

The Effect of Compulsory Schooling Age on Graduation Rates

Abstract

Exploiting an exogenous variation produced by a change in law in Michigan 2010, I was able to identify the causal effect of compulsory attendance law on graduation rates using a Difference-in-Differences method. Analysing data from NCES I produced the estimates considering as control group similar states that did not introduce the law. I have also checked the robustness of results with a placebo test and searched for dynamic treatment effects through event studies. The preliminary results are significant but not always sizeable. Raising compulsory education by 2 years reduces graduation rates by 2.2 percentage points, which corresponds to a 3% reduction over the sample mean. The effect is 38% higher, on average, for economic disadvantaged students. Even considering the limitations of my approach, the results suggest that, on average, individuals who were likely to drop out have simply postponed their choice of leaving school, without graduating.

Introduction

Economists are interested in compulsory schooling laws since Angrist and Krueger published a paper where they used quarter of birth as instrument for education with the aim of estimating the returns from schooling. They were able to use that instrument precisely because the existence of compulsory attendance law. In the following years, many authors used similar approaches to study relationship between education and very different outcomes such as wages, health (Kemptner et.al, 2011) or crime (Lochner and Moretti, 2003). In general, the main objective of compulsory attendance law is to reduce high school drop-out and increase completion rates. The reason is that education is supposed to be associated with higher earnings, better health status, better life conditions and low crime. Then, forcing students to attend more years of school can help to achieve those targets. In fact, many studies demonstrated that the policy is effective in reducing dropouts, in increasing earnings and reducing crime, but a smaller literature considered the effect of the reform on graduation. In this research, I want to understand if the policy can be useful for increasing the proportion of students with a diploma. Graduation is a relevant topic for many reasons. For instance, it can be considered as a signal sent by workers to employers, in order to demonstrate their ability, motivation and productivity. Then, employers can discriminate between high and low productivity workers using their qualification, and consequently set the wages as a best response of that signal. In the following paragraphs, I will try to provide some results to better understand the effect of rising compulsory schooling law, but first we have to see which are the channels through which the policy can affect graduation.

How School Leaving Age Reforms Can Affect Graduation

Rising compulsory schooling age may have several effects on graduation choices, and many of them can go in opposite direction. According to the economics literature, education can be seen as an optimal individual choice. For education people invest time, endure psychological efforts and sustain opportunity costs; therefore, they will choose to gain education only if the present value of the expected returns is higher than the costs. The present value is obtained by applying to the future earnings a discount rate that measures the individual level of patience. Inpatient individuals will tend to discount more the future returns of education, which can lead to low investments, on average. Therefore, if students have already chosen optimally when to leave, an increase in compulsory school attendance make them worse off (*Oreopoulos, 2006*). An implication is to leave school as soon as it is permitted by law, without getting a diploma. This is true based on the assumption that the optimal level of school is already chosen, and it doesn't change over time. If this assumption is not true and education has positive externalities, then, rising attendance law can even increase the probability that students graduate. The reason is that, attending more years of school can affect negatively risk aversion and positively patience, which can lead individuals to adopt a more forward-looking behaviour given their new lower discount rates (*Lochner and Moretti, 2003*). Empirical evidence can help in finding which theoretical view is the correct one. If compulsory schooling law leads to higher graduation rates, then it could be an important result in order to guide education policies.

Research Design and Methodology

To estimate the causal effect of compulsory school law on graduation, I exploited a change in the law which caused an exogenous variation of the explanatory variable. The US department of education shares a database containing data about every change in compulsory attendance requirements. I found that states changed their attendance law frequently, at least two times each, in the last twenty years. This gave me the opportunity to explore a quasi-experimental setting. In this case, it is possible to consider variation in time, namely pre- and post-treatment, but also to consider heterogeneity among units and controlling for time-invariant unobserved heterogeneity. In 2010 the Michigan state passed a law in which, beginning with 6th graders in 2009-10, compulsory attendance age will have increased from 16 to 18 years old. All Michigan students starting from that age cohort are considered treated because affected by the law. A 6th grader in 2008-09 in Michigan is not affected by the policy, i.e., she can drop out at 16. Before the reform, Michigan students were forced to attend school from 6 to 16 years old with a total of 10 years of compulsory education. In order to correctly identify the causal effect, the control group must have had the same age requirement of Michigan before the passage, and it must have maintained it after.

New York, Arizona and Vermont satisfy these two requirements and they belong to the control group. In fact, high school students from the control group must attend school from 6 to 16 years old, before and after the Michigan reform. The control group, if correctly identified, allow the researcher to construct the counterfactual, i.e., what would have happened if Michigan wouldn't have passed the law. One limitation of the treatment itself is the fact that the law was not applied for home-school students. In addition, parents with a special request could permit their children to drop out school earlier. Then, the results can underestimate the effect of the policy on graduation rates because the law was not so strict. The best methodology to apply, considering the research design and the available data, is the difference-in-differences method. In fact, with a two-way fixed effects estimation is possible to control for both variation in time and between groups. The US National Center of Education Statistics provides graduation data at school or district level. Thanks to the Urban Institute, an American think tank, these data are easily accessible through an API. Data refers to period between 2011 and 2019. Before 2011 graduation data was collected and elaborated by NCES to compute "the average freshmen graduation rate" (AFGR). After 2011, the US department started to use "the adjusted cohort graduation rate" (ACGR). Those two measures do not match perfectly, the first one is very sensitive to migration trends, and it also includes students who took more than four years to graduate. As a result, it is likely to be inflated with respect to the true ACGR. The ACGR is the percentage of students in the cohort who graduate within four years starting from the 9th grade. The ACGR of 2012-13 school year refers to the proportion of first-time 9th graders in fall 2009, the starting cohort, that got a diploma on time. The formula is reported in figure 1.

Figure 1: Formula for Calculating the Four-Year Adjusted-Cohort Graduation Rate

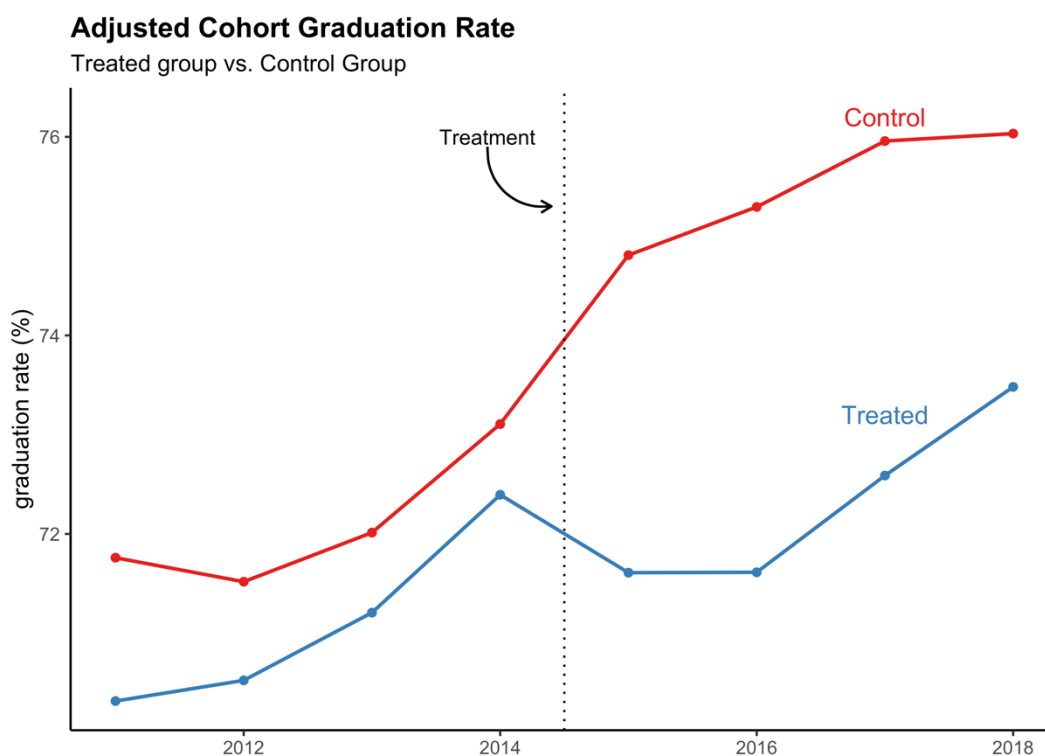
$$\frac{\text{Number of cohort members who earned a regular high school diploma by the end of SY 2018-19}}{\text{Number of first-time 9th graders in Fall 2015 (starting cohort) plus students who transferred in, minus students who transferred out, emigrated, or died during SY 2015-16, 2016-17, 2017-18, and 2018-19}}$$

Even if ACGR is precise, I will probably underestimate the proportion of students with a diploma, because it measures the proportion of students who graduates on time, excluding those that repeat grades. But with respect to AFGR, this remains the best solution, in fact if a student drops out during the high school she will enter in any case in the denominator, and consequently, the ACGR will decrease. Then, an increase in the compulsory attendance law, associated with a decrease of the graduation rates would probably mean that a lot of the less motivated students attended school without getting a diploma.

Data description

For my analysis the data have more than 2 million observation and 15 variables. After having filtered it for the states included in the treated or control group, and after having performed some cleaning, the data dimensionality reduces at 48517 rows. The data structure is a panel where each unit, the school, is repeated once per year from 2011 to 2019. For each unit, if the cohort contains less than 300 students, ACGR are reported as a range: “low”, “mid” and “high” which are the first, the median and the last quartile of the graduation rates distribution in the cohort. If the cohort is big, only the median value is reported, which will be the principal measure of the models. Every year in the panel refers to the first part of the school year e.g., 2015 refers to the 2015-16 school year. The schools are considered treated if they belong to Michigan after 2015, included. In fact, the graduation rates of 2015 refer to the cohort that was first time 9th graders in 2012-13 and that was 6th graders in 2009, hence it was the first cohort affected by the policy. From the very first raw data I constructed a graph displaying the trends in mid ACGR in control and treated group, before and after the treatment. The control group line is an average between New York, Arizona, Vermont school graduation rates. On the right side of the dashed line, we are after the law change. We can see a drop in 2015, which corresponds to the average mid ACGR of the first treated cohort. After that, we can see a phase of plateau and then a constant recovery. The trends in the pre-treatment period are very similar for the two groups. Clearly, this isn't enough to prove the parallel assumption, which is untestable, but it's enough to show that any difference after 2015 is not due to divergent trends before the law change.

Figure 2



Results

First, I estimated a simple model:

$$y_i = \alpha_{i,group} + \alpha_{i,post} + \beta_{1i}(D_i \times Post_t) + \varepsilon_i$$

Where α_{group} are fixed effects at a group level (treated, control) and the second term, $\alpha_{i,post}$, are fixed effects that consider 2 periods: pre and post treatment. The coefficient β_1 associated with the interaction between D_i and $Post_t$, measures the causal effect of compulsory school law on mid ACGR. D_i is an indicator variable taking value 1 if the school is treated, and zero otherwise. $Post_t$ is an indicator variable taking value 1 in the years after the treatment, and 0 before. If the parallel trends assumption holds, then β_1 correctly estimates the ATT. The results are reported in the first 2 columns of table 1. An increase of 2 years in the compulsory schooling law causes a reduction in the mid adjusted cohort graduation rates of about 2.2 percentage points. In the second column, errors are clustered at a state level, which allows residuals to be correlated between states. Even if the effect is highly significant, a reduction of 2 pp corresponds to a 3% reduction over the sample mean, which cannot be considered a big effect, especially considering the additional 2 years of schooling.

In the 3rd and 4th columns I reported the results of a more complicated regression model:

$$y_{it} = \alpha_{i,state} + \alpha_{i,year} + \beta_{1i}(D_i \times Post_t) + \varepsilon_{it}$$

Where α_{state} are fixed effects at a state level, whereas the second term are fixed effects at a year level. Controlling for state and year reduces slightly the size of coefficients to 2.19 pp. Even if it has increased the standard errors, we can always reject the null hypothesis that the coefficient is different from zero. The difference between 3rd and 4th column is how the residuals are clustered, in the 3rd column they are clustered at a state level but in the last column at a district level. As I said earlier, each unit, the schools, belongs to a district. Schools can share similar education policies decided at a local level. Also, schools tend to be very similar within the same district, some districts are very poor, and the quality of schools is under the average standards, but there can be many rich districts that contains all excellent schools. Therefore, clustering at a district level allows the residuals to be correlated across years within each district. This is important in order to estimate the right size of standard errors. In the model (4) we can see that the std. error is almost 3 times higher than before, but the estimated coefficient remains highly significant. As I said earlier, ACGR are reported as a range (low, mid, high) if the cohort contains less than 300 students. Then, could be interesting to see how compulsory schooling affect the distribution of graduation rates. The results are reported in table 2. We can see that all coefficients are significant at 1% level with the size of coefficients increasing if we consider higher part of the distribution. The high ACGR, which corresponds to the last quartile, reduces by 2.3 percentage points as an effect of the law change, whereas the low ACGR reduces by 2.1 pp.

Table 1: DD estimates on school average graduation rates

	ATT (1)	ATT (2)	ATT (3)	ATT (4)
Treatment	-2.227***	-2.227***	-2.194***	-2.194***
	(0.000)	(0.238)	(0.216)	(0.605)
Num.Obs.	48517	48517	48517	48517
Std.Errors	by: group	by: state	by: state	by: district
FE: state			X	X
FE: year			X	X
FE: group	X	X		
FE: post	X	X		

* $p < 0.1$, ** $p < 0.05$,
 *** $p < 0.01$

Table 2: DD estimates on school level graduation rates

	Low	Mid	High
Treatment	-2.088***	-2.194***	-2.265***
	(0.659)	(0.605)	(0.598)
Num.Obs.	48517	48517	48517
Std.Errors	by: district	by: district	by: district
FE: state	X	X	X
FE: year	X	X	X

* $p < 0.1$, ** $p < 0.05$,
 *** $p < 0.01$

Placebo estimates

We can't never prove the parallel trend assumption because it is based on a counterfactual that we are not able to see. What is feasible, though, is to perform a placebo test in order to see if the parallel trend assumption is at least credible. The idea is trying to find a treatment effect where the treatment hasn't really occurred. The reason is simple, if I find a similar and significant effect before the treatment, then any effect found after the policy change, it is difficult to attribute to the treatment itself. For doing it, I selected only observations before 2015, and I picked 2 different periods to use as fake treatment periods. The first fake treatment is supposed to be happened in 2014, and the second in 2012/2013 years. Any significant effect different from 0, will undermine the credibility of the previous estimates. Results are reported in table 3. The regression was done with state and year fixed effects, and the residuals clustered at a district level. The coefficients are almost equal to zero and we can't reject the null hypothesis that the coefficients are non-zero, given their huge standard errors. Before the treatment occurred, there was no sizable and significant difference between control and treated group. With this result the parallel trend assumption is more credible.

Table 3: DD Placebo estimates on mid ACGR

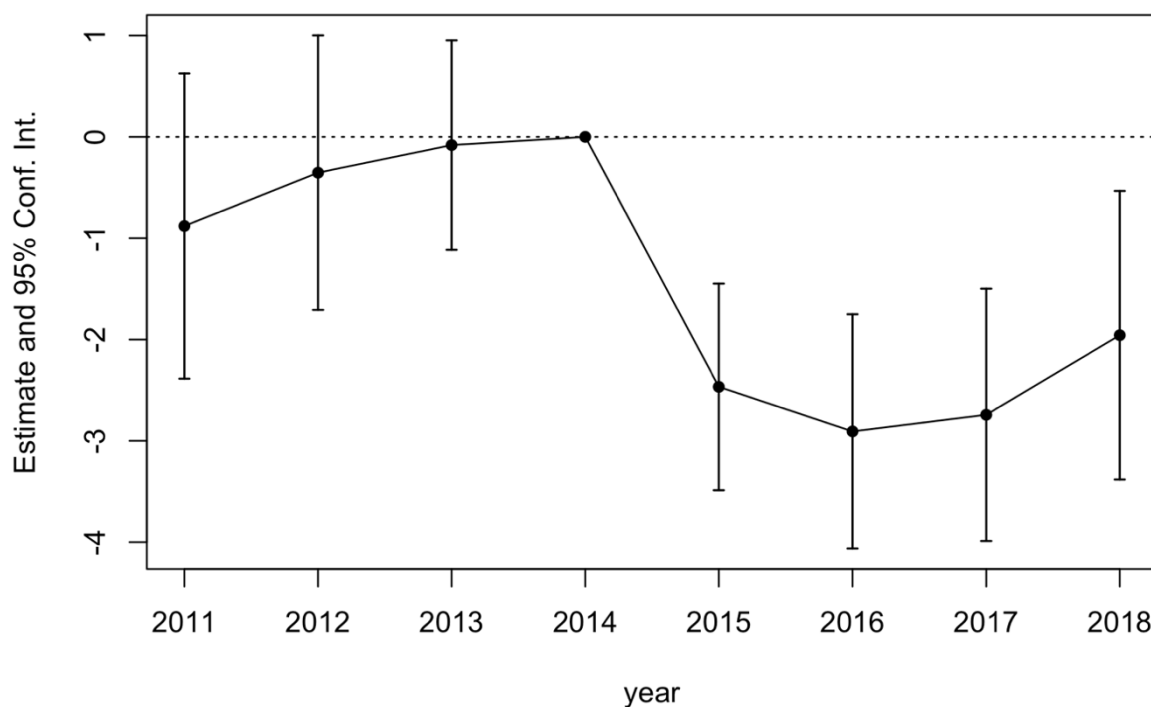
	Placebo 1	Placebo 2
Fake Treatment 1	0.435	
	(0.565)	
Fake Treatment 2		0.216
		(0.391)
Num.Obs.	23122	23122
Std.Errors	by: district	by: district
FE: state	X	X
FE: year	X	X
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$		

Event study

The two-way fixed effect approach allows to identify the effect of a treatment on a 2-period horizon: pre and post treatment. The problem is that sometimes the effects of a policy do not last for ever, or they can have different level of intensity according to the time passed from the intervention. It is reasonable if we think that policies can lose efficacy over time. With this perspective, event studies help us to determine a sort of dynamic treatment effect. We expect to find zero and insignificant effects before 2015, when the policy was not in force. After the treatment, we can see which is the intensity or the efficacy of the policy changing for each year until 2018. The results are relevant in order to guide policymakers, for instance, a law that lose efficacy quickly over time could become useless.

Figure 3

Effect of Change in Compulsory School on mid ACGR



We can see the estimates reported in the coefficient plot in figure 3. In 2011 the estimated treatment coefficient is -0.880 (std. error 0.769), in 2012 is -0.353 (std. error 0.691) and -0.081 (std. error 0.527) in 2013. Even if the first coefficient is almost 1, we can't reject the null that is non-zero. Even if not so precise, we can see there is no treatment effect before 2015 and significant negative effects after the policy change. In fact, in 2015 the estimated reduction in ACGR is -2.467 (0.520), slightly higher than the 2-way fixed effect estimates. The maximum reduction is reached in 2016, two years after the law change, with an estimated coefficient of -2.907 (0.590). Then, in 2017 and 2018 the effect reduces at -2.743 (0.636) and -1.957 (0.726). With all coefficients after the treatment are significant at 1% level.

The negative effects last for all the period considered, with an average reduction of about 2.4 pp per year. The negative effect on graduation rates does not seem to disappear quickly even if with an inverted trend from 2017. If the causal effect is correctly identified, this would mean that increasing compulsory school age affects graduation in a structural way.

Effect on Economic Disadvantaged Students

As I said in the previous paragraph, education is an investment that requires some economic efforts in order to be sustained. The present value of its returns depends on the individual intertemporal preferences. Inpatients individuals tend to discount more future returns, because they prefer immediate gains. Poor people, on average, will invest less in education for several reasons. First, they have not so much money to invest. Second, they prefer immediate returns. Work instead of studying can be their optimal choice with the aim of improving their social-economic condition. Therefore, if it's true that given the chosen optimal level of education, an increase in compulsory attendance law, make people worse off, then we could expect to see a higher effect on those who generally invest less in education. My dataset contains adjusted cohort graduation rates for all the students, and for the economic disadvantaged students. Low-income people have a higher probability of drop-out and so we can expect a higher reduction in graduation rates for this category. In order to perform the analysis, I repeat the same procedure but only considering economic disadvantaged students graduation rates. In table 5 are reported the effect of the policy on low, mid and high graduation rates.

Table 5: Effect on Economic Disadvantaged Students

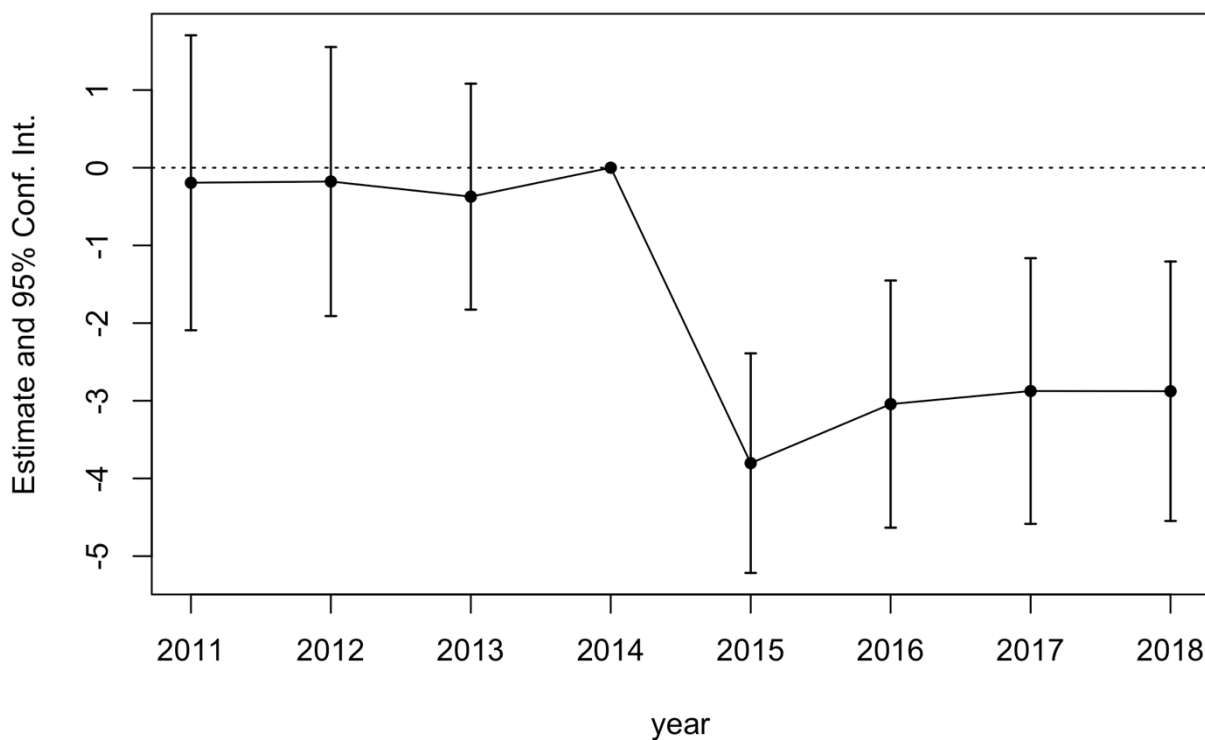
	Low	Mid	High
Treatment	-3.811*** (0.820)	-2.962*** (0.724)	-2.100*** (0.700)
Num.Obs.	20199	20199	20199
Std.Errors	by: district	by: district	by: district
FE: state	X	X	X
FE: year	X	X	X
* p < 0.1, ** p < 0.05, *** p < 0.01			

The model is the same, there are state and year fixed effects, and the residuals are clustered at a district level. The first result I want to stress is the magnitude of the coefficients. The policy caused a reduction in the mid ACGR of almost 3 percentage points, and it is highly significant. This result is 38% higher than the original one, the difference is notable. Mid ACGR of low-income students were, on average, at 68% before the treatment, then it corresponds to a 4% reduction in the ACGR caused by the policy. But the highest difference can be seen in the low ACGR which is almost twice with respect to the original one. The low ACGR for economic disadvantaged students reduces of about 6% after the treatment.

In figure 4 it is shown the event study of mid ACGR for low-income students. We can easily see that there is no effect before the policy was introduced, in fact the coefficients are zero and all not statistically significant. After the treatment we can see a sharp decrease which leads to a reduction of -3.8 percentage points. (std. error 0.72). Then starting from 2016 the reduction is constant at -2.9 pp. All coefficients are significant at 1% level.

Figure 4

Effect on mid ACGR for Economic Disadvantaged Students



Conclusion

Other considerations to do concern the limitation of these results. ACGR measures the proportion of students that graduate on time. Therefore, even if we found a negative effect on ACGR, it does not imply that the overall effect on graduation is that big. These results do not consider that some students may have spent 1 or more years to graduate with respect to the standard path and therefore the proportion of real graduated students will be underestimated. This can be a problem if we think that less able students, who are forced to attend more years of school, have a higher probability of repeating grades.

Many studies have demonstrated the importance of rising compulsory school attendance. The policy aims to minimize high school dropouts and maximize the level of education of people that otherwise would abandon school. I found no evidence of some positive effects of the policy on graduation rates. As a matter of fact, I found a notable negative effect, especially on the most disadvantaged students. This result is important in order to guide policy makers. Rising compulsory schooling age is not sufficient if the objective is increasing the number of students with a diploma. Qualifications are relevant for continuing the studies but also in order to get a higher paid job, and probably, some underlying factors such as motivation, ability, school performance, and socio-economic conditions are the most important drivers of graduation choices. Therefore, a policy more focused on improving the school environment and the integration of low-income students may be more effective.

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