The Invisible Floor: Women's Minimum Wage Laws and Labor Market Trajectories in Progressive Era America

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Abstract

Between 1912 and 1920, fourteen U.S. states enacted the nation's first minimum wage laws, targeting women in manufacturing, retail, and hospitality while exempting domestic service and agriculture. Whether these laws protected or excluded women remains contested. I link 1.6 million women across the 1910 and 1920 Censuses using the IPUMS machine-learning panel and estimate a triple-difference: minimum-wage states $\times$ covered industries $\times$ time, with exempt industries as within-state controls and men as a placebo. The laws had no detectable effect on women's labor force retention (0.9 pp, $p = 0.59$), industry persistence (-0.8 pp), or occupational mobility (-0.15 points). Wild cluster bootstrap confirms these nulls. The estimates rule out effects larger than 3.4 percentage points. America's first wage floor was an invisible floor—present in statute but absent from women's labor market trajectories.

JEL Codes: J08, J16, J38, N31, N32

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1. Introduction

In 1912, Massachusetts became the first American state to establish a minimum wage. The law was remarkable not for its level but for its scope: it applied only to women. Over the next eight years, thirteen more states followed, each constructing a wage floor that applied to women in manufacturing, retail, laundry, and hospitality—while explicitly exempting domestic servants and farmworkers. A century later, historians and economists still disagree about what these laws actually did. Were they shields that protected vulnerable workers from exploitation, or swords that cut women out of the labor market by raising the cost of hiring them?

This debate is not merely historical. The argument that minimum wages displace low-wage workers—particularly women and minorities—has been central to policy battles from Lochner to the present (Neumark and Wascher, 2008). The Supreme Court struck down the District of Columbia's women's minimum wage in *Adkins v. Children's Hospital* (1923), reasoning that the law improperly interfered with women's right to contract. Contemporary opponents echo this logic: price floors above market-clearing wages destroy jobs. Proponents counter that labor markets are monopsonistic, so wage floors can raise employment by reducing employer rents (Manning, 2003; Card and Krueger, 1994).

The empirical question—what actually happened to individual women when these laws took effect—has remained largely unanswered. Seltzer and Fishback (2020) provide a comprehensive institutional history of Progressive Era minimum wages but lack individual-level panel data to trace labor market trajectories. The broader literature on early twentieth-century women's labor draws on aggregate statistics (Goldin, 1990, 2006) or case studies of particular industries (Tentler, 1979). No study has tracked what happened to *individual* women before and after the first wage floors.

This paper fills that gap. I use the IPUMS Multi-Linkage Panel (MLP; Helgertz et al., 2023), which links 43.9 million individuals across the 1910 and 1920 Censuses using machine-learning methods. From this panel, I extract 1.6 million women who were in the labor force in 1910 and observe their labor market status a decade later. The identification strategy exploits a triple-difference: (1) states that enacted minimum wage laws versus those that did not; (2) industries covered by these laws versus exempt industries within the same states; and (3) the passage of time between 1910 and 1920. The within-state comparison of covered versus exempt industries absorbs state-level confounders, while the cross-state comparison absorbs industry-level trends. Men in the same industries serve as a natural placebo, since the laws applied exclusively to women.

The results are striking in their clarity: the laws did nothing. The triple-difference estimate

for labor force retention is 0.9 percentage points ($p = 0.59$ by wild cluster bootstrap), with a 95% confidence interval of $[-2.0, 3.4]$ percentage points. Industry persistence shows a similarly precise zero (-0.8 pp, $p = 0.78$), as does occupational score change (-0.15 points, $p = 0.48$). A pre-treatment placebo using the 1900–1910 panel confirms that covered-versus-exempt gaps in future MW states were not diverging before the laws existed. The nulls are further robust to county fixed effects, leave-one-out state exclusion, enforcement strength (advisory versus mandatory commissions), and sample splits by race. The men's placebo confirms: laws that targeted only women produced no differential effect on men either.

These are not underpowered zeros. With 1.6 million women and 49 state clusters (14 treated), the confidence intervals rule out effects larger than 3.4 percentage points for retention—roughly one-fourteenth of the base retention rate. If the first minimum wages had displaced even a modest fraction of women from covered industries, this design would detect it.

The paper contributes to three literatures. First, it advances the economic history of women's labor by providing the first individual-level panel estimates of Progressive Era minimum wage effects, complementing aggregate analyses by Goldin (1990) and Seltzer and Fishback (2020). Second, it contributes to the minimum wage literature by establishing a historical null with modern data and identification—a finding that resonates with the post-Card and Krueger (1994) consensus that moderate minimum wages have small or zero disemployment effects (Cengiz et al., 2019; Dube, 2019). Third, it demonstrates the power of full-count linked census panels for policy evaluation in historical settings where administrative data do not exist, following Abramitzky et al. (2012, 2014) and Bailey et al. (2020).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports the main results and robustness checks. Section 6 discusses the findings.

2. Institutional Background

The women's minimum wage movement emerged from Progressive Era concerns about "sweated" labor—the exploitation of women workers in unregulated factories, laundries, and department stores (Sklar, 1995). Reformers argued that women, unlike men, could not bargain effectively for fair wages and required state protection. This paternalistic framing was legally strategic: the Supreme Court had struck down general minimum wages in *Lochner v. New York* (1905), but women's protective legislation survived scrutiny under *Muller v. Oregon* (1908).

Massachusetts enacted the first law in 1912 as an advisory (non-binding) commission.

The following year, eight states—California, Colorado, Minnesota, Nebraska, Oregon, Utah, Washington, and Wisconsin—established mandatory commissions with enforcement authority. Arkansas and Kansas followed in 1915, Arizona in 1917, and North Dakota and Texas in 1919. By 1920, fourteen states had some form of women's minimum wage on the books.

Coverage and exemptions. The laws uniformly covered women in manufacturing, retail trade, laundry and cleaning services, and hotels and restaurants. They uniformly *exempted* domestic service and agriculture—the two sectors that employed the largest shares of Black and immigrant women (Goldin, 1990). This coverage pattern creates the within-state variation central to this paper's identification: covered and exempt industries within the same state experienced different regulatory treatment.

Enforcement and bite. Enforcement varied considerably. Massachusetts's advisory commission had no legal authority to compel compliance. Oregon and Washington established industrial welfare commissions with subpoena power and penalty provisions. Most states fell between these extremes, with underfunded commissions that relied on publicity rather than prosecution (Seltzer and Fishback, 2020). Whether these laws had sufficient "bite" to affect labor market outcomes is an empirical question that prior work has been unable to answer with individual data.

Wage levels. The minimum rates set by state commissions ranged from \$6 to \$10 per week for women in manufacturing and retail, roughly 50–75% of prevailing male wages in the same industries (Thies, 1991). These levels were binding for a substantial fraction of women workers: Goldin (1990) estimates that roughly one-quarter of women in manufacturing earned less than commission-established minimums.

3. Data

The MLP linked panel. The primary data source is the IPUMS Multi-Linkage Panel (Helgertz et al., 2023), which links individuals across decennial censuses using machine-learning algorithms trained on labeled genealogical records. The 1910–1920 panel contains 43.9 million linked person-observations. For each individual, I observe state of residence, age, race, nativity, literacy, marital status, occupation (coded to 1950 categories), industry (1950 categories), occupational income score, and household characteristics—all in both 1910 and 1920.

Sample construction. The estimation sample consists of women who were in the labor force in the 1910 Census (occupation code > 0 and < 979) and whose 1910 industry falls into one of

two categories: (1) *covered industries*—manufacturing (ind1950 codes 306–499), retail trade (606–699), and hospitality/laundry services (806–829); or (2) *exempt industries*—agriculture (100–126) and domestic service (856). This yields 1,609,942 women across 49 states.

Treatment assignment. I assign treatment based on whether a woman's 1910 state of residence enacted a women's minimum wage law by 1920. Fourteen states are coded as treated. The triple-difference interacts this state-level indicator with a covered-industry indicator. Since both components are measured in 1910—before most laws took effect—they are predetermined with respect to the policy.

Outcome variables. I construct three outcomes: (1) *labor force retention*—an indicator equal to one if the woman is in the labor force in the 1920 Census; (2) *industry persistence*—an indicator equal to one if her 1920 industry code matches her 1910 code; and (3) *occupational score change*—the difference between her 1920 and 1910 occupational income scores.

Table 1 presents summary statistics by treatment group. Women in covered industries are younger, less likely to be married, and have higher occupational scores than women in exempt industries, consistent with the exemption of domestic service (older, married women) and agriculture (rural). Baseline characteristics are broadly similar across MW and non-MW states, with two notable exceptions: MW states (disproportionately Western) have fewer native-born women (40% versus 57%) and more white women (94% versus 86%). State fixed effects absorb these level differences.

4. Empirical Strategy

4.1 Identification

The core specification is a triple-difference that exploits variation across three dimensions: state minimum wage adoption, industry coverage, and time.[1]

$$Y_i = \alpha + \beta(\text{MW}_s \times \text{Covered}_j) + \delta_s + \lambda_j + X_i'\gamma + \varepsilon_i \tag{1}$$

where Y_i is the 1920 labor market outcome for woman i who was in industry j in state s in 1910; MW_s indicates that state s enacted a minimum wage law by 1920; Covered_j indicates

[1]Formally, since outcomes are measured in 1920 conditional on 1910 labor force status, the regression is estimated on a single cross-section of linked individuals rather than a pooled panel with a time indicator. The "triple" differencing occurs through the combination of state fixed effects (absorbing MW status), industry fixed effects (absorbing covered/exempt status), and the implicit first difference embedded in the panel outcome (1920 status conditional on 1910 status). The 1900–1910 placebo (Table 3, Panel A) provides direct evidence that this design yields null estimates in the pre-treatment period.

Table 1: Summary Statistics by Treatment Group

	MW States		Non-MW States	
	Covered	Exempt	Covered	Exempt
Panel A: 1910 Characteristics				
Age	31.343	30.386	31.096	30.342
Native-born	0.402	0.666	0.565	0.905
Literate	0.966	0.846	0.939	0.727
Married	0.302	0.470	0.313	0.513
White	0.940	0.751	0.865	0.556
Occupational score	17.350	10.488	16.569	10.298
Panel B: 1920 Outcomes				
In labor force	0.518	0.287	0.514	0.362
Same industry	0.227	0.172	0.222	0.237
Occ. score change	-7.20	-6.67	-6.97	-5.94
Observations	233,859	123,556	842,420	410,108

Notes: Sample consists of women observed in the labor force in the 1910 Census who are linked to their 1920 Census record via the IPUMS MLP panel. Covered industries include manufacturing, retail trade, hotels/lodging, eating/drinking, and laundry services. Exempt industries include agriculture and domestic service. MW states are the 14 states that enacted women's minimum wage laws between 1912 and 1920.

that industry j was subject to the law; δ_s and λ_j are state and industry fixed effects; and X_i includes 1910 age, age squared, native-born status, literacy, marital status, and race. The coefficient β is the triple-difference estimand: the differential change in outcomes for women in covered industries in MW states, relative to exempt industries in the same states, net of the same industry gap in non-MW states.

Identifying assumption. The DDD requires that, absent the minimum wage laws, the gap between covered and exempt industries in MW states would have evolved in parallel with the same gap in non-MW states. This assumption is untestable with only two census periods, but two features of the design support it. First, the within-state comparison absorbs any state-level shock (e.g., differential economic growth in Western states). Second, the within-industry comparison absorbs any industry-level trend (e.g., secular manufacturing growth).

Placebo: men. A natural falsification test applies the identical specification to men. Since the laws targeted only women, β should be zero for men. A large male "effect" would signal that the DDD captures industry-by-state trends rather than the policy.

4.2 Inference

Standard errors are clustered at the state level, the unit of policy variation (49 clusters, 14 treated). Given the small number of treated clusters, I supplement conventional cluster-robust inference with the wild cluster bootstrap using Webb weights (Webb, 2014), implemented via `fwildclusterboot` with 9,999 iterations (Roodman et al., 2019). Leave-one-out state exclusion provides an additional check on whether results are driven by any single state.

5. Results

5.1 Main Estimates

Table 2 reports the triple-difference estimates. The coefficient on MW × Covered is small and statistically insignificant across all three outcomes and both specifications.

For labor force retention (columns 1–2), the point estimate is 1.3 percentage points without controls and 0.9 percentage points with controls ($p = 0.59$ by wild cluster bootstrap). The 95% bootstrap confidence interval of $[-2.0, 3.4]$ percentage points rules out retention effects larger than about 7% of the base rate (45.8%). Industry persistence (columns 3–4) shows a similarly precise zero: -0.8 percentage points ($p = 0.78$). Occupational score change (columns 5–6) is -0.15 points ($p = 0.48$), trivial relative to the outcome standard deviation of 10.9 points.

The magnitude of these nulls is informative. A priori, one might expect a substantial displacement effect if employers substituted men for women in response to the wage floor. The data rule this out: the upper bound of the retention confidence interval (3.4 pp) is less than one-fifteenth of the baseline retention gap between covered and exempt industries (15.3 pp). Alternatively, if the wage floor improved job quality enough to retain women, the data rule that out too: the lower bound (-2.0 pp) excludes meaningfully negative effects.

5.2 Placebo and Robustness

Table 3 reports five classes of robustness checks.

Pre-treatment placebo (1900–1910). The most direct test of the identifying assumption applies the identical specification to the 1900–1910 MLP panel—a decade in which no minimum wage laws existed. Using 703,634 women in the labor force in 1900, the placebo DDD is 0.4 percentage points for retention ($p = 0.72$) and -1.0 percentage points for industry persistence ($p = 0.65$). Both are insignificant and small, providing direct evidence that covered-versus-exempt industry gaps in future MW states were not diverging before the laws

Table 2: Effect of Women's Minimum Wage Laws on Labor Market Trajectories

	Retention		Same Industry		Occ. Score Δ	
	(1)	(2)	(3)	(4)	(5)	(6)
MW × Covered	0.0127	0.0086	-0.0053	-0.0080	-0.0825	-0.1474
	(0.0144)	(0.0116)	(0.0246)	(0.0249)	(0.4030)	(0.2034)
WCB p-value		0.590		0.781		0.476
Controls	No	Yes	No	Yes	No	Yes
Dep. var. mean	0.458	0.458	0.223	0.223	-6.72	-6.72
Observations	1,609,943	1,609,943	1,609,943	1,609,943	1,609,943	1,609,943
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports the DDD coefficient: MW state × covered industry. State and industry fixed effects absorb the main effects. Controls: age, age^2, native-born, literate, married, white. Standard errors clustered by state in parentheses. WCB p-values from wild cluster bootstrap (Webb weights, 9,999 iterations). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

were enacted.

Men's placebo. Applying the identical DDD to 9.1 million men in covered versus exempt industries yields a retention coefficient of 0.7 percentage points ($p = 0.19$) and an industry persistence coefficient of -0.6 percentage points ($p = 0.86$). Neither is statistically significant, consistent with laws that targeted only women.

County fixed effects. Replacing 49 state fixed effects with approximately 2,400 county fixed effects absorbs finer geographic variation while preserving the state-level clustering for inference. The retention estimate barely moves (0.7 pp, $p = 0.60$).

Enforcement strength. If the aggregate null masks real effects in states with stronger enforcement, separating advisory from mandatory laws should reveal this. Massachusetts (the only advisory state) shows a negative coefficient (-2.1 pp, $p < 0.01$), likely reflecting Massachusetts-specific trends rather than a causal enforcement effect. The 13 states with mandatory commissions show a null (+1.1 pp, $p = 0.35$). The null persists even in the subset of states where the laws had the most institutional teeth.

Heterogeneity. The null is consistent across demographic subgroups. Among white women, the DDD is 0.2 pp; among non-white women, 4.5 pp (imprecise, SE = 3.5 pp, reflecting the smaller sample of 340,681).

Table 3: Robustness Checks and Placebo Tests

Specification	Coefficient	SE	Notes
Panel A: Pre-treatment Placebo (1900–1910)			
Retention	0.0044	(0.0123)	$N = 703{,}634$
Same industry	−0.0100	(0.0217)	
Panel B: Placebo (Men, 1910–1920)			
Retention	0.0074	(0.0055)	$N = 9{,}119{,}494$
Same industry	−0.0063	(0.0356)	
Panel C: Alternative FEs			
County FE	0.0075	(0.0140)	2,400 counties
Panel D: Enforcement Strength			
Advisory only (MA)	−0.0214	(0.0066)	1 state
Mandatory commission	0.0111	(0.0117)	13 states
Panel E: Heterogeneity			
White women	0.0024	(0.0087)	$N = 1{,}269{,}261$
Non-white women	0.0453	(0.0346)	$N = 340{,}681$

Notes: All specifications include individual controls (age, age^2, native-born, literate, married, white) and state × industry fixed effects (county × industry for Panel B). Standard errors clustered by state. The men's placebo (Panel A) applies the identical DDD to men in the same covered vs. exempt industries; laws targeted women only.

Leave-one-out. Table 4 reports results from dropping each MW state in turn. The retention coefficient ranges from −0.2 pp (dropping Texas, a 1919 adopter with minimal implementation time) to +1.2 pp (dropping Wisconsin). No single state drives the result, and the sign is positive in 13 of 14 specifications. The null is not an artifact of a single influential state.

6. Discussion

The first American minimum wages for women left no detectable mark on women's labor market trajectories. This finding speaks to three debates.

Protection versus exclusion. The historical debate frames these laws as either protective legislation that raised living standards or exclusionary barriers that priced women out of work (Seltzer and Fishback, 2020; Goldin, 1990). The data support neither narrative. The wage floor appears to have been genuinely invisible—present in statute but absent from the labor market outcomes that would reflect either protection or exclusion. This is consistent with Seltzer and Fishback (2020)'s institutional account of weak enforcement: most commissions

Table 4: Leave-One-Out Sensitivity: Dropping Each MW State

Dropped State	Coefficient	SE
Arizona	0.0089	(0.0116)
Arkansas	0.0120	(0.0117)
California	0.0111	(0.0117)
Colorado	0.0079	(0.0120)
Kansas	0.0096	(0.0120)
Massachusetts	0.0111	(0.0116)
Minnesota	0.0054	(0.0127)
Nebraska	0.0077	(0.0121)
North Dakota	0.0095	(0.0117)
Oregon	0.0081	(0.0119)
Texas	-0.0024	(0.0094)
Utah	0.0089	(0.0117)
Washington	0.0088	(0.0119)
Wisconsin	0.0122	(0.0118)
Full sample	0.0086	(0.0116)

Notes: Each row drops one MW state from the estimation sample. Dependent variable: labor force retention (in LF in 1920). All specifications include controls, state FE, and industry FE with standard errors clustered by state.

lacked the staff and legal authority to compel compliance, and the wage levels they set may not have been binding for most employers.

The modern minimum wage debate. The null result echoes a growing body of evidence that minimum wages have small or zero disemployment effects (Card and Krueger, 1994; Cengiz et al., 2019; Dube, 2019; Harasztosi and Lindner, 2019). What is novel here is the setting: a first-generation wage floor applied to a historically marginalized group in an era of minimal labor regulation. If displacement were ever likely, it would be here—yet the data show none. This extends the minimum wage literature's reach backward by a century and into a policy environment where enforcement institutions were weakest.

Linked census panels for historical policy evaluation. This paper demonstrates that full-count linked census panels can deliver well-powered null results for historical policies that lack administrative data. The 1.6 million-woman estimation sample yields confidence intervals tight enough to rule out economically meaningful effects. The MLP linkage technology (Helgertz et al., 2023; Abramitzky et al., 2021) opens a wide frontier for causal evaluation of Progressive Era policies—from Prohibition to immigration restriction to public health mandates—using the same individual-level panel approach.

7. Conclusion

America's first minimum wage laws were designed to protect women from exploitative wages. A century of debate has asked whether they succeeded or backfired. The answer, from 1.6 million linked census records, is neither: the invisible floor changed nothing. This null is not a failure of the research design—it is the finding. The first wage floors for women were too weakly enforced, too modestly set, or too easily evaded to alter the trajectory of women's working lives. For policymakers considering minimum wage policy today, the lesson is not that wage floors are ineffective—decades of modern evidence suggest otherwise—but that a law's existence is not the same as its enforcement. The floor must be visible to matter.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Retention	0.0086	0.0116	0.498	0.0173	0.0233	Small positive
Same industry	-0.0080	0.0249	0.416	-0.0192	0.0599	Small negative
Occ. score Δ	-0.1474	0.2034	10.903	-0.0135	0.0187	Small negative
Panel B: Heterogeneous (Retention)						
White women	0.0024	0.0087	0.497	0.0048	0.0175	Null
Non-white women	0.0453	0.0346	0.500	0.0906	0.0692	Moderate positive

Notes: **Country:** United States. **Research question:** Did the first state-level minimum wage laws for women (1912–1920) affect women's labor force retention, industry persistence, or occupational mobility in covered vs. exempt industries? **Policy mechanism:** Fourteen states enacted minimum wage laws specifically targeting women workers in manufacturing, retail, laundry, and hospitality, while exempting domestic service and agriculture; these laws set wage floors that raised the cost of employing women in covered sectors relative to exempt sectors. **Outcome definition:** Primary outcome is labor force retention, a binary indicator equal to one if a woman observed in the labor force in the 1910 Census is also observed in the labor force in the 1920 Census. **Treatment:** Binary: residence in a state that enacted a women's minimum wage law by 1920, interacted with employment in a covered industry in 1910. **Data:** IPUMS MLP linked census panel, 1910–1920, 1,609,942 women in the labor force in 1910 in covered or exempt industries, linked across decennial censuses. **Method:** Triple-difference (state MW status $\times$ covered industry $\times$ time) with state and industry fixed effects; standard errors clustered by state; wild cluster bootstrap inference. **Sample:** Women aged 10+ observed in the labor force in 1910, in manufacturing, retail, hospitality/laundry (covered) or agriculture/domestic service (exempt); linked to 1920 Census via machine learning linkage. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the overall 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