Universidade Federal de Pernambuco
Centro de Ciências Sociais Aplicadas
Departamento de Economia
Programa de Pós-Graduação em Economia

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Essays on Education Economics

Recife
2018

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Tese submetida ao programa de Pós-graduação em Economia da Universidade Federal de Pernambuco, como requisito final para obtenção do grau de **Doutor em Economia**.

Área de Concentração: Economia Aplicada

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Recife
2018

Catalogação na Fonte
Bibliotecária Ângela de Fátima Correia Simões, CRB4-773

C957e Cruz, Giuseppe Trevisan
Essays on education economics / Giuseppe Trevisan Cruz. - 2018.
112 folhas : il. 30 cm.

Orientador: Prof. Dr. Breno Ramos Sampaio e Coorientador Prof. Dr. Gustavo Ramos Sampaio.
Tese (Doutorado em Economia) – Universidade Federal de Pernambuco, CCSA, 2018.
Inclui referências e apêndices.

1. Educação de elite. 2. Efeito dos pares. 3. Mercado de trabalho. I. Sampaio, Breno Ramos (Orientador). II. Sampaio, Gustavo Ramos (Coorientador). III. Título.

331 CDD (22.ed.) UFPE (CSA 2018 –081)

UNIVERSIDADE FEDERAL DE PERNAMBUCO
CENTRO DE CIÊNCIAS SOCIAIS APLICADAS
DEPARTAMENTO DE ECONOMIA
PIMES/PROGRAMA DE PÓS-GRADUAÇÃO EM ECONOMIA

PARECER DA COMISSÃO EXAMINADORA DE DEFESA DE TESE DO DOUTORADO EM ECONOMIA DE:

GIUSEPPE TREVISAN CRUZ

A Comissão Examinadora composta pelos professores abaixo, sob a presidência do primeiro, considera o Candidato Giuseppe Trevisan Cruz **APROVADO**.

Recife, 26/02/2018

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AGRADECIMENTOS

Este trabalho é uma consequência de anos de dedicação e renúncias, que culminam com a satisfação de dever cumprido. Sem a presença e apoio de minha esposa, família e amigos, jamais teria sido possível percorrer esta estrada. Esta tese é dedicada a essas pessoas que tanto contribuíram para o meu crescimento e amadurecimento profissional.

Primeiramente, gostaria de agradecer à minha esposa, Livia, por ser minha maior companheira em todos os momentos. Seu incentivo e amor foram essenciais nesta jornada, e serei eternamente grato a você.

Agradeço também à minha família, por proporcionar toda a estrutura de que precisei para seguir o caminho da academia, incentivando-me e investindo em meus estudos. Sou grato aos meus pais, Margarida e Idevaldo, e às minhas queridas irmãs, Daniele e Deborah, por todo esse apoio.

Não poderia deixar de agradecer a meus amigos do PIMES, que foram essenciais para meu desenvolvimento acadêmico: Vinícius, Vitor, Botler, Francisco, Rodrigo, Lucas, Cleyton, Dieison, Leonardo, Felipe, Julia, Angélica, Tássia, Guilherme Amorim, Flavius. Obrigado por todas as discussões, companheirismo e por serem uma referência para mim.

Ao meu orientador, Breno Sampaio, por acreditar no meu potencial e por ser peça fundamental em minha formação como acadêmico. Também gostaria de agradecer ao meu coorientador, Gustavo Sampaio, por todas as discussões e companheirismo. Agradeço também a todos os membros da banca por suas contribuições, deixando o trabalho mais rico.

Agradeço a todos os meus professores do departamento de Economia, que também foram importantes para minha formação. Sou grato também à CAPES por ter financiado este estudo.

Hoje, tenho um pouco de cada um em mim, e levarei comigo nos novos caminhos que trilharei. Obrigado, eternamente.

ABSTRACT

This dissertation is intrinsic related to the literature on Education Economics, where we investigate some inputs of the education production function. We possess data sources comprising characteristics of applicants and students to one of the major flagship universities in Brazil and very restrict data on tax-registered firms, that enable us exploit the influence of two of these inputs on a series of academic and labor outcomes. In the first chapter, we examine the labor market returns to attending free elite higher education. Using restrict-access data from a flagship university in Brazil and from tax-registered firms, we explore an entrance rule that generates exogenous variation close to admission cutoffs, allowing us to compare marginal applicants and to estimate the causal effect of enrollments on salaries and occupations. Our findings indicate that enrolling in the elite free university raises wage premiums on 8% and the likelihood of reach reputable occupations in the future. The benefits are more expressive among female applicants and those with poorer backgrounds. We also provide evidence that, much more than just having the advantage of have a higher degree diploma, the elite wage premiums are guided by better matches on jobs demanding more specialized tasks. Our results are valuable for policy debates related to interventions aiming on promoting access to selective higher education for disadvantaged social groups. In the second chapter, we address the impact of an almost unexplored side of peer effect on students achievement and incentives to graduation regarding social comparative advantages. We estimate the effect of perceived rank in college and show that being last among the best increases the willingness to switch careers and reduces the likelihood of having a more prestigious occupation. To do so, we exploit a discontinuity in the class assignment in a flagship university in Brazil that sends the median student to either a better or a worse class in the same major program. Since the skill difference between classes varies within and between programs, we find that the ranking effect can be cancelled out by a high increase in peer quality. Our findings imply that the perceived rank sends a misleading signal, making similar students in the same program take distinct decisions and have different long-term outcomes. Higher parental education and stronger convictions about future earnings reduce the influence of this signal.

Keywords: Elite Education. Peer effects. Ranking Effects. Regression Discontinuity Design. Education Economics. Labor Market.

RESUMO

Esta tese está intrinsecamente associada à literatura de Economia da Educação, onde pretende-se investigar alguns insumos da função de produção educacional. Sob a posse de dados que compreendem características de aplicantes (e também dos alunos já matriculados) em uma das maiores universidades do Brasil e dados restritos de firmas, podemos explorar a inflluência de dois desses insumos mencionados sobre indicadores acadêmicos e de mercado de trabalho. O primeiro capítulo examina os retornos no mercado de trabalho oriundos de se cursar uma instituição elite de ensino superior. Utilizando um banco de dados restrito de uma faculdade de referência no Brasil e dados de firmas, nós exploramos uma regra de entrada na faculdade que gera uma variação exógena perto do ponto de corte de admissão, o que permite comparar aplicantes que estão à margem do ingresso e estimar o efeito causal de se matricular na faculdade sobre salários e ocupações. Nossos achados indicam que a matrícula na universidade de elite aumenta o prêmio salarial em 8% e a probabilidade de alcançar ocupações mais prestigiosas no futuro. Os benefícios são mais expressivos entre mulheres e entre aqueles com piores backgrounds. Também mostramos evidências de que, muito mais do que apenas ter a vantagem de possuir um diploma universitário, são as melhores alocações em trabalhos que exigem maior nível de habilidade que guiam os prêmios salariais. Nossos resultados são valiosos para debates de políticas relacionadas a intervenções que visam promover acesso de grupos com grandes desvantagens sociais na educação de elite. No segundo capítulo, abordamos o impacto de um efeito de pares pouco explorado na literatura, que está relacionado à vantagens comparativas, sobre o desempenho acadêmico de universitários (performance) e incentivos para conclusão do curso. Nós desentrelaçamos o efeito do rankeamento ordinal da qualidade dos pares, e mostramos que ser o pior entre os melhores da turma aumenta a chance de se trocar de carreira e reduz a probabilidade de conseguir uma ocupação futura mais prestigiosa. Para isso, exploramos uma descontinuidade na determinação de turma que envia o estudante universitário mediano tanto ara uma turma melhor quanto para uma pior dentro de um mesmo curso.Uma vez que a diferença de habilidade entre as turmas variam entre e dentro do curso, encontrou-se que o efeito do rankeamento se cancela com o aumento da qualidade dos pares. Os resultados implicam que a percepção de sua posição na distribuição de habilidades envia um sinal enganoso, fazendo que estudantes parecidos num mesmo curso tomem decisões diferentes e obtenham resultados diferentes no futuro. O alto nível de educação dos pais e fortes convicções sobre o salário futuro reduzem a influência desse sinal.

Palavras-chave: Educação de elite. Efeito dos pares. Efeito do rank. Regressão Descontínua. Economia da Educação. Mercado de trabalho.

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SUMMARY

CHAPTER 1

The Economic Effects of Free Elite Education: Evidence from a Flagship University in Brazil

1.1 Introduction

Among developing countries, Brazil is known for having a significant fraction of its economically active population with lower levels of schooling. This is partly a legacy of the rare privilege to afford higher education. But over the past decades, access to higher education had become more accessible, specially for young students. According to the Brazilian Ministry of Education (MEC), enrollments in higher education institutions substantially enhanced from 2006-2016, where the public system represents 59% of this increase. Public universities are usually the most demanded institutions — mainly because they do not charge tuition fees —, figuring out among the best options in the national higher education system. In more recent years, the Government implemented affirmative actions[1] to promote the inclusion of minorities into the federal public universities, including the most prestigious ones. Without this intervention, it is very difficult to these disadvantaged groups attain these institutions. In the private system, the entrance of poorer students into selective colleges is essentially made through scholarship grants. However, the private elite institutions continue to absorb the best-performing students, making attendance on flagship colleges, in many cases, a privilege for a few.

[1]Given the high barriers for poorer background and low-performing students to entry in the elite education system, from 2012 the Ministry of Education introduced quotas to the public system for public high school students, indigenous, and Afro-descendants

Given this scenario, the role of elite education on labor market performance is of particular importance and interest for guiding students' career decisions and for policies that aim to promote access to the elite system. Using different research designs, some works related to this literature have found mixed results (Brewer et al., 1999; Dale & Krueger, 2002a, 2011; Black & Smith, 2004; Hoekstra, 2009; Anelli, 2016; Zimmerman, 2016; Jia & Hongbin, 2017), and explored different links that lead elite returns. In their contexts, tuition fees plays an important role on determining attendance to elite institutions, and they are not able to estimate the returns of a free attendance. Brazil is an ideal laboratory to explore this issue since some prestigious universities do not charge tuition fees, but the great challenge relies on how to disentangle the student's ability and background characteristics from elite attendance as both determine labor outcomes.

In this paper, we address this question and estimate the economic impacts of attending a free elite university on salaries and occupations. Using administrative data of one of the most recognized universities in Brazil, we match this information with restrict-access data on tax-registered firms and employ a regression discontinuity design to compare marginal applicants close to the admission cutoffs. Candidates compete to a place within elite programs — which they decided to apply prior to taking the entrance exam — and their admission is solely based on their final entrance score. The exogenous variation generated by the institutional entrance rule allows us to overcome the role of individual's ability and career preferences on labor outcomes and to estimate causal effects of enrollments.

Our findings reveal that students who ever enrolled in the free elite university have higher wage premiums and attain more prestigious occupations in the future. These results are more significant compared to admission impacts (threshold crossing effects). Specifically, enrollments raise hourly wages in around 8% and boost the probability of ever reach managerial posts and pursue careers in Government entities. The results are robust to a series of econometric specifications and to alternative bandwidths, and are not driven by unbalancing of baseline characteristics, selection into the labor market, or manipulation of the entrance score. Moreover, using quantile RDD, we show that these hourly gains are more pronounced among those in the lower tail of the salary distribution.[2]

The heterogeneity of the elite education effect also unveil interesting findings. While (non-free) elite education has been demonstrated to benefit more privileged groups (Hoekstra, 2009; Hastings et al., 2013; Zimmerman, 2016), we show the opposite in our context. In some manner, our results are linked to Saavedra (2009) findings. We find considerable wage premiums among applicants with poorer backgrounds — which is very correlated with lower family income — and among female candidates, specially in jobs which pay less. Individuals coming from public high schools and from less educated parents are more susceptible to reach public careers, which are known for being safer jobs in terms of long-term stability. Among females, they are more likely to ever be a manager in the future. In overall, these evidences support the idea that free elite education can minimize some labor market gaps between groups and can promote disadvantaged individuals to

[2] Andrews et al. (2012) find the opposite.

better jobs.

Our third set of results are related to elite returns across fields of study. We grouped similar programs to induce variation and allow the estimation of the parameters. We find elite education wage premiums among Health, Teaching, and Law programs, which is consistent with other findings in the literature (Zimmerman, 2016; Hastings et al., 2013; Kirkeboen et al., 2016a). In addition, we find an increase on the chance of Law elite students taking posts in public sector.

Our data does not allows us to track candidates who missed the admission cutoff or do not enrolled at the flagship university regarding their attachment into other education institutions, thus we face limitations when exploring underlying mechanisms leading elite returns. But we are pioneer on exploring a channel emerged from the labor market side and related to the quality of the job. Despite students who ever enrolled in the elite institution have higher probability of having a graduation degree in the future — which adds value on signals to the labor market demand —, our findings suggest that wage premiums are mostly due to better matches in the labor market. That is, affording jobs with specialized tasks, much more than just having a college degree, is more important on determining elite education wage premiums.

We add to the literature of elite education by estimating credible causal impacts of attending a free public university and by exploring a novel channel related to elite wage premiums. We also contribute to the growing literature on intergenerational mobility (Chetty et al., 2014), (Chen et al., 2015), showing that elite education is important to absolute mobility. Moreover, our empirical findings have policy implications. Our findings add to policy debates related to affirmative actions by giving inputs to proposal interventions aiming on promoting disadvantaged groups to accessing selective higher education.

The remainder of the paper is structured as follows. Section 2 discusses the institutional background. Section 3 presents detailed information on data sources, variables, and sample. Section 4 explains the identification strategy. In section 5, we discuss the main empirical results and explore mechanisms leading elite returns. Finally, in Section 6, we conclude the paper.

1.2 Institutional Background

1.2.1 The Flagship University

UFPE (*Universidade Federal de Pernambuco*) was founded in 1948 and is currently the major flagship university in North and Northeast of Brazil and one of the top twenty public universities in the country, according to the Ministry of Education.[3] In addition

[3] Yearly, MEC performs a stringent evaluation of Brazilian Higher Education Institutions (private and public) based in a vast range of inputs related to infrastructure, quality of majors and teachers, management effectiveness, and student's academic performance. UFPE always have been figured at the twenty best Brazilian public universities since the first MEC evaluation and is currently in the 2nd

to its high quality and reputation, it is a public university and does not charge tuition fees. Moreover, seats are not exclusively offered for local inhabitants, although only 16% of the candidates come from cities out of the Metropolitan Region of Recife, Pernambuco. Like most public universities in Brazil, UFPE is known for focusing on academic training. As a result, UFPE is the top choice of almost every high school student in the state of Pernambuco, regardless their social class and career choice.

The university offers 99 undergraduate programs[4] and, in general, is a four-year college, although some programs (34%) have a five-year duration.[5] Unlike in the US, the higher education system in Brazil requires that all students decide their major before applying to any college. Hence, UFPE students must provide several socioeconomic and family background information as well as their major preference (only one option) before taking the entrance exam. This implies that they compete for a spot at university only with those who choose similar majors. As we explain below, this setup is of particular importance for our empirical strategy and interpretation.

1.2.2 The Admission Process

Students are admitted to study solely based on their entrance exam performance called the *vestibular*.[6] About 68% of the candidates are students who have recently graduated from high school.[7] Half of these candidates is taking the *vestibular* for the first time and the other half is retaking it because they were not admitted in the previous year or plan to switch majors. The minority of candidates come from other institutions or study programs, graduated from the adult education program, or have not studied for a while. Anyone with a high school diploma or equivalent can apply to the university and, most importantly, their chances of being accepted depend exclusively on the *vestibular*. That is, the university cannot use any other admission criteria to leapfrog candidates.

The *vestibular* is held once per year over multiple days, with different subjects tested on each day. The exam has an initial stage with a broader scope covering all subjects and then a second round in which the candidate is tested in four specific subjects required by the intended major of study. In the first round, applicants are evaluated in the following subjects: Mathematics, Portuguese, a foreign language (English, French or Spanish), Literature, History, Geography, Physics, Chemistry, and Biology. The second-round exam

percentile on the distribution of institutions quality. See Table A.1 in the Appendix for the full list of institutions in the state of Pernambuco and their respectively national rank. More information about the evaluation process can be found at: http://portal.mec.gov.br.

[4]This number does not include special programs, such as those focused on distance learning and high school teachers without college degree.

[5]Due to its complexity, students must attend six years of college education to graduate in Medicine.

[6]In 2015, all programs started adopting the new national centralized entrance process (Unified Selection System, SISU) to public universities in Brazil, ending institution-specific exams.

[7]Students with high age/grade distortion may obtain secondary schooling with a method called *supletivo*, which is an alternative method to compensate the disadvantages related to opportunities in higher education assess. It basically summarizes all high school program, which usually takes 3 years, in one intensive year course.

comprises Portuguese (and a foreign language) and the three other subjects specifically required for the future program. The final entrance test score is a weighted average of the first- and second-round scores. Final entrance scores are eligible for consideration if none of the following exclusion criteria have been met: scoring 0 on one part, scoring below 2.5 on writing or scoring less than 80% of the mean of the intended major of study. Each program admits applicants from top to bottom until the seats are taken.[8]

Only a small fraction (around 10%) of the original candidates per program are admitted, given the limited number of seats. Students do not know the cutoff scores at the time of the exam nor at the time of the application, as these thresholds vary from year to year. Neither students nor the university can manipulate final scores. The final classification of candidates, organized by class and major, is fully disclosed by the admissions committee (*Comissão de Processos Seletivos e Treinamentos*, COVEST) through its website and printed on newspapers.

1.2.3 In-State Outside Options for Higher Education

Applicants who fail to be admitted at UFPE and wish to continue their education pathway have other private and public options in the state to acquire a higher degree diploma. The pool of non-selective institutions is prevailingly private (65%) and the majority of them (75%) is located in the metropolitan region of Recife. The private institutions charge very high tuition fees[9] and in recent years have been populated by students coming from the public secondary school system.[10] Due to known lack of good quality public schools in Brazil, public school students face severe barrier to entry at UFPE. According to Cavalcanti et al. (2010), for instance, their test scores are on average about 4.2-17% lower than that observed for private school students.

The higher education market in Pernambuco, specially for private institutions, has shown impressive growth by the earlier 2000s. In 2006, there were 78 higher education institutions in the state, in contrast to the ninety options in 2016. Table A.1 in the Appendix reports all the available in-state outside options and also information about their profile. The numbers on the table confirms why UFPE is a differentiated alternative for the candidates in terms of features and as a quality benchmark.

The best outside option for students in the metropolitan region is the *Universidade Federal Rural de Pernambuco* (UFRPE), which is also a public university.[11] Among

[8] We note that these eligibility criteria are only binding among very low performing students, imposing no additional restrictions to our empirical strategy.

[9] Most institutions charge at least a monthly tuition of about .4 minimum salary, which represents about 30% of average wages in the metropolitan region of Recife. In overall, the more selective the major is the higher the tuition fees. For instance, majors like Law and Medicine cannot be afforded by the average people as costs almost double their earnings.

[10] To expand assess to higher education, MEC implemented conditional scholarship programs destined to candidates who fill specific achievement prerequisites and are unable to pay the private college's fees.

[11] Unlike UFPE, UFRPE is a reputable federal higher institution which offers programs focusing (mainly) on agrarian sciences, which makes both universities complementary options.

privates, the best choice figures at the 241th national rank position. As in UFPE, the admission process for all these colleges is not centralized, allowing each of them to settle their own entrance rules. Despite these institutions offering a wide range of programs, they comprise only a subset of those available at UFPE.[12] As MEC establish standard requirements for regular operation of majors, the time to graduate within-majors and across colleges is usually the same. Furthermore, the vast majority of private colleges (profit-seeking) are more market-focused, while public and non-profit institutions, such as UFPE, focus on academic training and tend to have teaching programs in their portfolio.

1.3 Data Sources and Descriptive Statistics

1.3.1 Data Source: Flagship College Applicants

To obtain detailed academic information about applicants, we use two different data sources. The first one comes from the admissions committee (COVEST) of UFPE, which provides detailed information about every UFPE applicant, including the program chosen by the candidate, and the entrance test score for those who applied over the period 2006-2010. As we describe above, the entrance test score is the only determinant of university admission, hence it is used as the running variable for our fuzzy RD strategy, explained bellow. The fuzziness comes from the possibility of admitted candidates rejecting UFPE's offer, so the compliance rate is not perfect. To eliminate time effects and student's major preferences at the time of application, we log-standardize the entrance test score by year and program using the last student eligible to take a place in the program of admission, and the standard deviation of applicants' scores.

The COVEST data also includes a wide range of candidate's socioeconomic characteristics at the time of application, such as age, race, employment status, if attended a public or private high school, if attended a pre-college preparatory course, parent's education, the number of times she did the *vestibular* in the past, and her motivation to enter the university and to choose the major preference. With the exception of the number of *vestibular* tries, we generate binary indicators for all pre-determined student's traits. Based on these information, we restrict our sample to candidates who have 21 years old or less, which represents nearly 73% of all applicants who have a second round score in our data. Moreover, we keep only programs (77%) that have sufficient observations per year to allow for the existence of excess demand, making our estimates possible. Table A.2 illustrates the full list of UFPE programs and those included in our sample, with their expected time to graduate and field of study. We emphasize that the assignment variable distribution is obtained before we impose any restriction to the data, which makes comparisons between compliers more reliable.

The second data is UFPE's Academic Information System (Sistema de Informações

[12] Few institutions supply programs there are not included in UFPE's portfolio.

Acadêmicas, SIGA), which accurately relates the academic situation (active, graduated, or dismissed) of UFPE students until 2014 and, consequently, their enrollment status. While the entrance test score of the last admitted applicant determines the cutoff point, the enrollment determines the treatment status (a dummy variable) of the candidate. Aiming to cleanly estimate the returns of enrolling in a flagship university, we consider as enrolled those candidates who ever accepted the UFPE's offer at the time of application.[13] Assigning treatment on this manner informs the impact of free elite higher education for those who took the opportunity, which is of great interest for policy implications.

SIGA data is also valuable for recovering missing values of the gender variable obtained from COVEST, since the former has a precise registration regarding students' profile. For those who failed to be approved in *vestibular*, we recover the missing gender status on the Ministry of Finance. Unfortunately, with these two data-sets we cannot track individuals who failed to enter at UFPE regarding their enrollment into other education institutions. On the other hand, we have the advantage to track the whole sample of candidates into the formal labor market in every year and their maximum level of education attained (if employed).

1.3.2 Data Source: Earnings and Occupations

The outcomes of interest are measured using a federal restricted-access data set collecting information on tax-registered firms. The Yearly Social Information Report (*Relação Anual de Informações Sociais*, RAIS) is collected by the Brazilian Ministry of Labor. Every year, tax registered firms are legally required to report every worker formally employed during the previous calendar year. This data-set provides national coverage of the Brazilian formal labor market at the employee-employer level, allowing us to obtain earnings, the number of weekly hours worked, and occupation for each UFPE applicant working in 2002-2014. Moreover, given that RAIS also have the individual highest education level attained and the required education to the job assigned, it is possible to explore different mechanisms behind the gains in the formal labor market, for example whether higher earnings arise from additional years of experience, quantity of education, or assignment to high skilled positions. Matching the different data sources at the individual level is possible because in all data-sets students are uniquely identified on the basis of their social security number, which is required at the time of application (i.e., upon registration to take the admission exam).

As we want to understand future returns to being admitted at UFPE, we measure individual labor outcomes starting from the expected year of graduation of the competed program. For earnings, we use the sum of all salaries in a year (from 1th January to

[13] After enrolling in UFPE, students' academic pathway is uncertain. For instance, it is possible that, due to lack of motivation and persistence on finishing the chosen program, students decide to drop out or even switch programs between different colleges. Despite the fuzzy setting, the effect of interest would be more "like an intent-to-treat effect" since it captures the impact of attending the selective university regardless future withdrawal decisions.

December 31) and the average hourly wage,[14] both deflated to the December 2014 level using the Extended Consumer Price Index (IPCA). In addition to explore these outcomes in each year, we average them from the expected year of graduation onward. We define two different job positions based on the National Code of Occupations (CBO),[15] from RAIS: manager and public servant. We construct dummy variables to indicate that the applicant assigned the presumed position at least one time in the future.

In our design, all labor outcomes are conditioned to those who took a job in the future, implying that selection into the labor market may play a significant role in our results. We use RAIS to investigate employment status of the applicants since the time of application, as well as work experience (measured in years, number of jobs, and tenure).

1.3.3 Descriptive Statistics

Table 1.1 presents the data description of our sample, segregated by enrolled and non-enrolled candidates. The table reveals that the different patterns are particularly marked among these two groups. As expected, enrollees have a much higher final entrance score than applicants who did not enroll due to the high level of competition. In the labor market, they are less likely to be formally employed in the future (5 p.p. of difference), but in return, they achieve higher earnings. Despite the yearly earnings of enrolled applicants differs in about R$2,000.00 with non-enrolled ones (or 6 p.p higher), both high standard deviations suggest a very unequal distribution of gains. Moreover, enrollees differ from their counterparts in the hourly salary by around 20 p.p. The hourly salary of UFPE applicants is almost twice the size of metropolitan region of Recife ones, and it seems that enrolled students have even more advantageous returns to hour worked.

About 30% of UFPE candidates worked in the public sector in further years, and enrollees are 5 p.p. more prone to take these jobs. Candidates who take the *vestibular* also tend to occupy leadership (manager), but enrollees are more susceptible to take those positions.

In terms of demographics, applicants have nineteen years old on average, are predominantly females (58%), and live in the state at the time of application (86%). Moreover, eligible students tried more times to enter in the university (almost two attempts) and half of them attended pre-college preparatory courses. The last part of Table 1.1 also confirms that candidates who apply for UFPE have better background. They come majorly from private schools (75%) and for about 50% of them, parents (mother or father) have at least college degree. These disparities run in favour of enrolled individuals. In addition, their personal preferences for choosing the major (university) are more related to the prestige of profession (university) and to self-fulfilment (quality of the program) compared to non-

[14]We divide the average monthly salary by the monthly hours of the assigned occupation.

[15]The Ministry of Labor is responsible for recording all types of formal occupations based on the required skill and education levels, and profession. Our definition of occupation follows these records, where we grouped all jobs with the same core activity to create the job positions.

Table 1.1: Summary Statistics

	N	All		Enrolled		Non-enrolled	
		Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Final entrance score	*52,382*	-0.336	1.244	0.907	0.785	-0.897	0.978
Employed*	*52,382*	0.584	0.493	0.551	0.497	0.600	0.490
Salaries							
(ln)Earnings*	*30,609*	9.332	1.168	9.376	1.203	9.313	1.152
(ln)Hourly salary*	*30,609*	2.459	0.783	2.610	0.789	2.396	0.772
Hours worked (monthly)*	30,609	167.42	39.12	164.47	41.66	168.64	37.96
Job positions							
Manager*	*30,609*	0.105	0.306	0.109	0.311	0.103	0.304
Public job*	*30,609*	0.308	0.462	0.348	0.476	0.292	0.455
				Baseline characteristics			
Female	*51,937*	0.585	0.493	0.560	0.496	0.597	0.491
Age	*52,382*	19.04	1.108	19.14	1.065	18.99	1.123
Living in Pernambuco	*52,382*	0.856	0.351	0.848	0.359	0.859	0.348
White or asian	*38,006*	0.563	0.496	0.584	0.493	0.554	0.497
Number of *vestibular* tries	*46,177*	1.723	0.806	1.864	0.817	1.662	0.793
Attended pre-college preparatory course	*46,022*	0.486	0.500	0.545	0.498	0.460	0.498
Parents with college degree (or higher)	*46,201*	0.495	0.500	0.526	0.499	0.482	0.500
Parents with high school degree (or higher)	*45,686*	0.870	0.337	0.882	0.323	0.864	0.342
Attended (exclusively) private primary school	*45,944*	0.744	0.436	0.753	0.431	0.741	0.438
Attended (exclusively) private high school	*46,069*	0.749	0.433	0.749	0.433	0.749	0.433
Employed at application	*47,810*	0.095	0.294	0.087	0.282	0.099	0.299
Major choice motivation							
Prestige of the major/profession	*46,054*	0.241	0.427	0.218	0.413	0.251	0.433
Job market	*46,054*	0.029	0.169	0.021	0.143	0.033	0.179
Quality of the program	*46,054*	0.105	0.306	0.112	0.315	0.101	0.302
Personal self-fulfilment	*46,054*	0.535	0.499	0.554	0.497	0.526	0.499
Other	46,054	0.091	0.287	0.096	0.294	0.089	0.284
University choice motivation							
No tuition fees	*46,171*	0.276	0.447	0.247	0.431	0.288	0.453
Prestige of university	*46,171*	0.308	0.462	0.328	0.470	0.299	0.458
Quality of the program	*46,171*	0.329	0.470	0.328	0.470	0.330	0.470
Other	46,171	0.087	0.282	0.096	0.295	0.083	0.275

Note: Note: Summary statistics segregated by enrolled, and non-enrolled applicants. Non-enrolled candidates are those who scored bellow the admission cutoff or get admitted and do not enrolled in UFPE. Sample includes 2006-2010 application cohorts. Final entrance score is log-standardized by year and program using the last student eligible to take a place in the program of admission and the standard deviation of applicants' scores. * measured from the expected year of graduation for the competed program.

enrollees. In the next section, we explain how to disentangle these characteristics from the treatment effect of interest.

1.4 Estimation Strategy

We now focus on describing the empirical strategy. Estimating credible effects of going to a selective university is difficult due to many sources of selection bias. Given the high competition, admitted applicants to elite universities (tend to) belong to the pool of high-ability individuals, and this profile is highly associated with better family background and better school education. This implies that observed and unobserved students traits are essentially correlated with the opportunity of attending a selective university. Thus, to undermine confounding factors related to the treatment effect of interest, we use the admission cutoffs in a regression discontinuity design to compare marginally accepted to marginally non-accepted students.

Consider y_{imc} an individual i's labor market outcome and x_{imc} the individual i's entrance test score. Since our research design uses admission cutoffs as exogenous shocks to being accepted at UFPE, we define A_{imc} as a dummy equal to one if individual i is admitted to program m in cohort c, where $A_{imc} = 1[x_{imc} \geq 0]$, and consider the following model:

$$y_{imc} = \alpha \cdot A_{imc} + g(x_{imc}) + u_{imc}. \tag{1.1}$$

The function $g(\cdot)$ captures the systematic relationship between entrance test scores and the outcomes of interest and the coefficient α measures the discontinuity in this relationship around the admission cutoffs. u_{imc} is an error term. This reduced form captures the intent-to-treat effect of attending the selective university for students marginally accepted at UFPE. If every candidate admitted to UFPE wanted to enroll, α would reveal the local treatment effect of interest in a sharp discontinuity design. Since the compliance rate is not perfect because some accepted applicants can decline university invitation, to estimate the LATE we must consider the probability of enrolling in the program as a first stage. Hence, consider the model

$$P(enroll_{imc}) = \beta \cdot A_{imc} + h(x_{imc}) + \epsilon_{imc}, \tag{1.2}$$

where $enroll_{imc}$ is a binary variable equal to one if individual i in cohort c enrolled in program m. The coefficient β measures the correlation between being accepted and enrolling in the program (or the likelihood of enrollment if admitted to UFPE), which is expected to be significantly high, given the take up rates. To recover the returns to attending an elite university we therefore take the ratio of the two estimated parameters,

$\hat{\beta}$ and $\hat{\alpha}$, that is given by the following estimand:

$$\hat{\tau} = \frac{\lim_{x \downarrow \underline{x}} E(y|x \geq \underline{x}_k) - \lim_{x \uparrow \underline{x}} E(y|x < \underline{x}_k)}{\lim_{x \downarrow \underline{x}} E(enroll|x \geq \underline{x}_k) - \lim_{x \uparrow \underline{x}} E(enroll|x < \underline{x}_k)} = \frac{\hat{\alpha}}{\hat{\beta}} \tag{1.3}$$

Equation 2.5 means that, in a small boundary around the admission cutoff, we are taking the average difference in returns between candidates who barely were admitted and are surely enrolled at UFPE and those who were not admitted to UFPE by a small margin. Using observations inside a small window around the threshold is crucial to the identification strategy, which ensures that we are comparing more similar individuals. To obtain the optimal bandwidth and standard errors we use the selection procedures from Calonico et al. (2014) and Calonico et al. (2016) (CCT hereafter). Furthermore, we exploit robustness of the results by testing alternative ranges of bandwidths, as well as by including second order polynomials — as suggested by Gelman & Imbens (2017).

The estimates are obtained running local linear regressions. In addition, we also include fixed effects for field of study and application cohort in the main equation to control for differences in returns to field of knowledge[16] and labor market attachment, respectively. This full specification works mainly for the averaged version of outcomes. When investigating dynamic impacts, the outcomes take the form y_{imct}, where t indicates how many years have passed since the expected time to graduate. As candidates from earlier cohorts cannot be founded in the labor market further into the future, the number of cohort fixed effects drops as far as time elapses, making our estimates more time-specific.

Since there exists a different cutoff for each program in each year, we follow Pop-Eleches & Urquiola (2013) and Zimmerman (2016) and stack the data across all cutoffs, that is, we normalize each cutoff to zero by year and major. The immediate consequence is that an individual shall appear in the data multiple times, do to her attempts on entering in the university or even trying to switching majors. To deal with this issue, we cluster the standard errors at the student level when doing causal inference.

At the time the candidates take the exams, as well as when they apply for admission, they do not know what the exact cutoff will be since it varies each year. That is, there is no reason to believe that more ambitious students can manipulate their scores or that the university manipulates scores. Nevertheless, we further examine discontinuity in the density of scores at the threshold to check this possibility of sorting. We also implement balance tests of the pre-treatment variables by replacing our outcomes of interest for the socioeconomic and background characteristics described in the previous section. In addition, we explore a series of heterogeneous effects aiming to understand in which groups

[16]The major limitation of our data is that we do not observe the pathway chosen by non-enrollees. There are a few possible alternatives for those students. For example, candidates who barely fail to get admitted may decide going to the labor market as unskilled workers, or they could being get admitted in another college, which is very plausible. Our assumption is that marginally non-enrolled students may persist in a similar field of study to that competed at the time of application, whether in the labor market or in another program in other college. Thus, the field fixed effects intent to alleviate the sorting into different majors inside a similar area of knowledge on our estimates.

our results are more or less expressive.

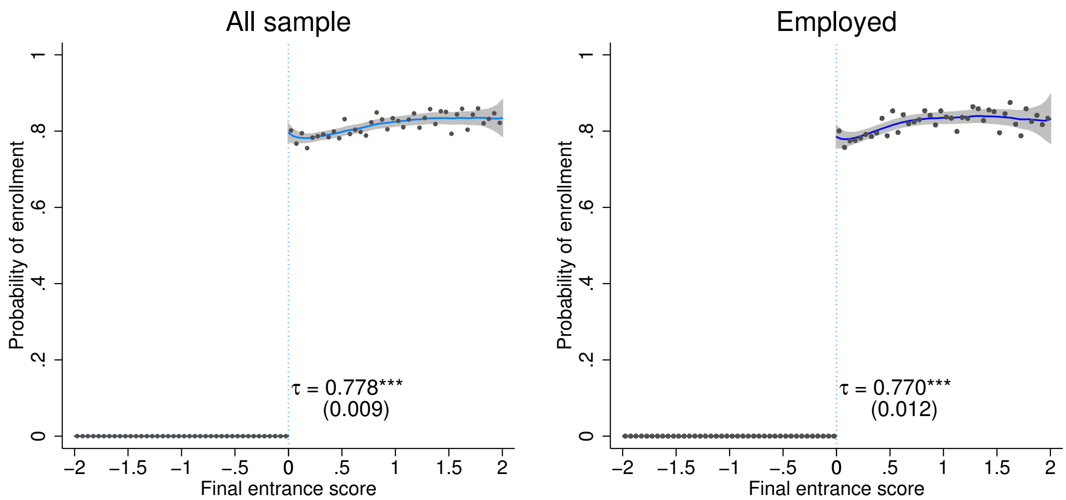
1.5 Results

Our results are divided into four parts. First, we verify how admission cutoffs explain enrollments in the elite university and provide the validation of our empirical strategy. Second, we investigate the average net elite university effects on salaries and job positions and explore these impacts segregated by groups. Third, we check how elite education affects the labor outcomes in different moments in the future. Finally, we exploit some links that explain our main results.

1.5.1 First-Stage Estimates and Validation

This section provides empirical evidence about the strength and validity of our identification strategy. We start by showing the compliance rate for marginal applicants. The first panel of Figure 1.1 reveals a jump in the probability of enrollment at the entrance score cutoff. Marginally admitted candidates are 79% more likely to enroll, and this estimate is highly significant. The high take up rate reflects the high cost of declining the flagship university's (free) offer.

Figure 1.1: Relationship between Final Entrance Score and Enrollment

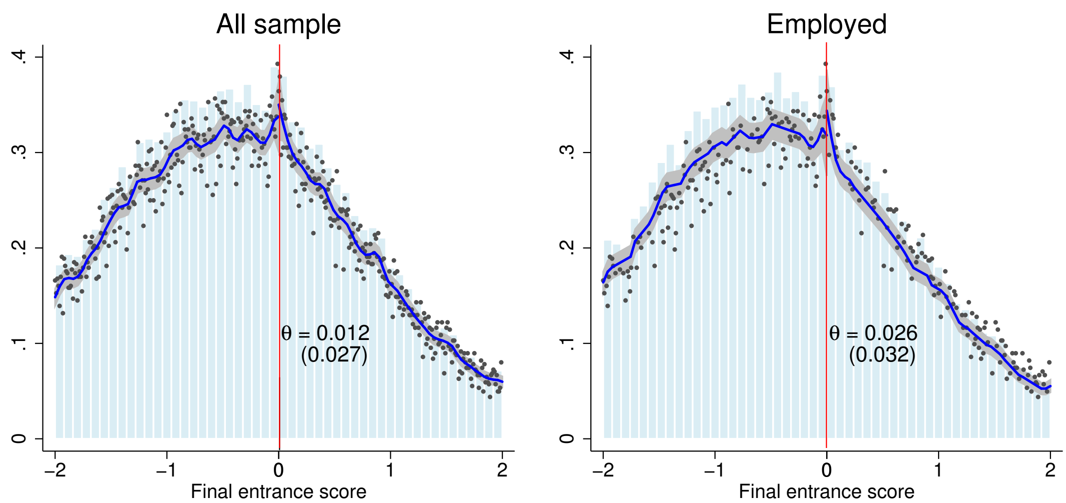


Notes: Final entrance score is log-standardized by program and year using the admission cutoff and the standard deviation of admitted candidates' scores. Sample restriction is described in Section 2.4.1. In the first panel, the treatment assignment is 1 if candidate enrolled in university and 0 otherwise. The second panel restricts enrollment to those who did not dropped out from UFPE. The first stage is estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014). τ is the regression discontinuity estimate, with standard errors clustered at the applicant level in parentheses. ***, **, * represent statistical significant at the 1%, 5% and 10% levels, respectively.

All our main findings, discussed in the next section, are obtained restricting the sample for individuals who were employed in the future. So one may ask if, on the extensive margin, the instrument is locally strong enough to induce admitted applicants to enroll at the university. The second panel of Figure 1.1 reports the estimated discontinuity conditioned for those who ever worked from the expected graduation on. The size of the coefficient is almost unchanged and the loss of around 40% in the sample do not compromise statistical power as the standard error remains very low.

The first-stage results indicate that admission cutoffs indeed raise the probability of attending the selective university, but we still may find some threats to exogeneity. One threat to identification relates to the possibility of manipulation of the entrance test score. Applicants are unaware of the cutoff score when taking the entrance exam, so we should expected no clumping in the distribution of the running variable at the right-side of the threshold. The McCrary test performed in Figure 2.2 formally tests the continuity of the entrance score density, confirming no manipulation around the admission cutoff neither to the whole sample nor conditioning to employed students.

Figure 1.2: Density of Final Entrance Score and McCrary Test



Notes: Final entrance score is log-standardized by program and year using the admission cutoff and the standard deviation of admitted candidates' scores. Sample restriction is described in Section 2.4.1. θ is the McCrary (2008) estimator for log density discontinuity, with standard error in parentheses. ***, **, * represent statistical significant at the 1%, 5% and 10% levels, respectively. Grey dots are bins of 0.02 s.d.

A second issue to worry about is the balance of pre-determined variables. If unobservable and observable characteristics are correlated with the treatment status our regression discontinuity design would not be valid. We test balance of baseline traits for all sample and restricting for applicants employed after expected graduation. Table 1.2 shows that, using the whole sample, there is a persevering non-smoothness in only one characteristic: marginal enrolled students are less likely to have well educated parents (at least one parent with college degree). The statistical significance persists even controlling for field and cohort fixed effects. This could bias downward our estimates if candidates who have well educated parents select into the labor market more easily. The evidences on the bottom of Table 1.2 suggest that, despite the expected negative discontinuity, marginally enrolled candidates have statistically the same probability to attach into the labor market in further years. Even before application, the likelihood of being working is the same between compliers. It suggests that selection do not plays a role on driving our results. Since characteristics of marginally employed applicants are well balanced, we have strong support for the validity of our strategy.

1.5.2 Impact on Salaries and Positions

1.5.2.1 Average Admission Effects

Now we discuss threshold crossing effects on the intensive margin. Since almost 60% of the applicants are employed starting from the expected year of graduation of the competed program, we have sufficient variation per cohorts to estimate local average treatment effects. We start by presenting graphical evidences of the relationship between labor outcomes as a function of candidates' entrance scores. All estimates are calculated using local regressions with first order polynomials and CCT's optimal bandwidths.

Figure 2.3 reports the effect of admission at the elite university on our measures of salary. The reduced form estimate on the top left panel indicates that the admission to UFPE drops the yearly earnings by 6 p.p., but the large standard error fails to reject the null hypothesis. Despite finding no significant discontinuity jump in yearly earnings, being admitted raises on 6 percentage points the average hourly salary. This wage premium of elite eligibility is in line with that found by (Jia & Hongbin, 2017), but unlike in China, students admitted to UFPE have the opportunity to have free higher education. The standard errors for the estimates regarding the occupation outcomes are not sufficient low to suggest an admission effect, as can be seen on the bottom of Figure 2.3. Admitted students are around 2 p.p. (significantly) more prone to ever being a manager further into the future and about 3 p.p. to take managerial posts, but it is not statistically different from zero.

1.5.2.2 Average Enrollment Effects

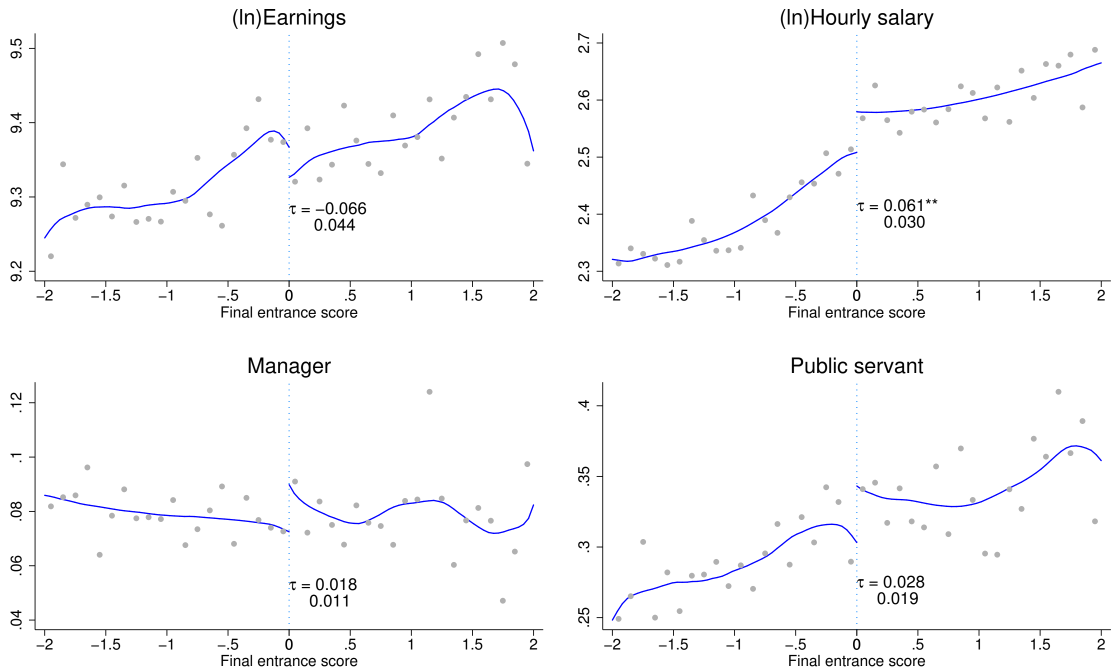
The previous findings broadly support the idea that having the opportunity to attend an elite university may benefit students in the medium-run regarding the elite wage premiums. Now we are interested on the returns for those who embraced the opportunity to selective education at the margin of admission cutoff. To do so, we exploit local average treatment effects using enrollment in UFPE as the treatment status. We present our findings stressing many specification forms and different sizes of bandwidths to certify robustness of the estimates.

Table 1.2: Balance Test

	Reduced form	Fuzzy estimate			
	All	All	All	All	Employed
Female	0.007	0.009	0.008	0.008	0.028
	(0.015)	(0.019)	(0.017)	(0.017)	(0.025)
Age	0.027	0.035	0.044	0.042	0.031
	(0.031)	(0.040)	(0.038)	(0.040)	(0.057)
Living in Pernambuco	-0.012	-0.015	-0.017	-0.019*	-0.018
	(0.010)	(0.013)	(0.013)	(0.011)	(0.019)
White or asian	0.006	0.008	0.006	0.004	0.028
	(0.018)	(0.023)	(0.022)	(0.022)	(0.031)
Number of *vestibular* tries	0.013	0.016	0.023	0.023	0.025
	(0.025)	(0.032)	(0.030)	(0.031)	(0.044)
Attended pre-college preparatory course	-0.028	-0.035	-0.028	-0.028	-0.011
	(0.017)	(0.022)	(0.021)	(0.020)	(0.029)
Parents with college degree (or higher)	-0.034**	-0.043**	-0.037*	-0.038*	-0.014
	(0.017)	(0.021)	(0.020)	(0.020)	(0.026)
Parents with high school degree (or higher)	-0.014	-0.017	-0.017	-0.018	-0.020
	(0.009)	(0.012)	(0.013)	(0.013)	(0.018)
Attended (exclusively) private primary school	-0.008	-0.010	-0.010	-0.011	-0.022
	(0.013)	(0.016)	(0.017)	(0.017)	(0.027)
Attended (exclusively) private high school	-0.018	-0.022	-0.022	-0.025	-0.021
	(0.014)	(0.018)	(0.017)	(0.018)	(0.028)
Employed at application	0.002	0.003	0.001	0.001	-0.001
	(0.009)	(0.011)	(0.011)	(0.011)	(0.018)
Major choice motivation					
Prestige of the major/profession	-0.003	-0.003	-0.005	-0.005	-0.002
	(0.014)	(0.018)	(0.017)	(0.017)	(0.026)
Job market	-0.001	-0.001	-0.001	-0.001	0.001
	(0.005)	(0.006)	(0.006)	(0.006)	(0.009)
Quality of the program	0.000	0.000	0.001	0.001	0.011
	(0.010)	(0.012)	(0.012)	(0.013)	(0.018)
Personal self-fulfilment	0.011	0.014	0.020	0.019	-0.001
	(0.015)	(0.019)	(0.017)	(0.018)	(0.025)
Other	-0.008	-0.010	-0.011	-0.011	0.006
	(0.008)	(0.011)	(0.011)	(0.011)	(0.015)
University choice motivation					
No tuition fees	-0.002	-0.003	-0.004	-0.004	-0.006
	(0.014)	(0.017)	(0.017)	(0.017)	(0.024)
Prestige of university	0.011	0.014	0.014	0.013	0.010
	(0.014)	(0.017)	(0.017)	(0.018)	(0.026)
Quality of the program	-0.003	-0.003	0.000	-0.001	0.017
	(0.015)	(0.019)	(0.019)	(0.019)	(0.027)
Other	-0.008	-0.010	-0.010	-0.010	-0.024
	(0.009)	(0.011)	(0.011)	(0.011)	(0.016)
		Selection into the labor market			
Employed before application	0.005	0.006	0.007	0.007	-0.001
	(0.009)	(0.011)	(0.011)	(0.011)	(0.019)
Employed from expected year of grad.	-0.012	-0.015	-0.016	-0.016	-
	(0.015)	(0.019)	(0.018)	(0.017)	-
Field fixed effect			✓	✓	
Cohort fixed effect				✓	

Note: This table shows the reduced forms and fuzzy estimates of the flagship university on baseline characteristics. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each cell reports the estimate and standard error of a separate regression. The last column (Employed) reports the fuzzy estimate conditioned to candidates who took at least one job from the expected year of graduation. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

Figure 1.3: Salaries Discontinuities



Notes: Final entrance score is log-standardized by program and year using the admission cutoff and the standard deviation of admitted candidates' scores. Salaries outcomes are averages measured starting from the expected year of graduation of the competed program. The occupation outcomes are binary variables indicating if the candidate took at least one occupation from the expected year of graduation for the competed program. All regressions are conditioned to employed candidates. Sample restriction is described in Section 2.4.1. τ is the regression discontinuity estimate, with standard errors clustered at the applicant level in parentheses. ***, **, * represent statistical significant at the 1%, 5% and 10% levels, respectively.

The main results are displayed in Table 2.3. Focusing on wage premiums, our fuzzy estimates show that, on average, candidates who ever enrolled in the flagship university earn less than non-enrollees, but this difference is not significant at conventional inference benchmarks. We'll come back to discuss this topic further, when investigating mechanisms. On the other hand, the hourly salary for those who attend UFPE is, on average, 6.8-10 p.p. higher relative to a mean of 250.6 percent. Translating to monetary terms, these students have a wage premium of around R$1.00 (8%) per hour worked. As expected, the inclusion of field and cohort fixed effects alleviates the enrollment effect in terms of magnitude, but not in a drastically manner. The result is also robust to the inclusion of second order polynomials and different bandwidth sizes.

To complement the analysis on wage premiums we turn attention to the flagship effect on other distribution moments of salary outcomes. The estimates are calculated implementing quantile regression discontinuity models, as proposed by Frandsen et al. (2012). Figure 1.4 reveals a non-linear impact along the entire distribution of earnings and hourly salaries, despite in many points the 90% and 95% confidence intervals are large enough to suggest no statistical difference between treated and untreated groups. Nevertheless,

Table 1.3: Effect of Flagship University on Salaries and Job Positions

	mean	(1)	(2)	(3)	(4)	(5)	(6)
Salaries							
(ln)Earnings	9.361	-0.086	-0.080	-0.087	-0.069	-0.072	-0.074
		(0.057)	(0.057)	(0.056)	(0.070)	(0.058)	(0.063)
(ln)Hourly salary	2.506	0.079**	0.084**	0.068**	0.102**	0.080**	0.089**
		(0.039)	(0.036)	(0.034)	(0.045)	(0.038)	(0.042)
Job positions							
Manager	0.108	0.037**	0.037**	0.037**	0.042**	0.034**	0.042**
		(0.018)	(0.017)	(0.018)	(0.020)	(0.017)	(0.019)
Public servant	0.326	0.036	0.049*	0.041	0.065**	0.031	0.046*
		(0.024)	(0.026)	(0.025)	(0.030)	(0.024)	(0.026)
Field fixed effect	–		✓	✓			
Cohort fixed effect	–			✓			
Bandwidth	–	CCT	CCT	CCT	CCT	CCT(125%)	CCT(75%)
Polynomial order	–	1	1	1	2	1	1

Note: This table shows the fuzzy estimates of the flagship university on labor outcomes. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. The first column (mean) shows the unconditional average of the dependent variable within the optimal bandwidth. Each cell in columns (1)-(5) reports the estimate and standard error of a separate regression. Salaries outcomes are averages measured from the expected year of graduation of the competed program. The job positions are binary variables indicating if the candidate took at least one related occupation from the expected year of graduation for the competed program. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

the second panel of Figure 1.4 shows that attending elite education is important for at least those who attain lower hourly wages (up to 39 pctl of hourly wage distribution) in the formal labor market on increasing their wage premium.

Figure 1.4: Quantile Effects on Salaries

Notes: This figure presents the quantile FRD estimates of the flagship university on salaries. Salaries outcomes are averages measured starting from the expected year of graduation of the competed program. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. All regressions are conditioned to employed candidates. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

Regarding occupational attainment, the middle part of Table 2.3 shows convincing evidence that taking selective education opportunity promotes students to occupy prestigious positions in the future. Marginally enrolled applicants are 3.7-4.2 p.p. more likely to ever have a managerial position from the expected graduation onward relative to an average of 10.7 percent. It means that these students have around 36% of likelihood to reach a leadership position, which is very expressive. The coefficients are precisely estimated and are robust to a series of specifications.

Table 2.3 also shows the enrolment impacts on the probability of being a public servant. The coefficients smoothly varies in terms of magnitude, but their standard errors are not stable enough to reject the null hypothesis in all specifications. One reasonable explanation is that, in fact, the elite university provides better competitiveness to enrollees, since some of those occupations (specially inside Judiciary sphere) are reached only by public tender offers, which is a very competitive process based on exams applied by governmental entities — we return to this matter when analyzing mechanisms. We further provide evidences to these arguments. Moreover, having a government job in Brazil represents employment stability and better retirement plans (Braga et al., 2009), a status desired by many. We show suggestive evidence that elite university entrants are more prone to (ever) choose public careers in the future.

1.5.2.3 Impacts by Demographics and Background

Although our validation tests assure the balance of our sample, we might expected that some groups may benefit more from the policy given the heterogeneity of the applicants. In this section we investigate the net elite university externalities by demographics and background to understand in which groups the impacts are more pronounced.

Table B5 provides the heterogeneity effects among males and females, and reveals interesting patterns. Despite we find no difference on earnings across these groups, we show convincing signs that the results among females drive the positive hourly wage premiums previously presented. Enrolled female applicants earn, per hour, around 14 p.p. more than their counterparts. The estimates are robust to the inclusion of cohort and field fixed effects. Despite the positive coefficients for men, treated and control groups have statistically similar wage premiums. In the first panel of Figure A.1 in the Appendix, we show that the effect for females is significantly more expressive up to the fifty pctl of hourly wage distribution. That is, the selective education matters more for those women who achieve lower hourly wages in the formal market, while for men it is practically indifferent on the entire distribution of salaries. This result suggests that elite education may attenuate some labor market distortions between males and females, by rising female returns and thus reducing the gender wage gap.

When investigating the effects on job positions we find that, among females, those who attended UFPE tend to managing firms (in private or public sector) with 5.3 p.p. of probability comparing to non-enrollees. We find no convincing evidence of the flagship

Table 1.4: Effects on Labor Market Outcomes by Gender and Race

	Male		**Female**		**White**		**Non-white**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Salaries								
(ln)Earnings	-0.086	-0.097	-0.063	-0.071	-0.136	-0.142	-0.105	-0.110
	(0.099)	(0.094)	(0.084)	(0.081)	(0.096)	(0.091)	(0.111)	(0.104)
(ln)Hourly salary	0.027	0.006	0.145**	0.140***	0.056	0.031	0.167*	0.282***
	(0.064)	(0.051)	(0.058)	(0.047)	(0.069)	(0.054)	(0.089)	(0.079)
Job positions								
Manager	0.011	0.012	0.053**	0.053**	0.032	0.032	0.062*	0.043
	(0.026)	(0.026)	(0.025)	(0.025)	(0.029)	(0.028)	(0.032)	(0.030)
Public job	0.034	0.035	0.035	0.044	0.052	0.059	0.030	0.020
	(0.038)	(0.037)	(0.030)	(0.031)	(0.041)	(0.036)	(0.048)	(0.046)
Field FE		✓		✓		✓		✓
Cohort FE		✓		✓		✓		✓

Note: This table shows the fuzzy estimates of the flagship university on labor outcomes segregated by gender. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each cell reports the estimate and standard error of a separate regression. Salaries outcomes are averages measured from the expected year of graduation of the competed program. The job positions are binary variables indicating if the candidate took at least one related occupation from the expected year of graduation for the competed program. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

effect on having a public job within genders. The last four columns show weak evidence that non-white flagship students tend to work for the Government and their wage premium is higher. According to Figure A.1, the returns are more expressive for those non-white students who earn up to the 45 pctl of hourly salary distribution. That is, elite education changes the wage premium for minorities, specially for those who compete for jobs which pay less.

The most prominent findings are related to background characteristics. For parents education, we split the sample into 'well educated parents' (mother or father with college degree) and 'less educated parents' (neither mother or father have college degree). According with Table 1.5, students from poorer backgrounds are those who benefits most from taking the opportunity to attend UFPE. Their gains per hour are significantly higher compared to their counterparts, and the magnitudes are much stronger than the baseline estimates obtained in Table 2.3. Enrollees who have less educated parents at the time of application have a hourly salary around 18 p.p. higher, which means an increase of R$1.78 per hour. The wage premium of elite education enrollment is even greater for those who come from public high schools (26-29 p.p. of difference). The fourth panel of Figure A.1 shows that public school students have elite wage premiums in almost the entire distribution of wages. The quantile results obtained among students who have less educated parents in qualitative terms are similar to those founded for females and non-white.

Moreover, students from modest backgrounds are more susceptible to pursue careers in public entities. Enrolling in UFPE enhances the probability of being a public employee by around 8 p.p. (16 p.p.) for those who have less educated parents (attended public secondary schools). Public tenders in Brazil involve a very competitive process, so we suggest that the enhance on on the chance of being a public servant is promoted by an

Table 1.5: Effects on Labor Market Outcomes by Parent's Level of Education and High School Type

	Well educated parents		**Less educated parents**		**Private sec. school**		**Public sec. school**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Salaries								
(ln)Earnings	-0.178*	-0.176*	-0.052	-0.054	-0.129*	-0.131*	-0.033	-0.090
	(0.096)	(0.093)	(0.082)	(0.094)	(0.069)	(0.068)	(0.135)	(0.126)
(ln)Hourly salary	0.006	-0.016	0.182***	0.190***	0.044	0.023	0.292***	0.257***
	(0.070)	(0.057)	(0.057)	(0.057)	(0.049)	(0.041)	(0.097)	(0.092)
Job positions								
Manager	0.029	0.030	0.038	0.039	0.040*	0.043**	0.005	0.006
	(0.025)	(0.025)	(0.025)	(0.026)	(0.021)	(0.022)	(0.033)	(0.034)
Public job	0.000	0.008	0.081**	0.078**	0.004	0.004	0.167***	0.147***
	(0.036)	(0.032)	(0.037)	(0.039)	(0.028)	(0.027)	(0.059)	(0.057)
Field FE		✓		✓		✓		✓
Cohort FE		✓		✓		✓		✓

Note: This tables shows the fuzzy estimates of the flagship university on labor outcomes segregated by race, parents education and type of secondary school. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each cell reports the estimate and standard error of a separate regression. Salaries outcomes are averages measured starting from the expected year of graduation of the competed program. The job positions are binary variables indicating if the candidate took at least one related occupation from the expected year of graduation for the competed program. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

improvement on enrollees' human capital.

These findings broadly indicates the following. Much of the future elite wage premiums are destined to applicants who have historically greater disadvantages in the labor market, suggesting an impact on intergenerational mobility. These premiums are more remarkable among those who earn least per hour. Finally, enrolling in a flagship university gives the opportunity to students from modest backgrounds to ascend to jobs with safer careers.

1.5.2.4 Impacts by Field of Study

As already mentioned, individuals who were at the margin of admission of the competed program and did not reach the minimum score to be accepted (or preferred to not enroll) have some alternatives to pursue. Almost 78% of eligible candidates declared choosing the major for reasons related to self-fulfilment and prestige of the career, and our validation results showed that marginal applicants are similar regarding their major choice motivation. Thus, it is reasonable to think that untreated students may keep pursuing careers and professions on fields of study that are related to the competed major at the time of application to UFPE. In this section, we estimate the heterogeneous flagship university impacts across the field groups to understand in which areas of knowledge taking the opportunity of elite higher education is more worthwhile.

The results reported in Table 1.6 confirms the heterogeneity on returns and on occupations among fields. Programs in Health, Teaching, and Law are the best deal for elite students in terms of hourly salaries compared to their controls, with gains ranging from 18-34 percentage points. According to Figure A.2 in the Appendix, students from the left tail of hourly salaries have benefited most from enrolling at these elite programs. Moreover, we also find a boost on the probability of Laws students taking posts in public entities. Top government careers in Laws are known for paying the highest salaries in Brazil among the public sector and for being a highly competitive market. Once again, our results support the idea that the elite university provides better educational inputs to its students — at least on this segment — to achieve those prestigious occupations.

1.5.2.5 Dynamics on Salaries and Job Positions

We also examine the flagship effect on labor outcomes at specific years since the time of expected graduation. Our purpose on this exercise is to understand where in the time enrollees take those occupations and achieve higher wage premiums. We note to the trade-off between statistical power and estimation bias. The further in the future, the higher the number of applicants that entered the labor market. Conversely, only older cohorts reached those years in the future.

The results are reported in Figure 1.5. All estimates are calculated including cohort and field fixed effects with first order polynomials. Regarding the hourly salaries, those who attended the elite university have an immediate (and significant) wage premium of 11 p.p. at the year they should graduate. Although the decline on the impact on the next three years (but positive on most of the time), the wage premium raises to the same level four years later. This finding does not seems to be transient since estimates become more accurate and less biased as we look further in the future — since more applicants are founded in the labor market.

Furthermore, Figure A.3 in the Appendix attests that the dynamics on hourly salary premiums obtained previously is conducted mainly by specific groups. The comparison among genders suggests that males pull downwards the baseline estimate in year 3, since the graph of females is almost equal to the first panel of Figure 1.5. Among non-white students and those who have less educated parents the wage premiums behave similarly for elite students — start positive, decrease in the next years, and raise in year 4.

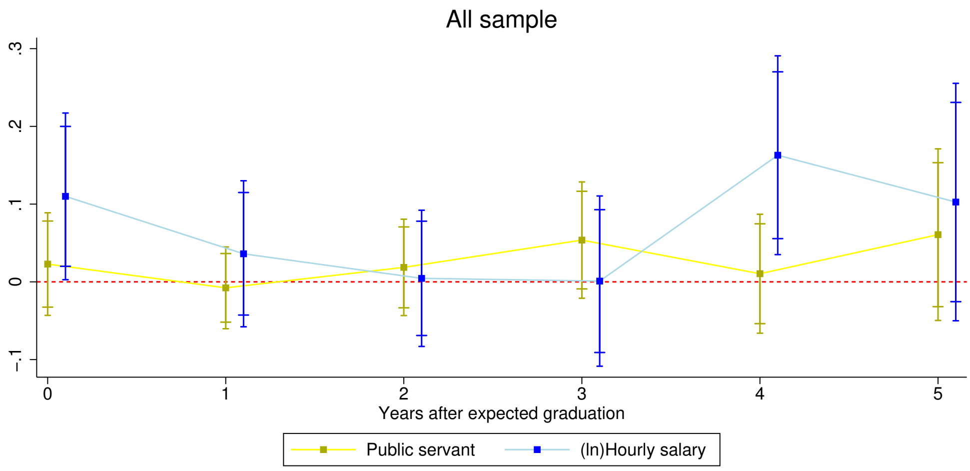
The yellow dots on Figure 1.5 depict the enrollment effects on the probability of being a public servant. Despite the positive estimates found from two years after expected graduation, we find no statistical significance on those years. The results are qualitatively the same among almost all demographics and background groups (see Figures A.3 and A.4). We highlight the growing probability of non-white enrolled students assuming managerial positions as we move to the future.

Table 1.6: Effect of Flagship University by Field of Study

	STEM	Social Sciences	Design	Arts/Hum.	Health	Teaching	Pol./Philos.	Comput.	Law	Physical Education	Geography	Tourism	Medicine
Salaries													
(ln)Earnings	0.081	-0.081	-0.121	-0.038	-0.038	0.042	-0.386	0.028	-0.112	-0.024	-0.340	-0.410	0.049
	(0.211)	(0.123)	(0.241)	(0.193)	(0.152)	(0.155)	(0.293)	(0.300)	(0.240)	(0.283)	(0.230)	(0.371)	(0.391)
(ln)Hourly salary	0.043	0.071	-0.122	-0.012	0.170*	0.239*	0.147	-0.064	0.344*	-0.011	-0.121	0.150	-0.225
	(0.113)	(0.080)	(0.169)	(0.103)	(0.091)	(0.125)	(0.206)	(0.167)	(0.188)	(0.149)	(0.122)	(0.186)	(0.220)
Job positions													
Manager	0.089**	0.028	-0.021	0.053	-0.014	-0.024	-0.091	0.011	0.112	-0.069	0.045	0.104	0.120
	(0.044)	(0.048)	(0.081)	(0.052)	(0.041)	(0.033)	(0.111)	(0.076)	(0.088)	(0.071)	(0.064)	(0.093)	(0.120)
Public servant	-0.082	0.082	-0.018	0.025	-0.046	0.098	0.075	-0.036	0.135*	-0.014	-0.003	0.167*	0.457**
	(0.080)	(0.058)	(0.089)	(0.062)	(0.065)	(0.081)	(0.144)	(0.107)	(0.085)	(0.108)	(0.101)	(0.099)	(0.199)
Cohort fixed effect	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: This tables shows the fuzzy estimates of the flagship university on labor outcomes segregated by field. See Table A.2 in the Appendix for the full list of programs falling in each field of study. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each cell reports the estimate and standard error of a separate regression. Salaries outcomes are averages measured starting from the expected year of graduation of the competed program. The job positions are binary variables indicating if the candidate took at least one related occupation form the expected year of graduation for the competed program. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level. '-' means no estimate due to lack of variation of the dependent variable.

Figure 1.5: Dynamics of Salaries and Job Positions



Notes: This figure presents the FRD estimates of the flagship university on salaries and job positions by each year of the expected graduation of the competed program. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. All regressions are conditioned to employed candidates. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

1.5.3 Possible Channels

1.5.3.1 What could explain elite education wage premiums?

In addition to examine the net effect of going to a flagship university, we try to enlighten possible channels that could be driving the wage premiums. Differently from Zimmerman (2016) and Jia & Hongbin (2017), we cannot distinguish channels related to college reputation, class ranking or even social networks (peer ties) since we do not clearly observe the education pathway of non-admitted applicants. Alternatively, we can investigate possible explanations emerged from the labor market side — such as experience and the quality of the job — and related to the quantity of education.

Top occupations are usually intrinsic related to better salaries — and to having higher education levels. Thus, they could be a link for the elite wage premiums observed previously. To verify such possibility we perform a series of checks, gradually including the occupation dummies as control variables (in the spirit of Jia & Hongbin (2017)) and conditioning the regressions on those characteristics. We are aware about the endogeneity between salaries and occupations, but we consider this approach only as an attempt to understand how wage premiums vary. Table A.3 in the Appendix shows the results for this exercise. Despite the loss on the precision (and even less in the magnitude) of the estimates when controlling for job positions, the results suggest that the wage premiums cannot be totally explained by having posts with such reputation. The variation also comes within job positions. Moreover, when we split the sample by occupations, the wage

Table 1.7: Evaluating Mechanisms on Elite Education

	From expected graduation					
	(1)	(2)	(3)	(4)	(5)	(6)
Number of jobs	-0.059	-0.047	-0.052	-0.002	-0.061	-0.031
	(0.056)	(0.057)	(0.053)	(0.069)	(0.054)	(0.059)
Experience	-0.232***	-0.235***	-0.233***	-0.191**	-0.227***	-0.218***
	(0.077)	(0.078)	(0.071)	(0.096)	(0.076)	(0.081)
Hours worked	-2.262	-1.548	-1.425	-2.980	-2.219	-2.790
	(2.216)	(2.068)	(2.089)	(2.619)	(2.163)	(2.339)
Quantity of education						
Graduation (at least)	0.081***	0.082***	0.070***	0.082***	0.080***	0.082***
	(0.026)	(0.025)	(0.023)	(0.031)	(0.025)	(0.027)
Quality of the job						
High-skilled position	0.070**	0.078***	0.070***	0.077***	0.071***	0.075**
	(0.028)	(0.028)	(0.027)	(0.030)	(0.028)	(0.030)
Field fixed effect		✓	✓			
Cohort fixed effect			✓			
Bandwidth	CCT	CCT	CCT	CCT	CCT(125%)	CCT(75%)
Polynomial order	1	1	1	2	1	1

Note: This table shows the fuzzy estimates of the flagship university on experience (measured in years of work), number of jobs, hours worked, quantity of education, and quality of the job. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each cell shows the estimate and standard error of a separate regression. High-skilled positions are binary variables indicating if the candidate took at least one job requiring at least incomplete higher education or technical college degree. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

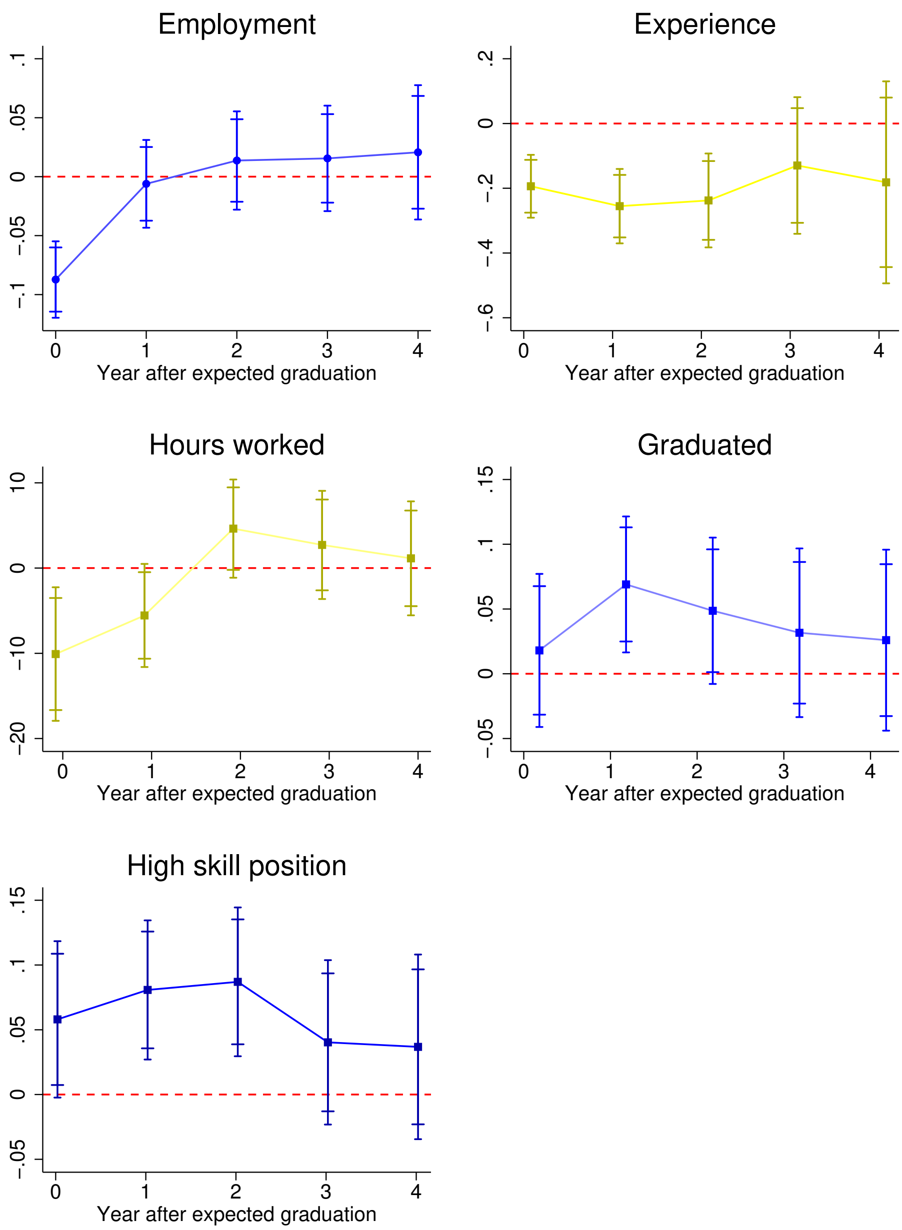
premium appears only for those who don't reach those selective posts.

We now explore some insights that could clarify the discontinuities found on salaries. The first row of Table 1.7 shows that there is no difference on tenure between compliers. On the other hand, students attending the elite university have less accumulated experience (years of work) in the labor market, and this can be justified by their decisions since the beginning of academic year (see Figure A.5 in the Appendix). On average, these students worked around .2 year less (two and half months). Moreover, the lower experience in further years cannot be attributed to differences prior to college admission, since treated and control groups have the same probability of being employed in the first year after application — and have the same work experience. Their behaviour during college experience may be a consequence of the trade-off between working and studying, since these elite students do not have to pay tuition fees. As a result, Figure 2.8 shows that enrollees remain with less work experience up to two years after expected graduation, and then have no statistical difference (despite the negative relation) with non-enrollees from the third year on.[17] Therefore, we should expected the total yearly earnings to be lower. In other words, elite students earned less because they worked less time in a year.

Regarding hourly salaries, we rule out that hours worked is a driving force of the main findings. Elite students work less hours on average, but this relation is not statistically significant at conventional levels. Even with the opposite relation between hourly salary

[17]We note that enrolled candidates are less likely to be employed in the year of expected graduation, but in our understanding this selection plays only a minor issue on our findings. In the worst case, it may push downward the coefficients and we are estimating a lower bound of the effect.

Figure 1.6: Dynamics of Employment, Experience, Education, and High-skill Position



Notes: This figure illustrates the fuzzy regression discontinuity (FRD) estimates of the flagship university effect. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Employment (high skill position) is a binary variable indicating if the candidate took at least one job (job requiring graduation degree level) in the assigned year. Experience is measured in years. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

and hours worked in the year of expected graduation and one year latter (Figure 1.5), the graph suggests that these two measures tend to go in the same direction as we go further in the future.

One potential mechanism leading elite wage premiums could be the difference on graduation rates. Indeed, the robust estimates in columns (1)-(6) of Table 1.7 show that selective students who ever worked in the future are about 8 p.p. more likely to have a higher education degree — according to Figure 2.8, this happens in years 1 and 2 after expected graduation. But this finding is followed by a higher probability (7 p.p. of difference) of these students taking jobs requiring higher levels of skill.[18] This is expected as the majority of high skilled occupations demand workers with college degree. At year 0, elite students have the same chance of having a college diploma but are more likely to have a high skilled position, thus the wage premium cannot be justified by differences in graduation rate. In year 4, neither channels seem to explain the rise on hourly salaries in Figure 1.5. That is, enrolling in the flagship university and having the advantage of being graduated do not totally justify the wage premiums.

To go deeper into this question, in Table B6 we split the sample into graduated vs non-graduated applicants and high-skilled vs non-high-skilled occupations, and explore the role of the quality of job and quantity of education. Among non-graduated applicants, attending the elite university increases the likelihood of having a job with higher levels of ability in 6.2 percentage points. Besides that, their hourly wages are 10 p.p. higher. This result reinforces the patterns found in Table A.3. In the sample of graduates, despite the economically meaningful impact, we find no statistical effects on hourly wages. On the other hand, students who ever enrolled in UFPE attain more complex jobs among this group. From columns (5)-(8), we can infer that graduation rates do not affect hourly returns regardless the quality of occupation. These findings suggest that affording more specialized jobs, much more than just having a college degree, is important for determining elite education wage premiums. If threshold crossing leads applicants to enroll in college with higher likelihood, the fact of just having the advantage of attending a higher education institution may contribute to attain these jobs. This is a reasonable argument, but we cannot sustain it since we have no data to check enrollment decisions of non-enrolled candidates.

1.5.3.2 Graduation Rates Always Justify Better Job Positions?

With exception of some leadership occupations and some careers inside the Government sector, the job positions explored in this paper require workers to have college diploma, which implies that graduation rates may be leading elite students to these posts. We now provide suggestive evidences that the access to those selective occupations is mostly guided by other links but differences on graduation rates between marginal applicants.

Table 1.9 reports the second-stage estimates for graduation rates and the quality of job. Among STEM and Social Sciences, neither measures are locally discontinuous, implying that students who ever attended the elite university attain managerial positions and

[18] Our definition of high skilled occupations includes posts which demand workers with at least incomplete higher education or technical college degree.

Table 1.8: Salary Premiums by Education Quantity and Quality of the Job

	Non-graduated		Graduated		Non-high-skill		High-skill	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(ln)Earnings	-0.169	-0.168	-0.088	-0.081	-0.294***	-0.299***	0.057	0.056
	(0.107)	(0.107)	(0.072)	(0.073)	(0.108)	(0.109)	(0.077)	(0.077)
(ln)Hourly salary	0.120**	0.106*	0.013	0.015	0.068	0.049	0.069	0.075*
	(0.060)	(0.057)	(0.044)	(0.044)	(0.058)	(0.055)	(0.044)	(0.043)
Quality of the job								
High-skilled position	0.064**	0.062**	0.053*	0.054*	-	-	-	-
	(0.030)	(0.029)	(0.031)	(0.031)	-	-	-	-
Quantity of education								
Graduation (at least)	-	-	-	-	0.059*	0.054*	-0.010	-0.007
	-	-	-	-	(0.030)	(0.028)	(0.025)	(0.024)
Field fixed effect	✓	✓	✓	✓	✓	✓	✓	✓
Cohort fixed effect		✓		✓		✓		✓

Note: This table shows the fuzzy estimates of the flagship university effect segregated by quantity of education and quality of the job. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each cell shows the estimate and standard error of a separate regression. Experience is measured in years worked. High-skilled positions are binary variables indicating if the candidate took at least one job requiring at least graduation or master degree levels. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

teaching posts for other motives (which could be signaling, human capital accumulation, or peer ties). As Law and Medicine applicants have the same chance of having a graduation degree, their access to careers in Government entities is probably due to higher levels of human capital acquired during college experience. Social networks and signaling, in this case, do not plays a role since the only way to go through is facing very selective entrance exams.

Table 1.9: Graduation Rates and High-skilled Positions by Field of Study

	STEM	Social Sciences	Design	Arts/Hum.	Health	Teaching	Pol./Philos.	Comput.	Law	Physical Education	Geography	Tourism	Medicine
Graduation (at least)	-0.114	0.065	0.067	0.074	0.143***	0.165**	0.216	0.029	-0.188*	0.103	0.116	-0.119	-0.018
	(0.082)	(0.052)	(0.114)	(0.082)	(0.054)	(0.084)	(0.133)	(0.096)	(0.102)	(0.149)	(0.096)	(0.131)	(0.036)
High-skilled position	-0.003	0.053	0.045	0.152*	0.102*	0.209**	0.173	0.037	0.023	0.132	0.076	0.029	0.363**
	(0.069)	(0.058)	(0.111)	(0.090)	(0.062)	(0.086)	(0.155)	(0.102)	(0.086)	(0.146)	(0.103)	(0.129)	(0.145)
Cohort fixed effect	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: This table shows the fuzzy estimates of the flagship university effect on graduation and quality of the job segregated by field of study. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each cell shows the estimate and standard error of a separate regression. Experience is measured in years worked. High-skilled positions are binary variables indicating if the candidate took at least one job requiring at least graduation or master degree levels. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

1.6 Conclusion

UFPE's entrance exams produce an ideal quasi-experiment close to admission cutoffs to investigate the role of elite postsecondary education on future labor market outcomes. Applying a standard RD design we disentangle the influence of ability and other personal (observed and unobserved) traits from the effect of interest, allowing us to interpret causal relations.

Our results are very promising. We unveil economically and statistically significant returns to elite education related to attainment of top occupations and higher hourly wages. Enrollments yield higher wage premiums specially among women and applicants from modest backgrounds, and raise the chances of these individuals to assume leadership positions or jobs with long-term stability in the future. This finding contributes to policy debates related to affirmative actions by giving inputs to substantiate proposal interventions aiming on promoting disadvantaged groups to accessing selective higher education.

In fact, there are some fields of study in which elite education propitiates better gains. Our heterogeneous results are consistent with Hastings et al. (2013) and Zimmerman (2016) on revealing higher returns among degrees in Health and Law. Moreover, access to free elite education helps students to upgrade their degree of schooling, which translates into a better signal for the labor market demand side.

We add to literature on elite education by assessing a novel channel intermediating the effect of attending a flagship university. We do not discard other underlying mechanisms that lead elite wage returns, but we provide suggestive evidence that, much more than just having a higher degree diploma, the wage premiums are guided by better matches on jobs demanding more specialized tasks.

CHAPTER 2

Can Good Peers Signal Less Success? The Disruptive Effect of Class Ranking on Career Investment

2.1 Introduction

It is well documented that being among better peers may improve the learning experience and productivity.[1] Peer's ability, however, can also work as a signal for what the candidate must possess to be successful in a certain career. The better the individuals choosing the same career, the lower the perceived return on this investment. On the other hand, having a natural advantage over other candidates can boost motivation and increase interest in more prestigious jobs.[2] In psychology, this event is named the 'big-fish-little-pond' effect (Marsh & Parker, 1984), in a reference to the fact that students in low-ability schools present higher self-concepts than those in high-ability schools.[3] In terms of career decisions, this effect could play not only against the benefit of having better peers but also against the return on joining elite institutions.[4]

In this paper we attempt to estimate the effect of perceived rank on career change,

[1]See Sacerdote (2001); Zimmerman (2003); Carrell et al. (2009); Imberman et al. (2012); Booij et al. (2016) for evidence of peer effects in the classroom and Falk & Ichino (2006); Mas & Moretti (2009); Jackson & Bruegmann (2009); Azoulay et al. (2010); Waldinger (2011); Herbst & Mas (2015); Cornelissen et al. (2017) for evidence in the workplace.

[2]For instance, studies on school starting age show the short- and long-term benefits of early maturity (Bedard & Dhuey, 2006; McEwan & Shapiro, 2008; Black et al., 2011; Fredriksson & Ãckert, 2014).

[3]See also Marsh (1987); Marsh & Hau (2003).

[4]This is in line with Arcidiacono & Lovenheim (2016) claim that under certain conditions, affirmative action can harm minority students due to their poor fit with the school.

earnings and occupation. To establish causality, we properly control for individual skills, institutional differences and the distribution of peers' ability by exploiting the rule of class assignment in a major flagship university in Brazil. In most of its undergraduate programs, students are assigned to one of two classes, which we name 'first' and 'second.' The candidates must choose both the program that they want to study and their preferred class before they take the entrance exam. After the exam is taken, students' rank and class assignment are publicly disclosed. While most of the best candidates go to the first class, some students are forced to attend the second class.

This arrangement allows us to compare similar candidates who are either at the bottom of the better class or at the top of the worse class. The comparison reveals that those at the bottom are more likely to try a different program and delay their graduation. In the future, these students will also have a lower chance of getting a prime occupation, such as manager or public servant, and will earn less at the start of their career. For women, the motivation given by a higher rank is found to help them to break the glass ceiling in job promotions (Bertrand & Hallock, 2001; Babcock et al., 2017). The same woman is 13 percentage points (p.p.) more likely to be a manager in the future if she attends the second class.

The empirical identification of the ranking effect is challenging for many reasons. First, rank and skills are by definition perfectly correlated. Second, students' skills determine their choice of school and the quality of teaching. Accordingly, we control for cognitive skills and institutional differences by applying a regression discontinuity (RD) design. Furthermore, students' rank is also correlated with peer quality, so their effects could simply cancel each other out. In addition to the standard RD design, we use the variation across program cohorts to estimate the nonlinear relationship between discontinuities and differences in the distribution of peers' ability. As the difference in peer distribution between classes moves to zero, the only remaining difference at the cutoff of test scores is in students' rank.

This relationship indicates that the effects of ranking on the willingness to switch majors, delay graduation, and future occupation can be mitigated by an increase in peer quality. We find, however, a distinction between genders. For women, the ranking effect on decisions in college is so weak that a small increase in peer quality brings the net effect of attending the first class close to zero. Yet the net effect on their likelihood of being a manager is still strongly driven by their rank. For men, the ranking effect on early changes is much stronger and cancelled out only by an abnormal difference in peer quality. A 10 pctl drop in their rank increases by 4.6 p.p. the chance of switching programs and decreases by 9.3 p.p. the chance of graduating at the proper time.

In addition to the main findings, the analysis with subsamples reveals that the discouragement about completing a program is not strictly related to absolute academic performance.[5] For instance, men's decision to switch majors is more sensitive to their

[5]Using an alumni survey, we also find no evidence that students' rank affects their personality traits in the long run. Results are available upon request.

rank in programs that have an easier curriculum and higher participation by women. Yet the effect on course failure is higher in other programs. Moreover, access to better information makes the ranking effect weaker. Most of the academic and labor outcomes are less affected if both parents have a college degree. Similarly, the ranking effect is less pronounced among candidates who choose their major on the basis of market opportunities and prestige, rather than other motives such as self-fulfillment and the program's reputation. These candidates are assumed to have a stronger conviction about their future earnings, making them less likely to update their choices in light of the new information (Hastings et al., 2016).

Although candidates are unaware of the cutoff between classes when they apply to the university, they could decline an offer as soon as the test scores and class order are revealed (Bond et al., 2017). To verify this type of selection bias, we run the McCrary (2008) test and find no evidence of missing students on either side of the threshold. We also test for differences between the instructors in these classes and find no significance. The last concern is related to the starting date of the two classes, which are five months apart. We estimate the effect of this delay on the academic outcomes of first-class students by using an unexpected strike in the university. If anything, the delay reduced the student's commitment to the program.[6]

Our findings are consistent with recent studies on the effect of class ranking in primary and secondary schools. These studies find that a lower perceived rank diminishes students' grades (Weinhardt & Murphy, 2016; Tincani, 2017), self-esteem (Cicala et al., 2016; Fabregas, 2017), and probability of attending college (Elsner & Isphording, 2017). Our work adds a new piece of evidence by showing that the perceived rank also induces career changes after students have enrolled in college and has consequences for their future occupation. According to Zafar (2011), Arcidiacono et al. (2012), and Stinebrickner & Stinebrickner (2012, 2014), students who are poorly matched in their programs adjust their optimistic beliefs and are more likely to drop out. In our model, however, we show that the perceived rank creates a false inference that students are poorly matched in their careers.[7]

This result is related to the broader evidence that students update the perceived return of schooling and career investment when they receive new signals (Jensen, 2010; Wiswall & Zafar, 2014; Hoxby & Turner, 2015). It may also explain why peer effects are found to be heterogeneous and sometimes harmful to disadvantaged candidates (Lavy et al., 2012,?; Carrell et al., 2013; Feld & Zölitz, 2017). Likewise, the benefit of joining a more selective school could be null if students see themselves at the bottom of the ability distribution (e.g., Dale & Krueger, 2002b; Ockert, 2010; Abdulkadiroğlu et al., 2014; Kirkeboen et al., 2016b; Heinesen, 2018; Hoekstra et al., 2018).[8] In addition to controlling for institutional

[6]See Section B.4 in the Online Appendix.

[7]Other related studies show that a lower perception of social rank reduces well-being (e.g., Daly et al., 2015; Perez-Truglia, 2016).

[8]A non-exhaustive list of studies on the short- and long-term effects of selective schools includes Hoekstra (2009); Zimmerman (2014); Dobbie & Fryer Jr (2014); Goodman et al. (2017); Canaan &

differences, our empirical strategy provides unique evidence of the relationship between peer quality and perceived rank.

The remainder of the paper is organized as follows. Section 2.2 presents a theory of the way in which the ranking effect coexists with the effect of peer quality. Section 2.3 describes the university's admissions policy and the rule of class assignment. Section 2.4 details the sample and data sources and presents the descriptive statistics. In section 2.5, we describe our empirical strategy. Section 2.6 presents all the empirical findings. Section 2.7 concludes the paper. In addition, the Online Appendix provides: proofs for all propositions; details on the estimation procedure; and several robustness tests.

2.2 Theoretical Framework

To understand the potential effects of class assignment on short-term decisions and long-term earnings, we present a simple model of career investment in the context of peer effects and unobserved skill distribution. Unlike the models proposed by Zafar (2011), Arcidiacono et al. (2012), Stinebrickner & Stinebrickner (2012, 2014), and Wiswall & Zafar (2014), this model explicitly considers the role of classmates' skills on the decision to switch programs and drop out of college. In our model, students' beliefs are assumed to be updated according to a Bayes' rule.

Consider a continuum of individuals who have to make a decision about their careers by the end of high school. Individual i has to choose either among K study programs (majors) or a career that does not require a college degree, denoted by $k = 0$. This decision is reversible and individuals may change their career paths later on, but at a cost. Skill level s_i is known by individual i, but it is distributed in the population according to an unknown function $F(s)$.

For $k = 0$, individuals can immediately find a job, but for $k > 0$ individuals must spend one period in college before going to the market. After college, individual i's utility in career k is given by:

$$u_i^k = v_i^k + w^k \, p\Big(h_i^k, h_{-i(k)}^k\Big) \, ,$$

where v_i^k is the individual taste for career k, w^k is the lifetime salary in this career, $p\Big(h_i^k, h_{-i(k)}^k\Big)$ is the probability of finding a job, h_i^k is the k-specific human capital accumulated by i, and $h_{-i(k)}^k$ denotes the quantiles of human capital among those who choose career k, excluding i. All individuals have their own taste for each high-skilled career, $\big\{v_i^1, \ldots, v_i^K\big\}$, which is independently drawn, but they do not know it until they go to college.

The probability of finding a job in career k is increasing in h_i^k, $\partial_1 p > 0$, and nonincreasing in $h_{-i(k)}^k$, $\partial_2 p \leq 0$. Based on the curvature of the probability function, we define two types of career: those in which most workers succeed, and those in which only a few

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workers succeed.[9]

Definition 1. *A career is* highly competitive *if the probability is convex in the individual human capital, $\partial_{11} p \geq 0$, and an increase in peers' ability reduces the individual return, $\partial_{12} p \leq 0$. A career is* less competitive *if the probability is concave in the individual human capital, $\partial_{11} p \leq 0$, and an increase in peers' ability increases the individual return, $\partial_{12} p \geq 0$.*

The human capital is a function of inherited skills, s_i, the effort applied during the study program, e_i^k, and the skill distribution of classmates, $s_{-i(c)}$:

$$h_i^k = h^k\left(e_i^k, s_i, s_{-i(c)}\right).$$

For every k, we assume that $\partial_1 h^k$, $\partial_2 h^k > 0$, and $\partial_{11} h^k < 0$. We also assume that peer quality increases human capital, $\partial_3 h^k > 0$, the return of effort (learning), $\partial_{13} h^k \geq 0$, and hence the probability of finding a job, $\partial_1 p\, \partial_3 h^k > 0$.[10] Given this human capital production function, peer effect is defined as:

Definition 2. Peer effect *is the direct effect that peer skills, $s_{-i(c)}$, have on the accumulation of human capital and on its derivatives.*

With $K+1$ options in hand, an individual's initial decision is based on the expected value of each career path. However, individuals do not know the true distribution of skills in the population and, as a result, the distribution among those who choose each career, F^k. Thus their initial decision is based on the belief that individual skills in their chosen career follow a prior distribution, $s_{-i(k)} \sim \tilde{F}_i^k$. Likewise, individuals also believe that the skill distribution of classmates is not different from the population of workers in k, so $s_{-i(c)} \sim \tilde{F}_i^k$. This prior distribution is randomly drawn among individuals, but it also depends on their initial information set I_i — i.e., how accurate their prior is. If $I_i \to \infty$, then $\tilde{F}_i^k \to F^k$. If $I_i = 0$, then individuals are clueless about the distribution of $s_{-i(k)}$ and heavily influenced by any new information.

During college, effort has a marginal disutility equal to γ. Given $\tilde{F}_i^k$ for every $k = 1, \ldots, K$, individual's problem is to choose k and $\{e^k\}_{k=1}^K$ so that their value function is

$$\begin{aligned} V_i &= \max_{k,\{e^k\}} \left\{V_i^0, V_i^1, \ldots, V_i^K\right\} \\ &= \max_{k,\{e^k\}} \left\{w^0, \theta \tilde{E}_i\big(w_i^1\big) - \gamma e_i^1, \ldots, \theta \tilde{E}_i\left(w_i^K\right) - \gamma e_i^K\right\}. \end{aligned} \tag{2.1}$$

[9] One may think of $p(.)$ not as the probability of employment, but as the cdf of salaries.

[10] The composition of peers can affect future earnings not only through its direct effect on individual ability but also through the social ties that are created among classmates (e.g., Black et al., 2013; Shue, 2013; Kramarz & Skans, 2014).

where $\theta \in (0,1)$ is a discount factor. $\tilde{E}_i\left(w_i^k\right)$ is individual i's subjective expectation of their future salary, which is given by:

$$\tilde{E}_i\left(w_i^k\right) = w^k p\left[h^k\left(e_i^k, s_i, \tilde{F}_i^{k^{-1}}\right), \tilde{F}_i^{k^{-1}}\right] = w^k \tilde{p}_i^k\left[\tilde{h}_i^k\left(e_i^k\right)\right],$$

where $\tilde{p}_i^k(.)$ and $\tilde{h}_i^k(.)$ are subjective functions derived from s_i and $\tilde{F}_i^k$. That is, the subjective expectation of the future salary depends on how the individuals see themselves in comparison to their envisaged peers.

After $k > 0$ is chosen, students get to know their classmates' skills, $s_{-i(c)}$, and this information is incorporated in the posterior distribution $\hat{F}_i^k$. If $s_{-i(c)} \geq \tilde{F}_i^{k^{-1}}$, then $\hat{F}_i^{k^{-1}} \geq \tilde{F}_i^{k^{-1}}$ as long as $I_i < \infty$. If $I_i = 0$, then $\hat{F}_i^{k^{-1}} = s_{-i(c)}$. Since $s_{-i(c)}$ is known, there is no longer any uncertainty regarding $h_i^k(.)$. In college, students also learn about their taste for the chosen career, v_i^k. With these adjustments, students face new decisions: how much their effort should change, and whether they should drop out of college (D), switch programs (S) or graduate (G). Their new value function is:

$$\hat{V}_i^k = \max_{\{D,S,G\},e^k} \left\{V_i^0,\, \theta V_i^{k'},\, \theta v_i^k + \theta w^k \hat{p}_i^k\left[h_i^k\left(e_i^k\right)\right] - \gamma e_i^k\right\} \tag{2.2}$$

where $V_i^{k'} = \max\left\{V_i^1, \ldots, V_i^{k-1}, V_i^{k+1}, \ldots, V_i^K\right\}$, which is given and does not vary with v_i^k and $s_{-i(c)}$ — i.e., neither their program nor their classmates provide any information on the value of other careers. Given the revelation of $s_{-i(c)}$, we define another effect:

Definition 3. Ranking effect *is the direct effect that peer skills, $s_{-i(c)}$, have on the subjective probability of being employed and on its derivatives.*

Suppose student i is randomly assigned either to class 1 or to class 2, with $\left(s_{-i(1)} \cup s_{-i(2)}\right) = F^{k^{-1}}$, $\left(s_{-i(1)} \cap s_{-i(2)}\right) = \emptyset$ and $s_{-i(1)} \geq s_{-i(2)}$. That is, the distribution of students in the program is equal to the true distribution of skills in the career, no student attends the two classes at the same time, and at least one student in class 1 has better skills than the rank-equivalent student in class 2. From the model above, we extract the following predictions.

Proposition 1. *The ranking effect increases (reduces) the student's probabilities of switching programs and dropping out of college in class 1 (class 2). The peer effect has the opposite consequence. Therefore, the net effect of going to the better class is ambiguous.*

Proposition 2. *If the career is highly competitive, then the ranking effect reduces (increases) this student's effort in class 1 (class 2), decreasing (increasing) their true expected salary. The peer effect has the opposite consequence. Therefore, the net benefit of going to the better class is ambiguous.*[11]

[11] If the career is less competitive, then the ranking effect increases the student's effort and future salary

Proposition 3. *The larger the initial information set, I_i, the lower the ranking effect on effort and career change. The peer effect does not depend on I_i.*

2.3 Institutional Background

The *Universidade Federal de Pernambuco* (UFPE) is the major flagship university in the Northeast of Brazil and one of the top ten institutions in the country.[12] In addition to its high quality and reputation, it is a public university and does not charge tuition fees. As a result, UFPE is the top choice of almost every high school student in the state of Pernambuco.

2.3.1 Admission Policy

About 95% of its undergraduate students are admitted through an exam, called *vestibular*, which is held only once a year.[13] Some 68% of the candidates are students who have recently graduated from high school. Half of them are taking the *vestibular* for the first time and the other half are retaking it because they were not admitted the year before. The minority consists of candidates who came from other institutions or study programs (12%), graduated from the adult education program (2.5%), or have not studied for a while (17.5%). In fact, anyone with a high school diploma or equivalent can apply to the university; the chances of being accepted depend uniquely on the test score.

The admission process in Brazil requires candidates to choose their major when they apply. That is, they are not admitted to the university as a whole, but to a particular undergraduate program offered by the institution. To switch majors, the student has to retake the *vestibular* and compete for a place in the new program. A very few students, less than 5%, are able to skip this process and join a program that is short of non-freshman students. Thus, starting a new program implies a substantial delay in graduation.

The *vestibular* has two rounds. The first one assess students' general knowledge and eliminates about 40% of the candidates.[14] In the second round, the remaining candidates are tested in Portuguese, a foreign language, and three other subjects that are particularly required for the major. The final score is a weighted average of the first- and second-round scores. Finally, each program admits those candidates with the best final scores until all the places are taken. Only 10% on average of the original candidates per program are admitted.

in class 1. The peer effect on effort is ambiguous because a better peer quality improves learning, but it also substitutes effort. However, the peer effect increases the future salary in class 1.

[12] According to the Ministry of Education, UFPE has always had the highest evaluations in the Northern and Northeastern regions of Brazil since 1995.

[13] In 2015, all programs began to adopt the new national entrance process (the Unified Selection System, SISU) to public universities in Brazil, ending institution-specific exams.

[14] Since 2010, the first round has been replaced by the National High School Exam (ENEM), which has a similar structure.

2.3.2 Class Assignment

Fifty-seven out of 99 programs offer two options for the freshmen. They can start studying either in the first semester (called the 'first class' hereafter) or in the second semester of the academic year (called the 'second class').[15] These classes must have the same number of students. Despite delaying graduation for at least half a year, starting later does not change a student's curriculum because all the required courses are offered every semester. Most importantly, students starting in different terms will have different classmates even though they attend the same institution.

In those programs, candidates are required to reveal their class preference before taking the entrance exam. In practice, almost 70% of the admitted students prefer to attend the first class. Given the limited number of seats, the order of preference is strictly based on their final entrance score. Once the first class is full, the remaining students have to join the second class, regardless of their initial choice. The final classification of candidates, organized by class and major, is fully disclosed by the admission committee (*Comissão para o Vestibular*, COVEST) through its website and printed in the newspapers. Candidates cannot switch classes after the final classification is revealed.

Figure 1.1 shows how this process creates a discontinuity in the relationship between entrance score and class assignment. This allows us to compare the last student who had the right to join the first class and the first student who did not have this privilege. Although they had essentially the same final entrance score, the latter is ranked higher in her own class than the former. On the downside, the higher rank is accompanied by worse peers.

Despite the initial class assignment, course retention forces first-class students to attend classes with second-class students, and vice versa. To keep our instrument valid, we analyze the effect of the initial assignment instead of the actual class composition. The bias created by mixing classes should pull our estimates towards zero. Another concern is that first and second classes may differ in terms of teaching. Although instructors often teach the same course every semester, any teaching discrepancy could compromise our analysis.[16]

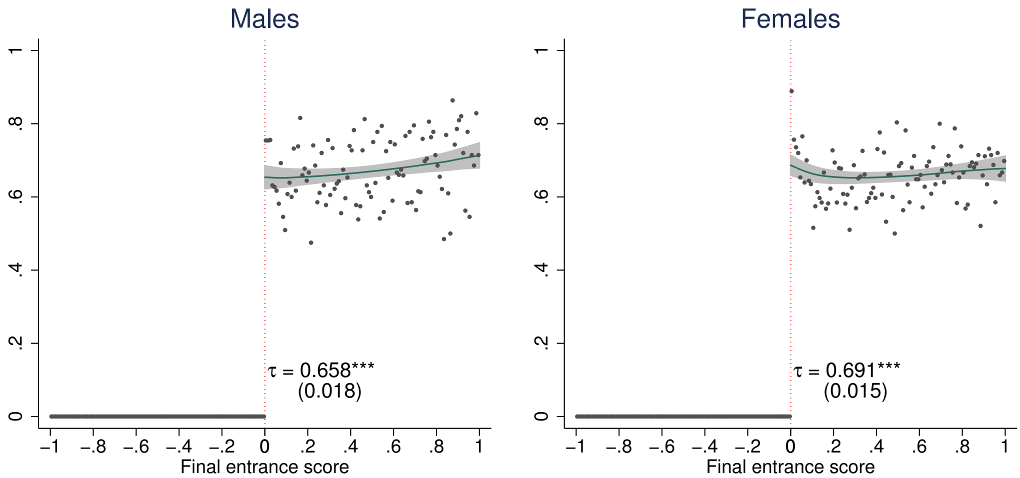
2.4 Data and Descriptive Statistics

2.4.1 Data Sources and Sample

Our data come from three different sources. The first is the admission committee (COVEST), which provides information on every applicant from 2002 to 2012. The second is UFPE's Academic Information System (*Sistema de Informações e Gestão Acadêmica*, SIGA), which provides information on students' enrollment, grades and status. The third

[15]Table B1 of the Appendix presents the list of programs, indicating those with two classes.
[16]Table B2 of the Appendix confirms that their characteristics are balanced in our setting.

Figure 2.1: Relationship between Final Entrance Score and Class Assignment



Notes: Final entrance score is standardized by program and year using the first-class cutoff and the standard deviation of admitted candidates' scores. Sample restriction is described in Section 2.4.1. Class assignment is 1 if candidate goes to the first class and 0 otherwise. It is estimated using triangular kernel with the bandwidth selection procedure proposed by (Calonico et al., 2014). τ is the regression discontinuity estimate, with robust standard errors in parentheses. ***, **, * represent statistical significant at the 1%, 5% and 10% levels, respectively.

is the Annual Social Information Report (*Relação Anual de Informações Sociais*, RAIS) from the Ministry of Labor, which contains information on every registered employee in Brazil.

Our sample of applicants ends in 2012 because since 2013 the university has adopted an affirmative action policy. This new policy affected the composition of classes and students' initial ranking. Since we perform a peer effect analysis, we also exclude cohorts (program-year) in which at least one class has fewer than 15 freshmen.[17]

2.4.1.1 Applications and Entrance Score

The COVEST data include the test scores from the first and second rounds and the final entrance score. Since all candidates take the same exam in the first round, the round 1 score is our proxy for cognitive skills, which is used to compare students across programs. This score is standardized by year using the mean and standard deviation of all the candidates. We also use the round 1 score to assess 'peer quality,' measured by the median score in the class, and 'peer heterogeneity,' measured by the standard deviation within a class.

The final score is the determinant of class assignment and ranking. We standardize this variable by program and year using the first-class cutoff — i.e., the final score of the last student in the first class — and the standard deviation of admitted candidates' scores.

[17]The inclusion of small cohorts adds noise to our estimates, but the estimated magnitudes do not change.

To rank students per class, we use the percentiles of the final score. The last student in a class has a rank equal to zero, while the first student's rank equals one.

The COVEST data also include the number of times each candidate did the entrance exam in the past, their previous score(s), motivation to enter the program, previous studies, and a long list of characteristics, such as age, gender, race, employment, and parents' education. On the basis of this information, we restrict our sample to candidates who are admitted by UFPE for the first time and join a program with two classes. Moreover, the sample excludes students who are admitted through a process other than the *vestibular* and who are more than 21 years old.[18] The final sample comprises 55% of the freshman students enrolled in two-class programs, representing 41% of all UFPE freshmen. It is worth mentioning that students' rank and peer quality were measured before the sample was restricted.

2.4.1.2 College Enrollment and Transcripts

SIGA provides detailed information on all students enrolled in 2002-2014, regardless of when they enter and leave the institution. Variables include students' academic status (active, graduated or dismissed), the number of missed sessions in each course enrolled, and the final grade of every course taken in the university.[19] These grades are used to calculate students' GPA, failure rate, dropout rate, and standardized grade by course. Based on the students' status, we also verify whether they switch programs before graduating. Students who did not enroll in any course in the first semester are excluded.

This source also contains the grade and status (attended or not) of all the students in the first midterm exam of mandatory courses, and the characteristics of all instructors, such as gender, age and academic position. To assess instructors' unobserved characteristics, we estimate instructor-specific parameters related to dropout and failure rates in their courses (see Section B.2 of the Appendix). Since each student takes several courses at the same time, with different instructors, all these variables are averaged per semester.

2.4.1.3 Earnings and Occupation

In Brazil, every registered firm is legally required to annually report every worker employed in the previous year, with information about salary, number of months worked, and type of occupation. This information is available on RAIS. Using students' social security number (*Cadastro de Pessoa Física*, CPF), we match the two previous data sources with RAIS to obtain their earnings and occupation for every year from 2002 to 2014.

Individual earnings are calculated as the sum of all salaries in 12 months, deflated to December 2014 using the Extended Consumer Price Index (IPCA). The 12 months

[18]Almost 75% of the candidates are 21 years old or younger.

[19]For 2002, we have only information on academic status.

are counted from the month of expected graduation, which is either January or July. As regards employment and occupation, we construct three variables: whether the student was employed for at least a month; whether she, if employed, had a management position (excluding supervisors); and whether she, if employed, worked as a public servant.

The final variables are constructed for each year after the students' expected graduation in their initially chosen program. This sample faces a restriction because the younger the cohort, the lower the number of years available. At the same time, the further we move into the future, the higher the probability of those students being employed. A year after the expected graduation, 67% of the original observations remain in our sample, but less than 35% of them are formally employed. Five years later, the employment rate is higher than 70%, but we only observe 30% of the original sample.[20]

2.4.2 Descriptive Statistics

Table 1.1 presents descriptive statistics for all the variables in our dataset. Due to the class assignment, described above, both the final entrance score and round 1 score are, on average, higher in the first class. The average GPA in the first two years and the rate of graduation are also higher in the first class, which confirms that it has indeed better students. However, these students, particularly the men, are also more likely to give up and join another program.

The average round 1 scores of the women are lower overall. Despite prevailing at UFPE, women are more likely to enroll in programs with less competitive admission. Nonetheless, their GPA is higher and they have a higher chance of graduating and a lower probability than men of switching majors. The covariates also confirm that the majority of students is white or comes from private high schools, and just 9% were already working at the time of the application. The greater part of disadvantaged students are in the second class — i.e., it has a greater proportion of black students, from public high schools, with less parental education, and who work and study at the same time. Therefore, the simple comparison between classes can be misleading because of differences in students' characteristics.

2.5 Empirical Strategy

Estimating peer effects is challenging because individuals are selected into groups by their unobserved skills. In addition to the biased selection, estimating ranking effects is even more difficult because the order of students depends on their peers' skills. Even if students were randomly assigned to different peer groups, a higher quality of peers would be associated with a lower rank. To deal with these identification problems, we use

[20]See Table B3 of the Appendix.

Table 2.1: Descriptive Statistics

	Males				**Females**			
	1st class		2nd class		1st class		2nd class	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Final entrance score	1.216	0.913	0.273	0.957	1.164	0.917	0.301	0.943
Round 1 score	0.532	0.989	0.179	0.963	0.203	0.986	-0.139	0.983
Class rank	0.473	0.278	0.480	0.287	0.462	0.282	0.469	0.284
Switched programs	0.086	0.280	0.068	0.252	0.046	0.210	0.037	0.188
Tried another vestibular	0.136	0.343	0.114	0.318	0.092	0.289	0.072	0.258
Graduated on time	0.483	0.500	0.432	0.495	0.665	0.472	0.644	0.479
Dropped out*	0.239	0.427	0.260	0.439	0.147	0.354	0.154	0.361
Number of courses taken**	5.399	1.114	5.444	1.239	5.840	1.511	5.755	1.572
Missed first midterm	0.061	0.209	0.075	0.231	0.038	0.171	0.049	0.193
First midterm grade	-0.161	0.713	-0.161	0.726	-0.041	0.634	-0.041	0.660
Number of absences**	1.263	4.184	1.325	3.928	0.970	3.245	1.199	3.887
Standardized course grade***	-0.171	0.695	-0.198	0.684	-0.019	0.616	-0.019	0.619
GPA***	7.184	1.426	6.983	1.457	7.707	1.063	7.614	1.088
Failure by grade***	0.094	0.165	0.119	0.189	0.049	0.116	0.057	0.124
Failure by attendance***	0.101	0.239	0.096	0.216	0.046	0.167	0.043	0.147
3 years after expected graduation								
Employed	0.573	0.495	0.593	0.491	0.607	0.489	0.614	0.487
Log salary	10.250	1.277	10.211	1.033	9.953	1.149	9.905	0.992
Government job	0.401	0.477	0.417	0.477	0.386	0.473	0.347	0.459
Manager	0.101	0.295	0.089	0.275	0.075	0.256	0.079	0.254
Covariates								
Age	18.98	1.056	19.07	1.034	19.04	1.051	19.08	1.059
White	0.612	0.487	0.569	0.495	0.579	0.494	0.548	0.498
Living in Pernambuco	0.873	0.332	0.877	0.329	0.886	0.317	0.891	0.311
From public high school	0.217	0.412	0.237	0.425	0.248	0.432	0.287	0.452
Employed at application	0.094	0.292	0.114	0.318	0.073	0.260	0.087	0.282
Number of vestibular tries	1.711	0.803	1.724	0.788	1.805	0.831	1.816	0.830
Both parents with college degree	0.321	0.467	0.284	0.451	0.258	0.438	0.202	0.402
No parent with college degree	0.414	0.493	0.462	0.499	0.505	0.500	0.569	0.495
Reason for choosing the program								
Opportunities and prestige	0.252	0.434	0.274	0.446	0.225	0.418	0.251	0.434
Self-fulfillment	0.532	0.499	0.529	0.499	0.596	0.491	0.569	0.495
Other motives	0.216	0.412	0.197	0.398	0.178	0.383	0.180	0.384
Instructor characteristics								
Female instructors	0.369	0.241	0.361	0.244	0.492	0.231	0.485	0.245
40+ year-old instructors	0.602	0.266	0.584	0.268	0.673	0.266	0.666	0.257
Assistant professors	0.451	0.264	0.467	0.248	0.506	0.258	0.502	0.235
Associate or full professors	0.356	0.280	0.338	0.272	0.331	0.277	0.324	0.268
Instructor quality								
Dropout rate	-0.043	0.027	-0.043	0.024	-0.033	0.019	-0.035	0.019
Failure rate	-0.018	0.022	-0.018	0.023	-0.010	0.015	-0.011	0.017
Number of observations	5,686		5,624		7,254		7,620	

Note: *Only for students who are at least two years at UFPE. **In the first semester. ***In the first year, sample does not include those who drop out before the third semester. Sample includes candidates admitted for the first time, who are 21 years or less.

UFPE's rule of class assignment and the variation in skills distribution across program cohorts.

Let y_{kci} be the outcome of interest of student i in class c of program k. This outcome is a function of each student's rank, r_{kci}, and peer quality, q_{kc}. These variables depend not only on the program k, chosen by the student, but also on the class assignment, which can be either $c = 1$ for those in the first class or $c = 2$ for those in the second class. To simplify our setting, we assume no time variation. But in practice we also exploit the fact that the class composition within programs changes every year. Then suppose that the outcome is a function of these explanatory variables in the following way:

$$y_{kci} = B(r_{kci}) + \gamma\, q_{kc} + u_{kci} \tag{2.3}$$

where $B(.)$ is a monotonic continuous function and $u_{kci} = \nu_k + \mu_i + \varepsilon_{kci}$. The identification problem is that we do not observe the same student in two different classes, so we cannot control for μ_i.

For each program, we consider that the last student joining the first class is very similar to the first student out of the first class. Let x_{ki} be the entrance score of student i in program k and $\underline{x}_k$ be the score of the last student joining the first class. If $x_{ki} \geq \underline{x}_k$, then the student can choose between classes 1 and 2. But if $x_{ki} < \underline{x}_k$, then the student must join the second class, which implies that $\Pr\left(c = 1 | x < \underline{x}_k\right) = 0$, as shown in Figure 1.1. For any variable z, the expected difference between classes for the last student in the first class is given by the following fuzzy estimand:

$$\begin{aligned} \Delta z &\equiv E\big(z | c = 1, x = \underline{x}_k\big) - E\big(z | c = 2, x = \underline{x}_k\big) \\ &= \frac{\lim_{x \downarrow \underline{x}} E\left(z | x \geq \underline{x}_k\right) - \lim_{x \uparrow \underline{x}} E\left(z | x < \underline{x}_k\right)}{\lim_{x \downarrow \underline{x}} \Pr\left(c = 1 | x \geq \underline{x}_k\right)}. \end{aligned} \tag{2.4}$$

Then from equation (2.3), the *net effect of the first class* is given by:

$$\Delta y = \beta\, \Delta r + \gamma\, \Delta q, \tag{2.5}$$

where $\beta = \left[B(\bar{r}_1) - B(\bar{r}_2)\right] / \Delta r$, with $\bar{r}_c = E\big(r | c, x = \underline{x}_k\big)$; and the net (naive) ranking effect given by the fuzzy estimand is:

$$\frac{\Delta y}{\Delta r} = \beta + \gamma \frac{\Delta q}{\Delta r}. \tag{2.6}$$

Both effects identified by the discontinuity in the class assignment depend on the difference in peer quality, which would cancel out the ranking effect according to Propositions 1 and 2.

Unlike Δr, which is a fuzzy estimand, Δq_k is observed for each program (every year). Even though its effect can be specific per student, its value is not specific to those close to the cutoff — i.e., $E\big(q|k, c, x\big) = E\big(q|k, c\big)$. The difference in peer quality between classes is common to all students in the same program. Hence, for programs in which classes are similar ($\Delta q_k = 0$), we can calculate the marginal *ranking effect* as follows:

$$
\begin{aligned}
\left.\frac{\Delta y_k}{\Delta r_k}\right|_{\Delta q=0} &= \frac{\lim_{x\downarrow\underline{x}} E\left(y|x \geq \underline{x}_k, \Delta q_k = 0\right) - \lim_{x\uparrow\underline{x}} E\left(y|x < \underline{x}_k, \Delta q_k = 0\right)}{\lim_{x\downarrow\underline{x}} E\left(r|x \geq \underline{x}_k, \Delta q_k = 0\right) - \lim_{x\uparrow\underline{x}} E\left(r|x < \underline{x}_k, \Delta q_k = 0\right)} \\
&= E\left(\beta_k|\Delta q_k = 0\right) \qquad (2.7)
\end{aligned}
$$

where $\beta_k = \left[B(\bar{r}_{k1}) - B(\bar{r}_{k2})\right] / \Delta r_k$, with $\bar{r}_{kc} = E\big(r|c, x = \underline{x}_k, \Delta q_k = 0\big)$.

By estimating the relationship between $(\Delta y_k, \Delta r_k)$ and Δq_k, we not only isolate the ranking effect at $\Delta q_k = 0$ but also verify how the net effect, Δy_k, changes with a higher peer quality in the first class. Consider that

$$
\frac{d\Delta y_k}{d\Delta q_k} = \gamma + E\left(\beta_k|\Delta q_k\right)\frac{d\Delta r_k}{d\Delta q_k} + \Delta r_k \frac{dE\left(\beta_k|\Delta q_k\right)}{d\Delta r_k}\frac{d\Delta r_k}{d\Delta q_k}. \qquad (2.8)
$$

Note that Δr_k is negative because the last student in the first class should always increase their rank by moving to the second class. Moreover, $d\Delta r_k/d\Delta q_k$ is negative because the wider the gap between the two classes, the sharper the discontinuity in the student's rank (see Table 2.2). If we assume that $B(.)$ is weakly monotonic, then $d\Delta y_k/d\Delta q_k > 0$ implies that $\gamma > 0$. That is, $d\Delta y_k/d\Delta q_k$ provides a lower bound estimator for the peer effect, γ.

The estimation of this relationship is possible because the peer quality is not measured by the entrance score (running variable) itself, but rather by a cognitive score that is comparable across programs. Since the entrance score is specific by program, it does not tell anything about how similar the classes are in comparison to those in other programs. In addition, the difference in peer quality, Δq_k, varies sufficiently across program cohorts, as we show in Figure 2.4. To verify the robustness of our findings, we also estimate the relationship between fuzzy discontinuities and the difference in the standard deviation of skills between classes, forcing it to be zero. Details on the estimation procedures are in the Appendix. Robust standard errors and optimal bandwidths are obtained as described by Calonico et al. (2014).

2.6 Results

Our results are presented as follows. First, we verify how much the mechanism of class assignment affects students' rank and peer quality and test whether it is manipulated by candidates. Second, we present the estimated effect of ranking on the willingness to change majors and on academic performance. Third, we present the long-term effects on earnings and occupation. Finally, we investigate some mechanisms that may explain our main findings. The Online Appendix provides additional tests to certify the validity of our results.

2.6.1 First-Stage Estimates and Manipulation Test

UFPE's rule for class assignment creates two types of exogenous variation at the cutoff: rank and peer quality. The first panel of Figure 2.2 shows the discontinuity in students' rank at the entrance score cutoff. The last student to the right of the cutoff is indeed expected to be at the very bottom of her class, while the first student to the left of the cutoff is expected to be at the top. In spite of the imperfect compliance with the final score, the ranking difference between these students is of 35 pctl for men and 39 pctl for women.

The difference in ranking at the cutoff is simply the consequence of having classes at different levels. However, the pool of students could be so homogeneous that the class difference would be irrelevant. The second and third panels of Figure 2.2 show that not only the expected difference in peer quality is around 0.21 s.d., but the variability in peers' scores is 0.03 s.d. higher to the right of the cutoff than to the left. Therefore, students who just miss the cut for the first class fall into a significantly worse and more homogeneous class.

If students anticipated the disadvantage of either being the worst student in the first class or falling into the second class, they could decline the offer and the sample would suffer from a biased selection. To verify if such a behavior occurs, Figure 2.3 presents the density of enrolled students, separately estimated for both sides of the cutoff. A visual inspection suggests that the density is continuous at the cutoff. To formally test this continuity, we also apply the Cattaneo et al. (2017) version of McCrary (2008) test.[21] This test does not indicate evidence of missing students on either side of the cutoff. In addition, we test for discontinuities in students' and their instructors' characteristics and find no significant difference (see Table 1.7 of the Appendix).

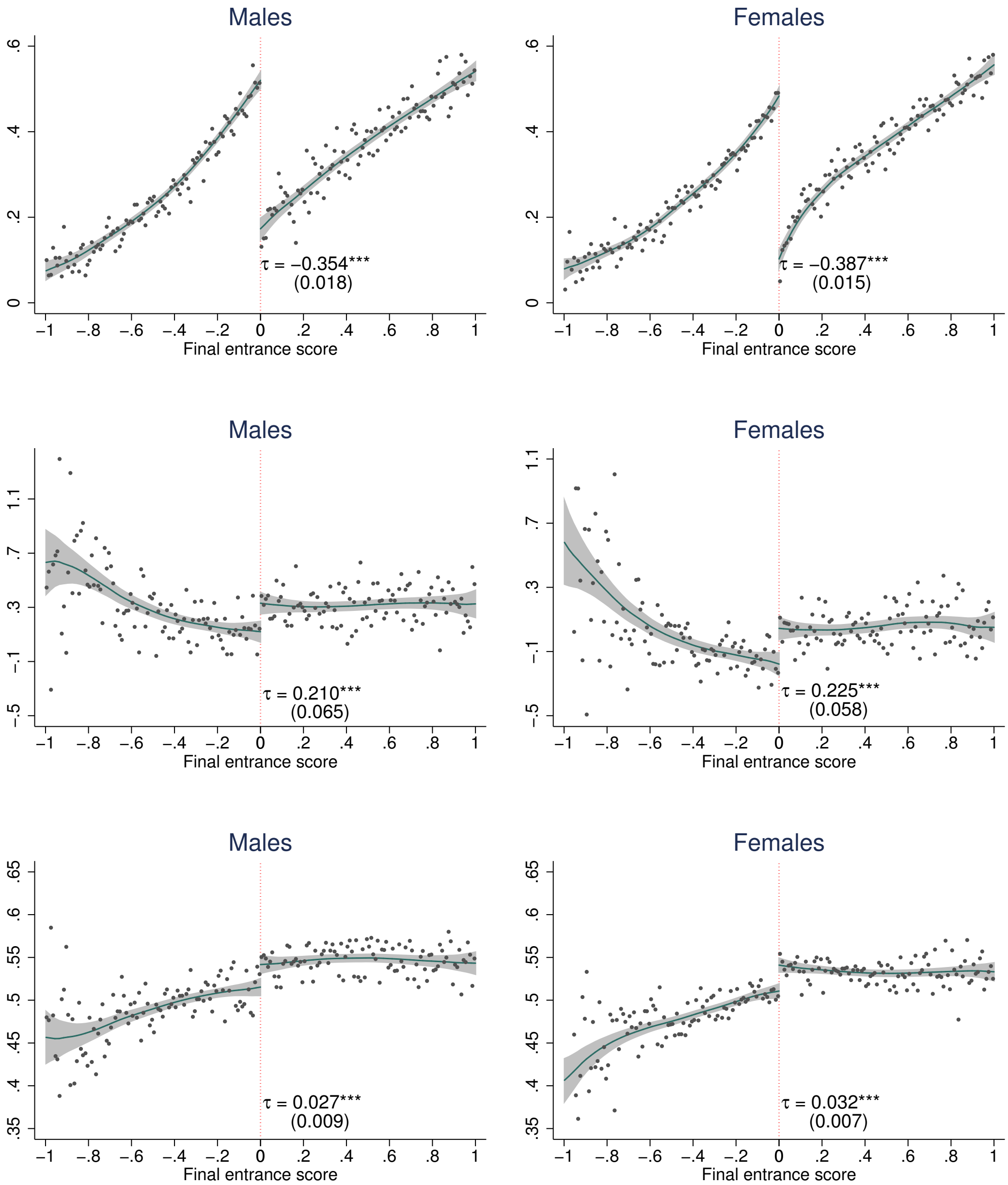
To separate the effect of ranking from the peer effect, we also exploit the fact that the difference in peer quality between classes varies across programs and over time. Figure 2.4 shows that facing almost no difference in peer quality is not rare. Although the mean difference in the median peer's score between classes is 0.37 s.d., for 9% of females and 5% of males, this difference is less than or equal to 0.1 s.d. Thus, the lower tail of these distributions should be fat enough to provide accurate estimates for the ranking effect. Another concern is that the lower tail represents a specific set of programs. Table B4 of the Appendix highlights all the programs that fell into this tail at least once, ensuring that the results are not driven by specific fields.

To estimate accurate ranking effects, the ranking discontinuity must be strong also

[21] Cattaneo et al. (2017) test is not sensitive to the choice of bin width. Results for the original McCrary's test are available upon request.

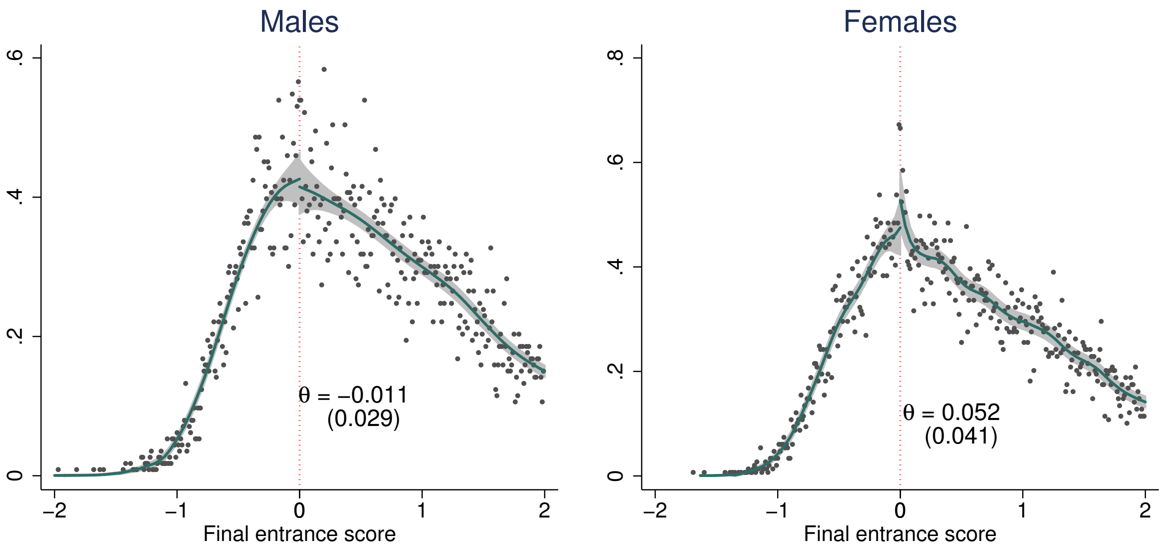
in cases in which the class difference is close to zero. Table 2.2 shows how the ranking discontinuity changes as a function of the difference in the median peer's score and the difference in peer heterogeneity. Although this discontinuity does not change much with the heterogeneity, it increases drastically with the peer quality. Hence, if both effects are monotonic, the non-marginal ranking effect must increase with the non-marginal peer effect.

Figure 2.2: Relationship Between Final Entrance Score and Treatments



Notes: Final entrance score is standardized by program and year using the first-class cutoff and the standard deviation of admitted candidates' scores. On the first panel, a student's rank is defined by the within-class percentile of their final entrance score. On the second panel, peer quality is measured by their median classmate's round 1 score and, on the third panel, peer heterogeneity is measured by the within-class standard deviation of round 1 scores. The sample comprises candidates admitted for the first time, who are 21 years or less. Functions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014). τ is the regression discontinuity estimate, with robust standard errors in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Figure 2.3: Density of Final Entrance Score and McCrary Test



Notes Final entrance score is standardized by program and year using the first-class cutoff and the standard deviation of admitted candidates' scores. The sample comprises candidates admitted for the first time, who are 21 years or less. θ is the Cattaneo et al. (2017) estimator for density discontinuity, with robust standard error in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively. Grey dots are bins of 0.02 s.d.

Figure 2.4: Distribution of Differences in Peer Quality and Heterogeneity

Figure 2.5: Peer Quality

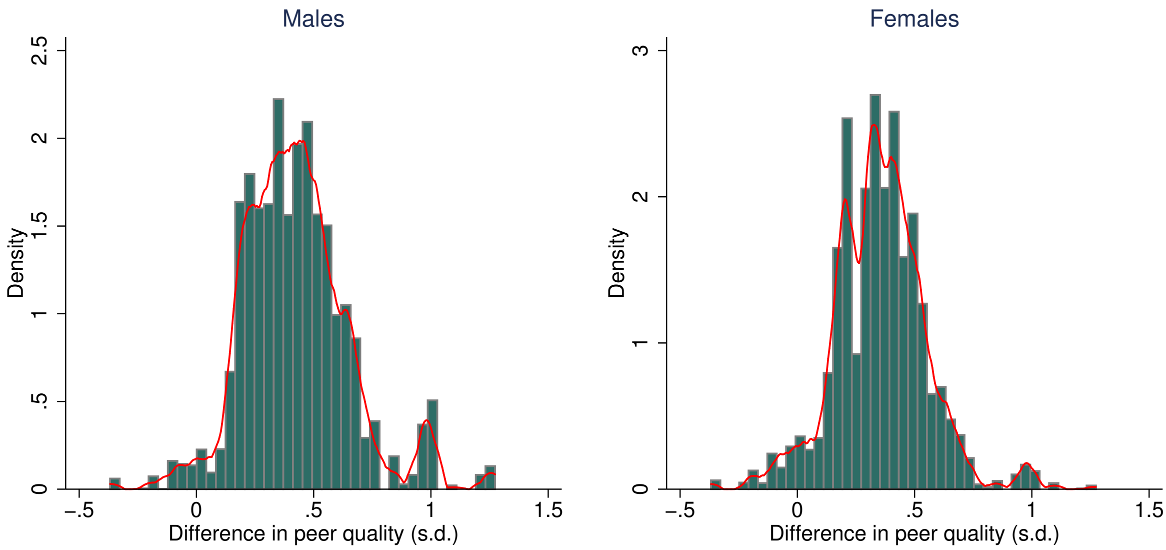
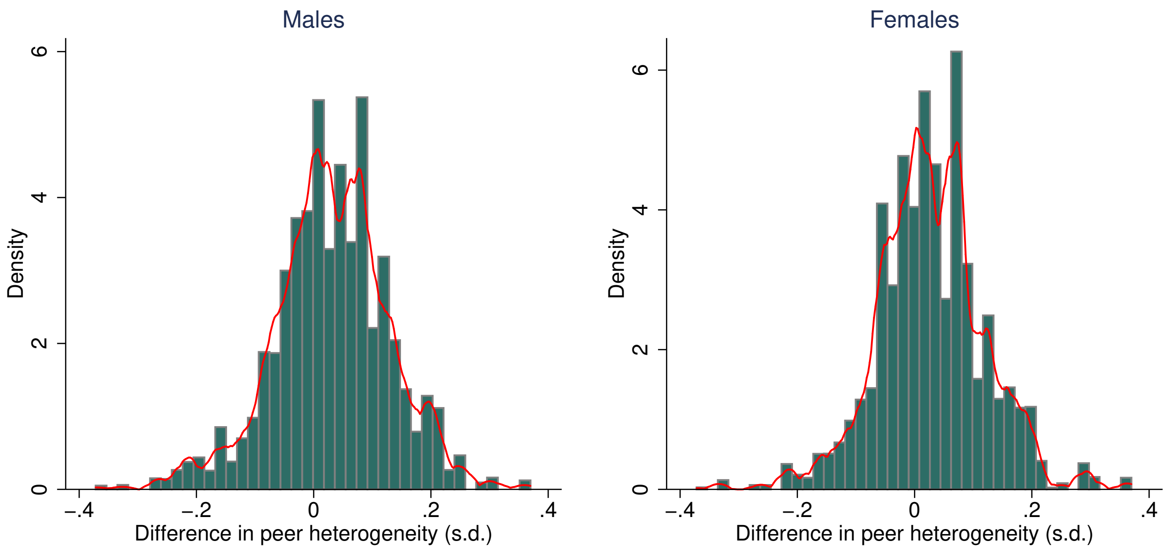


Figure 2.6: Peer Heterogeneity



Notes: This figure presents the histograms for differences in peer quality and heterogeneity between classes in the same program in the same year. Peer quality is measured by a student's median classmate's round 1 score and peer heterogeneity is measured by the within-class standard deviation of round 1 scores.

Table 2.2: Ranking Discontinuities by Difference in Peer Quality and Heterogeneity

	Difference in peer quality (s.d.)									
	Males					Females				
Difference in peer heterogeneity (s.d.)	Δ=0	Δ=.175	Δ=.350	Δ=.525	Δ=.700	Δ=0	Δ=.175	Δ=.350	Δ=.525	Δ=.700
Δ=-.20	-0.154***	-0.244***	-0.326***	-0.412***	-0.469***	-0.237***	-0.324***	-0.390***	-0.455***	-0.510***
	(0.031)	(0.028)	(0.028)	(0.029)	(0.032)	(0.025)	(0.026)	(0.027)	(0.030)	(0.034)
Δ=-.10	-0.172***	-0.259***	-0.336***	-0.417***	-0.476***	-0.230***	-0.318***	-0.386***	-0.463***	-0.522***
	(0.026)	(0.023)	(0.022)	(0.023)	(0.025)	(0.020)	(0.019)	(0.019)	(0.022)	(0.027)
Δ=0	-0.179***	-0.265***	-0.340***	-0.416***	-0.474***	-0.230***	-0.321***	-0.401***	-0.476***	-0.538***
	(0.026)	(0.022)	(0.019)	(0.020)	(0.022)	(0.018)	(0.016)	(0.015)	(0.018)	(0.024)
Δ=.10	-0.157***	-0.248***	-0.330***	-0.405***	-0.467***	-0.214***	-0.313***	-0.404***	-0.477***	-0.545***
	(0.028)	(0.023)	(0.021)	(0.021)	(0.023)	(0.022)	(0.018)	(0.017)	(0.018)	(0.025)
Δ=.20	-0.118***	-0.209***	-0.293***	-0.370***	-0.435***	-0.187***	-0.288***	-0.385***	-0.468***	-0.537***
	(0.036)	(0.031)	(0.028)	(0.027)	(0.030)	(0.029)	(0.026)	(0.024)	(0.025)	(0.030)

Note: This table presents the discontinuity in student's rank as a function of differences (Δ) in peer quality and heterogeneity between classes in the same program in the same year. Students' rank is defined by the within-class percentile of their final entrance score. Peer quality is measured by their median classmate's round 1 score and peer heterogeneity is measured by the within-class standard deviation of round 1 scores. The sample comprises candidates admitted for the first time, who are 21 years or less. Functions are estimated using triangular kernel with the bandwidth selection procedure adapted from Calonico et al. (2014). Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table 2.3: Net Effect of First Class on Major Switching and Graduation

	Males		**Females**	
	Reduced form	Net effect	Reduced form	Net effect
Switched programs	0.044**	0.065**	0.014	0.019
	(0.021)	(0.031)	(0.012)	(0.017)
Tried another vestibular	0.045**	0.068**	0.020	0.027
	(0.023)	(0.034)	(0.017)	(0.024)
Graduated on time	-0.070	-0.098	-0.015	-0.019
	(0.048)	(0.069)	(0.037)	(0.049)
Dropped out	0.023	0.034	0.040*	0.053
	(0.032)	(0.048)	(0.024)	(0.033)

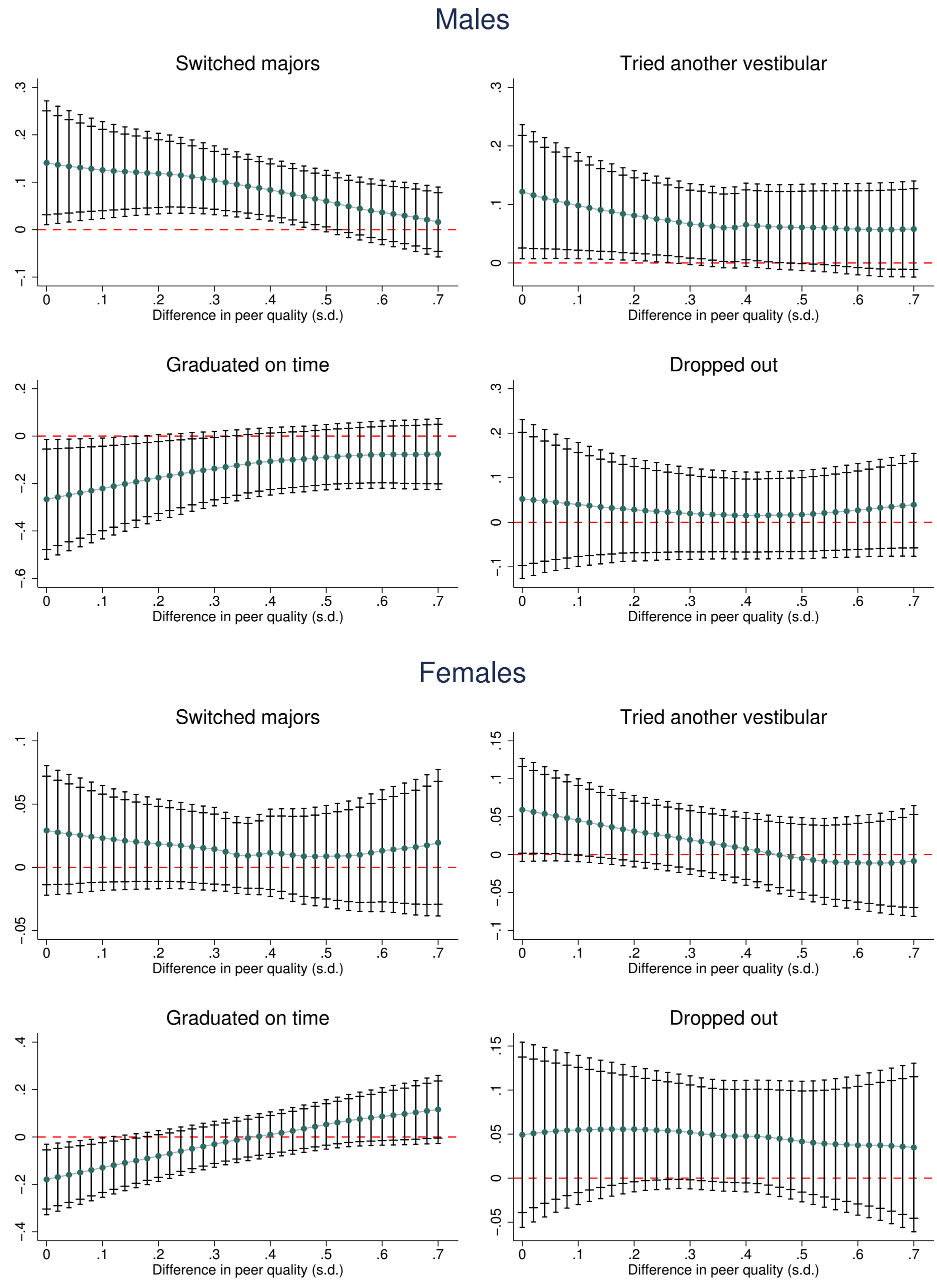
Note: This table presents the estimated regression discontinuity (RD) at the first class cutoff (reduced form) and fuzzy RD estimates of the first-class effect (net effect). The sample comprises candidates admitted for the first time, who are 21 years or less. RDs are estimated using triangular kernels. The bandwidth for entrance score is selected by using Calonico et al. (2014) procedure. Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

2.6.2 Major Switching, Graduation and Dropout

Being the last student in the best class may have pros and cons, as stated in Proposition 1. To weight these pros and cons, we first present the net effect of going to the first class on graduation and on the decision to switch majors. The findings presented in Table 2.3 suggest that it has almost no effect on females. For males, going to the first class increases their probability of trying another *vestibular* by 6.8 p.p. and switching majors by 6.5 p.p. Despite the difference in peer quality, going to the bottom of the first class makes male students more likely to give up their original major choice. This result is robust to the bandwidth choice, as shown in Figure B1 of the Appendix.

The next step is to verify how the net effect changes as a function of the difference in peer quality. Figure 2.7 presents the estimated relationship using the difference in class median scores. We also define the class difference using other percentiles and find similar patterns (see Figure B2 of the Appendix). If the peer difference is zero, both males and females at the bottom of the first class are less likely to graduate on time. This effect, however, diminishes with the difference in peer quality. Since the ranking discontinuity also increases with the difference (see Table 2.2), this pattern suggests that the peer quality offsets the ranking effect after a certain level. For men, this level is between 0.4 and 0.7 s.d., which implies that the ranking effect is predominant for at least 50% of the students close to the cutoff. For women, the ranking effect on graduation is predominant at least until 0.2 s.d., which represents almost 20% of the students close to the cutoff. This difference between males and females explains why the net effect of the first class is higher for men in Table 2.3.

Figure 2.7: Net Effect of First Class by Difference in Peer Quality



Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the first-class effect as a function of differences in the median peer's round 1 score (peer quality). The vertical bars represent the robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs and their relationships with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. and the bandwidth for entrance score is selected by using Calonico et al. (2014) procedure.

The lower, but increasing graduation rate in the first class is followed by a higher, but decreasing chance of trying another *vestibular*. For males, we also observe a similar pattern in major switching. For females, in contrast, the effect on major switching is flat and insignificant for all levels of peer quality. This result suggests that the pure ranking effect makes graduation in the first class harder for both genders. But while males respond to the difficulty by starting a new program, females are less able to do likewise. In fact, Table 1.1 shows that women already enroll in programs with less competitive admission, so they do not have as many remaining options as men. Despite the difficulty, neither gender is found to drop out of college because of the class assignment.

Given the way in which ranking effect and peer effect are confounded, we try to isolate the former by centering our estimates on cohorts in which the difference between classes is close to zero. Table A.3 shows that a 10 pctl drop in a student's rank increases the probability of trying another *vestibular* by 3.6 p.p. for males and 1.9 p.p. for females. For men, this effect is followed by an actual change in majors (of the same magnitude). The lower rank also reduces the chance of graduating on time by 9.3 p.p. for men and 5.6 p.p. for women.

In Table A.3, we also let the marginal ranking effect change conditionally on the difference in class heterogeneity. Although the ranking effect is even stronger with a greater deviation among students, our findings remain the same whenever there is no difference between classes. Figure B3 of the Appendix confirms that the findings are robust to the bandwidth choice for the entrance score and for the difference in peer quality.

So far, our findings suggest that the pure ranking effect makes graduation in a timely manner more unlikely for students at the bottom of the first class. We also verify whether those effects are related to grades and attendance in class. Table 1.4 of the Appendix shows that both men and women have worse grades in the first semester if they are at the bottom of the first class. Figure B4 confirms that students with lower initial rank continue to have lower GPA and higher failure rates in the following semesters. On the other hand, there is no strong evidence that these students take fewer courses or have a higher dropout rate. Finally, an increase in peer quality does not seem to mitigate the ranking effect on grades (see Figure B5).

2.6.3 Earnings and Occupation

For the cohorts that are observed from zero to five years after the expected graduation, we estimate the net effect of the first class and the ranking effect on employment, earnings and occupation. The further in the future, the higher the number of former UFPE students found in the labor market, which makes our estimates less biased and more accurate. However, only the older cohorts have reached those further years, which reduces our sample and makes our estimates more time-specific.

Figure 2.8 presents the net effect of the first class in those years,[22] while Figure 2.9

[22]For short programs, which last three to four years, year one means six to eight semesters after the classes started. For long programs, which last five years or more, year one means ten semesters later.

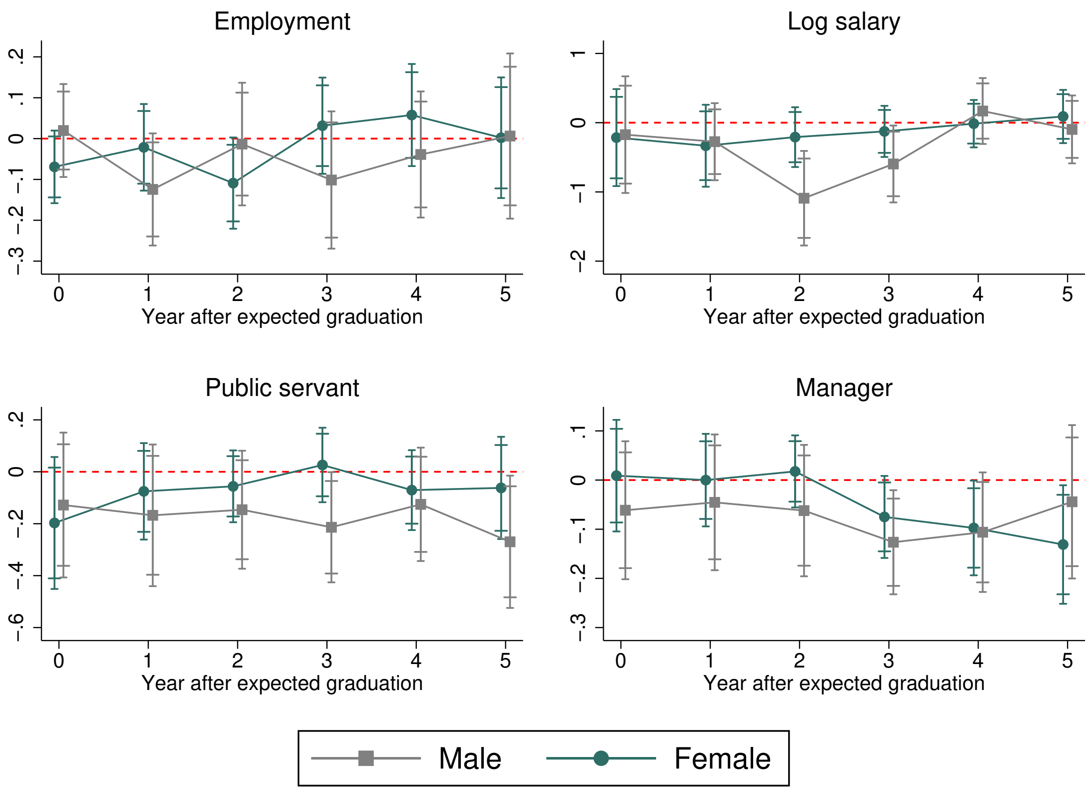
Table 2.4: Ranking Effect on Major Switching and Graduation

	Marginal Ranking Effect					
		By difference in peer heterogeneity (s.d.)				
	Average	Δ=-.20	Δ=-.10	Δ=0	Δ=.10	Δ=.20
				Males		
Switched Programs	-0.046**	-0.040	-0.032	-0.039*	-0.069**	-0.126*
	(0.023)	(0.036)	(0.023)	(0.021)	(0.033)	(0.070)
Tried another vestibular	-0.036*	-0.025	-0.031	-0.037*	-0.043*	-0.075
	(0.019)	(0.040)	(0.025)	(0.019)	(0.024)	(0.051)
Graduated on time	0.093**	0.131	0.099*	0.102**	0.127**	0.180**
	(0.045)	(0.080)	(0.056)	(0.047)	(0.053)	(0.085)
Dropped out	-0.014	-0.033	-0.020	-0.015	-0.007	0.008
	(0.029)	(0.054)	(0.033)	(0.028)	(0.038)	(0.067)
				Females		
Switched Programs	-0.009	-0.011	-0.006	-0.007	-0.012	-0.016
	(0.009)	(0.016)	(0.011)	(0.008)	(0.010)	(0.015)
Tried another vestibular	-0.019*	-0.026	-0.017	-0.015	-0.024	-0.036
	(0.012)	(0.020)	(0.013)	(0.011)	(0.015)	(0.024)
Graduated on time	0.056**	0.052	0.046*	0.057**	0.065**	0.076*
	(0.024)	(0.036)	(0.026)	(0.023)	(0.030)	(0.043)
Dropped out	-0.016	-0.038	-0.025	-0.015	-0.002	0.012
	(0.018)	(0.028)	(0.019)	(0.018)	(0.023)	(0.035)

Notes: This table presents the fuzzy regression discontinuity (FRD) estimates of the average ranking effect in the first column and the ranking effect conditional on differences in peer heterogeneity between the classes (Δ) in the remaining columns. The ranking effect is estimated for cohorts in which the difference between median scores is zero. Peer heterogeneity is measured by the within-class standard deviation of round 1 scores. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs and their relationships with peer quality and peer heterogeneity are estimated using triangular kernels. The bandwidth for peer quality is 0.9 s.d. and for peer heterogeneity is 0.4 s.d. The bandwidth for entrance score is selected based on Calonico et al. (2014) procedure. Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

presents the relationship between ranking effect and peer quality. First, we find that men are about 11 p.p. less likely to be employed one and three years after their expected graduation if they attend the first class. Likewise, women in the first class are 11 p.p. less likely to be employed in year two. These differences are in part related to the fact that first-class students graduate later. In year five, however, the difference is very close to zero.

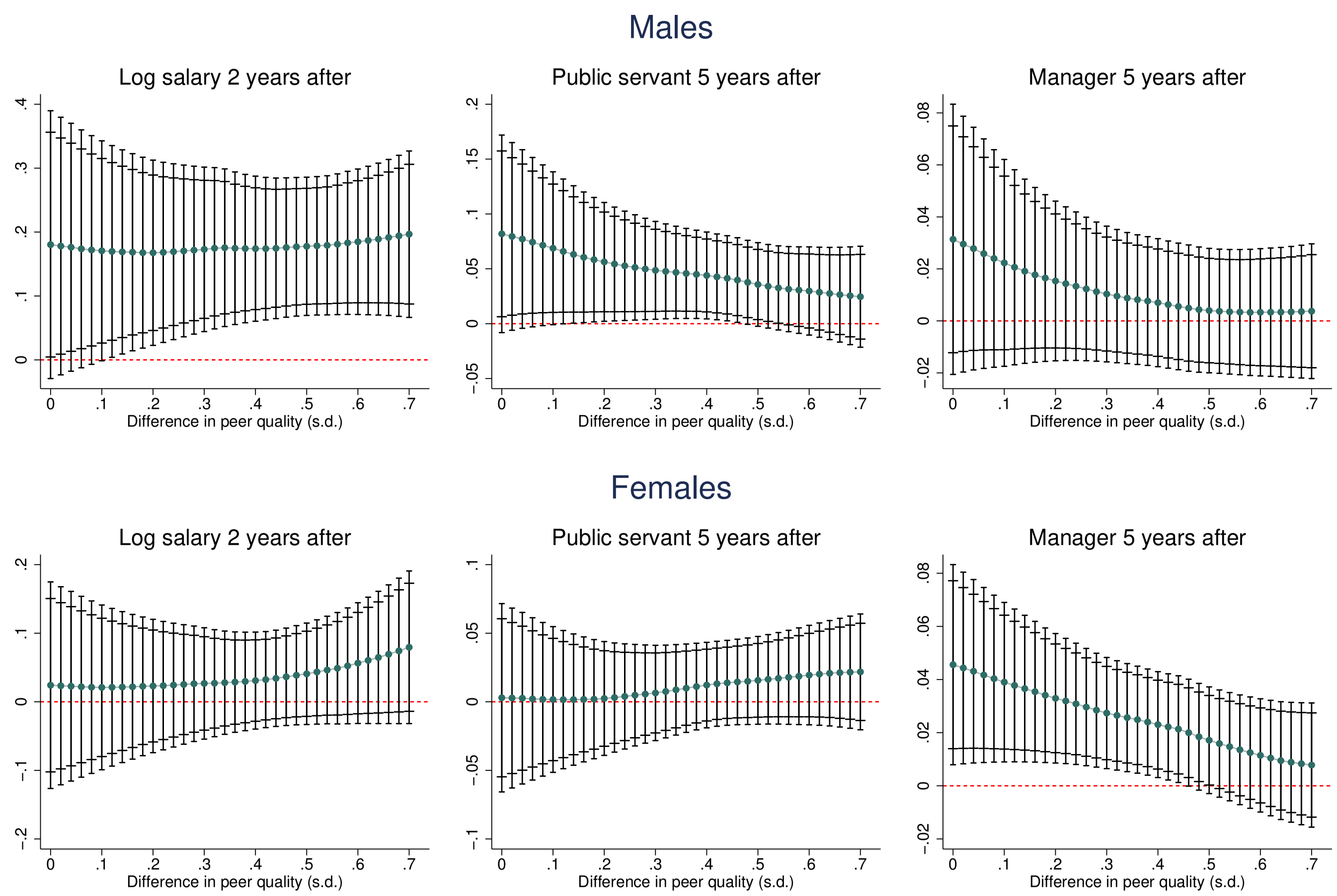
Figure 2.8: Net Effect of First Class on Labor Market Outcomes



Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the first-class effect on employment, earnings and occupation for each year after the expected graduation in the initial program. The vertical bars represent robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs are estimated using triangular kernels and the bandwidths are selected by using Calonico et al. (2014) procedure.

If employed, men in the first class also earn 66% less than those in the second class two years after the expected graduation and 45% less a year later. This net effect is largely explained by the rank discontinuity. If the difference in peer quality is zero, a 10 pctl drop in rank decreases by 20% the earnings in year two. In Figure 2.9, we observe that this ranking effect stays intact regardless of the gap in peer quality. Even though the effect on earnings is high and not moderated by peer quality, it disappears in year four. Using quantile regression discontinuity models, as proposed by Frandsen et al. (2012), we find that being assigned to the first class is particularly detrimental to the less productive workers (see Figure B6 of the Appendix).

Figure 2.9: Ranking Effect on Labor Outcomes by Difference in Peer Quality



Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the net ranking effect on labor outcomes as a function of differences in the median peer's round 1 score (peer quality). The vertical bars represent the confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FDRs and their relationship with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. and the bandwidth for entrance score is selected by using Calonico et al. (2014) procedure.

In spite of the transient effect on earnings, the first class also affects males' occupation. Figure 2.8 shows that five years after expected graduation the first class reduces by 27 p.p. their chance of being a public servant. In addition to a wage premium, government jobs in Brazil are considered safer and offer better retirement plans, particularly to high-skilled workers (Braga et al., 2009). Given the limited number of positions, the selection process is very competitive and based on specific exams applied by each governmental entity. As a result, some of the best college graduates end up having a public career.

For both men and women, attending the first class also affects the likelihood of being a manager. However, the effect on men is merely temporary, while the effect on women increases over time. In year five, women in the first class are 13 p.p. less likely to be a manager than similar women in the second class. According to Coelho et al. (2014), women find it harder, unless they outperform their male colleagues, to get promotion in Brazilian firms. The glass ceiling imposed on women's ascent may explain this long-term effect.

The probabilities of men being public servants and women being managers are much affected by their rank. Being 10 pctl higher in the class order increases by 8.2 p.p. the chance of men's working in the public service and by 4.6 p.p. the chance of women's having a management position in year five.[23] Even so, Figure 2.9 shows that these effects decline as a function of peer quality. The ranking effects are almost fully cancelled out in cohorts where the difference in peer quality is higher than 0.6 s.d. Nonetheless, this great difference between classes is found in less than 10% of the sample.

2.6.4 Heterogeneity in the Ranking Effect

To better understand the mechanism behind the ranking effect, we verify whether it is related to the type of program and students' characteristics. For the type of program, we split the sample into harder and easier curriculum, based on a program's average failure rate, into higher and lower shares of male students, and into harder and easier admission, based on a program's median round 1 score. For each gender and categorization, we make a median split so that the subsamples within males and females are of the same size. For the type of student, we separate the sample based on parents' education and reason for choosing the program. Parents' education can be either 'both parents have a college degree' or 'neither parent has a college degree,' while the reason for choosing the program can be either 'market opportunities and career prestige' or 'other motive,' which includes self-fulfillment, low competition, and parents' choice. Tables 2.5 and 2.6 present the estimated ranking effect for each group. Since sample sizes are smaller, estimates are less accurate.

Results for the type of program in Table 2.5 suggest that the ranking effects on academic performance and on the decision to change majors are not necessarily related.

[23]Figure B7 of the Appendix shows that these findings are robust to the bandwidth choice for the entrance score and for the difference in peer quality.

Poorly ranked men are more likely to switch programs if the original curriculum is easier and the share of female classmates is higher. However, their absolute performance is more sensitive to class ranking in programs with a harder curriculum and the presence of more males. For bottom-ranked women, the pattern is similar, except that they only try to change programs but do not go through with it.

If either the risk of failing or the share of men is higher, ranking matters not only for males' academic performance but also for their long-term outcomes in the job market. This finding is consistent with Proposition 2, which states that a lower rank has a negative effect on effort particularly in competitive environments. We also observe that these effects are higher in programs to which admission is easier, so that the overall quality of students is lower. This finding may be related to the fact that ranking has a higher effect on less productive workers. For women, we find that the easier the admission, the higher the effect on academic performance. However, the long-term effect on their likelihood of being a manager is felt particularly by the best college candidates, which is consistent with the glass-ceiling hypothesis.

Table 2.5: Heterogeneity in the Ranking Effect by Type of Program

	Males						Females					
	Difficulty of the curriculum		Share of males		Competition for admission		Difficulty of the curriculum		Share of males		Competition for admission	
	Easy	Hard	Low	High	Low	High	Easy	Hard	Low	High	Low	High
Academic outcomes												
Switched programs	-0.049**	-0.015	-0.048**	-0.022	-0.024	-0.027**	-0.004	-0.014	-0.015	-0.000	-0.009	0.000
	(0.020)	(0.018)	(0.022)	(0.020)	(0.028)	(0.013)	(0.007)	(0.015)	(0.011)	(0.010)	(0.013)	(0.008)
Tried another vestibular	-0.034	-0.020	-0.044**	-0.004	-0.041	-0.016	-0.021*	-0.013	-0.015	-0.013	-0.022	-0.003
	(0.023)	(0.021)	(0.022)	(0.022)	(0.042)	(0.014)	(0.011)	(0.018)	(0.016)	(0.012)	(0.017)	(0.011)
Graduated on time	0.157***	0.033	0.045	0.059*	0.043	0.036*	0.039	0.051	0.079*	0.035	0.055	0.031
	(0.058)	(0.032)	(0.047)	(0.035)	(0.112)	(0.020)	(0.026)	(0.048)	(0.041)	(0.026)	(0.046)	(0.021)
First midterm grade	0.044	0.071	0.042	0.058	0.121	0.021	0.009	0.138***	0.054	0.077**	0.072*	0.060*
	(0.064)	(0.052)	(0.063)	(0.057)	(0.110)	(0.033)	(0.031)	(0.049)	(0.048)	(0.033)	(0.041)	(0.035)
GPA year 1	0.090	0.172	0.060	0.110	0.138	0.085	0.044	0.351***	0.117	0.253***	0.227***	0.158***
	(0.085)	(0.114)	(0.093)	(0.122)	(0.164)	(0.073)	(0.052)	(0.096)	(0.084)	(0.069)	(0.084)	(0.060)
Failure rate year 1	-0.028*	-0.051**	-0.012	-0.040*	-0.038	-0.022	-0.005	-0.053***	-0.019	-0.029***	-0.046***	-0.010
	(0.016)	(0.023)	(0.019)	(0.023)	(0.034)	(0.013)	(0.008)	(0.017)	(0.014)	(0.011)	(0.016)	(0.010)
Labor market outcomes												
Employed 2 years after	0.013	0.001	-0.012	0.013	0.009	-0.003	0.025*	0.011	0.033**	0.003	0.025	0.016
	(0.019)	(0.016)	(0.019)	(0.015)	(0.021)	(0.016)	(0.013)	(0.019)	(0.015)	(0.014)	(0.017)	(0.012)
Log salary 2 years after	0.180*	0.283**	0.174*	0.179**	0.217***	0.130*	0.017	0.054	0.019	0.055	0.038	0.029
	(0.096)	(0.118)	(0.089)	(0.073)	(0.078)	(0.072)	(0.041)	(0.075)	(0.056)	(0.046)	(0.056)	(0.054)
Log salary 3 years after	0.025	0.211**	-0.004	0.177**	0.109	0.068	0.007	-0.009	-0.002	0.052	0.065	-0.018
	(0.065)	(0.083)	(0.049)	(0.073)	(0.071)	(0.054)	(0.044)	(0.061)	(0.047)	(0.049)	(0.044)	(0.049)
Public servant 3 years after	0.023	0.036	0.008	0.067**	0.068**	0.014	0.007	-0.036	0.017	-0.021	0.001	-0.010
	(0.026)	(0.023)	(0.023)	(0.027)	(0.033)	(0.017)	(0.017)	(0.028)	(0.020)	(0.018)	(0.023)	(0.016)
Public servant 5 years after	0.057	0.019	0.019	0.047*	0.066**	0.017	-0.005	0.006	-0.014	0.028	0.015	0.003
	(0.038)	(0.027)	(0.027)	(0.025)	(0.033)	(0.023)	(0.021)	(0.035)	(0.029)	(0.020)	(0.031)	(0.020)
Manager 3 years after	0.021	0.021**	0.016	0.023**	0.026**	0.014	0.012	0.029	0.006	0.021*	0.013	0.012
	(0.018)	(0.011)	(0.013)	(0.010)	(0.012)	(0.011)	(0.008)	(0.018)	(0.009)	(0.012)	(0.011)	(0.009)
Manager 5 years after	0.022	-0.000	0.000	0.011	0.013	0.000	0.027*	0.018	0.020*	0.023	0.005	0.032**
	(0.026)	(0.016)	(0.015)	(0.017)	(0.018)	(0.015)	(0.015)	(0.019)	(0.012)	(0.015)	(0.014)	(0.013)

Note: This table presents the fuzzy regression discontinuity (FRD) estimates of the ranking effect by type of program. The ranking effect derives from the discontinuity between classes in which the difference in median score is zero. 'Difficulty of the curriculum' is defined on the basis of the expected failure rate. 'Competition for admission' is defined on the basis of the program's median round 1 score. For both males and females, the sample of programs is split at the overall median. The sample includes candidates admitted for the first time, who are 21 years or less. FRDs and their relationships with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. and the bandwidth for entrance score is selected by using Calonico et al. (2014) procedure. Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

As regards individual characteristics, in Table 2.6, almost all the estimated effects are higher among those whose parents do not have a college degree and who choose their major for reasons other than market opportunities and prestige. The difference in those groups suggests that prior information plays a critical role in explaining the ranking effect, as stated in Proposition 3. If students are either better informed by their parents about their college experience or have a strong conviction about their career investment, they are less susceptible to their perceived rank. Otherwise, the class order will affect their academic performance, long-term occupation, and willingness to change careers. The only exception is the effect on management position for men, which is higher among those driven by market opportunities.

Table 2.6: Heterogeneity in the Ranking Effect by Individual Characteristics

	Males				**Females**			
	Parents with college degree		Career motivation		Parents with college degree		Career motivation	
	Both	Neither	Market	Other	Both	Neither	Market	Other
Academic outcomes								
Switched programs	0.023	-0.043	-0.010	-0.069**	0.006	-0.002	-0.009	-0.003
	(0.023)	(0.028)	(0.021)	(0.030)	(0.012)	(0.012)	(0.022)	(0.008)
Tried another vestibular	-0.033	-0.018	-0.014	-0.027	0.001	-0.014	-0.008	-0.012
	(0.041)	(0.028)	(0.024)	(0.026)	(0.020)	(0.015)	(0.032)	(0.011)
Graduated on time	-0.039	0.074	0.065	0.034	0.054	0.060	0.050	0.054**
	(0.050)	(0.065)	(0.077)	(0.036)	(0.039)	(0.039)	(0.158)	(0.023)
First midterm grade	-0.011	0.124	0.002	0.080	0.079*	0.083**	0.209**	0.047*
	(0.086)	(0.104)	(0.071)	(0.061)	(0.044)	(0.041)	(0.103)	(0.027)
GPA year 1	0.192	0.098	0.192	0.103	0.135*	0.268***	0.289*	0.214***
	(0.193)	(0.168)	(0.140)	(0.103)	(0.079)	(0.084)	(0.164)	(0.057)
Failure rate year 1	-0.025	-0.055*	-0.029	-0.035*	-0.021	-0.038***	-0.053*	-0.026***
	(0.027)	(0.031)	(0.024)	(0.019)	(0.014)	(0.014)	(0.029)	(0.009)
Labor market outcomes								
Employed 2 years after	-0.005	0.007	-0.022	0.012	-0.008	0.034**	0.027	0.012
	(0.022)	(0.021)	(0.027)	(0.015)	(0.019)	(0.015)	(0.027)	(0.012)
Log salary 2 years after	0.189	0.118	0.045	0.288***	0.013	0.108*	0.145	0.022
	(0.129)	(0.083)	(0.089)	(0.088)	(0.058)	(0.059)	(0.091)	(0.044)
Log salary 3 years after	0.038	0.086	-0.071	0.152**	-0.002	0.043	0.031	0.003
	(0.075)	(0.093)	(0.073)	(0.060)	(0.057)	(0.040)	(0.100)	(0.038)
Public servant 3 years after	0.044	0.088**	0.020	0.034*	-0.030	0.008	-0.006	-0.003
	(0.029)	(0.035)	(0.034)	(0.019)	(0.023)	(0.020)	(0.034)	(0.015)
Public servant 5 years after	0.021	0.082**	0.013	0.045**	0.008	0.012	-0.015	0.017
	(0.034)	(0.035)	(0.045)	(0.022)	(0.021)	(0.026)	(0.043)	(0.017)
Manager 3 years after	0.015	0.016*	0.055**	0.009	0.006	0.019**	0.011	0.018**
	(0.017)	(0.009)	(0.022)	(0.006)	(0.014)	(0.009)	(0.012)	(0.008)
Manager 5 years after	-0.021	0.009	0.025	-0.004	0.015	0.021	0.020	0.022*
	(0.020)	(0.016)	(0.027)	(0.012)	(0.017)	(0.013)	(0.017)	(0.012)

Note: This table presents the fuzzy regression discontinuity (FRD) estimates of the ranking effect by students characteristics. The ranking effect derives from the discontinuity between classes in which the difference in median score is zero. 'Career motivation' is split between market opportunities and prestige ('market') and other motives ('other'). The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs and their relationships with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. and the bandwidth for entrance score is selected by using Calonico et al. (2014) procedure. Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

2.7 Conclusion

Joining a better group of aspirants is not necessarily a better option for entering a chosen career. In a setting that we control for institutional aspects, such as teaching quality and reputation, we find that college students are more willing to change their major and less likely to graduate early if they are at the bottom of the better class. A lower rank is also found to reduce earnings at the start of their careers and the chance of getting a prime occupation.

This disruptive effect can be mitigated if the difference in peer quality between classes is high enough. For men, however, the ranking effect is so strong that going to the worst class is for most of them a better option. For women, on the other hand, the ranking effect is predominant only in some programs, where the difference between classes is small. On average, the small ranking effect on women's graduation is cancelled out by the peer effect.

In addition to institutional excellence and peer quality, the difference between the two groups, programs, or schools should also take perceived rank into account. The simple feeling of being at the bottom may undermine the benefit of joining more selective programs, which could in turn explain the dissenting findings in the literature.[24] Despite the distinct learning environment, top peers make bottom-ranked students underestimate their abilities and future returns in the chosen career. The discouragement in pursuing this career is not necessarily related to the risk of academic failure, but it is associated with parents' education and individual motivation. Students who are either better informed by their parents or who have a strong conviction about the value of their choice are less sensitive to the ranking effect.

[24] See, for instance, Dale & Krueger (2002b); Hoekstra (2009); Abdulkadiroğlu et al. (2014); Dobbie & Fryer Jr (2014); Zimmerman (2014); Dobbie & Fryer Jr (2014); Kirkeboen et al. (2016b).

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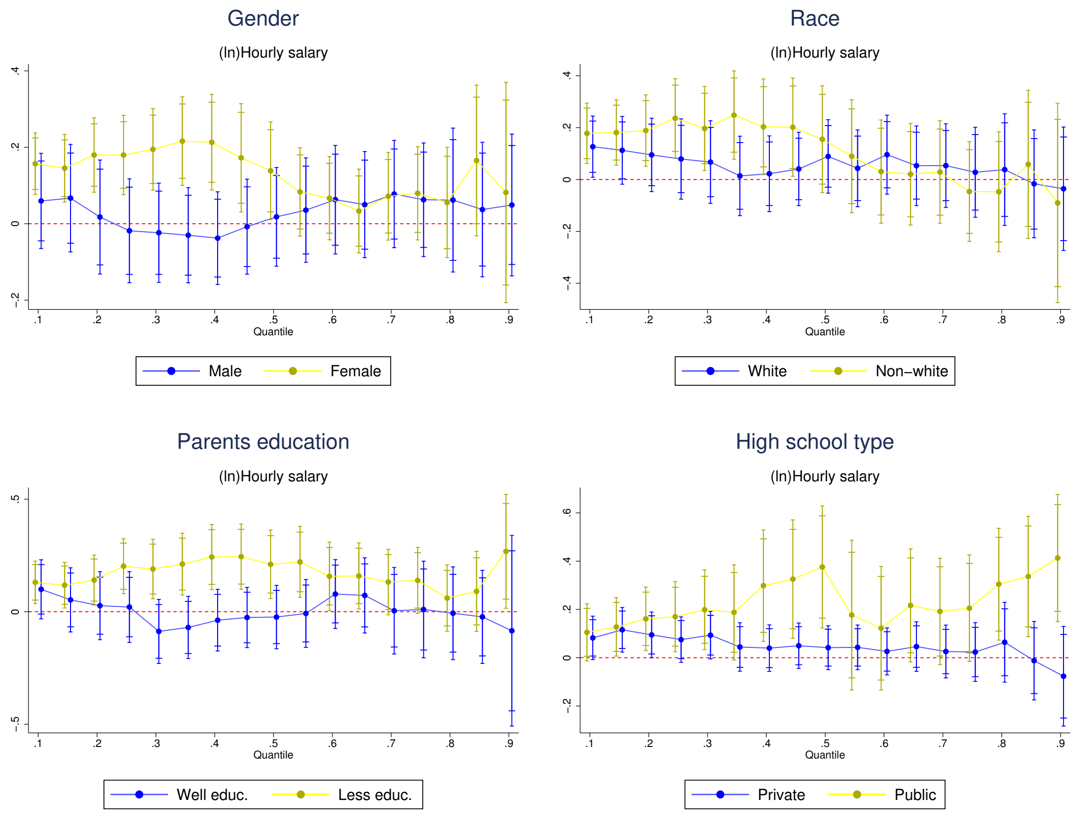
APPENDIX A

The Economic Effects of Free Elite Education

Figure A.1: Quantile Effects by Demographics



Notes: This figure illustrates the quantile fuzzy estimates of the flagship university effect on salaries by gender and race. Salaries outcomes are averages measured starting from the expected year of graduation of the competed program. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. All regressions are conditioned to employed candidates. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

Figure A.2: Quantile Effects by Fields of Study

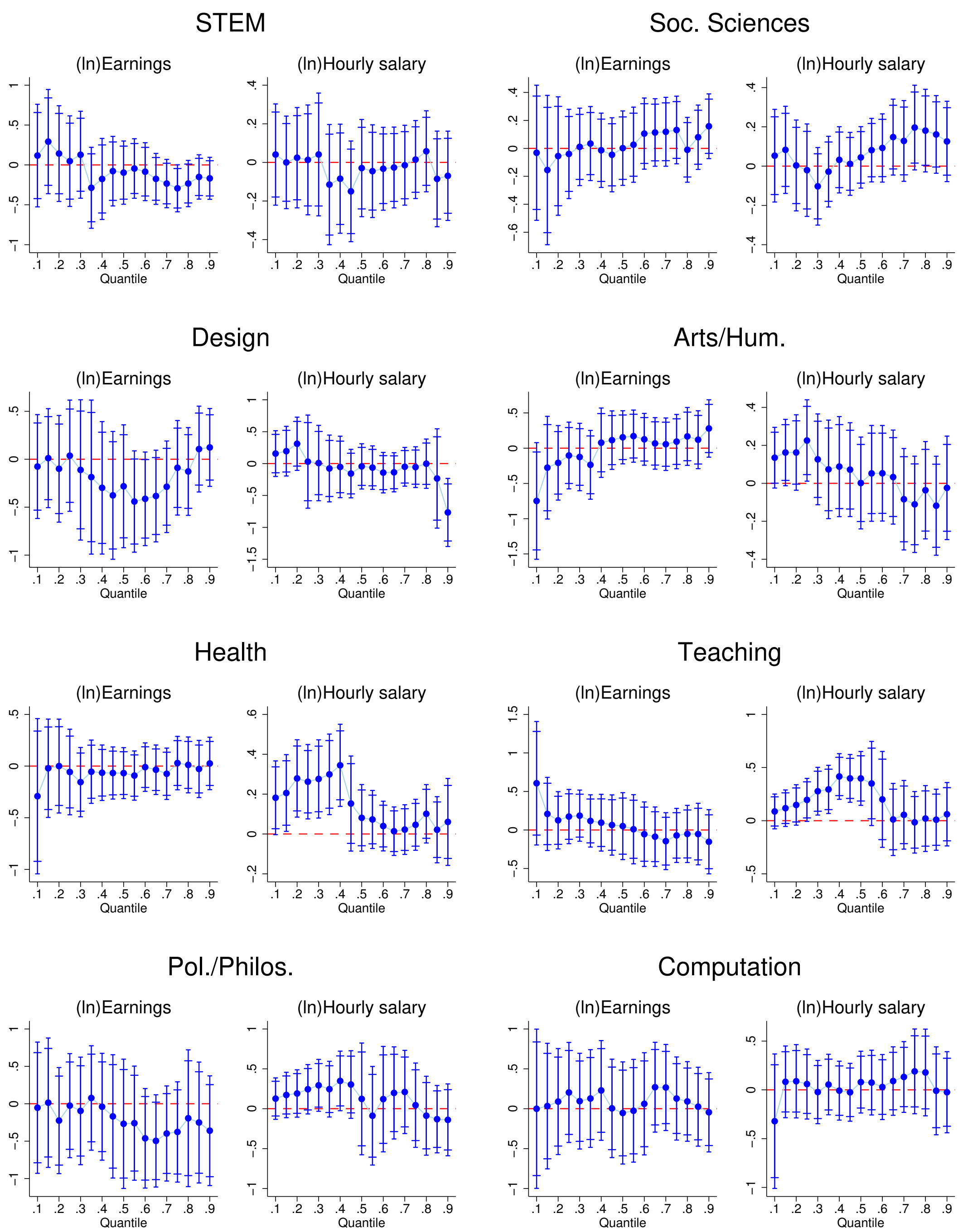


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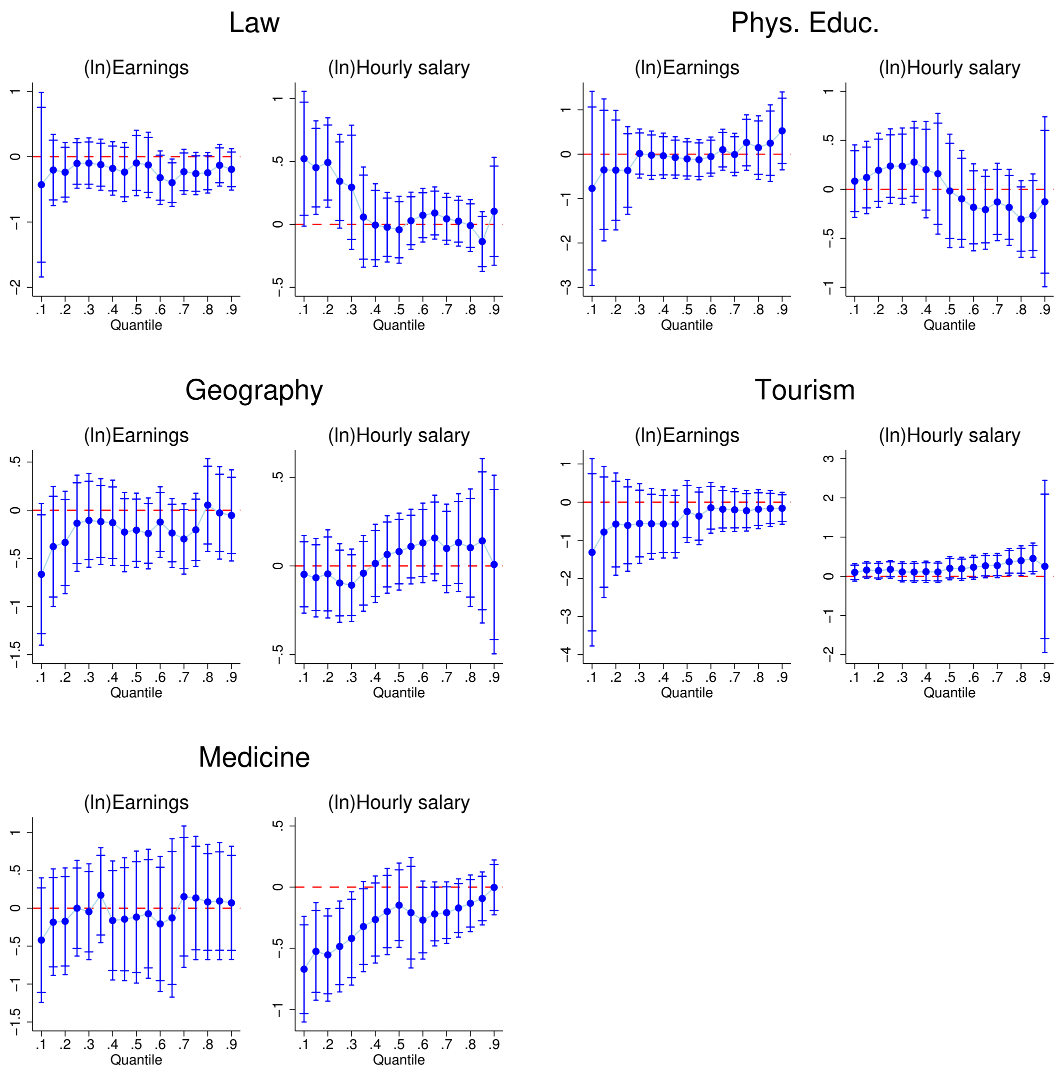
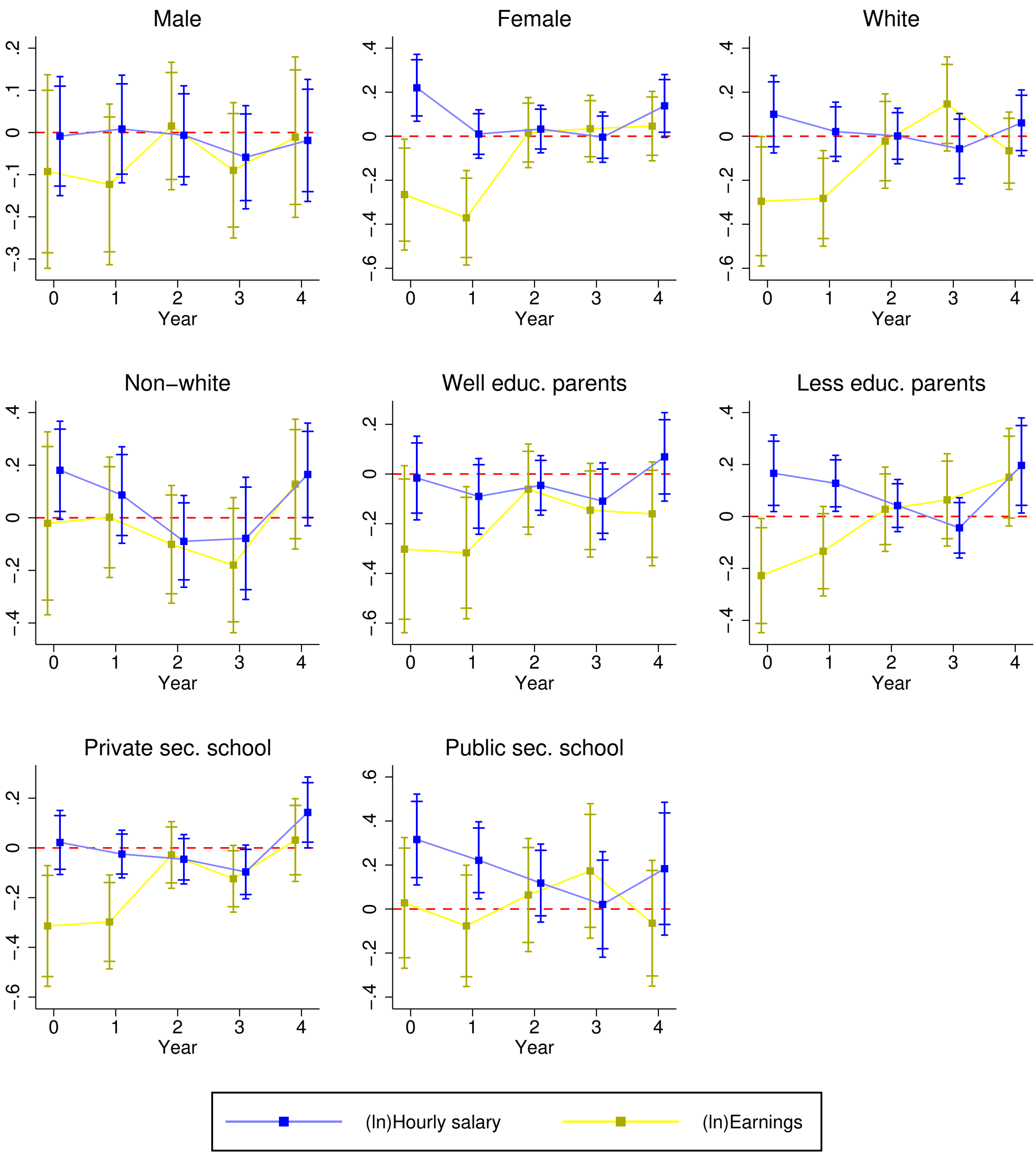
Figure A.2 – continued from previous page



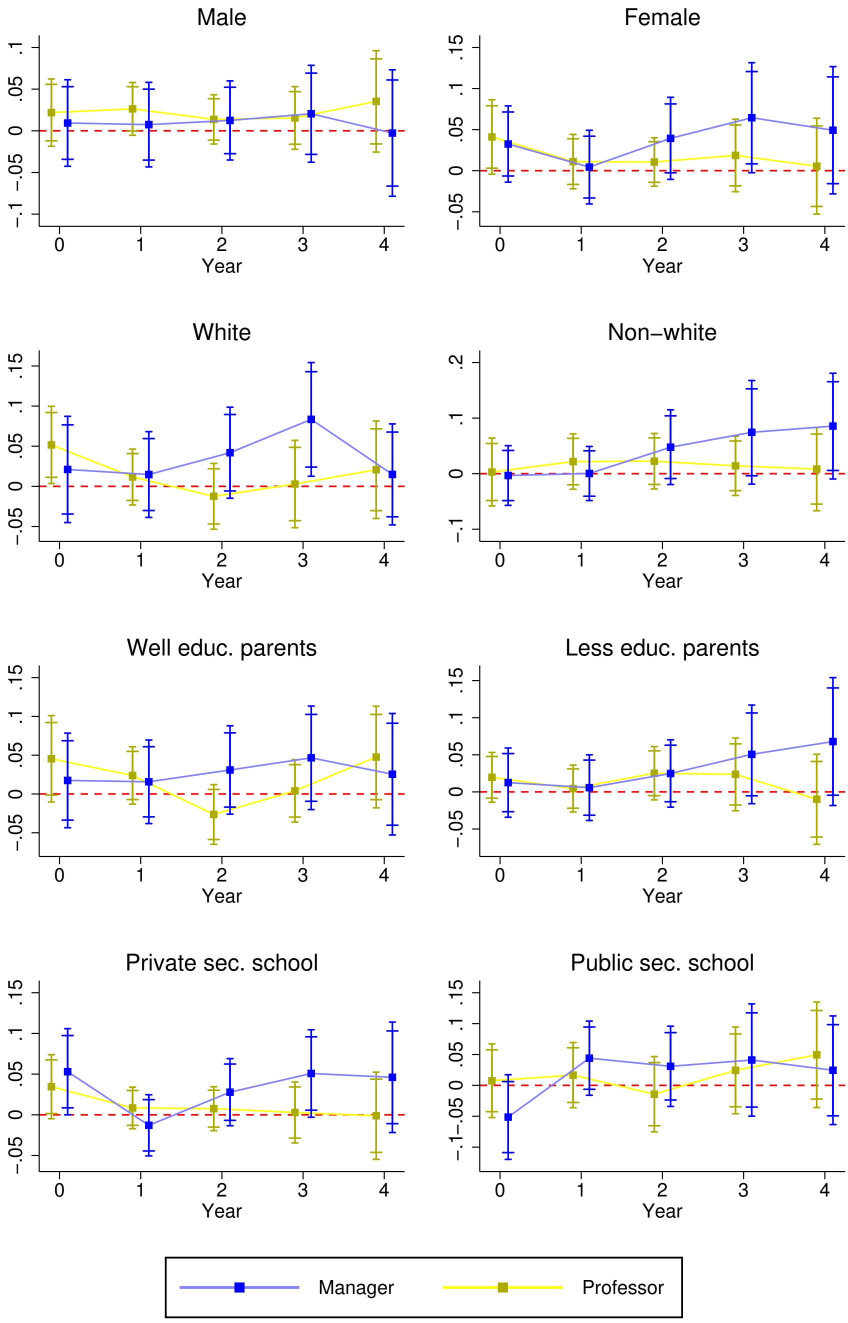
Notes: This figure illustrates the fuzzy regression discontinuity (FRD) estimates of the flagship university effect by fields of study. Salaries outcomes are averages measured starting from the expected year of graduation of the competed program. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. All regressions are conditioned to employed candidates. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

Figure A.3: Dynamics of Salaries by Demographics and Background



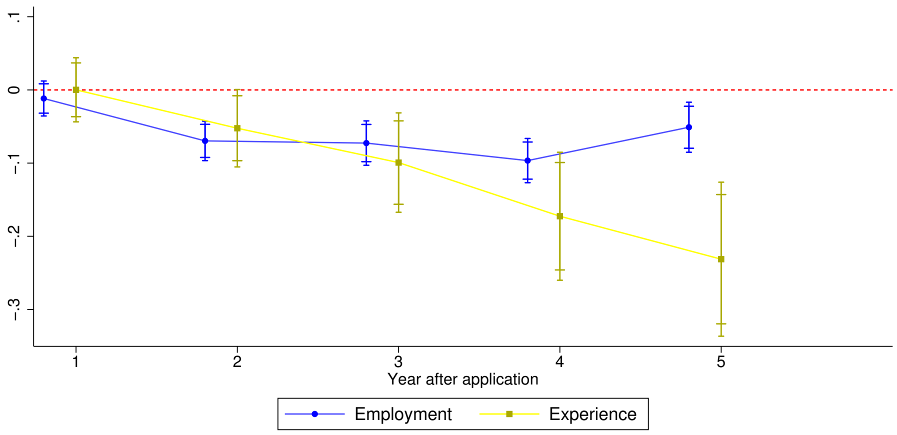
Notes: This figure presents the FRD estimates of the flagship university on salaries by each year of the expected graduation of the competed program. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. All regressions are conditioned to employed candidates. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

Figure A.4: Dynamics of Job Positions by Demographics and Background



Notes: This figure presents the FRD estimates of the flagship university on job positions by each year of the expected graduation of the competed program. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. All regressions are conditioned to employed candidates. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

Figure A.5: Employment and Experience in Years after Application



Notes: This figure presents the FRD estimates of the flagship university on employment and experience (years worked) in years after application. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. All regressions are conditioned to employed candidates. Vertical lines represent robust confidence interval at 90% and 95% levels. Sample restriction is described in Section 2.4.1. FRDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

Table A.1: Higher Education Institutions in Pernambuco

Institution	Rank	Type	# of programs	Teach. programs	Enrollees	Grad. stud.	Prop. PhD. professors	Institution	Rank	Type	# of programs	Teach. programs	Enrollees	Grad. stud.	Prop. PhD. professors
UFPE	46	**University**	**105**	**29**	**32,137**	**3,971**	**0.709**	FJN	1,286	College	13	1	5,626	786	0.151
UFRPE	100	University	47	16	11,572	1,182	0.747	FPDMB	1,306	College	9	6	1,143	239	0.075
UNIVASF	204	University	11	1	3,296	360	0.544	FCHPE	1,349	College	3	0	897	187	0.217
FNR	241	College	5	0	1,153	59	0.099	FACHO	1,367	College	6	2	1,412	245	0.162
FSH	285	College	3	0	1,509	205	0.250	FALUB	1,373	College	4	2	1,029	253	0.113
FACETEG	292	College	2	0	675	90	0.100	FAREC	1,375	College	8	0	1,290	305	0.047
FOCCA	356	College	8	1	1,888	190	0.211	IBRATEC	1,399	College	6	0	963	153	0.058
FACIPE	449	College	16	0	3,051	602	0.329	FADIC	1,536	College	4	0	1,011	116	0.408
FAINTVISA	453	College	18	5	2,580	489	0.185	FACESF	1,537	College	2	0	979	82	0.081
FASNE	465	College	4	1	759	71	0.174	FAFICA	1,545	College	11	5	1,123	350	0.086
IBGM / FGM	479	College	22	1	6,022	1,157	0.232	FACAPE	1,558	College	10	0	3,829	430	0.087
FACET	495	College	2	1	311	75	0.074	IESO	1,580	College	2	0	287	91	0.136
FBV	503	College	43	0	4,957	650	0.234	FAMA	1,601	College	3	0	386	154	0.094
FEPAM	529	College	1	0	104	15	0.087	FIS	1,624	College	9	0	2,122	281	0.076
FACIG	560	College	8	2	1,263	235	0.204	FACOTTUR	1,625	College	11	0	1,192	109	0.180
FIR	630	College	24	0	10,632	1,061	0.185	FAGA	1,715	College	3	0	505	100	0.093
UNIFAVIP	633	Univ. Center	30	0	8,825	1,016	0.180	IPESU	1,721	College	11	0	1,653	260	0.056
FG	649	College	40	1	11,003	1,686	0.117	ISEF	1,733	College	1	1	89	44	0.000
FAJOLCA	654	College	3	1	596	123	0.176	FATIN	1,768	College	2	0	634	160	0.111
FCHE	692	College	5	0	1,815	338	0.299	UNESJ	1,790	College	20	1	2,695	543	0.114
FMN Caruaru	703	College	12	1	2,737	241	0.107	ESSA	1,815	College	4	1	553	92	0.129
FACHUSST	707	College	1	0	173	42	0.278	CESA	1,816	College	7	6	1,149	306	0.099
UPE	712	University	56	24	14,313	1,631	0.463	ESM	1,819	College	2	0	296	43	0.103
UNICAP	737	University	37	9	9,805	1,464	0.440	FASUP	1,826	College	2	1	160	4	0.308
FAC. S. MIGUEL	779	College	18	1	3,247	170	0.233	FACEG	1,856	College	1	0	608	56	0.077
IFPE	792	Univ. Institute	17	7	2,798	262	0.239	FSM	1,917	College	2	0	12	0	0.167
IESP	796	College	1	0	45	33	0.000	CESVASF	1,918	College	8	8	892	94	0.022
FACOL	803	College	13	1	2,974	467	0.185	FAMASUL	1,926	College	6	6	929	309	0.075
FIBAM	829	College	14	0	1,579	231	0.200	FDG	1,928	College	1	0	1,054	144	0.081
FACCOR	831	College	1	0	74	18	0.111	ISES	1,958	College	1	1	64	24	0.091
FPS	839	College	6	0	1,837	244	0.221	FACIAGRA	1,967	College	2	0	255	59	0.043
ASCES	880	Univ. Center	17	1	4,425	673	0.288	FBJ	1,969	College	10	7	1,828	350	0.049
UNINASSAU	886	Univ. Center	42	0	21,292	2,170	0.197	ISEP	1,970	College	2	2	920	222	0.143
FAC. STA. EM.	981	College	7	1	854	144	0.135	UNESF	1,981	College	8	5	547	97	0.078
FAESC	995	College	6	2	1,006	208	0.205	FAFOPST	1,986	College	5	5	576	155	0.136
FOR	1,031	College	1	0	127	21	0.455	FACISST	2,011	College	1	0	234	54	0.148
FADIRE	1,088	College	3	0	623	239	0.094	FATEC	2,025	College	1	0	110	13	0.037
FAC. JOAQ. NAB.	1,101	College	4	2	378	31	0.286	FACIP	2,027	College	1	0	161	48	0.043
FCR	1,130	College	4	0	617	174	0.106	FACHUCA	2,036	College	4	2	768	70	0.060
FJN	1,143	College	11	1	3,322	464	0.176	FACHUSC	2,056	College	7	6	1,219	358	0.031
IF Sertão	1,176	Univ. Institute	12	7	1,724	140	0.225	FAFOPA	2,057	College	7	7	616	168	0.054
FASC	1,180	College	3	1	425	73	0.102	FACISA	2,083	College	2	0	567	119	0.032
FAFOPAI	1,241	College	4	4	551	149	0.036	FACAL	2,092	College	6	3	820	98	0.054
SENACPE	1,274	College	5	0	791	191	0.130	FACRUZ	2,102	College	1	0	44	11	0.095
FAFIRE	1,284	College	13	4	2,278	447	0.164	UNIVERSO	-	College	13	2	3,988	609	0.164

Note: This table shows the profile of higher education institutions in Pernambuco and their national rank position out of 2,132 institutions evaluated in 2016. In 2006, there were 78 higher education institutions in Pernambuco. Column “Grad. stud.” shows the total of graduating students in 2016.

Table A.2: All Regular Undergraduate Programs Offered by UFPE

Undergraduate Program	Sample	Exp. grad.	Area	Undergraduate program	Sample	Exp. grad.	Area
Accounting	✓	4	Social Sc.	Library Science	V	4	Arts/Hum.
Actuarial Science		4	Social Sc.	Linguistics and Literature	✓	4	Teaching
Archaeology	✓	4	Geography	Marine Engineering		4	STEM
Architecture	✓	5	Design	Marketing	V	4	Arts/Hum.
Audiophonology	V	4	Health	Materials Engineering		5	STEM
Audiovisual Communication	V	4	Arts/Hum.	Mathematics		4	STEM
Automation Engineering	✓	5	STEM	Mathematics Education	V	4	Teaching
Biology	✓	4	Health	Mathematics Education (CAA)	V	5	Teaching
Biology (CAV)	✓	5	Health	Mechanical Engineering	V	5	STEM
Biology - Medical Sciences	✓	4	Health	Media Communication	V	4	Arts/Hum.
Biology Education	✓	4	Teaching	Medicine	V	6	Medicine
Biomedical Engineering	✓	5	STEM	Mining Engineering	V	5	STEM
Biomedicine	✓	4	Health	Museology	✓	4	Geography
Business Administration	V	4	Social Sc.	Music (Instrument)		5	Arts/Hum.
Business Administration (CAA)	✓	4	Social Sc.	Music (Vocal)		5	Arts/Hum.
Cartographic Engineering	V	5	STEM	Music Education	V	5	Teaching
Chemical Engineering	✓	5	STEM	Nursing	V	5	Health
Chemistry		4	STEM	Nursing (CAV)	✓	4	Health
Chemistry Education	✓	4	Teaching	Nutrition	✓	4	Health
Chemistry Education (CAA)	✓	5	Teaching	Nutrition (CAV)	✓	4	Health
Civil Engineering	✓	5	STEM	Occupational Therapy	✓	4	Health
Civil Engineering (CAA)	✓	5	STEM	Oceanography	V	5	Geography
Computational Engineering	✓	5	STEM	Pedagogy	V	5	Teaching
Computational Science	✓	5	Computation	Pedagogy (CAA)		4	Teaching
Dance	✓	4	Arts/Hum.	Pharmacy	V	5	Health
Dental Medicine	✓	5	Health	Philosophy	V	4	Pol./Philos.
Design	✓	4	Design	Philosophy Education	V	4	Teaching
Design (CAA)	✓	4	Design	Physical Activity and Sports	V	4	Physical Ed.
Economics		4	Social Sc.	Physical Activity and Sports (CAV)		4	Physical Ed.
Economics (CAA)	✓	4	Social Sc.	Physical Education	V	4	Teaching
Electrical Engineering	✓	5	STEM	Physical Education (CAV)	V	4	Teaching
Electronics Engineering	✓	5	STEM	Physics	V	4	STEM
Energy Engineering	✓	5	STEM	Physics Education (CAA)	V	4	Teaching
Engineering	✓	5	STEM	Physics Education	✓	4	Teaching
Food Engineering	V	5	STEM	Physiotherapy	V	5	Health
Geography	✓	4	Geography	Political Science	✓	4	Pol./Philos.
Geography Education	✓	4	Teaching	Production Engineering	V	5	STEM
Geology	✓	4	Geography	Production Engineering (CAA)	V	5	STEM
Graphic Arts	✓	4	Arts/Hum.	Psychology	V	4	Health
History	✓	4	Geography	Public Health		4	Health
History Education	✓	4	Teaching	Secretarial Science	V	4	Arts/Hum.
Hotel Management	✓	4	Tourism	Sign Language Education	V	4	Teaching
Industrial Chemistry	✓	5	STEM	Social Sciences		5	Social Sc.
Information Management	✓	4	Social Sc.	Social Science Education	V	4	Teaching
Information Systems	✓	4	Computation	Social Service	✓	4	Pol./Philos.
Journalism	✓	4	Arts/Hum.	Statistics		4	STEM
Language Education (French)	✓	4	Teaching	Theatre	V	4	Arts/Hum.
Language Education (English)	✓	4	Teaching	Tourism Management	V	4	Tourism
Language Education (Spanish)	✓	4	Teaching	Visual Arts	V	4	Arts/Hum.
Law	V	5	Law				

Note: This table shows all undergraduate programs offered by UFPE and those included in the sample. It does not include special programs. "Expected graduation" is the number of years necessary to obtain the major degree. CAA and CAV are campi located in other cities.

Table A.3: Impact on Hourly Wages Controlling for Job Positions

	All sample with controls			**Manager**		**Public job**		**Neither**
	(1)	(2)	(3)	never	ever	never	ever	
(ln)Hourly salary	0.065*	0.059*	0.056*	0.061*	0.133	0.081*	0.032	0.066
	(0.034)	(0.033)	(0.033)	(0.036)	(0.106)	(0.044)	(0.056)	(0.043)
Manager FE	✓		✓					
Public servant FE		✓	✓					
Field FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: This tables shows the fuzzy estimates of the flagship university on labor outcomes. The treatment assignment is 1 if candidate enrolled in university, and 0 otherwise. Each column reports the estimate and standard error of a separate regression. Columns (1)-(3) include fixed effects in the RD estimation. The other columns split the sample according to the job position. Fuzzy regressions are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014), with standard errors clustered at the applicant level.

APPENDIX B

Can Good Peers Signal Less Success?

B.1 Proofs of Section 2.2

B.1.1 Proof of Proposition 1

From value function (2.2), given the initial choice for program k, the probability of switching programs is given by:

$$\begin{aligned}
\Pr(S) &= \Pr\left\{\theta V_i^{k'} \geq \theta v_i^k + \theta w^k \hat{p}_i^k \left[h_i^k\left(e_i^k, s_{-i(c)}\right)\right] - \gamma e_i^k\right\} \quad \text{(A.1)} \\
&= \Pr\left\{v_i^k \leq V_i^{k'} + \gamma e_i^k/\theta - w^k \hat{p}_i^k \left[h_i^k\left(e_i^k, s_{-i(c)}\right)\right]\right\} \\
&\propto \gamma e_i^k/\theta - w^k \hat{p}_i \left[h_i^k\left(e_i^k, s_{-i(c)}\right), s_{-i(c)}\right], \quad \text{(A.2)}
\end{aligned}$$

where e_i^k is given by the first-order condition:

$$w^k \frac{\partial \hat{p}_i}{\partial h}\left[h_i^k\left(e_i^k, s_{-i(c)}\right), s_{-i(c)}\right] \frac{\partial h_i^k}{\partial e}\left(e_i^k, s_{-i(c)}\right) - \frac{\gamma}{\theta} = 0. \quad \text{(A.3)}$$

Then differentiating (A.2) with respect to $s_{-i(c)}$ and with condition (A.3), we have:

$$\frac{\partial \Pr(S)}{\partial s_{-i(c)}} \propto -\frac{\partial \hat{p}_i}{\partial s_{-i(c)}} \left[h_i^k \left(e_i^k \right), s_{-i(c)} \right] - \frac{\partial \hat{p}_i}{\partial h} \left[h_i^k \left(e_i^k, s_{-i(c)} \right), s_{-i(c)} \right] \frac{\partial h_i^k}{\partial s_{-i(c)}} \left(e_i^k, s_{-i(c)} \right). \tag{A.4}$$

From Definition 3, $\partial \hat{p}_i / \partial s_{-i(c)}$ is a ranking effect and the first term on the RHS of (A.4) is non-negative. That is, an increase in peer skills should, if anything, reduce the subjective probability of finding a job in k and hence increase the probability of switching programs.

From Definition 2, $\partial h_i^k / \partial s_{-i(c)}$ is a peer effect and the second term on the RHS of (A.4) is non-positive. That is, an increase in peer skills should, if anything, increase human capital, which increases the subjective probability of finding a job in k and hence reduces the probability of switching programs.

The probability of dropping out of college, $\Pr(D)$, is also proportional to (A.2), so the same result applies to $\partial \Pr(D) / \partial s_{-i(c)}$.

B.1.2 Proof of Proposition 2

Given k, the second order condition for an optimal e_i^k is:

$$\partial_{ee} \hat{V}_i^k = \frac{\partial^2 \hat{p}_i}{\partial h^2} \left[h_i^k \left(e_i^k \right) \right] \cdot \left[\frac{\partial h_i^k}{\partial e} \left(e_i^k, s_{-i(c)} \right) \right]^2 + \frac{\partial \hat{p}_i}{\partial h} \left[h_i^k \left(e_i^k \right) \right] \cdot \frac{\partial^2 h_i^k}{\partial e^2} \left(e_i^k, s_{-i(c)} \right) < 0. \tag{A.5}$$

By differentiating (A.3) with respect to $s_{-i(c)}$, we have:

$$\begin{aligned} \frac{\partial e_i^k}{\partial s_{-i(c)}} &= \left(-\partial_{ee} \hat{V}_i^k \right)^{-1} \left[\frac{\partial h_i^k}{\partial e} \frac{\partial^2 \hat{p}_i}{\partial h \partial s_{-i(c)}} + \frac{\partial h_i^k}{\partial e} \frac{\partial^2 \hat{p}_i}{\partial h^2} \frac{\partial h_i^k}{\partial s_{-i(c)}} + \frac{\partial \hat{p}_i}{\partial h} \frac{\partial^2 h_i^k}{\partial e \partial s_{-i(c)}} \right] \\ &\propto \frac{\partial h_i^k}{\partial e} \frac{\partial^2 \hat{p}_i}{\partial h \partial s_{-i(c)}} + \frac{\partial h_i^k}{\partial e} \frac{\partial^2 \hat{p}_i}{\partial h^2} \frac{\partial h_i^k}{\partial s_{-i(c)}} + \frac{\partial \hat{p}_i}{\partial h} \frac{\partial^2 h_i^k}{\partial e \partial s_{-i(c)}}. \end{aligned} \tag{A.6}$$

From Definition 3, $\partial^2 \hat{p}_i / \partial h \partial s_{-i(c)}$ is a ranking effect, which is non-positive if the career is highly competitive (Definition 1). Thus, the first term on the RHS of (A.6) is non-positive — i.e., an increase in peer skills should, if anything, reduce the perceived return of human capital and hence reduce effort. In less competitive careers, $\partial^2 \hat{p}_i / \partial h \partial s_{-i(c)} \geq 0$ and the implied effect on effort is non-negative.

From Definition 2, $\partial h_i^k / \partial s_{-i(c)}$ and $\partial^2 h_i^k / \partial e \partial s_{-i(c)}$ are peer effects. If the career is highly competitive (Definition 1), then $\partial^2 \hat{p}_i / \partial h^2 \geq 0$ and the second term on the RHS of (A.6) is non-negative. That is, an increase in peer skills should increase human capital and, if anything, its perceived return and hence increase effort. In less competitive careers, $\partial^2 \hat{p}_i / \partial h^2 \leq 0$ and the implied effect on effort is non-positive — i.e., peer quality substitutes effort. As long as a higher peer quality improves learning, $\partial^2 h_i^k / \partial e \partial s_{-i(c)} > 0$, then the third term is positive.

For the same student, the effect of $s_{-i(c)}$ on the true expected salary is:

$$\begin{aligned}
\frac{\partial E\left(w_i^k\right)}{\partial s_{-i(c)}} \left(e_i^k, s_{-i(c)}\right) &= w^k \frac{\partial p}{\partial h} \left(\frac{\partial h_i^k}{\partial e} \frac{\partial e_i^k}{\partial s_{-i(c)}} + \frac{\partial h_i^k}{\partial s_{-i(c)}} \right) \\
&\propto \left(\frac{\partial h_i^k}{\partial e} \right)^2 \frac{\partial^2 \hat{p}_i}{\partial h \partial s_{-i(c)}} - \frac{\partial^2 h_i^k}{\partial e^2} \frac{\partial \hat{p}_i}{\partial h} \frac{\partial h_i^k}{\partial s_{-i(c)}} + \frac{\partial h_i^k}{\partial e} \frac{\partial \hat{p}_i}{\partial h} \frac{\partial^2 h_i^k}{\partial e \partial s_{-i(c)}} \qquad \text{(A.7)}
\end{aligned}$$

The second and third terms on the RHS of (A.7), representing the peer effect, are positive as long as it exists. The first term, representing the ranking effect, is negative only in highly competitive careers. Therefore, the peer quality can make students in class 1 better off due to the peer effect, but it can also make them worse off due to the ranking effect.

B.1.3 Proof of Proposition 3

Note that

$$\frac{\partial \hat{p}_i}{\partial s_{-i(c)}} = \frac{\partial \hat{p}_i}{\partial \hat{F}_i^{k-1}} \frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}},$$

and the relationship between $\hat{F}_i^{k-1}$ and $s_{-i(c)}$ depends on I_i, so that:

$$\left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}} \right)^{-1} \cdot \frac{\partial}{\partial I_i} \left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}} \right) \leq 0.$$

That is, the larger the information set I_i, the lower the adjustment in $\hat{F}_i^{k-1}$ given $s_{-i(c)}$.

Therefore, the relative adjustment in $\hat{p}_i$ given $s_{-i(c)}$ is:

$$\begin{aligned}
\left(\frac{\partial \hat{p}_i}{\partial s_{-i(c)}} \right)^{-1} \cdot \frac{\partial}{\partial I_i} \left(\frac{\partial \hat{p}_i}{\partial s_{-i(c)}} \right) &= \left(\frac{\partial \hat{p}_i}{\partial \hat{F}_i^{k-1}} \frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}} \right)^{-1} \cdot \frac{\partial \hat{p}_i}{\partial \hat{F}_i^{k-1}} \frac{\partial}{\partial I_i} \left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}} \right) \\
&= \left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}} \right)^{-1} \cdot \frac{\partial}{\partial I_i} \left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}} \right) \leq 0.
\end{aligned}$$

Similarly,

$$\left(\frac{\partial^2 \hat{p}_i}{\partial h \partial s_{-i(c)}}\right)^{-1} \cdot \frac{\partial}{\partial I_i}\left(\frac{\partial^2 \hat{p}_i}{\partial h \partial s_{-i(c)}}\right) = \left(\frac{\partial^2 \hat{p}_i}{\partial h \partial \hat{F}_i^{k-1}} \frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}}\right)^{-1} \cdot \frac{\partial^2 \hat{p}_i}{\partial h \partial \hat{F}_i^{k-1}} \frac{\partial}{\partial I_i}\left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}}\right)$$
$$= \left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}}\right)^{-1} \cdot \frac{\partial}{\partial I_i}\left(\frac{\partial \hat{F}_i^{k-1}}{\partial s_{-i(c)}}\right) \leq 0.$$

Function $h_i^k(.)$ does not depend on $\hat{F}_i^{k-1}$, but it depends directly on $s_{-i(c)}$.

B.2 Instructor Quality

Let y_{isp} be the performance of student i in course s, taught by instructor p. Let N_s be the set of students who took course s. The first step is to subtract the average outcome per course from the student's observed performance:

$$\hat{y}_{ip}(s) = y_{isp} - \frac{\sum_i 1(i \in N_s) \cdot y_{isp}}{\sum_i 1(i \in N_s)} \qquad \text{for all } s = 1, ..., S. \tag{B.1}$$

The second step is to calculate the student fixed-effect by averaging $\hat{y}_{ip}(s)$ per student:

$$\hat{\mu}_i = \frac{\sum_s 1(i \in N_s) \cdot \hat{y}_{ip}(s)}{\sum_s 1(i \in N_s)}. \tag{B.2}$$

Let $N_{s,p}$ be the subset of students who attended course s with instructor p. Then the instructor fixed-effect is given by:

$$\hat{\gamma}_p = \frac{\sum_s \sum_i 1(i \in N_{s,p}) \cdot \left[\hat{y}_{ip}(s) - \hat{\mu}_i\right]}{\sum_s \sum_i 1(i \in N_{s,p})}. \tag{B.3}$$

B.3 Estimation Procedure

Set $Y = [y_1 \ \ldots \ y_n]'$, $C = \left[\mathbf{1}(c_1 = 1) \ \ldots \ \mathbf{1}(c_n = 1)\right]'$, $R = [r_1 \ \ldots \ r_n]'$, and $X = \left[(1, x_1 - \underline{x}) \ \ldots (1, x_n - \underline{x})\right]'$, where n is the number of observations. Also set $W_- = \text{diag}\big(\mathbf{1}(x_1 < \underline{x})\, k_1, \ldots, \mathbf{1}(x_n < \underline{x})\, k_n\big)$ and $W_+ = \text{diag}\big(\mathbf{1}(x_1 \geq \underline{x})\, k_1, \ldots, \mathbf{1}(x_n \geq \underline{x})\, k_n\big)$, where diag(.) denotes a diagonal matrix and $k_i = \max\Big[0, \big(1 - |x_i - \underline{x}| / b\big)\Big]$ is a triangular kernel weight, with a chosen bandwidth b.

To estimate the standard fuzzy RD, we first apply the following locally weighted regression (LWR) estimator on each side of the cutoff:

$$\hat{\mu}_{-}^{z} = (1 \;\; 0)\left(X'W_{-}X\right)^{-1} X'W_{-}Z,$$

$$\hat{\mu}_{+}^{z} = (1 \;\; 0)\left(X'W_{+}X\right)^{-1} X'W_{+}Z.$$

Then the estimator for the net effect of the first class, equation (2.5), is:

$$\Delta y \widehat{\Delta y} = \frac{\hat{\mu}_{+}^{y} - \hat{\mu}_{-}^{y} - \hat{B}^{y}\left(b, b^{*}\right)}{\hat{\mu}_{+}^{c} - \hat{\mu}_{-}^{c} - \hat{B}^{c}\left(b, b^{*}\right)}, \tag{C.1}$$

and the estimator for the net (naive) ranking effect, equation (2.6), is

$$\frac{\Delta y \widehat{\Delta y}}{\Delta r \widehat{\Delta r}} = \frac{\hat{\mu}_{+}^{y} - \hat{\mu}_{-}^{y} - \hat{B}^{y}\left(b, b^{*}\right)}{\hat{\mu}_{+}^{r} - \hat{\mu}_{-}^{r} - \hat{B}^{r}\left(b, b^{*}\right)}, \tag{C.2}$$

where b is the optimal main bandwidth and b^{*} is the optimal pilot bandwidth. The bias estimator, $\hat{B}^{z}(.)$, adjusts the LWR estimates for a large, MSE-optimal bandwidth. See **?** for details of the bias correction and robust variance for estimators (C.1) and (C.2).

To estimate the RD conditional on Δq, first we set $XQ = \left[(1, x_1 - \underline{x}, \Delta q_1) \; \ldots \; (1, x_n - \underline{x}, \Delta q_n)\right]'$ and $V_{-}^{u} = \mathrm{diag}\big(\mathbf{1}(x_1 < \underline{x})\, k_1 h_1^{u}, \ldots, \mathbf{1}(x_n < \underline{x})\, k_n h_n^{u}\big)$ and $V_{+}^{u} = \mathrm{diag}\big(\mathbf{1}(x_1 \geq \underline{x})\, k_1 h_1^{u}, \ldots, \mathbf{1}(x_n \geq \underline{x})\, k_n h_n^{u}\big)$, where $h_i^{u} = \max\Big[0, \big(1 - \left|\Delta q_i - u\right| / d\big)\Big]$ is a triangular kernel weight, with a chosen bandwidth d. Then, for a chosen value u, we apply the following LWR estimator:

$$\hat{\eta}_{-}^{z}(u) = (1 \;\; 0 \;\; u)\left(XQ'V_{-}^{u}XQ\right)^{-1} XQ'V_{-}^{u}Z,$$

$$\hat{\eta}_{+}^{z}(u) = (1 \;\; 0 \;\; u)\left(XQ'V_{+}^{u}XQ\right)^{-1} XQ'V_{+}^{u}Z.$$

Hence, the estimator for the marginal ranking effect, equation (2.7), is:

$$\frac{\Delta y \widehat{\Delta y}(\Delta q = 0)}{\Delta r \widehat{\Delta r}(\Delta q = 0)} = \frac{\hat{\eta}_{+}^{y}(0) - \hat{\eta}_{-}^{y}(0) - \hat{B}^{y}\left(0, b, b^{*}\right)}{\hat{\eta}_{+}^{r}(0) - \hat{\eta}_{-}^{r}(0) - \hat{B}^{r}\left(0, b, b^{*}\right)}, \tag{C.3}$$

and the estimator for Δy as a function of Δq is:

$$\Delta y \widehat{\Delta y}(\Delta q = u) = \frac{\hat{\eta}_{+}^{y}(u) - \hat{\eta}_{-}^{y}(u) - \hat{B}^{y}\left(u, b, b^{*}\right)}{\hat{\eta}_{+}^{c}(u) - \hat{\eta}_{-}^{c}(u) - \hat{B}^{c}\left(u, b, b^{*}\right)}. \tag{C.4}$$

Given an arbitrary bandwidth d for the difference in peer quality, bandwidths b and

b^* are calculated using the following MSE-optimal estimators:

$$b = \left[\frac{\hat{V}_1}{4\hat{B}_1^2 + \hat{R}_1}\right]^{1/5} n^{-1/5} \quad \text{and} \quad b^* = \left[\frac{5\hat{V}_2}{2\hat{B}_2^2 + \hat{R}_2}\right]^{1/5} n^{-1/5}, \tag{C.5}$$

where for $q = 1, 2$, $\hat{V}_q = \mathcal{V}_q(\hat{\eta}_+^y) + \mathcal{V}_q(\hat{\eta}_-^y)$, $\hat{B}_q = \mathcal{B}_q(\hat{\eta}_+^y) - \mathcal{B}_q(\hat{\eta}_-^y)$, and $\hat{R}_q = \mathcal{R}_q(\hat{\eta}_+^y) + \mathcal{R}_q(\hat{\eta}_-^y)$. Functions $\mathcal{V}_q(.)$, $\mathcal{B}_q(.)$ and $\mathcal{R}_q(.)$ are specified by ?.

B.4 The Effect of a Delayed Start

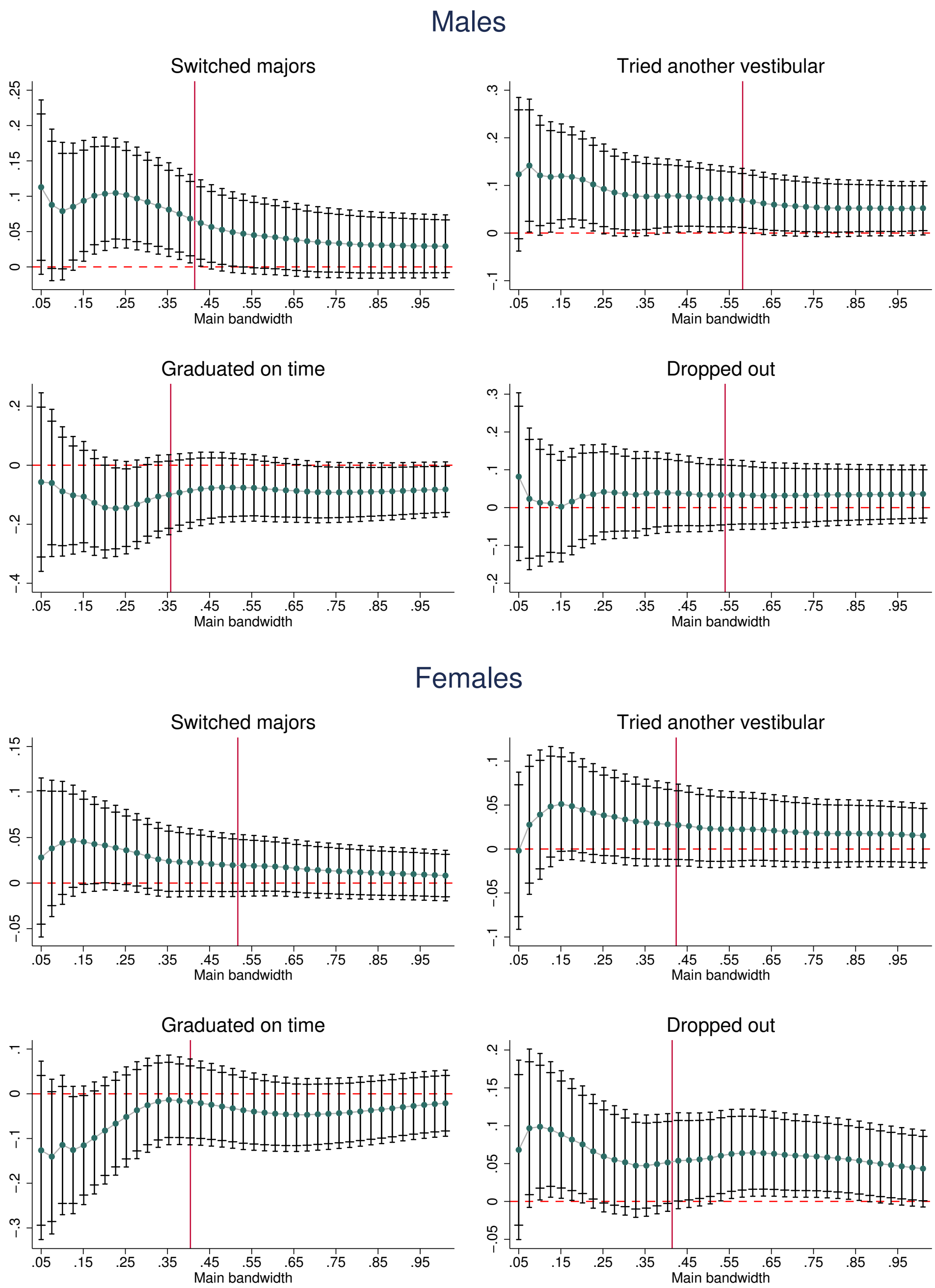
The class assignment at UFPE is also responsible for a five-month delayed start for students in the second class, which could explain our findings. In our design, the estimated ranking effect is for students who want to join the first class and the identification is possible because some of them are not able to. Hence, we ask what if first-class students had to delay their start, but without changing their rank or peer quality.

To answer this question, we exploit a strike in 2005 that caught all prospective students by surprise. This strike started after the 2006 cohort had applied to UFPE — so they could not have changed their preferred class — and delayed their initial courses by five months. By comparing the last student in the first class in 2006 and in cohorts that were not affected by strikes,[1] we mimic the effect of a delayed start in our design. In practice, we estimate the relationship between each outcome and the entrance score on the right side of the cutoff for the two types of cohort. Then we compare the predicted values for the last student in the first class. Table 1.8 presents our estimates.

These estimates show that the strike reduced the grades of male students and made them more likely to try another *vestibular* and drop out of the institution. For females, all differences are small and not significant, except for the number of courses taken in the first semester. Overall, these findings indicate that the enforced delay had, if anything, a negative effect on a student's commitment to the program, which is the opposite of the ranking effect that we find above. In fact, the effect of ranking on academic performance and dropout may be underestimated due to the enforced delay in our design.

[1] Namely, 2008 for 2+ year outcomes, plus 2009 and 2010 for 1-year outcomes and 2004, 2005 and 2011 for 1-semester outcomes.

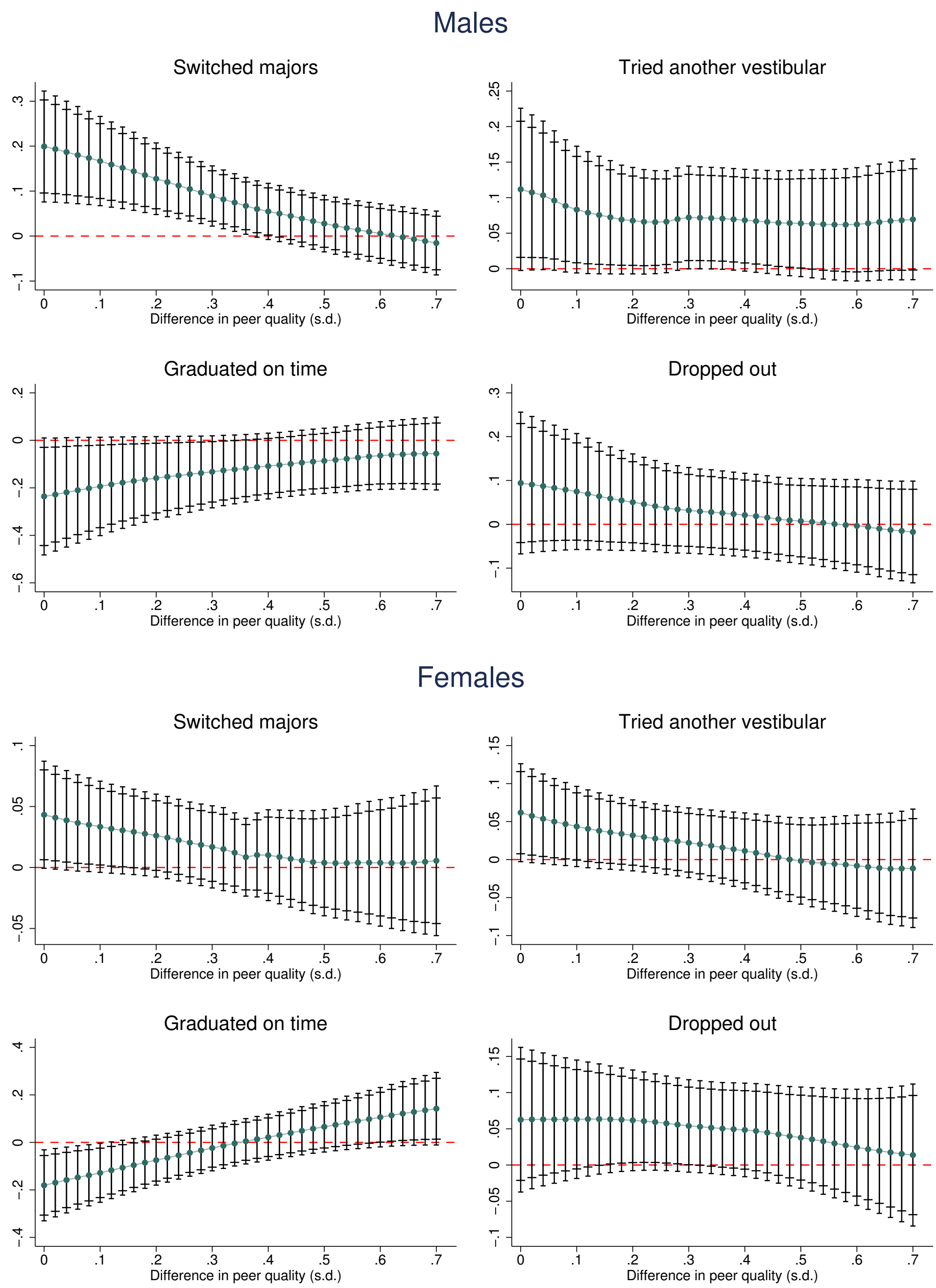
Figure B1: Net Effect of First Class Using Different Bandwidths



Notes: This figure presents estimates of the first-class effect using different bandwidths. The vertical bars represent the robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. Functions are estimated using triangular kernel. The vertical line indicates the main bandwidth obtained with the procedure proposed by Calonico et al. (2014).

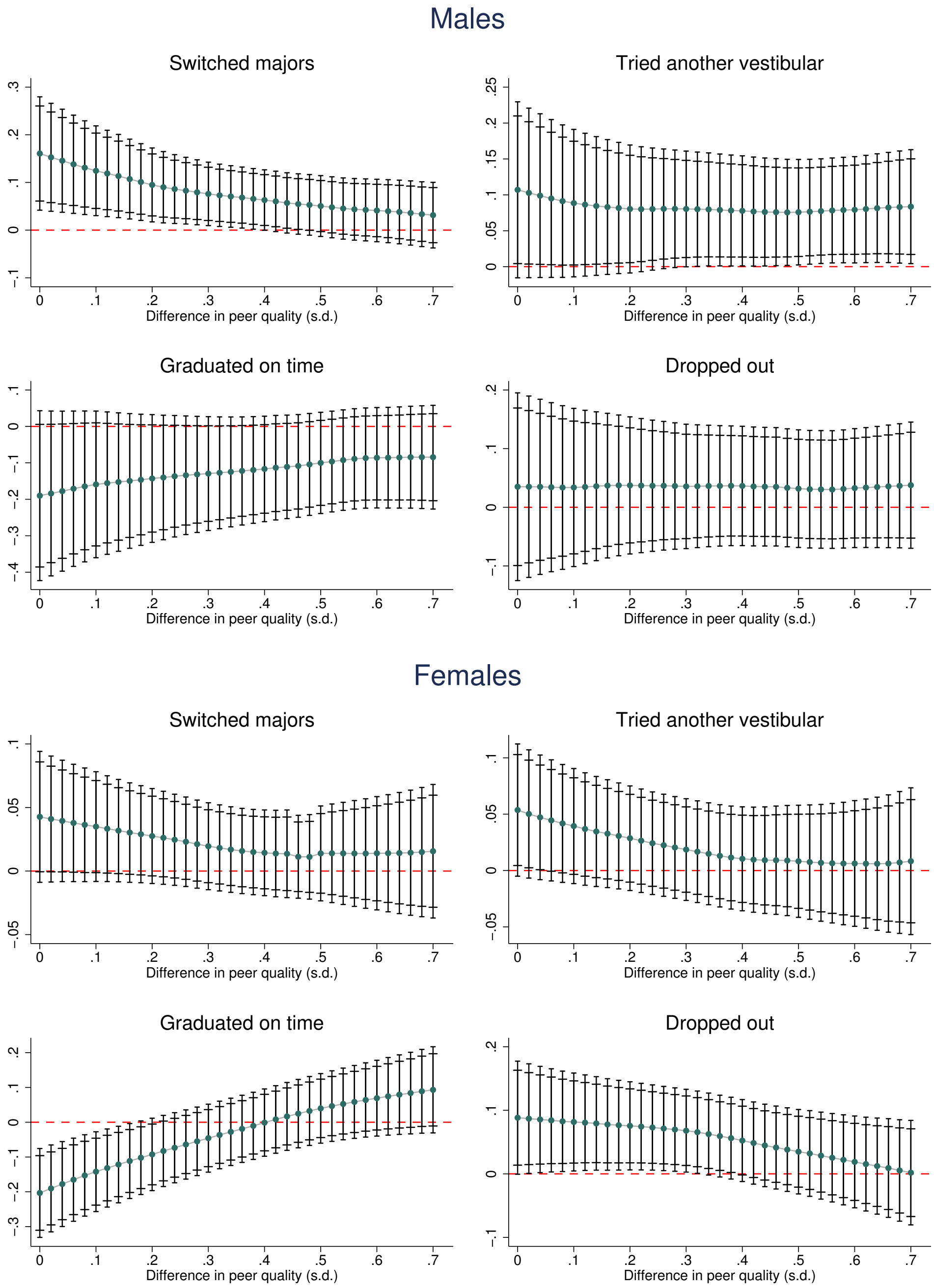
Figure B2: Net Effect of First Class by Difference in Other Percentiles of Peer Scores

a) Difference in the 20th percentile



(continuing)

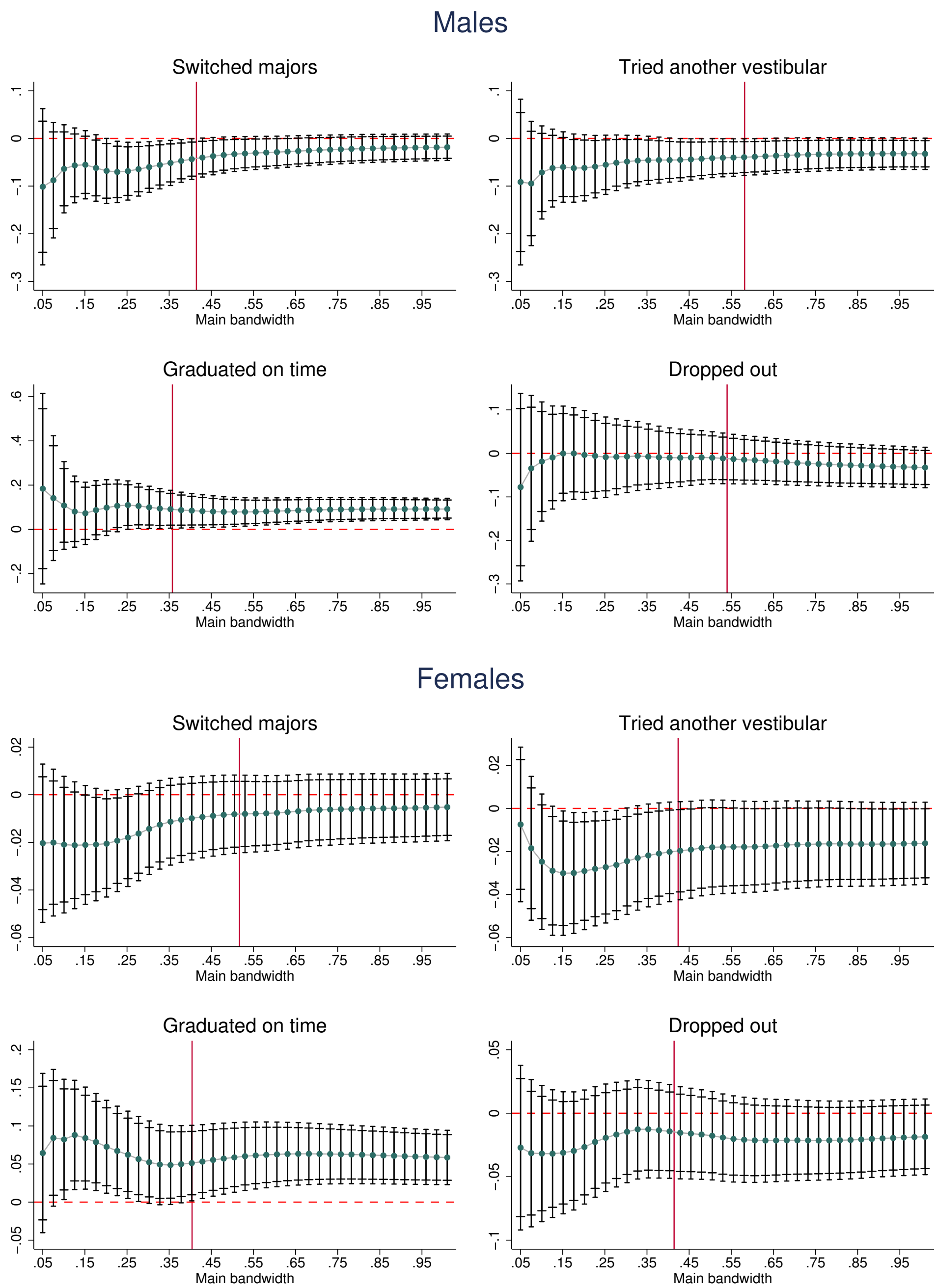
b) Difference in the 80th percentile



Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the first-class effect as a function of differences in the 20th percentile (panel a) and 80th percentile of round 1 scores (panel b). The vertical bars represent robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs and their relationships with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. and the bandwidth for entrance score is selected by using Calonico et al. (2014) procedure.

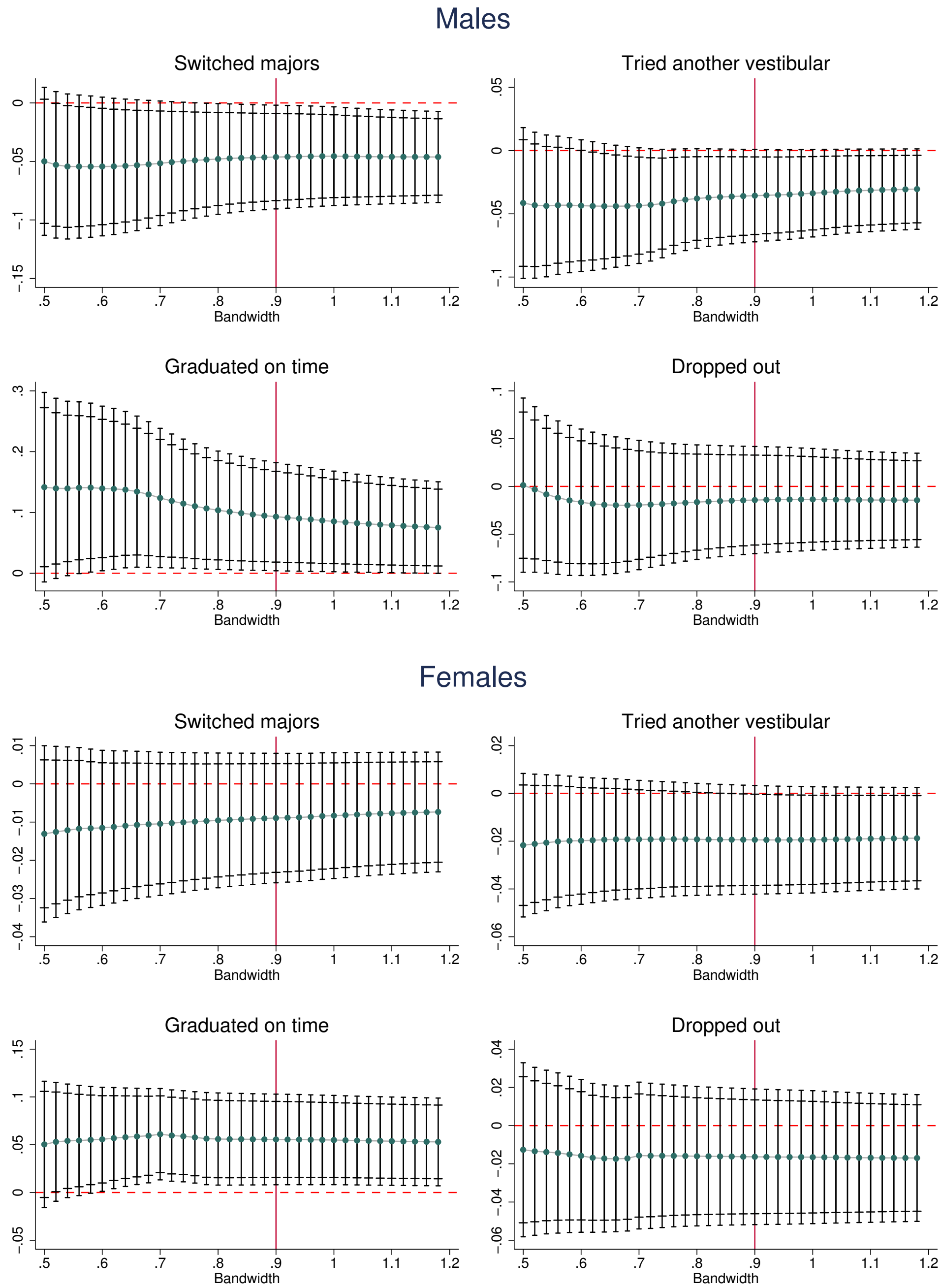
Figure B3: Ranking Effect on Major Switching and Graduation Using Different Bandwidths

a) Bandwidths for entrance score



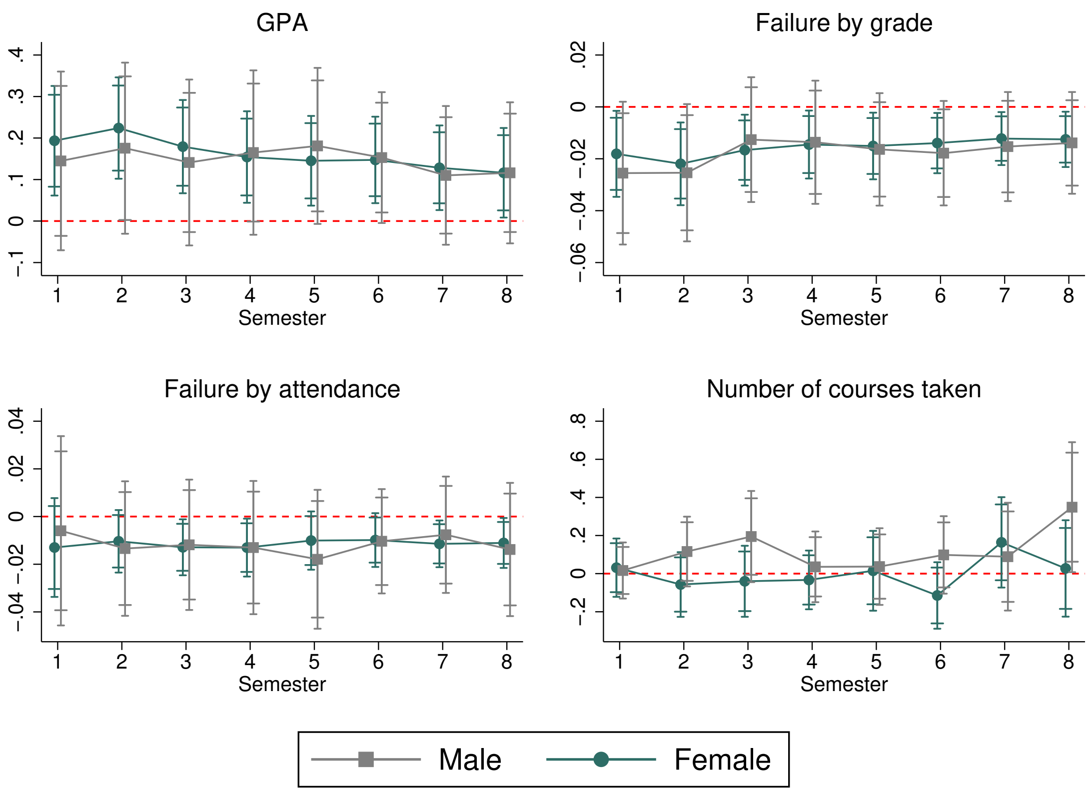
(continuing)

b) Bandwidths for difference in peer quality



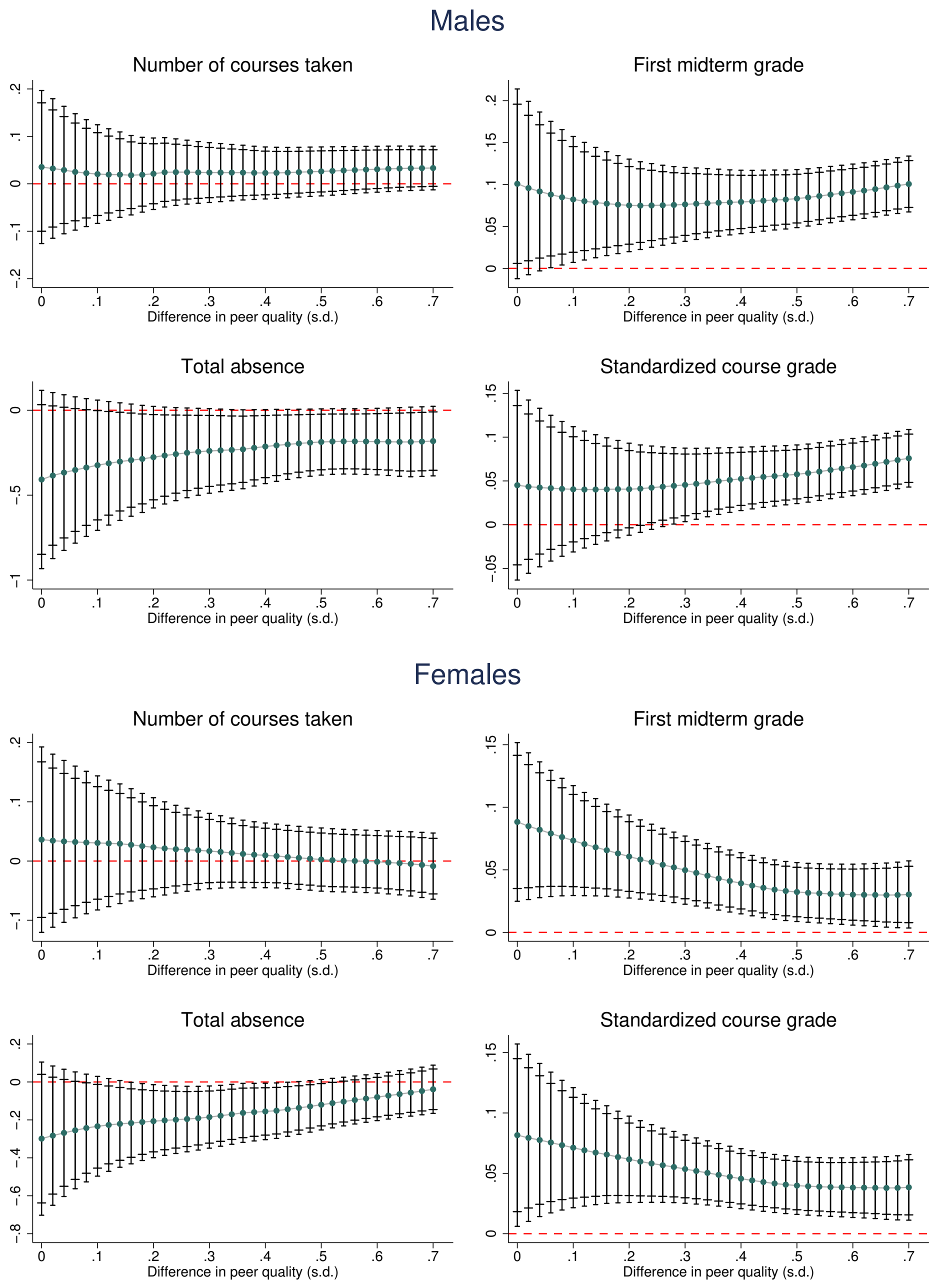
Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the ranking effect using different bandwidths for the entrance score (panel a) and difference in peer quality (panel b). The vertical bars represent robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs and their relationships with peer quality are estimated using triangular kernels. In panel (a), the vertical line indicates the main bandwidth obtained with the procedure proposed by Calonico et al. (2014). In panel (b), the vertical line indicates the bandwidth used in the main findings.

Figure B4: Ranking Effects on Academic Performance per Semester



Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the ranking effect on academic performance up to each semester. The ranking effect derives from the discontinuity between classes in which the difference in median score is zero. The vertical bars represent the robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs and their relationship with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. and the bandwidth for entrance score is selected by using Calonico et al. (2014) procedure.

Figure B5: Ranking Effect on Performance in the 1st Semester by Difference in Peer Quality



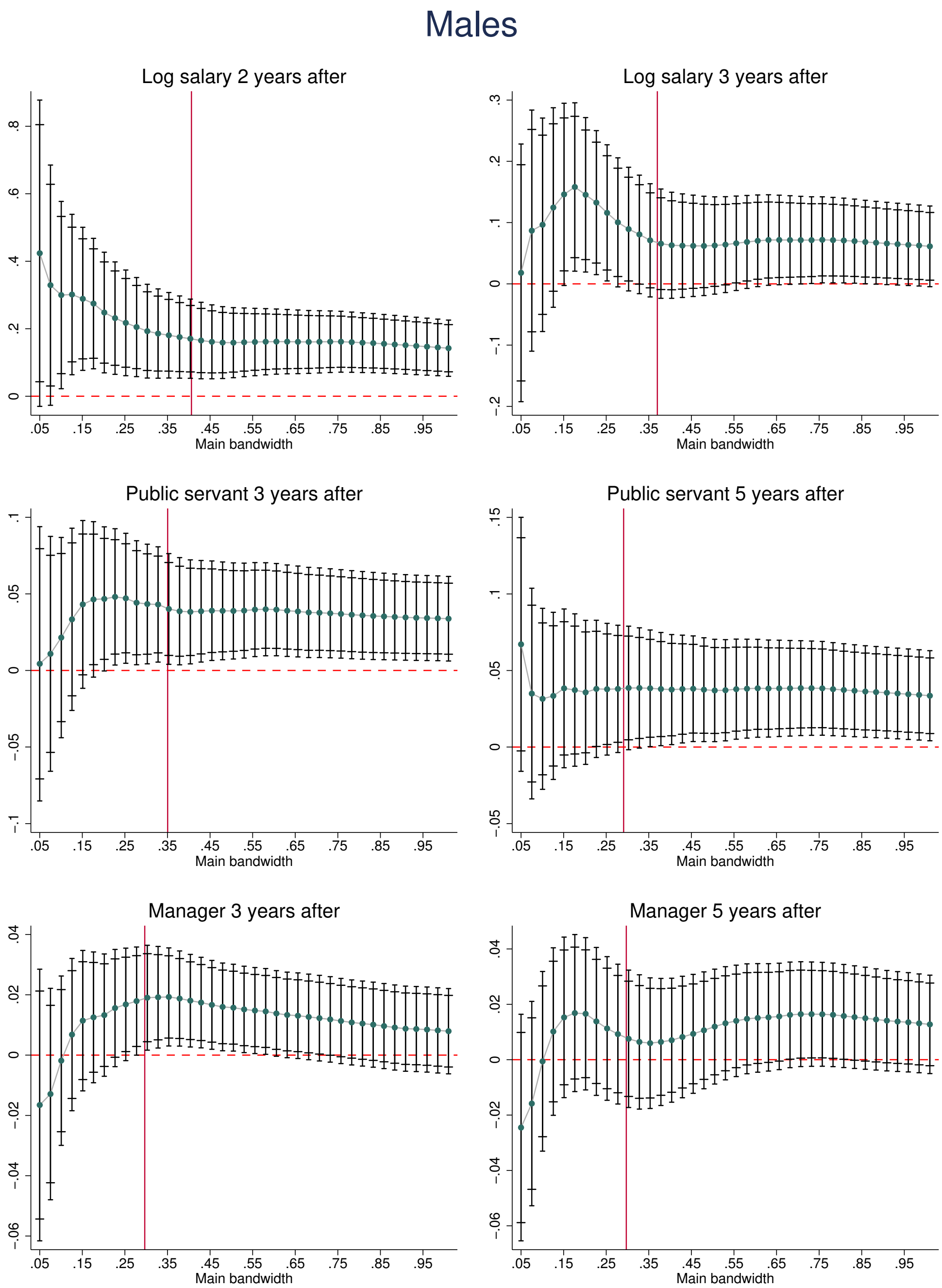
Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the net ranking effect as a function of differences in the median peer's round 1 score (peer quality). The vertical bars represent the robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FDRs and their relationships with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. and the bandwidth for entrance score is selected based on Calonico et al. (2014) procedure.

Figure B6: Quantile Effect of First Class on Earnings

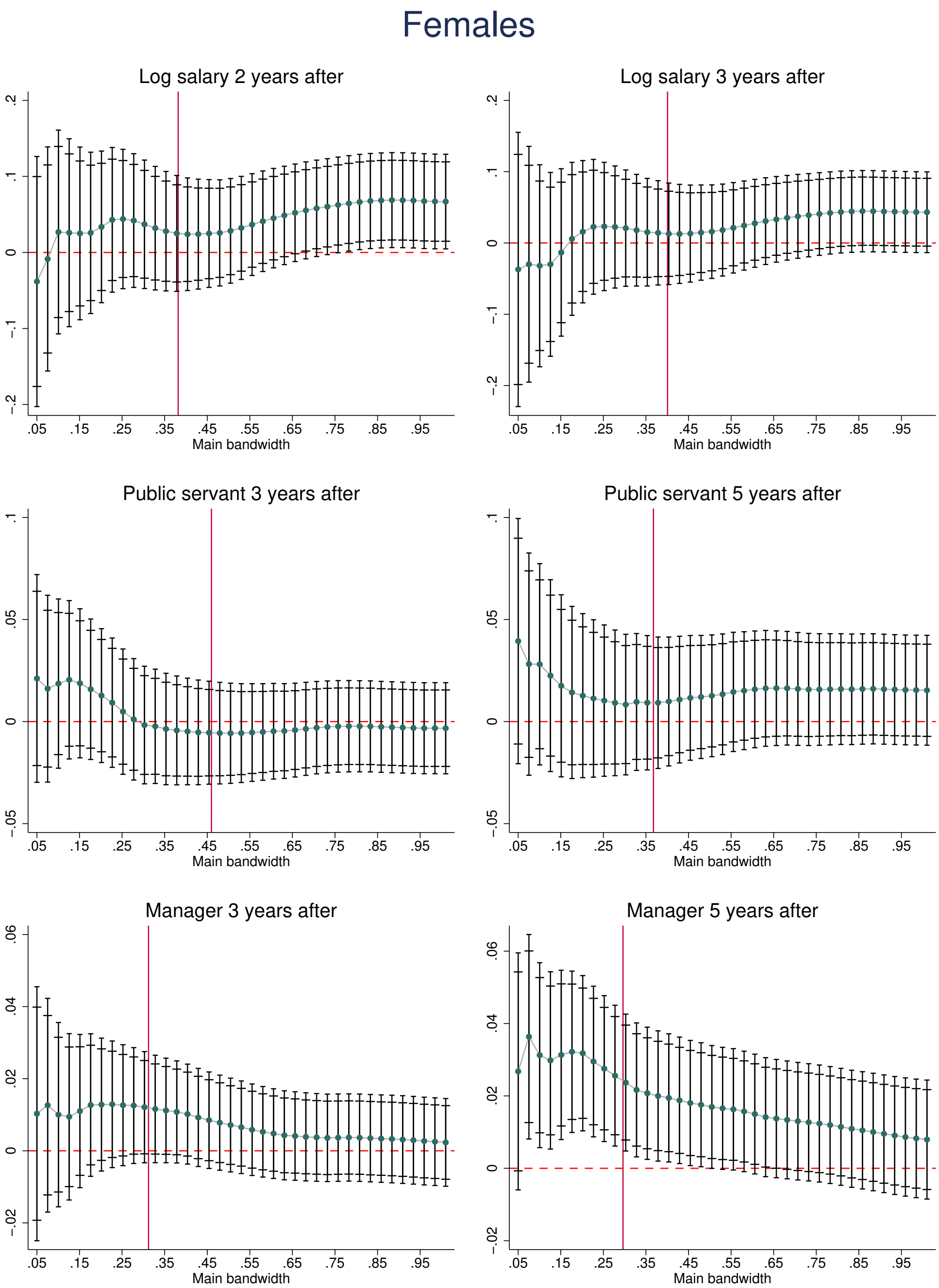
Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates for the first-class effect at different quantiles. The vertical bars represent the confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs are estimated using procedure proposed by Frandsen et al. (2012). Bandwidths are selected by using Calonico et al. (2014) procedure for the average effect.

Figure B7: Ranking Effect on Labor Market Outcomes Using Different Bandwidths

a) Bandwidths for entrance score

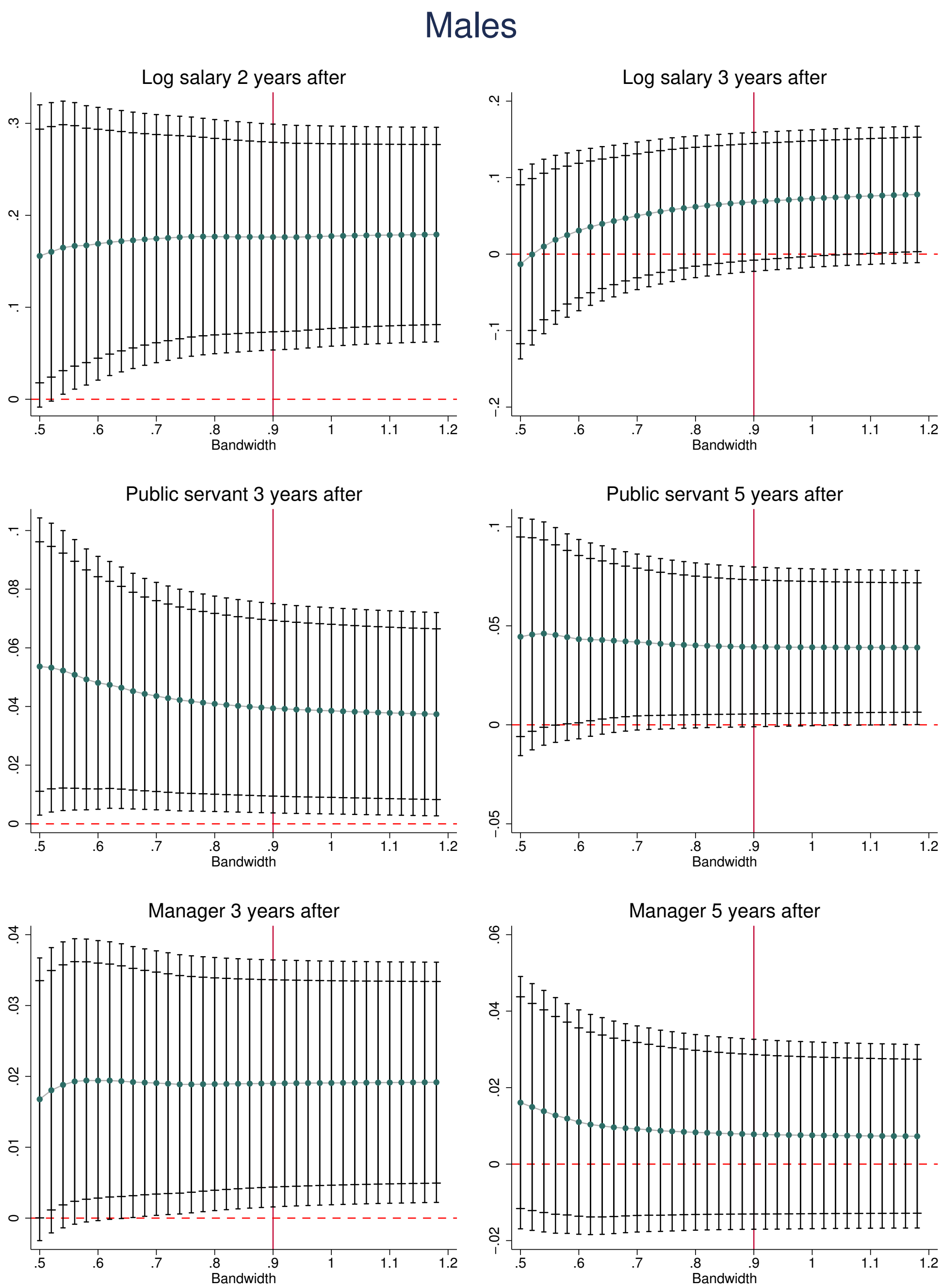


(continuing)

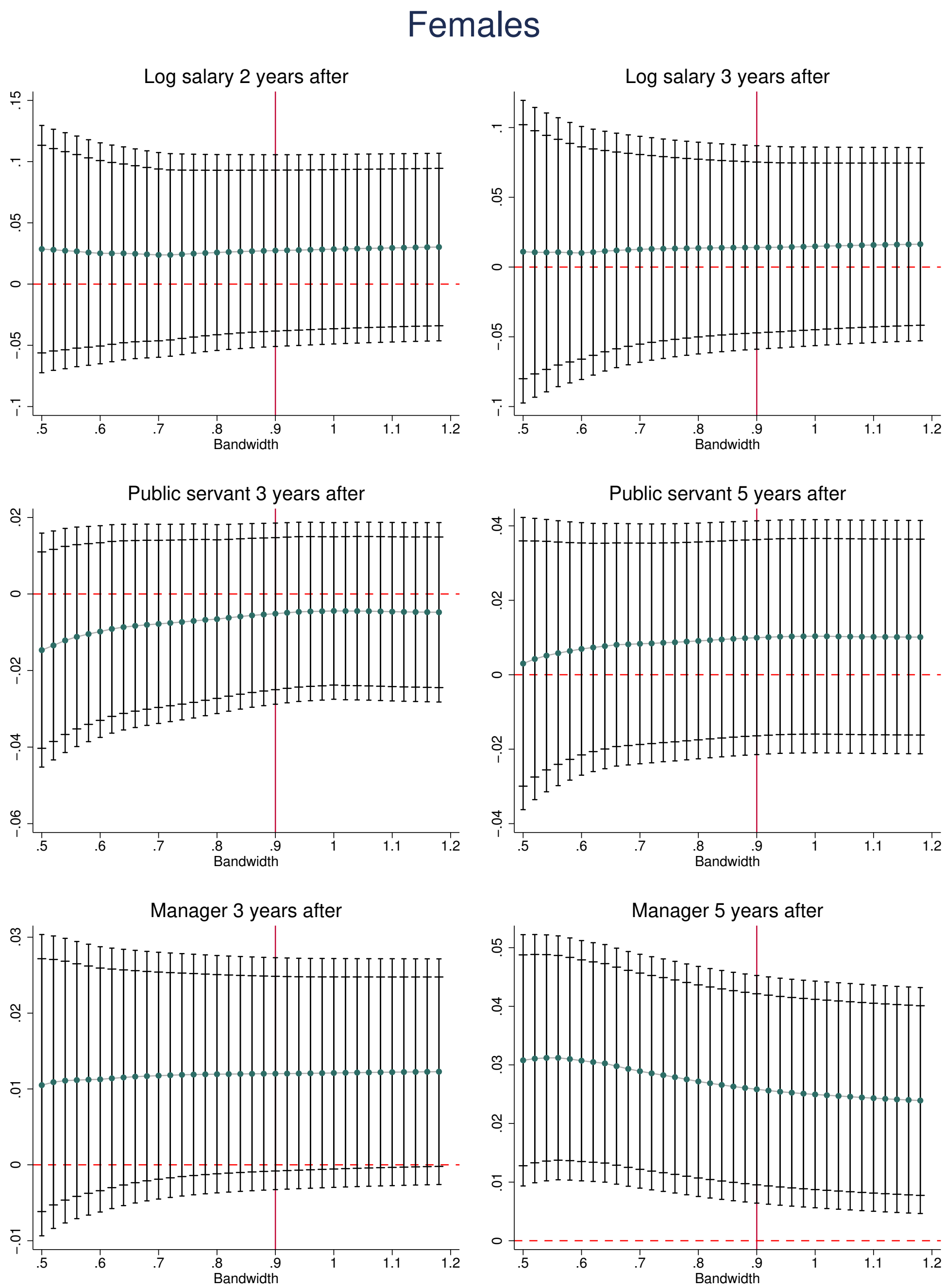

Females
Log salary 2 years after
Log salary 3 years after
Public servant 3 years after
Public servant 5 years after
Manager 3 years after
Manager 5 years after
Main bandwidth
.05 .15 .25 .35 .45 .55 .65 .75 .85 .95

(continuing)

b) Bandwidths for difference in peer quality



(continuing)



Notes: This figure presents the fuzzy regression discontinuity (FRD) estimates of the ranking effect using different bandwidths for the entrance score (panel a) and the difference in peer quality (panel b). The vertical bars represent the robust confidence interval at the 90% and 95% levels. The sample comprises candidates admitted for the first time, who are 21 years or less. FRDs and their relationships with peer quality are estimated using triangular kernels. In panel (a), the vertical line indicates the main bandwidth obtained with the procedure proposed by Calonico et al. (2014). In panel (b), the vertical line indicates the bandwidth used in the main findings.

Table B1: All Regular Undergraduate Programs Offered by UFPE

Program	Two classes	Undergraduate program	Two classes
Accounting	✓	Library Science	
Actuarial Science		Linguistics and Literature	✓
Archaeology		Marine Engineering	
Architecture	✓	Marketing	
Audiophonology		Materials Engineering*	✓
Audiovisual Communication	✓	Mathematics	
Automation Engineering	✓	Mathematics Education	
Biology	✓	Mathematics Education (CAA)	✓
Biology (CAV)	✓	Mechanical Engineering	✓
Biology - Medical Sciences	✓	Media Communication	
Biology Education	✓	Medicine	✓
Biomedical Engineering		Mining Engineering	✓
Biomedicine	✓	Museology	
Business Administration	✓	Music (Instrument)	
Business Administration (CAA)	✓	Music (Vocal)	
Cartographic Engineering		Music Education	✓
Chemical Engineering	✓	Nursing	✓
Chemistry		Nursing (CAV)	✓
Chemistry Education		Nutrition	✓
Chemistry Education (CAA)	✓	Nutrition (CAV)	✓
Civil Engineering	✓	Occupational Therapy	✓
Civil Engineering (CAA)	✓	Oceanography	
Computational Engineering	✓	Pedagogy	✓
Computational Science	✓	Pedagogy (CAA)	✓
Dance		Pharmacy	✓
Dental Medicine	✓	Philosophy	
Design	✓	Philosophy Education	
Design (CAA)	✓	Physical Activity and Sports*	✓
Economics		Physical Activity and Sports (CAV)	
Economics (CAA)	✓	Physical Education*	✓
Electrical Engineering	✓	Physical Education (CAV)	
Electronics Engineering	✓	Physics	
Energy Engineering		Physics Education (CAA)	✓
Engineering	✓	Physics Education	
Food Engineering		Physiotherapy	✓
Geography		Political Science	
Geography Education		Production Engineering	
Geology	✓	Production Engineering (CAA)	✓
Graphic Arts		Psychology	✓
History	✓	Public Health* (CAA)	✓
History Education	✓	Secretarial Science	✓
Hotel Management		Sign Language Education	✓
Industrial Chemistry		Social Sciences	
Information Management	✓	Social Science Education	
Information Systems	✓	Social Service	✓
Journalism		Statistics	
Language Education (French)		Theatre	
Language Education (English)		Tourism Management	✓
Language Education (Spanish)	✓	Visual Arts	
Law	✓		

Note: *Material Engineering and Public Health are not included in the sample due to the small number of freshmen; Physical Activity and Physical Education are not included because their ranking is not determined by cognitive skills only. CAA and CAV are campi located in other cities.

Table B2: Balance of Covariates at the Cutoff

	Males		**Females**	
	Estimate	p-value	Estimate	p-value
Age	0.044	0.563	0.063	0.335
	(0.077)		(0.066)	
White	0.055	0.250	-0.047	0.256
	(0.048)		(0.042)	
Living in Pernambuco	0.017	0.482	-0.010	0.641
	(0.024)		(0.021)	
From public high school	0.009	0.783	-0.024	0.452
	(0.031)		(0.032)	
Employed at application	-0.019	0.431	0.010	0.616
	(0.025)		(0.021)	
Number of vestibular tries	0.009	0.878	0.010	0.855
	(0.057)		(0.056)	
Both parents with college degree	-0.045	0.229	-0.016	0.566
	(0.038)		(0.028)	
Neither parent with college degree	0.021	0.594	0.016	0.640
	(0.039)		(0.034)	
Reason for choosing the program				
Opportunities and prestige	0.018	0.586	0.038	0.220
	(0.033)		(0.031)	
Self-fulfillment	-0.023	0.550	-0.014	0.682
	(0.039)		(0.035)	
Other motives	0.003	0.922	-0.017	0.485
	(0.031)		(0.025)	
Instructor characteristics				
Female instructors	0.003	0.865	0.006	0.719
	(0.020)		(0.016)	
40+ year-old instructors	0.027	0.214	0.007	0.694
	(0.022)		(0.017)	
Assistant professors	-0.007	0.742	0.009	0.552
	(0.021)		(0.016)	
Associate or full professors	-0.013	0.571	0.022	0.225
	(0.023)		(0.018)	
Instructor quality				
Dropout rate	0.001	0.604	0.001	0.316
	(0.002)		(0.001)	
Failure rate	0.002	0.415	-0.000	0.734
	(0.002)		(0.001)	

Note: This table presents the regression discontinuity (RD) estimates for all covariates observed at the application and the characteristics of instructors in the first semester. The sample comprises candidates admitted for the first time, who are 21 years or less. RDs are estimated using triangular kernel with the bandwidth selection procedure proposed by Calonico et al. (2014). Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table B3: Sample Size in Employment Data

Years after expected graduation	Males			Females		
	All	Employed	Rate	All	Employed	Rate
0	8,904	2,066	0.232	11,338	2,166	0.191
1	7,790	2,620	0.336	9,699	3,348	0.345
2	6,543	3,214	0.491	8,049	4,157	0.516
3	5,575	3,248	0.583	6,586	4,020	0.610
4	4,770	3,103	0.651	5,627	3,760	0.668
5	3,642	2,556	0.702	4,291	3,045	0.710

Table B4: All Sampled Classes by Undergraduate Program and Year

Program	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
Accounting*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Architecture	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Audiovisual Communication								✓	✓	✓	✓
Biology*	✓	✓	✓	✓	✓	✓	✓	✓	✓	x	✓
Biology (CAV)*						x	✓	✓	✓	✓	✓
Biology - Medical Sciences	x	x	x	x	x	x	x	✓	✓	✓	✓
Biology Education*	✓	✓	✓	✓	✓	✓	✓	✓	x	✓	✓
Biomedicine	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Business Administration*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Business Administration (CAA)					✓	✓	✓	✓	✓	✓	✓
Cartographic Engineering	x	x	x	x	x	x	x	x	x	x	x
Chemical Engineering	✓	✓	✓	✓	✓	✓	✓	✓	x	x	✓
Chemistry Education (CAA)*					x	x	x	x	x	✓	✓
Civil Engineering	✓	✓	✓	✓	✓	✓	✓	✓	x	✓	✓
Civil Engineering (CAA)					✓	x	x	✓	✓	✓	✓
Computational Engineering	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Computational Science	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Dental Medicine*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Design	x	x	x	x	x	x	x	✓	✓	✓	✓
Design (CAA)					✓	✓	✓	✓	✓	✓	✓
Economics (CAA)*					✓	✓	✓	✓	✓	✓	✓
Electrical Engineering	✓	✓	✓	✓	✓	✓	✓	✓	x	x	x
Electronics Engineering	✓	✓	✓	✓	✓	✓	✓	✓	x	x	x
Engineering							✓	✓	x	x	✓
Geology	x	x	x	✓	✓	x	x	✓	✓	✓	✓
History*	✓	✓	✓	✓	✓	x	✓	✓	✓	x	x
History Education	x	x	x	x	x	x	x	x	x	x	x
Information Management*								x	✓	✓	x
Information Systems										✓	✓
Language Education (Spanish)									x	x	✓
Law	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Linguistics and Literature	✓	✓	✓	✓	✓	✓	✓	✓	x	x	x
Mathematics Education (CAA)					x	x	x	x	x	✓	✓
Mechanical Engineering	✓	✓	✓	✓	✓	✓	✓	✓	✓	x	✓
Medicine	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mining Engineering	✓	✓	✓	✓	✓	x	x	x	x	x	✓
Music Education	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Nursing*	✓	✓	✓	✓	✓	✓	✓	✓	x	✓	✓
Nursing (CAV)*						✓	✓	✓	✓	✓	x
Nutrition*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Nutrition (CAV)*						✓	✓	✓	✓	✓	✓
Occupational Therapy*	x	✓	✓	x	✓	x	✓	x	✓	✓	✓
Pedagogy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pedagogy (CAA)*					✓	x	x	x	✓	✓	✓
Pharmacy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Physics Education (CAA)					x	x	x	x	x	✓	✓
Physiotherapy*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Production Engineering (CAA)*										✓	✓
Psychology*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Secretarial Science*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sign Language Education	x	x	x	x	x	x	x	x	✓	✓	✓
Social Service	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Tourism Management*	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: *Programs that fall within a small bandwidth (0.1 s.d.) in the difference in peer quality at least once. x means that the number of freshmen in either class is less than 15, so the cohort is not in the sample; while ✓ means that the cohort is in the sample. An empty cell means that the program was not available at the time.

Table B5: Net Effect of First Class and Ranking Effect on Performance in the 1st Semester

	Males			**Females**		
	Reduced form	Net effect	Ranking effect	Reduced form	Net effect	Ranking effect
Number of courses taken	-0.108	-0.159	0.016	-0.039	-0.053	0.031
	(0.089)	(0.134)	(0.075)	(0.102)	(0.143)	(0.078)
Missed first midterm	0.019	0.029	-0.012	0.020*	0.029*	-0.005
	(0.019)	(0.029)	(0.017)	(0.012)	(0.017)	(0.009)
First midterm grade	-0.282***	-0.427***	0.094*	-0.162***	-0.220***	0.087***
	(0.060)	(0.093)	(0.055)	(0.044)	(0.062)	(0.031)
Number of absences	0.741**	1.096**	-0.391	0.551**	0.730**	-0.300
	(0.359)	(0.537)	(0.261)	(0.271)	(0.370)	(0.200)
Standardized course grade	-0.182***	-0.272***	0.044	-0.183***	-0.241***	0.081**
	(0.060)	(0.090)	(0.052)	(0.048)	(0.065)	(0.037)
GPA	-0.265**	-0.394**	0.145	-0.285***	-0.371***	0.193***
	(0.130)	(0.195)	(0.110)	(0.089)	(0.118)	(0.067)
Failure rate	0.059**	0.086**	-0.030	0.066***	0.086***	-0.026**
	(0.027)	(0.040)	(0.024)	(0.018)	(0.024)	(0.013)

Note: This table presents the estimated regression discontinuity (RD) at the first class cutoff (reduced form) and fuzzy RD estimates of the first-class effect (net effect) and the ranking effect. The ranking effect derives from the discontinuity between the classes in which the difference in median score is zero. The sample comprises candidates admitted for the first time, who are 21 years or less. RDs and their relationships with peer quality are estimated using triangular kernels. The bandwidth for difference in peer quality is 0.9 s.d. (for the ranking effect) and the bandwidth for entrance score is selected basedby using Calonico et al. (2014) procedure. Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.

Table B6: Effect of Delayed Start Using Strikes

	Males		**Females**	
	All students	Sample	All students	Sample
Switched programs^{+}	-0.004	-0.003	-0.008	-0.005
	(0.004)	(0.007)	(0.006)	(0.005)
Tried another vestibular^{+}	0.027	0.071*	-0.003	0.000
	(0.025)	(0.042)	(0.019)	(0.023)
Graduated on time^{+}	0.032	0.007	0.042	-0.010
	(0.083)	(0.102)	(0.068)	(0.078)
Dropped out^{+}	0.109*	0.195***	-0.030	-0.012
	(0.058)	(0.074)	(0.038)	(0.049)
Number of courses taken semester 1	0.189	-0.179	-0.415***	-0.527***
	(0.138)	(0.163)	(0.135)	(0.166)
Missed first midterm	-0.029	0.010	0.017	0.013
	(0.032)	(0.041)	(0.031)	(0.032)
First midterm grade	-0.095	-0.306**	-0.031	0.027
	(0.092)	(0.125)	(0.082)	(0.095)
Number of absences semester 1	1.743	2.439*	1.351	1.313
	(1.106)	(1.377)	(0.847)	(0.935)
GPA semester 1	0.108	-0.229	0.078	0.060
	(0.195)	(0.265)	(0.143)	(0.161)
GPA year 1	-0.045	-0.294	0.112	0.072
	(0.172)	(0.250)	(0.125)	(0.150)
Failure rate semester 1	-0.012	0.070	-0.026	-0.012
	(0.043)	(0.053)	(0.033)	(0.034)
Failure rate year 1	0.043	0.105**	-0.017	-0.002
	(0.039)	(0.050)	(0.027)	(0.031)

Note: This table presents the difference in academic outcomes between the last student in first classes who faced an unexpected delay in the first semester of 2006 and the last student in first classes who faced no delay. The expected outcome for the last student is estimated using a local linear regression with the bandwidth selected using Calonico et al. (2014) procedure. 2004, 2005, 2008, 2009, 2010 and 2011 are the years in which the first class did not experience delays (strikes) in the first semester. Due to later strikes, we exclude 2004, 2005 and 2011 for one-year outcomes, and also 2009 and 2010 for 2+ year outcomes (+). 'All students' represents all freshmen with no sample restriction, while 'sample' represents the candidates admitted for the first time, who are 21 years or less, and enrolled on a program with two classes. Robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels, respectively.