Feeding Reentry: SNAP Drug Felon Ban Rollbacks and the Income Effect on Low-Education Employment

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Abstract

The 1996 Personal Responsibility and Work Opportunity Reconciliation Act permanently banned individuals with drug felony convictions from the Supplemental Nutrition Assistance Program. Between 2015 and 2019, eighteen U.S. states rolled back this ban. I exploit this staggered policy variation to estimate effects on formal employment using Quarterly Workforce Indicators data on 48 states over 2010–2022. Difference-in-differences estimates reveal a small negative effect on low-education employment (-2.5 percentage points for workers without a high school diploma, $p = 0.099$), concentrated in construction (-7.4pp, $p = 0.044$) and in states that fully removed the ban. High-education workers—a placebo group unlikely affected by the policy—show precisely estimated null effects. Pre-treatment trends are flat. These results are consistent with a modest income effect: restoring food assistance reduces labor supply among the most marginally attached workers rather than facilitating job search.

JEL Codes: I38, J22, K42
Keywords: SNAP, drug felony ban, employment, income effect, reentry, safety net

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1. Introduction

Every year, roughly 600,000 people leave U.S. prisons and attempt to rebuild their lives (Carson, 2020). A central challenge they face is securing stable employment—a factor consistently linked to lower recidivism (Western, 2018; Mueller-Smith, 2015). Yet the same welfare reform legislation that reshaped American social policy in the 1990s erected a peculiar barrier: the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) imposed a lifetime ban on Supplemental Nutrition Assistance Program (SNAP) benefits for anyone convicted of a drug felony (McCarty et al., 2016). No other class of felon faced this categorical exclusion from food assistance. The policy created a natural tension. On one hand, denying food assistance might compel job search out of sheer necessity. On the other, food insecurity could undermine the cognitive bandwidth and physical stability needed to find and maintain work (Mullainathan and Shafir, 2013).

This theoretical ambiguity—whether safety net access helps or hurts formal employment among formerly incarcerated individuals—has remained empirically unresolved. Tuttle (2019) provided the first causal evidence, showing that the original SNAP ban *reduced* employment and increased recidivism, suggesting the job search channel dominates. But her analysis exploited the ban's imposition, not its removal. The relevant policy margin today is restoration: as states have increasingly opted to roll back the ban, understanding whether this reversal produces symmetric effects is critical for the 30-plus states still enforcing some version of the restriction.

This paper estimates the employment effect of SNAP eligibility restoration for drug felons, exploiting staggered rollbacks across eighteen states between 2015 and 2019. I use Quarterly Workforce Indicators (QWI) from the Census Bureau's Longitudinal Employer-Household Dynamics program, constructing state-quarter panels by education level for five high-reentry industries. The identification strategy compares employment trends in rollback states to thirty states that had already opted out of the ban before 2010, using high-education workers (some college and above) as a within-unit placebo group. Because drug felony convictions are overwhelmingly concentrated among individuals without postsecondary education (Harlow, 2003), any effect should appear among low-education workers and be absent for higher-education groups.

The main finding is a small negative effect on low-education employment, inconsistent with the optimistic view that SNAP restoration would boost formal labor market outcomes. Workers without a high school diploma experience a 2.5 log-point decline in employment ($p = 0.099$), while the high-school-diploma group shows a smaller, statistically insignificant decline of 1.2 log points. The combined low-education estimate is -1.8 log points ($p = 0.204$).

High-education workers—the placebo group—show a precisely estimated null (+0.5 log points, $p = 0.669$), supporting the identifying assumption that the policy operates through the targeted population rather than through correlated state-level shocks.

Industry-level estimates reveal that the aggregate effect is driven almost entirely by construction (NAICS 23), where low-education employment falls by 7.4 log points ($p = 0.044$). This is consistent with construction's reliance on day-labor and informal arrangements that allow rapid transitions between formal and informal work (Apel and Sweeten, 2010). Retail, administrative support, healthcare, and accommodation/food services show small, statistically insignificant effects.

The effect is concentrated among states that fully removed the ban (-3.1 log points, $p = 0.078$) rather than those that adopted partial modifications (-0.5 log points, $p = 0.807$), consistent with a dose-response pattern. Pre-treatment event-study coefficients are uniformly flat and insignificant, with the largest pre-trend coefficient reaching only $p = 0.489$, alleviating concerns about differential trends.

This paper contributes to several literatures. First, it adds to the growing body of work on how safety net programs affect labor supply, particularly among disadvantaged populations. East (2020) finds modest negative labor supply effects of SNAP benefit increases, while Ganong and Liebman (2018) documents that most SNAP recipients are already working. My finding of a small negative employment effect from eligibility restoration aligns with the standard income effect prediction and with East's estimates, but contrasts with the larger positive effects implied by Tuttle (2019)'s analysis of the ban's imposition.

Second, the paper speaks to the criminal justice and reentry literature. Yang (2017) and Doleac and Hansen (2020) study how labor market access policies (ban-the-box) affect formerly incarcerated individuals, finding that removing barriers can have unintended consequences depending on employer responses. This paper examines a different margin—food assistance rather than information disclosure—and finds that the mechanism runs through labor supply rather than labor demand.

Third, the paper demonstrates the value of "hard null" results in policy evaluation (Abadie, 2020). The finding that SNAP restoration does not boost employment is itself informative: it rules out the claim that food assistance serves as a springboard to formal work, and suggests that policymakers should justify the policy on humanitarian rather than employment grounds. This echoes Hoynes et al. (2016)'s finding that food stamps improve health outcomes even when labor market effects are modest, and Homonoff and Somerville (2021)'s evidence that SNAP recertification barriers harm food security without meaningfully affecting work.

2. Institutional Background

The PRWORA Drug Felon Ban. Section 115 of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act imposed a permanent, lifetime ban on SNAP and Temporary Assistance for Needy Families (TANF) benefits for individuals convicted of a state or federal drug felony (McCarty et al., 2016). The provision was added as a floor amendment with minimal debate and applied categorically: it covered all drug felonies—from simple possession to distribution—regardless of sentence length, time since conviction, or evidence of rehabilitation. No other category of criminal offense triggered a comparable exclusion from federal nutrition assistance.

State Opt-Out Authority. PRWORA granted states the authority to modify or opt out of the ban entirely. By 2010, approximately 25–30 states had exercised this authority, either eliminating the ban or imposing time-limited restrictions with compliance requirements (such as completing drug treatment). These early opt-out states serve as the control group in this analysis, as their policy environment was stable throughout the study period.

The 2015–2019 Rollback Wave. Between 2015 and 2019, eighteen additional states modified their bans. Some states fully repealed the restriction (e.g., Alabama, Texas, Indiana), while others adopted partial modifications—typically requiring completion of a drug treatment program or imposing a waiting period after release. This staggered adoption provides the identifying variation for the difference-in-differences design. The rollback wave was driven by a combination of bipartisan criminal justice reform sentiment, fiscal pressures from rising incarceration costs, and advocacy by reentry organizations (Petersilia, 2003).

The Affected Population. Individuals with drug felony convictions are disproportionately male, non-white, and low-education (Harlow, 2003). Over 60 percent lack a high school diploma or GED, and an additional 25 percent have at most a high school credential. Upon release, they are concentrated in a handful of industries—construction, food service, temporary staffing, retail, and healthcare support—that are relatively tolerant of criminal histories (Apel and Sweeten, 2010). Average SNAP benefits for a single individual are approximately $200 per month, a non-trivial income supplement for individuals earning near the minimum wage in these sectors.

Theoretical Predictions. Restoring SNAP eligibility creates two competing effects on labor supply. The *income effect* predicts reduced work effort: SNAP benefits increase non-labor income, lowering the marginal utility of the additional consumption that a marginal hour of work provides. The *job search facilitation* channel predicts increased employment: food

security reduces the cognitive and physical toll of hunger, enabling more effective job search and reducing the need for informal or illegal income sources (Tuttle, 2019). The net effect is theoretically ambiguous and must be resolved empirically.

3. Data

The primary data source is the Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides administrative employment counts, hires, separations, and average earnings by state, quarter, industry (NAICS sector), and worker demographics including sex and education level. I use the sex-by-education tabulations (QWI-SE) crossed with NAICS sectors.

Sample Construction. I restrict the sample to 48 states (excluding states with incomplete QWI coverage) over the period 2010Q1–2022Q4, yielding 52 quarters. I focus on five NAICS sectors with high shares of formerly incarcerated workers: Construction (23), Retail Trade (44–45), Administrative and Support Services (56), Healthcare and Social Assistance (62), and Accommodation and Food Services (72). Employment is aggregated across sexes within each state-quarter-education cell, and the outcome variable is log quarterly employment (beginning-of-quarter count).

Education Groups. The QWI reports four education categories: E1 (less than high school), E2 (high school diploma or GED), E3 (some college or associate's degree), and E4 (bachelor's degree or higher). I classify E1 and E2 as "low education" and E3 and E4 as "high education." The low-education group captures the population most likely affected by SNAP drug felon ban rollbacks, given that over 85 percent of drug felons have at most a high school credential. The high-education group serves as a placebo.

Treatment Assignment. I code each state as treated in the quarter when its ban modification took effect, using legislative records compiled from the National Conference of State Legislatures and the Vera Institute of Justice. Eighteen states rolled back their bans between 2015 and 2019. I further distinguish between full removals (the ban was entirely repealed) and partial modifications (the ban was relaxed but not eliminated, e.g., requiring drug treatment completion). Thirty states that opted out of the ban before 2010 serve as always-treated controls; their policy was stable throughout the sample period.

Summary Statistics. Table 1 presents means and standard deviations of the key variables by treatment status and education level. Treated and control states have broadly similar employment levels, with somewhat lower earnings in treated states. The final sample comprises

9,860 state-quarter-education observations.

Table 1: Summary Statistics by Treatment Status and Education Level

Group	Education	Mean Emp	SD Emp	Mean Hires	Mean Earnings	N
Treated	Low (E1-E2)	423,259	499,240	92,387	4	1,812
Treated	High (E3-E4)	482,828	523,093	87,164	4	1,812
Control	Low (E1-E2)	431,869	437,960	86,097	3	3,118
Control	High (E3-E4)	553,919	512,892	91,671	3	3,118

Notes: QWI state-quarter-education aggregates for five NAICS sectors (23, 44-45, 56, 62, 72) across 48 states, 2010Q1–2022Q4. Treated: 18 states that rolled back the drug felon SNAP ban 2015–2019. Control: 30 states that opted out pre-2010. Low education: less than high school or high school/GED. High education: some college or bachelor's+. Earnings are average quarterly earnings per worker ($000s).

4. Empirical Strategy

4.1 Identification

The identification strategy exploits the staggered rollback of SNAP drug felon bans across eighteen states between 2015 and 2019. I estimate a difference-in-differences (DiD) specification comparing employment trends in rollback states to states that had already opted out of the ban before 2010. The key identifying assumption is parallel trends: absent the rollback, employment in treated states would have evolved in parallel with employment in control states, conditional on state and time fixed effects.

Two features of the setting strengthen identification. First, the control group consists of states that *already removed the ban*, not states that retained it. This avoids contamination from unobserved policy changes in ban-retaining states and ensures that control states experienced no contemporaneous change in SNAP felony policy. Second, the education dimension provides a within-unit placebo. Drug felony convictions are overwhelmingly concentrated among low-education individuals; high-education workers (some college, bachelor's or above) should be unaffected by the policy change. A null effect in the high-education group supports the claim that estimated effects among low-education workers reflect the policy rather than correlated state-level shocks.

4.2 Estimation

For each education group e, I estimate:

$$Y_{s,t}^e = \alpha + \beta^e \left(\text{Treated}_s \times \text{Post}_{s,t}\right) + \gamma_s + \delta_t + \varepsilon_{s,t} \qquad (1)$$

where $Y_{s,t}^e$ is log employment for education group e in state s and quarter t; Treated_s indicates that state s rolled back its ban during 2015–2019; $\text{Post}_{s,t}$ equals one in and after the quarter of rollback (state-specific timing); γ_s and δ_t are state and year-quarter fixed effects. Standard errors are clustered at the state level, the unit of treatment assignment, providing 48 clusters.

The coefficient of interest, β^e, measures the average post-treatment change in log employment in rollback states relative to control states, for education group e. Under the identifying assumption, β^e for low-education groups captures the causal effect of SNAP eligibility restoration on formal employment.

I also estimate a combined specification pooling education groups:

$$Y_{s,t}^e = \alpha + \beta \left(\text{Treated}_s \times \text{LowEd}_e \times \text{Post}_{s,t} \right) + \text{Two-way interactions} + \gamma_s + \delta_t + \varepsilon_{s,t} \quad (2)$$

which directly estimates the triple-difference: the differential effect of the rollback on low-versus high-education employment.

4.3 Threats to Validity

Parallel Trends. The central concern is that states rolling back their bans in 2015–2019 may have been on different employment trajectories than early opt-out states. I address this with an event-study specification that replaces $\text{Post}_{s,t}$ with year-by-year dummies interacted with treatment status, using 2010 as the reference year. If pre-treatment coefficients are close to zero and statistically insignificant, the parallel trends assumption is supported.

Compositional Confounds. The rollback wave coincided with broader criminal justice reform (e.g., sentencing reform, ban-the-box legislation). To the extent that these reforms affected all education groups similarly within a state, the education-based placebo test absorbs them. However, if companion reforms disproportionately affected low-education workers, the estimate would be biased. I note that most ban-the-box policies were adopted in different states and time periods than the SNAP rollbacks.

Statistical Power. With 48 states (18 treated), the effective sample for state-clustered inference is moderate. The estimates are noisy, and the main low-education result ($p = 0.099$ for the less-than-high-school group) should be interpreted as suggestive rather than definitive.

5. Results

5.1 Main Results

Table 2 presents the core difference-in-differences estimates. Each column reports $\hat{\beta}^e$ from Equation (1) for a different education group or education aggregate.

Table 2: Effect of SNAP Drug Felon Ban Rollback on Log Employment

	(1) <HS	(2) HS/GED	(3) Some Coll.	(4) BA+	(5) Low Ed	(6) High Ed
Treated × Post	-0.0253*	-0.0115	-0.0038	0.0134	-0.0184	0.0048
	(0.0150)	(0.0156)	(0.0117)	(0.0114)	(0.0143)	(0.0111)
Observations	2,465	2,465	2,465	2,465	4,930	4,930
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column estimates a separate DiD regression of log employment on the interaction of treatment status (state rolled back drug felon SNAP ban) and post-treatment indicator, with state and year-quarter fixed effects. Standard errors clustered at the state level (48 states). Columns (1)–(4) estimate effects by education group. Columns (5)–(6) combine education groups: Low = E1 (<HS) + E2 (HS/GED); High = E3 (some college) + E4 (BA+). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The most affected group is workers without a high school diploma (Column 1): log employment falls by 2.5 log points ($p = 0.099$) following the rollback. Workers with a high school diploma or GED (Column 2) show a smaller decline of 1.2 log points that is not statistically significant ($p = 0.465$). The combined low-education estimate (Column 5) is -1.8 log points ($p = 0.204$).

Crucially, the placebo groups show no effect. Workers with some college (Column 3) experience a negligible -0.4 log-point change ($p = 0.746$), and workers with a bachelor's degree or higher (Column 4) show a small positive estimate of $+1.3$ log points ($p = 0.245$). The combined high-education estimate (Column 6) is $+0.5$ log points ($p = 0.669$). The contrast between low- and high-education groups—negative and monotonically declining with education for the low-education groups, null or positive for high-education groups—is consistent with the policy operating through the targeted population.

The magnitudes are economically modest. A 2.5 log-point decline in less-than-high-school employment corresponds to roughly a 2.5 percent reduction. Given that drug felons constitute a small share of the total low-education workforce—Bureau of Justice Statistics data suggest roughly 1–3 percent of the working-age population has a drug felony conviction (Carson, 2020)—the per-capita effect among the directly treated population is likely an order of

magnitude larger. However, this intent-to-treat estimate cannot be disaggregated further with state-level QWI data. Wild cluster bootstrap inference yields similar p-values, and we note that the borderline significance ($p = 0.099$ for E1) should be interpreted as suggestive evidence rather than a definitive rejection of zero.

5.2 Industry Heterogeneity

Table 3 disaggregates the low-education employment effect across the five NAICS sectors.

Table 3: Industry Heterogeneity: Low-Education Employment Effects

Industry	NAICS	Coef.	SE	p-value	N
Construction	23	-0.0741**	(0.0360)	0.044	2,725
Retail	44-45	-0.0153	(0.0159)	0.339	2,725
Admin/Support	56	0.0114	(0.0188)	0.546	2,725
Healthcare	62	-0.0138	(0.0233)	0.556	2,725
Accommodation	72	-0.0088	(0.0143)	0.543	2,725

Notes: Each row estimates a separate DiD for low-education workers (E1–E2) in one NAICS sector. Dependent variable: log employment. State and year-quarter FE. Standard errors clustered at state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Construction stands out with a large and statistically significant negative effect of -7.4 log points ($p = 0.044$). This is the only sector where the estimate is significant at the 5 percent level. The construction result is consistent with the sector's distinctive labor market features: high rates of day labor and informal employment, strong seasonality, and the ability to transition quickly between formal and off-the-books work (Apel and Sweeten, 2010). When food assistance reduces the urgency of securing formal employment, workers in construction—where informal alternatives are readily available—may be the first to adjust their labor supply downward.

The remaining sectors show small, statistically insignificant effects. Retail trade (-1.5 log points, $p = 0.339$) and healthcare (-1.4 log points, $p = 0.556$) have mildly negative point estimates, while administrative/support services shows a small positive estimate ($+1.1$ log points, $p = 0.546$). Accommodation/food services is near zero (-0.9 log points, $p = 0.543$). The concentration of effects in construction, rather than across all low-skill sectors, suggests that the income effect operates most strongly where the formal-informal margin is most elastic.

5.3 Robustness

Table 4 presents three sets of robustness checks.

Table 4: Robustness: Placebo, Ban Type, and Pre-Trends

	Coef.	SE	*p*-value	N
Panel A: Placebo (High-Education Workers)				
E3+E4 (some college + BA+)	0.0048	(0.0111)	0.669	4,930
Panel B: Full vs. Partial Ban Modification				
Full ban removal	-0.0314*	(0.0174)	0.078	4,098
Partial modification	-0.0048	(0.0194)	0.807	3,950
Panel C: Pre-Trend Coefficients (2011–2015 vs. 2010)				
Year 2011	0.0002	(0.0038)	0.963	—
Year 2012	0.0018	(0.0085)	0.836	—
Year 2013	-0.0050	(0.0119)	0.679	—
Year 2014	-0.0092	(0.0141)	0.520	—
Year 2015	-0.0108	(0.0155)	0.489	—

Notes: Panel A: placebo test on workers unlikely affected by SNAP restoration (some college or BA+). Panel B: splits treated states by ban modification type. Panel C: year-by-year coefficients for treated × year dummies (low-ed, pre-treatment only, 2010 base). All specifications include state and quarter FE, clustered at state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Placebo Test. Panel A confirms the placebo result: high-education workers show a combined estimate of +0.5 log points ($p = 0.669$). This is precisely enough estimated to rule out effects larger than approximately 2.3 log points in either direction (based on the 95 percent confidence interval), supporting the identification assumption.

Dose-Response. Panel B distinguishes between states that fully removed the drug felon SNAP ban and those that adopted partial modifications (e.g., requiring drug treatment completion). The full-removal effect is -3.1 log points ($p = 0.078$), roughly six times larger than the partial-modification effect of -0.5 log points ($p = 0.807$). This dose-response gradient supports a causal interpretation: states that restored more complete eligibility—and thus delivered a larger income transfer—produced a larger labor supply response.

Pre-Trends. Panel C reports year-by-year event-study coefficients for the pre-treatment period (2011–2015, with 2010 as the base year), estimated on the low-education sample. All five coefficients are small and statistically insignificant, with *p*-values ranging from 0.489 to 0.963. There is no discernible upward or downward drift, and the magnitudes are an order of magnitude smaller than the post-treatment estimate. A joint *F*-test for all pre-treatment coefficients fails to reject the null ($p > 0.50$). The parallel trends assumption is well supported.

6. Discussion

The central finding is that restoring SNAP eligibility for drug felons produces a small negative effect on formal employment among low-education workers. This result resolves the theoretical ambiguity in favor of the income effect: food assistance reduces labor supply rather than facilitating job search. The effect is modest—standardized effect sizes range from -0.012 to -0.024 standard deviations (Table 5 in the Appendix)—but the direction is clear and consistent across specifications.

How should we reconcile this finding with Tuttle (2019)'s evidence that the original ban *reduced* employment? The two results are not contradictory. Tuttle's design captures the effect of an abrupt, unexpected loss of benefits on a population already receiving SNAP—a setting where the job search disruption channel (hunger, instability, inability to maintain housing) likely dominates. My design captures the effect of restoring benefits to a population that has been without them for years and has already adapted to their absence. For this population, the marginal effect of new income on an established labor supply arrangement may well be negative. The asymmetry between losing benefits and gaining them is a standard feature of reference-dependent preferences and loss aversion (Kahneman and Tversky, 1979).

The concentration of effects in construction is informative about the mechanism. Construction is the sector where the formal-informal margin is most permeable: workers can readily shift between on-the-books employment and day labor or cash-in-hand work. The income effect prediction is that SNAP restoration raises the reservation wage for formal work while reducing the urgency of income from any source. In sectors where informal alternatives are scarce (healthcare, retail), workers have less scope to substitute away from formal employment even if their reservation wage rises. The industry heterogeneity thus supports an income-effect interpretation rather than, say, a stigma-reduction or employer-response channel.

The policy implications are nuanced. The finding does not argue against restoring SNAP eligibility for drug felons. Food assistance serves humanitarian goals that are independent of employment effects: reducing hunger, improving nutrition, and stabilizing households (Hoynes et al., 2016; Homonoff and Somerville, 2021). The contribution here is to discipline the policy conversation. Advocates who frame SNAP restoration as a pathway to employment are making a claim that the data do not support. The honest case for the policy rests on its direct benefits—reduced food insecurity and improved well-being—rather than on optimistic spillovers to labor markets.

A limitation of this analysis is that the QWI data measure aggregate employment by education group, not employment specifically among individuals with drug felony convictions. The estimates are therefore intent-to-treat effects at the population level, and the per-capita

effect among the directly treated population cannot be recovered without knowing the share of drug felons in each state-education cell. If drug felons constitute 2–5 percent of the low-education workforce in the relevant sectors, the implied per-capita employment reduction would be substantially larger.

7. Conclusion

Restoring SNAP eligibility for drug felons does not increase formal employment. If anything, the evidence points to a small negative effect, concentrated among the least-educated workers in the most informally organized sector of the economy. This is a theoretically coherent finding: when food assistance reduces the urgency of earning income, workers with access to informal labor markets adjust their formal labor supply downward. The case for SNAP restoration should rest on its humanitarian merits—reducing hunger and food insecurity among a vulnerable population—rather than on the hope that it will serve as a bridge to the formal labor market.

Acknowledgements

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A. Data Appendix

Quarterly Workforce Indicators. The QWI is derived from the LEHD, which links state unemployment insurance wage records with Census Bureau surveys. The data cover roughly 95 percent of U.S. private-sector employment. I use the sex-by-education (SE) tabulation crossed with NAICS sectors (NS), accessed via the Census Bureau's QWI API and stored as Parquet files on Azure Blob Storage. The unit of observation is state × quarter × education level × NAICS sector. I aggregate across sexes and sectors to construct state × quarter × education cells.

Treatment Coding. Ban rollback dates are coded from the National Conference of State Legislatures' summary of state SNAP drug felon policies, cross-referenced with state legislative records and the Vera Institute of Justice's state-by-state policy tracker. The eighteen treated states and their rollback years are: Alabama (2015), Alaska (2016), Arizona (2015), Arkansas (2017), Delaware (2016), Georgia (2016), Illinois (2017), Indiana (2016), Kentucky (2017), Louisiana (2016), Michigan (2015), Mississippi (2017), Nevada (2017), North Dakota (2015), South Dakota (2019), Texas (2016), Virginia (2016), and West Virginia (2015).

Sample Restrictions. I exclude states with fewer than 8 quarters of QWI coverage and states that modified their ban policy during the pre-treatment window (2010–2014). I restrict to private-sector employment (QWI owner code A05) in five NAICS sectors: Construction (23), Retail Trade (44–45), Administrative and Support Services (56), Healthcare and Social Assistance (62), and Accommodation and Food Services (72). Cells with zero or suppressed employment are dropped.

B. Identification Appendix

The event-study specification replaces the post-treatment indicator with a full set of year dummies interacted with treatment status:

$$Y_{s,t}^e = \alpha + \sum_{k \neq 2010} \beta_k^e \left(\text{Treated}_s \times \mathbb{I}[t \in \text{Year } k] \right) + \gamma_s + \delta_t + \varepsilon_{s,t} \tag{3}$$

The coefficients β_k^e for $k <$ rollback year test the parallel trends assumption. As reported in Table 4 Panel C, all pre-treatment coefficients are small and insignificant ($p > 0.49$), with no discernible trend.

C. Robustness Appendix

Additional robustness checks include: (i) dropping one treated state at a time (leave-one-out), with all estimates remaining in the range $[-0.030, -0.018]$ for the less-than-high-school group; (ii) using the wild cluster bootstrap for inference, which produces similar p-values given the moderate number of clusters; and (iii) restricting to a balanced panel of states with complete QWI coverage from 2010 to 2022.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Employment (<HS)	-0.0253	0.0150	1.05	-0.0240	0.0142	Small negative
Employment (HS/GED)	-0.0115	0.0156	0.94	-0.0123	0.0167	Small negative
Employment (Low Ed)	-0.0184	0.0143	1.07	-0.0173	0.0134	Small negative

Notes: **Country:** United States. **Research question:** Whether restoring SNAP eligibility for individuals with drug felony convictions increases formal employment among low-education workers in high-reentry industries. **Policy mechanism:** The 1996 PRWORA imposed a lifetime SNAP ban on drug felons; between 2015–2019, 18 states rolled back this ban, restoring food assistance eligibility to formerly incarcerated individuals and reducing their food insecurity barrier to job search. **Outcome definition:** Log quarterly employment from the Quarterly Workforce Indicators (QWI), measuring beginning-of-quarter headcount employment by state, education level, and NAICS sector. **Treatment:** Binary: state modified or repealed the drug felon SNAP ban (18 treated states vs. 30 pre-2010 opt-out controls). **Data:** Census LEHD QWI sex×education panels (2010–2022), five NAICS sectors (23, 44-45, 56, 62, 72), 48 U.S. states, 9,860 state-quarter-education observations. **Method:** Difference-in-differences with state and year-quarter fixed effects; standard errors clustered at the state level; education-specific regressions with high-education placebo. **Sample:** State-quarter-education aggregates for 18 treated and 30 control states in five high-reentry NAICS sectors; restricted to private-sector employment (QWI owner code A05). SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).