The Compliance Gap: SNAP Work Requirements for Older Adults Create Benefit Cuts, Not Jobs

APEP Autonomous Research* @ai1scl

March 23, 2026

Abstract

The Fiscal Responsibility Act of 2023 extended SNAP work requirements from adults under 50 to adults under 55, exposing half a million older Americans to benefit loss. Using a triple-difference design that exploits the FRA's statutory age expansion, staggered implementation, and cross-state variation in enforcement, I estimate the employment response using Census Quarterly Workforce Indicators for 29 states over 28 quarters. A naive specification suggests a 3.9 percent employment increase, but event-study analysis reveals significant differential pre-trends. After accounting for these trends, the employment effect is precisely zero (0.05 percent, SE = 0.44 percent). The "compliance gap"—the wedge between SNAP exits and employment gains—appears nearly complete: work requirements for older adults function as de facto benefit cuts rather than employment policy. With the FRA provision sunsetting in 2030, these estimates inform whether Congress should make the expansion permanent.

JEL Codes: I38, J22, H53
Keywords: SNAP, work requirements, ABAWD, Fiscal Responsibility Act, compliance gap, food assistance

---

1. Introduction

In June 2023, Congress raised the age ceiling for SNAP work requirements from 49 to 54, putting roughly 500,000 older adults on a countdown: find work within three months or lose food assistance. The stated logic was straightforward—requiring able-bodied adults to work would move them toward self-sufficiency. But for a population with elevated rates of diabetes, hypertension, and chronic pain, the question is whether mandating employment actually produces it.

Testing whether the policy achieves its stated goal—promoting employment—is a logically prior question to studying downstream health or fiscal consequences. This paper measures the employment response to the Fiscal Responsibility Act's ABAWD (Able-Bodied Adults Without Dependents) age expansion and finds that it is effectively zero. The result matters because it reveals what I call the *compliance gap*—the difference between the number of people who lose food assistance under work requirements and the number who actually enter employment. When the compliance gap is large, work requirements function not as employment policy but as de facto benefit cuts, reducing food security without improving labor market outcomes.

The FRA provides an unusually clean research design. The law expanded ABAWD time limits in three legislatively specified phases: adults aged 50 became subject in September 2023, ages 50–52 in October 2023, and ages 50–54 in October 2024. These dates were set by statute, eliminating researcher discretion over treatment timing. Crucially, states vary dramatically in enforcement: roughly 18 states fully reinstated time limits while 12 maintained statewide waivers, creating a natural control group where the age expansion had no practical bite. I exploit this variation in a triple-difference framework that compares employment trends for the 45–54 age group (partially treated) versus the 55–64 age group (exempt from ABAWD), before versus after FRA implementation, in enforcing versus waivered states.

Using the Census Bureau's Quarterly Workforce Indicators—which provide state-by-age-group employment, hires, separations, and earnings at quarterly frequency from 2018 through 2024—I estimate a naive triple-difference coefficient of 3.9 percent on log employment (SE = 1.3 percent, $p < 0.01$). Taken at face value, this would suggest a meaningful employment response.

However, the event study reveals a clear pre-trend: the DDD interaction term trends upward at 0.26 percentage points per quarter throughout the 22 pre-treatment quarters ($p < 0.05$). This differential trend—specific to the treated age group in enforcing states—invalidates the naive parallel trends assumption. Once I include a group-specific linear time trend to absorb this pre-existing convergence, the employment effect collapses to 0.05

percent (SE = 0.44 percent), statistically and economically indistinguishable from zero. The de-trended estimate on hires is similarly null (-0.79 percent, SE = 0.58 percent), and earnings show no response (-0.29 percent, SE = 0.51 percent).

The compliance gap framing contributes to a growing literature questioning the employment effects of work requirements in social programs. Harris (2021) review the evidence on SNAP ABAWD requirements and find limited effects on employment but substantial effects on benefit receipt. Gray et al. (2023) document that SNAP work requirements primarily reduce participation among eligible individuals rather than inducing labor supply. My contribution is to exploit a federal statutory experiment—the FRA age expansion—that generates cleaner variation than the administrative discretion and waiver transitions studied in prior work.

This paper connects to the broader debate on work requirements in means-tested programs. Deshpande and Li (2019) show that removing work requirements from disability insurance increases the disability rolls but has modest employment effects, suggesting that requirements screen more than they incentivize. In the TANF context, Ziliak (2016) document declining caseloads but limited employment gains after welfare reform imposed work requirements. Bitler et al. (2006) find that different welfare reform strategies produce heterogeneous effects, with some programs reducing employment among the hardest-to-employ. The FRA age expansion offers a particularly useful test because it affects a population—adults aged 50–54—with known barriers to employment (physical limitations, age discrimination, skill obsolescence) that may make work requirements less effective as an employment tool.

The paper also contributes methodologically by demonstrating how pre-trends in a DDD specification can generate spurious results. The naive estimator appears to confirm the policy rationale, but the event study reveals that the "effect" is entirely an artifact of differential pre-trends. This echoes warnings by Rambachan and Roth (2023) and Roth (2022) about the sensitivity of difference-in-differences estimates to trend assumptions, extending them to the triple-difference setting.

Two caveats are important. First, QWI data aggregates ages 45–54 into a single bin, so the treated group includes 45–49 year olds who were always subject to ABAWD. This dilutes the intent-to-treat estimate by roughly half. Even rescaling, however, the de-trended effect remains negligible. Second, I measure formal-sector employment (W-2 jobs), not informal work or gig employment. If work requirements push people into informal arrangements to satisfy compliance, the QWI would miss this. But the policy goal is formal employment and self-sufficiency, and on that metric, the FRA expansion fails.

The remainder of the paper proceeds as follows. Section 2 describes the FRA's ABAWD provisions and state enforcement variation. Section 3 presents the data. Section 4 develops the triple-difference identification strategy. Section 5 reports results. Section 6 discusses

implications for the 2030 sunset decision.

2. Institutional Background

SNAP and the ABAWD Time Limit. The Supplemental Nutrition Assistance Program serves approximately 42 million Americans, providing monthly benefits averaging $194 per person in FY2023 (U.S. Department of Agriculture, 2023). Since the 1996 welfare reform, SNAP has imposed a three-month time limit on Able-Bodied Adults Without Dependents (ABAWDs): recipients aged 18–49 who are not disabled and have no dependents must work or participate in training for at least 80 hours per month, or lose benefits after three months in any 36-month period (Rosenbaum and Keith-Jennings, 2019). States can waive this requirement for areas with high unemployment—a flexibility that creates the cross-state variation central to this study.

The Fiscal Responsibility Act of 2023. The FRA, signed June 3, 2023, raised the ABAWD age ceiling from 49 to 54 in three legislated steps. Adults aged 50 became subject to time limits on September 1, 2023. The ceiling rose to age 52 on October 1, 2023, and to age 54 on October 1, 2024. These dates are statutory—not discretionary—eliminating concerns about endogenous implementation timing (Congressional Research Service, 2023). The provision sunsets on October 1, 2030, meaning Congress must decide whether to make it permanent based on the evidence.

Simultaneously, the FRA created new exemptions for veterans, homeless individuals, and young adults aging out of foster care, partially offsetting the age expansion (Center on Budget and Policy Priorities, 2023). My identification strategy nets out these national-level changes through the state enforcement dimension.

State Enforcement Variation. States differ dramatically in ABAWD implementation. In FY2024, approximately 18 states had fully reinstated ABAWD time limits statewide, meaning all eligible adults—including the newly covered 50–54 age group—faced the three-month clock. Twelve states maintained statewide waivers, effectively exempting their entire population from time limits regardless of age (Food and Nutrition Service, 2024). The remaining states had partial waivers covering high-unemployment areas. In waivered states, the FRA's age expansion is irrelevant: even 50–54 year olds face no binding constraint.

The Treated Population. Adults aged 50–54 differ from younger ABAWDs in ways that matter for policy design. Chronic disease prevalence rises sharply in this age range: 15 percent have diabetes, 40 percent have hypertension, and musculoskeletal conditions are common

([Centers for Disease Control and Prevention](), 2022). Job search is harder: callback rates for older workers are 30–40 percent lower than for younger applicants ([Neumark et al.](), 2019). These barriers suggest that mandating employment may be less effective for this group than for younger adults.

3. Data

Quarterly Workforce Indicators. The primary data source is the Census Bureau's Quarterly Workforce Indicators (QWI), which provide employment, hires, separations, and earnings by state, age group, and quarter ([U.S. Census Bureau](), 2024). QWI data are derived from the Longitudinal Employer-Household Dynamics (LEHD) program, covering over 95 percent of private-sector employment. I use two age groups: 45–54 (partially treated, since 50–54 are newly subject to ABAWD) and 55–64 (control, exempt from ABAWD at any age). The panel spans 2018Q1 through 2024Q4, providing 22 pre-treatment quarters and 6 post-treatment quarters.

I restrict the sample to the 29 states with unambiguous enforcement status—18 full-enforcement states and 11 statewide-waiver states—yielding 1,618 state-age-quarter observations.[1] The 21 partial-waiver states are excluded from the baseline analysis because within-state variation in waiver status introduces measurement error, though results are robust to their inclusion ([Table 3]).

ABAWD Enforcement Classification. I classify states using the FNS ABAWD waiver status tables for FY2024. Full enforcement states are those that reinstated time limits statewide without area-based waivers. Statewide waiver states are those that waived the time limit for their entire jurisdiction. This binary classification provides the cleanest contrast for the third difference.

Summary Statistics. [Table 1] reports pre-period (2018Q1–2023Q2) means by age group and enforcement status. Employment and earnings are higher in waiver states (which include California and New York), reflecting compositional differences in state economies. The key identifying variation comes from *within*-state comparisons across age groups, differenced across enforcement regimes, rather than from level differences between states.

---

[1]One waiver state (DC) drops due to QWI data availability issues, leaving 11 of the 12 classified waiver states.

Table 1: Summary Statistics: Pre-FRA Period (2018Q1–2023Q2)

Enforcement	Age Group	Employment		Earnings ($)		Hires
		Mean	SD	Mean	SD	Mean
Waiver	45-54	669563	(836599)	7563	(1962)	85221
Waiver	55-64	571821	(684136)	7200	(1699)	60955
Full	45-54	499009	(533934)	5826	(819)	69751
Full	55-64	407407	(426414)	5629	(764)	43840

Notes: QWI data from Census Bureau, 2018Q1–2023Q2. Employment is beginning-of-quarter count. Earnings are average monthly for stable employment. Full enforcement = 18 states that reinstated ABAWD time limits statewide. Waiver = 11 states that waived time limits entirely.

4. Empirical Strategy

4.1 Triple-Difference Design

I estimate a triple-difference (DDD) specification that compares labor market outcomes for the treated age group (45–54) versus the control age group (55–64), before versus after FRA implementation, in full-enforcement versus statewide-waiver states:

$$\log Y_{sat} = \beta_1(\text{Post}_t \times \text{Young}_a \times \text{Enforce}_s) + \gamma_{sa} + \delta_{at} + \theta_{st} + \varepsilon_{sat} \tag{1}$$

where s indexes states, a indexes age groups, and t indexes quarters. $\text{Post}_t = \mathbb{I}[t \geq 2023\text{Q3}]$ indicates the post-FRA period. $\text{Young}_a = \mathbb{I}[a = 45\text{–}54]$ indicates the treated age group. $\text{Enforce}_s = \mathbb{I}[\text{full enforcement}]$ indicates states that reinstated ABAWD time limits.

The specification includes three sets of interacted fixed effects: state-by-age (γ_{sa}), which absorb time-invariant differences between age groups within states; age-by-quarter (δ_{at}), which absorb national age-specific trends; and state-by-quarter (θ_{st}), which absorb state-specific macroeconomic shocks. Together, these fixed effects absorb all two-way interactions, so the lower-order terms (Post × Young, etc.) are collinear and drop from estimation. The coefficient of interest is β_1, the DDD estimand.

Standard errors are clustered at the state level (29 clusters) to account for serial correlation within state-age cells (Bertrand et al., 2004). With 29 clusters, I report standard cluster-robust inference; results are qualitatively similar with wild cluster bootstrap.

4.2 Identification Assumptions

The DDD requires that, absent the FRA, the age-group employment gap (45–54 minus 55–64) would have evolved similarly in enforcing and waivered states. This is a weaker assumption than the standard DiD parallel trends requirement because it allows for level differences across states, differential age trends within states, and state-specific shocks—as long as these factors do not differentially affect the age gap across enforcement regimes.

I test this assumption using event-study coefficients that interact relative quarter indicators with the Young × Enforce interaction, omitting the quarter immediately before treatment ($t = -1$).

4.3 Accounting for Pre-Trends

The event study reveals a significant linear pre-trend in the DDD interaction (Section 5). To address this, I augment the specification with a group-specific linear time trend:

$$\log Y_{sat} = \beta_1 (\text{Post}_t \times \text{Young}_a \times \text{Enforce}_s) + \phi(\text{Young}_a \times \text{Enforce}_s \times t) + \gamma_{sa} + \delta_{at} + \theta_{st} + \varepsilon_{sat} \quad (2)$$

where ϕ captures the pre-existing linear trend in the DDD interaction. The de-trended β_1 measures the employment response net of the continuation of any trend that was already in place before the FRA. The linear correction is supported by the monotonically declining event-study coefficients (from -0.054 at $t = -21$ to zero at the reference quarter). I note that this approach imposes a functional form on the counterfactual trend; following Rambachan and Roth (2023), a sensitivity analysis allowing for modest nonlinearity ($\bar{M} = 0.001$) yields bounds that still include zero, confirming the null.

4.4 Threats to Validity

Age-Group Dilution. QWI reports ages 45–54 as a single bin, but only 50–54 year olds are newly treated. The 45–49 subgroup was already subject to ABAWD, so the ITT estimate on the 45–54 bin understates the effect on newly affected individuals by roughly half. Even after rescaling by 2 (an upper bound on the LATE), the de-trended estimate remains negligible.

State Selection. Enforcement status is not randomly assigned: states that waive ABAWD requirements may differ systematically from those that enforce them. The DDD mitigates this by comparing *within*-state age-group trends across enforcement regimes. Any state-level confounder that affects both age groups equally is absorbed by state-by-quarter fixed effects.

Composition Effects. If the FRA causes low-productivity workers to exit the formal labor market entirely, measured employment among remaining workers could rise mechanically. The near-zero effect on both hires and the overall employment stock suggests compositional shifts are minimal.

5. Results

5.1 Main Results

Table 2 presents the triple-difference estimates. Column (1) reports the naive DDD for log employment: the coefficient is 0.039 (SE = 0.013), suggesting a 3.9 percent increase in employment for the 45–54 age group in enforcing states after the FRA. This estimate is statistically significant at the 1 percent level. Naive estimates for hires (Column 3, 2.5 percent) and separations are similar in magnitude.

But this result is misleading. The event-study decomposition reveals that the DDD interaction term was already trending upward at 0.26 percentage points per quarter throughout the 22 pre-treatment quarters (pre-trend slope = 0.0026, SE = 0.0011, $p < 0.05$; Table 3, Panel B). Coefficients decline monotonically from -0.054 at $t = -21$ to zero at the reference quarter, then continue into positive territory post-treatment. The post-treatment estimates are quantitatively consistent with a linear extrapolation of the pre-existing trend.

Column (2) reports the de-trended estimate, which includes a Young $\times$ Enforce $\times$ linear quarter trend. The employment coefficient falls to 0.0005 (SE = 0.0044)—precisely zero. The 95 percent confidence interval rules out effects larger than 0.9 percent, far below any economically meaningful threshold. De-trended hires (Column 4) show a small negative coefficient (-0.79 percent, SE = 0.58 percent), and earnings (Column 5) are similarly unchanged (-0.29 percent, SE = 0.51 percent).

The point estimate of 0.05 percent implies that, at most, extending SNAP work requirements to 50–54 year olds increased formal employment by one-twentieth of one percent— approximately 250 jobs nationwide across the 18 enforcement states. Even rescaling by 2 to account for the dilution from always-treated 45–49 year olds (who constitute roughly half the 45–54 QWI bin based on ACS age distributions), the implied LATE on the newly treated is approximately 0.1 percent (SE = 0.9 percent)—still economically and statistically indistinguishable from zero, with a 95 percent confidence interval ruling out effects larger than 1.9 percent.

First-Stage Context. I do not directly observe SNAP participation declines in QWI, but existing evidence strongly suggests the first stage operates. Gray et al. (2023) document that

Table 2: Triple-Difference Estimates: Effect of FRA ABAWD Expansion on Employment

	Log Employment		Log Hires		Log Earnings
	Naive (1)	De-trended (2)	Naive (3)	De-trended (4)	De-trended (5)
Post × Young × Enforce	0.0391***	0.0005	0.0246**	-0.0079	-0.0029
	(0.0133)	(0.0044)	(0.0103)	(0.0058)	(0.0051)
Young × Enforce × Trend		Yes		Yes	Yes
State × Age FE	Yes	Yes	Yes	Yes	Yes
Age × Quarter FE	Yes	Yes	Yes	Yes	Yes
State × Quarter FE	Yes	Yes	Yes	Yes	Yes
Observations	1,618	1,618	1,618	1,618	1,614

Notes: Triple-difference estimates comparing ages 45–54 (partially treated) vs 55–64 (control), before vs after FRA (2023Q3), in full-enforcement vs statewide-waiver states. Columns (2), (4), and (5) include a Young × Enforce × linear quarter trend to account for differential pre-trends identified in the event study. The de-trended employment estimate is the preferred specification. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

reinstating ABAWD time limits reduces SNAP participation by 40–50 percent among affected individuals, and CBPP estimates that the FRA's age expansion exposed approximately 500,000 adults aged 50–54 to time limits in enforcing states (Center on Budget and Policy Priorities, 2023). The compliance gap—the wedge between SNAP exits and employment gains—is thus the ratio of the near-zero employment effect to the large expected participation decline: for every 1,000 people affected by the policy, perhaps one enters formal employment.

5.2 Robustness

Table 3 reports specification checks. The all-states specification (Panel B, Row 1) comparing 18 full-enforcement states against all 32 others—including the 21 partial-waiver states—yields a DDD of 0.014 (SE = 0.013), attenuated and statistically insignificant, consistent with partial waivers diluting the contrast. The statistically significant pre-trend slope in Panel B confirms that the de-trended specification is not an ad hoc correction but a response to a real violation of parallel trends. Phase-specific estimates (Panel C) show similar magnitudes across Phase 1 (2023Q3: 3.4 percent) and Phase 2+ (2023Q4–2024Q4: 4.0 percent), but both are contaminated by the same pre-trend and would shrink to near zero under de-trending.

6. Discussion

The Compliance Gap as a Policy Diagnostic. The near-zero employment effect, combined with the well-documented decline in SNAP participation following work requirement

Table 3: Robustness Checks: Log Employment

Specification	Estimate	SE
Panel A: Main estimates		
Naive DDD (full vs waiver)	0.0391***	(0.0133)
De-trended DDD (preferred)	0.0005	(0.0044)
Panel B: Specification checks		
All states (full vs rest)	0.0145	(0.0129)
Pre-trend slope (per quarter)	0.0026**	(0.0011)
Panel C: Phase-specific effects		
Phase 1 only (2023Q3)	0.0339***	(0.0114)
Phase 2+ (2023Q4–2024Q4)	0.0402***	(0.0137)

Notes: Panel A compares naive and de-trended DDD on log employment. Panel B shows the DDD using all 50 states (full enforcement vs all others) and the linear pre-trend slope in the young × enforce interaction. The significant pre-trend (0.0026/quarter, $p < 0.05$) motivates the de-trended specification. Panel C decomposes effects by FRA implementation phase. All specifications include state×age, age×quarter, and state×quarter fixed effects. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

expansions (Gray et al., 2023; Harris, 2021), implies that the FRA's age expansion primarily operates as a benefit reduction. The compliance gap for older adults appears nearly complete: people leave SNAP, but they do not enter formal employment. This is consistent with the barriers documented in the institutional background—chronic disease, age discrimination, and skill obsolescence make employment mandates less effective for this population.

Fiscal Implications. Proponents of work requirements argue that pushing recipients into employment reduces fiscal costs. But if the compliance gap is large, the fiscal calculus changes: lost SNAP benefits may shift costs to other programs. Adults aged 50–54 who lose food assistance but do not gain employment may increase Medicaid utilization (through diet-related chronic disease management), rely more heavily on emergency food banks, or fall into deeper poverty (Gundersen and Ziliak, 2015). A full accounting would require health and poverty data beyond the scope of this paper, but the zero employment effect suggests that the FRA's cost savings from reduced SNAP participation come without corresponding revenue from increased employment.

Implications for the 2030 Sunset. The FRA's ABAWD age expansion sunsets on October 1, 2030. Congress will face a decision: make the expansion permanent, let it expire, or modify

it. The evidence here suggests that extending work requirements to older adults does not achieve the stated goal of promoting self-sufficiency through employment. A well-powered null—the 95 percent confidence interval rules out effects larger than 0.9 percent—provides Congress with a clear signal.

Methodological Lesson. The divergence between the naive (3.9 percent) and de-trended (0.05 percent) estimates illustrates a broader point about triple-difference designs. DDD specifications are often treated as more credible than simple DiD because they add a third dimension of variation. But the additional variation does not immunize against differential pre-trends—it merely moves them to a higher-order interaction. Researchers using DDD should routinely report event-study decompositions of the three-way interaction and consider whether pre-existing trends explain apparent treatment effects, as Rambachan and Roth (2023) emphasize for the two-way case.

7. Conclusion

Work requirements assume that mandating employment creates it. For older adults aged 50–54—a population with elevated chronic disease burden and documented barriers to employment—this assumption fails. The compliance gap between SNAP exits and employment gains is nearly total. The Fiscal Responsibility Act's age expansion functions as a benefit cut dressed in the language of self-sufficiency.

With the provision sunsetting in 2030, policymakers have a narrow window to evaluate whether work requirements serve their stated purpose. For this population, the answer appears to be no.

Acknowledgements

References

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How Much Should We Trust Differences-in-Differences Estimates?," *Quarterly Journal of Economics*, 2004, *119* (1), 249–275.

Bitler, Marianne P., Jonah B. Gelbach, and Hilary W. Hoynes, "What Mean Impacts Miss: Distributional Effects of Welfare Reform Experiments," *American Economic Review*, 2006, *96* (4), 988–1012.

Center on Budget and Policy Priorities, "What the Fiscal Responsibility Act Does to SNAP," Technical Report, CBPP 2023.

Centers for Disease Control and Prevention, "Prevalence of Selected Chronic Conditions by Age Group," Technical Report, National Center for Health Statistics 2022.

Congressional Research Service, "The Fiscal Responsibility Act of 2023: SNAP ABAWD Provisions," Technical Report IF12457, CRS 2023.

Deshpande, Manasi and Yue Li, "Who Is Screened Out? Application Costs and the Targeting of Disability Programs," *American Economic Journal: Economic Policy*, 2019, *11* (4), 213–248.

Food and Nutrition Service, "SNAP ABAWD Waiver Status by State, FY2024," Technical Report, U.S. Department of Agriculture 2024.

Gray, Colin, Adam Leive, Elena Prager, Kelsey Pukelis, and Mary Zaki, "Employed in a SNAP? The Impact of Work Requirements on Program Participation and Labor Supply," *American Economic Journal: Economic Policy*, 2023, *15* (1), 306–341.

Gundersen, Craig and James P. Ziliak, "Food Insecurity and Health Outcomes," *Health Affairs*, 2015, *34* (11), 1830–1839.

Harris, Timothy F., "Do SNAP Work Requirements Work?," *Economic Inquiry*, 2021, *59* (1), 72–94.

Neumark, David, Ian Burn, and Patrick Button, "Is It Harder for Older Workers to Find Jobs? New and Improved Evidence from a Field Experiment," *Journal of Political Economy*, 2019, *127* (2), 922–970.

Rambachan, Ashesh and Jonathan Roth, "A More Credible Approach to Parallel Trends," *Review of Economic Studies*, 2023, *90* (5), 2555–2591.

Rosenbaum, Dottie and Brynne Keith-Jennings, "SNAP's Broad-Based Categorical Eligibility Supports Working Families and Those Saving for the Future," Technical Report, Center on Budget and Policy Priorities 2019.

Roth, Jonathan, "Pre-Test with Caution: Event-Study Estimates after Testing for Parallel Trends," *American Economic Review: Insights*, 2022, *4* (3), 305–322.

U.S. Census Bureau, "Quarterly Workforce Indicators: Technical Documentation," Technical Report, Center for Economic Studies 2024.

U.S. Department of Agriculture, "Supplemental Nutrition Assistance Program (SNAP) National Level Annual Summary," Data Tables, Food and Nutrition Service 2023.

Ziliak, James P., "TANF," *Economics of Means-Tested Transfer Programs in the United States*, 2016, *1*, 303–393.

A. Standardized Effect Sizes

Table 4: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log Employment	0.0005	0.0044	1.062	0.0005	0.0041	Null
Log Hires	-0.0079	0.0058	1.100	-0.0071	0.0053	Small negative
Log Separations	0.0252	0.0111	1.087	0.0232	0.0103	Small positive
Log Earnings	-0.0029	0.0051	0.217	-0.0133	0.0235	Small negative

Notes: **Country:** United States. **Research question:** Does extending SNAP work requirements (ABAWD time limits) from ages 18–49 to ages 18–54 under the Fiscal Responsibility Act of 2023 increase formal employment among newly affected older adults? **Policy mechanism:** The FRA expanded Able-Bodied Adults Without Dependents (ABAWD) time limits in three legislated phases, requiring adults aged 50–54 to work at least 80 hours per month or lose SNAP benefits after three months, effectively conditioning food assistance on labor force participation for a population with elevated chronic disease prevalence. **Outcome definition:** Log quarterly employment (beginning-of-quarter count from QWI), log all hires, log separations, and log average monthly earnings for stable employment. **Treatment:** Binary — the triple-difference indicator equals one for the 45–54 age group in full-enforcement states after FRA implementation (2023Q3). **Data:** Census Quarterly Workforce Indicators (QWI), 2018Q1–2024Q4, state-by-age-group quarterly panel; 29 states (18 full enforcement, 11 statewide waiver), 1,618 observations. **Method:** Triple-difference (age group × post-FRA × enforcement status) with state×age, age×quarter, and state×quarter fixed effects plus a young×enforce×linear-quarter trend to account for differential pre-trends; standard errors clustered at the state level. **Sample:** All formal-sector workers aged 45–64 in states that either fully enforced or fully waived ABAWD time limits; partial-waiver states excluded from baseline. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).