Signaling on the Labor Market: Evidence from College Scorecards

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Abstract

Individuals often select which university they will attend based on the institution's reputation. They do so to signal to employers their abilities, which may affect their hireability and wages. We study these signaling effects by exploiting a change in Ecuador's governmental university ranking system that lowered employers' perceptions of the average quality of graduates of seven of Ecuador's 11 highest-ranked universities. Using proprietary data matching college graduation, employment, wages, and graduate degrees, we show that the effect of the ranking change matches all the theoretical predictions of the effect of the signal of graduating from a particular university. The ranking change decreased salaried employment and wages for inexperienced individuals who had newly entered the labor market and whose universities decreased in ranking. This effect fades over time as individuals reveal their productivity. These effects are driven by employers more likely to rely on signaling to infer employee productivity.

JEL codes: I23, I26, J20, J21, J23

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1 Introduction

In many market interactions, some participants do not have complete information about the other party, which is a crucial component of making informed and efficient decisions. In these settings with asymmetric information, theory shows that signals play a role in screening individuals and commodities to convey part of the missing information. This process is particularly relevant in labor markets where individuals have private information about their productivity. Employers require this information to set wages efficiently, so individuals have an incentive to find ways to signal their productivity to obtain a wage offer adequate to their abilities (Spence, 1973). In this setting, theory indicates that graduating from a particular university may convey information about an individual's capabilities to the market because of the institution's admission procedures and because individuals will self-select towards institutions suitable for their ability level (Stiglitz, 1975; Hershbein, 2013; MacLeod and Urquiola, 2015). More productive individuals will apply and attend more selective universities (Stiglitz, 1975). An employer can observe what university an individual attended and use that information to infer the individual's productivity. The employer can use this information when making hiring decisions and setting wages. However, testing if labor markets respond to this signal is challenging. High-ability individuals, who may have graduated from more rigorous colleges, may also have accumulated more human capital in college, making them more productive and increasing their earnings (Becker, 1964). Thus, the signal and additional human capital could drive the wage premium. Finding conditions where it is possible to untangle either effect is not easy.

In this paper, we study how the signal of graduating from a specific university affects labor outcomes by exploiting an exogenous change in the information content of this signal. If the signal of graduating from a particular university matters, then new information on the university's average quality should affect labor market outcomes. The change in the signal content of graduating from a university comes from a change in college ranking in Ecuador. In Ecuador, university rankings did not exist before 2009. That year, the government implemented a scorecard evaluating university infrastructure, research output, academic services, and management. Thus, this scorecard plausibly conveyed new information about each university's average quality of individuals entering the labor market. Universities were classified into five divisions: A, B, C, D, and E. In 2013, the second time the ranking was calculated, the government made an unanticipated change in the ranking methodology, dropping seven out of the eleven highest-ranked universities from Division A to Division B. No new universities achieved the highest ranking. We show no differences in the ranking dimensions related to human capital accumulation between those Division A universities that decreased in ranking and those that did not. Thus, for Division A universities, a lower ranking in 2013 was determined by the methodological changes that affected other categories unrelated to human capital accumulation. Since the ranking changed because of a methodological adjustment, information should be what is driving the effect on the labor market, not actual human capital.

We estimate the effect of the 2013 ranking change on the probability of salaried employment and monthly wages. We apply a difference-in-differences design to proprietary individual-level panel data from Ecuador. We compare individuals whose universities maintained the highest ranking in 2013 to individuals whose universities decreased their ranking from Division A to Division B in that year. The identification strategy requires that for universities that obtained the highest ranking in 2009, the unanticipated 2013 ranking change did not affect the composition of entering or graduating students. We restrict the sample to cohorts enrolled in college before 2013 that were expected to graduate after 2013. Within these cohorts, there are no composition changes between affected and unaffected universities.

The results are consistent with the predictions of an employer learning model where employers use signals to infer the productivity of individuals entering the labor market and adjust this expectation over time (Farber and Gibbons, 1996; Arcidiacono et al., 2010).[1] The ranking change decreased the probability of salaried employment by 3.9 percentage points (16.7 percent of the baseline) for individuals who graduated from the affected universities when they entered the labor market. This result suggests that the university quality from which an individual graduates is an important signal when entering the labor market for the first time. Consistent with employer learning, this effect faded over time as individuals provided additional signals on their productivity through postgraduate education and non-salaried employment.

To test for unobserved confounding shocks, we run a placebo check suggested by employer learning models. Individuals from older cohorts, who already have work experience, should not be affected by the ranking change. Consistent with our expectations, we find no significant effect from the ranking change for those individuals with work experience. The results are robust to several additional checks, including controlling for age, individual fixed effects, and diverging trends (Rambachan and Roth, 2020).

We find similar results in terms of monthly wages. The ranking change decreased the wages of the affected individuals in the short term, and this effect faded over time. However, the effect on salaried employment creates a sample selection problem when we try to isolate its effect on wages. The ranking change determines the individuals for whom we can observe wages. We bound the estimates to account for this issue, and the bounds display the same

[1]The model developed by MacLeod et al. (2017) yields similar predictions.

pattern as the point estimates. Nevertheless, for robustness, we focus on the probability of salaried employment for the rest of the analyses.

Having shown that the ranking change affected salaried employment participation and wages, we provide additional evidence supporting signaling as the main channel for the estimated effects. First, we present evidence suggesting that the 2013 ranking change was not caused by changes made by the universities that may have affected human capital accumulation. We test this hypothesis in three dimensions that the 2013 methodology weighted heavily: the faculty to administrative positions ratio, faculty with graduate studies, and academic publications. Finding differences in these measures could imply differences in faculty quality that may affect human capital accumulation (Kokkelenberg et al., 2008; Hoffmann and Oreopoulos, 2009; Bandiera et al., 2010). We do not find any difference in these measures between universities that maintained and decreased in ranking in 2013, suggesting that these universities did not implement changes that may have affected human capital accumulation and resulted in the ranking change.

Second, we exploit differences in the hiring between public and private employers to show that employers with less sophisticated hiring processes are the ones that react to the ranking change. Theory suggests that signals should not affect hiring and wage setting if the hiring process includes tests or other opportunities for an individual to reveal their productivity. In Ecuador, public sector hiring satisfies these conditions. Ecuadorian law allows two types of hiring processes in the public sector. The first type requires candidates to pass ability and knowledge tests. The second type allows hiring targeted individuals irrespective of their qualifications. Signals should not affect both types of hiring processes. On the other hand, private-sector hiring is more heterogeneous than public-sector hiring, and the complexity of the hiring process varies by company. Large companies may have sophisticated processes, while smaller companies often have very informal processes and may rely on signals more than larger companies (Barber et al., 1999; Greenidge et al., 2012). We find that private-sector employers entirely drive the effect of the 2013 ranking change. Estimates for public-sector employers are small and statistically insignificant. These results suggest that the 2013 ranking changes signaled new information to the labor market that affected employers' decisions.

Finally, we provide some evidence on the mechanisms individuals whose universities decreased in ranking may have used to signal their productivity after the ranking evidence. We find that some individuals pursued graduate studies to respond to their undergraduate ranking decrease. The effect on the probability of having a graduate degree follows the inverse pattern of the effect on the probability of salaried employment and wages for the affected individuals. A ranking drop increased the probability of an individual obtaining a graduate degree in the short term. However, in the long-term, the effect significantly lessens and we observe that it converges towards zero. This pattern suggests that some individuals who received lower wage offers because of the ranking change decided to get an additional degree to signal their productivity.

Previous research has focused on the effects of different signals. One branch of the literature studies the signaling effect of obtaining a degree or an additional year of education. These studies focus on signaling at the primary and secondary education levels with mixed results (Tyler et al., 2000; Clark and Martorell, 2014; Eble and Hu, 2019). Another branch of the literature studies if a signaling model explains observable behavior. Bedard (2001) finds that high school dropout rates are lower in locations where university access is constrained. This result is consistent with a signaling model where individuals who would otherwise be high school dropouts choose to complete high school because high-ability individuals cannot attend college, increasing the signal value of graduating from high school. Hershbein (2013) finds that the return of GPA is lower when the student attends a more selective college, which is also consistent with signaling. MacLeod et al. (2017) find that when students share results of an exit exam on their curricula vitae, the effect of college reputation on wages decreases. The authors use the average admission score of each university's graduates to measure college reputation. They find that the effect of this measure of reputation on wages increases with experience, suggesting that mean admission scores do not capture only signaling but may include other factors such as peer quality. Eble and Hu (2022) tackle the signaling effect of attending or graduating from a particular university by exploring the effect of a university's name change. The authors find that a name change attracts higher-quality students to the university but does not affect employers' hiring decisions. Similarly, Acton (2022) shows that colleges that rebrand themselves as universities in the United States increase enrollment. Finally, Sekhri (2020) shows that attending elite colleges in India does not affect exit exam grades but increases wages.

This paper contributes to our understanding of signaling by testing the predictions of signaling models directly for the signal of graduating from a particular university. In contrast with previous research, we can identify the dynamics behind the signaling effect of graduating from a university rather than testing if some observable behavior is consistent with signaling because we study a pure information shock to the perceived quality of a university. This is an advantage of looking at our specific data from Ecuador. First, we have access to data matching college graduation to employment, income, and graduate degrees. Second, university rankings did not exist in Ecuador prior to 2008 when the government developed the first ranking system. The new information on university quality comes from a source employers trust and leads them to adjust their behavior accordingly. The effect of the ranking change matches all the theoretical predictions of the effect of the signal of graduating from a particular university on hiring and wages (Stiglitz, 1975; Farber and Gibbons, 1996; Arcidiacono et al., 2010; Hershbein, 2013; MacLeod and Urquiola, 2015; MacLeod et al., 2017). We find that the short-term effects are sizable. The effect on the probability of salaried employment represents 16.7 percent of the study sample's salaried employment rate in 2013. While less precise, the effect on monthly wages is similar to the returns to one additional year of education, as documented by Card (1999, 2001). These magnitudes suggest that signaling plays an important role in determining employment and wages for individuals entering the labor market. We also provide evidence supporting the theoretical prediction that inexperienced individuals look for ways to signal their ability by showing that some obtained a graduate degree in response to the ranking change.

Our study also contributes to understanding the mechanisms behind college choice and the returns of college quality. Hoxby (2009) documents that students' choices regarding which college to attend are driven more by a college's resources and student body composition than the distance to home.[2] Eble and Hu (2022) find that students enroll in colleges they perceive to be of higher quality. On the employment and earnings side, there is evidence that a university's level of selectiveness may affect a student's post-graduation hireability and lifetime earnings (Dale and Krueger, 2002; Hoekstra, 2009; Hastings et al., 2013; Dale and Krueger, 2014; Darolia et al., 2015; Deming et al., 2016; Ge et al., 2022). Our results suggest that signaling is one of the potential mechanisms behind both results. We show how a pure information shock that decreased the perceived quality of some universities decreased employment and wages, suggesting that it is rational for students to consider college reputation and that this perceived reputation influences employers' actions.

Finally, this paper contributes to understanding government scorecards' effects on reputational markets. These scorecards aim to promote more efficient use of resources and to give agents information to bring about welfare-increasing choices. For example, there is evidence that school scorecards improve parent's school choices for their children, children's educational outcomes, and have an incremental, albeit short-lived, effects on housing prices (Black, 1999; Figlio and Lucas, 2004; Bayer et al., 2007; Ries and Somerville, 2010; Fiva and Kirkebøen, 2011; Andrabi et al., 2017); health insurance score cards affect health insurance choices (Dafny and Dranove, 2008; Jin and Sorensen, 2006; Chernew et al., 2008); nursing home scorecards affect demand for the lowest-ranked institutions (Feng Lu, 2012); and restaurant health scorecards have mixed effects on earnings for the highest-ranked establishments (Jin and Leslie, 2003; Ho et al., 2019). Our study shows that employers respond to a governmental college scorecard by altering their hiring decisions. Depending on the score-

[2]See also Hastings and Weinstein (2008)

card's quality, this information can attenuate or exacerbate existing information asymmetries increasing or decreasing welfare.

The rest of the paper is organized as follows. Section 2 discusses university rankings in Ecuador. Section 3 briefly discusses the main theoretical results through which the 2013 ranking change might affect labor market outcomes. Section 4 describes the data and the empirical strategy. Section 5 presents the results and robustness checks. Section 6 presents evidence supporting that the mechanism behind the estimated effects of the 2013 ranking change is a signaling effect. Section 7 concludes.

2 College Rankings in Ecuador

Prior to 2008, the Ecuadorian government did not control the quality of its universities. In that year, in response to a series of scandals involving some universities selling degrees, Constitutional Mandate No. 14 established a one-year term for the *Consejo Nacional de Evaluación y Acreditación* (CONEA) to prepare a technical report on the performance of all universities and colleges in order to guarantee their quality and encourage their improvement (Long et al., 2013).

The CONEA 2009 report reviewed 68 universities. This evaluation included four criteria: i) academic, ii) student and learning environment, iii) research and, iv) internal management. The report did not include information on student characteristics or outcomes. Each category had a specific weight and was evaluated through different indicators (Table 1). The academic section, which assessed teaching quality, had the highest weight, 41 percent. This report introduced, for the first time in Ecuador, a university ranking. This ranking created five different divisions: A, B, C, D, E. Each university was assigned to one of these divisions according to their total score in the report (CONEA, 2009; CACES, 2019).

Division A included the 11 universities with the highest scores. Overall, these institutions had faculty of scientific and professional quality and demonstrated interest in offering a good learning environment through functional infrastructure, research encouragement, scholarship policies, and structured management. Division B included nine universities with lower performance in the research and academic categories. Division C had 13 universities with mixed results and barely above average scores. There were nine institutions in Division D that exhibited large deficits in different indicators and showed faculty instability. Finally, Division E included 26 colleges that did not meet the necessary conditions required for a university (CONEA, 2009).[3]

[3] A special evaluation process was created for institutions in Division E. By 2012, three institutions had "acceptable" evaluations and eight institutions "partially acceptable" evaluations. These institutions were ranked in Division D. The rest of Division E institutions were closed.

Table 1: Criteria Considered in University Rankings

2009 Ranking				2013 Ranking			
Criteria	Criteria Weight	Indicator	Indicator Weight	Criteria	Criteria Weight	Indicator	Indicator Weight
		Faculty with graduate studies	13.2%			Faculty with graduate studies	16.0%
		Faculty teaching hours	6.3%			Faculty teaching hours	2.6%
Academic	41.0%	Faculty composition	3.4%	Academic	40.1%	Faculty composition	8.6%
		Faculty salary scale	14.3%			Faculty salary scale	11.3%
		Community outreach programs	3.9%				
						Proportion of women on staff	1.6%
		Graduation rate and time to graduation	1.8%			Graduation rate and time to graduation	4.5%
		Admission rate	5.3%	Academic Efficiency	10.0%	Admission rate	3.0%
						Retention rate	2.5%
		Student regulations	10.6%				
		Library	4.3%			Library	4.2%
Students and Environment	35.4%	Laboratories	10.7%				
		Internet connection and innovation	2.7%			Internet connection and innovation	2.8%
				Infraestructure	20.0%	Student spaces	6.0%
						Faculty spaces	4.0%
						Wellness spaces	3.0%
		Internal research regulations	7.3%			Internal research regulations	3.0%
		Research projects in progress	1.1%			Research projects in progress	9.0%
Research	15.0%	Publications by ranking	2.0%	Research	20.0%	Publications by ranking	8.0%
		Faculty and student engagement in research	0.7%				
		Research results (unpublished)	3.9%				
		Follow-up to graduates	1.3%			Follow-up to graduates	0.8%
		Financial results	1.9%			Financial results	3.8%
		Affirmative actions	1.3%			Affirmative actions	1.7%
		Information availability	0.6%			Information availability	1.4%
		Faculty spaces	0.6%				
Management	8.6%	*Wellness spaces*	0.6%	Management	9.9%		
		Infrastructure accesibility	0.9%				
		Administrative staff	1.4%				
						Community outreach programs	1.2%
						Academic system	1%

Notes: This table presents the criteria, indicator categories, and the weights used to rank universities in 2009 and 2013. Indicators in italics were present in both rankings but within different criteria.

In October 2010, the National Assembly issued a new law to regulate post-secondary education. This law created three regulatory agencies mandated to implement a second university evaluation in 2013. In November 2013, the *Consejo de Evaluación, Acreditación y Aseguramiento de Calidad de la Educación Superior* (CEAASES) issued a new report with the findings of the second evaluation. This report ranked 54 institutions in 4 divisions: A, B, C, and D. Seven out of the original 11 universities ranked A in 2009 were demoted to a B ranking. No new universities achieved the highest ranking. An important feature in the 2013 evaluation was that it unexpectedly changed the evaluation criteria, indicators, and weights from the 2009 report. Table 1 summarizes the methodological changes between the two evaluations. This unexpected change is the basis for the identification strategy detailed below.

After the 2013 report, universities could apply for a voluntary evaluation and receive a new ranking. The first voluntary evaluation occurred in 2015– 2016, and 13 institutions participated. No university in that group was given an A status. In the second voluntary evaluation, that took place in 2016–2017, all universities ranked in Division D improved their scores and were assigned to higher divisions. This was the last evaluation that ranked universities in divisions. A new law, issued in 2018, established that the evaluation process would be executed for accreditation purposes only and not to rank universities.

Two factors drive ranking changes between 2009 and 2013. First, universities had an incentive to implement changes to maintain or improve their rankings. The criteria and parameters evaluated in 2009 guided all changes. Melendez (2015) documents how lower-ranked universities hired more full-time professors and researchers with better qualifications and offered current instructors assistance to continue their education. This suggests that lower-ranked universities made changes that may have improved human capital accumulation. Hence, these universities cannot be used to identify signaling effects. On the other hand, while the highest-ranked universities in the country (Division A universities) may have also implemented changes, in Section 6 we show no difference in faculty composition, hiring, and publications between those Division A universities that decreased in ranking and those that did not.

Second, methodology changes also caused ranking changes between 2009 and 2013 (see Table 1). Easy-to-fulfill indicators, like a university having student regulations, were not considered in 2013. Other variables, like community outreach programs, had a lower weight in 2013. Categories related to inputs in the human capital production function, like faculty with graduate studies, faculty composition, and publications, had a higher weight in 2013. We show in Section 6 that there are no differences in faculty with graduate studies, faculty composition, and publications between those Division A universities that decreased in ranking and those that did not. Thus, for Division A universities, a lower ranking in 2013 was determined by the methodological changes that affected other categories unrelated to human capital accumulation. This suggests that the 2013 ranking does not reflect changes in human capital but plausibly changed employers' perceptions about the average quality of the graduates from Division A universities that decreased in ranking, affecting the signal of graduating from one of these universities. We focus on these universities for the empirical analysis in a difference-in-differences setting.

3 Theoretical Discussion

From a theoretical perspective, when workers enter the labor market, their education and other characteristics signal only partial information about their productivity to employers (Spence, 1973; Jovanovic, 1979). If the 2013 ranking change conveyed new information to the market about employee quality and skills, employers might react by adjusting hiring decisions to recognize the signal's change. The 2013 reduction of the ranking for seven out of the 11 Division A universities could signal that graduates from the affected universities had lower skills and productivity than expected. In this case, we would observe a decrease in wages for graduates from these universities and potentially an extensive margin response, reducing the probability of salaried employment after the rankings' release.

We can formalize these results with the standard employer learning model developed first by Farber and Gibbons (1996). Here, we closely follow the version formulated by Arcidiacono et al. (2010). The productivity of worker i with t periods of labor experience is

$$\chi_{it} = f(s_i) + \lambda_t(q_i + z_i) + H(t) \tag{1}$$

where $f(s_i)$ is the effect of education on productivity, q_i represents information about the worker's ability observed by the employer, z_i is a measure of ability not observed by the employer, and $H(t)$ captures the effect of experience on productivity. Workers graduate from different universities, and employers observe the average ability of the worker's *alma mater* $\bar{z} = E(z_i|s_i, t, university)$. This implies that $z = \bar{z} + e_i$, where e_i is an error term. We maintain the standard assumption in employer learning models that all employers have access to the same information (Farber and Gibbons, 1996). Under this assumption, wages offered to worker i with t periods of experience are equal to their expected productivity:

$$W_t = E_t(\chi_{it}|\bar{z}, q_i, s_i) \tag{2}$$

When an individual enters the labor market, their initial productivity is

$$\begin{aligned} \chi_{i0} &= f(s_i) + \lambda_0(q_i + \bar{z} + e_i) + H(0) \\ &= E_0(\chi|\bar{z}, q_i, s_i) + \lambda_0(e_i) \end{aligned} \tag{3}$$

This result implies that the 2013 ranking change may have decreased $\bar{z}$ for individuals whose universities decreased in ranking. Consequently, expected productivity would have decreased, and employers would have offered a lower wage. For some individuals, $W_0 < 0$, implying that the individual would not have entered salaried employment.[4]

Over time employers observe work performance and learn more about a worker's productivity, decreasing the error $\lambda_0(e_i)$. Also, individuals may pursue further education to increase $E_t(\chi|\bar{z}, q_i, s_i)$.[5] Let $\mu_o = \bar{z}$ be the initial belief about unobserved ability. Every period, employers receive a signal y_t and update their belief μ_t with an optimal Bayesian weight θ_t:

$$\mu_t = (1 - \theta_t)\mu_{t-1} + \theta_t y_t \tag{4}$$

As employers learn more about a worker's productivity, $(1 - \theta_t)\mu_{t-1} + \theta_t y_t$ converges to z and the error $\lambda_0(e_i)$ converges to 0. Thus, if individuals whose universities were affected by the ranking change on average have the same ability as individuals whose universities were not affected, the effect of the ranking change should fade away over time.

In summary, the model suggests that the most experienced workers will be the least affected by a ranking change while the least experienced workers will be most affected. This provides a helpful placebo check: labor market outcomes of more experienced workers should not be affected. Also, if the parallel trends assumption holds, the effect should fade over time for the affected individuals as they reveal their productivity through other signals. Finally, the model suggests that college quality information should only matter in hiring processes where candidates do not have opportunities to reveal their productivity z. It is important to note that the impact of the 2013 ranking change depends on the strength of market-based

[4]From a different perspective, Jovanovic (1979) presents a model that explains job separations (quitting/getting fired). The author shows that under the assumption that imperfect information exists for both the employer and employee, it is optimal to quit or get fired when new information about the match's quality becomes available. Information about the match's quality is accumulated with tenure. Thus, the model predicts that the probability of separation is negatively correlated to tenure. In this context, if an exogenous source conveys new information about the match's quality, this may be reflected in a change in the probability of salaried employment. In the context of Ecuador's university rankings, the 2013 change would reveal that the quality of graduates from the affected universities was lower than expected. In this case, we would observe a decrease in the probability of salaried employment after 2013.

[5]Furthermore, in developing countries there are ample opportunities for non-salaried employment, so even individuals who did not enter salaried employment can send new signals. In December 2019, almost 27 percent of college graduates work as independent professionals according to Ecuador's Employment Survey

mechanisms for learning about quality (Dafny and Dranove, 2008). If market-based sources are reliable and complete, or employers do not trust the rankings, these governmental reports should not affect labor market outcomes.

4 Empirical Approach

This section details the data we used in the analysis and the strategy we employed to estimate the causal effects of the 2013 change in Ecuador's college rankings on labor market outcomes.

4.1 Data

We obtained access to proprietary data from a financial services company in Ecuador. This company collects comprehensive demographic data on the country's adult population. The company's sources include banks, other financial institutions, and web scraping from governmental webpages to fill gaps. They merge data from these sources using national identification numbers. They released to us an anonymized dataset and authorized us to share it for replication purposes. Specifically, we accessed a panel of 377,146 individuals whose universities obtained a Division A ranking in 2009. The panel contains annual information on salaried employment, monthly salaries (conditional on employment), post-secondary education, and demographic characteristics between 2011 and 2019.

Information on post-secondary education was scraped from the web page of the *Secretaría de Educación Superior, Ciencia, Tecnología e Innovación* (SENESCYT).[6] These data include the university's name, university's country, mode of study, and study field. An individual can have several degrees registered that correspond to different levels of post-secondary education, including graduate degrees. The demographic data include sex, year of birth, province of birth and residence, canton of birth and residence, and marital status.[7] For labor outcomes, the data include whether the person worked as a salaried employee and, if so, the monthly salary he received. Labor data are measured in the second quarter of each year.

We made the following adjustments to the data to match the salary distribution reported in Ecuador's employment surveys for 2011–2019. We exclude observations with salaries greater than $100,000 and less than a quarter of the monthly minimum wage. Employment

[6]SENESCYT is a government institution in which Ecuadorians are required to register their academic degrees. If a degree is not registered with SENESCYT, then it is not valid in Ecuador. This includes degrees granted by foreign universities.

[7]A canton is a political and administrative subdivision in Ecuador similar to U.S. counties.

surveys suggest that no salaried employee earns more than \$100,000, suggesting that individuals earning more than that threshold in our data work in upper managerial positions. Also, earning less than a quarter of the minimum wage would imply working less than two hours per day. Employment surveys suggest that no salaried employee works less than two hours, which is consistent with labor laws prohibiting hourly hiring in Ecuador. We also excluded individuals older than 70 years old in 2011 and individuals under 18 years old in 2019. These individuals were not participating in the labor market in 2011–2019. These restrictions leave a final balanced panel of 373,297 individuals. All results are practically identical if we lift these restrictions.[8]

4.2 Empirical Strategy

To isolate the effect of the information shock created by the 2013 change in the college rankings, we focus on individuals whose universities were ranked in Division A in 2009. Drops in ranking for these universities can be attributed to the change in the ranking methodology in 2013. Lower ranked universities in 2009 had an incentive to improve their quality, so for these universities, the 2013 ranking change includes potential improvements that could have confounded the effect of the information change.

We use a 2×2 difference-in-differences design to estimate the effect of the 2013 ranking change for individuals whose universities were ranked as Division A universities in 2009. We compare individuals whose universities maintained the highest ranking in 2013 to individuals whose universities decreased in ranking in 2013. Specifically, for individual i in year t, we estimate

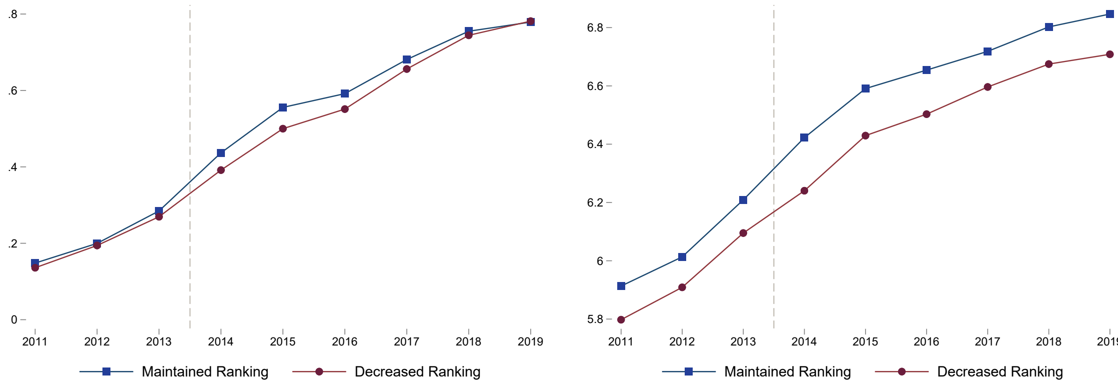
$$y_{it} = \alpha + \beta Change_i + \delta Post2013_t + \theta Change_i \times Post2013_t + u_{it} \tag{5}$$

where y_{it} is either an indicator of whether the person works as a salaried employee or the logarithm of the monthly wage (conditional on employment). $Change_i$ indicates if the individual's college ranking decreased in 2013, and $Post2013_t$ takes the value of 1 for years after 2013. Since there is no variation in treatment timing, estimating Equation 5 by OLS yields a consistent estimate of the average treatment effect on the treated (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

The theoretical background in Section 3 suggests that information changes should affect only individuals entering the labor market. We restrict the sample to individuals born between 1988 and 1992. These individuals graduated from college between 2011 and 2015. The last cohorts in that age group, born between 1991 and 1992, graduated college when the rankings were updated. Thus, they were affected by the ranking change. As a robustness

[8]These results are available from the authors on request.

Figure 1: Labor Market Outcomes for the 1988–1992 Cohorts



(a) Probability of salaried employment

(b) Mean log monthly wage

Notes: This figure presents the evolution of the probability of salaried employments and mean log wages for individuals born in 1988–1992 whose universities obtained a Division A ranking in 2009. The figure considers the group that maintained this division in 2013 as well as the group that decreased their ranking. Monthly wages are observed conditional on being employed.

check, in some specifications, we flexibly control for age fixed effects. Also, we use a sample of older individuals born between 1984–1987 for a falsification test, as theory predicts that individuals with labor experience should not be affected by a university quality information shock.

A potential concern with this sample is if the 2009 ranking induced a composition change in individuals who attended A-rated universities (Alter and Reback, 2014). The 2009 rankings may have affected to which universities the 1991 and 1992 cohorts applied since they turned 18 in 2009. These cohorts are affected by the 2013 ranking change. If student composition changed differently between universities that maintained and changed their ranking in 2013, the estimates would be biased. Appendix Figure A.1 plots differences in the proportion of women, the proportion of individuals born in the provinces where Division A universities are most concentrated, parent's age, and parent's education. It shows that there are no significant differences in student characteristics, alleviating the concern that composition changes may have biased the results.

The identifying assumption is that changes in salaried employment (log wages) after the 2013 ranking change for individuals whose universities maintained the highest ranking are a good counterfactual for changes in salaried employment (log wages) after 2013 for individuals whose universities decreased their ranking. We provide several pieces of evidence in support of this assumption. Figure 1 displays the evolution of the probability of salaried employment and mean log wages for both groups. It shows that until 2013, both groups followed the same trends without any significant difference. We test for differences in the pre-period and check if the results are robust to diverging trends (see below). The estimates pass both tests. We also check if the results are robust to a) introducing a dummy for 2013, b) university fixed effects, and c) individual fixed effects.

Figure 1 also shows that the gap in salaried employment probability between the two groups fades away with time. This pattern would be consistent with the theoretical prediction that as individuals obtain labor experience or a graduate degree to provide a new signal, the information content of the ranking change should disappear. Given this pattern, we use Equation 5 to estimate the effect of the ranking change up to 2015, when the 1992 cohort plausibly entered the labor market. Furthermore, we estimate a dynamic difference-in-differences specification to capture the long-term patterns. For individual i in year t, we estimate

$$y_{it} = \alpha + \beta Change_i + \sum_{t=2011}^{2019} \delta_t Year_t + \sum_{t=2011}^{2019} \theta_y Change_i \times Year_t + u_{it} \tag{6}$$

where $Year_t$ is a set of year indicators and 2013 is the excluded period because the data was measured before the ranking change. Equation 6 allows for directly testing for diverging trends in the pre-period, controlling for group-specific trends as an additional robustness check, and testing the robustness of the results to diverging trends (Wolfers, 2006; Rambachan and Roth, 2020; Goodman-Bacon, 2021). We also include the same controls mentioned before as additional robustness checks.

Figure 1 suggests an additional challenge to estimate the effect on wages. If the ranking change decreased the probability of salaried employment for individuals whose universities decreased in ranking, there would be a sample selection problem caused by treatment. The ranking change would affect the individuals for whom we observe wages. To address this concern, we estimate bounds following Lee (2009). We trim from the comparison group, that is, from individuals whose universities maintained their ranking, the excess proportion of individuals with salaried employment. We use Equation 6 estimates on the probability of salaried employment to determine the excess proportion in each year. Then, we cut this proportion from the highest-earning individuals in the comparison group and reestimate Equation 6 on log wages to obtain an upper bound of the effect of the ranking change. We trim this proportion from the lowest-earning individuals to obtain a lower bound. For this reason, we focus on Equation 6 for the analysis of the effect of the ranking change on wages.

Since treatment is assigned at the university level, standard errors should be clustered accordingly. However, there are only 11 universities that received the highest ranking in 2009, seven of which were affected by the 2013 change. With such a small number of clus-

Table 2: Short-term Effects of the 2013 Ranking Change on the Probability of Salaried Employment

	(1)	(2)	(3)	(4)	(5)
Change in ranking	-0.0395	-0.0395	-0.0395	-0.0379	-0.0379
	[-0.0826,-0.0010]	[-0.0826,-0.0010]	[-0.0826,-0.0010]	[-0.0762,-0.0048]	[-0.0765,-0.0047]
	(0.0400)	(0.0400)	(0.0400)	(0.0380)	(0.0380)
Lead dummy		x			
University FE			x		
Age FE				x	x
Individual FE					x
Observations			336,725		
Individuals			67,345		

Notes: The wild-cluster bootstrap 95% confidence interval is provided in brackets and the associated p-value in parentheses. This table presents the short-term effect of the 2013 university ranking change on the probability of salaried employment for the cohorts born in 1988–1992. The sample includes the period between 2011 and 2015.

ters, asymptotic variance-covariance approximations that account for intra-cluster correlation tend to over-reject the null in hypothesis testing. We account for this issue in two ways. First, we use a wild-cluster bootstrap to calculate valid p-values and confidence intervals (Cameron et al., 2008).[9] Second, we use an exact permutation test to calculate p-values. There are 330 possible combinations of seven treated universities out of 11. We estimate the treatment effect for every possible placebo combination and calculate p-values by comparing the true t statistic to the distribution of placebo t statistics that arises from the permutation. The next section presents the results.

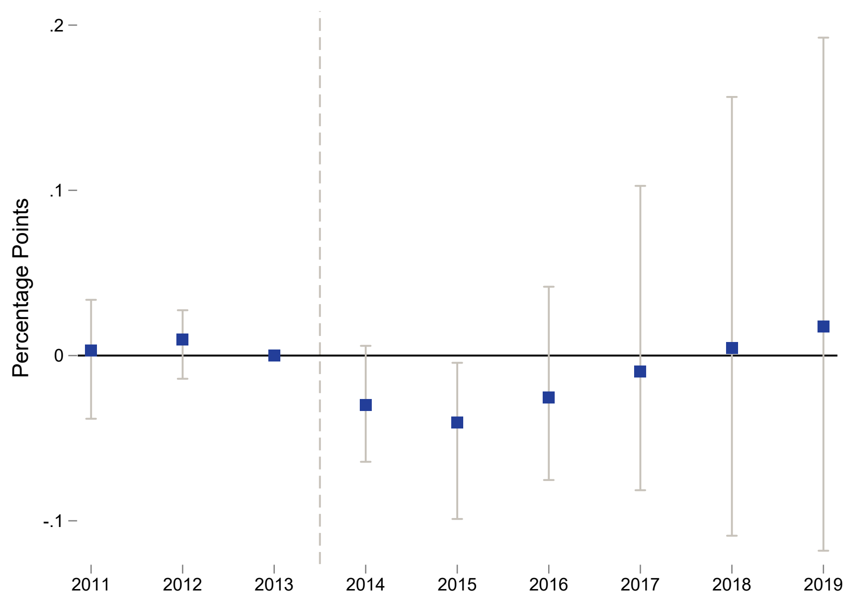
5 Results

5.1 Effect of the ranking change on salaried employment

Table 2 presents the short-term effect of the 2013 university ranking change on the probability of salaried employment. Column 1 displays the estimate of the base specification shown in Equation 5, while Columns 2—5 test the robustness of the effect to the inclusion of a series of controls. Compared to the group whose universities maintained their ranking in 2013, the ranking change decreased the probability of salaried employment by almost four percentage points for individuals who graduated from the affected universities. This effect is statistically significant at the 5 percent level (permutation p-value of 0.039) and represents

[9]We implemented the wild-cluster bootstrap in Stata using the command *boottest* developed by Roodman et al. (2019). We follow the recommendations in Canay et al. (2021) and use Rademacher weights.

Figure 2: Long-term Effects of the 2013 Ranking Change on the Probability of Salaried Employment



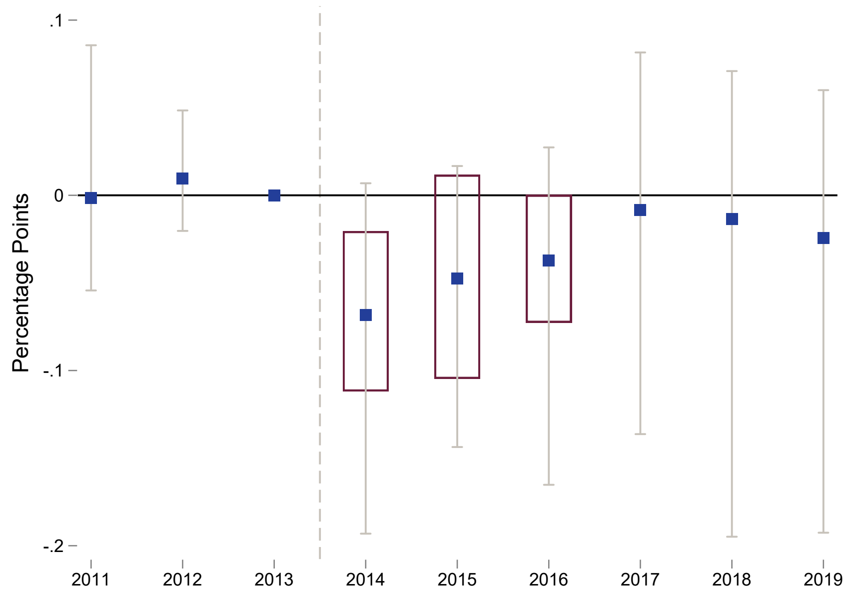
Notes: The figure displays wild-cluster bootstrap 95% confidence intervals. This figure presents the long-term evolution of the effect of 2013 university ranking change on the probability of salaried employment for the cohorts born from 1988–1992 (Equation 6).

16.7 percent of the probability of salaried employment for the 1989 cohort in 2012.[10]

As mentioned above, the theoretical discussion in Section 3 suggested that the ranking change effect should fade over time as individuals obtain experience. Figure 1 suggested that this prediction is true, and the estimates from Equation 2, plotted in Figure 2 and presented in Table 3, confirm this prediction. First, the estimates display no difference in trends between the two groups in 2011–2013 (permutation p-values of 0.882 in 2011 and 0.321 in 2012). This result supports the validity of the identification assumption. Second, in 2014 and 2015, when the 1991 and 1992 cohorts entered the labor market, the ranking change decreased the probability of salaried employment for individuals who graduated from the affected universities by three and four percentage points, respectively (permutation p-values of 0.045 in 2014 and 0.006 in 2015). These estimates are consistent with the aggregated short-term effect in Table 2, but they are slightly less precise. Finally, the effect started to fade away in 2016, and the gap closed by 2018 (permutation p-values of 0.155 in 2016, 0.745 in 2017, 0.891 in 2018, and 0.752 in 2019).

[10]The 1989 cohort turned 23 in 2012. We use the values of this cohort in this year as a baseline for comparisons.

Figure 3: Effect of the 2013 Ranking Change on Monthly Wages



Notes: The figure displays wild-cluster bootstrap 95% confidence intervals. This figure presents the long-term evolution of the effect of the 2013 university ranking change on the log of the monthly wage for the cohorts born in the years 1988–1992 (Equation 6). Boxes display bounds for the estimate in the years when the ranking change affected the probability of salaried employment. Bounds are constructed by trimming the excess proportion of individuals with salaried employment in the comparison group.

5.2 Effects of the ranking change on monthly wages

Figure 3 presents the effect of the 2013 university ranking change on monthly log wages. Again, the estimates display no difference in trends between the two groups in 2011–2013 (permutation p-values of 0.945 in 2011 and 0.388 in 2012). For the post-treatment period, the point estimates suggest a short-term effect that fades away for the period after the ranking change. The estimated bounds are consistent with this pattern. However, the point estimates are less precise than the estimates for the probability of salaried employment.

The effect on wages occurred faster than the effect on the probability of salaried employment. There was an immediate drop in wages in 2014. The 2014 point estimate suggests that wages decreased by almost seven percent for the affected individuals in that year (permutation p-value of 0.039). The effect on wages started to fade in 2015 and disappeared by 2017. In contrast, the probability of salaried employment had a smaller drop in 2014, and the largest response was in 2015.

While the effect on wages is consistent with the ranking change conveying new information to the labor market, sample selection caused by treatment is a concern that prevents us from

Table 3: Long-term Effects of the 2013 Ranking Change on the Probability of Salaried Employment

	(1)	(2)	(3)
Change in ranking 2011	0.0030		0.0018
	[-0.0359,0.0317]		[-0.0377,0.0305]
	(0.8509)		(0.9039)
Change in ranking 2012	0.0097		0.0089
	[-0.0129,0.0267]		[-0.0139,0.0250]
	(0.2853)		(0.3303)
Change in ranking 2014	-0.0300	-0.0312	-0.0292
	[-0.0663,0.0059]	[-0.0740,0.0110]	[-0.0636,0.0044]
	(0.1001)	(0.1151)	(0.0881)
Change in ranking 2015	-0.0405	-0.0403	-0.0396
	[-0.1027,-0.0004]	[-0.1377,0.0101]	[-0.1008,-0.0023]
	(0.0470)	(0.0831)	(0.0400)
Change in ranking 2016	-0.0252	-0.0235	-0.0248
	[-0.0726,0.0495]	[-0.1095,0.0648]	[-0.0749,0.0497]
	(0.2172)	(0.4344)	(0.2292)
Change in ranking 2017	-0.0097	-0.0064	-0.0101
	[-0.0838,0.1194]	[-0.1421,0.1433]	[-0.0886,0.1238]
	(0.7477)	(0.8929)	(0.7508)
Change in ranking 2018	0.0045	0.0092	0.0033
	[-0.1124,0.1791]	[-0.1771,0.2100]	[-0.1198,0.1868]
	(0.9169)	(0.8919)	(0.9479)
Change in ranking 2019	0.0177	0.0239	0.0159
	[-0.1215,0.2197]	[-0.2011,0.2635]	[-0.1300,0.2279]
	(0.7658)	(0.7758)	(0.7938)
Linear pre-trend		x	
Age FE			x
Individual FE			x
Observations		606,105	
Individuals		67,345	

Notes: The wild-cluster bootstrap 95% confidence interval is expressed in brackets and the associated p-value is provided in parentheses. This table presents the long-term effect of 2013 university ranking change on the probability of salaried employment for individuals born in 1988–1992.

drawing strong conclusions. Except for 2014, the bounds cannot reject that there is no effect. Thus, we focus on salaried employment, which is not affected by the selection problem, for further robustness checks and to explore the role of signaling as a mechanism behind the effect of the ranking change.

5.3 Robustness checks

We applied a series of robustness checks to validate the estimated effects and support the identification assumption. First, Table 2, Columns 2–5 show that the estimated short-term effect of the ranking change is robust after controlling for a 2013 dummy, university fixed effects, age-fixed effects, and individual fixed effects.

Second, Table 3 reports a series of estimates for the dynamic specification in Equation 2. Column 1 presents the baseline estimates plotted in Figure 2. These results are robust to allowing different linear trends for the two groups (Column 2). Column 3 shows that the results are robust to controlling for age fixed effects and individual fixed effects.[11]

Third, Table 4 shows the results of a placebo test that looks for effects on older individuals born between 1984 and 1987. These individuals had at least three years of experience in 2013 and should not be affected by the information provided by the ranking change. We find no significant effect for the older cohorts (permutation p-value of 0.573). The estimate's magnitude is slightly over a third of the main result in Table 2, positive and insignificant at conventional levels. This result is robust to controlling for a one-period lead dummy, university fixed effects, age-fixed effects, and individual fixed effects. Hence, the ranking change did not affect older cohorts, which is consistent with the theoretical predictions.

To further explore the robustness of the results to differential trends, we follow the inference methods proposed by Rambachan and Roth (2020) to check if the results are robust to further deviations in trends. These methods estimate robust confidence sets that check if the estimated treatment effect is robust to non-linearities in trends across groups. There is no reason to impose a priori monotonicity restrictions on potentially differential trends or the expected direction of biases in the current context. We perform a sensitivity analysis for multiple deviations from the common trends assumption up to the largest slope change observed before 2014.

Appendix Figure A.2 presents these results. Panels a and b show that the decrease in the probability of salaried employment in 2014 and 2015 is robust to deviations from parallel trends up to half of the largest slope change in the pre-period. For larger deviations, the confidence intervals are less precise but mainly include negative effects. For the later years (Panels c-f), the confidence intervals are consistent with the fading effects documented in Figure 2.

As a final check, we compare the estimated effect for both the short-term and long-term specifications with the distribution of estimates that arises from the permutation test we use to calculate randomization p-values. We examine whether the estimated effect of the actual

[11]Estimates of the effect on wages are also robust to these controls.

Table 4: Placebo Test of the 2013 Ranking Change on the Probability of Salaried Employment

	(1)	(2)	(3)	(4)	(5)
Change in ranking	0.0153	0.0153	0.0153	0.0169	0.0169
	[-0.0449,0.0607]	[-0.0449,0.0607]	[-0.0449,0.0607]	[-0.0475,0.0653]	[-0.0474,0.0643]
	(0.4765)	(0.4765)	(0.4765)	(0.4424)	(0.4444)
Lead dummy		x			
University FE			x		
Age FE				x	x
Individual FE					x
Observations			287,835		
Individuals			57,567		

Notes: The wild-cluster bootstrap 95% confidence interval is provided in brackets and the associated p-value in parentheses. This table presents the short-term effect of the 2013 university ranking change on the probability of salaried employment for the cohorts born in 1984–1987. The sample includes the period between 2011 and 2015.

ranking change is large relative to the distribution of the placebo effects estimated for the permutation. Appendix Figure A.3 presents the short-term results for the affected cohorts born between 1988 and 1992 and placebo cohorts born from 1984 to 1987. The estimate presented in Table 2 is at the left of the 2.5 percentile of the distribution for the affected cohorts. Thus, it is an outlier in the distribution, indicating a large negative effect. For the placebo cohorts, the estimate presented in Table 4 is near the middle of the distribution and close to zero.

Appendix Figure A.4 presents the long-term results for the affected cohorts. For 2011 and 2012, the effect presented in Figure 2 is close to the middle of the distribution and zero. For 2014 and 2015, the estimated effect is a clear outlier in the distribution of placebo effects, lying below the 2.5 percentile of the distribution. Finally, the estimated effects for 2016–2019, shift towards the middle of the distribution of placebo effects, consistent with the fading effect presented in Figure 2.

6 Evidence of Signaling as the Mechanism behind the Estimated Effect

We have shown that the 2013 ranking change affected the probability of salaried employment and wages. In this section, we provide evidence supporting that the mechanism behind the estimated effects of the 2013 ranking change is a change in the information content of the signal of graduating from a particular college.

6.1 Evidence of lack of differential improvements by universities

As mentioned in Section 2, the 2013 ranking change reflects both actions taken by the universities and the 2013 methodology change. We argue that Division A universities had few incentives to implement significant changes because they were recognized as the best in the country, and any change would have followed the guidelines defined by the 2009 ranking methodology. This suggests that Division A universities should not have differentially implemented changes to improve their scientific or academic quality, leading some to maintain their ranking and some to decrease in ranking. If universities that maintained their A ranking implemented measures that improved their scientific or academic quality more than universities that decreased in ranking, then the estimated effects could correspond to a change in human capital accumulation.

To argue against this concern, we exploit the fact that the 2013 methodology heavily weighted faculty indicators, promoting a higher ratio of faculty to administrative positions and rewarding having faculty with graduate studies.[12] Having more or better professors may imply better educational outcomes (Kokkelenberg et al., 2008; Hoffmann and Oreopoulos, 2009; Bandiera et al., 2010).[13] We test if there are differences in the faculty to administrative positions ratio between universities that maintained their ranking and universities whose ranking decreased in 2013. We also test for differences in the proportion of faculty with graduate studies between the two groups. One limitation of this analysis is that we only have labor market data starting in 2011. We do not have data that captures hiring behavior before the 2009 ranking, and we cannot observe changes in 2010. To better capture hiring dynamics over time, we take 2011 as the reference year in this analysis and look for changes until 2015, two years after the 2013 ranking.

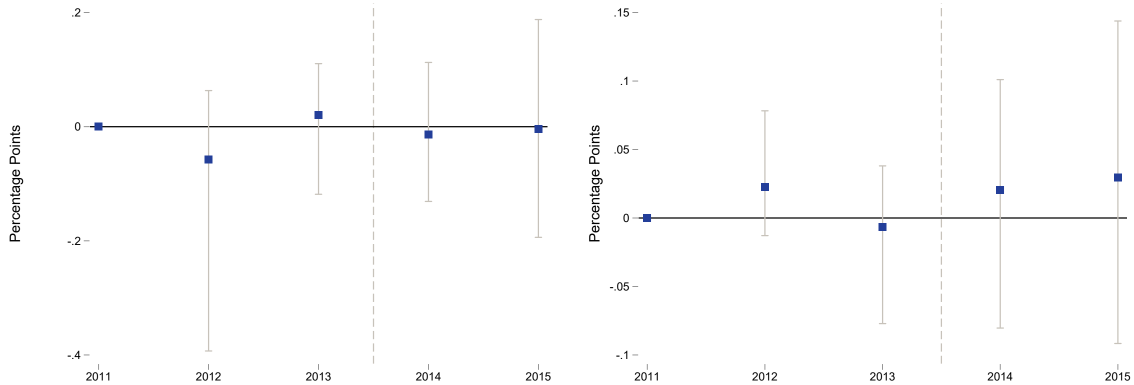
Figure 4 presents these results. We find no significant differences in the faculty to administrative positions ratio between universities that maintained their A ranking and universities that decreased their ranking in 2013 (Panel a). The point estimates are small, insignificant at conventional levels, and there is no evident pattern in the estimates that could suggest diverging trends (permutation p-values of 0.340 in 2012, 0.642 in 2013, 0.861 in 2014, and 0.970 in 2015). We find similar results when it comes to what proportion of faculty received some graduate training (Panel b, permutation p-values of 0.267 in 2012, 0.800 in 2013, 0.755 in 2014, and 0.615 in 2015).

As a second way to proxy faculty quality, we examine whether universities that main-

[12] Before 2009, it was common to have instructors who only had a college degree but had extensive professional experience.

[13] Section 4 shows no change in student composition between universities whose ranking decreased and universities that maintained their ranking.

Figure 4: Differences in Faculty between Universities that Maintained and Dropped in Ranking in 2013



(a) Faculty to administrative ratio (b) Proportion faculty with graduate studies

Notes: The figure displays wild-cluster bootstrap 95% confidence intervals. This figure presents difference-in-differences estimates in the faculty to administrative positions ratio and the proportion of faculty with graduate studies for universities that maintained their ranking and universities whose ranking decreased in 2013.

tained their ranking had more SCOPUS-indexed publications than universities that decreased their ranking. If Division A universities that maintained their ranking improved their scientific or academic quality, we could expect an increase in publications in leading journals. An advantage of this proxy is that we can collect data for an extensive pre-ranking period, starting in 2000.[14] Figure 5 shows that we find no change in SCOPUS publications between both groups. We find no statistically significant differences, suggesting no differences in faculty quality. Together with the hiring results, these estimates suggest no meaningful differences in hiring behavior between treated and comparison universities that could have led to changes in human capital accumulation.[15]

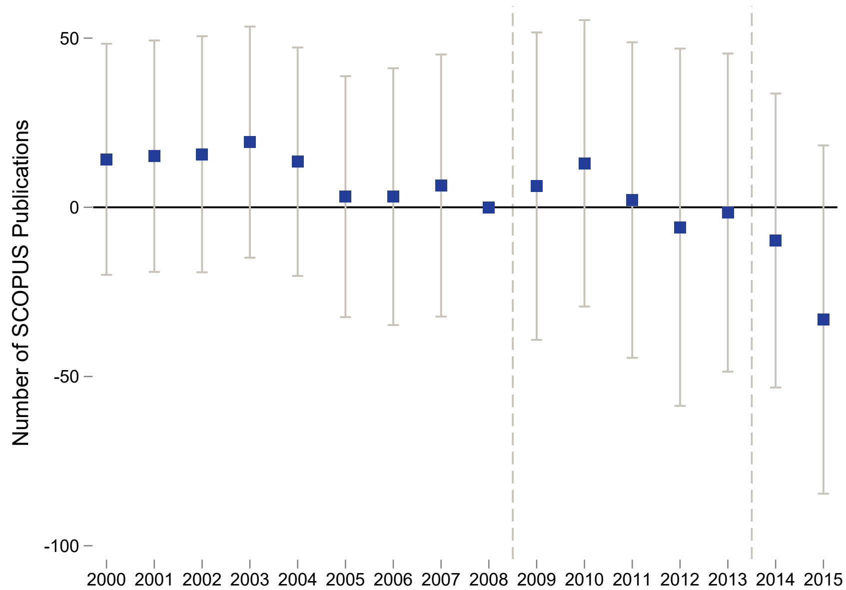
6.2 Evidence of an information effect

Having shown that there are no meaningful differences that could affect human capital accumulation, we now study if the effect of the ranking change is driven by the employers who should react to a change in the information content of the signal of obtaining a college de-

[14]We take 2008 as the reference year in this analysis. We collect data for each university and aggregate at the treatment level. In this case, the results of Bertrand et al. (2004) suggest that heteroskedastic robust standard errors are sufficient for inference.

[15]Ideally, we would test for differences on some measure of academic achievement, but we do not have data on GPA or other measures. We show that there is no difference in relevant inputs of the education production function such as student composition, hiring, and faculty quality. Thus, there is little reason to expect a change in human capital accumulation.

Figure 5: Differences in SCOPUS Publications



Notes: The figure displays heteroskedastic robust 95% confidence intervals.This figure presents difference-in-differences estimates of the number of SCOPUS publications for universities that maintained their ranking and universities whose ranking decreased in 2013. Data is collected at the university (treatment) level, so the results of Bertrand et al. (2004) suggest that heteroskedastic robust standard errors are sufficient.

gree. Theory predicts that college quality information only matter in hiring processes where candidates do not have opportunities to reveal their productivity. To identify employers who should be more or less likely to react to an information change, we explore if the ranking change has different effects on public and private sector employment. In Ecuador, there are two types of hiring in the public sector. The first type is heavily regulated. Article 65 of the Organic Law of the Public Sector dictates that applicants must pass a series of psychometric and knowledge tests and a series of interviews to get a job (Asamblea Nacional del Ecuador, 2010). This process gives applicants ample opportunities to reveal their abilities. Thus, we should not expect an effect from the ranking change. The second type is highly targeted hiring that focuses on a specific candidate without an HR process. In this case, the public employer is interested in hiring a specific person, independently of their qualifications, so the ranking change should also not matter. In contrast, private-sector hiring is more heterogeneous, and the complexity of the hiring process varies among companies . Large companies can have sophisticated processes, while small companies can have very informal processes and rely on signals (Barber et al., 1999; Greenidge et al., 2012). This suggests that private companies should drive the effects of the 2013 ranking change.

Table 5: Effect of the 2013 Ranking Change on the Probability of Salaried Employment in the Public and Private Sectors

	(1)	(2)	(3)	(4)
Change in ranking 2014	-0.0292	-0.0323	-0.0018	-0.0055
	[-0.0478,-0.0062]	[-0.0557,-0.0058]	[-0.0410,0.0215]	[-0.0557,0.0261]
	(0.0190)	(0.0120)	(0.8629)	(0.6727)
Change in ranking 2015	-0.0402	-0.0433	-0.0002	-0.0107
	[-0.0655,-0.0158]	[-0.0863,-0.0070]	[-0.0817,0.0387]	[-0.1169,0.0358]
	(0.0160)	(0.0180)	(0.9870)	(0.7127)
Change in ranking 2016	-0.0279	-0.0250	0.0031	0.0009
	[-0.0788,0.0275]	[-0.0862,0.0511]	[-0.0607,0.0613]	[-0.0816,0.0767]
	(0.2182)	(0.3594)	(0.8749)	(0.9730)
Change in ranking 2017	-0.0162	-0.0013	0.0071	0.0022
	[-0.1042,0.0725]	[-0.1126,0.1359]	[-0.0729,0.1001]	[-0.0924,0.1406]
	(0.6336)	(0.9770)	(0.8058)	(0.9419)
Change in ranking 2018	-0.0184	0.0161	0.0236	0.0110
	[-0.1408,0.1161]	[-0.1583,0.2207]	[-0.0799,0.1350]	[-0.1261,0.1998]
	(0.7137)	(0.8238)	(0.5475)	(0.8458)
Change in ranking 2019	0.0153	0.0613	0.0034	0.0007
	[-0.1576,0.1809]	[-0.1583,0.3123]	[-0.1053,0.1251]	[-0.1709,0.2317]
	(0.8088)	(0.4965)	(0.9209)	(0.9890)
Observations	606,105	514,551	606,105	408,860

Notes: The wild-cluster bootstrap 95% confidence interval is provided in brackets and the associated p-value in parentheses. This table presents the effect of the 2013 university ranking change on the probability of salaried employment in the public and private sectors for the cohorts born from 1988–1992. The first two columns present effects for the private sector. In Column 1, the counterfactual is either working in the public sector or not working as a salaried employee. In Column 2, the counterfactual is not working as a salaried employee only. The last two columns present effects for the public sector. In Column 3, the counterfactual is either working in the private sector or not working as a salaried employee. In Column 4, the counterfactual is not working as a salaried employee only. To increase precision, the estimates control for linear trends, age fixed effects, and individual fixed effects. Results are robust to excluding these controls.

We use Equation 6 to estimate the effect of the 2013 ranking change on the probability of being employed in the public or private sectors. We consider two counterfactuals for these estimates. First, we compare working in the private (public) sector to either working in the public (private) sector or not working as a salaried employee. Second, we compare working

Table 6: Effect of the 2013 Ranking Change on the Probability of Salaried Employment by Employer Type

	Small Employer	Large Employer	Not Top-Payer	Top-Payer
Change in ranking 2014	-0.0184	-0.0322	-0.0015	-0.0317
	[-0.0398,-0.0016]	[-0.0814,-0.0015]	[-0.0068,0.0076]	[-0.0757,0.0089]
	(0.0160)	(0.0370)	(0.6156)	(0.0931)
Change in ranking 2015	-0.0401	-0.0368	-0.0124	-0.0391
	[-0.0560,-0.0088]	[-0.1358,0.0022]	[-0.0224,-0.0001]	[-0.1369,0.0179]
	(0.0000)	(0.0661)	(0.0480)	(0.1031)
Change in ranking 2016	-0.0402	-0.0145	-0.0327	-0.0174
	[-0.0736,0.0394]	[-0.1064,0.0490]	[-0.0592,-0.0076]	[-0.1141,0.0793]
	(0.1562)	(0.5916)	(0.0260)	(0.5566)
Change in ranking 2017	-0.0209	-0.0121	-0.0129	-0.0073
	[-0.0878,0.1080]	[-0.1395,0.1168]	[-0.0483,0.0092]	[-0.1527,0.1765]
	(0.5235)	(0.7888)	(0.2382)	(0.8839)
Change in ranking 2018	-0.0147	0.0030	-0.0143	0.0112
	[-0.1388,0.1901]	[-0.1698,0.1795]	[-0.0546,0.0467]	[-0.1924,0.2466]
	(0.7798)	(0.9590)	(0.5566)	(0.8799)
Change in ranking 2019	0.0170	0.0067	0.0132	0.0193
	[-0.1605,0.2774]	[-0.2076,0.2373]	[-0.0614,0.0993]	[-0.2269,0.3052]
	(0.8318)	(0.9349)	(0.6907)	(0.8268)
Observations	449,399	474,012	351,852	571,559

Notes: The wild-cluster bootstrap 95% confidence interval is presented in brackets and the associated p-value in parentheses. This table presents the effect of the 2013 university ranking change on the probability of salaried employment in the private sectors for different types of employers noted in the column headers. To increase precision, the estimates control for linear trends, age fixed effects, and individual fixed effects. Results are robust to excluding these controls.

in the private (public) sector to not working as a salaried employee only. Table 5 presents these results. The results of employment in the private sector (Columns 1 and 2) are almost identical to the main results presented in Section 5. At the same time, there is no effect on the probability of employment in the public sector (Columns 3 and 4). The point estimates are small and insignificant. Together, these results suggest that the effect of the 2013 ranking change is driven by private companies that are more likely to respond to the change in the information content of obtaining a college degree.

We also look for different responses across different types of private employers. The data identify which employers have more than 100 employees and employers that belong to the top two salary deciles. Larger and more sophisticated employers are more likely to have a more complex hiring system (Barber et al., 1999; Greenidge et al., 2012). Thus, they should be less affected to the ranking change than smaller employers or employers with less sophisticated hiring systems. Table 6 presents these results. The estimates show that top-paying employers and larger employers react to the ranking change earlier than their smaller counterparts, but the 2013 ranking change affected smaller employers and employers who are not top payers more than larger employers and top-payer employers. However, these results are only suggestive as the estimates are imprecise, and we cannot rule out equality.

Overall, the results in this section suggest that the 2013 ranking change varied the information content of the signal of obtaining a college degree from a particular university.

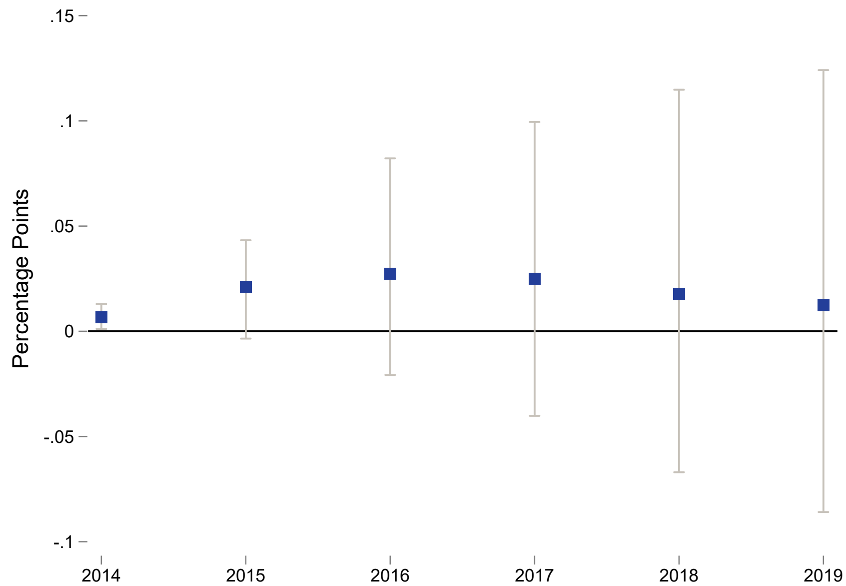
6.3 Individual responses to the ranking change

Signaling theory predicts that an information shock about the potential productivity of new workers should have a short-term effect that fades away as the affected individuals reveal their actual productivity. The results in Section 5 are consistent with this prediction, but they do not provide information regarding the potential mechanisms individuals employ to reveal their productivity. In this section, we explore this issue.

Graduate studies are one way an individual may signal their productivity. While the data includes graduate degrees, there is an additional empirical challenge to estimate the effect of the 2013 ranking change on the likelihood of obtaining a graduate degree. The 2010 post-secondary education law defined that only universities in Division A could keep offering existing graduate programs and open new ones. Programs offered by Division B universities entered a review process and, in some cases, were not allowed to recruit new students. This restriction affected the likelihood of the affected individuals attending graduate school because Ecuadorian universities heavily recruit students for their graduate programs from their alumni population. Appendix Figure A.5 shows that the ranking change decreased the likelihood of obtaining a graduate degree for individuals whose universities decreased their ranking in 2013. This effect holds even for older cohorts born between 1984 and 1987 who were not affected by the information change in their labor market outcomes.

To account for the regulatory change that affected graduate programs, we extend the estimation strategy to a triple difference approach. Since the older cohorts were not affected by the information change but are affected by the regulatory change to graduate programs, we can use them to difference out the effect of the regulatory change and isolate the effect

Figure 6: Effect of the 2013 Ranking Change on the Probability of Obtaining a Graduate Degree



Notes: The figure displays wild-cluster bootstrap 95% confidence intervals. This figure presents triple difference estimates of the effect of the 2013 ranking change on the probability of obtaining a graduate degree for the cohorts born from 1988 to 1992 (Equation 7). To increase precision, the estimates control for linear trends, age fixed effects, and individual fixed effects. Results are robust to excluding these controls.

of the information change. For individual i in year t, we estimate

$$\begin{aligned} Graduate\ degree_{it} &= \alpha + \beta Change_i + \gamma_1 Young_i + \sum_{t=2011}^{2019} \delta_t Year_t + \gamma_2 Young_i \times Change_i \\ &+ \sum_{t=2011}^{2019} \beta_y Change_i \times Year_t + \sum_{t=2011}^{2019} \gamma_t Young_i \times Year_t \\ &+ \sum_{t=2011}^{2019} \theta_t Change_i \times Young_i \times Year_t + u_{it} \end{aligned} \tag{7}$$

where $Young_i$ is an indicator of being born between 1988 and 1992. To increase precision, the estimates control for linear trends, age fixed effects, and individual fixed effects.

Figure 6 presents these results. The point estimates show an increasing effect on the probability of obtaining a graduate degree up to 2.7 percentage points in 2016, which starts to fade away in later years. The pattern is the inverse of the effect of the ranking change on salaried employment or wages, suggesting that some individuals may have decided to attend graduate school to obtain an additional signal that counters the effect of the ranking change. The fading away pattern does not coincide with the pattern on the probability of salaried employment presented in Figure 2. The effect on the probability of salaried employment diminishes, starting in 2016, while the effect on having a graduate degree peaked that year, which is consistent with the average time it takes to obtain a master's degree. However, the effects are imprecise and do not allow us to draw strong conclusions about the magnitude of the effects on graduate education.

7 Conclusion

This paper studies the signaling effects of graduating from a particular university on the probability of salaried employment and wages. We exploit a governmental university ranking change in Ecuador to identify these effects. Rankings of universities changed in 2013 due to a change in methodology when the rankings were updated. Thus, the ranking change provides new information about the perceived quality of recent graduates from affected universities. Employers reacted to the new information according to the predictions of employer learning models. We observe that the probability of salaried employment and wages decrease in the short term. This effect faded over time as the affected individuals found new ways to signal productivity to employers, including obtaining graduate degrees. We show that these results are driven by private employer that are most likely to rely on signals for hiring inexperienced individuals.

These results are consistent with Arcidiacono et al.'s (2010) finding that the returns to ability realize upon entering the labor market for college graduates. If students know the average quality of colleges, they will sort into the colleges that best suit their abilities. If employers know the average quality of colleges and know that students self-sort, they can take the signal of graduating from a particular university as a precise measure of unobserved ability and reflect that ability in their initial wage offer. In this equilibrium, employer learning happens in one step. Our results show that new information on university quality disrupts this equilibrium, and employers behave according to signaling models' predictions. This suggests that a governmental scorecard can lead to better matches and improve welfare in markets with imperfect information regarding university quality.

One limitation of this study is that we can only indirectly rule out changes in human capital accumulation. The institutional setting and the robustness checks suggest that we can interpret the results as the effect of the signal of graduating from a particular university. First, neither the 2009 ranking nor the 2013 ranking included information on student characteristics or outcomes, implying that these variables did not drive the ranking change.

Second, all universities ranked in Division A in 2009 had the same guidelines regarding how to maintain their status. Third, we find no difference in student composition between the affected and unaffected universities. Moreover, the fact that the ranking change effect fades over time is consistent with both university groups having students with similar ability levels. Fourth, we show no differences in faculty hiring and quality between the affected and unaffected universities. Finally, we show that private sector employers drive the results, while public sector employers are unaffected by the ranking change. Hiring practices in Ecuador's public sector are either highly sophisticated or targeted to specific individuals, implying that the signal of graduating from a particular university should not affect a public sector employer's hiring decisions and wages.

This paper documents how agents in a market with imperfect information react to an information signal. The magnitude of the estimated effects highlights that signaling is an important feature in these markets. This implies that policy interventions, like scorecards or rankings, can have meaningful welfare implications where the direction of the welfare change depends on the quality and credibility of the information. On the one hand, in the Ecuadorian setting, the effect of the ranking change fades over time, suggesting that individuals from affected and unaffected universities had, on average, similar abilities. Thus, the ranking change possibly decreased welfare as some of the affected individuals could not work as salaried employees immediately after graduation, and those who got a job received a lower wage. On the other hand, an accurate scorecard may have positive welfare implications in other settings, like nascent markets or where agents may signal false information. Future research is needed to quantify the welfare implications of scorecards, in particular, and signaling in general.

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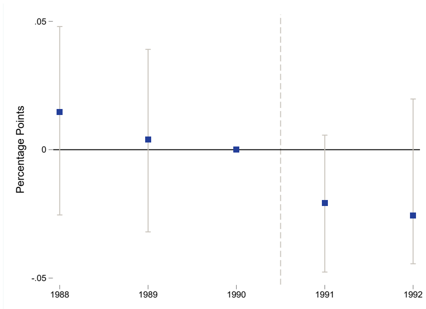
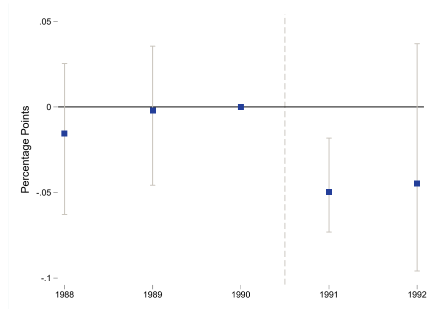
A Online Appendix Tables and Figures

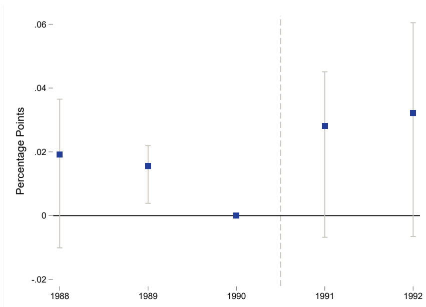
Figure A.1: Differences between Cohorts Born in 1988–1992



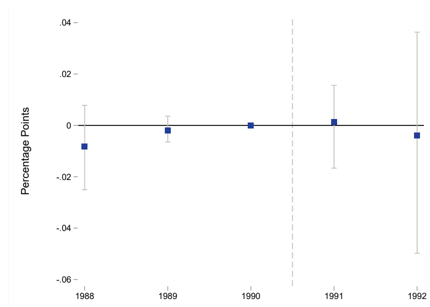
(a) Proportion women



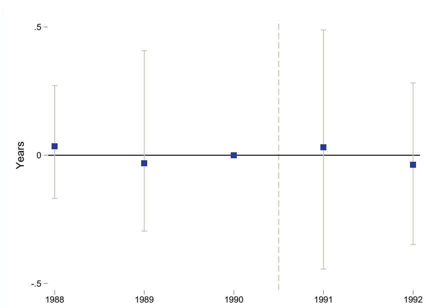
(b) Born in Pichincha



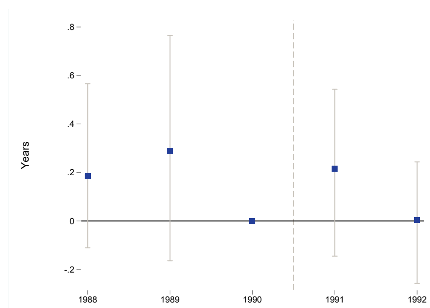
(c) Born in Guayas



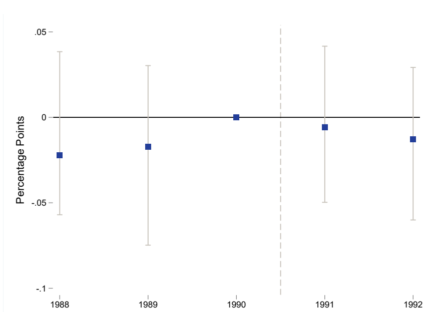
(d) Born in Azuay



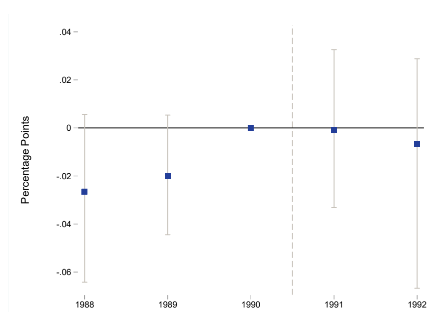
(e) Father's age



(f) Mother's age



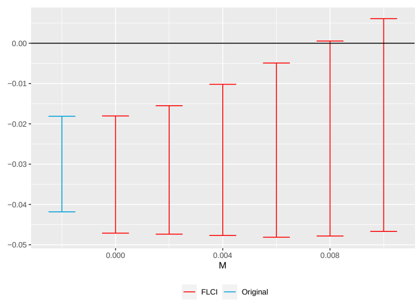
(g) Father graduated from college



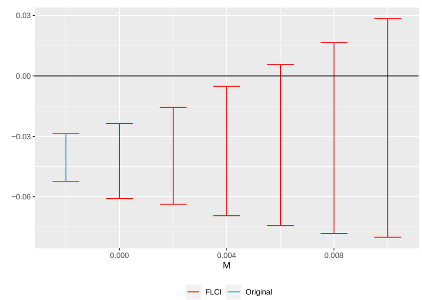
(h) Mother graduated from college

Notes: This figure presents differences across demographic characteristics for the cohorts born in 1988–1992.

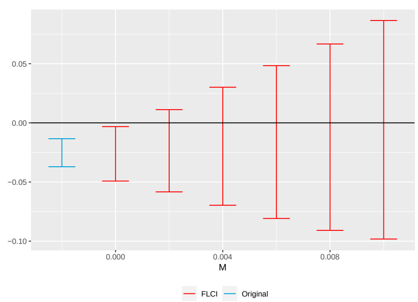
Figure A.2: Sensitivity Analysis to Deviations from Parallel Trends Assumption



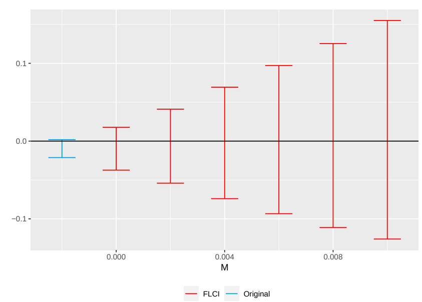
(a) 2014



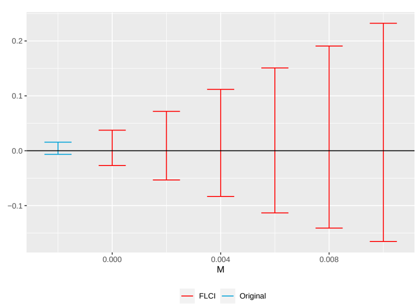
(b) 2015



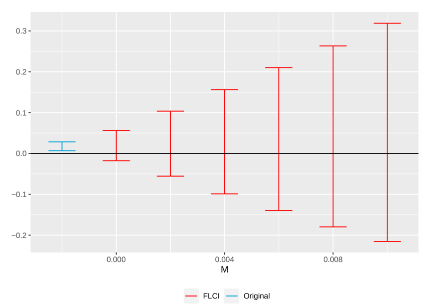
(c) 2016



(d) 2017



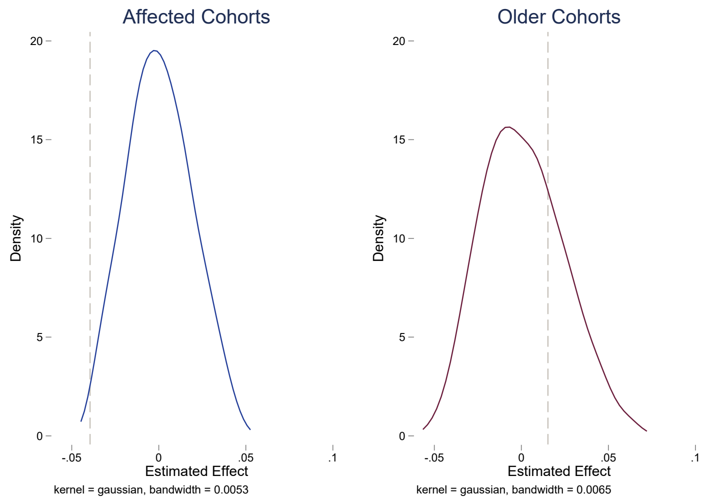
(e) 2018



(f) 2019

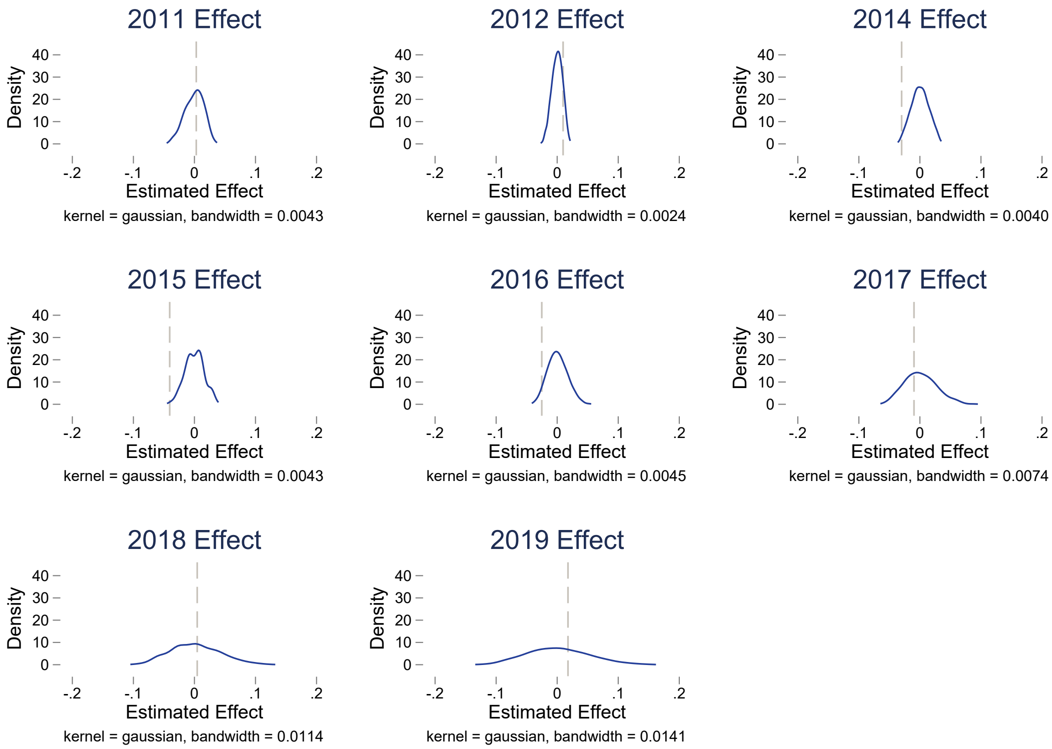
Notes: This figure presents confidence sets robust to deviations from the parallel trends assumption. These confidence sets follow the methods of Rambachan and Roth (2020). The blue line corresponds to the main estimates and the red lines to the sensitivity checks. We perform a sensitivity analysis for multiple deviations from the common trends assumption up to the largest slope change observed before 2014.

Figure A.3: Distribution of Estimates from Permutation Test - Short-Term Effect



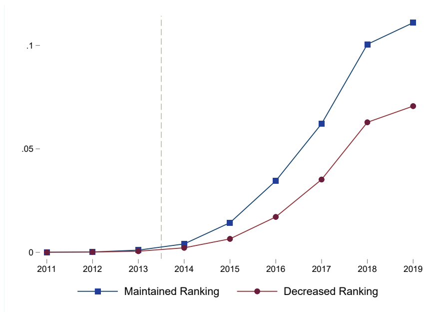
Notes: This figure presents the distribution of estimates from the permutation test performed over the estimates of the short-term effect of the 2013 ranking change for the cohorts born in 1988–1992 and the placebo cohorts born in 1984–1987. This figure corresponds to the results presented in Tables 2 and 4).

Figure A.4: Distribution of Estimates from Permutation Test - Long-Term Effect

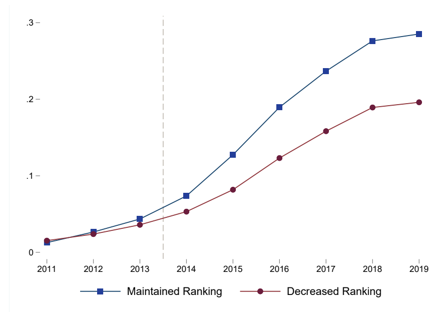


Notes: This figure presents the distribution of estimates from the permutation test performed over the estimates of the long-term effect of the 2013 ranking change for the cohorts born in 1988–1992. This figure corresponds to the results presented in Figure 2 and Table 3.

Figure A.5: Probability of Obtaining a Graduate Degree



(a) Affected Cohorts



(b) Placebo Cohorts

Notes: This figure presents the probability of obtaining a graduate degree for individuals whose universities maintained or decreased their Division A ranking in 2013. The cohorts born in 1988–1992 were affected by the raking change in terms of their labor market outcomes, while the cohorts born in 1984–1987 serve as a placebo.