

The Firm Response to State Elimination of Section 14(c) Subminimum Wages*

Michelle Yin[†] Hoa Vu[‡]

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Abstract

Section 14(c) of the Fair Labor Standards Act permits certified employers to pay workers with disabilities below the federal minimum wage. Existing research on state policies eliminating subminimum wages has focused primarily on worker outcomes, with less evidence on how disability-service providers adjust to these policies. We construct a novel employer-year panel of U.S. Section 14(c) certificate holders from 2015 to 2024 using comprehensive administrative records obtained through a Freedom of Information Act request to the U.S. Department of Labor and link these records to IRS Form 990 filings to measure nonprofit organizations' continued operations and financial outcomes. Exploiting the staggered implementation of state laws eliminating subminimum wages, we find that the annual probability that a certificate holder exits the Section 14(c) program increases by approximately 25 percentage points following policy implementation. In contrast, we find no corresponding increase in organizational closure as measured by the cessation of Form 990 filings. Among organizations that remain active, revenues increase by approximately 17 percent, while we find no statistically significant changes in officer compensation or non-officer wage expenditures. Finally, combining our firm-level estimates with existing evidence on workers' post-policy employment and earnings, we conduct a supplementary fiscal analysis. Under our baseline assumptions, the additional public costs of eliminating subminimum wages are more than offset by higher tax revenues and reductions in disability-benefit expenditures.

JEL Codes: J23, J38, J78, H53, I38, L31.

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[†]Northwestern University, School of Education and Social Policy. 2120 Campus Drive, Evanston IL 60208. Email: michelle.yin@northwestern.edu

[‡]Northwestern University, School of Education and Social Policy. 2120 Campus Drive, Evanston IL 60208. Email: hoa.vu@northwestern.edu

1 Introduction

Section 14(c) of the Fair Labor Standards Act permits certified employers to pay workers with disabilities less than the federal minimum wage. Although the provision has long been a central component of the U.S. disability-employment system, it has become the subject of increasing policy debate. Advocates of elimination argue that subminimum wages limit the economic opportunities of workers with disabilities and perpetuate their segregation from the broader labor market, while opponents have raised concerns that eliminating the provision may reduce employment opportunities or lead to the closure of disability-service providers ([U.S. Commission on Civil Rights, 2020](#)). Between 2015 and 2024, fifteen states enacted legislation eliminating the use of subminimum wages for workers with disabilities. Eleven of these laws became effective during this period, raising the applicable wage floor by \$3 to \$9 per hour for affected certificate holders (Table 1). These state-level reforms provide an opportunity to examine how employers adjust when a policy change fundamentally alters the economic basis of an existing employment model.

Existing research on the elimination of subminimum wages has focused primarily on workers. This literature examines whether state restrictions on Section 14(c) affect employment and other labor-market outcomes for individuals with disabilities ([Kakara, Bair and Venkataramani, 2024](#); [Yin, Seo and Vu, 2026](#)). The response of employers, however, is also central to understanding the consequences of these policies. Section 14(c) employment is concentrated among nonprofit disability-service providers that often combine employment, vocational rehabilitation, and other publicly supported services. If the elimination of subminimum wages causes these organizations to close or substantially contract, the consequences of the policy may extend beyond the workers directly affected by the wage increase. Alternatively, providers may adjust by discontinuing their Section 14(c) programs while continuing to operate, with services shifting toward other organizations. Distinguishing between these responses is important for understanding both the organizational consequences of the policy

and the mechanisms through which the disability-service sector adjusts to a higher wage floor.

This question also relates to a broader literature on how firms respond to minimum-wage increases. Previous work has examined adjustments in employment, firm entry and exit, prices, and the allocation of economic activity across firms ([Cengiz et al., 2019](#); [Aaronson et al., 2018](#); [Dustmann et al., 2022](#); [Harasztosi and Lindner, 2019](#)). The Section 14(c) setting differs from the conventional minimum-wage context in several important respects. Most certificate holders are nonprofit disability-service providers, and their revenues often depend on government contracts and Medicaid-funded services rather than market-determined prices ([U.S. Government Accountability Office, 2021](#)). These institutional features may limit the extent to which providers can respond to higher labor costs through price adjustments.

We examine these questions using a novel employer-year panel of every U.S. Section 14(c) certificate holder between 2015 and 2024. We obtain comprehensive administrative records on Section 14(c) certificate holders through a Freedom of Information Act (FOIA) request to the U.S. Department of Labor’s (DOL) Wage and Hour Division. We link these employers to IRS Form 990 filings to observe organizational outcomes and financial measures. We exploit the staggered implementation of state laws eliminating subminimum wages and estimate the effects of these policies using difference-in-differences and event-study designs, with estimates also examined using the interaction-weighted approach of [Sun and Abraham \(2021\)](#).

Our results show that eliminating subminimum wages substantially affects participation in the Section 14(c) program but does not lead to a corresponding increase in organizational closure. Following the implementation of a state elimination law, the probability that a certificate holder exits the Section 14(c) program increases by approximately 25 percentage points per year relative to providers in states that retain the policy. In contrast, we find no statistically significant increase in exit when organizational closure is measured using the cessation of Form 990 filings. The distinction between program exit and organizational exit is important: many affected providers appear to discontinue their use of subminimum wages

while remaining active as organizations.

We also find that revenues increase among providers that remain active following policy adoption. The estimated increase is approximately 17 percent. This pattern is consistent with a reallocation of disability-service activity across providers, although our data do not allow us to directly observe the movement of workers or service recipients from exiting Section 14(c) programs to surviving organizations. We therefore interpret the revenue results as evidence that surviving providers experience an increase in economic activity following policy adoption, rather than as direct evidence on the precise mechanism generating that increase. We find little evidence that these revenue gains are accompanied by systematic changes in compensation: the officer compensation share does not increase, and estimated effects on non-officer wage expenditures are positive but imprecisely estimated.

Finally, we combine our firm-level estimates with existing evidence on the employment and earnings consequences of leaving subminimum-wage employment to examine the potential fiscal consequences of eliminating Section 14(c). This exercise incorporates changes in vocational rehabilitation expenditures, disability-benefit receipt, and tax revenue associated with workers' subsequent employment and earnings. Across a range of plausible assumptions, the additional public costs associated with the policy are substantially offset by increases in tax revenue and reductions in disability-benefit payments. Under our baseline assumptions, the policy generates net fiscal savings. Because these calculations necessarily depend on assumptions about workers' post-policy employment trajectories, we view them as a supplementary assessment of the potential fiscal implications.

This paper contributes to the literature in two ways. First, to our knowledge, it provides one of the first evidence on the organizational response to the elimination of subminimum wages, complementing a growing literature that has focused primarily on worker outcomes (Yin, Seo and Vu, 2026; Kakara, Bair and Venkataramani, 2024). Second, the paper contributes to the broader literature on firm adjustment to minimum-wage increases by examining a setting in which conventional price adjustment may be limited. Previous studies have

documented firm responses through changes in employment, firm entry and exit, prices, and the reallocation of economic activity across firms (Cengiz et al., 2019; Aaronson et al., 2018; Dustmann et al., 2022; Harasztosi and Lindner, 2019). We provide evidence on how adjustment occurs among nonprofit service providers facing administratively determined revenues, where responses to higher labor costs may instead operate through changes in program participation, organizational structure, and the reallocation of activity across providers.

The remainder of the paper is organized as follows. Section 2 describes the institutional background of Section 14(c) and the state policies eliminating subminimum wages. Section 3 presents the conceptual framework. Section 4 describes the data and empirical strategy. Section 5 presents the main findings and robustness analyses. Section 6 examines the policy’s fiscal implications. Section 7 concludes.

2 Institutional Background

2.1 Section 14(c) of the Fair Labor Standards Act

Section 14(c) of the 1938 Fair Labor Standards Act authorizes the U.S. Department of Labor’s Wage and Hour Division to issue certificates permitting employers to pay workers with disabilities wages below the federal minimum wage. To obtain a certificate, employers must identify the prevailing wage paid to experienced workers without disabilities performing substantially the same work in the local area, establish a productivity standard based on such workers, and assess the quality and quantity of the worker with a disability’s productivity through time studies and work measurements. The resulting productivity assessment determines the worker’s proportional wage relative to the prevailing wage. These assessments must be updated periodically to reflect changes in worker productivity; hourly workers’ wages must be reviewed at least every six months, and wages for all workers must be adjusted at least annually to account for changes in prevailing wages (Office of the Federal Register,

2024). The Wage and Hour Division administers the certificate program.

The Section 14(c) certificate system covers four categories of employers. Community Rehabilitation Programs (CRPs) are nonprofit organizations that provide employment and vocational rehabilitation services to individuals with disabilities. School Work Experience Programs provide employment opportunities for students with disabilities as part of their transition from school to employment. Hospital Patient Worker Programs employ patients who perform work while receiving treatment or care. Business Establishments are for-profit firms that employ workers with disabilities under a 14(c) certificate. CRPs account for approximately 95 percent of certificate holders and workers in our sample.

The revenue model of CRPs is central to the setting we study. Section 14(c) certificate holders may be financed through a combination of public funding, revenue from their business activities, and charitable contributions. Certificate holders that also provide disability services may receive Medicaid payments for Home and Community-Based Services (HCBS), including employment-related services such as job coaching, employment preparation, and assistance with performing job duties. They may also receive revenue from public or private contracts to purchase goods or services produced by workers with disabilities. Workers performing contract-based production commonly work in congregate settings alongside other individuals with disabilities who may be employed under the same or other contracts or receive other disability services (U.S. Government Accountability Office, 2021).

This funding structure has important implications for the incidence of a change in the subminimum wage. To the extent that CRPs receive fixed payments for disability services or contracted goods and services, these revenues need not adjust automatically when the wages of 14(c) workers change. A higher wage floor can therefore increase labor costs without a commensurate increase in revenue, placing pressure on the CRP's operating margin. The magnitude of this effect depends on the extent to which certificate holders can adjust their service mix, contract prices, or other sources of revenue in response to higher labor costs.

At the beginning of our sample in 2015, the national 14(c)-covered workforce numbered approximately 206,000 workers across 2,870 certificate holders. By the end of 2024, the workforce had fallen to approximately 44,000 workers across 857 certificate holders, a decline of 78 percent in workers and 70 percent in certificate holders. Two forces drive the decline. First, beginning in 2016, the Department of Labor strengthened enforcement of the Section 14(c) program following the implementation of new requirements under the Workforce Innovation and Opportunity Act (WIOA), which imposed additional obligations on certificate holders ([U.S. Department of Labor, Wage and Hour Division, 2016](#)). Second, state legislatures enacted state-level elimination laws that render the certificate inapplicable within the state.

2.2 The Wave of State Elimination Laws

Beginning in 2015, a growing number of states adopted legislation eliminating the use of Section 14(c) certificates to pay subminimum wages. As shown in Table 1, the timing of these reforms varied considerably across states, with some laws taking effect immediately after enactment and others establishing delayed implementation dates. During our 2015–2024 observation window, eleven states had laws eliminating Section 14(c) subminimum-wage employment take effect. New Hampshire was the first, with its law taking effect in 2015, followed by Alaska in 2018, Maryland and Maine in 2020, Hawaii and Washington in 2021, Rhode Island and Tennessee in 2022, South Carolina and Oregon in 2023, and Delaware in 2024.

The reforms generally prohibit employers from paying workers with disabilities below the applicable minimum wage, thereby rendering Section 14(c) certificates inoperative within the state. Importantly, the reforms were implemented at different times, with effective dates spanning nearly a decade and generating substantial variation in treatment timing across states. We exploit this staggered implementation in our empirical analysis to compare

changes in outcomes following the effective date of state elimination laws with contemporaneous changes in states that had not yet eliminated Section 14(c), while accounting for the broader national decline in the number of 14(c) certificates and workers employed under them.

3 Conceptual Framework

A non-profit 14(c) certificate holder in state s at year t employs n_{it} workers with disabilities at wage $w_{it} \in [\underline{w}, m_{st}]$, where $\underline{w}$ is the certificate-holder-specific subminimum wage from the time-study prevailing wage rule and m_{st} is the state minimum wage. The holder's operating margin on its 14(c) program specifically is

$$\pi_{it} = p_{st} \cdot y(n_{it}) - w_{it} \cdot n_{it} \cdot h - F_i - \Gamma_{it}, \quad (1)$$

where p_{st} is the state contract reimbursement rate per unit service output, $y(n_{it})$ is service output as a function of workforce, $h = 1,820$ is annual work hours, F_i is a fixed cost of certificate administration, and Γ_{it} is a state-year cost shifter capturing secular administrative and compliance-cost pressure common to all certificate holders nationwide, including the career counseling and vocational rehabilitation referral requirements that Section 511 of the Workforce Innovation and Opportunity Act imposed on certificate holders beginning in 2016 ([U.S. Government Accountability Office, 2023](#)). The holder discontinues its 14(c) certificate, exiting the subminimum-wage program specifically, if $\pi_{it} < 0$.

Program exit is distinct from organizational survival. Certificate holders are typically nonprofit Community Rehabilitation Programs that operate several lines of state- and Medicaid-funded service beyond 14(c) ([U.S. Government Accountability Office, 2021](#)), so a holder can drop its 14(c) certificate while remaining open under its other revenue lines, provided its firm-wide margin stays non-negative. Organizational dissolution requires the firm-wide con-

dition to fail, a substantially less common event than program-specific insolvency whenever 14(c) is one revenue line among several. The model therefore distinguishes two margins of exit that need not move together: *program exit*, governed by π_{it} alone, and *organizational exit*, governed by firm-wide viability.

State 14(c) elimination in year τ_s raises the binding wage floor from $\underline{w}$ to $m_{s\tau_s}$, a change $\Delta w = m_{s\tau_s} - \underline{w}$ that under the GAO benchmark of $\underline{w} = \$4.15$ ranges from \$3.10 to \$9.35 per hour across the pre-elimination state minimum wages of our eleven treated states (\$7.25 to \$13.50) (U.S. Government Accountability Office, 2023). The wage-floor jump is thus a large fraction of the pre-elimination subminimum wage and represents an economically first-order shock to the 14(c) program’s operating margin.

The framework generates three testable predictions. First, on the extensive margin, the wage-floor jump reduces the 14(c) program’s operating margin by $\Delta w \cdot n_{it} \cdot h$, and holders whose pre-elimination program margin is close to the solvency threshold cross into $\pi_{it} < 0$ and exit the program, so the rate of program exit, observable as a holder disappearing from DOL certificate records, rises post-elimination. Because program exit does not require firm-wide insolvency, the rate of *organizational* exit should rise by much less than the rate of program exit, and may not rise detectably at all if 14(c) revenue is a modest share of certificate holders’ total operations.

Second, on the reallocation margin, workers of program-exiting holders remain in the local labor market for disability services and the state-contract caseload does not vanish when a holder exits the program, so surviving holders absorb a share $\eta_s \in [0, 1]$ of the exiting caseload and revenue at a surviving holder i is

$$R_{i,t \geq \tau_s} = R_{i,\tau_s-1} + \eta_s \cdot \sum_{j \neq i} \mathbf{1}\{\text{exit}_{jt}\} \cdot R_{j,\tau_s-1}, \quad (2)$$

which yields a revenue response at surviving employers proportional to aggregate exiter revenue; here exit_{jt} denotes holder j ’s *program* exit, since reallocation requires only that a

competitor stop serving the caseload and not that it cease to exist.

Finally, the intensive-margin response of wage spending among continuing employers is theoretically ambiguous. State elimination raises the wage floor for each subminimum-wage-eligible worker retained by a surviving certificate holder, from $\underline{w}$ to at least m_{sts} , mechanically increasing total wage spending, $w_{it} \cdot n_{it}$, holding employment fixed. However, this direct effect may be offset if employers facing a higher mandated wage reduce the number of subminimum-wage-eligible workers they retain without exiting the certificate program altogether. Thus, the overall effect on wage spending among continuing employers is ambiguous, reflecting the balance between higher wages for retained workers and potential reductions in employment. We test this prediction using total non-officer wage spending rather than DOL-reported worker counts because the reported 14(c) workforce is mechanically driven toward zero once a certificate holder exits the program, regardless of any change in its underlying employment.

4 Data and Empirical Strategy

4.1 Employer Panel and IRS 990 Linkage

Our primary data are from a Freedom of Information Act from the Department of Labor Wage and Hour Division on 14(c) Certificate Holder from 2015 through 2024. The data include employer name, mailing address, certificate type, workers paid subminimum wages, and certificate start and end dates. Deduplicating yields 3,854 unique employers who together contribute 34,604 employer-year observations, with each employer contributing a row for every year in which it held a certificate.

We link employers in the DOL FOIA data to their IRS Form 990 nonprofit filings through a multi-step matching procedure. First, for each certificate holder in the DOL FOIA data, we search ProPublica’s Nonprofit Explorer database using the employer’s name and restrict the search to organizations located in the same state. We then compute a word-overlap

similarity score between the normalized employer name and each candidate organization returned by the search. The highest-scoring candidate is selected if it exceeds a predefined similarity threshold. We use two matching standards: a permissive threshold of 0.5 and a more verified threshold of 0.9.

After identifying a matching organization, we use its Employer Identification Number (EIN) reported in the Form 990 records to retrieve the organization’s complete Form 990 filing history from ProPublica’s per-organization database. This retrieval is based on an exact EIN lookup across all available years.

The Form 990 filings provide information on total revenue, total functional expenses, compensation of current officers, other salaries and wages, and program service revenue. This procedure yields 11,906 employer-year Form 990 filings corresponding to 1,507 unique nonprofit organizations spanning 2013–2024.

We construct two complementary measures of firm exit using the DOL FOIA data and IRS Form 990 filings. The first measure, exit (DOL), equals one in an employer’s last observed year in the DOL FOIA certificate panel and zero in all earlier years. This measure captures exit from the Section 14(c) subminimum-wage program. However, an employer may disappear from the DOL data either because it permanently closed or because it stopped employing workers under Section 14(c) while continuing to operate. The DOL data alone cannot distinguish between these two possibilities.

To separate program exit from organizational closure, we construct a second measure, exit (990), using each employer’s complete IRS Form 990 filing history. We identify the last year the organization filed a Form 990, regardless of whether it appears in the DOL panel. The indicator equals one only in this final filing year, provided it falls within our sample period, and zero in all other linked employer-years. It is missing for employers that cannot be linked to Form 990 data.

The two measures capture different outcomes. Exit (DOL) measures whether an em-

ployer leaves the Section 14(c) program, while exit (990) measures whether the organization ceases operations altogether. Comparing them allows us to assess whether increases in DOL exits reflect firms shutting down or simply discontinuing their use of subminimum-wage employment.

4.2 Empirical Strategy

Our primary specification is a two-way fixed effects model:

$$Y_{ist} = \beta \text{State14(c)Elimination}_{st} + \alpha_i + \alpha_t + \varepsilon_{ist}, \quad (3)$$

where Y_{ist} is the outcome for employer i in state s and year t , and $\text{State14(c)Elimination}_{st}$ is an indicator equal to one in years when state s 's elimination of the 14(c) subminimum wage is in effect and zero otherwise. Employer fixed effects α_i absorb time-invariant employer characteristics, and year fixed effects α_t absorb shocks common to all employers in a given year. Standard errors are clustered at the state level. We fit outcomes for two discrete-time firm exit measures, exit (DOL) and exit (990), the log of total revenue at surviving employers with linked 990 records, the log of non-officer wage spending at continuing employers, and the officer compensation share of revenue.

Exit (DOL) and exit (990) are discrete-time hazard models estimated by linear probability regression. Each employer contributes an observation for every year it is at risk of exit, every year up to and including the year it exits, and is absent from the panel in every subsequent year, so the outcome equals one only in the single year the exit occurs and zero in every prior at-risk year for that employer.

We complement the two-way fixed effects specification with an event-study version of equation (3) that replaces $\text{State14(c)Elimination}_{st}$ with a full set of leads and lags relative

to each state’s elimination year τ_s :

$$Y_{ist} = \sum_{\substack{r=-5 \\ r \neq -1}}^2 \beta_r \mathbf{1}[t - \tau_s = r] + \alpha_i + \alpha_t + \varepsilon_{ist}, \quad (4)$$

with event time bottom-coded at -5 and the year before elimination ($r = -1$) omitted as the reference period; the horizontal axis is truncated at $r = 2$, the last event year with observations across all treated cohorts (Figure 2). As a check against the heterogeneous-treatment-effect bias two-way fixed effects event-study estimates can suffer under staggered adoption, we also estimate the interaction-weighted event-study estimator of [Sun and Abraham \(2021\)](#) (Appendix Figure B1).

4.3 Identification Assumptions

The empirical strategy identifies the effect of state elimination of Section 14(c) from changes in outcomes among employers in states that eliminate subminimum wages relative to changes among employers in states that do not. The key identifying assumption is that, in the absence of elimination, outcomes for employers in treated states would have followed the same underlying time path as outcomes for employers in the comparison states. This assumption does not require treated and comparison employers to have identical levels of outcomes; employer fixed effects allow for permanent differences across employers. Rather, it requires that, conditional on employer and year fixed effects, there are no systematic differences in the evolution of outcomes that are related to the timing of state elimination.

The institutional setting provides some support for this assumption. The timing of state elimination policies varies substantially across states, and the reforms arise from state-level changes in disability-employment policy rather than from changes at the individual employer level. Employers within the same state are therefore exposed to the policy at the same time, while employers in other states provide a comparison group over the same period.

We assess the plausibility of the identifying assumption using the event-study estimates in Figure 2. The pre-treatment coefficients provide a test of whether treated and comparison employers exhibit differential changes in outcomes before the policy takes effect. For log revenue and log non-officer wage spending, the pre-treatment coefficients are small and statistically indistinguishable from zero under both the two-way fixed effects specification and the interaction-weighted estimator of Sun and Abraham (2021). The evidence is less uniform for the two exit measures. Some pre-treatment coefficients are statistically significant, although they do not exhibit a common pattern across the two exit measures or across estimators.

5 Main Results

5.1 National Trends and the Extensive-Margin Firm Response

The Section 14(c)-covered workforce fell from 206,000 in 2015 to 44,000 in 2024 (Panel A of Figure 1), a 79 percent decline over the decade. Over the same period, the number of active certificate holders fell from 2,870 to 857. These trends indicate a substantial nationwide contraction in the use of Section 14(c) certificates and subminimum-wage employment. However, the national decline reflects both broader changes in the Section 14(c) workforce and the growing adoption of state-level policies restricting or eliminating subminimum-wage employment. Between 2015 and 2024, 15 states enacted laws eliminating Section 14(c) employment, rendering certificates legally inapplicable in those states once the laws took effect.

Panel B of Figure 1 disaggregates these trends by state policy status. It shows that 14(c) use declined gradually even in states without legislative bans (the “No law change” group), consistent with the broader secular erosion of the 14(c) program. In contrast, states that completed the phase-out experienced a sharp decline in 14(c) use, reaching zero by 2024. This distinction is central to our identification strategy: our two-way fixed-effects design

isolates the discrete policy shock associated with state elimination laws from the broader, gradual decline in 14(c) use that occurred in both treated and control states.

Building on these descriptive trends, we next use the two-way fixed-effects model in Equation 3 to estimate the effect of state elimination laws. Column (1) of Table 2 shows that elimination increases the annual DOL-measured exit hazard by 25 percentage points, a large and precisely estimated effect. The DOL-based exit measure, however, does not necessarily represent permanent firm closure. An employer can disappear from the DOL FOIA panel simply by terminating its 14(c) certificate while continuing to operate.

Column (2) of Table 2 addresses this distinction by reporting a 990-confirmed exit hazard constructed from each employer’s complete Form 990 filing history. We code an employer as exiting only in its true final filing year and only when that year falls within our observation window. Because conversion from nonprofit to for-profit status is uncommon, we treat the cessation of Form 990 filings as evidence that the organization has ceased operations. The corresponding two-way fixed effects estimate is small, negative, and statistically indistinguishable from zero. Thus, the large positive estimate in column (1) does not appear to reflect widespread permanent firm closure. Instead, it primarily captures employers’ withdrawal from the 14(c) program: following state elimination, many certificate holders stop reporting subminimum-wage workers to the DOL while continuing to operate and file with the IRS.

5.2 Revenue Response and Firm Adaptation

Section 5.1 shows that employers are substantially more likely to leave the 14(c) program following state elimination, but that this program exit does not translate into a corresponding increase in permanent organizational closure. We next examine how firms’ revenue responds to the loss of access to the 14(c) program.

Column (3) of Table 2 shows that firm revenue increases by approximately 17 percent

following elimination, relative to the change in revenue among firms in states that had not yet eliminated 14(c). Thus, elimination does not appear to reduce the overall revenue of affected employers. Instead, firms that remain operational experience faster revenue growth following the policy change.

Our data do not allow us to directly identify the source of this additional revenue or distinguish among potential adjustment mechanisms. In particular, we cannot determine whether firms expand alternative employment services, shift toward other publicly funded programs, serve a different mix of clients, or otherwise adjust their operations following the loss of 14(c). The positive revenue response is nevertheless consistent with firms adapting to elimination by shifting their activities toward other sources of revenue rather than simply shutting down.

5.3 Labor Spending and Officer Compensation

We next examine whether elimination affects labor spending or the distribution of revenue among employers that remain operational. Column (4) of Table 2 shows that non-officer wage spending increases by only 4.5 percent following elimination, and the estimate is not statistically distinguishable from zero. Because the outcome measures total non-officer wages rather than employment or wages per worker, the estimate cannot distinguish between changes in the number of workers and changes in their compensation. Nevertheless, the small and statistically insignificant effect indicates that elimination does not lead to a large reduction in aggregate labor spending among continuing employers.

We also examine whether the increase in revenue at continuing employers is reflected in greater officer compensation. Column (5) of Table 2 shows that the officer share of revenue is essentially unchanged following elimination, with a small and statistically insignificant point estimate of 0.010. The lack of an increase in officer compensation suggests that the additional revenue is not primarily reflected in greater compensation for officers. This pattern is

consistent with the nondistribution constraint faced by nonprofit organizations ([Hansmann, 1980](#)), although our data do not allow us to identify how the additional revenue is ultimately used.

5.4 Heterogeneity by Pre-Repeal State Minimum Wage

The effects of 14(c) elimination may depend on the size of the increase in wages required for workers previously employed under 14(c) certificates. In the federal data, workers employed under 14(c) certificates earned an average hourly wage of approximately \$4.15 ([U.S. Government Accountability Office, 2023](#)). We do not observe the average wage paid under 14(c) certificates separately by state, which prevents us from directly measuring the size of the wage increase associated with elimination. Because employers must comply with the applicable state minimum wage following elimination, however, states with higher minimum wages are likely to have faced larger increases in required compensation. We therefore examine heterogeneity by the pre-repeal state minimum wage, dividing the eleven treated states into two groups according to each state’s average minimum wage in the years preceding the effective date of repeal.

Panel A contains the five states with the lowest pre-repeal minimum wages: South Carolina, Tennessee, New Hampshire, Maryland, and Maine. Their average minimum wages range from \$7.25 to \$8.57. Panel B contains the six states with the highest pre-repeal minimum wages: Delaware, Alaska, Hawaii, Rhode Island, Oregon, and Washington, with average minimum wages ranging from \$8.72 to \$10.68. Each specification includes the treated states in the corresponding panel and all never-treated states as controls. The two panels therefore use disjoint sets of treated states.

The estimates in [Table 3](#) indicate that the effects are concentrated in states with higher pre-repeal minimum wages. In Panel A, none of the five outcomes is statistically distinguishable from zero. In Panel B, elimination is associated with a significant increase in the

DOL-based exit rate, a reduction in revenue, and an increase in the share of compensation paid to officers.

Both panels, however, draw their post-elimination identification from a small number of states. South Carolina supplies roughly 60 percent of Panel A’s post-elimination observations across outcomes, and Washington supplies a comparable share of Panel B’s. This concentration offers a more general explanation for both halves of the panel contrast: the precisely estimated nulls in Panel A are driven by South Carolina rather than a broad-based absence of response among the smaller pre-repeal wage states. Similarly, the significant Panel B coefficients, including the positive officer-share result, are better read as evidence about Washington in particular than as a general feature of the smaller pre-repeal wage group. Given this concentration we treat the contrast between panels as suggestive rather than as a precisely estimated.

5.5 Selection Among Survivors: Are Surviving Firms Larger?

We next ask whether providers that remain active after elimination differ systematically in pre-period size from those that exit. Answering this question requires distinguishing the relevant exit margin. Within treated states, 177 of 178 certificate holders (99.4 percent) show a DOL program exit somewhere in the sample window. This confirms the conceptual distinction in Section 3 between certificate exit, which state elimination induces for essentially every holder, and organizational exit, which it does not. DOL exit therefore provides little basis for a survivor-exiter comparison, since nearly every treated-state holder eventually exits the certificate. We instead use 990-confirmed organizational exit, which is the margin referred to as “firm survival” throughout the paper.

Table Appendix B3, Panel A, compares pre-period characteristics across these two groups. Survivors report substantially higher pre-period revenue (\$13.1 million versus \$2.2 million, $p = 0.012$) and higher assets (\$9.5 million versus \$4.3 million, $p = 0.084$). Pre-period 14(c)

worker counts, however, show the opposite pattern: organizations that later close have larger 14(c) workforces (77.4 versus 48.1 workers, $p = 0.090$).

Panel B formalizes these relationships using a linear probability model of 990-confirmed organizational exit on pre-period log revenue and log 14(c) worker count, with state fixed effects. Column (4), which includes both measures jointly, shows that overall organizational scale and 14(c)-specific exposure are associated with exit in opposite directions. A one-log-point increase in pre-period revenue is associated with a 6.4 percentage-point lower probability of closing ($p = 0.004$), whereas a one-log-point increase in pre-period 14(c) worker count is associated with a 6.7 percentage-point *higher* probability of closing ($p = 0.020$), conditional on overall revenue. Thus, although organizations that survive elimination are larger overall, organizations that close tend to have a greater concentration of their operations in the 14(c) program. This pattern is consistent with organizational exit being more closely related to exposure to the eliminated program than to overall organizational size.

5.6 Robustness Checks

We perform several tests to bolster a causal interpretation for our results. First, we assess the robustness of the main results to two different specifications. Second, we use placebo locations to show that the main results are specific to the treated states.

5.6.1 Robustness to Sample Construction

Table Appendix B1 reports two sample-exclusion checks on the main specification. Panel A excludes Vermont, Texas, and Illinois. Vermont eliminated 14(c) administratively in 2002, well before our 2013–2024 sample period begins. Texas and Illinois eliminate subminimum-wage pay for employers with state-use contracts rather than eliminating 14(c) statewide. Panel B excludes California, Colorado, Nevada, and Virginia, which have enacted 14(c) elimination laws whose effective dates fall after our sample window ends on December 31,

2024. Because their laws had not yet taken legal effect within our sample, these four states are coded as never-treated throughout the paper, and Panel B tests whether the results are robust to dropping these states from the control group.

In both panels, the estimated effect of state elimination on the DOL-measured exit hazard, the 990-confirmed exit hazard, revenue, wages, and officer share remains similar to the corresponding estimates in Table 2 in both sign and magnitude. The main results are therefore not being driven by the inclusion of control states whose classification is debatable on institutional grounds.

Table Appendix B2 reports another robustness check, tightening the fuzzy name-match threshold used to link certificate holders to their IRS Form 990 filer from 0.5 (the baseline used throughout the paper, already a nontrivial bar requiring a majority of the two organization names' words to overlap after normalization) to 0.9 in increments of 0.1. Every outcome retains its sign and magnitude across the full range. The most informative comparison is 0.5 to 0.7, where the coefficients are essentially flat; tightening further to 0.8 and 0.9 shrinks the sample by roughly 9 to 11 percent; because word-overlap similarity cannot distinguish a genuinely uncertain match from a correct match whose name differs only cosmetically (an abbreviated suffix, a dropped article, a minor spelling variant), these higher thresholds plausibly discard some correct matches along with any incorrect ones. The log revenue coefficient remains positive throughout this entire range and never falls below marginal significance, moving from the 5 to the 10 percent level only because of this narrower sample, not because the point estimate moves. The 990-linked results are therefore not an artifact of the specific similarity threshold used to construct the matched sample.

5.6.2 Permutation Tests

To assess whether our baseline results are specific to the treated states, we conduct a variant of Fisher's permutation (or randomization inference) test. Specifically, we estimate Equation

(3) 500 times, randomly assigning a placebo Section 14(c) elimination year to each state in each iteration. These placebo regressions allow us to assess whether the estimated effect in the actual treatment assignment is unusually large relative to effects obtained under randomly assigned treatment timing.

Figure 3 presents a histogram of the estimated β coefficients from these placebo tests when the outcome is $\ln(\text{revenue})$. The solid vertical line indicates the coefficient estimated using the actual treatment assignment, reported in Table 2, while the dashed lines denote the 5th and 95th percentiles of the placebo distribution. The actual estimate is larger in absolute value than 96% of the placebo estimates. This result suggests that the estimated effect of Section 14(c) elimination on firm revenue is unlikely to be driven by the particular states and treatment timing in our baseline specification.

5.7 Mechanism Tests

In this section, we examine the potential mechanisms through which the elimination of subminimum wages increased the revenues of firms that remained in operation. We consider two broad channels: (i) changes in grant and donation funding and (ii) reduced market competition following firm exit.

The first mechanism relates to the composition of nonprofit revenue. In addition to revenue generated from their core service activities, nonprofit organizations often rely on grants and charitable donations. One possibility is that, following the elimination of subminimum wages, state governments and private donors provided additional financial support to help organizations transition away from segregated sheltered workshops and toward competitive, integrated employment models. If so, the observed increase in total revenue could simply reflect an increase in external funding rather than stronger demand for the organizations' services. To evaluate this mechanism, we separately examine revenue from contributions, grants, and donations. The results, reported in Table 4, provide little evidence supporting

this explanation. We find no statistically significant increase in contribution revenue following the policy change, suggesting that the increase in total revenue is unlikely to be driven by additional grant or donation funding.

The second mechanism relates to changes in competitive pressure. If the elimination of subminimum wages caused some organizations to exit, the remaining providers may have benefited from less competition and captured a larger share of demand. Unfortunately, our data do not allow us to directly test this mechanism, as we lack information on market shares, client allocations, or service contracts. We therefore view this channel as a plausible explanation for the observed increase in revenues among surviving firms, rather than as a mechanism that can be empirically verified with our data.

6 Welfare Implications

In this section, we attempt to shed light on a welfare assessment of state 14(c) elimination. Such an assessment requires information on what happens to workers displaced from 14(c) employment, including where they go and how their earnings change, as well as the additional fiscal costs borne by state and federal governments following elimination. We therefore combine our reduced-form estimates with published evidence on post-elimination worker outcomes and public-use fiscal data to conduct a supplementary marginal-value-of-public-funds calibration following the framework of [Hendren and Sprung-Keyser \(2020\)](#). Rather than producing a single welfare estimate, this exercise characterizes a range of possible welfare effects under alternative assumptions.

Worker willingness-to-pay is defined as the present discounted value of the net earnings gain for workers who transition from subminimum-wage employment to competitive integrated employment following elimination. We subtract income and payroll taxes, as well as any reduction in SSDI or SSI benefits. In particular, under the Social Security Administration’s earnings rules, beneficiaries who earn above the Substantial Gainful Activity threshold

may no longer qualify for disability benefits or may experience a reduction in benefits. Thus, higher earnings may come at the cost of forgone SSDI or SSI income, which we incorporate into workers’ net willingness-to-pay.

The government fiscal cost includes vocational rehabilitation program expenditures, the loss of business tax revenue associated with firm exit, and the change in income tax revenue resulting from higher worker earnings, net of savings from reduced SSDI and SSI benefit payments when workers leave the disability rolls. Full input parameters are drawn from published benchmarks in [U.S. Government Accountability Office \(2023\)](#), [U.S. Government Accountability Office \(2025\)](#), [Rehabilitation Services Administration \(2023\)](#), and a shared SSA state-year benefit panel. The calibration procedure is detailed in Appendix A, and the full set of parameter values, sensitivity ranges, and sources is reported in Table A1. We evaluate the Marginal Value of Public Funds (MVPF) over a five-year horizon using a 3 percent real discount rate.

The aggregate MVPF is 4.49 at baseline, falls to 0.77 under the conservative parameter combination, and is Pareto-improving (net government cost turns negative) under the favorable combination (Table 5, Panel A); Panel B reports the key parameters governing this range, and Table A1 reports the full parameter set. The MVPF falls below unity only when the transition share, SSDI exit share, VR cost, and business tax spillover are all set to their least favorable values simultaneously, as in the conservative scenario.

7 Conclusion

This paper examines how 14(c) certificate holders respond when states eliminate the use of subminimum wages for workers with disabilities. The results indicate that these policies substantially alter the organization of subminimum-wage employment, but do not lead to a corresponding increase in organizational closure. Following state-level elimination, the probability that a certificate holder discontinues its subminimum-wage program increases by

approximately 25 percentage points per year relative to providers in states that retain the policy. By contrast, we find little evidence that affected organizations are more likely to cease operations altogether. Instead, the evidence is consistent with a reallocation of disability-service activity across existing providers. Among organizations that remain active, revenues increase by approximately 17 percent, suggesting that surviving providers absorb at least part of the activity previously carried out by organizations that discontinue subminimum-wage employment.

The institutional structure of the disability-service sector provides one explanation for this pattern of adjustment. In many conventional markets, firms facing higher labor costs can partially offset those costs through price increases, as documented in studies of minimum-wage policies in other settings ([Cengiz et al., 2019](#); [Dustmann et al., 2022](#); [Harasztosi and Lindner, 2019](#)). Disability-service providers, however, often operate under administratively determined reimbursement rates through state contracts and Medicaid programs ([U.S. Government Accountability Office, 2021](#)), limiting their ability to adjust prices directly. Under these conditions, adjustment may instead occur through changes in the composition and organization of service provision. Our findings are consistent with such a mechanism: some providers discontinue subminimum-wage operations, while service activity and associated revenues shift toward other organizations rather than disappearing from the sector altogether.

We also find little evidence that the additional revenue received by surviving providers is systematically reflected in higher compensation. Officer compensation share does not increase following policy adoption. Although estimated effects on worker wage expenditures are positive, they are imprecisely estimated and cannot be distinguished from zero. These results provide little evidence that the additional revenue accruing to surviving providers is associated with higher compensation for either employees or officers.

Finally, we consider the fiscal consequences of eliminating subminimum wages. A central concern is that workers displaced from subminimum-wage employment may rely more heavily

on public benefits or government-funded vocational rehabilitation services, increasing the public cost of the policy. Combining our estimates with published evidence on workers' outcomes after leaving subminimum-wage employment, we construct a fiscal accounting that incorporates additional vocational rehabilitation expenditures, changes in disability-benefit receipt, and changes in tax revenue associated with subsequent employment and earnings. Across a range of plausible assumptions, the additional public costs are substantially offset by higher tax revenue and reductions in disability-benefit payments. Under our baseline assumptions, the policy generates net fiscal savings. Although these calculations necessarily depend on assumptions about workers' subsequent employment and earnings, they suggest that eliminating subminimum wages need not impose substantial additional fiscal costs and may generate savings to the public sector.

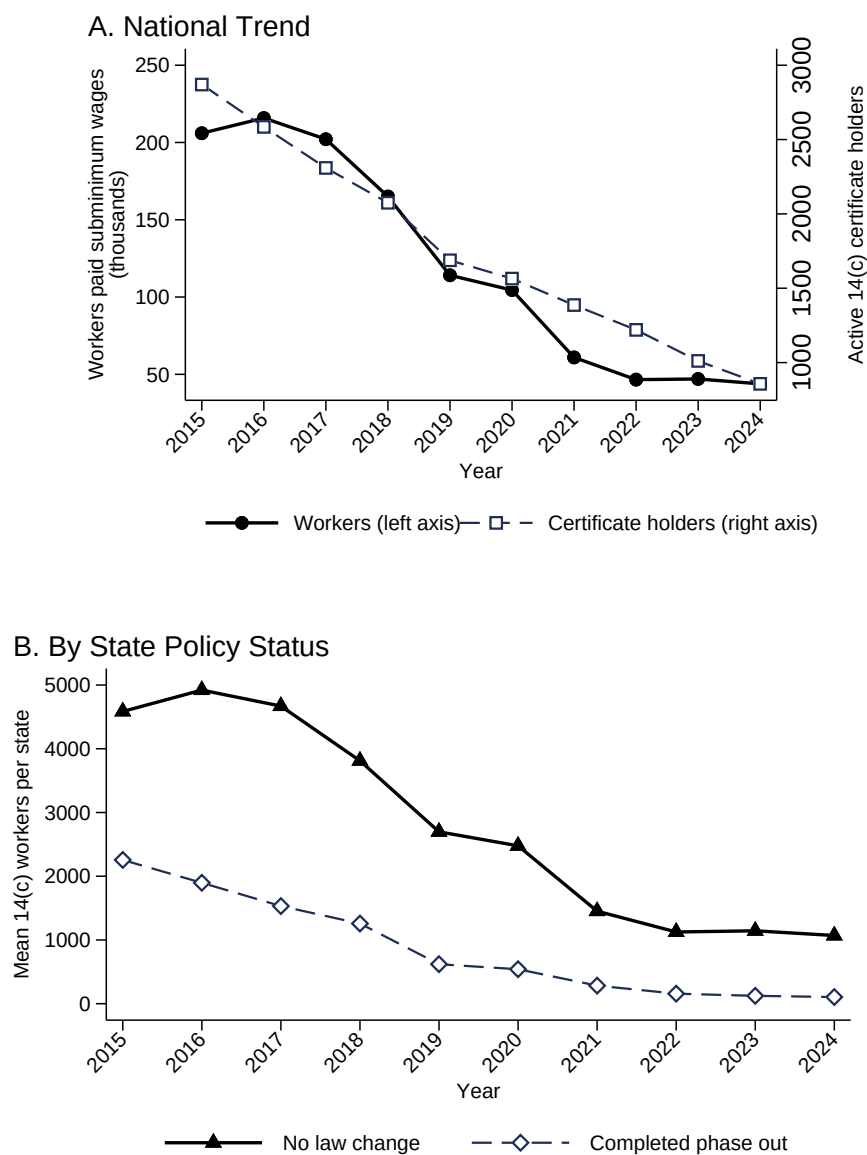
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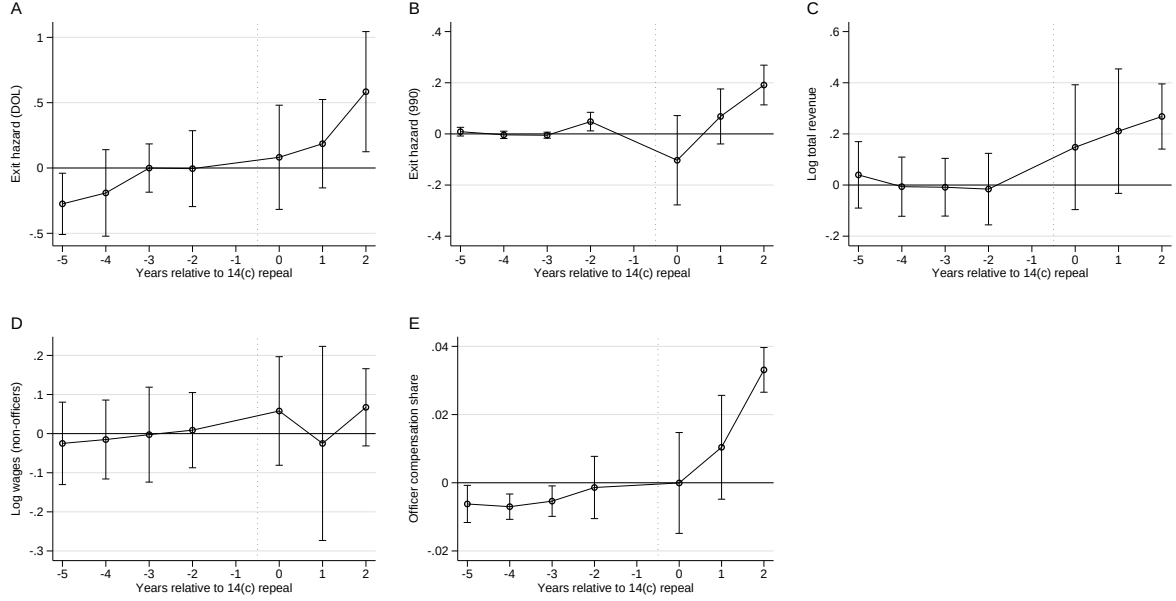
Figures and Tables

Figure 1. Section 14(c) Workforce and Certificate Holders, 2015 to 2024



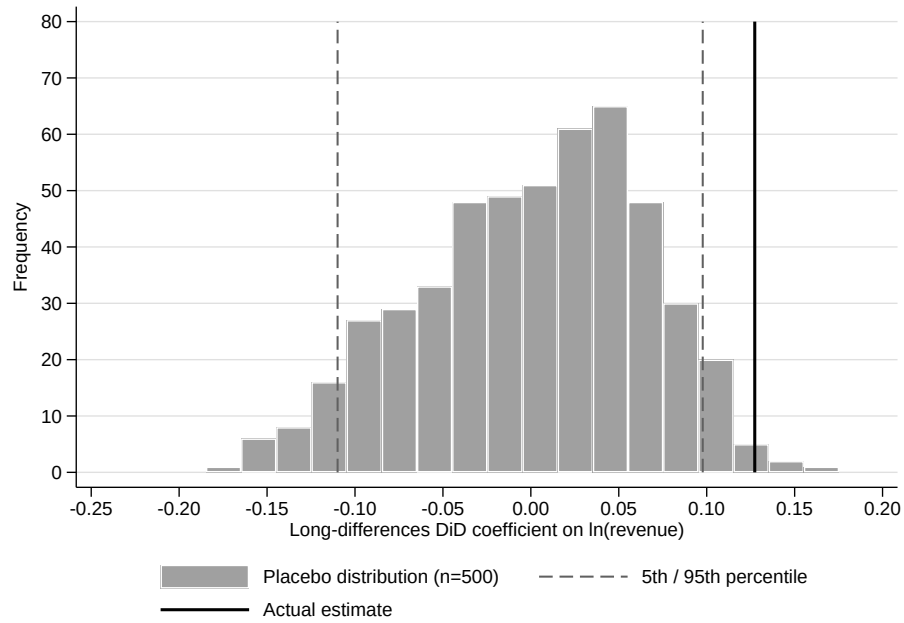
Notes: Data from Department of Labor Wage and Hour Division. In Panel A, the workforce (left axis, solid line) fell from 206,000 workers in 2015 to 44,000 in 2024, a 79 percent decline. The count of active certificate holders (right axis, dashed line) fell from 2,870 to 857, a 70 percent decline.

Figure 2. Effect of State 14(c) Elimination on Firm Outcomes, Event Study



Notes: Two-way fixed effects event-study estimates (employer and year fixed effects), the same specification and the same five outcomes and order as Table 2. Vertical bars are 95 percent confidence intervals (± 1.96 standard errors), clustered at the state level; event time -1 is the omitted base period. Panel A shows the DOL-based discrete-time firm exit hazard, panel B the 990-confirmed exit hazard (990-linked sample only), panel C the log of total IRS 990 revenue, panel D the log of non-officer salaries and wages, and panel E the officer compensation share of revenue, all restricted to the 990-linked sample under the permissive match in panels B through E. Event time is capped at -5 and $+5$ and the horizontal axis is truncated at $+2$, the last event year with observations across all treated cohorts.

Figure 3. Permutation tests on effects of Section 14(c) elimination on revenue



Notes: This figure shows the histogram of placebo estimates of Equation (3) 500 times by randomly assigning a placebo Section 14(c) elimination year to each state in each iteration. The vertical solid line represents our actual difference in differences estimate. The dashed lines are 5th and 95th percentile of the placebo estimates.

Table 1. State Legislation to Eliminate Subminimum Wage Employment

State	Bill No.	Enacted date	Effective date	Treated in our sample
Alaska	Public an- nouncement No. 18-04	2/16/2018	2/16/2018	Yes
California	2021 SB-639	9/27/2021	1/1/2025	No
Colorado	2021 SB-39	6/29/2021	7/1/2025	No
Delaware	2021 HB-122	10/20/2021	1/31/2024	Yes
Hawaii	2021 SB-793	6/21/2021	6/21/2021	Yes
Maine	2020 LD-1874	3/17/2020	3/17/2020	Yes
Maryland	2016 SB-417	5/19/2016	10/1/2020	Yes
Nevada	2023 AB-259	6/16/2023	1/1/2028	No
New Hampshire	2015 SB-47	5/11/2015	7/6/2015	Yes
Oregon	2019 SB-494	6/21/2019	7/1/2023	Yes
Rhode Island	2022 HB-7511	6/15/2022	6/15/2022	Yes
South Carolina	2021 S-553	6/1/2022	1/1/2023	Yes
Tennessee	2022 SB-2042	4/26/2022	7/1/2022	Yes
Virginia	2023 HB-1924	4/12/2023	7/1/2030	No
Washington	2021 SB-5284	4/16/2021	7/25/2021	Yes

Notes: As of August 2026, fifteen states have enacted legislation to eliminate Section 14(c) subminimum wage employment. In our sample, “treated” states are those for which the legislation took effect on or before December 31, 2024, the end of our sample period. The remaining four states, California, Colorado, Nevada, and Virginia, have enacted such legislation, but their effective dates fall after our sample period. Our results are robust to either including or excluding these four states from the control group.

Table 2. Effect of State 14(c) Elimination on Firm Outcomes

	Exit (DOL)	Exit (990)	Log revenue	Log wages (non-officers)	Officer share
	(1)	(2)	(3)	(4)	(5)
State 14(c) elimination	0.251** (0.116)	-0.036 (0.071)	0.166** (0.082)	0.045 (0.056)	0.010 (0.007)
Employer FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	State	State	State	State	State
Observations	17,971	8,407	8,390	7,338	8,153
R-squared	0.413	0.789	0.973	0.972	0.746

Notes: Column (1), exit (DOL), uses the full employer-year panel; the exit hazard is a discrete-time indicator equal to one in the employer's last-observed year in the DOL FOIA certificate data. Column (2), exit (990), is a 990-confirmed exit hazard restricted to the 990-linked sample. Officer share is the ratio of officer compensation to total revenue. Sample sizes differ substantially across columns because they draw on progressively narrower subsets of the same 18,590 employer-year observations. Column (1) uses the full panel. Columns (2) through (5) are restricted to the 46.3 percent of employer-years (8,600 of 18,590) that match an IRS Form 990 filing. Column (3), log revenue, loses almost no further observations to missingness (48 of 11,906 990-linked employer-years report zero or missing total revenue), so its sample loss is driven by 990 linkage coverage itself rather than gaps in the revenue field. Column (4) loses substantially more: 318 employer-years have a missing non-officer wage line and a further 713 report exactly \$0 (plausible for very small certificate holders with no non-officer staff), both excluded by the positive-value log restriction. Column (5) additionally requires non-missing officer compensation (missing for the same 318 employer-years, since both fields come from the same filing) and trims the top 1 percent of the officer-share distribution (115 employer-years). Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3. Effect of State 14(c) Elimination on Firm Outcomes, by Pre-Repeal State Minimum Wage

	Exit (DOL)	Exit (990)	Log revenue	Log wages (non-officers)	Officer share
	(1)	(2)	(3)	(4)	(5)
Panel A. Lower minimum wage states (SC, TN, NH, MD, ME)					
State 14(c) elimination	0.132	-0.103	0.195	0.070	-0.002
	(0.132)	(0.135)	(0.170)	(0.142)	(0.001)
Observations	17,491	8,100	8,084	7,056	7,861
R-squared	0.415	0.789	0.973	0.972	0.744
Panel B. Higher minimum wage states (DE, AK, HI, RI, OR, WA)					
State 14(c) elimination	0.419***	0.023	0.139***	0.029	0.019***
	(0.082)	(0.014)	(0.048)	(0.026)	(0.006)
Observations	17,177	8,129	8,112	7,103	7,888
R-squared	0.416	0.793	0.972	0.971	0.745

Notes: Two-way fixed effects DiD estimates on state 14(c) elimination, fit separately within two groups of states ranked by each state's own average minimum wage in the years before its 14(c) repeal took effect. Standard errors clustered at the state level in parentheses. Panel A comprises the five lowest-ranked states: South Carolina, Tennessee, and New Hampshire (average pre-repeal MW \$7.25), Maryland (\$8.56), and Maine (\$8.57). Panel B comprises the six highest-ranked states: Delaware (\$8.72), Alaska (\$8.76), Hawaii (\$8.79), Rhode Island (\$9.64), Oregon (\$10.54), and Washington (\$10.68). Each panel's regression sample includes that panel's treated states plus all never-treated states as controls; the other panel's treated states are excluded, so Panels A and B are estimated on disjoint treated groups. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Alternative Revenue and Financial Measures

	(1)	(2)	(3)
	Log total revenue	Log program revenue	Log contribution revenue
State 14(c) elimination	0.166** (0.082)	0.058 (0.220)	-0.093 (0.313)
Employer FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Cluster	State	State	State
Observations	8,390	7,872	7,787
R-squared	0.973	0.949	0.831

Notes: Two-way fixed effects DiD estimates on state 14(c) elimination, employer and year fixed effects, standard errors clustered at the state level, 990-linked (permissive match) sample, positive-value logs. Column (1) reproduces Table 2 column (3) (log total revenue) for direct comparison; it is not re-estimated. Column (2) decomposes total revenue into program service revenue, the portion of income tied directly to service delivery. Column (3) decomposes total revenue into total contributions and grants received, testing whether the revenue gain in column (1) reflects additional external funding rather than stronger demand for services. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. Aggregate Welfare Effects of Eliminating Section 14(c)

<i>Panel A: Aggregate welfare estimates</i>			
Scenario	PDV of Worker WTP	PDV of Government Cost	MVPF
Conservative assumptions	\$42.7m	\$55.2m	0.77
Baseline assumptions	\$90.0m	\$20.1m	4.49
Favorable assumptions	\$122.9m	-\$48.2m	Pareto improving
<i>Panel B: Key sensitivity parameters</i>			
Parameter	Baseline	Sensitivity range	
Transition to competitive integrated employment	27%	12–46%	
Post-transition hourly wage	\$13.53	\$12.92–\$14.14	
SSDI exit share	10%	5–25%	
SSI exit share	5%	2–15%	
Worker marginal tax rate	15%	10–20%	
Business tax spillover	10%	5–15%	
VR cost per participant	\$1,750	\$1,500–\$2,000	

Notes: Panel A reports the present discounted value (PDV) of worker willingness to pay (WTP) and government costs over a five-year horizon using a 3 percent annual discount rate. The baseline MVPF is 4.49, implying that each dollar of government cost generates \$4.49 in benefits to affected workers. Conservative and favorable scenarios jointly vary the key parameters shown in Panel B. A negative government cost indicates net fiscal savings; under the favorable scenario, the policy is therefore Pareto improving. The full set of calibration parameters, sensitivity ranges, and sources is reported in Table A1 in Appendix A.

Appendix A: Marginal Value of Public Funds Calibration Procedure

The supplementary marginal-value-of-public-funds calibration in the main text converts the firm-side revenue-reallocation estimate into worker earnings gains and government fiscal costs under the framework of [Hendren and Sprung-Keyser \(2020\)](#). We evaluate the MVPF over a five-year horizon at a 3 percent real annual discount rate. Table 5 in the main text reports the resulting MVPF under the baseline, conservative, and favorable parameter combinations (Panel A) alongside the key sensitivity parameters (Panel B); Table A1 below reports the full set of calibration parameters, their sensitivity ranges, and sources.

Calibration procedure. For each treated state s , the number of workers transitioning to competitive integrated employment is $n_s = N_s^{\text{pre}} \cdot \tau$, where N_s^{pre} is the three-year pre-repeal average 14(c) workforce and τ is the transition-to-CIE share. Annual worker willingness-to-pay and annual government fiscal cost in state s are

$$\text{WTP}_s = n_s(w_{\text{CIE}} - \underline{w})h(1 - m) - n_s \theta_{\text{SSDI}} \bar{b}_{\text{SSDI},s} - n_s \theta_{\text{SSI}} \bar{b}_{\text{SSI},s}, \quad (5)$$

$$\text{GC}_s = \phi n_s c_{\text{VR}} + \tau_{\text{biz}} L_s - n_s \theta_{\text{SSDI}} \bar{b}_{\text{SSDI},s} - n_s \theta_{\text{SSI}} \bar{b}_{\text{SSI},s} - m n_s (w_{\text{CIE}} - \underline{w})h, \quad (6)$$

where w_{CIE} is the post-CIE wage, $\underline{w}$ is the subminimum wage benchmark, $h = 1,820$ is annual hours, m is the worker marginal tax rate, θ_{SSDI} and θ_{SSI} are the SSDI and SSI exit shares, $\bar{b}_{\text{SSDI},s}$ and $\bar{b}_{\text{SSI},s}$ are state-level average annual benefits from the SSA state-year panel, ϕ is the VR cost passthrough share, c_{VR} is the VR cost per participant, τ_{biz} is the business tax spillover, and L_s is pre-repeal firm revenue at risk in state s (the mean post-repeal firm exit hazard times mean pre-repeal firm revenue times the number of pre-repeal firms). Present discounted values aggregate over states and over the horizon using discount factor

$\rho = \sum_{t=0}^{T-1} (1+r)^{-t}$ with $T = 5$ years and $r = 3$ percent:

$$\text{PDV}_{\text{WTP}} = \rho \sum_s \text{WTP}_s, \quad \text{PDV}_{\text{GC}} = \rho \sum_s \text{GC}_s, \quad \text{MVPF} = \frac{\text{PDV}_{\text{WTP}}}{\text{PDV}_{\text{GC}}}. \quad (7)$$

The policy is Pareto-improving, and the MVPF is reported as such rather than as a ratio, whenever $\text{PDV}_{\text{GC}} \leq 0$. The baseline scenario sets every parameter to the central value in Table A1. The conservative scenario sets τ and θ_{SSDI} to the low end of their range and c_{VR} and τ_{biz} to the high end simultaneously, so that worker WTP and government cost move against the policy together; the favorable scenario reverses all four. The remaining parameters ($\underline{w}$, w_{CIE} , θ_{SSI} , m , and ϕ) are held at baseline in both scenarios.

Table A1. Calibration Assumptions and Parameter Sources

Parameter	Baseline	Sensitivity range	Source
Subminimum wage benchmark	\$4.15/hr	\$3.50–\$4.15	GAO-23-105116
Post-transition hourly wage	\$13.53/hr	\$12.92–\$14.14	GAO-25-106471
Transition to competitive integrated employment	0.27	0.12–0.46	GAO-25-106471
SSDI exit share	0.10	0.05–0.25	Authors’ calculation
SSI exit share	0.05	0.02–0.15	Authors’ calculation
Worker marginal tax rate	0.15	0.10–0.20	FICA, state, and federal taxes
Business tax spillover	0.10	0.05–0.15	Authors’ calculation
VR cost per participant	\$1,750	\$1,500–\$2,000	RSA Annual Report, FFY2021–22
VR cost passthrough share	1.00	0.75–1.00	WIOA Section 511
Annual hours	1,820	–	35 hours/week × 52 weeks
Discount rate	3%/year	–	Standard assumption
Time horizon	5 years	–	Baseline specification

Notes: This table reports the full set of parameters used to calibrate the aggregate marginal value of public funds (MVPF). The subminimum wage benchmark is the GAO’s national average hourly wage for workers employed under Section 14(c). The post transition wage and transition to competitive integrated employment share are based on reported post-transition outcomes in Colorado and Oregon.

SSDI and SSI exit shares are authors’ calculations based on Social Security Administration rules. For SSDI, part-time earnings at the post-transition wage may remain below the substantial gainful activity threshold. For SSI, the \$85 earnings disregard and the subsequent 50 percent benefit phase-out imply a smaller probability of complete benefit exit. Because no published estimates directly measure benefit exit for the population affected by Section 14(c) elimination, these parameters should be interpreted as calibration assumptions.

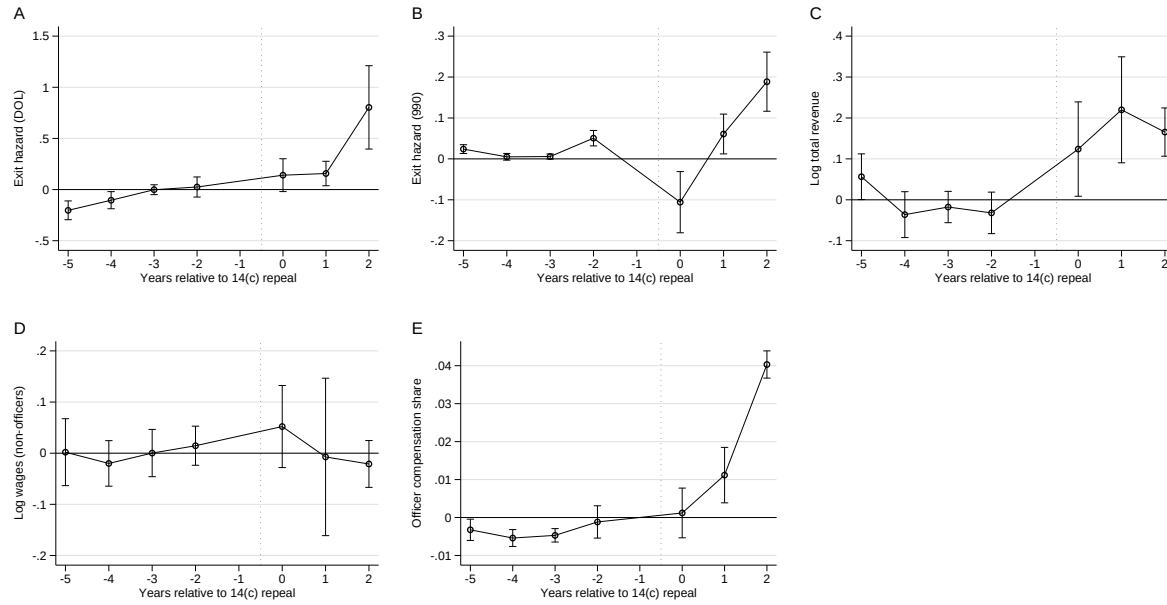
The worker marginal tax rate combines employee FICA contributions, state income taxes, and federal income taxes. The business tax spillover applies to certificate holders that are not organized as 501(c)(3) nonprofits. VR costs are based on the RSA Annual Report for FFY2021–22. The VR cost passthrough assumption reflects the VR referral requirements under Section 511 of the Workforce Innovation and Opportunity Act.

The sensitivity ranges are used to evaluate the robustness of the aggregate MVPF to alternative parameter values. In the main analysis, the conservative scenario uses $\tau = 0.12$, $\theta_{\text{SSDI}} = 0.05$, VR cost of \$2,000, and a business tax spillover of 15 percent. The favorable scenario uses $\tau = 0.46$, $\theta_{\text{SSDI}} = 0.25$, VR cost of \$1,500, and a business tax spillover of 5 percent. All remaining parameters are held at their baseline values.

Appendix B: Supplementary Figures and Tables

Figure B1 reports the interaction-weighted event-study estimator of [Sun and Abraham \(2021\)](#) as a robustness check on Figure 2: two-way fixed effects event-study estimates can be biased under staggered treatment timing when treatment effects are heterogeneous across cohorts, and the Sun-Abraham estimator corrects for this by estimating cohort-specific effects and aggregating them with weights that do not depend on already-treated cohorts serving as controls for later-treated ones.

Figure B1. Effect of State 14(c) Elimination on Firm Outcomes, Sun and Abraham (2021) Event Study



Notes: Interaction-weighted event-study estimates following Sun and Abraham (2021), with never-treated states as the comparison group. Vertical bars are 95 percent confidence intervals, clustered at the state level; event time -1 is the omitted period.

Table B1. Effect of State 14(c) Elimination on Firm Outcomes, Excluding Selected Control States

	Exit (DOL)	Exit (990)	Log revenue	Log wages (non-officers)	Officer share
	(1)	(2)	(3)	(4)	(5)
Panel A. Excluding VT/TX/IL					
State 14(c) elimination	0.244** (0.118)	-0.040 (0.072)	0.169** (0.082)	0.048 (0.056)	0.010 (0.008)
Employer FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	State	State	State	State	State
Observations	16,050	7,436	7,424	6,480	7,207
R-squared	0.410	0.793	0.973	0.971	0.750
Panel B. Excluding CA/CO/NV/VA					
State 14(c) elimination	0.254** (0.117)	-0.035 (0.072)	0.166** (0.082)	0.046 (0.055)	0.010 (0.008)
Employer FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	State	State	State	State	State
Observations	16,357	7,657	7,641	6,649	7,413
R-squared	0.415	0.792	0.972	0.973	0.741

Notes: This table reports two robustness checks of our main results. Panel A excludes Vermont, Texas, and Illinois. Vermont administratively eliminated Section 14(c) employment in 2002, before the beginning of our 2013–2024 study period, while Texas and Illinois eliminated subminimum-wage employment in the context of state use contracts. Panel B excludes California, Colorado, Nevada, and Virginia, which have enacted legislation to eliminate Section 14(c) employment but whose laws take effect after the end of our sample period on December 31, 2024. These seven states are treated as part of the control group throughout our main analysis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B2. Robustness to the IRS Form 990 Name-Match Threshold

	Exit (DOL)	Exit (990)	Log revenue	Log wages (non-officers)	Officer share
Panel A. Match score ≥ 0.5 (baseline, as in Table 2)					
State 14(c) elimination	0.2510** (0.1160)	-0.0356 (0.0709)	0.1648** (0.0822)	0.0450 (0.0557)	0.0099 (0.0075)
Observations	17,971	8,364	8,349	7,338	8,112
Panel B. Match score ≥ 0.6					
State 14(c) elimination	0.2510** (0.1160)	-0.0373 (0.0728)	0.1726** (0.0843)	0.0481 (0.0573)	0.0103 (0.0080)
Observations	17,971	8,012	7,999	7,095	7,784
Panel C. Match score ≥ 0.7					
State 14(c) elimination	0.2510** (0.1160)	-0.0141 (0.0550)	0.1830* (0.0971)	0.0533 (0.0671)	0.0110 (0.0081)
Observations	17,971	7,691	7,682	6,893	7,505
Panel D. Match score ≥ 0.8					
State 14(c) elimination	0.2510** (0.1160)	-0.0186 (0.0606)	0.1878* (0.1060)	0.0583 (0.0739)	0.0115 (0.0081)
Observations	17,971	7,580	7,571	6,816	7,406
Panel E. Match score ≥ 0.9					
State 14(c) elimination	0.2510** (0.1160)	-0.0190 (0.0606)	0.1877* (0.1057)	0.0575 (0.0741)	0.0115 (0.0081)
Observations	17,971	7,431	7,424	6,695	7,261

Notes: Each panel re-estimates all five columns of Table 2, restricting the 990-linked sample (columns 2-5) to employer-years whose fuzzy name-match score to the IRS Form 990 filer is at or above the stated threshold. Exit (DOL), column (1), is estimated on the full employer panel regardless of 990-match status and is identical across panels by construction; included only so the column structure matches Table 2. Match scores are word-overlap similarity between normalized employer names (Jaccard), the same measure used to construct the 0.5 and 0.9 samples referenced elsewhere. Employer and year fixed effects, standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B3. Selection on Pre-Period Firm Size Among Treated-State Employers

	Survivors	Closers	Difference	<i>p</i> -value
Panel A. Pre-period means, treated states)				
Revenue (\$)	13,073,000	2,169,122	10,904,000	0.012
Total assets (\$)	9,548,659	4,271,066	5,277,594	0.084
14(c) workers	48.06	77.42	−29.36	0.090
Panel B. Probability of 990 exit (linear probability, state FE, robust SE)				
	(1)	(2)	(3)	(4)
	Log revenue	Log assets	Log workers	Revenue + workers
Log pre-period revenue	−0.0669*** (0.0225)			−0.0637*** (0.0217)
Log pre-period assets		−0.0408* (0.0220)		
Log pre-period workers			0.0613** (0.0302)	0.0672** (0.0283)
Observations	89	89	78	78

Notes: “Survivors” and “closers” are defined by 990-confirmed organizational exit, not DOL certificate exit. Panel A reports raw group means and a two-sample *t*-test *p*-value. Panel B regresses an indicator for eventual 990 exit on pre-period log revenue, log total assets, and log DOL-reported 14(c) workers, with state fixed effects and heteroskedasticity-robust standard errors. Sample is treated-state certificate holders with IRS Form 990 linkage in their own pre-period window. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.