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ARTICLE

Is there e-learning penalty on wages?

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ABSTRACT
We investigate whether the wage premium associated with higher education differs between e-learning and traditional face-to-face courses. Using Brazilian microdata, we collected information on more than 6,000 students, about half of whom earned their degrees exclusively through online/offline study. We then tracked their labour market trajectories before and after earning their bachelor's degrees. We found that the market tends to pay better to people who earned their degrees through traditional means. However, our results suggest that this is not some sort of e-learning penalty. Rather, these lower wages are likely due to a prior disadvantage that most people who study online have.

KEYWORDS
E-learning; higher education; wage penalty; administrative microdata

JEL CLASSIFICATION
J31; J71; O33

I. Introduction

The adoption of e-learning technologies in higher education has increased in recent decades, even before the COVID-19 pandemic, when many universities made extensive use of these tools. In this context, some papers have investigated whether students of e-learning programmes (online) would achieve different exam scores if they attended traditional face-to-face courses (offline), and vice versa – see, for example, the literature review of Coates et al. (2004), Anstine and Skidmore (2005), Alpert, Couch, and Harmon (2016), or Nortvig, Petersen, and Balle (2018). When confounding factors are controlled, the conclusions seem to more often indicate that the factual and counterfactual outcomes of a given student would be similar. In short, it appears that student idiosyncrasies are more important to academic success than the online/offline teaching approach.

We found few attempts to analyse student outcomes after graduation in terms of salary or employability, similar to Agiomirgianakis et al. (2018b)'s efforts. We then contribute to filling this gap by investigating whether there are differences in wages paid for online/offline degrees in Brazil. Our identification strategy uses administrative microdata in some steps. First, we look for institutions that offer bachelor's degrees where all subjects are offered fully online or fully offline.

We then select students who actually completed these courses and draw a stratified random sample based on the observed characteristics to balance similar individuals, whose only apparent differences are in their academic degrees. These students are tracked among employment contracts to tabulate wages in the years before and after undergraduate.

Following Kleven et al. (2019)'s seminal study of child penalties on the labour market, we test the possibility of e-learning penalties on wages using an event-study model. In addition, we apply selection corrections and fixed effects as discussed by Wooldridge (1995) to control people's unobserved abilities that may bias our estimated results.

For the case examined here, we found that the labour market tends to pay better to people who graduate in the traditional way. However, our exercise suggests that this is not some sort of e-learning penalty. Rather, these lower wages are likely due to a prior disadvantage faced by most individuals who study online. This is a similar point that Dale and Krueger (2002) and Brezis (2018) have highlighted regarding the impact of college choice on the US and Nordic labour markets.

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II. Data

We examined the 2009–2011 editions of the Brazilian Census of Higher Education to determine which institutions offered bachelor's degree programmes in which all subjects were offered fully online/offline, and which students completed these programmes in the expected time. We then drew a stratified sample of 6,079 individuals with balanced characteristics in terms of age, gender, and place of residence, distributed across seven programmes at private universities/colleges with high/low quality indices in educational rankings: Accounting, Business, Computer Systems, History, Linguistics, Social Work, and Teaching. Table 1 shows the number of students observed by programme and selected statistics. In all cases, the mean age at the beginning of the programme was about 25 years old, with the percentage of males and institutional indicators varying somewhat by programme.

We then defined the year when each student earned your degree as $t = 0$ and tracked job outcomes between 2 years before ($t = -2$) and 4 years after ($t = 4$), exploring the Annual Social Information Reports (RAIS) that map the Brazilian labour market based on all formal employment contracts.

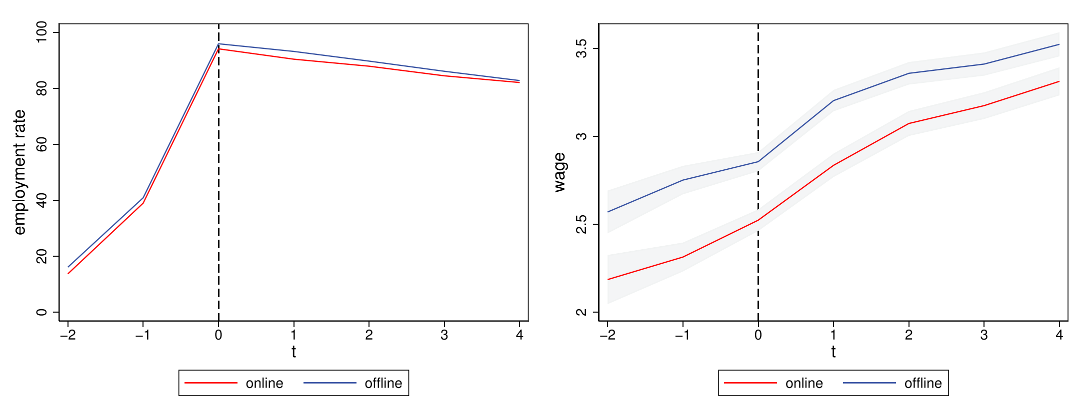
Given a dummy $= 1$ if there is a contract, we defined the employment rate as its mean multiplied by 100. Figure 1(a) shows the trajectories of this indicator by online/offline group. In both, about 20% of individuals had a contract at $t = -2$, almost all had a contract at $t = 0$, and there is a slight negative trend after this.

The outcome we are interested in is the mean monthly wage per hour based on the months worked in the year. To avoid problems related to inflation, we use the current legal minimum wage as an offset variable and multiply the ratio by 100.

Figure 1(b) shows the wage trajectories by group, where the grey areas represent 95% confidence intervals. There are three points to highlight. First, all trajectories show an upward trend, not only because people have become more educated but also because they gain experience over time, which translates into better wages. Second, the mean wage for a conventional course is persistently and

Table 1. Number of students observed by programme and selected statistics.

	Observations			Statistics			
Programme	Online	Offline	Total	Mean Age	% Men	% Universities	% High Quality
Accounting	167	286	453	25.7	38.4	48.5	14.7
Business	984	1,600	2,584	25.6	35.8	51.6	15.1
Computer Systems	26	52	78	25.9	76.9	55.1	23.1
History	76	99	175	25.7	47.4	57.7	32.6
Linguistics	100	153	253	25.8	27.7	43.4	20.2
Social Work	211	204	415	25.8	13.5	64.1	18.3
Teaching	1,029	1,092	2,121	25.7	7.7	54.3	11.9



(a) Employment rate **(b)** Mean wage

Figure 1. Employment rate and mean wage by group and year after graduation (t). The associated t-statistics (with unequal variances) on the equality of wage means for each of the seven periods are, successively: 4.09, 7.48, 7.98, 8.03, 5.88, 4.62, and 3.95.

statistically higher than that for e-learning. Third, those who graduated online earned less before and after graduation.

Since all observed characteristics are balanced between groups, these points suggest that there is something unobservable that favours those who learn face-to-face in the traditional way. In the Brazilian context, we believe that these unobservable predictors are related to family income, which unfortunately cannot be observed.

In fact, in Brazil and other countries, tuitions for offline courses are higher than for online programmes – see, for example, McPherson and Bacow (2015), Deming et al. (2015), Agiomirgianakis et al. (2018a,a,b), or Hanson (2022). In addition, face-to-face courses may have additional costs for transportation, food, clothing, and so on. These cost differences suggest that offline students are more likely to come from higher-income households. Assuming that higher-income families (with potentially better-educated parents) provide to their children more skills in the early stages of their studies, these differences in abilities would explain the difference in wages before and after graduation.

III. Methodology

We use an event-study model to estimate potential e-learning penalties11:

$$\ln w^g_{ist} = \sum_{j \neq 0} \alpha^g_j \times 1(j = t) + \sum_k \beta^g_k \times 1(k = \text{age}_{is}) + \sum_l \gamma^g_l \times 1(l = s) + \eta^g \times \hat{m}^g_{it} + f^g_i + u^g_{ist} \quad (1)$$

where: $\ln w$ represents wage (in logarithm) of an individual i employed in the calendar year s in the career year t; g indicates if i is from the online/offline group; Greek letters are parameters; 1 is the indicator function; $\hat{m}$ is an estimated inverse Mills ratio; f is a fixed effect to capture unobserved idiosyncrasies (constants in the observed period); and u is the error.

The first sum on the right of Equation 1 contains event-time dummies. We omit the dummy at $t = 0$, implying that the respective α measures an impact of graduation relative to the beginning of the career. Running the model separately between online/offline groups, each $\hat{\alpha}^{\text{online}} \neq \hat{\alpha}^{\text{offline}}$ (hats indicate estimates) is interpreted as a penalty percentage difference in the expected wage – in fact, each α is a semi-elasticity in relation to the pre- and post-graduation periods.

The second and third sums on the right of Equation 1 contain age and year dummies, respectively. By including them, we attempt to control for potential experience as in the Mincer earnings function and time trends such as business cycles – details in Kleven et al. (2019).

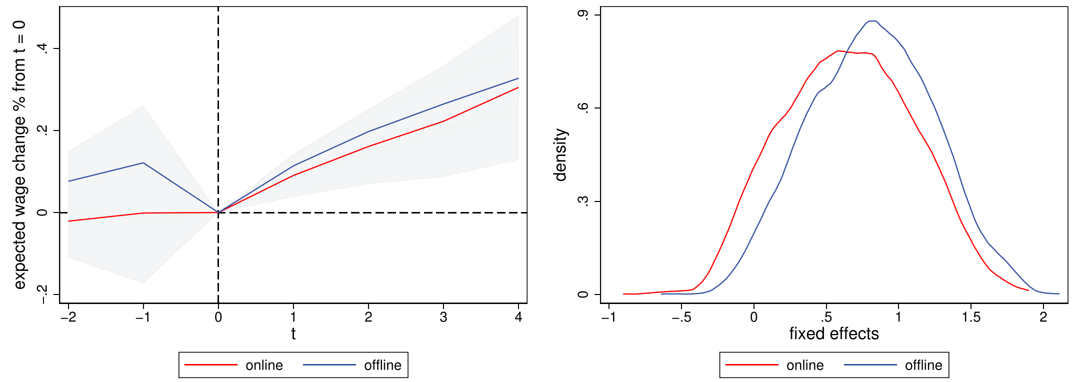
Notably, only workers' wages are observed, and workers may have better unobserved skills than jobless individuals, independently if they are from the online/offline group. This nuance can cause a selection bias. For this reason, we include $\hat{m}$ as a Heckman correction following Wooldridge (1995)'s Procedure 3.2.

Shortly, when $d = 1$ indicates employed, and u and v represent the unobserved factors that determine wage and employment status, respectively, under the hypotheses of joint normal distribution of (u, v), we can write $\ln w = X_2{}'\theta_2 + \rho\sigma_v \times \phi\left(X_1{}'\hat{\theta}_1\right)/\Phi\left(X_1{}'\hat{\theta}_1\right) + u = X_2{}'\theta_2 + \eta \times \hat{m} + u$, where: X_2 and X_1 are controls for Equation 1 and a probit $d = 1$, respectively; ρ is the correlation between u and v; σ_v is the standard deviation of v; and ϕ and Φ are the normal p.d.f. and c.d.f., respectively. Consequently, $\eta = 0$ occurs only if $\rho = 0$, and $\hat{\eta} \neq 0$ implies that we cannot reject the selection bias hypothesis. On the other hand, the presence of $\hat{m}$ mitigates an eventual bias – details in Wooldridge (1995).

Operationally, we run this probit in the first stage for each t and g, where X_1 are dummies for cohorts, courses, age, gender, university/college, and high/low quality. Then, we compute $\hat{m}$ and run Equation 1. Finally, it is important to note that the standard errors for $\hat{\theta}_2$ and $\hat{\eta}$ need to be correct using additional procedures described by Wooldridge (1995) or using a bootstrap.

IV. Results

Figure 2(a) shows all $\hat{\alpha}^g$ and the corresponding 95% confidence intervals calculated using bootstrap procedures – see details and other results in the supplementary material of the article. The



(a) Estimated event-time effects **(b)** Fixed effects densities

Figure 2. Main estimated results.

event-time dummies for both groups are statistically equal to zero for $t=-1$ and $t=-2$, indicating that expected wages before graduation are not different from those in the year of graduation. This reflects a good fit of the models, as these individuals have not yet benefitted from the wage premium for higher education. After $t=0$, the effects of higher education on wages for both groups grow equally within the margin of error. Thus, there is no evidence to support the e-learning penalty hypothesis.

Figure 2(b) shows the kernel densities for fixed effects between groups, f, which represent the idiosyncratic component of wages (or, the premium for a potential skill) that is constant in the observed period and independent of predictors. The mean value is significantly higher among those who studied in the traditional face-to-face way.

This pattern is very similar to what Dale and Krueger (2002) and Brezis (2018) found when they investigated whether the market pays different wages for graduates of elite colleges compared to graduates of non-elite colleges. As in these papers, we believe that a regular student using traditional methods tends to earn more than a regular e-learning student, not necessarily because the first degree has more prestige, but simply because it is chosen by the most able.

V. Conclusion

We found a wage gap in Brazil between graduates of e-learning programmes and graduates of traditional programmes that disadvantages the former. However, since students from high-income backgrounds would predominate in face-to-face courses, this gap would not be discrimination based on online/offline degrees, but on pre-academic skills that wealthier families can offer their children.

Naturally, for future research, the confounding factor discussed here would be clearer if parental wealth/income could be included in Equation 1. Or, alternatively, college entrance exam scores or other controls for pre-academic skills. If these variables positively affect earnings, and to a different extent between online and offline students, our conjectures may become evident.

Finally, as argued by Agiomirgianakis et al. (2018bb), considering that it is cheaper to offer online courses than offline courses, and if the labour market does not really discriminate against the type of graduate in this sense (given pre-academic skills), governments could, for example, redesign support programmes for low-income students, increase funding for online courses, and consequently produce more human capital with less financial resources. Of course, improving the pre-academic skills of low-income students would also be a policy proposal, but perhaps a longer-term one.

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