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Massimo Anelli
Giovanni Peri

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Massimo Anelli
Bocconi University and IZA

Giovanni Peri
University of California, Davis and IZA

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

Peers' Composition Effects in the Short and in the Long Run: College Major, College Performance and Income*

In this paper we use a newly constructed dataset following 30,000 Italian individuals from high school to labor market and we analyze whether the gender composition of peers in high school affected their choice of college major, their academic performance and their labor market income. We leverage the fact that the composition of high school classmates (peers), within school-cohort and teacher-group, was not chosen by the students and it was as good as random. We find that male students graduating from classes with at least 80% of male peers were more likely to choose "prevalently male" (PM) college majors (Economics, Business and Engineering). However, this higher propensity to enroll in PM majors faded away during college (through transfers and attrition) so that men from classes with at least 80% of male peers in high school did not have higher probability of graduating in PM majors. They had instead worse college performance and did not exhibit any difference in income or labor market outcomes after college. We do not find significant effects on women.

JEL Classification: I21, J16, J24, J31, Z13

Keywords: peer effects, high school, gender, choice of college major, academic performance, wages

Corresponding author:

Massimo Anelli
Universita' Bocconi
Via Rontgen 1
20136, Milano
Italy
E-mail: manelli@ucdavis.edu

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1 Introduction

Two important and established regularities motivate this paper. First, while women have caught up with, and surpassed, men in college graduation rates (Goldin 2006; Turner and Bowen 1999), their choices of college major have been and still are persistently different from those of men. Second, an extensive literature has shown that the peer environment in school can have important effects on the academic performance and on some choices of students, at least in the short run (e.g. Carrell et al. 2009 , Carrell and Hoekstra 2010). Combining these two facts we ask the following two questions: do men (women) who attended high school classes with a large share of males (females) show higher likelihood to choose "typically male" ("typically female") college majors? Does this effect on the initial choice of major have long-run consequences on college performance, graduation and eventually labor market outcomes, or does it "fade-out" in the following years?

In most developed countries, men represent a significant majority of enrollment in Engineering and Business/Economics majors, which we will call "Prevalently Male (PM) Majors". Those majors tend to be math-intensive, academically demanding and they usually lead to highly paid jobs. Instead, men represent a minority in the Humanities and Education majors, which we will call Prevalently Female (PF) Majors[1]. These majors are less math-intensive and are associated with lower paying jobs[2]. These differences in the choice of college major explain, in an accounting sense, a significant part of the existing male-female wage gap (Altonji et al. 2012). However, as college majors are chosen based on preferences, expectations and comparative skills, it is not clear whether exogenously shifting people into PM majors would improve their labor market performance. The gender composition of peers in high school could be one factor affecting the choice of college major. Previous studies have shown the peer environment at school and at work matter differentially for men and women and affects their performances, outcomes and choices[3]. Several studies have shown, for instance, that men and women are affected differently by a working environment made of same-sex peers. Men tend to be more confident (sometimes overconfident) in competitive situations with other men, while women under-perform and show lower confidence under competition pressure with men (Bengtsson et al. 2005, Niederle and Vesterlund 2011). Exposure to

[1] The U.S. Digest of Educational Statistics (2011) shows that only 18% of recent graduates in Engineering, but 64% of graduates in the Humanities, were women (U.S. Digest of Educational Statistics (2011).

[2] According to the U.S. Digest of Educational Statistics (2011) the average salary of a full-time employee one year after graduation was \$ 54,900 if she had a bachelor degree in Engineering but only \$ 31,500 if she had a bachelor degree in the Humanities (U.S. Digest of Education Statistics 2011 website - table 404).

[3] For instance Gneezy, Niederle and Rustichini (2003) Niederle and Vesterlund (2007) show different degrees of competitive behavior of men and women in same-gender environments, through an experimental setting. Booth and Nolen (2012) and Booth et al. (2013) show difference in performance of men and women in class with different gender composition.

different gender composition of peers in high school can affect the choice of college major. If this choice has long-run effects on college performance and labor market outcomes, then these peer effects can have long-lasting impacts.

We are among the first, to our knowledge, to address these questions in a rigorous quantitative setting. There are two important challenges in setting up an appropriate research design to answer them. First, one needs to observe the gender composition of peers at the time college major is chosen (in our educational setting the choice is made during the last grade of high school) and then follow individuals in their college career and into the labor market. To this end, we use a newly collected database of 30,000 individuals who graduated from college preparatory public high schools in the municipality of Milan, Italy, between 1985 and 2005, whose information was linked to their college career and to labor market outcomes. Second, to draw causal inference, one needs an exogenous source of variation in the gender composition of peers. To meet this requirement we define "peers" as an individual's high school classmates, since in our sample they were not chosen by the individual, but assigned by the school, and they are known to be the main group of social interaction for most youth in Italy. In particular, we exploit the random variation of class gender composition across cohorts within teacher-group and within school. By using a fixed-effect model that controls for school-cohort effects and teacher-group effects we eliminate any variation across classes that could be due to school-cohort characteristics or teacher-characteristics and we are left only with exogenous differences in peers. In the fixed effect model, variation in the gender composition of peers was totally accidental and we can establish its complete lack of correlation with individual characteristics and other peer characteristics.

The most relevant findings of our empirical analysis are as follows. First, we find that male students whose high school class consists of more than 80% male classmates have a probability to choose a Prevalently Male (PM) college major that is between 6 and 15 percentage points higher relative to the average probability for a male student in our sample, that equals 43%. The choice of college major of women, on the other hand, does not seem to be affected by a large share of their high school peers being either female or male. Second, the increase in probability of choosing PM Majors described above seems particularly strong for those in the lower part of the academic quality distribution. That group increased the frequency of PM major choice by 20 to 46 percentage points, from a baseline of 25.5 percent. Third, these effects on the choice of PM majors faded away by the time of graduation, because of attrition and change of major. Males graduating from classes with more than 80% male peers were not more likely to graduate from a PM major. They had marginally weaker university performance and lower probability of graduating overall than other identical males in more gender balanced high school classes. Finally, consistent with the idea that choice of major was reversed by the time of graduation, the increased likelihood of choosing PM majors did not translate into any significant effects on income, employment or occupation of male individuals who attended a high school class with more than 80% (or even 90%) of male peers.

A possible interpretation of the results is that a strong prevalence of males among peers puts pressure on male individuals towards choosing PM majors at the end of high school, even if those are not the best choice for them. These decisions are costly to reverse and they result in lower graduation rates in the long-run with a potentially detrimental effect on university performance and no positive effects in the labor market. This result shows the importance of evaluating peer effects in the long-run and the risk of estimating economic returns to PM majors relying on correlations. On one hand, we show that potential peer effects on college major choice fade-out in the long run. On the other, we also show that exogenously "pushing" some individuals towards PM major enrollment (as happens to males in classes with >80% males) does not ensure the economic gains associated with the PM majors in a simple regression. Interestingly, we do not find effects of classmates' gender on women's choice nor on their performance, in the short and in the long run. Perhaps women are less influenced by pressure from their peers. This is consistent with other recent studies (e.g. Park et al 2013) that found one-gender schooling only affects male students. Further research is certainly needed to understand whether these differences between men and women are systematic and apply to other choices and environments[4].

The rest of the paper is organized as follows. In section 2 we frame our paper within the literature. In section 3 we present our dataset and we illustrate more carefully the correlation of major choice with income and with earning gender gap. In section 4 we present our identification strategy, potential challenges and proposed solutions and we describe the empirical specification we use. In section 5 we show results on the choice of major, on college performance and labor market outcomes. In section 6 we explore some robustness checks and extensions and we suggest some possible mechanisms that explain the estimated effects. We conclude our analysis discussing implications of our findings in the final section 7.

2 Literature Review

The literature on the effect of peer gender composition on individual choices and outcomes is not very large, but is fast-growing. Most of the older literature analyzed differences

[4] An interesting recent paper (Stevenson, 2015) shows the difference in peer effects in juvenile jails between men, more negatively influenced by criminal and violent peers and women, more influenced by positive and less problematic peers.

between students in same-sex versus coed schools. Solnick (1995) considers data on the anticipated and final college majors of 1,700 female students at eight single-sex colleges and compares those with the choice of 818 female students at seven coed colleges. She tests whether women at single-sex colleges are more likely than their coed counterparts to stay in traditionally male-dominated fields. The possibility for women to sort themselves across schools and the differences between schools, however, generate serious potential for school- and individual-level omitted variables correlated with outcomes.

Billger (2002) exploits the case of colleges converting from all-female to coed to estimate the effect on the choice of college major, the probability of degree attainment, and occupational choice. The study compares women in all-female cohorts with those in coed cohorts to see whether women pursued different academic fields and careers. After the conversion to coed, female students were found to be less likely to pursue male dominated college majors and occupations. The interpretation of these results is that coeducational settings might reinforce gender stereotypes, while single-sex schooling might give more freedom to explore interests and abilities beyond socially constructed roles, especially for female students. However, a more recent paper by the same author (Billger (2009)) questions this earlier evidence by using more refined econometrics techniques. In this article Billger exploits the fact that "Title IX" allowed for the first time the creation of single-sex public schooling in the US and uses this to investigate whether single-sex schools leads to improved labor market outcomes. Results show that single-sex education graduates are no more likely to pursue college degrees and are less likely to meet their own educational expectations than graduates from coed educational environments. Of related interest is a recent working paper by Favara (2012) that looks in detail at educational choices in single-sex schools. She finds that attending a single-sex school leads students to a less stereotyped educational choice.

The contributions described above, however, have not dealt convincingly with the selection issue. The choice between single-sex or coed education is not random and is correlated with academic, personal and family characteristics of women (and men), both observable and unobservable. Moreover, coed and non-coed schools may differ across other dimensions such as teacher quality and resources. These characteristics are usually not observed by the researcher. Better labor market outcomes, or specific major choices, might be the result of career-focused women selecting single-sex institutions or they may derive from single-sex schools attracting more able and motivated teachers. To address these issues, recent studies have used random selection into different schools and classes to identify more credible causal effects. Park et al (2012) use the case of South Korea, where assignment to all-girls, all-boys or coeducational high schools is random. They use administrative data on national college entrance and mathematics examination scores for a longitudinal survey of high school seniors and they find significantly positive effects of all-boy schools in enhancing academic performance in Science, Math and Engineering. They do not find comparable effects for girls. In a related study Park et al (2013) also find that single-sex schools produce a higher percentage of graduates who attended four-year colleges and a lower percentage of graduates who attended two-year colleges. While these studies circumvent the problem of selection into a school, they are unable to separate the "peer-gender" effect from effects stemming from other unobservable school characteristics (gender and quality of teachers, spending per student in the school, and so on). Moreover the outcomes considered in these studies are all realized a short period after high school graduation; thus, the long-run effect of gender composition in high school is not analyzed.

A recent paper by Booth et al (2014) sets up an experiment to assign students to all-female, all-male, or coed sections within the same university major. This method solves the selection issues and it holds unobservable characteristics of the college and teachers fixed. The authors find that one hour a week of single-sex education benefits females by making them 7% more likely to pass their first year courses. They also score 10% higher in their required second year classes than their peers attending coeducational classes. This experiment is very interesting, but is limited to very short-run outcomes and was performed on a limited number of students who had already selected a male-dominated university major (Economics and Business). Therefore, little can be learned from this experiment about the effect of same-sex education on the choice of major.

Most closely related to our work is a paper by Schneeweis and Zweimüller (2012). They analyze the causal impact of the gender composition in coeducational schools on the choice of school type for female students attending primary schools in two Austrian cities. Using variation in the gender composition of adjacent cohorts within schools, they show that girls are less likely to choose a traditionally female dominated school type and more likely to choose a male dominated school type if they were exposed to a higher share of girls in previous grades. However, the sample size is relatively small, the authors do not have data on long-run outcomes, and they are unable to control for school-cohort and teacher effects. Moreover, the range of variation of the gender ratio across cohorts is quite small, as they consider the whole school cohort as peer-group (hundreds of people). For the law of large numbers, therefore, they cannot observe the outcome of an environment with very strong prevalence of one gender (such as 80% or more). Our analysis suggests that significant effects of peer gender composition arise only in strongly "gendered" environments and hence the identification strategy of Schneeweis and Zweimüller (2012) may not be able to detect these effects.

Finally, related to our paper in a more general sense, is the literature analyzing the interactions between gender and educational choice (e.g. Xie and Shauman, 2003 about women in STEM fields) and the literature on the effect of teacher gender on school choice and student performance (e.g. Carrell, Page and West 2010, Dee 2007). A growing experimental and theoretical literature has also analyzed behavioral differences between men and women as a consequence of peer gender composition. Gneezy, Niederle and Rustichini (2003) and Niederle and Vesterlund (2007, 2010) show, using experimental evidence, that the gender composition of opponents who meet in tournaments significantly affects performance, especially for women. Women perform significantly better in tournaments against other women than against men, while men perform better against other men.

Relative to the papers reviewed above, our analysis has several crucial advantages. First, our identifying variation is based on changes in gender composition across classes within school-teacher groups in a school. In such a setting we are controlling for both observable and unobservable teacher, school and cohort characteristics. Second, we observe long-run outcomes after the choice of college major, such as college performance, graduation rates, time to graduation and income on the labor market. These outcomes are realized between one and fifteen years after high school graduation and allow us to determine whether peer gender composition has effects only in the short-run or if it affects the long-run academic and labor market performance of individuals.

3 Data Description

3.1 Construction of the Data Set

Our sample comprises individuals who graduated from all but one[5] of the public college-preparatory high schools (Licei) in the city of Milan, Italy, between 1985 and 2005[6]. These individuals are currently between the ages of 28 and 48. We gathered information from hard copies of administrative records in the high schools by sending research assistant to collect and digitize them in each school. While some missing and destroyed records prevented full coverage, we were able to cover more than 90% of all records for students who graduated from the thirteen different schools between 1985 and 2005. The sample includes about 30,000 individuals distributed into 1,371 high school classes. We only have information relative to their last (fifth) year of high school. Six of the high schools included are of the "Classical" type and seven are of the "Scientific" type. Both tracks of high schools granted access to any college major in the considered period, but the first type has a curriculum more intensive

[5] Only the Liceo Classico Carducci did not authorize us to collect Data.

[6] There were also a few private Licei in Milan. They enrolled a much smaller number of students usually among the low performing ones from wealthy families.

in Humanities while the second type is more focused on Science and Math. Many college educated individuals from Milan became professionals in business, finance, administration, education and academia. Our analysis, therefore, pertains to a group in the upper tail of the income and educational distribution in Italy.

For these individuals we have information on the year of high school graduation, the score in the high school exit exam, the school attended, place of residence and the identity of their parents. Most importantly, we know the identity of their peers (classmates) in the last year of high school and the set of professors they shared.

We have linked these high school data (using name and date of birth) with records from all five universities in Milan (two private universities, Universita' Cattolica and Bocconi, and three public universities, Politecnico, University of Milano, University of Milano-Bicocca). Several of the best University Schools and Departments in Italy, in all majors, are located in Milan. It is overwhelmingly common for high school graduates in Italy to attend their local university, if one exists. For students graduating from a college preparatory school in Milan who intend to go on to college in Italy, enrolling in one of the local universities is usually the best choice. The information about their university career includes whether they graduated, the year, major, university of graduation and their overall exit score in University.

Further, we linked these records with 2005 personal income figures, as revealed to the internal revenue service. This is the total income of each individual as reported to the tax authority. The advantage of using these data is that the administrative file of reported income includes all individuals in the nation; it is mandatory to report any income. Hence, if a person does not appear, he/she has no income or he/she is not living within Italy.[7]. Self-employed are included in the sample. The limitation of these data is that we do not have a measure of labor supply (hours or weeks worked). Hence we will focus on yearly income. Finally, we linked the address where students lived at the time of high school to the average house value of their specific neighborhood[8]. We use this measure as a proxy for family wealth during high school, as the house is the most important financial asset of families in Italy.

Longitudinally linked individual data on income, university career, high school performance and family background are rare in any country. For Italy, our originally collected database is, to our knowledge, the only one that contains such information for such a large sample. Hence, this data provides an interesting tool to analyze the long-run effect of school-

[7]There is also a small category of employees with only standard salary income and no deductions that need not report it. Usually this is a very small percentage of the population.

[8]We have transformed each address into geographic coordinates using Google's Geocoding Service and then matched each address to market value per square meter as provided by the government agency " Agenzia del territorio" for 55 different homogenous areas in Milan.

ing on income. We use the income data only for people who last attended high school before 2000. Considering an average college attendance of four to five years, people in our sample would have been on the labor market between 0 and 15 years as of 2005. Hence, this provides a good assessment of the consequences of the last year of high school in the long-run (up to 20 years after high school graduation). Of the 30,000 individuals for which we have information about high school, 14,000 whom graduated between 1985 and 2000 were matched to the information on income in year 2005. For a stratified 10% random sub-sample of the initial sample (equal to around 3,000 individuals) we also collected more detailed information from telephone interviews conducted in June 2011 by the professional company " Carlo Erminero & Co.". The interviews contain additional information covering family background, parental income, and extra-curricular activities during high school. We will use some of this additional data in robustness checks and extensions.

3.2 Summary Statistics for Individuals and Classes

In our data we can identify individuals belonging to each fifth-year class in each section of each high school included in the sample. Students in Italian public schools are limited in their choice of high school. Those determined to pursue a college education usually choose one of two types of high schools, either " Classical" (Liceo Classico) or " Scientific" (Liceo Scientifico) and often decide to attend the school of the chosen type closest to their residence. Each public school had to admit all students that applied. During the period under analysis, classes were formed in the first year of high school by pooling all enrolled students and randomly drawing the students in each class. The entry cohort in a school, therefore, varied from year to year, mirroring the demographics of the relevant age group living in proximity to the school. Students in the same fifth-year class shared the same peers (classmates) and professors during the fifth year of high school and, likely, for most of the previous years. The group of high school classmates is, for the majority of people, a very important group of peers at the time of college choice. Frequent interactions and the shared school experience is likely to affect the information available to students, as well as their preferences. Among students attending the " Classical" high school track, the average share of men was 33%, while the " Scientific" track was 60% male. This implies that the average gender composition of classes was more male-dominated in the scientific high schools. However, while classes in those schools were more likely to have large shares of men, the variation in gender composition of classes was large.

In Table 1 we present descriptive statistics for our data. We divide variables between individual-level (top portion of the Table) and class-level (bottom portion). For individual-level variables we present statistics relative to the whole sample as well as the separate means for men and women (in columns 6 and 7). We also show the t-statistic of the difference in averages (men-women). The table summarizes the information about individuals' academic career in high school and in college. The most important pre-treatment socioeconomic characteristic available to us (besides age, gender, and school) is the value of the home each student lived in during their last year of high school. This serves as a proxy for the family's income as a house was the primary asset held by families, and the value of real estate varies significantly within the city of Milan.

In the sample, 52.3% of individuals are female and thus 52.3% is the average share of females in each class. However, as we noted above, there is a systematic difference in gender ratio between scientific and classical high schools. We have re-scaled the high school exit test score[9] to be between 0 and 1, with 0 being the minimum passing score (60 out of 100 on the high school exit test) and 1 the maximum score achievable (100 out of 100). Summary statistics show the distribution of scores is left-skewed (mean score is 0.416) and the mean score for women is substantially higher than for men (the t-statistics of the difference in means is 11.4). Even when we rank students according to exit-scores within school and cohort, females perform better, being ranked on average at 0.516 (ranked from 0 to 1) versus an average rank of 0.471 for men with a t-statistic for the difference of 12.4.

Of these 29,370 high school graduates, 23,118 students (79%) enrolled in one of the universities in Milan. Of those 23,118, 27.5% enrolled in PM majors (Engineering, Economics & Business) with a substantial gender difference: only 13.9% of women chose one of the PM majors compared to 42.3% of men. The t-statistic for the difference in means is 50.3. Of the 23,118 students enrolling in a university, 17,140 actually graduated (by 2011, when we collected the data), implying a 26% attrition rate. Out of all students enrolling in a university, 11.8% of women earned a degree in a PM major compared to 32% of men . Interestingly, conditional on enrolling in a PM major, the drop out rate among women was lower than among men: 17.5% versus 25% with a t-statistic of the difference equal to 6.

To complete the list of college outcomes in our dataset, women took an average of 3 months less than men (t-statistic is 7) to graduate from college when the average time to completion was 6 years and 8 months[10]. At the end of college, every Italian student receives a final test-score out of 110 points, computed on the basis of G.P.A. and a final thesis[11]. We

[9] At the end of high school, in the fifth grade, all students take an exit test, called " Maturitá". This test is prepared by the ministry of education and it is the same for all schools in the country and determines graduation.

[10] Italian students, especially during this period (1985-2005), had very long spells between college enrollment and graduation.

[11] The exception is engineering students who got a score out of 100. We re-scaled their scores accordingly.

have re-scaled this final score to between 0 and 1 (with 0 being the minimum passing score). Consistent with other outcomes, women perform better on average than men (0.86 versus 0.78 with a t-statistic of 30.25).

The averages for men and women suggest substantial differences in academic performance and academic choice. The log of house value does not show any statistically significant difference between men and women. This demonstrates the average family background of female and male students included in our sample was similar.

For labor market outcomes, we observe the income for 17,004 students attending fifth year in high school by year 2000: since we have only 2005 income data available, we exclude individuals that are still attending college by 2005, as their income might not be representative of their potential income (i.e. part-time job while in college). The average log wage is 9.68 and the statistically significant difference (t-statistics is 22) across gender is 0.45 log points in favor of men. From the randomly selected 10% sub-sample that was interviewed, we also know that women had 30.8% probability to reach a top occupation (defined as manager, professional or self-employed) versus 43.2% of men with a t-statistic of 7.1.

As for class-level data we observe a total of 1,371 graduating classes with an average size of 21.4 students and a standard deviation of 3.8. The average share of female per class is 52% with a large standard deviation of 0.18. The average students' exit score by class ranges from 0.12 to 0.78, showing high variance in the ability composition of classes. Finally, the average socioeconomic status of classes is captured by the percentage of students in the class that live in houses valued in the bottom 10% of the house value distribution. The variance of this statistic is large; for instance, there were classes composed of only students coming from families in the bottom decile of the wealth distribution (they were in schools located in poor neighborhoods). The highest percentage of students in a class living in houses at the top 10% of the value distribution was 57%.

3.3 Prevalently Male Majors, Prevalently Female Majors and Income

The main outcomes analyzed in this study are the choice of college major and the subsequent performance in college. In Italy college major is chosen at the end of high school and is costly to change later, as one must re-enroll and start the new major from the beginning. Hence, the major at enrollment is highly correlated with major at graduation. It is also correlated with labor market outcomes. In our analysis we consider 11 college-major categories spanning all college degrees awarded in our sample. To sharpen our analysis we organize those 11 majors into three groups associated with different shares of females among enrolled students. Figure

1 shows the female share among total enrolled students for the 11 college majors from our sample (measured in the right scale and reported as the darker grey histogram). We rank majors from left to right according to this share and define the two majors with the smallest share of women (Engineering and Business/Economics) as constituting the Prevalently Male (PM) group. Those are the only two majors in which females constitute less than 40% of enrolled students. At the opposite end of the spectrum, Humanities and Education are the two majors showing the largest share of women among enrolled students; we call them the Prevalently Female (PF) group. These two are the only majors where women make up more than 70% of enrolled students. The remaining seven Majors with a more balanced gender composition are called the Gender Balanced (GB) group.[12].

Figure 1 also shows, as light grey bars, the average income in year 2005 by major of graduation (left scale), as measured in our sample (not corrected for any characteristics). Notice the PM majors are associated with the highest income while the PF majors are associated with lowest income and GB majors are in between. This suggests the different choice of major across gender may account for a significant part of the earning gap between men and women. To make the above correlations more formal, and to quantify them, in Table 2 we regress the logarithm of income in 2005 on a series of individual characteristics and on the Prevalently Male (PM) and Prevalently Female (PF) dummies (leaving Gender Balanced majors -GB- as the omitted category). The dummies equal one for individuals who graduated in the corresponding majors and zero otherwise. The controls include the high school exit score, the final college exit score, a dummy for living in a house in the top 10% of price distribution at the time of high school, one for living in a house in the bottom 10% and school-by-cohort fixed effects to control for cohort-specific and high school-specific factors.

The regression results, first shown separately for men and women, in Columns 1 and 2 of Table 2, reveal substantial positive income gaps associated with graduating from a PM major and significant negative income gaps associated with graduating from a PF majors for both men and women. Women with similar observable characteristics earn 0.65 logarithmic points more (92% more) if they graduated from a PM major relative to those graduating from a GB major (the omitted category). On the other hand, they earn 23% less if they graduated from a PF major (relative to GB). The estimate for males is equal to a 73% income premium (0.55 log points) from PM majors relative to GB and to a 42% penalty for PF majors graduates. The regression also reveals that college exit score has a strong correlation with high school

[12]In robustness checks (available upon request) we alternatively considered the top and bottom three majors in terms of share of female, denoting them as PM and PF majors and perform similar analysis. The main findings presented in the paper remain unchanged.

exit score, while the family house value is much less correlated with it. Clearly no causal interpretation can be attached to these coefficients; they are simply a measure of the partial correlation between major and income.

Columns (3) and (4) of Table 2 show another interesting fact. They identify the role played by college major in accounting for the income gap between men and women. In column (3) men and women are pooled and the coefficient on the female dummy is an estimate of the average income gap (in logarithmic points) between women and men (controlling for all the individual and family characteristics described above). Remarkably, this difference equals -0.4 logarithmic points, about 33% [13], in favor of men, which is a very large average difference.[14]

In column (4) we simply add to the previous controls the PM and PF majors dummies. Besides being very significant, as expected, the introduction of these two dummies, by accounting for the major of graduation, reduces the men-women income gap from 0.4 to 0.25 logarithmic points (from 33% to 22%). Hence, one third of the male-female income gap is accounted for by their lower graduation rates from PM Majors and higher graduation rates from PF Majors. This is interesting, but as individuals choose the college major based on their abilities and preferences, the correlation shown above is far from establishing causality. One may be tempted to infer that pushing people into PM majors would increase their wage by the estimated amount. We analyze whether an increase in enrollment into PM majors, driven by randomly distributed peer characteristics - as good as random assignment - has any effect on the university performance and labor market income of individuals. In doing this we will discover the OLS coefficients of Table 2 is a misleading assessment of the effects a policy pushing individuals to enroll in PM majors would have on their income.

4 Identification Strategy and Empirical Specification

4.1 Variation of Class Gender Composition

The vast majority of Italian students, from rich and poor families, attended public high schools. In Milan, the very good reputation of the public college-preparatory high schools that we consider (which includes all but one of the public "Licei Classici" and "Licei Scientifici" in the municipality) implies that most youth who intended to go to college attended

[13] The conversion from logarithmic points into percentage is always calculated as the exponential of the logarithmic points minus one.

[14] This measure of earning gap is not far from what is estimated for Italy in recent years. The gender gap in yearly wages estimated from the EU-SILC data (a representative household sample), and limited to college educated over 25 was 34% in 2009, while it was 40% for all workers.

one of them.[15] Hence, our sample is representative of individuals who intend to go to college. As we consider public schools, there was virtually no difference in the monetary cost of attendance. [16].

Within each high school, the entry cohort of students was randomly assigned to one "section" (coded with a letter: A, B, C...) in the first year. This assignment corresponded to a set of teachers that remained unchanged year after year for each section. Students remained in this letter-section until their fifth year, except for attrition and a few transfers (which are discouraged and bureaucratically cumbersome). While the official procedure to determine the composition of sections was random assignment of students, we cannot document directly its implementation (no records of the procedures were kept) and we do not observe the class-composition in the first year; only its composition in the fifth year. Ideally, our data should rely only on the randomness of section assignment in the first year. In its absence, we will discuss potential threats to identification in the data available to us and present the methods we use to address them. We then test the randomness of the component of the class female share that we use to identify effects on outcomes.

There are three sources of variation in the gender composition of fifth-year high school classes in our data. The first is the gender composition of the school-cohort entering their first year. An entry cohort with a large share of female students would make the average first-year class in that school-cohort more likely to have a large share of women. Such shares were determined by demographics in the area of the school and by the choice of students between that college-preparatory and other high schools in the area. While such variation was outside the control of the school and its teachers, the choice of type of high school may differ between men and women. If correlated with some time-varying characteristics of the schools, which also affect the choice of college major, this could create an omitted variable bias. To avoid any possibility that unobserved school-cohort characteristics can cause omitted variable bias, we include school-cohort fixed effects in our regressions.

The second source of variation is the random assignment of students to "sections" within a school that partitions the entering cohort into several (usually 7-8) first-year "sections" (one class per section) with different female shares. This partition was random and assigned students to teacher groups that varied by section, but did not vary year-to-year . That is, section "X" in each school was assigned the same instructors every year. We want our identification strategy to capture this source of variation. The third source of variation is generated by abandonment, attrition, transfers and grade retention of students over the five

[15] A small number of private college-preparatory high schools existed in the city that accommodated a small fraction of generally wealthy, low performing students.

[16] All public schools charge minimal fee per year. Currently it is $150 per year. It was far less during the years 1985-2005.

years. This variation mainly depended on the teacher-group assigned to each section, which could make teaching either more selective or a better fit for a specific individual or gender. Consider, for instance, a specific group of teachers (assigned to one section) with particular abilities, teaching methods and attitudes that may better accommodate students of one gender, at the same time inspire their choices or affect their outcomes. This, over time, could affect attrition differentially and could produce a correlation, for example, of male-friendly professors with higher shares of male students that pressures them towards a PM major. More in general, a key concern of our analysis is separating the effect of peers from that of teachers. Our data allow us to do this by controlling for "school-section by five-year intervals" fixed effects. Those effects correspond to teacher-group fixed effects as different fifth-year cohorts in a school-section had the same set of teachers year after year. Teachers in these high schools were usually tenured public servants with very high job stability, so changes in the body of teachers in a section over time were infrequent. Our inclusion of a "section by five-year intervals" (quinquennium) fixed effect, therefore, is a conservative way of controlling for teacher fixed effects and potential attrition of teachers assigned to each section. Hence, we use the within-section-quinquennium (teacher-group) year-to-year variation in gender share of classes relative to the school-cohort average (controlled for by the set of fixed effects), due to different realizations of the random assignment.

This identification strategy is implemented empirically by estimating – separately for males and females – the following equation:

$$y_{i,l,s,t} = \lambda_{s,t} + \phi_{l,s,T} + \beta(CGC)_{i,l,s,t} + \delta X_{i,l,s,t} + \gamma Z_{l,s,t} + \varepsilon_{i,l,s,t} \tag{1}$$

The outcome $y_{i,l,s,t}$ is relative to individual i, in "letter-section" l, in school s, in the fifth-grade class graduating in year t (which corresponds to cohort t). In our main specifications the outcome variable equals one if the individual chooses a Prevalently Male (PM) college major (or a PF major, or a GB major) and 0 otherwise. The term $\lambda_{s,t}$ captures all the school by cohort fixed effects. The term $\phi_{l,s,T}$ captures school-section-quinquennium effects ($T = 5$-year interval) which, as explained above, controls for the teacher-group. The term $CGC_{i,l,s,t}$ represents the main explanatory variable of our analysis and hence β is the coefficient of interest. It captures a measure of the "Gender Composition of Classmates" of individual i graduating in year (cohort) t from section l of school s. Most frequently in our analysis, that variable will be a dummy equal to one for classes with more than 80% (90%) classmates of the same sex as student i and 0 otherwise.

The variables $X_{i,l,s,t}$ control for the pre-determined characteristics of individual i that we can observe, namely the measure of the student's family house value as proxy for family wealth. We also control for observable class-level characteristics $Z_{l,s,t}$. They include a dummy for class size in the bottom 25% of the observed class size distribution and one for class size in top 25%, class geographical concentration measured as Herfindhal index of concentration of the students' homes across city-blocks, the share of students in the class in the bottom 10% of house value and share of students in class in top 10% of house value. We first estimate specification (1) separately for men and women. We then consider it separately for students in different parts of the male-specific or female-specific ability distribution and in different parts of the wealth distribution, to see if peer gender has a different effect on the choices made by some specific subgroups.

We will also consider other outcomes $y_{i,l,s,t}$ besides the choice of college major, exploiting our ability to follow students in their university career and in the labor market thanks to the longitudinal nature of the dataset. We consider several measures of performance in college, such as the graduation rate, time to graduation and the exit test score in college. Other outcomes occur decades after the end of high school, such as realization of income on the labor market (log of income). In all the estimates, we cluster the standard errors at the school/cohort level[17].

4.2 Tests of Randomness of Class Gender Composition

Our identification strategy uses the residual variation of female shares across classes, once we control for school-cohort and teacher group fixed effects. It exploits the cohort-by-cohort variation of female-share in classes assigned to the same group of teachers in a school, relative to the average female-share in the school-cohort. This identification strategy allows us to isolate the impact of gender-share in the class, separating it from the potential effects of observable and unobservable characteristics of teachers and school-cohort. This identification strategy is more demanding than the identification used in other education contexts where there was no certainty of random assignment, such as Hoxby (2000), Carrell and Hoekstra (2010), Lavy and Schlosser (2011) and Schneeweis and Zweimüller (2012). Those studies rely solely on year-to-year variation in some demographic characteristics of entry cohorts .

To ensure that the residual variation of the female-share across classes is as good as random in our data, we performed a series of tests. First, we show the residual female share used for identification is normally distributed . Moreover, we check that classes with extreme gender composition – i.e. with more than 80% or 90% male or female– that turn out to have an important role in the empirical analysis, occur with probabilities consistent with pure randomness. Figure 2, plots the histogram of the female class-share residuals

[17] We have also clustered at the more conservative school level. Results are essentially identical. If anything, some standard errors become smaller for some female outcomes, possibly revealing some negative correlation of errors across cohorts within schools.

from our data (solid bars) and a histogram from a simulated normal distribution with the same standard deviation as our sample (equal to 0.10) and the same number of observations (1,371) and shows this as empty bars. As one can see, there is very little departure from normality in the observed distribution of residuals. The tails of the simulated normal distribution (corresponding to the classes with extreme gender composition), in particular, has a similar number of observations compared to the actual distribution. Specifically, the observed distribution of residuals has 66 observations (classes) outside the interval of +/- 2 standard deviations from 0, while the simulated distribution has 64 such observations. Several of these observations in the tails of the distribution are the "extreme composition classes" (having shares of males or female >80%), occurring at a frequency in line with pure randomness. In fact, their deviation from the mean (0) is larger than 2 standard deviations, but smaller than three. Only eight of the "extreme composition classes" are outside the reasonable distribution range of the residuals; namely, outside the +/- 3 standard deviations interval. Seven of those correspond to single-gender -only classes (6 males and one female) in the same school (the Liceo Einstein) between 1986 and 1988[18]. As this may imply the school manipulated the gender composition of its classes, we omit the whole school as a robustness check. Results are highly robust to this alternative specification, showing the effects are not driven by these outliers.

In Table 3, we show the correlation of different measures of the gender composition of a class with the observable predetermined characteristics of students in the class, controlling for the two sets of fixed effects ("school-cohort"and "teacher group"). The units of observations are classes and the dependent variables are described at the top of the Table for each specification. Specification (1) uses the share of women in the class as a dependent variable. The other specifications use dummies to capture classes with " extreme" gender composition. Specifications (2) and (3) use a dummy for a share of females (males) larger than 80% in the class (respectively). Columns (4) and (5) use a dummy for a share larger than 90% for females and males in the class, respectively. Specifications (2)-(5) are ways of conducting a balancing test between the extreme gender composition classes and the remaining classes within a regression context. We check the correlations with several predetermined characteristics of the class as a way of testing the orthogonality of gender composition. We include as explanatory variables the average log price of the house where a student lived to measure family wealth), as well as indicators for the presence of significantly rich and poor families in the class. Specifically, for the share of students from families living in houses valued in the top 10% of all houses, and the share of students living in houses valued in the bottom 10%.

[18]As we show in Section 6, results are robust to replicating the estimation without these outlier classes in terms of gender composition.

We also include the average size of the class (and two dummies for whether it was in the top 25% or the bottom 25% of the size distribution) to see whether gender composition was associated with class size. Finally, we include measures of the average distance of students from school, as well as an Herfindhal index of concentration of students in the class in city-blocks. These measures check whether classes with a certain gender composition are also made of students living close to each other, which could suggest the clustering of friends in the same class, possibly as result of transfers after section assignment. The coefficients show that none of these variables – family location, wealth, class size or neighborhood ties between students –has a significant correlation with any of the gender composition measures (share of female, or prevalence of males/females in the class). Each individual variable has an extremely low and non-significant coefficient. The F-test of significance of all variables together rejects their joint significance at the 1% confidence level for all dependent variables and at the 5% level for all but the >90% female classes. Overall, no predetermined characteristic of the class seems to be significantly correlated with the residuals of its gender composition.

Finally, we test the randomness of peer characteristics more stringently, conditional on school-cohort and teacher-group effects. In Table 4 we show the results of tests performed to verify that there is no correlation between any predetermined individual characteristics and the average characteristic of peers in the class (not just the gender ratio), after controlling for school-cohort and teacher-group effects. Following Guryan et al. (2009), in Table 4 we analyze the correlation between predetermined individual characteristics and the average of that predetermined characteristic among peers, once we control for the average characteristic of the cohort. We consider, as predetermined characteristics, the log value of the house, the distance from school, the probability of being in the top 10% or the bottom 10% of the house value distribution. We do not find any significant correlation between individual and peer characteristics in any case. All our tests are consistent with the identifying assumption that student characteristics across classes – including female share – within school-cohort and teacher-groups are as good as random. The correlation between each of these individual characteristics and the corresponding average characteristic among peers is always indistinguishable from 0. Reassured by these tests, we proceed under the assumption that, conditional on school-cohort and teacher-group, the distribution across classes of female (and male) shares is as good as random. We now move to estimate its causal impact on individual outcomes.

5 Main Results

We present our main results on different individual outcomes following a timeline: from those occurring right after high school to those occurring later in life. In the first subsection 5.1 we show how gender composition of the class affects the choice of college major at the end of high school. In subsection 5.2 we look at the effect of class gender composition on academic outcomes during college. In subsection 5.3 we look at the long-run effect of high school class gender composition on income after college.

5.1 Class Gender Composition and Choice of Major

Before analyzing the potential effects of peer gender on college choice, we analyze whether the female share in a class affected the academic outcomes of male or female students during high school. In this test we use a regression framework exactly as in (1) with the ranking of an individual's exit test score relative to his/her school-cohort as the dependent variable $y_{i,l,s,t}$. Such an index of academic performance measures the degree of learning and understanding of high school material. We first test whether the gender environment in the class affects high school academic performance. Table 5 reports the coefficients of different measures of class gender composition (CGC). Columns (1) to (4) report coefficients on female students' exit test score, while columns (5) to (8) report the coefficients on male students' scores. Within gender, the columns of the table differ in the specific explanatory variable used to capture the class gender composition. In specification (1) we include linearly the female share among classmates and in specification (5) we include the share of males among classmates. Then we analyze whether a variable capturing strongly unbalanced gender composition of the class, instead of the linear measure of shares, affects outcomes. As most studies in the literature consider same-gender groups vis-a-vis mixed-gender, in the following specifications we focus on the potential effects of rather "extreme" gender-composition of classes. In specification 2 (specification 5) the explanatory variable is a dummy equal to one for classes in which 80% or more of classmates are females (males). In specification 3 (specification 7) we consider classes in which 90% or more of classmates are females (males)[19]. Then in specification 3 (specification 8) we consider classes in which 10% or less of classmates are females (males) . The estimated coefficients in Table 5 show the impact of gender composition on individual exit test scores, standardized by school-cohort. The only coefficient significant at the 10 percent level is the one on the $> 90\%$-male classes for male test scores, and no coefficient is significant at 5%. It shows a small positive effect of $> 90\%$-male classes on male tests scores.

[19] For brevity in the rest of the paper we will call these classes as "$> 80\%$-male" (-female) and "$> 90\%$-male" (-female), respectively.

We do not detect any significant effect of the classes that are $>$ 80%-males (or female) on men's (or women's) test scores. This weak result, only in the $>$ 90%-male classes, suggests feeble evidence of any impact of peer gender on school performance: being in a same-sex environment does not seem to affect the academic performance of students once we control for individual and class/teacher characteristics. A small positive effect may exist for male performance.

Table 6 shows the main results when we use the choice of college major dummies as dependent variables and the same measures of class gender composition (CGC) in a sequence of specifications for females (1-4) and males (5-8), which is exactly the same as shown in Table 5. In Panel A of the table the outcome variable is a dummy for enrolling in a Prevalently Male (PM) college major, in Panel B the outcome is enrolling in a Gender Balanced (GB) major, while in Panel C the outcome is enrolling in Prevalently Female (PF) college major. The full set of individual level controls, class level controls and fixed effects is included in all regressions, as noted in the lower part of the table.

The estimates reveal interesting effects that differ significantly between men and women. First, our data show no evidence the share of same-gender peers affects – in a linear way – the probability of choosing PM or PF majors for either males of females. The point estimates are small and non-significant (they are only positive for men). This emphasizes that in the presence of "small variations" in gender composition of classes around the average (as is the identifying variation available to some previous studies such as Schneeweis and Zweimüller, 2012), one may not identify any effect of peer gender composition on the choice of college major. In largely mixed-gender classes it may not matter much if same gender peers are 40 or 50% of the total. The second interesting result is that the major choice of women is not significantly affected even by the most extreme gender composition of the classes. Girls in $>$ 80% or $>$ 90%-female classes (specifications 2 and 3) do not exhibit any different propensity of enrolling in PF (or in PM) majors. The point estimates of those coefficients are small (usually close to 0.01) and never significant. Similarly, girls in mostly male classes do not show any stronger propensity to enroll in PM majors (specification 4). Male students, however, show a significantly larger probability of enrolling in Prevalently Male majors if they have attended a $>$ 80%-male or, even more so, a $>$ 90%-male class (see specifications 6 and 7). The estimated effects are significant at the 5 and 1% level. By attending a >80% (>90%) -male class, a male student increases his probability of enrolling in PM majors by 6.3 (15.4) percentage points. In Table A1 in the appendix we also show a more systematic non-parametric approach to estimating the effect of classes with different shares of same-gender peers. We include nine different dummies capturing the effect of a share of same-sex peers (separately for men and women) between 10 and 20%, between 20 and 30%, between

30 and 40% and so on, omitting the reference category: <10%. The table shows that for women, the point estimates on each dummy are small and insignificant. For men the point estimates of the effect on PM major enrollment are small, but progressively increase (with a few exceptions) from the lowest value for the dummy 10-20% males (3.3 percentage points) to the largest value for the 80%-90% males (10.8) and for the > 90%-male (23.8) classes. Only the > 90%-males dummy is significant at the 1% level in this non parametric way of partitioning the effects.

The average probability of enrolling in PM majors is 42.7% for males; hence the effect described above is large and significant, increasing the probability of PM enrollment by between 1/7th and 1/3rd of its average. Interestingly, and somewhat symmetrically, males have a smaller probability of enrolling in PM majors if they attended a class that was >90% female. However, this effect (equal to -7.3 percentage points and reported in column 8 of Table 6) is not statistically significant. Panels B and C of Table 6 reveal the shift into PM majors for males takes place by diverting students out of Gender Balanced (GB) majors. The probability of males enrolling in GB majors decreases by about 4.5 percentage points (not statistically significant) if they attended a > 80%-male class and by 12.7 percentage points (statistically significant) if they attended a > 90%-male class. This suggests that some males with "marginal" preferences for GB majors might have been pushed into PM majors when attending the last year of high school with a group of peers that were overwhelmingly male. The negative effect on the probability of enrolling in PF majors for males who attended > 80%-male or > 90%-male classes was weaker, but significant for the > 80%−male group.

Given the small effect on academic performance of males (zero for > 80% male classes and significant but small for > 90%-male classes) shown in Table 5, we do not think that the effect on major choice is due to improved academic ability of males. Rather, it may be indicative of the fact that peers and their gender can affect preferences and attitudes, especially of male youths. It is consistent with the idea that males show a more confident and competitive behavior when exposed to competition with other males (as consistent with Bengtsson et al. 2005, Niederle and Vesterlund 2011). The increase in their probability of choosing PM college majors will expose them to a more academically challenging curriculum and competitive environment when in college. The average academic quality in PM majors is higher than in PF (or GB) ones. High school graduates enrolling in PM majors have an average high school exit score of 0.51, with respect to 0.42 for those enrolling in PF majors and 0.38 for GB majors. This may imply males from prevalently male high school classes choosing PM majors have a lower average quality than other male students going on to PM majors. They may be "pressured" into majors that are a worse match for their abilities, which can affect their performance in college. On the other hand, these majors are associated with a wage premium. Hence, in spite of the mismatch, this peer-effect may result in higher wages on the labor market if being exposed to knowledge in those fields increases earning ability. We will address these questions in the following sections.

Table 7 analyzes the effects of class gender composition on the choice of major, partitioned by student academic performance. We focus on the $> 80\%$ and $> 90\%$ single -sex share dummy as explanatory variables. We show, again, the main estimated effect of being in a class prevalently ($> 80\%$ or $> 90\%$) of the same gender (in column 1 and 4). We then separate the effects by splitting the sample between individuals whose measure of academic performance, as revealed by their relative gender-specific ranking in the high school text score, was in the bottom quartile of the gender/school/cohort distribution ("bottom quality" in the table) and those whose performance was in the top quartile ("top quality"). Let us emphasize that while this measure of academic quality is not fully predetermined with respect to the class gender composition, by taking the gender-specific ranking, we avoid mechanical correlation with gender shares. We also showed in Table 5 that there is an extremely low correlation between individual test score ranking and class gender composition.

We show the effect of the $> 80\%$-same gender dummy (first row of each Panel) and the $> 90\%$-same gender dummy (second row of each Panel), individually, on the bottom and top quartile for females (column 2 and 3 respectively) and for males (columns 5 and 6). The effects for women are mainly non-significant. Only the effect of $> 90\%$ female classes is significant for bottom-quality women and shows an increase in their probability of PM major choice. While the effect is not present for the $> 80\%$ female classes, nor does it affect the probability of other major choices, the point estimates suggest a mild potential effect of heavily-female classes in encouraging PM major choice for women, too. This may be consistent with the idea that women, especially those who are academically weaker, feel freer to make less "gender stereotypical" choices when in a mostly female environment. This is in contrast with men who are pushed to more "gender stereotypical" choices in prevalently male environments.

The positive average effect on the probability of enrolling in PM majors for men is significant and much larger in its point estimate for the academically weakest group. For this group of male students, being in a $> 80\%$ ($> 90\%$) male class increases the probability of choosing a PM college major by 20 (46) percentage points. For high quality students, however, the effect is much smaller, namely -5 (13) percentage points, and is statistically non-significant. The effect on low-academic-quality students is particularly remarkable because the average probability of choosing a PM major for bottom quartile male students is only 25.5 percent. Hence, graduating from a prevalently male class makes that probability two to three times larger. Panels B and C reveal between half and three-quarters of the shift to PM majors for bottom-quality students is from GB majors, and between one-quarter and half is from PF majors. These estimates may reveal students in a prevalently male class with a marginal preference for engineering-economics-business and are not too strong academically, are subject to pressure/imitation or overconfidence pushing them towards more demanding prevalently-male majors. On the other hand, higher achieving students (who were already choosing PM majors with high probability – 57% on average) seem to be less influenced by such class environments[20].

Table 8 separates the effects on students from families in the bottom from those in the top quartile of the house value distribution, which could be considered a proxy for economic status. This variable is fully predetermined with respect to the class gender composition. Similar to what is done in Table 7 we show again the total effects for males and females (in Columns 1 and 4) from being in a high school class with $>$ 80%-same gender (first row of each panel) or $>$ 90%-same gender (second row of each panel) classmates, and for the sub-group of students in bottom and top quartiles of house value (column 2 and 3 for women and 5 and 6 for men). The effects on women's PM major choice are non-significant, even in the stratified specifications. For male students, those from families in the bottom-part of the house value distribution drive the overall results. For students with low economic status, graduating from a class that was >80% (>90%) males implied a 14 (24) percentage points higher probability of choosing PM majors. Both estimates are significant at the 1% confidence level. Correspondingly, those males had a lower probability of enrolling in GB majors (between -7 and -21 percentage points) and lower probability of enrolling in PF majors (by -6.6 or -2.7 percentage points). To the contrary, male students from families in the top quartile of the house value distribution only show a weak increase in the probability of choosing a PM major when graduating from a $>$ 80% or $>$ 90% male class, (equal to 7 to 10 percentage points). It is possible the group of students from higher income families had parents that were more involved and influential in their children's decision of college major. Hence, they may have formed their preferences based on their family environment and were less subject to the effect of peers. To the contrary, male students from less wealthy families may have had less parental involvement in the process of major choice, which resulted in stronger peer effects[21]. Our regressions for male students in families in the top quartile of

[20] In Table A2 in the Appendix we split the male and female sample into above-below the median (rather than top-bottom quartile) of the academic quality distribution. Effects for the below-the-median subgroup are half of those for the bottom quartile in Table 7, confirming that most of the action is happening for the least proficient male students in classes with >80% and >90% male peers.

[21] In the 10% subsample for which we surveyed families, we also stratified the effects by education of the mother (college or less than college). Even in this case we find stronger peer-effects on the probability of choosing PM majors among children of less-educated mothers. The coefficient estimated for the effect of $>$ 90% male peers is 0.73 for children of non-college educated mothers and it is significant. The standard error,

the house value distribution suggest they are less likely to enroll in a PF major if graduating from a >90% male high school class (but not from a >80% male one). Hence, the dominant male composition of the class may affect this group, too, by shifting students out of PF majors. This effect does not seem as strong as for low house value students, who instead were pushed out of GB into PM majors in response to high percentages of male classmates.

5.2 Performance during College

At the universities included in our sample, PM majors (Engineering and Economics-Business) were more math intensive, more academically demanding and more selective than GB and PF majors. Using as a metric the high school exit test score, which is common to all schools (re-scaled to be between 0 and 1), students enrolled in the PM majors had an average of 0.51, those enrolled in PF majors averaged 0.42, and GB majors averaged 0.38. The differences are large and significant. Hence, if "prevalently male" high school classes pressured male students – particularly those with low academic quality – towards enrolling in those majors, this could imply the average college performance of males graduating from $> 80\%$ and $> 90\%$-male classes was worse than those of other males. If some marginal students were pushed into majors that were bad matches for their abilities this could lead to negative effects on their performance, and possibly to higher probability of dropping out or transferring to less demanding majors during college. Alternatively, if "prevalently male" high school classes positively affected the academic ability of male students, together with their preferences, then students graduating from those classes should have no disadvantage vis-a-vis the rest, and they would graduate from the more demanding PM majors in the same proportions as they enrolled. De Giorgi, Pellizzari and Redaelli (2010) found that peer-pressure, or peer-imitation, in college increased mismatch in the choice of classes and majors, with negative impacts on their performance. We want to test whether such an effect is present in our data. More importantly, we test whether the effect of gender composition of high school class persists in the choices and performance (of males) during college, fades away or is reversed during college. The effects of graduating from a $> 80\%$ and $> 90\%$ same gender class on different college outcomes are shown in Table 9. In Panel A the dependent variable is the probability of having graduated from college by 2011. In Panel B we analyze the probability of graduating in PM majors (as opposed to the probability of enrolling that was the dependent variable in Tables 6-8). In Panel C we analyze time-to-graduation (in months, controlling for major and university fixed effects to take into account average differences in time to degree across majors) for those who graduated, and in Panel D the outcome is the

however, is very large and does not allow the rejection of the hypothesis that this coefficient is the same as for children of non-college-educated mothers (point estimate 0.58).

final graduation score in college (standardized to be between 0 and 1), controlling for time-to-graduation, major and university fixed effects to take into account the different grading styles across degrees. The regressions include the individual and class controls as the previous tables.

Looking first at the impact on female outcomes, out of 24 specifications only one has an effect significant at the 5% level. Recall that graduating from a prevalently female class did not have any significant impact on the choice of major, or on performance in high school for women, hence it is reasonable to expect it did not affect performance in college either. Considering males, whose choice of major at enrollment was significantly affected by their high school class gender composition, we notice graduating from a $> 90\%$ male class is still associated with somewhat higher probability of graduating from PM majors (Panel B) and the effect is stronger for males in the bottom part of the academic quality distribution. These effects, however, are half the size of the corresponding effects on enrollment (shown in Table 6) and they are only significant for males who graduated in a $> 90\%$ male class, and for males in the bottom quality distribution from $> 80\%$ male classes. These estimates suggest that male peers might have pressured marginal male students to enroll into PM majors, but many of them did not have the commitment or ability to graduate from them, hence the effect on graduation was much smaller than the effect on enrollment. This possible mechanism is confirmed by the estimates of Panel A, in which males from $> 90\%$ male high school class have a (marginally) lower probability of graduating from college altogether (at the 10% confidence level). Some of them, faced with more demanding requirements in PM majors, may have dropped out. Others may have abandoned the major and enrolled in a non-PM major. The graduation rate effect suggests a potentially higher mismatch into PM majors for the group of students graduating from $>90\%$ male classes. Two other outcomes also appear to be negatively affected by having been in $> 90\%$ male class: time-to-graduation (which was longer, as seen in Panel C) and final graduation score (which was lower, as seen in Panel D), but only for students in the lower quartile of the academic quality distribution. As for those from the $> 80\%$-male classes, the positive and significant effect on PM major enrollment (which was weaker than in the $> 90\%$ male classes) completely faded away by the time of graduation and the other effects were not significant, implying the effect on major choice was only temporary and was reversed through transfers during the college years.

Combining the regression results, we can infer that the gender composition of high school classmates was a random event that affected male students' choice of college major during the last year of high school. However, such peer effects were not strong enough to change the long-run match between students and majors. In fact, high school peer composition may have generated a higher probability of short-run mismatch driven by overconfidence, or imitation in males, especially strong for males in the lowest portion of the academic performance distribution. For the individuals in the $>80\%$-male classes the initial major choice did not result in more males graduating in PM majors nor did it affect any other measure of performance by the end of their college career on average.[22] For males in $>90\%$ male classes, especially those in the low part of the academic quality distribution, a significant positive effect is still present (higher by 17 percentage points) on the probability of graduating from PM majors, but much smaller than the effect on enrollment (46 percentage points). Moreover, this slightly higher graduation probability of graduating in PM majors comes at the cost of lower academic performance, namely longer time to graduation (by 9.7 months) and significantly lower graduation scores (by almost one standard deviation). In the next section we analyze whether we can detect any long-run impact on income of individuals 1 to 15 years into the labor market. We focus only on individuals who graduated from $>90\%$ male classes for which we identified the strongest effects still present in some outcomes at the end of college[23].

5.3 Long-run Effects

By affecting their exposure to PM majors, on one hand, but decreasing the probability of graduating and their academic performance, on the other, the $>90\%$-male classes could have affected the long-run individual performance of men. Those two intermediate outcomes affect income in opposite directions. The net effect could be positive by increasing the probability of exposure, at least for a while, to majors such as Engineering and Business that are associated with high-paying jobs. However, it may also be negative: having either reduced performance and probability of graduation or having pressured individuals into sub-optimal choices. In this section we tackle this question. In Table 10 we first analyze whether there was an effect of >90% male classes on the probability of college enrollment (Panel A), and then on the probability of having nonzero income in 2005 (Panel B).

The data on income of the individuals are obtained from the Italian internal revenue service for the fiscal year 2005 and matched to individuals based on their name, date and place of birth. We consider all individuals who graduated from high school between 1985 and 2000 and hence a group of people that was on the labor market between 1 and 15 years as of 2005. As long as they remained in Italy and earned any income, their record was collected by the internal revenue service and is present in our data. We analyze whether there was any impact on total (log) income by considering all individuals (Panel C) , and then only those

[22] Only low skilled male students are marginally more likely to also graduate from a PM major

[23] We performed the analysis also on individuals graduated from >80%-same gender classes and all effects on post-college outcomes for males are non-significant.

who enrolled in college (Panel D). Panels B-D capture the long-run effect on labor market outcomes. In Panel E we consider only those individuals who enrolled in college (comparable to the sample used in D) and we associate to the "major of enrollment" (not of graduation) of the individual the average income for that major in 2005 (estimated using our sample). Such a variable allows us to isolate, in terms of income, the effect of higher enrollment in PM majors, zeroing out all other potential effects of the $> 90\%$ male classes. In this exercise we assume that individuals in those classes graduated from the PM major of enrollment at the average rate (which we know is not true from Table 9) and went on to earn the average wage for that major. The dependent variable is the log of income in Panels C, D and E , therefore the coefficients represent a percentage change in income.

The results reported in Panels A-D do not show any significant coefficient on the >90% male variable. The point estimates on the college enrollment and nonzero income are very small and non-significant. The point estimates on income (for all workers and for college educated only) are positive on average, but are very noisy and non-significant. Being in a >90%-male class increased exposure of students to PM majors (that generate high paying expertise), but then worsened the outcomes of their college career (hurting earning potential). These opposing effects seem to cancel out. For women we also find no effect. Certainly observing only total income and only one point in time after graduation (2005) limits the depth of an analysis of labor market dynamics and outcomes. However, we find no sign of a significant persistent effect of high school class gender composition.

Considering, instead, the wage-effect of major of enrollment alone, isolated in the estimates reported in Panel E, male students in $> 90\%$-male classes had about 10% higher expected potential income. If we only observed enrollment in those majors for the male students (and not their performance, completion rates and graduation rates) we would be tempted to infer beneficial effects of $> 90\%$ male high school classes on male labor market potentials. However, the analysis we are able to perform – because of our long-run longitudinal data – shows that the consequences of peer effects fade away in the long run. They are much less significant by the end of college and completely undetectable on the labor market. The peer effect on the choice of major of enrollment is significant the year after high school for males, but then the skills and abilities of individuals seem to be the more fundamental determinants of their choice of major of graduation and academic performance.

6 Robustness Checks and Channels

In this section we perform some robustness checks and we test whether other outcomes are affected in the short-run by peer-gender effects that could suggest mechanisms for the impact on the choice of major. As we found consistent effects only on males, we limit our analysis to the male sample.

First, we confirm the effect of $> 80\%$ and $> 90\%$-male classes is still present when we eliminate the extreme (one-gender-only) classes that occurred in the high school "Liceo Einstein" and corresponded to excessively large deviations from randomness (more than 3 standard deviations in the residual distribution as shown in section 4). As these classes raise some issues of departure from randomness in the mechanism of allocation of students to sections (classes) in the "Liceo Einstein", we drop all classes from this school in the estimates of Column (1) in Table 11. The estimated impact on PM major choice of $> 80\%$-male and 90%-male classes are actually strengthened. Certainly the key estimated effects do not hinge on peculiar behavior of people in those extreme classes.

The second specification of Table 11 shows a very demanding check. We consider only siblings in our dataset (identified by having the same last name and home address) and we include family fixed effects, school effects and the usual individual and class controls in the regression. In this case, the result is identified only by the different choices made by male siblings graduating from the same high school within a family differing only in the gender composition of their fifth-year-high school class. The fact that we are restricting our sample to siblings only implies the sample size is decreased by a factor of 8, from almost 10,000 to 1,300. The point estimate of the effect of the $> 90\%$-male classes is 14.3 percentage points, very close to the main estimate in Column (7) of Table 6, 15.4. The standard error, however, is more than three times larger, hence the coefficient is not significant. For the $>80\%$ male classes the point estimate is small and the standard error is high (0.10), so not much can be inferred. The almost-unchanged point estimate for the $> 90\%$ male classes, however, and the extremely demanding nature of the check, convey the idea that classes with $> 90\%$-male shares can be associated with a shift of $13 - 14$ points in the percent probability of attending a PM major by men.

In columns (3)-(6) we use information from the survey about non-academic student activity during the last year of high school, allowing us to introduce interesting information on other aspects of student life and other outcomes. This analysis, however, forces us to limit the sample to the 10% stratified sub-sample for which the survey was conducted. In specifications (3) and (4) we analyze whether, in the survey sub-sample, we are able to detect any effect of $> 90\%$ male classes on the probability of choice of PM major and on the probability of working in a top-end occupation, defined as managers, professionals, or self-employed. While the point estimate of the coefficient on the PM dummy is not far from the estimate using the whole sample (0.22 versus 0.15), the standard error in the sub-sample (which is 10 times smaller than the full sample) is too large to rule out zero effects. On the other hand, the point estimate on the top occupation effect is -0.038 with a large standard error, confirming no evidence on job market outcomes. The dependent variable in specification (5) is a dummy equal to 1 if the student participated in competitive sports during high school. The prevalently male environment could have encouraged typically male behavior in other aspects of the student's life (besides the choice of major). We do not find a significant effect on the probability of participating in sports, typically pursued outside the high school environment in Italy, and more common for men than for women. In specification (6) we analyze whether volunteering for charities (more common among women than men) was affected by the prevalently male environment of the class and find no significant effect. These last four regressions, however, must be taken with a grain of salt as they are performed only on those individuals randomly drawn in the telephone survey and hence the sample only has 1,173 individuals; about one-tenth the size of the whole sample. Overall, these results do not suggest $> 90\%$ male classes affected the non-academic behavior of male. Thus, a potential channel of acquiring alternative skills outside school through these activities does not seem to be at work as a determinant of higher enrollment in PM majors.

7 Discussion and Conclusions

This paper has found a significant tendency of males in prevalently male high school classes to choose typically male college majors. This confirms the relevance of peers as a factor affecting student choices and attitudes. However, thanks to the possibility of following students in the long-run, it also found the increased pressure to choose PM majors is largely reversed during college and has no measurable long-run effects on labor market outcomes. One explanation for this is that male-peer pressure may have caused mismatches between skills and majors for marginal students who would otherwise choose less competitive and less demanding majors. In the long run, these students reversed the PM major choice and, as a result of mismatch, ended up with lower performance in college and lower probability of graduating.

We can speculate about what drives these results and why they are not found for women. On one hand the existing literature finds significant evidence that women choose math-intensive and prestigious school tracks less frequently than men, in large part because they tend to shy away from highly competitive environments (e.g. Buser et al., 2014). We can, therefore, hypothesize that an all-male environment encourages overconfidence in males, while an all female environment does not boost confidence much among women. The interesting finding of our analysis is that this boost of confidence among males affects choices, but not long-run academic ability, and therefore does not result in better outcomes by the time college is complete. This confirms previous analysis that peer-pressure may result in mismatched choices (De Georgi et al. 2010). Alternatively, the effect on men may be linked to more risk-aversion among women, resulting in an unwillingness to modify choices in light of peer choice, perceiving more uncertainty from deviating from the median choice of female classmates. Males, to the contrary, may underestimate the negative consequences from mismatch, making them less resilient to a negative impact. Differences in risk aversion, however, have been shown to explain only a small part of differences in college major choice (Niederle and Vesterlund, 2007).

Overall, we think the results of our analysis constitute a cautionary tale on two important points. First, we show that PM majors are associated with higher wages and, based on correlations alone, one would expect a higher wage and better labor market performance from randomly shifting people from non-PM to PM majors. This wage gain, however, does not bear out in our analysis. Peer composition exogenously influences the choice of PM majors for males, but in the long run the mismatch produces a negative effect on college performance and no effect on wages. Second, the debate on the effects of peers in school has so far focused on academic outcomes and performances, while we emphasize the importance of looking at long-run effects and final outcomes. Further research is needed to shed more light on both these issues, and we hope this papers encourages similar data collections and research analysis in other countries and environments.

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Figures

Figure 1: Annual Income and female shares by major

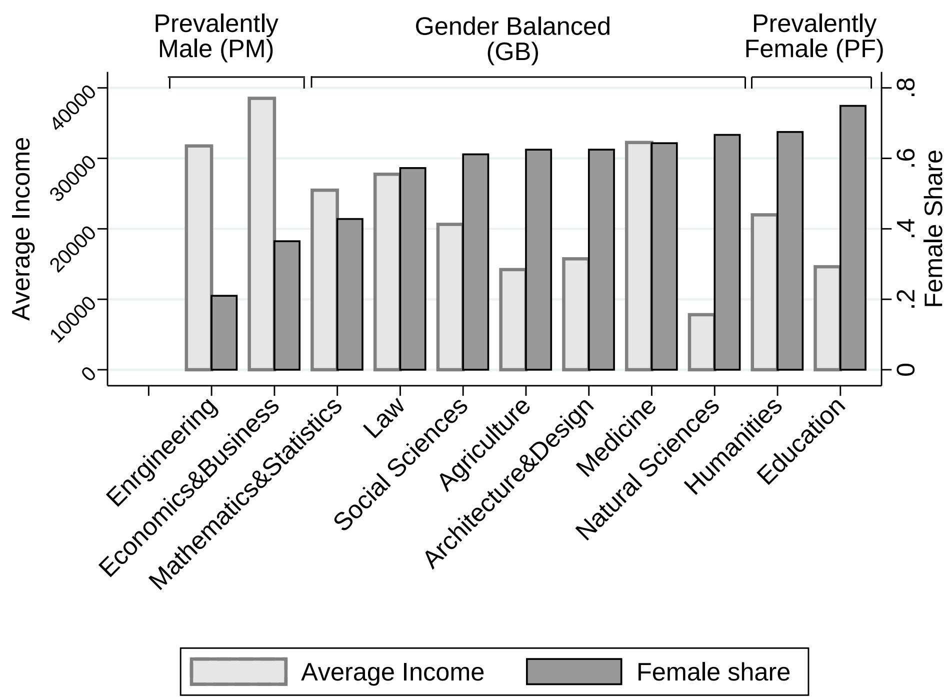
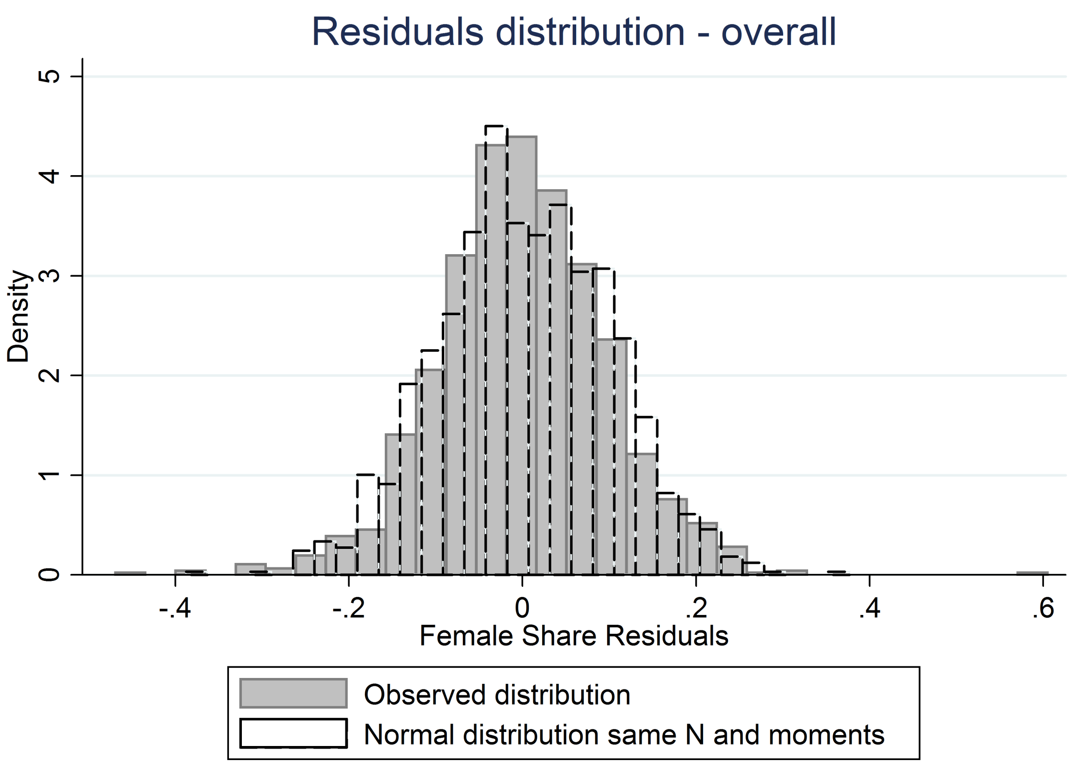


Figure 2: Residuals of female share after controlling for unobserved teachers and school/cohort effects



Tables

Table 1: Summary Statistics

Variable	Obs	All Mean	All Std.Dev.	All Min	All Max	Women Mean	Men Mean	Difference | T-stat |
Individual Variables:								
Female	29370	0.523	0.499					
High School Variables:								
High school exit standardized score	29370	0.416	0.299	0	1	0.435	0.395	11.4
Within school/cohort rank	29370	0.495	0.313	0	1	0.516	0.471	12.4
College Variables:								
Enrolled in Prevalently Male majors	23118	0.275	0.447	0	1	0.139	0.423	50.3
Graduated in Prevalently Male majors	23118	0.215	0.411	0	1	0.118	0.320	39.0
Prevalently Male majors dropout	6362	0.229	.420	0	1	0.172	0.250	6.92
College time to graduation	16193	79.669	26.156	0	1	78.531	80.994	6.0
College exit score	16144	0.822	0.173	0	1	0.860	0.778	30.25
Pre-treatment Variables:								
Log(house value)	22365	7.992	0.298	7.409	9.143	7.986	7.997	2.2
Outcome Variables:								
Log(Income)	17004	9.679	1.279	0.693	13.746	9.461	9.906	22.0
Top Occupation	2957	0.367	0.482	0	1	0.308	0.432	7.10
High School Class Variables:								
Class size	1371	21.432	3.801	10	35			
Female share	1371	0.522	0.181	0	1			
Average high school exit score	1371	0.419	0.104	0.118	0.780			
Average log(House Value)	1219	7.982	0.161	7.606	8.477			
% students in bottom decile of house value distribution	1219	0.094	0.105	0	1			
% students in top decile of house value distribution	1219	0.082	0.107	0	0.57			

Table 2: Major Choice and Income

VARIABLES	(1) Females Log(income)	(2) Males Log(income)	(3) Log(income)	(4) Log(income)
(Female=1)			-0.401***	-0.251***
			(0.031)	(0.031)
(Graduated in PM majors=1)	0.647***	0.549***		0.578***
	(0.043)	(0.043)		(0.031)
(Graduated in PF majors=1)	-0.260***	-0.541***		-0.335***
	(0.043)	(0.068)		(0.037)
H.S. exit score	0.161**	-0.078	0.391***	0.036
	(0.070)	(0.063)	(0.042)	(0.044)
College exit score	0.633***	0.890***	0.039	0.781***
	(0.125)	(0.111)	(0.073)	(0.084)
(House value in top 10%=1)	0.095	0.026	0.089*	0.068
	(0.061)	(0.065)	(0.048)	(0.047)
(House value in bottom 10%=1)	-0.047	-0.062	-0.090**	-0.052
	(0.058)	(0.051)	(0.043)	(0.040)
(Commuting into city=1)	0.009	0.086	0.044	0.041
	(0.060)	(0.065)	(0.046)	(0.044)
Constant	9.411***	9.812***	10.304***	9.703***
	(0.103)	(0.091)	(0.060)	(0.069)
Observations	4,768	4,438	9,206	9,206
R-squared	0.233	0.311	0.224	0.269
School X Cohort FE	X	X	X	X

Method: OLS, Standard errors clustered at school/cohort level in parenthesis, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Sample: Students graduated from high school between 1985 and 2000 and completing college.
Specifications: (1) Female students only. (2) Male students only. (3),(4) both male and female students.
Dependent variable: logarithm of personal income, as revealed to the internal revenue service in year 2005.
Independent variables: For all specifications School/Cohort fixed effects, high school exit score re-scaled between 0 and 1, college exit score (composite of G.P.A. and a score for dissertation) re-scaled between 0 and 1, dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city. For specifications (3) and (4) a dummy=1 if student is female.
Definitions: Following statistics computed on our sample we define Prevalently Female (PF) Majors to be Humanities and Education, Prevalently Male (PM) Majors to be Engineering, Economics & Business. The omitted variable is Gender Balanced (GB) Majors which includes all residual fields of study.

Table 3: Female share and Classroom characteristics

VARIABLES	(1) Female share	(2) Female >80%	(3) Male >80%	(4) Female >90%	(5) Male >90%
Class mean house value	-0.090	-0.201	0.104	-0.032	-0.012
	(0.124)	(0.262)	(0.119)	(0.053)	(0.016)
Class share top 10% house value	0.046	-0.035	0.039	0.026	0.032
	(0.101)	(0.107)	(0.097)	(0.039)	(0.033)
Class share bot 10% house value	0.056	0.216	-0.124	0.102	0.030
	(0.062)	(0.157)	(0.123)	(0.074)	(0.036)
Class size	0.000	-0.009	-0.005	-0.001	-0.001
	(0.004)	(0.008)	(0.006)	(0.004)	(0.001)
Class Size in Bottom 25%	0.006	-0.009	-0.013	-0.012	-0.023
	(0.022)	(0.041)	(0.049)	(0.023)	(0.020)
Class Size in Top 25%	-0.015	0.036	0.014	-0.007	-0.001
	(0.025)	(0.031)	(0.021)	(0.013)	(0.004)
Class mean distance from school	-0.003	-0.000	-0.003	-0.008	-0.002
	(0.006)	(0.016)	(0.006)	(0.006)	(0.003)
Class geo. concentration	0.040	0.066	-0.181	0.007	-0.016
	(0.063)	(0.054)	(0.163)	(0.062)	(0.058)
Observations	1,219	1,219	1,219	1,219	1,219
R-squared	0.721	0.424	0.389	0.417	0.383
School X Cohort FE	X	X	X	X	X
School X Teachers X 5yrs FE	X	X	X	X	X
Joint F-test p> F	0.19	0.50	0.27	0.02	0.92

Method: OLS, Standard errors clustered at school level in parenthesis, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Sample: Each observation corresponds to one class.
Dependent variable: Class female share for specification (1). In specifications (2) and (3) a dummy=1 if classes respectively have more than 80% females and more than 80% males. In specifications (4) and (5) a dummy=1 if classes respectively have more than 90% females and more than 90% males.
Independent variables: Class average logarithm of market value of the house where the students used to live at the time they attended high school, class share of students who used to live in a house valued in top decile of the house value distribution, class share of students who used to live in a house valued in top decile of the house value distribution, class size, a dummy for class size in the bottom 25% of observed class size distribution and one for class size in top 25%, class mean linear distance from school, class geographical concentration (measured as Herfindhal index of concentration in city-blocks). School/cohort fixed effects, group of teachers fixed effects every five years are also included (i.e. school-section fixed effects every five years).

Table 4: Class assignment and pre-determined characteristics

VARIABLES	(1) Log house value	(2) Distance from school	(3) Prob house top 10%==1	(4) Prob house bottom 10%==1
Peers' mean log house value	0.014			
	(0.017)			
Cohort mean house value	-101.873***			
	(10.657)			
Peers' mean distance from school		0.008		
		(0.016)		
Cohort mean dist from school		-119.195***		
		(8.171)		
Class share in top 10% of house values			0.002	
			(0.016)	
Cohort share in top 10% of house values			-127.612***	
			(15.516)	
Class share in bottom 10% of house values				-0.052
				(0.045)
Cohort share in bottom 10% of house values				-110.004***
				(10.554)
Constant	822.292***	315.823***	4.575***	12.282***
	(85.202)	(21.461)	(0.552)	(1.169)
Observations	26,184	25,945	25,945	25,945
R-squared	0.830	0.839	0.849	0.786
School X Cohort FE	X	X	X	X
School X Group of Teachers X 5years FE	X	X	X	X

Method: OLS, Standard errors clustered at school/cohort level in parenthesis, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Sample: High school students graduating between 1985 and 2005.
Dependent variable: for specification (1) the logarithm of market value of the house where the students used to live at the time they attended high school, for specification (2) linear distance from school to the house where the students used to live at the time they attended high school in meters, for specification (3) a dummy = 1 if the student used to live in a house valued in top decile of the house value distribution, for specification (4) a dummy = 1 if the student used to live in a house valued in top decile of the house value distribution.
Independent variables: for specification (1) class peers' and school/cohort peers' average logarithm of market value of the house where the students used to live at the time they attended high school, for specification (2) class peers' and school/cohort peers' linear distance from school to the house where the students used to live at the time they attended high school in meters, for specification (3) shares of class peers and school/cohort peers who used to live in a house valued in top decile of the house value distribution, for specification (4) shares of class peers and school/cohort peers who used to live in a house valued in top decile of the house value distribution. In all specifications school/cohort fixed effects, Group of teachers fixed effects every five year.

Table 5: The effect of high school class gender composition on school/cohort ranking

	Females				**Males**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Variables →	Same Gender Share	Same Gender Share >80%	Same Gender Share >90%	Same Gender Share <10%	Same Gender Share	Same Gender Share >80%	Same Gender Share >90%	Same Gender Share <10%
Dependent Variable ↓								
School/Cohort Rank	-0.012	-0.013	-0.030	-0.057	0.021	0.003	0.048*	0.000
	(0.031)	(0.014)	(0.027)	(0.059)	(0.031)	(0.023)	(0.027)	(0.041)
Observations	13,412	13,412	13,412	13,412	12,533	12,533	12,533	12,533
Dep. var. mean	0.517	0.517	0.517	0.517	0.472	0.472	0.472	0.472
Individual Controls:								
Fam. wealth proxy	X	X	X	X	X	X	X	X
Class-level controls:								
Size	X	X	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X	X	X
SXTeachersX5yrs FE	X	X	X	X	X	X	X	X

Method: Each coefficient corresponds to one specification. OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Specifications (1)-(4) on women only. (5)-(8) on men only.

Sample: All students completing college-prep high schools in Milan between 1985 and 2005.

Dependent variable: Within School/Cohort rank in the high school exit score. Rank was re-scaled to be between 0 (worst score) and 1 (best score)

Treatment: In spec. (1) and (5) share of same gender classmates (i.e. for spec. 1 female share and for 4 male share), in (2) and (6) a dummy=1 if class has more than 80% classmates of the same gender, in (3) and (7) a dummy=1 if class has more than 90% classmates of the same gender, in (4) and (8) a dummy=1 if class has less than 10% classmates of the same gender.

Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.

Class-level controls: dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years).

Table 6: The effect of high-school class gender composition on students' major choice

	Females				**Males**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment →	Same Gender Share	Same Gender Share >80%	Same Gender Share >90%	Same Gender Share <10%	Same Gender Share	Same Gender Share >80%	Same Gender Share >90%	Same Gender Share <10%
Dependent Variable ↓								
Panel A: Prevalently Male (PM) major choice at first college enrollment=1								
(PM Major=1)	-0.002	0.013	0.010	-0.056	0.030	0.063**	0.154***	-0.073
	(0.032)	(0.011)	(0.037)	(0.100)	(0.044)	(0.031)	(0.043)	(0.061)
Dep. var. mean	0.139	0.139	0.139	0.139	0.427	0.427	0.427	0.427
Panel B: Gender Balanced (GB) major choice at first college enrollment=1								
(GB Major=1)	0.062	0.005	-0.024	0.053	-0.023	-0.045	-0.127***	0.016
	(0.045)	(0.022)	(0.039)	(0.164)	(0.050)	(0.035)	(0.038)	(0.072)
Dep. var. mean	0.586	0.586	0.586	0.586	0.460	0.460	0.460	0.460
Panel C: Prevalently Female (PF) major choice at first college enrollment=1								
(PF Major=1)	-0.060	-0.018	0.015	0.002	-0.007	-0.018*	-0.027	0.057
	(0.041)	(0.020)	(0.053)	(0.112)	(0.028)	(0.011)	(0.016)	(0.072)
Dep. var. mean	0.275	0.275	0.275	0.275	0.113	0.113	0.113	0.113
Observations	10,318	10,318	10,318	10,318	9,773	9,773	9,773	9,773
Individual Controls:								
Fam. wealth proxy	X	X	X	X	X	X	X	X
Class-level controls:								
Size	X	X	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X	X	X
TeachersX5yrs FE	X	X	X	X	X	X	X	X

Method: Each coefficient corresponds to one specification. OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Specifications (1)-(4) on women only. (5)-(8) on men only.
Sample: high school graduates enrolling in college.
Dependent variables: In panel A dummy=1 if student enrolled in Engineering, Economics and Business at first college enrollment. In panel B a dummy=1 if student enrolled in Natural Sciences, Mathematics, Statistics, Computer Sciences, Medicine, Agriculture, Architecture, Design, Social Sciences or Law. In panel C a dummy=1 if student enrolled in Humanities or Education.
Treatment: In spec. (1) and (5) share of same gender classmates (i.e. for spec. 1 female share and for 4 male share), in (2) and (6) a dummy=1 if class has more than 80% classmates of the same gender, in (3) and (7) a dummy=1 if class has more than 90% classmates of the same gender, in (4) and (8) a dummy=1 if class has less than 10% classmates of the same gender.
Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.
Class-level controls: dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years).

Table 7: The effect of high-school class gender composition on major choice by academic quality

	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample→	All	Bot Qual	Top Qual	All	Bot Qual	Top Qual
Treatment↓						
	Panel A - Dep. Var.: (PM major=1)					
Same Gender Share>80%	0.013	0.021	-0.010	0.063**	0.201**	-0.051
	(0.011)	(0.018)	(0.031)	(0.031)	(0.095)	(0.040)
Same Gender Share>90%	0.010	0.076**	0.040	0.154***	0.461***	0.132
	(0.037)	(0.037)	(0.056)	(0.043)	(0.142)	(0.081)
Dep. var. mean	0.139	0.057	0.225	0.427	0.255	0.571
	Panel B - Dep, Var.: (GB major=1)					
Same Gender Share>80%	0.005	-0.034	0.035	-0.045	-0.095	0.022
	(0.022)	(0.060)	(0.048)	(0.035)	(0.096)	(0.038)
Same Gender Share>90%	-0.024	-0.090	-0.048	-0.127***	-0.320**	-0.161**
	(0.039)	(0.077)	(0.102)	(0.038)	(0.157)	(0.071)
Dep. var. mean	0.586	0.644	0.515	0.460	0.620	0.327
	Panel C - Dep. Var.: (PF major=1)					
Same Gender Share>80%	-0.018	0.013	-0.026	-0.018*	-0.106***	0.029
	(0.020)	(0.057)	(0.043)	(0.011)	(0.035)	(0.023)
Same Gender Share>90%	0.015	0.014	0.008	-0.027	-0.141***	0.029
	(0.053)	(0.094)	(0.092)	(0.016)	(0.053)	(0.036)
Dep. var. mean	0.275	0.299	0.260	0.113	0.125	0.102
Observations	10,318	2,100	3,075	9,773	1,973	2,864
Individual Controls:						
Fam. wealth proxy	X	X	X	X	X	X
Class-level controls:						
Size	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X
SXTeachersX5yrs FE	X	X	X	X	X	X

Method: Each coefficient corresponds to one specification. OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Specification (1) on women only. Specification (2) on women in the bottom quartile of the within school/cohort/gender rank in high school exit score, (3) women in the top quartile of the same rank. Specification (4) on men only. Specification (5) on men in the bottom quartile of the within school/cohort/gender rank in high school exit score, (6) men in the top quartile of the same rank.
Sample: High school graduates enrolling in college.
Dependent variables: In panel A dummy=1 if student enrolled in Engineering, Economics and Business at first college enrollment. In panel B a dummy=1 if student enrolled in Natural Sciences, Mathematics, Statistics, Computer Sciences, Medicine, Agriculture, Architecture, Design, Social Sciences or Law. In panel C a dummy=1 if student enrolled in Humanities or Education.
Treatment: In first row specifications of each panel dummy for classes with more than 80% of same-gender classmates. In second row of each panel dummy for classes with more than 90% of same-gender classmates.
Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.
Class-level controls: dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years).

Table 8: The effect of high-school class gender composition on major choice by parents' house value

	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample→	All	Low House Value	High House Value	All	Low House Value	High House Value
Treatment↓						
			Panel A - Dep. Var.: (PM major=1)			
Same Gender Share>80%	0.013	0.007	0.041	0.063**	0.139***	0.073
	(0.011)	(0.027)	(0.030)	(0.031)	(0.053)	(0.073)
Same Gender Share>90%	0.010	0.044	0.090	0.154***	0.240***	0.103*
	(0.037)	(0.103)	(0.071)	(0.043)	(0.069)	(0.057)
Dep. var. mean	0.139	0.145	0.137	0.427	0.443	0.426
			Panel B - Dep, Var.: (GB major=1)			
Same Gender Share>80%	0.005	0.091**	-0.040	-0.045	-0.073	-0.060
	(0.022)	(0.044)	(0.045)	(0.035)	(0.056)	(0.086)
Same Gender Share>90%	-0.024	-0.113	0.032	-0.127***	-0.213***	0.045
	(0.039)	(0.162)	(0.092)	(0.038)	(0.071)	(0.077)
Dep. var. mean	0.586	0.588	0.587	0.460	0.447	0.460
			Panel C - Dep. Var.: (PF major=1)			
Same Gender Share>80%	-0.018	-0.098**	-0.001	-0.018*	-0.066**	-0.012
	(0.020)	(0.041)	(0.052)	(0.011)	(0.027)	(0.037)
Same Gender Share>90%	0.015	0.068	-0.121	-0.027	-0.027	-0.148***
	(0.053)	(0.154)	(0.088)	(0.016)	(0.026)	(0.054)
Dep. var. mean	0.275	0.267	0.276	0.113	0.110	0.114
Observations	10,318	2,442	2,142	9,773	2,215	2,277
Individual Controls:						
Fam. wealth proxy	X	X	X	X	X	X
Class-level controls:						
Size	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X
SXTeachersX5yrs FE	X	X	X	X	X	X

Method: Each coefficient corresponds to one specification. OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Specifications: Specification (1) on women only. Specification (2) on women whose families used to live in a house in the bottom quartile of house price distribution of Milan at the time of high school attendance, (3) women in the top quartile of the same distribution. Specification (4) on men only. Specification (5) on men whose families used to live in a house in the bottom quartile of house price distribution of Milan at the time of high school attendance, (6) men in the top quartile of the same distribution.

Sample: High school graduates enrolling in college.

Dependent variables: In panel A dummy=1 if student enrolled in Engineering, Economics and Business at first college enrollment. In panel B a dummy=1 if student enrolled in Natural Sciences, Mathematics, Statistics, Computer Sciences, Medicine, Agriculture, Architecture, Design, Social Sciences or Law. In panel C a dummy=1 if student enrolled in Humanities or Education.

Treatment: In first row specifications of each panel dummy for classes with more than 80% of same-gender classmates. In second row of each panel dummy for classes with more than 90% of same-gender classmates.

Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.

Class-level controls: dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years).

Table 9: The effect of high-school class gender composition on college outcomes

	Females			**Males**		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample→	All	Bot Qual	Top Qual	All	Bot Qual	Top Qual
Treatment↓						
Panel A - Dep. Var.: (College Graduate=1)						
Same Gender Share>80%	0.004	-0.039	0.027	0.021	-0.011	0.020
	(0.021)	(0.049)	(0.028)	(0.021)	(0.093)	(0.034)
Same Gender Share>90%	-0.022	-0.114	0.024	-0.045*	-0.264	-0.067*
	(0.052)	(0.094)	(0.071)	(0.025)	(0.173)	(0.039)
Dep. var. mean	0.759	0.594	0.871	0.712	0.522	0.864
Panel B - Dep. Var.: (Ever graduated in PM Major=1)						
Same Gender Share>80%	0.008	0.006	-0.009	0.041	0.140**	-0.024
	(0.011)	(0.020)	(0.028)	(0.026)	(0.068)	(0.048)
Same Gender Share>90%	0.016	0.025	0.050	0.079***	0.170**	0.108
	(0.033)	(0.036)	(0.049)	(0.019)	(0.068)	(0.085)
Dep. var. mean	0.119	0.038	0.203	0.321	0.141	0.499
Observations	10,318	2,100	3,075	9,771	1,972	2,863
Panel C - Dep. Var.: Time to Graduation - first spell						
Same Gender Share>80%	1.037	-6.736*	1.429	0.542	4.802	1.298
	(0.987)	(3.889)	(1.640)	(1.514)	(6.814)	(2.494)
Same Gender Share>90%	2.544	-2.627	2.319	-1.623	9.763*	8.076
	(2.882)	(9.591)	(3.043)	(3.657)	(5.032)	(5.960)
Dep. var. mean	79.301	87.028	73.602	82.508	89.515	77.204
Observations	7,438	1,160	2,588	6,537	945	2,363
Panel D - Dep. Var.: College graduation score conditional on time to graduation						
Same Gender Share>80%	-0.001	-0.040	-0.002	0.007	-0.064	-0.008
	(0.007)	(0.024)	(0.008)	(0.010)	(0.045)	(0.016)
Same Gender Share>90%	-0.035**	-0.093*	-0.024	-0.012	-0.137***	-0.022
	(0.014)	(0.049)	(0.025)	(0.013)	(0.022)	(0.030)
Dep. var. mean	0.860	0.764	0.926	0.780	0.689	0.853
Observations	7,416	1,149	2,584	6,511	941	2,359
Individual Controls:						
Fam. wealth proxy	X	X	X	X	X	X
Class-level controls:						
Size	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X
SXTeachersX5yrs FE	X	X	X	X	X	X

Method: Each coefficient corresponds to one specification. OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Spec. (1) on women only, (2) on women in the bottom quartile of the within school/cohort/gender rank in high school exit score, (3) women in the top quartile. Spec. (4) on men only, (5) on men in the bottom quartile of the within school/cohort/gender rank in high school exit score, (6) men in the top quartile.

Dependent variables: In panel A dummy=1 if individual completed a college degree. In B dummy=1 if individual ever completed one of PM degree. In C time elapsed between high school graduation and completion of the degree chosen at first college enrollment (in months). In D college final score (computed as a weighted average of GPA and thesis evaluation and discussion) and conditional on time to graduation.

Treatment: In first row of each panel, dummy for classes with more than 80% of same-gender classmates. In second row, dummy for classes with more than 90%.

Individual Controls: dummy=1 if student used to live in a house in top (and one for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city. **Class-level controls:** dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years). **College-level controls:** In panels C and D major fixed effects and university fixed effects.

Table 10: The effect of high-school class gender composition on post college outcomes

	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample→	All	Bot Qual	Top Qual	All	Bot Qual	Top Qual
Treatment↓						
Panel A - Dependent Variable: (Enrolled in College=1)						
Same Gender Share>90%	-0.001	0.089	-0.077	-0.031	-0.008	0.049
	(0.029)	(0.061)	(0.064)	(0.040)	(0.113)	(0.038)
Dep. var. mean	0.784	0.741	0.824	0.793	0.749	0.838
Observations	13,168	2,833	3,733	12,322	2,633	3,416
Panel B - Dependent Variable: (Income is observed==1)						
Same Gender Share>90%	-0.007	0.137*	-0.098	-0.046	-0.043	0.005
	(0.053)	(0.071)	(0.073)	(0.047)	(0.104)	(0.063)
Dep. var. mean	0.713	0.690	0.724	0.728	0.716	0.730
Observations	13,168	2,833	3,733	12,322	2,633	3,416
Panel C - Dependent Variable: Log of Annual Earnings						
Same Gender Share>90%	-0.011	-0.251	0.356	0.145	0.178	-0.008
	(0.144)	(0.337)	(0.335)	(0.095)	(0.260)	(0.117)
Dep. var. mean	9.411	9.199	9.604	9.869	9.677	10.028
Observations	7,833	1,606	2,297	7,814	1,625	2,196
Panel D - Dependent Variable: Log of Annual Earnings (only enrolled in college)						
Same Gender Share>90%	-0.074	0.115	0.168	0.126	0.213	-0.065
	(0.134)	(0.404)	(0.317)	(0.078)	(0.287)	(0.093)
Dep. var. mean	9.422	9.207	9.617	9.849	9.651	10.008
Observations	6,508	1,279	1,999	6,587	1,303	1,933
Panel E - Dep. Var.: Log of expected annual earning, imputed based on major of enrollment						
Same Gender Share>90%	-0.017	0.081	-0.043	0.097***	0.246***	0.113***
	(0.046)	(0.110)	(0.068)	(0.028)	(0.086)	(0.031)
Dep. var. mean	9.981	9.919	10.048	10.178	10.086	10.253
Observations	10,311	2,097	3,073	9,770	1,972	2,863
Individual Controls:						
Fam. wealth proxy	X	X	X	X	X	X
Class-level controls:						
Size	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X
SXTeachersX5yrs FE	X	X	X	X	X	X

Method: Each coefficient corresponds to one specification. OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Specification (1) on women only. Specification (2) on women in the bottom quartile of the within school/cohort/gender rank in high school exit score, (3) women in the top quartile. Specification (4) on men only. Specification (5) on men in the bottom quartile of the within school/cohort/gender rank in high school exit score, (6) men in the top quartile.

Dependent variables: In panel A dummy=1 if individual enrolled in university. In panel B dummy=1 if individual tax return data matched. In Panel C logarithm of personal income, as revealed to the internal revenue service in year 2005. In panel D logarithm of personal income as in C restricting the specification to individuals who enrolled in college. In panel E logarithm of personal income calculated on our sample and imputed by choice of major

Treatment: Dummy for classes with more than 90% of same-gender classmates.

Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.

Class-level controls: dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years).

Table 11: Robustness checks

	Males					
	(1) Omitting Einstein H.S. PMM=1	(2) Family FE PMM=1	(3) PMM=1	(4) Top Occ.=1	(5) Sport=1	(6) Volunt. Charity=1
Explanatory Var.↓						
Same Gender Share>80%	0.089**	0.024	0.008	0.064	-0.072	0.002
	(0.040)	(0.105)	(0.129)	(0.098)	(0.095)	(0.092)
Same Gender Share>90%	0.271***	0.143	0.227	-0.038	0.039	0.277
	(0.075)	(0.163)	(0.382)	(0.153)	(0.090)	(0.190)
Dep. var. mean	0.403	0.442	0.306	0.306	0.306	0.306
Observations	8,639	1,301	1,173	1,309	1,309	1,309
Individual Controls:						
Fam. wealth proxy	X	X	X	X	X	X
Class-level controls:						
Size	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X
School FE	-	X	-	-	-	-
SchoolXCohort FE	X	-	X	X	X	X
SXTeachersX5yrs FE	X	-	X	X	X	X
Family Fe	-	X	-	-	-	-
Survey Sample	-	-	X	X	X	X

Method: Each coefficient corresponds to one specification. OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.
Sample: Male high school students for all Specifications. In (2) sample restricted to siblings only. In (3)-(6) sample restricted to individuals randomly drawn for a phone interview.
Dependent variables: In specification (1) (2) (3) dummy=1 if student enrolled in Engineering, Economics and Business at first college enrollment. In (4) a dummy=1 if individual was working as professional, manager or self-employed in 2011. In (5) a dummy=1 if student used to practice sport at competitive levels in high school. In (6) a dummy=1 if student used to volunteer at the time of high school attendance.
Treatment: In first row specifications dummy for classes with more than 80% of same-gender classmates. In second row of each panel dummy for classes with more than 90% of same-gender classmates.
Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.
Class-level controls: for all specifications dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value. For specifications (1), (3)-(6) School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years). For specifications (2) Family Fixed Effects, School fixed effects.

Figure A1: Dataset Structure

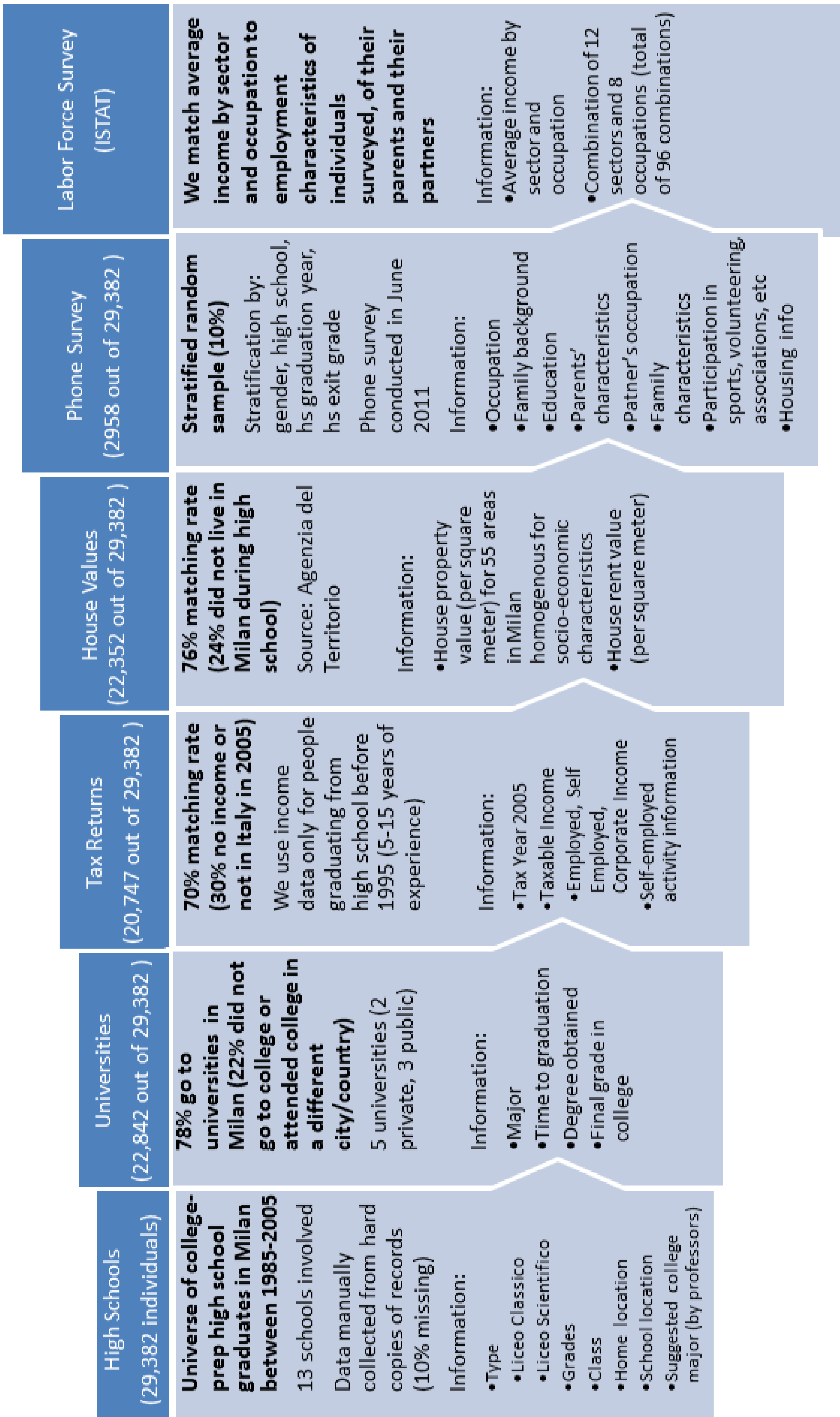


Table A1: The effect of high-school class gender composition on students' major choice by bin of same gender share

	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	PMM=1	GBM=1	PFM=1	PMM=1	GBM=1	PFM=1
Explanatory Var.↓						
Same Gender Share>10% and <20%	0.113	-0.115	0.002	0.033	-0.001	-0.032
	(0.112)	(0.175)	(0.119)	(0.065)	(0.078)	(0.081)
Same Gender Share>20% and <30%	0.043	-0.031	-0.012	0.065	-0.014	-0.050
	(0.101)	(0.161)	(0.110)	(0.062)	(0.074)	(0.072)
Same Gender Share>30% and <40%	0.059	-0.063	0.004	0.077	-0.014	-0.063
	(0.098)	(0.161)	(0.112)	(0.061)	(0.072)	(0.074)
Same Gender Share>40% and <50%	0.052	-0.051	-0.001	0.077	-0.018	-0.059
	(0.099)	(0.162)	(0.112)	(0.064)	(0.074)	(0.072)
Same Gender Share>50% and <60%	0.053	-0.040	-0.013	0.084	-0.016	-0.067
	(0.100)	(0.163)	(0.112)	(0.064)	(0.074)	(0.073)
Same Gender Share>60% and <70%	0.035	-0.040	0.005	0.078	-0.018	-0.059
	(0.100)	(0.163)	(0.113)	(0.065)	(0.074)	(0.073)
Same Gender Share>70% and <80%	0.047	-0.023	-0.025	0.033	0.006	-0.040
	(0.100)	(0.164)	(0.113)	(0.066)	(0.077)	(0.074)
Same Gender Share>80% and <90%	0.057	-0.026	-0.031	0.108	-0.032	-0.076
	(0.101)	(0.164)	(0.115)	(0.072)	(0.084)	(0.075)
Same Gender Share>90%	0.059	-0.056	-0.002	0.238***	-0.147*	-0.091
	(0.106)	(0.168)	(0.124)	(0.075)	(0.078)	(0.075)
Observations	10,318	10,318	10,318	9,773	9,773	9,773
Dep. var. mean	0.139	0.139	0.139	0.427	0.427	0.427
Individual Controls:						
Fam. wealth proxy	X	X	X	X	X	X
Class-level controls:						
Size	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X
SXTeachersX5yrs FE	X	X	X	X	X	X

Method: OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Specifications (1)-(3) on women only. (4)-(6) on men only.
Sample: high school graduates enrolling in college.
Dependent variables: In specifications (1) and (4) dummy=1 if student enrolled in Engineering, Economics and Business at first college enrollment. In (2) and (4) a dummy=1 if student enrolled in Natural Sciences, Mathematics, Statistics, Computer Sciences, Medicine, Agriculture, Architecture, Design, Social Sciences or Law. In (3) and (6) a dummy=1 if student enrolled in Humanities or Education.
Treatment: Dummies for each 10% bin of the share of same-gender classmates.
Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.
Class-level controls: dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years).

Table A2: Effect on major choice by academic quality (above/below median quality).

	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample→	All	Below Med Quality	Above Med Quality	All	Below Med Quality	Above Med Quality
Treatment↓						
	Panel A - Dep. Var.: (PM major=1)					
Same Gender Share>80%	0.013	0.038**	0.002	0.063**	0.090	0.011
	(0.011)	(0.017)	(0.021)	(0.031)	(0.055)	(0.037)
Same Gender Share>90%	0.010	-0.032	0.051	0.154***	0.240***	0.095
	(0.037)	(0.036)	(0.056)	(0.043)	(0.060)	(0.069)
Dep. var. mean	0.139	0.080	0.187	0.427	0.314	0.519
	Panel B - Dep, Var.: (GB major=1)					
Same Gender Share>80%	0.005	-0.002	0.008	-0.045	-0.044	-0.017
	(0.022)	(0.037)	(0.026)	(0.035)	(0.063)	(0.036)
Same Gender Share>90%	-0.024	0.051	-0.109**	-0.127***	-0.180***	-0.115**
	(0.039)	(0.050)	(0.053)	(0.038)	(0.063)	(0.057)
Dep. var. mean	0.586	0.630	0.549	0.460	0.565	0.375
	Panel C - Dep. Var.: (PF major=1)					
Same Gender Share>80%	-0.018	-0.036	-0.010	-0.018*	-0.046	0.006
	(0.020)	(0.034)	(0.025)	(0.011)	(0.028)	(0.016)
Same Gender Share>90%	0.015	-0.019	0.058	-0.027	-0.060**	0.020
	(0.053)	(0.063)	(0.065)	(0.016)	(0.027)	(0.022)
Dep. var. mean	0.275	0.290	0.264	0.113	0.121	0.107
Observations	10,318	4,641	5,677	9,773	4,393	5,380
Individual Controls:						
Fam. wealth proxy	X	X	X	X	X	X
Class-level controls:						
Size	X	X	X	X	X	X
Geo. Concentration	X	X	X	X	X	X
Fam. wealth proxy	X	X	X	X	X	X
SchoolXCohort FE	X	X	X	X	X	X
SXTeachersX5yrs FE	X	X	X	X	X	X

Method: OLS, Standard errors clustered at school/cohort level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Specification (1) on women only. Specification (2) on women below the median of the within school/cohort/gender rank in high school exit score, (3) women above the median of the same distribution. Specification (4) on men only. Specification (5) on men below the median of the within school/cohort/gender rank in high school exit score, (6) men above the same distribution. This table replicates table 7 but stratifies the sample in below vs. above the quality distribution median instead for bottom and top quartile.
Sample: High school graduates enrolling in college.
Dependent variables: In panel A dummy=1 if student enrolled in Engineering, Economics and Business at first college enrollment. In panel B a dummy=1 if student enrolled in Natural Sciences, Mathematics, Statistics, Computer Sciences, Medicine, Agriculture, Architecture, Design, Social Sciences or Law. In panel C a dummy=1 if student enrolled in Humanities or Education.
Treatment: In first row specifications dummy for classes with more than 80% of same-gender classmates. In second row of each panel dummy for classes with more than 90% of same-gender classmates.
Individual Controls: dummy=1 if student used to live in a house in top (and another dummy for bottom) 10% of house value distribution, dummy=1 if student used to commute from outside the city.
Class-level controls: dummy=1 if class size in the bottom 25% and one for class size in top 25%, class geographical concentration (measured as Herfindhal index of concentration in city-blocks), Class share in bottom 10% of house value, Class share in top 10% of house value, School/cohort fixed effects, Group of teachers fixed effects every five years (i.e. school-section fixed effects every five years).