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The effect of higher-achieving peers on major choices and labor market outcomes[☆]

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ABSTRACT

This paper investigates how exposure to higher-achieving male and female peers in university affects students' major choices and labor market outcomes. For identification of causal effects, we exploit the random assignment of students to university sections in compulsory first-year courses. We present two main results. First, studying with higher-achieving peers has no statistically significant or economically meaningful effects on educational choices. Second, we find suggestive evidence that women who have been exposed to higher- achieving male peers end up in jobs in which they are more satisfied.

1. Introduction

Women remain underrepresented in math-intensive subjects and majors (OECD, 2017). The desire to understand the origins of these gender differences has led to an interest in factors that influence women's and men's educational choices.

One factor that may influence students' educational choices is whether they study with higher-achieving peers. A large body of literature has shown that higher-achieving peers positively affect students' grades and the classroom atmosphere (Sacerdote, 2011). The effect of higher-achieving peers on educational choices, however, has received substantially less attention. Exposure to higher-achieving peers may lead to better grades, which may motivate students to choose more-challenging majors. Alternatively, students may be discouraged from entering challenging majors if they study with peers who seem to know everything. These effects may also be gender specific. For example, higher-achieving male peers may affect preferences for majors by creating a classroom atmosphere that men appreciate and women do not.

In this paper, we study how exposure to higher-achieving male and female peers in university affects students' course choices, major choices, and labor market outcomes. We use data from a Dutch business school in which students take eight compulsory courses in their first year of study and then specialize through elective courses and majors. Within compulsory courses, students are randomly assigned up to 15 section peers with whom they spend a large share of their university

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contact hours. This setting allows us to test how random exposure to peers with higher achievement, as measured by their past grade point averages (GPAs), affects students' educational choices. We look at the effect of peers on mathematical majors, which attract higher-achieving students and are associated with higher earnings in the labor market. Additionally, we conduct a graduate survey that allows us to document the labor market consequences of higher-achieving peers one to five years after graduation. Data from this survey provide a detailed picture of how peers affect different aspects of graduates' careers, including earnings and job satisfaction. We also test how students are affected by their peers in their first-year compulsory courses.

Our results show no significant effects of peer achievement on students' probability of choosing a mathematical elective course or major. The point estimates are small and precisely estimated. For example, our point estimates predict that being assigned to one course with male peers who have one standard deviation higher GPAs decreases women's probability of choosing a mathematical major by 0.82 percentage points. The corresponding 95 percent confidence interval allows us to rule out effects smaller than –2.01 and larger than +0.37 percentage points. When looking at the effect on labor market outcomes, we find no significant effects on earnings, working hours, or the probability of being employed. These estimates, however, are less precise. We do find suggestive evidence that women who were assigned to higher-achieving male peers are more satisfied in their job. When looking at peer effects on outcomes measured while students take their compulsory first-year courses, we see that women and men who were randomly assigned to higher- achieving male peers evaluate the classroom interaction with their peers more positively.

One might be concerned that we only find small peer effects estimates due to attenuation bias. This concern is valid, as we have shown in Feld and Zölitz (2017): in settings where students are randomly assigned to peer groups and achievement measures ability with classical measurement error, peer effects estimates are attenuated. The degree of attenuation bias is proportional to the test reliability of the ability measure, with more noisy measures leading to more attenuation bias. While this problem has been acknowledged in several peer effects papers, no study has corrected for it (e.g., Carrell et al., 2018; Fischer, 2017; Hill, 2017). Until now. We show how to estimate the test-reliability of GPA. Using these test-reliability estimates to adjust for attenuation bias leads to point estimates that are 6 percent larger for women and 4 percent larger for men. Attenuation bias is not a large concern in our setting.

Our paper complements four previous studies that have investigated the effect of higher-achieving peers on educational choices and labor market outcomes. Most related to our study, Fischer et al. (2021) investigate the effect of being randomly assigned to higher-achieving peers at the Copenhagen Business School. They find that studying with peers who have one standard deviation higher high school GPAs reduces women's earnings by 4 percent. This effect is driven by being assigned to higher-achieving male peers and becomes stronger over time. Cools et al. (2021) show that the presence of higher-achieving male peers in high school—as proxied by the education of their parents—reduces female students' math and science grades and their probability of completing a bachelor's degree. Similarly, Mouganie and Wang (2020) show that the presence of high achieving male peers in high school reduces female students' likelihood of choosing a science, technology, engineering, or math (STEM) major, and the presence of high-achieving female peers has the opposite effect. Fischer (2017) investigates how peer achievement—as proxied by the proportion of peers who are on track to graduate—affects educational choices without distinguishing peer achievement by gender. She exploits as-good-as-random assignment of students to classes with, on average, 330 students in an introductory chemistry course at the University of California, Santa Barbara. Her results show that women who are exposed to higher- achieving peers are less likely to graduate with a STEM degree. Taken together, these studies suggest that the presence of higher-achieving peers affects educational choices, and the impacts are particularly negative for women assigned to higher-achieving male peers.

Our study contributes to the literature by suggesting that both of these conclusions are not universal: peers do not always have measurable effects on educational choices, and, at least in our setting, we see suggestive evidence that women get some long-term benefits from being assigned to higher-achieving male peers. More generally, we contribute to the scientific discourse by answering the same research question with a credible identification strategy in a different context. This endeavor, together with a commitment to documenting null results, is a crucial part of the scientific discourse. It allows researchers and policymakers to assess the robustness and generalizability of effects.

This paper also relates to our previous work in which we use data from the same environment to investigate how students' educational choices and labor market outcomes are affected by the proportion of female peers (Zölitz and Feld, 2021). In this previous study, we show that women who are randomly assigned to a higher proportion of female peers are more likely to choose female-dominated majors like marketing and less likely to choose a male-dominated major like finance. Effects for men go in the opposite direction. After graduation, we see no effect on men's labor market outcomes, but women who had a higher proportion of female peers see slower wage growth. While our previous paper shows that the quantity of female peers matters, the current paper tests whether the quality of male and female peers—as measured by their GPA—matters as well.

2. Institutional environment and summary statistics

2.1. Institutional environment

We study the effects of peer achievement on course choice, major choice, and labor market outcomes using data from a Dutch business school for the academic years 2009/10 through 2014/15. We limit our analysis to two bachelor's programs: Business and Business Economics. Both programs take three years to complete. Each academic year consists of four eight-

Table 1
Compulsory courses in the first year.

Teaching period	Study Program: Business	Study Program: Business Economics
1	Management of Organisations and Marketing / *Quantitative Methods I*	Management of Organisations and Marketing / *Quantitative Methods I*
2	Accounting and Financial Reporting / *Economics and Business*	Accounting and Financial Reporting / *Microeconomics*
3	Strategy / *Quantitative Methods II*	Macroeconomics / *Quantitative Methods II*
4	*Finance* / Fundamentals of Supply Chain Management	*Finance* / International Economic Relations

NOTE — Courses in *italics* are mathematical courses based on the definition provided in the main text.

week teaching periods during which students typically take two courses simultaneously. In the first year of the bachelor's programs we focus on, students take eight compulsory courses in a fixed sequence and then choose several elective courses and one major in their second and third years.

Table 1 shows the list of compulsory first-year courses for each program. For our analysis, we distinguish between mathematical courses and non-mathematical courses. We define a course as mathematical if its description contains one of the following terms: math, mathematics, mathematical, statistics, statistical, or theory-focused. Following this definition, the students in the Business program take four mathematical compulsory courses (Quantitative Methods I and II, Economics and Business, and Finance) and students in the Business Economics program take four mathematical compulsory courses (Quantitative Methods I and II, Microeconomics, and Finance). While students in both programs take courses with the same name, these are offered under a different course code at the business school. Students are therefore only assigned to peers from their own study program.

Each course consists of multiple sections that have an average of 14 and a maximum of 16 students. The section composition is different for each course students take. Sections are the peer group we focus on in this paper. Over an entire course, students typically meet their section peers for twelve two-hour tutorial sessions. Besides tutorials, a typical course consists of three to seven two-hour lectures that all students attend.

During tutorial sessions, students typically discuss the course material and solutions to exercises with their section peers. The teaching style of this business school emphasizes classroom discussion, and students typically prepare the course material and solve exercises before the tutorial sessions. The main role of the tutorial instructor is to guide the discussion. Within a course, all sections use the same course material and follow the same course plan. Business school guidelines require students to attend the tutorial sessions and forbid them from switching between tutorial sections.

Panel A of Table 2 shows descriptive statistics for the sample we study. We observe 2903 students, about 38 percent of whom are female and whose average age is 19. Fifty-seven percent of students are German and 24 percent are Dutch. The business school's language of instruction is English.

A key feature of our environment is that the business school's scheduling department randomly assigns students to sections within each course. This assignment is done with scheduling software. Beginning with the 2010/11 academic year, the scheduling department additionally stratified section assignment by student nationality. After the initial assignment, the schedulers manually switch students between sections to resolve any scheduling conflicts. For first-year courses, scheduling conflicts are very rare because all students take the same set of compulsory courses. There are three main reasons for scheduling conflicts. First, a student is taking another course at the same time. This may happen for students who are repeating their first-year courses or students who take voluntary elective courses. Second, a student is taking a language course at the same time. Third, a student has opted out of evening classes. Evening sections are scheduled from 6:30 p.m. until 8:30 p.m. and students can opt out of these classes by completing an online form. Unfortunately, we do not observe scheduling conflicts but only the final section assignment.

We remove observations for which the business school deviated from its standard scheduling procedure. Appendix A.2 details these exceptions and our other sample restrictions. We test in Section 2.3 whether the section assignment is as good as random in our estimation sample.

2.2. Explanatory variables and outcome variables

Panel B of Table 2 shows the explanatory variables and outcome variables we use in this paper. We report the summary statistics for these variables at the student-course level instead of at the student-level. This presentation of the summary statistics gives more weight to students observed more often, as does our empirical analysis.

Explanatory variables: Throughout the paper, the explanatory variables of interest are the GPAs of male and female section peers. To avoid the reflection problem (Manski, 1993), each student's GPA is constructed based on grades obtained

Table 2
Descriptive statistics.

	(1) N	(2) Mean	(3) SD	(4) Min	(5) Max
Panel A: Student Demographic Characteristics					
Female	2903	0.375	0.484	0	1
Dutch	2903	0.235	0.424	0	1
German	2903	0.565	0.496	0	1
Age	2788	19.21	1.462	16.19	30.95
Panel B: Explanatory and Outcome Variables					
GPA of female peers	14,292	6.874	0.820	2.375	9.625
GPA of male peers	14,292	6.682	0.580	4.518	9.750
Number of students in section	14,292	13.92	0.947	8	16
Course and Major Choices:					
Any Quantitative Elective	14,292	0.480	0.500	0	1
Fraction Quantitative Electives	14,292	0.171	0.377	0	1
Quantitative Major	14,292	0.304	0.460	0	1
Labor Market Outcomes:					
Working	7281	0.633	0.482	0	1
Hourly earnings (in €)	4550	18.70	14.35	0.003	178.10
Yearly earnings (in €1000)	5720	44.10	42.16	0.001	650
Weekly hours worked	4797	47.93	12.17	2	100
Job satisfaction	4856	8.102	1.459	1	10
Subjective social impact	4867	0.639	2.701	−5	5
Students Grades and Course Evaluations:					
Course Dropout	14,292	0.0264	0.160	0	1
Course Grade	13,915	6.459	1.659	1	10
Self-reported Study Hours	5432	12.04	7.650	0	70
Subjective Overall Course Quality	5758	7.271	1.738	1	10
"My tutorial group has functioned well."	4850	3.860	0.971	1	5
"Working in tutorial groups with my fellow-students helped me to better understand the subject matter."	4872	3.978	0.940	1	5

NOTE — This table is based on our estimation sample. All explanatory and outcome variables are reported at the student-course level.

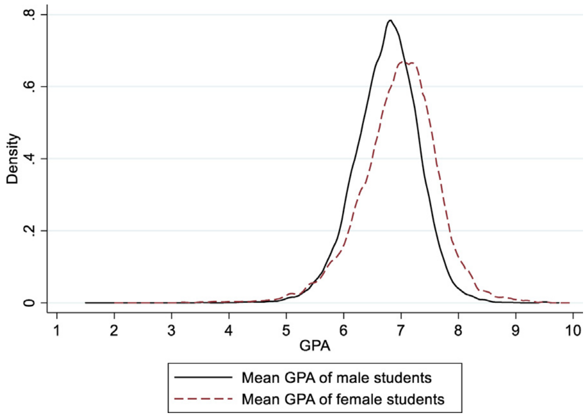

Fig. 1. Distribution of the section-level mean GPA of male and female students.

before they are assigned to a section. Male peer GPA and female peer GPA are the leave-out means of the pre-assignment GPAs, that is, they are the average GPAs of all male or female students in a given section except for the student in question. Because peer GPAs consist of pre-assignment grades, we cannot construct these measures of peer achievement for the courses that students take in the first teaching period. Our analysis therefore relies on the six compulsory courses students take in the second, third, and fourth teaching periods of their first year.

The average GPA of female peers is 6.9 on a 10-point scale, which is significantly higher than the 6.7 average GPA of male peers. This difference reflects that, at this business school, as in many other educational environments, women outperform men academically. Fig. 1 shows the average section-level GPA of male and female students, which provides us with an idea of the underlying achievement variation in the peer composition.

Table 3
Information about majors.

Major	(1) Major Classification	(2) Percent Compulsory Mathematical Courses in Major	(3) Percent Female	(4) First-Year GPA (Female)	(5) First-Year GPA (Male)	(6) Mean Annual Earnings in Thousand € (Female)	(7) Mean Annual Earnings in Thousand € (Male)
Finance	Mathematical major	50	18.28	7.31	7.17	53.20	55.17
IT Management	Mathematical major	50	31.11	6.97	6.63	39.86	45.09
Economics	Mathematical major	50	32.82	7.30	7.16	38.20	45.62
Supply Chain Mgmt	Non-mathematical major	25	52.09	6.97	6.56	36.25	42.86
Strategy	Non-mathematical major	0	37.72	6.94	6.55	38.15	36.86
Accounting	Non-mathematical major	0	40.66	7.34	7.20	40.96	45.16
Organization	Non-mathematical major	0	59.74	6.84	6.71	32.39	50.93
Marketing	Non-mathematical major	0	62.66	6.74	6.63	28.12	39.68

NOTE — This table is based on our estimation sample. Mean earnings by gender in columns (6) and (7) are taken from the graduate survey described below.

Outcomes variables—course and major choices: Our main academic outcomes are students' choices of mathematical courses and majors. Seventeen percent of all elective course observations are mathematical courses, which include mathematical electives and major-specific mathematical compulsory courses.

Each major consists of four major-specific compulsory courses. We define a major as mathematical if at least half of its compulsory courses are mathematical. This approach leaves us with three mathematical majors (Finance, IT Management, and Economics) and five non-mathematical majors (Strategy, Accounting, Supply-Chain Management, Organization, and Marketing). Students are free to choose any major, as there are no GPA requirements. Table 3 shows additional information on all eight majors. It shows that women are less likely to choose mathematical majors and that mathematical majors are associated with higher earnings.

Outcomes variables—labor market outcomes: In 2016, we gathered data on students' labor market outcomes by sending a survey to students who graduated between September 2010 and September 2015. From this survey, we use six outcomes: (1) a dummy variable indicating whether a person is employed (full-time employed, part-time employed, or self-employed); (2) hourly earnings; (3) yearly earnings from main job in euro before taxes; (4) weekly hours worked; (5) job satisfaction; (6) the subjective social impact of the job. We measured job satisfaction using the question, "*How satisfied are you, all in all, with your current work?*" Responses are based on a 10-point scale, with 10 being "most satisfied." We measured subjective social impact using the question, "*What do you think is the social impact of your current work?*" Here, responses are based on an 11-point scale, ranging from –5 *"very negative"* to +5 *"very positive,"* with 0 being *"no impact*." Panel B of Table 2 shows average earnings in our sample are 44,100 euro per year and respondents work, on average, 48 h per week. Average job satisfaction is 8.1 points, and the average social impact of the job is 0.6 points. The graduate survey response rate was 35 percent, and the probability that we observe students in the labor market—meaning they answered the survey and work—is unrelated to our peer variables of interest (see Table A2 in the Appendix).

Outcomes variables—students' outcomes in first-year course: To explore whether peers affect students in first-year courses, we also estimate effects of peer achievement on course dropout, grades, study effort, and their responses to the course evaluation survey. Students are counted as dropping out of a course if they are registered but we do not observe their final grades. Table 2 shows that the dropout rate in our sample is 2.6 percent. Students' grades in the first-year courses we study are exclusively based on the final exam. The business school uses the Dutch grading scale, which ranges from 1 to 10, with 5.5 being the lowest passing grade. Table 1 shows that the average grade is 6.5.

We use students' responses to the course evaluation survey to measure study effort, course satisfaction, and the satisfaction with peer interactions. The course evaluation survey was sent out at the end of each course but before students took the final exam. From the course evaluation survey, we obtain three variables of interest: (1) self-reported study hours per week, excluding contact hours; (2) subjective overall course quality on a 10-point scale, with 10 being very good; and (3) a quality of peer interaction index as the average of the standardized value of the two evaluation items: "*My tutorial group has functioned well*" and "*My fellow students helped me to better understand the subject matter*." The average reported weekly study hours per course are 12 h and the average rated course quality is 7.3 points. The survey response rate is 41.96 percent, and the probability of responding is unrelated to our peer variables of interest (see Table A2 in the Appendix).

2.3. Randomization check

To confirm that the peer composition is random, we test whether students' "pretreatment" characteristics, i.e., previous GPA, age, and the rank of the student ID — a proxy for a student's tenure at the business school — systematically relate to the GPA of their male and female peers as well as the proportion of their section peers that are female.

We implement these tests by regressing each peer variable (GPA of female peers, GPA of male peers, proportion of female peers) separately on one pretreatment characteristic (students' own GPA, age, rank of ID) and a set of fixed effects. The fixed effects are either course-year fixed effects or course-year fixed effects *and* parallel-course-year fixed effects, that is, fixed

Table 4
Test for random assignment-regression of peer gpa and gender on student pre-treatment characteristics.

Panel A: Women Dependent Variable:	(1) Std. GPA of Female Peers	(2) Std. GPA of Male Peers	(3) Std. GPA of Female Peers	(4) Std. GPA of Male Peers	(5) Proportion Female Peers
Std. GPA	0.0702	0.0048	0.0587	0.0052	0.0011
	(0.058)	(0.014)	(0.059)	(0.014)	(0.002)
Age	−0.0114	−0.0147	−0.0107	−0.0162	0.0003
	(0.012)	(0.010)	(0.012)	(0.010)	(0.002)
ID rank	0.0014	0.0024	0.0010	.0024	0.0002
	(0.004)	(0.003)	(0.004)	(0.003)	(0.001)
Observations	5517	5517	5504	5504	5517
Course-year FE	YES	YES	YES	YES	YES
Parallel Course-year FE	NO	NO	YES	YES	YES
Panel B: Men					
Std. GPA	0.0124	−0.0208	0.0150	−0.0314	−0.0001
	(0.013)	(0.034)	(0.013)	(0.035)	(0.002)
Age	−0.0014	−0.0098	−0.0005	−0.0091	0.0016
	(0.008)	(0.008)	(0.008)	(0.008)	(0.001)
ID rank	−0.0036	0.0022	−0.0037	0.0020	0.0001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.000)
Observations	8775	8775	8732	8732	8775
Course-year FE	YES	YES	YES	YES	YES
Parallel Course-year FE	NO	NO	YES	YES	YES

NOTE — Each cell in this table is estimated with a separate ordinary least squares regression including course-year fixed effects. ID rank is the rank of student ID divided by 1000 and serves as a proxy for students' tenure at the university. Regression estimates shown in columns (3), (4), and (5) additionally include parallel course–year fixed effects. Following the Guryan et al. (2009) correction method, we control for the leave-out mean of the peers' GPA at the course level when relating students' GPA to the GPA of their same gender peers (Panel A: specification reported in row 1, columns (1) and (3); Panel B: specifications reported in row 1, columns (2) and (4)). Robust standard errors clustered at the student level are in parentheses.

effects for the other course the students take in the same period. In specifications where we relate students' own GPA with the GPA of their same gender peers, we additionally control for the course-level leave-out mean of GPA. This approach was introduced by Guryan et al. (2009) who show that it accounts for the mechanical relationship between own- and peer-level variables. As in our main analysis, we perform these randomization tests separately for women and men.

Table 4 confirms that all three pre-treatment characteristics are unrelated to our peer variables of interest. All point estimates are small and none of the coefficients of interest are statistically significant.

We additionally perform a more flexible randomization check by testing whether section dummies predict the pre-treatment characteristics GPA, age, gender and student ID rank. For each course in our sample, we first regress one pre-treatment characteristic on section dummies and balancing indicators (dummies for student nationality and parallel course–year fixed effects), and then run an *F*-test for joint significance of the section dummies. This means we run 48 regressions for each of the pre-treatment characteristics. Under conditional random assignment, the *p*-values of the *F*-tests of these regressions should be uniformly distributed with a mean of 0.5 (Murdoch et al., 2008). Furthermore, if students are randomly assigned to sections within each course, the *F*-tests should reject the null hypothesis of no relation between section assignment and students' pre-treatment characteristics at the 5 percent and 1 percent significance levels for 5 percent and 1 percent of the cases, respectively. Table A1 and Fig. A1 in the Appendix confirm these predictions. Consistent with random assignment, we see that the *p*-values of the *F*-tests are roughly uniformly distributed, with means close to 0.5 and rejection rates close to the expected levels for each pre-treatment characteristic.

3. Empirical strategy

3.1. Estimating the effect of peer achievement

To understand how peer achievement affects students' specialization choices and labor market outcomes, we use ordinary least squares (OLS) regressions to estimate the following model:

$$y_{i\tau} = \alpha_1 \overline{Male\ GPA}_{ict-1} + \beta_1 \overline{Female\ GPA}_{ict-1} + \gamma' X + u_{ic\tau}, \tag{1}$$

where $y_{i\tau}$ is the course choice, major choice, or a labor market outcome of student i at time $\tau > t$, that is, after having taken the compulsory course c at time t, where she was exposed to a given group of section peers. We have two independent variables of interest. $\overline{Male\ GPA}_{ict-1}$ is the average of the GPAs of all male section peers based on all courses students took at time *t-1*, that is, before the start of the course. Analogously, $\overline{Female\ GPA}_{ict-1}$ is the average of the pre-assignment GPAs of all female section peers. Each student's pre-assignment GPA consists of all grades achieved before the start of the course, and therefore neither male nor female peer GPA contains any contemporaneous grades. X is a vector of control variables that includes course-year fixed effects and parallel course–year fixed effects, which are fixed effects for the other courses the students take in the same teaching period. X also includes students' own pre-assignment GPA as well as indicators for their nationality. $u_{ic\tau}$ is the error term.

The parameters of interest are α_1 and β_1. Parameter α_1 is the causal effect of a student's assignment to higher-GPA male peers, and β_1 is the causal effect of assignment to higher-GPA female peers on the outcome of interest.

Throughout, we estimate Eq. (1) separately for women and men. This approach allows the coefficients of interest as well as the control coefficients to vary by gender. We estimate Eq. (1) at the student-by-course level, where one observation refers to one student in one first-year course. This level of analysis allows us to include course fixed effects, which are required to obtain unbiased estimates of the impact of peers. We observe only one major choice for each student even though each student appears multiple times in our dataset with different peer groups. We take this data structure into account by clustering standard errors at the student level. In robustness checks, we also estimate models with one observation per student for which we cannot include course fixed effects.

For all estimates that rely on survey outcomes, we account for systematic differences in survey responses based on observable characteristics following the inverse probability weighting of Wooldridge (2007). Specifically, we first estimate the probability of observing students' labor market outcomes and answering the teaching evaluation survey (see Table A2 in the Appendix). We then winsorize these predicted probabilities at the 1st and 99th percentile of all positive predicted values. Finally, we estimate the effect of peer achievement on survey outcomes by weighting each observation by the inverse of the winsorized predicted response probabilities. To simplify the interpretation of our results, we standardize male peer GPA and female peer GPA over the estimation sample to have means of zero and standard deviations of one.

3.2. Adjusting for bias caused by measurement error

One might be concerned that peer ability instead of peer GPA drives peer effects, and peer GPA measures peer ability with some error. The direction of the bias caused by classical measurement error depends on how peers are assigned to groups. We have shown in Feld and Zölitz (2017) that if peer group assignment is non-random, classical measurement error in the peer variable of interest can lead to substantial overestimation of peer effects. If assignment to peer groups is random, classical measurement error will bias peer effects estimates toward zero. Since students in our setting are randomly assigned to sections, our estimates are likely attenuated.

We are not the only ones who face this problem. Many peer effects studies exploit random assignment to peer groups, and all studies have to rely on noisy ability measures. Furthermore, the degree of attenuation bias can contribute to variation in peer effects estimates, which may be confused with variations in true effect sizes.

We have shown in Feld and Zölitz (2017) that the magnitude of attenuation bias in the case of random assignment and classical measurement error is proportional to the test reliability of the ability measure (Feld and Zölitz, 2017; page 417). In our case this test-reliability is equal to $\frac{Var(a_i{}^*)}{Var(GPA_{it})}$, where a_i^* is student i's time-constant unobserved ability. OLS estimates of α_1 and β_1 are therefore attenuated by a factor of $\frac{Var(a_i^*)}{Var(GPA_{it})}$, and we can adjust for this bias by dividing our coefficients of interest by a measure of the test reliability of GPA.

We can estimate this test reliability with the help of one simplifying assumption: each grade consists of students' unobserved and time-constant ability (a_i^*) and an error term (ε_{it}), which is random and has the same variance for all grades. Building on this assumption, we can estimate the test reliability of GPA in three steps:

Step 1: Regress the grades of any two courses on each other and save the slope coefficient. This slope coefficient is equal to

$$\hat{\beta}_1 = \frac{Cov(grade2_{it},\ grade1_{it})}{Var(grade1_{it})}$$
$$\hat{\beta}_1 = \frac{Cov\left(a_i^* + \varepsilon_{it},\ a_i^* + \varepsilon_{it}\right)}{Var(grade1_{it})}$$
$$\hat{\beta}_1 = \frac{var\left(a_i^*\right)}{Var(grade1_{it})}\ . \tag{2}$$

Note that the covariance of the random terms is equal to zero in expectations, which leaves us with the variance of unobserved ability in the numerator in Eq. (2).

Step 2: Estimate the variance of students' unobserved ability by multiplying the slope coefficient (step 1) with the variance of any student grade.

$$\hat{\beta}_1 * Var(grade1_{it}) = \frac{Var(a_i^*)}{Var(grade1_{it})} * Var(grade1_{it})$$
$$\hat{\beta}_1 * Var(grade1_{it}) = Var(a_i^*) \quad . \tag{3}$$

Step 3: Estimate test reliability of GPA by dividing the estimate of the variance of students' unobserved ability (step 2) by the variance of GPA ($\frac{Var(a_i^*)}{Var(GPA_{it})}$).

Applying these steps to our setting, we estimate the test reliability of GPA separately for women and men as follows. First, we randomly select two first-year grades for each student (without replacement) and then regress the first grade on the second grade separately for women and men. We carry out this procedure 100 times and calculate the average slope coefficients, which are 0.502 for women and 0.510 for men. Second, we multiply these average slope coefficients by the variance of all grades of women (2.710) and the variance of all grades of men (2.780), respectively. The resulting estimates of the variance of the unobserved ability are 1.383 for women and 1.395 for men. Finally, we divide these estimates of the variance of unobserved ability by the variances of GPA (for women: 1.466, for men: 1.445). The resulting estimates of the test reliability of GPA are 0.943 for women and 0.966 for men. We use these estimates to adjust for attenuation bias by dividing the female peer GPA coefficients by 0.943 and the male peer GPA coefficients by 0.966.

This adjustment relies on the simplifying assumption that each grade consists of the same time-constant ability and a random error term with the same variance. Reality is more complex. Students grades likely also capture course-specific ability and the error term may have different variances for different grades. We therefore see our adjustment as a rough first step to solving a problem that has mostly been ignored in the peer effects literature.

4. Results

4.1. Effects on choice of mathematical majors and courses

Table 5 shows having higher-achieving male or female peers is not significantly related to students' probability of choosing a mathematical major. None of the four point estimates is statistically significant at the 5 percent level and all of them are small. For example, one point estimate suggests that being in one course with male peers who have one standard deviation higher GPAs reduces women's likelihood of choosing a mathematical major by only 0.82 percentage points. The 95 percent confidence interval of this estimate allows us to rule out effects smaller than –2.01 and larger than +0.37 percentage points. When we adjust this coefficient for attenuation bias by dividing it by 0.943 (our measure of the test reliability of GPA for women), the estimated effect increases to 0.87 percentage points. The equivalent coefficient for men is estimated with similar precision. It suggests that having male peers with one standard deviation higher GPAs increases men's likelihood of choosing a mathematical major by 0.26 percentage points. This point estimate increases to 0.27 percentage points if we adjust for attenuation bias by dividing by 0.966 (our measure of test reliability of GPA for men).

The estimated effects of having higher-achieving male or female peers on the probability of choosing a mathematical elective (column 2) or the fraction of mathematical electives (column 3) are also statistically insignificant and small. For women and men, all point estimates suggest that the effect of having male or female peers with one standard deviation higher GPAs on choosing any mathematical elective is smaller than 0.8 percentage points in absolute terms. The equivalent point estimates on the fraction of mathematical electives are all smaller than 0.5 percentage points in absolute terms. Taken together, we see little evidence that higher-achieving female or male peers affect students' specialization choices.

We conducted two robustness checks. First, we estimate specifications identical to those shown in Table 5 except that they additionally include a control variable for the proportion of section peers in a given course that are female. These specifications lead to almost identical results (see Table A3 in the Appendix). Second, we estimate models with observations at the student level (see Table A4 in the Appendix). In these specifications, our main independent variables are the average pre-assignment GPAs of all male or female peers that students had in teaching periods 2–4 (there is no pre-assignment GPA for teaching period 1). In each specification, we then regress for each student one outcome variable (e.g., mathematical major) on two independent variables of interest (male/female GPA of all peers over periods 2–4). We also control for cohort-study-program fixed effects (e.g., Business-2012) and dummy variables for student nationality (Dutch and German). In these specifications, two out of 12 point estimates are statistically significant at the 5 percent level. These estimates suggest that men who had female peers with one standard deviation higher GPAs across all their first-year courses are 4.9 percentage points less likely to choose a mathematical elective and choose 2.3 percentage points fewer mathematical electives. These point estimates go in the same direction as their counterparts in our main specification. However, we interpret these results as merely suggestive because students were randomly assigned to sections at the course level and these specifications do not allow us to include course-year fixed effects.

We further explore heterogeneous results in three ways. First, we estimate our main results by replacing our peer GPA measures with the *proportion of top decile female peers*, that is, the proportion of all female peers that are in the top

Table 5
The effect of peer achievement on course and major choice.

Panel A: Women Dependent Variable:	(1) Mathematical Major	(2) Any Mathematical Elective	(3) Fraction Mathematical Electives
Std. GPA of Male Peers	−0.0082	0.0000	−0.0006
	(0.0061)	(0.0070)	(0.0027)
Std. GPA of Female Peers	0.0015	0.0012	0.0014
	(0.0059)	(0.0068)	(0.0026)
Observations	5504	5504	5504
R-squared	0.2221	0.2534	0.1168
Mean Dependent Variable	.1893	.3272	.1037
Panel B: Men			
Std. GPA of Male Peers	0.0026	0.0022	0.0010
	(0.0065)	(0.0067)	(0.0034)
Std. GPA of Female Peers	−0.0066	−0.0079	−0.0045*
	(0.0056)	(0.0052)	(0.0026)
Observations	8768	8768	8768
R-squared	0.1493	0.1778	0.0545
Mean Dependent Variable	.3757	.5765	.2142

NOTE — All columns are estimated with ordinary least squares regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$.

10 percent of the GPA distribution of female students in a given course, and, equivalently defined, the *proportion of top decile male peers*. These measures allow us to explore whether using average peer GPA hides effects of very high- achieving peers on specialization choices. Second, we estimate our results separately for students whose GPA is above and below the median in a given course. Third, we estimate our result separately for mathematical and non-mathematical compulsory courses.

Out of 60 point estimates of interest in our heterogeneity analysis, only three are statistically significant at the 5 percent level. Table A5 in the Appendix shows no systematic effect of being assigned to peers with top decile GPAs. Table A6 shows that men *whose own GPAs are above the median* are less likely to choose a mathematical major after being assigned to higher-achieving female peers. Finally, Table A7 shows that men who have been assigned to higher- achieving female peers *in non-mathematical compulsory courses* are less likely to choose at least one mathematical elective and choose fewer mathematical electives. Overall, we see little evidence for meaningful heterogeneity.

Two other concerns are that the absence of meaningful effects is driven by peers not mattering in our setting or educational choices not being sufficiently malleable. Four studies with data from the same environment allow us to rule out both of these concerns. In Feld and Zölitz (2017), we show that having higher-achieving peers improves students' grades and course evaluations; Golsteyn et al. (2021) show that peer personality affects student achievement. In Zölitz and Feld (2021), we show that the proportion of female peers increases women's likelihood of choosing a male-dominated major and decreases their likelihood of choosing a female-dominated major; effects for men go in the opposite direction. Elsner et al. (2021) show that having a higher rank amongst one's tutorial peers increases the likelihood of choosing follow-up courses and majors in the same subject.

Our estimated effect of higher-achieving male peers on women's specialization choices are not consistent with those found in previous studies that estimate the effect of high school peers. Cools et al. (2021) show that female students with high-achieving male peers receive lower math and science grades, are less likely to complete a bachelor's degree, and are more likely have a child before they turn 18. Mouganie and Wang (2020) show that the presence of high-achieving male peers reduces female students' likelihood of choosing a STEM major. Our results are also not consistent with Fischer (2017),

Table 6
The impact of peer achievement on labor market outcomes.

Panel A: Women	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Employed	Log Yearly Earnings	Log Working Hours	Log Hourly Wage	Job Satisfaction 8+	Positive Subjective Social Impact
Std. GPA of Male Peers	−0.0104	−0.0572	−0.0152	−0.0668	0.0418***	0.0105
	(0.0105)	(0.0474)	(0.0107)	(0.0528)	(0.0141)	(0.0106)
Std. GPA of Female Peers	−0.0080	0.0159	0.0005	0.0633**	0.0171	−0.0103
	(0.0091)	(0.0344)	(0.0061)	(0.0296)	(0.0113)	(0.0089)
Observations	2840	2175	1820	1726	1872	1872
R-squared	0.2878	0.1513	0.1780	0.0989	0.0614	0.5310
Mean Dependent Variable	0.6194	10.0955	45.2934	2.4988	.7025	0.5951
Panel B: Men						
Std. GPA of Male Peers	0.0117	0.0482	0.0039	0.0329	0.0131	0.0054
	(0.0087)	(0.0297)	(0.0093)	(0.0258)	(0.0099)	(0.0092)
Std. GPA of Female Peers	−0.0104	−0.0260	−0.0050	0.0190	0.0114	−0.0026
	(0.0069)	(0.0268)	(0.0064)	(0.0213)	(0.0090)	(0.0076)
Observations	4434	3562	2970	2824	3000	3000
R-squared	0.2626	0.0565	0.0898	0.0537	0.0440	0.4632
Mean Dependent Variable	0.6416	10.363	49.5552	2.725	.772	.5073

NOTE — All columns are estimated with ordinary least squares regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. Following Wooldridge (2007) we weight the observations by the inverse of the probability of observing the outcome. Robust standard errors clustered at the student level are in parentheses. Robust standard errors clustered at the student level are in parentheses. ** $p < 0.05$, *** $p < 0.01$.

who finds that female university students with higher-achieving peers in an introductory STEM course are less likely to major in a STEM field.

4.2. Effects on longer-term outcomes

Table 6 shows how peer achievement affects women's and men's longer-term outcomes. For women, we see no significant effects on the probability of being employed, yearly earnings, or weekly hours worked. We do see significant effects for hourly wages. Women who in one course had female peers with one standard deviation higher GPAs earn 6.3 percent more per hour (6.9 percent adjusted for attenuation bias). However, this estimate is large and imprecise, which raises concerns that it might have been as result of chance. Indeed, the statistical significance vanishes ($p = 0.54$) once we adjust for the fact that we test 36 hypotheses using the Westfall-Young stepdown procedure with 1000 bootstrap replications (Westfall et al., 1993; Reif, 2017).[1] For men, we see no significant peer effects on the probability of being employed, earnings, weekly hours worked, or hourly wage, but our coefficients are imprecisely estimated. Overall, we lack sufficient statistical power to draw any conclusions on the effect of peer achievement on women's or men's employment status, earnings, or weekly hours worked.

The best evidence on the effect of peer achievement on earnings comes from Fischer et al. (2021), who observe earnings in Danish registry data for up to 25 years after enrollment. They find that being assigned to one standard deviation higher-achieving peers reduces women's earnings by 4 percent on average; this effect is driven by exposure to higher-achieving male peers and becomes stronger as graduates get older. We cannot directly compare our point estimates with the estimates

[1] We have considered nine outcomes of interest (three specialization choices and six longer-term outcomes). For each of these outcomes, we test four hypotheses (two for women and two for men) which adds up to 36 hypotheses.

Table 7
The effect of peer GPA on first-year outcomes.

Panel A: Women Dependent Variable:	(1) Dropout	(2) Std Grade	(3) Study Hours	(4) Std. Course Evaluation	(5) Std. Peer Interaction
Std. GPA of Male Peers	−0.0016	0.0109	−0.1138	0.0096	0.0881***
	(0.0018)	(0.0108)	(0.1926)	(0.0230)	(0.0297)
Std. GPA of Female Peers	0.0008	0.0044	−0.2483	−0.0295	0.0146
	(0.0020)	(0.0113)	(0.1683)	(0.0193)	(0.0242)
Observations	5504	5380	2350	2455	2066
R-squared	0.0864	0.5396	0.0779	0.1402	0.0867
Panel B: Men					
Std. GPA of Male Peers	0.0000	0.0145	0.0475	0.0376**	0.0841***
	(0.0020)	(0.0092)	(0.1803)	(0.0188)	(0.0243)
Std. GPA of Female Peers	−0.0015	−0.0036	0.0851	−0.0177	0.0105
	(0.0015)	(0.0077)	(0.1312)	(0.0166)	(0.0173)
Observations	8768	8514	3067	3286	2765
R-squared	0.0718	0.5098	0.0738	0.1493	0.0683

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. Following Wooldridge (2007), in columns (3)–(5), we weight the observations by the inverse of the probability of observing the outcome. Outcome variables in columns (2), (4), and (5) are standardized to have a mean of zero and standard deviation of one for our estimation sample. Robust standard errors clustered at the student level are in parentheses. ** $p < 0.05$, *** $p < 0.01$.

by Fischer et al. (2021) because standardized peer achievement is not comparable across settings. However, the direction of our point estimates also suggest that women earn less after being exposed to higher-achieving male peers.

Table 6 further shows the estimates of having higher-achieving peers on job satisfaction and subjective social impact of one's job. We have coded both measures as binary variables. *High Job Satisfaction* is equal to one if respondents rated their job satisfaction as 8 points or higher, and zero otherwise. *Positive Social Impact* is equal to one if respondents rate the social impact of their job as positive and zero if they rate it as neutral or negative. The results show no significant effects on women's or men's positive social impact. However, we do see a positive and precisely estimated effect of having higher-achieving male peers on women's job satisfaction. Women who had one course with male peers who have one standard deviation higher GPAs are 4.2 percentage points more likely to report high job satisfaction (4.5 percentage points adjusted for attenuation bias). This point estimate is statistically significant at the 1 percent level and remains significant for women ($p = 0.035$) after applying the aforementioned multiple hypothesis testing correction.

One concern is that we lose valuable information with the binary coding of job satisfaction and social impact. Could we just use both variables in their original scales? This is not trivial because job satisfaction and social impact are measured on ordinal scales. Any effect on the job satisfaction score is difficult to interpret because we do not know, for example, how an increase in job satisfaction from 4 to 5 points compares to an increase from 9 to 10 points. When comparing two groups, the group with the higher average job satisfaction *score* may actually be less satisfied on average. This problem is called a sign reversal, and there is an open debate in the economics of happiness literature on how likely this is in practice. Bond and Lang (2019, p. 1638)) conclude that "it is essentially impossible to rank two groups on the basis of their mean happiness using the types of survey questions prevalent in the literature." In contrast, Kaiser and Vendrik (2020) argue that sign reversals are often implausible or impossible because, among other things, people typically interpret ordinal scales fairly linearly. When we use the ordinal, non-transformed, scale of job satisfaction and social impact as dependent variables, we see significant results for both outcomes. Appendix Table A8 suggests that women who had male peers with one standard deviation higher GPAs are 0.13 points (9 percent of a standard deviation) more satisfied and report that their job has a 0.13 points (5 percent of a standard deviation) more positive social impact. Only the effect on job satisfaction remains marginally significant after correcting for multiple hypothesis testing ($p = 0.046$). Taken together with our estimates using binary dependent variables, we interpret these results as suggestive evidence that the presence of higher-achieving male peers in university increases women's job satisfaction.

One way to interpret this result is that being with higher-achieving male peers may affect women's job satisfaction without meaningfully affecting their specialization choices. Students interact in many ways with their peers inside and outside of the classroom and some of these interactions may change their life trajectory. For example, some women may marry the higher-achieving male peers they met in their first-year courses. Having found a husband with a good earnings potential may make women feel more comfortable in pursuing a more satisfying career. This mechanism is consistent with the fact that many people find their spouse in college (Macskássy and Adamic 2021)). Furthermore, Fischer et al. (2021) find that women who are exposed to higher- achieving peers have their first child earlier.

Table 8
The effect of peer achievement on course and major choice.

Panel A: Women	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Mathematical Major		Any Mathematical Elective		Fraction Mathematical Electives	
Definition of Peer GPA	Overall	Math	Overall	Math	Overall	Math
Std. GPA of Male Peers	−0.0082	−0.0137**	0.0000	0.0003	−0.0006	−0.0021
	(0.0061)	(0.0068)	(0.0070)	(0.0081)	(0.0027)	(0.0031)
Std. GPA of Female Peers	0.0015	0.0066	0.0012	0.0044	0.0014	0.0029
	(0.0059)	(0.0068)	(0.0068)	(0.0077)	(0.0026)	(0.0029)
Observations	5504	5504	5504	5504	5504	5504
R-squared	0.2221	0.2227	0.2534	0.2535	0.1168	0.1171
Mean Dependent Variable	.1893	.1893	.3272	.3272	.1037	.1037
Panel B: Men						
Std. GPA of Male Peers	0.0026	0.0057	0.0022	0.0007	0.0010	0.0004
	(0.0065)	(0.0070)	(0.0067)	(0.0073)	(0.0034)	(0.0035)
Std. GPA of Female Peers	−0.0066	0.0007	−0.0079	−0.0072	−0.0045*	−0.0036
	(0.0056)	(0.0063)	(0.0052)	(0.0058)	(0.0026)	(0.0028)
Observations	8768	8768	8768	8768	8768	8768
R-squared	0.1493	0.1492	0.1778	0.1777	0.0545	0.0543
Mean Dependent Variable	.3757	.3757	.5765	.5765	.2142	.2142

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. In specifications shown in odd-numbered columns, the peer GPA variable is based on all previous courses. In specifications shown in even-numbered columns, the peer GPA variable is based on previously taken mathematical courses only. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$, ** $p < 0.05$.

4.3. Effects on first-year outcomes

Since our estimates on students' labor market outcomes are too imprecise to rule out meaningful effects, we look at peer effects in first-year courses to see how plausible such effects are. If we find meaningful peer effects in the first year, it is more plausible that peers also affect students' choices and longer-run outcomes. In contrast, finding no meaningful peer effects estimates in the first-year courses makes it less plausible that peers have meaningful impacts on students' futures.

Table 7 shows estimates of having higher-achieving male and female peers on women's and men's probability of dropping out of the course, grades, study hours, course evaluation, and peer interaction. The results look similar for women (Panel A) and men (Panel B). For both, we see only small and statistically insignificant estimates on course dropout, grades, and study hours. However, both judge the peer interaction around 0.08 standard deviations more positively (0.09 standard deviations adjusted for attenuation bias) after when being assigned to male peers with a one standard deviation higher GPA. Men also evaluate their course significantly more positively after being assigned to higher-achieving male peers. Taken together, we see that students do notice their peers and it is plausible that these moderate effects on course enjoyment lead to effects in the long run that we cannot detect in our sample.

5. Results based on peer GPA in math courses

One might be concerned that the effects we are trying to uncover are driven by the *math* ability of peers instead of their overall ability.[2] For example, peers that are particularly good in math may act as role models and inspire students of the same gender to choose a mathematical major. While the peer GPA based on all courses and the peer GPA based on only math courses are strongly correlated ($r = 0.70$ for women, $r = 0.70$ for men), we might see stronger peer effects by focusing on math GPA if this is indeed a better measure of the relevant peer ability.

In the related literature, there is no consensus on which type of peer ability matters for students' choices. In the paper most related to ours, Fischer et al. (2021) base their measure of peer ability on students' high school GPA, without distinguishing between mathematical and non-mathematical grades. In contrast, Mougani and Wang (2020) focus on peers' ability in mathematical courses: they use the share of male and female students who scored in the top 20 percent on a standardized math test in China. They also show that basing their measure of peer ability on non-mathematical subjects (e.g., political science) leads to different results. Fischer (2017) and Cools et al. (2021) do not have direct measures of pre-assignment peer ability and therefore rely on more indirect measures. Fischer (2017) uses the share of peers who are on

[2] We thank an anonymous referee for this suggestion.

Table 9
The impact of peer achievement on labor market outcomes.

Panel A: Women	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent Variable:	Employed		Log Yearly Earnings		Log Working Hours		Log Hourly Wage		Job Satisfaction 8+		Positive Subjective Social Impact	
Definition of Peer GPA	Overall	Math	Overall	Math	Overall	Math	Overall	Math	Overall	Math	Overall	Math
Std. GPA of Male Peers	−0.0104	−0.0023	−0.0572	−0.0616	−0.0152	−0.0079	−0.0668	−0.0672	0.0418***	0.0416***	0.0105	0.0112
	(0.0105)	(0.0109)	(0.0474)	(0.0515)	(0.0107)	(0.0101)	(0.0528)	(0.0533)	(0.0141)	(0.0150)	(0.0106)	(0.0121)
Std. GPA of Female Peers	−0.0080	−0.0064	0.0159	0.0328	0.0005	0.0051	0.0633**	0.0650*	0.0171	0.0198	−0.0103	−0.0056
	(0.0091)	(0.0102)	(0.0344)	(0.0335)	(0.0061)	(0.0064)	(0.0296)	(0.0341)	(0.0113)	(0.0133)	(0.0089)	(0.0108)
Observations	2840	2840	2175	2175	1820	1820	1726	1726	1872	1872	1872	1872
R-squared	0.2878	0.2873	0.1513	0.1516	0.1780	0.1766	0.0989	0.0984	0.0614	0.0606	0.5310	0.5307
Mean Dependent Variable	0.6194	0.6194	10.0955	10.0955	45.2934	45.2934	2.4988	2.4988	.7025	.7025	0.5951	0.5951
Panel B: Men												
Std. GPA of Male Peers	0.0117	0.0082	0.0482	0.0468	0.0039	0.0015	0.0329	0.0290	0.0131	0.0130	0.0054	0.0029
	(0.0087)	(0.0088)	(0.0297)	(0.0327)	(0.0093)	(0.0079)	(0.0258)	(0.0280)	(0.0099)	(0.0101)	(0.0092)	(0.0102)
Std. GPA of Female Peers	−0.0104	−0.0109	−0.0260	−0.0324	−0.0050	−0.0040	0.0190	0.0251	0.0114	0.0106	−0.0026	−0.0033
	(0.0069)	(0.0077)	(0.0268)	(0.0385)	(0.0064)	(0.0077)	(0.0213)	(0.0302)	(0.0090)	(0.0094)	(0.0076)	(0.0084)
Observations	4434	4434	3562	3562	2970	2970	2824	2824	3000	3000	3000	3000
R-squared	0.2626	0.2623	0.0565	0.0565	0.0898	0.0896	0.0537	0.0536	0.0440	0.0436	0.4632	0.4631
Mean Dependent Variable	0.6416	0.6416	10.363	10.363	49.5552	49.5552	2.725	2.725	.772	.772	.5073	.5073

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. Following Wooldridge (2007), we weight the observations by the inverse of the probability of observing the outcome. In specifications shown in odd-numbered columns, the peer GPA variable is based on all previous courses as in Table 6. In specifications shown in even-numbered columns, the peer GPA variable is based on previously taken mathematical courses only. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10
The effect of peer GPA on first-year outcomes.

Panel A: Women	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable:	Dropout		Std Grade		Study Hours		Course Evaluation		Peer Interaction	
Peer GPA definition:	Overall	Math	Overall	Math	Overall	Math	Overall	Math	Overall	Math
Std. GPA of Male Peers	−0.0016	−0.0067**	0.0109	0.0042	−0.1138	−0.0012	0.0096	0.0259	0.0881***	0.0935***
	(0.0018)	(0.0029)	(0.0108)	(0.0118)	(0.1926)	(0.2296)	(0.0230)	(0.0249)	(0.0297)	(0.0325)
Std. GPA of Female Peers	0.0008	0.0031	0.0044	−0.0028	−0.2483	−0.3218	−0.0295	−0.0481**	0.0146	0.0149
	(0.0020)	(0.0024)	(0.0113)	(0.0123)	(0.1683)	(0.2197)	(0.0193)	(0.0213)	(0.0242)	(0.0276)
Observations	5504	5504	5380	5380	2350	2350	2455	2455	2066	2066
R-squared	0.0864	0.0879	0.5396	0.5395	0.0779	0.0780	0.1402	0.1415	0.0867	0.0863
Panel B: Men										
Std. GPA of Male Peers	0.0000	0.0015	0.0145	0.0106	0.0475	0.0430	0.0376**	0.0378*	0.0841***	0.1019***
	(0.0020)	(0.0024)	(0.0092)	(0.0104)	(0.1803)	(0.2152)	(0.0188)	(0.0196)	(0.0243)	(0.0257)
Std. GPA of Female Peers	−0.0015	−0.0006	−0.0036	−0.0007	0.0851	0.0655	−0.0177	−0.0293*	0.0105	0.0258
	(0.0015)	(0.0018)	(0.0077)	(0.0087)	(0.1312)	(0.1430)	(0.0166)	(0.0166)	(0.0173)	(0.0197)
Observations	8768	8768	8514	8514	3067	3067	3286	3286	2765	2765
R-squared	0.0718	0.0718	0.5098	0.5097	0.0738	0.0737	0.1493	0.1497	0.0683	0.0701

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel-course-year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. In specifications shown in odd-numbered columns, the peer GPA variable is based on all previous courses – as shown in Table 5. In specifications shown in even-numbered columns, the peer GPA variable is based on previously taken mathematical courses only. Following Wooldridge (2007), in columns (5)–(10), we weight the observations by the inverse of the probability of observing the outcome. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

track to graduate on time. Cools et al. (2021) use parents' education to proxy peer ability with the proportion of peers who have at least on parent with a post-college degree.

To see if the choice of peer ability measure matters, we re-estimate all our results with measures of peer GPA based on mathematical courses (peer math GPA). Table 8 shows the estimated effects of peer math GPA on educational choices. Table 9 shows the estimates on longer-run outcomes, and Table 10 shows the estimates on first-year outcomes. For comparison, we also include our original point estimates based on overall peer GPA in the odd-numbered columns.

Overall, the estimates look very similar across both types of specifications and the standard errors are slightly smaller when we use the overall peer GPA. Six out of 112 estimates are statistically significant at the 1 percent level. For these highly significant results, the peer achievement measures do not matter. Whether we use overall peer GPA or math peer GPA, we see that women who had higher-achieving male peers are more likely to report high job satisfaction, and both men and women evaluate peer interactions more positively in sections in which they were assigned to higher-achieving male peers. We see five more point estimates that are statistically significant at the 5 percent level, two for the overall and thee for math only peer GPA measure. Estimates based on the overall GPA suggest that women who had higher-achieving female peers earn more per hour, whereas men who had higher-achieving male peers evaluate their first-year courses more positively. In contrast, estimates based on math peer GPA suggest women who had higher-achieving male peers are less likely to choose a mathematical major and are more likely to drop out of first-year courses, and men who had higher-achieving female peers evaluate their courses more negatively.

When estimating many different specifications and trying to make sense of our results, we face a problem that is familiar to many researchers. It is not clear to what extent we see a meaningful pattern in the data and to what extent we engage in *p*-hacking and ex-post sense- making. Our reading of the overall results is that there is not much evidence that the measure of peer ability matters. However, we present all estimates next to each other so that readers can make up their own minds.

6. Conclusion

We have explored how having higher-achieving peers affects women's and men's educational choices and labor market outcomes. In contrast to previous studies, we see little evidence that peers affect students' educational choices. We have also shown suggestive evidence that women who have studied with higher-achieving male peers are more satisfied with their jobs. These results complement previous findings that have found several negative effects of studying with higher-achieving male peers on women's later-life outcomes. As peer effects on achievement, peer effects on choices also seem to be complex and context-dependent.Eq. (3)

Appendix

A.1 Additional tables

Table A1
Alternative randomization check.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Total Number of Courses	Number significant:			Percent significant:			Mean of p-value
		5%	1%	0.1%	5%	1%	0.1%	
Female	48	3	0	0	0.063	0.000	0.000	0.5347
GPA	48	4	1	0	0.083	0.021	0.000	0.4894
Age	48	4	1	0	0.083	0.021	0.000	0.4942
ID rank	48	0	0	0	0.000	0.000	0.000	0.6079

NOTE — This table is based on separate OLS regressions with female, GPA, age, and ID rank as dependent variables. The explanatory variables are a set of section dummies, dummies for the other courses taken at the same time, and dummies for being of German or Dutch nationality.

Table A2
Testing for attrition and selective survey response.

	(1)	(2)	(3)	(4)
Subsample:	Women		Men	
Dependent Variable:	Major Observed	Observed in Labor Market	Major Observed	Observed in Labor Market
Std. GPA of Male Peers	0.0022	0.0002	−0.0014	−0.0050
	(0.003)	(0.008)	(0.003)	(0.007)
Std. GPA of Female Peers	0.0035	−0.0049	0.0024	−0.0095*
	(0.003)	(0.007)	(0.003)	(0.005)
Dutch	0.0070	0.0148	0.0046	0.0790*
	(0.024)	(0.056)	(0.019)	(0.043)
German	0.0237	−0.0994**	0.0021	0.0075
	(0.020)	(0.049)	(0.016)	(0.037)
Pre-assignment GPA	0.0344***	0.0412***	0.0391***	0.0196*
	(0.008)	(0.014)	(0.006)	(0.011)
Observations	6089	5052	9411	7681
R-squared	0.601	0.077	0.518	0.083
Mean Dependent Variable Female Students	0.9762	.348	.	.
Mean Dependent Variable Male Students	.	.	.9738	.3661
p-value of Test for joint Significance of Peer Variables	.4385	0.8009	.5646	.1751

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. The dependent variable in column (1) and (3) is equal to one if we observe a student's major, and zero otherwise. The dependent variable in column (2) and (4) is equal to one if we observe the student in the labor market, that is, if the student has answered the alumni survey and indicated that they are part-time, full-time, or self-employed, and zero if they do not respond to the survey or they are not employed part-time, employed full-time, or self-employed, because they are still studying, for example. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3
Robustness-estimations controlling for the proportion female section peers.

Panel A: Women Dependent Variable:	(1) Mathematical Major	(2) Any Mathematical Elective	(3) Fraction Mathematical Electives
Std. GPA of Male Peers	−0.0080	0.0003	−0.0004
	(0.0061)	(0.0070)	(0.0026)
	0.0015	0.0012	0.0014
Std. GPA of Female Peers	(0.0059)	(0.0068)	(0.0026)
Observations	5504	5504	5504
R-squared	0.2221	0.2535	0.1170
Mean Dependent Variable Female Students	.1893	.3272	.1037
Panel B: Men			
Std. GPA of Male Peers	0.0025	0.0021	0.0010
	(0.0064)	(0.0067)	(0.0034)
Std. GPA of Female Peers	−0.0065	−0.0078	−0.0045*
	(0.0056)	(0.0052)	(0.0026)
Observations	8768	8768	8768
R-squared	0.1494	0.1781	0.0545
Mean Dependent Variable Male Students	.3757	.5765	.2142

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, share of female peers, individual students' pre-assignment GPA, and indicators for being Dutch or German. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$.

Table A4
Robustness-estimations with one observation per student.

Panel A: Women Dependent Variable:	(1) Mathematical Major	(2) Any Mathematical Elective	(3) Fraction Mathematical Electives
Std. GPA of Male Peers	−0.0215	−0.0143	−0.0058
	(0.015)	(0.018)	(0.007)
Std. GPA of Female Peers	0.0034	−0.0161	−0.0021
	(0.016)	(0.019)	(0.007)
Observations	985	985	985
R-squared	0.192	0.239	0.084
Mean Dependent Variable Female Students	.1898	.331	.1057
Panel B: Men			
Std. GPA of Male Peers	−0.0133	−0.0033	−0.0011
	(0.016)	(0.016)	(0.008)
Std. GPA of Female Peers	−0.0201	−0.0362**	−0.0183**
	(0.017)	(0.015)	(0.008)
Observations	1603	1603	1603
R-squared	0.082	0.168	0.028
Mean Dependent Variable Male Students	.3706	0.577	.2128

NOTE — All columns are estimated with OLS regressions that include cohort-study-program fixed effects and indicators for being Dutch or German. Heteroskedasticity robust standard errors are in parentheses. ** $p < 0.05$.

Table A5
Effect of female and male top GPA decile peers.

Panel A: Women Dependent Variable:	(1) Mathematical Major	(2) Any Mathematical Elective	(3) Fraction Mathematical Electives
Proportion Top 10 Male Peers	−0.0081	0.0475	0.0192
	(0.104)	(0.120)	(0.046)
Proportion Top 10 Female Peers	0.0731	0.0922	0.0114
	(0.108)	(0.129)	(0.059)
Observations	5504	5504	5504
R-squared	0.222	0.254	0.117
Mean Dependent Variable Female Students	.1893	.3272	.1037
Panel B: Men			
Proportion Top 10 Male Peers	0.0092	−0.0401	−0.0256
	(0.093)	(0.092)	(0.047)
Proportion Top 10 Female Peers	−0.2386*	−0.0776	−0.1035
	(0.122)	(0.128)	(0.065)
Observations	8768	8768	8768
R-squared	0.150	0.178	0.054
Mean Dependent Variable Male Students	.3757	.5765	.2142

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. The proportion of top 10 peers are based on course- level GPA distributions. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$.

Table A6
Heterogeneous effects by student GPA.

Dependent Variable:	(1) Mathematical Major	(2)	(3) Any Mathematical Elective	(4)	(5) Fraction Mathematical Electives	(6)
Panel A: Women	Below median GPA students	Above median GPA students	Below median GPA students	Above median GPA students	Below median GPA students	Above median GPA students
Std. GPA of Male Peers	−0.0011	−0.0135	0.0055	−0.0027	0.0012	−0.0016
	(0.007)	(0.009)	(0.009)	(0.010)	(0.003)	(0.004)
Std. GPA of Female Peers	−0.0043	0.0053	−0.0112	0.0077	−0.0048	0.0046
	(0.009)	(0.007)	(0.009)	(0.009)	(0.004)	(0.003)
Observations	2124	3374	2124	3374	2124	3374
R-squared	0.224	0.230	0.405	0.186	0.209	0.086
Mean Dependent Variable Female Students	.1224	.2318	.2764	.3592	.0786	.1195
p-values for Test of Gender Equality of GPA Male Peers	.8847	.1125	.5332	.7779	.6942	.6933
p-values for Test of Gender Equality of GPA Female Peers	.6191	.4835	.2351	.3905	.2108	.1802
Panel B: Men						
Std. GPA of Male Peers	0.0059	−0.0028	−0.0063	0.0088	−0.0020	0.0027
	(0.009)	(0.009)	(0.010)	(0.009)	(0.004)	(0.005)
Std. GPA of Female Peers	0.0043	−0.0156**	−0.0102	−0.0064	−0.0065*	−0.0035
	(0.008)	(0.008)	(0.008)	(0.007)	(0.004)	(0.004)
Observations	3856	4903	3856	4903	3856	4903
R-squared	0.079	0.128	0.236	0.122	0.042	0.047
Mean Dependent Variable Male Students	.2417	.4817	0.5005	.6368	.17	.2493
p-values for Test of Gender Equality of GPA Male Peers	.5058	.7581	.5178	.3386	0.6396	.5866
p-values for Test of Gender Equality of GPA Female Peers	.5898	.04	.1728	.3895	.0852	.3341

NOTE — This table shows OLS regression estimates that are separately estimated for students whose GPA was above the median and students whose GPA was below the median of the course-specific GPA distribution. Additional controls include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. Robust standard errors clustered at the student level are in parentheses. * $p < 0.1$, ** $p < 0.05$.

Table A7
Heterogeneity by first year course type (Math vs. Non-mathematical courses).

Dependent Variable:	(1) Mathematical Major	(2)	(3) Any Mathematical Elective	(4)	(5) Fraction Mathematical Electives	(6)
Panel A: Women	Math course	Non-Math Course	Math course	Non-Math Course	Math course	Non-Math Course
Std. GPA of Male Peers	−0.0100	−0.0060	0.0027	−0.0023	−0.0015	0.0003
	(0.0071)	(0.0081)	(0.0084)	(0.0090)	(0.0035)	(0.0035)
Std. GPA of Female Peers	0.0057	−0.0027	−0.0001	0.0025	0.0018	0.0011
	(0.0073)	(0.0072)	(0.0081)	(0.0083)	(0.0032)	(0.0032)
Observations	2779	2706	2779	2706	2779	2706
R-squared	0.2207	0.2221	0.2516	0.2508	0.1094	0.1198
Mean Dependent Variable	.1882	.1911	.3267	.3267	.3267	.3267
Panel B: Men						
Std. GPA of Male Peers	0.0094	−0.0041	0.0048	−0.0006	0.0033	−0.0015
	(0.0077)	(0.0079)	(0.0081)	(0.0081)	(0.0041)	(0.0041)
Std. GPA of Female Peers	−0.0034	−0.0104	−0.0041	−0.0134**	−0.0030	−0.0066**
	(0.0064)	(0.0071)	(0.0058)	(0.0067)	(0.0030)	(0.0033)
Observations	4452	4293	4452	4293	4452	4293
R-squared	0.1508	0.1495	0.1780	0.1768	0.0544	0.0512
Mean Dependent Variable	.3735	.3792	.5728	0.5809	0.5809	0.5809

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. Robust standard errors clustered at the student level are in parentheses. ** $p < 0.05$.

Table A8
Non-transformed job satisfaction and social impact.

Panel A: Women Dependent Variable:	(1) Job Satisfaction	(2) Social Impact
Std. GPA of Male Peers	0.1260***	0.1277***
	(0.0438)	(0.0444)
Std. GPA of Female Peers	0.0036	−0.0257
	(0.0339)	(0.0404)
Observations	1860	1872
R-squared	0.0659	0.6375
Panel B: Men		
Std. GPA of Male Peers	0.0559	0.0308
	(0.0341)	(0.0444)
Std. GPA of Female Peers	0.0457	−0.0765**
	(0.0329)	(0.0356)
Observations	2989	2988
R-squared	0.0502	0.5989

NOTE — All columns are estimated with OLS regressions that include course-year fixed effects, parallel course–year fixed effects, individual students' pre-assignment GPA, and indicators for being Dutch or German. Following Wooldridge (2007), for all specifications, we weight the observations by the inverse of the probability of observing the outcome. Robust standard errors clustered at the student level are in parentheses. ** $p < 0.05$, *** $p < 0.01$.

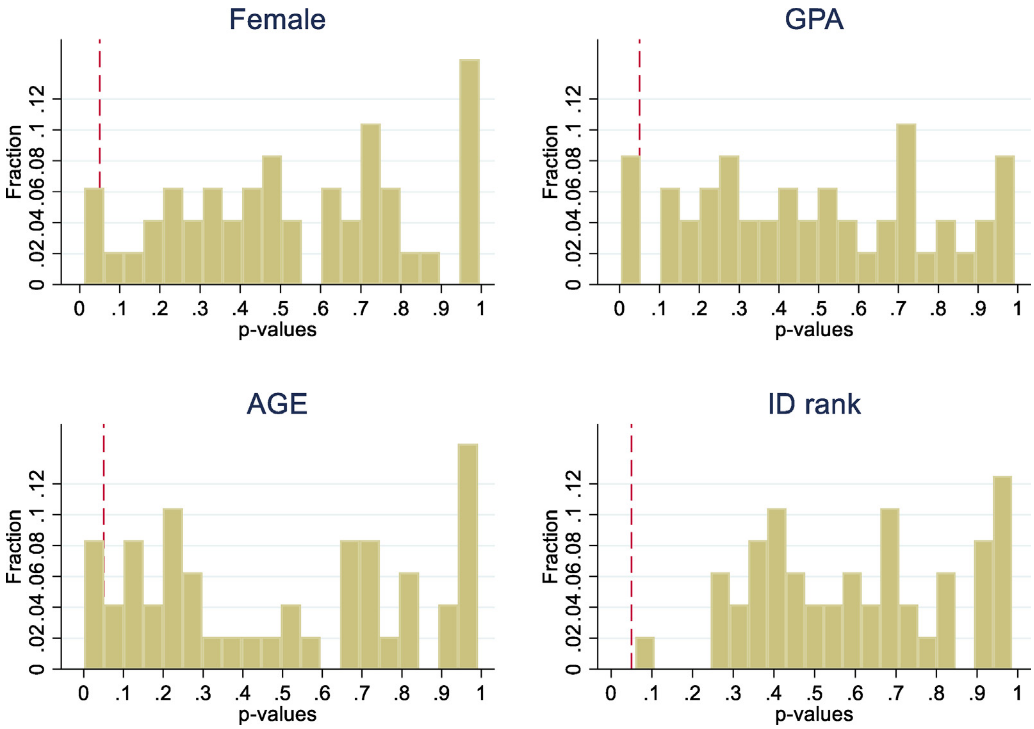

Fig. A1. Alternative randomization check-distributions of *p*-values.
NOTE — These are histograms showing the distribution of *p*-values from all the regressions reported in Table A1. The vertical line in each histogram shows the 5 percent significance level.

A.2 Sample restrictions

Our sample period comprises the academic years 2009/2010 through 2014/2015. We derive our estimation sample in three steps. First, we exclude several observations from our estimation sample because they represent exceptions from the standard section assignment procedure. These exceptions are the same as those documented in Feld et al. (2020) and Zölitz and Feld (2021), who use data from the same environment and sample period.

- We exclude eight courses in which the course coordinator or other education staff actively influenced the section composition. One course coordinator requested to balance student gender across sections. The business school's scheduling department informed us about these courses.
- We exclude 21 sections from the analysis that consisted mainly of students who registered late for the course. Before April 2014, the business school reserved one or two slots per section for students who registered late. In exceptional cases in which the number of late-registration students substantially exceeded the number of empty spots, new sections comprised mainly of late-registering students were created. The business school abolished the late registration policy in April 2014.
- We exclude 46 repeater sections from the analysis. One course coordinator explicitly requests that students who failed his/her courses in the previous year be assigned to special repeater sections.
- We exclude 17 tutorial groups that consisted mainly of students from a special research-based program. For some courses, students in this program were assigned together to separate tutorial groups with a more experienced teacher.
- We exclude 95 part-time MBA students because these students are typically scheduled for special evening classes with only part-time students.
- We exclude 4274 student-year observations for students who were repeating courses. These students follow a different attendance criterion and are graded under different standards.
- We exclude all observations from the first teaching periods in students' first years because for these observations, we have no measure of previous performance and therefore cannot calculate our measures of peer achievement.
- We exclude 1229 student-year observations from sections that take place after 6:30 p.m. because prior to the Fall 2015 semester, students could opt out of evening education, which makes the student assignment to these sections potentially non-random.

Second, we further limit our estimation sample to the bachelor's programs of Business and Business Economics offered at the business school from the academic years 2009/2010 through 2014/2015 because we can follow these cohorts from their first until their last bachelor's year and observe their major choices. For students in these programs, we only use peer

GPA from the first-year compulsory courses from teaching periods 2–4 (see Table 1). We exclude compulsory courses from the first teaching period because for these courses we have no measure of pre-assignment GPA. This sample restriction has two additional implications. First, we do not use peer GPA from any voluntary elective courses students may take on top of their compulsory courses. Second, we do not use peer GPA from compulsory courses from other bachelor's programs (e.g., bachelor's in econometrics) even if students in these programs later switched to a business or business economics bachelor's program.

Third, we exclude sections with fewer than two female or fewer than two male students. In these sections, one of our core independent variables, either the male peer GPA or the female peer GPA, is missing.

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