# The Schooling and Labor Market Effects of Eliminating University Tuition in Ecuador*

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November 18, 2020

**Abstract**

This paper estimates the effects of a 2008 policy that eliminated tuition fees at public universities in Ecuador. We use a difference-in-differences 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 and shifted people into higher-skilled jobs. We detect no statistically significant effects on income. Overall, the bulk of the benefits of this fee elimination were enjoyed by those of higher socioeconomic status.

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

*We thank Achyuta Adhvaryu, Jenny Aker, Arjun Bedi, Pascaline Dupas, Jun Goto, Gabriela Izurieta, Gaurav Khanna, Anant Nyshadham, Robert Sparrow, John Strauss, and seminar participants at LACEA-LAMES, Loyola Marymount University, Hawaii-Kobe Applied Econometrics Conference, Tinbergen Institute, Pac-Dev, and UH Manoa for helpful input. All errors are our own.

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# 1 Introduction

Across the globe, there is growing interest in the complete elimination of university tuition fees as a tool to expand and equalize access to tertiary education. In the United States, several candidates for the 2020 presidential election called for tuition-free college nationwide (Yglesias, 2019; Harris, 2019). Almost 20 states have adopted or are considering adopting some form of tuition-free tertiary education (CNBC, 2019). While there is an expanding literature that evaluates these "place-based" or "promise" programs in the U.S. (Bifulco et al., 2019; Gurantz, 2020; Andrews et al., 2010; Carruthers and Fox, 2016), empirical evidence on nationwide policies (which have been implemented recently in the Philippines and Chile) is lacking.

Even though these types of policies are often proposed as a way to reduce socioeconomic inequalities, it is not clear who would benefit most from a nationwide reduction in university tuition. On the one hand, binding credit constraints could mean that eliminating fees would allow lower-income students, who would have otherwise been unable, to enroll. On the other hand, if lower-income students are less likely to be able to attend college even with free tuition (if, for example, they are less likely to have a high school degree), a nationwide tuition elimination would disproportionately benefit higher-income students. For developing countries in particular, we know very little about how to effectively reduce inequalities in higher education in general, let alone whether tuition fee eliminations would succeed in this domain.[1]

This paper aims to shed light on these issues by evaluating an Ecuadorian policy that eliminated tuition fees at all public universities in 2008. We use a difference-in-differences strategy that compares 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. In a setting where migrating for university is uncommon, our second source of variation comes from geographic access to public universities: the distance between an individual's canton of residence and their closest public university in 2008.

Using both an event study and difference-in-differences analysis, we find that the policy increased college enrollment and affected job type, shifting people into high-skill white-collar jobs. In event

[1]A large number of studies evaluate the effects of policies designed to increase college attainment among the poor in the United States (David and Dynarski, 2009), but these policies are much more targeted than the one we evaluate in this paper. Evidence from lower-income countries is scarce: in a review of 75 studies that evaluate the effects of various higher education policies on disadvantaged students, only four are conducted outside of high-income countries (Herbaut and Geven, 2020).

study regressions, the coefficients demonstrate a non-linear pattern across cohorts that is consistent with the age distribution of university students in Ecuador, as opposed to a linear pattern (which would be suggestive of differential cohort trends for reasons unrelated to the policy).

There is a large literature that evaluates the effects of various financial aid policies on college enrollment and labor market outcomes in developed countries.[2] We contribute specifically to a much smaller body of evidence from lower-income countries, which so far has focused on studying the link between access to credit and college enrollment (Solis, 2017; Gurgand et al., 2011). In both developed and developing countries, empirical evidence on nationwide tuition reductions is lacking.

We uncover substantial heterogeneity in the effects of the fee elimination across socioeconomic status. 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 those born in poor areas saw no improvements in college enrollment or changes in job type, which likely reflects the fact that high school completion rates (and college preparedness in general) are much lower in these disadvantaged groups. This finding is consistent with the results of Bucarey (2018), which uses reduced form and structural estimates from Chile's expansion of scholarship eligibility to predict that free tuition policies would adversely affect low-income students.

Most of our knowledge about the effects of reducing education fees in the developing world has come from studying fee reductions at the primary or secondary school level. Though evaluations of these policies have generally found improvements in 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 (Garlick, 2017; Osili and Long, 2008; Keats, 2018). More importantly, these studies may not provide much guidance for university-level policies, given the different returns to tertiary education (Psacharopoulos and Patrinos, 2018), as well as the different opportunity costs.

[2]See, for example, Bound and Turner (2002); Dynarski (2002, 2003); Stanley (2003); Fack and Grenet (2015); Turner and Bound (2003); Abraham and Clark (2006); Cornwell et al. (2006); Angrist et al. (2014); Barr (2019); Angrist (1993); Angrist and Chen (2011); Scott-Clayton and Zafar (2019); Denning et al. (2019); Bettinger et al. (2019).

## 2 Background

Before 2008, both public and private universities in Ecuador charged fees, and each university set their own application process and acceptance criteria. Public tuition fees varied widely both across and within universities, with fees ranging from 250 USD per year (for a "traditional"' major in a large university) to 1500 USD per year (for a non-traditional major at a smaller university), for a student without any scholarships in 2007.[3]

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 established that the state would provide quality public education (including tertiary education) free of charge.[4] Starting in October of 2008, students (including those already enrolled) no longer had to pay tuition fees. 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) and Hora 25 (2017) for more details on the policy.) In the years that followed, a number of other changes were made to the education system. As we discuss in detail in Appendix section A.2, these changes either only affected a tiny share of students (for example, government scholarships with living stipends) or else primarily affected cohorts who were younger than the sample we study in this paper (for example, the closure of low-quality universities in 2012).

## 3 Data

We use the National Survey of Employment, Unemployment, and Underemployment (ENEMDU), conducted quarterly. We use all four (nationally representative) quarters of the 2014 to 2017 surveys, by when individuals of college-going age in 2008 were old enough to be in the labor market.

ENEMDU provides information on respondents' educational attainment, income, labor force

[3]Traditional fields, like science and engineering, often had lower fees than newer non-traditional fields, such as business-related majors. Because no national-level data on tuition fees exists prior to 2008, we obtain specific examples from internet archives and conversations with university administrators. For reference, median annual household income was approximately 3000 USD in 2007.

[4]Tertiary education in Ecuador consists of university-level education and "non-university" education at technological and technical institutes. The policy applied to all types of tertiary education institutions, but because enrollment at non-university institutes comprises such a small share of overall post-secondary enrollment (ranging between 3-6% between 2003 and 2013), we focus in this paper on university-level education.

participation, and occupation. We generate a college attendance indicator, equal to 1 for individuals whose highest level of education is university-level tertiary education or higher. While the survey does not ask respondents whether they have a college degree, it does ask for the number of years spent at each level of schooling, from which we generate an imperfect proxy for college completion: an indicator for those who attended at least 4 years of college. Another education outcome of interest is an indicator for individuals currently attending school (at the time of survey).

The survey also asks about labor force participation and income, which is missing for those who are not in the labor force. The income variable captures labor income from a worker's primary and secondary occupation in the previous month: wages for employees and profits for self-employed workers. Individuals also report 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 (4 and 5), high-skill blue-collar (6 and 7), and lower-skill blue-collar (8 and 9).

ENEMDU also records respondents' current residence and place of birth at the level of the canton, which is the administrative division just below the province. There are 225 cantons in Ecuador, with an average area of approximately 1,000 square kilometers and average population size of approximately 70,000 people (as of 2010).

Respondents report how long they have lived in their current canton of residence, and the canton from which they have most recently migrated. We use this information to determine the canton in which an individual was living in 2008. For individuals who have been living in their current canton of residence since 2008 or earlier, their 2008 canton is simply their current canton of residence. For the remaining individuals, we first record their 2008 canton as the canton from which they migrated. This will only be an accurate assignment if individuals did not migrate in between 2008 and their most recent migration date. This is likely to be the case for those whose most recent migration date is soon after 2008, but less clear for those with later migration dates. Therefore, we assume that recent migrants lived in their last canton of residence for at least five years, and consider the 2008 canton variable missing for those who migrated to their current canton after 2012. This restricts our entire analysis to individuals who migrated to their current canton of residence in 2012 or earlier, who make up 96% of the original ENEMDU. Our results are not sensitive to this choice of 2012 as the cutoff year.

We link individuals to universities using their 2008 canton of residence and a list of the 68 universities that were operating in Ecuador in 2008. For each of these universities, we collected information on the type (public or private) and the canton in which they were located. Using the GPS codes of each canton, we calculate the distance between an individual's 2008 canton and the canton of the nearest public university. By construction, distance is equal to zero for individuals who (in 2008) were living in a canton in which a university was located.[5]

We also use 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 that captures 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 generate a "below-median birthplace" indicator, equal to one for individuals whose canton of birth was in the bottom half of either the electricity or piped water distribution in the relevant census year.

Column 1 of Table 1 reports summary statistics for individuals younger than 40 in 2008, with a non-missing 2008 canton, who are at least 30 years of age when they are surveyed (in 2014 to 2017). We restrict to those aged 30 and older because we are interested in labor market outcomes, and by age 30, over 95% are out of school.[6] These restrictions mean that individuals in the sample were aged 21 to 39 in 2008, and aged 30 to 48 at the time of survey. In addition to summary statistics for the full sample, Table 1 reports statistics for specific cohorts and sub-groups, which we discuss in conjunction with our empirical strategy in the following section.

# 4 Empirical Strategy

In the existing work looking 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

[5] Because the distance distributions are right-skewed due to the Galapagos Islands, we winsorize the distance variables at the 99th percentile. For people who were living in a different country in 2008 (less than 2% of the sample), we also assign the 99th percentile. Results are almost identical when we instead drop those in the Galapagos or abroad.

[6] In the event study analysis looking at college attendance only, however, we relax this age 30 restriction.

over time (Post, 2011; Ponce and Loayza, 2012; Acosta, 2016), making it impossible to separate the effects of the policy from broader time trends or cohort trends. We overcome these limitations by using the difference-in-differences strategy described in this section, and expand the analysis with more recent data to estimate longer-run labor market effects.

To evaluate the effects of the 2008 elimination of tuition fees, we use an event-study analysis as well as a generalized difference-in-differences strategy. Because of our interest in labor market outcomes, we restrict most of our analysis to individuals at least 30 years old at the time of survey (with the exception of the college attendance event study analysis).

For both strategies, we compare the outcomes of those young enough to be affected by the policy to those past college-going age when the policy was implemented, across areas with differential access to public universities (where access is defined as distance to the nearest public university). Migrating for university is very uncommon in Ecuador: of all students attending university in 2007, 95% have lived in their current place of residence for at least five years. Thus, the underlying intuition is that the policy change should be relevant for those living near a public university but not for those living far away.

### 4.1 Event Study Analysis

We estimate the following specification, for individual $i$, who was aged $c$ and living in canton $j$ in 2008:

$$Y_{ijc} = \sum_{k=15}^{39} \beta_k 1(c = k) \times \text{Distance}_j + \mu_c + \delta_j + \epsilon_{ijc}. \tag{1}$$

Cohort ($\mu_c$) and canton ($\delta_j$) fixed effects account for any cohort-specific unobservables (that are fixed over cantons) and any canton-specific unobservables (that are fixed over cohorts). Our variables of interest are the interactions between each of the cohort dummies and $\text{Distance}_j$, which represents an individual's distance to a public university in 2008. The coefficient on a given interaction will inform us how the distance gradient in the outcomes for that particular cohort compares to the distance gradient in the omitted cohort category (age 32). If the policy had a positive effect on an outcome, we would expect a steeper negative distance gradient for younger age cohorts, for whom the policy change was more relevant.

## 4.2 Difference-in-Differences

In addition to the event study analysis, we also estimate a simpler difference-in-differences specification. The parsimony increases statistical power and ease of interpretation, which is especially important when analyzing heterogeneity across groups. We restrict to individuals aged 30 and older because we are interested in labor market outcomes, but this restriction has the additional advantage of ensuring that our sample individuals were largely unexposed to the additional tertiary education reforms made in 2012 or later (described in section A.2).

For college or labor market outcome $Y_{ijc}$ of individual $i$, living in canton $j$, who was aged $c$ in 2008,

$$Y_{ijc} = \beta \text{Exposed}_c \times \text{Distance}_j + \mu_c + \delta_j + \epsilon_{ijc}. \tag{2}$$

Here, $\text{Exposed}_c$ is an indicator equal to 1 for those 24 or younger in 2008 – "young enough" to be affected by the policy. As we show in Appendix Figure A1, this is the 75th percentile of age among university students in 2007. The policy would have affected the college continuation decisions of people in this age group who were already in college in 2008. In addition, the policy could have also motivated those in this age group who were not in college to go back to college: they would have been in an early stage of their careers and of similar age to the general university student population. Because we are restricting to people aged at least 30 at the time of survey, our youngest cohorts in this group were 21 in 2008.

$\text{Exposed}_c$ is equal to 0 for individuals past college-going age (ages 30 to 34 in 2008). In 2007, less than 5% of those aged 30 and older were attending university (see Figure A2). This variable is missing for those in between, for whom the relevance of the policy is more ambiguous.[7] In other words, this regression restricts to individuals aged 21 to 24 or 30 to 34 in 2008.

In this specification, a negative $\beta$ would indicate that the policy had a positive effect on the outcome of interest, as this would represent a steeper (negative) distance gradient for those young

[7] These people would have been more advanced in their careers by 2008, but if they did decide to go back to school, they would not be substantially different from the median (and very close to the 75th percentile) age student.

enough to be exposed to the policy. In all regressions, canton fixed effects ($\delta_j$) control for time-invariant unobservables that vary at the canton-level and might drive our outcomes of interest. Cohort fixed effects ($\mu_c$) control for non-linear trends across cohorts in our outcomes of interest. In later specifications, we also add province-by-cohort fixed effects to allow for different cohort trends across provinces. In all regressions, we control for gender, age, and survey wave (year-by-quarter) fixed effects. We run this specification for the full sample and then repeat it for separate groups defined by gender, race, knowledge of an indigenous language, and birthplace. While our main specification uses a continuous distance variable, we also use a binary distance variable (equal to one for cantons that have a public university) to allow for a non-linear relationship.

The identifying assumption is that the difference between exposed and unexposed cohorts would show no systematic variation across the $\text{Distance}_j$ distribution, in the absence of the policy. The event-study analysis will allow us to detect if there were any differential distance gradients across cohorts aged 30 and older in 2008. In addition, to ensure that the $\text{Distance}_j$ variable is not simply proxying for other canton-level characteristics that could be driving differential trends across cohorts, we run a number of robustness checks that add cohort fixed effects interacted with various canton-level characteristics.

Because our identification strategy relies on comparing cohort trends across individuals living different distances from a public university in 2008, we explore whether there are any systematic differences across individuals in different distance groups, among cohorts who were not exposed to the policy. Similar to a balance test of pre-intervention characteristics, the results of this exercise are reported in Table 1, where the second column onward restricts to cohorts not exposed to the policy. Column 2 reports means and standard deviations for unexposed individuals who were living between 25-50 km from a public university in 2008, the middle of a total of 5 distance bins. Columns 3 through 6 report the differences between this middle group and the four remaining distance groups: (unexposed) individuals living in the same canton as a public university, less than 25 km (but not in the same canton), 50-100km, and more than 100 km from a public university.

Those living in a canton with a public university are significantly different from those in the middle distance bin across most characteristics – the former are more highly educated, earn more income, have more skill-intensive jobs, and are more likely to come from well-off cantons. Interestingly, those living more than 100 km from a public university are also better off on some of these dimensions (this appears to be driven by the Galapagos Islands, as well as individuals who were living outside of the country in 2008). However, across the three middle distance bins, characteristics appear to be quite balanced.

Although identification does not require these groups to be the same (we include canton fixed effects and therefore only require that the cohort *trends* would have been similar in the absence of the policy), the relatively balanced characteristics across the three middle distance groups suggests that violations of the parallel trends assumption are less likely for individuals in these groups. Therefore, as a robustness check, we repeat our analysis restricting to these three middle distance categories.

## 5 Results

We begin with the event study analysis described by equation (1). In Figure 1, we plot the cohort-specific coefficients (and 95% confidence intervals) on each of the cohort-by-distance interactions. Because distance is negatively associated with college attendance overall, a negative coefficient for a given age cohort indicates that the difference between those living far and close to a public university is larger for that particular age cohort than for the cohort aged 32 in 2008, which is the omitted category (the median age of the "unexposed" cohorts defined by specification 2).

The coefficients for all cohorts who were aged 24 or younger in 2008 – young enough to be affected by the policy – are negative and statistically significant. Geographic access to public universities (distance) matters more for these cohorts than for those aged 32 in 2008. There appears to be a linear increase in the magnitudes moving from age 24 down to age 19, and then a flattening out after age 19. This is consistent with the fact that most people start university around age 19. Individuals older than this in 2008 should be slightly less affected (with this effect decreasing with age), while those younger than this should not necessarily be more affected (given that all of them are equally and fully exposed to the policy).

For cohorts aged 25 to 30, coefficient estimates are all negative, though generally smaller in magnitude, with only two significantly different from zero. Similarly, for cohorts aged 31 to 39 in 2008, all coefficients are positive but small in magnitude. Within each set of cohorts just described (25-29 and 31-39), there does not appear to be any increasing or decreasing trend across cohorts.

In sum, the policy seems to have had some effect on those in the ambiguous age range of 25 to 29, but no effect on those who were older than college-going age when the policy was implemented. Appendix Figure A3 displays the age cohort coefficients from an event study specification that adds province-by-cohort fixed effects. Though noisier, the estimates show a similar pattern.

We interpret these results as compelling evidence that the policy increased college attendance. In the remainder of the paper, we focus on individuals aged 30 and older at the time of survey and examine both education and labor market outcomes.

Table 2 reports our difference-in-differences estimates, first without and then with province-by-cohort fixed effects. In both specifications for college attendance, we report negative coefficients that are significant at the 1% level. Consistent with the event study analysis, these results indicate that free tuition significantly increased college attendance. Living 40km (approximately one standard deviation) further away from a public university is associated with an effect size of 2 percentage points (10% of the mean).

In addition, the fee elimination increased the likelihood of staying in college for at least four years (an imperfect proxy for college completion). There is also evidence that the policy increased the probability of attending school at the time of survey, although this is only significant with the inclusion of province-by-cohort fixed effects.

The fourth column of Table 2 reports the result of an important falsification test. Because our sample should have been done with high school by the time of the 2008 policy change, we should not see any effects of the policy on high school completion. However, if the significant estimates in columns 1 to 3 were driven by different trends (across cohorts, by distance) due to reasons other than the policy, we would likely also see significant coefficients in a regression on high school completion. Column 4 of Table 2 reveals no significant coefficients for either of the high school completion regressions.

Having established that the tuition fee elimination significantly increased college enrollment, we next ask how it affected labor market outcomes. Column 5 shows that the policy significantly increased the take-up of the highest-skilled white-collar jobs (legislators, managers, professionals, and technicians). A one-standard deviation change in distance corresponds to an effect size of about one percentage point (7% of the mean). In column 6, the more rigorous specification in panel B suggests that this may have been driven primarily by individuals shifting out of lower-skilled white-collar jobs (clerks and service, shop, and market workers).

The policy did not affect labor force participation (column 9), which suggests the policy affected the job choices of those already in the labor force. We do not detect any significant effects on income (conditional on being in the labor force). Given the magnitudes of the college enrollment effects, we would have needed substantial statistical precision to detect any significant income effects. Nevertheless, we note that the sign of the income coefficient in our preferred specification (Panel B) is inconsistent with the policy having a positive effect.[8]

Complementing this simple difference-in-differences strategy, Figure 2 plots the results of event study regressions for the two white-collar variables that were significantly impacted (using the specification with province-by-cohort fixed effects). Because we are restricting to those aged 30 and older, these event studies have smaller sample sizes (and fewer young age cohorts) than the college attendance analysis in Figure 1, but they are still informative because they provide us with the pattern of the cohort-specific coefficients. In addition, we expand the age cohort window to include cohorts up to age 39 in 2008, which allows us to detect potential pre-trends. This analysis is also valuable because it does not rely on the classifications of age cohorts into exposed and unexposed categories.

In the first panel, which reports the high-skill white-collar regression, there is a flat trend for the age cohorts 30 to 39, a slight shift downward for the age cohorts 23 to 30 (though many coefficients are close to zero), and larger drops moving to age cohorts 22 and then 21 (significantly different from zero). This is very similar to the pattern depicted in Figure 1 and offers strong evidence that the policy increased participation in these high-skill white-collar jobs. The lower-skill white-collar figure does appear to be the mirror image of the high-skill white-collar one, though the pattern is less sharp.

Appendix Figure A4 confirms the null effects on labor force participation and income reported in the previous table. The vast majority of coefficients are statistically indistinguishable from zero and do not exhibit any upward or downward trend in either the younger cohorts (indicating no effect

[8]We could be underestimating the income effect because those affected by the policy are more likely to still be attending school (column 3 of Table 2), potentially working in lower-paying temporary jobs. However, this is relevant for only a small share of the sample (only 3% are still attending school). We could also be underestimating the effect because people who spend more time in school (due to the fee elimination) have less work experience. However, workers in their twenties have the steepest returns to experience (proxied by age), as shown in Appendix Figure A5, which means this problem is less relevant for our sample of workers 30 and older.

of the policy) or the older cohorts (indicating no significant pre-trends). In sum, these event studies provide strong evidence that the tuition-free policy shifted workers into higher-skilled (white-collar) jobs, even though it had no effect on labor force participation or income.

In the appendix, we conduct a number of robustness checks to address potential threats to identification (discussed in detail in section A.3). In Table A5, we estimate several specifications that allow for differential trends based on various characteristics. First, we include distance to the nearest large metropolitan area (either Guayaquil or Quito) interacted with cohort fixed effects. Next, we include interactions between cohort dummies and canton-level averages of schooling (from the 2001 census) to allow for differential cohort trends based on pre-policy levels of schooling (both catch-up and dispersion). Finally, we include cohort dummy interactions with distance to nearest private university, and distance to nearest public technical institute. Across all specifications, the pattern of coefficient estimates remains largely unchanged.

We allow for the possibility of non-linearities by replacing our continuous distance measure with a binary variable equal to one for those living in the same canton as a public university. The gap in college attendance and high-skill white-collar jobs between exposed and non-exposed cohorts is significantly larger in cantons with a public university, which is consistent with our results using continuous distance.

We also explore alternate sample restrictions in Table A6. We show that our results are not sensitive to the decision of using 2012 as the migration cutoff date for considering the 2008 canton variable to be missing. Our conclusions still hold when we restrict to the middle distance categories, which were shown in Table 1 to have similar observables across distance groups.

Our results in Table A7 indicate that endogenous migration is unlikely to be a threat to identification: the exposed-by-distance interaction is not a significant predictor of migration. This table also shows that our policy variable of interest appears to be uncorrelated with changes in sample composition more generally (measured by gender, grade, language, and birthplace).

### 5.1 Heterogeneity

We next explore heterogeneity across gender, race, language, and birthplace characteristics. In each panel of Table 3, we first report the difference-in-differences coefficient of interest ($\beta$) 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 3 illustrates that men and women were affected similarly by the fee elimination. In Panel B, there do not appear to be substantial differences across race. Though some effects are larger for women (in Panel A) and the white and Mestizo group (in Panel B), there are no statistically significant differences across gender or race groups.

There is stronger 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.

We find similar results when we compare the effects of the fee elimination on groups of different socioeconomic backgrounds. The first row in panel D reports regressions for those born in a "below-median birthplace" (as described in section 3), and the second row for all others. Once again, the positive effects on college enrollment and improved job opportunities are only present among the above-median group.

Appendix Tables A1 to A4 show that the conclusions from this heterogeneity analysis are robust to the inclusion of the additional fixed effects and various sample restrictions conducted for the overall results.

## 6 Discussion

Using event study and difference-in-differences strategies, this paper evaluates the effects of an Ecuadorian policy that eliminated public university tuition in 2008. We find that it increased college enrollment and the take-up of high-skill white-collar jobs, primarily for those of higher socioeconomic status.

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 faced lower fees.[9] The elimination of

[9] These financial support policies were university-specific. For example, a student from the lowest income category was charged one-third of the fee paid by students from the highest income category at the University of Guayaquil

fees in 2008, therefore, may have resulted in a smaller price reduction for this group, though this price reduction could have still been large relative to total household income.

Second, and perhaps more importantly, tuition costs are not the only barrier to college enrollment. Those who do not 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. Similarly, those who speak an indigenous language are only half as likely to complete high school as those who do not.

For these disadvantaged groups, designing policies that ensure their access to a quality secondary school education would be an important first step to enabling them to benefit from tuition-free university. Without first ensuring equality in access to a quality secondary school education, policies at the tertiary level may be limited in their ability to impact inequality. For these reasons, tuition elimination policies would likely be more successful at promoting equality in countries where secondary school graduation rates are higher and more uniform across the socioeconomic distribution.

We find no evidence that the fee elimination increased income, though we acknowledge that we lack the statistical precision to uncover even large effects on income. Of course, this policy could still generate larger benefits in the longer run. For example, positive income effects might show up several years from now, if the college education of the affected individuals (who have taken up higher-skilled jobs) generates steeper wage trajectories over the course of their careers. In addition, if parents increase educational investments for young children because of the promise of free university, and if this response is strongest in socioeconomically disadvantaged households, the policy could also help promote equality in the future. In the decade after its implementation, however, the main beneficiaries of this policy were not the most disadvantaged individuals.

in 2007.

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# Tables and Figures

**Table 1.** Summary Statistics

| | All Cohorts | Not Exposed Cohorts (Aged 30-39 in 2008) | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Overall mean (SD) | 25-50km mean (SD) | Same Canton Diff (SE) | <25km Diff (SE) | 50-100km Diff (SE) | >100km Diff (SE) |
| Distance to Public University (in 100km) | 0.25 | 0.35 | -0.35*** | -0.20*** | 0.38*** | 0.97*** |
| | (0.40) | (0.06) | (0.01) | (0.02) | (0.02) | (0.07) |
| Attended College | 0.21 | 0.11 | 0.14*** | -0.00 | 0.01 | 0.02 |
| | (0.41) | (0.31) | (0.02) | (0.01) | (0.02) | (0.02) |
| Attended 4 Years of College | 0.14 | 0.08 | 0.09*** | 0.00 | 0.00 | 0.01 |
| | (0.35) | (0.27) | (0.01) | (0.01) | (0.01) | (0.01) |
| Attending School | 0.03 | 0.01 | 0.01*** | -0.00 | 0.00 | 0.01*** |
| | (0.16) | (0.11) | (0.00) | (0.00) | (0.00) | (0.00) |
| Graduated High School | 0.47 | 0.29 | 0.24*** | 0.01 | 0.03 | 0.11*** |
| | (0.50) | (0.45) | (0.03) | (0.02) | (0.03) | (0.04) |
| Higher Skill White Collar | 0.14 | 0.08 | 0.10*** | -0.00 | 0.01 | 0.03*** |
| | (0.35) | (0.27) | (0.02) | (0.01) | (0.01) | (0.01) |
| Lower Skill White Collar | 0.21 | 0.15 | 0.10*** | 0.01 | 0.02 | 0.05*** |
| | (0.41) | (0.36) | (0.01) | (0.01) | (0.02) | (0.02) |
| Higher Skill Blue Collar | 0.25 | 0.35 | -0.14*** | -0.01 | -0.02 | -0.07*** |
| | (0.43) | (0.48) | (0.02) | (0.02) | (0.03) | (0.02) |
| Lower Skill Blue Collar | 0.23 | 0.26 | -0.04*** | -0.00 | -0.01 | -0.02 |
| | (0.42) | (0.44) | (0.01) | (0.01) | (0.01) | (0.01) |
| In Labor Force | 0.84 | 0.84 | 0.01 | 0.00 | -0.01 | -0.01 |
| | (0.37) | (0.37) | (0.01) | (0.02) | (0.01) | (0.01) |
| Monthly Income (in 2014 USD) | 505.62 | 412.90 | 159.17*** | 2.44 | 22.26 | 185.33*** |
| | (683.33) | (718.70) | (33.43) | (21.68) | (27.27) | (47.92) |
| Male | 0.47 | 0.48 | -0.01* | -0.01 | 0.01 | 0.03** |
| | (0.50) | (0.50) | (0.01) | (0.01) | (0.01) | (0.01) |
| White or Mestizo | 0.82 | 0.80 | 0.07* | -0.03 | -0.04 | -0.01 |
| | (0.38) | (0.40) | (0.04) | (0.06) | (0.06) | (0.06) |
| Speaks Indigenous Language | 0.10 | 0.10 | -0.03 | 0.06 | 0.05 | 0.06 |
| | (0.30) | (0.29) | (0.04) | (0.06) | (0.05) | (0.06) |
| Below Median Birthplace | 0.34 | 0.57 | -0.37*** | -0.09 | -0.08 | -0.03 |
| | (0.47) | (0.49) | (0.07) | (0.10) | (0.11) | (0.08) |
| Age During Survey | 37.69 | 41.32 | -0.04 | -0.05 | -0.02 | -0.22*** |
| | (4.91) | (3.13) | (0.05) | (0.06) | (0.07) | (0.07) |
| Age in 2008 | 30.67 | 34.37 | -0.03 | -0.00 | 0.01 | -0.16** |
| | (4.92) | (2.84) | (0.04) | (0.05) | (0.06) | (0.06) |
| Observations | 148020 | 16437 | 77750 | 24556 | 18034 | 11243 |

Notes: Full sample, in column 1, includes individuals in the 2014-2017 ENEMDU surveys with a non-missing 2008 canton, younger than 40 in 2008, and at least 30 years old at the time of survey. The remaining columns restrict to individuals aged 30 to 39 in 2008 (who were not exposed to the policy). Column 2 reports means (and standard deviations) for non-exposed individuals living 25-50km from a public university in 2008. Columns 3 to 6 report the differences (and standard errors) between each of the remaining distance categories and the 25-50 km category, again for non-exposed individuals. Standard errors are clustered at the canton level. * p< 0.1, ** p< 0.05, *** p< 0.01.

**Table 2.** Effects of Tuition Fee Elimination on Education and Labor Market Outcomes

| | (1) Attended College | (2) 4 Years of College | (3) Attending School | (4) Graduated High School | (5) Higher Skill WC | (6) Lower Skill WC | (7) Higher Skill BC | (8) Lower Skill BC | (9) In Labor Force | (10) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|---|---|---|---|---|
| **A. Baseline Specification** | | | | | | | | | | |
| Exposed x Distance | -0.043*** | -0.039*** | -0.0025 | -0.0013 | -0.021** | 0.011 | -0.0058 | 0.013 | -0.0028 | -0.051 |
| | (0.010) | (0.0077) | (0.0064) | (0.015) | (0.0084) | (0.0096) | (0.0090) | (0.011) | (0.0088) | (0.033) |
| **B. Province-by-Cohort Fixed Effects** | | | | | | | | | | |
| Exposed x Distance | -0.058*** | -0.050*** | -0.015*** | 0.026 | -0.034*** | 0.025* | 0.0034 | 0.0039 | -0.0026 | 0.033 |
| | (0.014) | (0.012) | (0.0056) | (0.019) | (0.012) | (0.014) | (0.013) | (0.020) | (0.014) | (0.073) |
| Dep. Var. Mean | 0.21 | 0.14 | 0.028 | 0.48 | 0.14 | 0.21 | 0.25 | 0.24 | 0.84 | 6.26 |
| N | 110044 | 110044 | 110044 | 110044 | 109093 | 109093 | 109093 | 109093 | 110044 | 82864 |

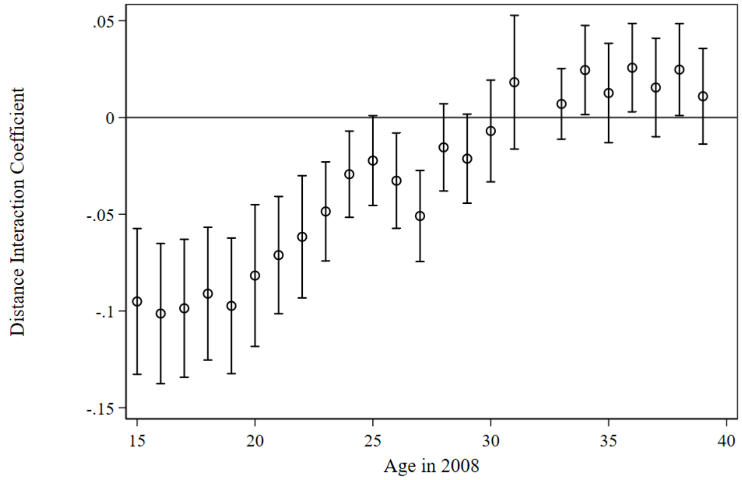
Notes: Standard errors, clustered at the canton level, are in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. These regressions restrict to individuals from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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 in 2008. All regressions control for gender, cohort, canton, age, and survey wave fixed effects.

**Table 3.** Heterogeneous Effects of Tuition Fee Elimination

| | (1) Attended College | (2) Higher Skill WC | (3) Lower Skill WC | (4) In Labor Force | (5) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|
| **A. By Gender** | | | | | |
| Male | -0.040** | -0.043** | 0.041* | 0.011 | 0.076 |
| | (0.019) | (0.017) | (0.024) | (0.0093) | (0.089) |
| Female | -0.077*** | -0.031* | 0.0098 | -0.0072 | -0.050 |
| | (0.019) | (0.016) | (0.020) | (0.027) | (0.13) |
| Difference | 0.037 | -0.013 | 0.031 | 0.018 | 0.13 |
| | (0.024) | (0.022) | (0.034) | (0.029) | (0.16) |
| **B. By Race** | | | | | |
| White or Mestizo | -0.053*** | -0.044*** | 0.029 | -0.0061 | 0.062 |
| | (0.016) | (0.014) | (0.018) | (0.015) | (0.087) |
| Other | -0.059** | -0.0033 | -0.012 | 0.014 | -0.052 |
| | (0.025) | (0.025) | (0.017) | (0.027) | (0.096) |
| Difference | 0.0062 | -0.041 | 0.041* | -0.020 | 0.11 |
| | (0.030) | (0.028) | (0.024) | (0.029) | (0.12) |
| **C. By Language** | | | | | |
| Speaks Indigenous | 0.016 | 0.043 | -0.034 | 0.045 | -0.00035 |
| | (0.020) | (0.028) | (0.032) | (0.031) | (0.24) |
| No Indigenous | -0.060*** | -0.040*** | 0.023 | -0.010 | 0.059 |
| | (0.014) | (0.012) | (0.016) | (0.015) | (0.075) |
| Difference | 0.076*** | 0.083*** | -0.057 | 0.055 | -0.060 |
| | (0.024) | (0.030) | (0.037) | (0.034) | (0.25) |
| **D. By Birthplace** | | | | | |
| Below Median | 0.029 | 0.023 | 0.043 | -0.016 | 0.16 |
| | (0.032) | (0.022) | (0.026) | (0.024) | (0.12) |
| Above Median | -0.100*** | -0.049*** | 0.0069 | -0.00026 | 0.038 |
| | (0.018) | (0.013) | (0.021) | (0.021) | (0.12) |
| Difference | 0.13*** | 0.072*** | 0.036 | -0.015 | 0.12 |
| | (0.035) | (0.026) | (0.034) | (0.029) | (0.17) |
| Dep. Var. Mean | 0.21 | 0.14 | 0.21 | 0.84 | 6.26 |

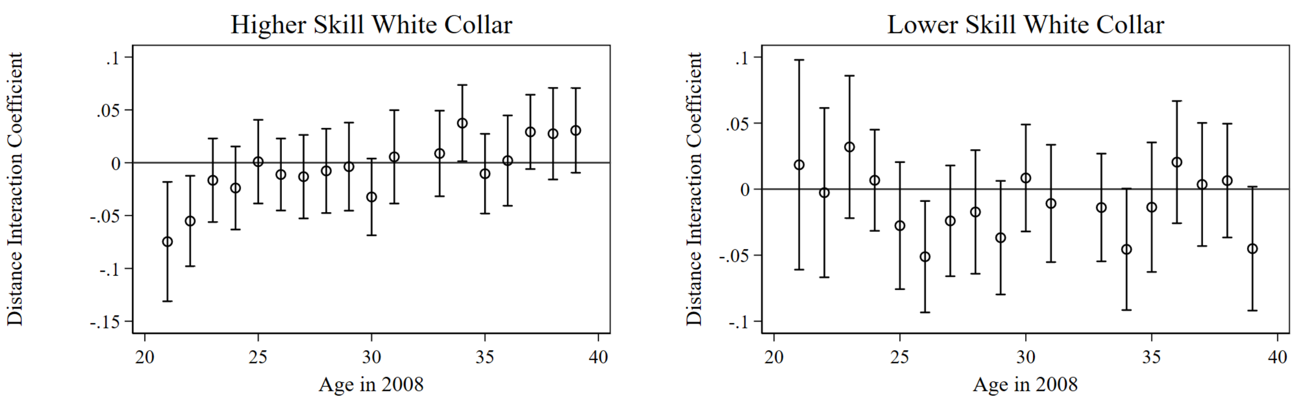
Notes: Standard errors, clustered at the canton level, are in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. Each panel reports the "Exposed x Distance to Nearest Public University" interaction for 2 regressions, conducted for each of the specified groups separately, as well as the difference between the two coefficients. These regressions restrict to individuals from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. All regressions control for gender, cohort, canton, age, survey wave, and province-by-cohort 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 (in the census preceding their birth).

**Figure 1.** College Attendance Event Study Coefficients




Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys who were aged 15-39 in 2008. Coefficients and 95% confidence intervals for the distance-by-cohort interactions from equation (1) are reported. Standard errors are clustered at the canton level.

**Figure 2.** Occupation Type Event Study Coefficients

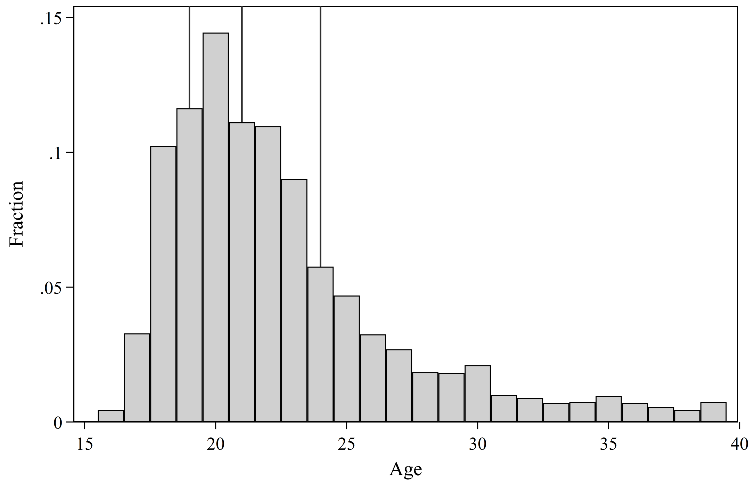



Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys who were aged 15-39 in 2008. Coefficients and 95% confidence intervals for the distance-by-cohort interactions from equation (1), with the addition of province-by-cohort fixed effects, are reported. Standard errors are clustered at the canton level.

# A Online Appendix

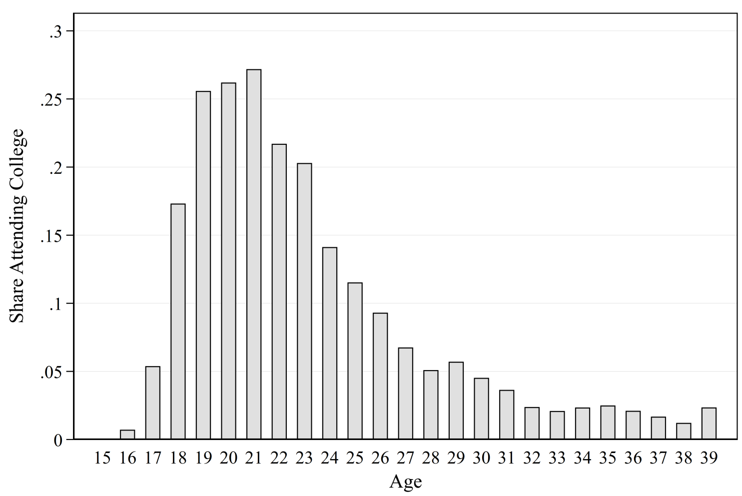
## A.1 Additional Figures and Tables

**Figure A1.** Age Distribution Among University Students, 2007



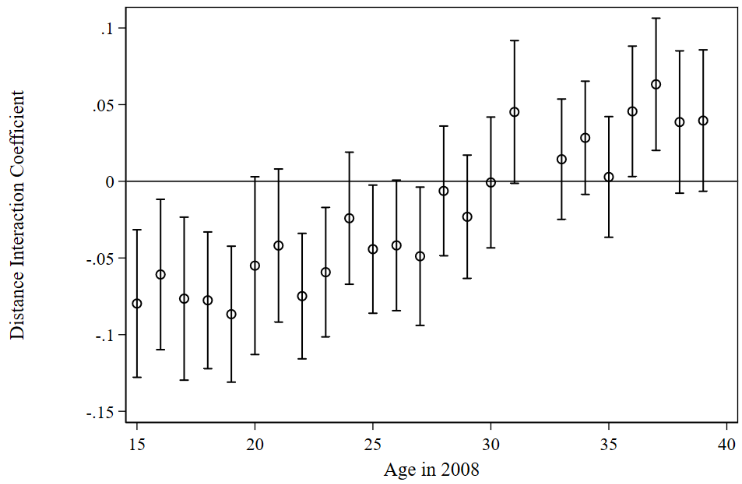

Notes: Sample includes individuals under 40 years old in the 2007 quarter 4 ENEMDU survey, currently attending university. Vertical lines represent the 25th, 50th, and 75th percentiles of the distribution. By restricting to individuals younger than 40, this figure omits less than 5% of current university students.

**Figure A2.** Current University Attendance by Age, 2007



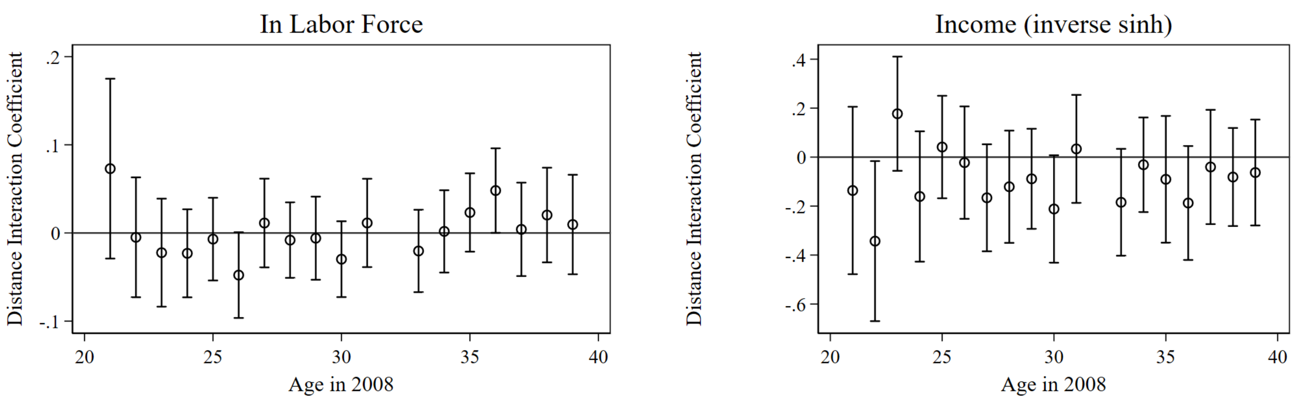

Notes: Sample includes individuals under 40 years old in the 2007 quarter 4 ENEMDU survey.

**Figure A3.** College Attendance Event Study Coefficients, with Province-by-Cohort Fixed Effects




Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys who were aged 15-39 in 2008. Coefficients and 95% confidence intervals for the distance-by-cohort interactions from equation (1), with the addition of province-by-cohort fixed effects, are reported. Standard errors are clustered at the canton level.

**Figure A4.** Labor Market Event Study Coefficients



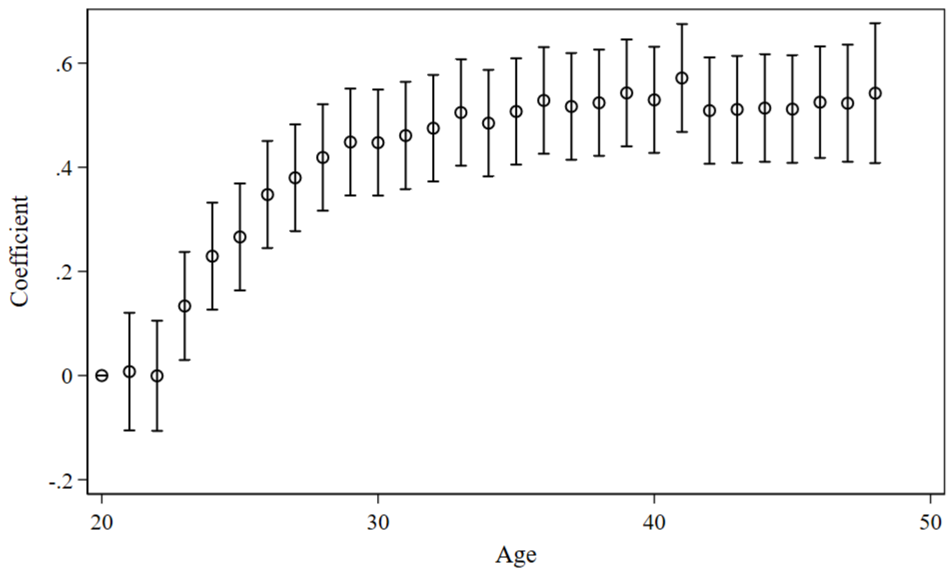

Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys who were aged 15-39 in 2008. Coefficients and 95% confidence intervals for the distance-by-cohort interactions from equation (1), with the addition of province-by-cohort fixed effects, are reported. Standard errors are clustered at the canton level.

**Table A1.** Gender Heterogeneity Robustness Checks

| | (1) Attended College | (2) Higher Skill WC | (3) Lower Skill WC | (4) In Labor Force | (5) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|
| **A. Cohort FE x Distance to Metros** | | | | | |
| Male | -0.046** | -0.052*** | 0.046* | 0.012 | 0.038 |
| | (0.021) | (0.017) | (0.025) | (0.0097) | (0.088) |
| Female | -0.081*** | -0.036** | 0.014 | -0.0093 | -0.062 |
| | (0.018) | (0.016) | (0.018) | (0.028) | (0.14) |
| Difference | 0.034 | -0.016 | 0.033 | 0.021 | 0.100 |
| | (0.024) | (0.022) | (0.033) | (0.030) | (0.17) |
| **B. Mean Reversion** | | | | | |
| Male | -0.031 | -0.033* | 0.032 | 0.015 | 0.14 |
| | (0.021) | (0.019) | (0.029) | (0.011) | (0.10) |
| Female | -0.072*** | -0.029 | 0.017 | -0.0062 | -0.13 |
| | (0.023) | (0.019) | (0.023) | (0.033) | (0.17) |
| Difference | 0.042 | -0.0044 | 0.015 | 0.021 | 0.27 |
| | (0.029) | (0.026) | (0.039) | (0.035) | (0.19) |
| **C. Additional Distance Interactions** | | | | | |
| Male | -0.048* | -0.027 | 0.049 | 0.00086 | 0.0074 |
| | (0.028) | (0.020) | (0.034) | (0.012) | (0.10) |
| Female | -0.043** | -0.030 | 0.020 | -0.012 | -0.026 |
| | (0.021) | (0.018) | (0.026) | (0.045) | (0.18) |
| Difference | -0.0045 | 0.0033 | 0.029 | 0.013 | 0.033 |
| | (0.031) | (0.022) | (0.050) | (0.045) | (0.22) |
| **D. Binary Distance Variable** | | | | | |
| Male | 0.020* | 0.013 | -0.023** | -0.0064* | -0.079** |
| | (0.011) | (0.0078) | (0.010) | (0.0038) | (0.039) |
| Female | 0.027*** | 0.018*** | -0.0058 | 0.0047 | -0.0018 |
| | (0.0087) | (0.0068) | (0.0087) | (0.0099) | (0.065) |
| Difference | -0.0072 | -0.0056 | -0.017 | -0.011 | -0.077 |
| | (0.013) | (0.0095) | (0.014) | (0.011) | (0.077) |
| Dep. Var. Mean | 0.21 | 0.14 | 0.21 | 0.84 | 6.26 |
| N | 110044 | 109093 | 109093 | 110044 | 82864 |
| **E. Middle Distances** | | | | | |
| Male | -0.040 | -0.062* | -0.012 | 0.0021 | -0.078 |
| | (0.051) | (0.035) | (0.044) | (0.017) | (0.15) |
| Female | -0.13*** | -0.051* | 0.018 | -0.069 | -0.30 |
| | (0.030) | (0.027) | (0.037) | (0.048) | (0.24) |
| Difference | 0.093* | -0.010 | -0.030 | 0.071 | 0.22 |
| | (0.055) | (0.043) | (0.064) | (0.048) | (0.27) |
| Dep. Var. Mean | 0.13 | 0.086 | 0.17 | 0.83 | 6.05 |
| N | 43690 | 43388 | 43388 | 43690 | 31669 |

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 from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. All regressions control for cohort, canton, age, survey wave, and province-by-cohort fixed effects.

**Figure A5.** Income-Age Relationship




Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys, aged 15-39 in 2008. Each point plots the coefficient and 95% confidence interval in a regression of the inverse hyperbolic sine of income on age (at time of survey) fixed effects.

**Table A2.** Race Heterogeneity Robustness Checks

| | (1) Attended College | (2) Higher Skill WC | (3) Lower Skill WC | (4) In Labor Force | (5) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|
| **A. Cohort FE x Distance to Metros** | | | | | |
| White or Mestizo | -0.054*** | -0.048*** | 0.035** | -0.0062 | 0.030 |
| | (0.017) | (0.015) | (0.018) | (0.016) | (0.087) |
| Other | -0.070*** | -0.015 | -0.016 | 0.016 | -0.073 |
| | (0.021) | (0.020) | (0.019) | (0.030) | (0.10) |
| Difference | 0.016 | -0.033 | 0.051* | -0.022 | 0.10 |
| | (0.028) | (0.025) | (0.027) | (0.033) | (0.13) |
| **B. Mean Reversion** | | | | | |
| White or Mestizo | -0.051*** | -0.038** | 0.022 | -0.0039 | 0.074 |
| | (0.019) | (0.015) | (0.021) | (0.019) | (0.10) |
| Other | -0.065** | -0.0091 | 0.0016 | 0.026 | -0.048 |
| | (0.025) | (0.025) | (0.016) | (0.030) | (0.11) |
| Difference | 0.013 | -0.029 | 0.020 | -0.029 | 0.12 |
| | (0.031) | (0.029) | (0.025) | (0.032) | (0.14) |
| **C. Additional Distance Interactions** | | | | | |
| White or Mestizo | -0.028 | -0.033* | 0.042* | -0.027 | -0.0052 |
| | (0.022) | (0.018) | (0.022) | (0.022) | (0.11) |
| Other | -0.053** | 0.0062 | -0.023 | 0.030 | -0.028 |
| | (0.021) | (0.026) | (0.027) | (0.042) | (0.14) |
| Difference | 0.026 | -0.039 | 0.065 | -0.056 | 0.023 |
| | (0.029) | (0.031) | (0.040) | (0.039) | (0.17) |
| **D. Binary Distance Variable** | | | | | |
| White or Mestizo | 0.016* | 0.014** | -0.012* | 0.0040 | -0.069* |
| | (0.0085) | (0.0065) | (0.0072) | (0.0057) | (0.039) |
| Other | 0.019 | -0.0032 | -0.0065 | -0.020 | -0.0010 |
| | (0.016) | (0.012) | (0.012) | (0.013) | (0.058) |
| Difference | -0.0032 | 0.017 | -0.0053 | 0.024* | -0.068 |
| | (0.017) | (0.013) | (0.014) | (0.014) | (0.065) |
| Dep. Var. Mean | 0.21 | 0.14 | 0.21 | 0.84 | 6.26 |
| N | 110044 | 109093 | 109093 | 110044 | 82864 |
| **E. Middle Distances** | | | | | |
| White or Mestizo | -0.10*** | -0.074*** | 0.0040 | -0.021 | -0.17 |
| | (0.034) | (0.025) | (0.029) | (0.035) | (0.16) |
| Other | -0.075** | -0.057 | -0.037 | -0.11*** | -0.28 |
| | (0.033) | (0.048) | (0.033) | (0.038) | (0.27) |
| Difference | -0.029 | -0.017 | 0.041 | 0.092* | 0.10 |
| | (0.043) | (0.056) | (0.039) | (0.053) | (0.29) |
| Dep. Var. Mean | 0.13 | 0.086 | 0.17 | 0.83 | 6.05 |
| N | 43690 | 43388 | 43388 | 43690 | 31669 |

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 from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. All regressions control for gender, cohort, canton, age, survey wave, and province-by-cohort fixed effects.

**Table A3.** Language Heterogeneity Robustness Checks

| | (1) Attended College | (2) Higher Skill WC | (3) Lower Skill WC | (4) In Labor Force | (5) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|
| **A. Cohort FE x Distance to Metros** | | | | | |
| Speaks Indigenous | 0.013 | 0.025 | -0.080* | 0.059 | -0.023 |
| | (0.024) | (0.030) | (0.044) | (0.037) | (0.26) |
| No Indigenous | -0.064*** | -0.046*** | 0.029** | -0.013 | 0.035 |
| | (0.015) | (0.013) | (0.015) | (0.015) | (0.075) |
| Difference | 0.077*** | 0.071** | -0.11** | 0.072* | -0.057 |
| | (0.026) | (0.031) | (0.047) | (0.039) | (0.26) |
| **B. Mean Reversion** | | | | | |
| Speaks Indigenous | 0.0026 | 0.031 | -0.025 | 0.033 | -0.015 |
| | (0.020) | (0.029) | (0.030) | (0.033) | (0.27) |
| No Indigenous | -0.056*** | -0.033*** | 0.023 | -0.0032 | 0.086 |
| | (0.016) | (0.012) | (0.018) | (0.019) | (0.086) |
| Difference | 0.059** | 0.064** | -0.048 | 0.036 | -0.10 |
| | (0.024) | (0.030) | (0.036) | (0.036) | (0.28) |
| **C. Additional Distance Interactions** | | | | | |
| Speaks Indigenous | 0.020 | 0.042 | -0.087* | 0.075 | -0.23 |
| | (0.028) | (0.042) | (0.052) | (0.051) | (0.33) |
| No Indigenous | -0.041** | -0.032** | 0.036** | -0.029 | 0.040 |
| | (0.020) | (0.015) | (0.018) | (0.025) | (0.096) |
| Difference | 0.061* | 0.074* | -0.12** | 0.10** | -0.27 |
| | (0.033) | (0.043) | (0.055) | (0.051) | (0.35) |
| **D. Binary Distance Variable** | | | | | |
| Speaks Indigenous | 0.0094 | -0.0080 | -0.0035 | -0.044*** | -0.013 |
| | (0.015) | (0.013) | (0.023) | (0.011) | (0.099) |
| No Indigenous | 0.018** | 0.013** | -0.011 | 0.0053 | -0.063* |
| | (0.0084) | (0.0062) | (0.0065) | (0.0056) | (0.035) |
| Difference | -0.0085 | -0.021 | 0.0071 | -0.049*** | 0.050 |
| | (0.017) | (0.015) | (0.023) | (0.012) | (0.094) |
| Dep. Var. Mean | 0.21 | 0.14 | 0.21 | 0.84 | 6.26 |
| N | 110044 | 109093 | 109093 | 110044 | 82864 |
| **E. Middle Distances** | | | | | |
| Speaks Indigenous | 0.0012 | -0.021 | -0.033 | -0.057 | 0.31 |
| | (0.029) | (0.039) | (0.067) | (0.045) | (0.34) |
| No Indigenous | -0.10*** | -0.067*** | -0.0050 | -0.043 | -0.21 |
| | (0.030) | (0.022) | (0.025) | (0.031) | (0.15) |
| Difference | 0.10*** | 0.046 | -0.028 | -0.014 | 0.52 |
| | (0.036) | (0.042) | (0.069) | (0.056) | (0.37) |
| Dep. Var. Mean | 0.13 | 0.086 | 0.17 | 0.83 | 6.05 |
| N | 43690 | 43388 | 43388 | 43690 | 31669 |

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 from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. All regressions control for gender, cohort, canton, age, survey wave, and province-by-cohort fixed effects.

**Table A4.** Birthplace Heterogeneity Robustness Checks

| | (1) Attended College | (2) Higher Skill WC | (3) Lower Skill WC | (4) In Labor Force | (5) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|
| **A. Cohort FE x Distance to Metros** | | | | | |
| Below Median | 0.037 | 0.019 | 0.061** | -0.028 | 0.19 |
| | (0.037) | (0.026) | (0.027) | (0.022) | (0.14) |
| Above Median | -0.099*** | -0.051*** | 0.0074 | -0.0059 | 0.0080 |
| | (0.019) | (0.014) | (0.024) | (0.020) | (0.12) |
| Difference | 0.14*** | 0.070** | 0.053 | -0.022 | 0.18 |
| | (0.040) | (0.029) | (0.037) | (0.028) | (0.18) |
| **B. Mean Reversion** | | | | | |
| Below Median | 0.027 | 0.032 | 0.038 | 0.0042 | 0.17 |
| | (0.035) | (0.024) | (0.030) | (0.026) | (0.15) |
| Above Median | -0.091*** | -0.049*** | 0.0098 | -0.0056 | 0.080 |
| | (0.018) | (0.013) | (0.020) | (0.022) | (0.12) |
| Difference | 0.12*** | 0.081*** | 0.029 | 0.0098 | 0.086 |
| | (0.039) | (0.028) | (0.036) | (0.031) | (0.18) |
| **C. Additional Distance Interactions** | | | | | |
| Below Median | 0.088** | 0.062* | 0.053 | -0.078** | 0.29 |
| | (0.041) | (0.033) | (0.035) | (0.036) | (0.19) |
| Above Median | -0.094*** | -0.048*** | 0.028 | 0.0018 | 0.093 |
| | (0.023) | (0.016) | (0.029) | (0.031) | (0.13) |
| Difference | 0.18*** | 0.11*** | 0.025 | -0.080* | 0.20 |
| | (0.047) | (0.037) | (0.045) | (0.044) | (0.23) |
| **D. Binary Distance Variable** | | | | | |
| Below Median | -0.0027 | 0.0065 | -0.037*** | 0.020** | -0.12* |
| | (0.017) | (0.0096) | (0.012) | (0.010) | (0.066) |
| Above Median | 0.040*** | 0.018** | -0.00076 | -0.0065 | -0.034 |
| | (0.011) | (0.0076) | (0.0089) | (0.0072) | (0.051) |
| Difference | -0.042** | -0.012 | -0.037** | 0.027** | -0.086 |
| | (0.020) | (0.012) | (0.015) | (0.012) | (0.082) |
| Dep. Var. Mean | 0.21 | 0.14 | 0.21 | 0.84 | 6.26 |
| N | 107592 | 106657 | 106657 | 107592 | 81093 |
| **E. Middle Distances** | | | | | |
| Below Median | -0.019 | -0.020 | 0.037 | -0.032 | 0.057 |
| | (0.057) | (0.040) | (0.040) | (0.039) | (0.20) |
| Above Median | -0.11*** | -0.081*** | 0.0015 | -0.067* | -0.31 |
| | (0.038) | (0.028) | (0.039) | (0.036) | (0.24) |
| Difference | 0.090 | 0.061 | 0.036 | 0.035 | 0.37 |
| | (0.067) | (0.048) | (0.056) | (0.049) | (0.32) |
| Dep. Var. Mean | 0.13 | 0.087 | 0.17 | 0.83 | 6.05 |
| N | 42673 | 42379 | 42379 | 42673 | 30895 |

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 from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. All regressions control for gender, cohort, canton, age, survey wave, and province-by-cohort 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 (in the census preceding their birth).

## A.2 Policy Background

In the years that followed the tuition elimination, there were a number of other changes made to the education system in Ecuador. For example, there was an increase in the number of government scholarships, which provided recipients with living stipends, in 2011 (for international scholarships) and 2013 (for national scholarships). Even in peak years, however, these scholarships only supported a fraction of a percent of the total university student population. Other changes were made in order to improve the quality of university education. An evaluation of all universities in the country was conducted, which led to the suspension and then the eventual closing of 14 poorly performing universities, in 2012 and 2016, respectively. Requirements for professor qualifications increased,[10] and regulations on the selection, evaluation, and renumeration of professors were approved. In 2014, a nationwide high school examination, which would be used to determine entry into university, was established.

While it is important to keep these other changes in mind when interpreting our results, we argue that the estimates in this paper are primarily capturing the effect of the fee elimination and not the subsequent quality-related changes, for two main reasons. First, the attempts to improve quality applied to all universities, not just public universities, and our estimation strategy relies on variation in access to public universities specifically. Second, because of the age restrictions made when selecting our sample, the majority of individuals in our analysis would have been out of college before any of the above changes were implemented. For this reason, we also note that the individuals in our analysis would have also been too old to have been affected by any changes made to the pre-university education system, which also underwent reform around the same time (see Cevallos Estarellas and Bramwell (2015) for more details on these pre-tertiary reforms).

## A.3 Robustness Checks

### A.3.1 Allowing for Other Differential Trends

In general, in order to interpret our coefficient estimates as the causal effect of the 2008 fee reduction, distance to a public university must not be proxying for other characteristics that could drive differential cohort trends for reasons unrelated to the tuition fee elimination. For example, if

[10]In 2010, universities were given a period of seven years to ensure that 40% of their full-time faculty had a doctorate.

distance to a public university is 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 find. We therefore test whether our results are robust to the inclusion of additional remoteness measures, interacted with cohort fixed effects. Specifically, we calculate the distance between an individual's canton of residence and the nearest large metropolitan area (either the city of Guayaquil or Quito, the largest two cities in Ecuador which are over double the size of the third-largest city), and we include cohort fixed effects interacted with this new distance variable. If the differential trends picked up in our main results were being driven by general remoteness and not access to public universities specifically, the inclusion of these controls would reduce the magnitude of the $\text{Exposed}_c$ x $\text{Distance}_j$ interaction coefficients. In Panel A of Table A5, we see no such reduction. Estimated coefficients are almost identical to those in Table 2, offering support for the validity of the identification strategy.

In Panel B, we address concerns that our coefficient estimates 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 at baseline. If these areas were simply catching up to higher access areas (with higher schooling at baseline), this would result in an underestimation of the true effect. On the other hand, if these high-access areas were pulling away from other areas due to their already high baseline levels of schooling, this would result in an overestimation of the true effect.[11] To determine whether either of these scenarios should be a concern, we estimate a specification that includes interactions between cohort dummies and canton-level averages of schooling (from the 2001 census). This allows for differential cohort trends based on pre-policy levels of schooling (both catch-up and dispersion). This specification yields slightly smaller but similar coefficients to the previous results, demonstrating significant positive effects on college completion and high-skilled white-collar jobs, but no effects on labor force participation or income. Differential trends based on baseline levels of schooling were not driving our main results and were not resulting in an underestimation of labor force participation or income effects.

[11] These explanations, however, would be unable to explain why the event studies demonstrate the particular pattern they have, rather than a simple linear trend in coefficients across all age cohorts.

In addition, we explore the sensitivity of our results to the inclusion of additional distance variables: specifically, interactions with distance to nearest private university, and distance to nearest public technical institute. Because this policy only affected tuition fees at public institutions, we should not expect to see the same effects coming from access to private universities instead of public universities. On the other hand, public technical institutes were affected by the policy and could have shifted enrollment away from university-level post-secondary education (our education outcome of interest). Because distance to public universities is positively correlated with distance to private universities and distance to public technical institutes, we estimate the baseline specification with the addition of the exposed indicator interacted with these two variables.

In Panel C, it is clear that the effects reported in the previous tables are driven by access to public universities rather than private universities or public technical institutes. Although the private university interaction is significant in some specifications, it tends to have the opposite sign as the public university interaction, and its inclusion does not change the finding that the policy significantly increased college attendance and high-skilled white-collar jobs. None of the coefficients on the technical institute distance interactions are significantly different from zero, and their inclusion does not change our main conclusions. This is consistent with the fact that only a small share of the population attended these institutes, both before and after the policy (less than 0.7% of the 15-39 year old population from 2003 to 2013).

### A.3.2 Alternate Distance Variable

Panel D explores robustness to a different definition of the distance variable that allows for the possibility of a non-linear relationship, which could be more appropriate if, for example, distance only affects the college enrollment decision up to a certain distance. In this regression, instead of a continuous distance measure, we use a binary variable equal to one for those living in the same canton as a public university. The coefficient on the interaction between this dummy variable and the exposed indicator is positive and significant in columns 1 and 2, which indicates that the gap between the exposed and unexposed cohorts is larger in cantons with a public university. Consistent with previous results, this provides evidence that the fee elimination increased college attendance and the takeup of high skill white collar jobs. The negative coefficient in the next column supports the finding that the latter was the result of people switching out of lower-skilled white collar jobs.

**Table A5.** Tuition Fee Elimination Effects, Robustness Checks

| | (1) Attended College | (2) Higher Skill WC | (3) Lower Skill WC | (4) In Labor Force | (5) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|
| **A. Cohort FE x Distance to Metros** | | | | | |
| Exposed x Distance | -0.063*** | -0.041*** | 0.031** | -0.0034 | 0.0031 |
| | (0.015) | (0.012) | (0.014) | (0.015) | (0.072) |
| **B. Mean Reversion** | | | | | |
| Exposed x Distance | -0.052*** | -0.028** | 0.025 | 0.000038 | 0.042 |
| | (0.017) | (0.013) | (0.017) | (0.017) | (0.088) |
| **C. Additional Distance Interactions** | | | | | |
| Exposed x Distance | -0.045** | -0.028* | 0.034** | -0.012 | -0.0015 |
| | (0.019) | (0.015) | (0.017) | (0.024) | (0.083) |
| Exposed x Distance: Pvt | 0.00086 | 0.012 | -0.040** | 0.017 | 0.21** |
| | (0.021) | (0.014) | (0.017) | (0.022) | (0.089) |
| Exposed x Distance: Tech | -0.024 | -0.021 | 0.015 | 0.0040 | -0.10 |
| | (0.028) | (0.020) | (0.024) | (0.026) | (0.11) |
| **D. Binary Distance Variable** | | | | | |
| Exposed x Same Canton | 0.023*** | 0.014*** | -0.013** | -0.00021 | -0.049 |
| | (0.0075) | (0.0055) | (0.0061) | (0.0054) | (0.034) |
| Dep. Var. Mean | 0.21 | 0.14 | 0.21 | 0.84 | 6.26 |
| N | 110044 | 109093 | 109093 | 110044 | 82864 |

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. These regressions restrict to individuals from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. Panel A includes cohort fixed effects interacted with the distance to the nearest large metropolitan area (either Guayaquil or Quito). Panel B includes cohort fixed effects interacted with average canton-level schooling levels from 2001. Panel C includes "Exposed" interacted with distance to nearest private university and "Exposed" interacted with distance to nearest public technical institute. All regressions control for gender and cohort, canton, age, survey wave, and province-by-cohort fixed effects.

### A.3.3 Alternate Sample Restrictions

In the main regressions, an individual's canton of residence in 2008 is crucial to the the cross-sectional variation that forms the basis of our empirical strategy – distance to a public university at the time of policy change. We are therefore careful to exclude any individuals for whom the canton of residence in 2008 is uncertain. Specifically, we drop individuals who migrated to their current canton after 2012 because we are not sure whether their previous canton of residence (which is available in the survey) is where they were living in 2008. We argue that using 2012 as the cutoff is reasonable: it excludes only 4% of the sample and assumes that the majority of migrants lived in their previous residence for at least 5 years. Nevertheless, we show in Table A6 that our results are not sensitive to the choice of cutoff year.

**Table A6.** Tuition Fee Elimination Effects, Alternate Sample Restrictions

| | (1) Selection into... | (2) Attended College | (3) Higher Skill WC | (4) Lower Skill WC | (5) In Labor Force | (6) Income ($\sinh^{-1}$) |
|---|---|---|---|---|---|---|
| **A. 2010 Cutoff** | | | | | | |
| | 2010 Sample | | | | | |
| Exposed x Distance | -0.0083 | -0.058*** | -0.034*** | 0.025* | 0.0028 | 0.041 |
| | (0.0070) | (0.015) | (0.013) | (0.014) | (0.014) | (0.076) |
| Dep. Var. Mean | 0.97 | 0.21 | 0.14 | 0.21 | 0.84 | 6.26 |
| N | 110044 | 106772 | 105899 | 105899 | 106772 | 80396 |
| **B. Full Sample** | | | | | | |
| | 2012 Sample | | | | | |
| Exposed x Distance | -0.0049 | -0.058*** | -0.030** | 0.027* | -0.0011 | 0.0044 |
| | (0.0094) | (0.014) | (0.012) | (0.014) | (0.013) | (0.071) |
| Dep. Var. Mean | 0.96 | 0.21 | 0.14 | 0.22 | 0.84 | 6.26 |
| N | 114411 | 114411 | 113248 | 113248 | 114411 | 86084 |
| **C. Middle Distances** | | | | | | |
| Exposed x Distance | | -0.077*** | -0.045** | 0.0084 | -0.034 | -0.20 |
| | | (0.029) | (0.020) | (0.023) | (0.025) | (0.13) |
| Dep. Var. Mean | . | 0.13 | 0.087 | 0.17 | 0.83 | 6.05 |
| N | . | 45099 | 44747 | 44747 | 45099 | 32745 |

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. These regressions restrict to individuals from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. All regressions control for gender, cohort, canton, age, survey wave, and province-by-cohort fixed effects. In column 1 of Panel A, the dependent variable is an indicator for individuals whose 2008 canton is known or who migrated to their current canton by 2010. In column 1 of Panel B, the dependent variable is an indicator for individuals whose 2008 canton is known or who migrated to their current canton by 2012. Panel C restricts to those in cantons more than 0km but less than 100km from a public unviersity.

First, we report the results from using 2010 as a cutoff, in Panel A of Table A6. This drops an additional 3% of the sample, but only requires that migrants lived in their previous residence for at least 3 years in order for the 2008 canton assignment to be accurate. In the first column of Panel A, we show that our policy variable of interest (the exposed-by-distance interaction) does not significantly affect selection from the full sample into this smaller sample. We then repeat our main regressions of interest using the smaller sample, which reveal almost identical effects to those discussed in Table 2.

Next, in Panel B, we report results that use the full sample. That is, we include all individuals, regardless of migration date, and assign individuals to their previous canton of residence if they migrated to their current place of residence after 2008. In the first column, we show that the policy variable does not significantly affect selection from the full sample into our existing sample (which uses 2012 as the cutoff year). In the remaining columns, we show that the main results described above are robust to the use of the full sample.

In this table, we also test whether our results still hold when restricting to individuals in the middle distance categories – that is, individuals who were not living in the same canton as a public university, but who lived less than 100km from one. Table 1 showed that observable characteristics were fairly balanced across the distance distribution within this range, therefore suggesting that a violation of the parallel trends assumption would be less of a concern among this restricted sample. Panel C of Table A6 reveals that the significant improvements in college attendance and high-skilled white-collar jobs are still present (and if anything, even stronger), among this restricted sample, which is about half the size of our original sample. Clearly, our results are not being driven by the systematically different characteristics of individuals who were living in the same canton as a public university in 2008.

### A.3.4 Migration

We also explore the extent to which endogenous migration could be a potential threat to the validity of our empirical strategy. In particular, although migration rates are low in this sample, selective migration that takes place during specific ages could be a cause for concern. We define our distance measure based on an individual's residence in 2008, when our exposed cohorts are 21-24 and our non-exposed cohorts are 30-34. If individuals tend to migrate in their late twenties, and if the propensity to migrate during this time varies by educational attainment, this would result in differential cohort trends across areas that we could be incorrectly attributing to the effect of the policy.

For example, even if place of residence during ages 21-24 is essentially random, if people who have college degrees are disproportionately more likely to migrate after these ages to areas with good jobs (which also happen to have more public universities), this would result in a larger share of college graduates in non-exposed cohorts near public universities, a smaller gap between exposed (younger and more educated) and non-exposed (older and on average less educated) cohorts in areas near public universities, and an underestimation of the effect of the tuition fee elimination.

To evaluate how relevant this explanation might be, we repeat our main difference-in-differences regression, using migration as an outcome variable. If selective migration to areas near public universities were indeed taking place among the older cohorts, we would expect significantly different gaps in migration rates between exposed and non-exposed cohorts, across areas of varying distances from public universities. That is, we would expect to see a statistically significant coefficient on the $\text{Exposed}_c$ x $\text{Distance}_j$ interaction in a regression on migration. Table A7 reveals that this is not the case. Coefficient estimates are very small relative to sample means, and not significantly different from zero, which suggests that differential migration across cohorts is unlikely to be an important confounder.

Table A7 also shows that our policy variable of interest appears to be unrelated to sample composition more generally. When we estimate our difference-in-differences specification using baseline characteristics like gender, race, language, and birthplace as our dependent variables, none of the interaction coefficients are significantly different from zero. If selective migration were indeed taking place, we would likely see some compositional effects on these variables. More generally, these regressions provide additional support for the parallel trends assumption by demonstrating that any differences in the sample composition of exposed and unexposed cohorts appear to be consistent across the distance distribution.

**Table A7.** Tuition Fee Elimination, Migration, and Sample Composition

| | (1) Migrated in Last 10 years | (2) Migrated in Last 5 years | (3) Male | (4) White or Mestizo | (5) Speaks Indigenous Language | (6) Below Median Birthplace |
|---|---|---|---|---|---|---|
| Exposed x Distance | 0.0018 | 0.0012 | -1.1e-17 | -0.011 | 0.018 | 0.092 |
| | (0.011) | (0.0047) | (1.0e-17) | (0.015) | (0.012) | (0.063) |
| Dep. Var. Mean | 0.085 | 0.019 | 0.47 | 0.82 | 0.10 | 0.34 |
| N | 110044 | 110044 | 110044 | 110044 | 110044 | 107592 |

Notes: Standard errors, clustered at the canton level, are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. These regressions restrict to individuals from the 2014-2017 ENEMDU, at least 30 years old when surveyed and aged 21 to 24 or 30 to 34 in 2008. "Exposed" is equal to 1 for individuals who were aged 21 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. All regressions control for canton, age, survey wave, and province-by-cohort fixed effects.