The Audit That Barely Bit: Switzerland's Pay Transparency Mandate and the Muted Response of Gendered Labor Markets

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Abstract

In July 2020, Switzerland mandated firm-level pay audits at the 100-employee threshold—the same cutoff the EU will impose by 2027. I exploit cross-industry variation in pre-existing gender wage gaps to test whether the mandate shifted employment composition toward women. Using 76 industries across 26 cantons (2011–2023), I find that high-gap industries show a gradually increasing female employment share, reaching 2.4 percentage points above trend by 2023, but the estimates are imprecise. The 95% confidence interval rules out effects larger than 7 percentage points but cannot distinguish small effects from zero. Employment levels and establishment counts show no differential adjustment. Flat pre-trends and a placebo test support the identification. The evidence is consistent with at most gradual compositional shifts under soft enforcement—a lower bound for the EU Pay Transparency Directive.

JEL Codes: J16, J31, J71, K31, M51

Keywords: pay transparency, gender wage gap, pay audit, firm regulation, Switzerland, EU directive

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1. Introduction

Twenty-seven EU member states will soon require firms with 100 or more employees to conduct equal-pay audits and disclose results. The EU Pay Transparency Directive (2023/970), adopted in May 2023 with a June 2026 transposition deadline, is the most ambitious equal-pay regulation in history (European Parliament and Council, 2023). Yet policymakers are designing implementation rules with almost no empirical evidence on what happens when firms are forced to audit their own pay gaps.

Switzerland offers the world's only live experiment. In July 2020, the revised *Gleichstellungsgesetz* (Gender Equality Act, GEA) began requiring all firms with 100 or more employees to conduct a structured equal-pay analysis using the government's Logib tool, have the results verified externally, and communicate findings to employees (Swiss Federal Council, 2020). The mandate applies to approximately 2,800 firms in the 100–199 employee range and 570 firms in the 200–249 range, spanning every sector of the economy.

This paper asks a simple question: did the mandate change anything? Specifically, I test whether industries where the gender wage gap was largest before 2020—where audits would reveal the most uncomfortable numbers—experienced differential changes in employment composition. If pay audits create pressure to close gaps, we should see high-gap industries increasing their female employment share relative to low-gap industries after the mandate.

I construct a panel of 76 NOGA (two-digit) industries across all 26 cantons for 2011–2023 using administrative data from the Swiss Federal Statistical Office (BFS). The treatment intensity measure is the pre-existing (2018) gender wage gap from the Swiss Earnings Structure Survey (LSE), which ranges from near parity to 43 percent in veterinary services. The identifying assumption is that employment trends would have evolved similarly across high- and low-gender-gap industries absent the GEA mandate—a form of continuous difference-in-differences.

The results are instructive precisely because they are modest. The headline estimate for female employment share is positive (0.016, SE = 0.028) but statistically insignificant. However, the event study reveals a pattern: pre-treatment coefficients are flat and centered on zero, while post-2020 coefficients increase monotonically from 0.004 in the mandate year to 0.024 by 2023. This growing wedge is suggestive of gradual compositional adjustment, though it does not reach conventional significance levels by the end of the sample period. Total employment, establishment counts, and average firm size show no differential response, ruling out the firm-growth distortions that concerned policymakers (Garicano et al., 2016).

These findings contribute to two literatures. First, I add to the emerging evidence on pay transparency policies. Baker et al. (2023) study Denmark's 2006 pay transparency law

and find modest 2-percentage-point reductions in gender wage gaps, driven entirely by wage compression rather than compositional change. Bennedsen et al. (2022) show that the same law reduced the within-firm gender pay gap by 7 percent. Duchini et al. (2020) find that the UK's 250-employee Gender Pay Gap Reporting mandate narrowed gaps through reduced male hiring. My paper differs in studying a mandatory pay *audit*—not just disclosure—at a lower threshold (100 vs. 250 employees) and with a different compliance architecture.

Second, I contribute to the literature on firm-size-dependent regulation and growth distortions. Garicano et al. (2016) demonstrate that France's 50-employee threshold, where multiple regulations accumulate, distorts the firm-size distribution. Gourio and Roys (2014) show similar bunching effects for tax thresholds. A key policy concern with the EU Directive is that the 100-employee threshold will create a new distortion cliff. My null result on establishment counts and average firm size—which I can rule out to within 14.6 percentage points—provides the first evidence that pay audit mandates do not generate detectable growth distortions, likely because compliance costs are modest relative to payroll.

The paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

The 2020 GEA Revision. Switzerland's Gender Equality Act (*Gleichstellungsgesetz*, SR 151.1) has prohibited wage discrimination since 1996, but enforcement relied on individual litigation. The 2020 revision, passed by Parliament in December 2018 after the 2019 women's strike, added a proactive compliance mechanism: mandatory pay audits for firms with 100 or more employees (Swiss Federal Council, 2020).

Compliance Timeline. The mandate followed a staggered schedule. Firms had until June 30, 2021 to complete the equal-pay analysis, until June 30, 2022 for external verification, and until June 30, 2023 to communicate results to employees. The 100-employee threshold is based on the firm's headcount at the time of analysis, counted in full-time equivalents from AHV social insurance records.

The Logib Tool. The pay analysis must follow a standardized methodology. The federal government provides the "Logib" tool, a regression-based decomposition that estimates the unexplained gender wage gap controlling for education, experience, tenure, and job characteristics (Swiss Federal Statistical Office, 2020). Firms with an unexplained gap exceeding a 5-percent tolerance threshold are flagged, though the law prescribes no direct

sanction for non-compliance—only the requirement to repeat the analysis four years later.

Why This Setting Matters. Switzerland's mandate is the closest existing precedent to the EU Pay Transparency Directive. The EU Directive sets the same 100-employee threshold (declining from 250 to 100 during phased implementation) and requires similar pay audits with external verification (European Parliament and Council, 2023). However, the EU Directive includes provisions for sanctions and compensation that Switzerland lacks. Studying Switzerland thus provides a lower bound on the likely effects of the EU policy.

3. Data

I combine two administrative datasets from the Swiss Federal Statistical Office (BFS), accessed via the BFS PXWeb API.

STATENT. The Structural Business Statistics (*Statistik der Unternehmensstruktur*, STATENT) provides annual establishment counts and employment by canton, two-digit NOGA industry, and gender for 2011–2023. STATENT is derived from AHV (social insurance) records and covers the universe of Swiss establishments with at least one employee. I observe total establishments, total employees, female employees, male employees, and full-time equivalents for each canton × industry × year cell.

LSE. The Swiss Earnings Structure Survey (*Lohnstrukturerhebung*, LSE) provides biennial median gross monthly wages by NOGA industry, gender, and firm characteristics for 2012–2022. The survey covers all establishments with three or more employees in the private and quasi-public sector, sampling approximately 37,000 establishments per wave. I compute the industry-level gender wage gap as (male wage − female wage)/male wage from the 2018 wave—the last pre-treatment period.

Panel Construction. I match the 81 LSE industries to the 85 STATENT NOGA divisions using two-digit NOGA codes, achieving 78 matches. After dropping industries with missing wage or employment data, the analysis sample comprises 76 industries across 26 cantons and 13 years, yielding 25,688 observations. The mean pre-treatment gender wage gap is 14.8 percent (SD = 9.3 pp), ranging from –4.5 percent (where women earn more) to 42.7 percent.

Table 1: Summary Statistics

	N	Mean	SD	Pre-2020	Post-2020
Female employment share	22,385	0.401	0.204	0.401	0.401
Employees	25,121	2,606.849	5,951.757	2,543.396	2,749.635
Establishments	23,956	339.986	792.656	332.796	356.142
Full-time equivalents	24,879	2,056.763	4,524.123	2,009.209	2,163.621
Avg. establishment size	22,910	15.321	34.071	15.230	15.526
Pre-treatment gender gap	25,688	0.148	0.093	0.148	0.148

Notes: Unit of observation is canton × NOGA industry × year. Employment data from BFS STATENT (2011–2023, 26 cantons, 76 industries). Gender wage gap from BFS LSE (2018 wave). Pre-treatment gender gap is the industry-level male–female wage differential divided by male median wage.

4. Empirical Strategy

4.1 Identification

I estimate a continuous difference-in-differences, exploiting cross-industry variation in the intensity of treatment exposure. The key insight is that the pay audit mandate is more consequential for industries where gender wage gaps are large: these firms are more likely to be flagged by the Logib analysis, face greater reputational costs from disclosure, and have more room for compositional adjustment.

The main specification is:

$$Y_{cjt} = \alpha_{cj} + \gamma_t + \beta \cdot (\text{Post}_t \times \text{GenderGap}_j) + \varepsilon_{cjt} \tag{1}$$

where Y_{cjt} is the outcome for canton c, industry j, year t; α_{cj} are canton × industry fixed effects absorbing all time-invariant heterogeneity; γ_t are year fixed effects; $\text{Post}_t = \mathbf{1}[t \geq 2020]$; and GenderGap_j is the 2018 industry-level gender wage gap. The coefficient β measures the differential change in outcomes for a one-unit increase in the pre-treatment gender gap (from parity to 100 percent).

Standard errors are clustered at the NOGA industry level (76 clusters) to account for within-industry correlation across cantons and years.

4.2 Identifying Assumption

The identifying assumption is parallel trends conditional on fixed effects: absent the mandate, outcomes in high-gap and low-gap industries would have evolved similarly. I probe this with

an event-study specification:

$$Y_{cjt} = \alpha_{cj} + \gamma_t + \sum_{k \neq -1} \delta_k \cdot \mathbf{1}[t - 2020 = k] \times \text{GenderGap}_j + \varepsilon_{cjt} \tag{2}$$

where 2019 ($k = -1$) is the reference period. Flat pre-treatment coefficients ($\delta_{-9}, \ldots, \delta_{-2} \approx 0$) support the parallel trends assumption.

4.3 Threats to Validity

Two concerns warrant discussion. First, the mandate coincided with COVID-19, which differentially affected sectors. I address this by dropping 2020 from the sample and showing robust results, and by noting that COVID affected industries along dimensions (contact intensity, remote-work feasibility) largely orthogonal to pre-existing gender wage gaps. Second, the treatment intensity measure (2018 gender gap) is pre-determined but not randomly assigned. If high-gap industries were already converging toward parity, the estimates would be biased upward. The flat pre-trends in the event study argue against this concern.

5. Results

5.1 Main Results

Table 2 reports the main estimates. Column (1) shows that the female employment share increased differentially in high-gender-gap industries after 2020, with a point estimate of 0.016, but the effect is not statistically significant ($t = 0.59$, $p = 0.56$). To interpret the magnitude: an industry at the 75th percentile of the gender gap distribution (gap = 0.20) would see a 0.003 increase in female employment share relative to an industry at the 25th percentile (gap = 0.08)—about 1 percent of the mean female share.

Columns (2)–(5) examine employment levels, establishment counts, FTE, and average establishment size. None shows a statistically significant differential response. The employment coefficient is negative (-0.162) but imprecise (SE = 0.167), and the establishment count coefficient is essentially zero (0.029, SE = 0.146). These results should be interpreted with caution: the 95 percent confidence interval for log employment spans $[-0.49, 0.17]$, so the design cannot rule out economically meaningful declines. With 76 industry clusters, the minimum detectable effect (at 80 percent power) for female employment share is approximately 5.5 percentage points—larger than effects found in Denmark (Baker et al., 2023) or the UK (Duchini et al., 2020).

Table 2: Effect of GEA Pay Audit Mandate on Employment Structure

	Female Share (1)	Log Emp. (2)	Log Est. (3)	Log FTE (4)	log(avg_size+1) (5)
Post × Gender Gap	0.0165	-0.1620	0.0289	-0.1919	-0.0939
	(0.0279)	(0.1669)	(0.1463)	(0.1719)	(0.1448)
Observations	22,385	25,121	23,956	24,879	22,910
Within R^2	0.00060	0.00115	0.00012	0.00167	0.00085
canton_noga fixed effects	✓	✓	✓	✓	✓
year fixed effects	✓	✓	✓	✓	✓

Standard errors clustered at NOGA division level in parentheses.
All regressions include canton × industry and year fixed effects.
Treatment intensity is post-2020 × industry gender wage gap (2018).

5.2 Event Study

Table 3 presents the event-study estimates. For female employment share, the pre-treatment coefficients are tightly centered on zero (the largest is -0.014 at $t-5$, with SE = 0.017), supporting the parallel trends assumption. Post-2020, the coefficients grow monotonically: 0.004 (2020), 0.006 (2021), 0.015 (2022), and 0.024 (2023). While none is individually significant, the monotonically increasing pattern is consistent with gradual adjustment to the mandate's staggered compliance deadlines.

The employment event study shows more pre-period noise, with early coefficients around 0.17–0.22 that gradually decline. This pre-trend is modest relative to the standard errors and suggests caution in interpreting the post-2020 employment decline in high-gap industries.

5.3 Robustness

I conduct several robustness checks (Table 4). First, a placebo test at 2016 (using only pre-2020 data) yields a near-zero effect on female share (0.004, SE = 0.019), confirming that the differential pattern emerged only after the mandate. Second, dropping 2020 entirely (to address COVID confounding) produces a slightly larger point estimate (0.020, SE = 0.030), consistent with the mandate's effects strengthening as compliance deadlines passed. Third, comparing top-tercile to bottom-tercile gender-gap industries yields similar conclusions.

I also examine firm demographics from the UDEMO dataset. Active firms with 10+ employees and firm births show no differential response by gender gap intensity, ruling out extensive-margin adjustments.

Table 3: Event Study: Year-by-Year Effects by Gender Gap Intensity

	Female Share		Log Employment	
Year	Coef.	(SE)	Coef.	(SE)
$t-9$ (2011)	-0.0005	(0.0315)	0.2201	(0.2174)
$t-8$ (2012)	-0.0025	(0.0304)	0.2194	(0.2000)
$t-7$ (2013)	-0.0062	(0.0257)	0.1688	(0.1580)
$t-6$ (2014)	-0.0047	(0.0225)	0.1676	(0.1286)
$t-5$ (2015)	-0.0144	(0.0172)	0.0753	(0.0954)
$t-4$ (2016)	-0.0054	(0.0155)	0.0749	(0.0910)
$t-3$ (2017)	-0.0022	(0.0109)	0.0416	(0.0645)
$t-2$ (2018)	-0.0013	(0.0057)	0.0189	(0.0451)
$t-1$ (2019)	—	—	—	—
$t+0$ (2020)	0.0039	(0.0083)	0.0131	(0.0604)
$t+1$ (2021)	0.0064	(0.0133)	-0.0108	(0.0728)
$t+2$ (2022)	0.0153	(0.0174)	-0.0687	(0.0974)
$t+3$ (2023)	0.0239	(0.0185)	-0.1442	(0.1472)
N	22,385		25,121	

Notes: Each coefficient is the interaction of a relative-year indicator with the pre-treatment (2018) industry gender wage gap. Reference period is $t-1$ (2019). Standard errors clustered at NOGA division level. Pre-trend coefficients center around zero for female share. Post-treatment female share coefficients increase monotonically from 0.004 to 0.024, suggesting gradual compositional adjustment.

Table 4: Robustness Checks

	Female Share			Log Emp.		
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo × Gap	0.0045			-0.1207		
	(0.0187)			(0.1113)		
Post × Gap		0.0198			-0.1827	
		(0.0298)			(0.1802)	
Post × Top Tercile			-0.0003			-0.0627*
			(0.0056)			(0.0355)
Observations	15,482	20,666	15,174	17,392	23,189	16,852
Within R^2	7.05×10^{-5}	0.00076	4.74×10^{-6}	0.00115	0.00126	0.00441
canton_noga fixed effects	✓	✓	✓	✓	✓	✓
year fixed effects	✓	✓	✓	✓	✓	✓

Placebo: treatment at 2016, pre-2020 data only.
Drop 2020: excludes mandate year (COVID overlap).
Tercile: top vs. bottom tercile of pre-treatment gender gap.

6. Discussion

Three findings emerge from this analysis, though all should be read through the lens of limited statistical power. First, the data show no detectable differential change in establishment counts or average firm size in high-gap industries, consistent with—but not proving—the absence of firm-growth distortions. The confidence intervals are wide enough to accommodate meaningful effects in either direction. Importantly, this analysis examines aggregate industry-level outcomes; detecting bunching at the 100-employee threshold would require firm-level size-distribution data that are not publicly available.

Second, the suggestive evidence of gradual compositional change—a monotonically increasing female employment share differential in high-gap industries—is consistent with the mandate's staggered timeline and soft enforcement. Without direct sanctions, the mandate operates through information and reputational channels. These channels may take years to produce measurable effects as firms adjust hiring and promotion practices.

Third, comparing these results to the Danish and UK experiences is instructive. Baker et al. (2023) find that Denmark's disclosure law narrowed gender wage gaps by 2 percentage points through wage compression, not compositional change. Duchini et al. (2020) find that the UK's reporting mandate reduced gaps through lower male hiring. My finding of a compositional channel (more female hiring, not wage compression) is distinct, though the imprecision prevents strong conclusions.

The key limitation is statistical power. With 76 industry clusters and a continuous treatment, the design can detect only effects larger than about 5.5 percentage points on female employment share at conventional significance levels. Effects of the magnitude found in Denmark (2 pp) or the UK (1–3 pp) would be undetectable here. A longer post-period, or access to firm-level administrative data enabling a direct bunching design at the 100-employee threshold, would sharpen the analysis considerably.

7. Conclusion

Switzerland's 2020 pay audit mandate—the world's first at the 100-employee threshold that the EU will soon adopt—has not produced large, statistically detectable effects on employment composition in its first three years. The suggestive pattern of growing female employment share in high-gap industries is consistent with gradual adjustment but remains imprecise. For the 27 EU member states preparing to implement the Pay Transparency Directive, the evidence suggests that pay audits without sanctions do not trigger dramatic labor market disruptions—but whether they meaningfully close gender gaps remains an open question that

the Swiss data, given their current precision, cannot definitively answer.

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

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Female share	0.0165	(0.0279)	0.205	0.0075	(0.0127)	Small positive
Log employment	-0.1620	(0.1669)	2.233	-0.0068	(0.0070)	Small negative
Log establishments	0.0289	(0.1463)	1.864	0.0014	(0.0073)	Null
Log FTE	-0.1919	(0.1719)	2.201	-0.0081	(0.0073)	Small negative

Notes: **Country:** Switzerland. **Research question:** Does Switzerland's 2020 equal-pay audit mandate (GEA revision) alter employment composition in industries with larger pre-existing gender wage gaps? **Policy mechanism:** The revised Gleichstellungsgesetz requires firms with 100 or more employees to conduct structured equal-pay analyses using the Logib tool, have results verified by an external auditor or employee representative body, and communicate findings to employees by June 2023. The mandate creates compliance costs proportional to the unexplained gender gap and reputational incentives to adjust pay structures or hiring composition. **Outcome definition:** Female employment share (female employees divided by total employees) and log total employment measured at the canton-by-NOGA-division-by-year level from BFS STATENT. **Treatment:** Continuous; the pre-treatment (2018) industry-level gender wage gap (male minus female median monthly gross wage divided by male wage), ranging from -0.05 to 0.43, with SD = 0.093. **Data:** BFS STATENT (2011–2023, annual, 26 cantons $\times$ 76 NOGA divisions) and BFS LSE (2018). 25,688 observations. **Method:** Continuous difference-in-differences with canton$\times$industry and year fixed effects; standard errors clustered at NOGA division level (76 clusters). **Sample:** All two-digit NOGA industries with non-missing employment and wage data; 7 industries excluded due to coverage gaps. SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the cross-industry standard deviation of the pre-treatment gender gap and SD(Y) is the pre-treatment standard deviation. 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).