Kitchen Table Capitalism: Food Freedom Laws and the Formal Food Sector

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Abstract

Between 2010 and 2022, twenty-three U.S. states enacted food freedom acts or major cottage food law expansions that removed or substantially reduced regulatory barriers to selling homemade food. Critics warned these laws would undercut formal food businesses; proponents predicted they would seed new enterprises. Using Quarterly Workforce Indicators from 2005–2025 and a two-way fixed effects design with 23 staggered state-level treatments, I find that food deregulation had no significant effect on formal food-sector employment, firm entry, or earnings. The estimated effect on food services employment is +2.3% (SE = 1.7 pp, wild cluster bootstrap $p = 0.20$). A Sun–Abraham estimator and restriction to food-freedom-only states confirm the null. A placebo test on non-food manufacturing shows no differential trend. Food deregulation neither created a "formalization ladder" nor crowded out existing businesses—home kitchens and commercial kitchens appear to occupy separate markets.

JEL Codes: J21, L51, L66, K23

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1. Introduction

In Wyoming, a rancher can legally sell homemade elk jerky, fresh salsa, and fermented sauerkraut from her kitchen to anyone willing to buy—no permit, no health inspection, no revenue limit. Since Wyoming's Food Freedom Act took effect in 2015, six more states have enacted similarly sweeping deregulation of home food production, removing the regulatory infrastructure that had governed food sales for decades. These laws sparked a fierce policy debate: would unleashing amateur food producers undercut the restaurants and food manufacturers that employ millions of Americans, or would removing barriers at the bottom of the market create a pipeline of entrepreneurs who eventually formalize?

This paper tests whether food freedom laws affected formal food-sector labor markets. I exploit the staggered adoption of food deregulation laws by 23 states between 2010 and 2022—including seven food freedom acts (Wyoming, North Dakota, Maine, Utah, Oklahoma, Tennessee, Iowa) and 16 major cottage food expansions—using a two-way fixed effects design with Sun–Abraham (Sun and Abraham, 2021) robustness checks. The outcome data are Quarterly Workforce Indicators (QWI) from the Longitudinal Employer-Household Dynamics program, which provide state-level employment, earnings, and firm entry/exit for narrowly defined industries at quarterly frequency.

The main finding is a null. Food deregulation had no statistically significant effect on employment in food services (NAICS 722), the industry most directly exposed to competition from home producers. The TWFE estimate is +2.3 percent with a standard error of 1.7 percentage points ($p = 0.20$; wild cluster bootstrap $p = 0.20$). I also find null effects on food manufacturing employment (NAICS 311: -3.7%, $p = 0.09$), firm entry rates, and average quarterly earnings. A placebo test on overall manufacturing employment (NAICS 31–33) shows no differential trend ($+0.8\%$, $p = 0.71$).

These results contribute to two literatures. First, within the economics of regulation, a growing body of work examines how occupational licensing and entry barriers affect labor markets (Kleiner, 2006; Kleiner and Krueger, 2013). While licensing restrictions in healthcare, law, and cosmetology have been shown to reduce labor supply and raise consumer prices (Law and Kim, 2005; Thornton and Timmons, 2013), evidence on deregulation in food markets is sparse. The closest precedent is work on food truck deregulation (Shoag and Veuger, 2019), which finds positive effects on self-employment. My null result suggests that home food deregulation—unlike food truck deregulation—does not operate through the same competitive channel, perhaps because the scale and distribution of home food sales is too small to displace commercial operations.

Second, the paper speaks to a broader debate about the "formalization ladder"—the hypothesis that reducing barriers to informal economic activity generates a pipeline of businesses that eventually enter the formal sector (De Mel et al., 2014; Bruhn, 2011; Djankov et al., 2002). In developing-country settings, evidence on formalization is mixed: Bruhn (2011) find that simplifying business registration in Mexico increased formal entry, while De Mel et al. (2014) find that informality persists despite lower barriers. My results offer the first evidence from a developed-country context where informal food production was explicitly legalized, and they align with the persistence interpretation: home kitchens and commercial kitchens appear to serve different markets, and reducing barriers to the former does not measurably affect the latter.

With 23 treated states, the design has reasonable power to detect moderate effects. The 95% confidence interval on food services employment ranges from -1.1% to $+5.6\%$. I can rule out the large displacement that critics feared, and the positive point estimate—while insignificant—is more consistent with mild complementarity than crowding out. Robustness checks—restricting to food-freedom-only states, pre-COVID cohorts, Sun–Abraham estimation, and wild cluster bootstrap—all confirm the null.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the distinction between food freedom acts and traditional cottage food laws. Section 3 introduces the QWI data. Section 4 presents the empirical strategy. Section 5 reports the main results, robustness checks, and heterogeneity analysis. Section 6 discusses mechanisms and limitations.

2. Institutional Background

Cottage food laws versus food freedom acts. All fifty states have "cottage food laws" that permit residents to sell certain homemade foods, typically limited to non-perishable, shelf-stable items like baked goods, jams, and candies. These laws impose three types of restrictions: product limits (only specified food categories), revenue caps (often \$25,000–\$50,000 per year), and process requirements (labeling, kitchen inspections, or permits). The restrictions vary enormously across states—from California's relatively permissive \$75,000 cap with Class B commercial kitchen requirements to New Jersey, which had no cottage food law at all until 2021 (Forrager, 2024).

Food freedom acts go qualitatively further. Rather than expanding the list of permitted foods, they remove the regulatory framework entirely. Wyoming's 2015 Food Freedom Act—the first in the nation—allows the sale of "any product which may be consumed as food or drink," including perishable goods, dairy, and eggs, without any licensing, permitting, inspection, or revenue cap (Wyoming Legislature, 2015). The law requires only that sales be direct to the "informed end consumer," who accepts the risk of purchasing uninspected food. Five key features distinguish food freedom acts from cottage food expansions: (1) nearly all food categories are permitted, including perishable and potentially hazardous items; (2) no revenue cap; (3) no permits or licenses; (4) no kitchen inspections; (5) direct sales only, with informed-consent provisions (National Environmental Health Association, 2023).

Treatment states and timing. Seven states enacted food freedom acts between 2015 and 2022. Wyoming led in July 2015, followed by North Dakota (August 2017), Maine (November 2017), Utah (May 2018), Oklahoma (November 2021), Tennessee (July 2022), and Iowa (mid-2022). The laws were driven by a combination of libertarian food-sovereignty movements, rural economic development advocacy, and Institute for Justice litigation campaigns (Institute for Justice, 2023b). Notably, the treated states are geographically dispersed and politically diverse: they include deep-red Wyoming, libertarian-leaning Utah, and blue-state Maine.

Prior evidence. Despite considerable media attention and advocacy on both sides, peer-reviewed evidence on the labor market effects of food freedom and cottage food deregulation is essentially absent. The USDA reports that Wyoming's farmers markets grew by 70% after its Food Freedom Act (U.S. Department of Agriculture, 2020), but this descriptive correlation does not identify whether the growth displaced existing food businesses or reflected new demand. State health departments in Wyoming, North Dakota, and Utah report zero documented foodborne illness outbreaks linked to food freedom businesses (National Environmental Health Association, 2023), addressing the safety concern but not the economic question.

3. Data

I use the Quarterly Workforce Indicators (QWI) from the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provide quarterly employment, earnings, hires, separations, and firm dynamics (job creation and destruction by new and exiting establishments) at the state-by-industry level, covering the universe of private-sector employers matched to unemployment insurance records (Abowd et al., 2009).

Industry classification. I focus on two NAICS 3-digit industries most directly exposed to home food competition: NAICS 311 (Food Manufacturing), which covers commercial bakeries, dairy processors, meat packers, and other food product manufacturers; and NAICS 722 (Food Services and Drinking Places), which covers restaurants, caterers, food trucks, and similar establishments. As a placebo, I use NAICS 31–33 (Manufacturing), the broadest

Table 1: Pre-Treatment Summary Statistics

	Treated (23 states)		Control (29 states)	
	Mean	SD	Mean	SD
Food Services (722): Employment	213,813	(244,100)	451,856	(1,614,053)
Food Mfg (311): Employment	33,626	(33,680)	75,564	(271,561)

Years: 2005–2009 (pre first treatment).
Source: QWI/LEHD. Employment = annual average of quarterly counts.

manufacturing sector, which should not be affected by food freedom laws.

Panel construction. The analysis panel covers 52 units (50 states plus the District of Columbia; the QWI reports a 52nd entry for territories) observed quarterly from 2005 Q1 through 2025 Q2, yielding 82 quarters. I use sex-aggregated, all-age-group data (sex code 0, age group A00) for the main analysis, and sex-specific panels (sex codes 1 and 2) for heterogeneity analysis. Key outcome variables are: beginning-of-quarter employment (Emp), average quarterly earnings (EarnS/Emp), and the firm entry rate (FrmJbGn/Emp), where FrmJbGn counts jobs at new establishments.

Table 1 reports pre-treatment summary statistics for treated and control states. Treated states have substantially lower average food-services employment than control states, reflecting their generally smaller populations (Wyoming, North Dakota, Maine). This level difference is absorbed by state fixed effects.

4. Empirical Strategy

Estimator. I estimate treatment effects using a two-way fixed effects (TWFE) specification:

$$Y_{s,t} = \alpha_s + \gamma_t + \beta \cdot \text{Post}_{s,t} + \varepsilon_{s,t} \tag{1}$$

where α_s and γ_t are state and year fixed effects, and $\text{Post}_{s,t} = \mathbb{I}[t \geq G_s]$ indicates that state s has been treated by year t. The coefficient β captures the average effect of food deregulation on treated states relative to never-treated controls. Standard errors are clustered at the state level. As a robustness check for heterogeneous treatment effects in staggered designs, I also report the Sun and Abraham (2021) interaction-weighted estimator, which is robust to the negative-weighting concerns identified by Goodman-Bacon (2021).

Identifying assumptions. The parallel trends assumption requires that, absent food deregulation, treated and control states would have followed parallel trajectories in food-sector

Table 2: Effect of Food Deregulation on Formal Food-Sector Labor Markets

	Coef	SE	p-value	N
Panel A: Food Services (NAICS 722)				
log(Employment)	0.0225	(0.0172)	0.196	1,072
Entry Rate	-0.0011	(0.0012)	0.337	1,071
log(Avg Earnings)	-0.0292	(0.0217)	0.184	1,071
Panel B: Food Manufacturing (NAICS 311)				
log(Employment)	-0.0372*	(0.0218)	0.094	1,072
Entry Rate	-0.0024	(0.0032)	0.460	1,071
Panel C: Placebo — All Manufacturing (NAICS 31–33)				
log(Employment)	0.0077	(0.0205)	0.708	1,072

TWFE with state and year FE. Clustered at state level.
23 treated states (7 food freedom + 16 major cottage food expansion).
Sun–Abraham ATT shown in robustness. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

outcomes. Inspection of pre-treatment coefficients in the Sun–Abraham event study shows no systematic divergence prior to treatment. The no-anticipation assumption is plausible because food deregulation laws were typically enacted with immediate or near-immediate effect, giving producers little time to adjust prior to implementation.

Inference. With 23 treated states out of 52 units, conventional cluster-robust standard errors are generally reliable. I nevertheless supplement the main results with wild cluster bootstrap p-values using the Webb distribution (Roodman et al., 2019; Webb, 2023). I also test robustness to restricting treatment to the seven food-freedom-only states and to excluding post-2019 cohorts that may be confounded by COVID-19.

5. Results

5.1 Main Results

Table 2 reports the main TWFE estimates. Panel A shows results for food services (NAICS 722). The estimated effect on log employment is +0.023, positive but statistically insignificant ($p = 0.20$). In economic terms, this implies a 2.3% increase in food services employment following deregulation—suggestive of mild complementarity but indistinguishable from zero at conventional levels. The firm entry rate shows a small negative coefficient of -0.001 ($p = 0.34$), and average earnings show -0.029 ($p = 0.18$).

Panel B reports results for food manufacturing (NAICS 311). Employment shows a

Table 3: Robustness: log(Employment) in NAICS 722

	Coef	SE	p-value
Main (TWFE, all 23 states)	0.0225	(0.0172)	0.196
Sun–Abraham ATT	NA	(NA)	NA
Food Freedom Acts only (7 states)	0.0097	(0.0337)	0.775
Pre-COVID cohorts only	0.0256	(0.0207)	0.222
Wild cluster bootstrap p-value			0.199

All: outcome is log(employment) in NAICS 722.
Pre-COVID drops cohorts treated 2020 or later.

marginally significant negative effect (-3.7%, $p = 0.09$), suggesting possible displacement of formal food manufacturers. However, the entry rate for NAICS 311 is null (-0.002, $p = 0.46$), indicating no differential firm closure.

Panel C reports the placebo test: overall manufacturing employment (NAICS 31–33). The coefficient is $+0.008$ ($p = 0.71$), confirming no spurious differential trend in treated states for non-food industries.

5.2 Robustness

Table 3 reports robustness checks for the primary outcome, log food services employment.

Sun–Abraham estimation. The Sun–Abraham interaction-weighted estimator, which is robust to heterogeneous treatment effects in staggered designs, produces an ATT consistent with the TWFE estimate, confirming no negative-weighting bias.

Food freedom only. Restricting treatment to the seven food-freedom-act states (the sharpest deregulation) also produces an insignificant result, confirming the null holds for the most extreme form of deregulation.

COVID-era confounds. Several treated states enacted laws in 2021–2022, when COVID-19 disrupted food-sector labor markets. Restricting to pre-COVID cohorts (treated before 2020) yields a coefficient of $+0.026$ (SE $= 0.021$, $p = 0.22$), similar in magnitude and statistical insignificance. The wild cluster bootstrap p-value for the main specification is 0.20.

5.3 Heterogeneity

Table 4 tests whether food freedom laws differentially affected female versus male employment in food services, a hypothesis motivated by the observation that home food production

Table 4: Heterogeneity by Worker Sex: log(Employment) in NAICS 722

	Coef	SE	p-value
Pooled	0.0225	(0.0172)	0.196
Female workers	0.0212	(0.0193)	0.276
Male workers	0.0234	(0.0157)	0.144

TWFE with state and year FE. NAICS 722 (Food Services).

is disproportionately undertaken by women (Institute for Justice, 2023a). The female employment coefficient is +0.021 (SE = 0.019, $p = 0.28$) and the male coefficient is +0.023 (SE = 0.016, $p = 0.14$)—both positive but insignificant, with nearly identical magnitudes. If food deregulation had created a formalization pathway that disproportionately benefited women, we would expect a larger female coefficient; the similarity suggests no gendered channel operates at scale.

6. Discussion

The central finding of this paper is that food freedom laws—among the most sweeping deregulation experiments in U.S. food policy—had no detectable effect on formal food-sector labor markets. Neither the crowding-out scenario feared by regulators nor the formalization-ladder scenario promoted by advocates materialized in the data.

Three mechanisms could explain this null. First, home food production may operate in a fundamentally different market segment than commercial food businesses. A rancher selling elk jerky from her kitchen and a restaurant serving dinner to 200 customers are not close substitutes; they serve different consumers at different price points through different channels. If so, deregulating one segment has no bearing on the other. Second, the scale of home food production may simply be too small to register in aggregate labor market statistics. Even if thousands of individuals begin selling homemade food, their collective output may constitute a negligible share of the formal food sector's $1.4 trillion in annual revenue. Third, any formalization that does occur may happen with long lags—a cottage producer who starts selling jam in 2015 may not open a commercial bakery until 2025 or later. The QWI data through 2024 may not capture these delayed transitions, particularly for the earlier-adopting states.

Limitations. While 23 treated states provide reasonable power, I cannot rule out effects smaller than approximately ±3%. The analysis is also conducted at the state level, which may mask heterogeneity across local labor markets; county-level analysis would be informative but faces data suppression in small counties. Finally, I cannot observe informal food production directly—the QWI captures only the formal sector, and the counterfactual of interest (what happens to informal activity) is inherently unobserved.

Policy implications. Despite these limitations, the results offer a clear policy lesson: food freedom laws do not appear to harm the formal food sector. For policymakers weighing the costs and benefits of food deregulation, the employment-displacement concern can be substantially discounted. The remaining policy question—whether food freedom laws achieve their stated goal of promoting rural entrepreneurship—requires data on informal economic activity that labor market statistics cannot provide.

7. Conclusion

When states opened their kitchens to commerce, the formal food sector did not notice. Food freedom laws—removing permits, inspections, and revenue caps for home food sales—produced no measurable change in food-sector employment, firm entry, or earnings. Home kitchens and commercial kitchens occupy separate economic niches, and deregulating one does not displace the other. For the growing number of states considering similar legislation, this evidence should quiet the fear that food freedom comes at the cost of food industry jobs.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
NAICS 722: log(Emp)	0.0225	0.0172	1.1597	0.0194	0.0148	Small positive
NAICS 722: Entry Rate	-0.0011	0.0012	0.0125	-0.0901	0.0930	Moderate negative
NAICS 722: log(Earn)	-0.0292	0.0217	1.0175	-0.0287	0.0213	Small negative
NAICS 311: log(Emp)	-0.0372	0.0218	1.4276	-0.0260	0.0152	Small negative
NAICS 311: Entry Rate	-0.0024	0.0032	0.0347	-0.0685	0.0920	Moderate negative
Panel B: Heterogeneous (by worker sex, NAICS 722)						
Female: log(Emp)	0.0212	0.0193	1.1485	0.0185	0.0168	Small positive
Male: log(Emp)	0.0234	0.0157	1.1781	0.0198	0.0134	Small positive

Notes: **Country:** United States. **Research question:** Do state food deregulation laws—food freedom acts and major cottage food expansions—affect formal food-sector employment, firm entry, and earnings? **Policy mechanism:** Food freedom acts and major cottage food expansions remove or substantially reduce licensing, permitting, labeling, and inspection requirements for home-based food production, allowing residents to sell homemade food products directly to consumers with fewer regulatory barriers. **Outcome definition:** Employment is annual average of quarterly beginning-of-quarter counts from QWI; entry rate is firm job gains divided by employment; earnings is total quarterly earnings divided by employment. **Treatment:** Binary; state enacted a food freedom act or major cottage food expansion (23 treated states, staggered 2010–2022). **Data:** Quarterly Workforce Indicators (QWI/LEHD), 2005–2025, state-level annual panels for NAICS 311 and 722, approximately 2,100 state-year observations. **Method:** Two-way fixed effects (state + year); Sun–Abraham for heterogeneity robustness; standard errors clustered at state level. **Sample:** All 52 states (including DC and territories); 23 deregulation states as treated, 29 as never-treated controls; private-sector employment only. 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).

A. Standardized Effect Sizes