From the holdings of the Department of Special Collections , Princeton University Library, Princeton, NJ

These documents are either in the public domain, or can only be used for educational and research purposes (Fair use) as per U.S. Copyright law (text below). By accessing this file, all users agree that their use falls in the public domain or within fair use as defined by the copyrightlaw. We do not charge any permission or usage fees for the publication of images of material inour collections, including those provided by Princeton University Library, via our website, catalog, or directly from staff.For more information regarding copyright, credit, and citation please review the information onthe Copyright, Credit and Citation Guidelines page of our website.
https://library.princeton.edu/special-collections/policies/copyright-credit-and-citation-guidelines. If you have any questions, including how to order higher-quality images from the collection, contact Special Collections Public Services via our Ask Us! Form. https://library.princeton.edu/special-collections/ask-us

---

Doctoral Dissertations, 2011-2026 Economics

2019

Essays on Gender and the Labor Market

Langan, Andrew Blake

Princeton, NJ : Princeton University

---

http://arks.princeton.edu/ark:/88435/dsp0173666737f

U.S. Copyright law: The copyright law of the United States (Title 17, §108, United States Code) governs the making of photocopies or other reproductions of copyrighted material. Under certain conditions specified in the law, libraries and archives are authorized to furnish a photocopy or other reproduction. One of these specified conditions is that the photocopy or reproduction is not to be used for any purpose other than private study, scholarship, or research. If a user makes a request for, or later uses, a photocopy or reproduction for purposes in excess of fair use that user may be liable for copyright infringement. This institution reserves the right to refuse to accept a copying order if, in its judgment, fulfillment of the order would involve a violation of copyright law. https://www.copyright.gov/title17/title17.pdf

Essays on Gender and the Labor Market

Andrew Blake Langan

A Dissertation

Presented to the Faculty

of Princeton University

in Candidacy for the Degree

of Doctor of Philosophy

Recommended for Acceptance

by the Department of

Economics

Advisers: Alexandre Mas and Leah Boustan

June 2019

Chapters 1 and 3 are copyright © 2019 by Andrew Blake Langan. All rights reserved.

Chapter 2 is copyright © by the American Economics Association; reproduced with permission of the *Journal of Economic Perspectives.*

Abstract

Despite decades of convergence, men and women's outcomes in the labor market remain highly unequal. This dissertation studies different aspects of gender inequality in the labor market, and policies that some firms have implemented that affect disparities between their male and female workers. I focus on tenure track academics in US research universities—a setting I argue generalizes in important respects to the broader high-skill, white-collar workforce, and which allows me to study individual outcomes in depth. I use newly-collected panel data, following faculty in four subjects—economics, political science, sociology, and accounting—from roughly five hundred departments across several decades, linked to their earnings at public universities and research output in top journals.

In chapter 1, I measure how female managers affect gender disparities in their workplaces. Using the timing of transitions between department chairs, I find female chairs reduce gender gaps in publications and tenure for assistant professors, shrink the gender pay gap for faculty by a third, and raise the number of incoming female graduate students by ten percent.

In chapter 2, co-authored with Leah Boustan, we study correlations between economics departments' characteristics like field concentrations and the prevalence of female faculty, and the relative performance of male and female graduates in their job placements and early careers. We conduct in-depth interviews with faculty and alumni from five departments, and synthesize their responses to make recommendations for departments seeking to improve gender equity among their graduates.

In chapter 3, I study the impacts of policies that extend faculty tenure clocks following childbirth or adoption. Recent research on top economics programs finds that gender-neutral policies benefit men while hurting women's odds of earning tenure. Using the same difference-in-differences empirical approach and the same set of institutions as this prior work, but focusing on accounting, political science, and sociology departments, I find policy impacts that are more favorable to women. Subgroup analysis shows effects for women are more

positive, and for men less so, in departments with more senior female faculty or for individuals working under female department chairs.

Acknowledgements

I owe many people a great deal of thanks for their support throughout graduate school and this dissertation process. I thank my advisors, Alexandre Mas and Leah Boustan, whose suggestions greatly improved my research. I thank Alex for showing me how to think about important, compelling questions, for encouraging me to explore new angles on my ideas, and for generously extending financial support to my unending pursuit of new and interesting data. I thank Leah for her close attention, and for her advice that demystified the research process. I also thank David Lee, who as my main supervisor in my time as a research assistant played an integral role in bringing me to Princeton. The student research meetings convened by Leah and David were also of great help in the early stages of my job market paper.

The Industrial Relations section has been my work home for the past seven years. I am grateful for the help from the wonderful staff here, past and present, especially Linda Belfield, Lori Mitrano, Jeannie Moore, and Patti Tracey.

I am grateful to the many advisors and mentors from before my Princeton career who encouraged me along the path of economics: Chuck Moul, for his rollicking intro micro course that first opened my eyes to the joys of the dismal science; Bob LaLonde, whose advice and recommendation was indispensable for my academic preparation and my jump into labor economics; and Betsey Stevenson, who taught me so much about communicating economics to broad audiences, and who was always present with practical advice before and during graduate school. I also thank Paige Shevlin, who as a fellow analyst at the Congressional Budget Office showed me how a true go-getter prepares for grad school, and who nudged me along at other critical junctures. Many other friends and advisors at the Illinois Math and Science Academy, Washington University, the Harris School, and in Washington, DC have nurtured and supported me on my path—there are too many to name here, but they all have my thanks.

I am grateful to the National Science Foundation for their financial support of my graduate study. This dissertation 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.

I am grateful to the many other people whose comments and suggestions greatly improved this research: Anne Case, Janet Currie, Hank Farber, Thomas Fujiwara, Seth Gershenson, Claudia Goldin, Larry Katz, Ilyana Kuziemko, Chris Neilson, Harvey Rosen, Ceci Rouse, and Betsey Stevenson. Craig Olson helped inspire this project during a conversation in summer 2017. Comments from Mallika Thomas inspired the placebo tests that help bolster my findings in chapter 1.

I thank Katie Hawn and LaShonda Spell for excellent assistance with gathering and digitizing the roster data. I also thank Bailey Palmer for her invaluable research assistance, and Leyla Mocan for conducting the interviews for chapter 2.

I thank Steve Mello and Mingyu Chen, my friends throughout our year as RAs and all through graduate school. They have each forgotten more about economics and programming than I will ever know, and I could not have done this without them. I thank all our cohort-mates for their friendship and help, especially Jessica Brown, David Cho, and Julia Fonseca. I also thank David Arnold, Reyhan Ayas, Emily Battaglia, Jiwon Choi, Judd Cramer, Felipe Goncalves, Dan Herbst, Elisa Jacome, Pauline Leung, Inessa Liskovich, Graham McKee, Terry Moon, Maarit Olkkola, Hannah Rubinton, Vivek Sampathkumar, Jakub Schlockermann, Stefanie Seele, and Neel Sukhatme for their friendship and camaraderie.

I thank my entire family, especially my parents Michael and Carolyn Langan. Their support throughout my life and career has given me the confidence I needed to take risks and go new places, knowing I always had a safe place to return to.

Most of all, I thank my wife, Luisa Langan. Her practical attitude and organization have kept me on task during the past seven years. Her love and support carried me through

self-doubt. Our life together gives me perspective every day, and keeps my focus on what truly matters. She has made life's low points not so low, and the high points even higher. I dedicate this volume to our children—I hope that my work will play some small role in improving the world they inherit.

Contents

Abstract . . . iii

Acknowledgements . . . v

List of Tables . . . x

List of Figures . . . xii

1 Female Managers and Gender Disparities: The Case of Academic Department Chairs 1

1.1 Introduction . . . 1

1.2 Data . . . 7

1.3 Methodology . . . 10

1.4 Descriptive Analysis . . . 17

1.5 Discussion . . . 22

1.6 Conclusion . . . 26

2 Variation in Women's Success across PhD Programs in Economics 42

2.1 The Share of Women Graduating from Economics PhD Programs . . . 45

2.2 Correlations with Observable Departmental Attributes . . . 46

2.3 Early Career Outcomes by Gender . . . 47

2.4 Documenting Differences in Relative Outcomes for Women across Departments 49

2.5 Interviews about Why Some Departments Have Better Relative Outcomes for Women . . . 50

2.6 Conclusions 59

3 Tenure Clock Extension Policies **67**

3.1 Introduction 67

3.2 Data 70

3.3 Specification and Results 73

3.4 Conclusion 77

A Appendix Tables and Figures **86**

B Ranking Methodology for Table 2.2 **100**

C Interview script **102**

Bibliography **104**

List of Tables

1 Early-Career Exposure to Female Chairs Shrinks the Tenure and Publication Gender Gap 27

2 The Effect of Exposure to Female Chairs on Tenure and Productivity Does Not Vary with the Gender of the Hiring Chair 28

3 Early-Career Exposure to Female Chairs—Not Any Female Senior Faculty—Shrinks the Tenure and Publication Gender Gap 29

4 Among Faculty, Women Earn Less than Observationally-Similar Men 30

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 31

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 32

7 Gender gaps in ability correlates do not shrink among students admitted by women 33

1 Effect of Department Characteristics on Share Women in Graduating Class, Economics PhDs 65

2 Comparative Success of Women by Program 66

1 Impacts of Tenure Clock Extension Policies on Promotion Likelihood 78

2 Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Including Controls for University-Year Level Covariates 79
3 Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Departments by Initial Share of Women on the Senior Faculty 80
4 Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Individuals who Work Under Female Department Chairs During Years 0-5 After Hiring 81
5 Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Departments by Initial Share of Women on the Senior Faculty (Separately by Subject) 82
6 Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Individuals who Work Under Female Department Chairs During Years 0-5 After Hiring (Separately by Subject) . 83
7 Crosswalking Impacts Calculated with Original ABS Econ Sample to Edited Econ Sample 84
8 Crosswalk from ABS Econ Data to my Edited Econ Data, Eliminating University-Year Controls 85

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 87
2 Female Chairs Raise Men's Likelihood of Exiting the Department 88
3 Difference-in-Differences Impact on Hiring of Men Versus Women, Excluding Chairs 89

List of Figures

1.1 Event Study – Women's Earnings Around a Chair Transition 34
1.2 Event Study – Men's Earnings Around a Chair Transition 35
1.3 Event Study – Difference in Women and Men's Earnings Around a Chair Transition 36
1.4 Women in Incoming Graduate Cohorts Around a Chair Transition. 37
1.5 Men in Incoming Graduate Cohorts Around a Chair Transition. 38
1.6 Total Students in Incoming Graduate Cohorts Around a Chair Transition. . 39
1.7 Event Study – Faculty Share Female Around a Chair Transition. 40
1.8 Event Study – Total Annual Publications Around a Chair Transition. . . . 41

2.1 Share Women PhD Recipients by Department: 1994 − 2005 vs 2006 − 2017 61
2.2 Share Women, PhD Recipients vs. First Years, by Department, Entering Classes of 1994-2012 62
2.3 Job Placement Rates by Gender and Department, Five Sectors 63
2.4 Post-PhD Outcomes for Men and Women by Graduate Department 64

A.1 Women's Share of Department Chairs, Graduating PhD Recipients, and Tenure-Track Faculty. Source: Author's calculation using faculty rosters. . . 90
A.2 Women's Share of Faculty and Students by Department 91
A.3 Department Chair Share Female around Four Types of Chair Transition . . . 92
A.4 Event Study – Total Number of Female Faculty Around a Chair Transition. . 93

A.5 Event Study – Total Number of Male Faculty Around a Chair Transition. . . 94
A.6 Event Study – Total Number of Faculty Around a Chair Transition. 95
A.7 Women in Incoming Graduate Cohorts Around a Female Senior Hire. 96
A.8 Men in Incoming Graduate Cohorts Around a Female Senior Hire. 97
A.9 Women in Incoming Graduate Cohorts Around an Extreme Change in the Number of Female Full Professors. 98
A.10 Men in Incoming Graduate Cohorts Around an Extreme Change in the Number of Female Full Professors. 99

Chapter 1

Female Managers and Gender Disparities: The Case of Academic Department Chairs[1]

1.1 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.[2] 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)

[1] Throughout this chapter 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.

[2] 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.

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.[3]

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 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

[3]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.

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 com-

mittees 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.[4] 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.

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.

[4]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 1.3—does not change the results for either.

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 levelsincluding middle managementon 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 predetermined 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.

1.2 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.[5] Finally, these fields have a comparable range of working environments for faculty and high topical overlap, which makes it more natural 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

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

compiled by third parties using information provided by the departments.[6] When gaps exist in these records, I refer to primary sources, usually individual faculty CVs or department websites accessed using the Wayback Machine.[7]

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 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

[6]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.*

[7]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.

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.[8] 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.[9] 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 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

[8]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.

[9]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.

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.

1.3 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 samewe 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 predetermined, 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.

1.3.1 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.

1.3.2 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.[10]

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

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.

[10]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.

1.3.3 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 settingslike 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 I observe first depends on data availability,

rather than fundamental department changes, and in general I have no reason to call one transition more significant than any other.[11]

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.[12] I will treat

[11] I can still, in principle, choose one event at each institution or a few with non-overlapping windows and analyze them as separate events. I could 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.

[12] 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

all "gender-static" transitions as the control group–in practice, the vast majority of these 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.[13] 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

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.

[13]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.

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 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.[14] 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

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

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.

1.4 Descriptive Analysis

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

1.4.1 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.[15] 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.

[15]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.

This raises the possibility that men and women manage departments in ways that differ, leading to the effects observed here.[16]

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.

1.4.2 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 1.4. 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 1.1-1.3, and Table 1.5 which summarizes the same information.)[17] 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

[16] 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.

[17] 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.

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 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 1.4, 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.

1.4.3 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 1.4). 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.[18]

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

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

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 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.[19] 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

[19]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.

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 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.

1.5 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 popu-

lation 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.

1.5.1 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.

1.5.2 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.

1.5.3 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 roughly with the number graduating if we suppose there are seven cohorts of assistant professors, each with about forty women. Based on Table 1.1, 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 1.1 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.

1.5.4 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.

1.6 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.

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	by Year 8	Ever	by Year 8	Top 5	Top 55
	(1)	(2)	(3)	(4)	(5)	(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 1.1, 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 Institution		Tenure, Any Institution		Publication Count	
	Ever	by Year 8	Ever	by Year 8	Top 5	Top 55
	(1)	(2)	(3)	(4)	(5)	(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 (1-2) or at any sample institution (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 or top 50 journals from one's own subject. 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 1.4, Column 4, but include person-level fixed effects and event-time indicators for periods around three types of chair transition shown. 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 transitionshown. 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.

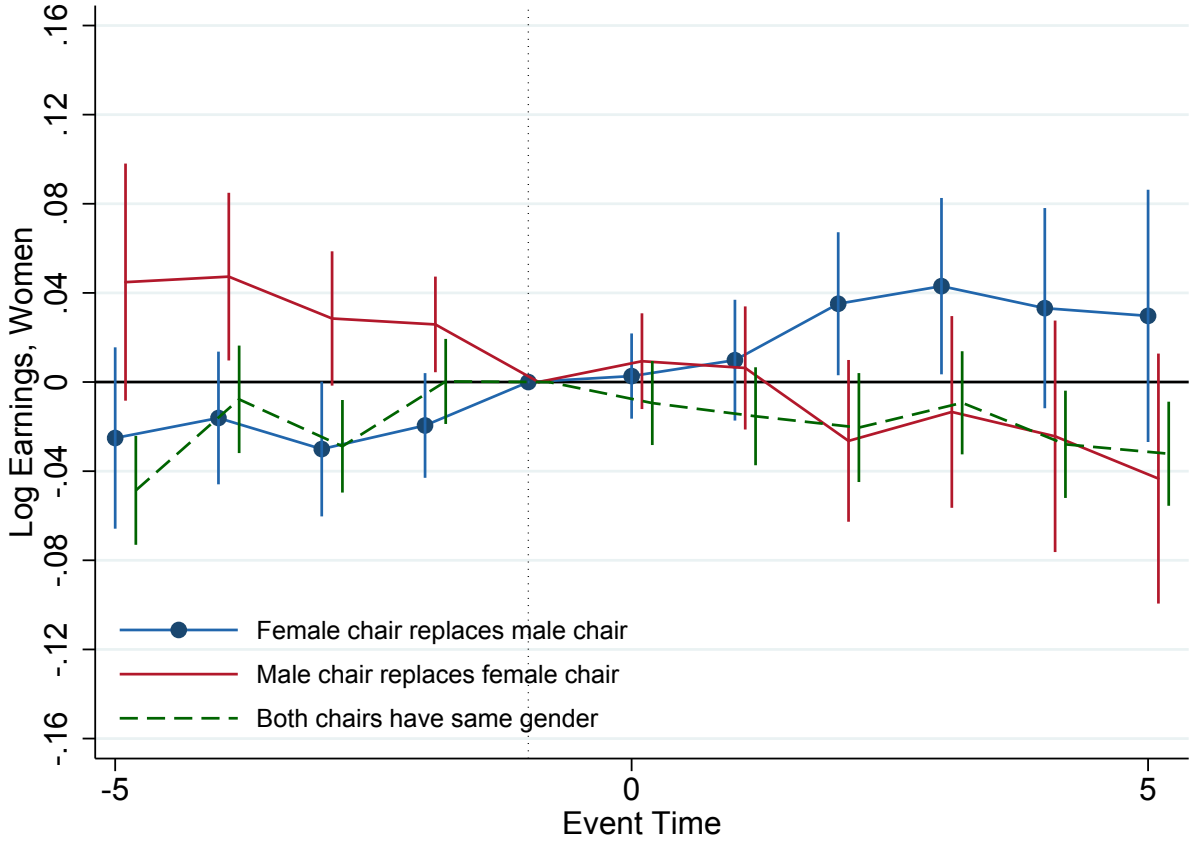
Table 7: Gender gaps in ability correlates do not shrink among students admitted by women

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

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

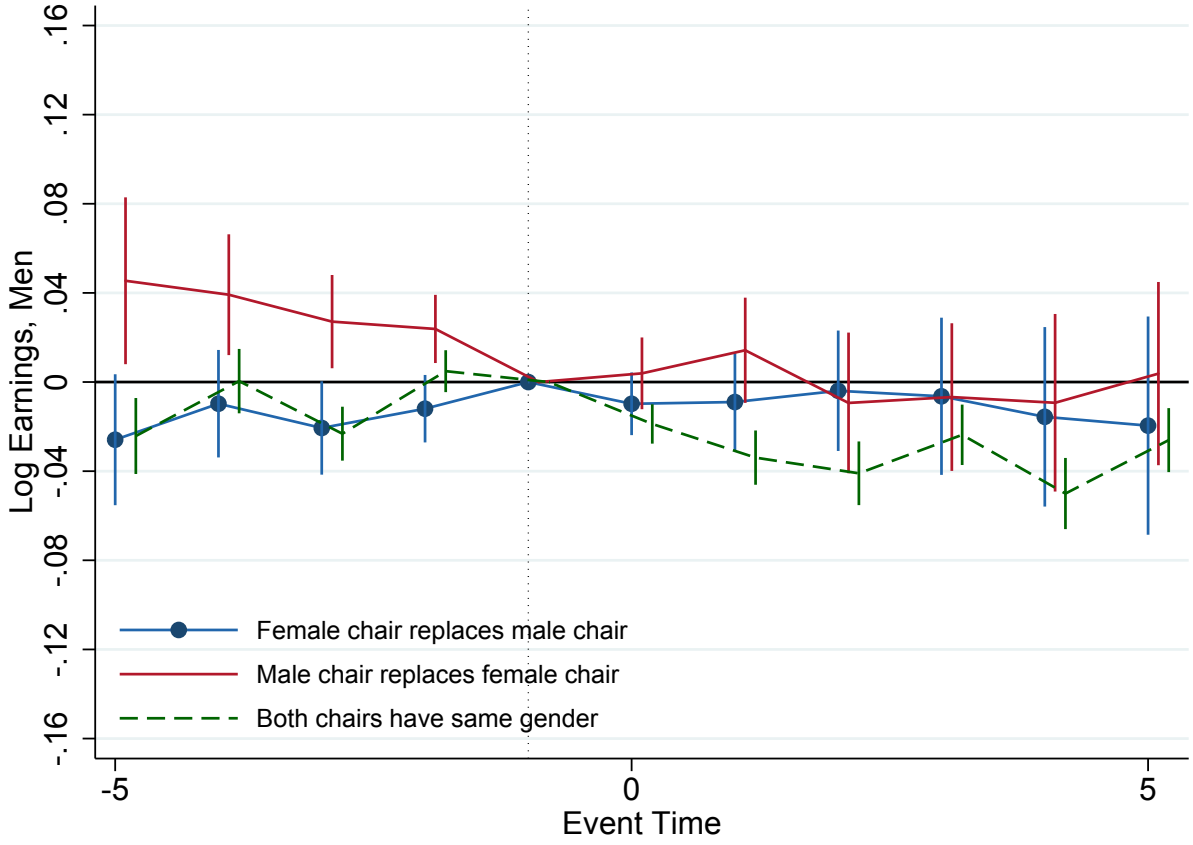
[2] Sample includes PhD-recipients responding to the NSF *Survey of Earned Doctorates* who entered programs in accounting, sociology, economics, and political science between 1987 and 2007. $Treat_i$ indicates an individual begin their PhD program at a time when, in the prior academic year (when admission 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 1.1: Event Study – Women's Earnings Around a Chair Transition



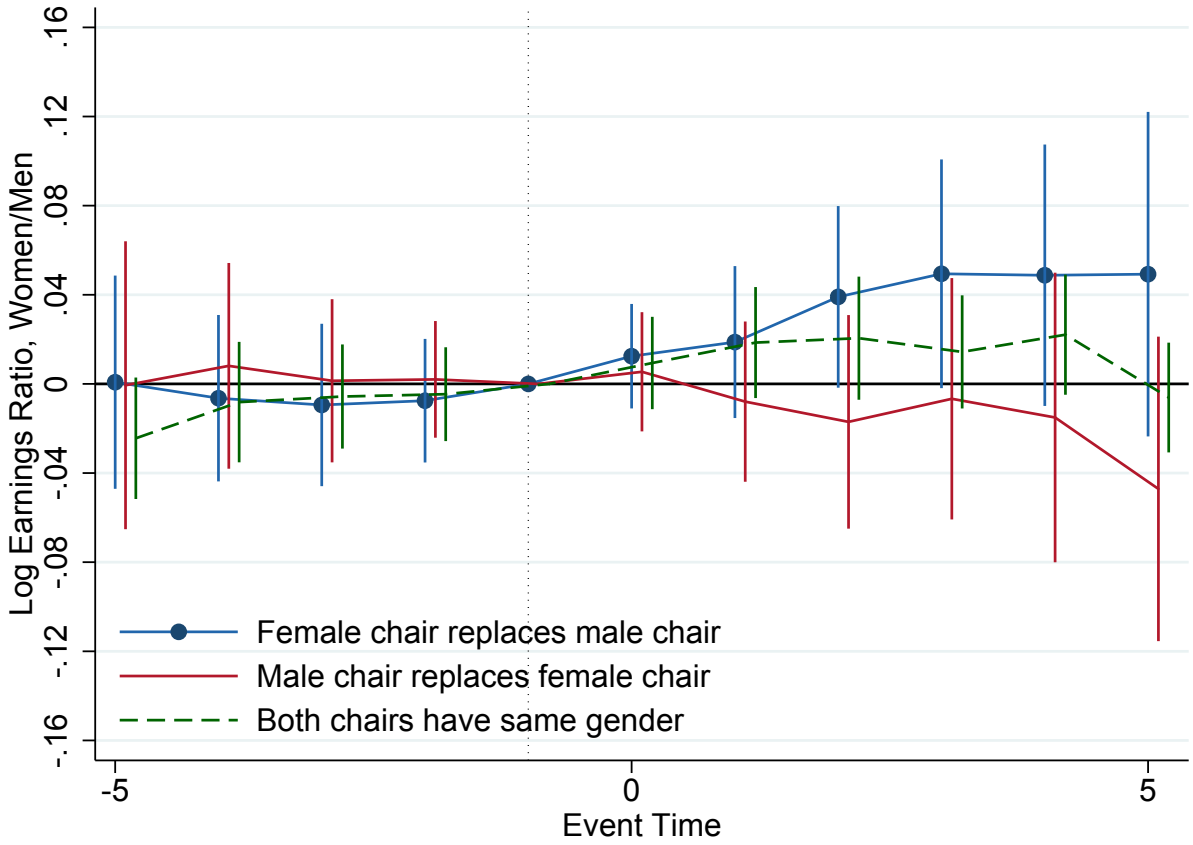
Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the person-year level. The outcome variable is natural log of earnings for female tenure-track faculty, regressed on event-time indicators plus person fixed effects, year fixed effects, an 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 1.2: Event Study – Men's Earnings Around a Chair Transition



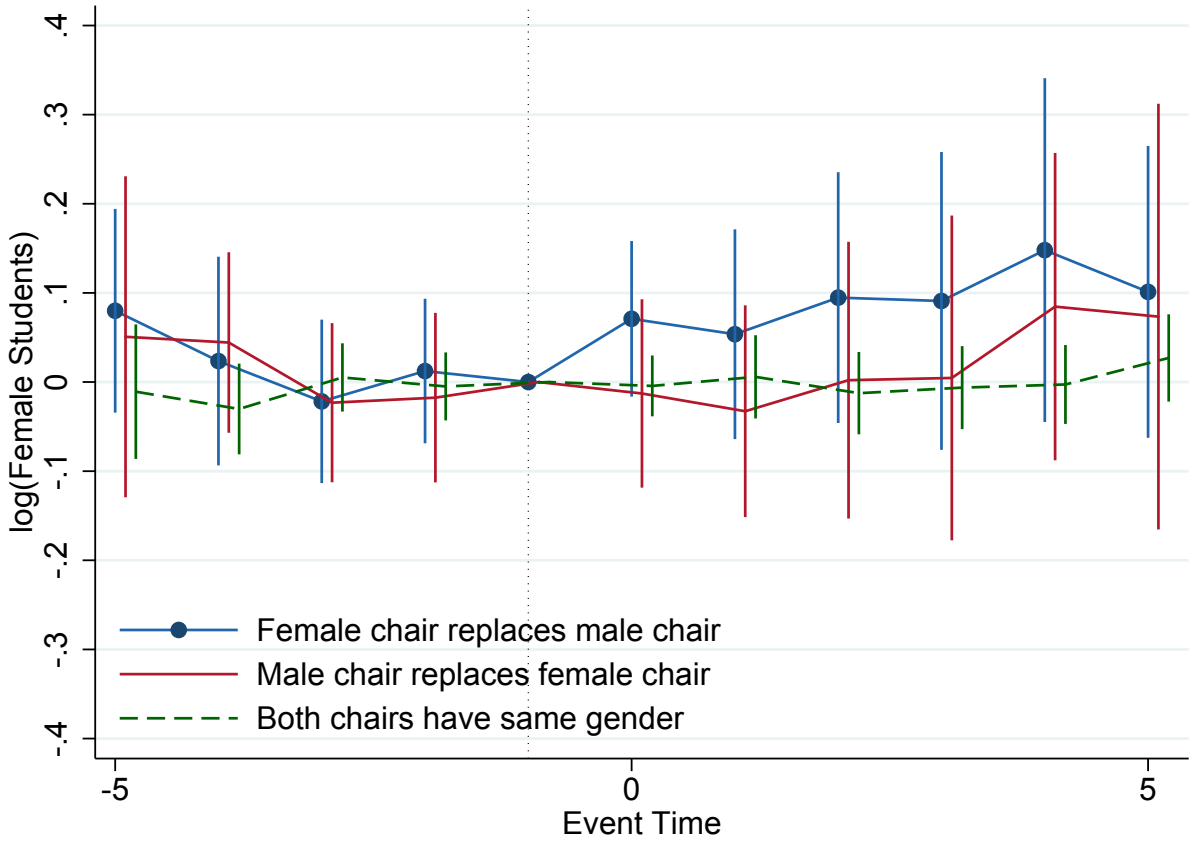
Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the person-year level. The outcome variable is natural log of earnings for male tenure-track faculty, regressed on event-time indicators plus person fixed effects, year fixed effects, an indicator 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 1.3: Event Study – Difference in Women and Men's Earnings Around a Chair Transition



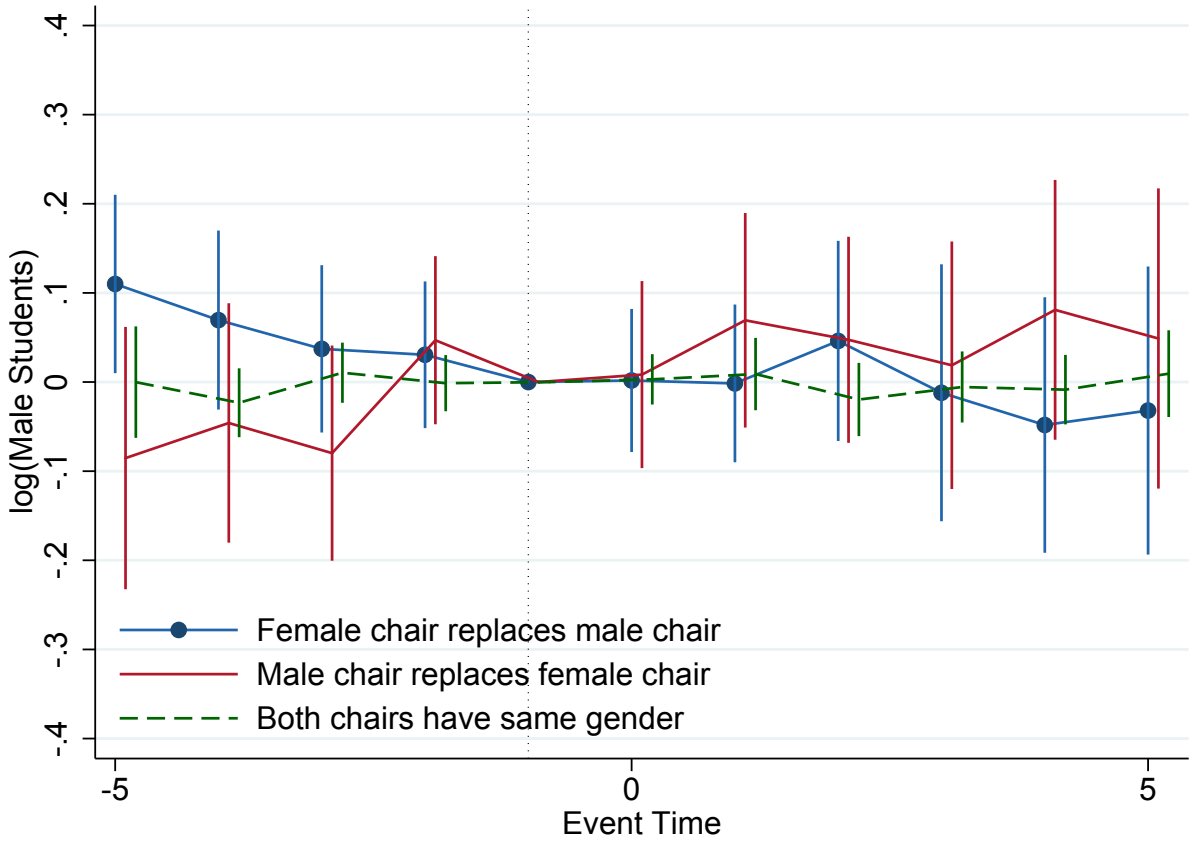
Coefficients and 95% confidence intervals are plotted with from an event study OLS regression on observations at the person-year level. The outcome variable is natural log of earnings for tenure-track faculty, regressed on event-time indicators plus person fixed effects, year fixed effects, an indicator 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 1.4: Women in Incoming Graduate Cohorts Around a Chair Transition.



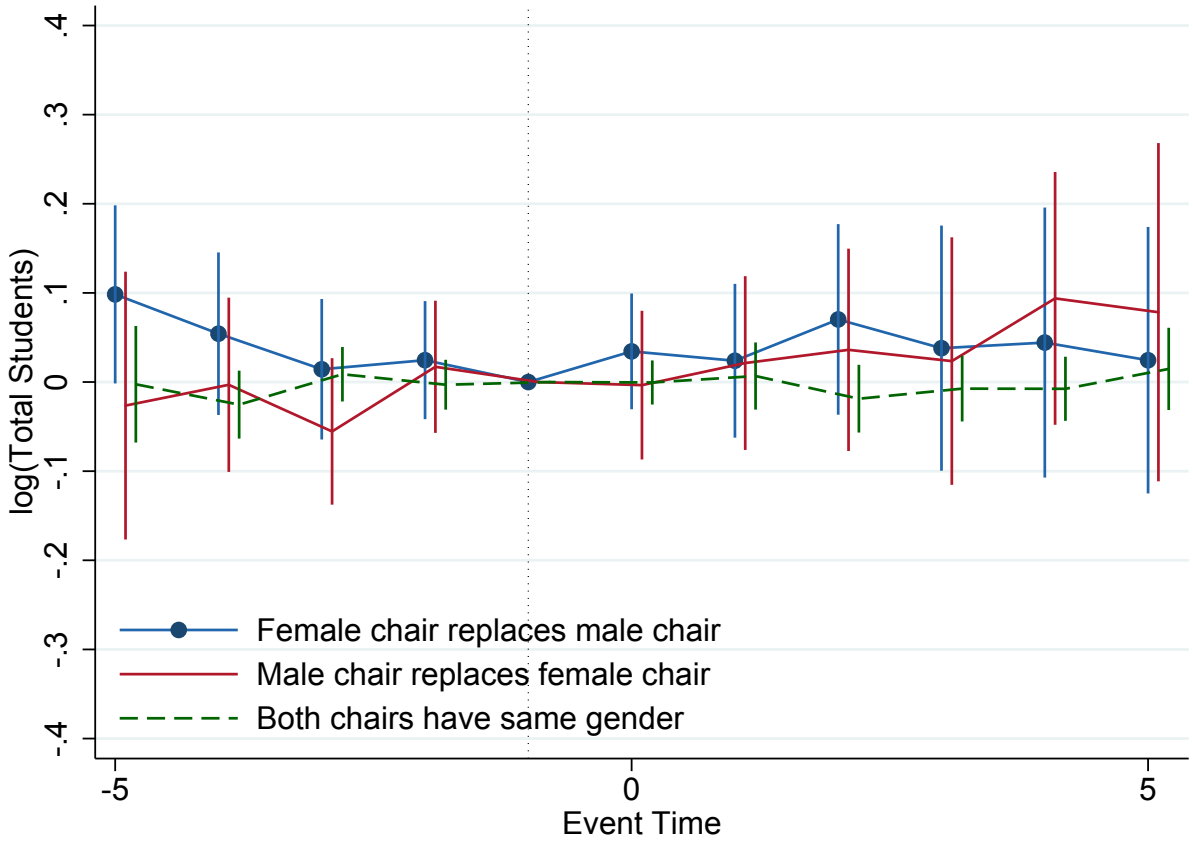
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of female first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 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 1.5: Men in Incoming Graduate Cohorts Around a Chair Transition.



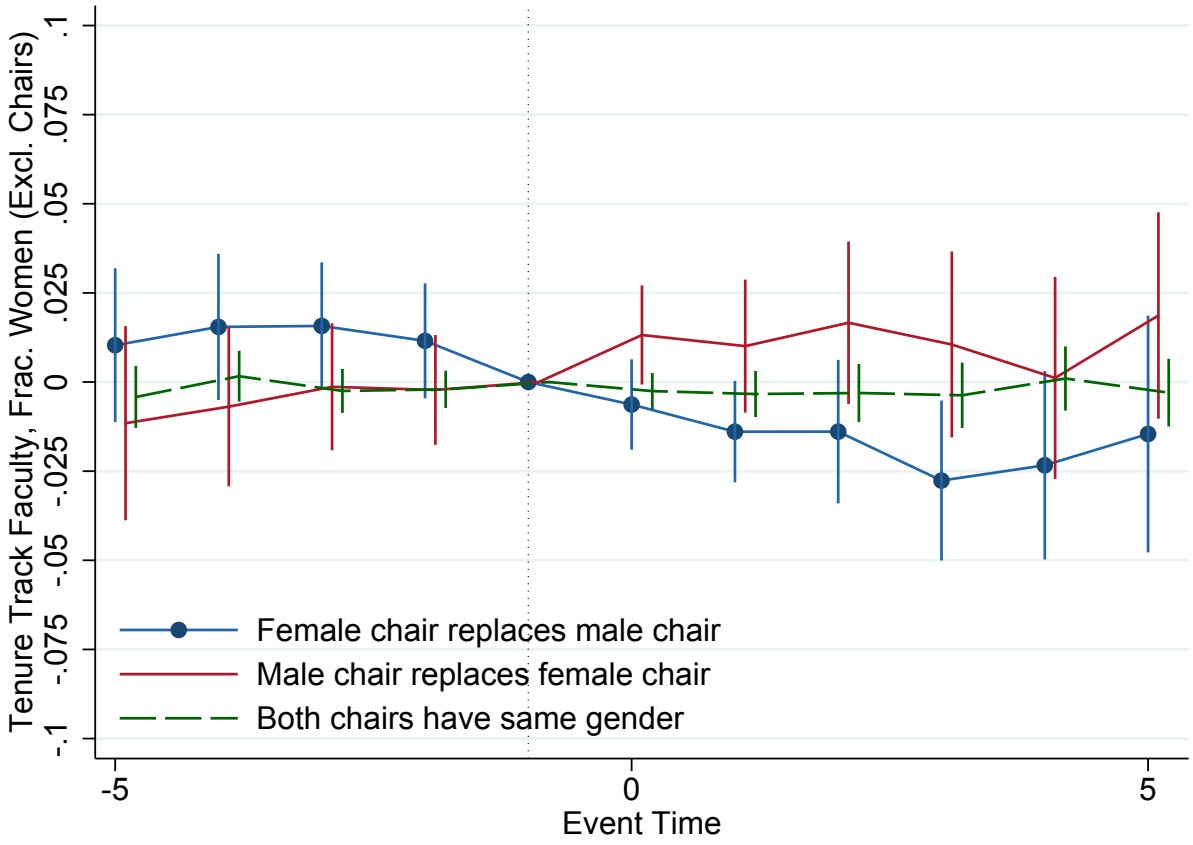
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of male first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 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 1.6: Total Students in Incoming Graduate Cohorts Around a Chair Transition.



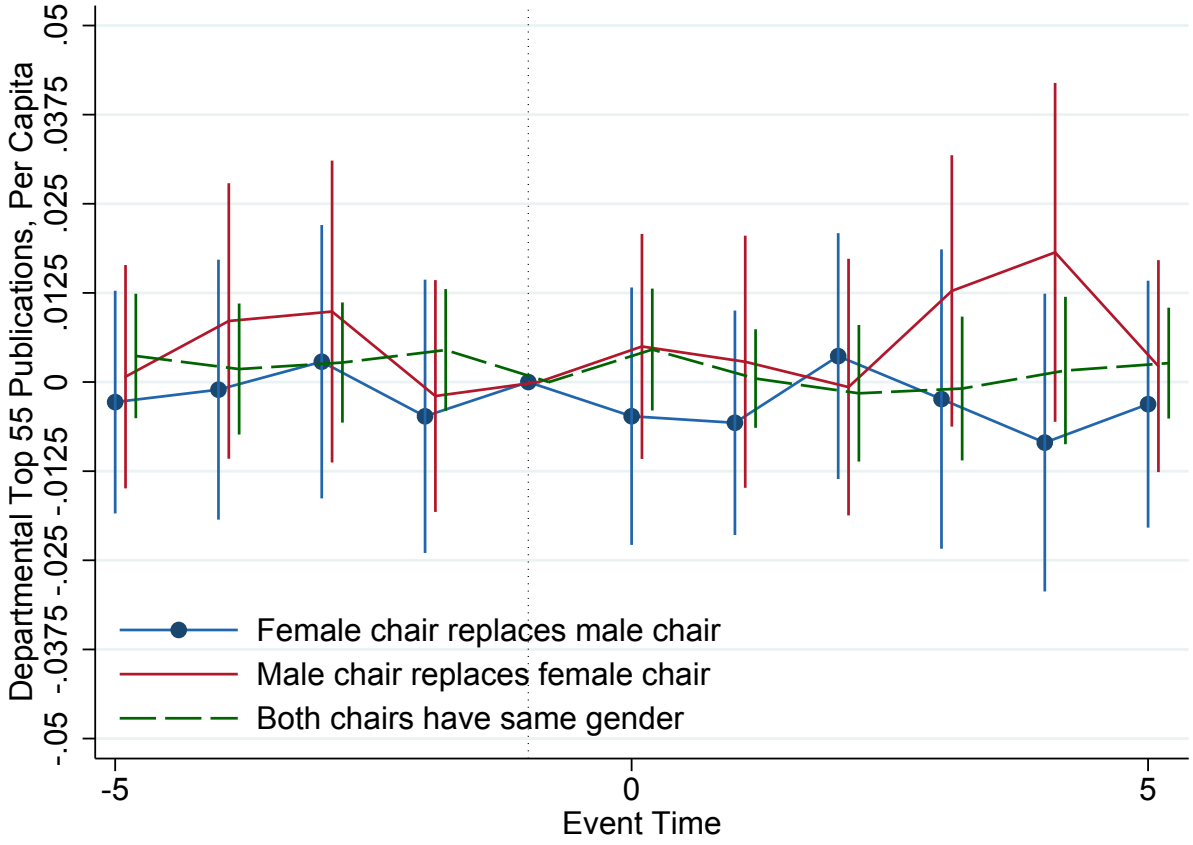
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 2016. 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 1.7: 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 1.8: 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).

Chapter 2

Variation in Women's Success across PhD Programs in Economics[1]

In 2017, women made up 32 percent of entering PhD students in economics. The share of women in economics is below many other fields including science, technology, engineering, and mathematics and has not increased since the 1990s (as discussed in this journal by Bayer and Rouse 2016). This paper adds new data—both quantitative and qualitative—on graduate programs in economics to understand the wide and persistent variation in women's success across departments. We then use these insights to suggest "best practices" for department chairs, PhD admissions committees, and others hoping to increase the number of women in economics.

Our quantitative data come from two sources: newly available annual surveys of graduate departments conducted by the Committee on the Status of Women in the Economics Profession (CSWEP), and our own hand-collected faculty rosters from PhD-granting economics departments in the United States from 1994 to 2017. The CSWEP survey contains information on the number of men and women graduating from each program by year, and

[1]This paper was coauthored with Leah Boustan, and was initially published in the *Journal of Economic Perspectives*, 33(1), Winter 2019, pp. 23-42. It is reprinted here with permission.

the number of job market placements by job type and gender.[2] We organize our roster data to associate each faculty member with their graduation cohort and alma mater to examine other early career outcomes, such as placement rank, publications, and promotion.

The opening four sections of this paper document a number of facts. First, there was wide variation in the average share of women in the graduating classes of economics PhD programs during the past 30 years, ranging from 10 percent to more than 50 percent of the class. This variation is primarily explained by differences in initial admission, rather than differential attrition from the program. Many programs, particularly the largest ones, increased the share of women on the student body from the 1990s to the 2010s. Yet the share of women in a PhD program tends to be a persistent attribute of a department, with a strong correlation in the gender composition of graduating classes over time.

Second, we show that departments with a greater share of women on their faculty also have more women in their student body: a 10 percentage point increase in faculty share is associated with a 2.5 percentage point increase in student share. This relationship could be causal if, for example, women on the faculty serve as role models for women students, or it could reflect other departmental attributes that are attractive to both women faculty members and graduate students.

Third, we document that, on average, men and women who graduated from the same program between 1994 and 2017 are no different in their propensity to be offered and accept a faculty position at a US PhD-granting department or to be promoted to associate professor within ten years of graduation. But conditional on taking a job in a US PhD-granting economics department, men land placements at higher-ranked departments and publish more in the top journals in the first seven years after obtaining their degree.

Fourth, we rank 22 large (anonymized) programs on the gender gap in these early career outcomes. We focus on programs with sufficient data for both men and women; these

[2]CSWEP's data are now posted on ICPSR (Study Number 37118); we received an early copy of the data for this paper. We thank Shelley Lundberg and Margaret Levenstein for access to the data. By agreement with CSWEP, names of the institutions are suppressed.

22 programs train two-thirds of the faculty at PhD programs in the United States. Our ranking identifies large variation in relative success for women across graduate programs in outcomes like job placement, publication, and promotion. For example, women graduating from departments with better relative outcomes have 9 percent higher placement rates than men, while women graduating from departments with worse relative outcomes have 8 percent lower placement rates than men. Yet in all cases, men are more likely than women to publish in top journals, suggesting that women face a common set of impediments in their early careers regardless of their graduate institution. This ranking across departments guided our selection of departments for a set of structured qualitative interviews designed to learn more about variation in the mechanics and culture of graduate instruction in economics. We conducted 31 interviews with faculty members and former students at five programs—two that achieved good relative outcomes for women, two that achieved poor relative outcomes for women, and one in the middle of the pack. All interviews were conducted and transcribed by Leyla Mocan, a Master in Public Policy student at the Woodrow Wilson School at Princeton. Our interviews confirm that having women on the faculty inspires women in the student body to succeed. The interviews also uncover several features of graduate programs that are associated with good relative outcomes for women: structuring the graduate program to formalize key aspects of advisorstudent contact; creating a collegial atmosphere in research seminars; and developing awareness of gender issues, especially by senior male faculty. We see many of these ideas as "gender neutral," in the sense that they would likely improve the climate of graduate instruction for all students regardless of gender. However, our interviews suggest that these reforms would likely have a disparate impact on women.

2.1 The Share of Women Graduating from Economics PhD Programs

This section presents new facts about variation in the share of women in graduating classes from economics PhD programs. We limit our analysis to the 88 out of 127 economics departments with a large enough number of entering and graduating PhD students who responded to the CSWEP survey. We focus on departments that were above the 25th percentile in the number of entering first-year students (123) and the number of graduates (68) reported to CSWEP during the full period, and for which we have faculty rosters, to permit other analyses. We aggregate the annual data into two broader graduation cohorts: students graduating between 1994 and 2005 and between 2006 and 2017. These periods divide our data in half.

Figure 1 illustrates that the share of women in the graduate student body ranges from 10 percent to more than 50 percent. The share of women in a program is a persistent attribute, with a correlation between the two time periods of 0.38 (0.45 if weighting by size of the program, and 0.53 when focusing on programs above the median size). Figure 2 shows a strong relationship between the share of women in entering first-year cohorts (19942012) and in graduating cohorts (19992017). The coefficient from regressing share of women in the graduating class on share of women in the entering class cannot be statistically distinguished from one, suggesting that variation in share of women across programs is not driven by differential attrition, such as gender differences in the probability of leaving the program after failing general exams. The year groupings in this figure are selected to match entering first-year cohorts at least approximately with corresponding graduating classes. We assume that students take around five years to graduate, but the result is robust to alternative assumptions.

2.2 Correlations with Observable Departmental Attributes

Differences in the share of women graduating from PhD departments in economics are correlated with some observable characteristics of these programs. Table 1 considers the association between share of women in the graduating class and six attributes of a PhD program: its *US News & World Report* department ranking; the share of women on its faculty; the field mix of its faculty; the average size of its entering class; the share of non-US-citizen entering students, and the institution's location in a small, medium, or large city.

The first six columns enter each attribute separately and the last column includes all control variables that were individually statistically significant. With 88 observations, we have limited power to detect relationships. Yet we find that the share of women in the graduating class rises in a clear way with the share of women on the faculty. The final column suggests that a 10 percentage point increase in the share of women on the faculty (roughly equivalent to the interquartile range) is associated with a 2.5 percentage point increase in the share of women in the graduating class. This positive association is consistent with Hale and Regev (2014), which uses retirements of men to consider exogenous changes in share of women on the faculty.

A few other departmental characteristics influence the gender composition of the student body. Programs located in the country's 15 largest metro areas have more women in the student body, as do departments further down the 2017 *US News* ranking. Faculty research field mix, program size, and the share of non-US students, as measured by the National Science Foundation's *Survey of Graduate Students and Postdoctorates in Science and Engineering*, do not seem to affect gender balance. In other calculations not shown here, we also see no association between any of these department attributes and the relative placement rates of women.[3]

[3]Neumark and Gardecki (1998) and Hilmer and Hilmer (2007) likewise find no positive association in the late 1970s through early 1990s between the share of women on the faculty and students' early career outcomes.

To measure gender and field composition of faculty, we compile data on academic rosters across economics departments from three sources: *the Prentice Hall Guide to Economics Faculty*, published in seven editions between 1994 and 2006; departmental and faculty web pages accessed using the Wayback Machine (www. archive.org); and the 1989 and 1993 biographical surveys of members by the American Economic Association. More details on construction of the faculty roster are available in Langan (2018).

2.3 Early Career Outcomes by Gender

We now turn to examining differences in early career outcomes of recent economics PhDs by gender. We calculate a set of relative ratios for men and women graduating from the same programs for outcomes like academic placement, placement rank, and so on. We then ask in the next section how those ratios vary across departments. Here, we start by identifying two average tendencies: 1) women and men who graduated from the same department are equally likely to place into US doctoral departments; and 2) conditional on receiving an academic placement, women place into lower-ranked departments and publish fewer papers in the first seven years of their career.

Figure 3 graphs the job placement rate by gender for graduates of the 88 PhD programs in our core sample from 1994 to 2017. We consider five placement categories: faculty jobs in US PhD-granting departments, other US academic jobs, US private sector jobs, US public sector jobs, and jobs outside of the United States. The p-values under each panel refer to the null hypothesis that rates of placement are equal for men and women (a small p-value indicates strong evidence against the null hypothesis). We find no statistically significant difference in the average placement rate of men and women graduates from the same department into faculty jobs in US PhD-granting institutions, as shown in the first panel. In contrast, women are more likely to accept other US academic jobs, and men are more likely to take jobs outside of the United States.

To develop more detailed early career outcome measures, we move from CSWEP's job placement data to our faculty rosters. We create ratios of men and women who graduated from each department by associating each faculty member in our roster with their department of graduate instruction. For example, we observe one of our coauthors, Leah Boustan, in her first job placement in the UCLA Department of Economics in 2006, but we associate her outcomes (including her placement at UCLA) to her graduation cohort at Harvard University. We use the reshaped roster to define additional outcomes for 22 departments with at least ten women placed into PhD-granting economics departments in order to calculate reasonable averages. Because our rosters include only academic economics departments and not, for instance, business, public policy, or resource economics programs, they constitute only a subset of the CSWEP placements, but the two placement rates are highly correlated overall, and by gender (correlation = 0.75).

Figure 4 compares a set of early career outcomes for men and women who graduated from each of the 22 departments. Men place at departments ranked higher by US News and have more publications within their first seven years of graduation. The average gap in placement rank between men and women graduates of the same program is 4.8 rank points, as shown in Figure 4A. Figures 4B and 4C show that men publish more papers in Top 5 or Top 55 economics journals within seven years of graduation; publications are culled from the table of contents data provided by journals to RePEc. The average gap in Top 55 publications between men and women graduates of the same program is 0.58.[4] Figure 4D shows that women are marginally less likely to be promoted to associate or full professor at a PhD-granting economics department within ten years of graduation, relative to men from their alma mater.[5] Balanced promotion rates by gender could arise if men publish more

[4]The relative publication records of men and women in our sample is consistent with Antecol, Bedard, and Stearns (2018) and Sarsons (2017) for economics, and with broader results across fields summarized in Ceci, Ginther, Kahn, and Williams (2014). For additional work on gender and publication in economics, see Blank (1991), McDowell, Singell, and Stater (2006), Abrevaya and Hamermesh (2012), Bransch and Kvasnicka (2017), and Hengel (2017).

[5]Ginther and Kahn (2004, 2015) find that men are more likely than women to receive tenure in economics, controlling for year of graduation and institutional quality of alma mater.

than women but also face a higher publication bar for tenure, given their higher-ranked first placement.

2.4 Documenting Differences in Relative Outcomes for Women across Departments

We are particularly interested in documenting the variation across departments in relative outcomes for women, including graduation rates, placement into an academic job, and later research performance. In Table 2, we rank 22 anonymized graduate programs from "A," the department with the best outcomes for women graduate students, relative to men in their programs, to "V," the department with the worst relative outcomes. The ranking is derived by converting each of the outcomes into a z-score and then calculating the mean z-score across outcomes for each department; details are presented in Appendix B.

Average outcomes for three ranked categories (better & best; neutral; worse & worst) are reported in the last three rows of the table. Across these three categories, women make up a similar share of the student body (28 to 30 percent) and first-year students are equally likely to graduate from the program (column labeled "Retention") regardless of gender. Differences arise at the job placement and assistant professor stages. At programs that are relatively better for women, women have 9 percent higher academic placement rates than men, placement ranks that are nearly identical to men in their program, and marginally lower numbers of top 55 publications. At programs that are relatively worse for women, women have 8 percent lower academic placement rates than men, place into substantially lower-ranked departments, and publish approximately 30 percent fewer papers in a top 55 journal.

Checkmarks in Table 2 indicate the departments that were in the top 10 percent (two checkmarks) or top 25 percent (one checkmark) of any given outcome (out of 22 departments), and 's indicate the departments that were in the bottom 10 percent (two 's) or

bottom 25 percent (one). Departments that have better relative outcomes for women on average are not *necessarily* better across the board. Even the best departments by our rankings have some poor relative outcomes for women (for example, Department C has low promotion rates), and even the worst departments have some good relative outcomes for women (Department T places women at highly-ranked departments relative to men).

2.5 Interviews about Why Some Departments Have Better Relative Outcomes for Women

Why are relative outcomes for women graduate students better at some departments than at others? One natural approach to answering this question for us, as empirical economists, might have been to gather systematic data about each department (perhaps by conducting a survey) and then performing a quantitative analysis to determine which attributes of a program predict success for women. However, we only have complete outcome variables for 22 programs, and we do not have a clear a priori sense of which explanatory variables may matter, nor how to measure them.

Instead, we conducted a series of exploratory interviews. We selected six departments to include in our interview sample: two departments with better than average outcomes (Departments B and C), two departments with worse than average outcomes for women (Departments O and T), and two departments in the middle of the group (Departments F and K). For each of these departments, we started by contacting the current department chair for a short interview. From there, we developed a snowball sample, asking the chair for a few names of faculty members and former students to reach out to next. In the end, we spoke with six or seven graduates or faculty from five departments, primarily in June and July 2018. Due to idiosyncratic scheduling difficulties we dropped one department (Department F) from the sample. Otherwise, we had a 91 percent response rate for all interview subjects

contacted. Our interview subjects were 55 percent women and 45 percent men. Interviews ranged from 8 to 45 minutes, with the typical conversation lasting 15 minutes.

Each interview consisted of nine questions. We asked respondents to describe student contact with advisors, seminar culture and job market preparation, and also included open-ended questions about women in economics; we report the standard interview script in Appendix C. All interview subjects were told that their answers would be confidential. In our discussion of qualitative patterns, we suppress all names of individuals and of institutions, and we redact a few details to prevent department or subject identification.

From the interview responses, we observe a series of differences between departments with better and worse relative outcomes for women graduate students that are worth keeping in mind for departments interested in improving gender diversity or balance in outcomes for their members. The departments with the best relative outcomes tended to have a commitment to hiring women onto the faculty; regular and transparent processes for studentadvisor contact; a more collegial seminar culture; and a stronger general awareness of gender issues among senior faculty.

Many of these departmental policies would be helpful for all students, but our interviews suggest that they may also help to narrow gender gaps. In the remainder of this section, we have organized a series of representative quotations from our interviews, emphasizing points of difference along these dimensions between departments with better and worse relative outcomes for women graduate students.

2.5.1 Women on the Faculty

One obvious difference between departments with better and worse relative outcomes for women graduate students is their commitment to and success in hiring women on the faculty. Students trained at Departments B and C talked about how valuable it was to have been taught by women faculty, and to have informal interaction with women faculty outside of the classroom. A former student from Department C said: "Every semester we always had

one female teaching us in the core first year classes. Starting with that introduction to the graduate department made me feel like 'I can do this and be a woman.' "[6] A faculty member at Department C agreed, touting the value of the "women in economics group, female faculty who have lunches and dinners and coffees with female graduate students, a group that has been around as long as I can remember." One faculty member from Department B described how the program was able to build up a group of women faculty, emphasizing that "our dean puts a lot of emphasis on diversity. It could even amount to an extra position if we come up with an additional excellent female candidate."

In contrast, interviewees from departments with worse outcomes for women graduate students noted—and often lamented—their historical lack of representation of women on their faculties. These departments were only recently beginning to prioritize hiring women or had limited plans to do so. At our median Department K, the faculty were concerned about how their historical lack of women faculty affected their graduate students. One faculty member said, "[W]ell it starts with hiring. I've seen over the years that role models are incredibly important. I know that in our [redacted] field, for example, we currently have five faculty members who are all male. And I think we are all nice guys and inviting and encouraging and try to be gender neutral, yet whenever we have a female [visitor or speaker], women graduate students flock to her much more readily, there's a comfort level and a role model that's very important." A faculty member at Department T concurs, relaying: "I have a student who is a woman who graduated maybe ten years ago . . . she thinks her experience would have been better if there were more senior female faculty, that she would have felt more comfortable. ... So we're working on that, we make offers, but like everyone else in the senior market it's really tough. . . . [I]t's hard to get acceptances." Unlike Departments B and C (and even Department K), Department T has not made an intentional effort to hire women. Instead, the chair simply tries to ensure that women are not overlooked on the job

[6]Throughout the paper, we use the terms "men and women" instead of "male and female" when discussing gender in order to emphasize that gender identity is a social phenomenon. However, we leave the quotations from our interview subjects as stated.

market, saying: "I look at the women we didn't fly out and I want to make sure that they're not better than the worst men that we fly out. . . . [We] pay attention to see if we're gender neutral, at least in our junior hiring."

Hiring women on the faculty strikes us as a concrete and low-cost approach to creating a productive learning environment that might encourage women to enroll in the graduate program. Our interview subjects also articulated clear mechanisms whereby women on the faculty might benefit graduate students by serving as role models and mentors.

2.5.2 Advisor Contact

In many other disciplines in the sciences and the humanities, prospective PhD students apply to work with a particular faculty member before admission. In economics, it is typical for prospective students to apply to the department as a whole without specifying an intended field. All of the programs in our interview set have a decentralized process for second- or third-year students to select dissertation advisors. Many students reported approaching faculty with whom they took a course in the first-year core or in second-year field courses. This student's experience at Department O is typical of the haphazard search process: "I got [Faculty Member X as my advisor] just by talking a lot in class and by doing reasonably well . . . that distinguished me and made me seen as a relatively attractive person to work with."

Students and faculty speculated that the current *laissez faire* approach can be intimidating for many students. It may be particularly hard for students who are in the minority in economics departments (including women) who may fear the actual or perceived skepticism of faculty. One student at Department C reported: "I think there was some implicit bias. I remember in the first year of the PhD program, I got [a high score on a core exam]. . . and my husband did not do particularly well in the course. But I remember the professor who was teaching the course reached out to my husband and tried to encourage him to go into [the field] and didn't say anything to me."

One feature that distinguished departments with better relative outcomes for women was the mandatory and regular nature of student works-in-progress seminars—that is, public venues for fourth- and fifth-year students to present new research and gather comments from a group of committed faculty. According to a student from Department C, "you would actually enroll in a course that was your lunchtime workshop; you were required to attend and present at least once a quarter. The faculty were really good about going to those things. They came regularly, and they would give you comments."

In contrast, at the departments with worse relative outcomes for women in our sample, works-in-progress seminars are offered for student research presentations, but students are not required to attend. As one faculty member describes the process, "[Department T] doesn't have . . . a centralized policy around making sure everyone presented once or twice a year. . . . There are certainly students who aren't going to any lunch, or if they are going they are not presenting. We don't do record keeping." Another faculty member agrees, saying that "one or two students get almost completely lost, they don't make appointments with advisors, and their advisors don't reach out. . . . There are some students who are not into getting advice very much. Unless they're a genius and come up with some fantastic thing, they tend to get lost."

Offering regular public venues for student feedback may be especially important for women, who mentioned off-campus advising settings in which they felt uncomfortable or to which they were not invited. This concern arose at departments of all levels on our ranking. One student from Department B described how "one senior male faculty was known for having these weekly 'salons.' He would take a bunch of students out to a bar. A bunch of students would go, it routinely felt like an old boys' club. . . . I know one woman who went, but I wouldn't have felt comfortable." A student from Department C reported that she missed advising opportunities because of her gender: "[My advisor] would often take graduate students for beers, and he would take the men one on one. When I was first working with him, he said 'we should just meet in the office, we shouldn't get a beer, it could be

misconstrued or you might feel I'm taking advantage of you.' So, either my husband would have to tag along, which was really boring for him, or another male graduate student would have to come, but I wouldn't get as much one-on-one attention."

Given the highly decentralized nature of the advising process in economics, we think that all departments have some room to experiment with developing more formalized points of student-advisor contact. One of our interview departments recently established a procedure to match incoming first-year students with a faculty mentor to address the unique challenges of the first-year program. Another department is experimenting with third-year research advising groups, wherein students are paired with field-specific faculty members. The chair of this department acknowledges that "before, it was all decentralized, students just had to reach out" and expressed hope for the new structure. Other departments have instituted regular faculty meetings to assess student progress and ensure that no students are falling between the cracks. One model for how to regularize the various steps of graduate training is the recent and widespread efforts to formalize the process of preparation for the job market—including holding information sessions, conducting mock interviews, designating a faculty member to be placement coordinator, and so on.

2.5.3 Seminar Culture

Another theme that emerged from our interviews is that departments with better relative outcomes for women are reported to have a less aggressive and more constructive climate in their research seminars. A faculty member at Department B portrayed seminars as "not enormously aggressive . . . we don't take someone apart for the sake of taking them apart." A student from Department C had a similar impression. "Like most economics departments, [the seminar] was aggressive, but it wasn't as aggressive as some I've been to. You would get through your first few slides without being asked 'why did you choose this title? This is stupid.' Especially with the graduate students. They would ask questions more gently like

'don't you think that this would be a problem with identification?' Compared to 'this is not how to identify it.' But they wouldn't sit and let you make mistakes."

Two of the three departments with worse outcomes for women were noted for having aggressive seminar styles. A faculty member from Department K said that "the word that comes to mind is combative, perhaps aggressive. People start to talk about their work and much of their audience seems to think it's their job to find the faults and tear it all down." A student trained at Department O recalled that seminars were "fiercely competitive." On the other hand, Department T was described as having seminars that are "pretty polite."

Unlike new hiring or a reorganization of the graduate program, working toward a more supportive seminar culture is an action that individual faculty members can take on their own, without the need to create consensus at the departmental level. One idea is to save some comments on student work for a short private meeting held after the public seminar. Meeting one-on-one after the seminar can help students take stock of the various suggestions and prioritize which next steps are most important without the immediate pressures of a presentation.

We are agnostic about why some of the specific policies described here, including developing a more supportive seminar culture, might be particularly helpful for women students. Interview subjects mentioned the possibility that women are more averse to competitive environments, more likely to take harsh feedback personally, more likely to be left out of informal ("old boy") networks, and so on. There is observational and experimental evidence for some of these channels (for example, see Goldin 2015 on differential response by gender to receiving low grades as undergraduates), and others channels would be interesting subjects for future study.

2.5.4 Awareness of Gender Bias

A final difference that we noticed between departments with better and worse relative outcomes for women was an awareness of gender bias, particularly in its more subtle and implicit

forms, among the senior male faculty who often make up the majority of leadership positions in a department. Awareness of gender bias may influence how these senior faculty interact with women in their classes and in their advisory roles on a day-to-day basis. Moreover, we noticed that senior faculty with more cognizance of gender bias react differently to incidents of harassment (which, although rare, were mentioned at three of our five interview departments). One woman made this point quite succinctly, saying "my personal view is that a lot of it has to come from the men, [the field is] still very male dominated, so until the men are comfortable, nothing much good is going to happen for the women."

Differences in awareness of gender bias were most obvious in responses to the interview question "do you see any difference in the [graduate department] environment for men and women?" Faculty at departments with better relative outcomes for women responded to the question in more observant and thoughtful ways. A senior faculty member at Department K was typical of this view in saying: "I'm confident that there is no explicit discrimination, but perhaps what is happening is more subtle or subconscious . . . The same behaviors in a man say he's forceful and defends his ideas; he's aggressive in a good way. Those same behaviors when taken by women tend to get a different reaction in a subtle kind of way."

In contrast, faculty members at Departments O and T focused exclusively on the lack of overt discrimination against women students and did not seem aware of (or to put much credence in) the subtle differences in the way men and women experience the culture of the field. A professor at Department O said "I'm not aware of people explicitly treating anyone different in one way or another [by gender], at least from faculty perspective. I can't—I'm not aware of any specific instances to that effect. As far as I can tell, nothing that I can think of comes to mind." A male faculty member from Department T answered similarly, reporting that he talked to graduate students and never heard complaints about poor treatment of women students. "I'm still a little perplexed about this," he said. "I ask graduate students what the atmosphere is like for them, and I never hear about any problem of them being harassed in the kind of extreme form, but even in more subtle ways. My sense,

and I acknowledge that I might be misreading, is that women in the department are treated well. That's my sense, but I'm suspicious about if I'm gathering information correctly."

Differences in awareness of gender issues can have important consequences in how department leadership respond to instances of harassment in a learning environment, which were mentioned at three of the five departments that we profiled. By harassment, we do not mean individual cases of unwanted sexual attention, although this may also happen. Instead, we heard about intentional attempts on the part of one or more men in the student body to make women graduate students feel unwelcome. These instances of harassment, while certainly not the norm, did occur across the board. We also we noticed clear differences in how the faculty at each department responded to such events. In each case, administrators condemned the harassment and sought to punish the individuals responsible, but the responses differed in whether further action was taken to change the underlying culture or institutions that contributed to the problem.

At Department C, the faculty considered the event to be outside of the norm and immediately searched for an underlying cause, which they then acted to remedy. A faculty member at Department C tells this story: "We actually had an incident [recently] that I think was unusual, [details redacted]. One thing we realized is that we had [a new admissions director] for the last couple of years, and [...] we had ended up with a couple of classes with way too few women students. Maybe only 20 percent, rather than [our usual] much higher figure. And so, the chair basically leaned on the person in charge of admissions that we had to have a lot more women, and our entering class this year is 5050, which is the first time it's been so high. We're hoping that means it won't happen again, if you have more women even guys who think like that will feel pressured not to act. That was the first time that we had anything like that in all the years I've been here, and I was really pleased that the Chair really got on it and pressured the admissions chair rather than ignoring it."

At Departments K and T, instead, the harassing behavior itself was condemned but there was no further action to assess and perhaps adjust the culture. A faculty member at

Department T described an instance of harassment and the departmental response: "We've had problems in the past. For example, there were some men [engaging in redacted activities], which is an extreme form of harassment ... In those cases, the chair went nuts. No one could figure out who was doing it, but they warned that they would be thrown out of the university if this happened again. I believe it has not happened anymore." According to a student trained at Department K, a similar reaction occurred there in response to one individual's harassing actions. "The Chair tried to make some statement the next year, held a meeting at the beginning of the year for all PhD students, and said that there had been [harassing behavior], people saying things about women that you shouldn't say because you could hurt someone's feelings."

We acknowledge that improving awareness of gender bias—in oneself and one's environment—is an amorphous recommendation, and hard to convert into direct action. Self-reflection, questioning one's assumptions, and striving to learn more about the experiences of others are habits worth cultivating for any individual and especially academics. Empathy is a human virtue, and a scholarly one too. On an organizational level, attention and sensitivity to diverse perspectives in a department should be considered when making departmental leadership decisions.

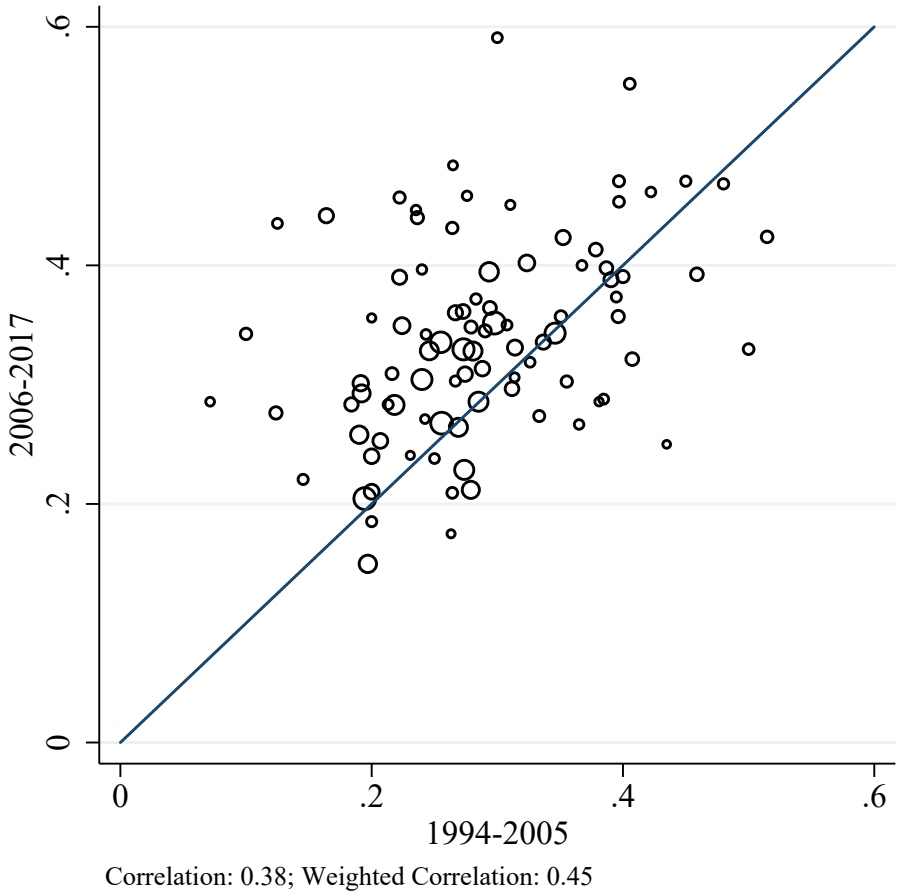
2.6 Conclusions

We document substantial variation in the representation of women and in the early career success of women across economics PhD programs. We provide suggestive evidence, based on a set of structured interviews, that there are large differences in culture and practices across graduate programs in economics; these differences are associated with better and worse relative outcomes for women. Important differences across departments include the number of women on the faculty, regularized opportunities for contact between advisors and students, and collegial research seminars.

Of course, such qualitative observations are only the beginning of establishing hypotheses about "what works" in increasing the representation of women in economics. We encourage other scholars to test the hypotheses suggested by this paper in more detail. In addition, it would be interesting to know whether prospective graduate students consider the types of factors underlying these rankings when selecting departments. If so, the association between share of women in the graduate program and positive early career outcomes could be, in part, due to students' choices in where to matriculate.

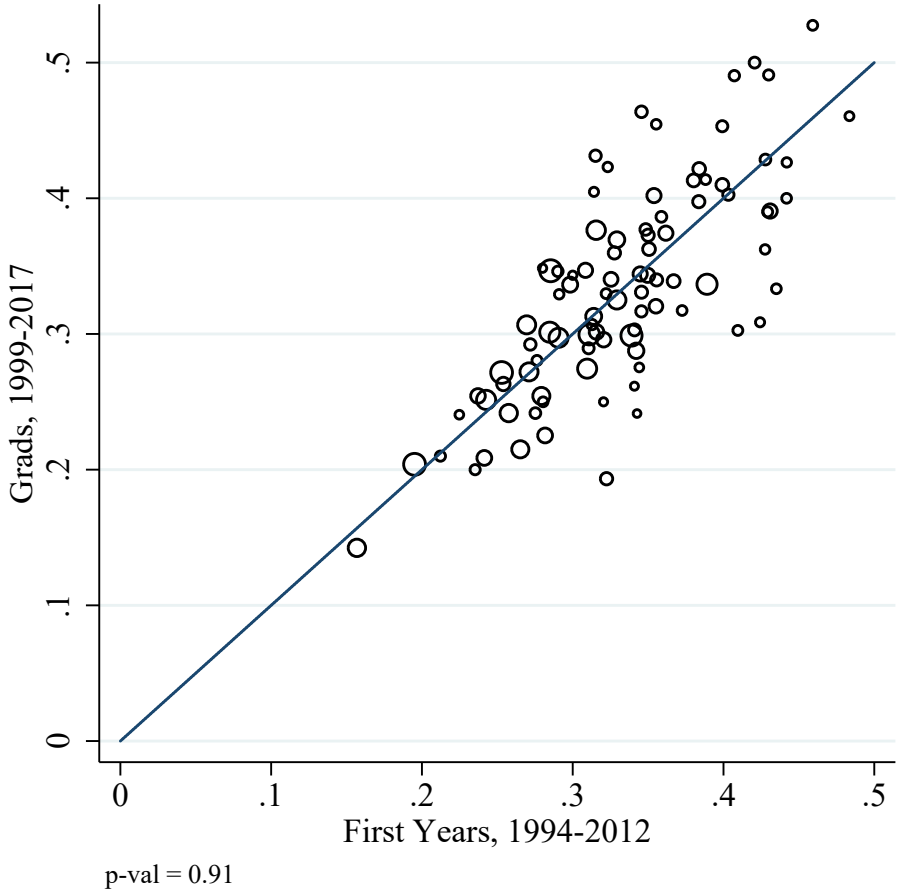
Typical studies of the underrepresentation of women in scientific and technical fields focus on early educational experiences (like differential treatment in high school courses by gender) or on the job market and in academic workplaces (for example, see Ceci, Ginther, Kahn, and Williams 2014). These emphases, while certainly important, overlook the potentially central role of the learning environment in graduate school itself. To the extent that the culture and policies of a graduate program matter, there may be meaningful steps that we can all take to encourage and support women in economics. We hope that our findings will help to inform ongoing discussions—or to initiate new ones—in departments seeking to ensure the best opportunities and outcomes for all their graduates.

Figure 2.1: Share Women PhD Recipients by Department: 1994 − 2005 vs 2006 − 2017



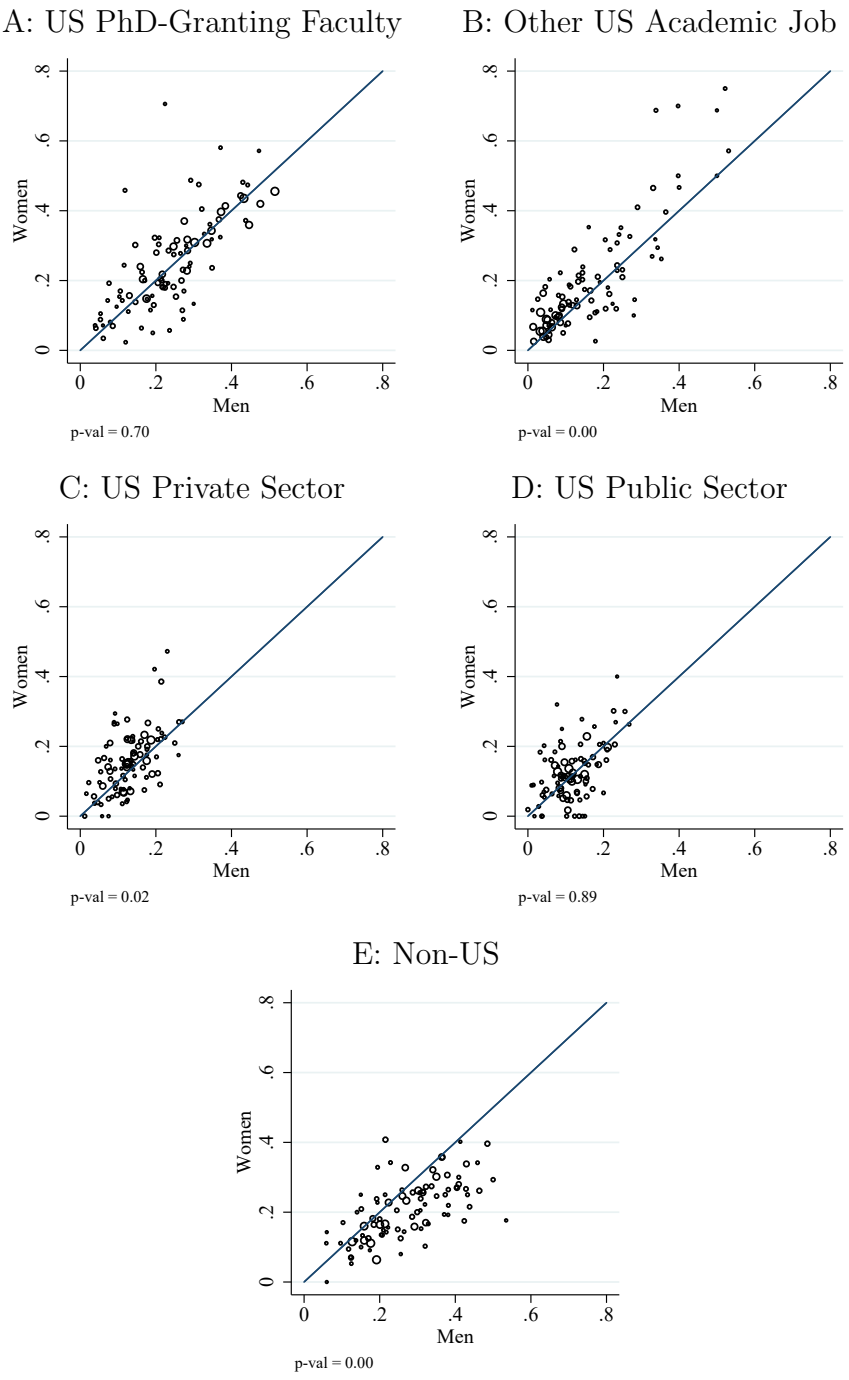
This figure plots women's share of economics PhD recipients from 2006 to 2017 against their share from 1994 to 2005, by department. Points are weighted by the total number of graduates over the entire period per school. We exclude years with imputed counts of PhD graduates and large outliers from department totals. Source: CSWEP department survey.

Figure 2.2: Share Women, PhD Recipients vs. First Years, by Department, Entering Classes of 1994-2012



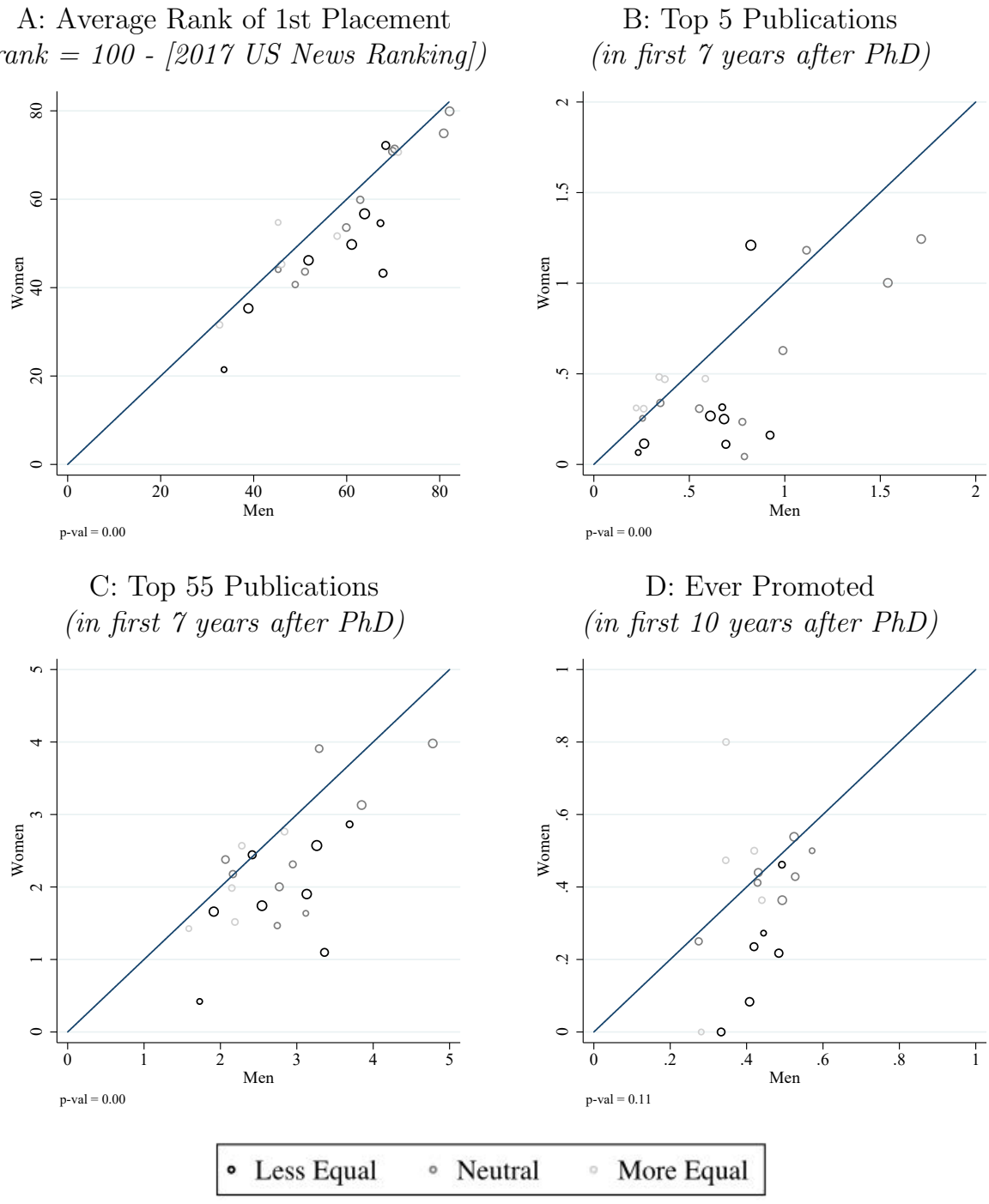
This figure plots women's share of economics PhD recipients from 1999 to 2017 against their share of first-year students from 1994 to 2012, by department. The p-value shown is taken from a t-test of the null hypothesis that the share of women is equal in the two groups within department. Points are weighted by the total number of graduates over the entire period per school. We exclude years with imputed counts of PhD graduates or first-years and large outliers from department totals. Source: CSWEP department survey.

Figure 2.3: Job Placement Rates by Gender and Department, Five Sectors



This figure plots women's versus men's rate of placement into five types of job, for economics PhD recipients in 1994-2017, by department. The p-values shown refer to a test of the null hypothesis that placement rates for a given job type are equal for men and women. US PhD-Granting Faculty refers to tenure-track faculty jobs in a department that awards doctoral degrees. Points are weighted by the total number of graduates over the entire period per school. We exclude years with imputed counts of PhD graduates or academic placements at PhD granting institutions and large outliers from department totals. Source: CSWEP department survey.

Figure 2.4: Post-PhD Outcomes for Men and Women by Graduate Department



This figure plots women's average outcome against men's for four early-career outcomes. We focus on PhD graduates from 22 large US economics programs, for which we can identify at least 10 women who place into a US economics doctoral program between the 1987 and 2017 graduating cohorts (or, in the case of promotion, 18 programs with 10 women placed between 1987 and 2010). In the top left, we plot the average 2017 *US News and World Report* rank for the first economics department where individuals place after graduation. Doctoral programs not ranked by *US News* are assigned rank 100, and then we replace rank with 100-rank, so that better outcomes lie up and to the right graphically. In the top right, we plot the average number of publications for men vs women graduates in a top 5 journal (*American Economics Review* (excluding Papers and Proceedings), *Quarterly Journal of Economics*, *Econometrica*, *Journal of Political Economy*, and *Review of Economic Studies*). In the bottom left, we plot publications in journals ranked 55 or better in the *RePEc* aggregate journal rankings as of January 4, 2018. (We chose 55 instead of 50 schools to achieve better representation of top field journals.) For both top 5 and top 55, total publications are predicted for post-2010 graduates with at least one publication. In the bottom right, we plot the share of men and women ever observed as an associate or full professor in our faculty rosters within 10 years after receiving their PhD. The p-values are taken from a t-test of the null hypothesis that men and women's average outcomes are equal within school. Points are weighted by the total number of graduates over the entire period per school. Source: Faculty rosters, Langan (2018).

Table 1: Effect of Department Characteristics on Share Women in Graduating Class, Economics PhDs

		Share Women Graduates						
Dept. Characteristics	*Mean*	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rank 11-20	0.102	0.024						0.035
		(0.034)						(0.034)
Rank 21-30	0.159	0.049						0.060*
		(0.031)						(0.032)
Rank 31+	0.625	0.080***						0.079**
		(0.025)						(0.036)
Faculty Share Women	0.156		0.409***					0.245*
			(0.127)					(0.130)
Share Appl Micro	0.389			0.231				
				(0.257)				
Share Theory	0.228			-0.090				
				(0.254)				
Share Macro	0.252			0.007				
				(0.258)				
Avg Cohort Size	15.961				-0.003**			-0.000
					(0.001)			(0.002)
Share Foreign Entrant	0.472					-0.006		
						(0.061)		
Medium City	0.375						0.032	0.023
							(0.020)	(0.019)
Large City	0.330						0.037*	0.050**
							(0.021)	(0.021)
Constant		0.272***	0.268***	0.261	0.383***	0.335***	0.308***	0.214***
		0.023	0.021	0.191	0.021	0.030	0.015	0.056
Obs.		88	88	88	88	88	88	88
R^2		0.136	0.108	0.027	0.073	0.000	0.042	0.243

This figure reports the result of regressing women's share of PhD recipients from economics departments on various department characteristics. The sample includes 88 departments that responded to the CSWEP survey, exceeded the 25th percentile of both the number of entering and graduating students among respondents, and for which we have departmental rosters available from Langan (2018). Economics department rank comes from the 2017 edition of *US News and World Report*. Faculty share women and average cohort size come from 1994-2016 CSWEP responses. Field composition is obtained from our academic roster (Langan, 2018) by counting total person-years in the department by individuals' stated fields, using JEL codes from the 1997 AEA members survey, the *Prentice Hall Guide to Economics Faculty*, 1994-2006, and keyword matching from statements on individual and department websites. Share of foreign first year students comes from 1994-2017 department responses to the NSF *Survey of Graduate Students and Postdoctorates in Science and Engineering*. Column 6 regresses share women on the size of the city in which the department is located: the top 15 metro areas are considered large and smaller areas with more than 150,000 population are considered medium. When calculating women's share of graduates from each department in the CSWEP survey data we drop department-year observations with imputed values for graduates or academic placements at PhD granting institutions and large outlier values.

Table 2: Comparative Success of Women by Program

Group	ID	Share Women	Δ Share Women	Retention	Plcmt at PhD	Plcmt Rank	Top 55	Top 5	Promotion
Highest relative outcomes for women	A	✓		✗	✓✓		✓	✓✓	
	B		✓	✓	✗			✓	✓✓
	C		✓✓			✓✓		✓	✗
Higher relative outcomes	D				✓			✓	✓
	E		✓	✓✓		✓			✓
Neutral	F		✗				✓		
	G			✓✓					
	H	✓					✗		
	I					✓			
	J			✗✗	✓		✓✓		
	K	✗	✗		✗	✓	✓✓		
	L			✓	✓	✗	✗	✗✗	
	M	✗							✓
	N		✓		✓✓			✗	
Lower relative outcomes	O	✗✗						✓✓	✗
	P						✗		
	Q	✓✓	✓✓	✓	✗✗	✗✗	✗✗	✗	
	R	✓		✗		✗			✗
	S	✓✓	✗	✗	✗				✗
Lowest relative outcomes	T	✗✗	✗✗			✗✗	✓	✗✗	
	U	✗			✗✗	✗			
	V		✗✗	✗✗		✓✓	✗✗	✗	
Higher & Highest	(A-E)	0.30	1.28	1.02	1.09	1.01	0.92	1.21	1.14
Neutral	(F-N)	0.28	1.13	1.01	1.08	0.94	0.84	0.66	0.91
Lower & Lowest	(O-V)	0.28	1.14	0.99	0.92	0.83	0.66	0.47	0.46

✓✓: Top 10 Percent ✓:Top 25 Percent ✗: Bottom 25 Percent ✗✗: Bottom 10 Percent

This table ranks 22 programs for which we can identify at least 10 women graduates who placed into an economics doctoral program from 1987 to 2017. The ranking is conducted according to relative student outcomes for women versus men. Details for the ranking procedure are in Appendix B. Icons represent a school's position in the distribution of each statistic, with averages by group in the lowest three rows. Share women is the fraction of first year women over all first year students for the whole period. For other statistics, programs are ranked on relative rates, where a value greater than 1 reflects women outperforming men (or positive growth in the case of Δ share women). Δ share women reports share women graduates (women graduates divided by total graduates) from 2006-2017 divided by share women graduates from 1993-2005 (see Figure 1). Retention is the share women graduates from 1999-2017 divided by share women first year students from 1994-2012. This statistic compares the gender ratio at entrance and graduation, allowing 5 years for matriculation. Placement at PhD is US doctoral placement from CSWEP, as defined for Figure 3. Placement rank, Top 55, Top 5, and Promotion are as defined for Figure 4.

Chapter 3

Tenure Clock Extension Policies

3.1 Introduction

In this paper, I explore how university policies granting tenure clock extensions around childbirth or adoption affect the likelihood of promotion for female and male junior academics in a variety of disciplines at top universities. A recent paper by Antecol, Bedard, and Stearns (2018), hereafter ABS, found that at top-50 economics departments, gender-neutral extension policies led to a significant increase in the likelihood that men would earn tenure in the institution where they were initially hired, but a decrease in the likelihood for women. Since many of these policies were initially adopted to help universities reduce gender disparities in their tenure process, these stark findings raised questions about whether such policies were well-advised, or should be abandoned. Notably however, ABS drew only on a sample of top departments of economics—a gender-imbalanced field compared to similar departments in the social science and business disciplines. This raises the question of whether gender-neutral tenure clock extension policies inherently advantage men over women, and if not what determines the impacts for men versus women.

This paper will follow ABS's approach in exploring the impacts of these tenure clock extension policies, looking at policy variation at the same set of universities and the same span of time, with the same specifications. Variation comes from the timing with which

female-only and gender-neutral tenure clock extension policies became available for new parents at the various universities. Unlike ABS, however, I examine outcomes for assistant professors from three other departments—sociology, accounting, and political science—at the same institutions. For these departments, I can draw on the full roster of assistant professors hired in this time frame, with the same covariates as used in ABS's analysis. In these cases, I can also use full faculty rosters and department chair lists to identify other key covariates for the departments. I find that unlike in economics, in these departments gender-neutral and female-only family leave policies increase the likelihood that assistant professors will earn tenure, both in absolute terms and relative to men. Although this finding by no means contradicts the results from ABS, it adds complication and nuance to our understanding of how these policies perform.

To explore one possible source of this divergence, I next conduct subgroup analysis to compare the impacts of gender-neutral family leave policies in departments with higher or lower female representation among key decision-makers. Whether we think that tenure is granted as a reward for past work, or based on expected future productivity, decision-makers in the department and the market more broadly must compare the work that was previously done against some standard factoring in the time allotted. It is reasonable to think that men and women may have different implicit assumptions about how the benchmark productivity should change for a new parent. More fundamentally, it is possible that faculty are imperfectly informed about the availability or implementation of such tenure clock extension policies, and that a junior professor's gender and the gender makeup of their department's leadership both matter in determining whether and how they use these policies.

I use two proxies for female decision-making power in a department. The first measures the share of women among the senior faculty in the department at the start of the analysis period, roughly in the early 1980s. The second is a measure, at the individual level, of how many years an assistant professor is exposed to female department chairs during their first six years on the job. Both in departments that start the analysis period with a high number

of female senior faculty, and among assistant professors who work under female department chairs, gender-neutral tenure clock extension rules lead to larger gains for women and smaller gains—or even losses—for men compared to departments with lower concentrations of senior women. This is true even when looking separately at ABS's sample of economics assistant professors versus sociology, accounting, and political science faculty.

This paper is related to an important body of work on family leave and flexibility policies and labor force outcomes, especially for women. This work generally reveals that such programs increase women's employment and even earnings. Thomas (2019) provides an excellent recent review focused mostly on the United States, while Hegewisch & Gornick (2010) provide a broader OECD-wide context. An important caveat to these findings has been in the nature of the jobs women return to after family leave. Blau and Kahn (2013) find that differences in family leave policies account for more than a quarter of the difference in labor force participation between the US and other developed nations, but that American women are more likely to work full time, as managers, or in professional occupations. Thomas (2019) demonstrates how family leave policies can blur the signals that let employers distinguish between family-oriented and career-oriented workers, reducing both investment in early-career women and their subsequent promotions, even while raising their labor supply. This paper probes one possible mechanism behind these types of unintended consequences.

This paper also connects to a body of a literature on how the characteristics of managers affects outcomes for their firms and workers. A growing number of papers study differences in how male and female managers affect their workplaces (Bertrand et al., 2018; Ehrenberg et al., 2012; Droganova, 2018; Husain et al., 2018; Langan, 2019; Matsa and Miller, 2013). This paper deepens that literature by focusing on how managers influence the impact of a single policy.

Finally, this paper contributes to the specific policy debate about tenure clock extension rules. ABS are a significant contributor to this literature, and their paper occasioned

a great deal of discussion about policies of this type, [1]. Past work on these policies includes Manchester et al. (2013) who examined the impacts of a clock-stopping policy at a single institution, and Thornton (2008) who studied implementation issues and perceptions surrounding tenure clock extension policies in US and Canadian economics departments.

The remainder of the paper is structured as follows: In section 2, I describe the data used in this project. In section 3, I lay out my specification and results, and in section 4, I conclude.

3.2 Data

My main sample consists of a list of assistant professors hired into departments of sociology, political science, and accounting at the forty-nine universities included in ABS's sample. For accounting and sociology, these lists are drawn from annual faculty rosters compiled by the Prentice Hall Guide to Accounting Faculty and the American Sociological Association's Guide to Graduate Departments of Sociology. My data for these publications stretches back to the mid-1970s. For political science, I rely on faculty lists available from editions of the Guide to Graduate Study in Political Science published in 1984, 1986, 1989, 1992, 1995, 1998, and 2002.[2] Here the sample period is truncated, and because these volumes are available only intermittently, I will miss any individuals in political science who both are hired into and exit the sample entirely within the interval between publication years. My sample also lacks some subject-university pairs, and duplicates pairs in other cases. For instance, Carnegie Mellon University and Washington University in St. Louis lacked a sociology department for much of the period covered by this analysis, while Ohio State University has both a Sociology and a Rural Sociology department represented in the sample.

[1] See, e,g., https://www.chronicle.com/article/Is-It-Time-to-Stop-Stopping/237391 and https://www.nytimes.com/2016/06/26/business/tenure-extension-policies-that-put-women-at-a-disadvantage.html

[2] Chapter 1 did not analyze faculty rosters in political science. They are included here since the limited number of departments and individuals involved simplified the task of obtaining outcome measures.

Overall, my data differ in two important respects from ABS's data. First is the outcome of interest: Because there are many individuals for whom I cannot observe final tenure status, my dependent variable is an indicator equal to 1 if I observe an individual as an associate or full professor at the institution where they were hired as an assistant professor. I observe promotion, while ABS use actual tenure status to account for schools who promote without necessarily granting tenure. For political science, since individuals may earn promotions but still leave the sample prior to being re-observed, I use web searches to confirm the final academic rank at the hiring institution for individuals who I do not observe as associate or full professors before they exit the sample.[3] In some cases I observe political science faculty move to a new university between survey years and cannot observe their final rank at the original university. When they move and remain an assistant professor, I count it as not having been promoted at the hiring university. When they receive a promotion to associate professor, I consider them to have been promoted at the hiring university only if they move to a substantially higher-ranked institution. If a faculty member does not reappear in a subsequent survey round and I cannot locate any information for them through a web search, I assume they were not promoted to associate before leaving the university.

The second difference pertains to their sample restrictions (see page 7 of their published version). Their sample includes individuals "who started their first job as an assistant professor at a top-50 economics department within two years of receiving their PhD", but excludes individuals who publish fewer than two research papers in their early career.[4] I can exclude individuals who take more than two years to start their job, and those who have previous jobs at departments that I can observe, but unless I observe someone start work immediately upon graduating I cannot ensure that they are in their first-ever job (or even first academic job). I also do not attempt to exclude based on publication counts.

[3]This includes all the faculty who enter the sample in the 2002 wave.

[4]There is some ambiguity in this sentence as to whether this must mean a sample member's first job ever, or their first job at a top-50 institution. Earlier versions of the paper suggest the first interpretation is correct.

Our data also differ slightly in the covariates we include. ABS control for whether an individual had ever done a post-doc, which I do not observe. They also control for several university-by-year-level covariates obtained from IPEDS: total number of faculty, undergraduates, and grad students, fraction of the faculty that are female and fraction that are full professors, average salary for full and assistant professors, and total university revenue. I have gathered the same set of covariates—with a few substitutions where I am unable to match their exact values—using linear interpolation and extrapolation to fill in any missing values. In my main specifications I omit these covariates because they reduce the adjusted R-squared value, but the sign and magnitudes of the impacts are essentially the same when I include them.

The second part of my analysis splits the sample departments into places with high and low female representation among decision-makers, to observe how this affects the impacts of gender-neutral tenure clock extension policies. This is done in two ways. First, I separate departments into those with below- or above-median representation of women among the senior faculty in the early 1980s, relative to other top departments in their field. Second, I separate individual assistant professors into those who ever—versus never—work under a female department chair in the first six years of their career. In this analysis, I include the data on economics departments gathered by ABS, although I find the same qualitative result when analyzing economics and the other fields separately. Data on the share of women on the senior faculty come from the same faculty rosters used to identify assistant professors.[5] Data on department chairs also come from these rosters for accounting and sociology. In political science and economics (where I likewise see only intermittent roster data over some time periods), I supplement the roster chair identities using data from Peterson's Guide to Graduate Programs in the Humanities and Social Sciences, or from individual CVs and other web search results.

[5]For economics, faculty rosters to identify senior faculty in the department are drawn from the AEA's biographical survey of members published in 1985.

The set of schools included in ABS's sample are those universities with top-fifty economics programs in 2010, minus Caltech, Brown, and Pitt (the first for its idiosyncratic program structure, the latter two for lacking a full roster of assistant professors for the sample period). A three-way tie for fiftieth place leaves forty-nine institutions in their sample. Since I use the same set of schools, the political science, sociology, and accounting programs are not necessarily all of the same top-fifty caliber. However, program ranks are highly correlated across the three social science departments. Accounting programs are not ranked by US News in the same way as other fields, but most of these schools issue PhDs in accounting, and in fact many of the largest programs are represented among these forty-nine.

3.3 Specification and Results

This paper explores the impact of a specific policy implemented at a non-comprehensive set of US universities. Identification comes from variation in timing of policy changes across universities, and in the timing of hires around the time the policy was first instituted. For these estimates to be unbiased, we must make a number of assumptions: most importantly, that the policy regime at an assistant professor's university at the time they are hired is uncorrelated with unobserved characteristics that lead them to be promoted. We might be concerned if we thought the best job candidates had a significant range of choice in the market, and that this policy was a deciding factor for them. However, since tenure clock extension policies are only one among many employee benefits provided in faculty jobs, it seems reasonable to assume that no significant bias would result from correlation between these policies and unobservable faculty characteristics.

My primary specification follows ABS, but omits the post-doc indicator and university-year covariates derived from IPEDS. I exclude the former because I cannot reliably measure it, and the latter because I find including these variables reduces the adjusted R-squared of the regressions without changing the qualitative finding. Furthermore, my data sources

permit me to add observations for individuals hired before 1980 in sociology and accounting for subsidiary analysis, but IPEDS covariates are not available prior to 1980.

Thus, for an individual i of gender g, hired in year t, in subject s at university s, my specification is as follows:

$$Y_{igsjt} = \sum_{g=m,f} \alpha_1^g * GNE_{jy}^g + \alpha_2^g * FOE_{jy}^g + \alpha_3^g * GN_{jy}^g + \alpha_4^g * FO_{jy}^g$$

$$+\sigma_{gsj} + \eta_{gys} + \beta X_i + \epsilon_{isjt}$$

Y is a binary outcome variable indicating whether an individual is ever observed as an associate or full professor at j, prior to moving permanently to another university. σ_{gsj} and η_{gys} are gender- and subject-specific indicators for hiring university and year. X_i is a set of subject-specific indicators for the revealed rank of an individual's PhD institution (based on their prevalence in the sample, following ABS).

There are four mutually-exclusive policy indicators that can be in effect at university j in year y: FOE (for individuals hired in year 0-3 after a female-only policy is instituted); FO (for individuals hired four or more years after a female-only policy is instituted); GNE (for years 0-3 after a gender-neutral policy); and GN (four or more years after a gender-neutral policy).[6] I do not include main effects for these four policies, but rather interact all four with separate indicators for men and women, creating eight mutually-exclusive gender-by-policy variables, indexed by the g superscript. This allows me to match the display format used by ABS in their tables, and also simplifies interpretation of the impact of the policies on men and women.

Column 1 of Table 1 shows the policy impacts for economics, controlling only for genders-specific department effects, gender- and subject-specific year effects, and subject-specific PhD rank effects.[7] As shown in their paper, men have a significantly higher change of earning

[6]In cases when a gender-neutral policy is instituted at a school with a pre-existing female-only policy, FO and FOE will both be set to zero.

[7]I have made two modifications to the ABS economics data that affect the results shown in tables 3.1 and 3.2. First, I recoded two observations in their data which appeared to have erroneous policy

tenure under a gender-neutral tenure clock policy than in the absence of a policy, while for women the point estimate of the impact is negative. The difference with the effect for men is highly significant. Column 2 shows the same policy impacts, estimated on sociology, accounting, and political science assistant professors hired between 1980 and 2005, while column 3 extends the sample period to include earlier cohorts of sociology and accounting assistant professors, as well (starting with the 1976 and 1973 cohorts, respectively). In these subjects, there is no impact on men, but a significant positive impact for women. Notably, in these fields female-only policies also have much higher point estimates than were estimated in economics, although they are still insignificant.

When we include university-year controls from IPEDS, we see the same patterns.[8] Although the estimated impacts of gender-neutral policies are no longer significant in the 1980-2005 sample using alternative fields, they are opposite-signed and significantly different than point estimates even half as large as those estimated for economics.

One possible mechanism by which heterogeneity could arise in the treatment effects across fields, or even across departments within a field, is through variation in the presence of women among a department's senior faculty. Female junior faculty may be more likely to learn about applicable policies, consider strategic ways to use them, or to have the circumstances of their tenure clock extension judged more favorably at the time of promotion when they have more female senior colleagues.

To test this possibility, I divide the sample in two ways. First, I calculate the median level of female representation on the senior faculty, by subject, in the earliest year for which I have data. I then estimate the impacts of tenure clock extension policies in departments

indicators. Furthermore, I have edited the IPEDS covariate series slightly because, in the course of extending them to my own data using publicly-available data from the Department of Education I was unable to precisely match their values in every case. Appendix Table 3.7 crosswalks the ABS main result using their data and my edits to their data, while Appendix Table 3.8 crosswalks their results to the version using only the controls available for the alternate departments sample—fixed effects for hiring department, year, gender, and PhD rank.

[8]These covariates are missing from years prior to 1980, so I set all values to zero in these years and allow the year-by-subject-by-gender and gender-by-department fixed effects to absorb this variation, on average.

with female representation above and below the median, as shown in Table 3.3. (Since three quarters of accounting departments have no women in the early 1980s, and how these departments are grouped affects the qualitative outcome, I estimate models on five different samples, grouping the median departments with both the high- and low-representation groups and also on their own.) In these and the regressions that follow I omit university-year controls, because of the reduced sample size.

Second, I use the year-by-year identities of department chairs to identify junior faculty who are ever exposed to a female department chair in their first six years on the job. I have not gathered chair data for all departments in the sample, and many accounting departments (which typically belong to larger business schools) lack their own chair. I exclude individuals from these departments, shrinking the sample by 363 individuals. I split the sample into those never versus ever exposed, since eighty percent of individuals in my sample never work under a female chair. Results are shown in table 3.4. For both these sample splits, I also show results separately for economics and for my three alternate department groups, in tables 3.5 and 3.6.

In many cases, the parameter values obtained are extreme, or even out of bounds for linear probability models. The sample is also rather small considering the large number of fixed effects included in the regressions, and the sample splits.[9] Rather than take the estimates at face value therefore, I note simply the consistency of their pattern. The departments with more senior women show lower policy impacts for men, and higher impacts for women, than the departments with fewer senior women. These results are consistent with what we would expect if senior women helped determine the impacts of tenure clock extension policies in their departments, and if their intervention tilted the scales in some regard towards female junior faculty.

[9]For instance, there are no individuals in the economics sample hired in the "early" period following the implementation of a female-only policy who ever work under a female department chair.

3.4 Conclusion

Unlike Antecol et. al (2018) in their study of economics programs, I find that in sociology, accounting, and political science gender-neutral tenure clock extension policies benefit women, both in absolute terms and relative to men. Though my findings for female-only policies are not statistically significant at traditional levels, their point estimates also look more beneficial to women than what ABS found.

A key difference between sociology, political science, and accounting and economics is their gender makeup—for much of the period covered by this research, all three alternate fields had substantially more women in the upper ranks of the faculty than did economics. And indeed, even when splitting the sample in economics, I find that exposure to more senior women is correlated with better impacts for female junior faculty.

The granting of tenure is a highly idiosyncratic, individualized process. In spite of the results ABS found for economics departments, my findings suggest we still have much to learn about whether and for whom tenure clock extension policies provide a benefit. Moreover, while my findings do not establish definitively that a manager's gender is the deciding factor, the subgroup analyses suggest female leadership could play a role in how these policies affect men and women's chances of earning tenure. The many choices that must be made to implement these policies provide ample scope for local conditions to affect their impacts, both within and across fields.

Table 1: Impacts of Tenure Clock Extension Policies on Promotion Likelihood

	(1)	(2)	(3)
	Econ	Soc., Acct., Poli. Sci.	
	1980-2005	1980-2005	All Pre-2006
New Hires, Policy Years 0-3			
Gender-Neutral	0.0441	0.00381	0.0343
	(0.0780)	(0.0572)	(0.0540)
Gender-Neutral * Female	0.0197	0.101	0.0997
	(0.105)	(0.0797)	(0.0772)
Female-Only	-0.0203	-0.0189	-0.00638
	(0.0665)	(0.0864)	(0.0819)
Female-Only * Female	0.201	0.141	0.144
	(0.124)	(0.101)	(0.0992)
New Hires, Policy Years 4+			
Gender-Neutral	0.150*	0.000227	0.0604
	(0.0782)	(0.0613)	(0.0556)
Gender-Neutral * Female	-0.173	0.167*	0.183*
	(0.113)	(0.0958)	(0.0940)
Female-Only	-0.0128	0.0568	0.0930
	(0.0713)	(0.0766)	(0.0726)
Female-Only * Female	0.0522	0.147	0.150
	(0.0976)	(0.108)	(0.103)
N	1392	2556	3090
R^2	0.164	0.260	0.233

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired through 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure (for model 1) or was promoted to Associate Professor (models 2-3) at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject). Standard errors clustered by department.

Table 2: Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Including Controls for University-Year Level Covariates

	(1)	(2)	(3)
	Econ	Soc., Acct., Poli. Sci.	
	1980-2005	1980-2005	All Pre-2006
New Hires, Policy Years 0-3			
Gender-Neutral	0.0502	-0.0118	0.0197
	(0.0802)	(0.0570)	(0.0528)
Gender-Neutral * Female	0.00502	0.0916	0.104
	(0.107)	(0.0785)	(0.0765)
Female-Only	-0.0183	-0.0406	-0.0113
	(0.0663)	(0.0849)	(0.0817)
Female-Only * Female	0.223*	0.141	0.147
	(0.121)	(0.101)	(0.0967)
New Hires, Policy Years 4+			
Gender-Neutral	0.172**	-0.0279	0.0540
	(0.0835)	(0.0659)	(0.0568)
Gender-Neutral * Female	-0.174	0.119	0.178*
	(0.110)	(0.0934)	(0.0928)
Female-Only	0.00332	0.0195	0.0529
	(0.0734)	(0.0668)	(0.0620)
Female-Only * Female	0.0633	0.115	0.121
	(0.101)	(0.0927)	(0.0878)
N	1392	2556	3090
R^2	0.167	0.273	0.245

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired through 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure (for model 1) or was promoted to Associate Professor (models 2-3) at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject), plus subject-specific linear controls in total number of undergrads, grad students, and faculty, female and full professor share of all faculty, average salaries for assistant and full professors, and total university revenue. Standard errors clustered by department.

Table 3: Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Departments by Initial Share of Women on the Senior Faculty

	Policy Impacts When Department-Specific Share Female is ... Relative to the Subject-Specific Median				
	Less	Less/Eq	Equal	Higher/Eq.	Higher
New Hires, Policy Years 0-3					
Gender-Neutral	0.0381	0.0298	0.0668	0.0227	0.0214
	(0.0580)	(0.0559)	(0.158)	(0.0648)	(0.0730)
Gender-Neutral * Female	-0.0344	0.0527	0.507**	0.167*	0.124
	(0.114)	(0.101)	(0.221)	(0.0910)	(0.107)
Female-Only	0.105	0.0549	-0.0354	-0.104	-0.159*
	(0.0837)	(0.0767)	(0.218)	(0.0787)	(0.0896)
Female-Only * Female	0.335***	0.267**	-0.170	0.0412	-0.00290
	(0.105)	(0.107)	(0.195)	(0.123)	(0.175)
New Hires, Policy Years 4+					
Gender-Neutral	0.222***	0.167***	-0.0397	-0.0489	-0.0320
	(0.0604)	(0.0596)	(0.153)	(0.0693)	(0.0777)
Gender-Neutral * Female	0.0343	0.106	0.533*	0.152	0.109
	(0.144)	(0.128)	(0.266)	(0.110)	(0.130)
Female-Only	-0.0240	0.0179	0.174	0.0529	0.0370
	(0.0638)	(0.0578)	(0.167)	(0.0726)	(0.0977)
Female-Only * Female	0.00535	0.0260	0.0209	0.202*	0.276**
	(0.124)	(0.107)	(0.200)	(0.117)	(0.137)
N	1707	2180	473	2241	1768
R^2	0.269	0.290	0.435	0.315	0.322

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired from 1980 to 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure (for econ) or was promoted to Associate Professor (for other subjects) at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject).

Table 4: Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Individuals who Work Under Female Department Chairs During Years 0-5 After Hiring

	Work Under Female Chair	
	Never	Ever
	(1)	(2)
New Hires, Policy Years 0-3		
Gender-Neutral	0.0185	-0.374***
	(0.0599)	(0.139)
Gender-Neutral * Female	-0.00330	0.207
	(0.0884)	(0.208)
Female-Only	0.00503	-0.505**
	(0.0598)	(0.210)
Female-Only * Female	0.210**	0.492
	(0.0847)	(0.432)
New Hires, Policy Years 4+		
Gender-Neutral	0.118*	-0.520**
	(0.0616)	(0.234)
Gender-Neutral * Female	0.0630	0.313
	(0.113)	(0.312)
Female-Only	0.0251	-0.275
	(0.0598)	(0.218)
Female-Only * Female	0.0860	0.433*
	(0.0982)	(0.233)
N	2934	651
R^2	0.260	0.507

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired from 1980 to 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure (for econ) or was promoted to Associate Professor (for other subjects) at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject). Standard errors clustered by department.

Table 5: Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Departments by Initial Share of Women on the Senior Faculty (Separately by Subject)

	Low		High	
	Econ	Others	Econ	Others
	(1)	(2)	(3)	(4)
New Hires, Policy Years 0-3				
Gender-Neutral	0.0660	0.0122	0.0276	0.00967
	(0.0632)	(0.0874)	(0.129)	(0.0820)
Gender-Neutral * Female	0.0348	0.00771	0.0280	0.139
	(0.208)	(0.104)	(0.154)	(0.129)
Female-Only	0.0516	0.0687	-0.0932	-0.267
	(0.122)	(0.100)	(0.0897)	(0.176)
Female-Only * Female	0.395*	0.220*	0.243	-0.0133
	(0.214)	(0.123)	(0.269)	(0.192)
New Hires, Policy Years 4+				
Gender-Neutral	0.269***	0.0740	-0.0316	-0.0265
	(0.0770)	(0.0769)	(0.122)	(0.103)
Gender-Neutral * Female	-0.308	0.173	0.125	0.108
	(0.213)	(0.145)	(0.194)	(0.153)
Female-Only	-0.0483	0.117	0.0222	0.0504
	(0.0722)	(0.0869)	(0.118)	(0.143)
Female-Only * Female	0.0974	-0.0335	0.373	0.268
	(0.155)	(0.133)	(0.259)	(0.161)
N	722	1459	670	1097
R^2	0.235	0.293	0.192	0.312

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired from 1980 to 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure (for models 1 and 3) or was promoted to Associate Professor (models 2 and 4) at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject). Standard errors clustered by department.

Table 6: Impacts of Tenure Clock Extensions on Promotion and Tenure Likelihood at the Policy Institution, Separating Individuals who Work Under Female Department Chairs During Years 0-5 After Hiring (Separately by Subject)

	Work Under Female Chair			
	Never		Ever	
	Econ	Others	Econ	Others
	(1)	(2)	(3)	(4)
New Hires, Policy Years 0-3				
Gender-Neutral	0.0453	-0.00726	-0.323	-0.380**
	(0.0810)	(0.0891)	(0.399)	(0.154)
Gender-Neutral * Female	0.00984	-0.00942	1.846***	0.157
	(0.109)	(0.109)	(0.333)	(0.212)
Female-Only	0.0205	-0.0215	N/A	-0.519**
	(0.0694)	(0.108)		(0.212)
Female-Only * Female	0.138	0.225**	N/A	0.467
	(0.136)	(0.105)		(0.433)
New Hires, Policy Years 4+				
Gender-Neutral	0.176**	0.0381	-0.227	-0.603**
	(0.0825)	(0.0922)	(0.974)	(0.229)
Gender-Neutral * Female	-0.222*	0.163	1.351***	0.256
	(0.119)	(0.145)	(0.255)	(0.323)
Female-Only	-0.0188	0.0941	-0.191	-0.286
	(0.0742)	(0.0892)	(0.935)	(0.231)
Female-Only * Female	-0.0137	0.130	1.063	0.396*
	(0.103)	(0.139)	(1.477)	(0.236)
N	1270	1664	122	529
R^2	0.168	0.254	0.510	0.497

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired from 1980 to 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure (for econ) or was promoted to Associate Professor (for other subjects) at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject). Standard errors clustered by department.

Table 7: Crosswalking Impacts Calculated with Original ABS Econ Sample to Edited Econ Sample

	Data Edits			
	None	Univ. Controls	Policy Dummies	Both
	(1)	(2)	(3)	(4)
New Hires, Policy Years 0-3				
Gender-Neutral	0.0511	0.0517	0.0512	0.0516
	(0.0787)	(0.0796)	(0.0786)	(0.0796)
Gender-Neutral * Female	-0.0166	-0.0316	-0.00344	-0.0185
	(0.107)	(0.107)	(0.109)	(0.108)
Female-Only	-0.00846	-0.00249	-0.00848	-0.00250
	(0.0670)	(0.0660)	(0.0669)	(0.0659)
Female-Only * Female	0.172	0.199	0.193	0.216*
	(0.140)	(0.128)	(0.130)	(0.113)
New Hires, Policy Years 4+				
Gender-Neutral	0.176**	0.171**	0.176**	0.171**
	(0.0826)	(0.0824)	(0.0825)	(0.0824)
Gender-Neutral * Female	-0.194*	-0.205*	-0.176	-0.186*
	(0.106)	(0.104)	(0.109)	(0.105)
Female-Only	0.00226	0.00829	0.00226	0.00839
	(0.0747)	(0.0745)	(0.0745)	(0.0744)
Female-Only * Female	0.0493	0.0520	0.0638	0.0664
	(0.101)	(0.105)	(0.103)	(0.105)
N	1392	1392	1392	1392
r2	0.165	0.162	0.165	0.163
r2_a	0.0510	0.0497	0.0512	0.0499

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired from 1980 to 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject). Standard errors clustered by department. Edits to ABS data include two hires in the same university and year which appear to have been assigned the wrong policy indicators, and small edits to the values of certain university-level covariate series when it was impossible to match them exactly based on IPEDS.

Table 8: Crosswalk from ABS Econ Data to my Edited Econ Data, Eliminating University-Year Controls

	Unedited		Edited	
	(1)	(2)	(3)	(4)
New Hires, Policy Years 0-3				
Gender-Neutral	0.0465	0.0441	0.0465	0.0441
	(0.0773)	(0.0780)	(0.0773)	(0.0780)
Gender-Neutral * Female	-0.000498	0.00869	0.00878	0.0197
	(0.103)	(0.104)	(0.105)	(0.105)
Female-Only	-0.00352	-0.0202	-0.00352	-0.0203
	(0.0662)	(0.0665)	(0.0662)	(0.0665)
Female-Only * Female	0.191	0.195	0.190	0.201
	(0.134)	(0.136)	(0.123)	(0.124)
New Hires, Policy Years 4+				
Gender-Neutral	0.158**	0.150*	0.158**	0.150*
	(0.0752)	(0.0782)	(0.0752)	(0.0782)
Gender-Neutral * Female	-0.188*	-0.190*	-0.173	-0.173
	(0.104)	(0.109)	(0.108)	(0.113)
Female-Only	-0.00531	-0.0128	-0.00531	-0.0128
	(0.0721)	(0.0713)	(0.0721)	(0.0713)
Female-Only * Female	0.0453	0.0414	0.0537	0.0522
	(0.0979)	(0.0953)	(0.100)	(0.0976)
PhD Rank Controls?	No	Yes	No	Yes
N	1392	1392	1392	1392
R^2	0.156	0.164	0.156	0.164

[1] Std. err. in parens. * $p < .1$, ** $p < .05$, *** $p < .01$

[2] Sample includes Assistant Professors hired from 1980 to 2005 in departments shown at 49 policy universities. Outcome variable is an indicator variable equal to 1 if the individual received tenure at the institution where they are observed. "Gender-Neutral" or "Female-Only" indicate that such a policy was in effect in the year the individual started working at the policy institution. All models include fixed effects for year and department of hire (separately by subject and gender) and degree institution category (separately by subject). Standard errors clustered by department. Edits to ABS data include two hires in the same university and year which appear to have been assigned the wrong policy indicators, and small edits to the values of certain university-level covariate series when it was impossible to match them exactly based on IPEDS.

Appendix A

Appendix Tables and Figures

Table 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	by Year 8	Ever	by Year 8	Top 5	Top 55
	(1)	(2)	(3)	(4)	(5)	(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 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] * $p < .1$, ** $p < .05$, *** $p < .01$

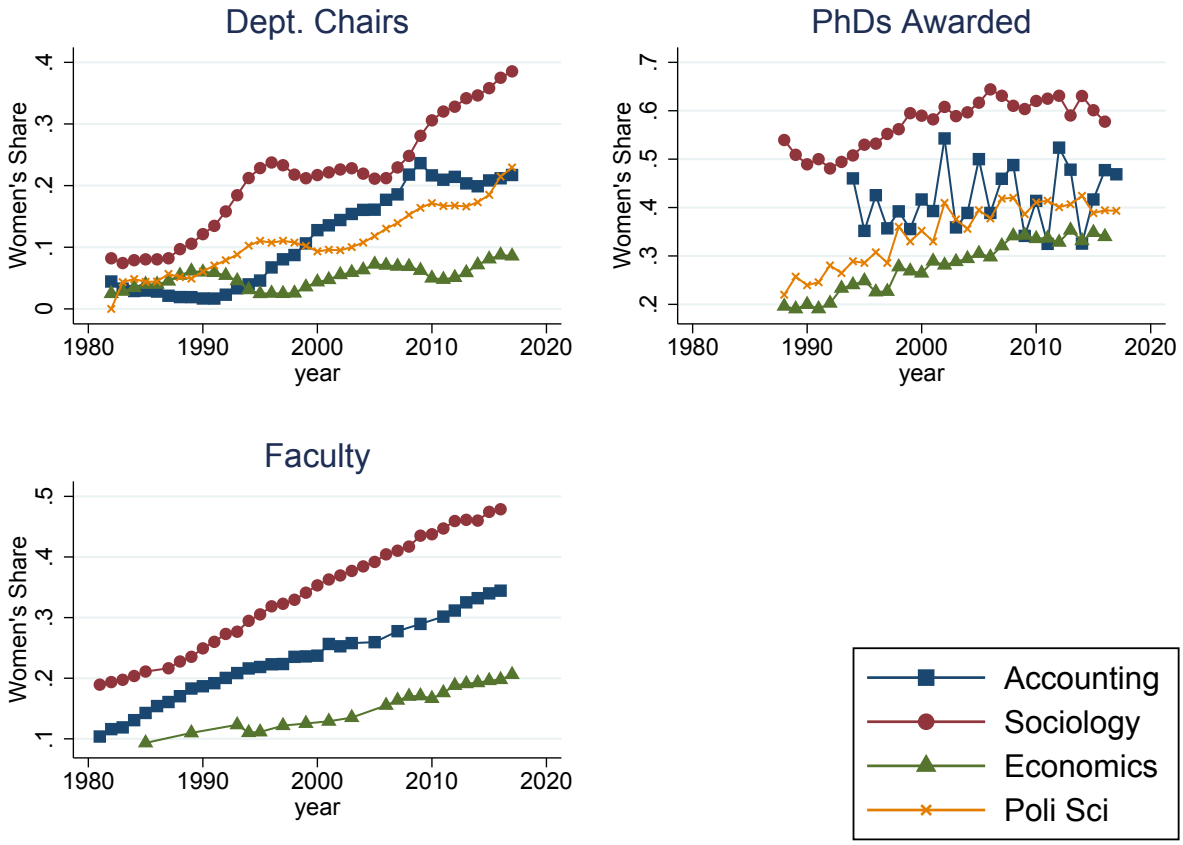
[2] Coefficients and 95% confidence intervals are plotted from linear probability models that control for department and subject-specific year and rank fixed effects, a third-order polynomial in time since receiving a PhD, and indicators for the chairperson or having been chair in the past five years. 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 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

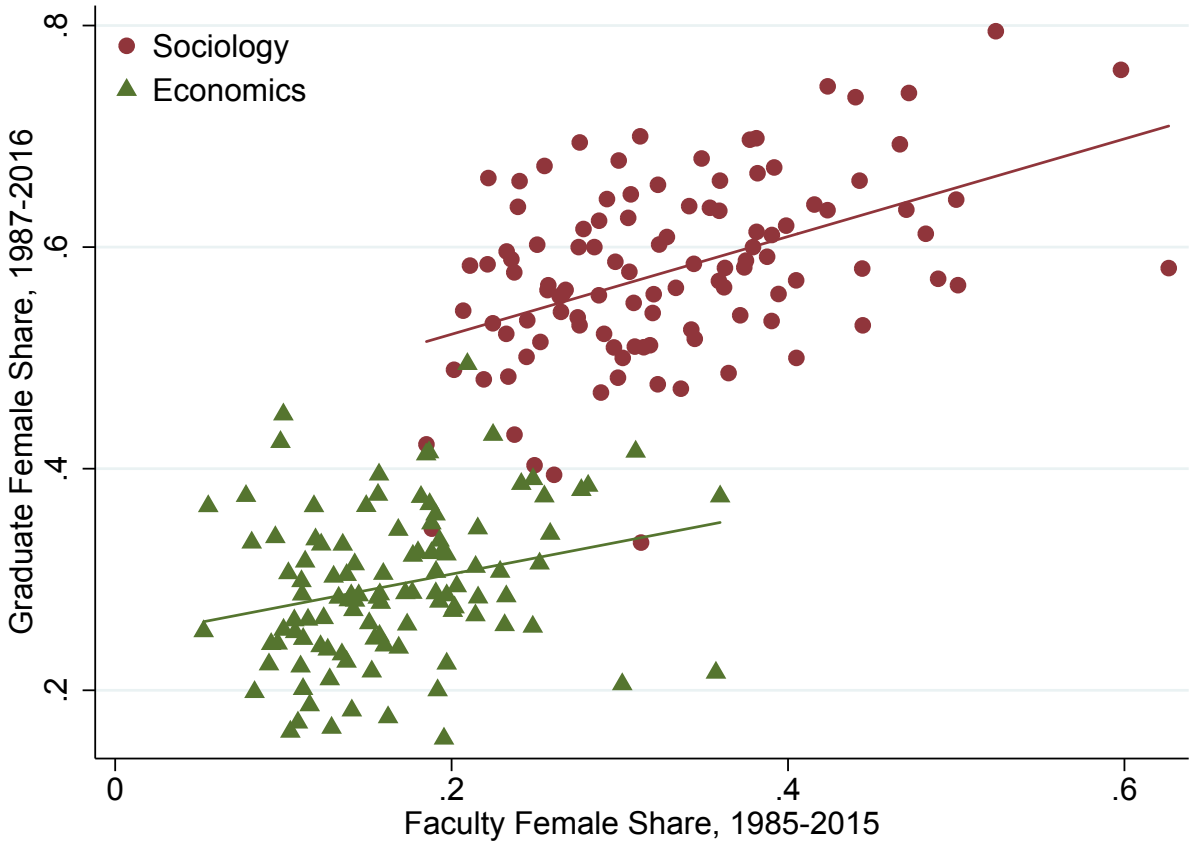
* $p < .1$, ** $p < .05$, *** $p < .01$. Results are from negative binomial regressions, whose outcome variable is a count of total hires of a given rank and gender in a given department and year. Treated observations represent those years in which the department is chaired by a woman. Models control 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.



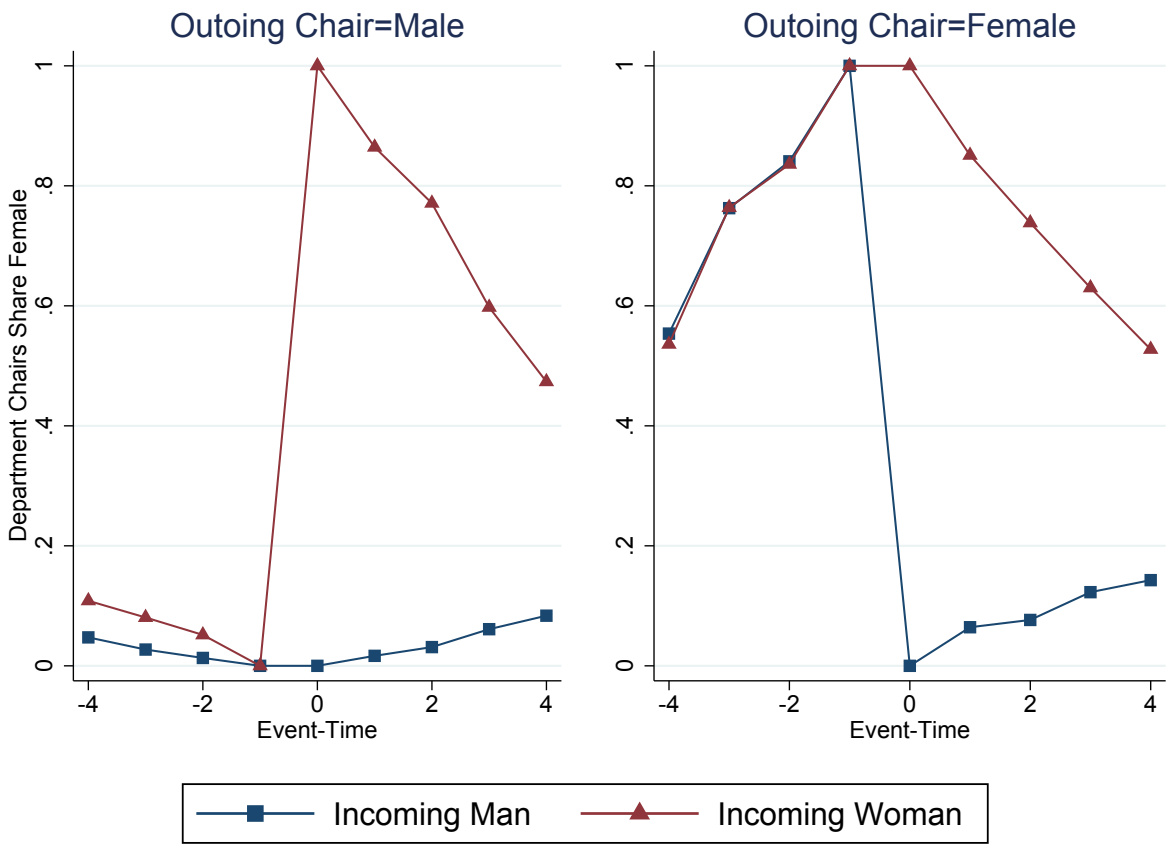
Source: Author's list of department chairs and faculty roster database (Faculty), IPEDS PhD count by field and institution (PhDs). Department chair series is a 5-year moving average to reduce volatility. Faculty roster data are unavailable for political science.

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



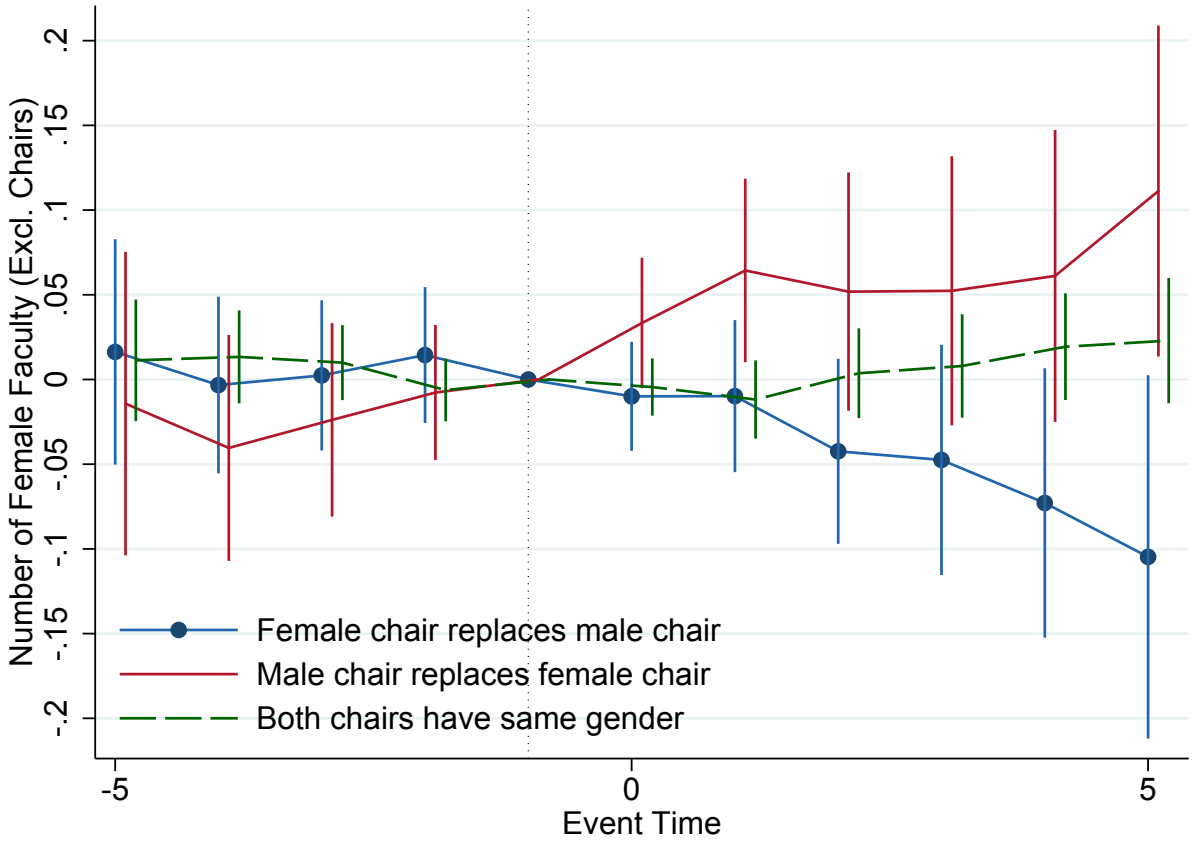
Source: Author's faculty roster database and IPEDS. Departments shown are limited to those with at least fifty PhD graduates since 1987. Accounting PhD departments are excluded because of their small size (only nine meet the size requirements), and Political Science departments are excluded because faculty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

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



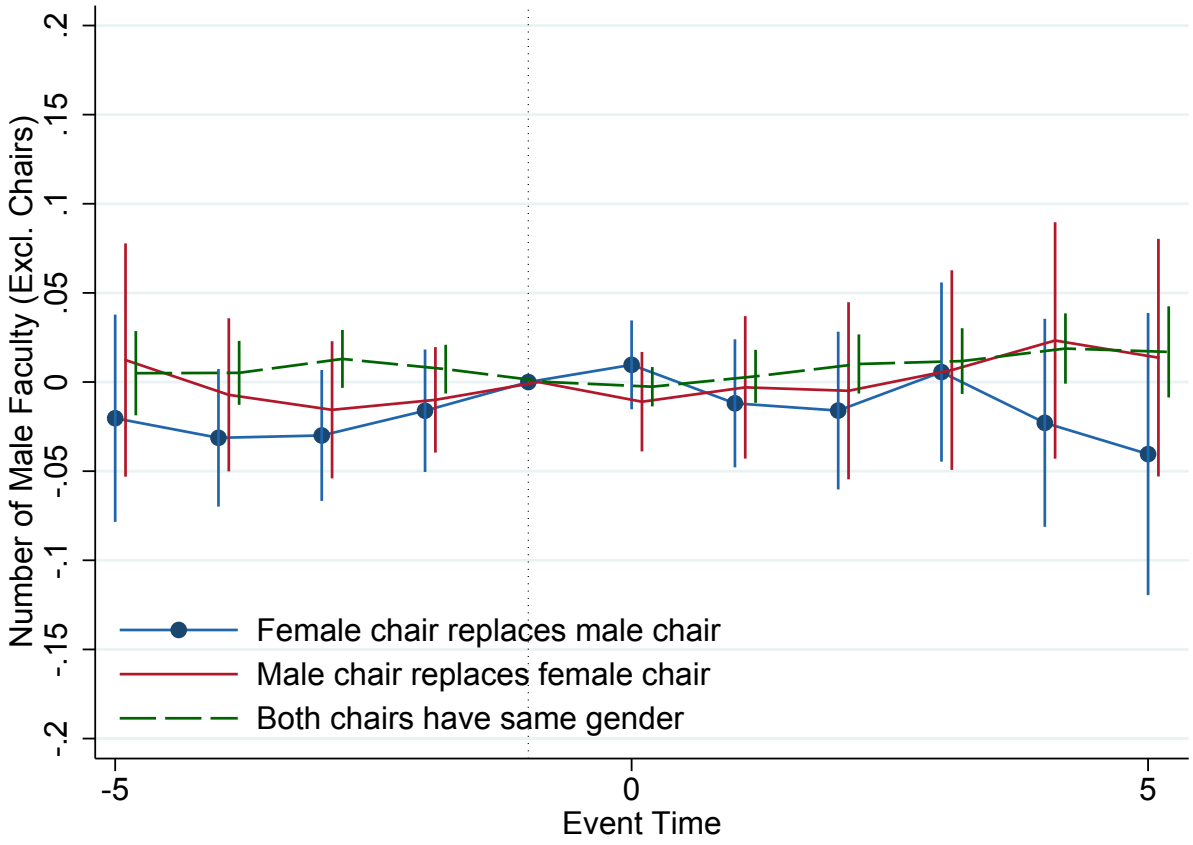
Figures show the share of female chairs at departments of economics, sociology, accounting, or political science experiencing one of four types of transition between two department chairs. Fractions in period -1 and 0 (the last year of the outgoing chair's term and first year of the incoming chair's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

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



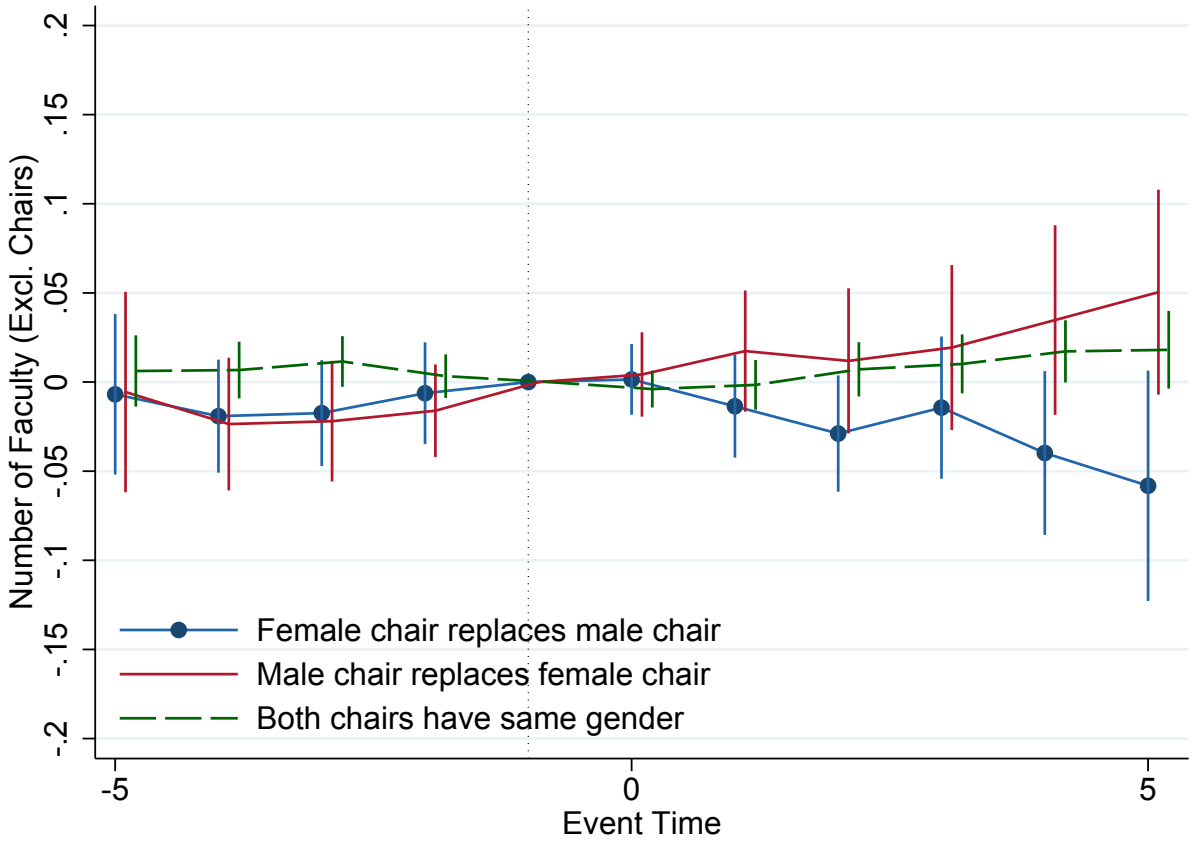
Coefficients and 95% confidence intervals are plotted with from an event study Poisson regression on observations at the department-year level. The outcome variable is the total number of female tenure-track faculty, excluding individuals who are ever observed as the department chair, regressed on event-time 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.



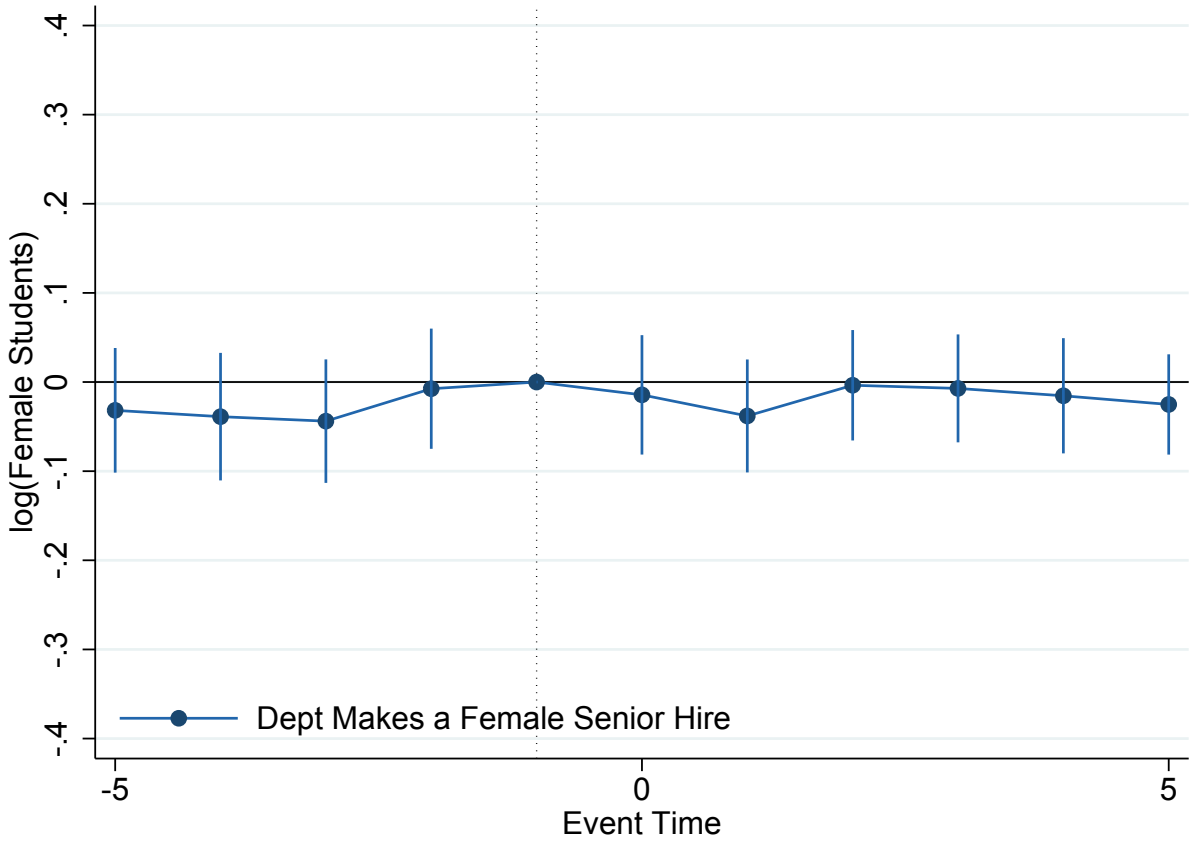
Coefficients and 95% confidence intervals are plotted with from an event study Poisson regression on observations at the department-year level. The outcome variable is the total number of male tenure-track faculty, excluding individuals who are ever observed as the department chair, regressed on event-time 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.



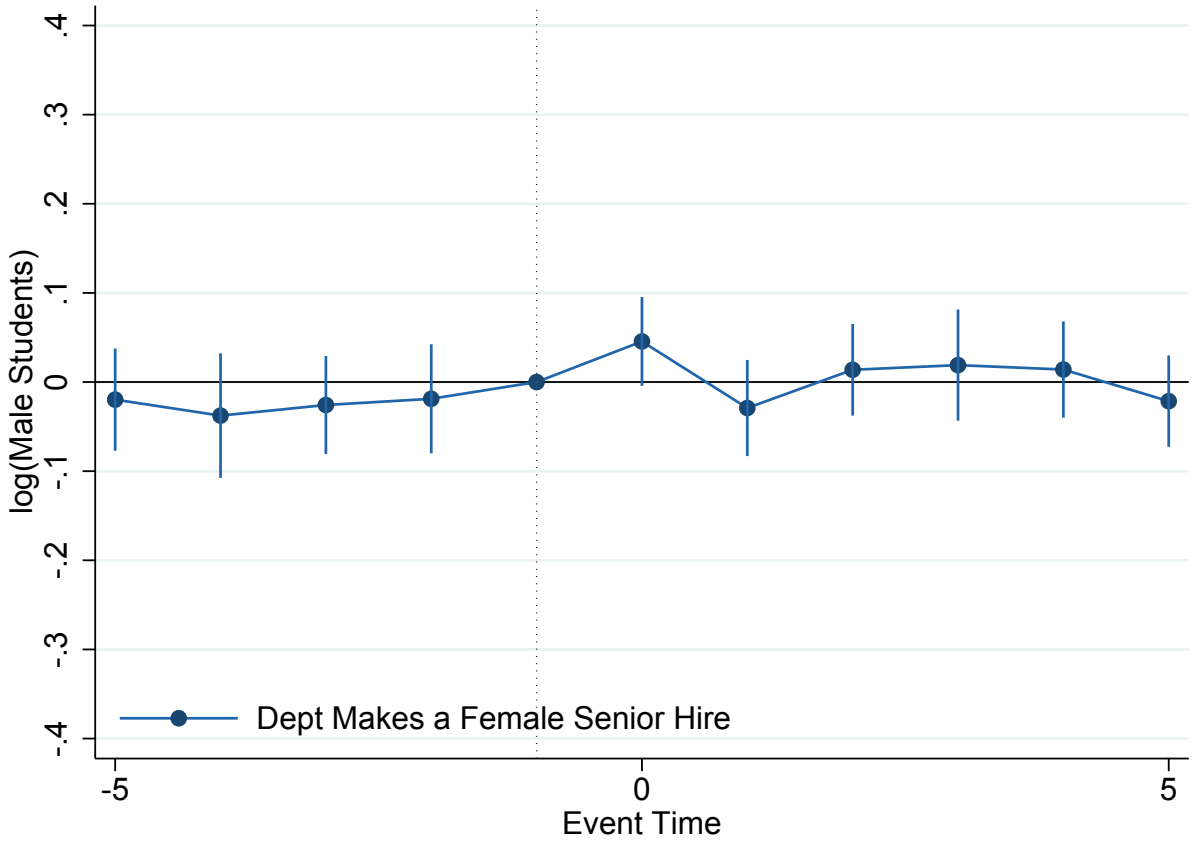
Coefficients and 95% confidence intervals are plotted with from an event study Poisson regression on observations at the department-year level. The outcome variable is the total number of tenure-track faculty, excluding individuals who are ever observed as the department chair, regressed on event-time 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.



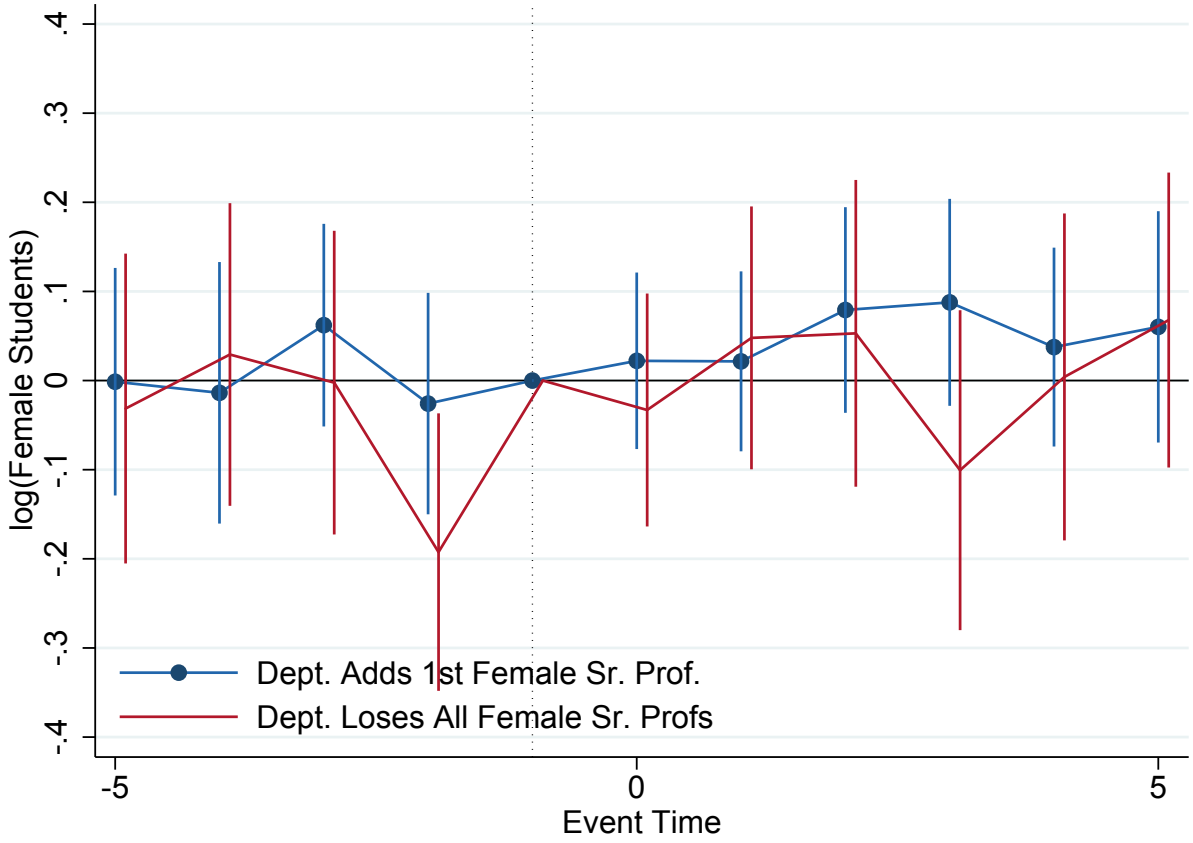
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of female first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 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.



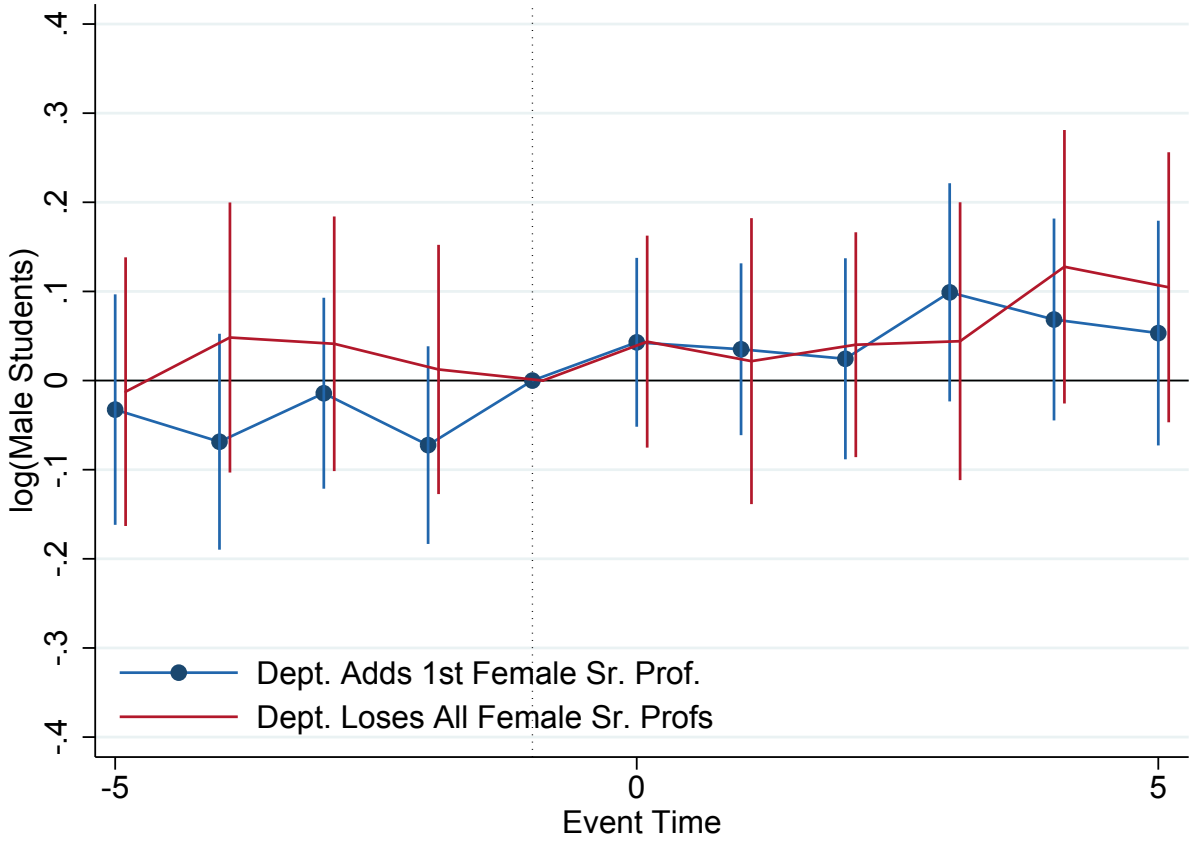
Event study coefficients and 95% confidence intervals are plotted from Poisson regressions on observations at the department-by-year level, whose outcome is the total number of male first-year graduate students entering doctoral departments in political science, sociology, and economics in 1986 through 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.*

Appendix B

Ranking Methodology for Table 2.2

To sort PhD programs according to women's success compared to men's, we used eight metrics for each department:

1. Share women: the share of all first-years in the department, from 1994-2012, who are women.

2. Change in share women: the percent change in the share of women among departmental PhD graduates from 1994-2005 to 2006-2017.

3. Retention:the share of women among graduates from 1999-2017 over the share of women among first year students from 1994-2012.

4. Doctoral Placement: women's placement rate, relative to men's, into tenure-track faculty roles in any PhD-granting department.

5. Placement Rank: the average US News rank for women's economics doctoral placements, relative to men's placements from the department. For consistency of sign with other metrics, we replace rank with 100-(US News rank), so that the "best" programs are ranked 99. Programs without a US News ranking are assigned a value of zero on this scale.

6. Top 55: the ratio between women's and men's average number of publications in top 55 journals within seven years after graduating with a PhD.

7. Top 5: the ratio between women's and men's average number of publications in top 5 journals within 7 years after graduating with a PhD.

8. Promotion: the relative probability that a woman, compared to a man, who graduated from the program before 2011 is observed as an associate or full professor within 12 years after receiving her/his PhD.

We then converted each of these relative rates into z-scores with respect to the distribution of 22 schools, and calculated the mean z-score over the eight metrics. (For the four schools where fewer than 10 women graduate before 2011, we treat the promotion measure as missing.) We first looked for jumps in the mean z-scores to establish natural groupings. On the "less equal" side of the distribution, there were two large jumps of .2 standard deviations each. These served as natural division points into the "less equal" (<-.2 SDs) and "least equal" (<-.5 SDs) groups. There were fewer large shifts in the "more equal" side of the distribution, so we identified smaller shifts in the same vicinity as the "less equal" thresholds. After sorting the PhD programs into one of the five categories, we assigned anonymous IDs from A-V based on the average z-score, with A being the highest.

Appendix C

Interview script

Hi, is this XX?

Hi XX, My name is Leyla Mocan and I'm a research assistant working with Professor Boustan at Princeton on a project about women in economics for the Journal of Economic Perspectives. How are you? We are talking to several departments and trying to learn more about the outcomes and experiences of men and women in graduate school. We were hoping to talk to you to learn more about the experience of students in your PhD Program. All of your comments will be anonymous, and we will not disclose your department either. Do you have any questions before we start? [pause]

- To start, could you tell me a bit about the graduate program? Do most students who start the program end up graduating with a PhD?
  - Followup: Are all students guaranteed funding or is the funding competitive?
- How do students first get connected with their advisors? When does this match tend to take place (first year, second year, after)?
- How frequent would you say the average students' contact with their advisor is? Do students tend to meet with their advisors weekly or monthly, or more sporadically?
- Are their regular seminars or workshops for students to interact with faculty?

  - What is the seminar culture like?

- What is the classroom culture like?
- What is job market preparation process like for the average student?
  - Is the process very structured or professionalized?
- What do you think is distinctive about your graduate program? What sets it apart?
- Is the department doing anything specific to address the special challenges faced by women in economics?
  - Do you see any difference in the environment for men and women?
- Is there anyone else who I should talk to?
  - Specifically, we were hoping to talk to two additional people to get a deeper sense of your department: one would be a faculty member (or former faculty member) who can offer a long-run perspective, and another would be a graduate of the PhD program, preferably from before 2010, again so that person would have a longer-run perspective.

Bibliography

Abrevaya, Jason and Daniel S Hamermesh, "Charity and favoritism in the field: Are female economists nicer (to each other)?," *Review of Economics and Statistics*, 2012, *94* (1), 202–207.

Antecol, Heather, Kelly Bedard, and Jenna Stearns, "Equal but Inequitable: Who Benefits from Gender-Neutral Tenure Clock Stopping Policies?," *American Economic Review*, 2018, *108* (9), 2420–41.

Athey, Susan, Christopher Avery, and Peter Zemsky, "Mentoring and diversity," *American Economic Review*, 2000, *90* (4), 765–786.

Babcock, Linda, Maria P Recalde, Lise Vesterlund, and Laurie Weingart, "Gender differences in accepting and receiving requests for tasks with low promotability," *American Economic Review*, 2017, *107* (3), 714–47.

Bagues, Manuel F and Berta Esteve-Volart, "Can gender parity break the glass ceiling? Evidence from a repeated randomized experiment," *The Review of Economic Studies*, 2010, *77* (4), 1301–1328.

Bagues, Manuel, Mauro Sylos-Labini, and Natalia Zinovyeva, "Does the gender composition of scientific committees matter?," *American Economic Review*, 2017, *107* (4), 1207–38.

Bayer, Amanda and Cecilia Elena Rouse, "Diversity in the economics profession: A new attack on an old problem," *Journal of Economic Perspectives*, 2016, *30* (4), 221–42.

Bertrand, Marianne and Esther Duflo, "Field Experiments on Discrimination," in "Handbook of Economic Field Experiments," Vol. 1, Elsevier, 2017, pp. 309–393.

_ , Sandra E Black, Sissel Jensen, and Adriana Lleras-Muney, "Breaking the glass ceiling? The effect of board quotas on female labor market outcomes in Norway," *The Review of Economic Studies*, 2014.

Bettinger, Eric P and Bridget Terry Long, "Do faculty serve as role models? The impact of instructor gender on female students," *American Economic Review*, 2005, *95* (2), 152–157.

Blank, Rebecca M, "The effects of double-blind versus single-blind reviewing: Experimental evidence from the American Economic Review," *The American Economic Review*, 1991, pp. 1041–1067.

Blau, Francine D and Lawrence M Kahn, "Female labor supply: Why is the United States falling behind?," *American Economic Review*, 2013, *103* (3), 251–56.

_ **and** _ , "The gender wage gap: Extent, trends, and explanations," *Journal of Economic Literature*, 2017, *55* (3), 789–865.

Bradley, Steven W, James R Garven, Wilson W Law, and James E West, "The Impact of Chief Diversity Officers on Diverse Faculty Hiring," Technical Report, National Bureau of Economic Research 2018.

Bransch, Felix and Michael Kvasnicka, "Male Gatekeepers Gender Bias in the Publishing Process?," Technical Report, Institute for the Study of Labor (IZA) 2017.

Brogaard, Jonathan, Joseph Engelberg, and Edward Van Wesep, "Do Economists Swing for the Fences after Tenure?," *Journal of Economic Perspectives*, 2018, *32* (1), 179–94.

Brown, Ryan, Hani Mansour, and Stephen OConnell, "Closing the Gender Gap in Leadership Positions: Can Expanding the Pipeline Increase Parity?," 2018.

Carrell, Scott E, Marianne E Page, and James E West, "Sex and science: How professor gender perpetuates the gender gap," *The Quarterly Journal of Economics*, 2010, *125* (3), 1101–1144.

Ceci, Stephen J, Donna K Ginther, Shulamit Kahn, and Wendy M Williams, "Women in academic science: A changing landscape," *Psychological Science in the Public Interest*, 2014, *15* (3), 75–141.

Clauset, Aaron, Samuel Arbesman, and Daniel B Larremore, "Systematic inequality and hierarchy in faculty hiring networks," *Science advances*, 2015, *1* (1), e1400005.

Dahl, Gordon, Andreas Kotsadam, and Dan-Olof Rooth, "Does integration change gender attitudes? The effect of randomly assigning women to traditionally male teams," Technical Report, National Bureau of Economic Research 2018.

Duflo, Esther, "Women empowerment and economic development," *Journal of Economic Literature*, 2012, *50* (4), 1051–79.

Ehrenberg, Ronald G, George H Jakubson, Mirinda L Martin, Joyce B Main, and Thomas Eisenberg, "Diversifying the faculty across gender lines: Do trustees and administrators matter?," *Economics of Education Review*, 2012, *31* (1), 9–18.

Foundation, The Rockefeller, "Women in Leadership: Why it Matters." https://www.rockefellerfoundation.org/report/women-in-leadership-why-it-matters/.

Gershenson, Seth, Cassandra MD Hart, Constance A Lindsay, and Nicholas W Papageorge, "The Long-Run Impacts of Same-Race Teachers," Technical Report, Institute for the Study of Labor (IZA) 2017.

Ginther, Donna and Shulamit Kahn, "Academic Womens Careers in the Social Sciences," in Alessandro Lanteri and Jack Vromen, eds., *The Economics of Economists: Institutional Setting, Individual Incentives, and Future Prospects.*

Ginther, Donna K and Shulamit Kahn, "Women in economics: Moving up or falling off the academic career ladder?," *Journal of Economic perspectives*, 2004, *18* (3), 193–214.

Giuliano, Laura, David I Levine, and Jonathan Leonard, "Manager race and the race of new hires," *Journal of Labor Economics*, 2009, *27* (4), 589–631.

Goldin, Claudia, "Gender and the Undergraduate Economics Major: Notes on the Undergraduate Economics Major at a Highly Selective Liberal Arts College." https://scholar.harvard.edu/files/goldin/files/claudia_gender_paper.pdf[Accessed April 25, 2019], year=2015.

Hale, Galina and Tali Regev, "Gender ratios at top PhD programs in economics," *Economics of Education Review*, 2014, *41*, 55–70.

Hasselback, James R, Alan Reinstein, and Mohammad Abdolmohammadi, "Benchmarking the research productivity of accounting doctorates," *Issues in Accounting Education*, 2012, *27* (4), 943–978.

Hegewisch, Ariane and Janet C Gornick, "The impact of work-family policies on women's employment: a review of research from OECD countries," *Community, Work & Family*, 2011, *14* (2), 119–138.

Hengel, Erin, "Publishing while Female. Are women held to higher standards? Evidence from peer review.," 2017.

Hilmer, Christiana and Michael Hilmer, "Women helping women, men helping women? Same-gender mentoring, initial job placements, and early career publishing success for economics PhDs," *American Economic Review*, 2007, *97* (2), 422–426.

Hsieh, Chang-Tai, Erik Hurst, Charles I Jones, and Peter J Klenow, "The allocation of talent and us economic growth," Technical Report, National Bureau of Economic Research 2013.

Jacobs, Jerry A, "Journal Rankings in Sociology: Using the H Index with Google Scholar," *The American Sociologist*, 2016, *47* (2-3), 192–224.

Karaca-Mandic, Pinar, Nicole Maestas, and David Powell, "Peer Groups and Employment Outcomes: Evidence Based on Conditional Random Assignment in the US Army," Technical Report, working paper 2013.

Kofoed, Michael S and Elizabeth mcGovney, "The effect of same-gender and same-race role models on occupation choice: Evidence from randomly assigned mentors at west point," *Journal of Human Resources*, 2017, pp. 0416–7838r1.

Kunze, Astrid and Amalia R Miller, "Women helping women? Evidence from private sector data on workplace hierarchies," *Review of Economics and Statistics*, 2017, *99* (5), 769–775.

Manchester, Colleen Flaherty, Lisa M Leslie, and Amit Kramer, "Is the clock still ticking? An evaluation of the consequences of stopping the tenure clock," *ILR Review*, 2013, *66* (1), 3–31.

Mansour, Hani, Daniel I Rees, Bryson Rintala, and Nathan Wozny, "The Effects of Professor Gender on the Post-Graduation Outcomes of Female Students," Technical Report, Institute for the Study of Labor (IZA) 2018.

Matsa, David A and Amalia R Miller, "Chipping away at the glass ceiling: Gender spillovers in corporate leadership," *American Economic Review*, 2011, *101* (3), 635–39.

_ **and** _ , "A female style in corporate leadership? Evidence from quotas," *American Economic Journal: Applied Economics*, 2013, *5* (3), 136–69.

McDowell, John M, Larry D Singell Jr, and Mark Stater, "Two to tango? Gender differences in the decisions to publish and coauthor," *Economic inquiry*, 2006, *44* (1), 153–168.

Moss-Racusin, Corinne A, John F Dovidio, Victoria L Brescoll, Mark J Graham, and Jo Handelsman, "Science facultys subtle gender biases favor male students," *Proceedings of the National Academy of Sciences*, 2012, *109* (41), 16474–16479.

Neumark, David and Rosella Gardecki, "Women helping women? Role model and mentoring effects on female Ph. D. students in economics," *Journal of Human Resources*, 1998, *33* (1), 220–247.

Porter, Catherine and Danila Serra, "Gender differences in the choice of major: The importance of female role models," Technical Report, Southern Methodist University, Department of Economics Departmental Working Papers 2017.

Sarsons, Heather, "Recognition for group work: Gender differences in academia," *American Economic Review*, 2017, *107* (5), 141–45.

Thomas, Mallika, "The Impact of Mandated Maternity Benefits on the Gender Differential in Promotions: Examining the Role of Adverse Selection," 2019. https://drive.google.com/file/d/1oCK7v2FUeWIyR39WFW10TKkH8npFdCJm/view.

Thornton, Saranna R, "The Implementation and Utilization of Stop the Tenure Clock Policies in Canadian and US Economics Departments," 2008. http://citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.208.8440&rep=rep1&type=pdf [Accessed March 13, 2019],.

Way, Samuel F, Daniel B Larremore, and Aaron Clauset, "Gender, productivity, and prestige in computer science faculty hiring networks," in "Proceedings of the 25th International Conference on World Wide Web" International World Wide Web Conferences Steering Committee 2016, pp. 1169–1179.

Weisshaar, Katherine, "Publish and perish? An assessment of gender gaps in promotion to tenure in Academia," *Social Forces*, 2017, *96* (2), 529–560.