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Col·lecció d'Economia E21/411

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Guilherme Strifezzi Leal

Álvaro Choi

ISSN 1136-8365

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JEL Codes: J15, I24, I28, H52

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Authors:

Guilherme Strifezzi Leal	Álvaro Choi
Universitat de Barcelona	Universitat de Barcelona
Email: guilhermesl@al.insper.edu.br	**Email:** alvarochoi@ub.edu

Date: April 2021

Acknowledgements: The authors of this research have received financial support from the EDU2016-76414-R project of the Spanish Ministry of Economy and Competitiveness.

RACIAL QUOTAS IN HIGHER EDUCATION AND PRE-COLLEGE ACADEMIC PERFORMANCE: EVIDENCE FROM BRAZIL

Guilherme Strifezzi Leal[1]

Álvaro Choi[1]

[1] University of Barcelona

Abstract

The effects of affirmative action on the incentives to human capital accumulation are ambiguous from a theoretical perspective and the scarce empirical evidence on the matter provides mixed results. In this paper, we address this issue by investigating the impacts of Brazil's Law of Quotas on the students' performance in the college entrance exam, the ENEM. The law established that a specific share of places in Brazilian federal universities should be filled by non-white students from public high schools. We employ a difference-in-differences approach in order to estimate the effects of the implementation of these quotas on the ENEM scores and provide causal evidence that the law fostered incentives to pre-college human capital accumulation. Moreover, the effects of the quotas were greater in more quantitative-intensive subjects but were not different by gender or parental education, and these impacts increased throughout the first years after the law's implementation.

Key words: Racial quotas; Higher education; Equality of opportunity; Academic performance; Difference-in-differences

JEL Codes: J15, I24, I28, H52

Contact details: Guilherme Strifezzi Leal (guilhermesl@al.insper.edu.br). Department of Economics, Faculty of Economics and Business, University of Barcelona. 690 Av. Diagonal, 08034, Barcelona, Spain.

Acknowledgments: The authors of this research have received financial support from the EDU2016-76414-R project of the Spanish Ministry of Economy and Competitiveness.

1. Introduction

Racial inequalities in education have equity and efficiency implications. While they exacerbate social inequalities and hinder intergenerational mobility, they also constitute a waste of human capital potential. Although racial inequalities in education have narrowed in the 20th century, educational gaps between students from different racial and ethnic backgrounds are still wide in several countries (Marteleto 2012; O'Gorman 2010). Therefore, in order to close gaps in access and outcomes, affirmative actions with different designs have been implemented throughout recent years and have included measures such as preferential treatment in admission processes, race-specific financial aid and scholarship policies (Arcidiacono 2005; Ibarra 2001).

An alternative and sometimes complementary approach has been the introduction of racial and ethnic quotas. At the higher education level, this measure consists of pre-establishing a share of seats in institutions to specific racial and/or ethnic groups and has been applied in countries such as Brazil, India and Malaysia. Those in favor of the implementation of these policies usually allude to their effects on the enrolment of minority groups, especially in more selective programs, and consequently to the potential labor market gains for these individuals (Rothstein and Yoon 2008b; Loury and Garman 1993). Those who oppose racial/ ethnic quotas ground their concerns on the so-called "mismatch hypothesis": minority students who are admitted through the quotas may never catch up with their college peers due to the previous accumulated learning deficits; thus, racial and ethnic quotas may do more harm than good to these students as they would benefit more from courses/institutions more suited to their skills (Frisancho and Krishna 2016; Rochstein and Yoon 2008a; Loury and Garman 1993 and 1995).

Interestingly, an additional issue that has been less explored by the literature regards the incentives to human capital accumulation that these policies yield to targeted students prior to college admissions – indeed, the few empirical research works that have investigated these effects reached diverging results (for instance, Saeme 2014 and Assunção and Ferman 2015). Moreover, there is a well-established literature on the importance of students' pre-college accumulated human capital in explaining the variation in college graduates' earnings (Walker and Zhu 2018; Dale and Krueger 2002 and 2014). Therefore, understanding the ex-ante effects of racial quotas is crucial not only to unravel the incentives provided by affirmative action but also because they play a key role in labor market success.

In this paper, we evaluate the ex-ante effects of a law that implemented quotas in higher education in Brazil. Differences in access to higher education by race are significant in this country: in the year 2010, according to Censo IBGE (*Instituto Brasileiro de Geografia e Estatítica*) and the Higher Education Census, black and brown persons represented 51% of the total population, but accounted for only 34% of higher education enrolments, whereas white persons made up 48% of the population and 63% of enrolments. In an attempt to mitigate this inequality of access to higher education, the federal government created in 2013 what came to be known as the Law of Quotas, establishing that a proportion of seats in Brazilian federal universities should be filled by non-white and low-income students from public high schools. In this research, we study the effects of the law on students' pre-college academic performance; that is, we investigate whether the increase in enrolment by these students was due only to the existence of an increased number of reserved seats or whether the policy itself had a positive incentive effect on human capital accumulation (i.e., if there were efficiency gains). More specifically, we focus on evaluating the efficiency effects of the racial criteria of the law.

We assess these effects by examining the extent to which the Law of Quotas affected the performance of students in the college entrance exam, the ENEM[1]. To this end, we employ a difference-in-differences approach by explicitly controlling for a set of student-specific variables contained in the ENEM's microdata. The repeated cross-sectional database provides information on socioeconomic factors, income, parental education, previous work experience and previous academic effort. To the best of our knowledge, this is the first paper to take advantage of a major country-level quotas law to investigate such effects on a national scale. We show that the law fostered incentives to pre-college human capital accumulation as it induced eligible students to attain higher scores on the ENEM exam. Furthermore, we test for the existence of heterogeneous effects by subject, gender and parental education. We also estimate both a two-periods difference-in-differences model and a regression with dynamic treatment effects.

This research shows that the effects of the law were greater in more quantitative-intensive subjects (Math and Natural Science) and that the impact of the law increased throughout the first years after its implementation. We do not find any evidence, however, that the effects of the law were distinct between genders and between students with and without a college-educated parent.

The paper is organized as follows. Section 2 provides a brief literature review on the effects of racial quotas. Section 3 expands on the institutional setting of the Brazilian educational system and of the Law of Quotas. Section 4 describes the data and the empirical strategy employed in the paper. Section 5 presents the results of the difference-in-differences models. Section 6 discusses the main implications of our findings. Finally, section 7 presents some conclusions.

2. Educational quotas in higher education

The introduction of racial and ethnic quotas in college admissions has become a common practice in a number of countries. In the United States, even though such policies have not been prescribed

[1] Exame Nacional do Ensino Médio.

by the Constitution, several guidelines issued by the U.S. education and justice departments have encouraged institutions to grant preferential treatment to applicants from minority groups in admissions to universities (Department of Education and Department of Justice, 2011). Moreover, in countries such as India, Malaysia and Brazil, a step further has been taken as these practices have been institutionalized by federal laws establishing that a certain percentage of seats in education institutions should be filled by specific racial or ethnic groups.

Economists have long been interested in understanding how these policies affect the college enrolment of students who benefit from the quotas (quota holders) as well as their effects on performance in higher education. The effects of such policies on college enrolment are rather controversial, as there is a large body of evidence suggesting that affirmative action plays an important role in increasing the enrolment of minority groups in higher education, especially in more selective programs (Vieira and Kuenning 2019; Francis and Tanuri-Pianto 2012; Tienda et al. 2008; Dickson 2006; Long 2004a and 2004b; Bowen and Bok 1998).

However, the effects of racial quotas on performance are still a subject of discussion. One concern that has been thoroughly explored is related to the initial gap between quota holders and their college peers, and their capacity to catch up with the remaining students as they progress through college. Evidence on this subject is mixed, as some studies indicate that minority students tend to fall behind their same-major peers (Frisancho and Krishna 2015; Sander 2004), while others find that there are no significant differences between each group's college achievement (Queiroz et al. 2015) or even that it varies depending on the major or on the student's social and academic background (Vidigal 2018; Ribas et al. 2015).

Although there is substantial literature on the ex-post effects of educational quotas, that is, the effects on quota holders after college admissions outcomes are determined, the ex-ante effects of such policies have been significantly less explored, and a particularly relevant issue concerns the incentives that these quotas provide to pre-college human capital accumulation. The incentive effects of affirmative action on pre-college human capital accumulation have been explored mostly from a theoretical perspective, and the results are ambiguous. On the one hand, such policies might lead to ex-post discrimination of minority groups, as argued by Coate and Loury (1993), Loury (1992), Milgron and Oster (1987) and Lundberg and Startz (1983), or even to more complacent students due to the high numbers of reservation quotas, especially among the smartest section of the minority group, as stressed by Kight and Hebl (2005) and Assunção and Ferman (2015), which could encourage quota-eligible students to reduce skill acquisition during basic school. On the other hand, affirmative policies might mitigate the so-called "discouragement effects", dislocating the students to the margin of selection and increasing the willingness to re-allocate leisure time towards building human capital as pointed out by Cotton et al. (2016) and

Furstenberg (2003). It has also been suggested that whether the incentives to invest in human capital are improved by affirmative action depends on the level of discrimination of the economy's initial equilibrium (Moro and Norman 2003).

Empirical investigations on the incentive effects of educational quotas, however, are still scarce. Khanna (2020) evaluates the effects of reservation quotas for college seats and government jobs in pre-college years of schooling in India and finds that affirmative action incentivizes about 0.8 additional years of education for the average minority group student and 1.2 more years of education for a student from a marginal minority subgroup.

In the Brazilian context, the literature has been mostly restricted to specific universities that have implemented racial quotas in their admission processes of their own will prior to the 2012 Law of Quotas. Saeme (2014) investigates the implementation of a 40% quota for black persons in the Federal University of São Carlos (UFScar) and finds that black students from public schools in São Paulo scored 1.54% higher on the ENEM as a result of the introduction of quotas in UFSCar admissions, while Francis and Tanuri-Pianto (2012) evaluate the adoption of racial quotas at the University of Brasilia in 2004 and find that the quotas did not reduce pre-college effort (it might have even raised pre-college effort, although the evidence is tenuous). Conversely, Assunção and Ferman (2015) evaluate the effects of the implementation of quotas in three public Universities in the States of Rio de Janeiro and Bahia from 2002 to 2004 and find that these quotas induced targeted groups to attain lower high school scores.

The introduction of the Law of Quotas in 2012 in Brazil, which ensured the implementation of racial quotas in all of the federal universities in the country, created an advantageous setup to expand the understanding of the ex-ante effects of these quotas and finally provide more clarity in the direction of these incentives. Most of the research that has been undertaken to evaluate the impact of the law, however, has focused on its effects on college enrolment and ex-post college performance (Vidigal 2018; Queiroz et al. 2015; Ribas et al. 2015).

Indeed, the effects of the law on students' pre-college behavior have been largely ignored by the literature. While Mello (2019) investigated how the Law of Quotas impacted the ex-ante decision between attending a private or public high school (since the law was only applicable to students that had previously attended public high schools), the effects of the law on pre-college academic performance are, to the best of our knowledge, yet to be examined. Therefore, we contribute to the literature by presenting causal evidence of the impact of an affirmative policy on pre-college performance at the national level.

3. Social and Institutional Background

In this section, we describe the social and institutional background relevant to this paper. Subsection 3.1 describes some key demographic characteristics of the Brazilian population; subsection 3.2 outlines the structure of the Brazilian higher education system; subsection 3.3 provides information on the ENEM exam; and finally, subsection 3.4 describes the 2012 Law of Quotas.

3.1. Socio-demographic characteristics

The Brazilian population's racial composition stems from a confluence of many different ethnic backgrounds, from indigenous people, black Africans and Portuguese that represented the majority of Brazil's inhabitants in the colonization period to the subsequent waves of Europeans, Arabs and Asians that arrived in the country throughout the 20th century. Consequently, most of the Brazilian population possesses some degree of mixed-race ancestry, which has led researchers that investigate the racial dynamics in the country to focus on the so-called black-to-white continuum.

The black-to-white continuum encompasses 99% of the Brazilian population and the national institute responsible for collecting and reporting sociodemographic data (the IBGE, *Instituto Brasileiro de Geografia e Estatística*) uses three different racial terms to identify individuals among this continuum: white (*branco*, which represents 48% of the country's population), brown (*pardo*, 43% of the population) and black (*preto*, 8% of the population). The remaining 1% of the population is composed mainly of Asians and indigenous ethnicities. Table 1 shows that the racial composition varies widely across Brazilian States.

Table 1 - Racial Composition by Brazilian State

Region	State	White	Black	Asian	Brown	Indigenous
Brazil	**Brazil**	**48%**	**8%**	**1%**	**43%**	**0.4%**
North	Rondônia	35%	7%	1%	56%	1%
	Acre	24%	6%	2%	66%	2%
	Amazonas	21%	4%	1%	69%	5%
	Roraima	21%	6%	1%	61%	11%
	Pará	22%	7%	1%	70%	1%
	Amapá	24%	9%	1%	65%	1%
	Tocantins	25%	9%	2%	63%	1%
Northeast	Maranhão	22%	10%	1%	67%	1%
	Piauí	24%	9%	2%	64%	0%
	Ceará	32%	5%	1%	62%	0%
	Rio Grande do Norte	41%	5%	1%	52%	0%
	Paraíba	40%	6%	1%	53%	1%
	Pernambuco	37%	6%	1%	55%	1%
	Alagoas	32%	7%	1%	60%	0%
	Sergipe	28%	9%	1%	61%	0%
	Bahia	22%	17%	1%	59%	0%
Southeast	Minas Gerais	45%	9%	1%	44%	0%
	Espírito Santo	42%	8%	1%	49%	0%
	Rio de Janeiro	47%	12%	1%	39%	0%
	São Paulo	64%	6%	1%	29%	0%
South	Paraná	70%	3%	1%	25%	0%
	Santa Catarina	84%	3%	0%	12%	0%
	Rio Grande do Sul	83%	6%	0%	11%	0%
Central West	Mato Grosso do Sul	47%	5%	1%	44%	3%
	Mato Grosso	37%	8%	1%	52%	1%
	Goiás	42%	7%	2%	50%	0%
	Distrito Federal	42%	8%	2%	48%	0%

Source: 2010 Census - IBGE

Among the three largest racial groups in the country (white, black and brown persons), a common concern is the substantial educational disparity between them, especially in terms of access to higher education. According to the IBGE[2], in 2010, 13% of the white population had a college degree, whereas the same was true for only 4% of the black and brown population. By 2019, these figures had evolved to 21% for white persons and 9% for black and brown persons. Moreover, the uneven playing field in the educational sphere also contributes to the perpetuation of inequality in income levels. In 2010, the average income for the white population was 1.9 times larger than it was for the black and brown population, while in 2019 it was 1.8 times larger.

[2] Census for 2010 data and PNAD (*Pesquisa Nacional por Amostra de Domicílio*) for 2019 data

3.2. Higher Education in Brazil

According to the 2019 Higher Education Census, the Brazilian Higher Education system serves 8.6 million students (in 2019, the average enrolment rate of individuals between 18 and 24 years old was 20.4%) and consists of 2,608 institutions, among which 2,306 (or 88%) are private and 302 (or 12%) are public. Private institutions, which are fee-paying, contain the vast majority of enrollments (6.5 million students in 2019, or nearly 76% of total enrollments). Each private institution has complete independence regarding tuition fees and runs its own admission process, which usually consists of written exams developed by the institution itself. Public institutions, in turn, are predominantly free of charge[3] and are managed by either the federal, state or municipal government. Federal (110) and State (132) HEIs (Higher Education Institutions) encompass most of the public enrollments (62% and 32%, respectively), while Municipal institutions (60) account for only 6% of public enrollments.

Public HEIs are generally more prestigious and since they are mostly tuition-free, these institutions have the most competitive selection processes in the country[4]. Until 2010, the admission process to public HEIs was highly decentralized and most institutions developed their own exam –indeed, some of them used the ENEM as part of the selection criteria. This structure led to tests with widely different contents and to a highly localized higher education market, since it induced students to restrict their study and preparation to admission processes for specific universities. On that count, in 2010 the Ministry of Education created the *Sistema de Seleção Unificada* (SISU), an online platform where Federal and State universities could use the grades of the students in the national standardized exam (the ENEM) for their admission processes. In order to be eligible for admission, students who take the ENEM exam must then complete a SISU application. By 2015, the system was being used by 108 public institutions, among which 92 were federal HEIs.

[3] Institutions maintained by federal and state levels of governments are forbidden by law to charge tuition fees, but municipal institutions are allowed and usually charge some tuition fees.

[4] Federal and State universities have higher average scores in the *Índice Geral de* Cursos (IGC), a quality index developed by the Ministry of Education, and comprise most of the higher ranked institutions in the *Ranking Universitário Folha* (RUF), an annual evaluation of the HEIs in Brazil developed by the *Folha de São Paulo* newspaper. According to Binelli et al. (2008), there were on average 9 applicants per seat at public institutions in 2003, while this ratio was 1.5 in private institutions.

3.3. The *Exame Nacional do Ensino Médio* (ENEM)

The ENEM is a national non-mandatory standardized exam organized by the INEP (*Instituto Nacional de Estudo e Pesquisas Educaionais Anísio Teixeira*) within the Ministry of Education that takes place once a year in Brazil, usually around October, and it is one of the largest national exams in the world with a yearly average, between 2010 and 2019, of 6.5 million test takers. Created in 1998 with the purpose of evaluating high school students' performance and learning, it now plays a multiple role: it is a mandatory exam for the SISU application (and therefore serves as an entrance exam to many HEIs in the country); one of the selection criteria in the Prouni (*Universidade para Todos*), a federal scholarship program established in 2005; and it is also used to evaluate and compare the quality of high school institutions in the country.

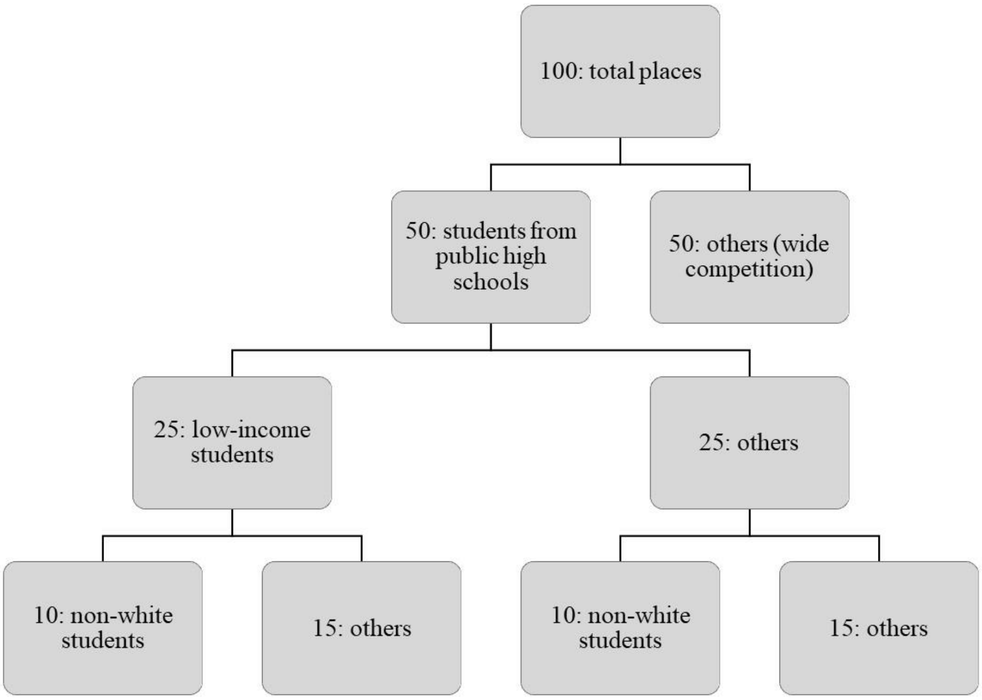
The ENEM is comprised of one multiple choice exam and one essay. The multiple choice (or objective) exam consists of four different subjects: natural sciences, social sciences, languages and math. In general, the natural science test covers physics, chemistry and biology related contents; the social sciences test covers history, geography, sociology and philosophy related contents; the languages test covers a broad range of contents from Portuguese and foreign languages to literature, arts and related topics; and finally the math exam encompasses solely mathematics. In the essay (or written exam), candidates must discourse upon a topic of public interest (usually about Brazilian social, political and/or economic issues). A detailed description of the ENEM is provided in the ENEM's Act (*Edital do ENEM*) and Syllabus (*Matriz de Referência ENEM*).

3.4. The 2012 Law of Quotas

As stated in subsection 3.1, access to higher education is significantly unequal in Brazil and has historically lacked representation of non-white students. Moreover, the inequality of access between students who attended private high schools and those who attended a public institution has also been an ongoing concern. According to IBGE's *Síntese de Indicadores Sociais*, in 2017, 79% of private high school graduates progressed to tertiary education, while the same was true for only 36% of graduates from public schools.

In view of that picture, a handful of Brazilian public universities began to implement racial quotas in their admission processes in the early 2000s. Finally, in August 2012, the Brazilian federal government established the Law 12.711/2012, which later came to be known as the Law of Quotas. The law stated that at least 50% of places in Federal HEIs should be filled by students that had attended the entire high school period (in Brazil, this consists of 3 years) in a public institution. Among this group, at least 50% (that is, 25% of the total) should be filled by students from public schools whose per capita family income amounts to at most one and a half times the minimum wage (approximately US$300 per month in 2020), and at least X% (that is, X*50% of the total) should be filled by black, brown, and indigenous students (from this point forward, we shall refer to this group as non-white) from public schools, where X represents the share of non-white students in the respective HEI's State population according to the 2010 Census. Figure 1 summarizes the rules of the law in a diagram.

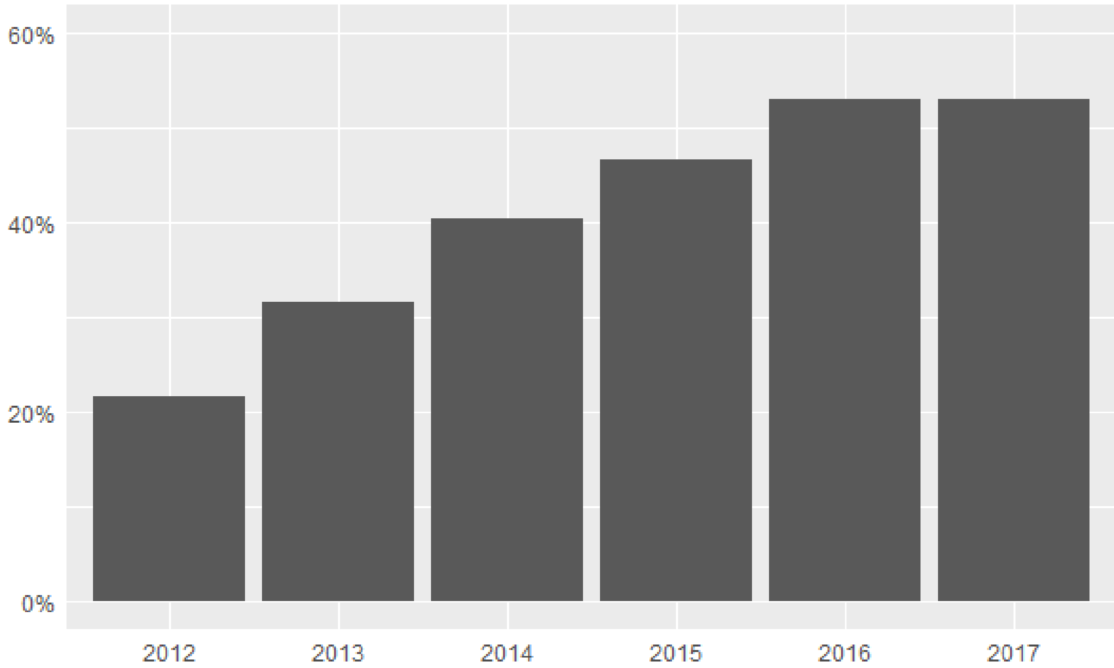
Figure 1 - The 2012 Law of Quotas



Note: The diagram above presents a simulation of the reserved places in a hypothetical federal university in which the total number of places equals 100 and in which the State's non-white individuals' percentage is of 40% (hence, 40% of 25 = 10).

Although it was announced in 2012, the law stated that HEIs had until 2016 to fully implement the quotas, but a minimum of 25% of the reserved seats should be implemented in each year from 2013 onwards. Therefore, universities had to reserve at least 12.5% of their seats in 2013, at least 25% in 2014, at least 37.5% in 2015 and finally the pre-established share of 50% in 2016, at the latest. Figure 2 illustrates the rate at which the law was implemented by universities.

Figure 2 - Average Percentage of Reserved Places in Federal Universities throughout the Years



Source: GEMAA (Grupo de Estudos Multidisciplinares de Ação Afirmativa)

4. Data and methodology

4.1. Data

This paper uses publicly available ENEM microdata, which has been published yearly since 1998 by the INEP (*Instituto Nacional de Estudo e Pesquisas Educacionais Anísio Teixeira*), an agency linked to the Ministry of Education. This repeated cross-sectional database provides information on the ENEM scores and key individual and household level variables of all students who sat the exam. Until 2008, the ENEM consisted of one essay and 63 multiple choice questions, with a score ranging from 0 to 100. In 2009, however, the exam was completely reformulated and, from that year onwards, the number of multiple choice questions increased to 180 (divided into 4 categories: natural science, social science, mathematics and languages) and all of the scores, including the essay, were measured on a scale from 0 to 1000, using the Item Response Theory[5]. Additionally, from 2009 onwards, the scores between different years have become comparable (*ENEM – Guia do participante*) and students were consequently allowed to use scores from previous years in the SISU application.

The ENEM's microdata on individual and household characteristics comes from a mandatory self-declared questionnaire that all the candidates must fill out when signing up for the exam. The

[5] The probability of obtaining a correct answer is assessed according to its difficulty, the probability that a student could guess a correct answer, and its ability to discriminate against students.

survey contains questions on basic socioeconomic factors (such as race, gender, age, marital status, city of residence, etc.), level of family income, parental education, high school record (if the candidate has ever been held back or dropped out of school), work-related factors (if and how much had the candidate worked during his/her life), school type in which the candidate was enrolled during high school and fundamental school (public or private), among others. It is important to note that the ENEM's microdata is not a panel data, since the set of students that take the exam changes every year, and even if a student takes the ENEM exam more than once, the database does not allow us to track this student's performance over time. However, as will be seen in the next subsection, in order to estimate the difference-in-differences model, we will cluster the students into specific groups that can be tracked throughout the years.

We will be looking at the years from 2010 to 2016, since the latter was the final year for the Law of Quotas to be fully implemented by all the institutions. We will not incorporate years prior to 2010 in our model, since the format and many of the mandatory questions from the ENEM's survey changed as of that year. We select students who had already completed high school or were to complete it in the year of the exam and those who actually attended the test. Hence, we exclude the students who only signed up for the exam but did not take it and those who were taking it as a practice test before graduating high school. Also, as mentioned earlier, we shall focus our analysis on the racial criteria of the law, since the ENEM's microdata only discloses income information on intervals of minimum wage, which hampers the evaluation of the effects of the law on individuals who are on the threshold of the income criteria.

Tables 2 and 3 provide the definitions and descriptive statistics of the variables included in the models to be presented in subsection 4.2. As displayed in Table 3, the percentage of non-white applicants on the ENEM exam increased significantly throughout the entire timespan of the database, especially after the implementation of the Law of Quotas (first put into effect in 2013). Furthermore, the variables used in the study were chosen such that we could preserve the largest amount of available data (that is, we gave priority to the mandatory questions of the ENEM' questionnaire) and, as shown in Table 3, the percentage of missing information is low and this was dropped from the analysis.

Table 2 - Key Variables

Variables	**Description**
Age	Numerical
Gender	Masculine or feminine
Marital status	Single, married, divorced or widowed
State	State of residence (27 federative units of Brazil)
Degree of ruralization	Percentage of rural households in the city of residence
High school type	Entirely in public school, entirely in private school or mixed
Average income	Average per capita family income in intervals of minimum wages
Race/Ethnicity	White, black, brown, indigenous or other
Parental education	Parental higher degree of education (6 categories)
Work factor	Dummy: 1 if student has ever worked before
Dropout/Grade repetition	Dummy: 1 if student has ever been held back or dropped out in HS

Source: ENEM's Microdata – INEP (Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira)

Table 3 - Summary Statistics

Variables	**2010**	**2011**	**2012**	**2013**
Age - mean (sd)	22.5 (7.2)	22.2 (7.0)	22.3 (7.2)	22.7 (7.5)
missing	*0%*	*0%*	*0%*	*0%*
Gender (M; F)	40%; 60%	40%; 60%	41%; 59%	42%; 58%
missing	*0%*	*0%*	*0%*	*0%*
Marital status (S; M; other)	86%; 14%; 0%	86%; 14%; 0%	87%; 12%; 1%	86%; 12%; 2%
missing	*0%*	*0%*	*0%*	*0%*
Ruralization - mean (sd)	11.5% (17%)	12% (17%)	12.1% (17%)	12.4% (17%)
missing	*0%*	*0%*	*0%*	*0%*
High school (pub.; priv. + mix.)	79%; 21%	79%; 21%	79%; 21%	80%; 20%
missing	*0%*	*0%*	*0%*	*0%*
Average income - mean (sd)	1 mw (1.5)	0.7 mw (1.1)	0.8 mw (1.05)	0.7 mw (1.02)
missing	*0%*	*0%*	*0%*	*0%*
Race (white, brown, black)	45%; 40%; 12%	43%; 41%; 12%	43%; 42%; 12%	40%; 44%; 13%
missing	*3.3%*	*2.4%*	*1.7%*	*1.6%*
Work factor	55% Y; 45% N	54% Y; 46% N	59% Y; 41% N	61% Y; 39% N
missing	*0%*	*0%*	*0%*	*0%*
Dropout/Grade Repetition	19% Y; 81% N	19% Y; 81% N	18% Y; 82% N	19% Y; 81% N
missing	*0%*	*0%*	*0%*	*0%*
Variables	**2014**	**2015**	**2016**	**2010-2016**
Age - mean (sd)	23.1 (7.7)	22.5 (7.3)	22.3 (7.2)	22.5 (7.3)
missing	*0%*	*0%*	*0%*	*0%*
Gender (M; F)	42%; 58%	42%; 58%	42%; 58%	41%; 59%
missing	*0%*	*0%*	*0%*	*0%*
Marital status (S; M; other)	85%; 13%; 2%	88%; 10%; 2%	89%; 9%; 1%	87%; 12%; 1%
missing	*0%*	*3.5%*	*3.5%*	*1.2%*
Ruralization - mean (sd)	12.3% (17%)	11.9% (17%)	12% (18%)	12% (17%)
missing	*0%*	*0%*	*0%*	*0%*
High school (pub.; priv. + mix.)	83%; 17%	81%; 19%	81%; 19%	81%; 19%
missing	*0%*	*0%*	*0%*	*0%*
Average income - mean (sd)	0.7 mw (1.02)	0.8 mw (1.1)	0.7 mw (1.02)	0.7 mw (1.1)
missing	*0%*	*0%*	*0%*	*0%*
Race (white, brown, black)	39%; 44%; 13%	38%; 46%; 13%	36%; 47%; 14%	40%; 44%; 13%
missing	*1.4%*	*1.7%*	*1.7%*	*1.9%*
Work factor	62% Y; 38% N	59% Y; 41% N	54% Y; 46% N	58% Y; 42% N
missing	*0%*	*0%*	*0%*	*0%*
Dropout/Grade Repetition	17% Y; 83% N	16% Y; 84% N	15% Y; 85% N	17% Y; 83% N
missing	*0%*	*0%*	*0%*	*0%*

Source: ENEM's Microdata – INEP (Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira)

4.2. Methodology

In order to study the causal effects of the creation of the Law of Quotas on the students' ENEM performances, we employ a difference-in-differences methodology. The idea behind this approach is fairly simple. Outcomes are observed before and after a specific treatment and between two groups; a treatment group that was exposed to the treatment and a control group that was not exposed to it. The treatment effect is then estimated by comparing the change in outcome between the two groups, while a set of control variables is added to the model in order to control for the individuals' specific characteristics. The fact that the Law of Quotas only applied to certain students and the presence of "clean" individuals who were unaffected by it makes difference-in-differences an appropriate methodology to evaluate the causal effects of the law on students' ENEM scores.

Before going into the details of our model, let us recall that the law applies to all students who have attended public high schools, but it provides special benefits for those who are non-white, since among the reserved seats based on the school criteria, there is a pre-established share of seats based on the racial criteria (see Figure 1). We will then separately estimate the effects of each one of these components of the Law of Quotas, which we shall call the "school component" and the "racial component", on the students' ENEM scores.

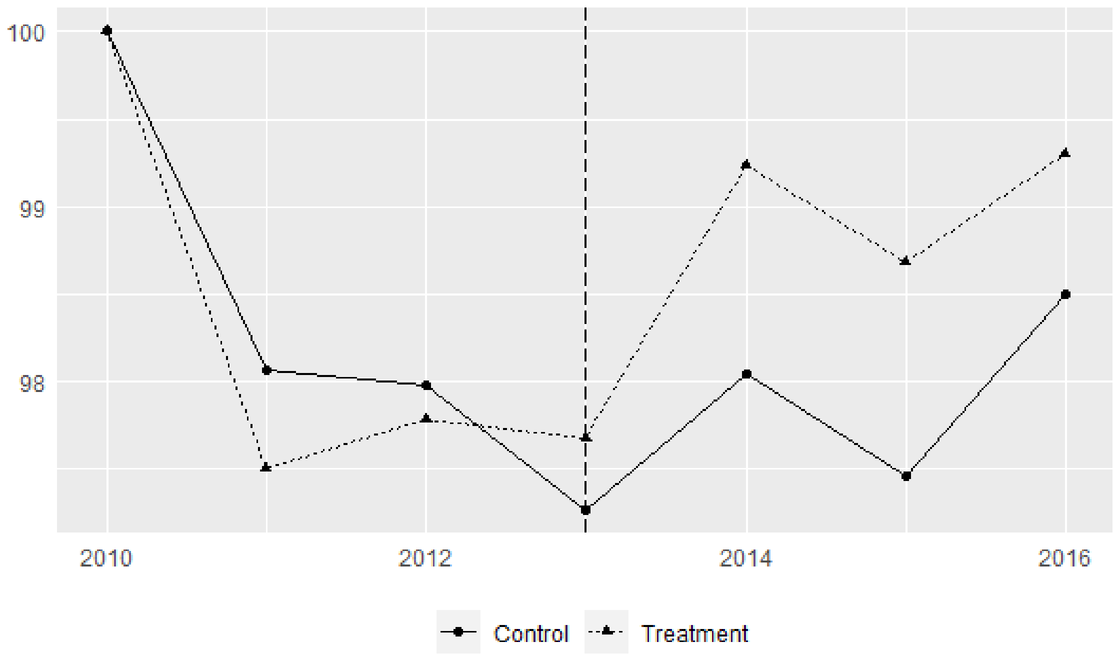
For this reason, we estimate the difference-in-differences model in two steps. First, we investigate solely the impact of the school component of the law. Hence, we observe the ENEM scores before and after the policy intervention (the introduction of the Law of Quotas in 2013) and between two groups: a treatment group composed of white students from public high schools (therefore impacted by the school component but not by the racial component of the law); and a control group composed of private high school students[6] from all ethnicities (i.e., not eligible for any quotas – remember that the law does not apply to students who attended private high schools, regardless of their race and income class).

Secondly, we assess the impact of the racial component of the law; that is, the effect on non-white public school students. In this case, the ENEM scores are again observed before and after the introduction of the law; however, the treatment group is now composed of non-white students from public high-schools, and the control group is composed of white students from public high schools (that is, both groups are affected by the school component of the law – since they are both public high school students - but only the treatment group is affected by the racial component).

[6] Those who attended only a part of high school in a private institution also compose the control group, since they are not eligible for the quotas. For simplicity, we shall refer to this group as private high school (or simply private school) students

Standard approaches for causal inferences in difference-in-differences are valid only under the assumption that the treatment and control groups display parallel trends before the policy intervention – which additionally is assumed to be a good post-treatment counterfactual. The previous trends for the control and treatment groups in the school component model are presented in Figure 3. The figure shows that, prior to the implementation of the Law of Quotas in 2013, the ENEM objective scores of white public school students and private school students present relatively similar, although not identical, trends. We tackle this issue following the method suggested by Rambachan and Roth (2019) for robust inference in difference-in-differences settings where the parallel trends assumption may be at stake.

Figure 3 - Yearly Average ENEM Objective Score (2010 = 100). Control Group: Private School Students; Treatment Group: White Public School Students



In order to apply the methodology proposed by Rambachan and Roth (2019), we estimate the following dynamic event-study regression:

$$\log\left(S_{it}\right) = c_0 + \phi_t + \lambda D_i^s + \gamma X_{it} + \sum_{s \neq 2012} \beta_s \times \mathbb{1}[t = s] \times D_i^s + \varepsilon_{it} \qquad (1)$$

where S_{it} denotes the ENEM total score on the multiple choice exam of a student i who took the exam in a specific year t; D_i^s is a dummy variable that equals one if the individual is a public high school student (i.e., if they belong to the treatment group); $\hat{\phi}$ and $\hat{\lambda}$ measure the time-specific and group-specific fixed effects, respectively; X_{it} includes the student-specific control variables described in Table 2; and the coefficients $\{\hat{\beta}\}$ account for the event-study coefficients (which measure the causal effect of the treatment plus the difference in trends between the treatment and control groups), where $\hat{\beta}_{2012}$ is normalized to zero (remember that 2012 was the last year before the implementation of the law).

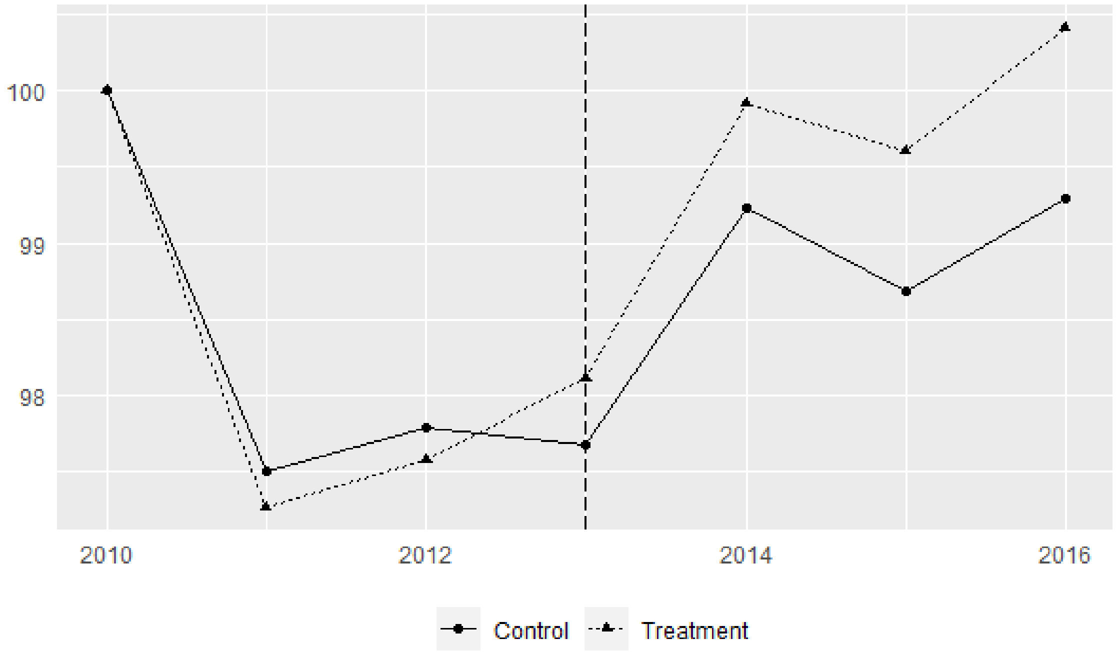
When parallel trends hold exactly – and assuming there is no causal effect of the treatment before the intervention -, the pre-treatment betas, that is, $\hat{\beta}_{2010}$ and $\hat{\beta}_{2011}$ are both equal to zero, since there is no difference in the pre-treatment trends between the two groups, and the post-treatment betas, that is $\hat{\beta}_{2013}$ to $\hat{\beta}_{2016}$ correspond to the dynamic treatment effect in each year relative to our reference period 2012. When parallel trends do not hold, however, the pre-treatment betas are significant and the post-treatment betas account for the dynamic treatment effect plus the post-treatment difference in trends (differential trends). In this case, a common approach for inferring causality is to assume that the pre-treatment differential trend will persist after the treatment and simply perform a linear extrapolation of it to the post-treatment period[7]. Rambachan and Roth (2019) propose an alternative approach to deal with this issue by considering robustness to some degree of deviation from the pre-existing differential trend. They introduce a parameter M which governs the maximum possible error of the linear extrapolation of the pre-treatment differential trend[8], therefore allowing us to evaluate the sensitivity of our results to violations of parallel trends.

We replicated the analysis for the racial component model. Figure 4 presents the previous trends for the control and treatment groups, and a simple visual inspection tells us that the parallel trends assumption seems to hold better in this case.

[7] There have been concerns, however, that assuming that pre-existing differences in trends continue in precisely the same way may be misleading (Lee and Solon 2011; Wolfers 2006).

[8] M governs the maximum amount by which the slope of the pre-treatment differential trend can change between consecutive periods. Take an illustrative example consisting of three periods $t = -1, 0, 1$, in which individuals receive a treatment between periods $t = 0$ and $t = 1$; $Y_{it}(1)$ and $Y_{it}(0)$ denote the potential outputs of individual i in period t associated with the treatment and control conditions, respectively; and D_i is a dummy variable that equals one if the individual is entitled to the treatment. Let $\delta_1 = E[Y_{i,1}(0) - Y_{i,0}(0)|D_i = 1] - E[Y_{i,1}(0) - Y_{i,0}(0)|D_i = 0]$ be the post-treatment differential trend (note that it does not include the treatment effect) and $\delta_0 = E[Y_{i,-1}(0) - Y_{i,0}(0)|D_i = 1] - E[Y_{i,-1}(0) - Y_{i,0}(0)|D_i = 0]$ be the pre-treatment differential trend. M is therefore defined so that $\delta_1 - \delta_0 \leq M$. See Rambachan and Roth (2019) for further details.

Figure 4 – Yearly Average ENEM Objective Score (2010 = 100). Control Group: White Public School Students; Treatment Group: Non-white Public School Students



At the same time, we again use Rambachan and Roth (2019) to evaluate the robustness of the model's results to deviations in the parallel trends assumption. To this end, we estimate the following dynamic event-study regression:

$$\log(S_{it}) = c_0 + \phi_t + \lambda D_i^r + \gamma X_{it} + \sum_{s \neq 2012} \beta_s \times \mathbb{1}[t = s] \times D_i^r + \varepsilon_{it} \qquad (2)$$

where our universe is now comprised solely of public high school students; D_i^r is a dummy variable that equals one if the student is non-white (i.e., if they belong to the treatment group); and the remaining variables are the same as for Equation 1.

Furthermore, for the racial component model, we will also test for the heterogeneous effects of the law on the different subjects of the exam, between genders and between students with and without college-educated parents. Additionally, we will estimate both a two-periods difference-in-differences model and a regression with dynamic treatment effects.

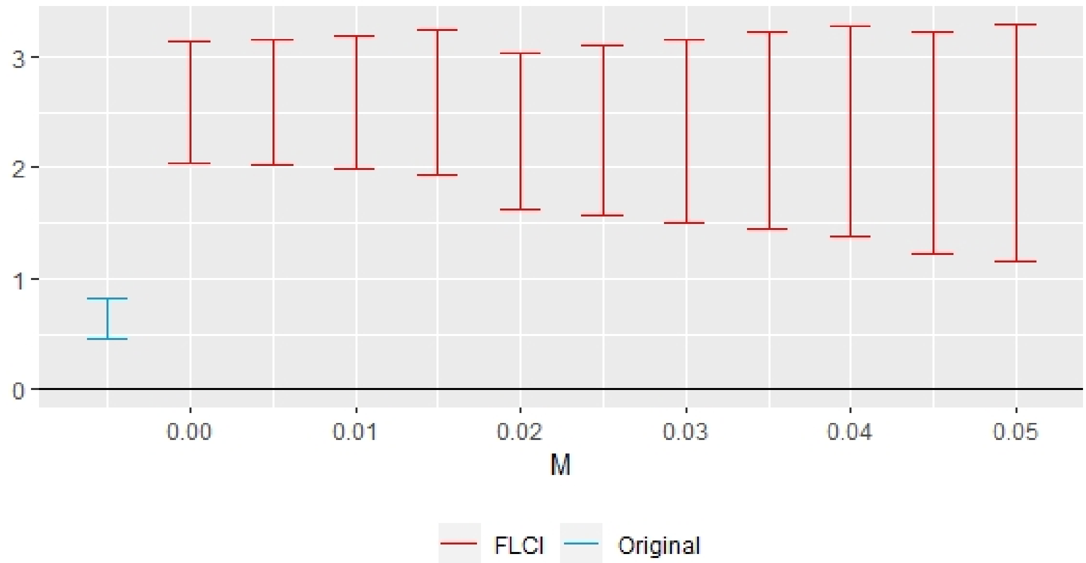
5. Results

5.1. The school component model

In this subsection, we present the results of our first model, which evaluates the impact of the school component of the Law of Quotas. The treatment group in this model is composed of white students from public schools (affected solely by the school component of the law) and the control group is composed of students from private schools (unaffected by the law).

Figure 5 plots set-identified estimations of the treatment effect $\hat{\beta}_{2016}$ (i.e., the effect in the final year for the law to be fully implemented) among different deviations of the pre-existing differential trend – in the figure, FLCI stands for Rambachan and Roth's optimal fixed length confidence interval. The entire set of estimated coefficients from Equation 1 is presented in Table A.1 in the Appendix. The original OLS estimate in Figure 5 shows the estimated treatment effect assuming that the parallel trend holds, while the remaining estimates consider linear extrapolations of the pre-treatment differential trend (if $M = 0$, then the linear extrapolation holds exactly, while $M > 0$ accounts for changes in the slope of the pre-treatment differential trend). The figure shows that the effect of the school component on the students' ENEM scores is positive under the entire set of violations considered (up to $M = 0.05$). In fact, we would need M to be as large as 0.11 (that is, the slope of the pre-treatment differential trend would have to change by 0.11 between years in the post-treatment period) in order to reject the null hypothesis that the treatment effect is significant (and, in this case, positive).

Figure 5 – School Component Model Treatment Effect ($\hat{\beta}_{2016}$) Sensitivity to Parallel Trends Violations. Original = OLS estimation; FLCI = Optimal Fixed Length Confidence Interval



The bottom line is that we can argue with reasonable confidence that the school component of the law did indeed have a positive impact on the eligible students' ENEM scores, a result that is robust to significant variations in the extrapolation of the pre-treatment differential trend (within the range of M evaluated in Figure 5, we estimate a treatment effect that ranges from a little above 1% to almost 4%).

5.2. The racial component model

Our second model evaluates the impact of the racial component of the Law of Quotas. As previously mentioned, the treatment group in this model is composed of non-white students from public schools (affected by both the school and racial components of the law), while the control group is composed of white students from public high schools (affected by the school component but not by the racial one).

As in the previous subsection, we begin by presenting the estimations of the effect of the policy (the racial component) and the sensitivity analysis of the treatment coefficient $\hat{\beta}_{2016}$ among different values of M. Again, the entire set of estimated coefficients in Equation 2 is presented in the Appendix (Table A.2).

Figure 6 – Racial Component Model Treatment Effect ($\hat{\beta}_{2016}$) Sensitivity to Parallel Trends Violations. Original = OLS estimation; FLCI = Optimal Fixed Length Confidence Interval

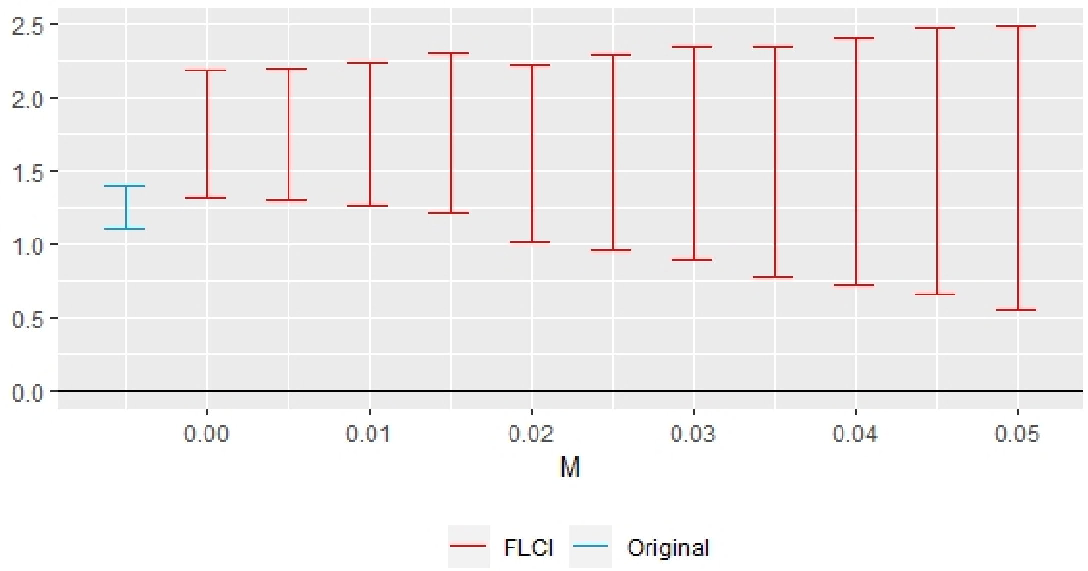


Figure 6 indicates that the racial component of the law also had a positive effect on the treatment group's ENEM scores (varying from approximately 0.5% to 2.5% within the range of M considered in the analysis). Again, this effect is robust to significant deviations of the pre-treatment differential trend's linear extrapolation. Furthermore, there is no statistical difference between the OLS original estimation and the set-identified estimation with positive M, which is yet another indication that the parallel trends assumption seems to hold better in this case. Therefore, in the following subsections, we explore the effects of the racial component of the law in further detail by making use of the OLS original estimations (that is, assuming that the parallel trends hold exactly). First (5.2.1), we present the results of a standard two periods difference-in-differences model, in which we estimate the average treatment effect for the entire post-treatment period (2013 to 2016) – more precisely, we contrast the results of a model with and without the set of control variables . Then, we test for heterogeneous effects by subject of the ENEM exam, gender and parental education (5.2.2). Third, we estimate a difference-in-differences model with dynamic treatment effects to investigate whether the impact of the law varied throughout the years (5.2.3). Finally, the results for a set of robustness checks are presented in 5.2.4. Except when stated otherwise, all the models in these next subsections make use of the same control and treatment groups as the ones in the current subsection (i.e., the racial component model control and treatment groups).

5.2.1. Standard two-periods model

We start by estimating a standard two-periods model, in which we divide our timespan into a pre-treatment period (2010-2012) and a post-treatment period (2013-2016). We contrast a model containing the set of control variables from Table 2 with a model without controls. Hence, the following two regression frameworks are estimated:

$$\log(S_{it}) = c_0 + \Phi W + \lambda D_i^r + \beta D_i^r W + \varepsilon_{it} \tag{3}$$

$$\log(S_{it}) = c_0 + \Phi W + \lambda D_i^r + \gamma X_{it} + \beta D_i^r W + \varepsilon_{it} \tag{4}$$

Once again, S_{it} denotes ENEM total score on the multiple choice exam of a student i that took the exam in a specific year t; D_i^r is a dummy variable that equals one if the student belongs to the treatment group; $\hat{\Phi}$ and $\hat{\lambda}$ measure the time-specific and group-specific fixed effects, respectively; and X_{it} includes the student-specific control variables described in Table 2. We then introduce W, which is a dummy variable that equals one if $t \geq 2013$ (that is, if it belongs to the post-treatment period), and finally the coefficient $\hat{\beta}$ measures the average treatment effect throughout the entire post-treatment period.

The first column in Table 4 presents the results of the regression specified in Equation 3 (i.e., without the set of control variables), while the second column contains the results of the regression specified in Equation 4 (i.e., with controls). The table shows a positive and significant estimated treatment effect that does not change significantly between the short and the augmented models, despite a substantial increase in the R-squared. Oster (2019) suggests a test for unobservable variable bias based on Altonji, et al. (2005) which makes use precisely of these two data (the change in coefficients and R-squared between the regression with and without control variables). Oster (2019) demonstrates that if the selection of the observed and unobserved controls is proportional, we are able to compute an identified set for the coefficient of interest, and then test whether this set includes zero. We follow Oster's recommended specification with $R_{max} = 1$, so that the bounding set becomes $[\tilde{\beta}, \beta^*(1,1)]$, where $\beta^*(1,1) = \tilde{\beta} - \frac{(\dot{\beta}-\tilde{\beta})(1-\tilde{R})}{\tilde{R}-\dot{R}}$ [9]. The recommended bounding set in our case is [0.0078, 0.0104], which safely excludes zero, thus providing evidence that the significant estimated treatment effects observed in Table 4 are not driven by non-observable factors.

In the augmented model, we estimate an average treatment effect of 1.04%. In other words, this model suggests that the racial component of the Law of Quotas induced eligible students to attain a 1.04% higher score in the ENEM exam, on average, during 2013 to 2016. For the reader's convenience, from this point forward, all the regressions to be presented and discussed include the entire set of control variables.

Table 4 - Racial Component Model without control variables: Standard Two Periods Regression (all coefficients multiplied by 100)

Independent Variables	(1)	(2)
Group Fixed Effect	-4.475 *** (0.037)	-2.088 *** (0.039)
Time Fixed Effect	0.440 *** (0.039)	0.320 *** (0.036)
Treatment Effect	1.093 *** (0.050)	1.044 *** (0.046)
Control	No	Yes
Observations	1,124,157	1,124,157
R^2	2.31%	18.11%

Robust standard errors in parenthesis
* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

5.2.2. Heterogeneous effects

We now use the same two-periods design as in 5.2.1. to investigate whether there were heterogeneous effects of the racial component of the law by (i) subject of the exam; (ii) gender; and (iii) level of parental education.

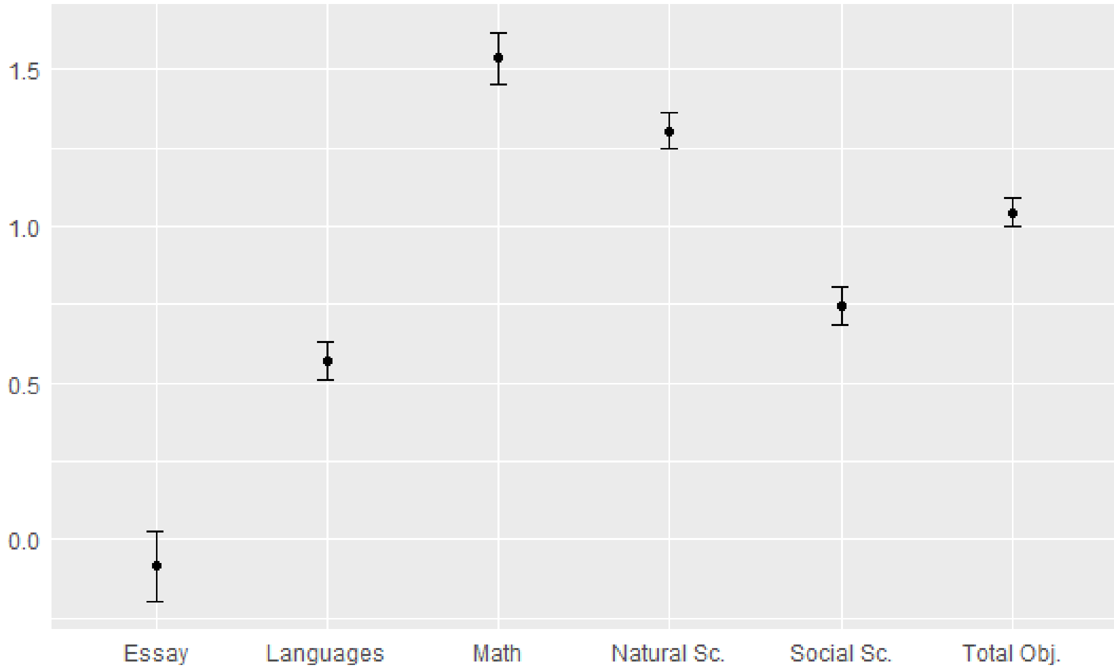
First, we examine the effects per subject. As mentioned in subsection 3.3, the ENEM consists of one multiple choice exam (containing four different disciplines: natural sciences, social sciences, languages and math) and one essay. Thus, we estimate five separate regressions, so that in each model, the response variable is the student's score in each of the five tests. Figure 7 displays the

[9] $\tilde{\beta}$ and $\tilde{R}$ account for the treatment coefficient and R-squared in the augmented regression, $\dot{\beta}$ and $\dot{R}$ for the treatment coefficient and R-squared in the short regression, and R_{max} is the R-squared of a hypothetical model that includes both observed and unobserved controls. See Oster (2019) for further details.

estimated treatment effect coefficient per subject plus the one for the overall objective exam already presented in subsection 5.2.1.

The effects of the racial component of the law were greater in more quantitative-intensive fields (natural sciences and mathematics). Actually, the estimated treatment effect coefficient for the essay is not statistically significant, indicating that the racial component of the law might not have had any effect on this specific part of the exam. The estimated coefficients for the equations summarized in Figure 7 are presented in Tables A.3 to A.7 in the Appendix.

Figure 7 - Treatment Effect Coefficient (multiplied by 100) per ENEM Subject



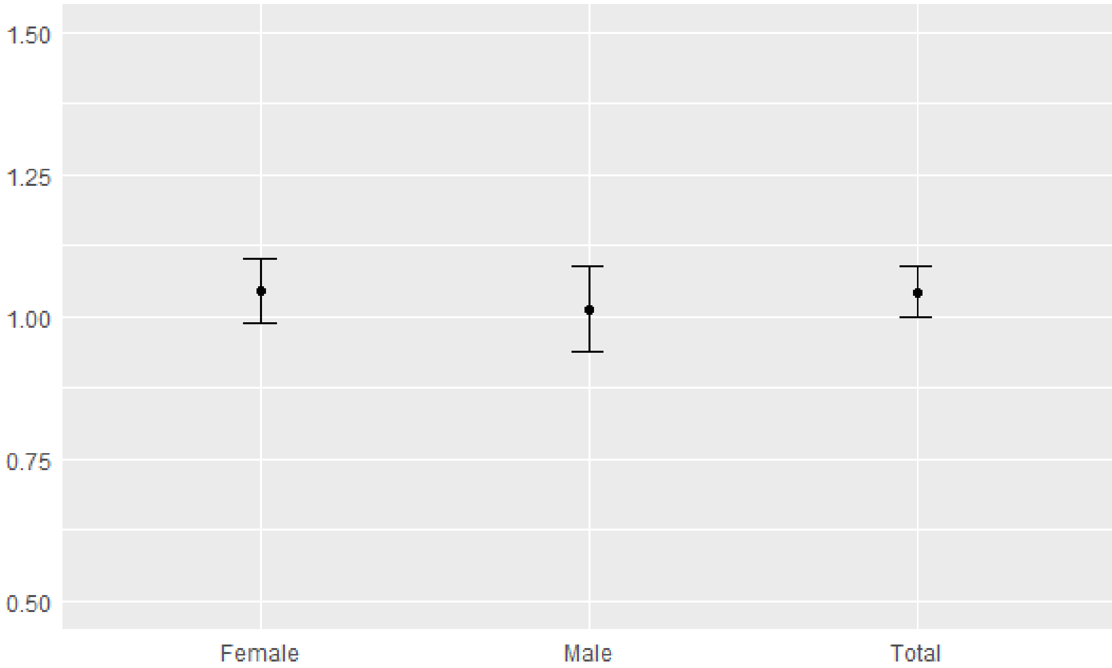
Second, we assess the possibility of heterogeneous effects of the racial component between genders. To this end, we estimate a two-periods difference-in-differences regression adding interactions of the models' variables by gender. The model is specified as follows:

$$\log(S_{it}) = c_0 + \Phi W + \Phi^m W D_i^m + \lambda D_i^r + \lambda^m D_i^r D_i^m + \gamma X_{it} + \gamma^m X_{it} D_i^m + \beta D_i^r W + \beta^m D_i^r D_i^m W + \varepsilon_{it} \tag{5}$$

where D_i^m is a dummy variable that equals one if the student is male and $\Phi^m, \lambda^m, \gamma^m$ and β^m refer to the incremental time-fixed, group-fixed, control variables and treatment effects, respectively, for male individuals. The remaining variables are the ones explained in section 4.2., including the response variable, which is the total score on the objective exam.

Figure 8 presents the estimated treatment effect by gender. The coefficients are not statistically different (at a 5% significance level) between males and females and they are also not statistically different from the treatment effect already estimated in section 5.2.1. ($\hat{\beta} = 1.044$). Table A.8 in the Appendix contains the remaining estimated coefficients from Equation 5.

Figure 8 – Treatment Effect Coefficient (multiplied by 100) per Gender



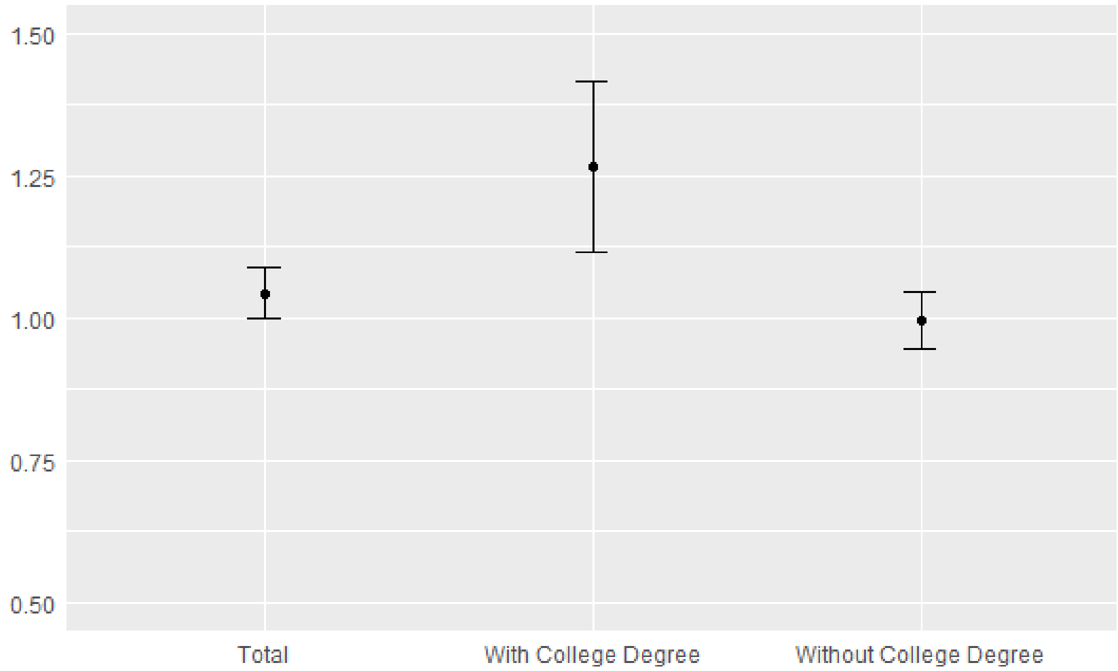
Third, we address whether the racial component of the law had different impacts on students whose parents hold different levels of education. Therefore, we estimate the following equation:

$$\log(S_{it}) = c_0 + \Phi W + \Phi^c W D_i^c + \lambda D_i^r + \lambda^c D_i^r D_i^c + \gamma X_{it} + \gamma^c X_{it} D_i^c + \beta D_i^r W + \beta^c D_i^r D_i^c W + \varepsilon_{it} \quad (6)$$

where D_i^c is a dummy variable that equals one if either the mother or the father of the student holds a college degree and $\Phi^c, \lambda^c, \gamma^c$ and β^c refer to the incremental time-fixed, group-fixed, control variables and treatment effects, respectively, for these students. Again, the remaining variables are the ones explained in section 4.2. and the response variable is the total score on the objective exam.

Figure 9 displays the estimated treatment effect by level of parental education. The coefficients are not statistically different between students with and without college-educated parents and they are also not statistically different from the treatment effect already estimated in section 5.2.1. (again, at a 5% significance level; however, the estimated treatment effects are different among levels of parental education at a 10% significance level, with a p-value of 8.62%). Table A.9 in the Appendix contains the remaining estimated coefficients from Equation 6.

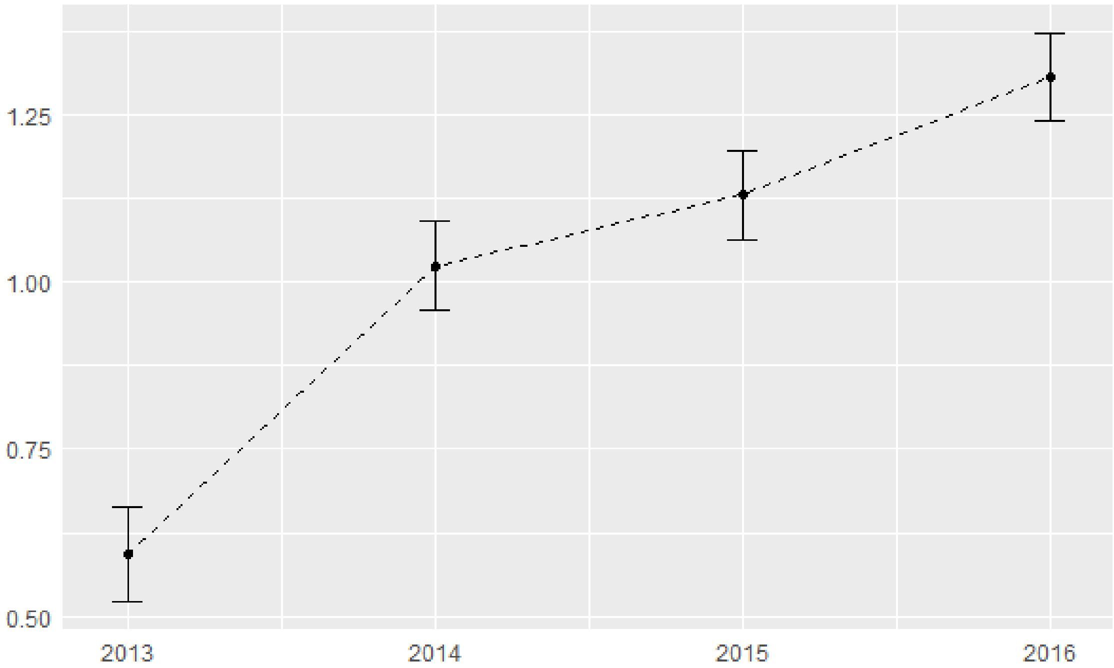
Figure 9 – Racial Component Model per Level of Parental Education (all coefficients multiplied by 100)



5.2.3. Dynamic treatment effects

In this subsection, we investigate whether the effect of the racial component of the Law of Quotas evolved over time. For this purpose, we estimate a dynamic event-study regression such as the one in Equation 2 (using the total score on the ENEM's multiple choice exam as the response variable). The only difference in this case is that we are assuming that parallel trends hold exactly. Therefore, we normalize all of the pre-treatment betas (i.e. $\hat{\beta}_{2010}$ to $\hat{\beta}_{2012}$) to zero and assume that the post-treatment betas (i.e. $\hat{\beta}_{2013}$ to $\hat{\beta}_{2016}$) measure solely the dynamic treatment effect. The dynamic treatment effect coefficients are presented in Figure 10, which indicates that the effect of the law on the students' ENEM scores in the objective exam increased throughout the first years after its implementation (the coefficients $\hat{\beta}_{2014}$ and $\hat{\beta}_{2015}$ are not statistically different at a 5% significance level, but the remaining coefficients in fact are). The entire set of estimated coefficients is presented in Table A.10 in the Appendix.

Figure 10 - Treatment Effect (multiplied by 100) throughout the Years



5.2.4. Robustness

Finally, we conduct four robustness exercises in order to further qualify the findings from the racial component model. First, we check for the anticipatory effects of the law; then, we perform two placebo tests: a first one using only private school students (who are not eligible for the Law of Quotas at all); and a second one using 2017, in which the law had already been fully implemented, as the cutting year in the difference-in-differences model. Finally, we use an alternative database to estimate the effects of the Law of Quotas on non-white students' high school completion rate – using the same two-periods difference-in-differences framework presented in previous subsections.

We start by checking for anticipatory effects of the treatment, that is, we check whether the law had any effect on students' ENEM scores before it was implemented (prior to 2013). First, it should be noted that it is unlikely that there should be any anticipatory effects, since (i) the law was published on August 29th, 2012, two months before that year's exam, and (ii) the law stated that the quotas (or at least a share of them) should be implemented by the institutions only from 2013 onwards. A visual inspection of Figure 4 suggests that an increase in the scores of the treatment group (supposedly due to the treatment effect) was found only in 2013, which would rule out the possibility of anticipatory effects. Nevertheless, we also estimate a two periods model following Equation 4 but excluding the years right before and right after the law's implementation (2012 and 2013) from the regression. Table A.11 in the appendix presents the results of this model. The treatment effect coefficient remains significant and very close to the coefficient estimated in the augmented model from section 5.2.1. (Table 4), which strengthens the hypothesis that there were indeed no anticipatory effects.

In our second robustness exercise, we perform a placebo test using only students from private high schools in both the treatment and control groups. Our concern here is that the increase in the ENEM score of non-white students is driven by some other factor other than the Law of Quotas, such as noisy data or some unobserved racial driver. Therefore, we estimate a two periods difference-in-differences model using non-white students from private schools as the treatment group and white students from private schools as the control group. Despite the racial difference between the groups, both of them are private school students, which means that they are not eligible for the quotas and we should not see any significant treatment effect. The results of this estimation are presented in Table A.12 in the appendix. The treatment effect coefficient in this case is insignificant, as shown in the table (p-value of 28%), suggesting that indeed there has been no effect of the law on private school students.

Third, we perform another placebo test, in which we use 2017 as the cutting year in the difference-in-differences model. In section 5.2.3, we found that the effects of the racial component of the law increased in each year up to 2016. From 2017 forward, however, it would be reasonable to see a stabilization of the effects of the law, due to the number of years since its implementation and to the fact that by 2017 the quotas had already been fully implemented by all institutions. Therefore, we again estimate a two periods difference-in-differences regression in which the post-treatment period comprises the years of 2017 and 2018. Table A.13 in the appendix presents the estimated coefficients. These results shall be taken carefully since this model does not include the work factor and previous academic effort factor variables due to changes in the ENEM's questionnaire (yet another reason why these years were not encompassed in the models from the previous sections). Nevertheless, the estimated treatment effect coefficient is insignificant in this case (p-value of 71%), which suggests both that the results obtained were not merely a placebo effect and that by 2017 the impacts of the law had completely stabilized.

Lastly, we perform a final exercise in which we use a similar difference-in-differences framework as the one in subsection 5.2.1 (a two-periods regression) so as to evaluate the impacts of the Law of Quotas on the high school completion rate of non-white individuals. For this estimation, however, rather than working with the ENEM microdata, we make use of IBGE's PNAD (*Pesquisa Nacional por Amostra de Domicílios*) – Brazil's national household sample survey, a yearly repeated cross-sectional database with information on housing, demography, migration, education, labor and income at both individual and household levels. We focus our analysis on young individuals of high-school graduate age (18 to 24 years old) and at the years from 2011 to

2015[10]. We then estimate a logistic difference-in-differences regression in which the output of interest is a dichotomous variable indicating whether the student completed high school. The treatment group in the regression is composed of non-white individuals and the control group by white individuals. Finally, we also add to the model a set of control variables from the PNAD database, more specifically, we control for: age, gender, state of residence, per capita family income, degree of ruralization and work factor – the last two variables are similar to the ones described in Table 2. Figure A.1 exhibits the evolution of the high school completion rate for the treatment and control groups, while Table A.14 presents the results of the estimation (both displayed in the appendix). The table shows that the treatment effect is positive and significant, providing evidence that the Law of Quotas had a positive effect on non-white students' pre-college effort (as measured by their high school completion rate), and hence yielding further robustness to our previous findings.

6. Discussion

The results obtained suggest that both the school component and the racial component of the Law of Quotas induced eligible students to attain higher scores on the ENEM exam. The presence of a pre-treatment differential trend in the first model hampers a more detailed evaluation of the impacts of the school component of the law, but the sensitivity analysis indicates that the significance of the treatment effect is robust to substantial violations in the parallel trends assumption. Since we are unsure of the direction and magnitude of the post-treatment differential trend, we shall not venture further into this result.

For the racial component model, however, we were able to explore the results in more depth. First, we estimated a standard two-periods difference-in-differences model, which indicated that the racial component of the Law of Quotas induced eligible students to attain a 1.04% higher score in the ENEM exam, on average, during 2013 to 2016.

Second, we checked for heterogeneous effects, the main findings being: i) the effect of the law was stronger in quantitative-intensive subjects (Math and Natural Sciences) than it was in the remaining fields (Language, Social Sciences and the Essay); and ii) the racial component of the Law of Quotas did not exert statistically significant differences by gender nor parental education. A possible explanation to the former might be that quantitative-intensive subjects might be less dependent on socioeconomic background (in other words, hours of self-study for the ENEM exam

[10] We abstract from 2010 since the survey was not conducted in that year due to the 2010 Census and from 2016, since from that year onwards the PNAD was replaced by its latest version, the PNAD *Contínua*

in mathematics are less conditioned to the students' social and home environment). Indeed, a number of research studies have suggested that math achievements tend to be more sensitive to teachers and schools' efficiency gains, while reading/linguistic achievements might be more dependent on socioeconomic status and parental occupation and/or involvement at school (Perry and McConney 2013; Cheadle 2008; Rimm-Kaufman et al. 2007; Sui-Chu and Willms 1996).

Third, we have also estimated a difference-in-differences model with dynamic treatment effects in order to evaluate whether the impact of the law evolved throughout the years after its implementation. We found that the treatment effect indeed increased from 2013 to 2016 and, therefore, this appears to be a case in which the effect of the policy intervention depends on the length of exposure to it. That is, while a student from the treatment group that took the exam in 2016 had four years to absorb the effects of the treatment and increase their investment in human capital, an individual that took the exam in 2013 had only one year to do so. An alternative and perhaps complementary explanation is that the increasing treatment effect is due to the design of the Law of Quotas. Since the law stated that universities had until 2016 to fully implement the quotas, the share of reserved seats presented an upward trend from 2013 to 2016 (see Figure 2), which could explain part of the dynamic observed in Figure 10. In any case, the "incentive" effect of the policy clearly outweighed the possible "relaxation" effect on students of the increase in the number of seats.

A possible concern that could arise from the estimation of the school component model is that the Law of Quotas could have increased competition for seats among private school students and therefore have impacted their pre-college performance as well, which would put the suitability of the control group at stake. However, the reduction of available seats for these individuals due to the law's implementation was attenuated by an overall increase in the number of seats in federal universities by 41% from 2012 to 2016. As we can see in Figure 3, the average ENEM score among private school students did not present significant changes after the introduction of the law (it may have decreased from 2010 to 2011 but remains reasonably stable thereafter). In order to qualify this hypothesis, we also estimated a regression for private school students only from 2011 to 2016 (excluding the drop in scores from 2010 to 2011 - before the Law of Quotas) with a time-fixed effect dummy that equals one from 2013 onwards (that is, after the law was implemented) and found that this coefficient is insignificant (p-value of 33%), which corroborates the hypothesis that the law did not affect the private students' scores. Table A.15 in the Appendix presents the results of this estimation. In the same manner, we assume that this 41% increase in the overall number of seats from 2012 to 2016 also mitigated any increase in competition for seats among white students from public schools that might have arisen from the racial component of the law.

Finally, our results suggest that the positive incentives provided by affirmative action, such as the mitigation of the discouragement effects described by Cotton et al. (2016) and Furstenberg (2003), might have prevailed over any negative incentive effect discussed by the theoretical literature that might have stemmed from the policy. Although the empirical investigations that have been previously performed were limited and mainly focused on specific universities, most of them pointed towards a positive effect of higher education quotas on pre-college effort and academic performance as well. Hence, our results both corroborate and strengthen these previous findings.

These results have strong policy implications as they indicate that educational quotas not only enhance the participation of disadvantaged groups in higher education directly through an increased number of seats but also by encouraging these individuals to invest in human capital and close the performance gap by the end of secondary education. Therefore, this behavioral response to the implementation of quotas should not be overlooked and should be taken into account by policymakers, especially in developing economies with a high level of inequality in education.

7. Conclusion

Several different measures have been implemented in recent years in an attempt to mitigate racial inequalities in education. One sort of intervention has been the establishment of reserved seats in higher education to specific racial groups and, although there is a rich body of evidence that investigates the ex-post effects of these quotas, little research has been done with respect to the effects that they have on pre-college academic performance. We contribute to this literature by evaluating how the Brazilian 2012 Law of Quotas affected the performance of students on the college-entrance exam, the ENEM.

Our results suggest that both the school component and the racial component of the Law of Quotas fostered incentives to pre-college human capital accumulation as it induced eligible students to attain higher scores on the ENEM exam. Indeed, the positive effects of the racial component of the law increased throughout the first years after its implementation.

Furthermore, we have also tested for the presence of heterogeneous effects of the racial component of the law across a set of different dimensions. While racial quotas had a larger effect on the scores of quantitative-intensive subjects than it had on linguistic/humanities related subjects, no evidence of heterogeneous effects was found by gender or parental education.

Although robustness exercises scaffold the validity of our results, we acknowledge some limitations in our strategy. First, we had to take an indirect strategy for controlling for the income criteria of the quotas, due to data restrictions. Second, we have controlled for a set of observable individual and socioeconomic characteristics, others remaining as non-observable. Third, since the ENEM's microdata does not disclose information on each candidate's SISU's application, we were not able to control for the actual university the students finally enrolled at (or at least were accepted in). Nevertheless, sensitivity analyses allow us to provide strong evidence that the Law of Quotas implemented in Brazil did indeed encourage eligible students to increase their pre-college academic performance (i.e., that the introduction of quotas in higher education not only promotes equity, but also brings about efficiency gains). Thus, this research helps to shed some light on the incentives provided by quotas in higher education and hence might serve as a guide to educators and policy makers whose aim is not only to increase the equality of educational opportunity, but also the efficiency of their educational system.

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Appendix

Figure A.1 – High School Completion Rate (PNAD classification - 2010 = 100). Control Group: White Individuals; Treatment Group: Non-white individuals

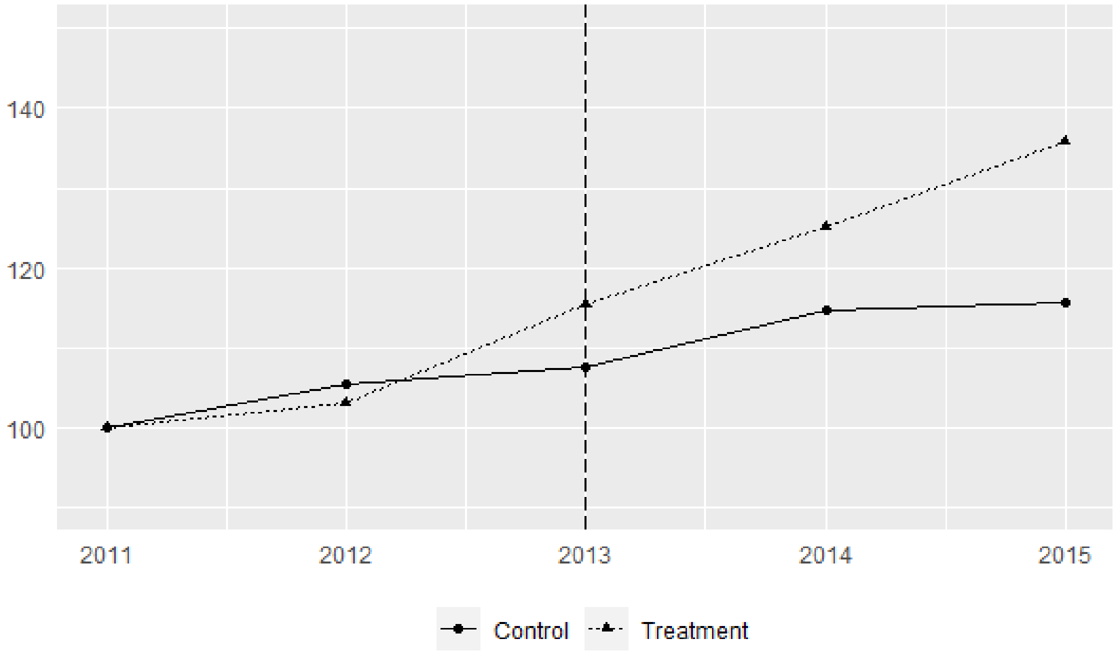

Table A.1 – School Component Model Dynamic Event Study Regression (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Dropout/Grade Repetition	-4.234 *** (0.041)
State	Yes ***	Group Fixed Effect	-7.330 *** (0.085)
Parental Education	Yes ***	Beta 2010	0.856 *** (0.112)
Age	0.005 (0.003)	Beta 2011	-0.263 * (0.114)
Gender (M=1)	3.637 *** (0.030)	Beta 2013	0.102 (0.112)
Average Income	1.950 *** (0.016)	Beta 2014	1.049 *** (0.112)
Ruralization	-6.528 *** (0.102)	Beta 2015	0.959 *** (0.110)
Work Factor	-0.902 *** (0.034)	Beta 2016	0.643 *** (0.111)
Observations	711,355		
R^2	30.62%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.2 – Racial Component Model Dynamic Event Study Regression (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Dropout/Grade Repetition	-2.667 *** (0.029)
State	Yes ***	Group Fixed Effect	-2.051 *** (0.064)
Parental Education	Yes ***	Beta 2010	0.189 * (0.090)
Age	-0.033 *** (0.002)	Beta 2011	-0.263 ** (0.090)
Gender (M=1)	3.812 *** (0.023)	Beta 2013	0.540 *** (0.086)
Average Income	3.943 *** (0.039)	Beta 2014	0.970 *** (0.084)
Ruralization	-5.231 *** (0.068)	Beta 2015	1.075 *** (0.084)
Work Factor	0.728 *** (0.026)	Beta 2016	1.252 *** (0.083)
Observations	1,124,157		
R^2	18.48%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.3 – Racial Component Two Periods Model per ENEM Subject – Languages (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-7.315 *** (0.093)
State	Yes ***	Work Factor	0.628 *** (0.035)
Parental Education	Yes ***	Dropout/Grade Repetition	-2.582 *** (0.041)
Age	-0.049 *** (0.003)	Group Fixed Effect	-1.543 *** (0.043)
Gender (M=1)	-0.318 *** (0.032)	Time Fixed Effect	-0.336 *** (0.047)
Average Income	3.421 *** (0.039)	Treatment Effect	0.568 *** (0.060)
Observations	1,124,157		
R^2	7.89%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.4 – Racial Component Two Periods Model per ENEM Subject – Math (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-4.273 *** (0.119)
State	Yes ***	Work Factor	0.728 *** (0.046)
Parental Education	Yes ***	Dropout/Grade Repetition	-3.388 *** (0.054)
Age	-0.202 *** (0.004)	Group Fixed Effect	-3.201 *** (0.059)
Gender (M=1)	8.183 *** (0.043)	Time Fixed Effect	-4.942 *** (0.064)
Average Income	4.843 *** (0.054)	Treatment Effect	1.536 *** (0.081)
Observations	1,124,157		
R^2	13.25%		

Robust standard errors in parenthesis
* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.5 – Racial Component Two Periods Model per ENEM Subject – Natural Sciences (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-3.726 *** (0.083)
State	Yes ***	Work Factor	0.026 (0.033)
Parental Education	Yes ***	Dropout/Grade Repetition	-2.282 *** (0.037)
Age	0.014 *** (0.003)	Group Fixed Effect	-2.218 *** (0.046)
Gender (M=1)	4.185 *** (0.029)	Time Fixed Effect	0.556 *** (0.044)
Average Income	3.767 *** (0.040)	Treatment Effect	1.302 *** (0.057)
Observations	1,124,157		
R^2	10.56%		

Robust standard errors in parenthesis
* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.6 – Racial Component Two Periods Model per ENEM Subject – Social Sciences (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-5.355 *** (0.089)
State	Yes ***	Work Factor	1.533 *** (0.033)
Parental Education	Yes ***	Dropout/Grade Repetition	-2.638 *** (0.038)
Age	0.081 *** (0.003)	Group Fixed Effect	-1.459 *** (0.049)
Gender (M=1)	3.093 *** (0.029)	Time Fixed Effect	5.271 *** (0.045)
Average Income	4.140 *** (0.042)	Treatment Effect	0.746 *** (0.059)
Observations	1,124,157		
R^2	13.14%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.7 – Racial Component Two Periods Model per ENEM Subject – Essay (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-7.240 *** (0.168)
State	Yes ***	Work Factor	0.336 *** (0.061)
Parental Education	Yes ***	Dropout/Grade Repetition	-5.462 *** (0.073)
Age	-0.233 *** (0.005)	Group Fixed Effect	-2.970 *** (0.112)
Gender (M=1)	-4.675 *** (0.055)	Time Fixed Effect	-4.533 *** (0.115)
Average Income	4.412 *** (0.093)	Treatment Effect	-0.085 (0.110)
Observations	1,124,157		
R^2	7.06%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.8 – Racial Component Model per Gender (all coefficients multiplied by 100)

Independent Variables			
Male		Female	
Marital Status	Yes ***	Marital Status	Yes ***
State	Yes ***	State	Yes ***
Parental Education	Yes ***	Parental Education	Yes ***
Age	0.027 *** (0.004)	Age	-0.071 *** (0.002)
Average Income	3.701 *** (0.050)	Average Income	4.326 *** (0.062)
Ruralization	-6.712 *** (0.117)	Ruralization	-4.295 *** (0.083)
Work Factor	0.477 *** (0.045)	Work Factor	0.884 *** (0.031)
Dropout/Grade Repetition	-2.870 *** (0.046)	Dropout/Grade Repetition	-2.551 *** (0.037)
Group Fixed Effect	-2.275 *** (0.065)	Group Fixed Effect	-1.967 *** (0.048)
Time Fixed Effect	-0.397 *** (0.045)	Time Fixed Effect	0.806 *** (0.045)
Observations	1,124,157		
R^2	18.38%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Note: For ease of exposition, we present the net coefficient for each gender (i.e., in the "Female" column we present the coefficients from Equation 5 that do not contain the "m" suffix, whereas in the "Male" column we present these same coefficients plus the incremental coefficients for males)

Table A.9 – Racial Component Model per Level of Parental Education (all coefficients multiplied by 100)

Independent Variables			
With College Degree		Without College Degree	
Marital Status	Yes ***	Marital Status	Yes ***
State	Yes ***	State	Yes ***
Parental Education	Yes ***	Parental Education	Yes ***
Age	0.081 *** (0.009)	Age	-0.041 *** (0.002)
Average Income	3.197 *** (0.063)	Average Income	4.332 *** (0.049)
Ruralization	-10.661 *** (0.226)	Ruralization	-4.537 *** (0.071)
Work Factor	-1.290 *** (0.082)	Work Factor	0.983 *** (0.027)
Dropout/Grade Repetition	-3.219 *** (0.107)	Dropout/Grade Repetition	-2.635 *** (0.030)
Group Fixed Effect	-2.597 *** (0.126)	Group Fixed Effect	-1.988 *** (0.040)
Time Fixed Effect	-0.132 (0.105)	Time Fixed Effect	0.387 *** (0.039)
Observations	1,124,157		
R^2	18.32%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Note: For ease of exposition, we present the net coefficient for each gender (i.e., in the "Without College Degree" column we present the coefficients from Equation 6 that do not contain the "c" suffix, whereas in the "With College Degree" column we present these same coefficients plus the incremental coefficients for students with a college-educated parent)

Table A.10 – Racial Component Model with Dynamic Treatment Effect (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Work Factor	0.766 *** (0.026)
State	Yes ***	Dropout/Grade Repetition	-2.664 *** (0.029)
Parental Education	Yes ***	Group Fixed Effect	-2.093 *** (0.039)
Age	-0.033 *** (0.002)	Beta 2013	0.593 *** (0.070)
Gender (M=1)	3.801 *** (0.023)	Beta 2014	1.024 *** (0.068)
Average Income	4.018 *** (0.040)	Beta 2015	1.129 *** (0.068)
Ruralization	-5.219 *** (0.068)	Beta 2016	1.306 *** (0.066)
Observations	1,124,157		
R^2	18.31%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.11 - Robustness Check: Anticipation of Treatment Effect Regression (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-4.876 *** (0.079)
State	Yes ***	Work Factor	0.997 *** (0.03)
Parental Education	Yes ***	Dropout/Grade Repetition	-2.693 *** (0.034)
Age	-0.040 *** (0.002)	Group Fixed Effect	-2.170 *** (0.047)
Gender (M=1)	3.681 *** (0.027)	Time Fixed Effect	0.467 *** (0.043)
Average Income	3.807 *** (0.043)	Treatment Effect	1.172 *** (0.056)
Observations	807,063		
R^2	17.71%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.12 - Robustness Check: Placebo Test with Private School Students Regression (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-8.215 *** (0.26)
State	Yes ***	Work Factor	-2.136 *** (0.069)
Parental Education	Yes ***	Dropout/Grade Repetition	-7.353 *** (0.104)
Age	0.023 ** (0.007)	Group Fixed Effect	-2.199 *** (0.092)
Gender (M=1)	2.845 *** (0.053)	Time Fixed Effect	-0.591 *** (0.066)
Average Income	1.309 *** (0.016)	Treatment Effect	0.122 (0.112)
Observations	214,501		
R^2	24.82%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.13 - Robustness Check: Placebo Test with 2017 as the Treatment Year Regression (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-4.477 *** (0.063)
State	Yes ***	Work Factor	No
Parental Education	Yes ***	Dropout/Grade Repetition	No
Age	-0.035 *** (0.002)	Group Fixed Effect	-1.799 *** (0.048)
Gender (M=1)	3.27 *** (0.022)	Time Fixed Effect	3.543 *** (0.039)
Average Income	3.787 *** (0.053)	Treatment Effect	0.018 (0.048)
Observations	1,122,718		
R^2	18.16%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.14 - Robustness Check: Two-Periods Difference-in-differences Logistic Regression – Output = High School Completion

Independent Variables			
State	Yes ***	Work Factor	0.077 *** (0.013)
Ruralization	Yes ***	Group Fixed Effect	-0.970 *** (0.021)
Per Capita Family Income	Yes ***	Time Fixed Effect	0.154 *** (0.017)
Age	0.236 *** (0.003)	Treatment Effect	0.118 *** (0.026)
Gender (F=1)	0.390 *** (0.013)		
Observations	205,285		
Nagelkerke R^2	14.35%		

Standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level

Table A.15 –Model with only Private School Students without Treatment Effect and without the Year 2010 (all coefficients multiplied by 100)

Independent Variables			
Marital Status	Yes ***	Ruralization	-8.116 *** (0.28)
State	Yes ***	Work Factor	-2.197 *** (0.075)
Parental Education	Yes ***	Dropout/Grade Repetition	-7.268 *** (0.104)
Age	0.026 ** (0.008)	Group Fixed Effect	-2.199 *** (0.111)
Gender (M=1)	2.817 *** (0.058)	Time Fixed Effect	-0.089 (0.091)
Average Income	1.456 *** (0.031)		
Observations	183,632		
R^2	24.73%		

Robust standard errors in parenthesis

* Significance at 5% level; ** Significance at 1% level; *** Significance at 0.1% level