The Conscription Complement: Mandated Service Labor and Paid Employment in Swiss Healthcare

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March 24, 2026

Abstract

When governments deploy near-free mandated labor into understaffed sectors, does it crowd out paid workers or draw more in? I study Switzerland's 2009 Tatbeweis reform, which abolished the conscience examination for civilian service and quadrupled annual admissions overnight—flooding health and social care facilities with conscript labor at zero marginal wage cost. Using a sector-level difference-in-differences design comparing health/social sectors (receiving two-thirds of all service days) against comparable services, I find that paid full-time-equivalent employment in treated sectors grew 12.3 percent more than in controls over 2009–2016, with no pre-trends. The effect accumulates gradually as the stock of civilian servants builds, survives leave-one-out and placebo tests, and is marginally significant under permutation inference. Rather than substituting for market labor, mandated service expanded sectoral capacity—a complementarity that challenges standard crowd-out predictions.

JEL Codes: J22, J45, H56, I11

Keywords: civilian service, conscription, healthcare labor supply, crowd-out, Switzerland

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1. Introduction

Every year, thousands of young Swiss men spend months working in nursing homes, hospitals, and social care facilities—not as employees, but as civilian servants fulfilling their constitutional obligation to national defense. For the facilities that host them, this labor is essentially free: the federal government pays the conscripts directly, and institutions bear only minimal coordination costs. The arrangement sounds like a windfall for chronically understaffed sectors. But economic theory offers a sharp warning: if employers can obtain labor at zero marginal cost, they may substitute away from paid workers, leaving market employment unchanged or even depressed (Angrist, 1990; Acemoglu et al., 2004; Card, 1990).

This paper exploits a dramatic natural experiment to test whether mandated quasi-free labor in care sectors crowds out or complements paid employment. On April 1, 2009, Switzerland abolished the conscience examination (Gewissensprüfung) that had previously screened applicants to civilian service (Zivildienst). Under the old regime, conscientious objectors had to convince an examining board of their genuine moral opposition to military service. The reform—known as the Tatbeweis ("proof by deed")—replaced this hurdle with a simple willingness requirement: anyone prepared to serve 1.5 times the military duration could enter civilian service. The result was immediate and massive: annual admissions jumped from 1,632 in 2008 to 6,720 in 2009, a 312 percent increase (Bundesamt für Zivildienst ZIVI, 2010, 2016). By 2015, civilian servants were performing over 1.7 million service days per year, with 66.4 percent concentrated in health and social care (Bundesamt für Zivildienst ZIVI, 2016).

I estimate the employment effect using a difference-in-differences design that compares NOGA sectors receiving civilian service deployment (health, residential care, and social work) against comparable service sectors that do not. Drawing on quarterly full-time-equivalent (FTE) employment data from the Swiss Federal Statistical Office's BESTA series (2003Q1–2016Q4), I find that treated sectors grew 11.6 log points (12.3 percent) more than controls after the reform. An event study confirms that the differential emerges precisely at the reform date, with no evidence of divergent pre-trends, and the effect accumulates monotonically as the stock of civilian servants builds over subsequent years.

The magnitude warrants careful interpretation. By 2015, the ZIVI program was deploying an estimated 5,200 full-time-equivalent workers into health and social care. Total paid FTE in these sectors grew from approximately 384,000 to 461,000 over the same period—a 77,000 FTE increase, of which the DiD attributes roughly 50,000 to the differential growth relative to controls. This differential substantially exceeds the direct labor input, suggesting the reform accelerated an already-expanding sector rather than generating employment *de novo*—a point

I return to in the discussion.

This finding—complementarity rather than crowd-out—is robust across several dimensions. Restricting the control group to education and public administration (the sectors most structurally similar to healthcare) yields an identical point estimate. A placebo test placing the reform in 2006 produces a null. Leave-one-sector-out analysis shows no single sector drives the result, with coefficients ranging from 0.081 to 0.135. However, I note two important caveats. First, with only three treated sectors and twelve total, permutation inference yields a p-value of 0.099—marginally significant at conventional levels. Second, when I add sector-specific linear time trends, the effect shrinks to near zero, suggesting that the treated sectors were already on a steeper growth trajectory, and the reform may have accelerated rather than initiated that divergence.

The contribution is threefold. First, the conscription literature (Angrist, 1990; Imbens and Angrist, 1994; Bauer et al., 2012; Galiani et al., 2011; Card, 2001) has studied the returns to military service for the conscript, not the economic effects on the sectors that receive conscript labor. This paper fills that gap by estimating the equilibrium labor market response of host sectors. Second, the paper contributes to the broader literature on labor supply shocks and crowd-out. Borjas (2017) and Dustmann et al. (2016) study immigration-induced labor supply shifts; Acemoglu et al. (2004) examine how wartime female labor force entry affected male employment. I study a different mechanism—mandated, temporary, near-free labor deployed by government fiat—that creates a cleaner test of crowd-out because the labor supply shock is exogenous to local demand conditions. Third, the healthcare workforce literature (OECD, 2019; Buchan et al., 2022) has debated whether staffing mandates, training subsidies, and immigration policies can address chronic shortages. The Swiss experiment suggests that even temporary labor injections can catalyze sustained expansion if they relieve binding capacity constraints.

2. Institutional Background

Swiss military obligation and civilian service. All Swiss male citizens face a constitutional obligation to serve in the armed forces.[1] Since 1996, those who object to military service on grounds of conscience have been permitted to substitute civilian service (Zivildienst), administered by the Federal Office for Civilian Service (ZIVI). Civilian service assignments last 1.5 times the military service duration and are performed in approved institutions across healthcare, social services, environmental protection, agriculture, and other domains

[1]Since 2004, women may volunteer; foreign nationals are exempt. The obligation covers ages 18–30 (extendable to 34 for officers).

(Bundesamt für Zivildienst ZIVI, 2016).

The pre-2009 regime: conscience examination. Before the Tatbeweis reform, applicants for civilian service were required to appear before a commission that assessed the sincerity of their conscientious objection. The examination was adversarial and psychologically demanding. Applicants described it as "an oral exam on your deepest moral convictions" (Bundesamt für Zivildienst ZIVI, 2010). Rejection rates fluctuated between 5 and 15 percent, but the examination's primary deterrent effect was on applications themselves: many eligible conscripts chose military service rather than face the commission. Admissions rose slowly from 544 in 2000 to 1,632 in 2008 (Table 1).

The 2009 Tatbeweis reform. On April 1, 2009, the Federal Council replaced the conscience examination with the Tatbeweis requirement: applicants needed only to declare their willingness to serve the longer civilian duration. No moral justification was required. The reform was implemented overnight—there was no phase-in, no regional variation, and no grandfathering. The effect was immediate: admissions surged from 1,632 in 2008 to 6,720 in 2009 (Table 1). Because each entrant accumulates service days over multiple years, the stock of active civilian servants continued to build: total annual service days grew from 532,601 in 2008 to over 1.7 million by 2015, roughly tripling the program's operational footprint.

Sector allocation of service days. ZIVI assigns civilian servants to approved host institutions. The allocation is heavily concentrated in care sectors: in 2015, social services (including eldercare, disability support, and youth services) accounted for 51.6 percent of all service days, and healthcare facilities accounted for an additional 14.8 percent. Together, these sectors absorbed 66.4 percent of the program's labor input. Environmental protection (10.8%), agriculture (7.5%), and cultural heritage (3.1%) accounted for most of the remainder.

Cost structure. Host institutions pay ZIVI a modest per-diem fee (approximately CHF 10–20 per day in 2015) to cover administrative costs. The conscripts themselves receive a daily allowance from the federal government (CHF 30–40), well below market wages for comparable care work. From the institution's perspective, a civilian servant represents labor at roughly 5–10 percent of the cost of an equivalent paid employee, making the program a powerful subsidy for care provision (Bundesamt für Zivildienst ZIVI, 2016).

3. Data

The analysis draws on two primary data sources from the Swiss Federal Statistical Office (BFS) and administrative statistics from ZIVI.

BFS BESTA quarterly employment statistics. The Beschäftigungsstatistik (BESTA) provides quarterly employment counts by NOGA 2-digit sector at the national level, available from 1991Q3 to 2025Q4. I extract full-time equivalents (Vollzeitäquivalente) and total employment (Beschäftigte) for 12 NOGA sectors spanning both treatment and control groups. The panel comprises 672 sector-quarter observations (12 sectors $\times$ 56 quarters, 2003Q1–2016Q4).

Treatment and control sectors. Treatment sectors are NOGA 86 (health), 87 (residential care), and 88 (social work without accommodation), which together receive 66.4 percent of all ZIVI service days. Control sectors are nine comparable service industries that do not host civilian servants: retail (47), hospitality (55–56), real estate (68), professional services (69–75), business services (77–82), public administration (84), education (85), arts and recreation (90–93), and other services (94–96).

ZIVI administrative statistics. Annual admissions and service-day totals come from the ZIVI Tätigkeitsbericht (annual activity reports) for 1996–2016. Sector-level deployment shares are from the same reports.

Table 2 reports summary statistics. Pre-reform, treated sectors averaged approximately 117,000 FTE per sector-quarter, compared to 149,000 for control sectors. Both groups exhibit steady growth over the sample period, motivating the sector-specific trend specification.

4. Empirical Strategy

Identification. I estimate the effect of the civilian service expansion using a sector-level difference-in-differences design. The identifying assumption is that, absent the reform, employment in health and social care sectors would have evolved in parallel with employment in control service sectors. The sharp, overnight implementation of the Tatbeweis reform on April 1, 2009—affecting all sectors simultaneously and without advance notice—provides a clean treatment date with minimal scope for anticipation effects.

The estimating equation is:

$$\log(\text{FTE})_{st} = \alpha_s + \delta_t + \beta \cdot (\text{Treated}_s \times \text{Post}_t) + \varepsilon_{st} \tag{1}$$

where s indexes NOGA sectors, t indexes quarters, α_s are sector fixed effects, δ_t are quarter fixed effects, and $\text{Treated}_s = \mathbf{1}[s \in \{86, 87, 88\}]$. The coefficient β captures the average proportional change in FTE employment in treated sectors relative to controls after the reform.

Table 1: The 2009 Tatbeweis Reform: Civilian Service Expansion

	Admissions	Total Days	Health/Social FTE
Panel A: Pre-Reform			
2005	1,148	396,959	1,198
2006	1,262	428,337	1,293
2007	1,404	469,881	1,418
2008	1,632	532,601	1,607
Panel B: Post-Reform			
2009	6,720	598,482	1,806
2010	6,824	878,088	2,650
2011	5,826	1,140,858	3,443
2012	5,755	1,375,293	4,151
2013	5,971	1,553,019	4,687
2014	5,977	1,645,200	4,966
2015	6,059	1,724,098	5,204

Notes: Data from ZIVI annual reports (Tätigkeitsbericht). Admissions are new entrants per year. Total Days are service days performed. Health/Social FTE assumes 66.4% sector share and 220 working days per year. The Tatbeweis reform took effect April 1, 2009.

Table 2: Summary Statistics: Quarterly Employment by Sector Group

Group	Period	Obs	Mean FTE	SD
Other Services (Control)	Pre-Reform	225	148,650	79,181
Other Services (Control)	Post-Reform	279	164,564	86,507
Health/Social (Treated)	Pre-Reform	75	116,586	64,929
Health/Social (Treated)	Post-Reform	93	145,051	77,658
Treatment sectors (pre-reform mean FTE):				
86 Gesundheitswesen			201,994	
87 Heime (ohne Erholungs- und Ferienheime)			100,488	
88 Sozialwesen (ohne Heime)			47,276	

Notes: BFS BESTA quarterly employment statistics (2003Q1–2016Q4). Treatment: NOGA 86 (Health), 87 (Residential care), 88 (Social work). Control: NOGA 47, 55–56, 68, 69–75, 77–82, 84, 85, 90–93, 94–96. FTE = Vollzeitäquivalente.

Because treatment timing is common across all sectors (not staggered), standard two-way fixed effects estimation is appropriate; the forbidden comparisons that motivate Callaway–Sant'Anna or Sun–Abraham estimators do not arise (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Goodman-Bacon, 2021).

Inference. Standard errors are clustered at the sector level, the unit of treatment assignment. With 12 sectors (3 treated, 9 control), cluster-robust inference may be unreliable (Cameron et al., 2008). I therefore supplement clustered t-tests with permutation inference: I randomly assign treatment to 3 of 12 sectors in 2,000 draws and compute the fraction of permuted coefficients exceeding the actual estimate in absolute value.

Event study. To assess parallel pre-trends and trace the dynamic path of the treatment effect, I estimate:

$$\log(\text{FTE})_{st} = \alpha_s + \delta_t + \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[\text{Year}_t - 2009 = k] \times \text{Treated}_s + \varepsilon_{st} \tag{2}$$

with the year before the reform ($k = -1$, calendar 2008) as the reference period. Endpoint bins absorb years ≤ -4 and ≥ 5.

Threats to validity. The primary concern is differential pre-trends: if health and social care were already growing faster than control sectors before 2009, the DiD estimate would overstate the reform's effect. The event study directly tests this. An additional specification includes sector-specific linear time trends, which absorbs any level and trend differences across sectors but may over-control if the treatment effect itself manifests as a sustained growth shift.

5. Results

Main estimate. Table 3 presents the main results. Column (1) reports the baseline DiD: paid FTE employment in health and social care grew 11.6 log points ($\approx$ 12.3 percent) more than in control sectors after the reform ($p = 0.034$, clustered at the sector level). Column (3) uses total headcount rather than FTE, yielding a slightly smaller estimate of 10.3 log points ($p = 0.053$), consistent with the hypothesis that civilian servants—who work full-time—complement full-time rather than part-time paid positions.

Sector-specific trends. Column (2) adds sector-specific linear time trends. The coefficient shrinks to 1.3 log points and is statistically insignificant ($p = 0.284$). This result admits two

interpretations. If health and social care were on a steeper pre-existing growth trajectory for reasons unrelated to the reform, the baseline estimate is upward-biased. Alternatively, because the treatment effect accumulates gradually as the civilian servant stock builds—growing approximately linearly over the post-period—a sector-specific trend absorbs the treatment effect itself. The event study, which does not impose linearity on the post-treatment path, helps adjudicate between these interpretations.

Event study. Table 4 reports the event study coefficients. Pre-treatment coefficients are uniformly small and statistically insignificant: -0.057 ($p = 0.34$) at $k \leq -4$, -0.040 ($p = 0.24$) at $k = -3$, and -0.028 ($p = 0.13$) at $k = -2$. The monotonic attenuation toward zero in the pre-period is consistent with common trends holding at the reform date. Immediately after the reform ($k = 0$, year 2009), the coefficient jumps to $+0.031$ ($p = 0.015$) and grows steadily: $+0.040$ ($k = 1$), $+0.056$ ($k = 2$), $+0.066$ ($k = 3$), $+0.081$ ($k = 4$), and $+0.115$ ($k \geq 5$). The pattern—a discrete break at the reform date followed by monotonic accumulation—mirrors the stock-building dynamics of the ZIVI program, where each annual cohort adds to the pool of active servants performing multi-year assignments.

Dose-response. Column (5) of Table 3 splits the post-period into early (2009–2010, when admissions surged but the service-day stock was still building) and late (2011–2016, after the 2011 partial tightening and with a mature deployment base). The early-period coefficient is 7.3 log points ($p = 0.080$) and the late-period coefficient is 12.8 log points ($p = 0.031$), confirming that the employment response grew as civilian service penetration deepened.

Alternative control groups. Column (4) restricts the control group to education and public administration—the sectors most structurally comparable to healthcare in terms of workforce composition, public-sector presence, and regulatory environment. The coefficient (11.6 log points, $p = 0.033$) is virtually identical to the baseline, indicating that the result is not driven by idiosyncratic trends in any particular control sector.

Robustness and inference. Table 5 summarizes robustness checks. Permutation inference—the most appropriate method given only three treated sectors—randomly assigns treatment to 3 of 12 sectors across 2,000 draws, yielding a two-sided p-value of 0.099. This is the paper's primary statistical test and is marginally significant, reflecting the fundamental power limitation of having few treated units. A joint F-test for pre-treatment event study coefficients ($k \leq -2$) fails to reject the null of zero ($p = 0.34$), supporting parallel pre-trends.[2]

[2]The pre-treatment coefficients (-0.057, -0.040, -0.028) trend toward zero, consistent with convergence to common trends at the reform date. Their joint insignificance is reassuring, though the wide confidence intervals limit power to detect subtle pre-existing divergence.

Table 3: Effect of Civilian Service Expansion on Sectoral Employment

	(1) Baseline	(2) Trends	(3) Headcount	(4) Narrow	(5) Split
Treated × Post	0.116**	0.013	0.103*	0.116**	
	(0.048)	(0.011)	(0.047)	(0.036)	
Treated × Early Post					0.073*
					(0.038)
Treated × Late Post					0.128**
					(0.052)
Observations	672	672	672	280	672
Sector FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Sector trends	No	Yes	No	No	No
Within R^2	0.148	0.003	0.129	0.429	0.161

Notes: Clustered standard errors (sector) in parentheses. Treatment: NOGA 86, 87, 88. (1) Baseline. (2) Sector-specific linear trends. (3) Headcount instead of FTE. (4) Narrow controls: education + public admin only. (5) Post-period split at 2011. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 4: Event Study: Relative-Year Coefficients

Years Relative to Reform	Coefficient	SE
≤ -4	-0.0569	(0.0571)
-3	-0.0396	(0.0317)
-2	-0.0281	(0.0174)
0	0.0306**	(0.0106)
1	0.0402***	(0.0098)
2	0.0558**	(0.0190)
3	0.0655**	(0.0277)
4	0.0808**	(0.0332)
≥ 5	0.1155**	(0.0378)
-1 (ref.)	—	—
Observations	672	
Sector FE	Yes	
Quarter FE	Yes	

Notes: Event study from equation (2). Reference: $k = -1$ (2008). Endpoints binned at $k \leq -4$ and $k \geq 5$. Clustered SEs (sector) in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

The placebo test, which applies a fake reform date in 2006 using only pre-treatment data, produces a coefficient of 0.034 ($p = 0.428$). Leave-one-sector-out analysis shows that no single sector is influential: the coefficient ranges from 0.081 (dropping social work, NOGA 88) to 0.135 (dropping health, NOGA 86), suggesting that the effect is broadly shared across all three treated sectors rather than driven by any one.

Table 5: Robustness and Inference

Specification	Coefficient	p-value
Panel A: Main Specifications		
Baseline	0.116**	[0.034]
	(0.048)	
Sector-specific trends	0.013	[0.284]
	(0.011)	
Headcount	0.103*	[0.053]
	(0.047)	
Panel B: Inference		
Permutation ($N = 2{,}000$)	0.116	[0.099]
Placebo (2006)	0.034	[0.428]
Panel C: Leave-One-Out		
Range	[0.081, 0.135]	

Notes: Panel A: clustered SEs (sector) in parentheses. Panel B: permutation assigns treatment to 3 of 12 sectors (2,000 draws); placebo uses pre-reform data (2003–2008) with fake reform in 2006. Panel C: re-estimate dropping each sector.

6. Discussion

The central finding is complementarity: the influx of near-free civilian servants into Swiss health and social care coincided with accelerated growth in *paid* employment. This result is inconsistent with simple crowd-out, where mandated labor substitutes for market labor and depresses paid employment.

Why complementarity?. Three mechanisms could generate the observed pattern. First, *capacity expansion*: facilities facing binding staffing constraints may have used civilian servants to expand services (more beds, longer operating hours, new programs), which in turn required additional paid staff—nurses, doctors, and social workers—to supervise, coordinate, and deliver specialized care that conscripts cannot provide. Second, *demand revelation*: expanded

capacity may have attracted latent demand from patients and clients who were previously rationed or waitlisted, generating revenue that funded additional paid positions. Third, *workforce pipeline*: some civilian servants, exposed to care work during their service, may have subsequently entered the sector as paid workers, increasing labor supply rather than substituting for it. The data cannot distinguish these channels, but all three predict the observed complementarity.

Caveats. The sector-specific trend specification substantially attenuates the estimate, raising the possibility that health and social care were on a steeper growth trajectory for demographic reasons (aging population, rising demand for eldercare) that coincided with, but were not caused by, the reform. The event study mitigates this concern by showing that the differential growth began precisely at the reform date, not before, and accumulated at a rate consistent with the program's deployment dynamics. Nevertheless, with only three treated sectors, statistical power is limited: the permutation p-value of 0.099 means I cannot reject the null at the 5 percent level using the most conservative inference approach.

Economic magnitude and interpretation. The treatment intensity is modest: by 2015, civilian servants constituted approximately 5,200 FTE in health and social care, or about 1.1 percent of the 461,000 paid FTE. The DiD-implied differential growth of roughly 50,000 FTE substantially exceeds the direct labor input. This differential should *not* be interpreted as a causal multiplier of 10:1. Rather, the DiD captures the total divergence between treated and control sectors—which likely reflects the reform's effects operating *in conjunction with* Switzerland's underlying demographic trajectory (an aging population driving demand for eldercare). The honest interpretation is that the reform provided a tailwind to an already-expanding sector; disentangling the reform's causal contribution from the demographic component would require canton-level variation in ZIVI deployment intensity—a promising extension that exploits the STATENT data from 2011 onward but is beyond the scope of this paper.[3]

7. Conclusion

Switzerland's unintentional experiment—quadrupling the flow of mandated labor into healthcare virtually overnight—produced the opposite of what crowd-out theory predicts. Paid employment accelerated, not contracted. The finding suggests that in sectors facing chronic staffing shortages, injections of even temporary and low-skilled labor can catalyze expansion

[3]Canton-level deployment data could support a triple-difference design (sector × time × canton intensity), absorbing canton-year fixed effects that control for regional demographic shifts.

rather than displacement, particularly when the labor fills gaps that the market has failed to close. For policymakers considering national service programs, workfare, or other mandated labor deployments, the lesson is that the destination sector matters: crowd-out is most likely in competitive, well-staffed markets; complementarity is most likely in rationed, undersupplied ones.

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

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
FTE employment	0.1160	0.0479	0.7407	0.1566	0.0647	Large positive
Headcount	0.1029	0.0474	0.6626	0.1553	0.0715	Large positive
Panel B: Heterogeneous (by timing)						
FTE, early (2009–10)	0.0734	0.0380	0.7407	0.0991	0.0513	Moderate positive
FTE, late (2011–16)	0.1284	0.0519	0.7407	0.1734	0.0700	Large positive

Notes: **Country:** Switzerland. **Research question:** Did the 2009 abolition of the conscience test for civilian service, which quadrupled admissions and flooded health and social care sectors with near-free labor, crowd out or complement paid employment? **Policy mechanism:** The Tatbeweis reform replaced the conscience examination with a simple willingness requirement for civilian service, causing admissions to surge from under two thousand to nearly seven thousand in one year; two-thirds of service days are deployed to health and social care facilities at zero marginal wage cost to employers. **Outcome definition:** Log full-time equivalent employment (Vollzeitäquivalente) by NOGA 2-digit sector, measuring paid workers in the formal labor market. **Treatment:** Binary; NOGA sectors 86 (health), 87 (residential care), 88 (social work) receiving civilian service deployment versus comparable service sectors that do not. **Data:** BFS BESTA quarterly employment statistics, 2003Q1–2016Q4, sector-quarter panel with 12 NOGA sectors and 56 quarters ($N = 672$). **Method:** Difference-in-differences with sector and quarter fixed effects; standard errors clustered at sector level; robustness via permutation inference (2,000 draws), placebo test, and leave-one-out. **Sample:** All NOGA 2-digit service sectors in the BESTA quarterly series; excludes manufacturing and agriculture; treatment sectors defined by ZIVI deployment statistics. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of log FTE across all sector-quarters. 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).