Peers from Venus and Mars: Higher-Achieving Men Foster Gender Gaps in Major Choice and Labor Market Outcomes*

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This paper investigates how achievement of university peers affects men's and women's course choices, major choices, and labor market outcomes. Exploiting random assignment of students to sections, we find that higher-achieving male peers cause men to take more mathematical courses. This effect persists in the labor market where men end up in higher-paying jobs. Women with higher-achieving male peers choose fewer mathematical courses and majors. These women end up in jobs where they earn less but are more satisfied. Thus, it is not obvious whether women's exposure to high-achieving male peers benefits or harms them.

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1. Introduction

Women earn less than men. One important driver of this gender pay gap is women's underrepresentation in high-paying technical occupations. These gender differences in the labor market have their origin, in part, in university, where women are less likely to choose mathematical majors (OECD 2017). The desire to understand the origins of the gender pay gap has therefore led to an interest in understanding what policies and environmental factors can encourage women to choose mathematical majors.

One environmental factor that may influence major choices are students' university peers. Students are often uncertain about which majors they will enjoy or perform well in. This uncertainty about preferences and ability can be seen, for example, with the large share of students who switch majors (Kugler, Tinsley, and Ukhaneva 2017; Astorne-Figari and Speer 2017) or by the fact that students' planned major at the time of entering university is not very predictive of their actual major choice (Sacerdote, 2001). In this uncertain environment, higher- achieving male or female peers might, for example, affect the classroom atmosphere and how much a student enjoys a subject. Students might also directly benefit from working with higher-achieving peers and achieve higher grades, which in turn may motivate them to choose more challenging majors. Alternatively, students might be discouraged and perform worse if they meet peers who just seem to "know it all." While it is not obvious how peers influence major choice, they are an integral part of students' social environment at university.

In this paper, we study how peer achievement affects men's and women's course choices, major choices, and labor market outcomes using data from a Dutch business school. In this business school, students take several compulsory courses in their first year of study and then specialize by selecting majors and elective courses. An important feature of this environment is that within compulsory courses, students are randomly assigned to teaching sections of up to 16 students—peers with whom they spend most of their university contact hours. Additionally, we conducted a graduate survey that allows us to see how these section peers affect students' labor market outcomes one to five years after graduation.

Our results show that having higher-achieving male peers in the first year of university has persistent effects on men's and women's course choices, major choices, and labor market outcomes. Men choose more mathematical courses when they have higher-achieving male peers. These effects persist: higher-achieving male peers also raise men's earnings. Women who have higher-achieving male peers choose fewer mathematical courses and majors, earn less, but they are more satisfied with their jobs. Thus, it is not obvious whether women's exposure to higher-achieving male peers benefits or harms them. We find no evidence that higher-achieving female peers affect students' educational choices or labor market outcomes.

When exploring potential mechanisms, we find that higher-achieving male peers cause men to study harder, to achieve better grades, and to be more satisfied with the course environment. This reaction is consistent with research showing that men thrive in more competitive environments (Niederle and Vesterlund 2007). Women's performance and study efforts are not affected by higher-achieving male or female peers, but they do evaluate the interactions with their peers more positively when they have higher-achieving male peers. We interpret this finding as evidence against the common explanation that higher-achieving men simply bully women into choosing fewer mathematical majors.

Our paper is most related to a number of studies that have investigated how peer achievement affects educational choices. The results of these studies differ by context. In a setting most similar to the one we study, Fischer (2017) exploits an as-good-as-random assignment of students to classes of an average of 330 students in an introductory Chemistry course at the University of California, Santa Barbara. In line with our findings, her results show that higher-achieving peers lower women's probability of graduating with a science, technology, engineering, or math (STEM) degree. However, Fischer does not distinguish whether higher-achieving male or female peers drive these results— a distinction that we show matters. In another study that also uses higher education data but a different definition of peer groups, Sacerdote (2001) looks at how randomly assigned dormmates at Dartmouth College affect a number of student outcomes. He finds no significant effect on major choice. In a substantially different setting in Chinese high schools, Mouganie and Wang (2017) exploit year-to-year variation in the gender composition of cohorts to test for peer effects on educational choices. They distinguish between the achievement of male and female peers and find that women who have more high-achieving female peers are more likely to choose a science track. The varying results across settings suggest that the educational context matters for the nature of peer effects estimates. This is consistent with findings in the broader peer effects literature (see Sacerdote (2014) for a review). In contrast to our paper, none of these studies test whether effects on educational choices translate into different labor market outcomes.

Our study contributes to the literature in two ways. First, we extend the peer effects literature with a well-identified study that highlights the importance of peers for students' specialization choices. Second, we show that peers not only affect university majors but also labor market outcomes. It is difficult to evaluate any effects on major choice without knowing whether and how these translate into students' longer-term outcomes such as earnings and job satisfaction.

2. Institutional Environment and Summary Statistics

2.1 Institutional Environment

We study the effect of peer achievement on course choice, major choice, and labor market outcomes using data from a Dutch business school from the academic years 2009/2010 to 2014/2015.

Table 1 shows descriptive statistics for the sample we study. We observe 3,037 students, about 40 percent of whom are female, and whose average age is nineteen. Although the setting is a Dutch business school, 56 percent of students are German and only 25 percent are Dutch. We limit our analysis to two bachelor programs for which students take eight compulsory courses in their first year and then choose from a number of elective courses and choose one major in their second and third years. The academic year at the business school consists of four eight-week teaching periods during which students typically take two courses simultaneously. Each course consists of multiple sections of up to 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 in a course 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 given 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. Feld and Zölitz (2017) as well as Zölitz and Feld (2017) provide a more detailed description of the institutional environment.

Table 1: Descriptive Statistics

	(1) N	(2) Mean	(3) SD	(4) Min	(5) Max
Student Demographic Characteristics					
Age	3,037	19.13	1.450	16.19	31.21
Female	3,037	0.386	0.487	0	1
German	3,037	0.564	0.496	0	1
Dutch	3,037	0.254	0.435	0	1
Explanatory Variables					
GPA of Female Peers	18,454	6.808	0.847	2.00	9.63
GPA of Male Peers	18,454	6.603	0.595	4.22	9.75
Outcome Variables					
Course and Major Choices:					
Mathematical Major	18,454	0.264	0.441	0	1
Any Mathematical Elective	18,454	0.493	0.5	0	1
Fraction Mathematical Electives	18,454	0.156	0.213	0	1
Labor Market Outcomes:					
Yearly Earnings in €1,000	6,358	43.5	42.19	0.01	650
Weekly Working Hours	5,347	47.74	12.04	2	100
Job Satisfaction	5,418	8.104	1.443	1	10
Subjective Social Impact	5,429	2.378	1.980	-5	5
Students' Grades and Course Evaluations:					
Course Grade	17,541	6.403	1.665	1	10
Self-reported Study Hours	6,307	11.8	7.647	0	70
Overall Course Quality	6,775	7.257	1.733	1	10
"My tutorial group has functioned well."	5,779	3.853	0.964	1	5
"Working in tutorial groups with my fellow students helped me to better understand the subject matter."	5,805	3.972	0.937	1	5

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

Table 1 shows the explanatory variables and outcome variables we use in this paper. We report the summary statistics at the student-course level, giving more weight to students observed more often – as does our empirical analysis. There are 3,037 students in this sample and we observe each student in 6.07 courses, on average, resulting in a total estimation sample of 18,454 student-course observations.

We run our analysis with observations at the student-by-course level, as opposed to the student level, because the randomization took place at the course-by-year level, and we therefore need to include course-by-year fixed effects for identification. This way of conducting the analysis implies that while we observe only one major choice for each student, each student appears multiple times in our data set with different peer groups. We account for this way of structuring the data by clustering standard errors at the student level and section level.

A key feature of our environment is that the business school's scheduling department randomly assigns students to sections and therefore to section peers. Beginning with the 2010/2011 academic year, the scheduling department additionally stratified section assignment by student nationality. We have excluded courses for which the scheduling department informed us that coordinators or other staff deviated from the random assignment policy and influenced the section assignment. Appendix A1 lists these sample restrictions in detail. Random assignment of students to sections implies that we can estimate the effect of peer achievement on students' specialization choices and labor market outcomes without having to worry about selection bias. We also do not worry about endogenous assignment of instructors to sections as Feld, Salamanca, and Zölitz (2018) and Mengel, Sauermann, and Zölitz (2018) have shown with data from the same environment that instructor characteristics are unrelated to student characteristics.

2.2 Explanatory Variables and Outcome Variables

Explanatory Variables: Throughout the paper, the explanatory variables of interest are the GPA of male and female section peers. Each student's GPA is constructed based on grades obtained before assignment to sections took place. Male peer GPA and female peer GPA are therefore the averages of the pre-assignment GPAs of all male or female students in a given section except for that particular student. Table 1 shows summary statistics of these key explanatory variables. The average GPA of female peers is 6.8 on a 10-point scale, which is significantly higher than the average GPA of male peers of 6.6, which reflects that at this business school, as in many other educational environments, women outperform men academically (see also Figure A1 in the Appendix for the distribution of the mean GPA of male and female section peers).

Outcomes Variables: Course and Major Choices: Our main academic outcomes are students' choices of mathematical course and majors. We define a course as mathematical if its description contains one of the following terms: math, mathematics, mathematical, statistics, statistical, theory focused. Table 1 shows that 16 percent of elective course observations are mathematical, which includes mathematical electives and major-specific mathematical compulsory courses. About half of the students choose at least one mathematical course.

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). There are no GPA linked requirements and students are completely free to choose any major. Table 1 shows that 26 percent of all major-choice observations are mathematical majors.

Table A1 in the Appendix gives additional information on all eight majors and shows that women are less likely to choose mathematical majors and that mathematical majors are associated with higher earnings.

Outcomes Variables: Labor Market Outcomes: To gather data on students' labor market outcomes, in 2016, we sent a survey to students who graduated between September 2010 and September 2015. From this survey, we use four outcomes: 1) yearly earnings in euro before taxes; 2) weekly working hours; 3) job satisfaction on a 10-point scale, with 10 being "most satisfied"; and 4) the subjective social impact of the job, ranging from -5 "very negative" to 5 "very positive," with 0 being "no impact." We measured job satisfaction with the question, "How satisfied are you, all in all, with your current work?" We measured subjective social impact with the question, "What do you think is the social impact of your current work?" Table 1 shows average earnings to be around 43,500 euros per year and average working hours to be about 48 hours per week. The average job satisfaction is 8.1, and the average social impact of the job is 2.4 points.

The graduate survey response rate was 33 percent. We show in Appendix Table A2 that survey response is unrelated to our peer variables of interest. To account for systematic differences in survey response based on observable characteristics, we follow Wooldridge (2007) and reweight all observations by the inverse of the predicted probability that we observe them in the labor market, thus giving more weight to students that are less likely to be observed in the sample. Empirically, this reweighting does not affect our result in any meaningful way.

Outcomes Variables: Students' Grades and Course Evaluations: To explore the mechanisms that may drive any effects on academic and labor market outcomes, we use students' grades and their responses to the course evaluation survey. Students' grades are the final course grade which, 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 6.4. For about 5 percent of our observations, students registered for a course, but we cannot observe their grade because they dropped out during the period. Appendix Table A2 shows that the probability of observing a grade for a student is unrelated to our peer variables of interest.

We measure study effort, course satisfaction, and the satisfaction with peer-to-peer interactions with data from students' course evaluations. The course evaluation survey is sent out at the end of each course but before students take 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; 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." Table 1 shows that students report studying on average 11.8 hours per course per week and rate the course quality on average at 7.3 on a 10-point scale.

In Column (3) of Appendix Table A2 we show that the course evaluation survey response is unrelated to our peer variables of interest. As for the labor market outcomes, following Wooldridge (2007), we reweight all estimates that use evaluation survey outcomes with the inverse of the predicted probability of answering the course evaluation survey. Results are almost identical if we do not reweight our observations.

2.3 Randomization Check

To confirm that the peer composition is random, we test whether the student “pretreatment” characteristics of previous GPA, age, gender, and the rank of the student ID— a proxy for a student’s tenure at the business school —systematically relate to the achievement of male and female peers. We implement these tests by regressing peer GPA separately on each of the four pretreatment characteristics and different sets of fixed effects. We follow Guryan, Kroft, and Notowidigdo (2009) and additionally control for the course-level leave-out mean of GPA to account for the mechanical relationship between own- and peer-level variables.

Table 2, in which each cell is based on a separate regression, shows the results of these randomization tests. The first row in Column (1) for example, shows the coefficient of regressions of own preassignment GPA on the average preassignment GPA of female peers as well as course-year fixed effects and the course-level leave-out mean of GPA of female peers. The second, third, and fourth observations in this column show similar regression coefficients for the other pretreatment characteristics. Column (2) shows results of the same regressions with the GPA of male peers as the dependent variable. Finally, Columns (3) and (4) show versions of the same randomization tests in which we additionally include parallel-course-year fixed effects as well as fixed effects for time and day of the tutorial sessions.

Table 2 shows that all four pretreatment characteristics are unrelated to our peer variables of interest. All point estimates are small and only one out of 16 coefficients of interest is statistically significant at the 10 percent level. We confirm this result using an alternative and more flexible randomization check, which we show in Appendix A2.

Table 2: Test for Random Assignment – Regression of Peer GPA on Student Pretreatment Characteristics

	(1)	(2)	(3)	(4)
Dependent Variable:	Std. GPA of Female Peers	Std. GPA of Male Peers	Std. GPA of Female Peers	Std. GPA of Male Peers
Std. GPA	-0.0030	-0.0167	-0.0028	-0.0178*
	(0.010)	(0.010)	(0.009)	(0.010)
Female	-0.0035	0.0202	-0.0003	0.0185
	(0.020)	(0.016)	(0.020)	(0.016)
Age	-0.0028	-0.0071	-0.0020	-0.0067
	(0.005)	(0.005)	(0.005)	(0.005)
ID Rank	0.0000	0.0000	-0.0000	0.0000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	18,454	18,454	18,454	18,454
Course-year FE	YES	YES	YES	YES
Time, Day and Parallel-course-year FE	NO	NO	YES	YES

NOTE — Each cell in this table is estimated with a separate ordinary least squares regression including course-year fixed effects. Following the Guryan, Kroft, and Notowidigdo (2009) correction method, we control for the leave-out mean of the peers' GPA at the course level in the first row. Robust standard errors using two-way clustering at the student level and section level are in parentheses.

3. Empirical Strategy

To understand how peer achievement affects students' specialization choices and labor market outcomes, we estimate the following model:

$$y_i = \alpha_1 Female_i \times \overline{MaleGPA}_{ic} + \alpha_2 Male_i \times \overline{MaleGPA}_{ic} \quad (1)$$

$$+ \beta_1 Female_i \times \overline{FemaleGPA}_{ic} + \beta_2 Male_i \times \overline{FemaleGPA}_{ic} + X\gamma' + u_{ic},$$

where y_i is the course choice, major choice, or a labor market outcome of student i, that is, after having taken the compulsory course c, where she was exposed to given group of section peers. We have four independent variables of interest. $Female_i \times \overline{MaleGPA}_{ic}$ and $Male_i \times \overline{MaleGPA}_{ic}$ are the average GPAs of all male section peers interacted with a female and male dummy. Analogously, $Female_i \times \overline{FemaleGPA}_{ic}$ and $Male_i \times \overline{FemaleGPA}_{ic}$ are the average GPAs of all female section peers interacted with a female and a male dummy. Each student's GPA consists of all grades achieved before the start of the course, which implies that neither male nor female peer GPA contain 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 course the students take in the same period. We include parallel-course-year fixed effects to account for potential nonrandom assignment due to scheduling conflicts. X also includes students' GPA at the start of the course as well as indicators for student gender and nationality. u_{ict} is the error term.

The parameters of interest are α_1, α_2, β_1, and β_2.Parameter α_1 captures the causal effect of a student's assignment to higher-GPA male peers on the outcome of interest for women, and α_2 shows the equivalent effect for men. Analogously, β_1 and β_2 show the causal effect of a student's assignment to higher-GPA female peers for men and women.

One might be concerned that peer ability, not peer achievement, affects students' choices, and that peer GPA is a noisy measure of peer ability. In Feld and Zölitz (2017), we show that classical measurement error in the peer variable of interest can lead to substantial overestimation of peer effects when peer group assignment is nonrandom. When peer group assignment is random, as in our setting, classical measurement error will bias peer effects coefficients toward zero. This implies that because peer GPA likely measures peer ability with some degree of error, estimates of α_1, α_2, β_1, and β_2 are likely attenuated. However, we believe that measurement error and the resulting attenuation bias will be small because GPA is the average of all previous grades of a student, and peer GPA consists of the average GPA over all female or male peers in a section.

To simplify the interpretation of our results, we standardize male peer GPA, female peer GPA, course grades, the peer interaction index, job satisfaction, and subjective social impact to have means of zero and standard deviations of one over the estimation sample. We cluster our standard errors at the section level and student level using two-way clustering. Clustering only at the section or only at the student level leads to same-sized or smaller standard errors.

4. Results

A. Effects on Choice of Mathematical Majors and Courses

Table 3 shows that having higher-achieving male peers reduces women's likelihood of choosing a mathematical major. The effects size is economically significant. Having male peers with a one standard deviation higher GPA reduces women's probability of majoring in Finance, IT management, or Economics by 1.7 percentage points, which is a 9 percent reduction compared to the baseline. Men's choice of mathematical majors is not significantly affected by having higher-achieving male peers, with point estimates close to zero. We find no evidence that the achievement of female peers matters for women's or men's major choices. These results are robust to an alternative definition of mathematical major that includes Supply Chain Management—the only other major that has one mathematical compulsory course.

Table 3: The Effect of Peer Achievement on Course and Major Choice

	(1)	(2)	(3)
Dependent Variable:	Mathematical Major	Any Mathematical Elective	Fraction Mathematical Electives
Female * Std. GPA of Male Peers	-0.0166***	-0.0252***	-0.0119***
	(0.006)	(0.007)	(0.003)
Male * Std. GPA of Male Peers	0.0079	0.0131**	0.0068**
	(0.006)	(0.006)	(0.003)
Female * Std. GPA of Female Peers	-0.0008	-0.0002	-0.0007
	(0.005)	(0.006)	(0.003)
Male * Std. GPA of Female Peers	-0.0005	-0.0029	-0.0007
	(0.005)	(0.005)	(0.002)
Female	-0.1437***	-0.1764***	-0.0958***
	(0.015)	(0.017)	(0.008)
Observations	18,454	18,454	18,454
R-squared	0.206	0.254	0.148
Mean Dependent Variable Female Students	.1781	.3651	.1019
Mean Dependent Variable Male Students	.3143	.5675	.1878
p-values for Test of Gender Equality of Std. GPA Male Peers	.0013	<.0001	<.0001
p-values for Test of Gender Equality of Std. GPA Female Peers	.9727	0.7555	0.9850

NOTE — All Columns are estimated with ordinary least squares regressions that include course-year fixed effects, parallel-course-year fixed effects, female, Std. GPA, Dutch and German. Robust standard errors using two-way clustering at the student level and section level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

These results are consistent with Fischer (2017) who finds that women with higher-achieving peers in an introductory STEM course are less likely to major in a STEM field. While Fischer does not distinguish between the achievement of male and female peers, our findings suggest that it is higher-achieving male peers who reduce women's STEM participation.

When looking at elective course choices, we see that women who had higher-achieving male peers are less likely to choose any mathematical elective (Column 2) and also choose fewer mathematical electives (Column 3). Our point estimates suggest that increasing the GPA of male peers by one standard deviation decreases the probability of choosing a mathematical course by about 2.5 percentage points, a 7 percent reduction from the baseline. For men, on the contrary, we find that having higher-achieving male peers increases the probability of choosing a mathematical course. A one standard deviation increase in male peers' GPA increases the probability of choosing any mathematical course by 1.3 percentage points, which is a 2.7 percent increase from the baseline. As for major choice, we find that female peers' GPA is not significantly related to men's or women's course choices, with all point estimates being close to zero. These results suggest that in our setting achievement of female peers does not play an important role in students' specialization choices.

We show in Zölitz and Feld (2017) that the proportion of female peers also influences course and major choice in the same setting. Controlling for the proportion of female peers barely affects our estimates. Therefore, the effect of peer ability on major choice we document in this paper is distinct from the effect of peer gender.

B. Effects on Labor Market Outcomes

Table 4 shows the estimated effects of peer achievement on a number of labor market outcomes. We see that the effects of exposure to higher-achieving male peers persist. For women, we find marginally statistically significant effects on earnings, which suggests that being assigned to male peers with a one standard deviation higher GPA reduces earnings by 9 percent. The estimated effects on working hours are economically small and not statistically significant. The most striking result is that male peers significantly affect women's job satisfaction and the subjective social impact of their job. Having male peers with a one standard deviation higher GPA increases women's job satisfaction by 8 percent of a standard deviation and the subjective social impact of their job by 7 percent of a standard deviation. Both estimates are highly statistically significant.

Table 4: The Impact of Peer Achievement on Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Log Yearly Earnings	Log Working Hours	Log Hourly Wage	Std. Job Satisfaction	Std. Subjective Social Impact
Female * Std. GPA of Male Peers	-0.0882*	-0.0147	-0.0921*	0.0825***	0.0713***
	(0.049)	(0.013)	(0.054)	(0.029)	(0.017)
Male * Std. GPA of Male Peers	0.0686**	0.0004	0.0490*	0.0057	0.0117
	(0.031)	(0.009)	(0.025)	(0.023)	(0.015)
Female * Std. GPA of Female Peers	-0.0223	0.0036	0.0243	0.0262	-0.0144
	(0.036)	(0.007)	(0.027)	(0.026)	(0.014)
Male * Std. GPA of Female Peers	-0.0039	-0.0031	0.0293	0.0129	-0.0142
	(0.025)	(0.006)	(0.022)	(0.021)	(0.012)
Female	-0.2800***	-0.0707***	-0.2663***	-0.0608	0.0572
	(0.088)	(0.024)	(0.086)	(0.075)	(0.041)
Observations	6,482	5,407	5,157	5,478	5,489
R-squared	0.109	0.136	0.063	0.025	0.700
Mean Dependent Variable Female Students	10.0562	44.9681	2.49	-.0364	.0184
Mean Dependent Variable Male Students	10.3377	49.2211	2.7204	.0366	-.1555
p-values for Test of Gender Equality of Std. GPA Male Peers	.0096	.3188	.0219	.0469	.0116
p-values for Test of Gender Equality of Std. GPA Female Peers	.6862	0.5297	0.8897	.7077	0.9881

NOTE — All Columns are estimated with ordinary least squares regressions that include course-year fixed effects, parallel-course-year fixed effects, female, Std. GPA, Dutch and German. Following Wooldridge (2007), for all specifications, we weight the observations by the inverse of the probability of observing the outcome. Robust standard errors using two-way clustering at the student level and section level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

These effects might be driven by the marginal women: (1) entering different occupations, or (2) fairing differently in the same occupation. Unfortunately, we cannot distinguish between these two explanations empirically because we do not have precise information on graduates' occupations such as ISCO codes. However, we do have information on nine industries that graduates use to describe their job tasks. Controlling for industry dummies does not affect our estimates in any meaningful way (see Table A3), which could mean that women's occupational choices are not affected by having higher-achieving male peers. An alternative explanation, which we find more plausible, is that our industries are only poor proxies for occupations and the affected women might work in different occupations within the same industry (see also Gillen, Snowberg, and Yariv (2017) for a discussion on inference using noisy controls).

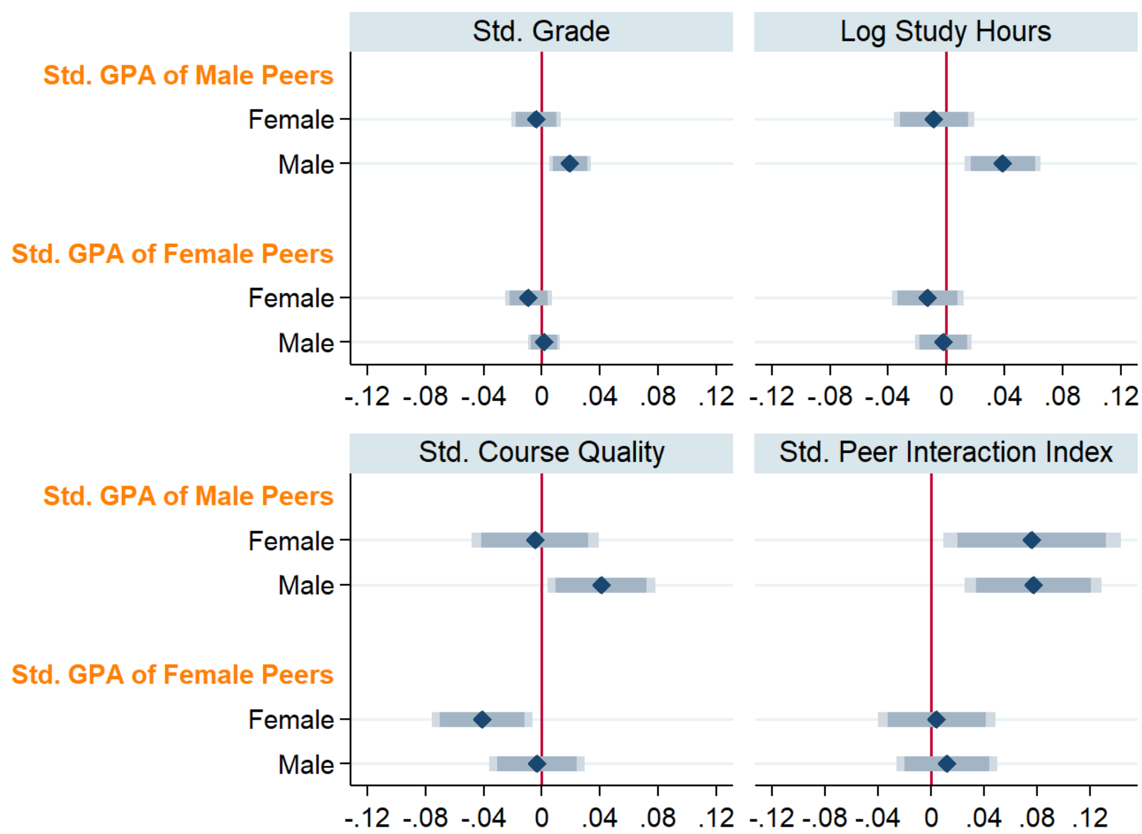
Male peers also affect men's labor market outcomes. Having male peers with a one standard deviation higher GPA causes men to earn 6.9 percent more per year. This point estimate reduces to 5.3 percent and remains marginally significant when we additionally control for industry dummies, which provides suggestive evidence that the wage effect is partly driven by the marginal men working in different industries (see Table A3). We find no effect on working hours and the estimated effect on hourly wage is similar in magnitude to the estimated effect on monthly wage. We find no evidence that male peers affect men's job satisfaction or the perceived social impact of their job. As for major and course choice, we do not find any evidence that higher-achieving women affect their peers' labor market outcomes.

C. Mechanisms

To better understand what might drive our observed peer effects on specialization choices and labor market outcomes, we take a closer look at how peer achievement affects students in their first-year compulsory courses. Figure 1 shows estimates of peer achievement on students' grades, study effort, their evaluation of the overall course quality, and their evaluation of group interaction in their first-year compulsory courses (see Table A4 in the Appendix for the underlying regression estimates).

The point estimates show that higher-achieving male peers affect men's performance and perceptions. Men who have male peers with a one standard deviation higher GPA receive a 2 percent of a standard deviation increase in grades and increase their weekly study hours by 4 percent. They also evaluate the course quality 4 percent of a standard deviation more positively and are 8 percent of a standard deviation more satisfied with the group interaction. In sum, men do better with higher-achieving male peers and subsequently choose more challenging mathematical electives. This reaction is consistent with gender differences in response to competition (see: Niederle and Vesterlund 2007). Men seem to thrive in environments that are more competitive due to higher-achieving peers.

Figure 1: Mechanisms—The Impact of Peer Achievement on Grades, Study Hours, Perceived Peer Interaction, and Course Quality



NOTE — This figure is based on point estimates shown in Table A4. All Columns are estimated with ordinary least squares regressions that include course-times-year fixed effects, parallel course fixed effects, female, Std. GPA, Dutch and German. Following Wooldridge (2007), for all specifications except the one with grade as dependent variable, we weight the observations by the inverse of the predicted probability of observing the outcome. The shown 90% and 95% confidence intervals are based on robust standard errors using two-way clustering at the student level and section level.

Women's experience in first-year courses appears to be less affected by their higher-achieving male peers. One exception is that they evaluate the group interaction more positively. This finding is evidence against the argument that higher-achieving men simply bully women into choosing fewer mathematical courses and majors. We also find that women evaluate the course as a whole more negatively if they had higher-achieving female peers, an effect that does not appear to translate into different specialization choices.

Throughout the paper, we have seen that both men and women are less affected by their female peers. This might be due to the discussion-based teaching style that is predominantly used at this business school. With such a large focus on discussions, women may be less likely to speak up and therefore be less likely to influence their peers' specialization choices (see Jule (2001) regarding gender differences in speaking up in the classroom).

6. Conclusion

We study how peer achievement during compulsory first-year courses affects women's and men's subsequent course choices, major choices, and labor market outcomes. Our findings show that male peers matter. Exposure to higher-achieving male peers causes women to choose fewer mathematical courses and majors and cause men to choose more mathematical courses. These effects persist. Higher-achieving male peers increase men's earnings and decrease women's earnings. Most strikingly, we also find that women exposed to higher-achieving male peers end up in jobs where they report higher job satisfaction.

Our results have potentially important implications for affirmative action policies designed to increase women's participation in mathematical fields. If these programs admit more women and fewer men to selective programs, they will mechanically increase the average achievement of male peers. Our findings suggest that these changes in the peer composition might have two types of unintended consequences for women. First, within these programs, women may become less likely to choose mathematical specializations. Second, when affirmative action policies are successful at raising women's wages, they might come—at least in the short run—at a cost to women's job satisfaction.

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APPENDIX

TABLES AND FIGURES

Table A1: The Major Choice Set

Major	(1) Major Classification	(2) Percent Compulsory Mathematical Courses in Major	(3) Percent Female	(4) First-Year GPA	(5)	(6) Mean Annual Earnings in Thousand €	(7)
				(Female)	(Male)	(Female)	(Male)
Finance	Mathematical major	50	21.50	7.29	7.15	55.86	58.33
IT Management	Mathematical major	50	30.43	6.78	6.50	43.63	43.31
Economics	Mathematical major	50	37.76	7.10	6.96	40.31	43.20
Supply Chain Mgmt	-	25	48.78	6.93	6.55	38.72	40.77
Strategy	-	0	35.64	6.94	6.52	43.58	47.87
Accounting	-	0	39.09	7.29	7.20	39.04	46.98
Organization	-	0	59.51	6.86	6.52	34.24	46.72
Marketing	-	0	60.34	6.81	6.61	40.14	45.72

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 in Section 2.

Table A2: Testing for Attrition and Selective Survey Response

	(1) Grade Observed	(2) Observed in the Labor Market	(3) Course Evaluation Survey Response
Female * GPA of Male Peers	-0.0026	-0.0004	-0.0081
	(0.002)	(0.008)	(0.008)
Male * GPA of Male Peers	-0.0018	-0.0055	-0.0076
	(0.002)	(0.007)	(0.006)
Female * GPA of Female Peers	-0.0021	-0.0017	-0.0038
	(0.002)	(0.007)	(0.007)
Male * GPA of Female Peers	0.0006	-0.0095*	0.0023
	(0.002)	(0.005)	(0.005)
Female	0.0037	-0.0122	0.0606***
	(0.004)	(0.019)	(0.014)
Observations	18,454	14,181	18,454
R-squared	0.168	0.110	0.088
Mean Dependent Variable Female Students	0.9631	.3318	.4282
Mean Dependent Variable Male Students	.9432	.3412	.3471
p-value of Test for joint Significance of Peer Variables	0.5649	0.3700	.5683

NOTE — All Columns are estimated with ordinary least squares regressions that include course-year fixed effects, parallel-course-year fixed effects, female, Std. GPA, Dutch and German. The dependent variable in Column (1) is equal to 1 if we observe a student's grade and 0 otherwise. The dependent variable in Column (2) is equal to 1 if we observe the student in the labor market, that is, if the student has answered the alumni survey and indicated that he/she is working and 0 otherwise. The dependent variable in Column (3) is equal to 1 if the student answered the course evaluation survey for a given course and 0 otherwise. We use the predicted value of the regressions shown in Columns (2) and (3) to weight observations in Tables 4 and A3 following Wooldridge (2007) to give more weight to students who we are less likely to observe. Robust standard errors using two-way clustering at the student level and section level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A3: Main Results Including Industry Fixed Effects

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Log Yearly Earnings	Log Working Hours	Log Hourly Wage	Std. Job Satisfaction	Std. Subjective Social Impact
Female * Std. GPA of Male Peers	-0.0832*	-0.0103	-0.0772*	0.0799***	0.0642***
	(0.048)	(0.011)	(0.044)	(0.029)	(0.017)
Male * Std. GPA of Male Peers	0.0528*	0.0009	0.0513**	0.0026	0.0139
	(0.029)	(0.009)	(0.026)	(0.023)	(0.014)
Female * Std. GPA of Female Peers	0.0237	0.0005	0.0213	0.0286	-0.0044
	(0.026)	(0.006)	(0.025)	(0.025)	(0.014)
Male * Std. GPA of Female Peers	0.0244	-0.0027	0.0285	0.0136	-0.0150
	(0.023)	(0.006)	(0.022)	(0.021)	(0.012)
Female	-0.2365***	-0.0476**	-0.2070**	-0.0251	0.0355
	(0.088)	(0.022)	(0.085)	(0.074)	(0.042)
Observations	5,192	5,401	5,157	5,472	5,483
R-squared	0.156	0.245	0.102	0.056	0.723
Mean Dependent Variable Female Students	10.2222	44.9681	2.49	-.0244	.0143
Mean Dependent Variable Male Students	10.5252	49.2732	2.7204	.0366	-.1555
p-values for Test of Gender Equality of GPA Male Peers	.0249	.3968	.0191	.0418	.0303
p-values for Test of Gender Equality of GPA Female Peers	.9855	.7491	.8405	.666	.5705

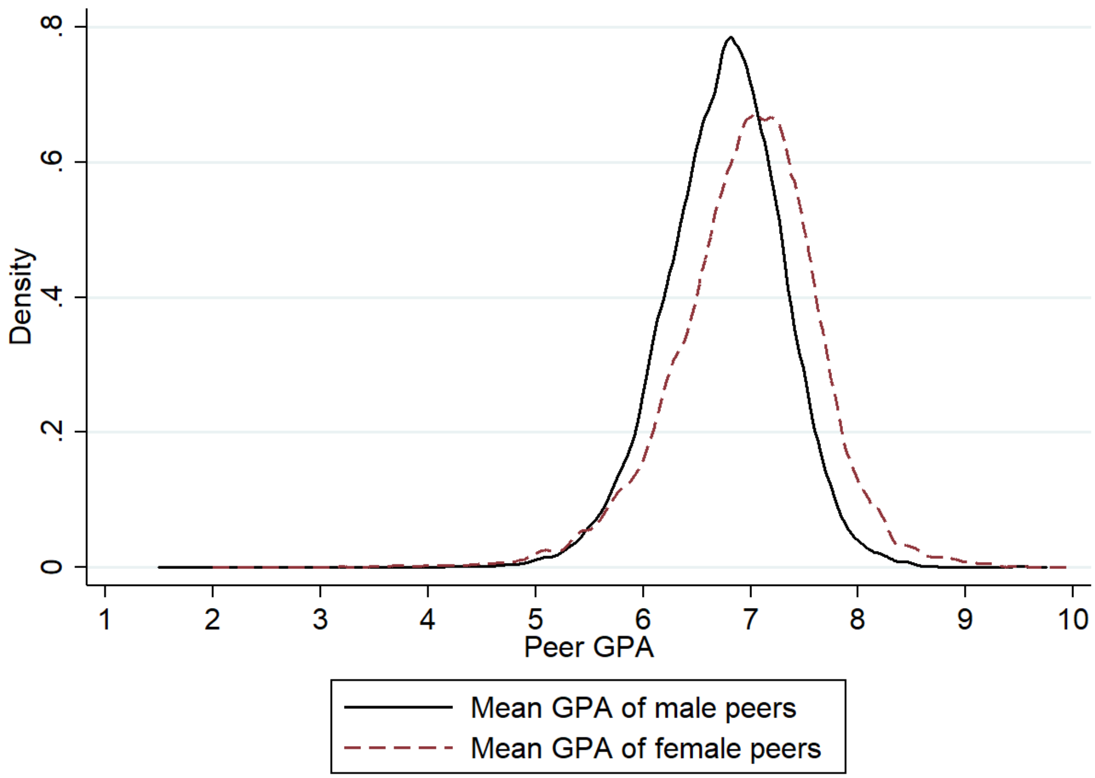
NOTE — All Columns are estimated with ordinary least squares regressions that include dummies for the following industries: (1) Marketing or Advertising of Goods or Services, (2) Finance, Banking, Trading, or Insurance, (3) Accounting, (4) Supply Chain Management, Logistics and Transportation, (5) Telecommunications, Information Technology, Internet, (6) Human Resource Management, (7) Health or Pharma, (8) Management Consultancy, (9) Other. Additional controls are the same as in Table 4 and include course-year fixed effects, parallel-course-year fixed effects, female, Std. GPA, Dutch and German. Following Wooldridge (2007), for all specifications, we weight the observations by the inverse of the probability of observing the outcome. Robust standard errors using two-way clustering at the student level and section level are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A4: Mechanisms—The Impact of Peer Achievement on Grades, Study Hours and Perceived Peer Interaction and Course Quality

	(1) Std. Grade	(2) Log Study Hours	(3) Std. Overall Course Quality	(4) Std. Peer Interaction Index
Female * Std. GPA of Male Peers	-0.0039	-0.0083	-0.0046	0.0762**
	(0.009)	(0.014)	(0.022)	(0.034)
Male * Std. GPA of Male Peers	0.0195***	0.0390***	0.0410**	0.0772***
	(0.007)	(0.013)	(0.019)	(0.026)
Female * Std. GPA of Female Peers	-0.0090	-0.0127	-0.0409**	0.0044
	(0.008)	(0.013)	(0.018)	(0.022)
Male * Std. GPA of Female Peers	0.0016	-0.0019	-0.0032	0.0121
	(0.006)	(0.010)	(0.017)	(0.019)
Female	-0.0393***	0.1199***	-0.0724**	0.0148
	(0.013)	(0.027)	(0.029)	(0.034)
Observations	17,536	6,035	6,357	4,862
R-squared	0.528	0.115	0.148	0.078
Mean Dependent Variable Female Students	.1329	2.3873	-.0188	.0208
Mean Dependent Variable Male Students	.0755	2.2646	.0716	-.0015
p-values for Test of Gender Equality of GPA Male Peers	.0271	.0111	.0648	0.9772
p-values for Test of Gender Equality of GPA Female Peers	.2541	.4899	.078	.7619

NOTE — All Columns are estimated with ordinary least squares regressions that include course-year fixed effects, parallel-course-year fixed effects, female, Std. GPA, Dutch and German. Following Wooldridge (2007), for all specifications, we weight the observations by the inverse of the predicted probability of observing the outcome. Robust standard errors using two-way clustering at the student level and section level are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Figure A1: Distribution of the Mean GPA of Male and Female Section Peers



APPENDIX

A1 Data Restrictions

In this paper, we use the same estimation sample as in Zölitz and Feld (2017) with the exception that we additionally have to exclude sections with fewer than two female or fewer than two male students because in these sections, one of our core independent variables, either the male peer GPA or the female peer GPA, is missing.

Our sample period comprises the academic years 2009/2010 through 2014/2015. We derive our estimation sample in two steps. First, we exclude a number of observations from our estimation sample because they represent exceptions from the standard section assignment procedure. These exceptions are the same as documented in as in Feld, Salamanca, and Zölitz (2017), who use data from the same environment and sample period. Second, we further limit our estimation sample to the two main bachelor's programs offered at the business school beginning in the academic years 2009/2010 until 2011/2012 because we can follow these cohorts from their first until their last bachelor's year and observe their major choices.

Below we list the observations we exclude due to exceptions to the scheduling procedure.

- 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 that registered late. In exceptional cases in which the number of late registration students substantially exceeded the number of empty spots, new sections were created that

mainly comprised late registering students. 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 4,274 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 of the first-year and the first-period students. For these observations, we have no measure of previous performance of the student at the business school, an essential variable in our analyses.
- We exclude all observations from the first teaching period of 2009, which is the first period in our data set, for the same reasons outlined above.
- We exclude 1,229 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 nonrandom.

A2 Alternative Test for Random Assignment of Students to Sections

To confirm that section assignment is random, we test whether section dummies jointly predict students' pretreatment characteristics when controlling for scheduling and balancing indicators. The randomization check we present here is almost identical to the one we show in the Appendix of Feld and Zölitz (2017); the main differences are that we now include six years of data instead of three years and that we limit our analysis to first-year compulsory courses in two bachelor programs. The pretreatment characteristics that we consider are GPA, age, gender, and student ID rank. We do not know the age, gender, and nationality of approximately nine percent of our sample, who are mostly exchange students. In the business school, student ID numbers increase with tenure at the university. ID rank is the rank of the ID number. We use ID rank instead of actual ID because the business school added a new digit to the ID numbers, which created a discrete jump in the series. As balancing indicators, we include parallel-course fixed effects and dummies for student nationality.

For each course in our sample, we run a regression of pretreatment characteristics on section dummies as well as scheduling and balancing controls, and then we run an F-test for joint significance of the section dummies. That means we run 138 regressions for each of the pretreatment characteristics, age and ID rank. For female and GPA, we could only run 116 and 109 regressions due to missing observations of these variables. 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, Tsai, and Adcock 2008). Furthermore, if students are randomly assigned to sections within each course, the F-test should reject the null hypothesis of no relation between section assignment and students' pretreatment characteristics at the 5 percent and 1 percent significance levels in close to 5 percent and 1 percent, of the cases, respectively.

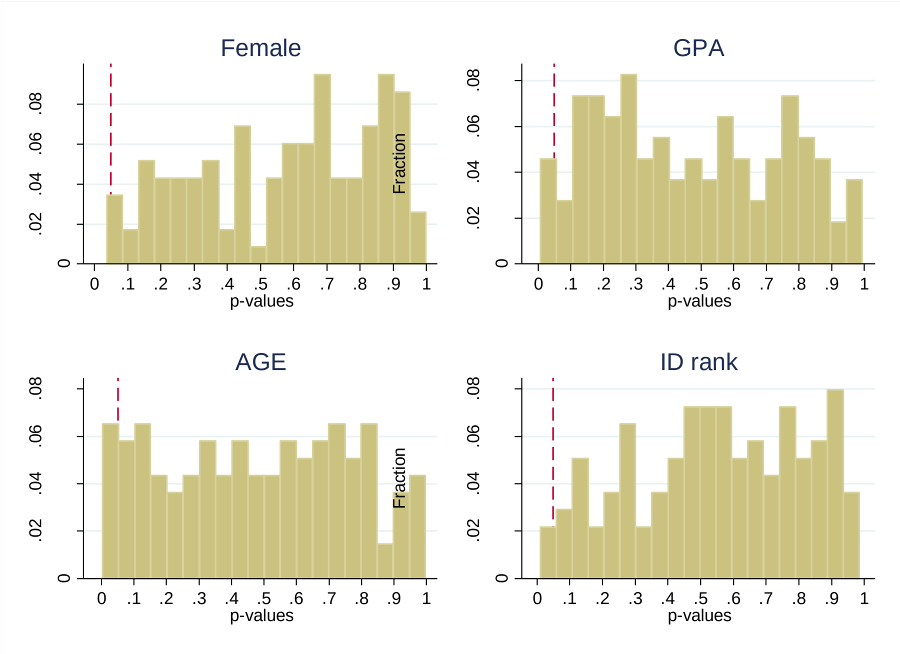
Table A5 shows the number of cases in which the F-test actually rejected the null hypothesis at the respective levels. Column (1) shows the total number of course-level regressions for each pretreatment characteristic. Columns (2) to (5) shows the actual rejection rates at the 5 percent and 1 percent levels respectively. All rejection rates are close to the expected rejection rates under random assignment. Column (6) shows that the averages of the p-values of the F-tests for each characteristic are close to 0.5. Figure A2 confirms that the p-values are roughly uniformly distributed. All together, we present additional evidence that section assignment in our estimation sample is random, conditional on scheduling and balancing indicators.

Table A5: Alternative Randomization Check

Dependent Variable	Number significant at:			Percent significant at:			Total Number of Courses	Mean of p-value
	5%	1%	0.1%	5%	1%	0.1%		
Female	4	0	0	0.034	0.000	0.000	116	0.5786
GPA	5	2	0	0.046	0.018	0.000	109	0.4795
Age	8	3	0	0.058	0.022	0.000	138	0.4793
ID rank	3	0	0	0.022	0.000	0.000	138	0.5534

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 parallel course taken at the same time, and dummies for day and time of the sessions, German, and Dutch.

Figure A2: Alternative Randomization Check - Distributions of p-values



NOTE — These are histograms with p-values from all the regressions reported in Table A1. The vertical line in each histogram shows the 5 percent significance level.