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Marginal Admission to Elite High Schools: Long-Run Effects on Labor Market Outcomes

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Marginal Admission to Elite High Schools: Long-Run Effects on Labor Market Outcomes*

Abstract

We estimate the long-run effects of marginal admission to elite public high schools on students' labor supply in the context of Mexico City's centralized high school admission system. Using a regression discontinuity approach, we compare students whose placement exam scores are just above and just below the elite admission threshold. We find that five and ten years after the admission exam, marginally admitted students are less likely to be employed in the formal private sector, and, if employed, they earn lower wages. However, these employment and wage gaps close after 15 years. Moreover, we find that marginal admission to elite high schools leads to delayed entry into the formal labor market, and, at least in the short run, students in elite high schools seem to sort into lower-productivity firms and industries.

JEL classification

I25, I26, J24, O17

Keywords

returns to education, human capital, education in developing countries, formal employment

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1 Introduction

Many secondary education systems are characterized by stark differences in perceived academic quality, with students often competing for seats in highly demanded "elite" high schools. Although the evidence on the academic benefits of elite schools is mixed (e.g., Dustan, de Janvry and Sadoulet, 2017; Beuermann and Jackson, 2022), they may offer other long-term benefits. An elite high school diploma may signal higher ability to employers and colleges, while peer and faculty networks may provide access to higher-quality jobs. The labor market benefits of elite high schools could be particularly important in developing countries, where college completion rates are relatively low and high school plays a central role in shaping early labor market outcomes.

This paper provides the first empirical evidence regarding the impact of elite public high school admission on labor market outcomes in a developing economy. Mexico City has two highly demanded elite high school subsystems, both of which participate in the centralized high school assignment system of the metropolitan area, which most students enter at ages 14-15.[1] The sharp exam score cutoffs for elite school assignment, generated by the assignment mechanism, allow for the identification of causal effects on marginally admitted students in a regression discontinuity design. We match the universe of several cohorts of participants in this assignment process to administrative social security records (IMSS), which cover the universe of all formal private sector jobs in Mexico, allowing us to estimate the impacts of elite school assignment on formal labor market employment and wages over a long time period. Formal sector employment is a particularly relevant outcome in developing economies, as formal jobs tend to be more resilient during recessions (Osuna-Gomez, 2023), are associated with higher wages (Abel et al., 2022), better career progression opportunities (Ulyssea, 2020), and social security benefits such as health insurance, affordable childcare, and pensions (Samaniego de la Parra and Fernández Bujanda, 2024).

[1] According to the 2019 National Survey on the Integration of High School Education Graduates into the Labor Market (ENILEMS), among graduates from elite high schools in the metro area of Mexico City, most chose their high school for its prestige or high academic quality (44.51%), followed by program offerings (24.85%) and proximity to home (13.08%). In contrast, students in nonelite high schools attended primarily because they were assigned or did not get into their preferred option (30.94%), because it was close to home (27.42%), or because of program offerings (24.44%).

We find that, for students near the cutoff for elite assignment, elite high school admission negatively impacts formal private labor market outcomes for at least a decade following assignment. The pattern of year-by-year impacts suggests immediate negative impacts on formal private sector employment and wages, followed by a slow recovery. Ten years after assignment (at ages 24-25), beyond the usual time frame for college completion, elite admission causes a 2.1 percentage point (p.p.) decrease in the probability of having ever held a formal job, a 1.7 p.p. decrease in the probability of holding a formal job in the past year, and, conditional on formal work, a 2.6% decrease in wages. Fifteen years after assignment (at ages 29-30), when a negligible number of individuals are exclusively pursuing further education and much of the college premium in wages has already materialized in the Mexican labor market (Benita, 2014), the impacts on these outcomes are close to zero, and the estimates are precise enough to rule out meaningful positive effects. In fact, we find small but statistically significant negative effects 15 years after assignment for men. These results, taken together, are inconsistent with short-run declines in work due to increased educational investments, which are subsequently offset by long-run improvements in formal employment and compensation.

We find suggestive evidence that elite high school admission causes students to work in lower-productivity firms and industries, at least in the short run. Conditional on formal private employment five years after assignment, marginally admitted elite students work in firms with 5.6% lower wages, similar to the estimated individual-level wage impact during this time period, and in marginally smaller firms. They also work in industries with 1.7% lower average wages and where employees have 0.13 years lower average employee education levels. The long-run impacts of elite admission do not reverse this pattern, but rather attenuate it: 15 years after assignment, marginally admitted and rejected students work in firms and industries that are, on average, very similar.

We show that students admitted to elite high schools delay entry into the formal labor market, thereby losing early work experience. Fifteen years after assignment, marginally admitted students, on average, have 2.4 fewer months of experience in the formal labor market. This tradeoff can be particularly important in developing countries, where labor markets are often less structured and formal employment opportunities are more limited. In such contexts, early work experience can be crucial in building practical skills, professional networks, and income-generating opportunities.

We examine potential channels through which this negative effect may manifest, drawing on the existing literature on elite schooling in Mexico City and other developing economies. Dustan, de Janvry and Sadoulet (2017) find that elite high school assignment in Mexico City increases the probability of dropout, concentrated among students with low middle school grade point averages. We find no evidence of heterogeneous labor market impacts with respect to middle school GPA, suggesting that negative average impacts are similar for high-GPA students. To the extent that this result reflects a lack of heterogeneity with respect to students' academic skills, it works against the explanation that these negative effects are solely due to marginally admitted students being near the bottom of the school-specific ability distribution. This is important in light of evidence showing that marginal admission to more competitive schools negatively impacts self-perceptions and induces behavioral responses in Mexico City's middle schools (Fabregas, 2023) and Romanian high schools (Pop-Eleches and Urquiola, 2013).

In the context of Chilean higher education, Zimmerman (2019) finds that labor market gains accrue to students from higher socioeconomic status backgrounds. We find no such heterogeneity with respect to parental education, suggesting that elite schools are not benefiting high-SES students while harming low-SES students. We also find that STEM-oriented elite schools may yield long-run benefits for women. Fifteen years after assignment, elite STEM schools increase the probability of ever having worked in the formal sector by 3.7 p.p., and there is suggestive evidence of increased wages conditional on employment. This result is consistent with the descriptive and correlational findings of a STEM premium for women in the labor market (Ngo and Dustan, 2024). We find no evidence of long-term benefits of STEM schools for men.

This paper contributes to the literature on the effects of school selectivity on student outcomes by being the first to causally identify the labor market effects of elite high schools in a developing country. In doing so, this paper provides a fuller picture of the impacts of these schools on student outcomes. A large literature has focused on short-run impacts on dropout and test scores, often finding null or small positive effects (e.g., Clark, 2010; Jackson, 2010; Pop-Eleches and Urquiola, 2013; Abdulkadiroglu, Angrist and Pathak, 2014; Kathryn et al., 2016). In Mexico City, marginal admission to an elite high school increases the probability of dropping out significantly, while also improving math test scores (Dustan, de Janvry and Sadoulet, 2017) and students' own expectations of future earnings (Estrada and Gignoux, 2017). In Tunisia, Luflade and Zaiem (2024) find that marginal admission to elite high schools increases the probability of eventual admission to university programs with lower overall post-graduation unemployment rates, suggesting the potential for improved labor market prospects.[2]

The closest paper to ours is Beuermann et al. (2023), which finds that more highly-demanded secondary schools in Trinidad and Tobago increase long-run formal labor force participation. Our paper differs in three key ways. First, they focus on admission at the lower secondary level (age at the time of assignment is 11), whereas we focus on older children (14- and 15-year-olds at the time of assignment) applying to the most common terminal stage of education in developing countries (high school). Second, the definition of elite schools in the Mexican context is a feature of the institutional setting: these schools are operated by prestigious national universities. In contrast, Beuermann et al. (2023) infer "good school[s]" through revealed parental preferences in applications. Third, while Trinidad and Tobago is classified as a high-income country, we focus on a middle-income country with worse labor market conditions and significant problems related to low and declining returns to higher education (Levy and Székely, 2016; Levy and López-Calva, 2020).

2 Background

2.1 The preference for formal jobs

In the Mexican context, a formal job is generally defined as one that provides health insurance (Abel et al., 2022; Osuna-Gomez, 2023; Samaniego de la Parra and Fernández Bujanda, 2024;

[2]Related work examines the effects of college selectivity on subsequent labor market outcomes (e.g., Hoekstra, 2009; Zimmerman, 2014; Black, Denning and Rothstein, 2023), finding substantial earnings gains for students attending more selective colleges in the US.

Samaniego de la Parra and Sharma, 2025). Although health insurance is important, it is only one of many reasons most workers seek formal employment. Formal jobs tend to be more resilient during recessions (Osuna-Gomez, 2023), have higher wages, and a higher probability of career progression (Abel et al., 2022; Ulyssea, 2020). They also provide other benefits, such as access to affordable childcare and pensions (Samaniego de la Parra and Sharma, 2025). In this regard, new estimates from Samaniego de la Parra and Sharma (2025) show that Mexicans are willing to pay 20% of their wages to have a formal job. As of the first quarter of 2025, only 48% of workers have one of these formal jobs in Mexico (National Employment Survey, ENOE).

While a large proportion of workers know of some of these benefits and would prefer a formal job,[3] the evidence suggests that those with a higher probability of getting one are the more educated (who are not still enrolled in school) and married males —older and married females being the less likely to have a formal job, even if they prefer one (Duval-Hernández, 2022; Conover, Khamis and Pearlman, 2022).

From a policy perspective, having more formal jobs is desirable because these jobs tend to be more productive (Fentanes and Levy, 2024). The reason behind these differences in productivity is that informal jobs are concentrated in informal firms (firms that do not pay social security taxes for any worker), which remain small to avoid government detection and pay additional taxes (Samaniego de la Parra and Fernández Bujanda, 2024). When firms remain small, they cannot take advantage of economies of scale, meaning that they are less productive (Busso, 2012).

Together, the previous literature implies that formal jobs are desirable for both governments and workers in developing economies.

2.2 Mexico's Education System

The Mexican education system is structured into three main levels: basic education, which includes preschool education (ages 3 to 5 years), elementary school (grades 1-6), middle school (grades 7-

[3] For example, 80% of informal workers in large urban areas state that they prefer a job that provides them with health coverage, even if this requires paying the taxes associated with the position (Duval-Hernández, 2022).

9); and high school (grades 10-12), and higher education.

Our analysis focuses on the metropolitan area of Mexico City, which has the highest net enrollment rates for both middle school and high school among all Mexican states. Specifically, in Mexico City, enrollment has increased considerably in the last two decades, reaching universal enrollment in middle school in the academic year 2015/16 and increasing the enrollment rate in high school, from 52.1% in 2000/01 to 82.5% in 2015/16 (Secretaría de Educación Pública, 2024).[4] Public education serves the majority of students, with approximately 91% of middle school students and 85% of high school students in Mexico City enrolled in public schools.

Public high schools in the Mexico City metro area are operated by the nine subsystems that form the Metropolitan Commission of High School Education Institutions (COMIPEMS). Two of these subsystems are in high demand and are considered “elite" due to their academic rigor and prestige: the Universidad Nacional Autónoma de México (UNAM) and the more technically oriented Instituto Politécnico Nacional (IPN). The UNAM high schools offer a standard liberal arts high school curriculum. Most IPN high schools are STEM institutions that focus intensively on academically rigorous science and engineering tracks, consistent with the polytechnic nature of the university with which they are affiliated. A small number of IPN campuses focus on non-STEM subjects in social sciences and business administration. The seven non-elite subsystems include those that offer traditional academic curricula, as well as technical subsystems that blend academic coursework with vocational training for careers such as programmer and electrician.

2.3 Mexico City’s Metro Area High School Admission Process

High school enrollment in public schools is highly competitive and requires students to take a high school admission exam, centrally administered by COMIPEMS since 1996. This exam is taken by all students seeking placement in a public high school in Mexico City’s metro area and is the only determinant of admission. Most students are 14 or 15 years old when they take this test. In Febru-

[4] In Estado de México, part of which belongs to the Mexico City metropolitan area, the net enrollment rate in middle school increased from 67.3% in 2000/01 to 85.8% in 2015/16, and for high school, it increased from 28.9% to 55%, respectively.

ary and March, students register for the exam, submit a priority list of up to twenty high schools, and complete a context questionnaire with demographic information. Students typically take the COMIPEMS exam in June, during their final year of middle school (ninth grade) or later. The exam consists of 128 questions, each worth one point. To be eligible for admission, students must have completed middle school and earned a minimum of 31 points on the COMIPEMS exam.[5] Elite high schools also require students to have a middle school GPA of 7.0 (out of 10) or higher.[6] In July, each high school reports the number of seats available in their school, and eligible students are ranked from the highest to the lowest score. Students are assigned to high schools through a serial dictatorship mechanism. That is, the highest-ranked student is assigned to their most preferred high school with available seats. The process continues until every student is assigned, except for those whose scores are too low to qualify for any of the high schools on their priority list. This assignment mechanism establishes sharp cutoffs for admission to elite high schools, which we exploit in our identification strategy.

3 Data

We use individual-level linked administrative records from the Mexican Secretariat of Public Education (SEP) and the Mexican Social Security Institute (IMSS). The sample comprises all students who took the COMIPEMS exam between 2003 and 2009 and were on the margin of admission to an elite public high school in the metro area of Mexico City.[7] We link these students with their labor outcomes in the formal market, three and up to 15 years after they take the high school admission exam.

[5]The 31 points minimum score restriction was eliminated in 2013 when high school education became compulsory.

[6]That is, even if the applicant has a COMIPEMS test score high enough to be admitted to an elite school, they are not eligible for admission to those schools. However, they may be assigned to other non-elite schools on their priority list.

[7]We focus on this sample because we can observe complete employment histories from 3 to 15 years after taking the COMIPEMS exam. Furthermore, with this sample, impacts 5 to 15 years after the exam are free of concerns about sample composition.

3.1 COMIPEMS Data

The COMIPEMS dataset, provided by the Mexican Secretariat of Public Education, contains administrative records for all applicants who took the COMIPEMS exam between 2003 and 2009. We observe each applicant's test score, priority list (with up to 20 high schools), middle school GPA, gender, parental education, and high school placement. We also observe whether the applicant earned a middle school diploma, is from outside the metro area, or is an adult learner.

We restrict our sample to students on the margin of admission to an elite public high school. That is, they scored at least 31 points on the COMIPEMS exam, earned a middle school diploma, have a middle school GPA of 7.0 or higher (i.e., they are eligible for admission to an elite high school), and list school preferences such that a sufficiently high score on the COMIPEMS exam would result in assignment to an elite high school and a sufficiently low score would result in non-elite assignment.[8]

3.2 IMSS Data

The IMSS dataset, accessed through Banco de México's EconLab, contains monthly longitudinal administrative records for every formal worker in the private sector in Mexico from November 2004 to July 2024.[9,10] We observe workers' wages, age, sex, municipality, and industry of employment, and unique employer and employee identifiers.[11]

Employers are legally required to report wages, separations, and new hires to the IMSS, with

[8]The technical basis for this restriction is detailed in the next section. We further restrict our sample to normative-age students who reside in the Mexico City metro area. That is, we drop students from outside the metro area and adult learners.

[9]Our data capture only formal workers affiliated with IMSS. While some formal employment occurs through other institutions, IMSS covers the vast majority of formal workers (84%). According to the 2019 National Employment Survey (ENOE), 31% of Mexican workers were affiliated with IMSS, compared to 5.5% with ISSSTE (government employees) and 0.6% with other formal systems, such as PEMEX, the state-owned oil company; the remaining 62.5% had no employer-provided healthcare or were informal workers. Importantly, among young workers (under age 25) with secondary education in Mexico City, only 1% of formal workers are covered by non-IMSS institutions (ENOE).

[10]Using a very simple linear regression on log-wages on an indicator for different health care providers, and controlling for age and sex, we know that IMSS wages are 35% higher than those of ISSTE and 30% lower than the wages that use other forms of health insurance, such as PEMEX (there are too few of these jobs). Therefore, IMSS jobs are the most attractive in the Mexican labor market wage-wise for most workers.

[11]This dataset does not include the names of any individuals or firms, nor any information that could reveal the identity of a specific individual or firm. Instead, it includes an anonymized version of the workers' and firms' identifiers.

the data updated monthly. The reporting process is remote, and as a result, the methodology and coverage of this dataset enable us to make accurate inferences, even during 2020, when the COVID-19 pandemic posed challenges to reporting employment data through traditional household surveys (Osuna-Gomez, 2023).

To merge COMIPEMS and IMSS data, EconLab uses information from the Unique Population Registry Code (CURP), a unique identifier assigned to all Mexican residents and citizens. This code has information on specific letters in the given name, surname, date of birth, gender, birthplace, and some random numbers to differentiate individuals with similar characteristics. We do not observe this information as users, but the EconLab team performing the merge does.[12] To merge COMIPEMS and IMSS data, EconLab uses information from the Unique Population Registry Code (CURP), a unique identifier assigned to all Mexican residents and citizens. This code has information on specific letters in the given name, surname, date of birth, gender, birthplace, and some random numbers to differentiate individuals with similar characteristics. We do not observe this information as users, but the EconLab team performing the merge does.[13]

The primary outcome variables include a dummy equal to one if the individual ever held a formal job (0 otherwise), the total number of months they were employed in a formal job, a dummy equal to one if the individual had a formal job in the past year (0 otherwise), and their wages. We measure these outcomes five, ten, and fifteen years after taking the COMIPEMS exam. Because the data cover the whole country, we can observe if someone is employed in the formal sector even if he moved outside of Mexico City. If someone migrates internationally, or is not employed in the private formal sector in Mexico, we assign a value of zero to our key dependent variable.

[12]To merge students' labor market outcomes and preserve anonymity according to EconLab rules, we limit the sample to students on the margin of admission to an elite high school who have the same covariate values as at least three other individuals in the analysis. The application of this criterion resulted in a reduction of 3% in our sample size. We also repeated the analysis choosing a different set of covariates (so we would drop a different 3% of the sample), and the results were similar to those presented in this paper.

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3.3 Census Data

To explore potential mechanisms, we use the 10% sample of Mexico's 2010 Population and Housing Census, obtained from IPUMS International (Ruggles et al., 2024). The dataset provides individual-level information on employment status, type of health coverage, sex, years of schooling, monthly earnings, and industry of employment. We restrict the sample to employed individuals covered by IMSS. Using this restricted sample, we calculate the average share of female workers, average years of schooling, and average monthly earnings by industry. Because IMSS classifies workers by industry using its own classification system, while IPUMS classifies workers using 3-digit NAICS codes (North American Industry Classification System), to merge industry characteristics with workers in our main dataset, we use an IMSS to NAICS codes correspondence created by EconLab at Banco de México.

3.4 Descriptive Statistics

Table 1 presents the summary statistics for students who gained admission to elite high schools and those who did not, as well as for those who were accepted and rejected within 10 points of the cutoff score required for admission to the least-competitive elite school on their preference list.[14] This second set of columns shows the characteristics of students who were admitted or rejected by a small margin on their entrance exam, and by doing so, decreases the possible differences in characteristics between accepted and rejected students. This 10-point window will be used as the preferred bandwidth in the regression discontinuity analysis. This table includes both characteristics at baseline for each group and outcomes at different points in time (five, ten, and fifteen years after assignment).

First, we compare students based on their characteristics at the time they took the COMIPEMS exam. At the time of the exam, 44% of the students in our sample who were rejected had at least one parent with high school education.[15] Among those accepted, 64% had parents with a high

[14]Section 4 formally defines the relevant cutoff school according to students' preference list.

[15]Parental education is imputed for the approximately 14% of students who did not self-report this measure on the student context survey at the time of application. This binary measure is imputed according to the proportion of

school education. The difference between these two groups is statistically significant. When we focus on those who were rejected and accepted within 10 points from the cutoff, the difference described in the previous two columns is smaller, but still statistically significant. We also find that those accepted had a higher GPA and were more likely to be men, even if we restrict the sample to those who were rejected and accepted within 10 points from the cutoff. We also observe that those accepted had higher test scores relative to the relevant cutoff score.

Second, we describe how these groups differ on several outcomes, 5 years after taking the COMIPEMS exam. At this point, most test-takers are 19-20 years old. Among those rejected, 19% had a formal job at any point in time; and among those accepted, 16% had a formal job. Once we focus on those who were rejected and accepted within 10 points from cutoff, the differences between these two groups is similar (20% vs. 16%). These differences are statistically significant. Similarly, we find that rejected students have higher probability of having a formal job in the previous year, have higher wages conditional on employment and more formal job experience than accepted students. These findings could be explained by the fact that the COMIPEMS exam is taken when students are about 14-15 years of age (meaning that five years after taking the exam, these individuals will be 19-20 years of age), and many of the best students may have more incentives to continue their education at that point.

Third, we show how these outcomes evolve 10 years after taking the exam. At this point in time, most of the people who took the COMIPEMS exam are between 24 and 25 years of age. It is important to note that the most common age for college graduation in Mexico is 22 years old, and college graduation after the age of 25 is rare (Osuna-Gomez, 2023). Furthermore, only a very small proportion of Mexicans attend graduate school (0.7%, National Employment Survey (ENOE)). This information implies that 10 years after taking the COMIPEMS exam, most test takers are not considering additional schooling, which may explain our findings: among those barely rejected, 35% had a formal job at any point in their lives; while among those barely accepted, 34% had a formal job at any point. Those barely rejected had more work experience in the formal sector and

COMIPEMS takers in the student's middle school cohort who had a high school-educated parent. If no students in the middle school cohort reported parental education, the sample-wide proportion was used for imputation.

lower wages conditional on working.

Finally, we describe the outcomes of these individuals 15 years after taking the test. At this point in time, most of these individuals are 29-30 years old. Those barely rejected had a lower probability of ever having a formal job (43 vs. 44%), and a lower probability of having a formal job in the previous year (37 vs. 38%). Those barely rejected had more months of work experience (32 vs 30 months) and lower wages conditional on working.

4 Identification Strategy

We use a regression discontinuity (RD) design to identify the causal effect of elite high school assignment on labor market outcomes. This approach isolates the causal impact of admission from the effects of student ability, preferences, and other factors that are correlated with both elite school assignment and labor market performance. The RD design compares students whose reported preferences were such that a sufficiently high entrance exam score would result in elite school assignment, and whose realized scores placed them either marginally above or marginally below the appropriate cutoff score for elite admission. This comparison requires definitions of the sample of potentially elite-eligible students and of the appropriate cutoff score for each student. We generally follow the approach from Dustan, de Janvry and Sadoulet (2017), which focused on admission to a subset of the elite schools considered here.

Each school j that fills its quota of available seats during the assignment process in year t has a cutoff score, $\underline{c}_{jt}$. This is the exam score obtained by the student assigned to the final seat—no student with a lower score can be admitted by this assignment mechanism. Schools that do not fill all their seats have a cutoff score of $\underline{c}_{jt} = 0$. Given a student's reported preference list and the cutoff scores of each school on that list, it is possible to know their counterfactual assignment for every possible realization of their exam score c_i. For example, suppose the student's reported preferences are $\{s_2, s_3, s_1, s_4\}$, with corresponding cutoff scores $\{\underline{c}_{2t}, \underline{c}_{3t}, \underline{c}_{1t}, \underline{c}_{4t}\} = \{75, 70, 80, 0\}$. For any score $c_i \geq 75$, the student will be assigned to s_2, his most-preferred school. For scores

$70 \leq c_i < 75$, the student is assigned to s_3. The student's reported preferences make assignment to s_1 impossible: for $c_i \geq 80$, the student also scores high enough for assignment to his more-preferred s_2. For $c_i < 70$, the student is assigned to s_4. Suppose that s_1, s_2, and s_3 are elite schools, and s_4 is not. Then, for this student, every $c_i \geq 70$ implies elite assignment and every $c_i < 70$ implies non-elite assignment. Because school s_3's cutoff is the boundary between elite and non-elite assignment, we call school 3 student i's "cutoff school." The student's overall "elite cutoff" in this example is $\underline{c}_{3t}$.

The analysis sample consists of students who have reported preferences such that an elite cutoff exists: there exists a score $\underline{c}_{jt}$ such that for every $c_i \geq \underline{c}_{jt}$, the student is assigned to an elite school, and for every $c_i < \underline{c}_{jt}$, the student is assigned to a non-elite school or is left unassigned by the assignment mechanism. Inclusion in this sample not only requires students to include at least one elite school in their reported preferences, but to do so in such a way that results in elite assignment for all scores above the elite cutoff.[16] Defining the student's "centered score" as $\tilde{c}_{ijt} = c_i - \underline{c}_{jt}$, students in this sample receive elite assignment for $\tilde{c}_{ijt} \geq 0$ and non-elite assignment for $\tilde{c}_{ijt} < 0$. While a centered score of $\tilde{c}_{ijt} = 0$ results in assignment to the cutoff school j, a sufficiently high score may result in assignment to a different elite school. That is, given the sample construction, this is a sharp RD design: the probability of elite assignment jumps from exactly zero to exactly one at the cutff. We further restrict the sample by requiring that marginal rejection ($\tilde{c}_{ijt} = -1$, recalling that the score is integer-valued) would result in assignment to a non-elite school rather than being unassigned by the mechanism altogether.

The RD design compares students whose preferences result in them having the same cutoff school j in year t and whose realized scores c_i place them either marginally above or marginally below that cutoff, resulting in either elite or non-elite assignment, respectively. The basic estimating equation is the following:

[16]This sample construction excludes students who list a non-elite school before an elite school in their reported preferences. It also excludes students who list schools such that their highest range of scores results in elite assignment, a lower range of scores results in non-elite assignment, and a still lower range results again in elite assignment. The latter case is rare because few non-elite schools have cutoffs that exceed those of elite schools.

$$y_{ijt} = \delta elite_i + \gamma_1 \tilde{c}_{ijt} + \gamma_2 \left(\tilde{c}_{ijt} \times elite_i\right) + \mu_{jt} + \varepsilon_{ijt}, \tag{1}$$

where y_{ijt} is a labor market outcome for student i who attended school j, $elite_i$ is a dummy variable equal to 1 if $\tilde{c}_{ijt} \geq 0$ so that the student is assigned to an elite school, μ_{jt} is a cutoff school-year fixed effect, and ε_{ijt} is an idiosyncratic error. The identifying assumption is that potential outcomes are continuous at the cutoff, such that the only differences in realized outcomes for students arbitrarily close to the cutoff are due to elite admission. Because the entrance exam score is integer-valued, the running variable is discrete. This complicates the use of typical bandwidth selection procedures (Calonico, Cattaneo and Titiunik, 2014), which assume continuity of the running variable at the cutoff. We instead report results for a range of bandwidths as sensitivity checks, while focusing on results from a single bandwidth of 10 points (about half a standard deviation) for all outcomes and subsamples. Observations are weighted using the edge kernel, following the standard practice for local linear RD estimators (Imbens and Kalyanaraman, 2012; Cameron and Miller, 2015). Inference procedures must also account for the discrete nature of the running variable. In addition to heteroskedasticity-robust standard errors, we report honest confidence intervals under the bounded second derivative approach (Kolesár and Rothe, 2018; Armstrong and Kolesár, 2020). We impose the conservative rule of thumb assumption from Armstrong and Kolesár (2020) that the second derivative of the true function relating test scores and outcomes at the cutoff is no greater than the maximum curvature from a global fourth-order polynomial fit to the data.

We also estimate heterogeneous effects of elite assignment with respect to binary student and cutoff school characteristics. To do so, we first report the estimates of Equation 1 separately by group, including honest confidence intervals. We then estimate a fully interacted model in which all parameters vary with respect to group membership ($g = 0$ or $g = 1$):

$$y_{ijtg} = \delta_g elite_i + \gamma_{1g} \tilde{c}_{ijt} + \gamma_{2g} \left(\tilde{c}_{ijt} \times elite_i\right) + \mu_{jtg} + \varepsilon_{ijtg}. \tag{2}$$

We test for equality of group-specific elite assignment effects, $\delta_0 = \delta_1$, using the robust standard errors.[17]

5 Results

We first present the causal effects of marginal elite school admission in the full RD sample, followed by heterogeneous impacts with respect to student and high school characteristics. We report impacts for three time periods after assignment: year five, at which time the vast majority of eventual high school graduates have earned their diplomas; year 10, two years beyond the typical normative time to complete both high school and an undergraduate program; and year 15, the maximum time after assignment available in the data for all analysis cohorts. Year-by-year results corresponding to these selected time periods are in the appendix. Finally, we summarize the results of validity and sensitivity analyses for the RD design.

5.1 Main impacts

Table 2, accompanied by Figure 1, traces out the causal impact of elite school assignment on formal private labor market outcomes over time.[18] Column 1 of Panel A shows that, five years after the assignment process (at ages 19-20, beyond high school age), elite assignment causes a 3.2 percentage point (p.p.) decline in the probability of ever having held a formal job. This is a 16% decline relative to the base rate of 20% for students missing the elite cutoff by one point. Over time, this elite disadvantage declines: after 10 years (at ages 24-25, after the typical age of college graduation), the effect is -2.1 p.p., while after 15 years (at ages 29-30) the estimated -0.2 p.p. impact is statistically insignificant at standard levels.[19] Panel B shows that contemporaneous

[17] The procedure for computing honest confidence intervals cannot be applied straightforwardly to estimates of the difference between two coefficients. The p-values associated with the robust standard errors are less conservative than what is implied by the honest confidence intervals.

[18] Appendix Figure A.1 presents results separately for years three through 15 after assignment.

[19] According to ENOE (2019), 78% of all residents of Mexico City are done with their studies at age 24-25; 11% study and work, and 11% study but do not work. Individuals who study and work are about equally likely to be employed in the formal and informal sectors. By age 29-30, approximately 1% of all individuals are enrolled in school, but the number of individuals who study and do not work is negligible. These numbers imply that by age 29-30, these results cannot be explained by individuals still pursuing further education.

formal job-holding follows a similar pattern: while elite assignment decreases the probability of formal employment in the fifth year after assignment by 3.1 p.p., in year 15 this effect falls to -0.6 p.p. and is statistically insignificant.

While the negative effect of elite assignment on formal employment eventually fades, it leaves elite students with a deficit in formal labor market experience. Panel C reports impacts on the cumulative number of months with a formal job. After five years, elite assignment reduces formal experience by 0.57 months, a 19% decrease compared to the mean among marginally rejected students. This deficit widens to 1.86 months after 10 years and 2.44 months after 15 years, representing 14% and 8% decreases compared to marginally rejected students, respectively.

Conditional on formal employment, marginal elite assignment causes a reduction in wages that fades out over time. Panel D shows the results of estimating the RD model on a sample restricted to those who worked in the previous year, and the outcome is the logarithm of the average wage in that period. Five years after assignment, elite assignment reduces wages by 5.3%. This reduction declines to 2.6% after 10 years. Although this difference is statistically significant at the 5% level with robust standard errors, the more conservative 95% honest confidence interval includes zero. After 15 years, the estimated effect declines to 0.2% and is statistically insignificant.

To provide evidence on the potential mechanisms driving these results, we estimate Equation 1 using alternative outcomes that capture firm and industry characteristics. These outcomes include the average log wage at the firm of employment, firm size (measured by the number of workers), the average log wage in the industry of employment, the proportion of female workers in the industry, and the average years of education in the industry of employment.[20] Table 3, accompanied by Figure 2, provides suggestive evidence that the short-run decline in formal sector wages is in part due to employment in lower-productivity firms and industries.[21] Five years after taking the exam, elite assignees (conditional on formal employment) work at firms with 5.6% lower wages, nearly the same as the individual-level wage effect. While this result is statistically significant at the 5%

[20] Appendix Table A.2 shows descriptive statistics of these outcomes for rejected and accepted students 5, 10, and 15 years after taking the COMIPEMS exam.

[21] Appendix Figure A.2 shows year-by-year results for all years three through 15.

level with robust standard errors, the 95% honest confidence interval for this effect covers zero, as do the confidence intervals for the other effects discussed in this table. Elite assignees may also work in smaller firms: after five years, the estimated impact on employer size is -0.095 (significant at the 10% level), compared to a base of 6.1 employees. More broadly, elite assignment may cause workers to sort into different, lower-paying industries in the short-run. The effect on average wage in industry of employment is -1.7%, while the effect on average years of education in industry of employment is -0.13 years.[22] These effects on industry of employment are not large, but they point in the same direction as the firm results: elite admission leads students to sort into industries and firms that pay less, potentially due to lower productivity, and earn lower wages themselves. In the longer-run, the firm- and industry-level impacts mirror the individual-level results: effect sizes decline and, by year 15, are close to zero and statistically insignificant.

5.2 Heterogeneous impacts

To shed further light on the potential mechanisms underlying the overall impacts of elite assignment, and to understand how these effects vary across the student population, we first estimate heterogeneous impacts by student characteristics.

Evidence suggests that students with a relatively high GPA are more likely to finish high school and attend college, indicating higher expectations for their future careers and a higher perceived intelligence (Elsner and Isphording, 2017). Motivated by these findings, Table 4 and accompanying year-by-year estimates in Appendix Figures A.3 and A.4 show heterogeneous impacts by whether the student has a middle school GPA above or below the median. Overall, middle school GPA is not predictive of elite high school assignment impacts. Five years after assignment, point estimates for formal sector participation are nearly identical across high-GPA (above sample median) and low-GPA (below sample median) groups. While the estimated wage impacts for high-GPA students (-7.3%) are higher than for low-GPA students (-3.4%), the difference between these estimates is

[22]The estimated impact on the gender ratio in the industry of employment is close to zero and statistically insignificant.

not statistically significant at conventional levels ($p > 0.10$). Effects on formal sector participation are still similar between groups 15 years after assignment. The estimated wage impacts reverse by this time, but neither the difference in effects nor the group-specific effects are statistically different from zero. Taken together, these results do not suggest a strong role for academic preparation in producing the observed pattern of labor market impacts. Given the apparent importance of middle school GPA in mediating elite school impacts on high school dropout and learning in Mexico City (Dustan, de Janvry and Sadoulet, 2017), one might expect marginal elite assignment to negatively affect low-GPA students by increasing dropout probability, while benefiting high-GPA students by increasing human capital and access to higher education without worsening dropout. Our results suggest that the lack of benefits in accessing formal sector jobs extends to high-GPA students.

Evidence has also documented the benefits of higher parental education on their offspring's later outcomes, including performance on high-stakes exams among middle education students (Dickson, Gregg and Robinson, 2016) and later children's earnings (Sikhova, 2023; Lee, Roys and Seshadri, 2024). Table 5 and accompanying year-by-year estimates in Appendix Figures A.5 and A.6 show a lack of evidence for heterogeneous impacts with respect to parental education, regardless of the time elapsed since assignment. The effects for both education groups correspond to the pooled results: employment and wage impacts are negative after five years and fade by year 15. This suggests that socioeconomic status, as captured by parental education, is not a key driver of elite school impacts on employment outcomes.

The heterogeneous results across parental education levels also suggest that our findings are not driven by rejected students enrolling in private high schools with stronger professional networks. If wealthier students are more likely to take this option, our negative results would be attributable to students with higher socioeconomic status (as measured by parental education) because they are more likely to be able to pay for private high schools. However, the results of our analysis for students with higher socioeconomic status are not different from those of students with lower socioeconomic status.

Motivated by the literature on gender differences on the effects of attending selective high schools in other contexts (e.g., Jackson, 2010; Clark and Del Bono, 2016; Hoekstra, Mouganie and Wang, 2018), where females benefit the most from attending selective schools, we estimate heterogeneous effects by gender. Table 6, accompanied by Appendix Figures A.7 and A.8, shows that elite assignment impacts male and female students similarly in the short run, while differentially benefiting females in the long run. After five years, both male and female students have had significantly lower engagement with the formal sector due to elite assignment, and have similar estimated wage penalties. After 10 years, however, impacts on formal employment have diverged somewhat and these differences are statistically significant: the negative impact on having ever worked in the formal private sector is 2.1 p.p. worse for males ($p = 0.025$) and the impact on having a formal job in the past year is 2.0 p.p. worse ($p = 0.028$). Elite admission has reduced cumulative formal sector employment by 0.93 more months for males than for females ($p = 0.043$). Estimated wage impacts are not significantly different, although the point estimates are now more negative for males than females. This trend continues at 15 years, as elite admission continues to affect males negatively while females no longer appear to experience detrimental impacts. For example, males are 1.6 p.p. less likely to have worked at a formal job in the past year, a difference that is significant at the 5% level according to robust standard errors but not the honest confidence interval, while the estimate for females is a statistically insignificant 0.6 p.p. increase. This 2.2 p.p. difference in effects is statistically significant ($p = 0.021$). The differences in effects on having ever worked in the formal sector and on cumulative formal job experience are also significant. The wage effects are less conclusive, but follow the same pattern: point estimates are negative for males, positive for females, and marginally significantly different from each other–the difference in estimates is 3.8% ($p = 0.098$).

To better identify which female students benefit from elite admission and why, we explore the differential effects of STEM-focused elite schools compared to elite schools that have more traditional liberal arts or business-focused curricula.[23] This distinction is important because a growing body of research indicates that high school majors influence both the choice of college

[23]Corresponding to the discussion in Section 2.2, STEM elite schools are those IPN schools offering STEM curricula, while non-STEM elite schools are the small number of non-STEM IPN schools and all UNAM schools.

major and subsequent career paths, with STEM majors in high school leading to higher earnings than majors in the social sciences or humanities (Cohodes, Ho and Robles, 2022; Dahl, Rooth and Stenberg, 2023). Table 7, along with Appendix Figures A.9 and A.10, shows these differential effects in the pooled sample (male and female). Focusing on the 15-year results, we see that while point estimates are generally more favorable for STEM schools, the STEM vs. non-STEM difference in impacts is not statistically significant for any outcome. Therefore, even within more rewarding STEM fields of study, marginal elite high school admission continues to show no short- or long-run benefits for labor market outcomes.

Estimating this dimension of heterogeneity separately by gender suggests that elite STEM assignment benefits marginal female students. Table 8 demonstrates that despite negative short-run effects, STEM admission positively impacts formal labor market outcomes later in females' careers. After 15 years, STEM admission increases the probability of having ever worked formally by 3.7 p.p. and of working in the past year by 2.9 p.p., though the latter effect is marginally significant. There is no evidence of a decrease in cumulative experience in the formal sector. Elite STEM assignment increases female wages by 12.6% in year 15. In contrast to STEM admission, non-STEM admission is estimated to have small, insignificant impacts on employment and wage outcomes, and the STEM-non-STEM wage effect differential of 12.4% is highly significant ($p = 0.002$). Taken together, the results suggest that elite STEM assignment is beneficial to female students, increasing formal employment and wages.

The results for males, found in Table 9, do not indicate long-run benefits from either elite STEM or non-STEM assignment. After 15 years, estimated impacts on all outcomes are negative for both school types, although statistical significance is mixed, and the honest confidence intervals cover zero in all cases except for the impact of non-STEM assignment on cumulative months with a formal job. Estimated impacts are more negative for non-STEM schools, although again, these differences are not statistically significant. In summary, there is only evidence that long-run benefits from elite assignment accrue to female students in STEM-focused schools.

5.3 Validity and robustness

Two diagnostic exercises support the validity of the RD design. First, Appendix Table A.1 and Appendix Figure A.11 show the results of estimating Equation 1 with predetermined student characteristics (indicators for gender, high school educated parent, and above-median middle school GPA) as the outcome variables. In each case, the point estimate for the elite assignment "effect" is close to zero: 0.0 p.p. for probability of being female, -0.3 p.p. for probability of having a high school educated parent, and 1.0 p.p. for probability of having an above-median middle school GPA. Using the robust standard errors for inference leads to rejection of balance in the probability of above-median middle school GPA at the 5% level, but the 95% honest confidence interval is much wider and includes zero. Taken together, the results suggest balance in the predetermined covariates across the elite assignment cutoff.

Appendix Figure A.12 shows the density of the centered score, both for the full support in Panel A and restricted to a ten-point bandwidth in Panel B. There is no visual evidence of a discontinuity in the density at the cutoff, consistent with our expectation, given that the scoring and assignment process is automated and does not provide opportunities for score manipulation. Testing for the presence of a discontinuity in this discrete running variable (Frandsen, 2017), we fail to reject continuity ($p = 0.68$), consistent with the visual evidence.

We have used a fixed bandwidth of 10 points throughout the RD analysis. Appendix Figures A.13 through A.16 plot, for bandwidths 3 through 40, the elite assignment effect point estimates and 95% honest confidence intervals for the main outcomes. The figures highlight two patterns. First and most importantly for assessing the robustness of the results, estimated impacts are insensitive to the choice of bandwidth, regardless of outcome or years since assignment. Second, beyond an initial narrowing, higher bandwidths tend to yield wider honest confidence intervals. This pattern is in contrast to standard error-based confidence intervals, reflecting the incorporation of worst-case bias due to misspecification error into the construction of honest confidence intervals. As the bandwidth grows, although the variance of the estimated impact decreases, the degree of worst-case misspecification error does as well.

Finally, we cannot observe whether an individual migrated to another country. If this occurs, the migrant does not appear in the IMSS dataset and the dependent variable "formal job in past year" is coded as zero. Two reasons make it unlikely that our results are driven by international migration. First, Mexico City has one of the lowest propensities for international migration in the country. In 2020, approximately 43,329 individuals–less than 0.5% of Mexico City's 9.2 million residents–migrated to another country (International Organization for Migration, 2022). Second, migration for the purpose of higher education is rare, alleviating the concern that elite high school admission induces students to study internationally and appear outside the formal labor market. In the 2024/2025 school year, only 15,652 Mexican nationals were international students in the United States (Institute of International Education, 2025). Furthermore, the primary source of funding for international students is family and personal funds. Applicants to public high schools are unlikely to have such means. To further assess this possibility, in Table 5, we estimated the effects by low and high parental education (a proxy of socioeconomic status) and found no significant differences across groups, suggesting that selective migration is not a threat to our identification.

6 Conclusion

This paper studies the causal effects of marginal admission to elite high schools on student formal employment outcomes up to 15 years after assignment. We show that, five and ten years after the admission exam, students just above the admission cutoff are less likely to be employed in the formal sector and, conditional on employment, earn lower wages. These employment and wage gaps gradually narrow and close after 15 years, when these individuals are 29-30 years old. However, at this point, marginally accepted students have accumulated less tenure in the formal sector. We also find that selection into low-wage firms and industries may partially explain these findings.

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Tables and Figures

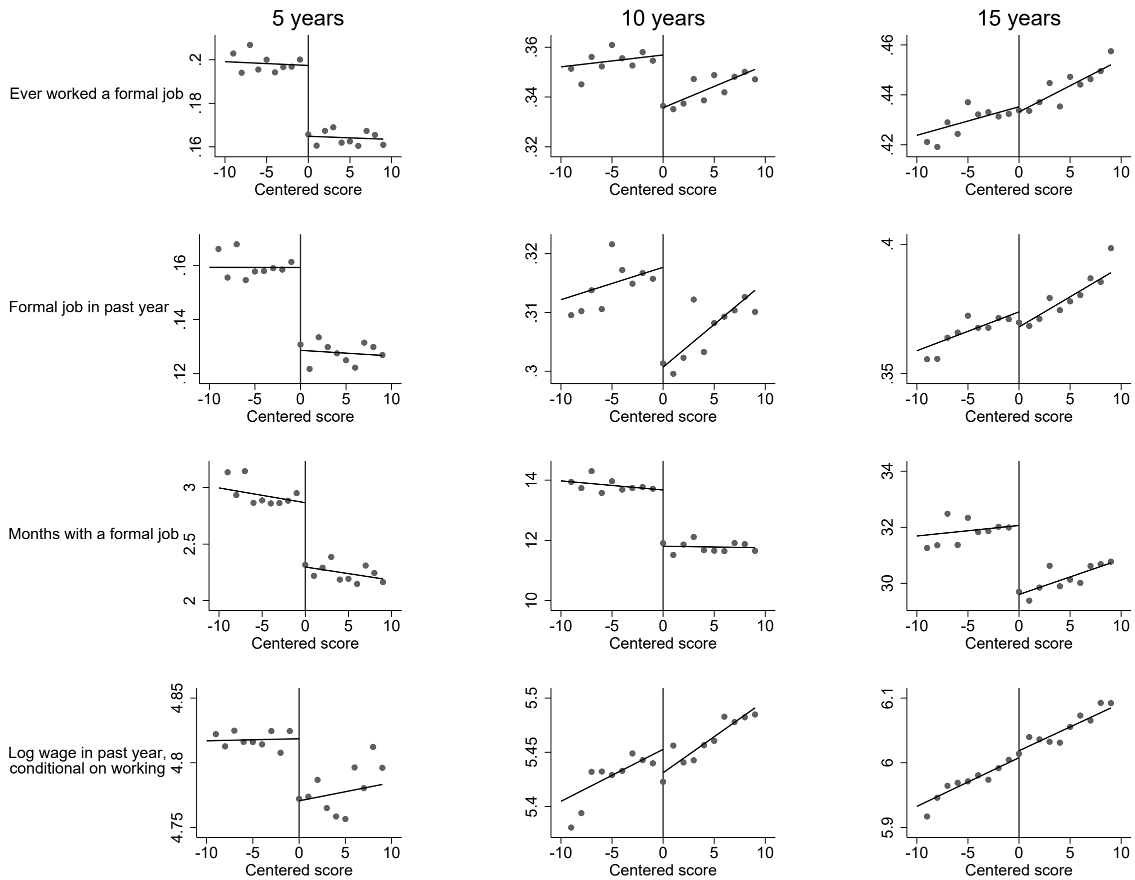


Figure 1: Elite high school assignment effects on formal labor market outcomes

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes. Columns represent number of years since assignment. Rows represent outcomes. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel.

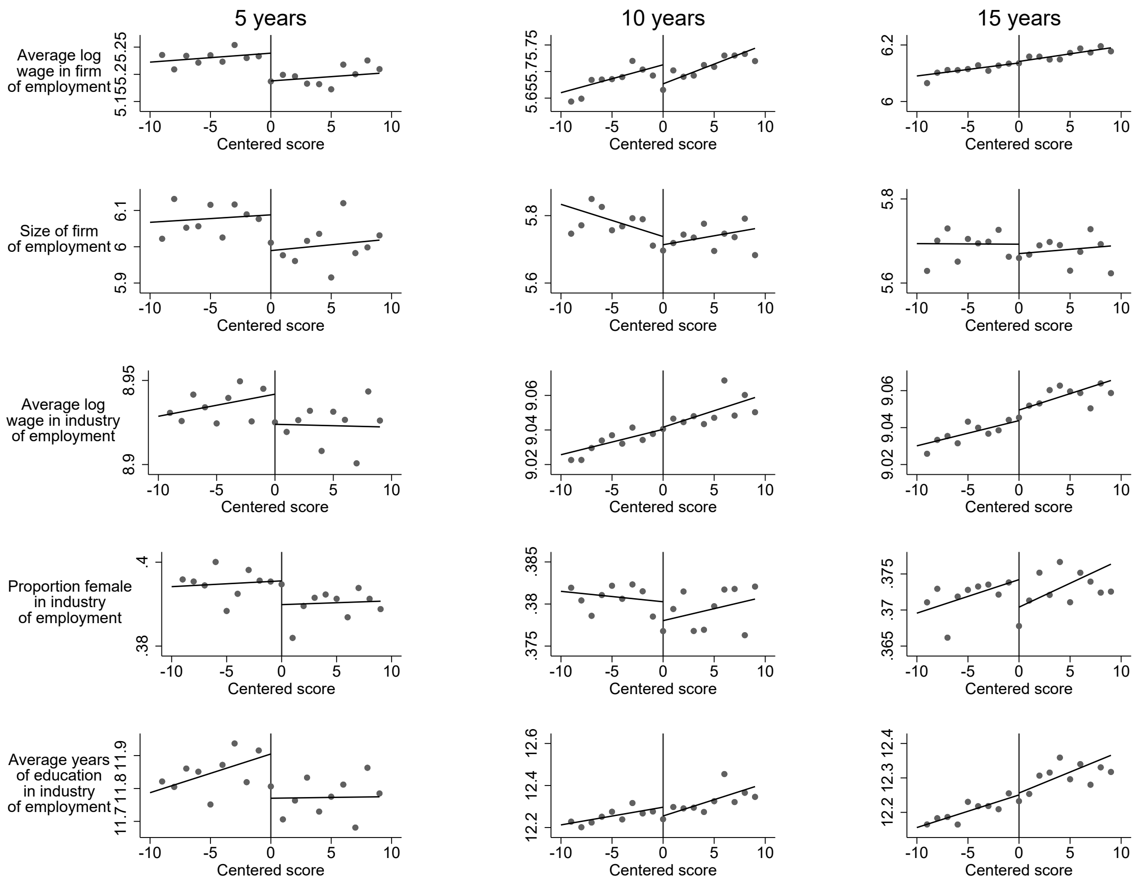


Figure 2: Elite high school assignment effects on formal job characteristics

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on formal job characteristics, conditional on formal employment. Columns represent number of years since assignment. Rows represent outcomes. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel.

Table 1: Characteristics of Students

	(1) Rejected	(2) Accepted	(3) t-test difference	(4) Rejected within 10 points from cutoff	(5) Accepted within 10 points from cutoff	(6) t-test difference
Panel A. Students' characteristics (COMIPEMS)						
Has parents with High School	0.442	0.644	-147.287	0.531	0.591	-26.194
	(0.497)	(0.479)		(0.499)	(0.492)	
Has high GPA	0.418	0.670	-190.000	0.521	0.604	-36.408
	(0.493)	(0.470)		(0.500)	(0.489)	
Women	0.581	0.432	106.581	0.514	0.469	19.492
	(0.493)	(0.495)		(0.500)	(0.499)	
Average Relative Score	-19.538	11.356	-1100.011	-5.123	4.195	-740.877
	(11.789)	(8.880)		(2.574)	(2.858)	
# Individuals	431660	174907		103313	87076	
Panel B. Outcomes 5 years after taking the test						
Ever worked a formal job	0.191	0.158	31.033	0.199	0.164	19.497
	(0.393)	(0.365)		(0.399)	(0.370)	
Formal job in past year	0.156	0.122	36.436	0.160	0.128	19.854
	(0.363)	(0.327)		(0.366)	(0.334)	
Months with a formal job	2.898	2.085	39.295	2.949	2.249	19.763
	(8.186)	(6.910)		(8.187)	(7.263)	
Log-wage in past year,	4.815	4.772	12.148	4.818	4.779	7.036
Conditional on working	(0.452)	(0.459)		(0.462)	(0.453)	
# Individuals	431660	174907		103313	87076	
Panel C. Outcomes 10 years after taking the test						
Ever worked a formal job	0.327	0.398	-20.842	0.354	0.343	5.172
	(0.469)	(0.490)		(0.478)	(0.475)	
Formal job in past year	0.291	0.338	-21.096	0.314	0.307	3.684
	(0.454)	(0.473)		(0.464)	(0.461)	
Months with a formal job	13.213	11.981	23.725	13.827	11.783	19.147
	(24.382)	(21.004)		(24.469)	(22.087)	
Log-wage in past year,	5.362	5.767	-46.061	5.425	5.459	-6.070
Conditional on working	(0.635)	(0.714)		(0.660)	(0.684)	
# Individuals	431660	174907		103313	87076	
Panel D. Outcomes 15 years after taking the test						
Ever worked a formal job	0.396	0.459	-44.794	0.429	0.442	-5.938
	(0.489)	(0.498)		(0.495)	(0.497)	
Formal job in past year	0.326	0.396	-51.289	0.366	0.378	-5.802
	(0.469)	(0.489)		(0.482)	(0.485)	
Months with a formal job	29.258	31.015	-14.518	31.825	30.126	8.476
	(44.314)	(42.020)		(44.998)	(42.319)	
Log-wage in past year,	5.887	6.142	-71.309	5.968	6.051	-14.654
Conditional on working	(0.698)	(0.805)		(0.733)	(0.767)	
# Individuals	431660	174907		103313	87076	

Notes: Column 1 focuses on all rejected students, column 2 on all admitted students, column 4 on all rejected within 10 points of the cutoff, and column 5 on all admitted within 10 points of the cutoff; columns 3 and 6 show the t-statistic for the t-test of the difference across groups. Standard deviations in parentheses.
Sources: IMSS administrative data and COMIPEMS.

Table 2: Elite high school admission effects on formal labor market outcomes

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Ever worked a formal job			
Score $\geq$ cutoff	-0.032	-0.021	-0.002
	(0.0038)***	(0.0047)***	(0.0049)
Honest 95% CI	[-0.047, -0.017]	[-0.037, -0.005]	[-0.017, 0.013]
Mean of DV 1 point below cutoff	0.200	0.355	0.432
Bandwidth	10.0	10.0	10.0
Observations	190389	190389	190389
Panel B. Formal job in past year			
Score $\geq$ cutoff	-0.031	-0.017	-0.006
	(0.0035)***	(0.0045)***	(0.0047)
Honest 95% CI	[-0.039, -0.022]	[-0.028, -0.006]	[-0.015, 0.004]
Mean of DV 1 point below cutoff	0.161	0.316	0.371
Bandwidth	10.0	10.0	10.0
Observations	190389	190389	190389
Panel C. Months with a formal job			
Score $\geq$ cutoff	-0.567	-1.861	-2.442
	(0.0763)***	(0.2296)***	(0.4296)***
Honest 95% CI	[-0.833, -0.301]	[-2.597, -1.125]	[-3.667, -1.216]
Mean of DV 1 point below cutoff	2.950	13.714	31.990
Bandwidth	10.0	10.0	10.0
Observations	190389	190389	190389
Panel D. Log wage in past year, conditional on working			
Score $\geq$ cutoff	-0.053	-0.026	0.002
	(0.0118)***	(0.0117)**	(0.0115)
Honest 95% CI	[-0.097, -0.010]	[-0.067, 0.015]	[-0.049, 0.054]
Mean of DV 1 point below cutoff	4.824	5.440	6.004
Bandwidth	10.0	10.0	10.0
Observations	27648	59171	70711

Notes: This table shows the long-term average effects of marginal admission to an elite high school on labor market outcomes. Each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. “Mean of DV” is the mean of the dependent variable. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The 95% honest confidence intervals are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Elite high school admission effects on characteristics of formal jobs

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Average Wage in Firm of Employment			
Score ≥ cutoff	-0.056	-0.039	-0.002
	(0.0139)***	(0.0123)***	(0.0110)
Honest 95% CI	[-0.127, 0.016]	[-0.091, 0.013]	[-0.042, 0.038]
Mean of DV 1 point below cutoff	5.233	5.692	6.134
Bandwidth	10.0	10.0	10.0
Observations	27648	59171	70711
Panel B. Size of Firm of Employment			
Score ≥ cutoff	-0.095	-0.024	-0.023
	(0.0523)*	(0.0358)	(0.0359)
Honest 95% CI	[-0.250, 0.060]	[-0.229, 0.180]	[-0.147, 0.102]
Mean of DV 1 point below cutoff	6.077	5.711	5.663
Bandwidth	10.0	10.0	10.0
Observations	27648	59171	70711
Panel C. Average Ln(Wage) in Industry of Employment			
Score ≥ cutoff	-0.017	0.002	0.006
	(0.0088)**	(0.0057)	(0.0050)
Honest 95% CI	[-0.064, 0.030]	[-0.025, 0.029]	[-0.007, 0.018]
Mean of DV 1 point below cutoff	8.945	9.038	9.044
Bandwidth	10.0	10.0	10.0
Observations	27648	59171	70711
Panel D. Proportion of Female in Industry of Employment			
Score ≥ cutoff	-0.005	-0.002	-0.003
	(0.0034)	(0.0025)	(0.0024)
Honest 95% CI	[-0.013, 0.002]	[-0.016, 0.013]	[-0.017, 0.010]
Mean of DV 1 point below cutoff	0.395	0.379	0.374
Bandwidth	10.0	10.0	10.0
Observations	27648	59171	70711
Panel E. Average Years of Education in Industry of Employment			
Score ≥ cutoff	-0.132	-0.036	0.010
	(0.0482)***	(0.0337)	(0.0303)
Honest 95% CI	[-0.421, 0.156]	[-0.156, 0.083]	[-0.074, 0.093]
Mean of DV 1 point below cutoff	11.916	12.276	12.255
Bandwidth	10.0	10.0	10.0
Observations	27648	59171	70711

Notes: This table shows the long-term average effects of marginal admission to an elite high school on firm and industry labor indicators. Each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. "Mean of DV" is the mean of the dependent variable. Observations are weighted using the edge kernel. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The 95% honest confidence intervals are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Heterogeneous effects of elite high school admission: middle school GPA

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Ever worked a formal job			
Low × (score ≥ cutoff)	-0.032	-0.027	-0.012
	(0.0060)***	(0.0070)***	(0.0073)*
High × (score ≥ cutoff)	-0.031	-0.016	0.005
	(0.0049)***	(0.0063)***	(0.0065)
Honest 95% CI, low	[-0.070, 0.006]	[-0.057, 0.002]	[-0.065, 0.041]
Honest 95% CI, high	[-0.058, -0.005]	[-0.046, 0.013]	[-0.028, 0.037]
Difference in effects	-0.001	-0.011	-0.017
p-value of difference	0.924	0.246	0.085
Mean of DV 1 point below cutoff, low	0.228	0.354	0.420
Mean of DV 1 point below cutoff, high	0.178	0.355	0.442
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel B. Formal job in past year			
Low × (score ≥ cutoff)	-0.034	-0.022	-0.012
	(0.0055)***	(0.0068)***	(0.0071)*
High × (score ≥ cutoff)	-0.027	-0.013	-0.001
	(0.0044)***	(0.0061)**	(0.0064)
Honest 95% CI, low	[-0.068, 0.001]	[-0.066, 0.022]	[-0.062, 0.037]
Honest 95% CI, high	[-0.045, -0.009]	[-0.034, 0.008]	[-0.023, 0.020]
Difference in effects	-0.006	-0.009	-0.011
p-value of difference	0.372	0.330	0.262
Mean of DV 1 point below cutoff, low	0.189	0.310	0.359
Mean of DV 1 point below cutoff, high	0.139	0.320	0.381
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel C. Months with a formal job			
Low × (score ≥ cutoff)	-0.544	-1.507	-2.312
	(0.1299)***	(0.3692)***	(0.6674)***
High × (score ≥ cutoff)	-0.558	-2.072	-2.513
	(0.0903)***	(0.2908)***	(0.5623)***
Honest 95% CI, low	[-1.244, 0.156]	[-3.359, 0.345]	[-6.093, 1.468]
Honest 95% CI, high	[-0.973, -0.144]	[-3.203, -0.941]	[-4.727, -0.300]
Difference in effects	0.014	0.565	0.201
p-value of difference	0.929	0.229	0.818
Mean of DV 1 point below cutoff, low	3.673	14.831	32.371
Mean of DV 1 point below cutoff, high	2.369	12.817	31.683
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel D. Log wage in past year, conditional on working			
Low × (score ≥ cutoff)	-0.034	-0.047	-0.022
	(0.0169)**	(0.0176)***	(0.0175)
High × (score ≥ cutoff)	-0.073	-0.014	0.016
	(0.0164)***	(0.0156)	(0.0152)
Honest 95% CI, low	[-0.121, 0.053]	[-0.200, 0.105]	[-0.075, 0.032]
Honest 95% CI, high	[-0.117, -0.029]	[-0.055, 0.026]	[-0.049, 0.080]
Difference in effects	0.039	-0.033	-0.038
p-value of difference	0.103	0.161	0.105
Mean of DV 1 point below cutoff, low	4.806	5.385	5.927
Mean of DV 1 point below cutoff, high	4.844	5.482	6.063
Bandwidth	10.0	10.0	10.0
N	27648	59171	70711

Notes: This table shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by students' middle school GPA. Each subgroup in each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. "Mean of DV" is the mean of the dependent variable. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The test for equality of group-specific elite assignment effects uses the robust standard errors. The 95% honest confidence intervals for each subgroup are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Heterogeneous effects of elite high school admission: parental education

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Ever worked a formal job			
Low × (score ≥ cutoff)	-0.035	-0.017	-0.003
	(0.0059)***	(0.0070)**	(0.0073)
High × (score ≥ cutoff)	-0.031	-0.024	-0.002
	(0.0050)***	(0.0062)***	(0.0065)
Honest 95% CI, low	[-0.052, -0.018]	[-0.043, 0.008]	[-0.024, 0.018]
Honest 95% CI, high	[-0.050, -0.012]	[-0.042, -0.006]	[-0.024, 0.021]
Difference in effects	-0.004	0.006	-0.001
p-value of difference	0.591	0.505	0.906
Mean of DV 1 point below cutoff, low	0.214	0.352	0.420
Mean of DV 1 point below cutoff, high	0.190	0.357	0.442
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel B. Formal job in past year			
Low × (score ≥ cutoff)	-0.035	-0.015	-0.006
	(0.0054)***	(0.0068)**	(0.0071)
High × (score ≥ cutoff)	-0.028	-0.018	-0.005
	(0.0045)***	(0.0060)***	(0.0064)
Honest 95% CI, low	[-0.053, -0.017]	[-0.032, 0.002]	[-0.024, 0.011]
Honest 95% CI, high	[-0.041, -0.015]	[-0.033, -0.004]	[-0.024, 0.014]
Difference in effects	-0.007	0.003	-0.001
p-value of difference	0.356	0.718	0.920
Mean of DV 1 point below cutoff, low	0.177	0.314	0.364
Mean of DV 1 point below cutoff, high	0.149	0.317	0.376
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel C. Months with a formal job			
Low × (score ≥ cutoff)	-0.561	-2.001	-2.835
	(0.1240)***	(0.3638)***	(0.6687)***
High × (score ≥ cutoff)	-0.593	-1.788	-2.181
	(0.0953)***	(0.2944)***	(0.5602)***
Honest 95% CI, low	[-1.052, -0.070]	[-2.783, -1.219]	[-4.493, -1.176]
Honest 95% CI, high	[-0.938, -0.249]	[-2.759, -0.818]	[-4.040, -0.322]
Difference in effects	0.032	-0.212	-0.653
p-value of difference	0.836	0.650	0.454
Mean of DV 1 point below cutoff, low	3.320	14.619	32.786
Mean of DV 1 point below cutoff, high	2.665	13.016	31.377
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel D. Log wage in past year, conditional on working			
Low × (score ≥ cutoff)	-0.045	-0.019	-0.016
	(0.0166)***	(0.0173)	(0.0171)
High × (score ≥ cutoff)	-0.062	-0.031	0.017
	(0.0167)***	(0.0159)**	(0.0154)
Honest 95% CI, low	[-0.096, 0.006]	[-0.059, 0.021]	[-0.077, 0.045]
Honest 95% CI, high	[-0.113, -0.011]	[-0.068, 0.005]	[-0.043, 0.078]
Difference in effects	0.017	0.012	-0.034
p-value of difference	0.469	0.607	0.146
Mean of DV 1 point below cutoff, low	4.803	5.402	5.946
Mean of DV 1 point below cutoff, high	4.844	5.469	6.048
Bandwidth	10.0	10.0	10.0
N	27648	59171	70711

Notes: This table shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by students' parental education. Each subgroup in each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. "Mean of DV" is the mean of the dependent variable. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The test for equality of group-specific elite assignment effects uses the robust standard errors. The 95% honest confidence intervals for each subgroup are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Heterogeneous effects of elite high school admission: gender

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Ever worked a formal job			
Male × (score ≥ cutoff)	-0.037	-0.031	-0.015
	(0.0057)***	(0.0067)***	(0.0069)**
Female × (score ≥ cutoff)	-0.027	-0.010	0.012
	(0.0050)***	(0.0065)	(0.0069)*
Honest 95% CI, male	[-0.053, -0.022]	[-0.047, -0.016]	[-0.030, -0.001]
Honest 95% CI, female	[-0.050, -0.003]	[-0.025, 0.004]	[-0.013, 0.037]
Difference in effects	-0.011	-0.021	-0.027
p-value of difference	0.152	0.025	0.005
Mean of DV 1 point below cutoff, male	0.239	0.390	0.462
Mean of DV 1 point below cutoff, female	0.160	0.318	0.401
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel B. Formal job in past year			
Male × (score ≥ cutoff)	-0.036	-0.027	-0.016
	(0.0053)***	(0.0065)***	(0.0068)**
Female × (score ≥ cutoff)	-0.024	-0.007	0.006
	(0.0044)***	(0.0062)	(0.0066)
Honest 95% CI, male	[-0.052, -0.021]	[-0.041, -0.013]	[-0.036, 0.004]
Honest 95% CI, female	[-0.041, -0.008]	[-0.026, 0.012]	[-0.015, 0.026]
Difference in effects	-0.012	-0.020	-0.022
p-value of difference	0.083	0.028	0.021
Mean of DV 1 point below cutoff, male	0.197	0.349	0.404
Mean of DV 1 point below cutoff, female	0.124	0.281	0.337
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel C. Months with a formal job			
Male × (score ≥ cutoff)	-0.680	-2.306	-3.382
	(0.1222)***	(0.3553)***	(0.6434)***
Female × (score ≥ cutoff)	-0.436	-1.380	-1.426
	(0.0895)***	(0.2863)***	(0.5644)**
Honest 95% CI, male	[-0.932, -0.427]	[-3.015, -1.597]	[-4.718, -2.046]
Honest 95% CI, female	[-0.768, -0.105]	[-2.186, -0.574]	[-2.692, -0.160]
Difference in effects	-0.243	-0.925	-1.956
p-value of difference	0.108	0.043	0.022
Mean of DV 1 point below cutoff, male	3.743	16.369	36.403
Mean of DV 1 point below cutoff, female	2.129	10.967	27.423
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel D. Log wage in past year, conditional on working			
Male × (score ≥ cutoff)	-0.050	-0.031	-0.015
	(0.0151)***	(0.0160)**	(0.0158)
Female × (score ≥ cutoff)	-0.057	-0.019	0.023
	(0.0188)***	(0.0172)	(0.0167)
Honest 95% CI, male	[-0.091, -0.009]	[-0.076, 0.013]	[-0.082, 0.051]
Honest 95% CI, female	[-0.176, 0.063]	[-0.085, 0.048]	[-0.025, 0.071]
Difference in effects	0.007	-0.013	-0.038
p-value of difference	0.783	0.582	0.098
Mean of DV 1 point below cutoff, male	4.834	5.429	6.017
Mean of DV 1 point below cutoff, female	4.809	5.454	5.989
Bandwidth	10.0	10.0	10.0
N	27648	59171	70711

Notes: This table shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by students' sex. Each subgroup in each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. "Mean of DV" is the mean of the dependent variable. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The test for equality of group-specific elite assignment effects uses the robust standard errors. The 95% honest confidence intervals for each subgroup are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Heterogeneous effects of elite high school admission: STEM elite school

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Ever worked a formal job			
STEM × (score ≥ cutoff)	-0.021	0.001	0.009
	(0.0075)***	(0.0092)	(0.0094)
Non-STEM × (score ≥ cutoff)	-0.036	-0.029	-0.007
	(0.0044)***	(0.0054)***	(0.0057)
Honest 95% CI, STEM	[-0.038, -0.005]	[-0.018, 0.020]	[-0.010, 0.028]
Honest 95% CI, Non-STEM	[-0.056, -0.017]	[-0.050, -0.009]	[-0.031, 0.018]
Difference in effects	0.015	0.030	0.016
p-value of difference	0.082	0.005	0.153
Mean of DV 1 point below cutoff, STEM	0.206	0.380	0.465
Mean of DV 1 point below cutoff, Non-STEM	0.198	0.345	0.421
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel B. Formal job in past year			
STEM × (score ≥ cutoff)	-0.026	-0.003	0.006
	(0.0069)***	(0.0090)	(0.0092)
Non-STEM × (score ≥ cutoff)	-0.032	-0.022	-0.010
	(0.0040)***	(0.0052)***	(0.0055)*
Honest 95% CI, STEM	[-0.043, -0.009]	[-0.022, 0.016]	[-0.019, 0.031]
Honest 95% CI, Non-STEM	[-0.052, -0.012]	[-0.034, -0.011]	[-0.034, 0.014]
Difference in effects	0.006	0.020	0.016
p-value of difference	0.449	0.059	0.138
Mean of DV 1 point below cutoff, STEM	0.171	0.351	0.390
Mean of DV 1 point below cutoff, Non-STEM	0.158	0.303	0.364
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel C. Months with a formal job			
STEM × (score ≥ cutoff)	-0.404	-1.354	-1.335
	(0.1471)***	(0.4597)***	(0.8513)
Non-STEM × (score ≥ cutoff)	-0.631	-2.058	-2.874
	(0.0893)***	(0.2648)***	(0.4975)***
Honest 95% CI, STEM	[-0.760, -0.047]	[-2.583, -0.125]	[-3.188, 0.519]
Honest 95% CI, Non-STEM	[-0.957, -0.305]	[-3.080, -1.037]	[-4.708, -1.041]
Difference in effects	0.227	0.704	1.540
p-value of difference	0.186	0.184	0.118
Mean of DV 1 point below cutoff, STEM	3.017	15.064	34.689
Mean of DV 1 point below cutoff, Non-STEM	2.926	13.227	31.017
Bandwidth	10.0	10.0	10.0
N	190389	190389	190389
Panel D. Log wage in past year, conditional on working			
STEM × (score ≥ cutoff)	-0.062	-0.017	0.025
	(0.0214)***	(0.0213)	(0.0206)
Non-STEM × (score ≥ cutoff)	-0.049	-0.030	-0.007
	(0.0141)***	(0.0140)**	(0.0138)
Honest 95% CI, STEM	[-0.119, -0.006]	[-0.061, 0.027]	[-0.051, 0.100]
Honest 95% CI, Non-STEM	[-0.106, 0.008]	[-0.065, 0.006]	[-0.054, 0.041]
Difference in effects	-0.013	0.013	0.032
p-value of difference	0.615	0.615	0.202
Mean of DV 1 point below cutoff, STEM	4.839	5.449	6.056
Mean of DV 1 point below cutoff, Non-STEM	4.819	5.436	5.985
Bandwidth	10.0	10.0	10.0
N	27648	59171	70711

Notes: This table shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by type of high school: STEM or Non-STEM. Each subgroup in each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. "Mean of DV" is the mean of the dependent variable. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The test for equality of group-specific elite assignment effects uses the robust standard errors. The 95% honest confidence intervals for each subgroup are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table 8: Heterogeneous effects of elite high school admission: STEM elite school, women subsample

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Ever worked a formal job			
STEM × (score ≥ cutoff)	-0.017	0.032	0.037
	(0.0114)	(0.0154)**	(0.0162)**
Non-STEM × (score ≥ cutoff)	-0.029	-0.020	0.006
	(0.0055)***	(0.0071)***	(0.0076)
Honest 95% CI, STEM	[-0.045, 0.011]	[-0.003, 0.066]	[-0.000, 0.074]
Honest 95% CI, Non-STEM	[-0.056, -0.001]	[-0.036, -0.003]	[-0.021, 0.034]
Difference in effects	0.012	0.051	0.030
p-value of difference	0.360	0.003	0.088
Mean of DV 1 point below cutoff, STEM	0.142	0.314	0.397
Mean of DV 1 point below cutoff, Non-STEM	0.164	0.319	0.402
Bandwidth	10.0	10.0	10.0
N	94005	94005	94005
Panel B. Formal job in past year			
STEM × (score ≥ cutoff)	-0.022	0.021	0.029
	(0.0102)**	(0.0150)	(0.0156)*
Non-STEM × (score ≥ cutoff)	-0.025	-0.013	0.001
	(0.0049)***	(0.0068)*	(0.0073)
Honest 95% CI, STEM	[-0.045, 0.001]	[-0.011, 0.053]	[-0.003, 0.060]
Honest 95% CI, Non-STEM	[-0.048, -0.002]	[-0.030, 0.005]	[-0.026, 0.027]
Difference in effects	0.003	0.034	0.028
p-value of difference	0.825	0.042	0.104
Mean of DV 1 point below cutoff, STEM	0.115	0.293	0.322
Mean of DV 1 point below cutoff, Non-STEM	0.126	0.279	0.341
Bandwidth	10.0	10.0	10.0
N	94005	94005	94005
Panel C. Months with a formal job			
STEM × (score ≥ cutoff)	-0.407	-0.831	0.422
	(0.2021)**	(0.6744)	(1.3274)
Non-STEM × (score ≥ cutoff)	-0.444	-1.499	-1.831
	(0.0998)***	(0.3164)***	(0.6237)***
Honest 95% CI, STEM	[-0.921, 0.107]	[-2.263, 0.601]	[-3.208, 4.052]
Honest 95% CI, Non-STEM	[-0.822, -0.066]	[-2.590, -0.409]	[-3.319, -0.343]
Difference in effects	0.037	0.668	2.253
p-value of difference	0.870	0.370	0.125
Mean of DV 1 point below cutoff, STEM	2.108	11.233	27.278
Mean of DV 1 point below cutoff, Non-STEM	2.134	10.911	27.454
Bandwidth	10.0	10.0	10.0
N	94005	94005	94005
Panel D. Log wage in past year, conditional on working			
STEM × (score ≥ cutoff)	-0.147	0.008	0.126
	(0.0444)***	(0.0377)	(0.0363)***
Non-STEM × (score ≥ cutoff)	-0.038	-0.025	0.001
	(0.0208)*	(0.0193)	(0.0188)
Honest 95% CI, STEM	[-0.316, 0.023]	[-0.120, 0.136]	[0.025, 0.226]
Honest 95% CI, Non-STEM	[-0.154, 0.077]	[-0.085, 0.035]	[-0.055, 0.057]
Difference in effects	-0.108	0.033	0.124
p-value of difference	0.028	0.435	0.002
Mean of DV 1 point below cutoff, STEM	4.882	5.468	5.989
Mean of DV 1 point below cutoff, Non-STEM	4.795	5.451	5.989
Bandwidth	10.0	10.0	10.0
N	10695	26416	32548

Notes: This table shows the long-term average effects of marginal admission to an elite high school on women's labor market outcomes by type of high school: STEM or Non-STEM. Each subgroup in each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. "Mean of DV" is the mean of the dependent variable. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The test for equality of group-specific elite assignment effects uses the robust standard errors. The 95% honest confidence intervals for each subgroup are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table 9: Heterogeneous effects of elite high school admission: STEM elite school, men subsample

	(1) 5 years	(2) 10 years	(3) 15 years
Panel A. Ever worked a formal job			
STEM × (score ≥ cutoff)	-0.022	-0.014	-0.004
	(0.0097)**	(0.0114)	(0.0116)
Non-STEM × (score ≥ cutoff)	-0.046	-0.042	-0.023
	(0.0071)***	(0.0083)***	(0.0085)***
Honest 95% CI, STEM	[-0.043, -0.002]	[-0.037, 0.009]	[-0.028, 0.021]
Honest 95% CI, Non-STEM	[-0.064, -0.028]	[-0.062, -0.021]	[-0.042, -0.003]
Difference in effects	0.024	0.028	0.019
p-value of difference	0.046	0.048	0.195
Mean of DV 1 point below cutoff, STEM	0.236	0.412	0.497
Mean of DV 1 point below cutoff, Non-STEM	0.241	0.378	0.444
Bandwidth	10.0	10.0	10.0
N	96384	96384	96384
Panel B. Formal job in past year			
STEM × (score ≥ cutoff)	-0.027	-0.013	-0.004
	(0.0090)***	(0.0112)	(0.0115)
Non-STEM × (score ≥ cutoff)	-0.042	-0.035	-0.024
	(0.0065)***	(0.0080)***	(0.0084)***
Honest 95% CI, STEM	[-0.048, -0.005]	[-0.041, 0.015]	[-0.034, 0.027]
Honest 95% CI, Non-STEM	[-0.059, -0.025]	[-0.054, -0.015]	[-0.043, -0.004]
Difference in effects	0.015	0.021	0.020
p-value of difference	0.165	0.121	0.160
Mean of DV 1 point below cutoff, STEM	0.197	0.378	0.423
Mean of DV 1 point below cutoff, Non-STEM	0.197	0.333	0.393
Bandwidth	10.0	10.0	10.0
N	96384	96384	96384
Panel C. Months with a formal job			
STEM × (score ≥ cutoff)	-0.361	-1.502	-2.005
	(0.1948)*	(0.5978)**	(1.0857)*
Non-STEM × (score ≥ cutoff)	-0.861	-2.772	-4.195
	(0.1560)***	(0.4418)***	(0.7987)***
Honest 95% CI, STEM	[-0.759, 0.036]	[-3.218, 0.215]	[-4.660, 0.650]
Honest 95% CI, Non-STEM	[-1.197, -0.525]	[-3.746, -1.797]	[-6.005, -2.384]
Difference in effects	0.500	1.270	2.190
p-value of difference	0.045	0.088	0.104
Mean of DV 1 point below cutoff, STEM	3.453	16.897	38.235
Mean of DV 1 point below cutoff, Non-STEM	3.901	16.081	35.406
Bandwidth	10.0	10.0	10.0
N	96384	96384	96384
Panel D. Log wage in past year, conditional on working			
STEM × (score ≥ cutoff)	-0.037	-0.027	-0.011
	(0.0245)	(0.0259)	(0.0250)
Non-STEM × (score ≥ cutoff)	-0.057	-0.035	-0.019
	(0.0192)***	(0.0204)*	(0.0204)
Honest 95% CI, STEM	[-0.103, 0.029]	[-0.089, 0.034]	[-0.109, 0.086]
Honest 95% CI, Non-STEM	[-0.145, 0.031]	[-0.100, 0.031]	[-0.079, 0.042]
Difference in effects	0.019	0.007	0.007
p-value of difference	0.531	0.824	0.823
Mean of DV 1 point below cutoff, STEM	4.827	5.442	6.080
Mean of DV 1 point below cutoff, Non-STEM	4.838	5.421	5.980
Bandwidth	10.0	10.0	10.0
N	16953	32755	38163

Notes: This table shows the long-term average effects of marginal admission to an elite high school on men's labor market outcomes by type of high school: STEM or Non-STEM. Each subgroup in each panel represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. "Mean of DV" is the mean of the dependent variable. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The test for equality of group-specific elite assignment effects uses the robust standard errors. The 95% honest confidence intervals for each subgroup are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Appendix A

In this appendix, we include the results of estimating the effect of marginal admission to elite high schools on several outcomes year-by-year, from 3 years to 15 years after taking the admission exam. We also include tests that suggest that there is no manipulation at the admission threshold and show that our results are robust under different bandwidths. Finally, we describe the sample by comparing the firm and industry characteristics of the admitted and rejected students.

Year-by-year Analysis

In the main text, we show the effect of elite high-school assignment on some formal labor market outcomes 5, 10 and 15 years after taking the COMIPMEMS exam. In this subsection of the Appendix, we do a year-by-year analysis of the treatment effect of elite high-school assignment on labor market outcomes, from 3 to 15 years after taking the COMIPEMS exam. This exercise has two purposes: to show that our results are not dependent on a particular chosen year (for example, 5 years or 6 years after taking the COMIPEMS exam) and to show the changes in trends as time goes by. The results are in Figures A.1-A.10, where each figure covers different subsamples or outcomes.

Diagnostic Exercises

In this subsection, we show diagnostic exercises that support the validity of the RD design.

Figure A.11 traces out predetermined student characteristics (indicators for gender, high school educated parent, and above-median middle school GPA) across the cutoff.

Table A.1 formalizes the analysis in Figure A.11, as it shows the results of estimating Equation 1 on predetermined student characteristics (indicators for gender, high school educated parent, and above-median middle school GPA).

Appendix Figure A.12 shows the density of the centered score, both for the full support in Panel A and restricted to a ten-point bandwidth in Panel B.

Bandwidth Sensitivity

In this subsection, we show that our results are robust to a large set of bandwidths. Specifically, we re-estimate our results for our main outcomes 5, 10 and 15 years after taking the COMIPEMS exam by bandwith. The results are shown in Figures A.13 through A.15.

Employment characteristics of Students 5, 10, and 15 years after taking the COMIPEMS Exam

Table A.2 presents additional summary statistics for admitted and rejected students, as well as for those who were accepted and rejected from an elite high school within 10 points of the score cutoff. More specifically, we repeat the analysis from Table 1, but with a different set of outcomes that can help us further characterize our sample. In particular, we focus on the average log-wage of the firm where the individual is employed, the firms' size, the average log-wage in the industry of employment, the proportion of women in that industry, and the average number of years of education in such industry. We compare these outcomes 5, 10, and 15 years after taking the COMIPEMS exam.

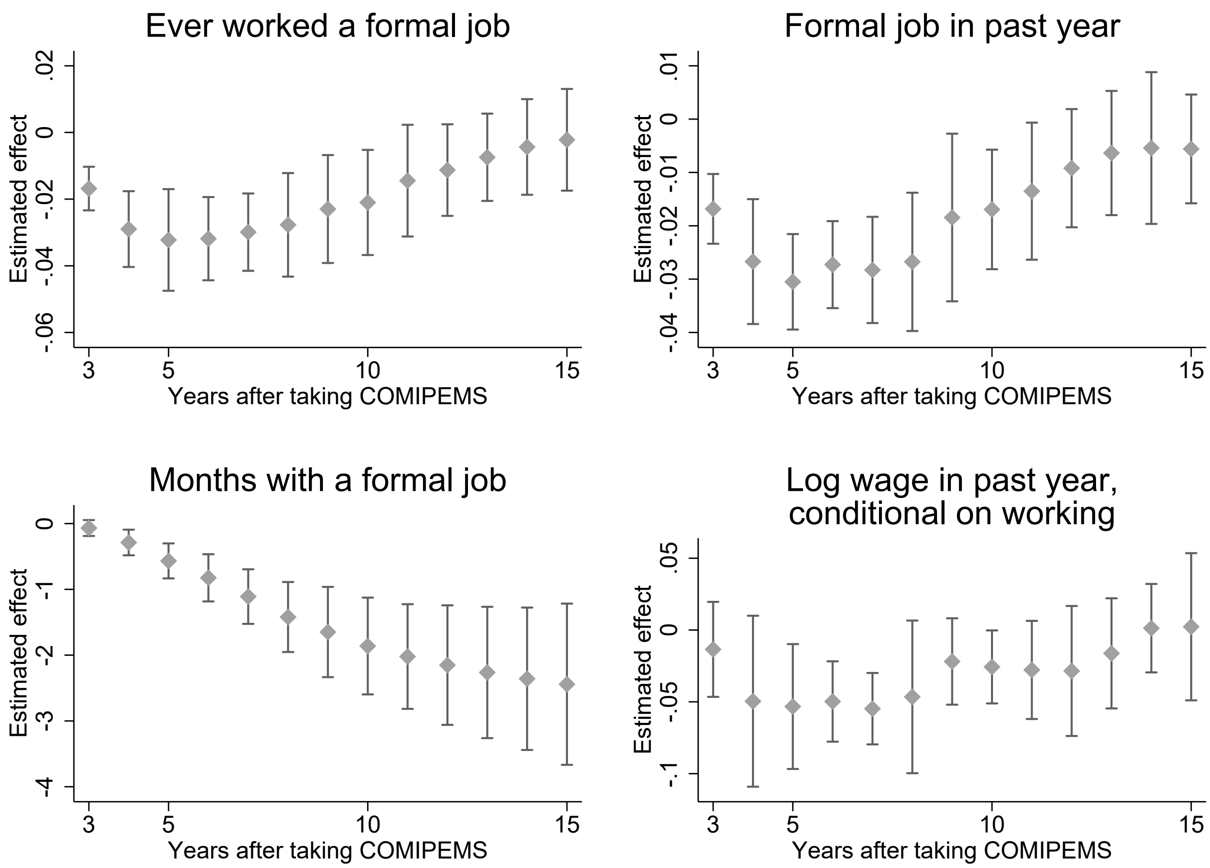


Figure A.1: Year-by-year analysis, main outcomes

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

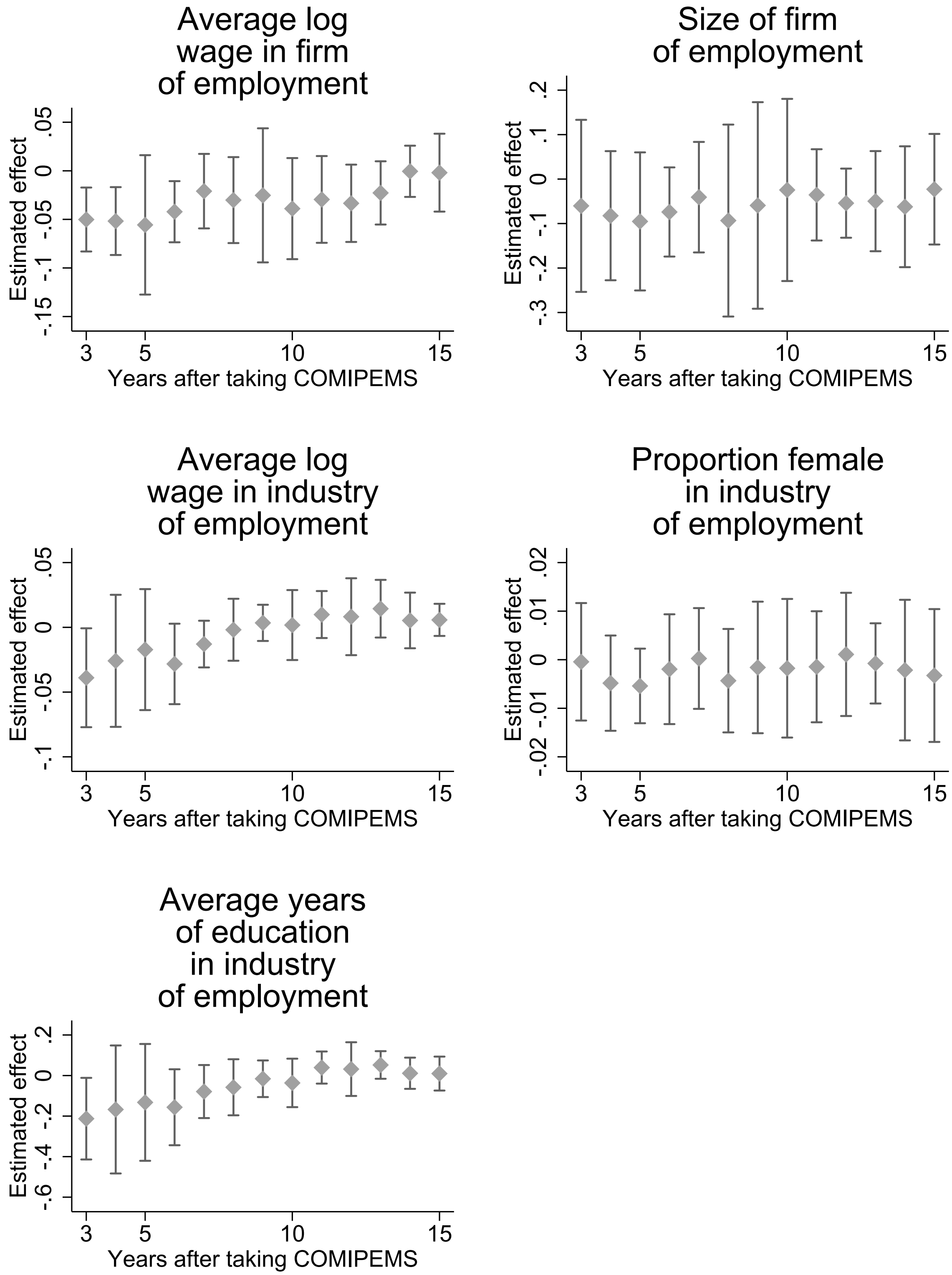


Figure A.2: Year-by-year analysis, job characteristics, full sample

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on firm and industry characteristics by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

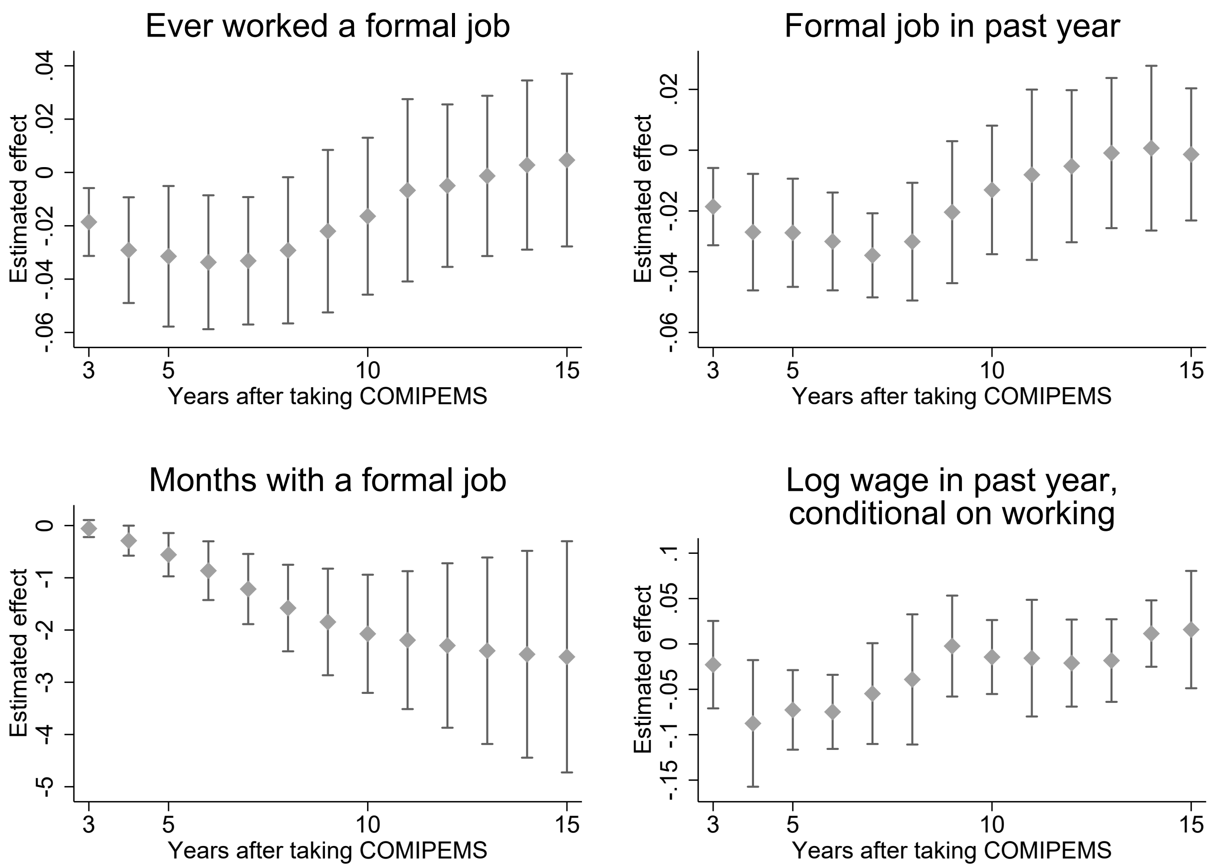


Figure A.3: Year-by-year analysis, high GPA

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

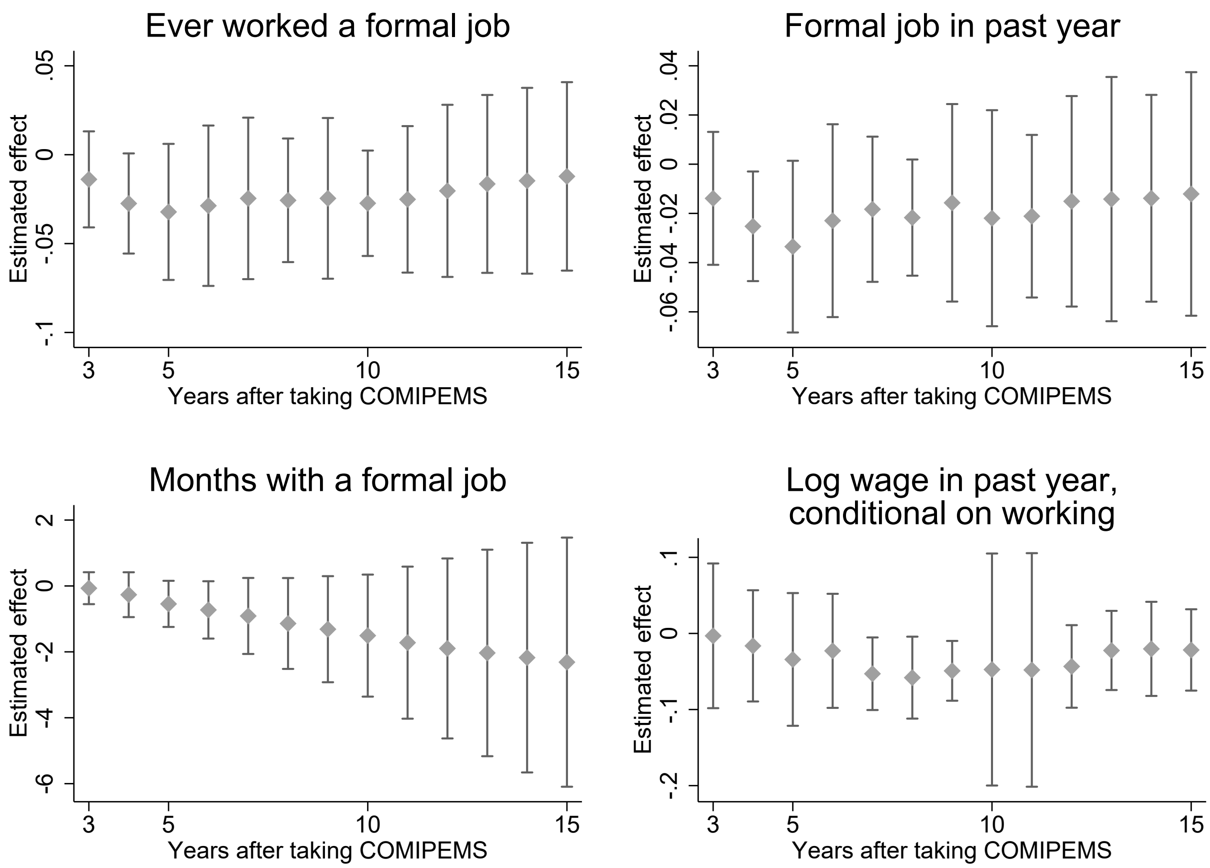


Figure A.4: Year-by-year analysis, low GPA

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

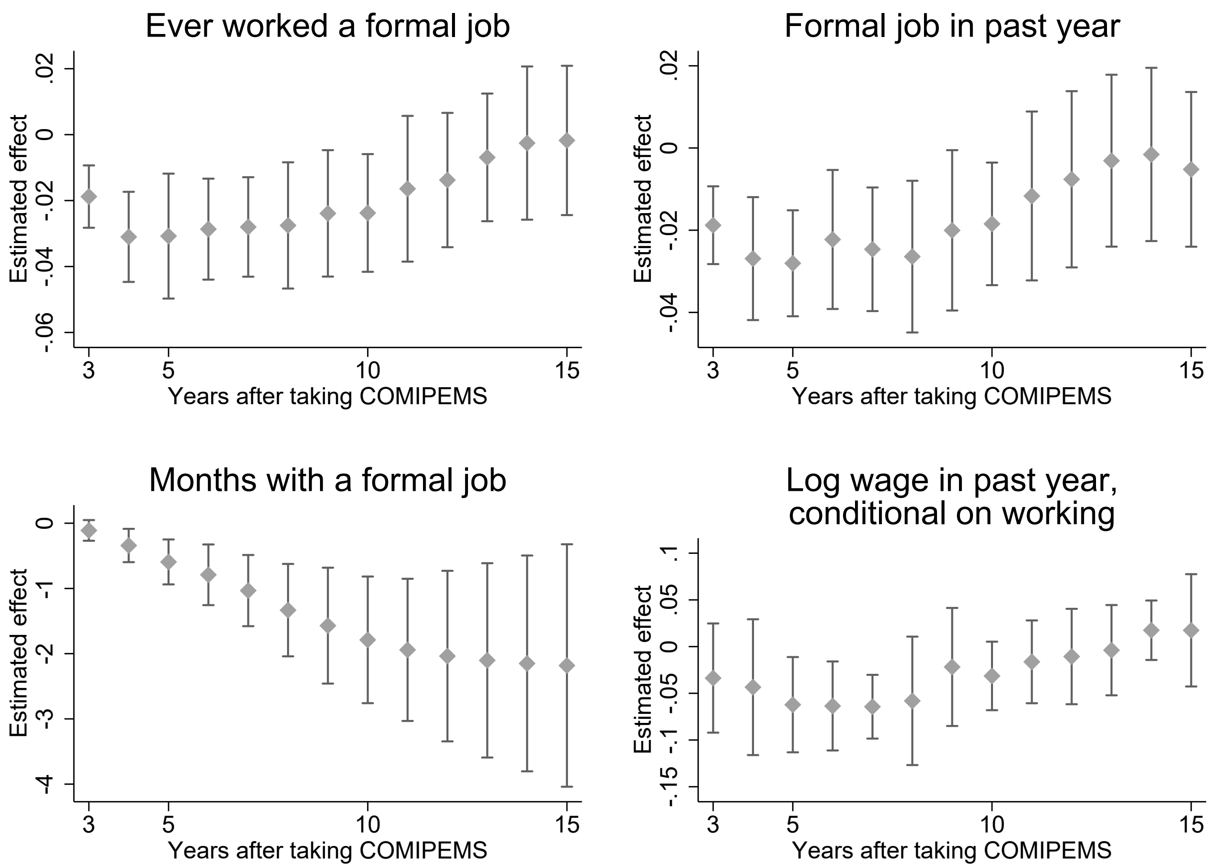


Figure A.5: Year-by-year analysis, parents with HS

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

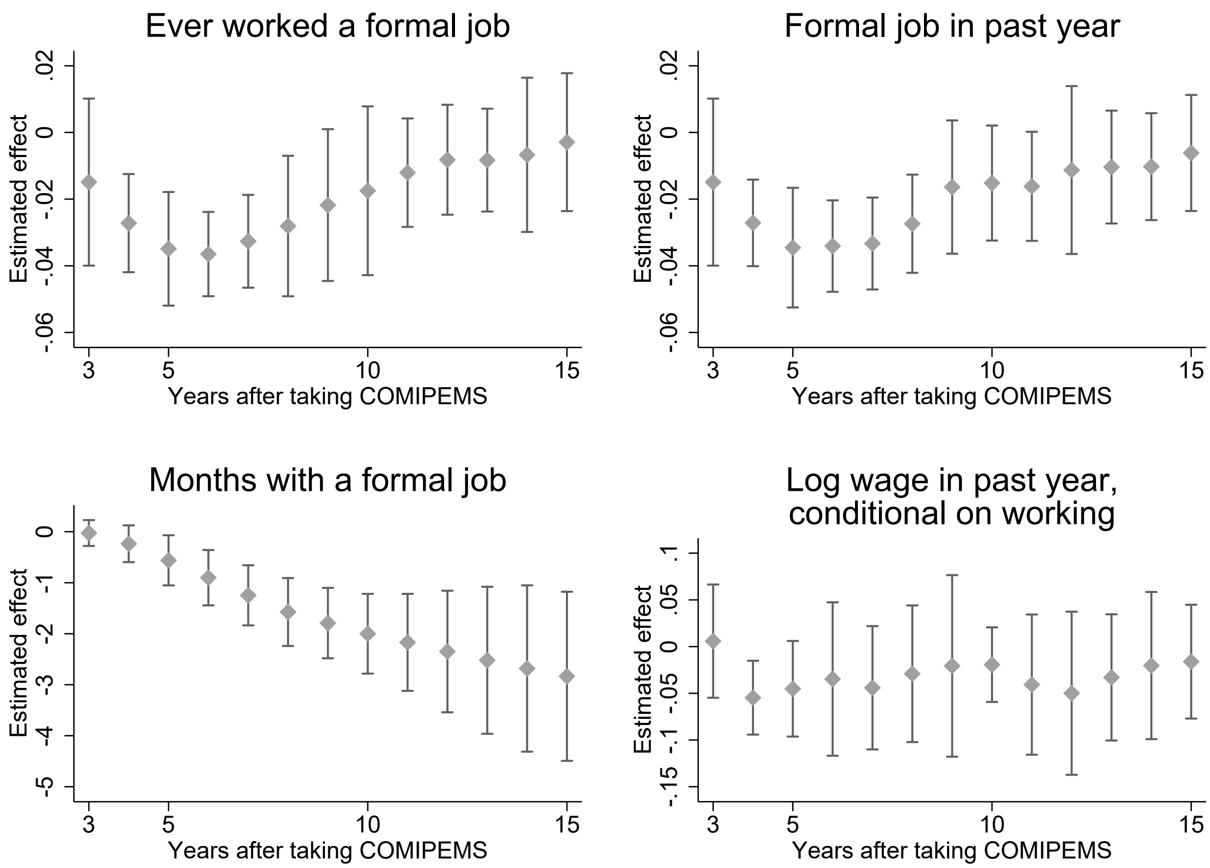


Figure A.6: Year-by-year analysis, parents with no HS

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

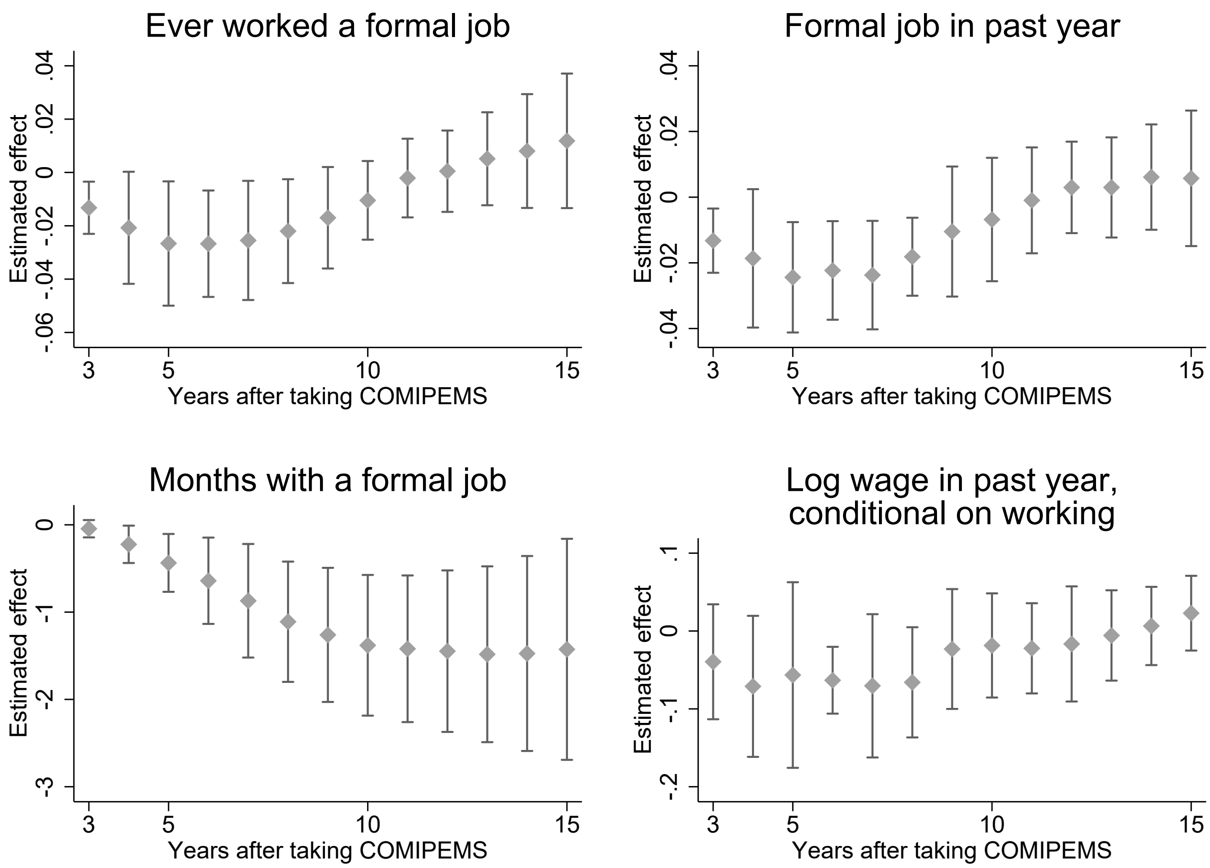


Figure A.7: Year-by-year analysis, women

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

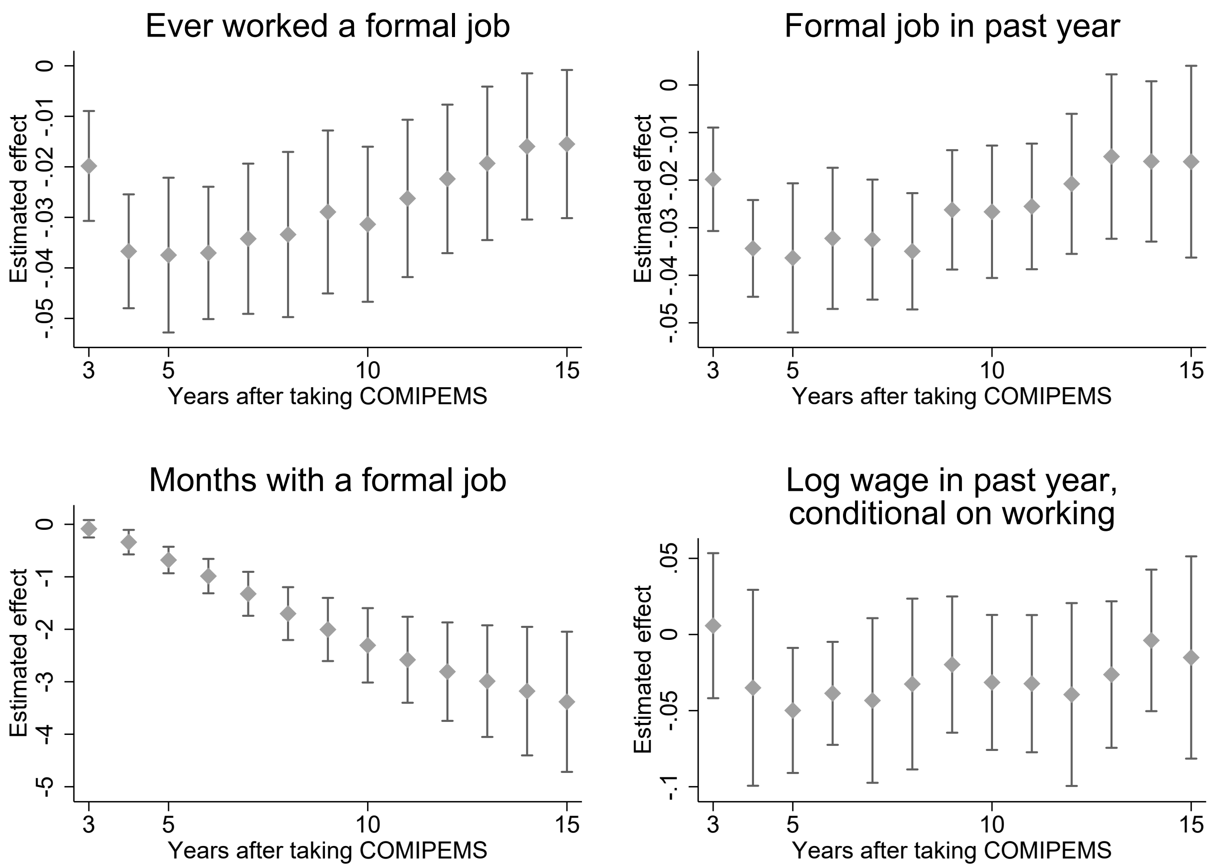


Figure A.8: Year-by-year analysis, men

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

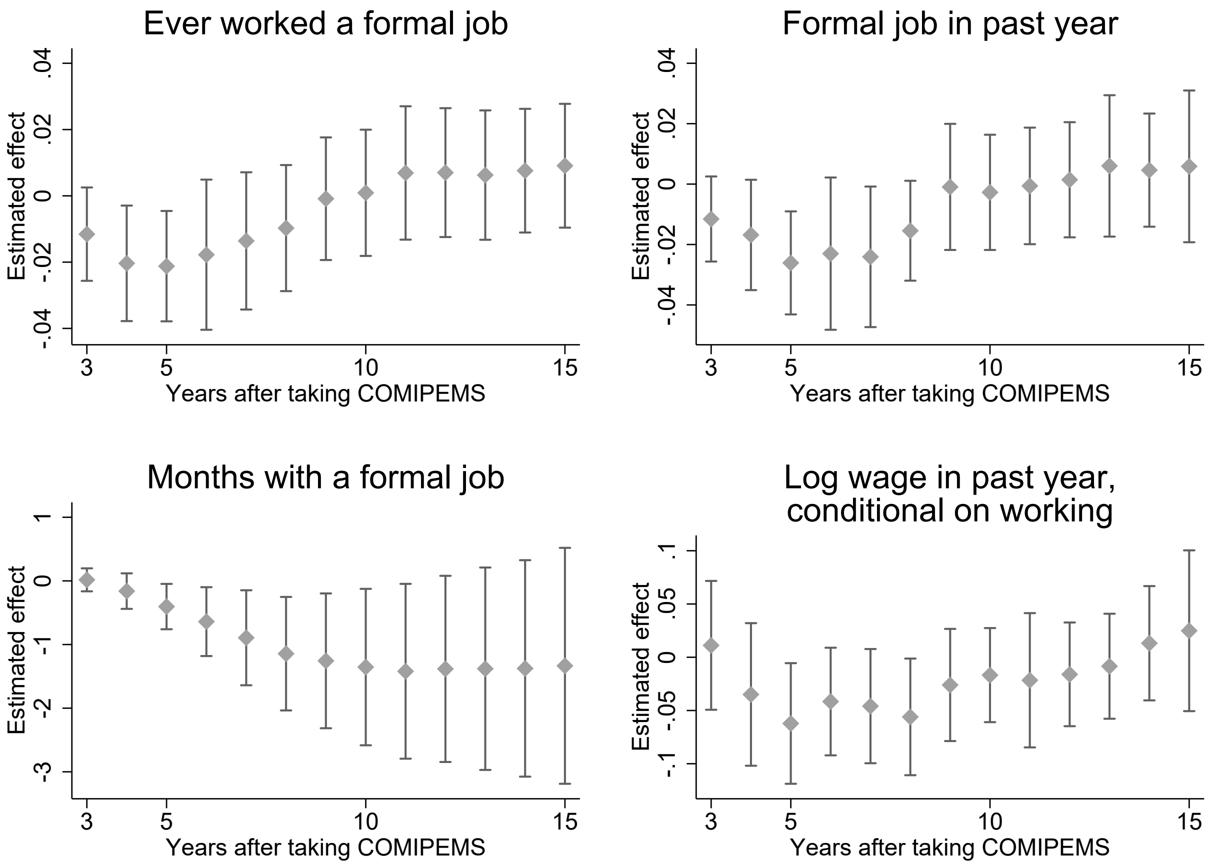


Figure A.9: Year-by-year analysis, STEM cutoff schools

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

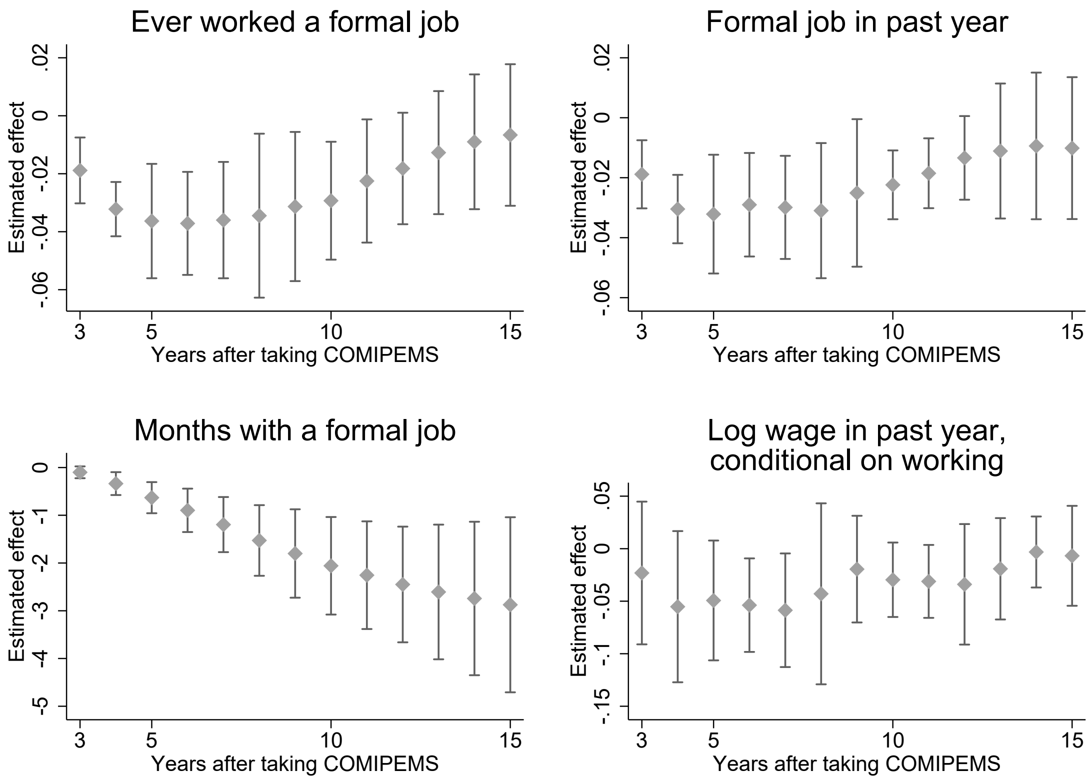


Figure A.10: Year-by-year analysis, non-STEM cutoff schools

Notes: This figure shows the long-term average effects of marginal admission to an elite high school on labor market outcomes by years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

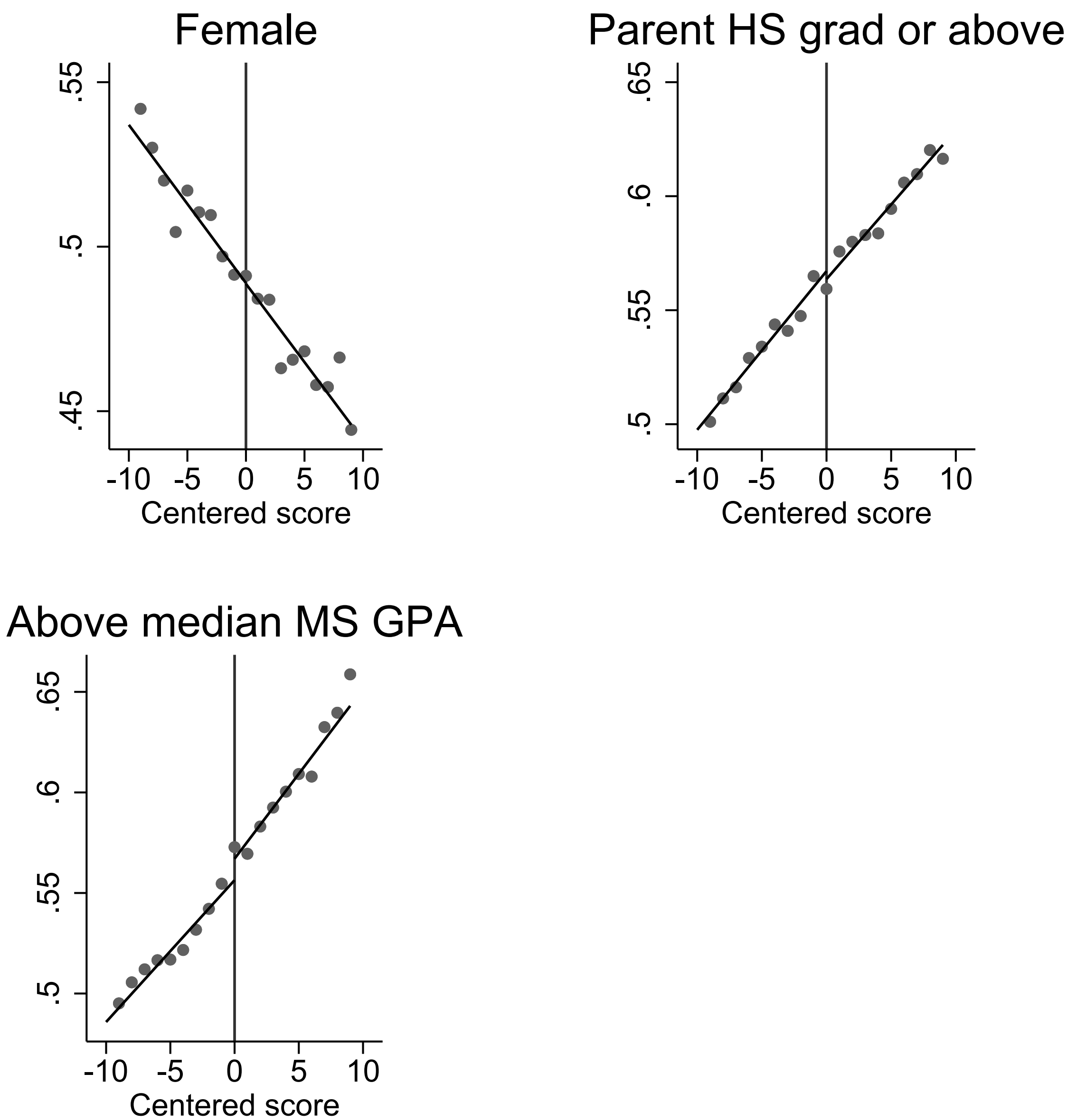
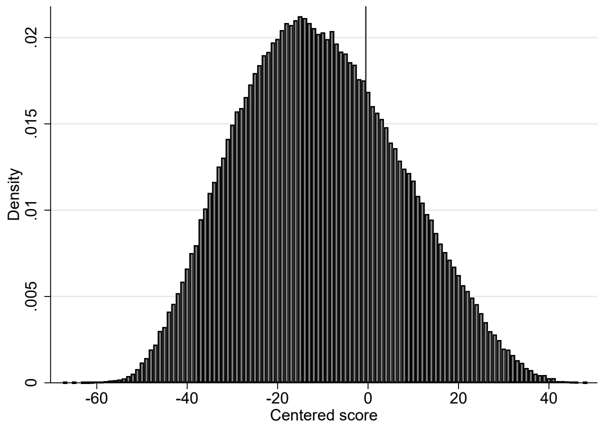
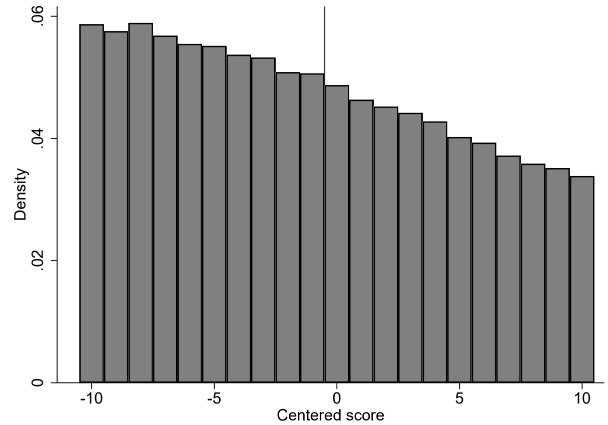


Figure A.11: Balance of exogenous covariates

Notes: This figure shows the average effects of marginal admission to an elite high school on students' characteristics. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel.



((a)) Full support



((b)) Support within 10-point bandwidth

Figure A.12: Density of running variable

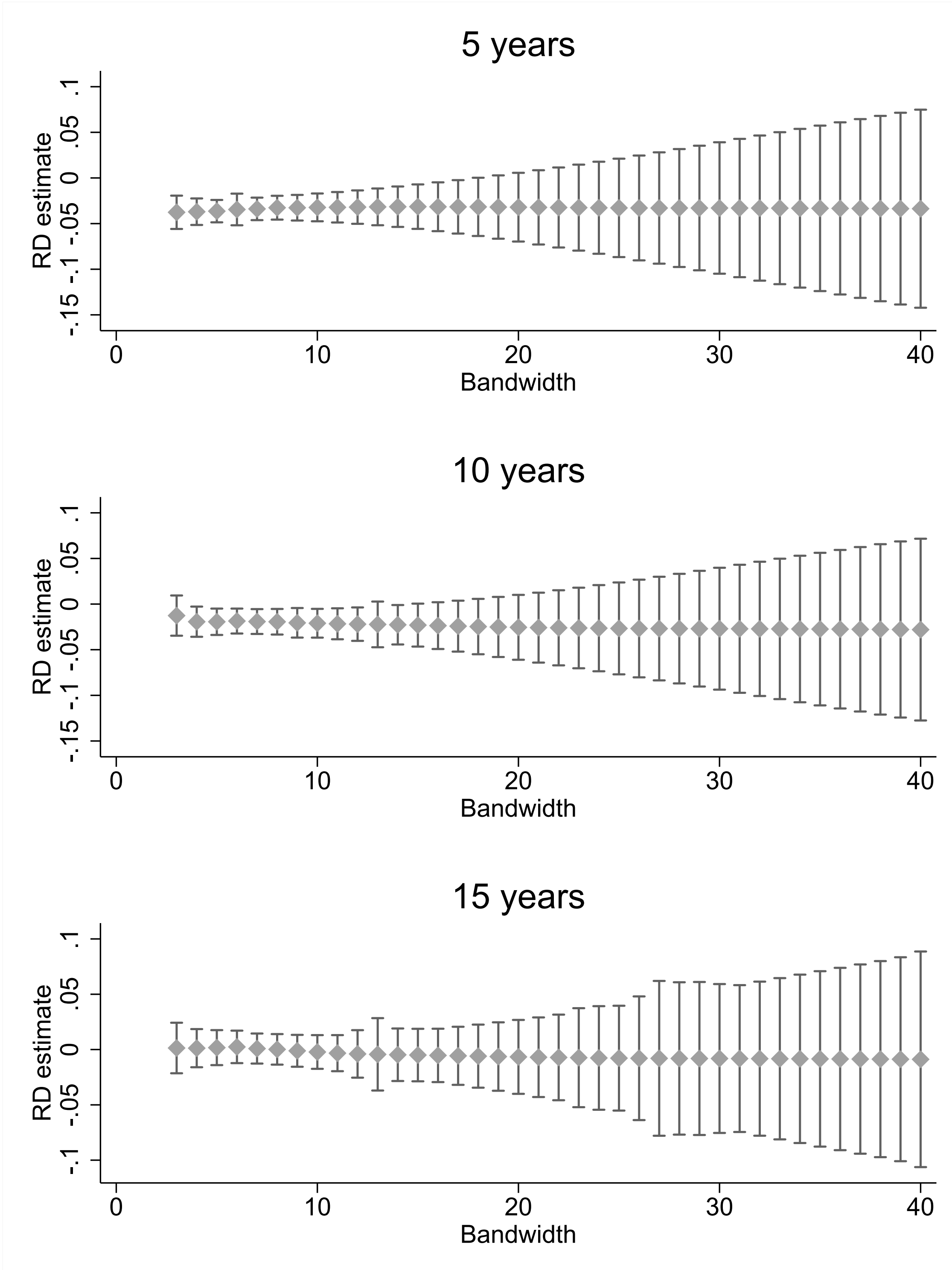


Figure A.13: Bandwidth sensitivity: ever worked a formal job

Notes: This figure shows the long-term average effects of marginal admission to an elite high school by bandwith and years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

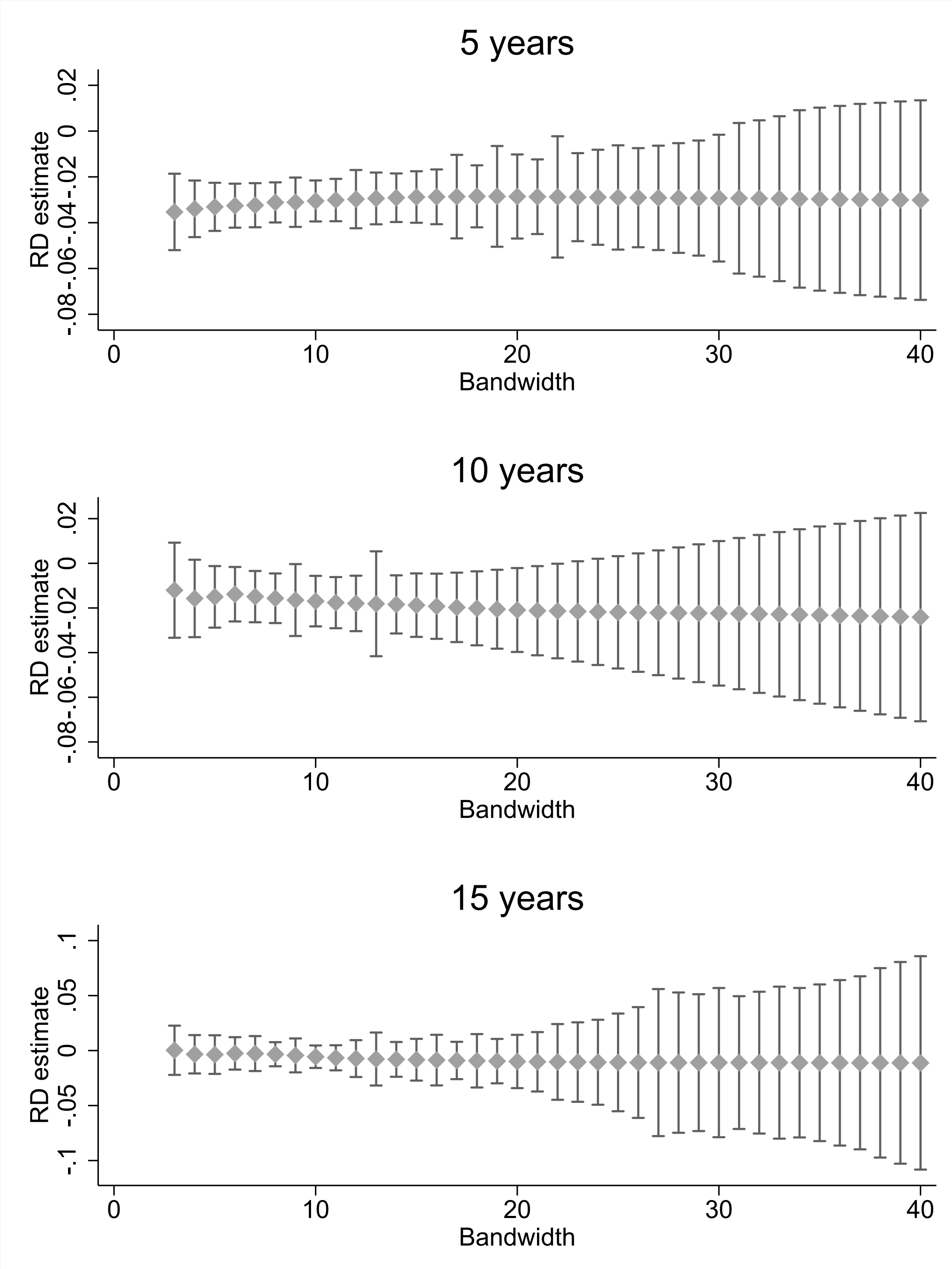


Figure A.14: Bandwidth sensitivity: formal job in past year

Notes: This figure shows the long-term average effects of marginal admission to an elite high school by bandwith and years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

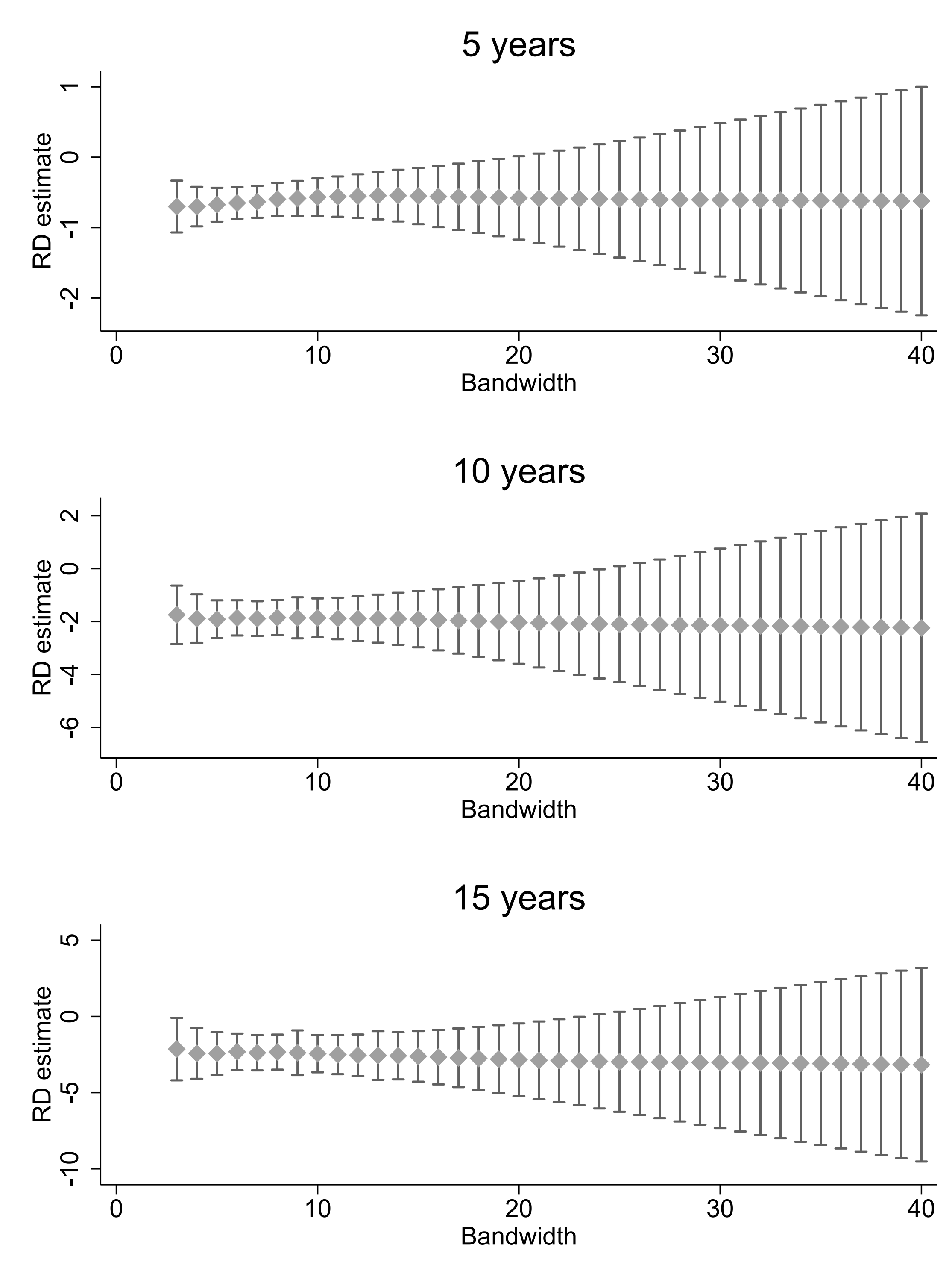


Figure A.15: Bandwidth sensitivity: months with a formal job

Notes: This figure shows the long-term average effects of marginal admission to an elite high school by bandwith and years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

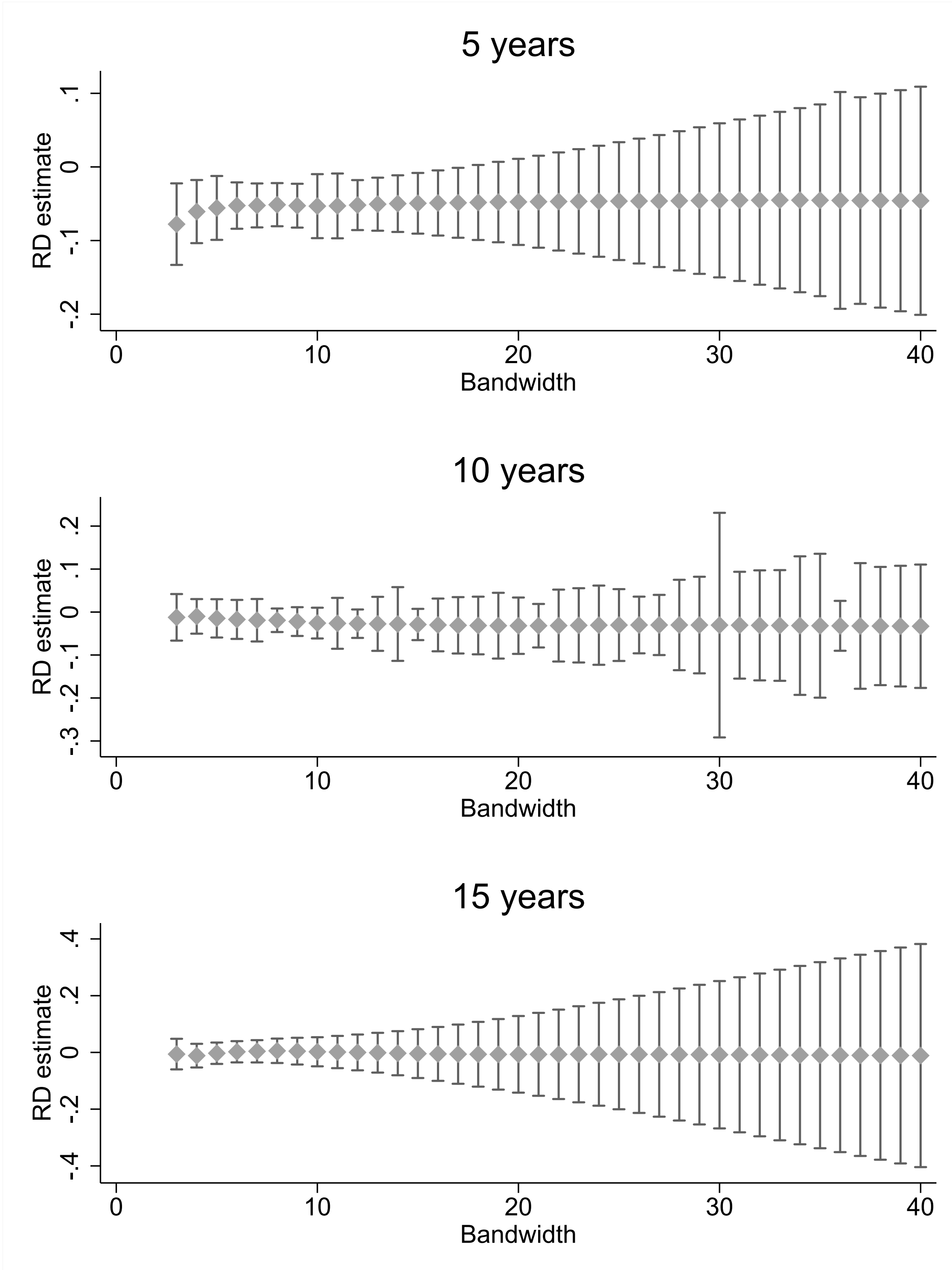


Figure A.16: Bandwidth sensitivity: Log wage in past year, conditional on working

Notes: This figure shows the long-term average effects of marginal admission to an elite high school by bandwith and years after taking the COMIPEMS exam. Each estimate and confidence interval represents a different estimation. Estimates are from local linear regressions. Observations are weighted using the edge kernel. The 95% honest confidence intervals are computed under the bounded second derivative approach.

Table A.1: Balance of exogenous covariates

	(1) Female	(2) Parent HS ed.	(3) High MS GPA
Score $\geq$ cutoff	0.000	-0.003	0.010
	(0.0048)	(0.0048)	(0.0048)**
Honest 95% CI	[-0.049, 0.050]	[-0.027, 0.021]	[-0.015, 0.036]
Mean of DV 1 point below cutoff	0.491	0.565	0.555
Bandwidth	10.0	10.0	10.0
Observations	190389	190389	190389

Notes: This table shows the average effects of marginal admission to an elite high school on students' characteristics. Each column represents a different estimation. Estimates are from local linear regressions with a bandwidth of 10 points of the COMIPEMS test. Observations are weighted using the edge kernel. Standard errors and p-values are computed with heteroskedasticity-robust standard errors. The 95% honest confidence intervals for each subgroup are computed under the bounded second derivative approach. * p<0.10, ** p<0.05, *** p<0.01

Table A.2: Employment characteristics of Students 5, 10 and 15 years after taking the COMIPEMS Exam

	(1) Rejected	(2) Accepted	(3) t-test difference	(4) Rejected within 10 points from cutoff	(5) Accepted within 10 points from cutoff	(6) t-test difference
Panel A. Outcomes 5 years after taking the test						
Av. Wage in Firm	5.217	5.192	6.081	5.231	5.503	5.271
	(0.528)	(0.531)		(0.531)	(0.556)	
Firms' Size	5.982	6.020	-2.415	6.076	6.063	2.906
	(2.015)	(2.028)		(2.013)	(2.087)	
Log wage in industry	8.920	8.930	-3.882	8.935	8.903	2.725
	(0.333)	(0.338)		(0.338)	(0.338)	
Proportion of Women	0.393	0.391	1.809	0.395	0.392	2.966
in Industry	(0.138)	(0.131)		(0.133)	(0.132)	
Av. Years of Education	11.747	11.816	-4.773	11.847	11.644	3.189
in Industry	(1.826)	(1.855)		(1.839)	(1.851)	
# Individuals	174907	21267		16510	11138	
Panel B. Outcomes 10 years after taking the test						
Av. Wage in Firm	5.627	5.763	-37.496	5.894	5.946	-4.090
	(0.669)	(0.731)		(0.685)	(0.714)	
Firms' Size	5.743	5.713	2.948	5.705	5.644	2.841
	(2.040)	(2.004)		(2.115)	(2.098)	
Log wage in industry	9.006	9.065	-36.419	9.029	9.050	-6.474
	(0.324)	(0.317)		(0.321)	(0.318)	
Proportion of Women	0.379	0.377	3.703	0.379	0.377	1.379
in Industry	(0.145)	(0.137)		(0.144)	(0.142)	
Av. Years of Education	12.100	12.392	-30.152	12.222	12.311	-4.145
in Industry	(1.894)	(1.907)		(1.897)	(1.913)	
# Individuals	125519	55697		32479	26692	
Panel C. Outcomes 15 years after taking the test						
Av. Wage in Firm	6.052	6.227	-52.328	6.111	6.164	0.799
	(0.679)	(0.738)		(0.703)	(0.719)	
Firms' Size	5.656	5.681	-2.454	5.689	5.675	1.289
	(2.213)	(2.206)		(2.206)	(2.213)	
Log wage in industry	9.007	9.070	-44.037	9.036	9.056	-8.495
	(0.313)	(0.303)		(0.311)	(0.306)	
Proportion of Women	0.370	0.372	-3.091	0.372	0.373	-0.744
in Industry	(0.151)	(0.145)		(0.147)	(0.147)	
Av. Years of Education	12.043	12.370	-37.744	12.203	12.301	-7.005
in Industry	(1.862)	(1.869)		(1.868)	(1.872)	
# Individuals	140649	69279		37761	32950	

Notes: Column 1 focuses on all rejected students, column 2 on all admitted students, column 4 on all rejected within 10 points of the cutoff, and column 5 on all admitted within 10 points of the cutoff; columns 3 and 6 show the t-statistic for the t-test of the difference across groups. Standard deviations in parentheses.
Sources: IMSS administrative data and COMIPEMS.