27th Annual Conference
Online

May
June
2021

THE EFFECT OF PRIMARY SCHOOL CONSTRUCTION ON LATER OUTCOMES

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The effect of primary school construction on later outcomes*

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February 9, 2021

Abstract

Improving the later outcomes of children through increasing their school attainment is a key policy priority in developing countries; yet, whether increasing government spending can improve school attainment is still an issue of debate. In this paper, we investigate the effect of a massive primary school construction program—which was launched as part of the 1997 schooling reform—on high school completion and labor force participation rates in Turkey. With this program, Turkey increased the number of primary education classrooms approximately by 31 percent from 1998 to 2002. Using the 2011 Population and Housing Census, we employ an identification strategy based on provincial differences in the intensity of construction program and the variation in exposure across birth cohorts induced by the timing of the program. The estimates suggest that the construction program increased high school completion rates by 2.1-2.4 percentage points for men and by 2.3-2.5 percentage points for women. While the program had no significant effect on male labor force participation, it led to a 2.2-2.6 percentage-point rise in female labor force participation. These findings suggest that the program has been effective in reducing the gender gaps in later outcomes. The results suggest that increasing primary school availability helps reducing gender gaps in later outcomes in a developing country context.

JEL codes: H52; I28; J21.

Keywords: Primary school construction; high school attainment; labor force participation; gender gaps.

*We thank Asena Caner, Meltem Dayioglu Tayfur, Pinar Derin Gure, Burak Gunalp, and seminar participants at Middle East Technical University and TED University for useful comments and suggestions. The views expressed here are of our own and do not necessarily reflect any third parties. All errors are ours.

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1 Introduction

These days, education research in developed countries tends to center on quality (Hanushek, 2002). However, Turkey and many other developing countries remain focused on attainment. In 2018, the share of 25-64 year-olds in Turkey with at least upper secondary education was 40.2 percent compared to the OECD average of 78.8 percent. In the past 20 years, Turkey has dramatically increased the resources devoted to boosting its average level of schooling. Nonetheless, few studies have analyzed the causal impact of these large governmental programs.

This study examines the spillover effect of Turkey's large-scale primary school construction program on high school attainment and labor force participation. The government launched this program in 1997, as part of that year's compulsory schooling law. Attended by high political expectations, the law passed quickly through parliament. It was widely perceived that the major motivation for the law was to restrict religious education by closing the three-year lower secondary Imam Hatip schools (Gunay, 2001; Pak, 2004). It was different from other traditional compulsory schooling laws because it did not increase the legal dropout age, which remained 15, or extend the duration of compulsory schooling. Instead, it introduced an uninterrupted eight-year education in the same school building. Before the 1997 law, basic compulsory education meant a five-year primary education and three-year lower secondary education predominantly in separate schools. Crucially, three-year lower secondary schooling was rarely enforced due to a lack of physical infrastructure (MONE-Baskent University, 2002; Dulger, 2004).

The implementation of this law thus required a major expansion of school infrastructure. First, 1.5 million out-of-school lower-secondary children needed to be schooled (MONE, 1996). Second, the implementation of the law resulted in unutilized school facilities due to the closure of many five-year primary schools and three-year lower secondary schools within high schools. For instance, by the 2002–2003 school year, over 15,000 primary schools were closed down in small villages (MONE, 1996, 2003).

This expansion requirement led to a government strategy focused on building new classrooms. With help from private contributions, the government increased the number of classrooms by 67,014 from 1998 to 2002 (an average of 6.2 classrooms per 1,000 children aged 6 to 13) — an approximately 31 percent increase in classrooms over this period (MONE, 1999, 2000, 2001, 2002, 2003).

Following Duflo (2001), in this study, we employ a difference-in-differences strategy that exploits provincial differences in the intensity of the construction program and the variation in exposure across birth cohorts resulting from program timing. The intensity of the construction program refers to the net increase in the number of classrooms from 1998 to 2002 per 1,000 children aged 6–13. The analyses rely on a unique dataset generated by combining the 2011 Population and Housing Census (PHC) and provincial educational data from National Education Statistics books published by the Ministry of National Education (MONE). The year of birth and the province of birth jointly determine an individual's exposure to the school construction program. After controlling for birth province and cohort fixed effects, the coefficients of interactions between cohort dummies and the net increase in the number of classrooms in primary education are plausibly exogenous variables because the timing of the policy was driven by political choices.

The analysis indicates that primary school construction has significant spillover effects on high school attainment for both men and women. The addition of one extra classroom of basic education per 1,000 children aged 6–13 increased high school attainment rates by 0.34–0.38 percentage point for men and by 0.37–0.40 percentage point for women for those exposed to the program. Since the program added 6.2 classrooms per 1,000 children from 1998–2002, the total increase in high school attainment rates was 2.1–2.4 percentage points for men and 2.3–2.5 percentage points for women.

Our findings also suggest that the construction program impacts only female labor force participation. The program's additional investments in educational infrastructure led to a 0.35–0.42 percentage point rise in female labor force participation for every additional classroom. The

addition of 6.2 classrooms in basic education per 1,000 children resulted in an increase of 2.2–2.6 percentage points in female labor force participation. The program did not significantly affect male labor force participation.

These findings have important implications for the literature on the impact of school construction on educational attainment in developing countries. This study is the first empirical analysis focusing on school construction programs in Turkey. Crucially, we isolate the causal effect of the construction program by also accounting for other governmental programs in relation to the compulsory schooling law of 1997 — such as programs for boarding schools and school transportation.

Research on evaluating the causal impact of large educational investment programs in developing countries remains limited. Duflo (2001) and Li and Liu (2014) examine the impact of primary school construction programs in Indonesia and China, respectively. Both studies show that these programs have some spillover effects on lower secondary school attainment. The common point for all three studies (ours included) is that large school construction programs often affect not only the attainment rates of the targeted level of schooling but also that of the subsequent education level.

This paper also adds to the literature examining the role of school construction in improving gender equality in the labor market for developing countries. As the level of educational attainment rises, female labor force participation increases in Turkey, as in most developing countries. This study indicates that Turkey's school construction program boosts labor force participation only for women, not men.

Importantly, this study is one of the few empirical analyses to use the 2011 Population and Housing Census data. The richness of the 2011 data enables more reliable estimates compared to other compulsory schooling law of 1997 studies using household surveys. While household surveys in Turkey provide statistics at 26 NUTS 2 regions level, the census provides at 81-province level. Thus, the census provides more robust standard errors when the standard

errors are clustered on the place of birth.

However, the study has a limitation to estimate the exact magnitude of the effects on the labor force participation. The 1992 and 1993-born cohorts in the treatment group recently completed their high schools or some individuals in these two cohorts were more likely to continue their college education at the time of census survey being conducted in 2011. Our estimates for labor force participation must therefore be seen as early outcomes.

The rest of the paper proceeds as follows. Section 2 discusses the literature. Section 3 provides information about the compulsory schooling laws in Turkey, the school construction program, and statistics about school enrollment rates. Section 4 discusses the data and the identification strategy. Section 5 exhibits the empirical findings of the construction program on educational attainment. Section 6 presents the estimates of the program effect on labor force participation. Section 7 provides robustness checks for the empirical findings. Section 8 presents a conclusion.

2 Literature Review

In this paper, we examine the effect of a large-scale school construction program on high school attainment and labor force participation. Following Duflo (2001), we employ a difference-in-differences strategy in our analysis that exploits provincial differences in the intensity of the construction program and the variation in exposure across birth cohorts induced by the timing of the program.

Duflo (2001) investigates a dramatic change in education policy that launched a major primary school construction program to target the children who had not previously been enrolled in Indonesia. Duflo finds some spillover effects for the lower secondary schooling. Similarly, Li and Liu (2014) examine the impact of constructing primary schools in rural areas of China in the late 1980s and early 1990s. They find that increasing primary school availability has significant positive spillover effects on girls' lower secondary school attainment.

There has been an extensive literature that studies on the effects of different programs or

natural experiments following the same strategy. Some of them are as follow: the effect of a large-scale construction of pre-primary school on pre-primary school attendance and labor supply in Argentina (Berlinski and Galiani, 2007); the impact of female education on fertility exploiting the Nigeria's Universal Primary Education program (Osili and Long, 2008); the effects of violent conflict on schooling outcomes exploiting temporal exposure to the 1992–98 armed conflict in Tajikistan (Shemyakina, 2011); the effect of Turkey's 30-year-long "internal armed conflict" on the educational attainment (Berker, 2012); the effects of education policy on early fertility in Sweden (Gronqvist and Hall, 2013); the effect of wars and armed conflicts on children in terms of the educational attainment, future health, and labor market outcomes (Akbulut-Yuksel, 2014).

The impact of the 1997 Turkish compulsory schooling law has been widely analyzed by researchers in Turkey. Kirdar, Dayioglu, and Koc (2011) estimate the impact of the law on marriage and fertility of teenage women in Turkey. Their methodology is based on the discontinuity of the time trends in schooling outcomes by comparing the birth-cohorts that were affected by the law with those that were not. They find that the compulsory education policy substantially reduces the probability of marriage and childbirth for teenage women. In another study, Kirdar, Dayioglu, and Koc (2016) investigate the impact of the law on schooling disparities by gender and rural/urban residence using a similar strategy. They find that the compulsory schooling law substantially equalizes the educational attainment of urban and rural children, but they find no evidence of a narrowing gender gap. Caner, Guven, Okten, and Sakalli (2016) use the 1997 law to examine the effect of the law on mothers' traditional views in determining children's educational attainment. They find that the law is helpful to reduce school dropout rates.

There have also been a number of studies using the Turkish compulsory schooling law of 1997 as an instrument and investigating causalities between education and other outcomes. Torun (2015) and Aydemir and Kirdar (2017) estimate the returns on schooling. Cesur and Mocan (2013) estimate the effect of education on religiosity and voting preference. Gulesci and Meyersson (2015) estimate the impact of education on religiosity and women's empowerment.

In these studies, the instrument is basically a policy dummy, which is equal to one for those who affected from the compulsory schooling law of 1997.

3 The School Construction Program

3.1 Compulsory Schooling Laws in Turkey

From the beginning, the Turkish Republic mandated five-year primary education as compulsory. In 1973, Turkey extended the compulsory education to eight years ("Milli Egitim Temel Kanunu" 1973), but the law required that the compulsory schooling consists of two different parts: primary school (five years) and lower secondary school (three years). Students received diplomas at the end of each level. The law also stated that schools for two compulsory schooling levels could be constructed independently or together depending on the facility conditions.

However, the compulsory three-year lower secondary schools were not enforced fully by the governments due to the lack of physical infrastructure before 1997 (MONE-Baskent University, 2002; Dulger, 2004). On the other hand, the period of 24 years between 1973 and 1997 can be thought as a transitional period including a variety of programs for the preparation of the eight-year compulsory education in the community (MONE-Baskent University, 2002; Dulger, 2004).

In 1996, the Welfare Party won the election and emerged as a principal partner in the coalition government. However, this party was seen as an Islamist party by the military and other key state institutions in Turkey (Onis, 1999). After one year, an indirect military intervention forced the collapse of the coalition government (Onis, 1999). The Turkish parliament soon passed a new compulsory schooling law (4306) requiring eight years of uninterrupted secular education. This was one of the main recommendations of the National Security Council, which was dominated by the military. This council announced 18 recommendations designed to stop the perceived growth of Islamism in Turkey on February 28, 1997 (Gunay, 2001).

The compulsory schooling law of 1997 is different from traditional compulsory schooling laws as it did not increase the legal dropout age, which remained 15, or extend the compulsory schooling duration. Instead, it introduced a continuous uninterrupted eight-year education in a single building. In addition, this new law abolished primary schooling and lower secondary schooling diplomas and instead introduced eight-year primary education diploma.

This legislation was widely perceived as targeting the Imam Hatip three-year lower secondary religious schools, which were subsequently closed in line with this requirement because some claimed that these schools, although state sponsored, were dominated and used effectively as indoctrination centers by Islamists and seen as a potential threat to the secular establishment (Gunay, 2001; Pak, 2004). Therefore, although the experts warned that there would not be enough available facilities for an uninterrupted eight-year education implementing in a single building, the government applied a "big bang" approach to introducing the reform and wanted to implement it as widely and rapidly as possible, rather than a step-by-step approach, which ran the risk of creating the political targets (Dulger, 2004). Thus, the government initiated a new school construction campaign relying on the temporary earmarked taxes (Law: 4306).

The closing down of three-year lower secondary religious schools triggered a big debate in Turkey. The government thus closed down all vocational three-year lower secondary schools by showing that it did not target only religious Imam Hatip lower secondary schools.

Fifteen years after introducing eight years of compulsory education, Turkey increased its compulsory education to twelve years in 2012. It also changed the structure of compulsory schooling from an uninterrupted eight-year basic education plus four years of high schools to three 4-year length components: primary school, lower secondary school, and high school. With this new structure, 4-year lower secondary religious Imam Hatip Schools were re-introduced into the education system. It will be beneficial then to consider briefly what the upcoming results can be expected in the light of this study.

3.2 The School Construction Program

Implementing an uninterrupted eight-year compulsory education in the same building would require a major expansion of school infrastructure. There were a couple of reasons for this expansion requirement. First, there were approximately 1.5 million out-of-school lower-secondary school children that needed to be schooled (MONE, 1996). Second, the implementation of the law led to unutilized school facilities due to the closure of the five-year primary schools in small villages and the closure of the lower secondary schools within high schools. For instance, by the 2002–2003 school year, over 15,000 primary schools were closed down in small villages (MONE, 1996, 2003). This expansion requirement led to a government strategy that focused on building new schools.

A temporary set of earmarked taxes was used to finance the school construction program. These new taxes raised $2 billion in new revenues to support the construction program (Dulger, 2004). With the encouragement of the government, the private sector also provided substantial donations. Annual expenditures for the program were estimated at over $3 billion for the first four years (Dulger, 2004).

Through a combination of government resources and private contributions, the government increased the number of classrooms by 67,014 between the 1998–1999 and the 2002–2003 school years (an average of 6.2 classrooms per 1,000 children aged 6 to 13). This corresponds to approximately a 31 percent increase in the number of classrooms from 1998 to 2002. Approximately 70 percent of this increase was occurred in urban areas due to the closure of primary schools in small villages (MONE, 1999, 2000, 2001, 2002, 2003).

It is expected that the program mainly targeted children who had not been schooled. The allocation rule for the construction program among the provinces was on the basis of schooling age children and non-enrollment (Dulger, 2004). Table (1) indicates a regression of the logarithm of the net increase in the number of classrooms in each province on the logarithm of the number of children and the logarithm of the non-enrollment rate. The rule implies that both coefficients would be close to 1 (Duflo, 2001).

Table (1) shows that the coefficient of the number of children is very close to 1. However, the coefficient of the non-enrollment rate is not significant and also not the expected sign. We also ran the regressions for the urban (predominantly urban and intermediate provinces) and rural (predominantly rural) provinces separately based on the OECD definition: the coefficient of the non-enrollment rate is positive but not significant for urban provinces, while it is negative for rural provinces. The reason could be that over 15,000 rural schools were closed down after the 1997 law, making the correlation negative after controlling for the number of children.

In addition to the school construction program, two other programs were implemented to improve access for children in rural areas. These were the school transportation program and the establishment of boarding schools. With the transportation program, students in small villages where primary schools had been closed were transferred daily from their homes to the primary education schools in nearby villages. If the distances were too great, children were boarded at free regional Boarding Primary Education Schools (YIBO). These schools boarded only their own students. In addition to this opportunity, children could also be accommodated in Primary Education Schools with Pension Housing (PIO), which boarded students attending different students and were available in relatively larger towns (Dulger, 2004).

Figure (1) presents the statistics of these two programs. The number of students transported increased sharply after the law introduced in 1997. It almost doubled in between 1997 and 2002 and increased from 281,833 to 654,093. The number of students enrolled within Boarding Schools Program also doubled in this period and it increased from 131,458 to 279,791 between 1998 and 2002. After 2002, the level of students benefited from the two programs remained almost the same.

3.3 School Enrollment Rates in Turkey

The Ministry of Education has provided net enrollment rates since 1994. Figure (2) represents these rates for compulsory schooling between 1994 and 2006. Before the 1997 law, the rate was separate for five-year primary and three-year lower secondary schools. After the law, the data has been provided for eight-year primary education. We also computed an approximate

net schooling rate for primary education for 1994, 1995, and 1996 based on the net enrollment rate and the number of students enrolled in these years.

Figure (2) clearly indicates that the law and implementing educational programs increased the net enrollment rate from approximately 75 percent in 1996 to 91 percent in 2002, an increase of 1.3 million students rise in primary education (MONE, 2007). The Figure also shows that net enrollment increased to 95 percent in 2000, but fell to 91 by 2002. Investment from the Ministry of National Education in Total Public Capital Investments from 1995 to 2004 follows a similar trend (see Figure (5)). That may show that net enrollments increase with more resources devoted to education. In addition, Figure (2) shows that both five-year primary schools' and three-year lower secondary schools' enrollment rates had not changed in the three years prior to 1997. Therefore, the law and implementing educational programs are the main drivers behind the enrollment increase after 1997.

Figure (3) plots the high school net enrollment rate. The law was applied in August 1997, it is thus expected that the spillover effect would be seen with the 1998-1999 school year. Indeed, the high school net enrollment rate started to increase in 1998 and rose to 57 percent in 2006 from 38 percent in 1997. Similarly to compulsory schooling rates, high school net enrollment rates had not changed in the four years prior to 1998.

4 Data and the Identification Strategy

4.1 Data

The study relies on a unique dataset generated by combining the 2011 Population and Housing Census (PHC) and provincial educational data from the National Education Statistics books published by MONE. The census dataset includes a large set of standard individual characteristics including province of birth, province of residence, year of birth, educational attainment, labor force condition, and other variables.

According to Turkish Statistical Institute (TURKSTAT, 2013), the 2011 census survey was

based on Address Based Population Registration System (ABPRS) and National Address Database registers and involved face-to-face interviews, with netbooks and paper-based questionnaires for data collection. Some 195,192 enumeration areas consisting of 100 dwellings were defined. Subsequently 22,861 sample enumeration areas (11.7 percent) were selected and 2.2 million households were interviewed. In all, about 9 million people were interviewed, with October 2, 2011, accepted as the reference day.

In this study, we focus on individuals born between 1971 and 1993. This identification ensures that individuals in the sample completed high school. Table (2), panel A presents summary statistics for this sample. There are 2,872,593 individuals in the sample. We match the individual census data with the provincial level data using information on the province of birth. In 1998, Turkey had 80 provinces. The net increase in the number of classrooms in each province between 1998–2002 and other provincial data are provided from the National Education Statistics Data books published by MONE. However, the number of primary education schools' classrooms is not available in the 1998 MONE book, so the total number of classrooms for this year is provided from the Ministry of Education. For the population of children by age groups, the 1990 or 2000 census can be used. We choose the closest census to 1997, which is the 2000. Enrollment rates are computed by the number of children enrolled in the school year 1997–1998 to the 2000 census' children aged 6–13, which is primary school age in Turkey. Table (2), panel B presents provincial level summary statistics.

4.2 Identification Strategy

For the 1997–1998 school year, Turkey began to implement a nationwide compulsory schooling law that introduced an eight-year uninterrupted primary education. This law passed very quickly and rather unexpectedly. The timing of the policy was driven by political choices and was thus independent of potential returns to educational attainment. Implementing an uninterrupted eight-year compulsory education in a single building created a major deficiency in the number of school classrooms after 1997. Thus, the government started a big school construction campaign by financing a temporary additional tax law and also encouraging pri-

vate donations. We are exploiting this exogenous political development to study the spillover effects of the primary school construction program by focusing on high school attainment.

The school construction campaign started in August 1997 with the law's passage in parliament. We would expect that at least one year later, we would see an exogenous increase in the number of classrooms in primary schools. This is confirmed in Figure (4), which represents Turkey's total number of primary education classrooms from the 1997–1998 school year through the 2004-2005 year, from MONE statistics books. In the first year of the law, the net increase in the number of classrooms was about 2,338 — a 1 percent increase from 1997. The next year saw 31,715 new classrooms — a 15 percent increase. As the figure shows, the increase levels off after 2002.

This exogenous increase can also be observed from Figure (5), which shows the share of the investment of the Ministry of Education in total public capital investments. In 1998, the share of investment of the Ministry jumps 23 percentage points. This increase comes back to its previous position in 2003. Dulger (2004) claims that in the first four years of the program, the government spent about $2 billion more than its planned expenditure to accelerate its construction campaign. Therefore, we take the 1998–2002 period as the exogenous increase in the primary school construction program.

The year of birth and the province of birth jointly determine an individual's exposure to the school construction program. Variation in program exposure across children occurs both because provinces differ in the timing of the program and because children within provinces differ in their dates of birth (Rosenzweig and Wolpin, 1986; Duflo, 2001). This identification strategy exploits variation in educational investment across provinces and cohorts that benefited from the program.

The 1988-born cohort is the first cohort definitely affected by the investment program. The 1997 law abolished the five-year primary school diploma and replaced it with an eighth-grade diploma for incentive. Therefore, the 1988-born cohort was forced to remain in school after 5th

grade because only 0.1 percent of this age cohort has only a five-year primary school diploma (see Table (3)). The 1987 and 1986 cohorts were similarly affected.

On the other hand, the 1982-born cohort did not benefit from the program. They were 14–15 years old in June 1997 and had moved on from the eight-year primary education. This can also be confirmed from students enrollment in the 1998–1999 school year in primary education. Table (4)) indicates that the percent of students enrolled in this year and born in 1982 or before was only 0.4 percent. Accordingly, cohorts born in 1982 and before did not benefit from the program, the cohorts born from 1983–1987 were partially affected, and those born in 1988 and after were completely affected.

The intensity of the program is defined as the net increase in the number of classrooms in primary education between 1998–2002 for 1,000 children aged 6–13, and it substantially varies across provinces (see Figure (6)). This is the second source of variation for educational attainments among individuals. The provinces with red and orange color saw the highest increase in the number of classrooms, whereas provinces with dark green saw the lowest increase.

5 Effect on High School Completion

5.1 Basic Results

One of the objectives of this study analyzes the impact of the school construction program on high school attainment. The variation in treatment intensity across provinces and born cohorts is exploited. We compare the difference between high school attainment of a young cohort exposed to the program (1988–1993 born cohorts) and that of an older cohort not exposed to the program (1977–1982 born cohorts). If additional classrooms led to an increase in educational attainment, the difference between two cohorts will be positively related to the additional number of classrooms in each province. Following Duflo (2001), we run the

following regression:

$$S_{ijc} = \alpha + \delta_j + \gamma_c + \beta(p_c \cdot d_{ic}) + (X_j \cdot d_{ic})\Pi + \varepsilon_{ijc} \tag{5.1}$$

- S_{ijc} is a dichotomous variable indicating whether the individual i born in province j in year c has at least completed high school
- δ_j is a province of birth dummy variable
- γ_c is a cohort dummy variable
- p_j denotes the intensity of the construction program in province j
- d_{ic} is a dichotomous variable that 1 indicates the year c to be between 1988–1993 and 0 indicates the year c to be between 1977–1982
- X_j is a vector of province-specific variables
- ε_{ijc} is the error term

The province of birth dummy variables control for cohort invariant province-specific unmeasured variables. The cohort dummy variables control for nationwide changes specific to cohorts and the country-level trends in schooling. It is also important to control for the number of children who are at the primary education age group and the pre-program enrollment rate since these are the determinant variables of the program.

The first-order effect of a higher enrollment rate in 1997 is a difference in level of education, which should affect all cohorts identically, and thus be captured by the province of birth fixed effect. However, changes in enrollment rates within in a province can be correlated with levels, thus controlling for the enrollment rate in 1997 interacted with year dummies is important only in this context (Duflo, 2004). In all specifications, we choose the linear probability model due to easy interpretation and the presence of a huge data set.

The cross correlation among the error terms of all individuals in a given province experiencing the same shocks may bias the standard errors downward (Moulton, 1986; Bertrand, Duflo, and

Mullainathan, 2004). Thus, in all specifications of this study, all standard errors are clustered by the individual's province of birth. We also alternatively apply two-way clustering on the individual's province of birth and year of birth using cgmreg command in Stata, but we do not cluster the standard errors on the province-year pairs. As Cameron and Miller (2015) point out that clustering should not be on province-year pairs because, for example, the errors for individuals born in Istanbul in 1990 are likely to be correlated with the errors for individuals born in Istanbul in 1991. In the two-way clustering method, three different cluster-robust variance matrices are obtained for the estimator: two one-way clustering and the intersection of the first and second. The two one-variance matrices are added and subtracted from the intersection for double counting (Cameron and Miller, 2015).

Table (5) displays the estimates obtained from equation (5.1) for the whole sample as well as for men and women separately. All control variables also changed with respect to gender identity. In panel A, we compare individuals born in 1988-1993 with individuals born in 1977–1982. Column (1) has the estimates of the effects of the construction program controlling for the interactions between the year of birth dummies with two control variables: the number of children and the pre-enrollment rate in the province of birth. This specification shows that in the whole sample, the addition of one extra classroom for 1,000 children increased the high school attainment rate by 0.37 percentage point for individuals exposed to the construction program. Since the total addition is 6.2 classrooms per 1,000 children during 1998–2002, the total effect of the program was a 2.3 (=6.2*0.37) percentage points increase in the high school attainment. The program had statistically similar effects on men (column 5) and women (column 9).

The regression estimates are based on two assumptions. First, the evolution of completing high school across birth cohorts would not have varied systematically across provinces in the absence of the construction program (Besley and Case, 2000; Duflo, 2001). Crucially, the program is an exogenous shock to the education system and independent of potential returns to educational attainments. This assumption is tested in Panel B of Table (5) in which we compare individuals born in 1977–1982 with individuals born in 1971–1976. If, before the

construction program was started, high school attainment increased faster in provinces that obtained more classrooms by the construction program of 1997, panel B would show positive significant coefficients. However, the effect of the increase of one extra classroom for 1,000 children is very small and never significant. These coefficients in panel B are statistically different from those coefficients in panel A. This identification assumption is more explored in the next sub-section. All these results provide some suggestive evidence of the strategy that we follow in this analysis is reasonable in estimating the causal effect of the construction program.

The second assumption of the regression estimations requires that there are no omitted time varying and province-specific effects correlated with the construction program. This assumption can be violated without controlling for the allocation rules of the construction of the primary schools to each province. In that case, the estimate can potentially confound the effect of the program with mean reversion that has occurred even in its absence (Besley and Case, 2000; Duflo, 2001). Thus, in all specifications, we include the interactions between the year of birth dummies with two control variables of the determinant of the construction program: the number of children and the pre-enrollment rate in the province of birth.

This identification assumption is also violated if other governmental programs in relation to the compulsory schooling law of 1997 correlated with the construction of the primary schools to each province, such as programs for boarding schools and school transportation. For these programs, we interact the year of birth dummies with the net increase in the number of students who benefited from these programs for 1,000 children between 1998 and 2002. Due to separate data availability for men and women, we take the period for school transportation program as 1999–2002. Controlling for these programs (columns 2 and 3; 6 and 7; 10 and 11) does not significantly change the estimates. The two-way clustering method increases the standard errors, but the coefficients are still significant (column 4, 8, and 12).

In this analysis, we match the individual census data with the provincial level data using information on birth province. Unlike the province of education, the province of birth is not

endogenous with respect to the construction program because all individuals in our sample were born before the construction program started. The sample data does not have the province of education, however, we run the regressions for individuals whose province of birth and province of residence are the same, and we get significantly similar results. That shows that the estimates are robust to the interprovincial migration due to the construction program.

5.2 Reduced-Form Evidence

The identification assumption can also be tested with a generalized interaction terms analysis as follows (Duflo, 2001):

$$S_{ijc} = \alpha + \delta_j + \gamma_c + \sum_{c=1972}^{1993} \beta_c (p_c \cdot d_{ic}) + \sum_{c=1972}^{1993} (X_j \cdot d_{ic}) \Pi_c + \varepsilon_{ijc} \tag{5.2}$$

- S_{ijc} is a dichotomous variable indicating whether the individual i born in province j in year c has at least completed high school
- δ_j is a province of birth dummy variable
- γ_c is a cohort dummy variable
- p_j denotes the intensity of the construction program in province j
- d_{ic} is a dummy that indicates whether individual i was born in year c
- X_j is a vector of province-specific variables
- ε_{ijc} is the error term

In an estimation of equation (5.2), individuals born in 1971 serve as the control group. Each coefficient of the interaction variable between the cohort dummy and program intensity (β_c) can be interpreted as an estimate of the effect of the program (the increase of one extra classroom for 1,000 children) on the given cohort. These estimations provide a robustness check for the identification assumption. We claim that the cohorts born in 1982 and before did not benefit from the program, the cohorts born from 1983–1997 were partly affected, and the cohorts born in 1988 and after were definitely affected by the construction program. Thus

we would expect the exposure of the program is increasing with cohorts of children born after 1982.

Figure (7) draws β_c coefficients with a 95-percent confidence interval by broken lines. The coefficients for the cohorts born before 1982 fluctuate around 0. However, after 1982, the effects of the program start to increase and all coefficients are significantly different from 0 except the 1991-born cohort. This systematic increase in the figure indicates that the increase in high school attainment is most likely due to the program. Thus, our identification strategy seems reasonable.

6 Effect on Labor Force Participation

Female labor force participation in Turkey is particular low. It was 29 percent in 2011, compared to 72 percent for males. Among sectors, agriculture has the highest female employment: 29 percent in our sample (individuals born between 1971 and 1993). The agricultural sector is dominated by small-scale family run establishments, and thus, the women employed in this sector are predominantly unpaid family workers (SPO and Bank, 2009; Uraz, Aran, Husamoglu, Okkali Sanalmis, and Capar, 2010; Dayioglu and Kirdar, 2010). Furthermore, these studies point out that female labor force participation is weakly associated with educational attainment in rural regions. Therefore, we exclude the agriculture sector in our sample to get more reliable results for the impact of the construction program on labor force participation.

We also regress the labor force participation (a dichotomous variable whether an individual participates the labor force) on the educational attainment controlling for province of birth and year of birth dummies to provide a picture for the associations between two variables. In these regressions, we omit individuals who have no lower secondary school diplomas and make them as reference groups. Table (6) shows that the relation between labor force participation and educational attainment is low for men, whereas, this relation is very high for women. Getting a high school diploma increases the associations between female labor force participation and educational attainment by 15 percentage points.

6.1 Basic Results for Labor Force Participation

The impact of the program on the labor force participation excluding agriculture sector is found by replacing the dependent variable S_{ijc} in the equation (5.1) with W_{ijc} (is a dichotomous variable indicating whether the individual i born in province j in year c is in the labor force in 2011).

Table (7) presents estimates of the regressions. In panel A, we compare individuals born in 1988–1993 with individuals born in 1977–1982. These estimates show that the construction program affects only women. All total sample and men's coefficients are not significantly different from 0. The addition of one extra classroom for 1,000 children lead to a 0.35–0.42 percentage point rise in female labor force participation (excluding agriculture sector). Thus, the total addition of 6.2 classrooms in basic education per 1,000 children makes the total effect range from 2.2 (=6.2*0.35) to 2.6 (=6.2*0.42) percentage points increase in female labor force participation. We also check the results by running the regressions for individuals, whose province of birth and province of residence are the same, and we get significantly similar estimates.

In panel B, we present the control experiment by comparing individuals born in 1977–1982 with individuals born in 1971–1976. The interaction coefficients are small and not significantly different from zero in all specifications.

6.2 Reduced-Form Evidence for Labor Force Participation

The identification assumption can also be tested with a generalized interaction terms analysis for the labor force participation. We follow a similar strategy in 5.2 and plot the coefficients of the interactions cohort dummies with the program intensity in Figure (8). In this estimation, the dependent variable S_{ijc} in the equation (5.2) is replaced with W_{ijc} (is a dichotomous variable indicating whether the individual i born in province j in year c in the labor force in 2011).

In this estimation, individuals born in 1971 serve as the control group. There is no systematic

increase for the cohorts born before 1982 and these coefficients fluctuate around 0. However, after 1982, the effects of the program start to increase and all coefficients of the cohorts born from 1986–1992 are significantly different from 0. However, the effects of the program significantly decrease with the 1992-born cohort due to the fact that the 1992 and 1993-born cohorts recently completed their high schools or some individuals from these two cohorts would still be in college.

7 Robustness Checks

7.1 Quality Bias

Estimates of the impacts of the program are biased if the construction program also affects the quality of education. With the program, approximately 70,000 new primary school teachers were recruited between 1998 and 2002. According to Dulger (2004), taking a "big bang" approach for applying the law, however, the government missed opportunities to make incremental changes that could have supported the program. After the law was passed, the five-year primary school and three-year lower-secondary general education curricula were joined. Cancelling all vocational and religion course electives made the curriculum a standard package for all students and there was no attention to upgrade the national curriculum (Dulger, 2004).

Previous studies (Card and Krueger, 1992; Altonji and Dunn, 1996) use teacher/student ratio, average length of school term, average annual teacher salary, and total expenditures per student as school quality measures. The teacher/student ratio and the average teacher salary account for the majority of variations in total expenditures per student (Card and Krueger, 1992). In the period from 1998 to 2002, the average length of school term and average annual teacher salary did not significantly change. Yet teacher/student ratio declined from 28.6 to 25.4 because of recruiting more teachers (see Table (2)). Reduction in this ratio could improve the quality of classroom instruction. We check the results by adding the net increase in the average student/teacher ratio between 1998 and 2002 as a control variable in the regressions. New estimations are presented in Table (8). It clearly indicates that the effects of the program

do not significantly change with adding this control variable (Column 2 and 4). Therefore, there is no suggestive evidence that the construction program significantly changes the quality of education.

7.2 Construction of High Schools from 1998 to 2002

The total number of classrooms in high schools increased 9,124 between the 1998–1999 and the 2002–2003 school years. This corresponds to about 13 percent increase in the number of classrooms. However, we only have data for the period of 1999–2002 at the provincial level. The increase in this period is about 6,288. We check the main estimates by adding the net increase in the number of classrooms in high schools between 1999–2002 for 1,000 children aged 6–13. The estimates are presented in Table (9). Estimates for high school attainment rate and female labor force participation are highly significant and similar to our baseline estimates in terms of magnitude. Clearly, our results are robust to the construction of high school in the same period of construction of primary schools.

8 Conclusion

In developing countries, the level of basic education is still low and gender equality needs to be addressed. Therefore, improving education is often a priority. Turkey has increased education funding dramatically over the last twenty years. In this paper, we evaluate the causal impact of a large-scale primary school construction program on high school attainment and labor force participation. This program launched in 1997 as part of the compulsory schooling law of 1997.

Through a combination of government resources and private contributions, the government increased the number of classrooms by 67,014 between the 1998–1999 and the 2002–2003 school years (an average of 6.2 classrooms per 1,000 children aged 6 to 13). This corresponds to approximately a 31 percent increase in the number of classrooms from 1998 to 2002. The program increased the high school attainment rate by 2.1–2.4 percentage points for men and by 2.3–2.5 percentage points for women. Furthermore, the program led to a 2.2–2.6 percentage

points increase in female labor force participation, excluding agriculture. The program did not significantly affect male labor force participation. Therefore, the program has improved gender equality in the labor market.

In 2012, Turkey increased its compulsory education from eight to 12 years and also changed the structure of the compulsory schooling from an uninterrupted eight-year to three 4-year components: primary school, lower secondary school, and high school. It is likely that increasing compulsory schooling and constructing high schools programs will improve Turkey's average level of schooling dramatically in the future. Based on Duflo (2001), Li and Liu (2014) and our findings, we conclude that the new programs will have spillover effects on college attainment. Due to the high correlation between college attainment and female labor force participation in Turkey, these developments will further address gender inequality in the labor market. Evaluating programs of the new increase in compulsory schooling, the construction of high schools, and the re-introduction of 4-year lower secondary religious Imam Hatip Schools will be the objects of future works.

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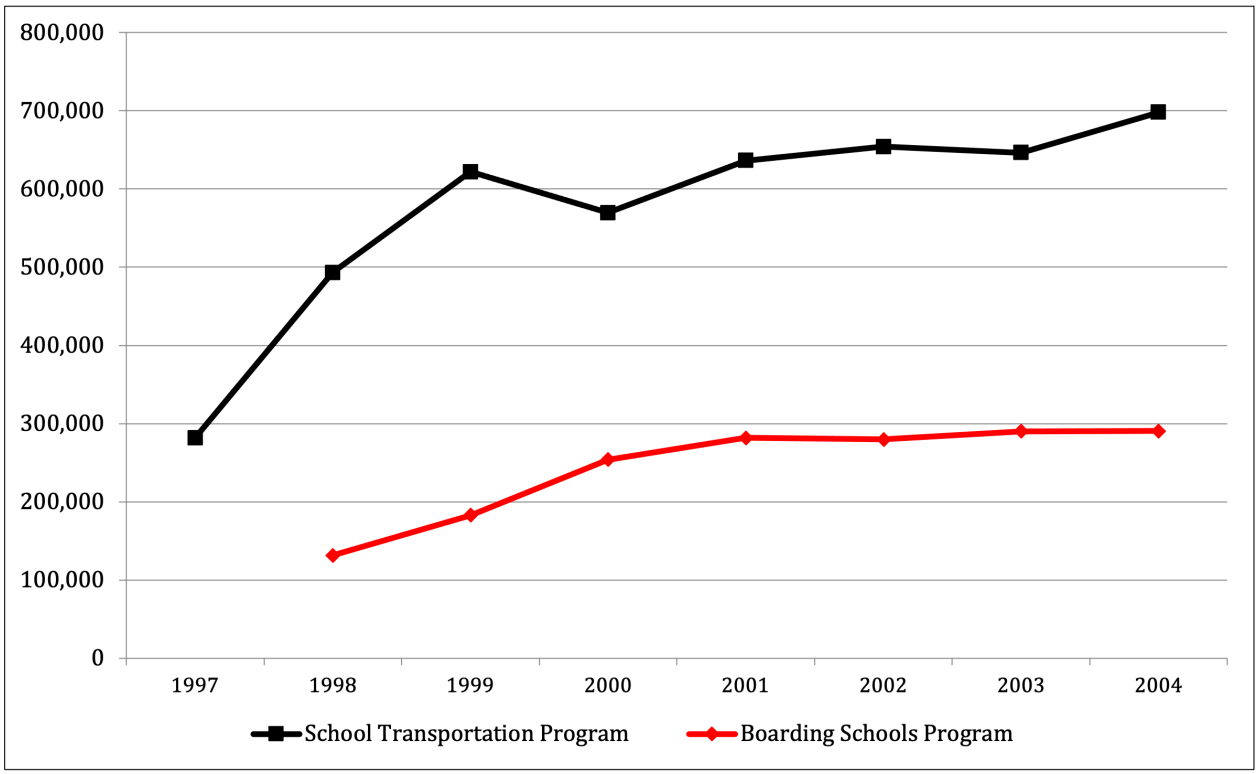
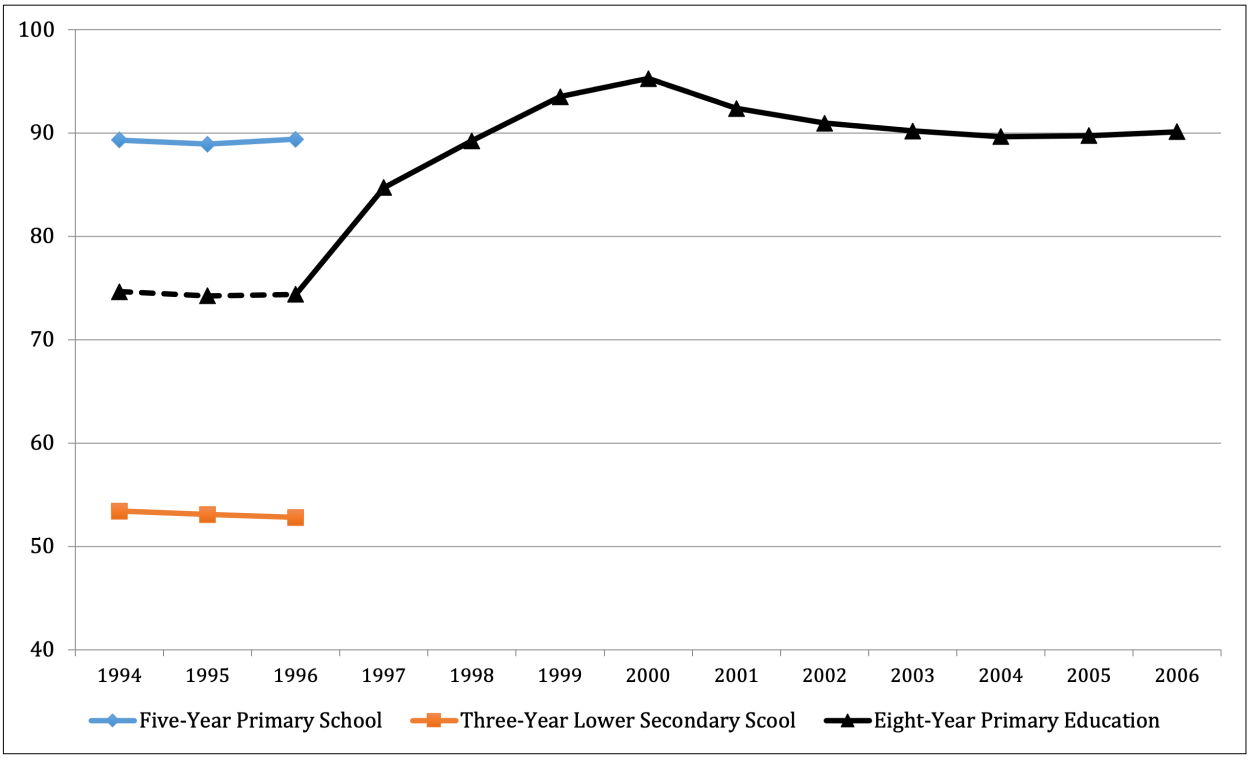
Figure 1: **The number of students transported within school transportation program and the number of students enrolled in boarding schools program**



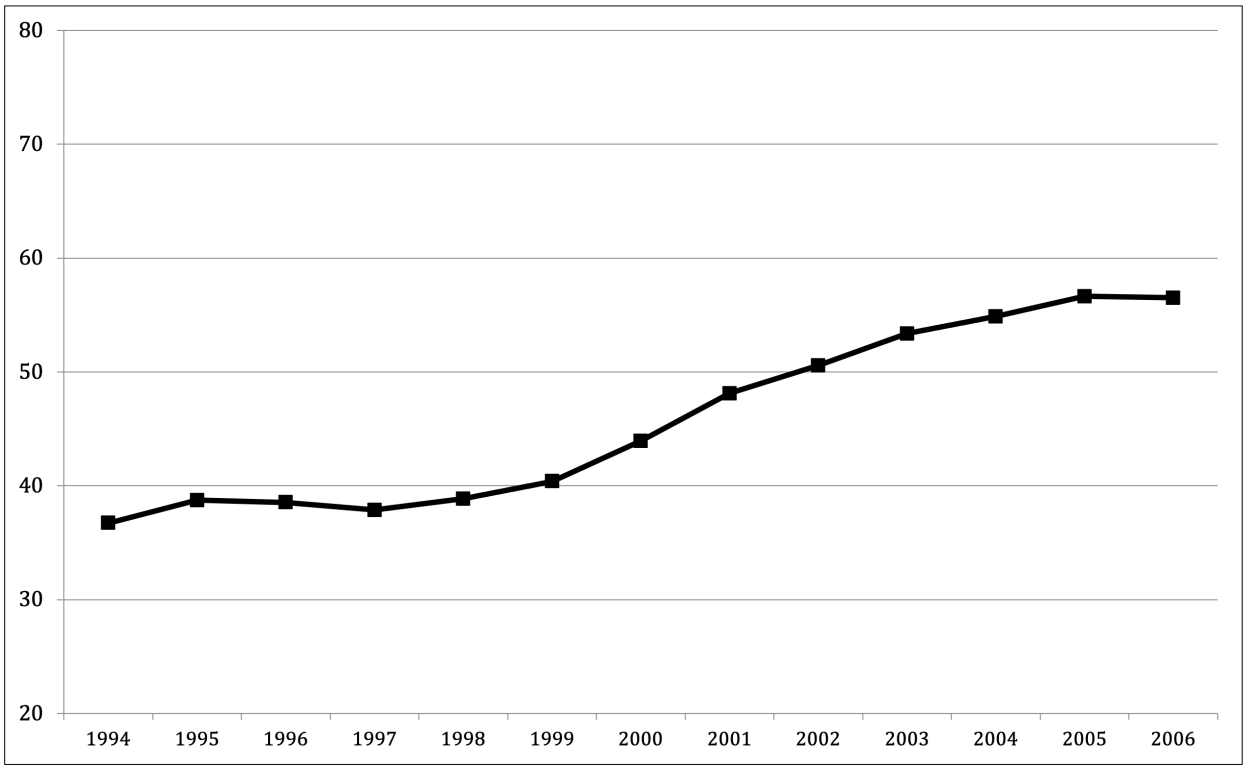
Source: The number of students transported in 1997 is obtained from MONE-Ankara University (2002); all other data are based on National Education Statistics books.

Figure 2: **Net enrollment rate in compulsory schooling (1994-2006)**



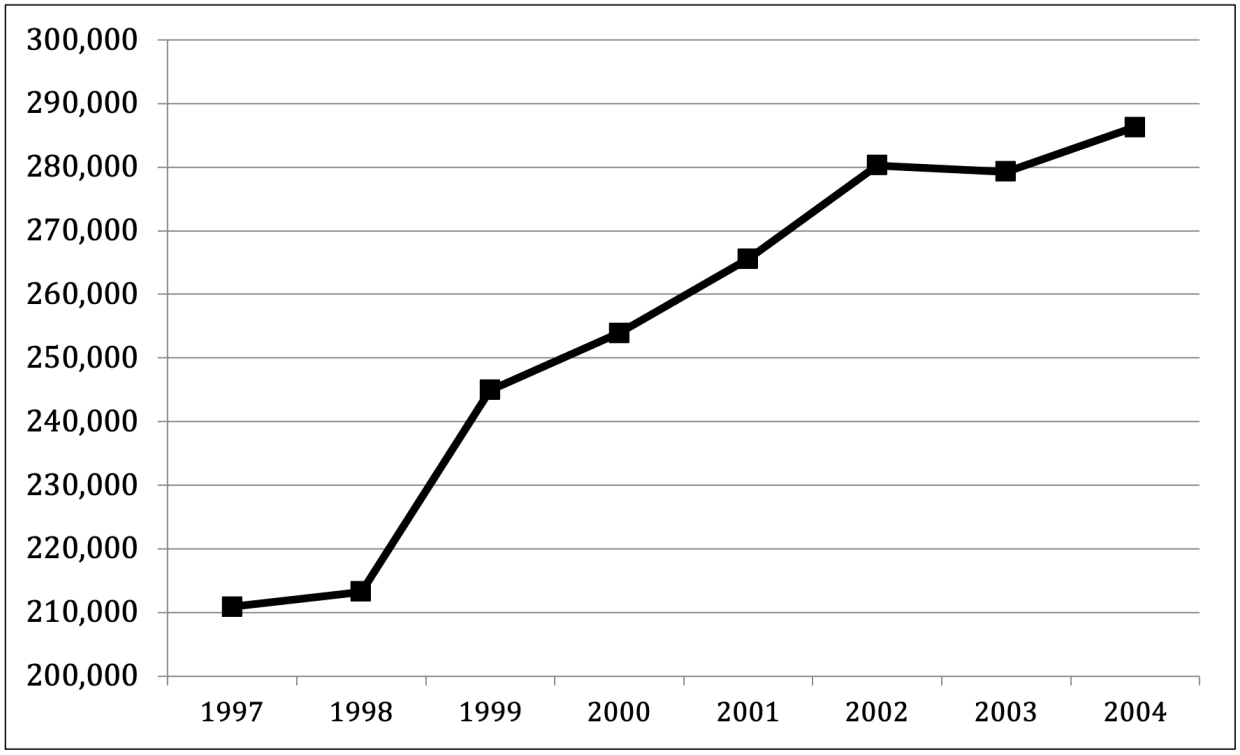
Source: Authors' calculations based on MONE (2007).

Figure 3: **Net enrollment rate in high school (1994-2006)**



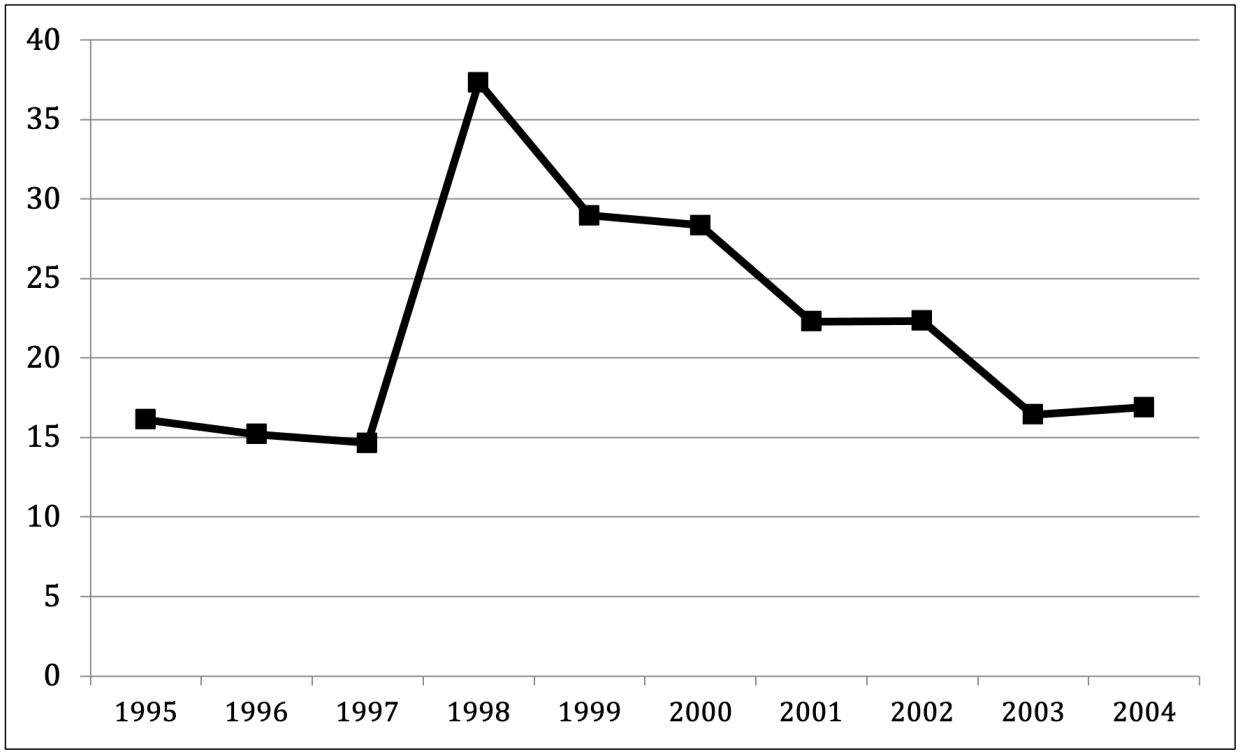
Source: Authors' calculations based on MONE (2007).

Figure 4: **The total number of classrooms in Turkish Primary Education (1997-2004)**



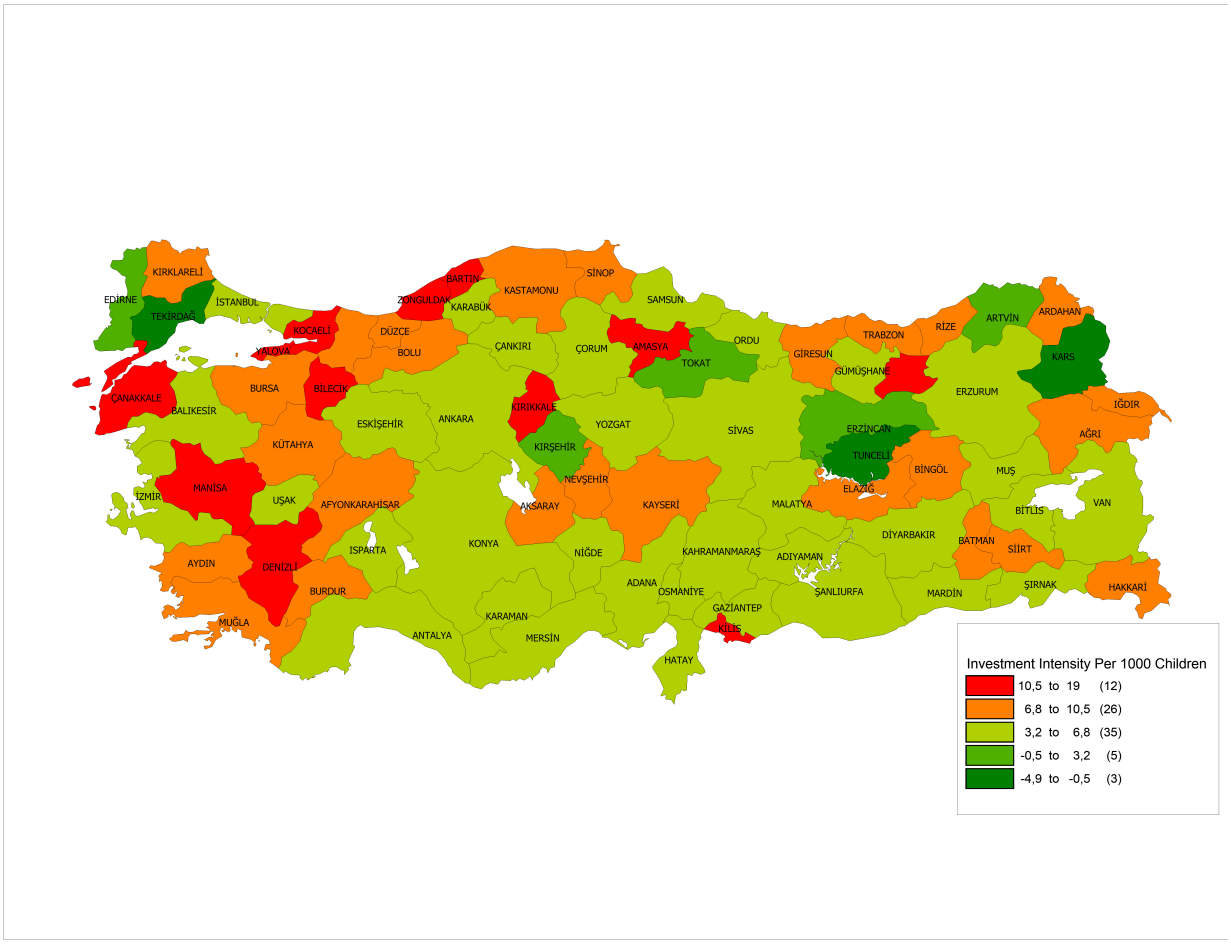
Source: Based on National Education Statistics books.

Figure 5: **Share of the investment of the Ministry of National Education in Total Public Capital Investments (1995-2004)**



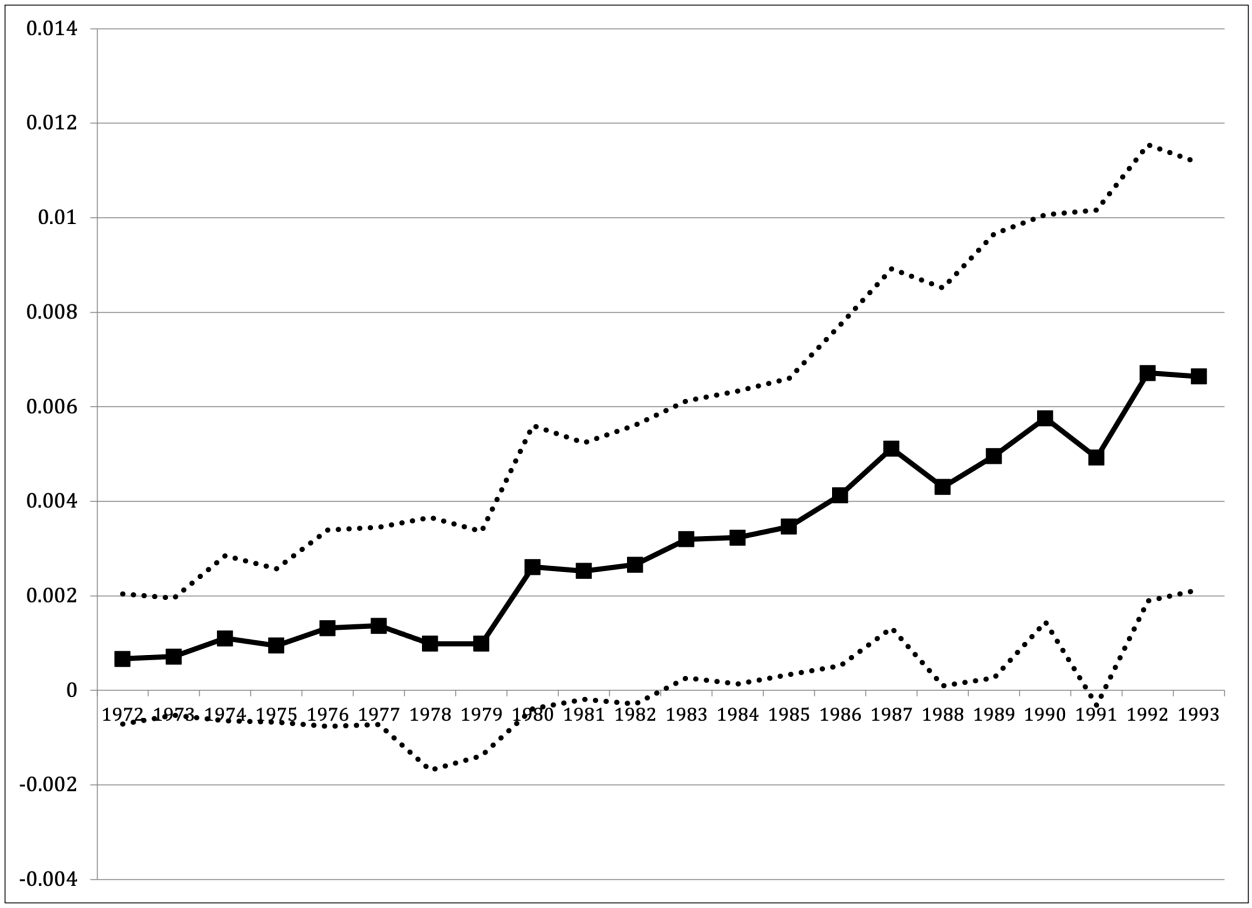
Source: Based on Dulger (2004).

Figure 6: **Net increase in the number of classrooms in primary education between 1998-2002 for 1,000 children (aged 6-13)**



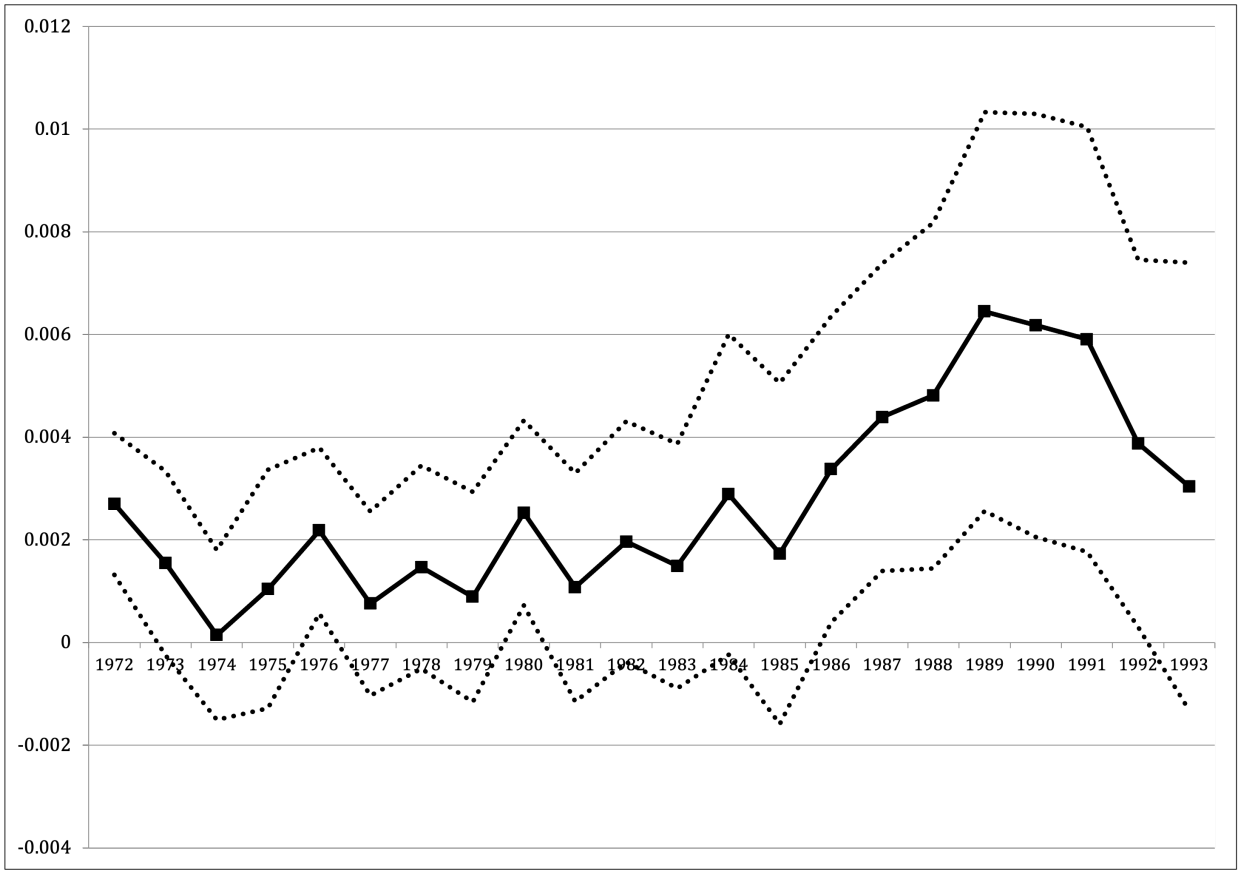
Source: Authors' calculations based on National Education Statistics books.

Figure 7: **Coefficients of the interactions cohort dummies with the intensity of the construction program in the province of birth for the high school attainment**



Notes: All specifications include province of birth and year of birth dummies, and interactions between the year of birth dummies and all control variables that could have correlations with the construction program including the number of children in the province of birth, the preenrollment rate in the province of birth, the net increase in the number of students who benefited from boarding schools and school transportation program. Broken lines indicate the 95-percent confidence interval based on clustered (on provinces) robust standard errors.

Figure 8: **Coefficients of the interactions cohort dummies with the intensity of the construction program in the province of birth for the female labor force participation (excluding agriculture sector)**



Notes: All specifications include province of birth and year of birth dummies, and interactions between the year of birth dummies and all control variables that could have correlations with the construction program including the number of children in the province of birth, the preenrollment rate in the province of birth, the net increase in the number of students who benefited from boarding schools and school transportation program. Broken lines indicate the 95-percent confidence interval based on clustered (on provinces) robust standard errors.

Table 1: **The allocation of classrooms to the provinces**

	Dependent Variable **Log (Number of Classrooms)**
Log (Number of Children	0.86
Aged 6–13 in the Province)	(0.07)
Log (Non-Enrollment Rate	-0.09
in the Primary Education in 1997)	(0.06)
# of Observations	77
R^2	0.70

Notes: The dependent variable is the log of the net increase in the number of classrooms in each province between 1998 and 2002. The non-enrollment rate=1-enrollment rate in primary education. The enrollment rate is the number of children enrolled in primary school in 1997 divided by number of children aged 6-13 in the provinces in 2000. If the enrollment rate is 1 or over, log (non-enrollment rate) is chosen 0 due to the definition of the logarithm function. Robust standard errors are in parentheses.

Table 2: **Summary statistics**

Variable	Mean
Panel A: Individual (Born Between 1971 and 1993) Level Means in 2011 (N=2,872,593)	
Primary Education Attainment Rate	0.75
High School Attainment Rate	0.48
Labor Force Participation Rate	0.58
Panel B: Provincial Level Means (N=80)	
Net Increase in the Number of Classrooms in Primary Education Between 1998-2002	67,014
Net Increase in the Number of Classrooms in Primary Education Between 1998-2002 for 1000 Children (Aged 6-13)	6.2
Net Increase in the Number of Students Boarded at Primary Education Between 1998-2002	148,333
Net Increase in the Number of Transferred Students in Primary Education Between 1999-2002	32,107
Number of Student per Teacher in the 1998-1999 School Year	28.6
Number of Student per Teacher in the 2002-2003 School Year	25.4
Enrollment Rate in Primary Education in the 1997-1998 School Year	82.8
Male Enrollment Rate in Primary Education in the 1997-1998 School Year	88.8
Female Enrollment Rate in Primary Education in the 1997-1998 School Year	76.2

Source: Authors' calculations based on the 2011 Population and Housing Census (TURKSTAT) and National Education Statistics books.

Table 3: **Number and percentage of the population who have primary school diploma (five-year) and born between 1980 and 1989**

Year	Observations	Primary School Diploma (5-Year)	Percent
1980	141,602	39,149	27.6
1981	147,557	40,065	27.2
1982	132,881	33,942	25.5
1983	127,519	31,886	25.0
1984	127,857	30,058	23.5
1985	126,181	25,957	20.6
1986	125,842	18,042	14.3
1987	123,586	8,365	6.8
1988	121,894	67	0.1
1989	119,178	0	0.0

Source: Authors' calculations based on the 2011 Population and Housing Census. The percent is the ratio of the number of observations who have primary school diploma to the total number of observations in the same born cohort.

Table 4: **Born cohorts and the number and percentage of the students who enrolled in the 1998–1999 school year in primary education (eight-year)**

Year	Number of Students	Percent
1993 and after	71,362	0.8
1992	1,126,852	11.8
1991	1,224,931	12.9
1990	1,235,245	13.0
1989	1,228,213	12.9
1988	1,230,472	12.9
1987	1,190,560	12.5
1986	989,392	10.4
1985	808,324	8.5
1984	277,175	2.9
1983	92,300	1.0
1982 and before	37,218	0.4

Source: Authors' calculations based on (MONE, 1999). The percent is the ratio of the number of enrolled students in a born cohort to the total number of enrolled students in the 1998–1999 school year.

Table 5: **Effect of the program on high school attainment**

	Obs. (Total)	**Dependent Variable: High School Completion**											
		Total				**Men**				**Women**			
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Treatment Experiment (Individuals Born in 1988–1993 versus in 1977–1982)	1,499,701	0.0037 (0.0017) **	0.0037 (0.0016) **	0.0037 (0.0016) **	0.0037 (0.0018) **	0.0037 (0.0018) **	0.0034 (0.0019) *	0.0038 (0.0017) **	0.0038 (0.0020) *	0.0040 (0.0018) **	0.0040 (0.0018) **	0.0037 (0.0017) **	0.0037 (0.0020) *
Panel B: Control Experiment (Individuals Born in 1977–1982 versus in 1971–1976)	1,520,010	0.0008 (0.0010)	0.0011 (0.0010)	0.0011 (0.0009)	0.0011 (0.0011)	0.0012 (0.0010)	0.0014 (0.0010)	0.0014 (0.0010)	0.0014 (0.0011)	0.0004 (0.0011)	0.0006 (0.0011)	0.0007 (0.0011)	0.0007 (0.0012)
Control Variables:													
Year of Birth*Boarding Schools Program		No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year of Birth*School Transpostation Program		No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Standard Errors:													
Cluster-Robust Standard Errors		√	√	√		√	√	√		√	√	√	
Two-way Cluster-Robust Standard Errors					√				√				√

Notes: All specifications include province of birth and year of birth dummies, the interactions between the year of birth dummies with two control variables of the determinant of the construction program: the number of children and the pre-enrollment rate in the province of birth. Clustered robust standard errors are in parentheses. In (1), (2), (3), (5), (6), (7), (9), (10), (11), the standard errors are clustering on the individual's province of birth. In (4), (8), (12), the standard errors are clustering on the individual's province of birth and year of birth. * $p < 0.10$ ** $p < 0.05$.

Table 6: **Associations between labor force participation and educational attainment in Turkey (excluding agriculture sector)**

Educational Attainment	**Dependent Variable: Labor Force Participation**			
	Men		**Women**	
	Percent	**Coefficient**	**Percent**	**Coefficient**
No Lower Secondary School Degree	14.92	Reference	31.45	Reference
Lower Secondary (8-year)	28.49	0.068***	24.62	0.078***
		(0.003)		(0.003)
High School or Equivalent	36.46	-0.001	27.69	0.149***
		(0.004)		(0.002)
Higher Education	20.14	0.051***	16.24	0.532***
		(0.003)		(0.010)

Notes: The regression includes province of birth and year of birth dummies. Robust standard errors, clustered on 81 provinces, are in parentheses. * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Table 7: **Effect of the program on labor force participation (excluding agriculture sector)**

	Obs. (Total)	Dependent Variable: Labor Force Participation											
		Total				Men				Women			
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Treatment Experiment (Individuals Born in 1988–1993 versus in 1977–1982)	1,389,469	0.0012 (0.0014)	0.0008 (0.0014)	0.0008 (0.0013)	0.0008 (0.0015)	-0.0010 (0.0018)	-0.0016 (0.0018)	-0.0017 (0.0018)	-0.0017 (0.0019)	0.0042 (0.0016) **	0.0039 (0.0017) **	0.0035 (0.0015) **	0.0035 (0.0017) **
Panel B: Control Experiment (Individuals Born in 1977–1982 versus in 1971–1976)	1,386,921	0.0004 (0.0004)	0.0005 (0.0005)	0.0005 (0.0004)	0.0005 (0.0006)	0.0002 (0.0002)	0.0003 (0.0002)	0.0003 (0.0002)	0.0003 (0.0003)	0.0002 (0.0005)	0.0002 (0.0006)	0.0003 (0.0006)	0.0003 (0.0008)
Control Variables:													
Year of Birth*Boarding Schools Program		No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year of Birth*School Transpostation Program		No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Standard Errors:													
Cluster-Robust Standard Errors		√	√	√		√	√	√		√	√	√	
Two-way Cluster-Robust Standard Errors					√				√				√

Notes: All specifications include province of birth and year of birth dummies, the interactions between the year of birth dummies with two control variables of the determinant of the construction program: the number of children and the pre-enrollment rate in the province of birth. Clustered robust standard errors are in parentheses. In (1), (2), (3), (5), (6), (7), (9), (10), (11), the standard errors are clustering on the individual's province of birth. In (4), (8), (12), the standard errors are clustering on the individual's province of birth and year of birth. * $p < 0.10$ ** $p < 0.05$

Table 8: **Effect of the program on high school attainment and labor force participation with adding a quality variable**

	High School Attainment Rate (Total sample)		**Female Labor Force Participation Rate**	
	(1)	(2)	(3)	(4)
Panel A: Treatment Experiment (Individuals Born in 1988–1993 versus in 1977–1982)	0.0037** (0.0016)	0.0037** (0.0016)	0.0035** (0.0015)	0.0040** (0.0014)
Panel B: Control Experiment (Individuals Born in 1977–1982 versus in 1971–1976)	0.0011 (0.0009)	0.0009 (0.0009)	0.0003 (0.0006)	0.0002 (0.0006)
Control Variables:				
Year of Birth*Boarding Schools Program	Yes	Yes	Yes	Yes
Year of Birth*School Transpostation Program	Yes	Yes	Yes	Yes
Year of Birth*Net Increase in the Teacher/Student Ratio	No	Yes	No	Yes

Notes: All specifications include province of birth and year of birth dummies, the interactions between the year of birth dummies with two control variables of the determinant of the construction program: the number of children and the pre-enrollment rate in the province of birth. Robust standard errors, clustered on 80 provinces, are in parentheses. * $p < 0.10$ ** $p < 0.05$

Table 9: **Effect of the program on high school attainment and labor force participation with adding high school construction variable**

	High School Attainment Rate (Total sample)		**Female Labor Force Participation Rate**	
	(1)	(2)	(3)	(4)
Panel A: Treatment Experiment (Individuals Born in 1988–1993 versus in 1977–1982)	0.0037**	0.0035**	0.0035**	0.0041**
	(0.0016)	(0.0016)	(0.0015)	(0.0017)
Panel B: Control Experiment (Individuals Born in 1977–1982 versus in 1971–1976)	0.0011	0.0005	0.0003	0.0004
	(0.0009)	(0.0010)	(0.0006)	(0.0006)
Control Variables:				
Year of Birth*Boarding Schools Program	Yes	Yes	Yes	Yes
Year of Birth*School Transpostation Program	Yes	Yes	Yes	Yes
Year of Birth*Net Increase in the Number of Classrooms in High Schools	No	Yes	No	Yes

Notes: All specifications include province of birth and year of birth dummies, the interactions between the year of birth dummies with two control variables of the determinant of the construction program: the number of children and the pre-enrollment rate in the province of birth. Robust standard errors, clustered on 80 provinces, are in parentheses. * $p < 0.10$ ** $p < 0.05$