Gender and Pay Trajectories in the US Federal Service: The Role of New Managers

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This paper exploits the appointment of new managers in a triple-differences event study design to trace the effects of managerial homophily (getting a same-sex manager) on employee pay using over 30 years of payroll data from the US Federal Civil Service. We find that getting a same-sex manager is particularly important for female employees, whose pay increases by an additional 1.5 log points relative to male counterparts. A novel finding is that these effects are largest for employees in less routine jobs, even within education levels. We conclude that even highly regimented pay systems are not immune to managerial discretion.

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I. Introduction

The US government has highly structured administrative pay scales, which theoretically limit the potential for demographic characteristics to influence pay through managerial discretion. Even in this rigid setting, we find that female employees disproportionally benefit from managerial homophily, specifically getting a same-sex manager. In the years following the appointment of a new same-sex manager, female employees' residual pay grows by up to 1.5 log points more than it does for male employees after the appointment of a male manager (herein the "differential homophily" effect).[1] These effects are economically significant and robust. Far from being an artifact of a bygone age, we document these effects across presidential eras: in fact, they have become more important as the female workforce became more highly educated and moved from clerical jobs into administrative careers (Goldin, Katz, and Kuziemko, 2006), and have, more generally, entered into less routine jobs. We also find that managerial homophily leads to higher promotion rates and occupational upgrading.

Using over 30 years of rich longitudinal data from the US Federal civil service, this paper investigates the effects of managerial homophily on employees' wage dynamics. During the study period (1984-2014), women were vastly under-represented in management, 45% of female federal employees encountered same-sex managers, while that percentage was 65% for male federal employees.[2] We ask: What effect does a same-sex supervisor have on their employees' careers and pay trajectories?

Taking advantage of longitudinal data, we exploit the appointment of new managers in a triple-differences event study design "à la Kleven" where those employees yet to be treated are used as controls. From the point of the employee, the timing of the arrival of the new manager is arguably as random as the arrival of a child for parents. Our event of interest is thus the appointment of a new manager at the office, and treatment occurs when the new manager is of the same-sex as the employee. That is, we compare employees in the same office for whom the managerial appointment generates a new manager of the same or opposite sex. Our event-study focuses on employees experiencing their first managerial team change.[3] Whether this first

[1] As explained below, we include traditional human capital variables as explanatory factors – age, tenure, education, locality-year and office and occupation classes when computing residual pay. Pay grades and levels are excluded as we show they are a key mechanism through which same-sex managers affect disparate pay by gender.

[2] The US federal service's female management share has increased steadily over time, yet it still under-represents its female workforce. The female management share in the US federal service increased from 24% in 1987 to 38% in 2014, while the female employee share remained fairly stable at around 48% (Appendix Figure A.1). For comparison, women comprise 38% of middle managers in US corporations (McKinsey, 2019).

[3] We focus our estimation on the effect of the first new manager who arrives at the office during each worker's tenure to avoid confounding it with the effects of subsequent managerial transitions.

managerial appointment is of the same-sex as the employee or not is our second difference and whether the employee is a female or not is the third difference. Thus the same managerial appointment in a particular office will correspond to a same-sex treatment for some employees and to an opposite-sex treament for others.

Our identifying assumption is that whether the first new manager at an employee's office is of the same-sex as the employee is conditionally exogenous. We condition on a host of fixed effects, including office-year fixed effects, which also control for the effects of executives at the sub-agency and agency levels, individual-office fixed effects, and quarter-year fixed effects. Controlling for office fixed effects helps assuage concerns about the potentially endogenous selection of new managers to be appointed to problematic workplaces. The dynamics and patterns of heterogeneity we find also contradict these selection concerns, as discussed in Section VI. Additionally, balance tests show new male and new female managers are similar, as are the offices they are appointed to. This conditional exogeneity assumption is further supported by parallel pre-event trends between female and male homophily effects. As a result, our differential (between female and male employees) homophily effects show no pre-trends. As shown in Olden and Moen (2022), the triple-difference estimator is identified in this case.

Our estimates correspond to an Intent-to-Treat (ITT) estimate for any individual employee, since a new manager may only impact a portion of the office's employees. We observe employees over the course of their federal careers and all managers appointed to their offices, but do not observe the exact pairing between employees and supervisors within each office. Our preferred estimates thus focus on the 80% of offices with 5 or fewer managers and at least 10 employees (approximately 3 managers and 25 employees, on average). In those settings, each manager represents a substantial share of a management team and a single managerial change results in a sizeable share of employees experiencing a change in supervision. We find the expected dose response in our treatment effects estimates when we move to offices with larger managerial teams. We complement our event-study analysis with a TWFE-DDD of log pay residuals and intermediate outcomes, including GS grade, office switches, employee retention, and occupational changes.

First and foremost, our central finding sounds a cautionary note for hopes that replacing performance pay and employee negotiations with algorithmic pay systems will close the gender pay gap. Our results show that administrative pay systems may simply provide the illusion

of a cure. Figure 1 illustrates the average unexplained pay gap in each year as the distance between the average male and female residuals (solid symbols), which hovers between 1.9 and 3.1 log points.[4] However, when we include endogenous pay plans and grades (hollow symbols), the male and female residuals are much closer to each other. The endogenous pay grid hides the underlying gender pay gap. One potential mechanism behind these sets of unexplained disparities is observationally equivalent male and female workers moving through the grid at different paces. Thus, Figure 1 illustrates a key point: most (85%) of the unexplained pay gap over time operates through workers placement on the pay grid, a decision heavily influenced by their managers.[5]

Below we present evidence that new managers, especially female managers, help employees move up the grid, therefore illustrating that placement in the grid has a discretionary component. Consistent with this, Biasi and Sarsons (2022) show that moving from seniority pay to performance pay led to a re-emergence of the gender pay gap in Wisconsin teachers' salaries, an effect that was muted in schools with female principals or supervisors In the case of race, Aneja and Guo (2022) show that employment segregation in the Wilson Era was effectively implemented through downward placement in the occupational grid.

The breadth of the Federal civil service is a key advantage of our setting. The US federal civil service employs over 1 million people each year, spread geographically in close to 75,000 unique local offices and across 500 different occupations with varying levels of female representation and pay.[6] This enables us to go beyond the existing literature, which typically focuses on a single firm or industry, and identify the circumstances and types of occupations where same-sex managers matter most.

Indeed, an important novel finding here is that, even within education levels, the differential homophily effects are largest in less routine jobs, which have increased from a third of federal employees in 1987 to almost a half by 2014. Such workers may have less objectively measurable productivity, leading to potentially more subjective evaluations. This result echoes Mastrorocco and Tesso's (2023) finding that in the 19th century US Civil Service workers with

[4]Pay residuals accounting for age, education, occupation, part-time status, tenure, office and locality, as explained below, are averaged in each year by gender for our sub-sample of offices with at most 5 managers.

[5]Appendix Figure A.1 plots the raw gender pay gap over time. Between 1987 and 2014 it decreased from 22 to 11 log points. Also shown is the pay gap adjusted for gender differences in observed human capital (education), occupation, and age. A sizeable gap, the vertical distance between the two curves (shaded area), remains unexplained. As the two curves evolve in parallel, the unexplained gap as a share of the raw gap increases over time: from 13% to 37% in Panel A and from 25% to 50% in Panel B.

[6]The OPM data include the employees' agency, sub-agency, and duty stations – the physical locations where the employees work. We define a local office or workplace as the agency-sub-agency-duty station where the employee works. Their occupations range from equipment operators earning $20,000 per year to aerospace engineers earning over $120,000 per year.

personal connections were preferred for remote positions, which were costly to monitor.

Our paper also contributes to the broad literature on the role of managers in worker's careers by outlining the circumstances under which managerial homophily is most consequential. Managers can impact workers' careers and compensation through several mechanisms, including direct effects on worker productivity (Adhvaryu, Nyshadham, and Tamayo, 2022), assignment of promotable tasks (Babcock et al., 2017), and managers' evaluation abilities and leniency (Frederiksen, Kahn, and Lange, 2020). When interacting with people like themselves, individuals update their beliefs faster (Golub and Jackson, 2012), are more likely to make professional referrals (Zeltzer, 2020), are happier subordinates (Husain et al., 2021, and Grissom et al., 2012) and may exert more effort (Spenkuch, Teso and Xu, 2023). Homophily can occur along any dimension but most of the empirical employment literature focuses on gender, likely due to its ready availability in administrative data sets.

A closely related private sector study is Cullen and Perez-Truglia (2023), which estimates the effect of managers using data from a Southeast Asian firm with more traditional gender roles. It also uses an event-study design to document the promotion advantage of employees who are socially close to their immediate supervisors; such employees tend to be of the same gender.[7] Cullen and Perez-Truglia (2023) document a significant male homophily advantage, finding that "male managers promote a disproportionate share of male employees" (p.1704). Differential promotion rates, particularly early in one's career, are a key contributor to the gender pay gap (Bertrand, Goldin, and Katz, 2010; Bronson and Thoursie, 2022).

This paper also contributes to the growing literature on when women in leadership are more or less successful at improving women's pay. While the evidence on high-level initiatives is mixed, studies have found a general positive impact of female leadership on female employees' wages and promotion rates in corporate settings across several countries.[8] Recent lab, field, and natural experiments suggest that critical mass is a key factor for women in leadership (e.g., Bagues, Sylos-Labini, and Zinovyeva, 2017; Born, Ranehill,and Sandberg, 2022) and could explain the mixed results in the literature. Our findings suggest that differential homophily effects are larger in settings where women achieve a critical mass either at the co-worker or manage-

[7]That is consistent with Castilla's (2011) earlier finding that workers' performance evaluations improve when they rotate to a manager of the same-sex.

[8]See, for Portugal: Cardoso and Winter-Ebmer (2010), for Norway: Kunze and Miller (2017), for Italy: Flabbi et al. (2019), and for Germany: Bhide (2019). For evidence on the effects of improving female representation on boards of directors on employees' wages, see Bertrand et al. (2019), Dalvit, Patel, and Tan (2021), Maida and Weber (2022).

rial levels. Our extensive workplace controls enable us to distinguish them from higher-level agency-wide initiatives.

To further ascertain that homophily effects are felt in the field, we match our data on the share of same-sex managers with data from the 2008-2014 Federal Employee Viewpoint Surveys (FEVS), an annual survey of US federal employees. Consistent with our findings on pay, female employees' self-reported perceptions of job satisfaction, including pay satisfaction and opportunities for career advancement, are higher when the share of same-sex managers is above the median. These results are in keeping with recent studies finding a positive impact of female leadership on workplace climate (Tate and Yang, 2015; Lucifora and Vigani, 2022; Alan, Corekcioglu, Kaba, and Sutter, 2023).

The remainder of the paper is organized as follows. In Section II, we provide background on the evolution of pay setting in the federal civil service and the role of managerial assessments. In Section III, we summarize the unique features of the longitudinal administrative data we use. In Section IV, we outline our estimation and identification strategy. Section V reports the event study and difference-in-differences estimates and discusses threats to identification and the robustness of the results. Section VI addresses alternate explanations, and section VII concludes.

II. Pay Setting in the US Federal Service

Because of the large number of employees involved, around 1.5 million people, any pay increases have considerable budgetary repercussions. As a result, pay increases have involved direct Presidential intervention since the 1970s.[9] Below we provide a brief overview of relevant features of the Federal civilian employee pay-setting process partly based on Buckley (2009).

A. General Pay Schedule

The Pendleton Civil Service Act of 1883 created a merit system for Federal employment. The Classification Acts of 1923 and 1949 linked salaries to duties and applied consistent standards across Federal agencies, creating the "General Schedule" (GS) of pay. Johnson and Libecap (1989) suggest that this bureaucratic system helps insulate the civil service from politics. Standardizing compensation should also limit the scope for demographic pay disparities.

The core of the GS classification system establishes 15 pay grades, which we observe. They are based on the position's difficulty, responsibility, and required qualifications (e.g. educa-

[9]The Federal Pay Comparability Act of 1970 allowed for GS pay adjustments via executive action.

tion). Some discretion in the assignment of initial grade at hiring can still be exercised through "superior qualification actions," which are more commonly given to men (OPM, 2014a). The base pay within each GS grade is set at one of 10 fixed levels, called steps, which we do not observe.[10] Employees with acceptable performance progress through the steps following statutory waiting periods (typically one to three years). Up to one additional step increase per year can be awarded for outstanding performance (a "Quality Step Increase" or QSI). Employees in occupations with job ladders may advance non-competitively to higher steps and grades at fixed intervals. However, advancement to the highest grade that an employee is eligible for may be discretionary and competitive. Therefore one possible way for gender pay disparities to emerge is through differentential performance evaluations and associated discretionary pay increases.[11]

Beyond job classification and career progression, the overall salary grid is also adjusted for the cost of living over time and across locations. The Federal Employees Pay Comparability Act (FEPCA) of 1990 introduced locality-based pay to address challenges in recruitment and retention in high-wage areas. It also set a timetable for making Federal pay more competitive with private sector wages for employees doing comparable work in the same locality.[12] We begin our analysis by stripping locality-year fixed effects from employees' pay to absorb locality pay adjustments as explained in Section III below.

FEPCA also set up a process for yearly general increases in Federal pay. These pay adjustments are reviewed annually by Congress, which may legislate a different adjustment from the one authorized by the President. Political attitudes towards federal compensation have varied over time. During most of Clinton and Bush 43, Congress generally exceeded the President's adjustment, but the Obama Era was marked by pay freezes and small (1%) adjustments.[13]

In summary, federal civil service employees are paid according to an administrative pay system. Grade increases, QSIs, and statutory increases in the federal pay schedule and locality

[10]The Data Appendix Table 1 reproduces the GS salary table for 2012 (with rates frozen at 2010 levels). It is a matrix of 15 grades with 10 steps. It shows that step increases range from 2.4% to 3.3%, while grade increases (at step 1) range from 8.7% to 18.1%. In practice, the basic pay workers in GS grades 5 and 12 actually receive shows more steps than the prescribed ten (as shown in Data Appendix Figure 4), owing in large part to the locality pay adjustments, making it not feasible to precisely identify the steps.

[11]There are several alternative pay plans to the GS schedule. They primarily cover highly specialized workers (e.g. air traffic controllers). They have different salary grids, but otherwise function similarly. Around 20% of all federal employees are paid under non-GS plans (10% in our estimation sample). Our main estimates are unchanged when we restrict the sample to GS employees.

[12]Initially, there were just 29 locality areas; thirteen new locality areas were added in 2016, and there are currently 47 locality areas – regions where employees receive higher salaries. See Table 1-1 of CRS (2010) for example of the sizeable locality adjustments required by FEPCA.

[13]See GAO (2021) for a comparison of pay locality increases proposed by the Federal Pay Council, the President's Agent and Congress' alternative plans for 2015 to 2019.

pay adjustments can all result in salary increases. In the analyses below, we will control for the latter and other bureaucratic determinants to isolate the potential role of managers in helping individual employees move along the pay grid.

B. The Role of Managers

Federal managers typically start their federal careers in non-supervisory roles and advance to managerial positions. Most are recruited through a competitive process involving public job postings, qualification assessments, and interviews. Some managerial roles are filled through merit promotions based on performance, experience, and qualifications. There are also situations where external candidates are brought in to fill managerial roles. This may happen when the agency is seeking specialized expertise or skills that are not readily available among current employees. Occasionally, agencies may use a "direct hire" authority to bring in qualified candidates from outside the federal workforce quickly.[14]

In offices we study, each manager supervises 5 to 10 employees, on average. In addition to assigning tasks and providing supervision, managers evaluate their supervisees' performance and recommend them for step and grade increases. Contrary to popular belief, the federal service does link financial rewards to performance, not just tenure. Oh and Lewis (2013), who have access to performance ratings from 1988 to 2003, show that employees with "outstanding" performance ratings received raises that were two-thirds of a percentage point higher than those with "less than fully successful" ratings and these ratings had measurable effects up to two years later.

There is no minimum time under a supervisor before an employee can receive a performance rating, but it is typically a year and can vary by agency or in special circumstances. Specifically, managers have to implement the five phases of the performance management cycle: "1) planning work and setting expectations; 2) continually monitoring performance; 3) developing the capacity to perform; 4) rating periodically to summarize performance; and 5) rewarding good performance" (OPM, 2017). This rigorous and analytical appraisal process should dampen the effects of homophily (see Blair-Loy et al. 2022).

Despite relatively constrained performance review and pay setting, Federal pay has varied by race, sex, and locality more than can be explained by observed measures of qualifications

[14]Other promotions may fall under "excepted service" not subject to the standard procedure, when a position is policy-determining, confidential, or of a temporary nature and thus unlikely to be in our sample. For more details, see https://www.dol.gov/general/jobs/understanding-the-federal-hiring-process and https://www.opm.gov/policy-data-oversight/human-capital-management/hiring-authorities/.

(Lewis and Oh, 2009; Foster et al., 2020). Droganova (2018) shows that the gender gap in federal employees' wages and promotions is correlated with the share of female managers in the office, and presents estimates exploiting variation in the gender mix of managerial retirements that provide supportive evidence in the same direction.

III. Data

A. Employment Data

The primary data source for the paper is quarterly administrative payroll data made public by the US Office of Personnel Management (OPM) under the Freedom of Information Act (FOIA). We focus our analysis on civilian white-collar salaried permanent employees working for the federal government between 1982 and 2014. They constitute the overwhelming majority of employees (nearly 90%).

The OPM data provides details on each employee's federal employment history and pay, including their place of employment. For brevity, we simply refer to the employee's agency-sub-agency-duty-station as their office.[15] The data also contains information on each employee's job, including their detailed occupation and employment type (e.g., full, part-time, salaried, or hourly). We follow employees throughout their careers in the federal service, including any transitions between roles, offices, and agencies. Importantly, the data includes a rarely available "supervisory status" variable (6 levels) to distinguish managers from employees. Finally, the data contains quarterly information on each employee's compensation, most notably the government pay schedule under which they are paid (herein called the pay plan). We observe each employee's pay grade, but not their step within the grade.[16]

We supplement the quarterly data with HR data collected at the time the employee was hired and when they separated from government employment. These data include the individual's age, education, any prior federal service, and the reason for separation from the federal government (e.g., retirement). Because these hiring data are only available beginning in 1982, we begin our sample then. To follow workers' progression through their careers, we also impose a cohort restriction and only keep workers born in or after 1955 – the post-Pill cohorts. A data appendix provides additional details on the construction of the OPM data and each of

[15]The "duty station" is the physical location of the office where the employee works. Multiple federal agencies or sub-agencies can be located in the same physical office building. As we are interested in the employee's immediate office environment, we code each agency-sub-agency-duty station combination as a unique duty station and refer to it as an office.

[16]The workers in our data are paid under 112 separate pay schedules, but most workers (around 80% in the full data and 90% in our main estimation sample) are paid under the GS schedule.

the variables we use.[17]

The data released by OPM does not have employee race or gender but does include first and last names for most. Therefore, we imputed gender based on employees' names. OPM redacted the names of all employees in sensitive occupations, primarily law enforcement, regulatory agencies, and security roles. We were unable to impute gender for those employees and therefore exclude all employees in such workplaces (e.g., the Inspector General's office). We were able to successfully impute the gender of 70% of those whose full names were provided by OPM.[18] We exclude those whose gender we could not identify (see data appendix) from the employee sample, but we include all managers irrespective of our ability to impute their gender. The data appendix details the gender imputation and the excluded sub-agencies.

To ensure that we have enough observations to estimate office fixed effects, we restrict our sample to offices with at least 10 employees. We also restrict the sample to employees who work for the government for at least five years to ensure we observe them for multiple periods in the event study. The sample that imposes cohort, minimum office employee size, and minimum employee tenure restrictions is called the "All Offices" sample; the restrictions remove noise from the estimation but do not otherwise meaningfully affect the estimates below. The "All Offices" sample consisting of approximately 18 million quarter-year observations from about 250,000 unique female employees and 200,000 unique male employees across more than 10,000 offices is our primary sample when estimating log pay residuals with more precision, as discussed below. The "Up to 5 managers" sample is a sub-sample of "All Offices" that is limited to offices with at most 5 managers. It consists of 2 million quarter-year observations from approximately 80,000 unique female and male employees spread across almost 10,000 offices. In our event studies, we further restrict the sample to employees who eventually experience the appointment of a first new manager, as explained in section IV.B.

The key variables and sample characteristics from the "Up to 5 managers" sample, for all employees and those in the"Event study" subsamples are summarized in Table 1. Like in the broader labor market, there is a roughly 15% raw gender pay gap among the workers in our sample. The average female employee earns $50,000 per year and the average male employee

[17]The data appendix is available at https://maritrehavi.github.io/FMRdataappendix.pdf

[18]We imputed gender based on the frequency of the first name in male and female babies born in the employee's birth cohort in the Social Security Name files. Names were coded as being indicative of gender if at least 85% of babies born in the employee's cohort with the name had the same gender. We cross-validated our gender imputation algorithm with an extract of the EHRI-SDM data from 1992-2012 from Vilhuber (2018), including information on employee gender. In the sub-sample contained in both data sets, our gender imputation algorithm accurately classifies 95.11% of males and 99.24% for female employees.

earns roughly $7,000 more (Table 1A). A portion of the raw gender disparity can be explained by differences in human capital, such as education. The overwhelming majority of white-collar federal employees have at least some college education, but male employees are slightly more educated (Table 1B).

B. Data on Managers

Women's presence in management grew alongside their employment shares during our study period (Figure A.1). By 2014, 38% of managers were women, up from 24% in 1987. Despite these increases, women remain under-represented in management: in our data, women make up 45% of employees but only about 30% of managers (Table 1C). In addition to the inter-temporal variation, there is substantial variation in female management shares across agencies and across offices within agencies in each period, ranging from 0% to 100%.[19]

Our data on managers is extracted from the same archive of federal employee data used for workers. The managers' sample is a distinct non-overlapping extract and includes individuals whose gender we could not identify (about 18% of the manager sample).[20] We identify the new managers in each office by comparing the list of all employees designated as having managerial responsibilities in each office in each quarter. We define a new manager as a new person appearing on the managerial list in an office for the first time. These new managers are roughly split between employees who previously worked in the office in non-managerial roles and those who served as managers in other offices within the federal service (47% each).[21] Six percent were hired from outside the federal service. We refer to new managers not employed in a particular office in the quarter prior to their appointment as "external hires". To avoid the confounding effects of previous managerial changes, we estimate the effect of the first managerial change an employee experiences during their federal employment.[22] For the managerial transition to have a meaningful impact, employees need sufficient time with their initial new manager to be evaluated. We, therefore, only count an employee as having experienced a managerial transition if the employee has worked in the federal service for at least 6 months prior to the transition.[23] The "new same-sex manager" variable is equal to 1 if the employee and the first new manager at

[19] Appendix Figure A.2 displays the female management and employe shares for 12 large agencies in 1995 and 2014.

[20] Workers who become managers in the event window are excluded from the employee sample. Appendix Table A.1 summarizes the characteristics of managers. Managers and employees are defined using the supervisory status variable provided by the OPM. Managers are denoted with codes that meet definitions of Supervisor or Manager, Supervisor (CSRA), Management Official (CSRA), Leader, or Team Leader.

[21] Table A.2 contains characteristics of managerial transitions by gender.

[22] Figure A.8 and Table A.13 contain event study estimates for the effect of the second new manager. The same-sex effects associated with the second manager, regardless of employee gender, are not substantial.

[23] Estimates are robust to defining treatment as the first new managerial appointment after 1 year of service.

their office have the same sex, leaving opposite-sex managers and managers with unobserved sex in the base group.[24] Under this definition, around 40% of female and male employees experience a first new manager over the course of the sample period and approximately 40% of women's and 60% of men's first new managers are of the same-sex.

IV. Identification strategy

This section details the events we exploit and the rich specification our data allows us to estimate. Given the under-representation of women in management, our focus is the differential impact of same-sex managers on women's and men's pay. We leverage managerial turnover to estimate the changes in female employees' pay following the appointment of a new same-sex manager in comparison to the changes in pay in the male counterpart pairing. Our event of interest is the appointment of the employee's first new manager, and treatment occurs for the subset of employees who are the same sex as their first new manager. One advantage of this ITT set-up is that it excludes any reshuffling of individual supervision assignments within an office due to manager-employee match quality, productivity, or similar confounding sorting.

As explained in Section II above, some aspects of federal employee pay are outside the scope of managers' control, such as employee characteristics and locality pay premia. We therefore employ a conditional exogeneity estimation strategy similar to the covariate adjustment strategy suggested by Freyaldenhoven, Hansen, and Shapiro (2019) who advocate correcting for potential confounds by first residualizing outcomes using covariates unaffected by the event. Our results are robust to estimating the event study in a single step without residualizing covariates (Figure 7B). We begin our estimation by constructing a log pay residual for each employee– the pay purged of the observable characteristics of employees, offices, and localities. We then conduct our event-study analysis using quarterly and yearly events. In addition, we complement the event-study analysis with a triple differences (TWFE-DDD) analysis of the overall effect of the appointment of same-sex managers on residual log pay, promotions, retention (office moves), and occupational changes. We test the robustness of the estimates using Borusyak et al's (2021) staggered event timing estimator.

A. Estimation of Unexplained Pay Variation

An important component of pay for many employees from 1994 onward comes from locality-specific pay adjustments for living standards in different localities. As explained above,

[24] Our results are robust to excluding the managers of unobserved genders (Figure 7C).

following FEPCA, locality pay adjustments became a substantial component of pay increases. Localities contain multiple offices, and the number of offices with locality premia nearly doubled over our period. We, therefore, begin by using the "All Offices" sample to separately estimate locality-purged pay for each year.[25]

We then use the locality-purged pay to estimate pay unexplained by observable characteristics, again estimating the regression separately by calendar year for the "All Offices" sample

$$\hat{w}_{iqy} = X'_{iqy}\beta_y + \theta_{dy} + \omega_{iqy}, \quad y = 1987, ..., 2014, \tag{1}$$

where $\hat{w}_{iqy}$ is the residual stripped of locality pay for individual i in quarter q and year y, X_{iqy} are observable characteristics, including age, education, occupation, a part-time indicator, and tenure.[26] Office fixed effects, θ_{dy}, capture the features of offices in each year y, such as size and the female employee share, that are shared by all employees at the office. Since offices are defined at the agency-sub-agency-duty station level, office fixed effects also control for the effects of executives at the sub-agency and agency levels. We denote the resulting log pay residual as $\hat{\omega}_{iqy}$. Estimating the effects of observables by calendar year corresponds to a model with a full set of interactions between the explanatory variables and years.

B. Event study DDD

Our preferred event-study sample is restricted to employees who eventually receive a first new manager in an office with at most 5 managers prior to the managerial appointment.[27] We estimate the differential effects on male and female employees of receiving a new manager of the same-sex as the employee, $NSM_{idt} \cdot F_i$, in a DDD event study on the log pay residuals, $\hat{\omega}_{itq}$, obtained from equation 1. Our first difference includes employees yet to receive a new manager as controls in an event study design "à la Kleven". Our second difference compares employees for whom an event generates same vs opposite-sex managers. The third difference contrasts these effects across male and female employees.

Our regression specification includes fixed effects for year-quarter and individual-office

[25] Using the "All Offices' sample, we estimate locality pay regressions: $W_{iqy} = \theta_{ly} + w_{iqy}, \quad y = 1987, ..., 2014$, where W_{iqy} is log annual pay for an individual i in quarter q and year y, and θ_{ly} are imputed locality fixed effects in a regression for year y, as described in the Data Appendix-A.

[26] The part-time indicator is equal to 1 in each quarter-year in which the employee worked less than full-time. OPM defines part-time permanent workers as those working between 16 and 32 hours a week; 16% of federal employees in the "All Offices" sample work part-time at some point in their careers.

[27] In the 14% of cases where more than 1 manager arrives in the same quarter, we consider both new managers. The estimates are robust to excluding all employees who receive multiple new managers in close succession, as shown in section V.B.

(TWFE), that is, we allow each individual's fixed effect to vary when they switch offices.[28] Specifically, we estimate:

$$\begin{aligned}\hat{\omega}_{itq} = \delta_k \cdot \mathbb{I}^{Event} \cdot NSM_{idt} + \delta_k^f \cdot \mathbb{I}^{Event} \cdot F_i \cdot \ NSM_{idt} \\ + \alpha_k \cdot \mathbb{I}^{Event} + \alpha_k^f \cdot \mathbb{I}^{Event} \cdot F_i + \gamma_i + \lambda_q + \varepsilon_{itq},\end{aligned} \tag{2}$$

where $\hat{\omega}_{itq}$ denotes the pay residual from equation 1 for an individual i in event time t and calendar year-quarter q and where $\mathbb{I}^{Event}$ is a vector of event times.[29] The indicator variable F_i denotes female employees and is used in interactions (the first-order coefficients are absorbed by the individual fixed effects). Thus α_k and α_k^f capture the pay dynamics around a new manager's appointment. The indicator variable NSM_{idt} is equal to one when the new manager is of the same sex as the employee, thus δ_k and δ_k^f capture managerial homophily effects at event time $k = t$. The fixed effects γ_i and λ_q denote individual fixed effects that vary with office moves and calendar year-quarter fixed effects, respectively. Event years correspond to four consecutive quarters prior to and following treatment; depending on when treatment occurs, event years might not be equivalent to calendar years. The event study residual is denoted by ε_{itq}.

The change in female wages following the arrival of a new same sex manager is represented by $\delta_k + \delta_k^f + \alpha_k + \alpha_k^f$, while the analogous wage change for male employees is $\delta_k + \alpha_k$. These new same-sex manager effects are the second difference. The difference between these second differences, the triple difference, is the gender differential in the effects of new same-sex managers: $\delta_k^f + \alpha_k^f$.[30]

V. Event-study DDD Results

We begin by presenting our main results for our preferred event-study sample. We then investigate heterogeneity in treatment effects in order to understand when and for whom same-sex managers have the greatest impact on pay. This is followed by a discussion of the robustness of the estimates and of potential threats to identification including balance of the observed

[28] About 40% of employees switch offices at some point in the event study sample. Estimates are robust to restricting each individual to a single fixed effect (Figure 7A).

[29] The vector of event times includes 16 event-quarter indicators before and after the event in event studies with quarterly event times and four event-year indicators before and after the event in event studies with yearly even times. The event time prior to treatment, $k = -1$ is our base period and its coefficient is set to zero. We report estimates for event quarters from -16 to $+15$ and event years from -4 to $+3$ and omit the lower and upper absorbing event times -5 and $+4$ in the figures.

[30] The opposite sex effects on female employees omit the same-sex coefficients and are $\alpha_k + \alpha_k^f$, while those on male employees also omit the female interaction coefficients leaving α_k. Thus, omitting α_k from the expression of $\delta_k + \delta_k^f + \alpha_k + \alpha_k^f$, one could compute the differential "effects" of female managers on female vs male employees. But we cannot claim to identify this differential, it would require parallel trends between a treated and an omitted group.

characteristics of employees and offices by sex of the new manager and analyses of the sensitivity of the estimates to key specification and sample choices.

A. Main Results

Figure 2 depicts our main event study results: the top panels display event study estimates using the more detailed quarterly event times, and the bottom panels use yearly event times, our preferred more concise specification.[31] Our first novel finding is that women benefit from the appointment of a new same-sex manager at their workplace and that new same-sex managers are more important for their pay trajectories than for men's. The differential homophily effects (Figure 2A and 2C), that is the difference between the female and male same-sex managers trajectories ($\delta_k^f + \alpha_k^f$), show a cleanly identified treatment effects, netting out any pay growth around the arrival of new managers (event time effects).[32] This effect is more important for female employees: following the appointment of a new same-sex manager, their pay grows by 1.5 log points more than male employees'.[33]

We illustrate the source of the differential in Figures 2B and Figure 2D. The solid orange line plots the residual pay of female employees before and after the appointment of a new same-sex manager ($\delta_k + \delta_k^f + \alpha_k + \alpha_k^f$). The blue line depicts the analogous residual pay curve for male employees who receive a new same-sex manager ($\delta_k + \alpha_k$).[34] Table 2 reports the corresponding point estimates with standard errors (clustered at the office level). All employees, male and female, are on an upward pay trajectory early in their careers when new managers arrive (Table 2, column 3). The residual growth in pay (the new manager coefficients) in the pre-period is the same for male and female employees and is independent of the new manager being of the same-sex. In the period prior to the new manager's arrival, the α^f, δ, and δ^f coefficients are negligible and insignificant (Figure A.3A and Table 2).

Educational attainment is a key factor in employees' initial GS grades and promotion ceilings. [35] Highly educated employees are eligible for a wider range of pay grades and benefit

[31] Figure A.3A graphs the raw coefficients that are combined to obtain the desired treatment effects.

[32] A new same-sex manager is also a new manager. We, therefore, focus on the total effect that includes the differential effect on female employees of both new managers (α^f) and same-sex managers (δ^f). This is a conservative choice. The estimates are more than twice as large if one only considers the same-sex female employee interaction, δ^f (Table 2 column 2).

[33] Estimates are nearly identical in the GS pay plan subsample (Figure A.4).

[34] The pay growth following the appointment of these new managers is sizeable, consistent with grade increases. The event-time indicators, the α_k, capture a pre-existing pay growth pattern common to male and female employees. The homophily gender gaps we focus on display no such pre-trends.

[35] Individuals with a high school diploma and no additional experience typically qualify for GS-2 positions, those with a Bachelor's degree – for GS-5 positions, and those with a Master's degree – for GS-9 positions.

from larger pay increases by moving up a grade (e.g., 16 to 18 log points for each grade increase from GS-12 to GS-15). It is also possible that discretion plays a larger role in their duties and evaluation. To gain insight into these competing mechanisms (scope vs discretion for pay increases), we estimate the new same-sex manager effects for those whose work involves more or less routine cognitive tasks and then distinguish this split for those with and without a Bachelor's degree. Managers may have less latitude to evaluate the performance of workers who perform codified work, such as compliance officers, than that of workers whose work is less routine, such as occupational therapists. To that end, we construct a custom occupational crosswalk between the 4-digit federal service occupation codes and the 7-digit O*NET occupation codes. We then use Acemoglu and Autor's (2011) "routine cognitive" scores (see Data Appendix–B). We coded occupations in the federal service with median or above-median routine cognitive scores as being more routine jobs, and those below-median were coded as being less routine jobs.[36]

Figure 3 present the estimates splitting the sample by employee education and occupational routineness. Even within education groups, the differential homophily effect appears to be concentrated among workers in less routine occupations (Figures 3A and 3C). The differential homophily effects appears to be driven by female employees in less routine (circles) occupations. In Figures 3B and 3D the same-sex effect for female employees in less routine occupations is roughly 2 log points higher than for those in more routine occupations. On the other hand, there is no difference by occupations' routineness in the same-sex effects for male employees. Figure 3C and 3E show the routineness distinctions persist across education levels, although the differential homoplhily effects appear more quickly and are somewhat larger for those with a Bachelor's degree. Yet, even within education groups, the differential homophily effects are driven by women in jobs with less routine tasks. Women's representation in these jobs doubled over our study period.

The outsized female homophily effects also apply to other definitions of routineness, including "routine cognitive" and to a less extent "non-routine cognitive analytical", where the less routine cognitve and more analytical the jobs, the larger the same-sex female homophily effects. In Appendix Figure A.7, we compare the differential impact of managers across lower and higher intensity levels of four task measures. We find differential impacts that decrease in magnitude

[36]White-collar occupations in the federal service have routine cognitive scores ranging from little routineness -2.7 (e.g., occupational therapists) to a lot of routineness 2.1 (e.g., nuclear medicine technician), with a median of zero (e.g., library technician or research laboratory mechanic). Note we do not observe within-occupation changes in cognitive routineness; changes over time arise as the share of non-routine occupations increases. For example, when clerical jobs are replaced by administrative support positions.

from routine cognitive (base measure) to non-routine cognitive analytical (still large and significant), non-routine cognitive interpersonal and independence (smaller and less significant).[37]

Over the period we study, women's education has increased considerably (Black and Juhn, 2000) as did the share of non-routine cognitive occupations (Acemoglu and Autor, 2011). The female management share has also increased. To illustrate the potential impact of those changes, Figure 4 presents our estimates of female and male homophily separately by presidential era: Reagan and Bush 41 (1987–1992), Clinton (1993-2000), Bush 43 (2001–2008), and the first six years of Obama (2008-2014).[38] During the Reagan-Bush Era, 72% of men's new managers were men, while only 39% of women's new managers were women. By the Obama Era, male employees still received disproportionately more new male managers, but the gap had narrowed to 65% and 47%, respectively. At first blush, the effects of female homophily (orange squares) seem to steadily increase across the first three eras, but this could be the result of the above demographic and employment changes.

As shown in Figure 4A, estimates are noisier in the earliest era (Reagan-Bush 41), likely due to the initial seam of our sample. Male employees appear to benefit relatively more than female employees from having a same-sex manager. The homophily effects converge in Clinton Era and then are significantly stronger for women in the Bush 43 Era. They remain positive in the Obama Era, but the overall effects are dampened for everyone, likely due to the 2011-2014 pay freeze that severely constrained managerial discretion.

To disentangle composition effects from actual changes in homophily premia across eras, Figure 4 also includes the presidential era estimates for workers in less routine occupations, the group driving the earlier effects. Among those in less routine jobs, the overall and differential homophily effects are fairly stable before 2001 but show larger increases in the Bush era. This result is consistent with Deming (2017) who finds increasing returns to non-cognitive skills, in particular social skills, in the 2000s. It also suggests that the growth in the differential homophily effects across eras reflects job changes among female employees rather than changes in managerial homophily effects.

Next, we ask if the effects of same-sex managers vary with the composition of the office (Figure 5), which impacts women's homophily effects far more than men's. We use the initial

[37]See Data Appendix - O*NET Task Data for a description of the precise O*NET measures used to construct the indexes.
[38]The Reagan and Bush era runs into the seam of our data and has a smaller sample compared to the later presidential eras: 4,205 compared with 6,670 in Clinton, 14,823 in Bush 43 and 8,593 in Obama.

same-sex share (from the first two-quarters of employees' tenure) to split the sample into employees in offices with an above and below the median share of same-sex employees (47%) . We find that the differential homophily effects are positive in above-median same-sex share offices and negative in below-median same-sex share offices. This comes from much larger he female homophily effects in above-median same-sex share offices. while the male homophily effects are unaffected by the composition of their offices. A similar but less stark pattern emerges when one compares the homophily effects across offices with above and below-median initial same-sex manager shares (46%). New same-sex managers increase female employees' pay the most when they are appointed in offices with above-median initial same-sex (female) manager shares. Once again, the effects are largest for those in jobs with less routine tasks (Figure A.6). These results are consistent with previous research (e.g., Flabbi et al., 2019) finding that female managers are relatively more effective at improving women's pay in more female-friendly environments.

In order to gain more insight into the mechanisms underlying our results, we next ask whether the effects vary with the type of manager appointed. We compare the effects of new same-sex managers who are new to both the role and the office (external appointments) to those who worked in the office prior to their managerial appointment (internal appointments). Roughly half of the new managers we observe are external appointments. External appointments maybe more arms length and have fewer social ties with their employees, at least initially. Consistent with recent studies demonstrating the importance of social ties in the workplace (Cullen and Perez-Truglia, 2022), our estimates of differential homophily effects are noticeably smaller when the new manager is an external hire (Figure 6A), although the differences are not statistically significant.

Gender norms around women's role in the workplace have shifted across cohorts, leading one to wonder whether younger generations are more immune to homophily. While we did not find substantial differences in effects across presidential eras above, we nonetheless reproduce our estimates by manager age. We compare those who are 35 or younger when they first become managers (the first quartile) to those who are older than 35 (Figure 6B). The estimates for the former are less precise, but the point estimates are indistinguishable, suggesting that generational change will not eliminate the gender pay gaps.[39] This also suggests younger managers in the federal civil service are likely different from "fast track" managers in the private sector (Minni,

[39]The average and median new manager is approximately 45 years old. We failed to find any statistically significant differences across a number of manager age splits including those that isolated the oldest managers.

2022).

Finally, our main analysis focused on offices with fewer than 5 managers because our data does not allow us to determine which manager supervises a given employee. Figure 6C provides our results across offices of different managerial sizes. The estimates decrease in magnitude as the number of managers increases. This is what one would expect in a dose-response as a new same-sex manager in a larger office likely directly affects a smaller share of the workers. This dose-response relationship is inconsistent with the effects stemming from some broader policy goal and is more consistent with managers having direct effects on the employees they supervise. Some new managers replace former ones, while others expand the team. To distinguish between these effects, we focus on offices with a single managerial change. The differential homophily effects (Figure 6D) in this subsample are similar to our main results. We find that managerial additions appear to have larger effects than managerial replacements, but they are not statistically different.

B. Threats to Identification and Sensitivity of Estimates

Our estimation strategy uses management turnover as a natural experiment. It is natural to ask whether the offices where male and female managers are appointed are comparable before their appointment. If, for example, women were more likely to be appointed as managers as part of a remediation scheme in offices with worsening gender gaps, our estimates would conflate the effect of same-sex managers on women with the circumstances of their appointment. Fortunately, that does not appear to be the case. The gender pay gaps are stable before a new manager and a new same-sex manager is appointed (the coefficients on the female employee interactions are 0 in the pre-event period, Table 2). It is also reassuring that external hires are not driving our results, as one might choose to bring in new leadership from outside the office when attempting to correct a problematic workplace culture or environment (Figure 6A).

Another possible concern is that the appointment of female managers was part of some higher level gender initiative that also led to improvements in women's career trajectories. If that were the case, we might expect to see systemic effects from all female managers on all female employees. Instead, our effects are driven by offices that already have a critical mass of female managers and employees (Figure 5) and by employees in occupations where managerial attention and support are likely to be most important—those with less routine tasks. Additionally, the dose-response pattern we see as managerial team size varies is more consistent with the direct

effect of managers.

One can never prove the absence of relevant unobserved variables, but we offer balance tables showing both employees and offices with new male and new female managers and the workers with new same and opposite-sex managers appear comparable in the preceding year (Table 3). Female employees who get new female managers have the same salaries, unexplained pay gaps, education levels, and federal job tenure as female employees who get new male managers (Table 3, columns 1 and 2, and columns 5 and 6). One notable exception is the share of female employees in the office – female managers are appointed at offices that have more female employees. This fixed and stable difference that should be absorbed by the office fixed effects from the yearly pay regressions and not affect the event-study estimates. Additionally, while the stock of existing male and female managers differs modestly on observables, most notably education, and birth cohort, newly appointed male and female managers have indistinguishable observable characteristics (Table A.1).

Consistent with the appointment of new male and female managers being independent of trends in their offices, our estimates are robust to numerous combinations of controls and fixed effects. Panel A of Figure 7 reproduces the main differential homophily effects from Figure 2C alongside estimates from specifications that include individual fixed effects that are no longer allowed to vary when individuals change offices (as they were in the base specification) and estimates that entirely exclude office fixed effects in the estimation of log pay residuals. The overall pattern of results is unchanged across these alternative specifications, and the estimates are similar.

Roughly 40% of employees in the event-study sample change offices at some point in their government service. While getting a new manager may be a motivation for changing jobs, our results are not driven by office moves. The effects are largely indistinguishable when the sample is restricted to employees who only work at one office during their careers (Figure 7B), and as shown below, the arrival of same-sex managers has, at most, a negligible effect on the rate of office moves. The estimates are also robust to excluding part-time workers and those with any part-time spells during their careers.[40] The estimates are also robust to collapsing the first and second stages into a single estimation (Figure 7B).[41] Figure 7C shows that our results are robust

[40]There are notable gender differences in the incidence of spells of part-time work: 12% of employees have at least one spell of part-time work during their federal careers in the event-study sample (16% of female employees compared with only 7% of male employees). The estimates are virtually identical when the sample is restricted to employees who only work full-time throughout their federal careers.

[41]Even the standard errors from the single step estimation do not meaningfully differ in the single step and two estimations

to the exclusion of managers of unidentified gender. Finally, Figure 7D shows that our homophily results vanish in the placebo where we match employees and managers on the basis of odd or even birth years.

C. Intermediate Outcomes

Compensation is an aggregate measure of workers' advancement. We next examine the direct effect of same-sex managers on the components directly affecting employee pay. In the highly structured federal system, managers can impact pay through merit increases in pay steps and expedited grade increases (our proxy for promotions). Managers may expand effort directly obtaining pay step or grade increases for their subordinates or direct them to the occupational upgrades required for such increases. We next test whether employees receive more pay grade increases and experience more occupational changes following the appointment of a new same-sex manager at their office.

Step increases can occur in consecutive years, but occupation and pay grade changes are less frequent. Because each government pay scale has a different number and spacing of pay grades, we focus this analysis on the 90% of employees paid according to the GS pay scale. The average GS-pay-plan employee in our sample experiences just over three pay grade increases and 0.75 occupational changes during the observation period. We therefore estimate the cumulative effect of same-sex managers on promotions using a two-way fixed effects triple-differences (TWFE-DDD) approach. As in the event studies, we limit the sample to those receiving a new manager during the observation period. We present estimates for residualized pay for comparison.[42] For example, we estimate the impact of same-sex managers on residualized pay grade, P_{itq}, in TWFE-DDD among employees under the GS-schedule:

$$P_{itq} = \delta Post_{it} \cdot NSM_{idt} \cdot + \delta^f Post_{it} \cdot F_i \cdot NSM_{idt} + \alpha Post_{it} + \alpha^f Post_{it} \cdot F_i + \gamma_i + \lambda_t + \epsilon_{itq}, \tag{3}$$

where F_i is an indicator that equals 1 if the employee is female and NSM_{idt} is an indicator that equals 1 if the employee's first new manager is the same-sex as them and 0 otherwise. $Post_{it}$ takes on the value 1 after the new manager arrives and 0 before (replacing the event time indicators). The individual fixed effects, γ_i, absorb the first-order effects of F_i. We include both the new

(Table **??**, column 5).

[42] For comparability with the event-study, the log pay residual is the remainder after controlling for location, age, education, tenure, part-time status, occupation, and office FEs in yearly regressions. GS grade is residualized using the same controls.

manager effect, α, and quarter-year fixed effects, λ_t, because employees, even in the same office, get treated at different times throughout the year. The pre- and post-treatment periods include all years from 1987 to 2014.

The resulting estimates for six outcomes of interest among employees at offices with up to 5 managers are reported in Table 4 with and without individual-level controls. In addition to the estimated coefficients of the arrival of a first new manager, α and α^f, and the estimated coefficients of the same-sex manager treatment, δ and δ^f, we present the sum of estimates (with standard errors) of female managerial homophily, $\alpha + \alpha^f + \delta + \delta^f$, male managerial homophily, $\alpha + \delta$, and of the difference between the two – differential homophily, $\alpha^f + \delta^f$. We confirm the robustness of our estimates to the recently identified issues with parallel pre-trend assumptions in staggered TWFE-DiDs (see, for example, Callaway and Sant'Anna, 2021, and Borusyak, 2021). Using Borusyak's (2021) heterogeneous effects estimator, we compute the same-sex manager effects separately for male and female employees. The resulting estimates and their difference are in Table A.12, columns 3, 6, and 9. Estimates are also robust to estimation using matched DDD (Table A.12, columns 2, 5, and 8) and to collapsing the data into two periods (Table A.11).

For reference and comparability with our earlier estimates, we provide estimates for the GS pay scale employee sub-sample using the pay residual variable used in the earlier event study analysis (Table 4A, column 2). Mirroring the event study results, the TWFE-DDD estimates show that female employees' residual pay increases roughly 1.5 log points more than male employees' pay following the appointment of a new same-sex manager (1.4 log points with TWFE and 1.6 log points with Borusyak's heterogeneous treatment effects estimator (Table A.12, column 3). The inclusion of occupation in the controls leads to more conservative estimates. Our estimated effects are indeed almost 1 log point larger (Panel B) when we do not control for it, likely an overestimate given that pay varies by occupation. This comparison highlights the importance of considering occupational changes as an outcome variable. In regressions with raw (unresidualized) log pay as the dependent variable and no individual controls (Table A.10, columns 1 and 2), our effect is present, but larger as one would expect.[43]

Next, we turn to the effect of new same-sex managers on pay grade. Once again, female employees see faster progression following the appointment of a new same-sex manager – female employees see their residual GS grade increase by 0.15 more (roughly 10% of a standard deviation)

[43]Like in the event studies, the single-step estimation with controls (column 3) provides nearly identical estimates to those using residualized pay (column 4)

than male employees who receive a same-sex manager (Table 4C). These grade changes can explain over 60% of the differential increase in women's residual wages following the appointment of a same-sex manager (Table A.9).

One way employees can increase their pay grades is to move into new roles with additional responsibilities or upgrade their skills. Managers could provide guidance about such opportunities, and favorable assessments could facilitate occupational upgrades; they could also simply reclassify existing positions. We cannot observe the exact tasks of each worker, but we observe their detailed occupation (4 digits) and occupational status.[44] To that end, we estimate Equation 3 replacing the dependent variable with an indicator equal to one when the worker's occupational code changes. We find that female employees experience significantly more occupation code changes overall and in comparison to male employees following the appointment of new same-sex managers (Table 4D and 4E). Female employees experience approximately 0.10 more occupation category changes and 0.04 more 4-digit occupation changes after the appointment of a new same-sex manager than men (the average number of occupation category and 4-digit occupation changes observed in our data are 0.26 and 0.33, respectively).

Getting a same-sex manager has a larger impact on women's careers, and this is reflected in employee retention. Female employees are significantly more likely than their male counterparts to still be working in the Federal government 3 years after receiving a new same-sex manager (Table A.11), although the effect is small.[45] There is no meaningful increase in office moves within the federal service following the appointment of new same-sex managers, all the point estimates are less than 0.01 on a mean of 0.32 (Table 4F).

D. Employee Sentiment

Managers' roles in workplaces extend beyond formal employee performance reviews. We seek to test whether employees' subjective views align with our data-driven finding that women's presence on management teams improves women's career trajectories. The Federal Employee Viewpoint Survey (FEVS), administered by OPM, was made mandatory by the National Defense Authorization Act in 2004. The Act specifies that the employees' survey is to assess leadership and management practices and provide a representative snapshot of employee experiences and

[44] We observe the following broad occupation status: Professional, Administrative, Technical, Clerical, and Other white-collar occupations. Moves from a clerical to administrative jobs would present opportunities for further increases in pay.

[45] To study exits from the federal service, we collapse the data into two snapshots: tenure 4 quarters prior to the new manager's arrival and 12 quarters after. We then estimate a two-period DDD shown in Table A.11, which also contains estimates for all of the outcomes discussed above. These results are similar to the uncollapsed DDD.

perceptions of agency management (OPM, 2014b).[46] It is regularly used by the government to evaluate human resource policies and programs. Each iteration of the survey contains demographic information on the respondents along with their responses to questions about their workplace experiences.

We link our OPM data on women's presence in management teams to FEVS data to examine the relationship between women's presence in management and employees' experiences in the workplace. Because the FEVS is an anonymous survey, we cannot link individual respondents to their payroll data. Instead, we link each employee's survey responses to the characteristics of the management teams at their agencies. For comparability with the preceding event-study analysis, we restrict the sample to the 29 agencies that appear in that sample and in the OPM survey.[47] The FEVS Public Release Data Files began in 2006. We use the 2006-2014 surveys in the analysis that follows.

For conciseness, we present the results for six FEVS items that speak to management effectiveness: "(1) Considering everything, how *satisfied* are you with your *job*? (2) Considering everything, how *satisfied* are you with your *pay*? (3) How satisfied are you with the *recognition* you receive for doing a good job? (4) My supervisor/team leader provides me with opportunities to demonstrate my *leadership skills*. (5) Supervisors/team leaders in my work unit support *employee development*. (6) Prohibited Personnel Practices (for example, illegally *discriminating* for or against any employee/applicant, obstructing a person's right to compete for employment, knowingly violating veterans' preference requirements) are *not tolerated*" (OPM, 2014b).

Table 5 presents OLS estimates of employee sentiment regressed on indicators for the respondent being a female employee, the majority of her agency's management being of the same sex, their interaction and agency and year fixed-effects. The dependent variables are on a 5-point scale, with 5 indicating the most satisfaction or agreement. The regression estimates show a significant female disadvantage across the survey questions, with the notable exception of pay satisfaction, which, although negative, is not significant. As in the event studies, the estimates show a differential homophily effect for female employees. Having a high share of same-sex managers is more important for female employees than for men. These results complement our

[46] The FEVS takes an annual probability sample from the population of permanent federal employees. Each year's sampling frame is designed to produce results that are representative of the federal workforce overall, and at the agency and sub-agency levels. Originally administered as the Federal Human Capital Survey (FHCS) in 2002, it was administered bi-annually until 2010, when it became an annual survey. The 98-item survey covers eight topic areas: personal work experiences, work unit, agency, leadership, satisfaction, work/life, and demographics. Additional information on the FEVS is available at: https://www.opm.gov/fevs/about/

[47] We use the survey weights provided by OPM (OPM, 2014b).

event-study estimates showing that same-sex managers matter more for women's pay and careers. Importantly, they show the importance of a critical mass of women in management for intangibles such as leader opportunities and employee development.

VI. Discussion

We seek to estimate the causal effect of differential managerial homophily on the pay of men and women, purged from deterministic (e.g., locality adjustments) and human capital (e.g., education) considerations. We argue that for any particular employee in the office, whether the first new manager is of the same sex as the employee or not is as good as random. This assumption is supported by the parallel trends in the pay trajectories of female and male employees prior to the arrival of their new manager and the balance in observed characteristics of the employees who received same and opposite-sex managers.

A natural concern is that new managers could be appointed as part of a remediation scheme targeting problematic workplaces. We first show that new female and male managers are comparable and that they are assigned to similar offices (Table A.1 and Table 3). Gender pay gaps are stable prior to the appointment of new same-sex managers, as evidenced by the zero coefficients on the female employee pre-event interactions (Table 2). The parallel pay trajectories prior to the event also do not show any sign of an Ashenfelter dip. Further, the treatment effect heterogeneity we observe is inconsistent with any potential remediation scheme. We do not find that managers appointed from outside of the office, or in offices with few female managers, or who are younger give higher raises to female employees. Pay raises appear larger when managers are internally promoted (Figure 6A), when there is a critical mass of employees or managers in the office (Figure 5), but not when the new manager is relatively young (Figure 6B). Lastly the dose-response pattern we document (Figure 6C) is consistent with managers having direct effects and is not what one would expect to see from higher level initiatives. Our results are more consistent with there being a direct effect of same-sex managers of employees in their offices. Consistent with managerial discretion, employees whose jobs are less routine benefit from larger pay increases across education levels (Figure 3) and Presidential eras (Figure 4).

The range of the effects of managerial homophily on residualized pay appears large but its magnitude is consistent with pay grade increases. Indeed when we explore the mechanisms by which new managers raise pay, our TWFE-DDD analysis confirms larger pay grade increases for women getting new same-sex managers, and increases in the probability of occupational

upgrading (Table 4). We do not know what motivates new managers to expend effort to improve their employees' pay. An important issue in the literature on the US federal service is employee retention we find modest effects on retention and no meaningful effect of managerial changes on office switching within the government (Table 4). Admittedly, it is limited by the fact that we cannot follow workers after they leave the federal service (unlike Foster *et al.*, 2020). Our analyses focused on the impact of immediate supervisors, the potential effects of higher level management initaitves are a fruitful area for future research and are beyond the scope of this paper because of data limitations. While many expected larger effects during the Democratic Obama Era, we did not find that to be the case, likely because of the pay freezes implemented during that time.

Some statistical theories of discrimination emphasize the noisiness or bias in the productivity signal of the minority group by comparison with the majority group to explain the pay or promotion differentials. These theories would be compatible with the fact that female managers sort through the noisiness of their employees' signal better than male managers given that the homophily differential is heightened when employees work in less routine jobs (Figure 3).

VII. Conclusion

The US Federal civil service provides an advantageous environment to study the potentially gendered impact of new managers on the pay of white-collar subordinates. A wide-ranging and well-documented set of rules and legislation constrains federal pay setting, with compliance closely monitored by a strong union and large bureaucracy. Yet, federal pay varies more with gender and race than can be explained by observed qualifications (Olson et al, 2000; Lewis and Oh, 2009; Oh and Lewis, 2013). We show that the demographic diversity of managers plays a role in these disparities.

Using over thirty years of rich longitudinal data containing over 15,000 employees' first managerial team changes, we estimate the effects of managerial homophily on employees' career trajectories by gender. Under the assumption that getting a first new same-sex manager is conditionally exogenous, we argue that the differential homophily effects found are likely causal. Our event-study results show that the appointment of the first same-sex managers has substantial effects on women's pay. The differential homophily effect on residual pay is approximately 1.5 log points. This is roughly equivalent to half a step increase within a pay grade and is close to half the average residual gender gap of 2.7 log points (Table 1, row 3). The movement of women

into managerial roles has reduced the homophily imbalance: At the end of our period, 48% of female employees' new managers were women, up from only 36% at the beginning. Because these homophily effects and the accompanying occupational upgrades happen early in female employees' careers and have become more frequent over time, the long-term impacts on the gender pay gap are likely larger.[48] Our estimates are robust to a host of alternate samples, specifications, controls, fixed effects, and treatment definitions.

We confirm the differential promotion (pay grade) mechanism behind these residual pay increases by directly estimating the effect of homophily on pay grade increases. Following the appointment of a first new manager, female employees receive significantly more pay grade increases if that manager is of the same sex. The actions through which female managers speed the progression of female employees are beyond the scope of this paper and are a fruitful topic for future research. Such actions could include assigning "promotable" tasks, making employees more likely eligible for performance-related pay increases. It is also possible that female employees are more effective at self-promotion or more likely to pursue pay increases when they have a female manager.

These findings sound a cautionary note for studies that estimate pay disparities conditional on pay grade or algorithmic score or job title. When the inputs into an algorithmic or deterministic pay setting are the product of discretionary decisions by human actors, the adoption of algorithmic or deterministic pay settings will formally incorporate these biased disparities. Then the adoption of these schemes will mask and deceptively rationalize disparities instead of eliminating them. That is indeed the case here. Figure 1 plots the unexplained pay gap with and without accounting for workers' potentially endogenous pay grades. It shows that the assignment of men and women to pay plan grades appears to "close the gender pay". Most (85%) of the unexplained pay gap over time operates through workers' placement on the pay grid – a decision influenced by their managers and their gender.

Our analysis of treatment heterogeneity makes a new and important contribution. We identify the circumstances under which managers are most effective at increasing employees' pay. The homophily gender differentials are larger when newly appointed managers come from internal appointments, who may have more established social ties with their workers. The effectiveness

[48] Formally estimating the total impact would require a structural dynamic model that accounts for the cumulative effects of each of the managers each employee is exposed to through their entire federal career. This is beyond the scope of this study.

of managers is more variable than previously thought; it is inextricably linked to the nature of jobs. We find substantial treatment effects heterogeneity by occupational routineness, even within education level. We find the largest effects for women in occupations with less routine tasks—where there is potentially more scope for discretionary evaluation. Less routine jobs have become increasingly important in the labor market and in women's careers. Over our study period, federal employment of women in less routine jobs increased from 21.7% to 42%. As the Presidential era analyses illustrate, managerial homophily has grown in importance in women's career trajectories as they have gained more education and occupations with less routine tasks have become more prevalent. Artificial intelligence is likely to heighten the spread between less routine and more routine jobs in the future.

Our findings also provide insights into the effects of policy innovations aimed at reducing the gender gap. Policies such as bans on asking about salary history at hiring and pay transparency initiatives (Baker et al., 2019) have focused on giving employees the knowledge to improve their salary position. Pay transparency policy and salary history bans may increase employees' current barganining power, but they will not prevent disparities in promotions and roles from influencing future pay. Our results show the critical role managers play in their employees' progress through career and salary ladders, particularly early in their careers.

The federal service encompasses a wide range of occupations and industries; however, we can only speculate as to the effects of female managers in the private sector. The civil service's pay system is, on average, more highly regulated and deterministic than many of those found in the private sector. Gender gaps in managers' assessments of subordinates are larger on more subjective measures (e.g. employee's potential) than on more concrete measures such as past productivity (Benson, Li and Shue, 2023). It is thus likely that, if anything, direct supervisors play an even larger role in career progression in the private sector.

A disheartening aspect of the narrowing of the gender pay gap over the last forty years is the increasing share of the pay gap unexplained by traditional employee-based characteristics, particularly at the upper end of the wage distribution (Fortin et al., 2017). Given the historical under-representation of women in management, our findings suggest managers are a key hidden factor in the previously unexplained gender gap.

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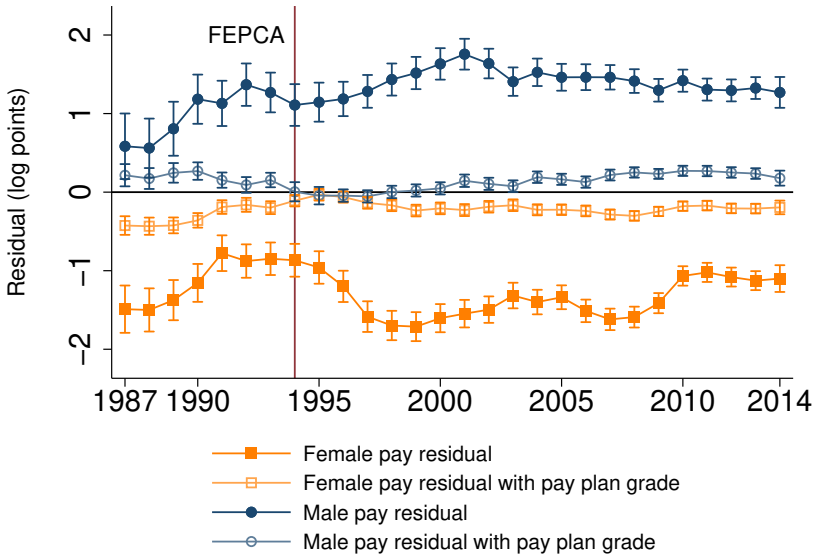

FIGURE 1. PAY RESIDUALS BY GENDER

Note: The sample is the "Up to 5 managers" as defined in Figure 1B. Female and male pay residuals are from yearly regressions of individuals' log pay on localities of offices, birth year bins, education, tenure, occupation, and office FEs. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

FIGURE 2. EVENT STUDY ESTIMATES: EFFECTS OF FIRST NEW MANAGERS

Note: The event-study sample is the sub-sample of employees in the "Up to 5 managers" sample who receive a first new manager. All event study specifications include quarter-year and individual-office FEs, see event study equation 2. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Point estimates and standard errors are reported in Tables 2 and **??**. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

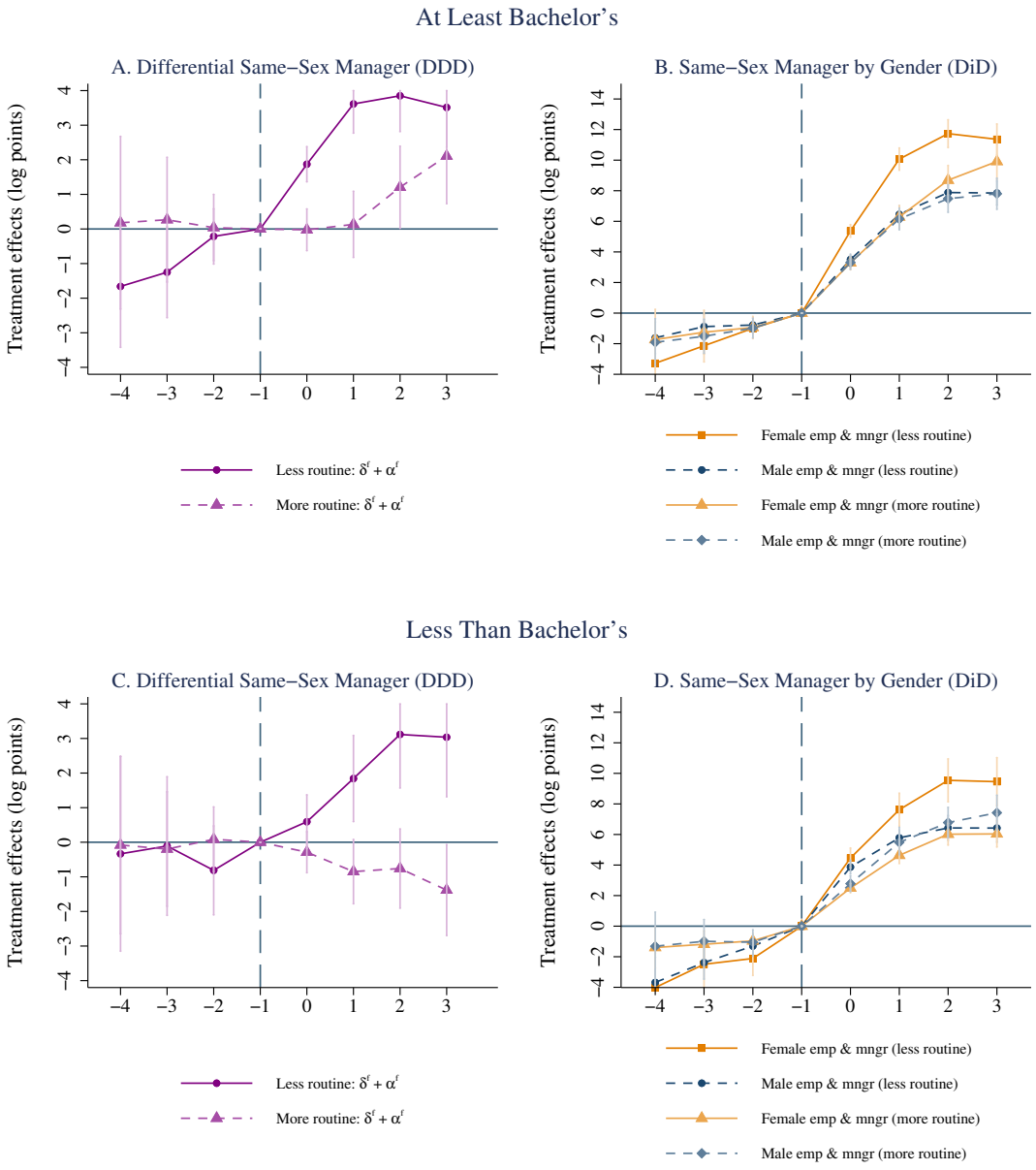

FIGURE 3. EVENT STUDY ESTIMATES BY OCCUPATIONAL ROUTINENESS AND EDUCATIONAL ATTAINMENT

Note: Treatments effects and specification are defined as in Figure 2C and 2D. Panels A and B split employees with less than a Bachelor's degree into employees with less routine and more routine occupations, defined by below-median and above-median or median routine cognitive O*NET index. Panels C and D split employees with at least a Bachelor's degree into employees with less routine and more routine occupations.Point estimates and standard errors are reported in Tables **??**, and **??**. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

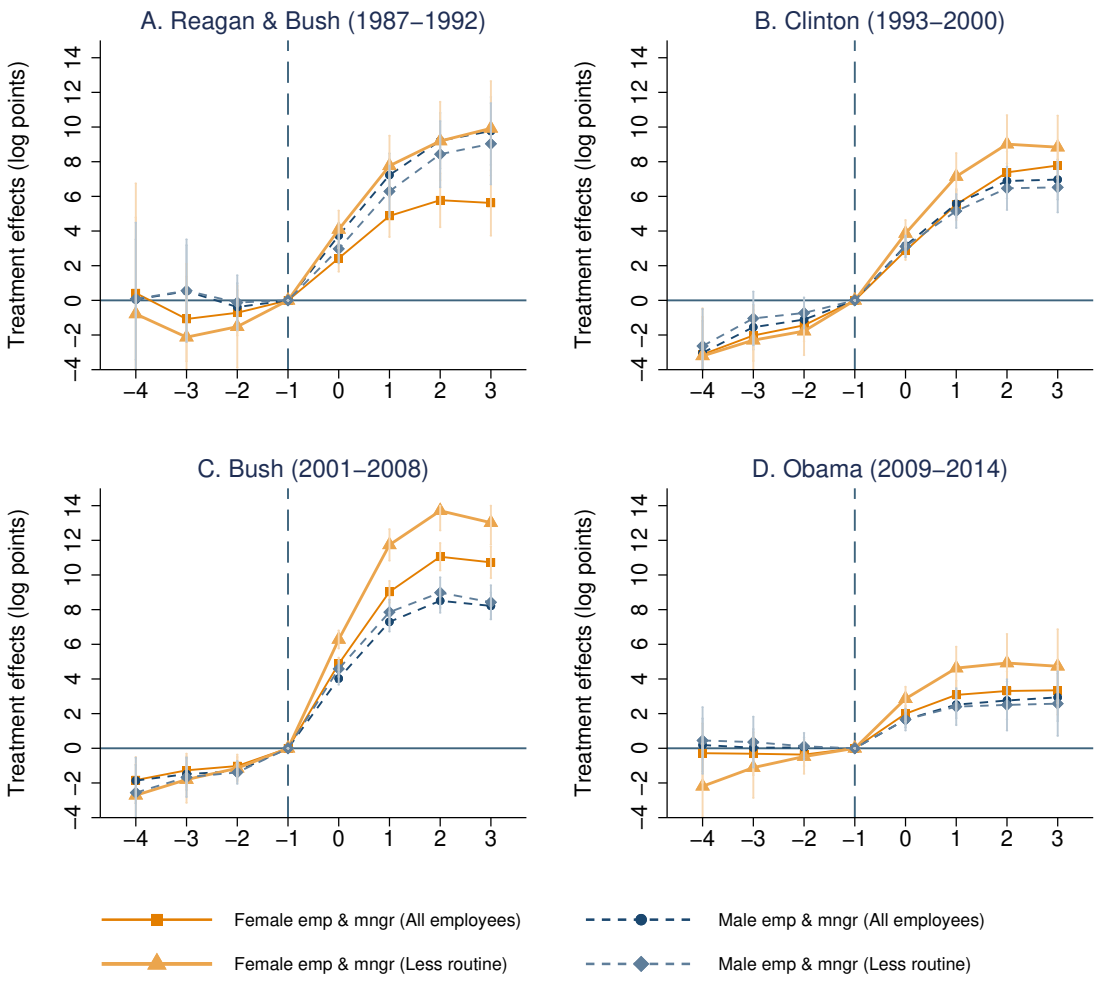

FIGURE 4. EVOLUTION OF HOMOPHILY EFFECTS OVER PRESIDENTIAL ERAS (DID)

Note: Coefficients and specification are defined as in Figure 2D. The Figure displays estimates for each presidential era. Employees are assigned to the era they received their first new manager. There are 4,205 employees in the Reagan and Bush (1987-1992) sample, 6,670 employees in the Clinton (1993-2000) sample, 14,823 employees in the Bush (2001-2008) sample, and 8,593 employees in the Obama (2009-2014) sample. The subsample of employees with less routine occupations is defined in Figure 3. There are 2,223 employees in the Reagan and Bush (1987-1992) era, 3,872 employees in the Clinton (1993-2000) era, 8,495 employees in the Bush (2001-2008) era, and 4,651 employees in the Obama (2009-2014) era. Point estimates and standard errors are reported in Tables **??**, **??**, **??**, and **??**. Vertical bars represent 95% confidence intervals with standard errors clustered by office. Evolution of differential same-sex manager effects over presidential eras is depicted in Figure A.5.

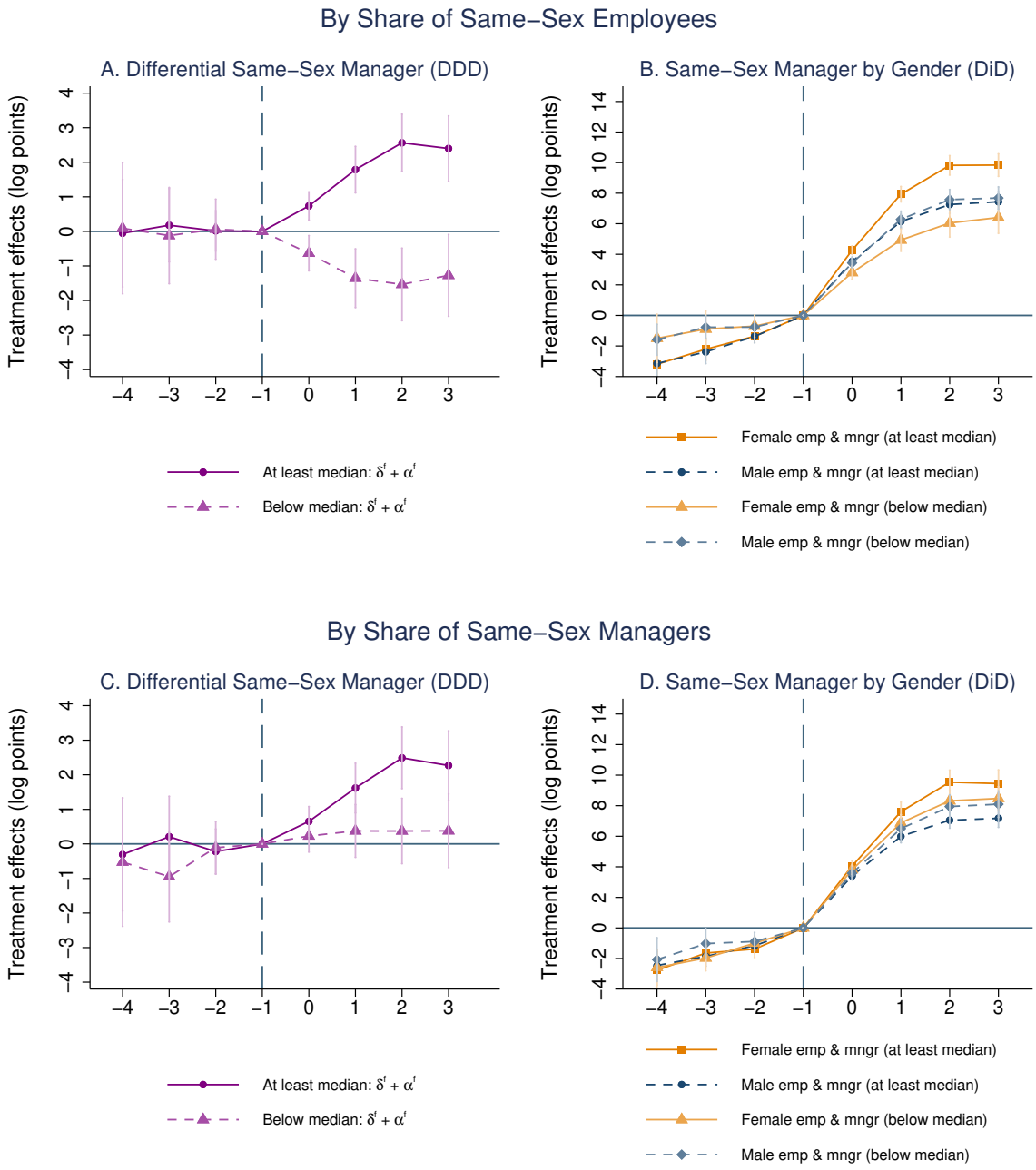

FIGURE 5. HETEROGENEITY BY INITIAL SAME-SEX EMPLOYEE AND MANAGER SHARES

Note: Treatments effects and specification are defined as in Figure 2C and 2D. Panels A and B split the sample from Figure 2 into employees with at least a median and below median share of same-sex employees (i.e. 47.45%) in the first two quarters of employees' tenure. Panels C and D split the sample from Figure 2 into employees with at least a median and below median share of same-sex managers (i.e. 45.65%) in the first two quarters of employees' tenure. Point estimates and standard errors are reported in Tables **??** and **??**. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

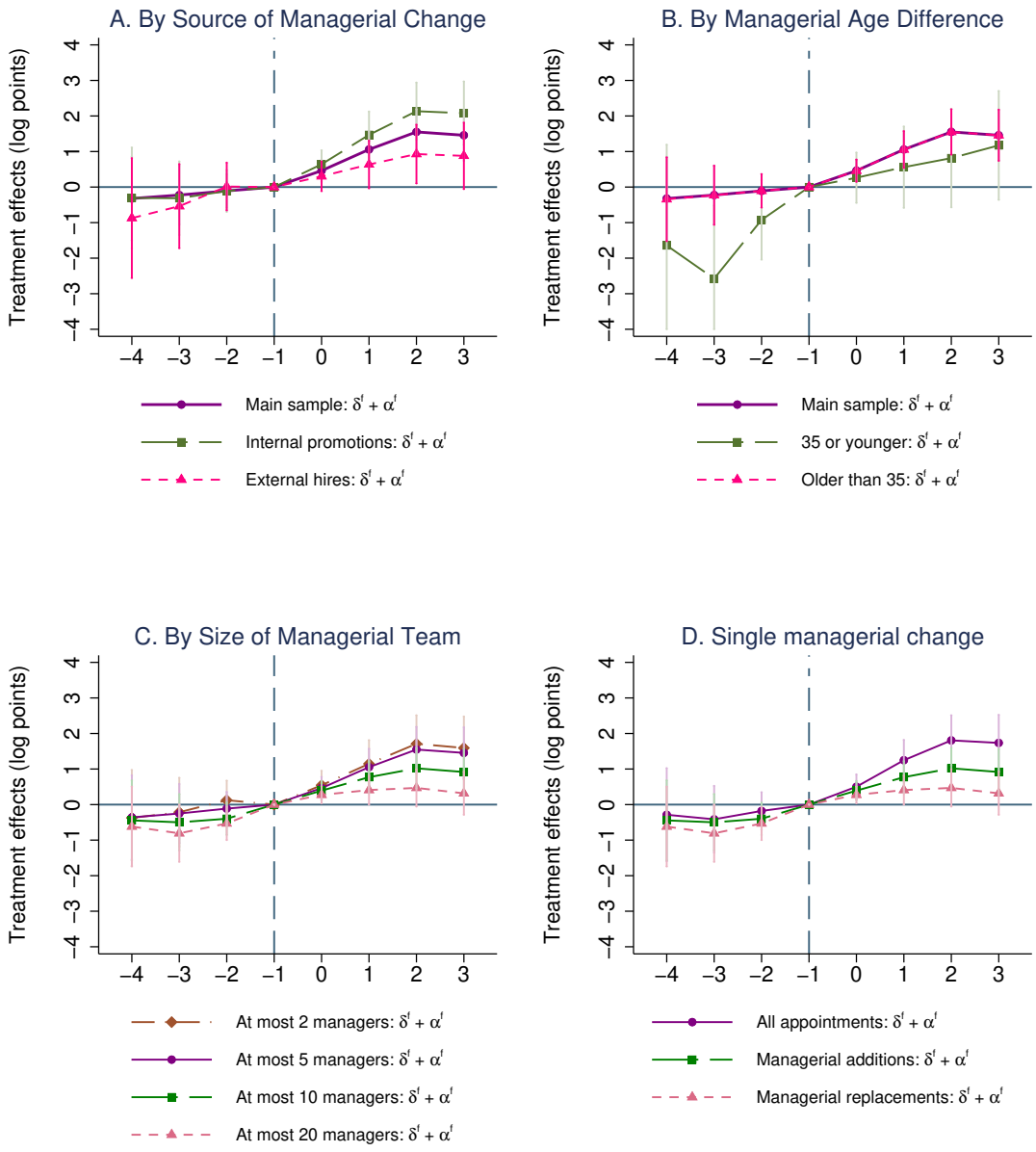

FIGURE 6. HETEROGENEITY OF DIFFERENTIAL HOMOPHILY EFFECTS (DDD) BY MANAGERIAL CHANGE

Note: Treatments effects and specification are defined as in Figure 2C. Each line plots differential homophily effect estimates from a separate regression using equation 2. **"Main Sample"** is the event-study sample and reproduces the differential homophily effect from Figure 2B for reference. Panel A splits the event study sample by the manager's previous location of employment. The **"external hires"** estimates are from employees whose new manager had not previously worked at the office, and **"internal hires"** are from employees whose new managers had previously worked at the office as employees. Panel B splits the event study sample by the age of the new manager at appointment. The **"35 or younger"** and **"older than 35"** estimates are from the subsamples of employees whose new manager was that age at appointment. Panel C splits the event study sample by the size of managerial and includes all employees working in offices with **at most 2, 5, 10, and 20 managers** in the quarter prior to getting a first new manager. Panel D includes employees treated with only a single new manager. **"All appointments"** include all single managerial changes, **"Managerial additions"** include managerial appointments that expand managerial teams, and **"Managerial replacements"** include managerial appointments after which the size of a managerial team remains the same or decreases. Point estimates and standard errors are reported in Tables **??** and **??** . Vertical bars represent 95% confidence intervals with standard errors clustered by office.

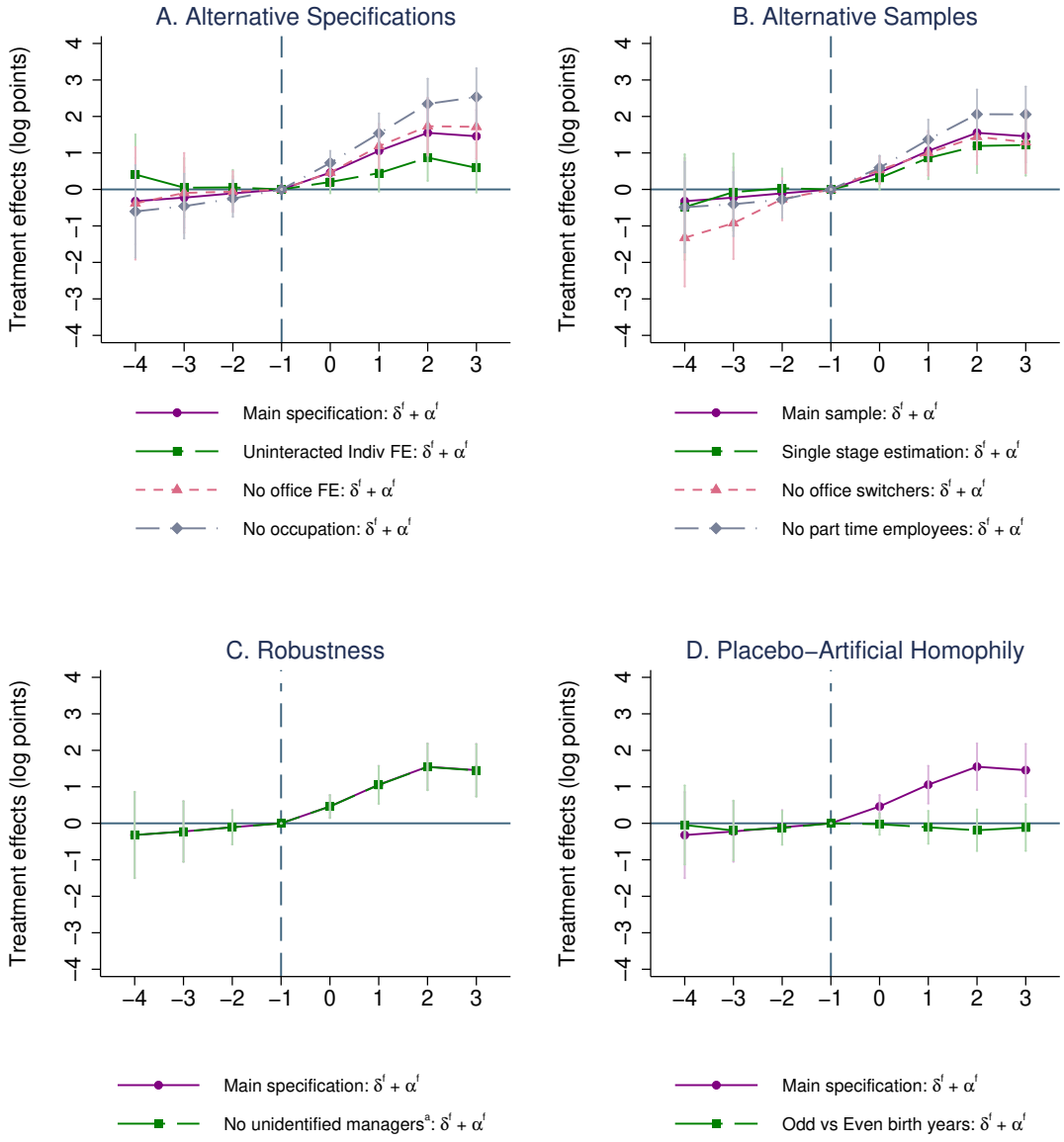

FIGURE 7. SENSITIVITY ANALYSIS: DIFFERENTIAL HOMOPHILY EFFECTS (DDD)

Note: Treatments effects and specification are defined as in Figure 2C. Each line plots differential homophily effect estimates from a separate regression using equation 2. **"Main Sample"** is the event-study sample and reproduces the differential homophily effect from Figure 2B for reference. Panel A uses **"uninteracted individual FE"** instead of individual-office FEs. **"No office FEs"** estimates exclude office FEs in the first stage, but include individual-office FEs in the second stage. **"No occupation"** does not include any occupation controls in stage 1. In Panel B, **"one stage estimation"** estimates the event study in one stage on the event study sample by adding first stage controls, as well as locality-year FEs, to the second stage. **"No office switchers"** excludes 40% of employees that switch offices at any point in the sample. **"No part-time employees"** excludes the 8% of employees who have a part-time spell during their federal service. In panel C, **"No unidentified managers"** excludes managerial appointments of unidentified gender. The estimates are nearly identical to the estimates in the "main specification". Panel D shows a **placebo test**, in which the treatment effects are redefined as getting a new manager of odd vs even birth year. Point estimates and standard errors are reported in Table **??** and **??**. Vertical bars represent 95% confidence intervals with standard errors clustered by office.
a Estimates in the "no unidentified managers" specification overlap with the estimates in the "main specification".

Table 1—Descriptive statistics – Offices Up to 5 Managers

Sample	Female employees		Male employees	
	All employees	Event study	All employees	Event study
	(1)	(2)	(3)	(4)
	Panel A: Outcomes			
Salary	50,633	50,051	57,781	57,142
	(23,654)	(22,949)	(26,090)	(25,714)
Log salary	10.72	10.72	10.86	10.85
	(0.48)	(0.47)	(0.47)	(0.46)
Unexplained log salary (x100)[a]	-1.32	-1.66	1.38	1.10
	(17.48)	(17.49)	(18.35)	(18.49)
N pay plan changes	0.02	0.01	0.03	0.02
	(0.29)	(0.14)	(0.23)	(0.22)
N grade changes (GS sample)	2.98	3.05	2.78	2.91
	(1.96)	(1.93)	(1.84)	(1.82)
	Panel B: Individual characteristics			
Birth year 1955-1960	0.22	0.19	0.23	0.20
	(0.42)	(0.39)	(0.42)	(0.40)
Birth year 1960-1965	0.24	0.21	0.25	0.23
	(0.43)	(0.41)	(0.43)	(0.42)
Birth year 1965-1970	0.21	0.21	0.21	0.21
	(0.41)	(0.41)	(0.41)	(0.41)
Birth year 1970-1975	0.14	0.16	0.14	0.16
	(0.35)	(0.37)	(0.35)	(0.37)
Birth year 1975-1980	0.10	0.12	0.09	0.11
	(0.30)	(0.33)	(0.29)	(0.31)
Birth year 1980+	0.08	0.10	0.07	0.09
	(0.27)	(0.30)	(0.26)	(0.28)
Education: High school or less	0.21	0.20	0.14	0.14
	(0.41)	(0.40)	(0.35)	(0.35)
Education: Some college	0.30	0.29	0.21	0.19
	(0.46)	(0.45)	(0.40)	(0.39)
Education: Bachelor's degree	0.36	0.40	0.48	0.49
	(0.48)	(0.49)	(0.50)	(0.50)
Education: Graduate degree	0.12	0.12	0.18	0.18
	(0.33)	(0.32)	(0.38)	(0.38)
Tenure (years)	8.69	7.29	9.09	7.87
	(6.60)	(5.93)	(6.73)	(6.24)
	Panel C: Workplace characteristics			
Female employee share at office	0.44	0.46	0.37	0.38
	(0.22)	(0.21)	(0.22)	(0.21)
Female management share at office	0.28	0.30	0.25	0.26
	(0.31)	(0.31)	(0.30)	(0.29)
N offices	9,890	6,925	9,280	6,457
N unique individuals	43,663	16,852	39,350	14,265
N individuals with part time spell	5,521	2,770	1,877	987
N treated individuals	16,852	16,852	14,265	14,265
N person quarters	1,178,351	723,911	990,484	594,845

Note: The "Up to 5 managers" sample has the same tenure, office size, and cohort restrictions as in Figure 1B. The event study sample further restricts the sample to employees who experienced an appointment of a first new manager, as explained in section III.B. Panel C is at the office–quarter level. Standard deviations are in parentheses.

[a] Unexplained log pay is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details.

Table 2—Event Study Estimates: Effects of First New Managers

Event year	New same-sex mngr (δ)	New same-sex mngr*female (δ^f)	New mngr (α)	New mngr*female (α^f)	Female-male same-sex mngr (DDD) ($\delta^f + \alpha^f$)
	(1)	(2)	(3)	(4)	(5)
-4	-0.09	-0.69	-2.22***	0.36	-0.32
	(0.63)	(0.88)	(0.50)	(0.62)	(0.60)
-3	-0.06	-0.74	-1.51***	0.51	-0.22
	(0.47)	(0.64)	(0.38)	(0.46)	(0.42)
-2	-0.03	-0.57	-1.04***	0.46*	-0.11
	(0.25)	(0.35)	(0.20)	(0.25)	(0.24)
0	-0.62***	1.16***	4.07***	-0.70***	0.46***
	(0.17)	(0.24)	(0.14)	(0.16)	(0.16)
1	-1.29***	2.62***	7.44***	-1.56***	1.06***
	(0.27)	(0.39)	(0.23)	(0.25)	(0.26)
2	-1.93***	3.67***	9.25***	-2.12***	1.55***
	(0.33)	(0.48)	(0.29)	(0.32)	(0.33)
3	-2.08***	3.77***	9.52***	-2.31***	1.46***
	(0.37)	(0.54)	(0.34)	(0.36)	(0.37)
N offices	8,377	8,377	8,377	8,377	8,377
N unique individuals	31,117	31,117	31,117	31,117	31,117
N person-qtrs	1,318,756	1,318,756	1,318,756	1,318,756	1,318,756

Note: The event-study sample is the sub-sample of employees in the "Up to 5 managers" sample who receive a first new manager. All event study specifications include quarter-year and individual-office FEs, see event-study equation 2. Estimates are event-year treatment effects reported in log points. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure 2.
Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 3—Balance: Pre-event Year Employee and Workplace Characteristics

Sample	Female employees		Male employees	
	New female mngr	New male mngr	New female mngr	New male mngr
	(1)	(2)	(3)	(4)
	Panel A: Outcomes			
Log salary	10.45	10.42	10.58	10.58
	(0.44)	(0.46)	(0.43)	(0.46)
Unexplained log salary (x100)[a]	-7.86	-7.81	-7.25	-5.68
	(18.78)	(18.90)	(20.73)	(20.31)
GS grade [b]	7.13	7.22	8.08	8.40
(GS sample)	(2.63)	(2.75)	(2.72)	(2.84)
	Panel B: Individual characteristics			
Birth year	0.14	0.16	0.14	0.16
1955-1960	(0.34)	(0.37)	(0.35)	(0.37)
Birth year	0.17	0.18	0.17	0.19
1960-1965	(0.38)	(0.39)	(0.37)	(0.39)
Birth year	0.19	0.19	0.19	0.19
1965-1970	(0.39)	(0.39)	(0.39)	(0.39)
Birth year	0.17	0.17	0.18	0.18
1970-1975	(0.38)	(0.38)	(0.39)	(0.38)
Birth year	0.16	0.15	0.15	0.15
1975-1980	(0.37)	(0.36)	(0.36)	(0.35)
Birth year	0.17	0.15	0.17	0.14
1980+	(0.37)	(0.35)	(0.38)	(0.35)
Education:	0.18	0.19	0.12	0.16
High school or less	(0.38)	(0.39)	(0.33)	(0.37)
Education:	0.29	0.27	0.21	0.21
Some college	(0.46)	(0.45)	(0.40)	(0.41)
Education:	0.4	0.39	0.49	0.45
Bachelor's degree	(0.49)	(0.49)	(0.50)	(0.50)
Education:	0.12	0.15	0.18	0.18
Graduate degree	(0.33)	(0.35)	(0.39)	(0.38)
Tenure (years)	1.97	2.19	2.56	2.92
	(2.79)	(3.06)	(3.57)	(3.77)
N unique individuals	7,066	8,325	4,228	8,285
N person quarters	24,648	28,705	14,682	28,586
	Panel C: Workplace characteristics			
	New female mngr		New male mngr	
Female employee share at office	0.54		0.38	
	(0.21)		(0.22)	
Female management share at office	0.35		0.24	
	(0.35)		(0.31)	
N offices	3,235		4,446	

Note: The sample is the event-study sample from Table 2. Columns (1) and (3) include female and male employees who receive new female managers and columns (2) and (4) include employees who receive new male managers. Workplace characteristics are at the office-quarter level. Standard deviations are in parentheses.

[a] Unexplained log pay is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details.

[b] GS grade is summarized at the time of arrival of the first new manager for the subsample of employees working under the GS schedule, which includes 90% of workers in the event-study sample.

Table 4—Estimates of Same-sex Manager Effects on Pay, Grade, and Occupation Switching (GS sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Panel A: Log pay		Panel B: Alt log pay[a]		Panel C: GS Grade	
Same-sex manager	-1.722***	-1.151***	-1.722***	-1.422***	-0.178***	-0.105***
(δ)	(0.436)	(0.320)	(0.436)	(0.338)	(0.043)	(0.030)
Same-sex manager*female	4.051***	3.169***	4.051***	3.718***	0.440***	0.299***
(δ^f)	(0.643)	(0.464)	(0.643)	(0.505)	(0.064)	(0.044)
New manager	11.029***	7.659***	11.029***	7.693***	0.939***	0.632***
(α)	(0.341)	(0.266)	(0.341)	(0.278)	(0.034)	(0.025)
New manager*female	-0.250	-1.784***	-0.250	-1.428***	0.012	-0.148***
(α^f)	(0.412)	(0.311)	(0.412)	(0.328)	(0.041)	(0.030)
Female employee and manager	13.108***	7.894***	13.108***	8.561***	1.213***	0.678***
($\delta + \delta^f + \alpha + \alpha^f$)	(0.335)	(0.242)	(0.335)	(0.273)	(0.035)	(0.024)
Male employee and manager	9.307***	6.509***	9.307***	6.271***	0.760***	0.527***
($\delta + \alpha$)	(0.285)	(0.212)	(0.285)	(0.221)	(0.028)	(0.019)
Female−male same-sex manager	**3.801*** **	**1.385*** **	**3.801*** **	**2.290*** **	**0.452*** **	**0.151*** **
($\delta^f + \alpha^f$): DDD	**(0.448)**	**(0.311)**	**(0.448)**	**(0.345)**	**(0.45)**	**(0.029)**
Dependent variable	Panel D: Occupation category change		Panel E: 4-Digit Occupation change		Panel F: Office switching	
Same-sex manager	-0.009	-0.009	-0.029***	-0.024***	-0.005*	-0.005*
(δ)	(0.007)	(0.006)	(0.008)	(0.006)	(0.003)	(0.003)
Same-sex manager*female	00.017	0.016	0.035***	0.020**	0.006	0.006
(δ^f)	(0.011)	(0.010)	(0.011)	(0.009)	(0.004)	(0.004)
New manager	0.022***	-0.002	0.054***	0.035***	0.009***	0.005***
(α)	(0.005)	(0.005)	(0.006)	(0.005)	(0.002)	(0.002)
New manager*female	0.091***	0.086***	0.053***	0.020***	-0.003	-0.004
(α^f)	(0.007)	(0.007)	(0.008)	(0.006)	(0.003)	(0.003)
Female employee and manager	0.122***	0.090***	0.113***	0.051***	0.007***	0.002
($\delta + \delta^f + \alpha + \alpha^f$)	(0.006)	(0.006)	(0.006)	(0.005)	(0.002)	(0.002)
Male employee and manager	0.013**	-0.011**	0.024***	0.011**	0.003*	-0.000
($\delta + \alpha$)	(0.004)	(0.004)	(0.005)	(0.004)	(0.002)	(0.002)
Female−male same-sex manager	**0.109*** **	**0.102*** **	**0.089*** **	**0.040*** **	**0.004**	**0.002**
($\delta^f + \alpha^f$): DDD	**(0.008)**	**(0.007)**	**(0.008)**	**(0.006)**	**(0.003)**	**(0.003)**
Individual controls	No	Yes	No	Yes	No	Yes

Note: The sample is the event-study sample from Table 2 employed under the GS schedule. In all panels, the estimation specification from equation 3 includes year-quarter and individual-office fixed effects. In columns 2, 4, and 6 the dependent variables are residualized as in the main event-study. In Panels A and B, log pay and log pay residual are scaled by 100. In Panel B, alternative log pay residuals are obtained as in the main event-study but without occupation controls in stage 1. In Panels, D, E, and F, individual controls include age categories, education, tenure, and a part-time indicator, and in Panels E and F they also include occupation categories. Average values with standard deviations in parentheses for dependent variables are: log pay 1,071 (44.73); log pay residual -1.18 (17.58); alt log pay residual[a] -1.69 (19.82); GS grade 8.97 (2.79) ; residualized GS grade -0.07 (1.47); occupation category change 0.26 (0.44); 4-digit occupation change 0.33 (0.47); office switching 0.32 (0.47). Each regression is comprised of 28,377 of unique individuals and 1,044,194 person-quarters. Standard errors clustered at the office level in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

[a] Log pay residual without occupation controls in stage 1.

Table 5—Federal Employee Viewpoint Survey: Female Management Shares and Employee Attitudes

Dependent variable: answers to questions on the scale 1-5	Job satisfaction[a]	Pay satisfaction[a]	Recognition[a]	Leadership opportunities[b]	Employee development[b]	Discrimination not tolerated[b]
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.055***	-0.022	-0.101***	-0.106***	-0.119***	-0.169***
	(0.019)	(0.020)	(0.016)	(0.015)	(0.010)	(0.008)
Same-sex management share $\geq$ median	-0.024	-0.035	-0.056**	-0.061***	-0.080***	-0.034*
	(0.026)	(0.043)	(0.025)	(0.017)	(0.018)	(0.019)
Same-sex management share $\geq$ median * Female	0.090*	0.119*	0.104**	0.119***	0.125***	0.089**
	(0.044)	(0.060)	(0.048)	(0.034)	(0.031)	(0.035)

Note: Estimation sample in all regressions contains 399,425 observations. All specifications include year and agency fixed effects. Same-sex management share $\geq$ median is an indicator equal to 1 when at least 46% of the management at the employee's agency are the same sex as the employee. Female is an indicator variable of the employee self-identifying as female. The sample is restricted to the subset of agencies whose female respondent shares in FEVS are within 15 percentage points of their shares in OPM. Standard errors clustered by agency are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

[a] Each question asks the worker to rate how satisfied they are with that aspect of their employment with 1 being the least satisfied and and 5 being the most satisfied.

[b] Each question asks the respondent to use a 1-5 scale to indicate how much she agrees with a statement with 5 indicating strong agreement. The text of the statements are: Leadership opportunities: "Supervisors/team leaders in my work unit provide employees with the opportunities to demonstrate their leadership skills." Employee development: "Supervisors/team leaders in my work unit support employee development. " Discrimination not tolerated: "Prohibited personnel practices (e.g. illegally discriminating for or against any employee/applicant, obstructing a person's right to compete for employment, knowingly violating veterans' preference requirements) are not tolerated.

Average responses are: job satisfaction – 3.78, pay satisfaction – 3.60, recognition – 3.38, leadership opportunities – 3.78, employee development – 3.78, discrimination not tolerated – 3.76.

NOT FOR PUBLICATION

Supplementary Appendices

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- Appendix A: Supplementary Tables and Figures

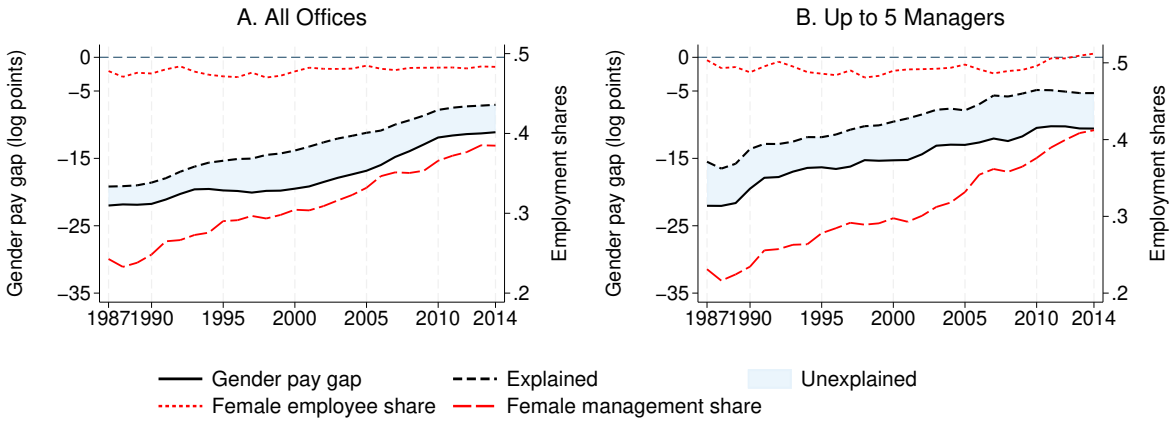


FIGURE A.1. GENDER PAY GAP AND FEMALE EMPLOYMENT

Note: The "All offices" sample in Panel A is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees, and born in the year starting 1955. Panel B further restricts the sample to offices with no more than 5 managers. The "Explained" pay gap is the gender pay gap that can be explained by locality FE, education, occupation, age, and age^2. Both female management share and female employee share are based on the managers and employees whose gender we were able to identify.

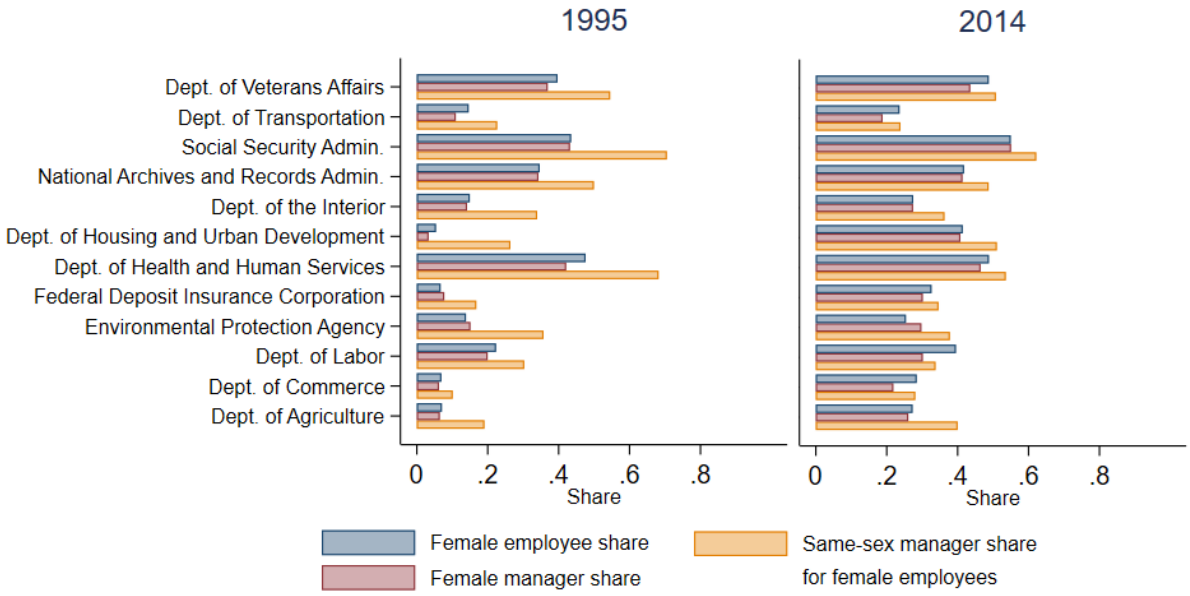
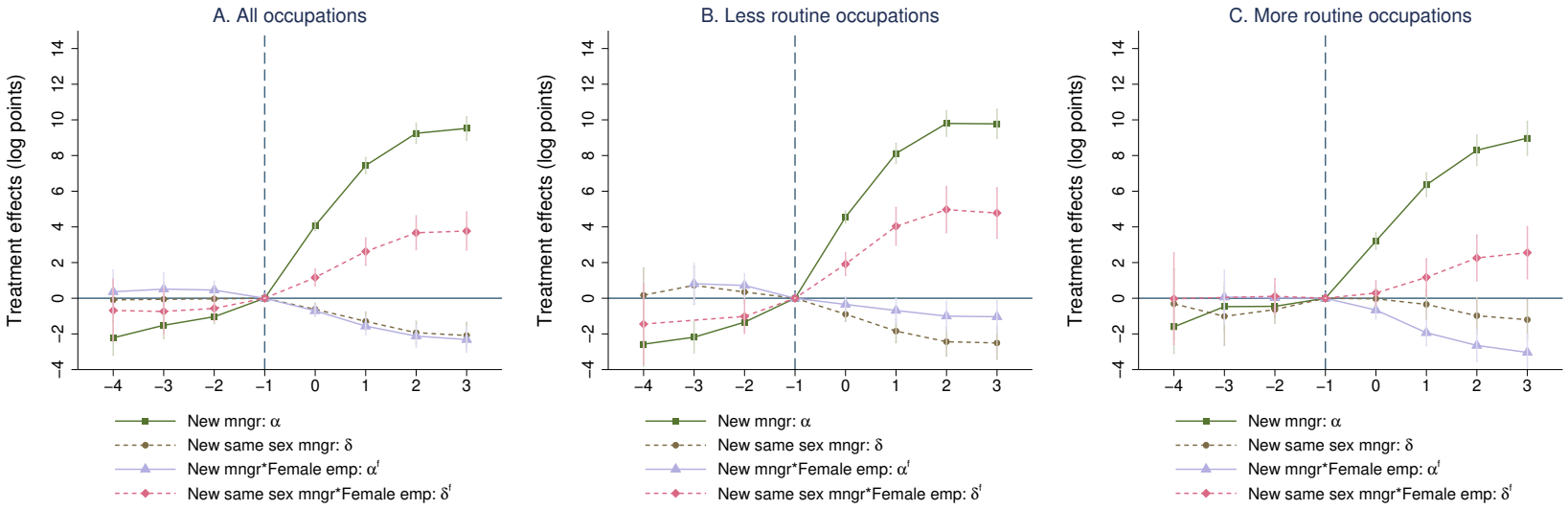


FIGURE A.2. FEMALE MANAGEMENT AND EMPLOYEE SHARES FOR SELECT AGENCIES

Note: The sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 managers, and are born in the year starting 1955.



A.1

FIGURE A.3. EVENT STUDY ESTIMATES: EFFECTS OF FIRST NEW MANAGERS

Note: Treatments effects, and specification are defined as in Figure 2. Panels A is for all employees and occupations in the event-study sample as defined in Figure 2.Panels B and C are for a subsample of employees with less routine occupations, as defined in Figure 3. Point estimates and standard errors are reported in Tables 2 and **??**. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

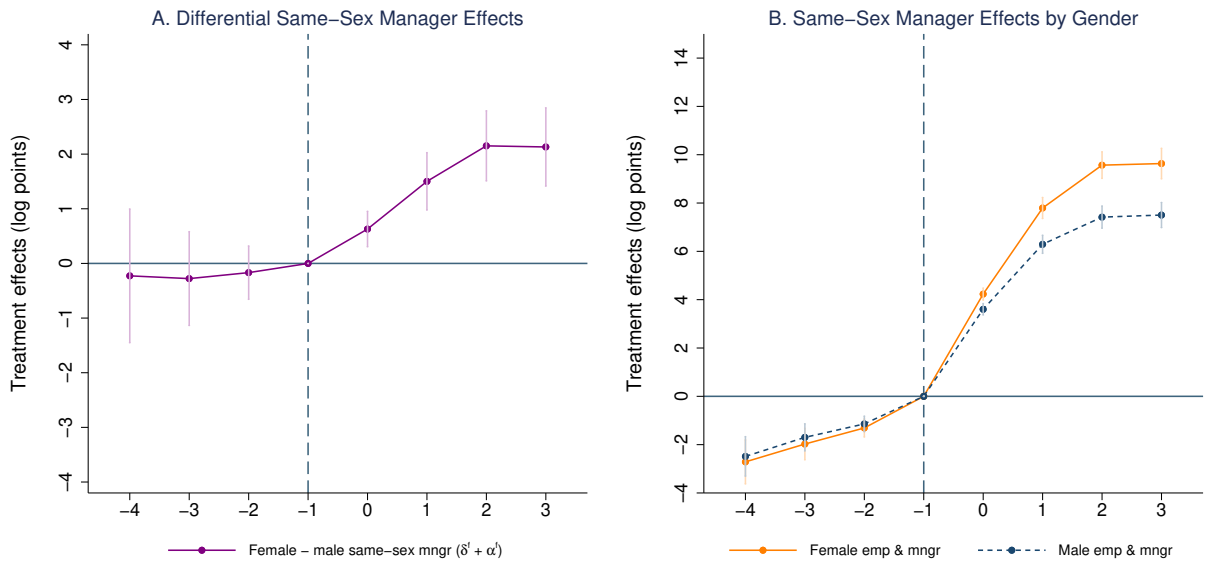


FIGURE A.4. EVENT STUDY ESTIMATES: EFFECTS OF FIRST NEW MANAGERS FOR GS SUBSAMPLE

Note: Treatments effects and specification are defined as in Figure 2. The sample includes employees that work under the GS schedule from the event-study sample. Point estimates and standard errors are reported in Table A.3. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

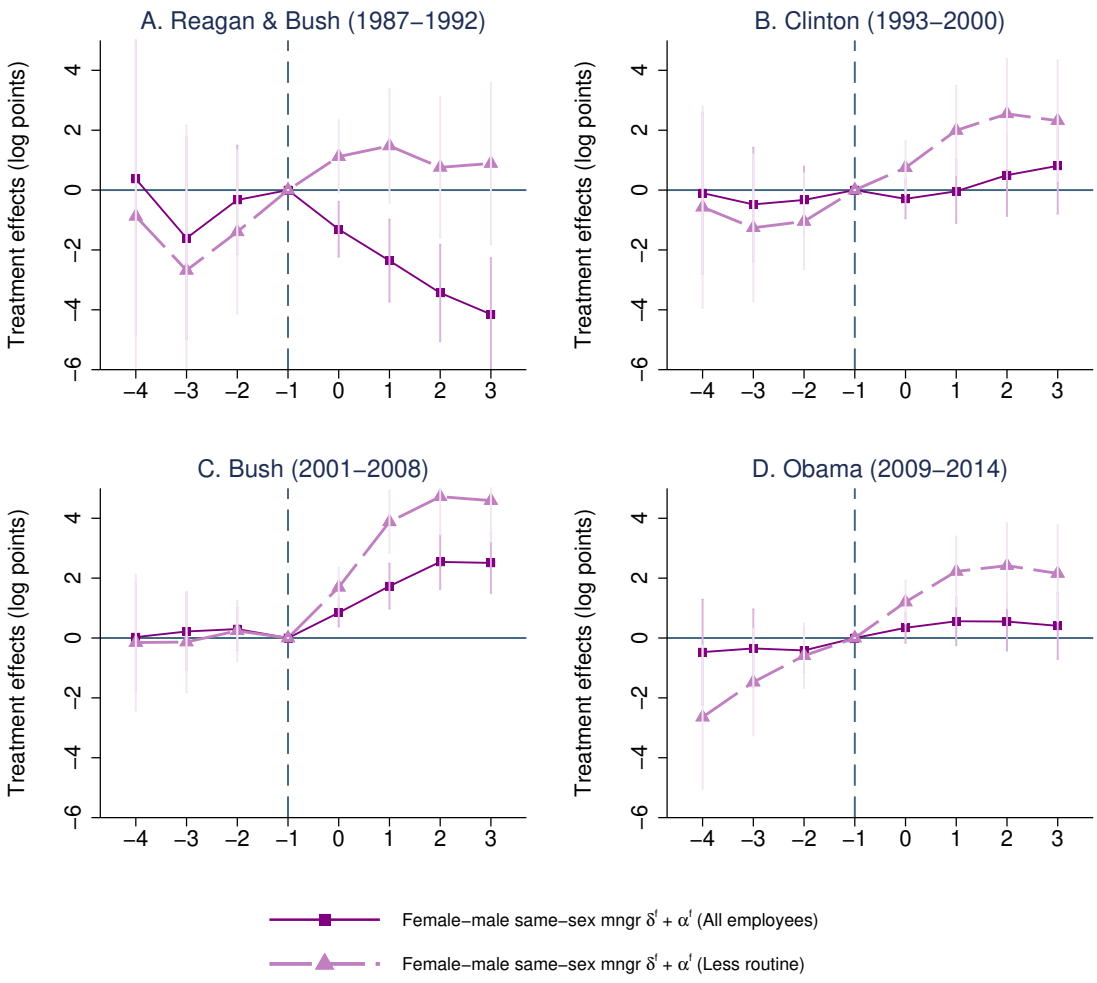


FIGURE A.5. EVOLUTION OF DIFFERENTIAL SAME-SEX MANAGER EFFECTS OVER PRESIDENTIAL ERAS (DDD)

Note: Coefficients and specification are defined as in Figure 2C. The Figure displays estimates for each presidential era. Employees are assigned to the era they received their first new manager. There are 4,205 employees in the Reagan and Bush (1987-1992) sample, 6,670 employees in the Clinton (1993-2000) sample, 14,823 employees in the Bush (2001-2008) sample, and 8,593 employees in the Obama (2009-2014) sample. The subsample of employees with less routine occupations is defined in Figure 3. There are 2,223 employees in the Reagan and Bush (1987-1992) era, 3,872 employees in the Clinton (1993-2000) era, 8,495 employees in the Bush (2001-2008) era, and 4,651 employees in the Obama (2009-2014) era. Point estimates and standard errors are reported in Tables **??**, **??**, **??**, and **??**. Vertical bars represent 95% confidence intervals with standard errors clustered by office. Evolution of homophily effects over presidential eras is depicted in Figure 4.

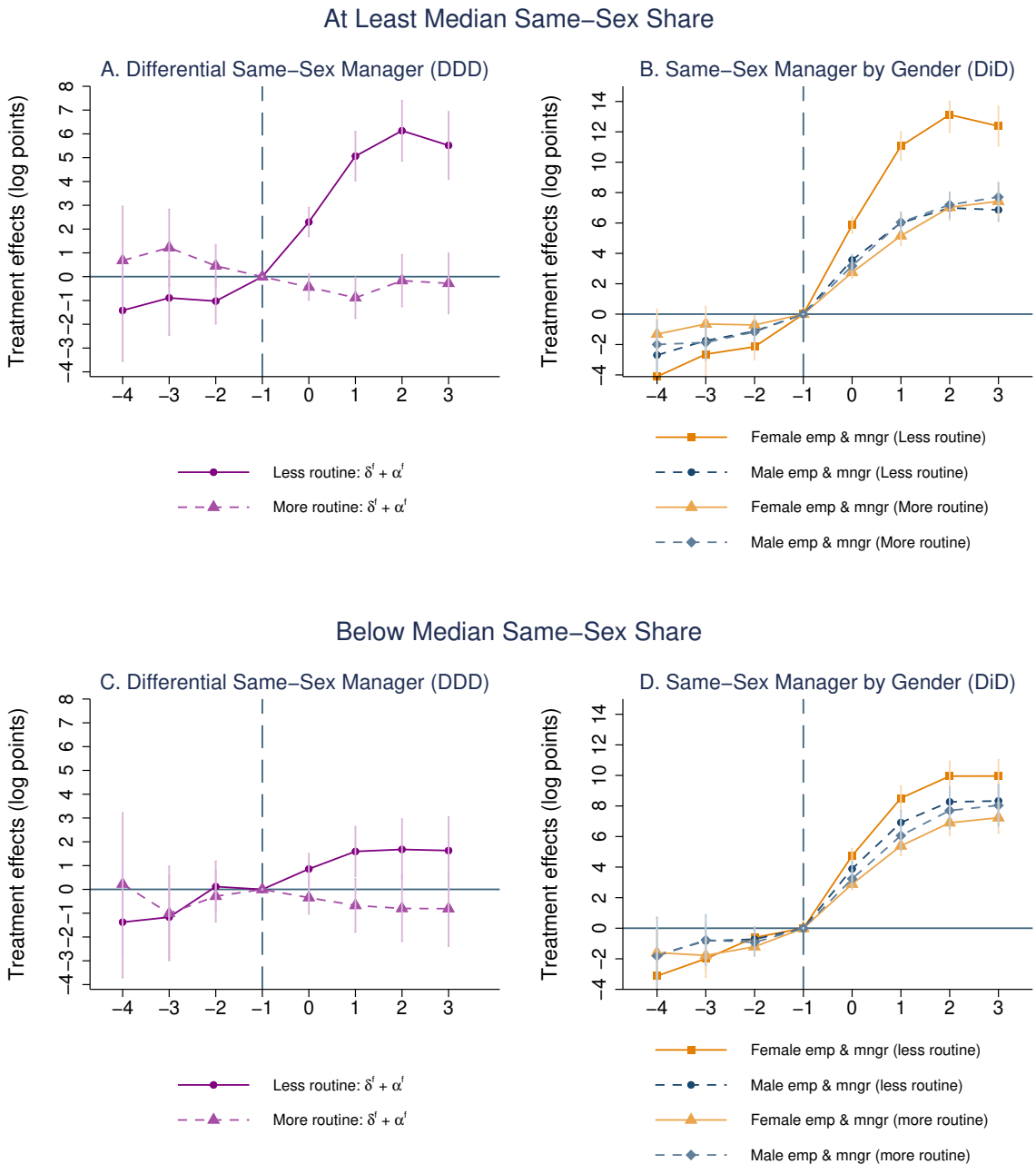


FIGURE A.6. OCCUPATIONAL ROUTINENESS ESTIMATES BY INITIAL SHARE OF SAME-SEX MANAGERS

Note: Treatments effects, sample, and specification are defined as in Figure 2. Panels A and B split the employees with an at least median share of same-sex managers in office (i.e. 45.65%) in the first two quarters of employees' tenure into employees with less routine and more routine occupations (i.e. occupations with a below median routine cognitive score). Panels C and D split employees with a below median share of same-sex managers into employees with less routine and more routine occupations. Point estimates and standard errors are reported in Tables A.4 and A.5. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

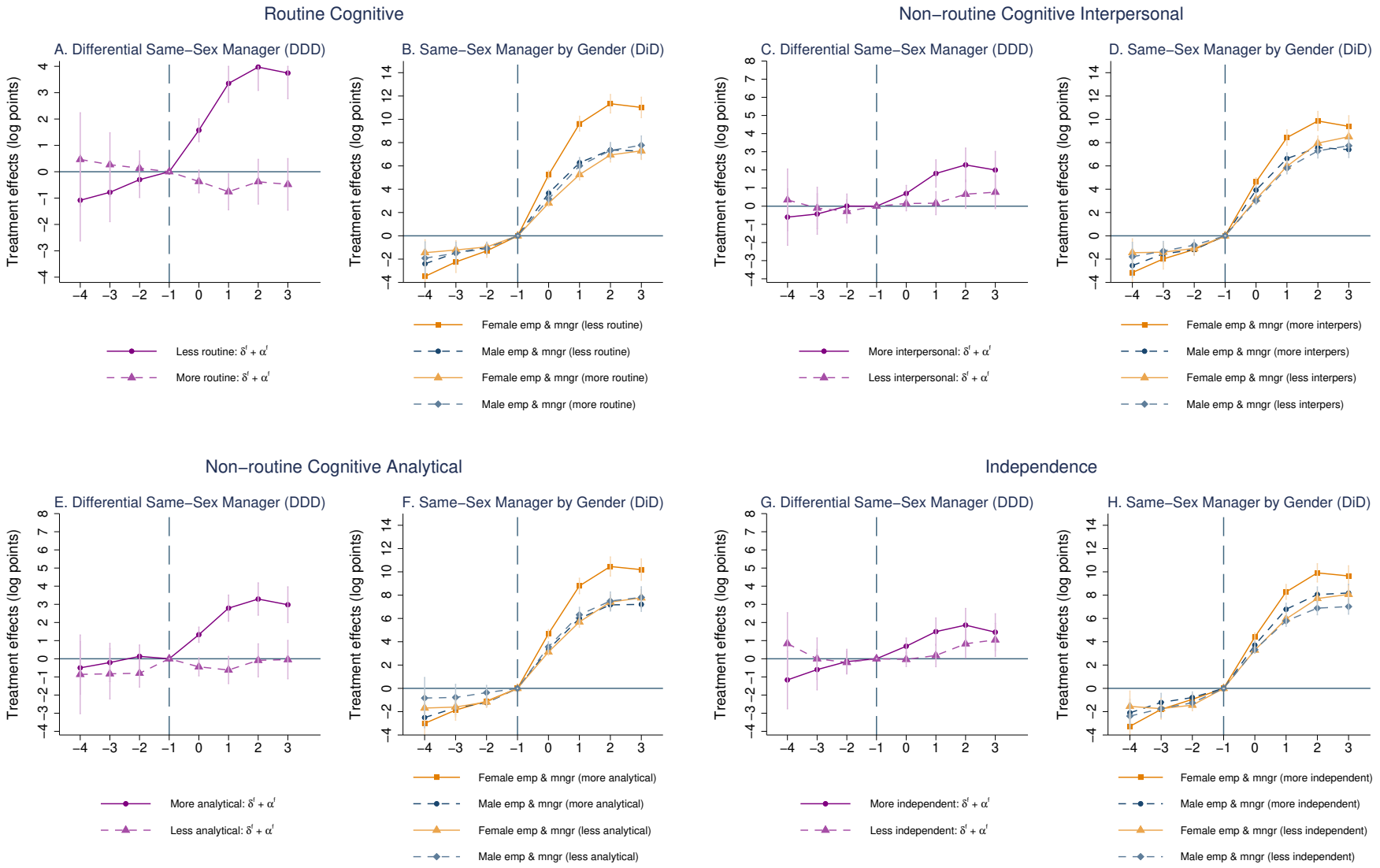


FIGURE A.7. EVENT STUDY ESTIMATES BY ALTERNATE O*NET INDICES

Note: Treatments effects and specification are defined as in Figure 2. Panels A and B split the sample from Figure 2 into employees by routineness of occupations, defined by below-median and above-median or median O*NET routine cognitive index (median – 0). Panels C, D and E, F split the sample by interpersonal and analytical non-routineness of occupations, defined by above-median and below-median or median non-routine interpersonal (median – 0) and analytical (median – 0.63) O*NET indices. Panels G and H split the sample by independence of occupations, defined by above-median and below-median or median independence (median – 3.97) O*NET index. Point estimates and standard errors are reported in Tables **??**, A.6, A.7, and A.8. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

FIGURE A.8. EVENT STUDY ESTIMATES: EFFECTS OF FIRST AND SECOND NEW MANAGERS

Note: Treatments effects, sample, and specification are defined as in Figure 2. The estimated equation is given by:
$\hat{\omega}_{itq} = \delta_k^1 \cdot \mathbb{I}^{Event_1} \cdot NSM_{idt} + \delta_k^{f1} \cdot \mathbb{I}^{Event_1} \cdot F_i \cdot \ NSM_{idt} + \alpha_k^1 \cdot \mathbb{I}^{Event_1} + \alpha_k^{f1} \cdot \mathbb{I}^{Event_1} \cdot F_i +$
$\delta_k^2 \cdot \mathbb{I}^{Event_2} \cdot NSM_{idt} + \delta_k^{f2} \cdot \mathbb{I}^{Event_2} \cdot F_i \cdot \ NSM_{idt} + \alpha_k^2 \cdot \mathbb{I}^{Event_2} + \alpha_k^{f2} \cdot \mathbb{I}^{Event_2} \cdot F_i + \gamma_i + \lambda_q + \varepsilon_{itq}$,
where $Event_1$ denotes the arrival of a first manager and $Event_2$ denotes the arrival of a second manager. The remaining notation is the same as in event study equation 2. Point estimates and standard errors are reported in Table A.13. Vertical bars represent 95% confidence intervals with standard errors clustered by office.

Table A.1—Managerial Descriptive Statistics

Sample	All managers			First new managers		
Manager sex	Female	Male	Unidentified	Female	Male	Unidentified
	(1)	(2)	(3)	(4)	(5)	(6)
Salary	89,459	95,265	84,496	68,124	71,545	59,234
	(38,136)	(42,251)	(39,229)	(26,170)	(29,087)	(26,497)
Log salary	11.31	11.37	11.24	11.05	11.09	10.89
	(0.45)	(0.45)	(0.47)	(0.42)	(0.42)	(0.45)
GS grade	12.36	12.80	12.41	11.78	12.14	11.68
(GS sample)	(2.29)	(2.11)	(2.31)	(1.96)	(1.96)	(2.18)
Birth year	0.42	0.51	0.63	0.36	0.41	0.63
1955-	(0.49)	(0.50)	(0.48)	(0.48)	(0.49)	(0.48)
Birth year	0.17	0.14	0.10	0.17	0.14	0.10
1955-1960	(0.37)	(0.35)	(0.29)	(0.37)	(0.35)	(0.31)
Birth year	0.13	0.10	0.06	0.15	0.13	0.07
1960-1965	(0.34)	(0.31)	(0.24)	(0.36)	(0.34)	(0.26)
Birth year	0.09	0.07	0.05	0.12	0.11	0.06
1965-1970	(0.29)	(0.26)	(0.21)	(0.33)	(0.31)	(0.23)
Birth year	0.05	0.04	0.03	0.07	0.08	0.04
1970-1975	(0.23)	(0.20)	(0.16)	(0.26)	(0.27)	(0.18)
Birth year	0.03	0.02	0.01	0.05	0.04	0.02
1975-1980	(0.17)	(0.15)	(0.12)	(0.22)	(0.20)	(0.13)
Birth year	0.11	0.11	0.13	0.07	0.09	0.09
1980+	(0.31)	(0.31)	(0.34)	(0.26)	(0.29)	(0.28)
Education:	0.15	0.10	0.13	0.16	0.09	0.15
High school or less	(0.35)	(0.30)	(0.33)	(0.37)	(0.29)	(0.36)
Education:	0.22	0.16	0.16	0.25	0.16	0.14
Some college	(0.41)	(0.37)	(0.37)	(0.43)	(0.36)	(0.35)
Education:	0.33	0.36	0.34	0.37	0.43	0.34
Bachelor's degree	(0.47)	(0.48)	(0.47)	(0.48)	(0.50)	(0.47)
Education:	0.31	0.37	0.36	0.21	0.30	0.34
Graduate degree	(0.46)	(0.48)	(0.48)	(0.41)	(0.46)	(0.47)
Total tenure	18.78	19.56	19.67	14.74	13.89	13.84
(years)	(9.84)	(10.07)	(10.31)	(9.72)	(9.29)	(10.64)
Managerial tenure	8.01	9.27	8.54	2.90	3.31	3.21
(years)	(7.14)	(7.42)	(6.78)	(5.42)	(5.82)	(5.48)
N unique individuals	85,253	127,380	46,502	4,814	7,936	2,872
N unique individuals (GS sample)	62,974	78,067	24,201	4,095	6,073	1,920
N offices	6,643	7,845	5,547	2,914	3,995	1,924

Note: Managerial characteristics are provided for offices in the event-study sample as defined in Table 1. Standard deviations are in parentheses.

Table A.2—Managerial Transitions (Event Study Sample)

Total managerial changes by sex	Female managers		Male managers		Unidentified managers	
	N (1)	Share (2)	N (3)	Share (4)	N (5)	Share (6)
By source of managerial change						
Appointments within Federal service	4,984	96%	8,031	94%	2,769	90%
Appointments within offices	2,656	51%	3,971	47%	1,334	44%
By type of managerial change						
Managerial additions	4,232	81%	6,929	81%	2,535	83%
Managerial replacements	985	19%	1,590	19%	525	17%
By increasing managerial share						
Increasing female management share	3,696	71%	837	10%	263	9%
Increasing male management share	326	6%	4,383	51%	294	10%
By age of new managers						
At least 5 years younger	1,475	28%	2,046	24%	561	18%
At least 5 years older	1,737	33%	3,344	39%	1,476	48%
Total managerial changes	5,217		8,519		3,060	
N unique individuals	4,814		7,936		2,872	
N offices	2,914		3,995		1,924	

Note: Managerial transitions are provided for first new managers in the event-study sample as defined in Table 1. Appointments within Federal service are when an incoming manager has worked within the US Federal service in the quarter prior to being appointed as a new manager, whereas appointments within offices are when new managers have been employed at the same offices. Managerial additions denote appointments that increase the number of managers at the office, while managerial replacements are appointments after which the number of managers at the office decreases or remains the same. Increasing female/male management share appointments include managerial changes after which the female/male managerial share increses at the time of managerial change relative to the previous quarter. At least 5 years older/younger appointments denote new managers that are older/younger than the average manager at the office by at least 5 years. Standard deviations are in parentheses.

TABLE A.3—EVENT STUDY ESTIMATES: EFFECTS OF FIRST NEW MANAGERS FOR GS SUBSAMPLE

Event year	New same-sex mngr (δ)	New same-sex mngr*female (δ^f)	New mngr (α)	New mngr*female (α^f)	Female-male same-sex mngr ($\delta^f + \alpha^f$)
	(1)	(2)	(3)	(4)	(5)
-4	-0.27	-0.58	-2.22***	0.36	-0.23
	(0.65)	(0.91)	(0.52)	(0.65)	(0.62)
-3	-0.02	-0.99	-1.68***	0.72	-0.28
	(0.46)	(0.64)	(0.37)	(0.46)	(0.44)
-2	-0.05	-0.68*	-1.09***	0.51*	-0.17
	(0.27)	(0.37)	(0.22)	(0.27)	(0.25)
0	-0.76***	1.42***	4.36***	-0.79***	0.63***
	(0.18)	(0.25)	(0.14)	(0.17)	(0.17)
1	-1.50***	3.07***	7.80***	-1.57***	1.50***
	(0.28)	(0.41)	(0.24)	(0.27)	(0.27)
2	-2.08***	4.15***	9.50***	-2.00***	2.15***
	(0.35)	(0.50)	(0.32)	(0.34)	(0.33)
3	-2.06***	4.11***	9.56***	-1.98***	2.13***
	(0.39)	(0.56)	(0.36)	(0.38)	(0.37)
N unique individuals	28,385	28,385	28,385	28,385	28,385
N person-qtrs	1,172,730	1,172,730	1,172,730	1,172,730	1,172,730

Note: The sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 mangers, born in the year starting 1955, are employed under the GS schedule, and experienced an appointment of a first new manager, as explained in section III.B. Employees in the GS subsample are spread over 7,799 offices. Event year treatment effects are reported in log points. All event study specifications include quarter-year and individual-office FEs, see event study equation 2. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure A.4. Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

TABLE A.4—OCCUPATIONAL ROUTINENESS ESTIMATES FOR EMPLOYEES WITH AT LEAST A MEDIAN SHARE OF SAME-SEX MANAGERS IN OFFICE

	Less routine				More routine				Less routine	More routine
Event year	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	Female-male same-sex mngr ($\delta^f + \alpha^f$)	Female-male same-sex mngr ($\delta^f + \alpha^f$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
-4	-0.13	-1.30	-2.57***	-0.12	-0.32	-0.61	-1.68*	1.29	-1.42	0.68
	(0.98)	(1.70)	(0.83)	(1.32)	(1.26)	(1.71)	(1.02)	(1.29)	(1.09)	(1.16)
-3	0.37	-0.95	-2.12***	0.06	-0.65	0.25	-1.22*	0.97	-0.89	1.22
	(0.69)	(1.22)	(0.58)	(0.94)	(0.91)	(1.25)	(0.72)	(0.94)	(0.80)	(0.82)
-2	-0.03	-0.91	-1.09***	-0.11	-0.47	-0.04	-0.70*	0.49	-1.03**	0.46
	(0.40)	(0.72)	(0.33)	(0.54)	(0.52)	(0.72)	(0.41)	(0.55)	(0.49)	(0.45)
0	-0.92***	1.70***	4.49***	0.59*	-0.00	0.19	3.18***	-0.63*	2.30***	-0.44
	(0.26)	(0.45)	(0.22)	(0.33)	(0.34)	(0.44)	(0.28)	(0.33)	(0.31)	(0.28)
1	-1.86***	3.55***	7.86***	1.52***	-0.09	0.68	6.14***	-1.57***	5.06***	-0.89**
	(0.41)	(0.74)	(0.35)	(0.53)	(0.54)	(0.69)	(0.46)	(0.54)	(0.53)	(0.44)
2	-2.56***	4.43***	9.55***	1.70***	-0.66	1.78**	7.86***	-1.95***	6.13***	-0.17
	(0.50)	(0.91)	(0.44)	(0.66)	(0.68)	(0.90)	(0.60)	(0.72)	(0.65)	(0.56)
3	-2.94***	4.19***	9.80***	1.33*	-0.91	2.01*	8.63***	-2.29***	5.52***	-0.28
	(0.56)	(1.01)	(0.50)	(0.72)	(0.76)	(1.03)	(0.68)	(0.82)	(0.72)	(0.65)
N unique individuals	8,784	8,784	8,784	8,784	6,859	6,859	6,859	6,859	8,784	6,859
N person-qtrs	341,427	341,427	341,427	341,427	270,930	270,930	270,930	270,930	341,427	270,930

Note: The event study sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 mangers, born in the year starting 1955, and experienced an appointment of a first new manager, as explained in section III.B. Employees are spread over 4,968 offices in the sample with less routine employees and 3,996 offices in the sample with more routine employees. Median initial share of same-sex managers is 45.65%. Event year treatment effects are reported in log points. All event study specifications include quarter-year and individual-office FEs, see event study equation 2. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure A.6. Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

TABLE A.5—OCCUPATIONAL ROUTINENESS ESTIMATES FOR EMPLOYEES WITH BELOW MEDIAN SHARE OF SAME-SEX MANAGERS IN OFFICE

	Less routine				More routine				Less routine	More routine
Event year	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	Female-male same-sex mngr ($\delta^f + \alpha^f$)	Female-male same-sex mngr ($\delta^f + \alpha^f$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
-4	0.99	-1.98	-2.74***	0.60	-0.28	0.22	-1.55	0.01	-1.38	0.23
	(1.25)	(1.64)	(0.97)	(1.12)	(1.69)	(2.04)	(1.15)	(1.35)	(1.19)	(1.53)
-3	1.55*	-2.39**	-2.38***	1.22	-1.23	-0.03	0.46	-0.98	-1.17	-1.01
	(0.91)	(1.19)	(0.70)	(0.81)	(1.51)	(1.69)	(1.26)	(1.34)	(0.88)	(1.01)
-2	1.03*	-1.22*	-1.75***	1.34***	-0.73	0.11	-0.19	-0.40	0.12	-0.29
	(0.56)	(0.72)	(0.42)	(0.49)	(0.62)	(0.73)	(0.45)	(0.51)	(0.54)	(0.55)
0	-0.74**	1.68***	4.63***	-0.82***	0.01	0.31	3.23***	-0.66	0.86***	-0.35
	(0.34)	(0.45)	(0.25)	(0.30)	(0.49)	(0.54)	(0.39)	(0.41)	(0.33)	(0.35)
1	-1.44***	3.41***	8.36***	-1.82***	-0.53	1.52*	6.60***	-2.20***	1.59***	-0.67
	(0.54)	(0.72)	(0.44)	(0.50)	(0.69)	(0.80)	(0.49)	(0.54)	(0.53)	(0.57)
2	-1.72**	4.01***	10.00***	-2.33***	-1.10	2.40**	8.80***	-3.20***	1.68**	-0.80
	(0.69)	(0.89)	(0.57)	(0.63)	(0.80)	(0.95)	(0.57)	(0.62)	(0.65)	(0.71)
3	-1.29*	3.66***	9.62***	-2.03***	-1.35	2.76**	9.39***	-3.57***	1.63**	-0.81
	(0.77)	(0.98)	(0.64)	(0.70)	(0.89)	(1.07)	(0.64)	(0.70)	(0.72)	(0.80)
N unique individuals	8,732	8,732	8,732	8,732	6,934	6,934	6,934	6,934	8,732	6,934
N person-qtrs	387,763	387,763	387,763	387,763	326,050	326,050	326,050	326,050	387,763	326,050

Note: The event study sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 mangers, born in the year starting 1955, and experienced an appointment of a first new manager, as explained in section III.B. Employees are spread over 5,023 offices in the sample with less routine employees and 4,707 offices in the sample with more routine employees. Median initial share of same-sex managers is 45.65%. Event year treatment effects are reported in log points. All event study specifications include quarter-year and individual-office FEs, see event study equation 2. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure A.6. Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table A.6—Event Study Estimates by Non-Routine Interpersonal Index

	More Interpersonal				Less Interpersonal				More interpersonal	Less interpersonal
Event year	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	Female-male same-sex mngr ($\delta^f + \alpha^f$)	Female-male same-sex mngr ($\delta^f + \alpha^f$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
-4	0.36	-1.07	-2.91***	0.47	-0.74	0.07	-1.06	0.28	-0.60	0.35
	(0.80)	(1.15)	(0.64)	(0.80)	(0.95)	(1.30)	(0.75)	(0.94)	(0.79)	(0.87)
-3	0.35	-0.98	-1.90***	0.55	-0.35	-0.70	-0.96*	0.60	-0.43	-0.10
	(0.62)	(0.86)	(0.52)	(0.62)	(0.67)	(0.89)	(0.52)	(0.65)	(0.57)	(0.58)
-2	0.02	-0.61	-1.20***	0.62*	-0.06	-0.55	-0.74**	0.27	0.01	-0.28
	(0.34)	(0.48)	(0.26)	(0.33)	(0.37)	(0.49)	(0.30)	(0.37)	(0.34)	(0.33)
0	-0.73***	1.31***	4.66***	-0.61***	-0.22	0.66**	3.22***	-0.52**	0.70***	0.15
	(0.23)	(0.34)	(0.19)	(0.22)	(0.24)	(0.30)	(0.19)	(0.22)	(0.23)	(0.21)
1	-1.73***	3.18***	8.36***	-1.38***	-0.44	1.58***	6.25***	-1.41***	1.81***	0.17
	(0.36)	(0.57)	(0.30)	(0.35)	(0.37)	(0.48)	(0.30)	(0.34)	(0.39)	(0.32)
2	-2.45***	4.20***	10.03***	-1.92***	-0.89*	2.56***	8.18***	-1.90***	2.28***	0.66*
	(0.44)	(0.69)	(0.38)	(0.43)	(0.46)	(0.59)	(0.39)	(0.43)	(0.47)	(0.40)
3	-2.56***	4.13***	9.96***	-2.13***	-1.19**	2.88***	8.92***	-2.11***	2.00***	0.78*
	(0.49)	(0.76)	(0.44)	(0.48)	(0.51)	(0.68)	(0.43)	(0.48)	(0.52)	(0.46)
N unique individuals	17,137	17,137	17,137	17,137	14,219	14,219	14,219	14,219	17,137	14,219
N person-qtrs	719,646	719,646	719,646	719,646	608,675	608,675	608,675	608,675	719,646	608,675

Note: The event study sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 mangers, born in the year starting 1955, and experienced an appointment of a first new manager, as explained in section III.B. Employees are spread over 6,392 offices in the sample with employees with more interpersonal occupations (above median non-routine interpersonal O*NET index) and 6,334 offices in the sample with employees with less interpersonal occupations (below median or median non-routine interpersonal O*NET index). Median non-routine interpersonal O*NET index is 0. Event year treatment effects are reported in log points. All event study specifications include quarter-year and individual-office FEs, see event study equation 2. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure A.7. Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table A.7—Event Study Estimates by Non-Routine Analytical Index

	More analytical				Less analytical				More analytical	Less analytical
Event year	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	Female-male same-sex mngr ($\delta^f + \alpha^f$)	Female-male same-sex mngr ($\delta^f + \alpha^f$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
-4	0.28	-0.35	-2.78***	-0.15	-0.26	-1.07	-1.06	0.21	-0.50	-0.86
	(0.74)	(1.10)	(0.61)	(0.80)	(1.19)	(1.48)	(0.75)	(0.95)	(0.74)	(1.11)
-3	0.58	-0.68	-2.24***	0.47	-0.93	-0.64	-0.96*	-0.19	-0.21	-0.83
	(0.57)	(0.81)	(0.48)	(0.60)	(0.80)	(1.03)	(0.52)	(0.67)	(0.54)	(0.71)
-2	0.16	-0.29	-1.39***	0.42	-0.16	-0.94*	-0.74**	0.14	0.13	-0.80**
	(0.30)	(0.46)	(0.25)	(0.33)	(0.45)	(0.56)	(0.30)	(0.38)	(0.32)	(0.39)
0	-0.77***	1.42***	4.13***	-0.10	-0.35	0.74**	3.22***	-1.18***	1.33***	-0.44*
	(0.21)	(0.32)	(0.18)	(0.21)	(0.29)	(0.35)	(0.19)	(0.24)	(0.21)	(0.25)
1	-1.89***	3.35***	7.89***	-0.56	-0.21	1.37**	6.25***	-1.99***	2.79***	-0.62
	(0.33)	(0.54)	(0.29)	(0.34)	(0.44)	(0.55)	(0.30)	(0.36)	(0.37)	(0.38)
2	-2.62***	4.26***	9.78***	-0.96**	-0.55	2.30***	8.18***	-2.39***	3.29***	-0.09
	(0.40)	(0.66)	(0.37)	(0.42)	(0.54)	(0.67)	(0.39)	(0.46)	(0.45)	(0.47)
3	-2.78***	4.22***	9.99***	-1.24***	-0.55	2.35***	8.92***	-2.40***	2.98***	-0.05
	(0.45)	(0.73)	(0.41)	(0.47)	(0.62)	(0.77)	(0.43)	(0.53)	(0.50)	(0.54)
N unique individuals	18,533	18,533	18,533	18,533	12,619	12,619	12,619	12,619	18,533	12,619
N person-qtrs	779,298	779,298	779,298	779,298	539,982	539,982	539,982	539,982	779,298	539,982

Note: The event study sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 mangers, born in the year starting 1955, and experienced an appointment of a first new manager, as explained in section III.B. Employees are spread over 6,557 offices in the sample with employees with more analytical occupations (above median non-routine analytical O*NET index) and 5,685 offices in the sample with employees with less analytical occupations (below median or median non-routine analytical O*NET index). Median non-routine analytical O*NET index is 0.63. Event year treatment effects are reported in log points. All event study specifications include quarter-year and individual-office FEs, see event study equation 2. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure A.7. Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table A.8—Event Study Estimates By Independence Index

	More independent				Less independent				More independent	Less independent
Event year	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	New same-sex mngr (δ)	New same-sex mngr* female (δ^f)	New mngr (α)	New mngr* female (α^f)	Female-male same-sex mngr ($\delta^f + \alpha^f$)	Female-male same-sex mngr ($\delta^f + \alpha^f$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
-4	0.74	-2.65**	-2.78***	1.48	-0.53	1.12	-1.86***	-0.28	-1.16	0.84
	(0.97)	(1.25)	(0.61)	(0.93)	(0.81)	(1.22)	(0.63)	(0.82)	(0.81)	(0.87)
-3	1.35**	-2.46***	-2.24***	1.87***	-0.86	0.28	-0.88*	-0.28	-0.60	0.00
	(0.68)	(0.90)	(0.48)	(0.67)	(0.63)	(0.86)	(0.52)	(0.62)	(0.57)	(0.59)
-2	0.81**	-1.34**	-1.39***	1.19***	-0.60*	-0.14	-0.63***	-0.06	-0.15	-0.20
	(0.41)	(0.53)	(0.25)	(0.39)	(0.31)	(0.46)	(0.25)	(0.32)	(0.35)	(0.32)
0	-1.25***	2.11***	4.13***	-1.42***	-0.04	0.12	3.36***	-0.15	0.69***	-0.04
	(0.25)	(0.35)	(0.18)	(0.23)	(0.23)	(0.30)	(0.19)	(0.21)	(0.23)	(0.20)
1	-2.50***	4.33***	7.89***	-2.83***	-0.21	0.79*	5.99***	-0.61**	1.50***	0.18
	(0.40)	(0.59)	(0.29)	(0.38)	(0.33)	(0.46)	(0.27)	(0.31)	(0.39)	(0.32)
2	-3.08***	5.17***	9.78***	-3.31***	-0.83**	1.99***	7.72***	-1.16***	1.86***	0.83**
	(0.51)	(0.73)	(0.37)	(0.49)	(0.40)	(0.56)	(0.33)	(0.38)	(0.47)	(0.40)
3	-3.00***	4.77***	9.99***	-3.31***	-1.11**	2.44***	8.13***	-1.39***	1.47***	1.04**
	(0.57)	(0.80)	(0.41)	(0.54)	(0.46)	(0.65)	(0.39)	(0.45)	(0.52)	(0.46)
N unique individuals	15,856	15,856	15,856	15,856	15,579	15,579	15,579	15,579	15,856	15,579
N person-qtrs	647,938	647,938	647,938	647,938	685,374	685,374	685,374	685,374	647,938	685,374

Note: The event study sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 mangers, born in the year starting 1955, and experienced an appointment of a first new manager, as explained in section III.B. Employees are spread over 6,563 offices in the sample with employees with more independent occupations (above median independence O*NET index) and 6,007 offices in the sample with employees with less independent occupations (below median or median independence O*NET index). Median independence O*NET index is 3.97. Event year treatment effects are reported in log points. All event study specifications include quarter-year and individual-office FEs, see event study equation 2. The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure A.7. Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table A.9—Share of the DDD Estimate Explained by Additional Controls (GS Sample)

Dependent variable	Log pay residual (X100)			
	(1)	(2)	(3)	(4)
Additional controls		Grade change	Higher pay occupation switch	(2) and (3)
Same-sex manager	-1.151***	-0.765**	-1.092***	-0.765**
(δ)	(0.320)	(0.29)	(0.317)	(0.286)
Same-sex manager*female	3.169***	2.467***	3.094***	2.468***
(δ^f)	(0.464)	(0.410)	(0.458)	(0.410)
New manager	7.659***	5.212***	7.690***	5.215***
(α)	(0.266)	(0.235)	(0.263)	(0.234)
New manager*female	-1.784***	-1.967***	-1.970***	-1.965***
(α^f)	(0.311)	(0.282)	(0.307)	(0.282)
Female employee and manager	7.894***	4.953***	7.722***	4.952***
($\delta + \delta^f + \alpha + \alpha^f$)	(0.242)	(0.204)	(0.239)	(0.204)
Male employee and manager	6.509***	4.453***	6.598***	4.450***
($\delta + \alpha$)	(0.212)	(0.189)	(0.210)	(0.189)
Female−male same-sex manager	1.385***	0.500***	1.124***	0.503***
($\delta^f + \alpha^f$): DDD	(0.311)	(0.272)	(0.306)	(0.272)
Share explained		0.639	0.188	0.637

Note: The sample is the event-study sample from Table 2 employed under the GS schedule. The estimation specification from equation 3 includes year and individual-office fixed effects. Column (1) replicates estimated from Panel A in Table A.12. Column (2) includes a dummy variable that is equal to 1 when an employee experiences a change in grade after getting a new manager and remains 1 until employee's last quarter in federal service. Column (3) includes a dummy variable that is equal to 1 when an employee changes to a higher paying occupation after getting a new manager and remains 1 until employee's last quarter in federal service. Column (4) includes both grade change and higher paying occupation change dummies. Share explained shows the share of the female-male same-sex manager estimate explained by additional controls in columns (2), (3), and (4). Each regression is comprised of 28,377 of unique individuals and 1,044,194 person-quarters. 18,597 employees experience a change in their grade at some point after getting a new manager. 7,631 employees switch to a higher paying occupation at some point after getting a new manager. 7,233 employees experience both a change in grade and a change to a higher paying occupation at some point after getting a new manager. Standard errors clustered at the office level in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

TABLE A.10—DDD ESTIMATES OF SAME-SEX MANAGER EFFECTS: ALTERNATIVE SPECIFICATIONS

	(1)	(2)	(3)	(4)
Dependent variable	Log pay	Log pay	Log pay	Log pay residual
Same-sex manager	-2.217***	-1.722***	-1.248***	-1.151***
(δ)	(0.424)	(0.436)	(0.336)	(0.320)
Same-sex manager*female	3.241***	4.051***	3.088***	3.169***
(δ^f)	(0.611)	(0.643)	(0.489)	(0.464)
New manager	12.239***	11.03***	8.495***	7.659***
(α)	(0.330)	(0.341)	(0.263)	(0.266)
New manager*female	-0.125	-0.250	-1.952***	-1.784***
(α^f)	(0.390)	(0.412)	(0.313)	(0.311)
Female employee and manager	13.139***	13.11***	8.382***	7.894***
($\delta + \delta^f + \alpha + \alpha^f$)	(0.328)	(0.335)	(0.250)	(0.242)
Male employee and manager	10.022***	9.307***	7.247***	6.509***
($\delta + \alpha$)	(0.289)	(0.285)	(0.217)	(0.212)
Female−male same-sex manager	3.116***	3.801***	1.135***	1.385***
($\delta^f + \alpha^f$)	(0.440)	(0.448)	(0.336)	(0.311)
Individual controls	No	No	Yes	Yes
Year-quarter fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	ID	ID-office	ID-office	ID-office

Note: The sample is the event-study sample from the main analysis employed under the GS schedule. Log pay and log pay residual are scaled by 100. Column (4) reproduces the main two-stage specification where the dependent variable is the log pay residual from stage 1 estimation (controlling for locations, age, education, tenure, occupation, and part-time status). Columns (1)-(3) use raw log pay. Individual controls in column (3) include age categories, education, tenure, and a part-time indicator, and occupation categories. ID fixed effects denote individual fixed effects, and ID-office denote individual-office fixed effects. Each regression is comprised of 28,377 unique individuals and 1,044,194 person-quarters. Standard errors clustered at the office level in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table A.11—Two-Period DDD Estimates (GS Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Panel A: Log pay residual (X100)		Panel B: Residualized GS Grade		Panel C: Staying Federal Service	
Same-sex manager		-1.540***		-0.144***	-0.328***	-0.329***
(δ)		(0.573)		(0.055)	(0.079)	(0.078)
Same-sex manager*female		3.494***		0.340***	0.123	0.145*
(δ^f)		(0.800)		(0.078)	(0.080)	(0.080)
New manager		8.200***		0.748***	4.362***	4.298***
(α)		(0.949)		(0.082)	(0.729)	(0.710)
New manager*female		-1.885***		-0.189***	0.027	0.027
(α^f)		(0.572)		(0.055)	(0.056)	(0.055)
Female employee and manager		8.269***		0.755***	4.184***	4.141***
($\delta + \delta^f + \alpha + \alpha^f$)		(0.839)		(0.074)	(0.690)	(0.670)
Male employee and manager		6.660***		0.603***	4.034***	3.968***
($\delta + \alpha$)		(0.822)		(0.071)	(0.676)	(0.658)
Female−male same-sex manager		1.608***		0.151**	0.150**	0.172**
($\delta^f + \alpha^f$): DDD		(0.500)		(0.049)	(0.055)	(0.059)
Individual controls		Yes		Yes	No	Yes
Dependent variable	Panel D: Occupation category change		Panel E: 4-Digit Occupation change		Panel F: Office switching	
Same-sex manager	0.000	-0.002	-0.010	-0.012	0.005	0.005
(δ)	(0.010)	(0.010)	(0.012)	(0.009)	(0.003)	(0.003)
Same-sex manager*female	-0.001	-0.000	0.011	0.010	-0.004	-0.004
(δ^f)	(0.016)	(0.016)	(0.017)	(0.013)	(0.004)	(0.004)
New manager	-0.034*	-0.021	-0.014	0.008	-0.009	-0.008
(α)	(0.019)	(0.018)	(0.019)	(0.017)	(0.007)	(0.007)
New manager*female	0.075***	0.071***	0.053***	0.020**	0.004	0.004
(α^f)	(0.011)	(0.011)	(0.012)	(0.010)	(0.003)	(0.003)
Female employee and manager	0.040**	0.048**	0.040**	0.027*	-0.004	-0.004
($\delta + \delta^f + \alpha + \alpha^f$)	(0.017)	(0.017)	(0.018)	(0.015)	(0.007)	(0.006)
Male employee and manager	-0.034**	-0.022	-0.024	-0.004	-0.004	-0.004
($\delta + \alpha$)	(0.016)	(0.015)	(0.017)	(0.015)	(0.007)	(0.007)
Female−male same-sex manager	0.074***	0.071***	0.065***	0.031**	0.000	-0.000
($\delta^f + \alpha^f$): DDD	(0.011)	(0.010)	(0.011)	(0.009)	(0.003)	(0.003)
Individual controls	No	Yes	No	Yes	No	Yes

Note: The sample is a collapsed panel of the event-study sample from Table 2. There are 2 observations for each employee – one year before getting a new manager and 3 years after. If an employee gets a new manager later than 1 year prior to the managerial appointment, all variables in the before time period for the employee are equal to the values in the quarter that we first observe the employee in the sample. If the employee leaves Federal Service within 3 years of getting a new manage, all variables in the second snapshot are equal to the values in the employee's last quarter of federal employment. In all panels, the estimation specification from equation 3 includes year-quarter and individual-office fixed effects. In Panels A and B, the dependent variables are residualized as in the main event-study. In Panel C, the dependent variable is employees' quarterly tenure. In Panels, C, D, E, and F, individual controls include age categories, education, tenure, and a part-time indicator, and in Panels C, E, and F they also include occupation category. Average values with standard deviations in parentheses for dependent variables are: log pay residual -3.89 (18.96); residualized GS grade -0.26 (1.60); number of quarters in Federal Service 16.81 (14.77); occupation category changes 0.16 (0.37); 4-digit occupation changes 0.20 (0.40); office switching 0.19 (0.39). Each regression is comprised of 26,945 of unique individuals and 53,890 observations. Standard errors clustered at the office level in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table A.12—DDD Estimates of Same-sex Manager Effects on Pay, Grade, and Occupation Switching (GS sample)

Specification	TWFE (1)	Matching (2)	Borusyak (3)	TWFE (4)	Matching (5)	Borusyak (6)	TWFE (7)	Matching (8)	Borusyak (9)
Dependent variable	Panel A: Log pay residual			Panel B: Alt log pay residual[a]			Panel C: Residualized GS Grade		
Same-sex manager	-1.151***	-0.620		-1.422***	-0.620		-0.105***	-0.087*	
(δ)	(0.320)	(0.474)		(0.338)	(0.474)		(0.030)	(0.047)	
Same-sex manager*female	3.169***	1.625**		3.718***	1.625**		0.299***	0.193***	
(δ^f)	(0.464)	(0.701)		(0.505)	(0.701)		(0.044)	(0.072)	
New manager	7.659***	9.383***		7.693***	9.383***		0.632***	0.800***	
(α)	(0.266)	(0.402)		(0.278)	(0.402)		(0.025)	(0.040)	
New manager*female	-1.784***	1.597***		-01.428***	1.597***		-0.148***	0.219***	
(α^f)	(0.311)	(0.542)		(0.328)	(0.542)		(0.030)	(0.056)	
Female employee and manager	7.894***	11.985***	8.135***	8.561***	11.985***	8.853***	0.678***	1.125***	0.709***
($\delta + \delta^f + \alpha + \alpha^f$)	(0.242)	(0.302)	(0.259)	(0.273)	(0.302)	(0.288)	(0.024)	(0.032)	(0.026)
Male employee and manager	6.509***	8.763***	6.533***	6.271***	8.763***	6.269***	0.527***	0.712***	0.542***
($\delta + \alpha$)	(0.212)	(0.274)	(0.240)	(0.221)	(0.274)	(0.249)	(0.019)	(0.027)	(0.022)
Female−male same-sex manager	1.385***	3.222***	1.602***	2.290***	3.222***	2.584***	0.151***	0.412***	0.168***
($\delta^f + \alpha^f$)	(0.311)	(0.421)	(0.324)	(0.345)	(0.421)	(0.351)	(0.029)	(0.043)	(0.031)
Dependent variable	Panel D: Occupation category change			Panel E: 4-Digit Occupation change			Panel F: Office switching		
Same-sex manager	-0.009	-0.001		-0.024***	-0.019**		-0.005*	-0.004	
(δ)	(0.006)	(0.008)		(0.006)	(0.009)		(0.003)	(0.003)	
Same-sex manager*female	0.016	-0.004		0.020**	0.014		0.006	0.006*	
(δ^f)	(0.010)	(0.013)		(0.009)	(0.013)		(0.004)	(0.004)	
New manager	-0.002	0.100***		0.035***	0.148***		0.005***	0.021***	
(α)	(0.005)	(0.006)		(0.005)	(0.008)		(0.002)	(0.003)	
New manager*female	0.086***	0.110***		0.020***	0.072***		-0.004	-0.005*	
(α^f)	(0.007)	(0.010)		(0.006)	(0.010)		(0.003)	(0.003)	
Female employee and manager	0.090***	0.205***	0.096***	0.051***	0.216***	0.063***	0.002	0.019***	0.001
($\delta + \delta^f + \alpha + \alpha^f$)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)	(0.002)	(0.002)	(0.002)
Male employee and manager	-0.011**	0.099***	-0.019***	0.011**	0.129***	0.009**	-0.000	0.017***	-0.000
($\delta + \alpha$)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.002)	(0.002)	(0.003)
Female−male same-sex manager	0.102***	0.106***	0.114***	0.040***	0.087***	0.053***	0.002	0.001	0.001
($\delta^f + \alpha^f$)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.003)	(0.002)	(0.003)
Individual controls	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
N unique individuals	28,377	20,071	28,377	28,377	20,071	28,377	28,377	20,071	28,377
N person-quarters	1,044,194	585,628	1,044,194	1,044,194	585,628	1,044,194	1,044,194	585,628	1,044,194

Note: The sample is the event-study sample from Table 2 employed under the GS schedule. In all panels, the estimation specification from equation 3 includes year-quarter and individual-office fixed effects. In Panels A, B, and C, columns with individual controls use dependent variables residualized as in the main event-study. In Panels A and B, log pay and log pay residual are scaled by 100. In Panel B, alternative log pay residuals are obtained as in the main event-study but without occupation controls in stage 1. In Panels, D, E, and F, individual controls include age categories, education, tenure, and a part-time indicator, and in Panels E and F they also include occupation categories. In matched sample regressions in Panels A, B, and C, fixed effects also include locality-years. Average values with standard deviations in parentheses for dependent variables in TWFE and Borusyak main sample are: log pay residual -1.18 (17.58); alt log pay residual[a] -1.69 (19.82); residualized GS grade -0.07 (1.47); occupation category change 0.26 (0.44); 4-digit occupation change 0.33 (0.47); office switching 0.32 (0.47).

The matched sample was obtained using propensity score matching for female and male employees separately. Treated employees were matched to their not-yet treated counterparts in a quarter prior to treatment based on age bins, education bins, tenure bins, year bins, part-time status, occupations, PATCO occupation category, pay pan, grade, agency, locality, and office female share. Average values with standard deviations in parentheses for dependent variables in the matched sample are: log pay 1,071 (43.38); GS grade 8.89 (2.78) ; occupation category change 0.22 (0.41); 4-digit occupation change 0.27 (0.44); office switching 0.17 (0.38). Standard errors clustered at the office level in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

[a] Log pay residual without occupation controls in stage 1.

Table A.13—Event Study Estimates: Effects of First and Second New Managers

	First new mngr				Second new mngr			
Event year	New same-sex mngr (δ^1)	New same-sex mngr*female (δ^{f1})	New mngr(α^1)	New mngr*female (α^{f1})	New same-sex mngr (δ^2)	New same-sex mngr*female (δ^{f2})	New mngr (α^2)	New mngr*female (α^{f2})
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
-4	-0.09	-0.72	-2.29***	0.39				
	(0.63)	(0.88)	(0.50)	(0.62)				
-3	-0.05	-0.77	-1.57***	0.54				
	(0.47)	(0.64)	(0.38)	(0.46)				
-2	-0.04	-0.58	-1.07***	0.47*				
	(0.25)	(0.35)	(0.20)	(0.25)				
0	-0.65***	1.10***	3.87***	-0.69***	0.09	0.57	1.18***	-0.18
	(0.17)	(0.24)	(0.14)	(0.16)	(0.37)	(0.51)	(0.24)	(0.28)
1	-1.30***	2.31***	6.97***	-1.37***	-0.00	0.91*	0.95***	-0.40
	(0.28)	(0.40)	(0.24)	(0.28)	(0.33)	(0.47)	(0.21)	(0.26)
2	-1.76***	2.94***	8.18***	-1.74***	-0.35	1.57***	1.56***	-0.55*
	(0.35)	(0.50)	(0.31)	(0.37)	(0.35)	(0.53)	(0.24)	(0.29)
3	-1.78***	2.90***	7.93***	-1.50***	-0.48	1.65***	2.02***	-1.00***
	(0.40)	(0.57)	(0.37)	(0.44)	(0.38)	(0.59)	(0.28)	(0.35)
N unique individuals	31,117	31,117	31,117	31,117	31,117	31,117	31,117	31,117
N person-qtrs	1,318,756	1,318,756	1,318,756	1,318,756	1,318,756	1,318,756	1,318,756	1,318,756

Note: The event study sample is restricted to workers who were employed by the US Federal service for at least 5 years, work in offices with at least 10 employees and no more than 5 mangers, born in the year starting 1955, and experienced an appointment of a first new manager, as explained in section III.B. Employees are spread over 8,377 offices. Event year treatment effects are reported in log points. All event study specifications include quarter-year and individual-office FEs. The estimated equation is given by:
$\hat{\omega}_{itq} = \delta_k^1 \cdot \mathbb{I}^{Event_1} \cdot NSM_{idt} + \delta_k^{f1} \cdot \mathbb{I}^{Event_1} \cdot F_i \cdot \ NSM_{idt} + \alpha_k^1 \cdot \mathbb{I}^{Event_1} + \alpha_k^{f1} \cdot \mathbb{I}^{Event_1} \cdot F_i +$
$\delta_k^2 \cdot \mathbb{I}^{Event_2} \cdot NSM_{idt} + \delta_k^{f2} \cdot \mathbb{I}^{Event_2} \cdot F_i \cdot \ NSM_{idt} + \alpha_k^2 \cdot \mathbb{I}^{Event_2} + \alpha_k^{f2} \cdot \mathbb{I}^{Event_2} \cdot F_i + \gamma_i + \lambda_q + \varepsilon_{itq}$, where $Event_1$ denotes the arrival of a first manager and $Event_2$ denotes the arrival of a second manager. The remaining notation is the same as in event study equation 2.
The dependent variable is the residual log pay after controlling for locations, five-year birth cohorts, education, tenure, and occupation dummies, and part-time status in yearly regressions. See footnote 25 and equation 1 for details. Estimates are depicted in Figure A.8. Standard errors clustered at the office are in parentheses. * p<0.10 ** p<0.05 *** p<0.01.