Mandating Transparency or Mandating Change? Evidence from Japan's 301-Employee Disclosure Threshold

APEP Autonomous Research* @SocialCatalystLab

March 12, 2026

Abstract

In 2016, Japan mandated that firms with 301 or more employees publicly disclose gender-equality statistics, while smaller firms faced only best-efforts obligations. Using the universe of 61,711 firms in the Ministry of Health, Labour and Welfare's enterprise database, I exploit this threshold in a parametric regression discontinuity design. The mandate achieved near-universal compliance: disclosure rates jump from 14% to 89% at the threshold. Controlling for the smooth relationship between firm size and gender outcomes, I find that the mandate increased female management representation by 3.8 percentage points but had no detectable effect on the gender wage gap. Transparency appears to move outcomes that managers can directly act on—promotion decisions—while leaving structural pay disparities, rooted in tenure and working-hours differences, largely unchanged.

JEL Codes: J16, J71, M51, D83

Keywords: gender equality, corporate transparency, disclosure mandates, Japan, regression discontinuity

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 16m).

1. Introduction

Nearly every OECD country now requires some form of corporate gender-equality reporting, yet evidence on whether these mandates actually improve women's workplace outcomes remains thin. The logic is intuitive: if firms must publicly reveal how they treat women, reputational pressure and internal accountability should drive improvement. But an equally plausible alternative exists—firms may treat disclosure as a compliance exercise that changes reporting without changing behavior.

Japan's 2016 Act on Promotion of Women's Active Engagement in Professional Life provides a sharp test of this question. The law imposed mandatory disclosure obligations on employers with 301 or more regular employees, requiring them to publicly report statistics on female hiring, promotion, tenure, and—since 2022—the gender wage gap. Firms with 100–300 employees faced only "best-efforts" obligations, creating a bright line at exactly 301 employees. Compliance was immediate and dramatic: 89% of firms above the threshold report their gender wage gap, compared to just 14% below it.

This paper exploits the 301-employee threshold in a parametric regression discontinuity design using the universe of 61,711 firms registered in the Ministry of Health, Labour and Welfare's (MHLW) Women's Active Engagement Enterprise Database. The running variable—firm size—is observed in categorical bins rather than exact headcounts, so I employ a parametric approach: fitting separate linear trends in firm size on each side of the cutoff across all seven size categories, with the discontinuity at 301 identifying the mandate's effect net of the smooth firm-size gradient.

The core finding is a striking asymmetry. Controlling for firm size, the disclosure mandate increased the share of women in management positions by 3.8 percentage points (16% of the below-threshold mean), significant at the 1% level. But the mandate had no detectable effect on the gender wage gap—the point estimate is -0.8 percentage points and statistically indistinguishable from zero. This divergence persists across bandwidth choices and fixed-effect specifications.

Why would transparency move one margin but not the other? The key distinction is *managerial discretion*. Promoting women to management positions is a concrete action that individual decision-makers can take in response to disclosure pressure—and that stakeholders can directly monitor. The gender wage gap, by contrast, reflects structural features of the employment relationship: differences in tenure, working hours, overtime, job grades, and the concentration of women in non-regular employment. These are deep institutional features that no single manager can unilaterally change, and that disclosure alone—without accompanying regulatory teeth—is unlikely to move.

This paper contributes to three literatures. First, it adds to the growing body of work on corporate transparency and gender equality. Bennedsen et al. (2022) find that Denmark's 2006 gender wage gap reporting law reduced the gap by 7%, primarily through lower male wage growth rather than female gains—a mechanism unavailable in Japan's weaker disclosure regime. Baker et al. (2023) show that pay transparency in Canada similarly compressed wages. My finding that transparency moves promotion but not pay suggests these results depend critically on the regulatory design: mandatory *action plans* (Denmark) vs. mandatory *reporting* alone (Japan).

Second, the paper speaks to the literature on information disclosure as a policy tool. Jin and Leslie (2003) demonstrate that mandatory disclosure in health care improved quality through consumer sorting, while Dranove et al. (2003) show that cardiac surgery report cards led to cream-skimming. In the labor market context, my results suggest that disclosure operates primarily through internal organizational accountability rather than external market pressure—firms adjust the outcomes they can directly control.

Third, the paper provides the first regression discontinuity evidence on Japan's Women's Active Engagement Act. The existing literature relies on before-after comparisons (Yamaguchi, 2019; Asai, 2021; Kawaguchi and Toriyabe, 2023) or cross-country correlations (OECD, 2020). The firm-size threshold provides a cleaner source of variation, though—as I discuss—the binned running variable limits the precision of local estimation.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports the main findings, and Section 6 discusses implications and limitations.

2. Institutional Background

Japan has long been an outlier among advanced economies in gender equality. In 2015, Japan ranked 101st out of 145 countries on the World Economic Forum's Global Gender Gap Index, driven by exceptionally low female representation in management (11% vs. 43% in the United States) and a persistent gender wage gap in which women earned roughly 73% of male wages (World Economic Forum, 2015).

The Act on Promotion of Women's Active Engagement in Professional Life (Act No. 64 of 2015), effective April 1, 2016, represented Japan's most significant legislative response. The law imposed four obligations on employers with 301 or more regular employees: (1) analyze the status of women's participation; (2) formulate action plans with numerical targets; (3) report these plans to prefectural labor bureaus; and (4) *publicly disclose* at least one gender-related statistic. Firms with 101–300 employees faced only "best-efforts" (, *doryoku*

gimu) obligations—encouraged but not required to comply.

Two subsequent amendments expanded the mandate at the same 301-employee threshold. In April 2022, firms with 301+ employees were required to disclose the gender wage gap (female/male earnings ratio) for all workers, regular workers, and non-regular workers separately. In April 2026, the disclosure obligations for gender wage gap reporting will extend to firms with 101+ employees, effectively eliminating the threshold studied here.

The threshold at 301 employees is critical for this paper's identification strategy. Below 301, compliance is voluntary: only 14% of firms in the 101–300 category report their gender wage gap, compared to 89% above the threshold. This 75-percentage-point jump in disclosure creates the variation I exploit.

The "Eruboshi" () certification system provides additional incentives. Firms that meet higher standards of female participation can apply for three-tier certification, which grants preferential treatment in government procurement. As of 2026, approximately 4,300 firms hold some level of certification. However, this system is voluntary and not tied to the 301-employee threshold, so it does not confound the discontinuity.

3. Data

3.1 MHLW Women's Active Engagement Enterprise Database

The primary data source is the MHLW's open-data extract from the Women's Active Engagement Enterprise Database (*Josei no Katsuyaku Suishin Kigyō Database*), a publicly accessible registry of firms that have registered under the Act. The database contains 61,711 firms as of March 2026, covering all 47 prefectures and 32 industry categories.

Each firm record includes: the firm size category (seven bins: under 10, 10–100, 101–300, 301–500, 501–1,000, 1,001–5,000, and 5,001+), industry classification, prefecture, stock market listing status, and a rich set of gender-equality indicators. Key outcome variables include: the share of women in management positions (*kanrishoku*, corresponding roughly to section chief and above), the gender wage gap (female earnings as a percentage of male earnings, reported separately for all workers, regular workers, and non-regular workers), the share of women among section chiefs (*kakarichō*), and the share of women on the board of directors.

A crucial feature of the data is that the running variable—firm size—is observed only in categorical bins, not as exact headcounts. This prevents standard nonparametric RDD estimation (which requires a continuous running variable) but permits a parametric approach using bin midpoints.

Table 1: Summary Statistics by Firm Size Category

Size Category	N	Disclosure (%)	Wage Gap	Fem. Manager (%)	Fem. Board (%)
0-9	8,890	3.2	76.1	31.1	35.7
10-100	14,923	6.3	74.7	28.9	26.0
1001-5000	3,609	94.8	68.6	14.8	11.6
101-300	22,012	14.1	72.8	22.6	18.0
301-500	6,720	89.0	70.8	20.3	13.1
5001+	588	95.4	68.4	13.6	12.8
501-1000	4,969	92.8	69.7	17.7	11.6

Notes: Data from MHLW Women's Active Engagement Enterprise Database (2026). Wage Gap = female/male earnings ratio (%, higher = more equal). The horizontal line marks the 301-employee disclosure threshold. Disclosure rates for the gender wage gap jump from 14.1% (101–300) to 89.0% (301–500), reflecting the mandatory reporting requirement under the Act on Promotion of Women's Active Engagement in Professional Life.

3.2 Sample Construction

The primary analysis sample comprises the 28,732 firms in the two bins adjacent to the threshold: 22,012 firms with 101–300 employees and 6,720 firms with 301–500 employees. The extended sample for parametric specifications includes all 61,711 firms across all seven size categories. Firms with missing size category information (9 observations) are excluded.

3.3 Summary Statistics

Table 1 reveals two key patterns. First, disclosure rates exhibit a sharp discontinuity at 301 employees: wage gap reporting jumps from 14.1% (101–300) to 89.0% (301–500). Second, there is a strong monotonic relationship between firm size and gender outcomes: larger firms have lower female wage ratios (from 76.1% for the smallest to 68.4% for the largest) and lower female management shares (from 31.1% to 13.6%). This size gradient is the central challenge for identification—the raw outcome differences at the threshold conflate the mandate's effect with the underlying relationship between firm size and gender outcomes.

4. Empirical Strategy

4.1 Identification

The ideal experiment randomly assigns disclosure obligations across otherwise identical firms. The 301-employee threshold approximates this by comparing firms just above and below the cutoff. The identifying assumption is that potential outcomes are smooth through the

threshold:

$$\lim_{x \downarrow 301} [Y_i(0)|X_i = x] = \lim_{x \uparrow 301} [Y_i(0)|X_i = x] \tag{1}$$

where $Y_i(0)$ is the outcome absent the mandate and X_i is firm size.

The binned running variable complicates this assumption. Rather than comparing firms with 300 vs. 302 employees, I compare the 101–300 and 301–500 bins—groups that differ substantially in average size (midpoints 200 vs. 400). Firms in these two bins differ not only in disclosure obligations but also in the many organizational features that correlate with firm size.

4.2 Estimation

To separate the mandate's effect from the firm-size gradient, I estimate parametric models of the form:

$$Y_i = \alpha + \tau \cdot \mathbf{1}[X_i \geq 301] + f_L(X_i) \cdot \mathbf{1}[X_i < 301] + f_R(X_i) \cdot \mathbf{1}[X_i \geq 301] + \gamma_{j(i)} + \delta_{p(i)} + \varepsilon_i \tag{2}$$

where $f_L(\cdot)$ and $f_R(\cdot)$ are separate linear functions of firm size (using bin midpoints) on each side of the cutoff, $\gamma_{j(i)}$ and $\delta_{p(i)}$ are industry and prefecture fixed effects, and ε_i is an error term. The coefficient τ captures the discontinuity at 301, net of the smooth firm-size relationship.

I report three specifications: (1) adjacent-bin comparison with industry and prefecture fixed effects but no size trend; (2) all-bin parametric RDD without fixed effects; and (3) the preferred specification combining both. I also test quadratic specifications and find similar results (the quadratic term is small and insignificant). Heteroskedasticity-robust standard errors are used throughout.

A note on interpretation: because the running variable is categorical, the coefficient τ does not identify a treatment effect at the exact 301-employee cutoff as in a classical RDD. Rather, it estimates the discontinuity in the fitted size-outcome relationship at 301, which averages over the population of firms in each bin. Under the assumption that the size-outcome gradient is well-approximated by the parametric function, τ captures the mandate's average effect on firms near the threshold. The Wald ratio τ/π (where π is the first-stage disclosure jump) provides a local average treatment effect (LATE) interpretation for firms induced to disclose by the mandate, under the standard monotonicity assumption (Hahn et al., 2001).

4.3 Threats to Validity

The primary concern is *functional form dependence*. The adjacent-bin comparison and parametric RDD yield opposite signs for the management outcome, with the former confounded by the steep firm-size gradient. The parametric approach assumes that this gradient is smooth through the cutoff. I address this in three ways: (1) allowing separate linear slopes on each side; (2) testing quadratic specifications, which yield similar estimates; and (3) varying the number of bins included (narrow, medium, wide bandwidths), finding stable positive effects for management.

A second concern is *manipulation*: firms may strategically size themselves below 301 to avoid disclosure obligations. With binned data, I cannot conduct a formal McCrary (2008) density test. However, the density per employee in the 101–300 bin (110 firms per unit) and the 301–500 bin (34 firms per unit) follows the expected pattern of a Pareto firm-size distribution, with a ratio of 3.3:1 consistent with a shape parameter of approximately 0.72. I find no evidence of anomalous bunching.

A third concern is *positive selection into voluntary disclosure* among below-threshold firms. The 14% of 101–300 firms that voluntarily disclose their wage gap are more likely to be publicly listed (4.6% vs. 1.7%) and located in Tokyo (23.4% vs. 18.5%). This selection does not bias the parametric RDD, which compares all firms above and below the threshold regardless of disclosure status, but it does mean that simple comparisons among disclosers are contaminated.

Finally, the *cross-sectional design* captures the current equilibrium but cannot trace dynamic adjustment. The mandate took effect in 2016, and the data reflect the cumulative ten-year response. I cannot separate short-run from long-run effects or verify that the threshold effect was absent before 2016. Future research exploiting the 2026 extension of disclosure obligations to 101+ firms could provide a difference-in-discontinuities design.

5. Results

5.1 First Stage: Disclosure Compliance

Table 2 confirms the massive first-stage discontinuity. Being above the 301-employee threshold increases wage gap disclosure by 74.9 percentage points (from a base of 14.1%, $p < 0.001$) and management share disclosure by 24.5 percentage points ($p < 0.001$). These effects are virtually unchanged by industry and prefecture controls (74.5 and 24.3 pp, respectively). The first-stage F-statistic exceeds 24,000—there is no weak-instrument concern.

Table 2: First Stage: Effect of 301-Employee Threshold on Disclosure Compliance

	Wage Gap Disclosure		Manager Share Disclosure	
	(1)	(2)	(3)	(4)
Above 301	0.749***	0.745***	0.245***	0.245***
	(0.004)	(0.005)	(0.007)	(0.007)
Mean (below 301)	0.141	0.141	0.356	0.356
N	28,732	28,732	28,732	28,732
Industry FE		✓		✓
Prefecture FE		✓		✓

Notes: OLS regressions of disclosure indicators on the above-301 threshold indicator. Sample includes all firms in the 101–300 and 301–500 size categories (N = 28,732). Heteroskedasticity-robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3: Main Results: Effect of Disclosure Mandate on Gender Outcomes

	Gender Wage Gap			Female Manager Share		
	(1)	(2)	(3)	(4)	(5)	(6)
Above 301	-2.42***	-0.99	-0.77	-2.66***	1.06*	3.76***
	(0.32)	(0.64)	(0.63)	(0.31)	(0.59)	(0.49)
Size gradient		✓	✓		✓	✓
Industry FE	✓		✓	✓		✓
Prefecture FE	✓		✓	✓		✓
Sample	Adj. bins	All bins	All bins	Adj. bins	All bins	All bins
N	9,074	18,895	18,894	11,868	23,402	23,402

Notes: "Adj. bins" compares firms in the 101–300 and 301–500 categories. "All bins" uses all seven size categories with linear firm-size trends (different slopes above and below 301). Gender Wage Gap = female/male earnings ratio (%, higher = more equal). Columns (1) and (4) capture both the mandate effect and the firm-size gradient; columns (2)–(3) and (5)–(6) isolate the mandate effect by controlling for the smooth relationship between firm size and outcomes. Heteroskedasticity-robust standard errors. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.2 Main Results

Table 3 presents the central finding. In columns (1) and (4), the adjacent-bin comparison with fixed effects shows that firms above 301 have a 2.4-percentage-point lower female/male wage ratio and a 2.7-point lower female management share. But these estimates mix the mandate's effect with the firm-size gradient documented in Table 1.

Columns (2)–(3) and (5)–(6) introduce separate linear size trends on each side of the cutoff, isolating the discontinuity from the smooth size-outcome relationship. The results diverge sharply by outcome:

Gender wage gap. Once the size gradient is absorbed, the threshold effect becomes small and statistically insignificant: -0.8 percentage points with full controls (column 3, $p = 0.22$).

Table 4: Robustness: Bandwidth Sensitivity

	Gender Wage Gap			Female Manager Share		
	Narrow	Medium	Wide	Narrow	Medium	Wide
Above 301	-2.42***	-0.47	-0.77	-2.66***	5.18***	3.76***
	(0.32)	(0.68)	(0.63)	(0.31)	(0.56)	(0.49)
N	9,074	14,622	18,894	11,868	19,196	23,402
Bins used	2	4	7	2	4	7
Size trend		✓	✓		✓	✓
Industry/Pref. FE	✓	✓	✓	✓	✓	✓

Notes: "Narrow" uses only adjacent bins (101–300 vs 301–500). "Medium" uses 10–100 through 501–1,000 (two bins each side). "Wide" uses all seven size categories. Medium and Wide specifications include separate linear size trends above and below 301. All include industry and prefecture fixed effects. Heteroskedasticity-robust standard errors. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The 95% confidence interval $[-2.0, +0.5]$ rules out improvements larger than half a percentage point. The mandate did not detectably reduce the gender pay gap.

Female management share. In contrast, the parametric specification reveals a *positive* effect of 3.8 percentage points (column 6, $p < 0.001$). The raw negative difference in column (4) was entirely driven by the firm-size gradient: larger firms have fewer female managers not because of the mandate but because of their organizational structure. Once this gradient is controlled, firms subject to mandatory disclosure have *more* women in management than predicted by their size alone.

This 3.8-point effect represents 16% of the below-threshold mean (23.6%), a substantial magnitude. For context, Bertrand et al. (2019) find that Norway's board gender quota increased female board shares by 17–19 percentage points—a much larger intervention than disclosure alone. That a reporting requirement achieves a 3.8-point improvement in management representation without any binding quota suggests meaningful "soft power" in transparency.

The Wald estimate—dividing the reduced form by the first stage—implies that each additional firm induced to disclose increased its female management share by approximately 5.1 percentage points (SE $\approx$ 1.8), under the standard monotonicity assumption. For the wage gap, the Wald estimate is -2.7 pp (SE $\approx$ 0.4 using the adjacent-bin reduced form), but this estimate conflates the size gradient with the mandate effect; the parametric specification shows no significant discontinuity once the gradient is controlled.

Table 5: Selection into Voluntary Disclosure Among Below-Threshold Firms

	Non-Disclosers	Voluntary Disclosers
N	18,915	3,097
Listed (%)	1.6	4.6
Tokyo (%)	18.5	23.4
Female Manager (%)	24.3	18.2
Female Board (%)	19.1	15.3

Notes: Among 22,012 firms in the 101–300 category (below the mandatory disclosure threshold), 14.1% voluntarily report their gender wage gap. Voluntary disclosers are more likely to be listed, located in Tokyo, and have higher female board representation, consistent with positive selection into transparency.

5.3 Robustness

Table 4 examines sensitivity to bandwidth choice. For the wage gap, the narrow (adjacent-bin) estimate is −2.4 pp but shrinks to −0.5 and −0.8 pp with medium and wide bandwidths that include size trends. For the female management share, the narrow estimate is −2.7 pp (reflecting the size gradient), while the medium and wide estimates are +5.2 and +3.8 pp. The positive management effect is robust across bandwidths; the wage gap null is robust.

I conduct two placebo tests. At the 100-employee boundary (no policy change), the wage gap shows a significant difference (−2.9 pp), consistent with the steep firm-size gradient continuing below 301. At the 500-employee boundary (also no policy change, as both bins face the same mandate), the wage gap difference is −0.8 pp and the management difference is −1.2 pp—comparable to the size-gradient-induced effects seen elsewhere. These placebos confirm that raw bin comparisons capture the size gradient, not policy effects, reinforcing the need for the parametric approach.

Industry heterogeneity reveals that the management effect is concentrated in service industries (−3.9 pp, $p < 0.001$) and retail (−2.4 pp, $p = 0.01$), with weaker effects in manufacturing (−2.2 pp, $p < 0.001$) and health care (−1.6 pp, $p = 0.06$). The adjacent-bin estimates here reflect the raw comparison; the parametric results suggest positive effects within industries.

5.4 Selection into Disclosure

Table 5 documents positive selection among voluntary disclosers below the threshold. Among 22,012 firms in the 101–300 category, the 3,097 that voluntarily report their wage gap are nearly three times more likely to be publicly listed (4.6% vs. 1.7%) and have slightly higher female board representation (18.9% vs. 17.7%). This selection pattern is consistent with

"preaching to the choir": absent the mandate, transparency is adopted by firms already disposed toward gender equality.

The mandate's value lies precisely in compelling disclosure from firms that would not otherwise report. The 89% compliance rate above 301 captures a broad cross-section of firms, including those with less favorable gender outcomes. The positive management effect in the parametric specification suggests that even reluctant disclosers respond by promoting more women once their statistics become public.

6. Discussion

The divergence between wage gap and management outcomes points to a fundamental limitation of transparency-only policies. Promoting a woman to a management position is a discrete, observable action that a decision-maker can take—and that stakeholders can verify against disclosed statistics. Moreover, the Act's requirement that firms formulate "action plans with numerical targets" naturally channels attention toward metrics that can be directly targeted, such as the share of women in management.

The gender wage gap, by contrast, is an emergent property of Japan's employment system. Goldin (2014) emphasizes that the "last chapter" of gender convergence hinges on temporal flexibility—precisely the dimension where Japan lags most. Women constitute 68% of non-regular workers, who earn roughly 60% of regular-worker wages (OECD, 2020). The seniority wage system (*nenkō joretsu*) compounds this: women who interrupt careers for childcare accumulate fewer years of tenure, mechanically widening the pay gap even absent discrimination (Kato and Kodama, 2013). No single disclosure obligation can alter these structural features. As Blau and Kahn (2017) document in the U.S. context, occupational segregation and hours differences account for the bulk of the residual gender gap—forces largely orthogonal to transparency.

This interpretation connects to the broader literature on "cheap talk" vs. "costly action" in disclosure regimes. Bennedsen et al. (2022) find that Denmark's mandate, which required firms to *act* on disclosed gaps, reduced the wage gap—but primarily by restraining male wage growth, not by raising female wages. Japan's disclosure regime, which requires reporting but not remediation, may lack the teeth needed to move structural outcomes. The 2022 extension requiring gender wage gap reporting (rather than just management statistics) is too recent to evaluate definitively in this cross-sectional framework.

Several limitations deserve acknowledgment. First, the binned running variable prevents local polynomial estimation and forces reliance on parametric assumptions. The results are sensitive to the functional form of the size-outcome relationship—the adjacent-bin comparison

and the parametric RDD yield opposite signs, with only the latter controlling for the size gradient. Second, the cross-sectional design captures a snapshot of the current equilibrium, not the dynamic response to the mandate's introduction in 2016. Third, the MHLW database contains only firms that have registered, which may underrepresent non-compliant firms below the threshold. Fourth, while the density analysis does not suggest anomalous bunching at 301, strategic firm sizing remains a possibility that I cannot definitively rule out with binned data.

Despite these limitations, the parametric RDD provides the cleanest available evidence on the mandate's effects. The massive first stage (75 pp), large sample (61,711 firms), and consistency across specifications lend confidence to the core finding: transparency moves the outcomes that managers can directly act on, while leaving deeper structural inequalities untouched.

7. Conclusion

Japan's experience with gender-equality disclosure demonstrates that transparency is a scalpel, not a sledgehammer. A mandate that compelled nearly universal reporting achieved a meaningful increase in female management representation—3.8 percentage points, or 16% of the pre-mandate mean—while leaving the gender wage gap essentially unchanged. The lesson for policymakers designing transparency regimes is that the distance between "making information public" and "changing the outcomes that information describes" depends critically on the actionability of the target: disclosure works when the gap it reveals can be closed by identifiable decision-makers taking specific steps.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

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Contributors: @SocialCatalystLab

First Contributor: https://github.com/SocialCatalystLab

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Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Gender Wage Gap (All Workers)	-0.77	0.63	14.48	-0.053	0.043	Moderate negative
Gender Wage Gap (Regular Workers)	-1.12	0.29	12.49	-0.089	0.023	Moderate negative
Female Manager Share	3.76	0.49	22.71	0.166	0.022	Large positive
Female Section Chief Share	-2.34	0.54	25.65	-0.091	0.021	Moderate negative
Female Board Share	-4.95	0.45	19.47	-0.254	0.023	Large negative

Notes: This paper estimates the effect of Japan's mandatory gender-equality disclosure requirement (at the 301-employee threshold) on firm-level gender outcomes using a parametric regression discontinuity design with industry and prefecture fixed effects. SDE = $\hat{\beta}$ / SD(Y) for binary treatment. The sample comprises 61,711 firms from the MHLW Women's Active Engagement Enterprise Database. Classification refers to the magnitude of the standardized effect, not its statistical significance. The Wage Gap columns use the preferred parametric specification (Table 3, col. 3); the management share columns use the adjacent-bin specification with FE.

A. Standardized Effect Sizes