increased both the educational attainment and earnings of those whose parents had acquired only the earlier compulsory level of education. However, the earnings of those with more educated parents declined, possibly because of a dilution of quality at the top end of educational levels. Although this study is a benchmark in the research on the long-lasting effects of education, it was not possible to separate in it the *quantitative* effects from the increase in years of schooling and the *qualitative* effects from the new curriculum and the elimination of selection by ability at the age of twelve. The effects found were also small (see table 1).[24]

21. Duflo asserts that this wage increase proves that there is a combined effect for quality and quantity changes in education leading to an increase in human capital.

22. She reports that her two-stage least squares estimates are similar to ordinary least squares estimates and also to most estimates reported for developed countries, but they are smaller than those of Psacharopoulos (1994) for developing economies.

23. With regard to general equilibrium effects, because the returns were measured twenty years after the program, in an environment where the educational levels were higher than when the program began, individuals' returns may be lower than they would be in other developing countries.

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

Finally, but perhaps most important, Schweinhart and others (2005) report the results of the High/Scope Perry Preschool Study through Age 40, which identifies "both the short- and long-term effects of a high-quality preschool program on young children living in poverty." The data come from a randomized experiment in which a sample of 123 low-income African American children, assessed to be at high risk of school failure, was split into a treatment group of 53 and a control group of 68. A variety of outcomes were measured at ages three, eleven, fourteen, fifteen, nineteen, twenty-seven, and forty. The authors find "evidence of positive effects on program-group children's intellectual performance, school experiences, lifetime earnings and crime rates. Their school achievement was at a higher level, they were more committed to school, and more of them graduated from high school than members of the no-program group. In their adult lives program participants have achieved higher earnings and committed fewer crimes than members of the no-program group" (Schweinhart and others 2005, pp. xv and xvi).

Natural and Randomized Experiments in Education

Fortunately, during this century, there has arisen a new class of natural and random experiments on the educational and labor market effects of different educational policies. This development has renewed hope for a better understanding of this very important issue, particularly after the disappointing results of the vast research program on the educational production function following the challenge posed by Coleman and others (1966).[25] Although more accurate than the previous research program, the most important common trait of this new crop of research is that, with some relevant exceptions, most of the effects of the measured educational policies on outcomes are positive but modest or very modest, as can be observed in table 1.[26] Regrettably, only a few experiments have studied the enduring effects of educational policies.

The following is a brief summary of the results shown in table 1. The educational policy (treatment) with more intense and widespread effects on income, educational, and employment outcomes is high-quality preschool education (Schweinhart and others 2005). Also worth mentioning are class size at the primary level, with strong effects on test score gaps (Piketty and Valdenaire 2006); conditional cash transfer effects on access to tertiary

25. To get a balanced view of the educational production function research program, see Glewwe (2002) and Akerlof and Kranton (2002).

26. Piketty (2004) argues that the class size is perhaps the clearest case of assessing the superiority of natural experiments.

TABLE 1. Compared Results of Natural and Random Experiments on Outcomes of Educational Policies[a]

Source and experimental type	*Treatment, location, and duration*[b]	*Results*
Preschool improvements		
Schweinhart and others (2005); R, preschool	High-quality preschool (High/Scope Perry Project); Chicago, Ill., U.S.; 37 years	*Graduation.* High school graduation rates of 77 percent in treatment group and 60 percent in control group. *Income.* Sixty percent of the treatment group vs. 40 percent of control group earning ≥US$20,000 a year. *Quality of life.* The treatment group at age 40 also had lower crime rates, higher employment rates, more fathers assuming child-rearing responsibilities, and higher scores in various intellectual and language tests at very different ages.
Kremer and Vermeersch (2004); R, preschool	Free school meals; Kenya; 1 year	*Attendance.* In spite of the increased fees in treatment schools, attendance at them improved by 8.5 pp (31 percent). Attendance gains were for both current students and students who had never attended before.
Berlinsky, Galiani, and Gertler. (2006); N, preschool	Vast preprimary classroom construction program; Argentina; 4–5 years	*Attendance.* One year of preprimary school increased average third grade test scores by 8 percent of a mean or by 0.23 SD of the distribution of test scores. *Noncognitive skills.* Preprimary school attendance positively affected students' self-control in the third grade as measured by behaviors such as attention, effort, class participation, and discipline.
Increase in school resources		
Duflo (2001); N, P and S	School construction program Indonesia; 20 years	*Graduation.* 12 percent increase in the probability of primary school completion. *Years of education.* Increase of 0.25 to 0.4 of a year. *Wages.* 3 percent to 7 percent increase in wages. *Rates of return.* 6.8 percent to 10.6 percent for primary education.
Glewwe, Kremer, and Moulin (2007); R, P	Random provision of textbooks to primary schools; Kenya; 4 years.	*Test scores.* No increase in test scores, contrary to the results of the previous literature. Textbooks increased scores for students with high initial

(*continued*)

TABLE 1. Compared Results of Natural and Random Experiments on Outcomes of Educational Policies[a] (*Continued*)

Source and experimental type	*Treatment, location, and duration*[b]	*Results*
		academic achievement. Students with weaker academic backgrounds did not benefit from the textbooks. Many of them could not read the textbooks, which were written in English, most students' third language.
Duflo and others (2006); Evans, Kremer, and Ngatia (2008); R, P	Provision of free uniforms with an average price of $5.82; Kenya; up to 5 years.	*Attendance.* For younger pupils, 6 pp increase (7 percent) in school attendance and 13 pp (15 percent) increase for students without a uniform prior to program. For older pupils, 13.5 percent decline in absenteeism. *Years of education.* Years of enrollment increased by 0.5 year (13 percent).
Class size		
Krueger (1999); R, P	Reduction of class size (Tennessee STAR Project); U.S.; 4 years	*Test scores.* 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. *Future earnings.* A 0.22 SD improvement in test scores resulting from smaller class sizes implied an improvement of 1.7 percent and 2.4 percent average male and female earnings, respectively.
Piketty and Valdenaire (2006); N, P and S	Class size exogenously determined by policy putting ceiling of 30 students per classroom; France; 6 years	*Test scores and test scores' gaps.* The reduction of one pupil per primary class allowed an increase in the range of 0.3–0.4 points in math test scores (0.7 in less socioeconomically endowed contexts). These results implied that 5 pupils less per classroom in the poorer zones could lead to closing 46 percent of the test result gap between them and the nonpoor zones. With the same policies, the gap would be closed 22 percent at the *college* level and only 4 percent at the *lycée.*

TABLE 1. Compared Results of Natural and Random Experiments on Outcomes of Educational Policies[a] (*Continued*)

Source and experimental type	*Treatment, location, and duration*[b]	*Results*
Dee and West (2008); R, early P	Reduction of class size (Tennessee STAR Project); U.S.; 8 years	*Noncognitive skills.* Class size reductions in early grades did improve subsequent student initiative, but these effects did not persist into the 8th grade. Smaller classes in the 8th grade led to improvements in measures of student engagement with effect sizes of 0.05 to 0.09, persisting two years later. Internal rate of return was 4.6 percent overall and 7.9 percent in urban schools.
Conditional cash transfers		
Schultz (2004); R, P and S	Cash transfers conditional on school attendance and take-up of health services; education grants reduced private costs of going to school by 50 to 75 percent (PROGRESA program); Mexico; 3 years	*Attendance.* Between 3.4 and 3.6 pp increase in attendance for all children in grades 1 to 8. An 11.1 pp increase (19 percent) in attendance for students who have completed 6th grade, and a 14.5 pp increase for girls who have completed 6th grade. Spillovers to ineligibles in treatment villages of 5 pp increase (7 percent) in secondary enrollment.
Barrera-Osorio and others (2008); R, P and S	Three kinds of conditional cash transfers; Bogotá, Colombia; 1 year	*1. Direct cash transfers.* *Attendance, permanence.* Increases of 2.8 pp in school attendance; 2.6 pp in school permanence. *Years of education.* Increases of 2.8 pp in the following year's enrollment and 23 pp in the probability to matriculate in tertiary studies. *Graduation.* 4.0 pp increase in graduation rates. *2. Part of the cash transfer postponed.* *Enrollment* in both secondary and tertiary institutions increased over the basic treatment by 3.6 and 3.3 pp, respectively, without reducing the current attendance. *Spillovers.* Negative spillovers on the nonselected siblings and some positive peer effects on educational outcomes.

(*continued*)

TABLE 1. Compared Results of Natural and Random Experiments on Outcomes of Educational Policies[a] (*Continued*)

Source and experimental type	*Treatment, location, and duration*[b]	*Results*
Additional teaching support		
Banerjee and others (2005); R, P	Additional teaching support to lagging children; India; 2 years	*Test scores.* 0.14 and 0.28 SD increase in test scores in the first and second year compared to nontreated peers. One year after the program, initial gains faded to about 0.10 SD.
Banerjee and others (2005); R, P	Computer-assisted learning program: 2 hours per week of shared computer time; India; 2+ years	*Test scores.* 0.35 and 0.47 SD increase in math scores in the first and second year of the program, respectively. After that, the increase tended to diminish.
Peer effects		
Goux and Maurin (2007); N, S	Neighbors' peer effects; France; cross-section, educational performance up to lower secondary	*Repetition.* The probability of repetition was found to be 0.20 SD higher for adolescents (15–16 years) living in neighborhoods with a higher proportion of mates that have already been held back a grade at age 15.
Training programs		
Attanasio, Kugler, and Meghir. (2008); R, training	Training program with school and on-the-job components; Colombia; 19 to 21 months	*Earnings and employment.* In the case of women, training increased wage and salaried earnings, probability of being employed, amount of days and hours worked, and probability of having a formal job with a written contract. Smaller effects on men: training only increased wage and salaried earnings and the probability of having a formal job and with a written contract, but not of having employment. Salaried earnings increased 18 percent for women and 8 percent for men. *Cost-Benefit.* The rate of return of the program emerging from cost-benefit analysis was 13.5 percent for women and 4.5 percent for men. On-the-job training intensity increased the returns of the program.
School reforms		
Meghir and Palme (2003, 2004); N, S	Vast school reform (1950s): increase in the years of compulsory schooling, new national curriculum, abolition	*Years of education.* Increase by 0.298 of a year, entirely due to the increase in the educational attainment of those with unskilled fathers.

TABLE 1. Compared Results of Natural and Random Experiments on Outcomes of Educational Policies[a] (*Continued*)

Source and experimental type	*Treatment, location, and duration*[b]	*Results*
	of selection by ability at the age of 12; Sweden; ~ 30 years	*Earnings.* Overall, 1.42 percent increase in earnings. For those with unskilled fathers, the reform increased earnings by 3.4 percent. *Rates of return.* If all the changes in earnings were due to changes in the quantity of education, the results implied returns of 6.0 percent for low-ability individuals, 11.6 percent for those with high abilities, and 8.4 percent overall. If other variables played a role, those returns were upper bounds.
Hoxby and Rockoff (2005); N, P	Comparison between "lotteried-out" and "lotteried-in" students applying to charter schools; Chicago, U.S.; 2 years (average)	Compared to their lotteried-out fellow applicants, students who applied to and attended charter schools an average of 2 years starting in the elementary grades scored about 6 national percentile rank points higher, both in math and reading.
Length of the school year		
Marcotte (2007); N, P and S	Longer school year as measured by harshness of winters; Maryland, U.S.; cross-section, same year	*Test scores.* Only between 1 and 2 percent fewer students performed satisfactorily after harsh winters than did students examined after mild winters.
Bellei (2009); N, S	Full-School-Day Program: increase from 955 to 1,216 hours a year in secondary schools; Chile; 2 years	*Test scores.* The program had a positive effect on students' achievement, both in language (between 0.05 and 0.07 SD) and math (around 0.06 SD). Effects were stronger in rural and municipal schools than in urban and private schools.
Sims (2006); N, P	Increase in school days, particularly ones devoted to test preparation; Wisconsin, U.S.; 5 years (average)	*Test scores.* Clear and positive relationship between math scores of 4th graders and days of preparation. The implied effect was small in both an absolute sense and relative to other educational reforms.

a. N, natural experiment; R, random experiment; P, primary education; S, secondary education; pp, percentage points; SD, standard deviation.

b. Duration refers to the time span between the treatment and the measurement of effects.

education (Barrera-Osorio and others 2008); training program impacts on wages and employment (Attanasio, Kugler, and Meghir 2008); effects of preschool meals on attendance (Kremer and Vermeersch 2004); and the impact of school construction on graduation rates and school attendance (Duflo 2001; Berlinsky and Galiani 2007). Surprisingly, and perhaps due to identification problems, vast school reforms appear to have more modest effects (Meghir and Palme 2003, 2004; Hoxby and Rockoff 2005). Regarding the lengthening of the school year, the only effects analyzed up to now are test scores, and the three studies reviewed (Marcotte 2007; Bellei 2009; Sims 2006) show smaller impacts on them than other treatments shown in table 1. Finally, some of the studies, such as Banerjee and others (2005) and Hoxby and Rockoff (2005), emphasize the importance of quality over "quantity" of education. This, as well as the critical importance of early childhood education, appears to be clearly proved by Schweinhart and others (2005), as can be seen in its impact on a rich set of dependent variables, from income to intellectual development. As we will show, the results of our experiment are very much in line with the relevant literature, both experimental and nonexperimental: that is, the effects are positive, nuanced, and stronger in lower socioeconomic strata and as regards quantitative outcomes.

Recent Revival of Increased Instructional Time as an Educational Policy

The aforementioned recent works of Meghir and Palme (2003, 2004), Banerjee and others (2005), Bellei (2009), and CIPPEC (2006) reveal a sort of revival of increased instructional time as an educational policy, perhaps due to the fact that it is a strategy relatively simple to implement.[27] However, impacts of additional instructional time per se seem, up to now, to be modest, validating the point made by Cotton (1989): "Significant increases in the quantity of schooling would be required to bring about even modest increases in achievement."

Methodology and Database

In this section we describe the methodology used and the way the database was constructed.

27. CIPPEC (2006) reviews some of the policy-oriented papers that analyze increased instructional time from different (mainly positive) points of view, including Husti (1992), Pereyra (1992a, 1992b), Slavin (1996), Martinic (1998, 2002), Aguerrondo (1998), and, more recently, Feldfeber, Gluz, and Gómez (2003), Boissiere (2004), and Llach (2006). The CIPPEC review also refers to some critical studies, such as those of Karweit (1985) and the National Education Commission (1994).

Methodological Approach

As stated earlier, the primary purpose of this paper is to estimate the impact of a double-shift educational schedule on schooling and earnings. The parameter of interest can be formally described as follows. For any student observed after implementation of the policy, define random variables representing what educational level the person would have achieved had he or she attended a double-shift school as opposed to a traditional single-shift school.[28] Denote these two potential outcomes by Y_1 and Y_0 and denote double-shift status by a dummy variable, D. For each person, we observe only $Y = Y_0 + (Y_1 - Y_0)D$, so Y_0 is not observed for students from double-shift schools ($D = 1$), and Y_1 is not observed for students from single-shift schools ($D = 0$). We still hope to identify certain averages of $Y_1 - Y_0$. The effect of treatment on the treated is one such parameter:

$$E\left(Y_1 - Y_0 \middle| D = 1\right) = E\left(Y_1 \middle| D = 1\right) - E\left(Y_0 \middle| D = 1\right).$$

This would tell us whether, on average, students benefited or suffered from double-shift schooling.

Comparisons by type of school shift can be decomposed as follows:

$$(1) \qquad E\left(Y_1 \middle| D = 1\right) - E\left(Y_0 \middle| D = 0\right) = E\left(Y_1 - Y_0 \middle| D = 1\right) + \left[E\left(Y_0 \middle| D = 1\right) - E\left(Y_0 \middle| D = 0\right)\right].$$

The term $E(Y_1 - Y_0|D = 1)$ is called the average treatment effect on the treated, and $[E(Y_0|D = 1) - E(Y_0|D = 0)]$ is the bias (Lee 2005). If the shift of the school were randomly assigned, then D would be independent of Y_0 and Y_1, implying that

$$E\left(Y_0 \middle| D = 0\right) = E\left(Y_0\right) \text{ and } E\left(Y_1 \middle| D = 1\right) = E\left(Y_1\right).$$

In this case, the effect of treatment on the treated is also the average treatment effect in the population subject to randomization and can be estimated by simple comparisons. The bias would be zero. We present these estimates by providing the mean differences of the outcome variables for the treated and the control groups. But, as we have seen, predetermined covariates are not

28. Also, we can analyze how much the person would earn in each case.

equal for both groups, which might mean that the probability of assignment to treatment might be influenced by them. Therefore we make a less restrictive assumption, which is in essence that the shift of the school is ignorable conditional on predetermined covariates, denoted by *X*. This assumption is also called selection on observables or ignorable treatment (Lee 2005) and can be expressed as follows:

$$E\left(Y\middle|D,X\right) = E\left(Y\middle|X\right). \tag{2}$$

With the conditional effect $E(Y_1 - Y_0|X)$ identified, we can get an *X*-weighted average effect (Angrist 1998). Therefore, in order to avoid the bias due to the existence of confounding factors, we use regression and matching estimations, the kind of natural experiment methodology normally applied to observational studies. In particular, we use propensity score matching to reduce the bias in the estimation of the outcomes, that is, comparing the outcomes using treated and control subjects who are as similar as possible (Becker and Ichino 2002).[29] Since this method is not sufficient to estimate the average treatment effect on the treated, we use 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.[30]

Database and Sample

The database comes from a randomly sampled survey applied to the 1971 cohort.[31] Selection of this cohort is methodologically very relevant because it was the first to attend the primary schools of the city of Buenos Aires after the generalization of the DS policy. Although we cannot say that this device elim-

29. Propensity score matching, as defined by Rosenbaum and Rubin (1983), is the conditional probability of receiving a treatment, given the pretreatment characteristics. The variables included in the estimation of the propensity score were age, gender, school SES, parents' SES, type of school (boys and girls together or separate), size of the class or section, number of sections in the school, educational level of the mother, and educational level of the father.

30. Both regression and matching estimations were done on the common support, which means that they were limited to covariate values where both treated and control observations were found. In the first case, it was done by saturating the model (Angrist and Pischke 2009), and in the second case, by not including in the sample individuals whose propensity score was lower than the minimum probability observed in the treatment group.

31. The survey was conducted by a specialized pooling agency, chosen by a process of competitive selection. For more information about the database, contact Juan Llach at jllach@iae.edu.ar.

TABLE 2. Characteristics of the Sample
Units as indicated

		Shift		*Parents' SES*			*Students' SES*		
Gender	*Total*	Simple	Double	High	Medium	Low	High	Medium	Low
Male									
No. obs.	185	86	99	22	73	90	61	59	65
Percent	48.7	50.0	47.6	53.7	50.0	46.6	56.0	41.8	50.0
Female									
No. obs.	195	86	109	19	73	103	48	82	65
Percent	51.3	50.0	52.4	46.3	50.0	53.4	44.0	58.2	50.0
Total obs.	380	172	208	41	146	193	109	141	130
Percent	100	100	100	100	100	100	100	100	100

inates the selection bias problems typical of such studies, it very probably helps at least to reduce them. 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.

As previously mentioned, DS was implemented in all the school districts of the city of Buenos Aires. To design the sample and obtain a good representation of the whole population (table 2), we took the following steps. First, each school district was assigned a level of SES (low, medium, or high) based on the proportion of households with unsatisfied basic needs, and the number of schools where the policy was implemented was calculated for each district (appendix B).[32] Then the distribution of the population by level of SES was determined. This exercise showed that either the authorities took into account the stratification of the population by SES, or the policy was implemented randomly. This means that if the people with the lowest SES at that time represented 10 percent of the population, only 10 percent of the schools selected for the policy were of low SES. Moreover, to calculate the number of observations by SES for the sample, we used the 1980 primary enrollment of the school districts.

In order to build the database, several steps had to be taken. First, to obtain the list of 1977 graduates, we had to acquire an authorization from Alberto Sileoni, the minister of education of the city of Buenos Aires at the time. Together with the request for authorization, we had to submit a list of the schools from which the student sample was to be drawn. Once the

32. Unsatisfied basic needs serve as a compound index of social indicators such as housing quality, employment status, and educational attainment.

authorization was obtained, members of the Ministry of Education contacted the supervisors of the school districts, who in turn informed the authorities of the chosen schools about the study. Only then could we request the list of 1977 graduates from the schools.[33] Finally, once we had the list of graduates, the telephone numbers had to be searched for in different databases.

The people we interviewed who finished primary school in 1977 fell into two categories: those who attended DS schools, where the policy was implemented in 1971 (treatment group [TG]), and those who attended simple shift schools with similar characteristics in pretreatment variables (control group [CG]). Interviews were conducted by telephone, which was possible because in the city of Buenos Aires the penetration of fixed telephony is very high. Almost all homes, even with the lowest incomes, own a phone. Furthermore, the response rate, once the person was located, was very high, regardless of the level of SES.

Results: More Education Does Not Imply Better Education

As an initial approach to finding the effects of the double-shift educational schedule, the mean differences of the outcome variables are presented. Then, for a more accurate estimation, the regression and matching estimators are presented.

Mean Differences

The mean differences between treatment and control groups are presented in table 3.[34] Some of the relevant variables that cannot be considered to have the same mean in both groups are

—conclusion of high school, which is higher in the TG (significant at the 1 percent level);

—knowledge of a foreign language and the educational level of the spouse, which are higher in the TG (significant at the 5 percent level);

—pursuit of postgraduate studies, which occurs more often in the CG and their members abandoned the second tertiary study later (both significant at the 5 percent level); and

33. Though they had the authorization from the Ministry of Education, some directors of the schools refused to provide the list of graduates in order to protect their private information. This problem was especially likely in high SES schools and delayed the work, since we had to request a new authorization for additional schools.

34. Descriptive statistics on pretreatment and treatment variables are shown in appendix C.

TABLE 3. Outcome Variables
Units as indicated

Outcomes	*No. obs.*	*Mean*	*Std. dev.*	*Treatment group (TG) mean (I)*	*Control group (CG) mean (II)*	*Difference in means (I–II)*
Educational						
Repetition in primary school (1: yes, 0: no)	380	0.06	0.23	0.05	0.07	−0.02
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
Tertiary (postsecondary) studies (1: yes, 0: no)	343	0.83	0.38	0.83	0.83	0.00
Conclusion of first tertiary study (1: yes, 0: no)	285	0.76	0.43	0.77	0.75	0.02
Timely conclusion of first tertiary study (1: yes, 0: no)	216	0.67	0.47	0.65	0.70	−0.05
Year in which first tertiary study was interrupted	64	2.44	1.28	2.50	2.36	0.14
Second tertiary study, if first was finished (1: yes, 0: no)	216	0.26	0.44	0.28	0.24	0.04
Conclusion of second tertiary study if first one was finished (1: yes, 0: no)	57	0.61	0.49	0.69	0.50	0.19*
Timely conclusion of second tertiary if first one was finished (1: yes, 0: no)	35	0.66	0.48	0.67	0.64	0.03
Year in which second tertiary study was interrupted if first one was finished	22	1.68	0.89	1.27	2.09	−0.82**
Postgraduate studies (1: yes, 0: no)	67	0.96	0.21	0.92	1	−0.08**
Type of postgraduate studies (1: post degree, 2: master, 3: PhD)	64	1.41	0.61	1.51	1.28	0.23*
Conclusion of postgraduate studies (1: yes, 0: no)	64	0.80	0.41	0.74	0.86	−0.12
Knowledge of foreign language (1: yes, 0:no)	380	0.87	0.33	0.90	0.84	0.06**
Income						
Net current monthly income[a]	295	3.23	2.05	3.19	3.27	−0.08
Others						
Students' SES at present (1: low, 2: medium, 3: high)	380	1.94	0.79	2	1.88	0.12*
Presence of children in current household (1: yes, 0: no)	380	0.78	0.42	0.78	0.77	0.01
Number of children in current household	380	1.55	1.11	1.50	1.60	−0.10
Educational level of spouse (2: lowest, 10: highest)	261	6.80	1.98	7.04	6.50	0.54**
Educational level of household head (2: lowest, 10: highest)	380	7.24	2.07	7.33	7.15	0.18

*Statistically significant at the 10 percent level; **statistically significant at the 5 percent level; ***statistically significant at the 1 percent level.

a. 1: <$323, 2: $324–$484; 3: $485–$635, 4: $636–$806, 5: $807–$968, 6: $969–$1290, 7: > $1,290.

TABLE 4. Outcomes of Students from Low SES Households
Units as indicated

Outcomes	*No. obs.*	*Mean*	*Std. dev.*	*TG mean (I)*	*CG mean (II)*	*Difference in means (I–II)*
Educational						
Repetition in primary school (1: yes, 0: no)	193	0.08	0.27	0.04	0.11	–0.07**
Conclusion of high school (1: yes, 0: no)	193	0.83	0.37	0.91	0.76	0.15***
Repetition in high school (1: yes, 0: no)	189	0.28	0.45	0.33	0.22	0.11*
Knowledge of foreign language (1: yes, 0: no)	193	0.83	0.37	0.89	0.78	0.11**
Others						
Number of children in current household	193	1.67	1.10	1.52	1.81	–0.29**

*Statistically significant at the 10 percent level; **statistically significant at the 5 percent level; ***statistically significant at the 1 percent level.

—conclusion of the second tertiary study, quality of postgraduate studies, and current SES, all of which are higher in the TG (all significant at the 10 percent level).[35]

Tables 4, 5, and 6 show the main and statistically significant mean differences in outcome variables of students originally coming from low, medium, and high SES levels, respectively. In the case of low SES students, conclusion of high school remains higher for the TG and is significant at 1 percent; the same happens with knowledge of a foreign language, but that is significant at 5 percent; and primary school grade repetition is lower in the TG, significant at 5 percent, while high school repetition is higher for the TG, but only at the 10 percent level of significance.

In the case of medium SES students, the means for both the conclusion of a second tertiary study and the quality of postgraduate studies are higher in the TG, but the members of the CG abandoned the second tertiary study later; all these results are significant at the 1 percent level. Contrary to what happened with low SES students, high school graduation rates are higher in the CG, significant at the 5 percent level. The middle SES group is the only one in which a significant (at 10 percent) mean difference appears with its current SES, which is higher in the CG.

35. The authors also analyzed some of the most important labor market variables (such as unemployment rate and job quality). The CG showed higher unemployment means, but only at the 10 percent level of significance. As regards the matching procedure, the only significant effect of the DS was that the changes of job were less frequent. These results might be attributable to selection, and since these values showed no relevance to subsequent analysis, they were not systematically presented here.

TABLE 5. Outcomes of Students from Medium SES Households
Units as indicated

Outcomes	*No. obs.*	*Mean*	*Std. dev.*	*TG mean (I)*	*CG mean (II)*	*Difference in means (I–II)*
Educational						
Conclusion of high school (1: yes, 0: no)	146	0.97	0.18	0.94	1.00	−0.06**
Conclusion of second tertiary study if first one was finished (1: yes, 0: no)	26	0.50	0.51	0.69	0.20	0.49***
Timely conclusion of second tertiary study if first one was finished (1: yes, 0: no)	13	0.62	0.51	0.72	0.00	0.72**
Year in which second tertiary study was interrupted if first one was finished	13	1.92	0.95	1.00	2.50	−1.50***
Type of postgraduate studies (1: post degree, 2: master, 3: PhD)	35	1.40	0.65	1.63	1.13	0.50***
Others						
Students' SES at present (1: low, 2: medium, 3: high)	146	2.26	0.69	2.20	2.36	−0.16*

*Statistically significant at the 10 percent level; **statistically significant at the 5 percent level; ***statistically significant at the 1 percent level.

Finally, in the case of the high SES group, repetition in high school is higher in the TG, and the quality of postgraduate studies is higher in CG, both results being significant at the 10 percent level.

Educational, Occupational, and Income Effects

With the exception of students' nationality and gender, pretreatment variables cannot be considered to have the same mean (appendix C). It follows that mean differences between the TG and CG in the outcome variables cannot be considered as the result of the treatment per se. In order to control for

TABLE 6. Outcomes of Students from High SES Households
Units as indicated

Outcomes	*No. obs.*	*Mean*	*Std. dev.*	*TG mean (I)*	*CG (II)*	*Difference in means (I–II)*
Educational						
Repetition in high school (1: yes, 0: no)	41	0.10	0.30	0.14	0	0.14*
Type of postgraduate studies (1: post degree, 2: master, 3: PhD)	13	1.62	0.65	1.44	2	−0.56*

*Statistically significant at the 10 percent level; **statistically significant at the 5 percent level; ***statistically significant at the 1 percent level.

TABLE 7. Average Effects of DS and Standard Errors (SE)[a]
Units as indicated

Outcomes	*OLS*	*SE (robust)*	*ATTK*	*SE (bootstrapped)*
Educational				
Repetition in primary school	0	0.03	−0.10	0.09
Conclusion of high school	0.05	0.04	0.21*	0.09
Repetition in high school	0.12*	0.06	0.03	0.07
Tertiary (postsecondary) studies	−0.08	0.05	−0.04	0.06
Conclusion of first tertiary study	0.07	0.06	0.06	0.14
Timely conclusion of first tertiary study	−0.02†	0.08	−0.16*	0.06
Year in which first tertiary study was interrupted	0.12†	0.44	−0.16	0.30
Second tertiary study if person finished first one	0.06†	0.07	0.15*	0.10
Conclusion of second tertiary study if first one was finished	0.35*	0.15	0.06	0.23
Timely conclusion of second tertiary study if first one was finished	0.06†	0.25	0.51*	0.24
Year in which second tertiary study was interrupted if first one was finished	−0.38†	0.50	−1.18*	0.56
Postgraduate studies	−0.07†	0.08	−0.07	0.14
Type of postgraduate studies	0.06	0.20	0.29	0.20
Conclusion of postgraduate studies	−0.32*	0.16	−0.21*	0.11
Knowledge of foreign language	0	0.04	0.09	0.09
Income				
Net monthly income of person being surveyed at present	−0.39	0.26	0.02	0.63
Others				
Students' SES at present	−0.08	0.09	0.20	0.18
Presence of children in current household	0.01	0.05	0.02	0.08
Number of children in current household	−0.04	0.14	0.03	0.17
Educational level of spouse	−0.03	0.28	0.93*	0.33
Educational level of household head	−0.36	0.24	0.73*	0.42

*Statistically significant at the 5 percent level; †model not significant at the 5 percent level.
a. OLS, ordinary least squares regression; ATTK, average effect of treatment with kernel matching.

these preexisting differences, we subjected the data to ordinary least squares (OLS) regressions and kernel propensity score matching estimates, which are presented in tables 7 through 10.

Regarding the regression results shown in table 7, we would like to mention that only three of them are statistically significant: repetition in high school and conclusion of the second tertiary study if the first one was finished, which are higher for DS graduates, and conclusion of postgraduate studies, which is lower for DS graduates. Kernel propensity score matching estimations of the average effect of the policy on the treatment group (ATTK), as reflected in the mean differences attributable to the treatment after controlling them for the differences in pretreatment variables, suggest the following.

First, DS is found to have positive and significant effects on these outcomes:

—conclusion of high school: the secondary school graduation rate is 21 percent higher in the TG, and this is one of the most relevant results of our research;

—access to and timely conclusion of a second tertiary study; and

—educational level of the spouse and of the household head.

On the other hand, the educational variables where the treatment appears to have negative and significant effects are the year in which second tertiary study was interrupted if first one was finished, the timely conclusion of the first tertiary study, and the conclusion of postgraduate studies.

The sign of the effects is the same in the regression and matching estimates for most of the outcome variables, and the cases in which it is not correspond to coefficients that are not statistically significant different from zero. However, the fact that the regression estimates and the matching estimates are not the same was expected.[36]

We also calculated the effects of the treatment for each level of student household SES. Tables 8 to 10 show the statistically significant effects for the ATTK estimates; although the OLS estimates are also provided, the following discussion centers on the ATTK estimates.

In the group of low SES students, the high school graduation rate is 22 percent higher in the TG—a very relevant result—and pursuit of a second tertiary study once the first one is finished is 25 percent more probable in the TG. However, repetition in high school also is higher in this group, by 13 percent.

In the group of students from medium SES households, a great variety of results appear. On the one hand, students at DS schools show a positive and significant effect in the quality of their postgraduate study. On the other hand, DS schooling has a small but negative and significant impact on the conclusion of high school and on access to tertiary studies. Another negative but stronger effect is on the access to postgraduate studies. The middle SES group is the

36. As Angrist and Pischke (2009) demonstrate, regression estimators differ from matching estimators in the weights used to combine the covariate-specific effects into a single average effect. In particular, while matching uses the distribution of covariates among the treated to weight covariate-specific estimates into an estimate of the effect of treatment on the treated, regression produces a variance-weighted average of these effects. Treatment on the treated estimator puts the most weight on covariate cells containing those who are most likely to be treated. In contrast, regression puts the most weight on covariate cells where the conditional variance of treatment status is largest. The regression and matching weighting schemes therefore differ unless treatment is independent on covariates.

TABLE 8. Average Effects of Double Shift and Standard Errors, Students from Low SES Households[a]
Units as indicated

Outcomes	*OLS*	*SE (robust)*	*ATTK*	*SE (boot-strapped)*
Educational				
Conclusion of high school	0.12*	0.06	0.22*	0.09
Repetition in high school	0.17*	0.08	0.13*	0.07
Second tertiary study if person finished first one	0.16	0.10	0.25*	0.16

*Statistically significant at the 5 percent level.
a. See notes to table 7.

TABLE 9. Average Effects of the Double Shift and Standard Errors, Students from Medium SES Households[a]
Units as indicated

Outcomes	*OLS*	*SE (robust)*	*ATTK*	*SE (boot-strapped)*
Educational				
Conclusion of high school	–0.06	0.04	–0.06*	0.03
Tertiary studies	–0.05	0.05	–0.05*	0.03
Postgraduate studies	–0.13	0.12	–0.44*	0.10
Type of postgraduate studies	0.22	0.34	0.66*	0.19
Conclusion of postgraduate studies	–0.11	0.23	–0.21*	0.11
Income				
Net monthly income of person being surveyed at present	0.03	0.48	1.04*	0.59
Others				
Students' SES at present	–0.36*	0.14	–0.50*	0.12

*Statistically significant at the 5 percent level.
a. See notes to table 7.

TABLE 10. Average Effects of the Double Shift and Standard Errors, Students from High SES Households[a]
Units as indicated

Outcomes	*OLS*	*SE (robust)*	*ATTK*	*SE (boot-strapped)*
Educational				
Repetition in high school	0.27	0.15	0.14*	0.08
Income				
Net monthly income of person being surveyed at present	–0.95	0.92	–1.57*	0.85
Others				
Number of children in current household	–0.07	0.55	1.22*	0.71

*Statistically significant at the 5 percent level.
a. See notes to table 7.

only one to exhibit a positive impact of DS on income but, surprisingly, a negative impact on the current students' SES.[37]

In the case of the high SES group, DS appears to be associated with a higher repetition rate in high school, lower income, and more children in the household.

Per level of SES, the regression and matching estimates tend to have the same sign, except for the number of children in the high SES households.

Discussion and Policy Implications

We have shown that the introduction of longer school days in half of the primary schools of the city of Buenos Aires in 1971 *has significantly improved only one, but very relevant, educational outcome. Students that attended DS primary schools had a secondary school graduation rate 21 percent higher than those that attended single-shift primary schools. Moreover, this result is mainly explained by what happened with the low SES students.* Since the introduction of DS implies an increase of around 40 percent in educational expenditures, a back-of-the-envelope calculation shows that the secondary school graduation expenditure elasticity is 0.5, not a low value, particularly considering that the average income of secondary school graduates is 23.7 percent higher than that of nongraduates (Adrogué 2006). As concerns tertiary and postgraduate educational levels, we have found both positive and negative impacts for DS.

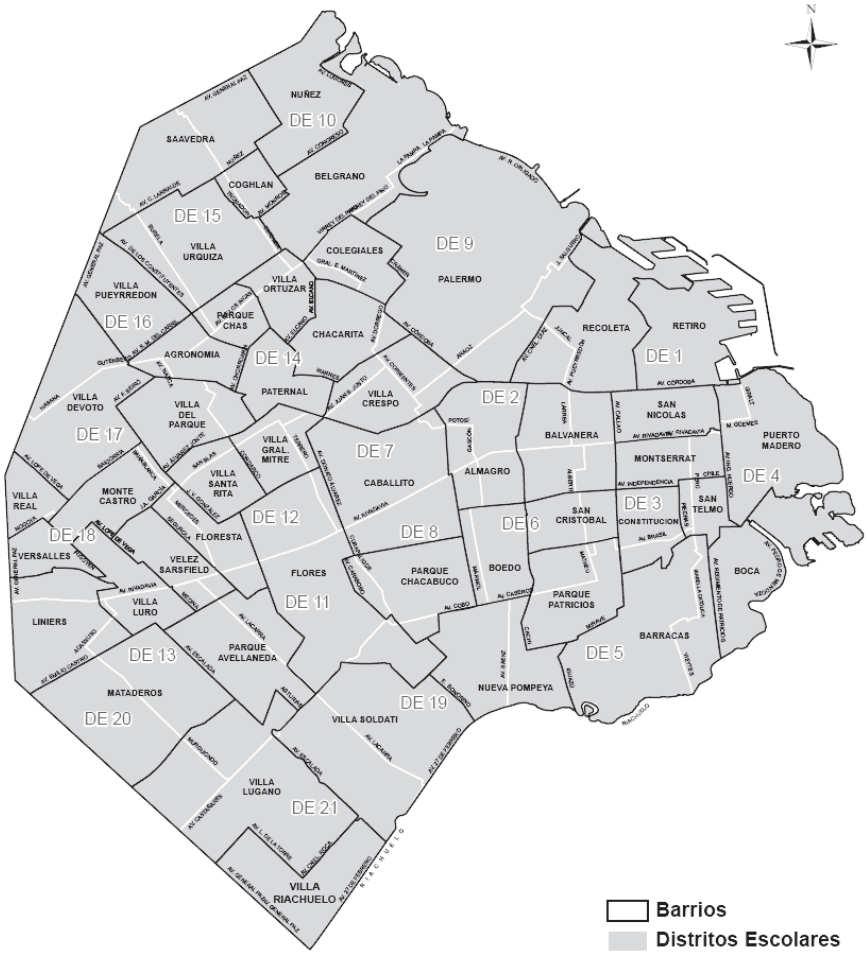
These last results, taken together with the absence of enduring effects of DS on income and employment and with the fact that DS students do not have a better knowledge of a second language, despite having had it as a subject in the school, suggest that the content and learning quality in DS schools was not too good. This is a very relevant finding when the extension of DS to other schools or to the whole educational system is being contemplated. *Our outcomes emphasize that the content of the additional hours is even more important than increasing the duration of the school day.* Just to give an example, the degree of improvement in academic results could be very different depending on whether the extra hours are just an extension of the current curriculum or whether they allow the disadvantaged students to develop their skills and abilities through instruction in and learning of a second language,

37. This is not incompatible because SES is measured only by educational and employment quality variables without including income.

sports, arts, and technologies—the same subjects that their advantaged schoolmates can normally learn and practice.

The general meaning of our results coincides with that of most of the literature reviewed here, even the one study performed with the more demanding methodology of natural experiments. A wide variety of educational policies can have large and relevant impacts on the quantity of education and on low-income populations but not necessarily on the quality of education or other lifelong effects. The main exceptions we found are for high-quality preschool (Schweinhart and others 2005) and, potentially, for class size in primary schools with low SES children (Piketty and Valdenaire 2006). In the first case, the effects are not only important, but also widespread to personality, labor, income, and citizenship outcomes. In the second case, the impact is very strong on test scores. All these results (including this paper's) seem to support the claim made by Piketty and Valdenaire (2006) 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 the effectiveness of this approach will be much greater if it begins from early childhood onwards. However, it also seems clear that more and better research is still required to understand what specific policies are needed to improve the quality of education for the poor.

Appendix A. Neighborhoods and School Districts



Source: Dirección de Investigación y Estadística, Dirección General de Planeamiento Educativo, Mapa Escolar, Ministerio de Educación del GCBA. Ley Orgánica de Comunas 1.777 of 1 September 2005, limits modified by Act 2.650 of 13 March 2008. School district limits were updated in September 2007 according to territorial modifications and changes in the land value and toponymy of the delimiting streets.

TABLE A-1. Ninety-Seven Schools Where Experimental Double-Shift Policy Was Implemented

Name[a]	*School district*	*School number*	*Zip code*	*Shift*[b]
1. Juan José Castelli	1	1	1112	DS
2. Onésimo Leguizamón	1	3	1060	DS
3. Presidente Quintana	1	10	1052	DS
4. Bernardino Rivadavia	1	23	1117	DS
5. Provincia de Jujuy	2	11	1182	DS
6. Presidente Mitre	2	16	1196	DS
7. Tomás Manuel de Anchorena	2	23	1170	DS
8. Provincia de Catamarca (S)	2	24	1215	SS: MA
9. Valentín Gómez	3	1	1099	DS
10. María Sánchez de Thompson	3	3	1071	DS
11. Carlos Pellegrini	3	21	1133	DS
12. Carlos Pellegrini (N, S)	3	22	1133	SS: MA
13. Gral. Viamonte	3	23	1219	DS
14. Alte. Brown (S)	4	1	1161	SS: MA
15. Gral. Aráoz de Lamadrid	4	10	1166	DS
16. República Italiana (N)	4	19	1168	DS
17. Dr. Guillermo Rawson	4	22	1103	DS
18. José Pedro Varela (S)	5	9	1279	SS: MA
19. Fray Mamerto Esquiú	5	13	1277	DS
20. Juan Enrique Pestalozzi	5	18	1264	DS
21. Nieves Escalada de Oromí	5	20	1276	DS
22. Manuel de Sarratea	5	27	1275	DS
23. Olegario Víctor Andrade	6	7	1242	DS
24. Francisco de Gurruchaga	6	10	1247	DS
25. Gral. José María Zapiola	6	13	1215	DS
26. Dr. Antonio Bermejo (N)	6	18	1210	DS
27. República de Colombia	6	26	1256	DS
28. Facundo Zuviría	7	12	1406	DS
29. Dr. Ernesto E. Padilla	7	14	1405	DS
30. Virgen Generala	7	24	1405	DS
31. Juan Agustín Maza (S)	8	10	1424	SS: MA
32. Presidente Marcelo T. de Alvear	8	11	1424	DS
33. Luis Vernet (NF)	8	18		
34. Antonio A. Zinny	8	22	1424	DS
35. Dr. Florián Oliver (N)	8	24	1424	DS
36. República de Cuba (N)	9	1	1414	DS
37. Juan Crisóstomo Lafinur	9	9	1414	DS
38. Capitán Gral. Bernardo O'Higgins (S)	9	15	1426	SS: MA
39. Blas Parera	9	17	1425	DS
40. Carlos María Biedma	9	20	1425	DS
41. Juan Bautista Alberdi	10	5	1428	DS
42. Manuel Dorrego	10	6	1429	DS
43. Joaquín María Cullen (N)	10	10	1429	DS
44. República Dominicana (S)	10	16	1428	SS: MA
45. José Alfredo Ferreira (N)	11	1	1407	DS
46. Benjamín Zorrilla (N)	11	4	1406	DS
47. República Oriental del Uruguay (S)	11	15	1406	SS: MA
48. Gral. Juan Galo Lavalle	11	17	1407	DS
49. Museo de Bellas Artes Gral. Urquiza	12	1	1406	DS
50. Florencio Varela	12	2	1406	DS

TABLE A-1. Ninety-Seven Schools Where Experimental Double-Shift Policy Was Implemented (*Continued*)

Name[a]	*School district*	*School number*	*Zip code*	*Shift*[b]
51. Emilio Giménez Zapiola	12	3	1407	DS
52. Jorge Newbery (N, S)	12	7	1416	SS: MA
53. Remedios E. de San Martín (NF)	12	13		
54. Ernesto Alejandro Bavio	12	16	1407	DS
55. Carlos Calvo (NF)	12	18		
56. Leandro Nicéforo Alem	12	19	1406	DS
57. Prof. José Onaindía	13	1	1407	DS
58. Eduardo Ladislao Holmberg	13	3	1440	DS
59. Carlos Guido y Spano	13	6	1407	DS
60. Dra. Elvira Rawson de Dellepiane	13	18	1407	DS
61. Rubén Darío	14	1	1427	DS
62. Ing. Álvarez Condarco	14	3	1427	DS
63. Carmen Catrén de Méndez Casariego	14	8	1427	DS
64. República del Ecuador	14	10	1416	DS
65. Cabildo de Buenos Aires	14	18	1427	DS
66. NF	15	5		
67. España	15	12	1431	DS
68. Naciones Unidas (S)	15	19	1431	SS: MA
69. Félix de Azara	15	22	1430	DS
70. Grecia	16	3	1419	DS
71. Cnel. Mayor Álvarez Thomas	16	4	1419	DS
72. Luis Pasteur	16	6	1431	DS
73. Congreso de Tucumán	16	11	1419	DS
74. Hilarión María Moreno (S)	16	13	1419	SS: MA
75. Dr. Antonio Dellepiane	17	1	1417	DS
76. Rafael Ruiz de los Llanos	17	4	1419	DS
77. Cap. Juan de San Martín y Gómez	17	5	1417	DS
78. Rodolfo Rivarola	17	17	1417	DS
79. Abel Ayerza	17	23	1419	DS
80. Provincia de Misiones	18	9	1417	DS
81. República del Perú	18	11	1407	DS
82. Manuel Peña	18	15	1407	DS
83. Provincia de Santa Cruz	18	22	1408	DS
84. Provincia de Corrientes	19	5	1263	DS
85. Juan Andrés de la Peña	19	10	1437	DS
86. Maestro Carlos Alberto Carranza	19	20	1437	DS
87. Gral. Félix de Olazábal	20	4	1408	DS
88. José María Torres (S)	20	9	1408	SS: MA
89. República de Filipinas (S)	20	13	1440	SS: MA
90. Islas Malvinas	20	14	1440	DS
91. Dr. Martiniano Leguizamón	20	16	1440	DS
92. Juan Ramón Jiménez	20	20	1440	DS
93. Armada Argentina	21	5	1439	DS
94. Félix F. Bernasconi (NF)	Ins. Ber.[c]	1		
95. Félix F. Bernasconi (NF)	Ins. Ber.	2		
96. Félix F. Bernasconi (NF)	Ins. Ber.	3		
97. Félix F. Bernasconi (NF)	Ins. Ber.	4		

a. N, name changed since policy was applied; S, shift changed since policy was applied; NF, school was not found.

b. Double shift (DS), simple shift (SS), morning and afternoon (MA).

c. "Ins. Ber." is the way this school is listed in the document where the policy was announced. It apparently did not belong to a school district.

Appendix B. Variables Used to Design the Sample

Units as indicated

School district	*Number of schools where policy was implemented*	*Share of all schools where DS policy implemented (percent)*	*SES level*	*Share of 1980 primary enrollment in DS schools (percent)*
1	4	5.3	High	10.6
2	3	3.9	Medium	8.4
3	4	5.3	Medium	4.1
4	3	3.9	Low	3.1
5	4	5.3	Low	2.9
6	5	6.6	Medium	4.7
7	3	3.9	Medium	5.5
8	3	3.9	Medium	5.0
9	4	5.3	High	9.4
10	3	3.9	Medium	8.4
11	3	3.9	Medium	3.7
12	5	6.6	Medium	3.9
13	4	5.3	Medium	3.6
14	5	6.6	Medium	3.3
15	2	2.6	Medium	3.6
16	4	5.3	Medium	3.1
17	5	6.6	Medium	4.7
18	4	5.3	Medium	3.6
19	4	5.3	Medium	5.3
20	4	5.3	Medium	3.1

Appendix C. Descriptive Statistics on Pretreatment and Treatment Variables[a]

Variables	*No. obs.*	*Mean*	*Std. dev.*	*TG mean (I)*	*CG mean (II)*	*Difference in means (I–II)*
Pretreatment						
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	−0.18***
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***
Parents' SES (1: low, 2: medium, 3: high)	380	1.60	0.68	1.69	1.49	0.20***
Type of school (1: boys and girls, 2: girls only, 3: boys only)	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	0.50***
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						
Foreign language as a subject (1: yes, 0: no)	380	0.52	0.50	0.93	0.02	0.91***
Cultural activities (1: yes, 0: no)	380	0.67	0.47	0.67	0.67	0.00
Place where cultural activities occur (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***
Attendance at lunch service (1: yes, 0: no)	221	0.66	0.48	0.70	0.07	0.63***

* Statistically significant at the 10 percent level; **statistically significant at the 5 percent level; ***statistically significant at the 1 percent level.

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