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The Labor Market Effects of Eliminating University Tuition in Ecuador

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Abstract

It is both theoretically and empirically unclear whether a drastic nationwide reduction in the cost of education should significantly improve individual labor market outcomes. This paper estimates the labor market effects of a 2008 policy that eliminated tuition fees at public universities in Ecuador. We use a difference-in-difference strategy that exploits variation across cohorts differentially exposed to the policy, as well as geographic variation in access to public universities. We find that the tuition fee elimination significantly increased college participation, but did not improve income. The policy had modest effects on job type, shifting people out of more physical jobs. However, the bulk of the benefits of this fee elimination were enjoyed by individuals of higher socioeconomic status.

Keywords: higher education, tuition reduction, Ecuador
JEL Codes: I23, I24, I28, O15

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1 Introduction

Education, an important form of human capital investment, is often viewed as a fundamental key to development. This is reflected in the international commitments made to education, in both the Millennium Development Goals and Sustainable Development Goals.[1] Economic theory suggests that one obvious way to increase educational attainment is to reduce its cost, but whether country-wide fee reductions will lead to substantial improvements in labor market outcomes (by increasing education levels) remains an open question.

At the individual level, it is well-documented that schooling increases income (Card, 2001). This does not imply, however, that reducing the costs of schooling for an entire population will result in higher income on average. For example, if there are substantial general equilibrium effects – that is, if a drastic fee reduction results in an increase in the supply of educated workers that is large enough to depress their wages – then the income benefits of a policy like this could be quite limited.

Our knowledge about the effects of reducing education fees in the developing world comes primarily from studying policies involving fee reductions at the primary or secondary school level. Around the turn of the century, a number of developing countries (mainly in Africa) abolished primary or secondary school fees in order to expand access to education. Evaluations of these policies have generally found that they improved enrollment and other short-term educational markers (World Bank, 2009; Lucas and Mbiti, 2012; Garlick, 2017). Evidence on long-run educational outcomes is more mixed,[2] and little is known about eventual labor market effects.

Even less is known about the effects of nationwide reductions in *university* fees, especially in the developing world. There is a large literature documenting that college tuition influences tertiary education decisions in the developed world (Dynarski, 2003; Stanley, 2003; Fack and Grenet, 2015), and a smaller body of research documenting positive effects of lower tuition costs

[1] Objective 2 of the MDGs was to achieve universal primary education. Objective 4 of the SDGs is to ensure quality education for all.

[2] Garlick (2017) shows that eliminating secondary school fees had small, positive effects on enrollment, but negligible effects on grade continuation and graduation in South Africa. Osili and Long (2008) and Keats (2018) document that eliminating primary school fees increased female educational attainment (and reduced fertility) for Nigeria and Uganda, respectively.

on labor market outcomes (Bound and Turner, 2002; Angrist and Chen, 2011). In developing countries, there is evidence that direct costs play a role in driving college enrollment decisions (Dinkelman and Martínez A, 2014; Kaufmann, 2014), but this leaves much to be learned about how a nationwide fee reduction would affect labor market outcomes.

This is an important, policy-relevant question in the developing world, where governments have begun showing interest in tuition-free tertiary education. The Philippines, for example, eliminated tuition at public universities in 2018 (Mendez, 2017; Morallo, 2018), and Chile passed a tuition-free policy, slightly more limited in scope, in 2016 (Delisle and Bernasconi, 2018). The aforementioned studies on primary and secondary school fee reductions may provide little guidance on university-level policies like these, given the potentially different returns to tertiary education (Psacharopoulos and Patrinos, 2018), as well as the different opportunity costs.[3] At the same time, the types of natural experiments studied in the developed world literature (the United States G.I. bill, for example) are less prone to the general equilibrium effects that are likely extremely important in this context.

This paper aims to shed light on these issues by evaluating an Ecuadorian policy that eliminated tuition fees at all public universities in 2008. In the existing work that looks at the short-run effects of this policy (using data up until 2010), the empirical strategies involve either comparing outcomes across cohorts or comparing the same cohort over time (Post, 2011; Ponce and Loayza, 2012; Acosta, 2016), which makes it impossible to separate the effects of the policy from broader time trends or cohort trends.

We overcome these limitations by using a difference-in-differences strategy (and more recent data) that utilizes two sources of variation. First, we compare individuals who were young enough to have been affected by the policy (college-aged in 2008) and individuals who were too old to have been affected. Second, we exploit variation in geographic access to public universities, for which we measure access in two ways: using distance to the nearest public university and using the number of public universities within a 20 kilometer radius. In both specifications, we find that the difference in college enrollment between exposed (younger) and unexposed (older)

[3]Becker (1975), for example, demonstrates that tuition and fees for university make up only a small proportion of the total cost of a college education, inclusive of opportunity costs.

cohorts is larger for those with greater access to public universities. Specifically, younger cohorts are more likely to have attended college than older cohorts across the entire sample, but this gap is larger among individuals with greater access to public universities. We interpret this as evidence that the policy increased college enrollment. We are able to rule out other explanations for these results, including mean reversion and differential trends across areas of varying levels of remoteness.

Importantly, we also look at labor market outcomes. Though the policy appears to have boosted college enrollment, we find no positive effects on income. There are modest effects on job type – individuals appear to be shifting out of more physical jobs. Although a primary goal of the policy was to increase equality in tertiary education access, we find that it disproportionately benefited those of higher socioeconomic status. Individuals who speak an indigenous language and individuals born in poor areas saw no improvements in college enrollment or changes in job type.

2 Background

Before 2008, Ecuador's higher education system was largely unregulated. Many higher education institutions had been established, though a large number were perceived to be of low quality. In particular, a number of institutions had been created to offer careers related to business and administration, leaving the areas of science and technology aside (Cabrera Narváez et al., 2017). Both public and private universities charged fees, and each university had their own methods of determining the acceptance process for a potential student. Though tuition fees varied across universities, in 2007, a typical public university charged 1200 to 1500 USD per academic year (for a student without any scholarships). This was a large share of median household income, which was approximately 3000 USD in 2007.

When President Rafael Correa took office in 2007, he proposed radical changes to the university education system. In 2008, the government approved a new constitution, which made two important changes to tertiary education. First, it established that the state would provide quality public education (including tertiary education) free of charge. Second, in order to achieve the goal of quality education, it mandated that the National Council for Evaluation and Accreditation (CONEA), which governed the universities of Ecuador at the time, should carry out a systematic evaluation of all universities in the country. In the years that followed, this nation-wide evaluation was conducted, and by 2016, 15 poorly performing universities were closed. Requirements for professor qualifications increased, and regulations on the selection, evaluation, and renumeration of professors were approved.

This policy, therefore, is one that drastically reduced the cost but also made serious attempts to increase the quality of public universities. It is important to note, however, that the steps taken to increase quality were more gradual, whereas the cost reduction was immediate – starting in October of 2008, students (including those already enrolled) no longer had to pay tuition fees. Of course, only qualified students were allowed to enroll – most public universities had entrance exams – and fees were not fully covered for students who failed any school year. (See Ponce and Loayza (2012) for more details on the policy.)

3 Data

The outcome variables in this analysis come from the nationally representative National Survey of Employment, Unemployment, and Underemployment (ENEMDU), conducted quarterly. We use all four quarters of the 2014 to 2017 surveys. This survey provides information on respondents' educational attainment, income, labor force participation, and job type. We use two types of job categorizations. First, individuals are categorized as employees, self-employed workers, laborers, or unpaid or domestic workers. They are also asked for their occupation type, which we classify into four groups using the International Standard Classification of Occupations (ISCO) codes: high-skill white-collar (ISCO occupation codes 1 to 3), lower-skill white-collar (ISCO occupation codes 4 and 5), high-skill blue-collar (ISCO codes 6 and 7), and lower-skill blue-collar (ISCO codes 8 and 9).

ENEMDU also records respondents' current residence and place of birth, at the canton level.

We link individuals to universities using their current canton of residence and a list of the 67 universities that were operating in Ecuador in 2008. For each of these universities, we collected the GPS location and type (public or private). For each individual, we calculate the distance to the nearest public university and the number of public universities within 20 km of their canton of residence as measures of public university "access."

We also utilize the Ecuadorian censuses of 1962, 1974, 1982, and 1990 to calculate canton-level indicators of economic development. We link individuals to their canton of birth around their year of birth in order to generate a variable intended to capture an individual's socioeconomic background. Specifically, in each census year, we calculate the canton-level share of households with electricity and share with piped water. We then assign each canton with an indicator for being below median in either of these canton-level distributions. Finally, we match individuals to their canton of birth and the census preceding their birth year. We assign a "below-median birthplace" indicator to individuals, for which a one denotes individuals whose canton of birth was in the bottom half of either the electricity or piped water distribution in the relevant census year.

3.1 Summary Statistics

Table 1 reports summary statistics for the study sample – individuals aged 18 to 34 in 2008, who are at least 25 years of age when they are surveyed (in 2014 to 2017). On average, individuals live 26km from a public university and have one public university within 20km of their residence. Less than a quarter of the sample attended college, even fewer stayed for at least 4 years, and a very small share (less than 5%) are currently still studying. Income and all job-type indicators are only reported for individuals in the labor force (approximately 83% of the sample). There are two types of job categorizations. Individuals are categorized into employees, self-employed workers, laborers, and unpaid or domestic workers. They are also asked for their occupation type, which we classify into four groups, as described above: high-skill white-collar, lower-skill white-collar, high-skill blue-collar, and lower-skill blue-collar. The sample is predominantly white or mestizo and does not speak an indigenous language. Slightly under half of the sample was born in a “below median birthplace,” an indicator we generate to proxy for low socioeconomic status.

Table 1. Summary Statistics

	Mean	Standard Deviation	Observations
Distance to Nearest Public University	0.26	0.394	230821
Number of Public Universities within 20km	1.01	0.996	230821
Attended College	0.24	0.425	231452
Attended 4 Years of College	0.18	0.382	228024
Attending School	0.05	0.211	231452
Log Income	5.59	1.618	170193
In Labor Force	0.83	0.372	231452
nonmissinc	0.74	0.441	231452
govt	0.13	0.336	191500
pvt	0.33	0.472	191500
Employee	0.47	0.499	191500
Self-Employed	0.31	0.461	191500
Laborer	0.11	0.310	191500
Unpaid or Domestic Work	0.12	0.327	191500
White Collar 1	0.18	0.383	190321
White Collar 2	0.26	0.437	190321
Blue Collar 1	0.28	0.449	190321
Blue Collar 2	0.29	0.452	190321
Male	0.47	0.499	231452
White or Mestizo	0.82	0.386	231452
Speaks Indigenous Language	0.10	0.302	231452
Below Median Birthplace	0.34	0.474	220770
Age during Survey	33.30	4.881	231452
Age in 2008	25.95	4.809	231452

Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys, aged 18 to 34 in 2008, and at least 25 years old at the time of survey. Distance to Nearest Public University is reported in 100km increments.

4 Empirical Strategy

To estimate the effect of the 2008 tuition fee elimination on various outcomes, we rely on a generalized difference-in-differences strategy. We compare the outcomes of individuals who were young enough to be affected by the policy to outcomes of those who were past college-going age when the policy was implemented, across areas with differential access to public universities. The underlying intuition is that the policy should primarily affect those with greatest access to public universities and should have no impact on those without access to these public universities. The differential change in outcomes across individuals with differential access to public universities can therefore be interpreted as the causal effect of the policy.

Our baseline specification is as follows. For individual i, living in canton j, who was aged a in 2008,

$$Y_{ija} = \beta \text{Exposed}_a \times \text{Access}_j + \alpha X_{iaj} + \mu_a + \delta_j + \epsilon_{ija}. \tag{1}$$

Here, Exposed_a is an indicator equal to 1 for individuals who were young enough to be affected by the policy (ages 18 to 24 in 2008), 0 for individuals too old to be affected (ages 30 to 34 in 2008), and missing for those in between, for whom the relevance of the policy is more ambiguous.[4] In other words, we restrict this regression to individuals aged 18 to 24 or 30 to 34 in 2008. We use two different variables to represent Access_j, and run separate regressions using each one. First, we define access as the distance between an individual's canton of residence and the nearest public university. In this specification, a negative β would indicate that the policy had a positive effect on the outcome of interest. Second, we define access as the number of public universities within 20 kilometers of the individual's canton of residence. For this specification, a positive β would mean that the policy increased the outcome of interest.

In all regressions, canton fixed effects (δ_j) control for any time-invariant unobservables that vary at the canton-level and might drive our outcomes of interest, while cohort fixed effects (μ_a) control for non-linear trends across cohorts in our outcomes of interest. We also include a vector of controls, X_{iaj} – basic demographic controls (gender and age at the time of survey), along with survey year fixed effects. The outcomes we consider include college-related and labor market outcomes, described in section 3. Because of our interest in labor market outcomes, we restrict

[4]In the 2014 to 2017 waves of ENEMDU, almost 20% of 24-year-olds were attending school, while only 5% were attending at age 30. Attendance proportions range from 7 to 14% for those aged 25-29, which is why we consider the relevance of the policy more ambiguous for this age group.

to individuals who were at least 25 years old at the time of the survey, in order to focus on individuals likely to have completed school and to be working full time.

In order for β to represent the causal effect of the policy on outcomes, it must be the case that the difference between exposed and unexposed cohorts would show no systematic variation across the Access_j distribution, in the absence of the policy. It is important, then, that the Access_j variable is not simply proxying for other canton-level characteristics that could be driving (or correlated with drivers of) differential trends across cohorts. We therefore run a number of robustness checks that add cohort fixed effects interacted with various canton-level characteristics.

We also run a more rigorous specification that incorporates a falsification test. Instead of using the simple Exposed_a indicator, which is missing for individuals in between our exposed and unexposed cohorts, we create separate indicators for three different age categories: fully exposed individuals (aged 18 to 24 in 2008), partially exposed individuals (aged 25 to 29 in 2008), unexposed individuals (aged 30 to 34 in 2008), and a placebo group (aged 35 to 39 in 2008). We include exposed, partially exposed, and placebo indicators (interacted with Access_j) in the regression, leaving the unexposed individuals as the omitted category (as in the previous specification).

$$\begin{aligned} Y_{ija} =& \beta_1 \text{Exposed}_a \times \text{Access}_j + \beta_2 \text{Partially Exposed}_a \times \text{Access}_j + \beta_3 \text{Placebo}_a \times \text{Access}_j + \\ & \alpha X_{iaj} + \mu_a + \delta_j + \epsilon_{ija}. \end{aligned} \tag{2}$$

Because we do not expect to see cohort trends vary systematically by our access variable among individuals not exposed to the policy, we expect β_3 to be equal to zero. A non-zero β_3 would suggest a violation of our identification assumption because it would imply differential cohort trends that varied systematically across the Access_j distribution, even for cohorts that should not have been affected by the policy.

5 Results

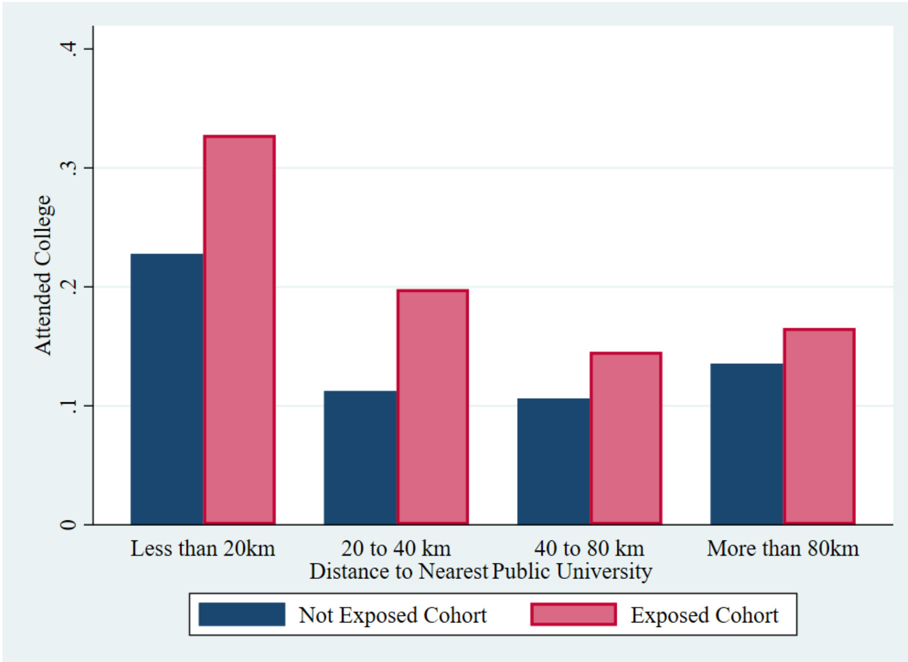
5.1 Graphical Evidence

We begin with a graphical representation of the data. Figure 1 illustrates the proportion of individuals that attended college, separately for exposed and not exposed cohorts, by distance to nearest public university. Across all distance categories, the proportion of exposed cohorts (aged 18 to 24 in 2008) that attended college is higher than the proportion of unexposed cohorts (aged 30 to 34 in 2008) that attended, which reflects the fact that college enrollment has been increasing over time. This also highlights why a simple comparison of college attendance rates before and after 2008 would not provide us with a causal estimate of the effect of the tuition reduction – this comparison would not be able to separate out time trends that would have existed even without the tuition change.

Importantly, this figure shows that the gap between exposed and not exposed cohorts is largest for those living less than 40km from a public university, and much smaller for the other two distance categories. This is what we would expect to see if the tuition-free policy had a causal impact on college decisions. Because individuals living further away face substantially higher non-tuition (transportation or moving) costs, the fee elimination represents a smaller reduction as a share of total costs.

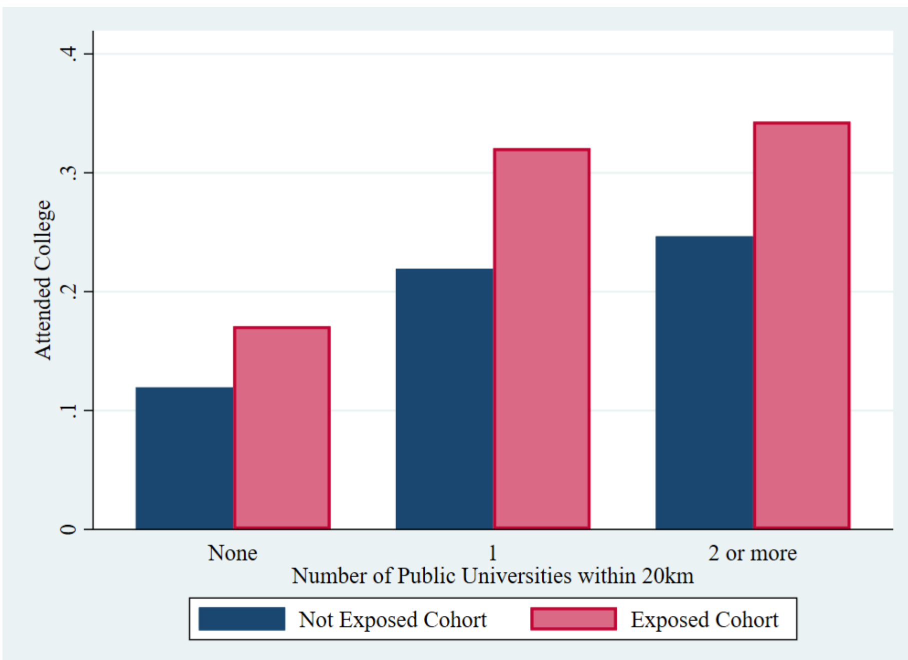
A similar result is presented in Figure 2, where we graph college attendance proportions for exposed and not exposed cohorts, by the number of public universities within 20km. Again, we see that the younger cohorts were more likely to attend college. However, this gap is larger for individuals living within 20km of at least one public university. Both of these figures show that those with the greatest access to public universities saw the largest increases in college enrollment after 2008, which suggests that the tuition-free policy may have increased college enrollment. In the next sub-section, we explore this using more formal regression analysis.

Figure 1. College Attendance, by Cohort and Distance to Nearest Public University



Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys, at least 25 years or older at time of survey, aged 18-24 or 30-34 in 2008. "Not Exposed Cohorts" were aged 30-34 in 2008, and "Exposed Cohorts" were aged 18-24 in 2008.

Figure 2. College Attendance, by Cohort and Number of Nearby Public Universities



Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys, at least 25 years or older at time of survey, aged 18-24 or 30-34 in 2008. "Not Exposed Cohorts" were aged 30-34 in 2008, and "Exposed Cohorts" were aged 18-24 in 2008.

5.2 Education Outcomes

Table 2 reports our estimates of the effect of the policy on college-related outcomes. Specifically, we report estimates of β (the coefficient on the interaction between Exposed_a and Access_j) in equation (1). The first row reports estimates that use distance to the nearest public university as the access variable of interest, while the second row reports regressions that use the number of public universities within 20 kilometers as the access variable. This table studies three outcomes of interest: the first (in columns 1 and 4) is simply an indicator for individuals that attended college. The second (in columns 2 and 5) is an indicator for individuals that attended college for at least four years, which provides a better measure of college continuation (though not a perfect proxy for college graduation). Finally, we look at an indicator for whether an individual is currently (at the time of survey, between 2014 and 2017) attending school. In addition to the basic specification (in columns 1 to 3), we also report the results from a more rigorous specification that controls for province-by-cohort fixed effects (in columns 4 to 6), which do not substantially affect coefficient estimates.

Table 2. Effects of Tuition Fee Elimination on Educational Outcomes

	(1) Attended College	(2) 4 Years of College	(3) Attending School	(4) Attended College	(5) 4 Years of College	(6) Attending School
Exposed x Distance	-0.065***	-0.047***	-0.030***	-0.079***	-0.048***	-0.052***
	(0.012)	(0.0100)	(0.0062)	(0.014)	(0.012)	(0.0076)
Exposed x Number	0.018***	0.011***	0.013***	0.023***	0.014***	0.019***
	(0.0039)	(0.0032)	(0.0033)	(0.0042)	(0.0031)	(0.0021)
Province-Age Fixed Effects	No	No	No	Yes	Yes	Yes
Dep. Var. Mean	0.24	0.18	0.054	0.24	0.18	0.054
N	161433	159093	161433	161433	159093	161433

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1 ** p< 0.05 *** p< 0.01. Each cell represents a different regression. These regressions restrict to individuals aged 18 to 24 or 30 to 24 in 2008. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. "Distance" is the distance (in 100 kilometers) between the individual's canton of residence and the nearest public university. "Number" is the number of public universities within 20km of the individual's canton of residence. All regressions control for gender and cohort, canton, and age and year fixed effects.

Across all outcomes and specifications, this table reveals that the 2008 tuition fee elimination had a significant effect on university enrollment. The negative and significant coefficients in the first row reveal that the difference between exposed and not exposed cohorts was significantly smaller for those living far away from public universities. Specifically, living 100km further away from a public university is associated with a 7-8 percentage point reduction in the likelihood of going to college, and a 5 percentage point reduction in the likelihood of staying for at least 4 years.

The positive and significant coefficients in the second row reveal that exposed cohorts were more likely to go to college (relative to not exposed cohorts) in cantons located near more public universities. An additional public university within 20km of an individual's canton of residence led to about a 2 percentage point increase in the likelihood of attending college, and a 1 percentage point increase in the likelihood of attending for at least 4 years.

Although only 5% of sample individuals are still attending school, we see (in columns 3 and 6) that the tuition fee reduction significantly increased the likelihood of an individual still being in school at the time of survey, 6 to 9 years after the policy change took place. This highlights that this fee reduction substantially altered the career trajectory of individuals, likely delaying entry into the workforce.

5.3 Labor Market Outcomes

Having established that the tuition fee elimination significantly increased college enrollment, we next ask how this affected labor market outcomes. Table 3 reports the coefficient estimates from the same specifications, focusing on a set of labor market outcomes as the dependent variables of interest. The results from column 1 reveal that the effects on income (conditional on being in the labor force) were in fact negative. In the first row, the positive and significant coefficient implies that the income gap (between exposed and non-exposed cohorts) was larger for those living further away from public universities. The negative and significant coefficient in the second row implies that the gap between exposed and not exposed cohorts is smaller for those living near fewer public universities.

These coefficients should be interpreted with caution. For example, it is important to note

Table 3. Effects of Tuition Fee Elimination on Labor Market Outcomes

	(1) Log Income	(2) In Labor Force	(3) Employee	(4) Self-Employed	(5) Laborer	(6) Unpaid or Domestic	(7) White Collar 1	(8) White Collar 2	(9) Blue Collar 1	(10) Blue Collar 2
Exposed x Distance	0.13**	0.025**	-0.053**	0.0071	0.029***	0.017	-0.032**	-0.0018	-0.024	0.058***
	(0.059)	(0.012)	(0.021)	(0.015)	(0.011)	(0.014)	(0.013)	(0.016)	(0.016)	(0.020)
Exposed x Number	-0.071***	-0.0055	0.010	0.0020	-0.0035	-0.0086**	0.0098***	0.013**	0.00076	-0.023***
	(0.019)	(0.0039)	(0.0073)	(0.0042)	(0.0049)	(0.0036)	(0.0036)	(0.0053)	(0.0052)	(0.0060)
Province-Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	5.58	0.83	0.47	0.30	0.11	0.12	0.18	0.26	0.28	0.29
N	117697	161433	132881	132881	132881	132881	132055	132055	132055	132055

Notes: Standard errors, clustered at the canton level, are in parentheses. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. Each cell represents a different regression. These regressions restrict to individuals aged 18 to 24 or 30 to 24 in 2008, and at least 25 years old at the time of survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. "Distance" is the distance (in 100 kilometers) between the individual's canton of residence and the nearest public university. "Number" is the number of public universities within 20km of the individual's canton of residence. All regressions control for gender and cohort, canton, and age and year fixed effects.

that, by increasing the length of time individuals spent in school, the fee reduction likely delayed their labor force entry. This is supported by the result in the first row of column 2, which shows that the tuition fee reduction reduced the likelihood of an individual being in the labor force at the time of survey (but not in the specification in the second row). In addition, Table 2 showed that individuals affected by the policy are significantly more likely to still be attending school, which means that even those who are in the labor force (and therefore included in the income regression) could be working disproportionately in temporary or part-time jobs that better accommodate a student's schedule. However, since the share of individuals currently attending school is so small (5%), this cannot be the only explanation for the negative income effect.

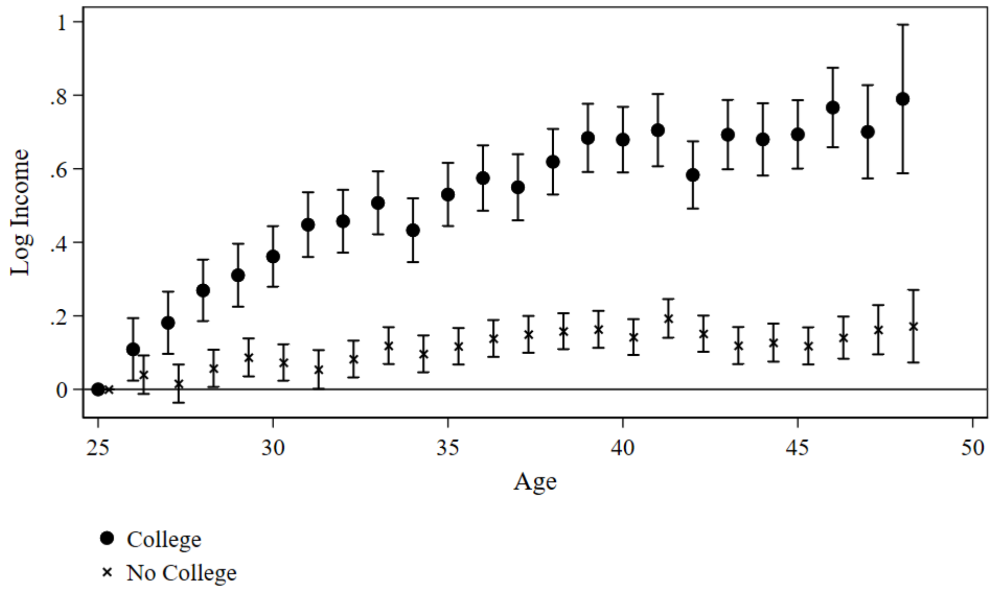
A potentially more important consideration relates to labor market experience. Individuals who spent more time in school (because of the fee elimination) have less work experience once they show up in the sample in 2014-2017. Their lower income might be a result of their shorter average tenure. In Figure 3, we plot the coefficients from a regression of log income on age dummies (separately for those who did and did not attend college). For college-educated workers, it is clear that there is a steep income-age gradient in their early twenties, which we interpret as large returns to experience in the first few years on the job. This suggests that the negative income effect could simply be due to "treated" individuals having less experience.

Indeed, when we restrict the sample to individuals aged 30 and older (after which the income-age gradient flattens out slightly),[5] the negative income effect disappears, as is clear in column 1 of Table A1. However, it is important to note that we still do not detect any positive income effects, even for this older sample. The absence of any income benefits could be stemming from general equilibrium issues relating to an oversupply of college-educated workers, occupational mismatch, or overeducation.

We also investigate, in the remaining columns of Table 3, whether the fee elimination had any impact on the types of jobs that individuals took up. Columns 3 through 6 investigate the classification of a worker as either an employee, self-employed, a laborer, or an unpaid or domestic worker. In the first row (using distance as our cross-sectional access variable), estimates suggest

[5] Unfortunately, the oldest individuals in our "exposed" cohort are 33, which makes it difficult to look at the effects for anyone much older than this.

Figure 3. Income-Age Profiles By College Attendance



Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys, at least 25 years or older at time of survey. Each point plots the coefficient and 95% confidence interval in a regression of log income on age dummies.

that the 2008 policy change increased the likelihood of individuals being employees, primarily by decreasing the likelihood of them being laborers. The second row shows a decrease in the likelihood of being an unpaid or domestic worker.

There is more consistency across specifications in columns 7 through 10, which investigate different worker types based on their occupation codes (which are then classified into high-skill white-collar, lower-skill white-collar, high-skill blue-collar, and lower-skill blue-collar jobs). Both specifications show that the fee reduction increased the likelihood of being in the highest-skill white-collar jobs and lowered the probability of being in the lowest-skill blue-collar jobs. It is also worth noting that these results generally persist even after restricting the sample to individuals aged 30 and older (in Table A1). In short, although the fee reduction had no effects on income, it appears to have changed the career trajectories of individuals. Whether this will lead to greater income in the future is unclear, but could be beneficial regardless if there are better amenities in white-collar jobs.

5.4 Robustness Checks

In the above regressions, our exposure variables are calculated using an individual's canton of residence at the time of survey because location in 2008 (which is the more relevant piece of information) is not recorded for individuals who have migrated. Fortunately, almost 90% of the individuals in the sample are, at the time of survey, living in the same canton in which they lived in 2008. In the appendix (Tables A2 and A3), we repeat our analysis restricting to individuals for whom our access variables are calculated correctly (that is, relative to their actual location in 2008). We find that our results remain consistent, with slightly larger coefficient estimates, suggesting that this source of measurement error may have been attenuating our estimates.

We also conduct a number of exercises to test the validity of our identification assumptions. Because this policy only affected tuition fees at public universities, we should not expect to see the same results using access to private universities instead of public universities. In panel A of Table 4, we add interactions between our exposed indicator and the relevant access variable, calculated relative to private instead of public universities. That is, in the first row, we include the exposed indicator interacted with the distance to the nearest private university, and in the second row, we include the exposed indicator interacted with the number of nearby private universities. Though these variables are highly correlated with the public university interactions, it is clear that the effects reported in the previous tables are being driven by access to public rather than private universities. Across all but one specification, it is only the interactions with public university access variables that are significant.

In general, in order to interpret our coefficient estimates as the causal effect of the 2008 fee reduction, our cross-sectional access variables (distance to and number of nearby public universities) cannot be proxying for other characteristics, across which cohorts could have been trending differently for reasons unrelated to the tuition fee elimination. For example, if these variables are simply capturing the general remoteness of a location, and if the educational outcomes in remote locations were falling behind those in more central areas (for reasons unrelated to the tuition reduction), this would generate the same pattern of results that we see. The results in Panel A help to alleviate some of these concerns by showing that distance to private university (which

Table 4. Robustness Checks

	(1) Attended College	(2) Attending School	(3) Log Income	(4) Employee	(5) Laborer	(6) Unpaid or Domestic	(7) White Collar 1	(8) Blue Collar 2
A. Private Uni Distances								
Exposed x Distance	-0.071***	-0.046***	0.12*	-0.039	0.029***	0.0045	-0.027	0.047**
	(0.016)	(0.011)	(0.061)	(0.030)	(0.011)	(0.018)	(0.017)	(0.023)
Exposed x Distance (Private)	-0.019	-0.013	0.028	-0.031	-0.0014	0.029	-0.011	0.025
	(0.018)	(0.012)	(0.071)	(0.033)	(0.014)	(0.019)	(0.018)	(0.024)
Exposed x Number	0.029***	0.023***	-0.072**	0.021*	-0.0092	-0.00031	0.0067	-0.022**
	(0.0072)	(0.0039)	(0.034)	(0.011)	(0.0070)	(0.0067)	(0.0072)	(0.0098)
Exposed x Number (Private)	-0.0022	-0.0014	0.000084	-0.0038	0.0019	-0.0028*	0.0011	-0.00034
	(0.0016)	(0.00087)	(0.0064)	(0.0029)	(0.0014)	(0.0016)	(0.0017)	(0.0024)
B. Age FE x Distance to Metros								
Exposed x Distance	-0.085***	-0.055***	0.14**	-0.058***	0.027**	0.020	-0.039***	0.063***
	(0.013)	(0.0074)	(0.063)	(0.022)	(0.012)	(0.014)	(0.012)	(0.018)
Exposed x Number	0.023***	0.018***	-0.071***	0.010	-0.0030	-0.0088***	0.010***	-0.023***
	(0.0042)	(0.0022)	(0.020)	(0.0074)	(0.0047)	(0.0034)	(0.0037)	(0.0060)
C. Mean Reversion								
Exposed x Distance	-0.057***	-0.029***	0.15**	-0.021	0.027**	-0.010	-0.020	0.025
	(0.019)	(0.0084)	(0.066)	(0.030)	(0.013)	(0.019)	(0.017)	(0.027)
Exposed x Number	0.015***	0.011***	-0.059**	0.0046	-0.0014	-0.0029	0.0068	-0.013
	(0.0050)	(0.0025)	(0.026)	(0.0097)	(0.0062)	(0.0059)	(0.0048)	(0.0078)
Province-Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	0.24	0.054	5.58	0.47	0.11	0.12	0.18	0.29
N	161433	161433	117697	132881	132881	132881	132055	132055

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1 ** p< 0.05 *** p< 0.01. Horizontal lines indicate separate regressions. These regressions restrict to individuals aged 18 to 24 or 30 to 24 in 2008, and at least 25 years old at the time of survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. "Distance" is the distance (in 100 kilometers) between the individual's canton of residence and the nearest public university, while "Distance (Private)" represents distance to the nearest private university. "Number" is the number of public universities within 20km of the individual's canton of residence, while "Number (Private)" represents the number of private universities within 20km. Panel B includes cohort fixed effects interacted with the distance to the nearest large metropolitan area (either Guayaquil or Quito). Panel C includes cohort fixed effects interacted with average canton-level schooling levels, electricity shares, and piped water shares from 2001. All regressions control for gender and cohort, canton, and age and year fixed effects.

may also be a proxy for remoteness) is not a significant driver of the results. In addition, we run another specification. In Panel B, we calculate the distance between an individual's canton of residence and the nearest large metropolitan area (either the city of Guayaquil or Quito), and we include age fixed effects interacted with this distance variable. Our results are largely unchanged.

In Panel C, we address concerns that our results could be confounded by differential trends based on baseline levels of schooling, including those due to mean reversion or catch-up. Specifically, we might be concerned that areas with lower access to public universities also had lower schooling and income at baseline. If these areas were simply catching up to higher access areas (with higher schooling and income at baseline), this would generate significant interaction terms that could bias our results. For schooling, this catch-up scenario would generate coefficients that are the opposite sign of our estimated coefficients (which would mean we are underestimating the true effect of the policy). For income, however, mean reversion would generate coefficients of the same sign as our estimated coefficients, which might mean they are driving the negative income effects. The results in Panel C are from a specification that includes interactions between cohort dummies and canton-level averages (from the 2001 census) of schooling, electricity share, and piped water share (income is not available). This allows for differential cohort trends based on pre-policy levels of schooling and development, both catch-up and dispersion. Our college and income conclusions remain the same, though some occupation regression coefficients become smaller in magnitude and insignificant.

In Table 5, we report the results of equation (2), which includes additional placebo individuals (aged 35 to 39 in 2008), and a more flexible exposure variable. We report the coefficients on the interactions between the relevant access variable and a fully-exposed indicator (for individuals aged 18 to 24 in 2008), a partially exposed indicator (for individuals aged 25 to 29 in 2008), and a placebo indicator (for individuals aged 35 to 39 in 2008), which leaves the unexposed group from the previous regressions (aged 25 to 29 in 2008) as the omitted category. Across almost all regressions, we see that the largest effects are coming from the youngest, fully exposed group, and that the partially exposed group occasionally shows some effects (which are generally smaller). Importantly, with only two exceptions, the placebo interaction coefficients are small and

Table 5. Placebo Checks

	(1) Attended College	(2) Attending School	(3) Log Income	(4) Employee	(5) Laborer	(6) Unpaid or Domestic	(7) White Collar 1	(8) Blue Collar 2
Exposed x Distance	-0.079***	-0.052***	0.13**	-0.054**	0.028**	0.017	-0.031**	0.057***
	(0.014)	(0.0075)	(0.059)	(0.021)	(0.011)	(0.014)	(0.013)	(0.020)
Partially Exposed x Distance	-0.059***	-0.019***	-0.0020	-0.048**	0.014	0.012	-0.020	0.030**
	(0.012)	(0.0042)	(0.052)	(0.022)	(0.012)	(0.011)	(0.012)	(0.014)
Placebo x Distance	0.012	-0.000053	-0.016	0.017	-0.0092	-0.012	-0.0041	-0.042**
	(0.012)	(0.0035)	(0.054)	(0.017)	(0.011)	(0.010)	(0.012)	(0.016)
Exposed x Number	0.022***	0.019***	-0.071***	0.0100	-0.0035	-0.0086**	0.0095**	-0.023***
	(0.0043)	(0.0021)	(0.019)	(0.0072)	(0.0048)	(0.0035)	(0.0036)	(0.0060)
Partially Exposed x Number	0.021***	0.0051***	-0.0055	0.017**	-0.00098	-0.0044	0.0095***	-0.014***
	(0.0035)	(0.0015)	(0.019)	(0.0074)	(0.0046)	(0.0033)	(0.0033)	(0.0047)
Placebo x Number	-0.0053	-0.0028**	-0.021	-0.0089	0.0036	0.0034	0.0014	0.0074
	(0.0051)	(0.0013)	(0.020)	(0.0058)	(0.0042)	(0.0030)	(0.0040)	(0.0047)
Province-Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	0.23	0.040	5.60	0.45	0.11	0.12	0.18	0.28
N	289226	289226	214639	240588	240588	240588	239173	239173

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1 ** p< 0.05 *** p< 0.01. Horizontal lines indicate separate regressions. These regressions restrict to individuals aged 18 to 39 in 2008, and at least 25 years old at the time of survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, "Partially Exposed"=1 for individuals0 for those aged 25 to 29 in 2008, and "Placebo"=1 for individuals aged 35 to 39 in 2008. The omitted category is for individuals aged 30 to 34. "Distance" is the distance (in 100 kilometers) between the individual's canton of residence and the nearest public university. "Number" is the number of public universities within 20km of the individual's canton of residence.

statistically insignificant, which is an indication of parallel trends across the access distribution for cohorts completely unaffected by the policy.

5.5 Heterogeneity

We next explore heterogeneity across gender, race, language, and birthplace characteristics. Table 6 reports this heterogeneity analysis using the distance variable, and Table 7 reports the heterogeneity analysis using the number of nearby universities. In each panel, we first report the difference-in-differences coefficient of interest (β) in two separate regressions, one for each of the sub-groups of interest, and then report the difference between the two.

Panel A of Table 6 illustrates that men and women responded similarly to the fee elimination. One exception is the income regression: the negative effect of the fee reduction on income (represented by a positive and significant coefficient) appears to be largely concentrated among men. There also do not appear to be substantial differences across race (in panel B), though where there are significant differences (in the effects on attending school and taking up a laborer job), it appears that white or mestizo individuals showed the largest responses.

In contrast, there is more evidence for heterogeneity across language and birthplace characteristics. Panel C shows that individuals who speak an indigenous language (who are more likely to be of native descent) were largely unaffected by the fee elimination. The positive effects of this policy change on college attendance and the likelihood of better jobs appear to be concentrated among those who do not speak an indigenous language. Because individuals with an indigenous background tend to be of lower socioeconomic status, this highlights that the elimination of university tuition could have actually exacerbated inequality.

This finding also shows up in panel D. Here, we compare the effects of the fee elimination on individuals from different socioeconomic backgrounds. While parental education is commonly used to capture family background, this variable is not available in ENEMDU. Instead, we use the "below median birthplace" indicator described in section 3.1. The first row in panel D reports regressions for individuals born in a canton that was deemed below-median in either the electricity or the piped water distribution. The second row in panel D reports regressions for the remainder of individuals (who were born in a canton that was above-median in terms of both characteristics). Once again, it is clear that the more disadvantaged individuals were largely untouched by the policy change. The positive effects on college enrollment and improved job opportunities are only present among individuals in the above-median group. In fact, one regression reveals a significant negative effect (at the 10% level) of the policy on indigenous-language individuals: in column 7, we see that the policy shifted these individuals out of white-collar jobs.

The results in Table 7 echo those reported in the previous table. There is limited evidence for differences across gender or race, but stark heterogeneity across language and socioeconomic background. Like in the above table, there is some evidence that groups of lower socioeconomic status may have actually been negatively affected by the policy: indigenous-language-speaking individuals are less likely to be in white-collar jobs and are more likely to be laborers (with both estimates significant at the 10% level).

The failure of the policy to benefit disadvantaged groups could be due to two factors. First, prior to the policy, individuals from poor households often received scholarships that helped alleviate the cost of college. The elimination of fees in 2008, therefore, may have resulted in a smaller price reduction for these individuals than their wealthier counterparts (though this price reduction could have still been large relative to total household income). Secondly, it is important to note that costs are not the only barrier to college enrollment. Individuals unable to complete high school and pass the university entrance exam are unable to take advantage of tuition-free college, and students from disadvantaged backgrounds are less likely to have the preparation needed to make it to university. For example, high school graduation rates are 15 percentage points (almost 40%) higher for individuals from above-median cantons compared to those born in below-median cantons. Even larger gaps are found across indigenous language status, where those who speak an indigenous language are only half as likely to complete high school as those who do not. These are important concerns that need to be kept in mind when thinking about tuition fee elimination as a means of decreasing inequality in access to college.

Table 6. Heterogeneous Effects of Tuition Fee Elimination, Using Distance to University

	(1) Attended College	(2) Attending School	(3) Log Income	(4) Employee	(5) Laborer	(6) Unpaid or Domestic	(7) White Collar 1	(8) Blue Collar 2
A. By Gender								
Male	-0.081***	-0.061***	0.22***	-0.061**	0.041**	0.015	-0.035**	0.063**
	(0.017)	(0.0092)	(0.065)	(0.027)	(0.017)	(0.017)	(0.016)	(0.027)
Female	-0.076***	-0.044***	0.00071	-0.040	0.0072	0.015	-0.027	0.037
	(0.019)	(0.0095)	(0.092)	(0.026)	(0.0099)	(0.023)	(0.021)	(0.025)
Difference	-0.0049	-0.016	0.21**	-0.021	0.034	-0.000015	-0.0085	0.026
	(0.023)	(0.011)	(0.10)	(0.032)	(0.021)	(0.028)	(0.026)	(0.034)
B. By Race								
White or Mestizo	-0.078***	-0.063***	0.12*	-0.070***	0.047***	0.024*	-0.047***	0.079***
	(0.015)	(0.0080)	(0.067)	(0.022)	(0.013)	(0.014)	(0.013)	(0.019)
Other	-0.069**	-0.019*	0.10	-0.028	-0.023	0.0081	0.00011	0.0070
	(0.028)	(0.011)	(0.074)	(0.033)	(0.017)	(0.024)	(0.026)	(0.020)
Difference	-0.0092	-0.044***	0.019	-0.041	0.069***	0.016	-0.047	0.072***
	(0.032)	(0.010)	(0.092)	(0.036)	(0.023)	(0.028)	(0.029)	(0.026)
C. By Language								
Speaks Indigenous	0.00038	0.022*	0.14	0.0025	-0.0085	0.015	0.056*	-0.020
	(0.021)	(0.013)	(0.18)	(0.052)	(0.026)	(0.038)	(0.032)	(0.043)
No Indigenous	-0.083***	-0.058***	0.13**	-0.063***	0.035***	0.021	-0.043***	0.075***
	(0.013)	(0.0071)	(0.063)	(0.020)	(0.012)	(0.013)	(0.011)	(0.017)
Difference	0.083***	0.080***	0.0093	0.065	-0.043	-0.0062	0.099***	-0.096**
	(0.024)	(0.015)	(0.19)	(0.054)	(0.029)	(0.041)	(0.033)	(0.046)
D. By Birthplace								
Below Median	0.0052	-0.031***	0.18**	0.038	0.0074	-0.0085	0.034	0.0021
	(0.025)	(0.0099)	(0.085)	(0.033)	(0.016)	(0.024)	(0.021)	(0.040)
Above Median	-0.11***	-0.045***	0.25***	-0.080***	0.045**	0.030*	-0.054***	0.084***
	(0.018)	(0.0097)	(0.093)	(0.028)	(0.021)	(0.017)	(0.016)	(0.023)
Difference	0.12***	0.014	-0.076	0.12***	-0.038	-0.038	0.088***	-0.082*
	(0.031)	(0.014)	(0.13)	(0.040)	(0.027)	(0.028)	(0.027)	(0.045)
Province-Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	0.29	0.062	5.67	0.53	0.093	0.099	0.21	0.26
N	101360	101360	75836	83487	83487	83487	82894	82894

Notes: Standard errors, clustered at the canton level, are in parentheses. * $p< 0.1$ ** $p< 0.05$ *** $p< 0.01$. Each panel reports the "Exposed x Distance to Nearest Public University" interaction for 2 regressions, conducted for each of the specified groups separately, as well as the difference between the two coefficients. These regressions restrict to individuals aged 18 to 24 or 30 to 34 in 2008, and at least 25 years old at the time of survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. All regressions control for gender and cohort, canton, and age and year fixed effects. "Below Median" refers to individuals born in a canton that was in the bottom half of the canton-level distribution of electricity and piped water access (during the census preceding their birth).

Table 7. Heterogeneous Effects of Tuition Fee Elimination, Using Number of Nearby Universities

	(1) Attended College	(2) Attending School	(3) Log Income	(4) Employee	(5) Laborer	(6) Unpaid or Domestic	(7) White Collar 1	(8) Blue Collar 2
A. By Gender								
Male	0.017***	0.021***	-0.089***	0.012	-0.0065	-0.0064*	0.0076**	-0.017**
	(0.0043)	(0.0027)	(0.028)	(0.0081)	(0.0081)	(0.0034)	(0.0038)	(0.0076)
Female	0.028***	0.017***	-0.043	0.0078	-0.0000079	-0.011*	0.014**	-0.031***
	(0.0057)	(0.0029)	(0.036)	(0.0099)	(0.0035)	(0.0064)	(0.0057)	(0.0078)
Difference	-0.011*	0.0040	-0.046	0.0041	-0.0065	0.0046	-0.0068	0.014
	(0.0058)	(0.0036)	(0.050)	(0.011)	(0.0089)	(0.0069)	(0.0061)	(0.0099)
B. By Race								
White or Mestizo	0.020***	0.020***	-0.069***	0.011	-0.0082*	-0.0077*	0.011***	-0.025***
	(0.0041)	(0.0023)	(0.022)	(0.0078)	(0.0048)	(0.0040)	(0.0040)	(0.0066)
Other	0.022**	0.0088**	-0.071**	0.0064	0.019*	-0.011	0.0020	-0.010
	(0.0086)	(0.0042)	(0.032)	(0.011)	(0.0100)	(0.0095)	(0.0061)	(0.0096)
Difference	-0.0014	0.011**	0.0025	0.0043	-0.027***	0.0035	0.0094	-0.015
	(0.0087)	(0.0044)	(0.038)	(0.0098)	(0.010)	(0.011)	(0.0070)	(0.011)
C. By Language								
Speaks Indigenous	-0.0029	-0.0066	0.0019	-0.00076	0.022*	-0.019	-0.015*	0.011
	(0.0085)	(0.0061)	(0.053)	(0.012)	(0.013)	(0.019)	(0.0085)	(0.017)
No Indigenous	0.022***	0.020***	-0.076***	0.0095	-0.0059	-0.0077**	0.011***	-0.026***
	(0.0040)	(0.0021)	(0.020)	(0.0072)	(0.0051)	(0.0034)	(0.0036)	(0.0063)
Difference	-0.025***	-0.026***	0.078	-0.010	0.028**	-0.011	-0.026***	0.038**
	(0.0087)	(0.0064)	(0.055)	(0.013)	(0.014)	(0.020)	(0.0091)	(0.018)
D. By Birthplace								
Below Median	-0.0027	0.017***	-0.041	-0.014	0.013	-0.0060	-0.0062	-0.00042
	(0.0055)	(0.0041)	(0.037)	(0.012)	(0.0081)	(0.0082)	(0.0066)	(0.012)
Above Median	0.026***	0.014***	-0.078**	0.016	-0.0090	-0.011**	0.012**	-0.030***
	(0.0058)	(0.0025)	(0.032)	(0.011)	(0.0074)	(0.0049)	(0.0053)	(0.0082)
Difference	-0.029***	0.0027	0.037	-0.030*	0.022*	0.0050	-0.018**	0.029**
	(0.0081)	(0.0053)	(0.047)	(0.018)	(0.011)	(0.0091)	(0.0085)	(0.014)
Province-Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	0.29	0.062	5.67	0.53	0.093	0.099	0.21	0.26
N	101360	101360	75836	83487	83487	83487	82894	82894

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1 ** p< 0.05 *** p< 0.01. Each panel reports the "Exposed x Number of Public Universities within 20km" interaction for 2 regressions, conducted for each of the specified groups separately, as well as the difference between the two coefficients. These regressions restrict to individuals aged 18 to 24 or 30 to 24 in 2008, and at least 25 years old at the time of survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. All regressions control for gender and cohort, canton, and age and year fixed effects. "Below Median" refers to individuals born in a canton that was in the bottom half of the canton-level distribution of electricity and piped water access (during the census preceding their birth).

6 Conclusion

In this paper, we evaluate the effects of an Ecuadorian policy that eliminated university tuition fees in 2008. Using a difference-in-differences strategy to identify the effects of the policy, we find that it increased college enrollment, shifted individuals into less physical jobs, but had no effects on income. The failure of the policy to boost income could be due to general equilibrium effects relating to the oversupply of college-educated workers, or occupation mismatch and overeducation.

The elimination of tuition fees does not appear to have promoted equality in access to college, which was a major motivation for the policy. In fact, those of higher socioeconomic status appear to have benefited the most. This could be due to the fact that poorer individuals were able to avail of scholarships even before the policy (which made the fee elimination a smaller price reduction for them). Perhaps more importantly, however, tuition fees are not the only barriers to a college education, and those with a socioeconomic disadvantage may have more important obstacles to overcome before they are able to attend university – even if it is free.

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A Appendix

Table A1. Effects of Tuition Fee Elimination on Labor Market Outcomes for Ages 30 and Older

	(1) Log Income	(2) In Labor Force	(3) Employee	(4) Self-Employed	(5) Laborer	(6) Unpaid or Domestic	(7) White Collar 1	(8) White Collar 2	(9) Blue Collar 1	(10) Blue Collar 2
Exposed x Distance	0.0048	0.0031	-0.053**	0.022	0.034**	-0.0033	-0.043**	0.031*	0.017	-0.0050
	(0.072)	(0.014)	(0.024)	(0.019)	(0.016)	(0.018)	(0.017)	(0.019)	(0.018)	(0.023)
Exposed x Number	-0.031	-0.0015	0.0018	-0.00095	0.0019	-0.0028	0.014***	0.0069	-0.014**	-0.0063
	(0.026)	(0.0053)	(0.0091)	(0.0067)	(0.0059)	(0.0045)	(0.0045)	(0.0059)	(0.0065)	(0.0074)
Province-Age Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	5.63	0.85	0.43	0.34	0.10	0.12	0.17	0.25	0.29	0.28
N	74418	98679	83235	83235	83235	83235	82649	82649	82649	82649

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1 ** p< 0.05 *** p< 0.01. Each cell represents a different regression. These regressions restrict to individuals aged 18 to 24 or 30 to 24 in 2008, and at least 30 years old at the time of survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. "Distance" is the distance (in 100 kilometers) between the individual's canton of residence and the nearest public university. "Number" is the number of public universities within 20km of the individual's canton of residence. All regressions control for gender and cohort, canton, and age and year fixed effects.

Table A2. Effects of Tuition Fee Elimination on Educational Outcomes for Non-Migrants

	(1) Attended College	(2) 4 Years of College	(3) Attending School	(4) Attended College	(5) 4 Years of College	(6) Attending School
Exposed x Distance	-0.073***	-0.059***	-0.029***	-0.083***	-0.054***	-0.056***
	(0.014)	(0.012)	(0.0067)	(0.015)	(0.012)	(0.0077)
Exposed x Number	0.020***	0.014***	0.012***	0.025***	0.017***	0.019***
	(0.0039)	(0.0034)	(0.0037)	(0.0046)	(0.0032)	(0.0023)
Province-Age Fixed Effects	No	No	No	Yes	Yes	Yes
Dep. Var. Mean	0.24	0.18	0.054	0.24	0.18	0.054
N	144007	141851	144007	144007	141851	144007

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1 ** p< 0.05 *** p< 0.01. Each cell represents a different regression. These regressions restrict to individuals aged 18 to 24 or 30 to 24 in 2008, at least 25 years old at the time of survey, and who had not moved cantons between 2008 and the current survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. "Distance" is the distance (in 100 kilometers) between the individual's canton of residence and the nearest public university. "Number" is the number of public universities within 20km of the individual's canton of residence. All regressions control for gender and cohort, canton, and age and year fixed effects.

Table A3. Effects of Tuition Fee Elimination on Labor Market Outcomes for Non-Migrants

	(1) Log Income	(2) In Labor Force	(3) Employee	(4) Self-Employed	(5) Laborer	(6) Unpaid or Domestic	(7) White Collar 1	(8) White Collar 2	(9) Blue Collar 1	(10) Blue Collar 2
Exposed x										
Distance	0.15**	0.029**	-0.058**	0.0032	0.030**	0.025	-0.033***	-0.0052	-0.036**	0.074***
	(0.062)	(0.014)	(0.023)	(0.016)	(0.012)	(0.016)	(0.013)	(0.015)	(0.017)	(0.022)
Exposed x										
Number	-0.077***	-0.0070	0.013*	0.00069	-0.0027	-0.011***	0.011***	0.012**	0.0056	-0.028***
	(0.022)	(0.0043)	(0.0076)	(0.0044)	(0.0051)	(0.0039)	(0.0038)	(0.0054)	(0.0060)	(0.0067)
Province-Age										
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. Var. Mean	5.57	0.83	0.46	0.30	0.11	0.13	0.18	0.25	0.28	0.29
N	105094	144007	118876	118876	118876	118876	118443	118443	118443	118443

Notes: Standard errors, clustered at the canton level, are in parentheses. * $p< 0.1$ ** $p< 0.05$ *** $p< 0.01$. Each cell represents a different regression. These regressions restrict to individuals aged 18 to 24 or 30 to 24 in 2008, at least 25 years old at the time of survey, and who had not moved cantons between 2008 and the current survey. "Exposed" is equal to 1 for individuals who were aged 18 to 24 in 2008, 0 for those aged 30 to 34 in 2008. "Distance" is the distance (in 100 kilometers) between the individual's canton of residence and the nearest public university. "Number" is the number of public universities within 20km of the individual's canton of residence. All regressions control for gender and cohort, canton, and age and year fixed effects.