

The Long-Run Effects of Reducing Early School Tracking*

Serena Canaan[†]

This Version: August 25, 2020

Abstract

This paper studies the effects of a French detracking reform on individuals' long-run education and labor market outcomes. The reform delayed the age at which students were separated into vocational and general education by two years, from age 11 to 13. The assignment of students to different types of education at age 11 was instead replaced by ability grouping, whereby students followed a common general education curriculum but were divided into different classrooms based on their academic achievement. Using a regression discontinuity design, I find that the reform raised individuals' level of education and increased their wages by 6 percent at ages 40 to 45. These effects are concentrated among men and individuals from low socioeconomic backgrounds. However, the reform solely benefited individuals whose parents were born in France, as it did not significantly affect children of immigrants' educational and labor market outcomes.

JEL Classification: I21, I28, J24

Keywords: tracking, returns to education, school quality

*I am grateful to Olivier Deschênes, Peter Kuhn and Heather Royer for their guidance and support. I thank Kelly Bedard, David Card, Pierre Mouganie, Dick Startz, members of the UC Santa Barbara human capital research group and participants at the 2017 SOLE meetings for helpful comments and suggestions. I also thank Jana Kontar for excellent research assistance and staff members at Centre Maurice Halbwachs for providing me with data. All errors are my own.

[†]Department of Economics, American University of Beirut, e-mail: sc24@aub.edu.lb.

1 Introduction

Separating students based on their academic achievement is a common practice. Nonetheless, the type of tracking that students are exposed to varies considerably across educational systems. In some countries such as Germany, students are divided from an early age into tracks that diverge substantially in terms of curricula—i.e., general versus vocational education—and lead to different types of degrees and career options. Other countries like the United States and Canada group students based on their ability into different classrooms (Betts, 2011). Under this more flexible form of tracking, all students follow the same curriculum within the general education system and have the option of pursuing the same type of degrees and career paths.

The issue of tracking is controversial and its predicted impact on students' academic and labor market outcomes is ambiguous. Tracking may be beneficial if it allows students to pursue an education that is adapted to their abilities and needs. On the other hand, 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 track 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 adopted detracking policies. In the 1960s and 70s, the United Kingdom abandoned the practice of separating students into different schools based on academic performance at age 11. Similar reforms were adopted in Finland, Norway, Sweden and France.

This paper studies the effects of a French detracking reform on students' long-run education and labor market outcomes. Prior to the reform and at the beginning of middle school, students aged 11 were separated into either a high or low track based on their academic performance. Students in the high track followed a general education curriculum, which allowed them to eventually obtain general or technical high school degrees and prepared them for higher education. In contrast, low-track students pursued vocational studies which precluded them from accessing postsecondary education. A major issue with the pre-reform tracking system was that students from low socioeconomic backgrounds were disproportionately assigned to the low track. To address this concern, the government implemented a reform aimed at unifying the middle school system. The reform ended the placement of students into high and low tracks at age 11 and introduced a common general education curriculum. It also intended to create mixed-ability classes but in practice, many schools grouped students in achievement-based classrooms. Furthermore, students could still be placed in vocational

studies after two years in middle school. As a result, tracking was not completely abolished but instead its intensity was decreased: the reform effectively delayed the separation of students into general and vocational education by two years, while maintaining the practice of grouping students into classrooms based on their academic performance at age 11.

The reform went into effect in the academic year 1977-78. This implies that individuals born before January 1, 1966 were exposed to the pre-reform tracking system i.e., they were separated into general and vocational tracks at age 11. On the other hand, those born after that date were exposed to the more flexible post-reform tracking system i.e., at age 11, they were grouped into achievement-based classes and then two years later, they were tracked into either general or vocational education. 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-discontinuities (RD-DID) design 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 share of individuals with technical degrees. This implies that students realized educational gains due to the reform, as technical degrees are a higher level of qualification than vocational diplomas in France. I further show that the reform increased wages by 6 percent at ages 40 to 45, but had no significant effect on employment. Both the education and labor market effects are concentrated among individuals from low socioeconomic backgrounds, who were most likely to be placed in the low track prior to the reform. Effects are also stronger for men compared to women. These gains were solely realized by individuals whose parents were born in France; the reform had no significant impacts on children of immigrants' educational and labor market outcomes. Overall, 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.

This paper is related to a literature that examines how different types of tracking affect student outcomes. Previous studies find that grouping students into ability-based classrooms from an early age has no significant impact on academic performance (Betts and Shkolnik, 2000; Figlio and Page, 2002; Zimmer, 2003; Lefgren, 2004). Recent evidence however indicates that ability grouping substantially increases students' test scores (Duflo, Dupas and Kremer, 2011; Cortes and Goodman, 2014; Card and Giuliano, 2016). Other studies focus on educational systems where students are separated into tracks with different curricula and career paths—typically general versus vocational education. Cross-country evidence indicates

that this form of tracking exacerbates socioeconomic inequalities (Hanushek and Wößmann, 2006; Brunello and Checchi, 2007; Schütz, Ursprung and Wößmann, 2008). In contrast, Dustmann, Puhani and Schönberg (2017) show that being assigned to a general versus vocational track at age 10 in Germany has no significant impact on earnings. My paper differs from this literature as it does not focus on one specific form of tracking. Instead, it studies the impacts of moving from a system where students are placed in vocational versus general education at age 11, to a setting where they are grouped into achievement-based classes at age 11 and then tracked into general versus vocational education two years later.

In doing so, this paper adds to a literature that evaluates the consequences of detracking reforms. Policies that delay or abolish tracking have been previously shown to improve educational attainment and test scores (Galindo-Rueda and Vignoles, 2007; Malamud and Pop-Eleches, 2011; Kerr, Pekkarinen and Uusitalo, 2013; Piopiunik, 2014; Lange and von Werder, 2017; Zilic, 2018). However, evidence on how they affect individuals' labor market outcomes 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, Mogstad and Mountjoy (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 two studies 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. On the other hand, the French reform essentially postponed tracking middle school students into general and vocational education by two years. In that sense, my paper is more directly related to a study by Malamud and Pop-Eleches (2010) who examine a policy that delayed the placement of Romanian students into high school vocational and academic tracks by two 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

¹Several 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, Salvanes and Vaage (2010) find that students experience an increase in earnings from reforms that abolished tracking and also raised the compulsory school leaving age in Sweden and Norway. Pekkarinen, Uusitalo and Kerr (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 delaying tracking, since no other policy coincided with the French detracking reform.

at ages 36 to 41. In contrast, I find that the French reform raised 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 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 six—the compulsory school starting 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 and 1976, around 18 percent 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

²The 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.

students could in principle switch to the high track if they had good academic performance, but only about 10 percent 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, most students spent the full duration of their middle school (four years) in the initial track they were assigned to.

Besides the clear disparities in peer ability, tracks offered distinct teachers, curricula and career 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 two-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 studies (henceforth high school). 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. 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—with the general

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

⁴Some 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. However, in practice, due to the shortage of teachers with college degrees, instructors who were supposed to teach in the low track often taught in the high track.

⁵For the low track, the four 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 one lesson to civics.

education path being the most selective and vocational studies being the least. 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 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 two-year trade certificate *Certificat d'Aptitude Professionnel (CAP)*, while high-track students pursued a three-year *Brevet d'Etudes Professionnelles (BEP)*—but still had the option of pursuing a *CAP*.⁶ 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 precluded students from accessing postsecondary studies. 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 and left school without a degree at age 16—the compulsory school leaving age.

2.3 The Haby Reform

The middle school tracks exhibited strong disparities in their pupils’ socioeconomic status. Specifically, 72 percent of students in the low track came from disadvantaged backgrounds. Furthermore, 95.5 percent of students whose fathers were in managerial occupations were placed in the high track versus only 74.8 percent of children whose fathers were manual

⁶Both 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.

workers.⁷ 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 objectives of this law. Haby intended to have a middle school that is common to all students, the *collège unique*. As a result, the reform which went into effect in the academic year 1977-1978, changed several aspects of the middle school system.

First, the law abolished the high and low tracks in middle school. 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 general 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 leaving school without a degree at age 16. Those who were not placed in pre-apprenticeship classes pursued two more common years of general education in middle school before being assigned to different types of studies in high school—i.e., to either general, technical or vocational education. As before, the type of studies that students could pursue in high school was based on their academic performance in middle school.

Second, the reform introduced a new common middle school curriculum. Columns (3) and (5) of Table A1 display 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 one to two-hour decrease in the weekly time allocated to French, mathematics, history-geography-civics, arts and physical education.⁸ On the other hand, time allocated for sciences increased and two 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.

⁷These 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).

⁸In French, the drop in the number of lessons was larger for low-track students (by three hours as opposed to one hour for the high track). High-track students also experienced a one hour decrease in the time allocated to a foreign language, while low-track students did not.

The total number of weekly regular lessons decreased by two to three hours, but the law introduced the “soutien” classes which 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 percent of classes were considered “soutien” in the three years after the reform (Lewis, 1985).

Third, the law was intended to create mixed-ability classes within the newly unified middle school system. However, teachers and principals were strongly opposed to the idea of mixed-ability grouping, and often found ways to circumvent the law and divide students into classrooms based on their abilities. A study conducted by the inspector general of the ministry of education, Jean Binon, concluded that only 45 percent 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.⁹ 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 percent) who take latin is 54.4 percent versus only 12.7 percent of low-skilled students (i.e., the bottom 25 percent).¹⁰

Finally, the reform made several changes in terms of educational inputs. As discussed in section 2.1, prior to the reform, there were two types of teachers: those who taught in the low track and those who taught in the high track. The main difference between them was that teachers in the high track held college degrees, while those in the low track either had two-year postsecondary degrees or on-the-job training. After the reform, instructors who were previously in the high track were assigned to teach the last two years of the common middle school, while teachers who were in the low track took on the first two years.¹¹ The reform was initially met with strong opposition from teachers’ unions. To alleviate this

⁹The 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.

¹⁰High-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).

¹¹In schools that had a shortage of teachers with college degrees, instructors with two-year postsecondary degrees or on-the-job training ended up teaching all four years of middle school after the reform.

opposition, Haby agreed to limit classes to 24 students and to hire more teachers—starting the year 1975.¹²

In practice, the reform did not completely abolish tracking. It eliminated the high and low tracks and allowed all students to follow a common general education for at least the first two years of middle school. However, students were still grouped in classrooms based on their academic achievement. The law itself also allowed tracking to persist given that students could be placed in pre-apprenticeship classes—i.e., vocational studies—after the first two years of middle school. As a result, the reform effectively delayed the age at which students were separated into general versus vocational education by two years (from age 11 to 13), while allowing them to be grouped into ability-based classrooms starting age 11.

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. Almost twice as many women as men are surveyed and so the initial data set includes 238,458 women and 121,312 men.¹³ 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 degree. 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 led to 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

¹²Between 1975 and 1978, the number of teachers with qualifications to teach in the high track increased from 63,600 to 68,800, while the number instructors with qualifications to teach in the low track remained relatively stable (increasing from 82,400 to 83,500) (Chapoulie, 1987).

¹³Women are intentionally over-surveyed in the “Enquête sur la Famille et les Logements” because the main purpose of this survey is to provide data on fertility and family structure.

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. Hence, 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, as those who are not born in France may have gone through a different education system abroad. I also limit the main samples to individuals whose parents are born in France. As I detail in section 5.5, this is because children of immigrants' educational experiences differ from non-immigrants. Nonetheless, I present results for immigrants' children separately and show that my main results are robust to their inclusion in the overall sample. I further drop those who have missing values in the baseline covariates—mainly father's occupation.

3.2 Summary Statistics

Table 1 reports, for different analysis samples, mean values of key outcomes. Column (1) focuses on the main overall sample which includes all individuals who are born within 15 months on either side of the January 1, 1966 cutoff (see section 4.1 for more details). Columns (2) and (3) further show outcomes' means by socioeconomic background, while columns (4) and (5) split the overall sample by gender. I use father's occupation as a proxy for individuals' 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 I consider an individual to be from a low socioeconomic background (low SES) if his/her father is in a low-skilled occupation, and from a higher socioeconomic background (high SES) if his/her father is in a high or middle-skilled occupation.¹⁵

¹⁴These constitute the official socioeconomic classification in France—“Nomenclature des professions et catégories socioprofessionnelles” (PCS)—and are used as reference in all collective agreements.

¹⁵Low-skilled occupations (i.e., *ouvriers* or *agriculteurs exploitants*) include unskilled manual workers, transportation workers such as drivers, industrial workers such as machine operators and mechanics, artisanal workers such as gardeners, butchers, electricians, carpenters, and agricultural workers. Middle-skilled occupations (i.e., *employés* or *artisans and commerçants*) comprise non manual routine tasks workers, clerks,

Panel A of Table 1 shows means for individuals' highest level of education. Prior to the reform, low-track students typically pursued a *CAP* degree or were placed in transitional classes until age 16 after which they left school without a degree. Since the reform abolished the low track, it is possible that it also decreased the likelihood of holding a *CAP* or leaving middle school without a degree. Column (1) indicates that the share of individuals in the overall sample who report holding a *CAP* degree is 20 percent. Furthermore, the share of individuals who completed at least some primary or middle school education but ultimately left school without a degree (i.e., the variable "Some middle school") is 8.8 percent. While this variable comprises individuals who were placed in transitional classes following the low track, one caveat is that it can also include (i) individuals who were placed in the high track but decided to drop out before completing their middle school education and (ii) those who enrolled in primary school but dropped out without a middle school education. Columns (2) and (3) reveal that 12 percent and 25.5 percent of low SES individuals respectively have some middle school education and hold a *CAP* degree, as opposed to only 5.1 and 13.7 percent of high SES individuals. 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. Columns (4) and (5) further show that while an equal share of men and women have some middle school education, men are more likely to hold a *CAP* degree (27.2 versus 16.2 percent).

In my analysis, I also focus on the different degrees that students from the high track could obtain. High-track students who left school after completing their middle school education were typically awarded a middle school degree. 5.1 percent of individuals in the overall sample have a middle school diploma as their highest degree (i.e., the variable "Middle school"), and this number does not vary substantially across socioeconomic background and gender. The least skilled students in the high track typically pursued a secondary school vocational degree (i.e., *BEP*). The share of individuals in the overall sample who have a *BEP* degree is 15.9 percent. A higher share of low SES individuals (18.2 percent) and women (16.4 percent) hold this degree compared to high SES individuals and men, respectively.

The more able students in the high track would pursue technical degrees—i.e., degrees obtained after technical education in high school such as the Technician's Baccalaureate and postsecondary technical degrees. Column (1) indicates that the share of individuals in the overall sample with such degrees is 28 percent. On the other hand, the highest-performing students in the high track typically obtained a general baccalaureate degree or college de-

service workers such as childcare and food preparation workers, hairdressers, cashiers, waitresses, administrative workers such as secretaries, artisans and shopkeepers, etc. High-skilled occupations (i.e., *professions intermédiaires* and *cadres*) comprise doctors, engineers, judges, lawyers, managerial positions, teachers, etc.

gree.¹⁶ The share of individuals in the overall sample with a general baccalaureate as their highest degree is 9 percent, while the share of those who hold a college degrees is 12.9 percent. Interestingly, a larger share of high SES compared to low SES individuals hold technical, general baccalaureate and college degrees—consistent with the fact that students from high socioeconomic backgrounds were disproportionately placed in the high track. Finally, a higher share of women than men hold technical and general baccalaureate degrees, but men complete college at a higher rate.

Panel B of Table 1 reports the employment rate and average monthly wages for individuals who are employed. In the overall sample, the employment rate is 86 percent and the average wage is 1,838 euros per month. High SES individuals (men) earn more and are more likely to be employed than the low SES (women). Finally, Panel C indicates that around 90 percent of individuals were born in France and 80 percent 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 impacts 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 start school in the same academic year at around age six. 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 post-reform educational system. Thus, I can capture the effects 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 reform 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

¹⁶College degree includes all postsecondary academic diplomas from universities and the *Grandes Ecoles*.

starting school at an older age. In section 5.6, I address this issue by showing that (i) 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 and, (ii) RD estimates are consistent with those from a difference-in-discontinuities design.

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

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

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 $g(\cdot)$ captures the relationship between Y and S . Further, I interact $g(\cdot)$ 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. 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 post-reform educational system—that is ITT effects—and not the effect of actually going through it.

In my analysis, I specify $g(\cdot)$ to be a linear function of S and estimate equation (1) over a bandwidth of 15 months on either sides of the cutoff, using a local linear regression with a triangular kernel. The preferred bandwidth is chosen using the procedure introduced by Calonico, Cattaneo and Titiunik (2014).¹⁷ I use robust standard errors since the practice of clustering standard errors by a discrete running variable leads to confidence intervals with worse coverage properties and does not resolve specification bias issues (Kolesár and Rothe, 2018).

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 pre-reform tracking system. However, the policy change was announced almost nine years after the birth of individuals around the cutoff, rendering manipulation

¹⁷Depending 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.

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 slight increase in the number of births at the 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 for whether 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. In Figures 3d and 3e, I further plot the probability that the individual is male and the likelihood that his/her father is in a low-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 percent 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 percent 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 percent in 1976 to 9.6 percent 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 percent in 1976 to 90.4 percent in 1977 to 100 percent in 1978.

In Figure 4c, I plot the evolution of the number of students in different tracks over time for the 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 percent), 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 percent between 1978 and 1979 and to 0 afterwards. The share of high-track students increased from 74 percent in 1978 to 80.8 percent in 1979 and to 86.8 percent in 1980. On the other hand, the share of students in pre-apprenticeship classes remained stable over that time period (13.4 percent in 1978 to 13 percent in 1979 to 13.2 percent 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 Motivating Event Study Analysis

Given the sharp drop in the share of students in the low track after the reform, we expect to see a decrease in the share of students with degrees that were obtained through this track. Low-track students typically pursued a vocational degree (*CAP*) or left middle school without a degree (after being placed in transitional classes). 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). To get a sense of whether the reform effectively changed educational attainment, Figure A2a plots the share of individuals with different types of degrees as a function of their year of

birth. The share of individuals with degrees that are typically accessed through the low track—i.e., *CAP* or leaving middle school without a degree—is generally decreasing over time. It progressively drops from 33 percent to 30.1 percent for cohorts born between 1959 and 1963, but becomes relatively stable until 1965. Interestingly, this share then steeply drops from 30.3 percent for the cohort born in 1965 to 27.6 percent for the treated cohort (i.e., those born in 1966) and continues to decrease for the following birth cohorts.

The figure also plots the evolution of other degrees—which were typically obtained by students pursuing the high track—across birth cohorts. The share of students with technical degrees is also stable at around 27 percent for cohorts born between 1962 to 1965. However, the treated cohort’s share in technical degrees increases to 29.1 percent and keeps rising for later birth cohorts. On the other hand, the shares of individuals with a *BEP*, a general baccalaureate or a college degree are relatively stable for cohorts born between 1965 and 1966. This suggests that the reform decreased the share of individuals with degrees that are accessed through the low track and simultaneously increased their likelihood of obtaining technical degrees.

To further explore these trends, I conduct an event study analysis using cohorts born from 1963 to 1969. Specifically, I regress the main education and labor market outcomes on a series of dummy variables for each birth year, with the year 1965 as the omitted category. Figures A2b to A2e plot estimates of the relationship between main outcomes and birth year dummy variables, along with their 95 percent confidence intervals. This analysis is conducted for the shares of students in different degrees but also for the employment rate and the natural log of wages. The set of pre-1966 estimates are statistically indistinguishable from 0 for all outcomes. However, starting the birth year 1966, there is a steep, persistent and statistically significant drop in the likelihood of leaving middle school without a degree or holding a *CAP* (Figure A2b), coupled with a significant increase in the likelihood of holding technical degrees (Figure A2c). While the likelihood of being employed is not statistically different from 0 for all birth years, the log of wages significantly increases for the cohort born in 1966 (Figure A2e). Taken together, these results suggest that the reform raised the level of education by changing the type of degrees that students pursue, and increased wages in the long run.

5.3 Effects of the Reform on Educational Attainment

As discussed 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. Furthermore, the event studies contained in Figures A2b and A2c

suggest that the reform led to a decrease in the share of students who hold a *CAP* degree or left middle school without a degree, and increased the share of individuals with technical degrees.

To estimate the causal impact of the reform on educational attainment, I next report results from the RD design as detailed in section 4.1. The different panels in Figure 5 plot the highest degree received as a function of the running variable for the overall sample, and column (1) of Table 2 reports the corresponding regression estimates. Figure 5a reveals that the probability of holding a *CAP* degree or leaving middle school without a degree—i.e., low-track students’ usual educational trajectory prior to the reform—exhibits a clear negative shift at the cutoff. The magnitude of this decrease is on the order of 6.1 percentage points (or 20.2 percent).¹⁸ To understand which of these educational levels individuals are more likely to switch away from, column (1) of Table 2 further presents RD estimates for the likelihood of holding *CAP* degrees and leaving middle school without a degree (i.e., the variable “Some middle school”), separately. Results indicate that the reform affects both of these outcomes as it caused a 3.2 and 3 percentage points drop in the likelihood of individuals leaving middle school without a degree and holding a *CAP* degree, respectively.

Figures 5b and 5c reveal that the probability of holding a middle school diploma (i.e., the outcome “Middle school”) or a *BEP* as the highest degree are smooth around the cutoff, and corresponding estimates are small and statistically insignificant. The first row of Table 2 further indicates that the reform has no significant effect on the share of students who received no education. On the other hand, the reform led to a significant 5.9 percentage point (or 22 percent) increase in the share of individuals who hold technical degrees (Figure 5d and column (1) of Table 2). Figures 5e and 5f indicate that the shares of students who hold a baccalaureate degree or a college degree do not change at the reform cutoff and their corresponding estimates are statistically insignificant.

As a robustness check, Table A3 reports RD estimates for main 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 reveal that estimates across varying specifications are comparable to the main results. 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.

Taken together, these results indicate that the reform induced individuals to switch from degrees that are typically accessed through the low track to those that are obtained after pursuing the high track (i.e., technical degrees). They are also consistent with the idea that

¹⁸The percent increase is based off of the control group mean.

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.

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-skilled occupations (low SES). The visual evidence in Figure A3 indicates that the impacts of the reform on the low SES group are similar to those of the overall sample. Low SES individuals are around 8.8 percentage points (or 22.2 percent) less likely to hold a *CAP* or leave middle school without a degree (Figure A3a and column (2) of Table 2). Specifically, the share of low SES individuals who left middle school without a degree decreased by 3.5 percentage points while the share of *CAP* holders dropped by 5.2 percentage points due to the reform. As in the overall sample, this is concurrent with a statistically significant 7.1 percentage points (or 29 percent) increase in the share of students with technical degrees (Figure A3d and column (2) of Table 2), while no significant effects are detected for the probabilities of holding other types of degrees (remaining panels of Figure A3). Figure A4 and column (3) of Table 2 show results for individuals whose fathers are in middle or high-skilled occupations (high SES). The figures do not reveal any visible discontinuities at the cutoff and corresponding estimates are statistically insignificant. However, reduced precision prevents me from making definitive conclusions regarding whether this group gained or lost from the reform.

Columns (4) and (5) of Table 2 and Figures A5 and A6 show heterogeneous effects by gender. The effects of the reform on men are comparable to the overall and low SES samples. Men are 11.4 percentage points (30 percent) less likely to hold a *CAP* or leave middle school without a degree. Specifically, they are 4.1 percentage points less likely to leave middle school without a degree and 7.2 percentage points less likely to hold a *CAP* due to the reform. Their share in technical degrees also increases by 7.2 percentage points (or 31 percent). Women are

also impacted by the reform—but the effects for men are stronger. They are 2.5 percentage points less likely to leave middle school without a degree but the reform does not significantly affect their likelihood of holding a *CAP*. They are also 5.2 percentage points (or 18 percent) more likely to hold technical degrees due to the reform. Finally, Tables A5 to A6 show that results for low and high SES samples as well as men and women are not sensitive to the choice of different bandwidths or to the inclusion of controls and month of birth fixed effects.

5.4 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 event study analysis in section 5.2 suggests that the reform had no significant impact on the employment rate but raised wages. Figures 6a and 6b respectively show the likelihood of being employed and the natural log of wages for the overall sample, as a function of distance to the cutoff. While there is no clear discontinuity in the employment rate, wages visibly increase at the cutoff. The relevant regression discontinuity estimates are shown in column (1) of Table 3. Consistent with the visual evidence, the effect on employment is not statistically significant at conventional levels, while wages increase by 6 log points or 6.1 percent due to the reform. Table A7 shows that these estimates are robust to bandwidth choice, as well as to the inclusion of controls and month of birth fixed effects. As further discussed in section 6, there are several potential explanations for the documented increase in wages, and it is thus not necessarily only driven by the increase in the level of education. 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 percent decrease in the likelihood of being separated into a low and high track.

I next examine whether labor market effects vary by socioeconomic background in Figures A7a to A7d. 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 (columns (2) and (3) of Table 3). On the other hand, low SES individuals' wages exhibits a clear discontinuity at the threshold (Figure A7b). The increase in wages for the low SES sample is on the order of 7.3 log points or 7.5 percent (column (2) of Table 3). The rise in wages is consistent with the documented increase in level of qualification for low SES individuals. For the high SES sample, Figure A7d reveals no clear shift at the cutoff, but

the reduced sample size does not allow me to conclusively rule out large effects on wages (column (3) of Table 3).

Consistent with the documented increase in their level of education, the reform raised men's wages by 7.9 log points or 8.2 percent (Figure A8b and column (4) of Table 3), but had no effect on their employment rate (Figure A8a). Figure A8d and column (5) of Table 3 further suggest that women experience a 6 percent increase in their wages, but the estimate is statistically significant only at the 10 percent level. Finally, Tables A8 and A9 show for the different heterogeneity samples, estimates for labor outcomes taken from regressions that use various bandwidths and include controls and month of birth fixed effects. Estimates from these specifications are similar to the main results.

5.5 Effects on Children of Immigrants

The analysis so far focuses on individuals whose parents are born in France. However, in the French context, individuals whose parents are immigrants are a particularly interesting group and the subjects of national policy discussions. This is mainly because their educational trajectories and experiences differ from non-immigrants' children along several dimensions. Specifically, the French middle school system allows for a non negligible degree of educational segregation based on immigrant status. For example, Felouzis et al. (2005) document that in the Bordeaux school region, 40 percent of immigrants and children of immigrants are concentrated in only 10 percent of middle schools. Additionally, even within the same socioeconomic group, children of immigrants tend to have worse academic outcomes compared to non-immigrants (Vallet and Caille, 1996).

Since children of immigrants are potentially more vulnerable than non-immigrants, an interesting question is whether they also benefited from the Haby reform. Hence, I show the impacts of the reform on the educational attainment of individuals whose parents are not born in France. The different panels in Figure A9 plot their highest degree received as a function of distance of their month-year of birth to the cutoff. All figures are smooth around the cutoff, indicating that the reform did not significantly change immigrant children's educational outcomes. Corresponding RD estimates across different bandwidths are reported in Table A10. Consistent with the visual evidence, all estimates are statistically insignificant but the degree of imprecision, due to reduced sample size, does not allow me to make definitive conclusions. Further, Figures A10a and A10b respectively show immigrant children's likelihood of being employed and natural log of wages as a function of the running variable. Unsurprisingly given the lack of effects on educational attainment, no clear discontinuities for the labor outcomes are visible at the cutoff and the corresponding RD estimates

in Table A11 are mostly statistically insignificant.

As a robustness check, I further show that my main results do not change when I add children of immigrants to the main analysis sample. Tables A12 and A13 respectively report regression discontinuity estimates of the impact of the reform on highest degree received and labor outcomes, for individuals whose parents are born both in and outside of France. Consistent with the main estimates, we see a significant drop in the likelihood of holding a *CAP* or leaving middle school without a degree combined with a 5.4 percentage point increase in the share of individuals with technical degrees. Estimates in Table A13 are also consistent with the main results; the employment rate is not significantly impacted but there is a 4 log point or 4.9 percent increase in wages due to the reform. In Table A14, I further show heterogeneous effects based on socioeconomic background and gender for individuals whose parents are born in and outside of France. The results mirror the ones from the main analysis, indicating that the educational and labor market benefits of the reform were mostly realized by low SES individuals and men.

Taken together, these results suggest that while the reform improved non-immigrants' outcomes, it was not as successful at breaking down educational barriers for children of immigrants. While it is beyond the scope of this paper to identify the exact reasons the reform did not affect the educational level of immigrants' children, I nevertheless provide several potential explanations for this finding. First, some studies suggest that teachers are biased against immigrant students and have a negative perception of schools with a large concentration of immigrants, which could in turn affect track placement (Brinbaum and Kieffer, 2009). Discrimination within the French educational system is a controversial issue and the corresponding evidence is mixed. Nonetheless, at the time of the reform, committees comprised of teachers typically decided on students' tracks and even after the reform, students could still be placed in the low track after two years in middle schools. Hence, if discrimination was pervasive in middle schools and the reform did not change the degree of discrimination, then the potential benefits of the reform could have been counteracted by teachers still placing children in the low track based on their immigrant status.

Second, some studies argue that immigrant parents may have a preference for the low track as it allows their children to obtain vocational education and directly enter the labor market (Boado, 2011). If immigrant parents' preferences are sticky, it is possible that the reform did not decrease their children's share in vocational education because this is indeed their preferred track. Finally, immigrant children typically attend schools with a large concentration of immigrants, and these schools tend to be located in poor areas. It is possible that school resources may play a role in limiting immigrant children's access to the high

track. For example, at the time of the reform, some schools in poor areas did not even offer the option of the high track.

5.6 Robustness Checks

As previously discussed, 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, Devereux and Salvanes, 2011). Nonetheless, I deal with this issue in two manners.

First, I show that there are no visible discontinuities in the main outcomes when using January 1 from years other than 1966 as a fake threshold. The different panels in Figure A11 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. Figure A12 repeats the same exercise but using January 1968 as a fake threshold. These cutoffs correspond to individuals who were born two years before (1964) and two years after (1968) the treated cohort. No visible discontinuities in any of the outcomes are apparent at these fake thresholds. 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-discontinuities (RD-DID) strategy are comparable to the ones from the regression discontinuity design. In this setting, the RD-DID compares outcomes of individuals born within 6 months on either side of the January 1, 1966 cutoff to outcomes of those born within 6 months on either side of January 1 in other years. The RD-DID estimates the discontinuity in outcomes of the treated cohort and differences out any potential discontinuities in the outcomes of untreated cohorts, thereby isolating the impacts of the reform from possible age of school entry effects. I use individuals born within 6 months on either side of January 1 in the years 1964, 1965, 1967 and 1968 as control groups, and estimate the following reduced form equation:

$$Y_i = \beta_0 + \beta_1 R_i + \beta_2 A_i + \beta_3 T_i + \beta_4 R_i * T_i + \beta_5 A_i * R_i + \beta_6 A_i * T_i + \beta_7 R_i * T_i * A_i + v_i \quad (2)$$

where the dependent variable Y represents one of various outcomes for individual i . R is the running variable and it is defined as months relative to January 1. A is a dummy variable that is equal to 1 if the individual is born within 6 months on either side of the reform cutoff (i.e., between July 1965 and June 1966) and 0 otherwise. T is a dummy variable that takes on the value of 1 for individuals born in the months of January to June, and 0 for those born in July to December. β_6 is the coefficient of interest and v_i is the error term. I use robust standard errors.

The different panels in Figure 7 show difference-in-discontinuities graphs for the overall sample's main education and labor outcomes. Specifically, the figures plot differences in the outcome's value between the reform cohort (i.e., individuals born between July 1965 and June 1966) and control cohorts, as function of the distance of month-year of birth from January 1. The visual evidence indicates that RD-DID results are consistent with those from the RD design. In Figure 7a, the share of individuals who left middle school without a degree or hold a *CAP* sharply decreases at the cutoff. This is concurrent with a significant increase in the share of individuals with technical degrees and in the log of wages (Figures 7b and 7f). On the other hand, the shares of individuals with a general baccalaureate or college degrees and the employment rate are smooth around the cutoff.

Tables 4 and 5 report RD-DID estimates from equation (2).¹⁹ The estimates are very close to the ones from RD regressions using a bandwidth of 6 months. For the overall sample, the RD-DID indicates a 7.5 percentage point decrease in the probability of having a *CAP* or leaving middle school without a degree, and a 7.3 percentage point increase in the likelihood of holding a technical degree (column (1) of Table 4). As in the main analysis, these effects are mostly driven by low SES individuals and men (columns (2) and (4) of Table 4).²⁰ Table 5 further shows that the reform has no significant impact on the likelihood of being employed for all samples. However, it led to a significant 9 percent increase in wages for the overall sample (column (1)), and comparable increases for low SES individuals and men (columns (2) and (4)).

¹⁹Tables A15 and A16 further report RD-DID estimates taken from regressions which include controls.

²⁰The estimate for men's likelihood of holding technical degrees is not statistically significant at conventional levels, but I cannot rule out effects that are comparable to the ones from the RD design.

6 Mechanisms

Overall, I find that the reform had long-term benefits as it raised individuals' wages at ages 40 to 45. A natural question that arises is what are the mechanisms through which this effect occurs?

6.1 Quality and Quantity of Education

In section 5.3, I find that the reform allowed individuals to switch from degrees that are typically accessed through the low track—i.e., vocational diplomas or leaving middle school without a degree—towards technical degrees. Hence, a natural explanation for the documented wage gain is that it is at least partially driven by changes in the type of degrees that individuals attain. Specifically, the reform may have implicitly raised both the quality and quantity of education pursued.

In France, technical degrees are a higher level of qualification and are considered to be of higher quality than vocational diplomas. The technical education path has a more academically focused curriculum, admits higher-performing students and is associated with higher wages compared to the vocational track. Using the Labor Force Survey, I can show that there is a large wage premium associated with technical degrees. Panel A of Table A17 reports estimates of the returns to different degrees. These estimates are taken from OLS regressions of the log of wages on dummy variables for different degrees—with no schooling as the omitted category—and a set of controls.²¹ For the overall sample, column (1) reveals that the wage gap between individuals with technical degrees and those with no schooling is 49.3 log points or 63.7 percent. On the other hand, the wage premium for degrees that individuals switched from due to the reform is much lower: there is no statistically significant wage difference between those who left middle school without a degree and those who have no schooling, while vocational diplomas yield a 17 percent wage premium. This implies that the wage gap between technical degrees and vocational diplomas or leaving middle school without a degree is about 34.4 percent ($p\text{-value}=0.00$)—which is larger than the reduced form effect of the reform on wages of 6 percent.

At the same time, technical degrees require more years of education to complete than vocational diplomas or middle school, suggesting that part of the wage gain from the reform is driven by an increase in years of education. Figure A13a plots individuals' number of years of education as a function of the running variable.²² The figure reveals a significant positive

²¹Controls include age, quarter and year of survey fixed effects and dummy variables for whether the father is in a low-skilled occupation (except in columns (2) and (3)) and for whether the individual is male (except in columns (4) and (5)).

²²I approximate each individual's years of education based on the number of years that are typically

shift at the cutoff. Panel B of Table A17 further reports RD and RD-DID estimates of the effect of the reform on years of education. For the overall sample in column (1), the reform led to a significant 0.329 increase in years of education. Consistent with the main results, this effect is concentrated among low SES individuals and men who experience a 0.369 (column (2)) and 0.509 (column (3)) increase in their years of education, respectively—while no statistically significant effects are detected for the high SES and women. Assuming that the wage effect of the reform is entirely driven by the increase in years of education, I can calculate an implied wage premium from one year of education due to the reform of about 18 percent in the overall sample (IV estimate=0.06/0.329). As a benchmark, the OLS estimate of the returns to one additional year of schooling in France is smaller at around 7 percent (Gurgand and Maurin, 2007). However, the IV estimate should be viewed as an upper bound to the returns to quantity of education since other factors—such as changes in education quality—may drive the reduced form effect on wages.

The change in the level of education may affect wages if technical degrees are more valuable for employers (signaling) and/or allow individuals to accumulate more human capital than vocational diplomas. This could in turn raise productivity and/or change the type of jobs that individuals hold. As is typical in most studies, I do not have a good measure of productivity. However, the Labor Force Survey allows me to check whether the reform impacted individuals' occupational choice. Panels (b) through (d) of Figure A13 plot the likelihood of holding different types of jobs as a function of the running variable. For the overall sample, Figure A13b reveals a drop at the cutoff in the probability of being an unskilled manual worker. On the other hand, there is an increase in the likelihood of being in a low or middle-skilled occupation in Figure A13c, with no significant changes in other types of occupations (Figure A13d). Corresponding RD estimates in column (1) of Table A18 show that the decrease in unskilled occupations is on the order of 2.8 percentage points and is concurrent with a significant 4.6 percentage points increase in the share of low and middle-skilled workers. Columns (2) through (5) further reveal that these impacts are strongest for the low SES sample and men. Estimates are mostly statistically insignificant for the high SES sample and women—except for a significant decrease in unskilled workers for the high SES.

6.2 Other Channels

Besides the increase in the quality and quantity of education, several other channels can potentially explain why the reform affected student outcomes. First, it is possible that

required to complete his/her degree.

students benefited from changes in the quality of school inputs. As discussed in section 2.3, the reform altered the content of the curriculum and limited classes to 24 students. It further changed the quality of teachers that students are exposed to. Specifically, prior to the reform, students in the high track were taught by teachers who had a higher level of qualifications than those in the low track. After the reform, high-track teachers were mainly assigned to teach the last two years of middle school, while low-track instructors took on the first two years. As a result, students who would have been previously assigned to the low track are now potentially exposed to teachers with a higher level of qualifications. Previous evidence indicates that smaller class sizes and higher teacher quality increase wages in the long run (Fredriksson, Öckert and Oosterbeek, 2012; Chetty, Friedman and Rockoff, 2014) and that curriculum content and instruction time are strongly correlated with student achievement (Altonji, Blom and Meghir, 2012).

The reform also led to the hiring of new teachers. However, this is unlikely to explain my main results since this process started at the end of 1975 and not in the year the policy went into effect (i.e., the academic year 1977-78). The first cohort that was affected by the hiring of new teachers was the one born in 1964. If teacher hiring had significant impacts on students' education and labor outcomes, I should be able to capture those effects by comparing individuals on either sides of the January 1, 1964 birth cutoff. In the different panels of Figure A11, I show that there are no discontinuities in my main outcomes when using January 1, 1964 as a fake cutoff, suggesting that teacher hiring does not explain my results.

Second, the creation of mixed-ability classes meant that students should have been exposed to more heterogeneous peers after the reform. As discussed in section 2.3, in practice, many schools still grouped students based on their achievements and by 1979-1980, only 45 percent of classes were mixed ability. Nonetheless, this still suggests that a non-negligible portion of students, who would have been previously placed in the low track, are now interacting with higher achieving peers. Exposure to high achieving peers could thus contribute to the documented increase in educational attainment and wages.²³

Finally, under the pre-reform tracking system, some students may have been mismatched to their middle school tracks. Specifically, it is possible that some high-ability students—who should have been assigned to the high track—were instead placed in the low track (i.e., they were “undermatched”). This is likely in this case since students were divided into tracks at an early age when information about abilities is still noisy and incomplete, and since track placement was not just based on academic achievement but was also highly correlated

²³A recent study by Carrell, Hoekstra and Kuka (2018) finds that exposure to one disruptive peer in U.S. elementary schools decreases earnings by 3 percent at ages 24 to 28.

with socioeconomic background. Being undermatched might negatively affect learning since students in the low track were taught in a way that was intended to cater to low-ability students (i.e., slower pace of instruction, easier curriculum, etc.). In other word, students who were potentially mismatched were not offered an education that is tailored to their abilities and needs, and were locked into tracks that led to a lower level of qualification than what they could have attained. By delaying the age at which students were placed in vocational versus general education, the reform could have benefited students through reducing their chances of being undermatched—especially for low SES students who are driving most of the education and labor market results.

While it is beyond the scope of this paper to pinpoint the exact channel driving the main effects, the documented wage gains due to the reform are likely driven by a combination of many factors. Specifically, they can be mainly attributed to increases in the quality and quantity of education, improvements in the quality of school inputs, exposure to better peers and a decrease in the mismatch between students and tracks.

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 different types of education—typically with a vocational and academic emphasis—and career options. Schools in the United States generally have in place a more flexible form of tracking where students are divided into achievement-based classes, but no significant disparities exist between classes in terms of curricula or future career paths.

This paper examines the impacts of a French reform aimed at delaying the placement of students into general versus vocational studies. Prior to the reform, students were allocated to these different educational paths at age 11. The reform postponed this process by two years. However, in practice, schools started grouping students into achievement-based classrooms from age 11. Using a regression discontinuity design, I find that the reform increased the overall level of education and long-run wages. The strongest effects were among individuals from low socioeconomic backgrounds, albeit children of immigrants did not benefit from the reform.

Perhaps the biggest difference between grouping students in ability-based classrooms and the pre-reform tracking system 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 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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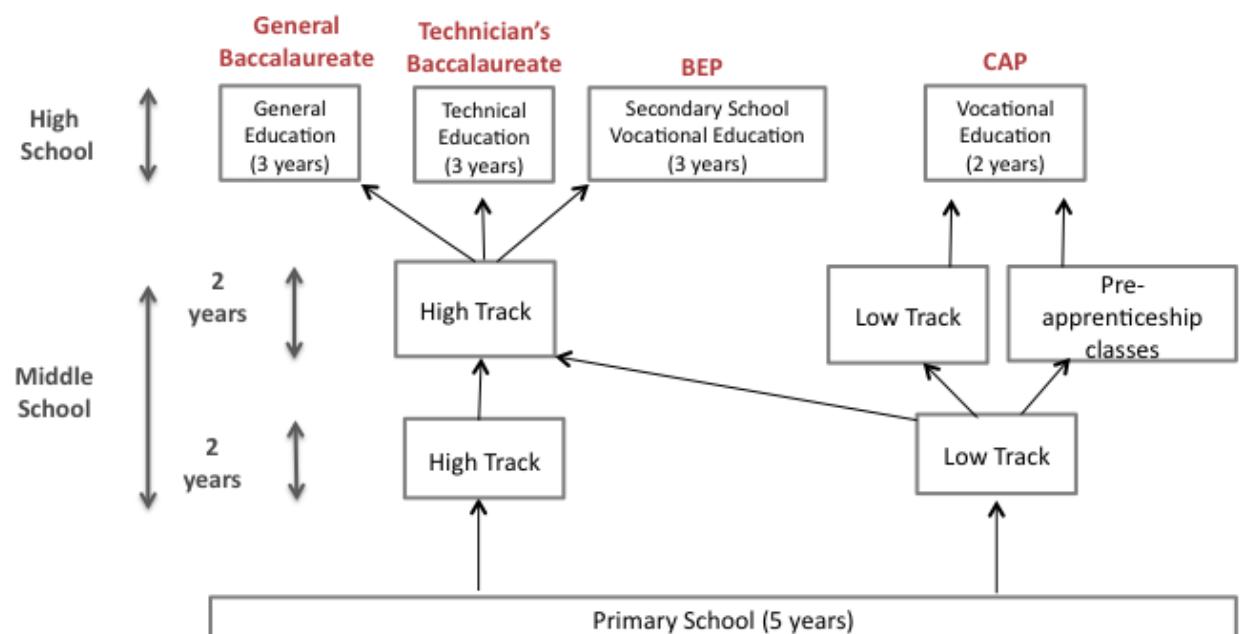
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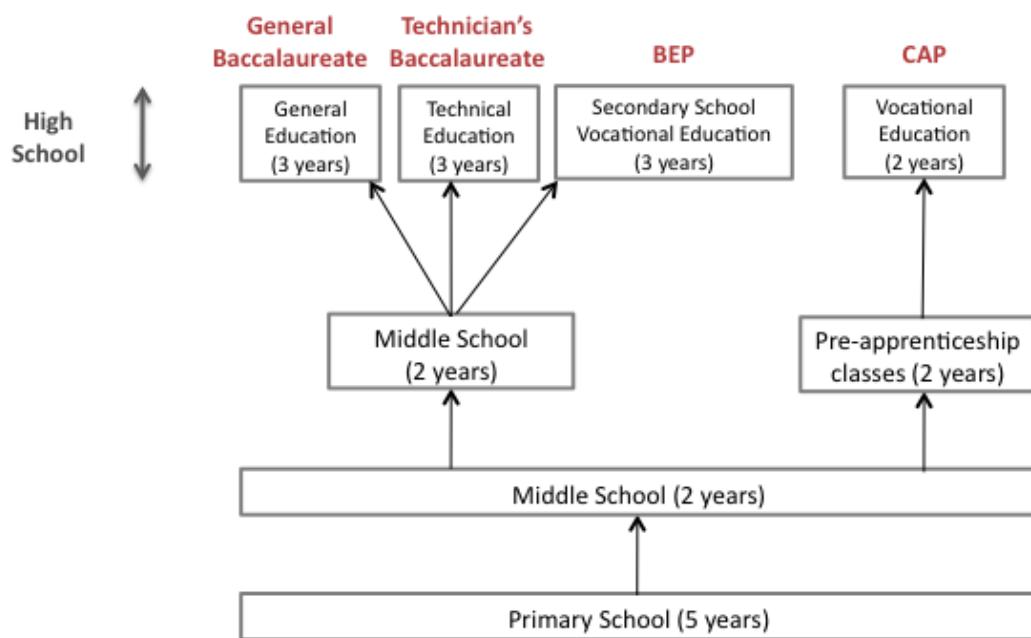
A Figures and Tables

Figure 1: Educational system prior to the reform



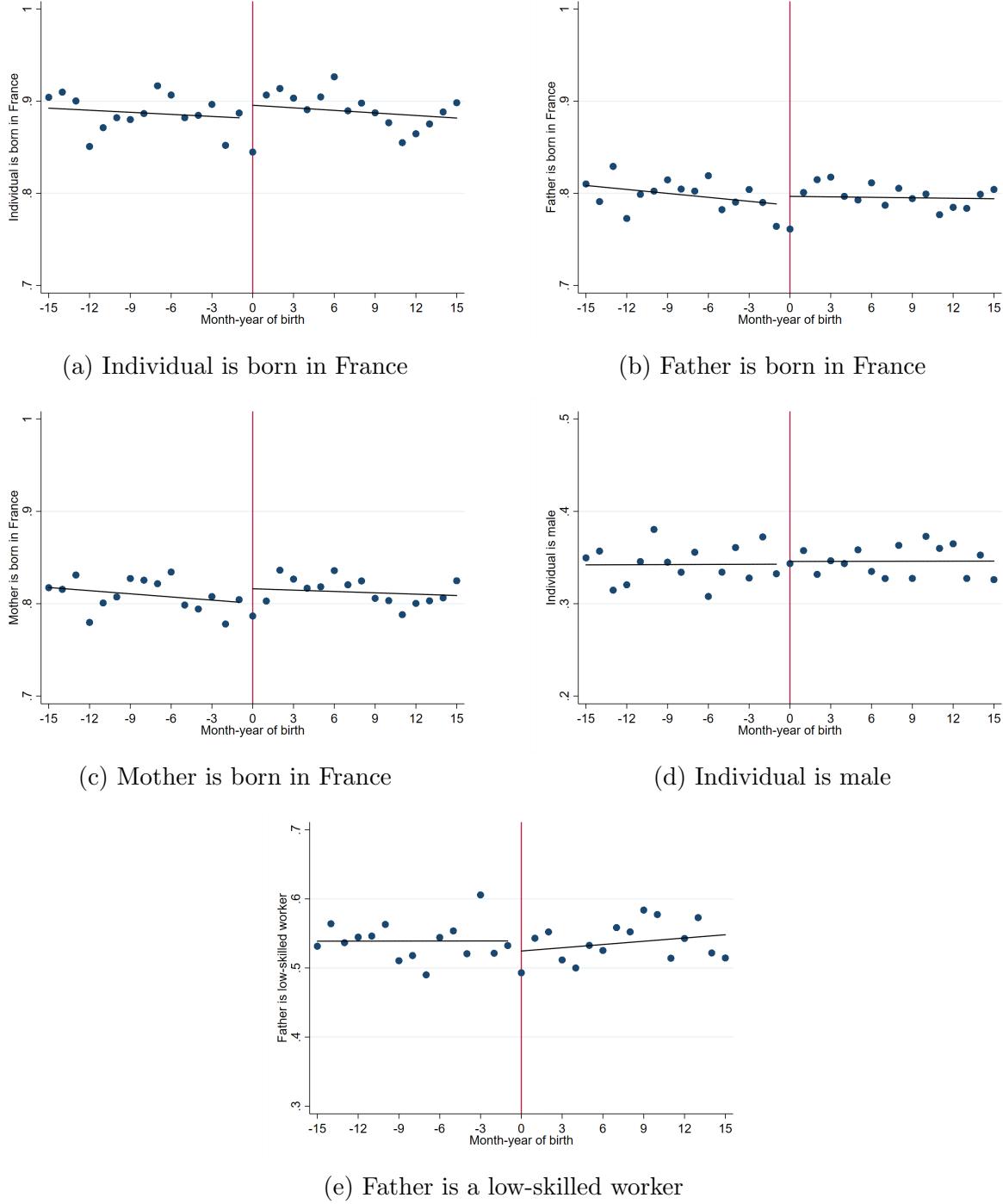
Notes: This figure shows the structure of the French school system prior to the Haby reform. The degrees that students can obtain at the end of high school are denoted in red.

Figure 2: Educational system after the reform



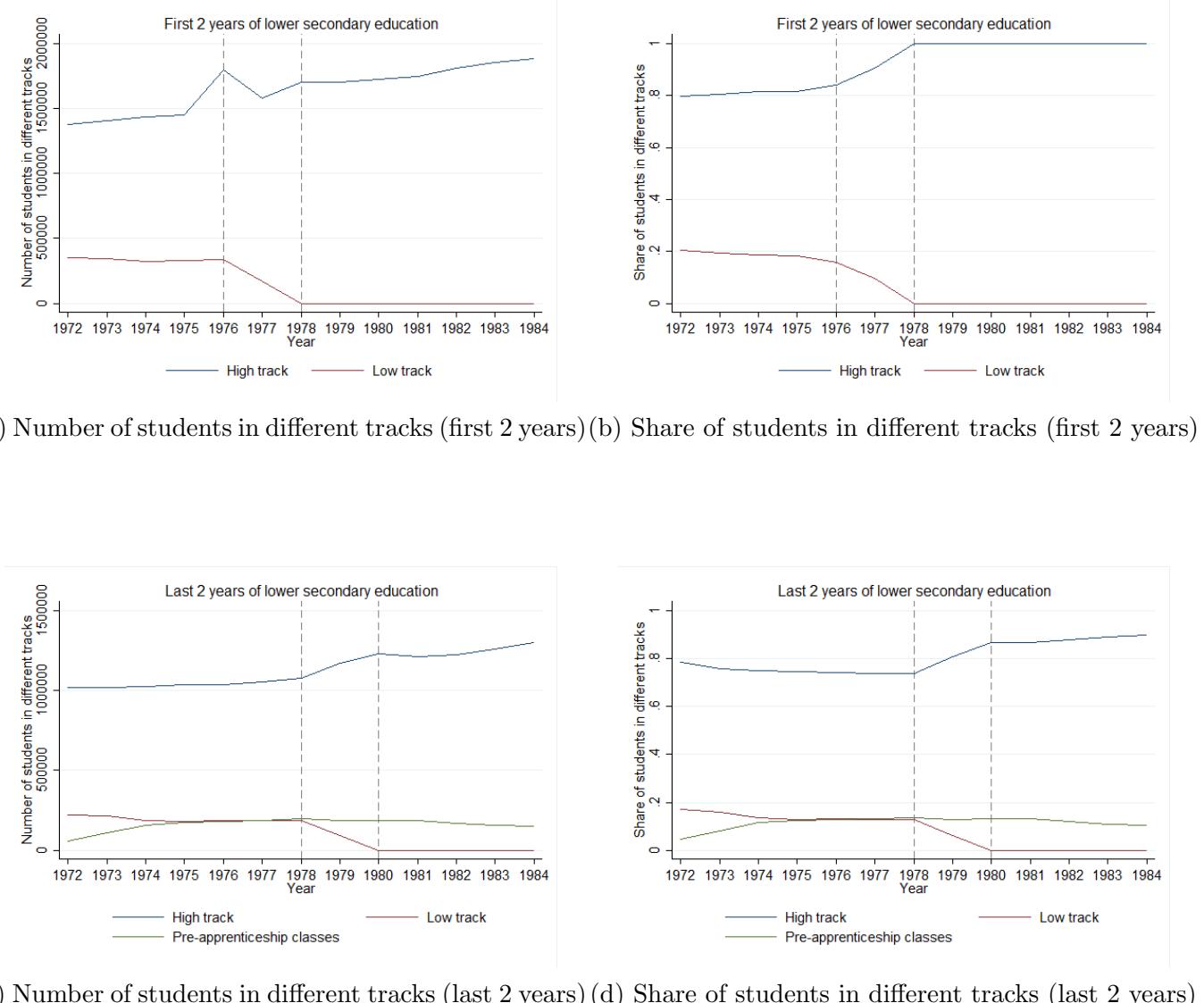
Notes: This figure shows the structure of the French school system after the Haby reform. The reform did not change the organization of primary schools and high schools or the compulsory school leaving age. The degrees that students can obtain at the end of high school are denoted in red.

Figure 3: Covariate Balance



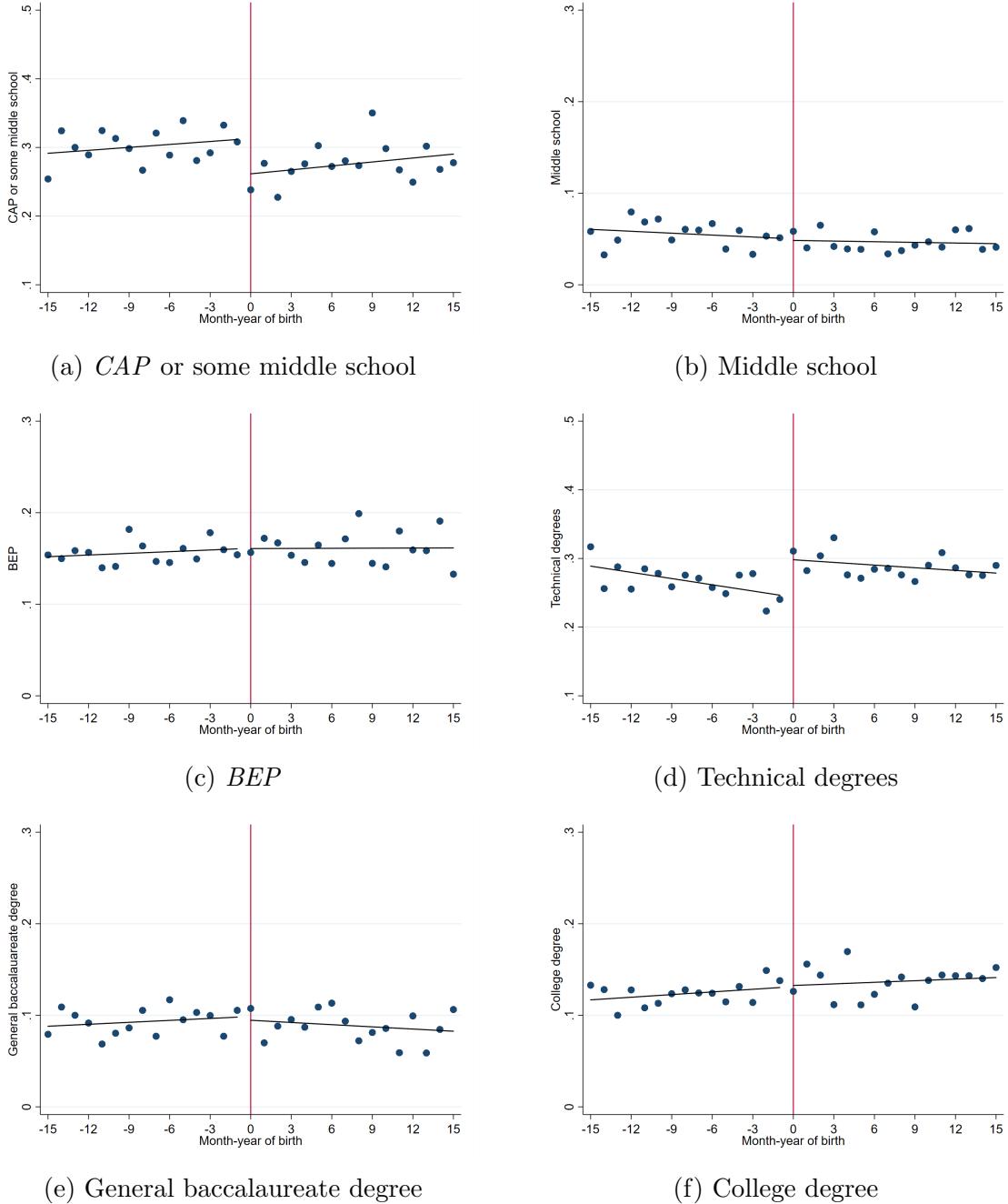
Notes: The different panels show various baseline covariates—i.e., dummy variables for whether an individual is born in France, his/her parents are born in France, is male and his/her father is in a low-skilled occupation—as a function of the distance of individuals' month-year of birth from the cutoff. 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 sample in panels (a) to (c) include all individuals born within 15 months on either side of the cutoff. Panels (d) and (e) restrict this sample to individuals who are born in France and whose parents are born in France. Data are taken from the survey “Enquête sur la Famille et les logements”.

Figure 4: Number and Share of Students Enrolled in Different Tracks



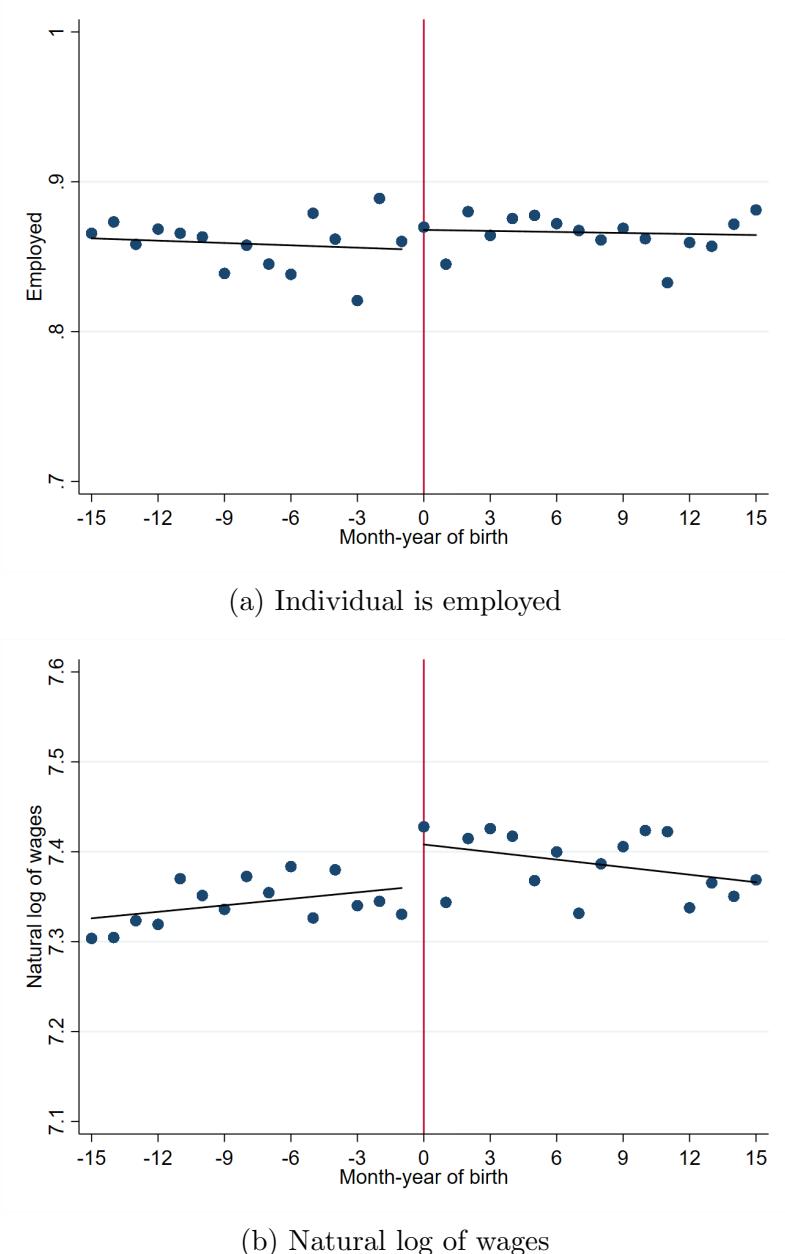
Notes: The different figures plot the evolution over time in the number and share of students in different tracks. Panels (a) and (b) correspond to students who are enrolled in the first two years of middle school. Panels (c) and (d) are students who are enrolled in the last two years of middle school.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

Figure 5: Effects of the Reform on Highest Degree Received



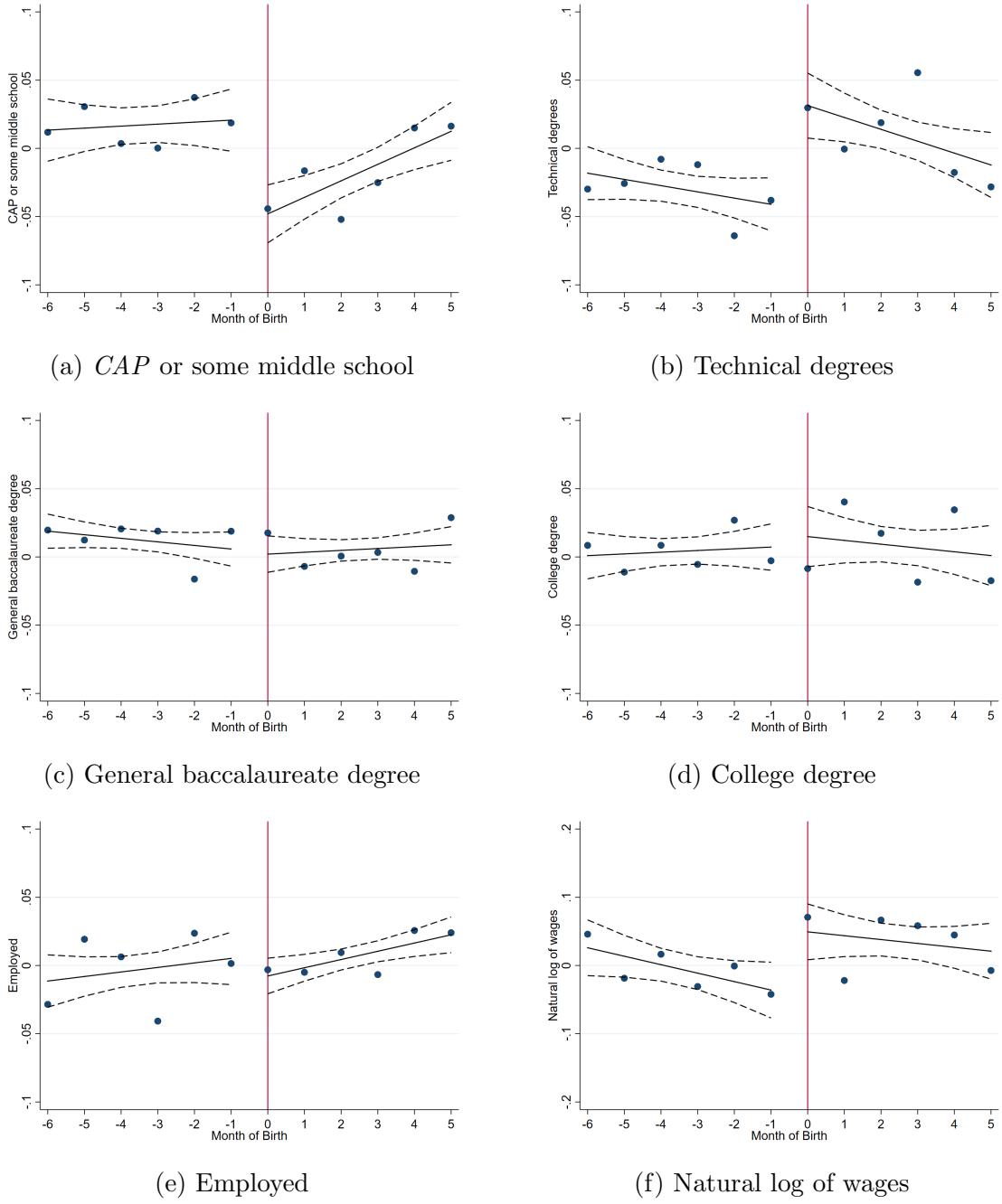
Notes: The different panels show the shares of individuals who hold different types of degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include all individuals who are born in France and whose parents are born in France. Data are taken from the survey "Enquête sur la Famille et les logements".

Figure 6: Effects of the Reform on Labor Market Outcomes



Notes: The different panels show labor market outcomes, as a function of the distance of individuals' month-year of birth from the cutoff. Panel (a) plots the likelihood of being employed and panel (b) plots the log of wages. 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. Samples include all individuals who are born in France, whose parents are born in France, and who are aged between 40 and 45 at the time of the survey. Data are taken from the French labor force survey.

Figure 7: Effects of the Reform on Main Education and Labor Outcomes, RD-DID



Notes: The different panels plot the difference between the treated cohort's outcome value and each control cohort's outcome value, as a function of the distance of individuals' month of birth from January. Circles represent the average over a one month range. The dashed lines are 95 percent confidence intervals. The treated cohort is the one born within 6 months on either side of January 1, 1966. Control cohorts include individuals who are born within 6 months on either side of January 1 in the years 1964, 1965, 1967 and 1968. In all panels, samples are restricted to individuals who are born in France and whose parents are born in France. Panels (a) to (d) plot the likelihood of holding different types of degrees and data are taken from the survey "Enquête sur la Famille et les logements". Panel (e) plots the likelihood of being employed and Panel (f) plots the log of wages. In these panels, samples are restricted to individuals aged 40 to 45 at the time of the survey and data are taken from the French labor force survey. 40

Table 1: Sample Means for Key Variables

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
A) Highest Degree Received					
Some middle school	0.088	0.120	0.051	0.088	0.089
<i>CAP</i>	0.200	0.255	0.137	0.272	0.162
Middle school	0.051	0.055	0.047	0.041	0.057
<i>BEP</i>	0.159	0.182	0.133	0.152	0.164
Technical degrees	0.278	0.257	0.303	0.246	0.295
General baccalaureate degree	0.090	0.072	0.112	0.060	0.106
College degree	0.129	0.057	0.214	0.139	0.124
<i>N</i>	12,240	6,589	5,651	4,221	8,019
B) Labor Market Outcomes					
Is employed	0.861	0.856	0.871	0.913	0.814
<i>N</i>	21,771	11,242	10,529	10,561	11,210
Monthly wages (in euros)	1,838	1,613	2,083	2,154	1,534
<i>N</i>	14,450	7,539	6,911	7,081	7,369
C) Demographic Characteristics					
Individual is born in France	0.888	0.887	0.888	0.890	0.886
Father is born in France	0.797	0.787	0.808	0.790	0.800
Mother is born in France	0.811	0.806	0.817	0.807	0.812
<i>N</i>	16,007	8,600	7,407	5,567	10,440

Notes: This table reports mean values for key variables. Column 1 includes all individuals observed within 15 months on either side of the January 1, 1966 cutoff. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. Samples in Panels A and B are restricted to individuals who are born in France and whose parents are born in France. Panel A shows the shares of individuals with different types of degrees. Education outcomes and demographic characteristics are taken from the survey “Enquête sur la Famille et les Logements”. Labor market outcomes are extracted from the French labor force survey.

Table 2: Effects of the Reform on Highest Degree Received, RD Estimates

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
No schooling	0.000 (0.002)	-0.000 (0.003)	0.001 (0.004)	0.001 (0.004)	-0.000 (0.003)
<i>CAP</i> or some middle school	-0.061*** (0.018)	-0.088*** (0.027)	-0.016 (0.023)	-0.114*** (0.033)	-0.031 (0.022)
Some middle school	-0.032*** (0.012)	-0.035* (0.018)	-0.022 (0.014)	-0.041** (0.020)	-0.025* (0.015)
<i>CAP</i>	-0.030* (0.016)	-0.052** (0.024)	0.006 (0.020)	-0.072** (0.030)	-0.002 (0.018)
Middle school	0.006 (0.009)	0.013 (0.013)	-0.001 (0.012)	0.000 (0.014)	0.009 (0.011)
<i>BEP</i>	-0.001 (0.015)	0.004 (0.021)	-0.006 (0.020)	0.007 (0.025)	-0.006 (0.018)
Technical degrees	0.059*** (0.018)	0.071** (0.024)	0.043 (0.027)	0.071** (0.030)	0.052** (0.022)
Baccalaureate degree	-0.004 (0.012)	0.006 (0.014)	-0.017 (0.019)	0.019 (0.016)	-0.015 (0.016)
College degree	0.000 (0.014)	-0.006 (0.013)	-0.004 (0.025)	0.015 (0.024)	-0.009 (0.017)
<i>N</i>	12,240	6,589	5,651	4,221	8,019

Notes: Each cell reports the reduced form estimate of the impact of the reform on the corresponding outcome (i.e., the likelihood of holding different types of degrees). Estimates are taken from separate local linear RD regressions using a bandwidth of 15 months and a triangular kernel. Column 1 includes all individuals born within 15 months on either side of the January 1, 1966 cutoff. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. All samples are restricted to individuals who are born in France and whose parents are born in France. Data are taken from the survey “Enquête sur la Famille et les Logements”. Robust standard errors are reported in parentheses. (***) $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Table 3: Effects of the Reform on Labor Market Outcomes, RD Estimates

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
Employed	0.009 (0.010)	-0.004 (0.015)	0.023 (0.015)	0.000 (0.012)	0.023 (0.017)
<i>N</i>	21,771	11,242	10,529	10,561	11,210
Natural log of wages	0.060*** (0.022)	0.073** (0.029)	0.043 (0.031)	0.079*** (0.025)	0.059* (0.033)
<i>N</i>	14,450	7,539	6,911	7,081	7,369

Notes: Each cell reports the reduced form estimate of the impact of the reform on the corresponding outcome (i.e., the likelihood of being employed and the log of wages). Estimates are taken from separate local linear RD regressions using a bandwidth of 15 months and a triangular kernel. Column 1 includes all individuals born within 15 months on either side of the January 1, 1966 cutoff. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. All samples are restricted to individuals who are born in France, whose parents are born in France and who are aged between 40 and 45 at the time of the survey. Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (***) $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Table 4: Effects of the Reform on Highest Degree Received, RD-DID Estimates

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
No schooling	-0.002 (0.003)	-0.006 (0.004)	0.002 (0.005)	-0.001 (0.005)	-0.003 (0.004)
<i>CAP</i> or some middle school	-0.075*** (0.028)	-0.117*** (0.041)	-0.023 (0.035)	-0.176*** (0.050)	-0.020 (0.033)
Some middle school	-0.045** (0.018)	-0.040 (0.028)	-0.049** (0.021)	-0.061** (0.030)	-0.036* (0.022)
<i>CAP</i>	-0.030 (0.024)	-0.077** (0.037)	0.026 (0.030)	-0.115** (0.047)	0.016 (0.027)
Middle school	0.006 (0.014)	0.013 (0.019)	-0.000 (0.019)	0.008 (0.021)	0.006 (0.017)
<i>BEP</i>	0.003 (0.022)	0.008 (0.032)	-0.003 (0.031)	0.016 (0.037)	-0.004 (0.028)
Technical degrees	0.073*** (0.027)	0.080** (0.036)	0.064 (0.040)	0.058 (0.045)	0.078** (0.034)
Baccalaureate degree	-0.000 (0.018)	0.018 (0.023)	-0.022 (0.029)	0.038 (0.025)	-0.021 (0.024)
College degree	-0.004 (0.021)	0.004 (0.020)	-0.018 (0.037)	0.058 (0.036)	-0.036 (0.026)
<i>N</i>	24,593	13,297	11,296	8,490	16,103

Notes: Each cell reports the reduced form estimate of the impact of the reform on the likelihood of holding different types of degrees. Estimates are taken from separate RD-DID regressions (equation (2)). Column 1 includes all individuals born within 6 months on either side of January 1 in the years 1964 to 1968. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. All samples are restricted to individuals who are born in France and whose parents are born in France. Data are taken from the survey “Enquête sur la Famille et les Logements”. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

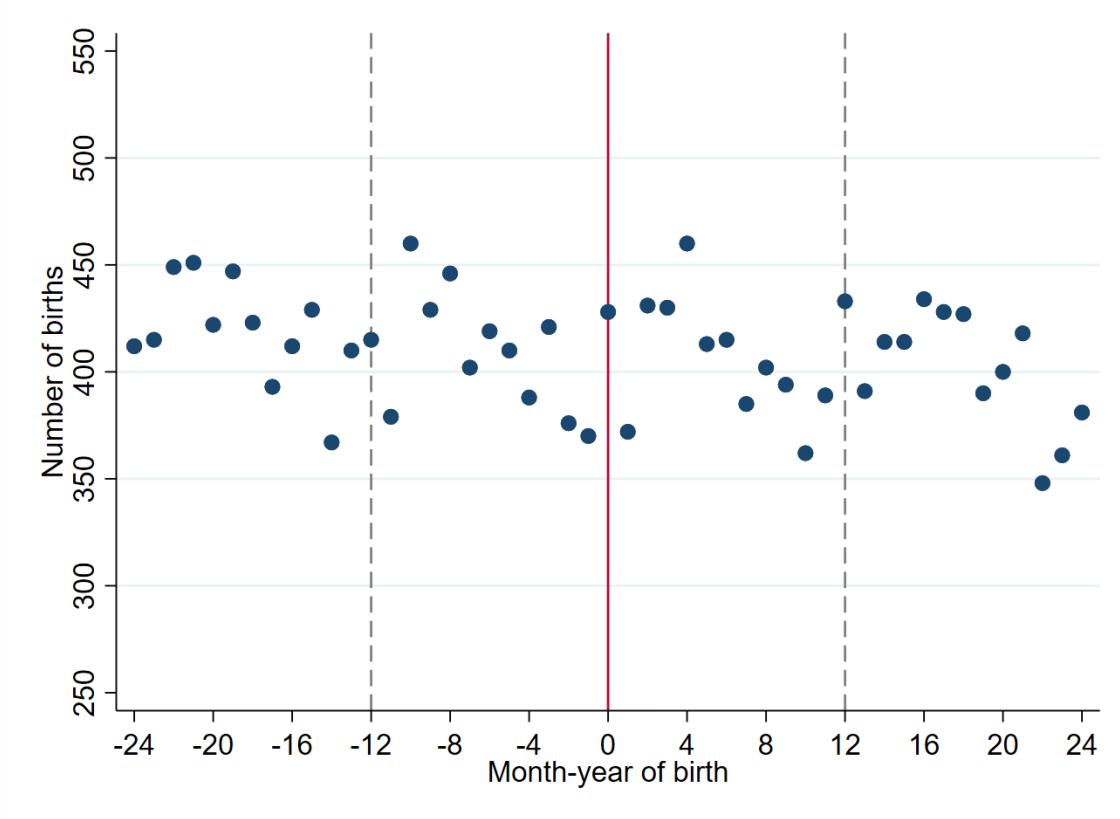
Table 5: Effects of the Reform on Labor Market Outcomes, RD-DID Estimates

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
Employed	-0.006 (0.016)	-0.025 (0.022)	0.015 (0.022)	-0.027 (0.018)	0.019 (0.025)
<i>N</i>	43,579	22,497	21,082	21,030	22,549
Natural log of wages	0.087*** (0.033)	0.107** (0.044)	0.060 (0.047)	0.113*** (0.038)	0.079 (0.049)
<i>N</i>	28,834	15,023	13,811	14,043	14,791

Notes: Each cell reports the reduced form estimate of the impact of the reform on the likelihood of being employed and the log of wages. Estimates are taken from separate RD-DID regressions (equation (2)). Column 1 includes all individuals born within 6 months on either side of January 1 in the years 1964 to 1968. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. All samples are restricted to individuals who are born in France, whose parents are born in France and who are aged between 40 and 45 at the time of the survey. Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (*** p < 0.01 ** p < 0.05 * p < 0.1).

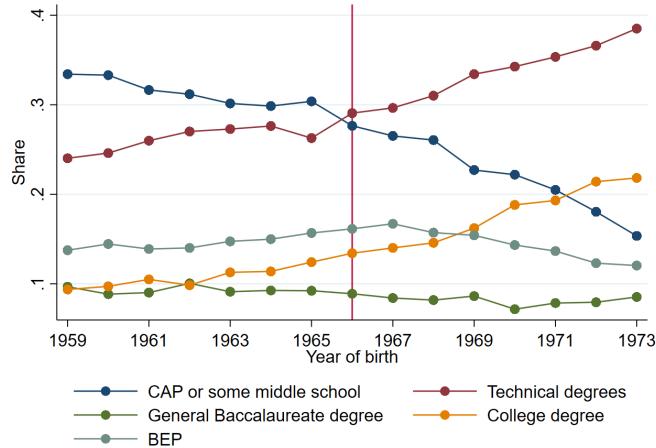
B Online Appendix Figures and Tables

Figure A1: Frequency of Births

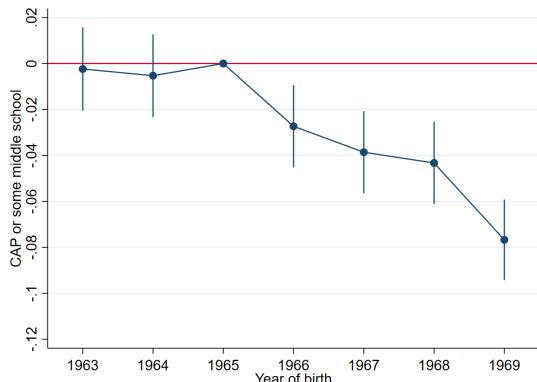


Notes: This figure shows the number of individuals born in each month-year around the January 1, 1966 cutoff (red vertical line). The dashed vertical lines mark births in January of the years before and after the treated year (i.e., 1965 and 1967). Data are taken from the “Enquête sur la Famille et les Logements”.

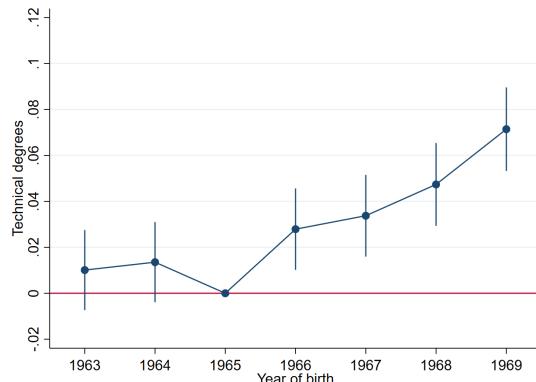
Figure A2: Event Study Graphs



(a) Share of individuals with different degrees



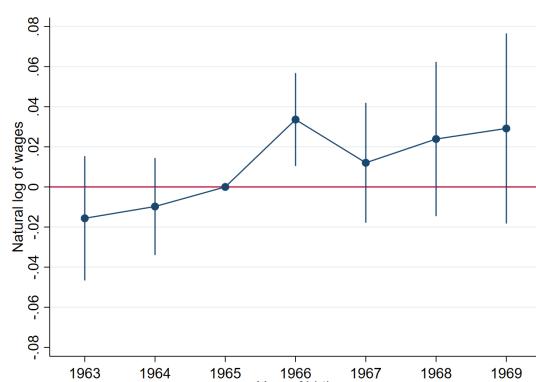
(b) CAP or some middle school



(c) Technical degrees



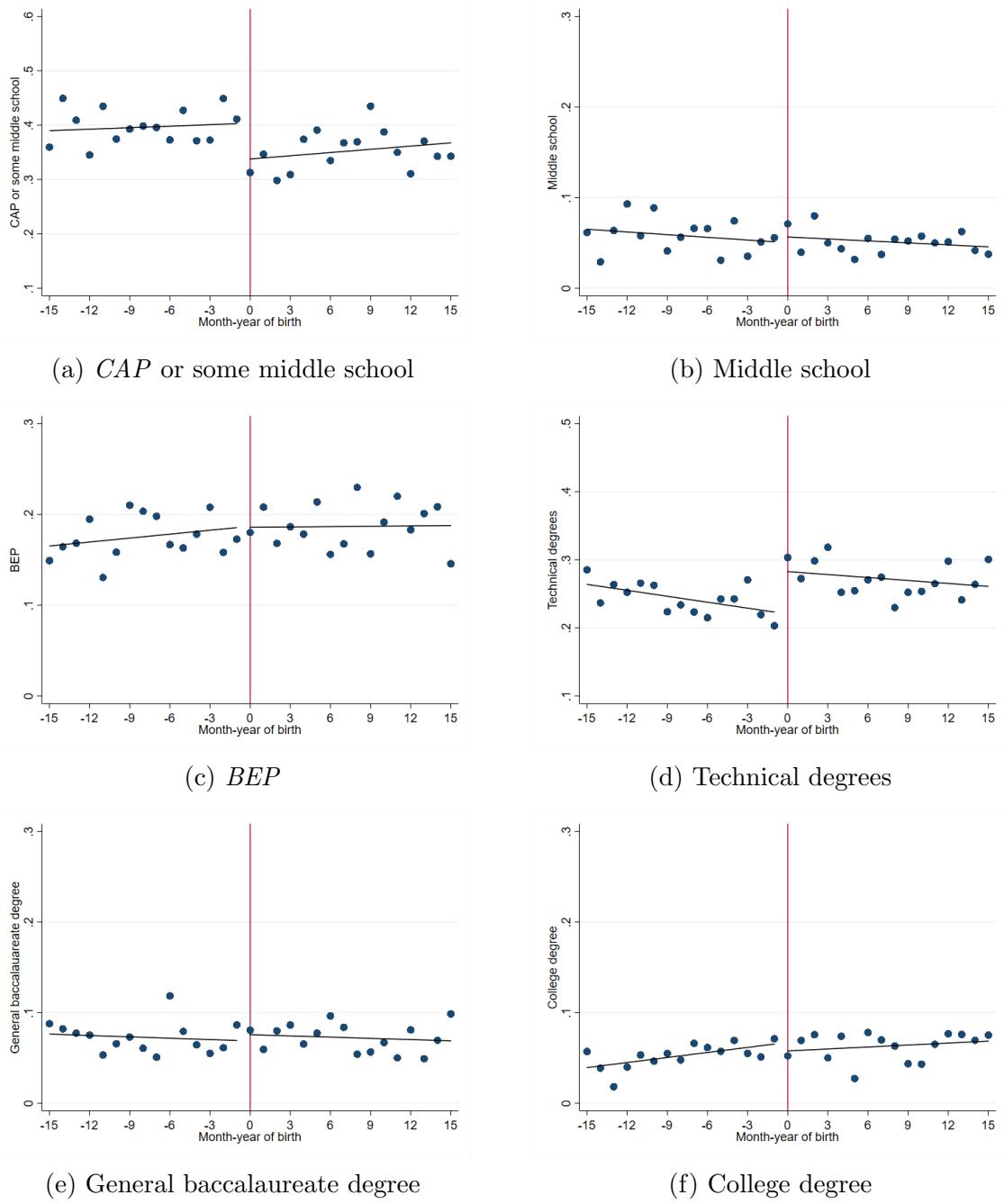
(d) Employed



(e) Natural log of wages

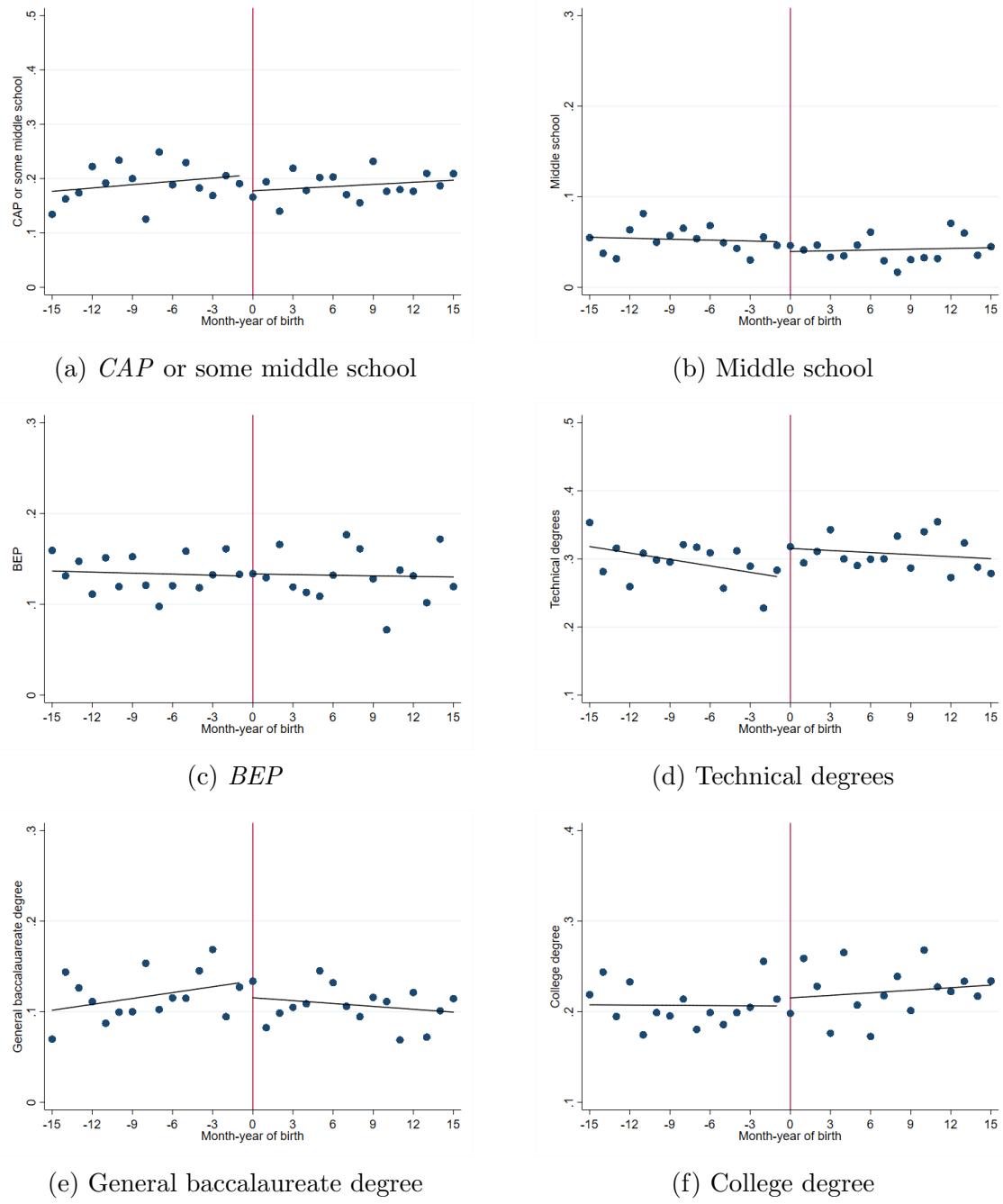
Notes: Panel (a) plots the evolution over birth cohorts in the share of individuals who hold different types of degrees. Panels (b) to (e) plot estimates from an event study analysis in which each outcome is regressed on dummy variables for each birth year—with 1965 as the omitted year—along with their 95 percent confidence intervals. Outcomes in Panels (b) and (c) are the likelihood of holding different types degrees. Panels (d) and (e) respectively show the likelihood of being employed and the log of wages, and corresponding regressions include year of survey and age fixed effects. The samples in all panels include individuals who are born in France and whose parents are born in France. Data in panels (a) to (c) are taken from “Enquête sur la Famille et les Logements”. Samples in panels (d) and (e) are taken from the labor force survey and are restricted to individuals aged 40 to 45 at the time of the survey.
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Figure A3: Effects of the Reform on Low SES Sample's Highest Degree Received



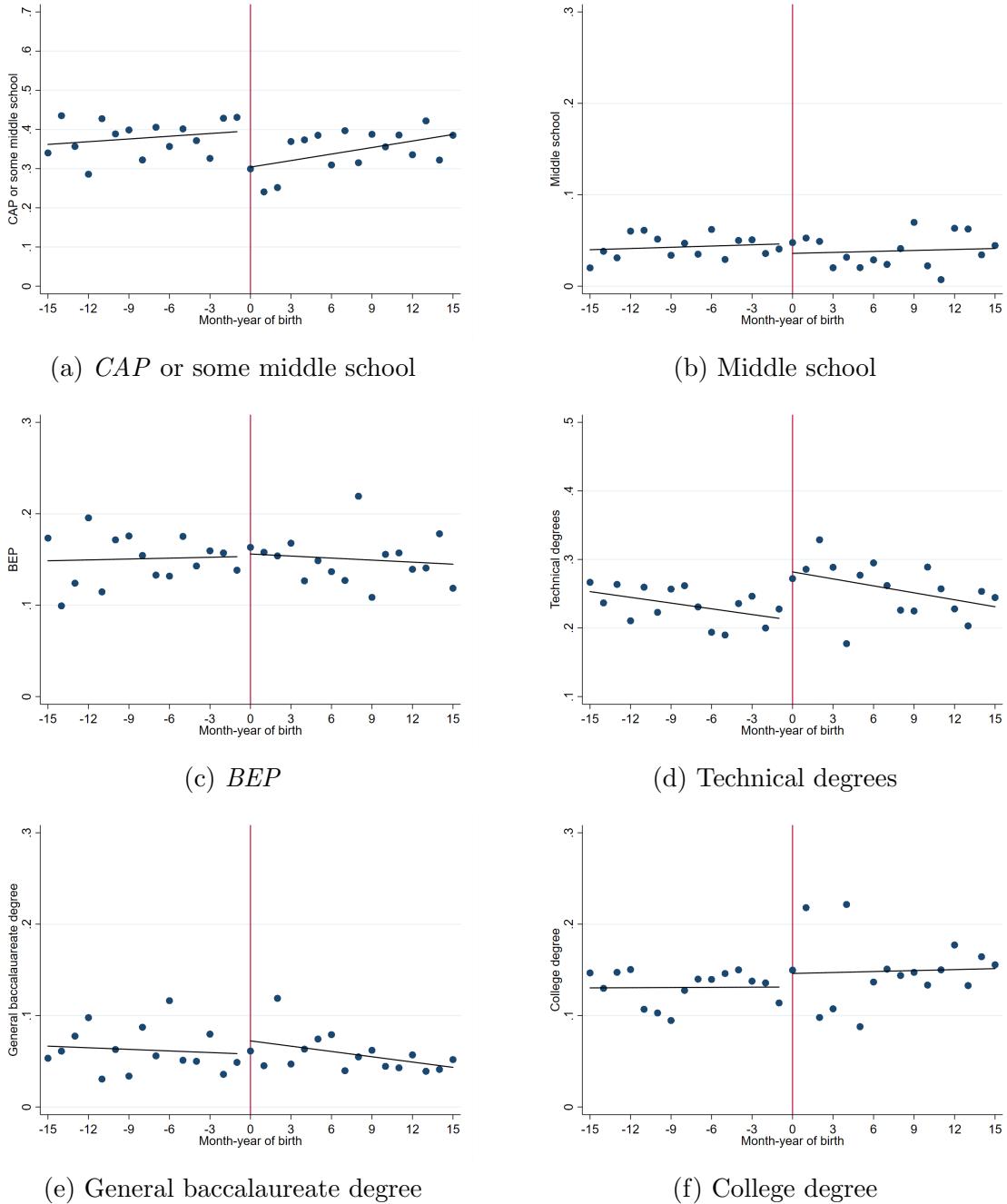
Notes: The different panels show the shares of individuals who hold different types of degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include all individuals who are born in France, whose parents are born in France and whose fathers are in low-skilled occupations (i.e., the low SES sample). Data are taken from the survey "Enquête sur la Famille et les logements".

Figure A4: Effects of the Reform on High SES Sample's Highest Degree Received



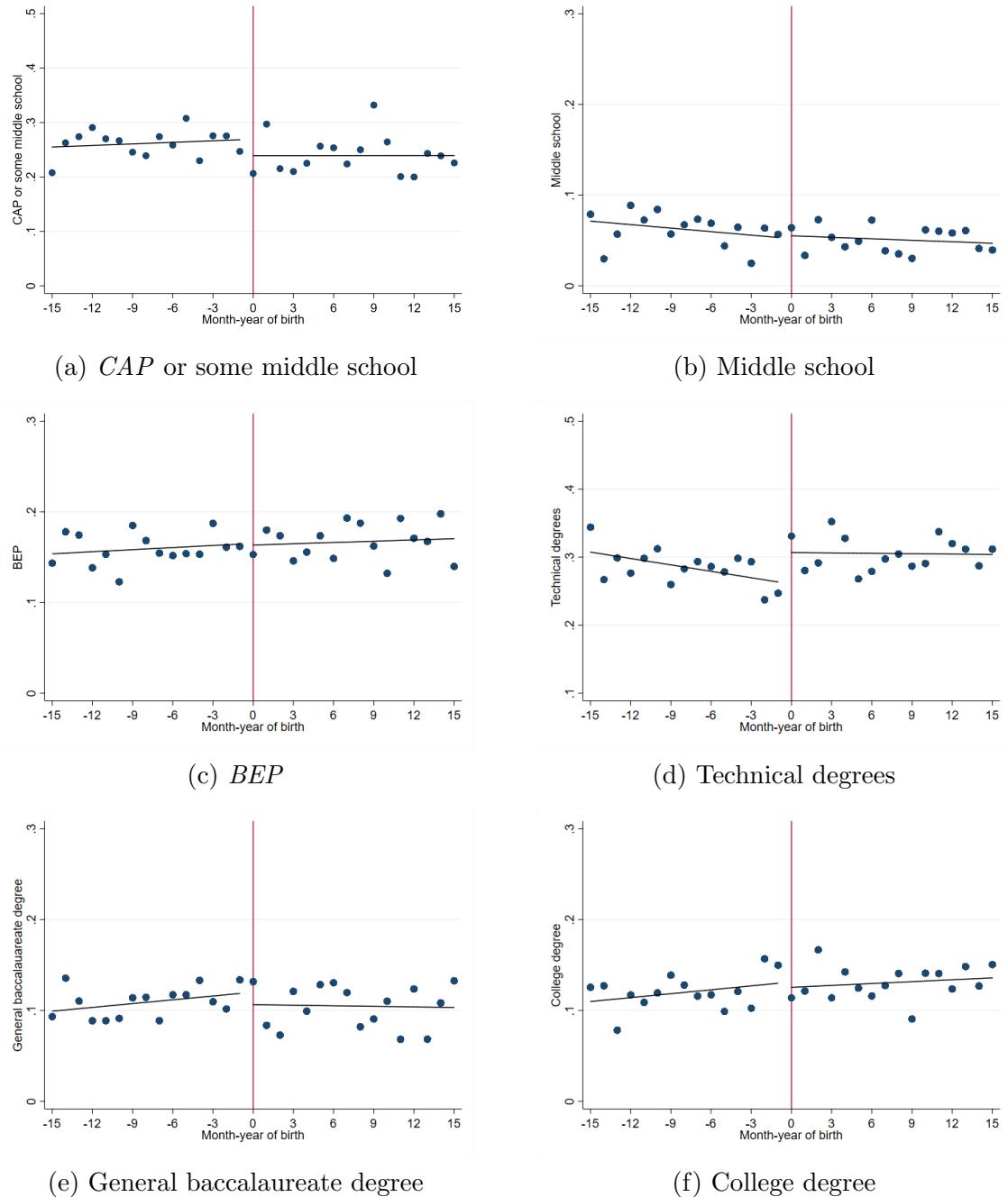
Notes: The different panels show the shares of individuals who hold different types of degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include all individuals who are born in France, whose parents are born in France and whose fathers are in middle or high-skilled occupations (i.e., the high SES sample). Data are taken from the survey "Enquête sur la Famille et les logements".

Figure A5: Effects of the Reform on Men's Highest Degree Received



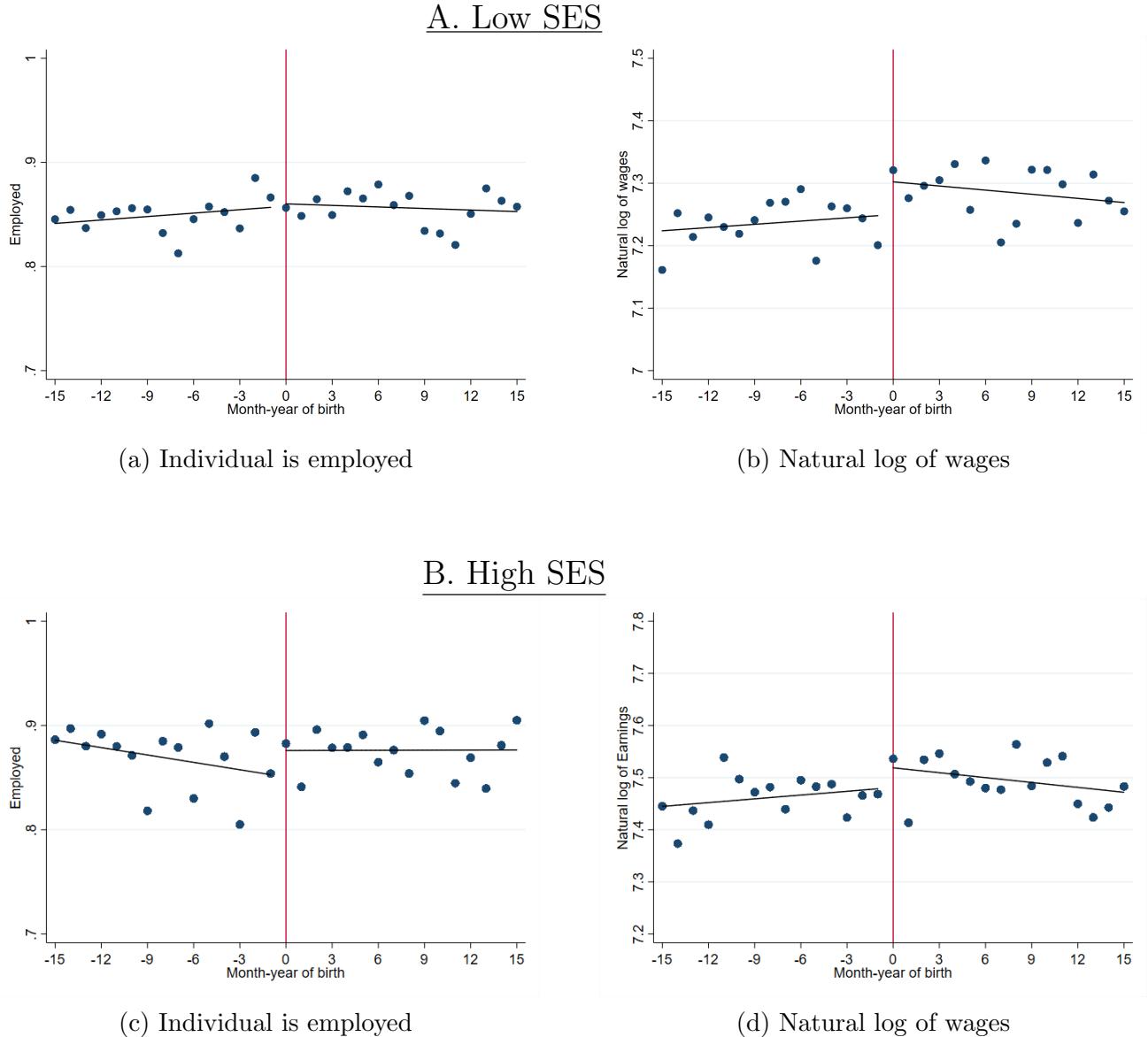
Notes: The different panels show the shares of individuals who hold different types of degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include men who are born in France and whose parents are born in France. Data are taken from the survey "Enquête sur la Famille et les logements".

Figure A6: Effects of the Reform on Women's Highest Degree Received



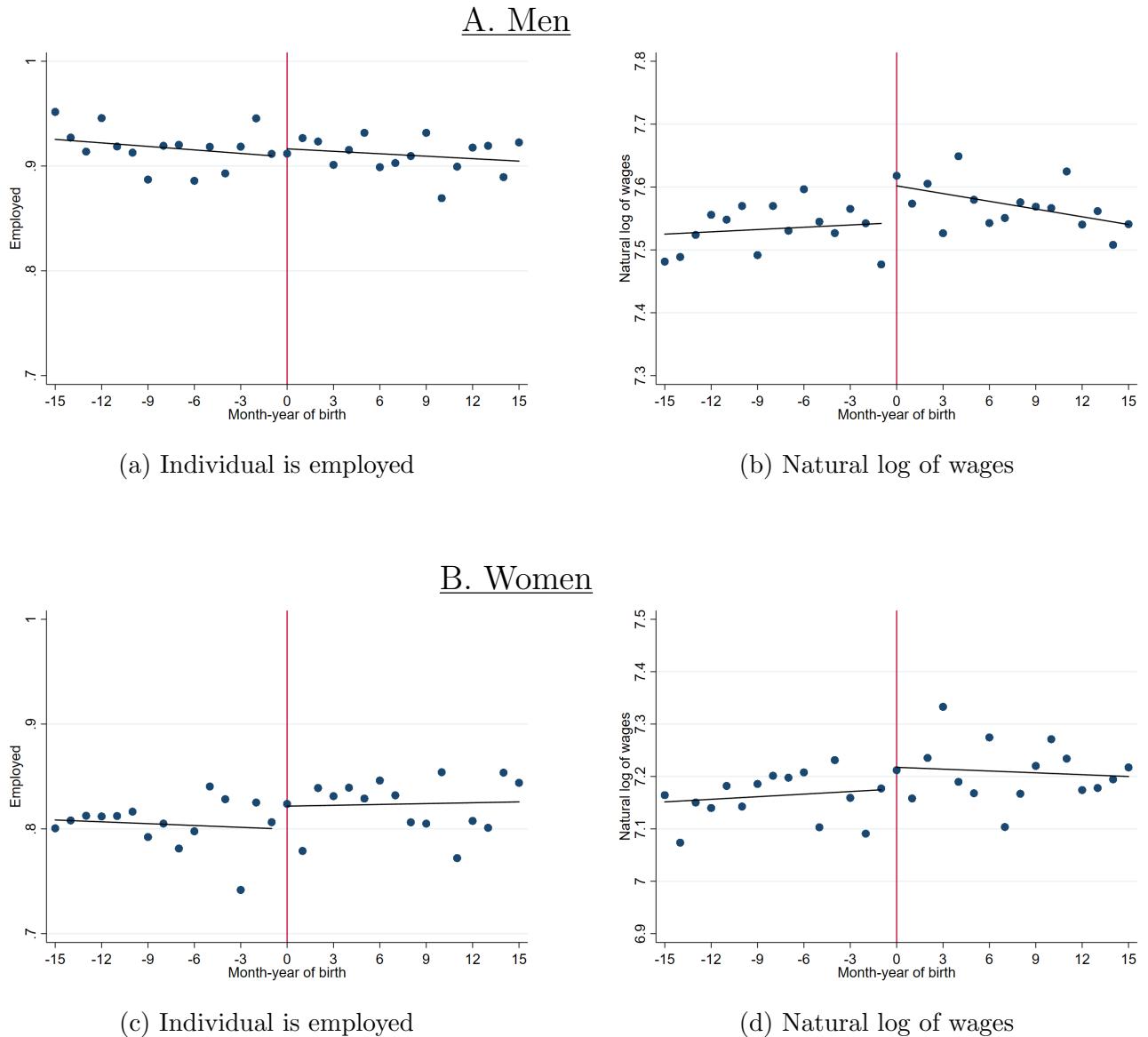
Notes: The different panels show the shares of individuals who hold different types of degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include women who are born in France and whose parents are born in France. Data are taken from the survey "Enquête sur la Famille et les logements".

Figure A7: Effects of the Reform on Labor Market Outcomes by Socioeconomic Background



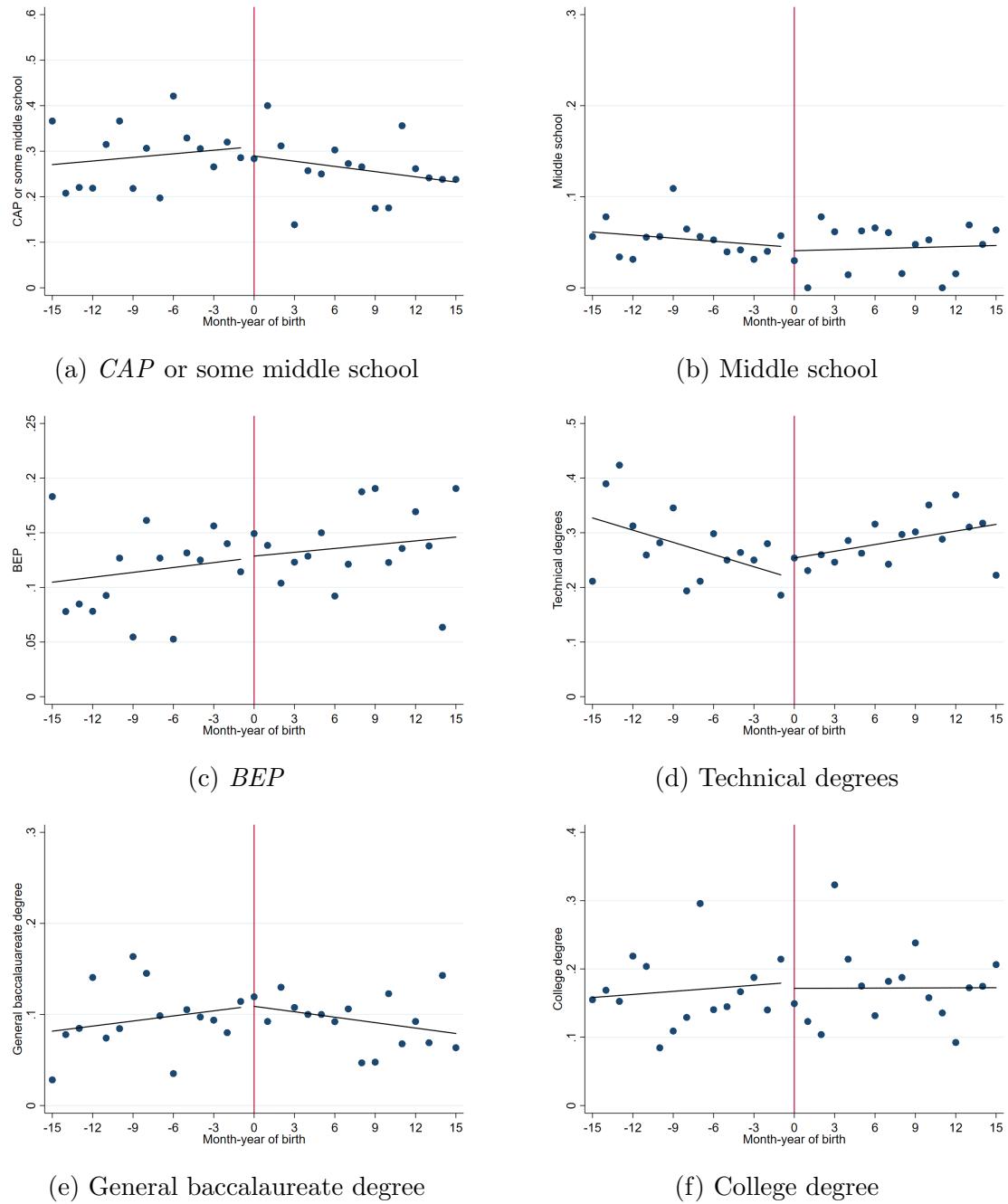
Notes: The different panels show the likelihood of being employed and the log of wages, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include individuals who are born in France, whose parents are born in France and who are aged between 40 and 45 at the time of the survey. Panels (a) and (b) are restricted to individuals whose fathers are in low-skilled occupations (low SES), while panels (c) and (d) are restricted to individuals whose fathers are in middle or high-skilled occupations (high SES). Data are taken from the French labor force survey.

Figure A8: Effects of the Reform on Labor Market Outcomes by Gender



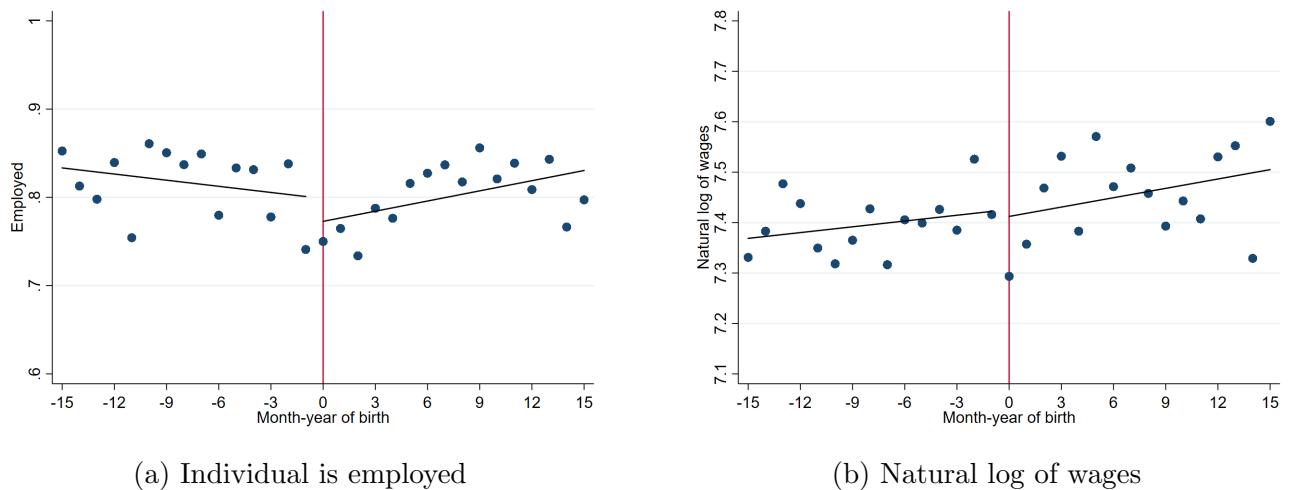
Notes: The different panels show the likelihood of being employed and the log of wages, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include individuals who are born in France, whose parents are born in France and who are aged between 40 and 45 at the time of the survey. Panels (a) and (b) are restricted to men, while panels (c) and (d) are restricted to women. Data are taken from the French labor force survey.

Figure A9: Effects of the Reform on Children of Immigrants' Education Outcomes



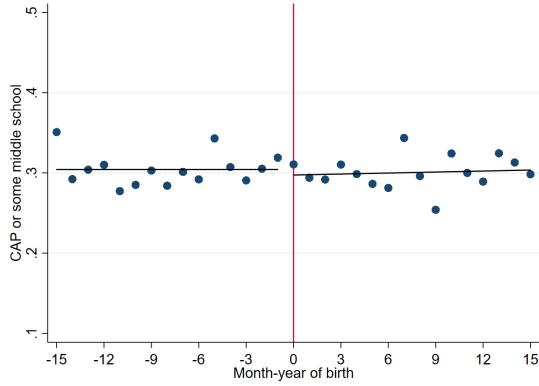
Notes: The different panels show the shares of individuals who hold different types of degrees, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include individuals who are born in France and whose parents are born outside of France. Data are taken from the survey "Enquête sur la Famille et les logements".

Figure A10: Effects of the Reform on Children of Immigrants' Labor Outcomes

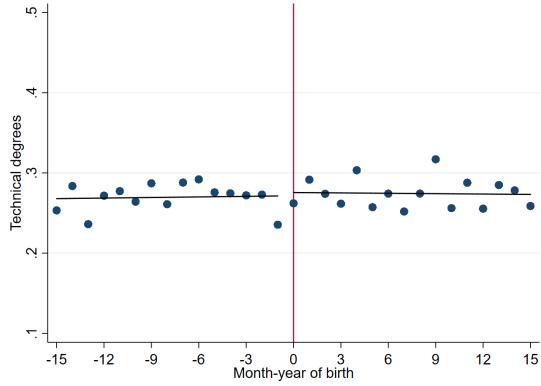


Notes: The different panels show the likelihood of being employed and the log of wages, as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include individuals who are born in France, whose parents are born outside of France and who are aged between 40 and 45 at the time of the survey. Data are taken from the French labor force survey.

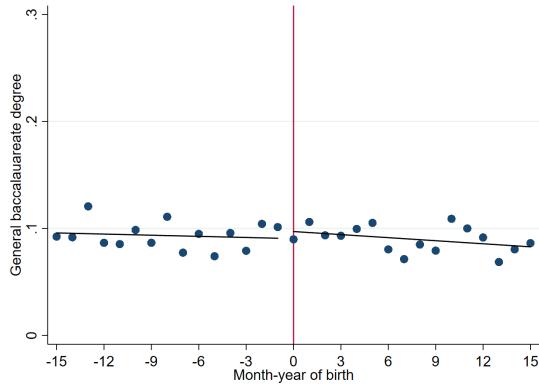
Figure A11: Placebo test for main outcomes using January 1964 as a fake cutoff



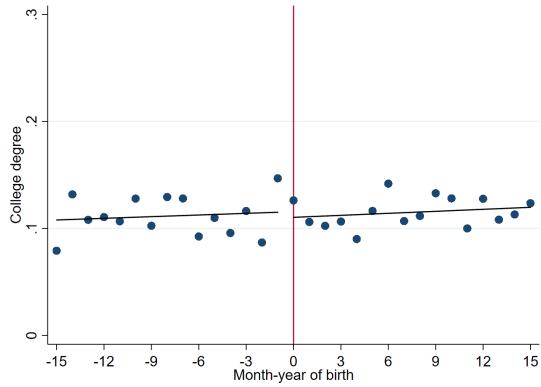
(a) CAP or some middle school



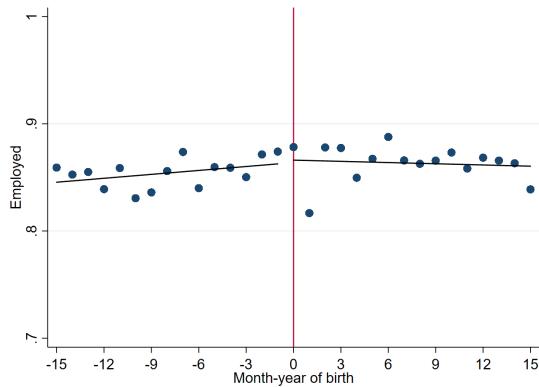
(b) Technical degrees



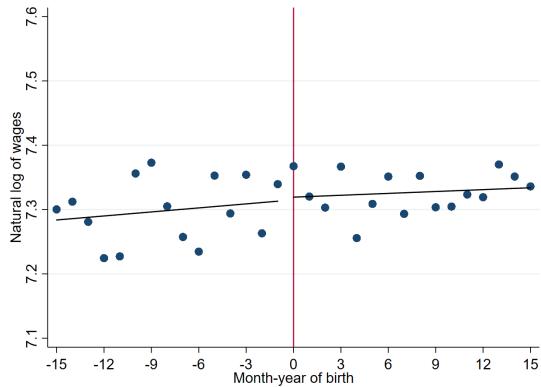
(c) General baccalaureate degree



(d) College degree



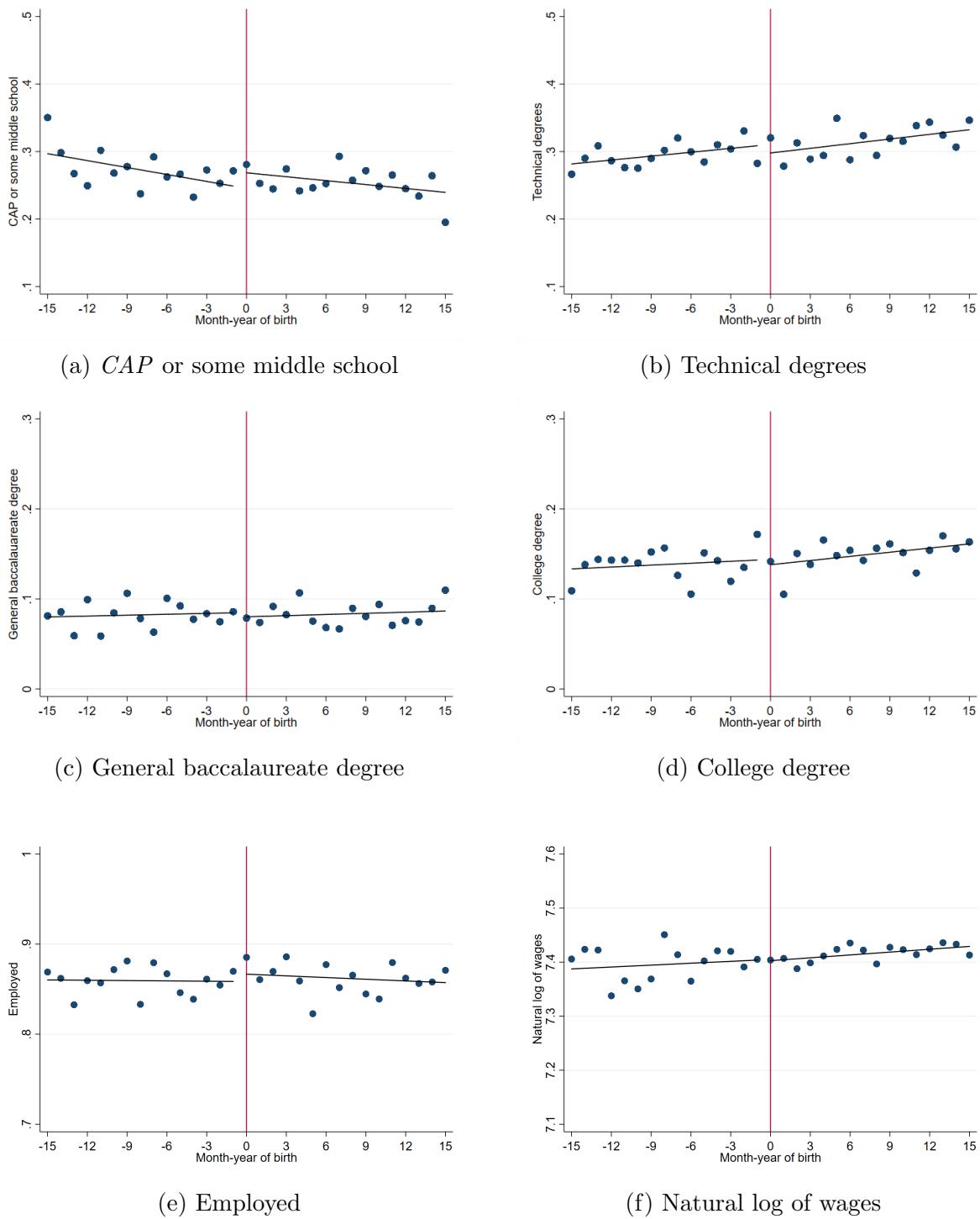
(e) Employed



(f) Natural log of wages

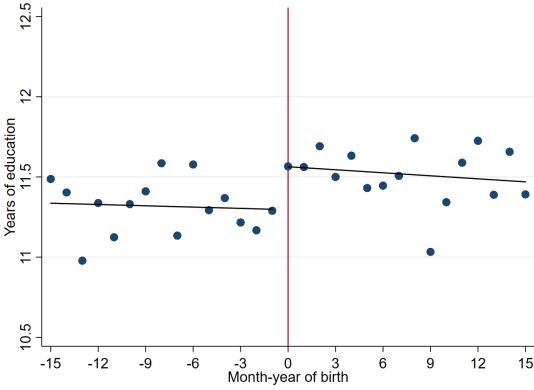
Notes: The different panels show various outcomes, as a function of the distance of individuals' month-year of birth from the fake cutoff of January 1, 1964. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include all individuals who are born in France and whose parents are born in France. Panels (a) to (d) show the shares of individuals with different types of degrees and data are taken from the survey "Enquête sur la Famille et les logements". Panels (e) and (f) show the likelihood of being employed and the log of wages. Corresponding samples include individuals who are aged between 40 and 45 at the time of survey and data are taken from the French labor force survey. ⁵⁶

Figure A12: Placebo test for main outcomes using January 1968 as a fake cutoff

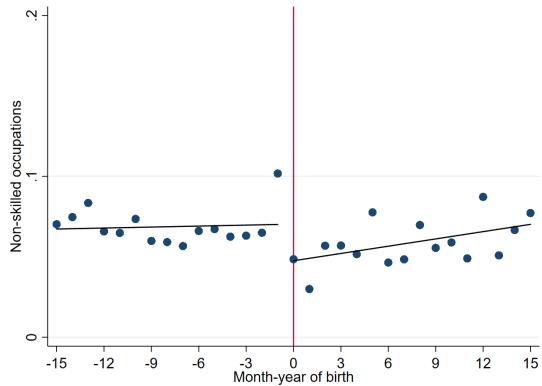


Notes: The different panels show various outcomes, as a function of the distance of individuals' month-year of birth from the fake cutoff of January 1, 1968. Circles represent each outcome's average over a one month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Samples include all individuals who are born in France and whose parents are born in France. Panels (a) to (d) show the shares of individuals with different types of degrees and data are taken from the survey "Enquête sur la Famille et les logements". Panels (e) and (f) show the likelihood of being employed and the log of wages. Corresponding samples include individuals who are aged between 40 and 45 at the time of survey and data are taken from the French labor force survey.

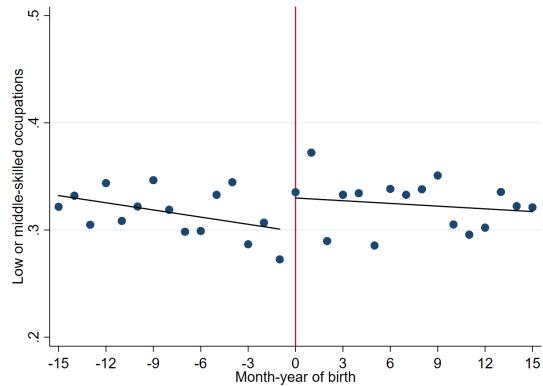
Figure A13: Additional Results



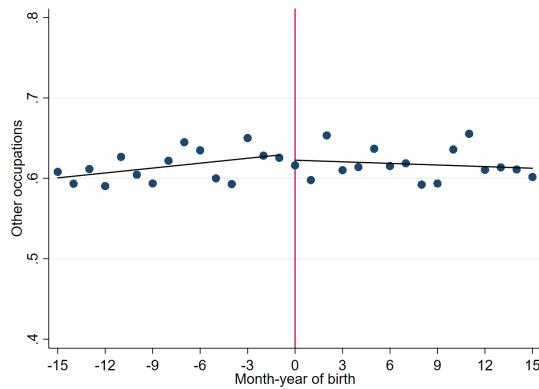
(a) Years of education



(b) Unskilled occupations



(c) Low or middle-skilled occupations



(d) Other occupations

Notes: Panel (a) plots the number of years of education as a function of the distance of individuals' month-year of birth from the reform cutoff. Panels (b) to (d) plot the likelihoods of holding different types of occupations as a function of the distance of individuals' month-year of birth from the reform cutoff. In all panels, samples include individuals who are born in France and whose parents are born in France. 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. Data for panel (a) are from the "Enquête sur la Famille et les logements". Panels (b) to (d) are restricted to individuals aged between 40 and 45 at the time of the survey and data are taken from the French labor force survey.
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Table A1: Middle School Curriculum Before and After the Reform

	1st year			Last year	
	Pre-reform		Post-reform	Pre-reform	Post-reform
	High track (1)	Low track (2)	(3)	High track (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
<i>Elective course: students choose one of the following subjects (in last year only)</i>					
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

Notes: 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 (columns (1), (2) and (4)) and after the reform (columns (3) and (5)). 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: RD Estimates for Baseline Covariates Using Different Bandwidths

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
Individual is born in France	-0.012 (0.020)	0.003 (0.016)	0.007 (0.012)	0.010 (0.010)	0.010 (0.010)	0.010 (0.009)	0.009 (0.008)
Father is born in France	0.006 (0.025)	0.012 (0.020)	0.012 (0.015)	0.010 (0.013)	0.007 (0.012)	0.005 (0.011)	0.004 (0.011)
Mother is born in France	-0.005 (0.024)	0.012 (0.019)	0.016 (0.014)	0.015 (0.013)	0.013 (0.012)	0.012 (0.011)	0.012 (0.010)
<i>N</i>	6,445	9,640	16,007	19,280	22,600	25,786	28,929
Individual is male	0.004 (0.033)	-0.003 (0.026)	-0.001 (0.019)	0.002 (0.018)	0.006 (0.016)	0.007 (0.015)	0.008 (0.014)
Father is low-skilled worker	-0.014 (0.035)	-0.033 (0.027)	-0.032 (0.020)	-0.024 (0.018)	-0.018 (0.017)	-0.011 (0.016)	-0.005 (0.015)
<i>N</i>	4,918	7,397	12,240	14,744	17,281	19,684	22,070

Notes: Each cell reports the reduced form estimate of the impact of the reform on baseline covariates. Estimates are taken from separate local linear RD regressions which use a triangular kernel. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. The second panel restricts the samples to individuals who are born in France and whose parents are born in France. Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (***) p < 0.01 ** p < 0.05 * p < 0.1).

Table A3: RD Estimates for Overall Sample's Highest Degree Received Using Different Bandwidths

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
No schooling	-0.002 (0.004)	-0.002 (0.003)	0.000 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
With controls	-0.002 (0.004)	-0.002 (0.003)	-0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
<i>CAP</i> or some middle school	-0.072** (0.032)	-0.073*** (0.025)	-0.061*** (0.018)	-0.055*** (0.017)	-0.050*** (0.015)	-0.045*** (0.014)	-0.041*** (0.013)
With controls	-0.072** (0.031)	-0.069*** (0.024)	-0.025 (0.018)	-0.033** (0.016)	-0.032** (0.015)	-0.030** (0.014)	-0.028** (0.013)
Middle school	0.003 (0.016)	0.010 (0.012)	0.006 (0.009)	0.003 (0.008)	0.003 (0.007)	0.002 (0.007)	0.002 (0.007)
With controls	0.003 (0.016)	0.010 (0.012)	-0.018** (0.009)	-0.010 (0.008)	-0.008 (0.007)	-0.007 (0.007)	-0.007 (0.007)
<i>BEP</i>	0.007 (0.025)	0.001 (0.020)	-0.001 (0.015)	-0.001 (0.014)	-0.001 (0.012)	-0.002 (0.012)	-0.003 (0.011)
With controls	0.008 (0.025)	0.001 (0.020)	0.004 (0.015)	0.001 (0.014)	0.001 (0.012)	0.000 (0.012)	-0.001 (0.011)
Technical degrees	0.078** (0.030)	0.071*** (0.024)	0.059*** (0.018)	0.056*** (0.016)	0.049*** (0.015)	0.044*** (0.014)	0.040*** (0.013)
With controls	0.078** (0.030)	0.070*** (0.024)	0.049*** (0.018)	0.049*** (0.016)	0.044*** (0.015)	0.040*** (0.014)	0.035*** (0.013)
General baccalaureate degree	-0.000 (0.021)	-0.004 (0.016)	-0.004 (0.012)	-0.004 (0.011)	-0.004 (0.010)	-0.003 (0.009)	-0.002 (0.009)
With controls	0.000 (0.021)	-0.004 (0.016)	0.002 (0.012)	-0.003 (0.011)	-0.003 (0.010)	-0.002 (0.009)	-0.001 (0.009)
College degree	-0.014 (0.024)	-0.003 (0.019)	0.000 (0.014)	0.000 (0.013)	0.002 (0.012)	0.002 (0.011)	0.003 (0.010)
With controls	-0.015 (0.023)	-0.006 (0.018)	-0.014 (0.014)	-0.006 (0.012)	-0.002 (0.011)	-0.001 (0.011)	0.000 (0.010)
<i>N</i>	4,918	7,397	12,240	14,744	17,281	19,684	22,070

Notes: Each cell reports the reduced form estimate of the impact of the reform on the probabilities of holding different degrees. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—and dummy variables for whether the individual is male and whether the father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France and whose parents are born in France. Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A4: RD Estimates for Overall Sample's Highest Degree Received Using Different Bandwidths and Survey Weights

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
No schooling	-0.002 (0.003)	-0.000 (0.003)	0.003 (0.003)	0.003 (0.002)	0.003 (0.002)	0.003 (0.002)	0.003 (0.002)
With controls	-0.002 (0.003)	-0.000 (0.003)	0.004* (0.003)	0.004* (0.002)	0.004 (0.002)	0.003 (0.002)	0.003 (0.002)
<i>CAP</i> or some middle school	-0.090** (0.039)	-0.080*** (0.030)	-0.062*** (0.022)	-0.054*** (0.020)	-0.049*** (0.018)	-0.046*** (0.017)	-0.043*** (0.016)
With controls	-0.090** (0.038)	-0.080*** (0.029)	-0.033 (0.022)	-0.037* (0.020)	-0.036** (0.018)	-0.034** (0.017)	-0.034** (0.016)
Middle school	-0.011 (0.020)	0.001 (0.015)	-0.001 (0.011)	-0.005 (0.010)	-0.005 (0.009)	-0.005 (0.008)	-0.005 (0.008)
With controls	-0.011 (0.020)	0.001 (0.015)	-0.028** (0.011)	-0.021** (0.010)	-0.018* (0.009)	-0.016* (0.008)	-0.015* (0.008)
<i>BEP</i>	0.036 (0.028)	0.020 (0.022)	0.007 (0.017)	0.005 (0.015)	0.003 (0.014)	0.000 (0.013)	-0.002 (0.012)
With controls	0.036 (0.028)	0.020 (0.022)	0.013 (0.017)	0.005 (0.015)	0.002 (0.014)	-0.000 (0.013)	-0.002 (0.012)
Technical degrees	0.068* (0.035)	0.063** (0.028)	0.059*** (0.021)	0.055*** (0.019)	0.049*** (0.018)	0.046*** (0.016)	0.042*** (0.016)
With controls	0.068* (0.035)	0.064** (0.028)	0.048** (0.021)	0.051*** (0.019)	0.046*** (0.018)	0.043*** (0.016)	0.039** (0.015)
General baccalaureate degree	-0.001 (0.021)	-0.007 (0.017)	-0.003 (0.013)	-0.003 (0.012)	-0.004 (0.011)	-0.004 (0.010)	-0.003 (0.009)
With controls	-0.001 (0.020)	-0.007 (0.016)	-0.001 (0.013)	-0.004 (0.012)	-0.005 (0.011)	-0.005 (0.010)	-0.003 (0.009)
College degree	-0.000 (0.030)	0.003 (0.024)	-0.003 (0.018)	-0.001 (0.017)	0.003 (0.015)	0.005 (0.014)	0.007 (0.013)
With controls	-0.000 (0.030)	0.003 (0.024)	-0.004 (0.018)	0.002 (0.017)	0.007 (0.015)	0.009 (0.014)	0.012 (0.013)
<i>N</i>	4,918	7,397	12,240	14,744	17,281	19,684	22,070

Notes: Each cell reports the reduced form estimate of the impact of the reform on the probabilities of holding different degrees. Estimates are taken from separate local linear RD regressions which use a triangular kernel and survey weights, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—and dummy variables for whether the individual is male and whether the father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France and whose parents are born in France. Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A5: RD Estimates for Highest Degree Received Using Different Bandwidths by Socioeconomic Background

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
A) Low SES							
CAP or some middle school	-0.113** (0.047)	-0.108*** (0.036)	-0.088*** (0.027)	-0.080*** (0.024)	-0.073*** (0.022)	-0.068*** (0.021)	-0.064*** (0.020)
With controls	-0.113** (0.046)	-0.108*** (0.036)	-0.030 (0.027)	-0.044* (0.024)	-0.045** (0.022)	-0.043** (0.021)	-0.042** (0.020)
Technical degrees	0.102** (0.041)	0.078** (0.032)	0.071*** (0.024)	0.066*** (0.022)	0.060*** (0.020)	0.054*** (0.019)	0.049*** (0.018)
With controls	0.102** (0.041)	0.078** (0.032)	0.056** (0.024)	0.052** (0.022)	0.048** (0.020)	0.043** (0.019)	0.040** (0.018)
General baccalaureate degree	-0.007 (0.026)	0.003 (0.020)	0.006 (0.014)	0.006 (0.013)	0.006 (0.012)	0.007 (0.011)	0.007 (0.011)
With controls	-0.007 (0.026)	0.003 (0.020)	0.003 (0.014)	0.006 (0.013)	0.006 (0.012)	0.007 (0.011)	0.006 (0.011)
College degree	-0.008 (0.024)	-0.002 (0.018)	-0.006 (0.013)	-0.005 (0.012)	-0.003 (0.011)	-0.001 (0.010)	0.000 (0.010)
With controls	-0.008 (0.024)	-0.002 (0.018)	-0.027** (0.013)	-0.014 (0.012)	-0.009 (0.011)	-0.006 (0.010)	-0.004 (0.010)
N	2,626	3,938	6,589	7,941	9,306	10,663	11,932
B) High SES							
CAP or some middle school	-0.022 (0.039)	-0.020 (0.031)	-0.016 (0.023)	-0.018 (0.021)	-0.016 (0.019)	-0.014 (0.018)	-0.013 (0.017)
With controls	-0.023 (0.039)	-0.020 (0.031)	-0.026 (0.023)	-0.027 (0.021)	-0.023 (0.019)	-0.021 (0.018)	-0.019 (0.017)
Technical degrees	0.051 (0.046)	0.062* (0.036)	0.043 (0.027)	0.042* (0.025)	0.036 (0.023)	0.032 (0.021)	0.028 (0.020)
With controls	0.051 (0.046)	0.061* (0.036)	0.046* (0.027)	0.050** (0.025)	0.042* (0.023)	0.038* (0.021)	0.033* (0.020)
General baccalaureate degree	0.006 (0.033)	-0.016 (0.026)	-0.017 (0.019)	-0.018 (0.018)	-0.017 (0.016)	-0.016 (0.015)	-0.013 (0.014)
With controls	0.007 (0.033)	-0.016 (0.026)	0.001 (0.019)	-0.014 (0.018)	-0.015 (0.016)	-0.014 (0.015)	-0.010 (0.014)
College degree	-0.026 (0.042)	-0.015 (0.033)	-0.004 (0.025)	-0.001 (0.022)	0.003 (0.021)	0.003 (0.019)	0.005 (0.018)
With controls	-0.026 (0.042)	-0.015 (0.033)	0.005 (0.025)	0.007 (0.022)	0.010 (0.021)	0.009 (0.019)	0.011 (0.018)
N	2,292	3,469	5,651	6,803	7,975	9,021	10,138

Notes: Each cell reports the reduced form estimate of the impact of the reform on the probabilities of holding different degrees. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—and a dummy variable for whether the individual is male. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France and whose parents are born in France. The samples in Panel A include low SES individuals (i.e., whose fathers are in low-skilled occupations), while the samples in Panel B include high SES individuals (i.e., whose fathers are in middle or high-skilled occupations). Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A6: RD Estimates for Highest Degree Received Using Different Bandwidths by Gender

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
A) Men							
CAP or some middle school	-0.181*** (0.057)	-0.157*** (0.044)	-0.114*** (0.033)	-0.100*** (0.030)	-0.087*** (0.028)	-0.076*** (0.026)	-0.069*** (0.024)
With controls	-0.175*** (0.056)	-0.148*** (0.043)	-0.082** (0.032)	-0.060** (0.029)	-0.050* (0.027)	-0.045* (0.025)	-0.045* (0.024)
Technical degrees	0.067 (0.051)	0.066* (0.040)	0.071** (0.030)	0.067** (0.027)	0.059** (0.025)	0.056** (0.023)	0.052** (0.022)
With controls	0.068 (0.051)	0.066* (0.040)	0.077** (0.030)	0.068** (0.027)	0.058** (0.025)	0.055** (0.023)	0.049** (0.022)
General baccalaureate degree	0.019 (0.026)	0.027 (0.021)	0.019 (0.016)	0.016 (0.015)	0.013 (0.014)	0.011 (0.013)	0.009 (0.012)
With controls	0.018 (0.026)	0.025 (0.021)	0.014 (0.016)	0.012 (0.015)	0.009 (0.014)	0.007 (0.013)	0.007 (0.012)
College degree	0.053 (0.040)	0.039 (0.032)	0.015 (0.024)	0.014 (0.022)	0.014 (0.020)	0.013 (0.019)	0.014 (0.018)
With controls	0.045 (0.039)	0.030 (0.031)	0.012 (0.023)	0.000 (0.021)	0.003 (0.019)	0.005 (0.018)	0.010 (0.017)
N	1,685	2,536	4,221	5,079	5,932	6,718	7,590
B) Women							
CAP or some middle school	-0.016 (0.037)	-0.028 (0.029)	-0.031 (0.022)	-0.032 (0.020)	-0.032* (0.018)	-0.030* (0.017)	-0.027* (0.016)
With controls	-0.017 (0.036)	-0.024 (0.028)	0.006 (0.021)	-0.019 (0.019)	-0.023 (0.018)	-0.023 (0.017)	-0.021 (0.016)
Technical degrees	0.083** (0.038)	0.073** (0.030)	0.052** (0.022)	0.050** (0.021)	0.044** (0.019)	0.038** (0.018)	0.034** (0.017)
With controls	0.081** (0.038)	0.071** (0.030)	0.036 (0.023)	0.041** (0.021)	0.037** (0.019)	0.033* (0.018)	0.029* (0.017)
General baccalaureate degree	-0.010 (0.029)	-0.020 (0.022)	-0.015 (0.016)	-0.014 (0.015)	-0.012 (0.013)	-0.010 (0.012)	-0.007 (0.012)
With controls	-0.012 (0.029)	-0.022 (0.022)	-0.004 (0.016)	-0.010 (0.014)	-0.009 (0.013)	-0.007 (0.012)	-0.005 (0.012)
College degree	-0.049* (0.030)	-0.025 (0.023)	-0.009 (0.017)	-0.007 (0.015)	-0.004 (0.014)	-0.003 (0.013)	-0.003 (0.012)
With controls	-0.043 (0.028)	-0.025 (0.022)	-0.030* (0.016)	-0.011 (0.015)	-0.007 (0.014)	-0.006 (0.013)	-0.005 (0.012)
N	3,233	4,861	8,019	9,665	11,349	12,966	14,480

Notes: Each cell reports the reduced form estimate of the impact of the reform on the probabilities of holding different degrees. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—and a dummy variable for whether the individual's father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France and whose parents are born in France. The samples in Panel A include only men while the samples in Panel B include only women. Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (***) p < 0.01 ** p < 0.05 * p < 0.1).

Table A7: RD estimates for Overall Sample's Labor Market Outcomes Using Different Bandwidths

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
Employed	-0.004 (0.017)	-0.003 (0.014)	0.009 (0.010)	0.012 (0.009)	0.014 (0.009)	0.015* (0.008)	0.014* (0.008)
With controls	-0.002 (0.017)	0.000 (0.013)	0.012 (0.010)	0.013 (0.009)	0.013 (0.009)	0.013* (0.008)	0.012 (0.008)
<i>N</i>	8,893	13,445	21,771	26,320	30,580	34,697	39,119
Log of wages	0.076** (0.037)	0.080*** (0.029)	0.060*** (0.022)	0.053*** (0.020)	0.046** (0.018)	0.041** (0.017)	0.039** (0.016)
With controls	0.080** (0.034)	0.085*** (0.027)	0.047** (0.020)	0.044** (0.018)	0.038** (0.017)	0.033** (0.015)	0.032** (0.014)
<i>N</i>	5,933	8,901	14,450	17,460	20,273	22,995	25,931

Notes: Each cell reports the reduced form estimate of the impact of the reform on labor market outcomes. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—age, quarter and year of survey fixed effects and dummy variables for whether the individual is male and whether the father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France, whose parents are born in France and who are aged between 40 to 45 at the time of the survey. Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A8: RD Estimates for Labor Market Outcomes Using Different Bandwidths by Socioeconomic Background

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
A) Low SES							
Employed	-0.025 (0.025)	-0.026 (0.019)	-0.004 (0.015)	0.000 (0.013)	0.004 (0.012)	0.007 (0.012)	0.007 (0.011)
With controls	-0.022 (0.024)	-0.023 (0.019)	-0.005 (0.015)	0.001 (0.013)	0.003 (0.012)	0.004 (0.011)	0.003 (0.011)
N	4,537	6,916	11,242	13,557	15,773	17,916	20,208
Log of wages	0.095* (0.051)	0.096** (0.039)	0.073** (0.029)	0.065** (0.027)	0.057** (0.024)	0.050** (0.022)	0.046** (0.021)
With controls	0.102** (0.047)	0.100*** (0.037)	0.059** (0.028)	0.051** (0.025)	0.046** (0.023)	0.040* (0.021)	0.038* (0.020)
N	3,041	4,643	7,539	9,071	10,575	11,982	13,526
B) High SES							
Employed	0.017 (0.025)	0.021 (0.019)	0.023 (0.015)	0.024* (0.013)	0.024* (0.012)	0.023** (0.011)	0.022** (0.011)
With controls	0.022 (0.025)	0.026 (0.019)	0.030** (0.015)	0.026** (0.013)	0.025** (0.012)	0.024** (0.011)	0.022** (0.011)
N	4,356	6,529	10,529	12,763	14,807	16,781	18,911
Log of wages	0.042 (0.051)	0.051 (0.041)	0.043 (0.031)	0.039 (0.028)	0.033 (0.026)	0.031 (0.024)	0.030 (0.023)
With controls	0.063 (0.050)	0.076* (0.039)	0.037 (0.030)	0.040 (0.027)	0.032 (0.025)	0.027 (0.023)	0.026 (0.021)
N	2,892	4,258	6,911	8,389	9,698	11,013	12,405

Notes: Each cell reports the reduced form estimate of the impact of the reform on labor market outcomes. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—age, quarter and year of survey fixed effects and a dummy variable for whether the individual is male. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Samples are restricted to individuals who are born in France, whose parents are born in France and who are aged between 40 to 45 at the time of the survey. Samples in panel A are further restricted to individuals whose fathers are in low-skilled occupations (low SES), while those in panel B are restricted to individuals whose fathers are in middle or high-skilled occupations (high SES). Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A9: RD Estimates for Labor Market Outcomes Using Different Bandwidths by Gender

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
A) Men							
Employed	-0.010 (0.020)	-0.009 (0.015)	0.000 (0.012)	0.003 (0.011)	0.002 (0.010)	0.002 (0.009)	0.001 (0.009)
With controls	-0.012 (0.020)	-0.010 (0.015)	-0.005 (0.012)	-0.001 (0.011)	-0.002 (0.010)	-0.003 (0.009)	-0.004 (0.009)
N	4,324	6,549	10,561	12,774	14,895	16,833	18,941
Log of wages	0.127*** (0.042)	0.111*** (0.033)	0.079*** (0.025)	0.069*** (0.023)	0.056*** (0.021)	0.045** (0.020)	0.041** (0.019)
With controls	0.126*** (0.040)	0.109*** (0.032)	0.059** (0.024)	0.045** (0.022)	0.033 (0.020)	0.024 (0.019)	0.022 (0.018)
N	2,922	4,377	7,081	8,567	9,987	11,259	12,676
A) Women							
Employed	0.006 (0.028)	0.009 (0.022)	0.023 (0.017)	0.024 (0.015)	0.027** (0.014)	0.028** (0.013)	0.027** (0.012)
With controls	0.012 (0.028)	0.012 (0.022)	0.029* (0.017)	0.027* (0.015)	0.029** (0.014)	0.029** (0.013)	0.027** (0.012)
N	4,569	6,896	11,210	13,546	15,685	17,864	20,178
Log of wages	0.041 (0.058)	0.073* (0.044)	0.059* (0.033)	0.049* (0.029)	0.044 (0.027)	0.042* (0.025)	0.040* (0.023)
With controls	0.028 (0.055)	0.063 (0.043)	0.037 (0.032)	0.045 (0.029)	0.045* (0.026)	0.043* (0.024)	0.042* (0.023)
N	3,011	4,524	7,639	8,893	10,286	11,736	13,255

Notes: Each cell reports the reduced form estimate of the impact of the reform on labor market outcomes. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—age, quarter and year of survey fixed effects and a dummy variable for whether the individual is male. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Samples are restricted to individuals who are born in France, whose parents are born in France and who are aged between 40 to 45 at the time of the survey. Samples in panel A are further restricted to men, while those in panel B are restricted to women. Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A10: Effect of Reform on Highest Degree Received for Children of Immigrants, RD Estimates

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
No schooling	-0.013 (0.024)	-0.007 (0.018)	-0.006 (0.012)	-0.005 (0.010)	-0.004 (0.009)	-0.003 (0.008)	-0.002 (0.007)
With controls	-0.013 (0.024)	-0.007 (0.018)	-0.001 (0.012)	-0.001 (0.010)	-0.000 (0.009)	0.000 (0.008)	-0.000 (0.007)
<i>CAP</i> or some middle school	0.052 (0.077)	0.029 (0.062)	-0.006 (0.046)	-0.012 (0.042)	-0.013 (0.038)	-0.008 (0.036)	-0.007 (0.034)
With controls	0.055 (0.076)	0.030 (0.061)	-0.045 (0.045)	-0.026 (0.041)	-0.025 (0.037)	-0.019 (0.035)	-0.018 (0.033)
Middle school	-0.033 (0.035)	-0.018 (0.027)	-0.002 (0.020)	-0.003 (0.018)	-0.005 (0.017)	-0.006 (0.016)	-0.007 (0.015)
With controls	-0.038 (0.034)	-0.020 (0.027)	-0.014 (0.020)	-0.008 (0.018)	-0.010 (0.017)	-0.012 (0.016)	-0.013 (0.015)
<i>BEP</i>	0.027 (0.056)	0.006 (0.045)	-0.012 (0.034)	-0.003 (0.031)	0.000 (0.028)	0.001 (0.026)	0.006 (0.025)
With controls	0.023 (0.056)	0.004 (0.045)	-0.009 (0.034)	0.005 (0.031)	0.008 (0.028)	0.009 (0.026)	0.014 (0.025)
Technical degrees	0.055 (0.070)	0.024 (0.056)	0.028 (0.043)	0.025 (0.039)	0.022 (0.036)	0.013 (0.034)	0.006 (0.032)
With controls	0.055 (0.069)	0.020 (0.056)	0.056 (0.043)	0.028 (0.039)	0.023 (0.036)	0.016 (0.034)	0.010 (0.032)
General baccalaureate degree	0.006 (0.053)	0.011 (0.042)	0.022 (0.031)	0.014 (0.028)	0.013 (0.026)	0.013 (0.024)	0.014 (0.023)
With controls	-0.000 (0.053)	0.010 (0.042)	-0.012 (0.031)	-0.012 (0.028)	-0.007 (0.026)	-0.006 (0.024)	-0.002 (0.022)
College degree	-0.093 (0.065)	-0.044 (0.051)	-0.025 (0.038)	-0.015 (0.034)	-0.014 (0.032)	-0.011 (0.029)	-0.009 (0.028)
With controls	-0.081 (0.063)	-0.037 (0.049)	0.024 (0.037)	0.014 (0.033)	0.012 (0.030)	0.012 (0.028)	0.009 (0.027)
<i>N</i>	813	1,207	1,968	2,380	2,778	3,152	3,513

Notes: Each cell reports the reduced form estimate of the impact of the reform on the probabilities of holding different degrees. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—and dummy variables for whether the individual is male and whether the father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France, but include those whose parents are born outside of France. Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A11: Effect of Reform on Labor Outcomes for Children of Immigrants, RD Estimates

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
Employed	0.010 (0.048)	-0.021 (0.037)	-0.037 (0.028)	-0.034 (0.025)	-0.032 (0.023)	-0.034 (0.021)	-0.032 (0.020)
With controls	0.021 (0.046)	-0.012 (0.036)	-0.029 (0.027)	-0.026 (0.024)	-0.022 (0.022)	-0.023 (0.021)	-0.020 (0.020)
<i>N</i>	1,816	2,773	4,605	5,602	6,532	7,353	8,112
Log of wages	-0.152** (0.069)	-0.137*** (0.053)	-0.059 (0.040)	-0.034 (0.036)	-0.016 (0.033)	-0.007 (0.031)	-0.001 (0.029)
With controls	-0.145** (0.063)	-0.133*** (0.049)	-0.025 (0.037)	-0.012 (0.034)	-0.000 (0.031)	0.002 (0.029)	0.004 (0.027)
<i>N</i>	1,057	1,685	2,829	3,421	3,993	4,525	5,016

Notes: Each cell reports the reduced form estimate of the impact of the reform on labor market outcomes. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—age, quarter and year of survey fixed effects and dummy variables for whether the individual is male and whether the father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France, but include those whose parents are born outside of France and who are aged between 40 to 45 at the time of the survey. Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A12: Effect of Reform on Highest Degree Received for Overall Sample Including Children of Immigrants, RD Estimates

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
No schooling	-0.004 (0.005)	-0.003 (0.004)	-0.001 (0.003)	-0.000 (0.002)	0.000 (0.002)	0.000 (0.002)	0.000 (0.002)
With controls	-0.004 (0.005)	-0.003 (0.004)	0.001 (0.003)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
<i>CAP or some middle school</i>	-0.055* (0.029)	-0.058** (0.023)	-0.054*** (0.017)	-0.049*** (0.015)	-0.045*** (0.014)	-0.040*** (0.013)	-0.036*** (0.013)
With controls	-0.054* (0.028)	-0.054*** (0.022)	-0.028* (0.017)	-0.033** (0.015)	-0.032** (0.014)	-0.030** (0.013)	-0.028** (0.012)
Middle school	-0.002 (0.014)	0.006 (0.011)	0.005 (0.008)	0.002 (0.007)	0.002 (0.007)	0.001 (0.006)	0.001 (0.006)
With controls	-0.002 (0.014)	0.006 (0.011)	-0.017** (0.008)	-0.010 (0.007)	-0.008 (0.007)	-0.008 (0.006)	-0.007 (0.006)
<i>BEP</i>	0.010 (0.023)	0.002 (0.018)	-0.003 (0.014)	-0.002 (0.012)	-0.001 (0.011)	-0.002 (0.011)	-0.002 (0.010)
With controls	0.010 (0.023)	0.003 (0.018)	0.003 (0.014)	0.002 (0.012)	0.002 (0.011)	0.001 (0.011)	0.001 (0.010)
Technical degrees	0.075*** (0.028)	0.065*** (0.022)	0.055*** (0.017)	0.051*** (0.015)	0.045*** (0.014)	0.040*** (0.013)	0.035*** (0.012)
With controls	0.074*** (0.028)	0.063*** (0.022)	0.053*** (0.017)	0.049*** (0.015)	0.042*** (0.014)	0.038*** (0.013)	0.033*** (0.012)
General baccalaureate degree	0.001 (0.020)	-0.002 (0.015)	-0.000 (0.011)	-0.001 (0.010)	-0.001 (0.009)	-0.001 (0.009)	0.000 (0.008)
With controls	-0.000 (0.019)	-0.004 (0.015)	0.000 (0.011)	-0.004 (0.010)	-0.004 (0.009)	-0.003 (0.009)	-0.001 (0.008)
College degree	-0.025 (0.023)	-0.009 (0.018)	-0.003 (0.013)	-0.002 (0.012)	0.000 (0.011)	0.001 (0.010)	0.002 (0.010)
With controls	-0.023 (0.022)	-0.011 (0.017)	-0.011 (0.013)	-0.004 (0.011)	-0.001 (0.011)	0.000 (0.010)	0.002 (0.009)
N	5,731	8,604	14,208	17,124	20,059	22,836	25,583

Notes: Each cell reports the reduced form estimate of the impact of the reform on the probabilities of holding different degrees. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—and dummy variables for whether the individual is male and whether the father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France, but include those whose parents are born in and outside of France. Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A13: Effect of Reform on Labor Outcomes for Overall Sample Including Children of Immigrants, RD Estimates

	BW=6 (1)	BW=9 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
Employed	0.002 (0.017)	-0.003 (0.013)	0.003 (0.010)	0.006 (0.009)	0.007 (0.008)	0.008 (0.008)	0.008 (0.007)
With controls	0.006 (0.016)	0.001 (0.013)	0.006 (0.010)	0.008 (0.009)	0.009 (0.008)	0.008 (0.008)	0.008 (0.007)
<i>N</i>	10,709	16,218	26,376	31,922	37,112	42,050	47,231
Log of wages	0.042 (0.033)	0.047* (0.026)	0.041** (0.020)	0.039** (0.018)	0.036** (0.016)	0.033** (0.015)	0.032** (0.014)
With controls	0.051* (0.031)	0.055** (0.024)	0.039** (0.018)	0.038** (0.016)	0.034** (0.015)	0.029** (0.014)	0.029** (0.013)
<i>N</i>	6,990	10,586	17,279	20,881	24,266	27,520	30,947

Notes: Each cell reports the reduced form estimate of the impact of the reform on labor market outcomes. Estimates are taken from separate local linear RD regressions which use a triangular kernel, both with and without controls. Controls include month of birth fixed effects—except for the bandwidths of 6 and 9 months—age, quarter and year of survey fixed effects and dummy variables for whether the individual is male and whether the father is in a low-skilled occupation. Each column uses the listed bandwidth (BW), and samples consist of all individuals born within those different bandwidths. Further, samples are restricted to individuals who are born in France, but include those whose parents are born in and outside of France and who are aged between 40 to 45 at the time of the survey. Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (***) p < 0.01 ** p < 0.05 * p < 0.1).

Table A14: Heterogeneity Analysis Including Children of Immigrants, RD Estimates

	Low SES (1)	High SES (2)	Men (3)	Women (4)
No schooling	-0.001 (0.004)	-0.000 (0.004)	0.001 (0.004)	-0.001 (0.003)
<i>CAP</i> or some middle school	-0.074*** (0.025)	-0.018 (0.022)	-0.116*** (0.030)	-0.020 (0.020)
Middle school	0.006 (0.012)	0.005 (0.011)	-0.001 (0.013)	0.009 (0.010)
<i>BEP</i>	-0.005 (0.020)	0.003 (0.019)	0.011 (0.023)	-0.009 (0.017)
Technical degrees	0.073*** (0.022)	0.031 (0.025)	0.067** (0.027)	0.048** (0.021)
Baccalaureate degree	0.003 (0.014)	-0.007 (0.018)	0.022 (0.015)	-0.012 (0.015)
College degree	-0.003 (0.013)	-0.014 (0.023)	0.016 (0.023)	-0.014 (0.016)
<i>N</i>	7,630	6,578	4,956	9,252
Employed	-0.010 (0.014)	0.018 (0.014)	-0.006 (0.011)	0.016 (0.016)
<i>N</i>	13,668	12,708	12,775	13,601
Natural log of wages	0.050* (0.026)	0.036 (0.028)	0.047** (0.023)	0.044 (0.029)
<i>N</i>	9,052	8,227	8,474	8,805

Notes: Each cell reports the reduced form estimate of the impact of the reform on the corresponding outcome. Estimates are taken from separate local linear RD regressions using a bandwidth of 15 months and a triangular kernel. All columns include individuals born within 15 months on either side of the January 1, 1966 cutoff. Columns 1-4 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. All samples are restricted to individuals who are born in France, and include those whose parents are born in and outside of France. Education data are taken from the survey “Enquête sur la Famille et les Logements” and labor market data are from the French labor force survey. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A15: Effects of the Reform on Highest Degree Received, RD-DID Estimates with Controls

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
No schooling	-0.002 (0.003)	-0.006 (0.004)	0.002 (0.005)	-0.001 (0.006)	-0.003 (0.004)
<i>CAP</i> or some middle school	-0.071*** (0.027)	-0.114*** (0.040)	-0.021 (0.035)	-0.166*** (0.049)	-0.020 (0.032)
Some middle school	-0.044** (0.018)	-0.040 (0.028)	-0.049** (0.021)	-0.058* (0.030)	-0.036 (0.022)
<i>CAP</i>	-0.027 (0.024)	-0.075** (0.036)	0.028 (0.030)	-0.108** (0.046)	0.016 (0.027)
Middle school	0.006 (0.014)	0.013 (0.019)	-0.000 (0.019)	0.008 (0.021)	0.005 (0.017)
<i>BEP</i>	0.003 (0.022)	0.007 (0.032)	-0.003 (0.031)	0.018 (0.037)	-0.005 (0.028)
Technical degrees	0.071*** (0.027)	0.079** (0.036)	0.063 (0.040)	0.056 (0.045)	0.077** (0.034)
Baccalaureate degree	-0.001 (0.018)	0.018 (0.022)	-0.023 (0.029)	0.037 (0.025)	-0.021 (0.024)
College degree	-0.005 (0.020)	0.004 (0.020)	-0.018 (0.037)	0.049 (0.035)	-0.034 (0.025)
<i>N</i>	24,593	13,297	11,296	8,490	16,103

Notes: Each cell reports the reduced form estimate of the impact of the reform on the corresponding outcome. Estimates are taken from separate RD-DID regressions with controls. Column 1 includes all individuals born within 6 months on either side of January 1 in the years 1964 to 1968. Further, samples are restricted to individuals who are born in France and whose parents are born in France. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. Controls include dummy variables for whether the father is in a low-skilled occupation (except in columns 2 and 3) and for whether the individual is male (except in columns 4 and 5). Data are taken from the survey “Enquête sur la Famille et les logements”. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A16: Effects of the Reform on Labor Market Outcomes, RD-DID Estimates with Controls

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
Employed	-0.005 (0.015)	-0.026 (0.021)	0.020 (0.022)	-0.028 (0.018)	0.020 (0.025)
<i>N</i>	43,579	22,497	21,082	21,030	22,549
Natural log of wages	0.093** (0.030)	0.103** (0.041)	0.088** (0.044)	0.110*** (0.036)	0.078* (0.048)
<i>N</i>	28,834	15,023	13,811	14,043	14,791

Notes: Each cell reports the reduced form estimate of the impact of the reform on the corresponding outcome. Estimates are taken from separate RD-DID regressions with controls. Column 1 includes all individuals born within 6 months on either side of January 1 in the years 1964 to 1968. All samples are restricted to individuals who are born in France, whose fathers are born in France and who are aged between 40 to 45 at the time of the survey. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. Controls include age, quarter and year of survey fixed effects and dummy variables for whether the father is in a low-skilled occupation (except columns 2 and 3) and for whether the individual is male (except columns 4 and 5). Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (***) p < 0.01 ** p < 0.05 * p < 0.1).

Table A17: Returns to Degrees and Effect of the Reform on Years of Education

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
A) Returns to Degrees, OLS					
Some middle school	-0.019 (0.039)	-0.033 (0.053)	0.009 (0.056)	-0.052 (0.047)	0.020 (0.062)
<i>CAP</i>	0.158*** (0.013)	0.158*** (0.016)	0.159*** (0.024)	0.143*** (0.013)	0.176*** (0.025)
Middle school	0.215*** (0.018)	0.182*** (0.024)	0.258*** (0.029)	0.222*** (0.022)	0.228*** (0.029)
<i>BEP</i>	0.238*** (0.016)	0.235*** (0.021)	0.245*** (0.026)	0.223*** (0.017)	0.265*** (0.027)
Technical degrees	0.493*** (0.014)	0.509*** (0.017)	0.485*** (0.023)	0.435*** (0.016)	0.552*** (0.024)
Baccalaureate degree	0.448*** (0.020)	0.440*** (0.030)	0.457*** (0.023)	0.411*** (0.027)	0.488*** (0.029)
College degree	0.818*** (0.016)	0.787*** (0.024)	0.834*** (0.025)	0.784*** (0.020)	0.861*** (0.026)
<i>N</i>	14,450	7,539	6,911	7,081	7,369
B) Years of Education					
RD estimate	0.329** (0.136)	0.369* (0.194)	0.189 (0.179)	0.509** (0.229)	0.234 (0.169)
<i>N</i>	12,240	6,589	5,651	4,221	8,019
RD-DID estimate	0.494** (0.203)	0.559* (0.292)	0.374 (0.263)	0.834** (0.346)	0.307 (0.251)
<i>N</i>	24,593	13,297	11,296	8,490	16,103

Notes: In panel A, each cell reports OLS estimates of the correlations between the likelihood of holding different types of degrees and log of wages, with no schooling as the omitted category. Corresponding regressions also include dummy variables for being male, for having a father in a low-skilled occupation, and fixed effects for age, year and quarter of survey. Column 1 consists of individuals who are born in France, whose parents are born in France, who are aged 40-45 at time of the survey, and are observed within 15 months on either side of the reform cutoff. Data are taken from the French labor force survey.

Panel B reports the RD and RD-DID estimates of the effect of the reform on years of education. RD estimates are taken from local linear regressions using a bandwidth of 15 months and a triangular kernel. For RD-DID, column 1 includes individuals born within 6 months on either side of January 1 in the years 1964 to 1968. Column 1 further includes all individuals who are born in France and whose parents are born in France. Data are taken from the survey “Enquête sur la Famille et les logements”. For all panels, Columns 2-5 respectively restrict the sample in column 1 to low SES individuals, high SES individuals, men and women.⁷³ Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).

Table A18: Effect of the Reform on Occupational Choice

	Overall (1)	Low SES (2)	High SES (3)	Men (4)	Women (5)
Unskilled occupation	-0.028*** (0.008)	-0.035*** (0.013)	-0.022** (0.009)	-0.040*** (0.012)	-0.014 (0.010)
Low or middle-skilled occupation	0.046*** (0.015)	0.078*** (0.022)	0.012 (0.020)	0.063*** (0.020)	0.024 (0.022)
Other occupation	-0.017 (0.016)	-0.044* (0.023)	0.009 (0.021)	-0.023 (0.021)	-0.010 (0.023)
<i>N</i>	18,768	9,591	9,177	9,644	9,124

Notes: Each cell reports the reduced form estimate of the impact of the reform on the probabilities of being in different types of occupations. Estimates are taken from separate local linear RD regressions using a bandwidth of 15 months and a triangular kernel, with controls. Column 1 includes all individuals born in France, whose parents are born in France and who are aged between 40 and 45 at the time of the survey. Columns 2-5 respectively restrict the sample to low SES individuals, high SES individuals, men and women. Low SES individuals are those whose fathers are in low-skilled occupations, while high SES individuals are those whose fathers are in middle or high-skilled occupations. Controls include month of birth, age, quarter and year of survey fixed effects and dummy variables for whether the father is in a low-skilled occupation (except in columns 2 and 3) and for whether the individual is male (except in columns 4 and 5). Data are taken from the French labor force survey. Robust standard errors are reported in parentheses. (** p < 0.01 ** p < 0.05 * p < 0.1).