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Abstract

Less schooling is associated with increased biological risks for chronic disease, but whether increasing years of schooling through policy interventions reduces these risks remains unclear. We examine the effect of a major education reform introduced in 1959 that raised the minimum school leaving age from 14 to 16 years in France, offering a unique natural experiment. We assess the causal impact of increased schooling duration on 16 biomarkers of cardiovascular, metabolic, organ and immune function in a large cohort of men and women born around 1953. Using a Regression Discontinuity Design, we find that the reform led to a significant increase in schooling duration among children from disadvantaged families; but longer schooling did not translate into better biomarker profiles in adulthood. Eligibility to the reform had no impact on the biomarker profile of respondents from intermediate or high social class families, while it led to increased blood pressure and white cells counts in adulthood among those from low parental social class. These findings were robust across several sensitivity analyses. They emphasize the importance of considering the institutional context and the respondents' social origins when evaluating the health effects of compulsory schooling reforms. Our results do not necessarily question the premise that education leads to better health, but they suggest that law-mandated increases in schooling alone may not improve the health of disadvantaged groups.

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Introduction

There is compelling evidence that higher educational attainment and longer schooling are associated with better health and longevity. This association, consistent across countries, outcomes and over time, has fueled interest in education policies as potential tools to improve population health (Klebanoff Cohen & Syme, 2013). However, understanding the causal nature of the association between education, schooling duration and health has proved challenging (Karas Montez & Freidman, 2015; Kawachi, Adler, & Dow, 2010). First, both health and education share common determinants including early life circumstances, time preferences and cognitive ability (Cutler & Lleras-Muney, 2012), which may confound the association. Second, childhood health may influence educational attainment and schooling duration, leading to reverse causality (Case, Fertig, & Paxson, 2005; Palloni, 2006). Third, few studies have been able to disentangle the relative contribution of the duration of schooling relative to that of educational credentials in the observed relationship between education and health. This is important because most European education reforms focused on increasing the duration of compulsory schooling. In addition, the biological mechanisms underlying the association between education and health remain elusive. Although there is evidence that lower educational attainment is associated with poorer biomarker profiles (Kavanagh et al., 2010; Seeman et al., 2008), this association varies by biomarker, measure of socioeconomic position and population subgroup (Dowd, Simanek, & Aiello, 2009; Seeman, Epel, Gruenewald, Karlamangla, & McEwen, 2010). For example, lower education was associated with lower risk of several chronic stress biomarkers, but higher risk of some cardiovascular markers among women in Taiwan (Dowd & Goldman, 2006). Overall, the causal nature and biological mechanisms underlying the relationship between education and health are only partially understood.

This paper aims to bring new evidence to this literature by examining how an exogenous change in schooling duration caused by a major education reform influenced biological risk profiles decades later. Compulsory schooling laws were introduced in many European countries and the United States during the 20th Century (Brunello, Fort, & Weber, 2009). An extensive literature in economics and public health has used these reforms to examine the causal effect of schooling duration on a range of adult specific diseases and risk factors (Banks & Mazzona, 2012; Davies, Dickson, Davey Smith, van den Berg, & Windmeijer, 2018; Dursun & Cesur, 2016; Glymour, Kawachi, Jencks, & Berkman, 2008; Huang, 2015; Nafilyan, Avendano, & De Coulon, 2017; Nguyen et al., 2016; Schneeweis, Skirbekk, & Winter-Ebmer, 2014), health behaviors (Jürges, Reinhold, & Salm, 2011; Silles, 2015), health-related knowledge (Johnston, Lordan, Shields, & Suziedelyte, 2015), and mortality (Albouy & Lequien, 2009; Clark & Royer, 2013; Gathmann, Jürges, & Reinhold, 2015; Lager & Torssander, 2012; Lleras-Muney, 2005). However, a recent review and meta-analysis of the health effects of compulsory schooling laws identified a paucity of research on biomarkers of health (Hamad, Elser, Tran, Rehkopf, & Goodman, 2018). Indeed, only three studies to date have focused on biomarkers; and their findings offer contradictory evidence of a causal relationship. Jürges and colleagues (2013) reported no significant effect of two English compulsory schooling reforms on biomarkers of inflammation and chronic stress. Examining the same reforms in the UK, Powdthavee (2010) found that the 1947 compulsory schooling reform reduced hypertension, while the second law change in 1972 did not induce significant effects on blood pressure. More recently, Barcellos and colleagues found that the 1972 schooling reform was associated with reduced body size and improved lung function, but increased blood pressure in mid-adulthood (2018). Overall, the available evidence is largely focused on UK reforms and offers no conclusive answer on the potential impact of schooling on biological markers.

In this study, we focus on the 1959 Berthoin reform, which raised the minimum school leaving age from 14 to 16 years for all French residents born after 1st January 1953. Previous work has examined the effect of this reform on employment and earnings (Grenet, 2013), but no studies to date have examined its impact on biological markers of disease. While no effect of the reform on mortality was found (Albouy & Lequien, 2009), the affected cohorts might still be too young, and mortality may not fully reflect potential impacts on health or sub-clinical disease. Biomarkers consequently offer several advantages to investigate further the health effects of the reform. First, they reflect subclinical disease that has not yet manifested in observable clinical outcomes. This is especially important if we want to understand early pre-disease pathways in middle adulthood. Biomarkers can also shed light on the mechanisms through which education may impact health, for example by identifying cardiovascular or immune pathways associated with increased schooling duration (Crimmins & Seeman, 2004). Third, biomarkers can provide a more 'objective' assessment compared to self-reported health measures susceptible to reporting or other measurement biases (Dowd & Todd, 2011). Our study, therefore, addresses a gap by assessing the effects of a major education reform on the biological profiles of a relatively young sample of adults.

Our study makes three key contributions to the existing quasi-experimental literature on the effects of schooling duration on health. First, we examine the effect of the Berthoin reform on 16 biological indicators of sub-clinical disease including anthropometrics, blood glucose, lipids, blood pressure, as well as liver and kidney function. This large array of biomarkers enables us to consider different health dimensions, as previous research has showed that compulsory schooling laws can improve some outcomes while negatively affecting others (Hamad et al., 2018). We use data from a unique population-based survey that collected both educational attainment data and extensive health and biological measures in nearly 60,000 adults living in France at the time of the study. Second, previous research has been hampered by the lack of information about parental socioeconomic background, an important variable given that compulsory schooling laws primarily affect the socioeconomic outcomes of individuals from disadvantaged families (Brunello et al., 2009). Our study includes information on parental social class during the respondent's adolescence, enabling us to explore heterogeneous effects of the reform across individuals from different socioeconomic origin. Understanding whether and why compulsory schooling laws affect population subgroups differently is crucial in the pursuit of health equity (Glymour & Manly, 2018). Finally, exploiting the nature of the reform, we isolate the impact of an increase in schooling duration on health, net of the effect of educational qualifications. This is possible because the reform successfully increased schooling duration but had no impact on the share of the population owning educational credentials (Donegani & Sadoun, 1976). Our study therefore estimates the health effects of a policy that increased the quantity of schooling, without altering other dimensions of the educational system.

Data and Methods

The 1959 Berthoin compulsory schooling law

The 1959 edict raised the minimum school leaving age by two years – from 14 to 16 - for all children born after the 1st of January 1953. The reform was applied uniformly across the country (Grenet, 2013). The number of pupils in secondary school increased rapidly after the reform, raising from 474,500 in 1959-60 to 789,300 in 1963-64 (Defresne & Krop, 2016). Large structural investments were made to accommodate these increases in enrollment, including the construction of new secondary schools (Grenet, 2013). **Appendix Figure 1** provides an overview of the French schooling system at the time of the reform. The French curriculum was organized around a set of academic and vocational qualifications which could be obtained through examination primarily at the end of upper secondary school when students were 17 or 18. An optional certificate could also be obtained at the end of lower secondary school (when students were 13 or 14) but it was not designed to certify completion of secondary education. Although the reform aimed to foster social mobility for children from disadvantaged backgrounds by building a comprehensive curriculum of secondary schooling, it did not couple the increase in schooling duration with a new certificate or diploma, nor did it alter the existing curriculum and tracking of students (Allaire & Franck, 1995; Donegani & Sadoun, 1976).

Study population

Our primary dataset is the Constances cohort, a sample of French adults aged 18-69 launched in 2012, with the aim of collecting data on 200,000 individuals over a 6-year period (Zins & Goldberg, 2015). Participants were randomly selected to take part in the study and invited to a one-day clinical examination in one of the 22 Health Screening Centres run by the National Health Insurance Agency. At baseline, a range of comprehensive health assessments were carried out by health professionals, and participants were asked to complete questionnaires about their health and socioeconomic circumstances. At the time of this study, clinical data was available for 58,029 respondents. As detailed below, we restrict our analysis to participants born at most 48 months before or after the cut-off for eligibility to the reform (1st of January 1953, N=18,915).

Anthropometry, blood pressure and blood-based biomarkers

Participants were invited to a health examination, where anthropometry, blood pressure and serum were collected. Quality control was in place to ensure the quality of physiological data across different sites, details of which have been previously described (Ruiz et al., 2016).

The clinical team measured weight, height as well as waist and hip circumference for all respondents. Body mass index (BMI) was calculated as weight in kilograms divided by height in meters squared. Blood pressure measurements (systolic and diastolic blood pressure) were taken from each arm after a one-minute rest. Blood samples were taken from all participants. They were instructed to fast 12 hours prior to collection, which was carried out between 8 and 10 am. Blood sugar levels were assessed by fasting blood glucose, and blood lipid levels by total cholesterol, high-density lipoproteins and triglycerides. Liver function was measured by gamma-glumamyltransferase (gamma GT) and alanine transaminase. Serum creatinine was used as an indicator of kidney function. Hematology measures included counts of white blood cells, hemoglobin, hematocrit and platelets. Biomarker data were screened for outliers and unreasonable values removed based on established guidelines for the Constances cohort (Magnusson Hanson et al., 2017). In addition, BMI, triglycerides, glucose, creatinine, gamma GT, white blood cell count and platelet values are log-transformed, while hematocrit values are squared to decrease skewness. The score for each biomarker is standardized to allow comparisons across measures.

Socio-demographic characteristics

Socio-demographic information comes from self-administered questionnaires. Educational attainment is categorized as primary, secondary and tertiary education. We classify respondents' and parental social class as low (e.g. manual worker, child minder, office employee), intermediate (e.g. teachers, nurses, technicians) and high (e.g. executives, engineers, physicians). Marital status is defined as married, in a civil partnership, single, separated, divorced or widowed. Country of origin is categorized as France (including French oversea territories), elsewhere in Europe, Northern Africa and other. Monthly household gross income is recorded as less than 450 euros, 450 to less than 1000 euros, 1000 to less than 1500 euros, 15000 to less than 2100 euros, 2100 to less than 2800 euros, 2800 to less than 4200 euros and 4200 euros or more.

Constances records the highest educational qualification, but not the number of years of schooling. This is problematic because the Berthoin reform increased the number of years of schooling, but had no effect on qualification attainment (Grenet, 2013). To document the effect of the reform on years of schooling, we therefore use data from the French Labor Force Survey (LFS) collected between 2003 and 2012. The French LFS is a household survey representative of the French population living in private households conducted by the French National Statistical Institute, the INSEE. We use data on the age at which respondents left full-time education to document the impact of the reform on school leaving age and years of completed schooling. We include in our analytical sample respondents born within three years of the reform. We focus only on data from the first interview, so that we only have one observation per individual. Our analytical sample to look at the effect of the reform on school leaving age using the French LFS includes 72,133 individuals born up to 48 months before or after the reform.

Study design

We analyze the effect of the Berthoin reform on each biomarker using a regression discontinuity design (RDD), an econometric approach increasingly used in epidemiology and public health (Huang & Zhou, 2013; Lee & Lemieux, 2010; Moscoe, Bor, & Barnighausen, 2015; Venkataramani, Bor, & Jena, 2016). The design takes advantage of policy implementation rules which assign individuals to receive the intervention or not depending on whether they fall above or below an arbitrary cut-off (in our case date of birth). Observations closely above an arbitrary cut-off are eligible to the reform, while those closely below the same cut-off are ineligible. Under a number of assumptions, the 'exogenous' sharp increase in compulsory school leaving age induced by the reform at the cut-off of the 1st of January 1953 allows us to investigate the causal relationship between schooling and a range of biomarkers. Following Albouy and Lequien (2009), we select individuals born at most 48 months before or after the Berthoin reform (born between 1950 and 1955, *N*=18,915). We estimate the discontinuity in the average school leaving age induced by the reform by comparing cohorts born before and after the cut-off for eligibility. Our main specification is as follows:

$$bio_{ict} = \beta_0 + \beta_1 D_{ic} + f(R_{ic}) + \boldsymbol{x}_{ict}\beta_2 + u_{ict}$$

Where bio_{ict} is the individual biomarker value for individual i from birth cohort c at time t; D_{ic} is a binary variable taking the value of 1 if an individual was born up to 48 months after the cut-off date (the treated group), and of 0 if the individual was born up to 48 months prior to the cut-off date (the control group); R_{ic} is an individual's birth cohort, relative to the cut-off measured in month; and x_{ict} is a vector of individual characteristics: age, age squared, gender and month of birth.

In theory, the reform would have only increased the educational attainment of respondents who would have dropped out of school earlier in the absence of the reform. In addition, potentially not all individuals complied with the law and dropped out of school earlier than age 16. Our results can therefore be considered as 'intent-to-treat' estimates, as they conflate the effect of eligibility to the reform among both respondents who increased their schooling duration in response to the reform and those who did not.

The validity of this approach relies on two key identifying assumptions. First, it assumes that individuals cannot manipulate the value of the treatment variable, so that eligibility to the reform is as good as random for observations close to the cut-off. To our knowledge, there is no evidence of manipulation as eligibility was based on the date of birth of children who were six at the time the reform was announced. Second, it assumes that the outcome would have been continuous at the cut-off in the absence of the reform. Although it cannot be directly tested empirically, we provide some evidence in support of this assumption in sensitivity analyses.

We test whether the effects differed by parental social class by stratifying all our analyses. We also investigate whether the effects differed by gender. To address multiple hypothesis testing, we implement a set of adjustment methods (Bonferroni-Holm, Sidak-Holm and Westfall-Young) using the wyoung command in Stata (Jones, Molitor, & Reif, 2018).

Finally, most existing studies of the effect of compulsory schooling on health have estimated average effects, which may conceal additional heterogeneous effects on specific parts of the health distribution (Barcellos et al., 2018). In supplementary models, we estimate quantile treatment effects (QTE) across the distributions of biomarkers (Bitler, Gelbach, & Hoynes, 2006; Djebbari & Smith, 2008). Whilst linear regression focuses on the effect of the reform on the conditional mean, quantile regressions allows us to estimate the effect of the reform on different sections of the conditional distribution. QTE are a way to test whether the impact of the Berthoin reform is constant or differs across quantiles of the outcome distributions (*e.g.* at the bottom of the distribution among the least healthy or at the top of the distribution among the healthiest). We estimate QTE at each decile of the distribution of biomarkers, and present results graphically.

All analyses were conducted using Stata 14.

Results

Table 1 displays the sample characteristics, by eligibility status to the Berthoin reform. The eligible and ineligible groups were broadly similar in terms of pre-reform characteristics, including gender and parental social class during adolescence. As eligibility is based on the date of birth, respondents from the control group were older (mean=63.67) than the treated group (mean=59.72).

Appendix Table 1 shows OLS estimates of the association between educational attainment and individual biomarkers. It confirms that in our sample, higher education was consistently associated with healthier biological profiles.

We now turn to our estimation of the effect of the Berthoin reform on biomarkers using an RDD. We start by examining the impact of the reform on schooling duration using the French Labor Force Survey. **Figure 1** shows the discontinuity in school leaving age for the cohorts born before and after 1st January 1953. The dotted line represents the cut-off for the first cohort affected by the reform. There is a clear discontinuity in school leaving age induced by the reform. These results are confirmed in **Table 2**, which summarizes the impact of eligibility to the reform on school leaving age and the odds of living school after the age of 16. As anticipated, the impact of the educational reform was concentrated among those coming from low social class families, the group most likely to otherwise have dropped out of school early. In this group, the reform increased the average time spent at school by an average of 3.9 months (95% CI 2.448 to 5.364) and the odds of leaving school after the age of 16 by 87.7% (OR=1.877, 95% CI 1.697 to 2.077).

Table 3 displays the effect of eligibility to the reform on individual biomarkers. In analyses for the total sample, as well as for those from intermediate or high parental social class, the reform had no impact on biomarker profiles. Among respondents from low parental social class, eligibility to the reform that raised compulsory schooling from age 14 to 16 years led to increased BMI, waist circumference, waist-hip ratio, systolic and diastolic blood pressure, triglycerides, white cell count, and hematocrit, all in the direction of *less healthy* profiles for those eligible for the reform. After correction for multiple testing, eligibility for the reform remained associated with a significant increase in diastolic blood pressure (β =0.153, 95% CI 0.066 to 0.239) and white cells count (β =0.146, 95% CI 0.052 to 0.240) among respondents from disadvantaged families. **Figure 2** illustrates these results by presenting the linear trends for these two outcomes by month of birth, among respondents from low social class families. For both outcomes, there is an apparent discontinuity for the 1953 cohort, the first cohort affected by the policy.

In supplementary analyses, we investigated whether the effect of eligibility to the reform differed by gender for the entire sample but no clear pattern emerged (see **Appendix Table 2**). **Appendix Figure 2** displays the effect of the reform across the diastolic blood pressure and white cells count distributions, estimated by quantile regressions. Each graph displays the estimate of the impact of the reform at accompanying 95% confidence intervals at different quantiles of the distribution of the two biomarkers. In both cases, results indicate that there are negative effects which are constant throughout the distribution, except in the very lowest and highest deciles.

Sensitivity analyses

One of the key assumptions of RDD is that the outcome probability is continuous at the cut-off in the absence of the reform (Smith, Levesque, Kaufman, & Strumpf, 2016; Venkataramani et al., 2016). To confirm that our estimates are driven by the policy change and not by secular trends, we estimate the effect of 'placebo reforms', *i.e.* estimates for years in which the reform did not take place. **Appendix Figure 3** displays estimates of discontinuity for cohorts born between 1947 and 1967. Reductions in diastolic blood pressure for the cohort born in 1950 might be associated with the May 1968 events. At the time, normal procedures for high school graduation were abandoned and as a consequence, more students could get a high school diploma and pursue higher education than would have been possible otherwise (Maurin & McNally, 2008). We speculate that higher graduation rates for this specific cohort might also have positive effects on health later in life. Three placebo reforms yielded significant effects for white blood cells counts – our results on this outcome should consequently be interpreted with caution.

Our RDD results for blood pressure and white blood cells counts are robust to different specifications, including a higher order polynomial (**Appendix Table 3**), the use of triangular kernel weights to give more weight to observations closer to the eligibility cut-off (**Appendix Table 4**), and different bandwidth sizes around the cut-off (**Appendix Figure 4**).

Discussion

In this paper, we exploit a reform implemented in 1959 in France to estimate the effect of extending compulsory schooling age from 14 to 16 on biological risk profiles decades later. Although there is a clear association between higher educational attainment and lower biological risk profiles in our sample, our results also indicate that a policy that increased schooling duration did not translate into improved health as measured by biomarkers. We find no evidence of health benefits for participants from intermediate and high parental social class, and we find some evidence that the reform may have had *negative effects* on blood pressure and white blood cells counts for respondents from poorer families.

Our findings for most biomarkers are consistent with those by Jürges and colleagues (2013) who found no significant effect of two British compulsory schooling reforms on biomarkers of inflammation and chronic stress for the total population. Their data, however, did not include information on parental social class – which might have concealed important heterogeneity in the health effects of the reform considered. In addition, our study covers a wider range of biomarkers, spanning metabolic processes, inflammation, anthropometric measures and organ functioning. The suggestion that the Berthoin reform may have had some negative effects on blood pressure is surprising but in line with recent findings from Barcellos et al. (2018) in the UK. In their study, eligibility to the 1972 British reform was associated with a significant increase in blood pressure, although it did not increase hypertension prevalence. In additional analyses, we confirm that eligibility to the Berthoin reform was not associated with increased hypertension (as measured by a diastolic blood pressure higher or equal to 90 mm Hg – **Appendix Table 5**). The negative effect of the reform on blood pressure remains under the high-risk threshold but might be indicative of underlying physiological processes in the exposed population that may manifest in poorer health in the long run. Indeed, clinical evidence shows that pre-high range levels of diastolic blood pressure (ranging from 75 mm Hg) are already associated with poor cardiovascular outcomes (Vasan et al., 2001).

A first explanation might lie in the fact that the reform impacted mainly one population sub-group: respondents from low parental social class who constitute about 45% of our sample. Galama, et al (2018) recently proposed a theoretical framework which may be useful in understanding why these respondents may have experienced low returns and high costs of longer schooling, potentially leading to negative health outcomes. The authors postulate that a critical pathway through which education affects health is skill formation; and that additional time spent in school following a compulsory schooling reform does not necessarily lead to additional skills. Indeed, the Berthoin reform did not require students to acquire a new diploma or qualification, which might have led to a larger transformation of the education system (Defresne & Krop, 2016). **Appendix Figure 5** displays the distributional effects of the reform on school leaving age. It indicates that the reform only impacted the bottom of the distribution (*i.e.* it reduced the proportion of respondents leaving school before the age of 16) but had no effect on the rest of the distribution, which would have been indicative of a longer-term effect on access to higher education for example. The contrasted effects of the Berthoin reform suggest that skill formation may be more important than the amount of time spent in school.

A related explanation comes from the limited effects of the reform on educational credentials (**Appendix Table 6**), which may be more critical than schooling duration to achieve better health outcomes. This would be consistent with the 'sheepskin effect', the idea that it is educational credentials which may potentially benefit health, and not the number of years of schooling (Liu et al., 2014). Two institutional features of the French schooling system in 1959 may explain why the Berthoin reform had limited effects on education attainment, even if it successfully increased schooling duration. First the reform only increased the odds of obtaining a lower secondary schooling certificate, an optional diploma with little value on the job market (Grenet, 2013; Le Rhun & Pollet, 2011). Second, raising the minimum school leaving age did not increase vocational qualifications – the most common pathway for children from poor families at the time of the reform - as these were only awarded at the end of the three-year vocational school curriculum, two years after the new minimum school leaving age. In the French context, where examination-based diplomas validate the completion of upper secondary schooling, education credentials may be an important factor to consider when assessing the health impact of law-mandated increases in compulsory schooling. Health returns on schooling might be higher when these reforms induce a significant increase in graduation rates for diplomas which validate skill acquisition and increase earnings (Grenet, 2013). It is also possible that the return on education is lower in France than in other countries such as the United States because of the generous redistribution and welfare system. Future research should investigate whether policy changes that led to higher graduation rates might have different effects on health (Etile & Jones, 2011).

A third explanation for the lack of positive health effects comes from the limited effects of the reform on labour market outcomes (Grenet, 2013). Education is hypothesized to influence health partly by enhancing access to health-promoting resources such as knowledge, social networks and better jobs and earnings. Previous evidence suggests that the reform did not lead to substantive improvements in these social and economic outcomes (Grenet, 2013). Supplementary analyses confirm that longer schooling did not translate into better wages (**Appendix Table 6**), indicating that those who were required to remain longer at school may not have benefitted economically from these additional years of schooling in the long run. In fact, for children from low parental social class, longer schooling duration may have represented a loss of work experience, which may have led to mismatched expectations on the potential returns on these additional years of schooling. This discrepancy might ultimately have led to increased levels of stress and poorer biomarker profiles in adulthood.

Our findings on diastolic blood pressure are in line with a recent study showing the detrimental effect of additional schooling on systolic blood pressure in England using a similar research design (Barcellos et al., 2018). It is also worth mentioning that the effect is similar in direction and magnitude for systolic blood pressure in our sample (β=0.128, 95% CI 0.038 to 0.217), although it did not remain significant once we account for multiple testing. A possible explanation could be blood pressure 'tracking' over the life course, *i.e.* the hypothesis that high blood pressure in adulthood is a result of high blood pressure in childhood (Chen & Wang, 2008). Our distributional analyses do not provide support to this hypothesis: quantile regressions showed that the effect of the reform was consistent over the health distribution for both blood pressure and white blood cells. The exact biological mechanism linking compulsory schooling to poorer health remains unclear and should be the subject of future research. Our findings relate to research in the United States showing that disadvantaged minorities with higher education reported higher allostatic load and cardiovascular biomarkers than their less educated counterparts (Brody et al., 2013; Gaydosh, Schorpp, Chen, Miller, & Mullan Harris, 2018; Miller, Yu, Chen, & Brody, 2015).

This study has several strengths and limitations. A major strength of our study is the use of a quasi-experimental design in a large population-based cohort. First, the data included a wide number of anthropometric and blood-based biomarkers for a large sample, which remains unusual for quasi-experimental designs. The biological markers used in this paper are objective health measures, likely to be more sensitive to subclinical changes in health risk than traditional measures of chronic health conditions or mortality. Second, although schooling reforms are often conceptualized in previous studies as an instrumental variable (IV) for increases in educational attainment, changes in mandatory schooling may influence other aspects of children's experience, such as income prospects, peer networks or school quality. Using an RDD, we estimate the overall effect of the reform, which would capture the overall effect of the policy, not only effects operating via increases in years of schooling. Based on the exogenous sharp increase in minimum school leaving age induced by the schooling reform, its causal effect on health can be estimated with fewer assumptions compared to other quasi-experimental designs (Moscoe et al., 2015; Venkataramani et al., 2016).

A first limitation is that our data are cross-sectional. However, our estimates focus on the impact of a reform affecting participants at a young age, long before biomarkers were collected in adulthood. Second, previous research has shown that a number of individual demographic, socioeconomic and health characteristics are associated with participation in the Constances cohort (Santin et al., 2016). In supplementary analyses, we showed that our estimates were robust to the inclusion of calibrated weights based on population totals from the LFS (results available upon request). Differential response rates by month of birth – the key variable on which eligibility to the reform is defined – would be a potential threat to the internal validity of our results. We found no evidence of significant differences in response rates between the eligible and ineligible cohorts (results available upon request). Another potential concern is that we cannot observe the effect of the reform on school leaving age in Constances. However, we carried out additional analyses that compared the impact of the reform on educational attainment between Constances and the French LFS. Results presented in **Appendix Table 6** indicate that the reform had nearly identical effects in the two surveys. Fourth, our RDD estimates should be understood as the long-term effects of a reform which only impacted respondents from low parental social class and those who would have left school earlier in the absence of the reform. These estimates cannot be extended to other cohorts or respondents whose time spent in school was not affected by the Berthoin reform. They also provide no information about other policies that may be more effective to shift the educational distribution towards higher educational attainment (Deaton, 2010; Heckman & Urzua, 2010). Finally, recent reviews outlined that the health effects of compulsory schooling laws are context- and time-dependent (Galama et al., 2018;

Hamad et al., 2018): reforms implemented in other countries earlier in the 20th century have often been more successful than the Berthoin reform in raising educational attainment, improving socioeconomic outcomes and ultimately health. These differences in the way compulsory reforms affect key intermediate outcomes should be considered when comparing estimates across countries.

Overall, our study contributes to a growing literature that uses quasi-experimental designs to estimate the impact of changes in schooling duration on health; and is one of the first to estimate the impacts of such reform on a wide range of biomarkers. We found no evidence of positive health benefits of increased schooling on biological markers of health, and some evidence of worsening blood pressure and inflammation for participants from lower parental social class. Law-mandated increases in schooling may not bring health benefits to respondents from disadvantaged backgrounds if longer schooling is not translated into improved intermediate socioeconomic outcomes. Our findings do not necessarily question the notion that education leads to better health, but they suggest that law-mandated increases in schooling duration alone may not be sufficient to improve the health of disadvantaged groups.

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FIGURE CAPTIONS

Figure 1. Impact of eligibility to the 1959 Berthoin reform on average school leaving age

Notes: Data are from the French Labor Force Survey (2003-2012). The dashed line represents the first cohort eligible to the reform (born after the 1st of January 1953). Each dot represents the average school leaving age for a specific month of birth, which are included are the distance between a respondent's month of birth and the eligibility cut-off (e.g. -12 corresponds to the average school leaving age of respondents born in January 1952). The fitted lines represent the linear trends for our analytical sample: respondents born up to 48 months before or after the reform.

Figure 2. Impact of eligibility to the 1959 Berthoin reform on diastolic blood pressure and white cells count among respondents from low parental social class

Notes: Data are from the Constances cohort. The dashed line represents the first cohort eligible to the reform (born after the 1st of January 1953). Each dot represents respondents' average health outcome for a specific month of birth, which are included are the distance between a respondent's month of birth and the eligibility cut-off. The fitted lines represent the linear trends for our analytical sample: respondents born up to 48 months before or after the reform.

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TABLES

Table 1. Sample characteristics by eligibility status (*N*=18,929)

	Eligible group (born on or after the 1st of January 1953, *N*=9,286)	Ineligible group (born before the 1st of January 1953, *N*=9,629)	*P* value
Mean Age (SD)	59.72 (1.64)	63.67 (1.65)	>0.001
Female (%)	51.98	51.24	0.31
Father's social class during adolescence (%)			0.41
Low	45.78	46.13	
Intermediate	38.74	39	
High	15.49	14.87	
Father's region of origin			0.09
France (mainland, oversea territories and departments)	84.65	84.70	
Europe	8.40	7.94	
Northern Africa	3.87	4.50	
Other	3.08	2.86	
Respondent's educational level (%)			0.84
Primary	3.24	3.35	
Secondary	53.98	53.94	
Tertiary	42.78	42.71	
Respondent's social class at entry in the survey			0.19
Low	11.27	10.37	
Intermediate	60.38	57.83	
High	28.35	31.8	

Notes: Data come from the Constances cohort. SD, Standard Deviation. Only respondents born within 48 months of the reform are included in our sample.

Table 2. Effect of eligibility to the 1959 Berthoin reform on school leaving age and odds of leaving school after the age of 16, by parental social class during adolescence

	All		High parental social class		Intermediate parental social class		Low parental social class	
	ß	95% CI	ß	95% CI	ß	95% CI	ß	95% CI
School leaving age	0.242***	0.151 to 0.333	0.154	-0.296 to 0.604	0.058	-0.109 to 0.225	0.360***	0.234 to 0.485
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Leaving school after the age of 16	1.698***	1.545 to 1.865	0.936	0.454 to 1.929	1.283*	1.060 to 1.553	1.875***	1.696 to 2.074

Notes: Data come from the French Labor Force Survey (2003-2012). All models control for age, age squared, month of birth, birth cohort relative to the cut-off point, interacted with the treatment. Standard errors are clustered at the month of birth level. Bandwidth is fixed at 48 months. ** $p<0.01$; *** $p<0.001$.

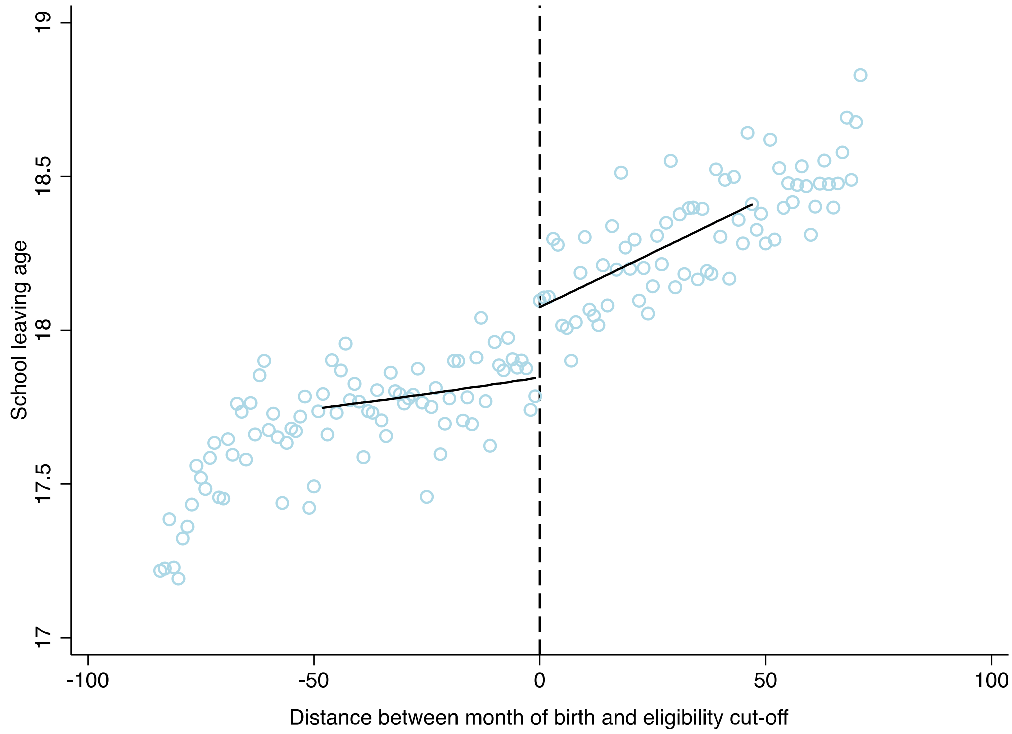
Table 3. Effect of eligibility to the 1959 Berthoin reform on anthropometry, blood pressure, glucose and lipids, liver and kidney functions and hematology, by parental social class during adolescence

	All		High parental social class		Intermediate parental social class		Low parental social class	
	ß	95% CI	ß	95% CI	ß	95% CI	ß	95% CI
BMI	0.024	-0.029 to 0.077	0.104	-0.038 to 0.247	-0.031	-0.117 to 0.054	0.084*	0.007 to 0.161
Waist circumference	0.009	-0.048 to 0.067	0.007	-0.144 to 0.159	-0.081	-0.164 to 0.0009	0.113**	0.029 to 0.196
Waist-hip ratio	-0.014	-0.073 to 0.044	-0.093	-0.250 to 0.064	-0.085	-0.172 to 0.002	0.086*	0.005 to 0.166
Systolic blood pressure	0.07	-0.016 to 0.124	0.103	-0.041 to 0.249	-0.014	-0.114 to 0.085	0.128**	0.038 to 0.217
Diastolic blood pressure	0.072	-0.018 to 0.126	-0.03	-0.158 to 0.097	0.005	-0.098 to 0.109	0.153***#	0.066 to 0.239
Blood glucose	0.047	-0.004 to 0.098	-0.016	-0.173 to 0.141	-0.052	-0.172 to 0.068	0.092	0.011 to 0.173
Total cholesterol	0.031	-0.018 to 0.081	0.189*	0.030 to 0.347	0.026	-0.062 to 0.115	0.033	-0.041 to 0.108
Cholesterol HDL	0.01	-0.047 to 0.068	0.17	0.002 to 0.339	0.04	-0.049 to 0.130	-0.083	-0.193 to 0.026
Triglycerides	0.052*	0.006 to 0.098	0.015	-0.122 to 0.153	0.025	-0.065 to 0.117	0.113***	0.044 to 0.181
Gamma GT	-0.006	-0.067 to 0.054	-0.068	-0.239 to 0.103	-0.06	-0.153 to 0.033	0.03	-0.062 to 0.124
Transaminase	0.005	-0.042 to 0.052	-0.023	-0.163 to 0.117	-0.004	-0.094 to 0.084	-0.02	-0.091 to 0.049
Creatinine	0.011	-0.046 to 0.069	-0.085	-0.254 to 0.083	0.022	-0.068 to 0.113	0.043	-0.042 to 0.128
White blood cells	0.042	-0.014 to 0.100	-0.003	-0.169 to 0.161	0.00001	-0.078 to 0.078	0.146***#	0.052 to 0.240
Haemoglobin	-0.036	-0.077 to 0.005	-0.094	-0.256 to 0.067	-0.058	-0.146 to 0.029	0.023	-0.058 to 0.106
Haematocrit	0.015	-0.037 to 0.0689	-0.058	-0.188 to 0.071	0.05	-0.059 to 0.161	0.01	-0.091 to 0.112
Platelet values	0.035	-0.020 to 0.091	0.037	-0.126 to 0.202	0.04	-0.059 to 0 .139	0.083*	0.001 to 0.165

Notes: Data come the Constances cohort. All models control for age, age squared, gender, month of birth, birth cohort relative to the cut-off point, interacted with the treatment. Standard errors are clustered at the month of birth level. The bandwidth is fixed at 48 months.
**p*<0.05, ***p*<0.01, ****p*<0.001.
Remains significant after adjustment for multiple testing using Bonferroni-Holm, Sidak-Holm, and Westfall-Young corrections.

FIGURES

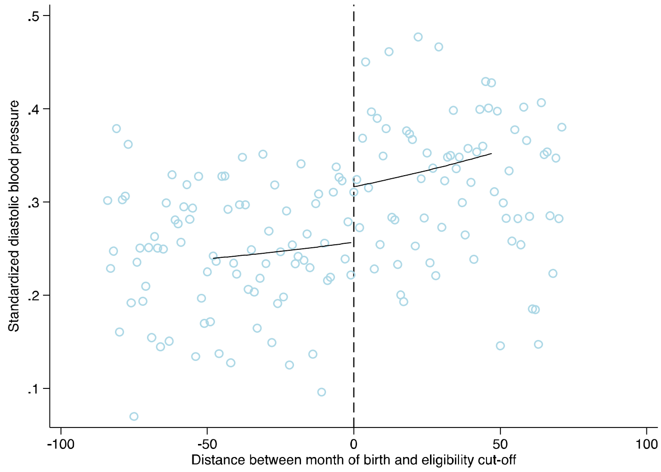
Figure 1. Impact of eligibility to the 1959 Berthoin reform on average school leaving age



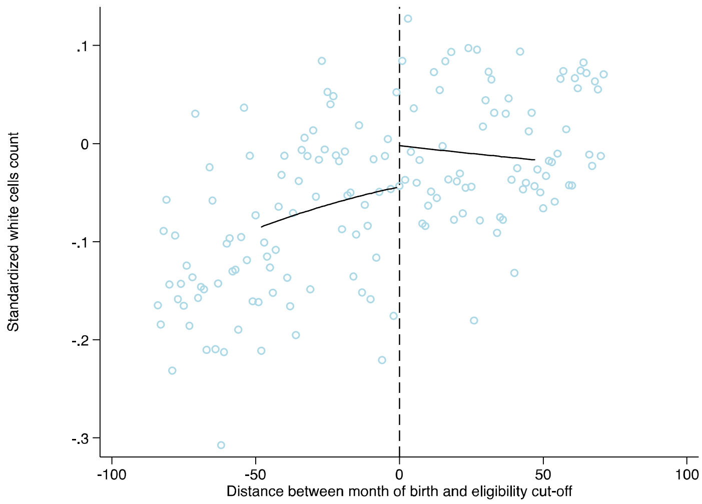
Notes: Data are from the French Labor Force Survey. The dashed line represents the first cohort eligible to the reform (born after the 1st of January 1953). Each dot represents the average school leaving age for a specific month of birth, which are included are the distance between a respondent's month of birth and the eligibility cut-off (e.g. -12 corresponds to the average school leaving age of respondents born in January 1952). The fitted lines represent the linear trends for our analytical sample: respondents born up to 48 months before or after the reform.

Figure 2. Impact of eligibility to the 1959 Berthoin reform on diastolic blood pressure and white cells count among respondents from low parental social class

Panel A. Diastolic blood pressure



Panel B. White blood cells count



Notes: Data are from the Constances cohort. The dashed line represents the first cohort eligible to the reform (born after the 1st of January 1953). Each dot represents respondents' average health outcome for a specific month of birth, which are included are the distance between a respondent's month of birth and the eligibility cut-off. The fitted lines represent the linear trends for our analytical sample: respondents born up to 48 months before or after the reform.

Research highlights

- We provide new evidence on the effect of compulsory schooling reforms on biomarkers
- Increased schooling duration was not associated with improved biological profiles
- It led to increased blood pressure and inflammation in respondents from poor families
- Trade-offs of schooling policies as tools to improve health need to be considered