

Research Institute for the Evaluation of Public Policies



IRVAPP
RESEARCH INSTITUTE FOR
THE EVALUATION OF PUBLIC POLICIES

‘Everyone in School’: The Effects of Compulsory Schooling Age on Drop-out and Completion Rates

Erica Raimondi

Loris Vergolini

November 2017

FBK-IRVAPP Working Paper No. 2017-05

‘Everyone in School’: The Effects of Compulsory Schooling Age on Drop-out and Completion Rates

Erica Raimondi

University of Trento

Loris Vergolini

FBK-IRVAPP

FBK-IRVAPP Working Paper No. 2017-05

November 2017



Research Institute for the Evaluation of Public Policies
Bruno Kessler Foundation
Vicolo dalla Piccola 2, 38122 Trento (Italy)

Phone: (+39) 0461.314209

Fax: (+39) 0461.314240

E-mail: info@irvapp.it

Website: <http://irvapp.fbk.eu>

The purpose of the IRVAPP Working Papers series is to promote the circulation of working papers prepared within the Institute or presented in IRVAPP seminars by outside researchers with the aim of stimulating comments and suggestions. Updated review of the papers are available in the Reprint Series, if published, or directly at the IRVAPP.

The views expressed in the articles are those of the authors and do not involve the responsibility of the Institute.

‘Everyone in School’: The Effects of Compulsory Schooling Age on Drop-out and Completion Rates

Erica Raimondi^(*) Loris Vergolini^()**

() University of Trento; (**) FBK-IRVAPP*

Abstract

In this paper, we analyse the effect of the Berlinguer reform that was implemented in Italy in 1999 and increased the compulsory school from eight to nine years. As a result of the reform, students had to attend school until age 15 instead of age 14 and thus had to attend at least one year of upper secondary school (for students with a regular career). Using data from Italian Labour Force Surveys (LFS) (1993-2010) and following a counterfactual approach, applying counterfactual time series and segmented regressions, we evaluate the effect of the Berlinguer reform on attendance and graduation rates.

The results show that the expansion of compulsory schooling leads to staying in school for a larger share of 16-year-olds, especially those who are judged to be more at risk of dropping out: students with less-educated parents and those with parents having a low occupational level. By age 17, however, part of the effect has already vanished, and no effects are found on graduation rates, even among at-risk youths. The compulsory schooling policy may have been more effective in adjusting the legislation to extant student behaviours than in producing relevant changes in educational decisions.

Keywords: Compulsory schooling; educational reform; Drop-out; Graduation; Italy

JEL classification: D04, I24, I28,

‘Everyone in School’: The Effects of Compulsory Schooling Age on Drop-out and Completion Rates

1. Introduction

Education remains a crucial factor for achieving a prestigious social position and mitigating the risks of unemployment, poverty and social exclusion. It is also widely recognised that dropping out from upper secondary school is a serious problem in Italy as well as in the rest of Europe (Borgna and Struffolino 2017). In fact, the early school-leaving issue has recently ascended to the top of policy agenda of many countries, and reducing it to less than 10 percent is one of the goals of the Europe 2020 strategy (European Commission 2010).

Increasing compulsory schooling is one of the straightforward systemic strategies to expand education and encourage youths to stay in school. The underlying idea is that forcing youths to attend additional years of school will incentivise them to obtain further qualifications, reducing the number of early school-leavers. The effectiveness of this kind of policy is not yet clear (Landis and Reschly 2010; Cabus and De Witte 2016); nor is the policy rationale behind these measures is always explicit (Hodgson 2016). Therefore, we aim to analyse whether increasing compulsory schooling successfully reduces early school-leaving in terms of both attendance and graduation rates. To address this issue, we exploit the 1999 Berlinguer reform in Italy that extended the minimum school leaving age from 14 to 15.

We add three main contributions to the literature. First, we shed more light on the effectiveness of extending compulsory schooling to prevent early school-leaving. In fact, in Italy, while the reforms of the late 1800s to early and mid-1900s have been broadly studied, new reforms that raise the compulsory schooling age have been carried out without in-depth analyses.¹ Because the contexts have changed and education is more widespread today, the effectiveness of this kind of policy may have changed as well. Second, we contribute to the debate about early school-leaving in Italy, where it is a significant but under-investigated phenomenon (Ballarino et al. 2011; Borgna and Struffolino 2017). Third, we examine not only the short-run effect of the reform but also whether the extension of the compulsory schooling can also affect graduation rates.

The remainder of the paper is structured as follows. The next section outlines the Italian educational system and the main characteristics of the Berlinguer reform. Section three presents previous literature and the research questions. In the fourth section, we describe the data used and the methods adopted to analyse the effect of the Berlinguer reform. The fifth section reports the results of our analysis, while the final section is devoted to concluding remarks and to policy implications.

2. The Italian educational system and the Berlinguer reform

The Italian education system has a first cycle of education lasting 8 years comprising primary education, lasting 5 years, from age 6 to 11 (Isced 1), and lower secondary education, lasting 3 years, from age 11 to 14 (Isced 2). The second cycle of education is upper secondary school (Isced 3), which is the first stage with tracks. There are three tracks available in the

¹ As far as we know, the Berlinguer reform has been investigated only by Brillì and Tonello (2015), who were interested in the effects of education on crime reduction, and partially by Schizzerotto et al. (2017), who focus on the transition rate at macro level in the short-run.

final five years of schooling:² academic, technical and vocational. Each track ends with an examination of competency known as the *Esame di Maturità*. In addition to these tracks, there are vocational and training programmes lasting 3 or 4 years³ that do not permit university access. In contrast, all the five-year tracks grant access at the university level (Isced 5A) that, since the Bologna process (2001), has been organised according to a 3+2 schema that includes a 3-year bachelor's programme and a 2-year master's programme.

In the last century, the main educational reforms were implemented in the 1960s, and for the following twenty years, there was an impasse regarding necessary reforms of the educational system. In 1996, the centre-left coalition gave impulse to educational policies. Among the measures enacted was the so called Berlinguer reform,⁴ which required youths to attend school for nine years until age 15 (instead of age 14 as previously required) beginning in the 1999/2000 school year and therefore to attend at least one year of upper secondary school (for students with a regular career). This meant that students with regular educational careers who were born in 1985 or later had to attend at least the first year of upper secondary school—and only after that they could move to vocational and training programmes or apprenticeships. The reform introduced the concept of compulsory education and training up to age 18. In fact, from age 16, youths must attend school or vocational and training programmes until they either reach the age of 18 or attain a post-lower secondary qualification. This reform also introduced a system of technical education and training courses in order to create a tertiary vocational alternative to university and thereby reduce unemployment (Benadusi and Niceforo 2010; Ballarino 2013, 2015).

This reform was intended as a solution for both the lack of qualifications and competences of young Italian people and the early school-leaving problem. Its aim was not just to push individuals to attend one additional year of school but also to propel more youths—particularly less advantaged ones—to attend upper secondary school and obtain a diploma. Moreover, expanding the minimum school leaving age to 15 meant that students with regular educational careers had to attend at least one year in 'formal education'—and not in vocational and training programmes. They could choose 'non-school' programmes only after one year in 'school' programmes. Thus, the reform attempted first to shrink the number of 'early leavers from formal education' and, only secondarily, the 'early leavers from education and training' (De Witte et al. 2013b and Nicaise et al. 2013).

Unfortunately, there are some drawbacks that could have limited the effect of the Berlinguer reform. First, the general belief that the lower secondary school exam was conclusive of the compulsory cycle of education did not change. In fact, youths who had experienced retention were not obliged to attend at least one year in upper secondary school, but only had to remain an additional year in lower secondary school, until age 15. Furthermore, the education curricula of the first year of upper secondary school were not officially modified and instead remained identical to the teaching programmes implemented before the new policy was introduced. Finally, the policy was not accompanied by effective checks on youths of age 15. The national student registry (*anagrafi degli studenti*)⁵ had no information about all youths aged 15. This meant that it was impossible to track down the non-compliers, because

² In 1999, students in the vocational track could decide to stop after 3 years (after a qualification exam).

³ Before 2003, vocational and training programmes were not homogeneous across Italy; each region decided on the supply of programmes and how many years each course lasted. Some programmes had a duration of only 2 years.

⁴ By the Berlinguer reform, we mean the policy that extended compulsory education (Law No. 9/1999), even if other policies were implemented during the tenure of Berlinguer as Minister of Education.

⁵ The '*anagrafi degli studenti*' are archives designed to collect information about all students in the country.

no authority could actually ascertain whether a person who should have continued schooling did not do so.⁶

3. Previous studies and research questions

To the best of our knowledge, only two studies have attempted to evaluate the effects of the Berlinguer reform. The first is a paper by Brillì and Tonello (2015) that uses the reform as an instrument for upper secondary school enrolment in order to estimate the causal effect of education on adolescent crime rates. They detect an increase of 7.6 percentage points of the overall enrolment ratio of 14-16 year-olds after the policy, and the coefficient of the first stage of the 2SLS model indicates the increase of enrolment is positive and significant. The second is a study by Schizzerotto et al. (2017), in which the authors use macro data and apply interrupted time series and shows a positive effect of the Berlinguer reform on high school participation (+6.7 percentage points).

This paper aims to build on these studies, deeply investigating the short- and long-run effects of the Berlinguer reform. More precisely, the short-run effect is analysed by examining school attendance (i.e., early school-leaving), while the long-run influence is studied by considering graduation rates. From a theoretical perspective, when mass schooling is consolidated, compulsory education interventions should act on specific sub-populations, namely, 'weaker youths' who would have left education otherwise. Such individuals are more likely to be low-performing students and come from disadvantaged socio-economic backgrounds (Oreopoulos 2007, 2009; Brunello et al 2009). Therefore, the obligation to stay in school longer should diminish their attainment gap with respect to students from more advantaged socio-economic backgrounds. Other research streams, however, interpret the increase in the age of compulsory education as a clever way to maintain educational inequalities since the upper class will gain further qualifications and/or use other channels in presence of inflated education credentials (Collins 1979; Walters 2000). From yet another perspective, compulsory education policies were implemented for bureaucratic reasons: public authorities implemented the laws when there were growing enrolments at school, simply to codify already existent behaviours. This means that the reforms were not intended to change a particular state of affairs, but merely follow the spontaneous trend of school participation (Shavit and Westerbeek 1997).

Given this discussion, we can formulate at least three broad research questions. The first one is:

Did youths stay in school for a longer time due to the reform, in particular at age 16 and 17?

The focus is on the short-term effects of the policy, i.e., whether it convinced youths to stay in school up to age 16 and 17, immediately after the end of the new minimum school leaving age (15), beyond the grade attended. Unfortunately, we can only examine the early school-leavers from the 'formal education' track, i.e., upper secondary school, because the data on vocational and training programmes and apprenticeships are lacking for the years when the policy was enacted.⁷ Thus, we cannot know whether 16-17-year-olds left school but

⁶ Several pieces of this information come from an interview with Giuseppe Bagni—an upper secondary school teacher and president of the CIDI teacher association (*Centro di Iniziativa Democratica degli Insegnanti*)—in June 2014. We thank Mr. Bagni for his willingness to be interviewed and mentioned in this paper.

⁷ Data on vocational and training programs and apprenticeship are available until 1999 and after 2004. They are completely missing in 2000, 2001, and 2002 Italian LFS, and variables collected in 2003 are not comparable with other surveys. Thus, it is impossible to compare attendance at vocational and training courses before and after the 1999 policy implementation (as after the policy and before the abrogation in 2003, we have no information).

remained in the broader education and training system and earned vocational qualifications. We can only evaluate the influence of the extension of compulsory schooling up to 15 without saying anything about the effects of the requirement to stay in the education and training system until attaining a post-lower secondary qualification or until age 18. In addition, we cannot separately examine youths who experienced retention—and could thus satisfy the requirement by doing one additional year in a lower secondary school—from youths who started upper secondary school due to the policy. Moreover, as the law was abrogated after four years by the Moratti reform,⁸ only youths born in four birth cohorts, from 1985 to 1988, were affected by the extension of compulsory schooling.

The second research questions concerns the possible long-term effects of the reform:

Did the upper secondary graduation rates increase due to the reform?

This question examines whether the policy was effective in inducing youths to complete upper secondary school, instead of only attending one or more years of school after reaching the mandatory age. In fact, this was one of the main aims of the policy. The reform extended compulsory schooling in the hope that a larger share of youths would complete upper secondary school instead of simply attending an additional year.

Another problem in the Italian education system is the presence of a high degree of social inequality. Students from disadvantaged socio-economic backgrounds tend to perform worse in school and attain lower educational levels (Pisati 2002; Contini and Scagni 2013). In fact, family background is one of the strongest predictors of early school-leaving in upper secondary education in Italy (Schizzerotto 1997; Checchi 2010; Fiorio and Leonardi 2010; Ballarino et al. 2011), due to the relevant role played by economic and cultural resources in shaping drop-out risk (O'Higgins et al. 2007; Mocetti 2012). Therefore, the third research question is:

Was the reform able to reduce social inequalities in the attendance and in the graduation rates?

Following Bukodi and Goldthorpe (2013), in our analyses we consider both parental education and parental social class. The former is more related to parents' cultural and educational resources, for instance, parents' capacity to participate in their children's educational careers by helping with school homework and providing informed guidance through the education system. The latter is mainly related to the family's economic resources and financial constraints.

4. Data, variables and empirical strategy

The data used for the analyses come from the Italian Labour Force Survey (LFS) 1993-2010, which collects information quarterly by interviewing a sample of nearly 77,000 households, representing 175,000 individuals living in Italy. As stressed in the previous section, we evaluate the effect of the reform on two outcomes: school attendance and upper secondary school graduation. Both are measured at the individual and aggregate level.⁹

⁸ Law No. 53/2003 and Ministry Decree No. 76/2005.

⁹ The analyses were performed in the Adele Laboratory Istat (*Laboratorio per l'Analisi dei Dati ELEMENTARI*) in compliance with legislation concerning the confidentiality of personal data. The ADELE Laboratory is a Research data centre (Rdc), a "secure" site accessible by researchers to conduct their own statistical analyses on microdata (<http://www.istat.it/en/information/researchers/analysis-of-individual-data>). We used the Adele lab because Istat does not release the birth cohort year by year, but only in 5-year classes. Results and opinions presented in this chapter are our responsibility and are not official statistics.

For school attendance at the individual level, we examine whether a student was attending school at age 16 and 17.¹⁰ At the aggregate level, it becomes the percentage of 16- and 17-year-olds in school. This value is calculated for each birth cohort officially affected by the policy—1985, 1986, 1987 and 1988—and for each previous birth cohort available. After selecting the corresponding survey and birth cohorts, we calculated the ratio of 16- to 17-year-olds who were attending school to all 16- to 17-year-olds. Thus, the measurement was made regardless of the students' particular grade. In fact, the reforms stated that compulsory schooling was accomplished once a pupil aged 15 had completed at least 9 years of schooling—regardless of the grade she/he passed—or the second year of upper secondary school (in regular careers, this means 10 years of schooling).

For the upper secondary school graduation, we examine whether a person earned a diploma at the individual level, and the graduation rate of the different birth cohorts (or presumed birth cohorts) at the aggregate level. The latter is calculated for each birth cohort officially affected by the policy—1985, 1986, 1987 and 1988—and for each previous birth cohort available. After selecting the corresponding survey and birth cohorts, we calculated the ratio of who earned a diploma versus all people who earned a lower secondary qualification. The calculation is made at age 23¹¹ to take into account students with irregular careers, who may graduate up to 4 years later.

The other variables used in the analyses are: birth cohort; geographical area; sex; month of birth; mother's and father's level of education (classified as up to primary school, lower secondary school, upper secondary school, tertiary education); and mother's and father's occupational level (unskilled, blue collar, white collar, high skilled). Unfortunately, it is not possible to get information on school tracks, previous school career and nationality. As far as we know, however, Labour Force Surveys are the only data available with sufficient observations to allow analyses of early school-leavers by birth cohort. These data, even with the abovementioned limitations, give us the chance to evaluate a reform that has been almost entirely unexplored and to study a phenomenon—early school-leaving—that is also largely unexplored, at least in Italy.

4.1. Empirical strategy

The effect of the Berlinguer reform is evaluated using a counterfactual approach (Morgan and Winship 2007). This reform was implemented nationwide simultaneously; thus we can exploit only the time discontinuities relying on a before-after approach. The before-after approach is valid according to stringent assumptions about the absence of pre-treatment trends in the outcomes of interest. Therefore, we apply two distinct analytical strategies based on different definitions of the control group in order to supply robust estimates.

The first strategy is based on counterfactual time series. We exploit the observations prior to the 1999 policy to predict the share of 16- to 17-year-olds in school and the graduation rates that should have been observed if the policy had not been implemented. The differences between predicted and observed values supply a reasonable estimate of the impact of the Berlinguer reform.¹² For these estimates, we rely on aggregated data at the level of geographical area of residence, in order to have observations.¹³ After assessing the forecast strength, we collapse the predictions at national and year level and predict values in the 4

¹⁰ We define the early school-leaving as the time spent in school, that is, the number of years in education. Hence, early school-leavers are students who left school after a certain number of years of education (e.g., 10) or at a certain age (e.g., 16).

¹¹ More precisely, we consider data referring to years in which individuals turn 23.

¹² This strategy was used by Schizzerotto and Vergolini (2015) to estimate the effects of the Bologna process on university enrolment in Italy.

¹³ Thus, for 16-year-olds' attendance at school who were born in 1985, we have 4 values: North-West, North-East, Centre, South and Islands.

years after the policy. To address the autocorrelation problem, we apply the Prais-Winsten regression (Woolridge 2009). More precisely, we apply the following model:

$$Y_{at} = \alpha + \beta Y_{at,t-1} + \gamma X_{at} + \varepsilon_i$$

The second strategy works as a robustness check for the counterfactual time series and is based on the so-called interrupted time series (Its) implemented through segmented regressions. The rationale of this method is to measure the impact of the intervention as a subsequent deviation of the outcomes of interest from the past pattern (Ramsay et al. 2003; Linden 2015). In this way, it is possible to take into account possible time trends using individual data. If the treatment had an impact, the trend of the outcome will have a different level or slope from those before the treatment (Shadish et al. 2002; Bloom 2003). The assumption is that the trend involving the outcome of interest would remain unchanged in the absence of the policy (Penfold and Zhang 2013). In this way, the counterfactual is represented by the level and the slope of the outcomes' trends. The time series are divided into two segments: the first part comprises the outcomes before the policy, and the second part comprises the outcomes after the policy; then, the level (d) and slope (ta) in the post-intervention period are statistically compared to the ones in the pre-intervention period.

Segmented regressions are performed by means of logistic regressions and we examine possible changes in the outcomes' probability. Each individual is assigned to the pre-policy or post-policy period due to her/his birth cohort going back in the birth cohort as much as possible with available data. The formula applied is:

$$Y_i = \alpha + \beta D_i + \gamma T_i^A + \delta T_i + \theta X_i + \varepsilon_i$$

in which Y is the outcome of interest (attendance at age 16 or 17 or graduation); D is the reform indicator (1 for the treated and 0 for the controls); T_i^A is the time passed after intervention;¹⁴ T is a continuous variable indicating the cohorts of birth from the start of the observation period ($T=1$ for the first cohort observed, $T=2$ for the second cohort observed and so on); X is a group of covariates that may affect early school-leaving and are added in the second step of the model; and ε is the error term (Wagner et al. 2002; Zhang et al. 2009; Penfold and Zhang 2013).

In this model, the intercept estimates the baseline level of the outcome; T estimates the baseline slope, i.e., the change in the outcome that occurs with each year before the intervention; D estimates the change in level immediately after the intervention; and T_i^A estimates the change in the slope after the intervention. The tables report the odds ratios in order to supply a more immediate and intuitive interpretation.

5. Results

This section is divided into two parts. In the first part, we present the main effects of the reform on the attendance and graduation rates. The second part is devoted to the analysis of the heterogeneous effects. We also wish to ascertain if the reform had different effects on students from different socio-economic backgrounds.

5.1. Main effects of the reform

Table 1 reports the estimates of the Prais-Winsten regression for the pre-reform period. It is notable that there is an enormous effect of the outcomes at $t-1$. This means that the school

¹⁴ It is a continuous variable that counts how many years after the implementation a person was affected by the intervention. It is equal to 0 for people not affected by the reform, to 1 for people born in 1985 (the first year of implementation), to 2 for people born in 1986, and so on.

behaviour of students (and families) in a given year is strongly influenced by the behaviour of students (and families) in the previous years. Moreover, the models produce accurate estimates before 1999, certified by the fact that the R^2 values for all the models are always higher than 0.98. We can also infer the goodness of our predictions by looking the difference between observed and predicted observations that are on average very small (Figure 1 and Table A1). Therefore, we can be confident about these models' abilities to produce sound counterfactual estimates of our outcomes. Figure 1 shows intuitively the observed and predicted values of our outcomes, and Table A1 in the appendix illustrates the precise numerical differences. The effect of the reform is retrieved comparing the observed values with the counterfactual ones after 1999.

Table 1 – Prais-Winsten regression of attendance and graduation rates before the 1999 reform.

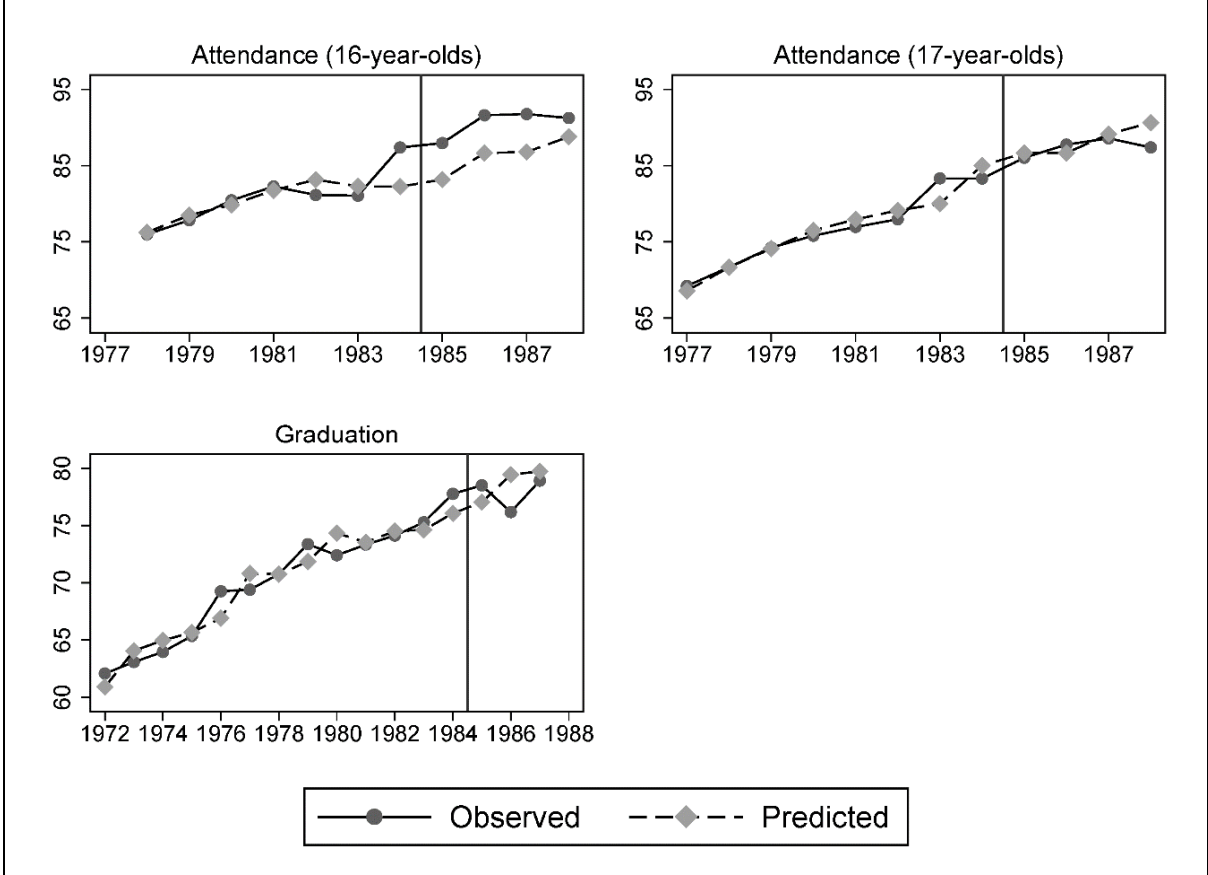
	Attendance				Graduation	
	16-year-olds		17-year-olds		Coeff.	S.E.
	Coeff.	S.E.	Coeff.	S.E.		
<i>Outcome at t-1</i>	0.74***	0.11	0.94***	0.05	0.92***	0.05
<i>Geographical area of residence</i>						
North-west	Ref.	–	Ref.	–	Ref.	–
North-east	0.17	1.06	0.20	0.70	-0.16	0.70
Central	1.21	1.17	-0.34	0.77	-0.35	0.71
South and Islands	-1.52	1.03	-0.55	0.70	-1.32*	0.76
<i>Constant</i>	23.09**	8.30	6.75*	3.77	7.84**	3.43
Observations	28		32		52	
R ²	0.99		0.99		0.98	
Durbin-Watson (original)	2.20		2.78		2.39	
Durbin-Watson (transformed)	1.99		2.21		1.77	

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.
Source: Own calculations on Labour Force Survey data.

The extension of compulsory schooling up to age 15 affected 16-year-olds at least in the first three years: a larger share was in school after 1999 with respect to what would have been expected in the absence of the policy. In particular, the percentage of 16-year-olds in school is almost 5 points higher than its counterfactual value for youths born in 1985, 1986 and 1987 (4.78, 4.95 and 4.97 percentage points, respectively), while the gap diminishes, but remains positive, in the 1988 cohort (2.45 percentage points). The increase started one year before, however: +5.13 p.p. for youths born in 1984. This could be an anticipatory effect: 16-year-olds born in 1984 attended school for a longer time because of the announcement of the policy. They may have decided to attend school for at least two reasons. First, they (and their families) may have known about the policy from media, teachers or from younger students. They may have been worried about being the last cohort not to have started upper secondary education, which could have consequences in terms of social status or the labour market: employers could choose youths from younger cohorts because even if they left school early, they attended at least one year of a vocational school. Second, they may have just 'followed the flow' of information. If attention is given to upper secondary schooling, it should be important and it could be rational to continue studies. Moving on to seventeen-

year-olds, their school attendance did not significantly increase due to the policy (Figure 1 and Table A1). In fact, observed and counterfactual values present very small differences that are comparable to differences detected before the policy enactment.

Figure 1 – Trends over time of observed and predicted attendance and graduation rate (Prais-Winsten, weighted percentages).



Source: Own calculations on Labour Force Survey data.
 Note: The vertical line distinguishes the cohorts affected and not-affected by the policy.

From the comparison between observed and predicted values of graduation rates (Figure 1 and Table A1), a null effect of the reform emerges. In fact, also in this case, the distance between observed and counterfactual trends is substantially negligible. Thus, from these results, it seems that the initial advantage in school attendance detected after the reform for 16-year-olds gradually diminished for 17-year-olds and completely disappeared in terms of graduation rates.

As a robustness check, we run segmented regression analyses on individual level data with two different specifications: with (models b) and without (models a) a set of control variables (Table 2). It emerges that after the reform, the probability of attending school at age 16 and 17, expressed by the variable 'intervention', increased. The higher propensity to attend school after the policy at age 17 contrasts with the null effect found with the counterfactual time series analyses. This discrepancy can be explained by focusing attention on the variable 'time after intervention' that indicates the change in slope after the implementation of the reform. We notice that for 16-year-olds, the effect is null (the odds ratio is practically one on both the specifications), while for the 17-year-olds, it is negative and statistically significant, indicating a flatter trend in the propensity to be in school after the increase of compulsory schooling.

Table 2 – Segmented regression of the attendance and graduation probabilities before and after the 1999 reform, odds ratios.

	Attendance												Graduation			
	16-year-olds				17-year-olds				17-year-olds (without ta)				(a)		(b)	
	(a)		(b)		(a)		(b)		(a)		(b)		(a)		(b)	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
<i>Intervention (d)</i>	1.38**	0.17	1.38**	0.17	1.33**	0.14	1.42***	0.16	1.02	0.07	1.09	0.07	0.84*	0.08	0.80**	0.09
<i>Time after int. (ta)</i>	0.98	0.05	1.00	0.05	0.89**	0.04	0.88**	0.04					1.03	0.04	1.05	0.04
<i>Time (t)</i>	1.11***	0.01	1.08***	0.01	1.14***	0.01	1.10***	0.01	1.13***	0.01	1.09***	0.01	1.07***	0.00	1.03***	0.00
<i>Sex</i>																
Female			1.16***	0.05			1.34***	0.05			1.34***	0.05			1.94***	0.06
<i>Area of residence</i>																
North-East			1.18**	0.08			1.08	0.06			1.08	0.06			1.18***	0.06
Central			1.35***	0.09			1.37***	0.08			1.37***	0.08			1.07	0.05
South and Islands			1.07	0.06			1.17***	0.06			1.17***	0.05			0.87***	0.03
<i>Parental</i>																
Lower secondary			2.07***	0.10			2.05***	0.09			2.05***	0.09			1.78***	0.06
Upper secondary			3.71***	0.24			4.60***	0.27			4.59***	0.27			4.71***	0.23
Tertiary			5.12***	0.60			8.21***	0.91			8.22***	0.91			11.79***	1.36
<i>Parental</i>																
Blue collar			1.23***	0.06			1.22***	0.05			1.22***	0.05			1.02	0.04
White collar			1.65***	0.10			1.77***	0.09			1.77***	0.09			1.45***	0.06
High skilled			2.08***	0.16			2.13***	0.14			2.12***	0.14			2.18***	0.12
Observations	25864		25864		28262		28262		28262		28262		37408		37408	
Pseudo-R ²	0.031		0.105		0.034		0.134		0.033		0.133		0.016		0.148	

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The reference categories are: Male; North-West; Primary; Low skilled.

Source: Own calculations on Labour Force Survey data.

Thus, it could be possible that the effect of the policy is relevant only after controlling for the slowdown of the slope, while it would disappear if we consider it globally (i.e., without distinguishing between level and slope as in the counterfactual time series). To check this intuition, we estimate the model removing the variable ‘time after intervention’. As expected, the bump almost disappears and becomes not statistically significant.¹⁵

Table 2 shows the results of the segmented regression analyses for the graduation rate. After the Berlinguer reform, the probability of earning a degree not only did not increase, but even diminished. As we have seen in the counterfactual time series analysis (Figure 1), graduation rates were slightly higher (almost similar) in the 1985 and 1988 cohorts, but decreased in the 1986 and 1987 cohorts. It is likely that the lower probability observed after the reform is due to the lower graduation rates of these two cohorts.

5.2. Heterogeneous effects

We performed counterfactual time series analyses¹⁶ stratified according to parental education and parental social class¹⁷ to test whether the Berlinguer reform was able to reduce social inequalities.

As in the previous analyses, the reform shows a higher impact on 16-year-olds in school, and the effect is particularly noteworthy for students with a disadvantaged family background. School participation grows up to 8.99 percentage points for youths with less-educated parents and up to 11.96 percentage points for students with parents in a low occupational class (Figure 2, Table A2). Students from advantaged social strata also attend school to a greater degree, but with less pronounced differences (especially in upper-class families). The big gain for youths with disadvantaged family backgrounds decreases for 17-year-olds. If they have parents with low education and from a low occupational class, they stay in school slightly longer after the compulsory schooling extension (maximum 5 percentage points) in the 1985 and 1986 cohorts (Figure 2, Table A2). In the third and fourth cohort after implementation, however, the difference between observed and counterfactual rates disappears even for youths from disadvantaged socio-economic backgrounds. The results also show that 17-year-olds with more advantaged family backgrounds do not change their behaviour due to compulsory schooling extension.

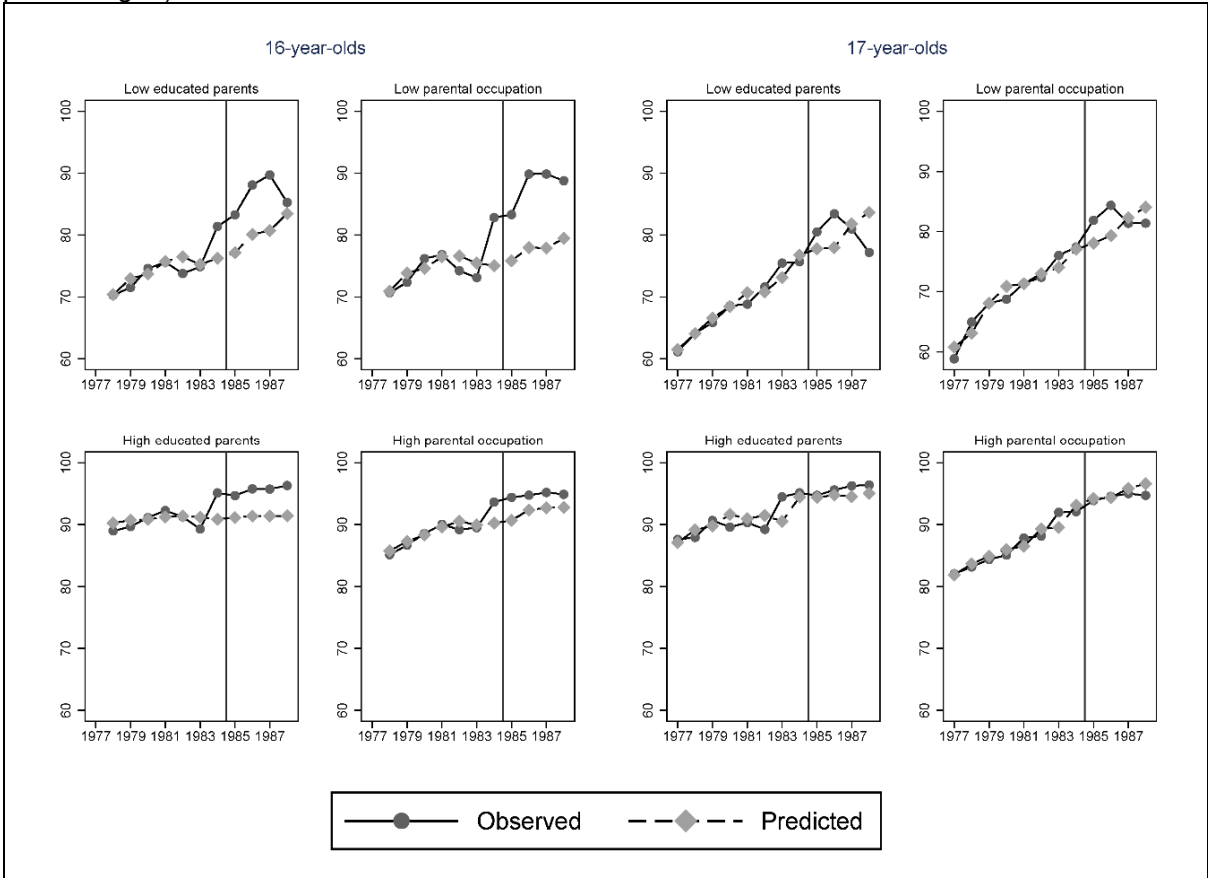
Stratifying by family background does not change the impression that the 1999 compulsory schooling extension was not able to influence the probability of attaining a diploma. From Figure 3, it is clear that individuals with less-educated parents and/or with parents from lower social classes have significantly lower graduation rates than those from affluent families. This state of affairs does not change after the policy implementation: graduation rates do not increase with respect to what would have occurred without the reform for youths from disadvantaged socio-economic backgrounds (Figure 3 and Table A3). The distance between actual and counterfactual values increases in the last cohorts (1987 and 1988), but does so regardless of social origins. Unfortunately—due to the policy’s abrogation in 2003—we cannot tell whether the gap between actual and counterfactual rates would have increased many years after the implementation, i.e., whether the policy would have had a ‘long-term’ impact.

¹⁵ We also estimate the regressions without ‘time after intervention’ on 16-year-olds, and as expected, in that case, almost nothing changed ($d=1.32$ and $d=1.38$, respectively, for a and b models, both significant $p<0.01$).

¹⁶ Given the equivalence of the results between the two analytical strategies, in this sub-section, we rely only on the counterfactual time series.

¹⁷ We code parental education and class in two categories. We consider parents with at most a lower secondary qualification as low educational level (and with a diploma or more as high educational level), and unskilled and blue-collar parents as low occupational class (and white-collar and high-skilled parents as high occupational class).

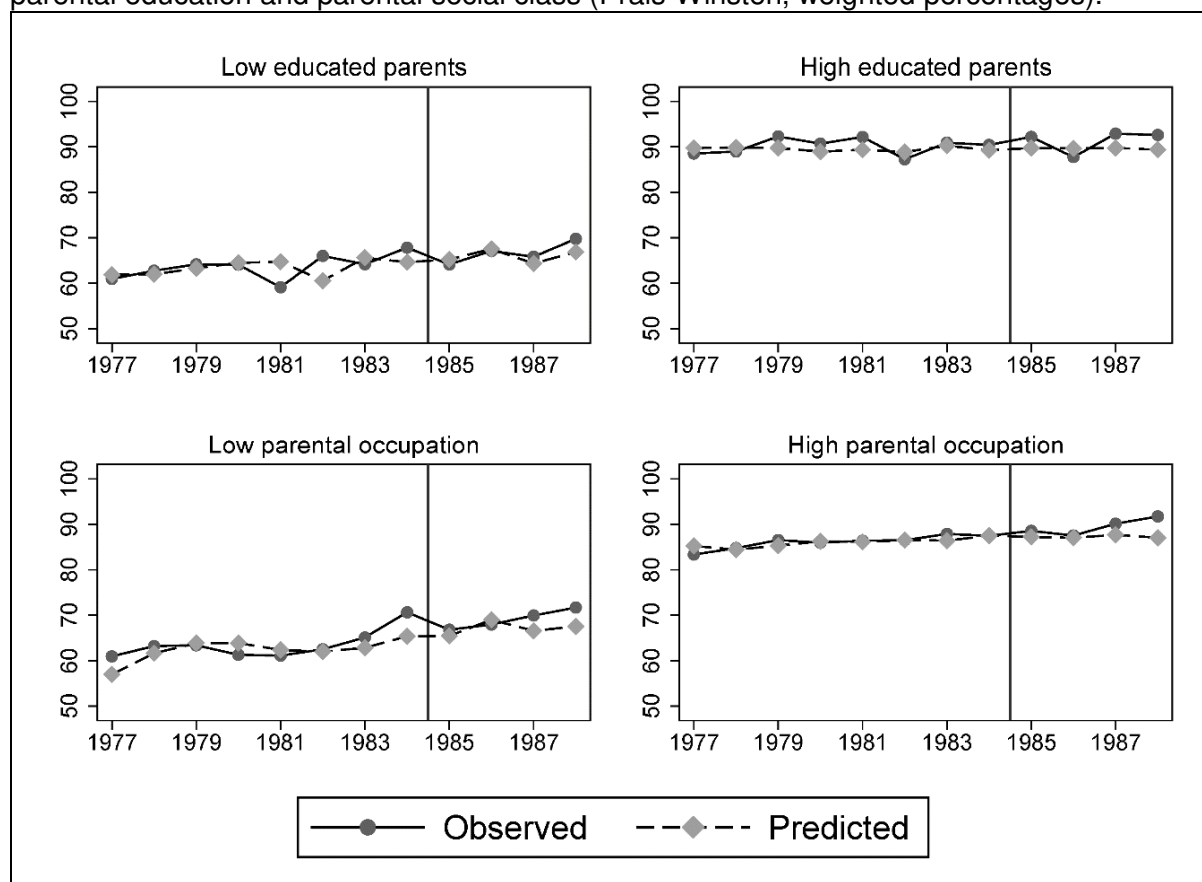
Figure 2 – Trends over time of observed and predicted attendance rates for 16- and 17-year-olds according to parental education and parental social class (Prais-Winsten, weighted percentages).



Source: Own calculations on Labour Force Survey data.

Note: The vertical line distinguishes the cohorts affected and not-affected by the policy.

Figure 3 – Trends over time of observed and predicted graduation rates according to parental education and parental social class (Prais-Winsten, weighted percentages).



Source: Own calculations on Labour Force Survey data.

Note: The vertical line distinguishes the cohorts affected and not-affected by the policy.

6. Conclusion

Despite the limits of available data, the results emerging from the two different analytical strategies are quite clear. The Berlinguer reform was able to increase youths' total amount of years of schooling, confirming the results of a recent study (Schizzerotto et al. 2017). In fact, a greater share of 16-year-olds attended school when they were no longer obliged to, with an overall gain of approximately 4 percentage points. If the policy had led all 16-year-olds to attend school, the gain would have been 17 points. This means that school participation grew after the reform by approximately a quarter of its maximum effect. Moreover, the consequences of compulsory schooling expansion were more marked for students from less advantaged socio-economic backgrounds.

This is the only undeniable influence of the 1999 policy. The significant influence among 16-year-olds diminished for 17-year-olds. Seventeen-year-olds had a higher probability of attending school, but this bump was visible only immediately after the policy, while it disappeared when summed with the trend in all the four years after the intervention. Indeed, this trend became significantly flatter with respect to the continuous increase in school attendance that was ongoing before the policy implementation. Unfortunately, we do not have information on the vocational and training system, and it is possible that the 1999 reform led some students to shift to vocational and training programmes and earn a qualification (if not a diploma) they would have not attained otherwise. If a growing interest in

vocational and training programme started with the 1999 policy, the reform could have been successful in reducing the number early school leavers from education and training. This hypothesis cannot be tested with available data and it should be investigated in future research.

Another possible explanation is that youths who were potentially more affected by the reform experienced retention to a greater degree than those less affected by the policy. Therefore, the former attended one additional year of schooling in lower secondary school, and perhaps completed it, thus attaining a qualification they otherwise would not have earned. This may partially explain the decrease of the effect on 17 year-olds. After lower secondary education—with one year of retention at age 15 and two years at age 16—they simply dropped out or went directly to vocational and training programmes.

The reform left no traces on upper secondary graduation rates. The increase of diploma-earners would have occurred without the reform, as seen through counterfactual time series and segmented regression analyses. The probability of attaining a diploma did not change even for individuals from disadvantaged backgrounds. Thus, neither the educational attainment nor educational inequalities were significantly altered.

The effectiveness of the reform may have been compromised by at least four factors. First is the lack of information on 15-year-olds and the lack of consequent checks on compliers. Second is the absence of or minor changes to the educational curricula in the first years of upper secondary school after the reform. Third is implementing the obligation to attend one additional year of school without compelling attendance of at least the first year of upper secondary schooling. Fourth is the general perception that the compulsory cycle of education actually finished with the completion of lower secondary school, even if the law stated differently.

The interventions may have been more effective if they were suited to students who are actually at-risk of leaving school before earning a diploma. For example, the promotion of a package of prevention policies would potentially address early school-leaving in a better and more effective way (Mackey and Duncan 2013; Agostino and Reese 2010). Students at risk of dropping out need specific interventions, such as personalized guidance, counselling at school, a diversified educational offer and the chance to personalize a school path to suit their learning teaching needs (OECD 2000; Rossi-Doria 2009; Bifulco et al. 2011).

References

- Agostino, Becky, and Alex Reese. 2010. “Does raising the compulsory school attendance age increase graduation rates?”, Policy Brief, Durham, NC : Duke University.
- Ballarino, Gabriele, Ivano Bison, and Hans Schadee. 2011. “Abbandoni scolastici e stratificazione sociale nell'Italia contemporanea.” *Stato e mercato*, 93: 479–518. doi:10.1425/35981.
- Bifulco, Lavinia, Raffaele Monteleone, Carlotta Mozzana, and Irene Rolfini. 2011. “The institutional mapping of Italy’s education system: Europeanization, centralization and regionalization.” 2nd International Workshop, FP7 EU Research Project Making Capabilities Work – WORKABLE, Lausanne, 18-20 November.
- Borgna, Camilla, and Emanuela Struffolino. 2017. “Pushed or pulled? Girls and boys facing early school leaving risk in Italy.” *Social Science Research*, 61: 298–313. <https://doi.org/10.1016/j.ssresearch.2016.06.021>.
- Brilli, Ylenia, and Marco Tonello. 2015. “Rethinking the crime reducing effect of education? Mechanisms and evidence from regional divides.” *Temi di Discussione*, Bank of Italy.

- Brunello, Giorgio, Margherita Fort, and Guglielmo Weber. 2009. "Changes in Compulsory Schooling, Education and the Distribution of Wages in Europe." *The Economic Journal*, 119, 516–539. doi:10.1111/j.1468-0297.2008.02244.x.
- Bukodi, Erzsébet, and John H. Goldthorpe. 2013. "Decomposing social origins: The effects of parents' class, status and education on the educational attainment of their children." *European Sociological Review*, 29 (5): 1024–1039.
doi:<https://doi.org/10.1093/esr/jcs079>.
- Cabus, Sofie J., and Kristof De Witte. 2016. "Why do students leave education early? Theory and evidence on high school dropout rates." *Journal of Forecasting*, 35: 690–702.
DOI:10.1002/for.2394.
- Cecchi, Daniele. 2010. "Percorsi scolastici e origini sociali nella scuola italiana." *Politica economica*, 25 (3): 359–387. doi:10.1429/33587.
- Collins, Randall. 1979. *The credential society: a historical sociology of education and stratification*. New York: Academic Press.
- Contini, Dalit, and Andrea Scagni. 2013. "Social-Origin Inequalities in Educational Careers in Italy." In *Determined to Succeed?*, edited by Michelle Jackson, 149–184. Stanford: Stanford University press.
- De Witte, Kristof, Ides Nicaise, Jeroen Lavrijsen, Georges Van Landeghem, Carl Lamote, C., and Jan Damme. 2013. "The Impact of Institutional Context, Education and Labour Market Policies on Early school-leaving: a comparative analysis of EU countries." *European Journal of Education*, 48 (3): 331–345. doi:10.1111/ejed.1203.
- European Commission. 2010. "Reducing early school-leaving." Commission staff working paper, Brussels.
- Fiorio, Carlo, and Marco Leonardi. 2010. "Mobilità intergenerazionale nell'istruzione in Italia". In *Immobilità diffusa. Perché la mobilità intergenerazionale è così bassa in Italia*, edited by Daniele Checchi, 51–34. Bologna, Il Mulino.
- Hodgson, David. 2016. "What do you want to do your life? Ethics and compulsory education." *Journal of Education Policy*, 31 (4): 495–507.
- Landis, Rebecca N., and Amy L. Reschly .2010. "An examination of compulsory school attendance ages and high school dropout completion." *Educational Policy*, 20 (10): 1–43. doi:10.1177/0895904810374851.
- Linden, Ariel. 2015. "Conducting interrupted time series analysis for single and multiple group comparisons." *The Stata Journal*, 15 (2): 480–500.
- Mackey, Philip E., and Teresa G. Duncan. 2013. "Does raising the state compulsory school attendance age achieve the intended outcomes?" Washington, DC: U.S. Department of Education.
- Morgan, Stephen L., and Christopher Winship. 2007. *Counterfactuals and Causal Inference: Methods and Principles for Social Research*. Cambridge: Cambridge University press.
- Mocetti, Sauro. 2012. "Educational choices and the selection process before and after compulsory schooling." *Education Economics*, 20 (2): 189–209.
<http://dx.doi.org/10.1080/09645291003726434>
- Nicaise, Ides, Kristof De Witte, Carl Lamote, Jeroen Lavrijsen, J., and George Van Landeghem. 2013. "Towards a basic qualification for all in the EU." *Euroforum KU Leuven*, No. 9.

- O'Higgins, Niall, Marcello D'amato, Floro E. Caroleo, and Adriana Barone, A. 2007. Gone for Good? Determinants of School Dropout in Southern Italy, *Giornale degli Economisti e Annali di Economia*, 66 (2): 207–245.
- Stable URL: <http://www.jstor.org/stable/23248213>.
- OECD. 2000. *From Initial Education to Working Life. Making Transitions Work*. Paris: OECD Publications Service.
- Oreopoulos, Philip. 2007. “Do dropouts drop out too soon? Wealth, health and happiness from compulsory schooling.” *Journal of Public Economics*, 91 (11-12): 2213–2229. <https://doi.org/10.1016/j.jpubeco.2007.02.002>.
- Oreopoulos, Philip .2009. “Would More Compulsory Schooling Help Disadvantaged Youth? Evidence from Recent Changes to School-Leaving Laws.” In *The Problems of Disadvantaged Youth: An Economic Perspective*, edited by Jonathan Gruber, 85–112. Chicago: University of Chicago Press.
- Pisati, Maurizio. 2002. “La partecipazione al Sistema scolastico”. In *Vite ineguali. Disuguaglianze e corsi di vita nell'Italia contemporanea*, edited by Antonio Schizzerotto, 141–186. Bologna: Il Mulino.
- Rossi-Doria Marco. 2009. *Di mestiere faccio il maestro*. Napoli: L' Ancora del Mediterraneo.
- Shavit, Yossi, and Karin Westerbeek. 1998. “Educational stratification in Italy. Reforms, expansion, and equality of opportunity.” *European Sociological Review*, 14 (1): 37–47. doi: <https://doi.org/10.1093/oxfordjournals.esr.a018226>.
- Schizzerotto, Antonio. 1997. “Perché in Italia ci sono pochi diplomati e pochi laureati?” *Polis*, 11 (3): 345–365.
- Schizzerotto, Antonio, Giovanni Abbiati, and Loris Vergolini. 2017. “L'influenza delle riforme scolastiche e delle crisi economiche sulla partecipazione ai processi formative in Italia dall'inizio del XX secolo ad oggi.” forthcoming in *Annali di Statistica*, Istat.
- Schizzerotto, Antonio, and Loris Vergolini. 2015. The Bologna process in Italy. A reassessment of its influences on university enrolments and inequalities of educational opportunities. Paper presented at the RC28 Spring Meeting, Tilburg, May, 28-30.
- Walters, Pamela B. 2000. “The limits of growth. School expansion and school reform in historical perspective.” In *Handbook of the sociology of education*, edited by Maureen T. Hallinan, 241–261. New York: Kluwer.
- Woolridge, Jeffrey M. 2009. *Introductory Econometrics. A Modern Approach*. Mason: South-Western Cengage Learning.

Appendix

Table A1 – Differences between observed and predicted attendance and graduation rates (Prais-Winsten, weighted percentage).

Year	Attendance						Graduation		
	16-year-olds			17-year-olds			Observed	Predicted	Difference
	Observed	Predicted	Difference	Observed	Predicted	Difference			
1972							62.07	60.89	1.19
1973							63.07	64.05	-0.98
1974							63.96	64.96	-0.99
1975							65.35	65.66	-0.31
1976							69.26	66.90	2.35
1977				69.21	68.58	0.64	69.40	70.81	-1.40
1978	75.99	76.24	-0.25	71.68	71.67	0.01	70.76	70.75	0.01
1979	77.85	78.50	-0.65	74.25	74.12	0.13	73.38	71.85	1.52
1980	80.45	79.86	0.59	75.81	76.50	-0.68	72.41	74.34	-1.93
1981	82.27	81.76	0.52	76.96	77.95	-0.99	73.34	73.54	-0.20
1982	81.15	83.17	-2.02	77.97	79.13	-1.16	74.13	74.52	-0.39
1983	81.06	82.29	-1.22	83.33	80.00	3.33	75.29	74.62	0.67
1984	87.38	82.25	5.13	83.31	85.01	-1.69	77.78	76.07	1.71
1985	87.96	83.18	4.78	86.04	86.65	-0.61	78.52	77.05	1.47
1986	91.61	86.66	4.95	87.76	86.66	1.09	76.18	79.45	-3.26
1987	91.76	86.79	4.97	88.58	89.14	-0.56	78.93	79.75	-0.81
1988	91.24	88.79	2.45	87.39	90.64	-3.26	80.27	77.80	2.47

Source: Own calculations on Labour Force Survey data.

Note: The dotted line distinguishes the cohorts affected and non-affected by the policy.

Table A2 – Differences between observed and predicted attendance rates for 16- and 17-year-olds according to parental education and parental social class (Prais-Winsten, weighted percentage).

Year	16-year-olds											
	Low educated parents			High educated parents			Low parental occupation			High parental occupation		
	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.
1978	70.27	70.37	-0.10	88.98	90.25	-1.27	70.69	70.91	-0.22	85.15	85.75	-0.60
1979	71.53	72.97	-1.44	89.68	90.73	-1.05	72.37	73.87	-1.50	86.68	87.31	-0.63
1980	74.59	73.70	0.89	91.15	90.86	0.29	76.19	74.63	1.56	88.55	88.32	0.23
1981	75.61	75.74	-0.13	92.27	91.26	1.01	76.84	76.48	0.36	90.04	89.60	0.44
1982	73.79	76.47	-2.68	91.26	91.38	-0.12	74.24	76.63	-2.39	89.19	90.55	-1.36
1983	74.89	75.27	-0.38	89.29	91.20	-1.91	73.13	75.47	-2.34	89.51	89.94	-0.43
1984	81.40	76.24	5.16	95.13	90.86	4.27	82.83	75.05	7.78	93.64	90.25	3.39
1985	83.27	77.16	6.11	94.68	91.14	3.54	83.31	75.85	7.46	94.40	90.69	3.71
1986	88.09	80.10	7.99	95.78	91.39	4.39	89.85	77.98	11.87	94.75	92.37	2.38
1987	89.71	80.72	8.99	95.74	91.39	4.35	89.86	77.90	11.96	95.20	92.71	2.49
1988	85.27	83.47	1.80	96.32	91.42	4.90	88.77	79.46	9.31	94.90	92.80	2.10

Year	17-year-olds											
	Low educated parents			High educated parents			Low parental occupation			High parental occupation		
	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.
1977	61.14	61.52	-0.38	87.62	87.09	0.53	58.85	60.80	-1.95	82.05	81.87	0.18
1978	64.11	64.09	0.02	87.93	89.15	-1.22	64.97	63.09	1.88	83.18	83.68	-0.50
1979	65.86	66.61	-0.75	90.67	89.78	0.89	68.09	68.10	-0.01	84.37	84.88	-0.51
1980	68.57	68.45	0.12	89.61	91.65	-2.04	68.78	70.91	-2.13	85.09	85.99	-0.90
1981	68.83	70.70	-1.87	90.33	90.96	-0.63	71.41	71.33	0.08	87.85	86.53	1.32
1982	71.63	70.82	0.81	89.24	91.49	-2.25	72.41	73.01	-0.60	88.17	89.32	-1.15
1983	75.47	73.13	2.34	94.49	90.52	3.97	76.04	74.05	1.99	91.98	89.56	2.42
1984	75.69	76.77	-1.08	95.14	94.45	0.69	77.43	77.06	0.37	92.10	93.13	-1.03
1985	80.50	77.77	2.73	94.72	94.40	0.32	81.88	78.08	3.80	93.90	94.22	-0.32
1986	83.44	77.97	5.47	95.61	94.76	0.85	84.36	79.34	5.02	94.56	94.36	0.20
1987	80.99	81.79	-0.80	96.27	94.52	1.75	81.37	82.31	-0.94	95.01	95.87	-0.86
1988	77.20	83.66	-6.46	96.40	95.05	1.35	81.39	84.06	-2.67	94.72	96.59	-1.87

Source: Own calculations on Labour Force Survey data.

Note: The dotted line distinguishes the cohorts affected and non-affected by the policy.

Table A3 – Differences between observed and predicted graduation rates according to parental education and parental social class (Prais-Winsten, weighted percentage).

Year	Low educated parents			High educated parents			Low parental occupation			High parental occupation		
	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.	Obs.	Pre.	Diff.
1972	54.90	54.34	0.56	89.70	89.75	-0.05	55.19	54.00	1.19	80.51	79.00	1.51
1973	55.75	56.91	-1.16	87.91	89.51	-1.60	54.43	56.96	-2.53	78.50	82.09	-3.59
1974	56.08	57.65	-1.57	88.27	89.99	-1.72	53.09	56.51	-3.42	81.07	80.67	0.40
1975	57.15	57.74	-0.59	88.28	89.89	-1.61	55.80	54.80	1.00	82.39	82.59	-0.20
1976	60.83	58.62	2.21	89.16	89.92	-0.76	55.12	57.54	-2.42	84.75	83.54	1.21
1977	60.94	61.85	-0.91	88.53	89.75	-1.22	60.94	56.98	3.96	83.36	85.25	-1.89
1978	62.74	61.97	0.77	88.97	89.85	-0.88	63.21	61.62	1.59	84.76	84.42	0.34
1979	64.15	63.28	0.87	92.29	89.75	2.54	63.38	63.86	-0.48	86.54	85.32	1.22
1980	64.06	64.49	-0.43	90.71	88.95	1.76	61.29	63.87	-2.58	85.97	86.26	-0.29
1981	59.11	64.71	-5.60	92.17	89.36	2.81	61.11	62.37	-1.26	86.39	86.14	0.25
1982	66.00	60.55	5.45	87.27	88.85	-1.58	62.50	62.00	0.50	86.51	86.53	-0.02
1983	64.17	65.61	-1.44	90.91	90.27	0.64	65.11	62.84	2.27	87.89	86.42	1.47
1984	67.81	64.62	3.19	90.43	89.26	1.17	70.64	65.36	5.28	87.48	87.50	-0.02
1985	64.12	65.22	-1.10	92.19	89.73	2.46	66.81	65.45	1.36	88.56	87.24	1.32
1986	67.09	67.53	-0.44	87.75	89.66	-1.91	67.95	69.00	-1.05	87.53	87.04	0.49
1987	65.81	64.33	1.48	92.90	89.74	3.16	69.94	66.56	3.38	90.14	87.69	2.45
1988	69.75	66.87	2.88	92.61	89.39	3.22	71.68	67.55	4.13	91.74	87.05	4.69

Source: Own calculations on Labour Force Survey data.

Note: The dotted line distinguishes the cohorts affected and non-affected by the policy.