Grades and Employer Learning

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We identify the labor market returns to university grade point average (GPA) by leveraging a nationwide change in the scaling of grades in Danish universities. Our results show that a reform-induced increase in GPA that is unrelated to ability causes higher earnings immediately after graduation, but the effect fades in subsequent years. The effect at labor market entry is largest for individuals with fewer alternative signals. Although employers initially screen candidates on the basis of skill signals, our findings are consistent with a model in which employers rapidly learn about worker productivity.

I. Introduction

A major challenge for employers is to identify the best-suited applicants for jobs. As productivity at labor market entry is imperfectly observed, employers must use signals of skills. While some studies using experimental

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variation in curriculum vitae details suggest that employers value education credentials (Koedel and Tyhurst 2012; Protsch and Solga 2015; Piopiunik et al. 2020), little is known about the actual impact of such skill signals on labor market outcomes.

This paper examines the signaling value of the grade point average (GPA) of a graduate job applicant at labor market entry and how this effect persists over time in the labor market. To empirically differentiate the impact of the signals from actual skills, we exploit a grading reform in Denmark that mimics an ideal experiment by creating variation in university graduates' GPAs. Students who were enrolled in university during the implementation of the reform had their existing grades recoded to a new grading scale based on a scheme by the Ministry of Education. The recoding caused substantial variation in GPAs that is unrelated to ability, as two individuals with identical prereform GPAs could end up with more than a standard deviation difference in their GPAs because of the recoding. We use this reform-induced change in GPA to produce credible estimates of the effects of grade signals that are unrelated to achievement in a naturally occurring setting.

We use a dataset containing GPAs for all students at the two largest universities in Denmark, Aarhus University and the University of Copenhagen, corresponding to around half of the total population of university students in Denmark.[1] Thus, we examine variation in salient grades among a large and diverse group of university students. Moreover, the detailed Danish administrative data allow us to study how the effect of the grade signal changes over time.

Our findings show that a reform-induced increase in GPA causes higher earnings in the initial years after graduation. However, this effect diminishes over time, and there is no detectable effect 3 years after graduation, which may suggest a rapid employer learning process. We assess the validity of the design and show that the variation caused by the recoding is not associated with individual characteristics that predict labor market outcomes (e.g., high school GPA, parental income, and parental schooling). Moreover, we

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[1] About one in ten of the Danish population aged 30–35 graduated from either the University of Copenhagen or Aarhus University, and more than one in five people in Denmark with a yearly income of more than 60,000 euros graduated from one of the two universities (see table A.1; tables A.1–A.3 are available online).

conduct placebo tests that demonstrate that the recoding algorithm does not predict future labor market outcomes for three nontreated cohorts.

The effect at labor market entry is strongest among graduates with parents without a university degree, graduates with a short work history, and graduates from areas with a small student labor market. These findings suggest that the signaling value of the GPA is particularly important for graduates with limited alternative signals, such as informal links (proxied by parental education) and work history. We also find that the effect is strongest for graduates from majors that are more likely to be employed in the private sector and in jobs with greater earnings dispersion. This indicates that the signaling effect is largest in less regulated labor markets and in labor markets with high variation in earnings.

While there may be other explanations, the diminishing effect of the reform-induced variation on earnings over time indicates an effect of an employer learning process. Looking into the wage adjustment process, we find no evidence of a link between reform-induced variation in GPAs and job changes in the first 5 years after graduation. Instead, we do find a slower earnings growth for individuals who experienced a positive reform-induced change in GPA in their second to third year after graduation. Although the adjustment is fastest among workers who change employers, the slower earnings growth is detectable for all workers, which suggests that the earnings adjustments occur both within and across employers.

Overall, these results demonstrate that grades are relevant in the labor market matching process for university graduates. If we give a student a different grade—all else equal (including exam performance)—the student will have a different labor market outcome in the short run. Moreover, our findings suggest that employer learning happens rapidly. An initially substantial earnings premium to variation in a signal of educational achievement that is unrelated to labor market productivity diminishes, and the adjustment occurs both within and across employers.

This study contributes to the literature on labor market sorting and employer learning (Roy 1951; Arrow 1973; Sattinger 1975; Wise 1975; Farber and Gibbons 1996; Altonji and Pierret 2001; Riley 2001; Lange 2007; Schönberg 2007; Lang and Manove 2011; Fredriksson, Hensvik, and Skans 2018). According to job market signaling theory, employers use completed schooling as a signal of labor market productivity to screen workers (Spence 1973). However, as the graduate workforce has grown, these credentials constitute very crude signals and mask valuable information about the applicant's ability.[2] Consequently, employers often face a choice between

[2] Between 2000 and 2016, the proportion of the population aged 25–34 who had attained a tertiary education increased on average across OECD countries from 26% to 43% (OECD 2017). For example, in the United Kingdom in 2016, more than 50% of the population aged 25–34 had completed a tertiary education program.

applicants with similar levels of educational attainment (e.g., a university degree) and may therefore look for other signals of productivity, such as information on educational achievement like the applicant's GPA.[3] While most existing research on signaling and sorting focuses on educational attainment, only a few studies examine the signaling value of educational credentials among students with similar degrees. One group of studies applies experimental curriculum vitae designs and demonstrates that variation in signals at the intensive margin (i.e., how well a person completed their degree in terms of their GPA) is related to the likelihood of being invited to a job interview (Koedel and Tyhurst 2012; Protsch and Solga 2015; Piopiunik et al. 2020). A second group of studies document returns to receiving an honors degree classification (Freier, Schumann, and Siedler 2015; Feng and Graetz 2017; Khoo and Ost 2018),[4] while a third group of studies exploit discontinuity around the passing cutoff for specific degrees (Tyler, Murnane, and Willett 2000; Clark and Martorell 2014; Jepsen, Mueser, and Troske 2016; Graetz 2021). Our study contributes to the literature by using a novel source of identifying variation to document the effect of the GPA on actual labor market outcomes over time for the population of students across university degree programs. Our findings reinforce the existing evidence that educational credentials are used to sort workers in the labor market and that employers learn rather quickly about actual productivity (Lange 2007; Aryal, Bhuller, and Lange 2022).

II. Background

A. The 2007 Danish Grading Reform

On August 1, 2007, the Danish government replaced the old "13 scale" with a 7-point grading scale in all educational programs from primary school to university. This was to harmonize grading across educational programs and to ease comparison of Danish grades with international grading systems. For students who had already graduated, the Ministry of Education provided a crosswalk from the old to the new GPA. However, for students already enrolled in degree programs on August 1, 2007, their prior degree program grades were recoded on the basis of the scheme shown in table 1. This recoding created the variation in GPAs we exploit in this study.

The first two columns of table 1 show the mapping from the old 13 scale to the new 7-point scale. There are two sources of variation in the recoding of the grades. First, as the new scale has fewer grades (seven compared with

[3] Signals of educational achievement such as a GPA are common in many countries. For example, US colleges use letter grades (A–F), which are converted to a numerical GPA, whereas UK universities assign scores on a 100-point scale, which are translated into a degree classification (e.g., first-class honors).

[4] An alternative signal of productivity is university prestige (Bostwick 2016; MacLeod et al. 2017; Bordón and Braga 2020).

Table 1
The Danish Grading System: Transformation from the Old to the New Scale

Old 13 Scale	New 7-Point Scale	ECTS	Description
00	−3	F	For a performance that is unacceptable in all respects
03 5	0	F+	For a performance that does not meet the minimum requirements for acceptance
6	2	E	For a performance meeting only the minimum requirements for acceptance
7	4	D	For a fair performance displaying some command of the relevant material but also some major weaknesses
8 9	7	C	For a good performance displaying good command of the relevant material but also some weaknesses
10	10	B	For a very good performance displaying a high level of command of most aspects of the relevant material, with only minor weaknesses
11 13	12	A	For an excellent performance displaying a high level of command of all aspects of the relevant material, with no or only a few minor weaknesses

SOURCE.—The Danish Ministry of Science, Innovation, and Higher Education.
NOTE.—The ECTS is the grading system defined by the European Commission. The passing threshold is 6 (old)/2 (new).

ten), three pairs of grades in the old scheme were collapsed into single new grades. For example, a student who only had grades of 8 on the old scheme and another student who only had grades of 9 on the old scale would have identical GPAs after the recoding. Second, the distances between the old and the new grades vary.

Although most students were downgraded in absolute terms, two students with identical prereform GPAs could have very different postreform GPAs because grades were recoded differently. Consider a student with grades 8 and 10 on the 13 scale and another student with two 9s. They both have a GPA of exactly 9.0. The recoding converts the 8 to a 7 and the 10 is unchanged, leading to a GPA of 8.5. The second student's 9s are transformed to a 7, leading to a GPA of 7.0. After the recoding, there is a 1.5-unit difference in the GPA across these two students with identical prerecoding GPAs.

B. The Implementation of the Grading Reform in the Danish Higher Education System

After completing upper secondary education, students can apply for university programs in Denmark. All programs are free, and all students over the age of 18 receive a monthly stipend to pay for their living costs.

Enrollment in university programs depends almost exclusively on high school GPA.

Denmark has adopted a 3-year bachelor's and a 2-year master's structure for most of its university programs.[5] As we focus on the importance of GPAs for labor market outcomes, we only consider graduates from master's programs. University modules are given a European Credit Transfer System (ECTS) weight according to their overall workload, and students are expected to earn 60 ECTS points in each year. A year is typically split into terms of 14–15 weeks (some programs have four terms of 8 weeks), and most programs end with a dissertation.

Students who had their prereform grades recoded also completed exams after the recoding. The final GPA is thus a weighted average of the recoded GPA and the GPA for exams after the recoding. Figure A.2 shows two examples of diplomas for treated individuals. They illustrate what the employers observe when making hiring and wage decisions. The diplomas show the student's GPA based on the recoded grades and provide information about individual grades both before and after the recoding. It is not possible to reconvert the new grades to the old scale (as the new scale has fewer grades). Although it would require a significant effort, employers could, in principle, calculate each candidate's prerecoding GPA or study their grades one by one. If such behavior is widespread, it would go against finding a labor market effect of the reform-induced variation in GPA.

III. Data

A. Data Sources and Sample Selection

We consider all students who were enrolled in a master's program on August 1, 2007, at Aarhus University (including Aarhus School of Business) or at the University of Copenhagen. Students will be at different stages of the programs, as illustrated in figure 1. As the treatment (i.e., the reform-induced variation in GPA) is caused by the recoding of their grades given up to August 1, 2007, we narrow our sample to those who are at the end of their studies (i.e., the upper row of fig. 1). Specifically, we restrict the sample to the students who had at most 40 ECTS points remaining on the day the grades were recoded.[6] The 40 ECTS criterion is selected on the basis of the fact that ECTS credits assigned to the dissertation vary between 30 and 60 in our sample.

[5] Most programs are 5-year programs in practice. More than 90% of the bachelor's graduates progress to a master's program within 2 years of completing their bachelor's program (see fig. A.1; figs. A.1–A.6, B.1, B.2 are available online). Some programs (e.g., medical school) are 6-year programs.

[6] Ideally, we would like to select students who were only waiting for their dissertation results. However, because university studies are very flexible in Denmark (meaning that students might complete some units after their dissertation) and because the credit load of the dissertation varies across years and programs, we cannot

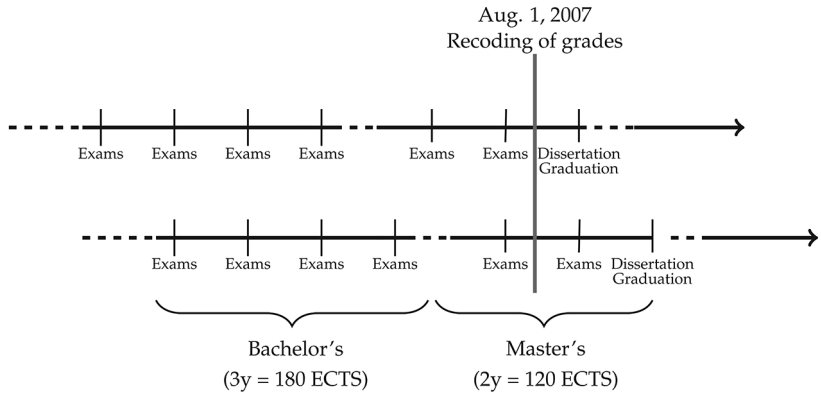

FIG. 1.—University students' exposure to the implementation of the new grading scheme.

To be able to follow the students in their first years on the labor market, we include only students who graduated before 2011. This sample restriction only requires students to have completed coursework that should take considerably less than a year within 3 years. As figure A.3 shows, 23% of the students had graduated by the end of 2007, and 94% had graduated by the end of 2009. We show that our conclusions are not sensitive to changing the sample selection criteria. The final sample consists of 3,813 students.

We merge the student records with administrative registers from Statistics Denmark using the unique personal identifiers. The registers provide individual background information (i.e., age, gender, high school GPA, parental income, and education) and information about labor market outcomes.

B. Variables

From the student records, we construct the final GPA, the GPA before the recoding, and the GPA after the recoding. We further record the number of credit points remaining at the time of the reform, the program studied, and the date of graduation.

Based on the register data, we create a set of student background variables including gender, age at the time of graduation, and high school GPA. For the students' parents, we generate variables on employment, total disposable income in the calendar year prior to the reform, and an indicator for at least one parent having a university degree. In cases where the value is missing for an individual, we set the value to 0 and include a dummy variable that equals 1 for all observations that have missing values.

impose such a criterion. As we demonstrate, our results are robust to different bandwidths.

Our main labor market outcome is log total gross earnings in each of the first five calendar years after graduation. In the model, individuals with zero earnings are excluded. However, to study whether the reform-induced variation in GPA affects the extensive margin, we separately estimate models using an indicator variable that equals 1 if the individual has positive earnings and 0 otherwise.

C. Descriptive Statistics

Table 2 shows the summary statistics for selected variables. The average age of the graduates is almost 31 years.[7] In our sample, 65% of the students are female, and 26% of the sample have parents with a university degree. Forty-one percent are students from the University of Copenhagen. On average, students were close to graduation (23 ECTS remaining), and the grades given before the recoding accounted for about 70% of the overall GPA.

Ninety percent have positive earnings in the year after graduation. A university graduate in our sample earns on average USD 48,000 (2015 level, gross) in the first calendar year after graduation, with 66% of them working in the public sector.

IV. Identification and Estimation

A. Empirical Strategy

In an ideal experiment, we would randomly assign different GPAs on graduates' diplomas and follow their labor market trajectory. In our empirical strategy, we exploit the grading reform's creation of a setting that very closely resembles this. The horizontal axis in figure 2 shows the GPA before the recoding, and the vertical axis shows the GPA after the recoding. Consider the students who had a GPA of 8.0 before the reform. Among these students, some had a GPA of 5.3 and some a GPA of 7.1 after the recoding. This 1.8-unit difference for students with identical prereform GPAs is larger than the standard deviation of 1.66 on the final GPA (see table 2). To test whether the GPA has a signaling value, we compare the labor market outcomes of these students.

As figure 2 shows, a cubic relationship between pre- and postrecoding GPA fits the variation well. In our main strategy, the deviation from this fitted line constitutes the treatment variable. We implement our empirical

[7] In fig. A.4 we show that the median age at graduation is 29.1 years and that the median graduate spent 2.3 years on the labor market before graduating with a master's degree and 9.5 years in postcompulsory schooling (which is scheduled to take 8 years). The unexplained 1.4 years might be spent on travel, military service, and other activities outside the labor force.

Table 2
Summary Statistics

	Mean	SD	P10	P50	P90
	A. Background				
Age at graduation (years)	30.87	5.82	27.51	29.08	32.12
Female	.65	.48	.00	1.00	1.00
Parental disposable income (1,000 euros)	39.41	39.78	26.56	34.30	43.05
Parents with university degree	.26	.44	.00	.00	1.00
	B. University Status				
University of Copenhagen	.41	.49	.00	.00	1.00
ECTS remaining	23.21	13.38	14.00	30.00	30.00
Share of ECTS prerecoding	.70	.12	.67	.71	.75
GPA	8.05	1.66	7.00	8.10	9.22
	C. Labor-Market Status in Year 1 after Graduation				
Earnings > 0	.90	.30	1.00	1.00	1.00
Unemployment	.08	.17	.00	.00	.05
Gross earnings (1,000 euros)	42.88	21.25	31.90	45.28	56.59
Public sector	.66	.47	.00	1.00	1.00
Observations	3,813				

NOTE.—P10, P50, and P90 refer to the 10th pseudo-percentile, the 50th pseudo-percentile, and the 90th pseudo-percentile, respectively. Pseudo-percentiles are created by the average across the actual percentile and the two values above and below the percentile. Parental income is the average across observed parents, measured in the calendar year before the focal individual graduates from university. Parents with a university degree is an indicator variable that takes the value of 1 if at least one parent completed a university degree. All monetary values are adjusted to the 2015 price level using the consumer price index. GPA is the grade point average on the new scale.

strategy by estimating the following equation using ordinary least squares (OLS):

$$Y_{it} = \beta_0 + \beta_1 \text{GPA7}_i + f(\text{GPA13}_i) + \boldsymbol{\lambda}' \boldsymbol{X}_i + e_{it}, \quad (1)$$

where Y_{it} is log earnings in year t after graduation for individual i with a postrecoding GPA, GPA7_i. In our main specification, we control for prerecoding GPA, GPA13_i, using a third-order polynomial, but we show that the results are not sensitive to changing the polynomial degree or using a less parametric approach.[8] As table 2 shows that 70% of the grades are affected by the recoding, β_1 in equation (1) captures the reduced-form estimate of

[8] In app. B (apps. A, B are available online), we provide a Monte Carlo simulation of our empirical setting. Figure B.2 shows that with a linear specification, we fail to reject a true null hypothesis of no relationship between GPA and earnings in nearly 100% of the cases using a 5% cutoff. The linear approximation works poorly in the upper and lower end of the GPA distribution. Both the second- and third-order polynomial specifications lead to rejection rates of the expected 5%. The fourth-order polynomial and the nonparametric approaches perform slightly worse than the second- and third-order polynomials, which is our motivation for using the third-order polynomial as the main specification.

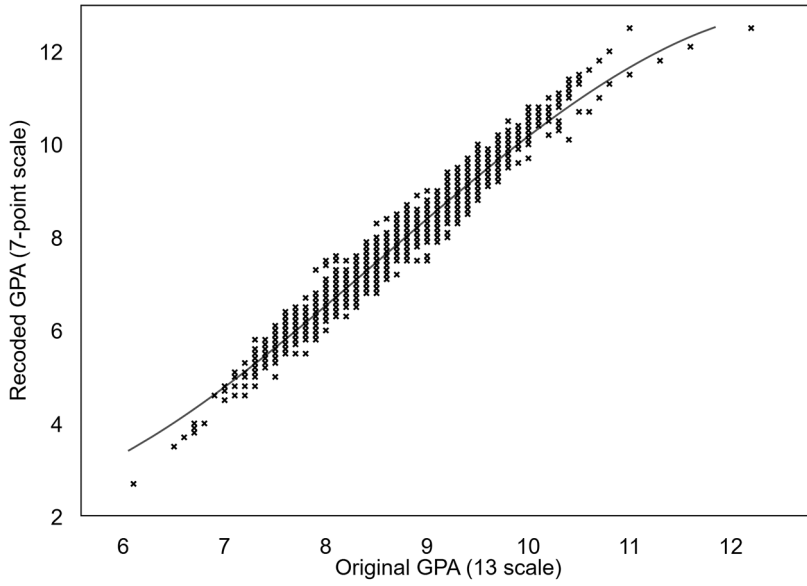

FIG. 2.—Pre- and postrecoding GPA. Each cross represents a combination of pre- and postrecoding GPA. Only grade combinations with at least three observations are shown.

both the signaling value of the GPA and the potential behavioral responses by the students on labor market outcomes. As we discuss in the following subsection, several direct tests suggest that there is no behavioral response.

Our strategy does not rely on any covariates.[9] However, to reduce the residual variance in the outcome variables and obtain more precise estimates, we include a range of controls in vector X_i. First, we expect earnings to be related to the program studied and the institution. We therefore include indicators for the program studied and for institution (University of Copenhagen or Aarhus University). Second, earnings may be related to individual characteristics and background. We therefore control for age, parental origin (non-Western or Western according to the definition by Statistics Denmark), parental income, parental unemployment, parental education, and gender. Parental variables are the mean across observed parents (except schooling, which equals 1 if at least one parent has completed a university degree). We include indicators for the number of parents with nonmissing data (i.e., 0, 1, or 2). Third, we control for high school GPA. The error term e_i includes all other factors affecting the earnings, which could be both other

[9] Estimating models without covariates give very similar results, as we show in sec. V.B.

signals of labor market productivity or factors directly related to productivity (e.g., cognitive and noncognitive skills). To allow for arbitrary correlation within prerecoding GPA cells, we cluster the standard errors on the prerecoding GPA level as reported on the diploma with one decimal point.

B. Identifying Assumptions

Identifying assumption 1.— The causal interpretation of β_1 requires the variation in recoded GPA, given prerecoding GPA, to be unrelated to individual characteristics that affect earnings. Such a correlation could arise for two reasons. First, the recoding algorithm in itself may capture characteristics that are valued on the labor market. To assess whether this is the case, we conduct placebo tests where we implement the recoding scheme on three nontreated cohorts. We apply the same sample selection on these cohorts and recode their GPA with exactly the same algorithm and estimate equation (1) using the same outcome variables.

Second, the variation in GPA induced by the recoding might be correlated with individual characteristics. For example, if more forward-looking students anticipate the reform and either advance their studies (if they would be punished by the reform) or delay their studies (if they would gain) relative to the reform implementation date.[10] To assess whether individuals who benefited from the recoding are systematically different from those who suffered, we show that the reform-induced variation in GPA is unrelated to observable individual characteristics, such as parental background, high school GPA, and undergraduate GPA.

Identifying assumption 2.—Another potential explanation for a link between the recoded GPA and earnings could be that students reacted to the recoding by adjusting their study effort or selecting different elective units after the recoding. Importantly, if students adjust their study effort to compensate for the change in GPA—as shown for high school students in Hvidman and Sievertsen (2021)—it would go against finding labor market returns to a positive reform-induced GPA. In addition, the reform-induced variation could affect the students' likelihood of graduation, time to graduation, and course selection after the recoding. We test for these postreform responses in terms of likelihood of graduation, time to graduation, study effort, and choice of elective units. Moreover, we show an instrumental variable (IV) specification in which we instrument the final GPA with the reform-induced variation in GPA. In the absence of any behavioral responses, the first-stage coefficient should be 0.7, corresponding to the share of grades affected by the recoding. Two features, however, suggest that such behavior might be less pronounced in universities than in high schools. First, for high school students, the GPA is particularly high stakes,

[10] While the reform was announced in advance, we observe relatively little Google search activity before the actual implementation, as shown in fig. A.5.

as it determines access to higher education. Second, as we restrict our sample to university students close to graduation at the time of the reform, they have limited time to react to the recoding.

V. Results

A. Returns to the Reform-Induced Variation in GPA

Panel A of table 3 shows the results from regressing log earnings on the final GPA, conditional on the full set of covariates. A 1-unit increase in the final GPA is associated with 2.3% higher earnings in the first year after graduation (corresponding to a 1 standard deviation increase in GPA being associated with $0.23 \times 1.66 = 3.8\%$ higher earnings). This relationship is relatively constant across the first 5 years after graduation.

Table 3
Regression Results—Effect of Grades on Earnings in Years 1–5 after Graduation

	Year after Graduation				
	1	2	3	4	5
	A. Raw Specification (Dependent Variable: Log Earnings)				
Final GPA	.023*	.017*	.022**	.022***	.025**
	(.011)	(.009)	(.009)	(.008)	(.010)
	B. Reduced Form (Dependent Variable: Log Earnings)				
Recoded GPA	.087***	.078**	.006	.002	−.019
	(.030)	(.033)	(.026)	(.027)	(.038)
	C. First Stage (Dependent Variable: Final GPA)				
Recoded GPA	.713***	.711***	.715***	.713***	.712***
	(.048)	(.048)	(.048)	(.047)	(.050)
	D. IV Specification (Dependent Variable: Log Earnings)				
Final GPA	.122***	.110**	.008	.003	−.026
	(.039)	(.043)	(.035)	(.038)	(.052)
Observations	3,445	3,465	3,423	3,388	3,366
F-statistic	221.21	219.83	226.27	232.60	201.75

NOTE.—Final GPA is the grade point average for the graduates after the recoding and including post-recoding assessment results. Final GPA is instrumented using the recoded grade point average as the IV, controlling for prerecoding GPA using a third-order polynomial. All specifications are estimated with program fixed effects and the full set of covariates, which includes an indicator for institution (University of Copenhagen or Aarhus University), age, an indicator for parental origin (non-Western or Western, according to the definition by Statistics Denmark), parental income, parental unemployment, parental education (indicator for university degree), gender, and high school GPA. Parental variables are created as the mean across observed parents (except schooling, which is 1 if at least one parent has completed a university degree). We include indicators for the number of parents with nonmissing income, unemployment, and education (i.e., 0, 1, or 2). Missing values are replaced with zeros, and an indicator for missing values is included. Standard errors clustered at the prerecoding GPA level are in parentheses. The *F*-statistic is for the excluded instrument in the first-stage specification.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Panel B of table 3 shows that a 1-unit increase in the recoded GPA is related to almost 8.7% higher earnings in the first year after graduation and almost 8% higher earnings in the second year. The effect decreases slightly from year 1 to year 2, and the coefficients are both smaller and not statistically different from zero in years 3–5 after graduation. Even though the decrease from year 1 to year 2 looks considerably smaller than the decrease from year 2 to year 3, the wage adjustments are not statistically different, suggesting that employers learn and adjust wages over the first 2–3 years, whereas the effect has disappeared from years 3 to 5.

Panel C of table 3 confirms a first-stage coefficient of 0.7 corresponding to the share of grades affected by the recoding and indicating no average behavioral response. If students had compensated for the recoding by increasing their effort in response to a negative GPA recoding, the coefficient would have been smaller than 0.7. Finally, panel D of table 3 shows that a 1-unit increase in final GPA induced by the reform causes 12% higher earnings in year 1 and 11% higher earnings in year 2. While we would expect the "raw" specification to be downward biased if the GPA is positively correlated with unobserved characteristics that are also positively linked to earnings, this conjecture is less obvious at labor market entry, where the wage setting very much depends on observable characteristics. Moreover, although the IV coefficients in panel D of table 3 are larger than the raw estimates in panel A, they are also less precise. Thus, both the IV and the raw specification includes returns of around 0.045 in the confidence interval.

The results in table 3 suggest that employers learn about the noise in the signal in about 2–3 years, which is slightly faster than the 5 years found in Lange (2007). Table A.2 shows results from estimating a less flexible model of employer learning in line with Farber and Gibbons (1996) and Altonji and Pierret (2001). We create a balanced pooled sample of the first 5 years after graduation and regress log earnings on the prereform GPA and the postreform GPA, as well as both terms interacted with experience. Column 5 of table A.2 corresponds to the specification in column 2 of table 1 in Altonji and Pierret (2001). The coefficient on the recoded GPA in column 5 is similar to the results in table 3: a unit-higher GPA is associated with 7% higher earnings in the first year. As expected, the coefficient on the interaction between the recoded GPA and experience is negative (corresponding to the interaction of education and experience in Altonji and Pierret 2001), suggesting that the return to observable signals decreases over time. The coefficient on the prereform GPA, which is hard to observe for employers, is close to zero in year 0, but the interaction with experience (corresponding to the interaction between the Armed Forces Qualification Test [AFQT] and experience in Altonji and Pierret 2001) is positive. Using the coefficients from this specification, the results suggest that the impact of the recoded GPA, covering the noisy signal, is zero after $(0.07/0.017) - 1 = 3.12$ years, which is in line with the more flexible specification in table 3.

B. Validity of the Research Design

1. Placebo tests: Does the recoding algorithm predict labor market outcomes for nontreated cohorts?—To assess whether the recoding algorithm captures aspects that are rewarded in the labor market, we conduct placebo tests on three cohorts that were unaffected by the reform. We implement the same sample selection criteria and the same recoding algorithm for cohorts in July 2002, 2003, and 2004 and conduct exactly the same analysis as shown in table 3. We consider all students who had 40 ECTS remaining at most on July 31 in each of these years. We then recode all of the grades given up to that point, define this GPA as GPA7, and estimate equation (1). Table 4 shows the results. We find no detectable association between the recoded GPA and earnings in any of the years for any of the three cohorts.[11] This supplementary analysis suggests that the results in table 3 do not simply reflect the algorithm capturing grade combinations that are appreciated on the labor market.

2. Covariate balance: Is the recoding related to background characteristics?—To assess whether the reform-induced GPA variation is related to individual background, we estimate equation (1) using covariates as the dependent variables. Column 1 of table 5 shows that the reform-induced variation in GPA is unrelated to an individual's gender, and columns 2 and 3 show that it is unrelated to high school GPA and undergraduate GPA. Columns 4–6 reveal that the reform-induced variation is unrelated to parental income, employment, and education, respectively. Finally, in column 7 we construct a weighted average of all covariates by regressing log earnings in the first calendar year on all covariates and constructing predicted earnings based on the estimated coefficients. The coefficient in column 7 is both very small and not statistically different from zero, suggesting that the reform-induced variation is not related to a weighted average of all of the observable characteristics. Together, the findings from table 5 suggest that the reform-induced variation in GPA is not related to any observable characteristics.

3. Did students change their behavior in response to the reform?—We also test whether the recoding of grades affected subsequent student behavior after the recoding. We test whether the recoding affected students' likelihood of graduation, their performance in subsequent exams, selection of different

[11] The placebo cohorts that we analyze are smaller than the treated cohort. The birth cohorts of the placebo cohorts are smaller. Furthermore, the student records from Aarhus University start in 2000. As some students spent more than 2 years completing their master's program, fewer students are included in the placebo analysis (particularly for the 2002 cohort) than in the main analysis. Table A.3 shows that estimating placebo specifications separately for each cohort confirms the conclusion from the pooled regression.

Table 4
Reduced-Form Regression Results—Placebo Cohorts
(Dependent Variable: Log Earnings in Years 1–5 after Graduation)

	Year after Graduation				
	1	2	3	4	5
Recoded GPA	.013	−.010	−.008	−.001	−;.004
	(.046)	(.036)	(.028)	(.035)	(.030)
Observations	3,029	3,095	3,098	3,102	3,074
R^2	.16	.17	.17	.15	.17

NOTE.—This table resembles table 3 but shows estimates based on pooling three placebo samples. The placebo samples are created by implementing a placebo recoding of grades on, respectively, July 31, 2002; July 31, 2003; and July 31, 2004, using the same recoding scheme, covariates, and sample selection as in the main specification. See the table 3 note.

elective units, and their time to graduation. Table 6 shows the results of these estimations.

We find no evidence of any behavioral responses. Columns 1 and 2 show that the reform-induced variation in GPA does not affect the likelihood of graduation or the time to graduation. Columns 3 and 4 show that the reform-induced variation is unrelated to unit difficulty and performance in subsequent assessments.[12]

4. Are the results sensitive to model specification?—To assess whether our findings are sensitive to the empirical specification, figure 3 shows point estimates and confidence intervals for β_1 based on 72 different specifications using log earnings in the first calendar year after graduation as the dependent variable. The empirical specification is indicated by the markers below the chart. All 72 specifications lead to positive and significant point estimates in the 0.05–0.10 range. The chart also shows that our main specification (indicated with a square marker) is not an outlier.

5. Is the relationship symmetric?—So far, we have assumed a linear relationship between the reform-induced variation in GPA and log earnings. However, there are reasons to expect a nonlinear relationship. For example, job candidates who receive a negative reform-induced change (relatively speaking) to their GPA have an incentive to inform employers about this, while job candidates who receive a positive shock have no incentive to do so. Thus, it could be the case that the relationship is driven by the positive shocks.

[12] We measure unit difficulty as follows. First, we consider all individual exam results for the units in the 6 years prior to the reform (2000–6). Second, we regress these individual exam results on unit fixed effects and students' high school GPA. A larger unit fixed effect suggests that given their high school GPA, a student receives a higher grade in that subject. In other words, the fixed effects capture unit difficulty (or grading generosity within a given unit). We then match these unit fixed effects to our treated cohort's attended units after the recoding of the grades and use the fixed effects as the dependent variable.

Table 5
Reduced-Form Regression Results—Covariate Balance (Dependent Variables in Column Header)

				Parents			
	Female (1)	High School GPA (2)	BSc/ BA GPA (3)	Income (4)	Unemployment (5)	University Degree (6)	Predicted Earnings (7)
Recoded GPA	−.018	−.001	.048	2.766	−.004	.020	−.000
	(.017)	(.031)	(.053)	(2.112)	(.004)	(.020)	(.004)
Observations	3,811	3,218	2,046	3,363	3,813	3,322	3,813
R^2	.14	.31	.31	.04	.02	.07	.94
Mean dependent variable	.65	.75	−.00	39.41	.02	.26	3.62

NOTE.—This table shows the coefficients from estimating eq. (1) using the variables denoted in the column headers as dependent variables. Parental variables are measured in the calendar year before graduation. Parental income is the average disposable income across the observed parents, measured in EUR 1,000 (2015 level). Unemployment is the average annual unemployment of the observed parents. University degree is an indicator for whether at least one parent has completed a university degree. All models are estimated without covariates but with program fixed effects. Predicted earnings is the predicted earnings based on coefficients from a regression of log earnings in the first year after graduation on all covariates. Standard errors clustered at the prerecoding GPA level are in parentheses.

Figure 4 shows the relationship between residualized earnings and residualized reform-induced GPA. We estimate the relationship using a natural cubic spline with three knots. The more flexible relationship (compared with the OLS relationship) shows a positive relationship throughout and is always within the 95% confidence interval of the linear OLS relationship. Our results show that the returns to the reform-induced variation appear to be fairly linear across the entire scale of the reform-induced variation.

Table 6
Reduced-Form Regression Results—Behavioral Responses (Dependent Variables in Column Headers)

	Graduated (1)	Time to Graduation (2)	Unit FE (3)	Post-GPA (4)
Recoded GPA	−.013	−.019	.000	.049
	(.012)	(.039)	(.000)	(.122)
Observations	4,579	4,048	3,696	4,048
R^2	.21	.15	.74	.24
Mean dependent variable	.88	.97	.00	8.18

NOTE.—This table shows the coefficients from estimating eq. (1) using the variables denoted in the column headers as dependent variables. Column 1 is an indicator for whether the focal individual graduated before 2011. Column 2 is the time from recoding to graduation, measured in years. Column 3 is the average unit-specific fixed effects of units completed after the recoding. The fixed effects are estimated on the basis of prereform cohorts by regressing exam grade as the dependent variable on unit indicators capturing the fixed effects and high school GPA. A positive fixed effect suggests that, conditional on high school GPA, this unit has historically been graded more generously. Column 4 is the grade point average of all units completed after the recoding. All models are estimated with the full set of covariates (see the table 3 note). Standard errors clustered at the prerecoding GPA level are in parentheses.

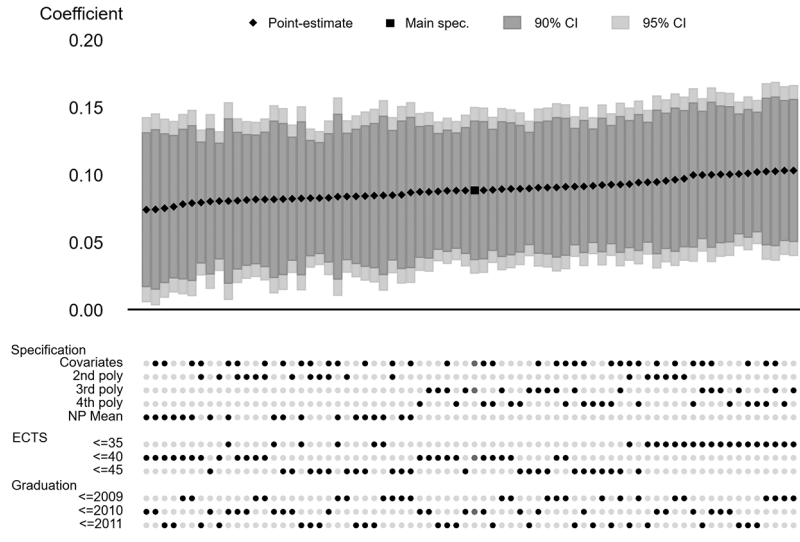

FIG. 3.—Specification curve. The chart shows the reduced-form point estimates and confidence intervals (CIs) using the specification indicated by the markers below the chart. The first row of markers indicates whether the specification is estimated with or without any control variables. Specifications with a black marker include the full set of controls. Specifications with a gray marker only include major fixed effects. The second to fourth rows of markers capture the functional form of $f(\text{GPA13}_i)$, where a black marker indicates that the specification named on the left is applied. The fifth row ("NP Mean") shows results from a less parametric approach where we compare an individual's recoded GPA to the mean recoded GPA among all other students with the same prerecoding GPA. This specification also includes prerecoding GPA fixed effects. In the two lower panels ("ECTS" and "Graduation"), we alter the sample selection criteria. ECTS relates to the number of ECTS credit points remaining at the time of the reform, and graduation refers to the time limit for graduation that was imposed.

C. Mechanisms and Heterogeneity

This section explores mechanisms that could drive the effect of the reform-induced variation in GPA and subgroup analyses.

Mechanisms.—In table 7, we investigate the effect of the reform-induced variation in GPA on alternative labor market outcomes over the first 5 years after graduation. We estimate equation (1) with different labor market outcomes as the dependent variable.

The first row of table 7 shows that there is no effect on the extensive margin of earnings. The second and third rows show the effects on log disposable income (after transfers and taxes). Without conditioning on actually having earnings (second row), the point estimates in years 1 and 2 are somewhat

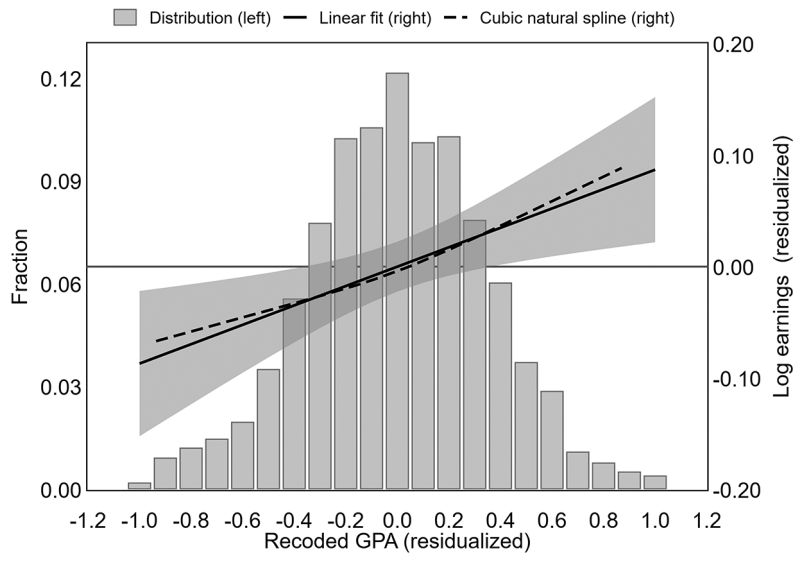

FIG. 4.—Relationship between log earnings in the calendar year after graduation and reform-induced variation in GPA. The solid line shows the linear relationship estimated in our main specification. The shaded area shows the 95% confidence interval. The dashed line shows the natural cubic spline using three knots. The reform-induced variation in GPA and the log earnings are residualized using all covariates in the main specification and program fixed effects.

noisy, but after conditioning on positive earnings (third row), we find that a 1-unit increase in reform-induced GPA leads to an increase in disposable income of respectively 4.6% and 3.7% in the first two calendar years after graduation.

The results in rows 4–7 suggest that there is no link between reform-induced GPA and unemployment, public sector employment, or job changes within the first 5 years after graduation. Finally, row 8 shows that a higher reform-induced GPA leads to lower earnings growth in year 3 after graduation, and as row 9 shows, this is also evident for employees who stay with the same employer.

Overall, the results in table 7 show that the reform-induced variation in GPA is unrelated to the extensive margin adjustments: there is no effect on labor market participation, sector, or job changes. However, we see evidence of earnings adjustments in years 2 and 3, and this also occurs within the firm.

Heterogeneity.—We consider two sources of heterogeneity: the potential for individual wage setting and the importance of educational credentials as signals.

The reform-induced GPA could play a bigger role in less regulated labor markets with greater earnings dispersion. Panels A–C of table 8 show point

Table 7
Reduced-Form Regression Results—Other Labor Market Outcomes (Dependent Variables in Row Titles)

	Year after Graduation				
	1	2	3	4	5
Earnings > 0	−.005	.008	.002	−.008	−.005
	(.012)	(.013)	(.013)	(.011)	(.013)
Log disposable income	.052	.082***	−.009	−.002	.005
	(.039)	(.031)	(.029)	(.022)	(.025)
Log disposable income|earnings > 0	.046***	.037**	−.006	−.012	−.010
	(.015)	(.014)	(.015)	(.015)	(.022)
Unemployment	−.006	−.005	−.006	.000	.012
	(.007)	(.006)	(.005)	(.004)	(.007)
Public sector	.008	.001	−.006	.005	.035
	(.017)	(.018)	(.019)	(.023)	(.046)
Job change		−.019	.020	−.016	.015
		(.016)	(.020)	(.021)	(.020)
Job change with earnings growth		.006	−.061	.003	.052
		(.047)	(.047)	(.053)	(.071)
Earnings growth year		−.006	−.069***	−.022	−.015
		(.030)	(.025)	(.020)	(.031)
Earnings growth|same employer		.032	−.046*	−.028*	−.002
		(.027)	(.024)	(.017)	(.022)

NOTE.—This table shows the coefficients from estimating eq. (1) using the variables denoted in the first column as dependent variables. All models are estimated with the full set of covariates (see the table 3 note). Standard errors clustered at the prerecoding GPA level are in parentheses.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

estimates for subsamples that vary in their structure. First, the earnings distribution for women tends to be more compressed than for men. We find the largest effects for men, as shown in panel A. Furthermore, based on cohorts graduating from 2002 to 2006, we split the sample according to the prereform wage dispersion (panel B) and public employment share (panel C) of the university programs. We find larger effects for majors with higher wage dispersion and lower public employment shares. Nevertheless, splitting the samples leads to small sample sizes, and none of the subgroup differences are statistically significant.

The second set of heterogeneity analyses investigates whether students with a strong network or extensive labor market experience are less dependent on skill signals (e.g., GPA). First, we split the sample by parental education. Graduates with better networks on the relevant job market—as proxied by parental education—could be less reliant on the GPA as a signal. In line with this hypothesis, we find stronger effects for children of parents without a degree (panel D). Relevant labor market experience might also lower the importance of educational credentials in the job search. Danish

Table 8
Reduced-Form Regression Results—Subgroups (Dependent Variable: Log Earnings Years 1–5 after Graduation)

	Year after Graduation				
	1	2	3	4	5
	A. By Gender				
Female = 0	.154**	.108	.031	.014	−.067
	(.057)	(.061)	(.054)	(.050)	(.065)
Female = 1	.056	.051	−.015	−.015	.004
	(.047)	(.043)	(.030)	(.032)	(.036)
p-value	.24	.48	.47	.58	.27
	B. Major Wage Dispersion				
<p(50)	.049	.060	−.009	−.063*	−.075
	(.038)	(.032)	(.030)	(.029)	(.044)
>p(50)	.130***	.083	.011	.071	.052
	(.048)	(.069)	(.049)	(.042)	(.055)
p-value	.16	.77	.73	.00	.06
	C. Major Public Sector Share				
<p(50)	.115*	.207**	.050	.039	.049
	(.056)	(.071)	(.057)	(.056)	(.071)
>p(50)	.073*	.018	−.017	−.016	−.048
	(.032)	(.035)	(.029)	(.037)	(.041)
p-value	.49	.02	.29	.43	.23
	D. Parents with University Degree				
No	.093*	.111*	.014	−.001	−.000
	(.039)	(.043)	(.036)	(.036)	(.043)
Yes	.028	−.035	−.024	−.033	−.106
	(.045)	(.057)	(.045)	(.050)	(.057)
p-value	.30	.05	.55	.61	.09
	E. Earnings while Studying				
<p(50)	.135*	.074	−.026	.107*	−.007
	(.058)	(.068)	(.049)	(.045)	(.061)
>p(50)	.063	.077**	.019	−.063*	.027
	(.038)	(.029)	(.031)	(.030)	(.045)
p-value	.31	.96	.45	.00	.78
	F. University				
Aarhus	.137**	.105	.016	.037	−.041
	(.048)	(.054)	(.039)	(.037)	(.051)
Copenhagen	.032	.039	−.015	−.050	.010
	(.034)	(.042)	(.035)	(.041)	(.048)
p-value	.06	.33	.55	.13	.44

NOTE.—The coefficients shown are for the subgroup denoted on the left. The *p*-value rows show the *p*-value for the test of the null hypothesis that the estimate of β_1 is the same in both subsamples. See the table 3 note. p(50) = 50th percentile.

university students often have part-time jobs that are relevant to their studies and later careers. Panels E and F show that effects are stronger for graduates with low earnings while studying (a proxy for little relevant labor market experience) and for graduates from Aarhus University. Because the student labor market is considerably larger in Copenhagen, the findings could suggest that graduates rely less on the GPA as a signal to employers if they have had better access to the labor market during their studies.[13]

VI. Conclusion

Using variation in university students' GPAs that is unrelated to labor market productivity, we document a signaling value of university GPAs at labor market entry. Studying outcomes over time, we find evidence that employers rapidly learn about productivity and that earnings adjustments occur both within and across firms.

We find no effects on the likelihood of employment just after graduation or on job changes within the first 5 years on the job market. Additionally, we find that the signaling value of GPAs is strongest for men and for children of parents with no university degree. The latter result may suggest that signals are more relevant to workers with no informal connections to the labor market. Finally, our findings indicate that the effect is strongest for majors that are related to larger wage dispersion and strongly connected to the private sector.

Our findings have important policy implications. First, they suggest that the grading system influences the labor market matching process. Grade inflation and bunching, for example, could make it harder for employers to identify the best applicant. Moreover, systems that focus on parts of the achievement distribution (e.g., through honors degrees) might involve lower matching efficiency at the lower end of the distribution. Second, our results illustrate the importance of developing systems that produce accurate skill signals. Several factors can affect assessments, including pollution (Ebenstein, Lavy, and Roth 2016), weather (Park et al. 2020), time of the day (Sievertsen, Gino, and Piovesan 2016), and teacher manipulation (Diamond and Persson 2016; Dee et al. 2019). Our findings indicate that such external factors have implications for labor market outcomes.[14] Our finding of a large initial signaling value of educational credentials to some degree

[13] There is no clear institution-level reputation difference between Aarhus University and the University of Copenhagen. To test this empirically, we estimated a regression of log earnings on GPA, major fixed effects, and institution fixed effect. The coefficient on the institution fixed effect is not statistically significantly different from zero.

[14] Note that none of the studies listed above are in a university setting. However, there is substantial anecdotal evidence for errors in grading in higher education (see, e.g., Nightingale 2017).

justifies the students' focus on grades. However, we find no evidence that the grades are important for securing a first job that puts the graduates on a different earnings trajectory that is impossible to catch up on. Third, Bar, Kadiyali, and Zussman (2009) show that students select into elective courses that are more leniently marked. Our short-run results justify this behavior, but the rapid employer learning suggests that the benefits of prioritizing the signal (a higher grade) over human capital (selecting units on the basis of content) might be temporary. Finally, while the setting that we study is unique, grading reforms are relatively widespread. Although the implementations might vary from reform to reform, they will typically generate some noise in the signaling process that could have important implications.

More generally, our findings provide evidence of the importance of skill signals in the labor market and key insights into the employer learning process. Future research on signaling and employer learning based on educational achievement could provide an even deeper understanding of this learning process by exploiting alternative sources of variation in signals.

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