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

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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 date to serve. This paper uses a regression discontinuity design to identify the effect of peacetime conscription policies on education and labor market outcomes. Results indicate that conscription eligibility induces a significant increase in years of education but has no effect on employment and wages at the ages of 30 to 36. I then examine several competing hypotheses as to why labor market outcomes were unaffected, despite evidence of increased educational attainment. The interpretation most consistent with findings is that the average marginal return to the additional schooling induced by conscription was low.

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

JEL Classification Numbers: I20, J24, J30

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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 2014) and health outcomes (Bedard & Deschenes 2006; Angrist et al. 2010; 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. Indeed, conscripted individuals are required 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. 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 and those contemplating reinstatement.[1]

In this paper, I study the effects of peacetime conscription on the education and labor market outcomes of young French men. Prior to 1997, all French male citizens had to undergo a 10 month compulsory military service at the age of 18. Most men preferred to defer their service, and the best way to do so was by staying enrolled in an educational institution. In May of 1996, the French government announced that individuals born after January 1, 1979 were no longer required to enlist, while those born prior to that date were still required to do so. Since there is no reason to believe the outcomes of men born on either side of the January 1, 1979 cutoff would have been different absent conscription policy, I use a regression discontinuity design to compare outcomes of individuals who were barely subject to or exempted from mandatory military service. This research strategy allows me to overcome selection bias in who is deemed physically and mentally fit for military service.

Results indicate that men born just before January 1, 1979 acquire 4 to 6 additional months

[1] In Europe: Austria, Denmark, Estonia, Finland, Greece, Norway, and Switzerland all still require their citizens to undergo mandatory military service. Additionally, Lithuania has decided to reintroduce conscription in 2015 and a recent poll in France reveals that 80 percent of the French population would like to reinstate compulsory national service (IFOP, 2015).

of education compared to those born just after this date—which is consistent with draft avoidance behavior. These additional years of schooling do not result in increased university degree completion, though I do find evidence of a 5 to 6 percentage point increase in high school graduation for individuals from wealthier backgrounds. Finally, I find that men subject to conscription are not more likely to be employed and do not have higher wages, but lose approximately 0.4 years of valuable early labor market experience. I then examine several competing hypotheses as to why conscription policy did not affect earnings, despite evidence of increased educational attainment for those under threat of service. To do so first requires understanding how military service directly affected labor market outcomes. On the one hand, if service had a negative effect on wage, then the education induced by conscription had a positive return which was offset by a combination of military service and a loss of early work experience. On the other hand, if military service had a non-negative effect on wage, then the average marginal return to schooling induced by conscription was lower than or equal to the loss of early labor market experience. In section 6, I provide suggestive evidence supporting the latter interpretation.

To test the validity of estimates, I show that results are robust to bandwidth choice, functional form and the addition of predetermined controls. I also address concerns over whether results are driven by school age starting laws; children in France normally start school in the calendar year in which they turn six. I do so by first showing that the addition of month of birth fixed effects does not significantly affect estimates. In addition, I also illustrate that there is no discontinuity in the education and labor market outcomes of women at the cutoff. Finally, I show that there is no effect of threshold-crossing at January 1 in other years.

There is a growing body of research that looks at the effect of peacetime conscription on educational attainment and labor market outcomes. Results are mixed. For instance, Cipollone & Rosolia (2007) and Hubers & Webbink (2015) find that conscription decreases educational attainment in Italy and the Netherlands respectively. Conversely, Card & Lemieux (2001), Maurin & Xenogiani (2007) and Bauer et al. (2014) show that mandatory military service led to an increase in education in the U.S., France, and Germany—consistent with draft avoidance behavior. Di Pietro (2013) finds that conscription has no effect on education in Italy, though he does detect evidence of heterogeneous enrollment effects based on socioeconomic status. The findings of studies that examine labor market outcomes are also mixed. Imbens & Van der Klauw (1995) and Hubers &

Webbink (2015) both find that peacetime conscription led to a significant reduction in long run wages in the Netherlands. Similarly, Galiani et al. (2011) shows that military service led to reduced earnings in Argentina. On the other hand, Maurin & Xenogiani (2007) and Card & Cardoso (2012) uncover positive returns to peacetime service in France and Portugal respectively; although results in Card & Cardoso (2012) are only significant for men with low levels of education. Finally, Grenet et al. (2011) and Bauer et al. (2012) find that mandatory military service has no effect on the long run earnings of conscripts in England and Germany respectively.

This paper is most similar to Maurin & Xenogiani (2007) who examine the impacts of the same conscription reform in France on similar outcomes. 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, their study looks at the outcomes of men working in the private sector, while I include men from both sectors of the workforce in my analysis. Third, I use a RD design that compares mean outcomes across men born one month apart whereas they utilize a difference-in-differences strategy that compares across men and women born after and before the reform date. Finally, I analyze wage outcomes for men at the ages of 30 to 36, whereas Maurin & Xenogiani (2007) look at the entry wages of men aged between 16 and 23 years old. Both approaches yield evidence of draft avoidance in terms of increased years of schooling, and their finding that this is driven by increased high school graduation is consistent with effects I find for higher-income families. However, this paper differs significantly from Maurin & Xenogiani (2007) with respect to the main findings. Specifically, they report that conscription policy led to large and significant wage effects. In contrast, I find no evidence that the increased schooling results in higher earnings.[2] In online Appendix A, I show that the differences in findings are due to a lack of robustness of results from the difference-in-difference design, which highlights the shortcomings of that strategy in this setting. Specifically, I use the same methodology as Maurin & Xenogiani (2007) to show that an alternative and arguably more correct classification of cohorts to treatment and control results in estimates qualitatively similar to mine, as does including public and private sector workers rather than only private sector employees as in Maurin & Xenogiani (2007).

[2] In addition, they report that results are driven by men from low socioeconomic backgrounds, while results in this paper are driven by men from wealthier households.

This paper contributes to the literature in several ways. First, I use a compelling research design that enables me to estimate the causal impact of conscription policy under a relatively weak assumption; determinants of individuals' outcomes vary smoothly across the January 1, 1979 cutoff. In fact, short of random assignment, regression discontinuity designs are arguably the most credible approach to identifying causal effects (DiNardo and Lee, 2011). As described above, this leads to significantly different findings from a previous analysis that relied on the much stronger identifying assumption of a difference-in-differences design. Specifically, Maurin & Xenogiani (2007) report that conscription policy led to a 12.5 percent increase in earnings in France. In contrast, I find evidence of no effect using a regression discontinuity design applied to the same French conscription reform, and am able to rule out earnings effects larger than 3 to 4 percent. These findings are consistent with the two other RD studies in the literature (Grenet et al., 2011 and Bauer et al., 2012) that both find that conscription policy has no effect on longer run earnings.

Second, I present estimates that speak to the impact of conscription policy on education and labor market outcomes jointly. This contrasts with most previous work on the topic which has looked at these two effects separately. This matters, as it enables me to better understand the role education plays as a mediating channel, providing for a more complete picture of the long run impacts of conscription policy.[3] Indeed, further analysis reveals that the average marginal return to the additional schooling induced by conscription was low. This suggests that 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.

Finally, this is the first paper to use a credible research design to examine the labor market effects of abolishing mandatory military service on cohorts born after the mid 1970s. Importantly, 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 military service policies—most of whom have similar conscription policies to France in both duration and intensity.

[3]For example, Maurin & Xenogiani (2007) and Bauer et al. (2014) consider the joint impact of conscription policy on schooling and labor market outcomes. Both studies find education to be an important mediating factor.

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 the period analyzed in this paper, conscription did not involve any combat duties and was mostly restricted to security posts inside and outside the army (police, technical assistance, overseas, etc...). In theory, an individual could postpone their 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. As a result, the effect of delaying and/or avoiding conscription both predict an increase in education uptake. 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 registered in military service in a specific year were enrolled in school in the preceding year, 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 nationwide 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. This was eventually formalized in November of 1997. The 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 date and decision of full conscription elimination was not announced in advance. However, men born in 1977 and 1978 were more likely to avoid service altogether, compared to older cohorts. This is because they did not have to serve in the military if they were still in the education sector at the ages of 24 and 23 respectively. 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.

To check that the 1997 conscription exemption policy was binding in practice, I show how male conscription rates varied by birth year cohort. To do so, I rely on data from *'Generation 1998 à 10 ans'*, an individual level survey. Particularly, I use the 3rd installment of this longitudinal data set (the 2008 wave) as it contains individual level data on conscription and year of birth. A few limitations of this survey are that it only contains the birth year of an individual and not the birth month. Further, the survey has high attrition and it only includes individuals with specific ties to the year 1998; whether these individuals are representative of France more broadly is an open question.[4] However, my goal in using this particular data set is solely to highlight the binding nature of the 1997 conscription exemption law. As a result, I am not too concerned with the above mentioned issues. Figure 1 reveals a large and visible discontinuity at the January 1, 1979 cutoff, confirming the existence of a 'first stage'. Indeed, individuals born just before January 1, 1979 were 23 percentage points more likely to have been conscripted, indicating that this policy was binding in practice.

Table D1 of the online appendix summarizes conscription rate patterns for different birth cohorts and for individuals from varying socioeconomic backgrounds. Rows 1 through 3 of Table D1 reveal that around 50 percent of all males born between 1974 and 1976 eventually served in the military.[5] 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

[4]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.

[5]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.

year implying that about 50% of a given birth cohort performed their national service duties. 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 in that individuals born just to the left of the cutoff were more likely to be exempt from conscription, as compared to older cohorts. Additionally, Table D1 also reveals that males from different socioeconomic backgrounds (S.E.S) were not serving at disproportionate levels. More specifically, conscription rates—averaged across the 1974 to 1978 cohorts—were similar; 45.2% of individuals from low S.E.S and 44.2% of individuals from high S.E.S backgrounds served in the military. After 1979, no individual from either subgroup served in the military. This alleviates concerns attributed to differential selection into conscription based on socioeconomic background.

3 Data and Sample Construction

My analysis uses data taken from 13 waves (2003-2015) of the French Labor Force Survey (LFS) administered by the *Institut de la Statistique et des Études Économiques* (INSEE). I focus on the most recent waves of the LFS as it allows me to observe earnings information for individuals in their early to mid 30s.[6] The 2003-2015 labor force surveys cover private households in metropolitan France. Participation in the survey is compulsory and subsequently the LFS has a high response rate of about 81 percent. Prior to 2003, the labor force survey was conducted annually and households were interviewed in person over 3 consecutive years. As of 2003, the LFS underwent various structural changes; chiefly the surveys are now conducted quarterly. As a result, each household is interviewed for six consecutive quarters with the first and sixth quarters' interviews administered in person and the others done over the phone. In an attempt to be as representative as possible, the LFS aims for a sampling rate of 1/150 of the total population. Importantly, all post 2003 surveys provide detailed information on individuals' month-year of birth, sex, education level, employment status, hours worked, wage and parents' occupation.

The initial sample consists of 187,624 French men born between 1975 and 1982. I restrict the sample to individuals whom I can observe both educational and labor market information for, i.e.

[6] I do not rely on earlier versions of the LFS (1991-2002) in the main analysis because they are structurally different than the post 2002 surveys. Particularly, the old LFS was conducted yearly and many variables are not identified through the same questions nor are they coded the same. However, in Online Appendix A, I do make use of these earlier versions in order to reconcile my results with those of Maurin & Xenogiani(2007).

individuals in the labor force. This leaves me with a sample of 54,145 men. As a final restriction, I focus on individuals who are between 30 and 36 years of age.[7] Descriptive statistics for this final sample of 24,414 men are reported in Table 1.[8] Respondents have an average of 14.65 years of education, which corresponds to the number of years an individual is observed within an educational institution. The average net monthly wage for 30 to 36 year old individuals in the sample is 1508 Euros and the employment rate stands at 90.5 percent. Further, the average reported hourly wage is 12.67 Euros with an average workweek of 39.4 hours. Additionally, 59 percent of men in my sample gradate high school by the ages of 30 to 36 while 18.7 percent graduate university.[9] Finally, 35.3 percent of men in the sample are from a high socioeconomic background (S.E.S). Here, I rely on fathers' occupation as a proxy for socioeconomic status. A detailed overview of how all key variables are constructed is available in Online Appendix B.

4 Identification Strategy

I use a standard regression discontinuity framework (Imbens & Lemieux, 2008; Lee & Lemieux, 2010) to estimate the effects of being exposed to conscription policy on educational attainment and labor market outcomes. 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 continuous at 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 the treatment estimates do not significantly change with the addition of covariates. Accordingly, any discontinuity observed in 1979 can be attributed

[7] Since I am using an RD design based on date of birth and data from multiple surveys at different points in time, then this restriction insures that I am not analyzing outcomes of individuals at significantly different ages; a problem that is exacerbated by the use of multiple bandwidths.

[8] In Figure C1 of the online Appendix, I show that there is no discontinuous attrition at the cutoff when moving from one sample to the other. Figure C1(a) reveals no discontinuity in the likelihood of being observed in the labor force sample at the threshold. Similarly, Figure C1(a) shows that the likelihood of being observed in the labor force sample at the ages of 30 to 36 is also smooth at the cutoff.

[9] For a detailed illustration of the available educational routes in the high school and higher education system, refer to Online Appendix Figures C8 and C9. 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.

to the causal effect of being born before January 1, 1979, and thus being subject to mandatory conscription policy. Formally, I estimate the following reduced form equation:

$$Y_i = \alpha_1 + \alpha_2 Distance_i + \alpha_3 Before_i + \alpha_4 (Distance_i * Before_i) + \alpha_5 X_i + \epsilon_i \quad (1)$$

Where the dependent variable Y_i is the outcome of interest for individual i. $Before_i$ is a dummy variable indicating whether individual i belongs to a pre-reform cohort or not (i.e. is born before January 1, 1979). $Distance_i$ is the running variable and measures an individual's month-year birth cohort relative to the cutoff date of Jan 1, 1979. Further, I allow the slopes of the fitted lines to differ on either side of the 1979 cutoff by interacting $Distance_i$ with $Before_i$. 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_i represents unobservable factors affecting outcomes for each individual i. The parameter of interest is α_3 which gives us the intent to treat (ITT) effect of being required to serve in the military.

I estimate the parameter α_3 using local linear regressions with uniform kernel weights. In all regressions, I use population survey weights and cluster standard errors at the month-year level as suggested by Lee & Card (2008).[10][11] I present estimates over varying bandwidths and I also present local mean and local quadratic estimates to test the sensitivity of results to bandwidth choice and functional form, as has become standard in the RD literature (Lee & Lemieux, 2010)

In what follows, I focus on reduced form results, which should be interpreted as the intent to treat (ITT) effects of conscription eligibility. The reason I do so is twofold: First, the discontinuity in the likelihood of conscription at the cutoff does not really affect educational outcomes as it is eligibility for conscription rather than conscription in itself that drives those results.[12] Second, re-weighting the reduced form estimates for labor market outcomes by the discontinuity in conscription using a Two Sample Two-Stage Least Squares (TS-2SLS) framework relies on a tenuous exclusion restriction, seeing as conscription eligibility has a dual effect on education and earnings.

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

[11] 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.

[12] Maurin & Xenogiani (2007) show that only 4% of French men who were in conscription in a specific year pursued education the following year.

5 Results

5.1 Educational attainment

In this section, I check whether conscription eligibility affects mens' education decisions. To do so, I first examine the impact of being born just before January 1, 1979 on years of education. Figure 2A shows the graphical relationship between years of education as a function of birth month-year relative to the January 1, 1979 conscription eligibility cutoff. These figures take the same form as those after them in that open circles represent local averages over a 1 month birth period. Further, all figures are drawn over a bandwidth of 48 months on either side of the cutoff using a linear fit. Figure 2A reveals a visible discontinuity in years of education for French men who are at the age of 30 to 36 years old. Corresponding regression estimates are summarized graphically in Figure 2B. Specifically, I plot various local linear RD estimates over a 12 to 48 month bandwidth to assess the robustness of results to bandwidth choice. Importantly, RD estimates for years of education are statistically significant at the 5% level regardless of bandwidth choice.

Table 2 summarizes these results more formally. Panel A depicts discontinuity estimates using different bandwidths and functional forms of the running variable, with standard errors clustered at the month-year level throughout. The estimates range from 0.3 to 0.56 years of extra education and are all statistically significant at the 5% level. As shown in Panel B of Table 2, the addition of controls slightly reduces the magnitude of effects, but does not significantly change estimates. This is consistent with the identifying assumption that other determinants of education are smooth at the cutoff. The controls used include birth month fixed effects, a binary variable for middle school enrollment and socioeconomic status. I conclude that conscription policy led to an approximate 4 to 6 month increase in educational attainment for young French men who were at risk of conscription. These results are in line with a draft avoidance hypothesis, i.e. acquiring extra education to avoid or delay military service.

Next, I examine whether the documented increase in years of education resulted in an increase in degree attainment, as measured by the likelihood of graduating high school and university.[13]

[13] 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 and doctorate level and find none. Finally, prior to 2003, students were able to get a 2 year university degree called "DEUG" (See Online Appendix Figure C9 for details on the higher education system). However, after the LMD reform of

Figure 3A suggests that the probability of high school graduation is not entirely smooth at the conscription eligibility cutoff. Indeed, Panel A of Table 3 indicates that conscription eligibility may have led to a 2 to 2.5 percentage point increase in high school graduation rates. However, these average effects are not statistically significant over varying bandwidths and functional forms, an issue I return to in section 5.3.[14] Conversely, Figure 3B reveals no discernible discontinuity in the probability of graduating college at the cutoff. Indeed, regression estimates from Panel B of Table 3 show no statistically significant eligibility effects on the likelihood of university graduation. In summary, I find that mandatory conscription significantly increases years of education for those eligible. I find no evidence that this is the result of increased university graduation and some evidence that this may be partly due to increased high school graduation rates.

5.2 Labor market outcomes

I now turn to whether this policy resulted in any labor market impact on cohorts of men required to serve. First, I check to see if the elimination of conscription generated any sharp changes in the likelihood of employment. Panel A of Figure 4 reveals no visible discontinuity in employment likelihood at the conscription eligibility cutoff. Corresponding regression estimates are shown in Panel A of Table 4. Regression discontinuity 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 significantly affect likelihood of employment.

Next, I look at whether there are any substantial wage effects for those barely subject to conscription. To do so, I analyze the relationship between various measures of earnings at the January 1, 1979 cutoff. Specifically, I look at how monthly wage, log wage and hourly wage vary at the conscription threshold. Monthly wages are measured in Euros and account for unemployment (zero earners). Log wages focus on intensive margin changes and hourly wages account for any behavioral responses in hours worked. Panels B through D of Figure 4 do not reveal any visual discontinuity in any of these wage measures at the conscription eligibility threshold.

2003 this degree was gradually eliminated until completely being phased out in 2010. Since many men in my sample would have attended college prior to the 2003 reform, I also check for a discontinuity in the likelihood of obtaining a DEUG degree and find none.

[14] To further check at which margin of the distribution men are increasing schooling, I run an RD quantile regression (Frandsen, Froelich, and Melly; 2012) with years of education as outcome. I find that there is a shift in the 70th percentile of educational attainment. Specifically, 70 percent of men born after January 1, 1979 have at least 15 years of education, compared to 16 years of schooling for those born before.

Having shown the raw patterns of labor market outcomes around the threshold, I now turn to regression-based estimates. Table 4 formally shows that there is no statistically significant relationship between labor market outcomes and being born just before January 1, 1979, regardless of choice of bandwidth and functional form. Importantly, estimates are reasonably precise. For example, I am able to rule out wage effects larger than 3 to 4 percent.[15] I conclude that conscription policy had no effect on labor market outcomes, despite the significant increase in years of education that it caused.[16]

5.3 Heterogeneous RD effects

While the above results indicate 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 financially if they choose to prolong education. Thus, in order to complement the initial results and shed light on potential mechanisms, I next look at reduced form effects for individuals from high versus low socioeconomic backgrounds (S.E.S) to assess if one group was differentially affected by this policy.[17] Table 5 summarizes results for both subgroups using local linear regressions. Specifically, in the first three columns, I report estimates for individuals from poorer households over a bandwidth of 24, 36 and 48 months. In the last three columns, I report estimates for individuals from more affluent backgrounds using the same bandwidths.

Results indicate no significant change in years of education for individuals from low S.E.S backgrounds, as shown in columns 1 to 3 of Panel A. In contrast, I find that high S.E.S individuals take on considerably more education. Specifically, I estimate a significant effect on the order of 0.55 to 0.89 additional years of education, depending on bandwidth choice. These results are consistent with a priori expectations, seeing as resources play an integral role in prolonging one's education.[18]

[15]This contrasts with Maurin & Xenogiani (2007) who find that the elimination of conscription policy in France led to a 12.5 percent increase in earnings for those required to serve (Table 3, Column 2 in Maurin & Xenogiani).

[16]To assess conscription eligibility effects on the distribution of earnings as opposed to just average earnings, I run RD quantile regressions using log earnings as an outcome variable (Frandsen, Froelich & Melly, 2012). The quantile treatment effects suggest insignificant estimates at all deciles of the income distribution, which are consistent with the average treatment effects. Results are summarized in Figure C2 of the online appendix.

[17]Differential selection into mandatory conscription by socioeconomic status could potentially bias these results. However, as shown in Table D1, I find no evidence of this.

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

I also find that while conscription eligibility does not influence high school graduation rates for individuals from less affluent backgrounds, it does lead to a 5 to 6 percentage point increase in the likelihood of graduating high school for individuals from more affluent backgrounds. Importantly, this finding helps explain the small and non-robust average high school graduation effect I find for the full sample. Indeed, the small documented average effect in section 5.1 seems to mask a statistically significant and meaningful impact for individuals raised in more affluent households combined with an insignificant impact on poorer households. In contrast, Panel C of Table 5 reveals no statistical relationship between conscription eligibility and college graduation for both subgroups, although effects are imprecisely estimated for the high S.E.S subsample.

Finally, I find no statistically significant effect for both subgroups in terms of labor market outcomes. Specifically, Panel D of Table 5 reveals no statistical relationship between the likelihood of employment and being born just before January 1, 1979 for both subgroups. I also estimate a zero effect for both groups with respect to monthly, hourly, and log wage. While these effects are reasonably precise for the low S.E.S sample, they are imprecisely estimated for the high S.E.S sample preventing me from making any definitive conclusions. In summary, the results from this section suggest that even though individuals from more affluent backgrounds were taking on more education, in the form of increased high school graduation, this did not seem to affect their labor market outcomes.

5.4 Robustness Checks

5.4.1 Sorting around the cutoff

I test the reliability and validity of the identification strategy used. One advantage of an RD strategy is that there are several tests that enable me 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, this type of sorting seems highly implausible as strategically altering one's birth certificate 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 change their birth documents in such a short period of time with such ease. Indeed, Figure C3 of the online Appendix depicts the distribution of observations in the sample, with no large mass evident to the right of the cutoff.[19]

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 I 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. Given the scope of this policy and the data at hand, I do not possess a rich set of predetermined characteristics at my disposal. I am, however, able to examine whether there is a documented discontinuity in socioeconomic status or in middle school enrollment rates.[20] Results depicted in Figure C4 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 D2 of the online Appendix. These results suggest that individuals on either side of the cutoff are similar in observable characteristics

5.4.2 Falsification Exercises

Recent studies have documented large seasonal birth effects on later lifetime outcomes (For recent developments; see, Buckles and Hungerman, 2013). In France, children normally start school in the calendar year in which they turn six years old. Grenet (2011) finds evidence suggesting that this adversely affects French children born in December relative to those born in January. To assess the extent to which age of school starting laws or some other confounding factor—that changed discontinuously across the January 1, 1979 threshold—may affect the interpretation of my results, I perform a series of falsification exercises. I start by checking for discontinuities among females, a group exempt from military service, but who are also exposed to the same school age starting law as males. Specifically, if women born just before January 1, 1979 also experience a significant increase

[19] 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...

[20] Middle school enrollment rates should not be affected by the policy since students at that age are not yet eligible for conscription. Further, I do not focus on middle school graduation rates since 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.

in school attainment, then one may be concerned about the results being driven by age of school entry cutoffs or some other policy affecting children born before the cutoff. Online appendix Figures C5a through C5f reveal no visible discontinuity in years of education, graduation rates, employment or wage for 30 to 36 year old French women born close to the January 1, 1979 cutoff. Corresponding regression estimates are reported in online Appendix Table D3. These formal regression estimates are consistent with visual findings and indicate no statistically significant discontinuity over a wide array of bandwidths and functional forms. If seasonal birth effects impact men and women equally, then this suggests that age of school entry laws are not driving the documented results for the male population.

As a further check, I next look at the relationship between years of education and various cutoff birth months around the January 1, 1979 eligibility threshold. To do so, I estimate 100 separate regressions using fake cutoff months as treatment and years of education as outcome. Figure C6 of the online appendix summarizes these findings by plotting t-statistics for these various placebo cutoff dates around the Jan 1, 1979 threshold. The zero cutoff point represents the conscription eligibility threshold with all others being placebo estimates for months relative to the original being used as simulated treatment. All significant estimates are highlighted in the figure with a large red filled circle.[21] The non-dashed vertical line passes through the cutoff threshold of January 1, 1979, while the vertical dashed lines highlight all other January 1 cutoffs between 1975 and 1983 (excluding 1979). Results indicate that the conscription exemption cutoff date of January 1, 1979 provides for the largest and most significant discontinuity. I also observe another 5 significant discontinuity estimates out of the remaining 100 fake cutoff months.[22] Importantly, all other January cutoffs reveal statistically insignificant effects with t-statistics close to zero. This provides further evidence that the documented significant treatment effect on years of education is in fact due to conscription eligibility and not a time invariant December/January birth month effect.

[21]Each point on the figure represents treatment t-statistics from local linear regressions of bandwidth = 24 months using years of education as the dependent variable.

[22]This is consistent with a Type-1 error of 5%. Further, some estimates close to the cutoff will mechanically be significant by nature of the empirical design.

5.4.3 Difference in Discontinuity Estimates

To further alleviate concerns over month of birth effects, I run a more formal check. Specifically, I utilize a difference in discontinuity design to measure the extent of a time-invariant January versus December bias. The intuition here is to compare the difference in life outcomes for people born just before and after January 1, 1979 to the difference for those born just before and after every other January within 4 years of 1979. Formally, I run the following reduced form equation for all 30 to 36 year old men born between 1975 and 1982:

$$\begin{aligned} Y_i = \beta_1 + \beta_2 Distance_i + \beta_3 Treatgroup_i + \beta_4 Before_i + \beta_5 (Distance_i * Treatgroup_i) + \\ \beta_6 (Before_i * Distance_i) + \beta_7 (Before_i * Treatgroup_i) + \beta_8 (Before_i * Treatgroup_i * Distance_i) + \epsilon_{im} \end{aligned} \quad (2)$$

Where Y_i is the outcome of interest. $Distance_i$ is the running variable and represents an individual's month of birth relative to January. $Treatgroup_i$ is a binary variable that takes the value of 1 for treated cohorts, corresponding to all individuals born within 6 months on either side of January 1, 1979 and 0 for control cohorts born within 6 months of January 1 in every year except 1979. $Before$ is a binary variable that takes the value of 1 for births taking place from July to December and 0 for births occurring between January and June. The parameter of interest in this regression is β_7, which gives us the difference in the discontinuous jump in all outcomes for males born shortly before January 1 to those born just after January 1 between the treated and the control cohorts.[23] This empirical strategy restricts the bandwidth to 6 months. As a consequence, the number of clusters will be small leading to downward biased cluster-robust standard errors. To overcome this problem, I use a cluster wild bootstrap t-procedure for inference (Cameron, Gelbach and Miller; 2008).[24]

Table D4 of the online appendix summarizes the results of this exercise for various outcomes of interest with p-values reported under each estimate. In column 2, I report the difference in

[23] The parameter β_3 summarizes the average difference in outcomes for males born just before January in the treated versus control cohorts. The parameter β_4 represents the average difference in outcomes for men born just before January to those born just after January in the control cohorts. In contrast, the parameter α_3 from equation (1) represents the average difference in outcomes for men born just before January 1, 1979 to those born just after this cutoff date.

[24] I report p-values from this procedure in Table D4, as the computation of standard errors can be computationally intense.

discontinuity estimate β_7 from equation (2) using a bandwidth of 6 months. For comparison, I report traditional local linear regression discontinuity estimates over the same bandwidth of 6 months in column 1 (i.e. α_3 from equation(1)). Column 2 of Panel B reveals that years of education increased by a statistically significant 0.7 years for males born shortly before to those born after January 1 between the treated and the control cohorts. I also find a statistically significant 0.77 increase in years of education using a traditional RD regression in Column 1. High school graduation likelihood also increased by a statistically significant 2.5 percentage points at the cutoff for the treated group relative to the control. Similarly, using a traditional RD design, I also find a 1.8 percentage points increase in high school graduation at the cutoff. All other education and labor market outcomes are statistically insignificant using either design. In summary, the findings from the difference in discontinuity analysis—and their consistency with traditional RD estimates—suggest that age of school entry laws do not have a significant effect on the main results.

6 Interpretation

So far I have estimated intent to treat effects for education and labor market outcomes. I find evidence of a discontinuity in educational attainment but none in labor market outcomes. However, because military service can have a direct effect on labor market outcomes, then interpreting these results as evidence of little to no returns to education would only be true under certain assumptions. Below I present a simple conceptual framework that enables me to speak to potential mechanisms and to better interpret findings.

To simplify, let us assume that birth date (B) is the only factor that determines conscription eligibility (e) and that e and conscription (C) are continuous variables over the real line.[25] Further, assume that schooling (S = S(e)) is an increasing function of eligibility and that labor market experience (Exp= Exp(e)) is a decreasing function of eligibility. Finally, assume that wage (W = W(S(e), C(e), Exp(e))) is a function of schooling, conscription and labor market experience.[26]

By a simple application of the chain rule, the reduced form effect of conscription eligibility

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

[26]I focus on these three earnings inputs because they are directly affected by the elimination of conscription policy and are the three main channels emphasized in the conscription literature. I do not account for the potential wage impact from age of school starting laws in France, since—as shown in Section 5.4—this does not have a significant direct effect on labor market outcomes, despite being correlated with the conscription cutoff date.

on wage can be decomposed into a schooling effect, a conscription effect and a work experience effect:

$$\frac{dW}{de} = \frac{\partial W}{\partial S} * \frac{dS}{de} + \frac{\partial W}{\partial C} * \frac{dC}{de} + \frac{\partial W}{\partial Exp} * \frac{dExp}{de} \tag{3}$$

From the results section, we know that conscription eligibility has no effect on wage ($\frac{d\hat{W}}{de} = 0$) and that it increases schooling by 0.3 to 0.5 years ($\frac{d\hat{S}}{de} = 0.4$). Further, Figure 1 reveals that men born just before January 1, 1979 are conscripted at a rate of 0.25. Since conscripts had to serve for a period of 10 months, then $\frac{d\hat{C}}{de} = \frac{(0.25*10)}{12} = 0.2$. Finally, since men born just before the cutoff had more schooling and were more likely to serve in the military, then this would mean that conscription eligibility delayed their entry into the workforce. Unfortunately, the LFS does not contain information on accumulated work experience or year of first occupation which makes it hard to estimate $\frac{dExp}{de}$ directly. However, I am able to provide an indirect estimate by exploiting the fact that information on employment is available for ever survey year combined with the fact that I have access to multiple waves of the Labor Force Survey. Specifically, I compute the age profile of accumulated labor market experience separately for the 1978 (non-exempt) cohort and 1979 (exempt) cohort. To do so, I estimate the likelihood of having an observed wage at the ages of 17 through 26 for men born in 1978 and for men born in 1979, using multiple waves of the LFS.[27] Figure C7 of the online appendix summarizes the results of this exercise. I find that the 1978 cohort had a lower likelihood of employment at the ages of 17 through 25 compared to the exempt 1979 cohort and that employment rates are similar at the age of 26. For example, at age 17 the likelihood of men being observed with a wage is 0.078 for the 1978 cohort compared to 0.093 for the 1979 cohort, a 1.5 percentage point or 0.015 year difference.[28] Accumulated, these differences result in an average 0.4 year loss ($\frac{d\hat{Exp}}{de} = -0.4$) of early labor market experience for men born just before January 1, 1979.[29] These results suggest that men exposed to the threat of conscription lost an average of 0.4 years of early labor market experience despite spending an extra 0.6 years in school and the military. This disparity could be due to the fact that individuals born

[27] For example, the 1998 LFS enables me to estimate the likelihood of employment for 20 year old men born in 1978 and 19 year old men born in 1979.

[28] The 1979 cohort had a 9.3 percent employment rate at age 17. Assuming these individuals were employed all year, then men born in 1979 had an average of 0.093(1)+(1-0.093)(0) = 0.093 years of experience at age 17. Similarly 17 year olds born in 1978 had 0.078 years of experience. This results in a 0.015 year difference for the 1979 cohort.

[29] The 1979 cohort has, on average, 0.015, 0.015, 0.036, 0.045, 0.101, 0.1, 0.05, 0.028, 0.01 and 0.003 years more employment at the ages of 17 through 26 respectively in comparison to the 1978 cohort.

before the cutoff were less likely to take time off before transitioning to the labor market because of late entry or it could be due to measurement issues in the work experience variable. Combined, the effects of conscription eligibility on the various outcomes of interest reduce equation (3) to the following:

$$0.4\frac{\partial W}{\partial S} + 0.2\frac{\partial W}{\partial C} - 0.4\frac{\partial W}{\partial Exp} = 0 \tag{4}$$

Accordingly, to further understand why the documented increase in schooling did not lead to positive returns, a more thorough understanding of the potential effects of conscription on wages ($\frac{\partial W}{\partial C}$) is needed. In order to shed light on the direct effect of conscription on wages, I rely on further analysis from individuals from lower socioeconomic backgrounds—a group whose education decisions are exogenous to treatment. Indeed, as I show in section 5.3, there is no statistically significant threshold crossing effect on education for this sample (i.e. $\frac{\partial \hat{S}}{\partial e} = 0$). Further, I find no evidence of a discontinuity in wage for this subgroup ($\frac{\partial \hat{W}}{\partial e} = 0$). Additionally, Table 1 suggests that individuals from poorer households were conscripted at the same rate as those from richer backgrounds and I also estimate that low S.E.S individuals born prior to January 1, 1979 lost an average 0.2 years of work experience. Accordingly, equation (4) simplifies to the following for low S.E.S men:

$$0.2\frac{\partial W}{\partial C} - 0.2\frac{\partial W}{\partial Exp} = 0 \implies \frac{\partial W}{\partial C} = \frac{\partial W}{\partial Exp} \tag{5}$$

Since wage is generally found to be non-decreasing in experience (i.e. $\frac{\partial W}{\partial Exp} \geq 0$), then equation (5) implies that $\frac{\partial W}{\partial C} \geq 0$. This would be true if a 2.5 month increase in conscription has no effect on earnings ($\frac{\partial W}{\partial C} = 0$) or if it has a positive impact on earnings ($\frac{\partial W}{\partial C} > 0$) that was offset by an equal loss of early labor market experience.

To shed light on which of the above two scenarios is more likely, I next focus my analysis on the entry wage of individuals from poorer households, since entry wage is not affected by labor market experience ($\frac{\partial W}{\partial Exp} = 0$). As a result, any documented discontinuity in the entry earnings of low S.E.S men could be attributed to the direct effect of conscription. Table A9 of the online Appendix reveals that low S.E.S individuals born just before January 1, 1979 have no entry wage premium compared to those born after this date despite having served in the military. This suggests that conscription has no direct impact on wage. However, while this estimate is statistically insignificant, it is quite imprecise; I am unable to rule out wage effects as large as 12 percent. This precludes me from making any definitive statements on the direct effect of conscription on earnings. Accordingly, I focus on two general cases for the overall sample. Conscription can either have a zero or positive effect on earnings.[30]

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

Assuming that serving in the military (C) has no direct effect on wage, equation (4) reduces to the following:[31]

$$0.4\frac{\partial W}{\partial S} - 0.4\frac{\partial W}{\partial Exp} = 0 \implies \frac{\partial W}{\partial S} = \frac{\partial W}{\partial Exp} \tag{6}$$

Equation (6) states that the documented 0.4 year increase in schooling had no effect on wage because it was offset by a 0.4 year reduction in early labor market experience. Put differently, men induced into education—to avoid conscription—did not benefit from this additional schooling because they lost equally valuable early work experience.

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

Assuming that conscription has a positive direct effect on wage, then equation (4) reduces to the following:

$$0.4\frac{\partial W}{\partial S} - 0.4\frac{\partial W}{\partial Exp} = -0.2\frac{\partial W}{\partial C} \implies \frac{\partial W}{\partial S} < \frac{\partial W}{\partial Exp} \tag{7}$$

Equation (7) implies that marginal students induced into education, due to conscription, were harmed by this decision. Indeed, since the return to 0.4 years of education is lower than the return to 0.4 years of early market experience then substituting additional schooling for early work would have led to higher wages.

In the above two cases, and under the assumption of homogeneous returns to military service across socioeconomic groups, I find that students induced into additional education were most likely better off acquiring early labor market experience. This presents a puzzle, as most studies find significant and positive returns to education—even after accounting for loss of early work experience. These studies generally focus on exogenous variation in the supply of schooling (See Card, 1999 for examples). Conversely, in my context, conscription led to a change in the demand

[30]This assumes homogeneous conscription effects across the population. Indeed, if individuals from high S.E.S backgrounds are negatively affected by conscription relative to low S.E.S individuals, then this conclusion may not hold.

[31]A recent study by Jorgensen & Breitenbauch (2009) finds that virtually all French individuals believed that there were no advantages to be gained from including military service experience on your curriculum vitae.

for education for individuals who wanted to avoid military service. Accordingly, one explanation for the lack of educational returns in my study is that the average marginal return to schooling 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.[32] This suggests that 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 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. Additionally, Öckert (2010) also finds little returns to college admissions in Sweden citing government polices that led to inflated enrollment at colleges as a possible factor.

7 Conclusion

This paper contributes to the returns to conscription literature by estimating the effects of conscription eligibility on education and labor market outcomes. To do so, I leverage the fact that men born just before January 1, 1979 had to serve in the military, while those born just after that date were exempt. I find that men barely eligible for conscription obtain approximately 4 to 6 additional months of education with evidence suggesting that this occurs primarily through increased high school graduation rates. Further analysis reveals that this result is driven by individuals from wealthier backgrounds. Finally, I find no reduced form effects on employment and wages for men at the ages of 30 to 36. These results are robust to varying bandwidths and functional forms.

I conduct further analysis to understand why this increase in education did not lead to positive labor market returns. Under certain assumptions, I show that the interpretation most consistent with findings is that this increased schooling did not provide a benefit that outweighed the loss of early labor market experience. This is most likely because the average marginal return for education induced by conscription policy was low and did not sufficiently contribute to human capital formation.

Finally, while the returns to education for non draft related reasons may still be positive, the

[32] Unfortunately, I do not posses data on quality of educational institution, so I am not able to directly test for the first mechanism.

finding of no substantial return to schooling suggests that there should be an increased focus on understanding and measuring why and how more schooling matters. This is important because different mechanisms have substantially different policy implications. For example, if increasing the demand for more education by artificially inducing more children into schooling does not lead to a benefit in labor market outcomes, then resources would be better spent on policies expanding the supply of education, perhaps through increased school construction in impoverished neighborhoods.

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A Figures

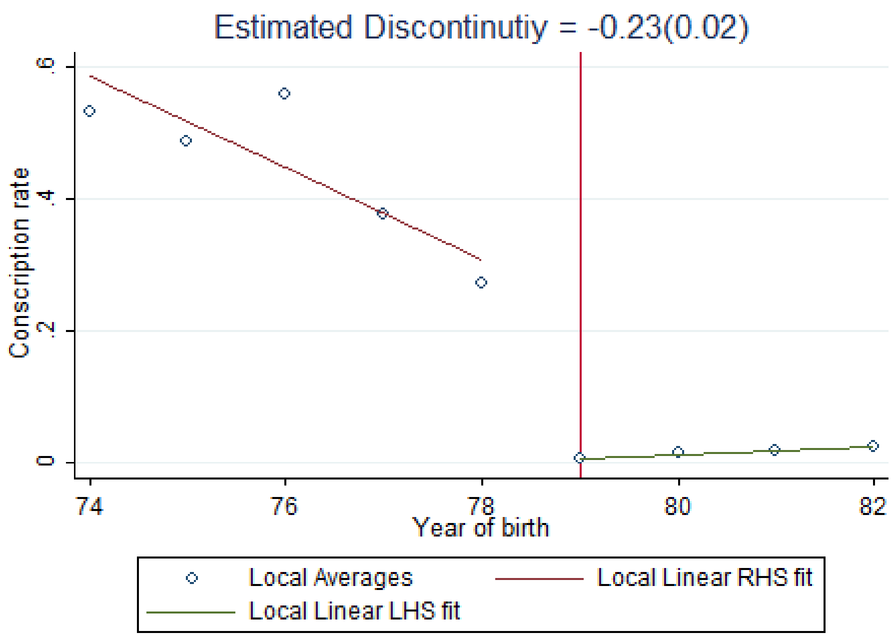
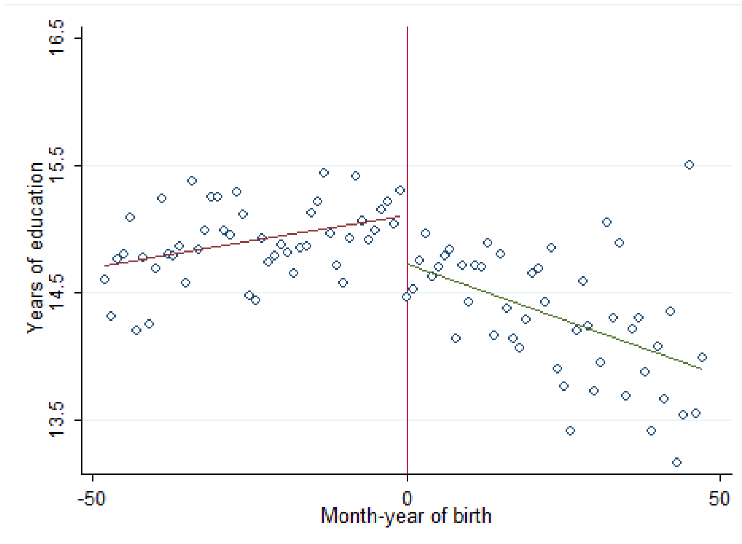
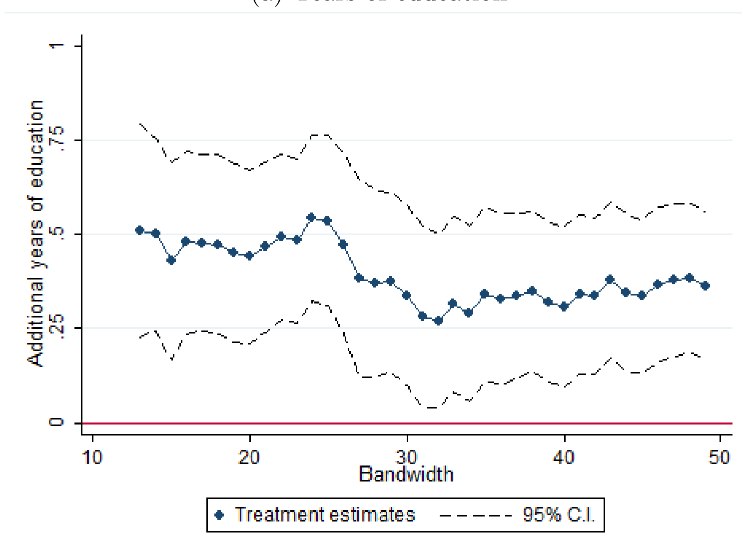


Figure 1: Likelihood of conscription based on birth year relative to Jan 1, 1979 cutoff.

- Notes: Sample includes French male citizens and is taken from the Generation 1998 à 10 ans survey. Standard errors reported in parantheses.



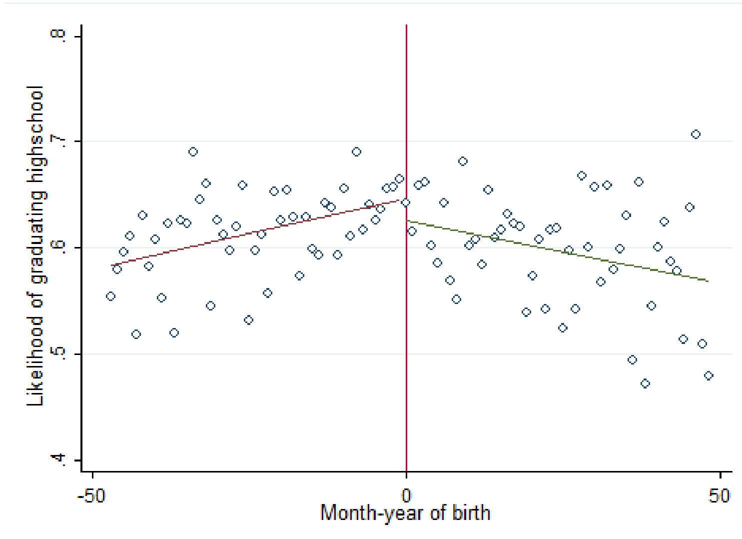
(a) Years of education



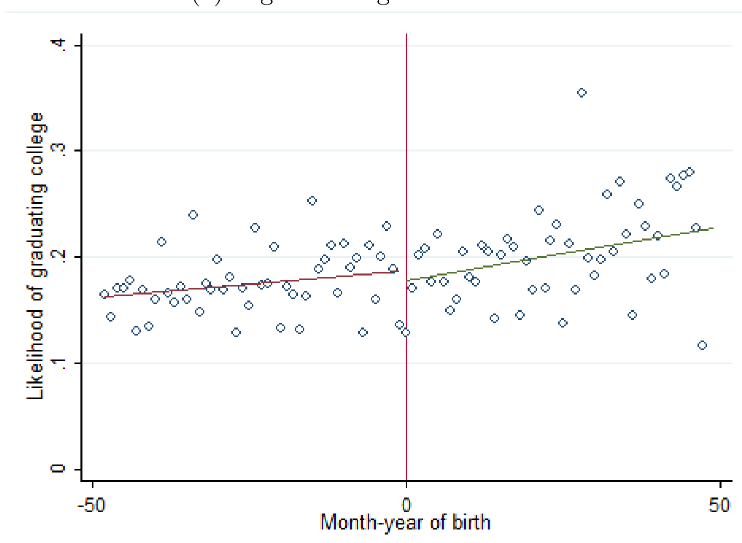
(b) Bandwidth sensitivity analysis for years of education.

Figure 2: Years of education estimates based on birth month relative to Jan 1, 1979 cutoff.

- Notes: Sample includes 30 to 36 year old French male citizens who are in the labor force. Uniform Kernel weights used for local linear regressions. For figure 1B: solid and dashed lines represents the point estimates and 95% confidence intervals respectively.



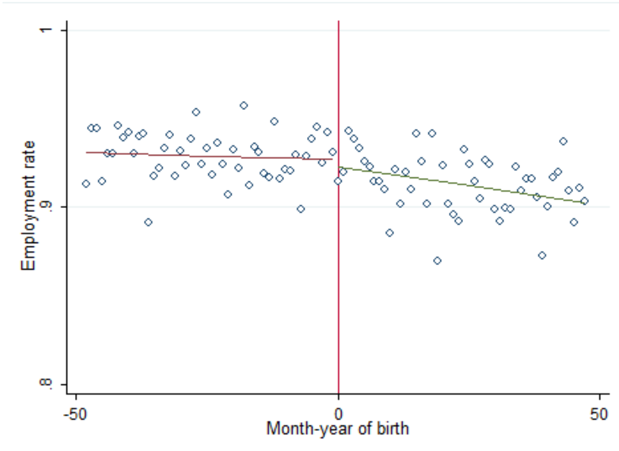
(a) High school graduation rates



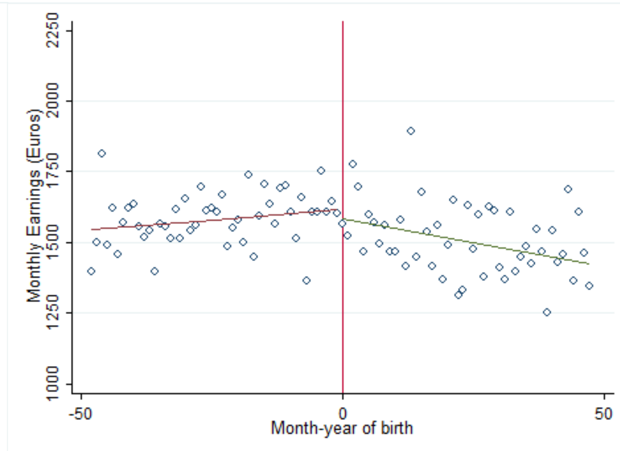
(b) College graduation rates

Figure 3: Graduation outcomes based on birth month relative to Jan 1, 1979 cutoff.

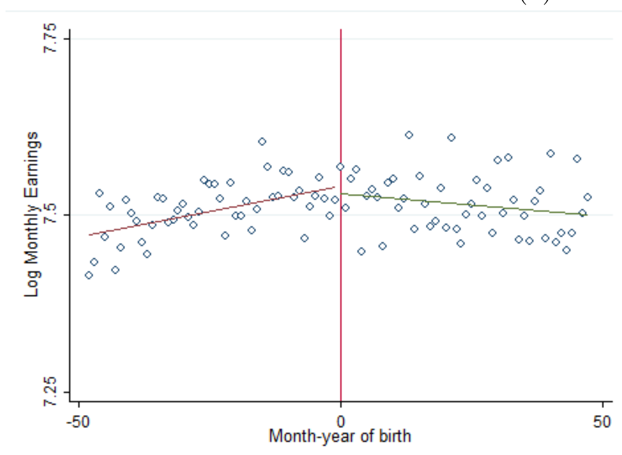
- Notes: Sample includes 30 to 36 year old French male citizens who are in the labor force. High school graduation includes all forms of baccalaureate degrees (technical, professional, general).



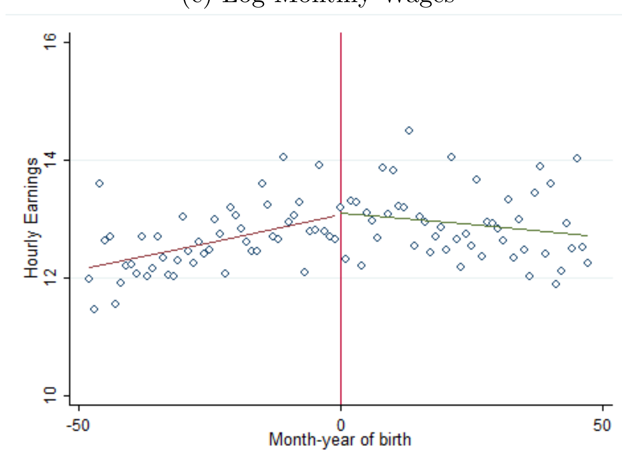
(a) Employment Rates



(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

Notes: Sample includes 30 to 36 year old French male citizens who are in the labor force. Wage includes zero earning individuals (unemployed), but log wages drops those unemployed. Hourly wages are conditional on at least 10 weekly hours of work

B Tables

Table 1: Summary statistics for 30 to 36 year old French male citizens of birth cohorts 1975 to 1982

Variable	**Mean**
Years of education	14.653 (3.107)
Monthly earnings (in Euros)	1508.012 (1221.365)
Employment rate	0.905 (0.278)
Log monthly earnings	7.501 (0.429)
Hourly wage (in Euros)	12.670 (6.420)
Hours worked per week	39.392 (8.095)
Middle school enrollment rate	0.995 (0.070)
High school graduation rate	0.590 (0.491)
University graduation rate	0.187 (0.390)
High socioeconomic status (S.E.S) rate	0.353 (0.477)
Observations	24,414

Source: French Labor Force Surveys (2003–2015)
Mean outcomes reported; Standard deviation in parentheses
Sample includes 30 to 36 year old French male citizens who are in the labor force.
Socioeconomic status (S.E.S) is determined by fathers' occupation. Specifically, a student is defined as coming from a low S.E.S background if his/her father is in a low skilled/manual job. High S.E.S individuals are those whose fathers were employed in medium/high skilled specialized jobs.

Table 2: Regression discontinuity estimates for years of education using different bandwidths and specifications.

Bandwidth	12 months 1	24 months 2	36 months 3	48 months 4	60 months 5	Post-reform (Control) mean
Panel A: Years of education	.389***	.562***	.303***	.357***	.419***	14.64
	(.093)	(.113)	(.114)	(.098)	(.124)	
Panel B: Years of education (+ controls)	.317***	.357**	.219*	.297**	.275*	14.64
	(.061)	(.140)	(.126)	(.124)	(.145)	
Observations	6,773	13,467	19,426	24,374	28,624	
Month Polynomial	Zero	One	One	One	Two	

Notes: Sample includes 30 to 36 year old French male citizens who are in the labor force.
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. of the cutoff. Post reform (Control) mean is the average outcome just to the right of the cutoff.
Controls include: 1) Birth month fixed effects 2) A binary variable for middle school enrollment 3) Socioeconomic status. 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 3: Regression discontinuity estimates for college/high school graduation rates using different bandwidths and specifications.

Bandwidth	12 months 1	24 months 2	36 months 3	48 months 4	60 months 5	Post-reform (Control) mean
Panel A: High school graduation	.024*	.025	.019	.023*	.012	0.62
	(.013)	(.015)	(.015)	(.013)	(.017)	
Panel B: University graduation	.005	.008	.015	.014	.002	0.17
	(.012)	(.019)	(.015)	(.013)	(.012)	
Observations	6,773	13,467	19,426	24,374	28,624	
Month Polynomial	Zero	One	One	One	Two	

Notes: Sample includes 30 to 36 year old French male citizens who are in the labor force.
Each cell represents a separate regression with graduation rates as outcomes 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. Post reform (Control) mean is the average outcome just to the right of the cutoff. High school graduation defined as finishing any Baccalaureate type (Professional, technical, general). 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 4: Regression discontinuity estimates for labor market outcomes using different bandwidths and specifications.

Bandwidth	12 months 1	24 months 2	36 months 3	48 months 4	60 months 5	Post-reform (Control) mean
Panel A:						
Employment likelihood	0.023	0.009	0.014	0.009	0.008	0.915
	(0.014)	(0.017)	(0.016)	(0.014)	(0.018)	
Panel B: Monthly wages	-20.306	-0.167	34.039	32.519	26.506	1602
	(65.991)	(53.823)	(46.056)	(42.559)	(49.988)	
Panel C: Log wages	0.002	-0.006	0.004	0.012	-0.002	7.53
	(0.014)	(0.019)	(0.016)	(0.015)	(0.018)	
Panel D: Hourly wages	-0.100	-0.116	0.003	-0.035	-0.029	12.9
	(0.206)	(0.277)	(0.233)	(0.216)	(0.264)	
Observations	6,773	13,467	19,426	24,374	28,624	
Month Polynomial	Zero	One	One	One	Two	

Notes: Sample includes 30 to 36 year old French male citizens who are in the labor force.
Each cell represents a separate regression with labor market outcomes 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. Post reform (Control) mean is the average outcome just to the right of the cutoff. Wage includes zero earning individuals (unemployed), but log wages drops those unemployed. Hourly wages are conditional on at least 10 weekly hours of work. 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: Local linear 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	High S.E.S	High S.E.S	High S.E.S
Bandwidth	24 months	36 months.	48 months	24 months	36 months	48 months
Panel A: Discontinuity						
in years of educ.	.303	.186	.250	.890***	.604**	.553**
	(.21)	(.17)	(.16)	(.32)	(.28)	(.25)
Panel B: Discontinuity						
in high school grad.	–.023	–.005	.013	.065*	.053*	.057**
	(.024)	(.022)	(.020)	(.033)	(.031)	(.028)
Panel C: Discontinuity						
in college grad.	–.004	.009	.012	.018	.026	.027
	(.015)	(.013)	(.012)	(.047)	(.036)	(.032)
Panel D: Discontinuity						
in employment	.017	.018	.007	–.018	–.012	–.004
	(.011)	(.013)	(.009)	(.027)	(.011)	(.010)
Panel E: Discontinuity						
in monthly wages	90.346	71.824	41.128	–190.257	–92.718	–8.734
	(57.631)	(54.036)	(48.222)	(120.469)	(98.098)	(90.755)
Panel F: Discontinuity						
in logged wages	.011	.008	.017	–.048	–.027	.004
	(.021)	(.019)	(.018)	(.038)	(.031)	(.031)
Panel G: Discontinuity						
in hourly wages	.127	–.005	.041	–.976	–.513	–.259
	(.236)	(.222)	(.205)	(.773)	(.642)	(.576)
Observations	6,790	9,934	12,511	3,707	5,226	6,519

Sample includes 30 to 36 year old French male citizens who are in the labor force.
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'. All specifications control for a flexible polynomial of age in which the slope is allowed to vary on either side of the cutoff. Socioeconomic status (S.E.S) is determined by fathers' occupation. Specifically, a student is defined as coming from a low S.E.S background if his/her father is in a low skilled/manual job. High S.E.S individuals are those whose fathers were employed in medium/high skilled specialized jobs. Standard errors are clustered at the month-year level and reported in parentheses. All regressions have been weighted by population. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$