THREE ESSAYS IN THE ECONOMICS OF EDUCATION

A Dissertation

by

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ABSTRACT

This dissertation introduces three essays on the short and long run consequences of educational choices. In the first essay "Conscription and the Returns to Education: Evidence from a Regression Discontinuity" we use a regression discontinuity design to first 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.

In the second essay "Quality of Higher Education and Earnings: Regression Discontinuity Evidence from the French Baccalaureate", we use a regression discontinuity design to examine the returns to quality of postsecondary education. We compare the outcomes of students who marginally pass and fail the first round exams of the French Baccalaureate, a degree that students must earn to graduate from secondary school. Marginally passing increases the likelihood of attending a higher quality university and a STEM major. Threshold crossing also increases earnings by 13.6 percent at the age of 27 to 29. After ruling out other channels that could affect earnings, we conclude that increased access to higher quality postsecondary education leads to a significant earnings premium.

In the third and final essay "Better or Best? High School Quality and Academic Performance" we look at the effects of attending a higher quality high school on the academic performance and college outcomes of young Chinese students. Specifically, in our analysis, we draw a distinction between going to a better school, regardless of tier, and going to a top-tier school. We find that college entrance exam test score gains and improved college outcomes are only realized for individuals attending the most elite set of high schools.These results are mainly driven by males as we find no significant effects on academic performance for females. Finally, we provide evidence suggesting that these academic gains are mostly due to variation in teacher quality.

DEDICATION

To My Parents;

None of this would have been possible without you and for that I am forever indebted.

To Serena;

For changing my life and making me want to be a better person.

To Louis, Leon, Wassim, Ronald and Francois;

I wouldn't be the person I am today without you guys.

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1. GENERAL INTRODUCTION

Over the past few decades, access to education has become more prevalent. For example, in the United States, the percentage of 18 to 24 year olds enrolled in postsecondary institutions increased from 25.7% in 1970 to 41% in 2012. With education becoming more attainable, students are now also encouraged to seek higher quality education, as it is associated with significant gains in the labor market. In fact, parents in the United States spend a large amount of resources to help their children gain access to "better" education. As a result, it is of equal importance to understand the short and long run gains to attending more education as well as better quality education. Naturally, a few questions arise: Do all policies that incentivize students to get more education work? Does attending better quality schooling result in significant academic and labor market gains? If so, do these results extend to all types of students and all types of institutions?

This dissertation attempts to answer these questions through three essays on the short and long run consequences of educational choices. One of the biggest challenges in studying the effects of educational choice on later lifetime outcomes is that these choices are effectively non-random. For instance, students with higher ability, more motivation, better parental support are more likely to make "better" educational choices. Furthermore, these attributes are usually unobservable to the researcher and cannot be fully controlled for. As a result, it is hard to understand whether it is the aforementioned attributes of these students as opposed to their educational choices that ultimately leads to better lifetime outcomes. To overcome this problem, we use a regression discontinuity design (RDD) as the main identification strategy in all three essays. This allows us to overcome the problem of selection into education by comparing two groups of students who are similar in observable and unobservable characteristics, thus isolating the effects of education from confounding factors.

The first essay (Chapter 2) looks at the labor market returns to increased educational attainment—brought about by the elimination of conscription policy in France. While, the general consensus in the literature is that more education leads to better lifetime outcomes, we provide evidence suggesting that this is not always the case. Specifically, we find that an increase in the quantity of education, due to draft avoidance behavior, does not necessarily lead to positive labor market outcomes. While education has positive effects in general, these results suggest that we should be wary of certain policies that induce individuals to attain more education than desired.

The second essay (Chapter 3) looks at the effects of increased access to better quality higher education on labor market outcomes for the low-skilled student. Previous research has documented significant gains from attending the most selective postsecondary institutions. However, less is known about the extent to which these gains are realized for the low skilled student who does not necessarily attend the most selective of institutions. We find a substantial wage premium for academically marginal students who attend better quality post secondary education characterized by better performing peers and increased enrollment in STEM majors. Specifically, we uncover a 13.6 percent wage premium attributed to increased access to higher quality schooling. This suggests that policies geared towards increasing lower skilled students' access to better quality education can result in significant labor market gains.

The final essay (Chapter 4) looks at the effect of increased access to better quality high schools on short run academic performance. Specifically, we find substantial test score gains from attending elite high schools which in turn translates into improved college outcomes. Strikingly, these results do not extend to better quality institutions that are not designated as tier-1 schools, indicating that academic gains are only realized for those attending the best of institutions. This finding holds despite the fact that attending both types of institutions leads to a similar increase in the average quality of peers. We provide for suggestive evidence indicating that these results are driven by teacher quality as opposed to peer quality, indicating that teacher quality may matter more for cognitive outcomes.

2. CONSCRIPTION AND THE RETURNS TO EDUCATION: EVIDENCE FROM A REGRESSION DISCONTINUITY

2.1 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; 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 chapter 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

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

to do so was by staying enrolled in an educational institution. In May of 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 us 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 us to overcome the selection bias 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; we find no discontinuity in percentage of degrees attained at the high school or college level. Strikingly, we also find no significant effect on employment or future earnings. The results are robust to different bandwidths and estimation procedures.

We 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 our 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

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

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 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 the educational effects of conscription and find that exemption from service lead to a 3 percentage point reduction in high school graduation rates in Italy. Di Pietro (2009) also analyzes the educational effect 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 the long run earnings of post World War II conscripts in England and Germany respectively. Both papers use regression discontinuity designs based on year and day of birth respectively and 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. (2014) find that this behavior did exist in Germany.

Our 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, we 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, we 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, we 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, our paper finds no evidence of a conscription effect on either graduation rates or earnings, which contrasts significantly with their study.[4]

This paper makes several contributions relative to the existing literature. First, we present estimates that speak to the impact of conscription policy on both education and labor market outcomes. Particularly, we do so using a compelling research design that enables us 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

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

[4]We 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.

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. We 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.

2.2 Mandatory military service 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. 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 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 A1 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. 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] Figure A1 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 23 percentage points more likely to have served in the military. Additionally, Table A1 also reveals that males from different socioeconomic backgrounds (S.E.S) were not serving at disproportionate levels. More specifically, conscription

[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.

[8]If these cohorts stayed on in education till the ages of 23 and 24 respectively, then they would be permanently exempt from serving.

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.

2.3 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).[9] We 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.

From the original sample of 420,000 observations, we drop all females and non-French citizens. We 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 A2. Respondents have an average of 14.77 years of education, which corresponds to the number of

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

years individuals are observed within educational institutions.[10] 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%.[11] For a detailed illustration of the available educational routes in the high school and higher education system, refer to Appendix Figures A2 and A3. 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, we 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.[12] Following Maurin & Thesmar (2004), our definition of high skilled workers includes the first two groups (a and b), while low skilled workers are in the last two (c and d).

[10]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.

[11]We 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.)

[12]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).

2.4 Identification strategy

We 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, we 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, we 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, we 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.

We use population survey weights in order to estimate the treatment parameter τ for the various outcomes.[13] Further, standard errors are clustered at the month-year level as suggested in Lee & Card (2008).[14] 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, we 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, we rely on intent to treat (ITT) estimates and only report reduced form results. The reason we 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, reweighing the reduced form estimates for labor market outcomes by the discontinuity in conscription rates in a Two Sample Two-Stage Least Squares framework (TS2SLS)

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

[14] 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.

relies on a tenuous exclusion restriction, seeing as conscription eligibility has a dual effect on education and earnings.

2.5 Results

2.5.1 Education

We first examine the impact of mandatory conscription on years of education.[15] 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 A4 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. We 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.[16] 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.[17] The optimal bandwidth for the local linear regression was selected using a robust data driven procedure (Henceforth CCT).[18]

[15]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.

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

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

[18]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

We 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 A4.

Corresponding regression estimates are shown in Table A3. All standard errors are clustered at the month-year level. Panel A depicts discontinuity estimates using different bandwidths 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—when the bandwidth chosen contains more than a year of data, 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 A5 and A6 of the appendix, we account for the addition of these exogenous controls by plotting the residuals from years of education regressions. We 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, we examine whether this increase in years of education resulted in an increase in degree attainment, as measured by high school and college graduation rates.[19] Figures A7 and A8 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

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.

[19]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. We also check for discontinuities at the masters/doctorate level and find none.

of 108 months. Table A4 depicts estimates for varying bandwidths and functional forms, with all results remaining statistically insignificant. In summary, we 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.

2.5.2 Labor market outcomes

We now turn to whether this increase in education quantity for those barely subject to conscription yields positive labor market returns. First, we check to see if this policy generated any sharp changes in the likelihood of employment. All regressions in Figure A9 use a population weighted quadratic fit over 108 months of data on either side of the Jan. 1, 1979 cutoff. In Panel A, we find an insignificant 1.9 percentage point change in employment rates for those who were barely eligible for conscription.[20] Corresponding regression estimates are shown in Panel A of Table A5 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. We conclude that being eligible for conscription does not seem to have significantly affected the likelihood of employment.

Next, we look at whether there are any significant effects on earnings for those barely subject to conscription. We begin by looking at monthly wages that include zero earners. Panel B of Figure A9 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 A9 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

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

behavioral responses in hours worked. We check for this by looking at any treatment effects on hourly wages. As can be seen from Panel D, we 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 A5. The results remain statistically insignificant over varying bandwidths and functional forms. We 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, we also run quantile treatment effect regressions on monthly earnings for the whole population of interest. We 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 A10 of the appendix.

2.5.3 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, we look at treatment effects for individuals from high versus low socioeconomic backgrounds to assess if one was differentially affected by this policy.[21] Table A6 depicts results for global polynomial and local linear estimates. The bandwidth varies with each outcome variable when reporting the local linear

[21] Differential selection into mandatory conscription by socioeconomic status could potentially bias these results. However, as shown in Table A1, we find no evidence of this.

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 A6. In contrast, we find that high S.E.S individuals take on significantly more education. Specifically, in columns 5 and 7, we 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.[22] 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. We also estimate a zero treatment effect for the two subgroups with respect to monthly, hourly, and logged wages. We 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.

2.5.4 Robustness checks

In this section, We 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

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

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 A11 depicts the distribution of observations in the sample, with no large mass evident to the right of the cutoff.[23]

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. We test for this by examining where there is a documented discontinuity in fathers' occupation or in middle school enrollment rates.[24] Results depicted in Figures A12 and A13 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 A7.

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

[23]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...

[24]Middle school enrollment rates should not be affected by the policy since those students are not yet eligible for conscription. Further, we 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.

other policy driving the results. Panels A to F of Figure A14 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, we report all outcomes over varying bandwidths and functional forms in Table A8. 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, we 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 our knowledge, no such policy relative to that specific birth cohort exists.[25] 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,

[25]We 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.

when analyzing the effects of conscription.

Finally, we check for discontinuities at pseudo cutoff dates around the original threshold. To do so, we 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 A15 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.[26] 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, we observe another 5 significant estimates out of the remaining 100 fake cutoff months.[27] 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.

2.6 What does this mean for the returns to education?

So far we have estimated reduced form effects for both education and labor market outcomes. We 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, we

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

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

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 (e) and that e and conscription (C) are continuous variables over the real line.[28] Further, assume that schooling (S = S(e)) is an increasing function of eligibility.[29] 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:

$$\frac{dW}{de} = \frac{\partial W}{\partial S} * \frac{dS}{de} + \frac{\partial W}{\partial C} * \frac{dC}{de} \tag{2.1}$$

Rearranging (1):

$$ReturnstoSchooling = \frac{\partial W}{\partial S} = \frac{(\frac{dW}{de} - \frac{\partial W}{\partial C} * \frac{dC}{de})}{\frac{dS}{de}} \tag{2.2}$$

Consistent with the previous literature on the topic, we focus on two general cases. Conscription can either have a zero or negative effect on wages.[30]

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

Assuming that serving in the military (C) has no direct effect on wages, equation 2.2 reduces to a traditional returns to education interpretation and is just the ratio

[28] 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.

[29] 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.

[30] 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).

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

Assuming that conscription has a negative effect on wages, then equation 2.2 no longer simplifies to a traditional returns to education interpretation. However, a bound for plausible returns to education estimates can be derived from equation 2.2. We know that $\frac{d\hat{W}}{de} = 0$ and $\frac{d\hat{S}}{de} = 0.5$. The compliance ratio can be estimated at $\frac{d\hat{C}}{de} \approx 50\%$.[31] 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), we estimate a bound to the order of $\frac{\partial W}{\partial S} \approx 2.5\% - 5\%$ returns to an additional year of education for the population of interest.[32]

In order to shed light on which of these possible interpretations is more likely to be true, we look at two subgroups whose education decisions are exogenous to treatment. As highlighted in section 2.5.3, there is no threshold crossing effect on education for individuals from low socioeconomic backgrounds. On the other hand, we 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.[33]

To complement these results, we also look at another subpopulation whose educa-

[31]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.

[32]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.

[33]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).

tion is orthogonal to treatment but who are ultimately still eligible for conscription. Specifically, we 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 A16 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 A17, which shows no effect on earnings. While these results are more suggestive than conclusive given that we are focusing on a very specific subgroup of individuals, they reinforce the conclusion that military 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 A18 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 returns to military service for our 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, our results suggest that the only potential effect conscription policy could have on earnings is through its effect on education. We 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.

2.7 Conclusion

This chapter 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. We use a regression discontinuity design to overcome selection bias attributed to serving in the military. We find that those barely eligible for conscription obtain approximately 6 additional months of education as a result. Specifically, We 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, We find no effects on employment rates, monthly, hourly or logged earnings. These results are robust to varying bandwidths and functional forms.

We 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, we 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.

3. QUALITY OF HIGHER EDUCATION AND EARNINGS: REGRESSION DISCONTINUITY EVIDENCE FROM THE FRENCH BACCALAUREATE

3.1 Introduction

An emerging body of literature looks at whether quality of postsecondary education has a significant impact on students' future labor market outcomes. Previous studies find large returns to the most selective institutions and degrees. However, it is unclear whether these effects persist for academically marginal students who attend mildly selective universities which differ in quality. This is an important question as most students do not decide between attending an elite versus a non-elite university. In fact, admission rates at most "Ivy League" institutions in the U.S. are lower than 10%. In France, less than 4% of students in 2013 enrolled in preparatory classes for elite universities. It is also of equal importance to understand how choice of major within an institution can affect future labor market outcomes. Recent reports suggest the presence of a persistent and growing wage premium for jobs in the fields sof science, technology, engineering, and mathematics (STEM) (Langdon et al., 2011). From a policy perspective, these fields are perceived to be the basis for innovation and governments have been increasingly investing in STEM education. Finally, there is also a need to understand the combined effect of university quality and major choice, as students often make these decisions simultaneously.

This chapter looks at the impact of the quality of higher education on labor market outcomes for the academically marginal student. In our context, quality of higher education refers to both quality of university attended and field of study pursued—where institution quality is proxied peer quality. This matters, as students in most countries often decide on an institution and field of study simultaneously. To do so, we exploit a unique feature of the French education system, the Baccalauréat Général or the General Baccalaureate.

The Baccalaureate is a national diploma granted to students in France upon graduating from secondary school and is required for university enrollment. In order to earn the degree, students have to sit for and pass national exit exams during their last academic year. Students are generally given two attempts to pass these "high stakes" exams within the same year. However, the standards for passing are significantly higher in the first round. We use a regression discontinuity design that compares the future educational and labor market outcomes of students who barely pass and barely fail the exam from the first attempt. This allows us to overcome selection bias arising from the fact that post-secondary educational choices are likely correlated with unobservable factors that may also affect future earnings, such as ability and motivation.

Results indicate that threshold crossing leads to an improvement in the average peer quality that students are exposed to in college, to the order of 0.11 standard deviations in Baccalaureate scores. We also find that marginally passing causes a 15.9 percentage point increase in the likelihood of pursuing a STEM degree in any university. Further, we uncover a 13.6 percent earnings premium associated with threshold crossing, approximately 9 to 10 years after having initially taken the exam, with no significant employment effects. We rule out other possible channels through which threshold crossing may affect earnings. Specifically, we find no significant effect on years of post-baccalaureate education, or on the likelihood of obtaining a post-baccalaureate degree. Moreover, we find no discontinuity in the probability of eventually obtaining the Baccalaureate degree. This rules out the direct signaling value of the Baccalaureate degree as a potential channel that could have contributed to the documented increase in earnings. Accordingly, we conclude that increased access to better quality postsecondary education raises earnings by 13.6 percent for academically marginal students.

Our paper is related to a large body of literature that provides estimates on the returns to quality of higher education. Dale and Krueger (2002, 2014) compare students who were accepted at similar universities, but chose to attend different colleges. They find that the earnings gains from more selective colleges are restricted to students from low socio-economic backgrounds and minorities. Brewer et al. (1999) and Black and Smith (2004, 2006) show that there are significant returns to attending more selective universities. Our study is closest to more recent work which uses regression discontinuity designs to identify the returns to quality and quantity of education. Hoekstra (2009) finds that attending a flagship state university increases earnings by 20 percent for white males. Saavedra (2009) uncovers an increase in the earnings and employment rate of applicants who are marginally above the entry cutoff at selective universities in Colombia. Zimmerman (2014) estimates the returns to attending a four year university by comparing students who are marginally above and below the admissions cutoff at the least selective university in Florida. Recent studies also use regression discontinuity designs to estimate the returns to different fields of study. Hastings et al. (2013) and Kirkbøen et al. (2014) exploit the existence of varying admissions cutoffs to certain majors at selective universities in Chile and Norway respectively. They find heterogeneous returns by field of study and positive returns for the most selective of degrees.

We contribute to this literature in several ways. First, our focus is on universities that are moderately selective but which slightly differ in quality amongst each other. In fact, students who marginally pass the General Baccalaureate exam do not normally attend elite universities in France. This is in contrast to the rest of the literature which usually estimates the returns to the most selective universities. Thus, it is interesting to find that even among universities that are of moderate selectivity, there are positive returns to attending a higher quality institution.

Furthermore, our estimates concern students with moderate academic abilities. This is potentially important as the returns to college quality can be heterogeneous (Andrews et al., 2012). Goodman et al. (2014) also focus on the benefits of higher quality education for lower ability students. They use the admissions cutoffs at various universities in Georgia and find that marginally missing these cutoffs decreases BA completion rates. However, they do not analyze any subsequent labor market effects. Second, we examine the returns to quality of postsecondary education using an entire national university system. Previous studies focused on the effects of attending a single institution or a subset of universities within a country. Additionally, our data allows us to track the complete educational paths for all students in our sample.

Third, we estimate that at least part of the observed effect of marginally passing on earnings is driven by pursuing STEM-designated degrees. We further provide suggestive evidence that the returns to STEM education, holding quality of institution constant, is 43 percent for students from lower socioeconomic backgrounds. The magnitude of this effect is comparable to estimates from previous studies in the U.S. Hamermesh and Donald (2008) estimate that the earnings of students who majored in engineering and hard business are respectively, 31.6 and 48.9 log points higher than those who majored in education. Altonji et al. (2012) suggest a wage premium of 56.1 and 51.8 log points for those who studied electrical engineering and finance relatively to education. Melguzio and Wozniak (2012) estimate a 25% to 40% STEM related premium for high achieving minority students. Further, they find a 50% STEM premium for individuals who ended up working in a congruent field.

This paper is related to another strand of literature which explores the effects of educational accountability programs on student outcomes. Recent studies show that the introduction of test-based accountability, like high school exit exams, increases the performance of students who are still in school (Jacob, 2005; Figlio and Rouse, 2006; Chiang, 2009). Other papers find that exit exams can increase high school dropout rates (Ou, 2010; Papay et al., 2010) and even reduce post-secondary educational attainment (Martorell, 2004).

We add to this literature in multiple ways. First, we find that marginally failing the exit exams from the first attempt does not have an impact on graduating from secondary school, graduating with a post-baccalaureate degree or years of education. This suggests that exit exams do not necessarily discourage students through increasing dropout rates and lowering higher educational attainment. In fact, when high-stakes exams with multiple retakes are given, students may persist in order to meet the graduation standards. In our case, this is reinforced by the fact that the second round of exams has lower standards for passing than the first one.

Second, we find that exit exams can have consequences that have not yet been considered in the literature. We show that marginally passing the exit exams from the first attempt increases the likelihood of accessing higher quality postsecondary education. This is due to either late enrollement in university/major combinations that are in high demand, a discouragement or a signaling effect. In our setting, the signaling effect explanation suggests that the timing of secondary school degree receipt may be a better signal of ability than whether the student actually receives the degree. In fact, allowing students to retake the exam with lower standards for passing on the second round could be devaluing the high school credential. This could partially explain recent findings that show that there is no signaling value to a high school credential (see Clark and Martorell, 2014). If students are allowed to retake exit exams until they eventually pass, employers and universities might question the true value of this credential.

3.2 Institutional background

3.2.1 The general baccalaureate

The Baccalauréat Général (or the General Baccalaureate) is a French national degree awarded to students in their last year of secondary school. It marks the completion of secondary education and is also required for enrollment in postsecondary institutions. Within the General Baccalaureate, students can choose one of three specializations: economics & sociology, literature or sciences. Specializations differ in terms of the subject matter that the curricula focus on. For instance, students specializing in literature have a curriculum predominately focused on subjects such as French literature and philosophy even though they are still required to take all subjects.[1] The percentage of students awarded the General Baccalaureate increased from 67.2% in 1975 to 80.3% in 2002 and 92% in 2013.

In order to be awarded the degree, students must pass a series of national written exams. The exams cover all subjects taken throughout the last academic year and are common to all students within the same specialization. Written and oral exams for the French literature section of the Baccalaureate are administered a year prior to all other tests. Each subject has a different weight depending on student specialization. The weighted average of all subjects is then used to compute the final score on the Baccalaureate exam.

After the exams are administered, they are randomly assigned to preselected secondary school teachers for grading. Two committees supervise the process to guarantee uniform grading. Juries across France then meet to decide whether or not a degree is conferred. Importantly, students' identities remain anonymous throughout

[1] In the results section, we control for exam specialization fixed effects, and the results remain unchanged.

this whole process. In order to be awarded the degree, a student's total weighted score must be greater than or equal to 10 out of 20 possible points. The student is also granted an *Assez Bien* (fairly good), *Bien* (good) or *Très Bien* (very good) distinction if he/she scores above a mark of 12, 14 and 16 respectively.

Students generally have two attempts to pass the exam in a given year. A student who fails the initial attempt can opt to retake the exam in the second round, conditional on scoring at least 8 points on the first try. With a total score below 8, the student has to wait an additional year to retake the exam. Students select two failing subjects to be retested on in the second round of exams. As a result, they vary from one student to the other. The new grades on these two subjects are then added back to the remaining grades from the first round to calculate a new total score. The student is granted the degree if his/her new average score is greater than or equal to 10. The second round exams of the Baccalaureate are often criticized for being unchallenging and unreliable. This is mainly because they are conducted orally and administered by only one teacher. This allows students to negotiate a passing score with their respective teacher.

3.2.2 The jury

Following the grading of the first round exams, juries consisting of secondary school teachers decide on the conferral of the degree. A key part of the jury's role is to determine whether a person who is marginally below a certain cutoff should be given extra points to reach that threshold. If students are awarded the extra points, their final score will be pushed to somewhere between X and X.1 points, where X represents a significant threshold.[2]

[2]For example, if a student initially has a score of 9.95 and is deemed worthy of a pass, his/her final posted grade will be between 10 and 10.1. In our dataset, we can only observe this final heaped grade.

Students are usually awarded extra points on the subjects for which they obtain the lowest scores. The jury member who specializes in the corresponding subject has to consent to giving the extra points. Decisions are made in a short period of time as juries need to go through hundreds of applications on a given day. Further, the juries tend to be fairly heterogeneous in their specializations. As a result, two classmates who both marginally fail the Baccalaureate because of their scores on the mathematics portion of the exam may be passed by one jury and not the other just because the former had a teacher in mathematics, while the latter did not. Students are not allowed to interact with jury members, nor do they know that their files are being reviewed until after the results are announced. Furthermore, students' names are hidden from the jury throughout the whole process, as to hinder any cheating or bribing.

The jury members observe students' Baccalaureate exams in all subject matter. They also have the option to access an academic report which contains teachers' evaluations of the student's performance in school. While this may raise concerns over strategic jury behavior in the allocation of extra points, anecdotal evidence suggests that this option is not always exercised. Furthermore, previous studies show that the presence of test-based accountability distorts teacher behavior. For example, Jacob and Levitt (2003) provide evidence of teacher cheating on the Iowa Test of Basic Skills in Chicago elementary schools. Dee et al. (2011) also show that teachers wanting to help their students, tend to inflate test scores on New York's high school assessment exams. In our case, it is possible that teachers' desire to help students might cause them to be more lenient in their evaluations. Thus, even if jury members take into consideration the teachers' evaluations, they may still be basing their decision on an unreliable assessment of the student's performance in school.

In section 3.5.1, we provide evidence of non-strategic jury behavior. Specifically, we show the smoothness of baseline characteristics at the passing threshold. Further, in section 3.5.6, we also show that excluding the small part of the sample whose scores could have been manipulated does not change the main results.

3.2.3 The higher education system

There are many academic routes that a student can take upon graduating from secondary school. In general, students can apply to universities, higher vocational institutes or the "Grandes Ecoles" – the most prestigious and selective institutions in France. Back in 2002, there was no national centralized system that students could use to apply to higher education establishments.[3] Further, students applied to an institution and major simultaneously.

The majority of universities in France are public and offer a variety of different majors. Time to completion for most degrees is three years.[4]

By law, the only requirement for admission is holding the Baccalaureate degree. However, in practice, universities are capacity constrained and a student can be denied admission to the university and major of his choice. Priority is usually given to students who reside in the same area as the university. Other students are admitted on a "first come, first serve basis". Although public universities are not normally selective, in a recent report, the National Union of Students in France (L'Union Nationale des Etudiants de France (UNEF)) found that some universities have been using the results of the Baccalaureate exam as a screening device to select more successful applicants.

[3] Although no national centralized system was in place, students from the Île-de-France region applied to higher education establishments via a centralized system called RAVEL.

[4] Students received an intermediate degree, the "Diplôme d'études universitaires générales" (or DEUG), after two years in universities. The "Licence" (or the equivalent of the Bachelor's degree) was awarded after an extra year. Starting 2003, the DEUG was gradually phased out. However, only 13 universities had partially eliminated the degree by 2003. We are not too concerned about the effects of this reform on our sample as more than 90% of the students who failed the first round of the 2002 exams had obtained their Baccalaureate degree by 2003.

Students need to have proof of Baccalaureate receipt in order to enroll in universities. They can apply for admission well after the results of the first and second rounds of the General Baccalaureate are announced. More specifically, for the academic year 2001-2002, the first round exams took place from June 13 to June 20. Students received the results of the first round on July 5. The second round oral exams were administered from July 8 to July 11. The final results were announced on July 11. Admissions to universities are usually open until the beginning of the academic year in September.

The "Grandes Ecoles" are the most prestigious and selective post-baccalaureate institutions in France. They offer degrees in a multitude of fields including engineering, business and political sciences. Time to completion for these degrees is usually five years. Students can enroll in the "Grandes Ecoles" either immediately after secondary school or after attending two years of preparatory classes in lyceums. Admission to both these routes is based on the students' academic results in the last two years of secondary education, their scores on the French literature portion of the baccalaureate exams and tests that are specific to each institution. Admissions decisions are made before students sit for the first round of baccalaureate exams.

Admissions to vocational and professional institutes are considered competitive. Most degrees require three years to complete. Students are in general admitted based on their academic results in the last two years of secondary education or upon obtaining a distinction on the baccalaureate exams.

3.3 Data

Our data links individual-level information on secondary and post-secondary education to labor market outcomes and are taken from three surveys, the "Panel d'élèves du second degré, recrutement 1995", administered by the French statistical office (INSEE).[5] The data contains student demographics, detailed scores on the baccalaureate exams taken from administrative records, post-secondary field of study, institution attended and graduated, earnings information and employment status.

Data on post-secondary education are available on a semiannual basis for up to 9 years after receiving the General Baccalaureate degree. Labor market outcomes are reported yearly from 2005 to 2012, up to 10 years after the General Baccalaureate exams. Thus, one advantage of our dataset is that we are able to observe detailed long-term outcomes. A potential drawback of the data is that it does not include outcomes for individuals working abroad. Also, some individuals do not report their earnings or drop out of the sample because they could not be followed by the interviewers. This could potentially cause problems insofar as it is correlated with treatment. We address these issues in section 3.5.1 by showing that there is no discontinuity in the probability of being observed in the labor market portion of the survey.

The initial sample consists of 17,830 students who were enrolled in grade 6 (6ème) in the academic year 1995-1996. We restrict our data to students who sat for the first round of the General Baccalaureate exam in the academic year 2001-2002.[6] We do not use the results from the second round because retaking the exam can induce differences between students who are marginally below and above the thresh-

[5]Data Source: Enquête sur l'entrée dans la vie adulte des élèves entrés en 6ème en 1995 (EVA) - 2005- 2011 - (2011) [fichier électronique], INSEE [producteur], Centre Maurice Halbwachs(CMH) [diffuseur].

[6]It is worth noting that grade repetition is very common in France, especially in early elementary school years. Given that we use the 2002 first round of exams, then by definition our final sample only includes students who did not repeat a grade between grade 6 and their last year of secondary school. If many students in our initial sample repeated an academic year between these two grades, this may put into question the external validity of our results. Fortunately, in our data, less than 5 percent of students sat for the General Baccalaureate exam for the first time after 2002. This is consistent with the fact that the majority of grade repetition in France takes place in early elementary grades. Further, students who follow the traditional route of education are less prone to grade repetition.

old (Martorell and McFarlin Jr., 2011). Further, the second round exams can be strategically manipulated as they are conducted orally and administered by only one teacher. We also exclude students who attended vocational secondary schooling as their post Baccalaureate academic options are limited.

The main labor market outcome of interest is the natural log of average monthly net earnings, stacked for the years 2011 and 2012. This results in up to two observations for each individual. Since earnings of individuals in their early twenties are not usually considered a good predictor of future income, we use earnings reported approximately 9 to 10 years after taking the baccalaureate exam, when the students are aged between 27 and 29.

In our analysis of the quality of post-Baccalaureate institutions, the main measure used is the average Baccalaureate score of all students in our sample attending a certain institution, i.e average peer performance.[7] Thus, we consider a university to be of "better quality" if it has higher performing peers. Concerning field of study, the main outcome of interest is a dummy variable that is equal to 1 if the student is enrolled in a STEM designated degree or an advanced business degree. Advanced business degrees are classified as STEM because they usually require good quantitative skills. A complete account of the majors we designate as STEM versus non-STEM can be found in Appendix Table B1.

Finally, we use father's occupation as a proxy for students' socioeconomic status. The occupation of the father is stratified into 42 different positions that are represented by two digit identifiers. The first digit of each identifier represents one of four main skill levels. These skill levels are the official French socioeconomic classification as represented by the "Nonmenclature des professions et categories socioprofes-

[7]Stratifying institutions by tier is not as straightforward in France as it would be in the U.S. Further, data on out of sample average Baccalaureate score by institution is not available.

sionelles" (PCS) and are used as a reference in all official collective agreements. Our definition of high skilled workers includes the first two skills levels, while low skilled workers are represented by the last two.

Descriptive statistics for students who sat for the first round of the 2002 General Baccalaureate are reported in Table B2. 38 percent of the students are male and 57 percent are from a high socioeconomic background.[8] The average score on the Baccalaureate exam is 11.17 points, with approximately 75 percent of students passing from the first round. Further, 98 percent of the students in our sample eventually graduate high school (i.e. eventually pass the Baccalaureate). Respondents have an average of 3.2 years of postsecondary education and 28 percent of students are enrolled in a STEM major in their first year of postsecondary education. As for labor market outcomes, the average monthly net earnings for individuals in the sample are 1,625 and 1,725 Euros for 2011 and 2012 respectively, with an employment rate of 93 percent for both years.

3.4 Identification strategy

We use a standard regression discontinuity framework (Lee and Lemieux, 2010; Imbens and Lemieux, 2008) to estimate the effects of passing the Baccalaureate exam from the first try on educational attainment, quality of education and future labor market outcomes. The key identifying assumption underlying an RD design is that all determinants of future outcomes vary smoothly across the threshold. In that sense, any observed discontinuity at the threshold can be attributed to the causal effect of scoring above a 10 on the Baccalaureate exam, i.e. passing on the first

[8] 51.61 percent of the students in our initial sample are male. This number is reduced to 38 percent after excluding students who were in vocational secondary schooling. However, this does not pose any threat to identification, as we observe no discontinuity in the likelihood of being of a certain sex at the threshold (See Section 5.1).

attempt.

Formally, we estimate the following reduced form equation:

$$Y_i = \alpha + g(S_i) + \tau D_i + \delta X_i + \epsilon_i$$

where the dependent variable Y is the outcome of interest, representing earnings and educational outcomes for individual i. D is a dummy variable indicating whether a person passed or failed the French Baccalaureate exam on the first try. S is the running variable and represents an individual's score on the first attempt of the exam. It is defined as grade points relative to the threshold passing grade of 10. The function g(.) captures the underlying relationship between the running variable and the dependent variable. We allow the slopes of our fitted lines to differ on either side of the passing threshold by interacting g(.) with treatment D in order to control for differential trends in grades. X is a vector of controls that should improve precision by reducing residual variation in the outcome variable, but should not significantly alter the treatment estimates. The term ϵ represents the error term. The parameter of interest is τ which gives us the local average treatment effect for each regression.

In all regressions, we use population survey weights to estimate treatment effects for the various outcomes of interest.[9] Further, heteroskedastic adjusted errors are used in all regressions.[10] There are two ways to estimate the parameter τ in an RD design. First, one can impose a specific parametric function for g(.), using all the available grade data, to estimate the above equation via ordinary least squares —typically referred to as the global polynomial approach. Alternatively, one can specify g(.) to be a linear function of S and estimate the equation over a narrower range of data, using a local linear regression. In this paper, the preferred specifica-

[9] Results remain unchanged when using un-weighted regressions.

[10] Our running variable is fairly continuous as it is reported to the nearest one hundredth of a decimal point (i.e 9.91, 9.92, etc...). Accordingly, we are not too concerned about random specification error resulting from a discrete running variable as reported in Lee and Card (2008).

tions are drawn from local linear regressions within 1.5 grade points on either side of the cutoff using uniform kernel weights. This avoids the problem of identifying local effects using variation too far away from the passing threshold. Our choice of bandwidth is motivated by graphical fit, data driven optimal bandwidth selectors and the existence of other cutoff grades. Specifically, we use a robust data driven procedure, outlined in Calonico, Cattaneo and Titiunik (2014), to predict the optimal bandwidths (Henceforth CCT).[11] 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. While our preferred specifications are drawn from local linear regressions, we still present results for a variety of bandwidths and functional forms, as has become standard in the RD literature (Lee and Lemieux, 2010). The results are robust to these varying specifications leading us to conclude that passing the Baccalaureate exam from the first attempt results in significant differences in quality of schooling and subsequent labor market outcomes.

3.5 Results

3.5.1 Tests of the validity of the design

A standard concern with any RD design is the ability for individuals to precisely control the assignment variable. In our context, this can occur if students and/or graders manipulate scores in such a way that the distribution of unobservable determinants of education and earnings are discontinuous at the cutoff. The first concern is if students themselves are able to precisely sort to either side of the cutoff, especially given that the cutoff score is known beforehand. However, the Baccalaureate

[11] The optimal local linear bandwidth for most of our specifications ranges from 1.2 to 1.5 score points.

exam comprises all subject matter taken during the year, most of which is in essay format, making it highly unlikely for any student to be able to precisely control their grade. A potentially more worrying concern is whether graders are sorting students to either side of the passing threshold in a non random way. Indeed, if borderline students with better future prospects are marginally passed at a higher rate than those with worse prospects, then our education and earnings estimates would most likely be upward biased.

In addressing these concerns, we consider a few tests that have become standard in the RD literature. The first informative test would be to check for any discontinuity in the density of grades at the cutoff point (McCrary, 2008). The rationale behind this test is that if individuals are manipulating grades around the cutoff, then the grade distribution will be discontinuously uneven for grades just below and above the cutoff. However, a running variable with a continuous density is neither necessary nor sufficient for identification. Specifically, this test may not be as helpful if discontinuities in the grade distribution can be attributed to other exogenous factors such as grade rounding.[12] As mentioned in Section 3.2.2, after the initial grading of the exams, jury members decide whether they should award extra points to individuals just short of an important cutoff. The empirical distribution in Panel A of Figure B1 is consistent with this idea. At each representative grade cutoff, we observe a dip in the number of students who are just short of said cutoff combined with a spike in the number of students who are just above it.[13] This heaping is consistent with a priori expectations that jury members are bunching grades at important cutoffs. These distributional discontinuities could be the result of strategic cutoff crossing, or an alternative random sorting process. While, the first case is

[12]See Zimmerman (2014) for such a case.

[13]Recall, that the cutoff grades of 8 ,10 ,12 ,14 and 16 all serve a specific purpose in terms of awarded degree.

obviously problematic, the latter poses no threat to identification. As highlighted in McCrary (2008):"If teachers select at random which students receive bonus points, then an ATE would still be identified." In what follows, we provide evidence against strategic cutoff crossing.

In the presence of a running variable that is discontinuously distributed for exogenous reasons, an informative visual test for grade manipulation is to verify the smoothness of baseline characteristics. This test has become standard in the RD literature as an alternative and often preferred approach for testing the validity of the RD design (Lee and Lemieux, 2010). The intuition here is that if we observe discontinuities in exogenous variables, then the treatment is not randomly assigned and an average treatment effect is not identified. Further, as part of this exercise, we also check for the presence of a discontinuity in the probability of being observed in the follow-up labor force segment of the survey. Specifically, if probability of survey response is correlated with treatment, then the standard interpretation of our treatment effect would be problematic.

All panels in Figure B2 present estimates of the effects of threshold crossing on baseline characteristics. These figures take the same form as those after them in that open circles represent local averages over a 0.25 score range. All figures represent local linear regressions within 1.5 score points of the cutoff. Further, estimates are computed using population weights with robust standard errors reported in parentheses.

We first check for the presence of a discontinuity in the averaged score of the oral and written French literature portion of the Baccalaureate exam. There are two advantages to looking at this variable. First, these exams are administered in grade 11, one year before all other Baccalaureate tests. In that sense, it is a very recent indicator of student ability. Second, jury members cannot award extra points on this particular component of the Baccalaureate exam. Panel A of Figure B2 reveals an insignificant treatment effect (0.0196) on the average score of the French literature exam. We further test for a discontinuity in the Brevet national exam test scores. This high stakes exam is taken in grade 9 and is required for entry into high school, with the grading scale also being from 0-20. We have the averaged score for the three major components of the Brevet exam (Mathematics, French and foreign language). We also look at another national exam taken at the beginning of grade 6. The goal of this exam is to evaluate the level of students in mathematics and its grading scale is from 0 to 78. In Panel B of Figure B2, we find an insignificant treatment estimate (0.158) on Brevet scores. Panel C of Figure B2 also reveals an insignificant treatment effect (-0.847) on the mathematics exam scores in grade 6. This eases concerns that jury members might be sorting students around the cutoff, based on their academic ability.

In Panel D, we check for the presence of a discontinuity in the likelihood of being from a high socioeconomic status (S.E.S). We also find no significant effect (0.022). Further, in Panels E through G, we check for the smoothness of covariates that are known to affect education and wages, but that should be independent of treatment. Estimates on gender (0.0029), order of birth (-0.098) and number of siblings (0.138) are all statistically insignificant. To alleviate any concerns over bandwidth and/or functional form chosen, we present the baseline characteristics over varying functional forms and bandwidths in Table B3. All estimates remain statistically insignificant. Finally, we show that the predicted Baccalaureate score, as a function of the above covariates, is continuous at the cutoff. Both panels in Figure B3 highlight these results using a local linear and global polynomial fit respectively.

These results reject the hypothesis of strategic threshold crossing in favor of a non strategic sorting hypothesis. They are also consistent with the fact that students' identities are never disclosed to neither graders nor jury members.

As highlighted in Barreca et. al (forthcoming, 2015), heaping in the running variable can have serious consequences if it is associated with determinants of the outcome variables. However, this will only bias the estimates to the extent that it creates imbalances in outcome determinants around the cutoff. Therefore, as a complement to our balanced characteristics test, we implement additional checks to further investigate the existence of strategic sorting. Specifically, we run 'Donut type' RDs that deal with the heaped data at each cutoff. Panel B of Figure B1 highlights the new distribution of grades resulting from Donut type RD regressions, which essentially involves cutting out all potentially manipulable data points. We implement these regressions in Section 3.5.6 with the main results remaining unchanged.

Finally, if marginally failing students were more likely to leave the country in order to have access to higher quality universities or if they endogenously chose not to respond to the follow up survey as a result of failing, then the interpretation of our results would be problematic. As an important RD validity check, we show that there is no significant threshold crossing effect on the likelihood of being observed in the follow-up wage survey. These results are reported in Panel H of Figure B2 and Table B3. The absence of any differential selection into the earnings sample alleviates any concerns attributed to leaving the sample due to barely failing the French Baccalaureate exam.

3.5.2 Is the baccalaureate cutoff rule binding in practice?

In this paper, we estimate the impact of passing the French Baccalaureate from the first try on future educational and labor market outcomes. Before proceeding with the results, we first show that there is a discontinuity in the first round pass rate at the threshold. Figure B4 is a graphical representation of the 'first stage', i.e. the probability of being awarded the Baccalaureate degree on the first round conditional on first round exam scores. The figure shows a clear discontinuity at the cutoff, with a sharp 100 percentage point jump in the probability of passing at the threshold. This indicates that the Baccalaureate cutoff rule was fully binding in practice and subsequently rules out any non-compliance issues.

3.5.3 Impact on quantity of education

In this section, we investigate whether marginally passing the Baccalaureate exam on the first round affects the quantity of education pursued.

We first check whether barely passing from the first attempt affects the likelihood of ever graduating from secondary school. In Panel A of Figure B5, we plot the probability of ever passing the French Baccalaureate exam as a function of the first exam score.[14] Panel A shows an insignificant treatment effect (0.003) on the probability of ever graduating from secondary school. Estimates for varying bandwidths and functional forms are reported in Panel A of Table B4, with the results remaining statistically insignificant.

Next, we look at whether there is a treatment effect on the likelihood of receiving a post-baccalaureate degree. Panel B of Figure B5 shows no significant effect of threshold crossing on the probability of having a post-baccalaureate degree. The results remain insignificant over varying bandwidths and functional forms as is evident from Panel B of Table B4.

We then look at whether threshold crossing leads to variation in the number of years of postsecondary education pursued. In Panel C of Figure B5, we plot the years of post baccalaureate education as a function of the first round exam scores. We also find no significant treatment effect. Corresponding regression estimates are

[14] Global polynomial figures for all "Quantity of education" variables can be found in Appendix Figure B6.

reported in Panel C of Table B4. The estimates are consistent with the figure and rule out any significant effects.

Finally, we investigate whether threshold crossing affects the age of post-secondary graduation. In Panel D of Figure B5, we plot the age at graduation as a function of first round exam scores. We find a significant treatment effect (0.39 years) when using a local linear regression over a bandwidth of 1.5 points. However, as shown in Panel D of Table B4, this estimate is not robust to different bandwidths and functional forms.

All results remain unchanged when we add controls. These controls include exam specialization fixed effects, date of birth, number of siblings, birth order, socioeconomic status, scores on the Brevet examination, scores on the French portion of the Baccalaureate taken in grade 11 and scores in the grade 6 national assessment exam in Mathematics.

In summary, we rule out that passing the General Baccalaureate on the first attempt affects the quantity of education pursued.

3.5.4 Impact on quality of education

In this section, we explore whether passing the Baccalaureate exam on the first attempt affects quality of postsecondary education pursued. Specifically, we look at the impact of threshold crossing on the quality of institution attended, proxied by peer quality, and the likelihood of enrolling in a STEM major.

We rely on in-sample institution average Baccalaureate score, i.e peer performance, as a proxy for institution quality.[15] In Panel A of Figure B7, we plot the mean student Baccalaureate score for each university as a function of first round

[15] A potential drawback to this approach is that the relatively small number of observations within each institution could lead to inference problems. Specifically, all individuals within the same institution share a common measurement error component. We correct for this by clustering at the institution level thus allowing for a grouped error structure.

exam scores. As in previous figures, open circles represent local averages over a 0.25 score range. All figures represent a population weighted local linear regression using data within 1.5 points on either side of the threshold, which has again been chosen by the CCT bandwidth selector.[16]

We find a significant threshold crossing effect to the order of 0.26 Baccalaureate points. This indicates that the average peer quality that students experience increases significantly and discontinuously—to the order of 0.11 of a standard deviation in Baccalaureate scores—as a result of passing from the first attempt. To put things into perspective, this would be comparable to attending a US college whose student body averaged 1024 SAT points off of a base college whose students averaged 1000 SAT points.[17] Since our study deals with marginal public universities on either side of the threshold, as opposed to elite versus non-elite type universities, the order of magnitude seems reasonable and economically significant. For example, Hoekstra (2009) finds that attending the most selective in-state university, whose peers average 65 SAT points higher than the next university, results in a 20 percent earnings premium for males.

Panel A of Table B5 depicts discontinuity estimates using different bandwidths and functional forms. The estimates range from 0.21 to 0.29 Baccalaureate points, equivalent to 0.09 to 0.13 of a S.D. in test scores, and are all statistically significant at the 5% level. Additionally, the inclusion of controls does not significantly change

[16]The negative slope on the left hand side of the cutoff is consistent with a first come, first served admissions mechanism (See Sections 6.2.3 and 6.6.3 for details). Specifically, students scoring just shy of a cutoff are more likely to pass on the second round than those farther to the left of the cutoff, who are more likely to pass on the first round of the following year. The unintended consequence of this is that students farther to the left of the cutoff have a better pick of universities the following year. However, we must also note that the negative slope is not statistically significant. In Appendix Figure B8, we also present global polynomial figures that reveal the entire fit.

[17]We arrive at this comparison in the following way: The average institution Baccalaureate score just to the left of the cutoff is 10.82 points. In our dsata, this corresponds to ranking in the 45th percentile of all students. We then compare this number to the 45th percentile score of SAT National Percentile Ranks, which is equivalent to 1000 (We use the Verbal + Mathematics percentile rank).

the estimates, which is consistent with the identifying assumption.

Students in France simultaneously enroll in a postsecondary institution and field of study. Consequently, we check whether there is a discontinuity in the likelihood of being enrolled in a STEM versus non-STEM major. Panel B of Figure B7 plots the probability of being enrolled in a STEM major as a function of first exam score. Threshold crossing induces a 15.9 percentage point increase in the probability of being in a STEM major. Panel B of Table B5 reports the discontinuity estimates using different bandwidths and functional forms and with the inclusion of controls. All estimates remain statistically significant at the 5% level.

The data allows us to observe whether an individual graduates from a certain institution rather than just being admitted to an institution. This is potentially important as completion rates are sometimes low and vary across institutions, which would in turn complicate the interpretation of the results. Consequently, we present local linear estimates on the quality of institutions that students graduate from as well as the likelihood of graduating with a STEM-designated major in Appendix Figure B9. All figures show a clear discontinuity at the threshold, similar to the initial attendance figures. This lead us to conclude that any potential labor market effects should be the result of both attending and graduating with higher quality schooling.

3.5.5 Impact on labor market outcomes

We now turn to whether the documented variation in quality of education is associated with positive labor market returns. Figure B10 graphically depicts the relationship between labor market outcomes and the distance from the first round exam cutoff. All panels report estimates from local linear regressions using a bandwidth of 1.5 points, with standard errors clustered at the individual level.[18] We first check whether threshold crossing generates any significant changes in the likelihood of employment. In Panel A of Figure 7, we find an insignificant -0.008 percentage point change in the likelihood of employment at the threshold. As shown in Panel A of Table 5, all regression estimates remain statistically insignificant over varying bandwidths and functional forms. Further, the addition of controls does not significantly affect estimates.

We then explore whether threshold crossing affects earnings. Specifically, we focus on the average monthly net earnings for the years 2011 and 2012. The earnings from both years are stacked, resulting in up to two observations per individual. Accordingly, standard errors are clustered at the individual level. We look at net monthly earnings as a function of exam score in Panel B of Figure B10. We find that first round passing is associated with a 252 Euro monthly premium. Additionally, in Panel C, we look at logged monthly earnings. We find that threshold crossing leads to a 12.8 log point (13.6 percent) increase in earnings. Corresponding regression estimates are shown in Panels B and C of Table B6. These estimates are robust to different bandwidths and functional forms. For instance, the estimates for logged earnings vary from 12.6 to 18 log points and are all statistically significant at the 1% level. Further, the addition of controls does not significantly alter the estimates for earnings, which is consistent with the identifying assumption. We conclude that while passing the Baccalaureate exam on the first try does not affect the likelihood of employment, it does significantly alter future earnings at the intensive margin.

[18] Global polynomial figures for all "Labor market" variables can be found in Appendix Figure B11.

3.5.6 Robustness checks

Before interpreting our results, we run additional robustness checks. Primarily, we address concerns that heaping in the running variable could lead to bias—even in the presence of balanced covariates. To alleviate such concerns, we run 'Donut type' RD regressions that involve dropping all potentially manipulable data points around the threshold (See Dahl et. al, 2014 and Zimmerman, 2014 for similar applications of Donut RDs). In our setting, scoring within 0.25 points to the left of a cutoff generally allows for a student's grade to be reconsidered. Further, grades are pushed to anywhere between X to X.1 points, with X representing a respective cutoff. As a result, we drop all individuals whose first exam grade lies anywhere between 7.75-8.1, 9.75-10.1, 11.75-12.1, 13.75-14.1 and 15.75-16.1 points. The new distribution of grades is highlighted in Panel B of Figure A1. Regression estimates from these 'Donut' type RD specifications can be found in Tables B7, B8 and B9, where we report modified treatment estimates for quantity of education, quality of education and labor market outcomes respectively. We report all outcome variables over the same bandwidths and functional forms previously analyzed. Precision is reduced in most specifications, which is to be expected given the reduced data. However, all previously significant treatment effects remain so. Further, point estimates slightly increase for most specifications, which is at odds with a strategic sorting story. If jury members were endogenously sorting students, then we would expect our new point estimates to be significantly reduced.

Generally, jury members give special attention to grades that are within 0.25 points short of a cutoff. However, we cannot rule out the possibility of certain jury members awarding extra points for scores that are even further away from the threshold. To further investigate this issue, we take a closer look at the distribution of Baccalaureate test scores within a 9 to 11 grade window in Figure B12. Noticeably, the distribution of test scores drops sharply and linearly in the range of 9.65 to 9.99 Baccalaureate points. This sudden drop in the distribution is consistent with the potential for manipulation of test scores as reported in Dee et. al (2011). As a result, we further exclude from our 'Donut RD' analysis all individuals scoring between 9.65 and 9.75 points which effectively takes care of all test scores that could potentially be manipulated. We then reestimate all treatment effects. These results are also reported in Tables 6, 7 and 8. Precision is further reduced in most specifications. Nonetheless, all previously significant effects remain so. Altogether, the results from both Donut RD specifications reject a strategic sorting hypothesis and are in line with our main results and conclusions.

Finally, we check for earnings discontinuities at pseudo cutoff scores around the passing threshold score. To do so, we gradually estimate treatment effects for 50 fake cutoff scores on either side of the original passing threshold. We use logged monthly earnings as the outcome variable in this placebo test. Results indicate that the cutoff score of 10 provides for the largest and most significant discontinuity. Figure B13 of the appendix summarizes these findings by graphing t-statistics for these various placebo cutoff scores. The estimated t-statistic at the zero cutoff score represents the original one, with all others being placebo statistics for fake cutoff treatments relative to the original.[19] All significant estimates are highlighted in the graph with a large red filled circle. We observe only 2 significant treatment effects out of a possible 100. We conclude that no other important cutoff value (8 ,12, 14, 16) has a significant effect on earnings except for the original high stakes passing cutoff of 10. These results also provide further evidence on the importance of passing the

[19]Each open circle represents the t-statistic from a local linear regression of bandwidth = 1.5 Baccalaureate points.

Baccalaureate exam from the first round and the significant earnings premium that this induced variation leads to.

3.6 Discussion

3.6.1 Interpreting the documented labor market premium

We interpret our results as "intent to treat" effects whereby increased access to higher quality schooling results in a 13.6 percent earnings premium for the low skilled student. Indeed, our "first stage" results show that a significant proportion of students who pass from the first round attend a university with higher performing peers and/or are more likely to pursue a STEM major. This allows us to measure the effect of increased access to higher quality education on later lifetime outcomes, but not the effect of any specific change in higher education quality.

Our interpretation hinges on the fact that only quality of education dimensions vary at the cutoff. As a result, we rule out other potential channels through which marginally passing the Baccalaureate exam on the first round could affect earnings. First, we show that there is no impact on the likelihood of ever being awarded the Baccalaureate degree. This is not surprising as students are required to hold the degree if they wish to enroll in postsecondary education. Furthermore, students who want to enter the labor force immediately after secondary school could use the baccalaureate degree as a signal of their ability to potential employers. Therefore, students are incentivized to retake the exam until they are awarded the degree. This is in line with recent evidence which finds that exit exams don't cause increased high school dropout rates (Clark and See, 2011). Second, we find that threshold-crossing has no impact on the likelihood of obtaining a post-baccalaureate degree nor on the years of postsecondary education. These results are expected given the vast number of non selective universities and majors in France whose only requirement for admission is holding the Baccalaureate degree.

Another factor that can affect the interpretation of our estimates is that the documented increase in earnings could be driven by employers who use passing on the first round as a signal of productivity. To alleviate such concerns, we focus on a segment of the population who have chosen not to attend college.[20] If employers are using the first round of the Baccalaureate exam as a signal of productivity, then we would expect the signal to be most pronounced for this segment of the population. Appendix Figure B14 shows that there is no threshold crossing effect on earnings for this subpopulation. While the estimate is not precise due to small sample issues, it is still comforting to see that there is no discernible discontinuity at the cutoff.[21] Furthermore, it is unlikely that employers are able to distinguish students who marginally passed and marginally failed the first round exams.

A final concern is that age of Baccalaureate or post Baccalaureate graduation is lower for marginal passers. In this case, at least part of the observed earnings premium might be explained by work experience. While we cannot reject the existence of a threshold crossing effect on age of post baccalaureate graduation, the results indicate that marginally passing from the first attempt potentially increases the age at graduation. This would cause us to understate the earnings estimate, in so far as work experience is positively correlated with earnings. The results are not surprising given that some STEM majors require more time to complete in France. For example, engineering degrees are awarded after five years in higher education, as opposed to three years for most other degrees. Finally, for age at Baccalaureate receipt, Appendix Figure B15 reveals no significant treatment effect.

[20] We have previously shown that there is no discontinuity in college attendance which is why we are not too wary about conditioning on non-college attendance.

[21] We use a quadratic polynomial regression because there is insufficient data to run meaningful local linear regressions.

3.6.2 Returns to STEM education?

Although it would be interesting to examine the effects for different subgroups of students, our sample size does not allow us to run a thorough heterogeneity analysis.

We do however investigate the impacts for students from lower socioeconomic backgrounds, with the caveat of reduced precision. This allows us to present suggestive evidence on the earnings premium of pursuing a STEM degree, holding quality of university constant. To do so, we look at a subpopulation of students whose quality of schooling is likely to differ in only one dimension. Specifically, students from lower socioeconomic backgrounds may be less likely to attend higher quality institutions. In fact, there is clear evidence that low-income students tend to "undermatch" and not attend the highest quality colleges available to them (Smith et al., 2013). This seems highly plausible in our context as the default choice of education in France is to attend the public university that is closest to the area of residence. These universities are not always of the highest quality and are a less expensive option for lower earning families, in terms of housing and transportation. Further, the possibility of pursuing a STEM degree at a lower quality university is higher than it would be at a better quality one. The results from Table 9 are consistent with this idea.

Our preferred specification in column 3 suggests that students from low socioeconomic backgrounds are not attending better universities. However, they are 26.4 percentage point more likely to pursue a STEM degree.[22] We also estimate an 11.4 percent earnings premium for this subgroup of students. If we were to believe that no other changes were happening at the threshold, then rescaling the reduced form wage estimate by the documented increase in the likelihood of pursuing a STEM major suggests a 43 percent return to pursuing a STEM designated major for students of

[22]We do not provide any figures for these results as the smaller samples leads to the undersmoothing of mean plots.

low socioeconomic backgrounds.[23]

3.6.3 Quality of education mechanisms

In this paper, we find that marginally passing the French Baccalaureate exam on the first attempt leads to an increase in quality of post secondary education as well as future earnings. We present three possible explanations for the observed increase in schooling quality.

First, students who sat for the second round of exams could have been at a disadvantage because universities admitted students on a "first come, first serve" basis. The results of the second round of exams were announced a week after the results of the first round. While this might seem like a short period of time, this extra week could still be an important advantage for those who wish to enroll in the university/major combinations that are in high demand. For instance, our education survey asks students whether they are content with the field they are pursuing and the reason they didn't enroll in the major of their choice. Amongst those who failed the first round, 11.88% said that they were too late in enrolling in their first choice of major. This number is only 4.88% for students who passed the first round exams.

Second, the documented impact of threshold crossing on higher education quality could be due to a discouragement effect. Marginally failing students may be discouraged by their results, making them more susceptible to impulsive educational decisions such as enrolling in lower quality universities or non-STEM majors. This discouragement effect has been previously documented in the literature by looking at whether exit exams induce increased high school dropout rates, with the results being mixed (Martorell, 2004; Reardon et al., 2010; Ou, 2010; Papay et. al, 2010;

[23]It should be noted that this is only suggestive evidence as the threshold crossing effect on quality of university is not very precise.

Clark and See, 2011).

Third, universities could perceive the timing of degree receipt as a signal of student ability which would then factor into admissions decisions. This is reinforced by the fact that the second round of exams have lower standards for passing and are often deemed unreliable (see Buchaillat et al., 2011). Most public universities in France are not selective but they are capacity constrained. Back in 2002, they were required to give priority in their admissions to students residing in the same area. Other students usually enrolled on a "first come, first serve" basis. However, in a recent report, the National Union of Students in France (L'Union Nationale des Etudiants de France) found that some universities were using the results from the Baccalaureate exam to select students into majors that are in high demand. Thus, we cannot rule out selection by universities as a channel through which marginally failing the first round affects the quality of higher education.

3.6.4 Earnings mechanisms

The two classical channels through which quality of higher education can affect earnings are human capital formation and signaling. Our measure of university quality is the average performance of peers in secondary school. Better peers can affect future earnings through both signaling (i.e. better peers attend better institutions) and human capital accumulation. One test of the signaling channel would be to look at whether the earnings effect decreases with age (Hoekstra, 2009). Our data only allows us to observe detailed labor market outcomes between the ages of 27 to 29.[24] Thus, we are unable to perform this test.

Another possible explanation for the earnings effect, that would favor the human capital channel, is that students who are marginally above the cutoff attend univer-

[24] Even though we observe labor market outcomes for years prior to this, detailed earnings data is only available for the last two years of the survey.

sity/major combinations that have more resources. Most universities in France are public and receive most of their funding from the government. Nonetheless, spending per student is different across institutions. For example, universities spent, on average, 6,500 euros per student in 2000. This number is 8,600 euros in higher technical institutes. Students in engineering schools and preparatory classes for the "Grandes Ecoles" benefited from 11,500 and 12,600 euros respectively. While students around the cutoff do not usually attend preparatory classes for the "Grandes Ecoles", there is still some heterogeneity in spending across different university/major combinations. Unfortunately, detailed data on spending per university is not available for the period of our study.[25] Therefore, we are unable to test whether higher quality universities and majors affect earnings through providing students with better resources.

3.7 Conclusion

This chapter estimates the labor market gains to higher quality postsecondary education for low skilled students. We use a regression discontinuity design that compares individuals who marginally pass and fail the French Baccalaureate exam on the first attempt. Specifically, we find that marginally passing increases enrollment in a university with better performing peers, to the order of 0.11 of a standard deviation in Baccalaureate test scores. It also leads to a 15.9 percentage point increase in the likelihood of pursuing a STEM degree. Further, we also find that threshold crossing leads to a 13.6 percent increase in earnings with no significant effects on employment. We find no impact on the likelihood of graduating from secondary school or college nor on years of postsecondary education. Accordingly, we interpret our results as intent to treat effects whereby increased access to higher quality postsec-

[25]This data was made available starting 2009. However, the algorithm used to allocate resources to universities also changed. As a result, it would misleading to use recent data on spending per university.

ondary education results in a 13.6 percent earnings premium for 27 to 29 year olds in France.

Our results have significant policy implications. First, while it remains an open question as to the extent to which our results apply to other settings, they suggest that academically marginal students benefit from higher quality postsecondary education in a significant way. From a policy perspective, this indicates that there are potential gains from increasing lower skilled students' access to higher quality institutions and STEM majors. Second, while we find that students who marginally fail the Baccalaureate are not discouraged from entering higher education or graduating college, our results suggest that there exist substantial costs associated with exit exams that have not yet been considered. Exit exams may restrict access to higher quality postsecondary education which in turn can have a significant impact on earnings. These costs should be considered in the current debate over the use of exit exams in the United States.

4. BETTER OR BEST? HIGH SCHOOL QUALITY AND ACADEMIC PERFORMANCE

4.1 Introduction

Parents willingly invest large amounts of resources in order to get their children into the best school possible. As a result, understanding the benefits to attending high achievement schools is of critical importance in the economics of education literature. Further, ability tracking into high schools is becoming increasingly popular. Notably, while many European and Asian countries have long used ability tracking to sort students into high schools, this phenomenon has been mostly restricted to selective high schools in large cities in the U.S. (e.g the Bostons Latin school and the Bronx school of science). More recently, however, the number of school districts offering test based admission standards into magnet schools has increased.

In China, all students wishing to enter high school must sit for a national high school entrance exam—the Zhongkao. Every year, millions of Chinese middle school students prepare to take this high-stakes entrance exam—the sole determinant of high school eligibility. Before sitting for this exam, students must submit an ordered list of preferred high schools they wish to enroll in. Accordingly, China provides for an interesting natural experiment, with far reaching policy implications, to analyze the effects of school quality on academic performance and college outcomes. To do so, we use a regression discontinuity design that compares the academic outcomes of students who are barely eligible and barely ineligible to enter a high school of higher quality. This allows us to overcome selection bias arising from the fact that choice of high school is likely correlated with unobservable factors that may also affect future performance, such as ability and motivation.

The main results of this chapter are divided into two parts. The first part relies on identification off of multiple between school cutoffs. This strategy allows us to determine the effects of attending a "better high school" in the spirit of Pop-Eleches & Urquiola (2013). Using this strategy, we find that relative to students who just miss scoring above a school cutoff, individuals experience, on average, a significant increase in the average high school entrance exam score displayed by their peers to the order of 0.15 - 0.17 standard deviations. However, this does not translate into better academic outcomes as measured by the high stakes college entrance exam (Gaokao) at the end of high school.

Our second set of results relies on identification off of a single between school cutoff. Specifically, this strategy looks at the effects of attending the "best high schools" available to a student within a town—defined as enrolling in a tier one high school. We find that having the opportunity to attend tier 1 high schools also results in a significant increase in peer effects in high school as measured by a 0.19 - 0.26 standard deviation increase in average peer test scores. This translates into improved academic performance in the college entrance exam to the order of 0.06 - 0.1 standard deviations. Importantly, this also results in a significant increase in students' eligibility to attend a four year college but not in their eligibility to attend an elite college. Notably, despite the fact that both males and females face a similar improvement in high school peers, the documented positive effects on academic performance are mainly driven by males.

An emerging body of literature looks at whether quality of schooling has a significant impact on academic performance.[1] However, the question of whether schools

[1]There is also a growing literature that has looked at the earnings effects of attending better quality schooling at the college or high school level (See, for example: Hoekstra, 2009; Saavedra 2009; Zimmerman 2014; Clark & Del Bono, 2014; Canaan & Mouganie, 2015). These papers conclude that higher quality schooling leads to better labor market outcomes.

with more attractive peers are really better in a value-added sense remains open. Using test based discontinuity designs, Jackson (2010) and Pop-Eleches & Urquiola (2013) find that attending a more selective high school has positive effects on academic performance in Trinidad & Tobago and Romania respectively.[2] On the other hand, Clark (2010); Abdulkadiroğlu et. al. (2014); Dobbie & Fryer (2014) and Lucas & Mbiti (2014) find that attending high achievement high schools does not result in improved academic performance.[3]

We contribute to the existing literature in the following ways. First, we present new RD estimates on the academic performance of students attending better quality high schools. Notably—taking advantage of our unique setting and data—we are the first paper to draw a distinction between going to a better high school and going to an elite high school within the same educational setting. This appears to be important as the reduced form effect on academic outcomes differs between both empirical strategies, despite the variation in peer composition being statistically similar in both. In other words, despite both sets of students, attending a better versus an elite high school, facing similar improvements in peer composition, academic gains are only documented in the case of attending the latter. This contrasts with recent work in the U.S showing no returns to elite schooling (Dobbie and Fryer, 2014; Abdulkadiroğlu et. al, 2014).

Second, taking advantage of a unique teacher designation in China, we provide for evidence to explain the disparity in academic outcomes from attending a better

[2]Ding & Lehrer (2007) use a selection on observables type design and conclude that high school peer quality has a significant and positive outcome on college entrance test scores in China. Berkowitz & Hoekstra (2011) compare the college outcomes of students who decided to enroll in a highly selective high school to those who did not despite being admitted. They conclude that attending a highly selective high school causes students to attend more selective colleges.

[3]Duflo et. al (2011) find scant evidence on the existence of positive peer effects on the short run academic outcomes of primary school children in Kenya. Similarly, Zhang (2014) concludes that attending high achievement public middle schools in China does not result in improved academic outcomes.

versus best high school. Teachers in China undergo rigorous, multifaceted evaluations of their performance each year, providing us with an excellent measure of teacher quality unavailable in most other contexts.[4] Particularly, we observe a compelling discontinuity in the ratio of superior teachers to students at the cutoff of attending a top tier high school. This effect is not visible when focusing on students who attend a better high school, regardless of tier. Assuming that peer effects are not highly non-linear in student ability, this suggests that teacher quality may explain the largest portion of our findings. These findings are in line with recent work by Jackson (2013) who finds that peer achievement can only explain a small fraction of the school selectivity effect on average. Further, this is also consistent with Park & Hannum (2001) who hypothesize that most of the variation in student test scores in China is likely due to teacher differences. They further show that teacher quality ranks in China contain a substantial amount of information about teacher quality not contained in traditional variables such as education and experience. This suggests that China provides for a good setting to analyze the academic effects of teacher quality. Importantly, our findings could also potentially help explain the mixed results in the literature on the effects of high achievement high schools.

Third, while other studies have found academic gains for females who attend higher quality schooling, our results indicate that in the Chinese context, these gains are restricted to males—despite both genders facing the same improvement in school inputs. For instance, Jackson (2010) finds that attending a better high school has twice the academic effect on girls than for boys in Trinidad and Tobago. This suggests that institutional setting plays a vital role when deciding policies that may expand access to higher quality schooling.

[4] In this paper, we focus on the variation in number of superior/advanced teacher designations which is given only to the best teachers in China. See Section 2 for more details.

4.2 The education system

4.2.1 Schooling in China

Children in China generally start elementary school at the age of six or seven. After spending six years in elementary school, children then move on to the first part of middle school—a 3-year junior middle school (7th to 9th grade) as part of the 9-year national compulsory education system.[5] Graduates from junior middle school then choose to pursue either vocational or traditional schooling. The traditional education path involves participating in the second part of middle school, which is equivalent to US high schools. Three years of high school are then followed by higher education (university/college) if one desires to continue their studies.

In China, elementary and middle school education are both free and compulsory. On the other hand, high school education is neither compulsory nor free. However, in most parts of China, the majority of high schools are public and charge relatively low tuition.[6] [7] In the large city we study, around 40% of junior middle school graduates go to vocational schools and 60% end up going to high school. As only high school graduates are eligible to take the college entrance test, students compete for positions in high school. Competition is most prominent and severe at the top ranked high schools. This competition is in the form of a city level exam compromised of seven subjects (Zhongkao or the HET). These subjects are Chinese language, Mathematics, English language, Physics, Chemistry, Political Science and Physical education. The weighted sum of these seven subjects is the one and only criterion for high school

[5]There are rare cases in which elementary schools are 5 years long while junior middle schools are 4 years long. However, for students in our province, it is a 6-3 system.

[6]Private high schools generally account for less than 5% of high schools and are generally not as good as public schools.

[7]For the sample of students in our province, it is around $200 per year and deductible if family income satisfies certain requirements.

admission for most students.[8] The HET is graded out of a possible 790 points.[9]

After high school, students wanting to go to college are required to take the college entrance test (Gaokao or the CET) after three years of high school. Students usually choose an academic concentration (Arts or Science) at the beginning of their junior year (2nd year) in high school. Some college majors only take students from either path and some take both, so this choice can be a combination of personal preference and comparative advantage. In theory, the choice of academic concentration is left to the student. The provincial college level entrance exam takes two forms—Arts or Sciences—depending on a student's concentration in high school. Students concentrating in arts take an exam that includes Chinese language, Mathematics for arts students, English language and a comprehensive arts test (Political Science, History and Geography). On the other hand, students concentrating in sciences take an exam that includes Chinese language, Mathematics for science students, English language and a comprehensive science test (Physics, Chemistry and Biology). The exam for both tracks is graded out of a possible 750 points.[10] Similar to the high school admissions process, the total weighted score on this high stakes exam is the one and only criterion for college admission. In fact, eligibility into a four years college is determined by a threshold determined within a province. In other words, unless a student scores above a score threshold set forth by each university (determined at the province level), then they are not eligible to enter that university. Further, unlike high schools, students in China are free to go to any university—regardless of

[8]The only exceptions are students with special talent, for example athletes. These students represent a very small portion of the whole population.

[9]Chinese, Math and English are each graded out of a possible 150 points. Politics, Physics and Chemistry are each graded from 0 to 100 points. Finally Physical education is graded out of a possible 40 points.

[10]**For the Science track:** Chinese, Mathematics for the sciences and English are each graded out of 150 points. The science comprehensive test is graded out of 300 points.
For the Arts track: Chinese, Mathematics for the arts and English are each graded out of 150 points. The arts comprehensive testis graded out of 300 points.

location within China—conditional on meeting that university's threshold score.

4.2.2 High school choice mechanism

High school admissions for the province we are looking at is centrally operated by each city's education administrators. In early June, students fill out application forms indicating their ordered preference of high schools. These students then take the high school entrance exam in mid June. High schools predetermine how many students they wish to admit for that year and grant them admissions based on students' preferences and test scores. Most provinces, including ours, use an admission procedure similar to the Boston Mechanism. In the first round of admissions, each college only considers students who list them as their first choice. Students with CET scores above a certain threshold are accepted and the rest are rejected and placed in a pool of candidates for to be considered by the next university on a student's list. Only in the event that a university still has any remaining slots after the first round will it consider admitting students who list them as their second or third choice. Once a student is granted admission by any college, the selection process ends for that student and he/she are not to be considered by any other college. For illustration, suppose school A plans to enroll 100 students for that academic year. Further, suppose that there are 80 students who indicate their first choice preference is to join that school. School A will first admit those 80 students, then proceed to rank students who listed A as their second choice—conditional on not yet being admitted by their first choice. If there are more than 20 of those applicants, school A will take the 20 highest scoring students. If admissions slots remain, then School A proceeds to fill the rest of their seats with students who list A as their third choice and so forth. In the more likely scenario that there are more than 100 students who list School A as their first preference, officials just pick the highest scoring 100 students. The lowest admitted student's score—regardless of preference order—is the official cutoff score for school A for that year. High schools go through this process simultaneously, as each student can be admitted by at most one school.

To ensure smoother and more transparent school-student matching, schools are divided into four groups. We call these groups tiers as they are divided by school quality. Tiers are determined by the city education department in advance of students applying for admissions and this information is made publicly available to them. [11]

Schools in the first tier are the first ones to start the admissions process. After tier one schools fill all their seats, tier two schools will start admitting students, then tier three and four. Accordingly, students list their preferences by tier. For each tier, a student has four ordered school choices. The order of choice is important as most competitive schools fill their slots solely with students who have them listed as their first choice. Students generally understand this and as a result most list their preferences by perception of school quality.[12] However, in the unlikely case that a student is not aware of the admissions system, then scoring extremely high on the test may not always guarantee one go to a top quality school.

School choice is different for students from different parts of the city we analyze. Specifically, the city is divided into twelve administrative districts by their geographic location and school choice is separated by these areas as well. Of the twelve districts, eight of them are mostly urban areas and relatively small. Students from these eight districts can go to high schools in their own district but also have access to schools in the other seven urban districts. Specifically, a student residing in an urban area can choose from almost all urban tier one schools—regardless of district—in addition to

[11]The best schools are assigned to tier one, then the second best are tier two and so forth. Schools may be in different tiers from one year to another to reflect quality changes but generally the composition is quite stable.

[12]It is very common for teachers to assist their students in filling these applications.

schools in tier two to four from their own district. On the other hand, students from rural districts have a choice set of only 2 tier one schools and 11 tier two-four schools to choose from, on average. Further, students residing in the four rural/suburban districts can only choose high schools in their own district. As a result, tier 1 high schools in rural areas can be thought of as magnet schools. Finally, the high school choice mechanism in China almost guarantees that all students residing in rural areas will list all tier-1 schools in order of rank as their first preference. For the above reasons, we restrict our sample to students from rural/suburban districts in our analysis.

Another interesting feature of our sample is that public high schools are allowed to set aside around 10% of their seats as "high priced" ones. Students enrolled through the high-priced channel pay a one-time fee to the school upon registration even though they receive the same education as the other regular students.[13] In the admissions process these high prices seats can be though of as their own independent school (with their own respective cutoffs). For illustration, suppose school A plans to set 90 regular seats and 10 high-priced seats. Then A (regular) and A (high-priced) independently go through the high school admissions process as described above. Thus, all schools with high-priced seats, will have two cutoffs—one for regular students and one for high priced students—and this information is released to the public by both education officials and the media. In our analysis, we drop all individuals who entered high school through this "high priced" process. Private schools do not have this fee distinction as they often charge high tuition. However, private high schools are rare and generally do not attract the best students in the city.

[13]This one-time fee is set by the schools and released to students before they apply. In the urban area it is usually around 40,000 Yuan (6,600 USD) and 20,000 Yuan (3,300 USD).

4.3 Data

We use student level administrative data on a large capital city of a densely populated province in China. As a condition of using the data, we are unable to reveal the name of the province and city. We restrict our data to individuals attending traditional high school as those attending vocational schooling are not generally eligible to take the college entrance examination. Further, we restrict our sample to three rural/suburban districts in that city for reasons previously discussed in section 4.2.2.[14] Finally, we drop all students attending school through the "high-price" channel. Our final sample consists of 15,367 students taking the high school entrance exam (HET) in the year 2007 and the college entrance exam (CET) in 2010. We observe both the HET and CET scores for all individuals in our data. We are also able to observe some basic characteristics of the students such as the middle school and high school they attended, gender, age, etc...The original data does not contain information on school and teacher characteristics. However, we are able to collect school level data from government reports and official school websites. Importantly, we are able to link this information to our original dataset through high school identifiers.

Within the three rural areas we analyze, students generally have at most 15 high schools to choose from with the tier of each school made clear to all. The main determinant of high school attendance is the score on the high school entrance exam. We observe detailed administrative data on test scores by subject and the eventual high school attended by the student. In our analysis on quality of high school attended, we document an improvement in high school quality through peer performance. Specifically, the peer quality within a high school is measured by the

[14] Technically, there are four such areas in this city but one of them does not have a single tier one high school thus we exclude that area from our study.

average exam score on the HET for each school.

The main outcome of interest is a student's total score on the college entrance exam as this high stakes exam determines a student's eligibility for college. A potential drawback of our data is that we are unable to observe the university that students eventually attend. However, eligibility for entry into a four year college and/or elite college is centrally determined by whether a student crosses the lowest threshold score imposed by a specific university. This threshold is common to all students in a province regardless of which city they reside in within the province. As a result, we are able to document whether students from our sample are eligible for entry into any four year college or/and an elite college simply from their final CET exam scores. Specifically, eligibility into a four year college is defined by whether a student scores above the lowest possible threshold score for entry into a four-year university. Similarly, eligibility into an elite college is defined by whether a student crosses the lowest threshold for entry into an elite university.[15]

Descriptive statistics for all students who sat for the 2007 HET are reported in Table C1. These statistics are reported for the whole sample and by high school tier. The average score on the HET and CET is 612 and 492 points with a standard deviation of 61 and 97 points respectively. These scores increase with the level of tier as to be expected. Females are slightly more represented in high school with 53% of high schools compromised of women. The likelihood of choosing to major in arts after your first year in high school is 48% for the whole sample. This number is much higher the lower you go down the tiers. A very small percentage of students are in private high schools (1%) and 57% of high schools are designated as being "high school only". The alternative to this is for a school to contain both middle

[15] Formally, universities in China are also divided into tiers. We define an elite university as a tier-1 university.

and high school students. Interestingly, schools in better tiers are more likely to be designated as “High school only”. 44% of our sample are eligible to go to a four year college. This number is as high as 67& for students attending tier one high schools and sharply drops to 28%, 7% and 1% for tiers two to four. Further, better tier schools tend to be larger in size and have more overall teachers. Importantly, the average number of superior teachers are much higher in better quality schools. Tier 1 schools have 92 superior teachers, whereas this number drops sharply to 45 for tier 2 schools. Finally, teacher gender is equally distributed with about 53% of all teachers in our sample being female. Tier 1 schools have a less proportion of female teachers (49%), whereas tiers 2 to 4 high schools have around 55% female teachers.

4.4 Identification strategy

4.4.1 Identification using a single between school cutoff

As mentioned earlier, high schools in the province we are analyzing are divided into four tiers with the first tier containing the best set of high schools within a district or town. Accordingly, we use a regression discontinuity design (Lee and Lemieux, 2010; Imbens and Lemieux, 2008) to estimate the causal impact of elite high school attendance (defined as going to a tier-1 high school) on college entrance exam scores and college outcomes. The key identifying assumption underlying an RD design is that all determinants of future outcomes vary smoothly across the tier-1 high school admissions threshold. This is likely to hold, as precisely manipulating the overall exam score is highly implausible. Furthermore, the admissions cutoff for each high school is only announced after exams are administered and graded. To support these claims, we show that there is no evidence of bunching around the threshold, and that treatment estimates do not significantly change with the addition of covariates. As a result, any discontinuity observed at the threshold score can be attributed to the causal effect of attending an elite high school. In our analysis of elite school attendance, we focus on three towns. Each town t = 1,2,3 contains i = 1,...N individuals and s = 1,...,S number of schools. Additionally let z represent the tier 1 cutoff within each respective town t. Formally, consider the following "reduced form" regression:

$$Y_i = \alpha + h(S_i) + \tau D_i + \delta X_i + \epsilon_i, \tag{4.1}$$

Where the dependent variable Y is the outcome of interest. D is a dummy variable indicating whether a person crosses the town specific score threshold for attending a tier 1 high school.[16] S represents student high school entrance test scores in 2007 measured in points relative to the cutoff score of each town relying on the fact that S represents the distance between each town's cutoff and the transition score of each student in that town. Formally, $S_i = grade_i - \overline{grade_z}$ for all individuals within a town facing a common cutoff z. The function h(.) captures the underlying relationship between the running variable and the dependent variable. We also allow the slopes of the fitted lines to differ on either side of the admissions threshold by interacting h(.) 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—consistent with our identifying assumption. The term ϵ represents unobservable factors affecting outcomes. Finally, the parameter τ gives us the average effect of having the opportunity to access a tier-1 high school for each outcome of interest.

In our analysis, we specify h(.) to be a linear function of S and estimate the equation over a narrower range of data, using local linear regressions with a uniform

[16] Essentially, we have 3 thresholds, each representing a different town.

kernel. This 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. As a result, the preferred specifications in this paper are drawn from local linear regressions with the optimal bandwidths chosen by a robust data driven procedure as outlined in Calonico, Cattaneo and Titiunik (2014)—Henceforth CCT. We also present results for a variety of bandwidths relative to the optimal bandwidth as has become standard in the RD literature (Lee & Lemieux, 2010). Further, standard errors are clustered at the high school score level, as suggested in Lee & Card (2008).

While we generally focus on reduced form results from specifications like (1), we also present estimates from an instrumental variables type specification. This allows us to infer the average effect of going to a tier-1 or elite high-school as opposed to just having the opportunity to do so. Formally:

$$Y_i = \alpha + h(S_i) + \beta E(C_i|S_i) + \gamma X_i + \epsilon_i, \tag{4.2}$$

$$E(C_i|S_i) = \alpha + h(S_i) + \lambda D_i + \theta X_i + \epsilon_i, \tag{4.3}$$

where (4.3) is the "first stage" or the compliance ratio. It denotes the ratio of tier-1 eligible students who actually take up this opportunity. β from equation (4.2) gives us the local average treatment estimate (LATE) of going to a tier-1 high school in a 2SLS framework. This is nothing more than the Wald estimate and can also be computed by re-weighting the ITT estimate (τ) by the first stage estimate (λ).

4.4.2 Identification using multiple between school cutoffs

Within each of the three towns in our sample, we rank schools according to their posted admissions cutoff score for that year (2007). This yields 34 quasiexperiments as each cutoff results in a potential RD analysis.[17] Following Pop-Eleches and Urquiola (2013), we focus on regressions that pool data across cutoffs. Specifically, we stack the data such that each student within a certain town serves as a separate observation for each cutoff.[18] Due to repeated observations, we cluster our standard errors at the individual level. This procedure is relevant as a student with the best ex-poste score in a specific town does not always attend the best school if his ex-ante school choices were conservatively chosen. Formally, our reduced form regression from this procedure takes the following form:

$$Y_{iz} = \alpha + h(S_{iz}) + \omega D_{iz} + \phi X_i + \epsilon_i \tag{4.4}$$

Here the subscript i still refers to students and the subscript z refers to all possible high school cutoffs facing an individual within a town (i.e. z= 1,...,H-1; where H represent the total number of high schools in that town ordered from worst to best based on their respective cutoff scores). ω gives us the ITT estimate of having the opportunity to go to a better school, regardless of tier. Further, the running variable is defined as $S_{iz} = grade_{iz} - \overline{grade_z}$ for all individuals within a town facing numerous cutoffs z. As a result, equation (4.4) takes the same form as equation (4.1) except for the fact that each individual can be observed multiple times depending on his/her relative position to a high school cutoff. However, regressions restricted to students scoring close to the cutoffs rarely use student-level observations more than once.

[17]We have an average of around 12 schools for each town resulting in 11 different cutoffs within each town.

[18]For instance, our smallest town has 13 different schools, leading to 12 separate cutoffs. This town also contains 4,595 students. For that town, our procedure produces a dataset of (4,595 $\times$ 12) 55,146 observations.

4.5 Results

4.5.1 Going to a better high school

In this section, we investigate whether going to a better high school—regardless of tier—has an effect on high school peer composition and academic outcomes.

4.5.1.1 Peer effect composition

We first check whether going to a better school leads to better peer composition in high school as measured by average high school entrance exam scores. In panel A of Figure C1, we plot the mean high school entrance exam (HET) score at an individual's high school as a function of his/her HET score. These figures take the same form as those after them in that open circles represent local averages over a 4 point score range. Further, all figures use a bandwidth of 50 HET test score points on either side of the cutoff. Using a local quadratic fit, we find that average peer quality significantly increases at the threshold that gives them to opportunity to access better quality schooling. Specifically, threshold crossing leads to an improvement in the average peer HET score in high school by 10.68 points—equivalent to 0.174 percent of a standard deviation in test scores.

In Table C2, we test the robustness of these results to various bandwidths and the inclusion of controls. All point estimates represent treatment effects from separate local linear regressions using high school peer quality as an outcome variable. Further, standard errors are clustered at the ID level.[19] The optimal bandwidth is selected using the CCT bandwidth selector.[20] We report estimates from various bandwidths relative to the optimal CCT bandwidth. Notably, results remain sta-

[19]Recall from section 4.4.2, identification using multiple between school cutoffs, can lead to repeated observations.

[20]For our peer composition outcome variable, the CCT predicts an optimal bandwidth of 14 score points on either side of the cutoff for local linear regressions (See Table C2).

tistically significant at the 1% level with estimates ranging from 0.14 to 0.18 of a standard deviation in HET test scores. Further, the inclusion of controls does not change the treatment effect on peer quality composition—consistent with the identifying assumption. These controls include age, gender, district and middle school fixed effects.

4.5.1.2 Academic outcomes

Next, we look at whether this documented increase in peer quality is associated with improved academic performance. Specifically in Panel A of Figure C2, using a local linear fit, we find that having the opportunity to access better schools does not lead to better performance in the high-stakes Chinese college entrance exams (CET). Further, in Panels A and B of Figure C3 we also show that threshold crossing does not lead to an increase in the eligibility to attend any four year college and/or an elite college.

In Table C3, we show that these results or not sensitive to choice of bandwidth and the inclusion of controls. In all specifications, having the opportunity to access higher quality schooling with better peers does not lead to an increase in college entrance exam test scores, eligibility to attend a 4 year college and the eligibility of attending an elite college.

4.5.1.3 Heterogeneous effects

One may wonder if females and males are differentially affected by the opportunity to access higher quality schooling in terms of academic outcomes. Before doing so, we first check whether females and males face significantly different peers when going to a better school. Results indicate that both males and females are exposed to a significant improvement in peer quality. Specifically, Panels B and C in Figure C1 indicate that males and females face an improvement in peer quality to the order of

0.156 and 0.19 of a standard deviation in HET exam scores. Next, we check whether this improvement in peer quality is associated with academic gains for a specific gender. Consistent with the average treatment effects, we find no significant effect of threshold crossing on the CET exam scores (Panels B and C of Figure C2), eligibility to attend a four year college (Panels C and E of Figure C3) as well as eligibility to attend an elite college (Panels D and F of Figure C3). All results from this gender analysis are summarized in Table C4, with results remaining unchanged. We conclude that having the opportunity to go to a better school leads to an improvement in the quality of high school peers. It does not, however, result in an improvement in college entrance exam scores or college outcomes. Further, these affects do not differ by gender.

4.5.2 Going to a top tier high school

In this section, we investigate whether going to a tier-1 or elite high school has an effect on high school peer composition and academic outcomes.

4.5.2.1 First stage and peer effect composition

Next, we turn to whether results change when focusing on a single between school cutoff within a town. Notably, focusing on a single between school cutoff allows us to also compute a 'first stage' for tier-1 eligible students. Specifically, using a quadratic fit, Panel A of Figure C4 highlights that 51.6 percent of people who score above the tier-1 threshold eventually attend a tier-1 school. We also check whether having the opportunity to access a tier 1 high school is associated with an improvement in peer quality. Similar to section 4.5.1.1, we find that having the opportunity to attend a tier 1 high school leads to a significant improvement in peer quality. Panel B of Figure C4 indicates that threshold crossing is associated with a 13.38 point increase in peer HET scores—equivalent to 0.218 of a standard deviation in test scores.

Table C5 highlights the robustness of these results to various bandwidths and the inclusion of controls using local linear regressions. Similar to previous analysis, controls include gender, age, district and middle school fixed effects. Further, standard errors are clustered at the running variable level, as suggested in Lee & Card (2008), and reported in parentheses. Panel A indicates that the compliance ratio is robust to various bandwidths and the inclusion of controls ranging from 52.2 to 57.2 percent. Further, Panel B shows that the documented discontinuity in high school peer quality remains statistically significant at the 1% level. These estimates range from 0.21 to 0.32 of a standard deviation in HET test scores.

4.5.2.2 Academic outcomes

We find that having access to a top tier school leads to an improvement in high school peer composition. Next, we look at whether it also leads to an improvement in academic outcomes. Panel A of Figure C5 is a graphical representation of CET scores as a function of students' HET scores. Using a local linear fit, we observe a significant discontinuity in CET scores at the threshold to the order of 8.18 points. To put things into perspective, this is equivalent to 0.084 of a standard deviation in CET test scores.

We further check whether threshold crossing is associated with significant changes in college outcomes. Panel B of Figure C5 reveals that having the opportunity to access elite high schooling increases students' eligibility in attending a four year college by 10 percentage points. However, as evident from Panel C, it does not seem to significantly increase the eligibility of attending an elite college.

Table C6 summarizes the reduced form impacts of having access to an elite school. Panel A presents the effects of threshold crossing on the CET exam scores using local linear regressions. The results are robust to various bandwidths and the addition of controls with estimates ranging from 0.052 to 0.096 of a standard deviation in test scores. Panels B and C report estimates on the eligibility of attending a four year college and an elite college respectively. Specifically, Panel B reveals that while the estimates for four year college eligibility are generally positive and significant, they are insignificant and imprecise for lower bandwidths. Panel C reveals that estimates for elite college eligibility are statistically insignificant for all bandwidths, yet economically meaningful.

4.5.2.3 Local average treatment versus intent to treat effects

All results reported thus far are intent to treat (ITT) estimates of the opportunity to access higher quality schooling. In what follows, we present local average treatment effects (LATE) attributed to attending a tier 1 high school. To do so, we re-weight the reduced form impacts of threshold crossing by the first stage compliance ratio in a 2SLS framework, as in equation (4.2).[21] In this section, we also present heterogeneous estimates by gender in order to see if the average treatment effects are being driven by a specific sex. Table C7 summarizes the ITT versus LATE estimates—for the whole population and by gender—using a bandwidth of 40 score points on either side of the cutoff. Further, all estimates are reported with the inclusion of controls and standard errors are clustered at the running variable level.

Panel A of Table C7 presents estimates on the likelihood of attending a tier 1 high school, i.e. the first stage. We find that the likelihood of attending a tier 1 school increases discontinuously at the threshold by 52.3 percentage points. Further, the first stage is similar in magnitude for both genders (53.1 for males versus 52.3 for females). Panel B presents ITT versus LATE estimates on high school peer

[21] In these specifications, elite school attendance is instrumented by scoring above the threshold for elite schooling. Here, the exclusion restriction needed to be satisfied is that scoring just above the threshold has no effects on future outcomes except through its effect on high school attendance, a reasonable assumption.

quality. Notably, the LATE effect of attending an elite high school is almost double the ITT effect. Specifically, we find that attending an elite high school increases the average peer quality a student is exposed to by 26 HET exam points—a 0.42 standard deviation improvement in peer test scores. These effects are similar for both males and females.

In Panel C, we report ITT versus LATE estimates for academic outcomes. Specifically, the effect of attending an elite high school on college entrance exam test scores is 15.87 points, equivalent to 0.16 of a standard deviation improvement in CET test scores. Strikingly, we also uncover substantial heterogeneity in the results. The ITT and LATE effect on college entrance test scores are statistically insignificant for the female population. On the other hand, attending an elite high school results in a 33 point test score improvement for males, equivalent to 0.34 of a standard deviation in CET scores. With regards to college outcomes, attending a tier-1 high school leads to a 15.7 percentage point increase in the eligibility of attending a four year college for the overall population, with the effects again being driven by males (23 percentage points). As for elite college attendance, ITT and LATE estimates are both statistically insignificant. Further, we observe no significant differences between both genders. We conclude that elite school attendance has significant and sizable effects on academic outcomes, mainly driven by the male population.

4.5.3 Robustness checks

4.5.3.1 Validity of the design

A concern with any regression discontinuity design is the ability for individuals to precisely control the assignment variable. This can occur if students and/or graders of the Chinese high school entrance exam are able to manipulate test scores in such a way that the distribution of unobservable determinants of education are discontinuous at the cutoff. This is highly unlikely in the context of the Chinese high school entrance exam as cutoff scores for each high school are only assigned after the exams are administered and graded. Specifically, all high school cutoffs are determined based on high school applicants' percentile rank—calculated after grades are determined. Further, any identifying information linking a student to an exam is unobservable to graders.

To formally address these concerns, we plot the distribution of test scores for all students taking the high school entrance exam in the year 2007.[22] Panels A and B of Figure C8 show the distribution of the 2007 high school entrance exam test scores using a bin width of 2 score points for both empirical analyses.[23] Both figures reveal no evidence of potential manipulation around the normalized thresholds for admission into better quality schooling.

An alternative test for the validity of an RD design is to check for the smoothness of baseline covariates known to affect outcomes (Lee & Lemieux, 2010). Our data set does not provide for a rich set of predetermined characteristics that affect academic outcomes. The only meaningful background information we are able to observe are students' gender and age when taking the HET. In panels C and D of Figure C8, the outcome of interest is students' predicted high school entrance exam scores conditional on sex, gender, district and middle school attended. Panel C plots the predicted HET score as a function of the actual scores for our multiple between school cutoff strategy, i.e. going to a better school. We observe no significant discontinuity at the threshold in terms of predicted scores. Similarly, in Panel D, we observe no significant discontinuity in predicted tests scores at the threshold for top-tier high

[22] For consistency with previous results, this distribution is conditional on a student being observed with a CET exam score also.

[23] Recall, the population of students remain the same in both empirical strategies. However, Panel A contains more data points as individuals can be observed more than once depending on their relative position to a high school cutoff.

school eligibility. Table C8 looks at the robustness of these local linear results to varying bandwidths. Panel A deals with estimating threshold crossing effects for attending a top-tier school, while Panel B deals with the case of going to a better school, regardless of tier. All estimates remain statistically insignificant for a range of different bandwidths. In Appendix Figure C9 and Table C9, we report threshold crossing effects for our baseline covariates separately.

4.5.3.2 Selection into the college entrance exam

The population of interest in this study consists of students whom have an observed high school and college entrance exam score. However, it remains a question as to whether selection into the college entrance exam is correlated with school quality and thus treatment—leading to potential bias in the estimation of academic outcomes. To investigate this, we match our current dataset to data containing the population of high school test score takers. Particularly, students who do not match are assumed to have not taken the college entrance exam.[24] We then check whether going to a better quality high school is correlated with treatment. Panel A of Figure C10 suggests that selection is not an issue when estimating the effects of attending a better high school. On the other hand, Panel B reveals that selection could be an issue when looking at the effects of attending a tier-1 school. In fact, students who attend top tier high schools are 1.69 percentage points more likely to take the CET. This result is consistent with ex ante expectations seeing as having the most number of students possible take the exam is a primary objective of a top quality high school. This suggests that marginal students who attend non-tier 1 schools are less likely to take the exam which would potentially lead us to underestimate any test score

[24] Using this matching procedure, we estimate that 91% of students entering high school sat for the CET exam. This is in line with official aggregate data for the districts being analyzing, which suggests that 90 to 94 percent of students take the CET exam with results varying from year to year.

treatment effects attributed to school quality. In Panels D and F of Figure C10, we investigate whether this selection effect differs by gender. Notably, the figures suggest that selection into the CET is driven by the female population.

We investigate the robustness of these results in Table C10. Panel B reveals that selection is not an issue for either gender when focusing on the effects of going to a better high school. On the other hand, results from Panel A suggest that while these estimates are not robust to varying bandwidths, selection into the CET exam may be a potential problem for our single between school cutoff strategy, i.e. going to a tier-1 school. Specifically, and although imprecise, this selection seems to be driven by the female population. This seems plausible as females not attending a top tier high school in rural China may be more likely to be discouraged by their parents' from pursuing their education.[25]. The next section deals with the potential for selection using a formal bounding analysis. Further, to the extent to which selection is driven by gender, all results in section 4.5 are reported with and without controls for gender, with the results remaining fairly unchanged.

4.5.3.3 Bounding analysis

To investigate the extent to which selection into the CET exam could potentially bias our results, we conduct a formal bound analysis using the trimming procedure suggested in Lee (2009). The intuition behind this test is as follows. To find a lower bound (worst case scenario) for the estimated impact of treatment on college exam scores, we assume that only the worst students attending the most selective high schools, who would have otherwise dropped out, select into the exam. Thus,

[25] This could also potentially explain results from the previous section indicating that the likelihood of observing a female in the sample is higher at the tier 1 cutoff for some bandwidths. See Appendix Table C9.

dropping the bottom distribution of the treatment group makes it comparable to the control group . Formally, the share of students to be trimmed from the treatment group is derived from the treatment estimate of the likelihood of selecting into the college entrance exam.[26] A similar procedure—trimming the top performing students in the treatment group—is used to estimate the upper bound (Best case scenario).

Table C11 summarizes the updated local linear regressions by comparing previous college test score RD estimates with those estimated using the trimming analysis.[27]. For consistency and comparability, we use the same bandwidths as the college entrance exam score regressions in Table C6. We present lower and upper bound estimates for all bandwidths, although the only significant specification arises from using a bandwidth of 51 score points. Notably, the lower and upper bound estimates from this specification remains positive and significant ranging from 8.12 to 13.89 points compared to the original estimate of 8.034. This suggests that selection into the CET exam does not bias our results in a meaningful way. We conclude that the potential for selection in the CET exam does not alter our results or conclusions.

4.6 Discussion

4.6.1 Interpretation of results

We show that having the opportunity to access a better quality high school in China, regardless of tier, is associated with an improvement in the average peer quality a student is exposed to. The magnitude of this improvement ranges from 0.15 to 0.17 of a standard deviation in HET scores. However, this is not associated with

[26] For example, using a local linear regression of bandwidth =50, we estimate that students are 1.69 percentage points more likely to select into the college entrance exam at the cutoff. To estimate the total percent of students to be trimmed, we merely divide 1.69 by the mean proportion of test takers for the control group at the threshold (90.5%). This results in us trimming 1.85 percent of the data.

[27] Bootstrapped standard errors are reported in parentheses for the lower and upper bound estimates

improved performance in the high stakes college entrance exam. Similarly, having the opportunity to access a tier-1 school also leads to a significant improvement in peer quality, to the order of 0.19 to 0.26 of a standard deviation in peer HET scores. This is associated with an improvement in performance on the national college entrance exams by 0.06 to 0.1 of a standard deviation, ultimately leading to a significant increase in the eligibility of attending a four year college. Our results are driven by the male population and are in line with recent research suggesting that being exposed to worse peers has more of a negative effect on boys as opposed to girls (See for example, Carrell & Hoekstra, 2010).

To the extent that peer effects are not highly non-linear in ability, differences in peer composition cannot fully explain the variation in academic outcomes we observe. For illustration, if peer effects were driving the results, then a 10.68 improvement in peer quality from going to a better school (See Figure C1, panel A) should lead to at least a 6.52 point increase in college entrance exam scores.[28] However, as evident from Panel A of Figure C2, we are able to rule out any effects larger than 2.7 points.

To investigate further, we look at whether school and teacher characteristics discontinuously change at either cutoff. Specifically, using school level data, we look at whether class composition, teacher quality and gender significantly differ at the threshold.[29] Figure C11 reveals the results of this exercise. All panels on the left relate to going to a tier 1 school while those on the right deal with the case of going to a better school, regardless of tier. Panel A of Figure C11 indicates that there are around 2 more students per classroom at tier 1 high schools.[30] This is most likely

[28]These results are extrapolated from Panel B of Figure C4 and Panel A of Figure C5 where having access to a tier 1 school is associated with a 13.38 improvement in peer test scores leading to a significant improvement in college test scores by 8.18 points. Formally, (8.18 * 10.68)/ 13.38 = 6.52.

[29]For this analysis, we present figures linking the school level data to our original individual level data. Accordingly, inference may be an issue here, so we stick to showing visual evidence.

[30]This is consistent with previous findings in China (See for example, Ding & Lehrer, 2007) and

attributed to the fact that tier 1 high schools are in such high demand in China. Panel B reveals no significant class size effect when focusing on multiple between school cutoffs.

Next, we check whether teacher quality varies at either threshold. Notably, as mentioned earlier, China has a unique way of ranking teachers according to their performance. Specifically, rather than focus on teacher attributes related to highest degree attained and years of experience, we focus on the number of students to superior (advanced rank) teachers in a school. In fact, Park & Hannum (2001) show that the quality ranks used in the Chinese schooling system contain a substantial amount of information on teacher quality that is not contained in conventional measures such as education of the teacher and years of experience. Panel C of Figure C11 reveals that the number of students to superior teachers falls by about 100—a 100 percent reduction—at the threshold. Put differently, the normalized number of superior teachers doubles at the threshold for attending a tier-1 school. On the other hand, Panel D indicates that this is not the case for a attending a better school, regardless of tier, as we observe no significant discontinuity at the threshold. These results are consistent with our previous results suggesting that peer effects alone cannot explain the variation in academic outcomes between both sets of identification strategies. They also suggest that teacher quality may have more of an impact on academic outcomes than peer effects. This is consistent with Park & Hannum (2001) who find suggestive evidence that much of the variation in Chinese elementary test scores (at least one fourth) is likely due to teacher differences.

Finally, we look at whether teacher gender differs at the cutoff. Specifically, we find no evidence of a compelling difference in teacher gender at the cutoff for attending a better school (Panel F) or a tier-1 school (Panel E), although the trends

is in contrast to what you would expect in top high schools in the U.S.

are different. This suggests that teacher gender does not explain the differences in academic outcomes by student gender as in Carrell et. al (2010).[31]

4.6.2 Threats to interpretation

One may wonder whether other factors unrelated to peer and school quality may be driving the results for academic outcomes. For instance, if going to to a tier 1 high school increases the likelihood of a student choosing a scientific track, then perhaps our results are mechanical in the sense that only students entering a scientific track witness an improvement in test scores, regardless of school quality.[32] To test for this, we check whether the probability of choosing an arts versus science track is discontinuous at the threshold for attending a tier 1 high school. Panel A of Appendix Figure C12 indicates that this is not a concern, as there is no significant change in the likelihood of entering into the arts track at the cutoff.

We also check whether age when taking the college entrance exam differs at the threshold for attending a tier 1 high school. Indeed, if tier-1 high schools were more likely to have their worse students repeat a grade, then perhaps the documented improvement in test scores in the 2010 college entrance exam is purely mechanical. [33] To test for this, we check if age at time of CET exam administration is discontinuous at the threshold for attending a tier-1 high school. Panel B of Figure C12 reveals this not to be the case, as we observe no significant difference in age at time of CET examination at the threshold.

[31]We must note however, that we have data only for all teachers (regardless of rank). There could still be substantial variation in the number of female versus male superior teachers.

[32]Recall, a student does not choose his/her track upon entry into high school. This choice is made in the 2nd year of high school.

[33]Although, we must note, grade repetition is far from common practice in China.

4.6.3 Long run outcomes

We have documented a significant increase in academic performance attributed to going to a tier 1 or elite high school. Further our LATE estimates imply that going to a tier 1 high school results in a 15 percent increase in the eligibility of attending a four year college. Naturally, one may wonder how these results extend into the long run—particularly into the labor market. While we do not have in sample information on earnings and employment for our population of interest, we draw from other studies to infer labor market returns to attending a tier 1 high school. Specifically, Giles et. al (2015) estimate a 37% return to attending a four year college in China. Similarly, Li et. al (2005) estimate that the return to each year of college is as much as 10%. While we are unable to see whether a student attends or graduates a four year college, our results indicate a significant 15% increase in the eligibility of attending a four year college. Given the importance of education in China, a large amount of students will end up taking this opportunity. This suggests significantly large labor market returns to attending a tier 1 high school in China.

4.7 Conclusion

In this chapter, we estimate the academic gains of having the opportunity to attend a higher quality high school. To do so, we use a regression discontinuity design that compares the academic outcomes of students who are barely eligible and barely ineligible to enter a high school of higher quality in three rural districts in China. Specifically, in this paper we draw a distinction between attending a better quality school, regardless of tier, and an elite high school. We find that having the opportunity to access better quality education results in a similar improvement in peer quality regardless of school tier. However, we find that only students who have access to a tier-1 high school witness an improvement in the high stakes college entrance exam. This leads to an increases in students' eligibility to attend a four year college, but not an elite college. We also find that these academic gains are driven by the male population despite both genders facing the same peer and school environment. Further, we find that the LATE estimates of attending a tier-1 school are approximately twice those of the ITT estimates of having the opportunity to access a tier-1 high school.

Assuming that peer effects are not highly non-linear in ability, we show that students' high school peer environment alone cannot explain the disparity in academic outcomes between going to an elite high school versus a better high school. Accordingly, utilizing a unique teacher rank designation in China, we provide evidence suggesting that variation in teacher quality may explain the disparity in results.

Our results have significant policy implications. First, while it remains an open question as to the extent to which our results apply to other settings, they suggest that the academic benefits to attending a top quality high school are sizable. As a result, expanding marginal students' access to elite high schools may result in significant gains in the college and the labor market. Finally, exploiting the unique setting of China, we find evidence suggesting that teacher quality has sizable effects on students' academic performance and potentially later lifetime outcomes. This suggests that a system of rewarding teachers based on performance may have potential merits.

5. GENERAL CONCLUSIONS

The three essays that compromise this dissertation have looked at the short and long run consequences of educational choices. In Chapter 2, we show that conscription policy leads to an increase in educational attainment—consistent with draft avoidance behavior. We further show that military service in itself does not affect labor market outcomes, suggesting that the returns to education induced by draft avoidance is zero. In Chapter 3, we find that increasing access to higher quality education—characterized by increased enrollment in a STEM major and in an institution with better performing peers—leads to a 13.6 percent earnings premium for low skilled students. In Chapter 4, we conclude that high school quality matters in terms of future academic performance—conditional on attending an elite high school. Further results suggest that this is mainly due to superior teaching quality in elite schools as opposed to improved peer quality.

All three essays use a regression discontinuity design, allowing us to overcome the problem of selection into education. While an RD design yields as good as random variation in treatment, this can often come at a cost to external validity. To the extent that our results generalize to other settings, they have significant policy implications. First, incentivized education—independent of the usual incentives to education—may not always lead to positive lifetime outcomes. Specifically, maximizing educational attainment, without granting students the right complements to education, can lead to undesirable outcomes. Second, increasing access to better quality education for low-skilled students can lead to significant labor market gains. Finally, our results also suggest that teacher quality matters more than peer quality for high school students. While more research on this matter is needed before generalizing any conclusions, the results do suggest that a system of compensating teachers based on student performance could be an effective tool.

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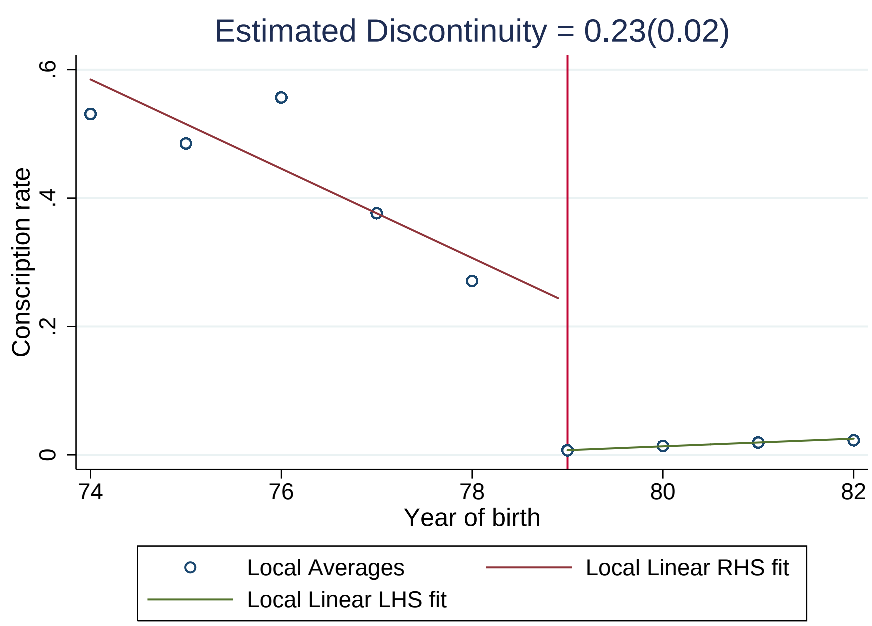
APPENDIX A

Figure A.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 Enquete 1998 a 10 ans. Standard errors reported in parantheses.

Figure A.2: Organization of high school in France.

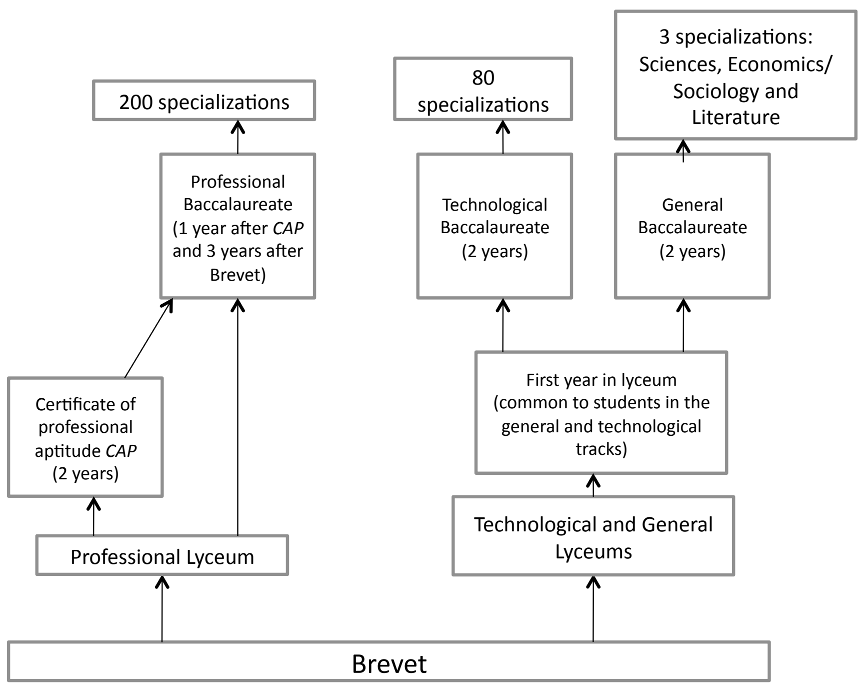
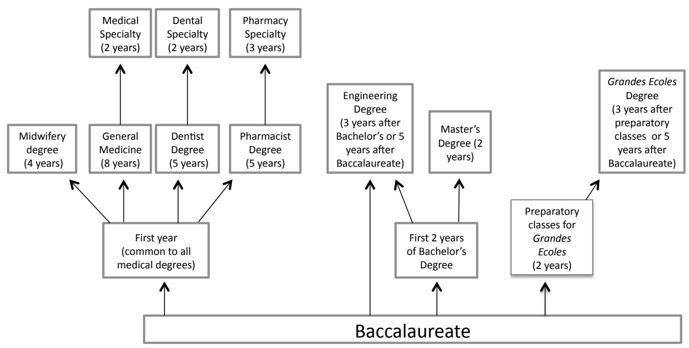


Figure A.3: Organization of higher education routes in France.

(a) Organization of traditional higher education in France



(b) Organization of vocational higher education in France

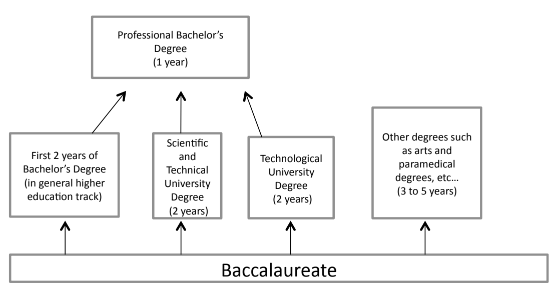
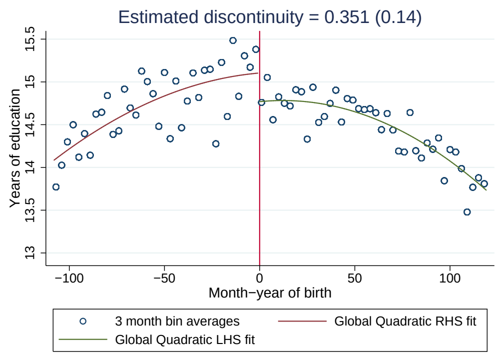
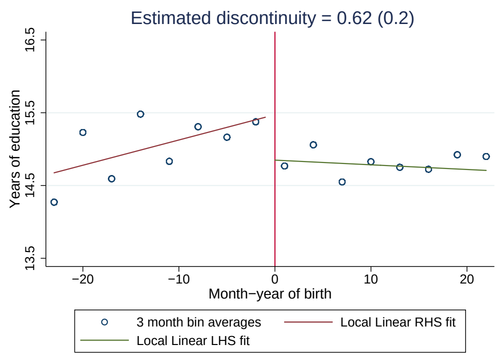


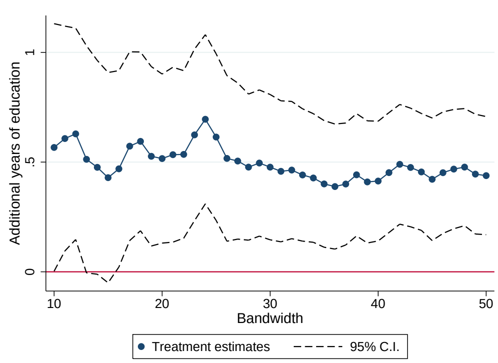
Figure A.4: Years of education estimates based on birth month relative to Jan 1, 1979 cutoff.



(a) Global quadratic graph.



(b) Local Linear graph.



(c) Bandwidth sensitivity analysis for local linear regression.

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

Figure A.5: Years of education residuals based on birth month relative to Jan 1, 1979 cutoff (Global quadratic graph).

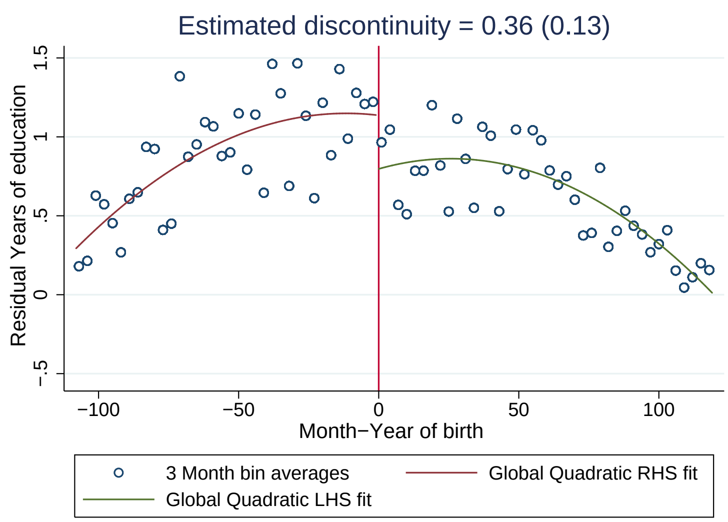
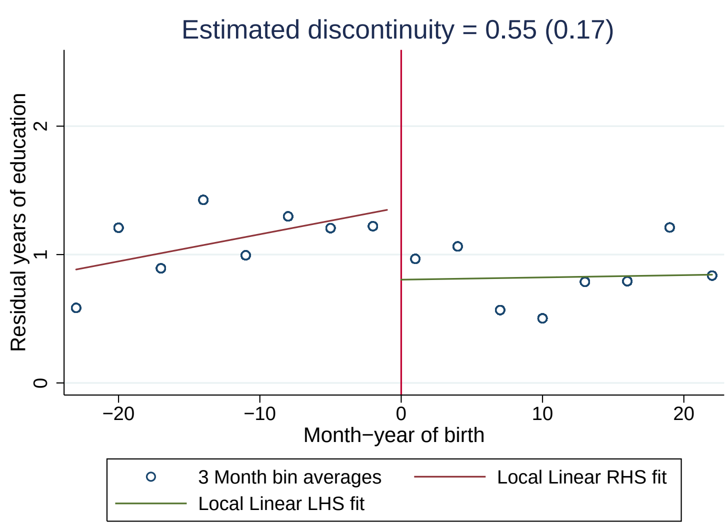


Figure A.6: Years of education residuals based on birth month relative to Jan 1, 1979 cutoff (Local linear graph).



- Notes: Controls used include month of birth fixed effects, a dummy for middle school enrollment as well as father's occupation.

Figure A.7: College graduation rates based on birth month relative to Jan 1, 1979 cutoff (Global quartic graph).

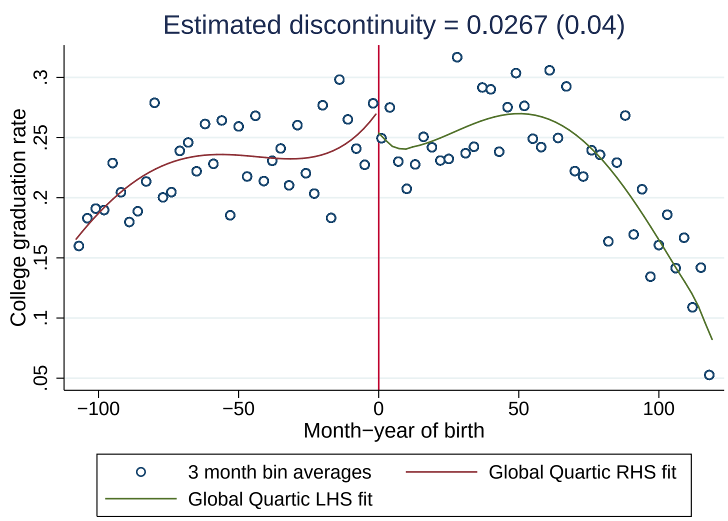
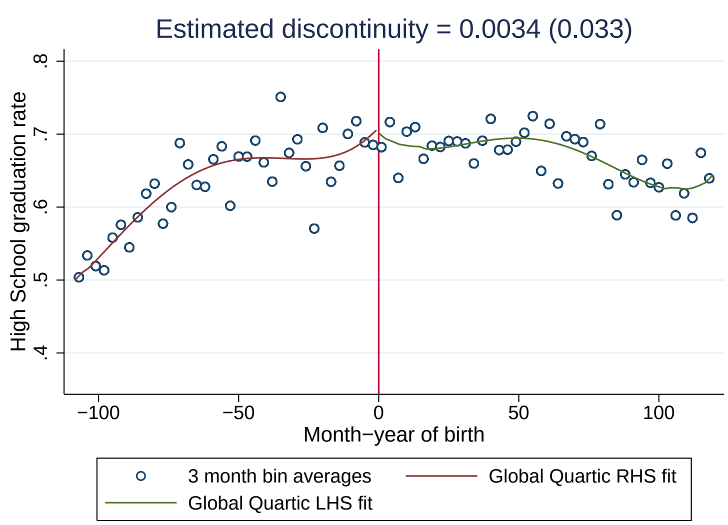
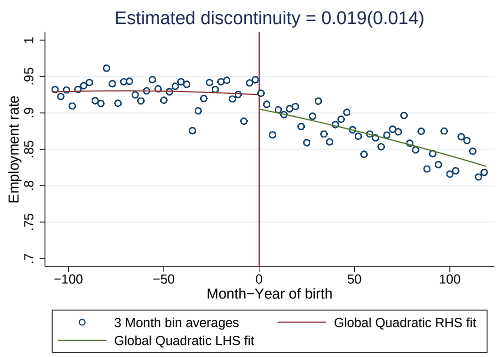


Figure A.8: High school graduation rates based on birth month relative to Jan 1, 1979 cutoff (Global quartic graph).

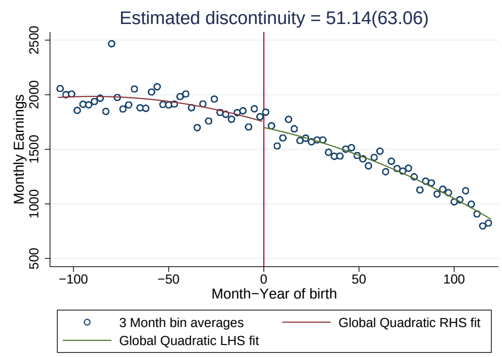


- Notes: High school graduation includes all forms of baccalaureate degrees (technical, professional, general). Standard errors are clustered at the birth-month year and reported in parentheses.

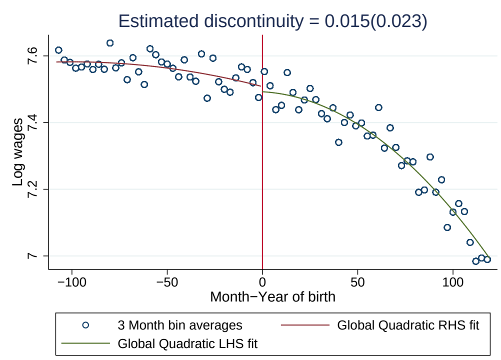
Figure A.9: Labor market outcomes based on birth month relative to Jan 1, 1979 cutoff (Global quadratic graphs).



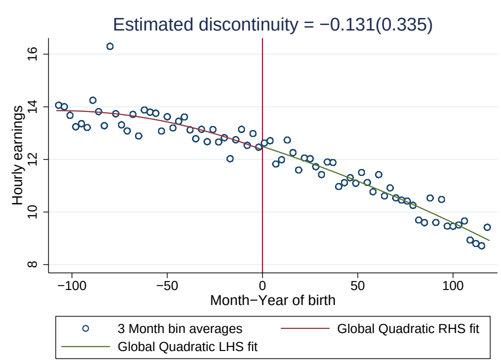
(a) Employment rates.



(b) Monthly wages.



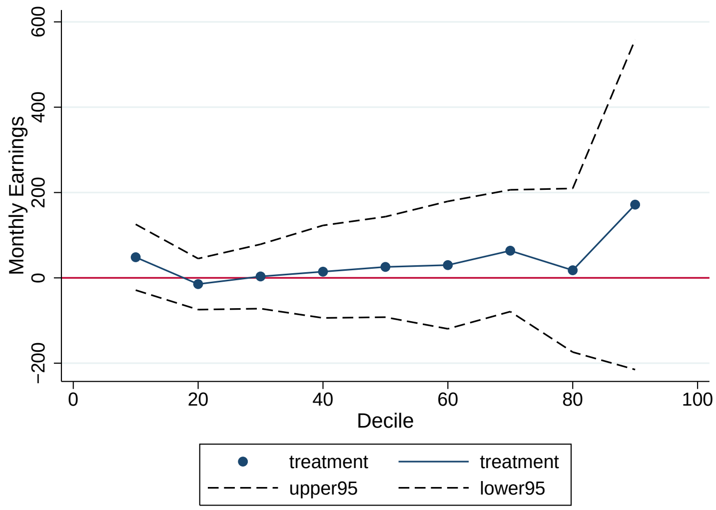
(c) Log monthly wages.



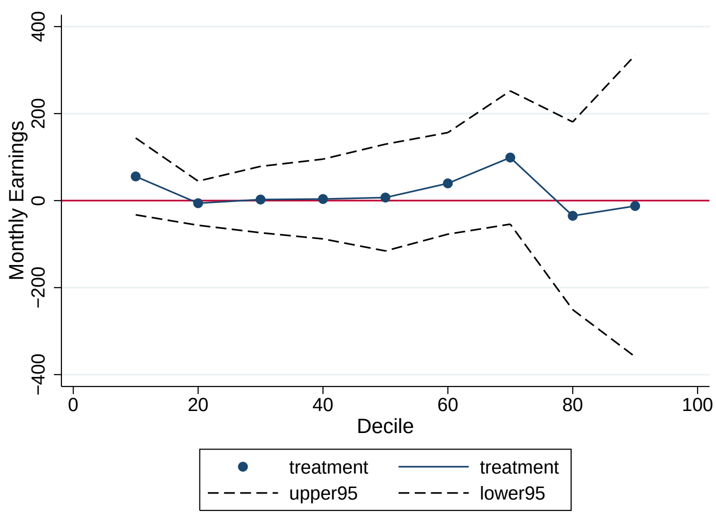
(d) Hourly wages.

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 A.10: Distributional treatment effects on monthly earnings.



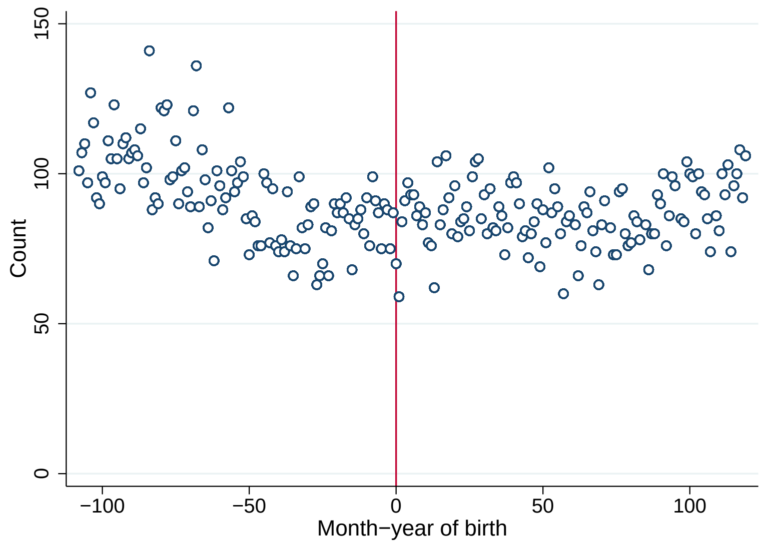
(a) Local linear regressions of h=30 months.



(b) Global quadratic regressions.

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 A.11: 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 A.12: Testing for the smoothness of predetermined characteristics (middle school enrollment) around cutoff using a global quartic graph.

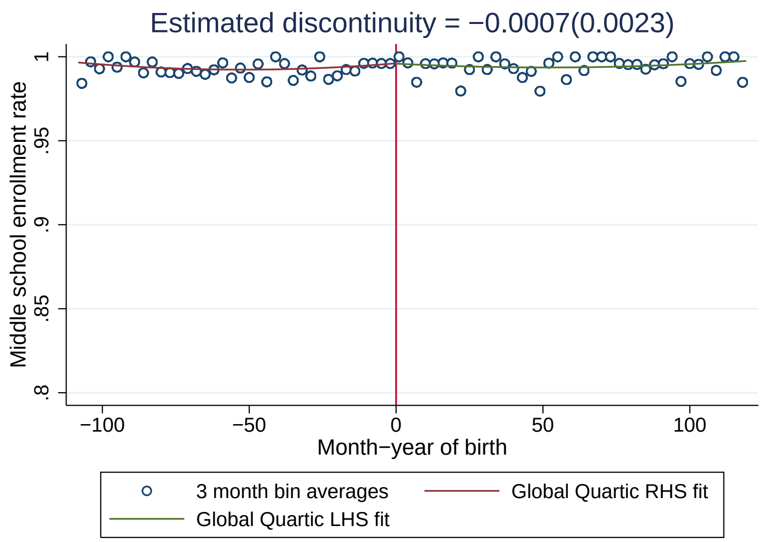
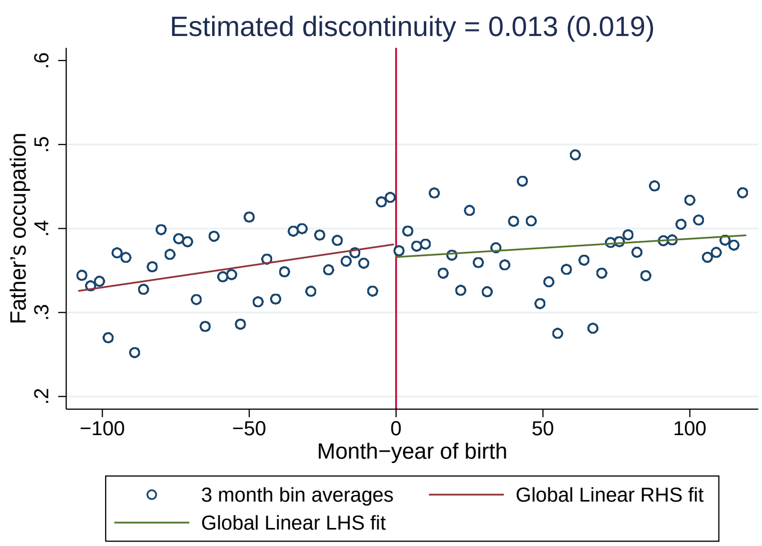
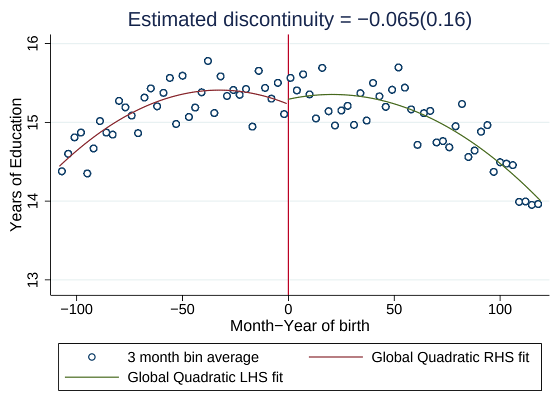


Figure A.13: Testing for the smoothness of predetermined characteristics (father's occupation) around cutoff using a global linear graph.

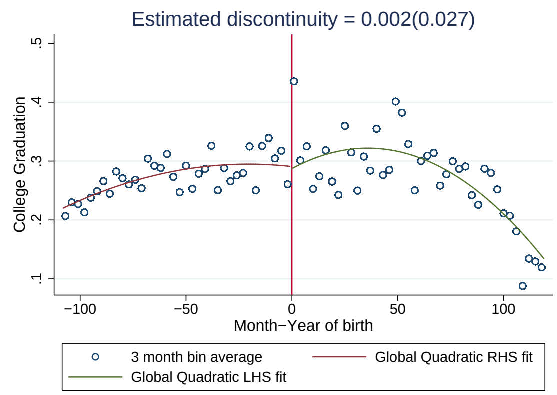


Notes: All regression estimates are weighted by population. Standard errors are clustered at the birth-month year and reported in parentheses.

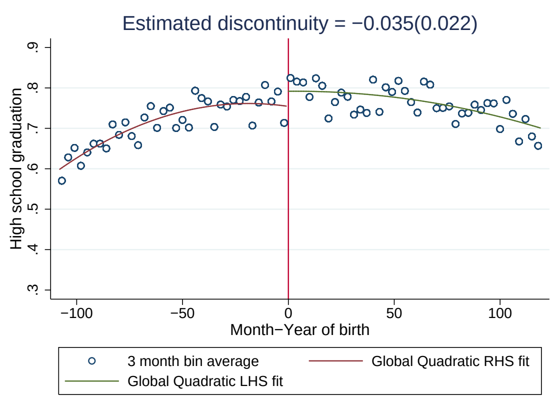
Figure A.14: Female outcomes based on birth-year month relative to Jan 1, 1979 cutoff (Global quadratic graphs).



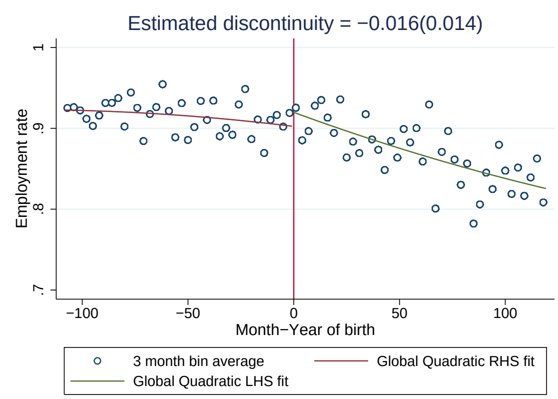
(a) Female years of education.



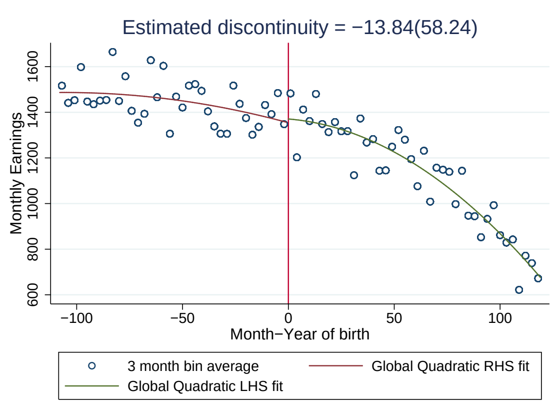
(b) Female college graduation.



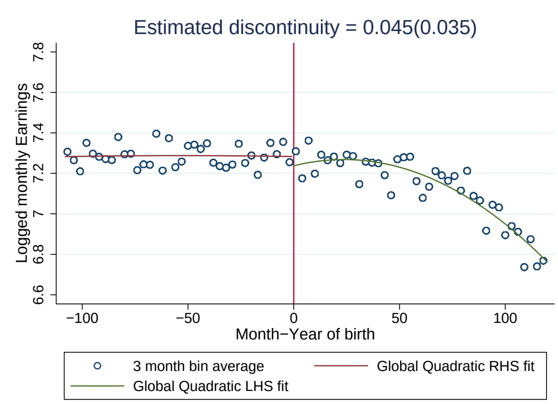
(c) Female high school graduation.



(d) Female employment rates

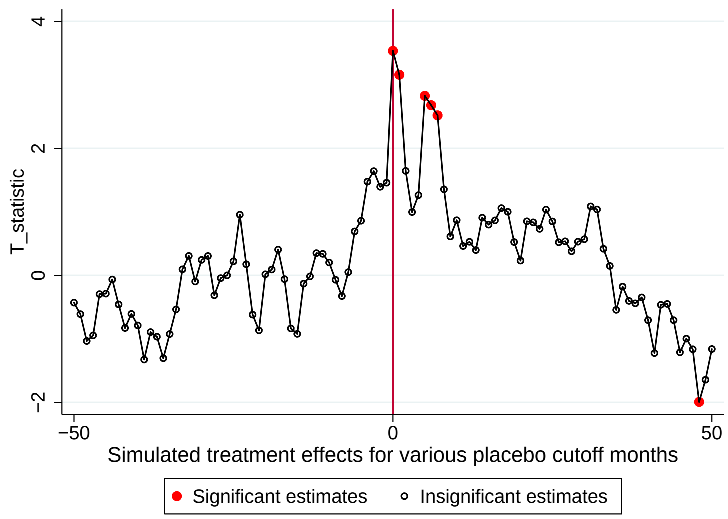


(e) Female monthly earnings



(f) Female logged earnings

Figure A.15: 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.

Figure A.16: Years of education for individuals with only primary schooling completed (Global linear graph).

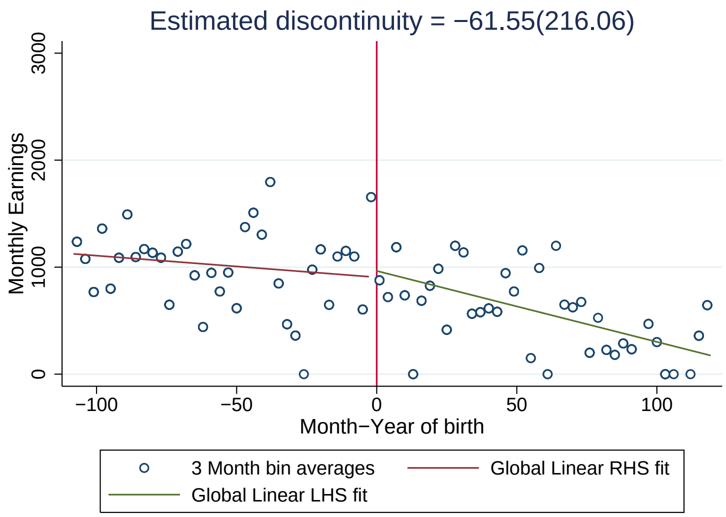
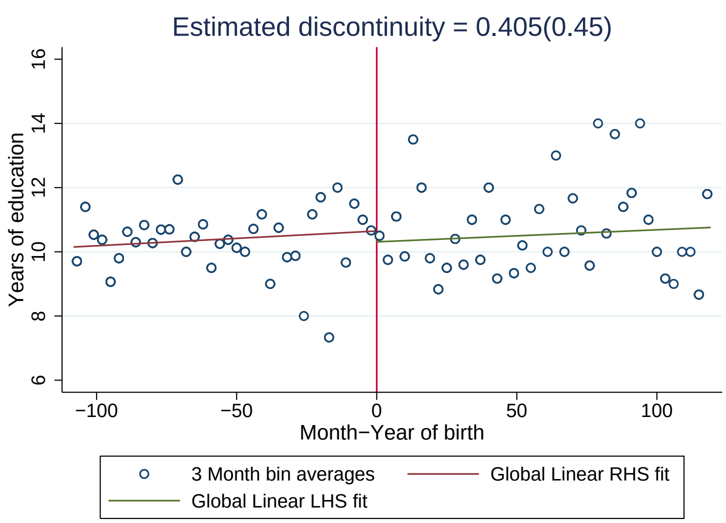
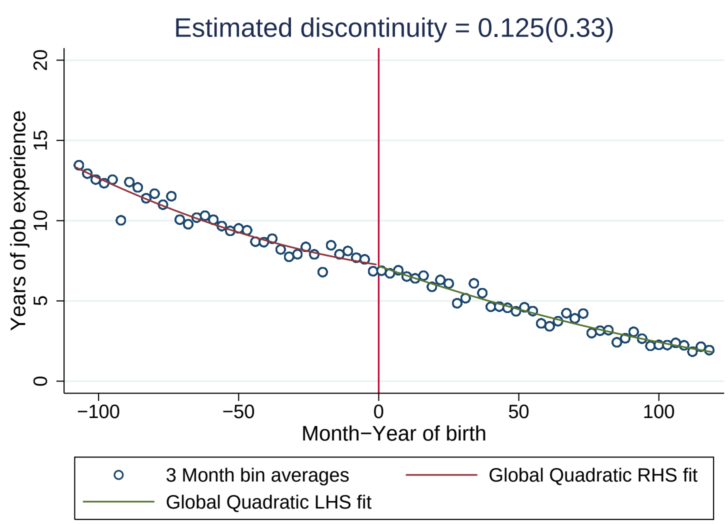


Figure A.17: Earnings for individuals with only primary schooling completed (Global linear graph.)



- 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 A.18: Years of job market experience (Global quadratic graph).



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

Table A.1: Male conscription rates based on birth year.

Birth cohort year	**All Males**	**Low S.E.S**	**High S.E.S**	**No. 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 A.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.

Table A.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	1340	3341	3822	4999	6622	6622	10051	13446	18975
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 (2014).
Controls include: 1) Birth month fixed effects (when bandwidth chosen is over a year) 2) A binary variable for middle school enrollment 3) father's occupation.
** p <0.01 ** p <0.05 * p <0.1

Table A.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 A.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 A.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	Degree	Local Linear	L.L BW*	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. All regressions have been weighted by population.
Sample includes French male citizens. Standard errors are clustered at the month-year level and reported in parentheses.
** p <0.01 ** p <0.05 * p <0.1

Table A.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

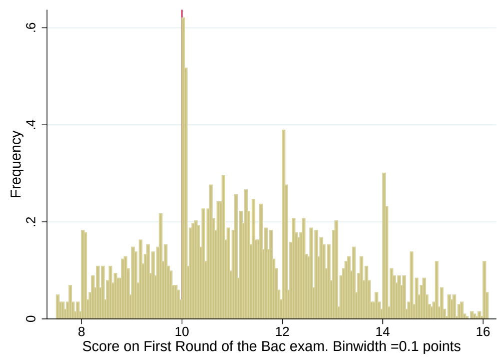
Table A.8: Placebo regression discontinuity estimates for females using different bandwidths and specifications.

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

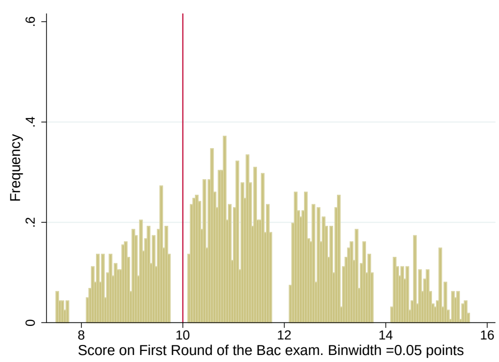
Notes: Sample includes only female French citizens. Standard errors are clustered by birth month-year and reported in parentheses. All regressions are weighted by population.
*** p <0.01 ** p <0.05 * p <0.1

APPENDIX B

Figure B.1: Distribution of scores on the first round of the French Baccalaureate in the year 2002.



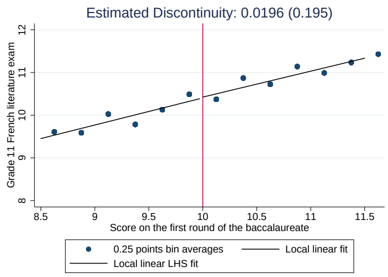
(a) Distribution of all students taking the exam.



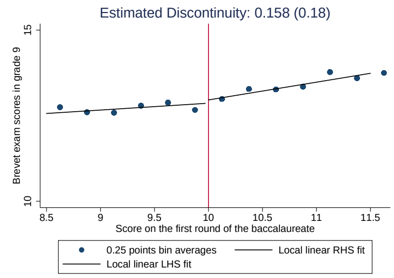
(b) Distribution of remaining students after dropping all heaped data.

Notes: Sample includes students who took the exam in the first round of the year 2002. Histograms reported with bin width of 0.05 points. Panel B drops all individuals scoring within 0.25 points to the left and 0.1 points to the right of each significant cutoff.

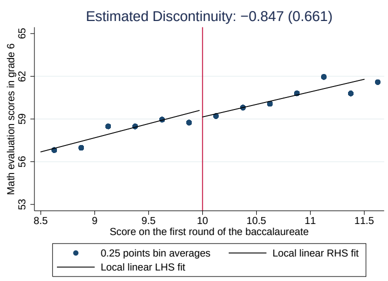
Figure B2: Testing for the smoothness of baseline characteristics.



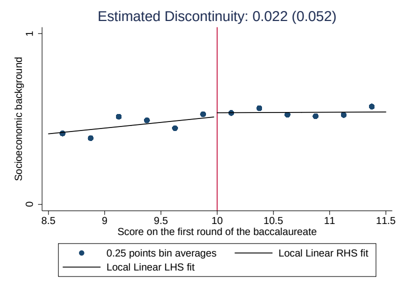
(a) National exam scores in French in grade 11.



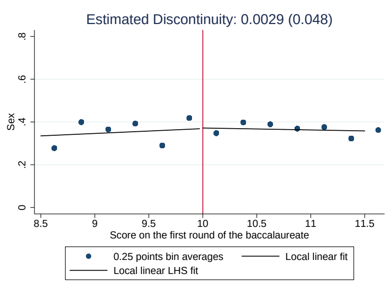
(b) Brevet exam scores in grade 9.



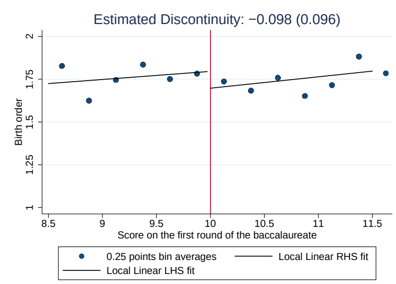
(c) Mathematics exam scores in grade 6.



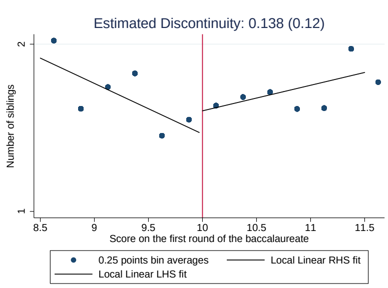
(d) Socioeconomic status/



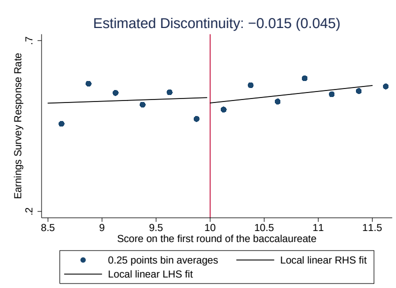
(e) Gender.



(f) Birth order.

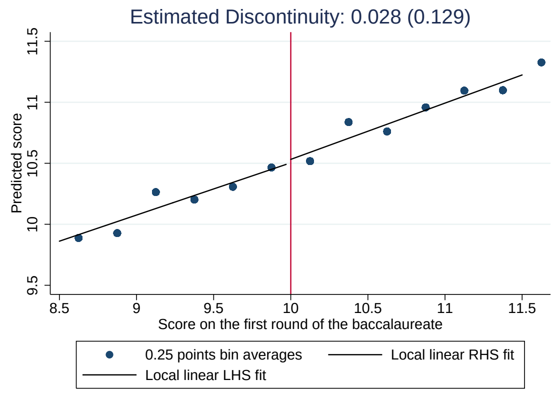


(g) Number of siblings.

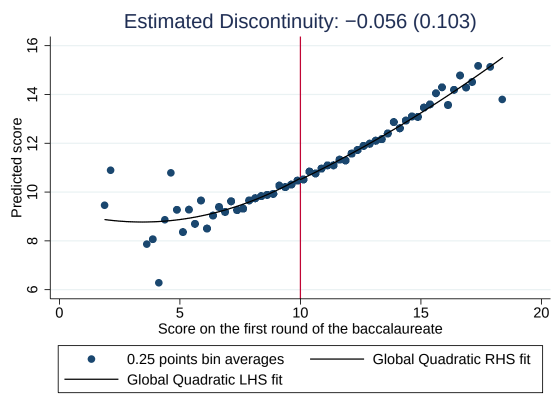


(h) Earnings survey response rate.

Figure B3: Predicted score based on baseline characteristics.



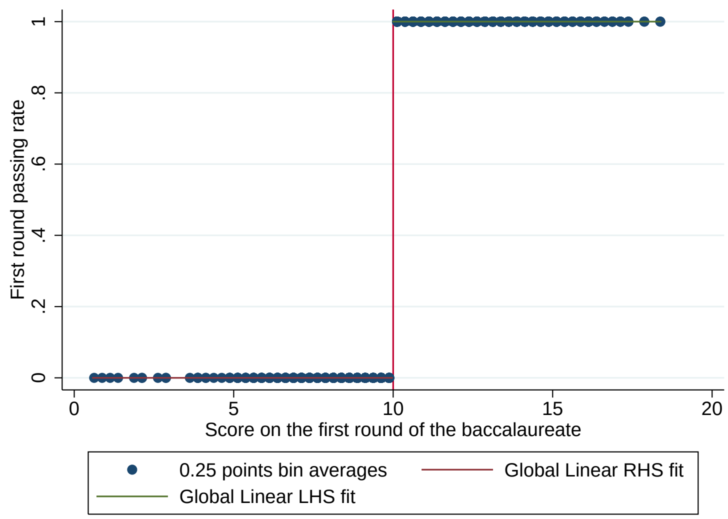
(a) Local linear.



(b) Global quadratic.

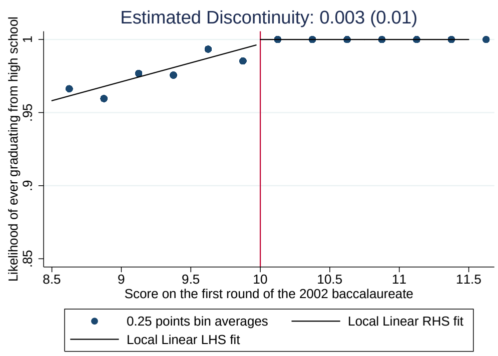
Notes: Robust standard errors reported in parentheses. Covariates include: Scores on the oral and written portion of the Grade 11 national French exam, Score on the Brevet exam in grade 9, mathematics scores on the grade 6 exam, socioeconomics status, number of siblings, birth order, place of residence and gender.

Figure B4: Likelihood of passing in the first round based on first round scores of the French Baccalaureate exam (Global linear graph).

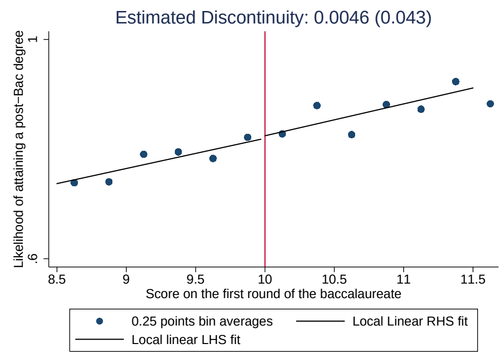


Notes: Sample includes students who took the exam in the first round of the year 2002. Robust standard errors reported in parentheses.

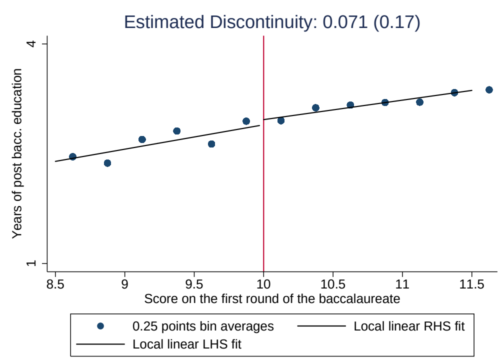
Figure B5: Quantity of education effects based on first round scores of the French Baccalaureate exam.



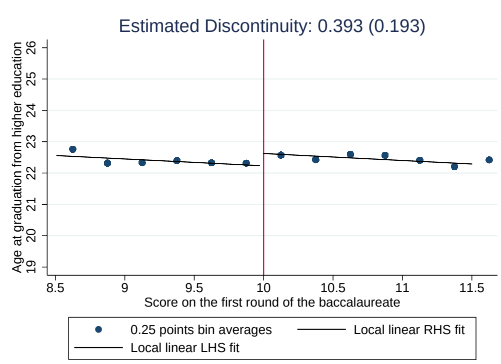
(a) Likelihood of attaining a high school degree.



(b) Likelihood of attaining a post-Baccalaureate degree.



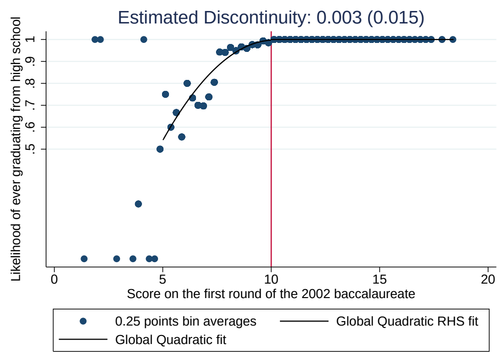
(c) Years of post-Baccalaureate education.



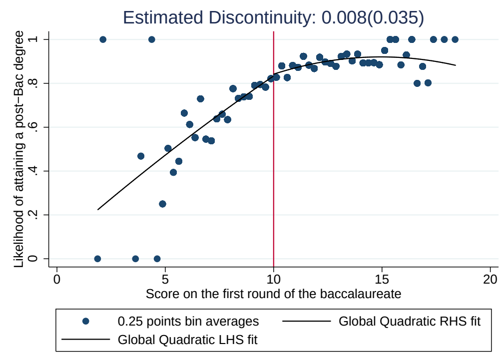
(d) Age at post-Baccalaureate graduation.

Notes: Robust standard errors reported in parentheses.

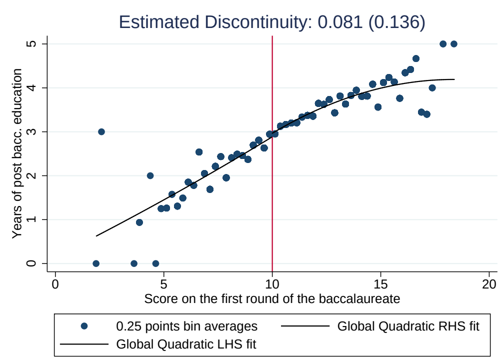
Figure B6: Quantity of education effects based on first round scores of the French Baccalaureate exam (Global polynomial graphs).



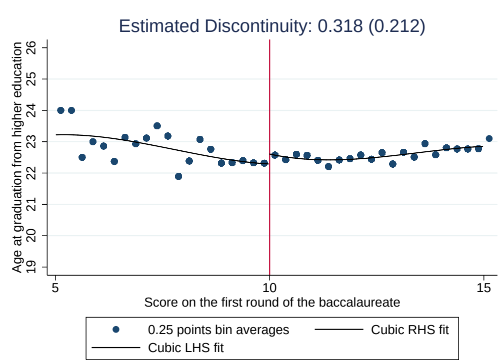
(a) Likelihood of attaining a high school degree.



(b) Likelihood of attaining a post-Baccalaureate degree.



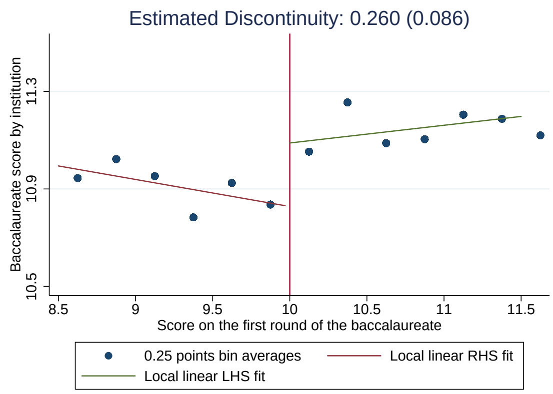
(c) Years of post-Baccalaureate education.



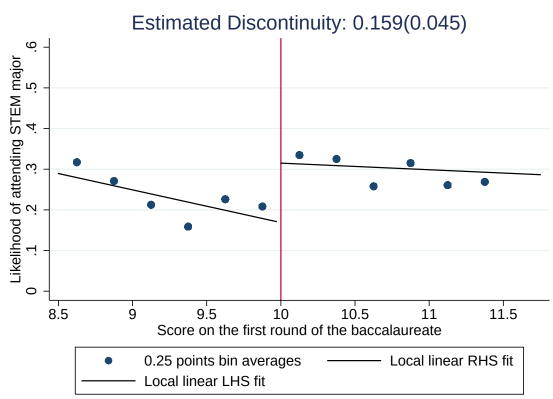
(d) Age at post-Baccalaureate graduation.

Notes: Robust standard errors reported in parentheses.

Figure B7: Quality of education effects based on first round scores of the French Baccalaureate exam.



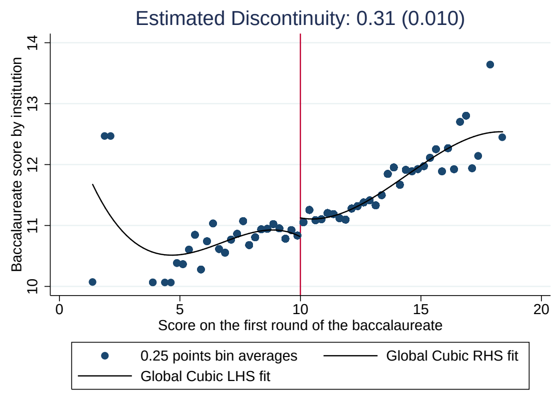
(a) Average Baccalaureate score by attended institution.



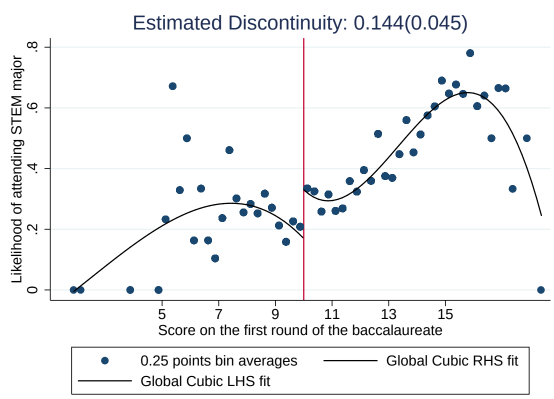
(b) Likelihood of attending STEM major.

Notes: Standard errors clustered by university and reported in parentheses (Robust standard errors used for STEM estimates).

Figure B8: Quality of education effects based on first round scores of the French Baccalaureate exam (Global polynomial graphs).



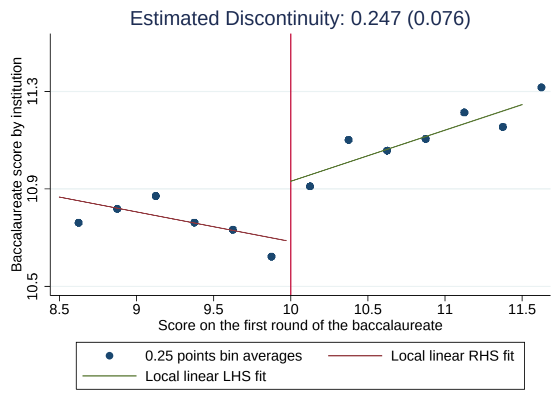
(a) Average Baccalaureate score by attended institution.



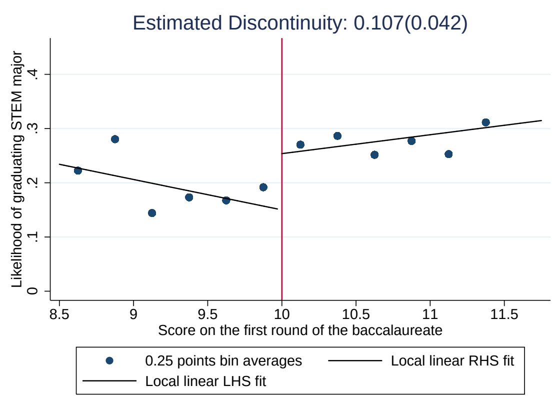
(b) Likelihood of attending STEM major.

Notes: Standard errors clustered by university and reported in parentheses (Robust standard errors used for STEM estimates).

Figure B9: Quality of education 'graduation' effects based on first round scores of the French Baccalaureate exam.



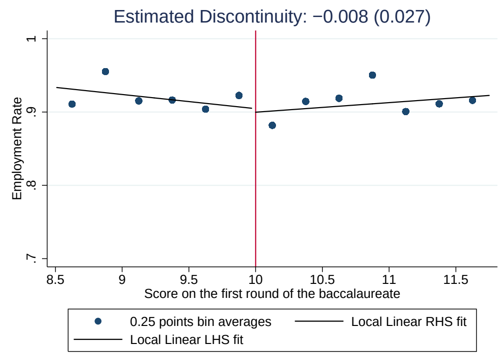
(a) Average Baccalaureate score by graduated institution.



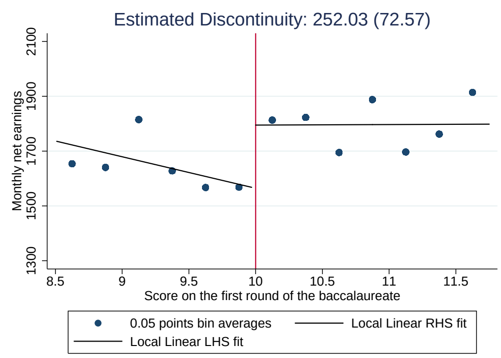
(b) Likelihood of graduating STEM major.

Standard errors clustered by university and reported in parentheses (Robust standard errors used for STEM estimates).

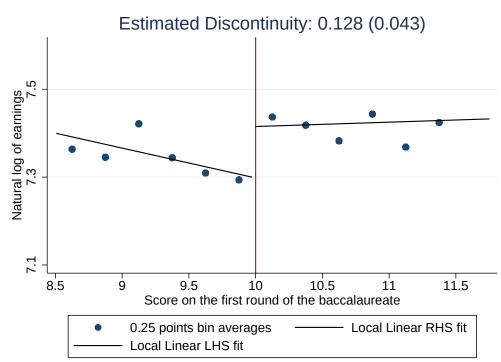
Figure B10: Labor market effects based on first round scores of the French Baccalaureate exam.



(a) Likelihood of employment.



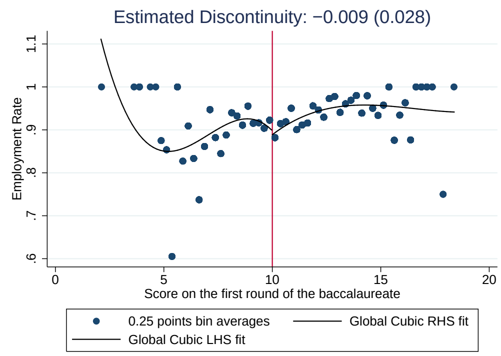
(b) Monthly earnings(in Euros).



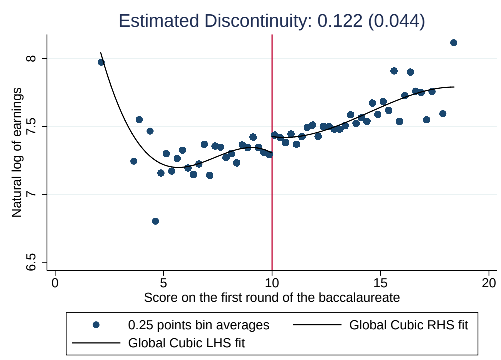
(c) Monthly logged earnings.

Notes: Wages are stacked for the two most recent years provided(2010-2011). Standard errors clustered at the individual level and reported in parentheses.

Figure B11: Labor market effects based on first round scores of the French Baccalaureate exam (Global polynomial graphs).



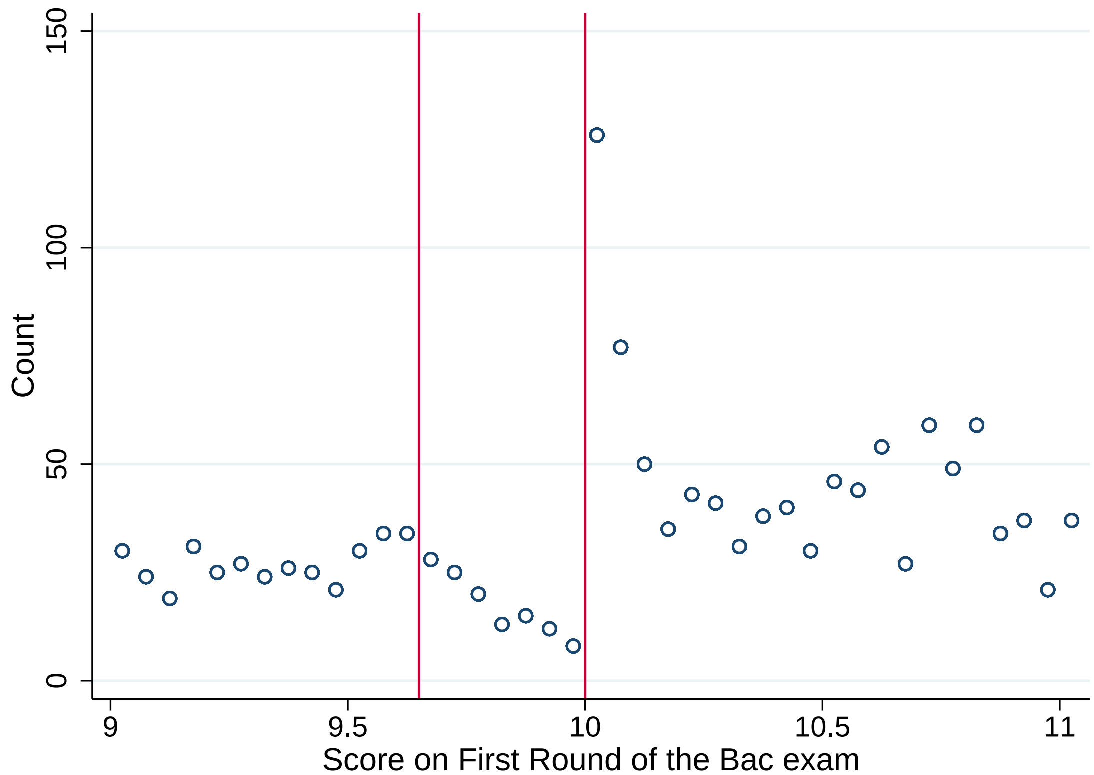
(a) Likelihood of employment.



(b) Monthly logged earnings.

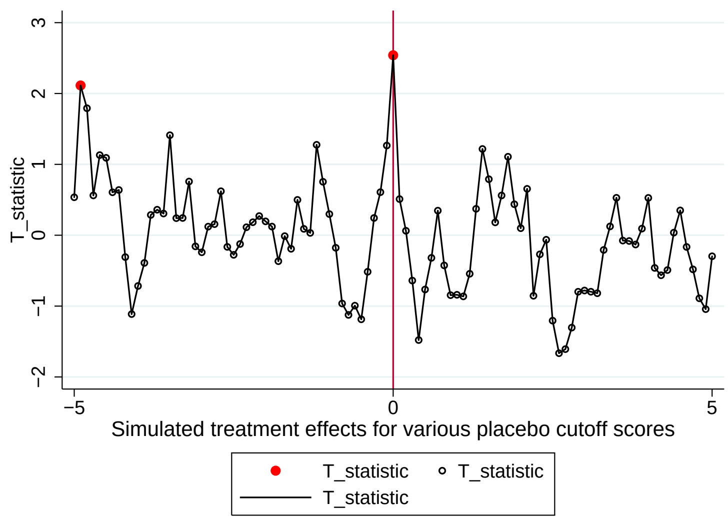
Notes: Wages are stacked for the two most recent years provided(2010-2011). Standard errors clustered at the individual level and reported in parentheses.

Figure B12: Distribution of scores on the first round of the French Baccalaureate in the year 2002 within a 9 to 11 test score grade window.



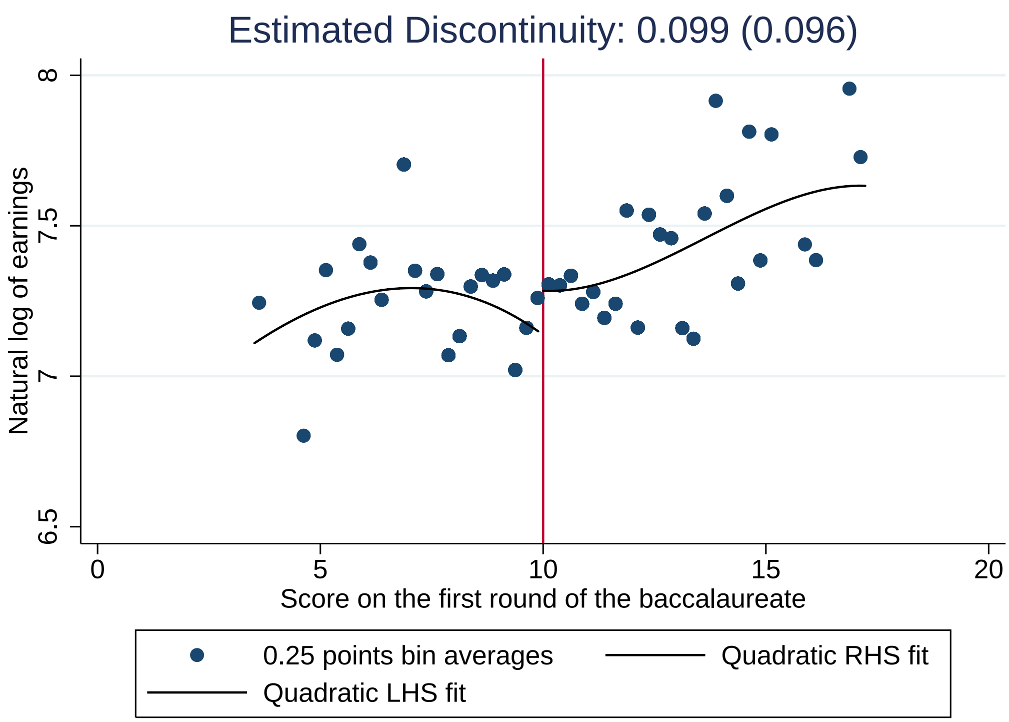
Notes: Histogram reported with bin width of 0.05 points.

Figure B13: Placebo test - T-statistics for reduced form effects on logged monthly wages using various fake cutoff scores.



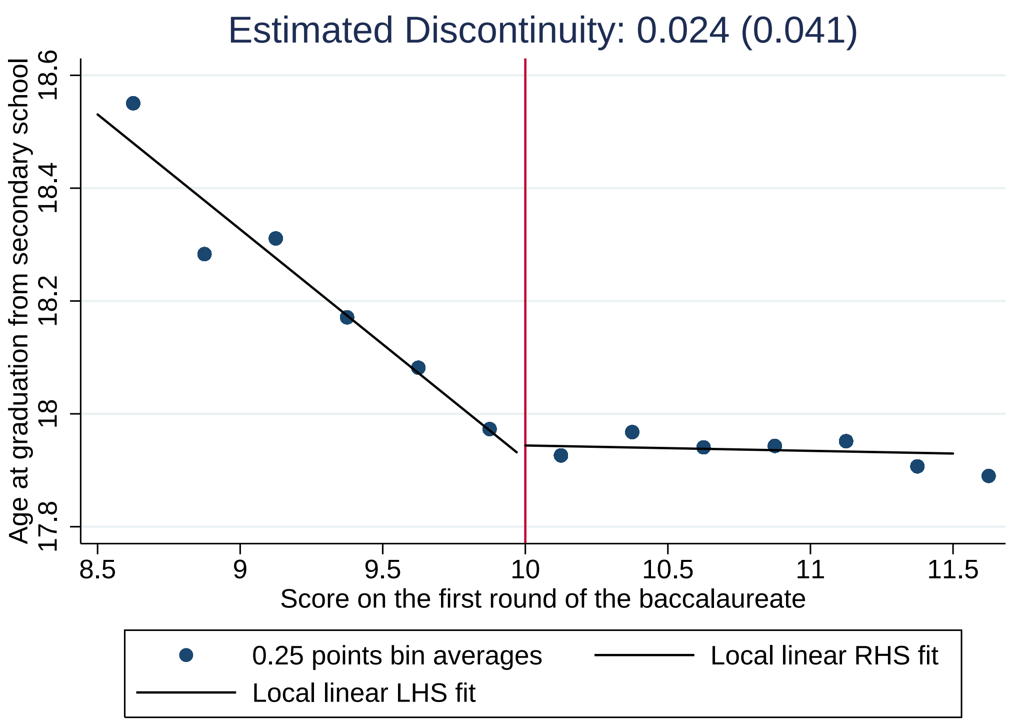
Notes: Each open circle represents the t-statistic from a local linear regression of bandwidth = 1.5 Baccalaureate points, using logged monthly wages as the dependent variable. A grade of zero on the x-axis represents the original passing threshold grade of 10, and we simulate 50 fake cutoff treatment effects to the right and left of that point within intervals of 0.1 score points. Clustered standard errors are used for computation of t-stats.

Figure B14: Discontinuity in earnings for individuals who never attended college (Global polynomial graph).



Notes: Wages are stacked for the two most recent years provided(2010-2011). Standard errors clustered at the individual level and reported in parentheses.

Figure B15: Discontinuity in age at graduation from secondary school.



Notes: Robust standard errors reported in parentheses.

Table B.1: Classification of majors into STEM and non-STEM degrees.

appendix.pdf

1. **STEM designated majors**

   Agricultural sciences
   Economic sciences
   Engineering
   Fundamental sciences and applications
   Life sciences, health and earth sciences
   Materials sciences
   Medical degrees
   Pharmacy
   Sciences and technology

2. **Non-STEM majors**

   Accounting degrees
   Arts
   Higher technical certificate of production
   Higher technical certificate of services
   Languages
   Paramedical degrees
   Political Sciences
   Professional degrees
   Social sciences and humanities degrees
   Social work degrees
   Sports
   Technical degrees

Table B.2: Summary statistics for students who sat for the first round of the 2002 general Baccalaureate exam.

Variable	**Mean**
Male	**0.38**
	(0.48)
Birth order	**1.74**
	(0.95)
Number of siblings	**1.68**
	(1.17)
High S.E.S.	**0.57**
	(0.49)
[0.2em] Score on the Brevet exam	**13.7**
	(1.95)
Score on the French oral exam	**12.2**
	(2.93)
Score on the French written exam	**10.2**
	(2.94)
Score on the Baccalaureate exam	**11.17**
	(2.24)
Percentage of first time passers	**0.75**
	(0.43)
High school graduation rate	**0.98**
	(0.14)
Years of Post-Baccalaureate education	**3.2**
	(1.63)
STEM enrollment rate	**0.28**
	(0.45)
Employment rate in 2011 and 2012	**0.93**
	(0.25)
Monthly earnings in 2011 (in Euros)	**1625**
	(818)
Monthly earnings in 2012 (in Euros)	**1725**
	(881)
Observations	4337

mean coefficients; sd in parentheses

The number of observations represents students with reported grades on the first round of the 2002 General Baccalaureate exam.

Table B.3: Regression discontinuity estimates for baseline covariates.

Bandwidth	0.25 points	1 points	1.5 points	2 points	2.5 points	5 points
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Discontinuity in Grade 11 French exam	–.119	–.098	.020	–.260	–.044	.042
	(.26)	(.24)	(.20)	(.26)	(.24)	(.24)
Panel B: Discontinuity in Brevet exam in grade 9	.250	.131	.158	.193	.160	.101
	(.24)	(.23)	(.19)	(.25)	(.23)	(.23)
Panel C: Discontinuity in National Grade 6 Maths exam	.424	.034	–.516	–1.323	–.352	–.786
	(1.06)	(1.09)	(.88)	(1.18)	(1.10)	(1.05)
Panel D: Discontinuity in S.E.S	.011	.094	.023	.037	.049	.049
	(.07)	(.07)	(.05)	(.07)	(.06)	(.06)
Panel E: Discontinuity in Gender	–.057	.002	.003	–.019	–.025	–.030
	(.07)	(.06)	(.05)	(.07)	(.06)	(.06)
Panel F: Discontinuity in birth order	.001	–.021	–.098	–.019	–.088	–.154
	(.13)	(.13)	(.10)	(.13)	(.12)	(.12)
Panel G:: Discontinuity in number of siblings	.080	.205	.138	.228	.103	.098
	(.14)	(.14)	(.12)	(.15)	(.14)	(.14)
Panel H:: Discontinuity in prob. of being in earnings survey	.045	–.020	–.016	–.010	–.011	.018
	(.05)	(.04)	(.04)	(.05)	(.04)	(.04)
Score Polynomial	Zero	One	One	Two	Two	Three
Observations	401	1310	1855	2314	2717	3802

Notes: Robust standard errors reported in parentheses. Socioeconomic status proxied by father's occupation.
** p <0.01 ** p <0.05 * p <0.1

Table B.4: Regression discontinuity estimates for quantity of education measures.

Bandwidth	0.5 points	1 points	1.5 points	2 points	2.5 points	5 points
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Discontinuity in likelihood of ever graduating secondary school	.010	.003	.003	−.005	−.008	.045**
	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)
With Controls	.012	.003	.009	−.000	−.003	.045**
	(.01)	(.01)	(.01)	(.02)	(.01)	(.02)
Panel B: Discontinuity in likelihood of having a post Baccalaureate degree	.050	.019	.005	−.018	.016	.023
	(.04)	(.05)	(.04)	(.06)	(.05)	(.05)
With Controls	.051	.007	.003	−.016	.013	.003
	(.04)	(.06)	(.05)	(.06)	(.06)	(.06)
Panel C: Discontinuity in years of Post-Bacc. education	.304**	.125	.071	.051	.070	.065
	(.14)	(.21)	(.17)	(.23)	(.21)	(.21)
With Controls	.355**	.185	.151	.172	.139	.079
	(.16)	(.23)	(.18)	(.24)	(.22)	(.22)
Panel D: Discontinuity in age at Post-Bacc. graduation	0.201	0.191	0.393**	0.361	0.332	0.317
	(0.15)	(0.23)	(0.19)	(0.23)	(0.21)	(0.21)
With Controls	.273	.229	.450**	.396	.355	.288
	(.18)	(.26)	(.21)	(.28)	(.25)	(.25)
Score Polynomial	Zero	One	One	Two	Two	Three
Observations	679	1310	1855	2316	2720	3807

Notes: Controls include exam specialization fixed effects, date of birth, number of siblings,birth order, socioeconomic status, scores on the Brevet examination, score on the grade 11 national French exam and scores in grade 6 national assessment exam in mathematics. *** p <0.01 ** p <0.05 * p <0.1. Robust standard errors reported in parentheses.

Table B.5: Regression discontinuity estimates for education quality measures using different bandwidths and specifications.

Bandwidth	0.5 points	1 points	1.5 points	2 points	2.5 points	5 points
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Discontinuity in average institution Baccalaureate score	.217***	.246**	.261***	.307**	.260***	.292**
	(.07)	(.10)	(.09)	(.12)	(.10)	(.12)
With Controls	.222***	.233**	.257***	.293***	.234***	.259***
	(.07)	(.09)	(.07)	(.10)	(.08)	(.09)
Panel B: Discontinuity in likelihood of being in STEM major	.107***	.124**	.159***	.151**	.160***	.170***
	(.04)	(.06)	(.05)	(.06)	(.06)	(.05)
With Controls	.116***	.115**	.161***	.157***	.167***	.163***
	(.04)	(.05)	(.04)	(.06)	(.05)	(.05)
Score Polynomial	Zero	One	One	Two	Two	Three
Observations	630	1254	1793	2245	2641	3715

Notes: Controls include exam specialization fixed effects, date of birth, number of siblings,birth order, socioeconomic status, scores on the Brevet examination, score on the grade 11 national French exam and scores in grade 6 national assessment exam in Mathematics. *** p <0.01 ** p <0.05 * p <0.1. Standard errors clustered by university and reported in parentheses (Robust standard errors used for STEM estimates)

Table B.6: Regression discontinuity estimates for labor market outcomes using different bandwidths and specifications.

Bandwidth	0.5 points	1 points	1.5 points	2 points	2.5 points	5 points
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Discontinuity in Employment rates	−.019	−.029	−.008	−.015	.009	.002
	(.02)	(.03)	(.03)	(.04)	(.03)	(.03)
With Controls	−.033	−.035	−.020	−.024	.006	−.003
	(.02)	(.04)	(.03)	(.04)	(.04)	(.04)
Panel B: Discontinuity in net monthly earnings	250.29***	343.57***	252.03***	340.58***	279.5***	255.40***
	(62.59)	(92.57)	(72.57)	(96.56)	(87.96)	(88.00)
With Controls	218.58***	275.13***	242.72***	313.47***	243.42***	222.49**
	(63.15)	(93.95)	(72.07)	(97.41)	(87.24)	(87.18)
Panel C: Discontinuity in monthly logged earnings	.126***	.180***	.128***	.176***	.142***	.147***
	(.04)	(.06)	(.04)	(.06)	(.05)	(.06)
With Controls	.120***	.140**	.129***	.172***	.132**	.144**
	(.04)	(.06)	(.05)	(.06)	(.06)	(.06)
Score Polynomial	Zero	One	One	Two	Two	Three
Observations	711	1404	1991	2532	3003	4296

Notes: Controls include exam specialization fixed effects, date of birth, number of siblings,birth order, socioeconomic status, scores on the Brevet examination, score on the grade 11 national French exam, scores in grade 6 national assessment exam in Mathematics. Standard errors are clustered at the individual level and reported in parentheses. *** p <0.01 ** p <0.05 * p <0.1

Table B.7: 'Donut' type regression discontinuity estimates for quantity of education variables.

Bandwidth	0.5 points (1)	1 points (2)	1.5 points (3)	2 points (4)	2.5 points (5)
Panel A: Discontinuity in likelihood of ever graduating secondary school					
(Excluding [**9.75-10.1**] region)	.007	−.006	−.002	−.023	−.024
	(.01)	(.01)	(.01)	(.02)	(.02)
(Excluding [**9.65-10.1**] region)	.011	−.000	.002	−.022	−.024
	(.01)	(.02)	(.02)	(.03)	(.02)
Panel B: Discontinuity in likelihood of having a post Baccalaureate degree					
(Excluding [**9.75-10.1**] region)	.010	.078	.068	.019	.003
	(.06)	(.08)	(.06)	(.09)	(.08)
(Excluding [**9.65-10.1**] region)	.085	.074	.017	−.009	.061
	(.05)	(.10)	(.06)	(.11)	(.09)
Panel C: Discontinuity in years of Post-Baccalaureate education					
(Excluding [**9.75-10.1**] region)	.443**	.318	.145	.226	.211
	(.17)	(.31)	(.22)	(.34)	(.30)
(Excluding [**9.65-10.1**] region)	.406**	.172	.039	.030	.059
	(.20)	(.37)	(.25)	(.41)	(.35)
Score Polynomial	Zero	One	One	Two	Two
Observations (excluding 9.75-10.1)	411	1042	1587	2048	2452
Observations (excluding 9.65-10.1)	361	992	1537	1998	2402

Notes: *** p <0.01 ** p <0.05 * p <0.1. Robust standard errors reported in parentheses.

Table B.8: 'Donut' type regression discontinuity estimates for quality of education variables.

Bandwidth	0.5 points	1 points	1.5 points	2 points	2.5 points
	(1)	(2)	(3)	(4)	(5)
Panel A: Discontinuity in average institution Baccalaureate score					
(Excluding [**9.75-10.1**] region)	.291***	.377***	.337***	.455***	.387***
	(.08)	(.13)	(.09)	(.17)	(.12)
(Excluding [**9.65-10.1**] region)	.286***	.425***	.362***	.530***	.414***
	(.09)	(.15)	(.11)	(.19)	(.13)
Panel B: Discontinuity in likelihood of being in STEM major					
(Excluding [**9.75-10.1**] region)	.111**	.150*	.187***	.209**	.209***
	(.05)	(.08)	(.06)	(.09)	(.08)
(Excluding [**9.65-10.1**] region)	.127**	.207**	.231***	.286***	.270***
	(.05)	(.10)	(.07)	(.11)	(.09)
Score Polynomial	Zero	One	One	Two	Two
Observations (excluding 9.75-10.1)	403	1027	1566	2018	2414
Observations (excluding 9.65-10.1)	358	982	1521	1973	2369

Notes: *** p <0.01 ** p <0.05 * p <0.1. Standard errors clustered by university and reported in parentheses.

Table B.9: 'Donut' type regression discontinuity estimates for labor market variables.

Bandwidth	0.5 points (1)	1 points (2)	1.5 points (3)	2 points (4)	2.5 points (5)
Panel A: Discontinuity in likelihood of employment					
(Excluding [**9.75-10.1**] region)	−.011	−.021	.012	.020	.050
	(.03)	(.05)	(.04)	(.06)	(.05)
(Excluding [**9.65-10.1**] region)	−.032	−.061	−.008	−.022	.031
	(.03)	(.06)	(.04)	(.07)	(.06)
Panel B: Discontinuity in net monthly earnings					
(Excluding [**9.75-10.1**] region)	276.763***	439.358***	275.388***	457.806***	321.402***
	(81.43)	(138.75)	(94.90)	(145.81)	(124.27)
(Excluding [**9.65-10.1**] region)	236.559***	401.969**	211.524**	367.397**	224.037
	(89.70)	(162.32)	(105.61)	(168.49)	(141.90)
Score Polynomial	Zero	One	One	Two	Two
Observations (excluding 9.75-10.1)	433	1128	1715	2256	2727
Observations (excluding 9.65-10.1)	375	1070	1657	2198	2669

Notes: *** p <0.01 ** p <0.05 * p <0.1. Std. errors clustered at the indiv. level and reported in parentheses.

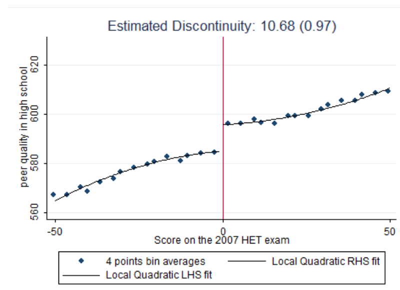
Table B.10: Regression discontinuity estimates for individuals from low socioeconomic backgrounds.

Bandwidth	0.5 points	1 points	1.5 points	2 points	2.5 points	5 points
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Averaged institution						
Baccalaureate score	.149	.107	.173	.235	.159	.059
	(.10)	(.17)	(.12)	(.18)	(.17)	(.10)
Panel B: Likelihood of being in						
STEM major	.178***	.234***	.264***	.296***	.304***	.317***
	(.05)	(.07)	(.06)	(.08)	(.07)	(.07)
Panel C: Monthly logged earnings	.138***	.187***	.108**	.188***	.126**	.129**
	(.04)	(.06)	(.05)	(.06)	(.06)	(.06)
Score Polynomial	Zero	One	One	Two	Two	Three
Observations	327	674	959	1181	1369	1795

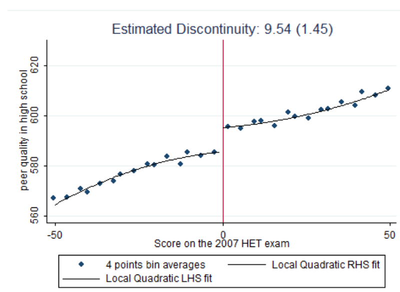
Notes: Standard errors are clustered at the individual level and reported in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

APPENDIX C

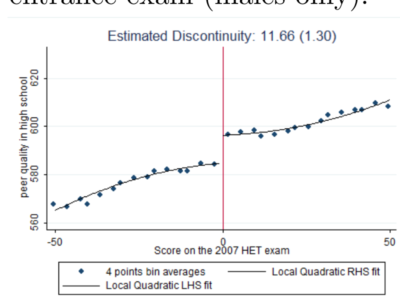
Figure C1: Local polynomial figures for school inputs [Going to a better school].



(a) Peer quality based on scores on high school entrance exam.



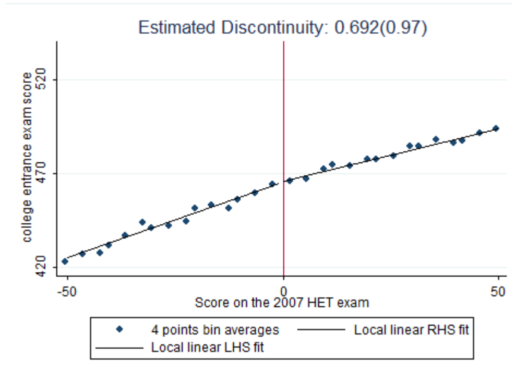
(b) Peer quality based on scores on high school entrance exam (males only).



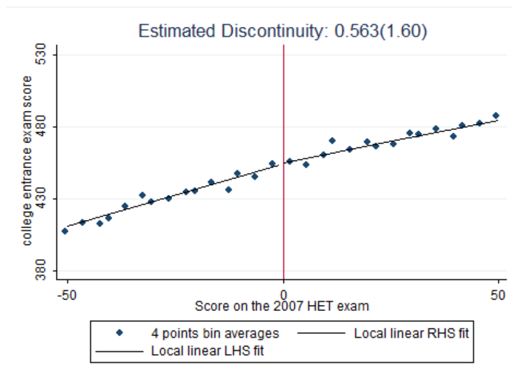
(c) Peer quality based on scores on high school entrance exam (females only).

Notes: Sample includes students who took the high school entrance exam (HET) in the year 2007.

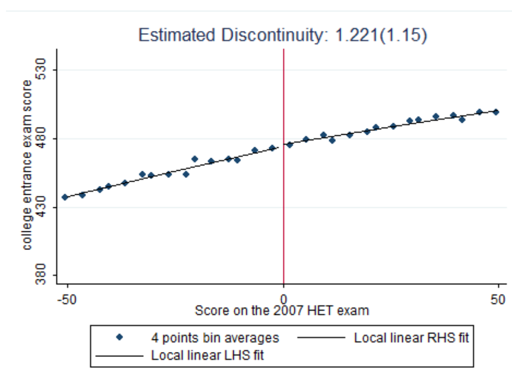
Figure C2: Local polynomial figures for college entrance exam test scores [Going to a better school].



(a) College entrance exam test scores.



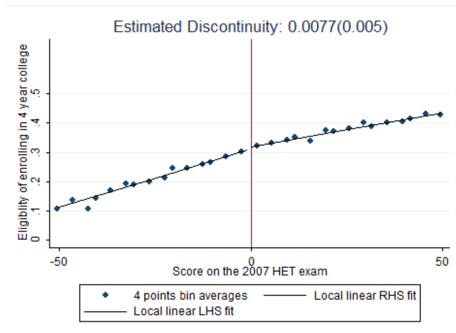
(b) College entrance exam test scores (males only).



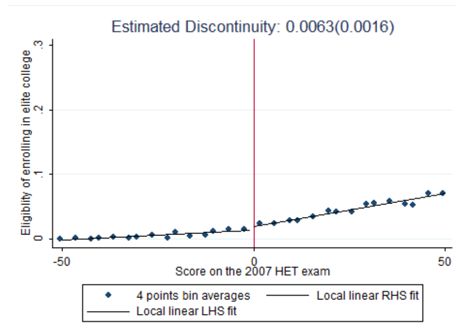
(c) College entrance exam test scores (Females only).

Notes: Sample includes students who took the high school entrance exam (HET) in the year 2007.

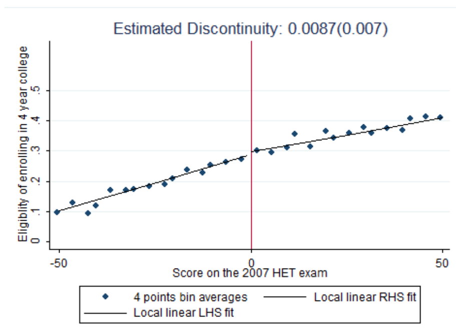
Figure C3: Local polynomial figures for college outcomes [Going to a better school].



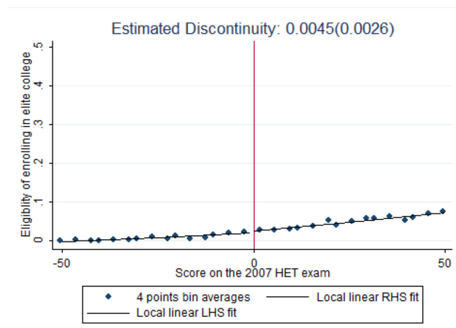
(a) Eligibility to attend a four year college.



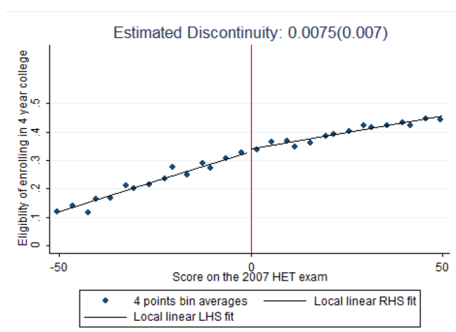
(b) Eligibility to attend an elite college.



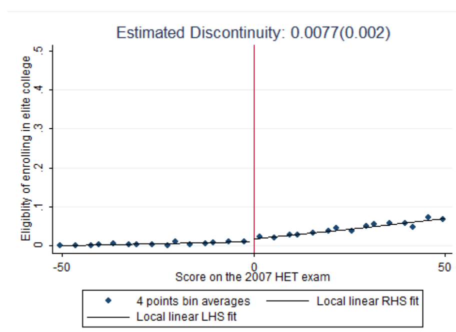
(c) Eligibility to attend a four year college (Males only).



(d) Eligibility to attend an elite college (Males only).



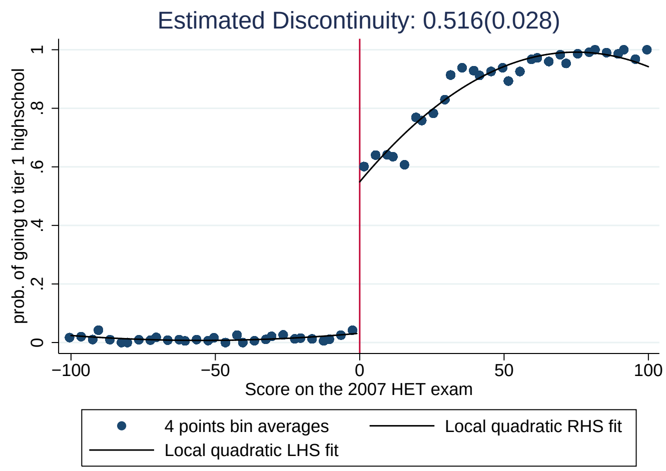
(e) Eligibility to attend a four year college (Females only).



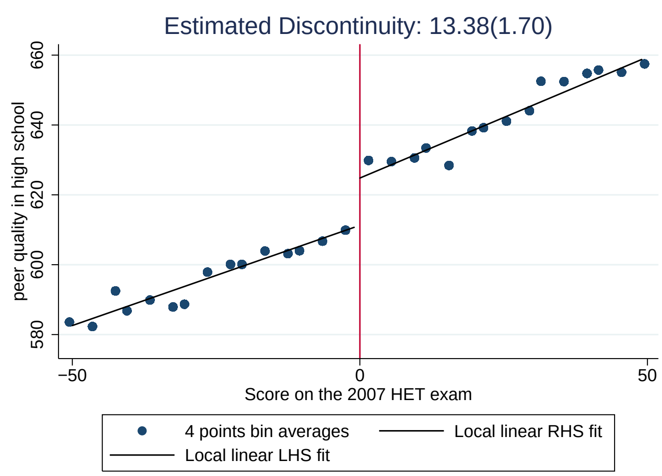
(f) Eligibility to attend an elite college (Females only).

Notes: Sample includes students who took the high school entrance exam (HET) in the year 2007. Since we observe individuals with multiple cutoffs, we cluster at the student ID level.
Eligibility defined as meeting the minimum score required to enter college type.

Figure C4: Local polynomial figures for the first stage as well as school inputs. [Going to a tier-1 school].



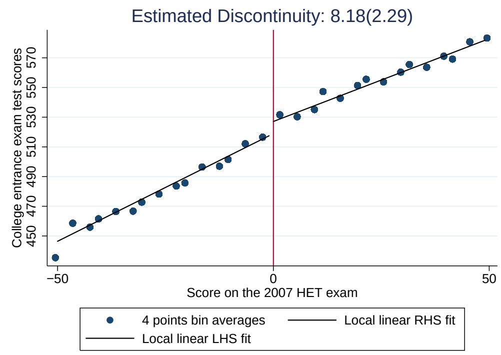
(a) Probability of attending tier 1 high school.



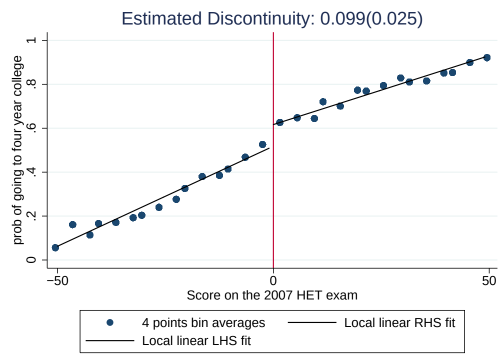
(b) Peer quality based on scores on high school entrance exam.

Notes: Sample includes students who took the high school entrance exam in the year 2007. Standard errors clustered at score level.

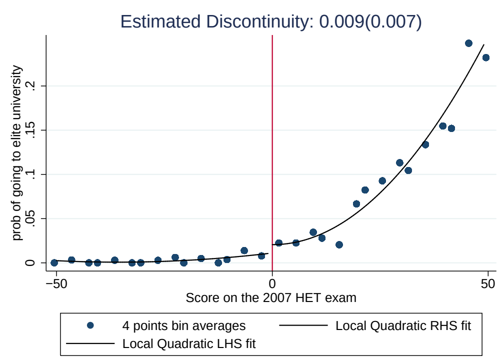
Figure C5: Local polynomial figures for college entrance exam test scores and college outcomes [Going to a tier-1 school].



(a) College entrance exam scores.



(b) Eligibility to attend a four year college.



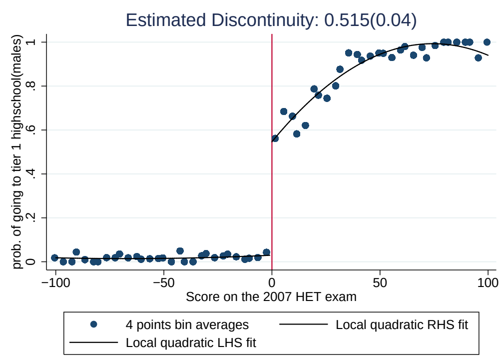
(c) Eligibility to attend an elite college.

Notes: Sample includes students who took the high school entrance exam in the year 2007. Standard errors clustered at score level.

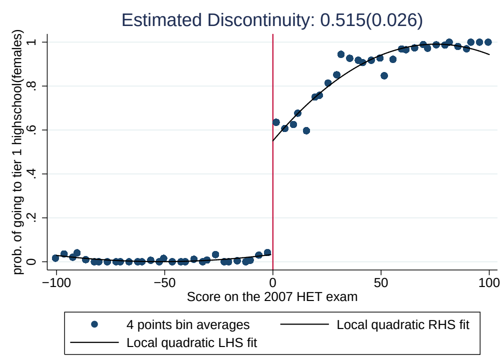
Figure C6: Local polynomial figures representing the first stage as well as school inputs for males versus females. [Going to a tier-1 school].

<u>Males</u>

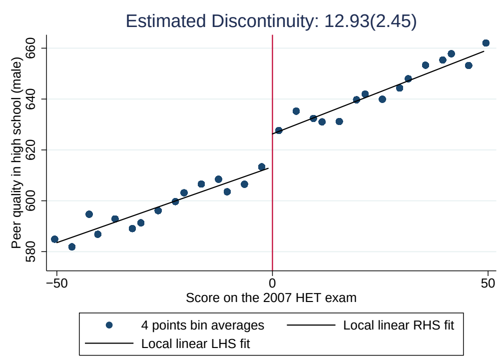
<u>Females</u>



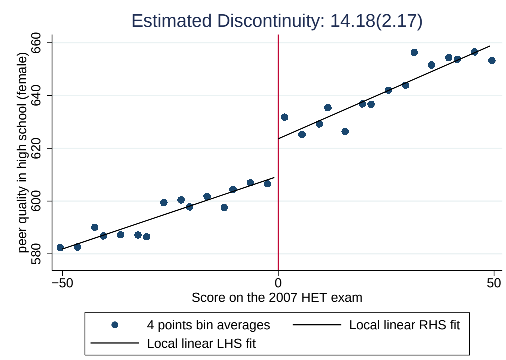
(a) Prob. of attending tier 1 school.



(b) Prob. of attending tier-1 school.



(c) High school peer quality.



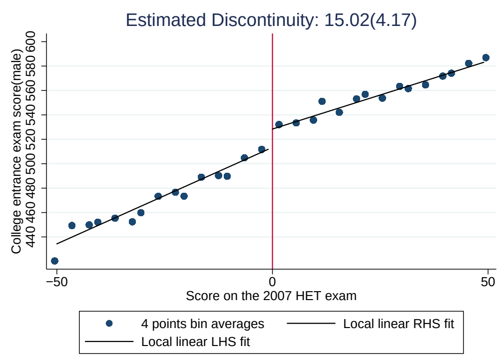
(d) High school peer quality.

Notes: Sample includes students who took the high school entrance exam in the year 2007. Standard errors clustered at score level.

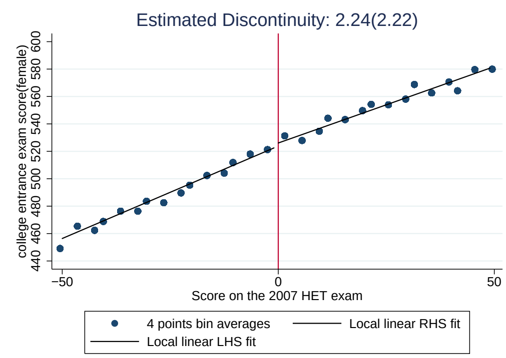
Figure C7: Local polynomial figures representing test score and college outcomes for males versus females [Going to a tier-1 school].

Males

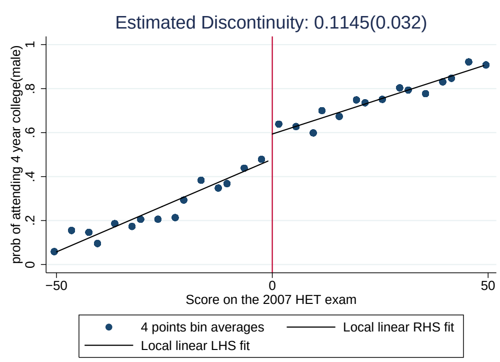
Females



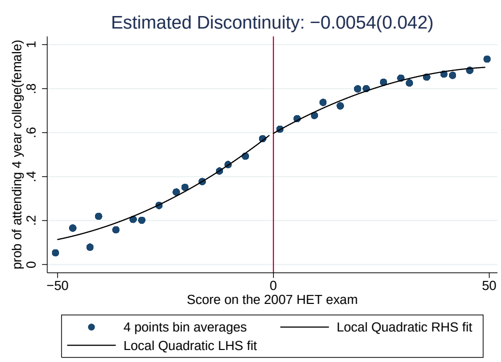
(a) College entrance exam scores.



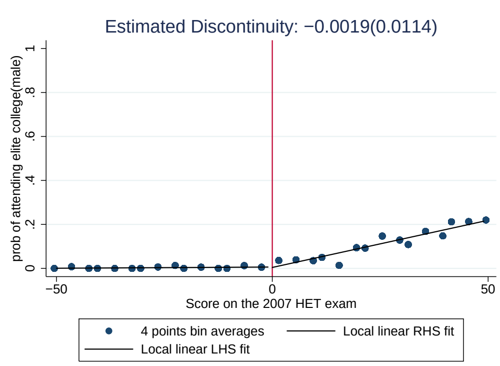
(b) College entrance exam scores.



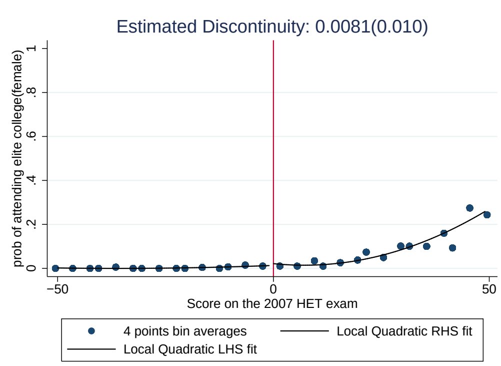
(c) Eligibility to attend a 4-year college.



(d) Eligibility to attend a 4-year college.



(e) Eligibility to attend an elite college.

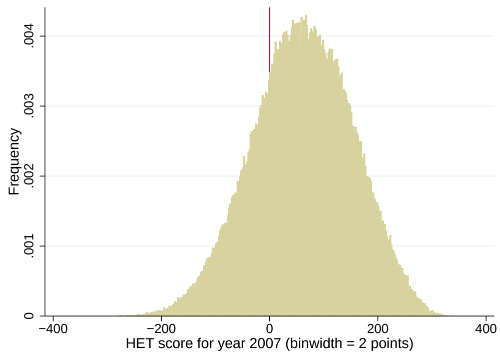


(f) Eligibility to attend an elite college.

Notes: Sample includes students who took the high school entrance exam in the year 2007. Standard errors clustered at score level.

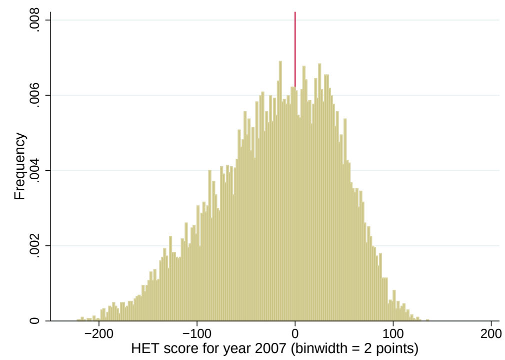
Figure C8: Validity of the RD design for both empirical strategies.

Going to a Better school

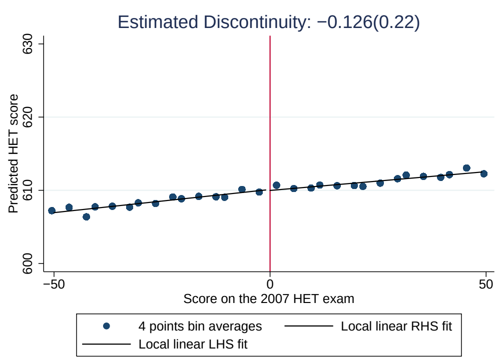


(a) Distribution of HET scores.

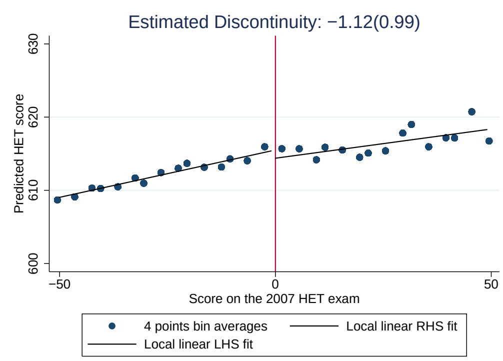
Going to a tier-1 school



(b) Distribution of HET scores.



(c) Smoothness of baseline covariates.

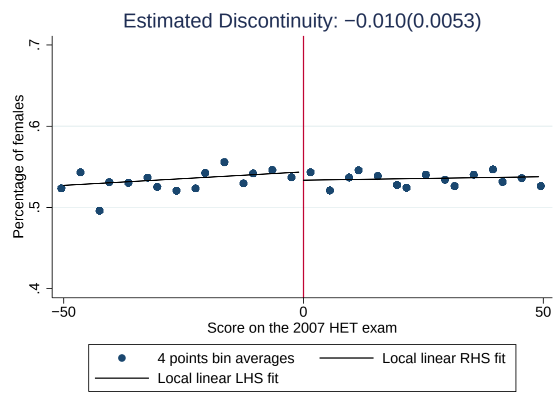


(d) Smoothness of baseline covariates.s

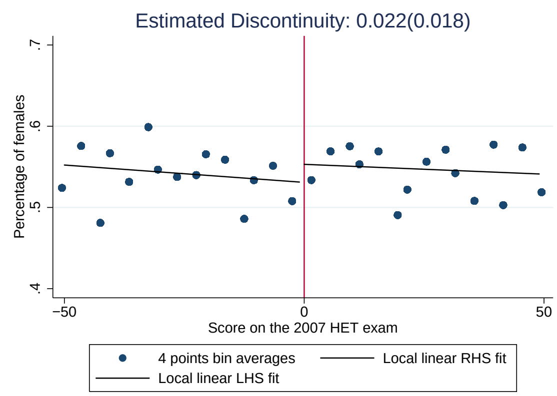
Notes: Sample includes students who took the HET exam in the year 2007.
Bins for histogram represent an average count of 2 score points.
Predicted score based on the following controls: sex, gender, district fixed effects, middle school fixed effect.

Figure C9: Smoothness of baseline covariates.

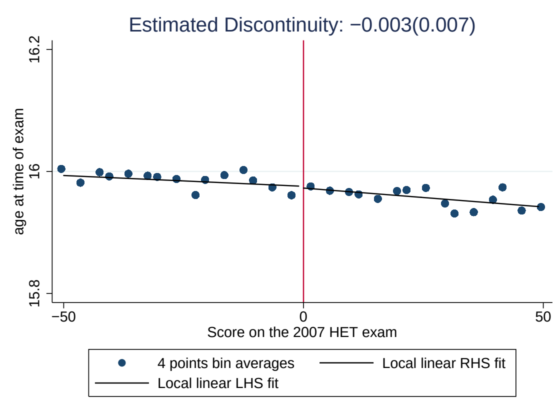
Going to a better school | Going to a tier-1 school



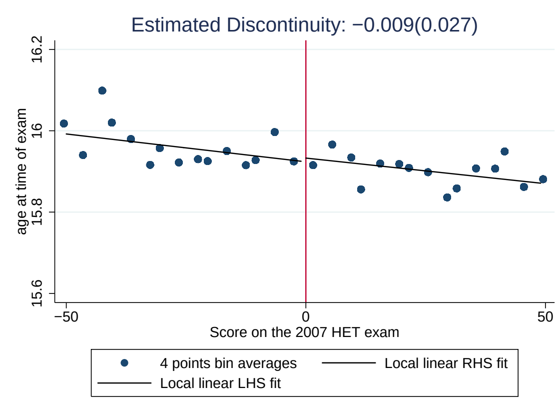
(a) Probability of being female.



(b) Probability of being female.



(c) Age at time of high school exam.



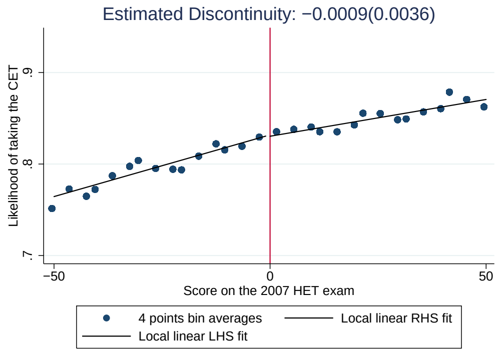
(d) Age at time of high school exam.

Notes: Sample includes students who took the high school entrance exam in the year 2007.

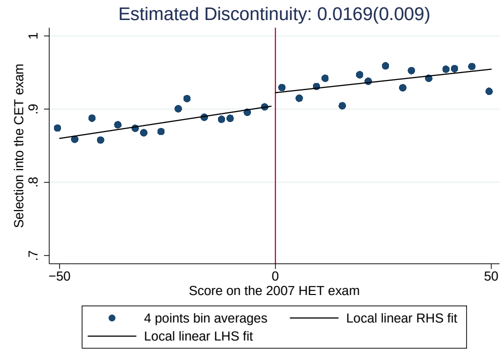
Figure C10: Selection into the college entrance exam.

Going to a Better school

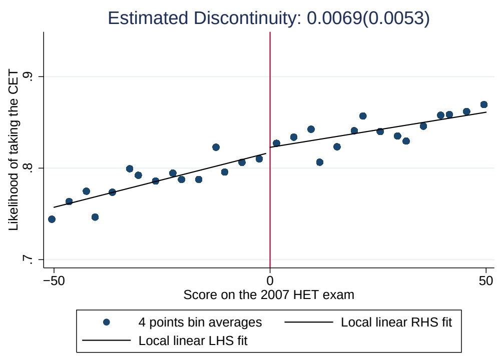
Going to a tier-1 school



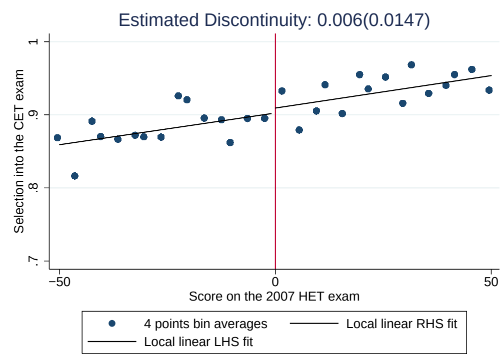
(a) Selection into the CET exam.



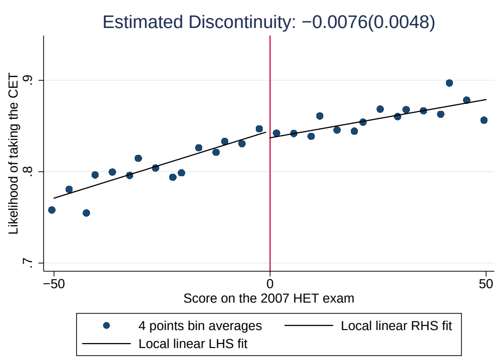
(b) Selection into the CET exam.



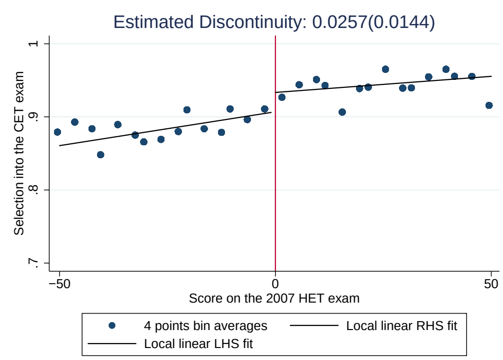
(c) Selection into the CET exam (Males only).



(d) Selection into the CET exam (Males only).



(e) Selection into the CET exam (Females only).

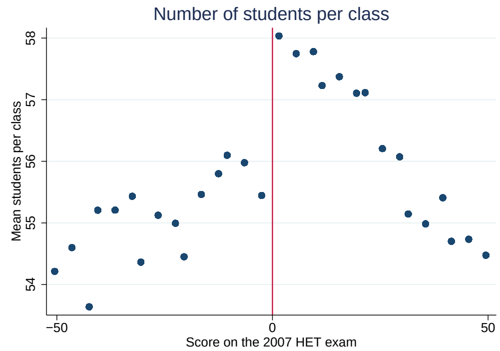


(f) Selection into the CET exam (Females only).

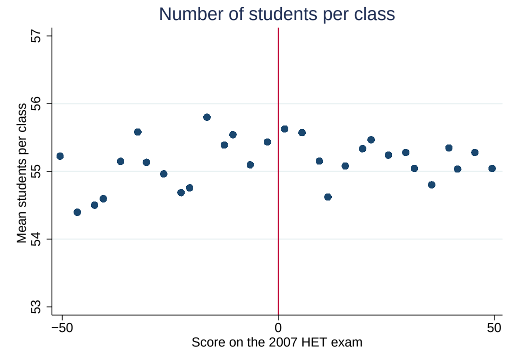
Notes: Sample includes students who took the high school entrance exam in the year 2007 (including those with no college entrance exam scores).

Figure C11: Teacher and school characteristics.

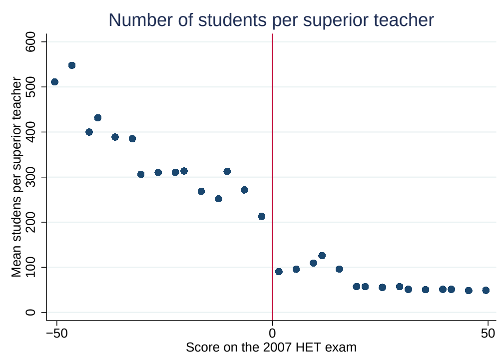
Going to a tier-1 school | Going to a Better school



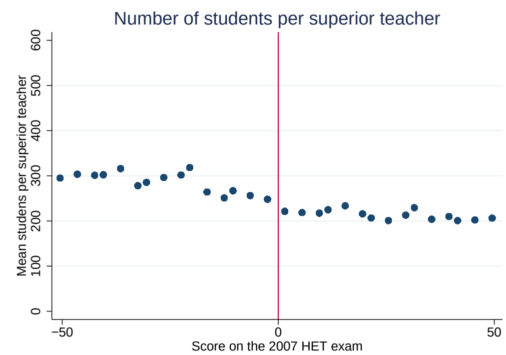
(a) Number of students per class at threshold of attending ‘best’ high schools.



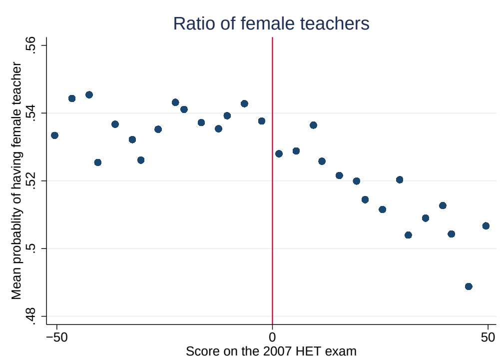
(b) Number of students per class at threshold of attending ‘better’ high schools



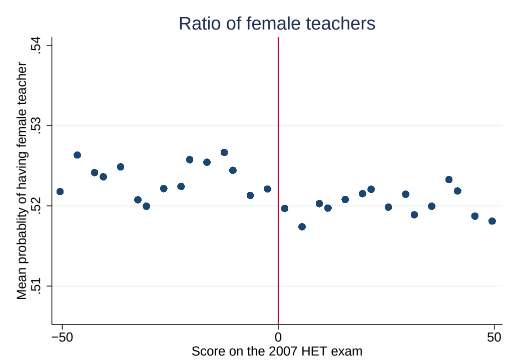
(c) Number of students per superior teacher at threshold of attending ‘best’ high schools.



(d) Number of students per superior teacher at threshold of attending ‘better’ high schools.



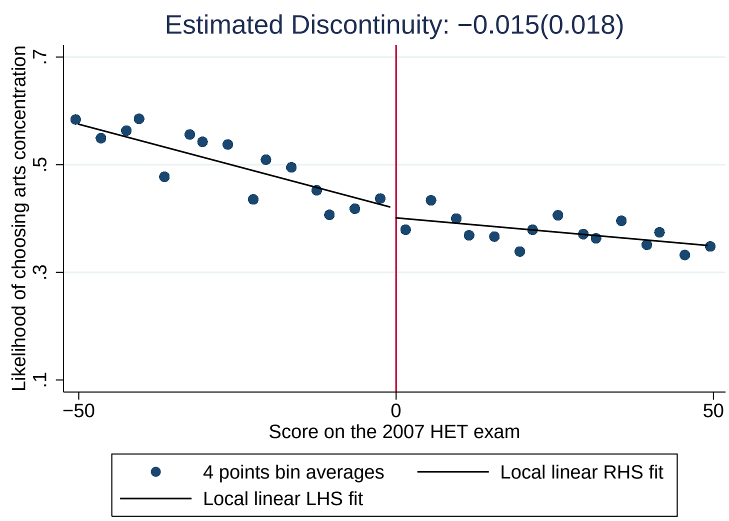
(e) Ratio of female teachers at threshold of attending ‘best’ high schools.



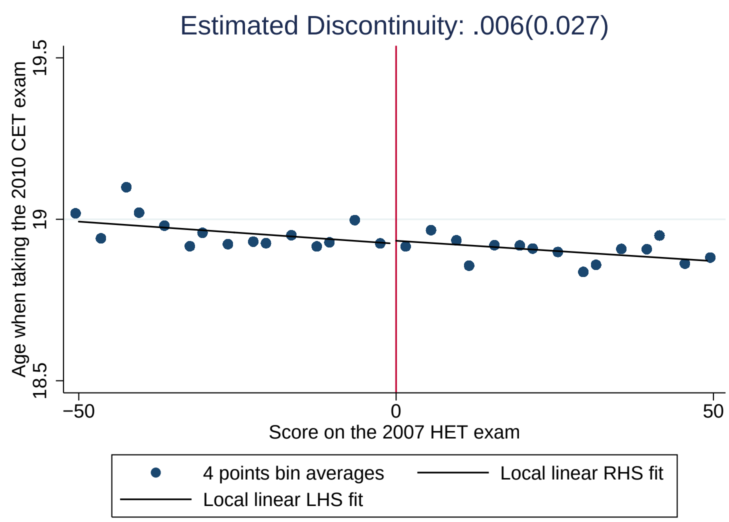
(f) Ratio of female teachers at threshold of attending ‘better’ high schools.

Notes: Sample based off of school level data.

Figure C12: Potential outcomes from going to a tier 1 high school (threats to interpretation).



(a) Likelihood of majoring in arts versus sciences in high school.



(b) Exact age when taking the 2010 CET exam.

Notes: Sample includes students who took the CET exam in the year 2010 with known high school cutoffs. Standard errors clustered at the running variable level.

Table C.1: Descriptive statistics for students taking the high school entrance exam in 2007 in the three Rural towns studied.

	(1) Whole Sample	(2) Tier 1 Schools	(3) Tier 2 Schools	(4) Tier 3 Schools	(5) Tier 4 Schools
High School entrance exam scores	612.39	643.28	607.5	537.06	495.21
	(61.14)	(53.54)	(33.99)	(48.91)	(31.92)
College entrance exam scores	492.66	540.31	470.99	399.67	342.47
	(96.92)	(81)	(79.44)	(79.55)	(66.88)
Gender of student (1= female)	0.53	0.54	0.53	0.52	0.50
Likelihood of choosing arts in high school	0.48	0.41	0.51	0.65	0.68
Percent private schools	0.010	0.006	0.015	0.018	0
School is High School only	0.57	0.69	0.60	0.17	1
Eligibility for four year college	0.44	0.67	0.28	0.067	0.016
Eligibility for elite college	0.086	0.165	0.012	0.002	0
Gender of teacher (1= female)	0.53	0.49	0.56	0.56	0.52
School Size	147.45	183.37	131.88	87.71	83
	(81.83)	(98.19)	(36.05)	(28.06)	(0)
Number of teachers	208.89	247	191	144	170
	(73.74)	(77.79)	(42.07)	(25.23)	(0)
Number of superior teachers	61.22	92.13	45.21	15.06	30
	(50.57)	(46.5)	(31.72)	(12)	(0)
Number of schools	36	11	12	12	1
Number of Students	15367	7462	5367	2220	309

Notes: Data taken from three rural districts in the Province for students taking the high school entrance exam in 2007.

Table C.2: RD estimates for high school peer quality attributed to going to a 'better high school'—using CCT optimal bandwidth.

Bandwidth	0.75 CCT	CCT	1.25 CCT	1.5 CCT	2 CCT	2.5 CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
Discontinuity in high school peer quality	10.945***	10.507***	11.494***	10.429***	10.041***	8.810***
	(1.48)	(1.30)	(1.15)	(1.02)	(.86)	(.71)
With Controls	10.971***	10.581***	11.393***	10.275***	9.845***	8.745***
	(1.40)	(1.24)	(1.09)	(.96)	(.82)	(.68)
Score Polynomial	One	One	One	One	One	One
Observations	13729	19409	24867	28997	38439	47872

Notes: Sample includes students who took the college entrance exam in the year 2007 with known high school cutoffs. Controls include: Age, gender, district fixed effects and middle school fixed effects. Optimal Bandwidth selected using the CCT bandwidth selector proposed in Calonico et al. (2015).

Optimal BW = 14 for high school peer quality regressions.

Since we observe individuals with multiple cutoffs, we cluster at the student ID level.

** p <0.01 ** p <0.05 * p <0.1

Table C.3: RD estimates for college outcomes attributed to going to a ‘better high school’—using CCT optimal bandwidth.

Bandwidth	0.75 CCT	CCT	1.25 CCT	1.5 CCT	2 CCT	2.5 CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Discontinuity						
in college entrance exam test scores	−.147	−.209	.412	.541	.722	−.100
	(1.95)	(1.62)	(1.40)	(1.09)	(.89)	(1.36)
With Controls	−.880	−1.098	−.442	−.205	−.201	−.557
	(1.86)	(1.57)	(1.31)	(1.02)	(.84)	(1.28)
Panel B: Discontinuity						
in eligibility of attending a 4-year college	.010	.001	.006	.004	.013***	−.003
	(.010)	(.008)	(.007)	(.006)	(.004)	(.007)
With Controls	.006	−.004	.002	.001	.009**	−.006
	(.010)	(.008)	(.006)	(.005)	(.004)	(.007)
Panel C: Discontinuity						
in eligibility of attending an elite college	.002	.003	.004	.003	.006***	.004
	(.004)	(.003)	(.003)	(.003)	(.002)	(.003)
With Controls	.002	.002	.004	.003	.006***	.003
	(.004)	(.003)	(.003)	(.003)	(.002)	(.003)
Score Polynomial	One	One	One	One	One	Two
Observations	30292	39801	49208	59548	77125	95051

Notes: Controls include: Age, gender, district fixed effects and middle school fixed effects. **Optimal BW = 29 for College entrance exam test scores regressions. Optimal BW = 31 and 22 for eligibility to attend 4-year and elite elite college regressions respectively.** *** p <0.01 ** p <0.05 * p <0.1

Table C.4: Heterogeneous RD estimates from going to a ‘better high school’ regressions.

Bandwidth	All (1)	Males (2)	Females (3)	Score Polynomial
Panel A: Discontinuity in inputs into education				
Peer Quality	11.478***	9.574***	13.106***	Quadratic
	(1.06)	(1.58)	(1.41)	
Panel B: Discontinuity in outcomes				
College entrance exam scores	–0.476	–0.701	–0.276	Linear
	(1.17)	(1.94)	(1.36)	
Eligibility to attend 4-year college	0.002	0.003	–0.001	Linear
	(.006)	(.009)	(.009)	
Eligibility to attend elite college	0.0020	-0.0025	0.0064	Quadratic
	(.004)	(.006)	(.0045)	
Observations	54367	25190	29177	

Notes: All regressions include controls: Age, gender, district fixed effects and middle school fixed effects.
For ease of comparison, all regressions use an equal bandwidth of 40 score points on either side of the cutoff.
Since we observe individuals with multiple cutoffs, we cluster at the student ID level.
** p <0.01 ** p <0.05 * p <0.1

Table C.5: RD estimates for high school inputs attributed to going to the 'best high schools'—using CCT optimal bandwidth.

Bandwidth	0.75 CCT	CCT	1.25 CCT	1.5 CCT	2 CCT	2.5 CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Discontinuity						
in Prob. of attending tier 1 school	.543***	.557***	.537***	.544***	.522***	.540***
	(.06)	(.05)	(.04)	(.03)	(.03)	(.03)
With Controls:	.568***	.572***	.542***	.546***	.523***	.539***
	(.06)	(.05)	(.04)	(.04)	(.03)	(.03)
Panel B: Discontinuity in						
high school peer quality	17.174***	19.848***	17.772***	17.165***	12.806***	13.591***
	(2.28)	(2.38)	(1.96)	(1.88)	(2.00)	(1.83)
With Controls:	21.201***	22.276***	19.327***	17.974***	13.704***	14.096***
	(2.80)	(2.47)	(2.02)	(1.80)	(1.92)	(1.67)
Score Polynomial	One	One	One	One	One	One
Observations	2575	3324	4227	4983	6607	8061

Notes: Number of observations correspond to the high school peer quality regressions.
Controls include: Gender, age when taking exam, district fixed effects and Middle school fixed effects.
Optimal Bandwidth selected using the CCT bandwidth selector proposed in Calonico et al. (2015).
Optimal BW = 20 for Probability of attending tier 1 school regressions.
Optimal BW = 18 for high school peer quality regressions.
Standard errors clustered at the score level. *** p <0.01 ** p <0.05 * p <0.1

Table C.6: RD estimates for college outcomes attributed to going to the 'best high schools'—using CCT optimal bandwidth.

Bandwidth	0.75 CCT	CCT	1.25 CCT	1.5 CCT	2 CCT	2.5 CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Discontinuity						
in college entrance exam test scores	5.151*	6.017**	7.703***	8.034***	8.816***	9.424***
	(3.06)	(2.61)	(2.34)	(2.23)	(1.91)	(1.69)
With Controls:	5.522**	5.855***	7.831***	7.607***	7.936***	7.945***
	(2.48)	(2.07)	(1.91)	(1.89)	(1.61)	(1.46)
Panel B: Discontinuity in						
eligibility to attend 4 year college	.055	.054	.053	.074**	.099***	.129***
	(.05)	(.04)	(.03)	(.03)	(.03)	(.02)
With Controls:	.061	.053	.051*	.073***	.097***	.125***
	(.04)	(.03)	(.03)	(.03)	(.02)	(.02)
Panel C: Discontinuity						
in eligibility to attend elite college	.013*	.002	.009	.010	–.002	–.009
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
With Controls:	.014**	.004	.009	.009	–.002	–.009
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Score Polynomial	One	One	One	One	One	One
Observations	4788	6233	7760	8956	11081	12601

Notes: Controls include: Gender, age when taking exam, district fixed effects and Middle school fixed effects.
Optimal BW = 34 for College entrance exam test scores regressions. Optimal BW = 25 and 13 for probability of attending 4 year and elite college regressions respectively.
*** p <0.01 ** p <0.05 * p <0.1

Table C.7: ITT and LATE RD estimates for all students, as well as male and female students going to the 'best high schools'.

Treatment effect	ITT	LATE	ITT	LATE	ITT	LATE	
Gender	All		Males		Females		Polynomial
Panel A: First stage							
Likelihood of attending tier 1 school	.523***	—-	.531***	—-	.523***	—-	Linear
	(.03)		(.04)		(.03)		
Panel B: Discontinuity in school inputs							
High school peer quality	13.449***	26.053***	13.496***	26.085***	13.714***	26.407***	Linear
	(1.79)	(2.38)	(2.15)	(2.70)	(2.03)	(2.85)	
Panel C: Discontinuity in outcomes							
College entrance exam test scores	8.023***	15.876***	16.427***	32.968***	0.799	1.131	Linear
	(1.95)	(3.88)	(3.75)	(7.407)	(2.13)	(3.99)	
Eligibility to attend a 4-year college	.080***	.157***	.110***	.230***	−.030	−.058	Linear
	(.025)	(.05)	(.03)	(.06)	(.047)	(.083)	
Eligibility to attend an elite college	.006	.012	.021	.037	-.005	−.009	Quadratic
	(.008)	(.015)	(.014)	(.025)	(.009)	(.016)	
Observations	7235	7235	3273	3273	3962	3962	

Notes: **For ease of comparison, all regressions use an equal bandwidth of 40 score points on either side of the cutoff.** *** p <0.01 ** p <0.05 * p <0.1

Table C.8: Regression discontinuity estimates for baseline covariates—using CCT optimal bandwidth.

Bandwidth	0.75 CCT	CCT	1.25 CCT	1.5 CCT	2 CCT	2.5 CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: (Going to a top-tier school)						
Predicted HET score	.098	−.202	.425	.202	−.831	−.595
	(1.66)	(1.53)	(1.38)	(1.28)	(1.16)	(1.03)
Observations	2575	3324	4227	4983	6607	8061
Panel B: (Going to a better school)						
Predicted HET score	.398	.020	−.051	−.149	−.080	−.194
	(.40)	(.36)	(.31)	(.30)	(.26)	(.23)
Observations	35749	46549	58246	68514	89320	108424
Score Polynomial	One	One	One	One	One	One

Notes: Controls for predicted HET score include: gender, age, middle school fixed effects and district fixed effects. Optimal Bandwidth selected using the CCT bandwidth selector proposed in Calonico et al. (2015).
Optimal BW = 18 for going to a top tier school.
Optimal BW = 34 for going to a better school.
** p <0.01 ** p <0.05 * p <0.1

Table C.9: Regression discontinuity estimates for separate baseline covariates—using CCT optimal bandwidth.

Bandwidth	0.75 CCT	CCT	1.25 CCT	1.5 CCT	2 CCT	2.5 CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: (Going to a top-tier school)						
Likelihood of being a female	.050*	.048**	.040**	.031	.023	.011
	(.03)	(.02)	(.02)	(.02)	(.02)	(.02)
Age when taking HET entrance exam	–.026	.001	.006	.004	.003	.009
	(.04)	(.03)	(.03)	(.03)	(.02)	(.02)
Observations	3669	4788	6028	7118	9135	10748
Panel B: (Going to a better school)						
Likelihood of being a female	–.006	–.010	–.011	–.009	–.008	–.008
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Age when taking HET entrance exam	–.005	.005	–.003	–.002	.002	–.002
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Observations	35749	46549	58246	68514	89320	108424
Score Polynomial	One	One	One	One	One	One

Notes: **Optimal BW = 26 for probability of being a female (going to the best schools). Optimal BW = 34 for age when taking high school entrance exam (going to the best schools). Optimal BW = 34 for probability of being a female (going to a better school). Optimal BW = 40 for age when taking high school entrance exam (going to a better school).** *** p <0.01 ** p <0.05 * p <0.1

Table C.10: Regression discontinuity estimates for selection into the college entrance exam—using CCT optimal bandwidth.

Bandwidth	0.75 CCT	CCT	1.25 CCT	1.5 CCT	2 CCT	2.5 CCT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: (Going to the best schools)						
Selecting into the CET entrance exam (All)	.022	.013	.018*	.0177*	.0167**	.006
	(.015)	(.011)	(.011)	(.01)	(.0084)	(.008)
Females only	.029	.016	.022	.023	.029**	.010
	(.02)	(.02)	(.02)	(.02)	(.01)	(.01)
Males only	.013	.009	.012	.011	.003	–.000
	(.02)	(.02)	(.02)	(.02)	(.01)	(.01)
Observations (females)	2395	3157	3927	4704	5919	6955
Observations (males)	2050	2681	3315	3982	5035	5908
Panel B: (Going to a better school)						
Selecting into the CET entrance exam (All)	.003	–.000	.004	.002	–.001	–.001
	(.01)	(.01)	(.01)	(.002)	(.003)	(.003)
Females only	–.005	–.011	–.007	–.004	–.006	–.005
	(.01)	(.01)	(.01)	(.01)	(.00)	(.00)
Males only	.012	.012	.012	.009	.005	.004
	(.01)	(.01)	(.01)	(.01)	(.01)	(.008)
Observations (females)	17778	23771	29785	35641	46253	57189
Observations (males)	15574	21030	26410	31474	40971	50681
Score Polynomial	One	One	One	One	One	One

Notes: **Optimal BW = 29 for likelihood of taking CET exam (going to the best school). Optimal BW = 27 for likelihood of taking CET exam (going to a better school).** *** p <0.01 ** p <0.05 * p <0.1

Table C.11: Bounding analysis for the college entrance exam score regressions (going to the best schools).

Bandwidth	26 points (1)	34 points (2)	43 points (3)	51 points (4)	68 points (5)	84 points (6)
Panel A: Selection into the CET exam	.0170	.0136	.0158	.0172*	.011	.003
	(.011)	(.011)	(.01)	(.009)	(.008)	(.008)
Constant (Control mean)	0.900	0.906	0.904	0.905	0.914	0.922
Proportion to be trimmed	1.8%	1.5%	1.74%	1.9%	1.2%	0.3 %
Panel B: College entrance exam test scores (Original regression estimates)	5.151*	6.017**	7.703***	8.034***	8.816***	9.424***
	(3.06)	(2.61)	(2.34)	(2.23)	(1.91)	(1.69)
College entrance exam test scores (Lower bound estimates)	4.206	5.75**	7.290***	8.120***	9.66***	9.64***
	(2.91)	(2.93)	(2.26)	(2.46)	(1.98)	(1.90)
College entrance exam test scores (Upper bound estimates)	8.63***	10.12***	13.27***	13.89***	13.03***	10.90***
	(3.24)	(2.56)	(2.23)	(2.14)	(2.01)	(1.86)
Observations	4788	6233	7760	8956	11081	12601
Observations after trimming	4743	6182	7685	8868	11016	12509
Score Polynomial	One	One	One	One	One	One

Notes: To ease comparison with our previous estimates, we use the same bandwidths predicted by the CCT for the original college entrance score regressions. Bootstrapped standard errors reported for the upper and lower bound estimates.*** p <0.01 ** p <0.05 * p <0.1