Compression, Not Uplift: Japan's Equal Pay Act and the Non-Regular Wage Gap

APEP Autonomous Research* @olafdrw

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Abstract

Over one-third of Japanese workers are "non-regular" employees earning 30–40% less than their regular counterparts—the largest such gap in the OECD. Japan's 2020 Equal Pay for Equal Work Act prohibited unreasonable pay differences between the two groups, rolling out to large firms in April 2020 and SMEs in April 2021. Exploiting this staggered adoption in a difference-in-differences framework with nine firm-size-by-sex panels over eleven years, I find the wage gap ratio narrowed by approximately 1.2 percentage points ($p < 0.01$). Observed convergence is driven by larger declines in measured regular wages (¥12,900/month) than in non-regular wages (¥3,500)—suggesting compression from above rather than uplift from below. The pattern is three times larger for women than men. These findings—the first quasi-experimental evidence on the Act—should be interpreted cautiously given the coincidence of treatment with COVID-19 and the aggregate nature of the data.

JEL Codes: J31, J38, J71, K31

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*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 29m).

1. Introduction

Japan's labor market is split in two. On one side sit "regular" workers (*seishain*)—permanent employees with seniority-based pay, bonuses, and lifetime employment expectations. On the other are "non-regular" workers (*hi-seiki*)—fixed-term, part-time, or dispatched employees who perform substantially similar tasks but earn 30–40% less (Aoyagi and Ganelli, 2015). By 2019, non-regular workers comprised 37.3% of the Japanese workforce, one of the highest shares in the OECD (OECD, 2019). This duality is not merely an artifact of occupational sorting: even within the same firm and occupation, the regular/non-regular wage gap persists, driven by institutional norms rather than productivity differences (Kambayashi and Kato, 2017; Hamaaki et al., 2012).

The economic stakes are enormous. The wage gap suppresses aggregate demand, exacerbates gender inequality (70% of female workers are non-regular), depresses fertility by making family formation financially precarious, and undermines human capital investment (Abe, 2010). Recognizing this, Japan enacted the revised Part-Time and Fixed-Term Employment Act in 2018, effective April 1, 2020 for large firms ($\geq$300 employees) and April 1, 2021 for SMEs. The law prohibits "unreasonable differences" in wages, bonuses, and benefits between regular and non-regular employees performing similar work—a prohibition enforced through administrative guidance and private litigation, but without statutory penalties (Ministry of Health, Labour and Welfare, 2020).

Despite the reform's scale and ambition, no causal evaluation exists. The closest work evaluates the distinct 2019 overtime cap reform (Burdin et al., 2024), leaving the equal pay provision unexamined. This paper provides the first such evidence.

The staggered rollout by firm size creates a natural experiment. Large firms entered treatment in April 2020, while SMEs remained under the old regime until April 2021. I exploit this staggered adoption using the Callaway and Sant'Anna (2021) estimator, which avoids the forbidden comparisons inherent in two-way fixed effects with differential timing. My outcome is the non-regular/regular wage gap ratio—the ratio of non-regular to regular monthly wages multiplied by 100—from the Ministry of Health, Labour and Welfare's Basic Survey on Wage Structure, spanning 2014–2024.

The headline result is a 2.2 percentage point increase in the wage gap ratio (indicating convergence), statistically significant at the 1% level. But the mechanism contradicts the reform's stated intent. Rather than non-regular wages rising to close the gap from below, *regular wages compressed*—falling ¥12,900/month relative to trend, while non-regular wages fell only ¥3,500. Convergence came not from uplifting the bottom, but from compressing the top. This "compression, not uplift" pattern is consistent with firms equalizing treatment by

leveling down benefits rather than leveling up pay—an unintended consequence documented in other anti-discrimination contexts (Autor et al., 2006; Dube et al., 2019).

The effect is strikingly gendered: 4.4 percentage points for women versus 1.6 for men, a 3:1 ratio consistent with women's overrepresentation in non-regular employment (56% of female workers vs. 22% of male workers are non-regular). Pre-trend tests show no differential movement in the gap ratio before treatment, and the staggered design provides partial protection against COVID-19 confounds—if both treatment waves show convergence at different dates, a single contemporaneous shock is an unlikely explanation.

I stress the identification limitations honestly. With three firm-size categories and annual data, the design has few effective clusters. The April 2020 treatment date coincides with Japan's first COVID-19 state of emergency, and differential COVID impacts across firm sizes cannot be fully ruled out. These results are best understood as first descriptive evidence with a credible research design, not definitive causal proof.

2. Institutional Background

Japan's dual labor market. The regular/non-regular divide traces to the 1990s recession, when firms responded to deflationary pressures by expanding non-regular hiring to avoid the implicit obligations of lifetime employment (Kambayashi and Kato, 2017). By 2024, 37.4% of employees held non-regular contracts. The gap in monthly scheduled wages was persistent: in 2019, non-regular workers earned ¥211,200 versus ¥325,400 for regular workers—a ratio of 64.9 (regular = 100).

The 2020 reform. The revised Part-Time and Fixed-Term Employment Act () was passed in June 2018 and enacted in two stages: April 1, 2020 for large enterprises ($\geq$300 employees) and April 1, 2021 for small and medium enterprises. The law establishes two principles: (1) prohibition of "unreasonable differences" (*fugorina kakusa*) in basic pay, bonuses, and allowances between regular and non-regular workers performing similar duties; and (2) an obligation for employers to explain pay differences when requested by workers. Supreme Court rulings in October 2020 (*Osaka Isui Service* and *Metro Commerce*) established initial judicial interpretation, ruling that certain allowance differentials were unreasonable but that base pay differences could be justified by differences in duties and deployment scope.

Enforcement mechanism. Crucially, the law carries no statutory penalties. Enforcement relies on administrative guidance from Prefectural Labour Bureaus and private litigation. This soft enforcement regime makes the ex ante effect ambiguous: it could produce meaningful compliance (as in Australia's Fair Work Act) or remain largely symbolic (as critics of Japan's

2015 "equal pay for equal work" guidelines predicted).

3. Data

I use the Basic Survey on Wage Structure (*Chingin Kōzō Kihon Tōkei Chōsa*), an annual census-scale survey of approximately 78,000 establishments conducted by the Ministry of Health, Labour and Welfare (MHLW). Published summary tables report average monthly scheduled wages separately for regular (*seishain*) and non-regular (*hi-seishain*) workers, disaggregated by firm size, industry, and sex.

I construct two panels from the published tables across eleven annual surveys (2014–2024):

Firm-size panel (primary). Three firm-size categories (large: ≥300 employees; medium: 100–299; small: 10–99) × three sex groups (total, male, female) × 11 years = 99 observations. Each cell reports average regular wage, average non-regular wage, and the wage gap ratio. This panel is the basis for the staggered DiD exploiting differential treatment timing.

Industry panel (supplementary). Twelve industries consistently reported across all years × three sex groups × 11 years = 396 observations. Pre-reform wage gap ratios range from 59.4 (wholesale/retail, largest inequality) to 79.8 (construction, smallest inequality). This panel supports a continuous-treatment specification.

Table 1 reports summary statistics. Mean non-regular wages are ¥223,000/month at large firms versus ¥203,000 at small firms, while regular wages show a much steeper gradient (¥375,000 vs. ¥286,000). The gap ratio averages 59.4 at large firms (largest inequality) and 71.2 at small firms (smallest inequality), reflecting the seniority premium concentrated in large-firm regular employment.

4. Empirical Strategy

Staggered difference-in-differences. The firm-size rollout defines two treatment groups: large firms ($g = 2020$) and SMEs ($g = 2021$). I estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) estimator:

$$ATT(g,t) = \mathbb{E}[Y_{it}(g) - Y_{it}(0) \mid G_i = g] \tag{1}$$

where $Y_{it}(g)$ is the potential outcome under treatment at time g and $Y_{it}(0)$ is the untreated potential outcome. The estimator uses not-yet-treated units as controls, avoiding the forbidden comparisons that bias TWFE in staggered settings (Goodman-Bacon, 2021). I aggregate to

Table 1: Summary Statistics: Wages by Firm Size and Employment Type, 2014–2024

	Variable	Mean	SD	Min	Max
Large firms (≥300 emp.)					
	Regular wage (¥1,000/month)	375.0	7.0	365.4	391.9
	Non-regular wage (¥1,000/month)	222.9	7.5	212.8	239.8
	Wage gap ratio (regular = 100)	59.4	1.6	56.9	61.2
Medium firms (100–299 emp.)					
	Regular wage (¥1,000/month)	315.5	11.1	302.9	342.0
	Non-regular wage (¥1,000/month)	212.4	11.9	193.1	233.6
	Wage gap ratio (regular = 100)	67.3	1.7	64.0	69.7
Small firms (10–99 emp.)					
	Regular wage (¥1,000/month)	285.5	12.3	272.1	309.1
	Non-regular wage (¥1,000/month)	203.2	9.0	192.6	219.2
	Wage gap ratio (regular = 100)	71.2	0.6	70.2	72.2
Industry panel	Wage gap ratio	69.7	6.2	58.0	86.1

Notes: Firm-size panel: 3 firm sizes × 11 years = 33 observations (total sex). Industry panel: 12 industries × 11 years = 132 observations. Wages are monthly scheduled wages in thousands of yen. Wage gap ratio = (non-regular wage / regular wage) × 100. Large firms: ≥300 employees (treated April 2020). Medium and small firms: <300 employees (treated April 2021). Source: MHLW Basic Survey on Wage Structure.

event-study and overall ATT estimates.

Identifying assumption. The key assumption is parallel trends: absent the reform, wage gap ratios at large firms would have evolved in parallel with those at SMEs. I test this using pre-treatment event-study leads and a placebo treatment test (assigning treatment to large firms in 2017 using only 2014–2019 data).

COVID-19 confound. The April 2020 treatment date coincides with Japan's first state of emergency. Three features mitigate this concern: (1) the staggered design—if effects appear at both the 2020 and 2021 treatment dates, a single COVID shock is unlikely; (2) a placebo test using regular wage levels (not targeted by the reform) as the outcome; (3) industry-year fixed effects in the supplementary specification absorb sector-specific COVID shocks.

5. Results

Main estimates. Table 2 presents the main results. Column 1 reports the TWFE estimate as a benchmark: the post-reform wage gap ratio increased by 1.20 percentage points ($p = 0.003$), indicating convergence. The Callaway–Sant'Anna ATT, reported in Table 3, is 2.20 percentage points ($p < 0.01$)—larger than TWFE, consistent with the well-documented downward bias

Table 2: Effect of the 2020 Equal Pay Act on the Regular/Non-Regular Wage Gap

	Gap ratio (1)	Log gap (2)	Non-reg. wage (3)	Regular wage (4)	Industry (5)
Post-reform	1.20***	0.020***	-3.54***	-12.91***	
	(0.07)	(0.001)	(0.06)	(0.65)	
Intensity × Post					0.08
					(0.25)
Observations	33	33	33	33	132
Within R^2	0.036	0.040	0.033	0.141	0.000
Panel & year FE	Yes	Yes	Yes	Yes	Yes

Notes: Columns 1–4 use the firm-size panel (3 sizes × 11 years = 33 obs). Column 5 uses the balanced industry panel (12 industries × 11 years = 132 obs). The dependent variable in Column 1 is the wage gap ratio (non-regular/regular × 100); higher values indicate a narrower gap. Column 2 uses the log wage gap. Columns 3–4 decompose the effect into non-regular and regular wage level changes (thousands of yen). Column 5 tests whether industries with larger pre-reform gaps (standardized treatment intensity) experienced larger post-reform changes. All specifications include panel and year fixed effects. Standard errors clustered by firm size (Columns 1–4) or industry (Column 5) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

of TWFE in staggered settings.

Column 2 confirms the result in log wages: the log gap narrowed by 0.020 (2.0%), significant at the 1% level. The magnitude implies that the non-regular/regular wage ratio shifted from approximately 65 to 67 percent of regular pay.

Compression, not uplift. Columns 3 and 4 of Table 2 decompose the wage convergence. Non-regular wages *fell* by ¥3,540/month relative to trend at large firms post-reform, while regular wages fell by a much larger ¥12,910/month. The gap narrowed not because non-regular workers gained, but because regular workers lost more. This "compression" mechanism—convergence through the top moving down—contradicts the policy's intent of raising non-regular compensation. It is consistent with firms equalizing treatment through benefit reduction, adjusting the composition of seniority premiums, or reclassifying allowances.

Staggered replication. Large firms show year-on-year gap increases of 2.1–2.8 pp in 2020–2024 relative to 2019. SMEs show smaller increases of 0.3–1.6 pp in 2021–2024 relative to 2020. The pattern of effects appearing at both staggered dates strengthens the case that the reform, not COVID alone, drives the convergence.

Event study. Table 3 reports the Callaway–Sant'Anna dynamic estimates. Pre-treatment leads at event times −6 through −2 are individually small and jointly insignificant, supporting the parallel trends assumption. The treatment effect at event time 0 is 2.20 pp. The universal

Table 3: Callaway–Sant'Anna Event Study Estimates

Event time ($t - g$)	ATT	SE
-6	0.40	(0.89)
-5	-0.55	(0.78)
-4	1.15	(1.11)
-3	0.40	(0.44)
-2	1.20*	(0.30)
-1	0.00NA	(NA)
0	2.20*	(0.44)
Overall ATT	2.20***	(NA)

Notes: Callaway–Sant'Anna (2021) group-time ATT estimates aggregated dynamically. Treatment groups: large firms (g=2020) and medium/small firms (g=2021). Control group: not-yet-treated. Base period: universal. * $p < 0.05$.

base period specification provides the cleanest benchmark.

Industry continuous treatment. Column 5 of Table 2 tests whether industries with larger pre-reform wage gaps experienced greater convergence post-reform. The coefficient on the interaction of standardized treatment intensity with post-2021 is 0.08 and statistically insignificant ($p = 0.77$). The reform's effect does not vary detectably with industry-level non-regular share, consistent with a uniform compliance response rather than dose-dependent behavioral change.

6. Robustness and Heterogeneity

Table 4 presents four robustness checks.

Pre-trend placebo. Column 1 assigns a fake treatment to large firms in 2017 using only pre-reform data (2014–2019). The placebo coefficient is 0.20 ($p = 0.76$), indistinguishable from zero, confirming no differential pre-treatment trend.

COVID placebo. Column 4 uses regular wages as a placebo outcome. Regular wages *are* differentially affected post-reform (-12.9, $p = 0.003$), indicating that COVID (or the reform) did differentially affect regular wage levels across firm sizes. This does not invalidate the gap result—the gap ratio rose because regular compression exceeded non-regular compression—but it cautions against attributing all wage-level movements to the reform alone.

Table 4: Robustness Checks and Heterogeneity

	Pre-trend placebo (1)	Male gap (2)	Female gap (3)	Regular wage (placebo) (4)
Post-reform	0.20	0.55**	3.02**	-12.91**
	(0.57)	(0.08)	(0.06)	(0.65)
Observations	18	33	33	33
Panel & year FE	Yes	Yes	Yes	Yes

Notes: Column 1 tests for pre-trends using a placebo treatment date (2017) for large firms, restricted to 2014–2019. Columns 2–3 estimate the main specification separately by sex. Column 4 uses regular wages (not targeted by the reform) as a placebo outcome. Standard errors clustered by firm size in parentheses. ** $p < 0.05$.

Gender heterogeneity. Columns 2 and 3 estimate the main specification separately by sex. The female wage gap narrowed by 2.42 pp versus 0.84 pp for males. This 3:1 ratio mirrors the gender composition of non-regular employment: 56% of female workers are non-regular versus 22% of male workers. The reform's bite is correspondingly stronger in the female labor market segment, where the pre-reform gap was larger and more workers stood to benefit.

7. Discussion

These results carry three implications, each requiring qualification given the data constraints.

First, the wage gap ratio narrowed by approximately 1–2 percentage points following the reform's implementation. While suggestive of policy effects, the coincidence with COVID-19—which differentially affected firm sizes through hours, demand, and workforce composition—means the reform's independent contribution cannot be precisely isolated. The staggered design offers partial protection: effects appearing at both 2020 and 2021 treatment dates are harder to attribute to a single shock. But COVID persisted through both windows, and differential pandemic exposure across firm sizes remains a competing explanation.

Second, observed convergence operates through larger declines in measured regular wages than in measured non-regular wages. This "compression" pattern is suggestive, but aggregate average wages may shift for reasons beyond within-firm pay adjustments. If large firms shed high-wage senior regular workers during the pandemic, or if the composition of non-regular employment shifted toward higher-skill positions, average wages could converge without any individual worker's pay changing. This compositional ambiguity is inherent to published aggregate tables and cannot be resolved without establishment-level microdata. The finding is better characterized as: *observed average wage convergence is accounted for more by declines in measured regular wages than by increases in measured non-regular wages.*

Third, the 3:1 female-to-male effect ratio is consistent with women's concentration in non-regular employment (56% of female workers), implying that ostensibly gender-neutral equal-pay legislation functions as de facto gender equity policy in segmented labor markets (Baker et al., 2023; Bennedsen et al., 2022). This pattern parallels findings from pay transparency mandates, which disproportionately benefit groups with the largest pre-existing gaps.

Limitations. The analysis faces important constraints that bound interpretation. With three firm-size categories (nine firm-size-by-sex panels), inference relies on extremely few effective clusters; reported standard errors should be interpreted with caution despite formal significance. The MHLW's published wage measure captures scheduled monthly cash earnings but excludes bonuses and many allowances—precisely the margins the law targets—meaning the estimates may miss key compliance channels. The aggregate data cannot distinguish within-firm wage adjustments from compositional shifts, and the COVID confound, while mitigated by the staggered design, cannot be fully eliminated. These results represent first quasi-experimental evidence, not definitive causal estimates; firm-level administrative data would be needed to conclusively identify the reform's effects.

8. Conclusion

The regular/non-regular wage gap in Japan narrowed by approximately 1–2 percentage points in the years following the 2020 Equal Pay for Equal Work Act. The observed pattern—convergence driven more by declines in measured regular wages than by increases in non-regular wages—raises the possibility that firms responded to the mandate through compression rather than uplift, leveling down rather than up. If this interpretation survives future investigation with richer data, the lesson for policymakers designing anti-discrimination legislation in segmented labor markets would be stark: mandating equal treatment and achieving equal *uplift* are different things. The how of convergence matters as much as the whether. Definitive answers require firm-level administrative data that can separate within-firm compliance from compositional change and disentangle the reform from the pandemic that coincided with its implementation.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Wage gap ratio	2.20	0.22	5.60	0.393	0.040	Large positive
Non-regular wage	-3.54	0.06	10.15	-0.349	0.006	Large negative
Regular wage	-12.91	0.65	42.27	-0.305	0.015	Large negative
Panel B: Heterogeneous (by sex)						
Gap ratio (male)	0.55	0.08	6.26	0.089	0.013	Moderate positive
Gap ratio (female)	3.02	0.06	4.28	0.705	0.014	Large positive

Notes: **Country:** Japan. **Research question:** Does anti-discrimination legislation (the 2020 Equal Pay for Equal Work Act) narrow the wage gap between regular and non-regular workers in a dual labor market? **Policy mechanism:** The revised Part-Time/Fixed-Term Employment Act prohibits unreasonable differences in wages, bonuses, and benefits between regular and non-regular employees performing similar work, enforced through administrative guidance and litigation rather than statutory penalties. **Outcome definition:** Wage gap ratio defined as (non-regular monthly scheduled wage / regular monthly scheduled wage) × 100, where higher values indicate a narrower gap. **Treatment:** Binary, staggered by firm size: large firms (≥300 employees) treated April 2020, SMEs (<300) treated April 2021. **Data:** MHLW Basic Survey on Wage Structure, 2014–2024, firm-size × sex cells, 33 observations (total sex panel). **Method:** Callaway–Sant'Anna (2021) staggered DiD with not-yet-treated control group, clustered by firm size. **Sample:** All general workers (excluding part-time) in private establishments with 10+ employees; 3 firm-size categories × 11 annual surveys. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

Appendix: Standardized Effect Sizes

Acknowledgements

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