Does compulsory education lower mortality?

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First version: September 2006
This version: December 2007

Abstract

Recent studies showed a significant causal impact of education on health status. Their empirical strategy usually relied on changes in compulsory schooling laws. Using a French longitudinal dataset, we focus on the effect of school leaving age on mortality at later ages. The two identifying shocks are the Zay and Berthoin reforms, which respectively raised the minimum school leaving age to 14 and 16 years. We implement a non-parametric regression discontinuity design, comparing cohorts born immediately before or after the reforms, and a parametric two-stage approach using information from a larger part of our sample.
None of these approaches reveal a significant causality of education on health. Despite the fact that these reforms increased education levels, and that subsequent declines in mortality are observed, none of these declines appear to be significant. We conclude with a discussion on possible limitations of these two reforms as identifying devices.

Keywords: Health, mortality, education, causality, regression discontinuity.

Classification JEL: I12, I20

The authors would like to thank participants at INSEE internal seminar, October 2006, and iHEA World Congress on Health, July 2007, for their helpful comments. All remaining errors are ours. This document does not reflect the position of INSEE but only its author's views.

Until recently, literature on return to education was mostly focused on monetary gains stemming from a higher level of education. Researchers tried to identify to what extent higher wages earned by more educated people were due to an improvement in human capital. A major extension consists in exploring non-monetary returns to education. Besides bettering standard of living, education has an influence on many aspects of daily life. It modifies social behaviors as diverse as fecundity, spending profile, or voting participation (Milligan et al., 2004). Our purpose is to determine whether education increases one's health capital. Hence we want to test if a given individual, when provided with a higher level of education, has a better subsequent health than in the absence of extra education.

A strong empirical correlation between education and health is now well established, and the debate among economists currently lies on possible mechanisms explaining that correlation. Two types of arguments are generally put forward. In the first place, there could be a third variable influencing both level of education and health status, and thus indirectly creating a link between them. Familial background and times preferences are likely candidates for creating an artificial correlation between education and health. On the other hand, several mechanisms could create causal links between education and health. Grossman (1972) proposed two causal paths linking education to health. Firstly, educated people have a greater productivity, and thus can produce a greater amount of health from a given quantity of inputs. Moreover educated people are able to allocate available inputs to produce health more efficiently than less educated people. Apart from these two theoretical mechanisms, a channel through which causality may run is via peer effects. The more one studies, the more one is in contact with educated people. This social segregation begins during school years, and then goes on in the workplace. If indeed educated people adopt healthy behaviors more frequently, being surrounded by educated friends and colleagues could increase the likelihood of behaving healthily as well. Eventually highly educated people have greater income, and thus can afford preventive care or costly medical treatments more easily. They can also choose to live in less polluted area if they want to (see Jusot (2003) for a detailed survey on the link between income and health). Even if the relationship between education and health is not a direct one, since it goes through income, it is still a causal link: increasing one's education level will lead to higher income, and this will have a positive impact on one's health.

We investigate the existence of a causal relationship from education to health, using two successive increases in compulsory education laws in France during the XXth century. Minimal school leaving age was first increased from 13 to the age of 14 for cohorts born after 1923 (Zay reform), and was extended by two more years for individuals born after 1953 (Berthoin reform). A relevant framework to exploit this kind of changes is the regression discontinuity approach, based on the fact that these reforms created two discontinuities in school attainment. The next section is a survey of the growing literature on the link between education and health. Then we detail the econometric framework we implemented, along with our nonparametric and semiparametric estimation strategies. Section three describes the dataset, while our results are presented in section four. The last section concludes with some suggestions for further research.

1 Previous studies

When studying the impact of education on health, the first methodological problem arises when one wants to measure people's health: health is not a one-dimensional variable. Defined as "state of complete physical, mental and social wellbeing and not merely the absence of disease or infirmity" by the World Health Organization in its 1946 Constitution, health is a fairly general concept. This lack of precision results in numerous health measures used in the literature on the link between education and health[1]. Regarding morbidity, Berger and Leigh (1989) studied blood pressure, Groot and Massen van den Brink (2007) focused on illnesses prevalence, Doyle et al. (2005) used chronic conditions. Body mass index is the health proxy in Spasojevic (2003), Chou et al. (2004), and Arendt (2005). Mortality is studied by Lleras Muney (2005) and Elo and Preston (1996) on American data, van Oers (2003) on Dutch data, and Bopp and Minder (2003) on Swiss data. As for subjective measures of health, Adams (2002), Arendt (2005), Spasojevic (2003), Oreopoulos (2006), Doyle et al. (2005) used self-rated health when they evaluated the impact of education on health. The latter can also be described with mobility limitations or difficulties to carry out daily activities (Adams, 2002 ; Arkes, 2003 ; Oreopoulos, 2006 ; Berger and Leigh, 1989). Eventually Kenkel (1991), De Walque (2003, 2007), Arendt (2005) and Kenkel et al. (2006) studied the influence of education on adoption or rejection of risky behaviors. It appears that all these papers find a strong correlation between the level of education and the measure of health they selected. This result still holds when income is controlled for.

To investigate whether causality between education and health may explain that correlation, most authors test for the existence of unobserved variables, which would influence simultaneously one's level of education and one's health status. To do so, they often implement an instrumental variable strategy. These instruments can either be institutional parameters, macroeconomic variables, or instruments specific to education issues (see Table 1). It appears that the very existence of a causal impact of education on health is not clearly established. Some authors do find causality with all the health measures they use (Oreopoulos (2006) with physical limitations and self-rated health), others only with some of them (Adams (2002) with some physical limitations but not all). As for Groot and Massen van den Brink (2007), they find a significant causality for men but not for women. Eventually several studies (Arendt, 2005 ; Auld and Sidhu, 2005) don't show any significant causal effect, despite testing the causality assumption on several health measures. The hypothesis that education would have an influence only on specific health dimensions could explain this lack of consensus, but it seems not to be the case. There are indeed contradictory results for several given measures of health (e.g. Arkes (2003) and Oreopoulos (2006) on physical limitations).

The use of different instrumental variables does not seem to be the reason for such diverse results either. For instance, among the seven papers listed in Table 1 which use changes in compulsory education laws as an instrument, four find a significant causal impact, two don't, and the last one has mixed results depending on the health measure considered. A possible explanation could be that they have various health measures, and that this type of instrument can only reveal differences in health on particular health characteristics. This assumption does not hold since two papers (Oreopoulos (2006) on UK data and Arendt (2005) on a Danish sample) end on opposite conclusions with the same health measure (self-rated health) and the same instrument. Discrepancy could also stem from fundamental differences between countries, since authors of the six aforementioned studies work on datasets covering four countries (USA, Sweden, United Kingdom, and Denmark). Yet it is hard to conceive causal mechanisms appropriate only to some of these countries. On the other hand, the power of the instrument can differ among countries. Depending on whether the rise in mandatory school leaving age was indeed a constraint for a large part of the population, the instrument will be more or less correlated with education. Eventually precision of estimations generally depends on the sample size. This could partly explain why some studies bring out a significant causality between education and health, while some others don't.

[1] See Grossman (2004) for a comprehensive survey.

To the best of our knowledge, mortality is currently an exception among health measures, since no paper has challenged the causality that Glied and Lleras Muney (2003), Lleras Muney (2005) and Cipollone et al. (2006) have found. Moreover, the link between education and health has not been studied on French data yet. Hence the goal of this paper is twofold: we wish to add the French case to the existing literature, and test the existence of a causal mechanism between education and mortality on French data. We put our work in the specific setting of regression discontinuities, as we believe it is the more appropriate framework for an analysis using school attainment legislation.

Table 1: Causality between education and health

Authors	Health measure	Source, country, year, sample size	Instrument	Significant causality	Remarks
Berger, Leigh (89)	- Blood pressure - Activity limitations between 20 and 40 years old	- NES, USA, 1970, 13 500 - NLS, USA, 1966-71, 3 600	- Per-capita State expenditures on education - Per-capita disposable income - IQ test	Yes	
Kenkel (91)	- Smoking - Drinking - Lack of physical activity	NHIS, USA, 1985, 33 000	Access to anti-alcohol and anti-tobacco public campaign	Yes	
Adams (02)	- SRH - Activity limitations between 51 and 61 years old	HRS, USA, 1992, 24 000	- Change in compulsory education laws - Quarter of birth	Yes /no	Causality only for some limitations
Spasojevic (03)	-Health index - BMI, at 50 years old	Sweden	Change in compulsory education laws	Yes	One fifth of education effect on health is an income effect
De Walque (03)	Smoking	IFLS, Indonesia, 1993, 3 000	Date of school construction	Yes	
Glied, Lleras-Muney (03)	Mortality after 70 years old	- SEER, USA, 1973/93, 600 000 - CMF, USA, 1960/90, 250 000	Change in compulsory education laws	Yes	
Arkes (03)	- Activity limitations - Work limitations - Require personal care, between 47 and 56	Census, USA, 1990, 400 000	Local unemployment rate	Yes /no	Causality on work limitations and personal care, but not on activity limitations
Doyle, Harmon, Walker (05)	- SRH - Chronic health condition *for children aged 8*	HSE, UK, 1997-2002, 7 000 children	- Change in compulsory education laws - Grand-parents smoke	No	No causal effect of income either
Arendt (05)	- SRH - BMI - Smoking	WECS, Denmark, 1990/95, 3 300	Change in compulsory education laws	No	

Authors	Health measure	Source, country, year, sample size	Instrument	Significant causality	Remarks
Lleras-Muney (05)	Ten-year mortality, around 50 years old	Census, USA, 1960-80, 800 000	Change in compulsory education laws	Yes /no	Regression discontinuity estimates are not significant
Auld, Sidhu (05)	Work limitations, between 36 and 43	NLSY79, USA, 2000, 6 400	- Parents level of education - Local unemployment rate	No	
Kenkel, Lillard, Mathios (06)	- Smoking - BMI between 34 and 41	NLSY79, USA, 1998, 6 500	5 education policy variables at State level (expenditures, etc.)	Yes /no	Causality only on smoking for men, when education is measured with "having completed high school or not'
Oreopoulos (06)	- SRH - Activity limitations between 25 and 84 years old	- GHS, UK, 1983-98, 66 185 - Census, USA, 1990/2000, 1 000 000	Change in compulsory education laws	Yes	Implement an IV/RD framework
Cipollone, Radicchia, Rosolia (06)	Mortality between 25 and 35	Census, Italy, 1981, 1981/91	Exemption from military service	Yes	
Groot, Maassen van den Brink (07)	- SRH - Chronic condition	DSCPB, Netherlands, 1999, 13 500	- Father has a managerial job - Number of workers supervised - Mother works	Yes /no	No causality for women, just significant for men
De Walque (07)	Smoking	NHIS, USA, 1940-2000, 80 000	Vietnam era draft lottery	Yes /no	Causality on probability of smoking, but not on probability of quitting in all specifications

Note: SRH stands for self-rated health, BMI is the body mass index.

2 Methodology

In literature on program evaluation, the probability of receiving a treatment usually depends (at least partly) on the value of a continuous variable. The key feature of regression discontinuity designs is that for a given cutoff value of this variable, the probability of been treated is discontinuous. The idea to exploit discontinuities in treatment assignment probabilities was first introduced by Thistlethwaite and Campbell (1960). More recent works include the evaluation of the impact of class size on student's achievement (Angrist and Lavy, 1999) or the influence of financial aid amount a college offers on students' decision to enroll in this college (Van der Klaauw, 2002). Hahn et al. (2001) set up the theoretical framework for regression discontinuity designs. Using their notations, let x_i be the binary treatment indicator: x_i=1 when individual i follows the treatment and x_i=0 otherwise. There are two possible outcomes: y_{0i} and y_{1i}. For each individual i, y_{0i} is health status if individual i has not been treated, y_{1i} if he has received the treatment. For individual i, the treatment effect equals $y_{1i} - y_{0i}$. As usual in evaluation studies, either y_{0i} or y_{1i} is observed for a given individual i, but not both of them. Let y_i be the observed outcome. y_i can be expressed as

$$y_i = \alpha_i + \beta_i x_i$$

where $\alpha_i = y_{0i}$ and $\beta_i = y_{1i} - y_{0i}$. β_i is the treatment effect for individual i. It may be heterogeneous among individuals. The probability of being treated is a function of an observed variable z_i. The basic feature in a regression discontinuity design is the following:

$$(H_0): \quad \exists\ z_0 \left| \ \lim_{z \to z_0^-} E\left(x_i / z_i = z\right) \neq \lim_{z \to z_0^+} E\left(x_i / z_i = z\right)\right.$$

(H_0) means that the probability of being treated, regarded as a function of z, is not continuous in at least one value z_0 of z. In our context, the treatment consists in attending school at least a given number of years. z_i is the year of birth, and the cutoff values are the years education reforms came into force. Raising mandatory school leaving age induced a fraction of the population to go on studying until they reached the new legal limit, which increased the proportion of treated. Obviously some individuals were already following the treatment before these reforms were implemented. This means that selection into treatment is not based solely on z_i, but also on other (un)observed variables. This case is usually referred to as a fuzzy design.

In case of a constant treatment effect, the minimal identification condition is

$$(H_1): E (\alpha_i / z_i = z) \text{ as a function of z is continuous at } z_0$$

meaning that in absence of treatment, individuals born just before z_0 and those born just after would have similar average outcomes. It implies that individuals whose z_i is close to z_0 are on average essentially the same as far as health is concerned, even in dimensions which are not observed by econometricians. Under assumptions (H_0) and (H_1), the constant treatment effect β is nonparametrically identified (Hahn et al., 2001):

$$\beta = \frac{y^+ - y^-}{x^+ - x^-}$$

where: $y^+ = \lim_{z \to z_0^+} E\left(y_i / z_i = z\right) \quad y^- = \lim_{z \to z_0^-} E\left(y_i / z_i = z\right)$

$$x^+ = \lim_{z \to z_0^+} E\left(x_i / z_i = z\right) \quad x^- = \lim_{z \to z_0^-} E\left(x_i / z_i = z\right)$$

Since (H_1) guarantees that the only parameter relevant for health distinguishing individuals above and below the cutoff value is education, differences in health between these two populations can be causally attributed to the differential in education. The treatment effect is equal to the difference in health, divided by the difference in treatment participation.

If the treatment effect is allowed to be heterogeneous among the population, two further assumptions are needed to identify a causal effect (Hahn et al., 2001):

$$(H_2):\ (\beta_i , x_i (z)) \text{ are jointly independent of } z_i \text{ in a neighborhood of } z_0$$

$$(H_3):\ \exists\, \varepsilon >0 \mid \forall\, e \in [0 ; \varepsilon],\ x_i (z_0 + e) \geq x_i (z_0 - e)$$

(H_2) is more flexible than the classical assumption of no selection into treatment. Self-selection is indeed allowed; the only requirement is that this selection would have followed the same rules just before and just after the cutoff point z_0 in the absence of reform. In particular, individuals can select into treatment according to the treatment effect they expect. In our context it is unlikely that individuals decide to attend school depending on the gain they expect to their health state, because this return is hard to quantify. Yet, they may make their education choices according to their return to education in terms of wages. If the link between education and health passes through an income effect, such a behavior rules out any identification strategy based on the absence of selection into treatment. Monotonicity assumption (H_3) means that there are no defiers (see Angrist, Imbens and Rubin, 1996): individuals who follow the treatment when they are not assigned to, would follow it as well if they had been assigned to. Under (H_0), (H_1), (H_2) and (H_3) the average treatment effect for compliers can be nonparametrically estimated:

$$\lim_{e \to 0^+} E\left(\beta_i / x_i(z_0 + e) - x_i(z_0 - e) = 1\right) = \frac{y^+ - y^-}{x^+ - x^-}$$

Compliers are those who were induced to follow the treatment by the reform. In our context, compliers are those who were induced by the legislative change to extend their schooling above the new legal age. From a public policy perspective, compliers are the relevant subsample to evaluate the impact of a reform since they are the ones directly affected by the policy change.

Regression discontinuity designs provide an estimation strategy which relies solely on individuals whose z_i is very close to the discontinuity point z_0. The principle is that z is a “local” instrumental variable: the discontinuity guarantees a strong correlation between z_i and x_i; reasoning at the limit ensures that the effect of z_i on y_i passes only through its effect on x_i. But it might be the case that this restricted sample is too small to allow the identification of a significant causal effect. Unfortunately, adding to the sample individuals less close to z_0 is not sufficient to overcome that problem, because z may not be a valid instrument on the extended sample. For instance, in the context of educational returns on health, birth cohort z_i is not a valid instrument since this exclusion condition may not hold due to a “generation effect”: it is likely that scientific knowledge, better preventive care or better personal hygiene induced an improvement in average health state at the age of 50 through generations. It is however possible to use information from a larger part of the sample under additional assumptions (Van der Klaauw, 2002). In a more classical setting, the economic model can be written with a standard endogenous dummy variable model:

$$y_i = \alpha + \beta x_i + u_i$$

with β the treatment effect (assumed to be constant only for presentation purposes) and x_i the dummy variable indicating participation into treatment. Formally, the “generation effect” means that u_i and z_i are correlated in the extended sample. However z_i may still be used as an instrumental variable if it is adequately controlled for. Indeed, the solution is to introduce an estimation of $E(u_i \mid z_i)$ as covariate in the equation above. Assuming that $E(u_i \mid z_i)$ is a continuous function of z_i at $z=z_0$ enables us to identify the variation in y_i attributed to the discontinuity of x_i at z_0 and to infer the local treatment effect. Doing so, it is important to allow the functional form of $E(u_i \mid z_i)$ to be as flexible as possible, since any misspecification may lead to biased estimations. $E(u_i \mid z_i)$ is usually parameterized as a polynomial function of z_i. Thus, the model will take the following form:

$$y_i = \alpha + + \beta x_i + k(z_i) + w_i$$

where $k(z_i)$ represents $E(u_i \mid z_i)$. Note that by construction the error term w_i is not correlated with z_i and that under (H_1) it is continuous at $z=z_0$. From now on, the problem of endogeneity of x_i, due to a possible selection into treatment, can be solved by a classical two-step instrumental method using z_i as an instrument. Identification of β stems from the fact that x is the only variable which depends discontinuously on z at $z=z_0$.

If we relax the assumption of constant treatment effect, and allow β_i to vary across individuals, the previous methodology remains valid under assumptions of (H_1) , (H_2) and (H_3). In that case, $k(z_i)$ does not model $E(u_i \mid z_i)$ any more but $E(u_i \mid z_i) + \{E(\beta_i \mid z_i) - E(\beta_i \mid z_0)\}\ E(x_i \mid z_i)$ and the procedure gives an estimation of $E\ (\beta_i \mid z_0)$ (see Van der Klaauw, 2002).

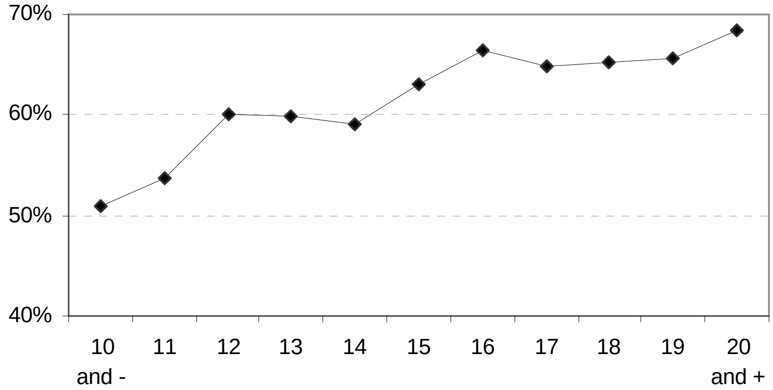
The last methodological point is about the definition of treatment. Literature on policy evaluation has widely used binary treatments, even when treatment is not homogeneous across individuals (e.g. Rosen and Willis, 1979, for return to education on wages). But using a binary treatment does not allow the estimation of the marginal effect of schooling on health. The framework depicted above can be extended to cases where treatment intensity can vary. Angrist and Imbens (1995) showed that the Wald estimator overestimates the marginal treatment effect when a treatment with variable intensity is specified with a dummy equal to one when intensity x is greater or equal than a given threshold. However there is no overestimation if the sole impact of the reform is to induce some individuals to extend their schooling by exactly one unity of treatment. Therefore we decided to use a treatment with intensity for the two reforms, and also to provide results with a binary treatment for the Zay reform which extended mandatory schooling by only one year.

3 Data

Our empirical work is based on the Echantillon Démographique Permanent (EDP thereafter). The EDP is an administrative dataset containing information on a one percent sample of the French population. It was built from the 1968 national census, by selecting all individuals born on the four first days of October. This longitudinal dataset has been growing each year with the addition of newborns of these four days of October. For all these individuals, data from the 1968, 1975, 1982, 1990 and 1999 national censuses are gathered, as well as information from the register of births, marriages and deaths until 2005. Censuses provide us with people's school leaving age. Our measure of health is mortality. We computed survival rates at a given age, taking advantage of the longitudinal structure of the EDP. Most papers relying on changes in school leaving age use cross-sectional data and have to take into account that people who studied under the pre-reform laws are older than those who studied after the reform. As a result, there might be differences in health between those two populations just because health is not measured at the same age for everyone. Authors generally add age as a covariate, in order to control for this age effect. But estimations will be biased if the age specification is not correct (in most cases if the effect of age on health is not linear). We are able to avoid this pitfall by measuring health status at the same age for everyone. For instance, as we have information on death up to 2005, survival rates are computed at the age of 80 (resp. 50) when we consider 1920 to 1925 (resp. 1950 and 1955) cohorts.

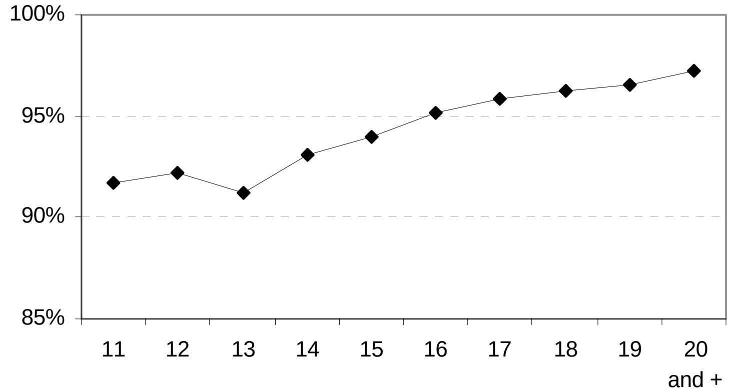
We selected individuals born at most three years before or after Zay and Berthoin reforms[2]. It represents 35 828 persons born between 1920 and 1925, and 47 337 persons born between 1950 and 1955 respectively. Figures 1 and 2 show the correlation between school leaving age and survival rate. A clear pattern is visible: the probability of being still alive at 50 or at 80 is growing with school leaving age. As expected, survival variability is larger at 80 than at 50, and survival rates at 50 are beyond 90% for every school leaving age.

Figure 1: School leaving age and survival rate at 80 years old, Zay reform



Scope: 35 828 individuals born between 1920 and 1925.

Figure 2: School leaving age and survival rate at 50 years old, Berthoin reform



Scope: 45 337 individuals born between 1950 and 1955.

Let's now focus on level of education. Figures 3 and 5 represent mean school leaving age by birth cohort. We notice the well-known positive trend, accounting for the general increase in education during the XXth century.

[2] By construction of the EDP, individuals born before 1968 have to be still alive in 1968 to appear in the EDP. To take into account this sample selection, we excluded from our Zay sub-sample those born between 1920 and 1925 who died before turning 48. We applied the same method on the Berthoin sub-sample, and kept only those still alive at 18. Therefore our health measures must be understood as *survival rate at 80 (resp. 50) given that one is still alive at 48 (resp. 18).*

The mean increase in education between 1920 and 1960 is around one month per year. Vertical lines indicate the dates Zay and Berthoin reforms came into effect. A sudden break is visible in 1953 (Figure 3): mean school leaving age increased by 4 months between 1952 and 1953. It comes from the change in compulsory education laws which extended mandatory schooling from 14 to 16 for those born after January 1st 1953. In theory, every individual born from 1953 on should have been to school until the age of 16. However this is not what we observe in our data. The proportion of people leaving school after 16 does increase sharply between 1952 and 1953 (see Figure 4), but there are still 15% of generations after 1953 who give up school before 16. This may be due to exemption clauses for work purpose. Yet this fact doesn't jeopardize our estimation strategy, since identification only requires that the institutional change induced a discontinuity in the average school leaving age.

Figure 3: Mean school leaving age by birth cohort, Berthoin reform

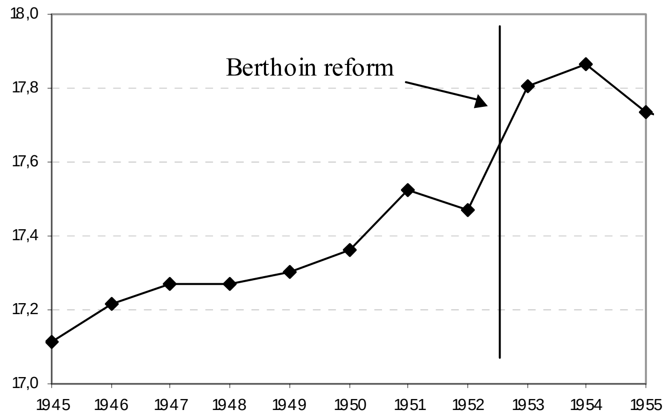


Figure 4: Proportion of each generation still studying at the age of 16, Berthoin reform

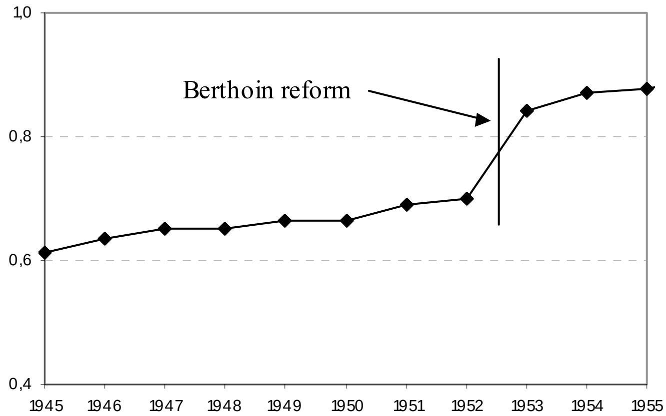
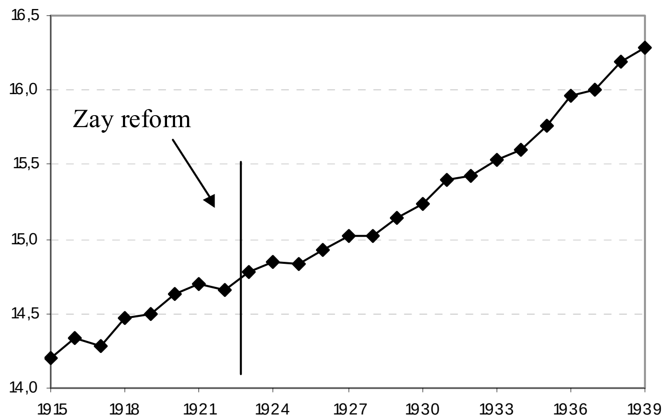
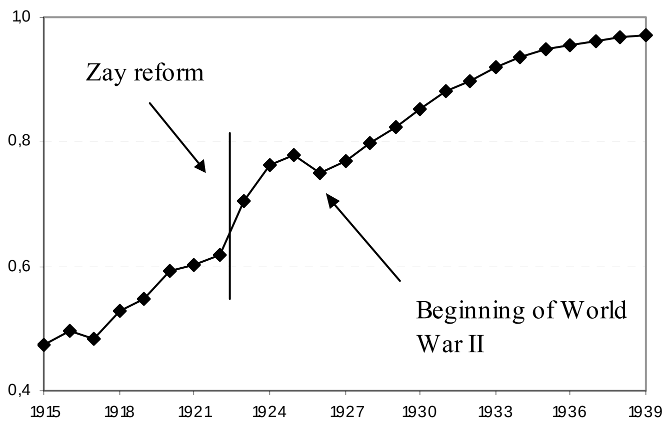


Figure 5: Mean school leaving age by birth cohort, Zay reform



Scope: Individuals born between 1915 and 1939.

Figure 6: Proportion of each generation still studying at the age of 14, Zay reform



Scope: Individuals born between 1915 and 1939.

The discontinuity induced by the Zay reform is less obvious (see Figures 5 and 6). Its magnitude is logically less important than in the Berthoin case, because Zay extended compulsory education by only one year, whereas Berthoin raised mandatory education by two years. Moreover, two historical reasons tend to smooth the impact of Zay reform on education. Lower secondary education gradually became free in France between 1928 and 1933 (Prost, 1968), which led to a dramatic increase in the number of students in secondary schools in the beginning of the 30ies. When Zay reform came into force in 1936, its impact on education was lessen because of that previous increase in education. The second reason is World War II, which created a drop in education level for those who left school after 1939. It is worth noticing that the war didn't have any impact until the 1926 generation on the share of each generation leaving school after turning 14. So this education indicator was not affected by the war for cohorts in our estimation sample (between 1920 and 1925). On the contrary, school leaving age was disrupted before 1925 on the high tail of the distribution[3]. As we wish to draw causal conclusions out of variations in education, it is essential to know precisely what are the sources of variation in education variables and how they affect these variables. Added to the fact that the interpretation of return to education in the Zay case does not depend on the treatment definition (binary or with variable intensity), it strengthens the use of a binary treatment with the Zay reform.

4 Results

Before investigating the very nature of the relationship between education and health, we first have to check whether there is indeed a significant correlation in our data. Figures 1 and 2 suggest that there is a strong positive link exists between school leaving age and survival at 50 or 80 years old. We regressed health status y_i of individual i (a dummy variable equal to 1 if i is still alive at 50 (resp. 80), and 0 otherwise) on his school leaving age x_i. Result of these probit estimations confirms that education is indeed correlated with survival rates at 50 and 80 years old (Table 2). This correlation doesn't necessarily mean that education has a causal impact on health. Omitted variables or a mix of unobserved variables and causal mechanisms could also create that correlation. To test the existence of causality, we now implement a regression discontinuity design.

Table 2: Regression of survival rate on school leaving age

	Estimations	
	Zay reform Dep. Var.: survival at 80	Berthoin reform Dep. Var.: survival at 50
intercept	-0.19*** (0,036)	0.83*** (0,06)
education	0.028*** (0,0024)	0.049*** (0,0035)

Standard errors in parenthesis. *** indicate a significant coefficient at the 1% confidence level.

Scope: Individuals born between 1920 and 1925 for the Zay reform, between 1950 and 1955 for the Berthoin reform. Sample sizes are respectively 35 828 and 47 337.

Under assumptions (H_0), (H_1), (H_2) and (H_3), the Wald quotient estimates the local average treatment effect for the compliers. Empirical counterparts of the four limits in the Wald estimator can be estimated by restricting the estimation sample to individuals just above and below the discontinuity z_0. As the Berthoin reform came into force for those born after January 1st 1953, we used the 1952 cohort to compute left limits y^- et x^- and the 1953 cohort to compute right limits y^+ et x^+. We also provided two alternative specifications, by gradually widening the number of cohorts around 1953 up to 6 cohorts. Table 3 shows the results of such estimations for the Berthoin reform, while Table 4 is devoted to the Zay reform. Health outcome y is a dummy equal to 1 if the individual is still alive at a given age. The treatment variable x is the school leaving age. Following last section discussion, we also used a binary treatment (whether or not studied after 14 years old) in the Zay case. From Table 3, one extra year of schooling increases by 0.013 the probability of being still alive at 52 years old. As expected, education indeed lowers mortality, but this treatment effect is not statistically different from 0 at the 10% confidence level. When we gradually widen the estimation window, neither the 1951-1954 nor the 1950-1955 Wald estimators are significant at the 10% confidence level.

[3] The decrease in education induced by the war was focused mainly on cohorts born after the implementation of the Zay reform. Hence the war may have reduced the size of the discontinuity in school leaving age created by the Zay reform.

Table 3: Wald estimators for the Berthoin reform

Generations	Wald estimator x = school leaving age
1952/1953 y = survival at 52	0.013 (0,011)
1951/1954 y = survival at 51	0.0072 (0,0072)
1950/1955 y = survival at 50	0.0033 (0,0061)

Standard errors in parenthesis, estimated by bootstrap. * indicates a significant coefficient at the 10% confidence level.
Sample sizes are respectively 15 518, 31 305, and 47 337.

Similar results on the Zay reform are presented in Table 4. Once again, all Wald estimators are positive but not statistically significant. Survival is measured around the age of 80, which seems to be old enough to observe differences in mortality. Yet it is possible that the Zay reform had too light an impact on education to significatively affect subsequent survival rates: it increased mandatory education by only one year, and both free secondary school and World War II reduced its impact on education.

Table 4: Wald estimators for the Zay reform

Generations	Wald estimator x = school leaving age	Wald estimator x = still study at 14
1922/1923 y = survival at 82	0.063 (0,62)	0.10 (0.11)
1921/1924 y = survival at 81	0.052 (0,067)	0.049 (0.053)
1920/1925 y = survival at 80	0.045 (0,035)	0.048 (0.036)

Standard errors in parenthesis, estimated by bootstrap. * indicates a significant coefficient at the 10% confidence level.
Sample sizes are respectively 11 586, 23 519, and 35 828.

The previous estimations were computed on samples containing between 2 and 6 births cohorts. It is important to keep in mind that regression discontinuity estimates are unbiased only if the populations below and above the cutoff point are identical in all dimensions relevant to health, except education. The validity of this continuity assumption becomes less and less likely as we widen the number of generations in our estimations, which is why we limited to 6 the maximum number of cohorts[4]. As described in section 2, the proper way to extend the number of generations in a regression discontinuity framework is to implement a two-step procedure. The first step equation is:

$$E(x_i \mid z_i) = a + b.z + c.(z - z_0).1_{z \geq z_0} + \delta.1_{\{z_i >= z_0\}}$$

[4] Distribution of observed variables such as sex and socio-professional group are quite similar in the six birth cohorts, which is a good sign that extending to six years does not invalidate the regression discontinuity approach.

where δ explicitly captures the discontinuity in education caused by the reform (z_0 equals 1923 for the Zay reform and 1953 for the Berthoin reform). The control function is linear in the year of birth z, its slope is allowed to be different before and after the reform date z_0.

Average school leaving age increases by 0.06 year each year between 1950 and 1952 (column 3 of Table 5). The magnitude of this coefficient is in line with the long-term temporal trend observed between 1920 and 1960. The coming into force of the Berthoin reform in 1953 causes a highly significant 0.28 year rise in education. This sudden increase seems to be absorbed during the following years as yearly growth rate becomes negative between 1953 and 1955. It is indeed a temporary phase because level of education increases at roughly the same rate in the fifteen years preceding the Berthoin reform and the fifteen years following it. The Zay reform created no clear discontinuity in average school leaving age. This was expected for reasons mentioned above. On the other hand, this reform had a significant impact on the proportion of individuals staying at school at least until they turn 14 years old: 8% of the generation 1923 was forced to go on studying with the implementation of the Zay reform.

Table 5: First stage estimations

	Estimations		
	Zay reform		Berthoin reform
	Binary treatment	Variable intensity treatment	Variable intensity treatment
a	0,33*** (0,09)	14,51*** (0,53)	14,58*** (1,25)
b	0,013*** (0,004)	0,0071 (0,025)	0,056** (0,025)
c	0,023*** (0,006)	0,028 (0,036)	-0,098*** (0,035)
δ	0,080*** (0,011)	0,11 (0,064)	0,28*** (0,063)

Standard errors in parenthesis. ** and *** indicate a significant coefficient at the 5% and 1% confidence levels respectively. In column 1 (binary treatment), the dependent variable is a dummy equal to one if individual was still attending school at the age of 14. School leaving age is the dependent variable in case of a treatment with variable intensity. We substracted 1900 to the year of birth. The Berthoin (resp. Zay) sample contains 47 337 (resp. 35 828) individuals born between 1950 and 1955 (resp. 1920 and 1925).

In a second step, we plugged the fitted value $\hat{E}(x_i|z_i)$ from the first stage estimation into the health equation. The return of one year of extra schooling β is estimated using the following probit model:

$$\begin{cases} y_i^* = \alpha + \beta \hat{E}(x_i / z_i) + \gamma z_i + v_i \\ y_i = 1_{\{y_i^* \geq 0\}} \end{cases}$$

Results of such an estimation are shown in Table 6. Like Wald estimators, school leaving age has a positive effect on survival at 50 years old. Nevertheless this coefficient is not significant at the 10% confidence level. Concerning the Zay reform, neither school leaving age nor the proportion of each generation still studying after

14 have a significant impact on survival at 80 years old. Eventually we don't find any causal impact of education on survival rate[5].

Table 6: Second stage estimations (probit)

	Estimations		
	Zay reform Dependent variable: survival at 80		Berthoin reform Dependent variable: survival at 50
	Binary treatment	Variable intensity treatment	Variable intensity treatment
α	-0,094 (0,13)	3,43 (4,41)	0,075 (1,51)
β	-0,32 (0,36)	-0,24 (0,32)	0,13 (0,11)
γ	0,020 (0,016)	0,017 (0,015)	-0,014 (0,012)

Standard errors in parenthesis. ** and *** indicate a significant coefficient at the 5% and 1% confidence levels respectively. We subtracted 1900 to the year of birth. The Berthoin (resp. Zay) sample contains 47 337 (resp. 35 828) individuals born between 1950 and 1955 (resp. 1920 and 1925).

5 Discussion

Our empirical work is based on large samples containing around 40 000 individuals. Zay reform allows us to measure health at 80, which should be old enough to detect differences in mortality due to education if education did help to preserve one's health capital. Zay reform certainly didn't increase much mean school leaving age, but it did force a significant share of the population to go on studying until 14. Berthoin reform indeed caused a sharp increase in school leaving age. Moreover 50 years old doesn't seem to be too young an age to reveal significant return to education on survival, since Lleras Muney (2005) finds a causal effect on mortality at similar ages. However, despite the large size of our dataset, we don't find any causal impact of education on health.

These results are not in the mainstream of the existing literature, since a majority of studies showed a significant return to education on health. Among authors who specifically exploited temporal variations in mandatory education as a source of identification, four of them found a significant impact of education on health (Spasojevic, 2003 ; Glied and Lleras-Muney, 2003 ; Lleras-Muney, 2005 ; Oreopoulos, 2006) while Doyle et al. (2005) and Arendt (2005) didn't. Adams (2002) had mixed results depending on the various health measures he considered. All these authors set up an instrumental variable framework. Moreover, Lleras-Muney (2005) implemented both an IV approach and a nonparametric regression discontinuity design. She showed a significant return to education only with the former framework, and explained away the non-significant RD results with imprecision in her computed death rates and a small sample size. We believe that RD designs are more relevant than IV when the exogenous variation in education is supposed to affect only individuals born after a given year. The reason is that changes in school leaving age, taken as an instrument, gradually lose some of their validity when the sample is extended to more birth cohorts. In particular the exclusion condition is no longer fulfilled if the sample is too wide, because year of birth may have a direct impact on health status, even when health is

[5] Other specifications have also been tested: separate regressions for men and women, and survival rates measured at different ages between 65 and 80 years old for the Zay reform. Results of these estimations, available upon request, are qualitatively similar to those presented in this paper. In particular, they don't show any significant causal impact of education on health.

measured as the same age for everyone. Indeed health state of the overall population keeps bettering over time thanks to more efficient medicine and hygiene: those turning 50 today are in a better shape than 50 years old were a few decades ago. These improvements in health cannot be ignored when cohorts born ten or twenty years apart are in the same sample. This leads to a violation of the exclusion condition, since the simple fact of being born before or after the reform took place has a direct impact on health, besides the possible indirect one passing through education. Therefore education laws are a fully valid instrument only when used on a very small number of consecutive birth cohorts. An alternative approach could be to try to control for this generation effect by adding the appropriate function of year of birth in the health and education equations, which is more or less the essence of the parametric estimation we provided. The obvious difficulty is here to choose the relevant functional form. Few authors considered this issue, mainly because they often simultaneously faced the fact that health was not measured at the same age for everyone in their sample. As the structure of their dataset didn't allow them to account for both effects, they chose to ignore the generation effect and focused on controlling the age effect by adding age as a covariate (Berger and Leigh, 1989 ; Arkes, 2003). It is likely that differences in health due to age are larger than those created by the generation effect, therefore their approach was certainly the correct one given their dataset. However, it would be more satisfactory to control for both sources of bias. Besides, assuming a linear dependence for the age or generation effects might be too restrictive, and a more flexible specification would be preferable. Lleras-Muney (2005) was able to include birth cohort dummies to account for the generation effect, but her model was identified solely because she took advantage of a very specific feature in her dataset covering all American states: changes in education laws did not happen the same year in all American states. Using the same source of identification on American data, Glied and Lleras-Muney (2003) opted for a flexible specification of the generation effect (birth cohort dummies) and a quadratic function of age. In general, changes in compulsory education were implemented at the country scale, which is the larger geographic area covered by usual datasets. In such a case it is impossible to separate the before/after reform dummy from the birth cohort dummies in an IV framework. Oreopoulos (2006) circumvented this issue on UK data by using quartic polynomial controls for birth cohort and age, instead of dummy variables. Our parametric estimation is similar to his. However, identification required that he specified a polynomial function for both birth cohort and age effects. We can avoid one of these two potential sources of misspecification, since our dataset enables us to measure health at the same age for everyone. By restricting our sample to only 6 birth cohorts, we limit the impact that (any misspecification of) the generation effect might have on the estimates.

6 Conclusion

Correlation between education and health is well documented in epidemiological literature. There is a strong empirical link, and this holds with various measures of health: self-rated health, morbidity, mortality, and physical limitations. However, possible mechanisms creating that correlation are still largely unknown. In the last years a growing number of studies have focused on determining whether causal mechanisms could explain part of that correlation. As education level is an individual choice depending on unobserved characteristics, an endogeneity problem arises when these characteristics also affect health status. Such variables could be familial background or time preferences. A common strategy to deal with this issue is to exploit exogenous variations in education. Such variations in education are not correlated with other variables influencing health, and thus it is possible to test the existence of a causality running from education to health. Changes in compulsory education laws are a good candidate for such exogenous shocks, and they are widely used to estimate both monetary and non-monetary returns to education. We exploit two changes in mandatory schooling in France, which increased minimal school leaving age by one year in 1923 and two more years in 1953.

Authors using that kind of reform usually estimate return to education in an instrumental variable framework. The first contribution of our paper is to use a regression discontinuity design, which is more appropriate to the nature of such legal changes. Moreover assumptions required for identification are weaker than in the instrumental variable approach. In particular it allows us to provide a nonparametric estimate of return to education. Our second main contribution is that health status is measured at the same age for everyone. As most empirical papers work on cross-sectional datasets, individuals in their sample don't have the same age when their health is measured. Therefore they must take into account a possible age effect on health if they want to estimate an unbiased return to education. Working on survival rates at a constant age is a convenient way to be sure that differences in health are not due to any misspecification in the health dependence in age.

Our results lead us to believe that education between 14 and 16 didn't have any influence on subsequent mortality for those affected by Zay and Berthoin reforms. However it is possible that years of schooling during early childhood or beyond the age of 16 did have an impact. Unfortunately our instruments don't allow us to test this hypothesis. It may also be the case that school leaving age is not the relevant proxy for education, and that relative level of education compared to those who left school during the same year is what really matters in terms of health. According to Grenet (2003), earnings in French labor market depended more on ranking in education level hierarchy than on the actual school leaving age in those days. As Zay and Berthoin reforms most likely didn't alter this ranking in education, job opportunities of a given decile of school leaving ages distribution did not change even if their mean school leaving age increased. Hence Zay and Berthoin reforms didn't trigger the indirect causal mechanism linking education and health through an income effect. In the same vein, possible causality passing through working conditions (less wearing jobs for the more educated) cannot be tested with our instruments. Therefore it is logical that we don't find any causal impact if mechanisms creating that causality are based solely on ranking in education levels. Finally, a key feature of both reforms is that they forced some people to study longer than they wished. Generally speaking, return to education might depend on student's motivation during schooling. Individuals obliged to stay at school may benefit less from an extra year of schooling than those voluntarily going on studying.

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