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IZA DP No. 13638

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AUGUST 2020

ISSN: 2365-9793

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ABSTRACT

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

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 affected occupation choice, shifting 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 individuals of higher socioeconomic status.

JEL Classification: I23, I24, I28, O15

Keywords: higher education, tuition reduction, Ecuador

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* We thank Jenny Aker, Arjun Bedi, Pascaline Dupas, Jun Goto, Gabriela Izurieta, Gaurav Khanna, 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.

1 Introduction

Education is an important form of human capital investment and often seen as a fundamental key to economic development. With primary school enrollment almost universal, and secondary school enrollment growing rapidly, tertiary education has become an increasingly important priority for governments across the globe (OECD, 2014). But tertiary enrollment rates remain low in many countries and are often highly unequal across socioeconomic strata.

Recent years have seen a growing interest in the complete elimination of tuition fees as tool to expand access (and equality of access) to tertiary education. The Philippines 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). In the United States, almost 20 states have adopted or are considering adopting some form of tuition-free college (CNBC, 2019). While there is a growing literature that seeks to evaluate these "place-based" or "promise" programs in the United States (Bifulco et al., 2019; Gurantz, 2020; Andrews et al., 2010; Carruthers and Fox, 2016), empirical evidence on nationwide policies outside the United States is lacking.

Importantly, even though these types of policies are often proposed as a means of reducing socioeconomic inequalities, it is not clear who would benefit most from a nationwide reduction in university tuition. 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. For our

[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).

second source of variation, we exploit variation in geographic access to public universities: the distance between an individual's canton of residence in 2008 and their closest public university.

Our first main finding is that the policy increased college enrollment. Using both an event study and a simple difference-in-differences analysis, we find that the difference in college enrollment between exposed (younger) and unexposed (older) cohorts is larger for those with greater access to public universities. Specifically, while younger cohorts are more likely to have attended college than older cohorts overall, this gap is larger among individuals living close to a public university in 2008. In our event 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 simple linear pattern (which would be suggestive of differential cohort trends for reasons unrelated to the policy). We interpret these results as evidence that the policy increased college enrollment, and are able to rule out other explanations for these results, including mean reversion and differential trends across areas of varying levels of remoteness.

The finding that reduced college costs increased enrollment is consistent with a large literature evaluating the effects of various G.I. bills and financial aid policies on college enrollment in the United States and other developed countries (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). Our results, more importantly, also contribute to a much smaller body of evidence from lower-income countries, which has focused on studying the link between access to credit and college enrollment (Solis, 2017; Gurgand et al., 2011).

The second main finding of this study is that Ecuador's fee elimination had significant effects on job type – individuals affected by the policy shifted into high-skill white-collar jobs – though we find no statistically significant effects on income. Again, while there is a growing literature on the labor market effects of college cost reductions in the United States

(Angrist, 1993; Angrist and Chen, 2011; Scott-Clayton and Zafar, 2019; Denning et al., 2019; Bettinger et al., 2019), evidence from developing countries remains scarce.

Finally, 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. This 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. It is also consistent with evidence from several U.S. natural experiments (Turner and Bound, 2003; Stanley, 2003; Dynarski, 2000), and other evidence documenting that public education spending, more generally, tends to disproportionately benefit the well-off (Mingat and Tan, 1986; Jimenez, 1986). Interestingly, these results are also in line with evidence from reforms of the opposite nature in England – ones that *increased* university tuition (and provided grants and loans to support low-income students). Murphy et al. (2019) provide descriptive evidence that the tuition fee system helped improve equity in higher education.[2]

This paper sheds light on an important issue especially understudied in developing countries. 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. 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,[3] and little is known about eventual labor

[2]Azmat and Simion (2018) focus on the 2006 and 2012 English reforms and find mixed evidence on distributional effects: no negative effects on enrollment but slightly larger negative effects on labor market outcomes for students of low socioeconomic status (though still small in magnitude).

[3]Garlick (2017) shows that eliminating secondary school fees had small, positive effects on enrollment,

market effects. More importantly, these studies may provide little guidance on university-level policies like the Ecuadorian one evaluated in this paper, given the potentially different returns to tertiary education (Psacharopoulos and Patrinos, 2018), as well as the different opportunity costs.

2 Background

2.1 2008 Tuition Fee Elimination

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 set their own application process and acceptance criteria. In 2007, 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.[4] For reference, median annual household income was approximately 3000 USD in 2007. Unfortunately, no national-level data on university tuition fees exists prior to 2008, but information from internet archives and conversations with university administrators have provided us with some specific examples.

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 educa-

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.

[4] Traditional fields, like science and engineering, often had lower fees than newer non-traditional fields, such as business-related majors.

tion) free of charge.[5] 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) and Hora 25 (2017) for more details on the policy.)

In the years that followed, a number of other changes were made to the tertiary education system. While we detail them here, we argue below that our estimated effects are unlikely to be capturing the impact of these changes. For example, there was a drastic 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,[6] 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,

[5]Tertiary education in Ecuador consists of university-level education and "non-university" post-secondary education, which includes programs 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 (see Figure A1), we focus in this paper on university-level education.

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

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). That said, this study should still be viewed as an evaluation of the combination of changes that took place in the Ecuadorian tertiary education system in the first few years of the policy, not an evaluation of tuition fee elimination in isolation.

In Figure 1, we illustrate the change in university attendance over the decade spanning this policy change. The lightest bar reports the share of individuals aged 15 to 39 currently attending university. The darkest bar reports the share currently attending a public university, and the last bar reports the share currently attending a private university. From 2003 to 2011, there is a clear upward trend in college attendance shares, driven primarily by an increase at public universities. Interestingly, the year 2011 appears to be the beginning of a decline in college enrollment, the reasons for which we leave for future research. Most of the individuals in our sample would have been out of college by 2012.

It is impossible to determine from Figure 1 whether the tuition fee elimination had any effect on college enrollment decisions. While college attendance does increase in the years after 2008, particularly at public universities, it is not clear whether this is part of a broader general trend, or whether it demonstrates a deviation from what would have happened in the absence of the policy. This is one of the key goals of this paper: to tease out the effect of the policy from broader time trends, by exploiting variation across individuals differentially "exposed" to the policy.

2.2 Pre-Policy Descriptive Statistics

For whom was this policy change actually relevant? To shed light on this question, we present summary statistics on university students in 2007, the year before the fee elimination. These statistics motivate several components of the empirical strategy, discussed in section 4. When thinking about who should have been affected by the policy, age is naturally an important consideration. We would not expect this policy to affect individuals who, in 2008, were much older than the typical university student.

In Figure 2, we illustrate the age distribution among students currently attending university. The age histogram in Panel A includes all university students, while the age histogram in Panel B restricts to students in their first year of university.[7] The modal university student is 20 years old, while the modal first-year student is 18. However, a large share of university students are much older. The vertical lines, which depict the 25th, 50th, and 75th percentiles, demonstrate that one fourth of all university students are aged 24 or older, and one fourth of first-year students are aged 21 or older.

Therefore, individuals aged 21 and younger (the 75th percentile of the first-year university student age distribution) in 2008 were certainly "young enough" to have their college-going decisions affected by the policy. Arguably, individuals aged up to 24 (the 75th percentile of the overall university student age distribution) were also "young enough" to have been affected. For those in this age group who were already in college in 2008, the policy could have affected the decision to continue their university education. At the same time, those in this age group who were not yet in university in 2008 would have been in an early stage of their careers and of similar age to the general university student population: the policy could have also motivated these individuals to go back to university.

These conjectures are also supported by Figure 3, which illustrates the share of each age cohort that was attending university in 2007. The 21-year-old cohort had the highest

[7] As discussed in the table notes, both panels restrict to individuals younger than 40, which drops less than 5% of the relevant sample in Panel A and less than 2% of the relevant sample in Panel B.

attendance share (26%). Notably, these shares remain quite high for older cohorts: 14% of 24-year-olds (which is over half of the 21-year-old share) and over 5% of all cohorts until age 29 report being a current university student in 2007.

Based on these statistics, as we discuss in section 4, we consider individuals aged 24 or younger to be young enough to be affected by the policy, and those aged 30 and older to be "too old" to be affected by the policy. The effect on individuals in between is somewhat less clear. On the one hand, they would have been more advanced in their careers by 2008, and therefore less likely to return to college. On the other hand, 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) student in terms of age. Therefore, we do not classify these age cohorts as exposed or unexposed.

In addition to age, access to public institutions is also an important determinant of an individual's "exposure" to the policy, given that tuition fees were only eliminated at public institutions. With regard to this, it is important to note that 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. Of all first-year university students, 96% have lived in their current place of residence for at least two years. Both of these statistics imply that the vast majority of university students attend a university close to where they are already living. Given this, we would expect individuals living near a public university to be much more affected by the policy than those living further away. Therefore, as we discuss in section 4, we use the distance between an individual's place of residence (in 2008) and the nearest public university as a measure of their geographic access to the policy.

3 Data

The descriptive statistics in the previous section, along with the outcome variables for the main analysis, come from the National Survey of Employment, Unemployment, and Underemployment (ENEMDU), conducted quarterly. Prior to 2014, only the fourth quarter survey of each year was nationally representative. In all years, the fourth quarter survey includes extra questions about school attendance, including current institution type and level of education. The figures and statistics presented above use information from the 2003 to 2013 fourth quarter waves. For our main analysis, 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 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 individuals 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. This includes wages for employees and profits for self-employed workers. Individuals 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). These categorizations are summarized in Appendix Table A1.

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), about half of the area and three-quarters the population of the average U.S. county.

Importantly, ENEMDU also asks individuals about migration. Specifically, individuals 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. For those whose most recent migration date is soon after 2008, this is likely to be the case. For those with more recent migration dates, it is less clear. 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 individuals who migrated to their current canton of residence 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 with the nearest public as well as the nearest private university.[8] By construction, distance is equal to zero for individuals who (in 2008) were living in a canton in which a university was located. The distance distributions are extremely right-skewed due to the Galapagos Islands, which are over 1,000 kilometers from the western coast of Ecuador. We therefore winsorize the distance variables, replacing values higher than the

[8] We do not use the precise GPS location of each university because the location of individuals is only precise to the canton-level.

99th percentile with the 99th percentile. In addition, for individuals who were living in a different country in 2008 (less than 2% of the sample), we also assign the 99th percentile of the distance distribution.

We conduct the same exercise for technological and technical institutes, which are considered to be "non-university" institutes of tertiary education and therefore also affected by the tuition fee elimination. Because enrollment at these institutes makes up such a tiny share of total enrollment in post-secondary education (see Figure A1), we do not focus non-university-level education in this paper, but we do include distance to the nearest public technical institute as a control variable in some robustness checks.

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 (income is not available). 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. In addition, we also use the 2001 census, the most recent census prior to the policy change, to control for canton-level baseline schooling levels in some specifications.

3.1 Summary Statistics

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% of individuals are out of school.[9] 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.

On average, individuals lived 25km from a public university in 2008. 21% of the sample attended college, 14% stayed for at least 4 years, and a very small share (3%) are currently still studying. The occupation indicators (which are set to zero for those not in the labor force) show that the majority of employed individuals work in blue-collar jobs. Income is only reported for individuals in the labor force (approximately 85% of the sample). We will therefore always analyze both selection into the labor force and income (conditional on being in the labor force). The sample is predominantly white or Mestizo and most do not speak an indigenous language. About one-third of the sample was born in a "below median birthplace," an indicator we generate to proxy for low socioeconomic status.

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 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

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

labor market outcomes, we restrict our main analysis to individuals at least 30 years old at the time of survey. However, when we conduct the event-study analysis on college attendance as the first outcome of interest, we relax this restriction and include individuals as young as 18 years old.

The intuition behind both strategies is the same. Essentially, 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 (where access is defined as distance to the nearest public university). 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. As discussed in section 2.2, migrating for university is very uncommon, which means that the policy should be substantially less relevant to those living far from a public university.

4.1 Event Study Analysis

Our event study analysis involves estimating the following specification, for individual i, who was aged c and living in canton j in 2008:

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

In this regression, we control for cohort (μ_c) and canton (δ_j) fixed effects, which 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 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 when we analyze labor market outcomes for individuals aged 30 and older. 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 2) or any changes to the pre-university education system implemented around 2008.

For 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, Exposed_c is an indicator equal to 1 for individuals who were young enough to be affected by the policy (21 to 24 in 2008). The upper bound of this age range is the 75th percentile of age among university students in 2007 (see Figure 3). The lower bound of this age range is a consequence of the restriction to individuals aged at least 30 at the time of survey. Exposed_c is equal to 0 for individuals past college-going age (ages 30 to 34 in 2008). This variable is missing for those in between, for whom the relevance of the policy is more ambiguous, as discussed in section 2.2. In other words, this regression restricts to individuals aged 21 to 24 or 30 to 34 in 2008.

In this specification, a negative β 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 enough to be exposed to the policy. 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. Cohort fixed effects (μ_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. The outcomes we consider include college-related and labor market outcomes, described in section 3. 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.

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 Distance_j distribution, in the absence of the policy. It is important, then, that the Distance_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. While our main specification uses a continuous distance variable, we also show the results using a binary distance variable (equal to one for municipalities within 25 km of a public university) to allow for a non-linear relationship.

In addition, this strategy is complemented well by the event-study analysis, which allows us to detect if there were any differential distance gradients across cohorts aged 30 and older in 2008. This would suggest a violation of our identification assumption because it would imply cohort trends that varied systematically across the Distance_j distribution, even for cohorts that should not have been affected by the policy.

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 terms of this distance variable, even for cohorts who were not exposed to the policy. It is important to note that our identification strategy does not rely on individuals being identical across distance categories: we include canton fixed effects and therefore only require that the cohort *trends* would have been similar in the absence of the policy. However, this exercise still offers some insight into the parallel trends assumption and motivates further tests of its validity.

We conduct this comparison exercise – similar to a balance test of pre-intervention characteristics – in Table 1, where the second column onward restricts to individuals in cohorts who were 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 individuals in the Galapagos Islands, as well as individuals who were living outside of the country in 2008). However, across the three middle distance bins, individual characteristics appear to be quite balanced.

As mentioned above, identification does not require these groups to be similar, but 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. As a robustness check, we therefore also repeat our analysis restricting only to individuals in these three middle distance categories.

5 Results

5.1 Education Outcomes

We begin with a simple graphical analysis. We compare individuals young enough to be exposed to the policy to those who were too old when the policy was implemented, across individuals living varying distances from a public university in 2008. We consider those aged 15-24 in 2008 as exposed to the policy, and those aged 30-39 as not exposed to the policy. Unlike the labor market outcomes we study later, college attendance does not require individuals to be out of school by the time they are surveyed; we therefore relax the age-30 restriction for this analysis.

Figure 4 illustrates the proportion of individuals that attended college, separately for these exposed and not exposed cohorts, across varying distances to the nearest public university. Across all but one distance bin, the proportion of exposed cohorts that attended college is higher than the proportion of unexposed cohorts that attended, which reflects the fact that college enrollment has been increasing across cohorts. 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 trends that would have existed even without the tuition fee elimination.

This figure also shows that, for both young and old cohorts, college attendance rates are generally highest for those living closest to a public university. Because there are substantial differences across the distance bins, even for cohorts not affected by the policy, a naive cross-sectional comparison of individuals living nearby and far from a university would likely be subject to endogeneity concerns. Notably, however, if we focus on the three middle distance bins, we see that college attendance levels for the unexposed cohorts are similar, suggesting that underlying differences between these groups are less of a concern (consistent with the summary statistics in Table 1). Yet for the exposed cohorts in these three distance bins, we see a significant negative trend in college attendance levels as we move to higher distance bins. In short, even across groups where the unexposed cohorts are quite similar, geographic access to public universities appears to predict college attendance for individuals affected by the policy.

Importantly, the gap between exposed and not exposed cohorts shows a striking pattern. Focusing on the gap (rather than the levels) essentially controls for any level differences in college attendance across distance bins, allowing us to take all five distance bins into account. Across all distance bins, the gap decreases with distance.

These observations are all consistent with the tuition-free policy having a causal impact on college decisions. Individuals young enough to be exposed to the policy had higher levels of college attendance than those who were not, and this difference was more pronounced for those who had access to the public universities where the policy was actually implemented. As discussed above, migrating to attend university is rare, which makes geographic distance a good measure of how relevant the policy was for a given individual.

We now move on to the event study analysis described by equation (1). In Figure 5, 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).

There are several important patterns to note. First, the coefficients for all cohorts who were aged 24 or younger in 2008 are negative and statistically significant. These are the individuals who were young enough to be affected by the policy. Geographic access to public universities (distance) matters more for these cohorts than for those aged 32 in 2008. Importantly, 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 exposed – that is, 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 in 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 A2 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. Before looking at their labor market outcomes, we explore other education-related outcomes for this sample, using the difference-in-differences specification reported in equation (2).

Table 2 reports our difference-in-differences estimates of the effect of the policy on education outcomes. Specifically, we report estimates of β (the coefficient on the interaction between Exposed_c and Distance_j) in equation (2), first without and then with province-by-cohort fixed effects. We begin with the same outcome variable (college attendance) to see if the conclusions from the event study analysis are supported by the results of the difference-in-differences specification using an older sample. In both specifications for college attendance, we report negative coefficients that are significant at the 1% level. Indeed consistent with the previous analysis, these results indicate that free tuition significantly increased college attendance. Specifically, 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). This is approximately 17% of the gap between individuals born in above-median and below-median birthplaces (13% of people from below median birthplaces ever attended college, compared to 25% from above-median birthplaces).

In addition, the fee elimination increased the likelihood of staying in college for at least four years (an imperfect proxy for college completion). In both specifications, living 40 km further away from a public university is associated with an effect size of 2 percentage points, approximately 14% of the mean. There is also evidence that the policy increased the likelihood of sample individuals being still in school at the time of survey, although this is only significant with the inclusion of province-by-cohort fixed effects.

The final column of Table 2 reports the result of an important falsification test. Because the individuals in 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 schooling trends (across cohorts) varying systematically across canton 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 that this is not the case. The policy had no significant effect on high school completion in either specification, providing further evidence that the results in the first three columns were due to the policy.

It is not straightforward to compare these effect sizes to those in the existing literature, which has generally studied contexts very different from Ecuador's and used strategies that yield estimates with different interpretations. With this caveat in mind, the effects estimated in Table 2 are comparable to existing estimates. For example, Dynarski (2003) estimates that an additional 1000 USD in financial aid induces a 3.6 percentage point increase in the share of high school graduates attending college, at a time when tuition and fees totaled approximately 1900 USD. In France, where tuition fees are close to zero and living expenses represent one of the major barriers to low-income students' college participation, Fack and Grenet (2015) document that a 1500-Euro cash allowance (approximately one-quarter of estimated living expenses) increases college enrollment of low-income students (around an eligibility cutoff) by 2.7 percentage points. Because both studies restrict to high school graduates, we scale our estimates by the share of high-school graduates in the sample (0.48) and estimate that the one-standard deviation effect of the Ecuadorian policy on college enrollment (among high school graduates) is 4 percentage points. In other words, the Ecuadorian policy led to a slightly larger effect than in the Dynarski (2003) study but covered a much larger share (100%) of tuition costs. Compared to the French policy, which subsidized living expenses rather than tuition, the Ecuadorian policy also led to a slightly larger effect (in level terms, and an even larger effect in percentage terms).

5.2 Labor Market Outcomes

Having established that the tuition fee elimination significantly increased college enrollment, we next ask how it affected labor market outcomes. Table 3 reports the coefficient estimates from the same specification (2), using various labor market outcomes as the dependent variables of interest.

In column 1, we see 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 and 14% of the socioeconomic gap in white-collar shares – comparing above-median and below-median birthplace individuals). The magnitude of this effect is consistent with the magnitude of the effect on college attendance (2 percentage points), given that slightly over half of those who have attended college in our sample end up in a white-collar job. In column 2, 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 does not appear to have affected labor force participation; the coefficients in column 5 are very small relative to the mean and statistically insignificant. This suggests that the policy affected the job choices of individuals already in the labor force rather than moving individuals from out of the labor force into high skilled jobs. In addition, we do not detect any significant effects of the policy on income (conditional on being in the labor force). The income coefficients switch signs across specifications and are not significant in either row. 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 even a small positive effect.

There are two other points to note about the income results. First, Panel B of 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. This could result in an underestimation of the income effect, though this underestimation would likely be quite small given that only 3% of the entire sample is still attending school.

Secondly, individuals who spend 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, and we do not have the information to calculate work experience in our data. However, the steepest returns to experience (proxied by age) are for individuals in their twenties, as shown by the age-income profile in Appendix Figure A3. This means this problem would be most serious for individuals under 30, who are not included in these regressions.

Complementing this simple difference-in-differences strategy, Figure 6 plots the results of event study regressions for the main outcomes of interest – the two white-collar variables that were significantly impacted, as well as labor force participation and income – using the specification with province-by-cohort fixed effects.[10] Because we are restricting to individuals

[10] Though the major conclusions across the two specifications are generally consistent, the province-by-cohort fixed effects is the preferred specification because it accounts for any differential trends across

age 30 and older, these event studies have smaller sample sizes (and fewer young age cohorts) than the college attendance analysis in Figure 5, 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 individuals up to age 39 in 2008, which allows us to detect the potential existence of pre-trends. This analysis is also valuable because it does not rely on the classifications of age cohorts into exposed and unexposed categories (which was motivated by data but also eventually determined by somewhat arbitrary decisions).

The first panel reports the coefficients from 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 5 and offers strong evidence that the policy increased participation in these high-skill white-collar jobs. The lower-skill white-collar figure does, generally, appear to be the mirror image of the high-skill white-collar one, though the pattern is less sharp.

The next two panels of Figure 6 confirm 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 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.

provinces. This allows for oil-reliant provinces, for example, to demonstrate different cohort trends than other provinces (potentially driven by oil-price shocks). It also allows for provinces that specialize in industries that trade with the United States to exhibit differential cohort trends due to the Great Recession.

5.3 Robustness Checks

5.3.1 Alternate Sample Restrictions

In the above 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 4 that our results are not sensitive to the choice of cutoff year.

First, we report the results from using 2010 as a cutoff, in Panel A of Table 4. 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 Tables 2 and 3.

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 4 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.

5.3.2 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 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 Exposed_c x Distance_j interaction coefficients. In Panel A of Table 5, we see no such reduction. Estimated coefficients are almost identical to those in Tables 2 and 3, 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.

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

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

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 (see Appendix Figure A1).

5.3.3 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 within 25km of 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 municipalities close to 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.

5.3.4 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 Exposed_c x Distance_j interaction in a regression on migration. Table 6 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 6 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.

5.4 Heterogeneity

We next explore heterogeneity across gender, race, language, and birthplace characteristics and report the results in Table 7. 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 7 illustrates that men and women were both affected by the fee elimination. The effect on female college attendance was slightly larger in magnitude than (though not significantly different from) the male effect. Both genders shifted into high-skill white-collar jobs.

In Panel B, there do not appear to be substantial differences across race. The college attendance effect on the white and Mestizo group is almost identical to the effect for all other races. The effect on high skill white-collar jobs appears to be concentrated among whites and Mestizos, though the effect sizes of the two groups are not significantly different.

There is much 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.

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. 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 essentially 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.

Appendix Tables A2 to A5 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. These tables demonstrate strong evidence that the policy affected primarily those of higher socioeconomic status. While there continues to be no evidence for racial differences in the effect of the policy, there are some specifications in which gender differences become more stark (and statistically significant), with larger effects for women.

With regard to socioeconomic heterogeneity, of particular importance are the results in Panel E, which restrict to individuals in the three middle distance bins. Because individuals who were born in below-median birthplaces are likely to also live further away from a public university in 2008, it is possible that the insignificant effects estimated for the below-median birthplace group are simply a result of most of them being too far from a public university. Table 1, for example, shows that the share of below-median birthplace individuals is substantially smaller among individuals who were living in the same canton as a public university, suggesting different distance distributions across the above- and below-median groups. However, Table 1 also shows that below-median birthplace shares are very similar across the three middle distance categories, which implies that the distance distribution for individuals in these these middle distance groups are actually quite similar across above- and below-median individuals.[12] Therefore, the fact that we still see such stark heterogneity across socioeconomic status among the restricted sample of middle distance individuals alleviates concerns about different distance distributions being the reason for the birthplace heterogeneity we document in Table 7. Although we note some of the socioeconomic differences are no longer statistically significant with the drastically reduced sample size, the magnitudes of the differences are still very large.

The failure of the policy to benefit disadvantaged groups could be due to two factors. First, prior to the policy, individuals from poor households could ask for financial support from their university to help alleviate costs, or faced lower fees, though 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 in 2007.) 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 tuition 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. Gaps also exist in the types of secondary school education students receive. Until 2011, secondary students could enroll in one of several different types of *Bachillerato* programs:

[12]We provide additional evidence for this in Appendix Figure A4, which illustrates the distance distributions separately for above- and below-median individuals, in the full sample and middle distance sample.

some are intended to prepare students for university, while others offer more trade-specific training. Many students in rural areas were "confined to secondary schools that typically offered non-college bound types of *Bachillerato*, and thus were automatically denied the possibility of accessing university" (Cevallos Estarellas and Bramwell, 2015, p. 344).

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. Without first ensuring equality in access to and completion of a quality secondary school education, policies at the tertiary level may be limited in their ability to impact inequality.

6 Discussion

In this paper, we evaluate the effects of an Ecuadorian policy that eliminated university tuition fees in 2008. Using event study and difference-in-differences strategies to identify the effects of the policy, we find that it increased college enrollment and shifted individuals into higher-skilled jobs. Specifically, living 40 km (1 standard deviation) closer to a public university translated into a 2 percentage point larger increase in the likelihood of attending college (and a 1 percentage point larger increase in the likelihood of having a high-skill white-collar job) for exposed cohorts compared to non-exposed cohorts.

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 from the policy in general. Individuals who speak an indigenous language and those who were born in poor cantons demonstrated no improvements in college enrollment or occupation choice as a result of the policy. This could be due to the fact that poorer individuals were able to avail of financial support even before the policy (which would make the fee elimination a smaller price reduction for them), but these financial support policies were university-specific and not always guaranteed. 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. For these disadvantaged groups, designing policies that ensure their access to a quality secondary school education would be an important first step to ensuring they have access to tuition-free university.

We find no evidence that the fee elimination increased income. Although we acknowledge that we may lack the statistical precision to uncover even large effects on income, we note that the sign of our estimated coefficient is inconsistent with a positive income effect. There are a few reasons why the policy may have failed to improve income. First, it is possible that the increase in college enrollment resulted in a decline in the quality of a university education due to an increase in student-teacher ratios. Our rough estimates, however, suggest that the increase in student-teacher ratios would not have been large enough to meaningfully affect the quality of instruction for our study cohorts. From 2007 to 2011 (the year of peak college enrollment, in which our youngest cohorts were turning 24), we estimate that the student-teacher ratio at public universities increased – at most – by less than 2 students per teacher per year, on average.[13]

It might also be the case that the individuals induced by the policy to attend college were low-ability individuals. This could have resulted in a decline in the quality of learning due to negative peer effects (Bianchi, 2018). Given the small size of the student-teacher ratio increase, any negative peer effects that do exist would likely be quite small. However, the possibility that low-ability students were most affected by the policy provides another explanation for the limited income benefits of this policy. If returns to college are heterogeneous across the ability distribution and if low-ability students have the lowest returns, increasing the educational attainment of these individuals would yield limited increases in income on average.

The fact that the policy significantly improved job opportunities for the affected individuals casts some doubt on the idea that only low-ability students were affected, or that the

[13]Because teacher counts are not available prior to 2012, we make conservative assumptions that provide us with an upper bound. See Appendix section A.1 for details.

quality of education was too low to improve skills. These individuals did succeed in getting more high-skilled white-collar jobs, but did not earn substantially higher income (despite the fact that these jobs pay on average more than all other job categories, at any age). This suggests that general equilibrium effects driven by an oversupply of college-educated workers could be in play.

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.

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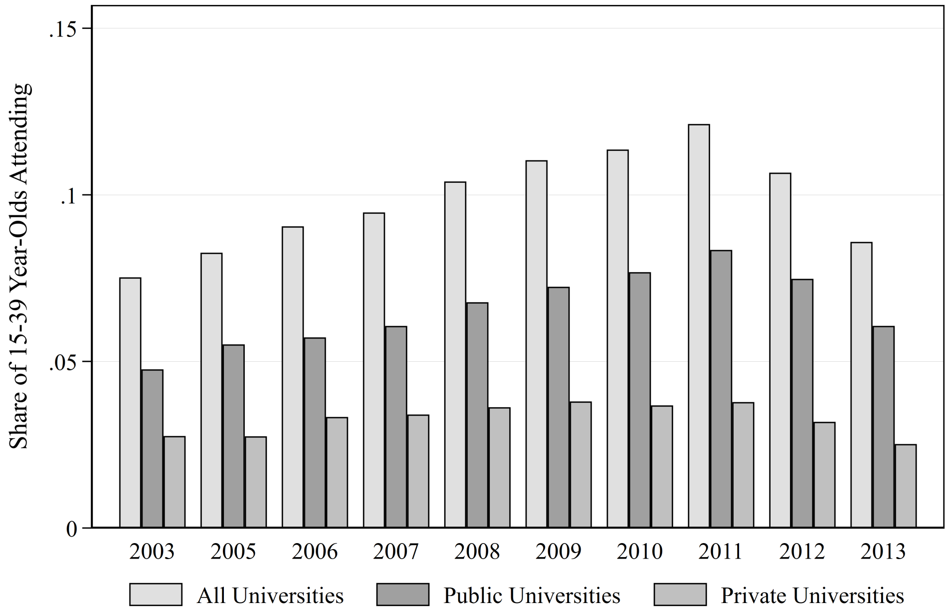
Tables and Figures

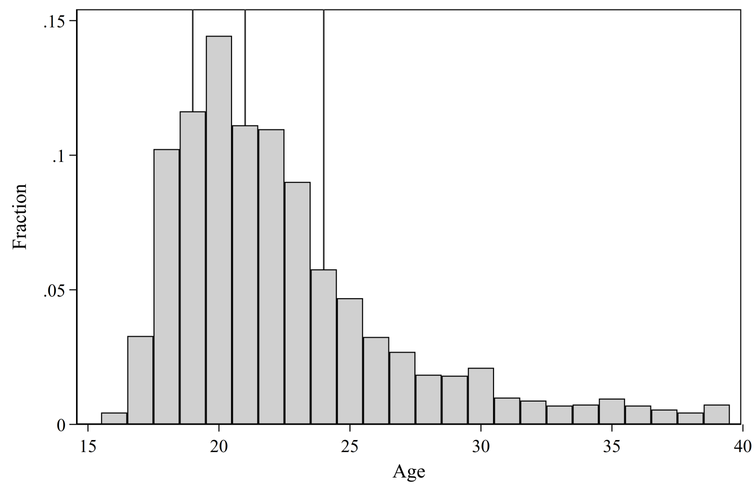
Figure 1. Current University Attendance by Year



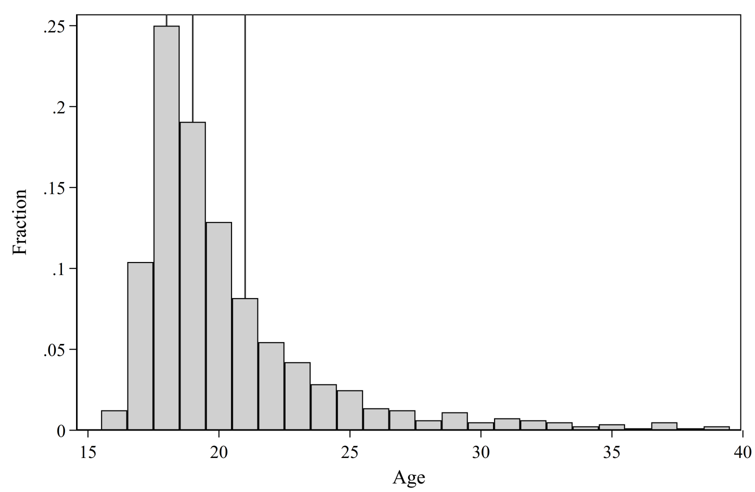
Notes: Sample includes individuals under 40 years old in the 2003-2016 quarter 4 ENEMDU surveys.

Figure 2. Age Distribution Among University Students, 2007

A. All University Students

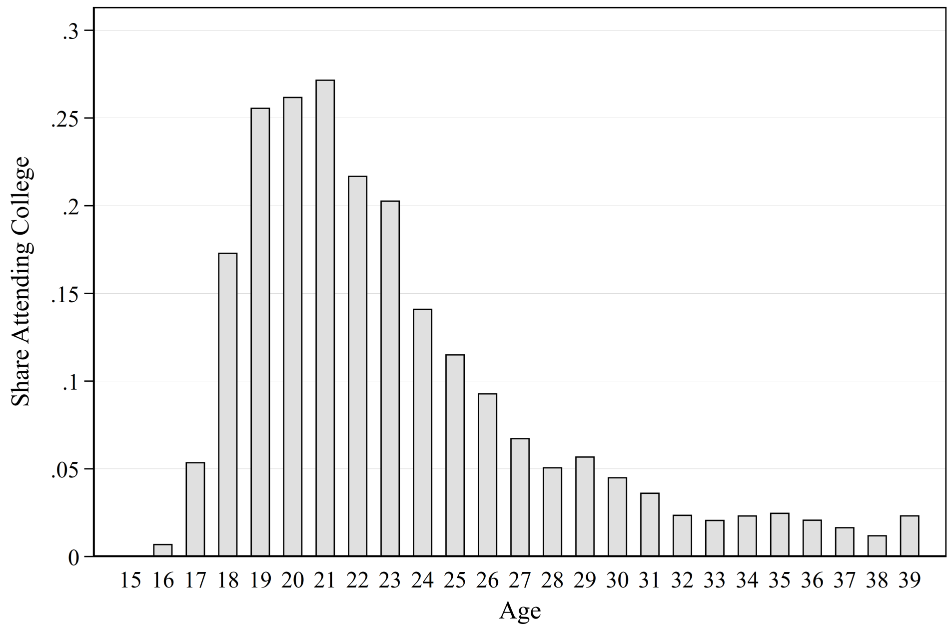


B. First-Year University Students



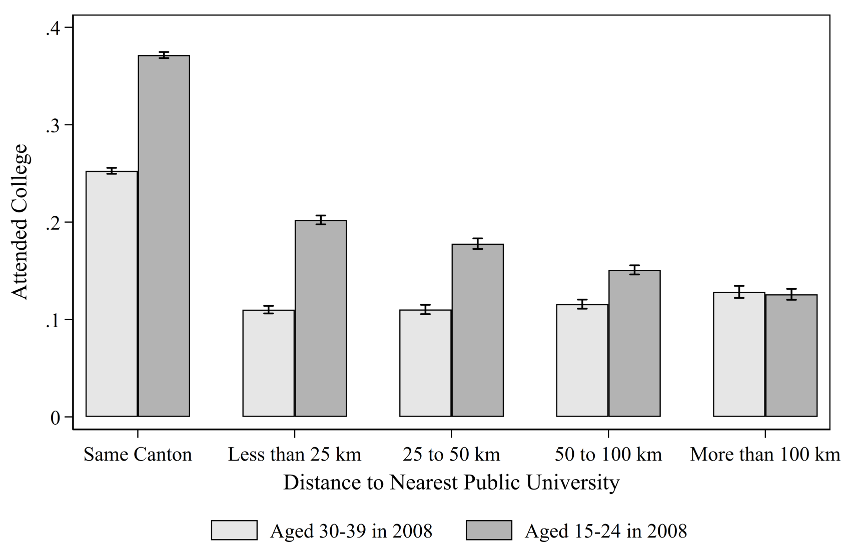
Notes: Sample includes individuals under 40 years old in the 2007 quarter 4 ENEMDU survey. Panel A restricts to individuals currently attending university. Panel B restricts to individuals currently attending their first year of university. Vertical lines represent the 25th, 50th, and 75th percentiles of the distribution. By restricting to individuals younger than 40, Panel A omits less than 5% of current university students and Panel B omits less than 2% of current first-year students.

Figure 3. Current University Attendance by Age, 2007



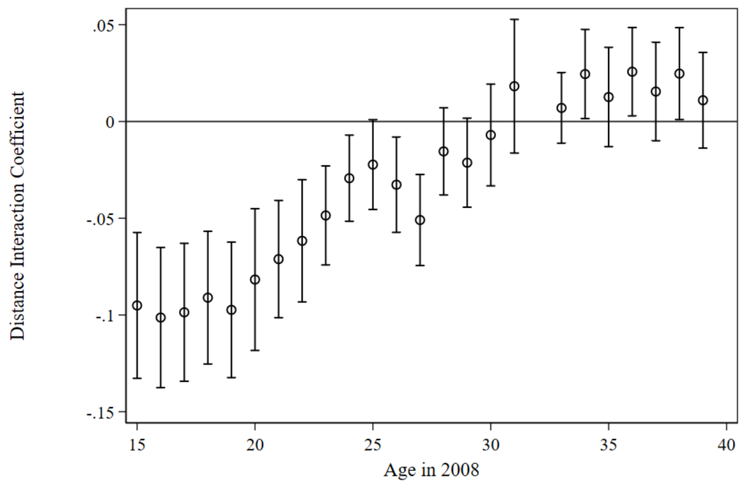
Notes: Sample includes individuals under 40 years old in the 2007 quarter 4 ENEMDU survey.

Figure 4. College Attendance, by Cohort and Distance to Nearest Public University



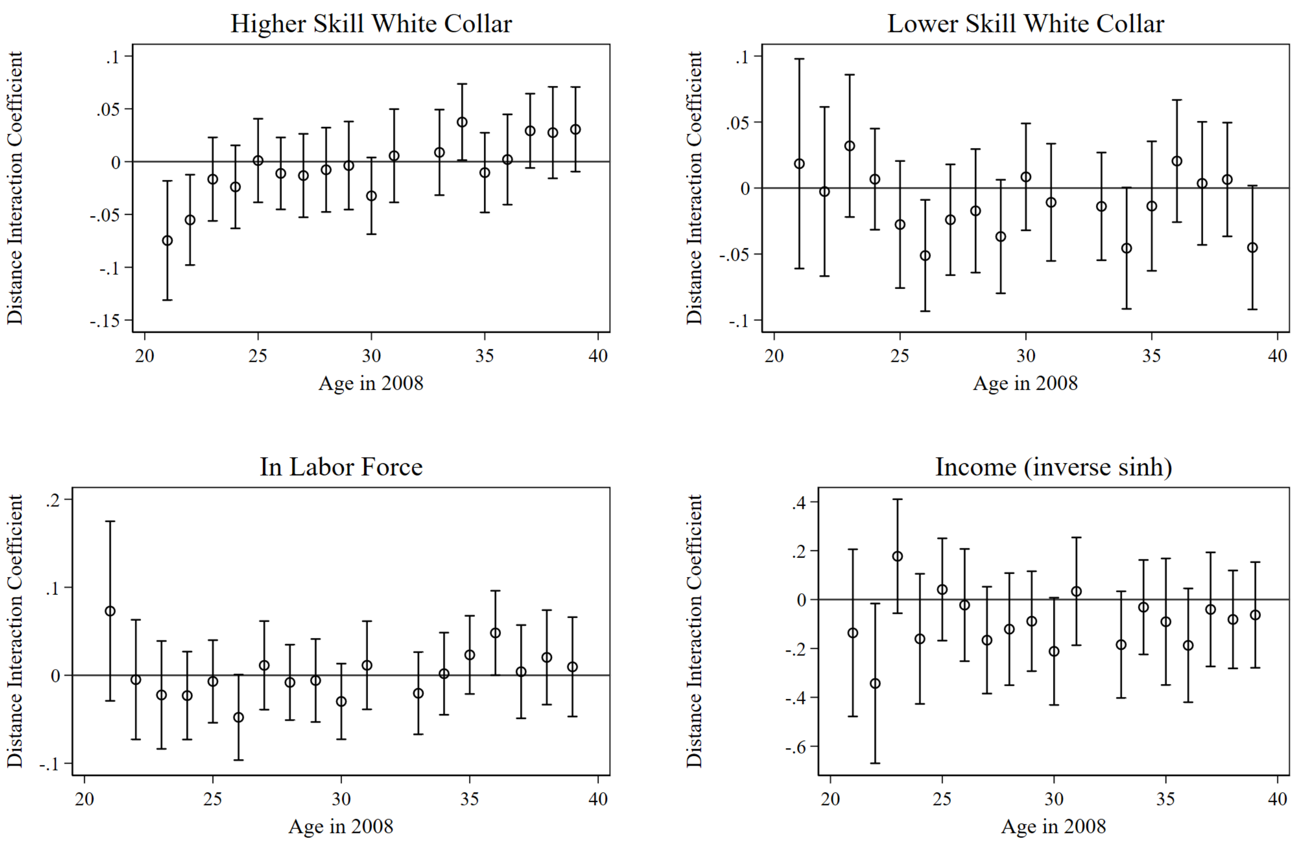
Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys who were aged 15-24 or 30-39 in 2008. Bars denote 95% confidence intervals.

Figure 5. 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 6. Labor Market Outcome 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 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)

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 Educational Outcomes

	(1) Attended College	(2) 4 Years of College	(3) Attending School	(4) Graduated High School
	A. Baseline Specification			
Exposed x Distance	-0.043***	-0.039***	-0.0025	-0.0013
	(0.010)	(0.0077)	(0.0064)	(0.015)
	B. Province-by-Cohort Fixed Effects			
Exposed x Distance	-0.058***	-0.050***	-0.015***	0.026
	(0.014)	(0.012)	(0.0056)	(0.019)
Dep. Var. Mean	0.21	0.14	0.028	0.48
N	110044	110044	110044	110044

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. Effects of Tuition Fee Elimination on Labor Market Outcomes

	(1) Higher Skill WC	(2) Lower Skill WC	(3) Higher Skill BC	(4) Lower Skill BC	(5) In Labor Force	(6) Income ($\sinh^{-1}$)
	A. Baseline Specification					
Exposed x Distance	-0.021**	0.011	-0.0058	0.013	-0.0028	-0.051
	(0.0084)	(0.0096)	(0.0090)	(0.011)	(0.0088)	(0.033)
	B. Province-by-Cohort Fixed Effects					
Exposed x Distance	-0.034***	0.025*	0.0034	0.0039	-0.0026	0.033
	(0.012)	(0.014)	(0.013)	(0.020)	(0.014)	(0.073)
Dep. Var. Mean	0.14	0.21	0.25	0.24	0.84	6.26
N	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 4. 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.

Table 5. 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 Distance $\leq$ 25km	0.028***	0.021***	-0.014*	-0.0010	0.027
	(0.0090)	(0.0071)	(0.0082)	(0.0080)	(0.042)
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.

Table 6. 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.

Table 7. 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).

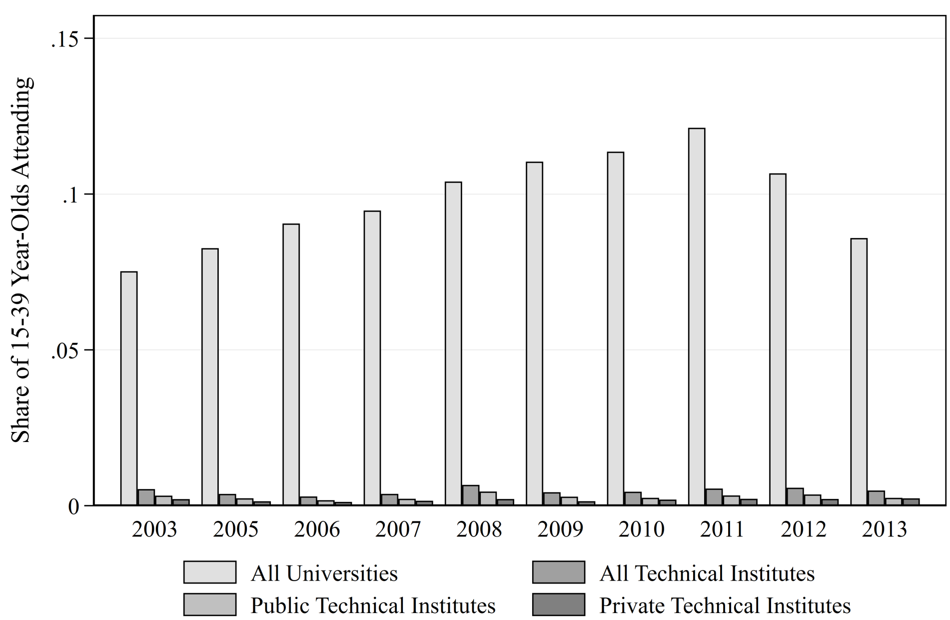
A Appendix

A.1 Estimating Student-Teacher Ratio Changes

To estimate the change in student-teacher ratios from 2007 to 2011 (the year when college enrollment peaked), we first calculate weighted counts of the total number of public university students from the 2007 to 2011 quarter 4 ENEMDU surveys. We also use data from SENESCYT on the number of teachers at each university (and use counts from public universities). Because SENESCYT only began collecting data on the number of teachers per university in 2012, we make the assumption that the number of teachers was flat from 2007 to 2012. Because teacher counts increased by 9% from 2012 to 2013 and by 7% from 2013 to 2014, we argue this is a conservative assumption that will give us an upper bound on the student-teacher ratio increase over the 2007 to 2011 time period.

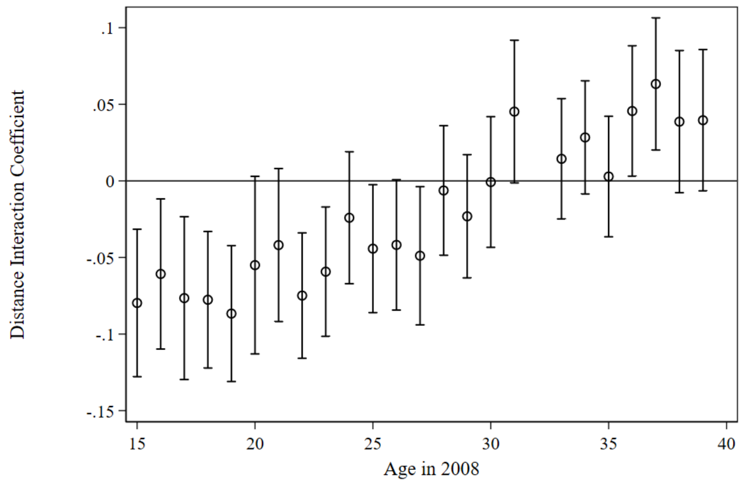
A.2 Additional Figures and Tables

Figure A1. Current Post-Secondary Attendance by Year



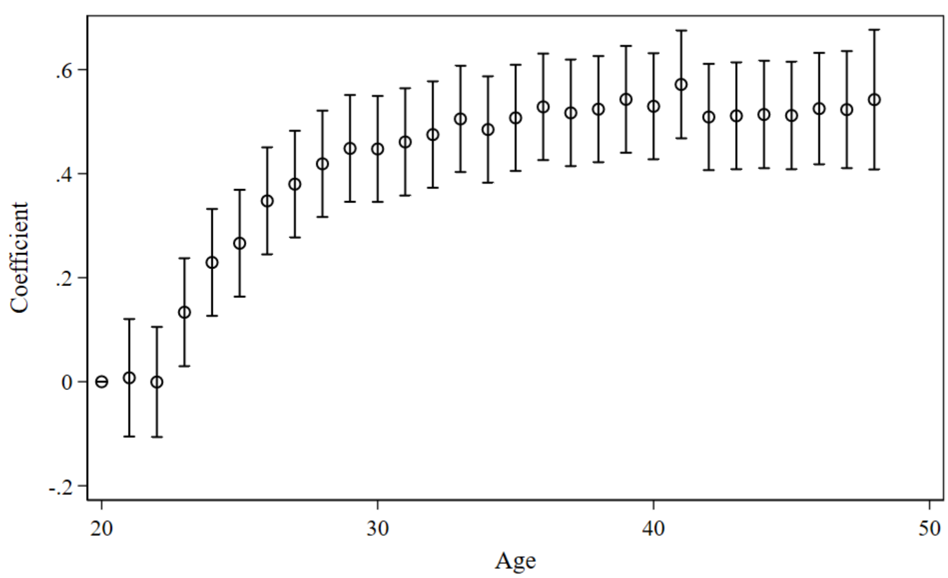
Notes: Sample includes individuals under 40 years old in the 2003-2016 quarter 4 ENEMDU surveys.

Figure A2. 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.

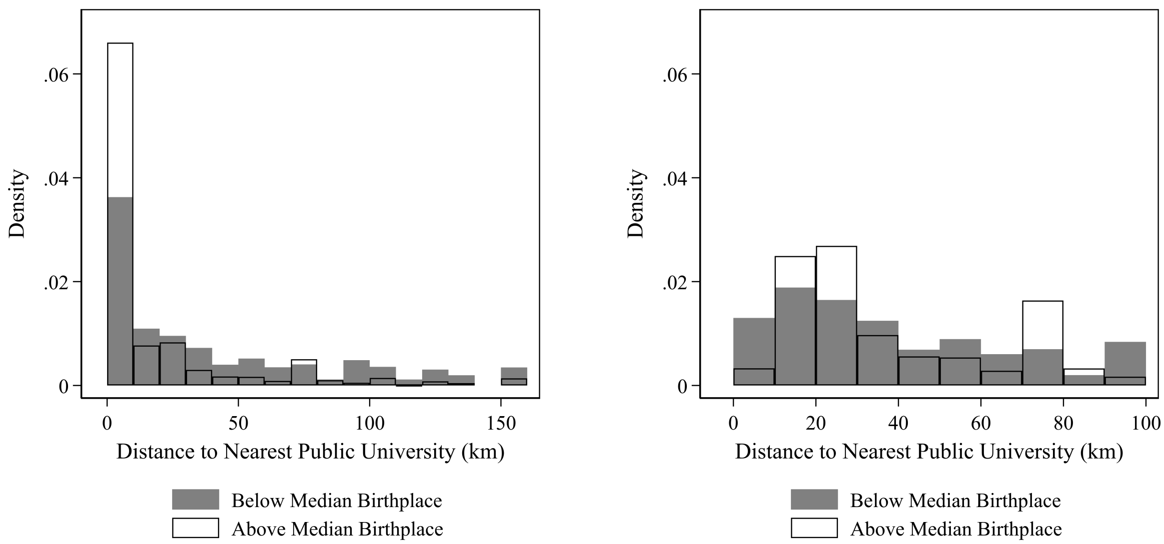
Figure A3. Income-Age Relationship



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.

Figure A4. Distance Histograms By Birthplace Type

A. Full Sample　　　　B. Middle Distances



Notes: Sample includes individuals in the 2014-2017 ENEMDU surveys, aged 15-39 in 2008. Distance is winsorized at the 99th percentile. Middle Distances restrict to individuals living less than 100 km from (but not in the same canton as a) public university in 2008.

Table A1. Occupation Classifications

ISCO Occupation Code	Occupation Classification
1 Legislators and managers	Higher Skill White Collar
2 Professionals	Higher Skill White Collar
3 Technicians	Higher Skill White Collar
4 Clerks	Lower Skill White Collar
5 Service/shop/market workers	Lower Skill White Collar
6 Skilled agricultural/fishery workers	Higher Skill Blue Collar
7 Crafts and related trades	Higher Skill Blue Collar
8 Plant operators	Lower Skill Blue Collar
9 Elementary occupations	Lower Skill Blue Collar

Table A2. 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 ($\leq$ 25km)					
Male	0.0087	0.022**	-0.021	0.00023	-0.041
	(0.013)	(0.0100)	(0.013)	(0.0048)	(0.045)
Female	0.046***	0.022***	-0.0089	-0.0052	0.12
	(0.011)	(0.0085)	(0.012)	(0.014)	(0.079)
Difference	-0.037**	-0.000022	-0.012	0.0054	-0.16*
	(0.016)	(0.012)	(0.019)	(0.015)	(0.092)
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.

Table A3. 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 (≤ 25km)					
White or Mestizo	0.024**	0.022***	-0.012	0.0016	0.012
	(0.0098)	(0.0080)	(0.0095)	(0.0086)	(0.047)
Other	0.036**	0.017	0.0040	-0.016	0.050
	(0.014)	(0.012)	(0.013)	(0.017)	(0.076)
Difference	-0.012	0.0045	-0.016	0.017	-0.038
	(0.017)	(0.014)	(0.016)	(0.019)	(0.090)
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 A4. 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 ($\leq$ 25km)					
Speaks Indigenous	0.0042	-0.00014	0.013	-0.034*	0.10
	(0.016)	(0.015)	(0.030)	(0.018)	(0.13)
No Indigenous	0.026***	0.020***	-0.011	0.0040	0.0055
	(0.0092)	(0.0072)	(0.0084)	(0.0081)	(0.042)
Difference	-0.021	-0.020	0.024	-0.038*	0.097
	(0.018)	(0.016)	(0.032)	(0.020)	(0.13)
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 A5. 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 ($\leq$ 25km)					
Below Median	-0.028*	-0.0033	-0.047***	0.010	-0.025
	(0.016)	(0.012)	(0.015)	(0.013)	(0.076)
Above Median	0.052***	0.025**	0.0056	-0.0011	0.024
	(0.011)	(0.010)	(0.013)	(0.012)	(0.060)
Difference	-0.081***	-0.028*	-0.053**	0.011	-0.049
	(0.018)	(0.016)	(0.021)	(0.016)	(0.097)
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).