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ABSTRACT

This paper studies how the type of education pursued at an early age affects family formation. I focus on a French reform that delayed the age of which students were tracked into either general or vocational education from age 11 to age 13. For the most part, tracking was replaced with grouping students into classrooms based on ability, but within a common general education curriculum. Using a regression discontinuity design, I show that the reform increased the likelihood of attaining a technical rather than a vocational degree, especially for individuals from low socioeconomic backgrounds. This indicates that the reform led to an increase in the quality of education. I further find that the reform increased completed fertility for individuals from low socioeconomic backgrounds, particularly women. In the marriage market, the reform changed the characteristics of women's partners without impacting marriage, cohabitation, or divorce rates. Specifically, women were more likely to have partners who were in high-skilled occupations and who were closer to their own ages. Taken together, these findings highlight that delaying early school tracking has significant consequences for family formation.

JEL Classification Codes: I21, I28, J24

Key Words: tracking, returns to education, fertility, marriage

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1 Introduction

One of the most consequential educational disparities between OECD countries is the degree to which students are sorted or tracked based on their academic achievement (OECD, 2013). In many European countries, such as Germany, students are divided into distinct school tracks. These tracks typically offer either academic or vocational curricula and hence lead to different types of degrees and career outcomes. On the other end of the spectrum are countries like the United States and Canada, which group students into achievement-based classrooms within comprehensive academic schools (Betts, 2011). Tracking has important implications for intergenerational income mobility, as low-income students are often disproportionately assigned to low-achieving tracks, which may in turn hinder their labor market outcomes (Dustmann, 2004). As such, a large body of work has focused on estimating the labor market returns to different tracks (Dustmann, Puhani, and Schönberg, 2017; Silliman and Virtanen, 2022) or the returns to delaying the age at which students are tracked (Malamud and Pop-Eleches, 2010; Canaan, 2020).

Yet much less is known about the relationship between school tracking and nonpecuniary outcomes such as family formation. This is despite a well-established theoretical link between investment in schooling and decisions about marriage and fertility (Becker, 1960; Willis, 1973; Chiappori, Iyigun, and Weiss, 2009). Furthermore, understanding the determinants of such decisions is crucial, as they have broad socioeconomic consequences. For example, assortative mating has been shown to contribute to household income inequality both in the United States and in Europe (Eika, Mogstad, and Zafar, 2019). Having children explains a significant share of gender inequality in the labor market—known as the "child penalty"—across a variety of developed countries (Kleven et al., 2019a and 2019b).

This paper aims to fill this gap in the literature by studying how delaying the age at which students are tracked into general and vocational education affects their long-term fertility and marriage outcomes. I focus on the French educational system, which divided middle school students into achievement-based tracks at age 11. High-achieving students were placed in a common middle school academic track. They then pursued either academic or technical high school degrees, which made them eligible to enroll in postsecondary studies. On the other hand, low-achieving students were assigned to a vocational education track, where they were trained for a specific occupation

with the goal of incorporating the labor market after high school. An issue with this system was that parental income was a strong predictor of track placement, as the majority of students in the vocational track came from low-income families. As such, starting in the academic year 1977–1978, the French government implemented a reform that eliminated tracking at age 11, introduced a common academic curriculum, and intended to create mixed-ability classrooms for all four years of middle school. In practice however, some students could still be placed in vocational education after two years in middle school, and a large share of students were divided into classrooms based on their abilities within the new comprehensive middle school system.[1] As such, the reform did not completely abolish middle school tracking. Instead, it replaced it with a system wherein students were grouped into achievement-based classrooms for the first two years, after which they could still be separated into general and vocational education.

To identify causal effects, I leverage the fact that the first cohort of students to be affected by the reform were those born on or after January 1, 1966.[2] I therefore use a regression discontinuity design that compares individuals who were born just after that date and went through the newly unified middle school system, to those who were born just before that date and were instead tracked at age 11. I dispel potential concerns that effects are driven by the age-at-school-entry policy in two ways.[3] I first show that being marginally born on either side of January 1 in years other than 1966 generates no significant discontinuities in key outcomes. I further show that my regression discontinuity estimates are in line with those from a difference-in-discontinuity design that uses, as a control group, individuals born on either side of the January 1 cutoff but in the four years preceding 1966.

I first find that the reform changed the type of degrees that individuals attain. Specifically, it increased the share of individuals who hold technical degrees—i.e., degrees that are typically accessed through the high track. It also simultaneously decreased the likelihood of individuals holding vocational degrees or leaving middle school without a degree—which is the typical trajectory of students in the low track. Effects are concentrated among both men and women from low socioeconomic backgrounds (low SES). These findings are consistent with a previous study (Canaan,

[1] In the academic year 1979–1980, only 45 percent of classrooms were considered mixed-ability (Lewis, 1985).

[2] This is due to the timing of the reform in combination with the school entry cutoff.

[3] In France, students born between January and December of the same year start school in the same academic year.

2020), which further documents that the reform increased long-term earnings. Next, I show that while the reform had no significant impact on fertility in the overall sample, it did increase low-SES individuals' total number of children at age 42 by 0.211, or 11 percent. This effect is driven by both low-SES men and women. On the other hand, the reform had no significant effect on teenage fertility and age at first childbirth. Turning to the mating market, the reform did not affect the likelihood of being married or cohabiting at age 44. However, as a result of the reform, women are 7.3 percentage points or 18 percent more likely to have a partner who is in a high-skilled occupation. Women's spouses are also 14 months closer to their age as a result of the reform.

This paper provides new causal evidence on the link between reducing early school tracking and family formation. It therefore relates to a long body of work looking at the consequences of reforms that delay or abolish tracking into general and vocational education, or increase the academic content of the vocational track (Pekkarinen, Uusitalo, and Pekkala Kerr, 2009; Malamud and Pop-Eleches, 2010 and 2011; Hall, 2012; Pekkala Kerr, Pekkarinen, and Uusitalo 2013; Zilic, 2018; Canaan, 2020; Bertrand, Mogstad, and Mountjoy 2021).

This paper adds to this literature in several ways. First, previous studies focus almost exclusively on how these types of reforms affect educational attainment and labor market outcomes. This paper instead highlights that the long-term impacts of reducing tracking also extend to other consequential outcomes, such as fertility and spousal characteristics. Two previous studies link family outcomes to educational reforms that increased the academic content of upper-secondary vocational tracks and made vocational students eligible for university studies. Bertrand, Mogstad, and Mountjoy (2021) find that Norway's Reform 94 had no effect on teenage fertility and likelihood of marriage, but did delay women's age at first marriage.[4] Grönqvist and Hall (2013) show a reduction in Swedish women's fertility at age 20 but not at age 32. In contrast to these studies, the French reform substantially impacted long-term completed fertility and spousal characteristics. A potential explanation for these contrasting results is that the French policy was more intensive, as it abolished the distinction between academic and vocational tracks (as opposed to increasing the academic content of the vocational curriculum) and was implemented at an earlier age (age 11 versus 16). This is in line with a long body of work emphasizing that early life experiences may be

[4]Their results also suggest that disadvantaged women were more likely to marry college-educated men, but their estimates are not statistically significant.

more consequential for later life outcomes (Cunha and Heckman, 2007).

Second, a unique feature of the French reform is that it does not completely eliminate tracking. Instead, it replaces the separation of students into academic and vocational tracks with grouping students into ability-based classrooms. This paper thus provides evidence on the implications of moving from the rigorous tracking system traditionally found in Europe to a more flexible system akin to the one prevalent in the United States and Canada. Third, a key insight of this paper is that while women in particular may not necessarily realize labor market gains from reducing tracking, they can instead benefit from these policies in the marriage market. Indeed, previous studies tend to find that any labor market returns from vocational education reforms are typically concentrated among men (Malamud and Pop-Eleches, 2010; Canaan, 2020; Bertrand, Mogstad, and Mountjoy, 2021).

More broadly, this paper relates to a long line of empirical work focusing on the role of education in decisions about family. Much of the quasi-experimental evidence comes from reforms that mandate increases in compulsory school-leaving age (Black, Devereux, and Salvanes, 2008; Monstad, Propper, and Salvanes, 2008; Cygan-Rehm and Maeder, 2013; Fort, Schneeweis, and Winter-Ebmer, 2016; Geruso and Royer, 2018). This allows studies to causally identify the impact of increasing the *quantity* of education on family formation. In contrast, this paper focuses on a reform that changed the *quality* or type of education, as it reduced the intensity of tracking but did not mandate changes in the number of required years of schooling.

Finally, this paper adds to a growing literature looking at how the quality or type of education affects marriage and fertility. Previous studies show that attending elite colleges decreases marriage rates and increases spousal quality, but has no effect on completed fertility (Ge, Isaac, and Miller, 2022; Kaufmann, Messner, and Solis, 2015). On the other hand, enrolling in preferred or selective secondary schools has been shown to reduce both teenage fertility (Beuermann and Jackson, 2022) and completed fertility (Clark and Del Bono, 2016). This paper complements this literature through its focus on different dimensions of educational quality—that is, general versus vocational education.

The rest of this paper is organized as follows. Section 2 describes the institutional setting. Section 3 outlines the data I use. Section 4 discusses the identification strategy. Section 5 presents the main results and robustness checks. In Section 6, I conclude.

2 Institutional Background

2.1 School System Prior to the Reform

Students in France start mandatory primary education in September of the year in which they turn six. Students are required to complete five years of general education in primary school, after which they can enroll in middle school. The reform targeted the structure of the middle school system. Figure 1 shows how middle schools operated prior to the reform. Starting with their first year of middle school, students aged 11 were placed in one of two tracks following the evaluation of their academic file by a committee comprised of teachers, a guidance counselor, and parent representatives. The academic file included students' grades, class rank, behavioral assessment, medical history, and interests (Hall, 1976).[5] High-achieving students were typically placed in the high track for the entire duration of middle school (four years). Low-performing students were placed in the low track for an initial period of two years, after which they were eligible to transfer to the high track, subject to excellent academic performance.[6] However, in practice, only about 10 percent of low-track students were able to switch to the high track (Defresne and Krop, 2016). The rest either continued in the low track or were placed in preapprenticeship classes if they had poor academic performance.[7]

The middle school tracks played a key role in determining students' future studies. Upon finishing middle school, both high- and low-track students were again assigned to different types of high school studies, based on their middle school performance. High-track students could be placed in general education, allowing them to graduate with a general baccalaureate degree (Baccalauréat Général), which is a prerequisite for accessing universities. High-track students could also be placed in technical education in order to obtain a Technician's Baccalaureate degree (Baccalauréat de Technicien). This allowed them to enroll in two-year postsecondary technical institutes, which eventually led to jobs such as senior technicians and technologists (Dundas-Grant 1987). Last, high-track students could pursue three years of vocational high school studies, which led to the

[5] If parents appealed the committee's decision, the student had to take an exam that determined his or her final track placement.

[6] Approximately 18 percent of middle school students were assigned to the low track from 1974 to 1976 (Defresne and Krop, 2016).

[7] Between 1974 and 1976, preapprenticeship classes enrolled around 34 percent of low-track students (Defresne and Krop, 2016).

Brevet d'Etudes Professionnelles (BEP) degree and trained them to work in certain fields.

High school options for low-track students were quite limited. These students were typically placed in vocational high schools for two years, after which they were awarded a trade certificate (Certificat d'Aptitude Professionnel, or CAP).[8] The CAP offers more specialized training than the BEP, as it trains students to work in a specific occupation as opposed to different occupations within a field.[9] Last, some low-track students with poor academic performance were placed in transitional classes until they reached the compulsory school leaving age of 16 and were not awarded any degrees.

2.2 The Haby Reform

A major issue with the prereform middle school system is that low-income students were disproportionately assigned to the low track.[10] In response to this issue, on July 11, 1975, the minister of education, René Haby, issued a series of proposals aimed at unifying the middle school system. The contents of the proposals were further detailed in 1977, and the new unified middle school or *collège unique* came into effect in the academic year 1977–1978.

Figure 2 shows the educational system after the reform. The main purpose of the reform was to abolish tracking within middle school. As such, it eliminated the high and low tracks, and all students were now required to follow a general education curriculum throughout the four years of middle school. The reform did, however, keep preapprenticeship classes, which some low-performing students could still be placed in after two years in middle school. This implied that middle school tracking was not completely abolished but instead was delayed by two years. The reform did not change the number of years of education nor the structure of primary schools and high schools.

The elimination of the low and high tracks implied that there were several important changes to the middle school experience. Perhaps the most consequential change is that the reform altered the composition of peers within classrooms. The reform intended to create mixed-ability classrooms within the newly unified middle schools. By 1979–1980, 45 percent of classrooms were considered mixed-ability (Lewis, 1985). Many schools, however, still grouped students based on their

[8] Students in preapprenticeship classes also eventually obtained a CAP. However, their training was done in apprenticeship centers as opposed to vocational high schools.

[9] For example, a CAP student wanting to work in the hotel and catering sector would need to obtain a certificate for a specific job within that sector, such as a catering worker's CAP or a cellarman's CAP. On the other hand, one can pursue a BEP degree in hotel and catering that allows work in both of these occupations (Dundas-Grant, 1987).

[10] Defresne and Krop (2016) estimate that in the few years preceding the reform, around 72 percent of students in the low track were from low-income families.

abilities, as teachers and principals were strongly opposed to mixed-ability classrooms. Another way students would be sorted based on their abilities was through the choice of foreign languages: High-performing students would be encouraged to take German in their first year and Latin or ancient Greek during their third year (see Landrier and Nakhili, 2010). As such, while a significant share of students were in mixed-ability classrooms after the reform, many were still grouped into achievement-based classrooms.

Another major change brought on by the reform was the introduction of a new general education curriculum common to all students. Table A1 reports the number of weekly hours allocated to each subject under the old and new curricula. For students who would have been placed in the high track prior to the reform, the new curriculum decreased the number of weekly hours devoted to mathematics, French, history/geography/civics, arts, and physical education in the first year of middle school. It instead increased time for sciences and introduced manual activities. For students who would have been assigned to the low track prior to the reform, the new curriculum reduced the number of hours allocated to manual activities, sciences, mathematics, and French. Instead, these students would take an additional foreign language or an ancient language. Last, the reform reduced class sizes to 24 students and likely changed the type of teachers that students were exposed to. Prior to the reform, high-track teachers typically had higher educational credentials than low-track teachers. Specifically, the former had college degrees while the latter either received on-the-job training or held two-year postsecondary degrees. The reform assigned low-track teachers to teach the first two years of middle school, while high-track teachers were given the last two years.[11]

3 Data

Data on education, marriage, and fertility are taken from the "Enquête sur la Famille et les Logements." This survey was administered to either all women or all men, at least 18 years of age, and living in households that were part of the 2011 census. Since the survey's goal was to collect information on fertility and family life, women were intentionally oversurveyed, so the initial data set contains data from 121,312 men and 238,458 women. Crucial to my analysis, the data contain each individual's month and year of birth, allowing me to use a regression discontinuity design that

[11] Between 1975 and 1978, around 5,200 new teachers with college degrees were hired to meet the demands of the reform. Canaan (2020) shows that the hiring of new teachers had no significant impact on students' outcomes.

compares outcomes of individuals born marginally on either side of January 1, 1966. As detailed in Section 4, my main sample includes all individuals born within 15 months on either side of this cutoff.

For the main sample, Table 1 reports means of all available baseline covariates and main outcomes. Around 81 percent of respondents have parents who are born in France. Canaan (2020) finds that the reform did not affect the educational outcomes of children of immigrants. As such, I restrict my main sample to individuals whose parents were born in France. Following this restriction, 66.4 percent of respondents are women—as is consistent with women being oversampled in the survey. Some 41.5 percent of respondents have fathers who are low-skilled workers, while 12.6 percent report having a mother who is a low-skilled worker.

I use this survey to look at three sets of outcomes, for which I report means in Panels B through D of Table 1. My first set of outcomes captures individuals' highest degree received. Canaan (2020) shows that the reform led to an increase in the share of technical degrees (i.e., Technician's Baccalaureate and postsecondary technical degrees), which are typically accessed through the high track. She also reports a decrease in the share of individuals who either hold a CAP degree or left middle school without a degree—the typical path for students from the low track—but finds no changes in attaining any other type of degree. As such, in my analysis, I focus solely on whether the reform affected the likelihood of having a technical degree, a CAP, or leaving middle school without a degree, and I group all other degrees in one outcome. Panel A and column (1) of Table 1 show that 28.5 percent of individuals in my main sample hold a technical degree, and 27 percent either have a CAP or left middle school without a degree. These numbers, however, mask considerable heterogeneity. Columns (2) and (3) show that 39.5 percent of individuals from low socioeconomic backgrounds hold a CAP or left middle school without a degree, while only about 24 percent hold technical degrees.[12] On the other hand, only about 18 percent of individuals from high socioeconomic backgrounds hold a CAP or left middle school without a degree. Columns (4) and (5) further show that women are more (less) likely than men to hold a technical degree (CAP or no degree).

[12]The survey reports fathers' occupations using a two-digit number which can be used to identify their socioeconomic status by looking at the first digit. Individuals from low socioeconomic backgrounds (low SES) are defined as those whose fathers were manual workers (or *ouvriers*), while all individuals whose fathers were in middle- or high-skilled occupations are classified as high SES.

The survey reports detailed information on individuals' fertility, such as the number of children, but also the date of birth of each child. In my analysis, I first look at teenage fertility, which I define as the total number of children the respondent has by age 19. An advantage of the survey is that it allows me to observe fertility when individuals are in their 40s. For individuals born within six months on either side of the cutoff, I can observe their fertility up until age 44. However, my main analysis sample includes individuals who are born within 15 months on either side of the cutoff. To allow all individuals in the main sample to be observed at the same age, I focus on fertility at age 42. I do, however, show that the reform has a similar effect on fertility at age 44 when using individuals born within six months on either side of the cutoff.

Panel C of Table 1 reports means for the main fertility outcomes. Compared to the high SES, low SES individuals have, on average, a higher number of children, both in their teenage years and by age 42 (around 1.85 versus 1.80 children at age 42), and have their first child earlier (at age 28.8 versus 30.2). Similarly, women have a higher number of children than men (1.90 versus 1.65 children by age 42) and at an earlier age (at 28.87 versus 31.27). My analysis also looks at marital outcomes for women and men separately. Panel D of Table 1 shows that around 76 percent of both men and women are either married or cohabiting at age 44. As a measure of spousal quality, I look at the likelihood that the respondent's partner is in a high- or middle-skilled occupation.[13] In my main sample, around 54.0 and 43.6 percent of women and men have a high-skilled partner, respectively. Finally, looking at the age gap between partners, women in my sample are around 30.2 months younger than their partners, while men are around 15.8 months older.

4 Empirical Strategy

The setting I focus on lends itself naturally to a regression discontinuity design (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). Specifically, the reform was implemented in the academic year 1977–1978, and students were required to spend five years in primary schools, starting the year in which they turned six. As a result, it affected individuals born on or after January 1, 1966. I can therefore find the causal effect of the reform by comparing outcomes of individuals marginally

[13] High- or middle-skilled occupations include three socioeconomic categories: 1) artisans, commerçants et chefs d'entreprise; 2) cadres et professions intellectuelles; and 3) professions intermédiaires. These include businessmen, doctors, engineers, judges, lawyers, managerial positions, teachers, technicians, etc.

born on either side of the January 1, 1966, cutoff. I estimate local linear regression discontinuity (RD) regressions with a triangular kernel that takes the following form:

$$Y_i = \alpha + \beta D_i + \gamma R_i + \tau R_i \times D_i + \epsilon_i, \tag{1}$$

where Y is one of various outcomes for individual i. D is a dummy variable that equals 1 if the individual is born on or after January 1, 1966, and 0 otherwise. R is the running variable. It is defined as each individual's month-year of birth relative to the cutoff, so that R takes on the value of 0 for those born on January 1, 1966. To allow for differential trends in the running variable on either side of the cutoff, I interact R with D. β is the parameter of interest, which captures the difference in outcomes between individuals born just after the cutoff (and thus were affected by the reform) and those born just before (and thus were not affected by the reform). Given that I do not have data to estimate the first stage, all RD estimates in this paper are intent-to-treat (ITT) effects of the reform.

Since the running variable is discrete, I follow Kolesár and Rothe (2018) and use robust standard errors. When using RD, another important choice is the preferred bandwidth. I use the robust data-driven procedure introduced by Calonico, Cattaneo, and Titiunik (2014) to guide my choice of bandwidth. Depending on the outcome of interest, this procedure picks bandwidths that are between 13 and 20 months. For consistency across outcomes, I present all main estimates using a bandwidth of 15 months. I do, however, show that my main estimates are robust to a variety of different bandwidths.

4.1 Identifying assumptions

The main assumption in an RD design is that individuals born on either side of the reform's cutoff are on average similar, and the only difference between them is that some were exposed to the reform and others were not. As such, any documented discontinuities in their outcomes can be interpreted as a direct result of the reform. For the identifying assumption to be valid, there has to be no manipulation or timing in month-year of birth around the reform's cutoff. Since the reform was announced years after the affected cohort was born, we should not expect to see sorting around the cutoff as a result of the reform. Nonetheless, I perform two formal tests of the validity

of the RD design. First, Figure A1 plots the number of individuals born in each month around the cutoff (where 0 is the cutoff) as a function of the running variable. If there was no manipulation of the running variable, we should see no discontinuity in the frequency of births at the cutoff (McCrary, 2008). The figure shows an increase in the number of births at the January 1, 1966, cutoff. However, this pattern is also observed in January of 1965 and 1967—i.e., in birth years of cohorts who were not exposed to the reform. This observation, along with the fact that the reform was not preannounced, indicates that the increase in number of births at the reform's cutoff is not due to timing but rather to seasonality in births.

Second, I show that there are no significant discontinuities at the reform's cutoff in baseline characteristics. The different panels of Figure A2 plot different baseline covariates as a function of distance of individuals' month-year of birth from the cutoff (where the cutoff is 0) and using one-month bins. These covariates include dummy variables that are equal to 1 if individuals are female, their parents were born in France, and their parents are in low-skilled occupations. No visible discontinuities at the cutoff are observed for any of these predetermined characteristics. Table A2 further reports corresponding RD estimates taken from Equation (1) using different bandwidths, including the preferred bandwidth of 15 months in column (3). In line with the visual evidence, no significant effects are detected for any of these baseline covariates. This indicates that individuals around the reform's cutoff are, on average, similar in observable characteristics, and it validates the RD's main identifying assumption.

5 Results

5.1 Effects of the Reform on Education

I start by providing evidence that the reform did indeed take effect in the year 1978 and hence mainly affected students born in 1966. Unfortunately, I do not have individual-level data on middle school track assignment. Instead, Defresne and Krop (2016) report the yearly aggregate number of students enrolled in the first two years of middle school and across different tracks.[14] Using these numbers, I plot the evolution of the number and share of students in the high and low tracks over time in Figure 3. The high track in this figure refers to the high track before the reform and to

[14]Their data are taken from the French Ministry of Education's annual census of educational establishments.

the common middle school after the reform. Since the reform was implemented in the academic year 1977–1978, we should see a significant decrease in the number of low-track students starting in 1977. Indeed, Panel (a) of Figure 3 shows that the number of students in the low track drops from 339,143 in 1976 to 167,827 in 1977 and then to 0 from 1978 onward. Turning to the shares of students in different tracks in Panel (b) of Figure 3, the impact of the reform is also striking: the share of students in the low track decreases from 15.9 percent in 1976 to 9.6 percent in 1977 and to 0 from 1978 onward. At the same time, the share of students in the high track goes up from 84.1 percent in 1976 to 90.4 percent in 1977 and 100 percent starting in 1978. Taken together, this indicates that the reform did indeed abolish the low track for the first two years of middle school starting with the academic year 1977–1978.

Although I do not have information on each individual's track assignment, I can observe their highest degree received. Canaan (2020) shows that the reform led to a decrease in the likelihood of having a CAP degree or of leaving middle school without a degree—i.e., the typical outcomes for students in the low track. She also reports that students are more likely to obtain technical degrees—which were typically accessed through the high track. For completeness, I rerun this analysis for the overall sample and all the subgroups that I focus on in this paper. The different panels of Figure 4 plot the likelihood of holding different types of degrees as a function of the running variable. For the overall sample, Panel (a) shows a clear positive discontinuity at the cutoff in the likelihood of having technical degrees, coupled, in Panel (b), with a drop at the cutoff in the probability of having a CAP or leaving middle school without a degree. This indicates that the reform led to a shift in the type of degree that individuals pursued and allowed them to access higher-quality degrees. Next, I plot the same educational outcomes but split the sample into individuals from low socioeconomic backgrounds (low SES) in Panels (c) and (d) and those from high socioeconomic backgrounds (high SES) in Panels (e) and (f). The figures indicate that the effects of the reform are concentrated among individuals from low socioeconomic backgrounds, as a clear positive (negative) discontinuity can be seen at the cutoff for technical degrees (CAP or some middle school), while no discontinuities are evident for the high SES. Table 2 reports the corresponding regression discontinuity estimates of the impact of the reform on highest degree received. Consistent with the graphical evidence, column (1) shows that the reform increases the probability of holding technical degrees by 6.8 percentage points for the overall sample. At the

same time, there is a 5.2 percentage point drop in the likelihood of having a CAP or leaving middle school without a degree, while no significant effects are detected for the likelihood of holding other degrees. For low SES in column (2), the reform leads to a 9.3 percentage point increase in technical degrees and an 8.5 percentage point drop in having a CAP or some middle school. On the other hand, we see no significant effects on the likelihood of holding other types of degrees for either the overall or the low SES samples. For the high SES, estimates are not statistically significant at conventional levels, but there seems to be a 4.6 percentage point increase in the share of technical degrees.

In subsequent analysis, I look at whether the reform affected men's and women's outcomes differently. As such, I report the impact of the reform on men's and women's educational outcomes in Table 3. Both men and women see an increase in the likelihood of having technical degrees. This increase is on the order of 5.5 percentage points for women and 9.3 percentage points for men (columns (1) and (4) in Table 3). Men are also 10.9 percentage points less likely to hold a CAP or leave middle school without a degree. While I detect no statistically significant impacts on this outcome for women, I cannot rule out a large decrease. As in the overall sample, this shift in the type of degree is concentrated among men and women from low SES backgrounds. Specifically, low SES women (men) are 8.5 percentage points (10.8 percentage points) more likely to hold a technical degree and are 6.5 percentage points (11.9 percentage points) less likely to hold a CAP or have some middle school. Taken together, these results confirm that the reform improved the level of education that individuals pursued by shifting them from degrees that were typically accessed through the low track (i.e., CAP degrees or leaving middle school without a degree) and into degrees that were typically obtained through the high track (i.e., technical degrees). This further suggests that the reform did indeed delay the assignment of students to the low track. The reform also benefited both men and women, especially those from low socioeconomic backgrounds who were disproportionately placed in the low track prior to the reform.

In Appendix Figure A3, I plot RD estimates taken from regressions with controls (including month-of-birth fixed effects) using different bandwidths. These estimates show that for the overall sample and the different subgroups (low and high SES, men and women), estimates are largely robust to the inclusion of controls and month-of-birth fixed effects, as well as across different bandwidths.

5.2 Fertility

As previously discussed, given that the reform increased affected cohorts' quality of education and earnings, it is possible that it also impacted their fertility decisions. In Figure 5 and Table 4, I report the RD estimates of the effects of the reform on various fertility outcomes. Previous studies show that reforms, which raised the compulsory school leaving age, reduced teenage childbearing (Black, Devereux, and Salvanes, 2008; Geruso and Royer, 2018). I therefore start by examining the impact of the reform on teenage fertility. Figure 5a plots the number of children by age 19 (between ages 15–19) as a function of the running variable for the overall sample. The graph is smooth around the cutoff, and the corresponding RD estimate in column (1) of Table 4 is also statistically insignificant and small in magnitude. Since the reform mostly benefited low SES individuals, I also examine fertility effects by socioeconomic background in Figures 5c and 5e and in columns (2) and (3) of Table 4. Table 5 further splits the sample by gender and socioeconomic background, as education may have a differential impact on men and women's fertility decisions. These results confirm that the reform had no significant impact on teenage childbearing for any subgroup, including the ones that most benefited from it.

I next examine whether the reform had an impact on completed fertility. In the overall sample, I document no statistically significant impact on the number of children at age 42 (Figure 5b and column (1) of Table 4), but the corresponding RD estimate is not precisely 0. Turning to individuals from low socioeconomic backgrounds, Figure 5d reveals a positive shift at the cutoff. The corresponding RD estimate in column (2) of Table 4 further indicates that the reform led to a statistically significant 0.211 increase in the number of children at age 42. On the other hand, no significant effects are detected for the high SES (Figure 5f and column (3) of Table 4). Splitting the sample by gender, this increase in long-term fertility is also apparent among low SES women who experience a significant 0.288 increase in the number of children (column (2) of Table 5). While the estimated effect for low SES men is not statistically significant at conventional levels, it is comparable in magnitude to the estimate for low SES women, and I cannot rule out large effects (column (5) of Table 5). This suggests that the reform led to an increase in both low SES men and women's fertility at age 42.

Next, I examine whether the increase in the total number of children is driven by an increase

in the likelihood of having at least one child or more than one child. Tables 4 and 5 indicate that the increase in cumulative fertility is driven by both margins. Low SES women are 6.1 percentage points more likely to have at least one child and 7.2 percentage points more likely to have more than one child as a result of the reform.

The reform may also have changed the timing of fertility. To evaluate this, I look at its impact on individuals' ages at the time of birth of their first child. RD estimates are reported in the last rows of Tables 4 and 5 for the overall sample and all different subgroups. Across different samples, there is no compelling evidence indicating that the reform affected age at first birth. Finally, in Panels (a) and (b) of Appendix Figure A4, I plot RD estimates of the reform's effects on low SES individuals' cumulative fertility at ages 19 and 42, taken from regressions with controls (including month-of-birth fixed effects) using different bandwidths. Importantly, the documented effects are insensitive to the choice of bandwidth and the inclusion of controls, confirming that the low SES experienced an increase in fertility at age 42 as a result of the reform, but no changes in teenage childbearing.

5.3 Marital Outcomes

I next examine whether the reform impacted the marriage market. Figure 6a shows no significant discontinuity at the cutoff in women's likelihood of being married or cohabiting at around age 44. The corresponding RD estimate in the first row and in column (1) of Table 6 is also small in magnitude and statistically insignificant. For men, the RD estimate in column (4) of Table 6 is larger in magnitude and on the order of 9.4 percentage points for low SES men—albeit it is only statistically significant at the 10 percent level. The corresponding RD Figure 6b also reveals a slight positive shift at the cutoff, suggesting that men are more likely to be married or cohabiting as a result of the reform. In the second row of of Table 6, I find that the reform had no significant effect on divorce rates for any group.

Despite the reform having no effect on couple formation for women, it is possible that the increase in quality of education affected the type of spouse or partner that they match with. I therefore focus on how the reform affected two spousal characteristics: 1) the likelihood that the spouse is in a high-skilled occupation, and 2) the age difference between the spouse and the individual affected by the reform. Results show that the reform seems to have affected the match

quality for women but not for men. Specifically, women are 7.3 percentage points more likely to have a spouse or partner who has a high-skilled occupation (Figure 6c and column (1), third row of Table 6). They also match with partners who are 14 months closer to their own age as a result of the reform (Figure 6e and column (1), fourth row of Table 6). Columns (2) and (3) of Table 6 further split the sample by socioeconomic background. Interestingly, results indicate that both low and high SES women experience similar changes in the characteristics of their spouses. On the other hand, I detect no statistically significant effects on men's spousal characteristics (Figure 6d, 6f, and column (4) of Table 6).

Panels (c) through (f) of Appendix Figure A4 plot RD estimates of the effects of the reform on marital outcomes using different bandwidths and with the inclusion of controls and month-of-birth fixed effects. Importantly, estimates are not sensitive to bandwidth choice and inclusion of controls: women are still significantly more likely to match with high-skilled partners and to match with spouses who are closer to their own age.

6 Conclusion

This paper provides new evidence on how changes in the quality of education at an early age impact long-term marriage and fertility. I focus on a French reform that delayed the placement of middle school students into general and vocational education by two years and instead allowed schools to group students into ability-based classrooms. I find that the reform changed the type of degree that individuals attain. Individuals exposed to the reform—especially those from low socioeconomic backgrounds—are more likely to hold technical rather than vocational degrees, which indicates an increase in their quality of education. Low SES individuals, particularly women, also experience an increase in their cumulative fertility measured in their mid-40s. Turning to the marriage market, women's likelihoods of being married or in a cohabiting relationship or divorced by their mid-40s are unchanged as a result of the reform. However, the reform does change the characteristics of their spouses/partners: these women are more likely to match with spouses/partners who are in high-skilled occupations and closer to their age. Taken together, these results highlight that the impacts of reducing the intensity of tracking at an early age extend beyond education and labor market outcomes to have significant implications for family formation.

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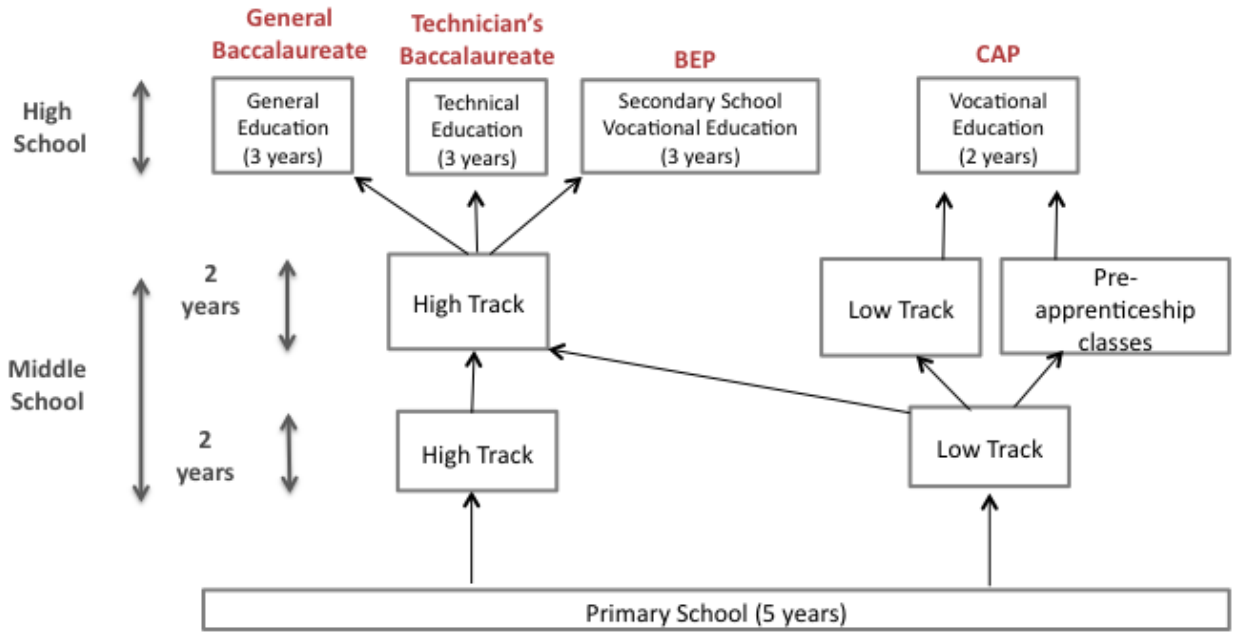
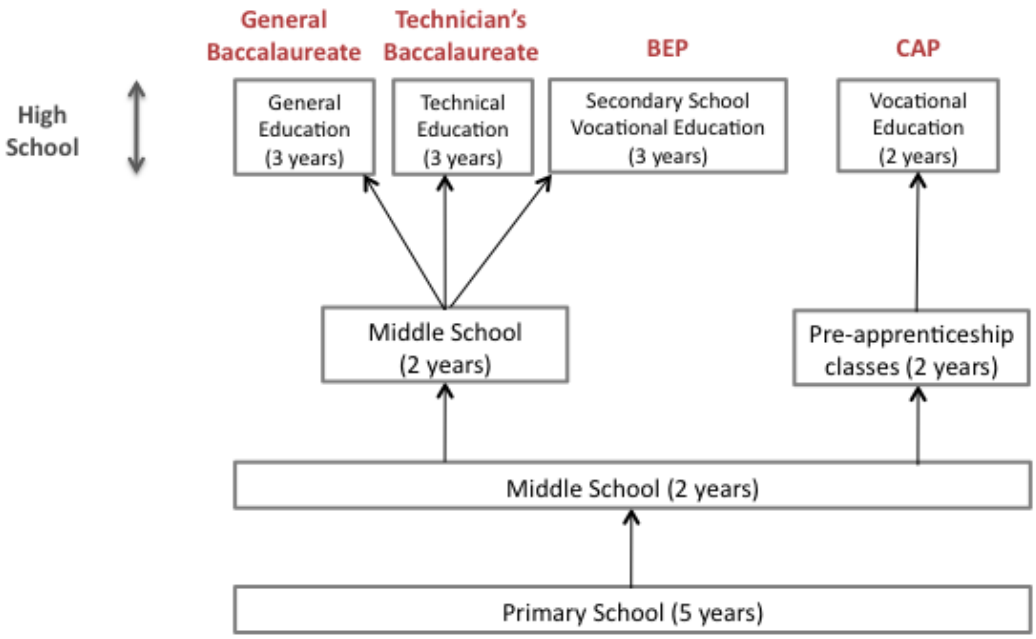
Figures and Tables

Figure 1: Educational System prior to the Reform



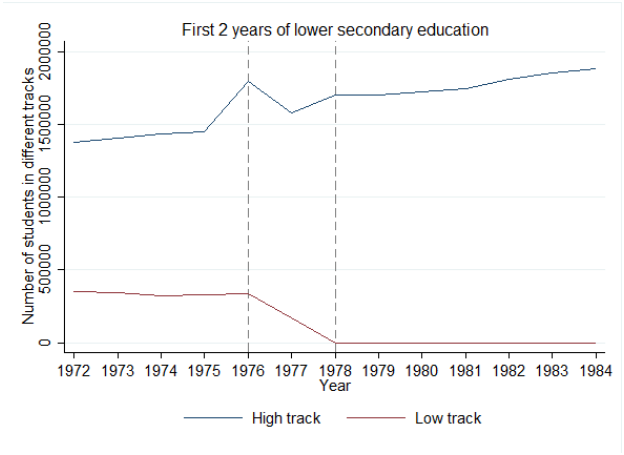
NOTE: This figure shows the structure of the French school system prior to the Haby reform. The degrees that students can obtain at the end of high school are denoted in red.

Figure 2: Educational System after the Reform

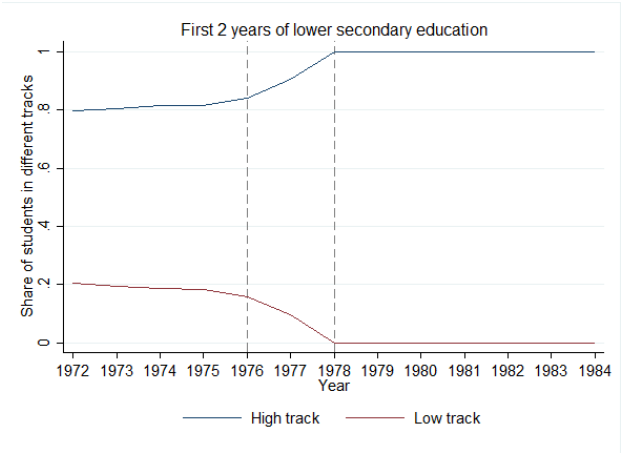


NOTE: This figure shows the structure of the French school system after the Haby reform. The reform did not change the organization of primary schools and high schools or the compulsory school leaving age. The degrees that students can obtain at the end of high school are denoted in red.

Figure 3: Number and Share of Students Enrolled in Different Tracks



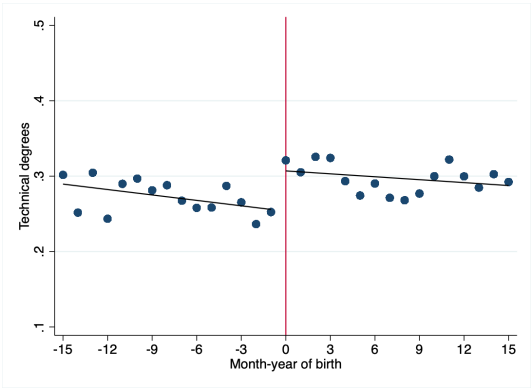
(a) Number of students in different tracks



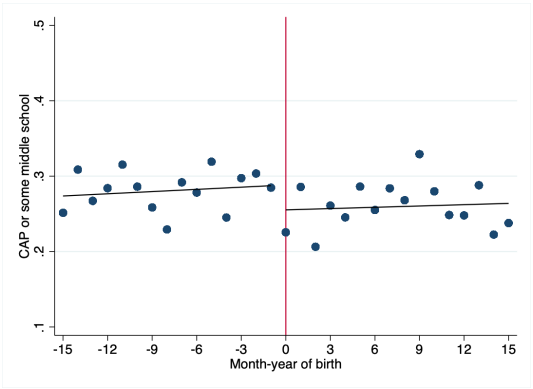
(b) Share of students in different tracks

NOTE: The different figures plot the evolution over time in the number and share of students in different tracks. Shares are computed based on the numbers in Panel (a). SOURCE: Defresne and Krop (2016).

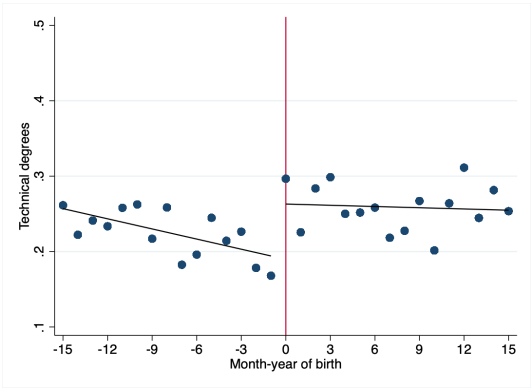
Figure 4: Effect of the Reform on Highest Degree Received



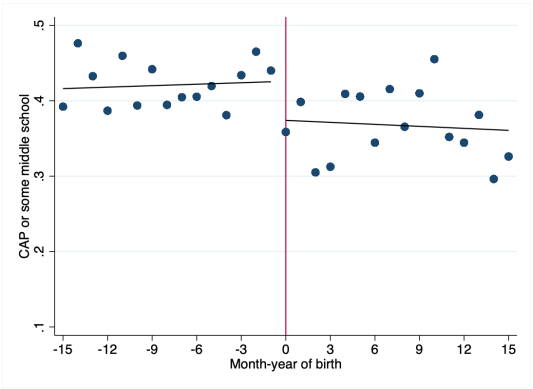
(a) All



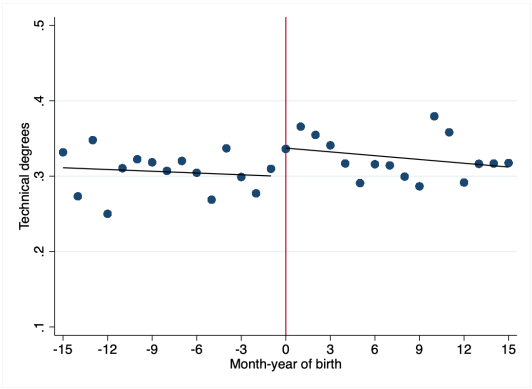
(b) All



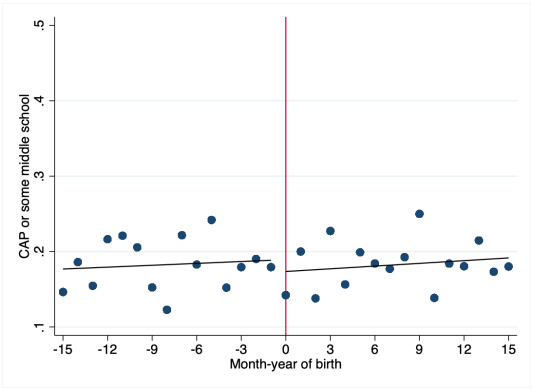
(c) Low SES



(d) Low SES



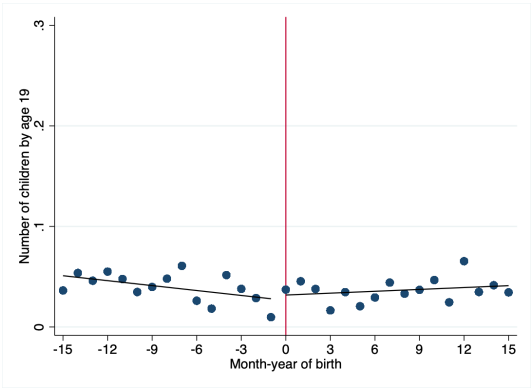
(e) High SES



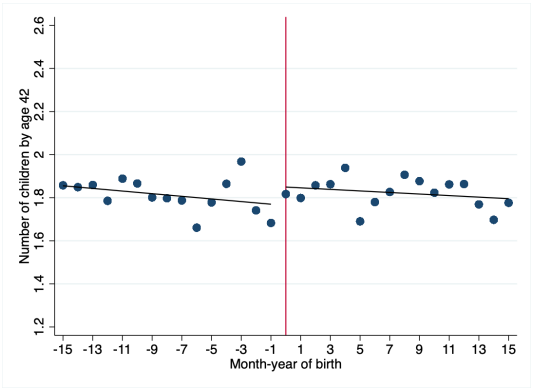
(f) High SES

NOTE: The different panels show the shares of individuals who hold different types of degrees as their last degree, 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. Panels (a) and (b) are for the overall sample. Panels (c)–(d) and (e)–(f) restrict the sample to individuals from low and higher socioeconomic backgrounds, respectively.

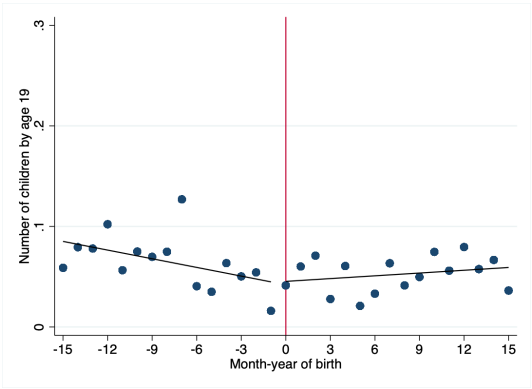
Figure 5: Effect of the Reform on Fertility



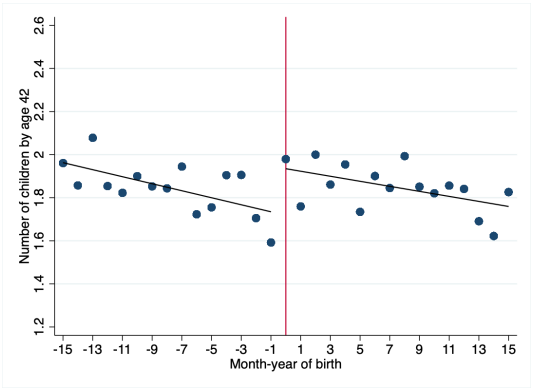
(a) All



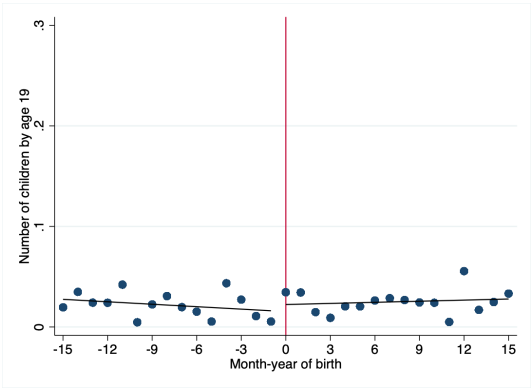
(b) All



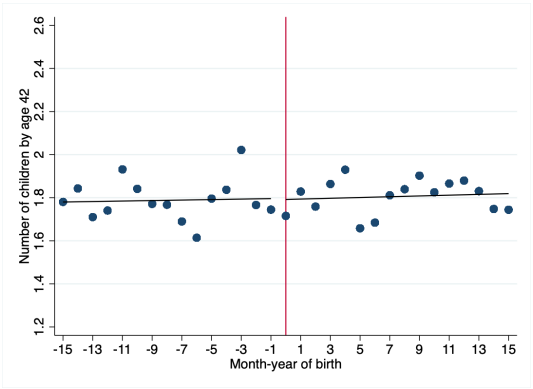
(c) Low SES



(d) Low SES



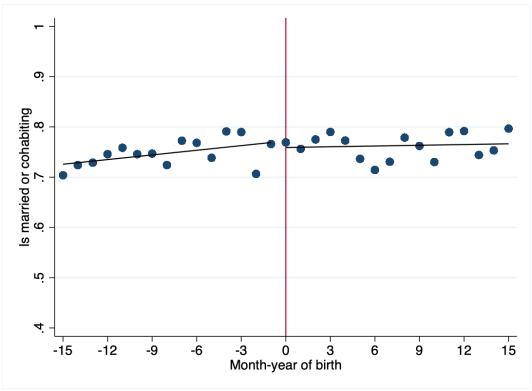
(e) High SES



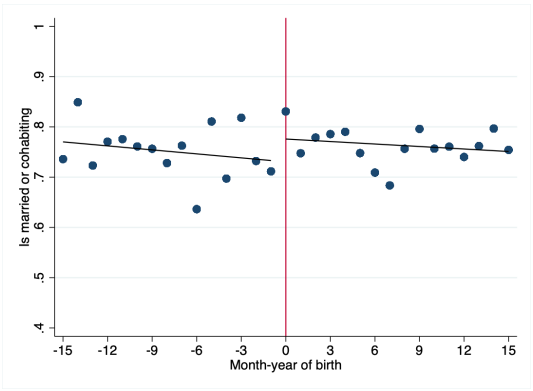
(f) High SES

NOTE: The different panels plot cumulative fertility at ages 19 and 42 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. Panels (a) and (b) are for the overall sample. Panels (c)–(d) and (e)–(f) restrict the sample to individuals from low and higher socioeconomic backgrounds, respectively.

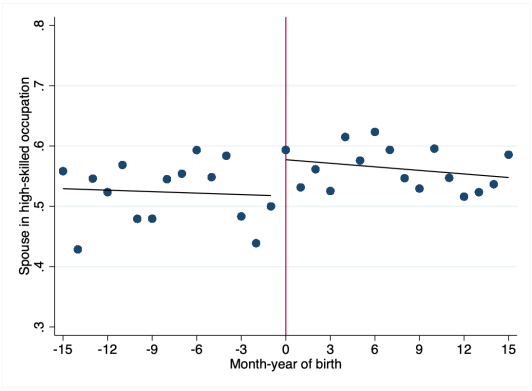
Figure 6: Effect of the Reform on Marital Outcomes



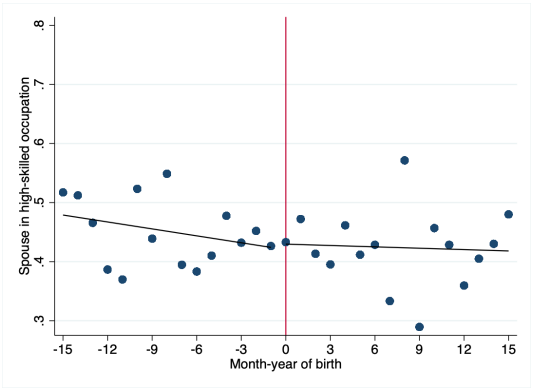
(a) Women



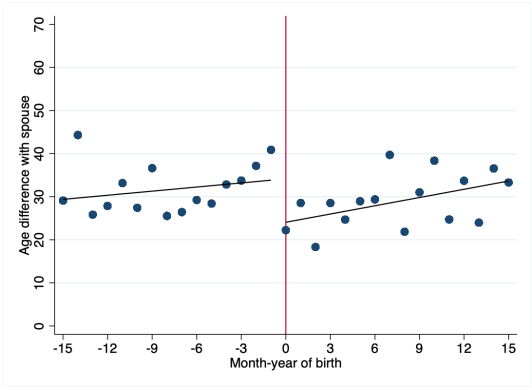
(b) Men



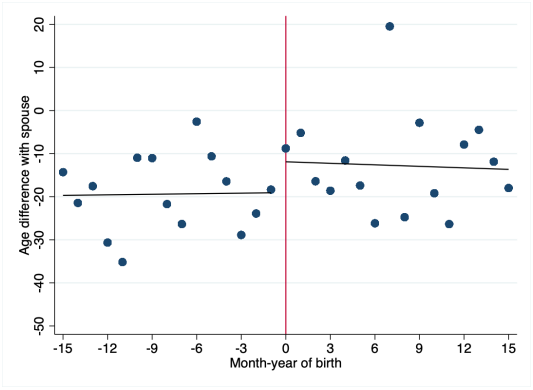
(c) Women



(d) Men



(e) Women



(f) Men

NOTE: The different panels show various marital outcomes as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one-month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Panels on the left restrict the sample to women, while those on the right show outcomes for men.

Table 1: Sample Means for Main Outcomes

	All (1)	Low SES (2)	High SES (3)	Women (4)	Men (5)
A) Background characteristics					
Mother is born in France	0.825	–	–	–	–
Father is born in France	0.810	–	–	–	–
N	12,935	–	–	–	–
Respondent is female	0.664	–	–	–	–
Mother is low-skilled worker	0.126	–	–	–	–
Father is low-skilled worker	0.415	–	–	–	–
N	10,110	–	–	–	–
B) Highest degree received					
Technical degrees	0.285	0.243	0.316	0.302	0.252
CAP or some middle school	0.270	0.395	0.182	0.234	0.344
Other degrees	0.444	0.362	0.502	0.464	0.404
N	10,110	4,194	7,934	6,716	3,394
C) Fertility					
Cumulative fertility by age 19	0.038	0.059	0.023	0.050	0.014
Cumulative fertility by age 42	1.819	1.849	1.800	1.901	1.657
Had at least one child by age 42	0.836	0.845	0.829	0.866	0.775
Had more than one child by age 42	0.660	0.658	0.661	0.684	0.612
N	10,110	4,194	5,916	6,716	3,394
Age at first birth	29.62	28.82	30.19	28.87	31.27
N	8,494	3,558	4,936	5,836	2,658
D) Marriage					
Married or cohabiting	–	–	–	0.754	0.758
Divorced	–	–	–	0.127	0.088
N	–	–	–	6,716	3,394
Spouse is in high-skilled occupation	–	–	–	0.542	0.436
N	–	–	–	4,896	2,344
Age gap (in months)	–	–	–	30.02	-15.84
N	–	–	–	5,081	2,624

NOTE: 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. The age gap is defined as the difference between the spouse's age and the respondent's age.

Table 2: Effect of the Reform on Highest Degree Received

	All (1)	Low SES (2)	High SES (3)
Technical degrees	0.068***	0.093***	0.046*
	(0.020)	(0.029)	(0.027)
CAP or some middle school	–0.052***	–0.085**	–0.018
	(0.020)	(0.034)	(0.022)
Other degrees	–0.015	–0.008	–0.028
	(0.022)	(0.034)	(0.029)
N	10,110	4,194	5,916

NOTE: 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) and (3), respectively, restrict the sample to individuals from low and higher socioeconomic backgrounds. 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 (* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$).

Table 3: Effect of the Reform on Highest Degree Received, Women and Men

	Women			Men		
	All (1)	Low SES (2)	High SES (3)	All (4)	Low SES (5)	High SES (6)
Technical degrees	0.055**	0.085**	0.028	0.093***	0.108**	0.078*
	(0.025)	(0.036)	(0.034)	(0.034)	(0.050)	(0.045)
CAP or some middle school	–0.023	–0.065	0.019	–0.109***	–0.119*	–0.085**
	(0.023)	(0.041)	(0.026)	(0.036)	(0.061)	(0.042)
Other degrees	–0.032	–0.020	–0.047	0.017	0.012	0.007
	(0.028)	(0.042)	(0.036)	(0.038)	(0.056)	(0.050)
N	6,716	2,861	3,855	3,394	1,333	2,061

NOTE: 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. Columns (1) and (4), respectively, include all women and all men born within 15 months on either side of the January 1, 1966, cutoff. Columns (2)-(3) and (5)-(6), respectively, restrict the sample to low SES women, high SES women, low SES men, and high SES men. 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 < 0.10; ** p <0.05; *** p <0.01).

Table 4: Effect of the Reform on Fertility

	All (1)	Low SES (2)	High SES (3)
Cumulative fertility by age 19	0.012	0.016	0.012
	(0.008)	(0.016)	(0.009)
Cumulative fertility by age 42	0.061	0.211**	–0.041
	(0.052)	(0.083)	(0.067)
one child by age 42	0.029*	0.045*	0.018
	(0.017)	(0.026)	(0.022)
more than one child by age 42	0.030	0.079**	–0.003
	(0.021)	(0.033)	(0.028)
at first birth	–0.448*	–0.603	–0.396
	(0.264)	(0.415)	(0.338)
N	10,110	4,194	5,916
(age at first birth)	8,494	3,558	4,936

NOTE: Each cell reports the reduced-form estimate of the impact of the reform on various fertility outcomes. 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) and (3), respectively, restrict the sample to individuals from low and higher socioeconomic backgrounds. 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 (* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$).

Table 5: Effect of the Reform on Fertility, Women and Men

	Women			Men		
	All	Low SES	High SES	All	Low SES	High SES
	(1)	(2)	(3)	(4)	(5)	(6)
Cumulative fertility by age 19	0.012	0.015	0.012	0.012	0.016	0.010
	(0.012)	(0.023)	(0.012)	(0.008)	(0.014)	(0.010)
Cumulative fertility by age 42	0.029	0.228**	–0.114	0.120	0.168	0.094
	(0.064)	(0.099)	(0.082)	(0.089)	(0.150)	(0.111)
Had a child by age 42	0.026	0.061**	0.001	0.032	0.007	0.050
	(0.020)	(0.031)	(0.026)	(0.032)	(0.049)	(0.041)
Had more than one child by age 42	0.008	0.072*	–0.040	0.074**	0.090	0.063
	(0.026)	(0.040)	(0.034)	(0.037)	(0.060)	(0.048)
Age at first birth	–0.246	–0.422	–0.138	–0.868*	–0.811	–1.011*
	(0.314)	(0.474)	(0.413)	(0.459)	(0.769)	(0.564)
N	6,716	2,861	3,855	3,394	1,333	2,061
N (age at first birth)	5,836	2,511	3,325	2,658	1,047	1,611

NOTE: Each cell reports the reduced-form estimate of the impact of the reform on various fertility outcomes. Estimates are taken from separate local linear RD regressions using a bandwidth of 15 months and a triangular kernel. Columns (1) and (4), respectively, include all women and all men born within 15 months on either side of the January 1, 1966, cutoff. Columns (2)–(3) and (5)–(6), respectively, restrict the sample to low SES women, high SES women, low SES men, and high SES men. 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 < 0.10; ** p <0.05; *** p <0.01).

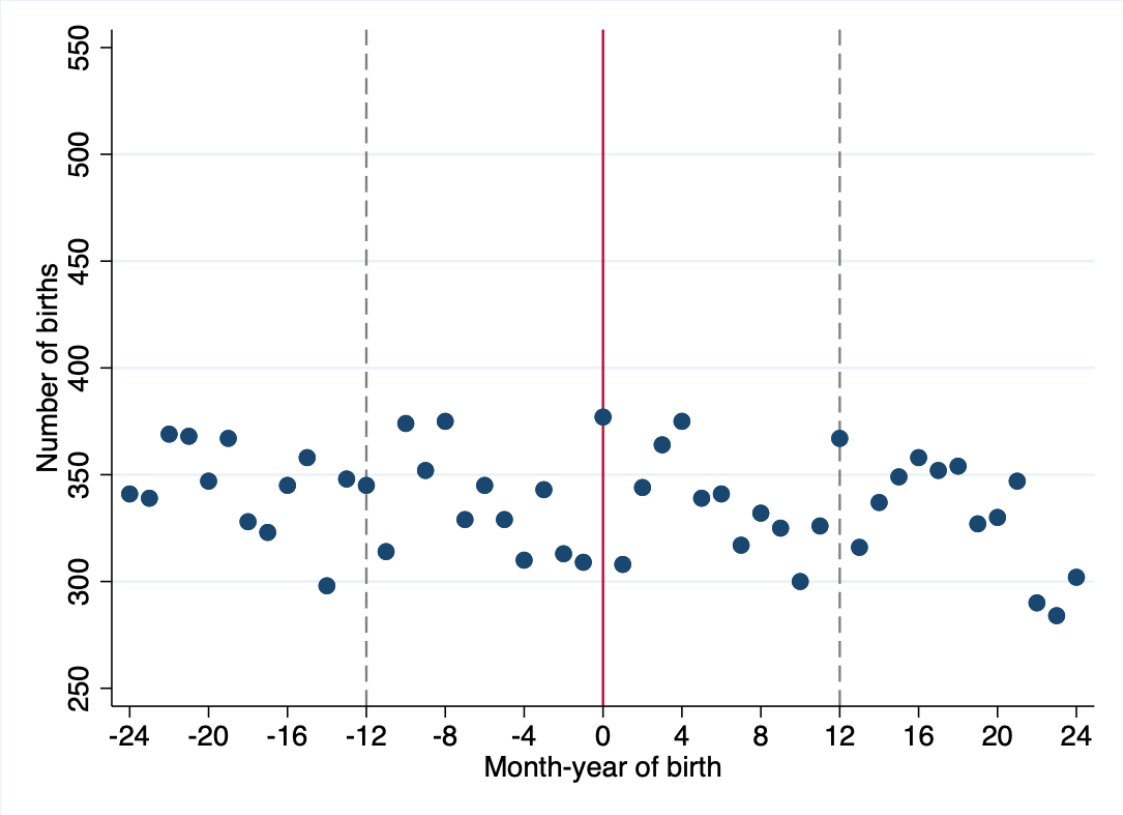
Table 6: Effect of the Reform on Marriage, Women and Men

	Women			Men		
	All (1)	Low SES (2)	High SES (3)	All (4)	Low SES (5)	High SES (6)
Married or cohabiting	0.005	–0.000	0.009	0.060*	0.094*	0.038
	(0.024)	(0.036)	(0.031)	(0.033)	(0.054)	(0.041)
Divorced	0.009	0.033	–0.009	0.001	0.015	–0.008
	(0.018)	(0.028)	(0.024)	(0.022)	(0.037)	(0.028)
N	6,716	2,861	3,855	3,394	1,333	2,061
Spouse in high-skilled occupation	0.073**	0.059	0.077*	0.007	0.033	–0.025
	(0.032)	(0.049)	(0.041)	(0.046)	(0.070)	(0.060)
N	4,896	2,095	2,801	2,344	901	1,443
Age gap between spouses	–14.087***	–19.488***	–10.077	8.911	5.644	10.777
	(4.532)	(6.667)	(6.144)	(6.966)	(10.929)	(9.009)
N	5,081	2,182	2,899	2,624	1,026	1,598

NOTE: Each cell reports the reduced-form estimate of the impact of the reform on various marital outcomes. Estimates are taken from separate local linear RD regressions using a bandwidth of 15 months and a triangular kernel. Columns (1) and (4), respectively, include all women and all men born within 15 months on either side of the January 1, 1966, cutoff. Columns (2)–(3) and (5)–(6), respectively, restrict the sample to low SES women, high SES women, low SES men, and high SES men. 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 < 0.10; ** p <0.05; *** p <0.01).

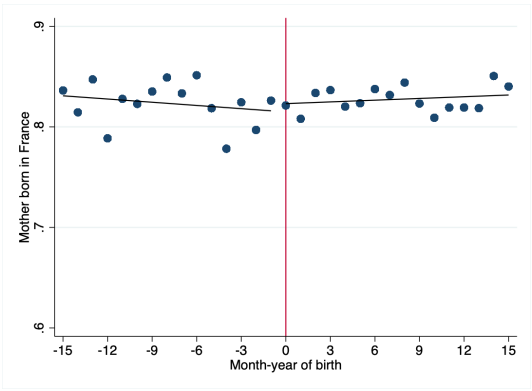
Online Appendix Figures and Tables

Figure A1: Frequency of Births

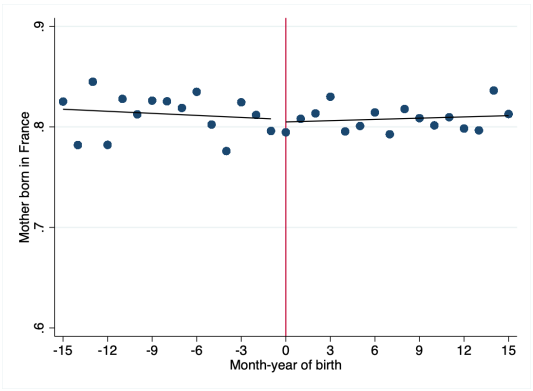


NOTE: This figure shows the number of individuals born in each month-year around the January 1, 1966, cutoff (red vertical line). The dashed vertical lines mark births in January of the years before and after the treated year (i.e., 1965 and 1967).

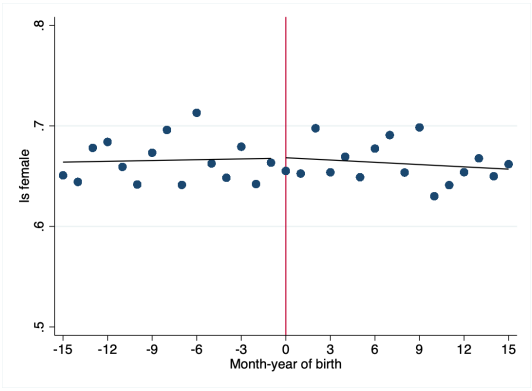
Figure A2: Effect of the Reform on Baseline Covariates



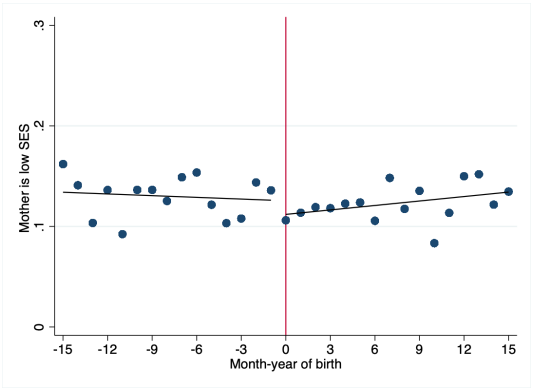
(a) Mother is born in France



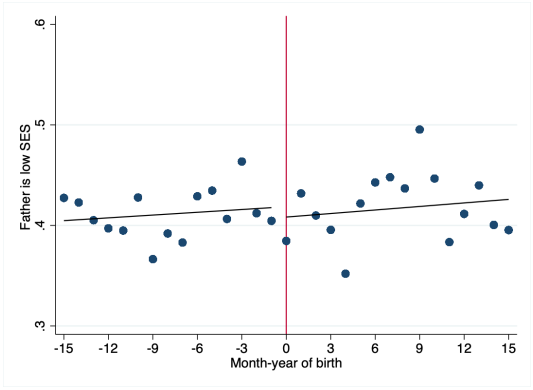
(b) Father is born in France



(c) Individual is female



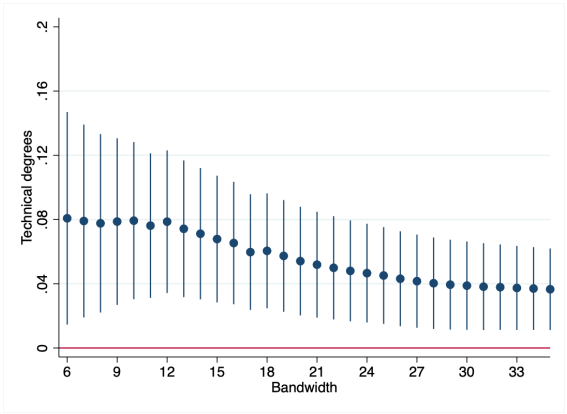
(d) Mother is low-skilled



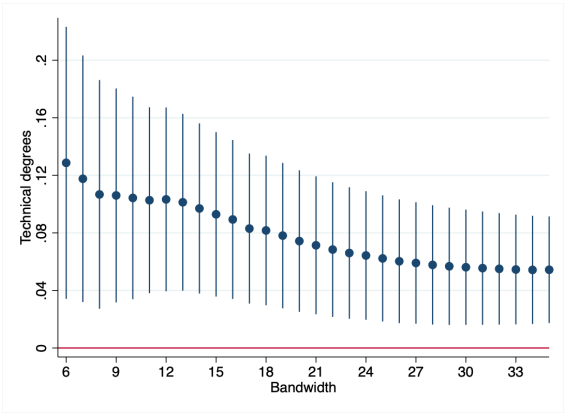
(e) Father is low-skilled

NOTE: The different panels show various baseline covariates as a function of the distance of individuals' month-year of birth from the cutoff. Circles represent each outcome's average over a one-month range. The fitted regression lines are taken from specifications with a bandwidth of 15 months. Panels (c)–(e) include all individuals whose parents are born in France.

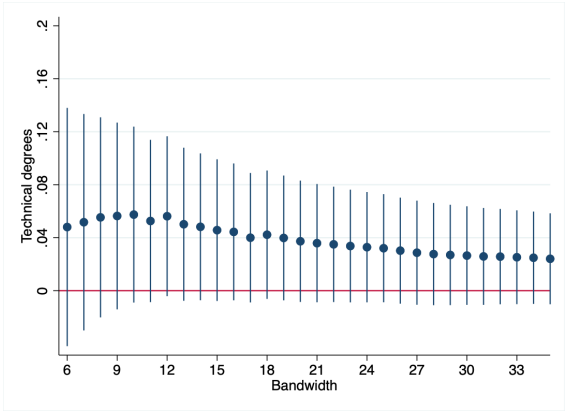
Figure A3: RD Estimates with Controls and across Different Bandwidths for Highest Degree Received



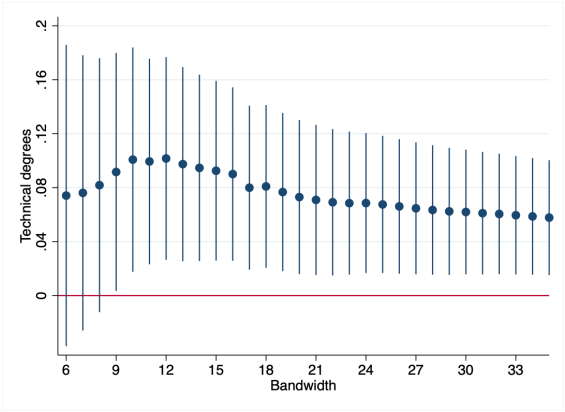
(a) All



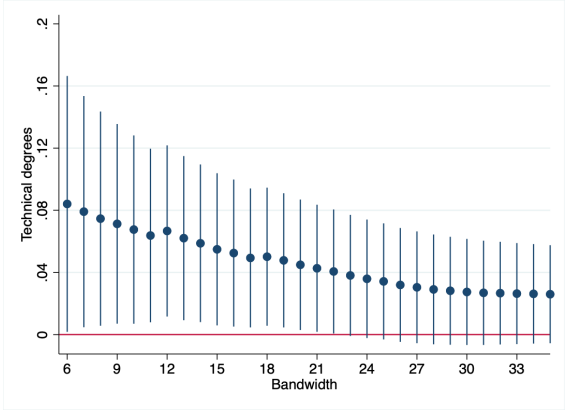
(b) Low SES



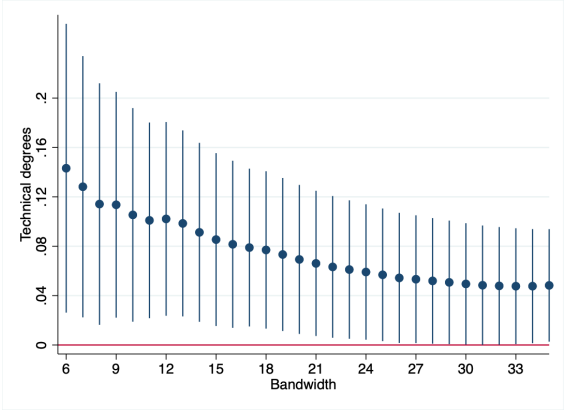
(c) High SES



(d) Men



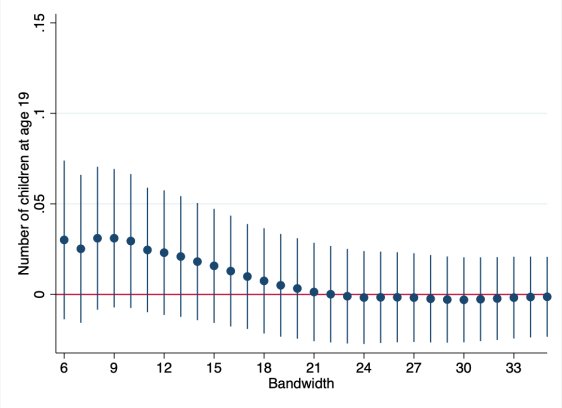
(e) Women



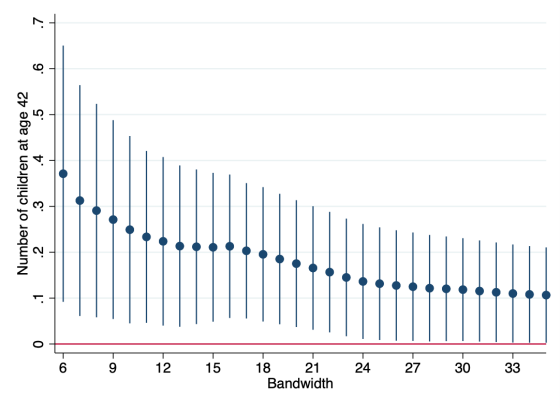
(f) Low SES Women

NOTE: The different panels show RD estimates—along with 95 percent confidence intervals—for the likelihood of having technical degrees as the highest degree received using different bandwidths. Estimates are taken from separate local linear RD regressions, which use a triangular kernel. When possible, regressions include controls and month-of-birth fixed effects. Controls include dummy variables for whether the individual is male, whether the father is in a low-skilled occupation, and whether the mother is in a low-skilled occupation.

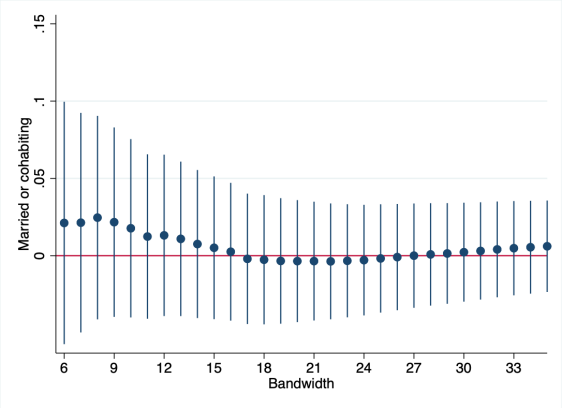
Figure A4: RD Estimates with Controls and across Different Bandwidths for Fertility and Marriage Outcomes



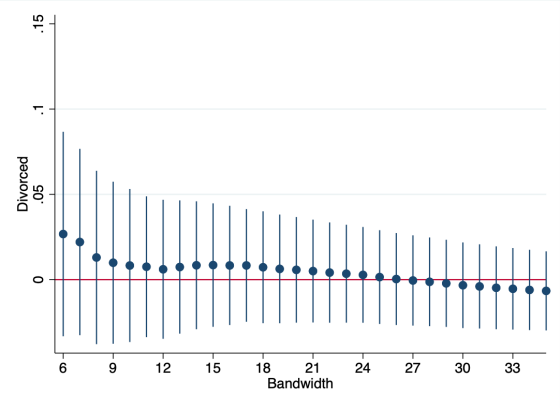
(a) Low SES



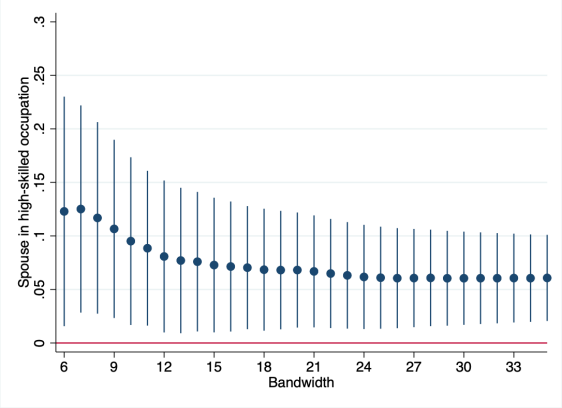
(b) Low SES



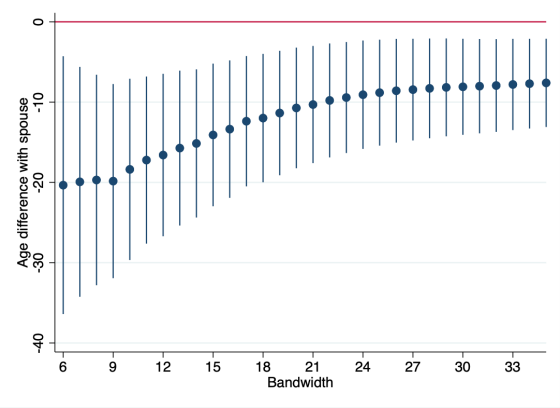
(c) Women



(d) Women



(e) Women



(f) Women

NOTE: The different panels show RD estimates—along with 95 percent confidence intervals—for fertility and marriage outcomes using different bandwidths. Estimates are taken from separate local linear RD regressions, which use a triangular kernel. When possible, regressions include controls and month-of-birth fixed effects. Controls include dummy variables for whether the individual is male, whether the father is in a low-skilled occupation, and whether the mother is in a low-skilled occupation.

Table A1: Middle School Curriculum Before and After the Reform

	First year			Last year	
	Prereform		Postreform	Prereform	Postreform
	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
Elective course: students choose one of the following subjects (in last year only)					
Latin	-	-	-	4	3
Ancient Greek	-	-	-	3	3
Second foreign language	-	-	-	3	3
Extra time in first foreign language	-	-	-	2	-
Technology	-	-	-	-	3
Total	27.5	27	24	27-29	27

NOTE: This table shows the number of weekly lessons allocated for each subject across different tracks in the first and last years of middle school, before [columns (1), (2), and (4)] and after [columns (3) and (5)] the reform. Prior to reform, (i) the duration of each lesson is between 50 minutes and one 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 include history and geography, so the corresponding lessons were fully allocated to civics. After the reform, (i) the duration of each lesson was 55 minutes; and (ii) around 30 percent of students took "soutien" classes, which offered them additional instruction time in subjects 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. SOURCE: Hall (1976) and Lewis (1985).

Table A2: RD Estimates for Baseline Covariates Using Different Bandwidths

	BW=6 (1)	BW=12 (2)	BW=15 (3)	BW=18 (4)	BW=21 (5)	BW=24 (6)	BW=27 (7)
Mother is born in France	0.012	0.016	0.013	0.011	0.009	0.008	0.008
	(0.013)	(0.017)	(0.015)	(0.014)	(0.013)	(0.012)	(0.011)
Father is born in France	0.003	0.007	0.004	0.000	–0.002	–0.004	–0.004
	(0.013)	(0.018)	(0.016)	(0.014)	(0.013)	(0.012)	(0.011)
N	5,219	10,353	12,935	15,589	18,273	20,813	23,323
Individual is female	0.004	0.004	0.005	0.001	–0.003	–0.003	–0.004
	(0.018)	(0.024)	(0.021)	(0.019)	(0.018)	(0.016)	(0.016)
Mother is low-skilled worker	–0.013	–0.016	–0.018	–0.016	–0.014	–0.013	–0.012
	(0.012)	(0.017)	(0.015)	(0.013)	(0.012)	(0.011)	(0.011)
Father is low-skilled worker	–0.022	–0.040	–0.034	–0.024	–0.016	–0.009	–0.003
	(0.019)	(0.025)	(0.022)	(0.020)	(0.018)	(0.017)	(0.016)
N	4,056	8,086	10,110	12,165	14,258	16,228	18,191

NOTE: 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 whose parents are born in France. Robust standard errors are reported in parentheses (* p < 0.10; ** p <0.05; *** p <0.01).