Peer Gender Composition and Undergraduate Achievement and Major Choice

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Abstract

Large gender differences exist in the completion of college majors across fields. I investigate how the gender composition of first-semester classes impacts women's and men's academic performance and major choices. I find that a larger proportion of male peers in a class makes women less likely to declare a related major, less likely to graduate, and less likely to graduate with a related degree, relative to men attending the same lectures. The effects of male peers on female non-persistence are largest in classes that are in overall male-dominated fields and are concentrated in the classes with the largest majorities of male students. These results suggest that peer gender effects may help to perpetuate male domination in certain fields, including many highly paid fields like engineering and economics.

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While women attend and graduate from college at comparable or higher rates than men, large gender differences persist in the take-up and completion of different college majors. Gender concentration within fields of study has important implications for both equity and efficiency. Given that men dominate many of the majors associated with the highest wages, such as engineering and economics, major choice may be an important contributing factor to the gender pay gap (Brown and Corcoran (1997), Gemici and Wiswall (2014), Patnaik et al. (2020)). Moreover, if there is reason to believe that gendered sorting to majors, and the occupations associated with those majors, does not reflect sorting to comparative advantage, this "friction" may dampen overall economic production (Hsieh et al. (2019)).

A substantial amount of work has gone into understanding the sources of gender differences in college major choice, with proposed factors ranging from high school preparedness (Card and Payne (2017), Aucejo and James (2021)), to preferences over non-pecuniary aspects of major-related occupations (Zafar (2013), Wiswall and Zafar (2017)), to competitiveness (Buser et al. (2014)), to the availability of role models (Carrell et al. (2010)). This paper considers whether peer gender effects within college classes play a contributing role. In equilibrium, peer composition may reinforce the gendered sorting to fields that stems from other sources if students tend to choose majors related to the classes where they have more same-gender peers.

I investigate this question using administrative data from the University of Illinois Chicago (UIC), a large, public research university. The data provides information on all class registrations, class outcomes, major declarations, and semesters of graduation for two entering cohorts of undergraduate students. I focus on the classes that students enroll in during their first semester at the university based on the idea that incoming students have not yet met their peers and thus cannot coordinate or intentionally select into specific classes based on their gender compositions.

For my primary empirical strategy, I estimate how the gap between female and male outcomes evolves with the class-level gender ratio. I control for course-specific female fixed effects, utilizing variation in peer composition across different lecture times within courses. Doing so accounts for any potential gender differences in course-specific tastes or academic preparation. Given that I am estimating an effect on the *gap* between women and men, I am also able to include fixed effects for each specific lecture time in each semester for each course, in order to control for any gender-neutral sorting or shocks to these specific offerings of the courses. I find that when first-semester classes have more men, women are less likely to choose majors associated with those classes and are less likely to graduate, relative to men attending the same lectures. Placebo tests that use pre-college variables as outcomes show that these results are not driven by observable characteristics of students, such as pre-college academic attainment.

The aforementioned results characterize female outcomes *relative* to male ones. However, it is not immediately clear whether those patterns are driven by the behavior of women, the behavior of men, or both. By omitting the fixed effects for specific lecture times, I am able to separately estimate the effects of peer composition on male students and on female students. The effect on graduation appears to be driven primarily by women being less likely to graduate when having more male peers in first-semester courses. When it comes to major choice, however, it is the case both that women are less likely to declare majors related to male-heavy classes and that men are more likely to declare such majors, with men having the larger effect size in terms of both magnitude and statistical significance.

These results connect to an existing literature on the effects of peer gender in educational contexts. There has been a great deal of work focusing on primary and secondary school contexts (Hoxby (2000), Whitmore (2005), Lavy and Schlosser (2011), Black et al. (2013), Hu (2015), Gong et al. (2021)). Some more recent work has considered the impact of peer gender in post-secondary education, with many papers focusing on peer gender and achievement (De Giorgi et al. (2010), Oosterbeek and van Ewijk (2014), Hill (2017). Bostwick and Weinberg (2018)).

The previous papers that are most comparable to this one are Griffith and Main (2019) and Zolitz and Feld (2020). Griffith and Main (2019) exploit random assignment to an introductory class for students entering an undergraduate engineering program at a US university. They find that having more female students in a class increases both grades and persistence beyond the first year of the program for male and female students. Zolitz and Feld (2020) similarly make use of random assignment in the context of compulsory, introductory classes at a Dutch business school. They find that having classes with more female peers increases the likelihood that both men and women select into majors that are more dominated by their own gender.

A key advantage of my context, relative to earlier work, is that I estimate effects using classes from across the full breadth of academic departments available at typical US universities. This allows me to consider heterogeneity across academic fields: I find that both the negative effects of male peers on women and the positive effects of male peers on men are strongly concentrated among male-majority departments. This implies that peer gender effects may be particularly important in many STEM and many traditionally highly paid fields. I also note that gender gaps in outcomes appear to be largest for classes that are either very male-dominated or very female-dominated. This implies that mixed or female-dominated introductory classes may help to support retention of women in traditionally male-dominated academic fields.

The rest of the paper proceeds as follows. Section 1 describes the administrative data and provides institutional context regarding UIC. Section 2 describes my empirical strategy and presents the results of balance tests used to validate the strategy. Section 3 presents and discusses the results as well as various robustness checks. Section 4 concludes.

1 Data and Institutional Context

I use an administrative dataset from the University of Illinois Chicago, a large, public research university. UIC is one of three universities in the University of Illinois System, and enrolls approximately 21,000 undergraduate students per year. The data covers all undergraduate students who first enrolled at UIC in the Fall of 2015 or Fall of 2016 semesters, including transfers, for a total of 9,797 students.

For the students in the dataset, I observe all class enrollments and outcomes, including grades and class withdrawals, for every semester the student is enrolled at UIC through six or seven years post-matriculation (for students entering in 2015 or 2016, respectively). Here and subsequently I use the term “class" to refer to a specific offering of a “course" that is uniquely identified by a particular semester, lecture time, and instructor. For instance, I would refer to Economics 101 as a “course" and the Fall of 2015, 9 A.M. lecture time for Economics 101 as a “class." I observe the name of the instructor for each class in the set of student-class observations, and, if the class has any associated discussion, laboratory, or experiential sections, the names of the teaching assistants (TAs) that lead those sections. While I do not directly observe characteristics of the instructors or TAs, I infer gender from names.[1] For each student in the sample, the administrative data provides information on several background characteristics, including race, ethnicity, gender, and pre-college academic achievement in the form of high school GPA and a composite ACT score. I also observe all major declarations for all students in the sample. Major information is by semester, meaning that I observe when each student first declares a major and if and when they switch majors.[2] If a student graduates from UIC during the covered period, the data also shows their semester of graduation.

My analysis focuses on incoming first-semester students in order to make the best argument for identification (as discussed in Section 2). I thus restrict my main sample to student-class-level observations corresponding to classes taken during a student's first semester. My primary estimating equation, equation (1), includes both course-specific female fixed effects and class fixed effects. Estimation of my parameter of interest – the differential effect of class-level male proportion on females compared to males – thus requires observations from courses with multiple classes, each of which has at least one female and one male student. I thereby eliminate observations from all courses and classes that do not meet this requirement from the main sample. This eliminates about 6.6 percent of first-semester observations. The remaining sample consists of 43,351 student-class observations.

Table 1 reports descriptive statistics for the main sample. Panel A reports background characteristics broken down by gender. Entering UIC, men and women look fairly similar,

[1] I predict gender using the R package *gender*, which utilizes Social Security data (Mullen (2021)).

[2] At UIC, the large majority of students start out “undeclared" and first declare a major some time after their initial semester.

with comparable ethnic compositions and standardized test scores (the differences in means for most of these variables are statistically significant but small in magnitude compared to the variation within groups). Female students have a statistically significantly advantage in mean high school GPA over their male peers, reflecting the pattern found in the overall population, although, again, the size of the gap is modest (a mean GPA of 3.35 for women compared to 3.22 for men). The overall sample has an average ACT composite score of approximately 24.5, which would place the mean student at approximately the 75th percentile of test-takers, indicating a student body that is academically above average among college-interested students.[3]

Panel B reports individual-level outcomes for the students in the main sample and panel C reports student-class-level outcomes and characteristics. Women tend to academically outperform men at UIC, being more likely to graduate within six years, earn higher grades, and pass their classes. However, despite being more likely to graduate in general, women are substantially less likely to graduate with a STEM major. Among students who graduate within six years, approximately 63% of male students graduate in STEM fields compared to about 49% of women, highlighting the gender differences in choice of field of study.[4]

Panel C also shows that about 40% of student-class observations belong to courses with an associated section, where “section" refers to any additional discussion, laboratory, or practical experience session, typically led by a TA. This subsample, with some additional sample restrictions, is used for a robustness check which relies upon variation in the proportion of male peers in sections within classes, rather than variation in classes within courses. About 20% of student-class observations are from courses for which only one lecture time is offered per year. This subsample is used for a robustness check where the variation in class-level peer composition is based largely on between-cohort variation. More details on each of these alternate specifications is provided in Section 3.2.

2 Empirical Strategy

My aim is to estimate how the difference in female and male outcomes evolves with the gender ratio in a class.[5] I do so by exploiting variation in peer composition across classes (specific lecture times in specific semesters) within courses (named curricular offerings). This may involve variation between lecture times within a semester, say 9 AM compared to 11 AM, and may involve variation between students taking the course in the Fall of

[3]This is based on the 2022-2023 reporting year statistics from ACT, Inc:
https://www.act.org/content/dam/act/unsecured/documents/MultipleChoiceStemComposite.pdf

[4]“STEM" is defined according to the 2022 Department of Homeland Security STEM Designated Degree Program List: https://www.ice.gov/doclib/sevis/pdf/stemList2022.pdf. The overall proportion of STEM graduates I observe is somewhat high in part due to the inclusion of certain majors that are not designated as STEM by other definitions, including psychology, economics, and certain pre-health majors.

[5]Gender ratio is measured based on all students in the class, including the student whose outcome is being modeled.

Table 1 – Descriptive Statistics by Gender - Main Sample

	Women	Men	P-Value of Difference
Panel A: Student characteristics			
Ethnicities			
White	0.30	0.32	0.01
	(0.46)	(0.47)	
Asian	0.20	0.22	0.05
	(0.40)	(0.41)	
Hispanic	0.34	0.33	0.32
	(0.47)	(0.47)	
African American	0.10	0.07	0.00
	(0.30)	(0.25)	
High school GPA	3.35	3.22	0.00
	(0.37)	(0.39)	
ACT composite score	24.02	24.74	0.00
	(4.02)	(3.93)	
Panel B: Student outcomes			
Graduate within 6 Years	0.70	0.64	0.00
	(0.46)	(0.48)	
Graduate with a STEM major within 6 Years	0.34	0.40	0.00
	(0.47)	(0.49)	
Observations	4960	4594	
Panel C: Student-class outcomes and characteristics			
Grade (GPA value)	3.07	2.90	0.00
	(1.05)	(1.14)	
Grade of B or higher	0.81	0.78	0.00
	(0.39)	(0.41)	
Passed class	0.96	0.94	0.00
	(0.20)	(0.24)	
Dropped class	0.05	0.05	0.47
	(0.22)	(0.22)	
Course only offers one class per year	0.18	0.21	0.00
	(0.39)	(0.41)	
Class has an associated section	0.41	0.42	0.00
	(0.49)	(0.49)	
Observations	22162	21189	

Notes: This table reports the means and standard deviations of several variables, by gender, as well as the p-values of t-tests of differences in means across genders for each variable. All statistics are estimated on the observations within the identifying set of the main estimating equation (as described in Section 1) for which the relevant variable is observed. The variables used in Panels A and B are student-level while the variables in Panel C are student-class-level. The numbers of observations listed below Panel B also apply to Panel A. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

2015 compared to the Fall of 2016.[6]

I focus solely on first-semester students. By doing so, I ensure that the peer composition of classes was not observable to students when they initially enrolled. Students register for first-semester classes in the summer prior to matriculation, before they have had a formal opportunity to meet their peers (other than the relatively small subset who attend their same orientation session).[7] Thus, the students in my sample were not purposefully selecting into peer groups when they chose classes.[8]

By comparing within courses, I allow men and women to systematically differ in course-specific aptitudes or preferences, accounting for the fact that men and women may have received different kinds of education or may have formed dissimilar interests prior to entering college. Moreover, because my primary empirical strategy estimates how the *gap* between women and men changes with peer composition, I am also able to account for class-level fixed effects. The class fixed effects allow for any kind of gender-neutral sorting to classes, within courses, or class-specific shocks. For instance, if more motivated students tended to take morning classes, as opposed to afternoon ones, this would be picked up by the class fixed effects, so long as the sorting behavior was similar between the male and female populations.

The remaining threat to identification stems from the possibility of differential sorting between men and women to classes. For instance, if women were both more likely to register for morning classes *and* the difference between morning-class and afternoon-class women was larger than the difference between morning-class and afternoon-class men, this would bias my estimates. I argue that this concern is minimal using balance tests, which I describe later in this section. I also perform multiple robustness checks intended to minimize the extent to which differential sorting may drive my results. I describe these alternative approaches in greater detail in Section 3.2.

I operationalize my identification strategy via the following econometric model of

[6]For the results presented in the main body of the paper, I pool across both within-semester and across-semester variation in classes to maximize power. However, each of these two sources of variation introduces separate concerns for identification. I thus also present results using only within- and only across-semester variation in Appendix A. Both sets of results are similar to my main results, although, naturally, less precise. This suggests that neither form of variation is solely driving the results.

[7]At UIC, incoming students (both freshmen and transfers) are required to attend an on-campus orientation session prior to their first semester of classes. In both summer 2015 and 2016, students attended one of fourteen sessions offered over the course of the summer, registering for their preferred session on a first-come, first-serve basis. At the summer sessions, students met with academic advisors and received course recommendations based on their stated academic interests, prior credits (from either AP/IB examinations or previous college enrollments), and their performance on placement tests taken prior to the orientation. After this meeting, students were free to sign up for classes, subject to capacity constraints.

[8]The data is comprised of the sample of class enrollments after the “add/drop period” during which students can leave classes they initially enrolled in and join classes that they had not enrolled in and which have open seats. While this could allow for some degree of selection into peers, the add/drop period is only about a week and a half, allowing for limited time to adjust on this margin.

student-class-level outcomes, $Y_{i,r,c}$:

$$Y_{i,r,c} = \alpha_0 + \alpha_1 \times Fem_i \times MP_c + \sum_{r=1}^{R} \gamma_r \times Fem_i \times I_r + \sum_{c=1}^{C} \delta_c \times I_c + X'_{i,r,c}\beta + u_{i,r,c} \tag{1}$$

where students are indexed by i, courses by r, and classes by c and there are R total courses and C total classes in the sample.[9] Fem_i is an indicator variable that takes a value of one if student i is female, while I_r and I_c are indicator variables that take on values of one if outcome $Y_{i,r,c}$ is associated with course r or class c, respectively. MP_c denotes the proportion of male students in class c, $X_{i,r,c}$ contains a vector of observable student and student-class attributes, and $u_{i,r,c}$ is an unobservable error term.[10]

The parameter of interest, α_1, measures how the gap between female and male outcomes evolves with the class-level male proportion.[11] Given the focus on an interaction term, I am able to include both course-specific female fixed effects, γ_r, and general class fixed effects, δ_c.[12] The course-specific female fixed effects restrict the identifying variation to be within-course. The class fixed effects pick up any gender-neutral sorting or shocks associated with specific classes, within a course.[13]

In order to identify my parameter of interest, I assume that the residual variation in $Fem_i \times MP_c$, conditional on the fixed effects and controls, is orthogonal to the residual variation in the error. Essentially, I assume that, within a course, students sort to classes in such a way that the differences between male and female students are not correlated with the gender composition of the class.[14] The residual, identifying variation in class

[9]Classes are nested within courses. Thus, any variable with a c subscript could alternatively be denoted with a double r,c subscript. I omit the course subscripts in these cases for readability.

[10]For regressions taking the form of equation (1), $X_{i,r,c}$ is generally composed of student underrepresented minority status (a dummy for Black, Hispanic, and native students) and instructor-student gender match. The specific set of controls used in each specification is described in the notes for the table reporting the corresponding results.

[11]Specifically, this parameter measures how the gap between female and male outcomes evolves *linearly* with class-level male proportion. Section 3.4 explores potential non-linearities in the effects. In general, it seems that both the absolute effects of male peers and the differences in effects of male peers on women compared to men are concentrated among the most male-dominated classes.

[12]The focus on a gap between groups of students and use of fixed effects bears some resemblance to Fairlie et al. (2014), although that paper makes use of a combination of individual fixed effects and class fixed effects.

[13]Given that there are multiple observations per individual, it is implementable to include individual-level fixed effects. I exclude such fixed effects from my main specification, but discuss and report results with such fixed effects in Appendix B. For four of my six outcomes, individual fixed effects either fully or partially preclude meaningful estimation, as discussed further in the Appendix section. Individual fixed effects do have potential econometric value in estimation of effects on two outcomes: grades and taking future courses in the same department as the current course. For grades, inclusion of individual-fixed effects substantially curtails the magnitude of the estimated effect. I thereby do not emphasize grades as an outcome in the paper. For taking future courses, inclusion of individual-fixed effects increases the magnitude and statistical significance of the estimated effect. I also do not emphasize this outcome in order to be consistent in my choice of primary estimating equation.

[14]Here, “sorting" refers both to within-semester selections of a specific class time slot and to between-cohort variation. For the between-cohort variation, I still need to assume that students are not deciding whether or not to register for a *course* based on knowledge of the gender composition of that course in that

Table 2 – Associations Between Class Male Proportion and Pre-College Characteristics

	Standardized ACT score	High school GPA	Underrepresented minority student	Predicted grade (GPA value)
Female student X	0.026	-0.073	-0.045	-0.079
class male %	(0.110)	(0.057)	(0.058)	(0.070)
Outcome Mean	0.007	3.306	0.523	2.969
Outcome SD	1.000	0.392	0.499	0.410
Observations	31674	31702	43351	23018

Notes: This table reports the results of regressions of pre-college characteristics, at a student-class observation level, on an interaction between a female-student dummy and the class-level male proportion. Each column corresponds to a separate regression. Each regression includes course-specific female fixed effects, class fixed effects, and a control for instructor-student gender match. The predicted grade outcome is based on another (unshown) regression of the GPA value of grades on ACT score, high school GPA, and minority status among students in the main estimation sample. The predicted grade regression only includes the observations used to form the predicted grades: student-class observations from graded classes that had information on both ACT scores and high school GPA. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

gender composition may come from a variety of sources, including capacity constraints on classes, student scheduling constraints, between-cohort variation in the numbers of men and women interested in each course, and idiosyncratic preferences for time slots. These sources may create noise in the class-level peer composition, which I argue is uncorrelated with differences in male and female characteristics.

My fixed effect strategy addresses many forms of potential endogeneity. As previously mentioned, the remaining threat to identification stems from the possibility of differential sorting between men and women to classes. While I cannot fully rule out the possibility of such differential sorting, I do test for it by estimating equation (1) for pre-college attributes that may be predictive of college outcomes: standardized ACT score, high school GPA, and underrepresented minority status.[15] I present the results of this exercise in Table 2.

The first three columns show that there is little systematic association between class male proportion and the differences in male and female pre-college academic aptitude or ethnicity, conditional on the full set of fixed effects. As it may be difficult to interpret the magnitudes of these estimates, I also form predicted course grades by regressing the GPA value of grades on the three pre-college attributes that I tested. I then use these background characteristic-predicted grades as an additional outcome, reported in the fourth column of Table 2. The point estimate suggests that a woman in a 100 percent male class would only be expected to receive a grade that is worth 0.08 fewer GPA points than

year.

[15] Although I use high school GPA and ACT scores as outcomes in the placebo tests, I do not use them as controls in the specifications reported in the main body of the paper, as these variables are missing for a substantial portion of the sample. In Appendix C, I report results excluding any individual controls and results that include high school GPA and ACT scores as controls. The former set of results are nearly always very similar to those reported in the main text. The latter results generally have similar point estimates to those of my preferred specification, but are less precise due to the curtailed sample sizes.

Table 3 – Sorting by Students to Female Instructors (Within Course)

	Female student	Male student
Female instructor	0.000*	-0.000
	(0.000)	(0.001)
Outcome Mean	0.511	0.486
Observations	43351	43351

Notes: This table reports the results of regressions of dummies for student gender, defined at a student-class observation level, on a dummy for the class being taught by a female instructor. Each column corresponds to a separate regression. Each regression includes course fixed effects. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

a women in a 0 percent male class, based on observable characteristics. Along with not being statistically significant, this estimate is absolutely small, less than one tenth of the difference between an A and a B, and relatively small compared to the estimated effects of peer gender on grades that I report in Section 3.1. I interpret this as evidence in favor of the necessary assumption of no differential sorting.

When selecting classes, the notable characteristics that students observe are the time period and, for most classes, the instructor. Thus, one particularly salient potential source of differential sorting is instructor gender. If students were more likely to enroll in classes taught by instructors of their own gender and students either performed better in the presence of same-gender instructors or only a certain type of student sorted into classes with same-gender instructors, this would bias my results.[16] I investigate this specific threat directly in Table 3, showing that, within courses, there is no meaningful degree of sorting into or out of female-taught classes by either female or male students. Nonetheless, I include a control for instructor-student gender match in all specifications taking the form of equation (1).

Given that identification is only possible via variation in class-level male proportion within courses, it is of interest how much such variation exists in the data. Figure 1 plots class-level male proportion against average course-level male proportion. While there is naturally a high degree of correlation between the two, there is meaningful variation between classes within course, particularly for courses that are closer to the center of the distribution and have many different observed classes. I plotted the three non-general requirement courses that have the most classes in separate colors, revealing that all three have classes spanning much of the range of possible gender ratios. This is the case in spite of the on-average male domination of Business Administration 100 and on-average female

[16]There is recent literature suggesting that female instructors may improve the achievement of female college students (see for example Hoffman and Oreopoulos (2009) and Carrell et al. (2010)), although there are mixed findings regarding how female instructors affect the future course and major selections of female students (see for example Bettinger and Long (2005) and Price (2010)).

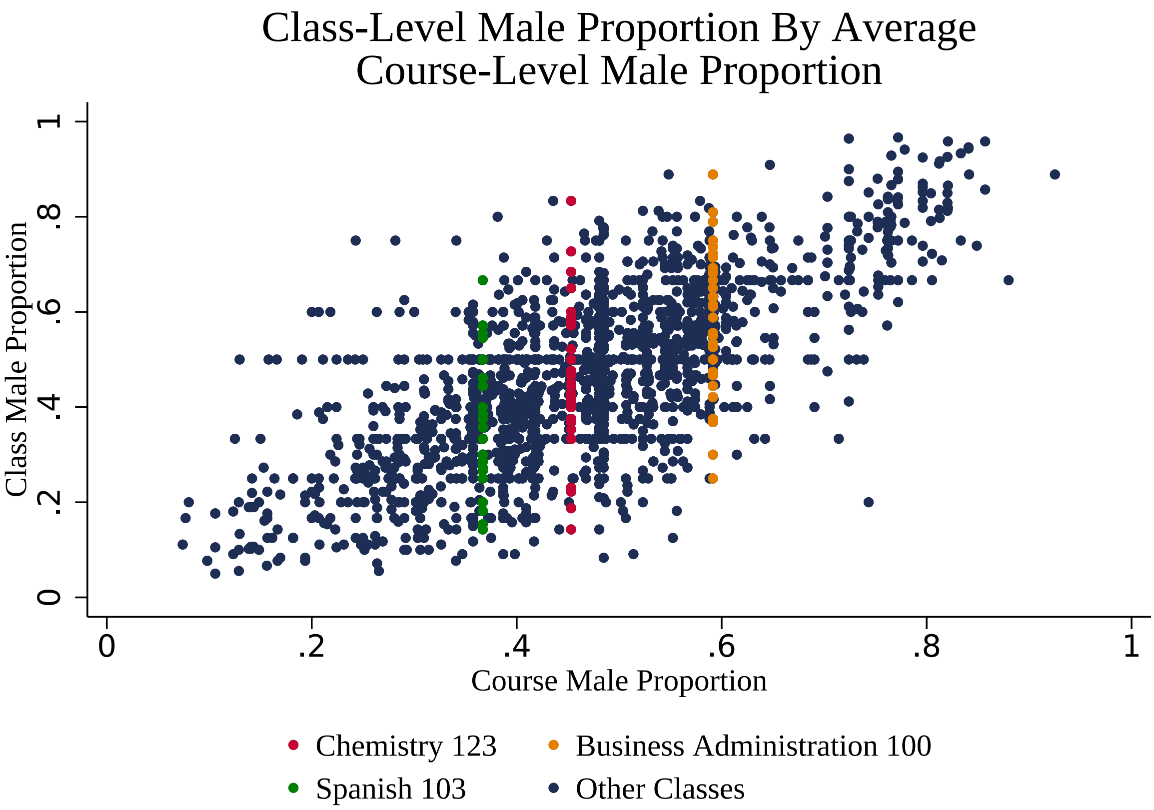

Figure 1: Figure depicts all classes in the main estimation sample with the proportion of males in the class on the Y-axis and the proportion of males across all classes in the corresponding course on the X-axis. The three non-general requirement courses with the largest numbers of classes are depicted in separate colors to demonstrate the variation of intra-course male proportion in the data.

domination of Spanish 103.

The empirical distribution of class male proportion is plotted in Figure 2. The blue line gives the raw distribution of class-level male proportion in the main estimation sample while the maroon line displays the residual variation conditional on course fixed effects. As would be expected, taking out course-level variation substantially condenses the distribution. The remaining variation is concentrated within a span of about twenty percentage points.

2.1 Within-Gender Empirical Strategy

Estimating equation (1) can establish whether or not peer gender composition creates a separation in the outcomes of male and female students. However, it cannot reveal whether this is driven by the responses of women, of men, or of both genders simultaneously. In order to estimate how the female and male populations separately respond to peer gender,

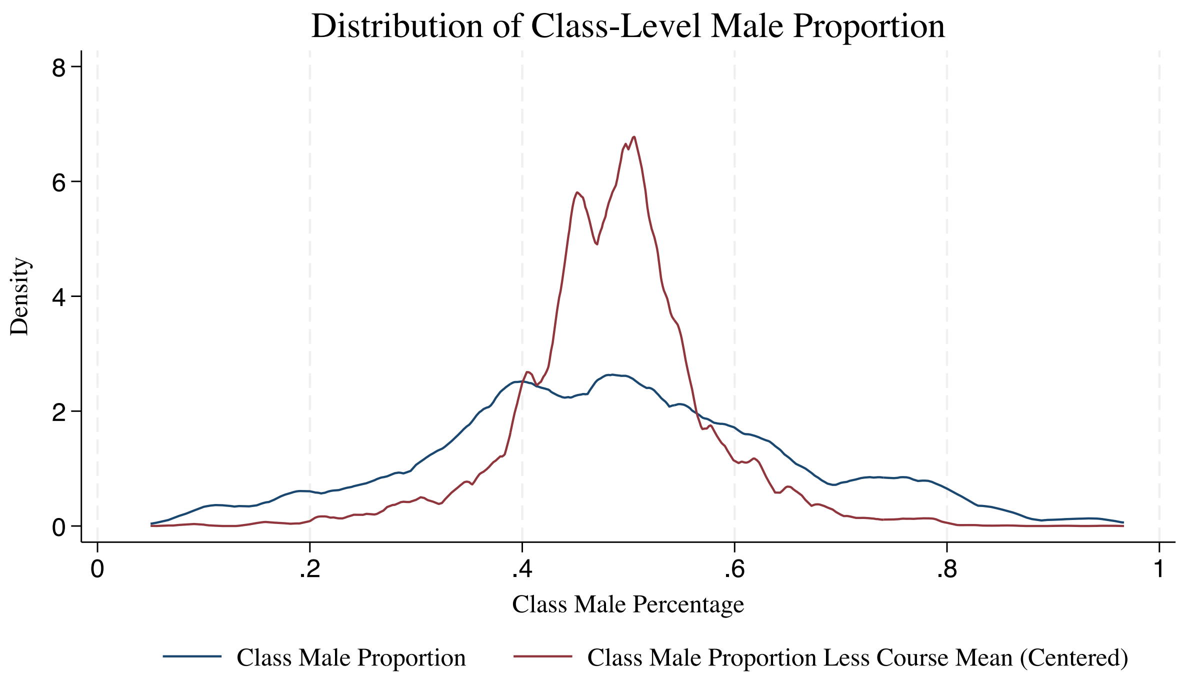

Figure 2: Figure depicts kernel densities of class male proportion across all classes in the main estimation sample. The blue line represents the unadjusted density. The red line represents the density after class male proportion is residualized on course fixed effects.

I estimate within-gender regressions, of the forms

$$Y^F_{i,r,c} = \alpha^F_0 + \alpha^F_1 \times MP_c + \sum_{r=1}^{R} \gamma^F_r \times I_r + (X^F)'_{i,r,c}\beta^F + u^F_{i,r,c} \tag{2}$$

$$Y^M_{i,r,c} = \alpha^M_0 + \alpha^M_1 \times MP_c + \sum_{r=1}^{R} \gamma^M_r \times I_r + (X^M)'_{i,r,c}\beta^M + u^M_{i,r,c} \tag{3}$$

where equation (2) is estimated only on the set of female students and equation (3) is estimated only on the set of males.

By necessity, these specifications exclude class fixed effects.[17] Thus, if there is any kind of absolute sorting to classes that have more male students among students of either gender, this will threaten the validity of my results. While this provides a weaker argument for identification, I again perform the placebo test of putting pre-college characteristics (and the grades predicted by those pre-college characteristics) on the left-hand side of equations (2) and (3). The results of this test are presented in Table 4.

The placebo test shows no strong evidence of students of either gender sorting to more male-dominated classes, within course, in terms of observable background characteristics. Focusing on the predicted grade outcome, which has the most interpretable magnitude,

[17]Class fixed effects can perfectly predict class male proportion. They cannot perfectly predict the interaction of class male proportion with an indicator for being female, which allows for their inclusion in equation (1).

Table 4 – Associations Between Class Male Proportion and Pre-College Characteristics, By Gender

	Standardized ACT score	High school GPA	Underrepresented minority student	Predicted grade (GPA value)
Only women:				
Class male %	0.007	-0.052	-0.063	-0.035
	(0.074)	(0.036)	(0.041)	(0.044)
Outcome Mean	-0.080	3.371	0.506	3.005
Outcome SD	1.005	0.379	0.500	0.411
Observations	16383	16393	22162	12166
Only men:				
Class male %	-0.116	-0.033	0.002	0.006
	(0.087)	(0.041)	(0.042)	(0.047)
Outcome Mean	0.102	3.235	0.542	2.929
Outcome SD	0.985	0.394	0.498	0.406
Observations	15291	15309	21189	10852

Notes: This table reports results of regressions of pre-college characteristics, at a student-class observation level, on class-level male proportion. Each cell corresponds to a separate regression with the outcome given by the column header. Top row results are estimated only on female students and bottom row results only on male students. Each regression includes course fixed effects and a control for instructor gender. The predicted grade outcome is based on another (unshown) regression of the GPA value of grades on ACT score, high school GPA, and minority status among students in the main estimation sample. The predicted grade regression only includes the observations used to form the predicted grades: student-class observations from graded classes that had information on both ACT scores and high school GPA. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

the results indicate that moving from an all-female to an all-male class would be expected to shift a female student's grade by 0.035 GPA points and a male student's grade by 0.006 GPA points, based on the average association between class male percentage and student observables. These predictions are again small not only in an absolute sense but also relative to the estimated effect of male peers on female grades that I report and discuss in Section 3.3.

3 Results

3.1 Main Results

I now present the main estimates of how the gap between female and male outcomes evolves with peer gender composition. Regression coefficients for the interaction between being a female student and class-level male proportion on class and college outcomes are reported in Table 5. I report results from a variety of specifications with differing fixed effects. Results from my preferred specification, characterized by equation (1), are displayed in column 4. In addition, the table presents results when including no fixed effects (column 1), including course and course-specific female fixed effects but no class fixed effects (column 2), and class fixed effects but no course-specific female fixed effects (column 3). Controls that are made redundant by fixed effects are excluded from the relevant specifications. Standard errors are clustered at the class level.

I estimate each model for six outcomes, exploring a range of short- and long-term effects. As an immediate outcome, I consider the GPA point value of the grade received in the class. Longer-term outcomes include dummy variables for whether or not a student is observed to take any future classes in the same academic department as the given class, whether the student switches major to another department (conditional on having declared a major in the department of the given class in the first semester), and whether the student goes on to declare a major in the same department.[18] Because only a small minority of students have declared majors in their first semester at UIC, the switching major outcome is estimated on only a small subset of students and is consequently imprecise. The longest term outcomes are dummy variables for whether or not a student graduates within six years of enrollment and whether or not a student graduates with a declared major in the same academic department as the given class, again within six years of enrollment.[19]

Column 1 of Table 5 reveals a general pattern of women having relatively worse academic performance and lower likelihood of persistence in more male-dominated academic departments. Introducing the full set of fixed effects in column 4 reveals the extent to which this pattern is explained by the causal effect of class-level peer gender composition. Starting with the short-term impact of male peers, the estimate from column 4 suggests that going from a class with no males to a class with all males would drive down the average female grade by a third of a GPA point, relative to men in the same class. However, this result should be interpreted with caution, as the inclusion of individual fixed effects drives the estimate close to zero, as presented in Appendix B. As I discuss further in the appendix section, it does not make sense to include individual fixed effects for most outcomes, so I exclude them from all specifications in Table 5. However, individual fixed effects do provide possible value for the grade outcome. The effect of individual fixed effects on the point estimate of the grade outcome may be concerning for the interpretation

[18]"Departments" are defined according to the UIC academic catalogue: https://catalog.uic.edu/ucat/degree-programs/degree-minors/. These departments can span multiple majors, although the number of majors is usually small. For instance, the English Department houses both the English major and the Teaching of English major.

[19]For the future course and graduation outcomes, I use all student-class observations in the identified set described in Section 2 and described in Table 1. For the grade outcome, I exclude observations where the student did not receive a typical letter grade either because the class only has Satisfactory or Unsatisfactory grades, the student chose to take the class as a "Pass/Fail" class, or the student withdrew, deferred, or received an incomplete in the class. The course being graded as Satisfactory/Unsatisfactory is by far the most common of these situations. For the switching major outcome, I only include students who have declared a major in their first semester, which is a slim minority of total students. For the declaring major outcome, I exclude students who had already declared a major in their first semester (the exact group included for the switching major outcome) as well as a small number of observations corresponding to classes that do not belong to a degree-conferring department (for instance some classes are given a generic Humanities class code, but there is no Humanities department).

Table 5 – Estimated Effect of Class Male Proportion on Student Outcomes

	(1)	(2)	(3)	(4)
Grade (GPA value) [Mean = 2.99]				
Female student X class male %	-0.539***	-0.125	-0.280***	-0.309**
	(0.110)	(0.143)	(0.090)	(0.151)
Observations	33071			
Take a future course in same department [Mean = .64]				
Female student X class male %	-0.013	-0.024	-0.118***	-0.067
	(0.068)	(0.044)	(0.038)	(0.047)
Observations	43351			
Switch major to another department [Mean = .14]				
Female student X class male %	0.372***	-0.034	0.054	0.031
	(0.056)	(0.111)	(0.064)	(0.146)
Observations	4750			
Declare a major in same department [Mean = .05]				
Female student X class male %	-0.018	-0.053**	-0.024	-0.047*
	(0.025)	(0.021)	(0.016)	(0.025)
Observations	37153			
Graduate within six years [Mean = .68]				
Female student X class male %	-0.208***	-0.092*	-0.022	-0.106*
	(0.038)	(0.052)	(0.033)	(0.056)
Observations	43351			
Graduate with a major in same department [Mean = .13]				
Female student X class male %	-0.417***	-0.047*	-0.003	-0.046*
	(0.075)	(0.025)	(0.024)	(0.028)
Observations	43351			
Fixed effects				
Course and course-female	No	Yes	No	Yes
Class	No	No	Yes	Yes
Controls				
Student gender	Yes	No	Yes	No
Instructor gender	Yes	Yes	No	No
Class male %	Yes	Yes	No	No

Notes: This table reports results of regressions of student-class outcomes on an interaction between a female-student dummy and class male proportion. Each cell corresponds to a separate regression, with outcome given by the row header and fixed effects and controls given by the column foot. Each regression includes controls for student underrepresented minority status and instructor-student gender match. All regressions are estimated on the observations within the identifying set of column (4) (as described in Section 1) for which the relevant outcome is observed. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

of the other estimates. However, for the taking-a-future-course-in-the-same-department outcome (the other outcome where individual fixed effects make econometric sense), inclusion of individual fixed effects increases the magnitude and significance of the estimated effect (the point estimate is -0.086 with a p-value of 0.051). I take this as suggestive evidence that effects on “choice” outcomes are more robust than those on “achievement” outcomes and focus on “choice” outcomes for the duration of the paper.

Considering other outcomes reveals that peer gender influences outcomes throughout the college career. My preferred specification suggests that having a first-year class with more men decreases the relative likelihood that women will go on to declare a major related to that class, graduate from college, and graduate with a major related to that class, all relative to male peers in the same class. All of these estimates are statistically significant and large relative to the mean likelihoods of these outcomes. In the estimation sample, going from a class at the 10th percentile of class male proportion to the 90th percentile, within a course, would correspond to a shift in the class-level male proportion of about 0.2. The estimates in Table 5 thus suggest that going from a class at the 10th percentile to a class at the 90th percentile of male proportion would make women 1% less likely to declare and graduate with a related major and 2% less likely to graduate in general, relative to men attending the same lectures.[20] These results suggest that peer gender composition may play a role in reinforcing gender gaps in major uptake and completion in male-dominated majors (which will tend to feature male-dominated classes).

The results are generally similar, in terms of sign, across different specifications. Under the assumption that the results of the specification used in column 4 are the “truth," the high degree of similarity between those results and the column 2 results, which exclude class fixed effects, might be taken as evidence that the exclusion of class fixed effects is not a major threat to identification. This is reassuring for the interpretation of the within-gender results, which are estimated without class fixed effects and are presented in Section 3.3.

3.2 Robustness Checks

The primary threat to the validity of the results presented in the prior section comes from the possibility of differential sorting, whereby the difference between men and women in a class is systematically related to the class gender ratio (holding fixed the average gender ratio among classes of that course). In order to assuage these concerns, I report results from two sets of alternative specifications that may offer stronger arguments against differential sorting. The first set, reported in Appendix A, restricts estimation to the set of courses for which only one class is offered per year. That is, if Economics 101

[20] Greater context on the magnitudes of the estimates is provided in Section 3.3, comparing the implied effect sizes to related estimates from the literature. This discussion is postponed as most papers do not tend to report results in terms of effects on gaps between genders, which is what is being estimated here, but rather in terms absolute effects within genders.

only had one lecture time in each of Fall of 2015 and Fall of 2016, Economics 101 would be included in this subset. For these courses, students in a given cohort do not have the ability to select between class offerings in ways that may be correlated with class gender makeup. The only way students could react to class characteristics so as to create differential sorting would be on the extensive margin of whether or not to take the course at all (during their first semester). Under the assumption that year-specific class characteristics do not have a large impact on course take-up, this specification relies on between-cohort variation in the numbers of men and women interested in a course. The results using this subsample of courses are generally similar to my main results, although less precise.

The other alternative specification focuses on peer gender composition in laboratory, discussion, or practical experience sections that are associated with classes. Because many classes have multiple associated sections, using section-level variation allows for the inclusion of class-specific female fixed effects, which would account for any kind of differential sorting to classes between men and women. The sorting to sections within classes might be considered "more idiosyncratic" than the higher level sorting to classes, as sections have tighter capacity constraints, may have less observable information than classes because some sections do not provide information on the TA in charge (and TAs may have less information available about them than faculty), and students may prioritize class selections over section selections, resulting in sections being subject to greater scheduling constraints (if class times are chosen "first" by students). The section-level analysis provides some supporting evidence for the results reported in the main paper, although the results are generally not statistically significant in the overall sample. Interestingly, it seems that section-level peer effects may be strongly concentrated in sections for female-majority classes. Appendix D further discusses the section-level analysis and reports the results.

3.3 Results Within Gender

Section 3.1 reports how the gap between male and female outcomes changes with peer composition, but does not reveal whether the behavior of men, women, or both drives the pattern. Table 6 presents estimates of how class-level male proportion affects outcomes for students of each gender separately using regressions of the forms of Equations (2) and (3). The point estimates suggest that having more males in a class reduces grades for both male and female students, although the results are not statistically significant. Including individual fixed effects also yields negative, insignificant point estimates for both male and female grades, but with a larger magnitude effect on men. The possibility that male peers reduce achievement for both men and women is consistent with studies from both primary and secondary school contexts (Hoxby (2000); Lavy and Schlosser (2011); Gong et al. (2021)). A variety of mechanisms have been proposed for this pattern, including disruptive behavior (Lavy and Schlosser (2011)) and teacher responses to class composition

Table 6 – Estimated Effect of Class Male Proportion on Student Outcomes, By Gender

	Grade (GPA value)	Future course in dept.	Switch major out of dept.	Declare major in dept.	Graduate within 6 years	Graduate in dept.
Only Women:						
Class male %	-0.170	-0.071**	-0.062	-0.021	-0.069**	-0.005
	(0.106)	(0.029)	(0.065)	(0.016)	(0.034)	(0.018)
Outcome Mean	3.068	0.663	0.141	0.060	0.707	0.135
Outcome SD	1.051	0.473	0.348	0.237	0.455	0.341
Observations	17284	22162	2537	18977	22162	22162
Only Men:						
Class male %	-0.044	-0.045	-0.026	0.032**	0.017	0.040**
	(0.129)	(0.036)	(0.095)	(0.015)	(0.039)	(0.018)
Outcome Mean	2.901	0.625	0.135	0.049	0.647	0.119
Outcome SD	1.143	0.484	0.341	0.216	0.478	0.324
Observations	15787	21189	2213	18176	21189	21189

Notes: This table reports results of regressions of student-class outcomes on class-level male proportion. Each cell corresponds to a separate regression, with outcome given by the column header. Top row results are estimated only on female students and bottom row results only on male students. Each regression includes course fixed effects and controls for student underrepresented minority status, instructor gender, and average classmate high school GPA. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

(Gong et al. (2021)). In the context of UIC, it is the case that that the male population has a lower average high school GPA than the female population. Thus, it might be suspected any effect of male peers is really a function of low-ability peers. However, the regressions reported in Table 6 control for average high school GPA. To the extent that ability is well captured by this measure, it appears that the effect of male students on achievement is not driven by ability and likely reflects other attributes of men or male-dominated environments.

There is a divergence in the signs of the effects of male peers on men versus women for longer term outcomes. Having more male peers makes men slightly and insignificantly more likely to graduate within six years. Female students, on the other hand, are significantly less likely to graduate when they have more male peers in their first-semester classes. The effect of male peers on the gap in graduation between men and women appears to be driven primarily by a negative effect of male peers on women.

Results on choice of major, however, appear to be driven more by men. While female students may be somewhat less likely to declare majors corresponding to their more male-dominated classes, there is a larger, positive effect of male peers on the likelihood of male students choosing a given major. Similarly, I estimate a null effect of male peers on female likelihood of graduating with a related major, contrasting with a significant, positive effect on the same outcome for male students. It seems that the presence of male students encourages other male students to persist in majors while having a more moderate impact on the choices of female students.

As noted previously, in the estimation sample, going from a class at the 10th percentile of class male proportion to the 90th percentile, within a course, would correspond to a shift in the class-level male proportion of about 0.2. The estimates in Table 6 thus imply that going from a 10th percentile class to a 90th percentile class, within a course, would reduce female likelihood of graduation by 1.4 percentage points, increase the likelihood of men declaring a related major by 0.6 percentage points, and increase the likelihood of men graduating in a related department by 0.8 percentage points, on average.

These effect sizes are comparable to related estimates from the literature. Zolitz and Feld (2020) estimate that, at a Dutch business school, an equivalent shift in male proportion in an introductory class would reduce female likelihood of enrolling in a female-dominate major by two percentage points, approximately twice the size of my estimated effect of male peers on male major choice. Carrell et al. (2010) estimate that, at the United States Air Force Academy, having a female instructor in an introductory STEM class makes women 2.86 percentage points more likely to graduate in a STEM department, approximately three times the magnitude of my estimated effect of a 20% increase in male peers on male likelihood of graduating in a related department.

Of course, when considering the full range of classes both within *and* across courses, students are exposed to more extreme variation in peer composition. Across all classes in the estimation sample, going from a a class at the 10th percentile of class male proportion to the 90th percentile would imply a change in class male proportion of 0.46. If my estimates are externally valid to how peer composition affects outcomes when comparing any two classes, rather than just two classes within a course, then going from a 10th percentile class to a 90th percentile class would be expected to make women 3.5 percentage points less likely to graduate and make men 1.9 percentage points more likely to graduate in a related department. However, it is difficult to gauge the extent to which my estimates may be valid for these kinds of counterfactuals.

The fact that male peers encourage men to persist in majors, more so than they discourage female persistence, may be considered surprising given that my results and the literature both suggest that male peers may hurt men's grades. One possible explanation could be that men are more likely to befriend fellow men. Having more male peers in a class may thus encourage friendship formation within that class's department which, in turn, encourages remaining in the department. I explore this possible mechanism in Appendix E and find some evidence to support the theory.

3.4 Heterogeneity of Effects Across Class Types

Previous work on this question has focused on peer gender effects within specific academic programs. An advantage of my approach is that I consider classes across the full breadth of academic programs at UIC. I can thus consider how the estimated effect varies at all different levels of class male proportion. The regression results presented so far have all implicitly assumed that the effect of peer gender is linear in class male proportion. Of course, this need not be the case. Figure 3 presents the results of local linear regressions of female and male outcomes on class male proportions, residualized on the full set of fixed effects and controls used in Equations (2) and (3). For both major declaration and graduation within a department, it appears that women actually out-perform men in both the most female- and male-dominated classes, although the estimates are naturally noisiest at the ends of the distribution and there are no specific points with a significant discrepancy. This implies that the linear point estimates from the Table 5 come from the discrepancy between men and women being *larger* in the most female-dominated classes than in the most male-dominated classes. A simpler pattern exists with the graduation outcome, with women being slightly, and insignificantly, more likely to graduate after taking very female-dominated first-semester classes and there being little noticeable difference between men and women for any other type of class.

Figure 3 was created using data from all classes across all departments. However, there are multiple dimensions to peer gender. Male proportion in a class will naturally be correlated with the department-level average male proportion. It may be possible that there are non-linear effects of peer gender within any given class and also that there are systematic differences in the effects of peer gender between male-dominated and female-dominated departments. For instance, peer gender might have a particular type of effect in STEM classes, which tend to be hosted by male-dominated departments. Figures 4 and 5 present separate local linear regressions among classes that are in female-majority and in male-majority departments in order to more finely consider which types of environments generate the largest discrepancies between male and female outcomes. Most of the female-majority departments are humanities, health-related, or education-related. Most of the male-majority departments are engineering or business-related.[21] Appendix G presents analogous linear regressions in the female-majority-department and male-majority-department subsamples.

The figures for female-majority departments all look quite similar to the figures for the entire sample. There is, again, female out-performance of men in terms of major declaration and graduation within a department for the most male-dominated and the most female-dominated classes, although the estimates at the tails are imprecise. For overall graduation likelihood, the female and male regression lines track each other nearly exactly. There are, again, no specific points where the difference between genders is statistically significant.

A clearer story emerges within male-majority departments. Among these departments, major declarations, graduation likelihood, and likelihood of graduating in department all follow a similar patter. When classes are most female-dominated (in a generally

[21] The full list of female- and male-majority departments is given in Appendix F.

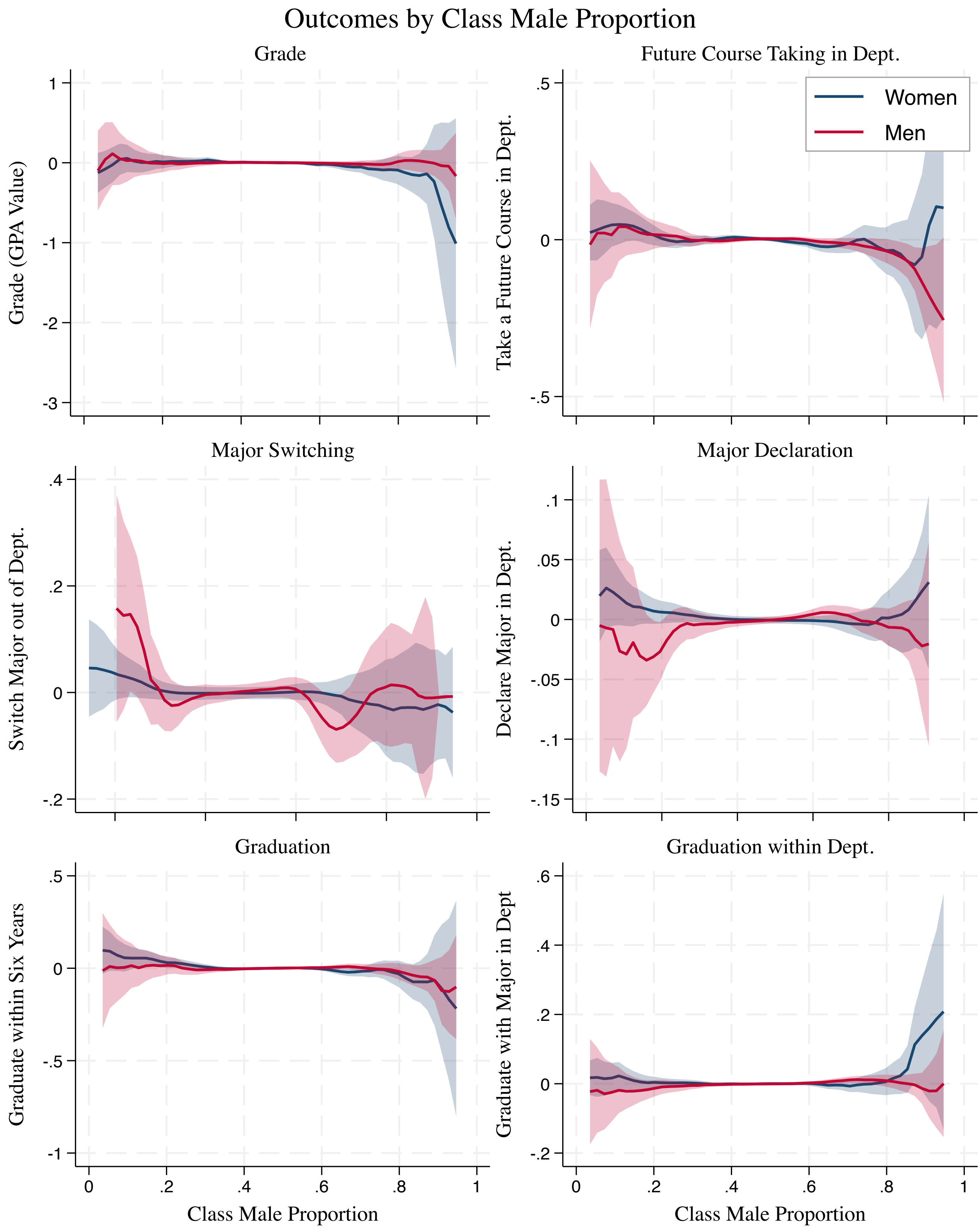

Figure 3: Figures are estimated using observations from the main estimation sample for which the outcome is observed. Blue and red lines represent local linear regression lines of the outcomes on class male proportions for women and men respectively, with both variables residualized on course fixed effects and controls for student minority status and instructor gender. Shaded areas show 95% confidence intervals.

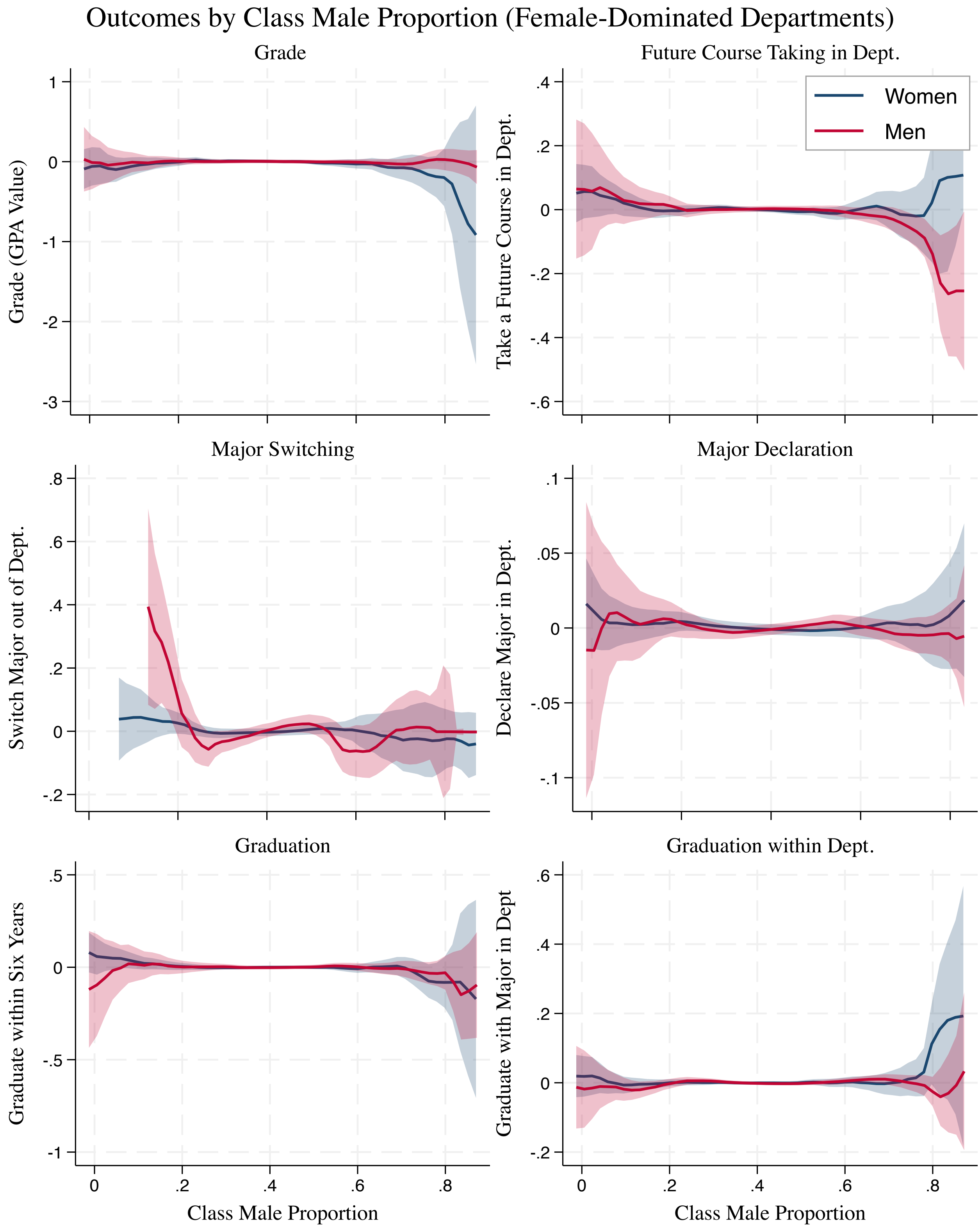

Figure 4: Figures are estimated using observations from the main estimation sample for which the outcome is observed and the class is in a department with an overall female majority of students. Blue and red lines represent local linear regression lines of the outcomes on class male proportions for women and men respectively, with both variables residualized on course fixed effects and controls for student minority status and instructor gender. Shaded areas show 95% confidence intervals.

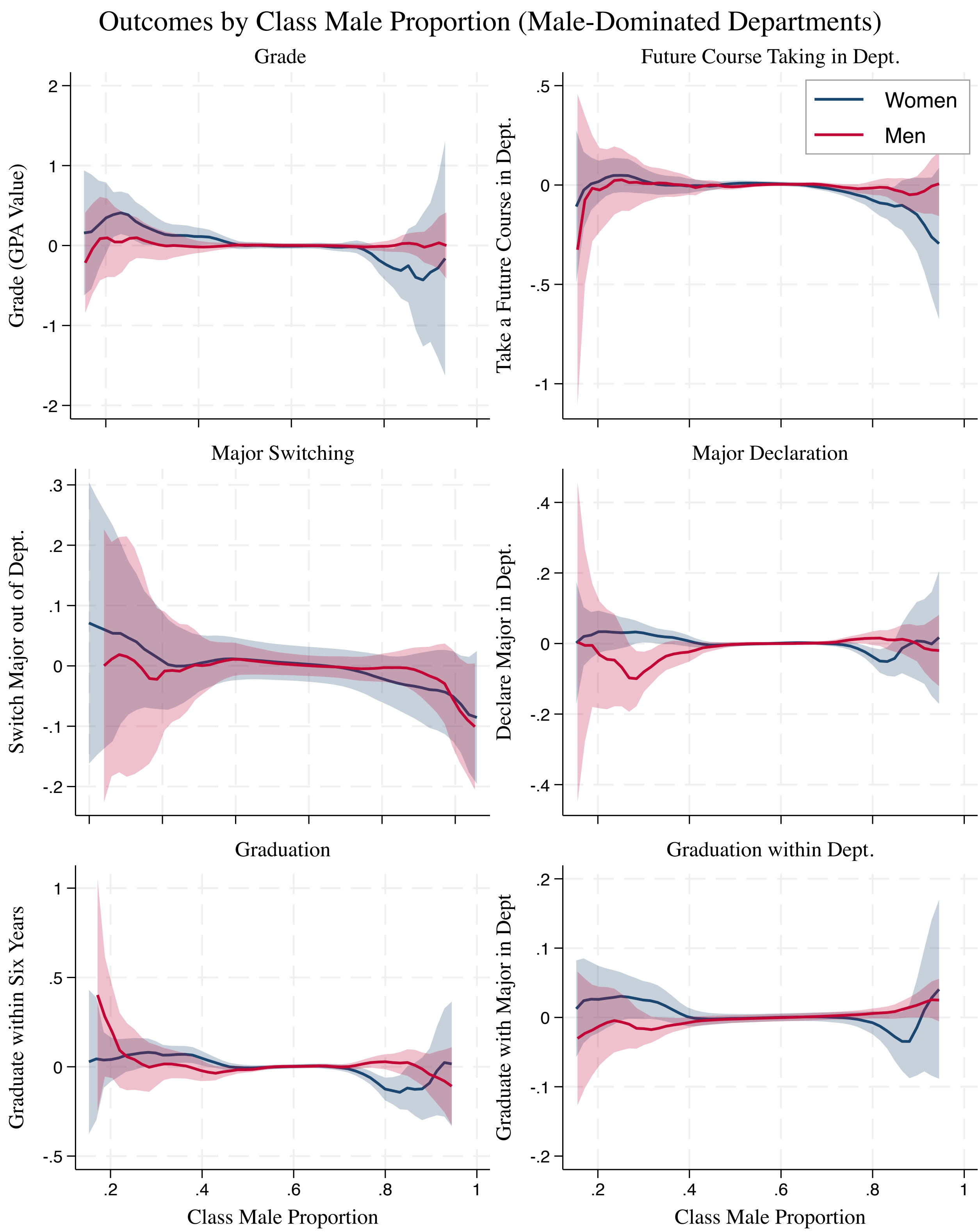

Figure 5: Figures are estimated using observations from the main estimation sample for which the outcome is observed and the class is in a department with an overall male majority of students. Blue and red lines represent local linear regression lines of the outcomes on class male proportions for women and men respectively, with both variables residualized on course fixed effects and controls for student minority status and instructor gender. Shaded areas show 95% confidence intervals.

male-dominated field) women are relatively more likely to declare a related major and graduate with that major. When classes are most male-dominated, women are relatively less likely to declare a related major, graduate, and graduate with a related major. Men exhibit the opposite pattern. The gender differences in major declaration and graduation are statistically significant at some of the most extreme values of class male proportion (at either end). The linear regression results from Appendix G similarly suggest that effects of male peers on the female-male gap are strongly concentrated in male-dominated departments. Thus, the overall patterns reported in Tables 5 and 6 are primarily driven by classes in male-majority fields.

These results have implications for policymakers interested in encouraging female persistence in traditionally male-dominated academic fields (a topic which tends to get more public and political attention than the reverse).[22] Within the UIC sample, the more women there are in a class in a male-dominated field, the more likely other women are to persist in that field. Enrollment policies that prevent large male majorities in introductory classes may thus encourage female persistence in related fields. These results also suggests that any policy that encourages women to take introductory classes in a male-dominated field may increase female graduation in those fields by two mechanisms. Increasing introductory enrollment mechanically increases the number of candidates to graduate in that field. In addition, the presence of more women in an introductory class may also behaviorally encourage other women in that class to remain in the field long term.

4 Conclusion

Gender differences in college major take-up and completion are of interest to policymakers due to the substantial labor market implications. The mechanisms that create and perpetuate these gender differences are imporant to understand. I find that when a first-semester class has more male students, women are less likely to declare and to graduate with majors associated with that class relative to men attending the same lectures. While those results pertain to the gap in outcomes between men and women, I also separately estimate student responses by gender in order to determine whether the results are driven by male or female behavior. I find that women are less likely to eventually graduate when they have more male peers while male achievement is not substantially affected by peer gender. On the other hand, when a first-semester class has more men, men are more likely to declare a related major while the effect on women's major choice is negative but smaller in magnitude than the effect on men.

These aggregate findings mask important heterogeneity. Considering the full breadth

[22]There are, for instance, many national and local organizations intended to support women in male-dominated academic fields. Three prominent examples include Girls Who Code, Society of Women Engineers, and Association for Women in Science. There are no comparably large organizations intended to support men in female-dominated fields.

of academic departments at the University of Illinois Chicago, I find that the effects of male peers are largest and clearest in departments that have more men than women overall. Peer gender thus provides a mechanism that reinforces male concentration in fields that are already male-dominated. Notably, many of the departments that are male-majority are associated with the highest post-college wages, including most of the engineering departments and economics. Within these male-majority departments, gender differences in outcomes were largest in the classes at the extremes of peer gender composition. Women had relatively higher persistence when classes had large female majorities and men had relatively higher persistence when classes had large male majorities. Policy-makers may thereby be able to encourage female persistence in traditionally male-dominated disciplines by preventing introductory classes in those disciplines from having very large male majorities.

There are several interesting directions to take future work on this topic. While my analysis necessarily focuses on first-semester classes, it may be interesting to consider how peer gender affects persistence in fields throughout the entire college experience and into the labor market. Future work may also consider the mechanisms by which peer gender influences major choices. I provide suggestive evidence that male peers may encourage male persistence by increasing the likelihood of forming within-major friendships, but this analysis is speculative and there may be other important mechanisms at play. It also remains to consider how to practically prevent large male majorities in introductory classes for fields that have mostly male interest.

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Appendix A - Separating Within-Semester and Across-Semester Variation

The results presented throughout the main body of the text utilize variation in classes within courses both between the Fall of 2015 and the Fall of 2016 and across lecture times within each semester. However, each form of variation may introduce separate identification concerns. I present here results that isolate the two forms of variation.

Table A1 only uses within-semester variation. The table presents estimates from equation (1), but including course-by-year-specific female fixed effects rather than course-specific female fixed effects. That is, in lieu of a fixed effect for women in Econ 101, I include one fixed effect for women in Econ 101 in Fall of 2015 and another fixed effect for women in Econ 101 in Fall of 2016.

Table A2 presents results for only across-semester variation. The table presents estimates from equation (1) but with the sample restricted to courses for which there is only one lecture time each in Fall of 2015 and Fall of 2016. The point estimates in Table A1 are quite similar to the main results presented in Table 5 column 4, albeit less precise. The point estimates in Table A2 all have the same sign as the main results and are all substantially larger in magnitude, although, again, less precise. In general, it does not appear that within- or across-semester variation in isolation is driving the overall results.

Table A1 – Estimated Effect of Class Male Proportion on Student Outcomes, Using only Within-Semester Variation

	Grade (GPA value)	Future course in dept.	Switch major out of dept.	Declare major in dept.	Graduate within 6 years	Graduate in dept.
Female student X	-0.227	-0.064	-0.083	-0.031	-0.078	-0.032
class male %	(0.169)	(0.051)	(0.231)	(0.024)	(0.061)	(0.027)
Outcome Mean	2.988	0.644	0.138	0.054	0.677	0.127
Outcome SD	1.099	0.479	0.345	0.227	0.467	0.333
Observations	33071	43351	4750	37153	43351	43351

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and class male proportion. Each regression includes course-by-year-specific female fixed effects, class fixed effects, and controls for student underrepresented minority status and instructor-student gender match. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

Table A2 – Estimated Effect of Class Male Proportion on Student Outcomes, Using only Between-Semester Variation

	Grade (GPA value)	Future course in dept.	Switch major out of dept.	Declare major in dept.	Graduate within 6 years	Graduate in dept.
Female student X	-0.463	-0.131	0.168	-0.064	-0.322*	-0.133
class male %	(0.291)	(0.150)	(0.181)	(0.078)	(0.183)	(0.101)
Outcome Mean	3.057	0.561	0.110	0.065	0.716	0.239
Outcome SD	1.051	0.496	0.313	0.246	0.451	0.426
Observations	6758	8481	1986	5848	8481	8481

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and class male proportion. Each regression includes course-specific female fixed effects, class fixed effects, and controls for student underrepresented minority status and instructor-student gender match. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed and the given course has only class in each of Fall of 2015 and Fall of 2016. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

Appendix B - Inclusion of Individual Fixed Effects

My primary estimating equation, equation (1), includes course-specific female fixed effects and class fixed effects as well as other individual- or class-level control variables. Because there are multiple individual-class-level observations per student, it is mathematically possible to include individual student fixed effects in the model. However, I choose to exclude such fixed effects from my main regressions. I discuss this issue and present results including individual fixed effects here.

Throughout the paper I consider six primary outcomes: grades, whether or not a student takes a future course in the same department as the current course, whether or not a student switches majors to a different department (conditional on having a declared a major in the department of the current course), whether or not a student declares a major in the department of the current course, whether or not a student graduates within six years, and whether or not a student graduates with a major in the department of the current course. Two of these outcomes, graduation and switching majors, are defined at an individual, rather than an individual-course, level. Thus, individual fixed effects perfectly predict these outcomes and there is no residual variation in the outcomes to estimate the causal effect of interest. I report results with individual fixed effects in Table B1 column (5). As would be expected, for both of these outcomes the estimated effect is precisely zero and the R^2 is exactly equal to one.

For the major declaration and graduation in a department outcomes there is a slightly more subtle problem. If a given student only takes classes within a given department, their individual fixed effect, in isolation, can predict their outcome with certainty. If they take classes in multiple departments, it is unclear what the fixed effect is controlling for, as the large majority of students only ever declare a single major.[23] For instance, if a student takes two classes, one in the English Department and one in the Economics Department, the student will most likely declare a major in one (or neither) of these two departments. An individual fixed effect thereby cannot effectively control for any attribute that may encourage the student to pick one of the two departments over the other, given that the fixed effect takes on the same value in predicting the major declaration outcome for both departments. Thus, the individual fixed effect has no econometric value for students with any type of class schedule.

This leaves two remaining outcomes for which individual fixed effects may present econometric value: grades and taking a future course in a given department.[24] As seen in Table B1, the inclusion of individual fixed effects drives the point estimate on grades close to zero and renders it insignificant. For this reason, I do not emphasize the grade outcome

[23] 95.04% of students in the sample hold one major at the time of graduation.

[24] While most students only declare one major, it is easier and more likely to take at least one additional class in multiple departments. For the future-class outcome, the individual fixed effect may thus plausibly control for individual attributes like a predisposition towards sustained academic interest in a subject or stick-to-itivness.

Table B1 – Estimated Effect of Class Male Proportion on Student Outcomes Under Varying Sets of Fixed Effects

	(1)	(2)	(3)	(4)	(5)
Grade (GPA value) [Mean = 2.99]					
Female student X class male %	-0.536***	-0.125	-0.276***	-0.309**	0.021
	(0.110)	(0.143)	(0.090)	(0.151)	(0.128)
Observations	33071				
R^2	0.022	0.144	0.195	0.206	0.761
Take a future course in same department [Mean = .64]					
Female student X class male %	-0.005	-0.024	-0.114***	-0.067	-0.086*
	(0.068)	(0.044)	(0.037)	(0.047)	(0.051)
Observations	43351				
R^2	0.006	0.305	0.324	0.335	0.617
Switch major to another department [Mean = .14]					
Female student X class male %	0.374***	-0.030	0.058	0.041	0.000
	(0.058)	(0.111)	(0.066)	(0.147)	(0.000)
Observations	4750				
R^2	0.024	0.341	0.396	0.424	1.000
Declare a major in same department [Mean = .05]					
Female student X class male %	-0.017	-0.053**	-0.024	-0.047*	0.016
	(0.025)	(0.021)	(0.016)	(0.025)	(0.024)
Observations	37153				
R^2	0.001	0.412	0.433	0.447	0.673
Graduate within six years [Mean = .68]					
Female student X class male %	-0.208***	-0.092*	-0.022	-0.106*	-0.000
	(0.039)	(0.052)	(0.033)	(0.056)	(0.000)
Observations	43351				
R^2	0.030	0.102	0.130	0.140	1.000
Graduate with a major in same department [Mean = .13]					
Female student X class male %	-0.414***	-0.047*	-0.002	-0.046*	0.007
	(0.075)	(0.025)	(0.024)	(0.028)	(0.029)
Observations	43351				
R^2	0.015	0.492	0.506	0.520	0.715
Fixed effects					
Course and course-female	No	Yes	No	Yes	Yes
Class	No	No	Yes	Yes	Yes
Individual	No	No	No	No	Yes

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and class male proportion. Each cell corresponds to a separate regression, with outcome given by the row header and fixed effects given by the column foot. All regressions include controls for student underrepresented minority status, student gender, instructor gender, instructor-student gender match, and class male percentage, excluding controls which are rendered redundant by fixed effects. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

in the main paper. The estimate on the future-course outcome, on the other hand, is larger in magnitude and more statistically significant than in the main estimating equation, presented in column (4). In order to be consistent in my choice of primary estimating equation, I also do not emphasize this result in the main paper. However, I do take it as evidence that the effect of peer gender on longer-term, choice-based outcomes in general are robust to controlling for individual-level attributes.

Appendix C - Alternative Control Schemes

Table C1 reports the results of estimating equation (1) while including both course-specific female fixed effects and class fixed effects but varying the sets of additional student and class-student controls. The specifications in column 1 include no controls beyond the fixed effects, column 2 specifications include controls for student minority status and student-instructor gender match (just as in column 4 of Table 5 in the main body of the paper), and column 3 includes the same controls as column 2 in additional to controls for high school GPA and ACT composite score (which I exclude from regressions reported in the main paper due to missing values for a non-negligible group of students). Table C1 reveals that the set of controls used has very little impact on the estimated effects of class male proportion on student outcomes. Importantly, including controls for individual pre-college academic aptitude has little impact on the qualitative interpretation of the results. It does however curtail the precision of the results, due to the required exclusion of the portion of the sample that is missing information on either high school GPA, ACT score, or both.

Table C1 – Estimated Effect of Class Male Proportion on Student Outcomes Under Varying Sets of Controls

	(1)	(2)	(3)
Grade (GPA value) [Mean = 2.99]			
Female student X class male %	-0.319**	-0.309**	-0.227
	(0.150)	(0.151)	(0.175)
Observations	33071	33071	23018
Take a future course in same department [Mean = .64]			
Female student X class male %	-0.067	-0.067	-0.079
	(0.047)	(0.047)	(0.055)
Observations	43351	43351	31645
Switch major to another department [Mean = .14]			
Female student X class male %	0.018	0.031	0.255
	(0.149)	(0.146)	(0.649)
Observations	4750	4750	1232
Declare a major in same department [Mean = .05]			
Female student X class male %	-0.047*	-0.047*	-0.049*
	(0.025)	(0.025)	(0.028)
Observations	37153	37153	29710
Graduate within six years [Mean = .68]			
Female student X class male %	-0.111*	-0.106*	-0.104
	(0.057)	(0.056)	(0.066)
Observations	43351	43351	31645
Graduate with a major in same department [Mean = .13]			
Female student X class male %	-0.047*	-0.046*	-0.052*
	(0.028)	(0.028)	(0.029)
Observations	43351	43351	31645
Controls			
Student underrepresented minority status	No	Yes	Yes
Student-instructor gender match	No	Yes	Yes
High school GPA	No	No	Yes
ACT composite score	No	No	Yes

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and class male proportion. Each cell corresponds to a separate regression, with outcome given by the row header and controls given by the column foot. Each regression includes course-specific female fixed effects and class fixed effects. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

Appendix D - Section-Level Analysis

The results reported in the main body of the paper exploit variation in peer composition across classes within a course. This strategy assumes that there are no systematic gendered sorting to the classes, within courses, that have more male students. This assumption is supported by the fact that first-semester students enroll in classes with little or no access to information about peer enrollments and by the comparable observables of students across classes with differing gender ratios, as reported in Table 2. However, there could still be reasonable concern that sorting to class characteristics, such as time slot or instructor traits, may induce differential sorting on unobservables between men and women, which would bias my results.

For classes that have associated laboratory, discussion, or practical experience sections, there is an additional level of variation to exploit: peer composition across sections, within classes. Utilizing this variation allows for any pattern of sorting to classes by students. Instead, identification requires the assumption that, within a class, there is not meaningful sorting to sections that have more men. There are reasons why sorting to sections within a class may be "more" exogenous than the initial sorting to classes: sections have tighter capacity constraints, differences between sections may be less visible or salient than differences between classes, and section choices may be more constrained by schedules if choice of preferred classes is prioritized by students over choice of preferred sections. However, focusing on sections, rather than classes, introduces the notable drawback of substantially reducing the sample size, as not all classes have sections, and reducing useful variation, as no classes have as many different sections as there are different classes within certain courses. Moreover, estimation using section-level variation implies a different parameter than estimation using class-level variation. It may be that class peers and section peers matter differently, depending on the mechanisms via which peers influence outcomes.

For my analysis of the effects of section peers on student outcomes, I emphasize two estimating equations. Analogous to my primary estimating equation for class-level analysis, equation (1), I estimate

$$Y_{i,r,c,s} = \pi_0 + \pi_1 \times Fem_i \times MP_s + \sum_{c=1}^{C} \rho_c \times Fem_i \times I_c + \sum_{s=1}^{S} \upsilon_s \times I_s + X'_{i,r,c,s}\tau + \varepsilon_{i,r,c,s} \tag{4}$$

where sections are indexed by $s \in \{1, ..., S\}$, MP_s denotes the proportion of male students in section s, I_s is an indicator variable that takes a value of one if outcome $Y_{i,r,c,s}$ is associated with section s, and $\varepsilon_{i,r,c,s}$ is the error term. π_1 captures how the gap between female and male outcomes evolves with MP_s. This specification includes both class-specific female fixed effects, ρ_c, section fixed effects, υ_s, and controls for student and student-section characteristics, τ. This allows for both any kind of sorting to classes and gender-neutral sorting to sections, meaning that differential sorting to sections within class by men compared to women is the only threat to identification.

In practice, the results of estimating equation (4) are too imprecise to be interpretable. I therefore focus on a specification without section fixed effects,

$$Y_{i,r,c,s} = \eta_0 + \eta_1 \times Fem_i \times MP_s + \sum_{c=1}^{C} \theta_c \times Fem_i \times I_c + \iota_c \times I_c + X'_{i,r,c,s}\kappa + \varepsilon_{i,r,c,s} \tag{5}$$

This specification still includes class-specific female fixed effects, here denoted θ_c, but foregoes section fixed effects for class fixed effects, ι_c. Identification with this model relies upon an assumption of no sorting, absolute or differential, to sections within a class. This is similar in spirit to the within-gender, class-level analysis presented in the main text.

I present the results of estimating equations (4) and (5) on pre-college characteristics, and the grades predicted by those characteristics, in Table D1. The top panel displays the results of estimating equation (4) and the bottom the results for equation (5). The estimates show that the differences between observable male and female characteristics do not change systematically with section-level male proportion, whether conditioning on section fixed effects or not. Importantly, observable differences between students predict no change in grade differential between men and women when going from a wholly female to a wholly male section, again whether conditioning on section fixed effects or not. I interpret this as evidence that the identifying assumptions for both specifications are satisfied in this sample.

Table D2 presents the estimated effects of section peer composition on student outcomes, across different specifications. Column 1 includes no fixed effects and documents a strong negative association between female academic performance and major choice with section-male percentage, matching the pattern seen in the class-level analysis. Column 4 reports the results of estimating equation (4), yielding results that, as previously mentioned, are too imprecise to make meaningful inference from. Column 2 reports results from equation (5), which I consider the preferred specification for the section-level analysis. Here, although no results are significant at conventional levels, there is suggestive evidence of a similar pattern to the results of the class-level analysis: worse female academic achievement in the face of more male peers. The strongest evidence of a section peer effect is for grades, with relatively meager evidence of effects on major choice.

I conclude my discussion of the section-level analysis with one interesting note on the distribution of estimated effects of section peer gender. It seems that any negative effect male section peers have on women, relative to men, is concentrated among female-majority classes. Table D3 reports results on the subset of classes that are female-majority while Table D4 does the same for classes that are male-majority. Focusing on the second column of Table D3, the point estimates of the effects of male section peers among female-majority classes are quite similar to the estimated effects of male class peers that are reported in the

Table D1 – Associations Between Section Male Proportion and Pre-College Characteristics

	Standardized ACT score	High school GPA	Underrepresented minority student	Predicted grade (GPA value)
With section fixed effects:				
Female student X	-0.140	0.063	-0.015	0.093
section male %	(0.185)	(0.076)	(0.082)	(0.099)
Outcome Mean	0.000	3.282	0.517	2.783
Outcome SD	1.000	0.385	0.500	0.452
Observations	13604	13612	19106	11870
With class fixed effects:				
Female student X	-0.000	0.004	0.007	0.024
section male %	(0.110)	(0.042)	(0.047)	(0.055)
Outcome Mean	0.000	3.282	0.517	2.783
Outcome SD	1.000	0.385	0.500	0.452
Observations	13604	13612	19106	11870

Notes: This table reports the results of regressions of pre-college characteristics, at a student-class observation level, on an interaction between a female-student dummy and the section-level male proportion. Each cell corresponds to a separate regression. Top panel regressions include section fixed effects. Bottom panel regressions include class fixed effects. All regressions include class-specific female fixed effects. The predicted grade outcome is based on another (unshown) regression of the GPA value of grades on ACT score, high school GPA, and minority status among students in the main estimation sample. The predicted grade regressions only include the observations used to form the predicted grades: student-class observations from graded classes that had information on both ACT scores and high school GPA. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

main body of the paper. The same is not true among male-majority classes, as seen in Table D4. In particular, coefficients on the differential effects of male peers on grades, major declarations, and graduation in a department are all negative in Table D3 and much larger in magnitude in Table D3 than in Table D4. It would be interesting to see if future work, which may have more data on sections, documents a similar pattern. Considering why the effects are concentrated among certain classes could help shed light on the mechanisms by which peers influence outcomes.

Table D2 – Estimated Effect of Section Male Proportion on Student Outcomes

	(1)	(2)	(3)	(4)
Grade (GPA value) [Mean = 2.83]				
Female student X section male %	-0.619***	-0.206	-0.196	-0.083
	(0.126)	(0.178)	(0.134)	(0.215)
Observations	16939			
Take a future course in same department [Mean = .64]				
Female student X section male %	-0.178***	-0.052	-0.190***	0.002
	(0.062)	(0.059)	(0.048)	(0.076)
Observations	19106			
Switch major to another department [Mean = .15]				
Female student X section male %	0.412***	-0.060	0.174	0.002
	(0.088)	(0.191)	(0.131)	(0.394)
Observations	2052			
Declare a major in same department [Mean = .08]				
Female student X section male %	-0.034	-0.015	-0.054*	-0.013
	(0.031)	(0.029)	(0.030)	(0.040)
Observations	16292			
Graduate within six years [Mean = .67]				
Female student X section male %	-0.228***	-0.048	-0.012	0.074
	(0.041)	(0.067)	(0.054)	(0.086)
Observations	19106			
Graduate with a major in same department [Mean = .14]				
Female student X section male %	-0.498***	-0.029	-0.071**	0.015
	(0.076)	(0.038)	(0.033)	(0.044)
Observations	19106			
Fixed effects				
Class and class-female	No	Yes	No	Yes
Section	No	No	Yes	Yes
Controls				
Student gender	Yes	No	Yes	No
TA gender	Yes	Yes	No	No
Section male %	Yes	Yes	No	No

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and section male proportion. Each cell corresponds to a separate regression, with outcome given by the row header and fixed effects and controls given by the column foot. Each regression includes controls for student underrepresented minority status and instructor-TA gender match. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

Table D3 – Estimated Effect of Section Male Proportion on Student Outcomes, Among Female Majority Classes

	(1)	(2)	(3)	(4)
Grade (GPA value) [Mean = 2.83]				
Female student X section male %	-0.240	-0.330	-0.184	-0.056
	(0.193)	(0.233)	(0.255)	(0.317)
Observations	8236			
Take a future course in same department [Mean = .64]				
Female student X section male %	-0.156	-0.116	-0.148	-0.127
	(0.097)	(0.082)	(0.096)	(0.101)
Observations	9552			
Switch major to another department [Mean = .15]				
Female student X section male %	0.534***	0.416	0.016	0.399
	(0.172)	(0.308)	(0.156)	(0.637)
Observations	998			
Declare a major in same department [Mean = .08]				
Female student X section male %	-0.028	-0.034	-0.112**	-0.054
	(0.049)	(0.043)	(0.051)	(0.057)
Observations	8221			
Graduate within six years [Mean = .67]				
Female student X section male %	-0.130*	0.086	0.058	0.182
	(0.073)	(0.088)	(0.092)	(0.113)
Observations	9552			
Graduate with a major in same department [Mean = .14]				
Female student X section male %	-0.280***	-0.101*	-0.114**	-0.055
	(0.076)	(0.057)	(0.051)	(0.062)
Observations	9552			
Fixed effects				
Class and class-female	No	Yes	No	Yes
Section	No	No	Yes	Yes
Controls				
Student gender	Yes	No	Yes	No
TA gender	Yes	Yes	No	No
Section male %	Yes	Yes	No	No

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and section male proportion. Each cell corresponds to a separate regression, with outcome given by the row header and fixed effects and controls given by the column foot. Each regression includes controls for student underrepresented minority status and instructor-TA gender match. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed and the given class is female-majority. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

Table D4 – Estimated Effect of Section Male Proportion on Student Outcomes, Among Male Majority Classes

	(1)	(2)	(3)	(4)
Grade (GPA value) [Mean = 2.83]				
Female student X section male %	-0.374*	-0.010	-0.099	-0.069
	(0.192)	(0.264)	(0.209)	(0.284)
Observations	8703			
Take a future course in same department [Mean = .64]				
Female student X section male %	-0.072	0.044	0.026	0.156
	(0.092)	(0.093)	(0.087)	(0.115)
Observations	9554			
Switch major to another department [Mean = .15]				
Female student X section male %	-0.080	-0.911*	0.041	-0.253
	(0.211)	(0.497)	(0.273)	(0.569)
Observations	1054			
Declare a major in same department [Mean = .08]				
Female student X section male %	0.123***	0.006	0.055	0.033
	(0.046)	(0.039)	(0.049)	(0.054)
Observations	8071			
Graduate within six years [Mean = .67]				
Female student X section male %	-0.296***	-0.155*	-0.128	-0.044
	(0.066)	(0.094)	(0.087)	(0.123)
Observations	9554			
Graduate with a major in same department [Mean = .14]				
Female student X section male %	-0.280***	0.046	0.003	0.091
	(0.097)	(0.046)	(0.057)	(0.060)
Observations	9554			
Fixed effects				
Class and class-female	No	Yes	No	Yes
Section	No	No	Yes	Yes
Controls				
Student gender	Yes	No	Yes	No
TA gender	Yes	Yes	No	No
Section male %	Yes	Yes	No	No

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and section male proportion. Each cell corresponds to a separate regression, with outcome given by the row header and fixed effects and controls given by the column foot. Each regression includes controls for student underrepresented minority status and instructor-TA gender match. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed and the given class is male-majority. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

Appendix E - Evidence on the Effect of Class Gender Composition on Friendship Formation

My results imply that male peers encourage men to persist in majors, possibly to a greater extent than they discourage women from persisting. This results may be surprising given that both the UIC data and the literature suggest that male peers either have no or negative effects on men's academic performance.[25] It thereby remains to consider the mechanism by which peer gender may influence male major choice. Prior work has found that having at least one peer choose a given major may increase the likelihood of take-up of that major, with one potential explanation being the direct utility value of studying with a friend (De Giorgi et al. (2010)). If men are more likely to form friendships with other men, then having a more male-dominated first class in a major may increase the expected number of friends who are interested in the same major. This could then translate into an increased willingness to declare and persist in that major.

While I cannot directly observe friendships in the data, I provide suggestive evidence using taking classes together as a proxy. Specifically, I consider all two-course sequences that are required for at least one of the twenty most popular majors at UIC and are commonly taken by first year students. A two-course sequence is defined as a set of two courses with one being a pre-requisite for the other. For instance, General Chemistry I and General Chemistry II is a two-course sequence that is required for the Biology and Chemistry majors, among others, and is recommended as first-year coursework for students interested in those majors. For students who take the first course in one of these sequences during their first semester and take the second course in a later semester, I define as outcomes the number of peers, of either gender, who were in the same class for both the first and second course and the number of peers who were in the same laboratory or discussion section for both the first and second course.[26]

I estimate the association between class-level male proportion and the number of same-subsequent-class peers using equations (2) and (3). Similarly, I look at the link between section-level male proportion and same-subsequent-section peers using the estimating equations

$$Y^F_{i,r,c,s} = \eta^F_0 + \eta^F_1 \times MP_s + \sum_{c=1}^{C} \theta^F_c \times I_c + \varepsilon^F_{i,r,c,s} \tag{6}$$

$$Y^M_{i,r,c,s} = \eta^M_0 + \eta^M_1 \times MP_s + \sum_{c=1}^{C} \theta^M_c \times I_c + \varepsilon^M_{i,r,c,s} \tag{7}$$

[25] See, for instance, Hoxby (2000), Lavy and Schlosser (2011), and Gong et al. (2021).

[26] This necessarily means that students who only take the first course in a sequence and never take the second are excluded. It also means that each peer pair is "double-counted" in the sense that any pair of students taking the same class for both courses in a sequence will be reflected in the outcome variable of both students.

where sections are indexed by s, MP_s denotes the proportion of male students in section s, η_1^g measures how the number of repeated peers evolves with MP_s for students of gender g, θ_c^g are class fixed effects, and $\varepsilon_{i,r,c,s}^g$ is the error term. Equation (6) is estimated only on the sample of female students and equation (7) only on male students. I utilize eleven total sequences between the class and section analysis.[27]

It is almost certainly the case that some students will enroll in the same class twice in a row by chance. However, male students being systematically more likely to take future classes with the same peers when their initial class has more men may represent an increased likelihood of forming friends and coordinating future enrollment with them.

The results of this exercise are presented in Table E1. While the estimated effects of peer composition on the number of same-subsequent-class peers are imprecise, the results suggest that more male-dominated classes tend to result in more continued peers in the next class for both men *and* women. Similarly, having more men in the section of a first class increases the likelihood of having any same-subsequent-section peers for both men and women, even within a given class. The observed results may reflect homophily in friendship formation among men with some complementary explanation for women, some greater overall degree of friendship formation in more male-dominated environments, or other explanations. In any case, if men are more likely to form same-major friends in more male-dominated environments, this offers one plausible mechanism for greater male persistence in majors associated to male-dominated classes, in spite of male peers seemingly not providing academic benefits to male students.

Table E1 reports statistical significance based on the null hypotheses of coefficients equalling zero. However, it is not clear that the absence of intentional sorting to classes with prior classmates implies a zero coefficient. For instance, the point estimates in Table 6 would suggest that male peers encourage male persistence in major to a greater extent than they discourage female persistence. These coefficients would imply that more male-heavy classes would have greater net persistence into related majors, which could mechanically generate the patterns seen in Table E1 (if students are more likely to take the second course in a sequence if they are also persisting in a related major). To address this, I benchmark the estimates I get from the data against simulated coefficients.

[27] For class-level results, I use all sequences for which there are at least two classes for the first course in the sequence in both Fall 2015 and Fall 2016: General Chemistry I and General Chemistry II; Introduction to Psychology and Introduction to Research in Psychology; Introduction to UIC and Professional Development and Business Professional Development II; Calculus I and Calculus II; Calculus II and Calculus III; General Physics I and General Physics II; Principles of Microeconomics and Microeconomics: Theory and Applications; Principles of Macroeconomics and Macroeconomics in the World Economy: Theory and Applications; and Biology of Cells and Organisms and Biology of Populations and Communities. For the section-level results, I use the same set of sequences, excluding Introduction to UIC and Professional Development and Business Professional Development II as they have no associated sections and including Program Design I and Program Design II and Introduction to Criminology, Law, and Justice and Foundations of Law and Justice as, although there is not sufficient variation in these sequences for class-level analysis, there is enough for section-level analysis.

Table E1 – Estimated Effect of Class and Section Male Proportion on the Number of Peers Taking Future Classes Together

	Number of peers in the same subsequent class	Number of peers in the same subsequent section
Only women:		
Class male %	4.738	-
	(3.679)	-
Section male %	-	0.362*
	-	(0.218)
Outcome Mean	9.278	0.348
Outcome SD	10.665	0.754
Observations	1027	880
Only men:		
Class male %	4.394	-
	(2.654)	-
Section male %	-	0.306
	-	(0.207)
Outcome Mean	8.459	0.550
Outcome SD	9.442	0.970
Observations	1319	1179

Notes: The first column of this table reports results of regressions of number of peers from a student's first class in a two-course sequence who take the same second class on class male proportion, conditional on course fixed effects. The second column reports results of regressions of number of peers from a student's first section in a sequence who take the same second section on section male proportion, conditional on class fixed effects. Each cell corresponds to a separate regression. All regressions are estimated on the set of students who take the first course in one of the listed two-courses sequences during their first semester and take the second course in a subsequent semester. Top panel regressions are only estimated on female students and bottom panel regressions only on male students. Standard errors are clustered at the first-class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

For the simulations, I use the same sample of students used for estimation in Table E1. I leave fixed their first-course enrollments and simulate fully random movement into classes for the second course in each sequence, preserving the observed sizes of the second classes. That is, if a student is observed taking the Fall 2015, 9 AM General Chemistry I and the Spring 2016, 9 AM General Chemistry II, in a simulation, I "leave" them in the Fall 2015, 9 AM General Chemistry I but randomly "place them" in a class for General Chemistry II, such that each simulated General Chemistry II class has the same number of students as is observed in the data. I then estimate the same regression models reported in Table E1 using the simulated data. This exercise fully conditions on which students choose to take both courses in a sequence. The comparison between my estimates from the data and the estimates from the simulations is thus based only on selection of class, conditional on choosing to complete the sequence.

The results are presented in Figure E1, with the distributions of simulated coefficients

Figure E1: For top row, red lines represent coefficients from regressions of number of peers from a student's first class in a two-course sequence who take the same second class on class male proportion, conditional on course fixed effects. For second row, red lines represent coefficients from regressions of number of peers from a student's first section in a sequence who take the same second section on section male proportion, conditional on class fixed effects. Left-column regressions include only women and right-column regressions include only men. Shaded areas represent distribution of coefficients from a simulation in which students moved from the first class/section to the second in a sequence randomly. 1,000 simulations were done per regression.

(from 1,000 simulations) appearing in blue and the estimates from the original data appearing in red. It appears that, for both women and men, random enrollments for second courses would not yield coefficients of zero on class-level male proportion. However, it is clear that the estimate magnitudes I observe in the data are unlikely to occur based on purely random enrollment. Focusing on the results for men, the class-level coefficient lies at the 98th percentile of simulated coefficients and the section-level coefficient lies at the 96th percentile. It is thus reasonable to conclude that men (and women) are more likely to take classes and sections with former classmates when their initial classes are more male-heavy, even fully accounting for the extensive margin of course selection. This may reflect a higher degree of friendship formation in more male-dominated classes, which would provide a plausible explanation for the positive effect of male peers on male persistence in majors.

Appendix F - List of Academic Departments at UIC by Gender Composition

Table F1 – List of Academic Departments at UIC by Gender Composition

Female-Majority Departments	Male-Majority Departments
Anthropology	Accounting
Art	Architecture
Art History	Business Administration
Biomedical and Health Information Sciences	Chemical Engineering
Biomedical Engineering	Civil Engineering
Biology	Computer Science
Black Studies	Design
Chemistry	Earth and Environmental Science
Classics	Economics
Communication	Electrical and Computer Engineering
Criminology	Finance
Curriculum and Instruction	History
Disability and Human Development	Information and Decision Sciences
Polish, Russian, and Lithuanian Studies	International Studies
Educational Psychology	Managerial Studies
Education	Marketing
English	Mathematics, Statistics, and Computer Science
French and Francophone Studies	Mechanical and Industrial Engineering
Gender and Women's Studies	Military Science
Germanic Studies	Moving Image Arts
Global Asian Studies	Music
Hispanic and Italian Studies	Physics
Integrated Health Studies	
Kinesiology and Nutrition	
Latin American and Latino Studies	
Liberal Studies	
Linguistics	
Nursing	
Philosophy	
Political Science	
Psychology	
Public Health	
Urban Planning and Policy	
Religious Studies	
Sociology	
Theater	

Appendix G - Heterogeneity of Effects Between Female- and Male-Majority Departments

Table G1 presents the results of estimating equation (1) separately among the subsamples of first-semester classes that are in overall female-majority and and in overall male-majority departments. Table G2 presents the results of estimating equations (2) and (3) separately among the same subsamples. The results demonstrate that the causal effect of male peers in creating a gap between male and female outcomes are strongly concentrated in overall male-dominated departments. Breaking results down by gender, both the negative effects of male peers on women and the positive effects of male peers on men are also concentrated in these departments. As can be seen in the lists of female- and male-dominated departments in Appendix F, this implies that the effects of male peers are largest in many departments that are associated with the highest post-college salaries, including most engineering majors, most business majors, and economics.

Table G1 – Estimated Effect of Class Male Proportion on Student Outcomes, By Department-Level Male Proportion

	Grade (GPA value)	Future course in dept.	Switch major out of dept.	Declare major in dept.	Graduate within 6 years	Graduate in dept.
Female-majority-department classes:						
Female student X	-0.231	-0.002	0.088	-0.015	-0.041	-0.008
class male %	(0.169)	(0.053)	(0.165)	(0.024)	(0.068)	(0.028)
Outcome Mean	3.067	0.645	0.151	0.050	0.676	0.126
Outcome SD	1.069	0.478	0.358	0.218	0.468	0.332
Observations	20769	25960	3022	22121	25960	25960
Male-majority-department classes:						
Female student X	-0.485	-0.226**	-0.105	-0.128**	-0.267***	-0.142**
class male %	(0.314)	(0.095)	(0.314)	(0.062)	(0.099)	(0.066)
Outcome Mean	2.856	0.643	0.115	0.061	0.679	0.129
Outcome SD	1.136	0.479	0.319	0.239	0.467	0.335
Observations	12302	17391	1728	15032	17391	17391

Notes: This table reports the results of regressions of student-class outcomes on an interaction between a female-student dummy and class male proportion. Each cell corresponds to a separate regression, with outcome given by the column header and the subset of students used for estimation given by the row header. Each regression includes course-specific female fixed effects and controls for student underrepresented minority status and instructor gender. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed and the department hosting the class is either female- or male-majority, respectively. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.

Table G2 – Estimated Effect of Class Male Proportion on Student Outcomes, By Gender and Department-Level Male Proportion

	Grade (GPA value)	Future course in dept.	Switch major out of dept.	Declare major in dept.	Graduate within 6 years	Graduate in dept.
Women in female-majority-department classes:						
Class male %	-0.067	-0.044	-0.016	-0.016	-0.037	0.002
	(0.117)	(0.030)	(0.073)	(0.016)	(0.040)	(0.019)
Outcome Mean	3.148	0.674	0.141	0.058	0.710	0.146
Outcome SD	1.016	0.469	0.348	0.233	0.454	0.353
Observations	12266	15203	2016	12704	15203	15203
Men in female-majority-department classes:						
Class male %	-0.054	-0.063	0.036	0.009	-0.010	0.008
	(0.159)	(0.040)	(0.125)	(0.013)	(0.050)	(0.017)
Outcome Mean	2.950	0.604	0.171	0.039	0.628	0.099
Outcome SD	1.131	0.489	0.377	0.194	0.483	0.299
Observations	8503	10757	1006	9417	10757	10757
Women in male-majority-department classes:						
Class male %	-0.501**	-0.160**	-0.329***	-0.039	-0.176***	-0.029
	(0.233)	(0.072)	(0.125)	(0.042)	(0.066)	(0.045)
Outcome Mean	2.873	0.637	0.138	0.063	0.698	0.111
Outcome SD	1.109	0.481	0.345	0.243	0.459	0.314
Observations	5018	6959	521	6273	6959	6959
Men in male-majority-department classes:						
Class male %	-0.038	-0.015	-0.122	0.076**	0.087	0.105**
	(0.223)	(0.075)	(0.147)	(0.037)	(0.063)	(0.043)
Outcome Mean	2.845	0.647	0.104	0.060	0.666	0.140
Outcome SD	1.154	0.478	0.306	0.237	0.472	0.347
Observations	7284	10432	1207	8759	10432	10432

Notes: This table reports the results of regressions of student-class outcomes on class-level male proportion. Each cell corresponds to a separate regression, with outcome given by the column header and the subset of students used for estimation given by the row header. Each regression includes course fixed effects and controls for student underrepresented minority status and instructor gender. All regressions are estimated on the observations within the identifying set of the main specification (as described in Section 1) for which the relevant outcome is observed and the department hosting the class is either female- or male-majority, respectively. Standard errors are clustered at the class level. * indicates significance at a level of 0.1, ** at a level of 0.05, and *** at a level of 0.01.