Female Managers and Gender Disparities: The Case of Academic Department Chairs

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This Version: January 23, 2019

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Abstract

Appointing female managers is a common proposal to improve women's representation and outcomes in the workplace, but it is unclear how well such policies accomplish these goals. I study the effect of female managers on workforce composition, the gender pay gap, productivity, and promotion in the context of academic departments. Using newly-collected panel data, I exploit variation in the timing of transitions between male and female department chairs with a difference-in-differences research design. I find female department chairs reduce gender gaps in publications and tenure for assistant professors and shrink the gender pay gap. Replacing a male chair with a female chair also increases the number of female students among incoming graduate cohorts by ten percent with no evidence of a change in ability correlates for the average student.

I am deeply grateful to Leah Boustan, David Lee, and Alex Mas for their comments, guidance, and support throughout all stages of this project. I also benefitted greatly from conversations and input from Jessica Brown, Anne Case, Mingyu Chen, Janet Currie, Hank Farber, Seth Gershenson, Claudia Goldin, Elisa Jacome, Larry Katz, Ilyana Kuziemko, Luisa Langan, Steve Mello, Chris Neilson, Harvey Rosen, and Betsey Stevenson, along with participants in the Princeton Industrial Relations Section labor lunch. Katie Hawn and LaShonda Spell provided excellent research assistance. All mistakes are my own. Throughout this paper I use "female" and "male" as the adjectival form of "woman" and "man," respectively, to refer to individuals' gender expression, and not to connote biological sex. This material is based upon work supported by the National Science Foundation Graduate Research Fellowship under Grant No. DGE 1148900. Any opinion, findings, and conclusions or recommendations expressed in this material are those of the author and do not necessarily reflect the views of the National Science Foundation.

1 Introduction

Occupational segregation and earnings disparities by gender are two enduring features of the US labor market (Blau and Kahn, 2017). Fewer women train for high-skill, high-pay fields like medicine, law, and academia, fewer enter the most prestigious career tracks, and fewer persist past the early years of their career. These patterns raise concerns about equity and efficiency both for individuals and society at large.[1] Appointing women to leadership positions is a common proposal to remedy gender disparities in business and political settings around the world (e.g. Rockefeller Foundation, 2016), but the management literature offers only mixed support for the efficacy of this strategy. For instance, Karaca-Mandic et al. (2013) find that assignment to a female manager shortens women's time to promotion among US military enlistees, but Bagues and Esteve-Volart (2010) find that women are less likely to be hired for positions in the Spanish judiciary when assigned to evaluation committees with a higher share of women.[2]

Academia offers an ideal setting in which to study the influence of managers. Worker productivity (in the form of academic journal publications) is observable, as are earnings at most public schools, and academics have predictable career progression benchmarks. And because most department regularly or semi-regularly rotate chairs, we can observe the impact of different managers on outcomes for the same individuals.

This paper studies how female department chairs affect gender gaps in outcomes like promotion to tenure, research productivity, pay, and workplace representation among faculty and students in graduate departments in economics, political science, accounting, and sociology across several decades. I collect a new panel database of department chairs and faculty in economics, sociology, accounting, and political science across nearly 200 institutions spanning more than 35 years. Many recent papers use faculty rosters or curriculum

[1] Hsieh et al. (2013) estimate that 15-20 percent of US GDP growth from 1960-2008 was due to reallocating talent across jobs, thanks to falling barriers to occupational entry by women and minorities.

[2] See also Matsa and Miller, 2011; Ehrenberg et al., 2012; and Kunze and Miller, 2017 for evidence in the affirmative, and Bertrand et al., 2018; Bagues and Esteve-Volart, 2010; Bradley et al., 2018; and Bagues et al., 2016 for null or even negative evidence.

vitaes as a source of linked employee-employer data to study the dynamics of the academic job market (Antecol et al. (2018), Sarsons (2017), Brogaard et al. (2018), Clauset et al. (2015), Weisshaar (2017)). As far as I am aware, the data used in this paper comprise the largest compilation of such rosters to date. These rosters give my paper an advantage over research on faculty hiring and diversity that relies on university-level data. Additionally, data from the National Science Foundation, public university earnings records, and bibliographic databases of journal articles allow me to analyze the impact of female chairs on a broad set of outcomes at all points of the academic career pipeline.

To study the impact of female chairs, I exploit cross-department variation in the timing of transitions between department chairs, and variation within department in the gender of different chairs over time. For some outcomes my treatment variable of interest will be the gender of the department chair in a given year, while in other cases it will be a measure of exposure to female chairs over several years, but the underlying variation will remain the same. Looking non-parametrically at years just before and after chair transitions in an event study framework helps guard against the possibility that a female chair's appointment may reflect, rather than cause, changes over time in department-level unobservables correlated with women's outcomes.

I find three results that indicate female department chairs help narrow gender gaps for faculty and students. First, among assistant professors, working more years under a female department chair is associated with smaller gender gaps in publication and tenure outcomes. Second, the gender earnings gap among faculty shrinks in the years after a woman replaces a man as chair. Finally, female chairs raise the number of women in incoming graduate student cohorts without affecting the number of men. I find no increase in women's representation on the faculty, and no effect of a female chair on ability correlates for incoming graduate students or the number of top papers published per capita at the departmental level, suggesting that the reduction in these gaps comes without penalty to departmental output.

What can a chair do to influence graduate student matriculation or research productivity and tenure for junior faculty? One possibility is that chairs act as mentors or role models, and steer the culture and tone of the department. Having a female role model as chair might increase women's demand for spots on the faculty or in the student body. Studies consistently find that exposure to teachers and role models of the same gender or race increase the likelihood that female and minority students will pursue a given field (Gershenson et al.; 2017; Kofoed and McGoveny, 2017; Carrell et al., 2010; Mansour et al., 2017; Porter and Serra, 2017; Bettinger and Long, 2005), and it is reasonable to speculate that such effects might extend to later stages of the career pipeline. However, although chairs have extra visibility and cachet, any senior faculty could fill these roles. Another set of mechanisms requires explicit action exclusive to chairs: dividing departmental resources, staffing committees for admissions and other processes, approving leave, negotiating with the university for additional resources, and negotiating with faculty who have received outside offers.

Three supporting results suggest that chairs take an active role in narrowing gender gaps in outcomes. First, tenure and publications gaps—and the equalizing impact of exposure to female chairs—persist among male and female assistant professors regardless of the gender of the chair who hires them. This pattern is contrary to a story about differential selection into departments by assistant professors that varies by the chair's gender. Second, I find the effects of exposure on tenure and publication gaps are not affected by controlling for exposure to any female senior faculty, suggesting that the presence of role models for female assistant professors is cannot account for the impact of female chairs.[3] Third, in contrast to the level shift in women's graduate matriculation I observe after a woman becomes chair, I see no such gender-differential trends following a female senior hire, or when the department hires or promotes its first female full professor. Thus the level shift observed in the number of female graduate students after a woman takes over as chair from a man cannot simply be explained as a reaction to the presence of female senior faculty.

[3] In fact, more time spent in departments with female senior faculty is associated with worse outcomes for women, even when controlling for chair gender. Controlling for both female chairs and female senior faculty in the same equation—as shown in Table 3—does not change the results for either.

I close the paper with an simple overlapping generations simulation to estimate the change in gender representation that would result over the long-run from a policy that temporarily replaced some male chairs in economics with women. I find that for the impacts described above, even a large policy effort to replace male chairs at 25 percent of departments would result in fairly small impacts on the number of female faculty twenty years in the future, relying on mechanical effects alone. So while female chairs meaningfully increase gender equity in outcomes, this exercise suggests some other important factors lay behind long-run demographic shifts observed in some fields.

Taken together these results suggest that chairs have important influence over outcomes in their departments. They also suggest that there has been room in the market to increase women's share of the academic workforce for the fields studied here without sacrificing average worker ability or research output.

This paper contributes to a large and growing literature studying how managers' characteristics affect workers' outcomes. While all of the outcomes in this paper have been studied on their own, this paper can jointly analyze promotion, pay, productivity, and hiring for the population in question, which helps distinguish between alternative potential mechanisms. My data also permit me to analyze earnings within firm conditioning on research output, which gives me an especially good measure of the unexplained portion of the gender pay gap relative to the existing literature (Cook et al. 2018 provides another good, occupation-specific measure). The academic setting is also useful for studying managers' impacts on gender representation, since graduate recruitment provides high-frequency "hiring" data where small demographic shifts can be readily observed.

Another valuable contribution is this paper's focus on a middle management position. The literature to date has mostly focused on front-line managers of entry-level workers (Glover et al. 2016; Karaca-Mandic et al. 2013; Giuliano et al. 2009) or top executives and managers or corporate boards (Matsa and Miller 2011; Bertrand et al. 2018; Tate and Yang 2015). Kunze and Miller (2017) study the impact of female representation at different hierarchical levels—including middle management—on women's promotion, but lack of data on actual managerial relationships, making it difficult to distinguish between role model effects and managers' actions. Husain et al. (2018) and Droganova (2018) both study managers in broadly similar settings to mine, with findings on quit rates and pay (respectively) that are broadly consistent with those described here. My study differs from these in the pre-determined timing of most chair transitions, which reduces the potential that unobserved changes in workplace policy drive both the appointment of female chairs and changes in gender gaps.

Policies of appointing female leaders are partly grounded in the idea that women will be less likely to discriminate based on gender, and though I cannot explicitly address discrimination this paper provides an indirect test of that hypothesis. One possible interpretation of the results described in this paper is that female chairs are on average less biased against women than male chairs. Past research provides some support for the existence of discrimination in academia (see e.g. Ginther and Kahn, 2004, Sarsons, 2017, and Weisshaar 2017 on publications and tenure), but less support for the idea that appointing women will reduce that bias. Moss-Racusin et al. (2012) find that female and male professors do not differ appreciably in their level of gender bias when assessing lab manager applications, while other studies have found that women's prospects for hiring or promotion do not improve—or even fall— when they are randomly assigned to committees with more female reviewers (Bagues and Esteve-Volart, 2010; Bagues et al., 2017).

An answer to why female leaders narrow gender gaps here but not in those settings may lie in the research that finds men and women manage in systematically different ways, to which this paper also contributes (Bertrand, 2011; Matsa and Miller, 2013; Bayer and Rouse, 2016). Future research can explore whether the effects I observe stem from differences in how male and female chairs distribute resources (see Duflo, 2012 for a review), assign less-prestigious tasks (Babcock et al., 2017), or mentor junior faculty (Athey et al., 2000), or if female chairs affect general gender attitudes in their departments (Dahl et al. 2018).

The remainder of this paper is structured as follows: Section 2 describes my data sources and variables of interest, Section 3 describes my methodology and identifying assumptions, and Section 4 outlines my key results. In Section 5 I discuss a simple simulation to gauge the potential impact of a policy to replace some male chairs with women in economics using the results from this paper. Section 6 concludes.

2 Data

The faculty analysis sample includes rosters for 153 doctoral departments in sociology and 135 in economics, plus 130 large accounting departments—all from US institutions—stretching from the present back to as early as 1974. Department chair names were also gathered for 87 US doctoral programs in political science, though high-frequency roster data are unavailable for that field. The overarching goal of the data collection for this project was to create an unbroken record, for each sample department, of the identity of the chair and other tenure track faculty in each year. For faculty members, I sought to establish where they worked in each year of their career, and when they were promoted to each successive rank in the tenure ladder. In order to record this, and to obtain other identifiers to link individuals when they moved to a different department, or when the spelling of their name differed from year to year, I also gathered the year and department where each individual received their PhD.

I focus on economics, sociology, accounting, and political science departments for several reasons. The progression from graduate school to faculty positions is relatively straightforward, compared to fields in which it is typical to spend time as a postdoctoral researcher in another scientist's lab. Second, it is easier to estimate individual productivity in these fields since papers are primarily authored by one person or a small group, while in many science fields large collaborations are fairly common.[4] Finally, these fields have a comparable range of working environments for faculty and high topical overlap, which makes it more natu-

[4] See https://www.natureindex.com/news-blog/paper-authorship-goes-hyper

ral to draw comparisons between them and their evolution over time. The time frame for the sample was chosen based on the availability of data that could be digitized reasonably quickly.

To facilitate comparisons over time, roster collection began with contemporaneous secondary sources—–mostly annual or semi-annual faculty directories or graduate study guides compiled by third parties using information provided by the departments.[5] When gaps exist in these records, I refer to primary sources, usually individual faculty CVs or department websites accessed using the Wayback Machine.[6]

As can be seen in Figure A.1, women's representation has grown over time across subjects, but at very different rates and from different initial points. Economics has the lowest share of women on the faculty at just sixteen percent, but also the largest departments, with the average department having roughly nineteen men and four women on the faculty. Women's representation among chairs lags behind total faculty representation, since chairs are drawn primarily from the ranks of more experienced professors. Among PhD students; I see the 1990s rise and mid-2000s plateau in female representation discussed for economics by Bayer and Rouse (2016) present in other fields also.

I link individuals across years using names, graduate institution, and year of PhD. Having multiple identifying variables allows me to make links across years that are robust to nicknames, name changes, and transcription errors. For individuals whose work histories were gathered using the internet, photographs and pronouns from departmental or personal websites were used to code an individual's gender expression using a binary variable for men versus women. In other cases, first names were matched to the Social Security Administration database to establish likely gender, and then to the commercial service Gender-API.com

[5] These sources included the American Sociological Association's *Guide to Graduate Departments of Sociology*; the *Prentice Hall Guide to Accounting Faculty*; the *Prentice Hall Guide to Economics Faculty*; and the American Economics Association's quadrennial member surveys for 1985, 1989, and 1993. For department-years with no chairperson named in those sources, I also consulted *Peterson's Guide to Graduate Programs in the Social Sciences and Humanities* and the American Political Science Association's *Guide to Graduate Study in Political Science.*

[6] The Wayback Machine (www.archive.org) stores cached snapshots of websites taken at various points in time. This essentially allows you to view time-stamped copies of previous versions of a website.

when SSA did not list the name or offered less than 95 percent certainty in one direction or the other. If no source provided a basis to infer an individual's gender as either male or female (roughly 2 percent of observations), their observations were dropped from the sample. However, special effort was also made to ascertain the gender of department chairs, including those with gender-neutral names, or names (like Andrea or Jean) associated with different genders in different cultures. This method no doubt led to some individuals being assigned an incorrect gender in my data. I apologize for any errors.

This paper also studies department chairs' influence on faculty productivity. For a measure of research productivity, I used two online sources of publication data to download full bibliographic data for articles from top journals in sociology, accounting, and economics.[7] For economics and some accounting journals I use the journal ranking database at *Research Papers in Economics*, which contains full bibliographic data for journals indexed by title and publisher.[8] For sociology and other accounting journals I used Scopus, an article database compiled by Elsevier. Neither source is fully comprehensive, but random inspection suggests coverage is very high. Using first and last name (for papers indexed by RePEc) or first initial and last name (for Scopus), I link publications to individuals in my faculty sample whose names allow them to be uniquely identified within the faculty sample.

To study changes in faculty pay, I obtained publicly-available earnings records for faculty at public research institutions with doctoral programs in the fields studied here. In cases where published salary data do not list academic department, I identified faculty in economics, sociology, and accounting departments using their names from my roster database. Since some data record actual disbursements instead of salary rates, I exclude an individual's first and last years of work from the analysis sample to avoid artificially low salary readings

[7]Top 5 and top 55 journals were identified using the RePEc aggregate journal rankings as of January 4, 2018, economics; Jacobs (2015) for sociology; and Hasselback et al. (2012) for accounting. For economics, I choose a cutoff of 55 instead of 50 to include a broader selection of field journals, and I choose the same number in sociology for consistency. Hasselback et al. (2012) rank only the top 40 accounting journals, so I use their list in its entirety.

[8]This RePEc service contains an automatically-generated list of papers for each journal issue provided by the publisher, and should not be confused with RePEc author pages, where individuals must take affirmative steps to identify their own work.

where an individual did not work the full year. I also exclude observations with less than $50,000 salary listed, although my results are robust to using other lower bounds. In all, my sample contains 21,000 person-year observations for 3,500 individuals from 105 departments at 43 schools.

To study graduate admissions, I obtained annual counts of matriculating graduate students and completed PhDs in doctoral departments of political science, sociology, and economics from the NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering* and the Department of Education's Integrated Post-Secondary Data System, respectively. Individual-level student data for graduating PhD recipients came from the NSF *Survey of Earned Doctorates.* These data sources are intended to cover all graduate students entering a department or graduating with a PhD, respectively, although only IPEDS is an administrative data source.

3 Methodology

I look at several different outcomes, using different specifications as appropriate for each one. From case to case, the exact identifying assumptions will differ, but the underlying source of variation will always remain the same—we will be comparing departments and individuals affected by female chairs versus male chairs at different points in time.

To get unbiased estimates of how female chairs affect outcomes, we must assume there are no unobserved factors influencing both departmental outcomes and the likelihood that the department appoints a female chair. Since the gender balance in each field has evolved differently over time (Figure A.1), I control for year fixed effects separately by subject in all regressions. And since women's representation—not to mention factors like the difficulty of earning tenure, the size of the graduate cohort, and the number of publications per faculty member—also varies systematically across departments (Figure A.2), I also include department fixed effects in each regression as well. To guard against department-specific trends driving both outcomes and the gender of the chair, for some outcomes I use event studies to test for the existence of pre-existing trends in outcomes leading up to the time a woman replaces a man as department chair.

Even ruling out department-specific trends in outcomes, we might be concerned about sudden shifts in departmental culture or priorities that affects outcomes and the gender of the chair going forward. One fact mitigating this concern is that, even if the gender of the department chair is endogenous, the timing of a transition between chairs is often pre-determined, since in many departments chairs serve for a regular term of three or four years. Moreover, although we cannot definitively rule out a sudden shock to departmental culture, we can look for other evidence that ought to exist if such a shift had taken place, like a change in the number of women hired or a change in the characteristics of women entering the department.

3.1 Difference-in-Differences

A simple difference-in-differences analysis, like the one I use to assess the impact of the chair's gender on the characteristics of admitted students, focuses either on individual outcomes, or outcomes aggregated by gender at the department-year level. In those cases, my specification is

$$Y_{gduy} = (\alpha_3 + \alpha_3[g = Female]) * Treat_{duy} + \beta * X_{iduy} + \epsilon_{gduy}$$

for gender-by-department-level aggregates or, for individual i of gender g

$$Y_{igduy} = \alpha_0 Female_i + (\alpha_1 + \alpha_2 Female_i) * Treat_{duy} + \beta * X_{igduy} + \gamma_{du} + \delta_{uy} + \epsilon_{igduy}$$

The variable of interest, in each case, is $Treat_{duy}$, an indicator for having a female chair in the year in question (e.g., the year the student was admitted to the program), and its impact on men versus women, measured by α_1 and α_2.

3.2 Exposure

When looking at productivity and tenure outcomes for assistant professors, I compare individuals who work more of their early career in departments chaired by women to those who work more for men. Indexing each person i by the department du and year y when they start work, I assess outcomes with the following specification.[9]

$$Y_{igduy} = \alpha_0 Female_i + (\alpha_1 + \alpha_2 Female_i) * Exposure_{duy} + \beta * X_{igduy} + \gamma_{du} + \delta_{uy} + \epsilon_{igduy}$$

The key explanatory variable here is $Exposure_{duy}$, defined at the department-year level. For an individual starting work in that department and year, it measures the fraction of years during their early career their department was chaired by a woman. In some specifications, I measure exposure out of six years, from y to $y+5$, since tenure review typically occurs in the sixth year. In other cases I measure it out of seven years, and include also the chair in $y-1$, when the assistant professor in question was likely hired. Note that exposure is an intent to treat measure, since I do not constrain the individual to stay in their first department for all six years. In addition to department and subject-year fixed effects, I also include fixed effects for the department from which individual i received their PhD.

The variable $Exposure_{iduy}$ takes values 0 through 1, inclusive, so that the coefficient tells us the difference between outcomes for an individual who works their entire early career for a female chair, versus one who works none. For these estimates to be unbiased, we must believe that in addition to the other assumptions discussed above, the number of years an assistant professor works for a male or female chair is uncorrelated with unobserved determinants of their research output and their likelihood of getting tenure. For this to be false in the context of the exposure measure, assistant professors would have to be able to choose the chair's gender in the department that hires them, foresee when the next chair transition (or transitions) in that department will occur, and anticipate the gender of future chairpersons.

[9] Not that each department d is associated with a single subject u.

Alternatively, chairs would have to front-load their recruitment of high-ability junior faculty of the same gender, and of low-ability junior faculty of the opposite gender, rather than recruiting the best available faculty in each year.

3.3 Event Studies

A static difference-in-differences relationship could be driven by an omitted variable, such as a general cultural change over time at a specific institution that increases women's prevalence among department chairs, faculty, and the student body. Therefore I also use event studies to determine the precise timing of changes in outcomes around the time of a transition from one department chair to another, and establish a relationship between the start of a new chair's term and the observed changes in outcomes. A sharp change following a chair transition, especially in the absence of differential pre-trends, suggests a relationship between the arrival of a new chair and any changes taking place after.

Like some other papers that use event studies, this project involves units of study (departments) with multiple events (chair transitions). Many department-year observations will be in the immediate pre- or post-event window for two or more events. Past researchers have used a variety of methods to account for this issue, such as analyzing only the first event, choosing the most significant event, physically duplicating observations, and allowing multiple event-time indicators to be activated for a single observation.

The first two methods mentioned above are not particularly useful for the task at hand. Choosing the first or most significant event is often reasonable in public policy settings—like the education funding court cases and legislation studied by Lafortune et al (2018)—wherein a state institutes a new policy and then may alter or expand that policy over time. In my case, each department has had semi-regular chair transitions stretching back to its first years, far outside the study window. Which transition Iobserve first depends on data availability, rather than fundamental department changes, and in general Ihave no reason to call one transition more significant than any other.[10]

A second method is to make a duplicate copy of the observations for a given department for each transition observed there, and analyze as though each duplicate was a unique department with a single event, reweighting observations to account for duplication. This method has been used in a number of other studies including Lafortune et al. (2018).

Arguing against the duplicate-copies method, Sandler and Sandler (2013) find that it may lead to biased estimates of trends before and after the event. They test the various methods mentioned here using Monte Carlo simulation, and recommend using the multiple-indicators approach. With this method, an observation falling in the first year after one event and the fourth year prior to another would have indicators turned on for both event time 1 and event time -4. Sandler and Sandler find that this method provides unbiased estimates of the true effects in each event-time category. I will use this as my main specification, and use the other methods mentioned above as robustness checks. A simple way to think about this event-study framework is that each transition-type-by-event-time category is a different "treatment", and that multiple treatments can be in effect for any given observation. Indeed, since multiple chair transitions are observed at every department, every observation has at least one treatment applied to it.

My event study framework will be further complicated by the fact that there are multiple types of events. Either the outgoing and incoming chairs have the same gender (i.e. a man replaces a man or a woman replaces a woman), or they have different genders.[11] I will treat all "gender-static" transitions as the control group—in practice, the vast majority of these

[10] Ican still, in principle, choose one event at each institution or a few with non-overlapping windows and analyze them as separate events. Icould also single out the first observed gender-switching chair transition as the "most significant" transition to compare. I will treat these as secondary robustness checks in an appendix.

[11] It could also be the case that a department goes from having a chair to having no chair, or vice versa. However, in these cases I assume either that the years for which no chair can be identified are counted in the term of the last person observed in the chair's position, or that having no department chair is equivalent to having a man as chair. The former model is preferable if you think of a chairperson as a clockmaker who sets the department to run in a certain fashion and then can leave it alone. The latter is preferable if un-chaired departments default to a mode of operation consistent with how the typical male chair would run things unless they are steered away from it (in this case by a female chair). In practice here, the two methods yield similar results.

are man-to-man switches. In my event study analyses, the outcome of interest for a given department in a given year is modeled using the following specification. For department du and year y:

$$Y_{duy} = \sum_{t=-5}^{5} (\alpha_t + \alpha_t^{MW} * s_{duty}^{MW} + \alpha_t^{WM} * s_{duty}^{WM}) * \tau_{duty}$$

$$+\beta X_{duy} + \gamma_{du} + \delta_{uy} + \epsilon_{duy}$$

Here, τ_{duty} indicates that department du experiences a chair transition t years after y.[12] If the transition is a male-to-female or female-to-male switch, I indicate this with s_{duty}^{MW} and s_{duty}^{WM}, respectively. The corresponding coefficients α^{MW} and α^{WM} yield the difference between the impact of the event-time indicators in each time bin. Department and subject-year fixed effects are represented by γ and δ.

The base period for the event study is the last year of the outgoing chair's term, and the omitted indicator is that same period for chair transitions where the incoming and outgoing chairs have the same gender. (In other words, the value of α_{-1} is constrained to be zero.) In each case, standard errors are clustered at the department level. In this specification, the α_t coefficients can be interpreted as the average difference in the level of the outcome variable in the t^{th} period before or after a gender-static chair switch, relative to the base period. The α_t^{MW} and α_t^{WM} coefficients give the average difference between the level in a time period before or after a man-to-woman [woman-to-man] chair switch relative to the level in the same period around a gender-static chair switch. That is, they tell me how the outcome variable changes after a gender-switching chair transition, over and above what one would

[12]Event studies with a pre-/post-event window of T periods usually apply the indicator variable for event time -T or T for observations more than T periods away – thus with a 5-period window the indicator for event time 5 equals 1 for periods 6, 7, and so on. In the multiple-indicators approach, if an observation is outside the event window for 2 or more events, the event-time -T/T "indicators", take on values greater than 1, equal to the number of events for which they lie outside the window. Thus for $t = \pm 5$, the full summand should be written

$$(\alpha_t * \tau_{duty} + \alpha_t^{MW} * s_{duty}^{MW} * \tau_{duty}^{MW} + \alpha_t^{WM} * s_{duty}^{WM} \tau_{duty}^{WM})$$

where τ_{duty} is the total number of transitions more than 4 periods away from the event, and τ_{duty}^{MW} and τ_{duty}^{WM} count how many are man-to-woman or woman-to-man transitions.

expect from a gender-static transition. To normalize the coefficients to the same base period for each chair transition type, I plot the event study effects for the t^{th} period using the point estimate and standard error for the linear combinations of $\alpha_t^{MW} - \alpha_{-1}^{MW}$, and $\alpha_t^{WM} - \alpha_{-1}^{WM}$.

This event study format is used for studying the level of a single outcome variable for an entire department. Occasionally, as when I study pay, I want to compare aggregate outcomes by gender within department, or outcomes for individuals with effects that vary by gender. In such cases, I add a second set of event-time indicators—here, the β coefficients corresponding to the α main effects—that apply only to women, and yield the difference between women's outcomes and men's.[13] Here, F_i indicates that an individual is a woman, or that the aggregate result is for women in the department (in which case I omit i subscripts):

$$Y_{igduy} = \sum_{t=-5}^{5} ((\alpha_t + \beta_t F_i) + (\alpha_t^{MW} + \beta_t^{MW} F_i) * s_{duty}^{MW} + (\alpha_t^{WM} + \beta_t^{WM} F_i) * s_{duty}^{WM}) * \tau_{duty}$$

$$+\beta X_{igduy} + \gamma_{gdu} + \delta_{guy} + \epsilon_{igduy}$$

In a typical event study framework with one event per unit of analysis, the effects can be summarized in a single number with a simple pre/post difference-in-differences estimate. Given the special case of the multiple-indicators event study specification, I will estimate pre/post effects using a single indicator that pools the impact of the event-time categories in the three periods before and the four periods after a chair transition. In some cases, my dependent variables are non-negative integers (e.g. the number of women in a graduate cohort or number of new hires). In these cases, instead of OLS regressions I use Poisson models, in which case the outcome variable is modeled as e raised to the power of the right-hand side of the equation. The coefficient on an explanatory variable in a Poisson regression is interpreted as the change in the log of the outcome, so a 0.1 coefficient would mean that increasing X by one unit increases the expected value of Y by ten percent.

[13] When looking at individual-level outcomes, I also include person fixed effects in X_{igduy}

4 Descriptive Analysis

Here I describe the key results of the analysis described above.

4.1 Worker Productivity and Tenure

Many authors studying the academic labor market have documented a publication gap between men and women (Ceci et al., 2010; Antecol et al. (2018)). Moreover, even conditioning on the number of publications, studies find women are less likely to receive tenure in economics and other fields (Weisshaar, 2017; Sarsons, 2017). My sample is no different. Depending on the exact metric, I find female assistant professors hired into my sample are between five and seven percentage points less likely to earn tenure, and publish roughly 25 percent fewer papers than men, counting either the "top five" or "top 55" journals.[14] These gaps remain even after conditioning on research output.

These gender gaps shrink significantly, and even reverse sign for assistant professors who spend a large portion of their early career in a department chaired by a woman. The effect is the same regardless of the gender of the chair who hired them (Table 2)—the impact seems to stem more from their exposure to female department chairs during the six years prior to their tenure review. The effect holds across all three fields for which I have faculty rosters. These regressions also control for the department from which each assistant professor earned their PhD, so even if female-headed departments affect the quality of the women who are hired, controlling for the department where they graduated does not eliminate the effects. This raises the possibility that men and women manage departments in ways that differ, leading to the effects observed here.[15]

[14]See my data appendix for a full list of journals. I choose 55 top journals instead of a round number like fifty in order to include additional field journals, and separate out the top five based on common practice in economics. My accounting journal sample includes fewer journals, limited to the top forty and top four.

[15]In the specifications for these outcomes, I control for the department where each individual earned their PhD, in addition to department and subject-year fixed effects. To maintain power for the regressions, I do not allow department and year fixed effects to vary by gender. The qualitative impacts of a reduction in the gender gap remains even when this constraint is lifted, but the effects are no longer statistically significant, as shown in Table A.1.

Two additional analyses suggest that these impacts can be attributed to the active effort of the department chair, rather than sorting of assistant professors into departments with same-gender chairs or role model effects for women on the junior faculty.

4.2 Faculty Pay Gap

Using data from public records available from research universities, I document a significant pay gap between male and female tenure-track faculty as shown in Table 4. The raw gap is quite large—roughly 23 percent—but results in large part from the distribution of men and women across different subjects, with accounting and economics being both more remunerative and also more male-dominated than sociology. Women are also younger on average, so I control for experience using the year each individual received their PhD. Even controlling for experience and faculty rank and comparing men and women in the same department and year, the gender pay gap stands over seven percent.

I conduct an event study around the time of a transition between two department chairs to measure whether female department chairs affect pay differently for men and women. (See Figures 1-3, and Table 5 which summarizes the same information.)[16] These regressions include controls for individual fixed effects and subject-specific quadratic polynomials in experience. I observe pre- and post-trends in pay for both men and women around some transition types, but in each case the trends before the chair transition are parallel for men and women. After a transition where the incoming and outgoing department chairs are of different genders, the gender earnings gap changes. In neither case do chairs lower average earnings by gender from baseline. After a woman replaces a man as chair, earnings rise for both men and women. While they flatten out for men after around two years at three or four percent above baseline, for women the rise is between five and six percent, closing the gender wage gap by about four log points in the latter years after a chair transition. The

[16] In reading the event study figures shown, it is useful to recall that the periods after a transition between chairs do not necessarily represent the year's of the new chairperson's term. The new chair may only serve one or two years. These graphs show department-level trends leading up to, or set in motion following a chair transition.

specification for these event studies does not control for title (since that may be an important margin along which pay adjusts), so it is most comparable to column four in Table 4, showing a gender gap of about eleven percent, meaning that by three to five years after a woman replaces a man as chair the gender gap closes by about one third. After a man replaces a woman, by contrast, men's earnings rise while women's rise by less or stay flat, re-widening the earnings gap by roughly the same amount.

These results are robust to including dummy variables for specific years of experience, and to excluding assistant professors, who have comparatively little wage variation within a department.

4.3 Women's Representation in the Department

Another common goal of diversifying management ranks along gender lines is to increase women's share of the workforce. I look at two aspects of a department's "workforce"—the faculty, and the graduate students. In both cases I conduct an event study around chair transitions. For faculty, this will measure changes in the total stock of the department's tenure track faculty. A flow measure is less useful given the relatively low number of faculty hires—the typical department in my sample hires about one faculty member each year, but the typical graduate cohort has twelve students. For graduate students, however, I can observe the number of men and women in each year's incoming graduate student cohort.

Among faculty (excluding the chairs themselves, whose arrivals or departures are often the cause of a transition rather than a result), I find no evidence of an increase in women's representation after a woman becomes chair. If anything, share of women on the faculty falls slightly after a woman replaces a man as chair, possibly due to a drop in hiring. I find no decrease in the number of papers published per capita in the department.

Among graduate students, on the other hand, I find an immediate jump of about seven percent in the number of female graduate students following a transition from a male to a female department chair (Figure 4). This level shift rises to about ten percent by the third cohort admitted after the transition, and persists into the years after the event window. When a man succeeds a woman as chair, by several years after such a transition the number of women admitted has, on average, risen again from the baseline level, though not by a statistically significant amount.[17]

One possible explanation for this shift in female graduate attendance is the suddenly-increased visibility of a senior female faculty member in the department. As a test of this possibility, I conduct a number of placebo event study analyses. I test two alternate event definitions, in place of a transition between chairs. In the first case, I designate an event every time a department hires a female full professor from outside the department. In the second case, I locate the year in which the department added its first female full professor to the faculty, whether through promotion or outside hiring. In this case, I can also observe events in the other direction, when because of faculty separations a department loses goes from having at least one female full professor to having none. As can be seen in the Appendix, none of these placebo events has the same differential impact by gender on graduate student matriculation.

Given the increase we see in the number of female graduate students, we might be interested in understanding what happens to the average ability of students in these incoming cohorts. One common concern with policies to actively recruit individuals from underrepresented groups is that doing so will reduce the quality of the people you admit, and the quality of their subsequent outcomes. If admissions committees in male-headed departments are biased against women, they might set a higher bar on the quality of applicants. There could also be discrimination in the pre-application stages (leading back to childhood) and other factors that lead to, on average, higher ability for incoming female students than male students.

I explore the influence of chair's gender on student ability using data from the National Science Foundation's Survey of Earned Doctorates. This survey is administered annually to

[17]Table 6 summarizes these impacts and others on the count of incoming first-years.

the entire population of graduates from US research doctorate programs. It includes sufficient information to link individuals back to the year in which they started their program, and thus to the chair who likely admitted them during the previous academic year. Although it does not include direct ability measures like GRE scores, it includes other variables that correlate with ability, such as identifiers for respondents' undergraduate institutions and their post-doctoral job outcomes.[18] In cases where respondents have a job lined up, it also lists the type of job and, for those bound for academia, what institution they will work at and whether their duties include research, teaching, administration, or something else. Beginning in academic year 2008, respondents with job arrangements could list their salary, which I use to predict salary for the full population of employed graduates, based on their subject, program rank, gender, and job type.

Table 7 shows the gender gap for four outcomes, along with gender-specific treatment effects of having been admitted by a female chairperson. (The effect on women is obtained by adding the coefficients for Treat and Treat*Female.) "College selectivity" is an ordered categorical variable defined for individuals who received undergraduate degrees from United States universities, listing their undergraduate institution's rating in the 2009 edition of *Barron's Profiles of American Colleges*, with 8 being the highest value and 1 being the lowest. "Top college" is a binary variable set to one for those who graduated from schools in the highest selectivity category. The results are qualitatively similar if I include undergraduates from top international universities on this list. An academic research job is any job affiliated with an academic institution where research is listed as one of the job duties. These regressions include fixed effects for departments and subject-year interactions.

Table 7 shows that there is, in fact, a sizable difference in the ability correlates for men and women, with women's values substantially higher. Meanwhile, women are less likely to get academic jobs and have lower predicted salaries than men. In no case does the gender gap close—in fact, for undergraduate institution quality, the gaps widen, although

[18] We might reasonably ask whether these variables truly proxy ability, but the gender gap's significance remains even if you think they stand in for some other advantage, like family resources or social connections.

not statistically significantly. In all, there is no indication that the ability of women admitted to the program under a female chair is lower than those admitted by men. Since job type and salary rely on both inherent student ability and value added by the doctoral program, it is theoretically possible that female chairs have offsetting effects on female students that result in no change to the gender gap in these outcomes. However, in auxiliary analysis I find that exposure to female chairs has no impact on the gender gap in job outcomes, even when conditioning on the gender of the chair at the time of admissions. This rules out a scenario with offsetting effects, suggesting female chairs do not affect the propensity of students to place into academic jobs.

5 Discussion

The results described above paint a picture of department chairs who make an important difference on the margin in outcomes for the faculty and graduate students in their departments.

Based on these findings, how much would we narrow the gender gaps in academia overall by appointing more female department chairs? To get a sense for the magnitude of the changes described here, I construct a simple mechanical overlapping generations model simulating the trajectory of the economics workforce over a twenty-year period, based on current population flows and stocks. (I choose economics for convenience, since by coincidence its population sizes are straightforwardly divisible.) its In this simulation, I assume the population of different groups evolve from some initial conditions according to prescribed laws of motion. I then ask, given the impacts observed above, what would be the effects of replacing male chairs with women in 25 percent of departments?

Academia, in my model, is made up of many identical departments composed of men and women belonging to different age cohorts who progress from one time period to the next through three groups: graduate students, assistant professors, and full professors. For simplicity, (since I find no significant effect of female chairs on hiring or job placement for their students), I will assume no crowd-out stemming from additional graduate students in the pipeline, or a rise in tenure rate. The small number of people involved will make this assumption less consequential than it might otherwise be.

5.1 Key Effects

Based on the findings above, I assign two key effects to female department chairs in this model. Namely, they (1) increase the number of female graduate students by 10 percent, persisting indefinitely and (2) increase the likelihood female assistant professors will earn tenure (to 0.7 from 0.6). The tenure effect is estimated based on full exposure to a female chair as an assistant professor, but for simplicity I assume it will apply to anyone who ever serves under a female chair.

A secondary consequence of both these effects will be to increase the number of women among tenured professors, and thus to increase the future likelihood that any given department will have a female chair. In practice, given the small number of affected individuals, and the size and gender balance of the existing population stock of professors, the number of departments affected by this change will be very small.

5.2 Graduate Students

In economics, roughly four hundred women earn PhDs each year. Replacing chairs at 25 percent of departments (with 100 female graduate students per cohort, since we assume identical departments) raises the number of women in the graduating class by 10 per year on an ongoing basis, starting six years in the future when the current cohort of first-years finish their programs.

According the recent annual reports from the Committee on the Status of Women in the Economics Profession, the placement rate into US doctoral departments for graduating PhD recipients has been about twenty percent over the past decade, but my data show only half that many placements. (Presumably the others are going to business and public policy schools or other departments, while my data cover only pure economics.) That means there are about forty junior placements into economics per year, and raising the number of female PhDs by ten per year will raise it by one additional woman each year. If we count the business and public policy placements as well, this rises to two per year instead, starting after six years.

5.3 Tenure

In the 2017-2018 academic year the economics department faculty rosters in my sample list around three hundred female assistant professors in economics, which lines up roughllly with the number graduating if we suppose there are seven cohorts of assistant professors, each with about forty women. Based on Table 1, about sixty percent will eventually get tenure somewhere in my sample of economics departments, or 24 per year.

Exposure to a female chair raises that number to seventy percent for affected women. (While the effect shown in Table 1 applies to individuals with a full seven years of exposure, for simplicity I assume it's an impact of ever working for a female chair.) Thus if we replace male chairs with female chairs in 25 percent of departments (which, assuming identical departments, have ten female assistant professors out of the forty in each cohort), the number of female assistant professors getting tenure each year rises from six to seven. At schools where a male chair is replaced with a woman, seven existing cohorts of assistant professors are exposed to female chairs who would have worked for male chairs otherwise, plus another three cohorts who enter over the subsequent three years. Thus an additional ten women earn tenure under the policy than would have otherwise.

5.4 Total Effects

I allow the model I have outlined to run forward in time twenty years. Given the changes to the population flows described above, replacing male chairs in this way would add twenty-eight additional assistant professors over fourteen cohorts, plus ten tenured professors, for a total of 38 additional faculty over twenty years. There were about 750 female tenure track faculty in economics departments in 2017-2018, meaning that this policy would increase the number of female faculty members by roughly five percent of their current population size.

There are many reasons to think this model may understate the impact of female chairs. Along with the addition of extra women into the pool of potential department chairs, a variety of mechanisms could lead to dynamic effects that increase the long-run impact of the policy modeled here, including changing early career academics' role-models or self-perceptions (Cech et al., 2011), gender spillovers in academic outcomes (Bostwick and Weinberg, 2017), or reduced prejudice on the part of male colleagues (Carrell et al., 2016; Dahl et al., 2018). Most important of all, perhaps, is the likelihood that providing a more balanced set of professional role models in a field will increase the diversity of undergraduate majors, graduate school applicants, and other aspiring economists, and will widen the pipeline of talented workers from currently-underrepresented backgrounds (Gershenson et al., 2017; Kofoed and McGoveny, 2017; Carrell et al., 2010; Mansour et al., 2017; Porter and Serra, 2017).

Ultimately, what this simple model says is that there is nothing in the simplest mechanics of the job market to suggest that temporarily adding female department chairs will bring substantially more women into the workforce. Although fields like sociology had substantially more women in positions of leadership in the early 1980s, these results suggest this was not the source of the rapid rise in female representation in those fields over the past several decades.

6 Conclusion

This paper finds that, for outcomes at the individual or department level, female managers in academia have important effects on gender disparities, likely due to of differences in active management practices. They narrow gender gaps in early-career achievement for faculty, raise women's earnings, and increase the number of women entering as graduate students, without changing the number of papers published in their departments or the characteristics correlated with ability among incoming cohorts of students. Despite these positive effects, the presence of female department chairs does not seem to be the factor behind long-run changes in women's representation across fields.

The lesson from this paper is not that it is always necessarily better for a woman to work in a female-chaired department, or that chairs show favoritism towards individuals of their own gender. Rather, this research reinforces other findings that suggest managers from different backgrounds often take different approaches, highlighting the value of diversity among decision-makers. Further research on the mechanisms at work here will hopefully lead to a better understanding of gender disparities more broadly, and identify management practices that will help all individuals and academic departments achieve their full potential regardless of gender or other characteristics.

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7 Tables and Figures

Table 1: Early-Career Exposure to Female Chairs Shrinks the Tenure and Publication Gender Gap

	Likelihood of Receiving Tenure				Count of Publications	
	First Inst.		Any Inst.			
	Ever (1)	by Year 8 (2)	Ever (3)	by Year 8 (4)	Top 5 (5)	Top 55 (6)
$Female_i$	-0.0480***	-0.0641***	-0.0560***	-0.0808***	-0.1096***	-0.4538***
	(0.0150)	(0.0150)	(0.0160)	(0.0164)	(0.0272)	(0.0638)
$Exposure_i$	-0.0831	-0.0414	-0.0144	0.0151	-0.0770	-0.1968
	(0.0511)	(0.0504)	(0.0451)	(0.0489)	(0.0821)	(0.1997)
$Exposure_i * Female_i$	0.1103*	0.0882	0.0963*	0.0818	0.1780*	0.5876***
	(0.0599)	(0.0607)	(0.0520)	(0.0597)	(0.0920)	(0.2132)
Sample Mean	0.50	0.40	0.64	0.49	0.41	1.54
R^2	0.1989	0.2160	0.1922	0.1979	0.3567	0.3485
N	6134	6134	6134	6134	5563	5563

[1] Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Results are from linear probability models (models 1-4) measuring the likelihood of receiving tenure at one's hiring institution (ever or within eight years, models 1 and 2) or at any sample institution (ever or within eight years, models 3 and 4) and OLS regressions (models 5-6) where the outcome is the total number of papers published within eight years of earnings a PhD in top 5 economics journals (or equivalent top journals in other subjects), or in top 55 journals from one's subject (with the cutoff chosen to include additional research field representation). See the data section for more information on which journals are included in each category. Each observation is an assistant professor hired at least eight years before the end of the sample period. Individuals who are observed fewer than three years are excluded. $Exposure_i$ takes values from 0 to 1 in intervals of $\frac{1}{7}$, measuring the share of years a professor's hiring department was chaired by a woman in years 1-6 of their career plus the year before they started work. Each model also controls for the individual's PhD department, plus hiring department and subject-specific year fixed effects. The sample in columns 5 and 6 is limited to individuals who are uniquely identified by their first initial and last name. Standard errors are clustered at the department level.

Table 2: The Effect of Exposure to Female Chairs on Tenure and Productivity Does Not Vary with the Gender of the Hiring Chair

	Tenure Ever, First Inst.		Count of Top 55 Publications	
Hiring Chair is:	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
$Female_i$	-0.0460***	-0.0691	-0.4702***	-0.3844
	(0.0154)	(0.1442)	(0.0657)	(0.7173)
$Exposure_i$	-0.1036*	-0.1061	-0.5576**	0.3943
	(0.0615)	(0.2612)	(0.2247)	(1.1449)
$Exposure_i * Female_i$	0.1127	0.0702	0.8535***	0.4669
	(0.0837)	(0.3020)	(0.2974)	(1.0828)
R^2	0.2106	0.6712	0.3594	0.6858
N	5561	573	5035	528

[1] Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Results are from OLS regressions measuring the likelihood of receiving tenure at one's hiring institution (ever) and the total number of papers published within eight years of earnings a PhD in top 55 journals from one's subject (with the cutoff chosen to include additional research field representation). See the data section for more information on which journals are included in each category. Each observation is an assistant professor hired at least eight years before the end of the sample period. Individuals who are observed fewer than three years are excluded. Unlike in Table 1, because the hiring chair's gender is pre-determined, $Exposure_i$ takes values from 0 to 1 in intervals of $\frac{1}{6}$, counting the number of years a professor's hiring department was chaired by a woman in years 1-6 of their career. Each model also controls for department and subject-specific year fixed effects. The sample in columns 3 and 4 is limited to individuals who are uniquely identified by their first initial and last name. Standard errors are clustered at the department level.

Table 3: Early-Career Exposure to Female Chairs—Not Any Female Senior Faculty—Shrinks the Tenure and Publication Gender Gap

	Tenure, First Inst.		Tenure, Any Inst.		Publication Count	
	Ever (1)	by Year 8 (2)	Ever (3)	by Year 8 (4)	Top 5 (5)	Top 55 (6)
$Female_i$	0.0404	0.0213	0.0350	-0.0059	-0.0701*	-0.2578***
	(0.0352)	(0.0324)	(0.0360)	(0.0345)	(0.0394)	(0.0979)
Exposure to Female Chairs (Out of 7 years)						
$Exposure_i$	-0.1020**	-0.0604	-0.0303	0.0003	-0.0703	-0.2042
	(0.0510)	(0.0506)	(0.0456)	(0.0496)	(0.0826)	(0.2017)
$Exposure_i * Female_i$	0.1380**	0.1150*	0.1243**	0.1051*	0.1866**	0.6389***
	(0.0600)	(0.0607)	(0.0534)	(0.0608)	(0.0927)	(0.2148)
Exposure to Any Female Full Professors (Out of 6 years)						
$Exposure_i$	0.1029***	0.1037***	0.0839**	0.0794**	-0.0387	0.0398
	(0.0361)	(0.0315)	(0.0358)	(0.0334)	(0.0493)	(0.1222)
$Exposure_i * Female_i$	-0.1181***	-0.1140***	-0.1216***	-0.1001**	-0.0523	-0.2591**
	(0.0393)	(0.0364)	(0.0407)	(0.0400)	(0.0514)	(0.1258)
Sample Mean	0.50	0.40	0.64	0.49	0.41	1.54
R^2	0.2011	0.2182	0.1942	0.1993	0.3569	0.3488
N	6134	6134	6134	6134	5563	5563

[1] Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Results are from OLS regressions measuring the likelihood of ever receiving tenure at one's hiring institution (models 1-2) or at any sample institution (models 3 and 4) ever or within eight years, and counting the total number of papers published within eight years of earnings a PhD in top 5 economics journals (or equivalent top journals in other subjects), or in top 55 journals from one's subject (with the cutoff chosen to include additional research field representation). See the data section for more information on which journals are included in each category. Each observation is an assistant professor hired at least eight years before the end of the sample period. Individuals who are observed fewer than three years are excluded. $Exposure_i$ takes values from 0 to 1 in intervals of $\frac{1}{6}$ in the case of female professors, measuring the share of years an assistant professor's hiring department had at least one female full professor during years 1-6 of their career. In the case of exposure to female chairs, $Exposure_i$ takes values from 0 to 1 in intervals of $\frac{1}{7}$, measuring the share of years a professor's hiring department was chaired by a woman in years 1-6 of their career plus the year before they started work. Each model also controls for department and subject-specific year fixed effects. The sample in columns 5 and 6 is limited to individuals who are uniquely identified by their first initial and last name. Standard errors are clustered at the department level.

Table 4: Among Faculty, Women Earn Less than Observationally-Similar Men

	Impact on Ln(Salary)						
						Matched w/Papers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Female_i$	-0.2326***	-0.2084***	-0.1327***	-0.1200***	-0.0759***	-0.0567***	-0.0429***
	(0.0191)	(0.0162)	(0.0145)	(0.0142)	(0.0125)	(0.0139)	(0.0136)
Fixed Effects							
Year		X	X	X	X	X	X
University		X	X	X			
Subject			X	X			
Department					X	X	X
Rank					X	X	X
Chair				X	X	X	X
Publication Count							X
Experience Controls							
Any		X					
Subject-Specific			X	X	X	X	X
Sample Mean	11.75					11.80	
R^2	0.0482	0.3230	0.4514	0.4914	0.5776	0.5967	0.6135
N	18920	18920	18920	18920	18920	13115	13115

* $p < .1$, ** $p < .05$, *** $p < .01$. Sample includes tenure track faculty in accounting, sociology, and economics at 42 public research universities. Results come from regressing log salary on a female indicator plus other controls as shown at the person-year level. Experience controls for a quadratic in years since finishing PhD. Department-level fixed effects interact university and subject indicators. Title fixed effects vary by subject. Chair indicator includes individuals who have been chair at any point in the past. Publication count controls for top 5 and top 55 journals by field, as of eight years after earning the PhD. Standard errors are clustered at the person level.

Table 5: Gender Gap in Pay Shrinks When a Woman Replaces a Man as Chair, Relative to Transitions Where the Outgoing and Incoming Chair Have the Same Gender

Depvar	Change in Level Relative to Transition Base Period			Difference in Change Relative to Same-Gender Transition		
ln(Salary)	Men	Women	Gender Gap	Men	Women	Gender Gap
Man-to-Man or Woman-to-Woman Chair Transitions						
5+ years before	-0.013*	-0.048***	-0.036***			
	(0.007)	(0.011)	(0.012)			
2-5 years before	0.001	-0.012	-0.013			
	(0.005)	(0.008)	(0.009)			
1-5 years after	0.006	-0.004	-0.01			
	(0.006)	(0.013)	(0.015)			
6+ years after	0.003	-0.016*	-0.019**			
	(0.005)	(0.008)	(0.009)			
Man-to-Woman Transitions						
5+ years before	-0.014	-0.023	-0.009	-0.002	0.026	0.027
	(0.014)	(0.019)	(0.023)	(0.015)	(0.02)	(0.025)
2-5 years before	-0.009	-0.02*	-0.011	-0.01	-0.008	0.002
	(0.008)	(0.012)	(0.014)	(0.008)	(0.012)	(0.014)
1-5 years after	0.004	0.018	0.014	-0.002	0.022**	0.024*
	(0.009)	(0.013)	(0.015)	(0.008)	(0.011)	(0.014)
6+ years after	-0.012	0.009	0.02	-0.015	0.025	0.039*
	(0.012)	(0.015)	(0.019)	(0.013)	(0.017)	(0.021)
Woman-to-Man Transitions						
5+ years before	0.037**	0.018	-0.019	0.05***	0.067***	0.017
	(0.015)	(0.023)	(0.028)	(0.015)	(0.024)	(0.028)
2-5 years before	0.02***	0.015	-0.005	0.019**	0.027**	0.008
	(0.008)	(0.012)	(0.014)	(0.008)	(0.012)	(0.014)
1-5 years after	0.016	-0.002	-0.018	0.01	0.002	-0.008
	(0.01)	(0.015)	(0.018)	(0.009)	(0.011)	(0.014)
6+ years after	0.021	-0.016	-0.036	0.017	0	-0.017
	(0.013)	(0.019)	(0.022)	(0.013)	(0.019)	(0.023)
Sample Mean	11.75					
R^2	0.904					
N	18403					

[1] Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Results come from a regression with the same specification as Table 4, Column 4, but include person-level fixed effects and event-time indicators for periods around three types of chair transition: those in which the gender of the incoming and outgoing chair are the same, versus transitions where a woman replaces a man and those where a man replaces a woman. Effects are pooled in the three periods before and five periods after the transition. Event time is numbered so that the last year of the outgoing chair's term is the last year prior to the transition, the the first year of the new chair's term is the first year "after" the transition, or $t+0$. Levels are plotted relative to the omitted base period around each type of transition, which is the last year of the outgoing chair's term. To correspond to the multiple-indicators event study format, the variable representing the edges of the event-time window (5+ years prior, $t-5$ and 6 years after, $t+5$) contain count variables of the number of transitions outside the event-window in either direction. 517 singleton observations have been omitted from the fixed-effects regression. Standard errors clustered at the person level.

Table 6: Number of Female First-Year Students Rises When a Woman Replaces a Man as Chair, Relative to Transitions Where the Outgoing and Incoming Chair Have the Same Gender

Depvar	Change in Level Relative to Transition Base Period			Difference in Change Relative to Same-Gender Transition		
N 1^{st} *Years*	Men	Women	Gender Gap	Men	Women	Gender Gap
Man-to-Man or Woman-to-Woman Chair Transitions						
5+ years before	-0.01	-0.017	-0.007			
	(0.029)	(0.036)	(0.014)			
2-5 years before	-0.026	-0.024	0.002			
	(0.021)	(0.025)	(0.019)			
1-5 years after	-0.056*	-0.046	0.01			
	(0.033)	(0.035)	(0.023)			
6+ years after	0.002	0.021	0.019*			
	(0.02)	(0.022)	(0.011)			
Man-to-Woman Transitions						
5+ years before	0.111**	0.069	-0.042	0.121**	0.087	-0.035
	(0.05)	(0.052)	(0.046)	(0.054)	(0.061)	(0.047)
2-5 years before	0.031	-0.009	-0.04	0.057	0.015	-0.042
	(0.038)	(0.038)	(0.043)	(0.042)	(0.044)	(0.045)
1-5 years after	-0.032	0.043	0.075	0.024	0.089*	0.065
	(0.05)	(0.059)	(0.053)	(0.04)	(0.051)	(0.049)
6+ years after	0.029	0.125*	0.096	0.026	0.103	0.077
	(0.061)	(0.068)	(0.059)	(0.061)	(0.068)	(0.059)
Woman-to-Man Transitions						
5+ years before	-0.055	0.036	0.091	-0.045	0.053	0.098
	(0.063)	(0.08)	(0.067)	(0.065)	(0.079)	(0.067)
2-5 years before	-0.023	-0.015	0.008	0.003	0.009	0.006
	(0.042)	(0.037)	(0.051)	(0.044)	(0.038)	(0.051)
1-5 years after	-0.018	-0.022	-0.003	0.038	0.024	-0.013
	(0.062)	(0.065)	(0.061)	(0.055)	(0.06)	(0.059)
6+ years after	0.041	0.1	0.059	0.039	0.079	0.04
	(0.087)	(0.118)	(0.086)	(0.082)	(0.112)	(0.083)
Sample Mean	11.75					
R^2	0.904					
N	18403					

[1] Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Results come from a Poisson regression that includes event-time indicators for periods around three types of chair transition: those in which the gender of the incoming and outgoing chair are the same, versus transitions where a woman replaces a man and those where a man replaces a woman. Effects are pooled in the three periods before and five periods after the transition. Event time is numbered so that the last year of the outgoing chair's term is the last year prior to the transition, the the first year of the new chair's term is the first year "after" the transition, or $t + 0$. Levels are plotted relative to the omitted base period around each type of transition, which is the last year of the outgoing chair's term. To correspond to the multiple-indicators event study format, the variable representing the edges of the event-time window (5+ years prior, $t - 5$ and 6 years after, $t + 5$) contain count variables of the number of transitions outside the event-window in either direction.

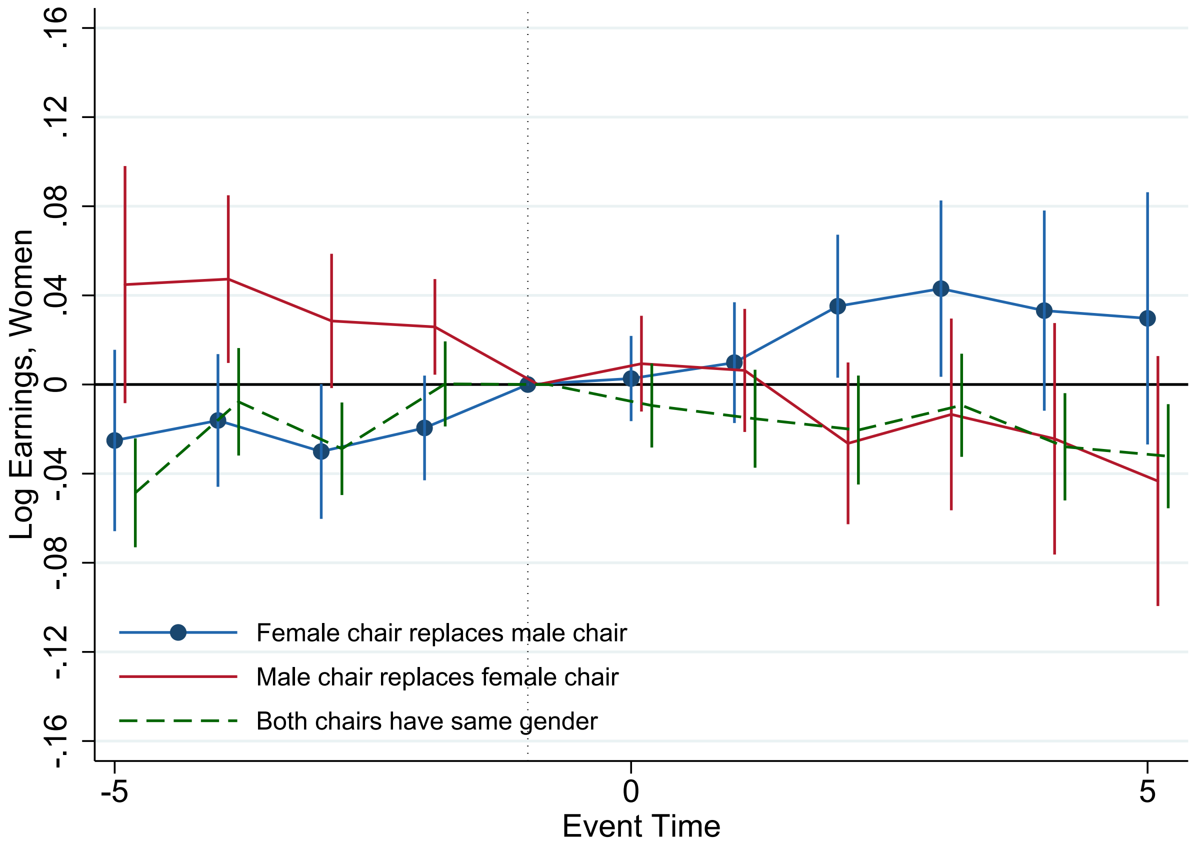
Table 7: Gender gaps in ability correlates do not shrink among students admitted by women

Dep. Var.	Top College	College Selectivity	Academic Rsch Job	Predicted Salary
$Female_i$	0.0173***	0.1412***	-0.0150***	-3166***
	(0.0052)	(0.0269)	(0.0040)	(598)
$Treat_i$	-0.0026	-0.0636	0.0113	391
	(0.0113)	(0.0586)	(0.0088)	(1353)
$Treat_i * Female_i$	0.0056	0.0653	0.0143	892
	(0.0151)	(0.0784)	(0.0122)	(1881)
Sample Mean	0.303	5.32	0.408	77882
R^2	0.2570	0.3098	0.0989	0.1738
N	29873	28932	49239	39419

* $p < .1$, ** $p < .05$, *** $p < .01$

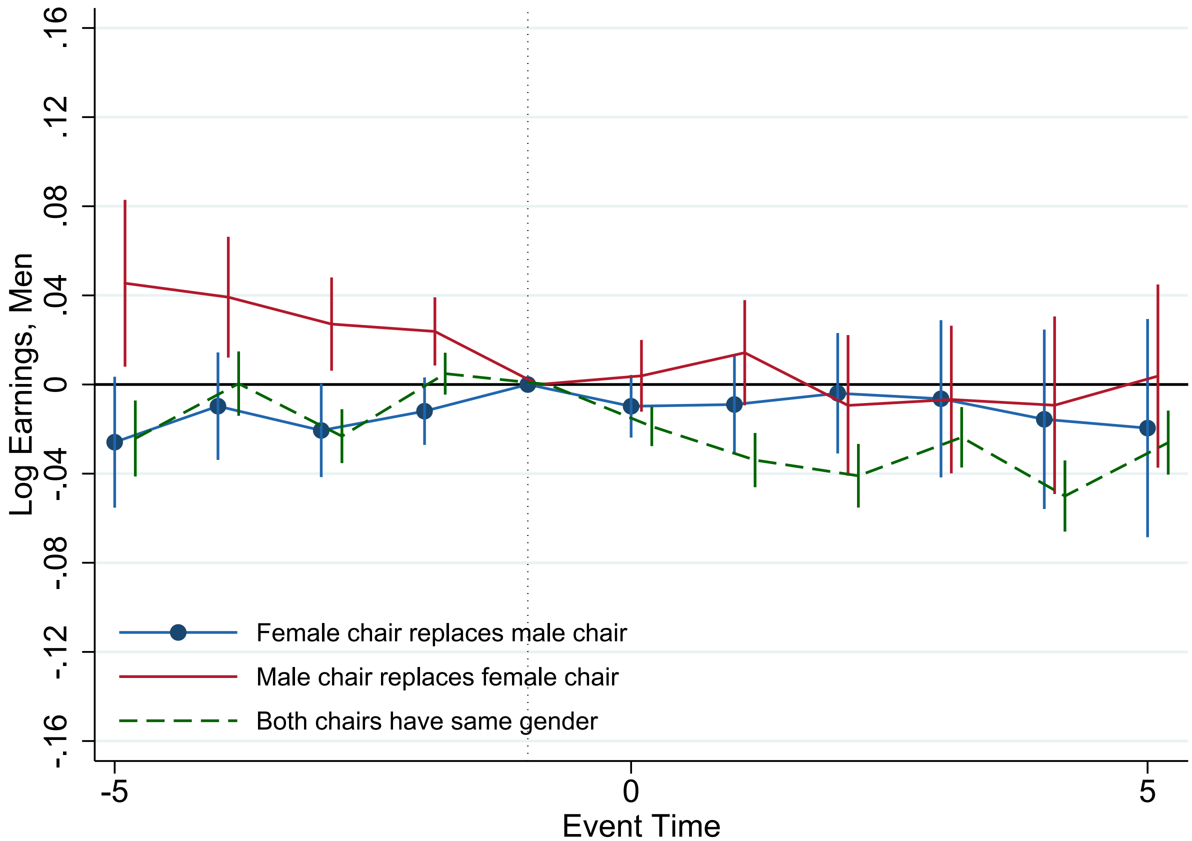
Sample includes PhD-recipients responding to the NSF *Survey of Earned Doctorates* who entered programs in accounting, sociology, economics, and political science between 1987 and 2007. $Treat_i$ indicates an individual begin their PhD program at a time when, in the prior academic year (when admission decisions were likely made) a woman was chairing their PhD department. Linear probability models include subject-specific year and department fixed effects. College selectivity is a ordered categorical variable, listing the selectivity rankings for US undergraduate institutions, from on the 2009 edition of *Barron's Profiles of American Colleges*, with 8 being the most selective institutions and 1 being the least selective. "Top College" indicates whether a graduate from a US college received their bachelor's degree from an institution ranked "most selective" (selectivity ranking 8). Academic research jobs are those affiliated with any academic institution in which research is listed as the first or second main job activity. "Predicted salary" is generated based on PhD program rank, PhD subject, and type of job for the full sample based on questions first asked of graduates in the 2007-2008 academic year.

Figure 1: Event Study – Women's Earnings Around a Chair Transition



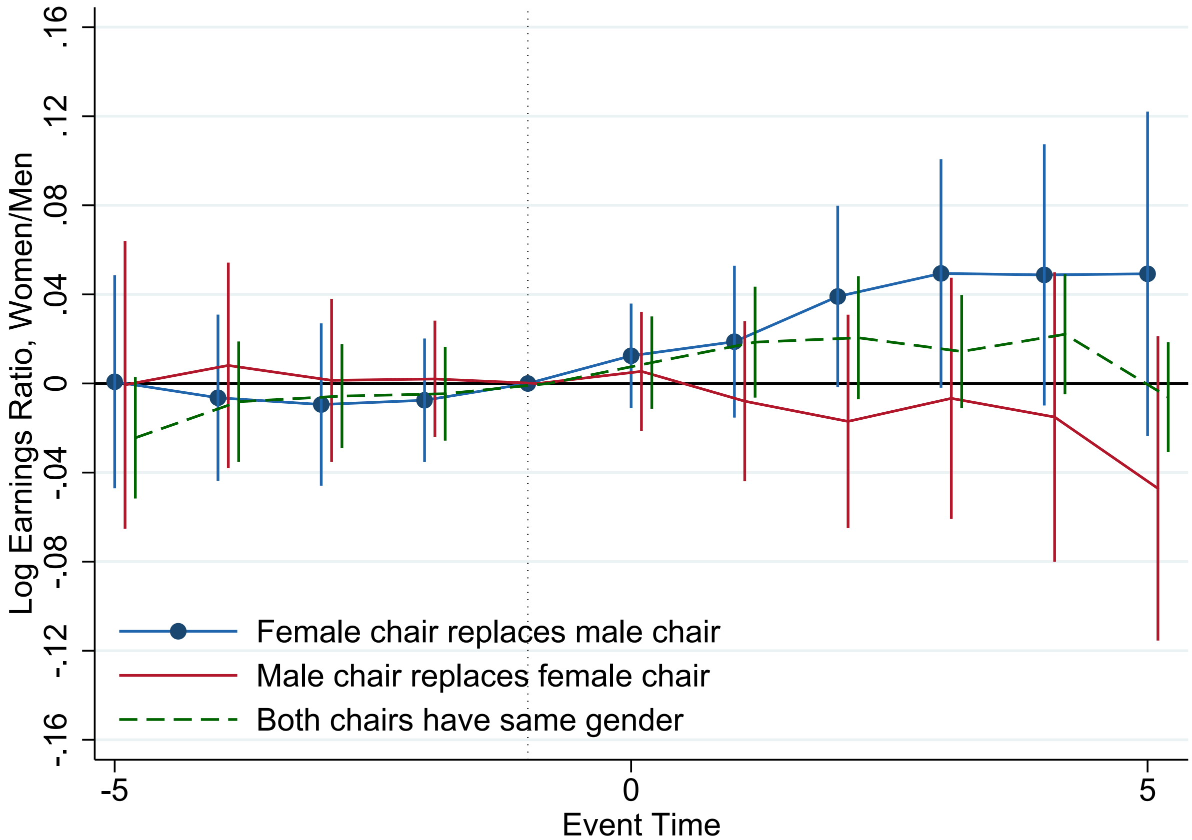
Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the person-year level. The outcome variable is natural log of earnings for female tenure-track faculty, regressed on event-time indicators plus person fixed effects, year fixed effects, an indicator for currently or ever previously worked as a department chair, and a subject-specific quadratic in years since obtaining a PhD. Standard errors clustered at the person level. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline trend in levels for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes individual pay records from doctoral departments in economics, sociology, and political science, plus large accounting departments included in the faculty roster sample, at 39 public R1 and R2 universities. Individuals' first and last year of work at the university are excluded.

Figure 2: Event Study – Men's Earnings Around a Chair Transition



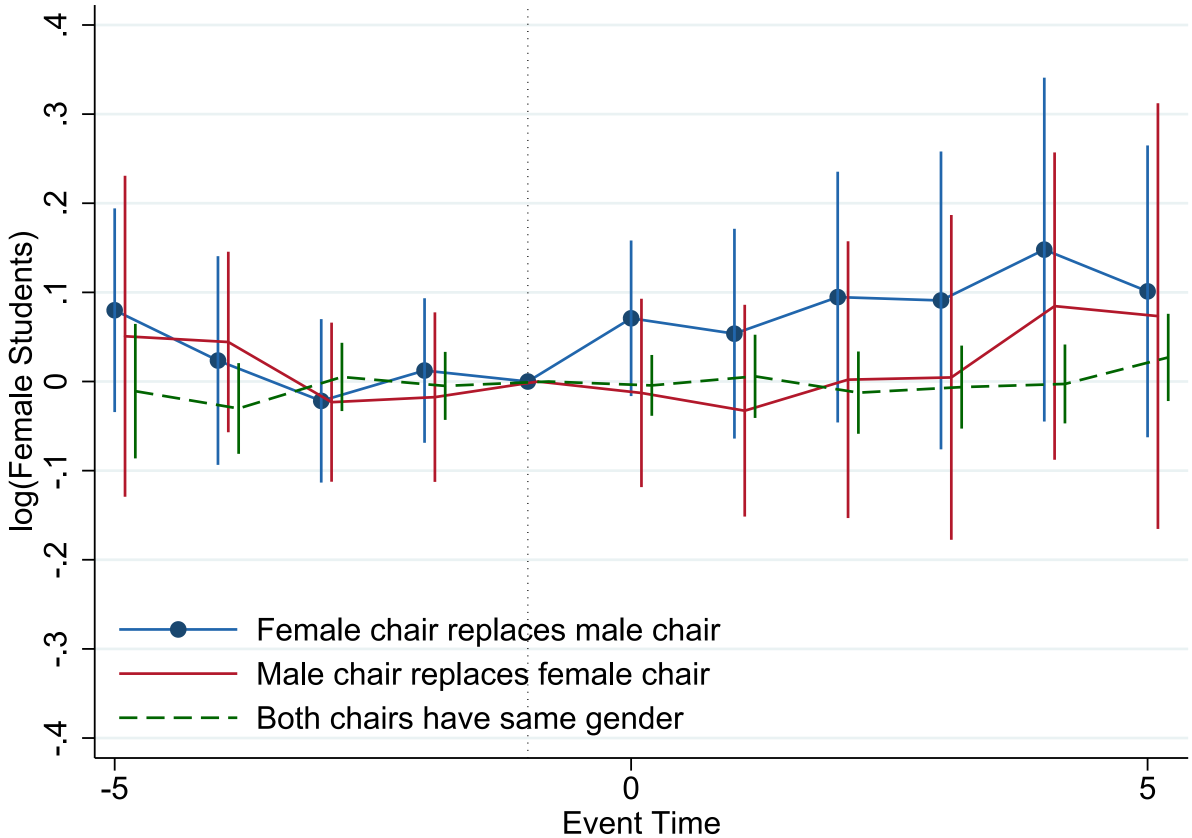
Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the person-year level. The outcome variable is natural log of earnings for male tenure-track faculty, regressed on event-time indicators plus person fixed effects, year fixed effects, an indicator for currently or ever previously worked as a department chair, and a subject-specific quadratic in years since obtaining a PhD. Standard errors clustered at the person level. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline trend in levels for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes individual pay records from doctoral departments in economics, sociology, and political science, plus large accounting departments included in the faculty roster sample, at 39 public R1 and R2 universities. Individuals' first and last year of work at the university are excluded.

Figure 3: Event Study – Difference in Women and Men's Earnings Around a Chair Transition



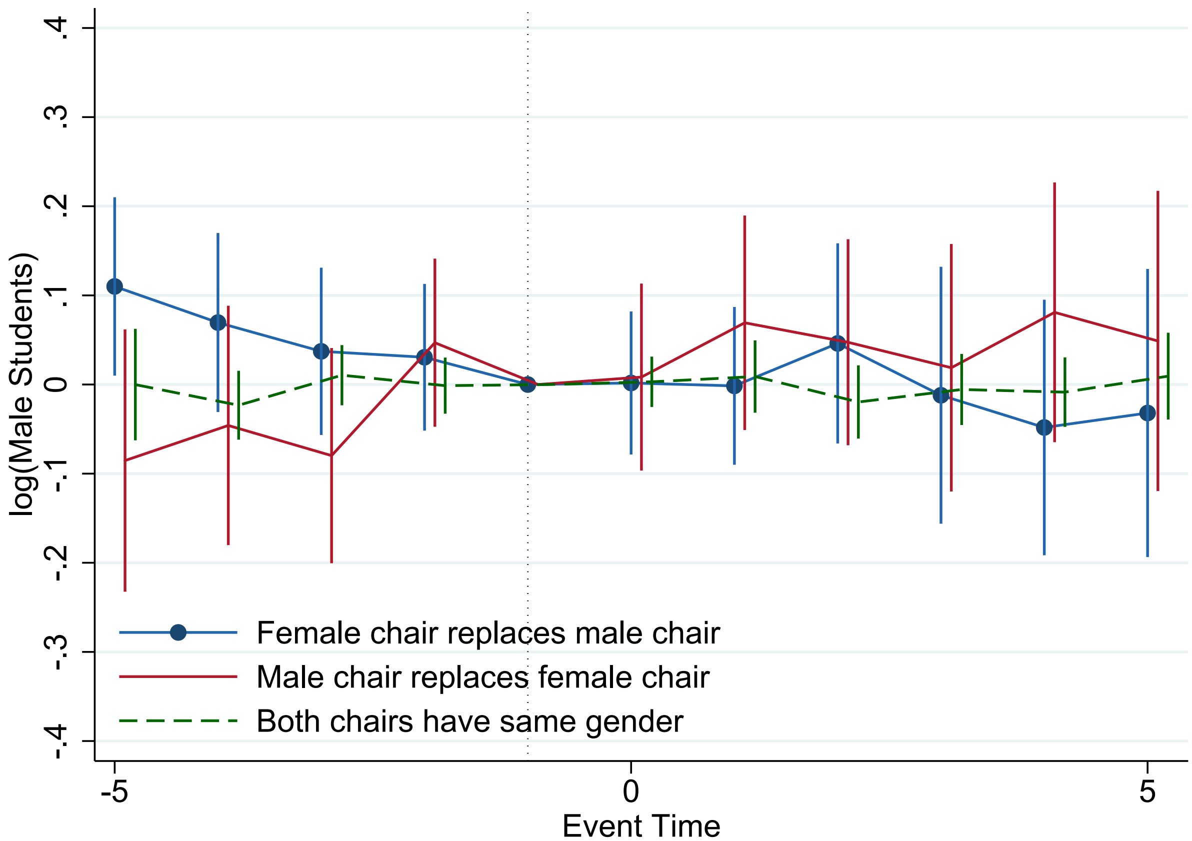
Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the person-year level. The outcome variable is natural log of earnings for tenure-track faculty, regressed on event-time indicators plus person fixed effects, year fixed effects, an indicator for currently or ever previously worked as a department chair, and a subject-specific quadratic in years since obtaining a PhD. Standard errors clustered at the person level. In each case, the coefficients plotted represent the additional effect of event-time indicators on women over and above the impact on men. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes individual pay records from doctoral departments in economics, sociology, and political science, plus large accounting departments included in the faculty roster sample, at 39 public R1 and R2 universities. Individuals' first and last year of work at the university are excluded.

Figure 4: Women in Incoming Graduate Cohorts Around a Chair Transition.



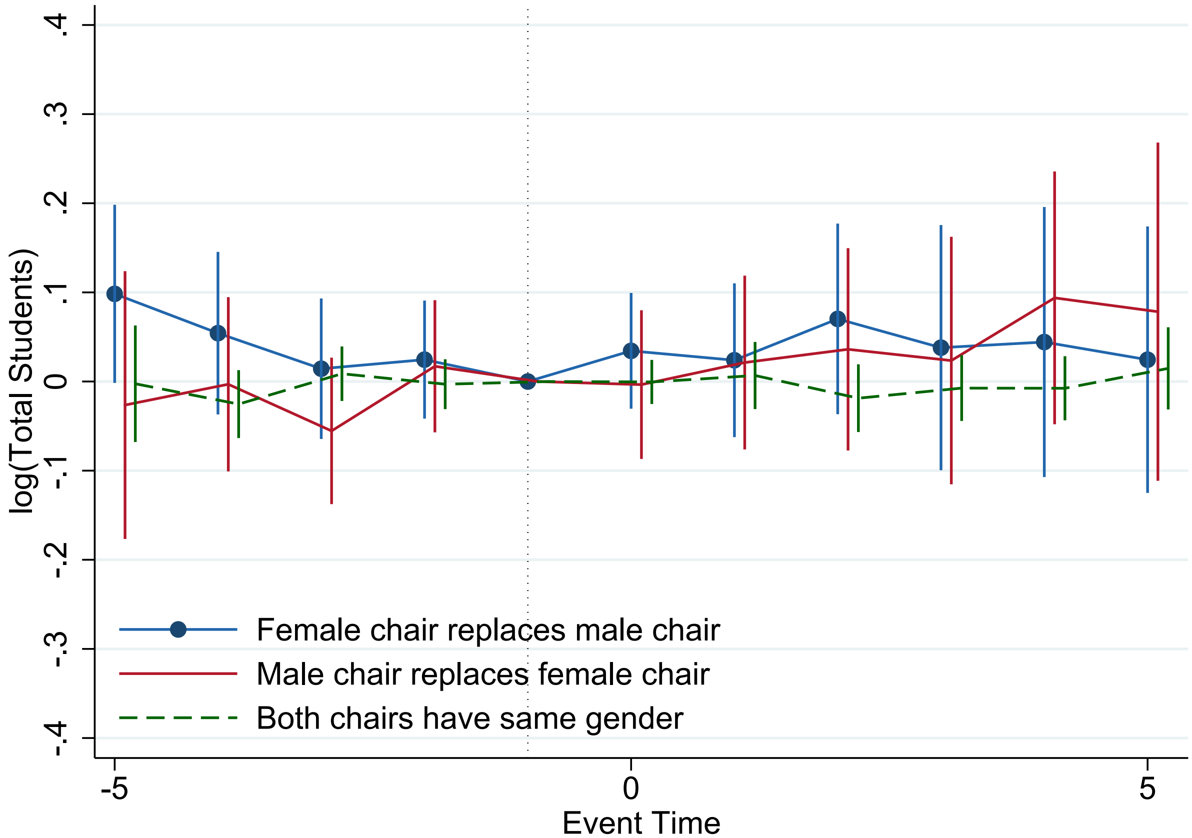
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of female first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. In addition to event-time indicators, each model controls for department and subject-specific year fixed effects. Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent. Standard errors are clustered at the department level. Source: NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering.*

Figure 5: Men in Incoming Graduate Cohorts Around a Chair Transition.



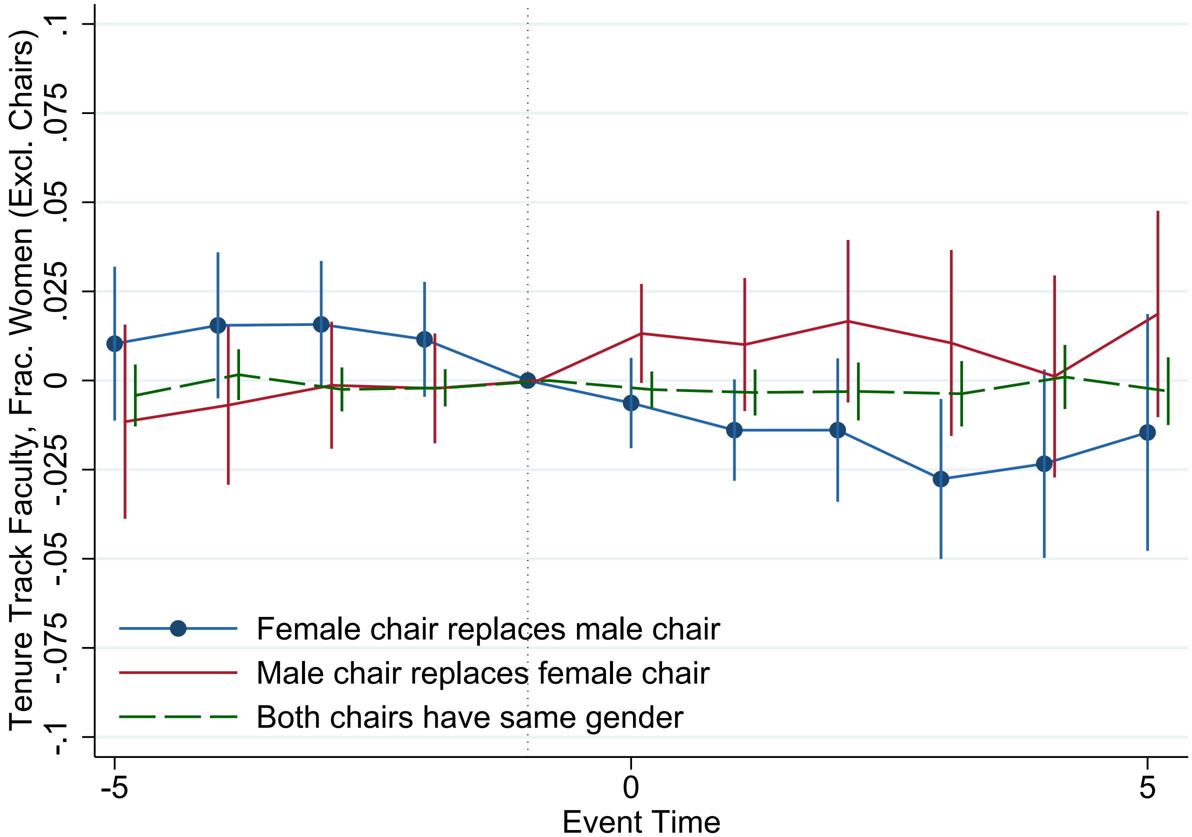
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of male first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. In addition to event-time indicators, each model controls for department and subject-specific year fixed effects. Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent. Standard errors are clustered at the department level. Source: NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering.*

Figure 6: Total Students in Incoming Graduate Cohorts Around a Chair Transition.



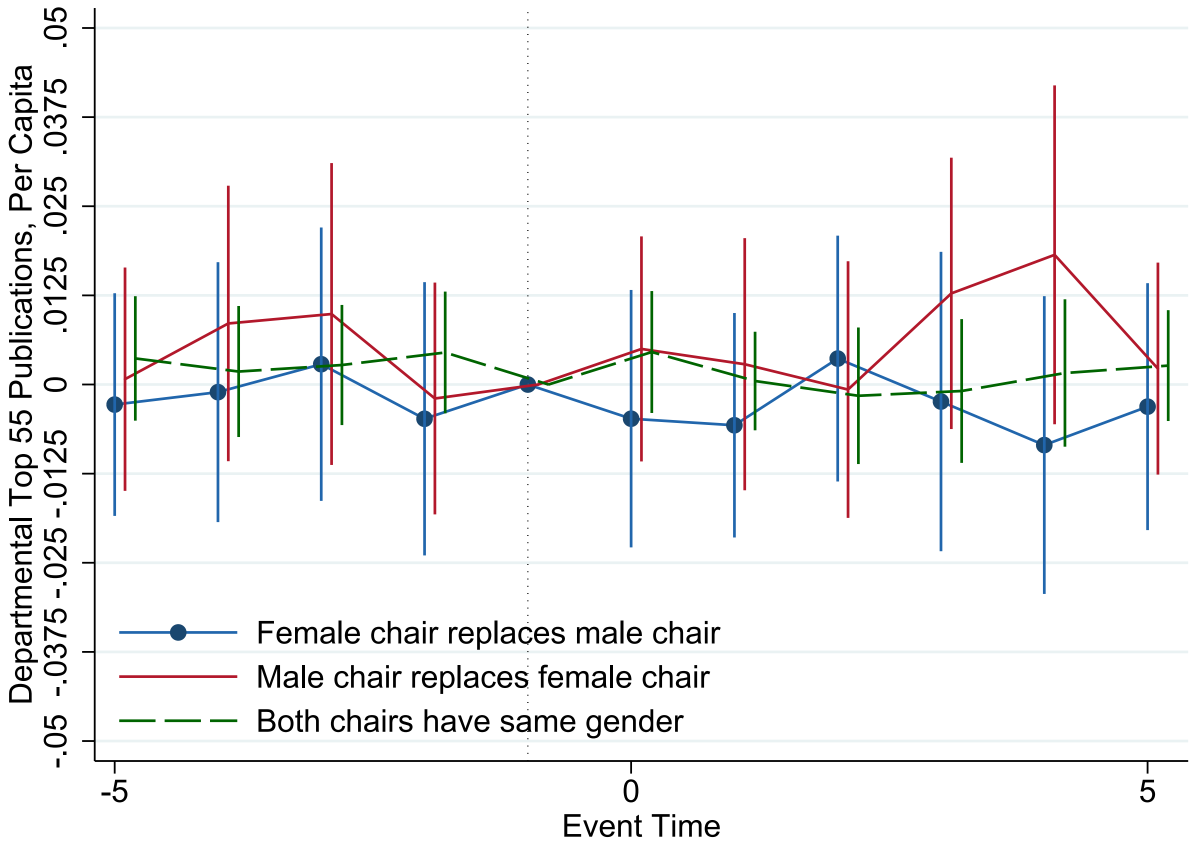
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. In addition to event-time indicators, each model controls for department and subject-specific year fixed effects. Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent. Standard errors are clustered at the department level. Source: NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering*.

Figure 7: Event Study – Faculty Share Female Around a Chair Transition.



Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the department-year level. The outcome variable is the female share of tenure-track faculty, excluding individuals who are ever observed as the department chair, regressed on event-time indicators plus department and subject-specific year fixed effects. Standard errors clustered at the department level. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline trend in levels for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes PhD-granting departments in economics (1994-2017), sociology (1976-2015), and 130 large accounting departments (1974-2016).

Figure 8: Event Study – Total Annual Publications Around a Chair Transition.



Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the department-year level. The outcome variable is the number of papers from top 55 journals in economics or sociology and top 40 in accounting, published by individuals who are uniquely identifiable in the rosters by first initial and last name. Regressions control for department and subject-year fixed effects, and standard errors are clustered at the department level. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline trend in levels for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes PhD-granting departments in economics (1994-2017), sociology (1976-2015), and 130 large accounting departments (1974-2016).

A Appendix Figures and Tables

Table A.1: Early-Career Exposure to Female Chairs Shrinks the Tenure and Publication Gender Gap, Allowing Subject-Year and Department Fixed Effects to Vary by Gender

	Likelihood of Receiving Tenure				Count of Publications	
	First Inst.		Any Inst.			
	Ever (1)	by Year 8 (2)	Ever (3)	by Year 8 (4)	Top 5 (5)	Top 55 (6)
$Exposure_i$	-0.0774	-0.0344	0.0087	0.0423	-0.0295	0.0161
	(0.0516)	(0.0514)	(0.0503)	(0.0511)	(0.0910)	(0.2151)
$Exposure_i * Female_i$	0.0957	0.0672	0.0521	0.0247	0.1083	0.2566
	(0.0681)	(0.0677)	(0.0663)	(0.0692)	(0.1119)	(0.2552)
Sample Mean	0.50	0.40	0.64	0.49	0.41	1.54
Adjusted Gender Gap	-0.05	-0.06	-0.05	-0.08	-0.11	-0.45
R^2	0.2538	0.2705	0.2517	0.2596	0.3873	0.3822
N	6052	6052	6052	6052	5469	5469

[1] Standard errors in parentheses. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Gender gap is unidentified with gender-specific fixed effects. Reference adjusted gap from table 1.

[3] Results are from linear probability models (models 1-4) measuring the likelihood of receiving tenure at one's hiring institution (ever or within eight years, models 1 and 2) or at any sample institution (ever or within eight years, models 3 and 4) and OLS regressions (models 5-6) where the outcome is the total number of papers published within eight years of earnings a PhD in top 5 economics journals (or equivalent top journals in other subjects), or in top 55 journals from one's subject (with the cutoff chosen to include additional research field representation). See the data section for more information on which journals are included in each category. Each observation is an assistant professor hired at least eight years before the end of the sample period. Individuals who are observed fewer than three years are excluded. $Exposure_i$ takes values from 0 to 1 in intervals of $\frac{1}{7}$, measuring the share of years a professor's hiring department was chaired by a woman in years 1-6 of their career plus the year before they started work. Each model also controls for the individual's PhD department, plus hiring department and subject-specific year fixed effects, separately by gender. The sample in columns 5 and 6 is limited to individuals who are uniquely identified by their first initial and last name. Standard errors are clustered at the department level.

Table A.2: Female Chairs Raise Men's Likelihood of Exiting the Department

	Any Exit (1)	Leave Sample (2)	W/in Sample (3)
$Female_i$	0.0015	0.0057***	-0.0041**
	(0.0024)	(0.0017)	(0.0019)
$Treat_{iy}$	0.0075**	0.0023	0.0051***
	(0.0031)	(0.0025)	(0.0019)
$Treat_{iy} * Female_i$	-0.0081*	-0.0020	-0.0060**
	(0.0048)	(0.0038)	(0.0030)
Sample Mean	0.091	0.063	0.028
R^2	0.0372	0.0391	0.0257
N	157605	157605	157605

[1] * $p < .1$, ** $p < .05$, *** $p < .01$

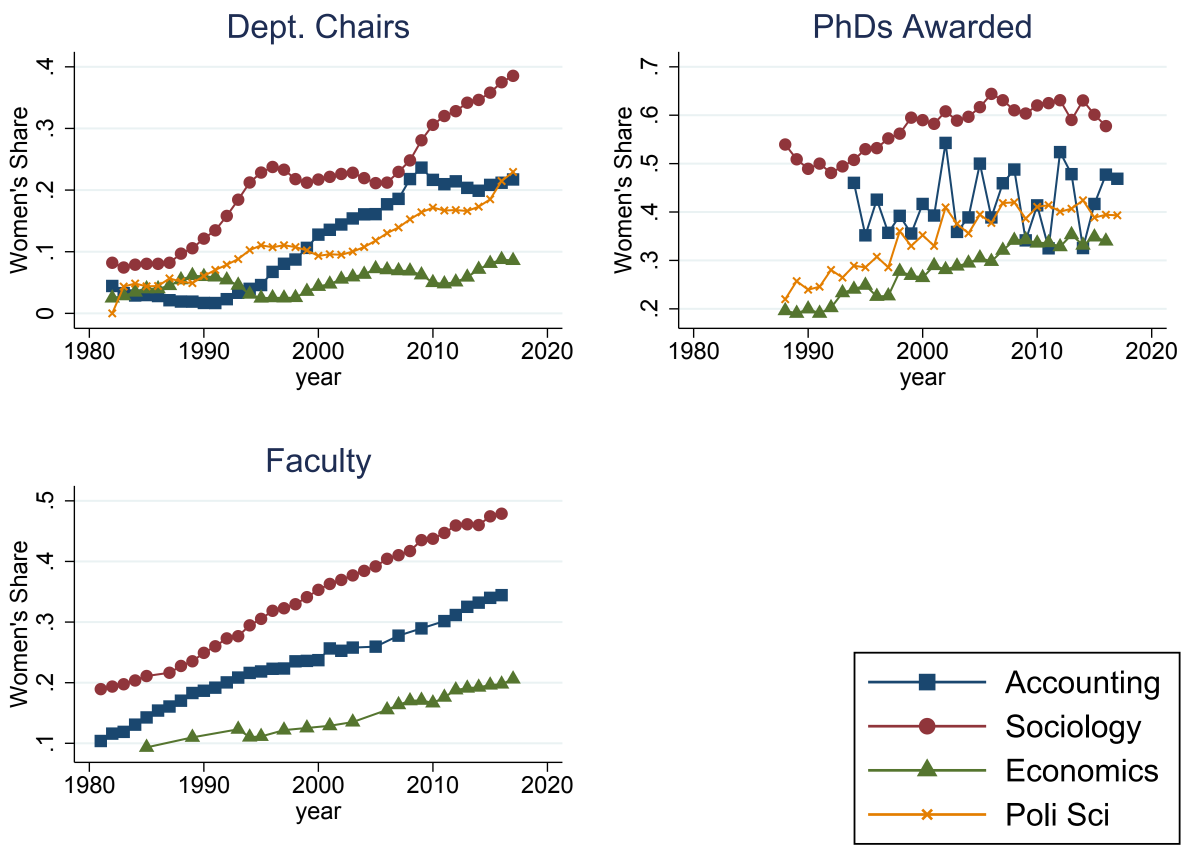
[2] Coefficients and 95% confidence intervals are plotted from linear probability models that control for department and subject-specific year and rank fixed effects, a third-order polynomial in time since receiving a PhD, and indicators for the chairperson or having been chair in the past five years. Sample includes all person-years where the associated department is observed in the next academic year. Treated person-years are those in which the individual's department is chaired by a woman. The outcome variables are set to 1 if the individual is not observed in the same department in the following year (column 1). In columns 2 and 3, those exiting their departments are further partitioned into those observed elsewhere in the sample and those who leave the sample altogether. Standard errors are clustered at the person level.

Table A.3: Difference-in-Differences Impact on Hiring of Men Versus Women, Excluding Chairs

	Hires by Rank & Gender		
	(1)	(2)	(3)
	Overall	Juniors	Seniors
	Unconstrained Effects		
$Treat_{dy}$	0.0308	-0.0188	0.1051
	(0.0457)	(0.0591)	(0.0777)
$Treat_{dy}$*Female Hires	-0.0575	-0.0172	-0.0948
	(0.0681)	(0.0773)	(0.1331)
Sample Mean	0.65	0.45	0.20
N	18328	18328	18328
	Effects Controlling for Total Hires		
$Treat_{dy}$	0.0236	0.0143	-0.0072
	(0.0276)	(0.0352)	(0.044)
$Treat_{dy}$*Female Hires	-0.05	-0.0262	-0.0107
	(0.0658)	(0.0743)	(0.134)
Sample Mean	1.05	0.87	0.74
N	11022	9050	4656

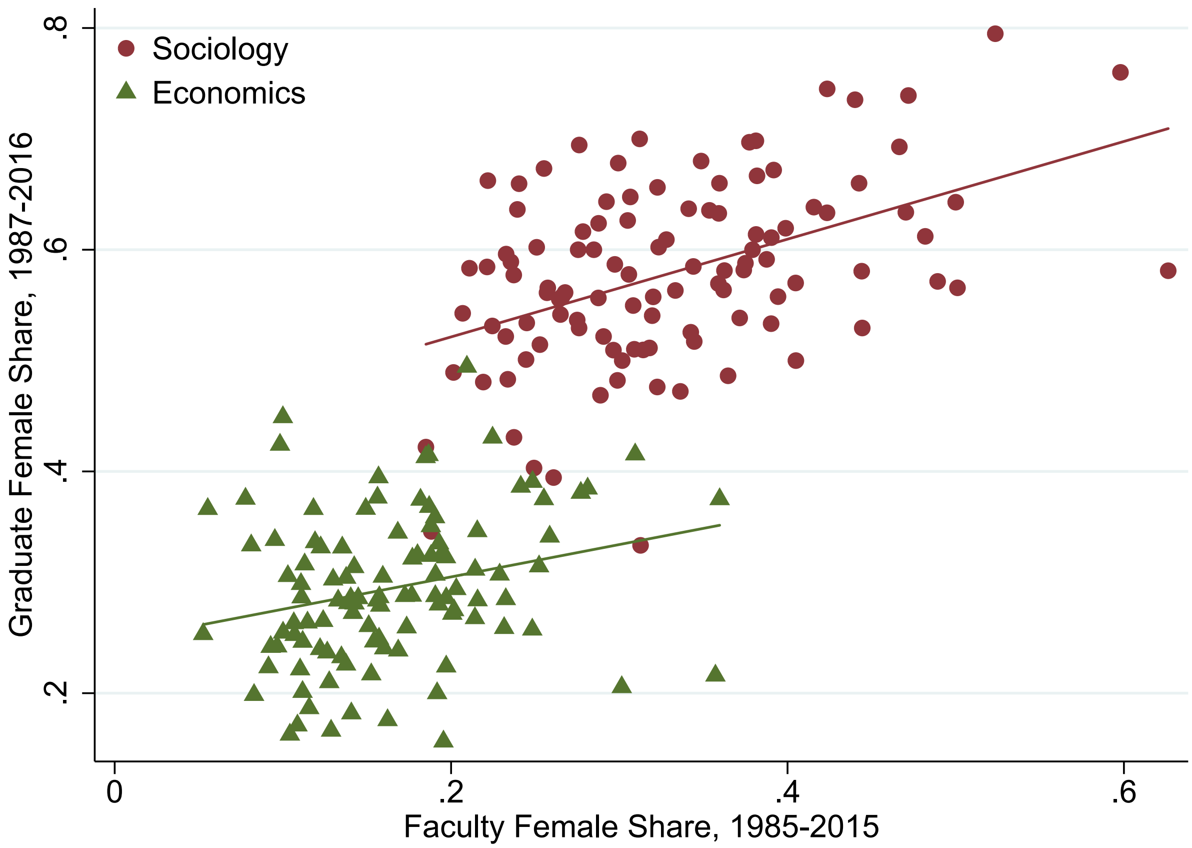
* $p < .1$, ** $p < .05$, *** $p < .01$. Results are from negative binomial regressions, whose outcome variable is a count of total hires of a given rank and gender in a given department and year. Treated observations represent those years in which the department is chaired by a woman. Models control for subject-and-gender-specific year fixed effects and gender-specific department fixed effects. Standard errors are clustered at the department level. Individuals who are ever observed as their department's chair are excluded from the count to avoid mechanical effects coefficients. The second panel of data restricts the sample to years with at least one hire of the given rank, for either gender. Negative binomial coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent, all else equal.

Figure A.1: Women's Share of Department Chairs, Graduating PhD Recipients, and Tenure-Track Faculty. Source: Author's calculation using faculty rosters.



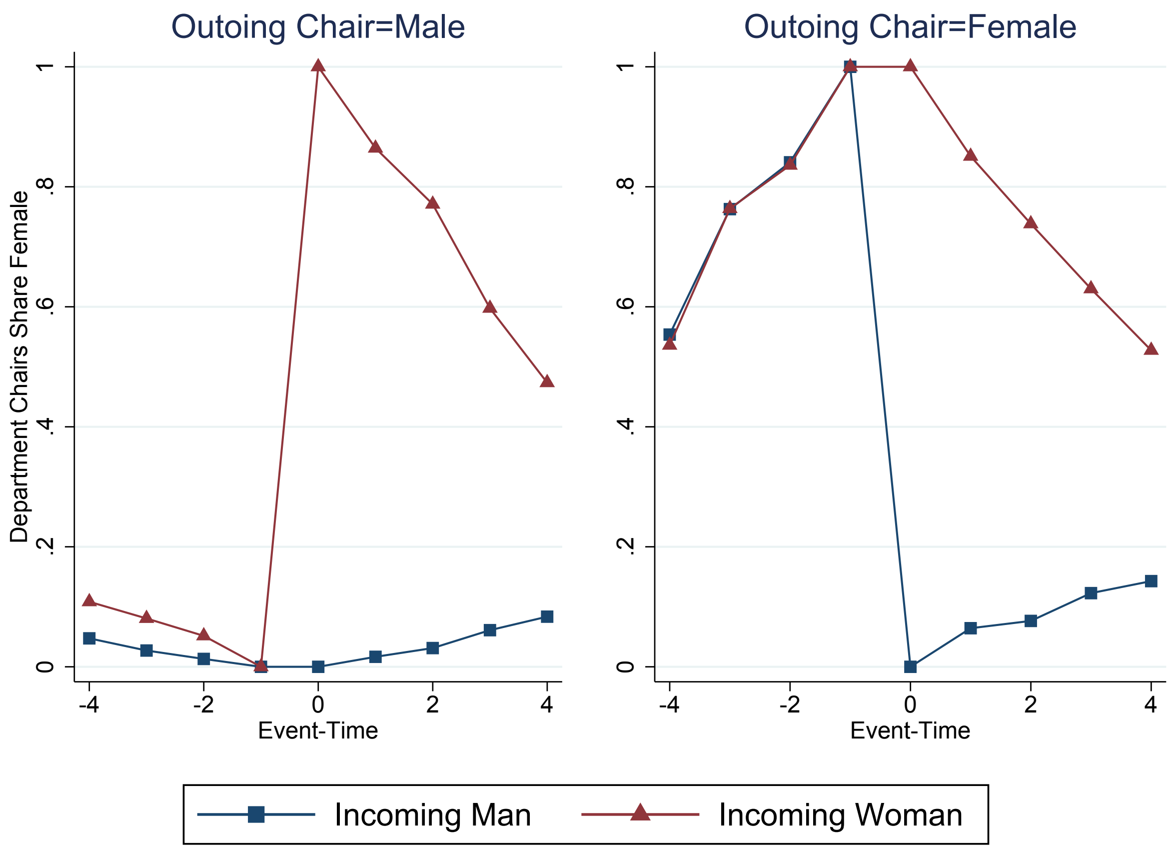
Source: Author's list of department chairs and faculty roster database (Faculty), IPEDS PhD count by field and institution (PhDs). Department chair series is a 5-year moving average to reduce volatility. Faculty roster data are unavailable for political science.

Figure A.2: Women's Share of Faculty and Students by Department



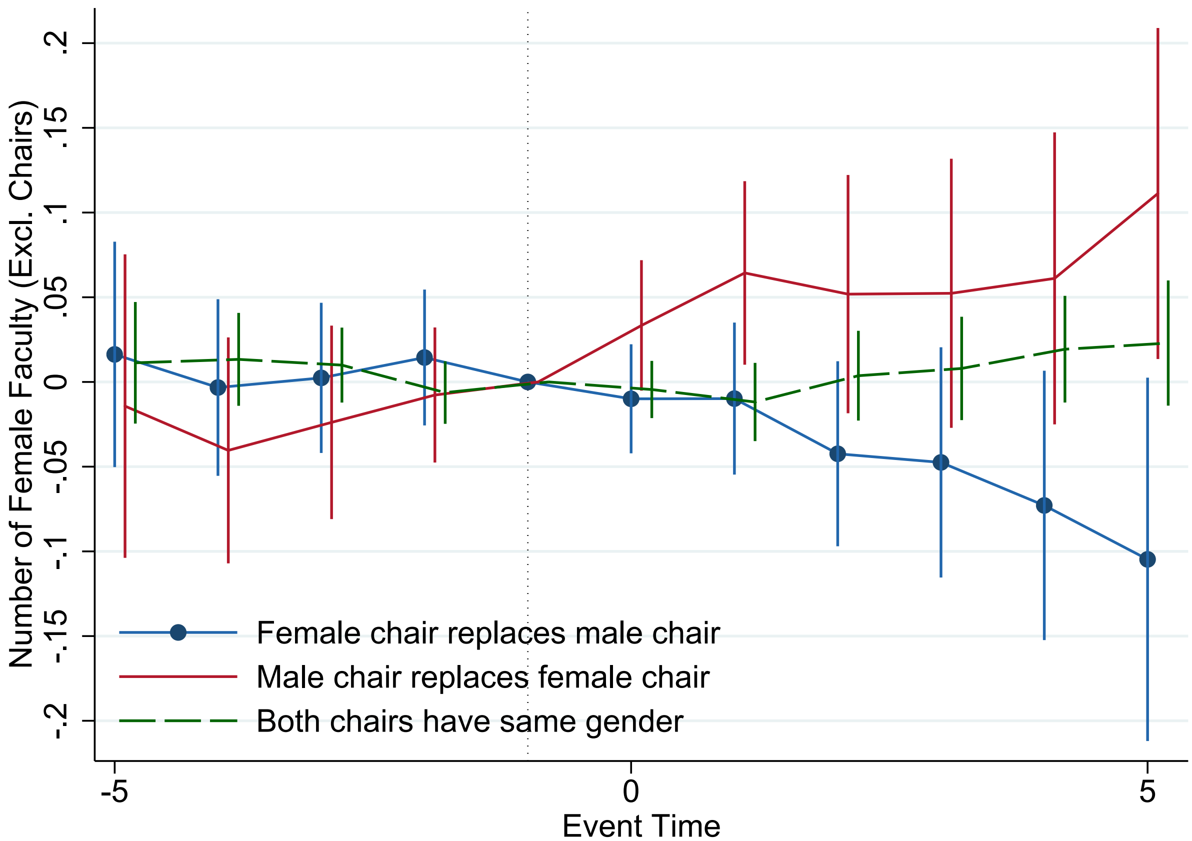
Source: Author's faculty roster database and IPEDS. Departments shown are limited to those with at least fifty PhD graduates since 1987. Accounting PhD departments are excluded because of their small size (only nine meet the size requirements), and Political Science departments are excluded because faculty roster data are unavailable.

Figure A.3: Department Chair Share Female around Four Types of Chair Transition



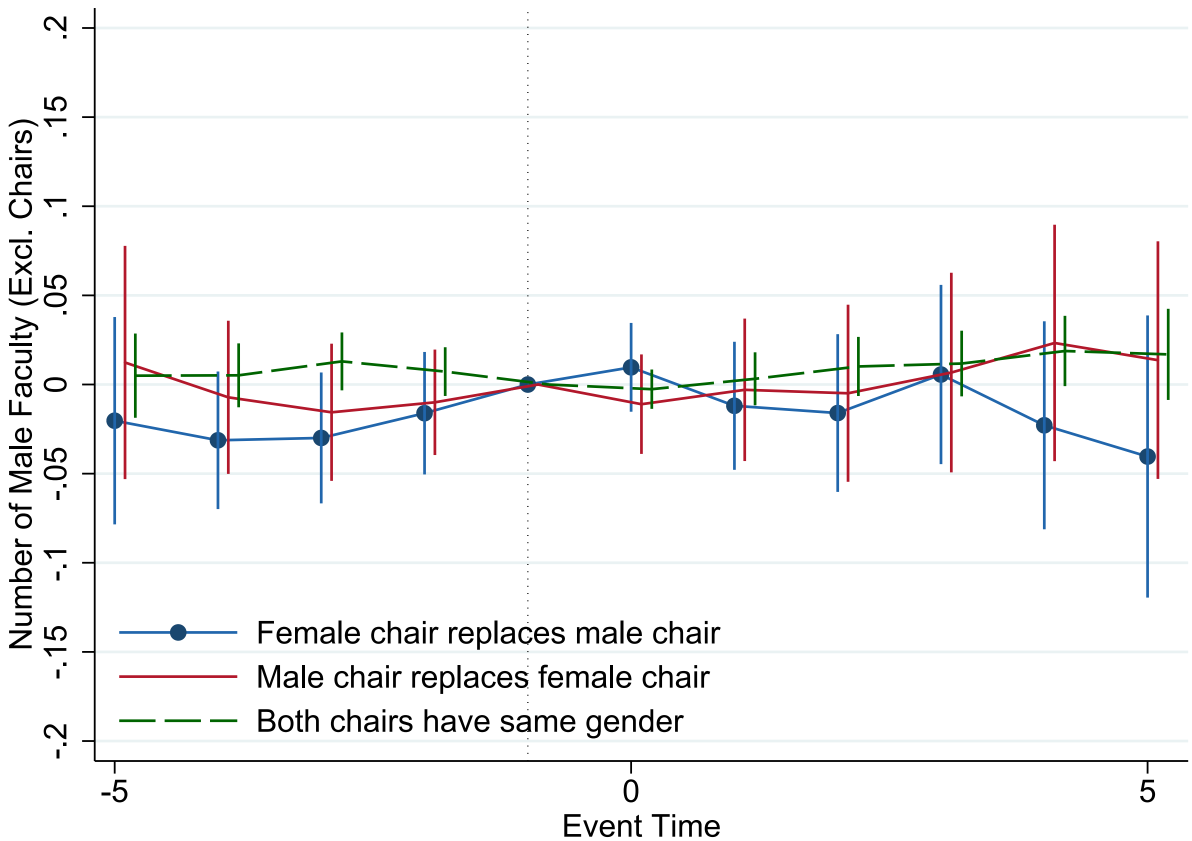
Figures show the share of female chairs at departments of economics, sociology, accounting, or political science experiencing one of four types of transition between two department chairs. Fractions in period -1 and 0 (the last year of the outgoing chair's term and first year of the incoming chair's term) are either 0 or 1 by construction.

Figure A.4: Event Study – Total Number of Female Faculty Around a Chair Transition.



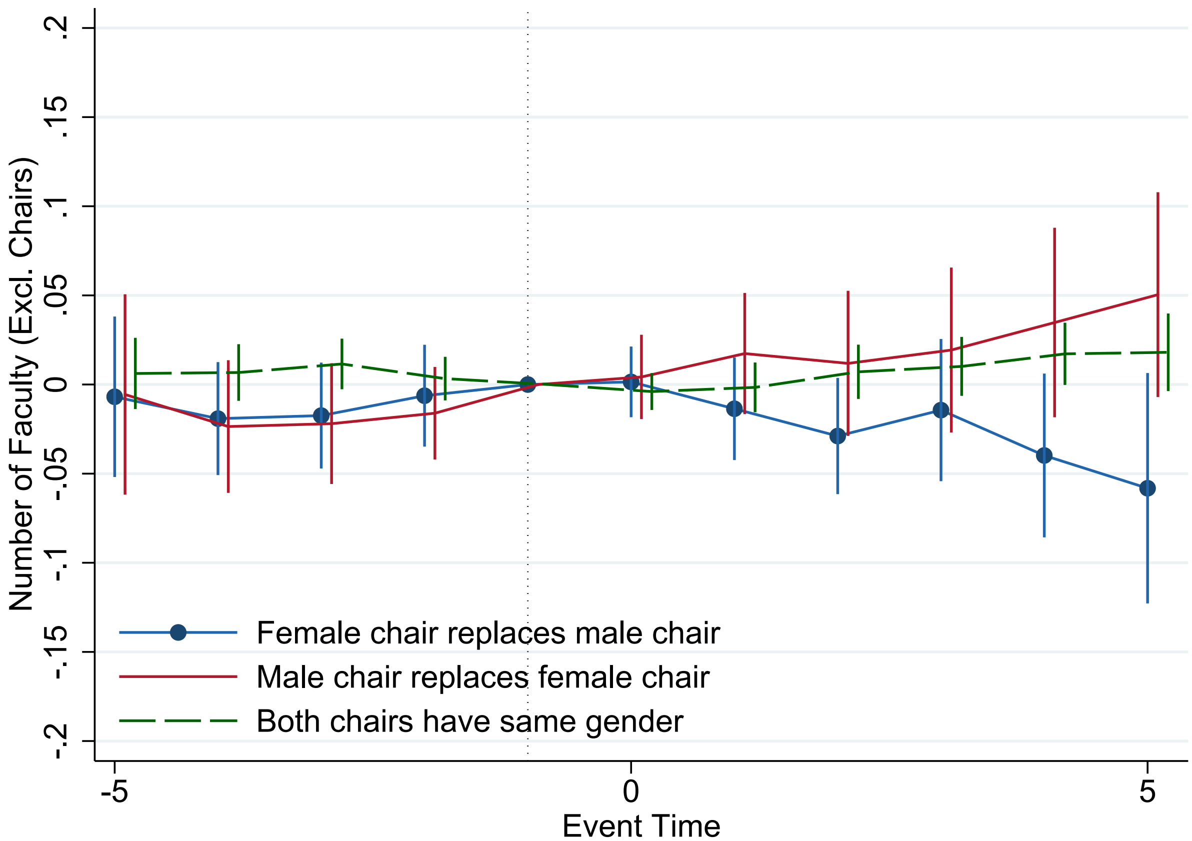
Coefficients and 95% confidence intervals are plotted with from an event study Poisson regression on observations at the department-year level. The outcome variable is the total number of female tenure-track faculty, excluding individuals who are ever observed as the department chair, regressed on event-time indicators plus department and subject-specific year fixed effects. Standard errors clustered at the department level. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline trend in levels for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes PhD-granting departments in economics (1994-2017), sociology (1976-2015), and 130 large accounting departments (1974-2016). Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent.

Figure A.5: Event Study – Total Number of Male Faculty Around a Chair Transition.



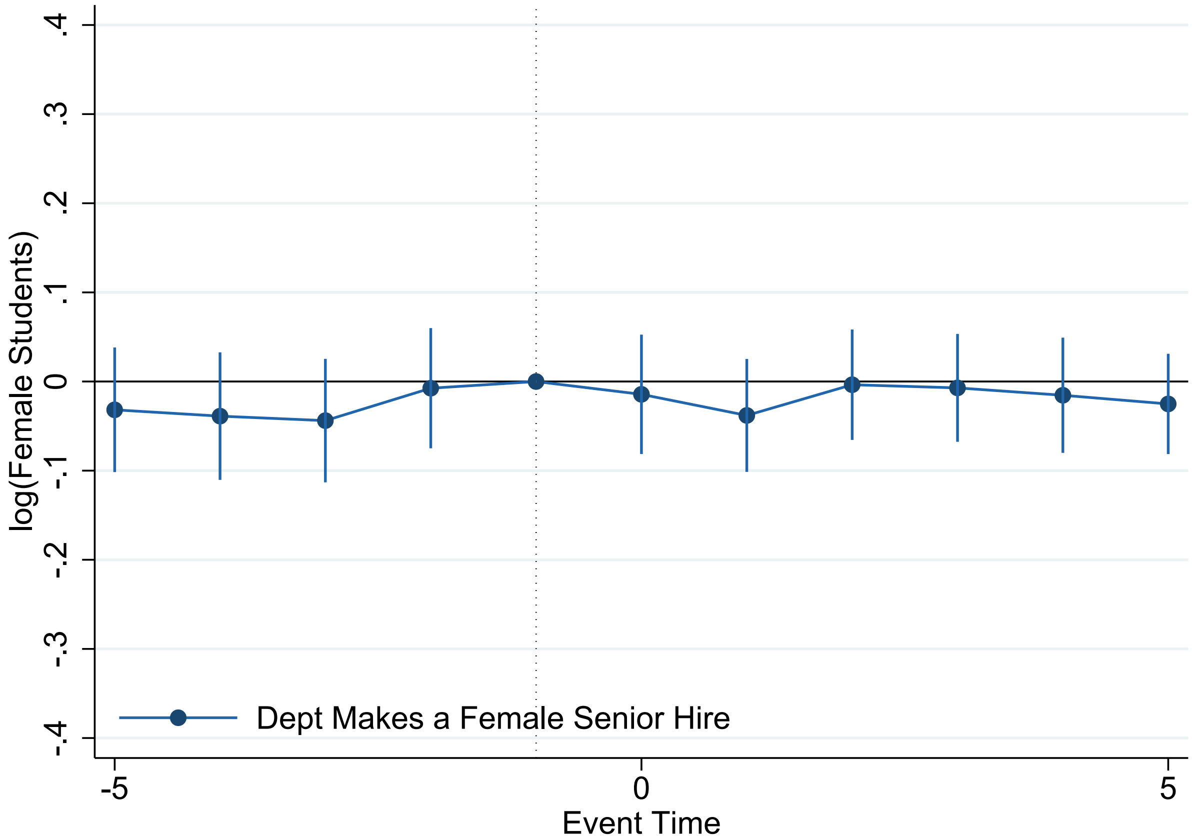
Coefficients and 95% confidence intervals are plotted with from an event study Poisson regression on observations at the department-year level. The outcome variable is the total number of male tenure-track faculty, excluding individuals who are ever observed as the department chair, regressed on event-time indicators plus department and subject-specific year fixed effects. Standard errors clustered at the department level. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline trend in levels for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes PhD-granting departments in economics (1994-2017), sociology (1976-2015), and 130 large accounting departments (1974-2016). Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent.

Figure A.6: Event Study – Total Number of Faculty Around a Chair Transition.



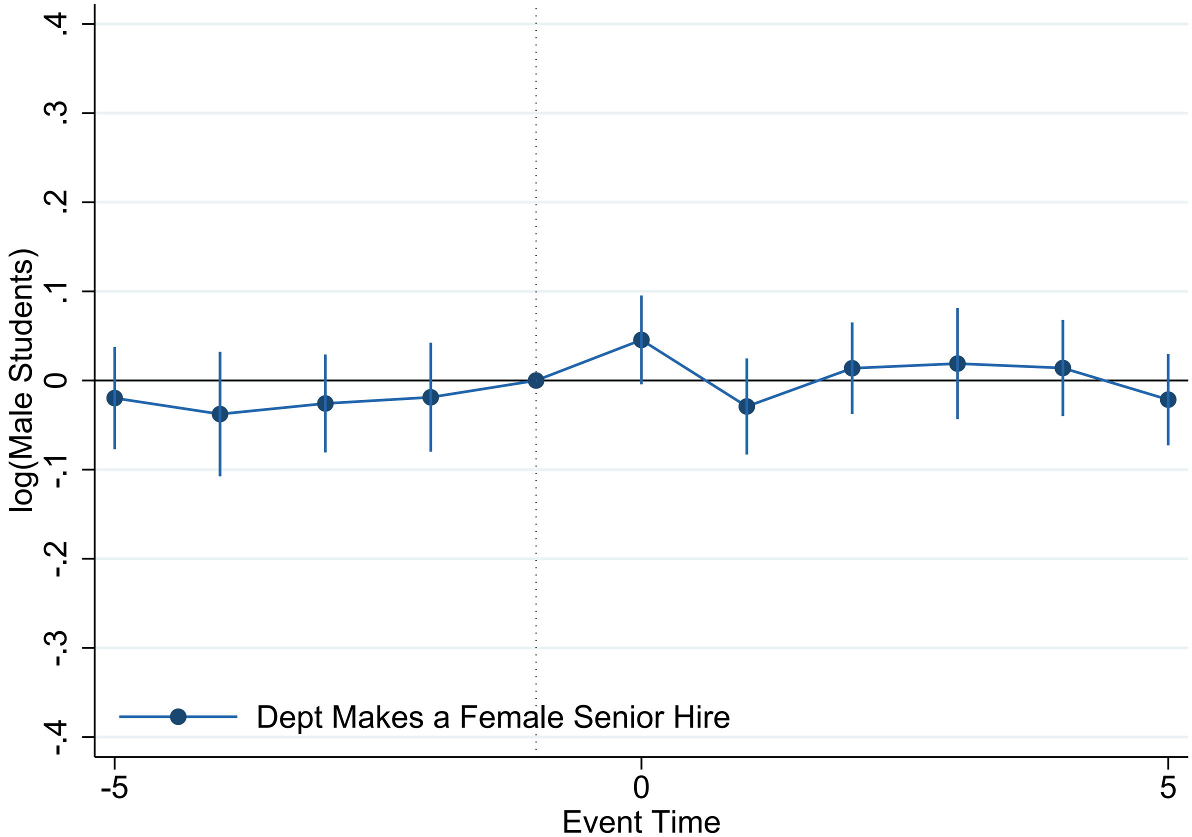
Coefficients and 95% confidence intervals are plotted with from an event study Poisson regression on observations at the department-year level. The outcome variable is the total number of tenure-track faculty, excluding individuals who are ever observed as the department chair, regressed on event-time indicators plus department and subject-specific year fixed effects. Standard errors clustered at the department level. For "treatment" events (i.e. male-to-female or female-to-male chair transitions), the coefficients plotted represent the additional effect of a treatment transition over and above the baseline trend in levels for a gender-static transition. Levels and margins are normalized to their value in the last year of the outgoing chair's term (event time -1). Sample includes PhD-granting departments in economics (1994-2017), sociology (1976-2015), and 130 large accounting departments (1974-2016). Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent.

Figure A.7: Women in Incoming Graduate Cohorts Around a Female Senior Hire.



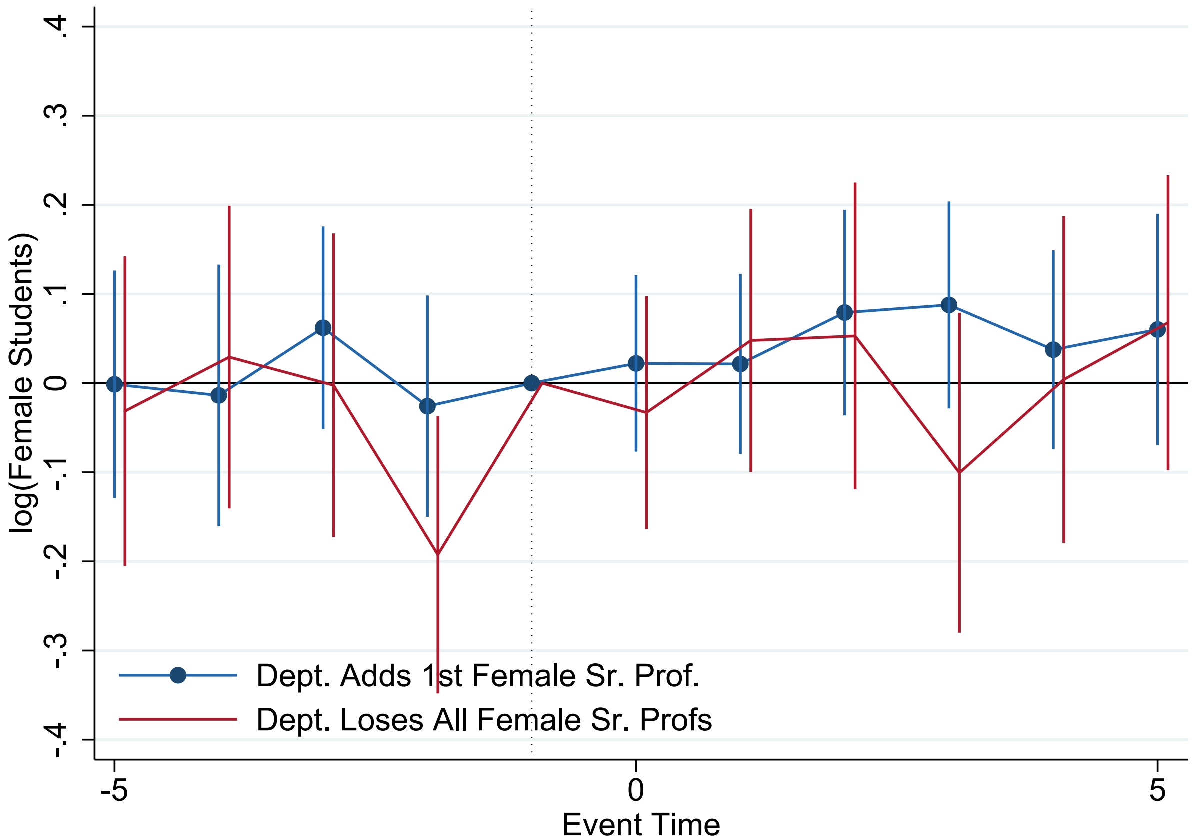
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of female first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. In addition to event-time indicators, each model controls for department and subject-specific year fixed effects. Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent. Standard errors are clustered at the department level. Source: NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering.*

Figure A.8: Men in Incoming Graduate Cohorts Around a Female Senior Hire.



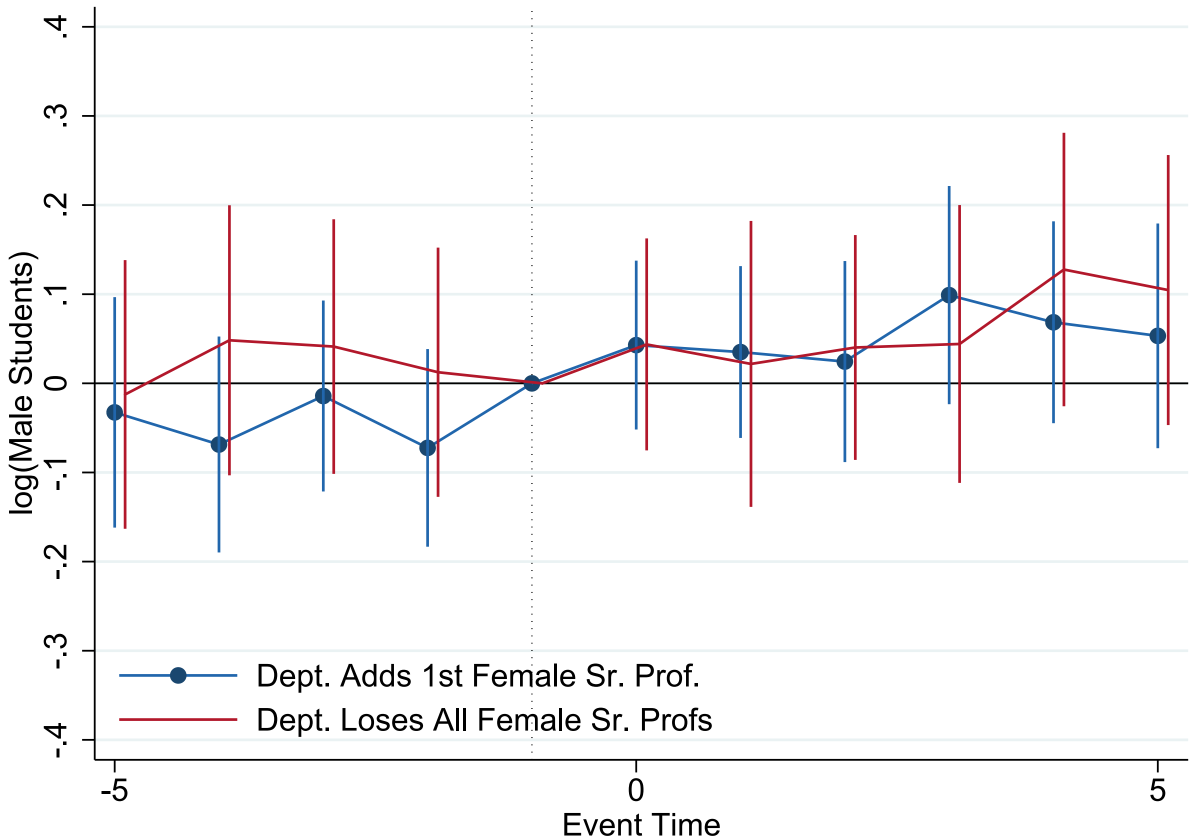
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of male first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. In addition to event-time indicators, each model controls for department and subject-specific year fixed effects. Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent. Standard errors are clustered at the department level. Source: NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering*.

Figure A.9: Women in Incoming Graduate Cohorts Around an Extreme Change in the Number of Female Full Professors.



Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of female first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. In addition to event-time indicators, each model controls for department and subject-specific year fixed effects. Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent. Standard errors are clustered at the department level. Source: NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering*.

Figure A.10: Men in Incoming Graduate Cohorts Around an Extreme Change in the Number of Female Full Professors.



Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of male first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. In addition to event-time indicators, each model controls for department and subject-specific year fixed effects. Poisson coefficients are interpreted in percentage change terms, thus a 0.1 coefficient indicates that a one-unit change in the explanatory variable raises the outcome variable by 10 percent. Standard errors are clustered at the department level. Source: NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering*.