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Abstract

We leverage variation in the timing of women's colleges' transitions to coeducation throughout the 1960s-2000s to study how exposure to a gendered social environment affects women's human capital investments. Applying event study and synthetic control analyses to newly collected historical data, we find that the share of women majoring in STEM at newly coeducational colleges declined by 2.0 percentage points (24%) after ten years of coeducation. Coeducation induced a large increase in the male share of the student body, but did not measurably influence the male share of faculty, capacity constraints or the ability composition of female students. A simple extrapolation of our main estimate suggests that gendered peer effects can account for 34% of the gender gap in STEM majoring. These findings support the hypothesis that non-pecuniary factors, related to social norms and gender roles, shape gender gaps in major choice.

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†Calkins: RAND Corporation, acalkins@rand.org; Binder: U.S. Census Bureau, ariel.j.binder@census.gov; Shaat: University of Illinois - Urbana-Champaign, shaat2@illinois.edu; Timpe: University of Nebraska - Lincoln, btimpe@unl.edu

1 Introduction

In 2016, women earned 57% of all baccalaureate degrees awarded in the United States, but only 37% of degrees awarded in STEM fields.[1] Gender gaps in major choice, and in occupation choice conditional on major choice, account for much of the remaining pay gap between college-educated men and women (Sloane et al., 2019; Blau and Kahn, 2017; Bertrand, 2017). Beyond promoting gender equality, improving gender diversity in high-paying fields has been shown to improve overall economic productivity and innovation (Hsieh et al., 2019). Designing policies to address these issues requires a complete understanding of the causes of the gender gap in choice of field.

Though canonical economic models explain college major choices in terms of pecuniary factors, i.e., the maximization of potential earnings (Willis and Rosen, 1979; Altonji et al., 2012), recent literature suggests non-pecuniary factors also play a significant role (Wiswall and Zafar, 2015; Patnaik et al., 2020b; Wiswall and Zafar, 2018). For example, a survey of Northwestern University students found gender differences in subjective preferences to be the largest determinant of the gender gap in major choice (Zafar, 2013). An experiment by Bursztyn et al. (2017) found that these subjective differences depended on the broader social environment: female MBA students displayed less competitive drive and preference for a high-paying occupation when they expected their behavior to be observed by male peers. On the other hand, male students' behaviors were not affected by peer observability. These findings provoke the question: To what extent do gendered social environments affect gender differences in choices to pursue STEM and other high-paying fields? Answering this question is challenging, as it requires the identification of variation in the real-world environments faced by college students.

In this paper, we analyze the impact of gendered social environments on women's major choices by studying a real-world setting: the decline of women's colleges in the United States. While women's colleges numbered in the hundreds in the early 1960s, most have since transitioned to coeducation. These transitions occurred at varying times and were driven by a number of factors, such as an increasingly competitive environment in higher education and the gradual liberalization of Catholic institutions (Goldin and Katz, 2011). We leverage this variation in a difference-in-differences research design to identify the effect of a quasi-random increase in the male share of the student body on women's major choices.

We first show that transition to coeducation exposed women to substantial inflows of male peers, but was not correlated with other large changes to the educational environment.

[1] These statistics are from the Integrated Postsecondary Education Data System (IPEDS). STEM includes life sciences, physical sciences, science technology, mathematics and statistics, engineering and engineering technology, and computer science.

Our event-study models leverage hand-collected information on the timing of U.S. colleges' transition to coeducation, which we merge to data on the near-universe of U.S. baccalaureate institutions. We find that the male share of the student body increased by about 25 percentage points in the ten years following the arrival of coeducational classes. We also estimate a marginally significant but small increase in the male share of faculty (5 percentage points). We find no evidence that coeducation created capacity constraints at newly coeducational institutions, or was correlated with labor market shocks that could alter the return to STEM degrees.

We find that this exposure caused women to substitute away from traditionally male fields. The share of women majoring in STEM fields decreased by 2.0 percentage points (24%) after ten years of education. This was driven by decreases in the share of women majoring in biology, physical sciences, and math. We also estimate substantial decreases in the shares of women majoring in economics and in business. Correspondingly, we find increases in the share of women choosing health, home economics, and psychology and social sciences other than economics.[2] Our findings are robust to the use of alternative comparison groups, including a synthetic control approach.

We then explore the mechanisms behind this shift toward less quantitative majors. The effect of coeducation on women's field of study could operate through two channels: coeducation could change both women's interest in quantitative majors conditional on attending a particular institution (an "environmental effect") and women's interest in attending the institution in the first place (a "composition effect"). We find no evidence of a change in entering freshman women's average math SAT scores, or the predicted likelihood of majoring in STEM (based on a vector of background characteristics, including SAT scores, high school grades and course-taking, and parental income). We also find no evidence that the transition to coeducation led to an inflow of women STEM majors at peer institutions that remained women-only. These results suggest that the composition effect is small relative to the environmental effect. We conclude that our estimated effects on female students' choice of major are driven by *gendered peer effects*—i.e. greater salience and enforcement of gender roles, gender stereotypes and marriage market considerations that accompany a shift to a coeducational environment.

The paper proceeds as follows. The next section summarizes related literature and our contribution. Section 3 supplies institutional background and discusses the potential effects of transitions to coeducation. Section 4 describes our data and research design. Section 5 presents our main results. Section 6 tests whether the composition of entering female

[2]The vast majority of degrees categorized under health majors are in nursing or allied health fields rather than pre-professional degree programs.

freshman changed in response to coeducation. Section 7 concludes by discussing the broader implications of our results. It presents a simple extrapolation exercise that finds that gendered social environments, and their attendant effects on non-pecuniary determinants of major choice, can account for 34% of the contemporary gender gap in STEM major choice.

2 Related Literature

This paper contributes to literature on the gender gap in STEM majoring at U.S. colleges. While gender differences in math ability (Turner and Bowen, 1999), STEM grades (Calkins, 2020; Goldin, 2015; Astorne-Figari and Speer, 2019), high school course-taking (Card and Payne, 2017) and other student characteristics all contribute to gender differences in college major choice, they only account for a small portion of the gap (Patnaik et al., 2020b). Heterogeneous preferences for majors are the primary determinant of college major choice and a driving factor of the gender gap in STEM majoring (e.g. Zafar, 2013; Wiswall and Zafar, 2015). Recent work has sought the ultimate causes of these heterogeneous preferences—finding, for example, that gender differences in the valuation of flexible jobs accounts for a moderate portion of major preference and choice differences (Wiswall and Zafar, 2018; Patnaik et al., 2020a).[3] Our paper adds to this literature by demonstrating empirically that women's preferences for STEM are affected by the gendered social environments of typical U.S. colleges. Our results suggest that exposure to male peers triggers greater salience of traditional gender roles, gender stereotypes and marriage market considerations—with downstream effects on women's major choices.

Our study of real-world exposure to male peers furthers a literature that has relied largely on laboratory experiments to explore the role of the social environment on gender gaps (Bertrand, 2011). Psychologists have consistently demonstrated that gender gaps in psychological traits and labor market aspirations depend on features of the social context (Spencer et al., 1999; Schmader, 2002; Hyde, 2005; Nguyen and Ryan, 2008). Economists have shown that women's willingness to compete and aptitude in competition are lower when competing against men than when competing only against other women (Gneezy et al., 2003; Niederle and Vesterlund, 2007, 2008). Women's stated preferences for competition and high-stakes scenarios appear to depend on women's social status in society (Gneezy et al., 2009), on exposure to male peers in elementary school (Booth and Nolen, 2012a,b), and on experimentally-manipulated observability by male peers (Bursztyn et al., 2017). Our paper helps link this experimental literature to real-world gender gaps in educational outcomes.

[3] On the other hand, these papers find a much smaller role for gender differences in psychological attributes such as risk aversion or patience.

Our analysis of the effects of male peer exposure also contributes to a growing literature on gendered peer effects in education (e.g. Mouganie and Wang, 2020; Brenøe, 2017; Brenøe and Zölitz, 2020). Existing studies of gendered peer effects in post-secondary education tend to leverage highly specialized settings—such as STEM doctoral programs in Ohio (Bostwick and Weinberg, 2018), the first coeducational class at West Point (Huntington-Klein and Rose, 2018), or introductory classes at a Dutch business school (Zölitz and Feld, 2018). We provide new evidence from a setting arguably more similar to the general American college undergraduate context. In addition, one feature of our context is that we study exposure to men in a mostly-female environment, rather than exposure to women in a mostly-male environment. This is closer to representative of the American higher education landscape today, where women substantially outnumber men on average.

Finally, our paper revitalizes an older literature on the educational role played by women's colleges. Early studies documented descriptive evidence that graduates of women's colleges earned higher income and had higher occupational prestige than graduates of coeducational colleges (Riordan, 1994; Tidball, 1980, 1989). Women at women's colleges have also reported greater satisfaction with educational aspects of the college experience and greater support in their educational endeavors (Smith, 1990; Miller-Bernal, 1993; Kinzie et al., 2007). In addition, Dasgupta and Asgari (2004) found that students at a women's college were less likely to form negative stereotypes about a woman's STEM ability than female students at a coeducational college.[4] Our paper is closely related to a case study by Billger (2002) that found a decrease in the number of women choosing traditionally male majors and occupations after a college transitioned to coeducation. We expand on this work by exploiting variation in the date of transition to coeducation among the near-universe of historical women's colleges in the United States, and studying changes in the composition of the student body as well as choices of major.

3 Women's Colleges and Transitions to Coeducation

3.1 Historical context

Women's colleges have been a part of higher education in the United States since 1836. Most early women's colleges were located in the Northeast and were progressive institutions designed to expand educational opportunities for women (Chamberlain, 1988). Their footprint grew as Protestant- and Catholic-affiliated schools opened, mostly in the South and

[4]Also related is evidence that single-sex classes within a coeducational university improve women's performance (Booth et al., 2018).

Midwest (Harwarth et al., 1997). Non-denominational public universities, such as Texas Women's University, and private women's colleges, such as Sarah Lawrence, added to the ranks in the 20th century. By the 1960s, between 233 and 315 U.S. colleges served a strictly female student body (Harwarth et al., 1997).

The decline of women's colleges began in earnest in the late 1960s, as competition in higher education rose and more women opted out of majors like home economics, food science, and education, which had been the staples of many early women's colleges. Roughly one-half of women's colleges disappeared or converted to coeducation by the early 1970s, with more ending single-sex education nearly every year since. Thirty-four women's colleges, and only three men's colleges, remain today.

The drivers of schools' conversion to coeducation, and the politics underlying these transitions, tended to be unpredictable (Goldin and Katz, 2011). The decision to transition involved the input of several groups of stakeholders: enrolled students, alumnae and alumnae organizations, faculty, the president and college administration, and the board of trustees (Miller-Bernal and Poulson, 2004; Goldin and Katz, 2011). On the one hand, transition was likely to expand enrollment and bring in more tuition revenue. Moreover, as high-achieving students increasingly came to prefer coeducation to single-sex learning environments, transition ensured the continued enrollment of quality students. On the other hand, transitioning to coeducation risked upsetting current students and alumnae, at the cost of vital donation dollars. The possibility of transition was not often a popular one among already-enrolled students and alumnae, who saw women's colleges as a place where women were "the focus of *all* the attention and *all* the opportunities" (Smith College, 2019). News of imminent transition often triggered student and alumnae unrest, which in a few cases prevented the actual transition.[5]

The actual transition to coeducation usually occurred swiftly once the trustees became convinced that coeducation was optimal. In most cases, men were able to enroll as new students in the following academic year and access the full range of classes and extracurricular activities offered by the college. Our research design, which we describe in the next section, leverages the unpredictable variation in the existence and timing of coeducation events to estimate the causal effects of coeducation on women's major choices.

[5] For example, students at both Mills College and Wells College, which considered becoming coeducation in 1990 and 2005 respectively, protested the decision. Only the protests at Mills College were successful. (`https://auburnpub.com/news/wells-students-sit-in-patterned-after-mills/article_f4b31c39-d415-5996-b823-ce16b40f2d15.html`.)

3.2 Expected effects of coeducation

Guided by the literature discussed in Section 2 and a formal model of women's college and major choices (presented in Appendix A), we discuss a number of mechanisms by which women's choices of degrees at formerly-women's colleges may be affected by the transition to coeducation. We primarily focus on the distinction between *composition* effects and *environmental* effects. Our empirical analyses, presented in sections 5 and 6, attempt to distinguish between these two effects, as well as some of the channels that comprise environmental effects.

First, women's college j's transition to coeducation may alter the enrollment decisions made by women high school seniors. Women who prefer a single-sex environment may substitute away from j and toward other women's colleges, while women who prefer a co-educational environment may substitute toward j. If women's propensities to major in STEM are correlated with preferences for single-sex educational environments, we may see changes in majoring behavior that are driven by changes in the *composition* of the student body.

For women who continue to enroll at college j, the transition to coeducation may impact major choices by changing the (perceived or realized) payoffs of majoring in STEM relative to non-STEM fields. This effect, which we refer to as the "environmental effect," may itself be the product of several different factors associated with the transition to co-education. For example, the presence of men on campus could increase the salience of gender identity norms, leading some women to choose non-STEM fields out of a sense of social conformity (Spencer et al., 1999; Steele, 1997; Nguyen and Ryan, 2008; Murphy et al., 2007). Gender identity norms could also play a role through the marriage market if majoring in a high-paying, STEM-related field is seen as a negative signal to potential future spouses (Bertrand et al., 2015; Bursztyn et al., 2017). We refer to this collection of mechanisms as *gendered peer effects*.

Other mechanisms determining the environmental effect may result from changes in the educational inputs provided by the newly-coeducational college. If the college hires more male faculty to prepare for the arrival of male students, one byproduct could be a weakening of role model effects that have been shown to draw women into quantitative fields (Carrell et al., 2010; Kofoed and McGovney, 2019; Bottia et al., 2015). In addition, even holding the gender mix of the student body constant, coeducation could influence women's major choices through gender-neutral peer effects—i.e. by changing the average quantitative ability of their peers.

Our data do not permit exact quantification of these various mechanisms. However, we use a variety of indirect approaches to rule out some mechanisms in favor of others. Our results suggest that mechanisms other than gendered peer effects are of limited quantitative importance.

4 Data and Research Design

4.1 Institution-level data

Our main analyses use data from the Higher Education General Information Survey (HEGIS) and the Integrated Postsecondary Education Data System (IPEDS). HEGIS and IPEDS provide information from 1966–1986 and 1987–2016, respectively, on the number of degrees awarded by year, institution, major, and gender. HEGIS collects data from all institutions of higher education subject to Title IV of the Higher Education Act of 1965. IPEDS collects data from all post-secondary institutions in the United States who participate in federal financial assistance programs—a larger sample than HEGIS. Many schools participate in IPEDS voluntarily even if they do not accept federal financial assistance, so the coverage of U.S. baccalaureate-degree-granting institutions in IPEDS is nearly universal. The data from the HEGIS 1970 issue on degrees awarded has historically not been available in digital form. Because this is an important time period for our study—a large number of institutions in our sample transitioned to coeducation in the late 1960s and early 1970s—we digitized the 1970 issue of the HEGIS data on degrees awarded.

We use these data to construct a measure of the share of graduates of gender G that earn a degree in concentration c at institution i in graduation year t:

$$s^G_{c,it} = \frac{\sum_\mu D^G_{\mu,it} \times \mathbb{1}(\mu \in c)}{\sum_\mu D^G_{\mu,it}}. \tag{1}$$

In this expression, $D^G_{\mu,it}$ is the number of degrees in major μ earned by graduates from institution i in year t of gender G. The data only provide a measure of degrees awarded: we do not observe individuals who matriculate but do not finish their degrees, and we do not observe the time to completion for those who finish their degree. As a result, we cannot investigate intermediate channels of degree progress.

HEGIS and IPEDS also provide information on institutional characteristics that may be related to women's choice of field and an institution's capacity to produce STEM majors. These data were collected less consistently and include total student enrollment and the number of full-time faculty by gender. For example, data on the number of male and female full-time faculty begins in the 1971-72 academic year, but the survey was run on an intermittent or every other year basis until the 1990s—at which point it became a yearly survey. We use these data to explore the mechanisms behind our main results, but interpret the results cautiously given the less reliable nature of these measures.

4.2 Hand-collected data on transitions to co-education

We are not aware of a comprehensive historical list of women's colleges in the United States, nor a reliable account of the date these schools transitioned to coeducation. Accordingly, we hand-collected a list of women's colleges and their dates of transition to coeducation. We view this as one important contribution of our study.

We construct this dataset via the following procedure. First, we identify all institutions that awarded over 90% of their degrees to women in the first year they they are observed in the HEGIS/IPEDS data. Because of concerns about response rates from institutions that entered the data after the transition to IPEDS, we restrict our sample to HEGIS-participating institutions that began offering four-year degrees before 1987.[6] For each of these institutions, we gathered information on whether it was once a women's college. If it was, we recorded the date of transition to coeducation based on the institution's website, the "Everywoman's Guide to Colleges and Universities" (Howe et al., 1982), or other online sources.[7] This procedure identified 219 institutions that were women-only in the first year they were observed, 155 of which eventually transitioned to coeducation.

After merging our hand-collected data to the HEGIS/IPEDS data, we make a number of sample restrictions. We drop a set of institutions where the transition to coeducation was a gradual process rather than a single event, including women's colleges that merged with men's colleges, institutions where male students were admitted as commuter students only, and institutions with large numbers of male graduates before the transition to coeducation but who did not have coeducational adult education programs.[8]. We also omit institutions that closed shortly after the transition to coeducation and institutions that did not award any STEM degrees to women in the first year we observed them. We are left with a sample of 87 institutions that switched to coeducation.

Our main comparison group consists of women's colleges that did *not* transition to coeducation during our sample frame. However, we also consider specifications the include institutions that were fully coeducational in 1966. To promote sample balance, we omit institutions that were in the data for less than 15 years from this group. We also omit military academies, institutions that were men's colleges in 1966, coordinate colleges, institutions

[6]The ICPSR documentation for the IPEDS financial data for fall 1987 (study 2220) discusses concerns about response rates and imputation for institutions which enter the data after the transition to IPEDS.

[7]Where possible, we relied on institutions' own websites for this information. Over 90% of our transition dates were found on *.edu* websites.

[8]Appendix D.3 includes a more thorough breakdown of our sample.

where no women were awarded STEM degrees in the first year we observe them,[9] and for-profit institutions. This leaves us with a total of 27 institutions that are always women's colleges and 921 institutions that are always coeducational.

Figure 1 documents the distribution of the years of transition to coeducation at women's colleges in our sample. The modal transition date is between 1969 and 1971, before the passage of Title IX in 1972.[10] Although Title IX affected the educational environment and outcomes of women at public institutions (Rim, 2020), it is unlikely to have caused meaningful change in the women's colleges in our sample—the majority of which were private and received little federal funding.[11]

4.3 Student-level data

To gauge the extent to which coeducation alters the underlying composition of women (i.e. the "composition effect" discussed in Section 3.2), our analysis also requires indicators of entering women students' preparedness for STEM coursework. These include math SAT scores, high school STEM coursework taken, parental education and income, self-assessments of STEM aptitude or ambition to pursue a career in STEM, and others. We collect these data from The CIRP Freshman Survey (TFS), produced by the Higher Education Research Institute (HERI) of UCLA. TFS surveys freshmen at participating institutions on their college and career plans as well as their academic preparedness and family background.[12] We link TFS to our IPEDS/HEGIS sample to include institution-level information in our TFS regressions. Our linked sample includes 723 institutions, 67 of which switched from women-only to coeducation and nine of which are currently women-only. Enrollment data

[9] Several institutions that were originally men's colleges transitioned to coeducation program-by-program, often establishing a nursing or teaching program before allowing women into academic programs. STEM programs were often the last to open to women.

[10] Our sample has switch dates that are more concentrated in the late 1960s and early 1970s than our full data on former women's colleges. A number of institutions that changed to coeducation in the 1980s and 1990s either closed shortly thereafter, did not have STEM programs in 1966, or had large numbers of male graduates before their official date to coeducation. However, the fact that the modal transition date is in the late 1960s does not change.

[11] Title IX prohibited discrimination on the basis of sex at undergraduate public institutions, but it did not initially apply to private colleges. Although Title IX did apply to other programs at private institutions if the institution received federal money, including student aid grants, until 1987, Title IX only applied to the program receiving federal money, rather than the entire institution (see pg. 8 and footnote 15 of Rim (2020)). We also believe that, aside from student aid grants, federal money was not a large driver of decisions at former women's colleges in our sample, the vast majority of which were small private colleges. Direct federal appropriations were very unlikely, and the median total expenditure on research per year was \$0 (according to the IPEDS finance data).

[12] Institutions decide each year whether to participate in TFS, and may not participate in every year. Students are surveyed before they begin their college careers; many institutions have students fill out the survey during freshman orientation.

is not part of our linked dataset; we therefore do not include the small schools specification in our TFS regressions.

4.4 Summary statistics

Table 1 presents descriptive statistics of our sample of switchers, as well as each of our four comparison groups. The comparison groups are relatively similar in the share of women choosing STEM in the first year they enter the data. The switchers are similar to the always-women's colleges in terms of initial enrollment, the initial share of women majoring in STEM, and the number of graduate degrees awarded. All public women's colleges eventually switched to coeducation, but the switchers sample is still 78% private, whereas only 30% of the always-coed sample is private. Forty-seven percent of our switchers were at some point affiliated with the Catholic church, compared to 33% of the always women's colleges and no more than 10% of the schools in the other comparison groups. And finally, switchers are less selective on average than the always women's colleges, and slightly more selective than always-coed colleges. (Selectivity is based on Barron's 1-7 ratings from 1972, where a rating of 1-3 is deemed "selective.") In our analysis, in addition to controlling for institution fixed effects, we also control non-parametrically for time trends in many of these characteristics, to ensure sample balance between treated and control observations.

As discussed in the section 3, the historical account suggests that women's colleges' transitions to coeducation were unpredictable events. To test this account, we estimated linear probability models of ever transitioning to coeducation on the full sample, as well as switching before 1972 on our sample of switchers. We included controls for region, religiosity, size (i.e. log enrollment in the first year of observation) and college selectivity rating. Table 2 reports these estimates. While most of these variables significantly predict the existence and timing of a transition to coeducation, they jointly explain less than 22 percent of the variation in each outcome. These results suggest that there is meaningful unexplained variation in transitions to coeducation available to analyze within a causal framework.[13]

As men joined the student body at former women's colleges, their presence was felt disproportionately in traditionally "male" disciplines. Figure 2 reports the distribution of majors chosen by students at our sample of former women's colleges and a comparison group of coeducational schools. Relative to men at schools that were coeducational throughout our sample period (light blue), the first cohorts of men who entered former women's colleges were less likely to choose heavily quantitative majors (medium blue): They were less than half as likely to major in STEM and more likely to specialize in health or social sciences

[13]For a more complete analysis that yielded similar conclusions, see Goldin and Katz (2011).

other than economics. However, relative to women at these schools (dark blue), they were more likely to choose relatively quantitative, high-return fields such as STEM, economics, and business. This suggests that the gender mix of the classroom in these fields changed quickly, as did classrooms in other fields such as sociology and psychology.

4.5 Empirical strategy

We exploit variation in the timing of women's colleges' transitions to coeducation to estimate effects of coeducation on women's major choices and the determinants of those choices. Because we expect these outcomes to evolve dynamically, our main results rely on the following event-study specification:

$$Y_{it} = \theta_i + W_i \sum_{k=-5}^{10} \beta_k 1\left(k = t - t_i^* - m\right) + \delta_{r(i)t} + \psi_{c(i)t} + \varepsilon_{it}. \tag{2}$$

In this equation, Y_{it} is an outcome of interest (e.g., the share of women graduating from institution i in academic year t with a STEM degree), and θ_i is an institution fixed effect.

The coefficients of interest are β_k, which flexibly trace out the effect of the transition to coeducation on outcome Y_{it} at each year relative to the transition k. The academic year in which school i switched to coeducation is given by t_i^*. When estimating specification 2 using data from the TFS, which measures the characteristics of the entering freshman class, we set $m = 0$. However, when using our IPEDS/HEGIS data on degree completion, the precise time relative to t_i^* at which any treatment effect should arise is theoretically ambiguous. If a student's choice of major is affected by the arrival of male students during her senior year, effects may show up immediately after the transition. On the other hand, if men filtered into former women's colleges gradually — a pattern we see empirically — and if women are most affected by men in their own cohort, we would expect to see an effect that manifests gradually with event time. For these specifications, we therefore set $m = 2$. We also normalize $\beta_{-1} = 0$. We therefore can interpret β_0 as the effect on the share of students earning a degree in a given field during the junior year of schools i's first mixed-gender cohort. Note that this is not a restrictive assumption; effects that manifest in the first two years of coeducation would show up as shifts during our "pre-period" from $k = -5$ to $k = -1$.

The indicator W_i is equal to 1 for schools that switched from female-only to co-education during our sample period. Conceptually, this means that we include a comparison group of institutions that did not convert to co-education during our sample period. Inclusion of these institutions improves precision. As shown in Table 1, women's colleges in 1966 were more likely to be private and religiously affiliated than colleges that were coeducational in 1966.

Women's colleges were also more common in certain parts of the country. This motivates the inclusion of $\delta_{r(i)t}$, a set of region-by-year fixed effects, and $\psi_{c(i)t}$, a set of institutional-control-by-year fixed effects. These fixed effects account nonparametrically for differential trends across regions of the United States and different types of institutions, respectively. Institutional control is measured at the first year we observe the institution and differentiates between public, Catholic private, and other private colleges.

In all analyses, we cluster standard errors at the institution level. Our preferred specifications are weighted by the total number of degrees earned by women. However, the results do not substantially change if we do not weight the regressions (Solon et al., 2015).

Our identifying assumption is that, conditional on these control variables, *there is no residual determinant of Y_{it} that is correlated with an institution's transition from a female-only student body to co-education.* While this assumption is fundamentally unverifiable, we conduct several falsification tests. First, our event-study specification enables us to estimate β_k before the reform. A series of coefficients that depart significantly from a flat pre-trend would suggest the presence of confounding variables. Second, we present our main results based on 4 different control groups: schools that were women-only throughout that period ("always women's" colleges), all non-switching schools in the sample, schools that were coeducational-only throughout the sample period ("always coed"), and schools with less than 5,000 students enrolled in the first year we observe their enrollment data (fall of 1968 or first year they enter the HEGIS sample—whichever is later).[14] We comment on the sensitivity of pre-trend results to the choice of control group where we find it, and what this implies about the proper choice of control group. Third, we conduct a synthetic control analysis on the main sample and verify that the results match those generated by the event studies.

In addition to these indirect tests, we directly test whether the switch to coeducation was correlated with certain labor market conditions that might determine women's STEM major choices. We use the March CPS to construct four measures of labor market conditions at the state-year level (Ruggles et al., 2020) and then regress these measures on equation 2 (Pei et al., 2018). The results of this exercise are shown in Figure A1. We find no effect on the unemployment rate, a common measure of the overall health of the labor market. The event-study is quite flat, and if we restrict equation 2 to a single β_k indicating post-transition, we obtain a precise 0 estimate. We also find no correlation between coeducation and the ratio of employment in STEM-related occupations to employment in non-STEM occupations

[14]The largest switcher in our sample, Texas Women's University, had slightly more than 5,000 students enrolled in fall 1968. This specification establishes balance between treated and control schools on initial enrollment. In this specification, we also restrict the treatment group to schools with enrollment under 5,000 in the first year we observe them.

among college-educated workers (difference-in-difference estimate of 0.00066, s.e. 0.0026). We do estimate a short-run increase in the earnings of *male* workers in STEM occupations (relative to male workers in non-STEM occupations) following coeducation. However, to the extent that this estimate indicates an increase in demand for STEM workers, we would expect it to *increase* the share of students majoring in STEM fields. Furthermore, this effect dissipates by the fourth year post-transition, and the difference-in-difference estimate is indistinguishable from 0 (0.019, s.e. 0.018). Finally, the relative earnings among *women* in STEM occupations exhibited a much flatter trend around the timing of coeducation (difference-in-difference estimate -0.0044, s.e. 0.017). These results suggest that relevant labor market conditions were orthogonal to the timing of coeducation.[15]

5 Results

5.1 The effect of coeducation on women's exposure to male peers

We start by examining how the change to coeducation altered the gendered social environment at former women's colleges. We focus first on the gender mix of the student body. We first use data from the CIRP Freshman Survey, which contains demographic information on entering freshman students. For each institution and year, we calculate the share of the entering class that is male and regress this on equation 2 with $m = 0$. Panel A of Figure 3 reports estimated event study coefficients. Coeducation induced a rapid increase in the male share of freshmen beginning with the first coeducational cohort. By year 10 after coeducation, the male share of the freshman class had risen by roughly 30 percentage points relative to the control group in all samples. Combined with the distribution of men's major choices shown in Figure 2, this result suggests that women, particularly in STEM-related classrooms, experienced substantial inflows of male peers.

One limitation of this exercise is that it only represents around one-third of the events in the main HEGIS/IPEDS sample. Though we cannot directly measure the gender mix of enrolled students in the main sample before the 1968-1969 academic year, we can construct a proxy using the share male among students who earned a degree from institution i in year t. This indirect measure may suffer from measurement error due the presence of transfer

[15] Another potential threat to identification would be the existence of capacity constraints that limited women's opportunity to major in quantitative fields due to the school's inability to handle an influx of students rather than via the gender mix of students or faculty. We note that capacity constraints are unlikely to drive our results because the transition to coeducation was often a response to a reduction in demand for single-sex education, suggesting that classroom, lab, and other capacities were slack at the time of coeducation. In Section E of the Appendix, we provide an indirect test of the capacity constraint hypothesis, and find that it cannot explain our results.

students, variation in time to degree completion, and the earning of multiple degrees. However, it is available for all institutions in our sample. We regress this measure on equation 2 and present estimates in Panel B of Figure 3. The results are similar to those in Panel A—both show a substantial increase in the male share of the student body that levels off after an initial spike.

5.2 The effect of coeducation on women's exposure to male faculty

Another important component of the gendered social environment is the composition of members of the faculty. In fall 1971, 51% of faculty were female at women's colleges, compared to only 23% at coeducational institutions. Information on faculty in the IPEDS/HEGIS data is spotty, but we do observe the number of faculty members by sex beginning in fall 1971, and then in select years until fall 1989, when it is reported relatively consistently. We construct the female share of faculty by year and institution and regress it on equation 2.

Although Kaplan (1978) notes that the faculty of Vassar College became substantially more male after the transition to coeducation, we do not find that this was true on average. Figure 4 reports the effect of transition to coeducation on the female share of faculty, based on estimates of Equation 2. We find a small negative effect that only becomes statistically significant in years 7-10 after the junior year of the first coeducational cohort. By year 10, the female share of the faculty had declined by only 5 percentage points. This is quite small relative to the 30 percentage point exposure to male peers estimated above. We therefore conclude that there is little evidence to suggest that transition to coeducation diminished the role-model effects of female faculty on women's major choices—certainly not within the several years after the first coeducational classes began choosing majors.

5.3 The effect of coeducation on women's STEM majoring

How did the sharp increase in male representation on campus affect the share of women earning degrees in quantitative fields? Figure 5 reports the effect of coeducation on the share of women who graduates with degrees in biology, physical sciences, math, and economics, based on estimates of Equation 2. We find evidence that the influx of men led to a persistent decrease in the share of women majoring in each field. Women were 0.9 percentage points less likely to major in biology, 0.3 percentage points less likely to major in physical sciences (primarily physics and chemistry), 0.5 percentage points less likely to major in math, and 0.7 percentage points less likely to choose economics.[16] To provide indirect evidence on the

[16] See Appendix Table A2 for difference-in-difference coefficients for all majors and specifications.

validity of our identifying assumptions, we test whether β_{-5}, β_{-4}, β_{-3}, and β_{-2} are jointly different from zero. We find no statistically significant evidence of a pre-trend.[17]

In Appendix Figure A2a, we also evaluate the effect on a more comprehensive category of STEM fields. Using our restricted, difference-in-difference version of equation 2, we find a strongly statistically significant decrease of 1.8 percentage points (22%) in the share choosing STEM.[18]

Appendix Figure A3 shows the response of other categories of majors to coeducation. In addition to STEM and economics, we divide the universe of majors in 9 mutually exclusive categories. We see clear evidence that women flowed into more traditionally female-dominated majors — particularly health, home economics, psychology, and other social sciences (not including economics) — in the wake of the transition to coeducation. In addition to STEM fields and economics, women were less likely to major in business. We return to these substitution patterns in section 5.4.

5.4 The effect of coeducation on the full distribution of major choices

To examine the effect of coeducation on the full distribution of fields chosen by women, we classify all fields into eleven major concentrations: STEM, art, business, economics, education, health, home economics, humanities, psychology, social sciences other than economics and psychology, and all other fields.[19] To provide a summary measure of the effects of coeducation across different majors, we restrict Equation 2 by estimating a single β_k for $k \geq 0$. This difference-in-difference coefficient, β_μ^{DiD}, describes the average effect of coeducation (over our 10-year window of observation) on the share of women majoring in μ. Similarly, we estimate the effect on the male share of the graduating class to obtain β_{mshare}^{DiD}.

We then scale this effect as follows. For each major μ, we report estimates of $\gamma_\mu^{DiD} = \beta_\mu^{DiD} \cdot 0.1/\beta_{mshare}^{DiD}$. That is, γ_μ^{DiD} describes the effect of a coeducation-induced 10 percentage

[17] The p-values for biology, physical sciences, math, and economics are 0.30, 0.28, 0.22, and 0.27, respectively.

[18] When using our alternative comparison groups, the full STEM estimates display some noisiness during the pre-period, which we attribute to the math major. We believe these pre-period effects may be due to a historical decline in math majoring in the late 1960s and early 1970s associated with the end of the Space Race and nationwide changes in the structure and focus of the math major (Tucker, 2013). The effect of coeducation on students who were juniors ten years after the shift to coeducation could be interpreted as a long-run effect. These students were 2.0 percentage points (24%) less likely to major in STEM as a whole following the transition to coeducation. This decline may have been larger at women's colleges, as other colleges may have only recently allowed women into their math programs. Panel B displays results for STEM degrees other than math.

[19] A list of the specific fields that make up these 11 categories is provided in Appendix B.2.

point increase in the male share of the student body on the share of women majoring in μ.[20]

Figure 6a ranks estimates of γ^{DiD} in increasing order across all eleven major concentrations. STEM experienced the largest outflows of women, with economics experiencing a smaller outflow. In contrast, we find suggestive evidence that health experienced large inflows of women, and home economics, pscyhology, social sciences other than economics, and the "other" concentration also experienced moderate inflows,[21] though none of the inflows were statistically significant. Recall that these estimates are expressed in terms of a 10 percentage point increase in the male share of the student body. As coeducation induced a long-run increase of 25-30 percentage points (Figure 3), these estimates should be multiplied by a factor of 2.5-3 if one wishes to measure the long-run consequences of coeducation.

Figure 6b presents semi-elasticity responses that account for the baseline share of women majoring in each concentration. For a given major μ, the semi-elasticity is given by $\epsilon_{\mu}^{DiD} = \gamma_{\mu}^{DiD}/s_{\mu,-1}^{W}$, where $s_{\mu,-1}^{W}$ is the share of women majoring in μ in the year before transition to co-education. The panel displays a pattern of substitution out of traditionally male majors and into traditionally female ones. An exogenous 10 percentage point increase in the male share of the student body is associated with a 17.4% decline in the share of women majoring in STEM, relative to control schools. Scaling this estimate by 2.5, we conclude that in the long run, coeducation induced roughly 45% lower relative growth in women's STEM majoring. The semi-elasticity is very large for economics—a 10 percentage point increase in male exposure causes a 37% relative decline in the economics share of female graduates—though imprecisely estimated.In contrast, we estimate substantially positive, though not statistically significant, semi-elasticities for health, home economics, and other.

5.5 Robustness check: the synthetic control method

As a robustness check on our main result, we use the synthetic control method to estimate the effect of transitioning to coeducation on women's STEM major choices. The synthetic control method offers a data-driven procedure to construct a control group that matches our treatment group based on pre-treatment characteristics. Thus, it may provide a valid comparison group even if our identification assumption fails in the standard difference-in-differences methodology used above.

[20]Because the estimate of γ^{DiD} is the product of two estimates, we conduct inference via a block bootstrap routine that accounts for the possibility of intraclass correlation at the institution level. We compute $1,000$ estimates of γ_{μ}^{DiD} and its underlying components via Monte Carlo resampling; the $2.5th$ and $97.5th$ percentiles of this distribution form the confidence interval of our estimate.

[21]"Other" contains a miscellaneous set of small majors, many of which were not likely offered by small private colleges (e.g. agriculture and forestry). The ones that were offered include interdisciplinary majors, theology and religious majors, and social service majors. Thus, at small private colleges, we believe that "other" consists primarily of traditionally female fields.

One complication of our setting is that we have multiple "treated" schools rather than the single treated unit that is standard in synthetic control settings (e.g. Abadie et al., 2010). We adjust the standard procedure in two ways to incorporate this complication. First, we group schools that switched to coeducation in the same year, so that the "treated" groups are effectively school-cohort combinations. Second, we construct a synthetic control group separately for each cohort of treated schools and then average the effects by year relative to the switch (Cavallo et al., 2013; Acemoglu et al., 2016).

Our baseline specification constructs a synthetic control group for each treated school-cohort observation by matching on the entire set of pre-treatment outcome variables (Ferman et al., 2020). Figure 7 reports the results of this estimation procedure. The synthetic control event study traces a similar path as did our standard event study (Appendix Figure A2): it shows a 2 percentage point decrease in the share of women majoring in STEM by five years after the transition to coeducation and a 3 percentage point decrease by nine years after the transition to coeducation. We calculate a "difference-in-differences" estimate by averaging the post-treatment coefficients and subtracting them from the average pre-treatment coefficients. The estimate of -0.02 is an outlier in the distribution of placebo effects, with a p-value of 0.007.[22] This estimate is almost identical to the one we obtain in our main event study model.

By matching on the entire set of pre-treatment outcome variables, this baseline approach reduces concerns about specification searching. In addition, in cases with sufficiently long pre-periods, it reduces the bias of the synthetic control estimator (Abadie et al., 2010; Kaul et al., 2018). However, its disadvantage is that it runs the risk of overfitting, particularly for our early-switching schools with few observed pre-treatment periods. To assuage these concerns, Appendix Figure A5 reports estimates from three other specifications (Ferman et al., 2020). The first specification replicates the baseline. The second specification matches on only the second half of pre-treatment outcomes as matching variables, rather than the full pre-period. The third specification matches on a five-year average of the pre-treatment period, a five-year average of the ratio of total PhD and professional degrees to bachelor's degrees, and the share of *all* students at a school that majored in humanities, social science, physical science, and business in the year before the reform. Finally, the fourth specification replaces the bachelor's-degree shares from event-year -1 with those same shares for the last half of the pre-period. While these robustness checks are generally noisier than our baseline estimates, the results are relatively consistent across specifications.

[22] We conduct inference by randomly reassigning treatment status and estimating the effect of the transition to coeducation on the placebo institutions (Abadie et al., 2015). If our estimated effect is either below the 2.5th percentile or above the 97.5th percentile of placebo effects, the effect is statistically significant.

6 Composition versus environmental effects

As laid out in Section 3, the treatment effect of coeducation on women's major choices can be divided into two main channels: a composition effect, in which the transition alters STEM-inclined women's matriculation decisions, and an environmental effect, in which a stable population of women responds to the arrival of male classmates. To discern the implications of policies that manipulate gendered social environments from our estimated treatment effect, it is important to quantify the composition channel. For example, suppose the composition effect is large—i.e. the estimated decrease in STEM majoring at the transitioning college is due to STEM-inclined women choosing to enroll at other colleges. Then, it is unclear whether the large-scale adoption of a policy that mitigates gendered social environments would produce more woman STEM majors in aggregate, or would simply induce a reallocation of woman STEM majors across college campuses. On the other hand, if the composition effect is small, then the treatment effect we have estimated is informative about the aggregate effect of large-scale "de-gendering" policies.

This section sheds light on the composition effect in two ways. First, we test for changes in the average preparedness for STEM coursework among entering female freshman. To do so, we collapse our student-level data from TFS at the institution-by-year level and merge it to the HEGIS/IPEDS data.[23] We then investigate whether freshman women's average math SAT score and other baseline predictors of STEM majoring respond to coeducation. Second, we test whether STEM majoring at a current women's college responds positively to the share of its competitors that have transitioned to coeducation. If so, this would suggest that STEM-inclined women substitute to comparable women's colleges when their desired college transitions to coeducation.

6.1 STEM preparedness of entering freshmen

We start by using the linked TFS-IPEDS/HEGIS data to estimate Equation 2, with $m = 0$, with the average math SAT score of freshmen women at institution i as the outcome. Figure 8a reports the event study estimates. We fail to find a significant effect—the event study is somewhat noisy but displays no discernible trend break after coeducation. We conclude that the average math ability of freshman women at newly coeducational institutions did not decline in response to coeducation.

While ability is important, it is not the only predictor of students' success in STEM courses (e.g. Turner and Bowen, 1999; Arcidiacono, 2004; Card and Payne, 2017). Ac-

[23] We implement a 4-year-lag when merging so that we match the characteristics of an entering freshman cohort to their major choices as graduating seniors.

cordingly, we use a two-step procedure to evaluate the effect of coeducation on a more comprehensive measure of STEM preparedness of freshman women. First, we estimate

$$s^W_{STEM,i,t+4} = \gamma Z_{it} + \xi_{it} \tag{3}$$

on our linked dataset. In this equation, $s^W_{STEM,i,t+4}$ is the share of female graduates four years in the future who will earn STEM degrees, and Z_{it} is a vector of characteristics of female students, including average SAT Math and Verbal scores, proportion reporting high school GPAs in each of the A, B, and C ranges, average parental income and education levels, average student age, and the share of female freshmen of each race and religion.[24] We use our estimate of Equation 3 to fit $\hat{s}^W_{STEM,i,t}$: that is, the predicted share of freshman women who will graduate from institution i with a STEM degree based on the characteristics of the average female freshman.[25] In this analysis, we use all colleges as our comparison group due to the relatively small number of women's colleges in TFS; however, the results are similar when using women's colleges as the comparison group.[26]

We then estimate Equation 2 with $m = 4$ and $\hat{s}^W_{STEM,i,t}$ as the outcome. Because our outcome variable is itself an estimate that is subject to error, we calculate standard errors of this estimation using a percentile block bootstrap with 1,000 replications. Figure 8b reports the results of the two-step procedure. Though our estimates are imprecise, the event study is extremely flat, displaying no discernible trend break after coeducation. Difference-in-difference estimation finds that the transition to coeducation raised the predicted share of women who chose STEM majors by 0.06 percentage points — an estimate that is neither statistically nor economically significant.[27]

These exercises strongly suggest that the composition effect is trivial. Is this plausible? That is, if coeducation exerted such a strong environmental effect on women's major choices, should we not also expect women to anticipate these effects and adjust their enrollment decisions accordingly? Previous work suggests that students do not correctly anticipate their own abilities or the effects of the collegiate environment on their abilities when forming enrollment and majoring decisions (Zafar, 2011; Stange, 2012; Owen, 2020). Even among students that declare a STEM major, nearly half end up switching to another field (Altonji

[24] We do not include school, region-by-year, or institutional-control-by-year fixed effects in equation 3 because we want to include variation across institutions and time that might lead women who want to study STEM to choose different institutions than men do. However, we include those fixed effects and some additional controls in an alternative specification, which we display in Appendix Figure A8.

[25] $\hat{s}^W_{STEM,i,t}$ is highly correlated with $s^W_{STEM,i,t}$; see Appendix Figure A7.

[26] Results available upon request.

[27] Our augmented specification, presented in Appendix Figure A8, is much more precisely estimated. This specification displays a small and insignificant pre-trend followed by a similarly flat event study. The difference-in-difference estimate is almost 0.

et al., 2016). In addition, students' understanding of the value of college is relatively weak (Gong et al., 2019, 2020). These findings suggest that women may make enrollment decisions without full or accurate knowledge about the effects of (gendered) social environments on their major choices. On the other hand, if preferences for coeducational environments matter more than preferences for specific majors—or are uncorrelated with preferences for specific majors—women could rationally expect large effects of coeducation yet still select into coeducation in an unbiased manner.[28] We view our results as consistent with either of these classes of educational choice models.

6.2 Substitution of STEM majors to other women's colleges

Another indicator of a "composition effect" of the transition to coeducation would be a substitution to the remaining single-sex colleges among women interested in quantitative fields. To test for this possibility, we draw on our IPEDS/HEGIS dataset and estimate the following equation:

$$s^W_{STEM,i,t+2} = \theta_i + \rho CW_{it} \times Exposure_{it} + \delta_{r(i)t} + \psi_{c(i)t} + \varepsilon_{it} \tag{4}$$

This equation contains the same fixed controls as the main event study specification. The independent variable of interest is $Exposure_{it}$, which equals

$$\frac{\text{Number of comparable coeducational colleges}}{\text{Number of comparable coeducational and women-only colleges}}$$

where "comparable colleges" are colleges within the same Census region and 1972 Barron's selectivity band as college i. The indicator CW_{it} is 1 if the institution is currently women-only and 0 otherwise. Thus, the parameter ρ captures the extent to which women's STEM majoring increases at current women's colleges as other comparable women's colleges increasingly transition to coeducation, relative to control institutions. Because former women's colleges may be directly affected by their own transitions to coeducation, we exclude them from the control group and instead consider two specifications: one including only colleges that are currently women-only and one that includes current women's colleges and colleges that were coeducational through the entire sample period.

Our results are reported in Table 3. When considering only colleges that were women-only in a given year, we find a coefficient of -0.00064—that is, a ten percentage point decrease

[28] Appendix Figure A9 presents estimates of the effect of coeducation on freshman women's stated intents to major in various STEM fields. We estimate flat event studies for math and for physical sciences, although a moderate negative effect on plans to major in biological sciences (driven mostly by a pre-trend).

in the share of comparable colleges that were women-only led to a 0.006 percentage point increase in the share of women majoring in STEM at a given women's college. This effect is neither statistically nor economically significant. When including institutions that were always coeducational, which better controls for regional- and selectivity-driven trends in women's STEM majoring, we find a coefficient of -0.0064. That is, a ten percentage point increase in the share of comparable colleges which were coeducational led to a 0.06 percentage point decrease in women's STEM majoring. This estimate, though much larger than the corresponding estimate while including only current women's colleges, is also neither statistically nor economically significant, especially considering that each women's college in our sample had on average 8.5 peer institutions within a region-by-selectivity band in the first year they appeared in the data, with larger numbers of peers at less-selective institutions. This evidence further indicates that the composition effect, if it exists, is quite small.

7 Conclusion

Our paper takes advantage of a unique natural experiment in the history of the American higher education—the transitions of hundreds of women's colleges to coeducation at varying times during the 1960s-2000s—to isolate the contributions of gendered social environments to gender disparities in field choice. This analysis expands a literature that has emphasized the role of non-pecuniary factors on major choice, such as subjective beliefs and preferences, but has yet to explore how major choices actually respond to changes in non-pecuniary features of the environment.

Drawing on a newly assembled historical dataset, we estimate event study and synthetic control specifications that compare the evolution of women's major choices at newly coeducational colleges to those at comparable colleges that transitioned at different times (or did not transition at all). In the ten years following the junior year of the first coeducational class, we find that the share of women majoring in STEM fell by around 2.0 percentage points (24%) relative to control colleges. We also estimate negative effects of coeducation on women's economics and business major choices. Our analysis suggests that these reductions are primarily driven by greater exposure to male peers, rather than a decrease in opportunities to interact with female faculty role models. We also find no evidence of capacity constraints or shifting ability composition as underlying mechanisms.

What do our estimates imply about the role of gendered peer effects on the overall gender gap in STEM? We can gauge this with a simple exercise. According to our 2016 data, 28 percent of baccalaureate degrees awarded to men were in STEM fields, but only 11.5 percent of degrees awarded to women were in STEM fields. In addition, 57 percent of total degrees were awarded to women, so the average woman's potential peer group was 43 percent male. What would be the effect of reducing this number to zero? Figure 6 reports that a ten percentage point increase in the male share of the student body caused a 1.3 percentage point reduction in the share of women majoring in STEM. Thus, a 43 percentage point reduction would be associated with a $4.3 \cdot 1.3 = 5.6$ percentage point increase in the share of women majoring in STEM. This amounts to 34 percent of the 16.5 percentage point gap.

Of course, this counterfactual exercise must be qualified in several respects. Our sample of switchers and women's colleges primarily contain students who live on campus, which is less often true of students at 4-year colleges today. In addition, most coeducation events occurred in the 1960s-80s, when gender roles may have been more salient than they are today. These considerations suggest that our exercise overstates the aggregate role of gendered peer effects. On the other hand, the men who entered newly coeducational colleges were less likely than the average man to enter traditionally-male fields. This suggests that women at newly coeducational colleges faced less competition from men in STEM classrooms than would be predicted based on the male share of the student body—implying that our exercise understates the aggregate role of gendered peer effects.

In either case, our findings indicate that interaction with male peers increases the salience of traditional gender identities and aspirations, stereotype threat, aversion to competition, and marriage market considerations—and that these factors meaningfully contribute to gender disparities in college major choice. While a large-scale return to single-sex educational environments is likely undesirable, policies that infuse features of single-sex learning environments into coeducational settings may be effective in closing the persistent gender gap in major choice.

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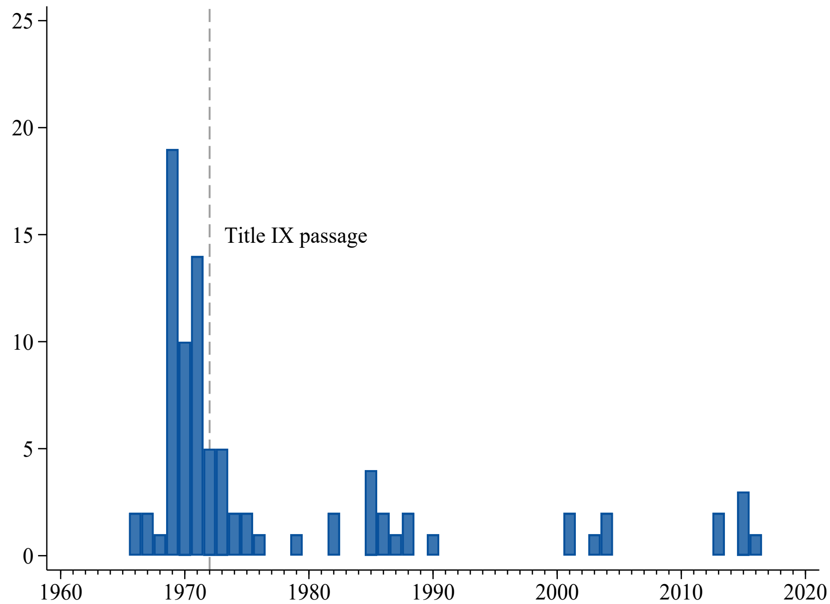
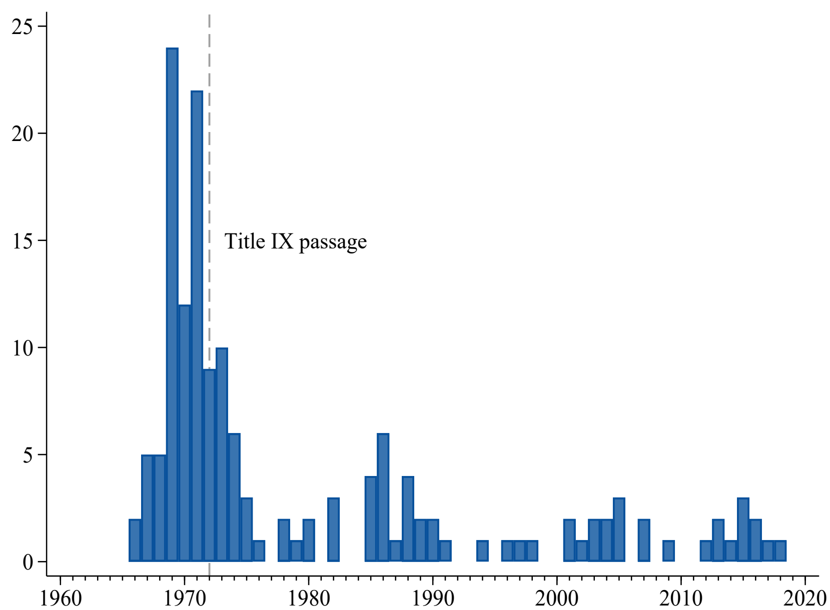
Figure 1: Distribution of the year of switch to coeducation

(a) Our sample

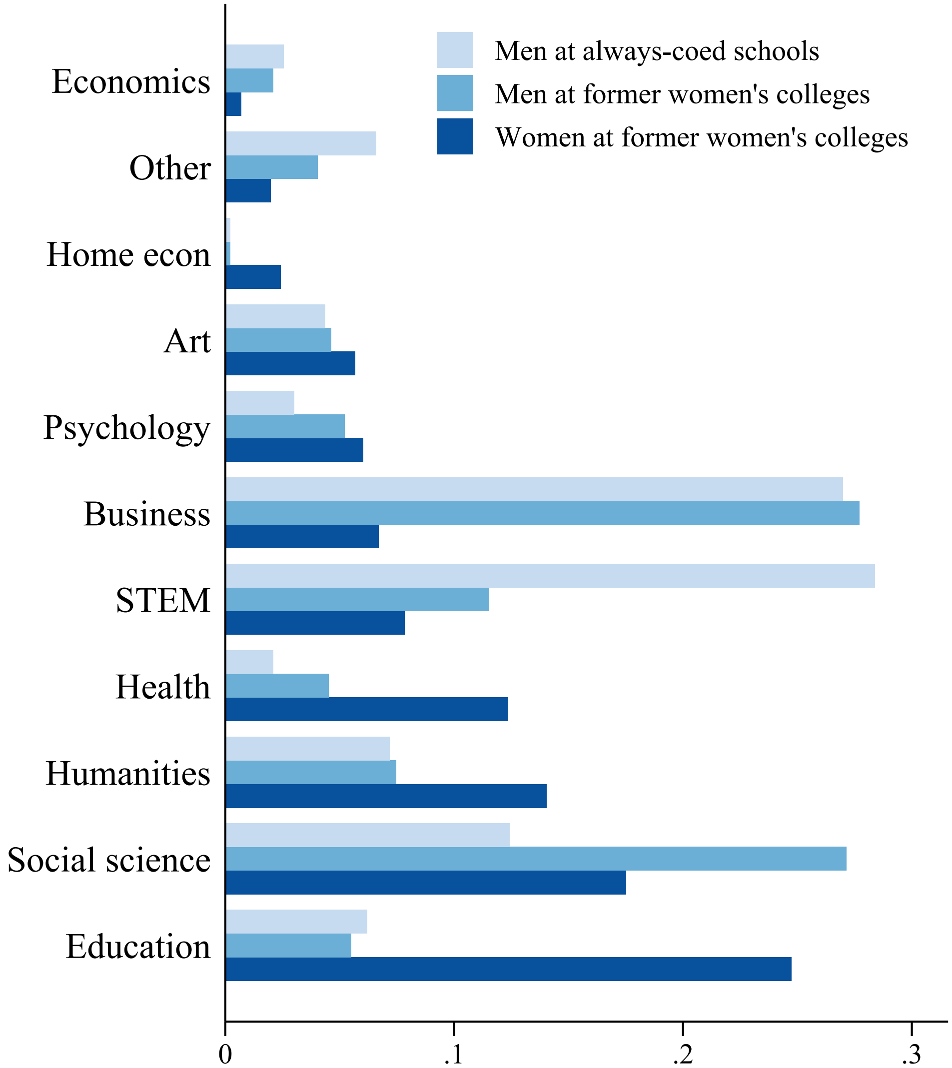


(b) All former women's colleges



Notes: Hand-collected data on the years that former women-only institutions switched to coeducation. See Section 4 for further background on how this list was compiled, and Appendix Table A1 for a comprehensive list of formerly women's colleges and sample inclusion criteria.

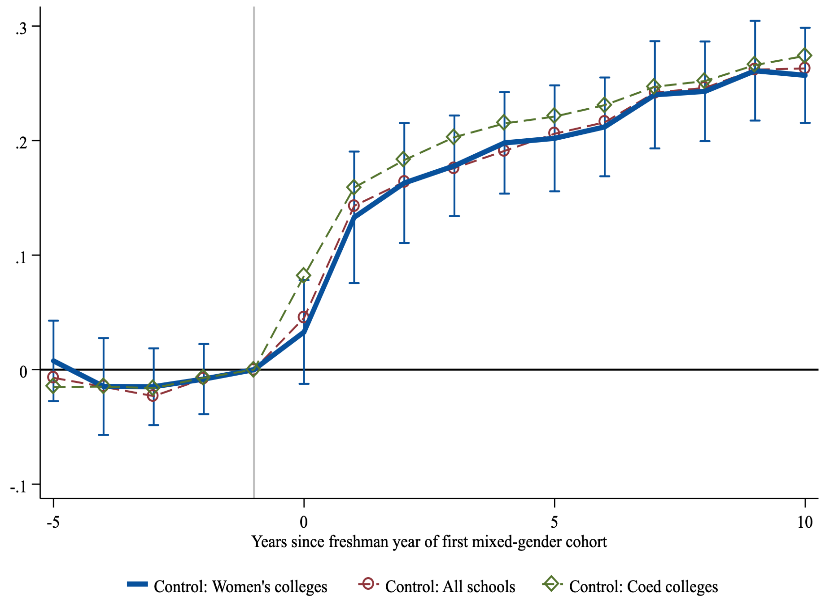
Figure 2: Field of major among students at coeducational and former women's colleges



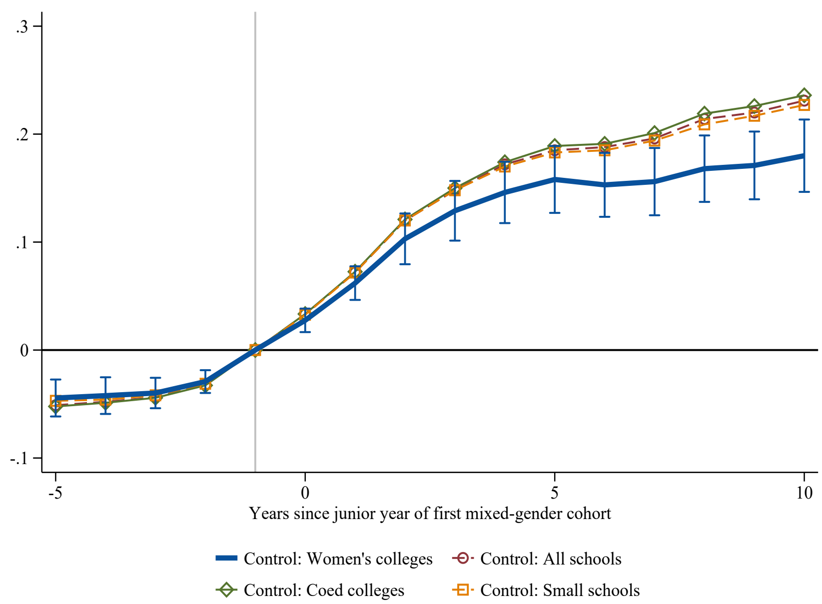
Notes: Data is drawn from records of the number of degrees awarded by year, institution, and gender from 1966 – 2016 in the Integrated Postsecondary Education Data System (IPEDS) and its predecessor, the Higher Education General Information Survey (HEGIS), linked to hand-collected data on the years that former women-only institutions switched to coeducation. Each bar shows the fraction of graduates of a given sex and college type earning degrees in the corresponding field. The distribution of majors among men at former women's colleges is calculated among men graduating in the first 10 years after the transition to coeducation. The distribution of majors among men at "always-coed" schools is calculated among men at schools that were coeducational throughout our sample period, weighted so that the distribution of years represented matches the distribution of years represented amogn men at former women's colleges. The distribution of majors among women is calculated among women graduating in the last 5 years before the transition to coeducation.

Figure 3: The effect of coeducation on the male share of the student body

(a) Share male among freshman class

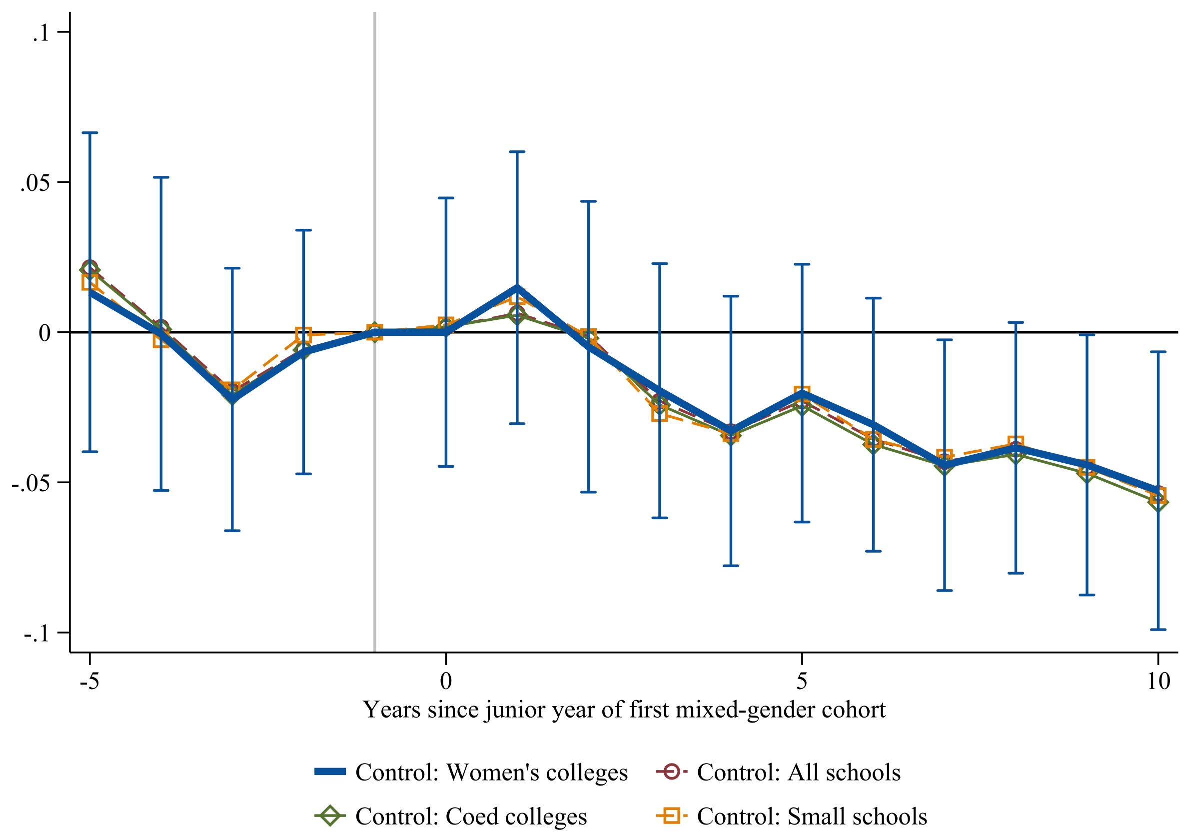


(b) Share of male degrees earned



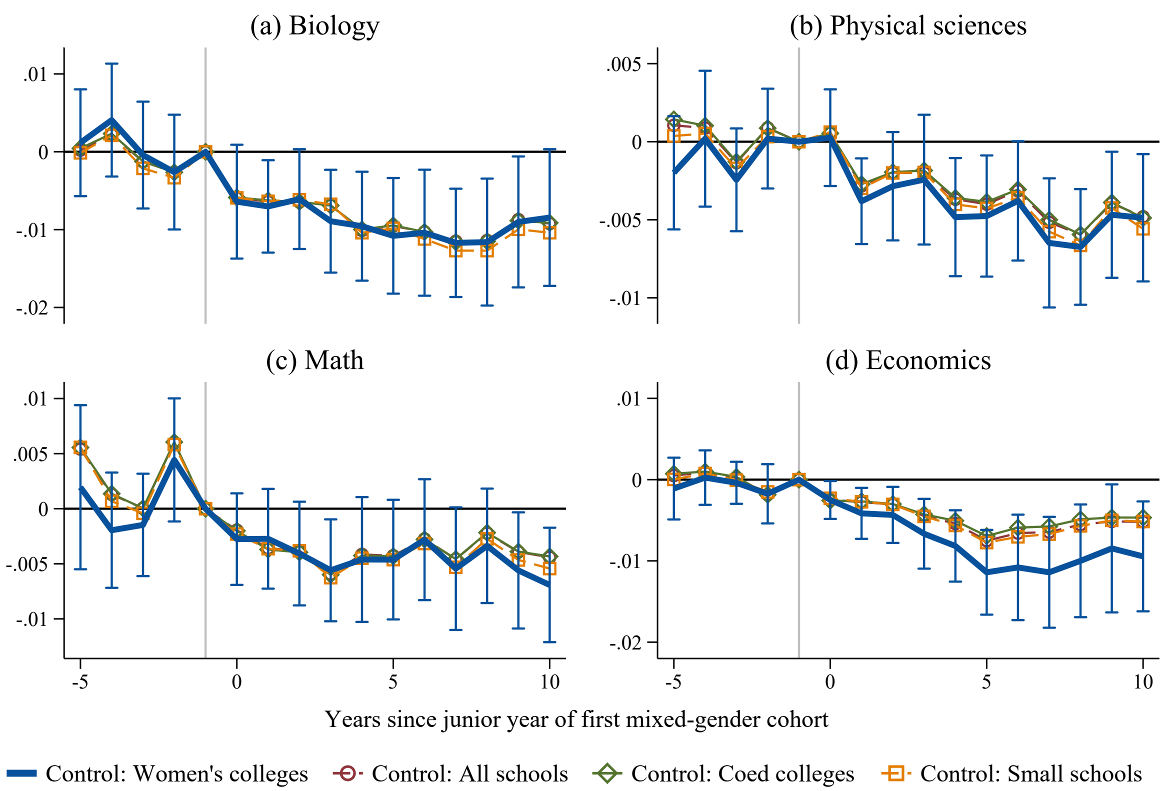
Notes: Panel A: Data drawn from the CIRP Freshman Survey, spanning 1966 to 2006, matched to hand-collected dates of transitions to coeducation by institution. Panel B: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, matched to hand-collected dates of transitions to coeducation by institution. See Section 4 for further detail. Panels display estimates of β_s from the modifications to equation 2 described in Section 5.2. Standard errors are clustered at the institution level.

Figure 4: The effect of coeducation on the female share of full-time faculty



Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1971-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 and Appendix Table A1 for further detail. The graph displays estimates of β_s from equation 2. Standard errors are clustered at the institution level.

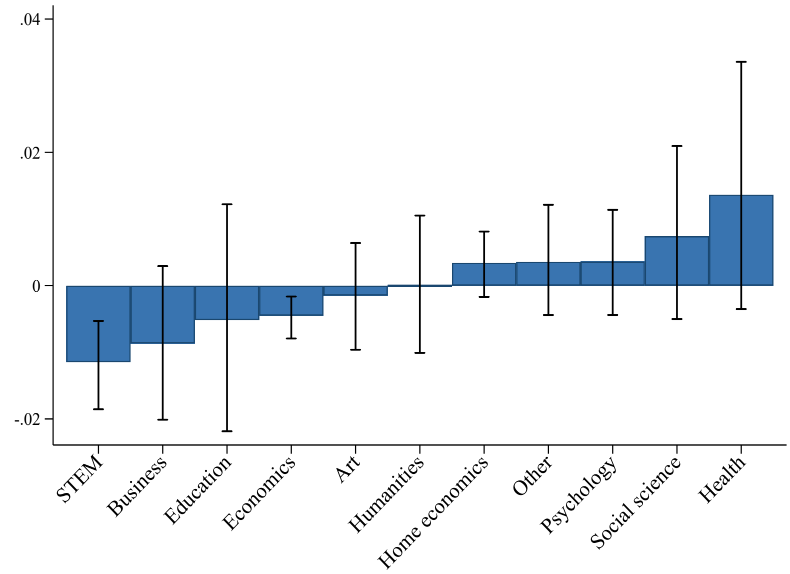
Figure 5: Field of major among students at coeducational and former women's colleges



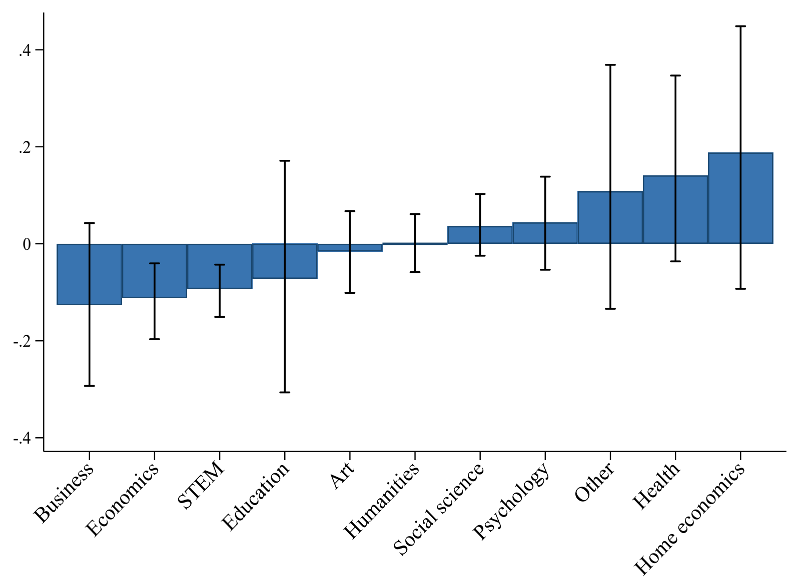
Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 and Appendix table A1 for further detail. Panels display estimate of β_s from equation 2. Standard errors are clustered at the institution level.

Figure 6: The effect of coeducation on the full distribution of women's major choices

(a) Percentage-point effects by major

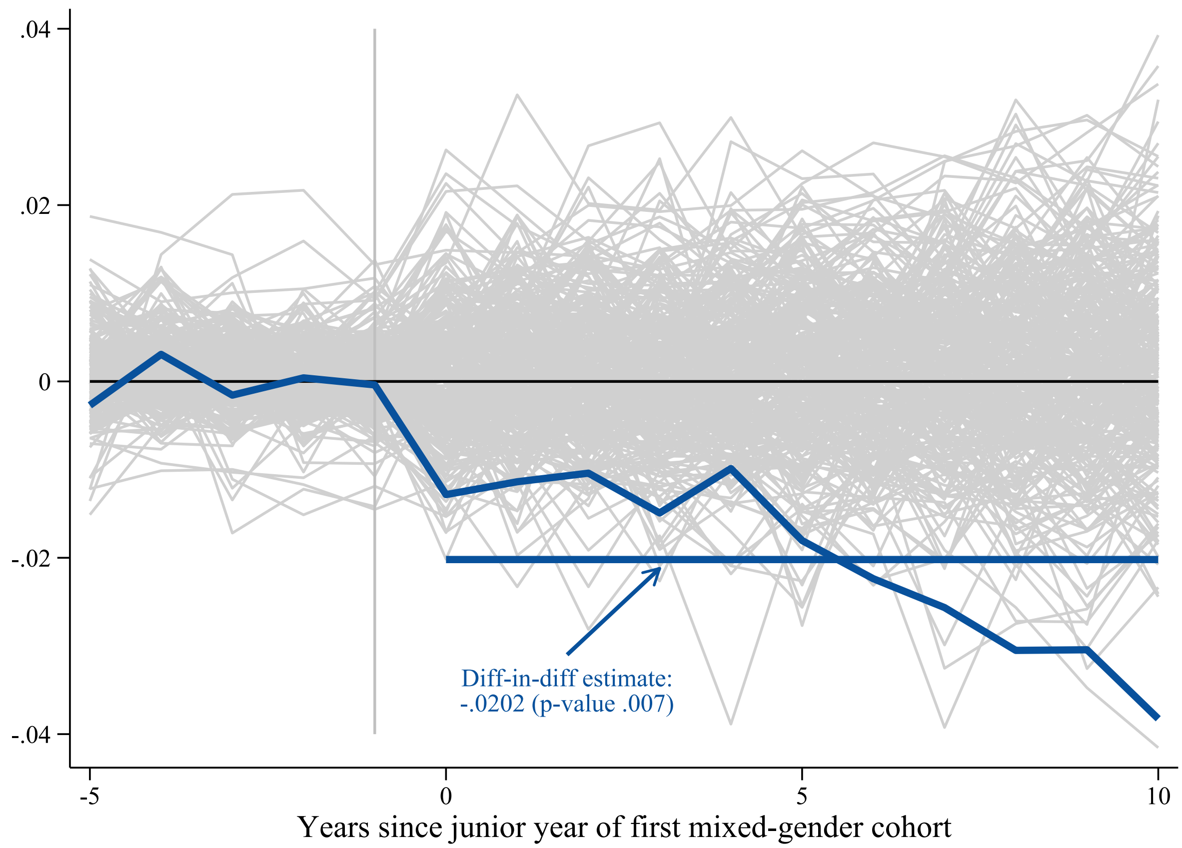


(b) Semi-elasticity by major



Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. The majors included in each concentration are described in Appendix B. Each panel displays estimates from a difference-in-difference version of Equation 2. Estimates are re-scaled so that they capture the majoring effect of a 10 percentage-point increase in the male share of the student body induced by coeducation. Confidence intervals account for error in estimation of the effect of coeducation on the male share of the student body via a block bootstrap routine. Specifically, the routine computes 1,000 re-scaled coefficients and reports confidence intervals corresponding to the 2.5th and 97.5th percentiles of this observed distribution. The top panel uses a percentage-point scale. The bottom panel reports semi-elasticities, which are constructed by dividing the re-scaled percentage-point estimates by the baseline shares of women majoring in each given concentration.

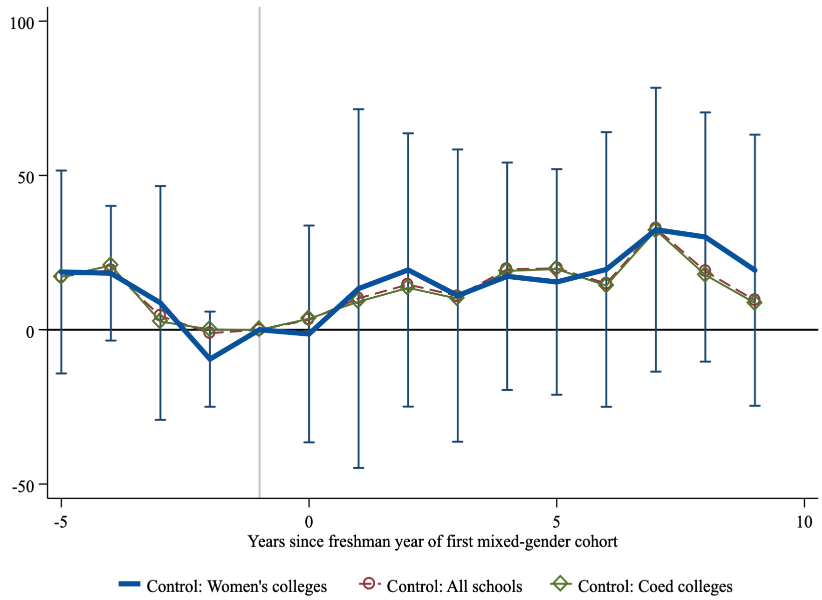
Figure 7: The effect of coeducation on the STEM share of degrees awarded to women: synthetic control specification



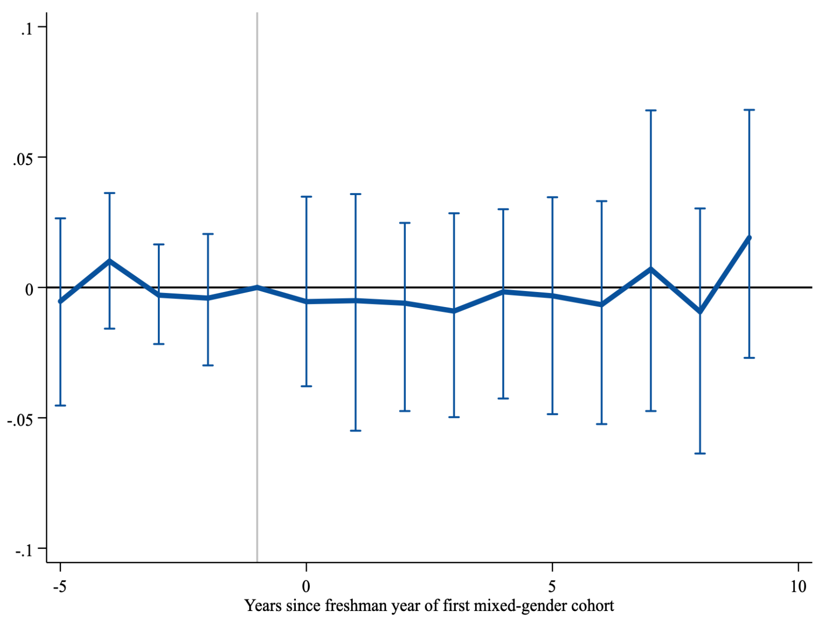
Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 for further detail. The majors included in the STEM concentration are described in Section 4 and Appendix B. See Section 5.5 for description of the synthetic controls procedure. Dark line reports the main estimate, while grey lines report the results of a randomization inference procedure with 300 replications.

Figure 8: Did coeducation induce a composition effect? Effects on freshman women's preparedness for STEM coursework

(a) Average math SAT scores among freshman women



(b) Estimated STEM preparedness among freshman women



Data drawn from the CIRP Freshman Survey (TFS), spanning 1966 to 2006, matched to hand-collected dates of transitions to coeducation by institution. The top panel considers average math SAT scores and presents estimates of β_s from equation 2. The bottom panel considers the average predicted propensity to major in STEM based on observed freshman characteristics (SAT scores, family background variables, high school course-taking and GPA, etc.). We predict this propensity by matching TFS data, at the institution-by-year level, to data drawn from HEGIS/IPEDS surveys on STEM degrees awarded. Then, we estimate equation 3 and predict STEM propensities at the institution-by-year level as fitted values from this estimation. To account for the fact that the left hand side is an estimate, standard errors are calculated using a block bootstrap with 1,000 repetitions.

Table 1: Summary statistics

	Switchers	Women's colleges	All non-switchers	Coed schools	Small schools
Share of women's degrees in STEM	0.12	0.12	0.07	0.07	0.08
	(0.04)	(0.04)	(0.05)	(0.04)	(0.06)
Total enrollment	1729.37	1609.61	11881.25	12143.32	2231.75
	(1305.10)	(699.06)	(10461.56)	(10462.53)	(1352.69)
Female share of degrees awarded	0.99	1.00	0.48	0.47	0.54
	(0.04)	(0.01)	(0.15)	(0.12)	(0.18)
Graduate degrees awarded	17.96	17.26	561.01	574.88	66.96
	(32.39)	(37.73)	(824.41)	(830.20)	(167.29)
Private school indicator	0.78	1.00	0.32	0.30	0.65
	(0.42)	(0.00)	(0.47)	(0.46)	(0.48)
Catholic school	0.47	0.33	0.05	0.04	0.10
	(0.50)	(0.48)	(0.21)	(0.20)	(0.29)
Selective admission	0.25	0.52	0.21	0.21	0.20
	(0.43)	(0.51)	(0.41)	(0.40)	(0.40)
Observations	87	27	948	921	667

Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 for further detail. Share of women in STEM, total enrollment, female share of graduate degrees, graduate degrees awarded, and private school indicator measured in first year observed in the data. Catholic affiliation is coded as 1 if the school was ever affiliated with the Catholic Church. Schools are coded as having selective admission if they received a Barron's rating of 1, 2, or 3 in 1972. The majors included in the STEM concentration are described in Section 4 and Appendix B. Sample statistics are weighted by the number of degrees awarded to women.

Table 2: Determinants of the transition to coeducation

	(1) Switched	(2) Switched	(3) Switched before 1972	(4) Switched before 1972
Region = Midwest	-0.0246	0.0565	-0.572***	-0.484***
	(0.130)	(0.127)	(0.163)	(0.177)
Region = South	0.156*	0.195**	-0.377***	-0.283**
	(0.0937)	(0.0909)	(0.128)	(0.131)
Region = West	-0.143	-0.172	-0.376**	-0.471***
	(0.136)	(0.135)	(0.177)	(0.167)
Ever Catholic	0.191*	0.108	-0.174	-0.333***
	(0.101)	(0.117)	(0.128)	(0.111)
Log enrollment	0.00456	0.00734	-0.0702	-0.0723
	(0.0901)	(0.0859)	(0.104)	(0.0973)
Selective		-0.219*		-0.317**
		(0.116)		(0.155)
Observations	112	110	85	83
R-squared	0.122	0.153	0.169	0.218

Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. Linear probability model estimates based on transitioning to coeducation (for all initial women's colleges) or transitioning to coeducation before 1972 (for all switchers) as the dependent variable. Robust standard errors in parentheses. Region refers to U.S. Census regions. Omitted region category is Northeast. "Selective" refers to a Barron's rating of 1, 2, or 3 in 1972.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Spillover effects analysis: the effect of coeducation on STEM majoring at comparable colleges remaining women-only

	Current women's colleges	Current women's + always coed
$CW_{it} \times Exposure_{it}$	-0.00064	-0.00639
	(0.0187)	(0.0218)
Observations	2,313	47,297
R-squared	0.738	0.721
Colleges	112	1,027

Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 for further detail. The majors included in the STEM concentration are described in Section 4 and Appendix B. The table presents estimates of the effect of exposure to coeducational colleges within the same region and selectivity band on the share of women graduating from women's colleges with STEM degrees. See Section 6 for further detail. Standard errors are clustered at the institution level. A small number of institutions were dropped due to missing Barron's selectivity ratings.
*** p<0.01, ** p<0.05, * p<0.1

A A formal model of the effect of coeducation on women's STEM majoring

We use a very simple Roy model of college and major choice to illustrate the possible effects of transition to co-education on subsequent women's outcomes. We assume there are 3 collegiate institutions in the market: h, j and k. There are two time periods: 0 and 1, which are separated by a substantial number of years. At $t = 0$, institutions h and j are women-only while k is co-educational. Between $t = 0$ and $t = 1$, institution j transitions to co-education. All institutions in each time period offer two majors: STEM (S) and non-STEM (NS). We assume away capacity constraints. (In Section E we show that evidence consistent with this assumption.)

Each time period consists of two stages. In the first stage, women make enrollment decisions η under uncertainty about the values of attending each college. In the second stage, women who have chosen to enroll in a college choose a major μ in which to graduate, with full information about major-specific payoffs. We assume that every woman enrolls in college, and that every woman who starts college completes a degree at her starting institution.

Consider a hypothetical high school senior w making decisions in period t. A given enrollment choice η_{wt} returns the expected payoff $V_{wt}(\eta_{wt})$. She chooses the enrollment choice η^*_{wt} that maximizes this function:

$$V_{wt}(\eta^*_{wt}) = \max \left\{ V_{wt}(h), V_{wt}(j), V_{wt}(k) \right\}. \tag{5}$$

After making her enrollment choice, woman w realizes her major-specific payoffs and chooses her major μ_{wt}. We represent her payoff from choosing major μ at institution η as $v_{wt}(\mu_{wt}; \eta)$. Her major choice μ^*_{wt} thus satisfies:

$$v_{wt}(\mu^*_{wt}; \eta) = \max \left\{ v_{wt}(S; \eta), v_{wt}(NS; \eta) \right\}, \ \eta \in \{h, j, k\}. \tag{6}$$

Woman w's expected payoff from enrolling at institution η is simply equal to the expected payoff from choosing her most-preferred major at η:

$$V_{wt}(\eta) = E \left[v_{wt}(\mu^*_{wt}; \eta) \right] \tag{7}$$

Assume there are many women w in the market with varying preferences for colleges and majors. Consider the students who chose to enroll at women's institution j in period t. Denote each enrolled woman as belonging to the set A_{jt}. The share of this student body graduating from h with a STEM degree is given by $s_{STEM,jt}$:

$$s_{STEM,jt} = \frac{\sum_{w \in A_{jt}} \mathbf{1}\left\{S = \text{argmax}\left\{v_{wt}(S;j), v_{wt}(NS;j)\right\}\right\}}{\sum_{w} \mathbf{1}\left\{j = \text{argmax}\left\{V_{wt}(h), V_{wt}(j), V_{wt}(k)\right\}\right\}} \tag{8}$$

Suppose that, aside from institution j transitioning to co-education, nothing else changes between periods 0 and 1. Then, the object

$$\Delta = s_{STEM,j1} - s_{STEM,j0}$$

describes the treatment effect of co-education on the production of women STEM majors at institution j.

Two channels determine Δ. First, suppose that the set of women enrolling at institution j, A_j, does not change between time periods 0 and 1. Then, Δ simply depends on how the transition to co-education alters the payoffs to majoring in STEM ($v_w(S;j)$), relative to majoring in non-STEM ($v_w(N;j)$), for this population of women. We call this the "environmental effect." See Section 3.2 for a discussion of the various channels determining this effect.

Second, the transition to co-education might induce a change in the enrolled set of students A_j. To see why this might be the case, plug (7) into (5) and re-express the optimal enrollment decision:

$$\eta_{wt} = \text{argmax}\left\{E\left[v_{wt}(\mu^*_{wt};h)\right], E\left[v_{wt}(\mu^*_{wt};j)\right], E\left[v_{wt}(\mu^*_{wt};k)\right]\right\} \tag{9}$$

That is, women forecast their (major-specific) payoffs from attending each institution, and use those expectations to guide their enrollment decisions. When institution h transitions to co-education, the women that strongly desire a single-sex environment may experience a reduction in $E\left[v_{wt}(\mu^*_{wt};j)\right]$ and may substitute from j to women's college h. Additionally, the women that strongly desire a co-educational environment may experience an improvement in $E\left[v_{wt}(\mu^*_{wt};h)\right]$, and may substitute from co-educational college k to j. If the women who most desire a single-gender environment also have the highest expected payoffs from majoring in STEM (say, because they are the most prepared for STEM coursework), then j's transition to co-education causes its subsequent population of women to become more negatively selected on expected STEM payoffs: plausibly leading to a reduction in STEM majoring. We call this channel the "composition effect."

In Section 5, we estimate the overall treatment effect Δ. Because the assumption that nothing else about the collegiate environment changes between periods 0 and 1 is likely false, we apply difference-in-difference methodologies to estimate Δ. That is, we compare the evolution of women's major choices at colleges that transitioned to coeducation to the evolution of major choices at comparable colleges that did not transition. Section 6 attempts to decompose Δ into composition versus environmental effects.

B Major codes

B.1 Coding scheme and crosswalks

This paper uses consistent 4-digit, 2-digit, and grouped 2-digit versions of major codes. The consistent coding scheme is based on the 1990 version of the Classification of Instructional Programs (CIP) from the National Center from Education Statistics (NCES).

Codes to describe college majors have been revised several times over our sample period. There were two sets of major codes in the HEGIS data, with a revision in 1970, and coding switched to the CIP in the early 1980s.[29] Revisions of the CIP occurred in 1985, 1990, 2000, and 2010.[30] Crosswalks between the 1970s HEGIS codes and the CIP, and between different versions of the CIP, are available from NCES, but they are not complete.

Similar to occupation codes, the CIP has 2-, 4-, and 6-digit versions of codes, while the HEGIS codes have only 2- and 4-digit versions. Revisions of the CIP only rarely move major categories across 2-digit codes,[31] though the 1990, 2000, 2010 revisions did move, split, and combine some two-digit codes.[32]

For this paper, all 6-digit codes were crosswalked to the 4-digit 1990 CIP. Where crosswalks provided by the NCES were incomplete, they were supplemented by lists and descriptions of CIP codes created by the NCES. When majors were not included in the NCES crosswalks, they were matched to the major of the most similar title and description in the 1990 CIP. If two 4-digit codes were combined in any version of major codings after 1970, they were combined in the consistent coding scheme. The same is true for the 2-digit codes. Six-digit majors that were created or deleted at any point were assigned to the same 4-digit code in the "other" category, and 4-digit codes that were ever created or deleted were as-

[29] The first version of the CIP was constructed in 1980, but HEGIS seems not to have adopted it until 1983.

[30] There seems to have been late adoption of the new coding schemes in the IPEDS data – the switches seem to have occurred in 1987, 1992, 2002, and 2012, and may not have occurred uniformly across schools. Revisions of the CIP vary in how many changes were made, with the 1985 revision being much smaller than subsequent revisions.

[31] Exceptions include clinical versions of the life sciences, materials science, and educational psychology, all of which could be considered to be part of multiple two-digit codes.

[32] For instance, the 1990 revision of the CIP combined category 17, Allied Health, with category 18, Health Sciences, into category 51, Health Professions and Related Sciences. Most of the 4-digit categories were preserved but re-numbered in the revision.

signed the the 4-digit code for "other" within the same 2-digit code.[33] Four-digit majors with fewer than 950 school-by-year observations were combined with majors that cover similar material[34] or with the "other" category within their two-digit code. Smaller 2-digit codes, such as Law, Library Science, and Military Science, were treated as a single 4-digit code.

For the main result, majors were combined into groups of 2-digit codes, with the most important of those groups being STEM. STEM in this case includes the 2-digit codes for Life Sciences, Physical Sciences, Engineering, Computer Science, and Mathematics. Alternative specifications also included Health Professions.

B.2 Categories of majors

The following list is the two-digit categories of majors in each group of 2-digit codes. Groups are in bold and the two-digit categories are listed afterward. Where the two-digit sets of codes are not informative, four-digit codes are included in parentheses. Some groups contain only one two-digit code. The "other" group includes majors that generally cannot be found at small liberal arts colleges or that are generally very small.

Art Visual and performing arts, architecture and related services

Business Business, marketing

Education All education fields (including math education)

Economics Economics (4-digit code)

Health Health professions and clinical services

Home Economics Home economics/family and consumer sciences

Humanities Area and group studies (e.g. gender studies, Hispanic Studies), English, foreign languages and linguistics, philosophy and religious studies

Psychology Psychology

Other Social Sciences Social sciences except economics (general social science, anthropology, criminology, demography, geography, history, international relations, political science, social science, urban studies), communications

[33] For instance, African Languages were not included in the 1990 CIP and were therefore assigned to the 4-digit code for Other Foreign Languages.

[34] For instance, Architectural Engineering and Civil Engineering, Business Administration and Enterprise Management, and the health categories such as medicine, dentistry, and others which require a professional degree.

STEM Life sciences, physical sciences, mathematics and statistics, computer and information science, engineering, engineering technology, science technology

Other Agriculture, forestry, law, trades/vocational, military science, library science, multi- and inter-disciplinary, theology and religious vocations, protective services, public administration and social services

C School Codes

NCES uses two different coding schemes for individual schools at different points in the data. HEGIS identifies schools using FICE codes, which is a six-digit identification code assigned to schools doing business with the Office of Education in the 1960s. IPEDS uses the UnitID, which is also a six-digit code. Our data uses the FICE as a consistent identifier throughout the survey, with some modifications as detailed below.

Not every institution has a FICE code. Institutions that do not have a FICE code are those that entered the IPEDS data after the Institutional Characteristics file stopped listing FICE codes (which was during the 1990s). We drop those institutions from our sample, as according to the ICPSR files for IPEDS financial characteristics between 1988 and 1990, institutions that entered the sample after the beginning of the IPEDS have a much lower response rate than institutions in the HEGIS sample. However, the data set itself has the UnitID entered in place of the FICE code for those institutions.

Some institutions have multiple FICE codes. In most of these cases, a public institution originally reported all branches under one observation, and then switched to reporting each branch separately. The vast majority of cases where all degrees awarded are reported under the main campus occur in 1966, with a few additional cases between 1967 and 1969. We do not link such cases together. In other cases, an institution switched FICE codes in the middle of the sample. We are generally not sure why this occurs. We do link these cases together so that we have a single FICE code for all years the institution was in the data. Finally, there are a few institutions (notably Cornell and Columbia) with several different administrative units that separately report degrees awarded to IPEDS and HEGIS. We treat these institutions as a single observation and collapse them to a single FICE code.

Some FICE codes apply to multiple institutions. In these cases, all institutions are part of the same system, and the majority of these cases occur among institutions who enter the data in 1987 and later, especially among for-profit institutions with multiple campuses nationwide (e.g. the University of Phoenix). There are some cases where a public college with several branches (e.g. the University of Pittsburgh) reported degrees separately from each branch but reported the same FICE from each school. Where we could, we assigned these institutions to separate codes for each branch, but the rest of them are collapsed to the FICE level. We have also dropped schools that are ever classified as for-profit schools from our sample, which removes many of these cases from our analysis.

D Years of the switch to coeducation

D.1 Data collection

We define the first year of coeducation as the first year that men were admitted to traditional four-year undergraduate programs with coeducational courses. Schools where men were admitted to these programs only as commuter students are counted as coeducational, but schools where men could only participate in evening or adult education classes or graduate programs are not. We exclude all coordinate institutions, that is, institutions such as Columbia and Barnard where a men's college and a women's college share a campus and allow cross-registration in classes. We also exclude cases where a women's college merged with a men's college.

We sourced the years that single-sex institutions switched to coeducation in three different ways. The first source of information was a comprehensive check of the top 120 liberal arts colleges and the top 80 universities in the 2018 *U.S. News and World Report* for the gender of the student body in 1966 and a date of switch to coeducation. The second source of information was a list of current and former women's colleges from the Women's College Coalition, including a date of switch to coeducation. Finally, we generated a list of institutions that awarded more than 90% of their degrees to women in the first year they appeared in the data and used a research assistant to track down which of those institutions are current or former women's colleges. The RA also found the date of the switch to coeducation for former women's colleges. The three lists were then compared. Institutions that appeared on multiple lists with matching switch dates were considered confirmed. Institutions with conflicts between the switch dates or that appeared on only one list were independently verified.

Our classification of the "gender" of an institution is based on the gender of the student body in 1966. Institutions that did not appear on any of the lists noted above were assigned a gender based on the HEGIS or IPEDS "sex code" variable in the first year they appeared in the data.

In total, we found 136 former women's colleges that switched to coeducation ("switchers"). Of these, 118 were never coordinate, entered the data before 1987, did not merge with a men's college, and were never for-profit.

D.2 Problems in the data

Sixty-four of our "switchers" had at least one man graduate before the official date of the full switch to coeducation. In the vast majority of cases, this seems to have been either occasional one-off male students or the introduction of a small coeducational adult education program. In other cases, schools either opened a men-only college on campus or had male commuter students before completely switching to coeducation. We denote problematic cases with the following flags:

1. A small number of men (≤ 10 per year) graduate from the institution before the switch to coeducation, or > 10 men graduate from the institution before the switch and we can verify the existence of a coeducational adult education program that most likely did not interact with traditional undergraduates ($n = 46$)

2. A large number of men (> 10 per year) graduate from the institution before the switch to coeducation, and we cannot verify the existence of a coeducational adult education program that did not interact with traditional undergraduates. ($n = 10$)

3. Men were allowed as commuter students long before the official date of coeducation. We were sometimes, but not always, able to identify the true date that men were allowed to register in traditional undergraduate coursework. ($n = 8$)

4. Rather than becoming fully coeducational, the school opened a men's college (or men-only program) on campus – basically becoming coordinate rather than coeducational. ($n = 2$)

We exclude flags 2 through 4 from the main specification. We also exclude institutions that awarded zero bachelor's degrees in STEM fields to women in 1966 and institutions that closed shortly after a switch to coeducation. These cuts leaves us with 73 switchers in the final sample.

D.3 Defining our sample

Our sample includes institutions that:

- Were woman-only or coeducational in 1966 and entered the data in 1987 or earlier[35] ($N = 1,876$; $N_{switch} = 135$)[36]

- If switched to coeducation, did not close 9 years after the switch to coeducation (but not omitting schools that switched to coeducation in 2007 or later); if untreated, was in data for at least 15 years ($N = 1,571$; $N_{switch} = 123$)

- Had at least one woman complete a STEM (life or physical science, math, engineering, computer science) degree in 1966 ($N = 1,065$; $N_{switch} = 102$)

- Were never classified as a coordinate institution (a women's college sharing a campus with a men's college)/were not part of a merger with a men's college after 1966 ($N = 1,047$; $N_{switch} = 93$)

- Were never a for-profit institution ($N = 1,040$; $N_{switch} = 93$)

- Had fewer than 10 male students per year graduate before coeducation started or had coeducational adult education program that we could verify dates of existence for – that is, did not have a flag of 2, 3, or 4 ($N = 1,013$; $N_{switch} = 79$)

E Testing for capacity constraints

One possible explanation for our finding of a reduction in the share of women majoring in STEM at newly coeducational colleges is that such colleges hit capacity constraints to a larger extent in STEM than in non-STEM fields. For example, if STEM is both more costly for colleges to provide and more popular among men, students might be more crowded out of STEM majors than non-STEM majors after the transition to coeducation. To test this hypothesis, we examine how the total number of degrees awarded in each STEM-related field changed, and whether these changes were correlated with the cost of each STEM field. If capacity constraints were important, we should expect an inverse correlation between the expense to colleges of offering different fields and the growth in those fields after the transition to coeducation.

[35] ICPSR has concerns about the accuracy of imputation for nonresponses beginning with the switch to the IPEDS data in 1987. The IPEDS data dramatically expanded the sample to include schools that had not been classified as "institutions of higher learning" under Title IX and the response rate of those new institutions was much lower than the response rate of institutions included in HEGIS. See the ICPSR documentation of the 1986-1987 academic year finance data for further details.

[36] Note that the sample sizes here are meant to note how each subsequent restriction decreases the size of the sample, and different restrictions can overlap.

For each of several candidate majors, we estimate a difference-in-difference version of Equation 2:

$$\log(Degrees)_{it\mu} = \theta_{i\mu} + W_i Post_{it}\gamma_\mu + \delta_{r(i)t\mu} + \psi_{c(i)t\mu} + \varepsilon_{it\mu}. \tag{10}$$

$\log(Degrees)_{it\mu}$ is the log total number of degrees awarded in major μ to (male and female) students graduating from school i in year t. Our coefficient of interest is γ_μ: the effect of the switch to coeducation on the log number of degrees awarded in field μ. As before, W_i is an indicator of being a women's college that switched to coeducation. $Post_{it}$ is an indicator that school i has already switched to coeducation. $\theta_{i\mu}$, $\delta_{r(i)t\mu}$, and $\psi_{c(i)t\mu}$ are year, region-by-year, and control-by-year fixed effects, similar to our event study specification. We then rank the size of γ_μ in order of the cost of providing each field as described in Hemelt et al. (2018).[37]

The most expensive field commonly offered at the institutions in our sample is nursing, followed by education. The most expensive STEM field is physics, followed by chemistry, biology, economics, and math.[38] Figure A6 reports the estimates of γ_μ for each of these fields, in order from most expensive to least expensive. If capacity constraints were the only factor responsible for changes in the share of women choosing each field, we would expect that the most expensive fields would grow the least and the least expensive fields would grow the most. In that case, the difference-in-difference coefficients would get larger (less negative or more positive) as the cost of offering the field decreased. This is not the case: there does not appear to be any relationship between cost and the change in the log number of degrees awarded by field. We therefore conclude that capacity constraints were not the only driver of women's substitution away from STEM after the transition to coeducation at former women's colleges.

F Tables and Figures

[37] Hemelt et al. (2018) rank fields of study by cost relative to the English major. They establish that costs vary widely by field, and interestingly, both math and economics are cheaper to offer than English (and many other non-STEM majors). The variance in costs of offering different fields is largely explained by differences in class size and faculty pay — economics and math are cheaper because they often have very large classes.

[38] Nursing is the second most expensive field ranked by Hemelt et al. (2018), and education is the fourth most expensive. The two most expensive STEM fields are electrical engineering (which is the most expensive field) and mechanical engineering (which is the third most expensive field). The vast majority of colleges in our sample do not offer engineering, so we omit electrical and mechanical engineering from our analysis. Computer science is also more expensive than physics. However, most of our sample switched to coeducation in the early 1970s, when computer science was just beginning to be offered as a degree (and before most small liberal arts colleges would have had a computer science program). Given the huge changes to the field of computer science, we are not confident that its place in the rank order of costs has been stable since 1966.

Table A1: Average share of women choosing each major at time -1

Field	Share of women
STEM	0.086
	(0.046)
Biological sciences	0.040
	(0.026)
Physical sciences	0.012
	(0.020)
Chemistry	0.011
	(0.019)
Physics	0.001
	(0.002)
Math and Computer Science	0.031
	(0.026)
Math and Statistics	0.029
	(0.020)
Computer Science	0.002
	(0.015)
Engineering	0.000
	(0.000)
Social Sciences	0.162
	(0.104)
Economics	0.007
	(0.018)
Business	0.060
	(0.112)
Health	0.123
	(0.176)
Education	0.244
	(0.205)
Humanities	0.137
	(0.095)
Psychology	0.060
	(0.074)

Notes: Table shows the average share of women choosing STEM, each STEM major, and economics at time -1 at schools in switcher sample. Math includes statistics and applied math. Physical sciences includes chemistry, physics, astronomy, and earth sciences. Computer science includes information science. We exclude baccalaureate programs in agriculture, architecture, law, library science, military science, multi/interdisciplinary, public administration, vocational and technical fields, and visual and performing arts from this table, but those students are included in the denominator.

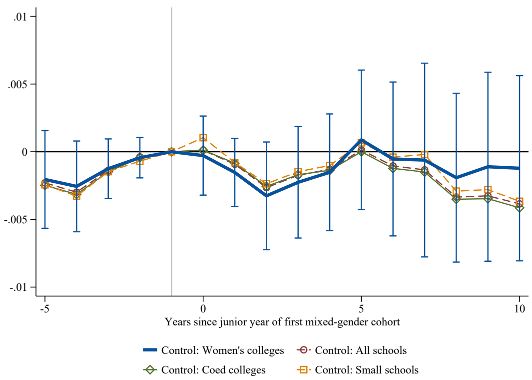
Table A2: Difference-in-difference coefficients for the effect of coeducation on the share of women choosing STEM and economics majors

	(1) STEM	(2) Biology	(3) Physical Science	(4) Math	(5) Economics
		Panel A: Control = Women's Colleges			
DiD	-0.0180***	-0.00945***	-0.00322***	-0.00497***	-0.00705***
	(0.00479)	(0.00271)	(0.00103)	(0.00185)	(0.00244)
Observations	5,675	5,675	5,675	5,675	5,675
R-squared	0.690	0.631	0.660	0.563	0.781
Time −1 mean	0.0804	0.0431	0.0108	0.0168	0.00913
		Panel B: Control = All Schools			
DiD	-0.0228***	-0.00832***	-0.00357***	-0.00614***	-0.00471**
	(0.00412)	(0.00234)	(0.000892)	(0.00190)	(0.00203)
Observations	50,558	50,558	50,558	50,558	50,558
R-squared	0.714	0.662	0.546	0.525	0.688
Time −1 mean	0.0804	0.0431	0.0108	0.0168	0.00913
		Panel C: Control = Coeducational Colleges			
DiD	-0.0233***	-0.00837***	-0.00361***	-0.00623***	-0.00442**
	(0.00415)	(0.00235)	(0.000897)	(0.00191)	(0.00205)
Observations	49,245	49,245	49,245	49,245	49,245
R-squared	0.712	0.659	0.521	0.523	0.649
Time −1 mean	0.0804	0.0431	0.0108	0.0168	0.00913
		Panel D: Control = Small Schools			
DiD	-0.0211***	-0.00833***	-0.00352***	-0.00650***	-0.00475**
	(0.00423)	(0.00245)	(0.000935)	(0.00179)	(0.00217)
Observations	36,548	36,548	36,548	36,548	36,548
R-squared	0.667	0.603	0.542	0.472	0.702
Time −1 mean	0.0816	0.0436	0.0111	0.0172	0.00940

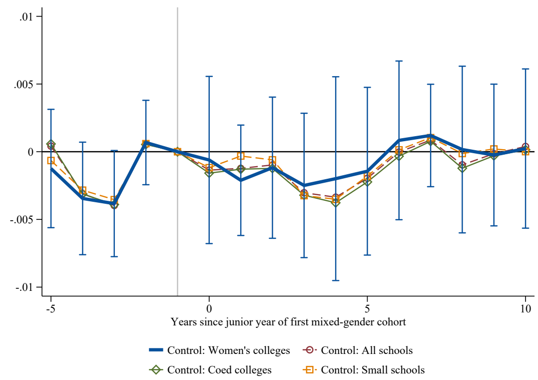
Notes: Table reports difference-in-difference coefficients corresponding to event study estimates from equation 2 and Figures 5 and A2a. Robust standard errors in parenthesis are clustered at the institution level. Time −1 mean refers to the mean share of women choosing each major at event time −1 in switcher institutions. Small schools refers to total enrollment under 5,000 students in the first year information on enrollment is available from HEGIS/IPEDS; the sample cut affects switcher schools as well as control schools.
$*p < 0.1; **p < 0.05; ***p < 0.01$

Figure A1: Tests for coinciding labor-market shocks

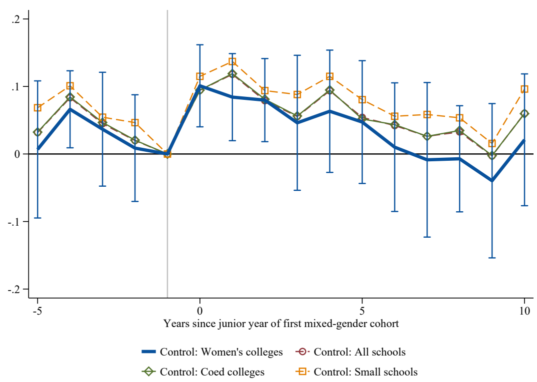
(a) Unemployment rate



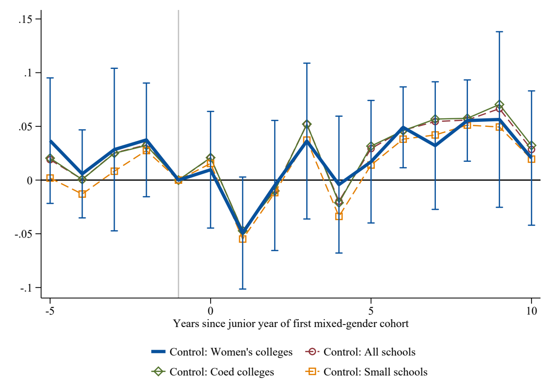
(b) Relative STEM employment



(c) Relative earnings, men in STEM jobs



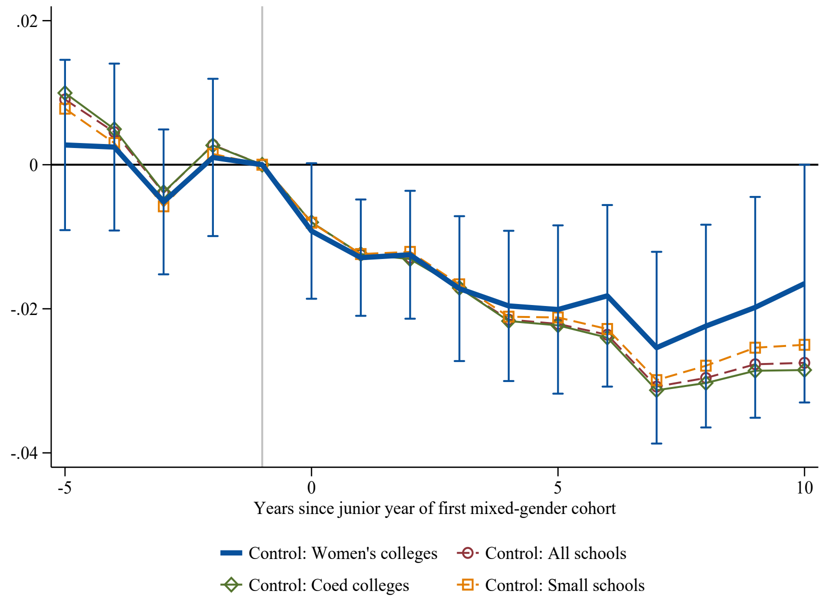
(d) Relative earnings, women in STEM



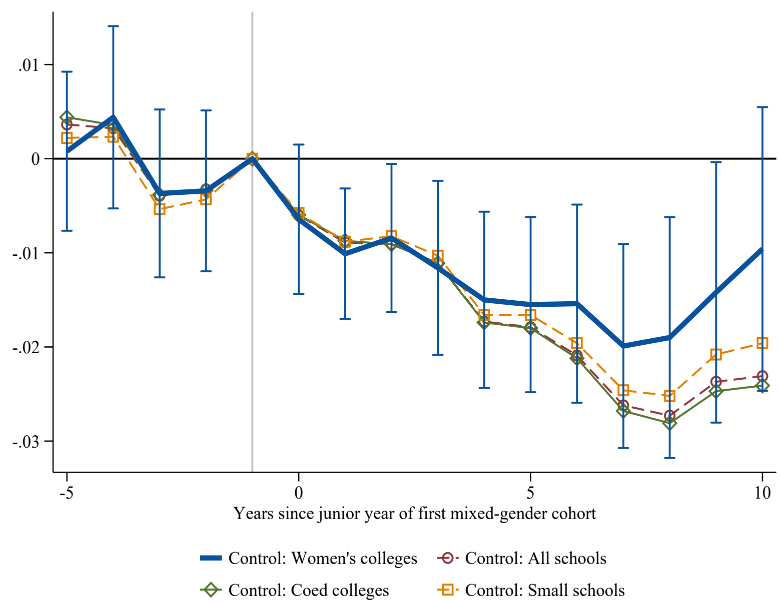
Notes: Data drawn from 1966-2016 CPS data accessed via IPUMS (Ruggles et al., 2020), linked to hand-collected dates of transitions to coeducation by institution's state. See Section 4 for further detail. Unemployment rate is measured among individuauls age 18-64. Relative STEM employment is constructed as the ratio of college-educated workers in STEM occupations to workers in non-STEM occupations. Relative income among men is constructed as the ratio of average annual income among college-educated men currently working in a STEM occupation to average annual income among college-educated men currently working in a non-STEM occupation. Relative income for women in STEM is constructed in the same manner, except that we include individuals with 0 earnings in the previous year. Panels display estimates of β_s from equation 2. Unemployment rate is calculated among all Standard errors are clustered at the state level.

Figure A2: The effect of coeducation on the STEM share of degrees awarded to women

(a) Effect on share of women majoring in STEM

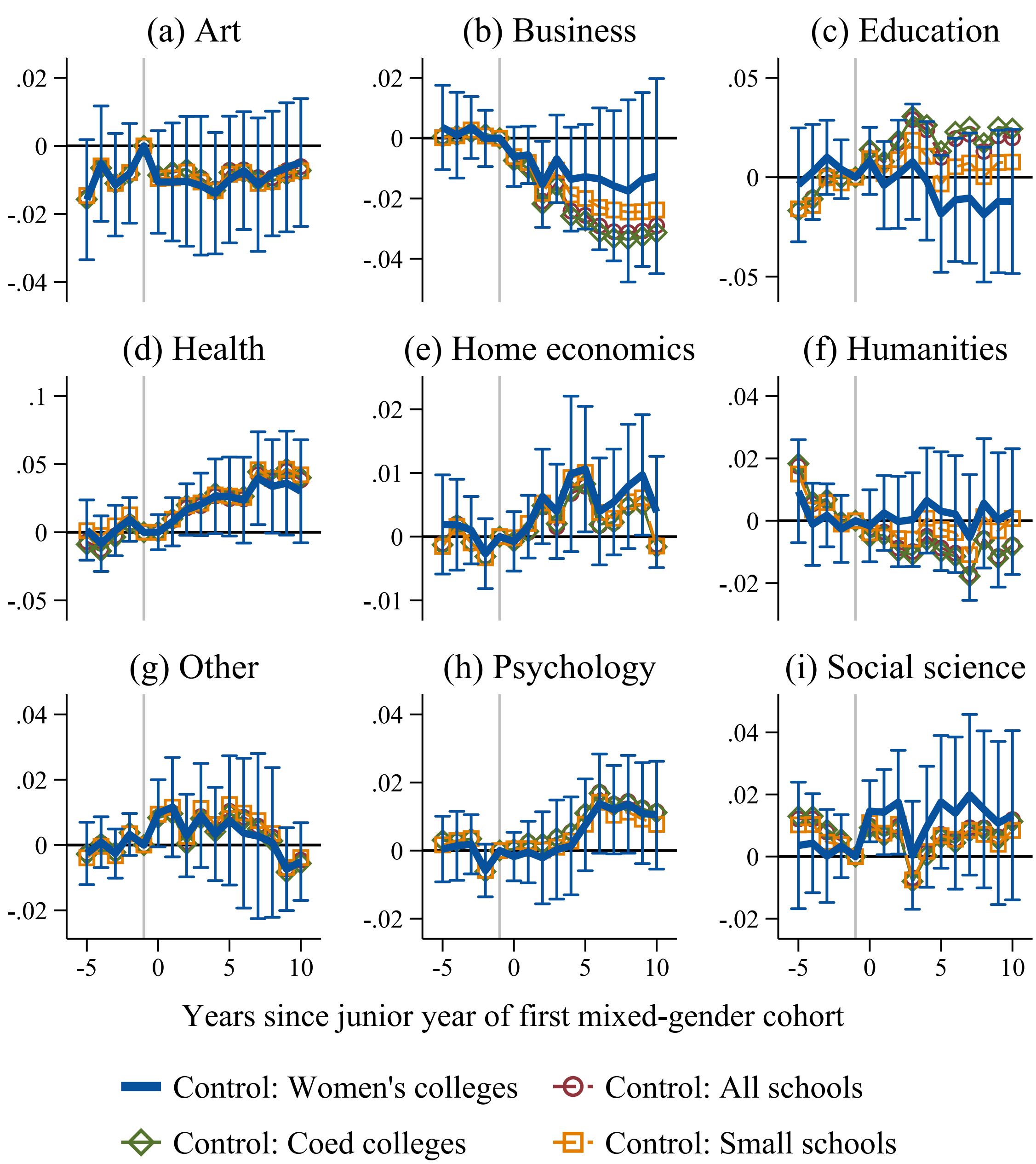


(b) Effect on share of women majoring in STEM, without math



Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 for further detail. The majors included in the STEM concentration are described in Section 4 and Appendix B. The bottom panel omits math, for reasons discussed in Section 5.3. Panels display estimate of β_s from equation 2. Standard errors are clustered at the institution level.

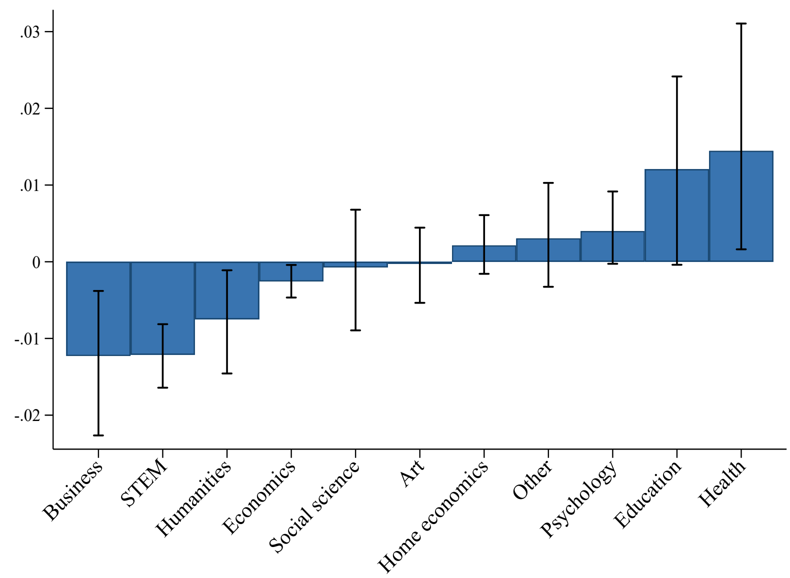
Figure A3: The effect of coeducation on the share of women choosing various degrees



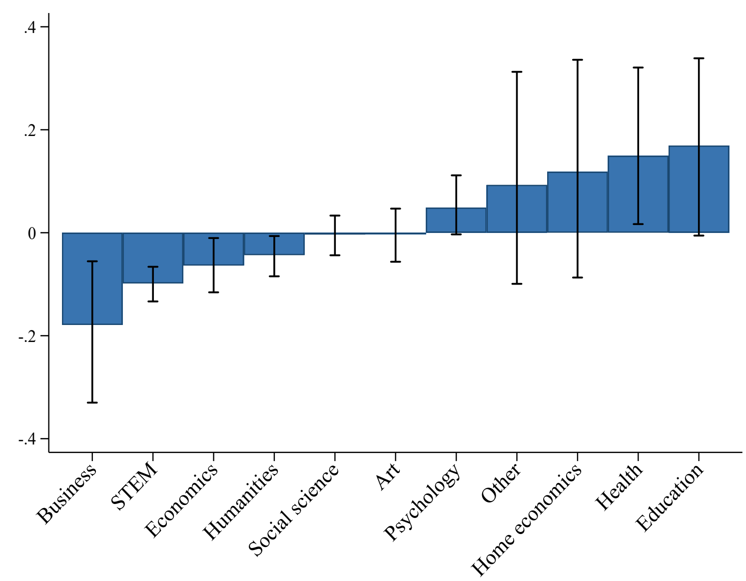
Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 and Appendix table A1 for further detail. Panels display estimate of β_s from equation 2, where the share of graduating women who choose each major is the dependent variable. Standard errors are clustered at the institution level.

Figure A4: The effect of coeducation on the full distribution of women's major choices, all-college specification

(a) Percentage-point effects by major



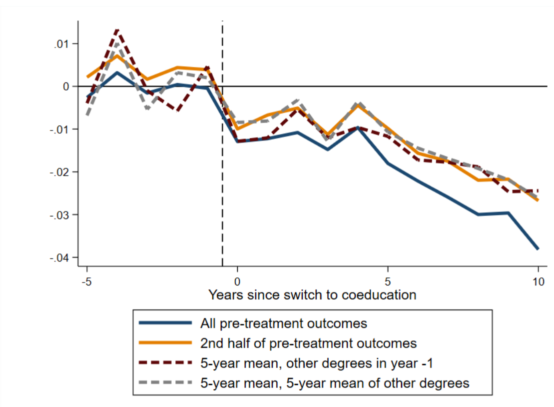
(b) Semi-elasticity by major



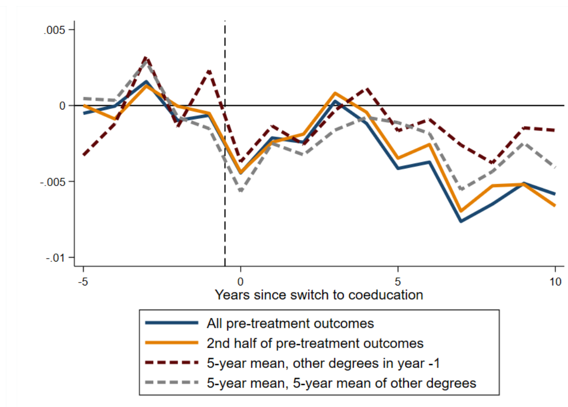
Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. The majors included in each concentration are described in Appendix B. Each panel displays estimates from a difference-in-difference version of Equation 2, using all colleges (other than men's colleges) as the comparison group. Estimates are re-scaled so that they capture the majoring effect of a 10 percentage-point increase in the male share of the student body induced by coeducation. Confidence intervals account for error in estimation of the effect of coeducation on the male share of the student body via a block bootstrap routine. Specifically, the routine computes 1,000 re-scaled coefficients and reports confidence intervals corresponding to the 2.5th and 97.5th percentiles of this observed distribution. The top panel uses a percentage-point scale. The bottom panel reports semi-elasticities, which are constructed by dividing the re-scaled percentage-point estimates by the baseline shares of women majoring in each given concentration.

Figure A5: Synthetic control estimates using alternate specifications and majors

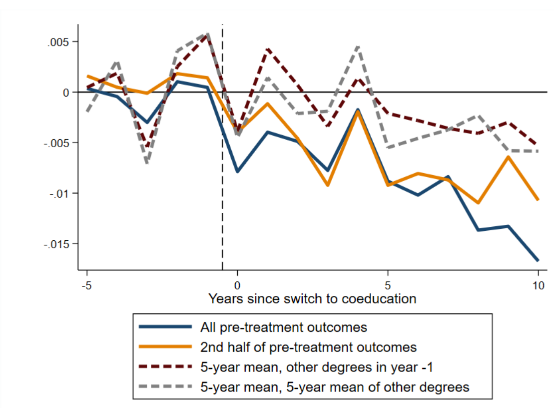
(a) STEM



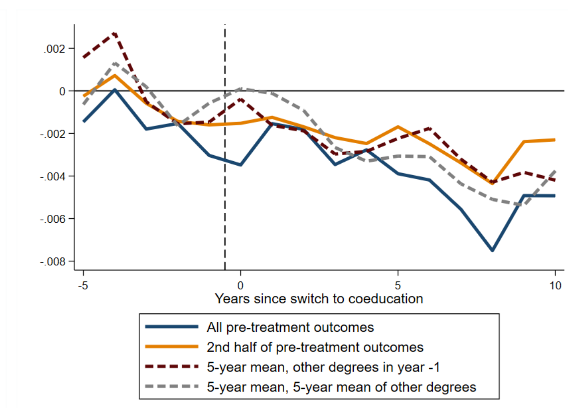
(b) Physical science



(c) Biology

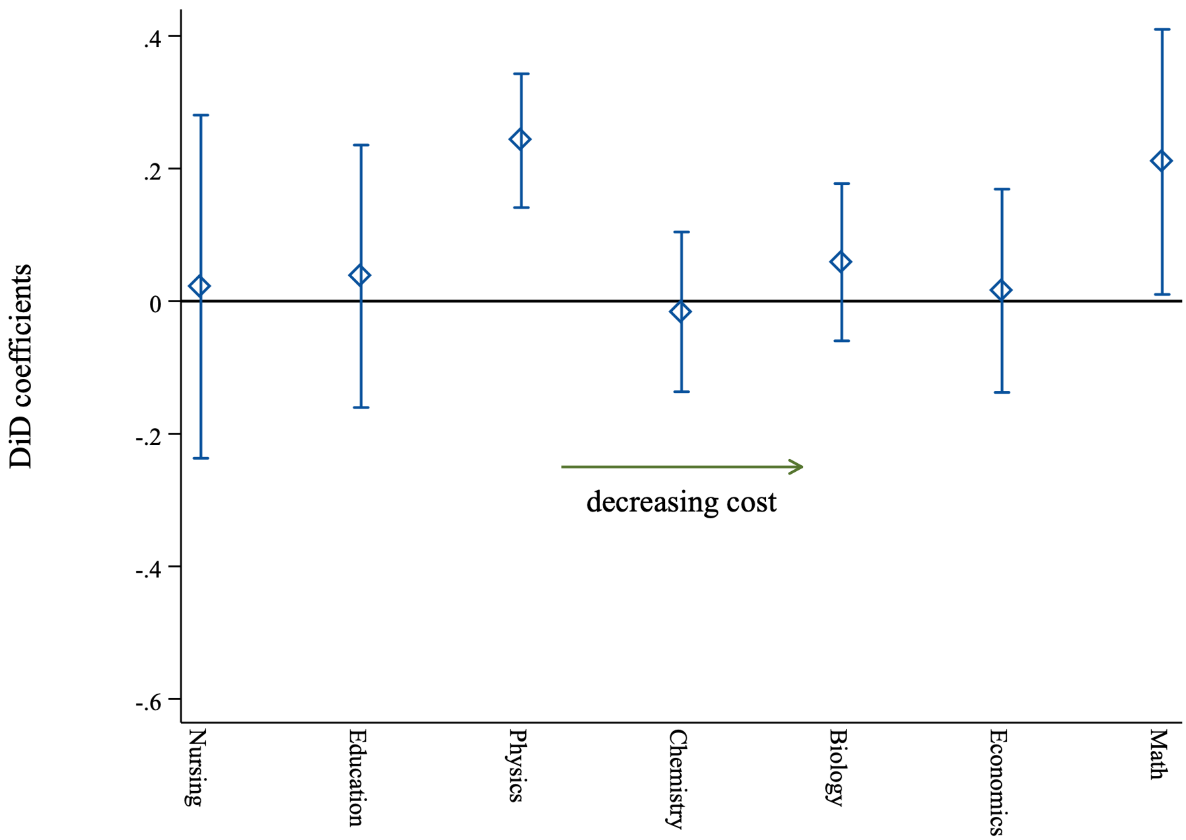


(d) Economics



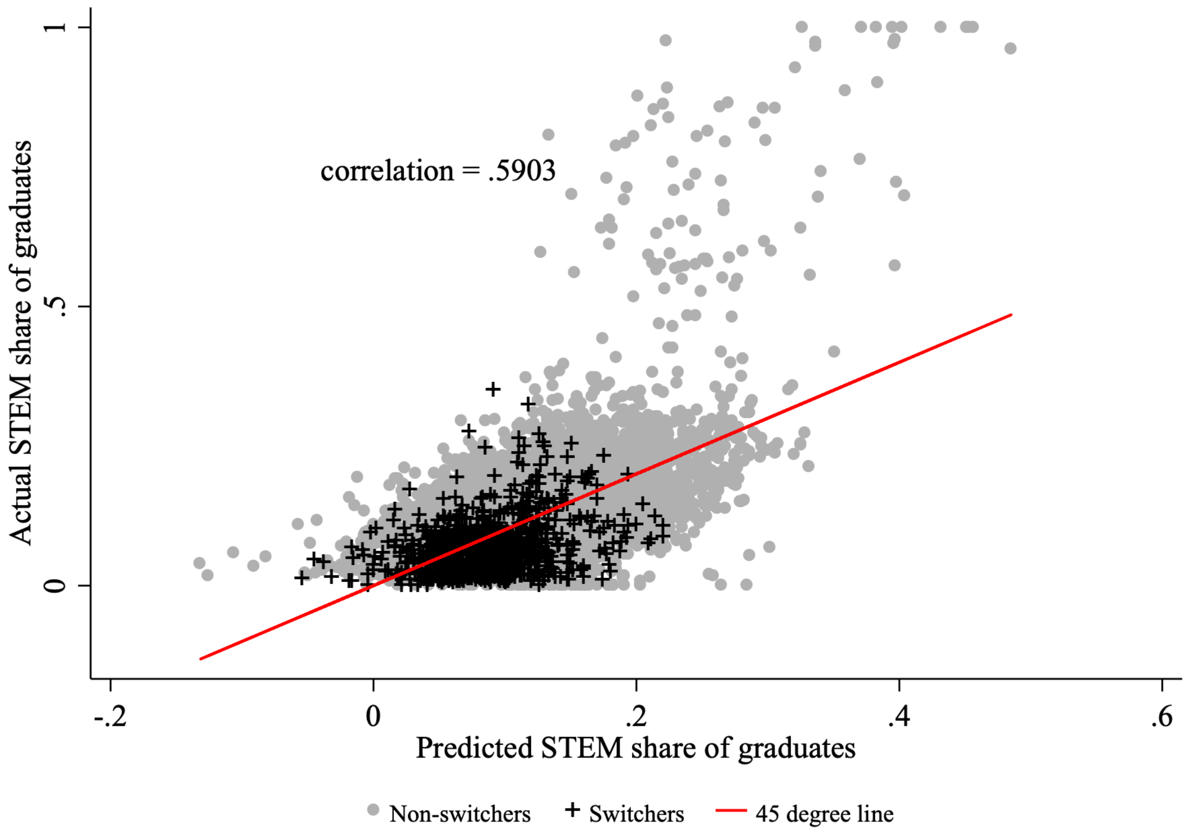
Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 and Appendix table A1 for further detail. See Appendix B.2 for information on the specific majors included within each concentration. Section 5.5 provides further detail on the synthetic control estimation procedure.

Figure A6: The effects of coeducation on log total number of degrees awarded by field, ordered by cost of field



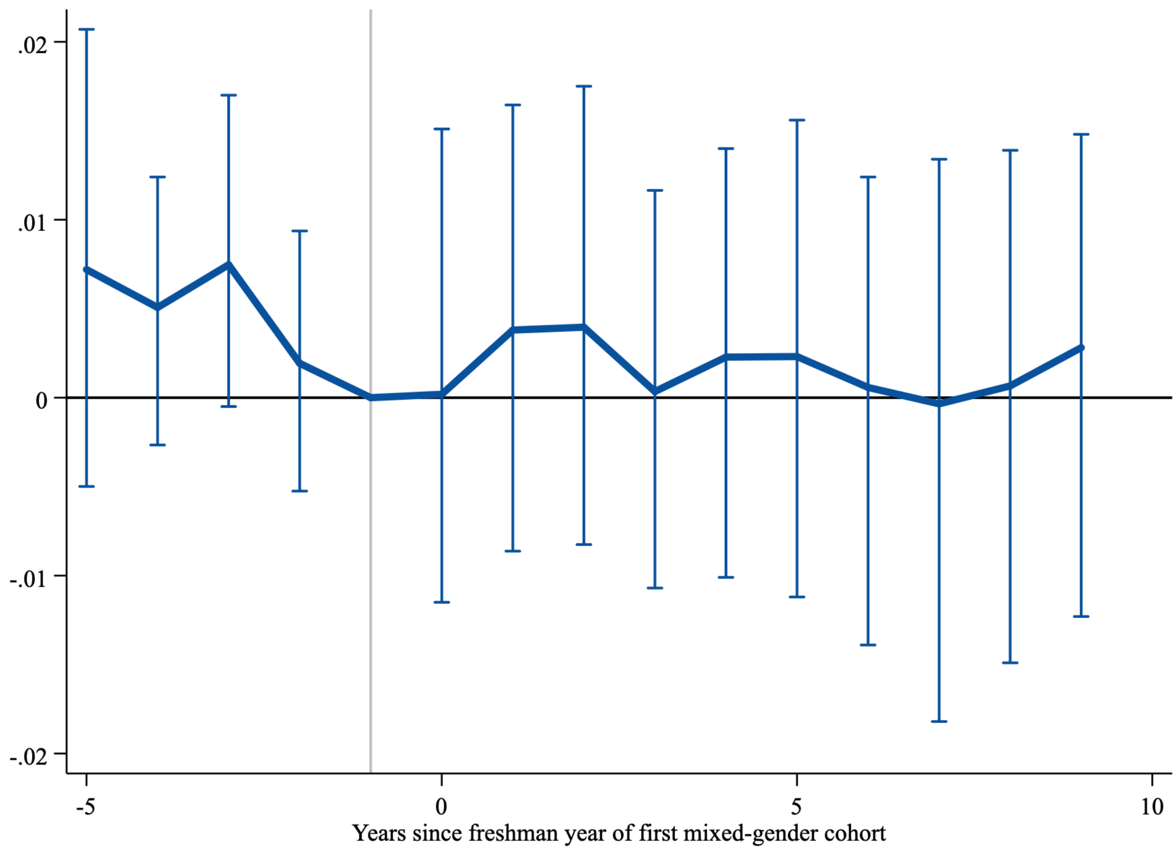
Notes: Data drawn from HEGIS/IPEDS surveys, spanning 1966-2016, linked to hand-collected dates of transitions to coeducation by institution. See Section 4 and Appendix table A1 for further detail. The graph displays estimates of γ_μ from Equation 10, which describes the difference-in-difference effect of coeducation on log total degrees awarded in major μ. Estimates are ordered by cost of field as estimated in Hemelt et al. (2018).

Figure A7: The correlation between women's actual STEM majoring and predicted majoring based on freshman characteristics



Notes: Data on freshman characteristics drawn from the CIRP Freshman Survey (TFS), spanning 1966 to 2006, matched at the institution-by-year level to data drawn from HEGIS/IPEDS surveys on STEM degrees awarded. (We implement a 4-year-lag between the two sources to map entering freshmen in TFS to graduating seniors in HEGIS/IPEDS.) Appendix B.2 describes the majors included in the STEM concentration. The graph plots the true share of women from each institution and year who graduated with STEM degrees against the predicted STEM share—$\hat{s}^{W}_{STEM,i,t=4}$. We predict this share based on equation 3.

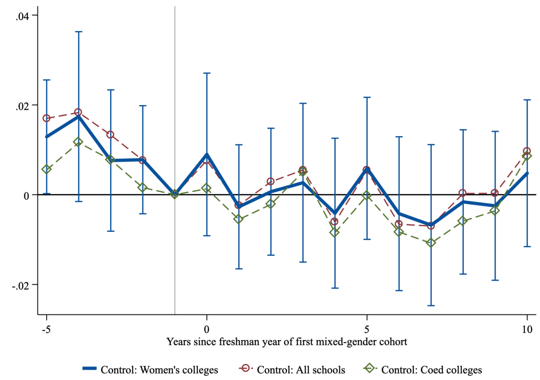
Figure A8: The effect of becoming coeducational on women's STEM preparedness, with fixed effects and additional controls



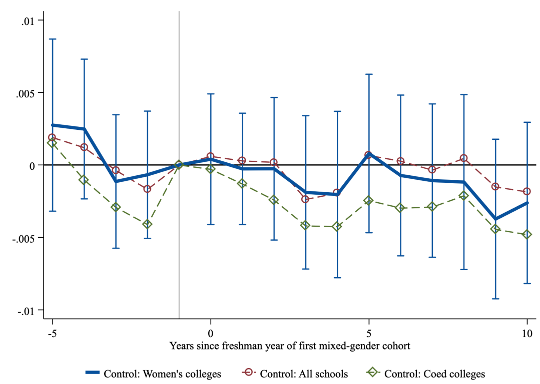
Notes: STEM degrees are defined as degrees in engineering, computer science, life and physical sciences, and mathematics. Data is drawn from records of the number of degrees awarded by year, institution, and gender from 1966-1969 and 1971-2016 in the Integrated Postsecondary Education Data System (IPEDS) and its predecessor, the Higher Education General Information Survey (HEGIS), linked to the CIRP Freshman Survey (TFS). Figure shows estimate of β_s from equation 1 with $\hat{s}^W_{STEM,i,t=4}$ calculated using equation 3, adding controls for the number of years of math, science, and computer science instruction in high school as well as missingness indicators for the high school course variables and school, region-by-year, and institutional-control-by-year fixed effects. Standard errors are calculated using a percentile block bootstrap with 1000 replications.

Figure A9: The effect of coeducation on women's plans for various STEM majors during freshman year

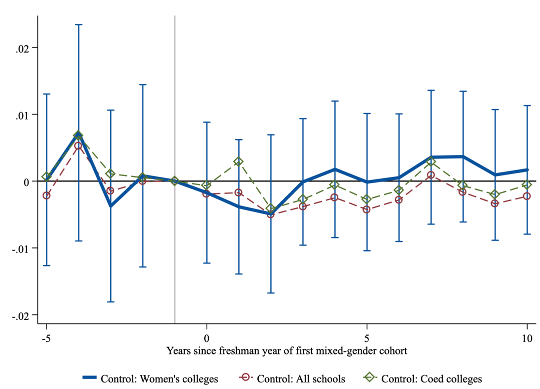
(a) Biology



(b) Physical Sciences



(c) Mathematics



Notes: Data is drawn from the CIRP Freshman Survey. Figure shows the effect of coeducation on the share of women at an institution who plan to major in each STEM field. Standard errors are clustered on the institution level. Mathematics includes statistics.