NETWORK LISER

Discussion Paper Series

IZA DP No. 18880 August 2026

Shifting Pathways: Displacement, Substitution, and the Equilibrium Effects of Preferential College Admissions

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ISSN: 2365-9793

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Abstract

Admission policies can reshape educational pathways before college entry by changing opportunities and incentives to invest. We study these responses using Brazil's 2012 Federal Quota Law, which expanded reserved seats for underrepresented students in elite public universities. Exploiting variation induced by pre-existing quota policies, we implement matched difference-in-differences and follow students through college completion. We find that targeted students increase high-school completion, exam participation, and college enrollment. In contrast, non-targeted students substitute toward private institutions rather than leave higher education and become more likely to complete college. Together, these responses reveal anticipatory adjustments and system-wide reallocation across higher education institutions.

JEL classification

I23, I24, I28, J24

Keywords

affirmative action, college admissions, human capital investment, educational attainment, displacement and reallocation

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* We thank participants and discussants at the conferences, workshops, and seminars at which this paper was presented. All errors are our own.

1 Introduction

Admission policies shape students' educational pathways long before they enter college. By changing the probability of admission to different institutions, these policies affect a sequence of interrelated decisions, including whether students persist through high school, prepare for college entrance exams, apply to post-secondary institutions, enroll in college, and ultimately complete a degree. These responses depend not only on the expected returns to education (Jensen, 2010), but also on students' perceived admission opportunities (Dynarski et al., 2021; Kapor, 2025) and on the availability of alternative higher education options (Barahona et al., 2023). As a result, the effects of admission policies extend well beyond the institutions directly affected by the reform. They may alter educational investments before college entry, reshape the allocation of students across the higher education system, and generate equilibrium responses that differ across groups facing distinct opportunity sets.

Preferential admissions simultaneously expand opportunities for targeted students while restricting access for non-targeted students, creating incentives that differ across groups and throughout the educational pipeline. Students who become more likely to access selective institutions may increase their educational investments before college entry, whereas students facing lower admission probabilities may respond by reallocating across alternative higher education options instead of withdrawing from post-secondary education altogether (Bodoh-Creed and Hickman, 2018). Yet most of the existing empirical literature focuses on admissions outcomes at specific institutions or among selected applicant populations,[1] making it difficult to assess how preferential admissions reshape educational pathways for the broader population of students and how these behavioral responses interact to produce reallocation across the higher education system.

In this paper, we study how high school students adjust their educational pathways in response to changes in college admission probabilities induced by the large expansion of affirmative action in Brazil. Exploiting the sharp, nationally mandated increase in reserved seats following the 2012 Federal Quota Law, we follow multiple cohorts from the beginning of high school through college graduation. For the population of students, this allows us to examine how changes in admission opportunities affect decisions throughout the educational pipeline, from high school persistence and college aspirations to enrollment and degree completion. By observing enrollment across all higher education institutions, we also study how students reallocate across post-secondary options when admission opportunities change.

[1] See Arcidiacono et al. (2015) for a review of the U.S. literature. For India, see Bagde et al. (2016); Bertrand et al. (2010). For Brazil, see Francis-Tan and Tannuri-Pianto (2024) for a review.

The Brazilian context is well suited for studying anticipatory and system-wide responses to shifts in admissions probabilities. The 2012 affirmative action law defined clear and salient eligibility criteria, requiring universities to reserve seats for students from public high schools, with additional sub-quotas for low-income and Black applicants. Admission to public universities is based primarily on entrance exam performance, in contrast to the holistic admissions processes common in the United States or those that combine entry exams with high school GPA, such as in Chile.[2] Since admissions to college in Brazil generally do not use high school grades, they are less susceptible to potential confounding effects from, for example, teacher manipulation of grades or other grade-related strategic responses.

We combine multiple administrative data sources to follow students' responses along the full secondary-to-tertiary pipeline across the full higher education system. We construct cohort-level measures of (i) high school graduation, (ii) demand for college, measured by take-up of the national college entry exam (the so-called ENEM)[3], (iii) college enrollment, and (iv) college graduation, tracing sequential margins of adjustment. High school graduation represents the first margin, as students may alter effort or persistence in response to updated beliefs about access to elite public universities. Exam participation is the next key decision. Since ENEM scores are required not only for admission to federal institutions but also for other public universities and for access to financial aid and scholarships, taking the exam expands students' effective post-secondary choice sets. Observing enrollment across all institution types allows us to capture not only changes in overall attainment but also how students reallocate across types of colleges under the new admissions policy. Finally, college completion allows us to assess whether these substitutions translate into persistent gains in schooling, with implications for the quality of student-institution matches.

Our identification strategy exploits variation in exposure to the quota law during high school. We compare cohorts that are enrolled in high school before and after the policy announcement, with treated cohorts experiencing a varying degree of exposure to the policy. In addition, universities with pre-existing quotas that did not comply with the law's required allocation were mandated to implement scheduled increases to meet the federal requirement. This policy-induced compliance created exogenous variation in treatment intensity across municipalities. For the analysis, we define treatment based on whether municipalities experienced above- or below-median increases in affirmative action seats. We then implement a matched difference-in-differences design, comparing exposed and unexposed cohorts across

[2]See Bleemer (2023) and Arcidiacono et al. (2022) for evidence on implicit preferential admissions in the U.S. For Chile, see Hastings et al. (2013).

[3]*Exame Nacional do Ensino Médio* (ENEM) is widely used for college admissions and student financial aid.

high- and low-expansion municipalities while balancing schools on pre-policy characteristics and outcome levels.

Our results show that exposure to affirmative action during high school reshapes students' educational trajectories by altering their effective post-secondary choice sets, with effects extending beyond the federal universities directly affected by the policy. Among targeted students, exposure to the reform increases high school completion by 2.7 percent, suggesting that improved access to elite public universities may strengthen incentives to persist through secondary education. The policy also increases participation in the national college entrance exam by 5.1 percent, an important margin because taking the exam is a prerequisite not only for admission to federal universities but also for accessing other public institutions and federal financial aid programs.

Additionally, we find that the pre-college effects translate into higher college enrollment. The increase in overall enrollment is followed by higher enrollment in federal universities and positive, although imprecise, estimates for enrollment in other public and private institutions, indicating that improved admission prospects expand students' opportunities beyond the institutions directly affected by the reform. The expansion also increases enrollment in highly selective degree programs by 2.1 percentage points. We do not find evidence of an increase in on-time college graduation among targeted students. This result, however, should be interpreted cautiously, since federal university students typically take longer to complete their degrees, and some policy-induced gains may therefore fall outside our observed window.

For non-targeted students, the policy generates a substantively different response. High school completion remains unchanged, but participation in the national college entrance exam falls by 2.6 percent, suggesting that some students revise downward their expected returns to competing for federal university seats. As expected, affirmative action reduces enrollment in federal universities among non-targeted students. However, a key finding of our study is that these losses do not translate into lower overall participation in higher education. Students therefore substitute toward alternative options: the 7.3 percent decline in federal enrollment is offset by a 5.8 percent increase in private enrollment. This substitution preserves overall college attainment, but shifts non-targeted students from more selective federal universities to less selective institutions. Most importantly, on-time college graduation increases by 4 percent, driven primarily by higher completion in the private sector. Their response therefore extends beyond the preservation of college enrollment: displaced non-targeted students ultimately become more likely to complete a college degree within the period we observe.

Taken together, these findings show that admission policies reshape educational pathways through both behavioral responses before college entry and reallocation across higher education institutions. The effects extend beyond the institutions directly covered by the policy, as affirmative action changes students' pre-college choices and their allocation across the broader higher education market. Our findings highlight the importance of accounting for outside options when evaluating preferential admissions policies, as reallocation across alternative institutions substantially mitigates the losses experienced by non-targeted students while expanding opportunities for targeted students.

We assess the robustness of these findings through a series of alternative empirical specifications. Our results remain qualitatively unchanged when we estimate a standard difference-in-differences model without matching, implement a synthetic difference-in-differences estimator, or adopt alternative definitions of treatment intensity. Although point estimates and statistical significance vary across specifications, the central qualitative patterns persist: affirmative action expands educational opportunities for targeted students, while non-targeted students respond primarily through substitution across higher education institutions rather than exiting from college education, with this reallocation ultimately leading to higher on-time college graduation.

To complement our reduced-form estimates, we implement a simple earnings-equivalent accounting exercise that translates the estimated changes in educational attainment into a common and economically meaningful metric. This exercise aims to assess the aggregate earnings implications of the estimated effects across targeted and non-targeted students, not the precise welfare effects of the Quota Law, which would require substantially stronger assumptions about labor market equilibrium and the returns to education. Our analysis suggests the expansion is associated with positive aggregate earnings-equivalent amounts for both groups, with the largest contribution for targeted students arising from increased high school completion, while the contribution for non-targeted students is driven primarily by higher graduation rates in the private sector.

Our paper contributes to the literature on preferential college admissions. A large empirical literature has examined affirmative action policies in the United States, including both their introduction and subsequent bans (Antonovics and Backes, 2014; Arcidiacono et al., 2022; Bleemer, 2023), as well as alternative admission rules such as percent-plan policies that guarantee admission to top-ranked students within high schools (Black et al., 2023; Bleemer, 2024). More recently, a growing body of work has studied preferential admissions outside the United States, particularly in India and Brazil (Bagde et al., 2016; Barahona et al., 2023; Bertrand et al., 2010; Estevan et al., 2019; Francis and Tannuri-Pianto, 2012; Mello, 2022; Melo, 2025). Across institutional settings, this literature documents important effects on college access, student sorting, and longer-run educational and labor market outcomes.

A smaller literature studies whether preferential admissions affect educational investments before college entry (Akhtari et al., 2024; Cassan, 2019; Estevan et al., 2019; Francis and Tannuri-Pianto, 2012; Khanna, 2020; Tincani et al., 2025). The evidence is mixed, with studies documenting positive, null, and even negative anticipatory responses across settings. More broadly, however, most existing work focuses either on specific institutions or selected applicant populations, making it difficult to understand how preferential admissions reshape educational trajectories for the broader population of students or how displaced students adjust across the full set of higher education opportunities.

Our paper contributes to the literature on preferential college admissions in three ways. First, we shift the focus from admissions outcomes at individual institutions to educational pathways for the full population of high school students, allowing us to estimate how changes in admission probabilities affect decisions from high school completion through college graduation. Following students through degree completion reveals that the reallocation of non-targeted students is accompanied by higher on-time college completion, an adjustment that cannot be observed from pre-college behavior or enrollment alone. Second, we explicitly account for students' outside options by following enrollment across the entire higher education system, showing how preferential admissions generate both displacement from treated institutions and substitution toward alternative colleges. Third, by estimating effects separately for targeted and non-targeted students, we characterize the system-wide effects of affirmative action for the two directly affected groups rather than implicitly treating one group as the counterfactual for the other.

Finally, closer to ours is Barahona et al. (2023), which studies Brazil's affirmative action within a centralized admissions system and also emphasizes the role of outside options in shaping distributional and efficiency effects. While their analysis focuses on college applicants within the centralized system, we follow multiple cohorts from the first year of high school through college graduation, allowing us to estimate aggregate effects for the full population of high school students and to examine pre-college margins that directly shape students' college choice sets. Together, these contributions provide a broader picture of how preferential admissions reshape pre-college behavior and student allocation across higher education.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting, the structure of secondary and tertiary education in Brazil, and the 2012 affirmative action law. Section 3 details the administrative data sources and the construction of outcomes and treatment measures. Section 4 presents the empirical strategy. Section 5 reports the main findings, and Section 6 discusses robustness exercises. Section 7 presents the earnings-based accounting exercise, and Section 8 concludes.

2 Education in Brazil

2.1 From High School to College

Education in Brazil is a mix of public and private institutions. At the high school level, most institutions are public, accounting for 87 percent of total enrollment (INEP, Censo Escolar 2024). There are no tuition fees to attend a public high school. Public schools are administered at the federal, state, and municipal levels, although state schools account for the vast majority of enrollments (81.1 percent). Private high schools charge tuition, with considerable variation in both costs and quality.

High school education lasts three years, and students are typically between 15 and 17 years old. High school enrollment among 15-17-year-olds has increased steadily in Brazil over the past decade, rising from 73 percent in 2014 to 83 percent in 2024. High school completion among 19-year-olds has also increased over the same period, from 55 percent in 2014 to 71 percent in 2024. However, large socioeconomic gaps remain: high school completion reaches 92 percent among individuals in the top income quintile, compared to 56 percent among those in the bottom quintile.[4] Despite these gains in completion rates, the immediate transition to higher education after high school has remained low and relatively stable over the past decade, averaging around 27 percent.

The ENEM (*Exame Nacional do Ensino Médio*) is a centralized, federally administered high school exam primarily used for college admissions. It is a voluntary national examination, administered annually, and takes place before the college application cycle, with testing locations distributed across the country. The exam is open to anyone, including high school seniors and adults of any age. ENEM functions both as an admissions instrument and as an informational device: students' scores provide a signal of their likelihood of admission to specific institutions and majors. Applicants may use their ENEM scores to apply to public universities through centralized or institution-specific processes and to access federal financial aid programs for private institutions, including scholarships and student loans.

At the tertiary level, private institutions account for the vast majority of enrollments, averaging roughly 80 percent between 2012 and 2015, and both quality and tuition levels vary widely. Admissions to private higher education institutions remain decentralized, as

[4] Source: Todos pela Educação, Anuário da Educação Básica 2025.

each institution defines its own selection process. Many use ENEM scores as an admissions criterion, either exclusively or in combination with other components, while others continue to administer their own institution-specific exams (*vestibulares*). Selectivity varies widely across private institutions and programs, and admitted students may apply for federal financial aid programs to help cover tuition costs. In contrast, public universities are administered at the federal, state, and municipal levels. They are tuition-free, research-intensive, and often serve as flagship institutions in their respective states. Applications to higher education in Brazil are at the college-major level. Admission to public higher education is based primarily on exam performance. Many public institutions, particularly federal institutions, allocate seats through SISU, while others retain institution-specific admissions processes.[5] As a result, admission to public institutions is highly competitive. In its January 2026 edition, the centralized admission system (SISU), responsible for admissions in the majority of public higher education institutions in the country, received 1.8 million applications competing for 274,000 seats (INEP).

Although students may apply to programs anywhere in Brazil, interstate mobility for college remains limited. According to the Census of 2010, approximately 84 percent of college students in Brazil report no inter-municipal migration after the age of 14 (Mello, 2023). Moreover, only 10 percent of enrolled college students in federal institutions attend an institution in a state other than their state of origin (Mello, 2022). The reasons for this low cross-state mobility are not fully understood, but likely contributing factors include the high costs of relocation, the absence of federal student aid covering living expenses, the limited availability of student housing, and cultural or family ties. These mobility constraints are central to our identification strategy.

2.2 Affirmative Action in Higher Education

Following intense public debate during the 1990s, the first universities in Brazil began adopting affirmative action policies in the early 2000s. In 2003, the State University of Rio de Janeiro implemented the first affirmative action policy, reserving half of university seats for students from local public schools and for Black students. In 2004, the University of Brasília reserved 20 percent of its total seats for Black applicants. These pioneering initiatives marked

[5] For decades, the system was decentralized: each university administered its own selection process, and applicants applied directly to one or more institutions, typically choosing a single major at each. Admissions were based on university-specific entrance examinations (*vestibulares*), often combined with scores from the ENEM. Starting in 2010, the federal government introduced a centralized platform, SISU (*Sistema de Seleção Unificada*), through which participating public institutions allocate seats solely or predominantly based on ENEM scores.

the start of a broader expansion of affirmative action policies across Brazilian universities.

In 2012, the federal government enacted a large-scale admissions reform requiring all federal higher education institutions to comply with minimum quota requirements. Federal Law 12.711/2012, commonly known as the *Lei de Cotas*, combines both race-neutral and race-conscious components. The law required federal institutions to allocate at least 50 percent of their total seats to students who completed high school in public institutions, with income and racial sub-components. The primary eligibility criterion requires that students complete all three years of high school in public schools.[6] The policy mandated a gradual increase in the minimum share of reserved seats: 12.5 percent in 2013, 25 percent in 2014, 37.5 percent in 2015, and 50 percent by 2016. Institutions were permitted to exceed these minimum thresholds.

Of the 94 federal higher education institutions operating in Brazil in 2010, a majority had already adopted some form of affirmative action before the federal mandate, although the average share of reserved seats remained around 20-25 percent before the federal law (Figure A.1). Following the law's implementation, adoption became universal, and the intensity of quotas increased sharply in line with the phase-in schedule. By 2015, all 103 federal institutions in operation had implemented affirmative action policies, and the average share of reserved seats had risen to approximately 50 percent, consistent with the legal requirement. In contrast, state institutions, which were not subject to the federal mandate, exhibited much smaller changes over the same period. While many state universities had pre-existing affirmative action policies, their average quota share increased only modestly, from roughly 29 percent in 2010 to 33 percent in 2015.

3 Data and Analysis Sample

3.1 Administrative Data and Record Linkage

We use three main datasets to link student records and construct outcomes at multiple educational milestones, from year t, their first year of high school, through $t + 8$, the end of our on-time college-completion window.

Census of Basic Education (CBE). First, we use the Census of Basic Education

[6]Within the public high school group, 50 percent of reserved seats are allocated to students from households with per capita income below 1.5 times the minimum wage, while the remaining 50 percent have no income restriction. Within each income category, a minimum share of seats is reserved for applicants who self-identify as Black (*pretos* or *pardos*) or Indigenous. This share varies by state and corresponds to the proportion of these groups in the state population according to the most recent census.

(CBE) from 2007 to 2014. The CBE is an administrative database containing individual-level information on the universe of students enrolled in basic education in Brazil. It is collected annually by the National Institute of Educational Studies and Research (INEP) of the Brazilian Ministry of Education. The individual-level module includes basic demographic characteristics (e.g., gender, age, and ethnicity), as well as unique student and school identifiers. We focus on students who start high school in year t between 2008 and 2012 in regular institutions.[7] As illustrated in Figure A.2, we follow these individuals throughout their high school trajectories. This longitudinal structure allows us to identify whether students graduate from high school on a regular trajectory in year $t+2$, as well as whether they drop out or repeat a grade at any point during high school. Our individual-level sample of first-year high school students comprises 13,000,610 observations: 2,514,501 from the 2008 cohort, 2,539,104 from 2009, 2,629,220 from 2010, 2,654,250 from 2011, and 2,663,535 from 2012.

National High School Exam (ENEM). Second, we use ENEM microdata from 2010 to 2014. ENEM is the national university entrance examination, and the microdata contain individual-level test scores for each exam component, as well as responses to a detailed questionnaire covering socioeconomic background, demographic characteristics, and student perceptions. Students who start high school in year t are linked to the ENEM microdata from year $t+2$, when they are expected to take the exam if they follow a regular high school trajectory. This linkage allows us to observe whether students from our main sample take the ENEM exam, an observable step toward pursuing higher education. The ENEM is a prerequisite for applying to most public higher education institutions in Brazil.

Census of Higher Education (CSUP). Third, we use the Census of Higher Education (CSUP) from 2011 to 2020. The CSUP provides information on the universe of undergraduate students enrolled in Brazilian higher education institutions. The student-level module identifies the program in which each student is enrolled (defined by institution and major), along with basic demographic characteristics. Additional modules contain information on institutions and programs, including the number of available slots, degrees offered, infrastructure, resources, and faculty characteristics. As shown in Figure A.2, we follow individuals who start high school in year t through their transition into higher education. We observe whether students enroll in college on the regular trajectory at $t+3$ and whether those students complete a degree by $t+8$.

[7] We exclude special high schools (e.g., schools for students with special needs or adult education programs) and high schools with non-standard grades or instructional hours. We retain only students who enter high school for the first time in year t. For example, a student who starts high school in 2008, repeats the year, and re-enters in 2009 is included only in the 2008 cohort. The microdata are first available in 2007. We use the 2007 records as a one-year lookback to identify repeaters in the 2008 entry cohort. The 2008 cohort is therefore the earliest cohort that can be constructed consistently under this rule.

Sample selection due to incomplete data linkage. Importantly, we observe CPF-linked outcomes only for individuals whose taxpayer identification number (CPF) is recorded in our data (approximately 81 percent of our sample of first-year high school students). For outcomes that require CPF linkage, the denominator is the total number of students in the school cohort with an observed CPF. This linkage requirement creates a concern that sample selection could be driving our results. We assess this concern using high school graduation, which is observed for every student and therefore allows us to estimate the same specification in both the full sample and the CPF-restricted sample. Section 6.3 discusses and compares the high school graduation estimates, showing the estimated effects are robust across the full and the CPF-restricted samples.

3.2 Outcome Construction

School-cohort aggregation and outcomes. All individual-level data construction and linkages described above are conducted within the Brazilian Ministry of Education's secure data rooms. Ministry data-sharing requirements permit only aggregated results to be exported from this environment. We therefore construct the share of students who graduate from high school at *t+2*, take the ENEM at *t+2*, enroll in higher education at *t+3*, and enroll at *t+3* and complete college by *t+8*, overall and by institution type. High-school graduation is calculated over all students in the school cohort, while the CPF-linked outcomes are calculated over students in the cohort with an observed CPF.

Elite degrees. We also construct a school-cohort measure of enrollment in elite degree programs. To identify eligible programs, we first restrict attention to higher education programs that existed in 2010 (before the AA law), had at least 10 incoming students, and for which at least 20 percent of the 2010 freshman cohort took the ENEM exam. Within this set of programs, we classify as elite those in the top decile of the distribution of ENEM test scores among incoming students in 2010. We then compute, at the school-cohort level, the share of students who enroll in an elite degree.

3.3 Affirmative Action Data

To measure local affirmative action expansion, we use a dataset on the adoption of affirmative action policies across all higher education institutions in Brazil between 2010 and 2015. Originally compiled by Mello (2022), this dataset reports the share of seats reserved by affirmative action type for each public higher education institution in the country. We merge these data with the Higher Education Census (CSUP), which provides information on the total number of seats offered by each institution in each municipality. Using the merged dataset, we construct the variable $AA_{m,t}$, defined as the share of seats reserved for public school students among all seats offered by *federal* higher education institutions in municipality m in year t.

3.4 Analysis Sample and Descriptive Statistics

The final analytical sample is a balanced panel of schools linked to municipality-level affirmative action data. We keep only schools located in the 329 municipalities where there is also a federal higher education institution, resulting in an initial sample of 8,020 schools (5,235 public and 2,785 private). For data protection reasons, we restrict the sample to schools with at least 10 students. To ensure a balanced panel, we exclude schools with missing information on school characteristics[8] or outcomes for any year between 2008 and 2012. The final analytical sample consists of 5,830 schools (3,860 public and 1,970 private) located in 328 municipalities. For the analysis of elite college enrollment, the sample comprises 4,280 schools (2,443 public and 1,837 private).

The distinction between targeted and non-targeted schools follows student eligibility under the public-school quota criterion. Students attending public high schools are eligible for reserved seats and form the targeted group. Students attending private high schools are ineligible under this criterion and form the non-targeted group.

Table 1 documents substantial pre-policy differences between targeted and non-targeted schools. Targeted schools are larger and enroll students who are slightly older and more likely to identify as *preto*, *pardo*, or *indígena* (PPI). These schools have lower average ENEM scores and lower rates of on-time high-school graduation, ENEM take-up, college enrollment, and enrollment at $t + 3$ followed by degree completion by $t + 8$. These differences characterize the structural disadvantage that the quota policy aimed to address.

[8] School characteristics include the average score on the National High School Exam (ENEM) and the proportion of students self-identifying as *preto*, *pardo*, or *indígena* (PPI).

Table 1: Descriptive Statistics: Targeted and Non-targeted Schools

Variable	Targeted	Non-targeted	Difference
School characteristics			
Total enrollment	181.2	79.9	+101.3***
	(123.8)	(71.5)	
Average student age	15.21	14.75	+0.46***
	(0.36)	(0.20)	
Share *preto*, *pardo* or *indígena*	0.561	0.304	+0.257***
	(0.270)	(0.270)	
Average ENEM score (standardized)	-0.175	0.905	-1.080***
	(0.430)	(0.504)	
Share of students with CPF	0.817	0.793	+0.023***
	(0.078)	(0.100)	
Pre-policy outcomes			
HS graduation ($t+2$)	0.566	0.799	-0.233***
	(0.141)	(0.151)	
ENEM take-up ($t+2$)	0.427	0.757	-0.330***
	(0.163)	(0.146)	
College enrollment ($t+3$)	0.147	0.523	-0.376***
	(0.112)	(0.142)	
College graduation ($t+8$)	0.060	0.236	-0.177***
	(0.056)	(0.104)	
Sample sizes			
Schools	3,860	1,970	
Municipalities	327	248	
Students	699,511	157,399	
Students with an observed CPF	569,992	124,469	

Notes: The table reports school-level descriptive statistics for the final analytical sample in the 2009 pre-policy cohort. "Targeted" refers to public schools, whose students are eligible for the seats reserved under the public high school quota criterion; "Non-targeted" refers to private schools, whose students are ineligible under that criterion. Cells report means with standard deviations in parentheses. The difference column reports the targeted-minus-non-targeted difference in means, with stars from a two-sample t-test. School characteristics and outcomes are measured for the cohort entering high school in 2009: high school graduation and ENEM take-up in $t+2$, college enrollment in $t+3$, and college graduation by $t+8$. The ENEM score is standardized across schools. Student counts are cohort enrollments. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4 Empirical Strategy

Our empirical strategy aims to estimate how a larger quota expansion, relative to a smaller one, changed the educational pathways of targeted and non-targeted students. The analysis follows both groups from high-school entry through college completion and estimates their responses separately at key educational milestones, differentiating gains among students eligible for reserved seats from responses among students facing greater competition for non-reserved seats. In our primary empirical framework, we combine cohort timing with geographic differences in the magnitude of the quota expansion in a matched difference-in-differences framework.

4.1 Cohort Design and Timing of Policy Exposure

We combine the timing of the 2012 Law with a cohort strategy to evaluate the effects of the quota expansion on the educational pathways of high school students. As summarized in Figure 1, we construct cohorts of high school students who were in their first year of high school between 2008 and 2012[9], whose expected on-time progression is to enter higher education between 2011 and 2015.

Figure 1: Cohort Design and Exposure During High School

High-school entry cohort	**On-track high-school progression** First year	 Second year	 Third year	**Total exposure**
2008	2008	2009	2010	**0 years**
2009	2009	2010	2011	**0 years**
2010	2010	2011	2012*	**1 year**
2011	2011	2012*	2013	**2 years**
2012	2012*	2013	2014	**3 years**

* Policy announcement   High-school exposure

Note: The figure classifies cohorts by their exposure to the policy announcement during on-track high-school progression. The law was announced in mid-2012. The 2008 and 2009 cohorts had completed high school before the announcement. The 2010, 2011, and 2012 cohorts learned about the law during their third, second, and first years of high school, respectively.

[9] We start from the 2008 cohort due to microdata availability, as discussed in Section 3. We stop with the 2012 high-school entry cohort to avoid including cohorts contaminated by the law-induced migration from private to public high schools (Mello, 2022).

The design contrasts cohorts that finished high school before the law was announced with cohorts that learned about it progressively earlier in their on-track progression.[10] The 2008 and 2009 cohorts completed their expected high-school progression before the policy announcement and form the pre-policy group. The 2010, 2011, and 2012 cohorts learned about the law during their third, second, and first high-school years, respectively, and form the post-policy group. They therefore experienced varied exposure to information about the policy, plus increasing minimum quota shares. Thus, the progressive exposure of the post-policy cohorts provides an informative test of effect heterogeneity. Although we cannot observe specific margins of adjustment during high school (e.g., effort, grades, and attendance), cohort-specific estimates can provide suggestive evidence on whether effects of the expansion grow with the opportunity to adjust during high school.

We index cohorts by year t, their first year of high school, and measure outcomes at common points along the expected educational trajectory: high-school graduation and ENEM participation at $t+2$, college enrollment at $t+3$, and college completion by $t+8$. Importantly, we define outcomes relative to the on-track trajectory to limit contamination from later policy exposure among the comparison cohorts since students in the 2008 and 2009 cohorts could learn about the policy after their expected transition out of high school and could apply to college in later years. Since the college-enrollment outcome is measured at $t+3$, enrollment by the comparison cohorts after the on-track transition does not affect the estimates. College completion at $t+8$ measures on-time completion and also limits the scope for delayed enrollment and completion to affect the analysis.

4.2 Construction of Treatment Variable

Our empirical strategy exploits law-induced differences across municipalities in the magnitude of the quota expansion. As detailed in Section 2, while some universities had adopted AA via state or internal decrees as early as 2002, the sharp increase in quotas among federal institutions was primarily driven by the 2012 mandate. In comparison, the quota share at state universities, which were not affected by the law, remained stable. Figure A.1 documents the resulting increase in affirmative action seats at federal institutions during the implementation period.

We measure the local quota expansion using the increase in the share of federal seats reserved for affirmative action between 2012 and 2015. Universities that had already imple-

[10] Throughout, a cohort is *on-track* if it advances one grade per year, so that students entering high school in year t complete it in $t+2$ and would enter higher education in $t+3$. Outcomes are measured at these on-track dates regardless of a student's realized progression.

mented extensive affirmative action policies experienced smaller changes under the minimum reservation floor than universities starting from zero. We capture this treatment intensity at the municipality level using the variable $Jump_m$, defined as:

$$Jump_m = AA_{m,2015} - AA_{m,2012}, \tag{1}$$

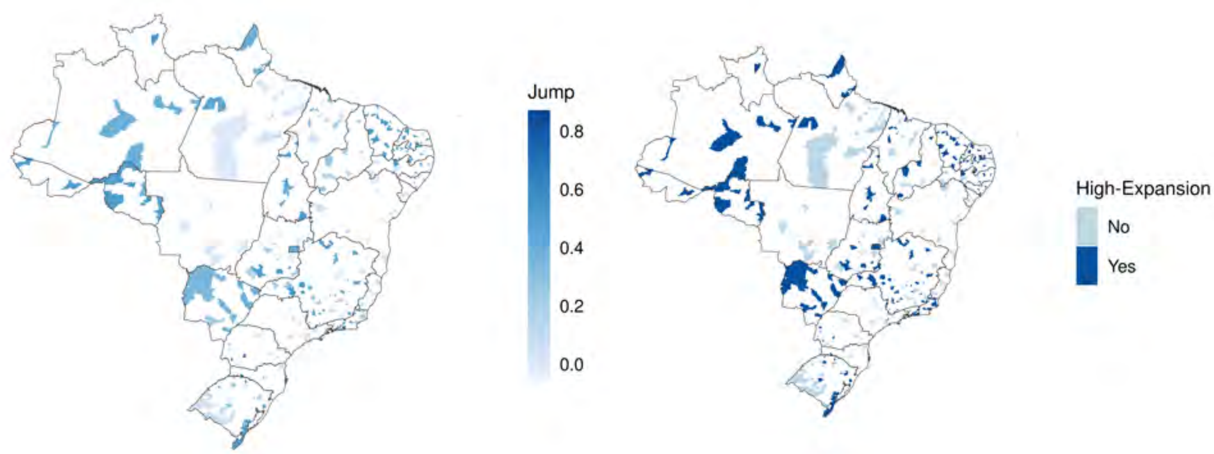
where $AA_{m,2015}$ and $AA_{m,2012}$ represent the shares of federal college seats offered in municipality m that were reserved for affirmative action in 2015 and 2012, respectively.

For the primary empirical strategy, we convert continuous $Jump_m$ into a binary measure of expansion magnitude that defines high- and low-expansion groups. We dichotomize $Jump_m$ at the sample median. Municipalities with $Jump_m \geq 0.20$ form the high-expansion group, while municipalities with $Jump_m < 0.20$ form the low-expansion comparison group. Both groups were exposed to the national reform, so the binary contrast compares the effects of a larger relative to a smaller quota expansion.

Figure 2 displays (a) the spatial distribution of continuous $Jump_m$ and (b) the corresponding median-based classification. While dichotomization improves interpretation, it also discards some of the underlying expansion variation and makes the comparison depend on a particular cutoff. Section 6 addresses this limitation by examining alternative thresholds and estimating a continuous-$Jump_m$ specification using the full variation in $Jump_m$.

Figure 2: Spatial Distribution of Policy Expansion

(a) Continuous Policy Expansion (b) Binary Expansion Classification



Notes: Panel (a) displays the spatial distribution of the *Jump* variable. Panel (b) presents the binary expansion classification. Municipalities with $Jump_m \geq 0.20$, the sample median, form the high-expansion group, while municipalities with $Jump_m < 0.20$ form the low-expansion group.

4.3 Estimation

Our empirical design uses difference-in-differences to compare cohort changes in high- and low-expansion municipalities. We estimate the comparison separately for targeted and non-targeted schools. Identification relies on the assumption that outcomes in high- and low-expansion municipalities would have followed parallel cohort trends absent differential quota expansion.

While we show that the effects estimated using the standard difference-in-differences approach are robust in Section 6.1, we adopt a matched difference-in-differences strategy as our benchmark specification as it improves precision. We perform matching separately for public and private schools using two sets of covariates. Following Abadie and Spiess (2022), we match without replacement using the Mahalanobis distance metric. First, we match schools based on four characteristics measured in 2009: total enrollment, average student age, percentage of non-white students, and overall average scores of the ENEM. Second, we incorporate our four primary outcome variables from 2008 and 2009 into the matching algorithm: high school graduation rates, ENEM take-up rates, and overall college enrollment and college graduation rates. Tables A.1 and A.2 show that matching improves comparability along selected dimensions, with the clearest gains among targeted schools, but does not eliminate all baseline differences.

Using the matched sample, we estimate the following equations:

$$\Big(Y_{j,t} - Y_{j,2009}\Big) - \Big(Y_{k,t} - Y_{k,2009}\Big) = \alpha + \beta \times Post_t + \varepsilon_{pair,t}, \tag{2}$$

and

$$\Big(Y_{j,t} - Y_{j,2009}\Big) - \Big(Y_{k,t} - Y_{k,2009}\Big) = \alpha + \sum_{\tau \neq 0} \beta_\tau \times \mathbf{1}\{t - 2009 = \tau\} + \varepsilon_{pair,t}. \tag{3}$$

Equation (2) represents the standard difference-in-differences specification, where j indexes a school in a high-expansion municipality and k its matched school in a low-expansion municipality, $Y_{i,t}$ denotes the outcome for school i and high-school entry cohort t, $Post_t$ is an indicator equal to one for post-policy cohorts (2010, 2011, and 2012), and $\varepsilon_{pair,t}$ is an idiosyncratic error term. The intercept α is the average double difference among the pre-policy cohorts. Since the double difference is identically zero at $t = 2009$ by construction, α equals half the 2008 differential in the balanced panel, summarizing the available pre-policy change. The coefficient β compares the average differential change in the post-policy cohorts (2010–2012) with the average differential change in the two pre-policy cohorts (2008–2009), capturing the change associated with a larger quota expansion in the matched sample. Standard errors are clustered at the high-expansion municipality level, i.e., on the municipality of school j.

In Equation (3), $\mathbf{1}\{t - 2009 = \tau\}$ is an indicator equal to one when the cohort year differs from 2009 by τ years. Here the omitted category is 2009 itself, so the intercept is the average double difference in that cohort and is identically zero; the 2008 coefficient β_{-1} carries the pre-trend on its own. The main coefficients of interest are β_τ for $\tau \geq 1$, which trace the dynamic post-treatment effects. Importantly, the post-policy coefficients also show whether the effects vary with the cohorts' progressively longer exposure during high school.

Both equations are estimated using weights so that each matched pair enters in proportion to the pre-policy size of its high-expansion school. For high-school graduation, the weight is total cohort enrollment in 2009. For ENEM take-up, college enrollment, and college graduation, the weight is the number of students with an observed CPF in 2009. The weight is fixed across cohorts, and the size of the matched comparison school does not enter it.

4.4 Identification

The validity of our matched difference-in-differences strategy requires that, conditional on the matched characteristics, outcomes in municipalities experiencing larger and smaller quota expansions would have followed parallel cohort trends absent the differential expansion. Several features of the setting support the plausibility of this assumption. First, the timing of the reform was determined by a top-down federal mandate. The 2012 law required all federal universities to adopt quotas and to meet common implementation targets by 2016. The timing of implementation was therefore unlikely to have been driven by contemporaneous shocks to the outcomes of students attending local high schools.

Cross-municipality variation in quota expansion arose because federal institutions entered the reform with different pre-policy quota shares. Institutions with more extensive affirmative action policies before 2012 required smaller adjustments to comply with the federal mandate, whereas institutions with limited or no pre-existing quotas were required to expand more sharply. A potential concern is that realized quota expansion may nevertheless have responded to contemporaneous changes in local demand for affirmative action. Evidence from Mello (2022), however, indicates that institutions' post-2012 quota expansions were not systematically related to changes in the demographic composition of their student bodies. This pattern is consistent with compliance with the federal mandate instead of in response to changing local demand. It also makes it less likely that quota expansion was driven by contemporaneous changes in educational outcomes at high schools located in the same municipalities.

These institutional features do not, however, imply that the magnitude of the local quota expansion was randomly assigned. Pre-policy quota adoption may be correlated with persistent institutional or municipal characteristics, and identification still requires assumptions about how outcomes would have evolved across municipalities experiencing different expansions. In particular, our design requires parallel cohort trends in the absence of the differential quota expansion. This counterfactual assumption is not directly testable.

However, within the constraints imposed by data availability, the event-study evidence supports the parallel-trends assumption. The only feasible pre-policy comparison contrasts the 2008 and 2009 high-school-entry cohorts, and the estimated differences are consistently small and statistically indistinguishable from zero across the main outcomes (Figures 3, 4, and 5). The absence of differential pre-policy changes across multiple educational margins, together with the federally determined timing of the reform, the law-driven expansion of quotas, the matched research design, and the robustness of the results to alternative counterfactual constructions, provides a coherent body of evidence supporting a causal interpretation of the estimated parameters.

5 Results

In this section, we report the results for the preferred matching difference-in-differences estimates of the effects of the expansion of affirmative action on students' educational trajectories, by targeted and non-targeted schools. The results show that a larger quota expansion reshaped educational pathways for both groups. Targeted students increased high-school completion, ENEM participation, and college enrollment, while non-targeted students reallocated across institution types and ultimately became more likely to complete college by $t + 8$.

5.1 Educational Attainment and College Participation

Table 2 presents the estimates for the primary outcomes: high school graduation, college-exam participation, and overall college enrollment and graduation. In Panel A, we report the effects on pre-college outcomes. For targeted students, a larger quota expansion led to statistically significant increases in high school graduation (1.5 p.p. or 2.7 percent relative to the pre-policy baseline) and exam take-up (2.2 p.p. or 5.1 percent). For non-targeted students, we find no corresponding effect on on-time high-school graduation, while exam take-up declines significantly (-2.0 p.p. or -2.6 percent). Both groups' responses are economically meaningful: a high school diploma is a prerequisite for college entry, and participation in the college-entry exam is required not only for admission to federal universities but also for access to other public institutions and financial aid programs. Together, graduation and exam participation shape students' effective post-secondary choice sets.

Table 2: Effects of a Larger Quota Expansion Along the Educational Pathway

	Panel A: High School			
	HS Graduation		**ENEM Take-up**	
	Targeted	Non-targeted	Targeted	Non-targeted
Estimated Effect	0.015*	0.005	0.022**	-0.020***
	(0.008)	(0.007)	(0.009)	(0.005)
Pre-policy mean (2009)	0.550	0.799	0.430	0.770
Observations	8,485	4,610	8,485	4,610
	Panel B: College			
	College Enrollment		**College Graduation**	
	Targeted	Non-targeted	Targeted	Non-targeted
Estimated Effect	0.006**	0.008	0.002	0.010**
	(0.003)	(0.007)	(0.002)	(0.004)
Pre-policy mean (2009)	0.153	0.548	0.061	0.251
Observations	8,485	4,610	8,485	4,610

Notes: The table reports matched difference-in-differences estimates of the effects of a larger quota expansion, estimated using Equation (2). Schools are matched without replacement using Mahalanobis distance. "Targeted" refers to public schools (eligible under the public high school quota criteria), while "Non-targeted" refers to private schools (ineligible under the law). Outcome variables are defined relative to year t, the student's first year of high school. *HS Graduation* refers to on-time high school completion in $t + 2$. *ENEM Take-up* denotes enrollment in the ENEM exam in $t + 2$. *College Enrollment* indicates enrollment in any higher education institution in $t + 3$ (one year after expected high school graduation). *College Graduation* refers to college graduation by $t + 8$. The HS Graduation regression is weighted by the 2009 number of students of the high-expansion school, while the other three regressions are weighted by the 2009 number of students with a CPF in each matched pair. Standard errors are clustered at the high-expansion municipality level and reported in parentheses. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In Panel B, we examine downstream effects on college enrollment and completion. For targeted students, expanded post-secondary options translate into higher college enrollment (0.6 p.p. or 3.9 percent relative to baseline). We do not find evidence that this increase in enrollment translates into higher graduation rates within our observation window. Importantly, college graduation is measured eight years after high school entry, corresponding to "on-time" completion. This window ends approximately six years after expected high-school completion and does not capture eventual degree completion. Therefore, the absence of significant effects on graduation for targeted students should be interpreted with caution, as it may reflect adjustment along the intensive margin, such as delayed progression or extended time to degree, not reduced persistence (Oliveira et al., 2024). Using the Higher Education

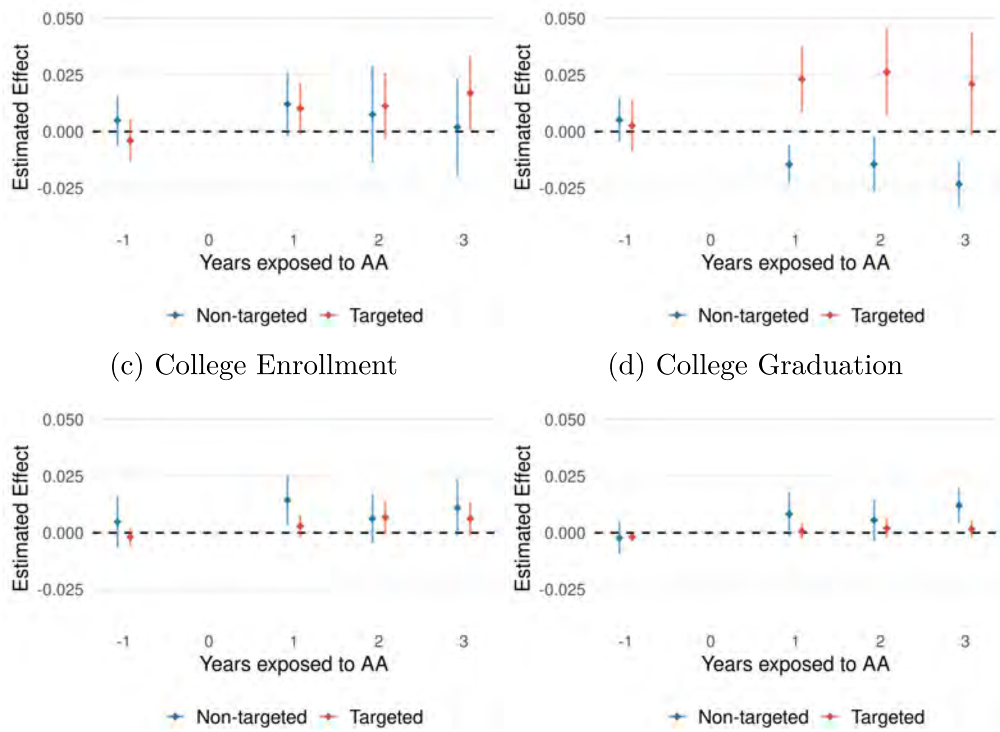
Census microdata (2010–2017), in-person bachelor's graduates at federal universities spend on average 5.7 academic years completing their degree, compared to 5.3 years at private institutions. Approximately 23 percent of federal graduates take more than six academic years to complete their degree, and would therefore not be captured as graduates within our eight-year observation window, versus 12 percent at private institutions.

For non-targeted students, we do not find negative effects on overall college enrollment despite the decline in exam take-up. Instead, we observe a positive effect on college graduation (1.0 p.p., or 4.0 percent relative to baseline). These patterns point to substitution, not exit: although non-targeted students mechanically lose seats at federal universities, they appear to reallocate toward alternative public or private institutions within the higher education system. These findings suggest that the availability of outside options mitigates the mechanical losses in access for the non-targeted induced by quota-type policies. Most importantly, this reallocation does not come at the expense of on-time college completion within the period we observe. Non-targeted students ultimately become more likely to complete a college degree by $t + 8$.

Figure 3 presents the dynamic event-study estimates. The pre-policy estimates are small and statistically indistinguishable from zero across the main outcomes, providing a pre-trends diagnostic. Post-policy, the cohort profiles do not provide clear evidence that the effects increase systematically with the length of high school exposure. This pattern may indicate that students respond relatively quickly to the change in admission incentives or that the difference between one and three years of exposure is insufficient to capture slower human-capital adjustments.

Figure 3: Effects Along the Educational Pathway by Years of High School Exposure

(a) High School Graduation (b) ENEM Take-up (c) College Enrollment (d) College Graduation



Notes: The figure reports matched difference-in-differences estimates of the effects of a larger quota expansion, estimated using Equation (3). “Targeted” refers to public high schools and “Non-targeted” to private high schools; outcomes are school-cohort shares. The pre-policy coefficient compares the 2008 and 2009 cohorts and serves as a pre-trends diagnostic. Outcome variables are measured relative to year t, the student’s first year of high school. High-school graduation and ENEM take-up are measured in $t+2$, college enrollment in $t+3$, and college graduation by $t+8$. Vertical bars denote 95 percent confidence intervals based on standard errors clustered at the high-expansion municipality level.

5.2 Reallocation Across Institutions and College Completion

The primary outcomes indicate that the expected displacement of non-targeted students from federal institutions did not reduce their overall college participation. Targeted students became more likely to enroll in college, while non-targeted students maintained overall enrollment and became more likely to graduate on time. These findings suggest that the reallocation of federal seats was accompanied by adjustments elsewhere in the higher education system. We therefore examine reallocation across institution types and along the institutional selectivity distribution.

Table 3 traces these adjustments from college enrollment through completion. Panel A reports enrollment by institution type. For targeted students, federal enrollment increases by

0.2 percentage points, while the estimates for other-public and private enrollment are positive but imprecise. The federal estimate indicates that part of the overall enrollment response occurs within the institutions directly affected by the quota law.

Table 3: College Reallocation and Completion

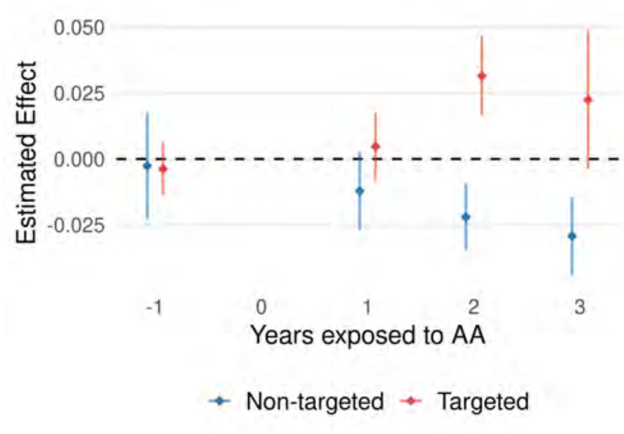
	Targeted		Non-targeted	
	Effect	Pre-policy mean	Effect	Pre-policy mean
Panel A: College enrollment by institution type				
Federal	0.002*	0.033	-0.013***	0.179
	(0.001)		(0.004)	
Other public	0.001	0.011	0.004	0.051
	(0.001)		(0.003)	
Private	0.003	0.112	0.020***	0.348
	(0.003)		(0.007)	
Panel B: College enrollment by institutional selectivity				
Elite	0.021***	0.067	-0.020***	0.271
	(0.008)		(0.006)	
Panel C: College graduation by institution type				
Federal	-0.000	0.012	-0.006*	0.071
	(0.001)		(0.003)	
Other public	0.000	0.005	0.001	0.023
	(0.000)		(0.001)	
Private	0.002*	0.044	0.014***	0.158
	(0.001)		(0.003)	

Notes: The table reports matched difference-in-differences estimates of the effects of a larger quota expansion. "Targeted" refers to public high schools and "Non-targeted" to private high schools; outcomes are school-cohort shares. Enrollment is measured in $t + 3$ and college graduation by $t + 8$. Panel A reports enrollment by institution type, while overall college enrollment is reported in Table 2. Panel B summarizes reallocation along the institutional selectivity distribution using enrollment in a major-institution combination in the top decile of the ENEM score distribution. Elite enrollment overlaps the institution-type categories and should not be added to the rows in Panel A. Panel C reports graduation by institution type. The elite-college sample contains 2,294 observations for targeted schools and 2,568 for non-targeted schools. All other rows contain 8,485 and 4,610 observations, respectively. Regressions are weighted by the 2009 number of students with a CPF of the high-expansion school in each matched pair. Standard errors clustered at the high-expansion municipality level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In contrast, the results for non-targeted students show evidence that their displacement from federal institutions was followed by substitution toward alternative outside options. As expected, their enrollment in public universities declines significantly (-1.0 p.p., or -4.4 percent relative to baseline), driven entirely by federal institutions (-1.3 p.p., or -7.3 percent). This displacement is accompanied by an offsetting adjustment outside the federal sector. The decline in enrollment in federal institutions among non-targeted students is accompanied by a larger, statistically significant increase in enrollment at private universities (+2.0 p.p., or 5.8 percent relative to baseline). Rather than exiting higher education, non-targeted students reallocate across sectors, substituting away from federal institutions toward alternative tertiary options. These findings show that the policy reshaped sorting across higher education institutions, redistributing seats at federal universities toward a historically disadvantaged group, while preserving overall college participation through substitution across available and accessible outside options.

Panel B of Table 3 and Figure 4 examine whether this reallocation also extends along the college selectivity dimension. Targeted students become 2.1 percentage points more likely to enroll in elite colleges, defined as major-institution combinations in the top decile of the ENEM score distribution, while non-targeted students become 2 percentage points less likely to do so. Selectivity provides an informative, though imperfect, proxy for the quality of the institutions students attend. These results therefore show that the quota expansion changed not only the types of institutions students attended but also their access to the most selective segment of higher education.

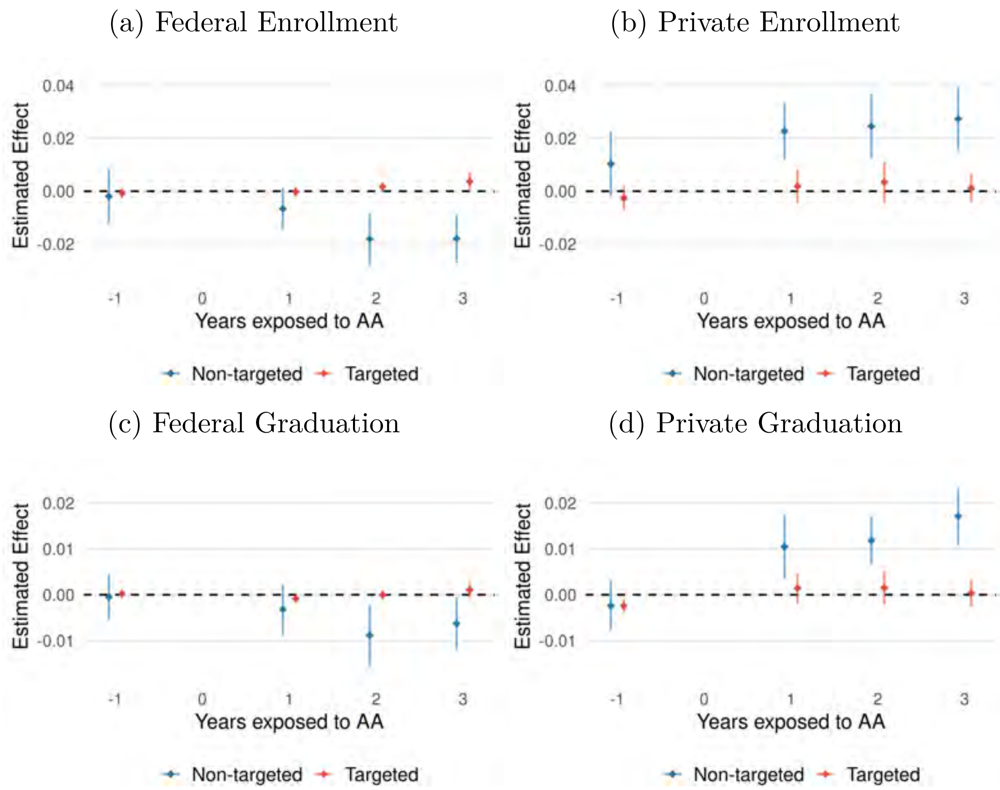
Figure 4: Effects of a Larger Quota Expansion on Elite College Enrollment



Notes: The figure reports matched difference-in-differences event-study estimates of the effects of a larger quota expansion on enrollment in top-decile ENEM colleges, estimated using Equation (3). The text in the upper-left corner displays the aggregated matched difference-in-differences estimate from Equation (2). "Targeted" refers to public schools; "Non-targeted" refers to private schools. The outcome is a school-cohort share. The pre-policy coefficient compares the 2008 and 2009 cohorts and serves as a pre-trends diagnostic. Vertical bars denote 95 percent confidence intervals based on standard errors clustered at the high-expansion municipality level.

Finally, Panel C of Table 3 shows how the enrollment reallocation carries through to on-time college completion. For targeted students, we detect a marginally significant increase in private graduation (+0.2 p.p., or 4.5 percent relative to baseline), with no significant effects emerging for public graduation. These estimates capture completion by $t+8$ and do not rule out differences in eventual graduation beyond the on-time window. For non-targeted students, the decline in graduation from federal institutions (-0.6 p.p., or -8.6 percent relative to baseline) is accompanied by a larger increase in graduation from private institutions (+1.4 p.p., or 8.9 percent).

Figure 5: Reallocation Across Institutions and College Completion



Notes: The figure reports matched difference-in-differences estimates of the effects of a larger quota expansion by years of high-school exposure. "Targeted" refers to public high schools and "Non-targeted" to private high schools; outcomes are school-cohort shares. Enrollment outcomes are measured in $t+3$ and graduation outcomes by $t+8$. The pre-policy coefficient compares the 2008 and 2009 cohorts and serves as a pre-trends diagnostic. Vertical bars denote 95 percent confidence intervals based on standard errors clustered at the high-expansion municipality level.

Most importantly, this reallocation is accompanied by an increase in overall on-time college completion within the period we observe. The higher completion rate may reflect faster progression or differences in institutional pathways following reallocation, although our design does not separately identify these mechanisms. Figure 5 provides the corresponding cohort profiles and shows that the enrollment and graduation responses extend across the post-policy cohorts. Appendix Figures A.4 and A.5 report the corresponding cohort profiles for enrollment and graduation in other-public institutions, and the net effect across all public institutions.

Differences in capacity across institution types provide a plausible explanation for why displacement from federal institutions does not translate into lower overall college enrollment. The natural zero-sum concern that gains for targeted students must come entirely at the expense of non-targeted students applies within a fixed federal sector, but not across the higher education system as a whole. Federal and private institutions operate under very different capacity constraints. Federal universities are nearly fully subscribed: virtually all available seats are filled, so the admission of additional targeted students necessarily displaces an equivalent number of non-targeted students. Private institutions, by contrast, fill only around half of their available seats (Appendix Figure A.3). When displaced non-targeted students turn to the private sector, they can be accommodated by previously unfilled seats without necessarily crowding out other students. This slack capacity is therefore consistent with displacement within federal institutions coexisting with stable overall enrollment among non-targeted students.

5.3 Heterogeneity by Socioeconomic Status

Responses to a larger quota expansion may vary with schools' socioeconomic composition because students differ in their baseline college attendance probabilities and in their capabilities to adjust their educational choices. Expanded admission opportunities may generate stronger pre-college responses in lower-SES schools, where baseline college-going rates are lower. The ability of non-targeted students to substitute toward private or other public institutions may also vary with socioeconomic resources as it affects, for example, their outside options.

We therefore examine whether the effects of a larger quota expansion differ across schools serving more or less disadvantaged students. We split the sample at the median value of the INSE socioeconomic index,[11] separately within the targeted and non-targeted groups. Using the same matching procedures as the main analysis, we compute within-pair DiD values and regress them on a post-treatment indicator interacted with the treated school's above-median INSE status.

Tables A.3–A.5 report the estimates for each SES group alongside the within-sector difference. The clearest socioeconomic heterogeneity appears in ENEM participation among tar-

[11] A school socioeconomic index calculated by the National Institute of Educational Studies and Research Anísio Teixeira (INEP).

geted students. Table A.3 shows that targeted students in below-median-SES schools increase ENEM participation by 3.3 percentage points, compared with an imprecise 0.6 percentage-point increase in above-median-SES schools; the 2.7 percentage-point difference is statistically significant. This stronger pre-college response among students with lower pre-policy college-going rates is consistent with changes in aspirations or perceived returns to college, but may also reflect information, exam preparation, or application decisions. Our design cannot distinguish among these channels. The effects on targeted students' high-school graduation and overall college enrollment, as well as the decline in ENEM participation among non-targeted students, are not statistically different across SES groups.

Reallocation across college types and higher on-time completion among non-targeted students are not confined to one SES group. Tables A.4 and A.5 show declines in federal enrollment and increases in private enrollment in both SES groups, with no statistically significant differences between them. Private graduation also increases in both groups. The increase is larger among below-median-SES schools, although the difference is statistically significant only at the 10 percent level. The remaining between-group differences in sectoral enrollment and graduation are statistically insignificant. The evidence therefore indicates that the primary reallocation and on-time graduation patterns extend across the socioeconomic distribution, while the strongest evidence of heterogeneity is the ENEM response among targeted students. Section 6 assesses whether the main findings persist across alternative estimation samples, counterfactual constructions, and definitions of policy expansion.

6 Robustness Checks

This section assesses whether the findings remain stable when we vary the construction of the counterfactual and the definition of quota expansion. We first compare the preferred matched difference-in-differences estimates with estimates from the full school panel and synthetic difference-in-differences. We then vary the contrast in quota expansion and use a continuous expansion measure. Finally, we assess whether restricting the sample to students with an observed taxpayer identification number biases our estimates.

6.1 Alternative Counterfactual Constructions

Alternative counterfactual constructions test whether the results depend on matching or the restricted matched sample. First, we present estimates using a standard two-way fixed effects (TWFE) event study without matching. This specification uses the full school-level panel with school and cohort fixed effects and standard errors clustered at the municipality level, providing a benchmark that relies directly on the parallel trends assumption without any reweighting of the sample. Second, we implement synthetic difference-in-differences following Arkhangelsky et al. (2021). The estimator reweights units and periods to construct an alternative counterfactual path for the larger-expansion group.

Appendix Table A.6 compares the preferred matched estimates with the full-sample DiD and synthetic DiD estimates. Panel A reports the primary outcomes, Panel B reports college enrollment by institution type, and Panel C reports enrollment in elite institutions. For students in targeted schools, the direction and magnitudes of the effects are broadly consistent with our preferred matched specification: exam take-up increases significantly, and college enrollment rises. High school graduation also increases for targeted students. For non-targeted students, the estimates remain qualitatively consistent, with a decline in exam take-up and a positive effect on college graduation. The positive college-graduation estimate for non-targeted students appears in all three specifications and is statistically significant in each.

The substitution pattern is also preserved: non-targeted students shift from federal to private institutions. The federal-enrollment estimates are negative and the private-enrollment estimates are positive for non-targeted students under every counterfactual construction. Enrollment in elite institutions also increases for targeted students and declines for non-targeted students across all three specifications. The convergence of these estimates shows that the main reallocation pattern is not specific to the matched comparison sample.

Figures A.6-A.8 show the synthetic DiD plots for all outcomes. Figure A.6 covers the four primary outcomes; Figures A.7 and A.8 present the enrollment and graduation breakdowns by institution type. Across panels, the treated-minus-synthetic differences are small over the available pre-policy cohorts, providing an additional pre-trends diagnostic. Post-treatment, the paths diverge in directions consistent with the point estimates and with the dynamic effects estimated in the preferred empirical strategy.

6.2 Alternative Definitions of Quota Expansion

Our second set of exercises assesses whether the results depend on dichotomizing $Jump_m$ at the sample median. First, we test the sensitivity of our results to the treatment definition by restricting the sample to municipalities with $Jump_m < 0.15$ (control group) and those with $Jump_m \geq 0.35$ (treated group), thereby increasing the expansion contrast while also changing the composition of the estimation sample. Second, we generalize the effects of the expansion by replacing the binary indicator $\mathbf{1}\{Jump_m \geq 0.20\}$ with continuous $Jump_m$ in a two-way fixed effects specification estimated on the full school-level panel without matching. For comparison, each coefficient and standard error from the continuous-$Jump_m$ specification is multiplied by 0.20, so the reported estimates represent a 20 percentage-point increase in $Jump_m$ and can be compared more directly with the preferred binary estimates.

Appendix Table A.7 compares the preferred median split, the higher-contrast binary definition, and a continuous expansion measure. The main qualitative patterns remain robust under the higher-contrast binary definition, although magnitudes and statistical significance vary as the expansion differential and estimation sample change. For students in targeted schools, the effect on high school graduation is no longer statistically significant under the higher-contrast definition, though its direction remains positive. The effect on exam take-up is even larger and more precisely estimated (3.3 p.p., $p < 0.01$) than in the main specification, consistent with a larger expansion contrast. College enrollment and graduation effects also become statistically significant for targeted students (1.1 p.p. and 0.5 p.p., respectively). For non-targeted students, we now observe a statistically significant negative effect on high school graduation (-0.8 p.p.) alongside a robust negative effect on exam take-up (-1.8 p.p.). College enrollment increases significantly for both groups.

The reallocation and elite-enrollment results are especially stable under the higher-contrast definition. The displacement of non-targeted students from federal institutions is even larger (-2.1 p.p., $p < 0.01$), and the private enrollment increase for non-targeted students is substantially larger (+3.1 p.p., $p < 0.01$). The results for elite college enrollment remain robust, with targeted students gaining (+2.4 p.p., $p < 0.05$) and non-targeted students losing access (-2.1 p.p., $p < 0.01$). The larger magnitudes on several margins are consistent with the sharper expansion contrast, although the accompanying change in sample composition prevents interpreting these differences exclusively or primarily as dose-response effects.

The continuous specification provides a separate robustness exercise that retains the full school panel and uses all available variation in quota expansion. The results preserve the main directions: targeted students increase exam participation and college enrollment, while non-targeted students shift from federal toward private institutions and become less likely to enroll in elite institutions. In summary, the ENEM and institutional-reallocation results, together with the positive college-completion effect for non-targeted students, remain qualitatively stable across expansion definitions. The high-school completion estimate for targeted students remains positive but is less stable in magnitude and precision.

6.3 Robustness to Missing CPFs

Finally, we examine the robustness of our results to the presence of missing taxpayer identification numbers (CPF). Outcomes beyond high school graduation, including ENEM participation, college enrollment, and college graduation, are identified via CPF linkage. Students without a CPF (approximately 19 percent of the population of first-year high school students) cannot be tracked to these downstream outcomes. We assess the selection induced by CPF availability using high school graduation, which is recorded in the school census for all students and does not rely on CPF linkage. Appendix Table A.8 compares estimates for the full school-level sample with estimates restricted to students with an observed CPF. This comparison shows whether the CPF restriction changes the estimated effect for an outcome observed in both samples. For targeted students, the estimated effect on high school graduation is marginally significant in the full sample ($+0.015^{*}$) and remains positive, although marginally smaller and statistically insignificant ($+0.011$), in the CPF-restricted sample. For non-targeted students, the estimates are small and statistically insignificant in both samples. The similar pattern across the full and CPF-restricted samples indicates that selection into observed CPF status is unlikely to drive the main results.

7 Earnings-Equivalent Implications of the Educational Effects

Our results show that a larger quota expansion changed both educational attainment and the allocation of students across institution types. Targeted students became more likely to complete high school and enroll in elite institutions. Non-targeted students were displaced from federal institutions, but maintained overall college enrollment by shifting toward private institutions and ultimately experienced higher college graduation rates. Assessing the net implications of these effects requires weighing the positive contributions from increased attainment against the potential negative contributions from displacement and reallocation.

We translate the causal effects estimated from our preferred strategy into an earnings-based accounting framework that places the estimated educational responses on a common, economically interpretable scale. Specifically, we multiply the estimated effect on each sequential educational margin by the observed earnings increment associated with that transition and sum the resulting contributions to construct an earnings-weighted index. We construct the index separately for targeted and non-targeted students and then aggregate each group-specific amount across the corresponding population in high-expansion municipalities. The observed earnings increments serve as fixed weights, so the calculation assumes that the earnings differentials associated with each educational outcome remain unchanged by the policy. In practice, affirmative action may alter realized earnings differentials by changing the composition of graduates or through equilibrium responses in the labor market.[12] The resulting index should therefore be interpreted as an accounting benchmark for the direction and scale of the combined educational effects, rather than as an estimate of realized earnings or welfare.

Earnings-weighted Index. The earnings-weighted index for group g is the expected earnings of a student drawn from that group, measured on a scale normalized by the average earnings of workers who completed high school and did not enter higher education. Let μ_j denote the average earnings benchmark for educational state j, and let $R_j = \mu_j/\mu_{\text{HS}}$ denote that benchmark relative to completed high-school earnings, so that $R_{\text{HS}} = 1$ by construction. Averaging the benchmarks over the group's distribution across educational states gives

$$\bar{w}_g = P_{\text{HSinc},g} R_{\text{HSinc}} + P_{\text{HS},g} + P_{\text{enr},g}(R_{\text{acc}} - 1) + \sum_k P_{\text{grad},k,g}(R_k - R_{\text{acc}}). \tag{4}$$

where $P_{\text{HSinc},g}$ and $P_{\text{HS},g} = 1 - P_{\text{HSinc},g}$ denote the shares of students in group g who do not and do complete high school, $P_{\text{enr},g}$ denotes the share who enroll in higher education, and $P_{\text{grad},k,g}$ denotes the share who graduate from institution type $k \in \{\text{federal}, \text{other public}, \text{private}\}$. The benchmarks R_{HSinc}, R_{acc}, and R_k correspond to incomplete high school, higher-education access without graduation, and graduation from institution type k, respectively. The second term carries no coefficient because $R_{\text{HS}} = 1$, and the college terms enter as increments over the state a student would otherwise have reached.

Holding the earnings benchmarks fixed, a larger quota expansion changes the earnings-weighted index through its effects on educational attainment and the allocation of students across institution types. Differencing Equation (4) and using $\Delta P_{\text{HSinc},g} = -\Delta P_{\text{HS},g}$, the first two terms collapse into a single high-school term with increment $1 - R_{\text{HSinc}}$, so the implied change in the index for group g is

$$\Delta\bar{w}_g = \Delta P_{\text{HS},g}(1 - R_{\text{HSinc}}) + \Delta P_{\text{enr},g}(R_{\text{acc}} - 1) + \sum_k \Delta P_{\text{grad},k,g}(R_k - R_{\text{acc}}). \tag{5}$$

The average change $\Delta\bar{w}_g$ is the core of the accounting exercise and provides a common basis for comparing the combined effects across targeted and non-targeted students. It is measured in units of the earnings of a worker with complete high school: a value of 0.01 means

[12] Obtaining estimates of realized earnings or welfare effects would require substantially stronger assumptions than those imposed by this exercise, including assumptions about how affirmative action affects employment, labor market sorting, equilibrium earnings differentials, and general equilibrium adjustments.

that the expected earnings of a student in group g rise by one percent of that benchmark. Each term combines the change in an educational share induced by a larger quota expansion with the observed, fixed earnings increment associated with that transition. The individual terms show the relative importance of high-school completion, college access, and graduation from each institution type. Their sum indicates whether the positive contributions associated with increased attainment exceed the negative contributions associated with displacement and reallocation across the educational margins observed in the paper. Importantly, their aggregate scale also depends on the size of each group. We therefore also recover the aggregate earnings-equivalent amount as $\Delta W_g = N_g \Delta \bar{w}_g$, where N_g is the number of students in group g in high-expansion municipalities.

Implementation. Implementing this accounting requires two sets of empirical inputs. The first consists of the policy-induced changes in educational shares, which we estimate using the preferred matched difference-in-differences specification. The second consists of earnings benchmarks for the educational states and institution types represented in Equation (5). The second set is constructed in Appendix C, which reports the two wage regressions, the calibration that places all benchmarks on a common scale, and the resulting values of R_{HSinc}, R_{acc} and R_k used below.

Calculating the Index. We calculate the estimated change in the earnings-weighted index by combining the calibrated earnings benchmarks (R) with the difference-in-differences estimates of the corresponding changes in educational shares ($\hat{\beta}$). From Equation (5), we use $\hat{\beta}_{\text{HS},g}$ as an estimate of $\Delta P_{\text{HS},g}$, $\hat{\beta}_{\text{enr},g}$ for $\Delta P_{\text{enr},g}$, and $\hat{\beta}_{\text{grad},k,g}$ for $\Delta P_{\text{grad},k,g}$. Treating the calibrated earnings benchmarks as fixed, substituting these estimates into Equation (5) gives the change in the index implied by the estimated educational responses.

The per-student index ($\widehat{\Delta \bar{w}_g}$) averages the earnings-weighted educational responses over all students in group g. Multiplying this average by group size produces the aggregate earnings-equivalent amount, $\widehat{\Delta W}_g = N_g \times \widehat{\Delta \bar{w}}_g$, where N_g counts the students enrolled in 2009 in high-expansion municipalities. The aggregate calculation is expressed in multiples of the earnings of a worker with complete high school over the same period. We take N_g to be the number of students enrolled in 2009 in high-expansion municipalities for whom the college outcomes are observed, $N_{\text{targeted}} = 278{,}451$ and $N_{\text{non-targeted}} = 62{,}933$, since the college margins carry most of the earnings-weighted response.

Results. Table 4 reports the results. The earnings-weighted index is positive for both targeted and non-targeted students. Two features explain these results. For targeted students, the largest individual contribution comes from the high-school completion effect, valued at an increment of 0.195 relative to the high-school-complete earnings benchmark. For non-targeted students, the positive contribution is driven primarily by the private graduation effect and its incremental benchmark of 0.853 beyond college access. This contribution more than offsets the decline in federal graduation, valued at an incremental benchmark of 1.065. These calculations imply that the positive contributions associated with increased attainment exceed the negative contributions associated with displacement and reallocation across the educational margins observed in the paper.

Table 4: Earnings-weighted Index of the Estimated Educational Effects

Outcome	Earnings increment ($R - R_{\text{base}}$)	Targeted: Effect ($\hat{\beta}$)	Targeted: Contribution $\hat{\beta} \times (R - R_{\text{base}})$	Non-targeted: Effect ($\hat{\beta}$)	Non-targeted: Contribution $\hat{\beta} \times (R - R_{\text{base}})$
HS graduation	0.195	0.015	0.0029	0.005	0.0009
College enrollment	0.309	0.006	0.0019	0.008	0.0025
Federal graduation	1.065	-0.000	-0.0000	-0.006	-0.0062
Other-public graduation	0.935	0.000	0.0001	0.001	0.0011
Private graduation	0.853	0.002	0.0020	0.014	0.0123
Per-student earnings-weighted index			**0.0069**		**0.0106**
Students in high-expansion municipalities (N)			278,451		62,933
Aggregate HS contribution			807		58
Aggregate college contribution			1,117		611
Aggregate earnings-equivalent amount			**1,923**		**669**
Combined aggregate amount			**2,592**		

Notes: Effects are the preferred enrollment-weighted matched difference-in-differences estimates. Each per-student contribution equals the estimated effect multiplied by $R - R_{\text{base}}$. For high-school graduation the increment is $1 - R_{\text{HSinc}}$; for college enrollment it is $R_{\text{acc}} - 1$; for graduation it is $R_k - R_{\text{acc}}$. High-expansion municipalities are those with $Jump_m \geq 0.20$, and N is the number of students enrolled in them in 2009 for whom the college outcomes are observed. Aggregate amounts equal the per-student contributions multiplied by N and are reported in multiples of the earnings of a worker with complete high school over the same period. All quantities are computed from unrounded coefficients and rounded only for display, so displayed components may differ from displayed totals by one unit. The underlying wage measures are monthly, but the earnings ratios and the resulting accounting are invariant to whether wages are expressed monthly or annually.

The aggregate earnings-equivalent amount shows the scale of the earnings-weighted educational responses across the population of high-expansion municipalities. The amount for targeted students equals the average earnings of approximately 1,923 workers with complete high school over the same earnings period. The corresponding amount for non-targeted students equals the average earnings of approximately 669 such workers, producing a combined benchmark equal to the earnings of approximately 2,592 workers with complete high school.

The per-student index is smaller for targeted than for non-targeted students, but the pronounced asymmetry in group sizes reverses that ordering in the aggregate, so the gains among the much larger targeted population dominate the overall earnings-equivalent amount.

We note three limitations that this accounting cannot address. First, the earnings benchmarks are held fixed, whereas an expansion of this magnitude may itself move the returns to schooling: if a larger supply of graduates compresses the college premium, the benchmarks used here overstate the earnings associated with the induced transitions. Second, the available graduation window may not capture all changes in degree completion. Students at federal universities tend to take longer to complete their degrees than those at private institutions, even conditional on program length. Students induced by the policy to enroll in federal universities may therefore not yet have graduated by the end of our observation window. More broadly, targeted students come from disadvantaged backgrounds and may take longer to graduate regardless of institution type, so the estimates should be read against the on-time outcomes our data observe. Third, the exercise counts gross earnings-equivalents and nets out no costs: tuition and other direct costs of attendance, earnings forgone while enrolled, and the public outlay required to expand access are all absent. For these reasons, the amounts reported here are interpreted as an accounting benchmark for the direction and scale of the combined educational effects, and not a net social return.

8 Conclusion

Our paper studies how high school students adjust their educational pathways in response to a large-scale expansion of affirmative action in elite public universities. We evaluate the expansion of affirmative action in Brazil following the 2012 Quota Law. Using population data on high school students, our paper is the first to estimate responses along the full educational pipeline, from high school graduation through college completion.

Our findings show that exposure to affirmative action during high school reshapes educational trajectories through both anticipatory responses and reallocation across higher education institution types. Targeted students increase high-school graduation, exam participation, and college enrollment, with gains extending beyond the federal institutions directly affected by the policy. At the same time, non-targeted students do not exit higher education despite losing access to federal universities. Instead, they substitute mainly toward private institutions, preserving overall enrollment and becoming more likely to complete college within the period we observe.

We complement our analysis with an earnings-based accounting exercise that uses observed earnings differentials as fixed weights to place the estimated educational responses on a common scale. The resulting index is positive for both groups and implies a larger aggregate earnings-equivalent amount for targeted students once group size is considered. Importantly, these calculations are benchmarks for direction and scale, not estimates of realized earnings or welfare.

Taken together, our results indicate that policies that alter admission probabilities at selective universities can induce forward-looking behavioral responses, with downstream effects that reshape the allocation of students across the entire post-secondary system. Understanding these general reallocation patterns is essential for assessing both the distributional and aggregate implications of preferential admissions reforms.

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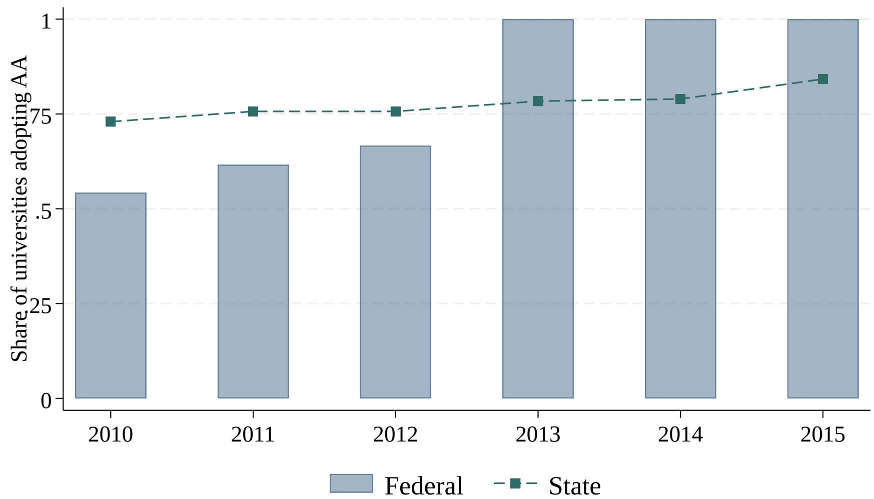
Online Appendix

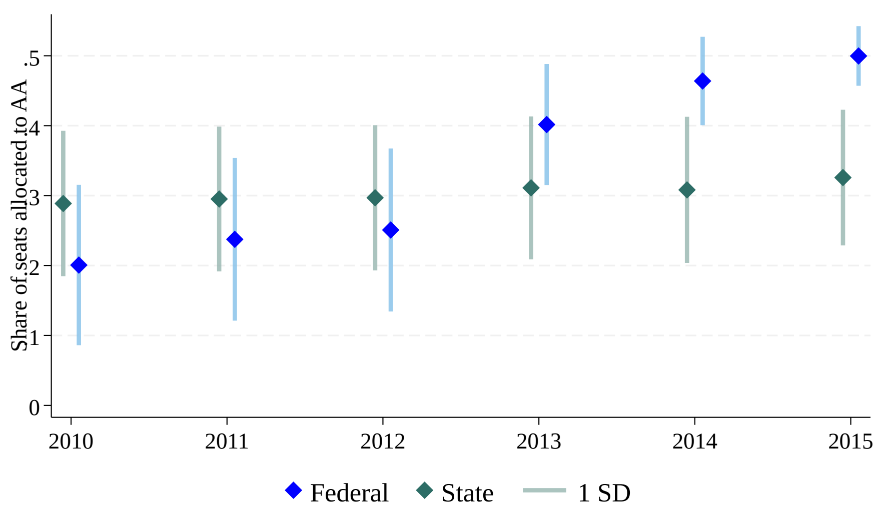
A Additional Figures

Figure A.1: Affirmative Action Policies: Adoption and Reserved-Seat Shares

(a) Share of Institutions with Public-School Quotas



(b) Average Share of Seats Reserved under Affirmative Action



Notes: The sample includes 94 federal and 37 state institutions in 2010, and 103 federal and 38 state institutions in 2015. Federal institutions comprise federal universities and federal institutes. State institutions include only universities, which are more directly comparable to federal universities. In 2015, federal institutions offered on average 2,881 total seats, while state universities offered on average 3,062 total seats.

Figure A.2: Longitudinal Data Construction and Policy Exposure by Cohort

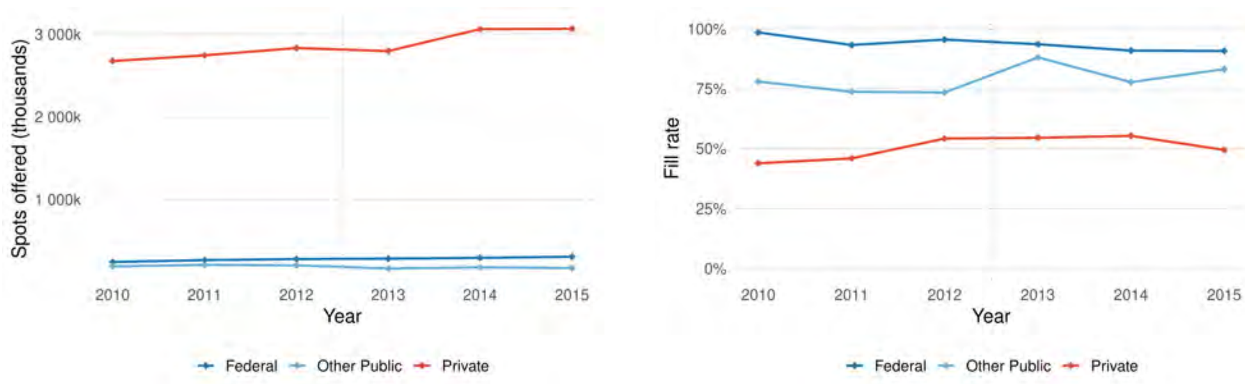
	High School			***Higher Education***					
	1st	2nd	3rd	1st	2nd	3rd	4th	5th	6th
Cohort	t	t+1	t+2	t+3	t+4	t+5	t+6	t+7	t+8
Control	2008	2009	2010	2011	2012	2013	2014	2015	2016
Control	2009	2010	2011	2012	2013	2014	2015	2016	2017
Treat	2010	2011	2012	2013	2014	2015	2016	2017	2018
Treat	2011	2012	2013	2014	2015	2016	2017	2018	2019
Treat	2012	2013	2014	2015	2016	2017	2018	2019	2020
Dataset	**CBE**	**CBE**	**CBE, ENEM**	**CSUP**	**CSUP**	**CSUP**	**CSUP**	**CSUP**	**CSUP**

Notes: The figure shows how we construct the individual-level longitudinal dataset. We start from students entering the first year of high school in cohorts t from 2008 through 2012 and follow them through $t+8$, observing high-school graduation, ENEM take-up, college enrollment, and college graduation. The dashed red boxes mark the announcement of the law in mid-2012. The 2010 cohort learned about the law during its third year of high school, before ENEM registration and college decisions for the 2013 entering class. The 2011 and 2012 cohorts learned about the law during their second and first years of high school, respectively. The 2008 and 2009 cohorts were not exposed to the law during their on-track progression from high school to college, although students from these cohorts could apply to college in later years.

Figure A.3: Higher-Education Capacity and Fill Rates by Institution Type

(a) Available Seats by Institution Type

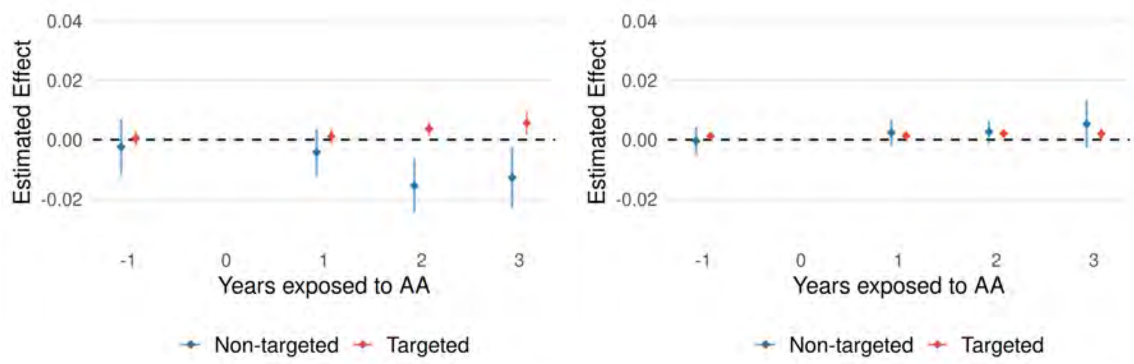
(b) Fill Rate by Institution Type



Notes: Panel (a) shows the total number of available seats by institution type (federal, other public, and private) over the period 2010–2015, based on the *Censo da Educação Superior*. Panel (b) shows the fill rate by institution type over the same period, defined as the ratio of enrolled students to available seats. The fill rate is weighted by the number of available seats in 2012 (the pre-treatment year). Federal institutions operate close to full capacity throughout the period, while private institutions consistently fill only around half of their available seats, indicating available capacity that could accommodate students displaced from the federal sector.

Figure A.4: Enrollment in Public Institutions by Years of High-School Exposure

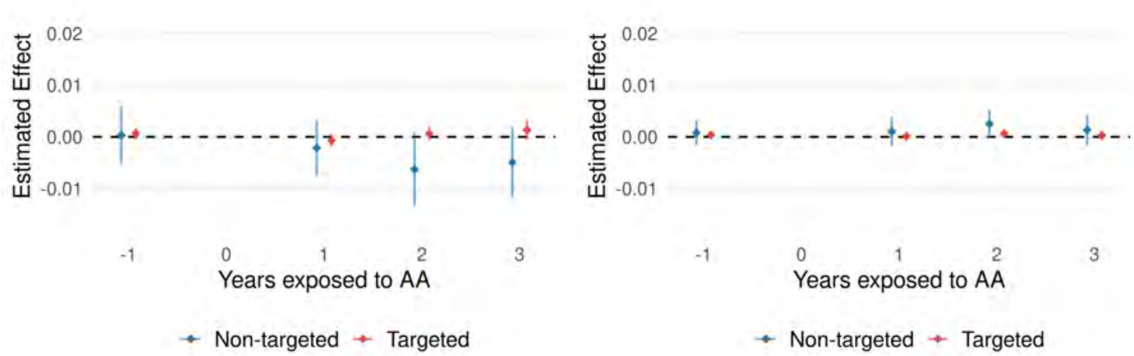
(a) Public Enrollment (b) Other Public Enrollment



Notes: The figure reports matched difference-in-differences estimates of the effects of a larger quota expansion. Enrollment is measured in $t+3$, three years after high-school entry. Public enrollment includes federal, state, and municipal institutions. Other-public enrollment includes state and municipal institutions not directly subject to the federal law. The pre-policy coefficient compares the 2008 and 2009 cohorts and serves as a pre-trends diagnostic. Vertical bars denote 95 percent confidence intervals based on standard errors clustered at the high-expansion municipality level.

Figure A.5: Graduation from Public Institutions by Years of High-School Exposure

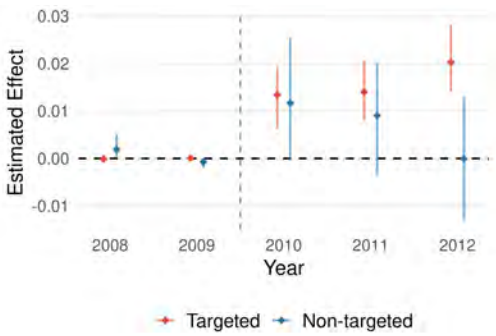
(a) Public Graduation (b) Other Public Graduation



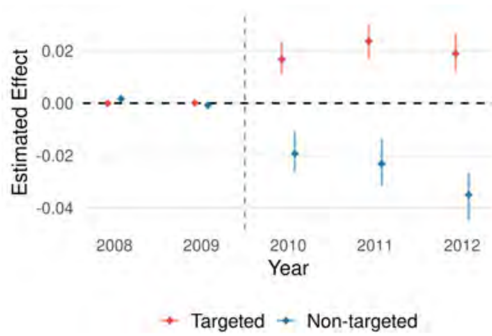
Notes: The figure reports matched difference-in-differences estimates of the effects of a larger quota expansion. Graduation is measured by $t+8$, eight years after high-school entry. Public graduation includes federal, state, and municipal institutions. Other-public graduation includes state and municipal institutions not directly subject to the federal law. The pre-policy coefficient compares the 2008 and 2009 cohorts and serves as a pre-trends diagnostic. Vertical bars denote 95 percent confidence intervals based on standard errors clustered at the high-expansion municipality level.

Figure A.6: Synthetic Difference-in-Differences: Primary Outcomes

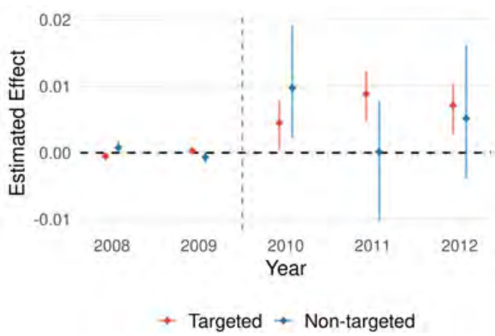
(a) High-School Graduation



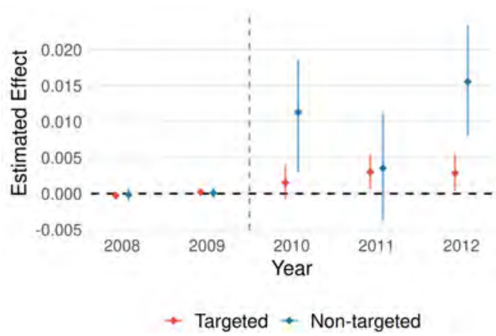
(b) ENEM Take-up



(c) College Enrollment

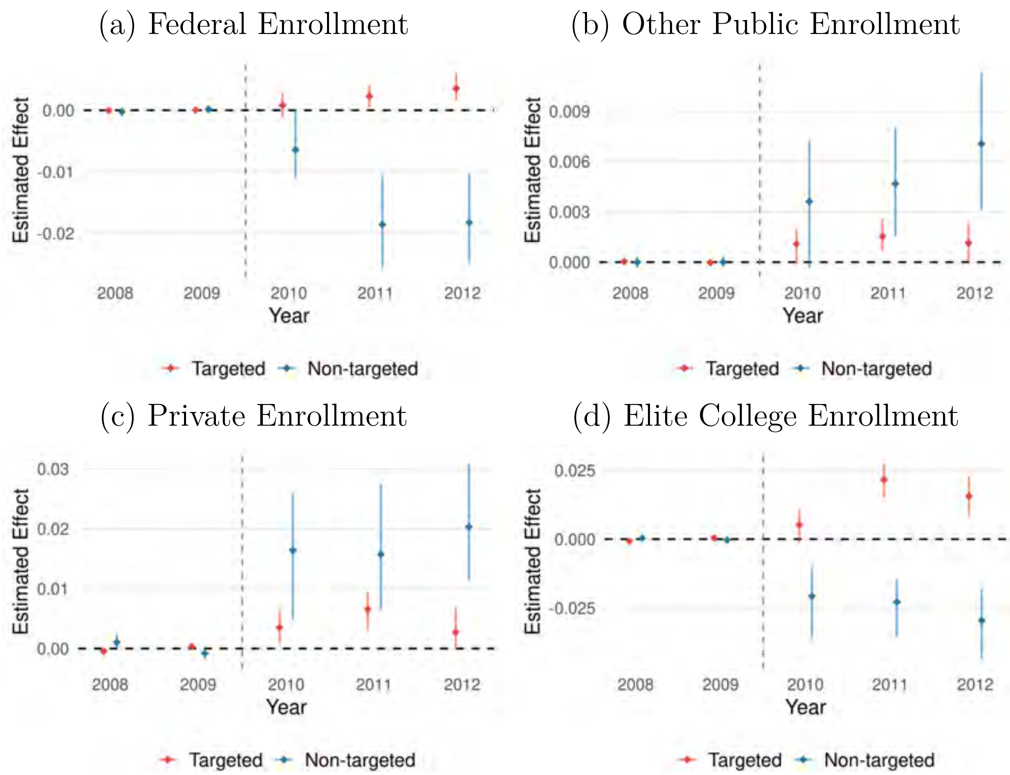


(d) College Graduation



Notes: Each panel shows the demeaned treated-minus-synthetic-control difference by calendar year, extracted from the synthetic difference-in-differences estimate. Within each school group, treated schools are located in high-expansion municipalities ($Jump_m \geq 0.20$), and the donor pool consists of schools in low-expansion municipalities ($Jump_m < 0.20$). The series is normalized so that the weighted pre-treatment average equals zero. The dashed vertical line marks the midpoint between the last pre-treatment and first post-treatment years. Shaded areas denote 95 percent bootstrap confidence intervals based on 100 draws, with clusters resampled at the school level. High-school graduation and ENEM take-up are measured in $t+2$, college enrollment in $t+3$, and college graduation by $t+8$. "Targeted" refers to public schools, while "Non-targeted" refers to private schools.

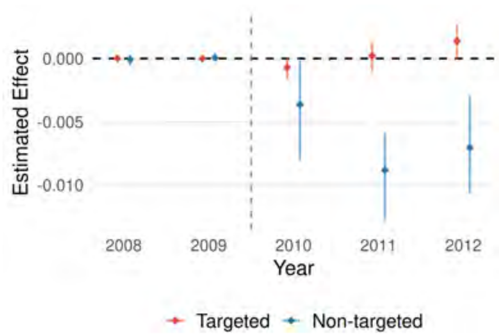
Figure A.7: Synthetic Difference-in-Differences: College Enrollment by Institution Type



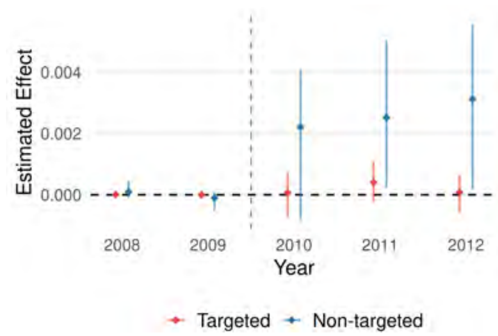
Notes: Each panel shows the demeaned treated-minus-synthetic-control difference by calendar year, extracted from the synthetic difference-in-differences estimate. Within each school group, treated schools are located in high-expansion municipalities ($Jump_m \geq 0.20$), and the donor pool consists of schools in low-expansion municipalities ($Jump_m < 0.20$). The series is normalized so that the weighted pre-treatment average equals zero. The dashed vertical line marks the midpoint between the last pre-treatment and first post-treatment years. Shaded areas denote 95 percent bootstrap confidence intervals based on 100 draws, with clusters resampled at the school level. Enrollment is measured in $t + 3$, three years after high-school entry. *Elite College Enrollment* is defined as enrollment in a major–institution combination in the top decile of the ENEM score distribution. “Targeted” refers to public schools, while “Non-targeted” refers to private schools.

Figure A.8: Synthetic Difference-in-Differences: College Graduation by Institution Type

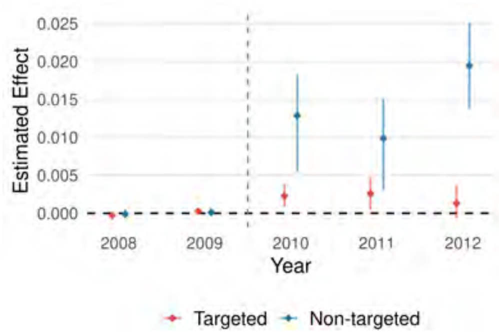
(a) Federal Graduation



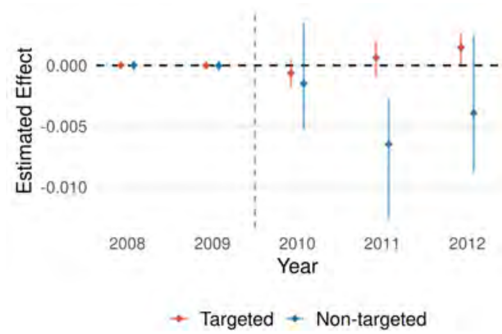
(b) Other Public Graduation



(c) Private Graduation



(d) Public Graduation



Notes: Each panel shows the demeaned treated-minus-synthetic-control difference by calendar year, extracted from the synthetic difference-in-differences estimate. Within each school group, treated schools are located in high-expansion municipalities ($Jump_m \geq 0.20$), and the donor pool consists of schools in low-expansion municipalities ($Jump_m < 0.20$). The series is normalized so that the weighted pre-treatment average equals zero. The dashed vertical line marks the midpoint between the last pre-treatment and first post-treatment years. Shaded areas denote 95 percent bootstrap confidence intervals based on 100 draws, with clusters resampled at the school level. Graduation outcomes identify students who enroll at $t + 3$ and complete a degree by $t + 8$. "Targeted" refers to public schools, while "Non-targeted" refers to private schools.

B Additional Tables

Table A.1: Covariate Balance Before and After Matching: Targeted Schools

	Pre-matching means		**Treated − Control**	
Variable	Control	Treated	Pre-matching	Post-matching
School location				
North	0.065	0.171	+0.107***	+0.099***
Northeast	0.196	0.252	+0.056***	+0.024
Southeast	0.470	0.384	-0.086***	-0.055***
South	0.192	0.072	-0.119***	-0.097***
Center-West	0.077	0.120	+0.042***	+0.029***
School characteristics				
Enrollment (2009)	176	187	+11.105***	+3.871
Share with CPF (2009)	0.826	0.805	-0.021***	-0.016***
Average age (2009)	15.154	15.282	+0.128***	+0.080***
Share non-white (2009)	0.513	0.622	+0.110***	+0.073***
Avg. ENEM score (2009)	-0.174	-0.177	-0.003	+0.006
Pre-policy outcomes				
HS graduation (2008)	0.568	0.552	-0.016***	+0.002
HS graduation (2009)	0.571	0.559	-0.013***	+0.005
ENEM take-up (2008)	0.364	0.402	+0.038***	+0.039***
ENEM take-up (2009)	0.409	0.449	+0.041***	+0.038***
College enrollment (2008)	0.128	0.136	+0.008**	+0.011***
College enrollment (2009)	0.141	0.155	+0.014***	+0.015***
College graduation (2008)	0.053	0.054	+0.000	+0.003
College graduation (2009)	0.058	0.062	+0.004**	+0.005**
Observations	2,163	1,697	1,697 matched pairs	

Notes: The first two columns report means for the full unmatched sample. Treated schools are located in high-expansion municipalities ($Jump_m \geq 0.20$), while control schools are located in low-expansion municipalities ($Jump_m < 0.20$). The last two columns report treated-minus-control differences before and after one-to-one matching. Schools are matched without replacement using Mahalanobis distance. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Covariate Balance Before and After Matching: Non-Targeted Schools

	Pre-matching means		**Treated – Control**	
Variable	Control	Treated	Pre-matching	Post-matching
School location				
North	0.046	0.090	+0.044***	+0.047***
Northeast	0.235	0.297	+0.062***	+0.064***
Southeast	0.468	0.460	-0.008	-0.001
South	0.173	0.038	-0.135***	-0.140***
Center-West	0.079	0.115	+0.036***	+0.030**
School characteristics				
Enrollment (2009)	81	79	-1.827	-1.543
Share with CPF (2009)	0.803	0.783	-0.020***	-0.021***
Average age (2009)	14.713	14.801	+0.088***	+0.089***
Share non-white (2009)	0.276	0.337	+0.061***	+0.064***
Avg. ENEM score (2009)	0.903	0.907	+0.004	+0.003
Pre-policy outcomes				
HS graduation (2008)	0.805	0.781	-0.024***	-0.029***
HS graduation (2009)	0.818	0.778	-0.040***	-0.037***
ENEM take-up (2008)	0.680	0.749	+0.068***	+0.054***
ENEM take-up (2009)	0.732	0.784	+0.052***	+0.044***
College enrollment (2008)	0.507	0.516	+0.009	+0.007
College enrollment (2009)	0.524	0.523	-0.001	-0.001
College graduation (2008)	0.247	0.222	-0.026***	-0.022***
College graduation (2009)	0.247	0.223	-0.024***	-0.021***
Observations	1,048	922	922 matched pairs	

Notes: The first two columns report means for the full unmatched sample. Treated schools are located in high-expansion municipalities ($Jump_m \geq 0.20$), while control schools are located in low-expansion municipalities ($Jump_m < 0.20$). The last two columns report treated-minus-control differences before and after one-to-one matching. Schools are matched without replacement using Mahalanobis distance. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Heterogeneity by School Socioeconomic Status: Educational Outcomes

	Targeted			Non-targeted		
	Below	Above	Diff.	Below	Above	Diff.
High School						
HS Graduation	0.014	0.016**	0.002	0.006	0.003	-0.003
	(0.009)	(0.008)	(0.007)	(0.006)	(0.010)	(0.008)
ENEM Take-up	0.033***	0.006	-0.027**	-0.015**	-0.024***	-0.009
	(0.012)	(0.007)	(0.013)	(0.007)	(0.007)	(0.009)
Pre-policy mean (2009): HS Graduation	0.549	0.553	–	0.807	0.792	–
Pre-policy mean (2009): ENEM Take-up	0.422	0.442	–	0.761	0.779	–
Observations	5,080	3,405	–	2,470	2,140	–
College						
College Enrollment	0.005*	0.008*	0.003	0.013	0.004	-0.009
	(0.003)	(0.005)	(0.005)	(0.010)	(0.007)	(0.011)
College Graduation	0.001	0.004	0.003	0.011**	0.009	-0.002
	(0.001)	(0.003)	(0.003)	(0.004)	(0.006)	(0.006)
Pre-policy mean (2009): College Enrollment	0.122	0.197	–	0.496	0.597	–
Pre-policy mean (2009): College Graduation	0.048	0.081	–	0.221	0.278	–
Observations	5,080	3,405	–	2,470	2,140	–

Notes: The table reports matched difference-in-differences estimates of the effects of a larger quota expansion, estimated using Equation (2). Schools are matched without replacement using Mahalanobis distance. "Targeted" refers to public schools (eligible under the public high school quota criteria), while "Non-targeted" refers to private schools (ineligible under the law). "Below" and "Above" denote schools below and above the median of the school socioeconomic index (INSE), respectively. "Diff." reports the above-minus-below difference. Outcome variables are defined relative to the student's reference cohort year ($t = 0$). *HS Graduation* refers to on-time high school completion in $t + 2$. *ENEM Take-up* denotes enrollment in the ENEM exam in $t + 2$. *College Enrollment* indicates enrollment in any higher education institution in $t + 3$ (one year after expected high school graduation). *College Graduation* refers to college graduation by $t + 8$. The HS Graduation regression is weighted by the 2009 number of students, while the other three regressions are weighted by the 2009 number of students with a CPF of the high-expansion school in each matched pair. Standard errors are clustered at the high-expansion municipality level and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: Heterogeneity by School Socioeconomic Status: College Enrollment by Institution Type

	Targeted			Non-targeted		
	Below	Above	Diff.	Below	Above	Diff.
Broad sector						
Public Enrollment	0.003**	0.003	0.000	-0.010**	-0.009	0.002
	(0.001)	(0.002)	(0.002)	(0.005)	(0.006)	(0.007)
Private Enrollment	0.002	0.005	0.003	0.025**	0.015**	-0.010
	(0.003)	(0.004)	(0.004)	(0.011)	(0.006)	(0.013)
Pre-policy mean (2009): Public Enrollment	0.037	0.055	–	0.195	0.263	–
Pre-policy mean (2009): Private Enrollment	0.088	0.147	–	0.323	0.370	–
Observations	5,080	3,405	–	2,470	2,140	–
Public sub-sector						
Federal Enrollment	0.002**	0.002	0.000	-0.014***	-0.013*	0.001
	(0.001)	(0.002)	(0.002)	(0.004)	(0.008)	(0.008)
Other public Enrollment	0.001	0.001	0.000	0.003	0.004	0.000
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.004)
Pre-policy mean (2009): Federal Enrollment	0.027	0.043	–	0.145	0.210	–
Pre-policy mean (2009): Other Public Enrollment	0.011	0.012	–	0.050	0.053	–
Observations	5,080	3,405	–	2,470	2,140	–

Notes: The table reports matched difference-in-differences estimates of the effects of a larger quota expansion on college enrollment by institution type, estimated using Equation (2). Schools are matched without replacement using Mahalanobis distance. "Targeted" refers to public schools (eligible under the public high school quota criteria), while "Non-targeted" refers to private schools (ineligible under the law). "Below" and "Above" denote schools below and above the median of the school socioeconomic index (INSE), respectively. "Diff." reports the above-minus-below difference. Outcome variables refer to enrollment in specific types of higher education institutions in $t + 3$ (one year after expected high school graduation). *Public* includes both federal and state/municipal institutions. *Private* refers to private institutions. *Federal* refers specifically to federal universities subject to the quota law. *Other public* refers to state or municipal institutions not subject to the federal law. Regressions are weighted by the 2009 number of students with a CPF of the high-expansion school in each matched pair. Standard errors are clustered at the high-expansion municipality level and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Heterogeneity by School Socioeconomic Status: College Graduation by Institution Type

	Targeted			Non-targeted		
	Below	Above	Diff.	Below	Above	Diff.
Broad sector						
Public Graduation	-0.000	0.001	0.001	-0.008***	-0.001	0.007
	(0.001)	(0.001)	(0.001)	(0.003)	(0.005)	(0.005)
Private Graduation	0.001	0.004	0.002	0.020***	0.010***	-0.010*
	(0.001)	(0.003)	(0.002)	(0.004)	(0.003)	(0.006)
Pre-policy mean (2009): Public Graduation	0.014	0.021	–	0.078	0.108	–
Pre-policy mean (2009): Private Graduation	0.034	0.060	–	0.143	0.171	–
Observations	5,080	3,405	–	2,470	2,140	–
Public sub-sector						
Federal Graduation	-0.000	0.000	0.000	-0.009***	-0.003	0.005
	(0.000)	(0.001)	(0.001)	(0.002)	(0.005)	(0.005)
Other Public Graduation	-0.000	0.000	0.000	0.000	0.002	0.002
	(0.000)	(0.000)	(0.001)	(0.002)	(0.002)	(0.002)
Pre-policy mean (2009): Federal Graduation	0.009	0.016	–	0.056	0.085	–
Pre-policy mean (2009): Other public Graduation	0.005	0.005	–	0.022	0.023	–
Observations	5,080	3,405	–	2,470	2,140	–

Notes: The table reports matched difference-in-differences estimates of the effects of a larger quota expansion on college graduation by institution type, estimated using Equation (2). Schools are matched without replacement using Mahalanobis distance. "Targeted" refers to public schools (eligible under the public high school quota criteria), while "Non-targeted" refers to private schools (ineligible under the law). "Below" and "Above" denote schools below and above the median of the school socioeconomic index (INSE), respectively. "Diff." reports the above-minus-below difference. College graduation by institution type is measured by $t+8$. *Public* includes both federal and state/municipal institutions. *Private* refers to private institutions. *Federal* refers specifically to federal universities subject to the quota law. *Other public* refers to state or municipal institutions not subject to the federal law. Regressions are weighted by the 2009 number of students with a CPF of the high-expansion school in each matched pair. Standard errors are clustered at the high-expansion municipality level and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Alternative Counterfactual Constructions

	Preferred matched DiD		Full-sample DiD		Synthetic DiD	
	Targeted	Non-targeted	Targeted	Non-targeted	Targeted	Non-targeted
Panel A: Educational pathway						
On-time HS graduation	0.015*	0.005	0.018*	-0.008	0.016***	0.007
	(0.008)	(0.007)	(0.010)	(0.018)	(0.003)	(0.006)
	[0.550]	[0.799]	[0.559]	[0.803]	[0.571]	[0.818]
ENEM take-up	0.022**	-0.020***	0.027**	-0.031	0.020***	-0.026***
	(0.009)	(0.005)	(0.011)	(0.019)	(0.003)	(0.004)
	[0.430]	[0.770]	[0.424]	[0.765]	[0.409]	[0.732]
College enrollment	0.006**	0.008	0.007	0.005	0.007***	0.005
	(0.003)	(0.007)	(0.005)	(0.009)	(0.002)	(0.004)
	[0.153]	[0.548]	[0.152]	[0.547]	[0.141]	[0.524]
On-time college graduation	0.002	0.010**	0.003	0.011*	0.002**	0.010***
	(0.002)	(0.004)	(0.002)	(0.006)	(0.001)	(0.003)
	[0.061]	[0.251]	[0.061]	[0.253]	[0.058]	[0.247]
Observations	8,485	4,610	19,300	9,850	19,300	9,850
Panel B: College enrollment by institution type						
Federal enrollment	0.002*	-0.013***	0.002**	-0.016**	0.002**	-0.014***
	(0.001)	(0.004)	(0.001)	(0.007)	(0.001)	(0.003)
	[0.033]	[0.179]	[0.032]	[0.172]	[0.026]	[0.125]
Other public enrollment	0.001	0.004	0.001*	0.005*	0.001***	0.005***
	(0.001)	(0.003)	(0.001)	(0.003)	(0.000)	(0.002)
	[0.011]	[0.051]	[0.011]	[0.054]	[0.011]	[0.055]
Private enrollment	0.003	0.020***	0.004	0.018*	0.004***	0.017***
	(0.003)	(0.007)	(0.005)	(0.010)	(0.001)	(0.003)
	[0.112]	[0.348]	[0.113]	[0.350]	[0.107]	[0.367]
Observations	8,485	4,610	19,300	9,850	19,300	9,850
Panel C: College enrollment by institutional selectivity						
Elite enrollment	0.021***	-0.020***	0.018**	-0.024***	0.014***	-0.024***
	(0.008)	(0.006)	(0.007)	(0.006)	(0.003)	(0.005)
	[0.067]	[0.271]	[0.067]	[0.280]	[0.064]	[0.231]
Observations	2,294	2,568	8,847	7,633	5,775	5,605

Notes: The table compares estimates using the same binary definition of larger quota expansion ($Jump_m \geq 0.20$ versus $Jump_m < 0.20$) but alternative constructions of the counterfactual. The preferred specification estimates difference-in-differences on matched school pairs. The full-sample specification retains the complete eligible school panel. Synthetic difference-in-differences reweights units and periods. Outcomes are school-cohort shares. High-school graduation and ENEM take-up are measured in $t+2$, college enrollment in $t+3$, and college graduation by $t+8$. Elite enrollment denotes enrollment in a major–institution combination in the top decile of the ENEM score distribution. The elite-enrollment outcome is available for a smaller sample; Panel C reports nonmissing school-cohort observations. Standard errors are reported in parentheses and pre-policy means in square brackets. The matched specification is weighted by the 2009 size of the high-expansion school in each pair, while the full-sample specification is weighted by each school's own 2009 size. For the HS graduation outcome we consider the size of a school to be the number of students within a school, for the other outcomes, we consider the number of students with a CPF. The matched and full-sample specifications cluster standard errors at the high-expansion municipality and municipality levels, respectively; synthetic DiD uses block-bootstrap standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Alternative Definitions of Quota Expansion

	Preferred binary definition $Jump_m \geq 0.20$ vs. $Jump_m < 0.20$		Higher-contrast binary definition $Jump_m \geq 0.35$ vs. $Jump_m < 0.15$		Continuous $Jump_m$ scaled by 0.20	
	Targeted	Non-targeted	Targeted	Non-targeted	Targeted	Non-targeted
Panel A: Educational pathway						
On-time HS graduation	0.015*	0.005	0.007	-0.008*	0.007*	-0.012*
	(0.008)	(0.007)	(0.006)	(0.004)	(0.004)	(0.007)
	[0.550]	[0.799]	[0.557]	[0.820]	[0.559]	[0.803]
ENEM take-up	0.022**	-0.020***	0.033***	-0.018**	0.018***	-0.016
	(0.009)	(0.005)	(0.010)	(0.009)	(0.006)	(0.010)
	[0.430]	[0.770]	[0.447]	[0.776]	[0.424]	[0.765]
College enrollment	0.006**	0.008	0.011***	0.014*	0.006**	0.005
	(0.003)	(0.007)	(0.003)	(0.008)	(0.002)	(0.005)
	[0.153]	[0.548]	[0.153]	[0.548]	[0.152]	[0.547]
On-time college graduation	0.002	0.010**	0.005**	0.010*	0.002**	0.006**
	(0.002)	(0.004)	(0.002)	(0.006)	(0.001)	(0.003)
	[0.061]	[0.251]	[0.063]	[0.255]	[0.061]	[0.253]
Observations	8,485	4,610	6,230	3,255	19,300	9,850

Notes: The table compares estimates under alternative definitions of municipal quota expansion. The first two specifications estimate matched difference-in-differences models. The preferred binary definition divides municipalities at the sample median of $Jump_m$. The higher-contrast definition retains municipalities with $Jump_m < 0.15$ as the comparison group and municipalities with $Jump_m \geq 0.35$ as the larger-expansion group. The continuous specification estimates difference-in-differences on the full school panel. Its reported coefficients and standard errors equal the original continuous-$Jump_m$ estimates multiplied by 0.20 and therefore represent a 20 percentage-point increase in quota expansion. Outcomes are school-cohort shares; college graduation is measured by $t + 8$. Standard errors are reported in parentheses and pre-policy means in square brackets. The matched specifications are weighted by the 2009 size of the high-expansion school in each pair, while the continuous specification is weighted by each school's own 2009 size. For the HS graduation outcome we consider the size of a school to be the number of students within a school, for the other outcomes, we consider the number of students with a CPF. The matched specifications cluster standard errors at the high-expansion municipality level, while the continuous specification clusters at the municipality level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Alternative Definitions of Quota Expansion

	Preferred binary definition $Jump_m \geq 0.20$ vs. $Jump_m < 0.20$		Higher-contrast binary definition $Jump_m \geq 0.35$ vs. $Jump_m < 0.15$		Continuous $Jump_m$ scaled by 0.20	
	Targeted	Non-targeted	Targeted	Non-targeted	Targeted	Non-targeted
Panel B: College enrollment by institution type						
Federal enrollment	0.002*	-0.013***	0.002	-0.021***	0.001**	-0.011***
	(0.001)	(0.004)	(0.001)	(0.006)	(0.001)	(0.004)
	[0.033]	[0.179]	[0.032]	[0.174]	[0.032]	[0.172]
Other public enrollment	0.001	0.004	0.002*	0.007**	0.001	0.003
	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.002)
	[0.011]	[0.051]	[0.012]	[0.054]	[0.011]	[0.054]
Private enrollment	0.003	0.020***	0.008***	0.031***	0.004*	0.014***
	(0.003)	(0.007)	(0.003)	(0.007)	(0.002)	(0.005)
	[0.112]	[0.348]	[0.113]	[0.346]	[0.113]	[0.350]
Observations	8,485	4,610	6,230	3,255	19,300	9,850
Panel C: College enrollment by institutional selectivity						
Elite enrollment	0.021***	-0.020***	0.024**	-0.021***	0.008**	-0.011***
	(0.008)	(0.006)	(0.010)	(0.008)	(0.003)	(0.004)
	[0.067]	[0.271]	[0.060]	[0.277]	[0.067]	[0.280]
Observations	2,294	2,568	1,687	1,811	8,847	7,633

Notes: The table compares estimates under alternative definitions of municipal quota expansion. The first two specifications estimate matched difference-in-differences models. The preferred binary definition divides municipalities at the sample median of $Jump_m$. The higher-contrast definition retains municipalities with $Jump_m < 0.15$ as the comparison group and municipalities with $Jump_m \geq 0.35$ as the larger-expansion group. The continuous specification estimates difference-in-differences on the full school panel. Its reported coefficients and standard errors equal the original continuous-$Jump_m$ estimates multiplied by 0.20 and therefore represent a 20 percentage-point increase in quota expansion. Outcomes are school-cohort shares; college graduation is measured by $t + 8$. Standard errors are reported in parentheses and pre-policy means in square brackets. The elite-enrollment outcome is available for a smaller sample; Panel C reports nonmissing school-cohort observations. The matched specifications are weighted by the 2009 size of the high-expansion school in each pair, while the continuous specification is weighted by each school's own 2009 size. For the HS graduation outcome, we consider the size of a school to be the number of students within a school, for the other outcomes, we consider the number of students with a CPF. The matched specifications cluster standard errors at the high-expansion municipality level, while the continuous specification clusters at the municipality level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Robustness to CPF Coverage: High-School Graduation

	All students		**Students with CPF**	
	Targeted	Non-targeted	Targeted	Non-targeted
Estimated Effect	0.015*	0.005	0.011	0.006
	(0.008)	(0.007)	(0.009)	(0.007)
Pre-policy mean (2009)	0.550	0.799	0.555	0.794
Observations	8,485	4,610	8,485	4,610

Notes: The table reports matched difference-in-differences estimates of the effects of a larger quota expansion on on-time high-school graduation, estimated using Equation (2). Schools are matched without replacement using Mahalanobis distance. "Targeted" refers to public schools (eligible under the public high school quota criteria), while "Non-targeted" refers to private schools (ineligible under the law). High-school graduation is observed for all students in the School Census. The first two columns use all students to construct school-cohort graduation shares, while the last two restrict both the numerator and denominator to students with an observed CPF. Regressions are weighted by the 2009 number of students with a CPF of the high-expansion school in each matched pair. Standard errors are clustered at the high-expansion municipality level and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C Earnings Benchmarks: Estimation and Calibration

This appendix reports the two wage regressions behind the earnings benchmarks used in Section 7, and the calibration that places all benchmarks on a common scale.

1. Estimating Earnings Benchmarks. We draw the earnings benchmarks from two sources. First, we use the *Relação Anual de Informações Sociais* (RAIS) for 2013–2015 to recover the earnings differentials associated with each level of schooling relative to a complete high school diploma. We estimate:

$$\begin{aligned}\ln w_{it} = \alpha &+ \beta_1 \mathbf{1}[\text{HS incomplete}]_i + \beta_2 \mathbf{1}[\text{HE incomplete}]_i \\ &+ \beta_3 \mathbf{1}[\text{HE complete}]_i + \mathbf{X}_i'\gamma + \delta_s + \delta_t + \varepsilon_{it},\end{aligned} \tag{6}$$

where $\mathbf{X}_i$ includes sex and race dummies, and δ_s, δ_t denote state and year fixed effects, HS refers to high school and HE to higher education. The sample is restricted to employed workers, with positive earnings. The omitted education category is workers who completed high school but did not enter higher education.

Second, to distinguish wages across federal, other public, and private institutions, we use data compiled by Feinmann and Rocha (2026), which contain program-level (institution $\times$ major) average wages ($\bar{w}$) for cohorts graduating between 2010 and 2015, observed in

2019–2021. We then estimate:

$$\ln \bar{w}_{ct} = \alpha + \beta_{\text{fed}}\mathbf{1}[\text{federal}]_c + \beta_{\text{otherpub}}\mathbf{1}[\text{other public}]_c + \mathbf{D}_c'\gamma + \delta_{\text{field}} + \delta_{\text{cohort}} + \delta_t + \varepsilon_{ct}, \quad (7)$$

where $\mathbf{D}_c$ contains program-level demographic averages (share female, share non-white, share with college-educated father, and average neighborhood income percentile), and fixed effects absorb field-of-study (OECD 4-digit), entry cohort, and observation year. Regressions are weighted by program-level enrollment. The omitted category is private institutions, and the sample includes only in-person degrees.

2. Common-scale Calibration. We place the earnings estimates on a common scale by expressing all sector-specific graduation benchmarks relative to workers who completed high school. Let $R = \exp(\hat{\beta})$ denote a relative earnings measure. The benchmarks for high-school completion ($1/R_{\text{HSinc}}$), higher-education access without completion (R_{acc}), and higher-education completion (R_{HE}) follow from Equation (6). Equation (7), however, provides earnings differentials across institution types relative to private institutions. We therefore set the private-sector benchmark so that the sector-share-weighted average of the benchmarks equals the overall higher-education benchmark R_{HE}. This yields the following calibration condition:[13]

$$R_{\text{HE}} = R_{\text{priv}}\Big(\omega_{\text{priv}} + \omega_{\text{otherpub}}\, R_{\text{otherpub/priv}} + \omega_{\text{fed}}\, R_{\text{fed/priv}}\Big), \quad (8)$$

where ω_k is the share of graduates from sector k (Federal: 12.1%; Other Public: 9.1%; Private: 78.8%), drawn from the *Censo da Educação Superior* 2013–2015.

Table A.9 reports both regressions, and Table A.10 summarizes the relative earnings benchmarks they imply. The first set (Panel A) reports relative earnings by schooling level, recovered from Equation (6): $R_{\text{HE}} = \exp(\hat{\beta}_3) = 2.195$ for higher-education completion, $R_{\text{acc}} = \exp(\hat{\beta}_2) = 1.309$ for higher-education access without completion, and $1/R_{\text{HSinc}} = \exp(-\hat{\beta}_1) = 1.242$ for high-school completion relative to incomplete high school. In Panel B, the institution-type earnings ratios therefore follow from Equation (7): $R_{\text{fed/priv}} = \exp(\hat{\beta}_{\text{fed}}) =$

[13] Equation (7) implies

$$R_{\text{otherpub}} = R_{\text{priv}} R_{\text{otherpub/priv}} \qquad \text{and} \qquad R_{\text{fed}} = R_{\text{priv}} R_{\text{fed/priv}}.$$

We impose

$$R_{\text{HE}} = \omega_{\text{priv}} R_{\text{priv}} + \omega_{\text{otherpub}} R_{\text{otherpub}} + \omega_{\text{fed}} R_{\text{fed}},$$

where ω_k is the share of higher-education graduates from sector k. Substituting the institution-type relationships into this condition yields Equation (8).

1.098 and $R_{\text{otherpub/priv}} = \exp(\hat{\beta}_{\text{otherpub}}) = 1.038$. Finally, we solve Equation (8) for the private-sector benchmark as well as R_{otherpub} and R_{fed}. The resulting sector-specific earnings benchmarks, each expressed relative to workers whose highest educational attainment is completed high school, are reported in Panel C of Table A.10. This adjustment allows the accounting to incorporate the earnings implications of substitution across federal, other public, and private institutions.

Table A.9: Wage Regressions Underlying the Earnings Benchmarks

Dependent variable: log earnings	(1) RAIS 2013–2015 Workers	(2) Feinmann and Rocha Graduates
Schooling level (vs. complete high school)		
Incomplete high school	-0.217***	
	(<0.001)	
	[-19.5%]	
Higher education, no degree	0.269***	
	(<0.001)	
	[+30.9%]	
Higher education completed	0.786***	
	(<0.001)	
	[+119.5%]	
Institution type (vs. private)		
Federal		0.094***
		(0.003)
		[+9.8%]
Other public		0.037***
		(0.003)
		[+3.8%]
Demographic controls	Sex, race	Program-level averages
Fixed effects	State, year	Field, cohort, wage year
Weighted by enrollment	No	Yes
Observations	108,618,792	177,537
R^2	0.349	0.528

Notes: Column (1) estimates Equation (6) on RAIS 2013–2015, restricted to employed workers with positive earnings and recorded schooling between incomplete high school and complete higher education; the omitted category is workers who completed high school and did not enter higher education. Column (2) estimates Equation (7) on the Feinmann and Rocha (2026) program-level data for cohorts graduating between 2010 and 2015, observed in 2019–2021, restricted to in-person degrees and weighted by program enrollment; the omitted category is private institutions. Standard errors are heteroskedasticity-robust and reported in parentheses. The implied earnings differential $(\exp(\hat{\beta}) - 1) \times 100$ is reported in brackets; these are the values that enter Table A.10. The RAIS standard errors round to zero at three decimals and are reported as < 0.001. $^{***}p < 0.01$.

Table A.10: Relative Earnings Benchmarks

	Relative earnings: R	Premium: $(R-1)\times 100$
Panel A: Earnings Differentials by Schooling Level (RAIS, 2013–2015)		
$1/R_{\text{HSinc}}$: HS completion (vs. HS incomplete)	1.242	+24.2%
R_{acc}: HE access, no graduation (vs. HS complete)	1.309	+30.9%
R_{HE}: HE graduation (vs. HS complete)	2.195	+119.5%
Panel B: Earnings Differentials by Institution Type Relative to Private (Feinmann and Rocha, 2026)		
$R_{\text{fed/priv}}$: Federal vs. private	1.098	+9.8%
$R_{\text{otherpub/priv}}$: Other public vs. private	1.038	+3.8%
Panel C: Sector-specific Earnings Benchmarks (vs. HS Complete)		
R_{fed}: Federal	2.374	+137.4%
R_{otherpub}: Other public	2.244	+124.4%
R_{priv}: Private	2.162	+116.2%

Notes: Panels A and B report transformed coefficients from Equations (6) and (7), with $R = \exp(\hat{\beta})$. Panel C is derived using $R_{\text{HE}} = R_{\text{priv}}(\omega_{\text{priv}} + \omega_{\text{otherpub}} R_{\text{otherpub/priv}} + \omega_{\text{fed}} R_{\text{fed/priv}})$ from Equation (8). The underlying wage measures are monthly. Since all benchmarks enter the accounting as earnings ratios, the results are invariant to whether earnings are expressed monthly or annually. Each ratio is relative to the comparison group indicated in the corresponding panel.