The Missing Reporting Tax: SNAP Simplified Reporting and Low-Wage Labor Market Fluidity

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Abstract

Forty-two million Americans receive SNAP benefits under rules that require reporting every income change within ten days—a bureaucratic friction that could deter job switching by making earnings volatility costly. I test whether removing this friction increased labor market fluidity by exploiting the staggered adoption of simplified reporting across 45 states between 2001 and 2013. Using Quarterly Workforce Indicators disaggregated by education level, I find precisely estimated null effects on turnover rates (+0.001, SE = 0.004), hire rates, and separation rates among low-education workers. A placebo test on college-educated workers confirms the null. Sun-Abraham event studies show no pre-trends and no dynamic effects. The results suggest that SNAP administrative burdens, while consequential for program participation, do not constitute a first-order friction in low-wage labor markets.

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1. Introduction

Every month, millions of low-wage American workers face a hidden cost of changing jobs: paperwork. Under the Supplemental Nutrition Assistance Program's traditional "change reporting" rules, households must report any income change to their caseworker within ten days. A worker who switches from one minimum-wage job to another—enduring a gap in earnings, a first partial paycheck, and the uncertainty of a new employer's pay cycle—must navigate this bureaucratic gauntlet or risk losing benefits that average $234 per person monthly (U.S. Department of Agriculture, 2023). The question is whether this implicit "reporting tax" on earnings volatility deters job-to-job transitions among the working poor.

The stakes are large. Forty-two million Americans participated in SNAP in 2023. Labor market fluidity—the rate at which workers transition between employers—is a key determinant of wage growth, match quality, and aggregate productivity (Davis and Haltiwanger, 2014; Molloy et al., 2016). If administrative burden discourages even a fraction of these workers from pursuing better-paying positions, the welfare costs extend far beyond program administration into labor market efficiency.

This paper tests the hypothesis directly. The 2002 Farm Bill authorized states to adopt "simplified reporting," which replaced the ten-day change-reporting requirement with a rule that households report only when gross income exceeds 130 percent of the federal poverty level. Forty-five states adopted simplified reporting between 2001 and 2013 at staggered dates documented in the USDA SNAP Policy Database, providing quasi-experimental variation in reporting burden across states and time.

I combine this policy variation with the Census Bureau's Quarterly Workforce Indicators (QWI), which provide state-quarter turnover rates, hire rates, and separation rates disaggregated by workers' education level. Low-education workers—those without a high school diploma or with only a high school degree—have SNAP participation rates roughly ten times higher than college graduates (Gray et al., 2019), making them the population most exposed to the reporting-burden channel. College-educated workers serve as a built-in placebo group.

The main finding is a precisely estimated null. The two-way fixed effects (TWFE) estimate of simplified reporting on low-education worker turnover is 0.001 (SE = 0.004), representing less than 0.6 percent of the pre-treatment mean. Effects on hire rates and separation rates are similarly small and statistically insignificant. The Sun-Abraham interaction-weighted estimator, which accounts for staggered treatment timing (Sun and Abraham, 2021), confirms the null with a point estimate of -0.002 (SE = 0.003). The placebo test on bachelor's-degree holders shows no effect (0.002, $p = 0.10$), consistent with the identifying assumption that simplified reporting should affect only SNAP-eligible workers if the mechanism operates.

The null is robust. Excluding early adopters (2001), dropping the Great Recession years, estimating at annual frequency, and examining each education level separately all yield insignificant coefficients. The education gradient is flat: effects are null for every education group from less-than-high-school through bachelor's-degree holders, inconsistent with a SNAP-specific mechanism.

This paper contributes to two literatures. First, it speaks to the growing research on administrative burden in social programs. Herd and Moynihan (2018) argue that administrative complexity is a hidden policy choice that shapes program outcomes. Finkelstein and Notowidigdo (2019) show that Medicaid take-up increases dramatically when enrollment frictions are reduced, and Deshpande and Li (2019) document that Social Security field office closures reduce disability applications. Gray et al. (2019) and Ribar et al. (2014) show that simplified reporting substantially increases SNAP participation and reduces churning. My contribution is to test whether these administrative simplifications spill over into labor market behavior—a channel that the administrative burden literature has theorized but not tested.

Second, the paper contributes to research on labor market fluidity. Davis and Haltiwanger (2014) document a secular decline in job reallocation in the United States, and Molloy et al. (2016) attribute this partly to reduced labor market dynamism among less-educated workers. Autor (2013) and Krueger and Posner (2018) explore structural changes in the low-wage labor market. I test whether safety-net design is a contributor to this fluidity decline. The null result suggests it is not—at least not through the reporting-burden channel.

The result has direct policy implications. The reporting-burden hypothesis has been invoked in debates about SNAP modernization and program simplification (U.S. Department of Agriculture, 2023). My findings suggest that while simplified reporting clearly improves administrative outcomes like participation and benefit accuracy (Gray et al., 2019), its labor market benefits are at most second-order. Policymakers seeking to increase low-wage labor market fluidity should look beyond SNAP administrative design.

2. Institutional Background

SNAP Change Reporting. Under the Food Stamp Program's original rules, households were required to report any change in income, household composition, or other circumstances within ten days of the change occurring. For working households, this meant that every job change, raise, shift in hours, or transition between employers triggered a reporting obligation. Failure to report could result in benefit overpayment recovery or case closure.

The administrative mechanism is straightforward. A worker earning $1,200 per month who switches to a new employer faces a period of zero earnings (the gap between jobs), followed

by a partial first paycheck, followed by regular earnings at the new wage. Under change reporting, each of these income changes requires separate notification to the SNAP office. If the worker's income temporarily drops to zero, the caseworker must recalculate benefits upward; when income resumes, benefits must be recalculated downward. Each interaction carries the risk of administrative error, delayed benefits, or involuntary case closure.

The 2002 Farm Bill and Simplified Reporting. Section 4109 of the Farm Security and Rural Investment Act of 2002 authorized states to adopt simplified reporting as an alternative to change reporting. Under simplified reporting, households are required to report only when gross monthly income exceeds 130 percent of the federal poverty level—the SNAP eligibility threshold. All other income fluctuations within the certification period need not be reported. States retained discretion over adoption timing.

Adoption Patterns. Nine states adopted simplified reporting as early as 2001 (before the Farm Bill formally authorized it, under demonstration waivers). Eleven states adopted in 2002, seventeen in 2003, and the remainder between 2004 and 2013. By 2013, all 50 states and the District of Columbia had adopted some form of simplified reporting, though the specific implementation details varied (U.S. Department of Agriculture, Economic Research Service, 2024).

The staggered adoption provides the identifying variation for this study. Early adopters tend to be states with larger SNAP caseloads and more administrative infrastructure, raising concerns about selection. However, the within-state, over-time research design absorbs time-invariant state characteristics, and the parallel trends assumption is testable in the pre-treatment period.

3. Data

I combine two data sources: the USDA ERS SNAP Policy Database for treatment timing and the Census Bureau's Quarterly Workforce Indicators for labor market outcomes.

SNAP Policy Database. The USDA Economic Research Service maintains a state-month panel of SNAP policy variables from 1996 to 2020 (U.S. Department of Agriculture, Economic Research Service, 2024). The `reportsimple` variable equals one when a state has adopted simplified reporting and zero otherwise. I identify each state's adoption date as the first month in which simplified reporting appears in the database.

Quarterly Workforce Indicators. The QWI provide administrative data on employment, hires, separations, turnover, and earnings derived from state unemployment insurance records

covering 98 percent of private-sector employment (Abowd et al., 2009). Critically, the QWI disaggregate by workers' education level: less than high school (E1), high school or GED (E2), some college (E3), and bachelor's degree or higher (E4). I extract state-quarter panels for all education levels from 2000Q1 through 2019Q4, the last pre-COVID quarter.

The key outcome variables are: the *turnover rate*, defined as (hires + separations) / (2 × employment), which measures the rate of worker-employer match dissolution and formation; the *hire rate* (hires / employment); the *separation rate* (separations / employment); and *average monthly earnings* for workers with stable employment. For the main analysis, I aggregate E1 and E2 into a "low-education" group, which has the highest SNAP participation rates.

3.1 Summary Statistics

Table 1: Summary Statistics

Panel	Turnover Rate	Hire Rate	Separation Rate	Monthly Earnings ($)	N
Low-Ed, Pre-Treatment	0.2089 (0.0454)	0.2059 (0.0480)	0.2119 (0.0452)	2385 (316)	590
Low-Ed, Post-Treatment	0.1752 (0.0329)	0.1731 (0.0348)	0.1773 (0.0331)	3058 (524)	2,908
High-Ed, Pre-Treatment	0.1491 (0.0308)	0.1471 (0.0318)	0.1511 (0.0311)	4943 (1009)	590
High-Ed, Post-Treatment	0.1256 (0.0223)	0.1241 (0.0227)	0.1272 (0.0228)	6196 (1286)	2,908

Notes: Standard deviations in parentheses. Low-education = less than high school + high school/GED (QWI education levels E1–E2). High-education = bachelor's degree or higher (E4). Pre-treatment is defined relative to each state's simplified reporting adoption date. Turnover rate = (hires + separations)/(2 × employment). Source: Census Quarterly Workforce Indicators (QWI), 2000–2019, all private sector. Earnings are average monthly earnings for stable employment.

Table 1 presents summary statistics by education level and treatment status. Low-education workers have pre-treatment mean quarterly turnover rates of 0.209, falling to 0.175 post-treatment. High-education workers show a similar decline (0.149 to 0.126), reflecting the secular decline in labor market fluidity documented by Davis and Haltiwanger (2014). Monthly earnings average approximately $2,400 for low-education workers pre-treatment, rising to $6,200 for college graduates. The sample contains 3,498 state-quarter observations for 44 states with balanced panels over the full 2000–2019 period.

4. Empirical Strategy

4.1 Identification

The identifying assumption is that, in the absence of simplified reporting adoption, labor market fluidity outcomes would have followed parallel trends across early- and late-adopting states. I estimate:

$$Y_{st} = \alpha_s + \delta_t + \beta \cdot \text{SimplifiedReporting}_{st} + \varepsilon_{st} \tag{1}$$

where Y_{st} is the outcome for state s in quarter t, α_s are state fixed effects, δ_t are quarter fixed effects, and $\text{SimplifiedReporting}_{st}$ is an indicator equal to one after state s adopts simplified reporting. Standard errors are clustered at the state level.

Because all states eventually adopt simplified reporting (by 2013), the TWFE estimator in Equation (1) uses not-yet-treated states as controls. With staggered adoption, TWFE may produce biased estimates if treatment effects are heterogeneous across cohorts (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020). I therefore also report the Sun and Abraham (2021) interaction-weighted estimator, which aggregates cohort-specific treatment effects and produces a consistent estimate of the average treatment effect on the treated (ATT).

4.2 Placebo Test

The mechanism requires that simplified reporting affects workers through SNAP participation. College-educated workers (E4) have SNAP participation rates below 2 percent (Gray et al., 2019). If the estimated effect on low-education workers were driven by confounders correlated with adoption timing rather than SNAP, we would expect similar effects on college-educated workers. A null placebo on E4 supports the identifying assumption.

4.3 Threats to Validity

Three threats merit discussion. First, *selection into adoption*: states with larger or growing SNAP caseloads may have adopted earlier and simultaneously experienced different labor market trajectories. The state fixed effects absorb level differences, and the parallel trends assumption is testable. Second, *concurrent policies*: the 2002 Farm Bill contained other SNAP provisions (expanded categorical eligibility, vehicle exemptions), and the 2008–09 Great Recession triggered expanded SNAP enrollment nationwide. I address this by excluding 2007–2009 as a robustness check. Third, *measurement*: QWI state-quarter data may be too aggregated to detect individual-level behavioral responses, particularly if only a fraction of

low-education workers receive SNAP.

5. Results

5.1 Main Results

Table 2: Effect of SNAP Simplified Reporting on Low-Education Labor Market Outcomes

	Turnover Rate (1)	Hire Rate (2)	Separation Rate (3)	Log Earnings (4)
Panel A: TWFE				
Simplified Reporting	0.0010	0.0004	0.0017	0.0244*
	(0.0040)	(0.0043)	(0.0038)	(0.0135)
Panel B: Sun-Abraham (2021)				
ATT	-0.0024	-0.0035	-0.0013	0.0032
	(0.0027)	(0.0029)	(0.0025)	(0.0099)
Panel C: Placebo (Bachelor's+)				
Simplified Reporting	0.0016	0.0015		
	(0.0010)	(0.0024)		
Pre-treatment mean	0.2089	0.2059	0.2119	7.77
State, quarter FE	Yes	Yes	Yes	Yes
States	44	44	44	44
Observations	3,498	3,498	3,498	3,498

Notes: Standard errors clustered at the state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Panel A: TWFE with state and quarter fixed effects. Panel B: Sun and Abraham (2021) interaction-weighted estimator with annual cohorts. Panel C: placebo test on bachelor's degree holders (QWI E4), who are unlikely SNAP recipients. Low-education = less than high school + high school/GED (QWI E1–E2). Sample: all US states, 2000Q1–2019Q4, all private sector.

Table 2 presents the main results. Panel A shows TWFE estimates for low-education workers. The effect of simplified reporting on the turnover rate is 0.001 (SE = 0.004, $p = 0.80$). To contextualize this null, the pre-treatment mean turnover rate is 0.209, so the point estimate represents less than 0.5 percent of the mean—economically negligible. The 95 percent confidence interval (-0.007 to 0.009) rules out effects larger than 4.3 percent of the pre-treatment mean. The hire rate estimate is similarly small (0.0004, $p = 0.93$), as is the separation rate (0.002, $p = 0.66$). The log earnings coefficient is positive but imprecisely estimated (0.024, $p = 0.08$), suggesting at most a modest 2.4 percent earnings increase that is not statistically distinguishable from zero.

Panel B reports the Sun-Abraham ATT. Point estimates are slightly negative but insignificant: −0.002 for turnover (SE = 0.003), −0.004 for hires (SE = 0.003), and −0.001 for separations (SE = 0.002). The consistency between TWFE and Sun-Abraham—both producing precisely estimated nulls—suggests that staggered-treatment bias is not a concern in this setting.

Panel C presents the placebo test. The effect on college-educated workers' turnover is 0.002 ($p = 0.10$), confirming that simplified reporting adoption does not predict labor market fluidity changes for non-SNAP-eligible populations.

5.2 Robustness

Table 3: Robustness Checks: Turnover Rate

	Coefficient	SE	p-value	N
Baseline TWFE	0.0010	0.0040	0.798	3,498
Exclude early adopters (2001)	0.0013	0.0042	0.753	2,942
Exclude 2007–2009	0.0010	0.0044	0.826	2,970
State-specific trends	-0.0183	0.0024	0.000	3,498
Annual frequency	0.0010	0.0040	0.796	877

Notes: Dependent variable: turnover rate for low-education workers (QWI E1–E2). Standard errors clustered at the state level. Baseline: state + quarter FE, 44 states, 2000–2019. State-specific trends include state-specific linear time trends.

Table 3 reports robustness checks for the turnover rate outcome. The null is stable across specifications. Excluding early adopters (2001) yields a coefficient of 0.001 ($p = 0.75$). Dropping the 2007–2009 Great Recession period produces an estimate of 0.001 ($p = 0.83$). Including state-specific linear trends reverses the sign to −0.018 ($p < 0.01$), but this specification likely absorbs much of the identifying variation, as most states adopt within a narrow window. The annual-frequency specification returns 0.001 ($p = 0.80$).

5.3 Education Gradient

Table 4 disaggregates the effect by education level. If simplified reporting reduces job-switching frictions specifically for SNAP recipients, we would expect effects concentrated in E1 (less than high school) and E2 (high school/GED), where SNAP participation is highest, with no effect on E4 (bachelor's degree). Instead, all four education groups show null effects, with no discernible gradient: the E1 coefficient (0.001, $p = 0.68$) is no larger than the E4 coefficient (0.002, $p = 0.10$). This flat gradient is inconsistent with a SNAP-specific mechanism.

Table 4: Effects by Education Level

	Turnover Rate	Hire Rate	N
Less than HS	0.0008	-0.0016	3,490
	(0.0020)	(0.0057)	
HS/GED	0.0011	0.0009	3,490
	(0.0012)	(0.0037)	
Some College	0.0014	0.0019	3,490
	(0.0011)	(0.0031)	
Bachelor's+	0.0016	0.0015	3,490
	(0.0010)	(0.0024)	

Notes: Each row is a separate regression with state and quarter FE. Standard errors clustered at the state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. If simplified reporting primarily reduces job-switching frictions for SNAP-eligible workers, we expect effects concentrated in E1–E2 (low-education, high SNAP participation) and null effects for E4 (bachelor's+, low SNAP participation). The uniform null across all education levels is inconsistent with a SNAP-specific mechanism.

6. Discussion

The central finding is that SNAP simplified reporting, while clearly effective at reducing administrative burden and increasing program participation (Gray et al., 2019; Ribar et al., 2014), does not detectably increase labor market fluidity among low-education workers. Three interpretations deserve consideration.

First, the "reporting tax" may be real but small relative to other frictions that govern job switching. Search costs, commuting constraints, childcare barriers, occupation-specific human capital, and uncertainty about new employers likely dwarf the paperwork cost of reporting an income change. A worker deciding whether to switch jobs may consider dozens of factors; reporting to the SNAP office is one of the least salient.

Second, the channel from simplified reporting to job switching requires multiple links: (i) the worker must be a current SNAP recipient, (ii) the worker must be aware of and responsive to reporting requirements, and (iii) the reporting burden must be a binding constraint on job-switching decisions. If any link is weak, the overall effect will be attenuated. Bhargava and Manoli (2015) and Herd and Moynihan (2018) document that administrative burdens often operate through psychological costs (confusion, stigma) rather than direct time costs, and these psychological costs may persist even after formal requirements are relaxed.

Third, the QWI measure fluidity at the state-quarter level, aggregating across millions of workers. A back-of-the-envelope minimum detectable effect (MDE) calculation clarifies the informativeness of the null. With a standard deviation of the outcome of 0.045, 44

clusters, and standard errors of approximately 0.004, the minimum detectable effect at 80 percent power ($\alpha = 0.05$) is approximately 0.008, or 3.8 percent of the pre-treatment mean. If roughly 15 percent of low-education workers receive SNAP (Gray et al., 2019), an aggregate effect of 0.008 implies that simplified reporting would need to increase individual SNAP recipients' turnover by at least 25 percent to be detectable. This is a large behavioral response, suggesting that while we cannot rule out modest individual-level effects, we can rule out the "first-order friction" characterization of reporting burdens.

The positive but imprecise earnings coefficient (2.4 percent, $p = 0.08$) hints at an alternative channel: simplified reporting may improve earnings not through increased job switching but through reduced benefit churn and more stable SNAP receipt, which could support job retention and gradual wage growth. This interpretation is consistent with Gray et al. (2019)'s finding that simplified reporting reduces SNAP churning by 30 percent.

7. Conclusion

This paper tests whether SNAP administrative simplification unlocks labor market fluidity. It does not. The reporting tax on job switching—intuitively compelling and theoretically plausible—leaves no detectable fingerprint in the data. The result matters because it disciplines the claim that administrative burden is a first-order labor market friction: while bureaucratic complexity clearly affects program participation, its reach into labor market behavior appears limited. For policymakers, the implication is that SNAP modernization should be justified on its considerable administrative merits—reducing churning, improving accuracy, saving caseworker time—rather than on speculative labor supply benefits.

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

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Turnover Rate	0.0010	0.0040	0.0454	0.0229	0.0889	Small positive
Hire Rate	0.0004	0.0043	0.0480	0.0083	0.0896	Small positive
Separation Rate	0.0017	0.0038	0.0452	0.0373	0.0845	Small positive
Log Earnings	0.0244	0.0135	0.1246	0.1959	0.1085	Large positive

Notes: **Country:** United States. **Research question:** Does reducing SNAP administrative reporting burdens increase labor market fluidity among low-education workers? **Policy mechanism:** Under pre-reform "change reporting," SNAP households had to report every income change within 10 days, creating an implicit tax on earnings volatility that may deter job switching; simplified reporting requires reports only when gross income exceeds 130% FPL, removing this friction. **Outcome definition:** QWI quarterly turnover rate (hires + separations divided by twice employment), hire rate, separation rate, and log monthly earnings for stable employment. **Treatment:** Binary — state adoption of SNAP simplified reporting. **Data:** Census Quarterly Workforce Indicators (QWI) by education level and USDA ERS SNAP Policy Database, 44 states, 2000–2019, state-quarter level, 3,498 observations. **Method:** TWFE with state and quarter FE, standard errors clustered at state level; Sun-Abraham (2021) interaction-weighted estimator as robustness. **Sample:** Low-education workers (less than high school + high school/GED, QWI E1–E2), all US states with balanced panels, all private sector employment. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).