Conscription and the returns to education: Evidence from a regression discontinuity

mouganie, pierre

Texas AM University

5 August 2014

Conscription and The Returns to Education: Evidence from a Regression Discontinuity

Pierre Mouganie*

Department of Economics, Texas A&M University

This Version: February, 2015

Abstract

In 1997, the French government put into effect a law that permanently exempted young French male citizens born after Jan 1, 1979 from mandatory military service while still requiring those born before that cutoff date to serve. This paper uses a regression discontinuity design to identify the effect of peacetime conscription on education and labor market outcomes. Results indicate that conscription eligibility induces a significant increase in years of education, which is consistent with conscription avoidance behavior. However, this increased education does not result in either an increase in graduation rates, or in employment and wages. Additional evidence shows conscription has no direct effect on earnings, suggesting that the returns to education induced by this policy was zero.

Keywords: Regression Discontinuity Design, Returns to Education, France, Conscription, Signaling

JEL Classification Numbers: I20, J24, J30

*Address: TAMU Department of Economics, 3035 Allen, College Station, TX 77840, USA, e-mail: *pmouganie@econ.tamu.edu*; I am especially grateful to Mark Hoekstra for his guidance and support. I am also thankful to Serena Canaan, Jose Gabriel Castillo, Jason Lindo, Eric Maurin, Jonathan Meer and Steven Puller for their invaluable comments and suggestions. Thanks also go to seminar participants at the Applied-Micro brown bag seminar at Texas A&M University and seminar participants at the American University of Beirut, for helpful comments and discussions. Finally, I would like to thank the people at the 'Centre Maurice Halbwachs' for assistance in providing me with the data used in this paper. All errors are my own.

1 Introduction

There has been considerable interest in analyzing the consequences of compulsory military service on a wide array of outcomes. These include earnings (Angrist 1990; Angrist & Krueger 1994; Angrist & Chen 2011), education (Card & Lemieux 2001), crime (Galiani et al. 2011; Lindo & Stoecker 2013) and health outcomes (Bedard & Deschenes 2006, Conley & Heerwig 2012). This is in part due to the general policy interest in understanding shocks that are expected to have large and persistent long run effects on outcomes. Indeed, conscripted individuals are obliged to serve in a crucial time of their lives, usually characterized by critical human capital investments. Moreover, the mechanics underlying these early adulthood shocks are themselves of considerable interest. For example, the military service and disruption caused by conscription may directly have long run effects on labor market outcomes. Alternatively, conscription may instead affect long run outcomes through its effect on educational attainment, since continuing one's education can typically allow one to defer mandatory military service. Finally, understanding the effects of compulsory military service has direct policy implications for countries that still have such policies in place.[1]

This paper focuses on the effects of a peacetime conscription shock on the education and labor market outcomes of young French males. Prior to 1997, all French male citizens had to undergo a 10 month compulsory military service at the age of 18. A lot of men preferred to defer their service, and the best way to do so was by staying enrolled in an educational institution. In 1996, the French government announced that people born after January 1, 1979 were no longer required to enlist, while those born prior to that were still required to do so. This enables me to use a regression discontinuity design framework that compares across cohorts that were barely subject to or exempted from mandatory military service. Since there is no reason to believe the outcomes of these groups would have been different absent this conscription policy, this research design allows me to overcome the selection bias

[1] In Europe: Austria, Denmark, Estonia, Finland, Greece, Norway, and Switzerland all still require their citizens to undergo mandatory military service.

in who is deemed physically and mentally fit for military service.

Results indicate that males born just before January 1, 1979 acquire on average half a year of education more than those born just after January 1, 1979, which is consistent with draft avoidance behavior. Specifically, this result is driven by individuals from high socioeconomic backgrounds. However, the additional years of education do not translate to degree completion; I find no discontinuity in percentage of degrees attained at the high school or college level. Strikingly, I also find no significant effect on employment or future earnings. The results are robust to different bandwidths and estimation procedures.

I then analyze several competing hypotheses as to why conscription would increase years of education but not earnings. On the one hand, if conscription has no direct effect on earnings, then my results imply zero returns to schooling. This could be the case if the average marginal return to education for those who were on the verge of dropping out of education is low, or because any positive returns to education in France may be mostly due to sheepskin effects and signaling, rather than human capital accumulation.[2] On the other hand, if we believe that conscription has a direct negative effect on earnings, reasonable assumptions regarding the magnitude of that effect from the existing literature, this results in returns to education estimates of 2.5 to 5 percent. Additional evidence provides support for the zero returns to education hypothesis.

In addressing the impacts of conscription on education and labor market outcomes, this paper joins a growing body of research on peacetime conscription. Imbens & Van der Klauw (1995) focus on the labor market effects brought about by peacetime conscription in the Netherlands and find a 5 percent negative return to earnings as a result of military service. They attribute this result to the corresponding loss of labor market experience. Galiani et. al (2011) uncover a negative earnings effect brought about by serving in the Argentinian military service. They conclude that this helps explain the documented increase in draftees' criminal behavior. On the other hand, Card & Cardoso (2012) find evidence of

[2]Of course, difference in ability to complete a degree may also reflect a difference in human capital acquisition.

positive military service effects on earnings. Specifically, they find a 4-5 percent increase in the wages of men with only primary education in Portugal. Cipollone & Rosolia (2007) focus on educational effects and find that exemption from service leads to a 3 percentage point reduction in high school graduation rates in Italy. Di Pietro (2009) also analyzes the educational effects of conscription in Italy by looking at university enrollment. The paper finds evidence of heterogeneous enrollment effects based on socioeconomic status. Grenet et al. (2011) and Bauer et al. (2012) examine the effect of peacetime military service on long run earnings of post World War II conscripts in England and Germany respectively. Both papers find no evidence of a long run earnings effect to conscription. Further, Grenet et al. (2011) uncover no evidence of draft avoidance behavior in terms of increased educational attainment in England, whereas Bauer et al. (2012) find that this behavior did exist in Germany.

This paper is most similar to Maurin & Xenogiani (2007) who examine the effect of conscription policy on both labor market and educational outcomes in France. They find that this policy leads to an increase in educational attainment, graduation rates and earnings. This study differs from Maurin & Xenogiani (2007) in several ways. First, I use the official birth date of January 1, 1979 as the cutoff for military service eligibility, whereas their study incorporates individuals born in 1978 in addition to 1979 in the treatment group. Second, I use a RD design which relies on less stringent assumptions as compared to a differences in difference model that compares across genders, which could be problematic to the extent that the educational decisions of men may affect those of women in a general equilibrium framework.[3] Third, I look at wages for individuals in their early 30s, which could potentially be more informative than entry wages examined in that paper. As a result, both papers reach different conclusions. More specifically, while we both uncover evidence of draft avoidance in terms of years of schooling, I find no evidence of a conscription effect on either graduation

[3]See, for example, the literature on gender classroom composition and educational outcomes (Hoxby (2000); Lavy & Schlosser (2011); Black et al. (2013).

rates or earnings, which contrasts significantly with their study.[4]

This paper makes several contributions relative to the existing literature. First, I present estimates that speak to the impact of conscription policy on both education and labor market outcomes. Particularly, I do so using a compelling research design that enables me to estimate the causal impact of conscription policy under assumptions that are less stringent than for many other research designs. This appears to be important, as this design yields no evidence of graduation effects or an earnings premium, which contrasts with previous work on the impact of conscription in France. In addition, because this study examines the impact of conscription using recent data on a relatively young cohort, it is particularly relevant for those countries in Western Europe that are considering changing their conscription policies.

This paper also adds to the much broader literature on education. Existing studies on the returns to education have mostly focused on exogenous variations in the supply of schooling and have generally found a positive returns to education (See Card (1999) for a survey of the literature). This paper focuses on a policy that induced variation in educational attainment through an increase in the demand for education. More specifically, conscription policy gave students an added incentive to pursue education—independent of the usual incentives to acquire more education. I find that the marginal student induced into acquiring education, due to conscription, does not seem to benefit from this added education. Specifically, while education has positive effects in general, we should still be wary of certain policies that induce individuals to attain more schooling than they would have had if it were up to them. This is consistent with recent findings by Pischke & Von Wachter (2008) and Grenet (2013) who document zero to little returns to education using variation from compulsory schooling laws in Germany and France respectively.

Section II presents more information on mandatory military service and subsequent re-

[4] I must acknowledge however that a direct comparison of both papers, specifically with respect to earnings estimates, could potentially be problematic. Differences in labor market outcome estimates across both studies may be attributed to conscription having a differential affect on early career wages for males in their early 20's versus longer term wages for those in their mid 30's. This is consistent with Angrist (1990); Angrist and Chen (2011) who initially finds a negative earnings impact to military service in the short run with these effects dissipating over time in the long run.

forms in France. Section III describes the data used in this paper. Section IV reviews the identification strategy. Section V presents the main empirical results as well as robustness checks. Section VI relates the findings to the returns to education literature. Section VII concludes.

2 Mandatory military service in France and subsequent reforms

France was the first modern state to introduce military conscription as a condition of citizenship during the French Revolution. It did so through the decree of the Jourdan Act of 1798 which stated "Any Frenchman is a soldier and owes himself to the defense of the nation." Conscription continued in various forms over the next 200 years until finally being phased out between 1996 and 2002. Individuals were called up for national service at age 18. In theory, one could postpone his service until the age of 22 without justification, though this was not usually done in practice. Instead, delaying conscription through acquiring extra education was the preferred route for several reasons. First, getting a full time job before conscription would require looking for a job twice. Second, more educated conscripts had access to higher responsibilities and milder forms of service. Further, staying on in education till the age of 26 allowed you to potentially avoid service all together. Finally, the abundance of low cost public schools, universities and technical/vocational institutions in France made it relatively easy to prolong education. In fact, Maurin & Xenogiani (2007) show that approximately two thirds of individuals enrolled in military service at time t, were still in school at t-1, whereas about 20% were unemployed and only 8% held permanent employment. They also show that the proportion of conscripts serving in the military aged 20 or older was 64%, with 90% of high school graduates serving after age 20.

International criticism over the performance of French deployed soldiers during the Gulf War caused the government to rethink their military service composition. This prompted a national wide debate over whether or not national defense should be left to the hands of professionals only. On February 22, 1996, President Jacques Chirac dramatically restructured the French army with the intent of having fully professional armed forces by 2002. A few months later, the president announced the end of conscription—the 1997 reform law granted national service exemption to men born after January 1, 1979. During the 1997-2002 transition period, men born before January 1979 were still required to perform 10 months of service. Compulsory military service was completely eliminated by a resolution adopted at the Cabinet meeting on June 27, 2001 which finally exempted those born before January 1, 1979 as well. The way these reforms were set up provides for an ideal quasi-experimental setup to test for the effects of conscription policy on education and future labor market outcomes.

Table 1 shows male conscription rates by birth cohort—taken from the *'Generation 1998 à 10 ans'* individual level survey. One limitation of this survey is that it only includes individuals with specific ties to the year 1998; whether these individuals are representative of France more broadly is an open question.[5] Table 1 indicates that around 50 percent of all males born between 1974 and 1976 eventually served in the military.[6] These numbers are consistent with those published by the French ministry of defense. They report that the number of conscripts during the early 1990's was stable at around 200,000 individuals per year implying that about 50% of a given birth cohort performed their national service duties.[7] The conscription rate drops to 37 and 27 percent for birth cohorts 1977 and 1978 respectively. This drop is consistent with the way conscription was abolished in France.

[5]The criteria for being included in Generation 1998 are as follows:
1) Enrollment in an educational institution in France in 1997-1998 or having left the education system in 1998. 2) No interruption in one's education exceeding a year (except for reasons of health, national service). 3) Having not returned to school during the year following entry into the labor market 4) Being at most 35 years old in the year 1998. 5) Being in France at the time of the survey.

[6]General exemptions were given to males with mental or health issues. Other possible exemptions include married men who had children and whose spouses had limited resources, family members of military martyrs, non-citizens as well as individuals with dual citizenship who had lived abroad for a significant portion of time.

[7]After the announcement of the military reform law in 1996 the number of individuals serving in the military gradually dropped from around 200,000 in 1996 to zero in 2002.

Individuals born in 1977 and 1978 were more likely to be exempt from service given that the system of conscription was fully abolished in August 2001.[8] Further, Table 1 affirms that the elimination of conscription induced sharp cross cohort variation in conscription rates around the January 1, 1979 serving threshold. Individuals born just before the cutoff were 27 percentage points more likely to have served in the military. Additionally, Table 1 also reveals that males from different socioeconomic backgrounds (S.E.S) were not serving at disproportionate levels. More specifically, conscription rates—averaged for cohorts 1974 to 1978—were similar with 45.2% of individuals from low S.E.S and 44.2% of individuals from high S.E.S serving in the military. These numbers drop to zero for individuals from both subgroups born after 1979. This alleviates concerns attributed to differential selection into conscription based on socioeconomic background.

3 Data and Sample Construction

The data used in this paper come from the 2011 French Labor Force Survey (LFS) conducted by the *Institut de la Statistique et des Études Économiques* (INSEE). I focus on the 2011 LFS as it offers the most up-to-date earnings data for this study. The LFS covers private households in metropolitan France. Participation in the survey is compulsory and has a high response rate of about 81%. As of 2003, a survey sample rotation of 6 quarters has been adapted. As a result, each household is interviewed for six consecutive quarters with the first and sixth quarters' interviews being conducted in person and the others done over the phone. The weighting factor applied to the survey is the population total (15 years or older) less the estimated number of people living in communities. The 2011 LFS surveys about 420,000 individuals with a sampling rate of about 1/150 of the total population. It provides information on individuals' month-year of birth, sex, educational level, occupational status, fathers' occupation, hours worked and wages.

[8] If these cohorts stayed on in education till the ages of 23 and 24 respectively, then they would be permanently exempt from serving. I must note however that people did not know this at the time, as the decision to permanently exempt all males in 2001 was not announced well beforehand.

From the original sample of 420,000 observations, I drop all females and non-French citizens. I further restrict the sample to people born within 48 months on either side of January 1, 1979. Descriptive statistics for the remaining 8,117 French male citizens born between 1975 and 1982 are reported in Table 2. Respondents have an average of 14.77 years of education, which corresponds to the number of years individuals are observed within educational institutions.[9] The average net monthly earnings for individuals in the sample (as of 2011) is 1681 Euros and the employment rate is 90.7%. Further, the average reported hourly wage is 12.27 Euros with an average workweek of 41 hours. The graduation rate for high school is 67%, and for college it is 24%.[10] For a detailed illustration of the available educational routes in the high school and higher education system, refer to Appendix Figures A7 and A8. Importantly, these figures highlight the fact that there were many different routes available to students wishing to extend their stay in either traditional or vocational schooling. Finally, I rely on fathers' occupation as a proxy for socioeconomic status (S.E.S) and find that 33% of individuals are classified as being from a high S.E.S. Particularly, occupation type is stratified into 355 different positions compromised of four digit identifiers in the LFS. This corresponds to the official French socioeconomic classification as represented by the *Nonmenclature des professions et categories socioprofessionelles (PCS)* and is used as reference in all official collective agreements. The first digit of each occupation makes it possible to identify the set of occupations that share the same position in the hierarchy of wages and education. More specifically, the first digit represents the four main skill levels used in official collective agreements.[11] Following Maurin & Thesmar (2004), my definition of high skilled workers includes the first two groups (a and b), while low skilled workers are

[9]Children in France usually start schooling at age 6. Further, there are different educational routes for schooling in France (professional, technical, academic) and this variable does not distinguish between the types.

[10]I include both the professional/technical and general baccalaureate in my graduating from high school variable. College graduation is defined as graduating with a licènse (the equivalent of a four year degree in the U.S.)

[11]The four main skill levels are as follows: a) cadres(mostly upper level managers, engineers and professionals); b) profession intermédiare (lower level managers and professionals, supervisors, and technicians); c) ouvriers et employés qualifiés (skilled manual and non-manual workers); d) ouvriers et employés non qualifiés (low skill manual and non-manual workers).

in the last two (c and d).

4 Identification Strategy

I use a standard regression discontinuity framework (Imbens & Lemieux, 2008; Lee & Lemieux, 2010) to estimate the effects of the abolishment of conscription on educational attainment as well as future earnings. The key assumption underlying an RD design is that the conditional expectation of the outcome variable with respect to birth cohort month-year (the running variable) is smooth through the January 1, 1979 birth cutoff. Intuitively, this means that all other determinants of outcomes must be smooth across the cutoff. This is likely to hold, as precisely manipulating one's date of birth in such a short time period is highly unlikely. Moreover, no other policy changes were occurring at the birth cohort threshold. To support these claims, I show that there is no evidence of bunching around the threshold, that observed determinants of education and earnings are smooth across the threshold and that treatment estimates do not significantly change with the addition of covariates. Accordingly, any discontinuity observed in 1979 can be attributed to the causal effect of being born before January 1, 1979, and thus being subject to the mandatory conscription policy. Formally, I estimate the following reduced form equation:

$$Y_i = \alpha + f(B_i) + \tau D_i + \Theta D_i * f(B_i) + \delta X_i + \epsilon_i$$

Where the dependent variable Y is the outcome of interest. D is a dummy variable indicating whether a person belongs to a pre-reform cohort or not (i.e. being born before January 1, 1979). B is an individual's month-year birth cohort measured in months relative to the cutoff date of Jan 1, 1979. The function f(.) captures the underlying relationship between the running variable and the dependent variable. Further, I allow the slopes of the fitted lines to differ on either side of the 1979 cutoff by interacting f(.) with the treatment dummy D. X is a vector of controls that should improve precision by reducing residual variation in the outcome variable, but should not significantly change the treatment estimate. The term

ϵ represents unobservable factors affecting outcomes. The parameter of interest is τ which gives us the treatment effect for each regression.

I use population survey weights in order to estimate the treatment parameter τ for the various outcomes.[12] Further, standard errors are clustered at the month-year level as suggested in Lee & Card (2008).[13] There are two ways to estimate the parameter τ. First, one can choose a parametric function for f(.) (eg. a quadratic or quartic polynomial in B) and use all the data available to estimate the equation above via ordinary least squares—typically referred to as the global polynomial approach. Alternatively, one can specify f(.) to be a linear function of B and estimate the equation over a narrower range of data, using a local linear regression. The latter approach can be viewed as generating estimates that are more local to the threshold and does not impose any strong functional assumptions on the data. While the preferred specifications in this paper are drawn from local linear regressions using uniform kernel weights, I still present results for a variety of bandwidths and functional forms, as has become standard in the RD literature (Lee & Lemieux, 2010).

In what follows, I rely on intent to treat (ITT) estimates and only report reduced form results. The reason I do so is twofold: First, the discontinuity in conscription rates at the cutoff does not really affect educational outcomes as it is eligibility for conscription rather than conscription in itself that drives those results. Second, re-weighing the reduced form estimates for labor market outcomes by the discontinuity in conscription rates in a Two Sample Two-Stage Least Squares framework (TS2SLS) relies on a tenuous exclusion restriction, seeing as conscription eligibility has a dual effect on education and earnings.

[12] Coefficients vary slightly when running un-weighted regressions, but significance decisions remain unchanged

[13] When the treatment determining covariate is discrete, reliance on functional form for estimation becomes more critical. Clustering at the level of the discrete running variable accounts for uncertainty in the choice of functional form for RD designs with discrete support.

5 Results

5.1 Education

I first examine the impact of mandatory conscription on years of education.[14] The results are shown in Panels A and B of Figure 1. These figures take the same form as those after them in that the open circles represent local averages over a 3 month period.

Panels A and B of Figure 1 depict a clear discontinuity in years of education for French male citizens around the Jan 1, 1979 threshold date. Panel A uses a population weighted quadratic fit with standard errors clustered at the month-year level. I use a bandwidth of 108 months on either side of the cutoff and estimate a significant discontinuity of 0.35 extra years of education attributed to being eligible for conscription. The trends in the global polynomial figures can be explained by the relative age of individuals in the sample. Relatively older cohorts to the far left of the cutoff systematically have less education.[15] Further, younger cohorts to the far right of the cutoff may still be in school. Panel B fits the data over a narrower bandwidth of 23 months using a local linear regression. Results indicate a significant 0.62 increase in years of education.[16] The optimal bandwidth for the local linear regression was selected using a robust data driven procedure (Henceforth CCT).[17] I also report local linear estimates over a wide array of bandwidths as to avoid over-reliance on a specific bandwidth choice, the results of which can be found in Panel C of Figure 1.

Corresponding regression estimates are shown in Table 3. All standard errors are clustered at the month-year level. Panel A depicts discontinuity estimates using different band-

[14]One may worry that the presence of too many people returning to education after military service would cause us to overstate any education effects. However, Maurin & Xenogiani (2005) show that only 4% of people who were in conscription at time t-1 pursued education at time t.

[15]The Labor force survey indicates that both males and females born during that period had lower educational attainment as compared to relatively younger generations

[16]The results from the global polynomial and local linear regressions are not statistically different from each other.

[17]As outlined in Calonico, Cattaneo & Titiunik (forthcoming), this bandwidth selector improves upon previous selectors that yield large bandwidths. Specifically, it accounts for bias-correction stemming from large initial bandwidth choice, while also correcting for the poor finite sample performance attributed to this bias correction. For instance, the LS-CV approach as outlined in Imbens & Lemieux (2008) gives an optimal bandwidth of 29 months.

widths and functional forms without the addition of controls. These estimates range from 0.41 to 0.62 years of extra education and are all statistically significant at the 5% level. The addition of controls does not significantly change the estimates, which is consistent with the identifying assumption. The controls used include birth month fixed effects, a binary variable indicating whether an individual was ever enrolled in middle school and father's occupation. Treatment estimates with controls are shown in Panel B and range from 0.35 to 0.55 years of education and are also statistically significant at the 5% level. In Figures A1 and A2 of the appendix, I account for the addition of these exogenous controls by plotting the residuals from years of education regressions. I conclude that conscription policy lead to a 6-month increase in education for young French males. These results are in line with a draft avoidance hypothesis (i.e. acquiring extra education to avoid military service).

Next, I examine whether this increase in years of education resulted in an increase in degree attainment, as measured by high school and college graduation rates.[18] Figures 2 and 3 show no evidence of a discontinuity in either college (0.0267) or high school (0.0034) graduation rates using a quartic fit over a bandwidth of 108 months. Table 4 depicts estimates for varying bandwidths and functional forms, with all results remaining statistically insignificant. In summary, I find that while mandatory conscription significantly increases years of education, it does not increase the likelihood of receiving a degree at the high school or college level.

5.2 Labor market outcomes

I now turn to whether this increase in education quantity for those barely subject to conscription yields positive labor market returns. First, I check to see if this policy generated any sharp changes in the likelihood of employment. All regressions in Figure 4 use a population weighted quadratic fit over 108 months of data on either side of the Jan. 1, 1979

[18]The French education system includes different types of high school degrees ranging from the professional/technical to the more traditional education route (The general baccalaureate). All these routes have been accounted for in the graduating from high school variable. I also check for discontinuities at the masters/doctorate level and find none.

cutoff. In Panel A, I find an insignificant 1.9 percentage point change in employment rates for those who were barely eligible for conscription.[19] Corresponding regression estimates are shown in Panel A of Table 5 with standard errors again clustered at the month-year level. The treatment estimates for likelihood of employment remain statistically insignificant over a wide range of bandwidths and functional forms. I conclude that being eligible for conscription does not seem to have significantly affected the likelihood of employment.

Next, I look at whether there are any significant effects on earnings for those barely subject to conscription. I begin by looking at monthly wages that include zero earners. Panel B of Figure 4 depicts an insignificant discontinuity in monthly earnings to the order of 51 Euros per month. Results remain unchanged when using logged earnings as the dependent variable and consequently dropping all zero earners from the sample. Panel C of Figure 4 depicts an insignificant 1.5 percentage point intensive margin change in monthly earnings. Finally, one may worry about whether monthly wages are not properly measuring effective salary dynamics due to behavioral responses in hours worked. I check for this by looking at any treatment effects on hourly wages. As can be seen from Panel D, I find no such effects and conclude that there are no significant changes in average earnings brought about by conscription. Corresponding regression estimates for all labor market outcomes are shown in Table 5. The results remain statistically insignificant over varying bandwidths and functional forms. I conclude that the conscription policy had no effect on labor market outcomes, despite the significant increase in years of education that it caused.

To assess conscription eligibility effects on the distribution of earnings as opposed to just average earnings, I also run quantile treatment effect regressions on monthly earnings for the whole population of interest. I estimate treatment effects on the probability of having earnings in each of 9 exhaustive groups. The quantile treatment effects suggest insignificant estimates at all deciles of the income distribution, which are consistent with the average treatment effects. Results are presented in Figure A3 of the appendix.

[19] I also re-estimate the global polynomial using a cubic and quartic fit and reach the same conclusion.

5.3 Heterogeneous treatment effects

While the above results show that conscription policy increases years of education on average, there are reasons to believe that some men should be more affected than others. For instance, individuals from wealthier backgrounds may find it easier to support themselves if they choose to prolong education. Thus, in order to complement the initial results, I look at treatment effects for individuals from high versus low socioeconomic backgrounds to assess if one was differentially affected by this policy.[20] Table 6 depicts results for global polynomial and local linear estimates. The bandwidth varies with each outcome variable when reporting the local linear estimates which are again chosen by the CCT bandwidth selector.

Results indicate that there is no significant change in added years of education for low S.E.S individuals, as can be seen in columns 1 and 3 of Table 6. In contrast, I find that high S.E.S individuals take on significantly more education. Specifically, in columns 5 and 7, I estimate a significant treatment effect to the order of 0.57 and 0.96 years of education, depending on the estimation procedure used. These results are consistent with a priori expectations, seeing as resources play an integral role in prolonging one's education.[21] However, treatment effects on high school and college graduation rates remain statistically insignificant for both subgroups. This suggests that even though individuals from more affluent backgrounds were taking on more education, this did not affect the average graduation rate of this subpopulation. As for labor market outcomes, there are no significant treatment effects on the likelihood of employment for either group. I also estimate a zero treatment effect for the two subgroups with respect to monthly, hourly, and logged wages. I conclude that mandatory military service has no effect on labor market outcomes for individuals from both spectra of the socioeconomic realm, even though it induced individuals from high S.E.S to take on more education.

[20]Differential selection into mandatory conscription by socioeconomic status could potentially bias these results. However, as shown in Table 1, I find no evidence of this.

[21]These costs can be direct (tuition, books, transportation, etc...) or indirect (opportunity cost of not being employed).

5.4 Robustness Checks

In this section, I test the reliability and validity of the identification strategy used. One advantage of a RD design is that there are several tests that allow us to indirectly test the plausibility of the research design. For instance, non-random sorting of individuals to either side of the January 1, 1979 birth cutoff would cause identification issues. Specifically, if individuals are strategically sorting to the right of the threshold in order to avoid conscription, and if this sorting is correlated with future outcomes, then the estimated treatment effects would be biased. However, manipulation of birth certificates would be an extremely hard thing to achieve in a developed nation such as France. Moreover, the fact that this policy was announced in 1996 makes it hard to believe that people would be able to manipulate birth documents in such a short period of time with such ease. Figure 5 depicts the distribution of observations in the sample, with no large mass evident to the right of the cutoff.[22]

Another informative visual test of manipulation involves testing for the smoothness of predetermined characteristics that are known to affect both earnings and education. The intuition here is that if we observe any discontinuity in exogenous characteristics, then this could be the result of strategic sorting by individuals or evidence of another policy occurring at the threshold. I test for this by examining where there is a documented discontinuity in fathers' occupation or in middle school enrollment rates.[23] Results depicted in Figures 6 and 7 confirm that there is no significant discontinuity in either of these baseline covariates. The robustness of these results to varying bandwidths and functional forms are presented in Table 7.

In addition, I perform several falsification exercises. I start by checking for discontinuities among females, who did not have to serve in the military. Specifically, I worry about there

[22]There is slight variation from month to month which is easily explained by the nature of the data (survey) and by the fact that data was trimmed by removing females, non-citizens, individuals living abroad, etc...

[23]Middle school enrollment rates should not be affected by the policy since those students are not yet eligible for conscription. Further, I do not focus on middle school graduation rates as that variable could potentially be endogenous to treatment in a dynamic choice model, seeing that passing the Brevet exam (middle school exit exam) or its equivalent is needed to stay enrolled in high school education and potentially delay one's service.

also being any positive treatment effects for females born just before the cutoff date. If males and females born just before Jan.1, 1979 both experience a significant and positive shock, then one may be concerned about some other policy driving the results. Panels A to F in Figure 8 confirm that there is no discontinuity in years of education, graduation rates, employment or wages for young French females. To investigate the robustness of these global polynomial estimates, I report all outcomes over varying bandwidths and functional forms in Table 8. All outcomes remain statistically insignificant except for high school graduation. High school graduation treatment effects yield a significant discontinuity in most specifications ranging from -4.8 to -8.2 percentage points. Even though these results are not robust to various bandwidths, I cannot reject the existence of a significant and negative treatment effect in the likelihood of graduating from high school for females born just before the cutoff. The two most plausible explanations for this effect are as follows: First, there may have been another exogenous shock—specific to the conscription birth date threshold—that negatively affected the high school graduation rate of both genders. This would cause us to potentially understate high school graduation effects for young males subject to conscription. To the best of my knowledge, no such policy relative to that specific birth cohort exists.[24] An alternative, and more likely scenario, is that the increase in female high school graduation rates for those born after the cutoff date may be the result of peer effects stemming from gender composition in the classroom. That is, having less males remain in educational capacities would automatically lead to an increase in the proportion of females in the classroom. This could ultimately influence the educational performance of females in the classroom. This is consistent with Black et al. (2013) who find that teenage schooling outcomes are influenced by the proportion of females in the grade, with effects differing by gender. These results also highlight a serious pitfall in using a difference-in-difference strategy, with females as the control group, when analyzing the effects of conscription.

Finally, I check for discontinuities at pseudo cutoff dates around the original threshold.

[24] I also check for discontinuities in high school graduation for both genders during the cutoff years of 1977, 1978 and 1980 and find no significant effects.

To do so I estimate treatment effects for 50 fake cutoff months on either side of the real threshold. This placebo test is implemented using years of education as the dependent variable since it is the only significant treatment effect found. Results indicate that the real cutoff date provides for the largest and most significant discontinuity. Figure 9 summarizes these findings by graphing t-statistics for these various placebo cutoff dates. The zero cutoff point represents the real threshold with all others being placebo estimates for months relative to the original being used as simulated treatment.[25] All significant estimates are highlighted in the graph with a large red filled circle. It is comforting to know that the most significant treatment effect occurs at the real cutoff month-year with a t-statistic of around 3. Further, I observe another 5 significant estimates out of the remaining 100 fake cutoff months.[26] These t-stats decrease the further we are from the real cutoff birth date. This provides further evidence that the significant treatment effect on years of education is in fact caused by conscription.

6 What does this mean for returns to education?

So far I have estimated reduced form effects for both education and labor market outcomes. I find evidence of a discontinuity in years of education, but not in the probability of getting a high school or college degree. Further, there are no significant effects on labor market outcomes. However, because conscription can have its own effect on labor market outcomes, interpreting these results as evidence for the lack of returns to education would only be true under certain assumptions. Below I present a simple conceptual framework that allows us to draw the distinction between "traditional" and "conscription confounded" returns to education estimates.

To simplify, let us assume that birth date (B) is the only factor that determines eligibility

[25] Each month represents a local linear regression of bandwidth = 24 months using years of education as the dependent variable.

[26] This is consistent with a Type-1 error of 5%

(e) and that e and conscription (C) are continuous variables over the real line.[27] Further, assume that schooling (S = S(e)) is an increasing function of eligibility.[28] Finally, assume that wage (W = W(S(e), C(e)) is a function of schooling and conscription.

By a simple application of the chain rule, the reduced form effect of conscription eligibility on wages can be decomposed into a schooling effect and a conscription effect:

$$\frac{dW}{de} = \frac{\partial W}{\partial S} * \frac{dS}{de} + \frac{\partial W}{\partial C} * \frac{dC}{de} \tag{1}$$

Rearranging (1):

$$ReturnstoSchooling = \frac{\partial W}{\partial S} = \frac{\left(\frac{dW}{de} - \frac{\partial W}{\partial C} * \frac{dC}{de}\right)}{\frac{dS}{de}} \tag{2}$$

Consistent with the previous literature on the topic, I focus on two general cases. Conscription can either have a zero or negative effect on wages.[29]

Case1 : $\mathbf{\frac{\partial W}{\partial C} = 0}$

Assuming that serving in the military (C) has no direct effect on wages, equation (2) reduces to a traditional returns to education interpretation and is just the ratio of reduced form estimates for wages and years of education. Put differently, this is equivalent to using date of birth as an instrument for education to estimate a local average treatment effect (LATE) for the returns to education. From the results section we know that $\frac{dW}{de} = 0$ indicating that the results point to no labor market returns for half a year of education. This result is consistent with findings by Pischke & Von Wachter (2008) and Grenet (2013) who document zero to minor returns to education using variation from compulsory schooling laws in Germany and France respectively. The former study concludes that the skills most relevant

[27]For ease of computation, think of conscription and eligibility as probabilities on a continuous scale from 0 to 1 as opposed to a binary variable.

[28]The assumption imposes the constraint of conscription in itself having no direct effect on education, which is a reasonable one to make seeing that only 4 % of students ever return to education after having served.

[29]In fact, almost all French individuals interviewed in a recent study agreed that there were no advantages to be gained from including military service experience on your curriculum vitae (Jorgensen & Breitenbauch (2009).

for the labor market are learned early on in Germany. On the other hand, Grenet (2013) provide an explanation based on academic credentials. Particularly, they document that countries such as England and Wales witnessed a surge in graduation rates after increasing the age of compulsory schooling, whereas no such effect was documented in France. They hypothesize that policies that do not induce an uptake in academic credentials may not necessarily result in positive returns to education.

One explanation for the lack of educational returns in my study is that the average marginal return to education for those who were on the verge of dropping out of education is low. That is, the type of education induced by draft avoidance is of a lower quality than typical education or the marginal student induced into education exerts less effort. Alternatively, since this increase in years of education for the eligible group is not accompanied by a significant increase in college or high school graduation rates, then perhaps the results could be attributed to the value of signaling in education. The latter scenario is consistent with Grenet (2013) and suggests that most of the returns to education for the population of interest seem to be stemming from sheepskin effects (signaling) as opposed to human capital acquisition in the form of additional years of education.

Case2 : $\frac{\partial \mathbf{W}}{\partial \mathbf{C}} < \mathbf{0}$

Assuming that conscription has a negative effect on wages, then equation (2) no longer simplifies to a traditional returns to education interpretation. However, a bound for plausible returns to education estimates can be derived from equation (2). We know that $\frac{d\hat{W}}{de} = 0$ and $\frac{d\hat{S}}{de} = 0.5$. The compliance ratio can be estimated at $\frac{d\hat{C}}{de} \approx 50\%$.[30] Further, previous returns to conscription literature, that have found negative effects to conscription, point to a $\frac{\partial W}{\partial C} \approx 2.5\% - 5\%$ negative returns to conscription. Plugging these estimates into equation (2), I estimate a bound to the order of $\frac{\partial W}{\partial S} \approx 2.5\% - 5\%$ returns to an additional year of

[30] Some may question why this number is at odds with the conscription rates for the years 1978 and 1979—reported in Table 1. As mentioned in section 2, one of the criteria for inclusion in the *Generation 1998 à 10 ans* survey is being in an educational institution in 1998. Thus, people who were not in education in 1998 or who had not left education by 1998 are excluded from the sample. These individuals likely have high service rates, which means Table 1 potentially underestimates conscription rates for some cohorts. As a result, we rely on a less conservative estimate of 50% at the cutoff.

education for the population of interest.[31]

In order to shed light on which of these possible interpretations is more likely to be true, I look at two subgroups whose education decisions are exogenous to treatment. As highlighted in section 5.4, there is no threshold crossing effect on education for individuals from low socioeconomic backgrounds. On the other hand, I observe a treatment effect on years of education for individuals coming from high socioeconomic backgrounds. However, both these groups experience no significant labor market treatment effects. As a result, and under the assumption of homogeneous conscription effects, serving does not seem to be directly affecting labor market outcomes.[32]

To complement these results, I also look at another subpopulation whose education is orthogonal to treatment but who are ultimately still eligible for conscription. Specifically, I restrict the data to people who have only completed primary schooling (i.e up to grade 6). These individuals' decision to not remain in school should be independent of conscription policy. Figure A4 confirms that there is no discontinuity in years of education for this subgroup. As a result, any discontinuity in earnings must be due to the effect of conscription itself, rather than to the combination of military service and increased education. Results are shown in Figure A5, which shows no effect on earnings. While these results are more suggestive than conclusive given I am focusing on a very specific subgroup of individuals, they reinforce the conclusion that service has no direct effect on earnings.

Additionally, the main hypothesized channel—in the conscription literature—through which military service affects earnings is through a loss of early labor market experience (Angrist, 1990; Imbens and Van Der Klauw, 1995). Figure A6 indicates that conscription policy does not induce a loss of early job market experience despite it leading to an increase in years of education. This finding is consistent with the proposed evidence suggesting zero

[31]This bound is calculated assuming a compliance ratio of 50%. Assuming a more conservative 25% compliance ratio at the cutoff, the bound becomes 1.25%-2.5% returns to a year of education.

[32]If individuals from high S.E.S reap some direct economic benefit from conscription relative to low S.E.S individuals, then this conclusion may not hold. However, the only cases in the literature where conscription has shown to have a positive impact on earnings were for individuals from less educated and/or disadvantaged backgrounds (Berger and Hirsch 1983; de Tray 1982).

returns to military service for my population of interest. Further, this also rules out the possibility that decreased job market experience is negating any potential positive earnings stemming from the documented increase in education. Altogether, my results suggest that the only potential effect conscription policy could have on earnings is through its effect on education. I conclude that the increased years of education observed in the original sample do not yield positive labor market returns. As stated earlier, this could be because the average marginal return of this type of education induced by conscription policy does not increase human capital, or because the returns to education work primarily through signaling rather than human capital accumulation.

7 Conclusion

This paper contributes to the returns to conscription literature by exploiting the abolition of mandatory military service in France in order to identify the effects of conscription eligibility on education and future earnings. I use a regression discontinuity design to overcome selection bias attributed to serving in the military. I find that those barely eligible for conscription obtain approximately 6 additional months of education as a result. Specifically, I find that this result is mainly driven by individuals from high socioeconomic backgrounds. However, there are no discontinuities in high school or college graduation rates, which suggests that the individuals do not use the additional time in school to finish degrees. Furthermore, I find no effects on employment rates, monthly, hourly or logged earnings. These results are robust to varying bandwidths and functional forms.

I also contribute to the returns to education literature by presenting several hypotheses that could plausibly be driving the above results. If we assume no direct returns to serving in the military, then the ratio of the two reduced form effects yield a zero estimate for returns to education. On the other hand, if we were to assume negative returns to conscription, then the reduced form results do not simplify to a traditional returns to education interpretation.

Back of the envelope calculations result in a bound of 2.5%- 5% returns to one year of education. Under the assumption of homogeneous returns to conscription, I provide evidence to the lack of any direct returns to conscription. This leans support to the zero returns to education hypothesis. No returns to schooling could be interpreted in several ways. First, the education induced by draft avoidance could be of lower quality than more typical education. Alternatively, since there is no increase in high school or college graduation rates, then the results could be driven by signaling and sheepskin effects.

These results have important implications. First, while it remains an open question as to the extent to which these results would apply in other contexts, these findings could provide some evidence on the potential effects of mandatory military service in European countries that have yet to abolish such policies. Finally, we should be wary about the possible consequences of certain policies that induce individuals to attain more schooling than they would have had if it were up to them. In fact, a good portion of these individuals could probably be better served acquiring early labor market experience. More specifically, while the returns to education for non draft related reasons may still be positive, policies that aim to increase educational attainment without targeting the necessary complements of education need to be looked at closer.

References

Angrist, J.D., 1990. Lifetime earnings and the Vietnam era draft lottery: Evidence from Social Security administrative records. *American Economic Review* 80(3), 313-336.

Angrist, J.D., Chen, S.H., 2011. Schooling and the Vietnam-Era GI Bill: Evidence from the Draft Lottery. *American Economic Journal: Applied Economics* 3(2), 96-118.

Angrist, J.D., Krueger, A.B., 1994. Why do World War II veterans earn more than nonveterans. *Journal of Labor Economics.* 12 (1), 74-97.

Angrist, J.D., Imbens, G.W, 1994. Identification and estimation of local average treatment effects. *Econometrica* 62(2), 467-475.

Bauer, T.K., Bender S., Paloyo A.R, Schmidt C.M., 2011. Do guns displace books? The impact of compulsory military service on educational attainment. *Ruhr Economic Papers 0260, Rheinisch-Westflisches Institut fr Wirtschaftsforschung, Ruhr-Universitt Bochum, Universitt Dortmund, Universitt Duisburg-Essen.*

Bauer, T.K., Bender S., Paloyo A.R, Schmidt C.M., 2012. Evaluating the labor-market effects of compulsory military service. *European Economic Review* 56. 814-829.

Bedard, K., Deschenes O., 2006. The Long-Term Impact of Military Service on Health: Evidence from World War II and Korea Veterans. *American Economic Review* 96(1), 176-194

Berger, M, C., Hirsch B, T., 1983. The Civilian Earnings Experience of Vietnam Era Veterans. *Journal of Human Resources.* 18(4), 455-79

Black, E. S., Devreux J. P., Salvanes, G. K., 2013. Under Pressure? The Effect of Peers on Outcomes of Young adults. *Journal of Labor Economics.* 31(1), 119-153.

Breitenbauch, H., Jorgensen H., 2009. What If We Gave Up Conscription. *Dansk Institut for Militaere Studier.* February, 2009

Calonico, S., Cattaneo, M,D., Titiunik, R., forthcoming. Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica.*

Card, D., 1999. The causal effect of education on earnings. *Handbook of Labor Economics, Chapter 30 Volume 3, Edited by O.Aschenfelter and D.Card.*

Card, D., Cardoso, A. R., 2012. Can Compulsory Military Service Raise Civilian Wages? Evidence from the Peacetime Draft in Portugal. *American Economic Journal: Applied Economics.* 4(4) 57-93.

Card, D., Lemieux, T., 2001. Going to college to avoid the draft: The unintended legacy of the vietnam war. *American Economic Review.* 91 (2), 97-102.

Carneiro, P., Heckman, J., and Vytlacil, E., 2011. Estimating Marginal Returns to Education. *American Economic Review.* 101 (6), 2754-81.

Cipollone, P., Rosolia,A., 2007. Social Interactions in High School: Lessons from an Earthquake. *American Economic Review* 97 (3), 948-965.

Conley, D., Heerwig, J., 2012. The long-term effects of military conscription on mortality: Estimates from the Veitnam-era draft lottery. *Demography* 49, 841-855.

De Tray, D., 1982. Veterans Status as a Screening Device. *American Economic Review* 72(1) 133-42.

Di Pietro, G., 2013. Military conscription and university enrollment: Evidence from Italy. *Journal of Population Economics* 26 (2), 619-644.

Enquete de emploi 2011 et Generation 1998 à 10 ans. [fichier electronique], INSEE [producteur], Centre Maurice Hallbwachs (CMH) [diffuseur].

Galiani, S., Rossi M.A., Schargrodsky, E., 2011. Conscription and Crime: Evidence from the Argentine Draft Lottery. *American Economic Journal: Applied Economics.* 3 (2), 119-136.

Grenet, J., Hart R.A, Roberts, J.E., 2011. Above and Beyond the Call. Long-term real earnings effects of British male military conscription in the post-war years. *Labour economics.* 18 (2011), 194-204.

Grenet, J., 2013. Is extending compulsory schooling alone enough to raise earnings? Evidence from the French and British compulsory schooling laws. *Scandinavian Journal of Economics* 115 (1) 176-210.

Herpin, N., Mansuy, M., 1995. Le role du service national dans 1' insertion des Jeunes. *Economie et Statistique* 283, 85-91.

Hoxby, C., 2000. Peer Effects in the Classroom: Learning from Gender and Race Variation. *National Bureau of Economic Research Working Paper 7867.*

Imbens, G.W., Lemiuex, T., 2008. Regression discontinuity designs: A guide to practice. *Journal of Econometrics* 142 (2), 615-35.

Imbens, G.W., Van Der Klauw, W., 1995. Evaluating the cost of conscription in the Netherlands. *Journal of Business & Economic Statistics* 13 (2), 207-215.

Irondelle, B., 2003. Civil-Military Relations and the End of Conscription in France. *Security Studies* 12 (3), 157-187.

Lavy, V., Schlosser A., 2011. Mechanisms and Impacts of Gender Peer Effects at School. *American Economic Journal: Applied Economics* 3 (2), 1-33.

Lee, D.S, Card, D. 2008. Regression discontinuity inference with specification error. *Journal of Econometrics* 142, 655-674.

Lee, D.S., Lemiuex, T., 2010. Regression doscontinuity designs in economics. *Journal of Economic Literature* 48 (2), 281-355.

Lindo, J.M., Stoecker, C., Forthcoming. Drawn into violence: Evidence on "what makes a criminal" from the Vietnam draft lotteries. *Economic Inquiry* 52 (1) 239-258.

Maurin, E., 2005. The French educational system: Issues and debates. *German Economic Review, Invited Paper, Special Issue on Economics of Education,* 6(3), 297-309.

Maurin, E., Thesmar, D., 2004. Changes in the functional structure of firms and the demand for skill. *Journal of Labor Economics* 22(3).

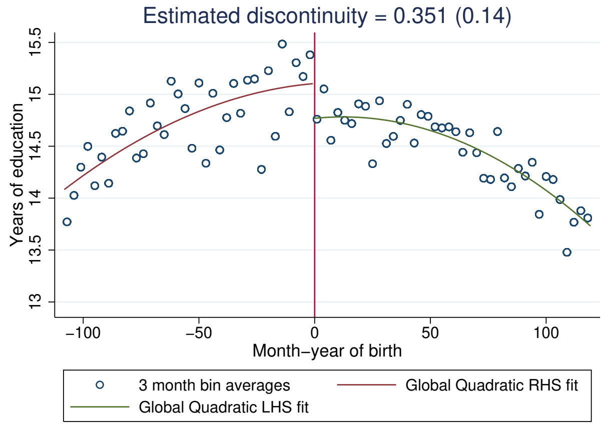
Maurin, E., Gurgand, M., 2007. A large scale experiment: Wages and educational expenses in France. *Paris School of Economics working papers*. No. 2007 -21.

Maurin, E., Xenogiani, T., 2007. Demand for education and labor outcomes: Lessons from the abolition of compulsory conscription in France. *Journal of Human Resources* 42 (4), 795-819.

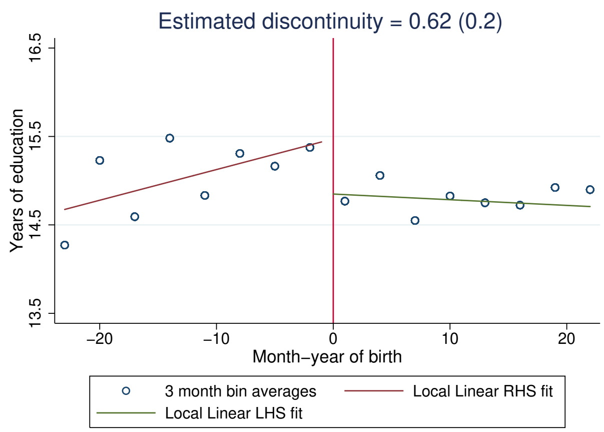
Pischke, J.S, Von Wachter, T., 2008. Zero returns to compulsory schooling in Germany: Evidence and interpretation. *The Review of Economics and Statistics* 90 (3) 592-598.

Spence, M., 1973. Job market signalling. *Quarterly Journal of Economics* 87 (3), 355-374.

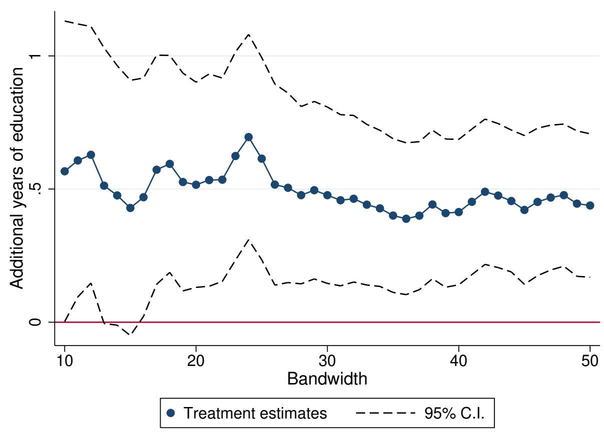
A Figures



(a) Global quadratic Graph



(b) Local Linear Graph



(c) Bandwidth sensitivity analysis for local linear regression

Figure 1: Years of education estimates based on birth month relative to Jan 1, 1979 cutoff.

- Notes: All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses. Uniform Kernel weights used for local linear regressions. For figure 1C: solid and dashed lines represents the point estimates and 95% confidence intervals respectively.

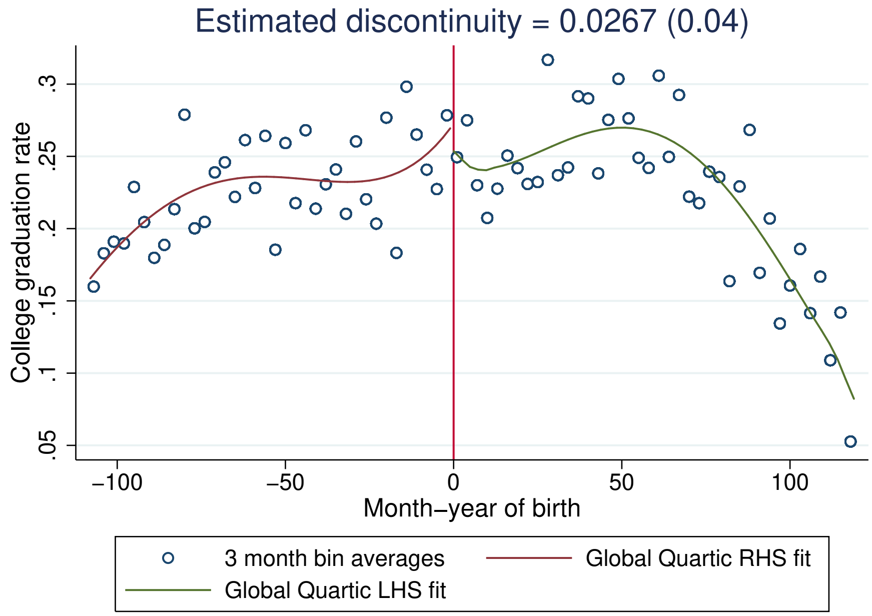


Figure 2: College graduation rates based on birth month relative to Jan 1, 1979 cutoff (Global Quartic graph).

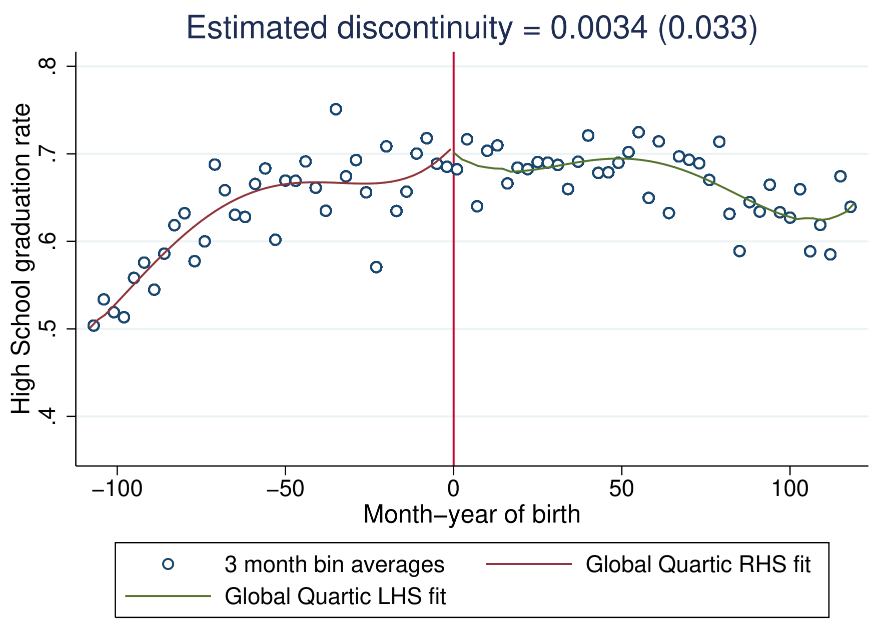
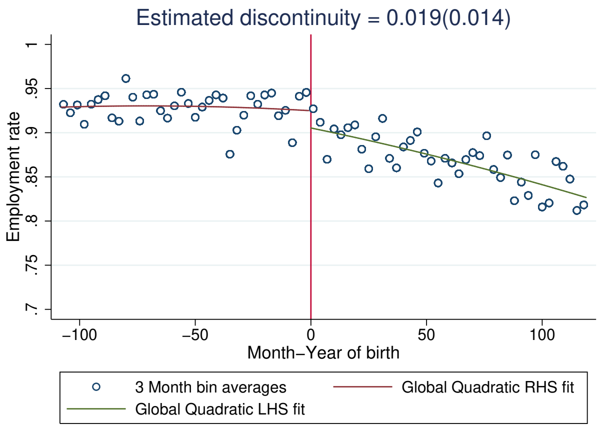
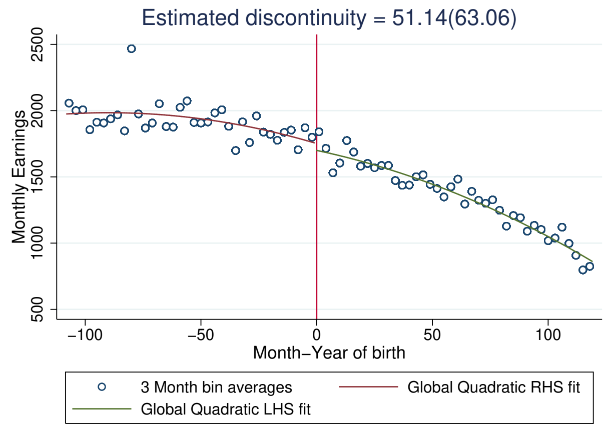


Figure 3: High school graduation rates based on birth month relative to Jan 1, 1979 cutoff (Global Quartic graph).

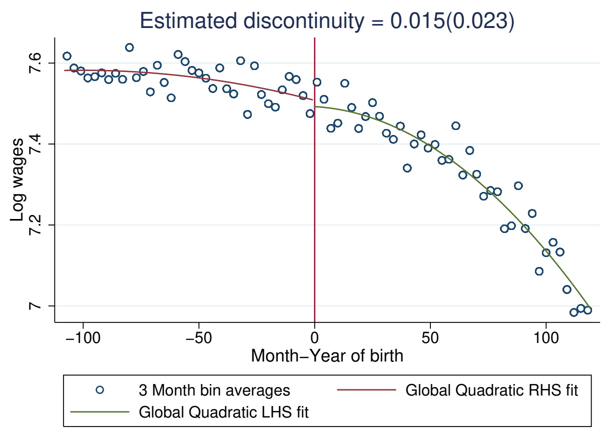
- Notes: High school graduation includes all forms of baccalaureate degrees (technical, professional, general). Sample includes French male citizens. All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses.



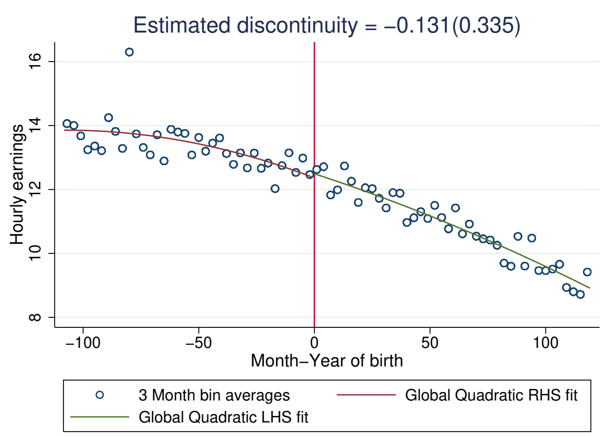
(a) Employment Rates



(b) Monthly Wages



(c) Log Monthly Wages



(d) Hourly Wages

Figure 4: Labor market outcomes based on birth month relative to Jan 1, 1979 cutoff (Global Quadratic graphs).

Notes: Sample includes employed and unemployed French male citizens with at least 10 hours of weekly work (conditional on employment). All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses.

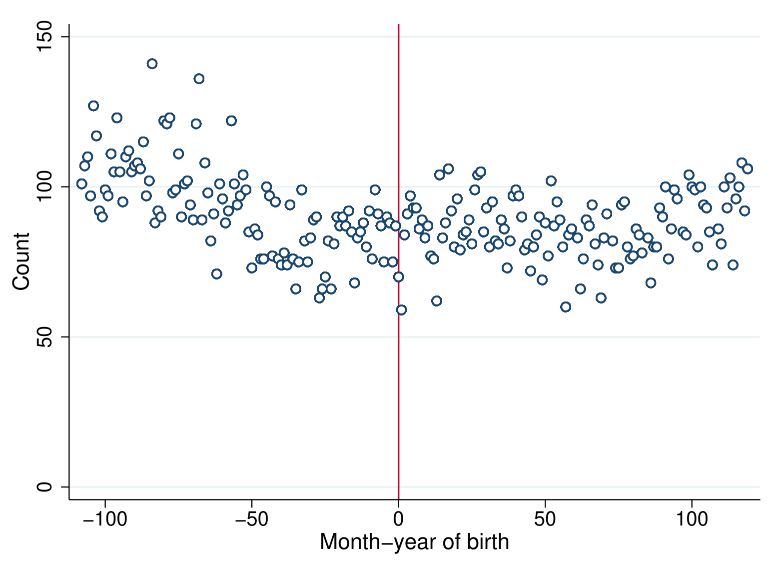


Figure 5: Distribution of birth month-year (running variable) near Jan 1, 1979 birth cutoff.

Notes: Monthly bins depict number of individuals born in each month-year. Sample includes French male citizens (employed +unemployed).

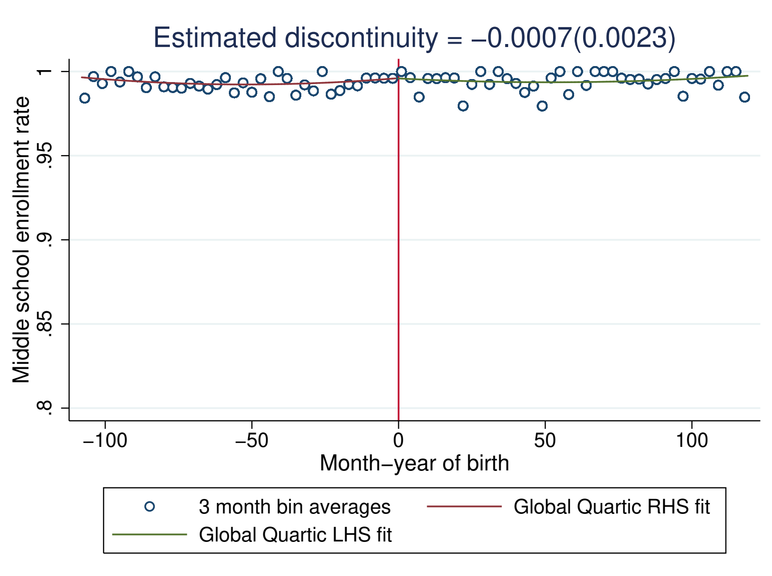


Figure 6: Testing for the smoothness of predetermined characteristics (Middle school enrollment) around cutoff using a global quartic graph.

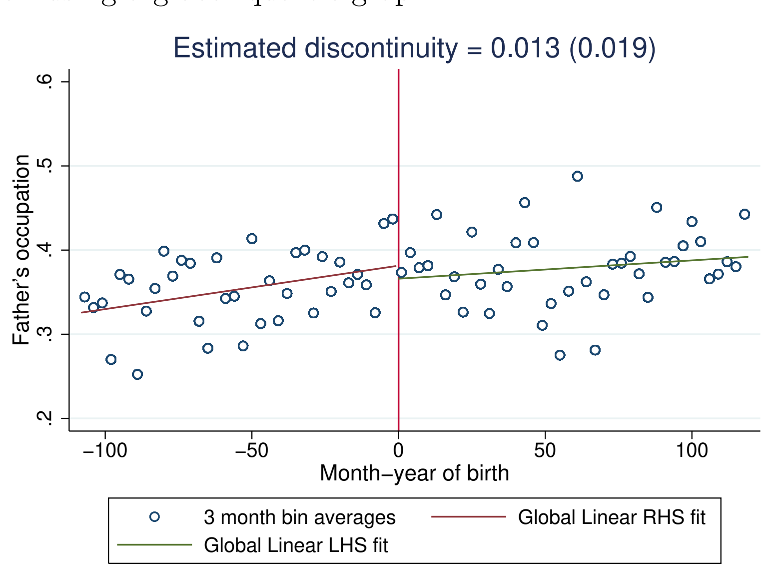
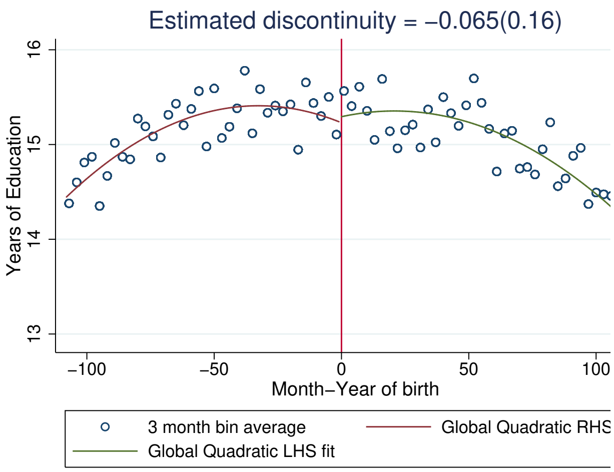
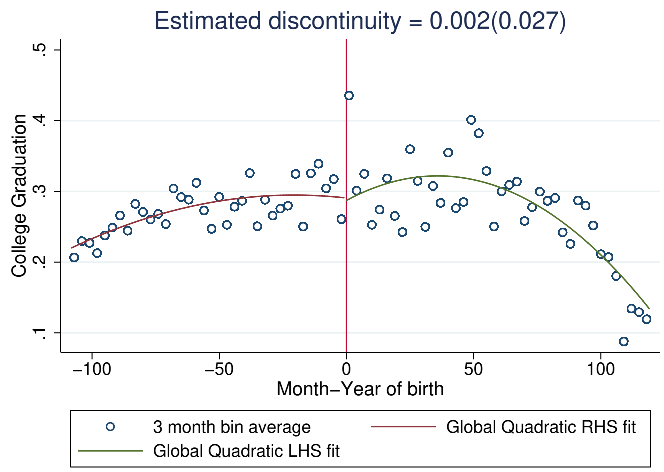


Figure 7: Testing for the smoothness of predetermined characteristics (Father's occupation) around cutoff using a global linear graph.

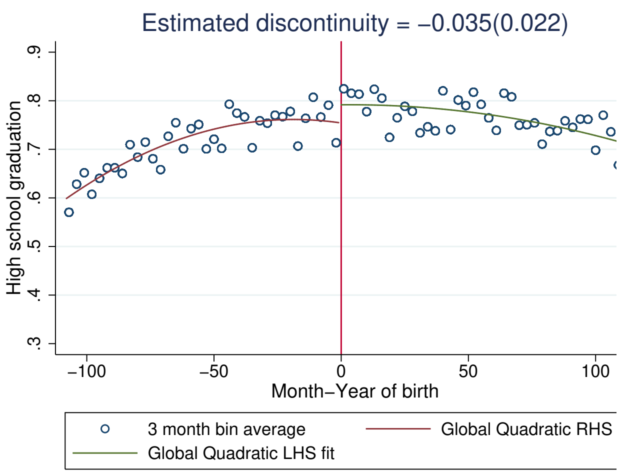
Notes: Sample includes French male citizens. All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses.



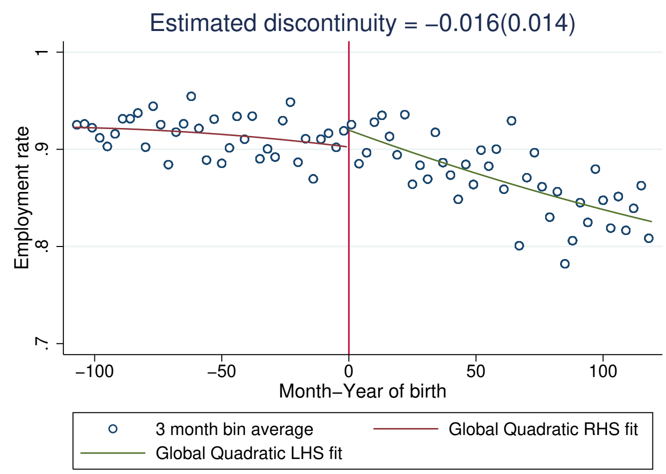
(a) Female years of educ.



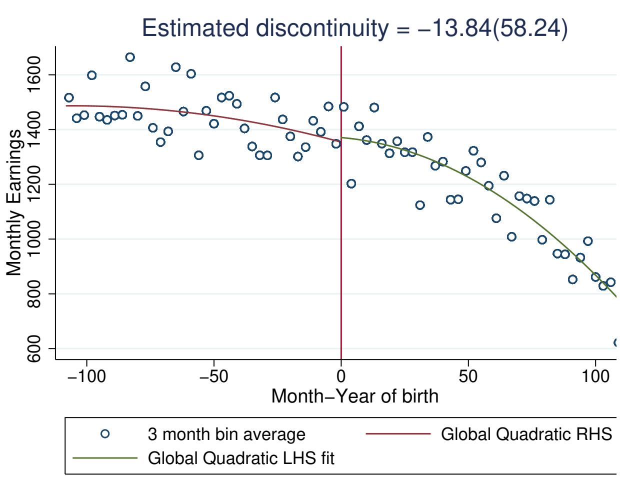
(b) Female college grad.



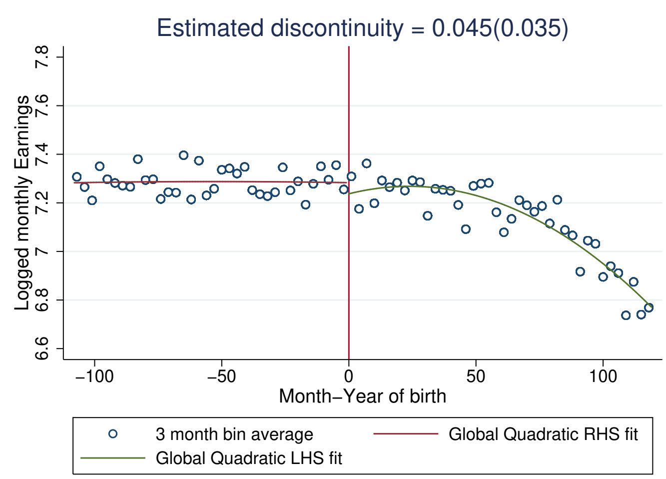
(c) Female high school grad.



(d) Female employment rates



(e) Female monthly earnings



(f) Female logged earnings

Figure 8: Female outcomes based on birth-year month relative to Jan 1, 1979 cutoff (Global Quadratic graphs).

Notes: Sample includes employed and unemployed French female citizens with at least 10 hours of weekly work (conditional on employment). Sample weighted by population. Clustered Standard errors reported in parentheses.

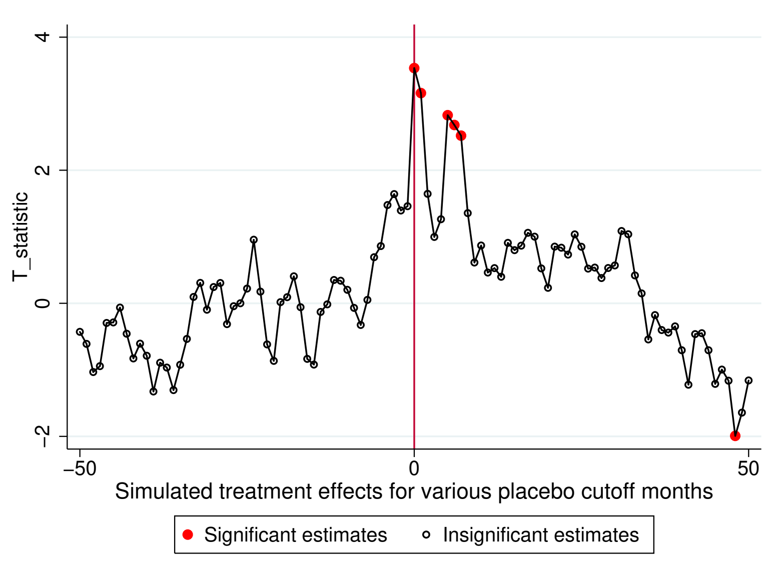


Figure 9: Placebo test - T-statistics for reduced form effects on years of education using various fake cutoff dates.

Notes: Sample includes French male citizens. Each open circle represents the t-statistic from a local linear regression of bandwidth = 24 months using years of education as the dependent variable. Month Zero is the 'real' cutoff date, and I simulate 50 fake cutoff months to the right and left of that point. Clustered standard errors used to compute all t-stats.

B Tables

Table 1: Male conscription rates based on birth year

Birth cohort year	All Males	Low S.E.S	High S.E.S	Number of Observations
1974	0.531	0.545	0.528	452
1975	0.485	0.525	0.454	538
1976	0.556	0.559	0.567	652
1977	0.376	0.397	0.361	789
1978	0.271	0.268	0.300	849
1979	0	0	0	711
1980	0	0	0	572

Source: *Generation 1998 à 10 ans*
Socioeconomic Status (S.E.S) determined by father's occupation and based on official French classification of jobs.

Table 2: Summary statistics for French male citizens of birth cohorts 1975 to 1982 as of 2011

Variable	**Mean**
Years of Education	14.77 (3.036)
Monthly Earnings (in Euros)	1682.1 (1057.9)
Log Monthly Earnings	7.481 (0.427)
Hourly Wage (in Euros)	12.27 (4.891)
Employment rate	0.907 (0.290)
Hours Worked per week	40.88 (10.10)
High S.E.S	0.331 (0.46)
Middle school enrollment	0.993 (0.0819)
High School graduation rate	0.668 (0.471)
University graduation rate	0.240 (0.427)
Observations	8117

Mean outcomes reported; Standard deviation in parentheses

*Data is taken from the French Labor Force Survey 2011 for male birth cohorts 1975 to 1982.

-Since the dataset is not balanced, the number of observations used to compute each variable does not necessarily match with the number of observations reported.

*Father's occupation is recoded as a dummy variable where 1 denotes higher skilled jobs and 0 denotes manual labor/ lower skilled jobs.

Table 3: Regression discontinuity estimates for years of education using different bandwidths and specifications.

Bandwidth	8 months	20 months	23 months	30 months	40 months	40 months	60 months	80 months	Global
	1	2	3	4	5	6	7	8	9
Panel A: no controls									
Discontinuity	.505***	.516**	.624***	.477***	.414***	.575**	.539***	.593***	.581***
	(.16)	(.19)	(.20)	(.17)	(.14)	(.22)	(.17)	(.20)	(.22)
Panel B: Controls	.461**	.450**	.548***	.362**	.357**	.488**	.397**	.497**	.478**
	(.16)	(.17)	(.17)	(.14)	(.15)	(.21)	(.17)	(.21)	(.23)
Observations (No Controls)	1340	3341	3822	4999	6622	6622	10051	13446	18975
Observations (+Controls)	1047	2556	2931	3836	5067	5067	7700	10300	14486
Month Polynomial	Zero	One	One	One	One	Two	Two	Three	Four

Notes: Sample includes French male citizens.
Each cell represents a separate regression with years of education as the dependent variable and the treatment variable 'born before January 1, 1979'.
All specifications control for a flexible polynomial of age in which the slope is allowed to vary on either side of the cutoff.
Standard errors are clustered at the month-year level and reported in parentheses. All regressions have been weighted by population.
Our preferred specification is the one using bandwidth of 23 months which has been computed using the method proposed in Calonico et. al (2013).
Controls include: 1) Birth month fixed effects 2) A binary variable for middle school enrollment 3) father's occupation.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 4: Regression discontinuity estimates for college/high school graduation rates using different bandwidths and specifications.

Bandwidth	4 months	8 months	12 months	20 months	30 months	40 months	50 months	60 months	Global
	1	2	3	4	5	6	7	8	9
Panel A: Discontinuity									
in college grad.	.008	−.005	.014	.007	.028	.021	.001	.010	.027
	(.04)	(.03)	(.02)	(.04)	(.03)	(.04)	(.05)	(.04)	(.04)
Panel B: Discontinuity									
in high school grad.	−.016	.008	.013	.006	.012	.012	.007	.005	.003
	(.03)	(.03)	(.02)	(.03)	(.03)	(.03)	(.04)	(.04)	(.03)
Month Polynomial	Zero	Zero	Zero	One	One	Two	Three	Three	four
Observations	633	1332	1991	3334	4992	6616	8254	10036	18985

Notes: Sample includes French male citizens.
High school graduation defined as finishing any Baccalaureate type (Professional, technical, general)
Each cell represents a separate regression with graduation rates as the dependent variables and the treatment variable being 'born before January 1, 1979'.
All specifications control for a flexible polynomial of age in which the slope is allowed to vary on either side of the cutoff.
Standard errors are clustered at the month-year level and reported in parentheses. All regressions have been weighted by population.
*** p <0.01 ** p <0.05 * p <0.1

Table 5: Regression discontinuity estimates for labor market outcomes using different bandwidths and specifications.

Bandwidth	6 months	14 months	20 months	30 months	40 months	50 months	60 months	Global
	1	2	3	4	5	6	7	8
Panel A: Discontinuity in employment.	.024	.033	.012	.016	.026	.019	.027	.019
	(.014)	(.021)	(.021)	(.017)	(.021)	(.019)	(.019)	(.014)
Panel B: Discontinuity in monthly earnings.	66.240	124.720	63.256	68.971	111.583	66.293	70.013	51.143
	(84.070)	(119.271)	(89.762)	(74.086)	(95.246)	(83.977)	(80.078)	(63.066)
Panel C: Discontinuity in log wages.	−.032	−.014	.004	.016	.010	−.001	.000	.015
	(.027)	(.043)	(.033)	(.029)	(.036)	(.032)	(.030)	(.024)
Panel D: Discontinuity in hourly earnings.	.053	.381	.245	.106	.244	.208	.091	−.132
	(.479)	(.637)	(.481)	(.396)	(.493)	(.444)	(.415)	(.335)
Month Polynomial	Zero	One	One	One	Two	Two	Two	Two
Observations	700	1649	2386	3601	4755	5956	7231	13779

Notes: Sample includes French male citizens.
Wages include zero earners, but logged wages drops those unemployed. Hourly wages are conditional on at least 10 weekly hours.
Each cell represents a separate regression with different labor market outcome dependent variables and the treatment variable being 'born before January 1, 1979'.
All specifications control for a flexible polynomial of age in which the slope is allowed to vary on either side of the cutoff.
Standard errors are clustered at the month-year level and reported in parentheses. All regressions have been weighted by population.
*** p <0.01 ** p <0.05 * p <0.1

Table 6: Regression discontinuity estimates for individuals from low versus high Socioeconomic backgrounds.

Socioecon. Char.	Low S.E.S	Low S.E.S	Low S.E.S	Low S.E.S	High S.E.S	High S.E.S	High S.E.S	High S.E.S
	Global	Global degree.	Local Linear	L.L BW*	Global	Global degree.	Local Linear	L.L. BW*
Panel A: Discont.								
in years of educ.	.23	2	.23	23	.57**	2	.96***	19
	(.19)		(.3)		(.25)		(.35)	
Panel B: Discont.								
in high school grad.	–.025	4	–.017	33	.03	4	.034	37
	(.045)		(.035)		(0.04)		(.032)	
Panel C: Discont.								
in college grad.	–.029	4	–.028	19	.10	4	.08	24
	(.04)		(.037)		(.084)		(.075)	
Panel D: Discont.								
in employment	.016	2	.022	28	–.007	2	–.015	25
	(.02)		(.027)		(.02)		(.032)	
Panel E: Discont.								
in monthly wages	–11.75	2	–5.17	29	–8.9	2	54.6	29
	(71)		(93.13)		(150.59)		(184.25)	
Panel F: Discont.								
in logged wages	–.025	2	–.046	21	.051	2	.059	30
	(.03)		(.041)		(.052)		(.063)	
Panel G: Discont.								
in hourly wages	–.35	2	–.47	26	.012	2	.31	36
	(.33)		(.43)		(.87)		(.99)	
Observations	9520		1858		4966		933	

* L.L. BW = Local linear bandwidth estimated using the CCT method.
* Number of observations for local linear regressions corresponds to observations from lowest bandwidth chosen (i.e 19 for both groups) Each cell represents a separate regression with a different dependent variable in each row and with the treatment variable being 'born before January 1, 1979'.
Low S.E.S coded as 0 for fathers' who are/were employed in less skilled/manual jobs. High S.E.S coded as 1 represents fathers who are employed in medium/high skilled specialized jobs.
All specifications control for a flexible polynomial of age in which the slope is allowed to vary on either side of the cutoff.
Sample includes French male citizens. Standard errors are clustered at the month-year level and reported in parentheses. All regressions have been weighted by population.
*** p <0.01 ** p <0.05 * p <0.1

Table 7: Regression discontinuity estimates for controls using different bandwidths and specifications.

Bandwidth	8 months	12 months	23 months	30 months	40 months	50 months	60 months	80 months	Global
	1	2	3	4	5	6	7	8	9
Panel A: Middle school enrollment discontinuity	.001	.002	.000	.002	.000	.004	.003	.003	.000
	(.002)	(.003)	(.003)	(.003)	(.003)	(.004)	(.004)	(.004)	(.004)
Panel B: Father's occupation disc.	.020	.004	.010	.015	.009	.010	.023	.016	.034
	(.04)	(.03)	(.04)	(.04)	(.03)	(.04)	(.04)	(.04)	(.04)
Month Polynomial	Zero	Zero	One	One	One	Two	Two	Three	Four
Observations	1340	1998	3813	4987	6608	8244	10029	13426	18973

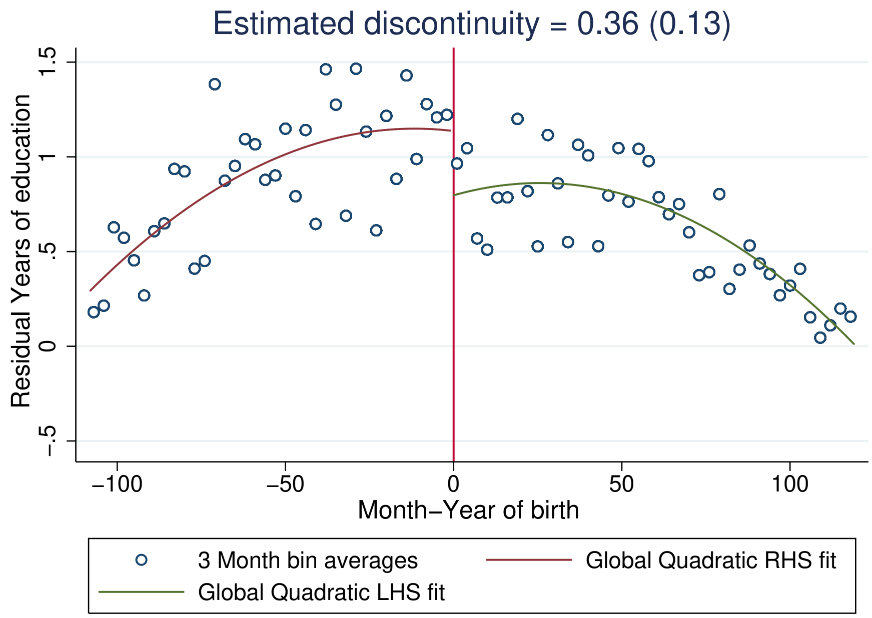
Notes: Sample includes French male citizens.
Each cell represents a separate regression with years of education as the dependent variable and the treatment variable 'born before January 1, 1979'.
All specifications control for a flexible polynomial of age in which the slope is allowed to vary on either side of the cutoff.
Standard errors are clustered at the month-year level and reported in parentheses. All regressions have been weighted by population weights.
*** p <0.01 ** p <0.05 * p <0.1

Table 8: Placebo regression discontinuity estimates for females using different bandwidths and specifications.

Bandwidth	10 months	20 months	30 months	40 months	50 months	Global
	1	2	3	4	5	6
Panel A: Female discont.						
in years of educ.	–.187	–.230	–.255	–.297	–.365	–.065
	(.17)	(.26)	(.20)	(.26)	(.24)	(.16)
Panel B: Female discont.						
in high school grad.	–.048**	–.074**	–.061**	–.071**	–.082***	–.035
	(.02)	(.03)	(.03)	(.03)	(.03)	(.02)
Panel C: Female discont.						
in college grad.	–.043	–.068*	–.021	–.063	–.059	.003
	(.03)	(.04)	(.03)	(.04)	(.04)	(.03)
Panel D: Female discont.						
in employment rates	.009	.005	–.017	–.005	–.014	–.016
	(.02)	(.02)	(.02)	(.02)	(.02)	(.01)
Panel E: Female discont.						
in monthly earnings	29.703	41.614	–4.805	28.358	28.910	–13.847
	(68.86)	(110.23)	(81.36)	(118.39)	(101.00)	(58.24)
Panel F: Female discont.						
in logged earnings	.024	.064	.042	.070	.075	.046
	(.04)	(.06)	(.04)	(.06)	(.05)	(.04)
Panel G: Female discont.						
in hourly earnings	.267	.364	.460	.550	.524	.239
	(.37)	(.59)	(.47)	(.65)	(.57)	(.34)
Month Polynomial	Zero	One	One	Two	Two	Two
Observations	1073	2290	3433	4556	5773	13463

Notes: Sample includes only female French citizens.
Each cell represents a separate regression with the treatment variable 'born before January 1, 1979'.
All specifications control for a flexible polynomial of age in which the slope is allowed to vary on either side of the cutoff.
Standard errors are clustered by birth month-year and reported in parentheses. All regressions are weighted by population.
Due to the unbalanced nature of the data, the number of observations using different dependent variables does not necessarily correspond with the last row.
**** p <0.01 ** p <0.05 * p <0.1

C Online Appendix Figures and Tables



1

Figure A1: Years of education residuals based on birth month relative to Jan 1, 1979 cutoff (Global quadratic graph).

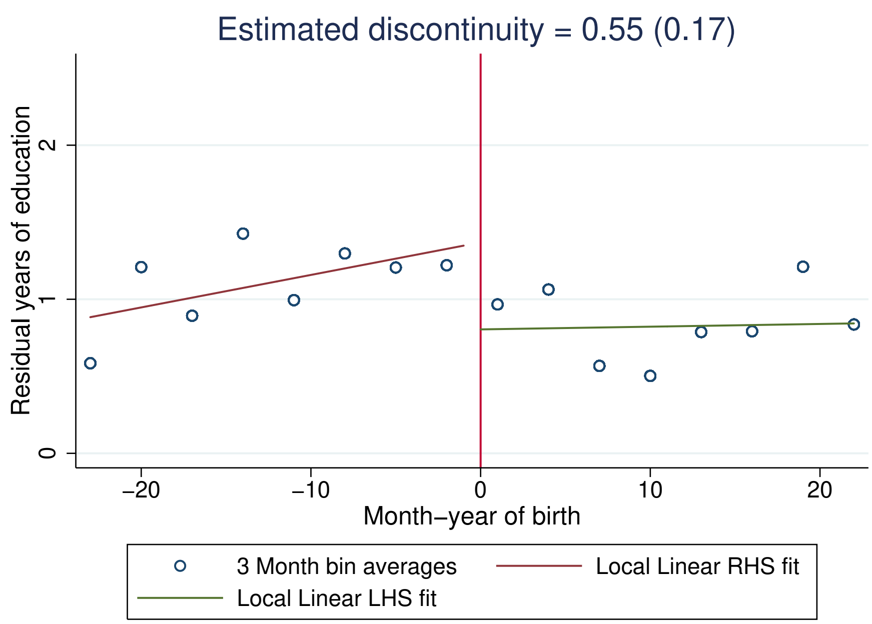
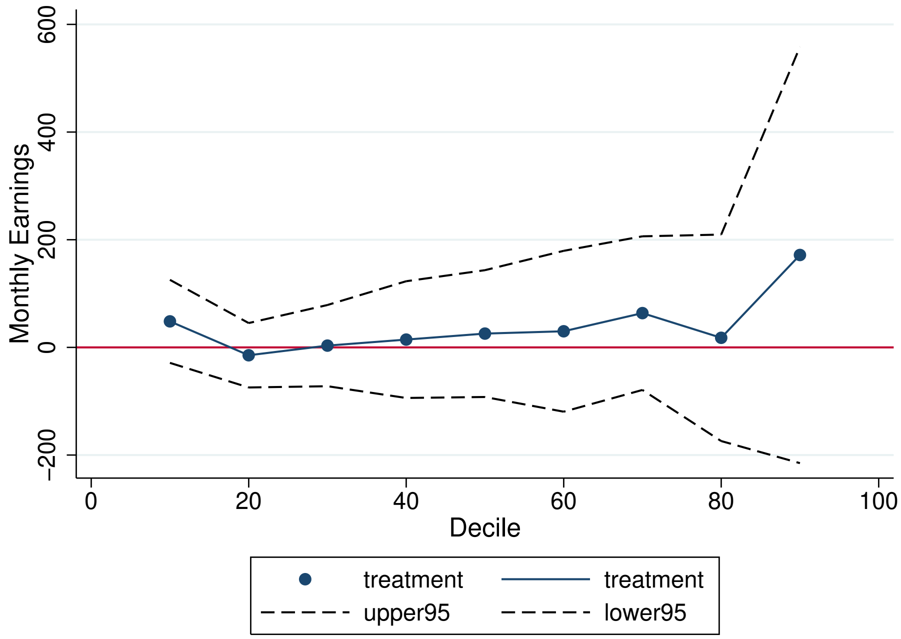


Figure A2: Years of education residuals based on birth month relative to Jan 1, 1979 cutoff (Local Linear graph).

- Notes: Sample includes French male citizens. All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses. Controls used include month of birth fixed effects, a dummy for middle school enrollment as well as father's occupation.

(a) Local linear regressions of h=30 months



(b) Global quadratic regressions

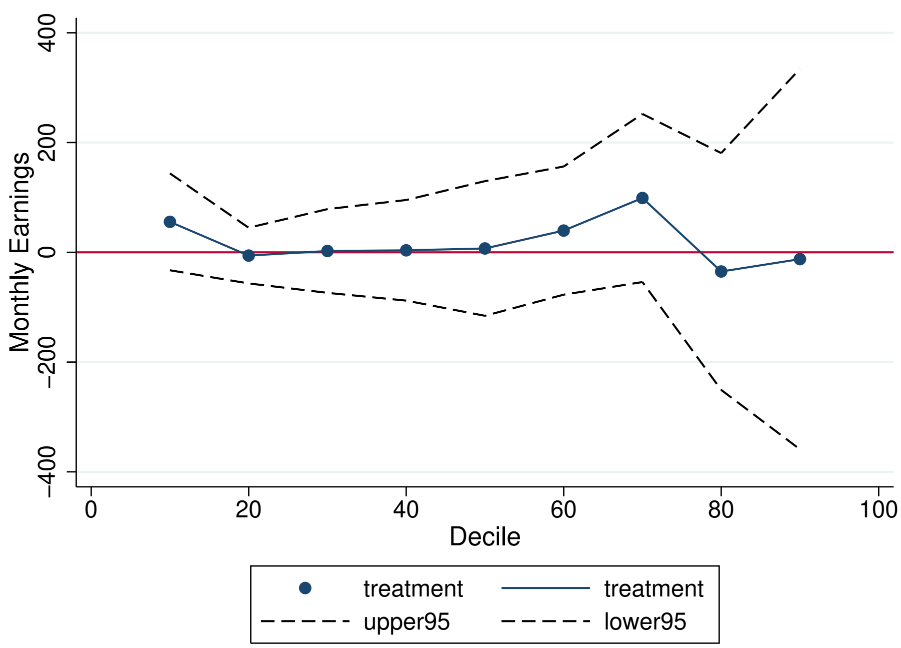


Figure A3: Distributional treatment effects on monthly earnings.

Notes: Solid and dashed lines represents the point estimates and 95% confidence intervals respectively. Sample includes French male citizens. All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses.

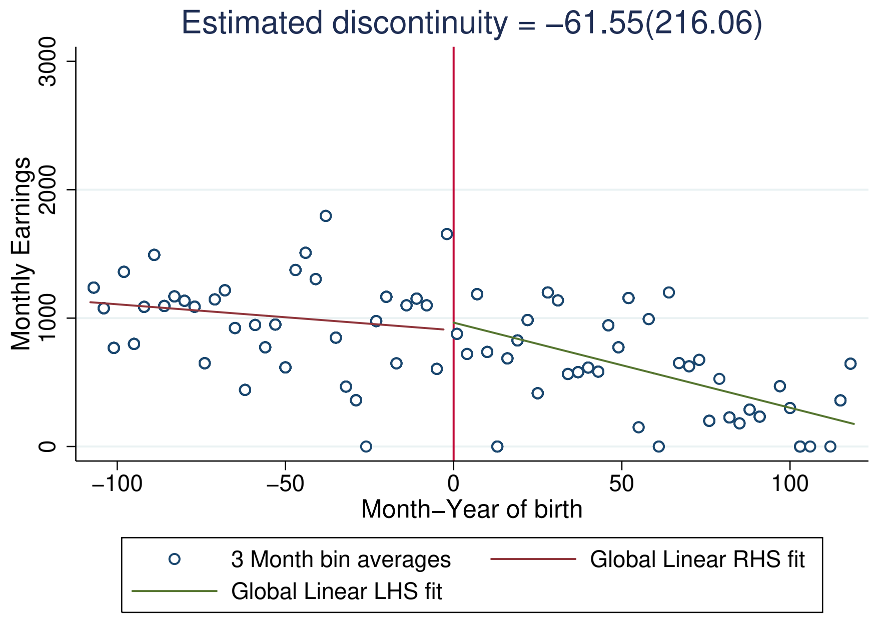


Figure A4: Years of education for individuals with only primary schooling completed (Global Linear graph).

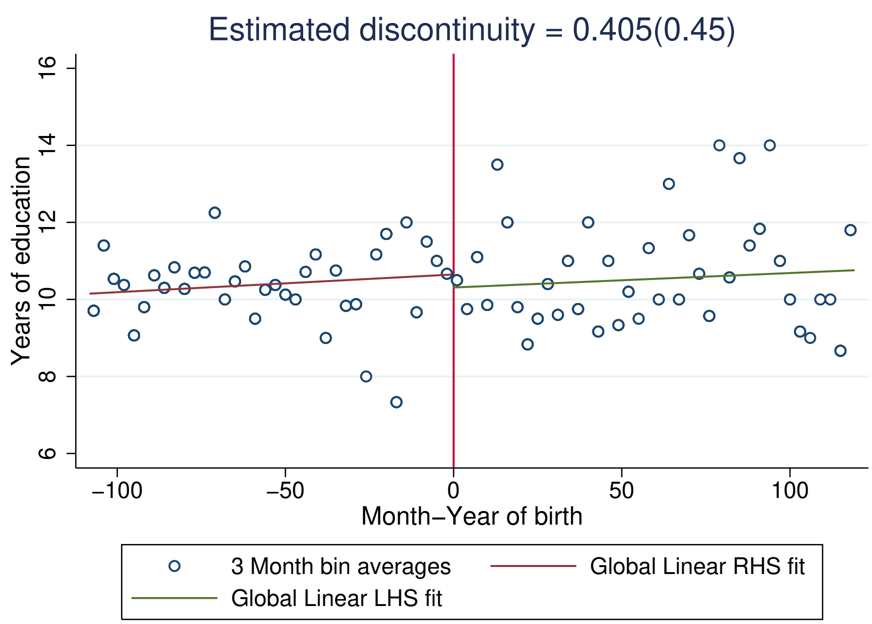


Figure A5: Earnings for individuals with only primary schooling completed (Global Linear graph.)

- Notes: Sample includes French male citizens who have completed only primary schooling. All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses.

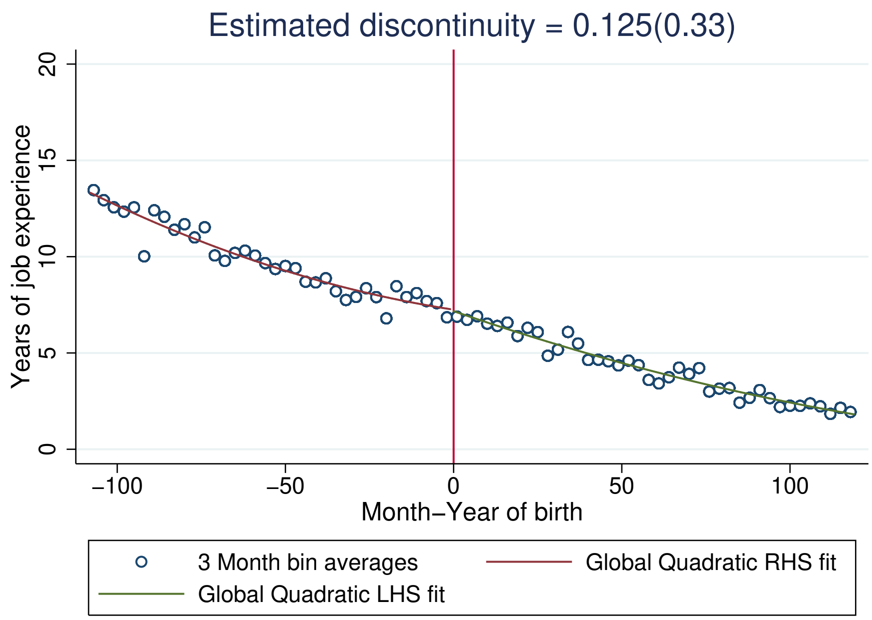


Figure A6: Years of job market experience (Global Quadratic graph).

- Notes: Sample includes French male citizens eligible for conscription. All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses.

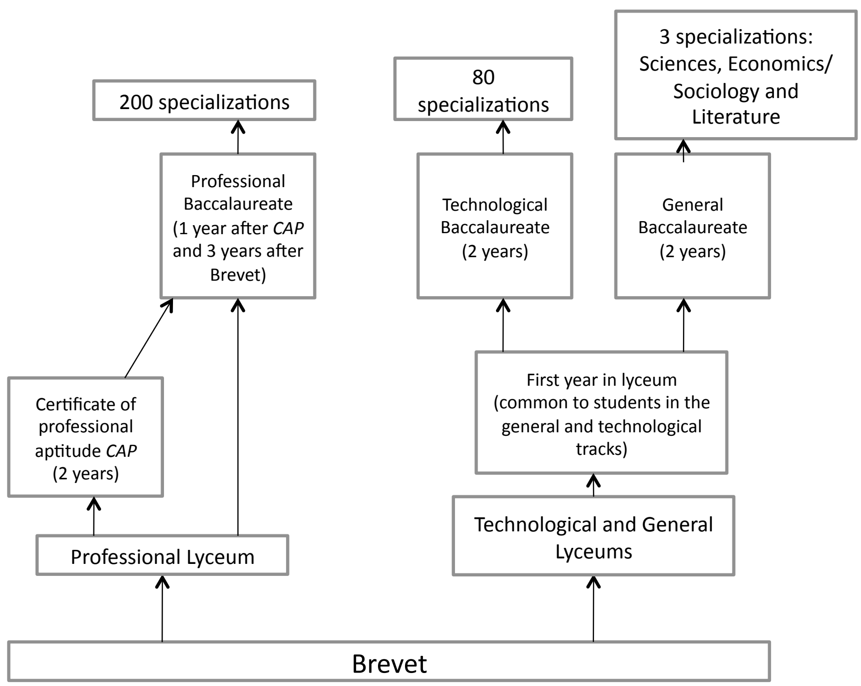
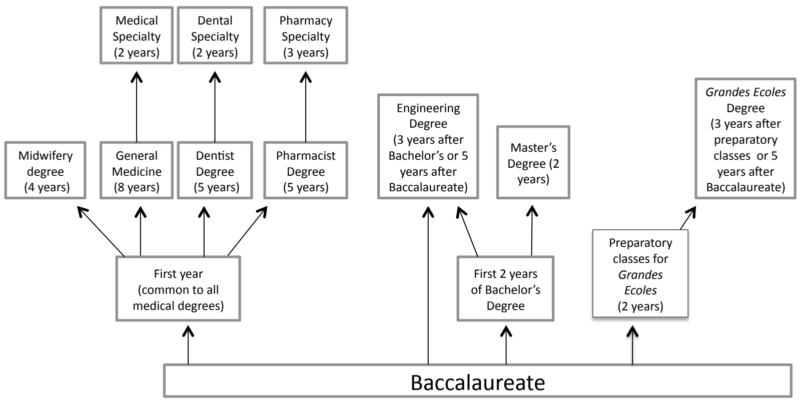
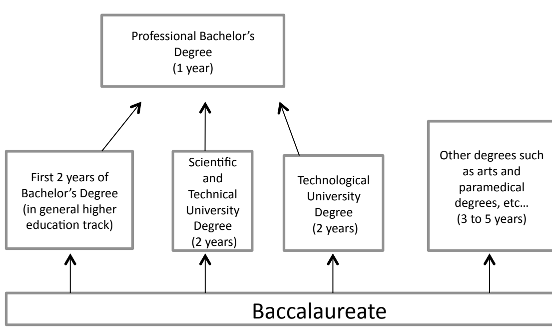


Figure A7: Organization of high school in France



(a) Organization of traditional higher education in France



(b) Organization of vocational higher education in France

Figure A8: Organization of higher education routes in France.