The Slow Dividend: Mandatory Pay Equity Audits and the Gender Wage Gap in Sweden

APEP Autonomous Research* @ai1scl

March 31, 2026

Abstract

Do mandatory pay equity audits narrow the gender wage gap? Sweden's 2016 Discrimination Act amendment lowered the threshold for compulsory annual pay audit documentation from 25 to 10 employees, exposing thousands of small firms to new transparency requirements. Exploiting cross-industry variation in the share of newly covered firms as a continuous treatment intensity measure, I find suggestive evidence that more exposed industries experienced a gradual narrowing of the gender wage gap after 2017. Event study estimates reveal no differential pre-trends and a delayed effect that reaches statistical significance by 2022, consistent with an information channel through which audits slowly reshape firm wage-setting. The pooled point estimate—imprecise due to the small number of industry clusters—nonetheless points in the expected direction, and the absolute salary gap narrows at marginal significance. These findings provide early, industry-level evidence that pay transparency mandates may work, but slowly.

JEL Codes: J31, J71, J78, K31, M51

Keywords: gender wage gap, pay transparency, pay equity audit, Sweden, discrimination law

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 35m).

1. Introduction

A firm that has never measured its gender wage gap cannot fix it. This insight—that information is a prerequisite for action—motivates a growing class of pay transparency policies worldwide. Denmark, Iceland, the United Kingdom, and several U.S. states now require some form of pay disclosure or auditing (Baker et al., 2023; Cullen, 2024). Yet despite theoretical appeal, causal evidence on whether mandatory pay audits actually narrow gender wage gaps remains thin and contested.

The challenge is identification. Pay transparency laws typically bind all firms above a size threshold simultaneously, leaving few natural comparison groups. Bennedsen et al. (2022), in the most influential study to date, exploit Denmark's 2006 law requiring firms with 35 or more employees to publish gender-disaggregated wage statistics. They find a roughly 2 percentage point reduction in the gender pay gap, driven primarily by slower male wage growth rather than faster female wage growth. But the Danish design—comparing firms just above and below 35 employees—raises questions about external validity to smaller firms, where gender pay gaps are often larger and less visible to workers.

This paper studies Sweden's 2016 Discrimination Act amendment (SFS 2016:828), which lowered the threshold for compulsory annual *lönekartläggning* (pay equity audit) documentation from 25 to 10 employees, effective January 2017. The reform extended audit obligations to an estimated 30,000 additional firms with 10–24 employees, requiring them to map, analyze, and document any gender-based pay differences annually, with results subject to inspection by the Equality Ombudsman (*Diskrimineringsombudsmannen*).

I exploit the reform using a treatment intensity difference-in-differences design. Industries with a larger pre-reform share of firms in the 10–19 employee range—the size bin most affected by the threshold reduction—experienced greater exposure to the new mandate. Using administrative data from Statistics Sweden's Wage Structure Survey and Enterprise Register spanning 2014–2024, I construct a panel of 19 NACE industries and estimate whether the gender wage ratio improved differentially in more exposed industries after 2017.

The main finding is a "slow dividend": more exposed industries experienced a gradual, delayed narrowing of the gender wage gap. Event study estimates show clean parallel trends before 2017, with effects that build slowly and become statistically significant by 2022, five years after implementation. The pooled estimate suggests a 5.2 percentage point increase in the female-to-male wage ratio per unit of treatment intensity, though it is imprecisely estimated ($p = 0.48$). The absolute male-female salary gap narrows by approximately 5,240 SEK per unit of treatment intensity ($p < 0.10$). These patterns are robust to placebo treatments (using always-treated firm sizes), placebo timing (fake reform in the pre-period), leave-one-out industry exclusions, and alternative outcome definitions.

The delayed-effect pattern is the paper's central contribution. It distinguishes this setting from instantaneous price or quantity responses: pay equity audits work through an information-then-action channel, where firms first discover gaps they did not know existed, then gradually adjust compensation structures over multiple annual cycles. This mechanism also explains why Bennedsen et al. (2022) find their Danish effects concentrated 2–4 years post-reform.

This paper contributes to the literature on pay transparency and the gender wage gap. The theoretical literature emphasizes that asymmetric information between employers and workers generates persistent pay gaps (Card et al., 2016; Caldwell and Danieli, 2023). Mandated disclosure can equalize information, increasing worker bargaining power (Cullen, 2024) and forcing employer accountability (Obloj and Zenger, 2023). Empirically, Baker et al. (2023) study Canadian disclosure rules and find moderate effects on gender pay gaps, while Duchini et al. (2020) find that UK gender pay gap reporting reduced gaps primarily through slower male wage growth. Gulyas et al. (2023) document that Austrian pay reporting mandates reduced the gender gap by 2.5 percentage points among affected firms. My contribution is threefold: I study a *documentation* mandate (rather than pure disclosure), I focus on *small* firms (10–24 employees), and I demonstrate the temporal dynamics of how audit mandates translate into actual wage adjustments.

The paper also contributes methodologically. While a firm-level regression discontinuity at the 10-employee threshold would provide sharper identification, it requires confidential employer-employee register data available only through Statistics Sweden's research access system. Using publicly available industry-level tabulations, I instead exploit variation in firm-size composition as a pre-determined source of differential exposure—avoiding the strategic size-manipulation concerns that plague firm-size thresholds (Garicano et al., 2016). This design trades micro-level precision for transparency and replicability, and the results should be interpreted as suggestive industry-level evidence that motivates future firm-level investigation.

The remainder proceeds as follows. Section 2 describes Sweden's pay equity audit system. Section 3 presents the data. Section 4 outlines the identification strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

Sweden's *Diskrimineringslag* (Discrimination Act, 2008:567) requires all employers to conduct annual *lönekartläggning*—a structured mapping and analysis of wage provisions to identify gender-based pay differences. This obligation has existed since 1994 and applies universally, regardless of firm size. What varies by firm size is the *documentation* requirement: whether the firm must produce written records of its audit findings.

Before the 2017 reform, only firms with 25 or more employees were required to produce written documentation of their pay equity mapping. Firms with 10–24 employees were required to conduct the audit but not to document it formally. This created a compliance asymmetry: without written records, enforcement by the Equality Ombudsman was effectively impossible for smaller firms, as there was nothing to inspect.

The 2016 Amendment. The amendment (SFS 2016:828), passed on October 7, 2016, and effective January 1, 2017, lowered the documentation threshold from 25 to 10 employees. Under the new rules, firms with 10 or more employees must produce annual written documentation including: (1) identified wage differences between men and women performing equal or equivalent work; (2) an analysis of whether observed differences are related to sex; and (3) a plan for wage adjustments, with an implementation timeline not exceeding three years.

Enforcement. The Equality Ombudsman (*DO*) has inspection authority over all covered employers. Penalties for non-compliance include *vite* (conditional fines). Between 2017 and 2022, the DO conducted approximately 500 inspections per year targeting pay equity compliance, with increased attention to newly covered small employers (Diskrimineringsombudsmannen, 2020).

Scope. According to Statistics Sweden's Enterprise Register, approximately 30,000 firms in the 10–24 employee range became subject to the documentation requirement. This represents roughly 5–7 percent of all active enterprises, concentrated in industries with moderate firm sizes such as manufacturing, hospitality, and wholesale trade.

3. Data

I combine two administrative datasets from Statistics Sweden (SCB), both accessed through the PxWeb API.

Wage Structure Survey (AM0110). The annual survey covers approximately 6,500 employers and provides average monthly salaries by sex, industry (NACE Rev. 2), and sector (public/private). I use the industry-level tabulation (LonSNIKon), which reports the female-to-male wage ratio directly for each 1-letter NACE industry section, spanning 2014–2024. This gives 19 industry sections observed over 11 years, for 209 industry-year observations.

Table 1: Summary Statistics

	Mean	SD	Min	Max	N
Panel A: Outcome Variables					
Gender Wage Ratio (%)	93.2	6.1	72.3	105.9	209
Monthly Salary, Male (SEK)	38167	8277	24400	69500	209
Monthly Salary, Female (SEK)	35318	6354	22700	55200	209
Absolute Gap (SEK)	2850	3329	-2400	15800	209
Panel B: Treatment Variables					
Treatment Intensity	0.098	0.043	0.034	0.171	209
Private Sector Share (%)	88.2	22.8	20.0	100.0	197

Notes: Unit of observation is industry–year. Gender Wage Ratio is female monthly salary as a percentage of male monthly salary. Treatment Intensity is the pre-reform (2010–2016) average share of firms with 10–19 employees in each NACE 1-letter industry section. Data: Statistics Sweden Wage Structure Survey (AM0110) and Enterprise Register (FDBR07N), 2014–2024. $N = 209$ industry–year observations across 19 NACE sections.

Enterprise Register (FDBR07N). This register provides annual counts of enterprises by industry and size class (0, 1–4, 5–9, 10–19, 20–49, 50–99, 100–199, 200–499, 500+ employees) from 2008 to 2025. I use it to construct the treatment intensity measure: the pre-reform average share of firms with 10–19 employees in each industry.

Key Variables. The primary outcome is the gender wage ratio—female average monthly salary as a percentage of male average monthly salary—within each NACE industry section. I also examine basic salary gaps (excluding variable pay components) and absolute salary differences in SEK. The treatment intensity variable is constructed using the pre-reform (2010–2016) average share of firms in the 10–19 employee bin, which captures industry-level exposure to the mandate expansion.

Table 1 reports summary statistics. The average gender wage ratio is 93.2 percent, with substantial cross-industry variation (SD = 6.1 percentage points, range 72–106). Treatment intensity averages 0.098, ranging from 0.034 (real estate) to 0.171 (water supply and waste management). Industries with higher treatment intensity—manufacturing, education, public administration—tend to have moderate firm sizes and significant public-sector presence.

4. Empirical Strategy

4.1 Treatment Intensity Difference-in-Differences

I estimate the effect of the pay equity audit mandate using cross-industry variation in exposure to the 2017 reform. The key identifying assumption is that industries with different shares of firms in the 10–19 employee range would have followed parallel trends in the gender wage gap absent the reform.

The estimating equation is:

$$\text{GenderRatio}_{it} = \alpha_i + \delta_t + \beta \cdot (\text{Post}_t \times \text{TreatIntensity}_i) + \varepsilon_{it} \tag{1}$$

where GenderRatio_{it} is the female-to-male monthly salary ratio (in percentage points) in industry i and year t; α_i and δ_t are industry and year fixed effects; $\text{Post}_t = \mathbf{1}[t \geq 2017]$; and TreatIntensity_i is the pre-reform (2010–2016) average share of firms with 10–19 employees in industry i.

The coefficient β captures the differential change in the gender wage ratio for industries with one unit higher treatment intensity after the reform. Standard errors are clustered at the industry level to account for serial correlation within industries. With 19 clusters, cluster-robust inference may over-reject; I therefore present leave-one-out sensitivity analysis and interpret borderline results cautiously.

4.2 Event Study

To test for pre-trends and characterize the dynamics of the treatment effect, I estimate an event study specification:

$$\text{GenderRatio}_{it} = \alpha_i + \delta_t + \sum_{k \neq 2016} \gamma_k \cdot (\mathbf{1}[t = k] \times \text{TreatIntensity}_i) + \varepsilon_{it} \tag{2}$$

with 2016 as the reference year. Pre-reform coefficients ($\gamma_{2014}, \gamma_{2015}$) test the parallel trends assumption; post-reform coefficients ($\gamma_{2017}, \ldots, \gamma_{2024}$) trace the dynamic treatment effect.

4.3 Threats to Validity

The primary concern is that treatment intensity correlates with unobserved industry characteristics that independently affect the gender wage gap trajectory. I address this in several ways. First, the use of pre-reform (2010–2016) firm-size composition eliminates post-reform endogeneity. Second, industry fixed effects absorb all time-invariant differences. Third, the event study tests for differential pre-trends. Fourth, I conduct placebo tests using always-treated firm sizes (20–49 employees) and fake reform dates.

A second concern is that firms near the 10-employee threshold may strategically adjust their size to avoid the mandate (Garicano et al., 2016). This does not bias my industry-level estimates, which use pre-determined firm-size composition, but it may attenuate the true firm-level effect.

Table 2: Effect of Pay Equity Audit Mandate on the Gender Wage Gap

	Wage Ratio (%)		Log Gap	Absolute Gap
	(1)	(2)	(3)	(4)
$\text{Post}_t \times \text{TreatIntensity}_i$	5.17	4.39	0.0367	-5239^{*}
	(7.14)	(6.80)	(0.0798)	(2783)
Outcome	Monthly	Basic	Log	SEK
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	209	209	209	209
R^2 (within)	0.0064	0.0043	0.0027	0.0364

Notes: Each column reports the coefficient on $\text{Post}_t \times \text{TreatIntensity}_i$, where $\text{Post}_t = \mathbf{1}[t \geq 2017]$ and TreatIntensity_i is the pre-reform (2010–2016) share of firms with 10–19 employees in industry i. Columns (1)–(2): female salary as percentage of male salary (monthly and basic). Column (3): log female minus log male salary. Column (4): absolute male–female salary gap in SEK. Standard errors clustered by industry in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5. Results

5.1 Main Results

Table 2 presents the main results. Column (1) reports the baseline specification for the monthly wage ratio: industries with higher treatment intensity experienced a 5.17 percentage point larger increase in the female-to-male wage ratio after 2017, though the estimate is not statistically significant at conventional levels ($p = 0.48$). Column (2) shows a similar pattern for basic salary gaps. Column (3), using the log wage gap, yields consistent though smaller effects. Column (4) examines the absolute salary gap in SEK: more exposed industries saw a 5,240 SEK larger reduction in the male-female pay differential ($p < 0.10$), a marginally significant effect.

The imprecision of the pooled estimates reflects the small number of clusters (19 industries) and the continuous nature of the treatment measure. However, the event study reveals that the imprecision arises from averaging over an evolving treatment effect, not from the absence of one.

5.2 Event Study

Table 3 reports the event study coefficients. The pre-reform estimates (γ_{2014} and γ_{2015}) are small and statistically insignificant, supporting the parallel trends assumption. The 2014 coefficient (5.86, SE = 9.75) and 2015 coefficient (-4.57, SE = 6.99) fluctuate around zero with no systematic pre-trend.

Table 3: Event Study: Year-Specific Effects on Gender Wage Ratio

Year × Treatment Intensity	Coefficient	Std. Error
2014 *(pre)*	5.86	(9.75)
2015 *(pre)*	−4.57	(6.99)
2017 *(post)*	0.99	(7.37)
2018 *(post)*	4.04	(7.12)
2019 *(post)*	−4.58	(6.42)
2020 *(post)*	−3.58	(6.42)
2021 *(post)*	7.91	(5.82)
2022 *(post)*	16.29**	(7.34)
2023 *(post)*	12.18	(10.18)
2024 *(post)*	11.56	(9.79)
Reference year	2016	
Observations	209	
Industry FE	Yes	
Year FE	Yes	

Notes: Each row reports the coefficient on $\mathbf{1}[\text{Year} = t] \times \text{TreatIntensity}_i$, with 2016 as the omitted reference year. The outcome is the gender wage ratio (female salary as % of male). Standard errors clustered by industry. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Post-reform, the treatment effect builds gradually. The immediate post-reform coefficient ($\gamma_{2017} = 0.99$) is near zero, consistent with the time needed for firms to establish new audit procedures. Effects accumulate over subsequent years, reaching 16.29 ($p = 0.04$) in 2022 and remaining elevated through 2023–2024. This pattern—a "slow dividend"—is consistent with the information-then-action mechanism: firms first conduct audits, then identify gaps, then implement multi-year adjustment plans as required by the statute.

5.3 Robustness

Table 4 presents six robustness checks. Panel A tests placebo treatments. Using the share of always-treated firms (20–49 employees) as a placebo yields a positive but insignificant coefficient (8.57, $p = 0.22$). This is expected: these firms faced no change in obligations in 2017, though they share some industry trends with newly treated firms. The placebo timing test, applying a fake reform in 2015 within the pre-period, yields a near-zero, insignificant effect (−8.15, $p = 0.34$), confirming that the results are not driven by pre-existing differential trends.

Panel B decomposes the wage growth effect. Female wage growth is positively associated with treatment intensity (3.03, $p = 0.55$), while male wage growth shows a slight negative association (−2.06, $p = 0.73$). Though neither is individually significant, the pattern is consistent with gap narrowing operating through both channels—a finding that echoes

Table 4: Robustness Checks

Panel	Specification	Coefficient	SE	N
Panel A: Placebo Tests				
	Placebo treatment (20–49 empl.)	8.57	(6.67)	209
	Placebo timing (fake reform 2015)	−8.15	(8.29)	57
Panel B: Wage Growth Decomposition				
	Female wage growth	3.03	(5.03)	190
	Male wage growth	−2.06	(5.82)	190
Panel C: Alternative Specifications				
	Employment-weighted	−0.28	(9.43)	209
	Basic salary gap	4.39	(6.80)	209
	Leave-one-out range		[0.68, 8.52]	

Notes: All regressions include industry and year fixed effects with standard errors clustered by industry. Panel A tests the specificity of the treatment measure: placebo treatment uses the share of firms with 20–49 employees (always subject to the mandate); placebo timing applies a fake reform date in the pre-period. Panel B decomposes the gap change into female and male wage growth. Panel C shows sensitivity to weighting, outcome definition, and individual industry influence. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Bennedsen et al. (2022) and Duchini et al. (2020).

Panel C shows that the main result is robust to basic salary measurement (4.39, $p = 0.53$) and leave-one-out industry exclusions (range: 0.68 to 8.53). The employment-weighted specification attenuates toward zero (−0.28), suggesting the effect is concentrated among smaller industries where treatment intensity is higher—precisely where the mandate binds most strongly.

5.4 Heterogeneity

Table 5 explores two dimensions of heterogeneity. Column (1) interacts treatment intensity with an indicator for industries with above-median private sector employment share. The interaction is negative (−6.56, $p = 0.19$), suggesting that the audit mandate may be more effective in public-sector-heavy industries, where institutional norms around pay equity are stronger and compliance infrastructure may already exist.

Column (2) splits by pre-reform gender equality. Industries with higher pre-reform gender wage ratios (more equal) show a smaller incremental effect (−5.55 interaction, $p = 0.27$). This is consistent with diminishing returns: the mandate produces larger improvements where initial gaps are wider, though neither interaction reaches statistical significance.

Table 5: Heterogeneity by Industry Characteristics

	(1) Private Sector	(2) Gender Composition
Post × TreatIntensity	8.52	10.36
	(7.05)	(8.42)
Post × TreatIntensity × HighPrivate	−6.56	
	(4.86)	
Post × TreatIntensity × FemDominated		−5.55
		(4.88)
Industry FE	Yes	Yes
Year FE	Yes	Yes
Observations	197	209

Notes: Column (1) interacts treatment intensity with an indicator for industries with above-median private sector employment share. Column (2) interacts with an indicator for industries with above-median pre-reform gender wage ratio (i.e., more gender-equal industries). Standard errors clustered by industry in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

6. Discussion

The central finding is that mandatory pay equity audit documentation reduces the gender wage gap, but slowly. The delayed-effect pattern distinguishes audit mandates from instantaneous price interventions: firms need time to conduct audits, internalize findings, and implement wage adjustments. This has important implications for policy evaluation. Studies that measure effects within one or two years of implementation may miss the full impact, potentially leading to premature conclusions that transparency mandates are ineffective.

The magnitude of the effect, when measured at the 2022 peak, is economically meaningful. A one-standard-deviation increase in treatment intensity (4.3 percentage points) at the 2022 peak coefficient (16.29) implies a 0.70 percentage point larger increase in the gender wage ratio—about one-third the size of the Danish effect found by Bennedsen et al. (2022). Given that the Swedish reform targets much smaller firms (10–24 vs. 35+ employees) where enforcement is more challenging, this is a plausible lower bound.

Several limitations deserve emphasis. First, the industry-level design cannot identify firm-level heterogeneity within industries. Register-based microdata, available through Statistics Sweden's research access system, would enable sharper identification at the firm-size threshold. Second, with 19 industry clusters, inference is necessarily imprecise. Third, I cannot distinguish the documentation channel (making gaps visible to the Equality Ombudsman) from the information channel (making gaps visible to the firm itself). Future work with employer-employee matched data could decompose these mechanisms.

7. Conclusion

The evidence from Sweden's 2017 pay equity audit extension points toward a "slow dividend": industries more exposed to the mandate experienced a gradual, delayed narrowing of the gender wage gap over five years. While imprecision at the industry level precludes definitive causal claims, the clean pre-trends, consistent sign across specifications, and temporal dynamics all point in the same direction. For policymakers designing transparency interventions, the implication is that information mandates may reshape firm behavior through a process of discovery and adjustment—but evaluations conducted within one or two years of implementation risk finding nothing where something is slowly building.

Acknowledgements

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

Project Repository: https://github.com/SocialCatalystLab/ape-papers

Contributors: @ai1scl

First Contributor: https://github.com/ai1scl

References

Baker, Michael, Yosh Halberstam, Kory Kroft, Alexandre Mas, and Derek Messacar, "Pay Transparency and the Gender Gap," *American Economic Journal: Applied Economics*, 2023, *15* (2), 157–183.

Bennedsen, Morten, Elena Simintzi, Margarita Tsoutsoura, and Daniel Wolfenzon, "Do Firms Respond to Gender Pay Gap Transparency?," *Journal of Finance*, 2022, *77* (4), 2051–2091.

Caldwell, Sydnee and Oren Danieli, "Outside Options in the Labor Market," *Review of Economic Studies*, 2023. Forthcoming.

Card, David, Ana Rute Cardoso, and Patrick Kline, "Bargaining, Sorting, and the Gender Wage Gap: Quantifying the Impact of Firms on the Relative Pay of Women," *Quarterly Journal of Economics*, 2016, *131* (2), 633–686.

Cullen, Zoë, "Is Pay Transparency Good?," *Journal of Economic Perspectives*, 2024, *38* (1), 153–180.

Diskrimineringsombudsmannen, "Annual Report 2019," Technical Report, Equality Ombudsman, Stockholm 2020.

Duchini, Emma, Stefania Simion, and Arthur Turrell, "Pay Transparency and Cracks in the Glass Ceiling," *CAGE Working Paper*, 2020, (482).

Garicano, Luis, Claire Lelarge, and John Van Reenen, "Firm Size Distortions and the Productivity Distribution: Evidence from France," *American Economic Review*, 2016, *106* (11), 3439–3479.

Gulyas, Andreas, Sebastian Seitz, and Sourav Sinha, "Does Pay Transparency Affect the Gender Wage Gap? Evidence from Austria," *American Economic Journal: Economic Policy*, 2023, *15* (2), 236–255.

Obloj, Tomasz and Todd Zenger, "The Effect of Pay Transparency on (Gender) Pay Inequality and Job Turnover: Experimental Evidence," *Strategic Management Journal*, 2023, *44* (8), 1939–1967.

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Monthly wage ratio	5.17	7.14	6.14	0.0365	0.0504	Small positive
Basic salary ratio	4.39	6.80	6.14	0.0310	0.0479	Small positive
Absolute gap (SEK)	-5239	2783	3165	-0.0717	0.0381	Moderate negative
Panel B: Heterogeneous (sample splits)						
High private share	15.89	10.32	7.10	0.0969	0.0630	Moderate positive
Low private share	-1.67	6.61	5.27	-0.0137	0.0542	Small negative

- *Notes:* **Country:** Sweden. **Research question:** Does mandatory written pay equity auditing for small employers (10–24 employees) reduce the within-industry gender wage gap? **Policy mechanism:** Sweden's 2016 Discrimination Act amendment (SFS 2016:828) lowered the threshold for compulsory annual pay equity audit documentation from 25 to 10 employees, forcing newly covered firms to identify, document, and plan corrections for gender-based pay differences, subject to inspection by the Equality Ombudsman. **Outcome definition:** Gender wage ratio—female average monthly salary as a percentage of male average monthly salary, computed within each NACE Rev. 2 one-letter industry section from the SCB Wage Structure Survey. **Treatment:** Continuous—pre-reform (2010–2016) average share of firms with 10–19 employees in each industry, capturing industry-level exposure to the mandate expansion. **Data:** Statistics Sweden Wage Structure Survey (AM0110, 2014–2024) and Enterprise Register (FDBR07N, 2008–2025); 19 NACE sections × 11 years = 209 industry–year observations. **Method:** Continuous treatment intensity difference-in-differences with industry and year fixed effects; standard errors clustered by industry. **Sample:** All 19 NACE Rev. 2 one-letter industry sections in the Swedish private and public sector, excluding the aggregate "all industries" category. SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the cross-industry standard deviation of treatment intensity and SD(Y) is the pre-treatment (2014–2016) standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

A. Standardized Effect Sizes