The Effect of Human Capital on Earnings: Evidence from a Reform at Colombia's Top University

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In this paper I test whether the returns to college education are the result of human capital accumulation. I exploit a reform at Universidad de los Andes, which in 2006 reduced the amount of coursework required to earn degrees in economics and business. The time to complete a degree decreased from 4.5 to 4 years, and 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 size of the entering class, their average high school exit exam scores, and graduation rates were not affected by the reform, indicating that the quantity and quality of students remained the same. Therefore, the reform decreased the human capital that 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. Surveying employers, I find that the decline in wages may have resulted from a decline in performance during the recruitment process, which led students to be placed in lower quality firms. 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 from being hired by 17 pp.

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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% 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). However, there is less consensus about the mechanisms through which education leads to higher wages. Papers that estimate causal returns to schooling cannot shed light on the sources of such return (Card, 2001). Two main channels have been proposed in the literature. First, the human capital theory argues that education increases productivity, and wages rise as a result (Becker, 1964 and Mincer, 1974). Second, the signaling theory posits that higher wages reflect the correlation between education and unobserved ability[2]. In both settings, higher-ability workers obtain higher levels of schooling and are paid more, which explains the difficulty in setting the two theories apart.

In this paper, I identify the effect of human capital accumulation on wages by exploiting a curriculum change at Universidad de 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] The identification strategy of this paper relies on the fact that 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 scores on the national standardized high-school exit exam (*SABER 11*). I show that the size of the entering class did not grow, nor did the average entrance test scores decrease, and dropout rates did not change with the reduction in the number of classes. Therefore, the reform had no short-run effect on the quality of the entering class after 2006, but it decreased human capital accumulation holding the signaling value of the degree constant. The human

2 Of course, the two theories are not mutually exclusive.
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 remained at five.

capital model predicts a decline in wages as a result of the reform, whereas the signaling model does not.

To estimate the effect of the reform, I use individual information on wages and educational attainment in a difference-in-differences (DID) framework. I compare wages in the formal sector before and after the reform for economics and business graduates of Los Andes and other top 10 schools in Colombia that did not reform their degrees. I find that after the reform, wages for students from Los Andes significantly decreased by 16% in economics and 12% in business. This suggests that human capital accumulation plays an important role in the determination of wages, and therefore I reject a model in which signaling is the only role of college education. Allowing for heterogeneous effects of the reform (using Athey and Imbens' (2006) changes-in-changes estimator), I find a homogenous impact along the wage distribution; this indicates that wages declined proportionally for high- and low-earners.

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, and that likelihood of being employed by the highest paying firms decreased. 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 administered during the recruitment process, and argued that some of the knowledge made optional in the new curriculum was vital for the jobs. Using data from the recruitment process for economists at the Central Bank from 2008 to 2014, I find that for graduates from Los Andes, the probability of being hired fell by 17 percentage points after the reform. This suggests that the reduction in courses introduced by the reform, decreased students' performance in recruitment processes, which in turn placed them in lower-quality firms and ultimately decreased their wages. Given that initial firm placement plays a significant role in determining long-term labor market success (Oreopoulus, von Wacther and Heisz, 2012), my results could also hint to possible longer-term effects.

Finally, I examine possible threats to my identification strategy. First, it could be possible that the reform in curriculum changed the pool of applicants and entrants in dimensions that are not captured by the SABER 11, but are relevant to the labor market. Specifically, given the

decline in requirements for graduation, lower-ability individuals 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 concern, I estimate an alternative specification, taking as the treatment group students at Los Andes who were already enrolled at the time of the reform but studied under the new curriculum. Results for this alternative treatment group are similar to the baseline specification. Second, my estimates might capture a negative trend in the return to a degree from Los Andes. To test whether this is the case, I perform two exercises: I replicate my baseline estimation using a placebo date for the reform, and I test my specification using a major at Los Andes that did not undergo a curriculum reform. However, I do not find evidence of wages change in either case. 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, in turn, delayed working—my result could be confounding a change in the composition of new graduates entering the labor market. I use LinkedIn data to check whether the reform increased the share of students attending graduate school, but find no evidence of such an increase.

This paper contributes to the existing literature by disentangling the role of human capital accumulation from that of signaling in determining wages. The college-level setting 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 for signaling ability. In addition, there is also great debate about the role of public spending in financing college education. To the best of my knowledge, this is also the first paper in this strand of the literature that investigates the mechanisms that led to changes in wages, which is important because it provides information about the tools employers use to learn about workers' expected productivity. I can only estimate the effect of the reform in the short-run. However, it is in the short-run where the signaling and human capital debate is particularly relevant, given that with time, employers learn about students' productivity on the job (Farber and Gibbons (1996) and Altonji and Pierret (2001)).

A number of papers have investigated this issue for primary and secondary education and

obtained mixed results. Eble and Hu (2016) exploit the introduction of one extra year in primary school in China in 1980. They find a 2% increase in wages; since this accounts for a small fraction of the overall return to schooling, they conclude that there is an important role for signaling in primary education. No extra coursework, however, was introduced in that additional year. Lang and Kropp (1986) and Bedard (2001) find secondary schooling decisions that are consistent with a signaling model, which would reject a pure human capital framework. Another strand of the literature attempts to directly measure whether there is a signaling value to academic 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.

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, empirical strategy, and results; Section 5 presents robustness checks; Section 6 explores the channels that explain the results; and Section 7 offers some concluding remarks.

II Theoretical framework

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 curriculum reduction at a top university and 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 with the level of human capital and decreases with 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 that *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.

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

Students choose the school j that maximizes wages net of effort costs:

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

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

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

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.
iii) The maximum number of students is binding before the curriculum change.

Then:

> By selecting students based on test scores the admissions criteria guarantee that the quality of the admitted class will not be affected by the reform, because the school

was already choosing a subset (i.e., those with highest ability) of the group of applicants who find it profitable to attend school No. 1.

And thus:

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

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

$$w_{itj} = a_0 + a_1 \mathbf{I}(\text{post}) + a_2 \mathbf{I}(school_1) + a_3 \mathbf{I}(post \cap school_1) + \varepsilon_{itj},$$

where 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, this suggests a role for human capital in the determination of wages.

III Institutional background and Reform

In this section, I discuss the salient characteristics of Colombian education and labor market institutions. On the education front, college admissions occur twice a year. Students apply directly to a major, and the gross enrollment rate in higher education is around 39%. On the labor market, recent graduates are typically recruited year-round, and only a few multinational companies have a formal recruitment season. Recruitment at this level usually consists of tests of specific knowledge, standard selection tests, and interviews. 25% of college graduates work in the informal sector. Los Andes is a private university in Colombia and is ranked first in the country.

III a Reform

In 2006, Los Andes, a private university, unilaterally decided to reduce the coursework required to earn a degree in most of its majors. [4] The reasons for the reform were to move towards international standards of shorter college degrees and encourage graduate school enrollment.

[4] Los Andes was the only school to implement this practice at the time.

Each department was autonomous in implementing the reform. In this paper I exploit the reforms implemented by the economics and business departments, which consisted solely of a reduction in required credits; in other departments, the change led to the complete overhaul of curricula. For those two majors, coursework was reduced from 4.5 years to 4 years. In economics, the curriculum was trimmed by 12 courses (20% of the total number of credits), which resulted in a median number of courses per term of five instead of six. Specifically, the reform: (i) converted six mandatory courses into electives (Monetary Policy, Public Finance, Trade, Marxist Economics, Colombian Economic Policy, and Social Programs Evaluation); (ii) reduced the number of electives by four; (iii) combined two probability and statistics courses into one; and (iv) combined accounting and economic measurement courses into one. The business department eliminated Computer Programming, Simulations, and Microeconomics I. In addition, the requirement of six upper-division electives was reduced to three.

The reform affected new students and students who, at the time it was implemented, were beginning their second year or earlier for economics, and in their third year or earlier for business. Other enrolled students were not affected by the change.

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

To disentangle the effects of the signaling and human capital models, I need an effective decline in the number of terms studied and credits earned, with no changes in the quantity and quality of the pool of students graduating from Los Andes. To investigate these points, in this section I present data on aggregate statistics from Los Andes' annual bulletins and micro data on credits earned by economics students.

Was the reform effective?

Figure 1 shows the effective average duration of undergraduate programs for both economics and business majors. There is a step down in these trends of about one semester at the time of the reform, which suggests that 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 classes?

To evaluate whether the reform affected the selection of students entering and/or graduating from Los Andes, I check the evolution of the size of the entering classes, their average SABER 11 scores, and average graduation rates. *Panel a* of **Figure 3** 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.[5] *Panel b* of **Figure 3** shows the average SABER 11 scores of the entering class. Fitted regressions around the reform do not suggest a change in the quality of the entering class. I also perform a DID estimation, similar to the one I perform for my baseline analysis, to determine whether the reform reduced the average SABER 11 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 that the reform had no effect on the dropout rate. I also perform a DID linear probability model regression to identify whether the reform changed the probability of graduating with an economics or business degree, and do not find evidence that it did (**Table A1.1**). **Figure A1.1** also shows that the reform did not change the share of students that graduated with a minor.

In the model, students use school rankings to choose where to attend college; if the reform decreased Los Andes' ranking, post-reform cohorts would have on average lower ability. Even though the above doesn't provide evidence of this, I examine this point directly, by looking at rankings and college exit scores. International rankings that include Latin American universities

[5] Though I do not find a discontinuity in test scores, there is a change in trends around the time of the reform. This could be problematic for my identification strategy if my control group behaves differently. To check for this possibility, Figure A1.2 shows SABER 11 scores for entering cohorts at Rosario University and reveals a similar pattern.

are only available since 2013, but from 2013 to 2016, Los Andes has been ranked as the best school in Colombia.[6] The Colombian Ministry of Education released its first rankings in 2015, and also ranked Los Andes first.[7] Finally, **Figure A1.3** shows the average college exit exam scores for Los Andes and the next three highest-ranked universities; Los Andes has the highest scores for most cohorts, both before and after the reform.

To summarize, the reduced curriculum translated into an effective cut of one semester from the average degree duration for economics and business, and a reduction in the number of credits per term, this constitutes an exogenous reduction in human capital. On the other hand, the number of new students, SABER 11 scores, and dropout rates suggest that the quantity and quality of students was unaffected, and therefore the value of the signal remained unchanged after the reform. This is an ideal environment to test the role of signaling and human capital in college education.

IV Effects of the Reform

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 in which human capital matters. I start by describing my data, continue with the identification strategy, and end with the results.

IV.a Data

Data are collected from three Ministry of Education databases. My main database is OLE (*Observatorio Laboral de Educación*), which is constructed to follow yearly earnings in the formal sector for college graduates in Colombia.[8] This information is recorded from Social

[6] 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.
[7] http://www.mineducacion.gov.co/cvn/1665/w3-article-351855.html Accessed February 10, 2016.
[8] 75% of workers with a college education are employed in the formal sector (Fedesarrollo, 2013)

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 that tracks college dropout rates. Like OLE it, contains data on university attended, but also has information on the first semester of college, which I needed in order to identify each student's curriculum. This database also contains household socioeconomic variables. The third database contains individual data on SABER 11 scores, and also has socioeconomic variables.

The three databases contain generated ID numbers to trace individuals. **Table 1** shows summary statistics of some relevant variables in the data. We can see that the average individual in my sample is 26 years old and has been working for almost three years[9]. On average, Los Andes graduates earn 45% more than graduates of the next 10 schools in the national rankings ("Top 10" hereafter) and have higher SABER 11 scores; their parents also have higher incomes.

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 slopes for wages are statistically the same. There was a constant premium for attending Los Andes of 36% for economics and 50% for business. With the curriculum change, this premium immediately declined for economics and gradually for business, for a final average reduction of 22 pp and 12 pp, respectively. **Figure 5** displays the wage densities for Los Andes and the 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, but for Los Andes, post-reform densities shift to the left. Both Figures 5 and 6 show that the reform had a starkly negative effect on the wage distribution of Los Andes graduates. To estimate the magnitude of human capital's role in wages, I estimate the following DID regression:

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

[9] The fact that my data consist of wages from individuals at the beginning of their professional careers poses a challenge to my specification, since wage profiles are very steep in terms of experience.

where $wage_{it}$ is the average monthly earnings of student *i* in year *t*, (in 2010 pesos). $Andes$ is a dummy equal to 1 if student i went to college at Los Andes, and 0 if he went to another Top 10 Colombian university (my baseline control group). $Post$ is a dummy equal to 1 if student started school after the date of the reform implementation, and 0 otherwise, and experience is measured I years since graduation. The coefficient β_1 captures the effect of graduating from Los Andes after 2006 on labor market wages. I also control for gender, year, and cohort effects in other specifications. I perform this estimation by major, and cluster standard errors at the school level.

IV.c Results

Table 2 shows my baseline results: *Panel a* presents estimates for economics, and *panel b* for business. The baseline estimation for equation (1), reported in column 1, indicate a statistically significant decline in wages by 16% for economics and 12% for business.[10] 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 should not change; given the magnitude of the decline, they demonstrate an important role for human capital in the determination of wages. Coefficients on experience and gender are similar to others found in the literature. I can only estimate the short-run effect of the reduction in human capital on wages, however, if employers learn about workers productivity on the job (Farber and Gibbons, 1996 and Altonji and Pierret, 2001), the signaling value of education would become less important with time, and thus it is the first years in the labor market when this questions is particularly relevant.

It is possible that the reform changed the pool of applicants and entrants in dimensions not captured by the SABER 11 that are relevant to the labor market. Specifically, given the decline in requirements to graduate, lower-ability individuals should be motivated to enroll in these programs, thereby decreasing the value of the signal and, in turn, wages. To address this, I

[10] To account for the small number of clusters, it may be necessary to bootstrap clustered standard errors (Cameron and Miller, 2015). My estimations, however, show that the bootstrap rather leads to even slightly lower standard errors if different at all. I therefore report the usual standard errors without bootstrapping, as it is the more conservative estimation method in our case.

estimate an alternative specification in which the treatment group consists of only Los Andes students who were already enrolled at the time of the reform, but studied under the new curriculum. **Table 3** shows 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.

Given that the number of years of wage observations by group is unbalanced (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** again suggest strong wage declines of the same magnitudes as the ones found before.

To make use of all the data available, and recognizing the potential for heterogeneous effects, I now turn to a changes-in-changes (CIC) estimation following Athey and Imbens (2006). I estimate CIC for the 10^{th} through 90^{th} percentiles after controlling for experience, gender, and cohort effects. As can be seen in **Figure 6**, there is little evidence of heterogeneity in the reform's effect on wages by percentiles and fields, suggesting that the assumptions of the traditional DID estimator hold.

V Mechanism

When and how do employers find out about the lower human capital of these graduates? Specifically, were they able to detect it in the recruitment process during tests or interviews? Or did they notice it on the job? Unfortunately, I do not have the information necessary to fully answer these questions, but I do have data from Los Andes on the employers of all economics graduates 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 that 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 likely to employ economists who graduated under the new curriculum, under which the classes Monetary Policy, Public Finance, and Colombian Economic Policy were no longer mandatory. Indeed, Figure A1.6 shows that there was a decline in the number of students

enrolled in these classes after the reform. From this comparison I also find that the likelihood of being employed by the highest paying firms decreased with the reform. Using a ranking of the 100 highest paying firms for recent graduates in economics, I find that the share of students in these firms fell from 24% to 14% after the reform.

I interviewed the most important employers to learn about their experience hiring economics graduates, and learned that: (i) most of them knew about the reform from talking to recent graduates; (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) they believe that taking fewer elective courses affects graduates' labor prospects beyond the recruitment process, because the professors in those courses are helpful with job offers and job referrals; and (v) wages for new graduates are fixed. All of the above offers suggestive evidence that under the new curriculum, the pool of jobs a graduate can obtain is smaller, either because they cannot succeed in the recruitment process—which 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, but this is not the case with the second one.

To evaluate whether the reform had an impact on students' ability to obtain jobs, I perform a DID exercise with data from the recruitment process for recently graduated economists at the Central Bank of Colombia. This consists of a written exam or presentation, which tests specific knowledge necessary for the position, as well as human resources tests and interviews with both human resources staff and department heads. Most such openings are publicly announced through employment websites and social networks, and are open to any and all applicants. I have data on university and enrollment term for all candidates for economist positions from 2008 to 2014, along with the final employment decision. For candidates who studied under the old curriculum, the probability of being hired was 27%; this fell to 6% with the reform. **Table 5** shows the results of the DID exercise. According to data from after the reform, there is a reduction of 16.7 pp in the probability of being hired by the Central Bank for students from Los Andes versus students from Top 10 schools. This suggests that one of the possible mechanisms that led to the decline in wages is a decline in the performance of students during

the recruitment process, as a consequence, the pool of offers a student could choose from was smaller and students started in lower paying jobs. Even though, I can only test the effect of the reform on wages and recruitment in the short-run, research on the long term effects of graduating during recessions, points out to the importance of initial placement on future labor market success.

VI Robustness Checks

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

It is possible that my estimates capture a negative trend in the return to a degree from Los Andes. To determine whether this is the case, I replicate my baseline estimation, using 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 would be negative and statistically significant. However, as shown in **Table 6,** all of the estimated effects are statistically equal to zero and smaller than 0.7% in economics, and positive for business.

An alternative placebo check to address this concern is to test what happens to law graduates (a major whose curriculum was not reformed) during the dates of the reform in economics and business. Results in **Table 7** show that there is no effect on wages for Los Andes law graduates on the date of the reform in economics or business. All of the above suggest that the strong decline in wages I find is not the result of trends or changes at Los Andes.

Table 8 presents a series of additional robustness checks. The first two columns 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 these results is that there is an age penalty in the labor market. We can imagine that if two graduates have the same credentials, employers might lean toward 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 8** suggest that there is a strong effect of the reform outside of age considerations. For economics, the effect is the same (-16%), and for business it is smaller (-9%).

One might also be worried about the fact that the reform generated two cohorts that graduated at the same time, which might have distorted wages by creating more competition. In *panel b* of **Table 8**, I exclude these two cohorts and perform my baseline estimation; results show that the effects hold, even with the 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 might 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 the next three highest-ranked schools, because it is likely that students from these institutions will face the same labor market as students from Los Andes. *Panel c* of **Table 8** presents the results of the reform's effect 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 address this concern is to include, in the control group, only students who had the academic credentials required to attend Los Andes. Specifically, I include students who attended Top 10 schools and had SABER 11 scores greater than the minimum per cohort observed at Los Andes in economics and business. This reduces the size of the control groups by around 30%. *Panel d* of **Table 8** 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 8** repeats the baseline estimation, excluding cohort 2007-1; as shown in **Figure 4**, this cohort had particularly low wages for students from Los Andes. Again, the results are very similar, suggesting strong declines in wages. Finally, *Panel f* includes SABER 11 scores as a covariate. We can see that when controlling for test scores, the results hold and even increase slightly.

Since there are multiple possible choices for control groups, I follow Abadie and Gardeazabal (2003) and perform a synthetic control exercise in which I look for the best combination of major and 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 9** shows the results of my baseline specification with respect to the optimally chosen control group. This group features engineering, business, and law graduates of Top 10 schools. Using this method, results are similar to the ones found previously: The reform's effect for economics graduates ranges from -7% to -13%, and for business graduates there is a larger dispersion, with effect ranging from -5% to -20%.

In the previous analysis I assumed that the reform did not have an effect on labor force participation. Since one of the motives for the reform was to increase graduate school enrollment, it is important to check for changes along this dimension. It is possible, for instance, that before the reform only students in the right tail of the ability distribution attended graduate school, but after the reform more students enrolled, and therefore the estimated difference in wages results from comparing wages from different segments of the ability distribution. To determine whether this is the case, I use LinkedIn and personal and firm websites to obtain information on graduate school enrollment for the last three cohorts that studied under the old curriculum and the first three that studied under the new one. **Figure A1.4** shows that the percentage of graduates found on LinkedIn—around 60%—is similar to the rates 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 obtaining an undergraduate degree, and the shares do not seem to increase with the reform. All of the above suggests that selection does not appear to be driving the decline in wages, and thus we can interpret this decline as being due to the causal return on human capital.

VII Conclusions

In this paper I identify the effect of human capital on wages by 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 or the school's ranking. Because wages should fall under the human capital model—but remain

constant under signaling—this constitutes an ideal natural experiment for learning about signaling vs. human capital.

Using administrative data on wages and college attendance from 2008 to 2012, I find that wages fell by 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 determination of wages. The results also reject a model in which signaling is the only function of college education. Note that this result does not rule out completely a role for signaling. For example, using also data on Colombia, Macleod et al. (2015) find evidence of a signaling role in college reputation.

I use data and interviews from employers of economics graduates to study the mechanisms that led to the decline in wages. I find that the distribution of employers changed with the reform, and that likelihood of being employed by the highest paying firms decreased. Employers argue that some of the content that was made optional in the new curricula was essential to the positions they offered; if that was the case, employers would have noticed that students had less human capital through 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 during the recruitment process, which subsequently decreases their wages. Using recruitment data from the Central Bank, I find support for this hypothesis and estimate that the reform reduced the probability of being successful by 17 percentage points.

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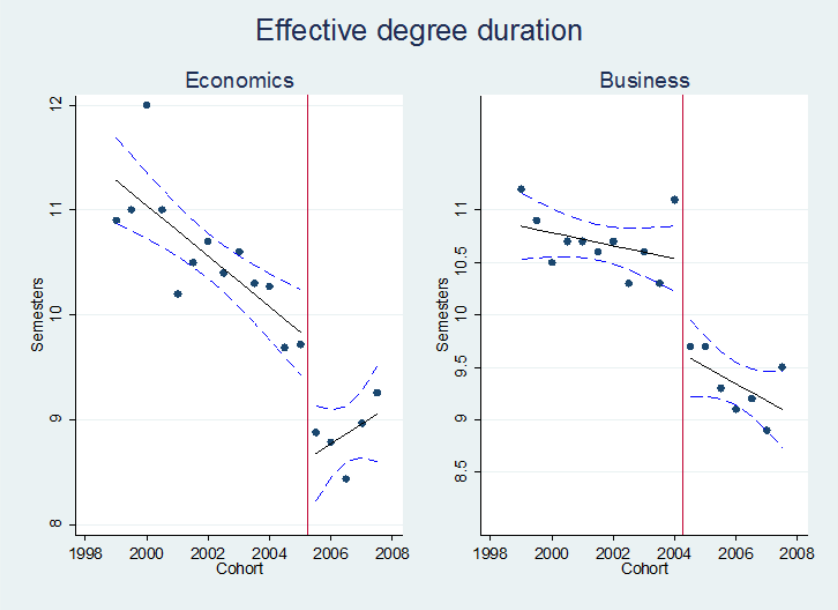
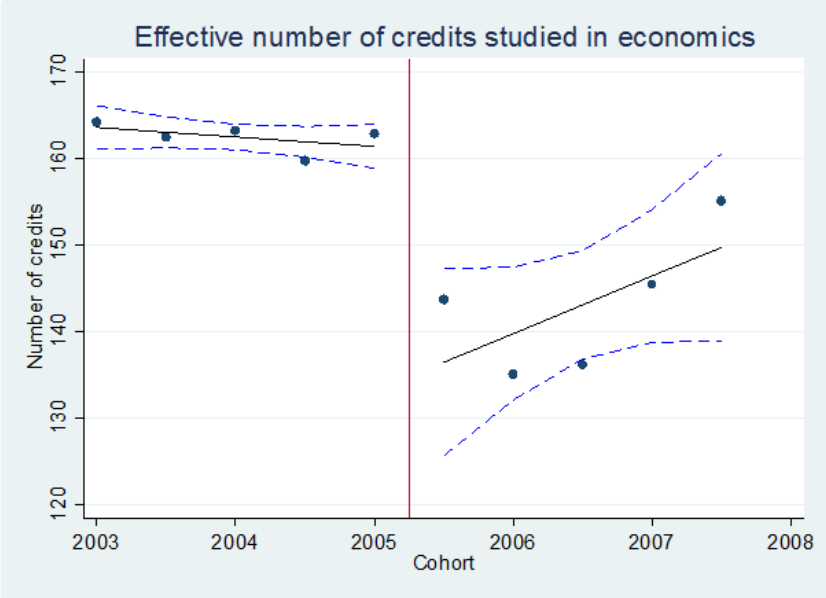
List of Figures

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. Solid lines are the fitted values of a regression on time, and dashed lines represent 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. Solid lines are the fitted values of a regression on time and dashed lines are the 95% CI of the estimation. The vertical line represents the time of the reform.

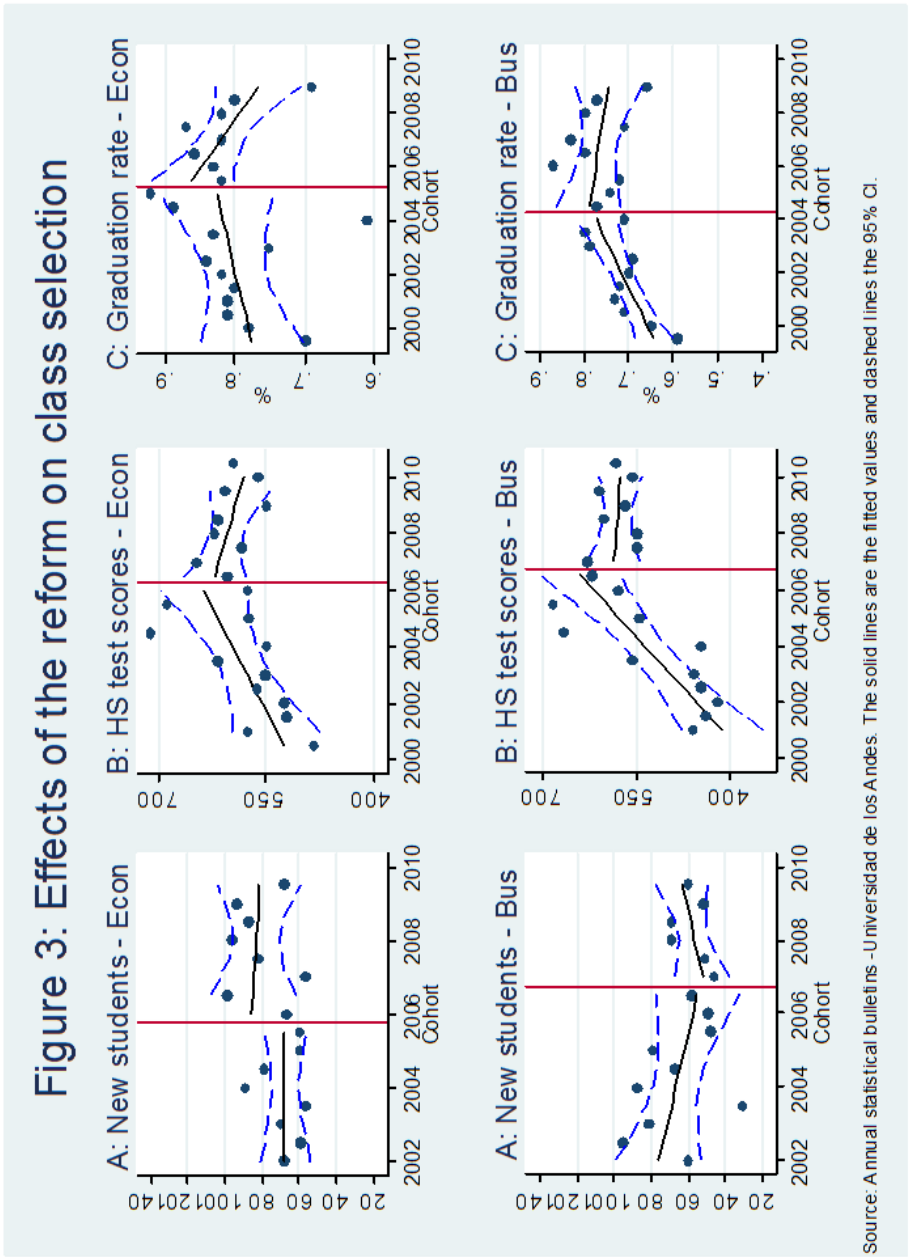
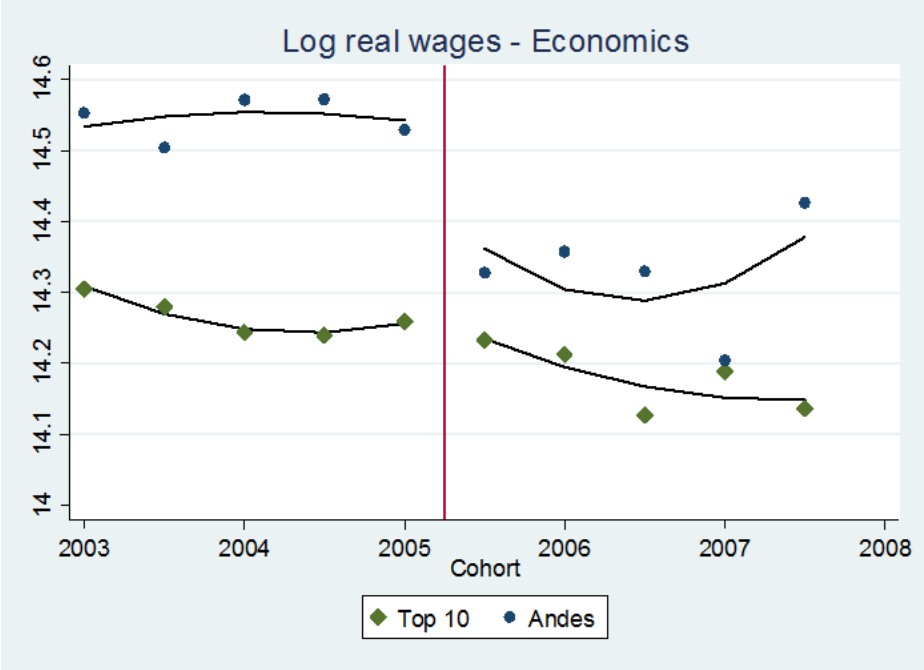


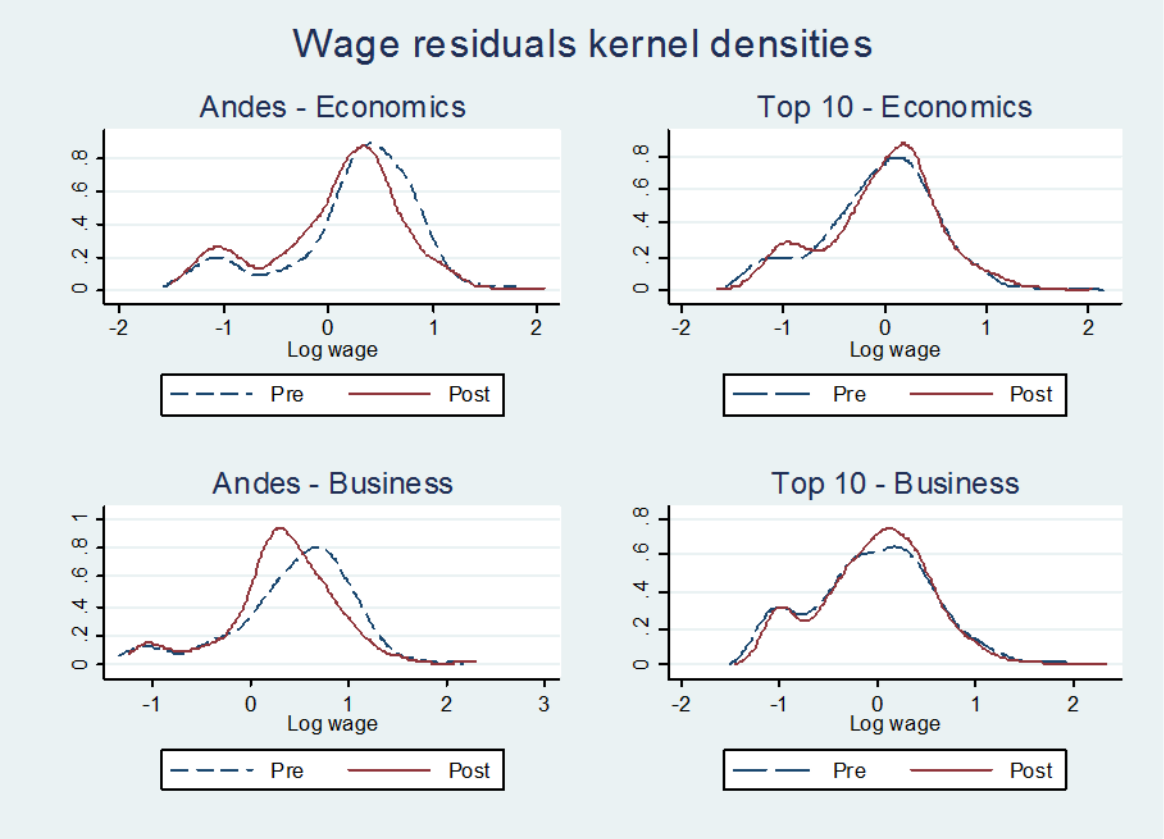
Figure 3: Effects of the reform on class selection
A: New students - Econ
B: HS test scores - Econ
C: Graduation rate - Econ
A: New students - Bus
B: HS test scores - Bus
C: Graduation rate - Bus
Cohort
%
Source: Annual statistical bulletins - Universidad de los Andes. The solid lines are the fitted values and dashed lines the 95% CI.

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



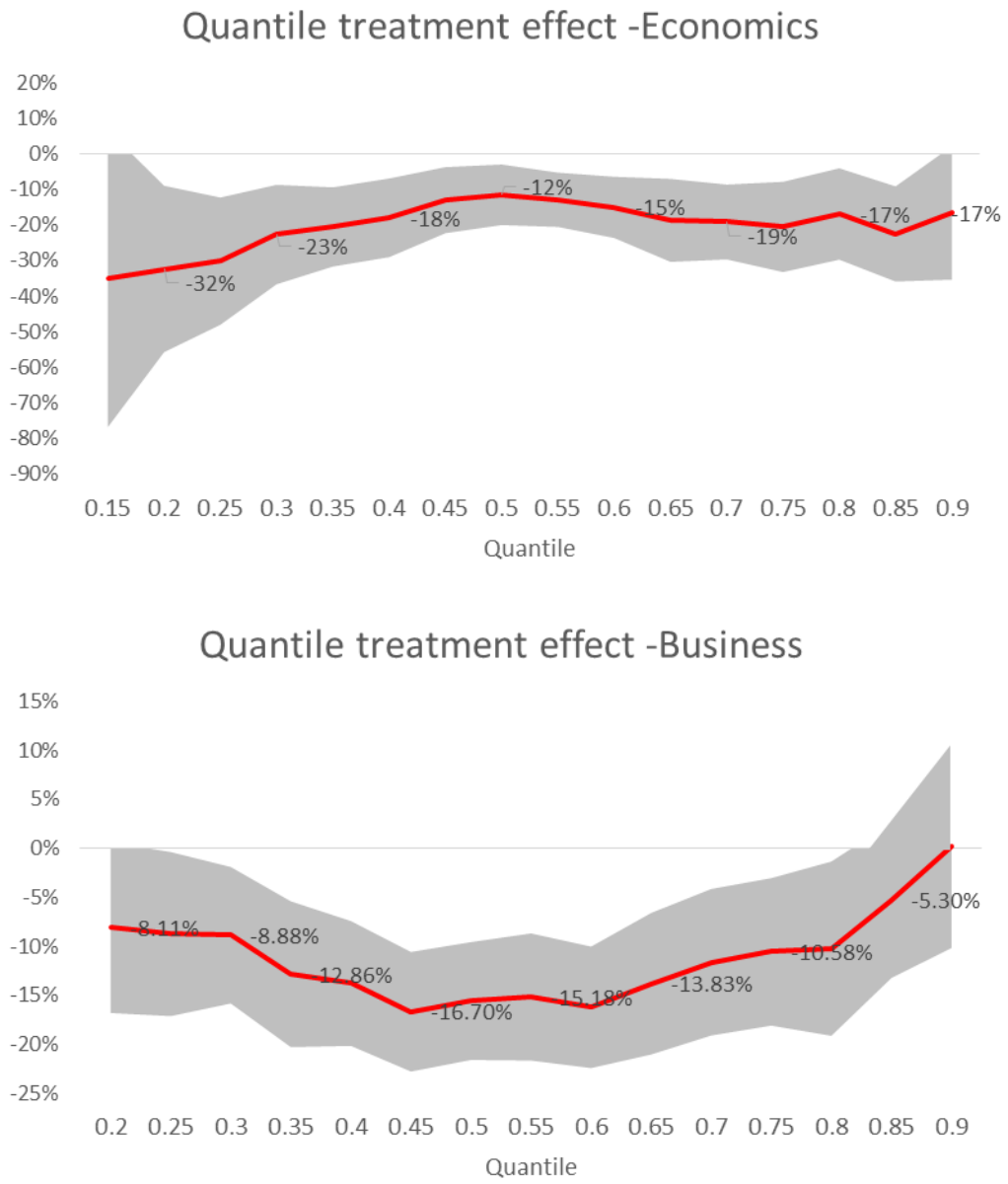
Source: Ministry of Education. Scatter plots are mean wages per cohort and school group. Lines are the fitted values of a regression quadratic on time. The vertical line represents the time of the reform.

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 squared, gender, and cohort control.

Figure 6: Changes in changes estimates



Source: Ministry of Education. CIC estimates of an estimation that controls 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 Los Andes	2,482,154	2.66	25.8	0.55	57.6	5.87	6,069
	1,695,091	1.99	2.2	0.50	5.4	1.53	

Note: Top rows show means and bottom 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 1,000 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***	-0.161***	-0.168***	-0.164***	-0.164***	-0.161***
	[0.0359]	[0.0356]	[0.0385]	[0.0384]	[0.0362]	[0.0360]
Post	0.0824*	0.0818*	0.0721*	0.0735*	0,0819	0,0863
	[0.0326]	[0.0325]	[0.0306]	[0.0300]	[0.0423]	[0.0414]
Andes	0.312***	0.301***	0.312***	0.300***	0.311***	0.299***
	[0.0450]	[0.0451]	[0.0443]	[0.0445]	[0.0452]	[0.0454]
Experience	0.135***	0.154***	0.137***	0.154***	0.135***	0.155***
	[0.00822]	[0.0251]	[0.00760]	[0.0249]	[0.0158]	[0.0278]
Experience sq		-0,0042		-0,00389		-0,00422
		[0.00548]		[0.00579]		[0.00511]
Female		-0.0911**		-0.0907**		-0.0913**
		[0.0272]		[0.0274]		[0.0287]
Constant	14.16***	14.20***	14.13***	14.17***	14.21***	14.19***
	[0.0416]	[0.0461]	[0.0756]	[0.0733]	[0.0449]	[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***	-0.121***	-0.126***	-0.126***	-0.121***	-0.121***
	[0.0229]	[0.0216]	[0.0237]	[0.0223]	[0.0228]	[0.0214]
Post	0.0846**	0.0840**	0.0480*	0.0484*	0.0904**	0.0928**
	[0.0198]	[0.0190]	[0.0195]	[0.0193]	[0.0269]	[0.0260]
Andes	0.425***	0.419***	0.428***	0.422***	0.425***	0.418***
	[0.0758]	[0.0719]	[0.0757]	[0.0718]	[0.0746]	[0.0707]
Experience	0.127***	0.140**	0.131***	0.142***	0.130***	0.147***
	[0.0102]	[0.0328]	[0.00942]	[0.0319]	[0.00923]	[0.0288]
Experience sq		-0,00299		-0,00257		-0,004
		[0.00667]		[0.00671]		[0.00664]
Female		-0.0990**		-0.0984**		-0.0994**
		[0.0284]		[0.0282]		[0.0290]
Constant	14.06***	14.11***	13.96***	14.01***	14.18***	14.10***
	[0.0618]	[0.0463]	[0.0950]	[0.0750]	[0.106]	[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 erros 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***	-0.162***	-0.169***	-0.165***	-0.164***	-0.162***
	[0.0371]	[0.0369]	[0.0395]	[0.0393]	[0.0373]	[0.0369]
Post	0.0774*	0,0762	0,0699	0,071	0,0756	0,0808
	[0.0344]	[0.0346]	[0.0332]	[0.0330]	[0.0457]	[0.0448]
Andes	0.313***	0.300***	0.312***	0.300***	0.312***	0.299***
	[0.0450]	[0.0452]	[0.0443]	[0.0444]	[0.0452]	[0.0454]
Panel B: Business						
Post*Andes	-0.104***	-0.104***	-0.110***	-0.109***	-0.104***	-0.104***
	[0.0209]	[0.0198]	[0.0215]	[0.0203]	[0.0210]	[0.0199]
Post	0.0802**	0.0798**	0.0438*	0.0441*	0.0838**	0.0866**
	[0.0191]	[0.0183]	[0.0189]	[0.0188]	[0.0266]	[0.0257]
Andes	0.426***	0.420***	0.429***	0.423***	0.426***	0.420***
	[0.0758]	[0.0719]	[0.0755]	[0.0717]	[0.0746]	[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 curricul but was enrrolled beofre the change, 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.

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***	-0.164***	-0.170***	-0.168***	-0.166***	-0.164***
	[0.0368]	[0.0378]	[0.0393]	[0.0403]	[0.0371]	[0.0382]
Post	0.0849*	0.0837*	0.0748*	0.0748*	0,0831	0,0859
	[0.0339]	[0.0348]	[0.0316]	[0.0317]	[0.0423]	[0.0426]
Andes	0.314***	0.305***	0.313***	0.304***	0.313***	0.304***
	[0.0458]	[0.0460]	[0.0448]	[0.0451]	[0.0460]	[0.0463]
Panel B: Business						
Post*Andes	-0.118***	-0.117***	-0.122***	-0.121***	-0.118***	-0.118***
	[0.0210]	[0.0196]	[0.0219]	[0.0204]	[0.0207]	[0.0192]
Post	0.0837**	0.0844***	0.0515*	0.0534*	0.0916**	0.0962**
	[0.0194]	[0.0184]	[0.0215]	[0.0213]	[0.0247]	[0.0234]
Andes	0.421***	0.415***	0.424***	0.418***	0.421***	0.414***
	[0.0759]	[0.0717]	[0.0762]	[0.0719]	[0.0746]	[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 curricul but was enrrolled beofre the change, 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.

Table 5: Effect of the reform on the recruitment process

Dependent variable: 1 if hired and 0 if not	
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 positions from 2008 to 2014

Source: Central Bank of Colombia.

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

Table 6: 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	-0.005	-0.007	-0.007	-0.002	-0.003
	[0.0481]	[0.0482]	[0.0488]	[0.0490]	[0.0482]	[0.0486]
Fake post	0.012	0.002	-0.017	-0.025	0.018	0.015
	[0.0458]	[0.0455]	[0.0605]	[0.0592]	[0.0498]	[0.0497]
Andes	0.313***	0.300***	0.315***	0.301***	0.309***	0.294***
	[0.0357]	[0.0366]	[0.0366]	[0.0375]	[0.0365]	[0.0375]
Panel B: Business						
Fake post*Andes	0.016	0.009	0.017	0.009	0.014	0.006
	[0.0838]	[0.0785]	[0.0915]	[0.0847]	[0.0812]	[0.0758]
Fake post	0.061	0.061	-0.057	-0.054	0.080	0.082
	[0.0772]	[0.0747]	[0.184]	[0.177]	[0.0821]	[0.0782]
Andes	0.420***	0.417***	0.423***	0.420***	0.420***	0.416***
	[0.0640]	[0.0593]	[0.0681]	[0.0618]	[0.0612]	[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 midle of the period (2004-1 for econ and 2003-2 for business).

Standard erros in brackets below the coefficients.

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

Source: Ministry of Education.

Table 7: 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	-0.00913	-0.00696	-0.00657	-0.00282	-0.00261
	[0.0525]	[0.0524]	[0.0535]	[0.0536]	[0.0572]	[0.0573]
Date of business reform						
Post*Andes	-0.0238	-0.023	-0.0224	-0.0216	-0.0103	-0.00964
	[0.0341]	[0.0342]	[0.0347]	[0.0348]	[0.0379]	[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 curricul but was enrrolled beofre the change, 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.

Table 8: Robustness checks

Dep variable: Ln wage	Economics (1)	Economics (2)	Business (3)	Business (4)
Panel a: Controlling for age				
Treatment	-0.162***	-0.158***	-0.0952**	-0.0950**
	[0.0510]	[0.0510]	[0.0410]	[0.0412]
Panel b: Without cohorts that graduated at the same time				
Treatment	-0.159***	-0.154***	-0.118***	-0.118***
	[0.0552]	[0.0552]	[0.0437]	[0.0439]
Panel c: Taking graduates from Top 3 schools as control (1)				
Treatment	-0.115**	-0.115**	-0.145***	-0.145***
	[0.0557]	[0.0557]	[0.0472]	[0.0472]
Panel d: Including in the control group only students that could have attended Los Andes				
Treatment	-0.186***	-0.184***	-0.152**	-0.151**
	[0.0434]	[0.0441]	[0.0640]	[0.0626]
Panel e: Without 2007-1 cohort				
Treatment	-0.152***	-0.146***	-0.117***	-0.118***
	[0.0510]	[0.0511]	[0.0418]	[0.0420]
Panel f: Controlling for HS exit scores				
Treatment	-0.185***	-0.180***	-0.161*	-0.160**
	[0.0469]	[0.0472]	[0.0624]	[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 erros in brackets below the coefficients.
*p<0.1, **p<0.05, ***p<0.01
Source: Ministry of Education.

Table 9: Synthetic control

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

Standard errors clustered by individual.

Standard erros in brackets below the coefficients.

The number in parenthesis is the optimal weight.

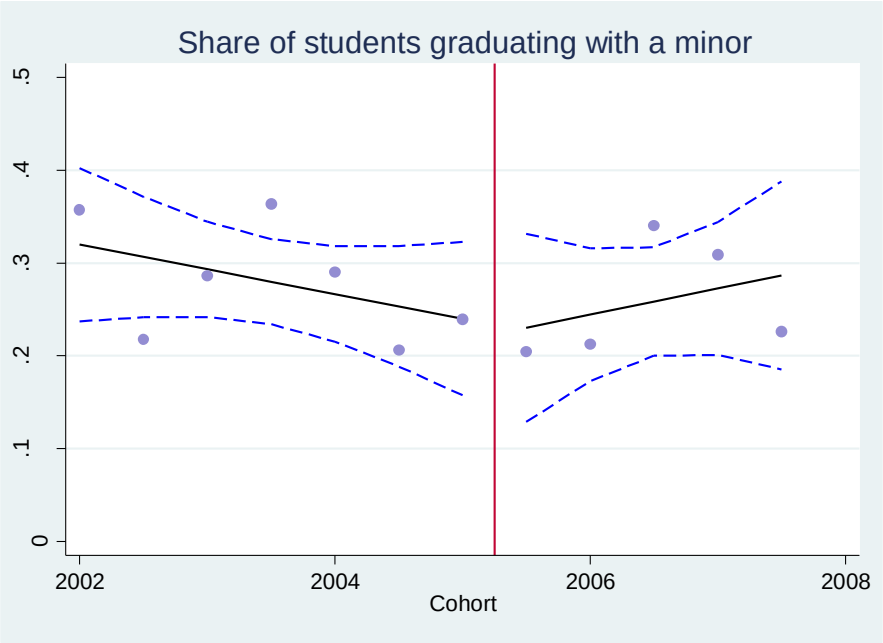
Column 1 includes experience and cohort controls, column 2 adds experience square and gender.

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

Source: Ministry of Education.

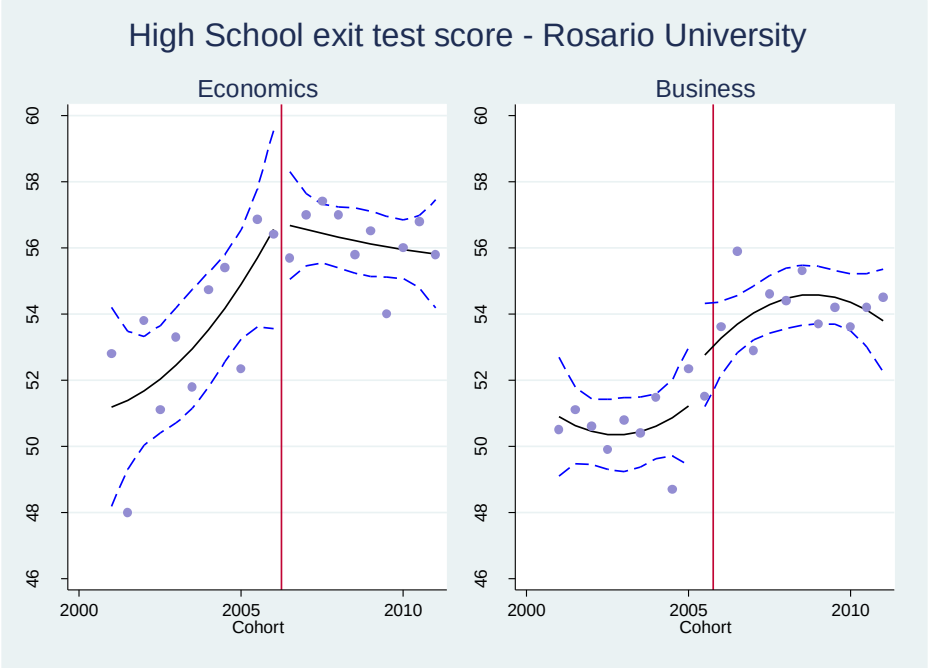
Appendix 1: Extra figures and tables.

Figure A1.1



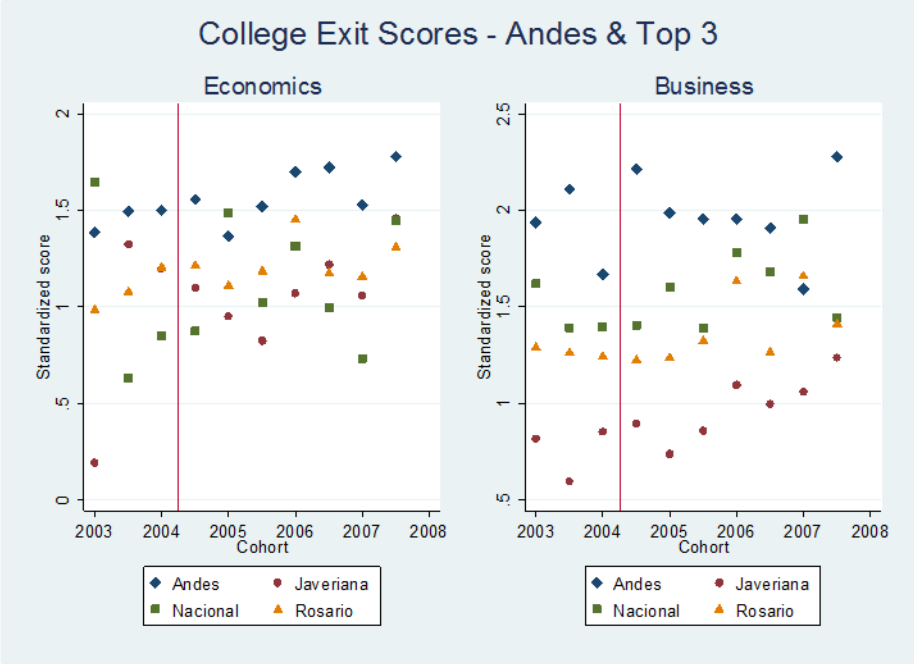
Source: Admissions Department – Universidad de los Andes

Figure A1.2



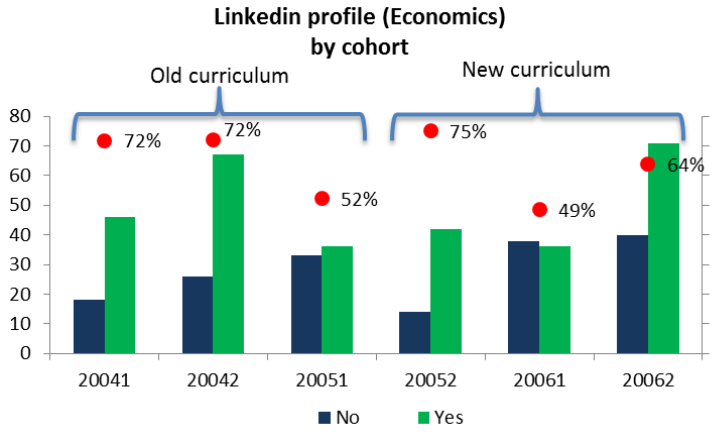
Source: Boletin Estadistico – Universidad del Rosario.

Figure A1.3: Effects of the reform on ranking



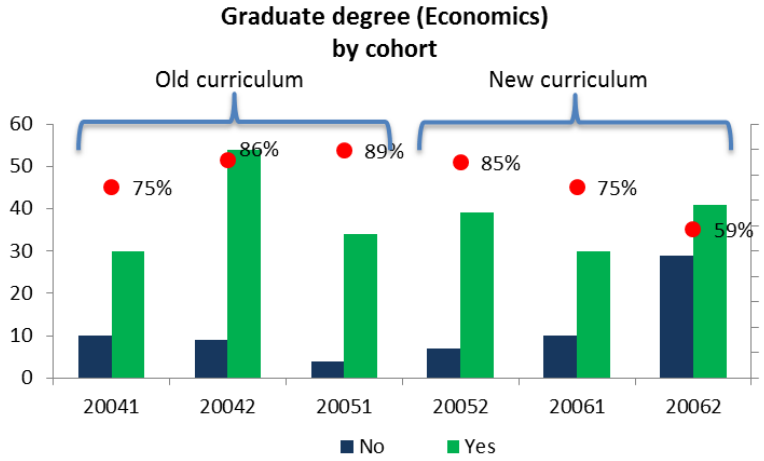
Source: Ministry of Education.

Figure A1.4: LinkedIn profile



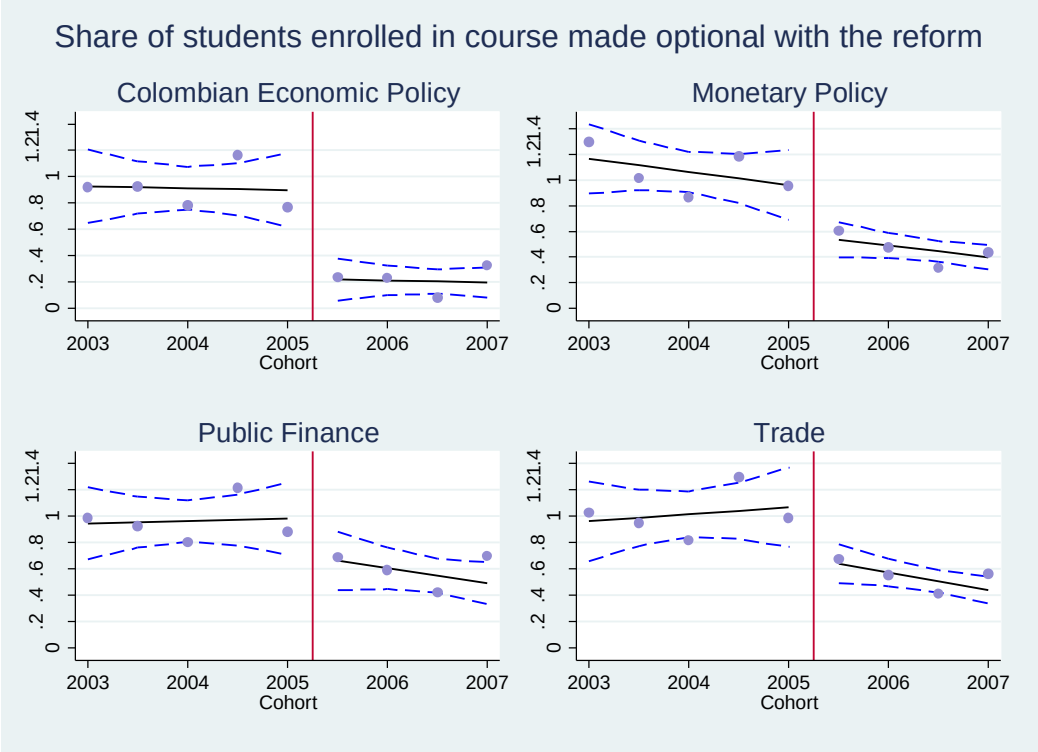
Source: Ministry of Education, Universidad de los Andes, and LinkedIn.

Figure A1.4: Graduate degrees



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

Figure A1.6: Share of students enrolled in courses made optional by 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	1.818***	Andes*Post	0.0283	-0.0016
	[0.620]	[0.446]		[0.0545]	[0.0505]
Post	1.632***	1.181***	Post	-0.0690*	-0.0499
	[0.389]	[0.189]		[0.0315]	[0.0305]
Andes	5.104***	5.799***	Andes	0.0412	0.0357
	[0.390]	[0.299]		[0.0364]	[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 Employers of Economics Graduates

Top Employers cohorts 2003-1 to 2007-2	
Universidad de los Andes	120
National Planning Dept	25
Central Bank	23
BANCO DE BOGOTA (Priv Bank)	21
FEDESARROLLO (research center)	18
Ministry of Finance	15
DAVIVIENDA (Priv Bank)	14
IADB	13
CITI	12
BANCOLOMBIA (Priv Bank)	11
ECOPETROL	8
AVIANCA	7
ANIF	7
Ministry of Defense	7

This accounts for 20% of the students

Top Employers			
Pre		Post	
Universidad de los Andes	32	Universidad de los Andes	35
National Planning Dept	**17**	**BANCO DE BOGOTA (Priv Bank)**	**9**
Central Bank	**15**	National Planning Dept	6
Ministry of Finance	**9**	Central Bank	5
IADB	**8**	Ministry of Finance	5
BANCOLOMBIA (Priv Bank)	**8**	IADB	5
FEDESARROLLO (research center)	**8**	FEDESARROLLO (research center)	5
Self employed	6	DAVIVIENDA (Priv Bank)	5
DAVIVIENDA (Priv Bank)	5	LAN AIRLINES	5
BANCO DE BOGOTA (Priv Bank)	5	CITI (Priv Bank)	4
BANCO DE CREDITO (Priv Bank)	5	World Bank	4
CITI (Priv Bank)	5	CORFICOLOMBIANA	4
ECOPETROL	5	OPORTUNIDAD ESTRATEGICA	4

Source: Department of Economics - Universidad de los Andes.