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The effect of education on earnings dynamics: evidence from Italy

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THE EFFECT OF EDUCATION ON EARNINGS DYNAMICS: EVIDENCE FROM ITALY[1]

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Abstract

We provide a detailed picture of the causal relationship between schooling and earnings dynamics by relying on the Italian case and estimating the effect of education on earnings level, mobility and volatility over the career. To our aim, we exploit the 1962 reform that extended compulsory schooling from 5 to 8 years and adopt a regression discontinuity design. We do not find a statistically significant effect of education's increase on men's earnings, whereas we find that extra schooling increases female earnings. We also find that, for female workers, the increase in compulsory education established by the reform contributed to reduce earnings mobility. Finally, we find a high degree of heterogeneity across regional macro-areas in terms of compliance with the policy and, consequently, in the effect of education on earnings

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1. Introduction

Extensive literature has evaluated the impact of education on the level of earnings (e.g., Oreopoulos, 2006; Brunello et al., 2009; Grenet, 2013; Brunello et al., 2017; Clark, 2023), but less is known about the causal relationship between education and earnings dynamics[2]. Looking at the earnings pattern means analyzing not only the effect of education on the level of earnings at a certain point in time (or, on average, along the career pattern) but also on the degree of earnings mobility that the worker experienced throughout her career and on the degree of stability of the income stream, i.e., earnings volatility.

Although there is strong evidence of a positive association between education and labor market outcomes, it is not easy to establish a causal relationship between the two because of the existence of unobservable characteristics (such as ability and motivation) associated with both education and earnings outcomes (Card, 1999; Webbink, 2005; Holmlund et al. 2011). Since Angrist and Krueger (1991), a large body of literature has used compulsory schooling reforms as an exogenous variation in educational attainment to estimate the causal effect of education on earnings and labor market outcomes[3]. This approach relies on the assumption that changes in years of compulsory education affect outcomes only by affecting years of schooling. Major educational reforms implemented by many Western countries between the 1940s and the 1960s, which increased the number of mandatory schooling years, contributed to expanding this body of literature by creating settings that mimicked the conditions of a randomized experiment (Angrist & Krueger, 2001).

However, the evidence from these studies on the returns to education in terms of earnings is mixed and has often led to conflicting results, even when analyzing the same reform. Early studies investigating the effect of education on earnings in the UK (Harmon & Walker, 1995; Oreopoulos, 2006) and the

[2] There is a large empirical literature that identifies the causal effects of education also on various non-pecuniary outcomes, such as religious practices and superstitious beliefs (Mocan & Pogorelova, 2017), health (Brunello et al., 2016; Barcellos et al., 2023), mental health (Avendano et al., 2020), crime (Lochner & Moretti, 2004; Machin et al., 2011), and fertility (Fort et al., 2016).
[3] Other studies have used different instruments to identify the casual effect of education on earnings, such as proximity to the nearest school (Card, 1995), as well as the construction of new schools (Duflo, 2001) and geographical expansion of university system (Suhonen & Karhunen, 2019)

USA (Acemoglu & Angrist, 2001) reported positive returns of 10 to 15 percent for each additional year of schooling. However, more recent studies analyzing the same policies in these countries (Devereux & Hart, 2010; Clark, 2023, for the UK, and Stephens & Yang, 2014, for the USA) suggest that the impact of education on subsequent earnings is smaller or even negligible. Small or zero returns to compulsory schooling have been also found in the Netherlands (Oosterbeek and Webbink, 2007), Germany (Pischke and von Wachter 2008) and France (Grenet, 2013)[4].

It is critically important to assess whether education truly has no effect on earnings, as such a finding would fundamentally challenge the validity of policy decisions traditionally grounded in the assumptions of human capital theory. Obviously, the diverging results reported in the literature may stem from differences in the methodologies employed, the data sources used, or the fact that various studies have analyzed distinct reforms affecting groups of compliers that vary in size and characteristics. Another plausible explanation for this lack of consensus lies in the fact that education may have an impact that varies over the course of an individual's life (Buscha & Dickson, 2012; Bhuller et al., 2011, 2017). Consequently, when current earnings are used as a proxy for lifetime earnings, the results may depend on the specific period during which earnings are measured (Buscha & Dickson, 2015). To minimize the significant life-cycle bias that may distort estimates of returns to schooling when based on current earnings (Haider & Solon, 2006; Bhuller et al., 2011), it is essential to observe workers' earnings over a substantial portion of their careers, enabling a more accurate approximation of permanent earnings. For this reason, in this paper, we leverage high-quality administrative data from Italy to compute precise measures of permanent earnings and provide new insights into the effect of education on earnings. Access to detailed earnings histories allows us to explore not only the impact of education on earnings levels but also its influence on various aspects of workers' careers. Specifically, we aim to assess how education affects earnings dynamics, focusing on its role in shaping earnings volatility and career mobility. While the limited evidence on the relationship between education and volatility suggests that higher levels of education reduce earnings volatility (Delaney & Devereux,

[4] However, conflicting results have also recently emerged for these countries. Contrary to Grenet (2013), Domnisoru (2021) found a positive impact of the same policy on children of low-educated parents. Additionally, a recent study for Germany reported returns of 6–8 percent (Cygan-Rehm, 2022).

2019), to the best of our knowledge, there is no existing research investigating the effect of education on career mobility.

To this aim we adopted a quasi-experimental design to extract exogenous educational variations and sought a 'natural' experiment, like a policy reform, that exogenously altered individuals' educational levels without directly affecting labor market outcomes. To our scopes, the most proper policy shock was due to the reform of the lower secondary education (*Riforma Scuola Media Unica*) which was approved in 1962 and phased in October 1963. From October 1963, achieving a lower secondary degree became mandatory, and as a result, the years of compulsory schooling increased from 5 to 8 [5]. the number of compulsory years of schooling enabled a large increase in the proportion of people born in and from 1950 having attended school for a higher number of years. We identify individuals born in 1950 as the first cohort potentially affected by the reform, as they were 13 years old in 1963. When the reform was implemented, it became mandatory for them to complete the final year of middle school and obtain a diploma. Therefore, 1950 represents a cutoff year such that individuals born before that date attended fewer years in school than those born on or after 1950. We use a Regression Discontinuity design (RDD), which has established itself in this literature as the standard empirical method for obtaining more reliable causal inferences. This method allows us to compare individuals born around 1950 who were subject to different compulsory minimum school-leaving ages. More specifically, because not all individuals born in or after 1950 completed 8 years of education, we implement a fuzzy RDD to estimate the effect of education on earnings. Although there was a sharp rise in middle school enrollment rates starting in the 1963–1964 school year (Brandolini & Cipollone, 2002), compliance with the 1963 reform was, in fact, gradual. The proportion of children attending middle school reached nearly 100% only by 1976 (Checchi, 1997). In particular, there were significant differences between the North-Center regions, where enrollment rates increased more rapidly, and the South regions, where growth was slower.

[5] The reform also mandated the postponement of academic tracking until the age of 14.

To estimate the effect of education on earnings dynamics, we exploit the innovative longitudinal AD–SILC dataset built matching the 2004-2017 cross-sections of IT–SILC (the Italian component of the EU-SILC) which records information on education with the administrative archives of the Italian National Social Security Institute (INPS) which track the working careers of all individuals interviewed in IT-SILC.

To give a thorough picture of the relationship between schooling and earnings dynamics, we compute several measures representing different aspects of the earnings pattern. All individuals in our sample are observed from age 35 to age 45. As concerns earnings level, we compute a multiyear measure of earnings by averaging earnings from age 35 to 45. To evaluate the impact of education on the level of earnings mobility that the worker experienced throughout her career, we calculate the individual income growth rate in the observed period. However, individuals are not only concerned with the level and the growth of their earnings but also with the degree of uncertainty of their income stream. Therefore, we take the annual growth rates of earnings from the age of 35 to 45, and then we compute the standard deviation of these growth rates as the measure of earnings volatility.

This article adds to the extensive literature that exploits compulsory schooling reforms to study the causal impact of education on earnings. For Italy, we are not the first to examine school reforms as sources of exogenous variation in years of education. Brunello and Miniaci (1999) and Brunello et al. (2000) used the 1969 reform that liberalized access to university as an institutional source of variation in schooling. This reform also allowed graduates from technical institutes and vocational schools (*istituti professionali*) to enroll in university programs across nearly all faculties.[6] Brunello and Miniaci (1999) find a return to education of 5.7 percent for males, while Brunello et al. (2000) find returns of 6 percent for males and 7.7 percent for females. Brandolini and Cipollone (2002), in their analysis of returns to education in Italy, highlight potential issues with using the 1969 reform. Specifically, the reform did not introduce a strictly exogenous change in individuals' educational attainment. Instead, it provided an opportunity for individuals to pursue university studies, with participation remaining

[6] Before 1969, only graduates from academic schools (*Licei*) were always eligible to pursue university education in any major.

voluntary and dependent on personal decisions. Thus, Brandolini and Cipollone (2002), like us, used the 1962 reform of lower secondary education as an instrument for schooling, finding returns of approximately 7–10 percent per year of schooling for female workers. However, unlike their study, which employed an Instrumental Variable approach, this paper contributes to the literature by using a Regression Discontinuity Design (RDD) that allows smoothly control for the evolution of schooling and earnings across cohorts. To the best of our knowledge, our estimates are the only ones for Italy based on an RDD.

Our results show noticeable discontinuities in years of education, particularly when we consider only low-achieving individuals, i.e., those who would not have completed middle school if not for the reform. In particular, the jump in women's education appears larger than in men's. However, we do not find a statistically significant effect of education on men's earnings, whereas we find that extra schooling increases female earnings. This finding contributes to the small body of literature that evaluates the differential impact of education on men's and women's labor market outcomes, finding that compulsory schooling reforms benefited women more than men (Fischer et al., 2016; Aydemir & Kirdar, 2017; De New et al., 2021). Moreover, our estimates suggest that female workers exposed to the reform subsequently had lower earnings mobility. Finally, we do not detect a statistically significant effect of education on the level of volatility the worker experienced throughout the career.

It is important to highlight that our regression discontinuity design estimates Local Average Treatment Effects (Imbens & Angrist, 1994; Hann et al., 2001), as the change in compulsory schooling influences the educational choices of a specific group that may not be representative of the entire population: individuals who obtained more education because of this policy change but would not have pursued further studies without the reform (i.e., the compliers). On the contrary, those already inclined to continue their education remain unaffected by the policy change. However, according to Oreopoulos (2006), when the policy change influences a large proportion of the population, the LATE estimate begins to align more closely with the average treatment effect (ATE).

The paper is organized as follows: Section 2 analyzes the Italian Middle School Reform and Section 3 describes data and the set of outcome variables. Section 4 presents the identification strategy. Sections 5 results. Section 6 investigates possible regional heterogeneity. Section 7 concludes.

2. The mandatory school reform

Currently, the Italian school system is divided into three tiers: primary school, which (normally) begins at age 6, lasts 5 years and provides basic education; lower secondary school (*Scuola Media*), which lasts 3 years (from age 11 to age 13), and upper secondary school (*Scuola Superiore*), which usually lasts 5 years. Upper secondary schools are divided into several types of schools, which are differentiated by subjects and type of programs (i.e. academic schools - *licei* -, technical schools - *istituti tecnici* -, and vocational schools - *istituti professionali*).

Until the beginning of the 1960s, the educational system was highly selective. At age 10 children had to choose between a vocational school (*Scuola di avviamento professionale*), which did not provide access to upper secondary school and prepared students to enter the labor market, and an academic school (*Scuola Media*), which had a selective admission test and was aimed at preparing students for higher education. Therefore, the different schools provided different type and quality of education. Furthermore, under the law in effect until 1962, known as the "Riforma Gentile" introduced in 1926, completing elementary education (a five-year program) was compulsory. In 1962, Law No. 1859 abolished tracking in lower secondary education by creating a comprehensive new lower secondary school (*Scuola Media Unica*) that replaced the two former differentiated schools and allowed students to enroll in all kinds of upper secondary schools. Noteworthy, this reform made enrolment in lower secondary schools mandatory and postponed tracking to the age of 14. Moreover, the lower secondary leaving certificate became mandatory to enter the labor market, thus actually increasing compulsory schooling from 5 to 8 years. Alternatively, those who did not obtain a middle school diploma were released from the obligation if, upon reaching the age of fifteen, they had attended school for at least eight years.

The reform became operative from October 1, 1963. As a result, children who were under 15 years old in 1963 and had not obtained a middle school diploma were compelled to remain in school longer than originally expected when they first enrolled. This displacement effect was probably negligible for those who were 14 years old in 1963, namely those born in 1950. In fact, the reform obviously did not impact students who, at 14, were already in their first year of high school but rather those who were still attending middle school due to having repeated a grade. Since the first school year affected by the reform ran from September 1963 to June 1964, repeaters born in 1950 between January and June—who turned 15 during the school year—were free to drop out without a diploma, as they had already attended school for more than eight years. However, the impact of the law was likely stronger for younger cohorts: those who were 13 in 1963 had to remain in school for an additional year (or even longer if they had repeated a grade, a fairly common situation at the time), while those who were 11 in 1963 were required to stay in school for three more years. Therefore, we consider as the first cohort potentially affected by the reform those born in 1950, that is, those who were in their final year of lower secondary school when the reform was implemented[7].

3. Data and main variables

3.1 Dataset and sample selection

We use data from the AD-SILC longitudinal dataset, developed by merging – using individuals' fiscal codes as the matching key – the INPS administrative archives with the 2004-2017 Italian cross-sectional samples of the European Union Statistics on Income and Living Conditions (IT–SILC).

[7] There is no consensus in the literature on identifying the first cohort potentially affected by the Italian middle school reform, as the law is not very clear. For example, Brandolini and Cipollone (2002) argue that the first affected cohort was born in 1949, while Checchi (2003) states that the reform first impacted those born in 1952, meaning those who started middle school in 1963. As we have stated, our own interpretation differs, and we believe it is supported by the data presented in the following sections, which show a sharp increase precisely for those born in 1950.

The INPS archives included in AD-SILC cover workers' earnings histories of all individuals working in Italy starting from 1975 up to the end of 2018. However, administrative archives do not provide information on workers' education, that is, instead, recorded in IT-SILC[8]. Our sample is then composed by the individuals interviewed in at least one IT-SILC cross-sectional wave, whose longitudinal workers' careers are observed from INPS archives.

The AD-SILC dataset provides information on real gross annual earnings from both employment and self-employment. We use yearly earnings to compute our dependent variables and assess the robustness of our results by also considering weekly earnings. Men and women are analyzed separately.

To estimate the impact of schooling on earnings levels, we start by constructing a multiyear measure of earnings, averaging annual earnings from ages 35 to 45. Annual earnings measures are often a poor proxy for permanent income, as they can fluctuate significantly over time. One key concern is the potential for lifecycle bias, which arises when earnings are observed too early in a person's career, leading to inaccurate estimates of long-term income. To mitigate this issue, research suggests focusing on men in their mid-career years—typically between ages 35 and 45—when earnings tend to be more stable (Haider & Solon, 2006). However, identifying a similar age range for women is more challenging, as their income trajectories tend to be more varied due to factors such as career breaks and part-time work (Bohlmark & Lindquist, 2006).

When constructing our measure of earnings, we exclude workers with fewer than three positive annual earnings observations and do not consider the top and bottom 0.1% of the earnings distribution, which effectively removes observations with zero income.

Table 1 shows the percentage of individuals who attained a middle school diploma by birth cohort. Between 1949 and 1950 (with 1950 as our pivotal cohort), we do not observe a sharp increase in the middle school graduation rate among men, as it rises by only 5 percentage points. However, we see a

[8] The AD-SILC data provides information on each individual's highest qualification achieved, which we convert into years of education by assuming that each qualification took the standard number of years to complete.

significant increase among women, where the rate jumps from 0.62 to 0.72, marking a 10 percentage point increase. A certain increase is also observed among men when we focus specifically on low achievers, defined here as those who obtained a middle school diploma but did not graduate from high school.

In 1950, the middle school graduation rate was only 75% when considering both men and women, despite compulsory schooling until the age of 14. This can be attributed to the widespread presence of informal work in Italy, particularly in the southern regions, where a middle school diploma was not essential for low-skilled workers to find employment. Additionally, rural areas in the South had a weak institutional presence, making it diffucult for authorities to effectively enforce the law

Table 1. Share of individuals with a lower secondary school diploma

	All Students				**Low Achievers**			
	Men		**Women**		**Men**		**Women**	
Cohort	**share**	**obs.**	**share**	**obs.**	**share**	**obs.**	**share**	**obs.**
1940	0.53	1153	0.38	674	0.33	813	0.26	562
1941	0.52	974	0.39	583	0.32	691	0.24	464
1942	0.54	977	0.43	606	0.35	696	0.3	493
1943	0.57	1021	0.45	606	0.37	694	0.32	485
1944	0.58	1051	0.45	605	0.39	726	0.3	474
1945	0.62	921	0.48	589	0.42	610	0.32	453
1946	0.65	1180	0.51	743	0.46	763	0.36	563
1947	0.68	1285	0.54	715	0.46	759	0.39	540
1948	0.72	1245	0.61	753	0.52	730	0.42	512
1949	**0.75**	1208	**0.62**	703	**0.54**	665	**0.42**	466
1950	**0.8**	1218	**0.72**	793	**0.63**	657	**0.54**	486
1951	0.81	1191	0.76	816	0.62	603	0.54	422
1952	0.85	1263	0.79	926	0.71	632	0.57	450
1953	0.86	1286	0.81	949	0.71	612	0.62	463
1954	0.87	1400	0.86	1004	0.72	645	0.67	432
1955	0.88	1466	0.88	1059	0.74	679	0.71	450
1956	0.9	1393	0.9	1076	0.78	618	0.74	437
1957	0.92	1486	0.91	1084	0.81	608	0.77	424
1958	0.94	1490	0.91	1181	0.84	617	0.77	452
1959	0.94	1633	0.94	1201	0.86	671	0.84	422

3.2 The Earnings Pattern

Our goal is to comprehensively depict the earnings trajectory over time, specifically focusing on how the reform influenced both the level and the dynamics of earnings. More specifically, we aim to evaluate the impact of schooling on earnings levels, mobility, and volatility. In this section, we discuss each measure and detail the exact implementation method within our dataset.

First, to assess the effect of education on the level of earnings, we take the logarithm of a measure of permanent earnings, which, as mentioned, is constructed as the average earnings recorded between ages 35 and 45 for individuals with at least three positive earnings observations.

Next, to assess the impact of education on earnings mobility throughout one's career, we compare income earned in the initial phase of the time period under consideration with that earned in the later phase. Specifically, we calculate the average earnings of the first 2 records for individuals aged 35 and older (i.e. at age 35 and 36 if the individual was employed at both ages), and the average earnings of the last 2 records for individuals aged 45 and younger (i.e. at age 44 and 45 if the individual was employed at both ages). We then calculate the growth rate between these two periods.

Moreover, we want to assess the impact of schooling on volatility that characterizes the earnings stream during the career. In particular, we want to assess if more years of schooling provide shelter against economic shocks. Therefore, we build a measure of earnings volatility by computing annual growth rates of earnings and then calculating the standard deviation of the growth rates.

4. Identification Strategy

We use the change in the minimum school living age length as a source of exogenous variation in education. In 1963, the Italian government increased compulsory education from 5 to 8 years. The first school cohort potentially affected by the reform consisted of students who attended the last year of middle school during the school year 1963/1964. As explained in Section 2, we identify those born in 1950 as the pivotal cohort. Therefore, 1950 represents a cutoff date, such that individuals born on or after that date had to attend more years of compulsory school than those born before. The Italian middle school reform was uniformly implemented nationwide for individuals born from 1950 onwards. Therefore, there are no regional variations in the timing of the policy, which prevents us from using, for example, a difference-in-differences (DID) framework. We argue that, given the characteristics of the policy implementation, an RDD approach is the most suitable estimation strategy for assessing the impact of education on earnings dynamics, as it allows us to account smoothly for the evolution of schooling and earnings across cohorts.

The reduced-form equation for the overall effect of the change in compulsory education on earnings is shown below:

$$Y_i = \gamma_1 + \gamma_2 T_i + f(X_i - 1950) + \boldsymbol{\gamma_3 Z_i} + \varepsilon_i$$

Where Y_i is the outcome of interest, T_i is a dichotomous indicator that equals 1 for individuals born from 1950 onwards, and zero otherwise; $f(X_i - 1950)$ is a polynomial in year of birth normalized with respect to the cut-off; $\boldsymbol{Z}$ is a vector of control variables that includes, birth geographical macro-area and sex when considering the full sample. Finally, ε_i is the idiosyncratic error term. Because there is imperfect compliance with the policy, we implement a fuzzy RDD to estimate the effect of education on earnings dynamics. As we have seen, achieving perfect compliance with the policy took many years. Therefore, we need to empirically test whether the policy could be a valid instrument.

The fuzzy RDD allows us to extract the part of the variation in years of schooling explained by the change in compulsory education and use this variation to estimate the effect of education on earnings. Thus, we implement the fuzzy RDD by regressing our dependent variables on years of education and using the variable T as an instrument for years of education, as specified below:

$$Y_i = \pi_1 + \pi_2 \hat{S}_i + f(X_i - 1950) + \boldsymbol{\pi_3 Z_i} + \omega_i$$

where measures π_2 the effects of an additional year of education on a given outcome.

The first-stage equation, which estimates the effect of the reform on educational attainment, is shown below:

$$S_i = \alpha_1 + \alpha_2 T_i + f(X_i - 1950) + \boldsymbol{\alpha_3 Z_i} + \mu_i$$

where S_i are years of schooling and μ_i and ω_i are the error terms. The change in the compulsory attendance law can be considered a valid instrument only if it fulfills two key conditions: it must increase educational attainment, demonstrating its relevance, and this increase must not be linked to unobservable characteristics (such as individual ability, financial constraints, family background and preferences), ensuring exogeneity.

We apply a nonparametric local regression strategy, where the treatment effect is evaluated using data from birth cohorts close to the first cohort impacted by the reform. The estimation employs a triangular kernel[9] and allows for different slopes on either side of the discontinuity. A crucial aspect of estimating local regressions is selecting the appropriate bandwidth, which, in our case, corresponds to the number of birth cohorts included on either side of the cutoff. We start selecting individuals born between 1940 and 1959, corresponding to 10 years on either side of the cut-off year. Since we are considering a relatively wide time window, we start with a linear model and then check the robustness of our results by fitting a second-order polynomial to the running variable[10]. To test the sensitivity of our results, we first narrow the focus around the cut-off by selecting only cohorts born within 5 years on each side of the discontinuity (i.e., those born between 1945 and 1954). Then, we also select the bandwidth with the optimal bandwidth algorithm developed by Calonico et al. (2014) resulting in bandwidths lower than 5 years[11].

[9] In Appendix A we show that results are robust to using a rectangular kernel.

[10] Using higher-order polynomials can result in overfitting and introduce bias, as noted by Gelman and Imbens (2019). They suggest instead employing local regressions, fitting linear and quadratic models only.

[11] By restricting the bandwidth, we also reduce our ability to fit higher-order polynomials in the running variable. In this region, when we narrow the time window to 5 years or even fewer years, as when choosing the bandwidth using the algorithm by Calonico et al. (2014), we use a local linear model. When the bandwidth is restricted, the literature on RDD is unanimous in considering local linear models as the best for providing a reasonable approximation of the true functional form within a small neighborhood of the cut-off (Van der Klaauw, 2008), and for preventing overfitting (Cattaneo & Titiunik, 2022)

The estimate of in Eq.(2) is a locale average treatment effect (LATE) and represents the effect of an additional year of compulsory schooling for those who would have left school at 10 in the absence of the middle school reform (the compliers).

Our identification strategy relies on the premise that the timing of the legislative reform is unrelated to any concurrent economic, political, or social shifts that could directly influence earnings. Put differently, the reform impacts earnings solely through its effect on educational attainment.

5. Results

We begin our analysis by providing a graphical representation between the running variable, birth cohort, and years of schooling and our set of dependent variables: permanent earnings, mobility, and volatility (Figure 1-5). In all 5 figures, the horizontal axes represent the standardized forcing variable, which is the deviation of the birth cohort from birth cohort 1950.

Figure 1.a Discontinuity in education – Full Sample

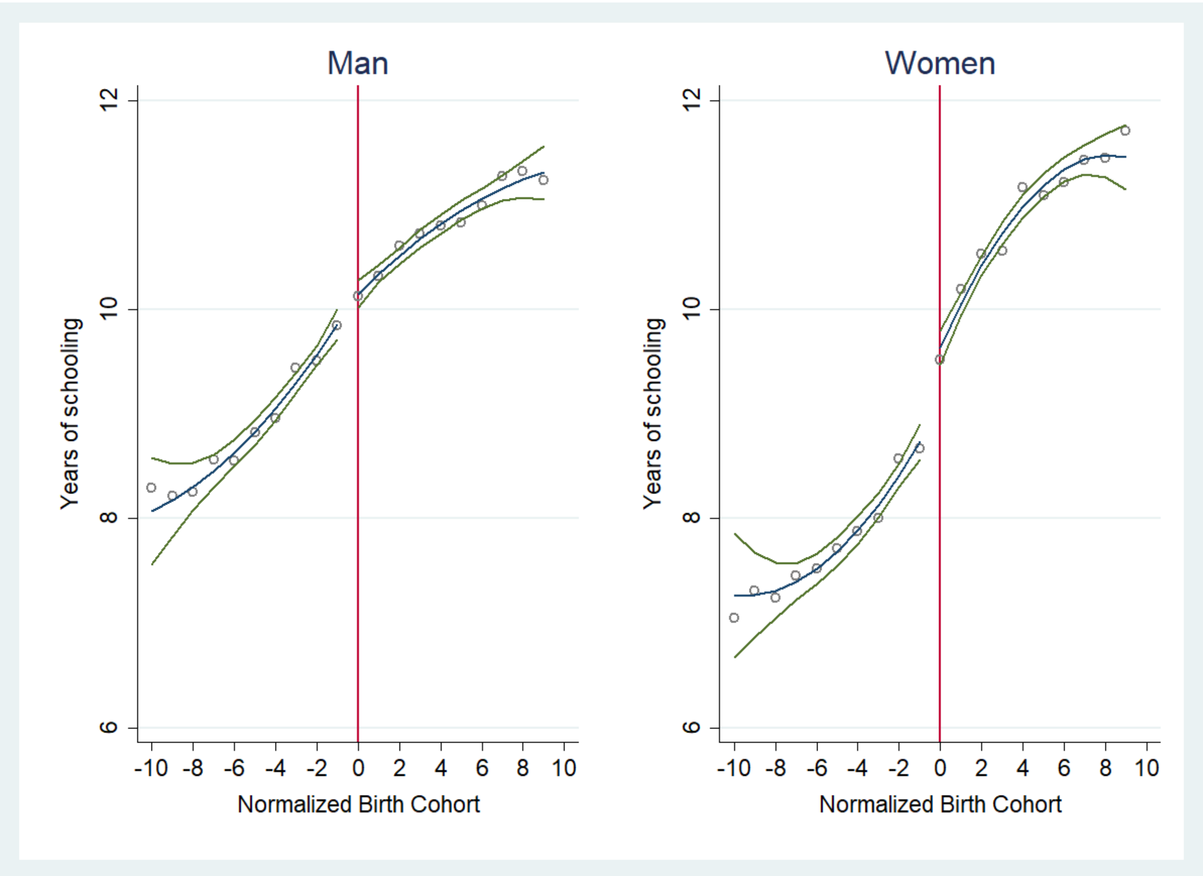


Figure 1.a presents graphical evidence of the reform's impact on years of schooling treating females and males separately. The solid line shows fitted values from a second-order polynomial. The figure shows no discontinuity in years of education around the cut-off cohort when we consider the male sample and a noticeable discontinuity when considering the female sample. In Figure 1.b, we restrict our analysis to low-achieving individuals. In this case, we also detect a small discontinuity at the cut-off for the male sample.

Figure 1.b Discontinuity in education – Low Achieving Individuals

These results are consistent with those of Brandolini and Cipollone (1992), who show that the impact of the reform appears to be stronger for females than for males. It is also interesting to analyze the presence of variation in the degree of compliance with the reform across different geographical areas (figure 2), which were characterized by varying levels of economic and social development, particularly at the time of the reform's implementation. While compliance, albeit imperfect, appears to be present across all three macro-areas for women, we can detect a discontinuity only in the central regions for men.

The very low level of compliance for men in the southern regions could be explained by the fact that these areas were more rural and had a higher prevalence of the informal sector and undeclared work. In contrast, the low level of compliance in the northern regions may be due to the higher level of industrial development, leading to a greater opportunity cost due to more abundant employment opportunities.

Figure 2. Discontinuity in Education – Differences by Geographical Macro-Areas

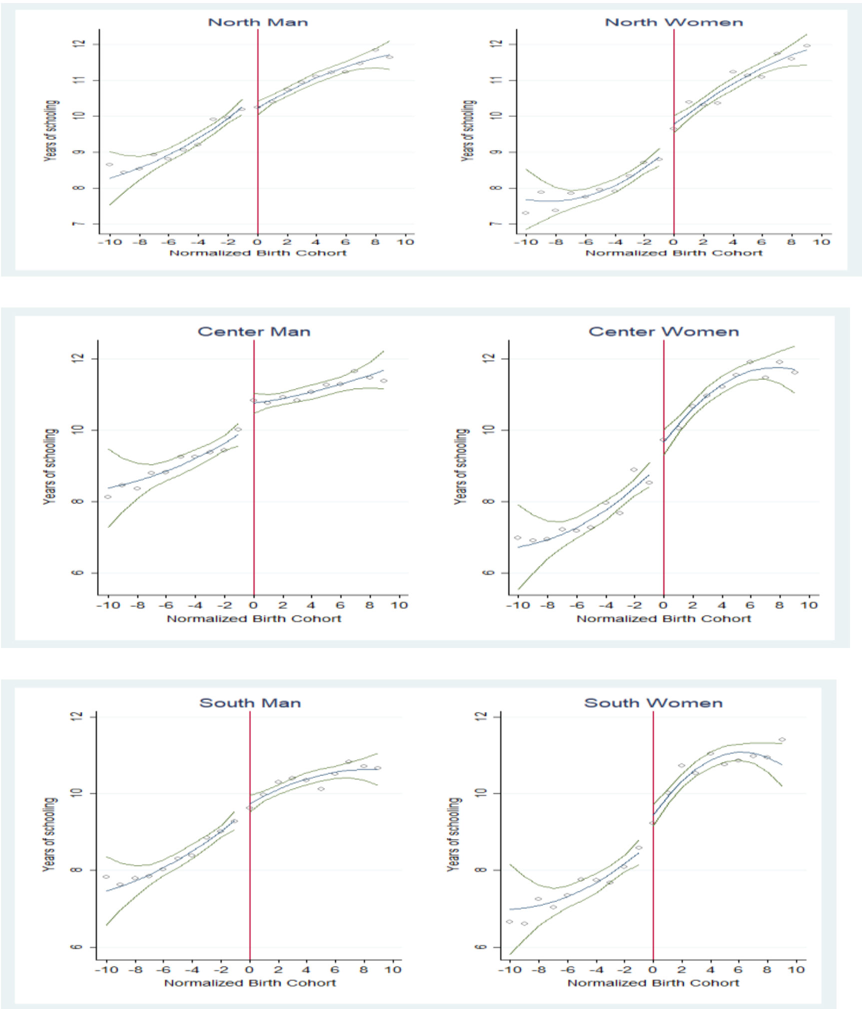


Figure 3, Figure 4, and Figure 5 provide graphical evidence of the overall effect of the reform on permanent earnings, mobility, and volatility, respectively[12]. Figure 3 shows no discontinuous change

[12] Graphical evidence for the effect of the reform on our three outcomes for the sample of low-achieving individuals is provided in the Appendix (figures 1A-3A).

in earnings for the male sample, whereas there is a clear jump for the female sample. Moreover, there is graphical evidence of a small mobility discontinuity around the 1950 cohort for both samples (Figure 4), whereas there is no jump in volatility for both men and women (Figure 5).

Figure 3. Discontinuity in permanent earnings

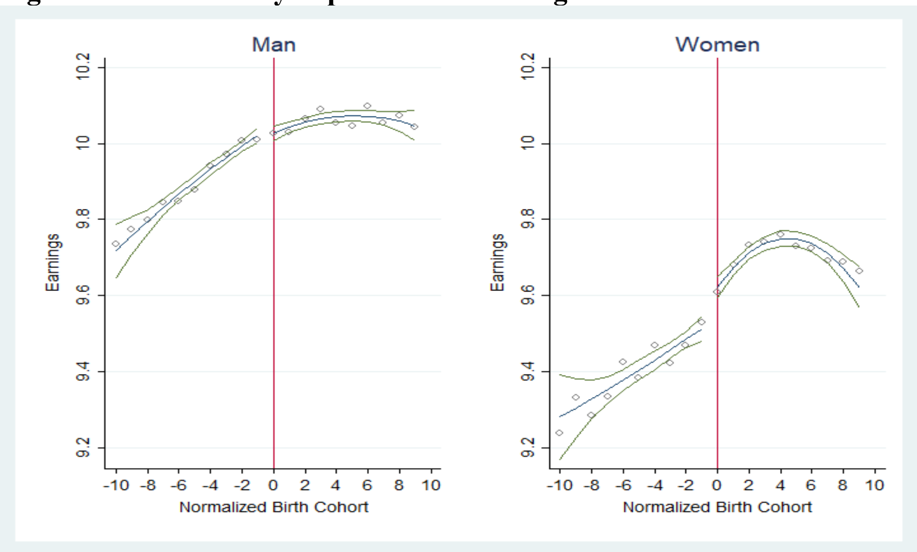


Figure 4. Discontinuity in earnings mobility

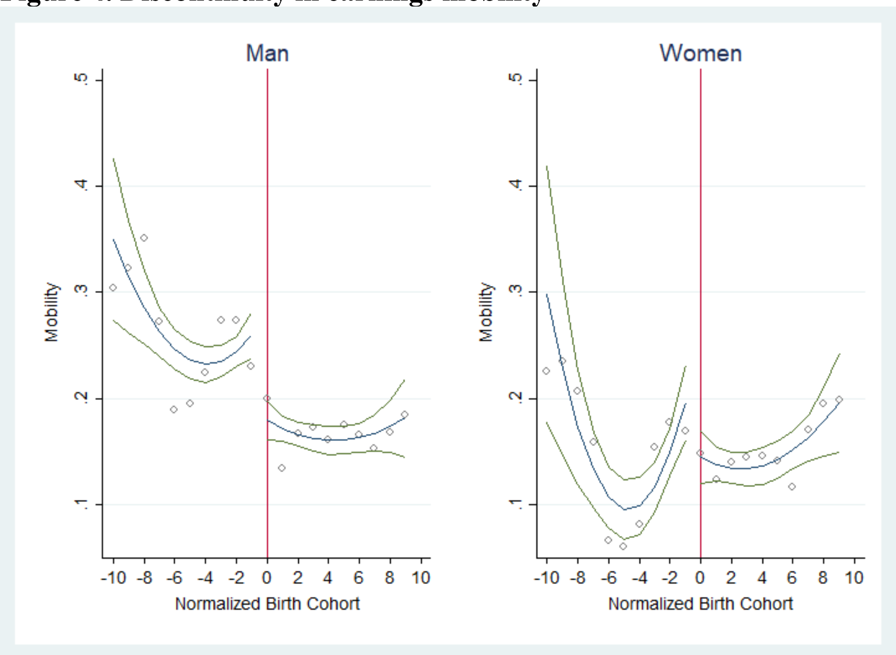
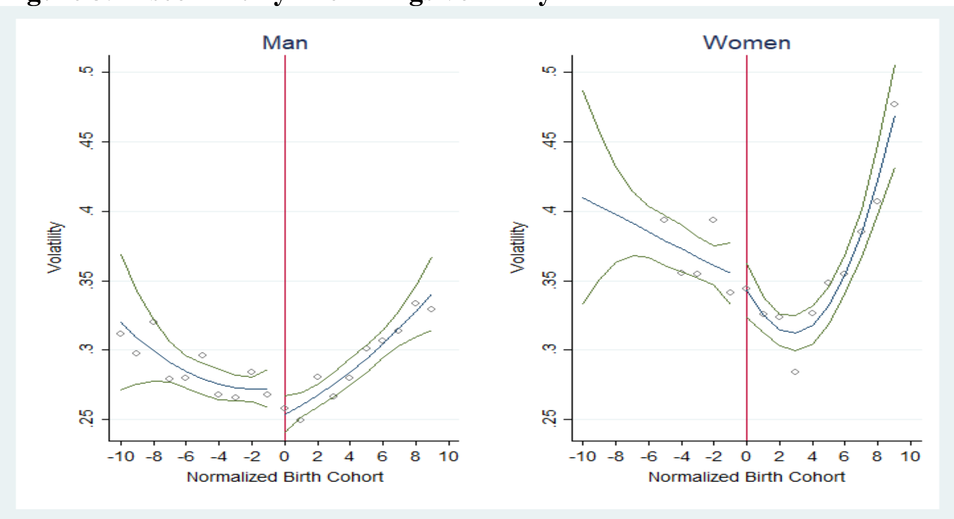


Figure 5. Discontinuity in earnings volatility



In Table 2, we present the first-stage RDD estimates for the 1940-1959 birth cohort. It can be observed that the extension of compulsory education led to an increase in the average number of years of education by 0.208 for the male sample when using the linear model, although this coefficient is not statistically significant when we estimate the quadratic model. Additionally, the F-statistics reported at the bottom of the tables are much lower than the recommended threshold of 10, indicating that for the male sample, exposure to the reform is a weak instrument. However, there is a notable difference between males and females, with the estimated coefficient for females being 1.033 (0.588 for the quadratic model). For the female sample, when we narrow the bandwidth to focus on those born between 1945 and 1954 and apply the optimal bandwidth algorithm (as shown in Table A1 in Appendix A), the results remain positive and statistically significant, with a coefficient of approximately 0.6. The F-statistics in these latter cases also exceed the threshold of 10.

Table 2. First-stage estimates, bandwidth 1940-1959

	Male		Female	
	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling
Born from 1950	0.208**	0.006	1.033***	0.558***
	(0.074)	(0.053)	(0.185)	(0.141)
N	23688	23688	15992	15992
Degree of Polynomial	First	Second	First	Second
F-Statistic	7.81	.013	31.34	15.62

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

We thus use first stage estimates to compute the effects of the increase in schooling on earnings dynamics. We first look at the effect on permanent earnings, i.e. on log of multiyear averages of annual earnings from age 35 to 45[13]. The reduced form RDD estimates, which can be interpreted as the overall effect of the reform on earnings, are reported in Table 3. The estimates are positive and statistically significant for female workers only. When we narrow the bandwidth (table A2 in the Appendix A) our estimates remain positive and significant. In contrast, the estimate for males is negative and not statistically significant.

Table 3. Reduced-Form Estimates –Earnings, 1940 – 1959 bandwidth

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Born from 1950	-0.013	-0.017	0.143***	0.087***
	(0.017)	(0.019)	(0.036)	(0.029)
N	23688	23688	15992	15992
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table 4 reports the corresponding IV estimates of the returns to education. In line with the reduced-form estimates, the return to schooling for females is positive and significant, with a coefficient of 0.138 for the linear model and 0.157 for the quadratic model. When we look at the alternative smaller bandwidths (table A3 Appendix) that coefficient ranges from 0.128 to 00.061 and is significant at the 1% confidence level. Conversely, it is confirmed that for males the estimated coefficients are not statistically significantly different from zero.

Table 4. IV estimates –Earnings, 1940 – 1959 bandwidth

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	-0.063	-2.833	0.138***	0.157**
	(0.087)	(26.645)	(0.023)	(0.074)
N	23688	23688	15992	15992
Degree of Polynomial	First	Second	First	Second

[13] In Appendix C, we replicate our models using weekly wages as the dependent variable and our results remain robust.

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Next, we evaluate the reform's impact on earnings mobility, i.e., the earnings growth rate over the career. Obviously, the fact that we observed a weak instrument problem in the first stage for the male sample affects the analysis, even when the dependent variable is the level of mobility or volatility. Therefore, we will focus our discussion on the results for the female sample only. The reduced form estimates are negative and statistically significant for the quadratic model (table 5) and for the alternative bandwidths (table A4 in the Appendix). For the female workers sample, the IV estimates are negative and statistically significant (Table 6 and Table A5 in the Appendix), indicating that additional education reduces mobility along the career.

Table 5. Reduced Form -Mobility 1940 – 1959 bandwidth

	Male		Female	
	Mobility	Mobility	Mobility	Mobility
Born from 1950	-0.067**	-0.100**	-0.032	-0.113***
	(0.027)	(0.048)	(0.023)	(0.040)
N	23688	23688	15992	15992
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table 6-IV – Mobility 1940 – 1959 bandwidth

	Male		Female	
	Mobility	Mobility	Mobility	Mobility
Years of Schooling	-0.323**	-16.637	-0.031	-0.202*
	(0.151)	(153.889)	(0.023)	(0.108)
N	23688	23688	15992	15992
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Lastly, we assess the impact of the reform on earnings volatility. The IV estimates have the same sign of the reduced form. When we instrument years of schooling with exposure to the reform, for the female sample, we obtain a negative and statistically significant coefficient only for the linear model with the

10-year time window (Table 8), suggesting that we do not have sufficient robust empirical evidence to claim that education reduces income volatility.

In Appendix A (Tables A8 to A10), to corroborate the robustness of our results, we replicate all our models using a rectangular kernel13. Our estimates remain robust to this alternative specification. Moreover, in Appendix C, we also replicate the models using the logarithm of weekly earnings as the dependent variable instead of annual earnings. The results do not change in either sign or statistical significance, confirming a positive effect of education on women's income levels.

Table 7. Reduced form – Volatility 1940 – 1959 bandwidth

	Male		Female	
	Volatility	Volatility	Volatility	Volatility
Born from 1950	-0.016**	-0.023**	-0.041	-0.009
	(0.008)	(0.010)	(0.025)	(0.025)
N	23511	23511	15774	15774
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table 8. IV – Volatility 1940 – 1959 bandwidth

	Male		Female	
	Volatility	Volatility	Volatility	Volatility
Years of Schooling	-0.078*	-1.872	-0.040*	-0.016
	(0.043)	(8.075)	(0.023)	(0.046)
N	23511	23511	15774	15774
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

6. Geographical Macro-Area Heterogeneity

In this section, we analyze whether the effect of schooling differs according to the geographical birth macro-areas[14]. To test whether there are statistically significant differences in the effect of extra education on earnings, mobility, and volatility, we run separate regressions for each macro area.

[14] Unfortunately, we have no information on which regions the workers studied in.

However, we are aware that the small sample size around the threshold may limit the statistical power to detect effects. First-stage estimates and reduced-form models for the 1945-1954 bandwidth and the optimal bandwidth are reported in Appendix B.

Table 9 presents first- stage estimates for the male sample and Table 10 for the female sample (table B1 and B2 in Appendix B present first-stage estimates for the 1945-1954 and the optimal bandwidth). We confirm the presence of a weak instrument problem for the male sample, whereas for the female sample, the F-statistics vary significantly depending on the model. In particular, when examining our smaller bandwidths in Table B1, we can use exposure to the reform as an instrument only for the North and the South. In the Center, however, the weakness of the instrument could lead to severely biased coefficient estimates.

For female workers in Northern Italy, the estimates in Table 12 and Table B4 consistently confirm a positive effect of schooling on earnings. However, for the South, the results vary across models, making it difficult to determine whether this effect exists and in which direction it operates. For the female sample, it appears that more education has reduced income mobility in the Center and the South, while no statistically significant effect has been found on income volatility.

Table 9. Heterogeneity by geographic area: first-stage estimates – male sample, 1940-1959 bandwidths

	North		Center		South	
	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling
Born from 1950	-0.140	-0.359***	0.693***	0.612***	0.351***	0.119
	(0.082)	(0.111)	(0.159)	(0.207)	(0.119)	(0.103)
N	10312	10312	5044	5044	8332	8332
Degree of Polynomial	First	Second	First	Second	First	Second
F-Statistic	2.88	10.52	19.03	8.73	8.72	1.34

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table 10. Heterogeneity by geographic area: first-stage estimates – female sample, 1940-1959 bandwidths

	North Years of Schooling	Years of Schooling	Center Years of Schooling	Years of Schooling	South Years of Schooling	Years of Schooling
Born from 1950	0.927^{***}	0.547^{**}	1.027^{***}	0.494	1.184^{***}	0.634^{**}
	(0.163)	(0.201)	(0.301)	(0.392)	(0.323)	(0.247)
N	7336	7336	3735	3735	4921	4921
Degree of Polynomial	First	Second	First	Second	First	Second
F-Statistic	32.25	7.44	11.60	1.59	13.47	6.60

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table 11. Heterogeneity by geographic area: first-stage estimates, earnings – male sample, 1940-1959 bandwidths

	North Log of Wages	Log of Wages	Center Log of Wages	Log of Wages	South Log of Wages	Log of Wages
Years of Schooling	0.266	0.101	0.037	0.078^{**}	-0.019	-0.296
	(0.229)	(0.089)	(0.029)	(0.031)	(0.050)	(0.416)
N	10312	10312	5044	5044	8332	8332
Degree of Polynomial	First	Second	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table 12. Heterogeneity by geographic area: first-stage estimates, earnings – female sample, 1940-1959 bandwidths

	North Log of Wages	Log of Wages	Center Log of Wages	Log of Wages	South Log of Wages	Log of Wages
Years of Schooling	0.111^{***}	0.131^{***}	0.207^{***}	0.342	0.124^{***}	0.074
	(0.025)	(0.049)	(0.057)	(0.287)	(0.029)	(0.090)
N	7336	7336	3735	3735	4921	4921
Degree of Polynomial	First	Second	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table 13. Heterogeneity by geographic area: first-stage estimates, mobility – male sample, 1940-1959 bandwidths

	North		Center		South	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	0.394	0.273***	-0.081*	-0.165**	-0.253**	-0.834
	(0.298)	(0.097)	(0.043)	(0.068)	(0.100)	(0.908)
N	10312	10312	5044	5044	8332	8332
Degree of Polynomial	First	Second	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table 14. Heterogeneity by geographic area: first-stage estimates, mobility – female sample, 1940-1959 bandwidths

	North		Center		South	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	-0.042*	-0.172*	-0.032	-0.265	-0.021	-0.203***
	(0.025)	(0.098)	(0.044)	(0.345)	(0.028)	(0.078)
N	7336	7336	3735	3735	4921	4921
Degree of Polynomial	First	Second	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table 15. Heterogeneity by geographic area: first-stage estimates, volatility – male sample, 1940-1959 bandwidths

	North		Center		South	
	Volatility	Volatility	Volatility	Volatility	Volatility	Volatility
Years of Schooling	0.103	0.066***	-0.067*	-0.086*	0.001	-0.022
	(0.081)	(0.019)	(0.034)	(0.050)	(0.035)	(0.139)
N	10275	10275	5015	5015	8221	8221
Degree of Polynomial	First	Second	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table 15. Heterogeneity by geographic area: first-stage estimates, volatility – female sample, 1940-1959 bandwidths

	North		Center		South	
	Volatility	Volatility	Volatility	Volatility	Volatility	Volatility
Years of Schooling	-0.000	0.006	-0.104***	-0.104	-0.037	0.030
	(0.033)	(0.044)	(0.039)	(0.101)	(0.026)	(0.063)
N	7257	7257	3689	3689	4828	4828
Degree of Polynomial	First	Second	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

7. Conclusions

In this study, we analyzed the impact of education on earnings dynamics by using as a quasi-experiment the compulsory school reform, the Riforma Scuola Media Unica, enacted in Italy in 1962, which extended compulsory schooling from 5 to 8 years. Specifically, we investigated how exposure to this policy influenced the earnings trajectory at middle age – from age 35 to age 45 –, examining the effects of education on earnings levels, career mobility, and earnings volatility.

Using the AD-SILC dataset, a novel source of information, we apply a Regression Discontinuity Design (RDD) methodology to compare individuals born just before and just after the policy change. Since compliance with the policy change was not strong, we identify the average effect of education among the set of compliers, i.e. those who would not have completed eight years of education if it hadn't been for the reform. 26

Our primary findings indicate that extending compulsory schooling duration affects female earnings but does not significantly impact male earnings. We find an effect on earnings mobility only for female workers, defined as the earnings growth rate over one's career. In particular, we find that extra education reduces earnings mobility. However, in contrast with previous literature (Delaney & Devereux, 2019), we do not find that higher levels of education are associated with reduced earnings volatility.

Our analysis has also highlighted the different levels of compliance between genders, which explains the variation in effects between men and women. Specifically, the reform appears to have had no impact on men's years of education, while it did increase women's years of schooling. Exploring compliance with the policy across geographical macro-areas, we observe that non-compliance among men was particularly pronounced in the North. This is likely due to higher opportunity costs, as the North was characterized by greater economic and industrial development, offering more job opportunities. Compliance was also low in the South, where the widespread presence of informal employment may have played a role.

Regarding the positive effect observed for the female sample, it is important to consider that, at the time, female labor force participation was extremely low. As a result, the sample of women in the labor market was highly selected, consisting of individuals with characteristics associated with positive earnings.

Unlike previous studies on Italy (Brunello and Miniaci, 1999; Brunello et al., 2000), our analysis does not find positive returns to education for men. This is likely because our empirical strategy allows us to account for the evolution of schooling and earnings across cohorts. However, our findings align with previous research on Italy that identifies a positive effect of education on women's earnings.

This divergence in results may also be due to the use of current income measures, which tie the findings to the specific period in which earnings are recorded. By leveraging administrative panel data on earnings, we constructed a measure of permanent earnings, making our estimates robust to life-cycle bias. In conclusion, our results are consistent with previous studies that find zero returns from the increase in the minimum school-leaving age for men (Pischke and von Wachter, 2008) but positive returns for women (Fischer et al., 2016; Aydemir & Kirdar, 2017).

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APPENDIX A

Figure 1A. Discontinuity in permanent earnings. Low-Achieving Individuals

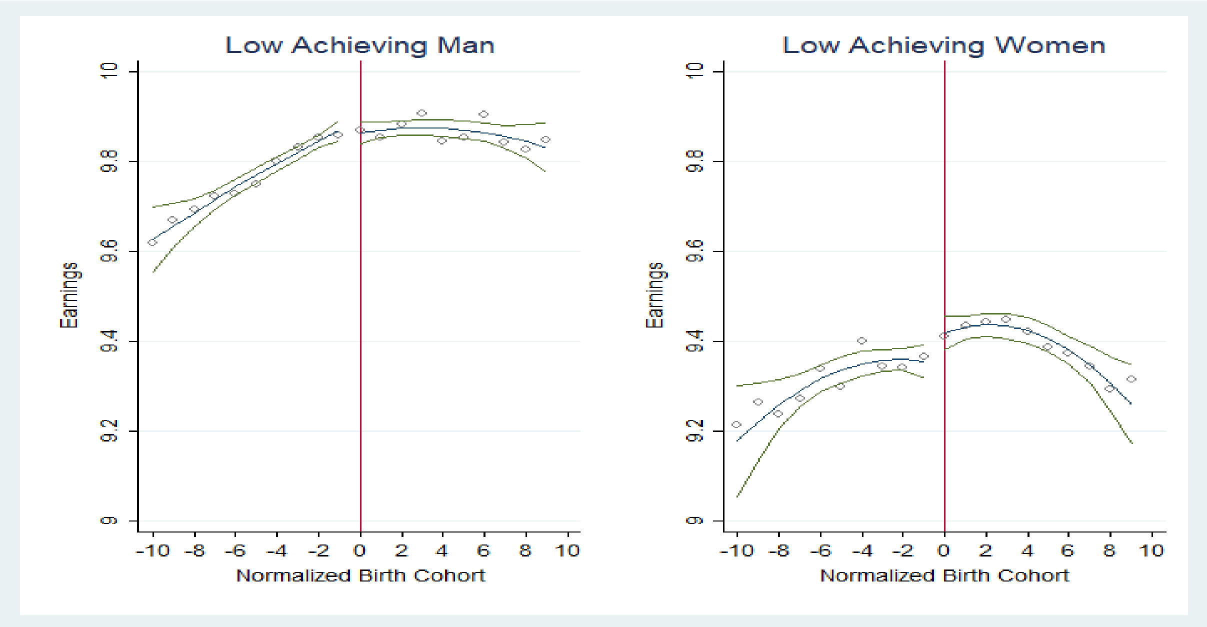

Figure 2A. Discontinuity in earnings mobility. Low-Achieving Individuals

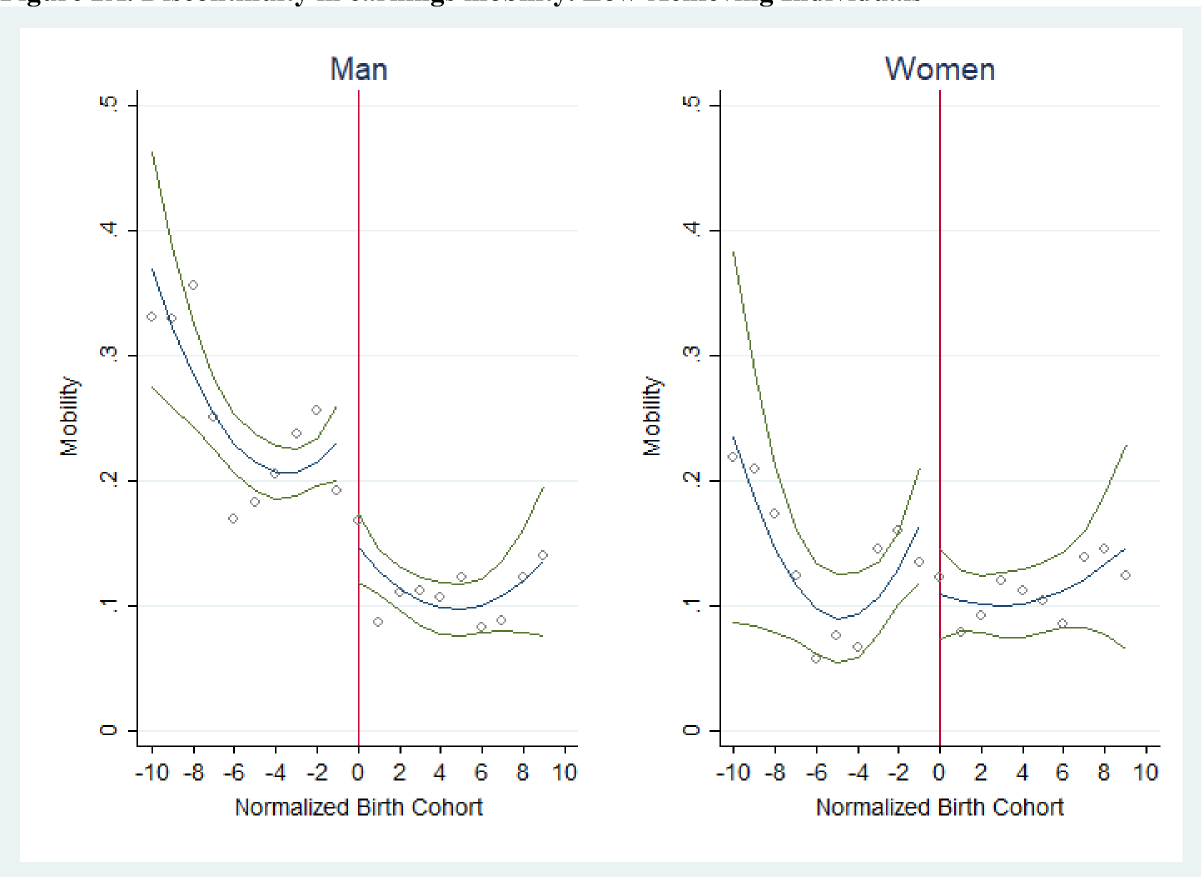

Figure 3A. Discontinuity in earnings volatility. Low-Achieving Individuals

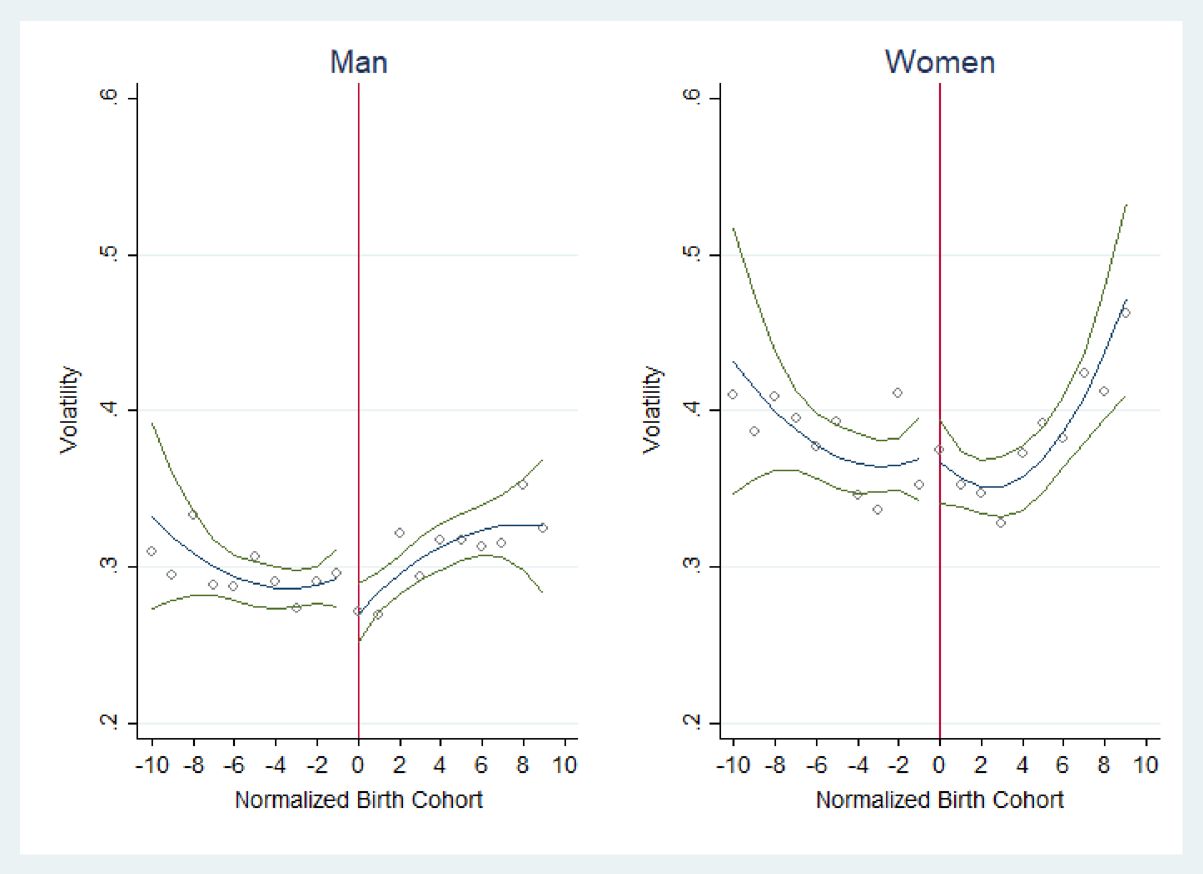

Table A1 First Stage estimates, alternative bandwidths

	Male		Female	
	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling
Born from 1950	0.060	0.034	0.632***	0.640***
	(0.053)	(0.056)	(0.138)	(0.147)
N	11276	8696	7402	5655
Bandwidth	1945-1954	Optimal	1945-1954	Optimal
F-Statistic	1.30	.38	20.96	19.06

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table A2 Reduced Form estimates, alternative bandwidths

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Born from 1950	-0.011	0.004	0.081***	0.039***
	(0.014)	(0.011)	(0.030)	(0.009)
N	11276	8696	7402	5655
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table A3 IV estimates –Earnings , alternative bandwidths

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	-0.179	0.124	0.128**	0.061***
	(0.287)	(0.530)	(0.054)	(0.015)
N	11276	8696	7402	5655
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table A4 Reduced form estimates – Mobility, alternative bandwidths

	Male		Female	
	Mobility	Mobility	Mobility	Mobility
Born from 1950	-0.055	0.007	-0.063***	-0.033***
	(0.034)	(0.013)	(0.024)	(0.011)
N	11276	6125	7402	5655
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table A5 IV estimates – Mobility, alternative bandwidths

	Male		Female	
	Mobility	Mobility	Mobility	Mobility
Years of Schooling	-0.913	-0.169	-0.100**	-0.025***
	(1.088)	(0.372)	(0.046)	(0.007)
N	11276	6125	7402	5655
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table A6 Reduced form estimates – Volatility, alternative bandwidths

	Male		Female	
	Volatility	Volatility	Volatility	Volatility
Born from 1950	-0.020**	-0.009	-0.012	0.014
	(0.009)	(0.008)	(0.025)	(0.025)
N	11191	8643	7310	5584
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table A7 IV estimates – Volatility, alternative bandwidths

	Male		Female	
	Volatility	Volatility	Volatility	Volatility
Years of Schooling	-0.331	-1.381	-0.019	0.025
	(0.279)	(11.725)	(0.041)	(0.029)
N	11191	8643	7310	5584
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

In this section, we present local fuzzy RD models using a rectangular kernel function

Table A8 IV estimates with a rectangular kernel. Outcome variable: earnings.

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	-0.003	0.337	0.127***	0.178***
	(0.041)	(0.380)	(0.020)	(0.058)
N	26452	26452	17895	17895
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table A9 IV estimates with a rectangular kernel. Outcome variable: mobility.

	Male		Female	
	Mobility	Mobility	Mobility	Mobility
Years of Schooling	-0.135*	1.381	0.004	-0.140**
	(0.077)	(1.577)	(0.027)	(0.067)
N	26452	26452	17895	17895
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table A10 IV estimates with a rectangular kernel. Outcome variable: volatility.

	Male		Female	
	Volatility	Volatility	Volatility	Volatility
Years of Schooling	-0.035	0.355	-0.052***	-0.022
	(0.022)	(0.466)	(0.019)	(0.039)
N	26257	26257	17653	17653
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

APPENDIX B

In this section, we present first-stage estimates and IV estimates for each geographic macro area for the alternative bandwidths

Table B1. Heterogeneity by geographic area: first-stage estimates – male sample, alternative bandwidths

	North		Center		South	
	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling
Born from 1950	-0.252**	-0.213***	0.595***	0.328**	0.150*	0.148***
	(0.104)	(0.019)	(0.162)	(0.113)	(0.077)	(0.035)
N	4893	2653	2349	1799	4034	3116
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal
F-Statistic	5.92	126.20	13.42	8.46	3.80	18.11

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table B2. Heterogeneity by geographic area: first-stage estimates – female sample, alternative bandwidths

	North		Center		South	
	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling
Born from 1950	0.654***	0.782***	0.732**	0.735**	0.534**	0.183***
	(0.188)	(0.190)	(0.314)	(0.317)	(0.198)	(0.041)
N	3390	2584	1716	1716	2296	1767
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal
F-Statistic	12.06	16.89	5.42	5.37	7.23	20.07

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table B3. Heterogeneity by geographic area: IV estimates, earnings – male sample, alternative bandwidths

	North		Center		South	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	0.088	-0.240***	0.046	-0.018	-0.143	-0.083**
	(0.102)	(0.059)	(0.032)	(0.062)	(0.200)	(0.032)
N	4893	2653	2349	1799	4034	3116
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table B4. Heterogeneity by geographic area: IV estimates – female sample, alternative bandwidths

	North Log of Wages	Log of Wages	Center Log of Wages	Log of Wages	South Log of Wages	Log of Wages
Years of Schooling	0.088^{***}	0.053^{***}	0.238^{**}	0.222^{**}	0.084	-0.467^{*}
	(0.027)	(0.017)	(0.119)	(0.107)	(0.101)	(0.260)
N	3390	2584	1716	1716	2296	1767
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table B5. Heterogeneity by geographic area: IV estimates, mobility – male sample, alternative bandwidths.

	North Log of Wages	Log of Wages	Center Log of Wages	Log of Wages	South Log of Wages	Log of Wages
Years of Schooling	0.231^{*}	-0.117^{*}	-0.106^{*}	-0.116	-0.295	0.072
	(0.122)	(0.068)	(0.060)	(0.087)	(0.326)	(0.128)
N	4893	2653	2349	1799	4034	3116
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table B6. Heterogeneity by geographic area: IV estimates, mobility – female sample, alternative bandwidths.

	North Log of Wages	Log of Wages	Center Log of Wages	Log of Wages	South Log of Wages	Log of Wages
Years of Schooling	-0.073^{**}	-0.046^{***}	-0.109	-0.013	-0.138^{***}	-0.138^{***}
	(0.033)	(0.013)	(0.111)	(0.040)	(0.030)	(0.031)
N	3390	2584	1716	1304	2296	2296
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table B7. Heterogeneity by geographic area: IV estimates, volatility – male sample, alternative bandwidths.

	North Volatility	Volatility	Center Volatility	Volatility	South Volatility	Volatility
Years of Schooling	0.063^{**}	0.063^{***}	-0.076	0.222^{***}	-0.074	-0.293^{***}
	(0.025)	(0.024)	(0.047)	(0.033)	(0.082)	(0.111)
N	4871	5888	2337	1236	3983	3088
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table B8. Heterogeneity by geographic area: IV estimates, volatility – female sample, alternative bandwidths.

	North		Center		South	
	Volatility	Volatility	Volatility	Volatility	Volatility	Volatility
Years of Schooling	0.012	0.012	-0.084	-0.041	0.014	0.236^{*}
	(0.037)	(0.036)	(0.060)	(0.030)	(0.068)	(0.130)
N	3359	3359	1698	1290	2253	1733
Bandwidth	1945-1954	Optimal	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

APPENDIX C

Table C1. First-Stage estimates on weekly earnings, bandwidth 1940-1959

	Male		Female	
	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling
Born from 1950	0.210^{**}	-0.000	1.035^{***}	0.550^{***}
	(0.077)	(0.054)	(0.186)	(0.140)
N	23659	23659	15933	15933
Degree of Polynomial	First	Second	First	Second
F-Statistic	7.53	.00	30.91	15.33

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table C2. First-Stage estimates on weekly earnings, alternative bandwidths

	Male		Female	
	Years of Schooling	Years of Schooling	Years of Schooling	Years of Schooling
Born from 1950	0.054	0.042	0.628^{***}	0.633^{***}
	(0.053)	(0.058)	(0.138)	(0.147)
N	11261	8686	7383	5645
Bandwidth	1945-1954	Optimal	1945-1954	Optimal
F-Statistic	1.05	.53	20.57	18.64

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table C3. Reduced-form estimates on weekly earnings, 1940-1959 bandwidth

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Born from 1950	-0.004	0.003	0.083^{***}	0.056^{***}
	(0.016)	(0.021)	(0.023)	(0.013)
N	23659	23659	15933	15933
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table C4. Reduced-form estimates on weekly earnings, alternative bandwidths

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Born from 1950	0.011	0.028**	0.054***	0.039***
	(0.015)	(0.012)	(0.015)	(0.010)
N	11261	8686	7383	5645
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses
* p<0.1, ** p<.05, *** p<0.01

Table C5. IV estimates on weekly earnings, 1940-1959 bandwidth

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	-0.018	-5.773	0.080***	0.102***
	(0.075)	(728.115)	(0.013)	(0.032)
N	23659	23659	15933	15933
Degree of Polynomial	First	Second	First	Second

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01

Table C6. IV estimates on weekly earnings, alternative bandwidths

	Male		Female	
	Log of Wages	Log of Wages	Log of Wages	Log of Wages
Years of Schooling	0.201	0.659	0.086***	0.061***
	(0.362)	(1.272)	(0.020)	(0.007)
N	11261	8686	7383	5645
Bandwidth	1945-1954	Optimal	1945-1954	Optimal

Standard errors are clustered by school cohort and are reported in parentheses.
* p<0.1, ** p<.05, *** p<0.01