Distortionary Consequences of Size-Based Mandates: Evidence from Unemployment Insurance*

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Abstract

We study firm responses to a major expansion of the U.S. Unemployment Insurance (UI) system. In 1954, federal law mandated that states extend UI coverage to all firms with four or more employees, a policy change that was binding for some states but not others. Using a difference-in-differences design, we find a sharp and persistent decrease in the number of employers with 4–7 employees in newly-covered states immediately following the law's implementation. Simultaneously, the number of employers with 0–3 employees increases. This response suggests that firms actively reduced employment to avoid coverage. Our findings imply that, under partial coverage, firms cannot fully shift the costs associated with coverage to workers. Size-based mandates can therefore create substantial and long-lasting distortions in the size distribution of firms.

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1. Introduction

Government mandates tied to firm size are a common feature of labor market policy. Policymakers routinely exempt small firms from regulations on the grounds that compliance may be prohibitively costly, and that such exemptions may encourage entrepreneurship and firm entry. In the United States alone, several major regulations, from the Affordable Care Act's employer mandate to federal anti-discrimination laws, are triggered based on firm size. These thresholds create a fundamental trade-off. While they may shield some small firms from regulatory burdens, they may also discourage employers from growing beyond the relevant size threshold. This incentive to stay small could distort the firm size distribution, misallocate labor toward less productive establishments, and lower aggregate output (Guner et al. 2008; Garicano et al. 2016). Despite the prevalence of this policy trade-off, direct causal evidence on it remains limited, especially in the United States.

The U.S. Unemployment Insurance (UI) system offers a useful setting to study the consequences of size-based mandates. For over three decades, from its inception in the 1930s until 1970, UI coverage depended on firm size. Employers below a statutory employment threshold were exempt, while crossing the threshold triggered payroll tax liability, entry into the experience-rating system, and additional administrative obligations. We study how firms responded to this size threshold. Their response, or lack thereof, also sheds light on a long-standing question about the incidence of UI coverage. With complete pass-through, wages in covered firms would adjust to offset the employer-side costs of coverage, leaving the firm size distribution unaffected. Therefore, if the threshold does distort the size distribution of firms, that implies employers bear at least some portion of the coverage costs.[1] Most existing evidence on UI tax incidence comes from variation in tax rates across already-covered firms; we instead study firm responses at the extensive margin of coverage itself.

Specifically, we leverage a major expansion of UI to study employer responses to a size-based coverage mandate. In 1954, federal law required states to extend UI coverage to all firms with four or more employees, yet roughly half of U.S. states already covered these firms. Our identification strategy relies on this pre-existing variation in state coverage laws. Using County Business Patterns (CBP) data on the employer size distribution from 1947 to 1973, we implement a difference-in-differences design comparing newly covered and already covered states. We find a sharp decrease in the number of employers with 4–7 employees; simultaneously, the number of employers with 0–3 employees increases. The number of employers with 8–19 employees also grows, although to a lesser extent.

Notably, by lowering the coverage threshold, the reform shifts a notch in labor costs,

[1] Incomplete pass-through in this setting could be a result of wage rigidities or a consequence of workers not valuing the benefits of UI at cost.

heightening the incentive to locate below four employees for some firms while relaxing the constraints of the previous threshold for others. Our reduced-form estimates reveal that employers actively reduced their size to avoid coverage. We see both increased bunching below the new threshold and a release of pent-up growth for employers whose behavior had been distorted by the prior threshold. This implies that wages do not fully adjust to offset the employer-side costs of coverage. The size-based coverage mandate thus leads to sizable employment distortions.

We also find evidence of strong path dependence. These distortions in the employer size distribution persist well after 1970, when federal law changed again to cover all firms. With the CBP data, we show that the effects of the reform last until at least 1973. Using data from the Business Dynamics Statistics, we show that the size distribution of firms in treated states remains visibly different from control states into the mid-1980s before gradually converging in the 1990s. This slow unwinding suggests that even temporary policy-induced distortions can become embedded in economic structure.

Despite these stark changes in the employer size distribution, we do not see strong evidence of employment responses in aggregate. It is possible that the reform led to a reallocation of labor from medium-size employers to small or large employers, or to sectors not covered by the UI system (e.g., public sector or agricultural work). However, these results are ultimately too imprecise to rule out large effects in either direction. Payroll per employee, on the other hand, falls sharply and remains depressed through at least 1969. Although this is an imperfect proxy, the evidence is consistent with reductions in labor demand driving down the equilibrium wage.

Our results also reveal substantial heterogeneity by industry and by the state-level payroll tax schedule. In the manufacturing sector, where fixed capital requirements could make shrinking difficult, we observe significant growth in the 8–19 employee bin. Conversely, in the retail and services sectors, the response is dominated by the avoidance margin: employers bunch heavily at 0–3 employees, and aggregate employment falls. Moreover, the effects on the employer size distribution dissipate faster in states with lower average payroll taxes.

A key concern with administrative tax data is the potential for strategic misreporting rather than real economic change. We address this in three ways. First, we note that CBP data are derived from a federal tax form that is filed by all employers and is distinct from UI coverage status. Second, we emphasize that several patterns in the data are not consistent with misreporting. For example, the effects do not dissipate even once federal law mandates UI coverage for all firms. This suggests the reform in 1954 led to real changes in the size distribution of employers and were not simply a reporting artifact. Finally, we cross-validate our findings using the Economic Census. We find similar patterns in the Census of Retail

Trade and verify that the discrepancy between CBP and Census employment counts does not differentially change in treated states after 1954.

Going beyond employment and payroll, we draw on historical state-level records from the Census of Manufactures and the Annual Survey of Manufactures. These records allow us to construct measures of value added per worker, the labor share, capital intensity, and total factor revenue productivity. We find that the manufacturing sector experienced increased value added per worker and a falling labor share in newly covered states post-reform. Moreover, capital intensity also rises. Finally, we find suggestive evidence of a temporary reduction in revenue productivity, which highlights the pervasive effects of size-based distortions. These patterns are consistent with both the growth in manufacturing we document and the incomplete wage adjustment. In other words, since wages do not fully adjust, employers are incentivized to substitute away from labor to capital. We see evidence of such employment reductions across sectors and in manufacturing, we see a shift to greater capital use.

Capital intensity within manufacturing determines whether the dominant response to the reform was avoidance or expansion. We measure capital intensity using each state's pre-reform industrial composition: states whose manufacturing sector was concentrated in industries that nationally used relatively more capital than labor are classified as high capital intensity. In both high and low capital intensity states, the share of small establishments rises and the 4–7 employee bin shrinks relative to control states. But in capital-intensive states, the 4–7 bin is losing firms from both ends: some shrink below 4, while others expand above 8, now freed from the prior threshold. In less capital-intensive states, no such expansion occurs. This shows up in the input data as well: capital per worker rises steadily only in high capital intensity states after the reform, while aggregate employment also increases. In low capital intensity states, capital per worker does not change differentially from the control group, and employment appears to go down, though these estimates are noisier.

This paper makes two core contributions. First, we provide novel evidence on the distortionary consequences of size-dependent social insurance coverage. This adds directly to the literature on firm responses to size-dependent regulations (e.g., Guner et al. 2008; Schivardi and Torrini 2008; Dharmapala et al. 2011; Hsieh and Olken 2014; Gourio and Roys 2014; Garicano et al. 2016). Much of this literature has focused on contemporary regulations in Europe or in developing countries. We demonstrate that even in the mid-20th century U.S., size thresholds generated substantial and persistent distortions in the organization of production. This adds to the limited evidence on the effects of size thresholds and "regulatory tiering" in the U.S. (e.g., Mulligan 2020; Dillender et al. 2022; Trebbi et al. 2023).

Our historical setting also allows us to observe the long-run and long-lasting effects of a change in the size threshold. In this vein, our paper also connects to a broader economic

history literature that has used Census of Manufactures records to study firm behavior and productivity (Atack, 1986; Atack et al., 2006; Ziebarth, 2015; Vickers and Ziebarth, 2019; Jaworski, 2017; Hornbeck and Rotemberg, 2024; Hornbeck et al., 2025). The postwar period we study falls between two well-covered eras in the microdata record: the interwar schedules and the Longitudinal Research Database, which begins in 1963. As plant-level files from the 1950s and 1960s remain fragmentary (Becker and Grim, 2011), we primarily rely on aggregate sources and complement them with a model of firm size choice to recover heterogeneity in responses.

Second, we contribute to the empirical literature on payroll tax incidence and other firm responses to UI. A large literature finds that market-level payroll taxes are passed to workers through lower wages (e.g., Gruber 1997*b*; Anderson and Meyer 1997, 2000), while firm-level tax variation leads to employment reductions (e.g., Saez et al. 2019; Benzarti and Harju 2021; Guo 2024). We extend this literature by leveraging a large-scale policy change that affected millions of firms and documenting clear responses at the extensive margin of coverage. Interpreting our findings through the lens of a standard mandated benefits model (Summers 1989; Gruber 1997*b*), the employment distortions indicate that either workers' implicit value of UI is below the cost of coverage to the employer or wages could not freely adjust to offset those costs. Lastly, by providing new evidence of how employers reacted to UI coverage and its ensuing costs, we also build on related work that explores the myriad ways in which firms respond to UI (e.g., Feldstein 1978; Johnston 2021; Lachowska et al. 2025) and add to the historical record of UI's impact during this critical period of expansion (Katz et al. 1998).

2. Background

The federal-state Unemployment Insurance (UI) system was established under the Social Security Act of 1935. The Act imposed a federal payroll tax on employers but offered a credit of up to 90 percent against this tax for contributions to a state UI program meeting federal standards. This "tax offset" mechanism effectively compelled states to establish their own UI systems while preserving state discretion over benefit levels, eligibility rules, and the scope of employer coverage.

As a result of this design, state UI provisions varied along many dimensions, one being the size threshold for employer coverage. Federal law initially applied only to employers with eight or more employees in at least 20 weeks of the year. States could set lower thresholds, extending coverage to smaller employers. By the early 1950s, roughly half of states had adopted thresholds at or below four employees, while the remainder maintained thresholds

of eight or more, following the original federal standard.[2]

In 1954, Congress amended the Federal Unemployment Tax Act (FUTA) to lower the federal coverage threshold from eight to four employees. This change was binding for states that had maintained the higher threshold but had no direct effect on states that had already extended coverage to smaller firms. The amendment took effect starting in tax year 1956.

For a newly covered firm, the reform effectively introduced a new payroll tax. Under each state's experience rating system, newly covered employers typically entered the system at the "standard" rate, which was generally between 2 and 3 percent of taxable payroll, depending on the state. With experience rating, the firm's rate could adjust based on its layoff history, with firms that rarely lay off workers potentially qualifying for lower rates.

The 1954 reform introduced sharp tax incentives: firms with three or fewer employees faced no UI tax, while firms with four or more employees did face the tax. Subsequent federal legislation continued to expand coverage. The Employment Security Amendments of 1970 lowered the coverage threshold further, from four employees to one, effectively mandating universal coverage of private-sector employers beginning in 1972.

3. Conceptual Model

In this section, we adopt the insights of Gruber (1997*b*) and Garicano et al. (2016) to consider the potential effects of a size-dependent benefit mandate. Following Garicano et al. (2016), consider a simple firm production function with a single input, labor. Firms are heterogeneous in productivity, z. When firms are covered by UI, they incur a payroll tax per worker, τ.[3] Let $\pi(z, n)$ represent firm profits at its optimal level of employment:

$$\pi(z) = \max_{n} \begin{cases} zf(n) - w_u n & \text{if } n < N \\ zf(n) - w_c(1+\tau)n & \text{if } n \geq N \end{cases} \tag{1}$$

[2]The coverage thresholds as of 1953 are largely the same as the coverage thresholds that states adopted upon the introduction of UI. What drives this initial variation in coverage thresholds across states? While Haber and Joseph (1941) argue that the "excluded groups" in UI were determined by a variety of political, economic, and administrative reasons, they also suggest that any initial rationale for exclusion was mostly irrelevant as of 1941. Moreover, Haber and Joseph (1939) write, "Size-of-firm restrictions were drafted without reference to the size of the establishments in the state." In our own calculations using Census of Manufactures data from 1929 – years before the drafting of UI provisions in the mid-1930s – we see that having a higher coverage threshold is unrelated to measures of workers per establishment and value added per worker. However, states with higher thresholds do have lower wages and a lower labor share; both are significantly lower by approximately 10%.

[3]To the extent that administrative costs to the firm are determined on a per-worker basis, this can be captured in the model by considering τ as a bundle of per-worker costs, inclusive of the payroll tax. We assume fixed costs associated with experience rating requirements and other administrative burdens are small.

We depart from Garicano et al. (2016) by introducing different wages for the covered and uncovered firms. To motivate this distinction, we draw on the logic of a standard mandated benefits model (Summers 1989; Gruber 1997*b*). Namely, workers assign a value, b, to the UI benefit and thus would be willing to accept lower wages at covered firms, such that: $w_c + b = w_u$. Thus, *if* wages can freely adjust and workers value the benefit at its cost to employers, then $b = w_c\tau$.[4] Under this condition, the firm profits are independent of the size threshold. Put differently, if workers fully value the benefit, firms are indifferent between: (1) locating just below the threshold and paying a higher wage and (2) locating just above the threshold, but paying a lower wage and incurring the payroll tax. Under these assumptions, the existence of a bunching response implies that workers value UI below cost, an important consideration in optimal UI calculations (see, e.g., Baily 1978; Gruber 1997*a*; Chetty 2006; Hendren 2017)

It bears emphasis that this conclusion requires wages that freely adjust. If wages are downwardly rigid, as they may be when the minimum wage is binding, then w_c cannot fall to absorb the costs of coverage, regardless of worker valuation. In this case, the bunching estimates are simply evidence that the cost of coverage to employers was not fully offset, without pinning down precisely *why*. Imposing the assumptions above allows a clean interpretation about the private value of UI to workers; however, even in the absence of those assumptions, we provide evidence that the incidence of these costs is not fully passed through.

In either case, if workers do not fully value the benefit (i.e., $b < w_c\tau$) or if frictions prevent full adjustment, then firms above the size threshold face a higher per-labor cost than firms below the size threshold. As in Garicano et al. (2016), this notch in per-labor costs will induce some firms to bunch below the threshold. To characterize which firms bunch, consider a firm with productivity z with unconstrained optimum $n^*(z) \geq N$. After the reform, the firm is indifferent toward bunching at $N-1$ when the profit from bunching is equivalent to operating at its unconstrained optima under the covered wage:

$$zf(N-1) - w_u(N-1) = zf(n^*(z)) - w_c(1+\tau)n^*(z) \tag{2}$$

Put differently, a firm is indifferent toward bunching below the threshold when its production gain from operating at its optimum under the covered wage (relative to bunching) is equal to the net cost of coverage:

$$z(f(n^*(z)) - f(N-1)) = w_c(1+\tau)n^*(z) - w_u(N-1) \tag{3}$$

From here, it is clear that bunching will be greater when: (1) the net cost of coverage is high

[4]This can be seen by substituting $w_c + b$ in for w_u in the first case of eqn. (B.2).

and (2) the production loss from decreasing employment is low. This latter point motivates our industry heterogeneity analyses in Section 6.3, and the analysis within manufacturing industries in Section 6.5.

In Online Appendix B, we formalize this conceptual framework in a calibrated Lucas span-of-control model, with sector-specific production technology and a size-based coverage threshold (Lucas 1978). The key insight is that we can recover the "undistorted" productivity distribution from the control states with universal coverage (i.e., those with a coverage threshold of one employee). Then, we identify the effective per-worker cost of coverage from the observed bin shares in treated states. The estimated per-worker cost ranges from 1.9% to 3.0%, which is roughly consistent with statutory rates for new entrants to the UI system. We validate the model by comparing its predicted effects of the 1954 reform with our reduced form difference-in-differences estimates. Finally, we use it to quantify the aggregate output consequences of size-dependent UI coverage.

4. Data and Descriptive Statistics

Data Sources We primarily rely on historical data spanning 1947 to 1973 from the County Business Patterns (CBP) series, as digitized and cleaned by Eckert et al. (2022). In particular, the CBP data contains county-by-industry aggregates of payroll in the first quarter of the year, employment, and employer counts broken out by size class.[5] Since the reform we consider changes the coverage threshold, the data on employer counts by size class allow us to home in on precisely the parts of the size distribution most likely to be affected.[6] We further aggregate these data to the state or state-by-industry level because the treatment varies across states. Figure A.1 maps the treatment status across the U.S.

To identify the treatment status of each state, we digitize coverage provisions of state Unemployment Insurance (UI) systems from the late 1940s to the late 1960s. We also digitize information about each state's experience rating system to examine heterogeneity by the size of the tax imposed on newly covered firms. In particular, we collect data on the average payroll tax paid in each state in 1949 and 1953 from historical reports.

In addition to the CBP, we draw on several supplemental datasets to better understand the main results. First, we use data from the Economic Census produced by the U.S. Census Bureau. This alternative source provides additional checks on the validity of the changes in employment we observe in the CBP. Second, we use data from the Social Security Adminis-

[5]The CBP counts are based on "reporting units." For manufacturing, a reporting unit corresponds closely with an establishment or plant. For all other industries, a reporting unit represents an employer, regardless of the number of establishments they have in the county.

[6]We cannot observe the within-class distribution. For example, we only see the number of units with 0–3 employees, not the number with exactly 3 employees, the number with exactly 2 employees, etc.

tration's Current Population Survey (SSA-CPS) Exact Match file from 1978. This provides an alternative source for the results on payroll from the CBP.

Sample Restrictions We make a few baseline sample restrictions that we impose throughout the paper. First, we remove Puerto Rico and Alaska from the main CBP sample. Data for Puerto Rico is only present in a few years, and in Alaska, the number of reporting units falls from approximately one hundred to zero for several years in the middle of the panel. Second, we remove Arizona and Ohio from our analysis sample. These states change their state-level coverage threshold during our pre-period and therefore are not clean controls.[7] Finally, we impute payroll with a linear interpolation for a very small number of observations that are extreme outliers in terms of within-state year-to-year change.[8]

Descriptive Statistics Table 1 reports summary statistics for our baseline sample. In this table we split the sample by treated and control states and by the pre- and post-periods. This table and the simplicity of our research design permit the construction of simple two-by-two difference-in-differences estimates that illustrate our main effects. For example, considering the share of employers with 4–7 employees, this two-by-two comparison can be taken directly from Table 1: $\delta_{2x2} = (0.200 - 0.227) - (0.206 - 0.200) = -0.033$. In treated states, the share of employers with 4–7 employees falls by about 3.3 percentage points after the reform, relative to control states.

5. Empirical Strategy

We study the effect of the 1954 UI reform that lowered the employer coverage threshold nationwide, from 8 or more employees to 4 or more employees. Our identification strategy leverages the fact that half of states had thresholds at or below 4 employees even prior to this change. We compare the evolution of outcomes pre- and post-1954 in those already-covered states to newly-covered states. Specifically, we estimate the following difference-in-differences event study:

$$Y_{st} = \alpha + \sum_{\substack{k \neq 1953, \\ 1947 \leq k \leq 1973}} \beta_k (\text{NewlyCovered}_s \times \mathbb{1}[\text{Year}_t = k]) + \gamma_s + \phi_t + \epsilon_{st} \tag{4}$$

where Y_{st} is the outcome of interest (e.g., count of reporting units with 4–7 employees, payroll per employee, etc.) in state s and year t. The variable NewlyCovered_s is equal to one for states with coverage thresholds that exceed 4 employees before 1954, and equal to

[7] We believe these restrictions are ex ante appropriate; however, we note that they do not ultimately affect our findings.

[8] In the state-year payroll panel, only four out of 554 observations are imputed in this way. In the state-year-industry payroll panel, only 98 out of 5,511 observations are imputed in this way. Along similar lines, when conducting the county-border-pair analysis, we remove four observations in which the county-year pair has at least one size class where the count of units in the size class exceeds the total count of reported units.

Table 1: Summary Statistics

	Control States		Treated States	
	Pre-1954	Post-1954	Pre-1954	Post-1954
Coverage threshold	2.000	1.583	7.833	3.958
	(1.422)	(1.154)	(0.556)	(0.200)
# of establishments	56,818.354	69,091.702	51,031.771	65,875.474
	(82,994.329)	(94,204.386)	(41,380.310)	(49,732.125)
# of establishments, 0–3 emp.	33,068.781	36,638.798	28,661.438	35,107.936
	(49,385.590)	(50,897.555)	(24,043.214)	(26,542.514)
# of establishments, 4–7 emp.	10,761.854	13,851.365	11,398.531	13,099.202
	(14,760.124)	(18,631.579)	(8,603.155)	(9,648.486)
# of establishments, 8–19 emp.	7,524.323	10,723.821	6,485.552	10,441.551
	(10,764.410)	(14,228.138)	(5,036.856)	(7,984.812)
# of establishments, 20+ emp.	5,463.969	7,875.833	4,463.010	7,222.561
	(8,237.697)	(10,895.165)	(3,901.480)	(6,153.152)
Share, 0–3 emp.	0.574	0.525	0.562	0.541
	(0.024)	(0.045)	(0.042)	(0.047)
Share, 4–7 emp.	0.200	0.206	0.227	0.200
	(0.017)	(0.014)	(0.017)	(0.013)
Share, 8–19 emp.	0.136	0.160	0.128	0.156
	(0.010)	(0.018)	(0.016)	(0.017)
Share, 20+ emp.	0.089	0.109	0.083	0.102
	(0.015)	(0.023)	(0.016)	(0.021)
Log(emp.)	12.692	13.079	12.998	13.361
	(1.314)	(1.225)	(0.963)	(0.973)
Payroll/emp., Q1 ($k)	0.697	1.236	0.625	1.133
	(0.119)	(0.236)	(0.125)	(0.238)
# of State-Years	96	312	96	312
# of States	24	24	24	24

Notes: Entries are means and standard deviations from County Business Patterns data. Columns 1 and 2 show this for the states in which employers with 4-7 employees are already covered, prior to 1954, by state law. Columns 3 and 4 show this for the states which are newly covered by the Federal coverage expansion in 1954. Columns 1 and 3 show summary statistics for pre-1954 years, and columns 2 and 4 show summary statistics for post-1954 years. The coverage threshold for control states changes slightly post-1954 because some states with threshold of four decrease to a threshold of one, over time.

zero otherwise. The year indicators, $\mathbb{1}[\text{Year}_t = k]$, are equal to one in the relevant year and zero otherwise. The exact years included in each specification depend on the dataset in use. For the CBP, for example, we have data from the years 1947, 1948, 1951, 1953, 1956, 1959, 1962, 1964-1973. In that case, we omit the year 1953, as it is the year preceding the policy announcement. Finally, we control for state fixed effects, γ_s, and year fixed effects, ϕ_t. Assuming that outcomes in the already covered and newly covered states would have evolved similarly in the absence of coverage expansion, the β_k coefficients identify the causal effect

of the reform in each year.[9]

In addition to the year-by-year event study outlined above, we also estimate difference-in-difference specifications that pool across multiple periods. For example, using the CBP data, we estimate the pooled effect in 1954-1959 (i.e., 1956, 1959), 1960-1965 (i.e., 1962, 1964, 1965), 1966-1970, and 1971-1973. These specifications take the following form:

(5)

$$Y_{st} = \alpha + \beta_1(\text{NewlyCovered}_s \times \text{Y1954to1959}_t) + \beta_2(\text{NewlyCovered}_s \times \text{Y1960to1965}_t)$$
$$+\beta_3(\text{NewlyCovered}_s \times \text{Y1966to1970}_t) + \beta_4(\text{NewlyCovered}_s \times \text{Y1971to1973}_t) + \gamma_s + \phi_t + \epsilon_{st}$$

To motivate our empirical strategy, Figure A.2 shows how employer size distribution evolves across time. Panel (a) shows the number of employers with 4–7 and 0–3 employees for the control and treated states. On the other hand, Panel (b) shows the share of employers for the same groups. We see in both graphs an increasing number and share of employers with 0–3 employees for the treatment group immediately after 1953, while the control does not evidence a major change in their trend. On the contrary, for the treatment group with 4–7 employees, there is a decrease after the reform took place.

To examine industry differences in the response to UI payroll taxes, we estimate equations 4 and 5 separately by industry. We also examine heterogeneity based on features of the state's experience rating tax schedule. To address the geographic concentration of states by treatment status, we estimate specifications with region-by-year fixed effects. Notably, these specifications will weight regions with more treatment variation more heavily. Along similar lines, we estimate specifications that include border-state-by-year fixed effects, effectively making comparisons only across neighboring states. Finally, we limit the sample to adjacent counties and estimate a specification with county-border-pair-by-year fixed effects.

6. Effects of UI Coverage Expansion

6.1. *Effects on the Employer Size Distribution*

Figure 1 presents direct evidence of firm responses to the introduction of UI coverage and the accompanying payroll tax. In particular, we examine the count of employers by size class and the share of employers by size class, and plot the β_k coefficients from eqn. (4). These

[9]The Fair Labor Standards Act Amendments of 1955 raised the federal minimum wage from \$0.75 to \$1.00, effective in 1956. We believe this coincident shock is unlikely to drive our results because, to do so, it must differentially affect the newly covered states *and* differentially hurt employers with 4–7 employees in those states. Moreover, during our sample period, the federal minimum wage also increased in 1950, 1961, 1963, 1967, and 1968, yet we only observe the sharp divergence between our treated and control states in 1956, consistent with the effects being driven by the shift in UI coverage (U.S. Department of Labor 2009). Still, we explore specifications that control for the distribution of state payroll per employee to further address this concern.

coefficients document how the size distribution evolved in newly covered (i.e., treated) states relative to already-covered (i.e., control) states, accounting for fixed differences across these states as of 1953, the omitted year.

Panel (a) of Figure 1 shows that the number of employers with 4–7 employees falls sharply in newly covered states while the number of employers with 0–3 employees rises. We are not able to reject the null that the rise in the 0–3 class is equivalent to the fall in the 4–7 class. The estimates also indicate a potential increase in the 8–19 and 20+ size classes, however, this is not statistically distinguishable from the null when examining counts. Panel (b) analyzes the share of employers in each class. We see similar patterns. The share of employers with 4–7 employees declines while the share with 0–3 employees increases. There is also a slight increase in the share of employers with 8–19 employees.[10] Table 2 displays the coefficient estimates from eqn. (5) for the same outcomes considered in Figure 1.[11]

In Section 3, we highlight that the employer response should be stronger when the net cost of coverage is higher. To explore this, we digitized average UI payroll tax rates by state in 1949 and 1953. We use the average of these two years to designate states as "high" or "low" tax states and estimate separate event studies including only the high tax treated states or the low tax treated states. Figure A.4 shows these heterogeneity results by state tax schedule for the employers with 0–3 and 4–7 employees. In the initial years of coverage, effects are similar in high and low tax states. This is expected since newly covered employers will pay the maximum payroll tax rate for 1-3 years under experience rating. Over time, however, we see the size distribution effects dissipate faster in low tax states.

Since treatment status is highly concentrated geographically, we estimate eqn. (4) adding region-by-year fixed effects, border-state-by-year fixed effects, or border-county-by-year fixed effects. Figures A.5- A.7 present the coefficient of interest β_k of eqn. 4.[12] The results are consistent, although differ slightly since areas with greater treatment variation receive more weight in these specifications and the samples differ.[13] There is a sharp increase in the share of employers with 0–3 employees and a sharp decrease in the share of employers with 4–7 employees. Relatedly, we see similar results when we split states by the median of payroll per employee in 1953, and add split-by-year fixed effects (see Figure A.9). This addresses concerns that the increase in the federal minimum wage in 1956 is driving our results.[14]

[10] As a robustness check, we include Figure A.3, which accounts for state-specific pre-treatment linear trends.

[11] While we cannot distinguish between new entrants and incumbents in these results, the speed of the response suggests adjustment from incumbent employers.

[12] Figure A.8 presents the raw data from a "case study" of two similarly-sized neighboring states.

[13] These specifications also invite greater bias from cross-state spillovers; nevertheless, we find broadly similar results.

[14] In order for this shift in the minimum wage to impact our estimates of the size distribution effects, it

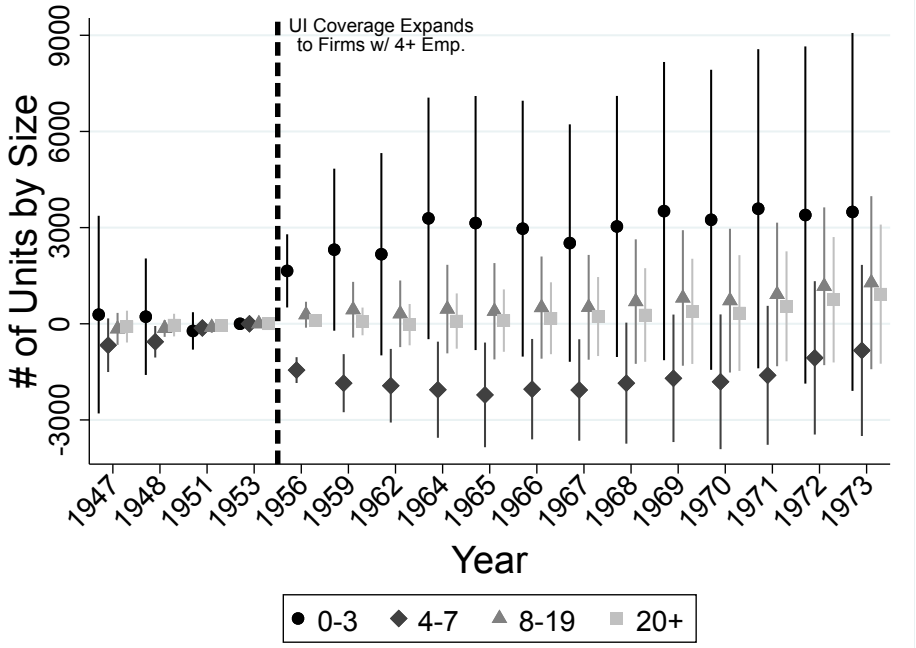
We also document the persistent effects of the reform in Figure 1. Despite the Employment Security Amendments of 1970 mandating a uniform threshold at one employee, treated and control states continue to differ through 1973. To trace the long-run dynamics of the firm size distribution, we turn to Business Dynamics Statistics (BDS) data beginning in 1978. Figure A.10 reveals suggestive evidence of hysteresis. Through the late 1970s and 1980s, treated states continued to exhibit an elevated share of very small firms and a depressed share of mid-sized and large firms relative to control states. A key intuition for understanding these long-run trends is that both treated and control states faced policy-induced distortions prior to 1954, albeit at different thresholds. The 1954 mandate initiated a partial convergence process, but it simultaneously introduced a new distortion. When the 1970 reform mandated a uniform threshold, these distortions slowly began to dissipate in full. This evidence underscores that even temporary size-based mandates can have long-lasting effects when firms make difficult-to-reverse structural choices based on those policies.[15]

must be the case that the change differentially hit states treated by the 1954 UI reform and, even within those states, differentially hurt employers with 4–7 employees. Again, while the federal minimum wage changes several times from 1950-1968, we only observe divergence between our treated and control states in 1956.

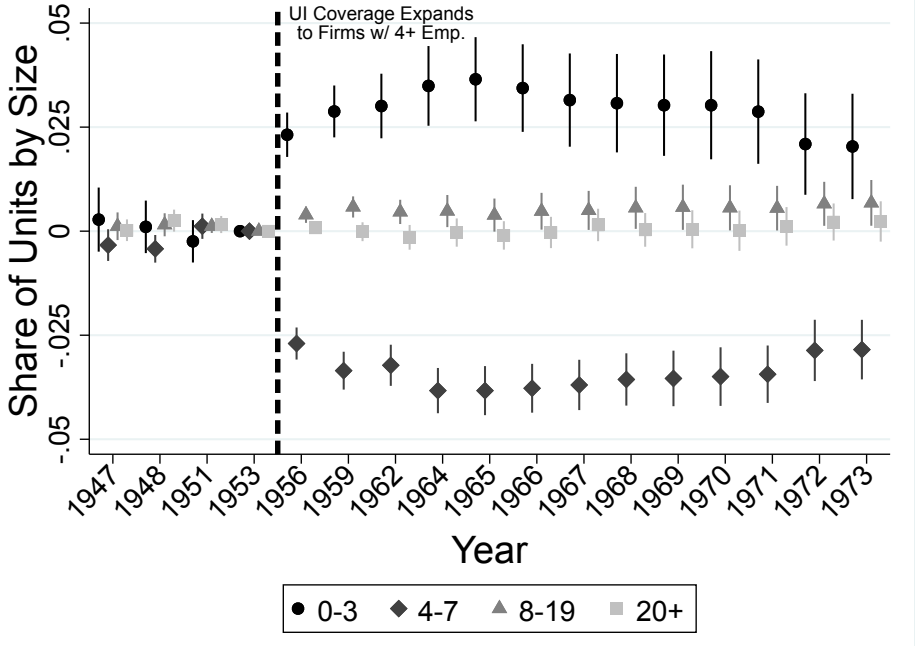
[15]The key difference between the immediate response in 1954 and the slow response in 1970 is that the 1954 reform introduced a salient coverage threshold at which avoidance had well-defined benefits.

Figure 1: Effects of UI Coverage Expansion on Employer Size Distribution

(a) Count



(b) Share



Notes: Figure displays the β_k coefficient estimates from eqn. (4). Panel (a) presents this for specifications with raw counts by state-year as the outcome. Panel (b) presents this for specifications with shares by state-year as the outcome. The coefficients correspond to the difference in the outcome between treated and control states in each year, relative to the difference between these states in the omitted year of 1953. Both panels present 90% confidence intervals based on standard errors clustered at the state level.

Table 2: Effects Pooled Across Multiple Years

	Count of Employers in Size Class				Share of Employers in Size Class					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	1,909.93	–1,308.64**	464.29	129.10	0.026***	–0.029***	0.004**	–0.001	–0.017	–0.023
	(1,758.95)	(488.13)	(483.34)	(313.10)	(0.004)	(0.002)	(0.002)	(0.001)	(0.035)	(0.015)
1960–1965 x Treated	2,796.07	–1,729.25*	490.67	96.96	0.033***	–0.035***	0.003	–0.002	–0.054	–0.037*
	(2,713.08)	(975.42)	(881.89)	(621.61)	(0.006)	(0.003)	(0.002)	(0.002)	(0.056)	(0.021)
1966–1970 x Treated	2,985.31	–1,553.49	751.25	323.73	0.031***	–0.035***	0.004	–0.001	–0.018	–0.034
	(2,839.40)	(1,231.60)	(1,235.45)	(993.38)	(0.007)	(0.003)	(0.003)	(0.003)	(0.062)	(0.030)
1971–1973 x Treated	3,419.89	–827.48	1,225.88	784.07	0.023***	–0.029***	0.005	0.001	–0.009	
	(3,300.93)	(1,593.85)	(1,570.13)	(1,286.27)	(0.008)	(0.004)	(0.003)	(0.003)	(0.078)	
Dep. Var. Mean	34,694.95	12,911.73	9,740.86	6,940.80	0.541	0.205	0.152	0.101	13.132	0.994
Observations	816	816	816	816	816	816	816	816	816	526
R-squared	0.98	0.98	0.97	0.96	0.927	0.861	0.923	0.955	0.991	0.978

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

6.2. *Effects on Employment and Payroll*

The results above provide prima facie evidence of employer responses to the introduction of UI coverage. If employers could fully pass on the payroll tax to employees, we should not observe these stark changes in the size distribution. Next, we examine effects on aggregate employment and payroll-per-employee to fully characterize the effects of the expanded coverage mandate.

Figure 2, panel (a) shows a noisy decline in aggregate employment in the newly covered states. While the employer responses we observe in Figure 1 may have led to a decrease in total employment, it is also possible that those responses lead to a reallocation of labor from medium-size employers to small or large employers, or to sectors not covered by the UI system (e.g., public sector or agricultural work). From panel (a) for Figure 2, it is difficult to say which is more likely. We explore this further in the section on industry-heterogeneity.

Panel (b) of Figure 2 documents a sizable decline in payroll-per-employee (based on quarter 1 payroll).[16] If we view this as a proxy for wages, this implies that some of the burden of the payroll tax is shifted to workers. However, it is also possible that when employers shrank in size, they let go of the more highly paid employees, leading to a decline in payroll-per-employee without any given employees' wages changing. We think the former explanation is most likely, particularly because we find similar declines when we include individual fixed effects in our supplemental analysis of earnings in the SSA-CPS Exact Match file.

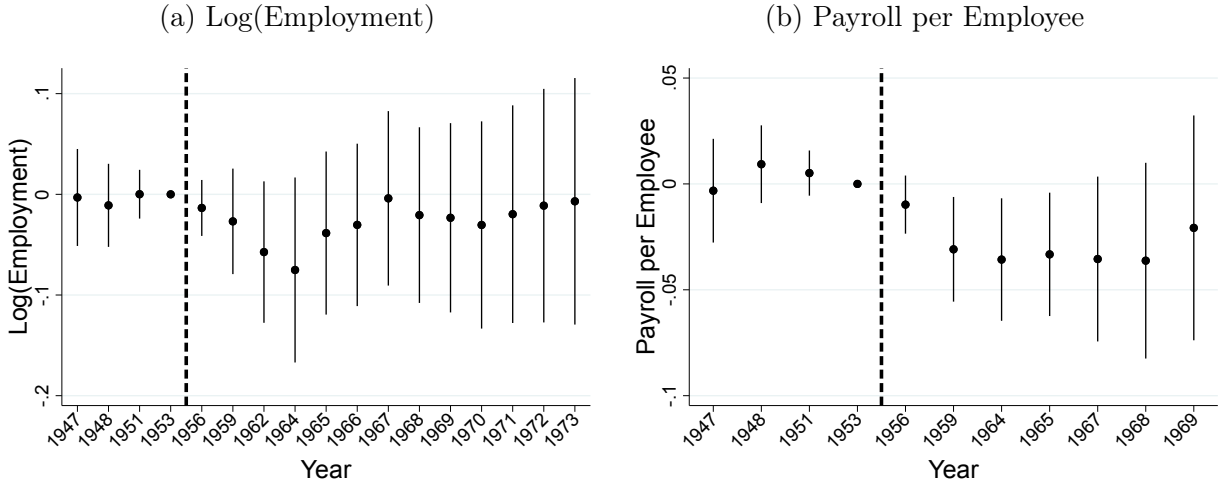
We also find that the coverage expansion compresses the state-level distribution of payroll per employee. First, in panel (a) of Figure A.11, we show that the natural log of payroll per employee is *not* falling in the treated states. Panels (b) and (c) of Figure A.11 clarify why the result differs in logs versus levels. Namely, the decline in payroll per employee is driven by higher payroll states, and in lower payroll states, payroll per employee exhibits a slight upward trend. As a result, the interquartile range of payroll per employee across the treated states falls relative to the control states. Panel (c) of Figure A.11 shows precisely this pattern. The 25th percentile of payroll per employee increases slightly in treated states relative to control states, whereas the median and 75th percentiles mirror the fall in the mean.

6.3. *Heterogeneity by Industry*

In the following sections, we examine industry heterogeneity in the response to the change in the coverage threshold. First, we present results for three large sectors: manufacturing,

[16] We find similar patterns using administrative earnings data from SSA in the SSA-CPS Exact Match file (see Figure A.12).

Figure 2: Effects of UI Coverage Expansion on Employment and Payroll



Notes: Figure displays the β_k coefficient estimates from eqn. (4). Panel (a) presents this for specifications with the natural log of employment by state-year as the outcome. Panel (b) presents this for specifications with payroll-per-employee as the outcome. The coefficients correspond to the difference in the outcome between treated and control states in each year, relative to the difference between these states in the omitted year of 1953. Both panels present 90% confidence intervals based on standard errors clustered at the state level.

retail, and services. Then, for the sake of brevity, we include results for the remaining sectors in Online Appendix A.

Effects in Manufacturing, Retail, and Services Figure 3 presents separate event-study estimates for three major sectors: manufacturing, retail trade, and services. These industries differ markedly in their capital intensity and labor costs, providing a test of how adjustment costs may mediate firm responses to the UI mandate.

In manufacturing, we see growth in the 8–19 employee bin, with a small and statistically imprecise decrease in the 4–7 employee bin. The count of employers with 0–3 employees shows little change. This pattern is consistent with the removal of the old coverage threshold at eight employees. Manufacturing firms that had previously constrained their employment below eight to avoid UI coverage were free to expand once the threshold was decreased. The relatively small avoidance response for manufacturing may reflect the high fixed capital costs in manufacturing, which make it particularly costly for firms to shrink or to operate at an inefficiently small scale.[17]

In retail trade, the pattern is very different. There is a large and statistically significant increase in employers with 0–3 employees, accompanied by a sharp decrease in the 4–7 employee bin. The 8–19 and 20+ bins, however, show little change. We see similar patterns

[17]It is also possible this heterogeneity partially reflects differential experience rating by sector. As of 1949, the average payroll tax in manufacturing was 1.47% compared to 1.18% in wholesale/retail trade and 1.14% in services (U.S. Department of Labor 1950).

for the services sector.

Figure 4, panels (b) and (c), show that total employment declines substantially in these sectors, but not in manufacturing, even though it presents an upward trend. Payroll per employee also decreases for retail trade and services, but not for manufacturing. Online Appendix Tables A.1, A.2, and A.7 show similar patterns when estimating eqn. (5) by industry.

Effects in Other Sectors Online Appendix Figures A.13, A.14, A.15, and A.16 present the same set of results for all other private, non-farm sectors: agricultural services; mining; contract construction; transportation and public utilities; wholesale trade; finance, insurance, and real estate; and establishments not elsewhere classified. The results are highly consistent with what we observed in the main industries we discussed earlier. In general, the share of employers with 0–3 employees had a sharp increase, and the number of employers with 4–7 employees showed a sharp decrease.

The results for employment are noisier. There is a trend downwards in construction and wholesale trade, but these are either short-term or not statistically significant. Payroll, on the other hand, shows a sharp decrease across industries, except for transportation and public utilities, where we cannot reject the null. Online Appendix Tables A.4–A.10 show the pooled results of eqn. 5 by industry.

6.4. *Concerns about Reporting*

A primary concern when working with administrative tax records is whether observed changes reflect real shifts in employer inputs or are due to changes in reporting (i.e., tax evasion). We argue that the changes documented in the preceding sections are the result of real activity for three main reasons.

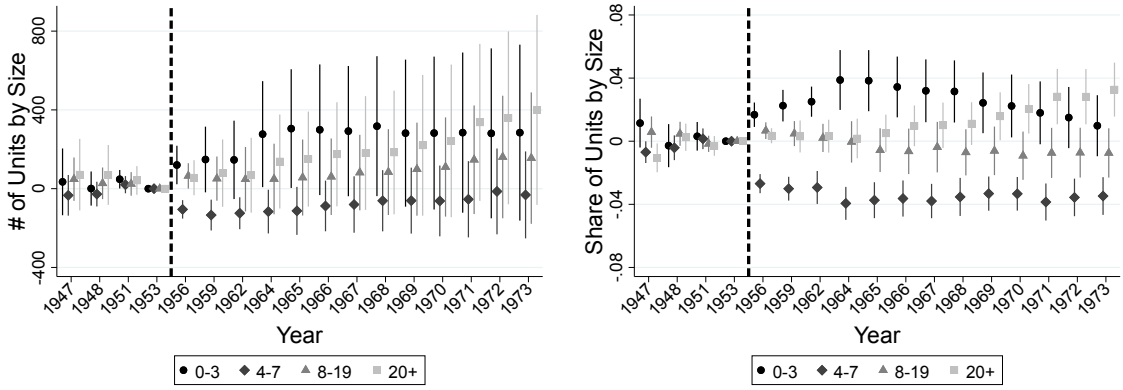
First, the historical County Business Patterns series is primarily derived from employment and payroll information collected by the Social Security Administration (SSA) from Treasury Form 941, the employer's quarterly federal tax return Schiedel (1973).[18] UI coverage, however, is based on state tax filings and Treasury Form 940. Misreporting thus requires that employers alter reported employment across multiple forms, including a Federal tax form not directly linked to their UI coverage status.

Second, we validate our main estimates by using an alternative source of employment records that is not based on tax records. Specifically, we compare employment in the County Business Patterns to employment in the U.S. Census Bureau's Economic Census, for manufacturing, retail trade, and wholesale trade. Figure 5, panel (a) explicitly tests whether

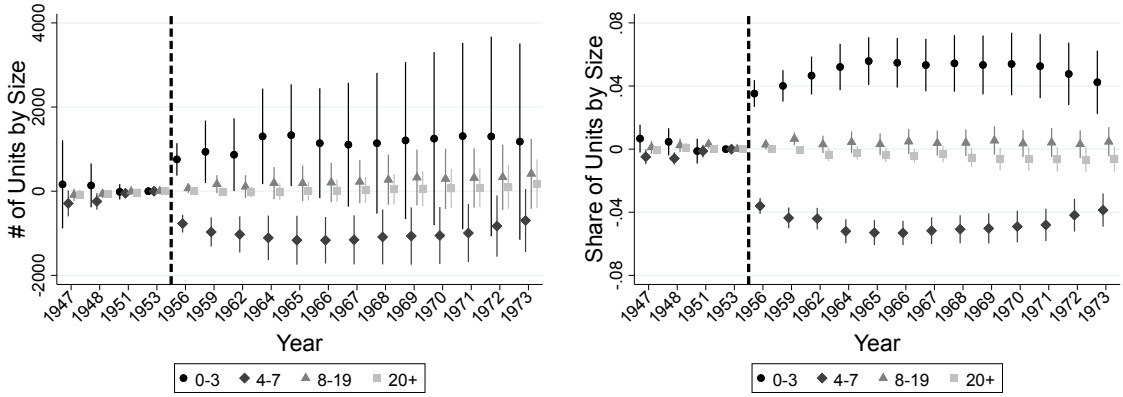
[18]On this form, employers are instructed to report the number of employees receiving any wages, tips, or other compensation; this includes part-time and seasonal employees.

Figure 3: Heterogeneity in Employer Size Effects by Industry

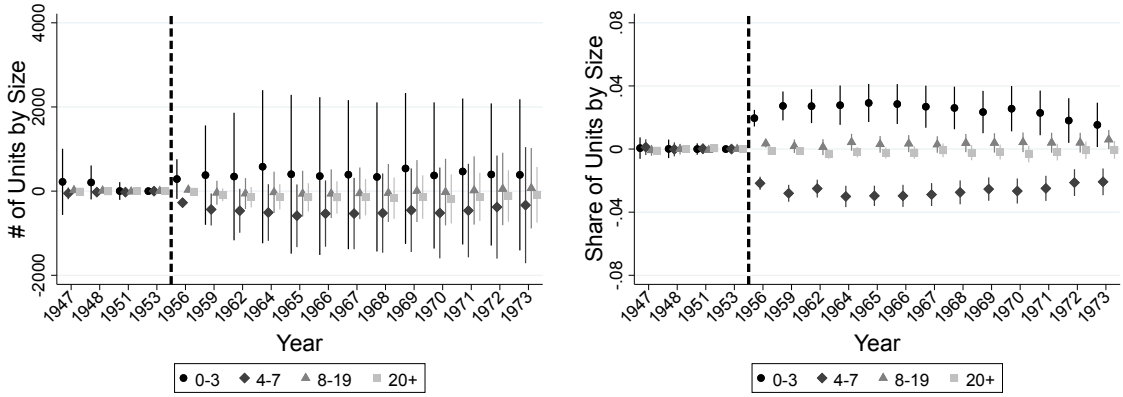
(a) Manufacturing



(b) Retail



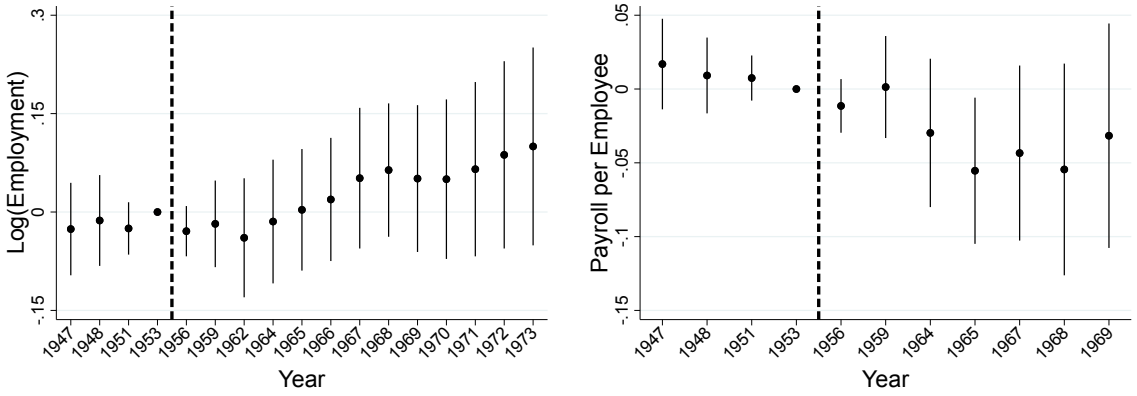
(c) Services



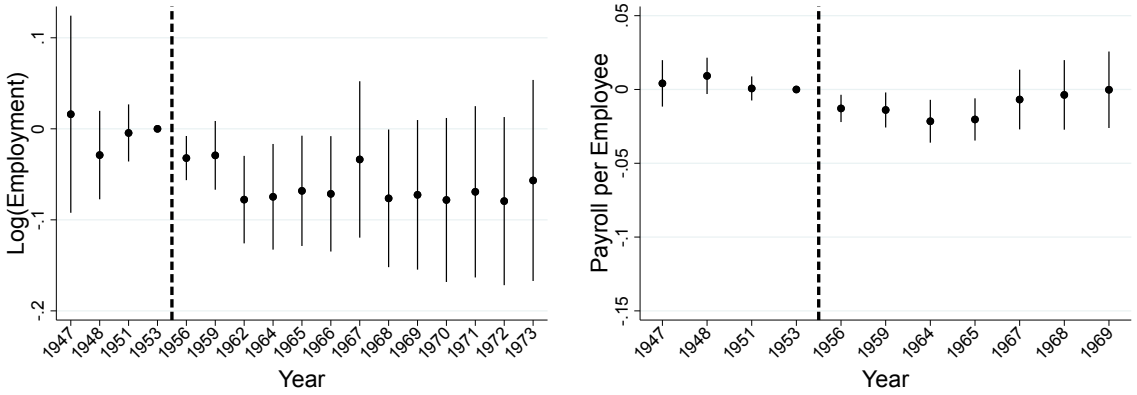
Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated by sector. Panels (a)-(c) present this for manufacturing, retail trade, and services, respectively. The figures on the left use raw counts by state-year as the outcome, while the figures on the right use shares by state-year as the outcome. All panels present 90% confidence intervals based on standard errors clustered at the state level.

Figure 4: Heterogeneity in Employment and Payroll Effects by Industry

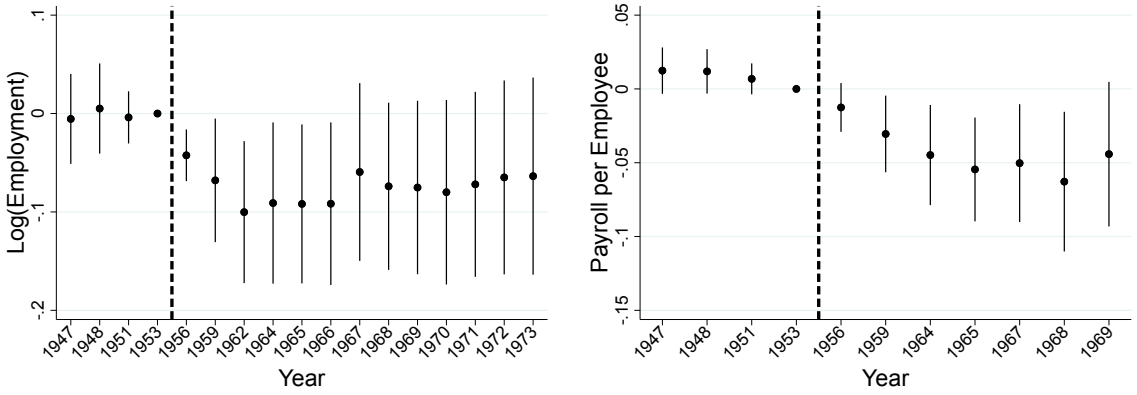
(a) Manufacturing



(b) Retail



(c) Services



Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated by sector. Panels (a)-(c) present this for manufacturing, retail trade, and services, respectively. The figures on the left use log(employment) as the outcome, while the figures on the right use payroll-per-employee as the outcome. All panels present 90% confidence intervals based on standard errors clustered at the state level.

employment differences between these series grow in treated versus control states, post-1954. We do not find any evidence that these series differ more in treated states after the Federal change in the coverage threshold. Panel (b) of Figure 5 replicates our estimate of the employment decline in retail trade from Figure 4. In the overlapping years, we find a very similar pattern using the Economic Census.

Third, we note several patterns that are not consistent with strategic misreporting. For one, we observe falling payroll-per-employee. If firms were merely decreasing reported employment to fall below the threshold, we would observe a rise in payroll-per-employee.[19] We also see slight increases in employers with 8–19 employees, particularly in manufacturing. This is not consistent with a pure misreporting channel. Finally, the changes in the firm size distribution persist until, at least, 1973. Since Federal law expanded coverage to all employers in 1970 (eff. date, 1972), there is no incentive for firms to continue misreporting.

A distinct issue when working with historical data from the County Business Patterns series is that for non-manufacturing industries, the counts by size class represent "reporting units" rather than establishments. A reporting unit in a county is effectively a single employer in that county, regardless of how many establishments that employer has.[20] One potential explanation for our results is that multi-establishment employers with 4–7 employees across establishments divide into separate legal entities with fewer than 4 employees each. This response would lead to a rise in reporting units with 0–3 employees and a fall in reporting units with 4–7 employees.[21] However, the decline in payroll per employee and the fall in total employment in certain sectors, like retail and services, cannot be explained by such a response. Moreover, in manufacturing, the unit counts correspond to establishment counts, and we observe similar changes in the size distribution in the manufacturing sector.

Finally, we consider the possibility that the decline in employers with 4–7 employees is driven by corporations with exactly 4 employees. Owners of these corporations could, in principle, avoid coverage without "real" changes in employment by removing themselves from payroll. To gauge the importance of this channel, we conduct a back-of-the-envelope exercise using statistics on the legal form of business enterprises in 1953 from the U.S. Internal Revenue Service and imposing assumptions about the within-class distribution of employers. In

[19]The fall in payroll-per-employee is mirrored by a fall in earnings for employed workers in the SSA-CPS Exact Match file (see Figure A.12). These numbers are drawn from distinct tax forms (e.g., Form 940 for CBP and Form W2 for the SSA-CPS Exact Match file).

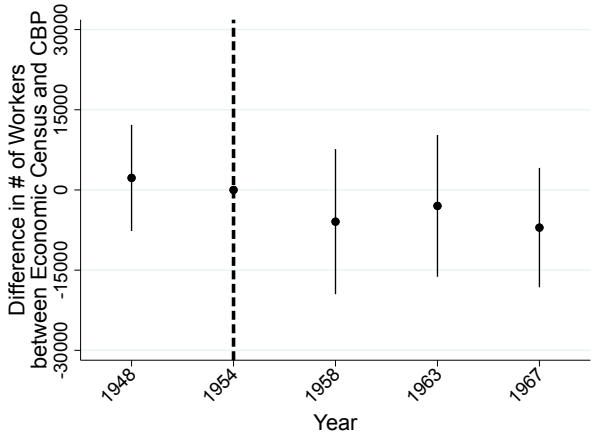
[20]SSA adopted the "Establishment Reporting Plan" to reduce reporting burden for employers. This plan allowed non-manufacturing employers "to aggregate employment and payroll data for a number of establishments with identical industrial activity in the same county, and supply the data for the establishment group as a whole, called a reporting unit." Schiedel (1973)

[21]Such restructuring would also involve real costs. That firms would bear these costs would itself imply a meaningful behavioral response to the tax, even if the mechanism is organizational restructuring rather than employment reduction.

1953, approximately 7.4% of business enterprises were corporations (82.4% were proprietorships and 10.2% were partnerships). Assuming a log-normal distribution of employer size, we estimate that approximately 37% of employers in the 4–7 employee size class have exactly 4 employees. Under the conservative assumption that 7.4% of these employers are corporations, we estimate that 2.7% of employers in the 4–7 employee size class are corporations with exactly 4 employees. If all owners of these firms responded by removing themselves from payroll, that change would only account for about 22% of the post-1954 decrease in treated states. This is likely an overestimate of the importance of this channel. Since corporations are disproportionately large, it is unlikely that the overall share of corporations is equal to the share of corporations among four-employee firms.

Figure 5: Tests of Strategic Misreporting in Tax Records

(a) Differences in CBP and Economic Census



(b) Estimates for Retail by Source

Notes: Figure displays the β_k coefficient estimates from eqn. (4). Panel (a) estimates this using the difference between employment in the Economic Census and the County Business Patterns as the outcome. In this panel, we restrict to the manufacturing, retail trade, and wholesale trade sectors, since those are the sectors for which we have Economic Census data. When the years available in the CBP and the Economic Census do not align, we use the closest available year. Specifically, we match 1954-EC with 1953-CBP, 1958-EC with 1959-CBP, and 1963-EC with 1962 and 1964-CBP. Panel (b) estimates this using log(employment) as the outcome, plotting results based on the Economic Census and the County Business Patterns alongside one another. Both panels present 90% confidence intervals based on standard errors clustered at the state level.

6.5. *Effects on Other Firm Inputs*

The County Business Patterns data allow us to observe the size distribution of employers, total employment, and payroll. However, they do not capture other important margins of firm adjustment, such as capital investment. The stark differences in responses between manufacturing, retail, and services documented in Section 6.3 may reflect differences in capital requirements and the degree of complementarity or substitutability between capital

and labor. Understanding how firms adjusted along these margins is crucial for interpreting the broader implications of the UI coverage expansion.

Manufacturing is particularly well-suited for exploring these questions. Its high capital intensity suggests that firms in this sector may face larger adjustment costs when reducing employment. Moreover, examining productivity in manufacturing can help us understand whether the distortions in the firm size distribution led to efficiency losses. Finally, it provides another check on the distortion affecting the real economy, not only reporting to the government. To this end, we turn to data from the Census of Manufactures (CM) and the Annual Survey of Manufactures (ASM), which provide detailed information on inputs and output at the state level.

Data and Measurement We digitized published state-level tables from the CM and the ASM, collecting data on value added, employment, the wage bill, and expenditures on new plant and equipment for census years 1947, 1954, 1958, and 1963, and non-census years 1949, 1951, 1952, 1953 and 1965.

Our measure of output is value added and our measure of labor input is the total wage bill, which weights workers by compensation and accounts for differences in human capital across workers (Hsieh and Klenow 2009). We construct the capital stock by the perpetual inventory method with state-specific steady-state growth rates.

We construct aggregate Total Factor Revenue Productivity (TFPR) following Hsieh and Klenow (2009) as the ratio of value added to $K^{\alpha_s}(wL)^{1-\alpha_s}$, where α_s is calibrated from national industry-level labor shares in 1947 weighted by each state's industry composition.

Online Appendix C provides more discussion on the assumptions required to interpret our estimates as productivity measures, and documents that treated and control states had similar production function parameters and productivity levels prior to the reform, though treated states were somewhat less capital-intensive, a difference that bears on the interpretation of the post-reform dynamics below.

Results We begin by examining outcomes that do not rely on parametric assumptions about production technology. Figure 6 presents event-study estimates for value added per worker and the labor share. Panel (a) shows that value added per worker increases in treated states relative to control states, with the effect emerging around approximately five years after the reform. By 1958, value added per worker is roughly 5 percent higher in newly covered states. This pattern is mirrored in panel (b), which shows a decline in the labor share, though the estimates are not statistically distinguishable from zero. The decline in the labor share indicates that value added grew in spite of the wage bill falling in treated states (see Figure 2), consistent with capital deepening or productivity improvements.

The increase in value added per worker is consistent with the patterns we observed in

the employer size distribution. Recall from Figure 3a that the number of manufacturing establishments with 8–19 employees increased immediately after 1954, and Table A.1 shows that the number of establishments with 20 or more employees grew in later periods. Firms that had been constrained by the old eight-employee threshold were able to expand once the threshold was lowered to four. This expansion may have allowed firms to achieve greater economies of scale or to adopt more capital-intensive production methods, both of which would raise value added per worker.

Figure 6: Effects of UI Coverage Expansion on Value Added and Labor Share

Notes: Figure displays the β_k coefficient estimates from eqn. (4). Panel (a) estimates this using the log of value added per worker as the outcome. Panel (b) estimates this using "salaries and wages, total" as a share of value added as the outcome. Both panels present 90% confidence intervals based on standard errors clustered at the state level.

Figure 7 presents estimates for capital per worker and Total Factor Revenue Productivity. Panel (a) shows a clear increase in capital per worker in treated states after 1953. This capital deepening is consistent with manufacturing firms substituting capital for labor in response to the payroll tax. If firms could not fully pass the cost of the tax to workers—as suggested by the employment distortions documented earlier—they may have responded by investing more in capital-intensive production methods. This interpretation aligns with the increase in value added per worker: firms that adopted more capital-intensive technologies would naturally produce more output per worker.

Panel (b) of Figure 7 presents estimates for TFPR. The pattern here is more complex. In the short term, TFPR appears to decline slightly in treated states, though the effect is only significant at the 10 percent level. Although the evidence here is more tentative, this initial decline is consistent with a period of re-composition in the manufacturing sector. With some firms contracting to avoid the new threshold and others expanding in response

Figure 7: Effects of UI Coverage Expansion on Capital per Worker and TFPR

Notes: Figure displays the β_k coefficient estimates from eqn. (4). Panel (a) estimates this using the log of capital stock per worker. Capital stock is constructed using perpetual inventories method, assuming different growth rates in steady state for each state. Panel (b) estimates this using Total Factor Revenue Productivity as the dependent variable. TFPR is the ratio between value added and an index of production inputs given by $K^{\alpha_s}(wL)^{1-\alpha_s}$. We calibrate $1-\alpha_s$ as the weighted average at the state level of national, industry-level labor shares in 1947, where weights are industry shares for each state. Both panels present 90% confidence intervals based on standard errors clustered at the state level.

to the removal of the old threshold, this reallocation could temporarily reduce aggregate productivity. However, by the mid-1960s, the TFPR differential between treated and control states dissipates, suggesting that any short-term productivity losses were transitory.

Taken together, these results help explain why manufacturing responded differently to the UI coverage expansion than retail and services. In manufacturing, we observe growth in the 8–19 and 20+ employee bins, rather than widespread bunching at 0–3 employees. The high capital intensity of manufacturing makes it costly for firms to shrink or to operate at an inefficiently small scale. Instead, firms that had been constrained by the prior eight-employee threshold expanded once that constraint was relaxed. This expansion was accompanied by capital deepening, which allowed firms to increase output per worker.

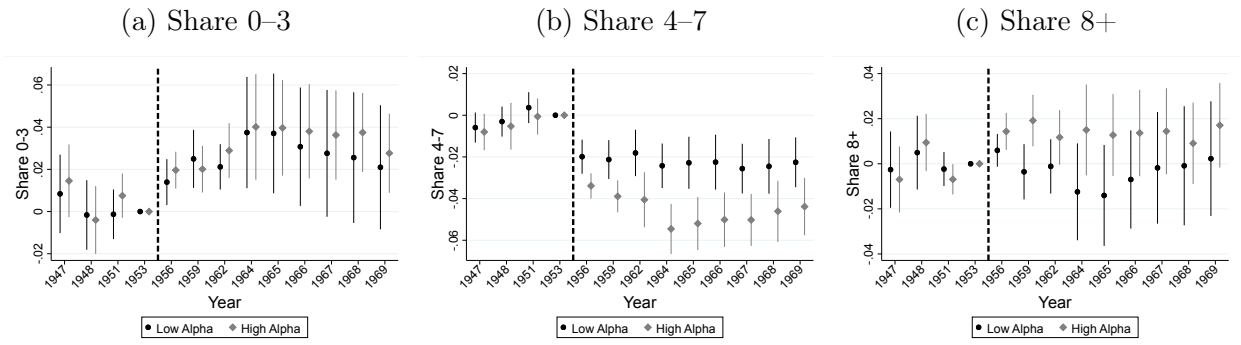
The evidence on capital deepening also speaks to the incidence of the UI payroll tax. If firms could fully shift the tax to workers through lower wages, we would not expect to see such stark changes in capital intensity. The fact that firms actively adjusted their capital-labor ratios suggests that they bore at least part of the tax burden and responded by substituting away from the now-more-expensive labor input. This interpretation is consistent with the employment distortions we documented earlier: firms cannot fully pass the tax to workers, and so they distort their input choices to minimize tax liabilities. The capital deepening response in manufacturing highlights an additional margin of adjustment that is less available

to labor-intensive sectors like retail and services, where the primary response was to reduce employment and bunch below the coverage threshold.

Within-Manufacturing Heterogeneity The aggregate results point to capital intensity and scale as key drivers of manufacturing's distinctive response to the reform. To test this more directly, we exploit the fact that states differed in their pre-existing industrial composition in ways that are informative about how costly it was for their firms to adjust employment. Our measure of capital intensity, α_s, is calibrated from national industry-level labor shares weighted by each state's 1947 industry mix. It therefore reflects the kinds of industries a state specialized in before the reform. Importantly, as documented in Online Appendix C, treated and control states had identical values of α_s in 1947.

We divide treated states into high and low α_s groups based on the median among treated states, comparing each to the control group. In states whose manufacturing sector was concentrated in capital-intensive industries, firms faced a higher cost of operating below their efficient scale, and the old notch at 8 was therefore more likely to have been a binding constraint on growth. Conversely, in states with more labor-intensive manufacturing, firms could shrink more cheaply, and the new notch at 4 was likely the more relevant margin.

Figure 8: Share of Units by Size: Heterogeneity by State-level Capital Share (α_s)



Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated separately for treated states with above- and below-median values of α_s ("High Alpha" and "Low Alpha"), each compared to the control group. Panel (a) uses the share of manufacturing establishments with 0–3 employees as the outcome. Panel (b) uses the share with 4–7 employees. Panel (c) uses the share with 8 or more employees. We calibrate $1 - \alpha_s$ as the weighted average at the state level of national, industry-level labor shares in 1947, where weights are industry shares for each state. All panels present 90% confidence intervals based on standard errors clustered at the state level.

Figure 8 shows the results. Both groups show a relative increase in the 0–3 share and a relative decline in the 4–7 share compared to control states. But the drop in the 4–7 share is notably larger in high α_s states — because in those states the 4–7 bin is losing mass from both ends simultaneously. Some establishments move down into the 0–3 bin, as in low α_s states, but others move up into the 8+ bin, as shown in panel (c). The rise in the 8+ share in

high α_s states, entirely absent in low α_s states, reflects firms that had been constrained just below the old threshold at 8 expanding once that constraint was removed. Where capital makes it costly to operate below efficient scale, the liberation from the old notch dominates. In low α_s states, the 4–7 bin loses mass only downward — there is no corresponding growth in the upper tail.

Figure 9 examines what accompanied this redistribution in the size distribution. In high α_s states, capital per worker rises steadily after the reform and continues growing through 1965, while in low α_s states capital intensity is flat. With aggregate state-level data we cannot separately identify whether these changes reflect incumbent firms expanding, new entrants arriving at larger scales, or the exit of small firms. What the aggregate patterns do establish is that in capital-intensive states the reform was accompanied by a reallocation of inputs toward larger establishments. Total employment moves in the same direction — rising in high α_s states and declining in low α_s states — though these estimates are considerably noisier.

Figure 9: Capital - Labor Substitution: Heterogeneity by State-level Capital Share (α_s)

Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated separately for treated states with above- and below-median values of α_s ("High Alpha" and "Low Alpha"), each compared to the control group. Panel (a) uses capital stock per worker as the outcome. Capital stock is constructed using the perpetual inventory method with state-specific steady-state growth rates. Panel (b) uses total manufacturing workers as the outcome. We calibrate $1 - \alpha_s$ as the weighted average at the state level of national, industry-level labor shares in 1947, where weights are industry shares for each state. All panels present 90% confidence intervals based on standard errors clustered at the state level.

7. Conclusion

We document substantial firm responses to a major expansion of the U.S. Unemployment Insurance system. Using variation across states in the bite of the 1954 federal mandate to

cover firms with four or more employees, we find that the new coverage threshold generated a dramatic and persistent shift in the firm size distribution. The number of employers with 4–7 employees fell, while the number with 0–3 employees rose. These patterns are consistent with active employment reduction by firms seeking to avoid incurring the payroll tax and other costs associated with coverage.

Our findings shed light on two central questions in public finance. First, our results demonstrate that size-dependent benefit mandates can generate substantial and long-lasting distortions in the organization of production. The firm size distribution shifted almost immediately after the 1954 reform and remained distorted through at least 1973, even after federal law eliminated the coverage threshold entirely in 1970. The firm size distribution in treated and control states remains markedly different well into the mid-1980s, reflecting both the changes in 1954 as well as the path dependent effects of the state-level variation in initial thresholds. This persistence suggests that even temporary policy-induced distortions can become embedded in economic structure, perhaps because firms that shrank to avoid the threshold subsequently made complementary investments or organizational choices that made it costly to later expand.

Second, we provide new evidence on firm responses to UI coverage and its ensuing costs. Through the lens of a standard mandated benefits model, firms will reduce employment when workers do not value UI coverage at cost or when wages cannot flexibly adjust to offset those costs. Our results therefore have meaningful implications for payroll tax incidence under partial coverage. Ultimately, we find that firm decisions are influenced by the UI system and should therefore be an important consideration in the design of social insurance policy.

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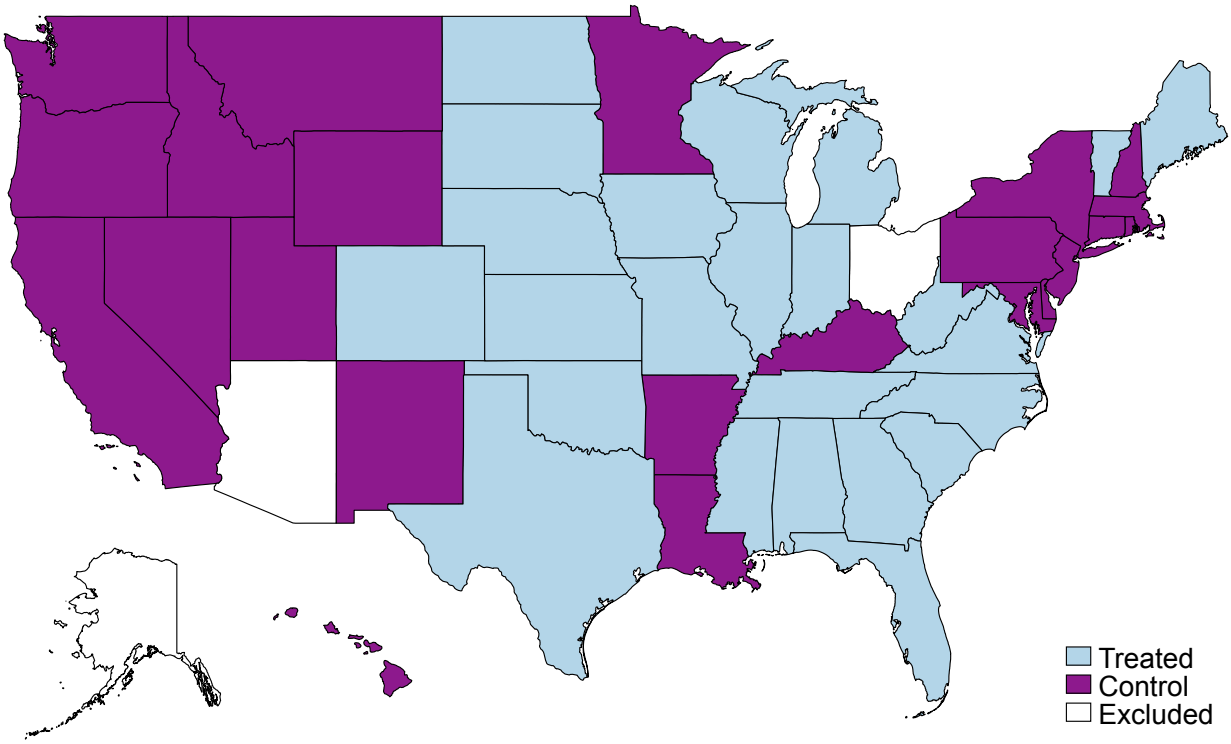
Online Appendix A: Supplemental Results

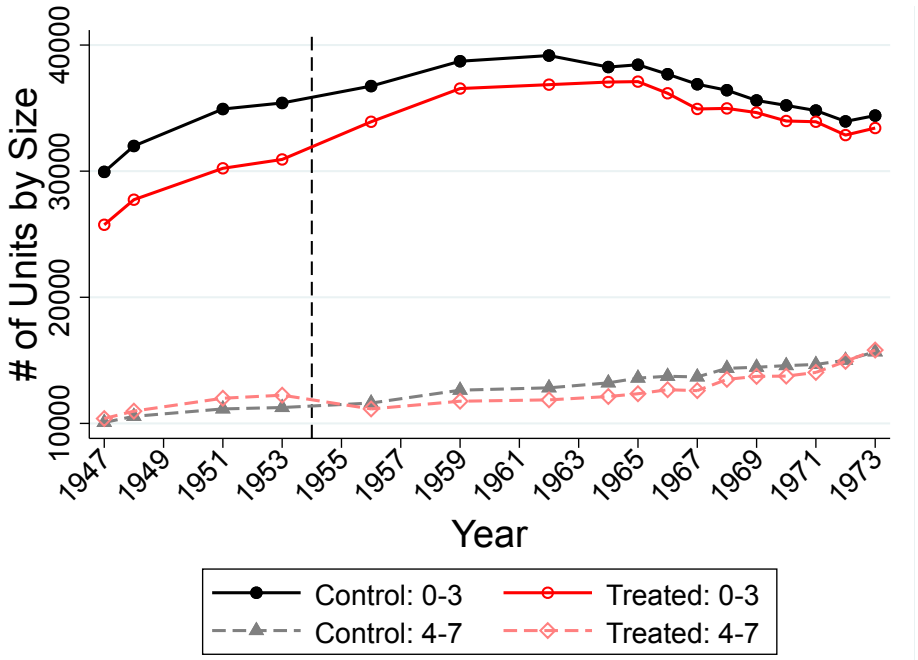
Appendix Figure A.1: Map of States by Treatment Status



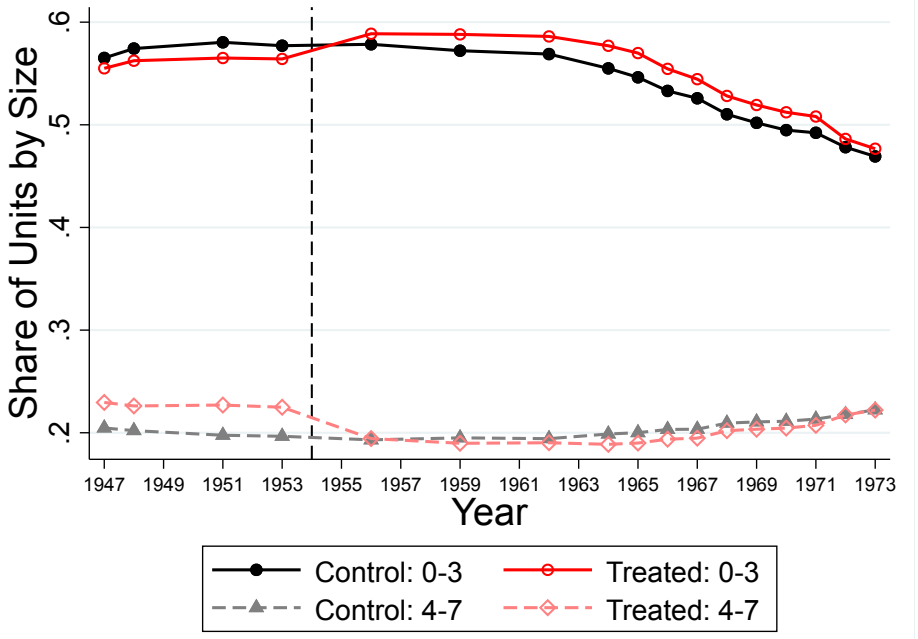
Notes: Figure displays a map of U.S. states color-coded based on treatment status. Treated states (light blue) are states which have a coverage threshold that is greater than 4 employees and control states (purple) are states which have a coverage threshold that is less than or equal to 4 employees. States coded as "excluded" (white) are states which we exclude because they had a change in their coverage threshold during our pre-period (Arizona and Ohio) or because they lack sufficient data in the County Business Patterns (Alaska). The newly-covered (treated) states are: AL, CO, FL, GA, IL, IN, IA, KS, ME, MI, MO, MS, NE, NC, ND, OK, SC, SD, TN, TX, VT, VA, WV, and WI. The already-covered (control) states are: AR, CA, CT, DE, DC, ID, KY, LA, MA, MD, MN, MT, NV, NH, NJ, NM, NY, OR, PA, RI, UT, WA, and WY.

Appendix Figure A.2: Trends of units by treatment status and size

(a) Count



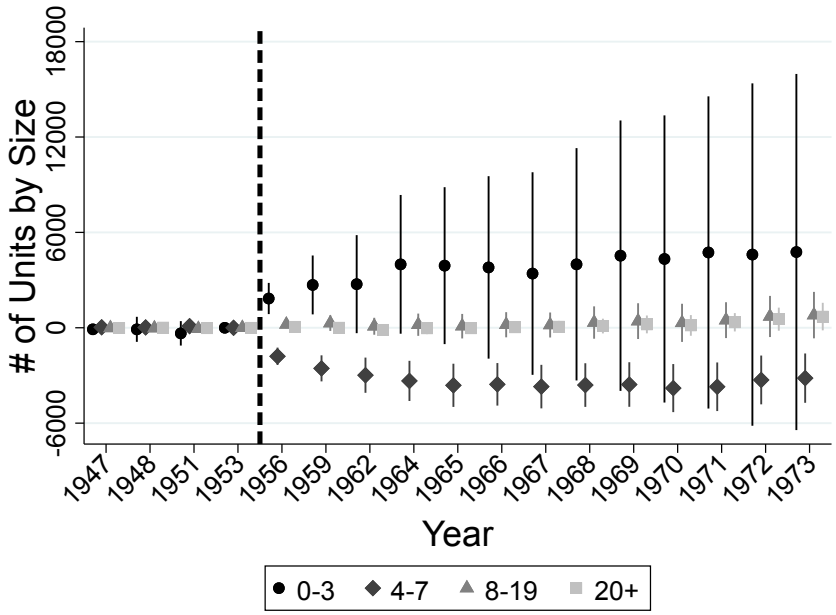
(b) Share



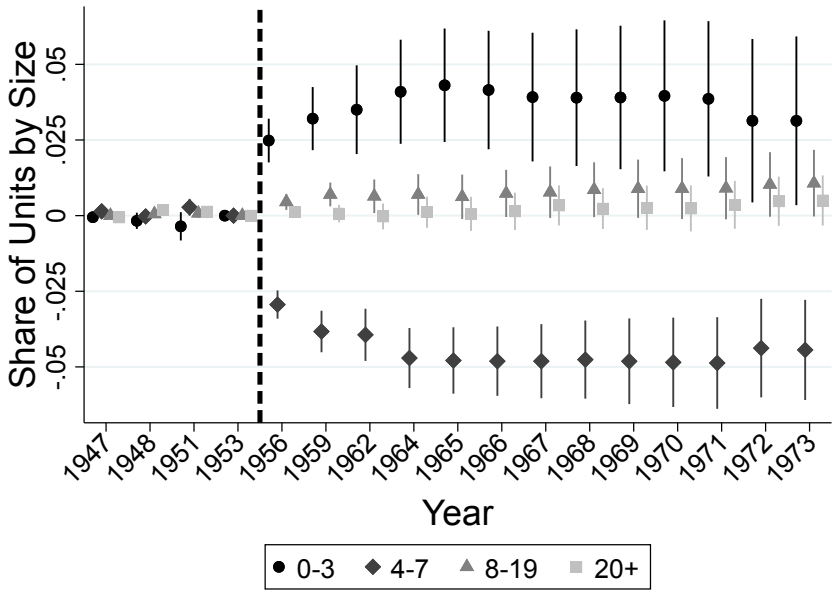
Notes: Figure displays raw data on reporting units in treated and control states over time. Panel (a) displays the number of reporting units by size class and panel (b) displays the share of reporting units by size class.

Appendix Figure A.3: Employer Size Effects with Linear State-Trend Controls

(a) Count

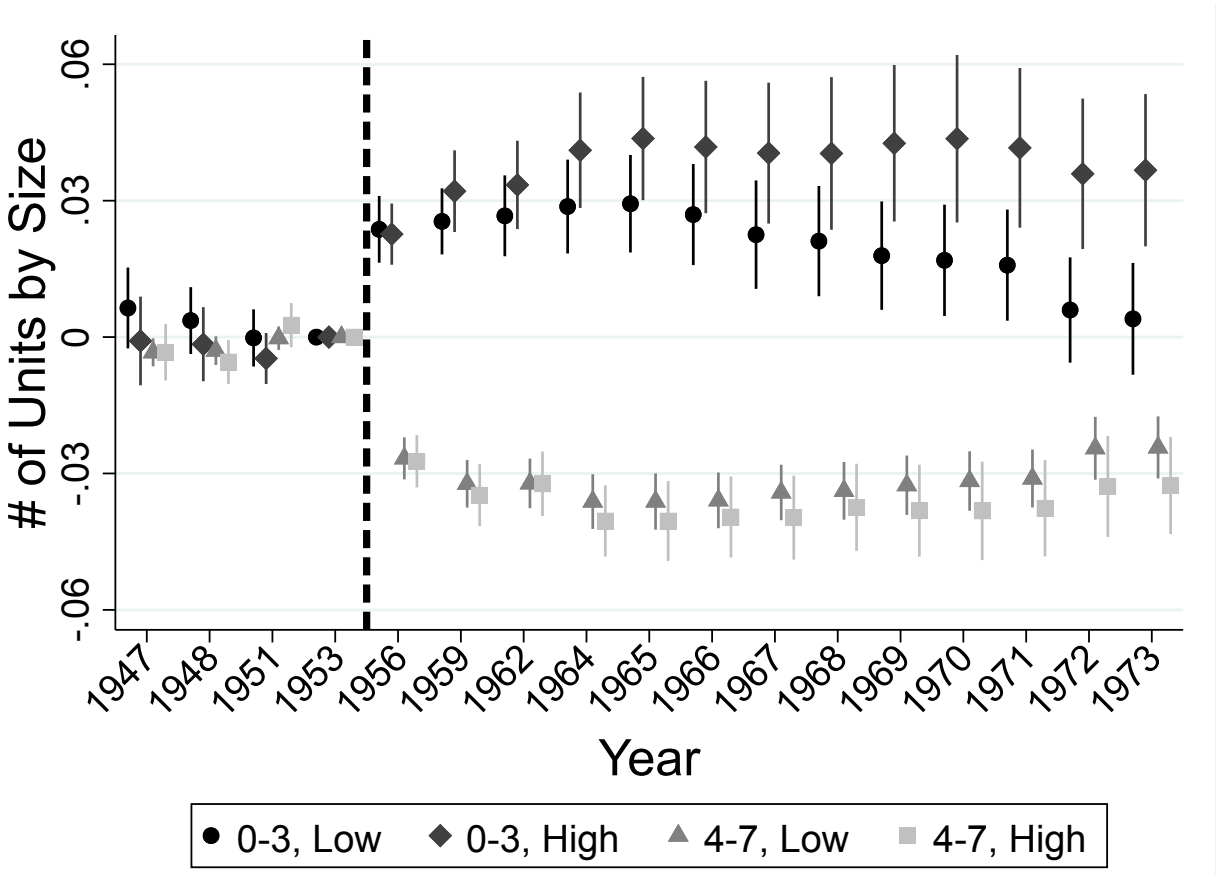


(b) Share



Notes: Figure displays the β_k coefficient estimates from eqn. (4), accounting for differential pre-treatment linear trends across treatment and control states. To do this, we estimate state-specific linear trends on the pre-treatment years for each outcome. Then, we predict post-treatment outcomes based solely on this estimated trend. For each outcome, we subtract its state-specific trend-based predicted outcome in each year. Finally ,we estimate 4 on this de-trended outcome. Panel (a) presents this for specifications with raw counts by state-year as the outcome. Panel (b) presents this for specifications with shares by state-year as the outcome. The coefficients correspond to the difference in the outcome between treated and control states in each year, relative to the difference between these states in the omitted year of 1953. Both panels present 90% confidence intervals based on standard errors clustered at the state level.

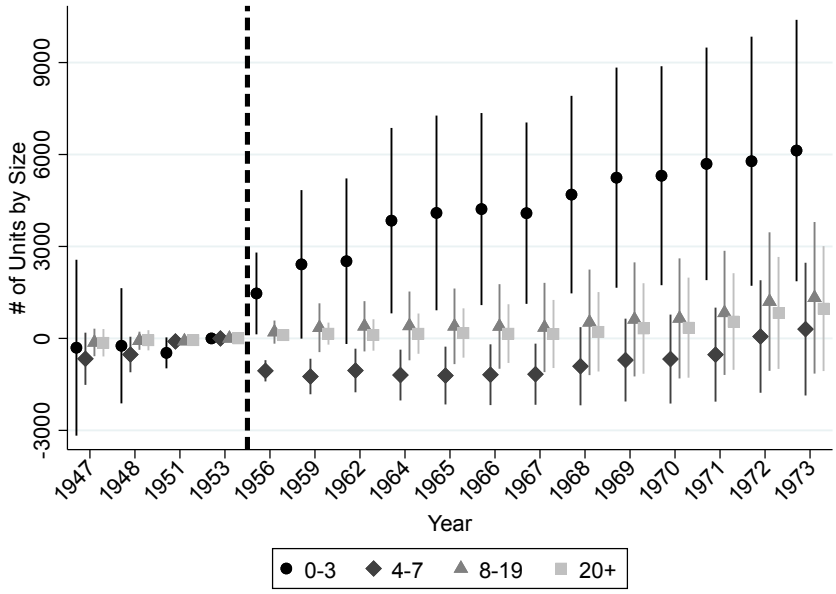
Appendix Figure A.4: Heterogeneity by State Tax Schedule



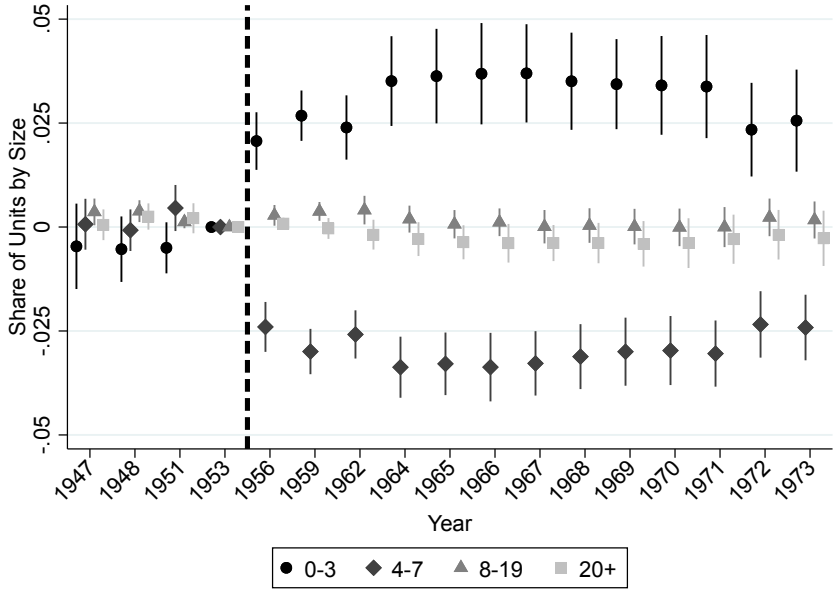
Notes: Figure displays the β_k coefficient estimates from eqn. (4), modified to permit heterogeneity across "high" and "low" tax states. State tax intensity is defined based on average UI payroll tax paid by firms in 1949 and 1953.

Appendix Figure A.5: Employer Size Effects with Region-by-Year FEs

(a) Count



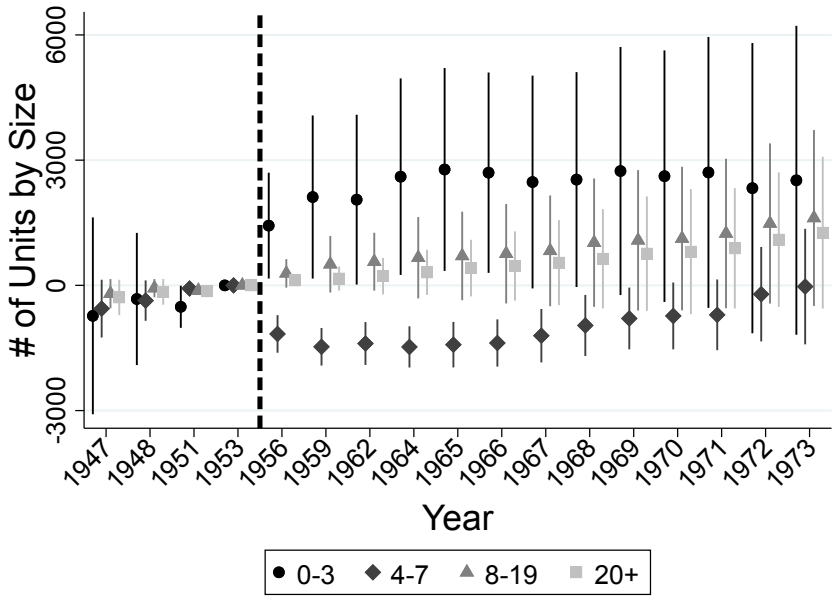
(b) Share



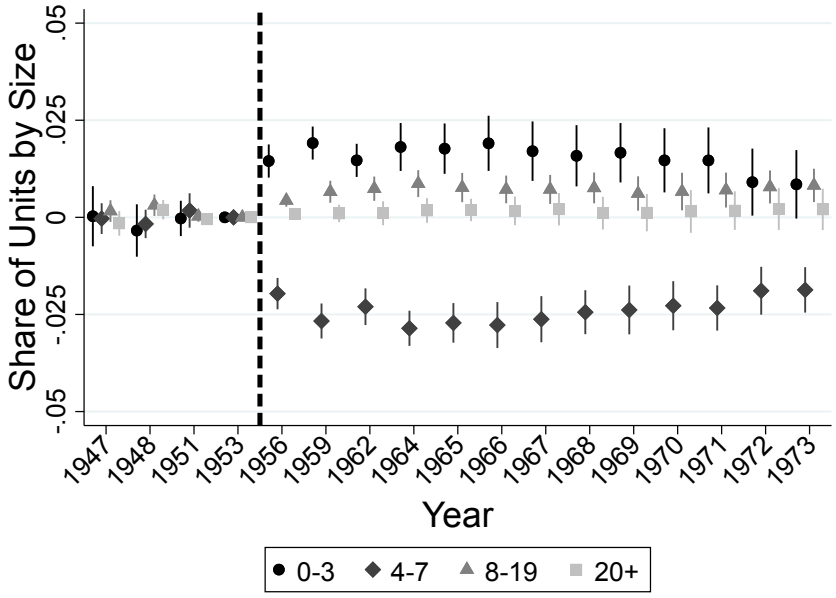
Notes: Figure displays the β_k coefficient estimates from eqn. (4), including region-by-year fixed effects. Notably, these estimates weight regions with more treatment variation more heavily, hence the slight differences from our primary specification. However, even in these more tightly controlled geographic comparisons, we see similar results: declining share of employers with 4–7 employees and growing share of employers with 0–3 employees.

Appendix Figure A.6: Employer Size Effects with Border-State-by-Year FEs

(a) Count



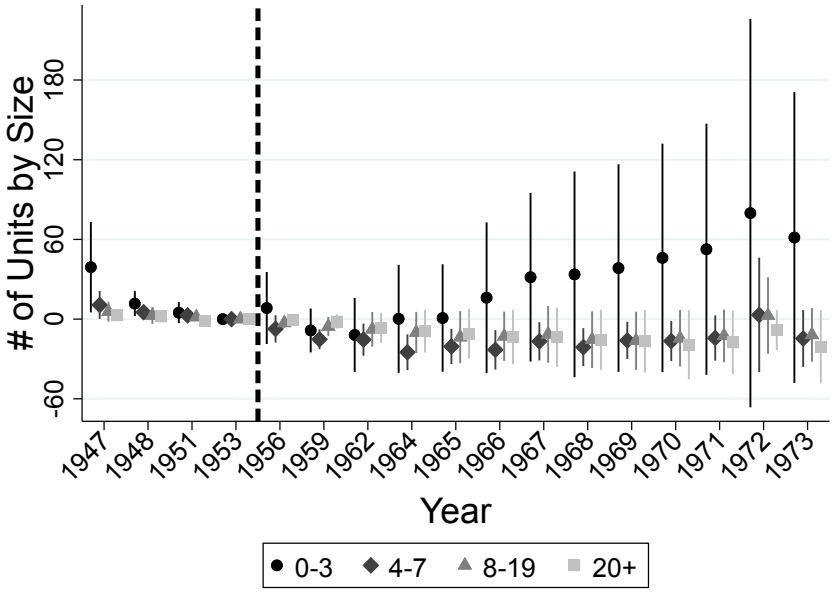
(b) Share



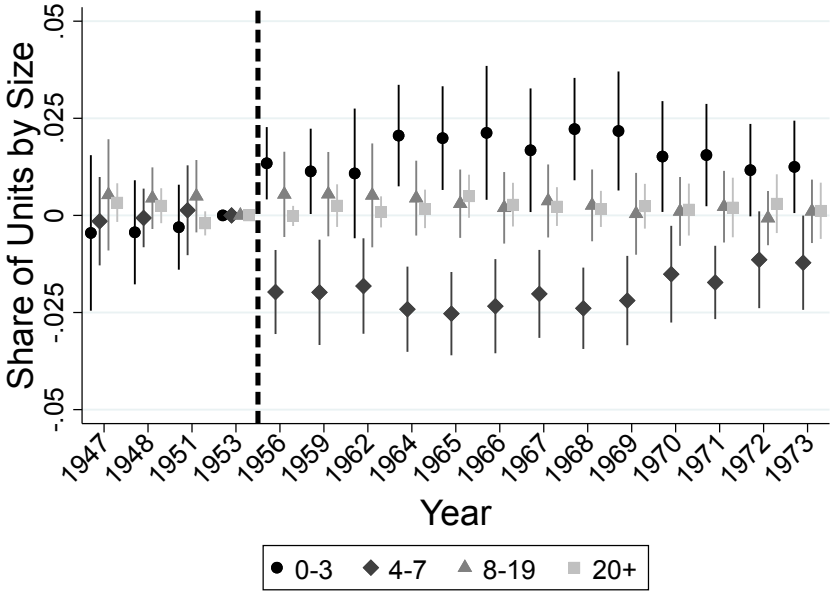
Notes: Figure displays the β_k coefficient estimates from eqn. (4), including border-state-by-year fixed effects. Notably, these estimates weight states with more border-pair treatment variation more heavily, hence the slight differences from our primary specification. However, even in these more tightly controlled geographic comparisons, we see similar results: declining share of employers with 4–7 employees and growing share of employers with 0–3 employees.

Appendix Figure A.7: Employer Size Effects with Border-County-by-Year FEs

(a) Count

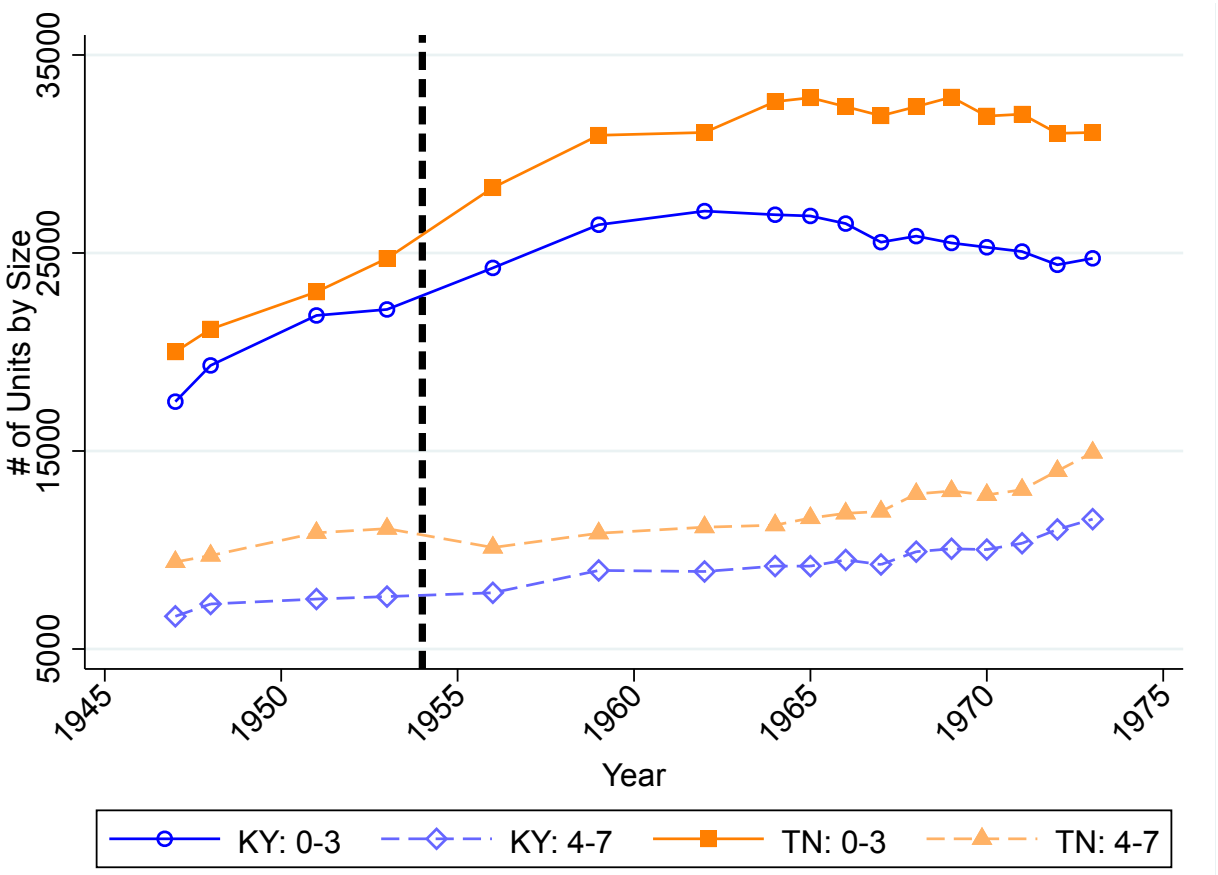


(b) Share



Notes: Figure displays the β_k coefficient estimates from eqn. (4), including border-county-by-year fixed effects. Each border-county observation is weighted by the inverse of the number of matched border observations in its state-year, so that states are given equal weight irrespective of how many border counties they have. Notably, these estimates rely solely on adjacent counties with border-pair treatment variation. However, even in these more tightly controlled geographic comparisons, we see similar results: declining share of employers with 4–7 employees and growing share of employers with 0–3 employees.

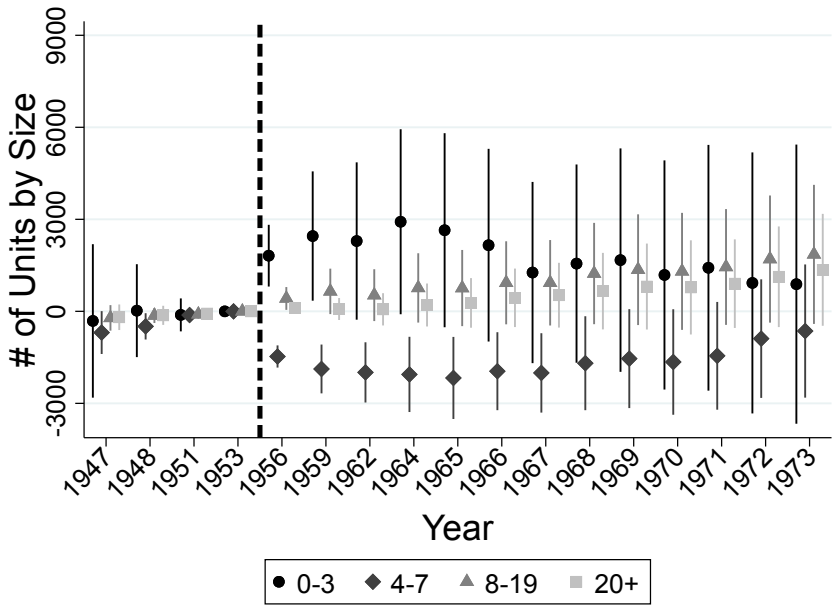
Appendix Figure A.8: Case Study Comparing Unit Counts in KY (Control) and TN (Treated)



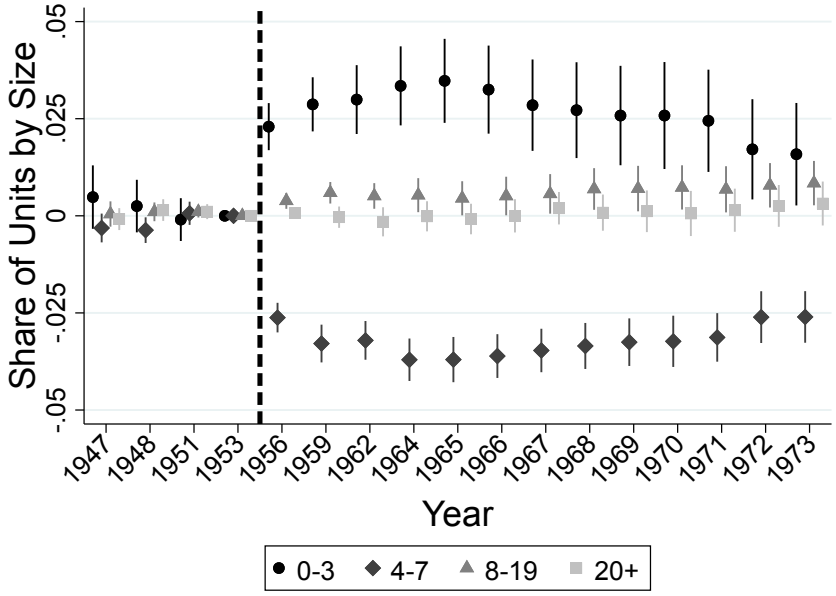
Notes: Figure displays raw count of reporting units by size class in two neighboring states, one treated and one control. This further addresses concerns related to the degree of geographic clustering in state treatment status.

Appendix Figure A.9: Employer Size Effects with Payroll Split-by-Year FEs

(a) Count

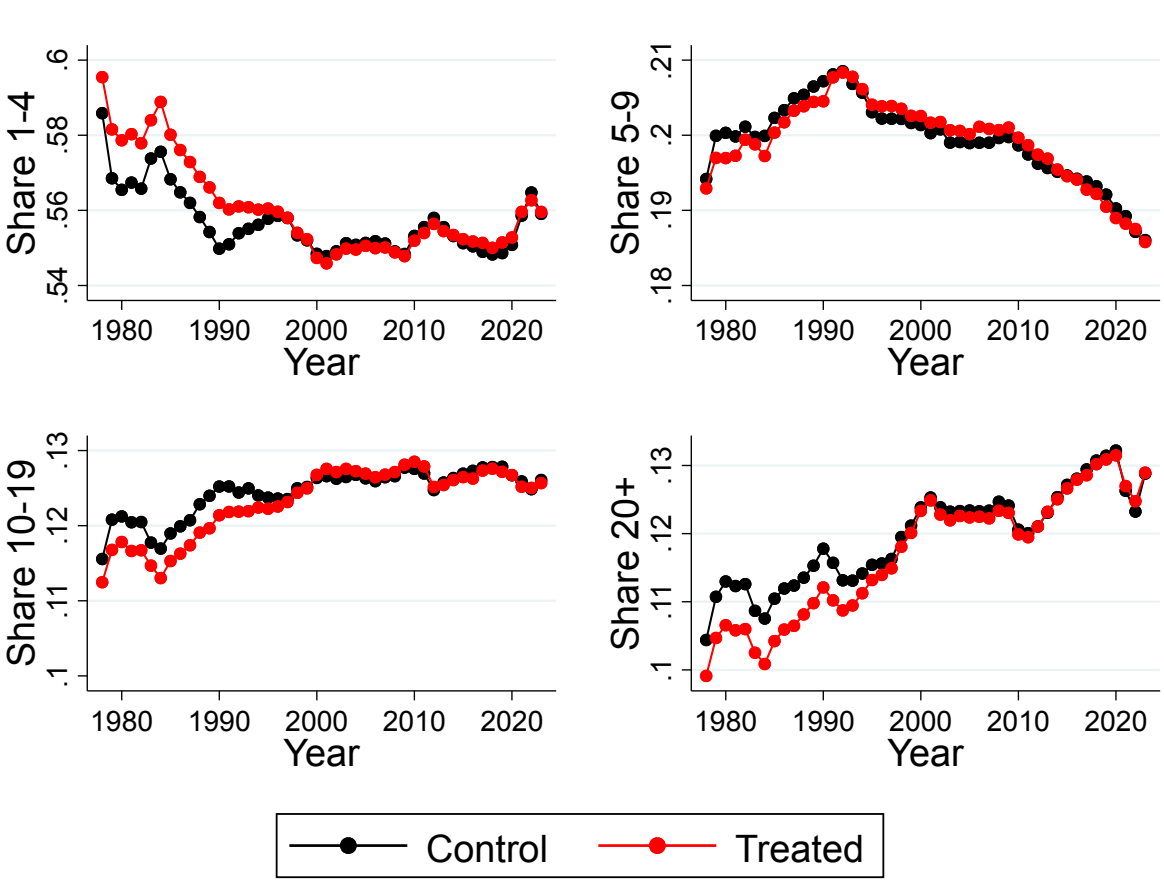


(b) Share



Notes: Figure displays the β_k coefficient estimates from eqn. (4), including payroll split-by-year fixed effects. Specifically, we split states by the median of payroll per employee in 1953, and then include split-by-year fixed effects.

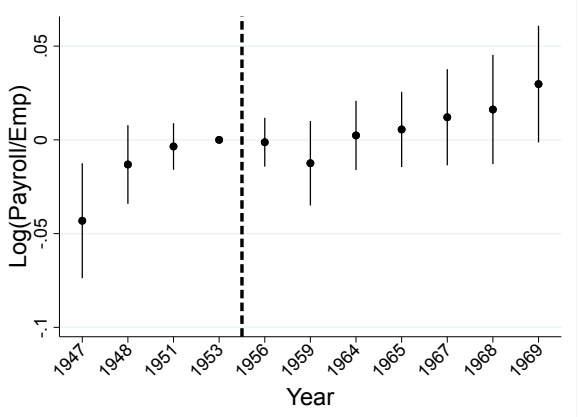
Appendix Figure A.10: Establishment Size Distribution from 1978 to 2023



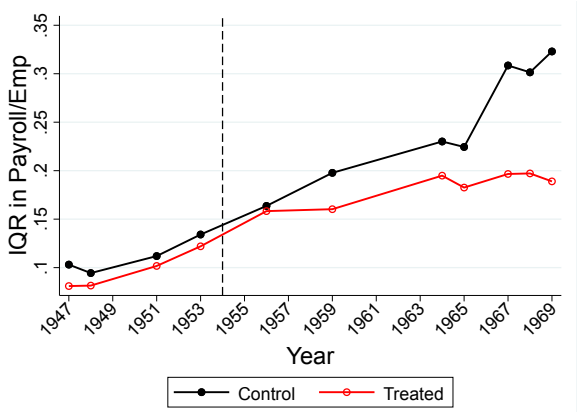
Notes: Figure displays the share of firms by establishment size class from 1978 to 2023, derived from the 2023 Business Dynamics Statistics data, and plotted separately for treated and control states. Note, the size bins differ slightly from the bins used in the historical CBP series. These patterns reflect both the distortions introduced by the varying state thresholds prior to 1954 and the new distortions that emerge with the shifting of the federal threshold in 1954.

Appendix Figure A.11: Effects on Distribution of Payroll per Employee

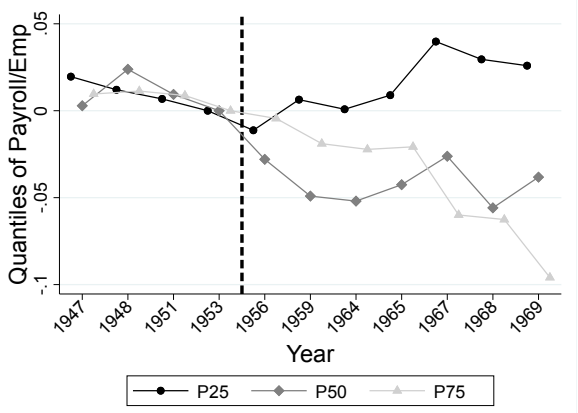
(a) Log(Payroll per Employee)



(b) Cross-State IQR of Payroll per Employee



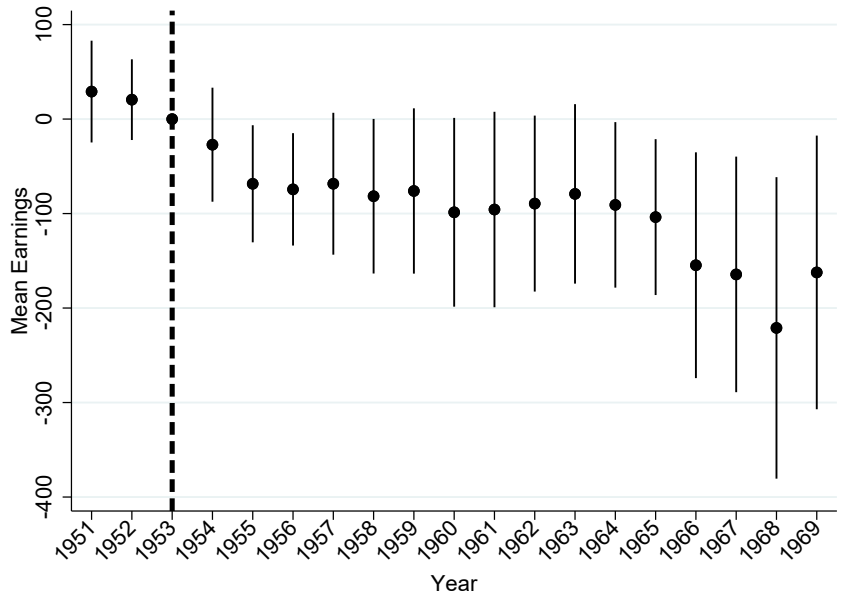
(c) Quantiles of Payroll per Employee



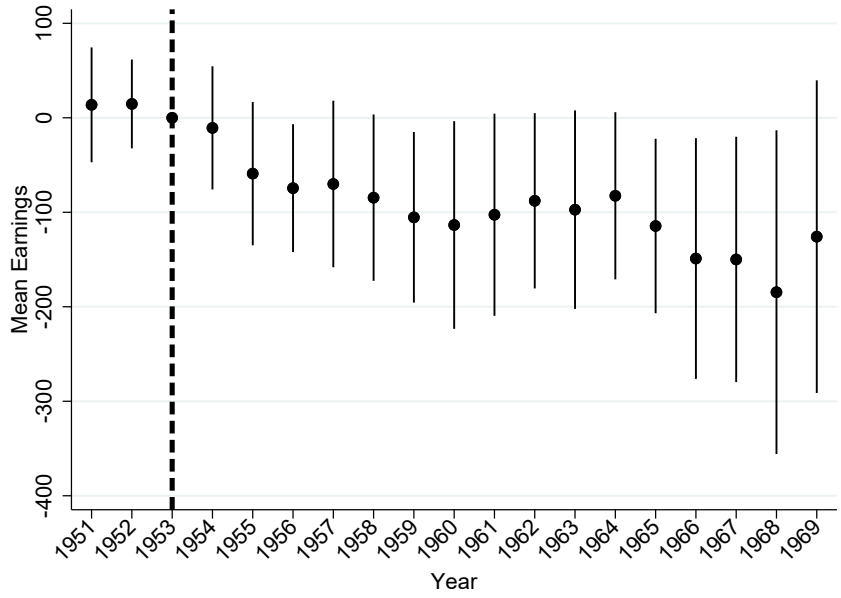
Notes: Figure displays various measures of payroll per employee over time by state treatment status. Panels (a) plots the β_k coefficient estimates from eqn. (4), with the natural log of payroll per employee as the dependent variable. Panel (b) plots the cross-state interquartile range in payroll per employee by year and state treatment status. Panel (c) plots the difference between treated and control states at various percentiles, relative to the differences between these states in 1953.

Appendix Figure A.12: Earning Effects using SSA-CPS 1978 Match

(a) Data Top-Coded



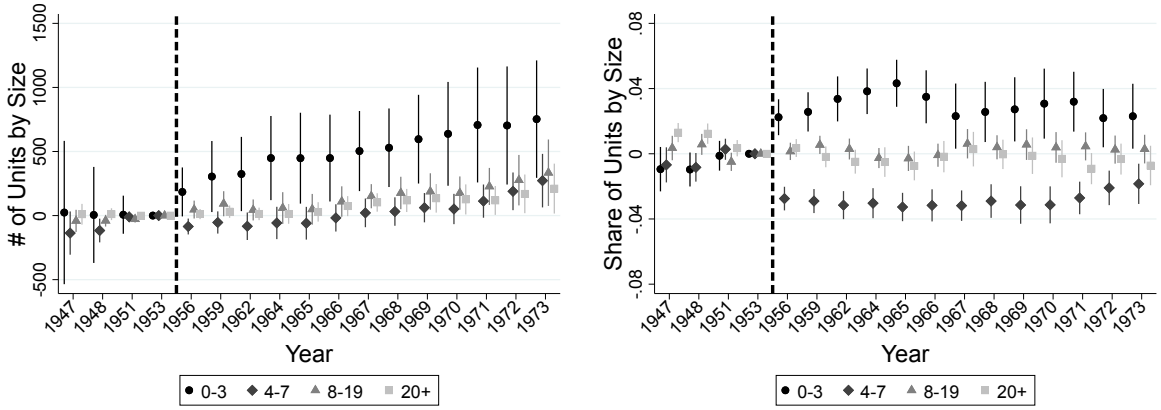
(b) Tobit Correction



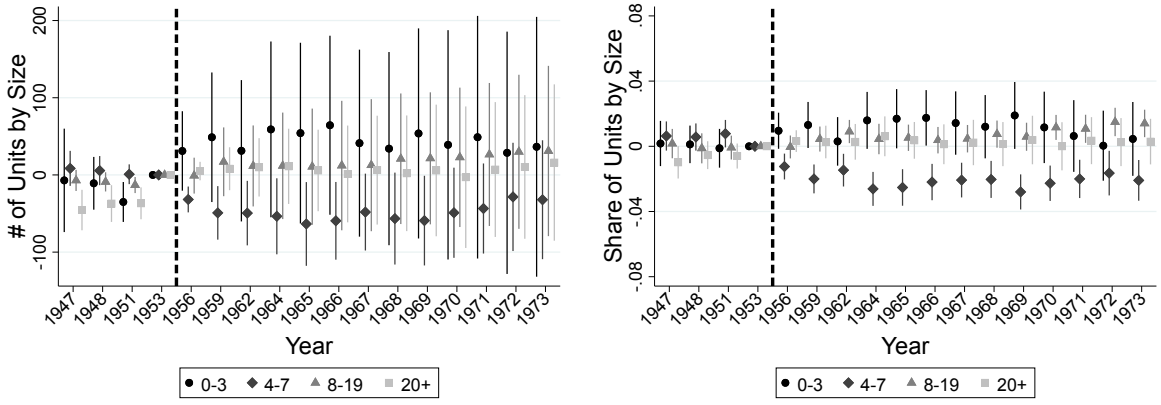
Notes: Figure displays β_k coefficient from eqn. 4. Panel (a) shows the data conditioning to people between 18 and 48 years old, with positive earnings, and in non-farm jobs. Since the SSA-CPS match is heavily top-coded in the period we study, panel (b) shows the results using a Tobit model to predict earnings, following Hungerford (2006). Specifically, we predict earnings using each individual's number of years of education, sex, race/ethnicity, age, age squared, and the interaction of the last two variables with the sex binary variable. We replace earnings with this predicted earnings when the predicted earnings are higher than the top-code.

Appendix Figure A.13: Heterogeneity in Employer Size Effects by Industry

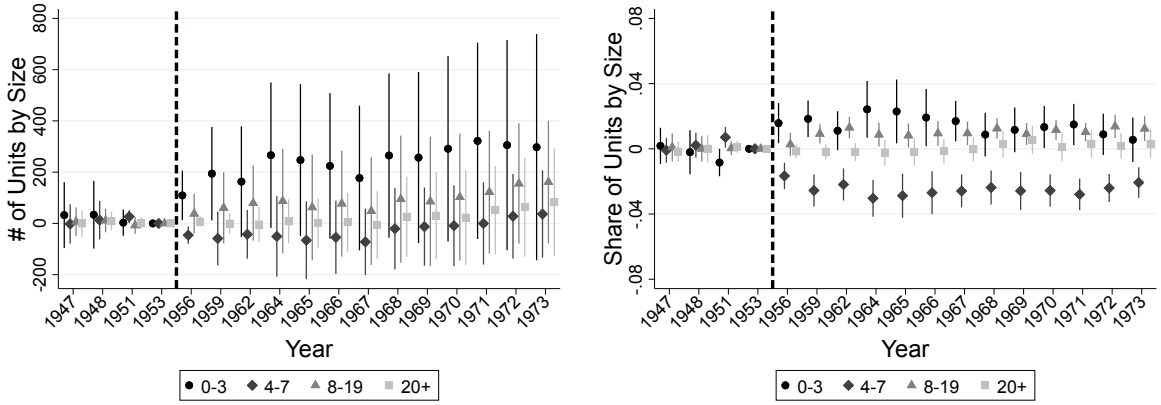
(a) Contract Construction



(b) Transportation and Public Utilities



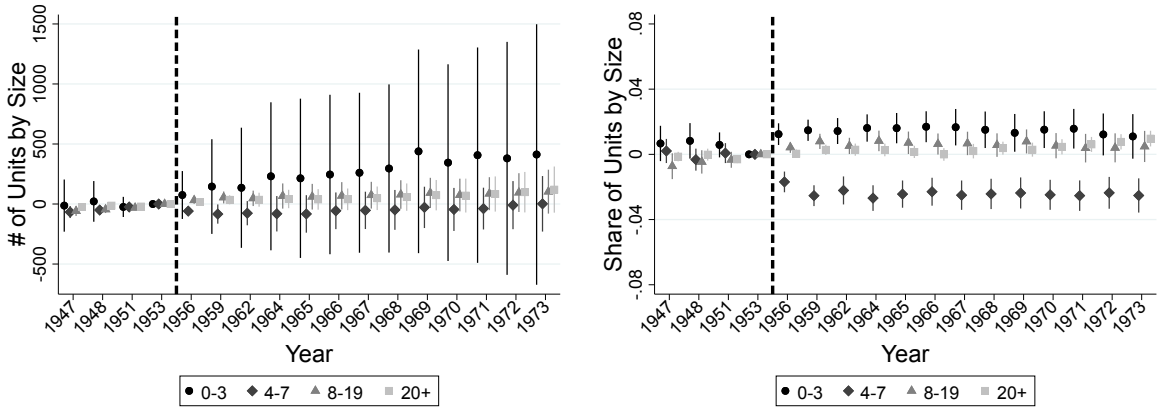
(c) Wholesale Trade



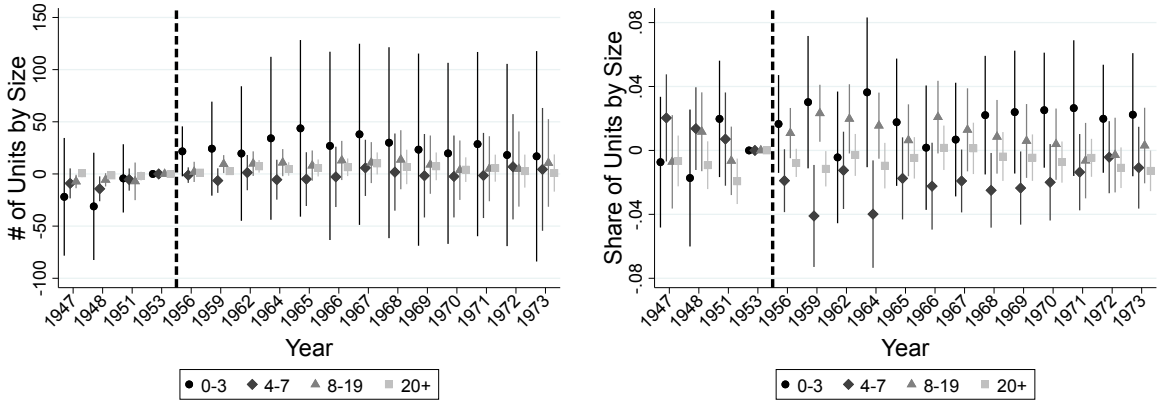
Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated by sector. Panels (a)-(c) present this for contract construction, transportation and public utilities, and wholesale trade, respectively. The figures on the left use raw counts by state-year as the outcome, while the figures on the right use shares by state-year as the outcome.

Appendix Figure A.14: Heterogeneity in Employer Size Effects by Industry

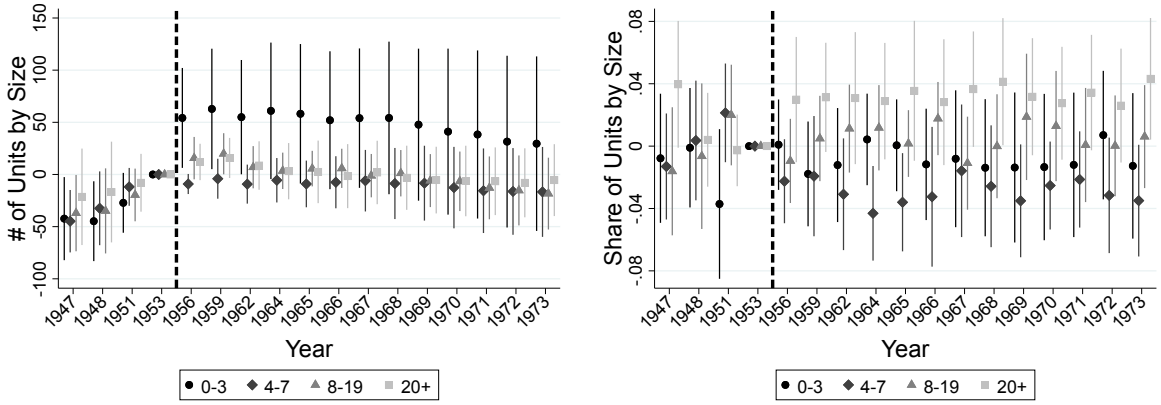
(a) Finance, Insurance, and Real Estate



(b) Agricultural Services



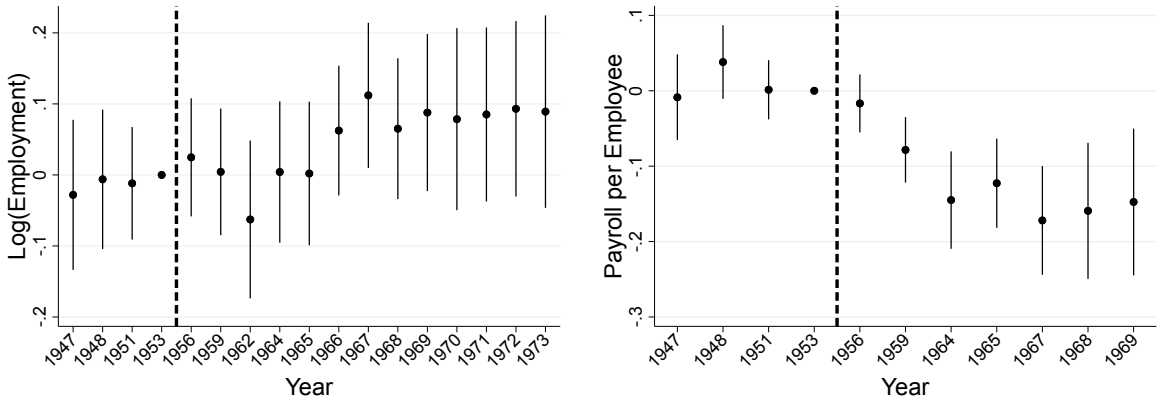
(c) Mining



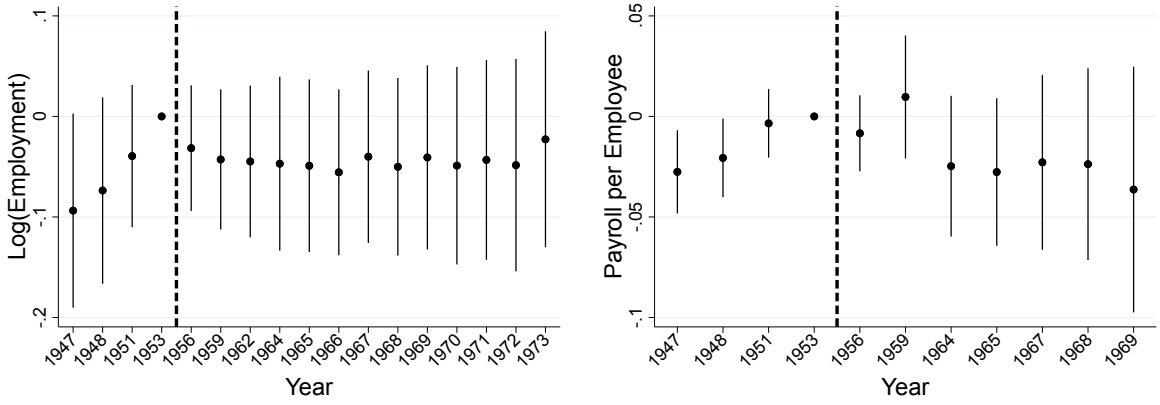
Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated by sector. Panels (a)-(c) present this for fire, insurance, and real estate; agricultural services; and mining, respectively. The figures on the left use raw counts by state-year as the outcome, while the figures on the right use shares by state-year as the outcome.

Appendix Figure A.15: Heterogeneity in Employment and Payroll Effects by Industry

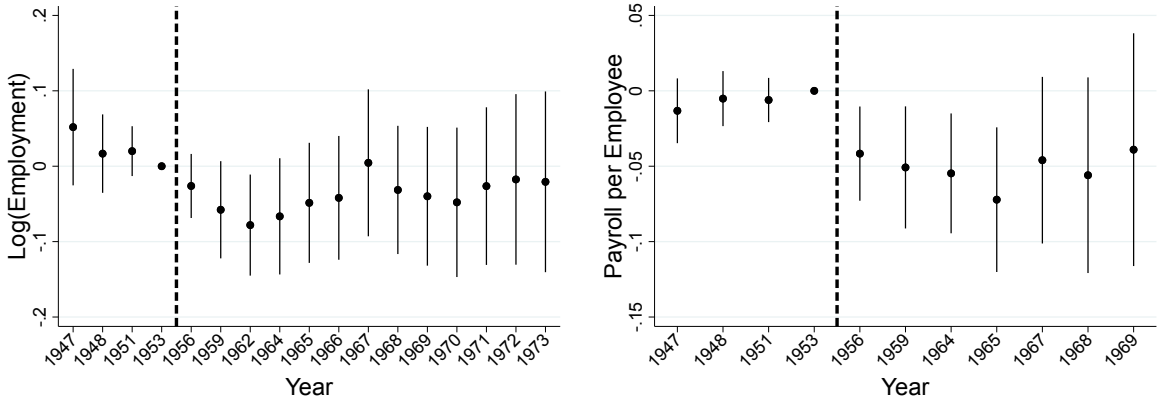
(a) Contract Construction



(b) Transportation and Public Utilities



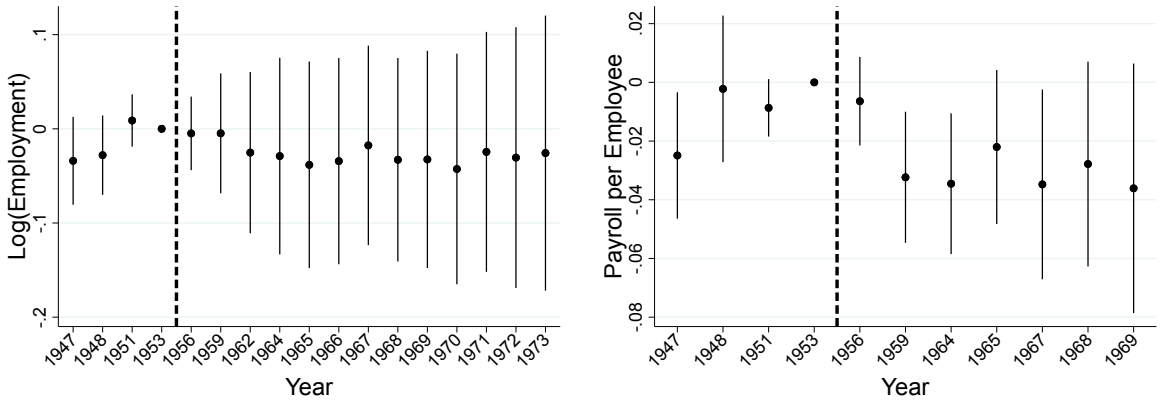
(c) Wholesale Trade



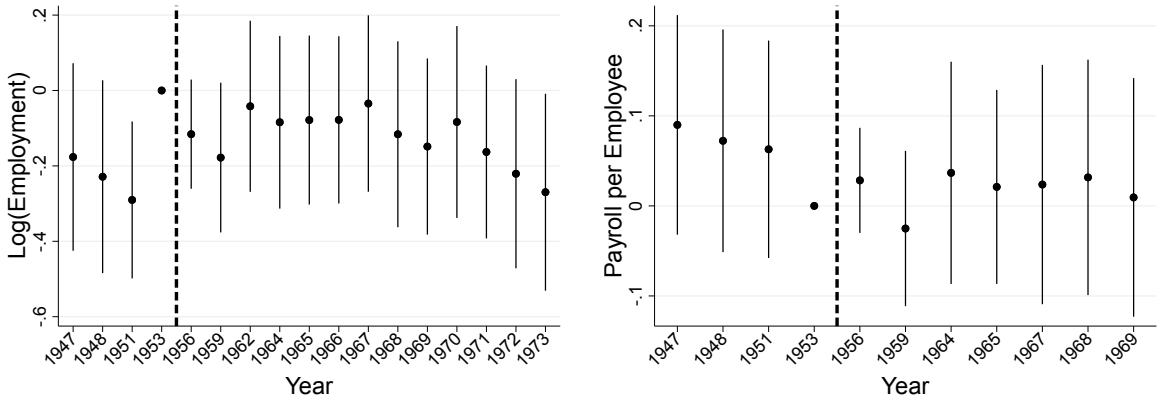
Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated by sector. Panels (a)-(c) present this for contract construction, transportation and public utilities, and wholesale trade, respectively. The figures on the left use log(employment) as the outcome, while the figures on the right use payroll-per-employee as the outcome.

Appendix Figure A.16: Heterogeneity in Employment and Payroll Effects by Industry

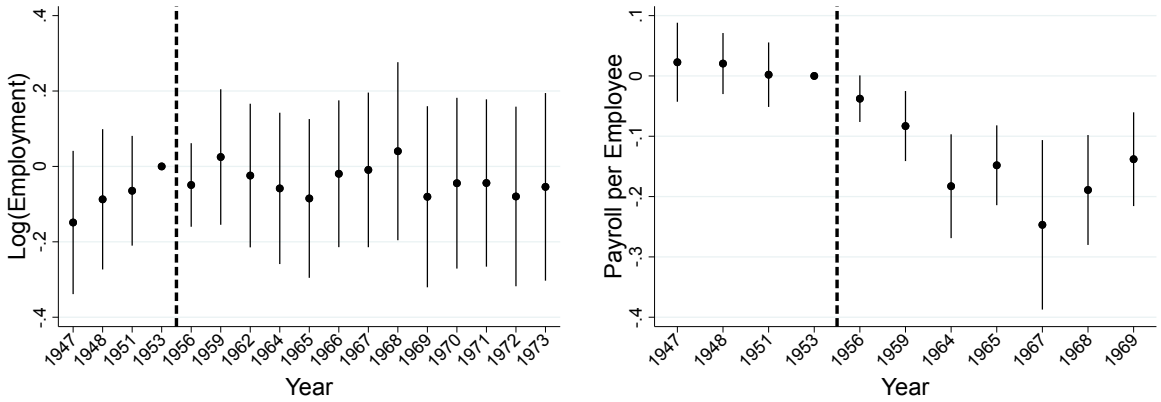
(a) Finance, Insurance, and Real Estate



(b) Agricultural Services



(c) Mining



Notes: Figure displays the β_k coefficient estimates from eqn. (4), estimated by sector. Panels (a)-(c) present this for fire, insurance, and real estate; agricultural services; and mining, respectively. The figures on the left use log(employment) as the outcome, while the figures on the right use payroll-per-employee as the outcome.

Appendix Table A.1: Effects Pooled Across Multiple Years, Manufacturing

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	113.31	–109.32*	33.12	21.57	0.017***	–0.026***	0.004	0.006	–0.008	–0.013
	(106.06)	(59.12)	(84.17)	(100.97)	(0.006)	(0.004)	(0.004)	(0.004)	(0.042)	(0.021)
1960–1965 x Treated	221.88	–107.64	26.38	73.09	0.031***	–0.033***	–0.004	0.006	–0.001	–0.051
	(176.09)	(85.37)	(137.11)	(162.58)	(0.010)	(0.006)	(0.008)	(0.007)	(0.064)	(0.034)
1966–1970 x Treated	273.80	–60.26	62.28	154.72	0.026**	–0.033***	–0.009	0.016*	0.063	–0.052
	(231.33)	(113.68)	(159.62)	(227.35)	(0.012)	(0.007)	(0.010)	(0.008)	(0.073)	(0.045)
1971–1973 x Treated	262.55	–22.91	129.42	319.66	0.011	–0.034***	–0.010	0.032***	0.100	
	(271.88)	(146.71)	(209.42)	(285.85)	(0.012)	(0.007)	(0.010)	(0.010)	(0.094)	
Dep. Var. Mean	1,441.96	972.39	1,250.99	2,016.20	0.277	0.182	0.216	0.325	11.900	1.158
Observations	816	816	816	816	816	816	816	816	816	526
R-squared	0.96	0.98	0.98	0.98	0.842	0.813	0.413	0.946	0.992	0.970

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.2: Effects Pooled Across Multiple Years, Retail Trade

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	775.34	–723.97***	157.01	41.54	0.035***	–0.037***	0.003	–0.000	–0.026	–0.017**
	(475.21)	(184.61)	(123.91)	(60.05)	(0.004)	(0.003)	(0.002)	(0.001)	(0.031)	(0.008)
1960–1965 x Treated	1,096.09	–954.32***	203.14	35.50	0.049***	–0.047***	0.002	–0.003	–0.069	–0.024**
	(675.48)	(330.72)	(238.85)	(126.61)	(0.008)	(0.004)	(0.004)	(0.003)	(0.045)	(0.010)
1966–1970 x Treated	1,097.11	–959.89**	304.07	88.38	0.051***	–0.048***	0.003	–0.005	–0.062	–0.007
	(922.84)	(410.58)	(377.45)	(233.85)	(0.010)	(0.005)	(0.005)	(0.004)	(0.056)	(0.014)
1971–1973 x Treated	1,191.73	–694.71	394.37	166.61	0.045***	–0.040***	0.002	–0.007	–0.064	
	(1,260.61)	(471.06)	(484.11)	(327.70)	(0.011)	(0.006)	(0.005)	(0.005)	(0.070)	
Dep. Var. Mean	10,847.63	4,694.06	3,147.05	1,488.54	0.529	0.236	0.162	0.073	11.552	0.714
Observations	815	815	815	815	815	815	815	815	813	523
R-squared	0.97	0.98	0.96	0.94	0.934	0.824	0.934	0.935	0.990	0.985

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.3: Effects Pooled Across Multiple Years, Services

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	226.41	–329.95*	–7.50	–46.88	0.023***	–0.025***	0.003	–0.001	–0.054*	–0.029**
	(668.86)	(166.94)	(119.03)	(63.11)	(0.005)	(0.003)	(0.002)	(0.001)	(0.029)	(0.014)
1960–1965 x Treated	335.23	–495.93	–53.30	–137.41	0.028***	–0.029***	0.003	–0.003	–0.093*	–0.058**
	(1,204.92)	(408.58)	(288.89)	(183.07)	(0.007)	(0.004)	(0.003)	(0.002)	(0.051)	(0.022)
1966–1970 x Treated	292.09	–487.79	–40.21	–152.65	0.026***	–0.028***	0.004	–0.002	–0.075	–0.060**
	(1,223.83)	(577.72)	(415.91)	(295.37)	(0.008)	(0.004)	(0.003)	(0.003)	(0.056)	(0.028)
1971–1973 x Treated	308.83	–366.82	41.19	–109.09	0.019**	–0.023***	0.005	–0.001	–0.066	
	(1,190.24)	(762.36)	(531.08)	(374.71)	(0.009)	(0.005)	(0.004)	(0.003)	(0.064)	
Dep. Var. Mean	10,759.36	2,959.04	1,858.71	1,105.66	0.658	0.176	0.106	0.061	11.223	0.725
Observations	816	816	816	816	816	816	816	816	815	525
R-squared	0.98	0.93	0.92	0.90	0.947	0.886	0.921	0.940	0.993	0.969

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.4: Effects Pooled Across Multiple Years, Contract Construction

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	235.67	–3.61	97.02*	15.97	0.029***	–0.025***	0.002	–0.006*	0.026	–0.055***
	(266.38)	(54.93)	(57.55)	(39.68)	(0.007)	(0.004)	(0.003)	(0.004)	(0.042)	(0.019)
1960–1965 x Treated	398.10	–0.99	77.90	13.20	0.043***	–0.028***	–0.002	–0.013**	–0.007	–0.141***
	(301.83)	(81.35)	(71.31)	(54.50)	(0.009)	(0.005)	(0.004)	(0.005)	(0.060)	(0.037)
1966–1970 x Treated	534.23**	95.36	187.43**	105.84	0.033***	–0.028***	0.003	–0.008	0.093	–0.167***
	(229.28)	(72.79)	(80.10)	(67.44)	(0.012)	(0.005)	(0.005)	(0.006)	(0.060)	(0.053)
1971–1973 x Treated	712.15***	257.54**	306.69**	160.89	0.031***	–0.019***	0.002	–0.014**	0.101	
	(247.52)	(110.32)	(126.02)	(102.64)	(0.011)	(0.005)	(0.005)	(0.006)	(0.076)	
Dep. Var. Mean	2,998.91	1,218.80	891.67	542.43	0.529	0.218	0.158	0.094	10.370	1.142
Observations	815	815	815	815	815	815	815	815	815	524
R-squared	0.97	0.98	0.97	0.95	0.933	0.812	0.888	0.905	0.983	0.962

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.5: Effects Pooled Across Multiple Years, Transportation and Public Utilities

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	53.15	–44.22**	15.34	36.35**	0.011	–0.021***	0.002	0.008*	0.015	0.014
	(51.36)	(16.65)	(20.58)	(16.04)	(0.007)	(0.003)	(0.004)	(0.005)	(0.036)	(0.011)
1960–1965 x Treated	61.38	–59.25*	18.66	38.75	0.012	–0.027***	0.006	0.009	0.005	–0.013
	(74.51)	(30.59)	(39.05)	(30.23)	(0.009)	(0.005)	(0.004)	(0.006)	(0.054)	(0.020)
1966–1970 x Treated	59.70	–58.20*	25.30	32.33	0.014	–0.028***	0.007	0.007	0.005	–0.015
	(81.76)	(34.56)	(51.14)	(48.18)	(0.011)	(0.005)	(0.004)	(0.007)	(0.059)	(0.028)
1971–1973 x Treated	51.23	–38.43	36.45	40.96	0.003	–0.024***	0.013**	0.008	0.014	
	(98.98)	(42.73)	(60.19)	(58.30)	(0.012)	(0.007)	(0.005)	(0.008)	(0.072)	
Dep. Var. Mean	1,055.66	446.01	439.63	417.99	0.445	0.192	0.189	0.174	10.460	1.171
Observations	816	816	816	816	816	816	816	816	815	525
R-squared	0.98	0.98	0.97	0.97	0.880	0.638	0.822	0.877	0.990	0.987

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.6: Effects Pooled Across Multiple Years, Wholesale Trade

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	134.45	–61.47	46.64	0.89	0.019***	–0.023***	0.006*	–0.002	–0.064*	–0.040*
	(101.44)	(46.28)	(68.84)	(31.86)	(0.006)	(0.005)	(0.003)	(0.003)	(0.035)	(0.022)
1960–1965 x Treated	208.21	–62.33	73.83	–0.93	0.022**	–0.029***	0.010**	–0.002	–0.086*	–0.057**
	(180.64)	(86.78)	(116.60)	(62.87)	(0.009)	(0.006)	(0.004)	(0.004)	(0.051)	(0.026)
1966–1970 x Treated	225.71	–43.00	79.13	13.06	0.016**	–0.028***	0.010***	0.002	–0.053	–0.041
	(216.12)	(97.95)	(145.52)	(103.15)	(0.007)	(0.005)	(0.003)	(0.005)	(0.060)	(0.040)
1971–1973 x Treated	290.96	12.17	143.82	63.68	0.012	–0.026***	0.012**	0.003	–0.043	
	(275.67)	(112.25)	(150.47)	(127.47)	(0.008)	(0.005)	(0.005)	(0.005)	(0.075)	
Dep. Var. Mean	2,219.26	1,234.75	1,205.75	762.63	0.423	0.227	0.219	0.131	10.486	1.188
Observations	816	816	816	816	816	816	816	816	814	523
R-squared	0.98	0.99	0.98	0.97	0.903	0.514	0.876	0.912	0.991	0.978

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.7: Effects Pooled Across Multiple Years, Finance, Insurance, and Real Estate

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	114.17	–35.93	76.91**	40.89*	0.008*	–0.021***	0.010***	0.003	0.008	–0.010
	(231.77)	(40.65)	(32.11)	(23.68)	(0.005)	(0.003)	(0.003)	(0.002)	(0.041)	(0.012)
1960–1965 x Treated	197.42	–44.66	92.05	53.32	0.010*	–0.024***	0.011***	0.004	–0.018	–0.019
	(395.59)	(85.87)	(60.23)	(49.62)	(0.006)	(0.005)	(0.004)	(0.003)	(0.070)	(0.016)
1966–1970 x Treated	321.11	–10.88	111.57	72.41	0.010	–0.024***	0.010**	0.004	–0.019	–0.024
	(464.09)	(104.47)	(77.48)	(73.60)	(0.008)	(0.005)	(0.005)	(0.003)	(0.076)	(0.022)
1971–1973 x Treated	403.82	20.16	128.64	115.50	0.008	–0.025***	0.008	0.009***	–0.014	
	(606.73)	(127.77)	(100.87)	(106.26)	(0.009)	(0.006)	(0.006)	(0.003)	(0.092)	
Dep. Var. Mean	3,853.72	929.81	635.47	435.25	0.624	0.178	0.123	0.075	10.232	1.015
Observations	816	816	816	816	816	816	816	816	815	524
R-squared	0.98	0.97	0.97	0.94	0.919	0.830	0.818	0.913	0.990	0.987

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.8: Effects Pooled Across Multiple Years, Agricultural Services

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	37.27	3.42	10.72**	2.69	0.025	–0.040***	0.018*	–0.001	0.027	–0.054*
	(30.04)	(6.93)	(4.26)	(1.63)	(0.019)	(0.012)	(0.010)	(0.005)	(0.088)	(0.029)
1960–1965 x Treated	46.80	4.11	14.56**	6.64	0.018	–0.034**	0.014	0.003	0.105	–0.027
	(53.76)	(13.56)	(6.07)	(4.22)	(0.022)	(0.013)	(0.010)	(0.006)	(0.118)	(0.033)
1966–1970 x Treated	41.90	7.31	14.97	7.63	0.017	–0.032***	0.011	0.006	0.081	–0.034
	(60.23)	(21.58)	(12.10)	(7.26)	(0.019)	(0.010)	(0.011)	(0.007)	(0.123)	(0.044)
1971–1973 x Treated	35.55	10.43	11.74	3.75	0.024	–0.020**	–0.002	–0.001	–0.044	
	(60.18)	(30.82)	(18.92)	(9.39)	(0.020)	(0.010)	(0.012)	(0.006)	(0.127)	
Dep. Var. Mean	326.01	107.03	64.19	24.52	0.629	0.205	0.119	0.046	7.385	0.686
Observations	816	816	816	816	816	816	816	816	815	524
R-squared	0.94	0.90	0.90	0.82	0.705	0.502	0.620	0.503	0.958	0.798

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.9: Effects Pooled Across Multiple Years, Mining

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	87.09*	15.67	40.67*	25.47	0.003	–0.024*	–0.002	0.020	0.063	–0.071***
	(43.50)	(13.51)	(21.01)	(26.71)	(0.019)	(0.014)	(0.013)	(0.020)	(0.105)	(0.023)
1960–1965 x Treated	86.61*	14.31	28.26	16.30	0.009	–0.040**	0.009	0.021	0.019	–0.177***
	(47.25)	(19.17)	(18.38)	(30.38)	(0.017)	(0.016)	(0.017)	(0.023)	(0.133)	(0.042)
1966–1970 x Treated	78.38	13.67	21.60	8.66	–0.001	–0.030*	0.008	0.023	0.052	–0.203***
	(49.99)	(26.21)	(21.19)	(32.49)	(0.023)	(0.015)	(0.021)	(0.020)	(0.143)	(0.046)
1971–1973 x Treated	61.70	6.09	7.41	4.91	0.006	–0.032**	0.003	0.024	0.015	
	(52.70)	(30.06)	(23.23)	(32.26)	(0.024)	(0.015)	(0.021)	(0.019)	(0.148)	
Dep. Var. Mean	202.69	97.78	112.75	113.71	0.357	0.190	0.229	0.223	8.396	1.270
Observations	816	816	816	816	816	816	816	816	815	524
R-squared	0.95	0.93	0.95	0.93	0.774	0.323	0.457	0.776	0.978	0.934

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Appendix Table A.10: Effects Pooled Across Multiple Years, Unclassified

	Count of Employers				Share of Employers					
	0-3	4-7	8-19	20+	0-3	4-7	8-19	20+	Log(Emp.)	Pay/Emp.
1954–1959 x Treated	133.07	–19.25	–5.64	–9.39	0.018	–0.015**	–0.005	–0.005**	0.021	–0.039*
	(97.41)	(24.19)	(29.15)	(14.14)	(0.012)	(0.006)	(0.004)	(0.002)	(0.075)	(0.023)
1960–1965 x Treated	144.34	–22.54	9.19	–1.51	0.030**	–0.029***	–0.001	–0.006***	0.029	–0.049
	(156.73)	(31.45)	(13.31)	(3.54)	(0.014)	(0.005)	(0.007)	(0.002)	(0.074)	(0.033)
1966–1970 x Treated	61.30	–49.81**	–14.89	–6.66	0.037**	–0.033***	–0.007	–0.004**	–0.041	–0.065*
	(96.85)	(19.39)	(27.00)	(8.99)	(0.014)	(0.007)	(0.005)	(0.002)	(0.076)	(0.034)
1971–1973 x Treated	106.82	–35.25	–1.59	–3.61	0.023	–0.024***	–0.005	–0.001	–0.118	
	(116.42)	(54.88)	(53.68)	(17.35)	(0.015)	(0.007)	(0.005)	(0.002)	(0.083)	
Dep. Var. Mean	1,006.73	259.31	139.61	36.36	0.703	0.180	0.094	0.023	8.135	0.677
Observations	816	816	816	816	816	816	816	816	814	525
R-squared	0.87	0.84	0.79	0.74	0.637	0.525	0.693	0.643	0.949	0.897

Notes: Entries are point estimates and standard errors from estimating eqn. (5). Columns 1-4 show specifications with raw counts by size class in each state-year as the outcome. Columns 5-8 show specifications with the share of units by size class in each state-year as the outcome. Columns 9 and 10 show specifications with log(employment) and quarter 1 payroll-per-employee as the outcomes, respectively. In these specifications, the reference period pools across all pre-1954 years. Standard errors are clustered at the state level.

Online Appendix B: Aggregate Effects of UI Policy Design

In this section, we estimate a structural static model of firm heterogeneity (Lucas 1978) to study the aggregate effects of the UI coverage system. We use empirical variation in the firm size distribution across states, sectors, and policy regimes to calibrate and validate the model, which we then use to quantify the aggregate output and productivity consequences of the changes in coverage.

The aggregate effects of threshold-based coverage designs depend critically on the sector. Relative to a system with identical tax rates but no coverage threshold, threshold designs reduce aggregate productivity in all sectors. Their effects on output, however, diverge sharply. In manufacturing, the pre-reform threshold of 8 suppresses output, so reducing it to 4 is beneficial; in retail and services, a threshold of 8 distorts the decisions of a limited set of firms while exempting the vast majority below it, so lowering the threshold erodes the exemption gain and expands the set of firms that bunch below the new, lower threshold.

Two data limitations guide our modeling choices. First, the CBP records employer size in discrete bins. Our primary set of moment conditions therefore comes from the distribution of establishments across size classes. Second, the CBP does not report employment or payroll separately within bins. We consequently have good variation in how firms sort across the size distribution, but limited information on labor market quantities within each size class.

The data also have features that are well-suited to our purpose. First, the CBP size bins align perfectly with the relevant coverage threshold categories–a feature we exploit in the difference-in-differences analysis and that proves equally useful here. Second, a subset of the control states not only maintained stable coverage thresholds through 1954 but effectively imposed no threshold over the entire sample period. Roughly two-thirds of control states had a coverage threshold of one employee. We refer to these states as "full coverage states."

This last feature directs us toward a model in which bunching behavior is the key identifying moment. The model allows us to recover a sector-level effective tax rate τ_f by studying differences in the firm size distribution between treated states and full coverage control states. Given the model, greater bunching below the coverage threshold reveals a higher τ_f. We assume a uniform rate for all covered firms within a sector. In practice, statutory payroll tax rates vary across states, experience rating generates within-sector variation, and wages may adjust to offset part of the coverage cost. Our model and data do not allow us to separate these channels; τ_f is best interpreted as a reduced-form summary of the full net cost of coverage borne by the firm, rather than a structural estimate of any single parameter.

B.1. *Model Set-up*

We adopt a Lucas span-of-control framework with a continuum of firms indexed by $i \in [0, 1]$. Firms are heterogeneous in productivity z_i, drawn independently from a lognormal distribution with sector-specific location and scale parameters (μ_j, σ_j^2). The span-of-control parameter $\gamma < 1$ generates decreasing returns to scale, ensuring a finite optimal firm size; α_j is the capital production function parameter for sector j.

$$\text{(B.1)} \quad y = z_i(k^{\alpha_j} n^{1-\alpha_j})^{\gamma}, \qquad \log(z_i) \sim N(\mu_j, \sigma_j^2)$$

We set $\gamma = 0.802$, following Guner et al. (2008), who calibrate this value in a span-of-control model matched to the U.S. establishment size distribution. For manufacturing, we infer α_j from the 1948 Census of Manufactures, which yields a capital value-added share of 0.497 (one minus the labor share); for retail and services, we use the capital share of 0.34 from Valentinyi and Herrendorf (2008). Together, γ and α_j pin down all technology parameters prior to estimation.

The model admits a tractable reduced-form representation that connects the firm's problem directly to the size distribution we observe. Define $a \equiv \alpha_j \gamma$ and $b \equiv (1-\alpha_j)\gamma$, so $a+b = \gamma < 1$. Substituting the capital optimality condition into the profit function eliminates capital and yields a reduced-form profit over employment alone:

$$\text{(B.2)} \quad \pi(n;\, z;\, w) = C z^{1/(1-a)} n^{\phi} \;-\; W(n), \qquad \phi \equiv \frac{b}{1-a}, \quad C \equiv (1-a)\Big(\frac{a}{r}\Big)^{a/(1-a)}$$

where $\phi < 1$ since $b < 1 - a$ whenever $\gamma < 1$, ensuring strict concavity in n. $W(n)$ are the labor costs:

$$\text{(B.3)} \quad W(n) = \begin{cases} w_u n & \text{if } n < N \\ w_c(1+\tau_i)n = w_u(1+\tau_f)n & \text{if } n \geq N \end{cases}$$

The parameter τ_f summarizes the net labor cost to the firm after accounting for potential wage adjustments. To see this, note that covered firms can pass through a share ρ of their UI statutory tax contribution (τ_i) to workers via lower wages, so the equilibrium wage is $w_c = w_u(1-\rho\tau_i)$. The firm's total labor cost per worker is then $w_c(1+\tau_i) = w_u(1-\rho\tau_i)(1+\tau_i)$, which defines $1 + \tau_f \equiv (1 - \rho\tau_i)(1 + \tau_i)$.

The unconstrained optimal employment, given below, is strictly increasing in z. The productivity distribution therefore maps monotonically into a size distribution.

$$\text{(B.4)} \quad n^*(z;\, w) = \left(\frac{\phi\, C\, z^{1/(1-a)}}{w} \right)^{1/(1-\phi)}$$

A firm facing threshold N falls into one of three regimes depending on its productivity draw. If $n^*(z;\, w_u) < N$, the firm is naturally uncovered and operates at its unconstrained optimum under the uncovered wage w_u. If $n^*(z;\, w_c) \geq N$, the firm faces a discrete cost at the threshold: it either bunches at $N-1$ or crosses into coverage and pays the labor costs of $w_u(1+\tau_f)$. The bunching interval $[z^-, \bar{z}]$ is defined by two boundary conditions. The lower bound z^- satisfies $n^*(z^-;\, w_u) = N$. It is the productivity level at which the unconstrained uncovered optimum just reaches the threshold. The upper bound $\bar{z}$ satisfies the indifference condition:

$$\text{(B.5)} \quad \pi(N-1;\ \bar{z};\, w_u) = \max_n\ \pi(n;\ \bar{z};\, w_c)$$

a firm with $z = \bar{z}$ is just indifferent between bunching at $N-1$ and operating under coverage. Firms with $z > \bar{z}$ are covered and choose $n^*(z, w_c)$. Substituting equation (B.2) into (B.5) yields an equation in $\bar{z}$ with no closed form, but which does have a unique solution that we solve numerically for each sector. Bunching is more extensive when τ_f is higher and when ϕ is smaller, making it less costly to hold employment at $N-1$.

The three regimes map directly to the observed CBP size bins. Since z_i is log-normally distributed with parameters (μ_j, σ_j^2), the probability that a firm falls in each bin is an analytic function of $(\mu_j, \sigma_j^2, \tau_f, N)$ in the pure theoretical model, obtained by integrating the log-normal density over the relevant subsets of the productivity support. In practice, however, we add classical measurement error to simulated employment draws following Gourio and Roys (2014); once measurement error is present, bin assignment is stochastic and the bin shares no longer have a closed form, requiring simulation to compute s_k^{model}. These model-predicted bin shares are the direct counterparts to the CBP data moments.

B.2. *Model Estimation*

We estimate the model in two steps using pre-reform data. Throughout, we normalize the uncovered wage to $w_u = 1$, so that τ_f is identified as the proportional increase in total labor costs faced by covered firms relative to uncovered firms. Under this normalization, the productivity parameters (μ_j, σ_j^2) estimated in Step 1 absorb the overall scale of wages; τ_f does not enter Step 1 and is recovered entirely in Step 2 from the relative employment

distortion between covered and uncovered firms in treated states.

The first step identifies the productivity distribution parameters (μ_j, σ_j^2) from the full coverage states. These states impose coverage on all employers regardless of size, so their firm size distribution is undistorted by any size-based avoidance margin and reflects the productivity distribution directly. We estimate (μ_j, σ_j^2) by simulated method of moments (SMM), minimizing the sum of squared deviations between model-predicted and CBP-observed establishment shares across seven size bins:

$$\min_{\mu_j, \sigma_j^2} \sum_{k=1}^{7} \left(\hat{s}_k^{\text{ctrl}} \; - \; s_k^{\text{model}}(\mu_j, \sigma_j^2, \; N = 1) \right)^2 \tag{B.6}$$

The SMM approach is necessary because we add classical measurement error to simulated employment draws following Gourio and Roys (2014), which accounts for the reporting noise inherent in administrative employment counts and breaks the closed-form expressions for bin shares. The calibrated values appear in Panel B of Table B.1. Figure B.2 (left panels) shows that the model fits the pre-period control group size distributions well across all three sectors.

The second step exploits the pre-reform firm size distribution in treated states (threshold $N_H = 8$). Treated states differ from controls in the relative mass of establishments in the 4–7 and 8–19 bins: treated states exhibit excess mass just below the threshold and a deficit just above it, reflecting bunching at $N_H - 1 = 7$. Given the productivity distribution calibrated in Step 1, the degree of this bunching is determined by τ_f alone (through the indifference condition (B.5)). We identify τ_f for each sector by a second SMM step, minimizing the distance between model-predicted and observed establishment shares in the treated pre-period distribution:

$$\min_{\tau_f} \sum_{k=1}^{7} \left(\hat{s}_k^{\text{trtd}} \; - \; s_k^{\text{model}}(\hat{\mu}_j, \hat{\sigma}_j^2, \; N = 8, \; \tau_f) \right)^2 \tag{B.7}$$

The estimated effective tax rates are $\hat{\tau}_f = 2.97\%$ for manufacturing, 1.87% for retail, and 2.13% for services. Manufacturing's higher $\hat{\tau}_f$ plausibly reflects a combination of institutional and labor-market factors. On the institutional side, the average payroll tax in manufacturing was 1.47% in 1949, compared to 1.18% in retail trade and 1.14% in services, an ordering consistent with our estimates. But the calibrated $\hat{\tau}_f$ exceeds the statutory rate in all sectors, suggesting that effective costs include components beyond the tax itself: compliance burdens associated with entering the experience rating system, administrative requirements, and

potentially less flexible wage adjustment in manufacturing than in the more competitive retail and service labor markets. Our model does not separately identify these channels; $\hat{\tau}_f$ absorbs them in aggregate. Figure B.2 (right panels) confirms that the model fits the pre-reform treated size distributions well.

B.3. *Model Validation: Comparison with Difference-in-Differences Estimates*

With the model calibrated to the pre-reform period, we assess its external validity by predicting the effects of the 1954 threshold change from $N_H = 8$ to $N_L = 4$. Because the model is static, we compare three scenarios: the pre-reform equilibrium at $N = 8$; a short-run post-reform scenario in which the threshold drops to 4 but capital remains at its pre-reform level k_0; and a long-run post-reform scenario in which both capital and labor fully adjust. The short run, long run distinction proxies for the time dimension of adjustment. In the short run, formerly-bunching firms (which held k_0 sized for $n_0 = N_H - 1 = 7$ workers) can expand employment above 8 but face a sub-optimal capital stock; their effective productivity $\tilde{z} \equiv z \cdot k_0^a$ is lower than the long-run value, dampening expansion relative to the eventual equilibrium. In the long run, both inputs re-optimize and firms freed from the old threshold fully expand along both margins.

Figure B.3 compares the model predictions to our reduced form difference-in-differences estimates. For retail and services, the model captures the sharp rise in the 0–3 share and the corresponding decline in the 4–7 share, with little movement in larger bins. These sectors are dominated by the avoidance margin. Firms reduce employment to stay below the new threshold rather than expanding, which the static model represents cleanly. For manufacturing, the model captures the decline in the 4–7 bin and the new avoidance response at 0–3, but underestimates the growth in the 20+ bin visible in the medium- and long-run DiD estimates. This gap is expected. A static model compares equilibria and cannot capture the dynamic expansion of firms that had been constrained at $N_H = 8$: once freed, these firms do not merely reallocate labor but invest in capital and grow over time, a process outside the scope of a static framework. A fully dynamic extension could add entry and exit margins, investment decisions contingent on the threshold regime, and the path-dependent capital accumulation patterns documented in Section 6.5. Despite this limitation, the static model provides a reasonable structural account of the dominant margin across all sectors, and the quantitative fit is sufficient to support the aggregate calculations that follow.

B.4. *Output per worker and Productivity*

We use the calibrated model to evaluate the aggregate output and productivity consequences of the size-based coverage threshold. Figure B.1 illustrates the core intuition. Rela-

tive to a benchmark in which all firms contribute to the UI system regardless of size ($N = 1$), any threshold regime with $N > 1$ generates two competing forces. First, an exemption gain: firms below the threshold are exempt from the payroll tax and therefore operate at a larger scale than they would under universal coverage. Second, a bunching loss: firms in the interval $[z^-, \bar{z}]$ operate at $N-1$ rather than their unconstrained optimum, sacrificing output to avoid the threshold. Whether aggregate output falls above or below the benchmark depends on the magnitude of τ_f, which governs the severity of both forces, and on the share of firms in each regime.

The results, reported in Panel C of Table B.1, reveal a sharp contrast between manufacturing and the other sectors. In manufacturing under $N = 8$, only 46% of establishments are naturally uncovered, so the exemption gain is modest relative to the bunching loss; aggregate output falls approximately 0.06 percent below the benchmark. In retail and services, 84–87% of establishments are naturally uncovered under $N = 8$, generating a large exemption gain that more than offsets the bunching loss; aggregate output exceeds the benchmark by roughly 1.2 percent in both sectors. The 1954 reform from $N = 8$ to $N = 4$ therefore has opposite aggregate effects: it reduces the bunching distortion in manufacturing and raises aggregate output, while eroding the exemption advantage in retail and services and reducing aggregate output (Panel D).

We report two measures of labor productivity: $E[y]/E[n]$, the ratio of aggregate output to aggregate employment, and $E[y/n]$, the unweighted average of output per worker across firms. Both capture distortions from the threshold, but they weight firms differently and tell a different part of the story.

In the full-coverage benchmark ($N = 1$), every firm pays $w_u(1 + \tau_f)$ per worker and optimizes freely. The first-order condition then implies $y/n = w_u(1 + \tau_f)/\phi$ for every active firm, independent of z. Output per worker is therefore identical across the entire productivity distribution, and the two statistics coincide: $E[y]/E[n] = E[y/n] = w_u(1 + \tau_f)/\phi$. Under a threshold $N > 1$, this uniformity breaks down in a way that differs across the three firm groups. Covered firms ($z > \bar{z}$) continue to face the same total labor cost $w_u(1 + \tau_f)$ and optimize freely, so their output per worker remains at the benchmark level, $y/n = w_u(1 + \tau_f)/\phi$. Uncovered firms ($n^*(z, w_u) < N$) hire more than they would under universal coverage; at their optimum, $y/n = w_u/\phi < w_u(1 + \tau_f)/\phi$, below the benchmark. Bunching firms ($z \in [z^-, \bar{z}]$) are constrained at $N - 1 < n^*(z, w_u(1 + \tau_f))$; because $\phi < 1$, average product $y/n = Cz^{1/(1-a)}(N - 1)^{\phi-1}$ is decreasing in n, so operating below the covered optimum raises their output per worker above the benchmark level.

The two productivity statistics are related by:

$$\frac{E[y]}{E[n]} = E\Big[\frac{y}{n}\Big] + \frac{\mathrm{Cov}(y/n,\ n)}{E[n]} \tag{B.8}$$

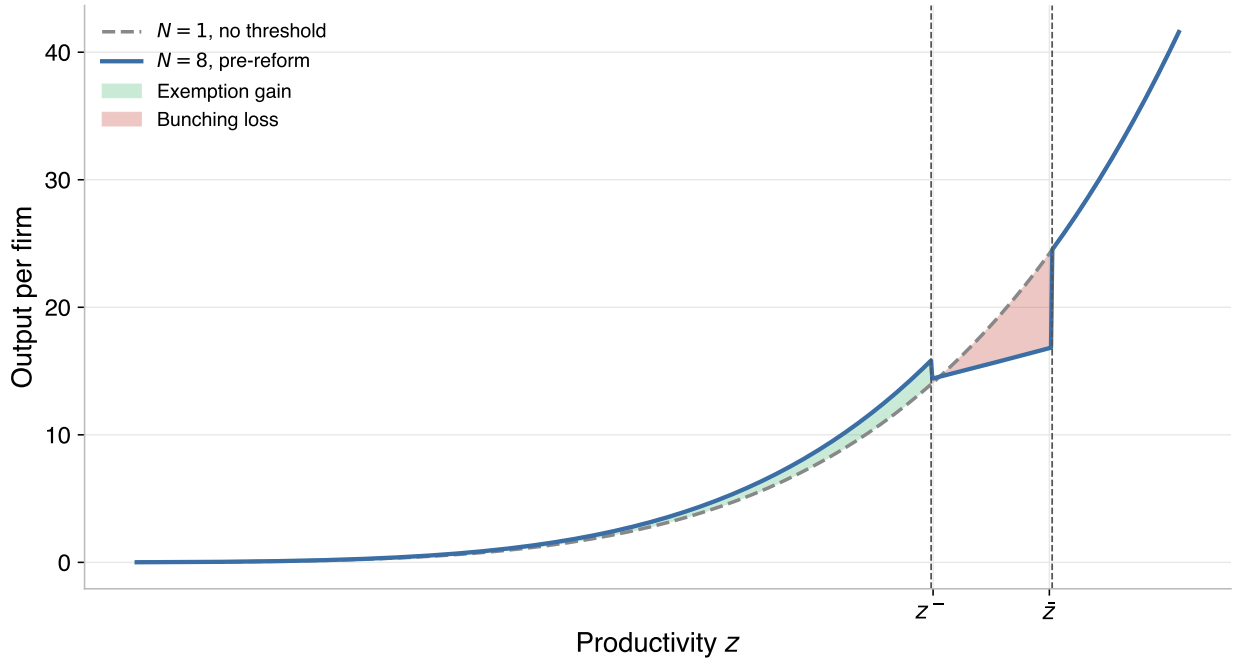
In the benchmark, the covariance is zero because all firms have the same y/n. Under the threshold, covered firms (large n, benchmark y/n) and uncovered firms (small n, below-benchmark y/n) together generate a positive covariance between output per worker and firm size: small firms have depressed y/n, large firms do not. Consequently, $E[y]/E[n] > E[y/n]$ in the distorted economy.

The numerical results in Table B.1 reflect these forces. $E[y/n]$ declines in all sectors under both thresholds. It drops by 0.48, 0.88, and 0.95 percentage points respectively in manufacturing, retail, and services under $N = 8$. The pattern for $E[y]/E[n]$ is more nuanced. In retail and services, where 84–87% of establishments are uncovered, aggregate output per worker also declines, but roughly half as much as $E[y/n]$. In manufacturing, $E[y]/E[n]$ is relatively close to the no threshold scenario under both thresholds (+0.002 percent and -0.004 percent respectively).

B.5. *Robustness*

Table B.2 examines the sensitivity of these results to three parameters: the measurement error standard deviation s_e, the span-of-control parameter γ, and the capital depreciation rate r. For each, we re-calibrate $(\mu_j, \sigma_j^2, \tau_f)$ with one parameter varied and all others held at their baseline values, then recompute the output and employment effects. The estimated $\hat{\tau}_f$ is somewhat sensitive to γ: a higher span-of-control parameter implies that crossing the threshold is more costly for the firm, so a lower $\hat{\tau}_f$ is needed to match the same observed bunching. The two parameters jointly govern the attractiveness of the bunching region and are imperfectly separated by our moments. The magnitudes of the output effects vary modestly in response. However, the qualitative results are stable across all specifications: manufacturing always differs from retail and services in the direction of the output effect, and the reform always reduces output per worker in all sectors.

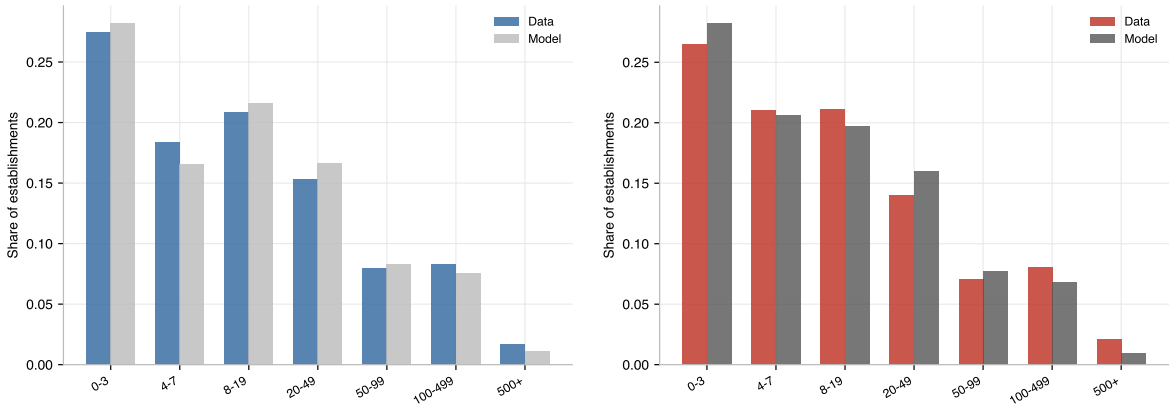
Appendix Figure B.1: Conceptual Output Changes from Sized-Based UI Contributions



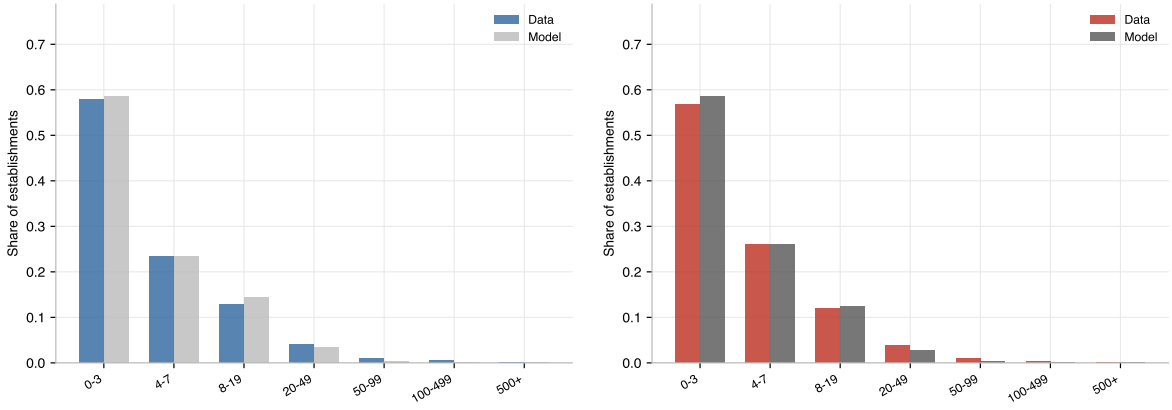
Notes: Figure illustrates the changes in output by level of productivity between a scenario with no bunching (threshold equal to 1) to the scenario of threshold of 8. The mass of uncovered firms have an exemption gain in the latter scenario. Firms within the bunching productivity set suffer an output loss.

Appendix Figure B.2: Firm Size Distribution in Control (Left) and Treated (Right) States

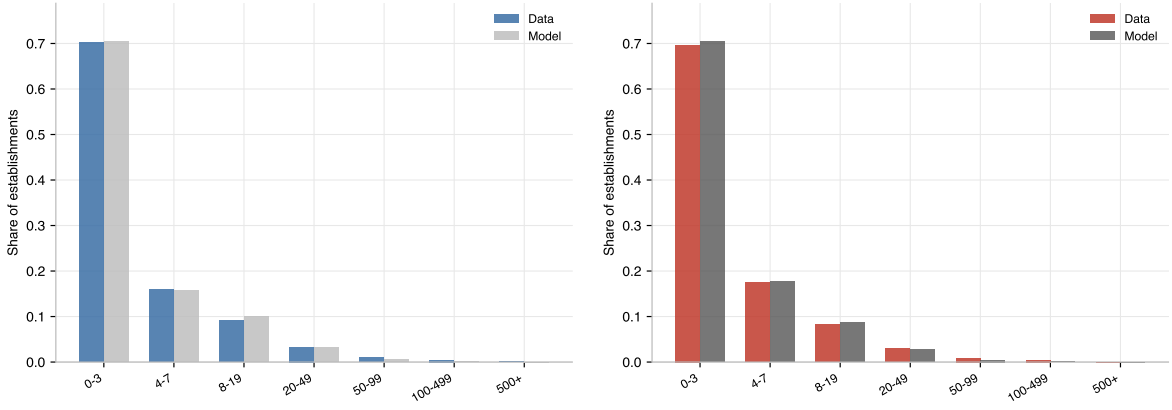
(a) Manufacturing



(b) Retail



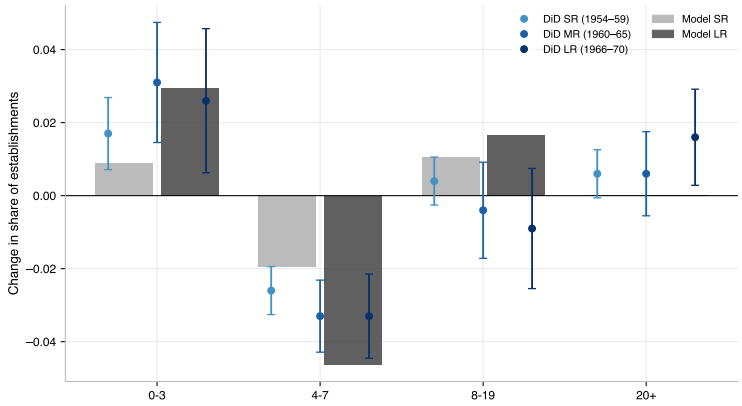
(c) Services



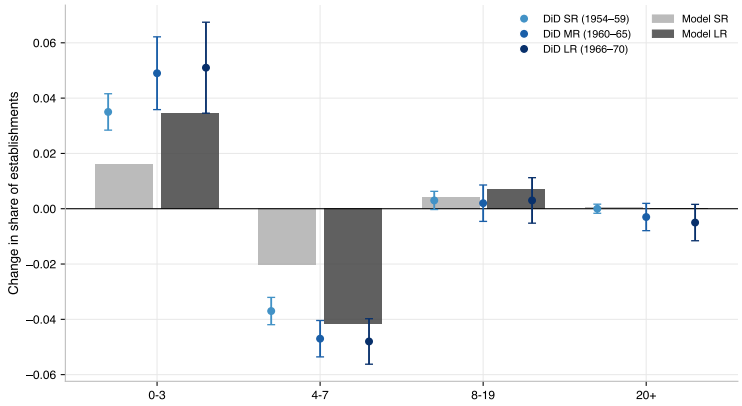
Notes: Figures show the average firm size distribution before 1954 for control (threshold equal 1, left) and treated states (threshold equal 8, right). For each panel we show distribution from CBP data as well as the model's prediction.

Appendix Figure B.3: Predicted Effect of 1954 Reform

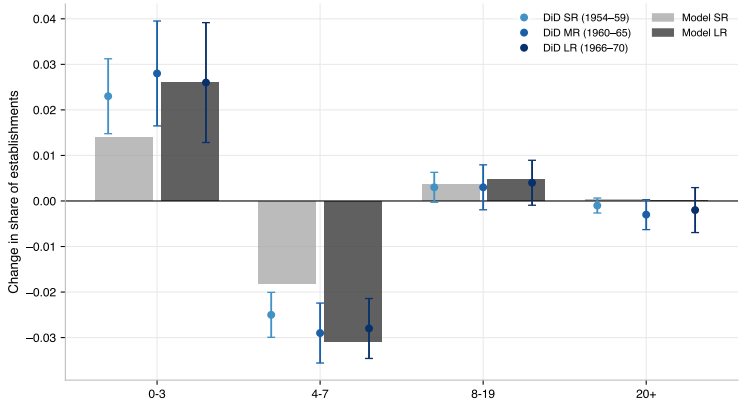
(a) Manufacturing



(b) Retail



(c) Services



Notes: Figures show the change in the share of firms by bin predicted from the calibrated model and from the difference-in-differences estimates for each sector. "SR" and "LR" refer to short and long run, respectively. For the model, in the LR scenario, firms can reoptimize capital. Empirical estimates with 95% confidence intervals come from Tables A.1, A.2, and A.3.

Appendix Table B.1: Model parameters and output costs by sector

	Manufacturing	Retail	Services
Panel A: Parameters from the literature			
γ	0.802	0.802	0.802
α_j		0.177	0.177
s_e	0.250	0.250	0.250
Panel B: Calibrated			
α_m	0.373		
μ_z	0.600	0.511	0.411
σ_z	0.337	0.208	0.268
τ_f	2.97	1.87	2.13
Panel C: Simulation $N = 8$			
Uncovered	46.2	84.1	87.5
$B(N = 8)$	9.1	6.2	4.5
Covered	44.8	9.7	8.0
$\Delta E[n]$	-0.06	1.60	1.63
$\Delta E[y]$	-0.059	1.160	1.182
$\Delta E[y]/E[n]$	0.002	-0.437	-0.440
$\Delta E[y/n]$	-0.476	-0.951	-0.878
Panel D: Simulation $N = 4$			
Uncovered	30.7	63.4	74.0
$B(N = 4)$	4.8	5.7	4.3
Covered	64.5	30.9	21.7
$\Delta E[n]$	-0.00	0.53	0.72
$\Delta E[y]$	-0.009	0.366	0.510
$\Delta E[y]/E[n]$	-0.004	-0.168	-0.209
$\Delta E[y/n]$	-0.189	-0.472	-0.516

Notes: Panel A sources: γ from Guner et al. (2008); α_j from Valentinyi and Herrendorf (2008); s_e from Gourio and Roys (2014). Panel B: α_m calibrated from the 1948 Census of Manufactures; (μ_z, σ_z) calibrated to match pre-reform establishment-size shares in control states; τ_f identified from excess bunching below the threshold in treated states before 1954. Panels C–D report weights of groups of establishments: uncovered is the percent of establishments below the threshold, $B(N)$ is the percent of establishments bunching at N, and covered is the percent of establishments contributing to UI. Changes in employment or output are expressed in percentage points deviations from the frictionless benchmark ($N = 1$).

Appendix Table B.2: Robustness checks: recalibrated model for each parameter variant and sector

Scenario		μ_z	σ_z	τ_f	Bunch %		$\Delta E[n]$		$\Delta E[y]$		$\Delta E[y]/E[n]$		$\Delta E[y/n]$	
					N=8	N=4	N=8	N=4	N=8	N=4	N=8	N=4	N=8	N=4
Manufacturing														
Baseline	—	0.600	0.337	2.970	9.090	4.800	-0.060	-0.000	-0.059	-0.009	0.002	-0.004	-0.476	-0.189
s_e	0.15	0.601	0.340	1.900	6.460	2.390	-0.030	0.020	-0.031	0.013	0.002	-0.008	-0.300	-0.197
	0.35	0.606	0.336	4.350	11.940	7.480	-0.090	-0.040	-0.095	-0.042	-0.003	0.001	-0.706	-0.176
γ	0.75	0.857	0.426	3.590	9.210	4.930	-0.060	-0.010	-0.056	-0.010	0.001	-0.005	-0.576	-0.221
	0.85	0.355	0.256	2.410	9.130	4.810	-0.070	-0.010	-0.064	-0.009	0.001	-0.004	-0.386	-0.154
r	0.08	0.534	0.337	3.110	9.420	5.110	-0.060	-0.010	-0.063	-0.013	0.001	-0.004	-0.499	-0.186
	0.12	0.655	0.338	2.860	8.870	4.580	-0.060	-0.000	-0.058	-0.007	0.002	-0.005	-0.455	-0.189
Retail														
Baseline	—	0.511	0.208	1.870	6.170	5.730	1.600	0.530	1.160	0.366	-0.437	-0.168	-0.951	-0.472
s_e	0.15	0.510	0.216	1.430	5.160	3.490	1.060	0.580	0.769	0.421	-0.288	-0.156	-0.695	-0.416
	0.35	0.511	0.206	2.730	7.740	9.180	2.670	0.460	1.919	0.263	-0.730	-0.197	-1.433	-0.602
γ	0.75	0.646	0.263	2.290	6.250	5.870	1.650	0.540	1.112	0.339	-0.531	-0.200	-1.156	-0.568
	0.85	0.370	0.156	1.370	5.840	5.050	1.460	0.560	1.135	0.425	-0.321	-0.136	-0.700	-0.366
r	0.08	0.480	0.208	1.890	6.220	5.810	1.610	0.530	1.165	0.363	-0.440	-0.168	-0.960	-0.474
	0.12	0.536	0.208	1.910	6.250	5.860	1.630	0.540	1.180	0.368	-0.446	-0.171	-0.970	-0.480
Services														
Baseline	—	0.411	0.268	2.130	4.520	4.300	1.630	0.720	1.182	0.510	-0.440	-0.209	-0.878	-0.516
s_e	0.15	0.412	0.270	1.370	3.360	2.020	0.960	0.680	0.704	0.506	-0.254	-0.173	-0.553	-0.393
	0.35	0.411	0.261	2.770	5.290	6.020	2.430	0.830	1.752	0.559	-0.658	-0.265	-1.184	-0.646
γ	0.75	0.519	0.338	2.570	4.530	4.330	1.670	0.730	1.128	0.480	-0.531	-0.250	-1.057	-0.619
	0.85	0.293	0.203	1.690	4.510	4.290	1.610	0.710	1.247	0.539	-0.352	-0.168	-0.700	-0.412
r	0.08	0.380	0.267	2.110	4.490	4.230	1.610	0.720	1.171	0.512	-0.436	-0.209	-0.870	-0.513
	0.12	0.435	0.270	2.220	4.640	4.490	1.660	0.710	1.204	0.500	-0.451	-0.210	-0.906	-0.527

Notes: Baseline: $s_e = 0.25$, $\gamma = 0.802$, $r = 0.10$. Each row recalibrates $(\mu_z, \sigma_z, \tau_f)$ with one parameter varied and all others held at baseline. All simulation values are percentage deviations from the frictionless benchmark ($N = 1$).

Online Appendix C: Data Appendix

C.1. *Manufacturing Data*

We digitized published state-level tables from the Census of Manufactures and the Annual Survey of Manufactures, produced by the U.S. Census Bureau. The tables report, for each state, total value added, average annual employment, total salaries and wages, and expenditures on new plant and equipment. Data are available for census years 1947, 1954, 1958, and 1963, and for non-census years 1949, 1950, 1951, 1952, 1953, and 1965 from the AMS. Because the tables are aggregated to the state level, they do not permit analysis of within-state reallocation across establishments.

Several features of the published tables differ from the County Business Patterns: employment is the annual average rather than first-quarter; salaries and wages reflect total annual compensation rather than first-quarter payroll; capital expenditures cover new plant and equipment only, excluding used capital; and establishment counts appear only in census years.

Our measure of output is value added. For the labor input, we follow Hsieh and Klenow (2009) in using the total wage bill rather than a headcount measure. The wage bill weights workers by their compensation, which under competitive labor markets reflects differences in human capital and skill, and is defined consistently across years.

We construct the capital stock using the perpetual inventory method and the limited data on investment. The stock evolves as $K_{st} = (1-\delta)K_{s,t-1} + I_{st}$ with depreciation rate $\delta = 0.08$. We initialize in 1947 assuming each state is on its balanced growth path: $K_{s,1947} = I_{s,1947}/(\delta + g_s)$, where g_s is calibrated from the linear trend in value added between 1919 and 1953. State-specific growth rates matter because states with faster-growing manufacturing sectors would have had systematically higher investment rates at the start of the sample. Since capital expenditure data are available only at the observation years listed above, we linearly interpolate investment between years, which introduces attenuation bias in our estimates.

To calibrate state-level production function parameters, we additionally digitized state-by-2-digit-industry tables from the 1947 Census of Manufactures, covering 20 industry groups with separate figures for value added, salaries and wages, and employment. Most states publish a residual "All other major industry groups" category to protect establishment confidentiality. We assign the average α_j of industries not reported individually for each state. States that report all industries individually, such as Indiana, Maryland, and Massachusetts, are unaffected.

Table C.1 reports, for each of the 20 industries, the shares of value added and employment in treated and control states alongside the national labor share $1 - \alpha_j$. There is meaningful

variation in industry composition across groups—treated states have larger shares of food products and textiles, control states more lumber and printing—but because industries with high and low labor shares are distributed across both groups, this does not translate into systematic differences in the weighted average α_s, as confirmed in Table C.2. That table also reports pre-reform levels of key outcomes in 1947. Treated and control states have nearly identical values of α_s, labor share, TFPR, and workers per establishment. They differ in levels of value added per worker and capital intensity, with control states more productive and more capital-intensive on average. We note these differences not to establish balance for identification—our design relies on parallel trends—but because they bear on the interpretation of the post-reform dynamics.

C.2. *Production Function Estimation and Productivity Measurement*

We construct Total Factor Revenue Productivity (TFPR) as the ratio of value added to $K^{\alpha_s}(wL)^{1-\alpha_s}$, following Hsieh and Klenow (2009). We calibrate $1-\alpha_s$ as the state-level weighted average of national industry-level labor shares in 1947, with weights equal to each state's industry shares of value added. The resulting state-level estimates range from 0.42 (WY) to 0.57 (CT), reflecting meaningful variation in industrial composition. Our approach maintains the standard assumptions of perfect competition and Cobb-Douglas technology at the industry level. Because the data are aggregated to the state level, we cannot apply plant-level methods for addressing endogeneity in input choices, nor exploit price data to separately identify demand- and supply-side parameters (Eslava et al. 2024).

State-level aggregation also prevents us from decomposing the aggregate TFPR change into within-firm and between-firm components (Foster et al. 2008; Haltiwanger 2000). Our estimates capture the net effect across all margins: distorted input choices within surviving firms, exit of less productive establishments, and reallocation across the size distribution.

A further caveat is that TFPR conflates technical efficiency with changes in output prices or markups (Hsieh and Klenow 2009; Foster et al. 2008). Following Hsieh and Klenow (2009), we recover an implied TFPQ measure under monopolistic competition with demand elasticity $\sigma = 3.5$. The implied decline in TFPQ is approximately 4 percentage points—at least as large as the TFPR estimate—though less precisely estimated, suggesting the result is not driven by price effects alone.

The TFPR results are robust to replacing state-specific α_s with the national aggregate labor share over 1947–1952, to imposing a common steady-state growth rate across states, and to using labor input alone. In all cases the estimates are somewhat weaker, as each alternative reduces the state-level heterogeneity in the productivity measure that our preferred specification exploits.

Appendix Table C.1: Industry Composition by Treatment Status, 1947

		VA Share			Emp. Share		
Industry	α_j	Control	Treated	p-val	Control	Treated	p-val
Food and kindred products	0.580	0.176	0.218	0.369	0.171	0.203	0.466
		(0.109)	(0.199)		(0.098)	(0.191)	
Lumber and products, except furniture	0.464	0.108	0.059	0.154	0.129	0.087	0.266
		(0.150)	(0.063)		(0.163)	(0.085)	
Chemicals and allied products	0.640	0.078	0.078	1.000	0.052	0.049	0.846
		(0.084)	(0.065)		(0.055)	(0.043)	
Printing and publishing industries	0.467	0.077	0.054	0.371	0.079	0.054	0.305
		(0.115)	(0.038)		(0.108)	(0.041)	
Textile mill products	0.469	0.064	0.113	0.268	0.067	0.118	0.253
		(0.102)	(0.187)		(0.101)	(0.185)	
Primary metal industries	0.377	0.059	0.049	0.580	0.056	0.051	0.752
		(0.062)	(0.061)		(0.063)	(0.059)	
Machinery (except electrical)	0.385	0.057	0.074	0.335	0.061	0.079	0.321
		(0.051)	(0.069)		(0.051)	(0.074)	
Petroleum and coal products	0.633	0.054	0.034	0.436	0.034	0.020	0.475
		(0.111)	(0.062)		(0.082)	(0.037)	
Paper and allied products	0.555	0.046	0.052	0.686	0.033	0.034	0.888
		(0.042)	(0.058)		(0.028)	(0.035)	
Fabricated metal products	0.424	0.043	0.042	0.882	0.046	0.044	0.778
		(0.030)	(0.028)		(0.032)	(0.029)	
Apparel and related products	0.431	0.039	0.031	0.420	0.052	0.046	0.675
		(0.043)	(0.024)		(0.048)	(0.036)	
Transportation equipment	0.366	0.035	0.051	0.394	0.042	0.055	0.515
		(0.039)	(0.079)		(0.050)	(0.082)	
Electrical machinery	0.417	0.030	0.023	0.398	0.032	0.024	0.429
		(0.030)	(0.028)		(0.035)	(0.031)	
Miscellaneous manufactures	0.423	0.030	0.012	0.028	0.033	0.016	0.040
		(0.035)	(0.010)		(0.035)	(0.013)	
Stone, clay, and glass products	0.475	0.028	0.039	0.217	0.033	0.041	0.478
		(0.021)	(0.039)		(0.030)	(0.045)	
Leather and leather products	0.430	0.026	0.017	0.452	0.029	0.023	0.694
		(0.051)	(0.027)		(0.058)	(0.040)	
Rubber products	0.399	0.014	0.009	0.168	0.013	0.008	0.161
		(0.014)	(0.008)		(0.012)	(0.008)	
Furniture and fixtures	0.402	0.014	0.018	0.260	0.017	0.024	0.154
		(0.012)	(0.015)		(0.014)	(0.018)	
Instruments and related products	0.354	0.013	0.006	0.022	0.013	0.006	0.030
		(0.014)	(0.006)		(0.013)	(0.006)	
Tobacco manufactures	0.679	0.009	0.021	0.185	0.009	0.018	0.207
		(0.015)	(0.040)		(0.015)	(0.029)	
States			23			24	

Notes: Each row reports the mean and standard deviation (in parentheses) of industry shares of value added and employment across control and treated states in 1947, prior to the reform. p-val is the p-value from the coefficient from a OLS regression of the shares on treatment indicator, with robust standard errors. $\alpha_j = 1 - \text{labor share}_j$ is the national capital share for industry j, estimated from the 1947 Census of Manufactures and used to calibrate state-level production function parameters.

Appendix Table C.2: Pre-Treatment Differences in Key Outcomes, 1947

	Control	Treated	Difference
Labor (000s)	307.4	251.0	-56.4
	(471.8)	(292.9)	(115.1)
Establishments	5855	3873	-1982
	(10449)	(3515)	(2293)
L/Estab	50.5	54.4	3.9
	(25.3)	(23.4)	(7.1)
VA/Estab	358.97	367.51	8.54
	(152.77)	(165.44)	(46.42)
VA/L	7.39	6.82	-0.56
	(0.99)	(0.93)	(0.28)
Labor Share	0.509	0.494	-0.014
	(0.053)	(0.045)	(0.014)
K/Y	0.969	0.769	-0.200
	(0.728)	(0.191)	(0.157)
α_s	0.491	0.497	0.005
	(0.035)	(0.033)	(0.010)
TFPR	1.544	1.651	0.108
	(0.284)	(0.183)	(0.070)
TFPQ	435.732	445.576	9.844
	(328.593)	(223.190)	(82.265)
States	23	24	

Notes: Each row reports the mean and standard deviation (in parentheses) for control and treated states in 1947, prior to the reform. The Difference column reports the treated minus control mean from an OLS regression with robust standard errors.