

Signaling vs. Human Capital: Evidence from a reform in Colombia's top University

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In this paper I test whether the returns to college education are due to increases in productivity (human capital theory) or instead, to the fact that attending college signals higher ability to employers. I exploit a reform at Universidad de Los Andes which in 2006 reduced the amount of coursework required to earn a degree in economics and business. The size of the entering class, their average high school exit test scores, and graduation rates were not affected by the reform, indicating that the quantity and quality of students remained the same. Thus, the reform decreased human capital students graduate with, while holding the value of the education signal constant. Using administrative data on wages and college attendance, I find that wages fell by approximately 16% in economics and 12% in business. These results suggest that human capital plays an important role in the determination of wages, and reject a pure signaling model. In addition, comparing this number to an OLS estimate that combines both human capital and signaling effects, my results imply that human capital accounts for most of the return in schooling. Surveying employers, I find that the decline in wages may have resulted from a decline in performance in recruitment processes, which led to a smaller pool of jobs to choose from. Using data from the recruitment process for economists at the Central Bank of Colombia, I find that the reform reduced the probability of students from Los Andes being hired by 17pp.

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I Introduction

Education is one of the most important determinants of wages at the individual level. Returns to a year of schooling are estimated to be positive and large in most countries, ranging from 2 to 20 percent around the world (Montenegro and Patrinos, 2014). Moreover, the earnings premium associated with college has risen substantially in the last decades (Oreopoulos and Petronijevic, 2013). In spite of this, there is much debate about the mechanisms by which education leads to higher wages. The human capital theory argues that education increases productivity, and rises wages as a result (Becker, 1964 and Mincer, 1974), whereas the signaling theory states that it reflects the correlation between education and unobserved ability. Spence (1973) provides a model in which higher ability individuals increase their education to signal their ability to employers, and thus increase their wages, but where education is otherwise useless in terms of productivity. If the signaling theory is important, it implies that the social returns to education could be lower than the private returns, and thus can call into question the rationale for public investment. Despite the importance of this debate, this remains an open question (Lange and Topel, 2006). The fundamental difficulty in distinguishing these two theories arises because many of the empirical implications are identical.² In both models, the decision processes of firms and workers are the same. Firms weigh the productivity of workers with different levels of education against their wages and select the education level that maximizes profits. Workers weigh the increased wages against the cost of education and choose the level of education that maximizes their utility. In both setting, higher ability workers obtain higher levels of schooling and are paid more. Of course the two theories are not mutually exclusive.

In this paper I identify the extent to which college education increases productivity and wages, by exploiting a curriculum change at Los Andes, the top university in Colombia. In 2006 the time required to earn a college degree in economics and business decreased from 4.5 to 4 years. This was accomplished by dropping 12 required courses in economics and 6 in business, which was equivalent to a reduction in credits of 20% and 14%, respectively.³ Crucial to my identification strategy, the reform did not alter the quality of the entering class. At Los Andes, the admission process is constrained by a limited number of slots and is solely based on the national standardized high school exit test. I show that the size of the entering class did not grow, nor the average entrance test scores decreased, and dropout rates didn't change

² Lang and Kropp (1986) stated that "Many members of the profession maintain (at least privately) that these hypotheses cannot be tested against each other and that the debate must therefore be relegated to the realm of ideology".

³ In economics the change in curriculum not only reduced the number of semesters, but also the number of courses per semester. Before the reform students were supposed to take six courses per term and this was changed to five. In business the number of classes per term was unchanged at five.

with the reduction in the number of classes. All together the reform had no short run effect on the quality of the affected entering class and thus, it decreased human capital exogenously, while at the same time held the signaling value of the degree constant. The human capital model predicts a decline in wages as a result of the reform, whereas the signaling model does not. This setting constitutes an ideal natural experiment to learn about signaling vs. human capital.

To estimate the effect of the reform I use individual information on wages and educational attainment, in a difference in difference framework. I compare wages in the formal sector before and after the reform for economics and business students from Los Andes and other top 10 schools in Colombia. This schools did not reform their degrees. I find that after the reform, wages for students from Los Andes fell by approximately 16% in economics and 12% in business, and that the effects are statistically significant. The result suggest that human capital plays an important role in the formation of wages and reject a model in which signaling is the only role of college education. Although I allow for heterogeneity in the effect of the reform using Athey and Imbens (2006) changes in changes estimator, I find a homogenous effect along the wage distribution, in other words, wages declined proportionally for high and low earners. Using data from the Survey of Quality of Life for 2008-2012, I estimate that the OLS returns to one year of higher education is 17%. Interpreting the reduction of graduation requirements for economics as a reduction of one year of schooling and in business as a reduction of one semester, my results provide novel evidence, they suggest that human capital accounts for the largest share of the return to college education.

I investigate the mechanisms that led to lower wages. Using data for economics graduates from Los Andes, I find that the distribution of employers changed with the reform. Moreover, there is a relationship between the classes dropped and the placement of graduates across employers. I interviewed many of the top employers and found that most of them knew about the reform, they stated that they were able to detect the change in human capital through tests performed in their recruitment process, and argued that some knowledge made optional in the new curriculum is vital to some jobs. All of the above suggest that under the new curriculum, the pool of jobs graduates can obtain is smaller because they performed worse during recruitment, which subsequently decreased their wages. I find support for this hypothesis. Using data from the recruitment process for economists at the Central Bank from 2008 to 2014, I find that the probability of being hired for graduates from Los Andes fell by 17pp with the reform.

There are several potential issues with my approach. It is possible that the reform in curricula changed the pool of applicants and entrants in dimensions, not captured by high school exit tests which are relevant to the labor market. Specifically, given the decline in requirements to graduate, lower ability

individual should be induced into enrolling in these programs, which would lead to a decrease in the value of the signal and in wages. In order to address this, I estimate an alternative specification, taking as the treatment group only the students at Los Andes who were already enrolled at the time of the reform but studied under the new curriculum. The result to this alternative treatment group are similar to the benchmark specification. Also, one might be worried about the possibility that my estimates capture a negative trend in the return to a degree from Los Andes. To test if this is the case I perform two exercises; I replicate my baseline estimation using a placebo date for the reform, and also test my specification using a major at Los Andes that didn't undergo a reform in curriculum. The data suggest there is no change in wages in the placebo date, or for the placebo group. My results are robust to several additional checks explained in the robustness section. Finally, in order to interpret the reduction in wages as the causal effect of human capital, the choices underlying labor force participation should be unaffected by the reform. One of the motivations behind the curriculum change was to increase graduate school enrollment. If the reform had this effect, and delayed working, my result could be confounding a change in the composition of the new graduates in the labor market. I use LinkedIn data to check if the reform increased the share of students attending graduate school, but find no evidence of an increase.

The primary contribution of this paper is to identify the role of signaling and human capital in a college setting using a natural experiment. A number of papers in the literature have investigated this issue for primary and secondary education and have found mixed results. Eble and Hu (2016), exploit the introduction of one extra year in primary school in China in 1980, and find a small increase in wages, which leads them to conclude that there is an important role for signaling in primary education. There was however, no extra coursework introduced in that additional year. Lang and Kropp (1986) and Bedard (2001) find secondary schooling decisions that are consistent with a signaling model, and that would reject a pure human capital framework. Another strand of the literature attempts to measure directly if there is a signaling value to education degrees. Tyler, Murnane, and Willett (2000) estimate the signaling value of the GED to be between 12% and 20%, whereas, Martorell and Clark (2014), find little evidence of high school diploma signaling effects.

To my knowledge this is the first paper to look at the signaling and human capital question at the college level. This is particularly relevant because universal enrollment in primary education and school leaving age laws, constrain schooling decisions in primary and secondary education, and as consequence make college a good candidate to signal ability. In addition it is easier to argue that skills provided in primary and secondary education are of use in the workplace, as opposed to those acquire in college, and finally, there is greater debate about the role of public spending in financing college education. This is also the first paper to look at the mechanisms that led to changes in wages, this is important because it

provides information about the tools employers use to learn about workers productivity.

The rest of the paper is structured as follows: Section 2 describes a simplified version of a signaling and human capital model to derive testable implications in my context; Section 3 discusses the curriculum reform at Los Andes; Section 4 describes the data, the empirical strategy, and the results; Section 5 presents some robustness checks; Section 6 explores the channels that explain the results; and the last section offers some concluding remarks.

II Signaling vs. Human Capital

In this section I lay out a simple model that allows me to derive a test of the signaling and human capital theories, by exploiting a reduction in curricula in the best university, in a context of ability based admissions and a binding number of slots.

Individuals have ability θ_i distributed with continuous support. There are J schools that offer different levels of human capital accumulation f_j , where higher human capital requires higher effort, and j indicates school ranking. The cost to attend school j for individual i increases in the level of human capital and decreases in the level of ability (single crossing property), such that $c(f_j, \theta_i) > c(f_k, \theta_i)$ for every i when $j < k$, meaning j offers higher human capital than k , and $c(f_j, \theta_i) < c(f_j, \theta_m)$ when $\theta_i > \theta_m$.

Firms' value $\mu(\theta_i, f_j)$, which is a linear transformation of unobservable intrinsic ability θ_i and human capital specific to each school f_j . In a separating equilibrium, agents signal their type, and firms will predict ability based on the observed level of human capital, and offer wages accordingly.

$$w_j = \mu(E[\theta_i | f_j], f_j) = \alpha_1 + \alpha_2 \bar{\theta}_j + \alpha_3 f_j \quad (1)$$

Students will choose school trying to maximize wages net of effort costs:

$$w_j - c(f_j, \theta_i) = \mu(E[\theta_i | f_j], f_j) - c(f_j, \theta_i) \quad (2)$$

Thus, a student chooses to attend the top school whenever:

$$w_1 - c(f_1, \theta_i) \geq w_2 - c(f_2, \theta_i) \quad (3)$$

Because both sides are strictly increasing in θ (single crossing property), there exists a unique θ^1 such that $\forall \theta \geq \theta^1$ (3) will hold. Subsequently, there is a threshold θ for each pair of schools that determines school choice over the school ranking.

In this framework the question of signaling vs. human capital comes down to learning about the values of α_2 and α_3 in (1). In order to identify the contribution of human capital to wages we need variation in f that holds θ constant. If school No.1 reduces the quantity of human capital produced ($\Delta f_1 < 0$), such that it is still higher than f_2 , this model would predict that since the effort required to attend school No.1 went down, the level of ability that determines for whom it is profitable to attend the best school would decrease, and thus $\bar{\theta}_1$ would decrease, and the fall in wages will confound the effects of the decline in the average ability of students and the decline in learning: $\Delta w_1 = \alpha_2 \Delta \bar{\theta}_1 + \alpha_3 \Delta f_1$. Note, however, that in an environment where school No.1:

- (i) Is constrained to admit a certain maximum number of students.
- (ii) Uses a proxy of ability to determine admissions.

We will have that if the maximum number of students is binding before the curriculum change then:

The admissions criteria guarantees (selecting students based on test scores) that the quality of the admitted class would not be affected with the reform, because the school was already choosing a subset (the ones with highest ability) of the group of people who find it profitable to attend school No.1.

And thus:

$$\Delta w_1 = \alpha_3 \Delta f_1 \quad (4)$$

In the next section of the paper I will go over the assumptions that lead to this result. Finally, to account for trends in wages I will use as controls, students from other schools and estimate the following difference in difference equation:

$$w_{itj} = a_0 + a_1 \mathbf{I}(\text{post}) + a_2 \mathbf{I}(\text{school}_1) + a_3 \mathbf{I}(\text{post} \cap \text{school}_1) + \varepsilon_{itj}$$

a_3 is the coefficient of interest and is my estimate of α_3 ; if it's zero, data support a pure signaling model, if it's negative and statistically significant, it suggests a role for human capital in the determination of wages.

III Reform

In 2006, Los Andes a private university, unilaterally decided to reduce the coursework required to earn a college degree in most of its majors.^{4,5} The reason behind this reform was to move towards international standards of shorter college degrees, and to encourage graduate school enrollment. Each department was autonomous in the implementation of the reform. In this paper I exploit the reform implemented by the economics and business departments, because in other majors the change led to a complete overhaul of the curricula, rather than a reduction of credits alone. For these two majors the requirement went from 4.5 years of course work to 4 years. In economics, the reform consisted of a reduction of 12 courses (20% of the total number of credits), which resulted in a curriculum of 4 years instead of 4.5 and a median number of courses per term of five instead of six. Specifically the reform consisted of: (i) turning six mandatory courses into electives (Monetary policy, Fiscal policy, Trade, Marxist economics, Colombian economic policy and Social programs evaluation); (ii) reducing the number of electives by four, for a reduction of ten courses; (iii) combining two courses of probability and statistics into one; (iv) combining courses of accounting and measurement in economics into one; for a final reduction of 12 courses. The reduction in business consisted of eliminating from the curriculum Computer Programming, Simulations and Microeconomics I. In addition, in the old curriculum students took six upper division electives and this requirement was reduced to three. The change in curricula covered new students, and students who by the time of the reform were in the beginning of their second year or less for economics, and in the beginning of their third year or less for business.

III a First stage: Empirical evidence of the reform for economics and business

In order to test the signaling and human capital models, I need an effective decline in the number of terms studied and credits taken; and for my identification strategy to be valid, I require no change in the quantity and quality of the pool of students graduating from Los Andes. This section features data on aggregate statistics from the annual bulletins from Los Andes, and micro data on credits taken by economics students to investigate these points.

⁴ Some institutional differences of the Colombian education system and labor market are in order. First, about the education system, admission are twice a year, students apply directly to a major and the gross enrollment rate in higher education is around 39%. On the labor market front, (i) recruitment of recent graduates are usually carried all throughout the year, only a few multinational companies have an organized recruitment season, (2) recruitment at this level usually consist of tests of specific knowledge, standard selection tests and interviews, and (3) 25% of college graduates work in the informal sector.

⁵ Los Andes was the only school undergoing this practice.

Was the reform effective?

Figure 1 shows the effective average duration of the undergraduate programs for both economics and business majors. We can see that there is actually a step down in these trends at the time of the reform of about one semester, suggesting the reform was effective in decreasing the average length of the program. For economics, the average duration went from 5 to 4.5 years, and for business the duration declined from 5.5 to 5 years. **Figure 2** shows the number of credits students graduated with in economics. We can observe a sharp drop at the time of the reform of around 16%.

Did the reform affect the size and composition of the entering and graduating class?

To evaluate if the reform affected the selection of students entering and graduating from Los Andes, I check the evolution of the size of the entering classes, their average High School exit exam, and average graduation rates. **Figure 3a** shows the evolution of the entering class in economics and business. I fit different trends before and after the reform. The graph shows that the number of entering students was not affected by the reform.⁶ Panel b of **Figure 3** shows the average High School exit test scores of the entering class. The fitted regressions around the reform do not suggest a change in the quality of the entering class. I also perform a difference in difference estimation, similar to the one I perform for my baseline analysis, to test if the reform decreased average High School exit score. **Table A1.1** shows that there is a small increase of approximately 0.2 to 0.3 standard deviations, which is not statistically significant. On the other hand, if the change in curriculum alters the quantity of students *graduating* from Los Andes, the value of the signal would change. This is plausible since the requirements to graduate decreased with the reform. Panel c of **Figure 3** shows the evolution of graduation rates, and suggests the reform didn't have an effect on the dropout rate. I also perform a difference in difference linear probability model regression where I test if the reform changed the probability of graduating from economics and business, and don't find evidence that it did (**Table-A1.1**). **Figure A1.1** in the appendix also shows that the reform did not change the share of students that graduated with a minor.

The above should imply that the ranking of Los Andes was not affected by the reform, however, to address this point directly I look at rankings and college exit scores. International rankings that include

⁶ Even though I don't find a discontinuity in test scores, there is a change in trends around the time of the reform, this can be problematic for my identification strategy if there is a different behavior in my control group. To check for this possibility in appendix 1, Figure A1.2 shows high school exit scores for the entering cohorts at Rosario University and find a similar pattern.

Latin American universities are only available since 2013, but from 2013 to 2016, Los Andes has been ranked as the best school in the country.⁷ In Colombia, the Ministry of Education released its first ranking on 2015 and Los Andes was also ranked first.⁸ Finally, **Figure A1.3** shows the average college exit exam for Los Andes and the next top 3 universities, according the data Los Andes has the highest score for most cohorts, both before and after the reform.

To summarize, the reduction in curricula was translated into an effective cut of one semester from the average degree duration in economics and business, which constitutes an exogenous reduction in human capital. On the other hand, the number of new students, high school exit test scores and dropout rates suggest that the quantity and quality of students was unaffected, thus the value of the signal remained unchanged with the reform. This constitutes an ideal environment to test the role of signaling and human capital in college education.

IV Effects of the Reform: human capital or signaling?

In this section I estimate the effect of the reduction of the curricula in business and economics on wages, to test the prevalence of a pure signaling model versus a model where human capital matters. I start by describing my data, continue with the identification strategy, and finally the results.

IV.a Data

My data consists of several databases from the Ministry of Education. My main database is OLE (*Observatorio Laboral de Educación*), constructed to follow yearly earnings for college graduates in Colombia in the formal sector.⁹ This information is recorded from social security payments from 2008 to 2012. OLE also contains education variables, such as university and program attended, graduation year, and personal characteristics.

SPADIES (*Sistema para la prevención de la deserción en la educación superior*) is a database built to track college dropout rates. Like OLE it, contains data on university attended, but additionally it has information on the first semester of college, which is needed in this paper to identify the curriculum for each student. This database also contains household socioeconomic variables. Finally, SABER 11 is a database that contains individual data on the national standardized High School exit test scores, and also

⁷ <https://www.timeshighereducation.com/world-university-rankings/2015/world-ranking#!/page/0/length/25>
<http://www.topuniversities.com/university-rankings/latin-american-university-rankings/2014#sorting=rank+region=+country=+faculty=+stars=false+search=>
Accessed February 10, 2016.

⁸ <http://www.mineducacion.gov.co/cvn/1665/w3-article-351855.html> Accessed February 10, 2016.

⁹ 75% of workers with college education are employed in the formal sector (Fedesarrollo, 2013)

has socioeconomic variables. SABER 11 is a test taken at the end of high school, and it is a very important input for college admissions. Specifically at Los Andes, it is the only factor taken into account in the admission process. These databases contain generated ID numbers to trace individuals. **Table 1** shows summary statistics of some relevant variables in my data. We can see that the average individual in my sample is 26 years old and has been working for almost three years¹⁰. On average, workers from Los Andes earn 45% more than workers from the top 10 schools, have higher High School exit test scores, and their parents have higher income.

IV.b Preliminary evidence and empirical strategy

Figure 4 shows a scatter plot of wages for graduates from Los Andes and Top 10 schools for economics and business by cohort. Before the reform the evolution in wages seems fairly parallel, and the slope in wages are statistically the same. There was a constant premium from attending Los Andes of 36% for economics, and 50% for business. With the change in curricula this premium decreased immediately for economics and gradually for business, for a final average reduction of 22 and 12pp, respectively. **Figure 5** displays the wage densities for Los Andes and top 10 schools, both before and after the reform. The graphs show that for the control group pre and post-reform wage densities overlap each other, whereas for Los Andes, post-reform densities shifted to the left. Both Figure 5 and 6 show that the reform had a stark negative effect on the wage distribution of graduates from Los Andes. To estimate the magnitude of the role of human capital in wages, I estimate the following difference in difference regression:

$$\ln wage_{it} = \beta_0 + \beta_1 Andes * Post_i + \beta_2 Andes_i + \beta_3 Post_i + \beta_4 experience_{i,t} + \varepsilon_{it}, \quad (1)$$

where $wage_{i,t}$ is the average monthly earnings of individual i in year t , in 2010's pesos. *Andes* is a dummy equal to 1 if the person i went to college at Los Andes, and 0 if he went to another university in the top 10 (my baseline control group). *Post* is a dummy that is 1 if the person started school after the date of the reform implementation, and 0 otherwise, thus $Andes * Post_i$ captures the diff-in-diff estimator of the reform. β_4 and β_5 capture the effect of experience on wages, where experience is measured as the difference between the current year in the data and the graduation year. I also control for gender, year and cohort effects in other specifications. I perform this estimation by major, and cluster the standard errors at the school level.

¹⁰ The fact that my data consists of wages from individuals in the beginning of their professional careers possess a challenge to my specification since the wage profiles are very steep in terms of experience.

IV.c Results

Table 2 shows my baseline results, panel a displays the estimates for economics, and panel b for business. In column 1, I estimate equation 1 and find a statistically significant decline in wages of 16% for economics and 12% business. Column 2 adds controls for experience squared and gender, and columns 3 through 6 add year and cohort controls to these specifications. Throughout all such specifications there is a negative and strong decline in wages as a result of the reform. These results reject a pure signaling model, in which wages shouldn't change; and given the magnitude of the decline, they evidence an important role for human capital in the determination of wages. The coefficients on experience and gender are similar to others found in the literature.

It is possible that the reform in curricula changed the pool of applicants and entrants in dimensions not captured by high school exit tests, which are relevant to the labor market. Specifically, given the decline in requirements to graduate, lower ability individuals should be induced into enrolling in these programs, decreasing the value of the signal, and thus wages. In order to address this, I estimate an alternative specification taking, as the treatment group only the students at Los Andes who were already enrolled by the time of the reform, but studied under the new curriculum. **Table 3** shows the results for this alternative treatment group. According to the data there is a strong and negative effect on wages of around 16% for economics and 10.5% for business, suggesting the pool of students wasn't affected by the reform.

Given that the years of wage observations by group (pre-reform vs. post-reform, and treated vs. untreated), in **Table 4** I include observations with at most three years of experience, to be sure that the treatment coefficient is not capturing differences in the slope of the experience profile. Results in **Table 4** suggest again strong declines in wages of the same magnitudes as the ones found before.

In order to make use of all the data available, and recognizing the potential of heterogeneous effects, I now turn to a changes-in-changes (CIC) estimation following Athey and Imbens (2006). I estimate CIC for the 10th through 90th percentiles after controlling for experience, gender and cohort effects. The results are displayed in **Figure 6**, there is little evidence of heterogeneity in the effect of the curriculum reduction on wages by percentiles and fields, suggesting that the assumptions of the traditional diff in diff estimator hold.

To quantify the relative importance of signaling and human capital, we can take what we learned in this paper one step forward. If we interpret the coefficient on the effect of the reform on wages as the casual estimate of human capital, we can compare this estimate to an OLS return to education that besides

this effect, would include the value of the signal. Using data on the Survey of Quality of Life for 2008-2012, I estimate an OLS return to a year of higher education of 16% (see appendix 2 for details). Interpreting the reduction in economics as a reduction of one year of schooling and that in business as of one semester, my results provide novel evidence; they suggest that human capital accounts for all of the return to college education.

V. Robustness Checks & Caveats

In this section I perform several robustness checks that address possible confounding factors in my estimation. Then I discuss some important caveats and limitations. In this section all standard errors will be clustered at the individual level.

One might be worried about the possibility that my estimates capture a negative trend in the return to a degree from Los Andes. To test if this is the case, I replicate my baseline estimation, but use a placebo date for the reform. Specifically, I take only the cohorts that studied under the old curriculum, and set a “fake” reform date in the middle of the period covered. If my results were driven by a decline in the return to Los Andes, any *post*Andes* interaction will be negative and statistically significant. This is not the case. According to the results in **Table 5**, all of the effects are statistically equal to zero and smaller than 0.7% in economics, and even positive for business.

An alternative placebo check to address this concern is to test what happens to law graduates (a major without a reform in its curriculum) in the dates of the reform in economics and business. The results in **Table 6** show there is no effect on wages from graduates from Los Andes on the date of the reform in economics or business. All of the above suggest the strong decline in wages I find is not the result of trends or changes at Los Andes.

Table 7 features a series of robustness checks, the first two column show results for economics and the last two for business, columns 1 and 3 estimate equation 1 with cohort controls, and columns 2 and 4 add experience squared and gender. A possible explanation for my results is that there is an age penalty in the labor market. We can imagine that if two graduates have the same credentials, employers can lean towards the older one, thinking that life experience is valuable for the job. In this case, having cohorts that graduate half a year younger would result in lower wages, regardless of human capital or signaling considerations. To check this possibility I include age as an independent variable in my baseline estimation. The results in *panel a* of **Table 7** suggest that there is a strong effect of the reform outside age considerations. For economics the effect is the same (-16%) and for business is smaller (-9%).

One might also be worried about the fact that the reform generated two cohorts graduating at the same time, and this could have distorted wages creating more competition. In panel b of **Table 7** I remove these two cohorts and perform my baseline estimation. The results show that the effects hold with this exclusion.

An additional concern about the previous estimates is the validity of the control group. Even though the pre trends in wages were similar, the control group may not be a good counterfactual, if for example the two groups face different labor markets, and these evolved in different ways after the reform. To address this, I limit my control group to students graduating from top 3 schools. It is more likely for students from these institutions to face the same labor market as the students from Los Andes. *Panel c* of the **Table 7** shows the results of the effect of the reform on wages under this alternative control group. We can see that there is a negative effect of the reform on wages of similar magnitude to the one found before.

An alternative way to go around this concern is to include in the control group only students who had the ability to attend Los Andes. Specifically, I include in the control group students who attended top 10 schools, and had high school exit scores greater than the minimum per cohort observed at Los Andes, in economics and business, respectively. This reduces the size of the control groups by around 30%. *Panel d* of **Table 7** shows the results of this alternative exercise: wages fall by a magnitude larger than in the baseline estimation (18% for economics and 15% for business).

Panel e of **Table 7** repeats the baseline estimation excluding cohort 2007-1, **Figure 4** shows it had particularly low wages for students from Los Andes. Again the results are very similar, suggesting strong declines in wages. Finally, *Panel f* included as a covariate high school exit test scores. We can see that, controlling for test scores, the results hold and increase a little in size.

It is evident that there are multiple choices of control group, and even though some are intuitive there is no clear rule to discriminate among them. To address this issue, I follow Abadie and Gardeazabal (2003), and perform a synthetic control exercise, where I look for the best combination of major/school to match the pre-trend data of my treated groups. The comparison unit in the synthetic control method is selected as the weighted average of all potential comparison units that best resembles the characteristics of the case of interest. **Table 8** shows the results of my baseline specification with respect to the optimally chosen control group. This group features graduates from engineering, business and law degrees in Top schools. Using this method, the results are similar to the ones found before: the effect of the reform in economics ranges from - 7% to -13%, for business there is a larger dispersion, and the effects ranges from -5% to -20%.

In the previous analysis I assumed that the reform didn't have an effect on labor force participation. Since one of the motives behind it was to increase graduate school enrollment, it's important

to check for changes along this dimension. It can be the case that before the reform, only students in the right tail of the ability distribution attended graduate school, but after the reform more students chose to attend graduate school, and thus the estimated difference in wages resulted from comparing wages from different segments of the ability distribution. To check if this is the case, I use LinkedIn and personal and firm websites to obtain information on graduate school enrollment for the last three cohorts who studied under the old curriculum, and the first three that studied under the new one. **Figure A1.4** shows the percentage of people found in LinkedIn. This number is around 60% and is similar before and after the reform. **Figure A1.5** also shows the share of graduates by cohort who enrolled in graduate school in the first four years after graduation. According to these data there doesn't seem to be an increase in this number with the reform. All of the above suggest that selection doesn't seem to be driving the decline in wages, and thus we can interpret this decline as due to the causal return on human capital.

VI Discussion

The previous section laid out evidence on the importance of human capital in the determination of wages. The next step is to think about the mechanisms that led recent graduates from Los Andes to earn lower wages. When and how do employers find out about the lower human capital of these graduates? Specifically, were they able to notice it in the recruitment process during tests or interviews? Or did they notice it on the job? Unfortunately I don't have information to fully answer these questions, but I have data from Los Andes on the current employers of all graduates from economics by cohort; which I use to investigate whether employers changed with the reform. **Table A1.2** lists the main employers before and after the reform and shows there are important differences. There seems to be a connection between the change in curriculum and the change in employers. The Central Bank, the Ministry of Finance and the National Planning Department are less common employers for economists who graduated under the new curriculum, in which the classes *Monetary Policy*, *Fiscal Policy* and *Colombian Economic Policy* were not mandatory, indeed **Figure A1.6** shows that there was decline in the number of students enrolled in these classes after the reform.

I also used this information to interview the most important employers to learn about their experience hiring graduates from economics. From these interviews I learned that: (i) most of them knew about the reform; (ii) they believe they can detect the change in human capital through tests they perform in their recruitment process; (iii) they argue that for some jobs the content made optional in the new curriculum is vital; (iv) taking fewer elective courses affects graduates' labor prospects beyond the recruitment process, because these professors are helpful with job offers and job referrals; and (v) wages

for recent graduates are fixed. All of the above suggest that under the new curriculum, the pool of jobs a graduate can obtain is smaller, either because they can't succeed in recruitment process that includes tests on content they didn't cover in school, or because they have less contact with professors who have connections in the job market. It is clear that the first reason is entirely due to a decrease in human capital, whereas this is not the case with the second one.

To evaluate if the reform had an impact on the ability of students to obtain a job, I perform a difference-in-difference exercise with data from the recruitment process for recently graduated economists at the Central Bank of Colombia. The process consists of a written exam or presentation, which tests specific knowledge for the position; as well as human resources tests and interviews with both human resources and department heads. Most processes are announced publicly through employment websites and social networks, and are open to everyone. I have data on university and enrollment term on all candidates to economist positions from 2008 to 2014, along with the final decision of the recruitment process. For candidates that studied under the old curriculum the probability of being hired was 27%, this number fell to 6% with the reform. **Table 9** shows the results of the diff-in-diff exercise: according to the data after the reform there is a reduction of 16.7pp on the probability of being hired in the Central Bank for students from Los Andes versus students in other top ten schools. This suggests that one of the possible mechanisms that led to the decline in wages is a decline in the performance of students in the recruitment process, which was in turn generated by the reduction in courses.

VII Conclusions

In this paper I identify the effect of human capital on wages exploiting a curriculum change at Universidad de Los Andes in Colombia. In 2006 the time required to earn a college degree in economics and, business decreased from 4.5 to 4 years. This was accomplished by dropping 12 courses in economics and 6 in business, which was equivalent to a reduction in credits of 20% and 14%, respectively. The reform did not alter the quality of the graduating class from Los Andes or the ranking of the school. Because wages should fall under the human capital model, but be constant under signaling, this constitutes an ideal natural experiment to learn about signaling vs. human capital.

Using administrative data on wages and college attendance, I find that wages fall by around 16% in economics and 10% in business. Given the statistically significant decline in wages, my estimates suggest that human capital plays an important role in the formation of wages. The results also reject a model in which signaling is the only function of college education. Even more, if we interpret the

coefficient on the effect of the reform on wages as the causal estimate of the effect of human capital on wages, we can compare it to an OLS return to education that includes the human capital effect as well as the value of the signal. Using data on the Survey of Quality, I estimate an OLS return to higher education of 17%. Interpreting the reform as a reduction of one year of schooling in economics, and of one semester in business, my results provide novel evidence, suggesting that human capital accounts for the largest share of the return to college education.

I use data and interviews from employers of economics graduates to study the mechanisms that led to the decline in wages. Employers argued that some of the content that was made optional in the new curriculum was vital to the positions they offered, and if that was the case, they would have noticed that students had less human capital in knowledge tests in the recruitment process. This suggests that under the new curriculum, the pool of jobs a graduate can obtain is smaller, because they perform worse on recruitment processes, which subsequently decreases their wages. Using data from the recruitment processes at the Central Bank, I find support for this hypothesis and estimate that the reform reduced the probability of being successful by 17pp.

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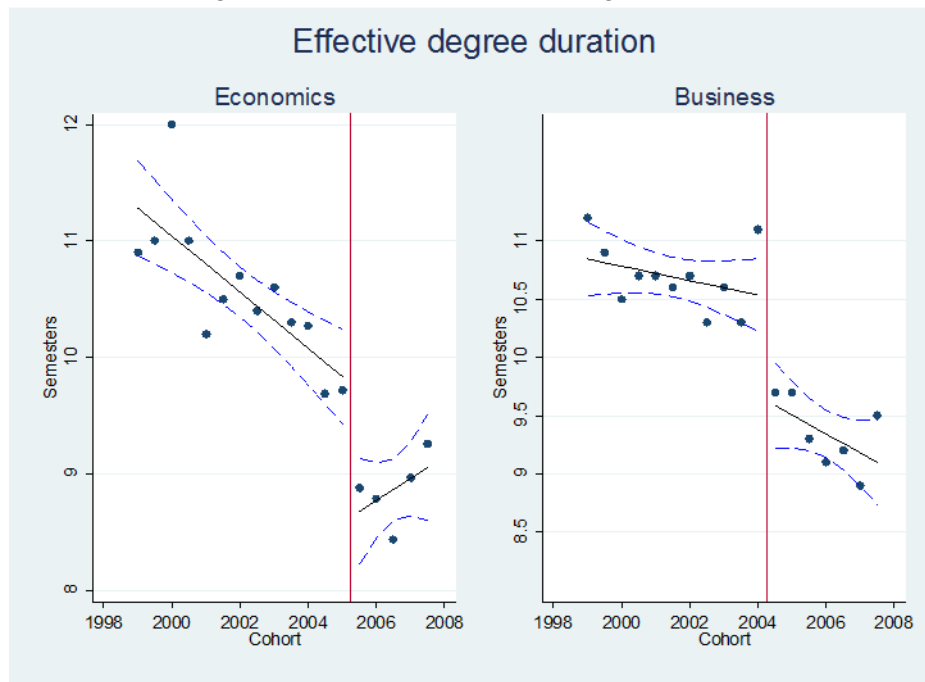
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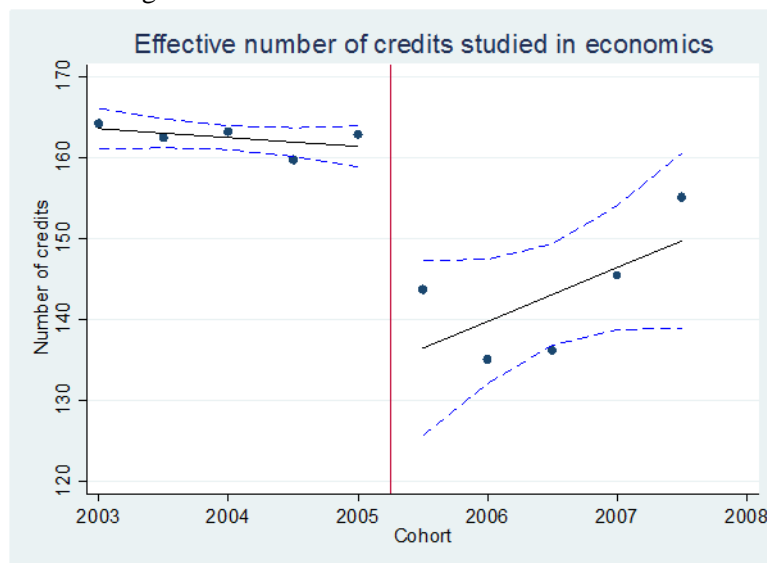
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Figure 1: Effect of the reform in degree duration



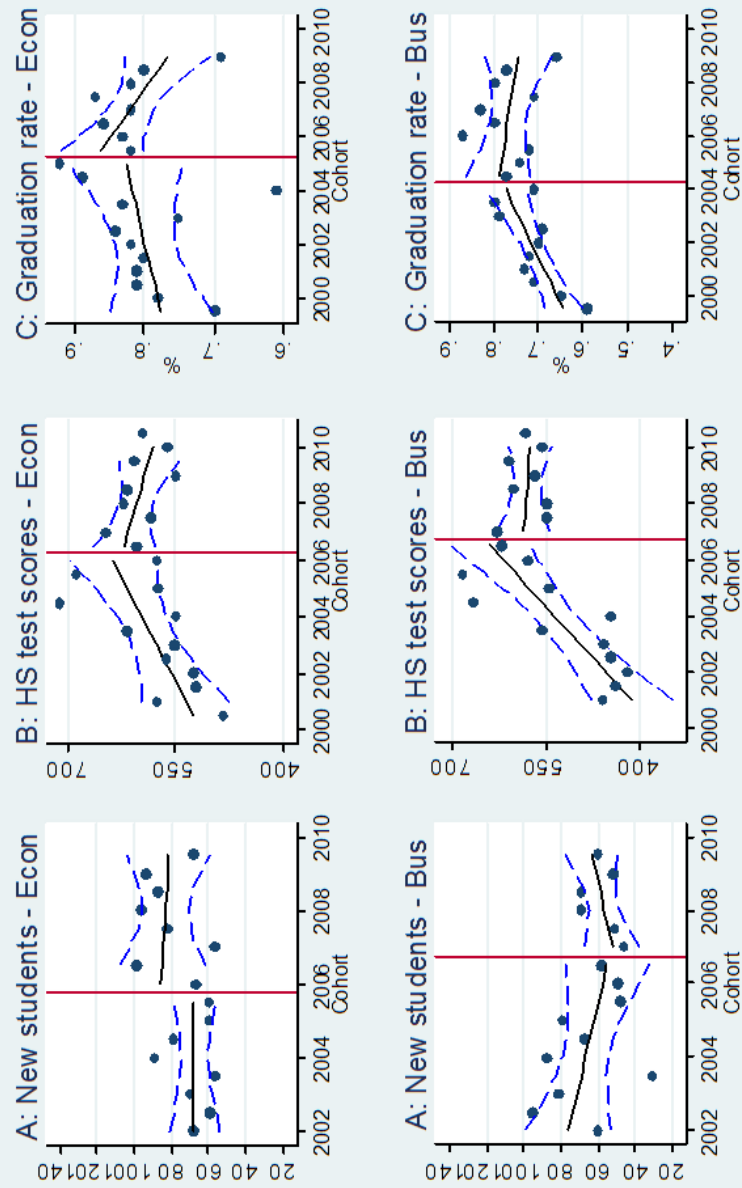
Source: Annual statistical bulletin – Universidad de los Andes. Scatter plots are mean degree duration per cohort. The solid lines are the fitted values of a regression on time and the dashed lines the 95% CI of the estimation. The vertical line represents the time of the reform.

Figure 2: Effect of the reform in credits studied



Source: Department of Economics – Universidad de los Andes. Scatter plots are credits studied by cohort. The solid lines are the fitted values of a regression on time and the dashed lines the 95% CI of the estimation. The vertical line represents the time of the reform.

Figure 3: Effects of the reform on class selection



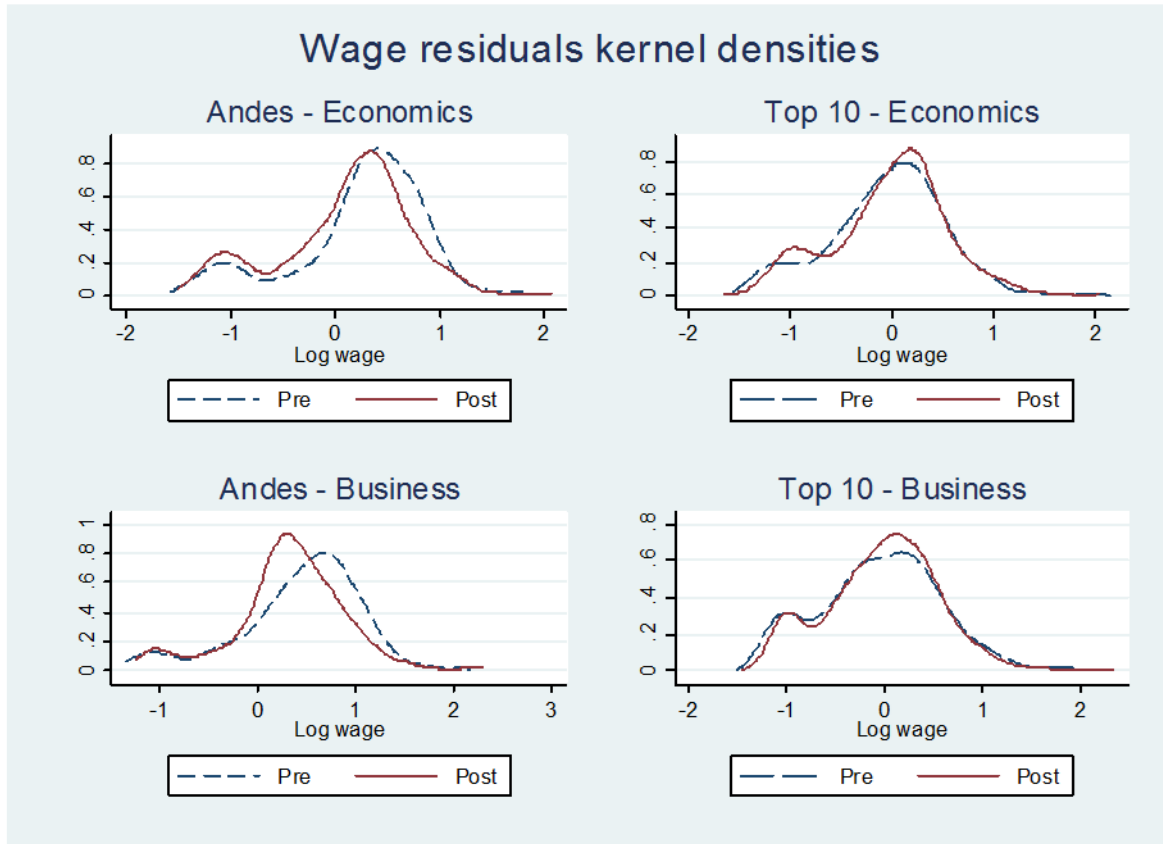
Source: Annual statistical bulletins - Universidad de los Andes. The solid lines are the fitted values and dashed lines the 95% CI.

Figure 4: Pre trends and the effect of the reform in wages



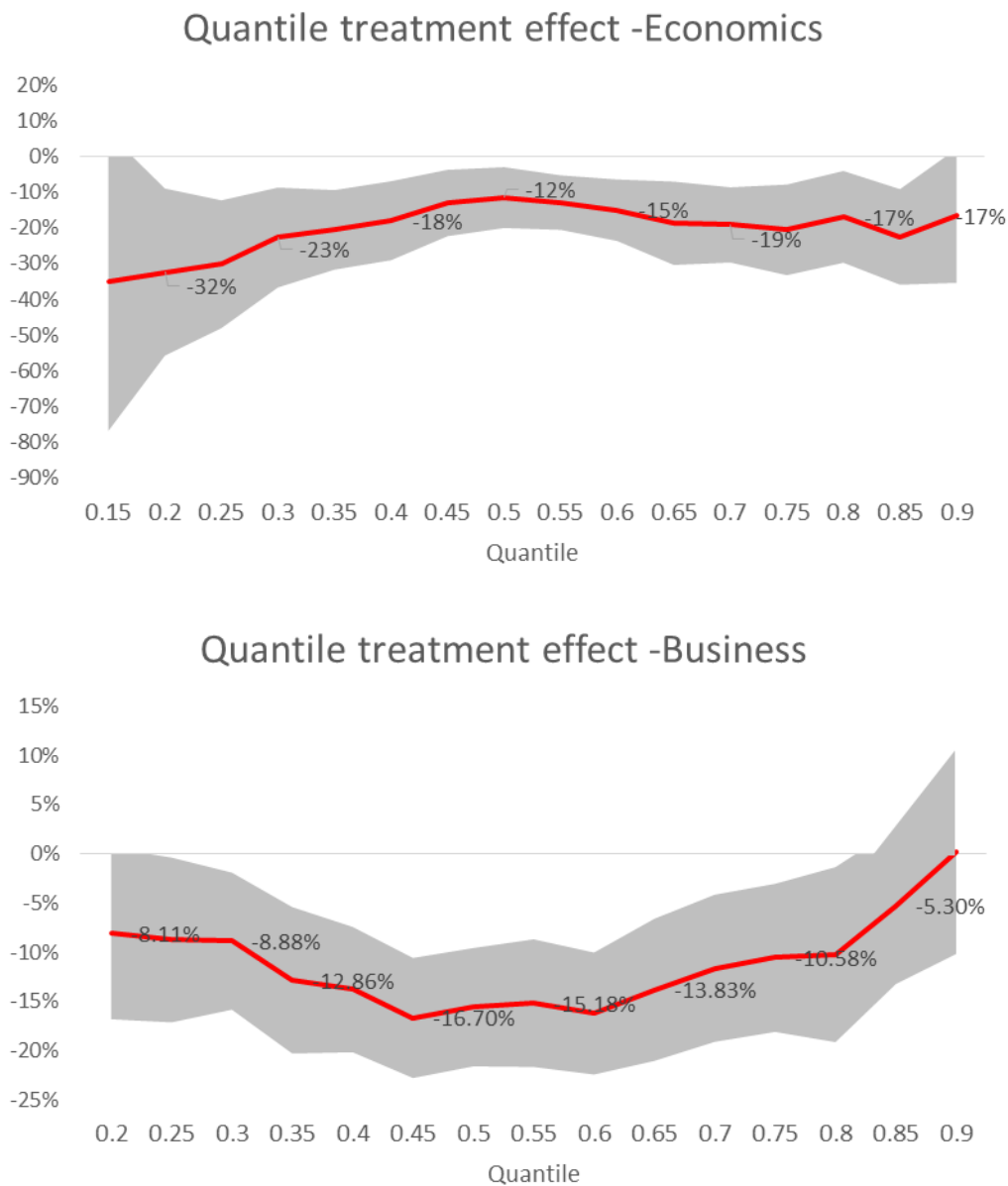
Source: Ministry of Education. Scatter plots are mean wages per cohort and school group. Lines are the fitted values of a regression quadratic on time. The vertical line represents the time of the reform.

Figure 5: The effect of the reform on the distribution of wages



Pre stands for the average across cohorts for all students that studied before the reform and post is the average of post reform cohorts. Kolmogorov-Smirnov test- Null hypothesis: both groups were sampled from populations with identical distributions. P-values: 0.000(Andes-Economics), 0.000(Andes-Business), 0.153(Top 10-Economics), 0.000(Top10-Business). Source: Ministry of Education. Residual of a regression that includes experience, experience square, gender and cohort control.

Figure 6: Changes in changes estimates



Source: Ministry of education. CIC estimates of an estimation that control for experience, gender and cohort variables. Confidence intervals at the 90th percent level. 10.000 bootstrap repetitions.
 Test - Economics: Constant effect: $QTE(\tau) = QTE(0.5)$ KS-statistic: 0.236. CMS-statistic: 0.227. Test - Business: Constant effect: $QTE(\tau) = QTE(0.5)$ KS-statistic: 0.101. CMS-statistic: 0.062.

List of Tables

Table 1: Summary statistics

	Real wage	Experience	Age	Female	HS test	Family income*	Obs
Andes Economics	3,017,001	2.6	25.8	0.46	58.1	5.93	1,736
	1,776,674	1.9	2.2	0.50	5.5	1.44	
Top 10	2,119,275	2.98	26.26	0.59	51.28	3.75	3,580
	1,457,070	1.98	2.83	0.49	6.01	1.76	
Andes Business	3,192,033	2.5	25.8	0.46	58.1	5.93	2,659
	1,959,143	1.8	2.2	0.50	5.5	1.44	
Top 10	2,141,599	2.90	26.24	0.59	51.33	3.82	22,505
	1,522,623	2.01	2.79	0.49	6.03	1.76	
Other majors at	2,482,154	2.66	25.8	0.55	57.6	5.87	6,069
Los Andes	1,695,091	1.99	2.2	0.50	5.4	1.53	

Note: Top rows show meand and the bottom rowd show standard deviation. * Based on a clasification over 9 categories of income. Data from cohorts that graduated after 2004. The top 10 universities were chosen using SABER PRO scores for schools of at least 1000 students. Source: Ministry of Education, Colombia.

Table 2a: Baseline results. Effect of the reform on wages.

Economics						
Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Post*Andes	-0.164*** [0.0359]	-0.161*** [0.0356]	-0.168*** [0.0385]	-0.164*** [0.0384]	-0.164*** [0.0362]	-0.161*** [0.0360]
Post	0.0824* [0.0326]	0.0818* [0.0325]	0.0721* [0.0306]	0.0735* [0.0300]	0,0819 [0.0423]	0,0863 [0.0414]
Andes	0.312*** [0.0450]	0.301*** [0.0451]	0.312*** [0.0443]	0.300*** [0.0445]	0.311*** [0.0452]	0.299*** [0.0454]
Experience	0.135*** [0.00822]	0.154*** [0.0251]	0.137*** [0.00760]	0.154*** [0.0249]	0.135*** [0.0158]	0.155*** [0.0278]
Experience sq		-0,0042 [0.00548]		-0,00389 [0.00579]		-0,00422 [0.00511]
Female		-0.0911** [0.0272]		-0.0907** [0.0274]		-0.0913** [0.0287]
Constant	14.16*** [0.0416]	14.20*** [0.0461]	14.13*** [0.0756]	14.17*** [0.0733]	14.21*** [0.0449]	14.19*** [0.0639]
Cohort control	N	N	Y	Y	N	N
Year D	N	N	N	N	Y	Y
Clusters	11	11	11	11	11	11
Obs	3.621	3.621	3.621	3.621	3.621	3.621
R-sq	0,157	0,165	0,157	0,165	0,159	0,167

Standard errors clustered at the school level.

Control group: students from economics at top 10 schools.

Cohort control: Semiannual GDP growth. Cohort refer to the semester and year the students started school. Year refers to the year of the wage observation.

Ln wage is the natural logarithm of the average monthly wage. Post is a dummy equal to one after the reform, Andes is a dummy equal to one if the student went to Los Andes. Experience is measured in years.

Standard erros in brackets below the coefficients.

*p<0.1, **p<0.05, ***p<0.01

Source: Ministry of Education OLE and SPADIES.

Table 2b: Baseline results. Effect of the reform on wages.

Business						
Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Post*Andes	-0.121*** [0.0229]	-0.121*** [0.0216]	-0.126*** [0.0237]	-0.126*** [0.0223]	-0.121*** [0.0228]	-0.121*** [0.0214]
Post	0.0846** [0.0198]	0.0840** [0.0190]	0.0480* [0.0195]	0.0484* [0.0193]	0.0904** [0.0269]	0.0928** [0.0260]
Andes	0.425*** [0.0758]	0.419*** [0.0719]	0.428*** [0.0757]	0.422*** [0.0718]	0.425*** [0.0746]	0.418*** [0.0707]
Experience	0.127*** [0.0102]	0.140** [0.0328]	0.131*** [0.00942]	0.142*** [0.0319]	0.130*** [0.00923]	0.147*** [0.0288]
Experience sq		-0,00299 [0.00667]		-0,00257 [0.00671]		-0,004 [0.00664]
Female		-0.0990** [0.0284]		-0.0984** [0.0282]		-0.0994** [0.0290]
Constant	14.06*** [0.0618]	14.11*** [0.0463]	13.96*** [0.0950]	14.01*** [0.0750]	14.18*** [0.106]	14.10*** [0.0610]
Cohort control	N	N	Y	Y	N	N
Year D	N	N	N	N	Y	Y
Clusters	12	12	12	12	12	12
N	10.970	10.970	10.970	10.970	10.970	10.970
R-sq	0,123	0,132	0,125	0,133	0,124	0,132

Standard errors clustered at the school level.

Control group: students from business at top 10 schools.

Cohort control: Semiannual GDP growth. Cohort refer to the semester and year the students started school. Year refers to the year of the wage observation.

Ln wage is the natural logarithm of the average monthly wage. Post is a dummy equal to one after the reform, Andes is a dummy equal to one if the student went to Los Andes. Experience is measured in years.

Standard errors in brackets below the coefficients.

*p<0.1, **p<0.05, ***p<0.01

Source: Ministry of Education OLE and SPADIES.

Table 3: Effect of the reform on wages. Alternative treatment group: students already in school by the time of the reform.

Panel A: Economics						
Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Post*Andes	-0.165*** [0.0371]	-0.162*** [0.0369]	-0.169*** [0.0395]	-0.165*** [0.0393]	-0.164*** [0.0373]	-0.162*** [0.0369]
Post	0.0774* [0.0344]	0,0762 [0.0346]	0,0699 [0.0332]	0,071 [0.0330]	0,0756 [0.0457]	0,0808 [0.0448]
Andes	0.313*** [0.0450]	0.300*** [0.0452]	0.312*** [0.0443]	0.300*** [0.0444]	0.312*** [0.0452]	0.299*** [0.0454]
Panel B: Business						
Post*Andes	-0.104*** [0.0209]	-0.104*** [0.0198]	-0.110*** [0.0215]	-0.109*** [0.0203]	-0.104*** [0.0210]	-0.104*** [0.0199]
Post	0.0802** [0.0191]	0.0798** [0.0183]	0.0438* [0.0189]	0.0441* [0.0188]	0.0838** [0.0266]	0.0866** [0.0257]
Andes	0.426*** [0.0758]	0.420*** [0.0719]	0.429*** [0.0755]	0.423*** [0.0717]	0.426*** [0.0746]	0.420*** [0.0707]

Standard errors clustered at the school level.

(1) experience. (2) experience, experience squared and gender. (3) experience and cohort controls. (4) experience, experience squared, gender and cohort controls. (5) experience and year dummies. (6) experience, experience squared, gender and year dummies.

Cohort control: Semiannual GDP growth. Cohort refer to the semester and year the students started school. Year refers to the year of the wage observation.

Ln wage is the natural logarithm of the average monthly wage. Post is a dummy equal to one if a person studied with the new curriculum but was enrolled before the change, Andes is a dummy equal to one if the student went to Los Andes. Experience is measured in years.

Standard errors in brackets below the coefficients.

*p<0.1, **p<0.05, ***p<0.01

Source: Ministry of Education.

Table 4: Cap at three years of experience

Panel A: Economics						
Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Post*Andes	-0.167*** [0.0368]	-0.164*** [0.0378]	-0.170*** [0.0393]	-0.168*** [0.0403]	-0.166*** [0.0371]	-0.164*** [0.0382]
Post	0.0849* [0.0339]	0.0837* [0.0348]	0.0748* [0.0316]	0.0748* [0.0317]	0.0831 [0.0423]	0.0859 [0.0426]
Andes	0.314*** [0.0458]	0.305*** [0.0460]	0.313*** [0.0448]	0.304*** [0.0451]	0.313*** [0.0460]	0.304*** [0.0463]
Panel B: Business						
Post*Andes	-0.118*** [0.0210]	-0.117*** [0.0196]	-0.122*** [0.0219]	-0.121*** [0.0204]	-0.118*** [0.0207]	-0.118*** [0.0192]
Post	0.0837** [0.0194]	0.0844*** [0.0184]	0.0515* [0.0215]	0.0534* [0.0213]	0.0916** [0.0247]	0.0962** [0.0234]
Andes	0.421*** [0.0759]	0.415*** [0.0717]	0.424*** [0.0762]	0.418*** [0.0719]	0.421*** [0.0746]	0.414*** [0.0703]

Standard errors clustered at the school level.

(1) experience. (2) experience, experience squared and gender. (3) experience and cohort controls. (4) experience, experience squared, gender and cohort controls. (5) experience and year dummies. (6) experience, experience squared, gender and year dummies.

Cohort control: Semiannual GDP growth. Cohort refer to the semester and year the students started school. Year refers to the year of the wage observation.

Ln wage is the natural logarithm of the average monthly wage. Post is a dummy equal to one if a person studied with the new curriculum but was enrolled before the change, Andes is a dummy equal to one if the student went to Los Andes. Experience is measured in years.

Standard errors in brackets below the coefficients.

*p<0.1, **p<0.05, ***p<0.01

Source: Ministry of Education.

Table 5: Placebo test 1- Alternative date of the reform

Panel A: Economics						
Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Fake post*Andes	-0.004 [0.0481]	-0.005 [0.0482]	-0.007 [0.0488]	-0.007 [0.0490]	-0.002 [0.0482]	-0.003 [0.0486]
Fake post	0.012 [0.0458]	0.002 [0.0455]	-0.017 [0.0605]	-0.025 [0.0592]	0.018 [0.0498]	0.015 [0.0497]
Andes	0.313*** [0.0357]	0.300*** [0.0366]	0.315*** [0.0366]	0.301*** [0.0375]	0.309*** [0.0365]	0.294*** [0.0375]
Panel B: Business						
Fake post*Andes	0.016 [0.0838]	0.009 [0.0785]	0.017 [0.0915]	0.009 [0.0847]	0.014 [0.0812]	0.006 [0.0758]
Fake post	0.061 [0.0772]	0.061 [0.0747]	-0.057 [0.184]	-0.054 [0.177]	0.080 [0.0821]	0.082 [0.0782]
Andes	0.420*** [0.0640]	0.417*** [0.0593]	0.423*** [0.0681]	0.420*** [0.0618]	0.420*** [0.0612]	0.416*** [0.0567]

Standard errors clustered at the school/cohort level.

(1) experience. (2) experience, experience squared and gender. (3) experience and cohort controls. (4) experience, experience squared, gender and cohort controls. (5) experience and year dummies. (6) experience, experience squared, gender and year dummies.

I take only the students that studied under the old curriculum and set the reform date on the middle of the period (2004-1 for econ and 2003-2 for business).

Standard errors in brackets below the coefficients.

*p<0.1, **p<0.05, ***p<0.01

Source: Ministry of Education.

Table 6: Placebo test 2 – Reform evaluated using data from law graduates

Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Date of economics reform						
Post*Andes	-0.00952 [0.0525]	-0.00913 [0.0524]	-0.00696 [0.0535]	-0.00657 [0.0536]	-0.00282 [0.0572]	-0.00261 [0.0573]
Date of business reform						
Post*Andes	-0.0238 [0.0341]	-0.023 [0.0342]	-0.0224 [0.0347]	-0.0216 [0.0348]	-0.0103 [0.0379]	-0.00964 [0.0380]
Obs	3,388	3,388	3,388	3,388	3,388	3,388
R-sq	0.12	0.12	0.12	0.12	0.13	0.13
St errors clustered at the school/cohort level.						

(1) experience. (2) experience, experience squared and gender. (3) experience and cohort controls. (4) experience, experience squared, gender and cohort controls. (5) experience and year dummies. (6) experience, experience squared, gender and year dummies.

Cohort control: Semiannual GDP growth. Cohort refer to the semester and year the students started school. Year refers to the year of the wage observation.

Ln wage is the natural logarithm of the average monthly wage. Post is a dummy equal to one if a person studied with the new curriculum but was enrolled before the change, Andes is a dummy equal to one if the student went to Los Andes. Experience is measured in years.

Standard errors in brackets below the coefficients.

*p<0.1, **p<0.05, ***p<0.01

Source: Ministry of Education.

Table 7: Robustness checks

Dep variable: Ln wage	Economics (1)	Economics (2)	Business (3)	Business (4)
<i>Panel a: Controlling for age</i>				
Treatment	-0.162*** [0.0510]	-0.158*** [0.0510]	-0.0952** [0.0410]	-0.0950** [0.0412]
<i>Panel b: Without cohorts that graduated at the same time</i>				
Treatment	-0.159*** [0.0552]	-0.154*** [0.0552]	-0.118*** [0.0437]	-0.118*** [0.0439]
<i>Panel c: Taking graduates from Top 3 schools as control (1)</i>				
Treatment	-0.115** [0.0557]	-0.115** [0.0557]	-0.145*** [0.0472]	-0.145*** [0.0472]
<i>Panel d: Including in the control group only students that could have attended Los Andes</i>				
Treatment	-0.186*** [0.0434]	-0.184*** [0.0441]	-0.152** [0.0640]	-0.151** [0.0626]
<i>Panel e: Without 2007-1 cohort</i>				
Treatment	-0.152*** [0.0510]	-0.146*** [0.0511]	-0.117*** [0.0418]	-0.118*** [0.0420]
<i>Panel f: Controlling for HS exit scores</i>				
Treatment	-0.185*** [0.0469]	-0.180*** [0.0472]	-0.161* [0.0624]	-0.160** [0.0611]
Experience	Y	Y	Y	Y
Experience squared	N	Y	N	Y
Gender	N	Y	N	Y
Cohort effects	Y	Y	Y	Y

Standard errors clustered by individual.

(1) Top 3 schools are Nacional, Javeriana and Rosario.

Standard errors in brackets below the coefficients.

*p<0.1, **p<0.05, ***p<0.01

Source: Ministry of Education.

Table 8: Synthetic control

Dep variable: Ln wage	(1)	(2)
Panel a: Economics		
<i>Control: Industrial Engineering - Javeriana (70.8%)</i>		
Treatment	-0.133** [0.0632]	-0.134** [0.0635]
<i>Control: Industrial Engineering-Nacional (16.3%)</i>		
Treatment	-0.0719 [0.0695]	-0.07 [0.0695]
<i>Control: Oil Engineering-Nacional (7%)</i>		
Treatment	-0.11 [0.0786]	-0.111 [0.0791]
<i>Control: Industrial Engineering- U Norte (6%)</i>		
Treatment	-0.134** [0.0615]	-0.133** [0.0614]
Panel b: Business		
<i>Control: Oil Engineering-Nacional (46%)</i>		
Treatment	-0.197*** [0.0539]	-0.201*** [0.0539]
<i>Control: Business - EAFIT (38.3%)</i>		
Treatment	-0.101* [0.0578]	-0.101* [0.0578]
<i>Control: Industrial Engineering - Javeriana (14%)</i>		
Treatment	-0.0971* [0.0551]	-0.0961* [0.0549]
<i>Control: Law - Andes (1%)</i>		
Treatment	-0.0508 [0.0579]	-0.0506 [0.0577]

Standard errors clustered by individual.

Standard errors in brackets below the coefficients.

The number in parenthesis is the optimal weight.

Column 1 includes experience and cohort controls, column 2 adds experience square and gender.

* p<0.1, ** p<0.05, *** p<0.01

Source: Ministry of Education.

Table 9: Effect of the reform in the recruitment process

<u>Dependent variable: 1 if hired and 0 if not</u>	
Andes*Post	-0.167** 0.073
Post	-0.049 0.031
Andes	0.163*** 0.058
Constant	0.112*** 0.023
Obs	438
R squared	0.03

Standard errors below the coefficients

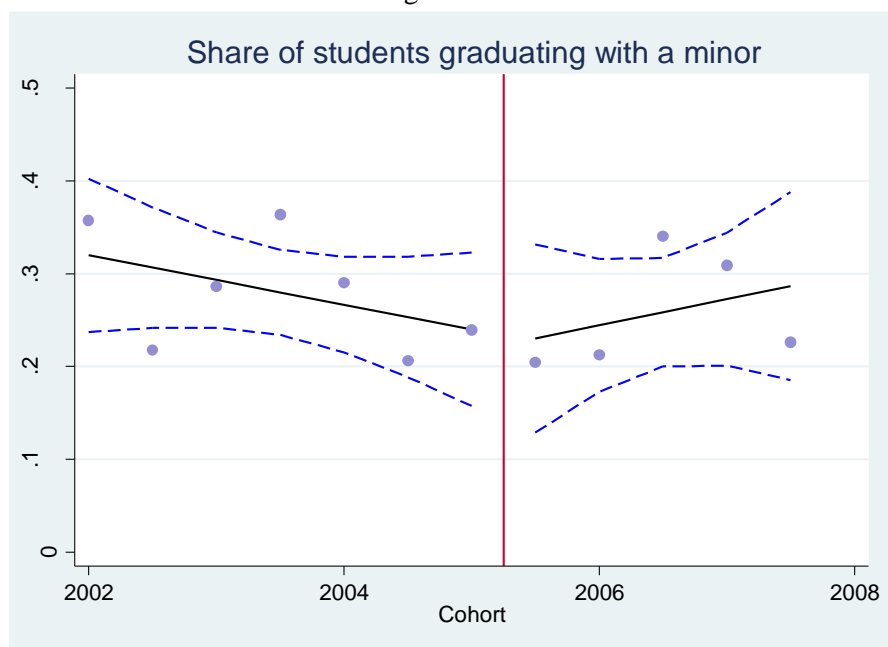
Data from the recruitment process for
economist position from 2008 to 2014

Source: Central Bank of Colombia.

* p<0.1, ** p<0.05, *** p<0.01

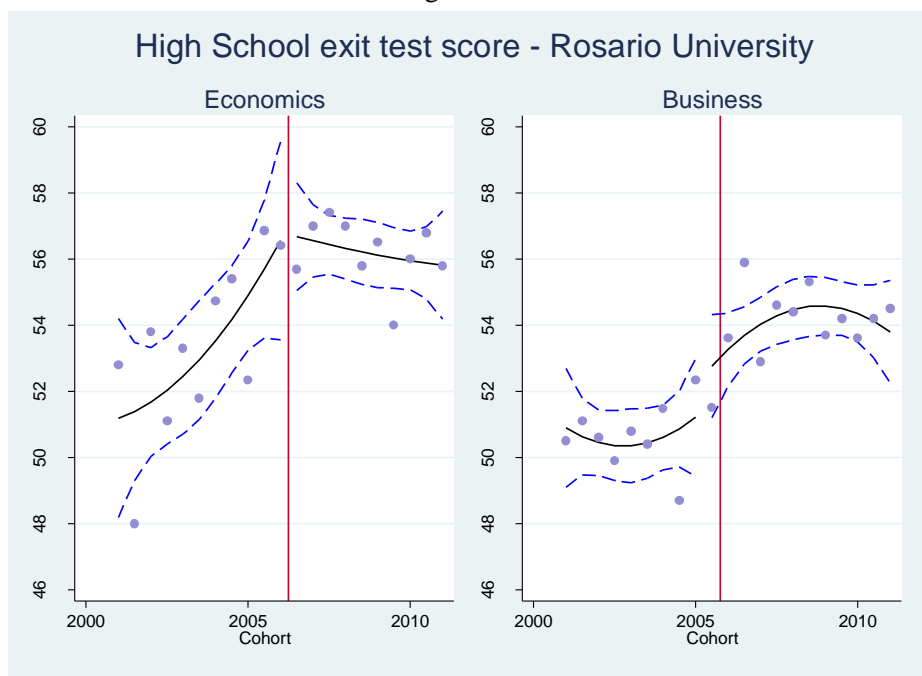
Appendix 1: Extra Figures and tables.

Figure A1.1



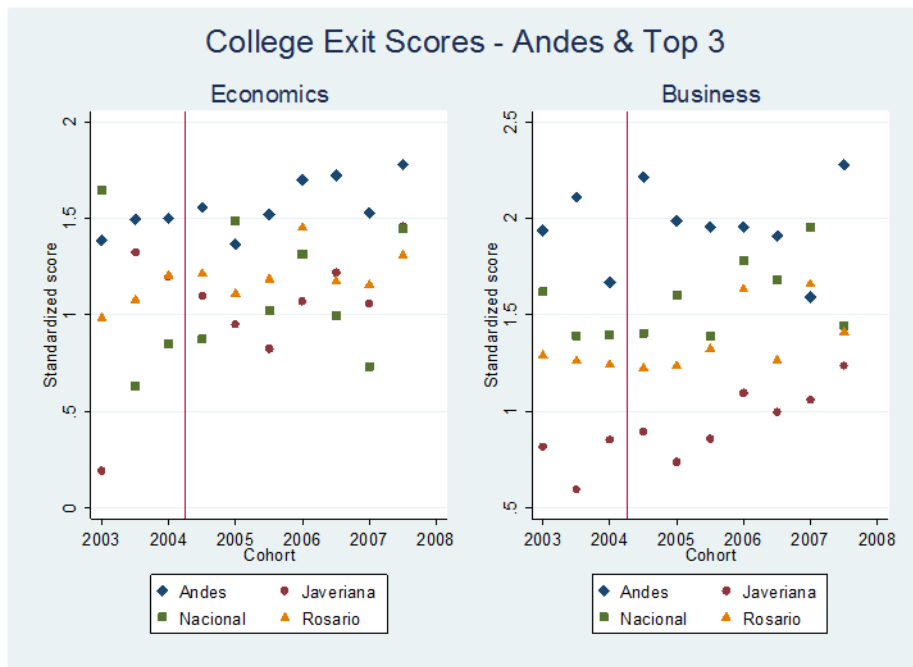
Source: Admissions Department – Universidad de los Andes

Figure A1.2



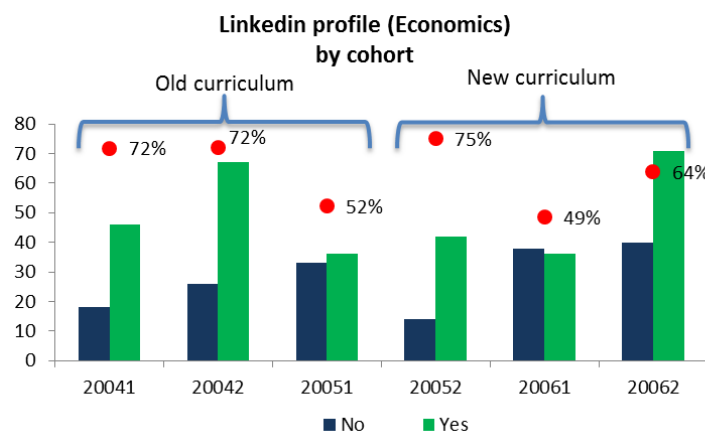
Source: Boletín Estadístico – Universidad del Rosario.

Figure A1.3: Effects of the reform on the ranking



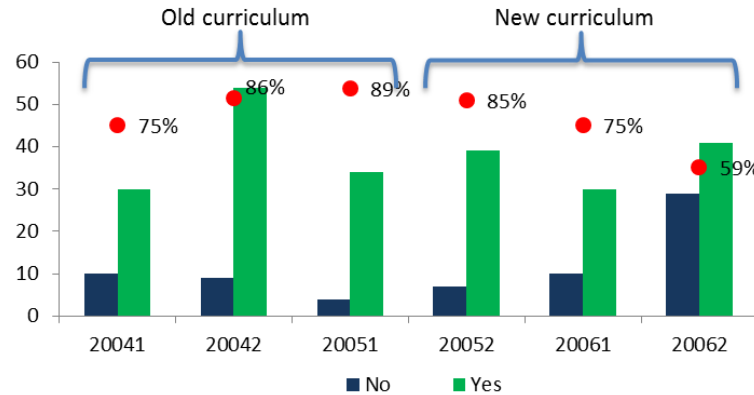
Source: Ministry of Education.

Figure A1.4: LinkedIn profile



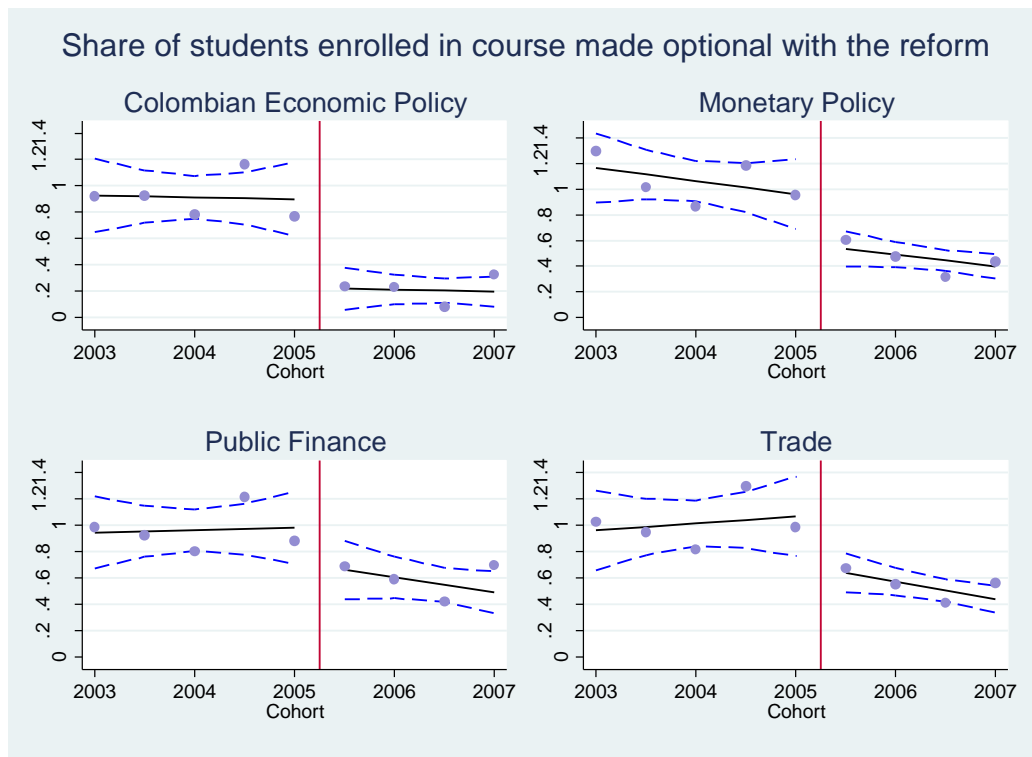
Source: Ministry of Education, Universidad de los Andes and LinkedIn.

Figure A1.4: Graduate degree
Graduate degree (Economics)
by cohort



Source: Ministry of Education, Universidad de Los Andes and LinkedIn.

Figure A1.6: Share of students enrolled in courses made optional with the reform



Source: Universidad de Los Andes

Table A1.1: Pre estimation tests

	Economics	Business		Economics	Business
Panel a:			Panel b:		
Dep variable - High school exit test score			Dep variable - Graduation rates		
Andes*Post	1.163 [0.620]	1.818*** [0.446]	Andes*Post	0.0283 [0.0545]	-0.0016 [0.0505]
Post	1.632*** [0.389]	1.181*** [0.189]	Post	-0.0690* [0.0315]	-0.0499 [0.0305]
Andes	5.104*** [0.390]	5.799*** [0.299]	Andes	0.0412 [0.0364]	0.0357 [0.0378]
Obs	3436	9844	Obs	1782	2274
Test mean	56.1	52.4			
Test standard dev	5.7	5.2			
Andes*Post in sd	0.2	0.3			

*p<0.1, **p<0.05, ***p<0.01

Note: Panel a and b regressions include time controls, panel b regression also includes an individual risk variable.

Source: Ministry of Education.

Table A1.2: Top Econ Employers

Top Employers cohorts 2003-1 to 2007-2		Top Employers			
		Pre		Post	
Universidad de los Andes	120	Universidad de los Andes	32	Universidad de los Andes	35
National Planning Dept	25	National Planning Dept	17	BANCO DE BOGOTA (Priv Bank)	9
Central Bank	23	Central Bank	15	National Planning Dept	6
BANCO DE BOGOTA (Priv Bank)	21	Ministry of Finance	9	Central Bank	5
FEDESARROLLO (research center)	18	IADB	8	Ministry of Finance	5
Ministry of Finance	15	BANCOLOMBIA (Priv Bank)	8	IADB	5
DAVIVIENDA (Priv Bank)	14	FEDESARROLLO (research center)	8	FEDESARROLLO (research center)	5
IADB	13	Self employed	6	DAVIVIENDA (Priv Bank)	5
CITI	12	DAVIVIENDA (Priv Bank)	5	LAN AIRLINES	5
BANCOLOMBIA (Priv Bank)	11	BANCO DE BOGOTA (Priv Bank)	5	CITI (Priv Bank)	4
ECOPETROL	8	BANCO DE CREDITO (Priv Bank)	5	World Bank	4
AVIANCA	7	CITI (Priv Bank)	5	CORFICOLOMBIANA	4
ANIF	7	ECOPETROL	5	OPORTUNIDAD ESTRATEGICA	4
Ministry of Defense	7				

This accounts for 20% of the students

Source: Department of Economics - Universidad de los Andes.

Appendix 2: Estimating the OLS return to a year of higher education

I use the Survey of Quality of Life for the years 2008, 2010, 2011 and 2012 to estimate the return to a year of higher education for a group as similar as possible to the one in my estimation. I estimate mincer type equations where I control for experience, experience squared, gender and my main dependent variable is the number of years of higher education. To capture a group similar to the one in my estimation I take samples that proxy workers in the formal sector from privileged backgrounds. **Table A2** shows the results from this estimation, according to the data an OLS return to a year of higher education ranges between 15% and 17%.

Table A2: Return to higher education

	(1)	(2)	(3)
Years of higher education	0.169*** 0.0061	0.153*** 0.006684	0.164*** 0.011762
Experience	0.029*** 0.0049	0.022*** 0.00553	0.019*** 0.009457
Experience squared	-0.0003*** 0.0001	-0.0002*** 0.000113	-0.0002*** 0.000202
Gender	0.15*** 0.0240	0.125*** 0.026364	0.065*** 0.045918
Constant	12.5*** 0.0577	12.75*** 0.065413	12.62*** 0.103879
Obs	10,522	8,157	3,650
R Sq	0.0987	0.0859	0.0717
Group	A	B	C
Survey D	Y	Y	Y

A: Workers with a labor contract

B: Workers with a labor contract and professional risk insurance

C: Workers with a labor contract and fathers education was higher or equal than High School

Standard errors below the coefficients

* p<0.1, ** p<0.05, *** p<0.01

Source: Quality of life survey. Colombia