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# Unintended Consequences of Gender Equality Legislation: Evidence on Male Job Satisfaction from Japan

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Institute of Social and Economic Research of Osaka University and the JILPT data archive to borrow the data. Other than these, the authors have no conflicts of interest directly relevant to the content of this article.

**Author Contributions**

Kazuma Sato conducted the study, including material preparation, data collection, and analysis, and co-wrote the first draft with Junji Kageyama.

**Declaration of Generative AI and AI-assisted technologies in the writing process**

During the preparation of this work, the authors employed ChatGPT to enhance the clarity and readability of the manuscript. Following the use of this tool, the authors thoroughly reviewed and edited the content, and they take full responsibility for the final version of the published article.

**Compliance with Ethical Standards**

**Research involving Human Participants and/or Animals**

Permission to use the data used in this paper was obtained from the participants for academic purposes and on the condition that the data be anonymized.

**Informed consent**

Not applicable.

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## Abstract

The Act on Promotion of Women's Participation and Advancement in the Workplace, enacted in Japan in 2016, requires firms with 301 or more employees to disclose gender-disaggregated workplace indicators and to formulate action plans aimed at promoting women's advancement into managerial positions. While such disclosure-based policies are intended to benefit female employees, they may also generate unintended consequences for male workers by altering perceived promotion opportunities within firms. This study provides the first empirical evidence on this possibility by examining the association between the Act and job satisfaction among non-managerial male regular employees. Using panel data from the Japan Household Panel Survey on Consumer Preferences and Satisfaction (JHPS-CPS) and a difference-in-differences framework with individual fixed effects, we exploit the policy's firm-size threshold to identify its impact. The estimated coefficient on the treatment–post interaction is negative and statistically significant, suggesting a meaningful reduction in job satisfaction among non-managerial men in firms covered by the Act. Beyond job satisfaction, the policy is also associated with declines in self-assessed health and increases in reported stress. These findings should not be interpreted as a case against gender equality policies; rather, they highlight the importance of complementary measures to mitigate adjustment costs for male workers while advancing the core objectives of the Act.



**Keywords**: The Act on Promotion of Women's Participation and Advancement in the Workplace; Job satisfaction; difference-in-differences analysis

**JEL Classification**: J08, J16, J28

## 1. Introduction

Over the past several decades, women's enrollment in higher education has increased substantially across many countries, accompanied by a steady rise in female labor force participation. As a result, women now account for roughly half of the workforce in many advanced economies and have become an integral part of modern labor markets (World Economic Forum 2023). Despite this convergence in overall employment, large cross-country differences remain in women's representation in leadership positions. According to ILOSTAT data for 2024, women hold 42.6 percent of managerial positions in the United States and 40.2 percent in the United Kingdom, with similarly high shares in countries such as Sweden and Finland[1]. In contrast, the corresponding shares are considerably lower in countries such as Germany (28.7 percent) and Italy (28.0 percent). The gap is even larger in Asia: women account for only 14.6 percent of managers in Japan and 16.3 percent in South Korea.

A growing body of research suggests that greater female representation in leadership positions may contribute to economic growth and improved firm performance (Glass and Cook 2018; Kong et al. 2024). Consequently, policymakers in many countries have introduced policies aimed at expanding women's representation in managerial roles.

In Japan, the government enacted the Act on Promotion of Women's Participation and Advancement in the Workplace in 2016 to promote gender equality in both the public and private sectors. The Act requires national and local government institutions, as well as firms with 301 or more employees, to assess women's participation in the workplace and to formulate and publicly disclose action plans to promote women's advancement. The share of female managers is treated as a key indicator of progress, and firms are encouraged to expand promotion opportunities for female employees. According to the Ministry of Health, Labour and Welfare's Basic Survey of Gender Equality in Employment Management, the share of female managers increased from 11.9 percent in 2015 to 13.1 percent in 2024, suggesting that the policy may have modestly expanded promotion opportunities for women.

At the same time, policies that encourage the appointment of female managers may also influence the career prospects of male employees. Because the number of managerial positions within firms is typically limited in the short run, expanding opportunities for women may reduce the expected promotion prospects of non-managerial men or intensify competition for leadership positions. This possibility may be particularly relevant in Japan, where traditional gender norms remain influential and men are often expected to act as primary breadwinners (Ikeda 2019). If male employees perceive a decline in promotion opportunities, increased uncertainty about future income and career progression may generate psychological strain and reduce well-being, particularly in the form of lower job satisfaction.

Despite these potential spillover effects, existing research has largely focused on the benefits of

[1] ILOSTAT (https://ilostat.ilo.org/data/)

gender equality policies for women, with little empirical attention to their consequences for male employees. This gap raises an important question: are policies designed to expand women's career opportunities linked to the well-being of male workers?

To address this question, this study examines the association between the Act and job satisfaction among non-managerial male employees. Using panel data from the Japan Household Panel Survey on Consumer Preferences and Satisfaction (JHPS-CPS), we analyze whether the policy is linked to the well-being of male workers facing potential shifts in their promotion prospects. While the policy aims to improve women's representation in leadership positions, it may also reshape the allocation of promotion opportunities within firms. By identifying this potential unintended consequence, the study contributes to a more comprehensive evaluation of gender equality policies.

The policy debate surrounding efforts to increase the share of female managers under the Act is closely related to the broader literature on gender quotas. In recent years, many countries have introduced quota policies in economic, academic, and political institutions to increase women's representation in leadership positions. For example, the European Commission has adopted legislation requiring publicly listed firms in the European Union to ensure that women account for at least 40 percent of non-executive directors or 33 percent of all board members by 2026 (Huet 2022).

Because such policies constitute a strong form of affirmative action, they often generate substantial public debate and resistance. A consistent finding in the literature is that men tend to express less support for gender quota policies than women (Bell et al. 2000; Harrison et al. 2006; Kravitz et al. 2008). One explanation for this gender gap in attitudes is self-interest. While quotas are designed to expand women's access to leadership positions, they may also reduce men's expected promotion opportunities or intensify competition for a limited number of positions. Consequently, men may perceive such policies as disadvantageous and therefore exhibit lower levels of support (Harrison et al. 2006; Kane and Whipkey 2009; Martins and Parsons 2007; Olsen et al. 2016). In addition, Zehnter and Nater (2025) argue that concerns about potential stigmatization of women promoted under quota systems may further reinforce opposition to such policies.

Taken together, these literatures suggest that gender quota policies can alter the distribution of opportunities and status within organizations, potentially generating resistance among groups that perceive themselves as disadvantaged. From this perspective, policies that promote women's advancement may have broader consequences for workplace dynamics beyond their intended benefits. The Japanese Act on Promotion of Women's Participation and Advancement in the Workplace may operate through a similar mechanism. By encouraging firms to increase the representation of women in managerial positions, the policy may connect with expectations regarding the allocation of a limited number of managerial posts. If male employees perceive that their promotion prospects have declined, this may generate psychological strain or perceptions of unfairness.

Empirical evidence regarding the Act in Japan remains limited. A small number of studies—

including Ohmachi and Mitsusada (2021), Kurumada (2022), and Nishihata and Yamamoto (2021)—provide the main empirical contributions. Ohmachi and Mitsusada (2021) examine whether female employment indicators changed following the introduction of the Act and whether such changes were associated with firm performance. They find that indicators such as the share of female employees improved after the policy was implemented, particularly among firms that initially employed relatively few women. Moreover, firms that had already appointed relatively more women to managerial positions prior to the policy experienced improvements in sales performance. Kurumada (2022) analyzes the policy's impact on women's promotion using a "promotion ratio," defined as the gender ratio among managers relative to the gender ratio among employees. This measure accounts for gender differences in workforce composition that may bias conventional indicators based solely on the share of female managers. The results show that the promotion ratio from rank-and-file positions to section chief positions increased significantly after the policy's introduction. Nishihata and Yamamoto (2021) examine the relationship between women's advancement and firm performance using data from 2010 to 2015. Although their analysis predates the implementation of the Act, it provides relevant insights. They show that improvements in the female managerial promotion ratio are positively associated with both return on assets (ROA) and total factor productivity (TFP).

Overall, existing studies suggest that the policy relates to female employment patterns and certain aspects of firm performance. However, little empirical attention has been paid to how the policy aligns with workers' well-being. In particular, there is virtually no evidence on whether the policy shares an association with the job satisfaction of male employees who are not in managerial positions.

To address this gap, we exploit the fact that the Act applies to national and local government institutions and to firms with 301 or more employees. Using a difference-in-differences framework with individual fixed effects, we estimate the policy's impact on job satisfaction using panel data from the JHPS-CPS.

The analysis yields two main findings. First, job satisfaction declined among non-managerial male employees working in firms with 301 or more employees after the Act was implemented in 2016, and this result is robust to a range of robustness checks. Second, the policy is also associated with a deterioration in subjective rated health and an increase in reported stress among non-managerial male employees.

## 2. Institutional Background, Theoretical Considerations, and Trends in Female Managerial Representation

### 2.1 Overview of the Act on Promotion of Women's Participation and Advancement in the Workplace

The Act on Promotion of Women's Participation and Advancement in the Workplace was introduced to address gender disparities in employment conditions and career opportunities and to foster a labor

market environment in which women can participate more fully in economic activity. The Act came into force in April 2016 and requires firms with 301 or more employees and public sector organizations to formulate and publicly disclose action plans designed to promote women's participation and advancement within the workplace.

Two main factors motivated the introduction of the Act. First, Japan has a much lower share of women in managerial positions than other advanced economies. As noted above, women hold over 40% of managerial roles in countries such as the United States and the United Kingdom, compared with only 14.6% in Japan. This underrepresentation may limit the inclusion of women's perspectives in corporate decision-making and slow the adoption of workplace practices that support women's career advancement. It also reduces the availability of female role models, which can weaken women's career aspirations and make long-term advancement within firms harder to envision—thereby reinforcing the low share of women in leadership. Second, the Act responds to growing concerns about future labor shortages. Japan's population is declining and is projected to fall below 90 million in the coming decades, with a corresponding shrinkage in the working-age population. Expanding women's opportunities to fully utilize their skills and participate in the labor market is therefore seen as a key strategy to sustain economic growth. By enabling women to build stable careers and remain in the workforce, the policy may help mitigate the economic impact of a declining labor supply.

Against this backdrop, the Act requires both private firms and public-sector organizations to implement several measures aimed at improving women's career opportunities. First, employers must assess the status of women's participation within the organization and identify existing challenges. This assessment includes indicators such as the share of female hires, gender differences in tenure, working-hour patterns, and the proportion of women in managerial positions. Second, based on this assessment, employers are required to formulate an action plan to promote women's advancement. The plan specifies the implementation period, numerical targets, policy measures, and the timeline for these initiatives, and it must be communicated to employees within the organization. Third, firms are required to submit the action plan to the prefectural labor bureau. Finally, employers must publicly disclose information regarding women's participation and advancement within the firm.

Among these requirements, the share of female managers has emerged as one of the most visible indicators of women's advancement within organizations. A higher share of female managers may signal to the labor market that a firm actively promotes gender diversity and supports women's career development. Such signals can enhance corporate reputation and increase a firm's attractiveness to potential job seekers.

### 2.2 Why does a disclosure-based policy generate a psychological shock?

This study focuses on the association between the Act on Promotion of Women's Participation and Advancement in the Workplace and job satisfaction among male regular employees in non-managerial

positions. However, a natural question arises: the Act does not constitute a quota system. It does not impose legally binding targets for female managerial representation, nor does it penalize firms that fail to achieve specific outcomes. Its core requirements are limited to information disclosure, the formulation of action plans, and the public reporting of gender-related workplace indicators. Why, then, should such a seemingly "soft" regulatory instrument generate a shock strong enough to affect the subjective well-being of non-managerial male employees?

We argue that the answer lies in the sequential operation of three distinct channels — organizational response, expectation revision, and procedural fairness — through which the Act translates its formal disclosure requirements into psychological effects on individual workers.

**Channel 1: Organizational response to disclosure-based reputational pressure**

Although the Act does not mandate specific numerical outcomes, its public disclosure requirements create strong reputational incentives for firms to demonstrate visible progress on gender equality. This pressure operates through two complementary mechanisms. Externally, the public reporting of gender-disaggregated workplace statistics exposes firms to scrutiny from investors, prospective employees, and the media. Consistent with this interpretation, Ohmachi and Mitsusada (2021) find that firms with initially low female employee shares experienced the largest improvements following the Act's implementation, suggesting that reputational pressure translated into tangible organizational adjustment. Internally, the Act's requirement that firms develop and communicate specific action plans made organizational commitment to gender equality visible within the firm. This internal dimension is important: whereas external disclosure operates through market and reputational signals, the mandatory circulation of action plans to employees directly altered the information environment inside affected firms. Non-managerial male workers were thereby exposed to formal evidence that the criteria governing career advancement were being revised, regardless of whether actual promotion decisions had yet changed.

**Channel 2: Forward-looking expectation revision**

An insight from the economics of happiness literature is that subjective well-being responds not only to current circumstances but also to perceived future prospects (Clark et al. 2018; Dolan et al. 2008; Jahoda 1982). Once workers observed organizational responses of the kind described above, they had reason to revise their beliefs about career advancement within their firms. Male employees may have updated their expectations regarding the likelihood of future promotion, the timing of advancement, or the degree of competition for a fixed number of managerial positions. Crucially, such forward-looking revisions can reduce job satisfaction even before any observable change in actual promotion outcomes occurs. This mechanism parallels findings in the job insecurity literature, which document that the anticipation of adverse labor market outcomes — rather than their realization —

exerts strong negative effects on well-being (Francis 2009; Green 2009; Knabe and Rätzel 2010).

**Channel 3: Perceived erosion of procedural fairness**

The expectation revision described in Channel 2 need not operate solely through a calculus of reduced promotion probabilities. A complementary pathway runs through employees' evaluations of the fairness of the promotion process itself. In the organizational psychology literature, perceived procedural justice — the perceived fairness of the rules and procedures used to make allocation decisions — is a key determinant of job satisfaction, independent of the actual outcomes received (García-Izquierdo et al. 2012; Greenberg 1990; Lambert et al. 2010). When male employees observed that firms were publicly committing to gender-based targets and revising promotion criteria, some may have interpreted these changes as weakening meritocratic principles, regardless of whether this perception accurately reflected actual policy implementation. Such perceptions of procedural unfairness can reduce job satisfaction through a channel entirely separate from the revision of expected promotion probabilities.

Taken together, these three channels suggest that the magnitude of the psychological effect documented in this study need not be proportional to the formal stringency of the regulation. Rather, the Act functioned as a highly visible institutional signal that the implicit norms governing career advancement in Japanese firms were evolving. In this interpretation, the decline in job satisfaction among non-managerial male workers reflects not necessarily realized career losses, but a subjective reassessment of future career prospects and promotion fairness in response to perceived changes in organizational norms. This distinction is important for evaluating the broader welfare implications of gender-equality policies.

### 2.3 Institutional Evidence on Policy Implementation

The Act on Promotion of Women's Participation and Advancement in the Workplace required firms with 301 or more employees to formulate and publicly disclose action plans for promoting women's advancement, while smaller firms were encouraged but not legally required to undertake these measures. Consequently, the statutory threshold generated substantial differences in firms' incentives to implement organizational changes related to women's career advancement.

Figure 1 presents aggregate evidence on changes in female managerial representation using the Basic Survey on Equal Employment Opportunity conducted by the Ministry of Health, Labour and Welfare. The figure plots changes in the share of female managers relative to the 2015 level separately for firms with 300 or more employees and firms with fewer than 300 employees, together with the difference between the two groups. Following the implementation of the Act in 2016, the increase in female managerial representation became consistently larger among covered firms. By 2019, for

example, the increase relative to the 2015 baseline reached 3.2 percentage points among firms with 300 or more employees, compared with −0.1 percentage points among firms below the statutory threshold, yielding a difference of 3.3 percentage points. Positive differences remained evident through 2021. These aggregate statistics are consistent with the view that firms covered by the legislation experienced greater progress in women's managerial advancement following its implementation.

To examine whether these aggregate patterns were accompanied by concrete organizational responses at the firm level, we next use employer-side microdata from the Survey on Women's Active Participation and Work-Life Balance, conducted by the Japan Institute for Labour Policy and Training (JILPT) in 2018. The survey was specifically designed to assess firms' responses to the Act and contains detailed information on policy implementation, organizational practices, and changes in women's career advancement. The sample comprised companies nationwide with 30 or more regular employees. These companies were stratified into three size categories: 30–99, 100–299, and 300 or more employees. From each category, 5,000 companies were sampled so that their distribution matched the proportions of the 16 major industry groups based on the Japan Standard Industrial Classification. Following a mail survey, the response rates for each category were 15.7% (30–99 employees), 19.8% (100–299 employees), and 25.6% (300 or more employees), yielding a total of 3,054 valid responses. The survey was conducted over a one-month period in January 2018.

Using this data, we examine whether firms covered by the statutory threshold exhibited stronger organizational responses than uncovered firms. Specifically, we estimate probit models and regression controlling for firm establishment year, industry, and the average age and tenure of male and female employees. Table 1 summarizes the results[2]. The results indicate substantial differences in policy implementation across firm-size groups. Compared with firms employing fewer than 300 workers, covered firms were 47.5 percentage points more likely to formulate a general employer action plan under the Act (column (1)), 18.6 percentage points more likely to conduct assessments of women's promotion (including female managerial representation) (column (2)), and 14.4 percentage points more likely to establish numerical targets related to women's promotion in their action plans (column (3)). They also publicly disclosed significantly more policy-related indicators concerning women's advancement (column (4)). These organizational responses were accompanied by observable changes in women's managerial advancement. Relative to 2014, firms with 300 or more employees were

[2] The dependent variables in Table 1 are defined as follows: the general employer action plan in column (1) is a dummy variable equal to 1 if the firm formulated an action plan under the Act, and 0 otherwise; assessment of female promotion in column (2) equals 1 if the firm assessed women's promotion prior to the Act's implementation, and 0 otherwise; and numerical targets for female promotion in column (3) is 1 if the firm established numerical targets in its action plan, and 0 otherwise. Column (4), disclosure items, indicates the number of female advancement indicators publicly disclosed by the firm. "Increase in female section managers" in column (5) is 1 if the number of female section-level managers increased between 2014 and 2018, and 0 otherwise. Finally, "increase in female assistant-manager promotions" in column (6) equals 1 if promotions to assistant-manager-level positions increased between 2014 and 2018, and 0 otherwise.

significantly more likely to report increases in the number of female section-manager-level employees and increases in promotions to such positions by 2018 (column (5) and (6)).

Taken together, the aggregate evidence in Figure 1 and the employer-level evidence in Table 1 suggest that firms above the statutory threshold not only experienced larger increases in female managerial representation but also implemented substantially more of the organizational practices explicitly required or encouraged by the Act, including action-plan formulation, numerical target setting, information disclosure, and initiatives related to women's promotion. Although these descriptive analyses do not establish the causal effect of the legislation, they indicate that statutory eligibility corresponded to meaningful differences in actual policy exposure rather than merely differences in firm size. This evidence alleviates the concern that the treatment indicator simply captures firm size itself and provides institutional support for the interpretation of the treatment variable employed in the subsequent difference-in-differences analysis.

## 3. Data

The data used in this study come from the JHPS-CPS, a nationally representative panel survey conducted by Osaka University. The survey began in 2003 and covers men and women aged 20 to 69 across Japan. Data are collected using a self-administered questionnaire distributed and retrieved through a drop-off and pick-up method. The initial sample in 2003 consisted of 1,418 respondents. To maintain sample size and representativeness, additional cohorts were added in subsequent waves: 3,161 respondents in 2004, 1,396 in 2006, and 3,704 in 2009. The available survey waves cover the years 2003–2013, 2016–2018, and 2021. The survey collects detailed information on individuals and households, including demographic characteristics, employment status, income, and consumption. Importantly for this study, it also includes a measure of job satisfaction, which serves as the main dependent variable in our analysis.

Our analysis focuses on male employees who are regularly employed in non-managerial positions at private-sector firms in each survey wave included in the analysis[3]. Although public sector employees are also subject to the Act, they were excluded from our primary sample due to differences in promotion mechanisms between the public and private sectors. Nonetheless, we include the public sector sample in our robustness checks in Section 5.6.

To ensure consistent identification of the policy effect, we further exclude individuals whose firm size changes during the observation period, as such changes may confound the measurement of the effect of the Act[4]. We also restrict the sample to individuals aged 59 or younger, because employment

[3] If a sample was promoted to manager during the analysis period, it was excluded from that point onward. There were 95 such manager promotions in our data. An additional analysis including the post-promotion period produced largely the same results as Table 3.

[4] As a robustness check, we re-estimated our models including job changers during the analysis period and extending the sample to incorporate 2021 data; the results remained qualitatively unchanged. Details are provided in Section 5.2. and 5.6.

arrangements after the statutory retirement age in Japan often differ substantially from standard regular employment relationships. In addition, although the dataset includes a survey wave in 2021, we limit the analysis period to 2018 in order to avoid potential confounding effects associated with the COVID-19 pandemic. After applying these restrictions and dropping observations with missing values for the variables used in the analysis, the final sample consists of 1,357 observations[5].

A measurement issue related to the firm-size threshold warrants explicit acknowledgment. The Act imposes mandatory obligations on firms with 301 or more employees, yet the JHPS-CPS records firm size in the following categorical brackets: 1–5, 6–29, 30–99, 100–299, 300–499, 500–999, 1,000–4,999, and 5,000 or more employees. We construct the treatment indicator by assigning a value of 1 to respondents employed in firms with 300–499 or more employees and 0 to those in smaller categories, following the closest available approximation to the statutory threshold. This categorical structure introduces a narrow but acknowledged boundary misalignment: firms with exactly 300 employees fall within the treatment category in our data, yet are not formally subject to the Act's mandatory requirements. On the control side, no such misalignment arises, as the adjacent category (100–299 employees) contains only firms strictly below the legal threshold. To the extent that workers in 300-employee firms behave more similarly to those in sub-threshold firms than to those in clearly covered firms, their inclusion in the treatment group would attenuate the estimated treatment effect toward zero. Our estimates should therefore be interpreted as a conservative lower bound on the true impact of the Act.

Job satisfaction, the main dependent variable in our analysis, is constructed from a survey question that asks respondents to evaluate their satisfaction with several aspects of their lives. One of these items concerns satisfaction with work. Responses are recorded on a five-point scale ranging from 1 (“very satisfied with work”) to 5 (“very dissatisfied with work”). For ease of interpretation, we reverse the scale so that higher values indicate higher levels of job satisfaction. Information on job satisfaction is available in the survey waves for 2012, 2013, 2016, 2017, 2018, and 2021. Because the analysis excludes the 2021 wave to avoid potential confounding effects related to the COVID-19 pandemic, the empirical analysis uses observations from the years 2012, 2013, 2016, 2017, and 2018.

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[5] To assess whether our sample provides adequate statistical power, we calculate the minimum detectable effect (MDE) implied by our sample composition. Using the within-person standard deviation of job satisfaction ($\sigma_w = 0.528$), a two-sided significance level of 5%, and the observed sample sizes of 789 control observations and 568 treated observations (N = 1,357 in total), the MDE at 80% statistical power is 0.081 points on the five-point job satisfaction scale, corresponding to a standardized effect size of 0.154 within-person standard deviations. This falls below the conventional threshold for a "small" effect (Cohen's d = 0.2), indicating that the full sample is adequately powered to detect effects of policy-relevant magnitude.

## 4. Empirical strategy

To examine whether exposure to the Act is associated with job satisfaction among male regular employees in non-managerial positions, we estimate the following individual fixed-effects model:

$$JS_{it} = \beta_0 + \beta_1 FS_{it} + \beta_2 Y2016_{it} + \beta_3 (FS_{it} \times Y2016_{it}) + \beta_4 X_{it} + \mu_i + \varepsilon_{it} \quad (1)$$

where $JS_{it}$ denotes the job satisfaction of individual *i* in survey year *t*. $FS_{it}$ is a treatment indicator that equals 1 if the respondent works at a firm with 300 or more employees and 0 otherwise. This classification provides the closest available approximation to the statutory threshold of 301 or more employees established by the Act. This treatment definition is supported by the institutional evidence presented in Section 2, which shows that firms above the statutory threshold experienced substantially greater organizational responses to the Act than firms below the statutory threshold. These findings suggest that the statutory threshold captures meaningful differences in policy exposure rather than merely differences in firm size. $Y2016_{it}$ is a post-policy dummy that equals 1 for survey years after the implementation of the Act in 2016 and 0 otherwise. The interaction term $FS_{it} \times Y2016_{it}$ captures the differential post-policy change in job satisfaction associated with employment in firms covered by the Act. The coefficient on this interaction term, $\beta_3$, represents the difference-in-differences (DID) estimate of the association between statutory policy exposure and job satisfaction. Under the identifying assumption that, absent the Act, job satisfaction would have followed parallel trends between workers employed in firms covered by the Act and those employed in firms not covered by the Act, $\beta_3$ captures the differential post-policy change in job satisfaction associated with exposure to the Act[6]. If exposure to the Act leads male non-managerial employees to perceive reduced promotion opportunities or delays in promotion timing, it may lower job satisfaction by increasing dissatisfaction or perceptions of unfairness. Moreover, if exposure to the Act increases uncertainty regarding future earnings or induces a sense of career stagnation, it may impose additional psychological burdens on male employees in non-managerial positions. Under the proposed interpretation, the coefficient on the interaction term, $\beta_3$, is expected to be negative. To examine this possibility, we estimate equation (1) using a FE-OLS model[7].

$X_{it}$ is a vector of time-varying individual characteristics, including age dummies (20s, 30s, 40s, and 50s), marital status, the number of children, log earnings, weekly working hours, occupation, and industry. The term $\mu_i$ denotes individual fixed effects, while $\varepsilon_{it}$ represents the idiosyncratic error

[6] The parallel trends assumption is examined at the robustness check in Section 5.2.

[7] Recent methodological contributions have shown that the two-way fixed-effects (TWFE) estimator can produce biased estimates when treatment is staggered across units over time (Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021). In the present setting, however, this concern does not apply. The Act came into force simultaneously for all firms above the 300-employee threshold in April 2016, with no variation in treatment timing across units. The decomposition problems identified in the staggered-adoption literature therefore do not arise, and the standard TWFE estimator remains appropriate for the present research design.

term. The basic statistics of the variables used in this study are presented in Table 2.

## 5. Estimation results

### 5.1 Basic results

Table 3 reports the relationship between the implementation of the Act on Promotion of Women's Participation and Advancement in the Workplace and job satisfaction among non-managerial male regular employees, estimated using a fixed-effects OLS specification. Column (1) presents the baseline results without individual-level control variables, while column (2) additionally includes a set of individual characteristics.

The coefficient of primary interest is the interaction term between the indicator for firms subject to the Act and the post-2016 dummy. Across both specifications, the estimated coefficient on this interaction term is negative and statistically significant. This result suggests that, following the implementation of the Act, job satisfaction was lower among non-managerial male employees working in firms covered by the policy than among those employed in firms not subject to the Act[8].

One possible interpretation is that organizational changes associated with the Act may have altered employees' perceptions of future promotion opportunities within affected firms. If the Act led to

[8] A potential concern with our threshold-based identification strategy is that firms may have strategically reduced their workforce to below 300 employees in anticipation of the Act's compliance requirements. If such manipulation occurred, the composition of firms just above and below the threshold would shift systematically following the Act's announcement or implementation, potentially biasing our difference-in-differences estimates. Because firm size is measured categorically in the JHPS-CPS, we assess this possibility by testing whether the distribution of workers across firm-size categories changed differentially around the 300-employee threshold following the Act's implementation. We compare the share of observations in each firm-size category between the pre-policy period (2012–2013) and the post-policy period (2016–2018) using chi-square tests of independence. The results provide no evidence of threshold manipulation. A chi-square test across all eight firm-size categories yields $\chi^2(7) = 4.605$ ($p = 0.708$), indicating that the overall distribution of workers across firm-size categories did not change significantly between the pre- and post-policy periods. Restricting the test to the two categories straddling the 300-employee threshold — firms with 100–299 employees (below the threshold) and firms with 300–499 employees (above the threshold) — yields $\chi^2(1) = 0.138$ ($p = 0.711$). A Fisher's exact test for the same comparison produces an identical conclusion ($p = 0.714$). The proportion of workers employed in firms just below the threshold (100–299 employees) was 60.5 percent in the pre-policy period and 39.5 percent in the post-policy period, while the corresponding figures for firms just above the threshold (300–499 employees) were 58.3 percent and 41.7 percent — a difference of only 2.2 percentage points that is far from statistically significant. Taken together, these results provide no evidence that firms strategically manipulated their workforce size to avoid crossing the 300-employee threshold in response to the Act, supporting the validity of our threshold-based identification strategy.

increased attention to gender diversity in managerial positions, male employees may have perceived a delay in promotion timing, a reduction in available promotion slots, or greater uncertainty regarding future career progression and income growth. Such changes in perceived career prospects could reduce job satisfaction among non-managerial male employees.

To assess the economic significance of our estimates, we express the effect in standardized units. Using the within-person standard deviation of job satisfaction ($\sigma_w = 0.528$; footnote 5), the estimated decline of −0.301 points corresponds to a standardized effect size of approximately −0.570 within-person standard deviations — a magnitude conventionally characterized as medium to large (Cohen 1988). This suggests that the estimated effect is economically meaningful. As an additional point of reference, Clark et al. (2008), one of the most widely cited studies of unemployment and subjective well-being, document that unemployment is associated with a substantial reduction in life satisfaction (0.87–0.92 points on a 0–10 scale). While a linear rescaling to a five-point range yields an approximate reduction of 0.35–0.37 points, such comparisons should be interpreted with considerable caution, as life satisfaction and job satisfaction capture distinct dimensions of well-being and may not be directly comparable in cardinal terms. We therefore view this comparison as providing only a rough benchmark[9]. Under this interpretation, our estimate appears non-trivial in magnitude, but we refrain from drawing direct quantitative equivalence across outcomes.

Taken together, these findings suggest that policies aimed at increasing gender diversity in leadership may unintentionally generate concerns about career prospects among male employees, potentially reducing their job satisfaction. This pattern is consistent with findings from studies of gender quota policies, which show that men tend to view such measures less favorably when they perceive a reduction in promotion opportunities or intensified competition for leadership positions (Harrison et al. 2006; Kane and Whipkey 2009; Martins and Parsons 2007; Olsen et al. 2016; Kim and Kweon 2022).

### 5.2 Identification and Validity

A central concern in our empirical design is whether the estimated relationship between the Act and job satisfaction reflects differential policy exposure rather than other differences between firms above and below the statutory threshold. In particular, the difference-in-differences approach may be affected by violations of the parallel trends assumption, the potential endogeneity of firm size, and sample selection. To assess the robustness of our interpretation, we implement a set of complementary validation exercises, including tests of pre-treatment trends, restrictions to firms near the policy threshold, propensity score matching (PSM) to improve comparability in observable characteristics,

[9] While several key studies examine the determinants of job satisfaction (Clark 1996; Clark and Oswald 1996; Clark et al. 1996), their results are difficult to compare with the present findings because they utilize ordered logit or probit models.

and analyses that incorporate job changers.

First, we assess the plausibility of the parallel trends assumption using an event-study specification. The key identifying assumption of the DID estimator is that, in the absence of the Act, job satisfaction among employees in firms covered by the Act and those in firms not covered by the Act would have followed similar trends over time. Although this assumption cannot be tested directly, we evaluate its plausibility by estimating a specification that replaces the single post-2016 indicator with a full set of interactions between year dummies and an indicator for firms covered by the Act, spanning the period from 2012 to 2018[10]. Under the parallel trends assumption, the coefficients on the interaction terms for the pre-policy years should be statistically indistinguishable from zero, whereas the coefficients for the post-policy years capture the policy's effects. The estimates are reported in column (1) of Table 4 and displayed graphically in Figure 2. The coefficient on the interaction term for 2012 is statistically insignificant and close to zero, suggesting no systematic difference in job satisfaction between employees in firms covered by the Act and those in firms not covered by the Act prior to the policy. By contrast, the interaction terms from 2016 onward are negative and statistically significant, and Figure 2 shows that this downward shift is abrupt at the time of the Act's implementation and persists through the post-policy period. Taken together, these results provide supportive evidence for the parallel trends assumption and are consistent with a negative effect of the policy on male employees' job satisfaction[11].

Second, we examine whether the baseline results are sensitive to the inclusion of firms far from the policy threshold. One potential concern with the difference-in-differences design is that the treatment group, defined as firms with 300 or more employees, includes firms substantially larger than the threshold—up to 4,999 or more employees. These very large firms may differ systematically from firms near the cutoff in ways unrelated to the Act. For example, they may adopt different managerial strategies, or operate in more competitive product markets, any of which could independently influence employees' job satisfaction after 2016. To address this concern, we re-estimate the baseline

[10] The reference group of the year dummy is 2013.

[11] We acknowledge that the pre-treatment period available in our data is limited to the 2012 and 2013 survey waves because job satisfaction was not collected in earlier waves of the JHPS-CPS. Consequently, the event-study specification provides only a limited assessment of the parallel trends assumption. Nevertheless, two considerations lend support to its plausibility. First, we conduct a placebo-in-time test using only the pre-policy period (2012–2013), treating 2013 as a fictitious policy year. The interaction coefficient is small and statistically insignificant, providing no evidence of differential pre-policy trends between the treatment and comparison groups (results not reported). Second, because treatment status is determined by the Act's statutory applicability threshold rather than firms' own choices, assignment to the treatment group is unlikely to reflect workers' pre-existing job satisfaction. While these tests cannot establish the parallel trends assumption definitively, they provide indirect evidence consistent with its plausibility.

model using a restricted sample that includes only firms in the two size categories surrounding the policy threshold: firms with 100–299 employees (control group) and firms with 300–499 employees (treatment group)[12][13]. This restriction limits the comparison to firms located close to the threshold and reduces the influence of characteristics specific to very large firms. The results, reported in column 2 of Table 4, show that the interaction term between the firm-size indicator and the post-2016 dummy remains negative and statistically significant at the five percent level, with a coefficient of −0.573. This finding suggests that the baseline result is not driven by characteristics unique to very large firms and remains robust when the analysis is restricted to firms near the policy threshold[14].

Third, we enhance pre-treatment comparability between the treatment and control groups using PSM (Heckman et al., 1997). A potential concern with our baseline approach is the lack of comparability between workers in covered and uncovered firms prior to the treatment. To address this, we apply PSM to the 2013 baseline wave to construct a matched comparison group that closely resembles the treatment group in observable characteristics, and then re-estimate the DID model using this matched sample. Matching is implemented via both kernel and radius matching[15]; as reported in columns (3) and (4) of Table 4, the two approaches yield qualitatively identical results. This combined matching-and-DID strategy improves pre-treatment balance on observable characteristics while simultaneously controlling for unobserved time-invariant heterogeneity through individual fixed effects, thereby providing a more conservative estimate of the policy-induced change in job satisfaction.

Fourth, we examine whether the exclusion of job changers introduces sample selection bias. In the

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[12] An alternative identification strategy would be a regression discontinuity design (RDD) exploiting the statutory threshold at 301 employees. In principle, an RDD would strengthen causal identification by comparing firms just above and just below the threshold. However, this approach is not feasible with the JHPS-CPS data because firm size is reported in categorical brackets rather than as a continuous measure. The survey records firm size in eight discrete categories (e.g., 100–299 and 300–499 employees), which prevents the construction of a continuous running variable required for local polynomial estimation and also precludes standard density tests for manipulation around the threshold (McCrary 2008). In the absence of a continuous running variable, a fuzzy RDD is likewise infeasible, as it would require estimating policy exposure as a smooth function of distance from the threshold.

[13] As an additional assessment of this concern, Section 5.6 estimates a specification using more detailed firm-size categories (1–29, 30–299, 300–999, and 1,000 or more employees), rather than a single indicator for firms above the statutory threshold. If the baseline estimates were driven primarily by characteristics unique to very large firms, the negative association would be expected to be concentrated among firms with 1,000 or more employees. Instead, the estimated results show that negative coefficients are observed for both the 300–999 and the 1,000-or-more employee categories, and the estimated magnitudes are similar. This pattern suggests that the baseline findings are unlikely to be driven solely by characteristics unique to the largest firms.

[14] As a further check, we re-estimate the event-study specification. The result is presented in Appendix 1. The coefficients on the interaction terms for 2016, 2017, and 2018 remain negative and statistically significant, while the pre-treatment coefficient for 2012 is statistically indistinguishable from zero. This pattern is consistent with the full-sample results in Table 4, and provides additional support for the parallel trends assumption within the subsample of firms located in close proximity to the policy threshold.

[15] The individual attributes listed in Table 3 are included as covariates in the PSM procedure. For kernel matching, we employ the Epanechnikov kernel with a bandwidth of 0.06, while for radius matching, the caliper is set at 0.01. The table in the Appendix 2 presents the covariate balance statistics before and after PSM using the kernel matching method. Following the matching, all mean differences across covariates became statistically insignificant, indicating that a satisfactory balance was achieved. These results are robust to alternative specifications, as similar balance was obtained using radius matching.

baseline specification, we restrict the sample to workers who remain with the same employer throughout the observation period, because a change in employer size makes the treatment status ambiguous. However, this restriction could introduce sample selection bias if workers who anticipated adverse career consequences under the Act were more likely to voluntarily leave large firms. To assess the importance of this concern, we re-estimate the baseline model using a sample that includes job changers. The estimates reported in column (5) of Table 4 are consistent with those in Table 3, indicating that the inclusion of job changers does not materially alter the results.

### 5.3 Statistical Inference and Functional Form

We assess the robustness of our findings to alternative inference and model specifications. In particular, we examine sensitivity to standard error clustering and inference procedures suitable for a small number of clusters. We also estimate a fixed-effects ordered logit model to account for the ordinal nature of the dependent variable. These results confirm that our conclusions are not driven by specific modeling or inference choices.

First, we assess the sensitivity of statistical inference to alternative assumptions about the error structure. In the baseline specification, we report heteroskedasticity-robust standard errors clustered at the individual level. Although individual-level clustering accounts for serial correlation within persons, it may not capture common shocks shared by workers employed in firms of similar size or operating in the same industry, given that our treatment variable varies primarily at the firm-size category level. To address this concern, we adopt three alternative clustering strategies, the results of which are reported in columns (1)–(3) of Table 5. First, we cluster standard errors at the firm-size category level (eight clusters). The point estimate remains consistent with that in Table 3, and we implement wild cluster bootstrap inference to account for the small number of clusters. Following the recommendation of Webb (2023), we employ Webb weights with 9,999 replications, yielding a t-statistic of −3.896 ($p = 0.009$), which confirms statistical significance at the one percent level. As a supplementary check, Rademacher weights produce a p-value of 0.008 and a 95 percent confidence interval of [−0.531, −0.115]. Second, recognizing that workers within the same industry may be exposed to common shocks, we cluster at the industry-by-firm-size-category level, which yields 78 clusters[16]. The estimated effect remains statistically significant at the five percent level even after allowing for within-industry-and-size-group error correlation (column (2)). Third, as a further check that captures time-varying industry-level shocks, we cluster at the industry-by-year level (56 clusters), and the coefficient remains significant at the one percent level (column (3)).

Second, we verify that our findings are robust to alternative estimation methods. Specifically, we

[16] While crossing industry (12 categories) with firm size (8 categories) can produce 96 categories, some were excluded due to an insufficient sample size. Industry (12 categories) by year (5 categories) yields 60 categories, though some were omitted for the same reason.

address the concern that our dependent variable is measured on an ordinal scale, which the linear fixed-effects OLS estimator may not fully account for. To exploit the discrete and ordered nature of the outcome, we re-estimate the baseline model using the fixed-effects ordered logit estimator proposed by Baetschmann et al. (2020). The results, reported in column (4) of Table 5, are qualitatively consistent with those from the baseline OLS specification[17][18].

### 5.4 Additional Outcomes

We examine additional outcomes to assess the scope of the estimated effects. We re-estimate the baseline model using self-rated health and self-reported stress, each measured on a five-point scale, as alternative outcome variables[19]. Columns (1) and (2) of Table 6 report the results. Following the implementation of the Act, non-managerial male employees report significantly poorer health and higher stress. These findings suggest that the estimated association is not limited to job satisfaction but extends to other dimensions of work-related well-being.

### 5.5 Indirect Evidence on Potential Mechanisms

To provide suggestive evidence on potential interpretations of the estimated effects, we examine heterogeneity by career stage, managerial status and preferences toward competition, which may shape how workers respond to organizational changes following the Act.

The framework developed in Section 2.2—organizational response, expectation revision, and perceived changes in workplace evaluation processes—motivates the hypothesis that the estimated association may differ across workers depending on how relevant future career opportunities are to them. Although the available data do not allow these channels to be tested directly, they provide a basis for examining whether the estimated effects vary across groups whose career stages and preferences imply different exposure to organizational changes following the Act. Under the proposed interpretation, the structure of Japan's seniority-based internal labor market suggests three patterns that would be broadly consistent with the underlying mechanisms. First, workers in their 30s and 40s are realistic candidates for managerial advancement but have not yet secured such positions[20], and may therefore respond more strongly to organizational changes related to career progression than workers at earlier or later career stages. By contrast, workers in their 20s have generally not yet entered the most competitive stage of promotion, while workers in their 50s face less uncertainty regarding future

[17] In line with our results, Ferrer-i-Carbonell and Frijters (2004) show that the choice between ordinal and cardinal assumptions in OLS and ordered logit frameworks does not substantially alter the findings.
[18] The estimated result in column (4) of Table 5 shows the coefficient.
[19] Subjective-rated health is assessed using the question, 'I feel anxious about my health,' while stress levels are measured by the statement, 'I have been feeling stressed lately.' Responses are recorded on a scale from 1 (Strongly agree) to 5 (Strongly disagree), such that higher scores reflect better health conditions and lower stress.
[20] According to the Basic Survey on Wage Structure, the average age of managers fluctuated between 47.8 and 49.3 years from 2016 to 2024.

advancement and are thus expected to be less responsive. Second, workers with longer tenure have accumulated more firm-specific human capital and may therefore respond more strongly to organizational changes that alter expectations regarding career progression than workers with shorter tenure. Third, workers who have already attained managerial positions may be less responsive to such organizational changes because much of the uncertainty surrounding future promotion has already been resolved, although they may still be affected through other organizational channels.

The results in Table 7 are broadly consistent with the proposed interpretation. Among workers in their 30s and 40s, the estimated coefficient on the interaction term is −0.387 and statistically significant at the five percent level, whereas the corresponding estimate among workers in their 20s and 50s is statistically indistinguishable from zero (columns (1) and (2)). The tenure-based results show a similar pattern: workers with ten or more years of tenure exhibit a negative and significant coefficient of −0.303, whereas no significant effect is observed among those with fewer than ten years of tenure (columns (3) and (4)). Furthermore, we examine heterogeneity by managerial status. The coefficient for managerial workers is smaller and statistically insignificant (column (5))[21]. Although managerial employees may also experience organizational changes following the Act, this pattern is consistent with the possibility that workers whose own promotion uncertainty has largely been resolved respond differently from non-managerial employees.

Next, we examine heterogeneity by workers' competitive orientation to explore whether the estimated effects differ across groups that may place different importance on career advancement and workplace competition. We exploit an attitudinal measure from the JHPS-CPS: respondents' agreement with the statement "I enjoy competing with others," measured on a five-point scale from 1 ("strongly agree") to 5 ("strongly disagree"). We use responses from the 2012 survey wave—four years prior to the Act's implementation—to ensure that this measure is predetermined with respect to the policy. Respondents who answered 1 or 2 are classified as competition-oriented, while those who answered 3–5 are classified as less competition-oriented. If organizational changes following the Act influenced workers partly through perceived career opportunities or workplace evaluation processes, stronger responses may be observed among competition-oriented workers, who may place greater weight on future advancement opportunities. The results, reported in columns (6) and (7) of Table 7, are broadly consistent with this expectation. Among competition-oriented workers, the estimated coefficient on the treatment–post interaction is −0.723 and statistically significant at the five percent level, whereas among less competition-oriented workers the coefficient is negative but statistically indistinguishable from zero.

Taken together, these heterogeneous patterns are broadly consistent with the interpretation

[21] We additionally estimated a model including a triple interaction between treatment status, the post-policy indicator, and managerial status. The coefficient on the triple interaction was positive but statistically insignificant, indicating that the estimated post-policy association does not differ significantly between managerial and non-managerial male employees.

developed in Section 2.2. At the same time, because the JHPS-CPS does not contain direct measures of promotion expectations, perceived procedural fairness, or awareness of the Act, these analyses should be interpreted as indirect and suggestive evidence.

### 5.6 Alternative Explanations

While the results presented above align with the policy, alternative explanations unrelated to the Act may also account for the observed decline in job satisfaction. In particular, workplace characteristics, industry shocks, firm size differences, or concurrent policies could generate similar patterns. We address these concerns by examining these factors directly and by extending the analysis to include additional years of data, allowing us to assess whether the results are driven by period-specific shocks.

First, we examine whether these observed patterns reflect workplace characteristics rather than the Act itself. A potential concern is that the post-2016 decline in job satisfaction among employees in firms covered by the Act may be driven by features of workplace environments. We focus on two dimensions of workplace organization. First, if the observed decline were limited to firms that rely on performance-based pay systems, these patterns might coincide with intensified merit-based competition rather than the gender-equality provisions of the Act. To assess this possibility, we estimate separate specifications for firms with and without performance-based pay systems[22]. Second, if the decline were concentrated among workers in high-intensity work environments—where employees already face heavy workloads—this would suggest that work pressure rather than changes in the promotion environment explains the result. We therefore examine whether the observed patterns differ between high- and low-intensity work environments[23]. The estimates reported in Table 8 show that the decline in job satisfaction among male employees appears in both types of workplaces. Columns (1) and (2) indicate that the negative effect is present regardless of whether the firm adopts a performance-based pay system. Similarly, columns (3) and (4) show a comparable decline in both high- and low-intensity work environments. These results suggest that the estimated effect is unlikely to be driven solely by workplace characteristics.

Second, we control for potential industry-specific shocks after 2016 that may confound the baseline estimates. One concern is that firms covered by the Act may be disproportionately concentrated in particular industries that experienced negative shocks after 2016 for reasons unrelated to the policy. To address this possibility, we augment the baseline specification by including interaction terms

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[22] The extent of performance-based pay was assessed by the question, 'To what extent is your and your spouse's job pay structure performance-based?' Respondents chose from four options: (1) almost entirely, (2) heavily influenced, (3) slightly influenced, or (4) not at all influenced by performance. We categorized responses 1, 2, and 3 as 'performance-based pay,' while response 4 was defined as 'seniority-based pay.'

[23] Work intensity was assessed using the question: 'Considering your work pace per hour, how would you describe your current workload?' Respondents chose from five options: (1) working at maximum possible capacity, (2) working very hard, (3) working with no room for error, (4) working with some room for error, and (5) working with considerable room for error. We categorized responses 1, 2, and 3 as 'working at full capacity (no slack),' while responses 4 and 5 were defined as 'working with slack or flexibility.'

between industry dummy variables and a post-2016 indicator. This specification allows post-2016 trends to vary across industries and accounts for industry-specific shocks that may have occurred after the policy implementation. The estimates reported in column (5) of Table 8 are consistent with those in Table 3, suggesting that industry-specific shocks in the post-policy period are unlikely to drive the baseline results.

Third, we use a more detailed classification of firm size to examine whether the baseline results are sensitive to alternative firm-size classifications. The baseline specification classifies firms into those above and below the statutory threshold, but such a binary treatment indicator may mask important heterogeneity across the firm-size distribution. In particular, very small firms often differ from medium-sized firms in organizational structure, labor market conditions, and managerial practices, and these differences may independently affect employee well-being. Likewise, very large firms may also differ from other covered firms in organizational characteristics that are associated with employee well-being. To examine these possibilities, we replace the binary treatment indicator with four firm-size categories: 1–29, 30–299, 300–999, and 1,000 or more employees. Within the same specification, we also extend the sample to include public-sector employees. Public-sector organizations are also subject to the Act but operate under personnel and promotion systems that differ from those of private-sector firms. Including this group therefore provides an additional assessment of whether the estimated association is observed across different organizational settings covered by the Act. The estimates in column (6) of Table 8 show that job satisfaction among male employees in firms with 30–299 employees is statistically indistinguishable from that in firms with 1–29 employees. This finding suggests that the estimated post-2016 association is unlikely to be driven solely by unobserved factors specific to very small firms. Negative coefficients are also estimated for firms with 300–999 employees ($-0.338$, $p < 0.10$), firms with 1,000 or more employees ($-0.380$, $p < 0.05$), and public-sector organizations ($-0.453$, $p < 0.05$). Thus, the negative association extends across multiple organizational categories covered by the Act rather than being confined to a specific firm size. Moreover, the similarity in the estimated coefficients for firms with 300–999 and 1,000 or more employees suggests that the baseline estimates are not driven solely by characteristics unique to the largest firms. Finally, the significantly negative coefficient for public-sector organizations indicates that the estimated association is not confined to the private sector. Although public-sector organizations operate under different personnel and promotion systems, they are also subject to the Act's disclosure requirements. This pattern is consistent with the interpretation that the estimated association is observed across diverse organizational settings covered by the Act.

Fourth, we examine whether the exclusion of the 2021 survey wave affects the baseline results. In the main analysis, we exclude observations from 2021 because the COVID-19 pandemic substantially disrupted labor market conditions and subjective well-being, raising the possibility that pandemic-related shocks could confound the estimated policy effects. However, excluding this wave also

prevents us from assessing whether the effect of the Act persists over a longer time horizon. To address this concern, we re-estimate the baseline model including the 2021 wave. The estimates reported in column (7) of Table 8 are consistent with those in Table 3, indicating that the negative effect on male employees' job satisfaction remains statistically significant when the 2021 observations are included.

Finally, we examine whether the estimated effects may reflect other institutional developments around 2016 rather than the Act itself. Several concurrent changes in Japan's regulatory environment warrant consideration. The Corporate Governance Code, introduced by the Tokyo Stock Exchange and the Financial Services Agency in June 2015, encouraged listed firms to strengthen board independence and improve disclosure practices, including on gender diversity at the board level. The Work Style Reform legislation, enacted in 2018 and implemented from 2019 onward, introduced legally binding limits on overtime hours and other changes to working-time regulations aimed at reducing long working hours across the workforce. The revised Stewardship Code of May 2017 strengthened expectations on institutional investors to engage with investee companies on ESG issues, including gender diversity. The amendment of the Childcare and Family Care Leave Act, which came into force in January 2017, introduced new obligations on firms regarding childcare leave and flexible working arrangements to facilitate work–family balance.

Three considerations suggest that these alternatives are unlikely to account for our findings. First, the timing of the estimated effect does not align with these concurrent reforms. Our event-study estimates show an abrupt decline in job satisfaction in 2016, which predates the enforcement of Work Style Reform (2019), the Childcare and Family Care Leave Act amendment (January 2017), and the Stewardship Code revision (May 2017).

Second, the direction of the estimated effect is difficult to reconcile with policies aimed at improving working conditions or work–family balance. Work Style Reform, childcare leave expansion, and ESG-related governance reforms would be expected to improve, rather than reduce, job satisfaction. The decline we document among male employees runs counter to this prediction and is instead consistent with the promotion-channel proposed in Section 2.2.

Third, the pattern of heterogeneity in our results is inconsistent with a general large-firm deterioration driven by ESG pressure or governance reform. If broad organizational changes were responsible, we would expect the negative effect to be shared across all employee groups in large firms. Instead, the decline is concentrated among non-managerial workers (Table 3), workers in their 30s and 40s (Table 7), and competition-oriented workers (Table 7)—precisely the groups most directly affected by changes in the promotion environment under the Act. Male managerial employees, who are equally employed by large firms and thus equally exposed to any firm-wide governance change, show no significant effect (Table 7, column 5). Furthermore, the public-sector extension in column (6) of Table 8 shows a comparable decline among public-sector employees, who are covered by the Act but not subject to the Corporate Governance Code or investor ESG pressure—a pattern difficult to reconcile

with a governance-reform explanation.

While we cannot fully rule out all contemporaneous institutional influences, the convergence of evidence from event-study timing and heterogeneity analyses consistently points to the Act as the most plausible driver of the estimated effect.

## 6 Conclusion

Japan's 2016 Act on Promotion of Women's Participation and Advancement in the Workplace aims to expand women's career opportunities and leadership representation. While beneficial for women, such policies may alter internal promotion dynamics and relate to male employees' well-being. Using individual-level panel data from Japan, this study examines the association between the Act and the job satisfaction of non-managerial male employees. Exploiting the policy's applicability to firms with 301 or more employees, we evaluate this relationship using a difference-in-differences framework with individual fixed effects. The results show that post-implementation job satisfaction declined among non-managerial male employees in covered firms, a finding robust across alternative specifications. Further analyses reveal a corresponding deterioration in self-assessed health and increased stress. These findings indicate that gender equality policies have broader workplace well-being implications, consistent with the possibility that policy-driven organizational changes influence employees' career perceptions.

This study makes two main contributions to the literature. First, it provides quantitative evidence on a previously overlooked dimension of gender equality policies by examining their unintended well-being consequences. While policies promoting women's advancement are essential for reducing labor market inequality, their potential spillover effects on other workers within organizations have received little attention. By focusing on job satisfaction, this study clarifies the broader impacts of policies designed to expand women's career opportunities. Second, the findings contribute to the international debate on gender quotas. Many Western countries have introduced quotas to increase women's leadership representation. Although previous research shows that men often express lower support for such policies, empirical evidence regarding their association with men's well-being remains limited. Our results suggest that initiatives increasing women's representation are linked to lower well-being among male employees who perceive a decline in their own promotion opportunities. Thus, while gender equality initiatives are crucial, their broader implications for workplace well-being merit serious consideration across different institutional contexts.

These findings should not be interpreted as a case against gender equality policies. The decline in male job satisfaction represents an unintended consequence of a policy whose core objective—reducing gender inequality—remains legitimate and socially valuable. A comprehensive welfare assessment must weigh these costs against the broader benefits to women and society; the aggregate effect may well be positive despite transitional adjustment costs for some male workers. Nevertheless,

our findings suggest that complementary organizational measures could mitigate these costs while preserving the Act's objectives. In particular, firms could introduce transparent promotion criteria to clarify advancement decisions, and invest in leadership development programs that expand the overall pool of managerial opportunities rather than simply redistributing fixed positions. Such measures would address the documented adjustment costs without compromising gender equity.

Finally, several limitations should be noted. Although the institutional evidence in Section 2 suggests that covered firms implemented substantially more organizational initiatives, these employer-level data cannot be linked to our individual-level panel data. Consequently, this analysis cannot directly examine the specific organizational channels through which the Act influenced employee well-being. Future research combining employer- and employee-level data would provide a valuable opportunity to investigate these underlying mechanisms in greater detail.

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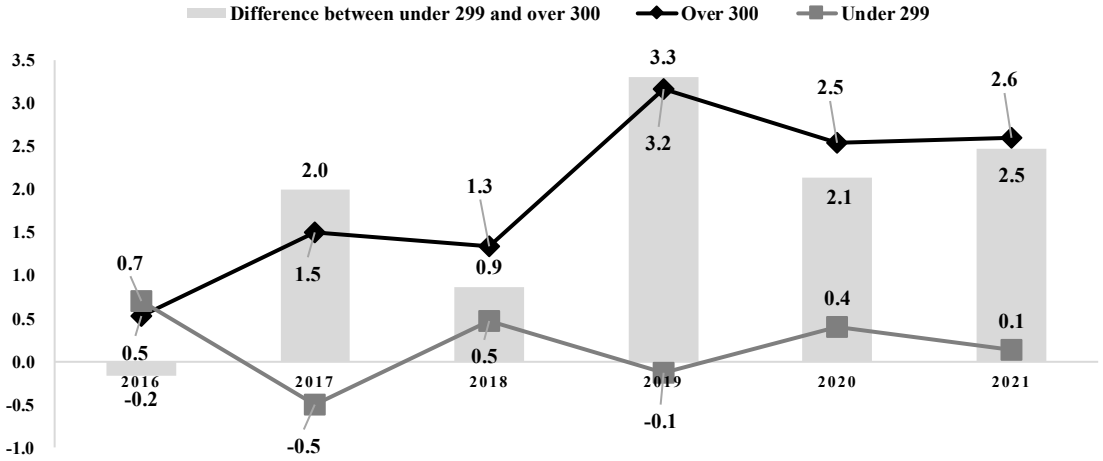
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## Figure 1 Changes in the Share of Female Managers




Note: The figure plots changes in the share of female managers relative to the 2015 level separately for firms with 300 or more employees and firms with fewer than 300 employees.
Source: Ministry of Health, Labour and Welfare, Basic Survey on Employment Equality

**Table 1: The Association Between Firm Eligibility Under the Act on Promotion of Women's Participation and Advancement in the Workplace and Changes in Corporate Behavior**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dependent variables | Action plan | Assessment of women's promotion | Numerical target for women's promotion | Disclosure items | Increase in female section managers | Increase in promotions to female section managers |
| | Marginal effect | Marginal effect | Marginal effect | | Marginal effect | Marginal effect |
| 300+ employees | 0.475*** | 0.186*** | 0.144*** | 2.249*** | 0.179*** | 0.092*** |
| | (0.007) | (0.041) | (0.039) | (0.122) | (0.017) | (0.018) |
| Contorl variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Estimation methods | Probit | Probit | Probit | OLS | Probit | Probit |
| Observations | 2,893 | 821 | 1,034 | 2,693 | 2,594 | 2,331 |
| R-squared | | | | 0.185 | | |

Note: The action plan is set to 1 when the firm formulates a general employer action plan under the Act, and becomes 0 otherwise. Assessment of women's promotion is 1 if the firm assessed women's promotion before implementing the Act, and 0 otherwise. The numerical target for women's promotion is 1 if the firm has established numerical targets for women's promotion in its action plan, and 0 otherwise. Disclosure items indicate the number of women's advancement indicators publicly disclosed by the firm. Increase in female section managers is 1 if the number of female section-manager-level employees increased between 2014 and 2018, and 0 otherwise. Increase in promotions to female section managers is 1 if promotions to female assistant-manager-level positions increased between 2014 and 2018, and 0 otherwise. Control variables include firm establishment year, industry, and the average age and tenure of male and female employees.

Source: Survey on Women's Active Participation and Work-Life Balance, conducted by the Japan Institute for Labour Policy and Training

**Table 2 Summary statistics**

| | | Mean | Std. dev. |
|---|---|---|---|
| Job satisfaction | | 2.967 | 0.968 |
| | | | |
| 300+ employees×Post-2016 dummy | | 0.172 | 0.378 |
| 300+ employees | | 0.419 | 0.494 |
| Post-2016 dummy | | 0.388 | 0.487 |
| | | | |
| Age dummy(20-29) | | 0.028 | 0.165 |
| Age dummy(30-39) | | 0.174 | 0.379 |
| Age dummy(40-49) | | 0.410 | 0.492 |
| Age dummy(50-59) | | 0.388 | 0.488 |
| Marriage | | 0.803 | 0.398 |
| Number of children | | 1.665 | 1.170 |
| Log earning | | 6.131 | 0.469 |
| weekly working hours | | 50.043 | 12.011 |
| Industry | Agriculture, forestry, fisheries, and mining | 0.015 | 0.121 |
| | Construction | 0.095 | 0.293 |
| | Manufacturing | 0.264 | 0.441 |
| | Wholesale and retail | 0.116 | 0.320 |
| | Finance, insurance, and real estate | 0.041 | 0.197 |
| | Transportation and communications | 0.115 | 0.319 |
| | Electricity, gas, water, and heat supply | 0.027 | 0.161 |
| | Services | 0.195 | 0.396 |
| | Education and other | 0.134 | 0.341 |
| Occupation | Clerical work | 0.173 | 0.379 |
| | Sales | 0.139 | 0.346 |
| | Professional/technical occupations | 0.258 | 0.438 |
| | Service occupations | 0.193 | 0.395 |
| | Operational occupations and others | 0.237 | 0.425 |
| | N | 1,357 | |

**Table 3 Effect of the Act on Promotion of Women's Participation and Advancement in the Workplace on Job Satisfaction of Non-Managerial Male Employees**

| | (1) | (2) |
|---|---|---|
| 300+ employees×Post-2016 dummy | -0.301** | -0.301** |
| | (0.117) | (0.119) |
| 300+ employees | 0.034 | 0.045 |
| | (0.172) | (0.180) |
| Post-2016 dummy | 0.129* | 0.131 |
| | (0.075) | (0.083) |
| Independent variables | | Yes |
| Estimation methods | FE OLS | FE OLS |
| Observations | 1,357 | 1,357 |
| R-squared | 0.012 | 0.026 |

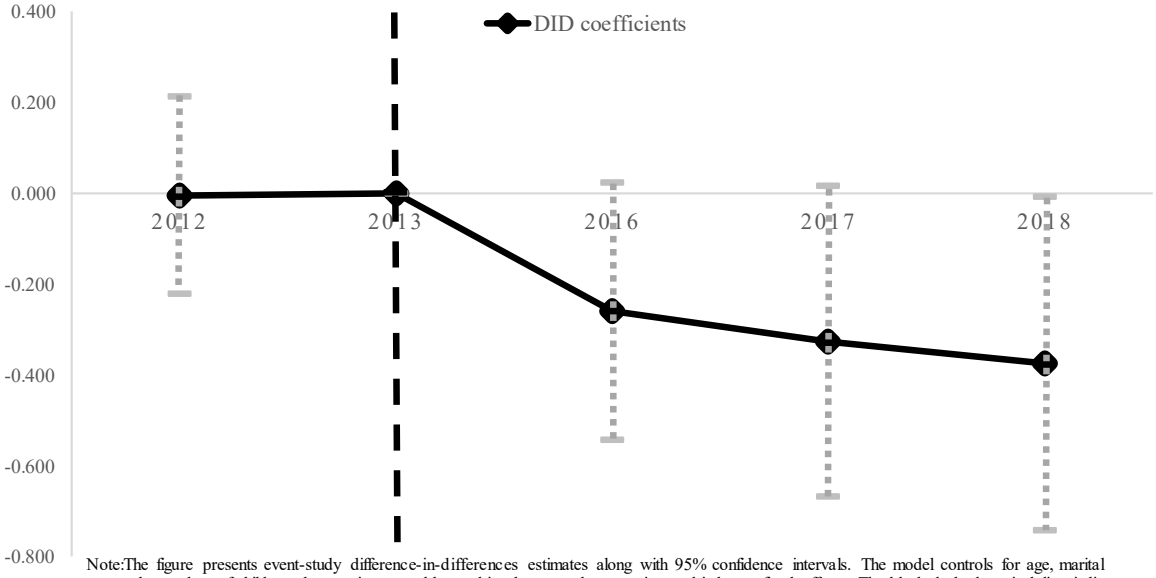
Note: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Individual attributes include age dummies, marital status, the number of children, log earnings, weekly working hours, occupation, and industry.

**Table 4. Identification and Validity: Event Study, Threshold Restriction, Matching, and Sample Selection**

| | Event study analysis | Restricting the sample to 100–299 employees and 300–499 employees | PSM-DID | | Includes job-changer samples |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| 300+ employees×2012 dummy | -0.004 | | | | |
| | (0.111) | | | | |
| 300+ employees×2016 dummy | -0.260* | | | | |
| | (0.144) | | | | |
| 300+ employees×2017 dummy | -0.326* | | | | |
| | (0.175) | | | | |
| 300+ employees×2018 dummy | -0.374** | | | | |
| | (0.187) | | | | |
| 300+ employees | 0.048 | 0.418* | 0.041 | 0.087 | 0.064 |
| | (0.188) | (0.251) | (0.198) | (0.193) | (0.159) |
| 2012 dummy | 0.066 | | | | |
| | (0.070) | | | | |
| 2016 dummy | 0.194* | | | | |
| | (0.101) | | | | |
| 2017 dummy | 0.111 | | | | |
| | (0.116) | | | | |
| 2018 dummy | 0.104 | | | | |
| | (0.141) | | | | |
| 300+ employees×Post-2016 dummy | | -0.573** | -0.382*** | -0.340** | -0.252** |
| | | (0.283) | (0.128) | (0.132) | (0.108) |
| Post-2016 dummy | | 0.135 | 0.159* | 0.144 | 0.123 |
| | | (0.160) | (0.089) | (0.095) | (0.079) |
| Independent variables | Yes | Yes | Yes | Yes | Yes |
| PSM: Matching methods | | | Kernel | Radius | |
| Estimation methods | FE OLS | FE OLS | FE OLS | FE OLS | FE OLS |
| Observations | 1,357 | 298 | 1,027 | 943 | 1,608 |
| R-squared | 0.033 | 0.185 | 0.036 | 0.028 | 0.020 |

Note: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Individual attributes include age dummies, marital status, the number of children, log earnings, weekly working hours, occupation, and industry. Column 2 restricts the sample to workers in firms with 100–299 employees (control) and 300–499 employees (treatment), limiting the comparison to firms in close proximity to the policy threshold.

## Figure 2 Event Study Estimates: Effect of the Act




Note:The figure presents event-study difference-in-differences estimates along with 95% confidence intervals. The model controls for age, marital status, the number of children, log earnings, weekly working hours, and occupation and industry fixed effects. The black dashed vertical line indicates the reference period.

**Table 5. Statistical Inference and Model Specification**

| | Using the firm-level cluster | Using the industry-by-firm-size-category level cluster | Using the industry-by-year level cluster | Fixed effect ordered logit |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 300+ employees×Post-2016 dummy | -0.301** | -0.301*** | -0.301*** | -0.919** |
| | (0.096) | (0.113) | (0.104) | (0.359) |
| 300+ employees | 0.045 | 0.045 | 0.045 | 0.205 |
| | (0.103) | (0.133) | (0.196) | (0.429) |
| Post-2016 dummy | 0.131 | 0.131 | 0.131 | 0.368 |
| | (0.131) | (0.093) | (0.078) | (0.243) |
| Independent variables | Yes | Yes | Yes | Yes |
| Estimation methods | FE OLS | FE OLS | FE OLS | FE Ologit |
| Observations | 1,156 | 1,155 | 1,155 | 1,146 |
| R-squared | 0.665 | 0.665 | 0.665 | |

Note: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Individual attributes include age dummies, marital status, the number of children, log earnings, weekly working hours, occupation, and industry. The estimated result in column (4) shows the coefficient.

**Table 6. Additional Outcomes**

| | Dependent variable=subjective rated health | Dependent variable= stress |
|---|---|---|
| | (1) | (2) |
| 300+ employees×Post-2016 dummy | -0.230** | -0.222* |
| | (0.117) | (0.126) |
| 300+ employees | 0.274* | 0.073 |
| | (0.151) | (0.149) |
| Post-2016 dummy | 0.121 | 0.035 |
| | (0.081) | (0.085) |
| Independent variables | Yes | Yes |
| Estimation methods | FE OLS | FE OLS |
| Observations | 1,355 | 1,354 |
| R-squared | 0.033 | 0.040 |

Note: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Individual attributes include age dummies, marital status, the number of children, log earnings, weekly working hours, occupation, and industry.

**Table 7. Potential Mechanisms: Heterogeneity by Career Stage and Competition Preferences**

| | Age groups (30s and 40s) | Age groups (20s and 50s) | Tenure under 10 years | Tenure over 10 years | Male full-time managerial employee | competition-oriented workers | less competition-oriented workers |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| 300+ employees×Post-2016 dummy | -0.387** | 0.040 | -0.107 | -0.303** | -0.196 | -0.723** | -0.203 |
| | (0.167) | (0.291) | (0.366) | (0.135) | (0.167) | (0.311) | (0.127) |
| 300+ employees | 0.092 | -0.344 | 0.745 | -0.061 | -0.147 | 0.237 | 0.007 |
| | (0.204) | (0.443) | (0.582) | (0.221) | (0.304) | (0.315) | (0.219) |
| Post-2016 dummy | 0.093 | 0.102 | -0.017 | 0.126 | 0.100 | 0.175 | 0.109 |
| | (0.126) | (0.124) | (0.198) | (0.098) | (0.125) | (0.198) | (0.093) |
| Independent variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Estimation methods | FE OLS | FE OLS | FE OLS | FE OLS | FE OLS | FE OLS | FE OLS |
| Observations | 792 | 565 | 283 | 1,074 | 532 | 244 | 1,103 |
| R-squared | 0.043 | 0.108 | 0.242 | 0.031 | 0.089 | 0.112 | 0.034 |

Note: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Individual attributes include age dummies, marital status, the number of children, log earnings, weekly working hours, occupation, and industry. (*)

**Table 8. Alternative Explanations: Workplace Characteristics, Industry Effects, Firm Size, and Additional Period Analysis**

| | Performance -based pay | Seniority -based pay | Working at full capacity (no slack) | Working with slack/flexibility | Adding industry× post-2016 dummy | Using a detailed firm category + adding public sector employees | Including the sample of 2021 |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| 300+ employees×Post-2016 dummy | -0.326** | -0.697*** | -0.430** | -0.300* | -0.294** | | -0.209* |
| | (0.149) | (0.259) | (0.185) | (0.167) | (0.122) | | (0.109) |
| 300+ employees | 0.001 | 0.159 | 0.224 | -0.457 | 0.041 | | 0.038 |
| | (0.236) | (0.278) | (0.235) | (0.314) | (0.175) | | (0.159) |
| Post-2016 dummy | 0.112 | 0.325** | 0.107 | 0.208 | 0.245* | 0.211* | 0.096 |
| | (0.106) | (0.145) | (0.135) | (0.129) | (0.144) | (0.113) | (0.079) |
| 30-299 employees×Post-2016 dummy | | | | | | -0.108 | |
| | | | | | | (0.156) | |
| 300-999 employees×Post-2016 dummy | | | | | | -0.338* | |
| | | | | | | (0.181) | |
| 1000+ employees×Post-2016 dummy | | | | | | -0.380** | |
| | | | | | | (0.156) | |
| Public sector employees×Post-2016 dummy | | | | | | -0.453** | |
| | | | | | | (0.221) | |
| 30-299 employees | | | | | | -0.005 | |
| | | | | | | (0.195) | |
| 300-999 employees | | | | | | 0.096 | |
| | | | | | | (0.232) | |
| 1000+ employees | | | | | | -0.010 | |
| | | | | | | (0.266) | |
| Public sector employees | | | | | | -0.086 | |
| | | | | | | (0.290) | |
| Independent variables | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry × Post-2016 dummy | | | | | Yes | | |
| Estimation methods | FE OLS | FE OLS | FE OLS | FE OLS | FE OLS | FE OLS | FE OLS |
| Observations | 910 | 447 | 767 | 590 | 1,357 | 1,504 | 1,505 |
| R-squared | 0.049 | 0.163 | 0.079 | 0.133 | 0.034 | 0.029 | 0.031 |

Note: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Individual attributes include age dummies, marital status, the number of children, log earnings, weekly working hours, occupation, and industry.

**Appendix 1 Event-Study Estimates Using the Sample to 100–299 Employees and 300–499 Employees**

| | Restricting the sample to 100–299 employees and 300–499 employees |
|---|---|
| 300+ employees×2012 dummy | -0.260 |
| | (0.285) |
| 300+ employees×2016 dummy | -0.561* |
| | (0.330) |
| 300+ employees×2017 dummy | -0.754** |
| | (0.368) |
| 300+ employees×2018 dummy | -0.918* |
| | (0.507) |
| 300+ employees | 0.590* |
| | (0.299) |
| 2012 dummy | 0.197 |
| | (0.130) |
| 2016 dummy | 0.015 |
| | (0.160) |
| 2017 dummy | 0.395** |
| | (0.191) |
| 2018 dummy | 0.164 |
| | (0.253) |
| Independent variables | Yes |
| Estimation methods | FE OLS |
| Observations | 298 |
| R-squared | 0.221 |

Note: *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level. Individual attributes include age dummies, marital status, the number of children, log earnings, weekly working hours, occupation, and industry. This analysis restricts the sample to workers in firms with 100–299 employees (control) and 300–499 employees (treatment), limiting the comparison to firms in close proximity to the policy threshold.

**Appendix 2 Covariate balance before and after kernel-based propensity score matching**

| | | Mean | | Differences |
|---|---|---|---|---|
| | | Treated | Control | |
| Age dummy(20-29) | Unmatched | 0.026 | 0.029 | -0.003 |
| | Matched | 0.038 | 0.040 | -0.002 |
| Age dummy(30-39) | Unmatched | 0.158 | 0.185 | -0.027 |
| | Matched | 0.190 | 0.181 | 0.009 |
| Age dummy(50-59) | Unmatched | 0.352 | 0.414 | -0.062** |
| | Matched | 0.316 | 0.329 | -0.012 |
| Marriage | Unmatched | 0.849 | 0.769 | 0.079*** |
| | Matched | 0.861 | 0.869 | -0.008 |
| Number of children | Unmatched | 1.762 | 1.596 | 0.167** |
| | Matched | 1.791 | 1.791 | 0.000 |
| Log earning | Unmatched | 6.332 | 5.987 | 0.345*** |
| | Matched | 6.294 | 6.259 | 0.035 |
| weekly working hours | Unmatched | 48.576 | 51.100 | -2.524*** |
| | Matched | 47.892 | 47.952 | -0.060 |
| Industry: Agriculture, forestry, fisheries, and mining | Unmatched | 0.004 | 0.023 | -0.019*** |
| | Matched | 0.013 | 0.011 | 0.002 |
| Industry: Construction | Unmatched | 0.044 | 0.132 | -0.088*** |
| | Matched | 0.025 | 0.034 | -0.009 |
| Industry: Wholesale and retail | Unmatched | 0.083 | 0.139 | -0.057*** |
| | Matched | 0.089 | 0.084 | 0.005 |
| Industry: Finance, insurance, and real estate | Unmatched | 0.055 | 0.030 | 0.024** |
| | Matched | 0.044 | 0.067 | -0.023 |
| Industry: Transportation and communications | Unmatched | 0.146 | 0.093 | 0.054*** |
| | Matched | 0.114 | 0.151 | -0.037 |
| Industry: Electricity, gas, water, and heat supply | Unmatched | 0.040 | 0.016 | 0.024*** |
| | Matched | 0.038 | 0.016 | 0.022 |
| Industry: Services | Unmatched | 0.185 | 0.202 | -0.017 |
| | Matched | 0.152 | 0.142 | 0.010 |
| Industry: Education and other | Unmatched | 0.153 | 0.120 | 0.033* |
| | Matched | 0.177 | 0.138 | 0.039 |
| Occupation: Sales | Unmatched | 0.116 | 0.156 | -0.040** |
| | Matched | 0.101 | 0.093 | 0.008 |
| Occupation: Professional/technical occupations | Unmatched | 0.305 | 0.224 | 0.080*** |
| | Matched | 0.316 | 0.281 | 0.035 |
| Occupation: Service occupations | Unmatched | 0.185 | 0.199 | -0.014 |
| | Matched | 0.165 | 0.165 | 0.000 |
| Occupation: Operational occupations and others | Unmatched | 0.136 | 0.309 | -0.174*** |
| | Matched | 0.184 | 0.172 | 0.011 |

Note: Values represent covariate means for the treated and control groups before and after kernel-based propensity score matching.