
University of Brasilia

Economics and Politics Research Group

*A CNPq-Brazil Research Group*

*http://www.EconPolRG.wordpress.com*

Research Center on Economics and Finance–CIEF
Research Center on Market Regulation–CERME
Research Laboratory on Political Behavior, Institutions and Public Policy–LAPCIPP
Master's Program in Public Economics–MESP

# Black movement: Estimating the effects of affirmative action in college admissions on education and labor market outcomes

**Andrew Francis-Tan and Maria Tannuri-Pianto**

**Emory University and UnB**

**Economics and Politics Working Paper 63/2016**
**March, 30th, 2016**

**Economics and Politics Research Group**
**Working Paper Series**

# Black movement:
# Estimating the effects of affirmative action in college admissions on education and labor market outcomes

Andrew Francis-Tan*
Emory University

Maria Tannuri-Pianto
University of Brasilia

The recent adoption of race-targeted policies makes Brazil an insightful place to study affirmative action. In this paper, we estimate the effects of racial quotas at the University of Brasilia, which reserved 20% of admissions slots for persons who self-identified as black. To do so, we link the admissions outcomes of high-performing applicants in 2004-2005 to their education and labor market outcomes in 2012. We adopt methods that make use of sharp discontinuities in the admissions process. In summary, the policy of racial quotas mostly improved outcomes for the targeted group. Relative to quota applicants below the cutoff, quota applicants above the cutoff enjoyed an increase in years of education, college completion, and labor earnings. Those gains were concentrated among men and applicants in more selective majors. For the large part, mismatch was not prevalent. However, there was evidence of mismatch among those quota students in less selective majors.

Keywords: affirmative action, racial quotas, mismatch, educational policy, college quality, minorities, Brazil.

* Andrew Francis-Tan, Associate Professor of Economics and Adjunct Associate Professor of Sociology, Emory University, Atlanta, GA (andrew.francis@emory.edu). Maria Tannuri-Pianto, Associate Professor of Economics, University of Brasilia, Brazil (tannuri@unb.br).

## 1. Introduction

Affirmative action policies – which aim to promote the status of historically disadvantaged groups in educational and labor market settings – have roots in 1960s America (Holzer and Neumark 2000, Arcidiacono, Lovenheim, and Zhu 2015). After decades of expansion, court rulings and state-specific laws have diminished the scope of these policies since the 1990s. While affirmative action may be on the decline in the United States, it is on the rise in other countries including India, South Africa, Israel, China, Malaysia, and Brazil (Darity, Deshpande, and Weisskopf 2011). Most of the scholarly literature concerns the U.S. Yet, certain policy contexts outside the U.S. are better suited for identifying the causal effects of affirmative action.

We study affirmative action in Brazil. About 10 times more slaves arrived in Brazil than in British Mainland North America (Eltis 2001). For this reason, Brazil has had a large black and mixed-race population. In 2014, about 45.5% of the population of 203 million was "branco" or light-skinned, 45.0% "pardo" or brown-skinned, and 8.6% "preto" or dark-skinned (IBGE 2015). Despite persistent disparities along the lines of skin color, most public policies in Brazil have been race-blind. However, in the 2000s, several universities began to adopt race-based affirmative action in admissions.

In this paper, we estimate the effects of racial quotas at the University of Brasilia, which reserved 20% of admissions slots for persons who self-identified as black. To do so, we link the admissions outcomes of high-performing applicants in 2004-2005 to their education and labor market outcomes in 2012. The university provided complete admissions records for these applicant cohorts, and the government provided comprehensive data on all formal workers in the country. We focus on applicants who obtained an entrance exam score "close" to their major-

specific cutoff. Note that an applicant's entrance exam score is the primary basis for admission, and the minimum score for admission differs by major. Nearly all the applicants in our estimation sample ranked above the 85th percentile in the full applicant pool. We adopt regression methods to compare quota and non-quota students with similar admissions credentials and to compare high-performing applicants above and below admissions cutoffs.

In summary, the policy of racial quotas mostly improved outcomes for the targeted disadvantaged group. Relative to quota applicants below the cutoff, quota applicants above the major-specific cutoff enjoyed an increase in years of education, college completion, and labor earnings. Those gains were concentrated among men and applicants in more selective majors. For the large part, mismatch was not prevalent. Differences in outcomes between quota and non-quota students were not significant when controlling for entrance exam score. However, there was some evidence of mismatch among those quota students in less selective majors. Quota students in less selective majors had lower earnings than non-quota students with similar admissions credentials, and quota applicants in less selective majors had negative returns to admission. More broadly, the results confirm the importance of college quality. But the fact that economic returns to admission varied widely by area of study may suggest that major is more relevant than institution.

Several features distinguish our study from the literature. Previous studies of affirmative action in education primarily concern the U.S., where the admissions process is complex and researchers do not observe all relevant variables. Regardless of their geographic focus, few studies investigate the impact of affirmative action on labor market outcomes, and those that do typically rely on samples of individuals who self-report earnings. In our context, admissions is simple and our admissions data is complete. This enables the use of regression discontinuity

(RD) design as a method to estimate the effect of admission through racial quotas. Furthermore, we have exceptional access to administrative data on labor market outcomes. In sum, this is one of only a handful of empirical studies to estimate the effects of affirmative action in college admissions on both education and labor market outcomes. It is the first to implement an RD design with administrative data. Studying affirmative action in Brazil is valuable not only because of the methodological advantages but also because affirmative action is new, many people are affected, and much is at stake.

The remainder of the article is organized as follows. Section 2 reviews related studies, Section 3 describes the data and methods, Section 4 presents the results, and Section 5 discusses the conclusions.

## 2. Related studies

### 2.1 Effect of affirmative action on educational outcomes

Most previous studies on affirmative action in college admissions focus on educational outcomes. Part of this literature concerns the academic performance of underrepresented minorities and their fit with their institution of study. This research indirectly or directly addresses the "mismatch hypothesis" which contends that underrepresented minorities would have had more favorable outcomes if it were not for affirmative action. For example, Rose (2005) examines the University of California at San Diego in the early 1990s. She finds that students admitted under affirmative action had somewhat lower GPAs and much lower graduation rates than students not admitted under affirmative action. Those differences partially narrow when the comparison group is restricted to students in a marginal admissions category.

Alon and Tienda (2005) assess the mismatch hypothesis using data on students who attended college in the 1980s and early 1990s. They claim that mismatch can be rejected if statistically similar underrepresented minorities are equally or more likely to graduate from selective institutions than from nonselective institutions. In models that account for selection on unobservables, attending a selective institution appears to raise the likelihood that black and Hispanic students graduate within six years of entering college. Thus, the authors find no evidence of mismatch.

Fischer and Massey (2007) and Massey and Mooney (2007) take a similar approach to distinguish empirically between the effects of mismatch and the effects of stereotype threat on the college academic outcomes of minorities. Mismatch is underperformance associated with the difference between an individual student's SAT score and the institutional average SAT score, while stereotype threat is underperformance associated with the difference between average minority SAT score and the institutional average. To test for the presence of these effects, they use longitudinal data on students who were freshmen in 1999 and enrolled in selective U.S. colleges. They find no evidence of mismatch but some evidence of stereotype threat.

In an important recent publication, Arcidiacono, Aucejo, and Hotz (2016) study the science graduation rates of minorities enrolled in the University of California system during the late 1990s. To do so, they estimate a model of student persistence in college majors and graduation. They account for possible selection bias associated with choice of UC campus by controlling for the set of campuses to which a student applied and the set of campuses which accepted him or her. They find that less prepared minorities at higher ranked campuses would have had higher science graduation rates if they counterfactually had attended lower ranked campuses.

Not only does the literature seek to estimate the effects of affirmative action in U.S. colleges and universities but also U.S. graduate schools, particularly law school. Sander's 2004 study on affirmative action in law school is a seminal contribution in this area. He draws on the Bar Passage Study, a national longitudinal survey of persons who entered law school in 1991. The dataset contains information on admissions credentials, law school tier attended, academic outcomes during law school, and labor market outcomes. Sander characterizes the extent to which racial preferences impact the sorting of black students into law schools and documents large disparities in academic outcomes between black and white law students. According to the analysis, these disparities are mostly attributable to differences in admissions credentials, i.e., LSAT score and college GPA. His conclusions are stark: due to mismatch, affirmative action substantially weakens black performance in law school and, in turn, reduces the total number of black lawyers.

Rothstein and Yoon (2008a, 2008b) revisit the same issues as Sander. They use the same data but refine the identification strategy. Ideally, researchers could observe black performance in the absence of affirmative action. In practice, their empirical method entails comparing blacks and whites with similar credentials. Any difference in outcomes is attributed to mismatch as blacks tend to attend higher-ranked schools due to admissions preferences. As they note, this yields an overestimate of mismatch, since there are other possible explanations for such differences. Educational outcomes include class rank, graduation, and bar passage. The findings reveal no evidence of mismatch for blacks with higher admissions credentials. However, blacks with relatively low admissions credentials do underperform, which may be due to mismatch or another explanation. Rothstein and Yoon also conclude that the number of black lawyers would fall greatly if affirmative action were eliminated.

Fewer studies examine affirmative action outside of the United States. Bagde et al. (2011) consider India where affirmative action in admissions favors individuals from underrepresented castes. They have access to data on applicants to engineering colleges in one Indian state. The data include college entrance exam scores matched with high school exam records as well as performance on a college exam administered at the end of the first year for students enrolled in private colleges. Based on structural modeling techniques, the authors find that for targeted students, affirmative action increases college attendance as well as first-year academic achievement.

Robles and Krishna (2015) study the experience of 2008 graduates from a selective engineering college in India. The authors rely on institutional records and an exit survey administered to graduating students in 2008. First-year GPA serves as a proxy for college entrance exam score which was unavailable. To assess mismatch, the outcomes of students in more selective majors are compared with the outcomes of same-caste students in less selective majors. As for educational outcomes, they report that admissions preferences are effective in targeting economically disadvantaged castes. However, students from underrepresented castes tend to have lower academic performance in selective majors, which may indicate mismatch.

Alon and Malamud (2014) examine a class-based affirmative action policy in Israel during the early 2000s. In Israel, college applicants can opt to have their affirmative action eligibility examined. A centralized nonprofit organization determines their eligibility score, which is reported to colleges. University departments make admissions decisions based on this information along with academic credentials including overall entrance exam score, subscores, and/or high school grades. The authors are able to implement regression discontinuity design in this setting. Four selective universities provided access to admissions records covering first-time

applicants considered for affirmative action. Outcomes of interest include admission status, selectivity of major, and enrollment. For applicants who are admitted and enroll in one of the four universities, outcomes include GPA and graduation status. They find that eligible applicants had a higher likelihood of admission and enrollment. Moreover, eligible students were not falling behind academically, even in selective majors.

Francis and Tannuri-Pianto (2012, 2013) examine the racial quotas policy at the University of Brasilia. They focus on students who matriculated between 2003 and 2005, a period including two admissions cycles before and three admissions cycles after the implementation of quotas. The authors obtained administrative records (admissions and college academic outcomes) as well as conducted a detailed survey with student photos. In this context, they are able to adopt a difference-in-difference framework to estimate the effect of affirmative action on pre-college academic effort, college academic performance, and racial identification. They report that racial quotas increased the proportion of black students, and that displacing applicants were, by many measures, from families with lower socioeconomic status than displaced applicants. Difference-in-difference regressions suggest that the policy did not reduce the pre-university effort of applicants or students and did not exacerbate racial disparities in college academic performance. Additionally, this and related research (Francis-Tan and Tannuri-Pianto 2015) provide an array of evidence that racial quotas had inspired a persistent shift in racial identification from lighter to darker racial categories both during and after college.

### 2.2 Effect of affirmative action bans on educational outcomes

Since the 1990s, court rulings and state-specific laws have reshaped affirmative action policies in the U.S. Theory papers analyze the potential effects of a general ban on affirmative

action in college admissions. Chan and Eyster (2003) model the admissions process and characterize admissions rules under affirmative action and alternative regimes. They find that an admissions rule that partially ignores standardized test scores (e.g., the Texas top 10% law) may be less efficient than affirmative action at achieving a diverse and high quality student body. Long (2004a) conducts a simulation exercise with a nationally representative sample of college applicants in the early 1990s to study the consequences of eliminating affirmative action. He concludes that an affirmative action ban would decrease the minority share of admitted students considerably, and that a policy awarding admission to students ranking among the top x% of their high school class would only modestly increase the minority share. Howell (2010) develops a model of college application, admission, and matriculation to simulate a ban on race-based admissions at all four-year colleges. She reports that the broad adoption of race-neutral admissions would reduce minority enrollment by about 2% overall and by about 10% at selective institutions.

Research examines the specific case of California where affirmative action was banned by state proposition (Prop. 209) in 1996 (effective 1998). An early study (Conrad and Sharpe 1996) predicts that the elimination of affirmative action in California would redistribute underrepresented minorities from the most selective to the least selective public institutions. Using administrative data from the University of California system, Kate Antonovics and collaborators were able to evaluate the longer-term impacts of the state ban. Antonovics and Sander (2013) find no evidence that yield rates declined for minorities. To the contrary, they uncover a modest "warming effect" whereby minorities were more likely to enroll conditional on acceptance. Antonovics and Backes (2013) explore the effects on minority application rates, proxied by SAT score-sending rates. They report that the decline in score-sending rates was

modest and mostly concentrated at selective institutions for students with weaker credentials. Lastly, Antonovics and Backes (2014) find that changes in admissions policies were able to partially counteract the decrease in the minority admission rate that resulted from the ban, and that student quality, as measured by first-year college GPA remained largely unchanged.

Research also examines the case of Texas where affirmative action was banned in 1996 by court ruling (Hopwood v. Texas) and replaced in 1997 by a program to admit the top 10% of each high school graduating class into state colleges. The initial studies were primarily concerned with minority enrollment outcomes (Bucks 2004, Kain et al. 2005, Dickson 2006, Long and Tienda 2008). These studies generally find that the policy changes in Texas led to a reduction in minority enrollment, particularly at selective institutions. In addition to examining enrollment, research on Texas also examines college graduation and academic performance. Adopting a difference-in-difference approach, Cortes (2010) presents results that suggest the elimination of affirmative action may have reduced minority retention and graduation rates. Adopting a regression discontinuity approach, Fletcher and Mayer (2014) compare students above and below the top 10% cutoff. They conclude there is little evidence that the 10% law raises campus diversity or induces mismatch in academic performance.

Furthermore, other studies estimate the effect of affirmative action bans using data from multiple states. The evidence is mixed regarding how the elimination of affirmative action may have impacted minority applicants' SAT-sending behavior (Long 2004b, Card and Krueger 2005). Building on this line research, Hinrichs (2012) and Backes (2012) take advantage of temporal and geographic variation in bans as well as data on actual enrollment decisions, not just SAT-sending. They both find that state-level affirmative action bans decrease minority enrollment in selective institutions. Most recently, Hinrichs (2014) follows an analogous

methodology to examine graduation rates. He reports that affirmative action bans have led to a decline in the number of underrepresented minorities who graduate from selective colleges, though those who do attend are more likely to graduate.

### 2.3 Effect of affirmative action on labor market outcomes

Fewer studies of affirmative action in college admissions concern labor market outcomes. The general literature on returns to college quality may be relevant (Andrews, Li, and Lovenheim 2016, Brewer et al. 1999, Dale and Krueger 2002, 2014, Black and Smith 2004, 2006, Hoekstra 2009). In sum, these studies demonstrate the positive economic returns to college quality. For example, Hoekstra (2009) adopts a regression discontinuity design to estimate the effect of attending a flagship state university on the earnings of white men who applied in the late 1980s. He finds that attending a selective state university increases earnings by about 20%. Thus, it can be inferred that affirmative action may raise earnings by moving minorities from lower to higher quality colleges.

Other studies employ selection-on-observables or matching techniques. Dale and Krueger (2014) link the college characteristics of persons who entered college in 1976 and 1989 with their earnings in 2007 as reported by administrative data. The returns to attending a selective college are close to zero in models that adjust for selection. However, returns remain positive and large for blacks, Hispanics, and those whose parents have relatively low education. Andrews, Li, and Lovenheim (2016) explore the returns to college quality by earnings decile and by racial group for males who graduated from the University of Texas system and completed high school between 1996 and 2002. They find that quality does not impact earnings uniformly. For instance, black and Hispanic students at the top of the earnings distribution, as well as blacks

at the bottom, gained more from UT-Austin graduation than black and Hispanic students in the middle of the distribution.

Other studies explore the labor market effects of affirmative action more specifically. Loury and Garman (1993, 1995) use the National Longitudinal Study of the Class of 1972 (NLS72) to investigate the effect of attending a selective institution on the earnings of black and white males. Among other results, their analysis reveals that blacks enjoy larger returns to college selectivity, GPA, and major, but that these gains are offset when their SAT scores are much lower than the median SAT of the college they attend. For this reason, the authors speculate that "mismatched" blacks would have had higher earnings if they had attended less selective colleges. Wydick (2002) develops a game theoretic model to compare the potential labor market effects of alternative college admissions policies. He finds that a race-based policy may benefit both employers and minority employees, but labor market discrimination may offset gains if racial preferences or heterogeneity in socioeconomic disadvantage is too strong. As a result, he favors a policy that enhances the college preparation of targeted groups.

In a seminal contribution, Arcidiacono (2005) estimates a forward-looking dynamic discrete choice model with NLS72 data in order to examine the relationship between racial preferences in admissions and labor market earnings. He uses estimates from the four-stage model to forecast how black outcomes would change if they faced the same admissions rules as whites. Arcidiacono reports that the counterfactual elimination of affirmative action would only slightly reduce the earnings of blacks. The effect is modest because affirmative action mostly impacts where students attend college, not if they attend, and because the returns to college quality are relatively low. In contrast, the returns to certain majors, e.g., science, are relatively high.

Not only does the literature examine racial preferences in U.S. college admissions but also in U.S. graduate school admissions. In his 2004 study, Sander analyzes the labor market outcomes of black law students in addition to their academic outcomes. He regresses earnings on law school prestige, law school GPA, race, and other covariates. For one, the coefficient on black is positive and significant, which may reflect preferences for blacks in the labor market. Moreover, Sander emphasizes that the returns to prestige are smaller than the returns to GPA. He concludes that despite the increase in prestige that blacks receive, affirmative action must lower black earnings because they have substantially lower grades in law school. Rothstein and Yoon (2008a, 2008b) take issue with Sander's findings. Regressing labor market outcomes on race and law school credentials, they argue that any racial differences in outcomes could be attributed to mismatch. However, they do not find any evidence of black underperformance in the labor market. Grove and Hussey (2014) adopt Rothstein and Yoon's estimation strategy to examine mismatch in U.S. MBA programs. Exploring the question with longitudinal data on individuals who registered for the GMAT in the 1990s, they do not find any mismatch effects for blacks or Hispanics.

Little research concerns the effect of affirmative action on labor market outcomes outside of the U.S. As previously discussed, Robles and Krishna (2015) study one cohort at a selective engineering college in India. Besides academic outcomes, they also examine labor market outcomes, as self-reported on an exit survey of graduates. The earnings of students in more selective majors are compared with the earnings of same-caste students in less selective majors. In models that allow unobservables to influence selection into majors, minority students in more selective majors earn less than those in less selective majors, which the authors interpret as evidence of mismatch.

The most related paper is by Bertrand et al. (2010) who investigate the effects of an affirmative action policy that reserves more than 50% of admissions slots for lower-caste groups at engineering colleges in one Indian state. Engineering college aspirants in the state must pass a first-round exam and take a second-round exam. They are ranked according to their score on the second-round exam. Beginning with those with the highest scores in each caste, individuals are invited to decide whether to attend an engineering college, which one to attend, and which major to study. The authors had exam scores and personal information for applicants who took the second-round exam in 1996. Note that the precise caste-specific cutoff scores for admission to engineering college were unknown and thus estimated using data on all applicants who attended such colleges in the state. Additionally, the authors conducted a survey of applicants roughly 8-10 years after they took the second-round exam. They sampled applicants who scored in the upper half of the second-round exam and were able to interview about 36% of target households for a sample size of 721.

In the analysis, Bertrand et al. (2010) regress earnings on engineering college attendance and a number of controls including gender, age, parent education, and so on. A binary indicator for having an exam score above the admissions cutoff is used as an instrument for college attendance. Sample restrictions are imposed in some specifications, e.g., only including applicants with scores in the 25th to 75th percentiles of those surveyed in their caste group. In summary, the results demonstrate that affirmative action successfully targets the economically disadvantaged, although the policy may reduce the number of females in engineering colleges. Lower-caste applicants obtain positive returns to admission, but income gains for displacing applicants appear to be smaller than income losses for displaced applicants.

## 3. Data and methods

### 3.1 Policy context

Brazil has been a nation historically characterized by race-blind public policies despite persistent disparities along the lines of skin color. Myriad factors led to the recent adoption of race-based affirmative action: increasing social awareness of racial inequality, formal acknowledgement of race issues by the Cardoso administration (1995-2003), and rise of the Black Movement or Movimento Negro (Bailey 2009). In 2001, the Ministry of Agrarian Development became the first federal ministry to adopt racial quotas in employment. Although a number of universities already had quotas based on income or public school attendance, it was not until 2001 that two state universities in Rio de Janeiro became the first to adopt quotas based on race. Other universities followed, including the University of Brasilia.

The University of Brasilia (UnB), a tuition-free public institution, is one of the best universities in Brazil. It is located in Brasilia, a metropolitan area of 4 million and the nation's capital. Undergraduates are admitted biannually through the "vestibular" system. Applicants select one major and take an entrance exam specific to UnB (the vestibular). It is a two-day exam with questions on a variety of subjects. The overall score on the vestibular is the primary basis for admission, and the minimum score for admission differs by major.[1] Applicants are either admitted to their selected major or they are rejected. The acceptance rate varies widely by major. For example, the acceptance rate for medicine is roughly 1%, the rate for biology is 6%, and the rate for economics is 12%. Approximately 75% of those applicants who are admitted choose to matriculate at UnB. This yield rate is comparable to that at Ivy League colleges in the U.S.

[1] As part of the exam, applicants write an analytical essay, which is only graded if their overall score qualifies them for admission. About 1.5% of applicants who have scores above the admission cutoff are not admitted due to poor essays.

In July 2004, the University of Brasilia implemented racial quotas, becoming the first federal university in the country to have a race-targeted admissions policy. UnB's policy of racial quotas was adopted by the administration without public vote or debate. 20% of each major's vestibular admissions slots are reserved for applicants who identify as black (negro). To be considered for these reserved slots, applicants must opt into the quota system, identify as black, and select one major when they register for the entrance exam. Admitted quota applicants are interviewed by a university panel in order to verify black identity. Upon matriculation, quota students have access to university-sponsored programs intended to support their academic and social development, including tutoring services, public seminars on the value of blacks in society, and a campus meeting space to study and interact.

Recent events continue to change the landscape of university education in Brazil. In April 2012, the Brazilian Supreme Court unanimously upheld the constitutionality of racial quotas at UnB. In August 2012, the National Congress passed legislation mandating that all federal universities adopt a complex system of quotas involving criteria of race, public school attendance, and family income. Universities were given a four year period to reach the goal of reserving 50% of their total admissions slots for students who attended a public secondary school. Within the 50% quota, students from low income families must represent half of slots and students who identify as non-white must represent the proportion of slots corresponding to the proportion of non-whites in the geographic area where the university is located.

### 3.2 Sample

The analysis focuses on persons who took the UnB vestibular exam between 2004 and 2005, a period which includes three admissions cycles following the implementation of racial

quotas (2-2004, 1-2005, 2-2005). The university granted us complete access to admissions records for this period. We had comprehensive information on major, system (quota/non-quota), semester of exam, vestibular scores, and outcome (accepted/rejected). Note that the admissions track – the group of applicants who are directly competing for a fixed number of admissions slots – is determined jointly by major, system, and semester of exam. Recall that the cutoff score for admission varies by admission track, and applicants in each track are either accepted or rejected. We also had responses to a short voluntary questionnaire administered upon registration for the vestibular, which asked questions regarding parental and personal characteristics. About half of applicants completed the questionnaire.

With these admissions records, we identified applicants who obtained exam scores "close" to their track-specific cutoff: (a) accepted applicants and (b) high-performing rejected applicants. High-performing rejected applicants were those who scored at or above the 90th percentile among all rejected applicants with a valid score in the same admissions track. Excluded from the sample were observations of applicants who applied to a major that required a special exam in addition to the vestibular (music, architecture, performing arts, and visual arts); applied to a track that either admitted or rejected all candidates; were rejected then but were accepted during another admissions cycle; and were accepted then but were also accepted during a more recent admissions cycle.

All in all, the sample includes 7,747 records of high-performing applicants in 318 admissions tracks. 95.7% of applicants have only one record in the sample; all of those with multiple records are rejected applicants. It may be helpful to visualize the selectivity of the estimation sample. Figure 1 displays the fraction of applicants in the sample by entrance exam score percentile rank in the full applicant pool. 95% of applicants in the estimation sample

ranked at or above the 85th percentile, and 81% ranked at or above the 90th percentile. Figure 2 displays the fraction of high-performing applicants admitted and enrolled around UnB admission cutoffs. As the figure shows, almost all applicants who scored above their track-specific cutoff were admitted. About 75% of applicants who were admitted chose to enroll at UnB.

### 3.3 Outcomes

In this paper, we link the admissions outcomes of high-performing applicants in 2004-2005 to their education and labor market outcomes in 2012. We use an administrative dataset called "RAIS" which was collected by the Brazilian Ministry of Labor (Ministério do Trabalho e Emprego, 2009-2012). Employers annually provide information on employees to the Ministry of Labor, which utilizes the information to study the labor market and determine certain labor benefits. RAIS covers formal workers in the private and public sector. It includes self-employed persons who own a formal company. The Ministry of Labor estimates that RAIS covers about 97% of all formal workers in Brazil, approximately 50 million formal sector jobs. However, RAIS does not cover informal workers, i.e., persons who are employed without a labor card or are self-employed but have not formally established a company.

Undoubtedly, the key challenge of this research project was to link the UnB admissions records to RAIS, since UnB did not collect applicant employment identification numbers (called "CPF" in Brazil). In short, we matched by name. We benefited from the fact that Brazilian names tend to be long (and unique), and that Distrito Federal – the state where the vast majority of applicants resided – is relatively small. The objective was to identify the CPF of all high-performing UnB applicants who worked in the formal sector in any state at any time between 2009 and 2012.

Name matching occurred in stages. First, the RAIS data was limited to those workers who completed at least secondary school and was divided into five geographic units (DF, Goiás, Minas Gerais/Rio de Janeiro, São Paulo, and other states). Second, for all high-performing applicants in the admissions records, an "exact name" search was performed by geographic unit. Exact name matches were assigned a quality score based on information about educational attainment and birth year. The quality score helped to resolve cases of multiple matches in the same geographic unit. Third, for high-performing applicants who did not have any exact name matches, a "partial name" search was done by geographic unit. Research assistants conducted partial name searches name by name. Fourth, the authors resolved multiple matches with the same score in the same geographic unit utilizing additional information provided by the third to last digit of the CPF, which signals region of CPF registration. An algorithm was developed to select the best single match over the several possible matches discovered across geographic units. Priority was given to a match discovered in the state of residence noted in admissions records. Otherwise, priority was given to a matched discovered in DF. Lastly, the authors reviewed all matches and overrided the algorithm when a better match was available.

It is informative to examine the accuracy of our CPF match results. Luckily, UnB did collect the employment identification number of persons who matriculated. Our match result is correct if either the CPF that we found is the same as the CPF that UnB provided or we did not find a CPF and the CPF that UnB provided does not exist in RAIS 2009-2012. Based on this calculation, the overall match accuracy rate is 91%. In particular, "exact name" match accuracy is almost 95% and "partial name" match accuracy is 66%. Below we describe an exercise that explores the robustness of our main results with respect to match quality. Note that for the part of the analysis that only involves UnB students, we use the CPF provided by UnB.

RAIS was used to construct education and labor market outcomes. "Years of education" is the number of years of education completed, based on the highest level of education reported by a person's employer during 2009-2012. Persons with no records in RAIS have missing values for this variable. "College completion" is a binary indicator that equals one if a person's employer during 2009-2012 reported that he or she had completed college and equals zero if no employer reported he or she completed college. "Residence in Distrito Federal" is a binary indicator that equals one if a person was residing in DF during the most recent year reported in RAIS between 2009 and 2012 and equals zero if he or she was residing in another state. "Formal employment" is a binary indicator that equals one if a person had any positive earnings in 2012 and equals zero if he or she had no earnings in 2012 or had no records in RAIS. "Public sector job" is a binary indicator that equals one if a person's principal job in 2012 was in the public sector (e.g., state and federal government, public universities, public foundations) and equals zero if his or her job was in the private sector. Public jobs, especially federal jobs, pay well and are plentiful in Brasilia. "Log annual labor earnings" is the natural logarithm of all earnings reported in 2012. Earnings are expressed in real 2014 reais. Persons with no earnings in 2012 or no records in RAIS have missing values for this variable. Table 1 reports descriptive statistics for persons in the estimation sample.

### 3.4 Statistical procedure

The empirical analysis consists of two parts. In the first part, we compare quota and non-quota students with similar admissions credentials. For persons who enrolled at UnB, outcomes $Y_i$ are regressed on an indicator for quota system $Q_i$, entrance exam score $X_i$, and a set of binary controls for major and semester of exam $\gamma_{ms}$. The following equation is estimated:

$$Y_i = \alpha_0 + \alpha_1 Q_i + \alpha_3 X_i + \gamma_{ms} + \varepsilon_i.$$

Regressions are stratified by gender and area of study, respectively. Note that the influence of measurement error is minimal, since we link admissions and labor market records using the correct CPF provided by UnB. Note also that we use college completion as reported by UnB, not RAIS.

This may be seen as a test of mismatch in the spirit of Rothstein and Yoon (2008a, 2008b). Any difference in outcomes might be attributed to mismatch as blacks are able to matriculate in more competitive majors with the implementation of racial quotas. But as Rothstein and Yoon point out, this yields an overestimate of mismatch, since there are alternative explanations for such differences.

In the second part of the empirical analysis, we implement a regression discontinuity (RD) design with the sample of high-performing applicants to estimate the effect of college admission. The advantage of an RD design is that it is like a randomized experiment in the neighborhood of the admissions cutoff. Thus, each of the 318 admissions tracks can be thought of as a separate experiment. As discussed above, Figure 2 illustrates the discontinuity in admission at track-specific cutoffs. This discontinuity is almost perfectly sharp, as only a few applicants with scores above the cutoff were not admitted because their analytical essay received a failing grade. Strictly speaking, our estimator for the effect of admission is an "intent to treat" estimator.

The key assumption of an RD design is that individuals are unable to precisely manipulate the assignment variable. In our context, applicants did not know admissions cutoffs before they took the entrance exam. Cutoffs, which varied from exam to exam, were a function of the number of available slots and the distribution of exam scores. Figure 3 plots the

distribution of normalized entrance exam scores.[2] An applicant's normalized entrance exam score is the difference between his or her overall vestibular score and his or her track-specific cutoff, i.e., $S_i = X_i - C_t$. The figure shows there is no dramatic jump around the discontinuity. It is also possible to test the validity of the RD design by confirming that the baseline covariates are balanced at the admissions threshold. Appendix Table 1 performs the test. The results indicate there are no systematic differences in demographic characteristics between high-performing rejected and accepted applicants.

In our particular adaption of the RD design, outcomes $Y_i$ are regressed on an above cutoff indicator $A_i$, i.e., $A_i = 1\{S_i \geq 0\}$, normalized entrance exam score $S_i$, and an interaction between the above cutoff indicator and normalized score, all of which are fully interacted with indicators for quota $Q_i$ and non-quota $N_i$ system. Regressions also include a set of binary controls for track $\theta_t$, which is jointly determined by major, quota/non-quota system, and semester of exam. The following equation is estimated:

$$Y_i = \beta_0 + \beta_1 A_i \cdot N_i + \beta_2 A_i \cdot Q_i + \beta_3 S_i \cdot N_i + \beta_4 S_i \cdot Q_i + \beta_5 S_i \cdot A_i \cdot N_i + \beta_6 S_i \cdot A_i \cdot Q_i + \theta_t + \varepsilon_i \ .$$

Regressions are stratified by gender and area of study, respectively. Note that there exists some degree of measurement error because we were not always able to match admissions and labor market records correctly, but estimates suggest that our match accuracy was over 90%.

In adopting an RD design, this paper contributes to those lines of research that take an RD approach to study college quality (Hoekstra 2009, Öckert 2010, Saavedra 2008) and affirmative action in college admissions (Alon and Malamud 2014, Bertrand et al. 2010, Fletcher and Mayer 2014). This paper is also roughly comparable to Pop-Eleches and Urquiola (2013)

[2] Applicants with normalized entrance exam scores of zero are excluded from the figure. The jump at exactly zero is mechanical since every admissions track must have at least one normalized score of zero, by construction.

who take an RD approach to estimate the effect of access to quality secondary school in Romania. Like they do, we consider a situation with a large number of quasi-experiments.

Robustness exercises are also conducted. Recall that the sample is already highly restricted (see Figure 1). The median number of observations in an admissions track is only 16. Nevertheless, it may be useful to narrow the bandwidth further. Appendix Table 2 displays the RD results with applicants who had normalized entrance exam scores between -80 and +80, which excludes the tails of the distribution (see Figure 3). Recall that we were unable to match all admissions and labor market records correctly. It may be useful to restrict the estimation sample to high-quality matches. Appendix Table 3 displays the RD results with only applicants whose CPF was identified via "exact name" match. Measurement error is much lower in this subsample.

## 4. Results

### 4.1 Comparing quota and non-quota students

First, we compare quota and non-quota students with similar admissions credentials. Table 2 displays pooled regressions with all students as well as regressions stratified by gender. The estimated coefficients displayed in the table are those on quota status. All specifications include a set of controls for major and semester of exam. Columns labeled "adjusted difference" also control for entrance exam score, while columns labeled "unadjusted difference" do not.

To begin, we consider regressions with all students who matriculated at UnB. Unadjusted differences in years of education and labor earnings are notable. Relative to non-quota students, quota students had significantly less education and less earnings. In particular, estimates indicate that quota students had about 0.20 fewer years of education completed and about 12%

(100*[exp(-0.131)-1]) lower annual earnings. Unadjusted differences in college completion, formal employment, and public sector jobs are not significant, and the difference in DF residence is significant only at the 10% level. Controlling for entrance exam score shrinks the differences in years of education and labor earnings considerably. Quota students had about 0.12 fewer years of education completed, which is significant at the 10% level. They had about 5% less earnings, which is not significant. It is also illuminating to consider regressions stratified by gender. Patterns are similar to patterns for pooled regressions. However, relative to male non-quota students, male quota students were significantly more likely to reside in DF and significantly less likely to have a public sector job. Estimates from adjusted specifications indicate that male quota students were about 5 percentage points more likely to reside in DF and 6 percentage points less likely to have a public sector job.

Table 3 displays regressions stratified by area of study. Areas of study are ordered according to selectivity of majors. The most selective is professional (medicine and law), and the least selective is teaching. As in the previous table, the coefficients displayed are those on quota status, and all specifications include controls for major and semester of exam. The table illustrates that the largest differences in educational attainment occurred among students in the most selective areas of study. In both unadjusted and adjusted specifications, quota students in professional and engineering majors tended to complete about half of a year less education than non-quota students. The difference was significant for engineering but not for professional majors. Nevertheless, there was no significant difference in college completion in any of the areas of study. Quota students in professional, engineering, social science, and business areas had a modestly elevated likelihood of DF residence, but none of the coefficients were significant at the 5% level in adjusted specifications. Moreover, all of the differences between quota and

non-quota students in formal employment and most of the differences in public sector jobs were not significant. Quota students in science and business were less likely to have a public sector job. The table reveals few differences in labor earnings, except in two of the least selective areas of study. To be specific, quota students in business and teaching had significantly less earnings, even controlling for entrance exam score. These quota students earned approximately 20% less than non-quota students with comparable admissions credentials.

### 4.2 Comparing applicants above and below admissions cutoffs

Then, we compare high-performing applicants above and below admissions cutoffs to estimate the effect of admission to UnB. We implement the regression discontinuity design as outlined previously. Table 4 displays pooled regressions with all high-performing applicants as well as regressions stratified by gender. The estimated coefficients displayed in the table are the ones on the above cutoff indicator for non-quota applicants and the above cutoff indicator for quota applicants. All specifications control for normalized entrance exam score, interactions between above cutoff indicators and normalized score, and a set of controls for major, quota/non-quota system, and semester of exam.

To start, we consider regressions with both genders. It is useful to confirm that having an entrance exam score above the track-specific cutoff is associated with admission to UnB. The likelihood of admission for non-quota applicants above the cutoff was greater than 98%, and the likelihood for quota applicants above the cutoff was greater than 99%. As the estimates demonstrate, the effect of admission on education differed by quota status. Relative to non-quota applicants below the cutoff, non-quota applicants above the cutoff had completed about 0.08 more years of education and were 3 percentage points more likely to have completed college, but

neither of these differences was statistically significant. In contrast, quota applicants experienced significant gains in educational outcomes. Quota applicants above the cutoff had completed about 0.46 more years of education and were 10 percentage points more likely to have completed college. Both quota and non-quota applicants with an exam score above the cutoff were approximately 10 percentage points more likely to reside in DF. Non-quota applicants above the cutoff were more likely than non-quota applicants below the cutoff to have formal employment as well as a public sector job, whereas quota applicants above the cutoff were no more likely. In the specification with both genders, non-quota applicants received larger monetary returns to admission than quota applicants. Relative to non-quota applicants below the cutoff, non-quota applicants above the cutoff had 11% (100*[exp(0.104)-1]) higher labor earnings. Relative to quota applicants below the cutoff, quota applicants above the cutoff had 6% higher earnings.

Furthermore, regressions stratified by gender yield insightful results. They show that the gains to admission were concentrated among male applicants. Male quota applicants above the cutoff had completed about 0.72 more years of education and were 16 percentage points more likely to have completed college. Contrarily, female quota applicants did not have any significant increases in educational outcomes. Labor market returns to admission were significant only for males, and quota applicants received larger returns than non-quota applicants. Male non-quota applicants above the cutoff had 17% higher labor earnings than male non-quota applicants below the cutoff, while male quota applicants above the cutoff had 39% higher earnings than male quota applicants below the cutoff.

Table 5 displays regressions stratified by area of study. Areas of study are ordered according to selectivity of majors. As in the previous table, the only coefficients displayed are the ones on the above cutoff indicator for non-quota applicants and the above cutoff indicator for

quota applicants. Note that stratification reduces sample size drastically for quota applicants. Nevertheless, patterns do emerge. For most areas of study, quota applicants had positive educational gains to admission and had larger gains than non-quota applicants. However, for science, quota applicants above the cutoff had fewer completed years of education and were less likely to complete college relative to quota applicants below the cutoff, though these differences are not statistically significant. Except for quota applicants in science, applicants above the cutoff had an elevated likelihood of residing in DF, regardless of quota status or area of study. With regard to formal employment and public sector jobs, the estimates do not exhibit a discernible pattern across areas of study. But again for science, quota applicants above the cutoff were less likely to have formal employment and less likely to have a public sector job. Interestingly, monetary returns to admission were highest for quota applicants in the most selective areas (e.g., professional and engineering) and lowest for quota applicants in the least selective areas (e.g., teaching and other). That is, quota applicants above the cutoff in a professional or engineering major had more than 50% higher labor earnings than quota applicants below the cutoff. In contrast, quota applicants above the cutoff in a teaching or "other" major had roughly 25% less labor earnings than quota applicants below the cutoff.

## 5. Discussion

### 5.1 Summary of results

In this paper, we estimate the effects of affirmative action at the University of Brasilia with data linking the admissions outcomes of high-performing applicants in 2004-2005 to their education and labor market outcomes in 2012.

First, we compared quota and non-quota students with similar admissions credentials. In regressions controlling only for major and semester of exam, quota students had significantly fewer years of education completed and lower annual earnings than non-quota students. However, these differences narrowed considerably when entrance exam score was also included as a control. Differences in college completion, formal employment, and public sector job were not significant in either unadjusted or adjusted specifications. Patterns were fairly similar by gender. Stratification by area of study revealed that the largest differences in years of education occurred among students in the most selective areas. In adjusted regressions, quota students did not have significantly lower earnings, except in two of the least selective areas of study.

Second, we compared high-performing applicants above and below admissions cutoffs to estimate the effect of admission to UnB. Having an entrance exam score above the track-specific cutoff was highly associated with admission in all specifications. Both quota and non-quota applicants above the cutoff were more likely to reside in DF. Quota applicants experienced larger gains in educational outcomes than non-quota applicants. Relative to quota applicants below the cutoff, those above the cutoff had significantly more years of education completed and were significantly more likely to have completed college. Non-quota applicants above the cutoff were more likely than non-quota applicants below the cutoff to have formal employment as well as a public sector job, whereas quota applicants above the cutoff were no more likely. When both genders were pooled, quota applicants above the cutoff had a smaller increase in earnings than non-quota applicants above the cutoff.

Patterns were strikingly dissimilar by gender, as the results showed the gains to admission were concentrated among male applicants. Male quota applicants above the cutoff had significantly higher educational outcomes, while female quota applicants above the cutoff did

not. Moreover, labor market returns to admission were significant only for males, and unlike pooled regressions, quota applicants received much larger returns than non-quota applicants. Stratification by area of study uncovered further insights. Labor market returns were positive and relatively large for quota applicants in the most selective areas but negative and relatively large for quota applicants in the least selective areas.

## 5.2 Implications

The findings shed light on the effects of affirmative action in college admissions. All in all, the admissions policy that UnB implemented in 2004 improved outcomes for the targeted disadvantaged group. For the most part, mismatch was not prevalent. But there was some evidence of mismatch among those quota students in less selective majors. The gains to admission through racial quotas were largely positive. Quota applicants above the track-specific cutoff enjoyed an increase in years of education, college completion, and labor earnings. Those gains were concentrated among men and applicants in more selective majors.

It is useful to place the results in the context of the prior literature. This study is consistent with those studies that did not find widespread mismatch (e.g., Alon and Tienda 2005, Fischer and Massey 2007, Rothstein and Yoon 2008a, 2008b) and is contrary to those studies that did (e.g., Loury and Garman 1993, 1995, Sander 2004, Robles and Krishna 2015). Like Rothstein and Yoon (2008a, 2008b), we only encountered mismatch among persons with less competitive admissions profiles. Additionally, the results broadly confirm previous studies of college quality (e.g., Black and Smith 2004, 2006, Hoekstra 2009), since college admission raised the earnings of non-quota and quota applicants. But that those returns varied considerably by area of study underscores the notion that college major may be more important than

institution (Arcidiacono 2005, Arcidiacono, Aucejo, and Hotz 2016). Like quota applicants in India (Bertrand et al. 2010), quota applicants at UnB, especially males, had positive labor market returns, but unlike their Indian counterparts, they received higher returns than non-quota applicants.

The findings also shed light on the reliability of statistical methods used to assess affirmative action. In this paper, we adopted regression methods to compare quota and non-quota students with similar admissions credentials and to compare high-performing applicants above and below admissions cutoffs in an RD framework. Variants of the first method prevail in the literature (e.g., Rothstein and Yoon 2008a, 2008b, Grove and Hussey 2014). As we have shown, both methods yielded comparable results regarding mismatch, i.e., whether the policy made targeted individuals worse off. However, they differed in that the RD design was superior at identifying the gains associated with the policy. Thus, when data on rejected applicants is not available, a method comparing targeted and non-targeted students with similar admissions credentials, though not ideal, is reliable in detecting mismatch.

### 5.3 Contributions and caveats

In sum, this is one of only a handful of empirical studies to estimate the effects of affirmative action in college admissions on both education and labor market outcomes. It is the first to implement an RD design with administrative data. Studying affirmative action in Brazil is valuable not only because of the methodological advantages but also because affirmative action is new, many people are affected, and much is at stake. It is also good to acknowledge the limitations of the study. Key aspects of college admissions in Brazil, namely, use of a single test score to determine admission and direct application to major, have parallels throughout the world

but not in the United States, the prime focus of prior literature. These distinguishing aspects of college admissions therefore limit informative comparisons with the U.S. Also, admissions records did not contain employment identification numbers, so we had to link admissions and labor market data using applicant names. Therefore, measurement error likely affects the RD results to some degree. We believe its influence is minor, given that estimates suggest our match accuracy was over 90%, and that regressions restricted to high-quality matches yield comparable results, as Appendix Table 3 indicates.

Future research may be able to extend the analysis. It would be interesting to study the long-run effects of the quotas policy on labor, demographic, and health outcomes. In light of recent legislation, it appears likely that group-targeted educational programs will continue to expand in Brazil and elsewhere, and future research can study these programs as they emerge. Indeed, understanding more about the impact of educational policy on life outcomes is important and timely, as affirmative action remains an issue of current debate and affects the lives of millions of people worldwide who belong to historically disadvantaged groups.

## References


Alon, Sigal and Marta Tienda. 2005. "Assessing the 'mismatch' hypothesis: Differences in college graduation rates by institutional selectivity." Sociology of Education, 78(4): 294-315.

Alon, Sigal and Ofer Malamud. 2014. "The impact of Israel's class-based affirmative action policy on admission and academic outcomes." Economics of Education Review, 40(June): 123-139.

Andrews, Rodney J., Jing Li, and Michael F. Lovenheim. 2016. "Quantile Treatment Effects of College Quality on Earnings." Journal of Human Resources, 51(1): 200-238.

Antonovics, Kate and Ben Backes. 2013. "Were Minority Students Discouraged from Applying to University of California Campuses after the Affirmative Action Ban?" Education Finance and Policy, 8(2): 208-250.

Antonovics, Kate and Ben Backes. 2014. "The Effect of Banning Affirmative Action on College Admissions Policies and Student Quality." Journal of Human Resources, 49(2): 295-322.

Antonovics, Kate L. and Richard H. Sander. 2013. "Affirmative Action Bans and the 'Chilling Effect'" American Law and Economics Review, 15(1): 252-299.

Arcidiacono, Peter, Michael Lovenheim, and Maria Zhu. 2015. "Affirmative Action in Undergraduate Education." Annual Review of Economics, 7: 487-518.

Arcidiacono, Peter. 2005. "Affirmative Action in Higher Education: How Do Admission and Financial Aid Rules Affect Future Earnings?" Econometrica, 73(5): 1477-152.

Arcidiacono, Peter, Esteban M. Aucejo, and V. Joseph Hotz. 2016. "University differences in the graduation of minorities in STEM fields: Evidence from California." American Economic Review, 106(3): 525-562.

Backes, Ben. 2012. "Do Affirmative Action Bans Lower Minority College Enrollment and Attainment? Evidence from Statewide Bans." Journal of Human Resources, 47(2): 435-455.

Bagde, Surendrakumar, Dennis Epple, and Lowell J. Taylor. 2011. "Dismantling the legacy of caste: affirmative action in Indian higher education." Working Paper, Carnegie Mellon University, Pittsburgh, PA.

Bailey, Stanley R. 2009. Legacies of Race: Identities, Attitudes, and Politics in Brazil. Stanford: Stanford University Press.

Bertrand, Marianne, Rema Hanna, and Sendhil Mullainathan. 2010. "Affirmative Action in Education: Evidence From Engineering College Admissions in India." Journal of Public Economics, 94(1-2): 16-29.

Black, Dan A. and Jeffrey A. Smith. 2004. "How robust is the evidence on the effects of college quality? Evidence from matching." Journal of Econometrics, 121(1-2): 99-124.

Black, Dan A. and Jeffrey A. Smith. 2006. "Estimating the returns to college quality with multiple proxies for quality." Journal of Labor Economics, 24(3): 701-728.

Brewer, Dominic J., Eric R. Eide, and Ronald G. Ehrenberg. 1999. "Does it pay to attend an elite private college? Cross-cohort evidence on the effects of college type on earnings." Journal of Human Resources, 34(1): 104-123.

Bucks, Brian. 2004. "Affirmative Access versus Affirmative Action: How Have Texas' Race-Blind Policies Affected College Outcomes?" Working Paper, Texas Schools Project.

Card, David and Alan B. Krueger. 2005. "Would the Elimination of Affirmative Action Affect Highly Qualified Minority Applicants? Evidence from California and Texas." Industrial and Labor Relations Review, 58(3): 416-34.

Chan, Jimmy and Erik Eyster. 2003. "Does banning affirmative action lower college student quality?" American Economic Review, 93(3): 858-872.
Conrad, Cecilia A. and Rhonda V. Sharpe. 1996. "The Impact of the California Civil Rights Initiative (CCRI) on University and Professional School Admissions and the Implications for the California Economy." Review of Black Political Economy, 25(1): 13-59.
Cortes, Kalena E. 2010. "Do bans on affirmative action hurt minority students? Evidence from the Texas Top 10% Plan." Economics of Education Review, 29(6): 1110-1124.
Dale, Stacy Berg and Alan B. Krueger. 2002. "Estimating the payoff to attending a more selective college: An application of selection on observables and unobservables." Quarterly Journal of Economics, 117(4): 1491-1527.
Dale, Stacy and Alan B. Krueger. 2014. "Estimating the effects of college characteristics over the career using administrative earnings data." Journal of Human Resources, 49(2): 323-358.
Darity, William, Jr., Ashwini Deshpande, and Thomas Weisskopf. 2011. "Who Is Eligible? Should Affirmative Action be Group- or Class-Based?" American Journal of Economics and Sociology, 70(1): 238-268.
Dickson, Lisa M. 2006. "Does ending affirmative action in college admissions lower the percent of minority students applying to college?" Economics of Education Review, 25(1): 109-119.
Eltis, David. 2001. "The Volume and Structure of the Transatlantic Slave Trade: A Reassessment." The William and Mary Quarterly. 58(1): 17-46.
Fischer, Mary J. and Douglas S. Massey. 2007. "The effects of affirmative action in higher education." Social Science Research, 36(2): 531-549.
Fletcher, Jason M. and Adalbert Mayer. 2014. "Tracing the Effects of Guaranteed Admission through the College Process: Evidence from a Policy Discontinuity in the Texas 10% Plan." Contemporary Economic Policy, 32(1): 169-186.
Francis, Andrew M. and Maria Tannuri-Pianto. 2012. "Using Brazil's Racial Continuum to Examine the Short-Term Effects of Affirmative Action in Higher Education." Journal of Human Resources, 47(3): 754-784.
Francis, Andrew M. and Maria Tannuri-Pianto. 2013. "Endogenous Race in Brazil: Affirmative Action and the Construction of Racial Identity among Young Adults." Economic Development and Cultural Change, 61(4): 731-753.
Francis-Tan, Andrew and Maria Tannuri-Pianto. 2015. "Inside the black box: affirmative action and the social construction of race in Brazil." Ethnic and Racial Studies, 38(15): 2771-2790.
Grove, Wayne A. and Andrew J. Hussey. 2014. "Preferential Admission and MBA Outcomes: Mismatch Effects by Race and Gender." B.E. Journal of Economic Analysis & Policy, 14(3): 613-668.
Hinrichs, Peter. 2012. "The Effects of Affirmative Action Bans on College Enrollment, Educational Attainment, and the Demographic Composition of Universities." Review of Economics and Statistics, 94(3): 712-722.
Hinrichs, Peter. 2014. "Affirmative action bans and college graduation rates." Economics of Education Review, 42(October): 43-52.
Hoekstra, Mark. 2009. "The Effect of Attending the Flagship State University on Earnings: A Discontinuity-Based Approach." Review of Economics and Statistics, 91(4): 717-724.
Holzer, Harry and David Neumark. 2000. "Assessing Affirmative Action." Journal of Economic Literature, 38(3): 483-568.
Howell, Jessica S. 2010. "Assessing the Impact of Eliminating Affirmative Action in Higher Education." Journal of Labor Economics, 28(1): 113-166.

IBGE (Instituto Brasileiro de Geografia e Estatística). 2015. "Pesquisa Nacional por Amostra de Domicílios: Síntese de indicadores 2014." IBGE: Rio de Janeiro. Accessed online at http://biblioteca.ibge.gov.br/visualizacao/livros/liv94935.pdf.

Kain, John F., Dan O'Brien, and Paul Jargowsky. 2005. "Hopwood and the Top 10 Percent Law: How They Have Affected the College Enrollment Decisions of Texas High School Graduates." Working Paper, Texas Schools Project.

Long, Mark C. 2004a. "Race and College Admissions: An Alternative to Affirmative Action?" Review of Economics and Statistics, 86(4): 1020-33.

Long, Mark C. 2004b. "College Applications and the Effect of Affirmative Action." Journal of Econometrics, 121(1-2): 319-342.

Long, Mark C. and Marta Tienda. 2008. "Winners and losers: Changes in Texas university admissions post-Hopwood." Educational Evaluation and Policy Analysis, 30(3): 255-280.

Loury, Linda Datcher and David Garman. 1993. "Affirmative Action in Higher Education." American Economic Review, 83(2): 99-103.

Loury, Linda Datcher and David Garman. 1995. "College Selectivity and Earnings." Journal of Labor Economics, 13(2): 289-308.

Massey, Douglas S. and Margarita Mooney. 2007. "The effects of America's three affirmative action programs on academic performance." Social Problems, 54(1): 99-117.

Ministério do Trabalho e Emprego (Ministry of Labor). 2009-2012. Relação Anual de Informações Sociais (RAIS): microdados. Brasília, DF.

Öckert, Björn. 2010. "What's the value of an acceptance letter? Using admissions data to estimate the return to college." Economics of Education Review, 29: 504–516.

Pop-Eleches, Cristian and Miguel Urquiola. 2013. "Going to a Better School: Effects and Behavioral Responses." American Economic Review, 103(4): 1289-1324.

Robles, Verónica C. Frisancho and Kala Krishna. 2015. "Affirmative action in higher education in India: targeting, catch up, and mismatch." Working Paper, Pennsylvania State University, University Park, PA.

Rose, Heather. 2005. "The effects of affirmative action programs: Evidence from the University of California at San Diego." Educational Evaluation and Policy Analysis, 27(3): 263-289.

Rothstein, Jesse and Albert H. Yoon. 2008a. "Affirmative action in law school admissions: What do racial preferences do?" University of Chicago Law Review, 75(2): 649-714.

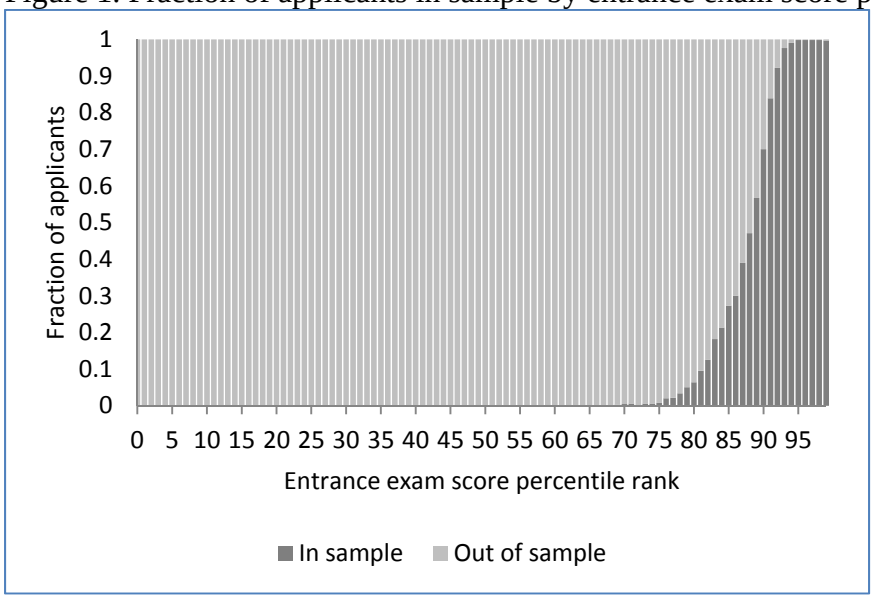
Rothstein, Jesse and Albert H. Yoon. 2008b. "Mismatch in Law School." NBER Working Paper No. 14275.

Saavedra, Juan E. 2008. "The Returns to College Quality: A Regression Discontinuity Analysis." Working Paper, Harvard University.

Sander, Richard H. 2004. "A systemic analysis of affirmative action in American law schools." Stanford Law Review, 57(2): 367-483.

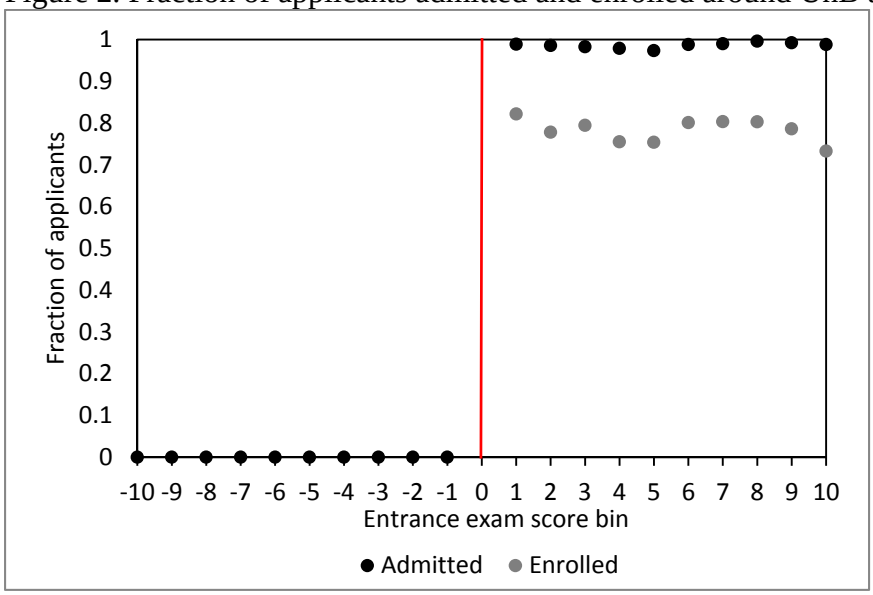
Wydick, Bruce. 2002. "Affirmative action in college admissions: Examining labor market effects of four alternative policies." Contemporary Economic Policy, 20(1): 12-24.

Figure 1. Fraction of applicants in sample by entrance exam score percentile rank



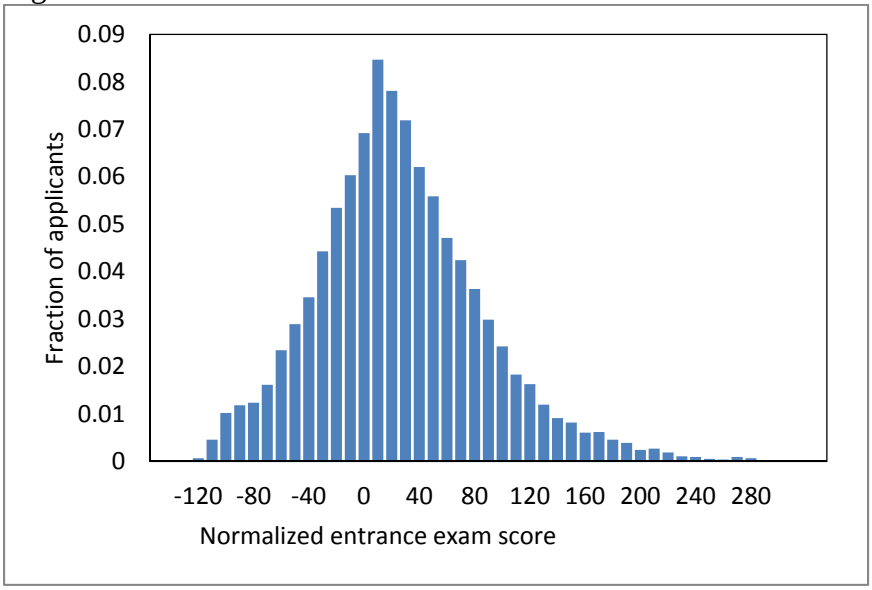

NOTE. Percentile rank is calculated by major, system, and semester of exam. The figure considers all applicants who took the vestibular in 2-2004, 1-2005, and 2-2005.

Figure 2. Fraction of applicants admitted and enrolled around UnB admission cutoffs




NOTE. The figure displays the fraction admitted and the fraction enrolled by entrance exam score bin. Applicants are sorted into ten 8-point bins below admission cutoffs and ten 8-point bins above admission cutoffs. The lowest and highest bins are wider than 8-points to include the tails of the standardized entrance exam score distribution. The vertical line indicates the standardized admissions cutoff.

Figure 3. Distribution of normalized entrance exam score




NOTE. Applicants who had a zero normalized entrance exam score are excluded from the figure. Bin width is 10 points.

**Table 1**
**Descriptive statistics for persons in estimation sample**

| | Non-quota applicants | | Quota applicants | |
|---|---|---|---|---|
| | Male | Female | Male | Female |
| UnB admission outcomes | | | | |
| Admitted (%) | 63.1 | 61.7 | 69.1 | 67.9 |
| Enrolled (%) | 45.4 | 51.9 | 58.0 | 61.5 |
| Education/labor outcomes in 2012 (RAIS) | | | | |
| Years of education | 15.2 | 15.5 | 14.8 | 15.3 |
| College completion (%) | 72.3 | 81.5 | 64.6 | 75.2 |
| Residence in Distrito Federal (%) | 71.5 | 74.2 | 81.9 | 79.6 |
| Formal employment (%) | 65.4 | 64.3 | 74.0 | 69.4 |
| Public sector job (%) | 52.4 | 53.1 | 53.8 | 51.8 |
| Log annual labor earnings | 11.0 | 10.6 | 10.8 | 10.5 |
| Demographic characteristics | | | | |
| Female (%) | 0.0 | 100.0 | 0.0 | 100.0 |
| Birth year | 1981.8 | 1983.8 | 1982.2 | 1983.7 |
| Completed UnB admissions survey (%) | 42.1 | 47.5 | 80.5 | 81.3 |
| Mother college completion (%, admissions survey) | 53.5 | 54.5 | 37.9 | 35.3 |
| Family monthly income (%, admissions survey) | | | | |
| R$750 or less | 7.9 | 9.0 | 14.3 | 20.4 |
| R$750-2500 | 28.3 | 29.9 | 40.1 | 42.3 |
| R$2500-5000 | 25.0 | 27.1 | 21.4 | 21.5 |
| R$5000 or more | 26.8 | 24.8 | 12.8 | 8.5 |
| Don't know | 12.0 | 9.2 | 11.4 | 7.3 |
| Identified as PPI (%, admissions survey) | 42.2 | 39.7 | 99.3 | 99.4 |
| N | 3,853 | 2,529 | 754 | 611 |

NOTE. The sample includes UnB applicants between 2-2004 and 2-2005 including (a) all persons who took the entrance exam and were admitted and (b) all persons who were not admitted but had an exam score in the 90th percentile or higher. Earnings are expressed in real 2014 R$. Sample size is smaller for variables derived from the admissions survey.

**Table 2**
**Differences in 2012 education and labor market outcomes between persons who were quota students and those who were non-quota students at UnB, stratified by gender**

| | All students | | Male students | | Female students | |
|---|---|---|---|---|---|---|
| Dependent variable | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference |
| Years of education | -0.201*** | -0.117* | -0.214** | -0.111 | -0.227** | -0.146 |
| | (0.068) | (0.070) | (0.101) | (0.106) | (0.092) | (0.091) |
| College completion | -0.010 | -0.006 | -0.003 | 0.005 | -0.011 | -0.011 |
| | (0.016) | (0.016) | (0.023) | (0.024) | (0.022) | (0.022) |
| Residence in Distrito Federal | 0.028* | 0.028* | 0.063*** | 0.050** | -0.019 | -0.003 |
| | (0.015) | (0.015) | (0.020) | (0.022) | (0.024) | (0.024) |
| Formal employment | 0.010 | 0.017 | 0.025 | 0.024 | -0.007 | 0.010 |
| | (0.017) | (0.018) | (0.024) | (0.025) | (0.026) | (0.027) |
| Public sector job | -0.037 | -0.014 | -0.081** | -0.063* | 0.014 | 0.043 |
| | (0.024) | (0.025) | (0.034) | (0.035) | (0.038) | (0.040) |
| Log annual labor earnings | -0.131*** | -0.051 | -0.141*** | -0.058 | -0.132** | -0.055 |
| | (0.039) | (0.040) | (0.051) | (0.053) | (0.065) | (0.065) |

NOTE. The sample includes all persons who matriculated at UnB between 2-2004 and 2-2005. In this analysis, the CPFs of enrolled students are corrected, and college completion is based on UnB records, not RAIS. The table displays the estimated coefficient on quota status from linear regressions that control jointly for major and semester of exam. "Adjusted" coefficients come from regressions that also control for UnB entrance exam score. Robust standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 3**
**Differences in 2012 education and labor market outcomes between persons who were quota students and those who were non-quota students at UnB, stratified by area of study**

| | Professional (1) | | Engineering (2) | | Health (3) | | Social science (4) | |
|---|---|---|---|---|---|---|---|---|
| Dependent variable | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference |
| Years of education | -0.467 | -0.494 | -0.591*** | -0.470** | 0.098 | 0.088 | -0.110 | -0.013 |
| | (0.365) | (0.357) | (0.211) | (0.221) | (0.177) | (0.197) | (0.144) | (0.146) |
| College completion | 0.033 | 0.036 | -0.070 | -0.057 | -0.002 | 0.010 | 0.023 | 0.028 |
| | (0.031) | (0.033) | (0.045) | (0.049) | (0.041) | (0.051) | (0.033) | (0.035) |
| Residence in Distrito Federal | 0.071 | 0.094* | 0.097** | 0.070 | -0.082 | -0.066 | 0.064* | 0.055 |
| | (0.052) | (0.050) | (0.048) | (0.051) | (0.056) | (0.063) | (0.033) | (0.036) |
| Formal employment | -0.041 | -0.046 | 0.058 | 0.032 | 0.015 | 0.034 | -0.019 | 0.008 |
| | (0.076) | (0.080) | (0.051) | (0.056) | (0.055) | (0.060) | (0.044) | (0.046) |
| Public sector job | -0.063 | -0.061 | 0.040 | 0.023 | -0.093 | -0.026 | -0.023 | 0.020 |
| | (0.083) | (0.084) | (0.066) | (0.072) | (0.077) | (0.084) | (0.058) | (0.059) |
| Log annual labor earnings | -0.198 | -0.187 | -0.105 | -0.016 | 0.007 | 0.162 | -0.057 | 0.043 |
| | (0.182) | (0.180) | (0.102) | (0.106) | (0.125) | (0.130) | (0.096) | (0.105) |
| | Science (5) | | Business (6) | | Other area of study (7) | | Teaching (8) | |
| Dependent variable | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference | unadjusted difference | adjusted difference |
| Years of education | -0.187 | -0.058 | -0.174 | -0.033 | -0.088 | -0.074 | -0.216 | -0.113 |
| | (0.253) | (0.279) | (0.175) | (0.181) | (0.182) | (0.181) | (0.159) | (0.163) |
| College completion | -0.025 | 0.018 | -0.055 | -0.040 | -0.020 | -0.029 | 0.027 | 0.010 |
| | (0.066) | (0.066) | (0.049) | (0.052) | (0.042) | (0.042) | (0.040) | (0.040) |
| Residence in Distrito Federal | -0.031 | -0.015 | 0.047 | 0.066* | -0.018 | -0.026 | 0.031 | 0.031 |
| | (0.053) | (0.057) | (0.036) | (0.038) | (0.040) | (0.042) | (0.028) | (0.028) |
| Formal employment | 0.066 | 0.080 | 0.007 | 0.008 | -0.008 | 0.005 | 0.005 | 0.012 |
| | (0.063) | (0.066) | (0.045) | (0.047) | (0.044) | (0.044) | (0.038) | (0.040) |
| Public sector job | -0.189** | -0.189** | -0.161** | -0.116* | 0.061 | 0.070 | 0.021 | 0.041 |
| | (0.086) | (0.090) | (0.064) | (0.068) | (0.066) | (0.067) | (0.058) | (0.059) |
| Log annual labor earnings | -0.045 | 0.053 | -0.271*** | -0.241** | 0.035 | 0.093 | -0.307*** | -0.231** |
| | (0.140) | (0.145) | (0.097) | (0.099) | (0.090) | (0.085) | (0.097) | (0.096) |

NOTE. The sample includes all persons who matriculated at UnB between 2-2004 and 2-2005. In this analysis, the CPFs of enrolled students are corrected, and college completion is based on UnB records, not RAIS. The table displays the estimated coefficient on quota status from linear regressions that control jointly for major and semester of exam. "Adjusted" coefficients come from regressions that also control for UnB entrance exam score. Areas of study are ranked by selectivity. Robust standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 4**
**RD analysis of 2012 education and labor outcomes around UnB admission cutoffs for applicants between 2004 and 2005, stratified by gender**

| | Treatment effects | | | | | |
|---|---|---|---|---|---|---|
| | Both genders | | Male applicants | | Female applicants | |
| Dependent variable | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota |
| Admitted to UnB | 0.984*** | 0.991*** | 0.982*** | 0.991*** | 0.987*** | 0.987*** |
| | (0.003) | (0.005) | (0.004) | (0.006) | (0.005) | (0.010) |
| Years of education | 0.080 | 0.459** | 0.028 | 0.720*** | 0.148 | 0.159 |
| | (0.077) | (0.183) | (0.106) | (0.277) | (0.113) | (0.261) |
| College completion | 0.033 | 0.099* | 0.021 | 0.158** | 0.051 | 0.013 |
| | (0.023) | (0.051) | (0.031) | (0.072) | (0.035) | (0.076) |
| Residence in Distrito Federal | 0.114*** | 0.088** | 0.114*** | 0.108* | 0.106** | 0.073 |
| | (0.024) | (0.043) | (0.030) | (0.058) | (0.041) | (0.072) |
| Formal employment | 0.047** | -0.003 | 0.054** | 0.026 | 0.039 | -0.052 |
| | (0.021) | (0.039) | (0.027) | (0.056) | (0.035) | (0.064) |
| Public sector job | 0.068** | -0.051 | 0.103*** | -0.067 | 0.006 | 0.027 |
| | (0.029) | (0.059) | (0.036) | (0.078) | (0.051) | (0.105) |
| Log annual labor earnings | 0.104* | 0.058 | 0.157** | 0.328** | 0.031 | -0.031 |
| | (0.055) | (0.107) | (0.070) | (0.150) | (0.096) | (0.177) |

NOTE. The sample includes UnB applicants between 2-2004 and 2-2005 including (a) all persons who took the entrance exam and were admitted and (b) all persons who were not admitted but had an exam score in the 90th percentile or higher. Additional controls include normalized entrance exam score x non-quota applicant, normalized entrance exam score x quota applicant, indicator for above cutoff x normalized entrance exam score x non-quota applicant, indicator for above cutoff x normalized entrance exam score x quota applicant, and a set of binary indicators for admissions track (jointly determined by major, quota/non-quota system, and semester of exam). Robust standard errors in parentheses are adjusted for clustering on individuals. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Table 5**
**RD analysis of 2012 education and labor outcomes around UnB admission cutoffs for applicants between 2004 and 2005, stratified by area of study**

| | Treatment effects | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Professional (1) | | Engineering (2) | | Health (3) | | Social science (4) | |
| Dependent variable | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota |
| Admitted to UnB | 0.997*** | 1.000*** | 0.987*** | 0.992*** | 0.990*** | 0.986*** | 0.995*** | 0.993*** |
| | (0.000) | (0.000) | (0.006) | (0.011) | (0.008) | (0.015) | (0.003) | (0.016) |
| Years of education | 0.495* | 0.680 | -0.047 | 0.336 | -0.040 | 0.268 | 0.132 | 0.355 |
| | (0.256) | (0.682) | (0.225) | (0.649) | (0.273) | (0.488) | (0.178) | (0.457) |
| College completion | 0.114* | 0.174 | 0.015 | 0.066 | 0.002 | 0.020 | 0.050 | 0.005 |
| | (0.068) | (0.171) | (0.065) | (0.170) | (0.077) | (0.139) | (0.057) | (0.151) |
| Residence in Distrito Federal | 0.191** | 0.182 | 0.172** | 0.107 | 0.172* | 0.215 | 0.123** | 0.094 |
| | (0.084) | (0.129) | (0.073) | (0.132) | (0.092) | (0.134) | (0.061) | (0.129) |
| Formal employment | -0.061 | 0.063 | 0.091 | 0.034 | 0.104 | -0.107 | 0.123** | -0.066 |
| | (0.072) | (0.148) | (0.060) | (0.113) | (0.075) | (0.115) | (0.053) | (0.122) |
| Public sector job | 0.045 | -0.039 | 0.117 | 0.263 | 0.088 | -0.091 | 0.081 | -0.227 |
| | (0.085) | (0.202) | (0.078) | (0.186) | (0.106) | (0.195) | (0.071) | (0.155) |
| Log annual labor earnings | 0.541** | 0.492 | -0.021 | 0.552* | -0.075 | 0.107 | -0.052 | -0.086 |
| | (0.223) | (0.465) | (0.150) | (0.302) | (0.176) | (0.347) | (0.136) | (0.263) |

| | Treatment effects | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Science (5) | | Business (6) | | Other area of study (7) | | Teaching (8) | |
| Dependent variable | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota |
| Admitted to UnB | 0.985*** | 0.984*** | 0.977*** | 0.982*** | 0.968*** | 1.006*** | 0.972*** | 0.990*** |
| | (0.009) | (0.021) | (0.011) | (0.014) | (0.011) | (0.015) | (0.008) | (0.011) |
| Years of education | 0.081 | -0.082 | 0.213 | 0.528 | -0.034 | 0.791* | 0.033 | 0.183 |
| | (0.287) | (0.662) | (0.252) | (0.718) | (0.251) | (0.454) | (0.266) | (0.388) |
| College completion | 0.029 | -0.041 | 0.086 | 0.068 | -0.001 | 0.256** | 0.031 | 0.044 |
| | (0.091) | (0.193) | (0.077) | (0.178) | (0.077) | (0.126) | (0.078) | (0.118) |
| Residence in Distrito Federal | 0.121 | -0.255* | 0.006 | 0.024 | 0.071 | 0.198* | 0.059 | 0.106 |
| | (0.079) | (0.131) | (0.073) | (0.126) | (0.071) | (0.118) | (0.067) | (0.106) |
| Formal employment | -0.047 | -0.245** | 0.068 | 0.191 | -0.014 | -0.102 | 0.116* | 0.093 |
| | (0.069) | (0.123) | (0.066) | (0.147) | (0.069) | (0.095) | (0.063) | (0.095) |
| Public sector job | 0.044 | -0.452*** | -0.105 | -0.173 | 0.158* | 0.049 | 0.046 | -0.070 |
| | (0.098) | (0.160) | (0.094) | (0.198) | (0.089) | (0.158) | (0.085) | (0.151) |
| Log annual labor earnings | -0.143 | 0.373 | 0.066 | -0.280 | 0.059 | -0.299 | 0.291** | -0.360* |
| | (0.174) | (0.506) | (0.165) | (0.366) | (0.189) | (0.272) | (0.145) | (0.216) |

NOTE. The sample includes UnB applicants between 2-2004 and 2-2005 including (a) all persons who took the entrance exam and were admitted and (b) all persons who were not admitted but had an exam score in the 90th percentile or higher. Additional controls include normalized entrance exam score x non-quota applicant, normalized entrance exam score x quota applicant, indicator for above cutoff x normalized entrance exam score x non-quota applicant, indicator for above cutoff x normalized entrance exam score x quota applicant, and a set of binary indicators for admissions track (jointly determined by major, quota/non-quota system, and semester of exam). Areas of study are ranked by selectivity. Robust standard errors in parentheses are adjusted for clustering on individuals. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Appendix Table 1**
**RD analysis of demographic characteristics around UnB admission cutoffs for applicants between 2004 and 2005**

| | Treatment effects | |
|---|---|---|
| Dependent variable | above cutoff x non-quota | above cutoff x quota |
| Female | 0.015 | 0.012 |
| | (0.020) | (0.042) |
| Birth year | 0.210 | 0.082 |
| | (0.363) | (0.638) |
| Completed UnB admissions survey | 0.010 | 0.056 |
| | (0.021) | (0.034) |
| Mother college completion | -0.003 | -0.004 |
| | (0.033) | (0.045) |
| Family monthly income | | |
| R$750 or less | -0.011 | 0.020 |
| | (0.018) | (0.038) |
| R$750-2500 | 0.025 | -0.007 |
| | (0.030) | (0.050) |
| R$2500-5000 | 0.009 | -0.017 |
| | (0.030) | (0.044) |
| R$5000 or more | -0.019 | 0.009 |
| | (0.029) | (0.027) |
| Don't know | -0.004 | -0.004 |
| | (0.022) | (0.031) |
| Identified as PPI | -0.027 | 0.001 |
| | (0.035) | (0.007) |

NOTE. The sample includes UnB applicants between 2-2004 and 2-2005 including (a) all persons who took the entrance exam and were admitted and (b) all persons who were not admitted but had an exam score in the 90th percentile or higher. Additional controls include normalized entrance exam score x non-quota applicant, normalized entrance exam score x quota applicant, indicator for above cutoff x normalized entrance exam score x non-quota applicant, indicator for above cutoff x normalized entrance exam score x quota applicant, and a set of binary indicators for admissions track (jointly determined by major, quota/non-quota system, and semester of exam). Robust standard errors in parentheses are adjusted for clustering on individuals. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Appendix Table 2**
**RD analysis of 2012 education and labor outcomes with bandwidth sample restriction**

| | Treatment effects | | | | | |
|---|---|---|---|---|---|---|
| | Both genders | | Male applicants | | Female applicants | |
| Dependent variable | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota |
| Admitted to UnB | 0.986*** | 0.994*** | 0.987*** | 0.997*** | 0.984*** | 0.989*** |
| | (0.004) | (0.007) | (0.005) | (0.008) | (0.007) | (0.012) |
| Years of education | 0.050 | 0.438** | 0.002 | 0.568* | 0.128 | 0.066 |
| | (0.090) | (0.211) | (0.129) | (0.327) | (0.132) | (0.322) |
| College completion | 0.017 | 0.076 | -0.001 | 0.098 | 0.049 | -0.035 |
| | (0.027) | (0.061) | (0.038) | (0.087) | (0.041) | (0.096) |
| Residence in Distrito Federal | 0.077*** | 0.074 | 0.067* | 0.088 | 0.073 | 0.105 |
| | (0.027) | (0.052) | (0.036) | (0.068) | (0.046) | (0.097) |
| Formal employment | 0.068*** | 0.022 | 0.081** | 0.073 | 0.056 | -0.062 |
| | (0.025) | (0.047) | (0.032) | (0.072) | (0.040) | (0.075) |
| Public sector job | 0.057* | -0.124* | 0.068 | -0.187* | 0.026 | 0.044 |
| | (0.034) | (0.069) | (0.044) | (0.099) | (0.060) | (0.127) |
| Log annual labor earnings | 0.085 | -0.052 | 0.106 | 0.341** | 0.043 | -0.178 |
| | (0.063) | (0.123) | (0.081) | (0.174) | (0.108) | (0.220) |

NOTE. Only normalized entrance exam scores between -80 and +80 were included in the estimation sample. Additional controls include normalized entrance exam score x non-quota applicant, normalized entrance exam score x quota applicant, indicator for above cutoff x normalized entrance exam score x non-quota applicant, indicator for above cutoff x normalized entrance exam score x quota applicant, and a set of binary indicators for admissions track (jointly determined by major, quota/non-quota system, and semester of exam). Robust standard errors in parentheses are adjusted for clustering on individuals. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Appendix Table 3**
**RD analysis of 2012 education and labor outcomes with CPF quality sample restriction**

| | Treatment effects | | | | | |
|---|---|---|---|---|---|---|
| | Both genders | | Male applicants | | Female applicants | |
| Dependent variable | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota | above cutoff x non-quota | above cutoff x quota |
| Admitted to UnB | 0.985*** | 0.989*** | 0.981*** | 0.990*** | 0.988*** | 0.981*** |
| | (0.003) | (0.005) | (0.004) | (0.007) | (0.005) | (0.011) |
| Years of education | 0.106 | 0.436** | 0.073 | 0.786*** | 0.138 | 0.090 |
| | (0.080) | (0.188) | (0.110) | (0.284) | (0.121) | (0.269) |
| College completion | 0.039 | 0.097* | 0.029 | 0.181** | 0.052 | 0.011 |
| | (0.024) | (0.053) | (0.033) | (0.074) | (0.037) | (0.078) |
| Residence in Distrito Federal | 0.107*** | 0.091** | 0.107*** | 0.106* | 0.103** | 0.145* |
| | (0.025) | (0.044) | (0.032) | (0.058) | (0.043) | (0.074) |
| Formal employment | 0.047** | -0.002 | 0.054* | 0.018 | 0.039 | -0.050 |
| | (0.022) | (0.040) | (0.028) | (0.058) | (0.037) | (0.068) |
| Public sector job | 0.063** | -0.090 | 0.103*** | -0.094 | -0.006 | -0.067 |
| | (0.030) | (0.061) | (0.038) | (0.080) | (0.053) | (0.111) |
| Log annual labor earnings | 0.135** | 0.002 | 0.189** | 0.264* | 0.070 | -0.136 |
| | (0.057) | (0.104) | (0.074) | (0.147) | (0.098) | (0.173) |

NOTE. In this analysis, "partial name" CPF matches were excluded from the estimation sample. Additional controls include normalized entrance exam score x non-quota applicant, normalized entrance exam score x quota applicant, indicator for above cutoff x normalized entrance exam score x non-quota applicant, indicator for above cutoff x normalized entrance exam score x quota applicant, and a set of binary indicators for admissions track (jointly determined by major, quota/non-quota system, and semester of exam). Robust standard errors in parentheses are adjusted for clustering on individuals. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

The **Economics and Politics (CNPq) Research Group** started publishing its members' working papers on June 12, 2013. Please check the list below and click at http://econpolrg.com/working-papers/ to access all publications.

| Number | Date | Publication |
|---|---|---|
| 63/2016 | 03-30-2016 | Black movement: Estimating the effects of affirmative action in college admissions on education and labor market outcomes, Andrew Francis-Tan and Maria Tannuri-Pianto |
| 62/2016 | 01-13-2016 | Electronic voting and Social Spending: The impact of enfranchisement on municipal public spending in Brazil, Rodrigo Schneider, Diloá Athias and Maurício Bugarin |
| 61/2015 | 12-02-2015 | Alunos de inclusão prejudicam seus colegas? Uma avaliação com dados em painel de alunos da rede municipal de São Paulo, Bruna Guidetti, Ana Carolina Zoghbi and Rafael Terra |
| 60/2015 | 12-02-2015 | Impacto de programa Mais Educação em indicadores educacionais, Luís Felipe Batista de Oliveira and Rafael Terra |
| 59/2015 | 10-21-2015 | Eficiência de custos operacionais das companhias de distribuição de energia elétrica (CDEEs) no Brasil: Uma aplicação (DEA & TOBIT) em dois estágios, Daniel de Pina Fernandes and Moisés de Andrade Resende Filho |
| 58/2015 | 10-14-2015 | Determinantes do risco de crédito rural no Brasil: uma crítica às renegociações da dívida rural, Lucas Braga de Melo and Moisés de Andrade Resende Filho |
| 57/2015 | 10-07-2015 | Distribuição da riqueza no Brasil: Limitações a uma estimativa precisa a partir dos dados tabulados do IRPF disponíveis, Marcelo Medeiros |
| 56/2015 | 10-01-2015 | A composição da desigualdade no Brasil. Conciliando o Censo 2010 e os dados do Imposto de Renda, Marcelo Medeiros, Juliana de Castro Galvão and Luísa Nazareno |
| 55/2015 | 09-24-2015 | A estabilidade da desigualdade no Brasil entre 2006 e 2012: resultados adicionais, Marcelo Medeiros and Pedro H. G. F. Souza |
| 54/2015 | 09-24-2015 | Reciclagem de plataformas de petróleo: ônus ou bônus?, Roberto N. P. di Cillo |
| 53/2015 | 09-09-2015 | A Progressividade do Imposto de Renda Pessoa Física no Brasil, Fábio Castro and Mauricio S. Bugarin |
| 52/2015 | 07-03-2015 | Measuring Parliaments: Construction of Indicators of Legislative Oversight, Bento Rodrigo Pereira Monteiro and Denílson Banderia Coêlho |
| 51/2015 | 06-29-2015 | A didactic note on the use of Benford's Law in public works auditing, with an application to the construction of Brazilian Amazon Arena 2014 World Cup soccer stadium, Mauricio S. Bugarin and Flavia Ceccato Rodrigues da Cunha |
| 50/2015 | 04-29-2015 | Accountability and yardstick competition in the public provision of education, Rafael Terra and Enlinson Mattos |
| 49/2015 | 04-15-2015 | Understanding Robert Lucas (1967-1981), Alexandre F. S. Andrada |
| 48/2015 | 04-08-2015 | Common Labor Market, Attachment and Spillovers in a Large Federation, Emilson Caputo Delfino Silva and Vander Mendes Lucas |
| 47/2015 | 03-27-2015 | Tópicos da Reforma Política sob a Perspectiva da Análise Econômica do Direito, Pedro Fernando Nery and Fernando B. Meneguin |
| 46/2014 | 12-17-2014 | The Effects of Wage and Unemployment on Crime Incentives - An Empirical Analysis of Total, Property and Violent Crimes, Paulo Augusto P. de Britto and Tatiana Alessio de Britto |
| 45/2014 | 12-10-2014 | Políticas Públicas de Saúde Influenciam o Eleitor?, Hellen Chrytine Zanetti Matarazzo |
| 44/2014 | 12-04-2014 | Regulação Ótima e a Atuação do Judiciário: Uma Aplicação de Teoria dos Jogos, Maurício S. Bugarin and Fernando B. Meneguin |
| 43/2014 | 11-12-2014 | De Facto Property Rights Recognition, Labor Supply and Investment of the Poor in Brazil, Rafael Santos Dantas and Maria Tannuri-Pianto |
| 42/2014 | 11-05-2014 | On the Institutional Incentives Faced by Brazilian Civil Servants, Mauricio S. Bugarin and Fernando B. Meneguin |
| 41/2014 | 10-13-2014 | Uma Introdução à Teoria Econômica da Corrupção: Definição, Taxonomia e Ensaios Selecionados, Paulo Augusto P. de Britto |
| 40/2014 | 10-06-2014 | Um modelo de jogo cooperativo sobre efeitos da corrupção no gasto público, Rogério Pereira and Tatiane Almeida de Menezes |
| 39/2014 | 10-02-2014 | Uma análise dos efeitos da fusão ALL-Brasil Ferrovias no preço do frete ferroviário de soja no Brasil, Bruno Ribeiro Alvarenga and Paulo Augusto P. de Britto |

| Number | Date | Publication |
|---|---|---|
| 38/2014 | 08-27-2014 | Comportamentos estratégicos entre municípios no Brasil, Vitor Lima Carneiro & Vander Mendes Lucas |
| 37/2014 | 08-20-2014 | Modelos Microeconômicos de Análise da Litigância, Fábio Avila de Castro |
| 36/2014 | 06-23-2014 | Uma Investigação sobre a Focalização do Programa Bolsa Família e seus Determinantes Imediatos. André P. Souza, Plínio P. de Oliveira, Janete Duarte, Sérgio R. Gadelha & José de Anchieta Neves |
| 35/2014 | 06-22-2014 | Terminais de Contêineres no Brasil: Eficiência Intertemporal. Leopoldo Kirchner and Vander Lucas |
| 34/2014 | 06-06-2014 | Lei 12.846/13: atrai ou afugenta investimentos? Roberto Neves Pedrosa di Cillo |
| 33/2013 | 11-27-2013 | Vale a pena ser um bom gestor? Comportamento Eleitoral e Reeleição no Brasil, Pedro Cavalcante |
| 32/2013 | 11-13-2013 | A pressa é inimiga da participação (e do controle)? Uma análise comparativa da implementação de programas estratégicos do governo federal, Roberto Rocha C. Pires and Alexandre de Avila Gomide |
| 31/2013 | 10-30-2013 | Crises de segurança do alimento e a demanda por carnes no Brasil, Moisés de Andrade Resende Filho, Karina Junqueira de Souza and Luís Cristóvão Ferreira Lima |
| 30/2013 | 10-16-2013 | Ética & Incentivos: O que diz a Teoria Econômica sobre recompensar quem denuncia a corrupção? Maurício Bugarin |
| 29/2013 | 10-02-2013 | Intra-Village Expansion of Welfare Programs, M. Christian Lehmann |
| 28/2013 | 09-25-2013 | Interações verticais e horizontais entre governos e seus efeitos sobre as decisões de descentralização educacional no Brasil, Ana Carolina Zoghbi, Enlinson Mattos and Rafael Terra |
| 27/2013 | 09-18-2013 | Partidos, facções e a ocupação dos cargos de confiança no executivo federal (1999-2011), Felix Lopez, Mauricio Bugarin and Karina Bugarin |
| 26/2013 | 09-11-2013 | Metodologias de Análise da Concorrência no Setor Portuário, Pedro H. Albuquerque, Paulo P. de Britto, Paulo C. Coutinho, Adelaida Fonseca, Vander M. Lucas, Paulo R. Lustosa, Alexandre Y. Carvalho and André R. de Oliveira |
| 25/2013 | 09-04-2013 | Balancing the Power to Appoint officers, Salvador Barberà and Danilo Coelho |
| 24/2013 | 08-28-2013 | Modelos de Estrutura do Setor Portuário para Análise da Concorrência, Paulo C. Coutinho, Paulo P. de Britto, Vander M. Lucas, Paulo R. Lustosa, Pedro H. Albuquerque, Alexandre Y. Carvalho, Adelaida Fonseca and André Rossi de Oliveira |
| 23/2013 | 08-21-2013 | Hyperopic Strict Topologies, Jaime Orillo and Rudy José Rosas Bazán |
| 22/2013 | 08-14-2013 | Há Incompatibilidade entre Eficiência e Legalidade? Fernando B. Meneguin and Pedro Felipe de Oliveira Santos |
| 21/2013 | 08-07-2013 | A Note on Equivalent Comparisons of Information Channels, Luís Fernando Brands Barbosa and Gil Riella |
| 20/2013 | 07-31-2013 | Vertical Integration on Health Care Markets: Evidence from Brazil, Tainá Leandro and José Guilherme de Lara Resende |
| 18/2013 | 07-17-2013 | Algunas Nociones sobre el Sistema de Control Público en Argentina con Mención al Caso de los Hospitales Públicos de la Provincia de Mendoza, Luis Federico Giménez |
| 17/2013 | 07-10-2013 | Mensuração do Risco de Crédito em Carteiras de Financiamentos Comerciais e suas Implicações para o Spread Bancário, Paulo de Britto and Rogério Cerri |
| 16/2013 | 07-03-2013 | Previdências dos Trabalhadores dos Setores Público e Privado e Desigualdade no Brasil, Pedro H. G. F. de Souza and Marcelo Medeiros |
| 15/2013 | 06-26-2013 | Incentivos à Corrupção e à Inação no Serviço Público: Uma análise de desenho de mecanismos, Maurício Bugarin and Fernando Meneguin |
| 14/2013 | 06-26-2013 | The Decline in inequality in Brazil, 2003–2009: The Role of the State, Pedro H. G. F. de Souza and Marcelo Medeiros |
| 13/2013 | 06-26-2013 | Productivity Growth and Product Choice in Fisheries: the Case of the Alaskan pollock Fishery Revisited, Marcelo de O. Torres and Ronald G. Felthoven |
| 12/2013 | 06-19-2003 | The State and income inequality in Brazil, Marcelo Medeiros and Pedro H. G. F. de Souza |
| 11/2013 | 06-19-2013 | Uma alternativa para o cálculo do fator X no setor de distribuição de energia elétrica no Brasil, Paulo Cesar Coutinho and Ângelo Henrique Lopes da Silva |

| Number | Date | Publication |
|---|---|---|
| 10/2013 | 06-12-2013 | Mecanismos de difusão de Políticas Sociais no Brasil: uma análise do Programa Saúde da Família, Denilson Bandeira Coêlho, Pedro Cavalcante and Mathieu Turgeon |
| 09/2013 | 06-12-2103 | A Brief Analysis of Aggregate Measures as an Alternative to the Median at Central Bank of Brazil's Survey of Professional Forecasts, Fabia A. Carvalho |
| 08/2013 | 06-12-2013 | On the Optimality of Exclusion in Multidimensional Screening, Paulo Barelli, Suren Basov, Mauricio Bugarin and Ian King |
| 07/2013 | 06-12-2013 | Desenvolvimentos institucionais recentes no setor de telecomunicações no Brasil, Rodrigo A. F. de Sousa, Nathalia A. de Souza and Luis C. Kubota |
| 06/2013 | 06-12-2013 | Preference for Flexibility and Dynamic Consistency, Gil Riella |
| 05/2013 | 06-12-2013 | Partisan Voluntary Transfers in a Fiscal Federation: New evidence from Brazil, Mauricio Bugarin and Ricardo Ubrig |
| 04/2013 | 06-12-2013 | How Judges Think in the Brazilian Supreme Court: Estimating Ideal Points and Identifying Dimensions, Pedro F. A. Nery Ferreira and Bernardo Mueller |
| 03/2013 | 06-12-2013 | Democracy, Accountability, and Poverty Alleviation in Mexico: Self-Restraining Reform and the Depoliticization of Social Spending, Yuriko Takahashi |
| 02/2013 | 06-12-2013 | Yardstick Competition in Education Spending: a Spatial Analysis based on Different Educational and Electoral Accountability Regimes, Rafael Terra |
| 01/2013 | 06-12-2013 | On the Representation of Incomplete Preferences under Uncertainty with Indecisiveness in Tastes, Gil Riella |