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Affirmative action and the choice of schools ☆

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ABSTRACT

Socioeconomic-based affirmative action in higher education has gained importance following controversies over race-based alternatives. In many settings, these interventions use a school-based criterion that selects beneficiaries relative to their peers. Exploiting a nationwide quota policy in Brazil that reserved a large share of vacancies in higher education for public-school students, I show that the reform increases movements from private to public schools by 31% and that movers come disproportionately from low-SES and low-quality private schools. An exploration of the mechanisms shows that movers increase their future probability of higher education attendance at the expense of attending poorer and lower-performing public schools. The reform also leads to changes in school choice of indirectly exposed cohorts and general-equilibrium effects in the form of school closure.

1. Introduction

Affirmative action (AA) policies in higher education have been adopted in different countries to mitigate inequality in access, performance, and graduation. In the US, these traditionally race-based policies started in the 1960s but were subject to many legal disputes leading to their ban in eight states in the 1990s (Akhtari et al., 2020). In response, some US states adopted the so-called "Top N-percent Plans", a school-based affirmative action that benefits students that graduated in the top N% of their school.[1] In other settings, such as Brazil, Israel, Chile, France, and the UK, socioeconomic-based affirmative action policies targeting low-SES schools have also been implemented as an alternative or as a complement to race-based AA.[2]

Using the school as a criterion for affirmative action eligibility, this type of intervention increases individuals' incentives for choosing an institution that improves their likelihood of acceptance into higher education. In this context, the literature has shown that the Top Percent Plans increase students' movements across high schools and high school integration (Cullen at al. (2013) and Estevan et al. (2020)). However, despite the increasing popularity of this type of initiative worldwide, little is known about its effects on school choice and school systems beyond the context of the Top Percent Plans in the US. Moreover, little is known about the mechanisms that drive the changes observed in individuals' decisions. This paper helps to fill this gap by analyzing how and why a large nationwide affirmative action targeted at

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[1] See Fryer et al. (2008) and Ellison and Pathak (2021) for a comprehensive analysis of color-conscious versus color-blind affirmative action in education in the US. See Horn and Flores (2003) for a comparison of the "Top N-percent Plans" adopted in three large states (Florida, Texas, and California).

[2] See Alon and Malamud (2014),Gallegos (2016), Thibaud (2019) and Sen (2022) for analysis of affirmative action policies with a socioeconomic criterion related to the individual's school in Israel, Chile, France, and the UK.

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public-school students in Brazil induces changes in school-choice decisions.

On August 29, 2012, the federal government approved the so-called "Quota Law" (hereafter QL). It established that 50 percent of vacancies in each major of each federal higher education institution must be reserved for students that attended all three years of upper secondary education in a public school.[3] Furthermore, it stipulates sub-quotas, within these 50 percent, for racial and economic-based minorities. Federal tertiary education institutions in Brazil are widely recognized in the country for their high quality and are free of any charge. Therefore, competition for a spot is fierce. By reserving a substantial percentage of vacancies for certain demographic groups, the federal government increased incentives for public school attendance in high school, especially for non-white and low-income individuals interested in progressing to higher education.

The adoption of QL creates a differential impact on each of the 93 federal institutions in Brazil, depending on their pre-reform levels of quotas. For example, some institutions already reserved as much as 50 percent of their slots for public-school students (e.g. Federal University of Juiz de Fora - UFJF). Others, however, had no quotas whatsoever (e.g. Federal University of Pernambuco - UFPE). QL exogenously imposes that all institutions adjust their quotas to 50 percent, creating cross-sectional variation among institutions which allows for the causal identification of policy effects. My benchmark analysis focuses on students in 9th grade since transitions to the public system need to occur exactly between 9th and 10th grade - between middle and high school - for AA eligibility. Although the effect of QL in each federal institution is observed, I do not know how a student in middle school is exposed to treatment affecting tertiary institutions. Based on the fact that previous place of residence is a large determinant of the locality of higher education attendance,[4] I assume that individuals in 9th grade are exposed to treatment happening at the higher education institutions of their microregion of residence. I then aggregate the treatment of all federal institutions within the microregion weighting by size and use data from the universe of students in the fifty microregions in Brazil where there is at least one headquarters of a federal university.[5]

My empirical strategy consists of a dynamic differences-in-differences model that compares changes in the average transition rates between the private and the public systems of 9th graders within the same school and microregion across time. The treated units are the schools located in microregions that experience variation in their levels of exposure to quotas. The controls, in contrast, are the schools located in microregions in which these levels stayed invariant. I estimate a dynamic specification with pre and post-periods and show that coefficients for trends in pre-periods are close to zero and not significant.

Benchmark results show that full exposure to QL increases movements of 9th graders from private middle schools to public high schools by 4.8 p.p. or 31% considering post-years 2012 to 2016 jointly, with stronger effects for non-whites, who benefit from additional sub-quotas. Movements are much higher among students attending 9th grade in low-SES and low-performing private schools, schools with a low probability of future federal higher education attendance, and private schools with two or more public schools within 1 km of distance. Taken together, these results suggest that individuals that choose to move from the private to the public system are likely to benefit the least from private education and face lower costs of transitioning to the public system.

I proceed by investigating which mechanisms explain these patterns of change in school choice. I show that QL directly increases the probability of federal higher education attendance for public-school students and decreases the probability for private-school students. Also, students that move from the private to the public system induced by the reform trade down and enroll in lower-SES and lower-performing public schools. This shows that, as predicted by classical human capital theory, the decrease in returns to private-school investment pushes individuals at the margin to the public system. Second, I show that QL also increases movements to the public system among individuals in 10th or 11th grades (although in a much lower magnitude than for 9th graders), who are not directly affected by a change in the probability of higher education attendance. This suggests the existence of spillover effects on indirectly exposed cohorts, such as an improvement in peer quality and in the environment of public schools. Indeed, suggestive evidence shows that QL reduces dropout rates for public high schools.[6] Finally, I show that the reform leads to private-school closure in the affected microregions three years after its implementation, suggesting that, in later periods, movements from private to public schools are magnified by general-equilibrium effects.

My paper broadly contributes to the literature that investigates how AA in undergraduate education impacts pre-college outcomes. One strand of this literature focuses on how such policies affect human capital investments.[7] More related to this paper, the second strand of this literature investigates how AA policies in higher education that use the school as a criterion for eligibility affect students' choices and the school system. As far as I know, the two contributions in this topic focus on the consequences of the Top 10-percent Plan in Texas. Cullen et al. (2013) compare students in the state with and without strategic school choice opportunities before and after the policy change, showing that among the one-quarter of individuals identified as having the opportunity to change schools, 1.3% alter their choice.[8] Estevan et al. (2020) provide a framework that rationalizes some empirical regularities observed following the Top N-percent Plans and show theoretically that, due to general-equilibrium effects in the form of a cascade, movements of students

[3] Admissions through the quota law are not based on high-school GPA and within-school rankings as in the US Top Percent Plans. Instead, all applicants from public schools are ranked (separately from applicants from private schools) based on performance on the admission exam for higher education. Public school attendance is used as a proxy for socioeconomic status. Section 2 provides more details on the institutional background.

[4] 84% of college students in Brazil report no inter-municipal migration after the age of 14 (Census of 2010).

[5] In Appendix B.2, I relax these restrictions and show that results are also robust to a measure of exposure that allows students to be affected by the treatment happening in higher education institutions of other localities.

[6] This evidence is in line with recent evidence found by Akhtari et al. (2020) in the case of Texas. The authors show that the reinstatement of race-based AA increases the pre-college human capital investment of minorities and decreases the racial gap in graduation.

[7] Bodoh-Creed and Hickman (2019) develop a structural model of college admissions framed as a contest in which the outcome is decided by the student's choice of human capital. Cotton et al. (2022) use a simple version of this model to derive testable predictions and find, through an experimental approach, that AA increases effort levels of the benefited group, while not affecting the non-benefited students. Akhtari et al. (2020) show that the reinstatement of race-based AA in Texas increases the pre-college human capital investment of minorities and decreases the racial gap in graduation. In contrast, Antonovics and Backes (2014) find no evidence that banning AA at the University of California affected human capital accumulation for high school students. In Brazil, Francis and Tannuri-Pianto (2012) and Estevan et al. (2019) find no behavioral effects of AA policies implemented in two different Brazilian universities on pre-college human capital accumulation, while Assunção and Ferman (2015) find that an AA policy implemented by the State University of Rio de Janeiro decreased investments by black students, the target group.

[8] They also show that among those also likely to be interested in attending a flagship university, the implied take-up rate is 5.8%. The authors argue that in the short run analyzed, the number of students affected is small enough that the impact on the distribution of peer quality across high schools is negligible. In the longer run, however, these changes might become more systemic, and lower-achieving schools might be indirectly affected.

will not be confined to the ones close to the N% threshold in their original school.[9]

My paper adds to the literature in two key dimensions. First, I exploit a nationwide policy experiment and rich administrative datasets both at the individual and the school level, allowing the identification of mechanisms that were not previously shown in the literature. Both Cullen et al. (2013) and Estevan et al. (2020) use data from the state of Texas only and define their control group as students not directly affected by the Top Percent Plan (students without opportunity for strategic school choice in the former, and students in lower grades in the latter). These control groups, however, could be indirectly affected by spillover and general-equilibrium effects of the policy, such as changes in peer quality or the segregation of schools, as both papers discuss. In my paper, instead, the nationwide QL reform allows me to define my control group as students from the same grade as the ones in the treated group, but that live in regions not exposed to the policy change. To the best of my knowledge, this is the first paper to show causal evidence that school-based AA policies lead to indirect effects, inducing changes in the school-choice decisions of students that, in principle, were expected to be unaffected by the reform, and to general-equilibrium effects in the form of school closure. This combined evidence shows that school-based AA can lead to unintended effects that are larger and more immediate than anticipated by the economic literature and policy-makers.

Second, I provide clear evidence of the existence of changes in school-choice decisions following school-based AA in a context beyond the US. This gap in the literature is important, considering the increase in socioeconomic AA interventions across the globe and that, in many settings, race-based AA is not legal or politically feasible. Despite some similarities in terms of overall objectives between QL and the Top N-percent Plans, the two AA initiatives are substantially different in terms of policy design and the institutional context in which they have been implemented. While the Top N-percent Plans are localized - as they were implemented at the state level -, QL is a nationwide aggressive quota-type policy that affected all federal institutions, including some of the best universities in the country, in a setting of high inequality and substantial returns to higher education. On the one hand, the institutional context in Brazil provides high incentives for a change in school choice, which might lead to larger policy effects. On the other hand, the substantial inequality of the country creates a highly segregated school system with a large public–private gap, increasing the costs for transitions between the public and the private system.[10] Therefore, it is not clear a priori whether this type of policy would lead to similar effects in settings of such heterogeneous costs and returns. Finally, as with other papers in this literature, my work also contributes more generally to the study of unintended effects of educational policies in which individuals are graded relative to a certain group of peers.[11]

2. Institutional background

2.1. Brazilian education system

The basic mandatory education in Brazil is comprised of 12 years. Grades 1 to 5 correspond to primary education, grades 6 to 9 to lower secondary, and grades 10 to 12 to upper secondary.[12] Students start first grade at age 6 and should finish high school at age 17 or 18, before entering university. Although the government offers universal access to all grades of basic education, the public system coexists with a large number of private schools. According to the Census of Basic Education of 2011, there were 151,544 active schools in Brazil, 82% offering only primary (grades 1–5) and lower-secondary education (grades 6–9), and the remaining 18% offering also, or exclusively, upper secondary levels (grades 10–12). Around 85% of those are public schools. Similarly, 86% of enrollments in basic education are concentrated in the public sector, with little variation across education levels (see Table A.1 for more details).

The public and private systems are also very different in other key dimensions. Private schools are, on average, of better quality than their public counterparts. From the top 100 high schools in Brazil, according to the National Standardized Exam of 2011 (ENEM)[13], 93 are private. Moreover, from the 10,077 schools evaluated, the 4,799 private schools perform considerably better, as seen in Fig. 1 Panel A. Additionally, private schools' socioeconomic level is substantially higher. For example, among the top decile of socioeconomic status, only 28% of schools are public, even if they represent 85% of all schools in the country.

In contrast, tertiary education presents an opposing scenario, with public institutions performing, on average, better than their private counterparts, as seen in Fig. 1 Panel B. According to the Census of Higher Education, in 2012, right before the implementation of the Quota Law, the Brazilian higher education system was comprised of 2,416 institutions, 284 public and 2,132 private. The public system was a mix of federal (103), state (116), and municipal (65) institutions, corresponding, respectively, to 17, 9, and 1 percent of the total undergraduate enrollment of around 5.9 million students. Federal and state institutions are, by law, free of any charge. Private institutions, in contrast, charge tuition fees that may vary substantially, but were, on average, equal to 898 Brazilian Reais per month in 2017 (or 95.8% of the minimum wage)[14]. Public tertiary institutions (especially federal) are widely recognized in the country for their average high quality. For instance, the federal institutions scored, on average, 3.2 on a scale of 1 to 5 of

[9] Empirically, they show that the Top 10-percent policy in Texas affects the high school system in the form of a decrease in ethnic segregation in 12th grade compared to 9th grade. Finally, they find that students who have moved schools in the 12th grade were more likely to choose their new school strategically than students in lower grades.

[10] Brazil is the 11th most unequal country according to its Gini Index (World Development Indicators, 2019). One of the main reasons behind the perpetuation of its long-term inequality is precisely the education system. In contrast with most developed countries, Brazilian educational expenditures are concentrated in tertiary, not on basic education. Brazil spends 3.8 thousand USD annually per student in primary education, while the OECD average is 8.7. In contrast, Brazil spends 11.7 thousand USD per student in tertiary education, similar to European countries such as Italy (11.5) and Portugal (11.8). The OECD average is 16.1, due to countries with substantially higher average spending, such as the US (29.3) and the UK (24.5) (OECD (2017), Table B1.1). Nevertheless, access to higher education (especially public) is extremely unequal and returns are substantial. According to the Population Census of 2010, the share of college enrollment for individuals aged 18 to 22 is equal to 3.7 percent in the lowest quartile and 34.2 percent in the top quartile of family per capita income. In parallel, the earnings of workers with a tertiary degree are 2.5 times higher than the ones of workers with upper secondary education. The OECD average is 1.56 (OECD, 2017). The adoption of QL aims at mitigating this issue.

[11] Calsamiglia and Loviglio (2019) show that having better peers leads to worse grades in internal evaluations. When these grades are important determinants of future outcomes, movements to schools with relatively worse peers might occur. Since internal grades are used solely or as part of the admissions to determine access to subsequent education levels in many institutional contexts, such unintended consequences on school-choice decisions should also be considered when evaluating the efficacy of these policies.

[12] From 2013, pre-primary education for children aged 4 and 5 also became mandatory.

[13] This is a national standardized exam handled by the National Institute of Educational Studies and Research (INEP) and available once a year across the whole country. The exam is taken by high school seniors and consists of 180 multiple-choice questions in four areas—Mathematics, Humanities, Sciences, and Languages—and a written essay.

[14] According to data from *Mapa do Ensino Superior no Brasil 2017*, from the *Sindicato das Mantenedoras de Ensino Superior*. Information is available at: https://educacao.uol.com.br/noticias/2017/08/28/mensalidade-de-curso-superior-no-brasil-custa-em-media-r-898-diz-estudo.htm.

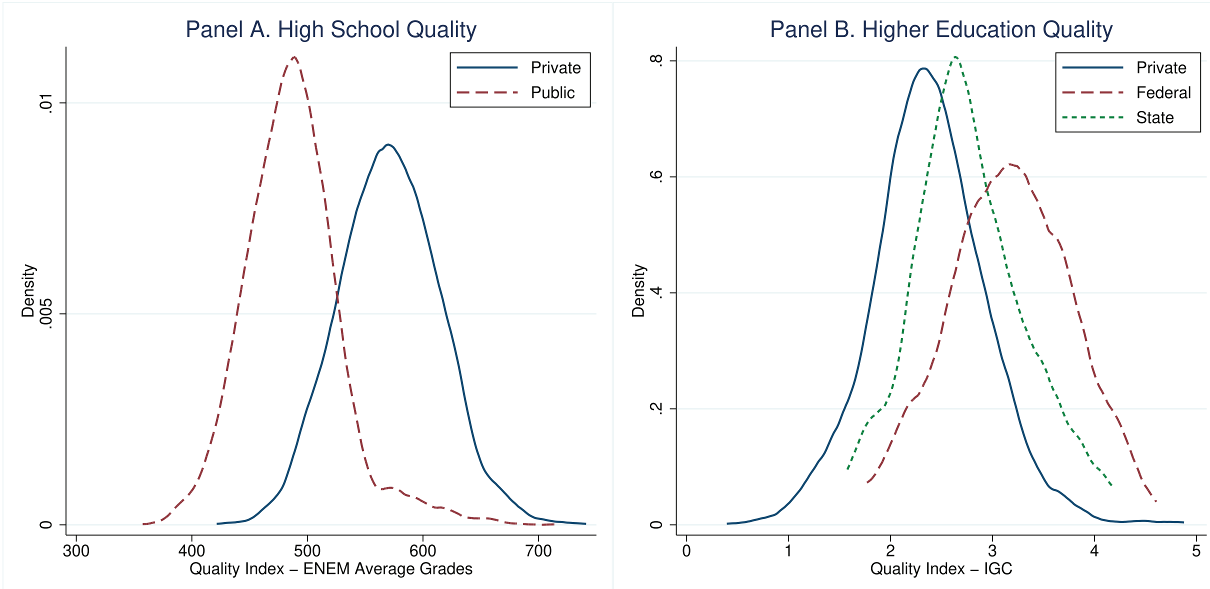

Fig. 1. Comparison between private and public systems in basic and higher education. Notes: Panel A shows the distribution of private and public high schools in Brazil according to performance in the National High School Exam (*ENEM Escola 2011*). Panel B shows the distribution of private, federal, and state higher education institutions according to performance in the *Índice Geral de Cursos 2012* (IGC), a quality index elaborated by the INEP based on performance evaluations of undergraduate and graduate programs.

the *Índice Geral de Cursos 2012* (IGC)[15], a quality index elaborated by the National Institute of Educational Studies and Research (INEP) based on performance evaluations of undergraduate and graduate programs. State institutions scored 2.8, and private institutions 2.4. Furthermore, among the universities only, 18 are federal, 5 are state and only 2 are private institutions among the top 25 in the country. On an alternative ranking - *Ranking Universitário Folha (RUF) 2012*[16], elaborated by *Folha de São Paulo*, the newspaper with the highest circulation in Brazil - a similar pattern appears. Among the top 25 universities, 17 are federal, 6 are state, and 2 are private. Therefore, due to their high quality and free tuition, public tertiary institutions usually attract a large number of applicants.

2.2. The quota law in higher education

Access to public undergraduate education in Brazil is highly competitive. For example, according to the Centralized Admission System of 2016 (SISU 2016), 2,664,211 students applied for 242,984 slots in federal institutions, a rate of 11 students per vacancy. Therefore, only students with high grades can successfully obtain a spot in these competitive colleges.[17] As a consequence, access to public higher education in Brazil has historically been unequal. For instance, only 15% of seniors from public high schools and 11% of non-white seniors from public high schools that took the ENEM exam in 2010 progressed to public higher education. For private-school students, the rate of progression was 26%.[18] Moreover, around 87% of high-school seniors attended a public school in 2010, while only 54% of incoming students in public higher education were graduates from public high schools. Similarly, around 46% of high-school seniors were non-white and attended a public school, while only 24% of first-year students in public universities belonged to the same group (Mello, 2022a).

To improve equality in access to the federal tertiary education system, the government of Brazil approved Law 12.711 on August 29, 2012, the so-called "Quota Law" (QL). It established that 50% of all vacancies in each major at each federal institution must be reserved for students that attended all three years of secondary education in a public school. Moreover, there are sub-quotas, within these 50%, destined for racial and economic minorities. Figure A.1 shows an example of how QL was implemented in the state of Bahia. Take, for example, a major that offers 100 vacancies at the Federal University of Bahia. Of these spots, 50 are reserved for students that attended all high school in a public school. Within these 50, 25 are reserved for public-school students that belong to a family with a per capita income of less than 1.5 minimum wage. Also, within these 50, 38 are reserved for black, mixed, or indigenous

[15] The IGC is an index of quality elaborated by INEP that evaluates all higher education institutions in Brazil. The index aggregates information on (i) the average performance of college seniors on a national standardized exam (the so-called ENADE) in the previous three years weighted by the number of enrollments in each major evaluated; (ii) the average grade of post-graduate programs in the previous three years weighted by the number of enrollments (according to data from CAPES); (iii) the distribution of students across the different education levels.

[16] The RUF is an annual assessment of Brazilian higher education carried out by the newspaper Folha de São Paulo since 2012. The Brazilian universities, public and private, are classified based on five indicators: research, internationalization, innovation, teaching, and labor market value. The research component is based on the number and quality of publications and citations; the internationalization component on international citations and coauthorships; the innovation component on the number of patents and partnerships; the teaching component on the education level of professors, the performance of students and the opinion of higher education professors surveyed in a poll; and the labor market component on the opinion of employers surveyed on a poll regarding their hiring preferences.

[17] Admissions processes to federal higher education institutions in Brazil are based, exclusively, on grades in one admission exam. Today, most federal institutions offer vacancies based on grades in the National Standardized Exam (ENEM), mostly through a centralized admission system (SISU). Some of these institutions also offer part of their vacancies based on an institution-specific exam (Vestibular). In any case, admissions are decided exclusively based on entrance exams, not taking into account high school performance. See Appendix C.3 for more details on the SISU system and higher education admissions in Brazil.

[18] The rate of progression to any higher education institution is 22% for public-school students, 18% for non-white public-school students, and 50% for private-school students.

students (non-white). The fraction of vacancies reserved for non-white students vary by state according to its share in the last Population Census.

By reserving 50 percent of vacancies at highly competitive institutions for public-school students, QL likely increases incentives for public school attendance. Moreover, by establishing one sole national level of quotas for all federal institutions, it impacted each institution differently, depending on their pre-reform levels of quota adoption. For example, while some institutions already reserved as much as 50 percent of their vacancies for public-school students (e.g. Federal University of Juiz de Fora - UFJF), others had no quotas at all (e.g. Federal University of Pernambuco - UFPE). Fig. 2 Panel A shows the level of quotas for public-school students adopted by each federal higher education institution in Brazil in the admission process of early 2012, before the national reform, confirming the high degree of heterogeneity across institutions. Additionally, QL possibly creates a heterogeneous impact by majors within the same institution, depending on the actual share of public-school students enrolled in each program before the adoption. For instance, Fig. 2 Panel B shows the distribution of public-school students by major in federal institutions in 2012, before QL, and in 2016, after the complete implementation of the policy. In 2012, a large portion of programs already had more than 50% of public-school students. In spite of that, the distribution remarkably shifted to the right in 2016, largely as a result of the implementation of the reform. The adoption of QL creates a quasi-experiment for testing how affirmative action in undergraduate education impacts students' choices.

3. Data sources and construction

3.1. Census of basic education

This paper uses two main datasets. The first one is the Brazilian Census of Basic Education (CBE) from years 2007 to 2017.[19] This is administrative individual data from the universe of students enrolled in primary and secondary schools in Brazil. It is collected yearly by the National Institute of Educational Studies and Research (INEP) of the Brazilian Ministry of Education. The individual module of the CBE contains basic demographic characteristics of students (e.g. gender, age, ethnicity) and unique individual and school identifiers. This allows for the construction of a panel dataset both at the individual and the school level across time.

To construct the main sample of analysis, I select all individuals enrolled in the final year of primary education - 9th grade - in year t from 2007 to 2016. Then, using their unique individual identifier, I link the students' information of year t with the CBE of the following year $t + 1$.[20] This allows the identification of students that advanced to the first year of upper secondary education, 10th grade. Moreover, I can identify whether individuals changed schools and, more importantly, whether they moved from their original education system (from private to public or vice versa). The transition from private to public schools between 9th and 10th grade is exactly the one that needs to occur for AA eligibility. It is also one of the most important transitions in basic education in Brazil, as it marks the end of lower secondary and the beginning of high school. This is why I focus on students from 9th grade in my benchmark analysis. Later, in Section 6, I extend the sample and analyze transitions of cohorts from other grade levels to study indirect mechanisms.

Around 83% of individuals that attend public higher education in Brazil do so in the same municipality where they resided around age 14 (Census of 2010).[21] Moreover, around 87% of individuals that enroll in federal higher education upon the completion of high school in a microregion with a headquarters of a federal university do so in this same microregion (see Table B1.2 for detailed statistics). Therefore, I restrict my benchmark sample to students that reside in one of the 50 microregions where there is a headquarters of a federal university.[22] These locations contain 45% of the country's total population, including all the state capitals and the Federal District (see Fig. 3 for their location), and 62% of the population of private-school students in 9th grade. By selecting this group of students, I focus on individuals that are more likely to attend federal higher education and, thus, have more motive and opportunity to respond to QL.[23]

Table A.2 plots some descriptive statistics for the students of 9th grade from my benchmark sample. The share of students in the private system increased from 15.6 to 20.9%, while movements from the private to the public system between the 9th and 10th grades increased from 14.6% in 2007 to 21.1% in 2016. Movements from public to private schools remained relatively stable at a much lower level, varying from 1.9 to 1.5%. Students from the private system are less likely to be non-white and female, more likely to live in urban areas, and are on average younger than students from the public system. I also use the CBE to construct a school-level panel dataset from 2008 to 2017 to study the potential effects of QL on the school system. More details on this sample are presented in Section 6.

3.2. Treatment data

The second main dataset used in this study is the Affirmative Action Quotas Data (Mello, 2022b). It contains detailed information on the number of vacancies destined for each category of affirmative action quotas at each public higher education institution in Brazil from 2010 to 2015. I constructed this dataset by collecting information from public documents on admission processes (*Editais*) and by directly contacting institutions through the Electronic System of Information of the Federal Government (e-SIC). The data is complemented with the Census of Higher Education of 2012, in which I gather information on the type, municipality, and state of the institution.

I keep only information on AA adoption in the admission processes for incoming students in 2012, the last one before the implementation of the Quota Law. Since QL only directly affects federal institutions, I drop all state universities from the sample. I compute variable $Q_{u,2012}$, which measures the percentage of quotas at institution u of microregion m destined for students that attended all secondary education in a public school in 2012. I then construct variable $Treat_{u,m} = 2(0.5 - Q_{u,2012})$, which measures how institution u is treated by the QL reform. If the institution had no quotas in 2012 ($Q_{u,2012} = 0$), $Treat_{u,m} = 1$. On the other hand, if it already had 50 percent of reserved vacancies before the implementation of the law, then $Treat_{u,m} = 0$.[24] If $0 < Q_{u,2012} < 0.5, Treat_{u,m}$ will

[19] Year 2007 was the first in which individual-level data was collected by the government. Therefore, it is the first year I use in my analysis.

[20] For instance, for each student that appears in 9th grade in year $t = 2007$, I use their unique individual *id* to find this same student in the Census of year $t + 1 = 2008$. I can then track students between two consecutive editions of the CBE, and observe if the student advanced to the 10th grade and whether he or she moved schools in this grade transition.

[21] This share is 85% for individuals attending private higher education.

[22] In Appendix B.1, I provide detailed descriptive statistics on the flows of students from high school to federal higher education to support my choice of sample.

[23] In Appendix B.2, I conduct a robustness exercise including the universe of students from all microregions and using an alternative measure of treatment that accounts for indirect exposure to QL. Results are very similar to the benchmark results.

[24] Two institutions, *IF Sudeste MG* and *IF Farroupilha*, had more than 50% of quotas for public-school students and also received $Treat_{u,m} = 0$.

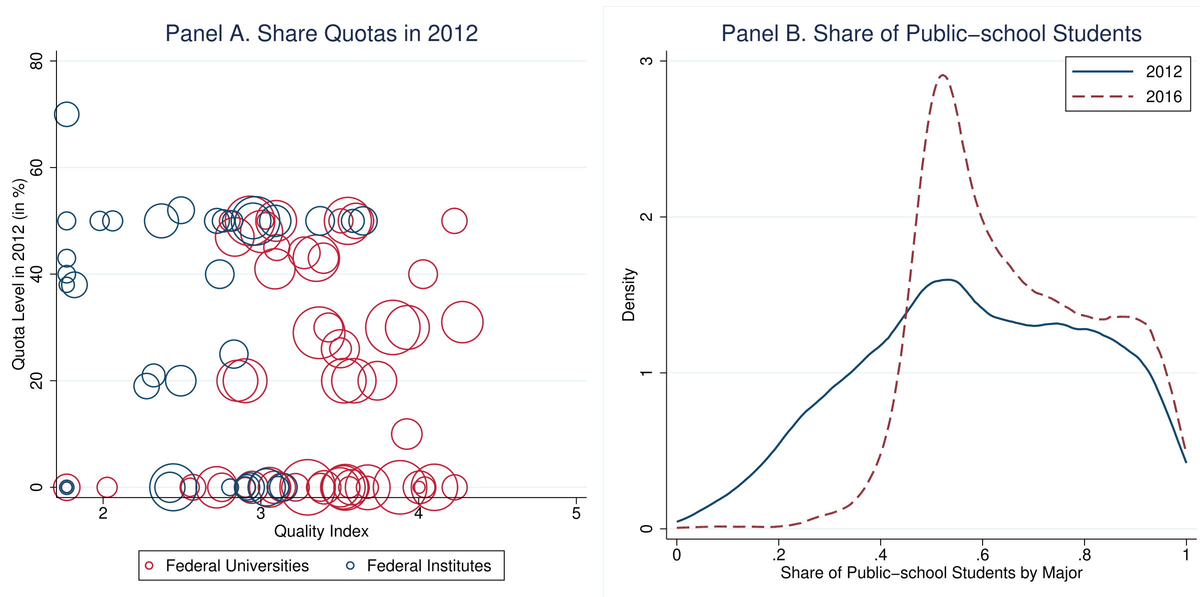

Fig. 2. Descriptive statistics of the quota law. **Notes:** Panel A plots the share of quotas reserved for public-school students by institution in 2012, before the implementation of the Quota Law. Each circle represents an institution, and the size of the circle represents the number of incoming students in 2012. Panel B plots the share of public-school students by major-institution in the cohort of incoming students in the federal higher education system in Brazil, in 2012 and 2016.

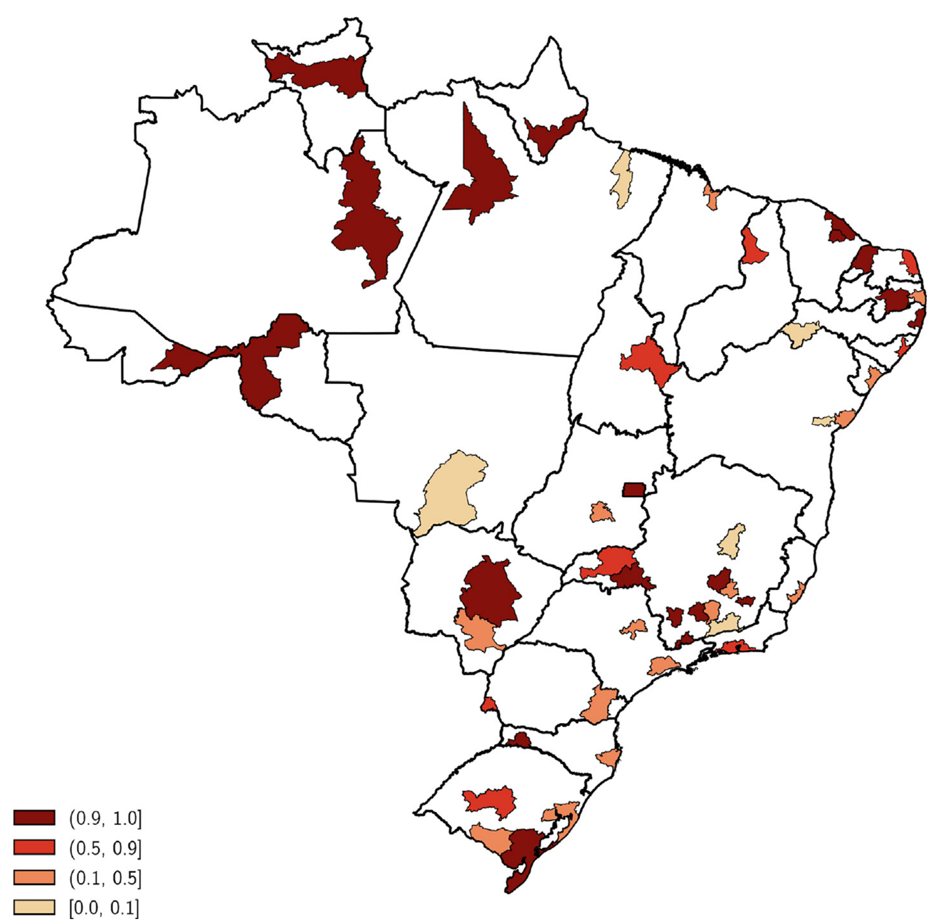

Fig. 3. Distribution of treatment variable. **Notes:** This map shows the location of the 50 microregions in Brazil with a headquarters of a federal university. The microregions are colored according to their level of treatment $Treat_m$, which can vary from zero (non-treated) to 1 (fully-treated). Regions in white do not have a headquarters of a federal university and are not present in the main sample.

assume a value between zero and one. Intuitively, $Treat_{u,m}$ measures how each federal institution in Brazil was exposed to QL, based on their pre-reform level of quota adoption. Although different institutions had different levels of quotas $Q_{u,2012}$ due to specific (probably non-random) university policy, the adoption of the national law forces all institutions to reach the same level of 50%. This adjustment creates a specific pattern of variation across institutions and microregions that is unlikely to be correlated with other time-varying confounders, providing an exogenous variation for the causal identification of policy effects.

Although I directly observe how QL affects all Brazilian tertiary federal institutions that offered vacancies in 2012, I do not know how students in middle or high school respond to changes happening at the higher education level. To obtain a proxy for that, I aggregate all values n of $Treat_{u,m}$ of the same locality to create a measure of treatment exposure at the microregion level:[25]

$$Treat_m = \frac{\sum_{u=1}^{n} Weight_{u,m} \times Treat_{u,m}}{\sum_{u=1}^{n} Weight_{u,m}}, \tag{1}$$

where $Weight_{u,m}$ is the size of each institution, measured by the number of new vacancies offered by the institution in 2012. Note that if the microregion has only one federal university, $Treat_m = Treat_{u,m}$.

Fig. 3 shows the distribution of variable $Treat_m$ and the location of the 50 microregions with a headquarters of a federal university. First, note that they are spread throughout different regions of Brazil. As determined by federal law, all states need to have at least one federal university. Second, note that they are relatively far from each other, minimizing concerns about spillover effects. Third, note that variable $Treat_m$ is sufficiently distributed between values zero and one. Although 21 out of 50 microregions are highly treated ($Treat_m > 0.9$), 29 microregions have values of $Treat_m$ that vary from 0 to 0.9. It is this variation that allows for the causal identification of the effect of QL.

3.3. Other datasets

I use the Population Census of 2010 to obtain a series of covariates at the microregion level. Data on local population and public spending in education are obtained from IPEAdata, while information on local GDP is obtained from the Brazilian Institute of Geography and Statistics (IBGE). These variables are displayed in Table 1 Panel A. The pre-reform information on school socioeconomic index (NSE) comes from Gonzaga et al. (2014) and is computed based on student-level microdata from 2003 to 2011. Finally, I obtain the pre-reform information on school performance (ENEM Escola 2011) and the geographical coordinates from the schools in Brazil from INEP.

3.4. Comparison of microregions

I divide the 50 microregions from my benchmark analysis into two groups, the "Low Treated" contains the 25 regions below the median of variable $Treat_m$, and the "High Treated" the 25 microregions above. A comparison of a series of pre-reform characteristics at the microregion level (Table 1 Panel A) shows that the highly treated localities are on average smaller in terms of population and poorer in terms of GDP per capita. They also have a lower share of college residents, a higher share of rural and non-white residents, a lower share of formal sector workers, and a larger share of workers in the primary sector.

Panel B shows how the students from 9th grade in the benchmark sample of the two groups of microregions compare. The highly treated localities have a greater share of individuals in the North (14.8%) and the Northeast (30.1%) regions compared to the low treated group (respectively 4.4% and 16.5%). These are the regions in Brazil with the highest fraction of people of color, and the reason why nearly 65% of students in the "High Treated" group are non-white, compared to only 46.8% of students in the "Low Treated". Finally, 77.8% of students attend public schools, and 17.4% move from private to public schools between 9th and 10th grade in the highly treated group, compared to 82.8% and 14.1% in the low-treated localities.

The main takeaway from Table 1 is that microregions in the two groups are considerably different regarding their economic structure and demographic composition. These differences in levels of observable characteristics are not necessarily a problem, as long as *changes* in the share of students moving from private to public schools in low and high-treated localities follow parallel trends. I show below that this is likely the case.

4. Empirical strategy

Main Specification. I use a dynamic differences-in-differences design to study how the QL reform affects school choice. My main empirical model is:

$$Y_{ismt} = \sum_{y=2007}^{2010} \beta_y \mathbb{1}_{\{y=t\}} \mathbb{Treat}_m + \sum_{y=2012}^{2016} \beta_y \mathbb{1}_{\{y=t\}} \mathbb{Treat}_m + \gamma \mathbb{X}_{ismt} + \sum_{z \in Z_m} \delta_z (z \times \alpha_t) + \alpha_{sm} + \alpha_{rt} + \varepsilon_{ismt} \tag{2}$$

where Y_{ismt} is the outcome of student i, microregion m, school s that is enrolled in 9th grade in year t. Eq. (2) is estimated separately for students attending a private or a public school. For students from a private (public) school, Y_{ismt} takes the value 1 if he or she moved to a public (private) school for 10th grade, and value zero if he or she advanced to 10th grade but stayed in a private (public) school. The treatment variable $Treat_m$ defines how individual i was exposed to the QL reform, depending on the microregion where he or she attended 9th grade, as explained in the previous section. This variable is interacted with a dummy $\mathbb{1}_{\{y=t\}}$ that takes the value 1 if y is equal to the year of cohort t or zero otherwise. The reform was announced in August of 2012. Therefore, the first cohort expected to be influenced is precisely the one finishing 9th grade in that year.[26] In this specification, the interactions between $Treat_m$ and dummies for years 2007 to 2010 serve as a test of parallel trends in the pre-periods, the interaction $Treat_m \times \mathbb{1}_{2011}$ is omitted from the regression (2011 is the baseline year) and the interactions between $Treat_m$ and dummies for years 2012 to 2016 estimate the effects of QL in each of the post-periods.

The vector X_{ismt} controls for individual characteristics, such as gender, age, ethnicity, and urban status. The term $\sum_{z \in Z_m} \delta_z (z \times \alpha_t)$ controls for interactions between year fixed effects and a set of pre-reform microregion characteristics Z_m. In the main specification, Z_m includes the log of population and log of GDP per capita, while in the robustness exercise presented in Table C1.1 vector Z_m includes all microregion characteristics from Table 1: share of residents with a college degree; shares of rural, non-white and female residents; share of workers in service and manufacturing (primary sector omitted); share of formal sector workers, share

[25] Remember that around 87% of the students in my benchmark sample that attend federal higher education do so in the same microregion where they lived while in 9th grade.

[26] Note that to benefit from QL, students need to stay the full 3 years of high school in a public institution. Therefore, students already enrolled in secondary education cannot benefit from the Quota Law.

Table 1
Comparison of low and high treated microregions.

	Low Treated		High Treated	
	Mean	SD	Mean	SD
Panel A: Microregion Characteristics				
$Treat_m$	0.27	0.21	0.94	0.10
Population	1,897,157.6	3,842,779.0	1,559,749.3	2,522,552.5
GDP Per Capita	31,831.7	15,454.2	27,842.0	15,818.1
Share of residents with college	12.1	3.5	10.8	3.7
Share of rural residents	11.4	10.3	14.6	11.8
Share of non-white residents	51.0	22.1	57.0	18.4
Share of female residents	51.7	1.0	51.1	1.2
Share of manufacturing workers	9.9	3.9	9.6	4.6
Share of service workers	78.6	9.0	75.6	12.3
Share of primary sector workers	11.6	10.2	14.8	11.4
Share of formal workers	56.8	7.3	53.3	9.8
Share of government workers	9.6	2.2	11.1	4.7
Spending in Educ Per Capita	606.7	143.5	641.7	281.5
Number of Microregions	25		25	
Panel B: Student-level Characteristics				
9th graders in Public Schools (%)	82.8	37.7	77.8	41.6
Movement from Private to Public (%)	14.1	34.8	17.4	37.9
Movement from Public to Private (%)	2.1	14.4	2.4	15.2
Share of non-white students	46.8	49.9	64.9	47.7
Share of female students	50.8	50.0	52.8	49.9
Share of urban students	94.9	21.9	91.4	28.0
Average age	14.7	1.0	15.0	1.1
North (%)	4.4	20.6	14.8	35.5
Northeast (%)	16.5	37.1	30.1	45.9
Southeast (%)	55.9	49.6	42.4	49.4
South (%)	15.8	36.5	2.8	16.6
Centerwest (%)	7.4	26.1	9.8	29.8
Number of 9th graders	763,950		564,749	

Notes: This table shows descriptive statistics of the baseline sample in the pre-period separately by treatment status. Low (High) treated microregions are the ones with treatment below (above) median. Panel A displays characteristics at the microregion level in 2010. GDP per capita and spending in education per capita are displayed in constant prices of 2018. Share of residents with college, and shares of rural, non-white and female residents correspond to the share of residents from 16 to 65 years old that belong to each demographic group. Shares of manufacturing, services, and primary sector workers correspond to the share of workers aged 16 to 65 employed in each of the broad sectors and add up to one. Shares of formal and government workers refer to workers aged 16–65 employed in the formal sector and the public administration. All shares come from the 2010 Population Census. Panel B displays characteristics from the baseline individual-level data of 9th-grade students for 2011.

of workers in public administration, and log of education spending per capita. These interactions aim to control for any time-varying shocks that affect microregions with high and low treatment status differently and that could be correlated with the main outcome of interest.

Finally, I include school fixed effects α_{sm} (which also absorb microregion fixed effects) and year by broad region (North, Northeast, Centerwest, Southeast and South) fixed effects α_{rt}. Standard errors are clustered at the microregion level.

Identification. The main identifying assumption for the causal interpretation of parameters β_{2012} to β_{2016} is that dynamics in the outcome variable for treated and control units would have been similar in absence of the treatment. The presence of school-microregion fixed effects absorbs all unobservable time-invariant characteristics at school or microregion levels that might be correlated with the outcome. However, the existence of time-varying unobservable characteristics that are correlated with the outcome could still be a threat to causal identification. Indeed, since high and low-treated microregions are considerably different in observable dimensions as shown in Table 1, it is possible that they also suffer from different time-varying shocks that could impact the likelihood of students switching from private to public schools.

To minimize this concern, I include interactions of $Treat_m$ with dummies for the pre-periods 2007 to 2010. If pre-trends are parallel, I expect to find coefficients β_{2007} to β_{2010} to be close to zero and not significant. This would provide suggestive evidence that trends between treated and control microregions, in absence of treatment, would likely have also been parallel between 2011 and post-reform years. Although reassuring, this experiment does not guarantee that starting in 2012, these trends would remain parallel. A particular concern is the economic crisis that affected Brazil in the last decade. If microregions in the highly treated group are also more affected by the recession, my post-reform estimates would be capturing the impact of the economic crisis on the transitions from private to public schools, and not the effect of the introduction of QL.

Although I cannot completely rule out this possibility, I believe it is unlikely that my results are driven by the recession. First, my estimates control for broad region by year fixed effects. This means that identification comes from variation in exposure to the QL reform *within* the five broad regions of Brazil: North, Northeast, Centerwest, Southeast, and South. Microregions within these broad areas are much more similar in terms of observable characteristics, and the economic crisis is more likely to have had more homogeneous impacts within these broad areas. Second, the term $\sum_{z \in Z_m} \delta_z (z \times \alpha_t)$ attempts to control for any time-varying shocks that affect microregions with high and low treatment status differently. The fact that results (presented in Table C1.1) remain extremely robust even after controlling for the interaction between year fixed effects and the full set of pre-reform characteristics contained in Table 1 is reassuring. Finally, while QL is announced in 2012, the recession starts in Brazil in 2014. Since we already observe families responding to the QL reform in 2012, it is very unlikely that my results capture predominantly the spurious correlation between the QL reform and the economic crisis. In Appendix C.1, I present further evidence supporting the internal validity of my empirical strategy related to the recession of 2014–2016.

Presentation of Results. Results are presented in the form of event-study graphs following Eq. (2) and through tables following the more parsimonious specification (3) below. The dummy $Before_t$

takes the value 1 for pre-periods 2007 to 2010, and the value zero otherwise, while the dummy $Post_t$ takes the value 1 for post-periods 2012 to 2016 and the value zero otherwise. The interaction $Treat_m \times \mathbb{1}_{2011}$ is still omitted from the regression (2011 is the baseline year).

$$Y_{ismt} = \gamma_1 Treat_m \times Before_t + \gamma_2 Treat_m \times Post_t + \gamma X_{ismt} + \sum_{z \in Z_m} \delta_z (z \times \alpha_t) + \alpha_{sm} + \alpha_{rt} + \varepsilon_{ismt}. \quad (3)$$

Note that, following this specification, γ_2 represents the average of the treatment effect of QL on all post-periods jointly, in relation to the baseline year 2011. The estimate γ_1, instead, represents the average of the treatment effect of QL on the pre-periods 2007 to 2010 jointly, also relative to the baseline year 2011.

5. High school choice

5.1. Main results

Fig. 4 shows the event-study graph from an OLS estimation of Eq. (2). According to Panel A, full adoption of QL increases movements of 9th graders from private to public schools by 2.9 p.p. in 2012, 4.6 p.p. in 2013, 6.5 p.p. in 2014, 6.5 p.p. in 2015, and 3.3 p.p. in 2016. All coefficients are statistically significant, except the one for the cohort 2016. Moreover, the coefficients for the years 2007 to 2010 are close to zero and not significant, corroborating the identifying assumption of parallel trends between treated and control groups.

Since the reform was approved on August 29 2012, the cohort of 9th graders in 2012 had less time to respond to changes, if compared to later cohorts. The academic year in Brazil goes from February to December and children need to enroll in public schools around October of the preceding year. Therefore, the 2012 cohort had approximately two months (September and October) to respond to the policy change. Later cohorts, on the other hand, had over a year to adjust. This could partially explain the difference in magnitude between the estimates for the year 2012 and later years. The full effect of the reform is observed three to four years after adoption, in the cohorts of 2014 and 2015. Finally, the effect seems to lessen in 2016. It is possible that, as time passes, part of the movements between private to public schools occur in earlier grades, reducing the magnitude of the movement from 9th to 10th grade in the cohort of 2016. I return to this point in Section 6.

In turn, one could expect that due to the increase in incentives for public school attendance, movements from the public to the private system would decrease. Panel B shows that this does not occur. This is possibly because these movements were already too low before QL. In 2011, only 2.2 percent of 9th graders that attended a public school moved to a private school in 10th grade.

Table 2 shows the results of the OLS estimation of different specifications of Eq. (3). Column (1) includes only time and microregion fixed effects. Column (2) adds school fixed effects. Column (3) includes individual-level controls for gender, age, ethnicity and urban status. Finally, column (4) adds an interaction between pre-reform levels of log population and log GDP per capita interacted with year fixed effects. Results are extremely robust across all specifications. According to column (4), the QL reform increased movements from private to public schools by 4.8 percentage points considering post-periods 2012 to 2016 jointly (interaction $Treat_m \times Post_t$), relative to the base year 2011. This represents an increase of 31% compared to the movement rate of 15.7% in 2011. The coefficient of pre-periods 2007 to 2010 jointly (interaction $Treat_m \times Before_t$), also relative to the base year 2011, is close to zero and not significant.

5.2. Heterogeneity

In addition to the quotas destined for public-school students, the QL reform reserved sub-quotas for individuals self-declared as black, mixed, or indigenous (non-white). Therefore, this demographic group has higher incentives for a change in the choice of school. This pattern is confirmed in Table 3 Columns (2) and (3). Full adoption of QL increases moves from private to public schools by 7.7 p.p. for non-whites, compared to 2.9 p.p. for whites. Estimates are statistically different at the 1% level. Pre-trends are close to zero and not significant. Columns (4) and (5) compare results by gender. Full adoption of QL increases moves from private to public school by 5.2 p.p. for females, compared to 4.3 p.p. for males, although estimates are not statistically different at the 10% level.[27] Coefficients on the pre-trends are also close to zero and not significant for both groups. The event-study graphs for the four groups are shown in Figure A.2 in the Appendix.

Table 4 shows heterogeneous results with respect to students' schools of origin. Panel A shows that movements from private to public schools come essentially from students attending private schools of low socioeconomic status, as measured by the NSE (*Nível Socioecômico das Escolas*), an index computed using pre-reform data from 2003 to 2011. The effect of QL on post-periods 2012–2016 jointly is 5.9 p.p. for students attending private schools below the median of socioeconomic status (column 2) and 1 p.p. for students attending private schools above the median (column 1). Coefficients on the pre-trends are close to zero and not significant in both groups. In Panel B, I divide schools by the average predicted probability of federal higher education attendance based on data from the pre-reform cohort of 9th graders in 2008. [28] Results show that movements from private to public schools induced by QL are 1.6 p.p. in schools with a high probability of federal higher education attendance ($Prob > 5\%$) and 9.9 for students coming from schools with a low probability ($Prob \leqslant 5\%$). Similarly, Panel C Column (2) shows that movements are much higher (6.5 p.p.) among students coming from schools below the median of the distribution of the National Standardized Exam (*ENEM Escola 2011*) if compared to students attending schools above-median (3.0 p.p.). Coefficients on the pre-trends for all groups are close to zero in magnitude and not significant. Taken together, results from Table 4 Panels A-C suggest that students pushed out of the private system by QL are individuals interested in public tertiary education that, most likely, benefit less from staying in a private school (since they attend low-performing private institutions). Panel D shows results separately by the availability of public schools within 1 km of the private school where the student attends 9th grade. Movements from private to public schools increase by 5.4 p.p. among the group with two or more public schools within a 1 km radius, while it increases only by 3.1 p.p. for students in private schools with one or less public schools within the same distance. This suggests that higher costs of moving also play a role in the school choice decision.

Table A.3 in the Appendix shows two additional heterogeneity results with respect to microregion characteristics. Panel A shows that movements from private to public schools come almost

[27] In theory, heterogeneous effects by gender could be rationalized by the fact that females are more likely to enroll in and complete high school, and, later, enroll in higher education. It could be expected that they are the subgroup most affected by changes in higher education policy.

[28] I use data from the cohort of 2008, which was not affected by QL, and estimate the following regression: $Y_{ism} = \beta_0 + \gamma X_{ism} + \alpha_{sm} + \varepsilon_{ism}$. Variable Y_{ism} is a dummy that takes the value one if the 9th grader of 2008 enrolls in federal higher education upon the conclusion of high school. Vector X_{ism} contains individual characteristics (ethnicity, gender, urban status, age, and disability), and α_{sm} is a vector of school fixed effects. Then, I divide schools into two groups: high predicted probability of future federal higher education attendance ($\hat{\alpha}_{sm} > 5\%$) and low probability ($\hat{\alpha}_{sm} \leqslant 5\%$).

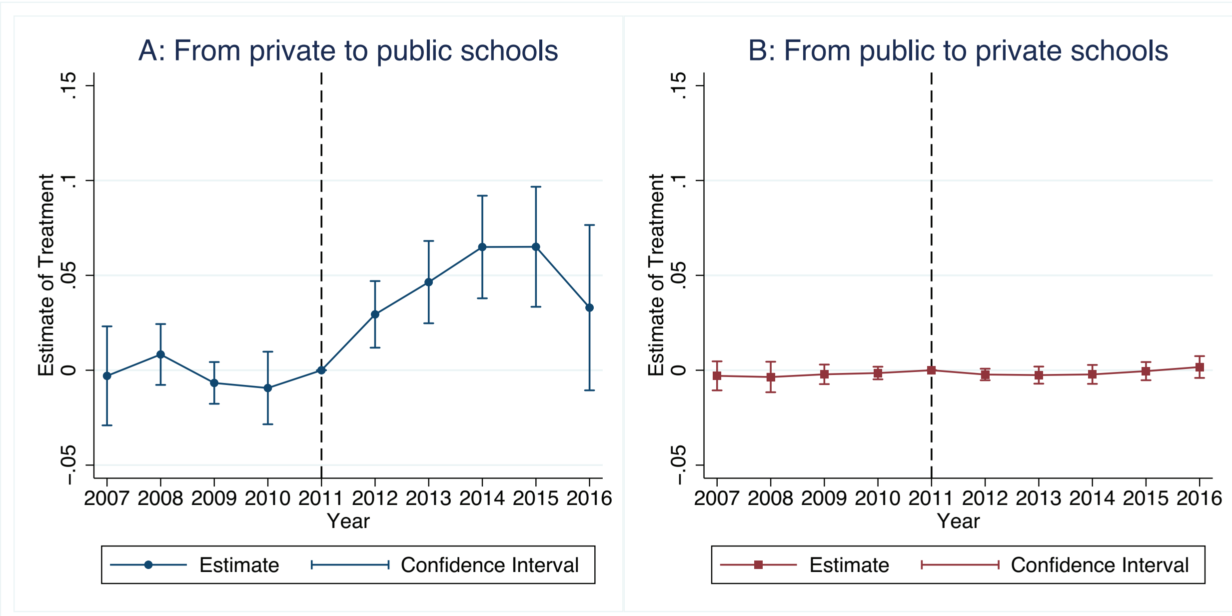

Fig. 4. Effect of QL on school movements from 9th to 10th Grade. **Notes:** This figure plots the estimates of the treatment effects of QL on movements of 9th graders from private (public) to public (private) secondary schools with 95% confidence intervals, following the specification of Eq. (2). The baseline mean of the outcome variable is 0.157 for private-school and 0.022 for public-school students. The year 2011 is the baseline, 2007 to 2010 are pre-periods and 2012 to 2016 are treated periods. Both panels include broad region by year FE, school-microregion FE, pre-reform levels of log population and log GDP per capita interacted with year dummies, and individual controls (gender, age, ethnicity and urban status).

Table 2
Effect of QL on movements from private to public schools.

	(1)	(2)	(3)	(4)
$Treat_m \times Before_t$	−0.009	−0.003	−0.003	−0.003
	(0.010)	(0.007)	(0.007)	(0.007)
$Treat_m \times Post_t$	0.047***	0.048***	0.048***	0.048***
	(0.012)	(0.012)	(0.012)	(0.012)
N	2,117,238	2,117,184	2,117,184	2,117,184
Year x Broad Region FE	Yes	Yes	Yes	Yes
Microregion FE	Yes	Yes	Yes	Yes
School-microregion FE		Yes	Yes	Yes
Individual controls			Yes	Yes
Pop and GDP x Year FE				Yes

Notes: This table shows the estimates of the treatment effects of QL on movements of 9th graders from private to public secondary schools. The baseline mean of the outcome variable is 0.157. $Treat_m \times Before_t$ is an interaction between treatment and a dummy for years 2007 to 2010, the pre-periods. $Treat_m \times Post_t$ is an interaction between treatment and a dummy for years 2012 to 2016, the post-periods. The interaction between treatment and baseline year 2011 is omitted. Individual controls include gender, age, ethnicity, and urban status. Column (4) includes the log of population and log of GDP per capita in 2010 (pre-period) interacted with year fixed effects. Standard errors are shown in parenthesis and are clustered at the microregion level. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 3
Effects of QL on school movements by ethnicity and gender.

	(1)	(2)	(3)	(4)	(5)
	All	Non-white	White	Female	Male
$Treat_m \times Before_t$	−0.003	−0.006	−0.007	−0.002	−0.004
	(0.007)	(0.011)	(0.007)	(0.006)	(0.009)
$Treat_m \times Post_t$	0.048***	0.077***	0.029***	0.052***	0.043***
	(0.012)	(0.016)	(0.009)	(0.012)	(0.013)
N	2,117,184	330,027	683,676	1,086,015	1,030,983
Mean in Baseline	0.157	0.214	0.128	0.154	0.161
Test difference between groups		F(1,49) = 28.03		F(1,49) = 2.39	
		P-value = 0.0000		P-value = 0.1289	

Notes: This table shows the estimates of the treatment effects of QL on movements of 9th graders from private to public secondary schools by ethnicity and gender. The year 2011 is the baseline, 2007 to 2010 are pre-periods (Before) and 2012 to 2016 are treated periods (Post). All columns include broad region by year FE, school-microregion FE, pre-reform levels of log population and log GDP per capita interacted with year dummies, and individual controls (gender, age, ethnicity, and urban status). Standard errors are shown in parenthesis and are clustered at the microregion level. The F-test indicates whether coefficients for groups are statistically different. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 4
Effects of QL on school movements by school of origin characteristics.

	(1)	(2)	(3)	(4)
	A: School Socioeconomic Status		B: School Probability of Higher Ed Attendance	
	Above Median	Below Median	Prob > 5%	Prob ⩽5%
$Treat_m \times Before_t$	0.000	−0.004	−0.008	0.010
	(0.004)	(0.011)	(0.006)	(0.016)
$Treat_m \times Post_t$	0.010***	0.059***	0.016	0.099***
	(0.004)	(0.016)	(0.010)	(0.020)
N	832,652	497,613	921,027	899,100
Mean in Baseline	0.064	0.146	0.097	0.198
	C: School Performance in National Exam		D: Availability of Public School within 1 km	
	Above Median	Below Median	One or less	Two or more
$Treat_m \times Before_t$	−0.005	−0.005	−0.002	−0.001
	(0.006)	(0.011)	(0.009)	(0.008)
$Treat_m \times Post_t$	0.030***	0.065***	0.031**	0.054***
	(0.008)	(0.018)	(0.013)	(0.013)
N	1,055,632	1,061,552	743,735	1,076,562
Mean in Baseline	0.087	0.231	0.128	0.161

Notes: This table shows the estimates of the treatment effects of QL on movements of 9th graders from private school to public secondary schools by (a) Panel A: school socioeconomic status (NSE 2003–2011); (b) Panel B: school probability of future federal higher education attendance; (c) Panel C: school performance at the ENEM National Exam (ENEM Escola 2011) and (d) Panel D: availability of a public high school within 1 km of the private school of 9th grade. $Treat_m \times Before_t$ is an interaction between treatment and a dummy for years 2007 to 2010, the pre-periods. $Treat_m \times Post_t$ is an interaction between treatment and a dummy for years 2012 to 2016, the post-periods. The interaction between treatment and baseline year 2011 is omitted. All columns include broad region by year FE, school-microregion FE, pre-reform levels of log population and log GDP per capita interacted with year dummies, and individual controls (gender, age, ethnicity and urban status). Standard errors are shown in parenthesis and are clustered at the microregion level. * p < 0.1, ** p < 0.05, *** p < 0.01.

entirely from microregions with low high school segregation, i.e., microregions with less unequal (more integrated) high school systems. The costs of moving away from the private system are likely to be lower in these localities. Finally, Table A.3 Panel B shows that movements from private to public schools come disproportionately from microregions with less competitive federal higher education systems, which is consistent with the fact that students in these localities are then more likely to actually benefit from QL and end up in federal higher education.

6. Mechanisms

6.1. Direct effects: Changes in Returns to Investments.

Different mechanisms could explain the results observed in Section 5. According to classical human capital theory (Becker, 1994), non-credited constrained families would invest in private-school tuition to maximize children's future productivity. This increased productivity could come from higher value-added acquired by attending a (higher quality) private school or a (higher quality) federal higher education institution.

However, QL directly changes the probability of federal higher education attendance depending on the type of high school. Table 5 shows that QL decreases by 2.7 p.p. the probability of federal higher education attendance for the cohort of 9th graders attending a private school in 2012, with heterogeneous effects for switchers to the public system (2.8 p.p.) and stayers in the private system (-3.9 p.p.). In contrast, the reform raises by 1.1 p.p. the probability of federal higher education attendance for 9th graders from public schools, an increase of 52% from a baseline average of 2.1%. Therefore, if families value federal higher education attendance, QL decreases the value of investing in private-school tuition.[29]

This would lead individuals that are at the margin (who benefit less from their investments in private schools) to shift to the public system. They would trade part of the value-added obtained from a private school for a higher probability of future federal higher education attendance obtained from public schools. Unfortunately, I cannot directly test whether students that move from private to public schools induced by QL obtain less value-added from high school after moving, since I do not observe individual performance. However, below I provide some suggestive evidence in favor of this hypothesis.

To do so, I match each student in 9th grade with a pre-reform measure of socioeconomic status and performance of the school he or she decided to attend in 10th grade (destination school). I then estimate the baseline Eq. (2) using as an outcome these measures for the destination school of each individual. Fig. 5 shows that cohorts of 9th graders impacted by QL end up attending schools in 10th grade of lower socioeconomic status (Panel A) and of lower performance (Panel B). This provides suggestive evidence that students that move from private to public schools do not only move to public schools that are similar to the ones they would have attended in absence of QL. Instead, treated students trade down and move to poorer and lower-performing schools in exchange for a higher probability of future federal higher education attendance.

Fig. 5 shows the reduced-form effect of QL on the average student of 9th grade from a private school. Yet, the effect of the reform on the group of *compliers*, i.e., students that actually move from the private to the public system induced by QL is of particular interest in this case. To compute this effect, I estimate the following equation:

$$DestinationSchoolOutcome_{ismt} = \beta MovePublic_{ismt} + \gamma X_{ismt} + \sum_{z \in Z_m} \delta_z (z \times \alpha_t) + \alpha_{sm} + \alpha_{rt} + \varepsilon_{ismt}, \quad (4)$$

where $DestinationSchoolOutcome_{ismt}$ is the pre-reform time invariant school outcome defined for the high school in which student i, who

[29] Differently from the previous tables, Table 5 compares the cohort of 9th graders of 2012 (treated) with the cohort of 9th graders in 2008 (control) in a differences-in-differences model with two time periods. This is so because the intermediate cohorts of 2009 to 2011 (used as the baseline (2011) and for the pre-trends (2009–2010) in the analysis of school-choice decisions of 9th graders) are still in high school when QL was announced. The reform can then still impact their higher education choices, making them an inadequate control cohort for the outcome analyzed here. A placebo experiment in Table A.4 Panel A shows that the adoption of QL is not correlated with trends in enrollments in federal higher education when comparing cohort 2008 with cohort 2007.

Table 5
Effects of QL on enrollments in federal higher education.

	(1)	(2)	(3)	(4)	(5)	(6)
		Private School Students			Public School Students	
	All	Switchers	Stayers	All	Switchers	Stayers
$Treat_m \times [2012]$	−0.027*	0.028*	−0.039**	0.011***	0.000	0.012***
	(0.014)	(0.016)	(0.016)	(0.003)	(0.036)	(0.003)
N	319,590	57,575	262,015	1,233,429	24,227	1,209,202
Mean in Baseline	0.135	0.078	0.147	0.021	0.095	0.020

Notes: This table shows the estimates of the treatment effects of QL on the probability of enrollment in federal higher education in a regular trajectory (with no repetition or gap years) for the cohort of 2012 of students from 9th grade. Year 2008 is the baseline. The outcome is computed by linking students' information to the Census of Higher Education using the individual identifier available at the INEP's restricted room (See more information in Appendix E). Note that results of this table are conditioned on individuals for which a valid social security number (CPF) is observed (about 78% of 9th graders in 2008 and 73% in 2012). Panel B of Table A.4 in the Appendix shows that results are similar if we condition on a stable sample of schools and non-missing CPFs at the school level. All columns include broad region by year FE, school-microregion FE, and individual controls (gender, age, ethnicity, and urban status). Standard errors are shown in parenthesis and are clustered at the microregion level. * p < 0.1, ** p < 0.05, *** p < 0.01.

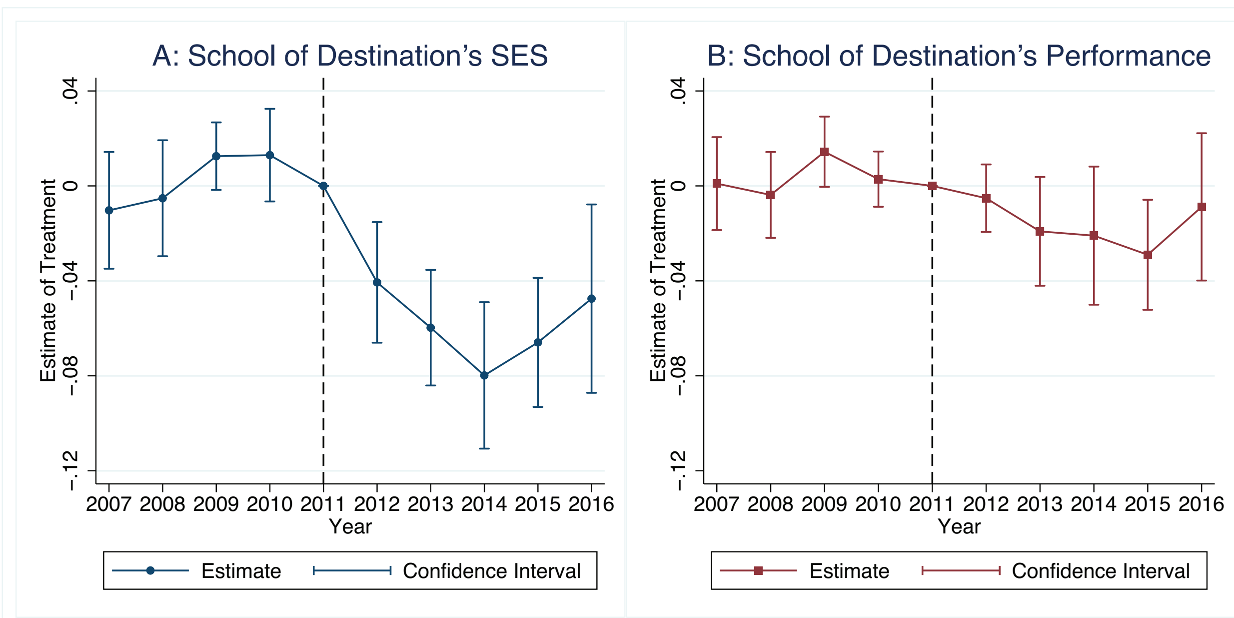

Fig. 5. Effect of QL on the characteristics of the school of destination. **Notes:** This figure plots the estimates of the treatment effects of QL on the characteristic of the school of destination (in 10th grade) of students originally attending 9th grade in a private school (with 95% confidence intervals). In Panel A, the school of destination outcome is the average time-invariant pre-reform level of the school socioeconomic index (NSE 2003–2011). In Panel B, it is the time-invariant pre-reform school performance in the National Standardized Exam (ENEM Escola 2011). The baseline mean of the outcome variable is 2.11 s.d. for Panel A and 0.86 s.d. for Panel B (both variables are standardized to have mean zero and variance one based on all the schools in 2011). Year 2011 is the baseline, 2007 to 2010 are pre-periods and 2012 to 2016 are treated periods. Specifications include broad region by year FE, school-microregion FE, pre-reform levels of log population and log GDP per capita interacted with year dummies, and individual controls (gender, age, ethnicity, and urban status).

attends 9th grade at school s, enrolls in 10th grade. As before, subscript m defines the microregion of treatment and subscript t the year of cohort. The parameter β measures the effect on the outcome if student i moves from a private to a public school. In a standard OLS specification, β would be biased, since individuals that move from a private to a public school are different in both observables and unobservables from the ones that decide not to move. However, the introduction of QL exogenously pushes students to public schools, compared to their counterparts in cohorts and regions not affected by the reform. Therefore, I use the introduction of QL as an instrumental variable (IV).

The instrument is exactly the interaction between $Treat_m$ and the dummy $Post_t$, which takes the value zero for pre-periods 2007–2011 and the value 1 for post-periods 2012–2016. The exclusion restriction holds in this case if QL affects the (pre-reform time-invariant) characteristics of the school of destination of private-school students only through their movements to the public system.

Table A.5 Panel A shows results in which $DestinationSchoolOutcome_{ismt}$ equals the time-invariant pre-reform school of 10th-grade socioeconomic index (NSE). Column (3) presents the IV estimates, showing that movements from private to public schools driven by QL induce students to attend high schools that are 1.6 standard deviations lower in socioeconomic status. Table A.5 Panel B shows results analogous when $DestinationSchoolOutcome_{ismt}$ is the time-invariant pre-reform school of 10th-grade average performance on the National Standardized Exam (ENEM Escola 2011). Column (3) shows that movements from private to public schools driven by QL induce students to attend high schools that are 0.64 standard deviations worse in terms of performance. In line with what Cullen et al. (2013) and Estevan et al. (2020) found for the Top 10-percent Plan in Texas, these results show that movers (compliers) end up attending high schools that are poorer and worse in overall performance than the schools they would have attended in absence of QL.

Taken together, these results show that by increasing the probability of federal higher education attendance for public-school students, QL induces movements from private to public schools for individuals at the margin. These movers are willing to trade the higher value-added obtained from a private school for an increase in the probability of attending higher-quality federal higher education.

Finally, it is important to note that families are making decisions in a setting of imperfect information and high uncertainty, making it difficult to precisely estimate whether their behavior is in line with rational expectations. Acceptance in federal higher education is indeed very competitive and individuals benefitting from the quotas have higher chances of being admitted.[30] In this context, moving to a public school increases the likelihood of acceptance into a federal university, as shown in Table 5.[31] Given the high value of attending these high-quality tuition-free institutions in Brazil, it is not surprising that individuals react even to small changes in admission probabilities. On the other hand, even these quota-eligible spots in federal higher education remain very competitive, and students willing to be accepted need to perform well in the National High School Exam. In this context, moving to a lower-performing public school might be a risky decision. Importantly, students are making choices under uncertainty, as they do not know precisely how much the introduction of quotas will affect the cutoff scores, nor whether their performance will be enough for admission into federal higher education 3 years later.

6.2. Indirect effects: Public School Environment

In addition to its direct effects on returns to investment, QL might also lead to indirect effects on school choice through positive changes in the public school environment. By increasing the probability for public-school students to attend federal higher education, QL may lead to a positive effect on these students' persistence and effort during high school. This would be in line with recent evidence by Akhtari et al. (2020) in the case of Texas. The authors show that the reinstatement of race-based affirmative action leads to a reduction in racial gaps in SAT scores, grades, attendance, and college applications, driven by improvements in minorities' outcomes. Although I cannot look at how QL affects eligible students' grades due to data limitations, Table 6 provides suggestive evidence of positive changes in the public-school environment. Column (1) shows that the adoption of QL decreases the dropout rates in public high schools by 2.2 percentage points. Moreover, this is not only a result of the change in composition, i.e., an increase in the share of (better) private school students moving to public schools. Column (2) estimates the effects on the sample of high schools not affected by composition due to QL.[32] Even in these schools, QL decreases drop-out rates in public high schools by 2.0 percentage points. This is suggestive evidence that the reform increases persistence in high school for public school students, which can then result in more motivated peers and a better environment in public schools. This better environment, in turn, can lead to additional moves from private to public schools.

Table 6
Effects of QL on dropout rates of public high schools.

	All	Not Affected	Affected
$Treat_m \times Before_t$	−0.005	−0.004	−0.007
	(0.010)	(0.010)	(0.012)
$Treat_m \times Post_t$	−0.023**	−0.020*	−0.023***
	(0.010)	(0.011)	(0.008)
N	50,436	22,293	28,143
Mean in Baseline	0.093	0.090	0.095

Notes: This table shows the estimates of the treatment effects of QL on dropout rates of public high schools (grades 10 to 12 combined). Column "Not affected" contains public high schools that did not experience an increase in the share of private-school students received after QL. It includes public high schools with fewer than 1% of students coming from private schools every year from 2008 to 2016, or schools that have fewer students coming from private schools in the post-quota period than in the pre-quota period. Column "Affected" contains public high schools that might have experienced an increase in the share of private school students received after QL. It includes schools that have more than 1% of students coming from private schools in at least one year from 2008 to 2016 and that have more students coming from private schools in the post-quota period than in the pre-quota period. Estimations are restricted to schools of at least 20 students in high school. All columns include broad region by year FE, school-microregion FE, pre-reform levels of log population and log GDP per capita interacted with year dummies, and individual controls (gender, age, ethnicity, and urban status). Standard errors are shown in parenthesis and are clustered at the microregion level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Fig. 6 and Table A.6 display the effects of QL on movements from private to public schools of cohorts originally from 6th to 11th grades separately, revealing that the reform also induces an increase in changes to public schools in transitions beyond the 9th to 10th grade.[33] The last two panels of the figure are particularly important to corroborate the argument of this section. QL leads to an increase of 1.3 and 1.1 percentage points in movements from private to public schools for cohorts transitioning from 10th to 11th, and from 11th to 12th grade respectively. Importantly, individuals moving in these transitions *cannot* directly benefit from QL due to the rules determined by the policy (individuals need to transition between 9th and 10th grade at the latest).[34] Thus, the existence of movements in these specific grades can only occur in the presence of indirect incentives, such as an improvement in the environment or in the quality of peers from public schools, as discussed previously.[35]

[30] In SISU 2016, considering only federal higher education institutions, there were 108,630 vacancies available for open competition, and 119,652 vacancies available for public-school students under the Quota Law. There were 2,593,966 applicants in total for these vacancies, and 1,506,570 applicants for the Quota Law. This means that the average number of applicants per vacancy is around 23.4 for the open-competition spots and 12.6 for the quota-law spots. Furthermore, cutoff scores for the reserved spots are lower, especially for high-earnings degrees, such as Medicine (see Appendix Figure A.3).

[31] Table 5 shows that exposure to QL increases by 2.8p.p. (36%) the probability of being admitted to a federal higher education institution for private-school students that switch to public schools, while it decreases by 3.9 p.p. (26%) the likelihood for private-school students that stay in private schools.

[32] I define this sample as the schools that received fewer private-school students after QL than before, or that receive fewer than 1% of private-school students in every year of the sample.

[33] In the benchmark analysis, I focused on movements from the cohort of individuals transitioning from 9th to 10th grade because students needed to move exactly at this stage to become beneficiaries of QL. Now, I use an extended sample in which I select all students enrolled in 6th to 11th grades in year t from 2007 to 2016. Using their individual *id*, I link each observation with their own information in year $t + 1$ from 2008 to 2017, identifying movements from private to public schools in the transitions between 6th and 7th grade, between 7th and 8th grade, and so on. I then estimate the benchmark dynamic differences-in-differences model for cohorts of each grade transition separately.

[34] One might worry that movements in other grade levels, especially 10th and 11th grades, are a result of other time-varying confounders that affect microregions more affected by QL. In Appendix C2.1, I conduct a placebo experiment where I use students in 10th grade as an additional comparison group in a triple-differences model that controls for microregion by year fixed effects. Results show that QL substantially increases the gap in transitions from private to public schools for 9th graders compared to 10th graders in microregions more affected by the policy change, even after controlling for all time-varying confounders that change at the microregion and at the school level.

[35] The first three panels correspond to Columns (1) to (3) of Table A.6 and show that QL has an effect of around 1.3, 1.9, and 1.9 p.p in movements to public schools among cohorts of students transitioning from 6th to 7th, from 7th to 8th, and from 8th to 9th grade. Since moving to a public school might be costly in terms of value-added (as shown in Table A.5), one could expect that families would delay this transition as much as possible. Thus, the existence of such movements in 6th to 8th grades could imply a more generalized increase in the value families give to public schools (although an anticipation effect of families that want to save on private-school tuition could not be ruled out in these cases).

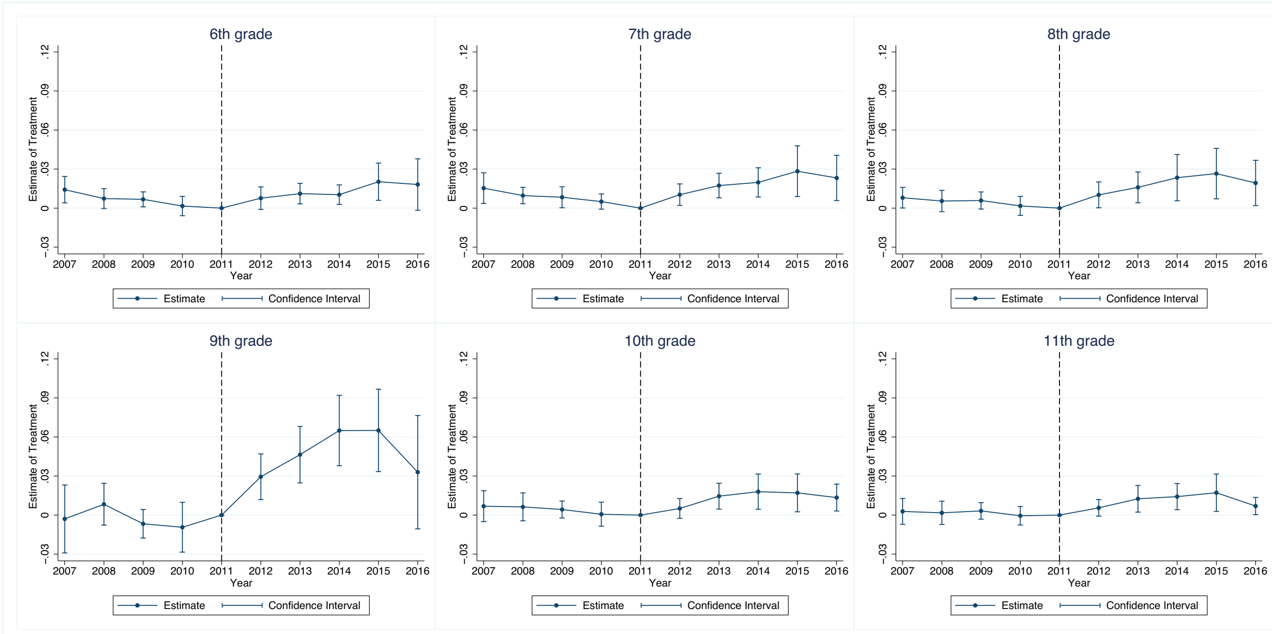

Fig. 6. Effect of QL on school movements of cohorts of different grades. **Notes:** This figure plots the estimates of the treatment effects of QL on movements of students from private to public schools for 6th to 11th grades separately (with 95% confidence intervals). The baseline mean of the outcome variable is 0.051 for 6th grade, 0.044 for 7th, 0.039 for 8th, 0.157 for 9th, 0.043 for 10th and 0.031 for 11th (also see Table A.6). Year 2011 is the baseline, 2007 to 2010 are pre-periods and 2012 to 2016 are post-periods. All panels include broad region by year FE, school-microregion FE, pre-reform levels of log population and log GDP per capita interacted with year dummies, and individual controls (gender, age, ethnicity, and urban status).

Finally, alternative mechanisms that could rationalize movements from private to public schools in 10th and 11th grades include the presence of siblings in earlier grades, mistakes made by students that do not know the rules of the QL reform, an increase in the price of private-school tuition and a change in expectations regarding the value of attending public schools in the future (since more peers from the public school will likely enter federal higher education). Unfortunately, I cannot test whether the indirect effects of QL also operate through these alternative mechanisms.

6.3. Spillovers and general-equilibrium effects

Finally, there is the possibility of spillover and general-equilibrium effects on the school system that might generate additional effects on school movement. For example, movements from private to public schools might induce changes in school size, in the value of private-school tuition, and finally, in the number of schools in the market.

To investigate that, I build a panel containing all the high schools active before QL (period 2008 to 2012) and follow them until 2017. Then, I estimate the following model at the school level:

$$Y_{smt} = \sum_{y=2008}^{2011} \beta_y \mathbb{1}_{\{y=t\}} \mathbb{Treat}_m + \sum_{y=2013}^{2017} \beta_y \mathbb{1}_{\{y=t\}} \mathbb{Treat}_m + \sum_{z \in \mathbb{Z}_m} \delta_z (z \times \alpha_t) + \alpha_m + \alpha_t + \varepsilon_{smt} \quad (5)$$

where the outcome Y_{smt} for school s, from microregion m, at time t is either the number of students at the school (from grades 6 to 12) or a dummy that takes the value 1 if the school remains active or the value zero if it becomes inactive or closes. In this case, 2012 is the baseline year, as effects at the school level are only expected from the school-year beginning from year 2013 on. Years 2008 to 2011 are the pre-periods. The model includes microregion fixed effects, broad region by year fixed effects, and pre-reform levels of log of population and log GDP per capita interacted with year dummies.

Results from Table A.7 Panel A show that QL decreases the average number of students at private schools by 21 students or 7.6% in the period 2013 to 2017. This effect starts right after the approval of QL, affecting the school year of 2013 and increasing with time, as shown in Fig. 7 Panel A. Panel B of Table A.7 shows that QL also increases the likelihood of school closure by around 2 p.p. or 38% in post-periods. This effect, however, is not immediate, as seen in Fig. 7 Panel B. QL does not impact the probability of school closure in the two first years following the policy implementation, 2013 and 2014. Only in the third year after losing students to the public system, do private schools start to close.

Taken together, these results suggest that the movements from the private to the public system induced by QL become large enough to generate spillover and general-equilibrium effects in the school system. These effects, in turn, might become an additional mechanism that induces movements from private to public schools.

6.4. Discussion

The implementation of QL in federal higher education increases movements from private to public schools through different channels. First, by directly changing the returns to investment in private-school tuition, QL induces movements from students that benefit the least from private schools and were on the margin of public-school attendance. Those were the students that attended low-SES and lower-quality private schools in 9th grade and that are willing to trade the additional value-added from private schools for a higher probability of future (high quality) federal higher education attendance. Second, movements from private to public schools are also observed (although in lower magnitudes) in grades 10 and 11 since 2012. Students in these transitions have no direct incentives to move school systems since the policy rules require that changes occur in 9th grade or earlier. The existence of

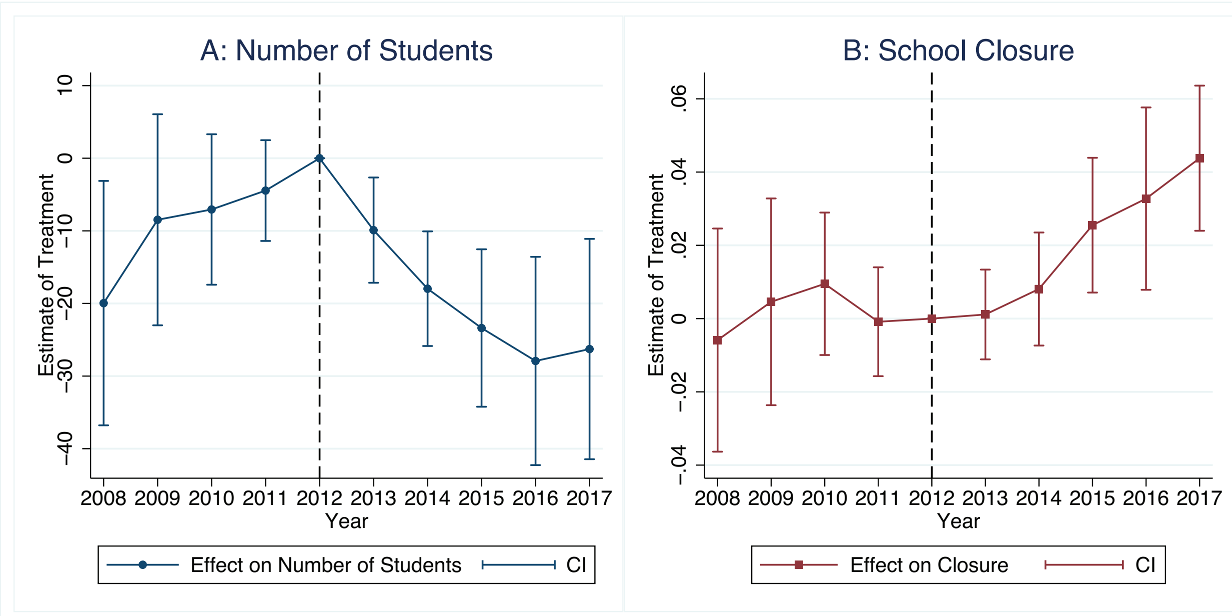

Fig. 7. Effect of QL on School Size and School Closure in the Private System. **Notes:** This figure plots the estimates of the treatment effects of QL on the total number of students from grades 6 to 12 and school closure (with 95% confidence intervals). The baseline mean for the number of students is 278 and for the probability of school closure, it is 0.056. The year 2012 is the baseline, 2008 to 2011 are pre-periods and 2013 to 2017 are treated periods. Both panels include broad region by year FE, microregion FE, and pre-reform levels of log population and log of GDP per capita interacted with year dummies.

such effects provides suggestive evidence that indirect channels, such as an improvement in the public school environment and in the quality of peers, may also play a role in inducing movements to public schools. Finally, in the third year following the implementation of QL, there is evidence of larger adjustments in the private school system through school closure. Since schools close, some individuals that would attend these specific private schools could be forced to change to the public system (for example, if they do not find a substitute private school nearby). Importantly, the channel of school closure only affects the cohorts moving from 2014 onward, and, indeed, the full effect of QL is only observed in years 2014 and 2015, as shown in Fig. 4 Panel A. For the cohorts of 9th, 10th and 11th graders in 2014, I can roughly approximate that the school closure channel explains around 11, 55 and 65% of the full effect of movements from private to public schools.[36]

7. Robustness

Online Appendix B.1 provides additional data to show that higher education markets remain local and concentrated within microregions during the whole period analyzed in this paper. In Appendix B.2, I construct an alternative measure of treatment exposure that allows for individuals to be affected by institutions outside their microregion. I then show that the effect of QL on movements from private to public schools remains similar to the benchmark when this measure is used to produce estimates in the universe of microregions in Brazil. Appendix B.3 shows that results are robust to the exclusion of microregions where local universities experienced changes in their quota policy in the years preceding the national law. Appendix C.1 shows that the economic crisis that affected Brazil starting in 2014 is unlikely to be driving the effects of QL on school choice. In Appendix C.2, I perform an additional robustness exercise by estimating a triple-differences model that controls either for microregion-by-year fixed effects or for school-by-year fixed effects. Appendix C.3 shows that the expansion of SISU, the Brazilian centralized admission system for higher education, between 2010 to 2016, did not impact the benchmark effects of QL. Appendix D.1 shows that results remain robust to alternative treatment definitions, while Appendix D.2 provides a thorough discussion on whether individuals react to changes in intensity versus the adoption of quotas. Finally, Table A.8 shows that QL does not affect dropout, repetition, or migration rates of private or public-school students from 9th grade.[37]

8. Concluding remarks

I study how a national affirmative action initiative that reserved a large share of vacancies in Brazilian federal higher education for graduates of public high schools impacted school-choice decisions. Results show that full adoption of QL increases movements from private to public schools by 31% percent considering all the post-periods jointly. Different mechanisms are likely to explain this finding. First, I show that individuals that move from private to public schools due to QL are trading, on average, a higher-performance private school for a higher probability of attending

[36] The causal effect of QL on school closure in 2015 is equal to 2.55 percentage points, which is equivalent to around 57 schools closing in highly treated microregions due to QL. Schools that closed in 2015 in highly treated microregions are small, with, on average, 10.4, 16.2, and 13.8 students in 9th, 10th, and 11th grade respectively. This means that around 596, 926, and 785 students from 9th, 10th, and 11th grades move from public to private schools in the cohort of 2014 due to schools that close at the beginning of the academic year 2015. Moreover, the causal effect of QL on movements from private to public schools in the student sample for the cohort of 2014 is, respectively, 5.74, 1.62, and 1.35 p.p. for the 9th, 10th, and 11th grades, adding up to 5,518, 1,689 and 1,202 students moving to public schools because of QL. Thus, the share of the total effect explained by the school closure channel is roughly equal to 596/5518 = 0.11, 926/1689 = 0.55 and 785/1202 = 0.65 in 9th, 10th, and 11th grade respectively.

[37] Table A.8 column (2) shows that QL has a small negative effect on the repetition rates of private school students. Rather than a direct effect of the reform, the decrease in repetition rates for private-school students is likely a consequence of the change in school choice. When students move from a private to a public school, it is possible to be reclassified to the next grade in the new school, instead of repeating the year in the school of origin.

a higher-quality tertiary education institution in the future. This provides evidence that QL indeed changes the returns to the investment in private education by directly decreasing the probability of higher education attendance of private-school students. I also find that QL increases movements to private schools among cohorts of 10th and 11th graders, who are not directly affected by the policy rules. This suggests the existence of spillover effects that could involve an increase in peer quality in public schools. Finally, I show that in the third year following the policy, there is a positive effect on private-school closure, suggesting that, with time, general-equilibrium effects also increase the transitions to the public system.

Important policy recommendations come from these findings. Affirmative action policies that use the school to determine eligibility are becoming a useful tool to target low-SES students without explicitly asking for information about family income or ethnicity. However, as with other education policies that evaluate individuals relative to their peers at the school level, they lead to unintended consequences, such as a change in school-choice decisions. Understanding the extent and the mechanisms behind these unanticipated effects is important for efficient policy design. This paper takes an important step in this direction by uncovering different channels through which these unintended consequences operate. However, the reduced-form approach and the unavailability of more detailed data regarding individuals' and peer grades still limits a more precise quantification of the importance of each of the mechanisms separately. As these policies become more popular as an alternative or a complement to race-based affirmative action, this emerges as an important avenue for future research.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2023.104824.

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