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ABSTRACT

The Long-Run Effects of Reducing Early School Tracking*

Grouping students by ability is a controversial issue, and its impacts are likely to depend on the type of tracking students are exposed to. This paper studies a reform that moved French schools from a rigorous tracking system, which assigned students to tracks with significantly different learning environments and career options, to a milder form of ability-tracking that only grouped students into different classrooms. Using a regression discontinuity design, I find that the reform raised individuals' level of education and increased their wages by 4.7 percent at ages 40 to 45, with the strongest effects occurring among individuals from low socioeconomic backgrounds.

JEL Classification: I21, I28, J24

Keywords: tracking, returns to education, school quality

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1 Introduction

Separating students into classes or tracks based on their academic achievements is a common practice in many educational systems. While tracking is widespread, the manner in which it is implemented varies across countries. European tracking systems typically lock students into tracks which provide significantly different learning environments. For example, in Germany, students are divided at age 10 into three tracks that substantially diverge in terms of resources and career paths. In the United States, tracking is less rigid as students are grouped in ability-based classes as early as middle school and other than peer quality, no substantial disparities usually exist between classes (Betts, 2011).

The issue of tracking is controversial and its predicted impact on students' academic and labor market performance is ambiguous. Tracking may be beneficial if it allows students to pursue an education that is adapted to their abilities and needs. However, opponents of tracking policies fear that they can harm low-achieving students' prospects by putting them at a learning disadvantage. Misallocating students across tracks is another potential issue since assignment is often done at an early age when information about students' abilities is still incomplete. Additionally, family background usually influences track choice, sparking concerns that tracking might exacerbate social inequalities in education and labor market opportunities (Dustmann, 2004). In response to these concerns, many governments implemented detracking policies. In the 1960s and 70s, the United Kingdom abandoned the practice of tracking students into different schools based on academic performance at age 11. Similar reforms were put in place in Finland, Norway, Sweden and France.

Previous evidence on the effects of tracking is mixed. Separating students into ability-based classes from an early age—as is typically done in the U.S.—has been shown to improve academic performance (Betts and Shkolnik, 2000; Figlio and Page, 2002; Zimmer, 2003; Lefgren, 2004; Duflo, Dupas and Kremer, 2011; Cortes and Goodman, 2014; Card and Giuliano, 2016). These findings contrast with cross-country evidence indicating that tracking at an early age exacerbates inequalities (Hanushek and Wößmann, 2006; Brunello and Checci, 2007; Schütz et al., 2008). Betts (2011) argues that "The fact that the American literature tends to find less evidence that tracking generates inequality than does the international literature may reflect differences in resources." Indeed, in many European countries, pupils are locked into tracks that differ drastically in terms of peer and teacher quality, curricula, academic versus vocational focus and postsecondary options. On the other hand, schools in the United States use a milder form of tracking that involves grouping students into classes based on prior achievement, but these classes do not typically diverge in terms of resources, curricula or career opportunities.

This paper studies the impact of shifting from a European-style tracking system to a milder form of tracking on students' education and labor market outcomes. In this context, the milder form of tracking involves mostly separating students into classes that diverge in terms of peer quality, but not in terms of curriculum, teachers' qualifications or career options. Answering this question is important as it can inform policy discussions regarding the design of tracking programs and help reconcile the mixed findings from the previous literature.

To do so, I exploit a reform of the middle school system in France. Prior to the reform and at the beginning of middle school, students were separated at age 11 into either a high or low track based on previous academic performance. Students in the high track were eventually placed in general or technical education in high school, allowing them to access postsecondary studies. On the other hand, low-track students typically pursued vocational degrees, aimed for direct entry in the labor market. The middle school tracks further presented clear disparities in terms of curriculum content, pace and level of instruction, teachers' qualifications, peer quality and socioeconomic background. The reform abolished the middle school tracks and introduced a common curriculum. A major implication was that students' high school and postsecondary options were no longer tied to track assignment at age 11. The reform also intended to create mixed-ability classes but in practice, many schools kept grouping students in achievement-based classes. As a result, tracking was not completely abolished, but middle schools rather shifted to a milder form of tracking. Hence, one difference between this study and the previous literature is that the institutional context is well suited to compare the two most prevalent forms of tracking.

The reform, which took effect in the academic year 1977-78, meant that individuals born after January 1, 1966 were exposed to the new tracking system while those born before that date went through the more rigorous form of tracking. This enables me to use a regression discontinuity design (RD) that compares outcomes of students who were born marginally before and marginally after this date of birth cutoff. To alleviate concerns over the possibility that my results are driven by seasonal and age of school entry effects, I show that all RD estimates are consistent with those from a difference-in-difference estimation using individuals born on either sides of January 1 in years other than 1966 as a control group.

I find that the reform decreased the share of individuals holding vocational diplomas and simultaneously raised the probability of having technical degrees. This indicates that students realized educational gains from the reform, as technical degrees are considered to be a higher level of qualification than vocational diplomas in France. I further show that the reform increased wages by 4.7 percent at ages 40 to 45, but had no significant effect on employment. Both the education and labor market benefits are concentrated among individuals from low socioeconomic backgrounds, who were most likely to be placed in the low track prior to the reform. My results indicate that decreasing the intensity of tracking at an early age has long-lasting benefits and can potentially narrow socioeconomic inequalities in the labor market.

My paper adds to the tracking literature by showing that decreasing the intensity of tracking at an early age raises individuals' wages in the long run. Other policies that delay or abolish tracking have been shown to improve educational attainment and test scores (Galindo-Rueda and Vignoles, 2007; Malamud and Pop-Eleches, 2011; Kerr et al., 2013; Piopiunik, 2014; Lange and von Werder, 2017; Zilic, 2018). However, evidence on how these types of reforms affect individuals' labor market opportunities is relatively scarce.[1] Hall (2012, 2016) studies a Swedish reform that increased the academic content of the upper secondary vocational track and gave vocational students eligibility to attend college. The author finds that the reform has no significant impact on college attendance or earnings, and instead increased high school dropout and unemployment risk for some students. Bertrand et al. (2019) study a similar reform in Norway that additionally enhanced the quality of the vocational track by increasing students' access to apprenticeships. The reform increased men's likelihood of holding vocational rather than academic degrees and raised their earnings at ages 25 to 34. While these studies also look at labor market outcomes, the reforms that they examine are very different than mine. Specifically, they focus on policies that improved the quality of the high school vocational track. In contrast, the French reform abolished tracking in middle school. Finally, Malamud and Pop-Eleches (2010) examine a policy that delayed the placement of Romanian students into high school vocational and academic tracks by 2 years. The authors show that the reform increases men's likelihood of pursuing general rather than vocational studies. They further document that men are less likely to work in manual occupations due to the reform, but find no significant effects on their earnings at ages 36 to 41. In contrast, I find that decreasing the intensity of tracking raises longer-term wages. A potential explanation for these contrasting results is that the intervention that they focus on occurred when students were older than those in my study—at approximately age 14 versus 11. My findings are consistent with the idea that tracking at an earlier age can

[1] Several studies document that students incur labor market benefits from policies that abolished tracking and simultaneously changed other major aspects of Scandinavian educational systems. Meghir and Palme (2005) and Aakvik et al. (2010) find that students experience increases in earnings from reforms that abolished tracking and also raised the compulsory school leaving age in Sweden and Norway. Pekkarinen et al. (2009) further show that men's intergenerational mobility decreases due to a reform that eliminated the two-track school system in Finland and concurrently put private schools—i.e., most secondary schools—under municipal ownership. In contrast to these papers, my results are *entirely* attributed to a decrease in the intensity of tracking, since no other policy coincided with the French detracking reform.

increase the misallocation of students across tracks—and hence exacerbate inequalities—as the decision is likely based on noisy or incomplete information about their abilities. My results are also in line with the idea that early educational choices may be more consequential for later life outcomes (Cunha and Heckman, 2007).

The rest of this paper is organized as follows. Section 2 provides a detailed description of the institutional setting. Section 3 introduces the data that I use. Section 4 outlines the identification strategy. Section 5 presents the main results and robustness checks. In section 6, I discuss possible explanations for my findings and I conclude in section 7.

2 Institutional Background

Children in France first enroll in primary school in September of the year in which they turn 6—the compulsory school age. Hence, all children born in the same calendar year begin their education in the same academic year. Primary schools offer five consecutive years of general education that are common to all students. The organization of the primary education system remained unchanged before and after the reform. After completing primary school, students start four years of lower secondary education (henceforth middle school) at age 11.

2.1 Middle School Before the Reform

Figure 1 illustrates the educational system prior to the reform. Students aged 11 were separated into two distinct tracks starting their first year of middle school. A committee assigned students to the different tracks based on its evaluation of each pupil's academic file (or *dossier*).[2] High-performing students were placed in a high track and low-performing students in a low track. Between 1974 and1976, around 18% of students were placed in the low track. The two tracks were housed within the same schools, and students were locked into their respective tracks for an initial period of two years. After that, low-track students could in principle switch to the high track if they had good academic performance, but only about 10% of them did so due to the significant differences between tracks (Defresne and Krop, 2016). Most of these students were instead kept in the low track, and the least skilled amongst them were placed in pre-apprenticeship classes for the next two years.[3] As a result,

[2]The committee was comprised of primary and secondary school teachers, a guidance counselor and representatives of parents' committees. The academic file included the student's academic transcript, class rank, interests, medical history, an assessment of his/her behavior inside and outside of school, and occasionally an evaluation by a school guidance counselor (Hall, 1976). Parents who were unsatisfied with the track assignment, could request that their child sits for an exam upon which a final decision is made.

[3]From 1974 to 1976, 34% of low-track students were in pre-apprenticeship classes.

most students spent the full duration of their middle school (4 years) in the initial track they were assigned to.

Besides the clear disparities in peer ability, tracks offered distinct teachers, curricula and postsecondary options. First, teachers in the high track held college degrees and qualifications that allowed them to teach all secondary school classes—up until the last year of high school. In contrast, instructors in the low track typically held 2-year postsecondary degrees and were trained to only teach middle school classes.[4] Second, the style of learning and syllabi differed across tracks. Specifically, the pace and method of instruction were adapted to students' differing levels of ability, and the tracks offered different subjects and course content. Columns (1), (2) and (4) of table A1 show for each track, the instruction time allocated to subjects taken in the first and last years of middle school, prior to the reform. In the first year, high-track students spent more time learning a foreign language and less time on French than those in the low track. Low-track students did not study several subjects that students in the high track had access to such as history, geography, and sciences.[5] In the last year, high-track students were given the option to study another foreign language or an ancient language such as latin and ancient greek. Instead, low-track students did manual activities and spent more time learning French and sciences. Furthermore, the high track emphasized abstract learning while the low track focused more on the concrete.

2.2 High School

At the end of middle school, students were assigned to different types of upper secondary (henceforth high school) studies.[6] Importantly, students' middle school track played a key role in determining their high school path. Students from the high track in middle school were typically placed in either general, technical or vocational education for three years. Students who pursued general education in high school were eventually awarded the General Baccalaureate degree (*Baccalauréat Général*), which allowed them to enroll in universities and the *Grandes Ecoles*, the most selective postsecondary institutions. Students who enrolled in technical education in high school were awarded a Technician's Baccalaureate degree (*Baccalauréat de Technicien*). Upon graduation, they could attend the traditional university system but mostly either went into the labor market or enrolled in postsecondary

[4]Some low-track teachers did not hold postsecondary degrees. They simply received on-the-job-training after finishing grade 10. The division of teachers into different tracks was mandated by the 1959 Berthoin law.

[5]For the low track, the 4 lessons corresponding to "history/geography/civics" were fully allocated to civics. In contrast, for the high track, 2.5 lessons were allocated to history and geography and 1 lesson to civics.

[6]Specifically, a committee decided on the type of studies a student could pursue in high school based on regular assessments of his/her performance throughout middle school.

technical institutes that offered two-year technical degrees. Graduates of these institutes gained knowledge in applied scientific techniques required for fields such as commerce, administration, social work, industry, communications, and took jobs as senior technicians and technologists (Dundas-Grant, 1987).

Finally, some high-track students and students from the low track in middle school —including those in pre-apprenticeship classes—were mainly placed in vocational education. Specifically, low-track students typically pursued a 2-year trade certificate *Certificat d'Aptitude Professionnel (CAP)*, while high-track students pursued a 3-year *Brevet d'Etudes Professionnelles (BEP)*—but still had the option of pursuing a *CAP*.[7] The main difference between the two degrees is that holders of the *CAP* were trained for a specific job, while those who pursued the *BEP* specialized in a field and could work in different jobs within that field. "For instance, in the hotel, catering and institutional sector, there is a cellarman's CAP, a laundry worker's CAP, a catering worker's CAP, but one single less specialized BEP covering these activities as a whole" (Dundas-Grant, 1987). Vocational education intended to provide immediate access to the labor market, and students received a combination of general education and vocational courses as well as on-the-job training. Finally, low-track students who were not admitted into vocational education (i.e., the lowest-performing students in middle school) remained in transitional classes until age 16—the compulsory school leaving age—after which they sought employment.

2.3 The Haby Reform

The middle school tracks exhibited strong disparities in their pupils' socioeconomic status. Specifically, 72% of students in the low track came from disadvantaged backgrounds. Furthermore, 95.5% of students whose fathers were in managerial occupations were placed in the high track versus only 74.8% of children whose fathers were manual workers.[8] The victory of Valéry Giscard d'Estaing in the 1974 French presidential elections was a major turning point for the middle school system. The new president believed "that those of equal ability should be given equal opportunity" (Lewis, 1985). Hence, in 1975, the minister of education René Haby introduced a series of controversial proposals aimed at reforming the education system. Haby's education law was circulated on July 11, 1975 but its wording was vague. A subsequent publication by the Ministry of Education in 1977 clarified the ob-

[7] Both pre-apprenticeship and low-track students pursued a *CAP* degree. However, pre-apprenticeship students typically did so in apprenticeship centers, while low track students attended vocational high schools.

[8] These numbers are estimated using data from the 1972-1973-1974 survey "Panel d'élèves". This survey was administered by the French National Institute of Statistics and Economic Studies (INSEE) and provides information on the academic trajectories of students who were enrolled in the first year of middle school in the years 1972-1973-1974. Source: Defresne and Krop (2016).

jectives of this law. Haby intended to have a middle school that is common to all students, the *collège unique*. The law mainly abolished the different middle school tracks and created mixed-ability classes. The organization of primary schools and high schools as well as the number of years of education remained unchanged.

Figure 2 shows the educational system after the Haby reform. After primary school, all students now received a common middle school education for at least two years. After the first two years, low-performing students could still however be placed in pre-apprenticeship classes—which as before, led to either vocational education in high school or employment at age 16. Those who were not placed in pre-apprenticeship classes pursued two more common years in middle school before being assigned to different types of studies in high school—i.e., to either general, technical or vocational education. The law further introduced a new common middle school curriculum. Columns (3) and (5) of table A1 displays the instruction time devoted for each subject under the new common curriculum. In the first year of middle school, both low and high-track students experienced a 1 to 2-hour decrease in the weekly time allocated to French, mathematics, history-geography-civics, arts and physical education.[9] On the other hand, time allocated for sciences increased and 2 hours of manual activities were introduced to the new curriculum. In the last year of middle school, high-track students did not see major changes to their curriculum, except for a drop in the number of lessons allocated to physical education and the introduction of manual activities. Low-track students experienced a decrease in the number of hours of French (from 8 to 5), sciences and mathematics, and manual activities. Instead, they were given the option of taking another foreign language or an ancient language. The total number of weekly regular lessons decreased by 2 to 3 hours, but the law introduced the "soutien" classes wich offered additional instruction time in subjects that students had difficulties in. This implied that the weekly number of hours of instruction could go up to 28 in the first year, and to 32.5 in the last year. However, most schools did not offer these classes, as only 30% of classes were considered "soutien" in the three years after the reform (Lewis, 1985).

The reform was initially met with strong opposition from teachers' unions. To alleviate this opposition, Haby agreed to limit classes to 24 students and to hire more teachers—starting the year 1975—with the ultimate goal of having equal shares of high and low track instructors. Between 1975 and 1978, the number of high-track teachers increased from 63,600 to 68,800 while the number of low-track instructors remained relatively stable (increasing from 82,400 to 83,500) (Chapoulie, 1987). The reform went into effect in the aca-

[9] In French, the drop in the number of lessons was larger for low-track students (by 3 hours as opposed to 1 hour for the high track). High-track students also experienced a 1 hour decrease in the time allocated to a foreign language, while low-track students did not.

demic year 1977-1978, but its implementation faced several challenges. First, teachers and principals were still strongly opposed to the idea of mixed-ability grouping, and often found ways to circumvent the law and divide students into classes based on their abilities. A study conducted by the inspector general of the ministry of education, Jean Binon, concluded that only 45% of middle school classes were mixed-ability in 1979-1980. This is because some schools grouped low-performing students or those who attended good primary schools in the same classes.[10] Elective courses were further used to divide students into achievement-based classes. Specifically, students were required to choose a foreign language in their first year of middle school and another course in their third year. High-achieving students would be directed towards specific languages—that is, german in the first year and latin or ancient greek in the third year—thus allowing them to be grouped in the same classes. Using longitudinal survey data on students who were in the first year of middle school in 1989, Cibois (1996) shows that the share of high-skilled students (i.e., the top 30%) who take latin is 54.4% versus only 12.7% of low-skilled students (i.e., the bottom 25%).[11] Before the reform, the option of latin was reserved to high-track students, and 21.4% of them chose it in 1974. Under the new unified system, all students—except those in pre-apprenticeship classes—could take latin, but the share of students selecting it did not substantially increase and was 24.4% in 1979.

One of the reasons why schools maintained ability-grouping was because teachers did not receive any training and had difficulties adjusting their instruction to mixed-ability classes. Many also believed that the standards of education were falling. The law itself also allowed middle school tracking to persist given that students could be placed in pre-apprenticeship classes after two years. Nonetheless, the reform still had several major implications. All students now followed a common education for at least the first two years of middle school. Furthermore, the distinction between high and low tracks was completely abolished. As a result, the type of studies that students could pursue in high school and subsequent post-secondary options were no longer tied to their middle school track.[12]

[10]The law inadvertently gave schools some leeway to keep ability-grouping, since it stated that students who had difficulties could be placed in "soutien" classes where they would receive extra instruction time. However, most schools did not adopt these classes.

[11]High-skilled students are defined as those who received distinctions on math and french assessments taken during their first year of lower secondary school. Low-skilled students are those who did not receive any distinctions on these assessments.
The grouping of students based on the choice of languages is widely documented by a series of studies conducted by the ministry of education (see Landrier and Nakhili, 2010).

[12]This only exception is if a student enrolls in pre-apprenticeship classes.

3 Data

3.1 Data and Sample

My analysis uses data from two different sources. The data do not cover the same individuals but both include each person's month and year of birth. As detailed below, this allows me to compare the education and labor outcomes of individuals born marginally before and after January 1, 1966.

Educational outcomes and demographic characteristics are taken from the survey "Enquête sur la Famille et les Logements". The data set includes information on various aspects of family life for households that are also part of the 2011 population census. Within each household, the survey is administered to either all men or all women, aged 18 years and above. The initial data set includes 238,458 women and 121,312 men. Given that the survey was conducted in 2011, individuals around the cutoff are observed when they are approximately 45 years-old. The data set includes each individual's highest diploma. The track students enrolled in at age 11 strongly determined the type of high school studies and ultimately the type of degree they could obtain. Therefore, I assess whether the reform had educational gains by looking for changes in the type of highest degree received.

Information on individuals' labor market outcomes is extracted from the 2003 to 2013 French Labor Force Survey (LFS). The LFS is a nationally representative household survey administered on a quarterly basis by the French National Institute of Statistics and Economic Studies (INSEE). Each household member aged 15 years and above is interviewed for up to 6 quarters. Individuals report their employment status on a quarterly basis, and their wage from their main occupation on their first and last interview. Therefore, my main labor market sample only includes the first and last quarter an individual is observed in the data set. I further restrict my sample to individuals who are observed at least once between the ages of 40 to 45. As detailed in section 4.1, my analysis sample includes individuals who are born within two years around the threshold of January 1, 1966. While I can observe individuals born in 1966 when they are aged 47 (in the 2012 LFS), those who are born in 1968 can only be followed up until age 45. Therefore, restricting my sample to those aged between 40 and 45 guarantees that I can observe all individuals around the threshold at the same ages regardless of their birth year. I restrict both the education and labor market samples to individuals who are born in France and whose parents are born in France. This is because those who are not may have gone through a different education system abroad. I further drop those who have missing values in the baseline covariates—mainly father's occupation.

3.2 Summary Statistics

Column (1) of table 1 reports the means of key outcomes for the main sample. Columns (2) and (3) further show each outcome's mean based on socioeconomic background. In my main analysis, I document the impacts of the Haby reform on the education and labor market outcomes of all affected students, but also focus on heterogeneous effects by socioeconomic background.[13] This is because track assignment was largely unequal across different socioeconomic backgrounds. I use father's occupation as a proxy for an individual's socioeconomic background. Father's occupation is identified by a two-digit number, with the first digit representing a specific skill level or socioeconomic status.[14] Specifically, I consider an individual to be from a high socioeconomic background (high SES) if his/her father is in a high-skilled occupation, and from a lower socioeconomic background (low SES) if his/her father is in a low or middle-skilled occupation.[15]

Panel A shows the share of individuals with different types of degrees. The share of individuals who hold a vocational degree (i.e., *CAP*) or report having just a middle school education is around 34%.[16] Low-track students typically pursued a *CAP* or just received a middle school education. Since the Haby law abolished the low track, it is likely that this outcome will decrease following the reform. Column (2) reveals that 36% of low SES students have a *CAP* or just a middle school education, as opposed to only 13% of high SES students (column (3)). This is consistent with the fact that students from affluent families dominated the high track, while more disadvantaged students were disproportionately assigned to the low track.

In my analysis, I also focus on the different degrees that high-track students typically pursued. The least skilled students in the high track pursued a secondary school vocational degree (i.e., *BEP*). 15.9% of individuals in the main sample hold this degree. Again, low SES individuals (16.8% in column (2)) are more likely to have a *BEP* than those from affluent backgrounds (7.6% in column (3)). The share of individuals with technical degrees—i.e., degrees obtained after technical education in high school such as the Technician's Baccalaureate and postsecondary technical degrees—is around 28%. Finally, around 9% of individuals

[13]While it would be interesting to document how the reform impacted children with different levels of ability, relevant data are not available.

[14]These constitute the official socioeconomic classification in France—"Nomenclature des professions et categories socioprofessionelles" (PCS)—and are used as reference in all collective agreements.

[15]High-skilled occupations comprise doctors, engineers, judges, lawyers, managerial positions, etc. Middle-skilled occupations consist of school teachers, secretaries, healthcare workers such as nurses, massage therapists and dental assistants, as well as various clerks and technicians, etc. Finally, low-skilled workers are cleaning and maintenance workers such as janitors and housekeepers, as well as personal care and service workers such as childcare and food preparation workers, hairdressers, cashiers, waitresses, etc.

[16]Individuals that have a middle school education but no *CAP* are most likely those who were enrolled in transitional classes or dropped out from school at age 16.

hold a general baccalaureate degree and around 13% have a college degree.[17] 40% of high SES individuals hold a college degree as opposed to only 10% of the low SES sample.

Panel B of table 1 reports the employment rate and average monthly wages for individuals who are employed. The employment rate stands 86%, and low SES individuals earn less than high SES individuals (1,753 versus 2,559 euros per month). Finally, panel C indicates that around 90% of individuals are born in France and 80% have parents who were born in France.

4 Empirical Strategy

4.1 Regression Discontinuity Design

Several features of the institutional setting allow me to use a regression discontinuity design to estimate the impact of the reform on subsequent education and labor market outcomes (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). All students born between January and December of the same year started school in the same academic year at around age 6. Specifically, students born in 1966 enrolled in primary school in the academic year 1972-1973. After completing five years of education, they entered middle school in the academic year 1977-1978—the year the policy went into effect. As a result, students born before January 1, 1966 should have still been exposed to the pre-reform tracking system while those born after should have gone through the milder form of tracking. Thus, I can capture the effect of the reform by comparing students born barely before and barely after January 1, 1966.

The main assumption in this design is that individuals on either sides of the January 1, 1966 cutoff are similar, except for the fact that some were exposed to the milder tracking system and others were not. In that case, any observed discontinuity at the threshold can be attributed to the causal impact of the reform. A potentially complicating factor in this setting is that January 1 is also the school entry cutoff. In other words, on average, students who are born in December enter school at a younger age than those born in January. Hence, comparing students on either side of this threshold might be capturing *both* the effects of the policy and starting school at an older age. In section 5.4, I address this issue by showing that there are no significant threshold-crossing effects on any outcome of interest when using January 1 from years other than 1966 as a fake cutoff. I further present estimates from a difference-in-differences strategy with individuals born around this fake threshold as a control group.

[17]College degree includes all postsecondary academic diplomas from universities and the *Grandes Ecoles*.

When using the RD design, I estimate the following reduced form equation:

$$Y_i = \alpha + \beta D_i + \gamma g(S_i) + \tau g(S_i) \times D_i + \epsilon_i \tag{1}$$

where the dependent variable Y represents one of various outcomes of interest for individual i. D is a dummy variable that is equal to 1 if the student is born on or after January 1, 1966. S is the running variable and represents an individual's month and year of birth. It is defined as months relative to the cutoff. The function $g(.)$ captures the relationship between Y and S. Further, I interact $g(.)$ with D to allow the slopes of the fitted lines to differ on either side of the threshold and control for differential trends in date of birth. Finally, ϵ is the error term. β is the coefficient of interest which represents the intent-to-treat (ITT) impact of the reform on education and labor outcomes. I would need to divide the reduced form estimate by an estimate of the first stage in order to obtain average treatment effects (ATE). Given that I do not have access to data on students' track assignment at age 11, all estimates in this paper capture the impact of being *eligible* for the less rigorous tracking system—that is ITT effects—and not the effect of actually going through it.

In my analysis, I specify $g(.)$ to be a linear function of S and estimate equation (1) over a bandwidth of 15 months on either sides of the cutoff, using local linear regression with a uniform kernel. The optimal bandwidth is chosen using the procedure introduced by Calonico, Cattaneo and Titiunik (2014).[18] Since the running variable is reported in months, I cluster standard errors at the month-year of birth level to deal with concerns over random specification error (Lee and Card, 2008).

4.2 Tests of Identification

A potential concern with the RD design is that the estimated treatment effects would be biased if individuals are strategically sorting to the right of the date of birth cutoff. In this setting, this problem would occur if parents time the date of birth of their child in order to avoid the more rigorous tracking system. However, the policy change was announced almost 9 years after the birth of individuals around the cutoff, rendering manipulation of the running variable impossible. A standard test of the validity of the RD design is to look for a discontinuity at the threshold in the density of the running variable (McCrary, 2008). In Figure A1, I plot the frequency of births in each month as a function of distance from the cutoff. The figure reveals a possibility of a slight increase in the number of births at the

[18]Depending on the outcome of interest, the chosen bandwidths vary between 15 and 19 months. For consistency, I fix the preferred bandwidth at 15 months for all outcomes. I do however show that estimates do not change over a range of other bandwidths.

threshold. Given the timing of the policy announcement, this increase is more likely due to seasonality in births rather than manipulation of the assignment variable. In fact, a similar pattern emerges when looking at the number of births in January of 1965 and 1967 (marked by the vertical dashed lines in figure A1).

A more informative test in this case is to examine whether individuals around the threshold are similar in terms of pre-determined characteristics. Given that I restrict my sample to French-born individuals, I first examine whether there is a discontinuity in the likelihood of being born in France. Figures 3a to 3c respectively plot dummy variables that are equal to 1 if the individual is born in France, the individual's father is born in France and his/her mother is born in France, as a function of distance of his month-year of birth from the cutoff and using one-month bins. Figures 3d and 3e further plot the probability that the individual is male and the likelihood that his/her father is in a low or middle-skilled occupation, as a function of distance of month-year of birth from the cutoff. All graphs are smooth around the threshold and the corresponding regression discontinuity estimates reported in table A2 are small and statistically insignificant across different bandwidths.

5 Results

5.1 Evidence on the implementation of the reform

As previously discussed, I cannot provide an estimate of the first stage using my sample since I do not have information on students' track assignment in the first year of middle school. To provide evidence that the reform was implemented, I use aggregate statistics taken from the ministry of education's annual censuses. The different panels in figure 4 show how the number and share of students in different tracks evolve over time. The reform was implemented in the academic year 1977-78. We therefore expect to see a decrease in the number and share of students placed in the low track starting 1977. However, since students were still tracked in 1976-77, some students should still be observed in the low track in 1977. Figure 4a shows that the number of low-track students is relatively stable between 1972 and 1976, but quickly decreases from 339,143 in 1976 to 167,827 in 1977. This corresponds to a 50.5% drop in the number of students placed in the low track between these two years. By 1978, the number of students in the low track drops to 0. Between 1976 and 1977, the number of high-track students also decreased but at a much lower rate than the low track (by 12% from 1,795,181 to 1,579,780 students), then started increasing after 1977. It should be noted that in all these figures, "high track" refers to the high track before the reform and to the common middle school system after the reform. Figure 4b shows that among students enrolled in the first two years of middle school, the share of low-track students drops from 15.9% in 1976 to 9.6% in 1977 and to 0% in 1978. This is concurrent with a simultaneous increase in the share of students placed in the high track (i.e., the common middle school) from 84.1% in 1976 to 90.4% in 1977 to 100% in 1978.

Figure 4c plots the evolution of the number of students in different tracks over time for last two years of middle school. In this case, we expect to see a drop in the number of low-track students two years after the reform (i.e., in 1979), when the affected cohort starts enrolling in the third year of middle school. Indeed, between 1978 and 1979, the number of students in the low track decreased from 183,668 to 89,416 (or by 51.3%), and to 0 from 1980 onwards. At the same time, the number of students in the high track (i.e., the common middle school) started increasing, while the number of students in pre-apprenticeship classes remained relatively stable. Figure 4d reveals that among students enrolled in the last two years of middle school, the share of low-track students moved from 12.6% to 6.2% between 1978 and 1979 and to 0% afterwards. The share of high-track students increased from 74% in 1978 to 80.8% in 1979 and to 86.8% in 1980. On the other hand, the share of students in pre-apprenticeship classes remained stable over that time period (13.4% in 1978 to 13% in 1979 to 13.2% in 1980). Put together, these results suggest that students who would have been placed in the low track prior to the reform are now pursuing the common middle school system (and not pre-apprenticeship classes).

5.2 Effects of the Reform on Educational Attainment

Prior to the reform, students' high school studies and subsequent degrees were tied to their middle school track. Specifically, low-track and pre-apprenticeship students typically pursued a vocational degree (*CAP*) or left school at age 16. On the other hand, high-track students could eventually obtain another vocational diploma (*BEP*), technical degrees or general education degrees (i.e., the general baccalaureate and subsequently college). As shown in section 5.1, students who would have been previously placed in the low track, are mostly enrolling in the common middle school—and not in pre-apprenticeship classes—after the reform. Hence, it is likely that the reform changed the type of degrees that individuals hold.

I start by examining whether students experienced educational gains after the reform. The different panels in Figure 5 plot the highest degree received as a function of the running variable for all individuals in my sample, and panel A of table 2 reports the corresponding regression estimates. Figure 5a shows no discontinuity in the likelihood of not having a diploma. The corresponding regression estimate reported in column 1 of table 2 is small and statistically insignificant. In Figure 5b, I look at the probability of holding a *CAP* degree or leaving school after middle school—i.e., low-track students' usual educational trajectory prior to the reform. The figure reveals a clear negative shift at the cutoff. The magnitude of this decrease is on the order of 5.5 to 5.7 percentage points (or 16 percent), without and with controls respectively (column 2 of table 2). This is concurrent with a comparable 5.7 percentage points (or 21 percent) increase in the share of individuals who hold a technical degree—that is either a secondary school or a postsecondary technical degree (figure 5d and column 3 of table 2). These results indicate that the reform induced individuals to switch from degrees that are typically accessed through a low track to those that are obtained after pursuing a high track. They are also consistent with the idea that individuals who would have been placed in the low track prior to the reform are now more likely to pursue a common middle school education. Finally, figures 5c, 5e and 5f and their corresponding regression estimates in columns (3), (5) and (6) of table 2 show no evidence of individuals being drawn to the *BEP*, the general baccalaureate or to college degrees following the reform. As a robustness check, I present estimates for all education outcomes taken from regressions that use different bandwidths, as well as without and with controls and month of birth fixed effects. The different columns in table A3 reveal that the estimates remain statistically significant and similar in magnitude across varying specifications. Since women are over-surveyed in the "Enquête sur la Famille et les Logements", I also present estimates taken from regressions that include survey weights in table A4. Again, across different bandwidths, estimates are consistent with the main results, both with and without the inclusion of controls.

Overall, the reform allowed individuals to attain a higher level of qualification. However, its impacts are likely to vary with the type of track a student would have been placed in prior to the reform. Track assignment prior to the reform was based on students' academic achievement in primary schools, but was also highly correlated with socioeconomic status. Students from high socioeconomic backgrounds were significantly more likely than others to be placed in the high track. This correlation between socioeconomic status and track choice is pervasive in countries that track at an early age. Brunello and Checchi (2007) highlight that in most tracking systems, parental background usually affects assignment to different tracks. Dustmann (2004) further documents that in Germany—where tracking also takes place at the end of primary school—family background plays a key role in the type of track a student enrolls in. I therefore look at heterogeneous effects based on socioeconomic background.

I first focus on individuals who were potentially most likely to be placed in the low track prior to the reform—i.e., those whose fathers were in low or middle skilled occupations (low SES). The visual evidence in figure 6 indicates that the impacts of the reform on the low SES group are similar to those of the overall sample. Specifically, low SES individuals are around 6 percentage points (or 15.8 percent) less likely to hold a *CAP* or leave school after middle school (figure 6b and panel B column 2 of table 2), and experience a 5.4 percentage points (or 20.3 percent) increase in the probability of having technical degrees (figure 6d and panel B column 3 of table 2). As in the overall sample, I find no statistically significant impacts on the probabilities of not having a diploma, or holding a *BEP*, a general baccalaureate or a college degree (remaining panels of figure 6 and columns 1, 4 and 5 of panel B in table 2). Figure 7 and panel C of table 2 show results for indivduals whose fathers are in high skilled occupations (high SES). The figures do not reveal any visible discontinuities at the cutoff. The corresponding estimates are for the most part statistically insignificant. However, the reduced sample size and precision prevent me from making definitive conclusions regarding whether this group gained or lost from the reform. Tables A5 to A8 show that results for both low and high SES samples are not sensitive to the choice of different bandwidths or to the inclusion of survey weights.

5.3 Effects on Labor Market Outcomes

Having shown that the reform allowed students to attain a higher level of qualification, I next examine whether it also led to gains in the labor market. The visual evidence is presented in figures 8a and 8b, whereby I respectively plot the likelihood of being employed and the natural log of wages for the entire sample, as a function of distance to the cutoff. While there is no clear visual discontinuity in the employment rate, wages visibly increase at the cutoff. The relevant regression discontinuity estimates are shown in panel A of table 3. Consistent with the visual evidence, the effect on employment is not statistically significant at conventional levels (column (1)), while wages increase by 4.6 log points or 4.7% due to the reform (column (2)). Panel A of table A9 shows that these estimates are robust to bandwidth choice. As further discussed in section 6, the documented increase in wages is not necessarily driven by the rise in the level of qualification but rather by the decrease in tracking intensity. In other words, to get the average treatment effect of the reform, one cannot divide the wage increase by the estimate for highest degree received. Instead, we would need an estimate of the first stage—i.e., whether students were separated into low and high tracks after the reform. The data that I use do not include such information, but the aggregate statistics in section 5.1 suggest that the reform was fully implemented—i.e., that there was a 100% decrease in the likelihood of being separated into a low and high track.

I next examine whether labor market effects vary across socioeconomic backgrounds in figures 9a to 9d. For both the low and high SES samples, no significant employment effects are apparent at the threshold and the corresponding regression estimates are not statistically significant (column (1) of panels B and C of table 3). On the other hand, low SES individuals' wages exhibits a clear discontinuity at the threshold (figure 9b). The increase in wages for this subsample is on the order of 5.1 log points or 5.2% (column (2) of panel B of table 3). The rise in wages is consistent with the documented increase in level of qualification for this subsample. For the high SES sample, figure 9d reveals no clear shift at the cutoff, but the reduced sample size does not allow me to conclusively rule out large effects (column 2 of panel C of table 3). Finally, table A9 shows employment and earnings estimates taken from regressions that use different bandwidths for the full (panel A), low SES (panel B) and high SES (panel C) samples. Estimates from these specifications are similar to the main results.

5.4 Robustness Checks

As previously mentioned, a potential concern with the identification strategy in this context is that January 1 is also the school entry cutoff. The RD design essentially compares students who are born in December 1965 to those born in January 1966. However, individuals who are born in December are relatively younger when they start school than those born in January. Being younger than other students in the same school cohort could potentially have an impact on a range of education and labor outcomes (Bedard and Dhuey, 2006). As a result, the previously presented estimates could be capturing both the effects of the reform and being relatively older than individuals in the same school cohort. This concern is dampened in this case since I focus on outcomes measured at older ages—when individuals are in their 40s—and previous studies find that relative school entry age effects tend to disappear over the long run (Black et al., 2011). Nonetheless, I deal with this issue in two manners.

First, I show that there are no visible discontinuities in my main outcomes when using January 1 from years other than 1966 as a fake threshold. The different panels in figures A2 and A3 plot individuals' main education and labor market outcomes as a function of distance of their month-year of birth to January 1964—which is indicated by zero on the x-axis. Figures A4 and A5 repeat the same exercise but using January 1968 as a fake cutoff. These are individuals who were born 2 years before (1964) and 2 years after (1968) the treated cohort. No visible discontinuities in any of the outcomes are apparent at these fake thresholds, both for the full sample (panel A) and the low SES sample (panel B). The absence of discontinuities in untreated cohorts' outcomes indicates that the impact of the school entry cutoff is not necessarily confounding the estimated treatment effects.

Second, I show that estimates from a difference-in-differences (DID) strategy are comparable to the ones from the regression discontinuity design. Specifically, I compare individuals born in the 6 months before and after the January 1, 1966 cutoff to those born within the same months but in years where there was no policy change. By using individuals born around January 1 in years other than 1966 as a control group, the DID should separate the impacts of the policy from any existing age of school entry effects—assuming that the magnitudes of these effects are comparable across cohorts. I estimate the following reduced form equation:

$$Y_i = \eta + \delta D_i^{1966} + \lambda M_i^{Jan} + \mu D_i^{1966} \times M^{Jan} + \upsilon_i \tag{2}$$

where Y is an outcome of interest for individual i, D_i^{1966} is a dummy variable that is equal to 1 if an individual is born in the treated cohort (that is between July 1965 and June 1966) and 0 if the individual is born in untreated cohorts (that is between July 1962 and June 1969 and excluding the treated cohort). M_i^{Jan} is a dummy variable that is equal to 1 if the individual is born between January and June (versus between July and December). The interaction between D_i^{1966} and M_i^{Jan} indicates that an individual is born in the treated year and months. Therefore, μ is the coefficient of interest and υ is the error term. Standard errors are clustered at the month-year of birth level.

The DID estimates for all education and labor market outcomes are presented in table 4, without and with controls and across different subsamples. Similarly to the RD results, I can detect statistically significant effects on the likelihood of holding *CAP* and technical degrees, as well as the natural log of wages for only the full and low SES samples. These effects are smaller in magnitude, but close to the ones from the RD design. For the full sample, the DID indicates a 3.1 percentage points decrease in the probability of having a *CAP* or having only a middle school education (panel A, column (2)), and a 3.6 percentage points increase in the likelihood of holding a technical degree (panel A, column (4)). I further find no significant employment effects, but a 3.8 log points or 3.9% rise in wages for the full sample (panel A, columns (5) and (6) of table 4). As in the main analysis, these effects seem to be driven by low SES individuals (panel B). Indeed, they experience a 3.6 percentage points decrease in the likelihood of having a *CAP* or only a middle school education, and a 3 percentage points increase in the share of technical degrees. This is coupled with a 4.6% increase in their wages. For the high SES sample (panel C), I cannot detect statistically significant effects for most outcomes but I also cannot rule out relatively large effects due to reduced sample size.

6 Discussion

Overall, I find that the Haby reform had long-term benefits as it raised individuals' wages at ages 40 to 45, and increased the probability that individuals hold technical rather than vocational degrees. One potential explanation for the documented benefits is that the reform improved the quality of middle schools. Specifically, the Haby law changed the content of the curriculum, limited classes to 24 students and allowed for the hiring of new teachers. I therefore examine whether these changes are driving my results. First, the hiring of new teachers started at the end of 1975—and not in the year the policy went into effect (i.e., the academic year 1977-78)—thus its impact should not be captured by my main estimates. The first cohort that was affected by the hiring of teachers was the one born in 1964. If teacher hiring had significant impacts on students' education and labor outcomes, I should be able to observe those effects by comparing individuals on either sides of the January 1, 1964 birth cutoff. In figures A2 and A3, I show that there are no discontinuities in my main outcomes when using January 1, 1964 as a fake cutoff. This indicates that the hiring of new teachers does not explain my results.

Second, to rule out the possibility that changes in class size and curriculum may be driving the wage effects, I examine how education and labor estimates vary by municipality of residence. Prior to the reform, urban and rural municipalities exhibited strong differences in their middle school system. In rural—and particularly small—municipalities, the separation of students into high and low tracks was less rigid than in urban municipalities, as many rural schools did not offer the high track. This implies that compared to rural areas, urban municipalities experienced a larger decrease in the intensity of tracking. On the other hand, changes in curriculum and class size were applied to both types of municipalities. Hence, if curriculum and class size changes are driving my results, then my estimates should be similar across different types of municipalities.[19] Column (2) of table A10 reveals that the likelihood of holding a *CAP* or having only a middle school education decreased by 6.6 and 5.4 percentage points for rural and urban residents, respectively. Columns (3) and (4) show that rural residents are more likely to pursue a *BEP* degree, while urban residents are more likely to hold technical degrees after the reform. This indicates that individuals from rural and urban municipalities experienced an increase in their level of education, as both the *BEP* and technical degrees are considered to be a higher level of qualification than the *CAP*. Strikingly, column (6) shows that urban residents' wages increase by 6.5 log points or 6.7%, while the impact on wages of rural residents is small and statistically insignificant. The

[19] In results available upon request, I find that the likelihood of residing in a rural versus urban municipality is not impacted by the reform.

absence of wage effects for rural residents indicates that class size and curriculum changes are not necessarily driving my main estimates.[20] Taken together, these results indicate that the documented wage gains can be attributed to shifting from a rigid to a milder form of tracking and not to changes in the quality of education.

An interesting question is how much of the wage effect is accounted for by the documented increase in the level of qualification. The results in table A10 can help shed light on the matter. Specifically, both urban and rural residents experienced an increase in their level of education, as they are respectively more likely to hold technical and *BEP* degrees (and less likely to hold a *CAP* or a middle school education only). However, only urban residents' wages increased due to the reform. This suggests that the increase in the level of education is not necessarily driving the documented impact on wages. Furthermore, Malamud and Pop-Eleches (2010) find that delaying tracking in Romanian high schools increased the likelihood that men attain general rather than vocational degrees, but had no significant effects on their earnings. They "conclude that differences in labor market returns between graduates of vocational and general schools are largely driven by selection". Taken together, these results suggest that the documented earnings gains in my context are most likely driven by a decrease in the intensity of tracking at an early age—and not necessarily by an increase in educational levels.

7 Conclusion

Many countries stream students in classes or tracks based on their abilities. Critics fear that tracking might increase initial disadvantages in academic achievement and widen economic inequalities. However, the effects on students' education and labor market opportunities largely depend on the design of tracking systems. In Europe, tracks usually provide students with substantially different learning experiences and postsecondary options. Schools in the United States generally have in place a milder form of tracking where students are divided into achievement-based classes, but no significant disparities exist between classes—in terms of curricula, academic focus or future career paths.

This paper examines the impacts of shifting from a system where tracks offer considerably different learning environments to simple ability-grouping. I focus on the French middle

[20] Furthermore, previous literature on the impact of smaller classes on long-run wages is mixed. Leuven and Løkken (2018) find that a one-student decrease in Norwegian middle school classes has no significant effect on earnings at age 48. Fredriksson et al. (2012) show that decreasing class size by one pupil in Swedish primary schools increases wages by 0.6 percent between the ages of 27 to 42, and this effect is driven by high-performing students. In contrast, the wage increase in my setting is driven by students who were most likely to be placed in the low track (i.e., potentially low-performing students).

school system where students were allocated to tracks that differ in terms of curricula, level and pace of instruction and career paths. I exploit a reform which abolished the tracks—hence the dependence between middle school track and future career options—and set a common curriculum, but still allowed for students to be grouped in classes based on their abilities. Using a regression discontinuity design based on date of birth, I find that the reform increased the overall level of education and long-run wages for students at the margin of eligibility. These effects are concentrated among individuals from low socioeconomic backgrounds, who were most likely to be allocated to the lower tracks prior to the reform.

Perhaps the biggest difference between simply grouping students in ability-based classes and European-style tracking is that the latter locks students from an early age in tracks that lead to different career options. The pre-reform French system was also rigid in that students could rarely change tracks. As a result, misallocation of students to tracks cannot easily be corrected, whereas this is easier to achieve with a milder form of ability-grouping or with systems that have built-in flexibilities and allow for track reversal (Dustmann et al., 2017). Finally, my findings suggest that decreasing the intensity of tracking at an early age can have long-term educational and labor market benefits, and may reduce socioeconomic inequalities.

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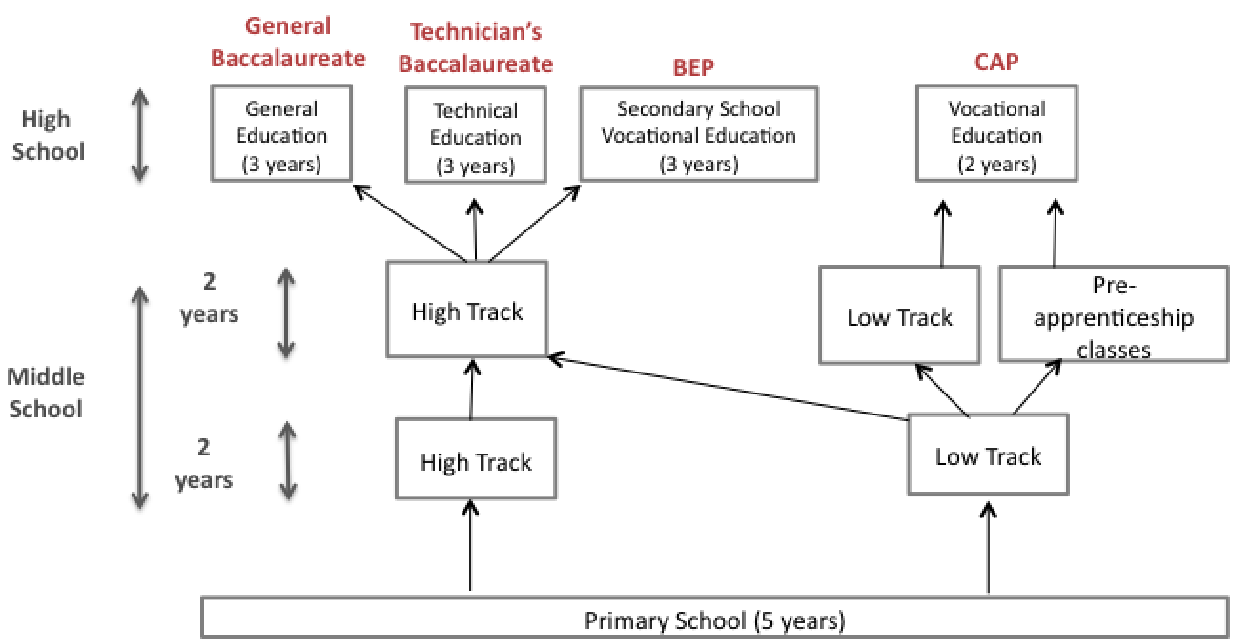
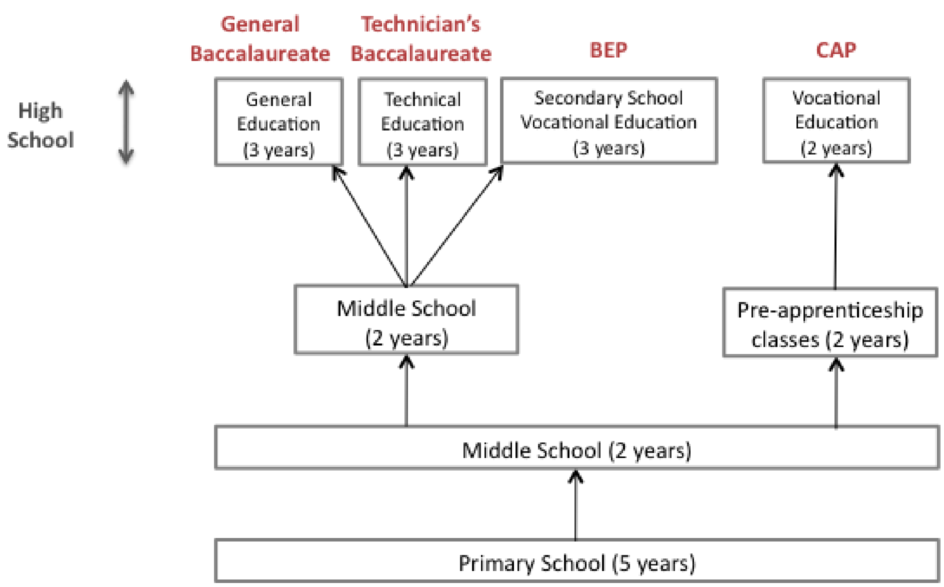
A Figures and Tables

Figure 1: Educational system prior to the reform



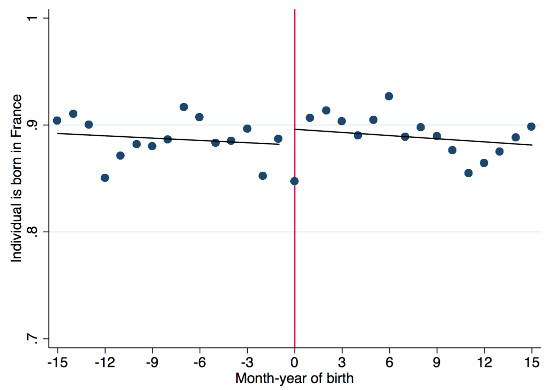
Notes: This figure shows the structure of the French school system prior to the Haby reform. The degrees that students can obtain at the end of high school are denoted in red.

Figure 2: Educational system after the reform

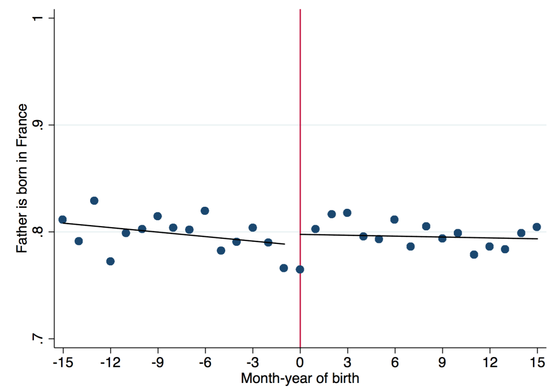


Notes: This figure shows the structure of the French school system after the Haby reform. The reform did not change the organization of primary schools and high schools or the compulsory school leaving age. The degrees that students can obtain at the end of high school are denoted in red.

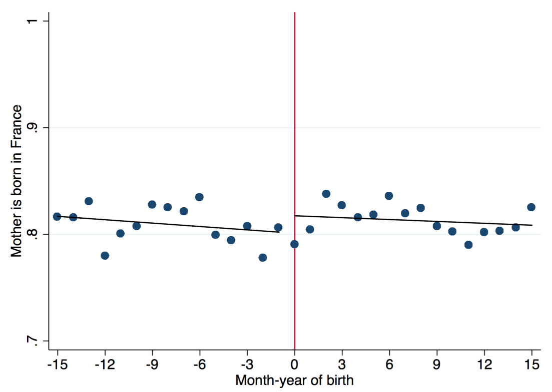
Figure 3: Covariate balance



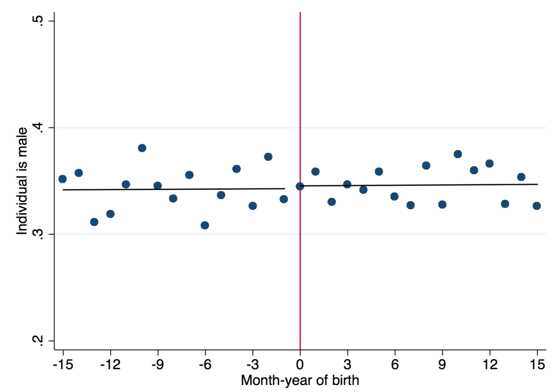
(a) Individual is born in France



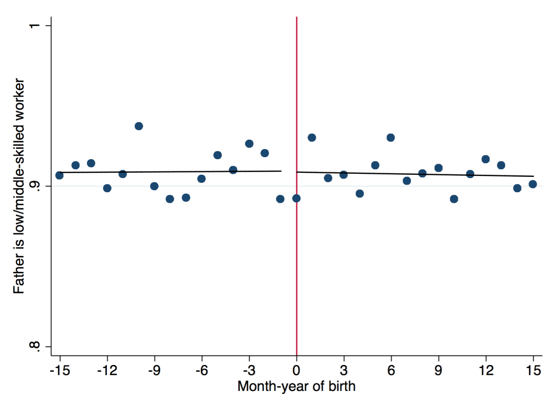
(b) Father is born in France



(c) Mother is born in France



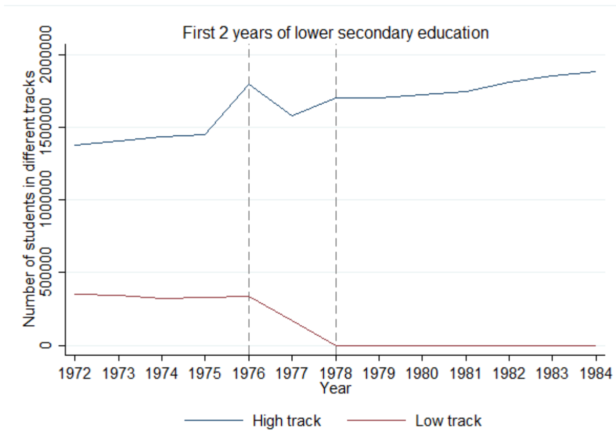
(d) Individual is male



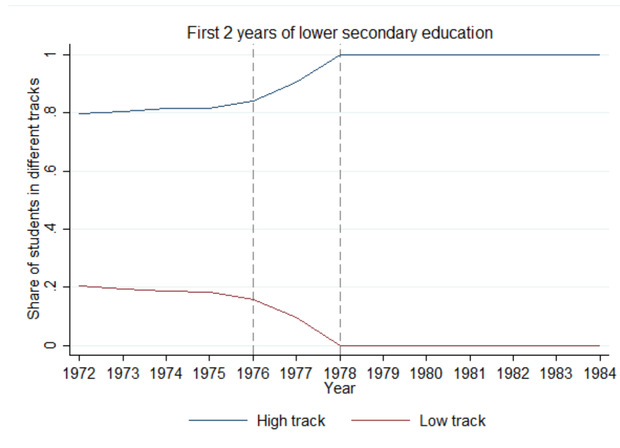
(e) Father is a low or middle skilled worker

Notes: The different panels show various baseline covariates, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

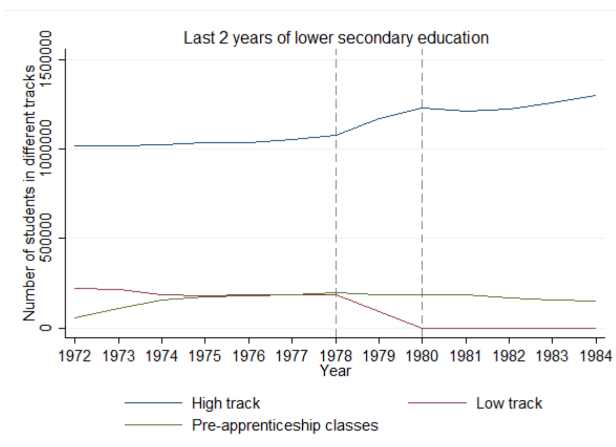
Figure 4: Number and share of students enrolled in different tracks



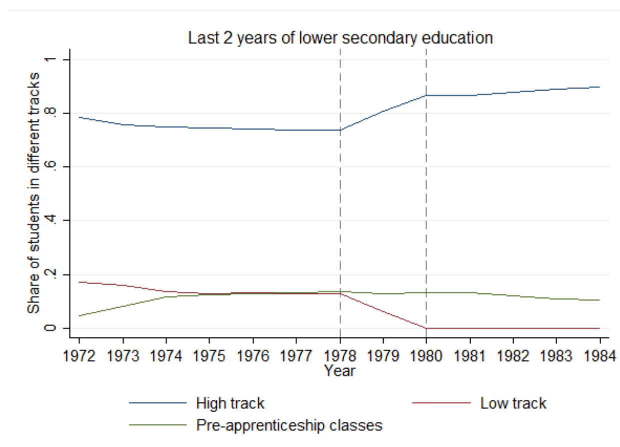
(a) Number of students in different tracks (first 2 years)



(b) Share of students in different tracks (first 2 years)



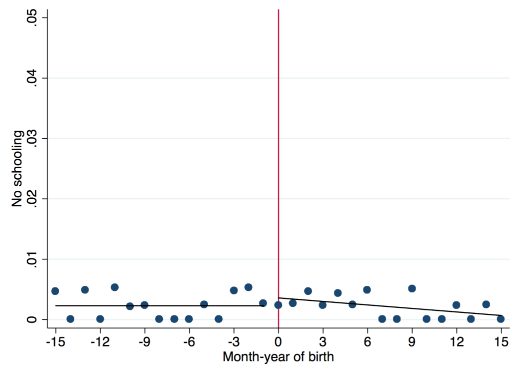
(c) Number of students in different tracks (last 2 years)



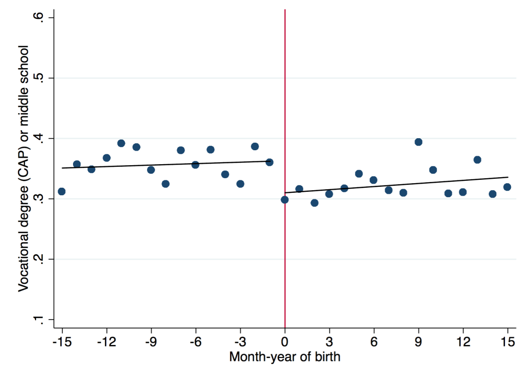
(d) Share of students in different tracks (last 2 years)

Notes: The different figures plot the evolution over time in the number and share of students in different tracks. Panels (a) and (b) correspond to students who are enrolled in the first 2 years of middle school. Panels (c) and (d) are students who are enrolled in the last 2 years of middle school. Shares are computed based on the numbers in panels (a) and (c). Source: Defresne and Krop (2016).

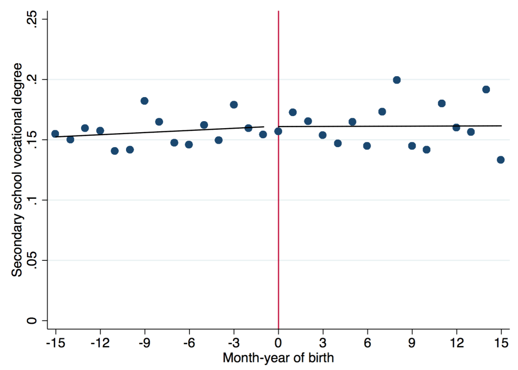
Figure 5: Effect of the reform on highest degree received (overall sample)



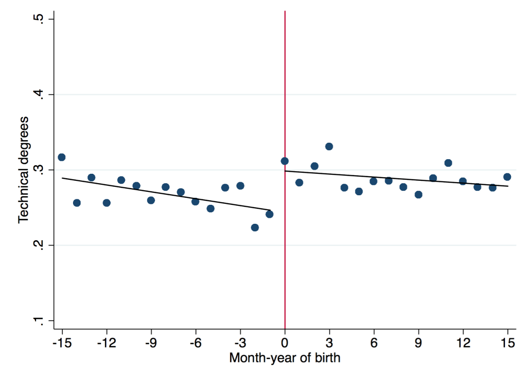
(a) No schooling



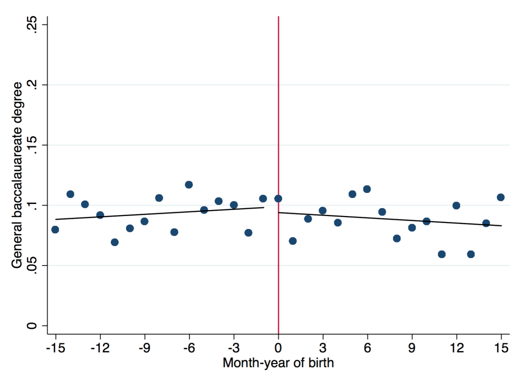
(b) *CAP* or middle school only



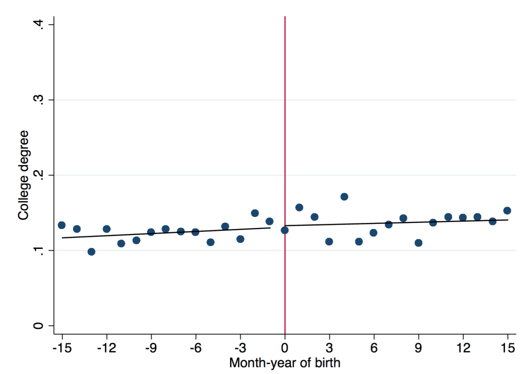
(c) Secondary school vocational degree (*BEP*)



(d) Technical degrees



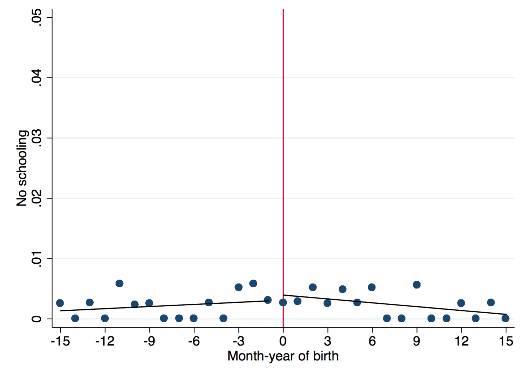
(e) General baccalaureate degree



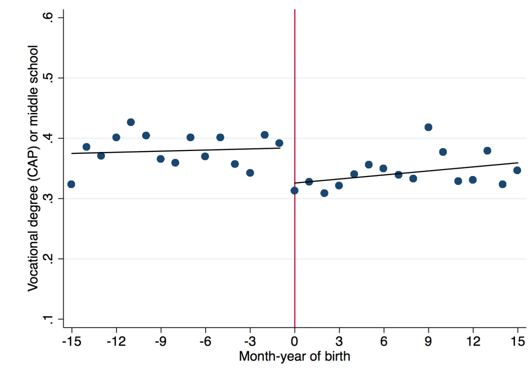
(f) College degree

Notes: Sample includes all individuals. The different panels show the probabilities of holding various degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

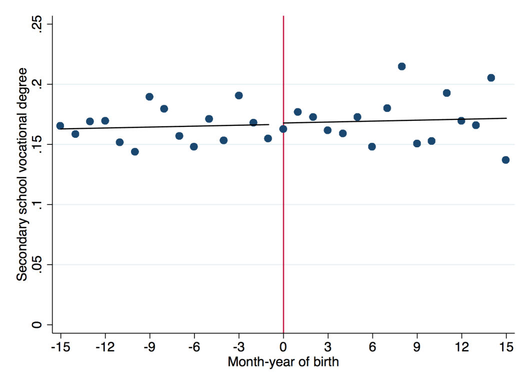
Figure 6: Effect of the reform on highest degree received (low SES sample)



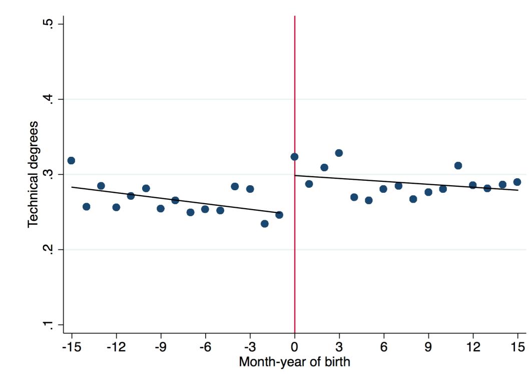
(a) No schooling



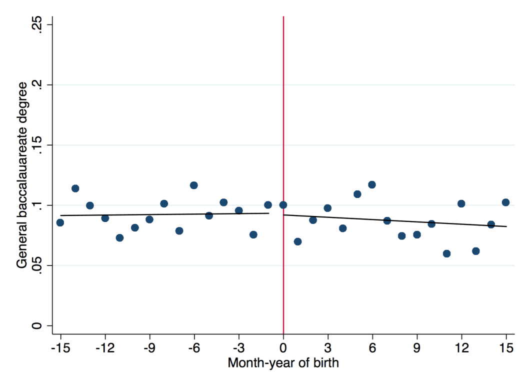
(b) *CAP* or middle school only



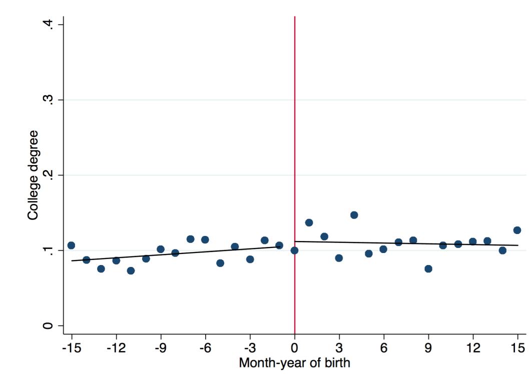
(c) Secondary school vocational degree (*BEP*)



(d) Technical degrees



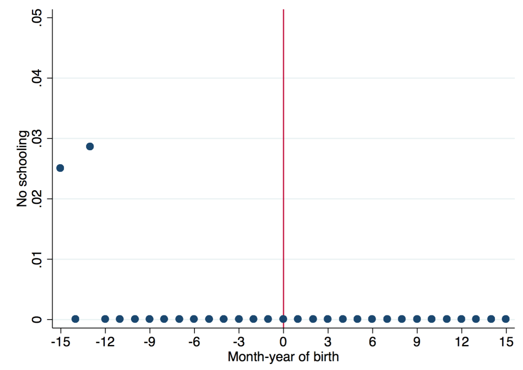
(e) General baccalaureate degree



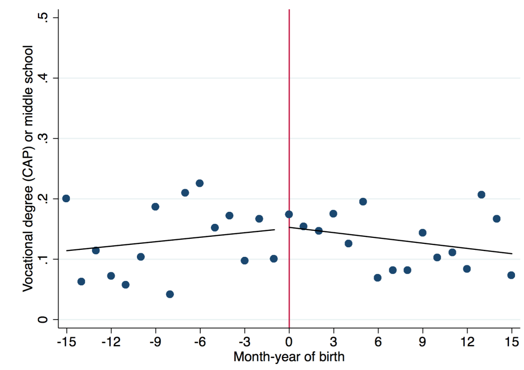
(f) College degree

Notes: Sample includes individuals whose fathers are in low or middle skilled occupations. The different panels show the probabilities of holding various degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

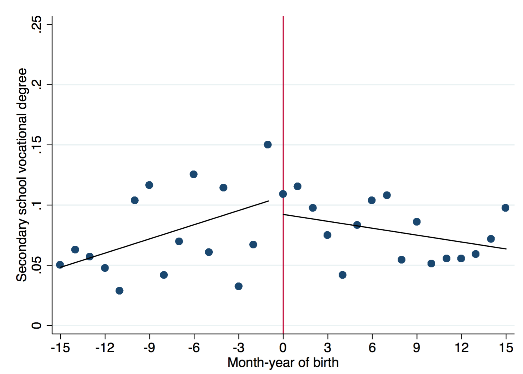
Figure 7: Effect of the reform on highest degree received (high SES sample)



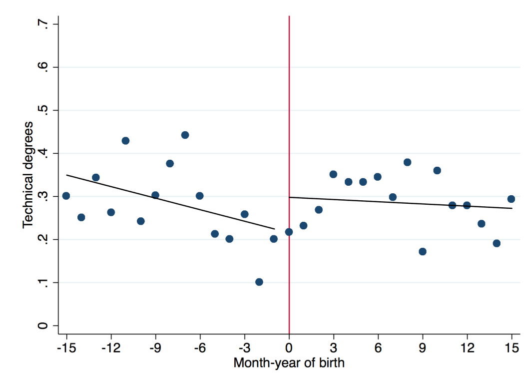
(a) No schooling



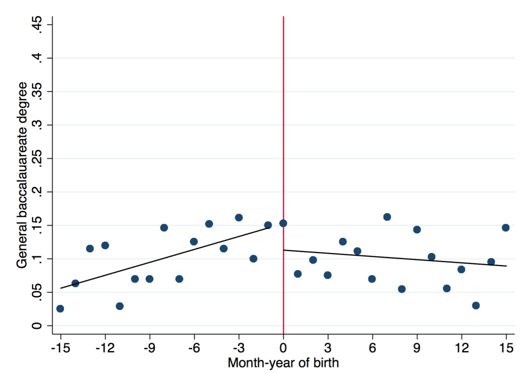
(b) *CAP* or middle school only



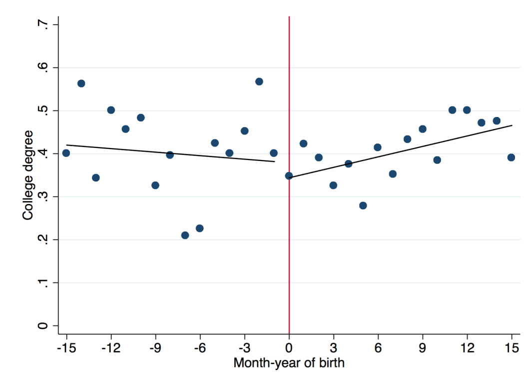
(c) Secondary school vocational degree (*BEP*)



(d) Technical degrees



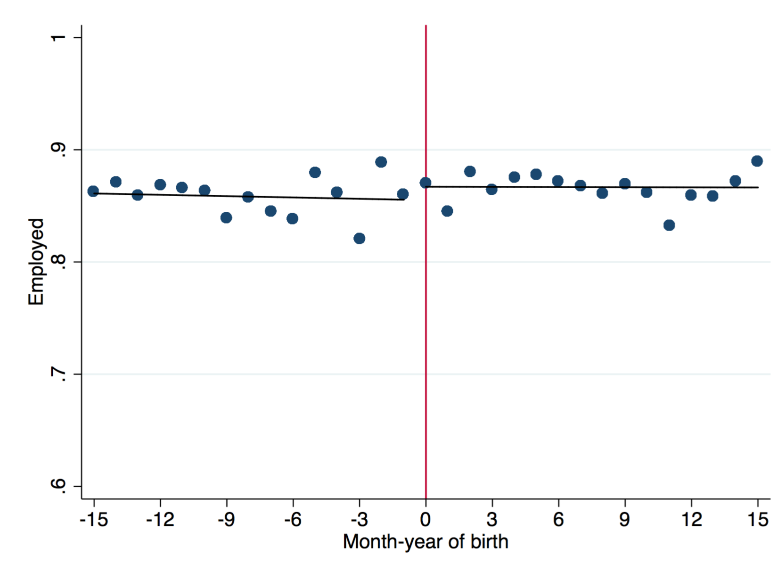
(e) General baccalaureate degree



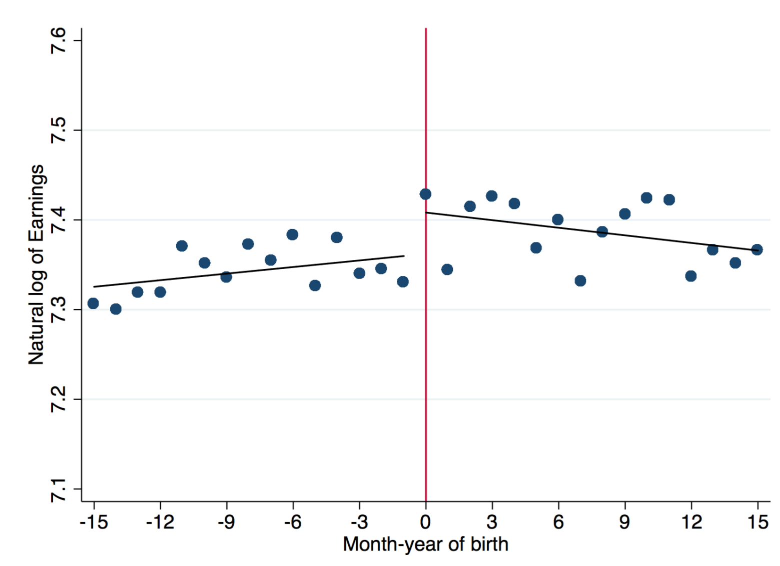
(f) College degree

Notes: Sample includes individuals whose fathers are in high skilled occupations. The different panels show the probabilities of holding various degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

Figure 8: Effect of the reform on labor market outcomes (overall sample)



(a) Individual is employed

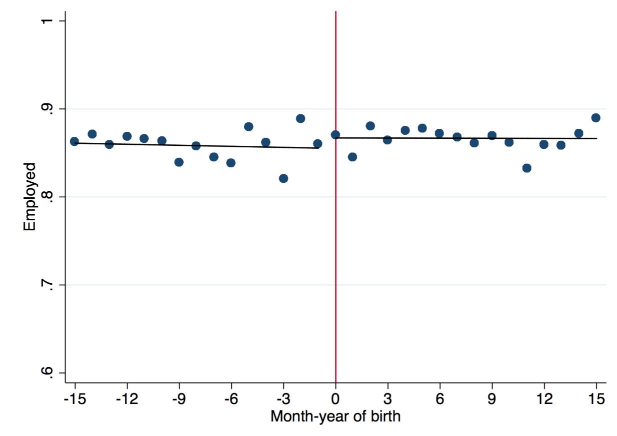


(b) Natural log of wages

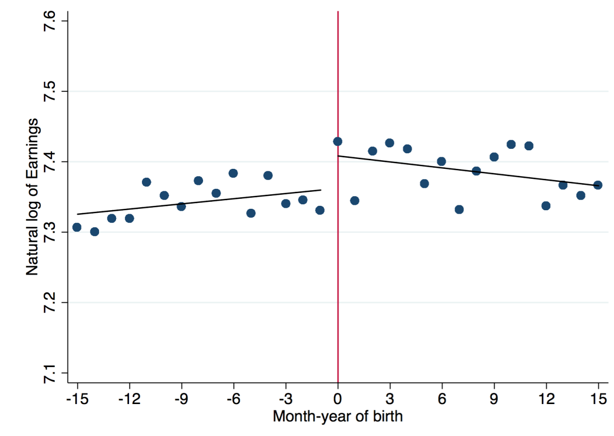
Notes: Sample includes all individuals. The different figures plot labor market outcomes as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

Figure 9: Effect of the reform on labor market outcomes (low and high SES samples)

A. Low SES

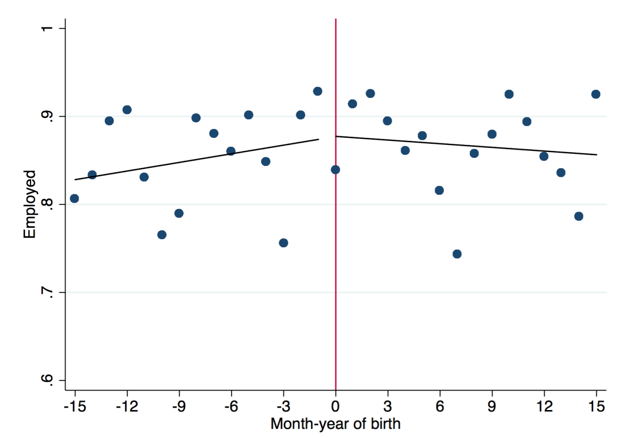


(a) Individual is employed

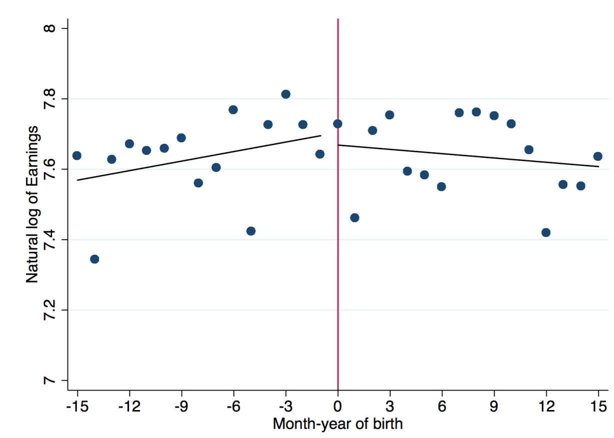


(b) Natural log of wages

B. High SES



(c) Individual is employed



(d) Natural log of wages

Notes: The different figures plot labor market outcomes, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. The figures on the top correspond to low SES individuals, and the ones on the bottom to high SES individuals.

Table 1: Sample means for key variables

	Overall sample (1)	Low SES (2)	High SES (3)
A. Education outcomes			
Vocational degree (CAP) or middle school only	0.339	0.361	0.133
Secondary School vocational degree (BEP)	0.159	0.168	0.076
Technical degrees	0.278	0.277	0.286
General baccalaureate degree	0.090	0.089	0.099
College degree	0.129	0.101	0.403
N	12,211	11,093	1,118
B. Labor market outcomes			
Is employed	0.861	0.862	0.858
N	21,674	19,285	2,389
Monthly wages (in euros)	1,838	1,753	2,559
N	14,386	12,870	1,516
C. Demographic characteristics			
Individual is born in France	0.887	0.889	0.869
Father is born in France	0.796	0.799	0.778
Mother is born in France	0.811	0.814	0.785
N	15,967	14,405	1,562

Note: Each cell reports means for key variables. Column 1 includes all individuals observed within 15 months on either side of the threshold. Columns 2 and 3 restrict the sample to low SES and high SES individuals, respectively. Education outcomes and demographic characteristics are taken from the “Enquête sur la Famille et les Logements”. Labor market outcomes are extracted from the French labor force survey.

Table 2: Regression discontinuity estimates for highest degree received

	No schooling	*CAP* or middle school only	Secondary school vocational degree (*BEP*)	Technical degrees	General baccalaureate degree	College degree
	(1)	(2)	(3)	(4)	(5)	(6)
A. Overall sample						
No controls	0.001	– 0.055***	– 0.004	0.057***	– 0.002	0.004
	(0.001)	(0.015)	(0.008)	(0.014)	(0.010)	(0.012)
With controls	0.001	– 0.057***	– 0.003	0.056***	– 0.000	0.002
	(0.001)	(0.014)	(0.009)	(0.010)	(0.008)	(0.009)
Observations	12,211	12,211	12,211	12,211	12,211	12,211
B. Low SES						
No controls	0.001	– 0.060***	– 0.004	0.054***	0.001	0.008
	(0.001)	(0.017)	(0.009)	(0.015)	(0.009)	(0.011)
With controls	0.000	– 0.062***	– 0.003	0.054***	0.003	0.007
	(0.001)	(0.015)	(0.011)	(0.010)	(0.008)	(0.010)
Observations	11,093	11,093	11,093	11,093	11,093	11,093
C. High SES						
No controls	– 0.004	– 0.004	– 0.010	0.085	– 0.032	– 0.046
	(0.002)	(0.034)	(0.024)	(0.051)	(0.021)	(0.058)
With controls	– 0.003	– 0.010	– 0.003	0.093**	– 0.034*	– 0.056
	(0.002)	(0.034)	(0.018)	(0.038)	(0.017)	(0.035)
Observations	1,118	1,118	1,118	1,118	1,118	1,118

Note: Each cell reports the reduced form estimate of the impact of the reform on the corresponding outcome. Estimates are taken from local linear regressions using a bandwidth of 15 months, without and with controls respectively. Controls include month of birth fixed effects and a dummy variable for whether the individual is male. In panel A, controls also include a dummy variable for whether the individual is from a low socioeconomic background. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table 3: Regression discontinuity estimates for labor market outcomes

	Employed (1)	Natural log of wages (2)
A. Overall sample		
No controls	0.015	0.046**
	(0.014)	(0.020)
With controls	0.017*	0.046***
	(0.009)	(0.017)
Observations	21,674	14,386
B. Low SES		
No controls	0.016	0.059***
	(0.013)	(0.020)
With controls	0.016	0.051***
	(0.010)	(0.016)
Observations	19,285	12,870
C. High SES		
No controls	0.010	– 0.031
	(0.039)	(0.079)
With controls	0.024	– 0.002
	(0.031)	(0.051)
Observations	2,389	1,516

Note: Each cell reports the reduced form estimate of the impact of the reform on the corresponding outcome. Estimates are taken from local linear regressions using a bandwidth of 15 months, without and with controls respectively. Controls include a dummy variable for whether the individual is male, and fixed effects for month of birth, age, quarter and year of survey. In panel A, controls also include a dummy variable for whether the individual is from a low socioeconomic background. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

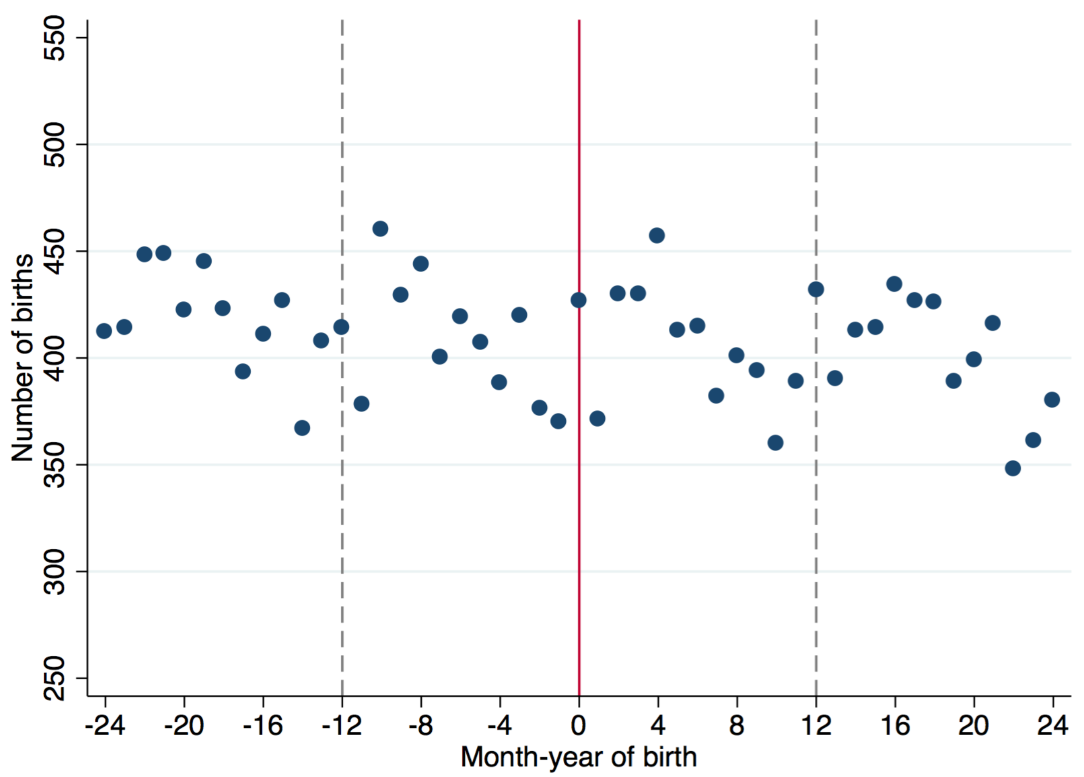
Table 4: Difference-in-differences estimates

	No schooling	*CAP* or middle school only	Secondary school vocational degree (*BEP*)	Technical degrees	General baccalaureate degree	College degree	Employed	Natural log of wages
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Overall sample								
No controls	0.000	– 0.031**	0.001	0.035***	– 0.007	0.001	0.010	0.050***
	(0.001)	(0.015)	(0.007)	(0.013)	(0.008)	(0.012)	(0.011)	(0.019)
With controls	0.000	– 0.031**	0.001	0.036***	– 0.007	0.001	0.008	0.038*
	(0.001)	(0.015)	(0.007)	(0.013)	(0.008)	(0.012)	(0.010)	(0.020)
Observations	34,178	34,178	34,178	34,178	34,178	34,178	51,193	33,960
B. Low SES								
No controls	0.000	– 0.035**	0.004	0.030**	– 0.007	0.008	0.008	0.055***
	(0.001)	(0.016)	(0.008)	(0.014)	(0.008)	(0.011)	(0.011)	(0.019)
With controls	0.000	– 0.036**	0.004	0.030**	– 0.006	0.008	0.006	0.045***
	(0.001)	(0.015)	(0.008)	(0.014)	(0.008)	(0.011)	(0.010)	(0.019)
Observations	31,020	31,020	31,020	31,020	31,020	31,020	45,574	30,382
C. High SES								
No controls	0.002	0.012	– 0.024	0.089**	– 0.013	– 0.066	0.025	0.001
	(0.002)	(0.026)	(0.022)	(0.037)	(0.019)	(0.050)	(0.032)	(0.072)
With controls	0.002	0.011	– 0.024	0.091**	– 0.013	– 0.067	0.022	– 0.014
	(0.002)	(0.026)	(0.022)	(0.037)	(0.019)	(0.050)	(0.030)	(0.067)
Observations	3,158	3,158	3,158	3,158	3,158	3,158	5,619	3,578

Note: Each cell reports the difference-in-differences estimate of the effect of the reform on the corresponding outcome. Results are shown both with and without controls. Controls include a dummy variable for whether the individual is male. For labor market outcomes, controls also include age, quarter and year of survey fixed effects. For panel A, controls further comprise a dummy variable for whether the father is a low/middle skilled worker. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

B Appendix Figures and Tables

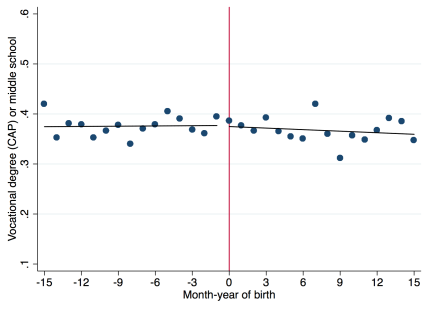
Figure A1: Frequency of births



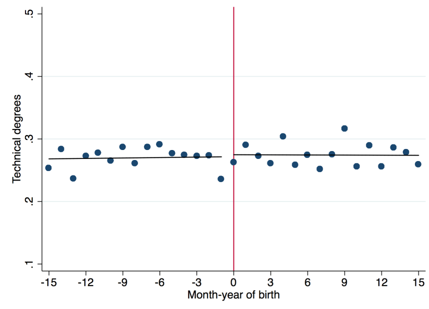
Notes: Data are taken from the "Enquête sur la Famille et les Logements". The figure represents the number of individuals born in each month-year around the cutoff (red vertical line). The dashed vertical lines mark births in January of the years before and after the treated year (i.e. 1965 and 1967).

Figure A2: Placebo test for main education outcomes using January 1964 as a fake cutoff

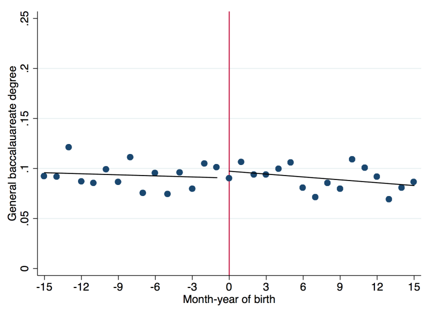
A. Overall sample



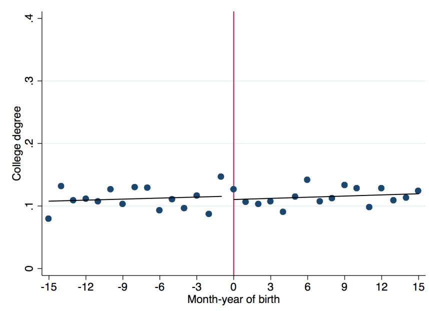
(a) *CAP* or middle school only



(b) Technical degrees

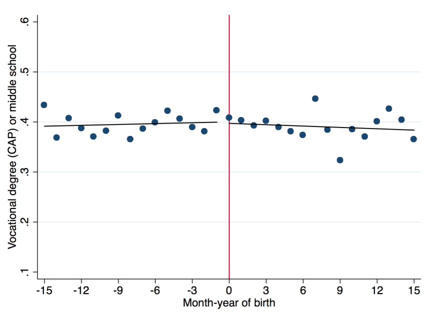


(c) General baccalaureate degree

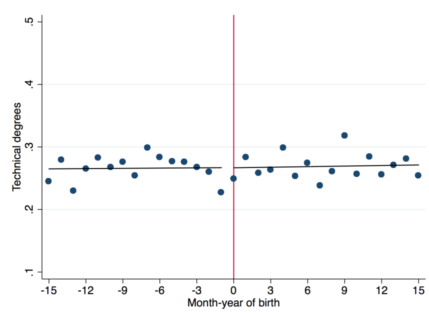


(d) College degree

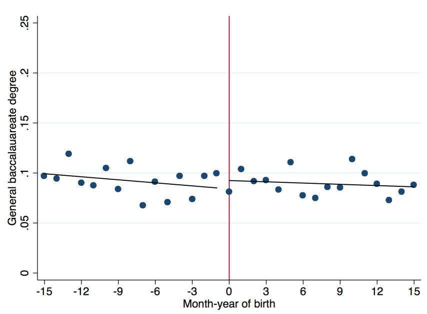
B. Low SES



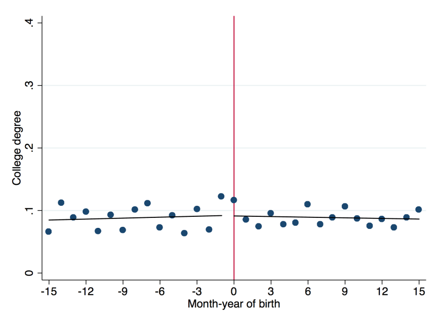
(e) *CAP* or middle school only



(f) Technical degree



(g) General baccalaureate degree

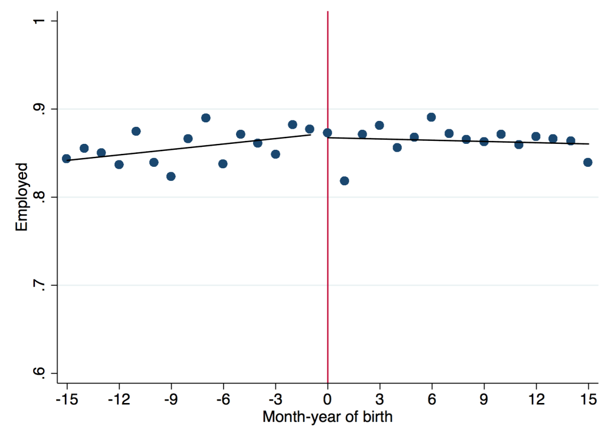


(h) College degree

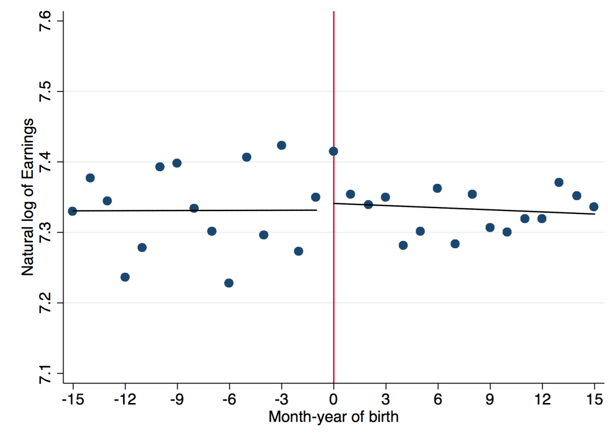
Notes: The figures plot the likelihoods of holding various degrees as a function of the distance of individuals' month-year of birth from January 1964. The figures in panel A are for the overall sample. Those in panel B are for low SES individuals. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

Figure A3: Placebo test for labor market outcomes using January 1964 as a fake cutoff

A. Overall sample

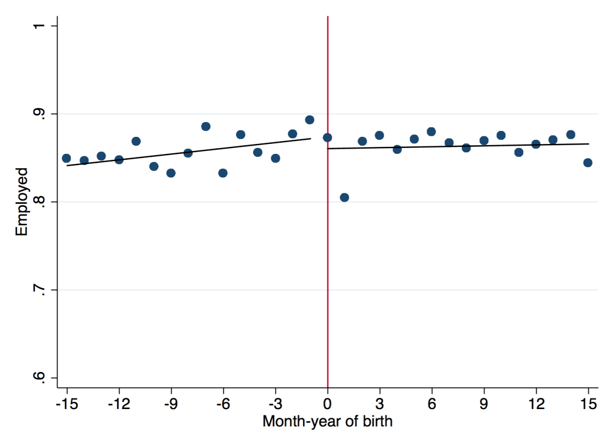


(a) Is employed

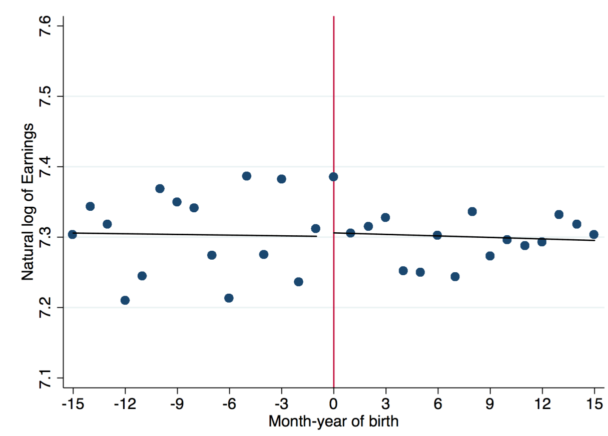


(b) Natural log of wages

B. Low SES



(c) Is employed

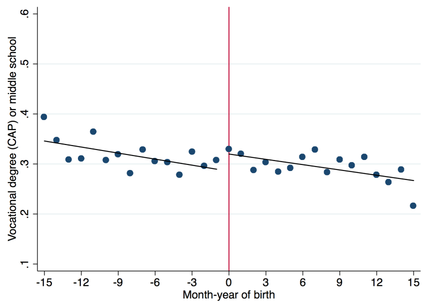


(d) Natural log of wages

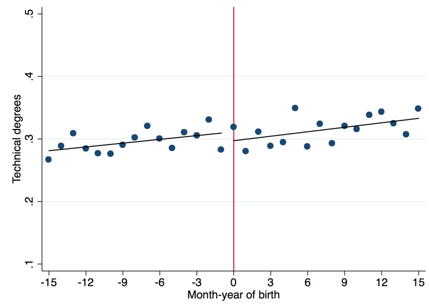
Notes: The figures plot labor market outcomes as a function of the distance of individuals' month-year of birth from January 1964. The figures in panel A are for the overall sample. Those in panel B are for low SES individuals. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

Figure A4: Placebo test for main education outcomes using January 1968 as a fake cutoff

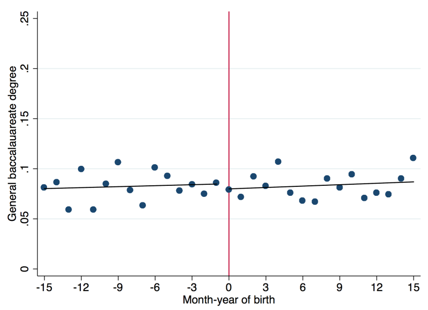
A. Overall sample



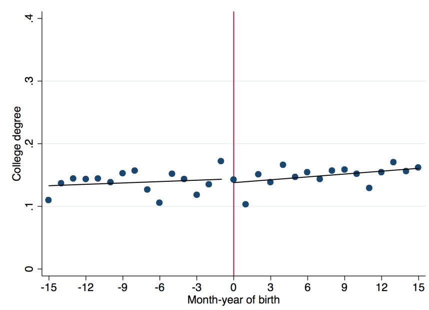
(a) *CAP* or middle school only



(b) Technical degrees

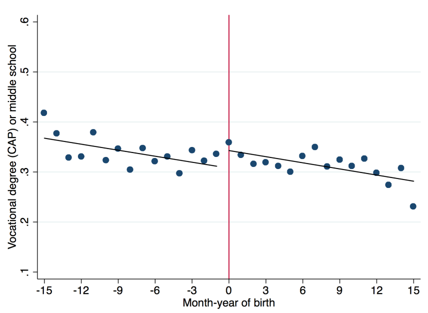


(c) General baccalaureate degree

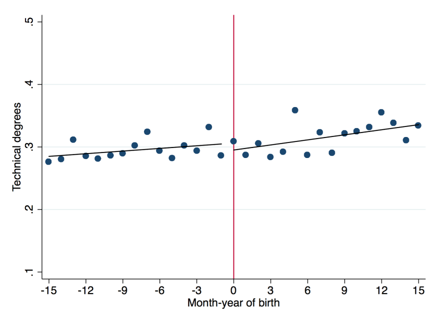


(d) College degree

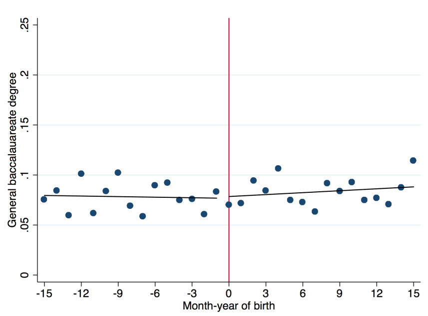
B. Low SES



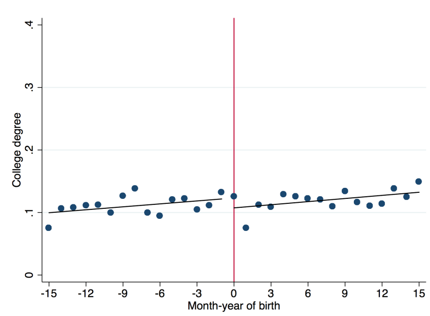
(e) *CAP* or middle school only



(f) Technical degree



(g) General baccalaureate degree

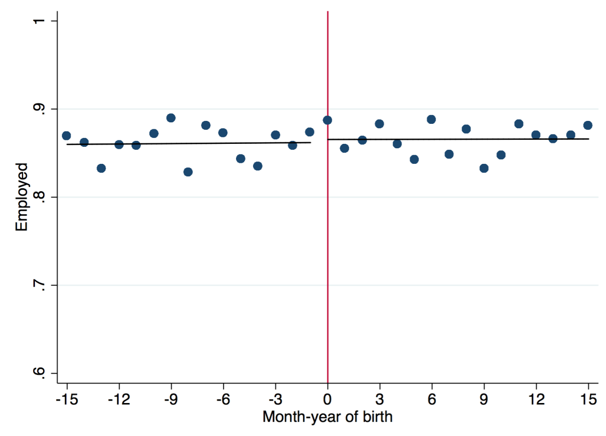


(h) College degree

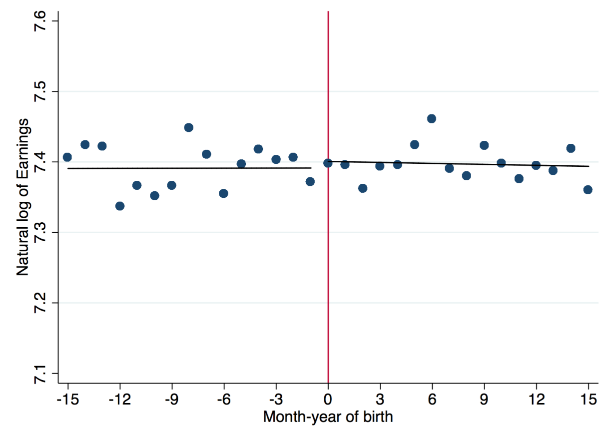
Notes: The figures plot the likelihoods of holding various degrees as a function of the distance of individuals' month-year of birth from January 1968. The figures in panel A are for the overall sample. Those in panel B are for low SES individuals. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

Figure A5: Placebo test for labor market outcomes using January 1968 as a fake cutoff

A. Overall sample

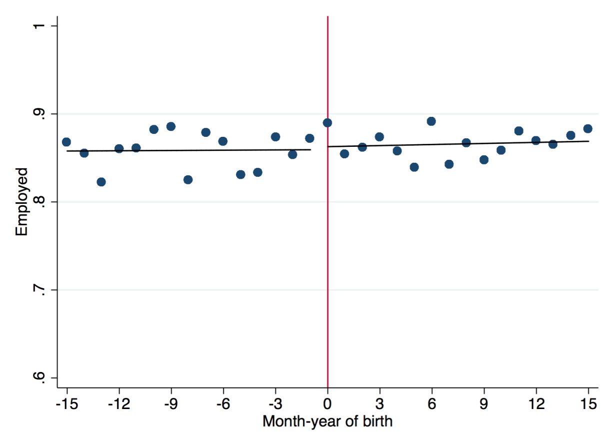


(a) Is employed

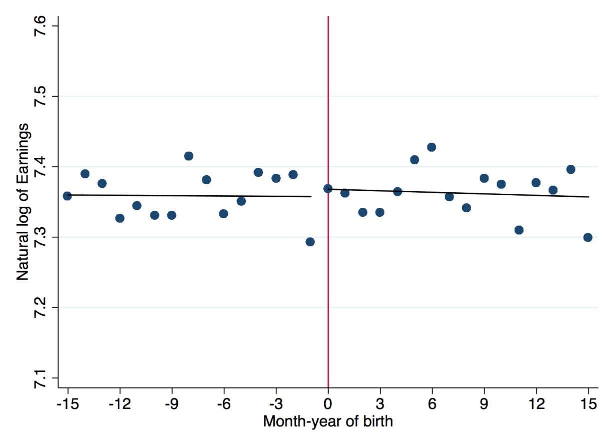


(b) Natural log of wages

B. Low SES



(c) Is employed



(d) Natural log of wages

Notes: The figures plot labor market outcomes as a function of distance of individuals' month-year of birth from January 1968. The figures in panel A are for the overall sample. Those in panel B are for low SES individuals. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months.

Table A1: Middle school curriculum before and after the reform

	1st year			Last year	
	Pre-reform		Post-reform	Pre-reform	Post-reform
	High track	Low track		High track	
	(1)	(2)	(3)	(4)	(5)
French	6	8	5	5	5
Mathematics	4	4	3	3	3
Foreign language	4	3	3	3	3
Sciences and Technology	2	-	3	3	3
History/Geography/Civics	3.5	4	3	3	3
Arts/Music/Handicrafts	3	3	2	3	2
Manual activities	-	-	2	-	2
Physical education	5	5	3	5	3
Elective course: students choose one of the following subjects (in last year only)					
Latin	-	-	-	4	3
Ancient greek	-	-	-	3	3
Second foreign language	-	-	-	3	3
Extra time in first foreign language	-	-	-	2	-
Technology	-	-	-	-	3
Total	27.5	27	24	27-29	27

Note: This table shows the number of weekly lessons allocated for each subject across different tracks in the first and last years of middle school, before and after the reform. Prior to reform, (i) the duration of each lesson is between 50 minutes and 1 hour, (ii) in the last year of the low track, students take 8 lessons in French, 12 in mathematics, science and technology and 3 lessons in manual activities and, (iii) the first year of low track and the last year of high track did not comprise history and geography so the corresponding lessons were fully allocated to civics. After the reform, (i) the duration of each lesson is 55 minutes and, (ii) students in “soutien” classes received additional instruction time in subjects that they had difficulties in, implying that the weekly number of hours of instruction could go up to 28 in the first year, and to 32.5 in the last year. Sources: Hall (1976) and Lewis (1985).

Table A2: Regression estimates for baseline covariates using different bandwidths

	BW=9 (1)	BW=15 (2)	BW=18 (3)	BW=21 (4)	BW=24 (5)	BW=27 (6)
Individual is born in France	0.009	0.017	0.010	0.008	0.006	0.008
	(0.009)	(0.017)	(0.015)	(0.013)	(0.013)	(0.012)
Father is born in France	0.002	0.012	0.004	0.003	0.001	0.005
	(0.008)	(0.013)	(0.011)	(0.011)	(0.010)	(0.010)
Mother is born in France	0.008	0.019	0.011	0.011	0.010	0.012
	(0.008)	(0.012)	(0.011)	(0.010)	(0.010)	(0.009)
Observations	9,615	15,967	19,238	22,549	25,727	28,862
Individual is male	0.004	–0.000	0.015	0.010	0.009	0.004
	(0.007)	(0.012)	(0.011)	(0.010)	(0.010)	(0.010)
Father is low or middle skilled worker	0.003	–0.001	–0.003	0.001	–0.001	–0.002
	(0.006)	(0.010)	(0.009)	(0.009)	(0.008)	(0.007)
Observations	7,379	12,211	14,713	17,243	19,642	22,022

Note: Each cell reports the estimate of the effect of the reform on the corresponding baseline covariate. Estimates are taken from local linear regressions and each column uses the listed bandwidth (BW). Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A3: Regression discontinuity estimates for overall sample's education outcomes across different bandwidths

	BW=9 (1)	BW=15 (2)	BW=18 (3)	BW=21 (4)	BW=24 (5)	BW=27 (6)
No schooling	0.001	0.001	0.001	0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
With controls	0.001	0.001	0.001	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CAP or middle school only	–0.041***	–0.055***	–0.040**	–0.031**	–0.029**	–0.030**
	(0.009)	(0.015)	(0.015)	(0.014)	(0.013)	(0.012)
With controls	–0.042***	–0.057***	–0.041***	–0.033**	–0.032**	–0.031***
	(0.009)	(0.014)	(0.013)	(0.014)	(0.014)	(0.011)
Secondary school vocational degree (*BEP*)	0.003	–0.004	–0.000	–0.004	–0.004	–0.002
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)
With controls	0.003	–0.003	0.002	–0.001	–0.001	0.001
	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Technical degrees	0.032***	0.057***	0.041***	0.036***	0.032***	0.027**
	(0.009)	(0.014)	(0.013)	(0.012)	(0.011)	(0.011)
With controls	0.032***	0.056***	0.043***	0.035***	0.033***	0.025**
	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.010)
General baccalaureate degree	–0.004	–0.002	–0.005	–0.003	–0.002	–0.002
	(0.006)	(0.010)	(0.009)	(0.008)	(0.008)	(0.007)
With controls	–0.004	–0.000	–0.006	–0.003	–0.001	–0.002
	(0.006)	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)
College degree	0.009	0.004	0.005	0.003	0.003	0.006
	(0.008)	(0.012)	(0.011)	(0.010)	(0.009)	(0.009)
With controls	0.009	0.002	0.002	0.001	0.001	0.006
	(0.007)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	7,379	12,211	14,713	17,243	19,642	22,022

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions and each column uses the listed bandwidth (BW). Results are shown both with and without controls. Controls include month of birth fixed effects—except for the bandwidth of 9 months—and dummy variables for whether the individual is male and whether the father is a low/middle skilled worker. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A4: Regression discontinuity estimates for overall sample's education outcomes across different bandwidths (with survey weights)

	BW=9 (1)	BW=15 (2)	BW=18 (3)	BW=21 (4)	BW=24 (5)	BW=27 (6)
No schooling	0.001	0.003	0.002	0.001	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
With controls	0.001	0.003*	0.002*	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CAP or middle school only	–0.047***	–0.061**	–0.046**	–0.040**	–0.037**	–0.039**
	(0.014)	(0.023)	(0.022)	(0.020)	(0.018)	(0.017)
With controls	–0.048***	–0.068***	–0.049**	–0.044**	–0.043**	–0.044***
	(0.014)	(0.023)	(0.021)	(0.020)	(0.020)	(0.016)
Secondary school vocational degree (*BEP*)	0.004	0.000	0.002	–0.006	–0.006	–0.006
	(0.009)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)
With controls	0.003	0.001	0.001	–0.003	–0.004	–0.004
	(0.009)	(0.013)	(0.012)	(0.012)	(0.012)	(0.011)
Technical degrees	0.033**	0.057***	0.038**	0.041***	0.037***	0.031**
	(0.012)	(0.017)	(0.016)	(0.015)	(0.013)	(0.013)
With controls	0.034**	0.058***	0.041**	0.039***	0.037***	0.028**
	(0.012)	(0.013)	(0.015)	(0.014)	(0.014)	(0.012)
General baccalaureate degree	–0.004	–0.003	–0.004	–0.005	–0.003	–0.002
	(0.008)	(0.011)	(0.010)	(0.009)	(0.009)	(0.009)
With controls	–0.004	–0.005	–0.006	–0.006	–0.005	–0.002
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
College degree	0.012	0.003	0.009	0.009	0.008	0.016
	(0.012)	(0.015)	(0.015)	(0.014)	(0.013)	(0.013)
With controls	0.014	0.011	0.010	0.013	0.013	0.021**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
Observations	7,379	12,211	14,713	17,243	19,642	22,022

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions using survey weights and each column uses the listed bandwidth (BW). Results are shown both with and without controls. Controls include month of birth fixed effects—except for the bandwidth of 9 months—and dummy variables for whether the individual is male and whether the father is a low/middle skilled worker. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A5: Regression discontinuity estimates for low SES individuals' education outcomes across different bandwidths

	BW=9 (1)	BW=15 (2)	BW=18 (3)	BW=21 (4)	BW=24 (5)	BW=27 (6)
No schooling	0.001	0.001	0.000	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
With controls	0.001	0.000	0.001	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CAP or middle school only	–0.044***	–0.060***	–0.045***	–0.037**	–0.035**	–0.035***
	(0.009)	(0.017)	(0.016)	(0.015)	(0.013)	(0.013)
With controls	–0.045***	–0.062***	–0.047***	–0.038**	–0.037**	–0.036***
	(0.009)	(0.015)	(0.015)	(0.015)	(0.016)	(0.012)
Secondary school vocational degree (*BEP*)	0.003	–0.004	0.002	–0.004	–0.004	–0.002
	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
With controls	0.003	–0.003	0.004	0.000	–0.000	0.001
	(0.008)	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)
Technical degrees	0.033***	0.054***	0.038**	0.036***	0.031**	0.027**
	(0.009)	(0.015)	(0.014)	(0.013)	(0.012)	(0.011)
With controls	0.033***	0.054***	0.039***	0.035***	0.032***	0.024**
	(0.009)	(0.010)	(0.012)	(0.011)	(0.011)	(0.010)
General baccalaureate degree	–0.003	0.001	–0.002	–0.002	–0.000	–0.002
	(0.006)	(0.009)	(0.009)	(0.008)	(0.007)	(0.007)
With controls	–0.002	0.003	–0.003	–0.001	0.000	–0.002
	(0.006)	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)
College degree	0.010	0.008	0.007	0.006	0.008	0.012
	(0.007)	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)
With controls	0.010	0.007	0.005	0.003	0.004	0.012
	(0.007)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
Observations	6,696	11,093	13,357	15,663	17,834	19,988

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions and each column uses the listed bandwidth (BW). Results are shown both with and without controls. Controls include month of birth fixed effects—except for the bandwidth of 9 months—and a dummy variable for whether the individual is male. Standard errors are clustered by month-year of birth and are reported in parentheses (*** p < 0.01 ** p <0.05 * p <0.1).

Table A6: Regression discontinuity estimates for low SES individuals' education outcomes across different bandwidths (with survey weights)

	BW=9 (1)	BW=15 (2)	BW=18 (3)	BW=21 (4)	BW=24 (5)	BW=27 (6)
No schooling	0.002	0.002	0.002	0.001	0.002	0.002
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
With controls	0.002	0.002	0.002	0.001	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CAP or middle school only	–0.053***	–0.069**	–0.054**	–0.048**	–0.045**	–0.048**
	(0.015)	(0.027)	(0.025)	(0.022)	(0.021)	(0.020)
With controls	–0.054***	–0.076***	–0.055**	–0.052**	–0.051**	–0.053***
	(0.015)	(0.025)	(0.024)	(0.023)	(0.023)	(0.018)
Secondary school vocational degree (*BEP*)	0.005	0.001	0.004	–0.006	–0.006	–0.006
	(0.011)	(0.015)	(0.013)	(0.013)	(0.012)	(0.012)
With controls	0.005	0.002	0.004	–0.002	–0.003	–0.003
	(0.011)	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)
Technical degrees	0.038***	0.060***	0.040**	0.045***	0.039***	0.033**
	(0.012)	(0.017)	(0.017)	(0.015)	(0.014)	(0.013)
With controls	0.038***	0.062***	0.042**	0.043***	0.040***	0.031**
	(0.012)	(0.012)	(0.016)	(0.014)	(0.014)	(0.012)
General baccalaureate degree	–0.005	–0.004	–0.004	–0.006	–0.005	–0.005
	(0.009)	(0.011)	(0.010)	(0.010)	(0.009)	(0.009)
With controls	–0.005	–0.005	–0.005	–0.006	–0.004	–0.004
	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
College degree	0.014	0.010	0.011	0.014	0.014	0.024**
	(0.010)	(0.014)	(0.014)	(0.012)	(0.012)	(0.012)
With controls	0.014	0.015	0.013	0.015	0.016	0.027**
	(0.010)	(0.012)	(0.011)	(0.012)	(0.012)	(0.011)
Observations	6,696	11,093	13,357	15,663	17,834	19,988

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions using survey weights and each column uses the listed bandwidth (BW). Results are shown both with and without controls. Controls include month of birth fixed effects—except for the bandwidth of 9 months—and a dummy variable for whether the individual is male. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A7: Regression discontinuity estimates for high SES individuals' education outcomes across different bandwidths

	BW=9 (1)	BW=15 (2)	BW=18 (3)	BW=21 (4)	BW=24 (5)	BW=27 (6)
No schooling	0.000	–0.004	0.002	–0.000	–0.001	–0.002
	(0.000)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
With controls	0.000	–0.003	0.005	0.002	0.002	–0.001
	(0.000)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
CAP or middle school only	–0.013	–0.004	0.015	0.021	0.025	0.028
	(0.026)	(0.034)	(0.028)	(0.028)	(0.026)	(0.026)
With controls	–0.013	–0.010	0.012	0.017	0.020	0.028
	(0.025)	(0.034)	(0.026)	(0.026)	(0.026)	(0.023)
Secondary school vocational degree (*BEP*)	–0.002	–0.010	–0.020	–0.003	–0.001	–0.001
	(0.016)	(0.024)	(0.023)	(0.022)	(0.020)	(0.019)
With controls	–0.002	–0.003	–0.025	–0.007	–0.005	–0.002
	(0.016)	(0.018)	(0.019)	(0.020)	(0.019)	(0.018)
Technical degrees	0.029	0.085	0.070	0.031	0.036	0.028
	(0.038)	(0.051)	(0.048)	(0.046)	(0.042)	(0.040)
With controls	0.028	0.093**	0.092**	0.041	0.040	0.029
	(0.039)	(0.038)	(0.038)	(0.042)	(0.042)	(0.036)
General baccalaureate degree	–0.014	–0.032	–0.038*	–0.018	–0.012	0.003
	(0.017)	(0.021)	(0.022)	(0.027)	(0.024)	(0.022)
With controls	–0.014	–0.034*	–0.040**	–0.024	–0.018	0.001
	(0.017)	(0.017)	(0.017)	(0.023)	(0.023)	(0.019)
College degree	0.000	–0.046	–0.029	–0.031	–0.047	–0.056
	(0.038)	(0.058)	(0.051)	(0.050)	(0.047)	(0.044)
With controls	0.000	–0.056	–0.044	–0.029	–0.039	–0.054
	(0.038)	(0.035)	(0.038)	(0.042)	(0.045)	(0.039)
Observations	683	1,118	1,356	1,580	1,808	2,034

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions and each column uses the listed bandwidth (BW). Results are shown both with and without controls. Controls include month of birth fixed effects—except for the bandwidth of 9 months—and a dummy variable for whether the individual is male. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A8: Regression discontinuity estimates for high SES individuals' education outcomes across different bandwidths (with survey weights)

	BW=9	BW=15	BW=18	BW=21	BW=24	BW=27
	(1)	(2)	(3)	(4)	(5)	(6)
No schooling	0.000	0.005*	0.001	–0.000	–0.001	–0.002
	(0.000)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
With controls	0.000	0.009**	0.005	0.002	0.002	–0.001
	(0.000)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)
CAP or middle school only	–0.002	0.005	0.018	0.028	0.038	0.040
	(0.028)	(0.050)	(0.041)	(0.037)	(0.035)	(0.033)
With controls	–0.002	0.003	0.010	0.025	0.028	0.040
	(0.029)	(0.050)	(0.037)	(0.035)	(0.033)	(0.030)
Secondary school vocational degree (*BEP*)	–0.015	–0.005	–0.020	–0.006	–0.005	–0.008
	(0.018)	(0.030)	(0.028)	(0.026)	(0.023)	(0.023)
With controls	–0.015	–0.002	–0.021	–0.012	–0.009	–0.013
	(0.018)	(0.027)	(0.026)	(0.024)	(0.024)	(0.023)
Technical degrees	–0.004	0.033	0.022	0.002	0.014	0.003
	(0.049)	(0.072)	(0.065)	(0.059)	(0.055)	(0.053)
With controls	–0.004	0.039	0.046	0.007	0.015	0.001
	(0.049)	(0.057)	(0.052)	(0.055)	(0.054)	(0.050)
General baccalaureate degree	0.006	0.006	–0.009	0.005	0.007	0.019
	(0.016)	(0.019)	(0.018)	(0.022)	(0.022)	(0.020)
With controls	0.006	–0.007	–0.019	–0.014	–0.012	0.013
	(0.015)	(0.020)	(0.019)	(0.023)	(0.027)	(0.021)
College degree	0.015	–0.044	–0.012	–0.029	–0.053	–0.052
	(0.046)	(0.069)	(0.063)	(0.059)	(0.057)	(0.053)
With controls	0.015	–0.043	–0.020	–0.008	–0.024	–0.041
	(0.046)	(0.044)	(0.045)	(0.048)	(0.052)	(0.043)
Observations	683	1,118	1,356	1,580	1,808	2,034

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions and each column uses the listed bandwidth (BW). Results are shown both with and without controls. Controls include month of birth fixed effects—except for the bandwidth of 9 months—and a dummy variable for whether the individual is male. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A9: Regression discontinuity estimates for labor outcomes across different bandwidths

	BW=9 (1)	BW=15 (2)	BW=18 (3)	BW=21 (4)	BW=24 (5)	BW=27 (6)
A. Overall sample						
Employed	0.014*	0.015	0.017	0.019*	0.013	0.012
	(0.008)	(0.014)	(0.013)	(0.011)	(0.010)	(0.009)
With controls	0.007	0.017*	0.014	0.015	0.011	0.007
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.008)
Observations	13,445	21,674	25,873	29,619	33,054	36,544
Log of wages	0.040***	0.046**	0.040**	0.036**	0.046***	0.050***
	(0.013)	(0.020)	(0.019)	(0.018)	(0.017)	(0.016)
With controls	0.037**	0.046***	0.034**	0.029*	0.031*	0.032**
	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)	(0.015)
Observations	8,901	14,386	17,171	19,659	21,936	24,255
B. Low SES						
Employed	0.016**	0.016	0.017	0.020*	0.013	0.012
	(0.007)	(0.013)	(0.012)	(0.011)	(0.010)	(0.010)
With controls	0.007	0.016	0.011	0.014	0.008	0.007
	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)	(0.009)
Observations	11,965	19,285	23,015	26,350	29,415	32,544
Log of wages	0.045***	0.059***	0.048**	0.043**	0.054***	0.055***
	(0.014)	(0.020)	(0.020)	(0.019)	(0.018)	(0.016)
With controls	0.048***	0.051***	0.038**	0.032*	0.033*	0.036**
	(0.015)	(0.016)	(0.017)	(0.017)	(0.017)	(0.016)
Observations	7,968	12,870	15,346	17,578	19,615	21,705
C. High SES						
Employed	0.000	0.010	0.025	0.016	0.020	0.012
	(0.026)	(0.039)	(0.038)	(0.035)	(0.032)	(0.030)
With controls	0.007	0.024	0.040	0.024	0.031	0.014
	(0.034)	(0.031)	(0.029)	(0.030)	(0.029)	(0.027)
Observations	1,480	2,389	2,858	3,269	3,639	4,000
Log of wages	0.001	−0.031	−0.008	−0.009	−0.014	−0.011
	(0.052)	(0.079)	(0.071)	(0.066)	(0.064)	(0.059)
With controls	−0.052	−0.002	−0.004	0.008	0.014	−0.001
	(0.067)	(0.051)	(0.052)	(0.049)	(0.053)	(0.047)
Observations	933	1,516	1,825	2,081	2,321	2,550

Note: Each cell reports the reduced form estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions and each column uses the listed bandwidth (BW). Results are shown both with and without controls. Controls include month of birth, age, quarter and year of survey fixed effects—except for the bandwidth of 9 months—and a dummy variable for whether the individual is male. In panel A, controls include a dummy variable for whether the individual's father is a low/middle skilled worker. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).

Table A10: Heterogeneous effects by rural and urban municipalities

	No schooling	*CAP* or middle school only	Secondary school vocational degree (*BEP*)	Technical degrees	General baccalaureate degree	College degree	Employed	Natural log of wages
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Rural municipality								
With controls	0.001	– 0.066***	0.046**	0.032	– 0.005	– 0.007	0.024	– 0.002
	(0.002)	(0.016)	(0.019)	(0.022)	(0.016)	(0.015)	(0.021)	(0.019)
Observations	4,387	4,387	4,387	4,387	4,387	4,387	5,557	3,700
B. Urban municipality								
With controls	0.001	– 0.054***	– 0.031***	0.069***	0.003	0.011	0.015	0.065**
	(0.002)	(0.018)	(0.006)	(0.014)	(0.009)	(0.014)	(0.011)	(0.024)
Observations	7,824	7,824	7,824	7,824	7,824	7,824	16,117	10,686

Note: Each cell reports the regression discontinuity estimate of the effect of the reform on the corresponding outcome. Estimates are taken from local linear regressions using a bandwidth of 15 months. Results are shown with controls, which include month of birth fixed effects and dummy variables for whether the individual is male and whether his/her father is in a low/middle skilled occupation. For labor market outcomes, controls also include age, quarter and year of survey fixed effects. Standard errors are clustered by month-year of birth and are reported in parentheses (*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$).