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Published in:
Economica

DOI:
10.1111/ecca.12367

Published: 01/04/2021

Document Version
Publisher's PDF, also known as Version of record

Please cite the original version:
Domnisoru, C. (2021). Heterogeneity across Families in the Impact of Compulsory Schooling Laws. *Economica*, *88*(350), 399-429. https://doi.org/10.1111/ecca.12367

Economica

Economica (2021) **88**, 399–429
doi:10.1111/ecca.12367

Heterogeneity across Families in the Impact of Compulsory Schooling Laws

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Final version received 13 January 2021.

This paper aims to reconcile diverging results in the literature on the effects of compulsory schooling reforms on earnings. I point out, through a simple model of human capital accumulation, the importance of identifying parental education information to better target the set of potential compliers. Using parental background data, the empirical analysis uncovers the large and positive effects of a French school leaving age reform previously shown to have produced zero and statistically insignificant effects on the earnings of impacted cohorts. The analysis suggests that identifying parental background information is likely a crucial effort in analysing contemporary compulsory schooling policies.

Introduction

Changes in compulsory schooling laws can plausibly generate exogenous variation in educational attainment, which can then be used to trace the causal impacts of education on subsequent lifetime outcomes. This approach has been used to understand the effects of education on an extensive list of outcomes—earnings, unemployment, marital stability, mobility, mortality, criminal behaviour, and so on.[1] Governments requiring would-be dropouts to stay in school longer may incur additional staffing, school construction and enforcement costs. In part, such investments are warranted by the belief that the economic outcomes of potential dropouts will improve as a result of the additional schooling. As such, the cost–benefit analysis of raising the minimum school leaving age is informed by the monetary rate of return to the additional required years of schooling. This rate of return can be estimated using the later life earnings of individuals affected by the compulsory schooling reform.[2] Seminal papers in this literature (Harmon and Walker 1995; Angrist and Krueger 1991; Acemoglu and Angrist 2001; Oreopoulos 2006; Clay *et al*. 2012) find instrumental variable returns to compulsory schooling reforms in the USA, Canada and the UK to be close to, or even larger than, OLS returns to schooling. These results suggest that government intervention resulted in monetary gains for the group targeted by the policy, namely the potential dropouts. More recent analyses, however, point to zero returns to compulsory schooling in the USA (Stephens and Yang 2014), Germany (Pischke and von Wachter 2008) and France (Grenet 2013). National-level estimates in the literature exploring instrumental variable returns to compulsory schooling now range from 0% to 15%, for estimates within and across countries.

In part, the differences in estimates across the literature can be explained by the institutional features of educational systems in different countries, as Pischke and von Wachter (2008) and Grenet (2013) point out. Results may also be sensitive to the specification or datasets used: for example, they may not be robust to the inclusion of regional time trends (Stephens and Yang 2014), or they may become smaller when using administrative—as opposed to survey—data (Devereux and Hart 2010). In this paper, I highlight the fact that compulsory schooling laws have historically targeted low-income, low-education parents, who were deemed by policymakers to be providing too little education for their children in the absence of schooling mandates. We should therefore expect the effects of compulsory schooling laws to be heterogeneous along parental education lines. With this observation in mind, I use a French social mobility survey, *Formation et Qualification Professionnelle* (FQP, Training and Vocational Skills Survey), the French 1968 Census and information on father's occupation in the French Labour Force Survey to identify lower-education parents, who were the target group for the policy. The gains in precision help me to uncover the positive effects of a French 1967 compulsory schooling reform previously shown (Grenet 2013) to have produced close to zero and statistically insignificant effects on the earnings of impacted cohorts.

I set out to illustrate the implications of heterogeneity in the effects of compulsory schooling laws through a straightforward economic model of human capital accumulation. Compulsory schooling laws are binding for children at an age when they are minors, and likely still residing at home.[3] We should therefore expect significant parental involvement in their schooling acquisition process. As such, the model developed in this paper follows in the tradition of Becker and Tomes (1979), Loury (1981) and Eckstein and Zilcha (1994), placing the human capital investment decision process at the family level, and dependent on a variety of factors, namely the child's ability, the degree of altruism toward the child, and parents' income and education. The model makes two key predictions. First, compulsory schooling laws are more likely to be binding for parents who have low levels of education. Second, children of highly educated parents who are bound by compulsory schooling laws are likely to be a select group, disproportionately composed of low-ability children, and/or children of low-altruism parents. As such, compulsory schooling laws will increase educational attainment for a mix of relatively high-ability children from low-education families, and lower-ability children from high-education families. If we expect that returns to schooling depend on innate ability, then the overall policy effect of compulsory schooling laws on later-life earnings should be higher for high-ability children of low-education parents. The overall effect of such policies at the national level is, however, a weighted average of a stronger policy effect for children of low-education parents, and a weaker effect for low-ability children from high-education families.

I illustrate the implications of the model through an empirical analysis of a French 1967 compulsory schooling policy, which raised the minimum school leaving age from 14 to 16. I estimate the impact of the reform using a regression discontinuity framework, as the policy impacted cohorts born after 1 January 1953. As predicted by the model, I find that the positive impact of the reform accrued mainly to children of lower-education parents, who saw large and statistically significant gains in educational attainment and later-life earnings. In turn, the effects of the reform are small and statistically insignificant for children whose parents had higher educational attainment. Other analysts have found similar heterogeneity in the effects of compulsory schooling policies along parental education lines. Analysing a schooling reform in Sweden in the 1950s, Meghir and Palme (2005) find positive effects on the earnings of children whose fathers had attained only the compulsory level of schooling, but negative effects for children whose parents had more education than the minimum required level. Meghir and Palme (2005) explain the negative effect of the Swedish reform on the earnings of individuals with higher-education fathers, discussing a potential dilution in the quality of education at the top of the distribution as a result of the educational expansion. The model developed in this paper suggests an additional explanation: children of higher-education parents who were impacted by the reform may have relatively lower ability.

Turning to estimates for the entire cohort, I find statistically significant earnings gains for men using both the Labour Force Survey and the FQP survey, in contrast to Grenet (2013), who finds close to zero and statistically insignificant gains. I reconcile the conflicting findings by pointing out that the exceptional early retirement patterns of French men introduce positive selection in the pre-reform control group used in Grenet's analysis.[4] I discuss further bandwidth, dependent variable measurement and specification choices that tend to depress results in Grenet (2013).

While the statistical significance and magnitude of national-level results may be sensitive to age range and specification choices, children of lower-education parents display large earnings gains for all specifications, including the one employed by Grenet (2013). Similarly, for women, 2SLS estimates of the effect on earnings are only statistically significant for the lower parental education sample. In general, estimates for the entire cohort are noisier since they are weighted averages that include the statistically insignificant earnings gains for children of higher-education parents. Overall, these results suggest that identifying parental background is likely a crucial effort in uncovering the effects of contemporary compulsory schooling policies in developed countries, as these policies target a small subset of the population. Large positive effects for this subset may be diluted in a nationally representative sample.

The paper proceeds as follows. In Section I, I provide a brief overview of the conflicting results in the literature on the effects of compulsory schooling on earnings, highlighting the need for heterogeneity analyses based on parental background. In Section II, I set up a theoretical model of parental human capital investment and compulsory schooling. Proceeding with empirical analyses, I provide institutional background on the French compulsory schooling policy in Section III, and a description of datasets used and summary statistics in Section IV. Sections V–VII present the empirical methodology, main estimations results and conclusions.

I. For Whom are Compulsory Schooling Standards Binding?

Policymakers analysing the effects of compulsory schooling laws on the earnings of potential dropouts are faced with estimates that range from 0% to 15%, in analyses within and across countries. Analysts who found zero or statistically insignificant returns to compulsory schooling have attempted to explain the null results by analysing institutional factors. For example, Pischke and von Wachter (2008) investigate whether wage rigidity, or the existence of an apprenticeship training system, may explain the zero returns to compulsory schooling policies in West German states from 1948 to 1970. They argue that the null results may be explained by the structure of the curriculum in German schools, which emphasizes labour market relevant skills earlier than in other countries. Pischke and von Wachter suggest that the additional years of required schooling added little in terms of labour market relevant skills, therefore the extra schooling had low monetary returns. Grenet (2013) argues that the French 1967 school leaving age reform produced statistically insignificant monetary gains because it did not induce students to stay in school long enough to obtain an educational credential. Grenet contrasts the French policy to a 1972 reform in England and Wales, which resulted in a statistically significant increase in earnings. The difference between the two policies is that British students could obtain a diploma at the end of compulsory schooling, whereas French students would have had to pursue additional schooling in order to obtain educational credentials. Results in this literature also diverge across papers analysing the same policy: Devereux and Hart (2010) find returns in the 4–7% range to a 1947 compulsory schooling policy in the UK, while Oreopoulos (2006) finds returns of up to 15%. Devereux and Hart argue that the dataset that they use has superior earnings information, allowing for more precise results in a regression discontinuity estimation framework. Stephens and Yang (2014) argue that the monetary returns to early 20th century compulsory schooling laws in the USA are null and statistically insignificant when controlling for regional time trends, whereas previous results (Acemoglu and Angrist 2001) indicate returns of about 9% for the additional year(s) of required schooling.

Analysts who found zero or very small returns to compulsory schooling laws have compared their results to prior national-level estimates from the same country, or to results using representative samples of the population from other countries. But historically, the target group for such laws has been a particular subset of the population, namely families that were deemed to be providing too little education for their children. Compulsory schooling reforms have historically aimed to promote social mobility, reduce differences between provinces or between urban and rural areas, and increase the skills of the workforce. Through such legislation, governments aimed to create a modern workforce by educating children beyond what was perceived as the necessary minimum for work in the primary sector, often coming into conflict with parents, who were not convinced of the need for extra schooling. The tension between the modernizing goals of educational reforms and narrower local realities was apparent at the time of the French 1967 reform, which required all children—including those in rural areas—to remain in school until age 16. One anecdotal example of the opposition to the reforms comes from interviews of parents in rural areas at the time of the reform:[5]

> Parent: Primary school until 16, that's great, but at sixteen, what do they want them to learn, they have to go and do an apprenticeship afterwards, as well? That makes them already late!
> Reporter: But culture is important, nowadays!
> Parent: You know, culture is the fifth wheel of the cart right now.

Estimation of the effects of compulsory schooling laws may be improved using information on the parental background of students, as children of low-education parents represent the bulk of compliers, since they are in fact the target group for the policy. Several analysts have used information on the parental background of students to highlight the heterogeneous effects of educational policies. Meghir and Palme (2005) use Swedish data that include information on parental education to estimate the effects of early 1950s schooling reforms, which extended the minimum number of years of schooling in Sweden from seven to nine. When running first-stage regressions of the effect of the laws on educational attainment, they remark: 'The entire effect is due to the increase in the educational attainment of those with unskilled fathers' (Meghir and Palme 2005, p. 418). They find that the instrumental variable effect on earnings is statistically significant only for children whose fathers had low levels of schooling. Aakvik *et al.* (2010) analyse the effect of a Norwegian compulsory schooling reform implemented between 1960 and 1972, allowing the effect of the reform to differ along parental education lines. They find that the reform increased equality of opportunity, as it attenuated the effect of parental background on educational attainment.

II. A Model of Parental Human Capital Investment and Compulsory Schooling

This section provides a simple model that aims to explain how compulsory schooling laws interact with parental decision-making in regards to child schooling. The model builds on the work of Becker and Tomes (1979), Loury (1981) and Eckstein and Zilcha (1994). The human capital investment decision is assumed to take place at the family level. For simplicity, I take a family to consist of one parent and one child. The parental utility function depends on the consumption expenditures of the parent c, as well as on the human capital of children h. Consumption is the difference between the parent's income and the expenditures on education e.

The child's human capital is built according to a simple 'production function' $h(\alpha,e)$. Let $h(\alpha,e) = \alpha e$, where α is the labour productivity of children while in employment later in life, determined by innate ability, and e is the parental investment in education.[6] I assume that parents know their children's level of innate ability α, a positive parameter randomly assigned at birth. The parental problem is thus maximizing utility by choosing investments in their offspring's human capital.

The analysis is particularly transparent if the utility function over consumption and children's human capital is assumed to be constant elasticity of substitution. Parental utility is then

$$u(c,h) = (c^{\rho} + ah^{\rho})^{1/\rho},$$

where $-\infty<\rho<1$ is a parameter related to the degree of substitution between consumption c and children's human capital h, and $a>0$ is a parameter that scales the parent's level of altruism.

Parents maximize utility subject to the human capital production technology $h = \alpha e$ and the budget constraint $c+e = I$. The chosen level of investment in the child's human capital is then

$$e^{*} = \frac{(a\alpha^{\rho})^{1/(1-\rho)}}{1+(a\alpha^{\rho})^{1/(1-\rho)}} I. \tag{1}$$

The schooling investment choice e^{*} is clearly increasing in the level of altruism displayed by the parent, $\partial e^{*}/\partial a>0$, as well as in parental income, $\partial e^{*}/\partial I>0$. Higher-income parents invest more in their children's education than do poor parents. As pointed out by Loury (1981) and Eckstein and Zilcha (1994), such an educational investment allocation is inefficient. Income-constrained parents of highly able children underinvest in their offspring's education, as they cannot borrow against the future higher incomes of their children. A planner can make a socially optimal education choice by explicitly taking into account the positive externality of parental investments in human capital on the next generation's income. In Online Appendix B, I sketch a simplified model of the planner's problem and show that the socially optimal educational investment is higher than the private parental choice e^{*}. Loury (1981) and Eckstein and Zilcha (1994) elaborate on mechanisms that improve on the inefficient income-constrained parental choice. In Loury's setup, a public policy that equalizes education (funded by a non-distortionary tax) dominates the non-intervention equilibrium. In

Eckstein and Zilcha (1994), a compulsory schooling policy, financed by an income tax, can place the economy on a higher growth path.

Further insights into the parental investment choice e^* arise if the parental income function is represented as a function of the parent's human capital. Let $I = r_p \alpha_p e_p$, where r_p is the monetary returns to parental education, α_p is the labour productivity of the parent, and e_p is the parent's level of schooling. Then the level of investment in children's schooling is increasing in the level of parental education: $\partial e^*/\partial e_p > 0$.

The relation between educational investment and the ability of the offspring depends on the substitution parameter ρ in the parent's utility function:

$$\frac{\partial e^*}{\partial \alpha} > 0 \quad \text{if } 0 < \rho < 1,$$
$$\frac{\partial e^*}{\partial \alpha} = 0 \quad \text{if } \rho = 0,$$
$$\frac{\partial e^*}{\partial \alpha} < 0 \quad \text{if } \rho < 0.$$

The higher the child's level of innate ability, the lower the cost to the parent (in forgone consumption) of increasing the child's future wellbeing. If the parent's own consumption and child's wellbeing are substitutes, as is the case when ρ>0, then the parent invests more heavily as ability increases. Suppose, instead, that the parent's consumption and the child's human capital are complements in the determination of utility. The clearest case is when they are perfect complements (which happens when $\rho \rightarrow -\infty$). For a parent with such preferences, an increase in her own consumption cannot improve happiness unless it is accompanied by an increase to her child's wellbeing. Thus, conditional on income, a parent with a low-ability child will be very unhappy unless she invests heavily in education. As such, ability and educational investment are inversely related in the case ρ<0.

Panel A of Figure 1 shows the level curves for the level of parental investment in education e^* for the case ρ=0.5 and the level of parental altruism fixed at a=1. Investment in education increases in both parental education and offspring's ability. Panel B shows the effect of the introduction of a compulsory schooling law. The grey area indicates combinations of parental education and child's ability that will be bound by the compulsory schooling standard. As can be observed, the model predicts that the policy impacts low-ability children of highly educated parents, and children of all ranges of ability whose parents have the lowest levels of education. The case $\rho = -0.5$, $a = 1$ is graphically represented in panel C. If parental consumption and the child's income are complements, then the compulsory schooling law continues to affect mainly low-education parents, and offspring of all levels of ability.

The model makes two key predictions that can inform the setup of empirical analyses. First, compulsory schooling laws are most likely to be binding for low-education parents. Panels B and D of Figure 1 illustrate that this prediction holds regardless of whether we assume that parental consumption is a substitute, or a complement to investment in children's education. Second, when compulsory schooling laws are in fact binding for highly educated parents, they target children of low levels of ability, as can be observed in panel B. The model therefore suggests that compulsory schooling laws affect not just children of low ability, but also highly able students whose parents could not afford to invest in their education. This suggests that the monetary

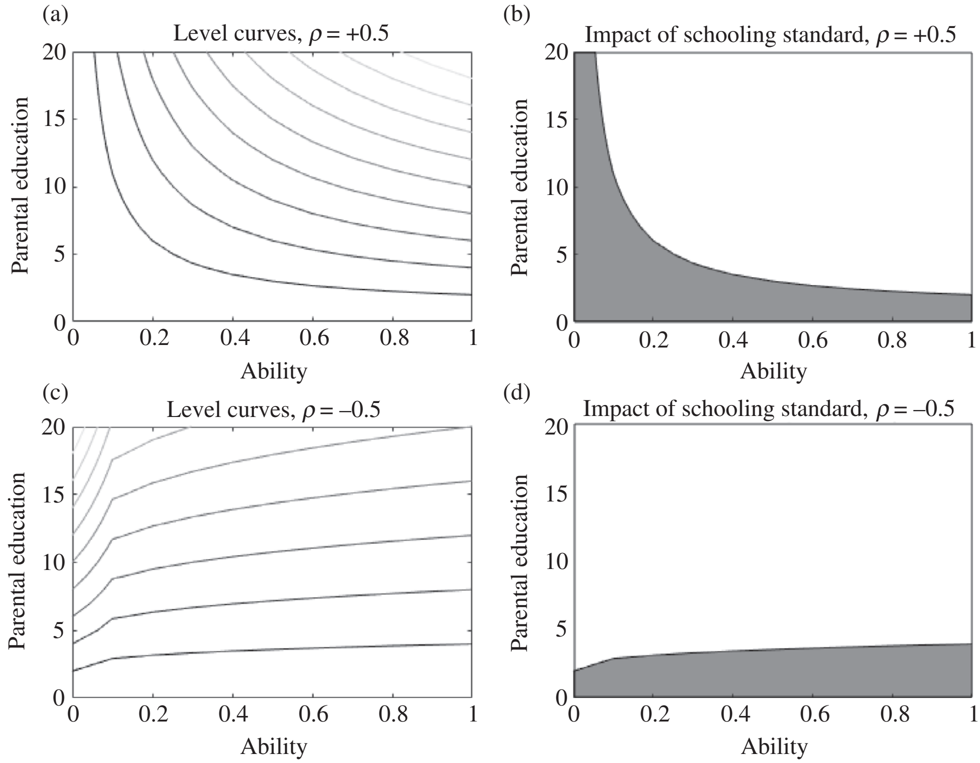


FIGURE 1. Level curves describing parental investment in children's schooling. *Notes*: The figure represents a plot of equation (1). Level curves are shown in panels A and C, and the impact of a compulsory schooling reform is shown in panels B and D, where the grey area highlights the combinations of parental education and child's ability for which the compulsory schooling law is binding.

returns to a compulsory schooling policy may differ systematically between children of low-education parents and children of highly educated parents, and that they need not be *a priori* very low.[7] One argument in opposition to compulsory schooling reforms is that potential dropouts have low academic skills, and therefore are unlikely to reap the benefits of additional compulsory schooling in the labour market. In contrast, the model suggests that schooling mandates are also likely to affect highly able students, whose educational attainment is low because of parental income constraints.

III. THE FRENCH 1967 BERTHOIN REFORM

In 1967, the minimum school leaving age in France was raised from 14 to 16. The extension of mandatory schooling had been envisioned as part of an ambitious plan to reform the French educational system, drafted by the National Council of the *Résistence*. The plan (Langevin–Wallon plan of 15 March 1944) proposed educational reforms that would allow war-torn France to compete again economically with other developed countries by increasing the compulsory schooling age and thus improving the skills and productivity of its workforce. The extension of compulsory schooling was eventually legislated in 1959, during the ministerial tenure of Jean Berthoin. The law provided that starting in 1967, all students born after 1 January 1953 would be required to remain in school until the age of 16, two years more than the requirement for students born before 1 January 1953. Prior to the reform, students who did not proceed to further academic training stayed in extended primary schools until the age of 14. Most students started schooling at age 6 and spent five years in primary school until age 10. After primary school, students were tracked into an extended primary school or into middle school, the *collège*. The extended primary school did not lead to any further schooling options, while the *collège* was the path to either vocational schools or high schools (*lycée*). By raising the school leaving age to 16, the reform induced students to either attend a vocational school or continue their education in high school. Since students who did not repeat grades would normally finish middle school or the extended primary school before age 16, they would have to enrol in either vocational schools or high schools until at least age 16 to comply with the new minimum school leaving age.

The enforcement of the policy was ensured by linking compulsory schooling compliance to family benefits. Since the law was passed in 1959 and produced effects on the first cohort eight years later, there was sufficient time for schools to adapt their logistics, particularly as the increase in the number of pupils after 1967 was gradual. Panel A of Figure 2 shows that the reform resulted in a sharp decrease in the fraction of the cohort leaving school at age 14, the previous school leaving age. As the new school leaving age was raised to 16, it also impacted students who would have stayed in school until age 15, as can be seen from the sharp decrease in the fraction of students leaving school at age 15 or younger. Panel B indicates that, as intended by policymakers, the reform increased the fraction of students staying in school until age 16 or older. In principle, the reform could induce potential dropouts to acquire more schooling than the new required minimum. Panel B shows no discernible jump in the fraction of the cohort leaving school at age 17 or older.

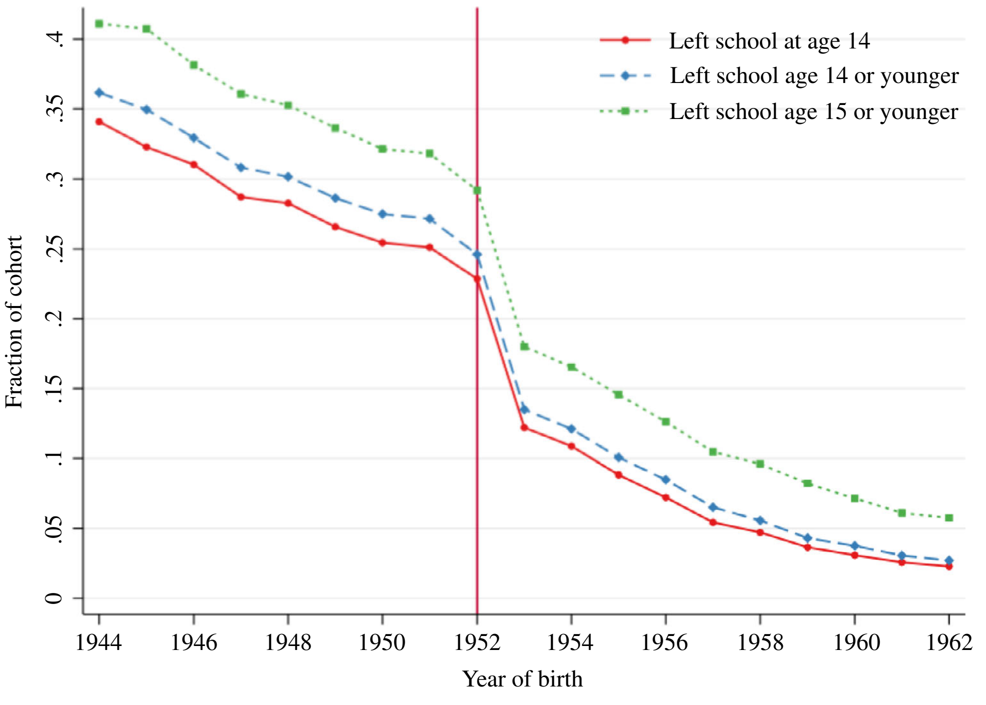
Figure 3 shows the effect of the policy on the fraction of students leaving school at age 16, by parental education category. Previewing the main empirical results, Figure 3 illustrates that the effects of the reform were stronger for children whose fathers worked in occupations with lower average educational attainment.

IV. DATA

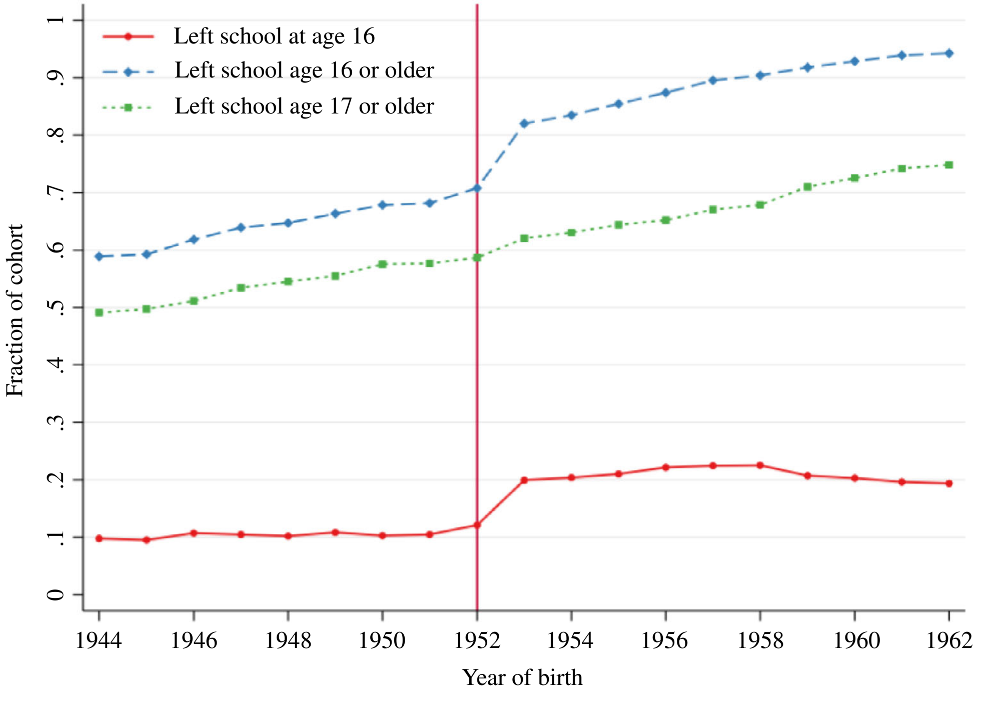
I use the 1990–2002 waves[8] of the French Labour Force Survey (LFS).[9] Table 1 presents descriptive statistics for the entire sample of men and women as well as for subsamples of interest, analysed in subsequent sections. The sample is restricted to individuals born in France between 1944 and 1962. Hourly earnings (in euros) are obtained by dividing monthly net earnings by the number of usual hours worked per week. Earnings are inflated to 2002 values. While information on parents' education is not available, the survey does provide information on father's occupation. Using the French 1968 Census (Minnesota Population Center 2018), which captures a snapshot of educational attainment in France at the time of the Berthoin reform, I identify occupations in which a large fraction of employees held no educational credentials (see Table A1 of the Online

FIGURE 2. The effect of the Berthoin reform on the school leaving age. *Notes*: FLS 1990–2002. The sample includes 172,013 men and women, born in France between 1944 and 1962. The vertical line indicates 1952, the year before the first birth cohort was affected by the policy. Each dot represents the fraction of students leaving school at the specific age category indicated, for each birth-year cohort. [Colour figure can be viewed at wileyonlinelibrary.com]

(a) Effects of the reform on the fraction of the cohort leaving school before age 16



(b) Effects of the reform on fraction of the cohort leaving school age 16 and older



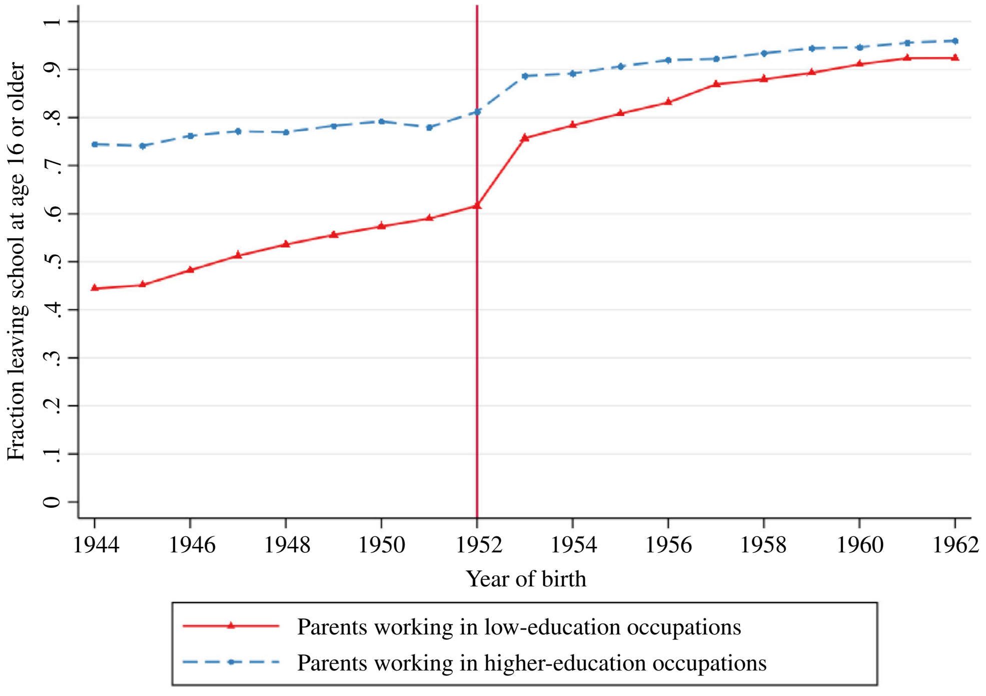


FIGURE 3. The effect of the Berthoin reform on the fraction of the cohort leaving school at age 16 or older, by father's occupational category. *Notes*: FLS 1990–2002. The sample includes 166,505 men and women, born in France between 1944 and 1962, for whom father's occupation can be identified. The lower educational credential category includes 81,717 individuals, or 49.08% of the total. The vertical line indicates 1952, the year before the first birth cohort was affected by the policy. Each dot represents the fraction of students leaving school at age 16, for every year in the sample. [Colour figure can be viewed at wileyonlinelibrary.com]

Appendix). I then identify the corresponding occupational groups in the French Labour Force Survey (the matching of occupations is described in Table A2 of the Online Appendix). I create an indicator variable that identifies whether respondents' fathers were employed in occupations in which a low fraction of employees held educational credentials. Following Grenet (2013), I group the respondents' educational certifications into six categories: no educational certification, primary schooling certificate (CEP), lower vocational qualifications (CAP, BEP), junior secondary certificate (BEPC), intermediate vocational qualifications (individuals holding CAP or BEP as well as the BEPC), and senior secondary certificate or higher (including the *Baccalauréat* and advanced professional certifications, as well as college and graduate degrees). The CEP (*Certificat d'Etudes Primaires*) was the diploma that students earned at the end of primary schooling. As the minimum school leaving age stood at 14, beyond 11, the typical ending age for primary school, the CEP was used to mark the completion of extended primary schooling only for students who did not go to middle school (the *collège*) or for adults who went back to finish primary schooling. At the end of junior secondary schooling, students could obtain the BEPC (*Brevet d'Etudes du Premier Cycle*) after passing a general academic exam. The BEPC was, however, not required in order for students to continue their studies. Two years after the end of middle school, vocationally inclined students could take the CAP (*Certificat d'Aptitude Professionelle*) exam, which was generally geared towards testing specialized professional aptitudes in

TABLE 1
SUMMARY, STATISTICS, FRENCH LABOUR FORCE SURVEY

	Men, all	Men, under 50	Women, all	Women, 38–49
Average age	42.63 (6.49)	40.73 (5.20)	42.73 (6.49)	43.35 (3.31)
Age range	28–58	28–49	28–58	38–49
Fraction employed	89.37	90.56	71.13	73.02
Average age left schooling	17.54 (2.88)	17.61 (2.82)	17.65 (2.71)	17.67 (2.71)
Log monthly earnings	7.37 (0.51)	7.35 (0.49)	6.99 (0.64)	7.00 (0.65)
Log hourly earnings	2.35 (0.46)	2.33 (0.45)	2.19 (0.49)	2.20 (0.50)
Fraction exposed to Berthoin reform	54.12	64.57	53.82	51.01
Fraction of parents in lower-education occupations	48.17	47.76	47.81	48.34
Fraction missing data on parental occupation	3.27	3.27	3.11	3.08
Fraction holding:				
No degree	17.17	17.61	17.13	16.65
Primary schooling certificate (CEP)	11.27	10.12	15.92	16.97
Low vocational qualification (CAP/BEP)	28.43	28.24	15.97	16.28
Junior secondary certificate (BEPC)	7.70	7.80	10.47	10.34
Intermediate vocational qualification (CAP/ BEP and BEPC)	9.12	9.88	10.56	10.17
Senior secondary certificate or higher	26.31	26.34	29.96	29.58
Observations	83,115	69,938	88,898	54,136

Notes
Labour Force Survey (1990–2002) samples are restricted to French citizens born in France between 1944 and 1962, whose years of completed education range between 6 and 25. Standard deviations in parentheses. The lower educational credential category includes individuals whose fathers were employed in occupations with the highest percentage of employees holding no educational qualifications, as detailed in Tables A1 and A2 of the Online Appendix.

view of early labour market entry. The BEP (*Brevet d'Etudes Professionelles*) was introduced in 1967 as a less specialized alternative to the CAP, testing skills that would be more transferable across sectors. The BEP also gave students the option to pursue a more advanced degree, the professional *Baccalauréat*. Since the BEPC was not mandatory, some students could finish their schooling with only a vocational qualification such as the CAP or the BEP. Other students passed the academic BEPC exam as well as a vocational qualification exam such as the BEP or the BEPC. The highest academic credential in secondary school was the *Baccalauréat*, for which students took an exam at the end of high school.

I supplement the LFS analysis by employing a set of surveys on social and professional mobility, the *Formation et Qualification Professionnelle* (FQP) surveys.[10] These surveys provide detailed information on the socioeconomic background of respondents. I use the 1977, 1985 and 1993 nationally representative samples.[11] Summary statistics are presented in Table A3 of the Online Appendix, for individuals born in France between 1944 and 1962. Given the timing of the FQP surveys, the sample age range is 25–49. The surveys provide information on net salaries in the year prior to the survey, as well as months worked during the previous year. The wage information for 1977 and 1985 is inflated to 1993 prices (in French francs). I create a parental background indicator using the same classification of occupations as described in Tables A1 and A2 of the Online Appendix.

V. Empirical Methodology

I use a regression discontinuity approach in estimation, as the policy introduced a cut-off in the minimum number of years of required schooling for cohorts born before and after 1 January 1953. I compare educational attainment and earnings before and after the policy change, while controlling for trends in schooling and earnings across cohorts. To control for cohort-specific random specification errors, observations are clustered at the cohort level. Regressions also allow for an intercept shift at the cut-off.

To estimate the values of the conditional expectation function at the cut-off, I employ a local linear approximation. In robustness checks, I also present local quadratic estimates. To ensure comparability with Grenet (2013), I also proceed with a global polynomial approach. As pointed out by Gelman and Imbens (2019), the use of higher-order polynomials may lead to noisy and misleading estimates if the order of the polynomial is not a good approximation of the underlying conditional expectation function. As such, I show results using a quartic polynomial approximation employed by Grenet (2013), as well as quadratic polynomials on either side of the discontinuity. The quadratic polynomial order was selected using a BIC criterion among linear, quadratic, cubic and quartic specifications.

The first-stage specification regresses the age at which individuals report leaving full-time education (S_i) on the policy instrument variable, an indicator for whether their cohort was affected by the school leaving age increase (Z_i), controlling for f^1 and f^2, functions of the year of birth cohort respectively before and after the reform, as well as for survey year fixed effects λ_t:

$$S_i = \alpha_0 + \alpha_1 Z_i + f^1(B_i - C) + f^2(B_i - C) + \lambda_t + \varepsilon_i. \tag{2}$$

The 2SLS estimates are obtained by regressing the log of wages on years of completed schooling S_i, which are instrumented using the post-reform cohort indicator variable Z_i:

$$\ln W_i = \gamma_0 + \gamma_1 \widehat{S}_i + f^1(B_i - C) + f^2(B_i - C) + \lambda_t + \varepsilon_i \tag{3}$$

In specifications aiming to replicate Grenet (2013), the relation between educational attainment and the policy instrument is modelled according to a flexible fourth-order polynomial of the birth cohort B_i, allowing for an intercept shift at the cut-off C, and including survey year fixed effects (λ_t):

$$S_i = \alpha_0 + \alpha_1 Z_i + f(B_i - C) + \lambda_t + \varepsilon_i, \tag{4}$$

$$\ln W_i = \gamma_0 + \gamma_1 \widehat{S}_i + f(B_i - C) + \lambda_t + \varepsilon_i. \tag{5}$$

The simple theoretical model presented in Section II indicates that the effects of the reform are heterogeneous along parental education lines, and estimation may be improved by identifying the target group for the policy, namely low-education parents. As such, I estimate equations (2)–(5) separately for the lower and higher parental education categories described above.

VI. Results

I begin by presenting local linear estimates of the effect of the reform on the educational attainment and earnings of men, using the LFS and FQP samples. The FQP survey presents the advantage that individuals are observed at younger ages than in the LFS. This distinction turns out to be important in interpreting differences in results between surveys, given the prevalence of early retirement among French men over the age of 50, as I show below. Then, using the global polynomial approach, I discuss the differences between my preferred specification and Grenet (2013), along several dimensions: the degree of the polynomial, bandwidth, choice of hourly versus monthly earnings, and age ranges. I also present results for women, which should be interpreted with more caution, given potential selection associated with their lower labour force participation rate. Finally, I discuss the effects of the reform on educational credential attainment, and show that they are consistent with the observed gains in earnings.

Results for men

Figure 4 provides a visual indication of the effect of the reform on the average age at which individuals left school, and their earnings later in life, observed in the French Labour Force Survey. The earnings profile is downward trending, as individuals born later are younger on average, and would have acquired less labour market experience. Cohorts exposed to the reform see an upward shift in the age–earnings profile. Both lower- and higher-education parental groups see an increase in the average school leaving age, but the increase is smaller and noisier for children of higher-education parents.[12] In turn, the upward shift in the age–earnings profile is visible only for children of lower-education parents. Figure A1 of the Online Appendix provides a similar visual representation of the effects of the reform using the FQP samples. As in Figure 4, the effects are stronger for the lower parental education group, and smaller and noisier for children of higher-education parents.

Local regression estimates Table 2 presents local linear estimates of the effect of the reform on education and monthly earnings for men. In panel A, first-stage estimates indicate an increase of 0.22–0.34 years in the average school leaving age for the full sample,[13] while 2SLS effects range from 2.8% to 3.8%, depending on the bandwidth. The first stage is larger for children of lower-education parents (panel B), effects ranging from 0.28 to 0.37 years of schooling, while effects for children of more educated parents are smaller in magnitude and display lower levels of statistical significance (panel C). In turn, 2SLS effects for children of lower-education parents range from 4.2% to 8.0%, while

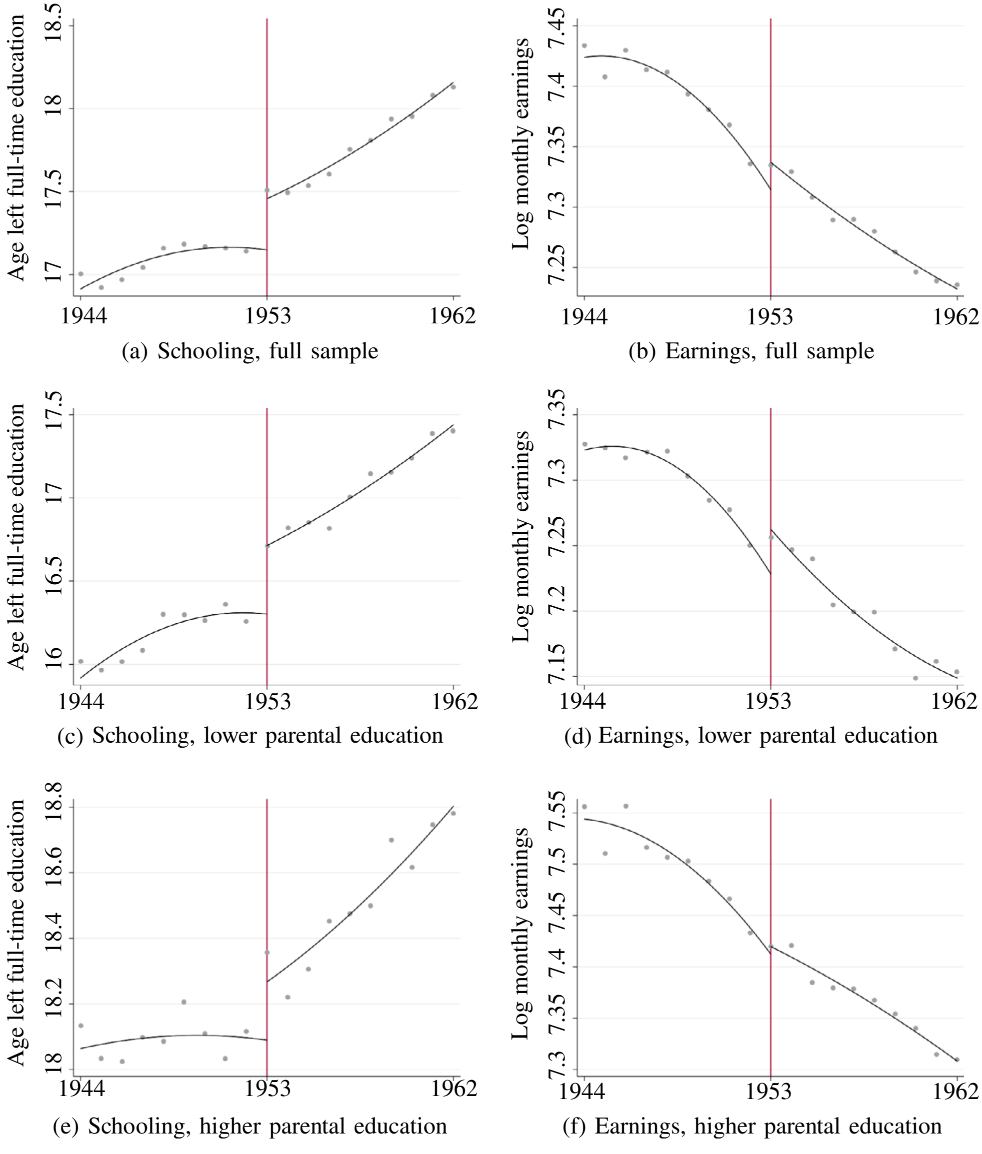


FIGURE 4. Effects of the Berthoin reform on the school leaving age and monthly earnings of men, LFS. *Notes*: FLS 1990–2002. The sample includes men born in France between 1944 and 1962. The lower parental education category includes individuals whose parents are employed in the occupations listed in Table A2 of the Online Appendix. The vertical line indicates 1953, the year when the first birth cohort was affected by the policy. Figures created using the `rdplot` command (Calonico *et al.* 2018), using a quadratic polynomial on either side of the policy discontinuity. [Colour figure can be viewed at wileyonlinelibrary.com]

effects on earnings for children of higher education parents are not statistically significant and likely biased, considering the low levels of first-stage statistical significance.

Table 2 presents a range of bandwidths, up to ten cohorts on either side of the policy discontinuity, which is the maximal bandwidth analysed by Grenet (2013). A linear regression may be a poor approximation of the conditional expectation function for

TABLE 2
EFFECTS OF THE BERTHOIN REFORM ON EDUCATIONAL ATTAINMENT AND MONTHLY EARNINGS, LFS DATA, LOCAL LINEAR REGRESSION ESTIMATES, MEN

Bandwidth	First stage	(S.E.)	Reduced form	(S.E.)	2SLS	(S.E.)
A. Full sample						
4	0.342***	(0.032)	0.009**	(0.004)	0.028**	(0.011)
5	0.320***	(0.036)	0.011***	(0.003)	0.034***	(0.011)
6	0.297***	(0.043)	0.011***	(0.003)	0.038***	(0.010)
7	0.270***	(0.050)	0.010**	(0.004)	0.038***	(0.012)
8	0.247***	(0.053)	0.008*	(0.004)	0.034**	(0.014)
9	0.230***	(0.055)	0.007	(0.004)	0.030*	(0.016)
10	0.217***	(0.055)	0.006	(0.004)	0.029*	(0.017)
B. Parents in lower-education occupations						
4	0.370***	(0.054)	0.016***	(0.004)	0.042***	(0.008)
5	0.361***	(0.047)	0.019***	(0.005)	0.053***	(0.009)
6	0.345***	(0.042)	0.024***	(0.005)	0.069***	(0.014)
7	0.318***	(0.049)	0.025***	(0.005)	0.079***	(0.018)
8	0.298***	(0.052)	0.024***	(0.006)	0.080***	(0.019)
9	0.282***	(0.053)	0.021***	(0.005)	0.076***	(0.019)
10	0.282***	(0.053)	0.019***	(0.005)	0.076***	(0.019)
C. Parents in higher-education occupations						
4	0.232***	(0.065)	−0.006	(0.004)	−0.027**	(0.013)
5	0.219**	(0.095)	−0.008	(0.008)	−0.038	(0.046)
6	0.190**	(0.094)	−0.009	(0.007)	−0.050	(0.053)
7	0.167*	(0.092)	−0.010	(0.006)	−0.064	(0.061)
8	0.140	(0.088)	−0.012*	(0.006)	−0.088	(0.079)
9	0.122	(0.085)	−0.012**	(0.005)	−0.100	(0.093)
10	0.109	(0.082)	−0.011	(0.008)	−0.105	(0.103)

Notes
Sample restricted to men aged 28–49. Sample sizes range from 19,152 to 45,874 for the full sample, from 9227 to 21,530 for lower parental education occupations, and from 9351 to 22,947 for higher parental education occupations. Standard errors (in parentheses) are clustered at the year of birth level.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

larger bandwidths. For example, in Table 2, the magnitude of the first-stage coefficients decreases as the window around the policy discontinuity widens. This raises the question of whether a different polynomial form might be more appropriate for larger bandwidths. Using a polynomial selection procedure developed by Pei *et al.* (2018), I find that a quadratic local regression results in lower asymptotic mean squared error for bandwidths larger than 7. Estimates are shown in Table A4 of the Online Appendix, and indicate the same patterns as seen in Table 2, namely that effects for children of parents in lower-education occupations are larger and statistically significant for all bandwidths, while children of higher-education parents do not see any statistically significant gains in earnings. For this latter group, reduced form and 2SLS effects on earnings are never positive, despite a positive and statistically significant first-stage effect. This result is in line with the secondary prediction of the model, namely that to the extent that they are affected by compulsory schooling reforms, children of higher-education parents may be of lower ability.

TABLE 3
EFFECTS OF THE BERTHOIN REFORM ON EDUCATIONAL ATTAINMENT AND MONTHLY EARNINGS, FQP SURVEY DATA, LOCAL LINEAR REGRESSION ESTIMATES, MEN

Bandwidth	First stage	(S.E.)	Reduced form	(S.E.)	2SLS	(S.E.)
A. Full sample						
4	0.199***	(0.052)	0.011**	(0.005)	0.055*	(0.031)
5	0.271***	(0.065)	0.017***	(0.005)	0.063***	(0.023)
6	0.302***	(0.063)	0.020***	(0.004)	0.068***	(0.017)
7	0.334***	(0.066)	0.023***	(0.004)	0.071***	(0.014)
8	0.323***	(0.070)	0.021***	(0.005)	0.067***	(0.015)
9	0.317***	(0.072)	0.022***	(0.004)	0.069***	(0.015)
10	0.302***	(0.075)	0.019***	(0.006)	0.061***	(0.019)
B. Parents in lower-education occupations						
4	0.476***	(0.127)	0.035	(0.022)	0.074*	(0.039)
5	0.434***	(0.118)	0.030	(0.020)	0.069*	(0.038)
6	0.465***	(0.120)	0.034*	(0.018)	0.073**	(0.036)
7	0.555***	(0.135)	0.035**	(0.016)	0.063**	(0.032)
8	0.561***	(0.138)	0.029	(0.017)	0.051	(0.032)
9	0.560***	(0.133)	0.028	(0.017)	0.049	(0.031)
C. Parents in higher-education occupations						
4	−0.155	(0.168)	−0.015	(0.010)	0.101	(0.060)
5	−0.017	(0.177)	0.001	(0.017)	−0.067	(1.667)
6	−0.036	(0.163)	−0.039	(0.606)	0.001	(0.016)
7	−0.090	(0.151)	0.006	(0.015)	−0.070	(0.272)
8	−0.115	(0.159)	0.011	(0.014)	−0.096	(0.244)
9	−0.130	(0.085)	0.013	(0.014)	−0.106	(0.233)
10	−0.130	(0.085)	0.013	(0.014)	−0.106	(0.233)

Notes
Sample sizes range from 5488 to 12,773 for the full sample, from 2772 to 6415 for lower parental education occupations, and from 2655 to 6240 for higher parental education occupations. Standard errors (in parentheses) are clustered at the year of birth level.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

Table 3 presents local linear regression estimates using the FQP sample, which provides earnings information at younger ages.[14] First-stage estimates for the entire sample are similar to LFS estimates, ranging from 0.2 to 0.33 years of schooling, while 2SLS effects on earnings tend to be larger, ranging from 5.5% to 7.1% for an additional year of compulsory schooling. One explanation for the larger magnitudes of the earnings gains observed in the FQP survey is that sample members are surveyed at younger ages (see Table 1, and Table A3 of the Online Appendix), when the labour market returns to additional high school education may be stronger. These gains may dilute over time, as the earnings premium associated with additional high school education becomes less important, given technological change and higher levels of college attainment in the workforce.[15]

Given the lower sample size of the FQP survey, subsample analyses in panels B and C of Table 3 generally have high standard errors, but the same patterns emerge as seen in Table 2. The first-stage effect of the reform is considerably higher and statistically significant in the lower parental education group,[16] and not statistically significant for children of higher-education parents. In turn, 2SLS effects are positive and generally statistically significant only for the lower parental education group.

TABLE 4

EFFECTS OF THE BERTHOIN REFORM ON EDUCATIONAL ATTAINMENT AND EARNINGS, GLOBAL POLYNOMIAL APPROACH, COMPARISON WITH GRENET (2013)

	(1)	(2)	(3)	(4)	(5)
A. Full sample					
First-stage estimate	0.328***	0.248***	0.270***	0.270***	0.222***
	(0.050)	(0.064)	(0.057)	(0.057)	(0.055)
2SLS estimate	0.054***	0.037*	0.027	0.018	0.004
	(0.017)	(0.019)	(0.027)	(0.023)	(0.018)
AR confidence interval	[0.018,0.088]	[−0.009,0.070]	[−0.059,0.084]	[−0.028,0.068]	[−0.036,0.044]
F-statistic	42.94	33.30	14.81	22.36	16.66
Wild bootstrap *p*-value	0.023	0.123	0.475	0.555	0.843
Observations	42,214	45,874	54,590	54,590	54,590
B. Parents in lower-education occupations					
First-stage estimate	0.390***	0.317***	0.308***	0.308***	0.258***
	(0.056)	(0.055)	(0.081)	(0.081)	(0.071)
2SLS estimate	0.093***	0.091***	0.065**	0.052**	0.048**
	(0.024)	(0.023)	(0.025)	(0.021)	(0.019)
AR confidence interval	[0.047,0.141]	[0.034,0.144]	[0.023,0.140]	[0.016,0.116]	[0.011,0.103]
F-statistic	48.35	44.60	14.32	14.32	13.24
Wild bootstrap *p*-value	0.001	0.000	0.003	0.086	0.118
Observations	19,949	21,530	26,155	26,155	26,155

TABLE 4
CONTINUED

	(1)	(2)	(3)	(4)	(5)
C. Parents in higher-education occupations					
First-stage estimate	0.219*	0.134	0.144	0.144	0.117
	(0.110)	(0.104)	(0.102)	(0.102)	(0.095)
2SLS estimate	−0.030	−0.067	−0.063	−0.056	−0.071
	(0.067)	(0.076)	(0.143)	(0.133)	(0.120)
AR confidence interval	[. . .,0.057]	[. . .,0.019]	. . .	. . .	. . .
F-statistic	3.94	3.86	1.97	1.97	1.63
Wild bootstrap *p*-value	0.676	0.316	0.668	0.734	0.569
Observations	20,986	22,947	26,639	26,639	26,639
Age range	29–49	28–49	28–58	28–58	28–58
Cohorts	1946–1960	1944–1962	1944–1962	1944–1962	1944–1962
Earnings	Monthly	Monthly	Monthly	Hourly	Hourly
Polynomial	Quadratic	Quadratic	Quadratic	Quadratic	Quartic

Notes
Regressions include survey year fixed effects, and specifications (1)–(3) include controls for part-time work status. Standard errors (in parentheses) are clustered at the year of birth level. Ellipses (. . .) indicate that confidence interval bounds could not be calculated.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

Global polynomial approach Table 4, and Table A5 of the Online Appendix, present the results from a global polynomial approach to estimation and a comparison of results with those presented by Grenet (2013). Differences between my preferred specification, shown in column (1) of both tables, and that of Grenet, in column (5), emerge along four dimensions: bandwidth, age range, use of monthly versus hourly earnings, and use of a quadratic versus a quartic specification. In general, a smaller bandwidth may be preferable to the extent that it reduces biases associated with using observations far away from the cut-off. Restricting the age range to below 50 reduces selection bias resulting from early retirement in France. In contrast to hourly earnings, monthly earnings also capture the effects of the reform on work intensity. Finally, as argued by Gelman and Imbens (2019), higher-order polynomials may lead to noisy and imprecise estimates, and therefore a quadratic specification may be preferable to a quartic.

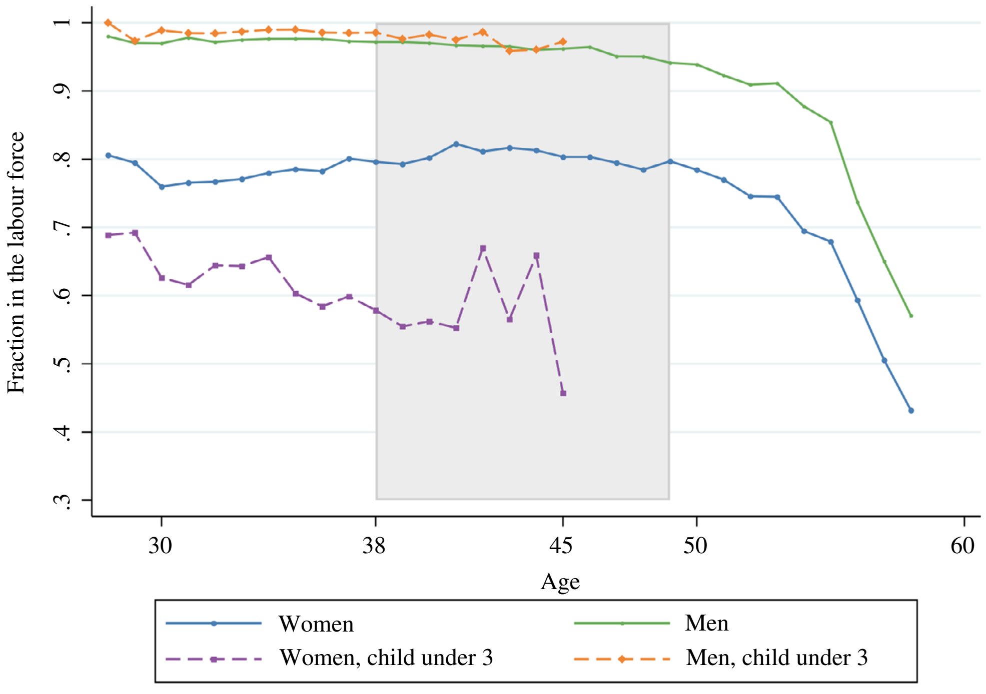
Table 4 presents first-stage and 2SLS effects of the reform on educational attainment, while Table A5 of the Online Appendix provides the accompanying reduced form and OLS estimates. Results from my preferred specification are shown in column (1), and indicate, for the full sample in panel A, a first-stage effect of 0.33 years of schooling and a corresponding 2SLS effect of the reform on earnings of 5.4%. These results are comparable to the magnitudes of local linear and local quadratic specifications. As conventional confidence intervals are incorrect in the presence of weak instruments (Staiger and Stock 1997), I proceed to report confidence intervals using an Anderson–Rubin (AR) test that accounts for the clustered nature of standard errors.[17] The test rejects the null hypothesis of weak instruments. Given the low number of clusters (17), we may be concerned that clustered standard errors are incorrect and too low. Following Cameron *et al.* (2008), I report wild bootstrap *p*-values for the null hypothesis (Roodman *et al.* 2018).

Specification (1) of Table 4 focuses on individuals born between 1946 and 1960, effectively using a 'bandwidth' of eight cohorts. As shown in Table 2, first-stage and 2SLS results tend to be smaller in magnitude when including ten cohorts before and after the policy discontinuity. While it may be useful for increasing sample sizes and the number of clusters, analysing observations far away from the policy discontinuity runs the risk of introducing biases. To illustrate, when I extend the sample to cohorts 1944–1962 in specification (2), first-stage coefficients and the *F*-test value decrease. The resulting 2SLS estimate of 3.7%, while similar to local linear and local quadratic results in Table 2 and Table A4 of the Online Appendix in terms of magnitude, is not robust to weak confidence interval tests, and the wild bootstrap *p*-value increases to 0.12.

In specification (3) of Table 4, I extend the age range to 58, to include the maximum age considered by Grenet (2013). The 2SLS estimate of 2.7% is no longer statistically significant, and the AR confidence interval widens. To evaluate the effect of the inclusion of men past the age of 50 in the sample analysed in specification (3), consider Figure 5. The labour force participation of French men declines considerably after age 50, and the decline is steeper for men with lower levels of schooling. The first-stage and 2SLS estimates are restricted to individuals earning a wage, and the older cohorts, born as early as 1944, are captured in the LFS up to age 58. As such, older workers who are part of the 'control' group before the policy discontinuity are positively selected, to the extent that individuals sort into early retirement based on low labour market earnings potential.[18] Restricting the age range to under 50 limits the extent to which a positively selected control group puts downward bias on the the effect of the reform on earnings.

Using hourly, as opposed to monthly, earnings in specification (4) of Table 4 further lowers the 2SLS estimate. As hourly earnings are obtained by dividing reported monthly

(a) Labour force participation by presence of a child under the age of 3 in the household



(b) Labour force participation rates by education

FIGURE 5. Labour force participation of French men and women. *Notes*: FLS 1990–2002. The sample includes men and women born in France between 1944 and 1962, aged 28–58. The graph shows only the labour force participation rate of individuals with children under the age of 3 until age 45, after which the number of observations is small, and averages become noisy. In panel A, the shaded area indicates the age range for which I observe individuals in both the pre-reform and post-reform samples. Each dot represents the fraction of individuals in the labour force, for every year in the sample. [Colour figure can be viewed at wileyonlinelibrary.com]

earnings by usual hours worked, they may be subject to additional measurement error in reported hours. Any effects of the reform on work intensity[19] are also captured only by the monthly earnings measure.

Finally, specification (5) of Table 4 includes a quartic, as opposed to a quadratic polynomial, and is the one employed by Grenet (2013).[20,21] As pointed out by Gelman and Imbens (2019), the use of higher-order polynomials runs the risk of leading to noisy estimates. This appears to be the case at hand, as the change in the order of the polynomial from specification (4) to specification (5) leads to a lower *F*-statistic, lower magnitude of the first-stage coefficient, and lower 2SLS effects. Specification (5), employed by Grenet (2013), seems to indicate that the reform, despite increasing the educational attainment of a sizeable fraction of the cohort, was not associated with any monetary gains for compliers. Specification (1) reaches the opposite conclusion, based on four changes: a narrower bandwidth motivated by concerns about biases arising from using observations far away from the policy discontinuity, an age range restriction that aims to alleviate biases resulting from early retirement, the use of monthly as opposed to hourly earnings, and a change in the degree of the polynomial.

Panels B and C of Table 4 conduct the same exercise of comparing global polynomial specifications to Grenet (2013), focusing on children of lower- and higher-education parents, respectively. For all specifications, first-stage and 2SLS effects for children of lower-education parents are positive, generally statistically significant, and higher than the overall sample estimates. In turn, effects for children of higher-education parents are never statistically significant. For children of lower-education parents, however, results remain positive and statistically significant through all specification changes,[22] although the magnitude of the 2SLS coefficient decreases as I approach the Grenet (2013) specification.

Table A7 of the Online Appendix presents global polynomial estimates using the FQP sample and a quadratic specification. First-stage estimates indicate gains of 0.45 years of schooling, larger for children of lower-education parents (0.59 years), and not statistically significant for children of higher-education parents. In turn, reduced-form and 2SLS effects on earnings are statistically significant for the entire sample and for children of lower-education parents, but 2SLS estimates are severely biased for the higher parental education group. In columns (4) and (5) of Table A7, I conduct an additional analysis that exploits the fact that the FQP survey provides information on father's degree attainment.[23] Results for these subsamples, shown in columns (4) and (5), are very similar to results using the grouping of parents by occupational category in columns (2) and (3).

Results for women

Local regression estimates The analysis of the effects of compulsory schooling on the later-life earnings of women presents a few complications. As shown in Figure 5, women who had a child under the age of 3 in the household were more likely to be out of the labour force. As the 'treatment' allocation rule is being born after 1 January 1953, younger women surveyed between 1990 and 2002 in the LFS are more likely to have faced the higher compulsory schooling age, or 'treatment'. Conversely, there is a larger share of older women in the control group, whose labour force participation is less likely to be influenced by the presence of small children in the household. As the analysis focuses on women who are employed, selection into employment may bias results, particularly for older women, as high rates of early retirement introduce further selection biases. To alleviate these concerns, I restrict the analysis to women aged 38–49.[24] This age range ensures support in both the treatment and control groups.

Local linear results for the LFS sample of women aged 38–49 are presented in Table 5.[25] Despite large and precisely estimated first-stage effects on educational attainment, 2SLS effects on earnings in panel A are not statistically significant for the full cohort, a result in line with Grenet (2013). Effects are, however, higher for women whose parents had lower levels of educational attainment (panel B). 2SLS effects range from

TABLE 5
EFFECTS OF THE BERTHOIN REFORM ON EDUCATIONAL ATTAINMENT AND EARNINGS, LFS DATA, LOCAL LINEAR REGRESSION ESTIMATES, WOMEN AGED 38–49

Bandwidth	First stage	(S.E.)	Reduced form	(S.E.)	2SLS	(S.E.)
A. Full sample						
4	0.288***	(0.021)	0.002	(0.003)	0.008	(0.010)
5	0.304***	(0.021)	0.003	(0.003)	0.011	(0.009)
6	0.326***	(0.026)	0.004	(0.003)	0.014	(0.010)
7	0.338***	(0.050)	0.006	(0.004)	0.018	(0.011)
8	0.332***	(0.027)	0.007	(0.004)	0.020*	(0.011)
9	0.315***	(0.027)	0.005	(0.004)	0.016	(0.013)
10	0.306***	(0.027)	0.004	(0.005)	0.012	(0.015)
B. Parents in lower-education occupations						
4	0.306***	(0.027)	0.004	(0.005)	0.012	(0.015)
5	0.351***	(0.026)	0.008***	(0.001)	0.022***	(0.002)
6	0.351***	(0.030)	0.010***	(0.001)	0.031***	(0.004)
7	0.334***	(0.028)	0.011***	(0.003)	0.034***	(0.011)
8	0.321***	(0.029)	0.010***	(0.003)	0.033***	(0.012)
9	0.307***	(0.031)	0.008**	(0.003)	0.028**	(0.012)
10	0.305***	(0.031)	0.007*	(0.004)	0.022	(0.011)
C. Parents in higher-education occupations						
4	0.194***	(0.011)	0.002	(0.004)	0.009	(0.021)
5	0.232***	(0.022)	0.002	(0.004)	0.012	(0.018)
6	0.284***	(0.040)	0.007	(0.006)	0.026	(0.018)
7	0.314***	(0.043)	0.007	(0.006)	0.023	(0.018)
8	0.311***	(0.040)	0.008	(0.006)	0.027	(0.019)
9	0.295***	(0.037)	0.008	(0.007)	0.027	(0.021)
10	0.315***	(0.027)	0.005	(0.004)	0.016	(0.013)

Notes
Sample sizes range from 15,788 to 30,049 for the full sample, from 7487 to 14,170 for lower parental education occupations, and from 7884 to 15,105 for higher parental education occupations. Standard errors (in parentheses) are clustered at the year of birth level.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

1.2% to 3.4%, and are statistically significant for most bandwidths, but lower than estimates for sons of lower-education parents, which range from 4.2% to 8% (Table 2). Despite the high first-stage estimates, 2SLS effects for women in the higher parental education category are not statistically significant. This result is in line with the model prediction that to the extent that they are affected by compulsory schooling reforms, children of higher-education parents may disproportionately be of lower ability.

One notable difference compared to results for men is that daughters in the higher parental education category see positive and statistically significant educational attainment gains for all bandwidths. The first-stage coefficients in panel C of Table 5 range from 0.19 to 0.31, only slightly smaller than effects in panel B, for lower-education parents, where estimates range from 0.3 to 0.35 years of schooling. This pattern contrasts with results for men, as sons of less-educated fathers saw notably higher educational gains. To understand the difference between first-stage estimates for men and women by parental education group, I analyse the French 1968 census, focusing on the labour force status of women who were not enrolled in school at age 15 or 16 before the reform. In both parental education categories, a relatively large fraction of young women residing in the parental household were not enrolled and not employed: 22.6% of women in the higher parental education category, and 19.52% in the lower parental education category. In contrast, young men aged 16 and not enrolled in school were less likely to be out of the labour force. Only 7.5% of 16-year-olds in higher-education households, and 4.4% of young men in the lower-education group, were out of the labour force. As such, the higher school leaving age imposed a clear trade-off between early employment and continued schooling for young men. For a significant fraction of young women, however, it was not uncommon to reside in the parental household at age 16 and not be enrolled or employed. As such, the opportunity cost for parents of all levels of education to comply with the policy was lower in the case of daughters.

Global polynomial approach Table 6 presents first-stage and 2SLS effects from a set of global polynomial specifications, and Table A6 of the Online Appendix shows the corresponding OLS and reduced-form effects. Given the lower sample size for women, results are generally more imprecise. However, in agreement with local linear estimates in Table 5, results suggest that earnings gains for women are smaller than for men, but not negligible. First-stage estimates are statistically significant for the entire sample as well as for parental education subgroups, and point to educational attainment gains of 0.37–0.39 years of schooling, slightly larger than the local linear estimates in Table 5, which ranged from 0.19 to 0.35. 2SLS effects on earnings are generally noisy and not robust to wild bootstrap procedures or weak confidence interval tests. For all specifications, 2SLS earnings estimates for women in the lower parental education category are larger than gains for women with higher-education parents. While specifications (1) and (2) focus on cohorts born 1946–1960, specification (3) expands the cohort range to a 'bandwidth' of 10, resulting in depressed first-stage effect magnitudes, lower F-statistics and lower 2SLS estimates. Inclusion of the full age range for women in the survey, 28–58, further leads to lowered 2SLS estimates. Finally, the change from a quadratic to a quartic polynomial in specification (5) leads to the Grenet (2013) conclusion that despite a statistically significant first-stage effect for women, effects on earnings were close to zero and statistically insignificant. As in the case of results for men, the very low earnings gains reported by Grenet (2013) are depressed by the choice of a quartic polynomial, selection issues arising from early retirement and the choice of a large bandwidth.

TABLE 6

EFFECTS OF THE BERTHOIN REFORM ON EDUCATIONAL ATTAINMENT AND EARNINGS OF WOMEN, COMPARISON WITH GRENET (2013)

	(1)	(2)	(3)	(4)	(5)
A. Full sample					
First-stage estimate	0.395***	0.395***	0.333***	0.305***	0.278***
	(0.064)	(0.064)	(0.065)	(0.050)	(0.036)
2SLS estimate	0.026	0.032	0.005	−0.022	−0.032
	(0.019)	(0.021)	(0.025)	(0.021)	(0.025)
AR confidence interval	[−0.017,0.058]	[−0.022,0.066]	[−0.030,0.045]	[−0.119,0.020]	[−0.058,0.033]
F-statistic	37.41	37.41	25.71	37.04	58.25
Wild bootstrap p-value	0.423	0.467	0.904	0.336	0.376
Observations	27,249	27,249	33,026	52,912	52,912
B. Parents in lower-education occupations					
First-stage estimate	0.369***	0.369***	0.293***	0.279***	0.251***
	(0.043)	(0.043)	(0.060)	(0.054)	(0.043)
2SLS estimate	0.044	0.065**	0.039	0.018	−0.007
	(0.029)	(0.025)	(0.037)	(0.039)	(0.036)
AR confidence interval	[−0.052,0.076]	[0.001,0.123]	[−0.053,0.079]	[−0.091,0.055]	[−0.090,0.040]
F-statistic	73.56	73.56	23.57	36.35	34.14
Wild bootstrap p-value	0.296	0.072	0.386	0.705	0.841

TABLE 6
CONTINUED

	(1)	(2)	(3)	(4)	(5)
Observations	12,868	12,868	15,470	24,444	24,444
C. Parents in higher-education occupations					
First-stage estimate	0.370***	0.370***	0.325***	0.275***	0.245***
	(0.091)	(0.091)	(0.090)	(0.058)	(0.042)
2SLS estimate	0.027	0.009	−0.023	−0.062	−0.067
	(0.029)	(0.038)	(0.048)	(0.051)	(0.052)
AR confidence interval	[−0.052,0.076]	[−0.102,0.032]	[−0.064,0.054]	[−0.100,0.043]	[−0.092,0.061]
F-statistic	16.46	16.46	13.01	22.20	33.51
Wild bootstrap *p*-value	0.581	0.879	0.729	0.282	0.381
Observations	13,689	13,689	16,673	27,046	27,046
Cohorts	1946–1960	1946–1960	1944–1962	1944–1962	1944–1962
Age range	38–49	38–49	38–49	28–58	28–58
Earnings	Monthly	Hourly	Hourly	Hourly	Hourly
Specification	Quadratic	Quadratic	Quadratic	Quadratic	Quartic

Notes
Standard errors (in parentheses) are clustered at the year of birth level.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

Effects on educational credential attainment

Grenet (2013) attributed the small and statistically insignificant effects on earnings to the weak effects of the reform on educational credential attainment. Students were induced to stay in school until age 16, but school leavers at 16 would not automatically receive an educational credential. Table 7 presents results from reduced-form regressions of indicators of educational credential attainment on the policy instrument variable.[26] The effects of the reform on diploma attainment are indeed modest, as argued by Grenet (2013), but they are not, however, negligible, and are in line with the observed effects on earnings. For example, results in Table 7 for samples from the LFS indicate that the reform had a positive and statistically significant effect on the attainment of the junior secondary certificate (BEPC), in parallel with negative effects on the share of individuals holding no educational credential. In Table A8 of the Online Appendix, I show results from a basic Mincerian regression, in order to gauge the effect of educational credential attainment on earnings in the LFS data. Attaining the BEPC is associated with a significant increase in earnings of 17.6% for men and 14% for women, relative to individuals holding no educational qualifications. Moreover, as indicated in Table A9 of the Online Appendix, the effects of the reform on degree attainment are concentrated among children whose fathers are included in the lower-education category. For this group, the reform raised the fraction of students holding a BEPC by 1.7 percentage points for men, and 3.3 percentage points for women. The stronger effect on degree attainment for individuals in the lower parental education category is consistent with the estimated higher later-life earnings gains for these individuals.

TABLE 7
EFFECTS OF THE BERTHOIN REFORM ON EDUCATIONAL CREDENTIAL ATTAINMENT

	FQP		LFS	
Type of diploma	Men	Women	Men	Women
No qualification	−0.021**	0.036	−0.011	−0.021***
	(0.010)	(0.020)	(0.007)	(0.007)
Primary school certificate (CEP)	−0.010	−0.004	0.002	0.009
	(0.023)	(0.021)	(0.004)	(0.006)
Lower vocational qualifications (CAP/BEP)	−0.026**	−0.060***	−0.009	−0.011*
	(0.012)	(0.014)	(0.010)	(0.006)
Junior secondary certificate (BEPC)	0.018*	0.013	0.012***	0.020***
	(0.010)	(0.014)	(0.003)	(0.002)
Intermediate vocational (CAP/ BEP and BEPC)	0.003	0.007	0.004	−0.004
	(0.008)	(0.009)	(0.004)	(0.004)
Senior secondary certificate and above	0.031**	−0.001	000	0.007
	(0.013)	(0.014)	(0.004)	(0.008)
Observations	18,331	14,375	83,102	88,881

Notes
Samples are restricted to individuals born in France between 1944 and 1962. Regressions include survey year fixed effects and a quartic polynomial in year of birth allowing for an intercept shift at the cut-off. Standard errors (in parentheses) are clustered at the year of birth level.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

VII. Conclusion

A large literature uses changes in compulsory schooling laws as instrumental variables to estimate the effects of additional schooling on later-life earnings. The results in this literature have begun to diverge, as estimates of the monetary returns to various national compulsory schooling policies now range from 0% to 15%. As governments contemplate further raising the minimum school leaving age, policymakers are faced with a competing set of results, as some analysts suggest that compulsory schooling policies may have little impact on the later-life earnings of potential dropouts, while others point to significant gains.

The French 1967 reform analysed in this paper was previously shown (Grenet 2013) to have produced statistically insignificant effects on earnings. Using two distinct datasets, I find large and statistically significant earnings gains for impacted cohorts of men, ranging from 2.8% to 7.1% increases in monthly earnings.

Analysing heterogeneity by parental education reveals that the effects of the policy are concentrated among children of lower-education parents, as predicted by the simple model of human capital accumulation laid out in this paper. First-stage estimates indicate educational attainment gains ranging from 0.28 to 0.56 for children of lower-education parents, while gains for children whose parents had higher educational attainment are smaller and generally statistically insignificant. Earnings gains for men with lower-education fathers range between 4.2% and 9.3%. For women, effects on earnings for the overall cohort are not statistically significant, but estimates by parental education group follow the same pattern as for men, showing larger earnings gains for women in the lower-education category. I reconcile the findings in this paper to those of Grenet (2013) by pointing out that patterns of early retirement in France are likely to bias estimates if the sample includes older workers still in the labour force, who are likely to be a select group. Furthermore, choices such as larger bandwidths around the policy discontinuity cut-off, and the use of higher-order polynomials in estimation, lead to depressed estimates.

These results suggest that the reform led to a narrowing of the educational and earnings gaps between children of lower- and higher-education parents. This finding is consistent with results found by LeFranc (2018), who documents a fall in intergenerational earnings elasticity for cohorts born in the 1950s, and argues that it is partially attributable to educational expansions such as the reform analysed in this paper, which affected cohorts born as early as 1953.

In the case of contemporary compulsory schooling policies in developed countries, low-education parents represent a narrow subset of the population. As such, this paper suggests that information on parental background should play a key role in analysing the effects of compulsory schooling reforms, as national-level estimates may obscure strong effects on children of low-education parents, who are ultimately the target population of compulsory schooling policies.

ACKNOWLEDGMENTS

I wish to thank Serena Canaan, David Card, Karen Clay, Lucien Grenet, Brian Kovak, Adriana Lleras-Muney, Robert Rosenman, Melanie Zaber, participants at the 2018 WEAI meeting and two anonymous referees for their helpful comments and suggestions. I am grateful to my PhD advisor, Lowell Taylor, for his guidance and detailed comments on this paper. All remaining errors are my own.

NOTES

1. The literature that takes this approach is large and influential. The following examples provide only a partial listing of the many papers in this literature: health (Lleras-Muney 2005; Mazumder 2008), criminal behaviour (Lochner and Moretti 2004; Bell *et al.* 2016, 2018), political participation (Milligan *et al.* 2004), mobility (Machin *et al.* 2012), mortality (Black *et al.* 2015; Clark and Royer 2013; Malamud *et al.* 2018), and teenage childbearing (Black *et al.* 2008).
2. In practice, estimates are often based on earnings observed at different points in individuals' careers, in (repeated) cross-sectional data. Due to non-stationarities, cross-sectional observations may be poor indicators of the actual lifetime earnings profile. Using a long panel from Norway, Bhuller *et al.* (2017) illustrate how internal rate of return calculations of the benefit of an additional year of schooling may be understated using cross-sectional data, as opposed to long panel data that capture actual lifetime earnings profiles.
3. For example, in the French 1968 Census, which represents a snapshot of the French population at the time of the introduction of the compulsory schooling policy analysed in this paper, 93.75% of men and 93.92% of women aged 14–16 were living in a household headed by a parent. Only 0.36% of men and 0.3% of women were a household head or spouse of the household head.
4. In 2003, the OECD labour force participation rate of workers aged 55–64 stood at 37% in France, considerably lower than the OECD average and other OECD member countries (e.g. UK, 55.4%, USA 59.9%, Norway 66.9%). Source: OECD, Employment rate by age group; available online at doi: 10.1787/084f32c7-en (accessed 16 January 2021).
5. Author's translation of TV archival footage. Source: Office de Radiodiffusion-Télévision Française, 'L'allongement de la scolarité jusqu'à 16 ans', available online at https://enseignants.lumni.fr/jalons/fiche-media/InaEdu01804 (accessed 23 January 2021).
6. Although e indicates expenditures on education, if parents purchase education on a per-year basis, at a per-year price that does not vary across students, then the model can be scaled so that e is the number of years of education purchased by the parent.
7. If the degree of altruism is allowed to vary across parents, then these same observations will still pertain, but the set of children affected by the reform will also include a disproportionate number of children for whom parental altruism is low.
8. I employ the 1990–2002 waves to ensure comparability with Grenet (2013). Earlier waves of the survey did not contain earnings information, while later waves are not comparable as the sampling plan and definition of variables changed.
9. *Enquête Emploi* (série complète) 1968–2002 – () [fichier électronique], INSEE [producteur], Centre Maurice Halbwachs (CMH) [diffuseur].
10. *Formation Qualification Professionnelle* standard série 1964–2003 – () [fichier électronique], INSEE [producteur], Centre Maurice Halbwachs (CMH) [diffuseur].
11. Other FQP surveys were conducted in 1964, 1970 and 2003. I omit the 2003 survey, as information on wages is grouped into broad categories, and the 1964 and 1970 surveys, as the reform started producing its effects in 1968, and I observe very few individuals who had completed their education by the time of the 1964 and 1970 surveys.
12. In particular, school leaving age for the 1949 birth cohort appears as a potential outlier for children of higher-education parents. Maurin and McNally (2008) show that individuals born in 1949 experienced lower university admission thresholds because of the May 1968 protests in France, and in turn had higher educational attainment.
13. To put the first-stage effect size in context, consider Figure 2, which indicates that the reform increased persistence in school until age 16, and reduced the fraction of children dropping out at ages 14 and 15, as intended by policymakers. However, despite effectively requiring an additional two years of schooling, the reform targeted the schooling decisions of students dropping out at age 14 or younger, who represented about 20% of the school cohort. As such, average effect sizes of 0.2–0.3 years of schooling for the entire cohort are in line with the fraction of the target population in the overall cohort.
14. Local quadratic specifications are not reported, as they are noisier and have higher asymptotic mean squared error than local linear estimates, likely given the smaller sample size in the FQP survey.
15. As a basic test of this hypothesis, I estimate simple OLS regressions of the log of monthly earnings regressed on an indicator for a school leaving age of 16, relative to earlier school leaving ages. Regressions include a quadratic age polynomial and are run separately for the FQP 1977, 1985 and 1993 samples of individuals born between 1944 and 1962, capturing three-yearly earnings snapshots. Leaving school at 16 relative to younger ages is associated with an 11.6% statistically significant earnings premium in 1975 (p=0.000), a 4.3% premium in 1985 (p=0.019), and a 6.3% premium in 1993 (p=0.085).
16. In order to ensure comparability with the LFS analysis, the parental education groups in Tables 2 and 3 use the same definitions of 'lower' and 'higher' education.
17. In practice, estimation is conducted using the Stata `weakiv` command (Finlay *et al.* 2013).
18. As indicated in panel B of Figure 5, early retirement is more prevalent among lower-education workers. Even among workers who completed fewer than 16 years of schooling, early retirees aged 50–58 have 0.08 fewer years of schooling than the employed.

19. Local linear estimates of the effect of the reform on number of hours worked by men aged 28–58, for a bandwidth of three cohorts around the policy discontinuity, indicate that cohorts exposed to the reform worked an average of 0.6 hours longer (p=0.000).
20. After communication with Julien Grenet, exact replication of results was not possible. One notable difference appears to be the coding of educational credential attainment. Table 1 of Grenet (2013) indicates that only 16% of the sample had attained senior secondary schooling or above, a category that includes the *Baccalauréat*, advanced vocational training and higher education degrees. In Table 1 in this paper, I find that this fraction was considerably higher in both the LFS and the FQP survey, around 0.28–0.3. Maurin and McNally (2008) similarly find (Table 1) that the fraction of students holding the *Baccalauréat* or a higher credential was 0.282 for cohorts born 1946–1952.
21. Inclusion of quartic age controls in Grenet (2013) leads to small changes in coefficients. In results available on request, I find that the inclusion of age controls—quadratic, cubic or quartic—has virtually no impact on my estimates in column (5) of Table 4. Quartic age controls are not statistically significant, and all age controls have extremely large variance inflation factors, given the multicollinearity between year of birth and age controls.
22. Differences between the preferred specification (1) of Table 4 and the specification employed by Grenet (2013) follow the same pattern as in panel A. Extending the bandwidth to 10 leads to slightly smaller first-stage and 2SLS effects. Increasing the age range further reduces effect sizes and leads to a drop in the *F*-test value. Estimates using hourly, as opposed to monthly, estimates in specification (4) are smaller, and further decrease when using a quartic polynomial.
23. At the time of the reform, very few fathers had attained higher educational credentials. In practice, over 70% of fathers observed in the FQP sample either had no educational credential or held only a primary school completion certificate. In order to balance subsample sizes, I group fathers with no educational certification and those holding only a primary schooling certificate working in the lower occupational group described in Online Appendix Table A2. All other fathers are grouped in the higher parental education category.
24. Other analyses of compulsory schooling laws have similarly addressed concerns raised by the effect of childbearing and childcare on female labour force participation. For example, Devereux and Hart (2010) restrict their female sample to the 35–50 age group. Stephens and Yang (2014) acknowledge that about 40% of the women in their sample of interest—ages 25–54—were not working, and they ask the reader to view results with caution due to sample selection.
25. Results using the FQP survey are very noisy and are not reported, but are available on request. The sample size for women in the FQP survey is very small, and women are observed in the FQP survey at younger ages, when their labour force participation is more likely to be affected by fertility choices. Also due to the small sample size, local quadratic estimates, not reported, are noisy and have higher asymptotic mean square error than local linear estimates.
26. In the analysis of educational credential attainment in Table 7 and in Tables A8 and A9 of the Online Appendix, I employ the global quartic specification and maximal sample sizes used by Grenet (2013), to ensure comparability. Local linear and quadratic polynomial estimates are very similar to global quartic specifications, all pointing to statistically significant gains in the attainment of the BEPC, coupled with decreases in the attainment of lower credentials, and are available from the author. Note that the FQP and LFS estimates, while similar, diverge on occasion. In general, the small FQP sample sizes result in noisier estimates, which are also more sensitive to specification, in contrast to LFS estimates, which change very little when using a local linear or quadratic polynomial specification. FQP and LFS estimates also tend to converge when using a common age support, 28–49.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

A Additional figure and tables
B Additional derivations for Section II