Journal of Public Economics 157 (2018) 212–225

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

The effect of human capital on earnings: Evidence from a reform at Colombia's top university☆

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ARTICLE INFO

JEL classifications:
I23
I25
I26
J24
J31

Keywords:
Education
Human capital
Signaling

ABSTRACT

In this paper I test whether the return to college education is the result of human capital accumulation or instead reflects 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 degrees in economics and business by 20% and 14%, respectively, but did not change the quality of incoming or graduating students. The size of the entering class, their average high school exit exam scores, and graduation rates were not affected by the reform, indicating that selection of students into the degrees remained the same. Using administrative data on wages and college attendance, I estimate that wages fell by approximately 16% in economics and 13% in business. These results suggest that human capital plays an important role in the determination of wages and reject a pure signaling model. Surveying employers, I find that the reduction 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 Los Andes graduates' being hired by 17 percentage points.

1. 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). There is less consensus about the mechanisms through which education leads to higher wages. Studies that estimate causal returns to schooling cannot shed light on the sources of such returns (Card, 1999). 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, 1962 and Mincer, 1974). Second, the signaling theory posits that higher wages reflect the correlation between education and unobserved ability.[1] 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, separate from that of signaling, by exploiting a curriculum change at Universidad de Los Andes, the top university in Colombia. In 2006, the number of credits required to earn a college degree in economics and business decreased by 20% and 14%, respectively. This was accomplished by dropping 12 required courses in economics and 6 in business, and a reduction in instruction time from 4.5 to 4 years.[2] The identification strategy of this paper relies on the fact that the reform did not alter the selection of entering or graduating students. 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 (the Saber 11). I show that the size of the entering class did not grow, nor did average entrance test scores decrease, and dropout rates did not change with the reduction in the number of classes. Therefore,

☆ I am grateful to the Colombian Ministry of Education, the Central Bank of Colombia, and the Economics Department at Universidad de Los Andes for providing the data for this study. I would like to thank Magne Mogstad and two anonymous referees for their excellent comments. I am extremely grateful to Adriana Lleras-Muney for her encouragement and suggestions. I also want to thank David Atkin, Leah Boustan, Moshe Buchinsky, Michela Giorcelli, Carlos Medina, Maurizio Mazzocco, Rodrigo Pinto, Sarah Reber, Juan E. Saavedra, Andres Santos, and Till von Wachter for their comments and feedback. I am grateful to my colleagues Pasha Andreyanov, Tiago Caruso, Richard Domurat, Keyoung Lee, Rustin Partow, and Maria Lucia Yanguas for insightful suggestions and discussions. I thank seminar participants at UCLA, SOLE, LACEA, EBE, Universidad de Los Andes and the Central Bank of Colombia for valuable comments.

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[1] Of course, the two theories are not mutually exclusive.

[2] 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 expected to take six courses per term; this was changed to five. In business, the number of classes per term remained at five.

https://doi.org/10.1016/j.jpubeco.2017.10.007
Received 14 November 2016; Received in revised form 2 October 2017; Accepted 29 October 2017
Available online 08 November 2017

the reform had no short-run effect on the quality of the entering class after 2006, but it decreased human capital accumulation. The human capital model predicts a decline in wages as a result of the reform, whereas a pure 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 decreased by 16% in economics and 13% 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 the likelihood of being employed by the highest-paying firms decreased. Moreover, I find that there is a relationship between the classes dropped and the placement of graduates across employers. Using data from the recruitment process for economists at the Central Bank from 2008 to 2014, I find that for graduates of 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 (Oreopoulos et al., 2012), my results could also hint at possible longer-term effects. These results, however, are not estimates of the internal rate of returns to investment in additional schooling, but simply the effect on wages early in people's careers.

Finally, I examine possible threats to my identification strategy. First, it could be that the curriculum reform 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 to enroll in these programs, which would lead to a decrease in the value of the signal and in wages. 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: First, I replicate my baseline estimation using a placebo date for the reform, and second, I test my specification using a major at Los Andes that did not undergo a curriculum reform. I do not find evidence of wage changes in either case. My results are robust to several additional checks explained in the robustness section. Lastly, 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. To check that this is the case, I estimate the effect of the reform on the probability of being employed in the formal sector. I find that for both economics and business the effects are very close to zero and statistically insignificant.

This paper contributes to the literature by estimating the effect of human capital accumulation on wages, separate from that of signaling, in a college setting. To the best of my knowledge, this is also the first paper to investigate the mechanisms that led to changes in wages; as a result, the study provides important information about the tools employers use to learn about workers' expected productivity.

A number of papers have investigated this issue in primary and secondary education settings and obtained mixed results. Eble and Hu (2016) exploit the introduction of one extra year in primary school in China in 1980 and 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, however, no extra coursework was introduced in that additional year. Lang and Kropp (1986) and Bedard (2001) find secondary schooling decisions that are consistent with a signaling model and 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 et al. (2000) estimate the signaling value of the GED to be between 12% and 20%, whereas Clark and Martorell (2014) find little evidence of high school diploma signaling effects.

Finally, my results suggest that human capital accumulation is an important explanation for the returns to college education. This finding relates to a growing literature that estimates the returns to different types of post-secondary education by separating the effect of the institution versus the field of study. This literature suggests that what matters the most is the type of degree as opposed to the institution from which it was obtained (Dale and Krueger, 2002; Kirkebøen et al., 2016, and Hastings et al., 2013). This paper contributes by providing evidence that suggests that the source of heterogeneity in returns may be due to the different sets of skills and knowledge acquired in each degree, and not to differences in selectivity.

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, and Section 4 describes the data, empirical strategy, and results. In Section 5, I explore the mechanisms that explain my results. Section 6 presents robustness checks, and Section 7 concludes.

2. 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, 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 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, such that $c(f_j, \theta_i) > c(f_k, \theta_i)$ for every i when $j < k$, that is, when j offers higher human capital than k, and $c(f_j, \theta_i) < c(f_j, \theta_m)$ when $\theta_i > \theta_m$. In addition, $\frac{\partial^2 c(f,\theta)}{\partial f \partial \theta} < 0$, meaning that the cost of attending harder schools increases less for higher-ability individuals.

Suppose that productivity is a linear function of ability and human capital such that for a given set of beliefs regarding the assignment of students to colleges, expected productivity takes the form in Eq. (1).

$$w_j = \mu(E[\theta_i \mid f_j], f_j) = \alpha_1 + \alpha_2 \overline{\theta}_j + \alpha_3 f_j \tag{1}$$

These beliefs are written $E[\theta_i \mid f_j] \equiv \overline{\theta_j},$ where $\theta_{j,}$ is decreasing in j (i.e., firms believe that average ability is greater in higher-ranked colleges), and firms observe the level of instruction f_j. In a separating Perfect Bayesian Equilibrium, agents signal their type, and firms predict ability based on the observed level of human capital and offer wages accordingly. Students choose the school j that maximizes wages net of effort costs:

$$w_j - c(f_j, \theta_i) = \mu(E[\theta_i \mid f_j], f_j) - c(f_j, \theta_i) \tag{2}$$

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

$$w_1 - w_2 \geq c(f_1, \theta_i) - c(f_2, \theta_i) \tag{3}$$

The left-hand side of Eq. (3) is a positive constant, whereas the right-hand side is decreasing in θ_i. Then, there exists a unique θ^1 such that $\forall \theta \geq \theta^1$ (3) will hold (see Appendix 1 for the proof and a simple example). Subsequently, there is a threshold θ for each pair of schools that determines school choice over the school's ranking.

In this framework, the question of signaling vs. human capital comes down to learning about the values of $\alpha_2\bar{\theta}$ and $\alpha_3 f$ in Eq. (1). 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 also 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 in which 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:

$$\Delta w_1 = \alpha_3 \Delta f_1 \tag{4}$$

If α_3 is zero, the data support a pure signaling model in which wages are solely determined by the school's average student ability; see Eq. (1). If α_3 is statistically different from zero, this suggests a role for human capital in the determination of wages. In the next section, I will review the assumptions that lead to this result.

3. Institutional background and reform

I will first describe the salient characteristics of Colombian education and labor market institutions and details of the curriculum reform. 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%. Regarding 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 human resources selection tests, and interviews. Twenty-five percent of college graduates work in the informal sector. Los Andes is a private university, and is ranked first in Colombia.

3.1. Reform

In 2006, Los Andes unilaterally decided to reduce the coursework required to earn a degree in most of its majors.[3] The reasons given for the reform were to move toward international standards of shorter college degrees and to encourage graduate study. Each department was autonomous in implementing the reform. In this paper, I exploit the reforms implemented by the economics and business departments. These consisted solely of a reduction in required credits; in other departments, the change led to the complete overhaul of curricula. In economics, the curriculum was trimmed by 12 courses (20% of the total number of credits), which resulted in a median number of five courses per term instead of six. Specifically, the reform: (i) took six mandatory courses and change them to optional courses (Monetary Policy, Public Finance, Trade, Marxist Economics, Colombian Economic Policy, and Social Programs Evaluation); (ii) reduced the number of optional courses 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. For both majors, instruction time was reduced from 4.5 years to 4 years.

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.

3.2. First stage: empirical evidence of the reform for economics and business

To separately estimate the effects of human capital from that of signaling, I need an effective decline in the number of terms studied and credits earned, with no changes in the quantity or 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?

Fig. 1 shows the 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 duration declined from 5.5 to 5 years. Fig. 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%. Table 1 shows regression estimates from fitting different linear trends around the reform.

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

To evaluate this question, I check the evolution of the size of the entering classes, their average Saber 11 scores, and average graduation rates. *Panel a* of Fig. 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.[4] *Panel b* of Fig. 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. Specifically, I compare Saber 11 scores for incoming cohorts before and after the reform for Los Andes and other Top 10 schools. Table A2.1 shows that there is no evidence of a reduction in scores after the reform at Los Andes.

On the other hand, if the change in curricula 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 Fig. 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 test whether

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

[4] Even 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 the control group does not experience the same change in trend as Los Andes. To check for this possibility, Figure A 1.2 shows Saber 11 scores for entering cohorts at Rosario University and reveals a similar pattern.

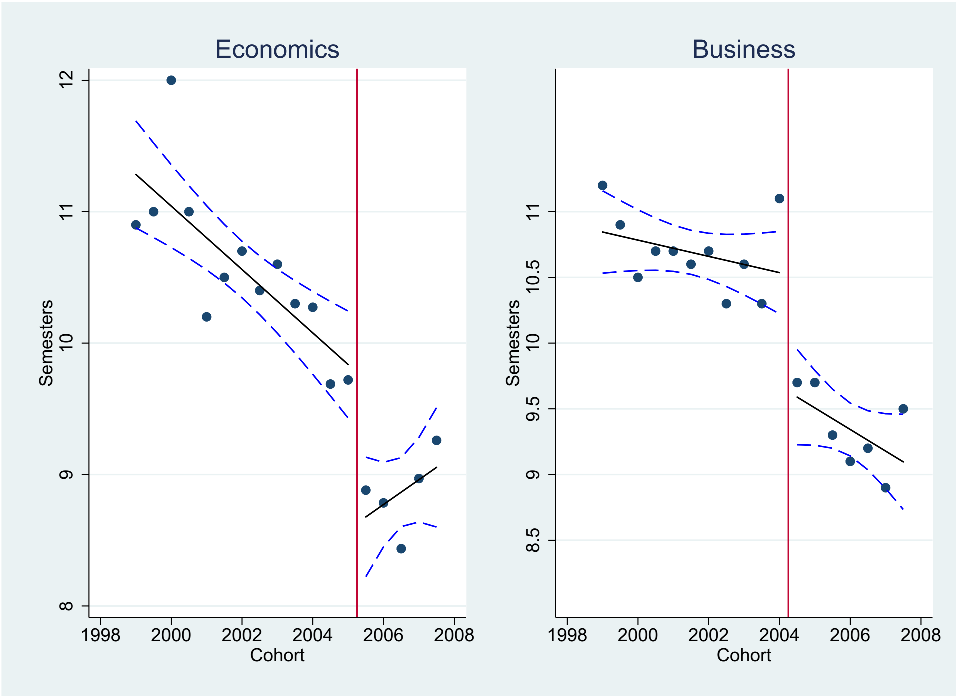

Fig. 1. Effect of the reform in degree duration.
Source: Annual statistical bulletin – Universidad de Los Andes. Scatter plots are mean degree duration per cohort. A cohort is defined by the semester the student started school. This graph includes all students who started the program. Solid lines are the fitted values of a linear regression on time, and dashed lines represent 95% CI of the estimation. The vertical line represents the time of the reform.

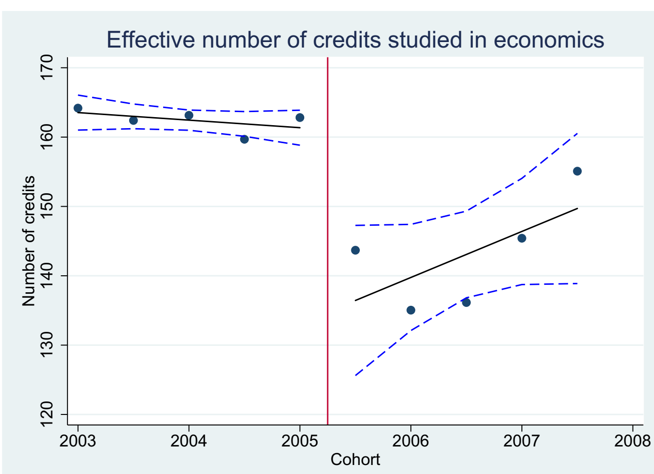

Fig. 2. Effect of the reform in credits studied.
Source: Department of Economics – Universidad de Los Andes. Scatter plots are credits studied by cohort. Solid lines are the fitted values of a linear regression on time and dashed lines are the 95% CI of the estimation. The vertical line represents the time of the reform.

the reform changed the probability of graduating with an economics or business degree, and do not find evidence that it did (Table A2.1). Fig. A3.1 also shows that the reform did not change the share of students who graduated with a minor.

In the model, students use school rankings to choose which college to attend; 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 exam scores. International rankings that include Latin American universities are only available since 2013, but from 2013 to 2016, Los Andes has been ranked as the best school in Colombia.[5] The Colombian Ministry of Education released its first rankings in 2015, in which it also ranked Los Andes first.[6]

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 selectivity of the degrees remained unchanged after the reform. This is an ideal environment to test the role of signaling and human capital in college education.

4. Effects of the reform

In this section, I estimate the effect of the reduction of the curricula 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.

4.1. Data

I use administrative data from the Ministry of Education. My main database is the OLE (*Observatorio Laboral de Educación*), which is constructed to follow yearly earnings in the formal sector for college graduates in Colombia.[7] This information is recorded from Social Security payments from 2008 to 2012. The OLE also contains education variables, such as university and program attended, graduation year, and personal characteristics.

[5] 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.

[6] http://www.mineducacion.gov.co/cvn/1665/w3-article-351855.html Accessed February 10, 2016.

[7] 75% of workers with a college education are employed in the formal sector (Fedesarrollo, 2013).

Table 1
First stage – The effect of the reform on instruction and class quality.
Source: Annual bulletin – Universidad de los Andes & Department of economics.

Dep variable	Degree duration		No. of credits	Class size		HS test scores		Graduation rates	
	Econ	Buss	Econ	Econ	Buss	Econ	Buss	Econ	Buss
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post	− 1.038**	− 0.916***	− 24.37**	8.56	− 22.15	− 1.396	36.42	0.0192	0.00097
	[0.367]	[0.262]	[6.751]	[14.74]	[32.20]	[40.97]	[71.51]	[0.0635]	[0.0496]
Trend pre	0.0943	− 0.0821	3.317*	1.024	2.086	− 6.272	− 1.655	− 0.0135	− 0.00503
	[0.119]	[0.0528]	[1.637]	[2.248]	[4.124]	[5.546]	[7.913]	[0.0110]	[0.00606]
Trend post	− 0.121***	− 0.0309	− 0.545	0.0952	− 2.309	12.90***	19.93***	0.00692	0.0145**
	[0.0280]	[0.0266]	[1.637]	[2.248]	[1.899]	[3.755]	[3.801]	[0.00596]	[0.00606]
Constant	9.716***	10.51***	160.8***	68.08***	64.35***	637.6***	557.0***	0.842***	0.789***
	[0.222]	[0.181]	[5.430]	[9.403]	[5.537]	[21.28]	[16.13]	[0.0438]	[0.0376]
Obs	18	18	10	16	16	21	21	20	20
R squared	0.868	0.881	0.867	0.233	0.171	0.45	0.672	0.163	0.427

Standard errors in brackets below the coefficients.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

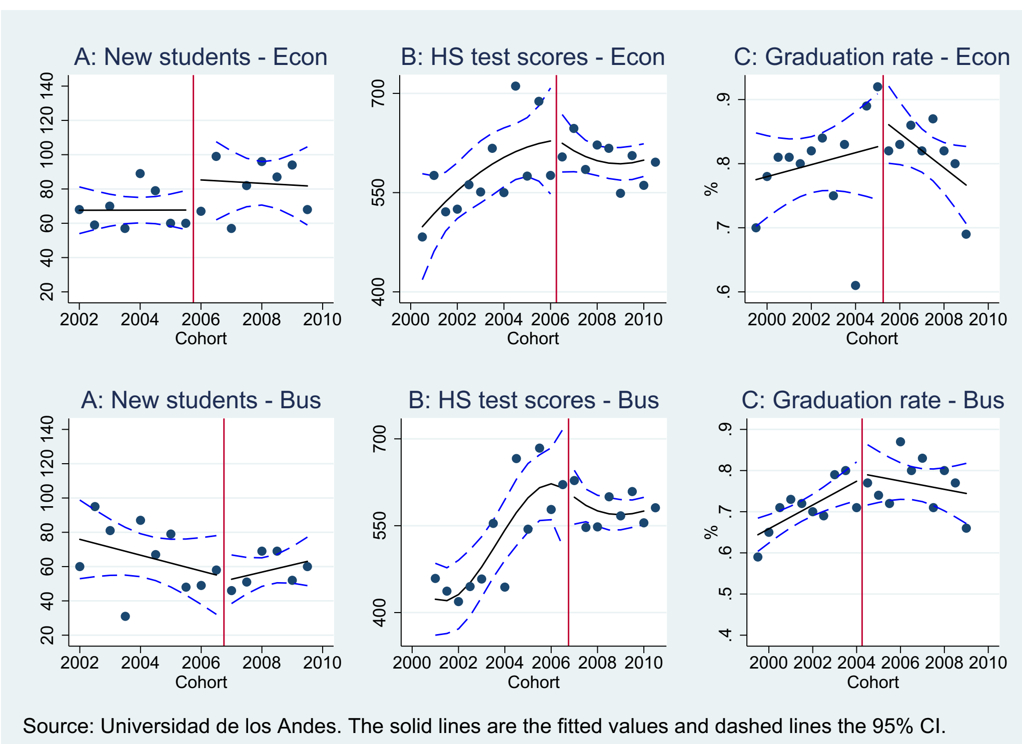

Fig. 3. Effects of the reform on class selection.

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

The three databases contain generated ID numbers to trace individuals.[8] My baseline database contains all students who started college between 2002 and 2007 and graduated after 2004 from Los Andes and other top-10 schools, and who enrolled in economics or business. Table 2 shows summary statistics of some relevant variables in the data. 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.

[8] Anonymized identifiers are generated using national identification numbers, name, and date of birth.

[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.

Table 2
Summary statistics.
Source: Ministry of Education, Colombia.

	Real wage	Experience	Age	Female	HS test	Family income[a]	Obs
Andes economics	3,017,001	2.6	25.8	0.46	58.1	5.93	1736
	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	3580
	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	2659
	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	22505
	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	6069
	1,695,091	1.99	2.2	0.50	5.4	1.53	

Note: Top rows show means and bottom standard deviation. Data from all students who started college between 2002 and 2007, and graduated after 2004. The top 10 universities were chosen using SABER PRO scores for schools of at least 1000 students.
[a] Based on a classification over 9 categories of income.

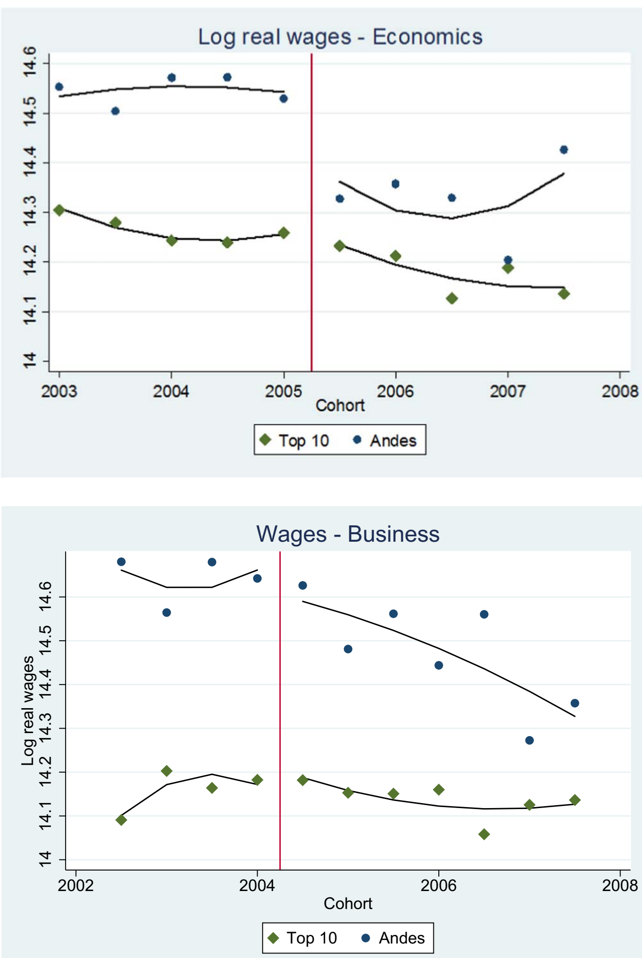

Fig. 4. Pre trends and the effect of the reform on wages.
Source: Ministry of Education. Scatter plots are mean wages per cohort and school group. Lines are the fitted values of a regression quadratic on time. The vertical line represents the time of the reform.

4.2. Preliminary evidence and empirical strategy

Fig. 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, the premium immediately declined for economics and gradually for business, for a final average reduction of 22 percentage points and 12 percentage points, respectively. Fig. A3.3 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 Figs. 4 and A 2.3 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:

$$\ln wage_{it} = \beta_0 + \beta_1 Andes_i * Post_t + \beta_2 Andes_i + \beta_3 Post_t + \beta_4 experience_{i,t} + \varepsilon_{it} \quad (5)$$

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 a student started school after the date of the reform implementation and 0 otherwise, and experience is measured in years since graduation. The coefficient β_1 captures the effect of graduating from Los Andes after 2006 on wages. I also control for gender, year, and cohort effects in other specifications.

4.3. Results

Table 3 shows my baseline results: *Panel a* presents estimates for economics and *panel b* for business. The baseline estimation for Eq. (1), reported in column 1, indicates a decline in wages by 16% for economics and 13% for 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 should not change; given the magnitude of the decline, they demonstrate an important role for human capital in the determination of wages.

While estimation is straightforward in this setting, statistical inference is not. Moulton (1990) shows that the failure to account for the presence of common group errors leads to insufficiently conservative inference. In response to this concern, a clustering procedure emerged whereby inference relies on the asymptotic approximations associated with the assumption that the number of individuals within a group and/or the number of groups grows large. However, this assumption does not apply in my setting. To address this concern, I follow Abadie et al. (2010).[10] Their inferential exercise examines whether the estimated

[10] See also Imbens and Wooldridge (2009) and Cunningham and Shah (2017) for recent applications of this method.

Table 3
Baseline results. Effect of the reform on wages.
Source: Ministry of Education OLE and SPADIES.

Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Panel a: Economics						
Post*Andes	− 0.163**	− 0.161**	− 0.167***	− 0.164**	− 0.164**	− 0.161**
	[0.0500]	[0.0501]	[0.0505]	[0.0505]	[0.0501]	[0.0501]
Post	0.0817**	0.0819**	0.0721*	0.0744*	0.0810*	0.0865*
	[0.0293]	[0.0292]	[0.0311]	[0.0310]	[0.0366]	[0.0360]
Andes	0.312***	0.301***	0.312***	0.300***	0.311***	0.300***
	[0.0304]	[0.0301]	[0.0304]	[0.0301]	[0.0304]	[0.0301]
Experience	0.135***	0.154***	0.137***	0.154***	0.135***	0.156***
	[0.00842]	[0.0173]	[0.00841]	[0.0173]	[0.0127]	[0.0188]
Experience squared		− 0.00424		− 0.004		− 0.00429
		[0.00431]		[0.00429]		[0.00431]
Female		− 0.0912***		− 0.0908***		− 0.0914***
		[0.0223]		[0.0223]		[0.0224]
Constant	14.16***	14.20***	14.13***	14.17***	13.96***	14.19***
	[0.0197]	[0.0238]	[0.0495]	[0.0511]	[0.0846]	[0.0383]
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
Panel b: Business						
Post*Andes	− 0.136***	− 0.136***	− 0.141***	− 0.141***	− 0.135**	− 0.136**
	[0.0410]	[0.0413]	[0.0412]	[0.0414]	[0.0412]	[0.0414]
Post	0.0952***	0.0940***	0.0555**	0.0558**	0.0971***	0.0991***
	[0.0153]	[0.0152]	[0.0185]	[0.0185]	[0.0189]	[0.0188]
Andes	0.429***	0.423***	0.432***	0.427***	0.429***	0.423***
	[0.0312]	[0.0316]	[0.0312]	[0.0316]	[0.0312]	[0.0315]
Experience	0.124***	0.145***	0.128***	0.147***	0.125***	0.151***
	[0.00517]	[0.0115]	[0.00512]	[0.0115]	[0.00782]	[0.0120]
Experience squared		− 0.00525		− 0.00481		− 0.00635*
		[0.00303]		[0.00303]		[0.00302]
Female		− 0.0976***		− 0.0969***		− 0.0979***
		[0.0147]		[0.0147]		[0.0148]
Constant	14.06***	14.11***	13.96***	14.00***	14.15***	14.10***
	[0.0129]	[0.0160]	[0.0317]	[0.0337]	[0.0968]	[0.0243]
Cohort control	N	N	Y	Y	N	N
Year D	N	N	N	N	Y	Y
Clusters	11	11	11	11	11	11
N	10,348	10,348	10,348	10,348	10,348	10,348
R − sq	0.122	0.130	0.124	0.132	0.122	0.131

Standard errors clustered at the individual level.
Control group: students from business at top 10 schools.
Cohort control: Semiannual GDP growth. Cohort refers 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.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

effect of the actual intervention is large relative to the distribution of the effects estimated for schools that are not affected by the reform. To implement this procedure, I estimate Eq. (1) an additional 10 times, replacing Los Andes with an indicator for one of the other 10 schools. For all cases, the estimate for Los Andes was the largest (Table 4). I also evaluate Eq. (1), changing the date of the reform, in addition to the treated school. Fig. 5 and Supplementary Figs. S1 and S2 show the distribution of treatment effects: In all specifications the effect I estimate is at the 5th percentile mark, or to the left.

With the data available, I can only estimate the effect of the reform on earnings early in students' careers. However, it is at this stage that the debate over signaling and human capital is particularly relevant, given that with time, employers learn about students' productivity on the job (Farber and Gibbons, 1996; Altonji and Pierret, 2001 and Lange, 2007). Specifically, consistent with my findings, Lange (2007) finds that employers learn quickly; initial expectation errors decline by 50% within 3 years. Lange also estimates that signaling contributes < 25% to gains from schooling.

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 estimate an alternative specification in which

Table 4
Placebo coefficients.
Source: Ministry of Education OLE and SPADIES.

Diff in Diff coefficient	(1)	(2)	(3)	(4)	(5)	(6)
Panel a: Economics						
Andes	− 0.163	− 0.161	− 0.167	− 0.164	− 0.164	− 0.161
Placebo school 1	− 0.145	− 0.145	− 0.150	− 0.148	− 0.148	− 0.146
Placebo school 2	− 0.127	− 0.104	− 0.129	− 0.106	− 0.123	− 0.099
Placebo school 3	− 0.041	− 0.053	− 0.045	− 0.045	− 0.044	− 0.057
Placebo school 4	− 0.039	− 0.030	− 0.032	− 0.035	− 0.037	− 0.031
Placebo school 5	− 0.026	− 0.024	− 0.022	− 0.021	− 0.027	− 0.025
Placebo school 6	0.080	0.071	0.081	0.074	0.078	0.070
Placebo school 7	0.081	0.073	0.087	0.077	0.085	0.075
Placebo school 8	0.106	0.104	0.109	0.107	0.108	0.103
Placebo school 9	0.113	0.105	0.118	0.109	0.111	0.106
Placebo school 10	0.197	0.220	0.200	0.222	0.206	0.230
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
Panel b: Business						
Andes	− 0.136	− 0.136	− 0.141	− 0.141	− 0.135	− 0.136
Placebo school 1	− 0.085	− 0.083	− 0.088	− 0.087	− 0.084	− 0.082
Placebo school 2	− 0.059	− 0.055	− 0.052	− 0.049	− 0.058	− 0.054
Placebo school 3	− 0.045	− 0.045	− 0.044	− 0.043	− 0.046	− 0.045
Placebo school 4	− 0.040	− 0.033	− 0.038	− 0.035	− 0.039	− 0.032
Placebo school 5	− 0.032	− 0.033	− 0.034	− 0.032	− 0.029	− 0.032
Placebo school 6	− 0.025	− 0.017	− 0.033	− 0.024	− 0.020	− 0.011
Placebo school 7	− 0.008	0.001	0.000	0.008	− 0.010	− 0.001
Placebo school 8	0.017	0.006	0.025	0.014	0.025	0.015
Placebo school 9	0.072	0.075	0.077	0.079	0.074	0.077
Placebo school 10	0.104	0.100	0.113	0.108	0.107	0.103
Cohort control	N	N	Y	Y	N	N
Year D	N	N	N	N	Y	Y
Clusters	11	11	11	11	11	11
Obs	10,352	10,352	10,352	10,352	10,352	10,352

Controls: (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.

the treatment group consists solely of Los Andes students who were already enrolled at the time of the reform, but studied under the new curricula. Table 5 and Supplementary Fig. S1 show 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 12% 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 6 and Supplementary Fig. S2 I include observations with at most three years of experience, to ensure that the treatment coefficient is not capturing differences in the slope of the experience profile. Results in Table 6 again suggest strong wage declines of the same magnitudes as those found previously.

To make use of all data available, and recognizing the potential for heterogeneous effects, I now turn to a changes-in-changes (CIC) estimation following Athey and Imbens (2006) and Melly and Santangelo, 2015 that extends the model to include covariates. I estimate CIC for the 10th through 90th percentiles after controlling for experience, gender, and cohort effects. As can be seen in Fig. 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.

The decline in wages I find is large, and suggests sizable estimates of the return to attending college at Los Andes. To better understand the magnitude of my estimates, I perform a back of the envelope calculation that attempts to quantify the reduction in the wage premium of attending college at Los Andes. Using a cross-section estimate of the return to college and an estimate of the return to Los Andes from Saavedra (2008), I find that the return to attending college at Los Andes relative to not attending college is 110% (details of this calculation are explained in Appendix 2). This implies that the reform reduced the premium by 14.5% and 11.8% as a result of a reduction in credits of 20% and 14% in economics and business, respectively. However, in the absence of a causal estimate of the return to college for this setup, I cannot decompose this return.

5. Mechanism

When and how do employers find out about these graduates' lower human capital? 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 collected by the economics department at Los Andes, on the employers of all economics graduates by cohort, which I use to investigate whether employers changed with the reform. Table A3.3 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 courses Monetary Policy, Public Finance, and Colombian Economic Policy were no longer mandatory. Indeed, Fig. A3.6 shows that there was a decline in the number of students enrolled in these classes after the reform. From this

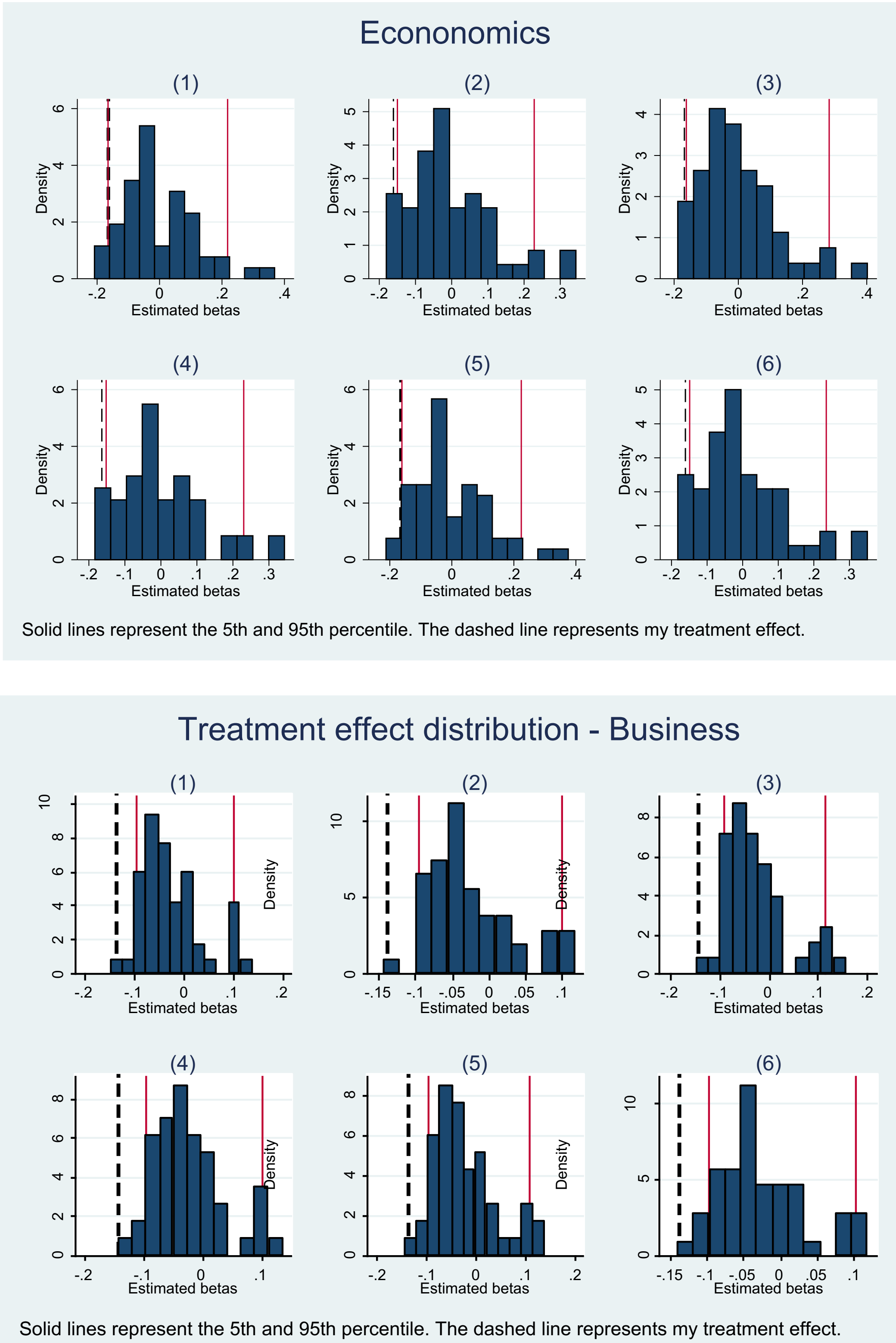

Fig. 5. Treatment effect distribution (Table 3a and b).

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 employers to learn about their experiences with hiring economics graduates, and as anecdotal evidence I learned that[11]: (i) most knew about the reform from talking to recent graduates; (ii) they believe they can detect changes in human capital through tests they

[11] I conducted interviews with 11 out of the 14 employers listed on the left panel of Table A2.3.

Table 5
Effect of the reform on wages. Alternative treatment group: students already in school by the time of the reform.
Source: Ministry of Education.

Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Economics						
Post*Andes	− 0.161***	− 0.159***	− 0.165***	− 0.161***	− 0.161***	− 0.159***
	[0.0534]	[0.0536]	[0.0538]	[0.0540]	[0.0537]	[0.0539]
Post	0.0737**	0.0726**	0.0669**	0.0681**	0.0713**	0.0768**
	[0.0288]	[0.0289]	[0.0308]	[0.0309]	[0.0344]	[0.0343]
Andes	0.312***	0.300***	0.312***	0.300***	0.311***	0.298***
	[0.0304]	[0.0301]	[0.0304]	[0.0301]	[0.0304]	[0.0301]
Obs	3485	3485	3485	3485	3485	3485
Panel B: Business						
Post*Andes	− 0.118***	− 0.118***	− 0.124***	− 0.124***	− 0.117***	− 0.118***
	[0.0420]	[0.0422]	[0.0172]	[0.0423]	[0.0421]	[0.0424]
Post	0.0925***	0.0913***	0.0525**	0.0527***	0.0933***	0.0955***
	[0.0157]	[0.0156]	[0.0191]	[0.0185]	[0.0191]	[0.0190]
Andes	0.429***	0.424***	0.433***	0.427***	0.430***	0.424***
	[0.0312]	[0.0315]	[0.0811]	[0.0316]	[0.0312]	[0.0315]
Obs.	9979	9979	9979	9979	9979	9979

Standard errors clustered at the student 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 refers 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.
* $p < 0.1$.
** $p < 0.05$.
*** $p < .01$.

Table 6
Cap at three years of experience.
Source: Ministry of Education.

Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Economics						
Post*Andes	− 0.166***	− 0.164***	− 0.170***	− 0.167***	− 0.166***	− 0.164***
	[0.0484]	[0.0486]	[0.0487]	[0.0488]	[0.0485]	[0.0487]
Post	0.0846***	0.0835***	0.0751**	0.0752**	0.0829**	0.0858**
	[0.0281]	[0.0281]	[0.0304]	[0.0304]	[0.0343]	[0.0343]
Andes	0.314***	0.305***	0.313***	0.304***	0.313***	0.304***
	[0.0283]	[0.0282]	[0.0283]	[0.0282]	[0.0283]	[0.0282]
Obs	3,314	3,314	3,314	3,314	3,314	3,314
Panel B: Business						
Post*Andes	− 0.129***	− 0.128***	− 0.134***	− 0.132***	− 0.129***	− 0.128***
	[0.0402]	[0.0403]	[0.0403]	[0.0404]	[0.0403]	[0.0404]
Post	0.0953***	0.0951***	0.0600***	0.0609***	0.101***	0.104***
	[0.0152]	[0.0151]	[0.0184]	[0.0183]	[0.0187]	[0.0187]
Andes	0.422***	0.415***	0.425***	0.419***	0.421***	0.415***
	[0.0300]	[0.0302]	[0.0300]	[0.0302]	[0.0300]	[0.0303]
Obs	9,627	9,627	9,627	9,627	9,627	9,627

Standard errors clustered at the student 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 refers 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.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

administered in the recruitment process; (iii) they argue that for some jobs, the content made optional in the new curriculum is critical; (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 provides suggestive

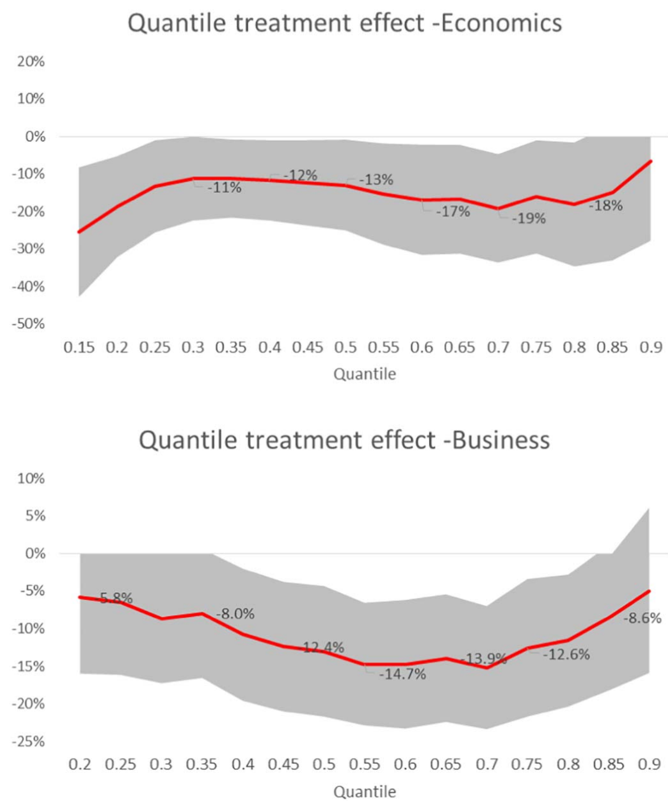

Fig. 6. Changes in changes estimates.
Source: Ministry of Education. CIC estimates of an estimation that controls for experience, gender, and cohort variables. Confidence intervals at the 95th percent level.
Test—Economics: Constant effect: QTE(x) = QTE(0.5) for all x; KS-statistic: 0.61; CMS-statistic: 0.47. Test—Business: Constant effect: QTE(x) = QTE(0.5) for all x; KS-statistic: 0.374; CMS-statistic: 0.276.

Table 7
Effect of the reform on the recruitment process.
Source: Central Bank of Colombia.

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 in parenthesis.
Data from the recruitment process for economist positions from 2008 to 2014.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

evidence that under the new curricula, 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 did not 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.

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, 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 terms for all candidates for economist positions from 2008 to 2014, along with the final hiring decision. For candidates who studied under the old curriculum, the probability of being hired was 27%; this fell to 6% with the reform. Table 7 shows the results of the DID exercise. According to data, after the reform there is a reduction of 16.7 percentage points 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 result, the pool of offers a student could choose from was smaller, and students started in lower-paying jobs.

The previous mechanism points to an environment in which employer learning happens rapidly due to the availability of tests on specific content used in the recruitment process. Thus, one would expect that employers notice the reduction in instruction at Los Andes soon after the first students enter the job market, which is what happened in economics (Fig. 4). For business, however, if the recruitment process relies less on testing specific knowledge, we would not expect to see this pattern. Interviews with recruitment agencies suggest that this is the case, since a large share of the openings for recent graduates in business are also available to graduates from economics and engineering, and thus tests on content are less appropriate. As a consequence, the qualitative evidence suggests that it might take longer for employers of business students to notice the differences in human capital of the new cohorts.

6. Robustness checks

In this section I perform several robustness checks to address possible confounding factors in my estimation. I then discuss some important caveats and limitations.

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 include only cohorts that studied under the old curricula, 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 8, 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 9 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 other trends or changes at Los Andes.

Table 10 presents a series of additional robustness checks. The first

Table 8
Placebo test 1—Alternative date of the reform.
Source: Ministry of Education.

Dep var: Ln wage	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Economics						
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 student 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.
*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.

Table 9
Placebo test 2—Reform evaluated using data from law graduates.
Source: Ministry of Education.

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	3388	3388	3388	3388	3388	3388
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 refers 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.
*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.

two columns show results for economics and the last two for business; columns 1 and 3 estimate Eq. 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 10 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 10, I exclude these two cohorts and perform my baseline estimation; results show that the effects hold, even with the exclusion.

Table 10
Robustness checks.
Source: Ministry of Education.

	Economics	Economics	Business	Business
Dep variable: Ln wage	(1)	(2)	(3)	(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 errors in brackets below the coefficients.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

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 10 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 the concern about the validity of the control group is to include only students who had the academic credentials required to attend Los Andes in the control group. Specifically, I include students who attended Top 10 schools and had Saber 11 scores greater than the minimum per cohort observed at Los Andes for economics and business. *Panel d* of Table 10 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 10 repeats the baseline estimation, excluding cohort 2007–1; as shown in Fig. 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 11 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 the effect ranging from − 5% to − 20%.

Finally, in the previous analysis I assumed that the reform did not have an effect on labor force participation. To check that this is the case, I estimate the effect of the reform on the probability of being employed in the formal sector. Table A3.3 shows the results of a DID regression on the probability of being employed. For both economics and business the effects is very close to zero and statistically insignificant.[12] In addition, 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

[12] Saavedra (2008) finds a positive effect of attending Los Andes on employment (p. 22, Table 8). The first difference of the regression on Table A3.3 supports this finding.

Table 11
Synthetic control.
Source: Ministry of Education.

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 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.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

studied under the new one. Fig. A3.4 shows that the percentage of graduates found on LinkedIn—around 60%—is similar to the rates before and after the reform. Fig. A3.5 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.

7. 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 amount of coursework required to earn a college degree in economics and business decreased by 20% and 14%, respectively. This was accomplished by dropping 12 courses in economics and 6 in business, and a reduction in instruction time of one semester. The reform did not alter the quality of the entering or graduating classes or the school ranking. Because wages should fall under the human capital model—but remain constant under pure 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 13% in business. Given the size and statistical significance of the 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, also using data on Colombia, MacLeod et al., 2017 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 the 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 curricula, 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.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2017.10.007.

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