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The Employment Effects of Gender-Specific Minimum Wage

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Stigler Center

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**The Employment Effects of Gender-Specific
Minimum Wage**

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The Employment Effects of Gender-Specific Minimum Wage*

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Abstract

During the 1910's, twelve states passed and implemented the first minimum-wage laws in the history of the United States. These laws were applying to specific industries and only to female employees. This paper studies the employment impact of these gender-specific minimum-wage laws, using full count Census data from 1880 to 1930. We apply a triple-difference strategy exploiting variation across states, industries, and time, to both the full sample of U.S. counties and to the restricted group of contiguous county pairs. We estimate separate models for male and female adults, and find that these laws led to a decrease in female employment and an increase in the employment of adult men. Guided by a simple labor demand setting, we estimate the average elasticity of substitution between male and female labor, and show that the two inputs were, on average, gross substitutes. We provide suggestive evidence of a long-run impact of gender-specific minimum-wage laws on female labor force participation, after the Fair Labor Standards Act.

JEL codes: J16, J23, J31, N3

Keywords: *Minimum wage, labor demand, gender gap*

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1 Introduction

Since prior to the enactment of the first federal level minimum wage in the United States — part of the Fair Labor Standards Act (hereafter, FLSA) of 1938 — the employment effects of minimum-wage laws have sparked heated debates among economists, politicians, and policy makers (Neumark and Wascher, 2007). The abundant existing literature in economics has shown that no indisputable and infallible fundamental law can be defined that unambiguously predicts the effects of a minimum-wage setting.¹ Instead, the answer to the fundamental question of how minimum-wage laws affect employment seems to depend on multiple factors, including, but not limited to, the industry considered in the study, other contemporaneous labor market regulation, and the degree of competition in the labor market.

This paper offers comprehensive empirical evidence on how the setting of a minimum wage affects employment in much less regulated, more competitive labor markets than those in the contemporary United States.² We use the implementation of the first minimum-wage laws in the history of the United States as a unique historical setting to estimate the effect of minimum wage on employment. These laws were predominantly gender-specific in that they were imposing a lower bound only on women’s earnings, and they were passed in selected states, applying only to selected industries. This paper offers the first empirical evidence on the effects of pre-FLSA minimum-wage laws for women, using full count Census data from 1880 to 1930. We identify the impact of minimum-wage laws by employing a triple-difference research design that exploits variation at the state, industry, and decade level, applying it to both the entire universe of U.S. counties and to the restricted group of counties sharing a border, in a contiguous-county research design.

Starting in 1912, 11 U.S. states (Arizona, Arkansas, California, Kansas, Massachusetts, Minnesota, North Dakota, Oregon, Utah, Washington, and Wisconsin) and the District of Columbia passed laws guaranteeing a minimum wage for female laborers.³ In some jurisdictions (CA, KS, MA, ND, and DC), minimum wage decrees covered women only in certain industries. These laws immediately spurred fierce debates, and minimum wage in Washington DC was found unconstitutional by the Supreme Court in 1923. While that court rule slowed down further adoption of the laws in other states, this gender-specific regulation continued to exist until the introduction of the universal federal minimum wage.⁴

The identification of the employment impact of minimum-wage setting has posed several challenges to researchers. First, the universal nature of minimum wages makes it difficult to find a suitable ‘control group,’ forcing researchers to make assumptions related to a particular industry (e.g., restaurants in Leamer et al. (2019)) or geographic unit of interest (e.g., Seattle minimum wage in Jardim et al. (2017)). Second, studies using contemporary data often have to rely on aggregate statistics or may introduce measurement error in the data collection phase (e.g., imperfect response rates in the surveys conducted for the pioneering studies by Katz and Krueger (1992) and Card and Krueger (1994)). Third, if all people in a certain area have the same minimum wage, substitution into illegal employment might be more likely (Bernhardt et al., 2009),

¹What initially started as a primarily theoretical debate about which assumptions are the most suitable to predict and understand the effects of a minimum wage (e.g., Webb, 1912; Filene, 1923; Brown, 1940; Stigler, 1946; Lester, 1947) gradually assumed an empirical nature, thanks to the growing availability of data and policy events. The most recent wave of empirical evidence started at the beginning of the 1990’s with contributions including Holzer et al. (1991), Card (1992), Neumark and Wascher (1992), Card and Krueger (1994), Card et al. (1994), and continued to grow since then. To these days, economic research on the impact of minimum wage on the economy is still extremely active (e.g., Aaronson et al., 2018; Cengiz et al., 2019; Clemens and Wither, 2019; Luca and Luca, 2019; Okudaira et al., 2019).

²American labor markets between 1890 and 1930 were largely unregulated; i.e., there were no unemployment insurance, social security, the National Labor Relations Board, anti-discrimination laws, and many other modern regulatory influences (Fishback, 1998, pp. 722-23).

³The first state to pass such law was Massachusetts in 1912, but the law was not enacted before 1914. According to the Department of Labor’s Bulletin of the Women’s Bureau n. 40, printed in 1924, the first ever minimum-wage law to be enacted was the universal minimum wage for women in Oregon, in 1913.

⁴We provide background information on the time-line and coverage of the gender-specific minimum-wage legislation in Section 2.1.

which would induce non-classical measurement error in the estimates. Finally, the limited availability of longitudinal data on earnings makes it difficult to disentangle the within-worker impact of minimum-wage laws on earnings from a composition effect.

This paper addresses the challenges mentioned above by (i) using high-quality comprehensive data, (ii) exploiting policy changes that allow to identify well defined control groups, and (iii) showing semi-parametric evidence on the effects of minimum wage on earnings through longitudinal observations. First, using full count Census data from 1880 to 1930, we construct an industry-occupation-gender- county-decade panel dataset. Second, after digitizing minimum-wage laws, we link them to industries and states. Then, we use the imposition of gender-specific minimum-wage legislation in 12 U.S. states (including DC) as a labor-market shock and estimate the impact of these laws on employment, separately for adult men and women. Since this was the first minimum-wage legislation, that lifted the minimum wage from zero to a positive level (e.g., \$10 per week in AZ), we estimate our model using both (i) a semi-parametric specification with a binary treatment variable and (ii) the dollar value of the minimum wage for industry-specific minimum-wage legislation as our main variables of interest.

We estimate difference-in-differences and triple-differences regressions using a set of specifications with a variety of fixed effects, the most conservative including county-year, state-industry, state-occupation, industry-year, and occupation-year fixed effects. In our preferred specification, which uses a contiguous-counties research design, the identification strategy relies on comparing within-gender changes in employment in the same industry, across counties in neighboring county-pairs that straddle state borders (hereafter, Contiguous Border County-Pair or CBCP Sample). In the vein of [Dube et al. \(2010\)](#), by focusing only on such pairs, we are able to account for local heterogeneity and local trends in unobservables; in our case unobservables include changes in local demand for female labor force, gender discrimination, and local institutions.⁵ However, our setting differs from one in [Dube et al. \(2010\)](#) in three major ways. First, we can estimate employment separately by gender and industry-occupation. Second, men (and in some cases entire industries) are never treated in states with minimum wages. Third, in a contemporaneous setting, differences between federal and state minimum wages could be small; while, in our study, the absence of a federal minimum wage level implies that we compare states/industries with minimum wage to states/industries without any minimum wage.

We find that, on average, the adoption of minimum-wage legislation decreases employment of women by around 4.5%. At the same time, treated industries observe a 1.3% increase in the employment of men. Our most conservative specification absorbs all county-pair-year specific trends, any state-year specific changes in legislation, as well as any industry trends or state-specific industry factors. Our results are not sensitive to the exclusion of any state or industry, and hold if we include non-occupational groups.⁶

Our main results suggest that there might have been substitution between genders in the labor markets analyzed; although, one should be aware that the results show average effects that might mask heterogeneity in substitutability across industries and occupations. In particular, this result is not necessarily driven by certain industries that at the same time reduce female employment and increase male employment. To investigate this further, we set up a simple labor demand framework in which a representative firm demands labor from men and women according to a Cobb-Douglas production function with a nested constant elasticity of substitution (CES) aggregator. The model gives us an equation relating the gender ratio of labor demand with the relative labor cost. We bring the model-generated equation to the data and, together with calibrated

⁵ The advantages of state border discontinuities for identification are well understood. They have also been used in other contexts, e.g., manufacturing ([Holmes, 1998](#)), banking ([Huang, 2008](#)), suffrage ([Naidu, 2012](#)), or private prisons ([Poyker and Dippel, 2019](#)).

⁶ Throughout the paper we code non-occupational groups as missing, thereby analyzing data on employed individuals only. However, as mentioned, results are not sensitive to this restriction.

wages effects, we use it to identify the elasticity of substitution between male and female labor. First, we find that minimum-wage laws decrease the ratio of female-to-male labor demand by 4.7%. Second, using a set of different calibrated wage changes, we find that, in industries in which both men and women were employed, the elasticity of substitution is greater than one, making men and women, on average, gross substitutes. To our knowledge, this is the first estimate of the gender elasticity of substitution.

As we previously mentioned, earnings effects of a minimum-wage law are difficult to identify. Beyond the mere lack of high quality data on earnings, the absence of any longitudinal information makes it impossible to disentangle an increase in average earnings due to a composition effect (firing of low productivity employees and hiring of new, more productive ones) from an increase due to a simple raise in the wage rate of the same employees. We address this question by using unique longitudinal data from Oregon. Oregon’s Bureau of Labor (Obenauer and von der Nienburg, 1915) collected wage data on a set of women employed both before and after the implementation of the minimum-wage law. We digitized those data and provide semi-parametric longitudinal evidence on the effects of minimum-wage laws on the wages of a sample of 374 employed women in Oregon. In particular, we show that (i) minimum-wage legislation lead to an average wage increase for women who were previously employed at below-minimum-wage levels; (ii) the legislation did not cause any detectable changes, on average, to wages of women who were already earning more than minimum levels required by the newly enacted law.

Finally, we provide suggestive evidence that the prolonged exposure to minimum-wage laws discouraged women from entering the labor force. Using the full count Census of 1940 — the first census collected after a federal minimum-wage law was implemented — we show that the number of years a state had an active gender-specific minimum wage decree is negatively correlated with female labor-force participation in 1940. While the number of years that a state had active minimum-wage laws may be determined in negotiations between states and industry lobbies, we view this variable as exogenous to any variation in local gender-specific employment over time. At the same time, we find no evidence of any long-run impact of these laws on the labor force participation of men. These results hold when we control for pre-minimum-wage trends in labor force participation.

Our findings speak to a large literature on the effect of minimum wage (Neumark et al., 2014; Dube et al., 2016; Aaronson et al., 2018; Azar et al., 2019; Cengiz et al., 2019). However, compared to the existing literature this paper makes two main contributions. First, by studying the first minimum-wage laws in the United States, this paper offers the unique contribution of estimating the effect of a switch from zero to positive lower bound on wage, rather than the effect of a marginal increase in minimum wage. Second, this study focuses on a unique example of minimum-wage legislation in that the policy analyzed is gender-specific and often industry-specific. This variation induced by the decrees allows us to study the effects of minimum wage in the most transparent environment using the universe of the U.S. adult population.

We also contribute to the growing literature on the development of American labor institutions at the beginning of the 20th century (Fishback, 1998; Goldin, 2000; Naidu and Yuchtman, 2016; Farber et al., 2018), and to the literature on female labor outcomes during the same time period (Landes, 1980; Goldin, 1986, 1988, 1994; Naidu, 2012; Poyker, 2019). First, we estimate the impact of one of the most important labor institutions in this country. Second, we document the existence of substitution of women employees by men as a result of states’ economic policy interventions. We argue that the resulting new equilibrium increased the employment gap between men and women, but it may have decreased the earning gap, conditional on employment.

The rest of this paper is organized as follows. Section 2 introduces the history of gender-specific minimum-wage laws in the U.S, describes the novel data, and shows motivating facts. Section 3 discusses our identification strategy. Section 4 presents results, and Section 5 estimates gender elasticity of substitution. Section 6

concludes.

2 Historical Background, Data, and Motivation

2.1 Historical Background

Starting in 1912, several U.S. states introduced a minimum wage for female workers. The most accepted reason for the enactment of these laws was that the majority of women could not afford to satisfy their basic needs with the current wage levels. For example, the [Kansas Industrial Welfare Commission \(1917\)](#) surveyed 5,436 women employees and found that 31% of them earned below \$6 per week, thus concluding that “they hardly have enough to sustain life.”⁷ The highest lower bound to wage rates was set in North Dakota at \$20 per week for women working in office occupations. The Women’s Bureau of Labor monitored the effect of minimum-wage laws on earnings ever since they were first passed, and reported that these laws were effective in raising the salary for low-skilled women (e.g., [Obenauer and von der Nienburg, 1915](#); [Massachusetts Minimum Wage Commission, 1916](#)). These reports also concluded that there was no evidence of lower employment levels for women as a result of the setting of a minimum wage, although we have not found any statistics that would back these claims.⁸

By 1920, all 12 states (see [Figure 1](#)) have adopted their minimum-wage-related laws.⁹ Arizona, Minnesota, Oregon, Utah, Washington, and Wisconsin, eventually, adopted minimum-wage laws covering women in all industries, while Arkansas, California, District of Columbia, Kansas, Massachusetts, and North Dakota implemented minimum-wage laws only to selected industries.¹⁰ See [Online Appendix Table 1](#) for the complete list of minimum-wage laws by industry and year of adoption. Penalties were put in place in all the states that would punish employers failing to comply with these laws. The penalty was either a fine or imprisonment. The Women’s Bureau of the ([Department of Labor, 1928](#), Chapter XII) describes the enforcement of these laws, the penalties, and the methods and results of investigations.¹¹

Almost immediately after the implementation of the first law by the state of Oregon, manufacturers started to oppose minimum wage. This opposition created legal disputes which resulted in the first Supreme Court case, *Settler v. O’Hara*, in 1917. With a tie (4:4), the Supreme Court upheld Oregon’s minimum wage ([McKenna and Zannoni, 2011](#)). However, opponents of minimum wage continued in their crusade that culminated in the Supreme Court case against District of Columbia’s minimum-wage law. With a 5-to-3 vote, the Supreme Court of D.C. struck down its minimum-wage law in *Adkins v. Children Hospital* in 1923 and deemed it unconstitutional under the due process clause of the Fifth Amendment.¹²

After *Adkins v. Children Hospital*, minimum-wage laws were abolished in Arizona (1925), Arkansas (1927), California (1925), Kansas (1925), Utah (1929), and Wisconsin (1924). However, in Massachusetts, Minnesota, North Dakota, Oregon, and Washington these laws existed until they become obsolete with

⁷The number of employed women that earned below \$6 in other states are: 9% in Oregon in 1912, 21% in Ohio in 1913, and 22% in Michigan in 1913 ([Thies, 1990](#), p. 724).

⁸For example, [Wisconsin Industrial Commission \(1921, p. 65\)](#) said that “... there has also been no reduction of opportunities for employment of women” without providing any data to prove their point.

⁹With the exception of four industries in California (1922) and Massachusetts (1924, 1925, and 1927).

¹⁰Colorado, Nebraska, South Dakota, and Texas also imposed minimum-wage legislation but they have never enforced them; as a result they were ineffective ([Department of Labor, 1927](#)). Puerto Rico also adopted a gender-specific minimum wage in 1919 but we exclude it from our analysis because it does not have border-states.

¹¹States allowed sub-minimum wages for (i) inexperienced (less than a year of experience) female workers (generally their wages were \$1 less than minimum wages); and (ii) “slow” workers ([Department of Labor, 1928](#), pp. 278-79.). However, states required employer to receive official confirmation that a particular worker was not productive enough (“slow”), and very few of these licenses were issued (Washington issued 50, DC issued 87, and California issued a maximum of 2,400 licenses for substandard workers ([Thies, 1990](#), p. 740)).

¹²See [McKenna and Zannoni \(2011\)](#) for additional legal details.

the imposition of universal federal wage in 1938. [Online Appendix Table 2](#) summarizes the timing of implementation and abolition of the minimum-wage laws.

Due to the attention these laws attracted in the states that adopted them, the Bureau of Labor and local industrial commissions published reports that were trying to evaluate their effects on earnings and employment. The reports by the Bureau of Labor — summarized in [Thies \(1990\)](#) — surveyed women and firms and concluded that the laws were efficient in raising their wages and did not result in women losing their jobs. Reports made by non-governmental industrial commissions (e.g., [Merchants and Manufacturers Massachusetts \(1916\)](#) investigating the effect of minimum-wage laws in Massachusetts’ brush industry) were more likely to note both an increase in wages and a decrease in women’s employment.

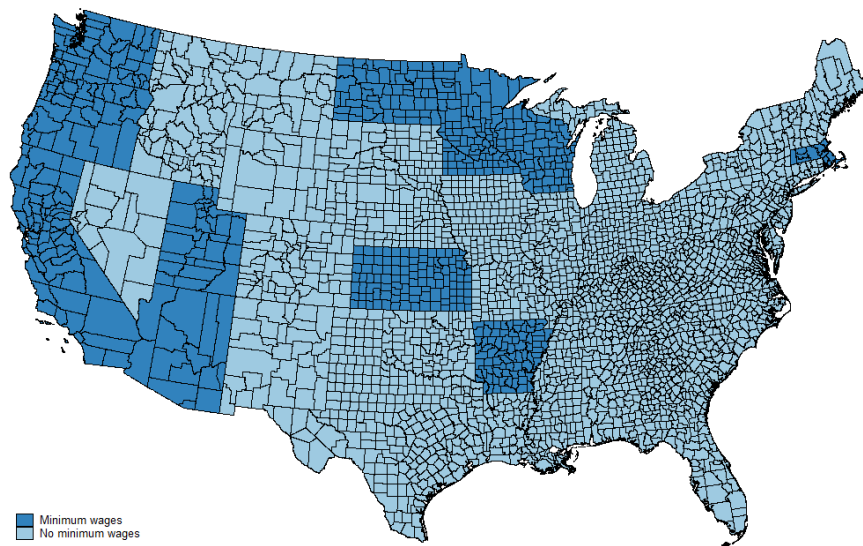


Figure 1: Map of female-specific minimum-wage laws. Dark blue color shows states with minimum-wage laws: Arizona, Arkansas, California, Kansas, Massachusetts, Minnesota, North Dakota, Oregon, Utah, Washington, Wisconsin, and the District of Columbia. Light blue color shows states that don’t have minimum-wage laws.

Although most modern economists are familiar with the empirical studies by [Neumark and Wascher \(1992\)](#), [Card and Krueger \(1994, 1995\)](#), and the strand of literature they inspired, the academic debate on the minimum wage dates back to the first half of the 20th Century, when the available evidence on the first minimum wage experiments — including the first at the federal level, part of the FLSA of 1938 — was discussed. On the one hand, [Lester \(1941\)](#) studying women’s minimum-wage laws concluded “that minimum-wage regulation has not caused a relative reduction in the level of employment for women, and that there has been no widespread tendency for men to replace women as a result of raising women’s wages by law.” On the other hand, [Peterson \(1959\)](#) went against what he perceived as common wisdom among policy analysts and labor economists at the time, that minimum wages had essentially no employment effects, and, using three pre-FLSA case studies of minimum-wage laws for women, claimed that minimum-wage laws did decrease women’s employment. However, overall, pre-FLSA minimum-wage laws have never been empirically analyzed beyond the case-study approach adopted by [Lester \(1941\)](#) and [Peterson \(1959\)](#), partly because of extremely scarce data availability.

2.2 Data

The data on minimum-wage laws comes from the U.S. Department of Labor Statistics. The Women’s Bureau published ([Department of Labor, 1924, 1927, 1928, 1937a, 1939](#)) a list of laws related to employment

of women that we collected and read. We summarized these laws in the [Online Appendix Table 1](#) and the [Online Appendix Table 2](#).¹³

To measure the impact of minimum wage enactment on employment we use full count census data from 1880, 1900, 1910, 1920, and 1930 ([Ruggles et al., 2019](#)). We construct a panel dataset of gender-industry-occupation-county-decade cells containing the number of individuals in each cell. Then we count the number of observations in each gender-county-decade cell, and use the ratio of employed in each gender-industry-occupation-county-decade cell over the total in each gender-county-decade cell as the primary left hand side variable of interest. We discuss the empirical model and the construction of variables of interest in more detail in [Section 3](#) below.

2.3 Two Empirical Facts About Gender-Specific Minimum Wage

Fact 1: Effects on Wages: Evidence from Oregon’s Longitudinal Wage Data

The Bureau of Labor studied the effects of minimum-wage laws on women’s outcomes since the introduction of these laws (e.g., [Department of Labor, 1928](#)). Nevertheless, data availability on wages between the 19th and the 20th is scarce. For this reason it is hard to estimate the impact of minimum-wage laws on wage levels for the entire country. Here we utilize unique longitudinal data collected for the one of the first minimum wage studies by [Obenauer and von der Nienburg](#) in Oregon in 1915.¹⁴

The Bureau of Labor Statistics published in 1915 a report featuring data collected to study the impact of minimum-wage legislation in Oregon. Among the variety of different data and statistics, they collected data on wages for a sample of around 370 women across the state with longitudinal information about their wage levels before and after the enactment of the law.

These data allow for a within-worker semi-parametric analysis of the impact of minimum-wage laws on wage levels. We plot the weekly wage level after the imposition of a minimum wage as a function of the wage before, and compare the resulting curve with a 45 degrees line, which represents the locus where the empirical curve would lie if wages were constant for each wage rank. What we observe in [Figure 2](#) is that all women with pre-legislation wages below the highest newly implemented minimum wage level (\$9.25 weekly, in Portland) show an increase in weekly earnings, while the wage level is almost unchanged for workers with pre-legislation earnings above the highest minimum wage. This result provides strong evidence that, at least in Oregon, the cost of labor increased but only for employees for whom minimum-wage laws were binding.¹⁵

Fact 2: Effects on Employment

While we estimate the causal impact of gender-specific minimum-wage laws on employment in [Section 4](#), here we provide factual records from sources collected by local statistical bureaus and industrial commissions during the period around the implementation of these laws.

Mary Elizabeth Pidgeon, a research economist for the U.S. [Department of Labor](#) Women’s Bureau said:

¹³We only codified laws that were enacted up to 1930. In a few cases the dollar value of the minimum wage changes several times between Censuses. When we compute dollar-value measure of the minimum wage, we use the first implemented minimum wage in such cases, because we want to capture the effect of moving from zero to non-zero minimum wage. Because these changes are very small all our results are almost identical if we use minimum-wages active at the time of the 1920 and 1930 censi, or if we use weighted by years wages. Results are available upon request.

¹⁴These data were analyzed in [Kennan \(1995\)](#). The main conclusion drawn from [Kennan](#)’s analysis was that in the majority of the cases observed, wages remained unchanged after the implementation of minimum-wage legislation.

¹⁵Other examples (albeit non-longitudinal) and of the effectiveness of the minimum-wage laws on raising the wages of females can be found in ([Thies, 1990](#), pp. 727–735). For example, [Thies](#) analyzes the case of wage increase in Brush industry in 1911–1914 in Massachusetts.

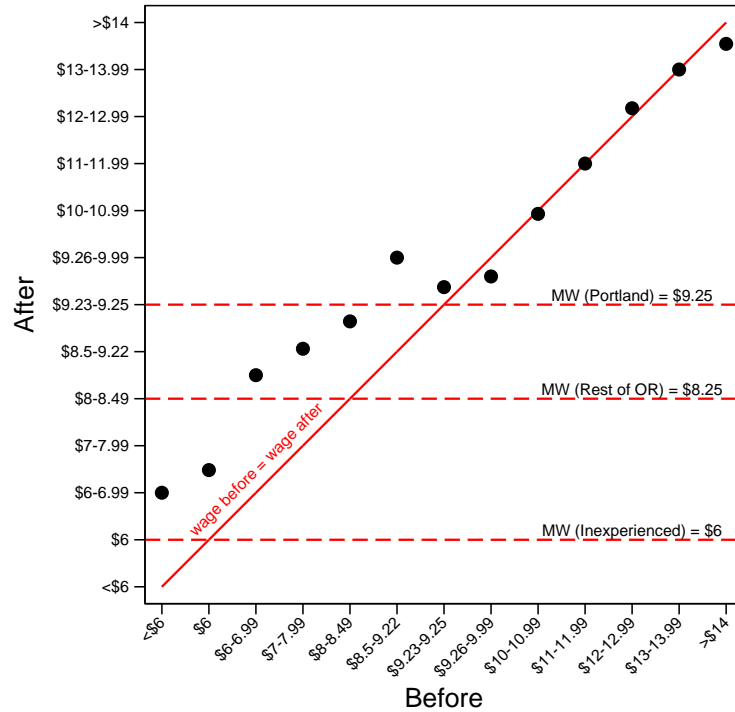


Figure 2: Changes in the weekly rate in Oregon before and after minimum wage determination, for 374 women interviewed by the Bureau of Labor Statistics. We do not observe whether each particular woman is located in Portland or in other Oregon’s location.

The universal experience with minimum-wage legislation [...] is that it had materially raised the wages [...] of women. [...] In regards to women’s employment, the usual experience has been that it continue to increase regardless of whether or not there is a minimum-wage legislation. (Department of Labor, 1937a, pp. 8–9)

As can be seen in Online Appendix Figure 1 female employment in affected industries in treated states appears to be growing at a comparatively faster pace before the implementation of the decrees. Thus, the statement above may be entirely explained by pre-enactment trends in the data.

On the other hand the economics literature at the time noted that these minimum-wage laws may adversely affect female employment. E.g., Taussig (1916) states:

Higher wages for the unskilled women are likely to lead to more or less replacement by men, skilled or unskilled.

Similarly, non-governmental industrial commissions documented the negative effect of minimum-wage laws on female employment. Merchants and Manufacturers Massachusetts (1916), for instance, describes the following cases:

[Exhibit 1] The owner [...] of one of the largest stores in Massachusetts, situated in Boston, personally stated to the writer a few weeks prior to the going into effect of the Minimum Wage Decree in his store that on one floor alone he should discharge fifty-five girls, solely because of the law itself.

[*Exhibit 5*: A letter from another large Boston department store, 1916] “We have severed connection with about fifty employees since the Minimum Wage went into effect. You are correct in assuming that the reason for our severing connection with the fifty employees mentioned was the Minimum Wage law itself.”

Women who were fired were predominantly the lowest-skilled or the most inexperienced (Stecker, 1927, p. 140).¹⁶ At the same time, the employment of skilled women was not affected to the same degree (Stecker, 1927, pp. 174–184). Since Sundstrom (1988) presents evidence that firms in the 19th century U.S. relied on internal promotion to fill skilled positions, which suggests that workers acquired valuable firm-specific skills and firm-specific human capital on the job, we would expect that minimum-wage laws discouraged young women from getting jobs and accumulating skills. Consistent with this framework, we provide suggestive evidence on the long-run effect of female-specific minimum-wage laws on female labor force participation later in Section 6.

3 Identification in the Border-County-Pair Setup

3.1 Sample Construction

Contiguous county-pairs form the best treatment-control comparison because counties within these pairs are the most comparable in terms of local conditions that can affect labor demand. Thus, our preferred sample consists of only contiguous counties that straddle the state border. Our 12 states with gender-specific minimum wages (hereafter, we refer to the District of Columbia as a state) have 24 unique adjacent states. They cover 701 border counties in 419 distinct county-pairs. Figure 3 shows a map of the contiguous counties included in the analysis. Counties located in minimum wage states are colored in dark blue, and those located in non-minimum wage states are colored in light blue. Table 1 provides a more detailed breakdown of the number of pairs on each of 42 state border-segments and clarifies how many segments are linked to each state.¹⁷

Utah, for example, is one of the most ‘connected’ states in our data, sharing border-segments with six states (segments # 11, 34, 35, 36, 37, and 38). Utah adopted a universal female minimum wage (i.e., in all industries). Therefore, by comparing Utah and Colorado (segment #34) we will utilize variation in all industries. Utah shares segment #11 with Arizona, which also has a minimum-wage law for women in all industries. Thus, this segment will not generate any variation for the specification with the dummy variable. However, it will provide variation in a specification with a dollar value because weekly minimum wage in Utah is equal to \$7.5 and in Arizona it is equal to \$10.

Similarly, segment #13 that is shared by California and Oregon will provide identifying variation since, while Oregon has a universal minimum wage law for women, Californian minimum-wage laws cover only a subset of industries (see Online Appendix Table 1 for details). In segment #28, both Minnesota and Wisconsin have minimum-wage laws that cover women in all industries; however, Wisconsin abolished the law in 1924 (see Online Appendix Table 2), thus while this segment does not contribute to the identification in 1920, it generates identifying variation in 1930, when Minnesota’s side of the border segment is treated and Wisconsin’s side is not.¹⁸

¹⁶Also see Merchants and Manufacturers Massachusetts (1916); Peterson (1959); Peterson and Stewart (1969).

¹⁷We define a border segment as the set of all counties on both sides of a border between two states.

¹⁸All segments generate variation for the dollar value specification; two segments (#11 and #33) do not contribute to the identification for the binary-variable specification i.e., if both states have adopted minimum-wage laws for all industries, these border-segments are, essentially, dropped.

Table 1: Contiguous-Border County-Pairs

| Segment | Pairs | | # counties | | | Types of min.wage laws | | Avg. weekly min. wage, \$ | | # periods when laws are active | |
|---------|-------|----|------------|----|--------|------------------------|------|---------------------------|------|--------------------------------|---|
| | 1 | 2 | 1 | 2 | #pairs | 1 | 2 | 1 | 2 | 1 | 2 |
| 1 | AR | LA | 6 | 8 | 14 | ind. | no | 13.3 | 0 | 1 | 0 |
| 2 | AR | MO | 12 | 11 | 22 | ind. | no | 13.3 | 0 | 1 | 0 |
| 3 | AR | MS | 5 | 6 | 10 | ind. | no | 13.3 | 0 | 1 | 0 |
| 4 | AR | OK | 8 | 5 | 12 | ind. | no | 13.3 | 0 | 1 | 0 |
| 5 | AR | TN | 2 | 4 | 6 | ind. | no | 13.3 | 0 | 1 | 0 |
| 6 | AR | TX | 2 | 2 | 3 | ind. | no | 13.3 | 0 | 1 | 0 |
| 7 | AZ | CA | 2 | 3 | 4 | all | ind. | 10 | 11.8 | 1 | 1 |
| 8 | AZ | CO | 1 | 1 | 1 | all | no | 10 | 0 | 1 | 0 |
| 9 | AZ | NM | 3 | 6 | 8 | all | no | 10 | 0 | 1 | 0 |
| 10 | AZ | NV | 1 | 2 | 2 | all | no | 10 | 0 | 1 | 0 |
| 11 | AZ | UT | 4 | 3 | 6 | all | all | 10 | 7.5 | 1 | 1 |
| 12 | CA | NV | 10 | 7 | 17 | ind. | no | 11.8 | 0 | 1 | 0 |
| 13 | CA | OR | 3 | 5 | 7 | ind. | all | 11.8 | 8.3 | 1 | 2 |
| 14 | DC | MD | 1 | 2 | 2 | ind. | no | 15.9 | 0 | 1 | 0 |
| 15 | DC | VA | 1 | 3 | 3 | ind. | no | 15.9 | 0 | 1 | 0 |
| 16 | KS | CO | 7 | 6 | 12 | ind. | no | 8.8 | 0 | 1 | 0 |
| 17 | KS | MO | 10 | 12 | 21 | ind. | no | 8.8 | 0 | 1 | 0 |
| 18 | KS | NE | 12 | 13 | 26 | ind. | no | 8.8 | 0 | 1 | 0 |
| 19 | KS | OK | 14 | 13 | 26 | ind. | no | 8.8 | 0 | 1 | 0 |
| 20 | MA | CT | 3 | 4 | 6 | ind. | no | 11.4 | 0 | 2 | 0 |
| 21 | MA | NH | 4 | 3 | 6 | ind. | no | 11.4 | 0 | 2 | 0 |
| 22 | MA | NY | 1 | 3 | 3 | ind. | no | 11.4 | 0 | 2 | 0 |
| 23 | MA | RI | 5 | 1 | 5 | ind. | no | 11.4 | 0 | 2 | 0 |
| 24 | MA | VT | 2 | 2 | 3 | ind. | no | 11.4 | 0 | 2 | 0 |
| 25 | MN | IA | 9 | 11 | 19 | all | no | 8.0 | 0 | 2 | 0 |
| 26 | MN | ND | 6 | 6 | 12 | all | ind. | 8.0 | 15.5 | 2 | 2 |
| 27 | MN | SD | 7 | 6 | 14 | all | no | 8.0 | 0 | 2 | 0 |
| 28 | MN | WI | 7 | 7 | 19 | all | all | 8.0 | 11 | 2 | 1 |
| 29 | ND | MT | 6 | 6 | 11 | ind. | no | 15.5 | 0 | 2 | 0 |
| 30 | ND | SD | 8 | 8 | 16 | ind. | no | 15.5 | 0 | 2 | 0 |
| 31 | OR | ID | 3 | 6 | 9 | all | no | 8.3 | 0 | 2 | 0 |
| 32 | OR | NV | 3 | 2 | 4 | all | no | 8.3 | 0 | 2 | 0 |
| 33 | OR | WA | 10 | 10 | 20 | all | all | 8.3 | 9.9 | 2 | 2 |
| 34 | UT | CO | 4 | 8 | 12 | all | no | 7.5 | 0 | 1 | 0 |
| 35 | UT | ID | 3 | 3 | 7 | all | no | 7.5 | 0 | 1 | 0 |
| 36 | UT | NM | 1 | 1 | 1 | all | no | 7.5 | 0 | 1 | 0 |
| 37 | UT | NV | 7 | 3 | 9 | all | no | 7.5 | 0 | 1 | 0 |
| 38 | UT | WY | 3 | 4 | 5 | all | no | 7.5 | 0 | 1 | 0 |
| 39 | WA | ID | 4 | 6 | 9 | all | no | 9.9 | 0 | 2 | 0 |
| 40 | WI | IA | 3 | 3 | 5 | all | no | 11 | 0 | 1 | 0 |
| 41 | WI | IL | 5 | 6 | 11 | all | no | 11 | 0 | 1 | 0 |
| 42 | WI | MI | 5 | 4 | 11 | all | no | 11 | 0 | 1 | 0 |
| Total | 36 | | 419 | | | | | | | 58 | |

Notes: This table decomposes the sample of 419 border-counties into 42 state-border-segments. The table clarifies how many border-segments are linked to each state, and which segments are dropped when a state is dropped from the analysis, as the robustness check reported in Figure 4 will do. The table also visualizes states' average minimum wages across industries with the minimum wage (or all-state minimum wages).

While the advantages of border-discontinuities in terms of statistical identification (which are discussed in more detail in Section 3.2) do not really depend on this, the generalizability of the results will be higher if the border counties are representative of all counties in a state on observable characteristics. To confirm

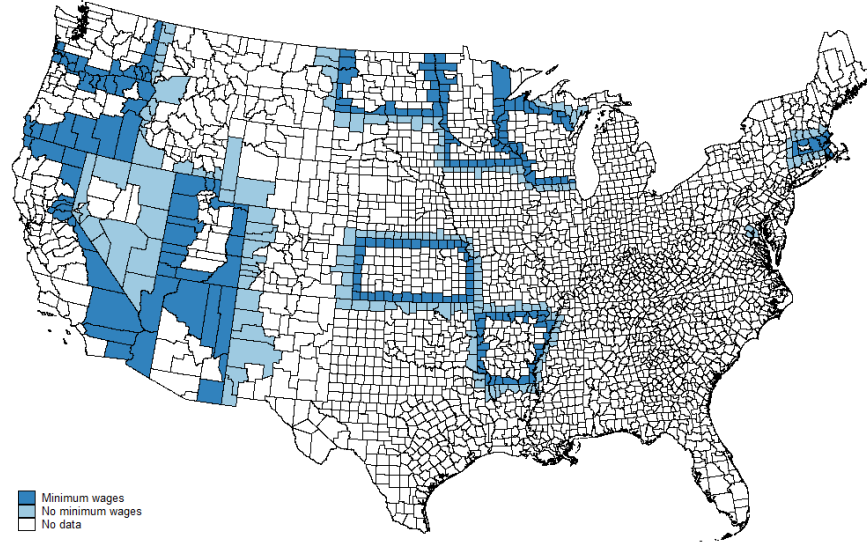


Figure 3: Contiguous-Border County-Pairs in our Sample

Table 2: Border-County Balance Table

| | I | | II | | III | |
|---|-------------------|-----------|---|-----------|-------------|---------|
| | All-County Sample | | Contiguous Border County-Pair Sample | | Differences | |
| | Mean | s.d. | Mean | s.d. | Mean | P-value |
| <u>County Controls (1920):</u> | | | | | | |
| Population | 118,437 | (300,947) | 139,626 | (393,022) | 21,189 | [0.613] |
| # prime age adults | 70,930 | (187,088) | 84,282 | (243,564) | 13,352 | [0.606] |
| Ratio of employed women to employed men | 1.052 | (7.631) | 1.088 | (8.138) | 0.036 | [0.375] |
| Share Black | 0.018 | (0.009) | 0.019 | (0.010) | 0.001 | [0.160] |
| Share literate | 0.733 | (0.076) | 0.744 | (0.066) | 0.011 | [0.175] |
| Share rural | 0.604 | (0.317) | 0.589 | (0.328) | -0.015 | [0.645] |
| Share women | 0.006 | (0.003) | 0.006 | (0.003) | 0.000 | [0.304] |
| Labor-force participation | 27.4 | (447.5) | 29.0 | (554.8) | 1.55 | [0.688] |
| # of counties | 3,065 | | 701 | | | |
| # of county(-pair)-ind.-occ. observations | 1,470,617 | | 329,176 | | | |

Notes: This table shows that the border-county sample of the 36 states is representative of the full sample of counties in the United States. Here, an observation is a county(-pair) industry-occupation in 1920.

that this is the case, Table 2 provides summary statistics on the socioeconomic characteristics of the border counties in column-set I. Column-set II reports the same for all other counties in the full sample of states and counties. Reassuringly, column-set III confirms that border counties are representative of counties in their states more broadly, as we cannot reject the null that the difference between the two samples is zero at any conventional significance level.

3.2 Empirical Specification

We start by first estimating the effect of minimum wage on the full sample of all the counties in the United States. The specification is as follows:

$$\ln \left(EmpShare_{gic(s)t} \right) = \beta \cdot \mathbb{1}Minimum\ wage_{ist} + \mu_{st} + \Psi_{ct} + \Phi_{is} + \Phi_{it} + \epsilon_{gic(s)t}, \quad g = \{m, w\} \quad (1)$$

where the unit of observation is an industry-occupation i , in county c , nested within state s , in decade t . We estimate the specification separately for men and women.

Following the previous literature (e.g., [Neumark et al., 2014](#)), our dependent variable of interest is the logarithm of the size of employment in a certain industry relative to the total adult population within a given location and time period:

$$\ln \left(EmpShare_{gic(s)t} \right) = \ln \left(\frac{\#employed_{gic(s)t}}{\#total_{gc(s)t}} \right),$$

where i refers to industry-occupation groups, c is a county in state s , and t is a decade. The variable is naturally computed once for each gender g .

We use two measures of the explanatory variable of interest. The first — $\mathbb{1}(\text{Minimum wage})_{ist}$ — is an indicator variable, equal to 1 if the industry-occupation i in state s at year t has a minimum-wage legislation, and zero otherwise. Thus the coefficient should be interpreted as a percentage change in employment after introducing minimum wage. The second, — Minimum wage, $\$_{ist}$ — is a dollar value of the minimum wage (or zero if there is no minimum wage) in industry-occupation i in state s at year t . Here, the coefficient should be interpreted as a percentage change in employment after increasing minimum wage by one dollar. Men are always treated if women are treated. In this manner, we address the fact that in equilibrium they were not a ‘control group’, and they were instead treated by female-specific minimum-wage legislation in their state-industry.

μ_{st} are state-specific time controls, Ψ_{ct} are county-decade fixed effects. Violations of minimum-wage laws were not unusual and there is reason to believe that there was heterogeneity in law enforcement and penalties across states (Women’s Bureau of the [Department of Labor, 1928](#)). State- and county-decade fixed effects allow us to absorb location-specific trends in law-enforcement.

Φ_{is} , and Φ_{it} are industry-state and industry-decade fixed effects. The former address possible state-specific support to certain industries, or nationwide trends in some industries (e.g., technological progress). The latter absorbs industry-specific trends.

The coefficient β essentially represents a difference-in-differences-in-differences estimator, since treatment is administrated at the state-year level, but only a subset of industries is affected.

In the full sample specification, we double-cluster standard errors by state and industry/occupation ([Cameron et al., 2011](#)).

We also estimate the following empirical specification for the CBCP sample:

$$\ln \left(EmpShare_{gip(c)t} \right) = \beta \cdot \mathbb{1}Minimum\ wage_{ist} + \mu_{st} + \Psi_{p(c)t} + \Phi_{is} + \Phi_{it} + \epsilon_{gip(c)t}. \quad g = \{m, w\} \quad (2)$$

Here, only contiguous county-pairs that straddle state-border can contribute to the identification of β . In the specification in (2), this is reflected in the presence of county-pair-specific time fixed effects, $\Psi_{p(c)t}$.

While the specification in (1) is able to control for a verity of location and industry trends, the identifying variation still relies on comparing (after absorbing fixed effects) a county-industry cell in (e.g.,) North Dakota

to a county-industry cell in Pennsylvania. [Dube et al. \(2010\)](#) provides a set of compelling reasons for focusing on county pairs across bordering states when identifying the effect of state-level changes in minimum wages. Primarily, what this sample selection achieves is to better control for local trends. In our setting, this means trends in local demand for female-labour force, gender discrimination, and the evolution of labor local institutions. At the same time not-industry-specific legislative trends, which are not local, will still be absorbed by state-decade fixed, μ_{st} , effects and by county-pair-decade fixed effects, $\Psi_{p(c)t}$, in our preferred specification.

The presence of a single county in multiple pairs along a border segment induces a mechanical correlation across county-pair, and along an entire border segment. To account for these sources of correlation in the residuals, we triple-cluster standard errors by state, industry, and border-segment level.¹⁹

4 Results

Panel A of [Table 3](#) contains results for female employment. Columns I–V present the results of the estimation of [equation \(1\)](#), using the full sample of U.S. counties. Specifications get incrementally more demanding across columns: Column I reports results for the specification with (time-invariant) county fixed effects Ψ_c , as well as industry Φ_i and year fixed effects μ_t .²⁰ Column II adds state-specific industry and occupation fixed effects Φ_{is} , absorbing all state-specific institutional factors that may affect development of certain industries. Column III includes state-year fixed effects that absorb any changes in state-level legislation that may affect all industries. Column IV allows industries and occupations to be on separate time trends by adding industry- and occupation-year fixed effects Φ_{it} . This specification is equivalent to a triple-differences specification. Finally, Column V replaces state-year fixed effects with county-year fixed effects. The inclusion of the aforementioned fixed effects does not substantially affect the coefficient on $\mathbb{1}$ (Minimum wage) and it remains marginally insignificant even when we introduce more restrictive local time trends in Column V.

Columns VI–VII present the results of the estimation of [equation \(2\)](#), using only counties that straddle state-boundaries. Column VI has the same set of the fixed effects as in Column IV except that it has county-pair fixed effects $\Psi_{p(c)}$, instead of county fixed effect. Column VII repeats specification in Column V except with county-pair-year $\Psi_{p(c)t}$ fixed effects instead of county-year fixed effects. The estimate in Column VII means that introducing minimum wage for women decreases female employment in that industry by 4.5 percent.

Following [Dube et al. \(2010\)](#), our preferred specification is the one using the CBCP sample. This is because with that empirical setting we are able to control for local unobserved trends that could correlate with female employment, in particular, female discrimination. We would expect that areas that passed laws with the goal of protecting women would be less inclined to discrimination than areas that did not introduce any minimum wage for women. This would imply the risk of attenuating the negative effect of a minimum wage, which would be confounded with an improvement in women’s rights in minimum wage areas. By concentrating on pairs of counties that have similar trends in attitudes towards working women we avoid this bias. The coefficient on $\mathbb{1}$ (Minimum wage) keeps the same sign but increases two-fold from Columns IV and V to Columns VI and VII, respectively, and becomes highly significant.

The second row of the Panel A contains estimates from the same specifications but uses dollar value of minimum wage as an explanatory variable. The size of the Minimum wage, $\$_{ist}$ estimate is moving in the same way as the dummy across the specifications. Using the estimate from our preferable most conservative

¹⁹All results hold if in a full sample we cluster by state or double-cluster by state and border-segment. Results available upon request.

²⁰Raw data results (without fixed effects) can be seen in [Online Appendix Figure 2](#).

Table 3: The Effect of Minimum-Wage Legislation on Employment, by Gender

| | I | II | III | IV | V | VI | VII |
|--|--|----------------------|--------------------|---------------------|---------------------|----------------------|----------------------|
| Panel A: Women | | | | | | | |
| Sample | Dependent variable: Log employment share (women) | | | | | | |
| | All | | | | CBCP | | |
| 1(Minimum wage) | -0.017 (0.015) | -0.034* (0.018) | -0.035 (0.022) | -0.024 (0.016) | -0.022 (0.015) | -0.050*** (0.008) | -0.045*** (0.008) |
| Minimum wage, \$ (mean min wage \$10.2) | -0.002* (0.001) | -0.003 (0.002) | -0.001 (0.002) | -0.000 (0.001) | -0.000 (0.001) | -0.003** (0.001) | -0.003** (0.001) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.644 | 0.672 | 0.673 | 0.689 | 0.697 | 0.734 | 0.740 |
| Observations | 1,363,979 | 1,363,979 | 1,363,979 | 1,363,979 | 1,363,979 | 272,397 | 272,397 |
| Panel B: Men | | | | | | | |
| Sample | Dependent variable: Log employment share (men) | | | | | | |
| | All | | | | CBCP | | |
| 1(Minimum wage) | -0.102*** (0.026) | -0.102*** (0.028) | -0.032* (0.017) | 0.011*** (0.004) | 0.009*** (0.003) | 0.009** (0.004) | 0.013*** (0.001) |
| Minimum wage, \$ (mean min wage \$10.2) | -0.009*** (0.002) | -0.009*** (0.003) | -0.002 (0.002) | 0.001*** (0.000) | 0.001*** (0.000) | 0.001 (0.001) | 0.001*** (0.000) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.559 | 0.581 | 0.583 | 0.598 | 0.608 | 0.647 | 0.654 |
| Observations | 4,139,089 | 4,139,089 | 4,139,089 | 4,139,089 | 4,139,089 | 801,903 | 801,903 |

Notes: This table reports on the baseline results from estimating equation (1) (Columns I–V) and equation (2) (Columns VI and VII). Each row contains coefficients from separate regressions for $\mathbb{1}$ (Minimum wage) and Minimum wage in dollars_{ist} as explanatory variables. Panels A and B estimate the same models for women and men, respectively. Each observation is a county-industry-occupation-year-sex cell. Observations in Panel B are much more than in Panel A because many cells show no employed women. Columns I–V are estimated on the full sample of U.S. counties. Column I reports includes only (time-invariant) county, industry, and occupation fixed effects. Column II adds industry-state and occupation-state fixed effects. Column III adds state-year fixed effects. Column IV adds industry-year and occupation-year fixed effects. Column V replaces county and state-year fixed effects with county specific year fixed effects. Columns VI and VII are estimated on the CBCP sample. Column VI replicates specification in Column IV but uses county-pair fixed effects instead of county fixed effects. Column VII replicates specification in Column V but uses county-pair specific year fixed effects instead of county-year fixed effects. In Columns I–V standard errors are double-clustered on state (49) and industry-occupation (6,965) level. In Columns VI and VII standard errors are triple-clustered on state (36), industry-occupation (4,714), and border-segment level (42). Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

specification in Column VII we can evaluate the effect of minimum wage. Moving from no minimum wage to the mean minimum wage \$10.2 (among state-industries that had adopted it) female employment share declines by 3.2%. This coefficient is remarkable close to our dummy estimate of 4.5%.²¹

To demonstrate that our results are not driven by some specific state, Panel A of Figure 4 reports on the robustness of our preferred estimate in Column VII in Panel A to dropping one state at a time. The estimated coefficient always remains significantly different from zero. Dropping Massachusetts, which shares

²¹Online Appendix Table 4 reports result for the specification with log of minimum wage; its coefficients can be interpreted as elasticities. However, we prefer to use dollar values as a baseline because most of our changes are between zero and non-zero. Although we use inverse hyperbolic sin (see footnote 29) to keep observations with zero minimum wage, this specification would be more preferable in contemporaneous cases with pre-existing minimum wages.

border-segments with five states (segments # 20, 21, 22, 23, 24 in Table 1), has almost no effect on the coefficient. Dropping California, which shares border-segments with three states (segments # 7, 12, 13), decreases the coefficient the most, from -0.045 to -0.049 . Dropping District of Columbia, which shares border-segments with two states (segments # 14, 15), increases the coefficient the most, from -0.045 to -0.043 .

Similarly, in Panel A of Online Appendix Figure 4 we show the robustness of our preferred estimate in Column VII in Panel A to dropping one industry at a time. The estimated coefficient always remains significantly different from zero. Dropping manufacturing non-durable goods decreases the coefficient the most, from -0.045 to -0.062 . Dropping retail industry increases the coefficient the most, from -0.045 to -0.032 .²²

In Panel B of Table 3, we estimate the effect of female minimum-wage laws on male laborers. The coefficient of $\mathbb{1}(\text{Minimum wage})$ remains negative until inclusion of the industry- and occupation- trends in Column IV.²³ After that, the coefficient remains consistently positive and significant. In our most preferred (and most restrictive) specification in Column VII, the coefficient implies that introducing a minimum wage for women increased male employment in that industry by 1.3 percent. Similarly, the second row of the Panel B reports coefficients for Minimum wage, \$. A \$10 increase in female weekly minimum wage increases male employment by 1%. In Panel B, we do not observe any jump in the coefficients of $\mathbb{1}(\text{Minimum wage})$ and Minimum wage, \$ when we move from the full sample to the CBCP sample supporting our hypothesis, that trends in female labor discrimination are the source of omitted variable in the full sample specification.

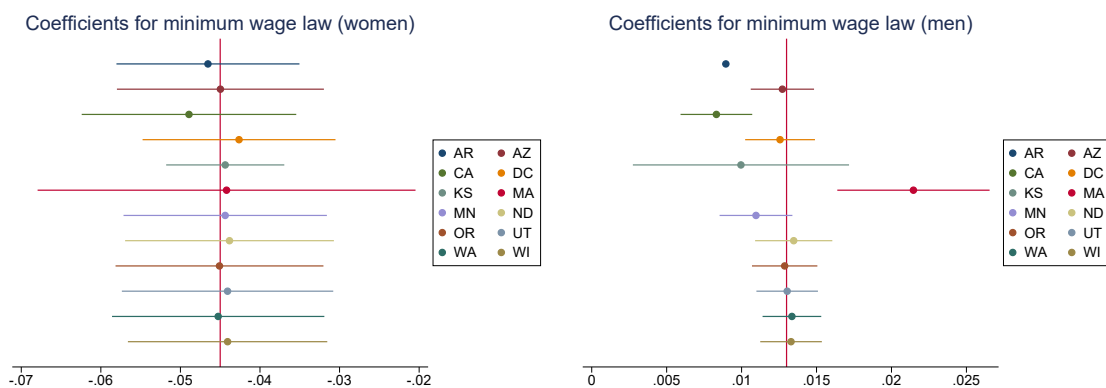


Figure 4: State-Exclusion Robustness of the Results for $\mathbb{1}(\text{Minimum wage})_{ist}$ in Table 3

Notes: This figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the core specification in Column VII of Table 3, dropping one state at a time. The left figure shows results for women. The right figure shows results for men. One dropped state may imply dropping several state-border-segments, See Table 1. The (red) vertical line is the baseline point estimate. The results are sorted top-to-bottom in alphabetical order, i.e., omit AR, then AZ, then CA, etc.

To provide additional insights about the economic magnitude of the impact of minimum-wage laws on men and women employment, we look at it through the lens of general equilibrium, in a back-of-the-envelope calculation. Between 1910 and 1920, in treated states, male employment-to-population ratio decreased by 4.35%, and for women this ratio decreased by 0.5%. Assuming that minimum-wage laws affected the level

²²State- and industry-exclusion robustness checks for Minimum wage, $\$_{ist}$ estimates are shown in Online Appendix Figure 3 and Online Appendix Figure 5.

²³The negative coefficient is driven by the industry-specific time trends in industries that did not employ women. The coefficient is positive for the subsample of industries and occupations where both men and women are employed even for specifications in Columns I-III. Results available on request.

of employment in absolute terms, our estimates show that, absent minimum wage, female employment would have increased by 4%. At the same time, male employment would have decreased at a rate about $\left(\frac{4.35\%+1.3\%}{4.35\%} - 1\right) \times 100\% = 30\%$ larger.

Panel B Figure 4 reports on the robustness of our preferred estimate in Column VII in Panel B to dropping one state at a time.²⁴ Similarly, the estimated coefficient always remains significantly different from zero. Dropping California, decreases the coefficient the most, from 0.013 to 0.008. Dropping Massachusetts, increases the coefficient the most, from 0.013 to 0.022. Panel B of Online Appendix Figure 4 reports a robustness check to dropping one industry at a time. The estimated coefficients are more heterogeneous than those in the sample of women. Nevertheless, all except one (dropping wholesale industry) are positive and significantly different from zero. Dropping the wholesale industry reduces the coefficient of $\mathbb{1}$ (Minimum wage) to 0.007 and increases the amount of noise.²⁵

4.1 Robustness of the Identification Strategy

We provide additional evidence to support our identification strategy. First, as usual with quasi-experimental research designs based on “differencing out” endogenous variation, we provide evidence suggesting that, absent the treatment, treated cells would behave in the same way as untreated cells. This is usually done in the literature by showing that, *before* the treatment, units follow parallel trends. Second, we show that potentially confounding factors, such as contemporary labor legislation, are not driving the results.

To address the latter concern, we directly control for another piece of labor legislation that was passed at around the same time as minimum wage, i.e., maximum weekly hours for female workers (Department of Labor, 1927, 1937b).²⁶ In Online Appendix Table 6 we include an interaction term between a binary variable indicating that a county-industry cell has working-hours regulation and a binary variable equal to one if the state has ever had minimum-wage legislation to our main specification. We confirm that the main coefficients of interest are almost unchanged after the inclusion of this control. In doing this, we are making sure that our main results are not driven by laws regulating maximum working hours.²⁷

To tackle the aforementioned issue of the “parallel trends,” in Online Appendix Table 7, we provide several placebo tests that demonstrate that our results are not driven by pre-existing local industry and gender-specific trends. With the full set of fixed effects included, identification of our baseline estimates in Table 2 comes from within-state-industry variation. To check that this variation truly estimates the effect of changes in minimum-wage legislation, rather than local industry trends, we shift the time-period of the treatment by 20 years ($t - 2$), always evaluated relative to a state-specific and industry-specific year fixed effects. This means that we use exactly the same treatment in terms of industries, states, but now the minimum wage that was active at $t = 1920$ is set at $t' = 1900$, and minimum wage of $t = 1930$ is set at $t' = 1910$. We exclude 1920 and 1930 years from the regressions to make sure that truly treated states-counties are not in the regression. None of the resulting estimates in for women (Columns I–III) or men (Columns IV–VI) has a significant coefficient, making it unlikely that unobservable confounders could drive our baseline results.

²⁴Dropping Arkansas result in the coefficient $\mathbb{1}$ (Minimum wage) = 0.0127 and s.e.= 0.00077; resulting confidence intervals are very small and are not visible on the figure even when we plot 99% confidence intervals.

²⁵Our results hold if we keep non-occupational industries instead of setting them being missing observations. We repeat our baseline results for women and men in the Online Appendix Table 3.

²⁶The best reference on this topic may be Goldin (1988).

²⁷ It is not our contention to causally estimate the effect of maximum (weekly) working hours of women on female (and male) employment in this paper. Therefore, we only coded working-hours legislation in minimum-wage states that can be confounded to our treatment. More generally, in our follow-up work we plan to use the full set of working-hours regulations for women to study their effects on female labor outcomes.

It is worth mentioning the interpretation of our results when accounting for the possibility of migration across borders. First, all estimated models in this paper, by applying a triple-difference strategy, take into account county-level changes in employment rates over time. Second, permanent migration is taken into account by the fact that our specifications all have employment rates on the left hand side, thereby accounting for changes in population across counties over time. Finally, even in the unlikely scenario in which women from untreated areas are attracted by higher minimum wages and move alone to treated areas without changing their residence, these movements of workers would result in an attenuation of our results.

5 The Gender Elasticity of Substitution

We further explore the impact of minimum wages on labor demand by introducing a simple model focusing on one industry, i , in which both men and women are employed, in a closed local labor market. Suppose that the production takes capital and labor as inputs in a Cobb-Douglas function. Moreover, suppose that labor is inelastically supplied by women and men and it enters the production function through a CES aggregate.

$$Y_i = AK_i^{\alpha_i} L_i^{1-\alpha_i}, \quad (3)$$

$$L_i = \left[(\theta_{w_i} W_i)^{\frac{\sigma_i-1}{\sigma_i}} + (\theta_{m_i} M_i)^{\frac{\sigma_i-1}{\sigma_i}} \right]^{\frac{\sigma_i}{\sigma_i-1}}. \quad (4)$$

In the formalization above, θ_{s_i} is a productivity parameter for sex $s \in \{w, m\}$, and σ_i is the elasticity of substitution between female labor, W_i and male labor M_i . Women and men are gross substitutes if $\sigma_i > 1$ and gross complements if $\sigma_i < 1$. Women and men are paid in equilibrium ω_{w_i} and ω_{m_i} respectively. From the first order conditions of a representative firm we derive an expression describing the relative demand for women and men as a function of relative wages and relative productivity, as follows:

$$\log\left(\frac{W_i}{M_i}\right) = (1 - \sigma_i) \log\left(\frac{\theta_{m_i}}{\theta_{w_i}}\right) - \sigma_i \log\left(\frac{\omega_{w_i}}{\omega_{m_i}}\right). \quad (5)$$

In what follows, we will initially treat the whole economy as only one industry so we will drop the subscript i . As we mentioned earlier in the paper, we treat minimum-wage laws as shocks to the cost of female labor. Assuming that relative productivity is industry-locality specific, and therefore absorbed by fixed effects, observing both relative wages and employment levels would allow us to estimate the elasticity of substitution between men and women. However, the main difficulty with estimating σ in equation (5) is that we do not observe wages directly. The approach that we take here consists of two steps. First, we estimate the impact of minimum-wage laws on the (log) relative employment as $\hat{\beta} = \sigma \cdot \left[-\Delta \log\left(\frac{\omega_w}{\omega_m}\right) \right]$. Second, to separately identify σ , we use estimates of $\widehat{\Delta \log\left(\frac{\omega_w}{\omega_m}\right)}$ derived from other samples (e.g., the Oregon sample) and compute $\hat{\sigma} = \frac{\hat{\beta}}{\widehat{\Delta \log\left(\frac{\omega_w}{\omega_m}\right)}}$.

We estimate equation 5 using our most conservative regression specification from Column VII of Table 3 and $\log\left(\frac{W}{M}\right)$ as the dependent variable.

$$\log\left(\frac{\#EmployedWomen_{ic(s)t}}{\#EmployedMen_{ic(s)t}}\right) = \beta \cdot \mathbb{1}Minimum\ wage_{ist} + \mu_{st} + \Psi_{p(c)t} + \Phi_{is} + \Phi_{it} + \epsilon_{ip(c)t}. \quad (6)$$

The resulting estimate yields $\hat{\beta} = -0.047$ (see Table 4, Column I). This result is consistent with our findings in Table 3 suggesting the presence of substitution of female laborers with male laborers in treated industries.

Table 4: Female-to-Male Elasticity of Substitution

| | I | II | III |
|---|---|---------------------|---------------------|
| | Dependent variable: Log (emp. women/emp. men) | | |
| National share of women in industry i , % | [0;100] | [25;75] | <25 & >75 |
| 1(Minimum wage) | -0.047*** (0.017) | -0.075** (0.031) | -0.037** (0.014) |
| Δ s.e. | | | -0.038** (0.018) |
| R-squared | 0.76 | 0.53 | 0.81 |
| Observations | 167,717 | 58,039 | 109,678 |

Notes: This table estimates the same specifications as the baseline Table 3 but with the dependent variable is defined as $\log\left(\frac{\#EmployedWomen_{ic(s)t}}{\#EmployedMen_{ic(s)t}}\right)$. This table reports on the baseline results from estimating equation (2) (Columns VII). The number of observation is smaller than number of women or men separately in CBCP Sample in Table 3 because not all observations (defined on the county, industry-occupation, and decade level) had both employed men and employed women. National share of women employed in industry i is defined on the full sample of states in 1880, 1900, 1910, 1920, and 1930. Standard errors are triple-clustered on state, industry-occupation, and border-segment level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

From the longitudinal data in Oregon we know that wages for women increased on average by 6.8 percentage points. We also expect wages for men to grow given the increase in demand. While we do not observe male wages, in [Online Appendix Table 8](#) we estimate growth in male earnings in two ways. First, we calibrate it using national male wage growth rates in two female-labor-intensive industries, shoe making ([Department of Labor, 1919](#)) and clothing ([Department of Labor, 1925b](#)), between 1913 and 1914. We report wage growth rates for specific subgroups within these industries in Column I. Men’s wages grew from as little as 2.3% (row 4) to as much as 6.1% (row 6). This in turn implies that $\hat{\sigma} \in \left[\frac{-0.047}{0.068-0.023}, \frac{-0.047}{0.068-0.061}\right] = [1.05, 5.87]$. Second, we look at the average wage growth in Portland, Oregon between 1913 and 1914 ([Department of Labor, 1914, 1916](#)). In this alternative scenario we calibrate male wage growth between 4% among bakers (row 7) and 6% in printing industry (row 8), which implies an elasticity of substitution of $\hat{\sigma} \in \left[\frac{-0.047}{0.068-0.04}, \frac{-0.047}{0.068-0.06}\right] = [1.68, 5.87]$. Overall, whether we calibrate men’s wage growth using national wages in female-labor-intensive industries or available wages in Portland, the resulting estimates of the elasticity of substitution deliver $\sigma > 1$, suggesting that female and male laborers are gross substitutes.

We further explore to what extent the elasticity of substitution between sexes is related to the share of women in a given industry. To do this, in Columns II and III of Table 4, we estimate $\hat{\beta}$ for two sets of industries. In particular, we re-estimate the same model as in Column I, but computed on a subset of industries with share of women between 25% and 75% (in Column II), and on a subset of industries with either low (< 25%) share of women or a high (> 75%) share of women (Column III). The coefficient for less gender-segregated industries is double the one for segregated industries. This is consistent with the view that the elasticity of substitution between men and women employees is larger in industries where no sex is overwhelmingly predominant. Indeed, in male-dominated industries, women are more likely to be complements to men, while in women-dominated industries women are either much more valuable than men, or complements, or both. The highest degree of substitutability is observed in non-segregated industries.

6 Discussion and Conclusion

A natural follow-up question that arises from our findings is whether the fact that gender-specific minimum-wage laws, existing for up to 26 years before the introduction of the FLSA in 1938, discouraged women from participating in the labor force, by decreasing female labor demand. We cannot provide a well-identified answer to this question with the existing data but we provide suggestive evidence. We estimate the following cross-sectional regression separately for women and men on the full sample of counties:

$$LFP_{c(s),1940} = \alpha + \beta \cdot MinWageLegacy_s + LFP_{c(s),1910} + \Delta LFP_{c(s),1900-10} + \epsilon_{cs}, \quad (7)$$

where $LFP_{c(s),1940}$ is a labor-force participation in 1940, after the federal minimum wage was introduced and $MinWageLegacy_s$ is a measure of exposure of women in state s to minimum-wage laws. We use two measures of $MinWageLegacy_s$. First, for the sake of interpretability we define it as a dummy equal to unity if state had gender-specific minimum-wage laws at least for 10 years.²⁸ Second, we use log of number of years that minimum-wage laws were active.²⁹ Because it is a cross-section and the treatment is administrated at the state-level, we cannot control for state fixed effects. However, we control for population, pre-treatment labor-force participation $LFP_{c(s),1910}$ and pre-treatment trend in the dependent variable $\Delta LFP_{c(s),1900-10}$.

Table 5: Long-Run Effect of Minimum-Wage Laws

| Panel A | I | II | III | IV | V | VI |
|---|---|-------------------|--------------------|-------------------|--------------------|-------------------|
| | Dependent variable: Labor-force participation in 1940 | | | | | |
| | Women | Men | Women | Men | Women | Men |
| State had min. wage laws for at least 10 years | -0.021** (0.010) | -0.001 (0.003) | -0.020* (0.010) | -0.001 (0.003) | -0.020* (0.010) | -0.001 (0.003) |
| Labor-force participation (1910) | | | X | X | X | X |
| Δ Labor-force participation (1900-1910) | | | | | X | X |
| R-squared | 0.092 | 0.001 | 0.100 | 0.002 | 0.103 | 0.003 |
| Observations | 3,099 | 3,099 | 2,946 | 2,946 | 2,818 | 2,818 |
| Panel B | I | II | III | IV | V | VI |
| | Dependent variable: Labor-force participation in 1940 | | | | | |
| | Women | Men | Women | Men | Women | Men |
| Log # years under min. wage. laws | -0.006* (0.003) | -0.000 (0.001) | -0.005* (0.003) | -0.000 (0.001) | -0.006* (0.003) | -0.000 (0.001) |
| Labor-force participation (1910) | | | X | X | X | X |
| Δ Labor-force participation (1900-1910) | | | | | X | X |
| R-squared | 0.090 | 0.001 | 0.098 | 0.002 | 0.101 | 0.003 |
| Observations | 3,099 | 3,099 | 2,946 | 2,946 | 2,818 | 2,818 |

Notes: Observation is a county. The explanatory variable in Panel A is an indicator variable equal to unity if state had minimum-wage laws for at least 10 years before the FLSA (AR (12 years), CA (12), KS (10), MA (26), MN (25), ND (19), OR (25), UT (16), WA (25), and WI (11)); AZ (8) and DC (5) are treated as zeroes. The explanatory variable in Panel B is inverse hyperbolic sin of the number of years that state had minimum-wage laws (see footnote 29). All regressions are estimated separately for the sample of men and women. We control for labor-force participation in 1910 of women (men) in Columns III and V (IV and VI). Column V (VI) also includes pre-treatment changes labor-force participation of women (men) in 1900–1910. Number of observations declines in Columns III–VI because some counties that existed in 1940 did not exist in 1910 and 1900. All columns include constant and county’s population. Standard errors clustered by state are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

²⁸We exclude DC and AZ because they only had minimum-wage laws for 5 and 8 years, respectively.

²⁹Here while we refer to the logarithm, it is shorthand for using the inverse hyperbolic sin ($\log(y_i + (y_i^2 + 1)^{1/2})$), which can be interpreted in exactly the same way as a standard logarithmic variable but without needing to change zero values (Burbidge et al., 1988; Card and DellaVigna, 2017).

Panel A of Table 5 reports the result for the more-than-10-years-minimum-wage-history dummy. Having minimum-wage laws for at least 10 years is associated with lower female labor-force participation by 2.1 percentage points and it is not associated with any change in the labor force participation of men. The coefficient does not change when we add pre-treatment dependent variable (Columns III–IV) or pre-treatment trend in the dependent variable in Columns V and VI.

In Panel B we use a more accurately defined treatment — log number of years with active minimum-wage laws. The estimated coefficients in the model for women are stable and remain significant across specifications. Doubling the number of years that a state had active minimum-wage laws is associated with lower female labor-force participation by 0.6 percentage points. Conditional on having minimum-wage laws, the average number of years that they were active was 16 years; therefore, an average treated state has 0.73 percentage points lower female labor force participation. At the same time, we find no evidence that labor-force participation of men is correlated with minimum wage legacy.

To substantiate the hypothesis further, we explore individual-level Census data. In Columns I and II of Online Appendix Table 9 we re-estimate equation (7) and show that women in states with minimum wages are less likely to participate in the labor force. Then, to show that the effect is associated with individual persistent behavior rather than location effects, we omit all women from states with minimum-wages laws in Columns III and IV and keep only women that migrated to the twelve minimum-wage states from states that did not have minimum-wage legislation. In other words, in the 12 treated states we only have women who were *not* exposed to minimum wages. If the long-run impact is driven by the effect on individual behavior, these women should not be affected. Indeed, the resulting estimates are close to zero and not statistically significant.

We propose two mechanisms to explain this result. First, the discouragement of women from participating to the labor force due to a decrease in the demand for their labor might have affected cultural norms regarding whether women should work or not. Second, the perceived lower returns from job search due to a lower demand for female labor might have persisted across generations.

In conclusion, gender-specific minimum-wage laws not only adversely affected female employment but also might have discouraged women from working even when the minimum wage equalized across genders. This result is in line with Sorkin (2015) who finds a long-run effect of minimum wage on employment elasticities.³⁰

While our evidence is produced using historical data, the parameters estimated in this paper speak to the contemporary policy debate.³¹ Recently, Raise the Wage Act of 2019 (HR 582) proposes to raise the federal minimum wage gradually from \$7.25 today to \$15 by 2024. For example, Godøy and Reich (2019) estimated that such wage increase will not have any negative effect on employment. Even in Alabama and Mississippi, two of the lowest-wage states, the relative minimum wage — ratio of minimum wage to the state’s median wage — would rise to 0.77 and 0.85, respectively. In this paper we showed that an unprecedented increase in minimum wage (e.g., the ratio of minimum wage over median wage in the Oregon data is around 0.92) had sizable negative effects on the employment of women.

This paper shows three main results. First, pre-FLSA minimum-wage legislation was effective in increasing wages for women with lower pre-minimum-wage earnings. Second, these minimum-wage laws caused a decline in female employment and a (smaller in magnitude) increase in male employment. Third, the

³⁰ At the same time such economic policies as minimum wage may have non-economic positive effects on the population. E.g., Dow et al. (2019) show that increase in minimum wage reduced suicides among low-wage workers.

³¹ This paper gives avenue for a follow-up work, as one can expand our analysis and include Canadian census data, given that Canada too imposed gender-specific minimum-wage laws at the beginning of the 20th Century (Department of Labor, 1925a; Russell, 1991). In addition, collecting more wage data for states other than Oregon may allow a deeper investigation of the earnings effects of these laws. The goal is a greater understanding of the political economy of introduction of minimum wages in a more institutionally simple environment. The evidence produced by this research program will be useful to economists, policymakers, and the public.

gender-specificity of these laws allows us to show that men and women were, on average, gross substitutes (constant elasticity of substitution greater than 1) in the American labor market at the beginning of the 20th Century.

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Online Appendix

to

“The Employment Effects of a Gender-Specific

Minimum Wage”

Table Online Appendix Table 1: List of the Industries Covered by minimum-wage legislation by State

| State | Year issued | Year effective | Industry |
|----------------------|-------------|----------------|---|
| Arizona | 1917 | 1917 | ALL |
| Arkansas | 1920 | 1920 | Mercantile |
| California | 1916 | 1916 | Fruit and vegetable canning |
| California | 1917 | 1917 | Mercantile |
| California | 1917 | 1918 | Laundry and dry cleaning industry |
| California | 1918 | 1918 | General and professional offices |
| California | 1918 | 1918 | Unskilled and unclassified occupations |
| California | 1918 | 1919 | Manufacturing industry (excluding printing) |
| California | 1919 | 1919 | Hotels and restaurants |
| California | 1920 | 1920 | Agricultural occupations |
| California | 1920 | 1920 | Manufacturing industry (including printing) |
| California | 1922 | 1922 | Needle-trades industry |
| District of Columbia | 1919 | 1919 | Printing publishing and allied industries |
| District of Columbia | 1919 | 1919 | Mercantile |
| District of Columbia | 1920 | 1920 | Hotels and restaurants |
| District of Columbia | 1921 | 1921 | Laundry and dry cleaning industry |
| Kansas | 1918 | 1918 | Mercantile |
| Kansas | 1918 | 1918 | Laundry and dry cleaning industry |
| Kansas | 1918 | 1918 | Telephone operators |
| Kansas | 1919 | 1919 | Manufacturing |
| Massachusetts | 1914 | 1914 | Brush |
| Massachusetts | 1915 | 1915 | Laundry and dry cleaning industry |
| Massachusetts | 1915 | 1916 | Retail |
| Massachusetts | 1916 | 1917 | Women's clothing factories |
| Massachusetts | 1919 | 1919 | Office and other building cleaners |
| Massachusetts | 1919 | 1920 | Candy making occupation |
| Massachusetts | 1919 | 1919 | Fruit and vegetable canning |
| Massachusetts | 1920 | 1920 | Paper box occupation |
| Massachusetts | 1923 | 1924 | Druggists preparations |
| Massachusetts | 1925 | 1925 | Bread and other bakery products |
| Massachusetts | 1927 | 1927 | Toys games and sporting goods occupation |
| Minnesota | 1914 | 1914 | Mercantile |
| Minnesota | 1914 | 1914 | Manufacturing |
| Minnesota | 1918 | 1918 | ALL |
| North Dakota | 1920 | 1920 | Public housekeeping occupation |
| North Dakota | 1920 | 1920 | Personal service occupation |
| North Dakota | 1920 | 1920 | Office occupation |
| North Dakota | 1920 | 1920 | Manufacturing |
| North Dakota | 1920 | 1920 | Laundry and dry cleaning industry |
| North Dakota | 1920 | 1920 | Student nurses |
| North Dakota | 1920 | 1920 | Mercantile |
| North Dakota | 1920 | 1920 | Telephone operators |
| Oregon | 1913 | 1913 | ALL |
| Utah | 1913 | 1913 | ALL |

Notes: Source: Department of Labor (1927, 1937a, 1939).

Continuation of the **Online Appendix Table 1**: List of the Industries Covered by minimum-wage legislation by State

| State | Year issued | Year effective | Industry |
|------------|-------------|----------------|-----------------------------------|
| Washington | 1914 | 1914 | Mercantile |
| Washington | 1914 | 1914 | Manufacturing |
| Washington | 1914 | 1914 | Laundry and dry cleaning industry |
| Washington | 1914 | 1914 | Telephone operators |
| Washington | 1914 | 1915 | Office employment |
| Washington | 1915 | 1915 | Hotels and restaurants |
| Washington | 1918 | 1918 | ALL |
| Wisconsin | 1917 | 1917 | Pea canning |
| Wisconsin | 1919 | 1919 | ALL |

Notes: Source: Department of Labor (1927, 1937a, 1939).

Table Online Appendix Table 2: Timing of Imposing and Abolishing of Minimum-Wage Legislation

| # | State | Year when first law is imposed | Year when abolished | # of years active before FLSA | Comments |
|----|----------------------|--------------------------------|---------------------|-------------------------------|---|
| 1 | Arizona | 1917 | 1925 | 8 | Overtured by Supreme Court in <i>Murphy v. Sardell</i> . |
| 2 | Arkansas | 1915 | 1927 | 12 | Overtured by Supreme Court in <i>Donham v. West Nelson Manuf. Co.</i> |
| 3 | California | 1913 | 1925 | 12 | Withdrawn by state in <i>Gainer v. A.B.C. Dorhram</i> . |
| 4 | District of Columbia | 1918 | 1923 | 5 | Overtured by Supreme Court on a 5-3 vote in <i>Adkins v. Children's Hospital</i> . |
| 5 | Kansas | 1915 | 1925 | 10 | Overtured by Kansas Supreme Court in <i>Topeka Laundry Co. v. Court of Industrial Relations</i> . |
| 6 | Massachusetts | 1912 | - | 26 | |
| 7 | Minnesota | 1913 | - | 25 | |
| 8 | North Dakota | 1919 | - | 19 | |
| 9 | Oregon | 1913 | - | 25 | |
| 10 | Utah | 1913 | 1929 | 16 | Repealed. |
| 11 | Washington | 1913 | - | 25 | |
| 12 | Wisconsin | 1913 | 1924 | 11 | Overtured by federal district court following <i>Adkins</i> . |

Notes: Source: Levitan (1979) and Thies (1990).

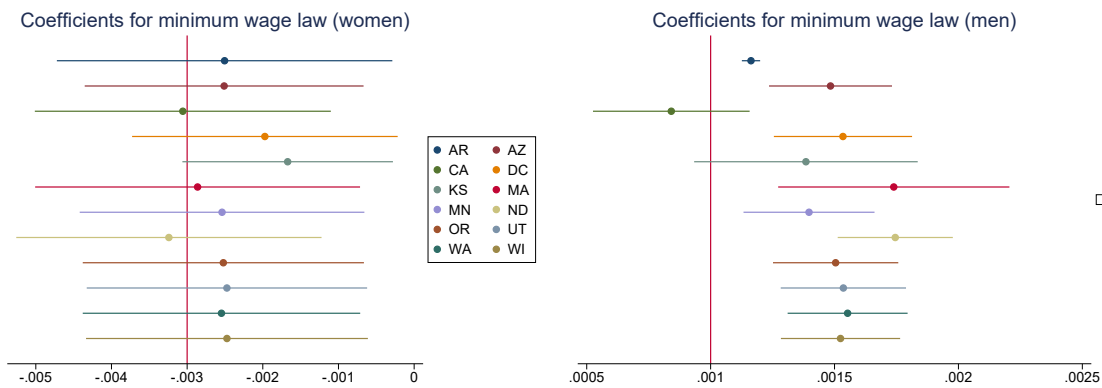


Figure Online Appendix Figure 3: State-Exclusion Robustness of the Results for Minimum wage, $\$_{ist}$ in Table 3

Notes: This figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the core specification in Column VII of Table 3, dropping one state at a time. The left figure shows results for women. The right figure shows results for men. One dropped state may imply dropping several state-border-segments, See Table 1. The (red) vertical line is the baseline point estimate. The results are sorted top-to-bottom in alphabetical order, i.e., omit AR, then AZ, then CA, etc.

Table Online Appendix Table 3: The Effect of Minimum-Wage Legislation on Employment, by Gender, no missing occupations

| ~Baseline, no missing non-occup. | I | II | III | IV | V | VI | VII |
|--|--|----------------------|---------------------|-------------------|-------------------|----------------------|----------------------|
| Panel A: Women | Dependent variable: Log employment share (women) | | | | | | |
| Sample | All | | | | CBCP | | |
| 1(Minimum wage) | -0.006 (0.015) | -0.026 (0.025) | -0.024 (0.064) | -0.028 (0.019) | -0.025 (0.019) | -0.056*** (0.011) | -0.050*** (0.010) |
| Minimum wage, \$ (mean min wage \$10.2) | 0.001 (0.002) | -0.000 (0.002) | 0.004 (0.006) | -0.002 (0.002) | -0.002 (0.002) | -0.005** (0.002) | -0.004*** (0.001) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.644 | 0.672 | 0.673 | 0.689 | 0.697 | 0.734 | 0.740 |
| Observations | 1,363,979 | 1,363,979 | 1,363,979 | 1,363,979 | 1,363,979 | 272,397 | 272,397 |
| Panel B: Men | Dependent variable: Log employment share (men) | | | | | | |
| Sample | All | | | | CBCP | | |
| 1(Minimum wage) | -0.104*** (0.024) | -0.110*** (0.027) | -0.086** (0.036) | 0.007 (0.013) | 0.005 (0.012) | 0.004 (0.007) | 0.010* (0.005) |
| Minimum wage, \$ (mean min wage \$10.2) | -0.009*** (0.002) | -0.009*** (0.003) | -0.004 (0.003) | 0.000 (0.001) | -0.000 (0.001) | 0.001 (0.001) | 0.001* (0.001) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.524 | 0.544 | 0.546 | 0.570 | 0.579 | 0.647 | 0.654 |
| Observations | 4,548,699 | 4,548,699 | 4,548,699 | 4,548,699 | 4,548,699 | 801,903 | 801,903 |

Notes: This table estimates the same specifications as the baseline Table 3 but without dropping non-occupational industries. This table reports on the baseline results from estimating equation (1) (Columns I–V) and equation (2) (Columns VI and VII). Columns I–V are estimated on the full sample of U.S. counties. Column I reports includes only (time-invariant) county, industry, and occupation fixed effects. Column II adds industry-state and occupation-state fixed effects. Column III adds state-year fixed effects. Column IV adds industry-year and occupation-year fixed effects. Column V replaces county and state-year fixed effects with county specific year fixed effects. Columns VI and VII are estimated on the CBCP sample. Column VI replicates specification in Column IV but uses county-pair fixed effects instead of county fixed effects. Column VII replicates specification in Column V but uses county-pair specific year fixed effects instead of county-year fixed effects. In Columns I–V standard errors are double-clustered on state and industry-occupation level. In Columns VI and VII standard errors are triple-clustered on state, industry-occupation, and border segment level. Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 4: The Effect of Minimum-Wage Legislation on Employment, by Gender, elasticity

| | I | II | III | IV | V | VI | VII |
|--|--|-----------|-----------|-----------|-----------|-----------|-----------|
| Panel A: Women | | | | | | | |
| | Dependent variable: Log employment share (women) | | | | | | |
| Sample | All | | | | CBCP | | |
| log (Minimum wage) | -0.007 | -0.011* | -0.009 | -0.005 | -0.004 | -0.014*** | -0.012*** |
| inverse hyperbolic sin | (0.005) | (0.006) | (0.008) | (0.005) | (0.005) | (0.003) | (0.003) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.644 | 0.672 | 0.673 | 0.689 | 0.697 | 0.734 | 0.740 |
| Observations | 1,363,979 | 1,363,979 | 1,363,979 | 1,363,979 | 1,363,979 | 272,397 | 272,397 |
| Panel B: Men | | | | | | | |
| | Dependent variable: Log employment share (men) | | | | | | |
| Sample | All | | | | CBCP | | |
| log (Minimum wage) | -0.033*** | -0.033*** | -0.010 | 0.004*** | 0.003*** | 0.002 | 0.004*** |
| inverse hyperbolic sin | (0.008) | (0.010) | (0.007) | (0.001) | (0.000) | (0.003) | (0.001) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.559 | 0.581 | 0.583 | 0.598 | 0.608 | 0.647 | 0.654 |
| Observations | 4,139,089 | 4,139,089 | 4,139,089 | 4,139,089 | 4,139,089 | 801,903 | 801,903 |

Notes: This table estimates the same specifications as the baseline Table 3. This table reports on the baseline results from estimating equation (1) (Columns I–V) and equation (2) (Columns VI and VII). Columns I–V are estimated on the full sample of U.S. counties. Column I reports includes only (time-invariant) county, industry, and occupation fixed effects. Column II adds industry-state and occupation-state fixed effects. Column III adds state-year fixed effects. Column IV adds industry-year and occupation-year fixed effects. Column V replaces county and state-year fixed effects with county specific year fixed effects. Columns VI and VII are estimated on the CBCP sample. Column VI replicates specification in Column IV but uses county-pair fixed effects instead of county fixed effects. Column VII replicates specification in Column V but uses county-pair specific year fixed effects instead of county-year fixed effects. In Columns I–V standard errors are double-clustered on state and industry-occupation level. In Columns VI and VII standard errors are triple-clustered on state, industry-occupation, and border segment level. Standard errors are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table Online Appendix Table 5: The Effect of Minimum-Wage Legislation on Employment, by Gender, elasticity, no missing occupations

| ~Baseline, no missing non-occup. | I | II | III | IV | V | VI | VII |
|--|--|-----------|-----------|-----------|-----------|-----------|-----------|
| Panel A: Women | Dependent variable: Log employment share (women) | | | | | | |
| Sample | All | | | | CBCP | | |
| log (Minimum wage) | 0.007 | -0.001 | 0.017 | -0.008 | -0.008 | -0.017*** | -0.015*** |
| inverse hyperbolic sin | (0.007) | (0.009) | (0.026) | (0.007) | (0.007) | (0.004) | (0.004) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.635 | 0.656 | 0.658 | 0.696 | 0.702 | 0.704 | 0.710 |
| Observations | 1,649,686 | 1,649,686 | 1,649,686 | 1,649,686 | 1,649,686 | 309,891 | 309,891 |
| Panel B: Men | Dependent variable: Log employment share (men) | | | | | | |
| Sample | All | | | | CBCP | | |
| log (Minimum wage) | -0.031*** | -0.034*** | -0.019 | -0.001 | -0.002 | 0.001 | 0.003 |
| inverse hyperbolic sin | (0.007) | (0.009) | (0.012) | (0.006) | (0.006) | (0.003) | (0.002) |
| County/county pair f.e. | X | X | X | X | | X | |
| County/county-pair-year f.e. | | | | | X | | X |
| Industry-state & occupation-state f.e. | | X | X | X | X | X | X |
| State-year f.e. | | | X | X | X | X | X |
| Industry-year & occup.-year f.e. | | | | X | X | X | X |
| R-squared | 0.536 | 0.555 | 0.557 | 0.583 | 0.592 | 0.613 | 0.621 |
| Observations | 4,643,808 | 4,643,808 | 4,643,808 | 4,643,808 | 4,643,808 | 877,661 | 877,661 |

Notes: This table estimates the same specifications as the baseline Table 3 but without dropping non-occupational industries. This table reports on the baseline results from estimating equation (1) (Columns I–V) and equation (2) (Columns VI and VII). Columns I–V are estimated on the full sample of U.S. counties. Column I reports includes only (time-invariant) county, industry, and occupation fixed effects. Column II adds industry-state and occupation-state fixed effects. Column III adds state-year fixed effects. Column IV adds industry-year and occupation-year fixed effects. Column V replaces county and state-year fixed effects with county specific year fixed effects. Columns VI and VII are estimated on the CBCP sample. Column VI replicates specification in Column IV but uses county-pair fixed effects instead of county fixed effects. Column VII replicates specification in Column V but uses county-pair specific year fixed effects instead of county-year fixed effects. In Columns I–V standard errors are double-clustered on state and industry-occupation level. In Columns VI and VII standard errors are triple-clustered on state, industry-occupation, and border segment level. Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 6: The Effect of Minimum-Wage Legislation on Employment, by Gender, with Maximum Working Hours Controls

| | I | II | III | IV | V | VI |
|---|--|---------------------|----------------------|--------------------|---------------------|-------------------|
| | Dependent variable: Log employment share | | | | | |
| | Women | | | Men | | |
| 1(Minimum wage) | -0.043*** (0.006) | | | 0.022** (0.011) | | |
| Minimum wage, \$ (mean min wage \$10.2) | | -0.003** (0.001) | | | 0.002*** (0.000) | |
| log (Minimum wage) inverse hyperbolic sin | | | -0.012*** (0.003) | | | 0.004* (0.002) |
| 1(Max. working hours law) x 1(State ever had minimum wage) | 0.001 (0.015) | 0.003 (0.015) | 0.003 (0.015) | 0.015* (0.008) | 0.014 (0.009) | 0.014 (0.010) |
| R-squared | 0.740 | 0.740 | 0.740 | 0.654 | 0.654 | 0.654 |
| Observations | 272,397 | 272,397 | 272,397 | 801,903 | 801,903 | 801,903 |

Notes: This table estimates baseline specification from Table 3. This table reports on the baseline results from estimating equation (2) (Column VII of the CBCP Sample). Standard errors are triple-clustered on state, industry-occupation, and border segment level. Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 7: Placebo Estimates with 1900–1910 treatment instead of 1920–1930

| | I | II | III | IV | V | VI |
|--|--|------------------|------------------|-------------------|-------------------|-------------------|
| | Dependent variable: Log employment share | | | | | |
| | Women | | | Men | | |
| 1900-1910 placebo treatment | | | | | | |
| 1(Minimum wage) | 0.012 (0.029) | | | -0.035 (0.022) | | |
| Minimum wage, \$ (mean min wage \$10.2) | | 0.002 (0.002) | | | -0.002 (0.002) | |
| log (Minimum wage) inverse hyperbolic sin | | | 0.005 (0.008) | | | -0.009 (0.008) |
| R-squared | 0.78 | 0.784 | 0.784 | 0.656 | 0.656 | 0.656 |
| Observations | 93,947 | 93,947 | 93,947 | 335,623 | 335,623 | 335,623 |

Notes: This table estimates baseline specification from Table 3 but defines states-industries treated in 1920–1930 as treated in 1900–1910. We drop 1920 and 1930. This table reports on the baseline results from estimating equation (2) (Column VII of the CBCP Sample). Standard errors are triple-clustered on state, industry-occupation, and border segment level. Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 8: Calibrations

| | I | II | III | IV | V |
|---|-------------------------|---|-----|--|----------|
| # | Wage growth (1913-1914) | Comments | Sex | Source | σ |
| 1 | 4% | Boots and Shoes (cutting department) | Men | "Wages and hours of labor in the boot and shoe industry: 1907-1918," BLS bulletin, No.260, 1919, Table 1 | 1.68 |
| 2 | 5% | Boots and Shoes (lasting department) | Men | | 2.61 |
| 3 | 2.5% | Boots and Shoes (fitting and stitching) | Men | | 1.09 |
| 4 | 2.3% | Clothing (bushelers and tailors) | Men | "Wages and hours of labor in the men's clothing industry: 1911-1924," BLS bulletin, No.387, 1925, Table 1 | 1.05 |
| 5 | 2.7% | Clothing (cutters, cloth, hand and machine) | Men | | 1.16 |
| 6 | 6.1% | Clothing (hand sewers, coat) | Men | | 6.94 |
| 7 | 4% | Bakers (Portland, OR, all) | All | "Union scale of wages and hours of labor, May 1,1915," BLS, No.194, 1916 and "Union scale of wages and hours of labor, May 15,1913," BLS, No.143, 1914 | 1.68 |
| 8 | 6% | Printing (Portland, OR, all) | All | | 5.87 |

Notes: Source: Department of Labor (1914, 1916, 1919, 1925b).

Table Online Appendix Table 9: Long-Run Effect of Minimum-Wage Laws: Placebo with Out-of-Minimum-Wage States Migrants

| | I | II | III | IV |
|--|---|------------------------------------|-------------------|-------------------|
| | Dependent variable: 1(Woman in labor force) | | | |
| Sample | All | Migrants from non-min.-wage states | | |
| State had min. wage laws for at least 10 years | -0.025*** (0.007) | | -0.010 (0.011) | |
| Log # years under min. wage. laws | | -0.001** (0.000) | | -0.000 (0.001) |
| R-squared | 0.25 | 0.25 | 0.24 | 0.24 |
| Observations | 36,706,502 | 36,706,502 | 29,924,279 | 29,924,279 |

Notes: This table estimates baseline specification (7) from Table 5 but uses individual-level data. Thus, the dependent variable is a dummy equal to unity if woman participated in the labor force and zero otherwise. In Columns III and IV, in the twelve states with minimum wages we exclude all locals; i.e., we only include those people who chose as a state of residence 5 years before 1940 to be a state that did not have minimum-wage laws. Here we control for marital status and census region. Standard errors are clustered on state level. Standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

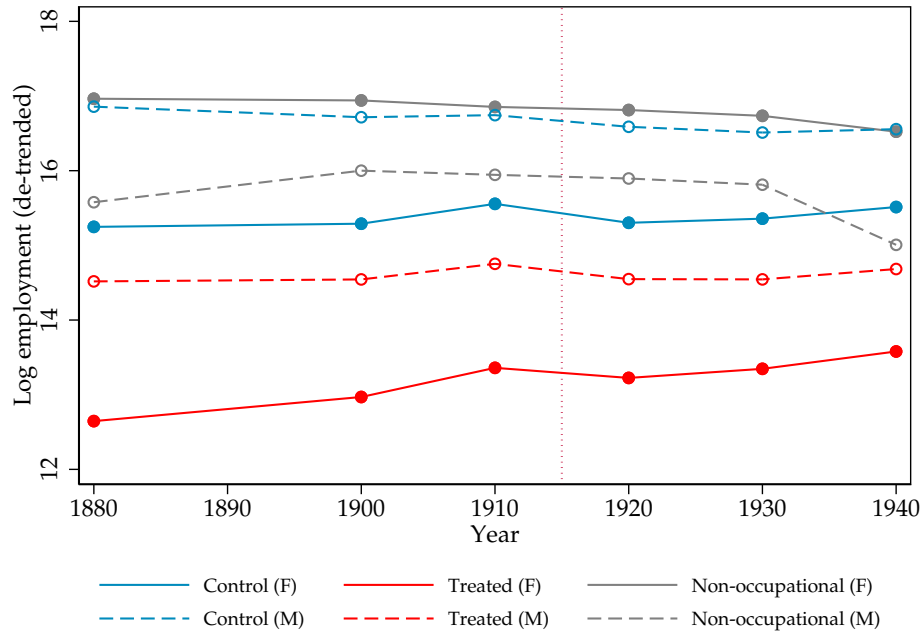


Figure Online Appendix Figure 1: Figure shows de-trended log-employment.

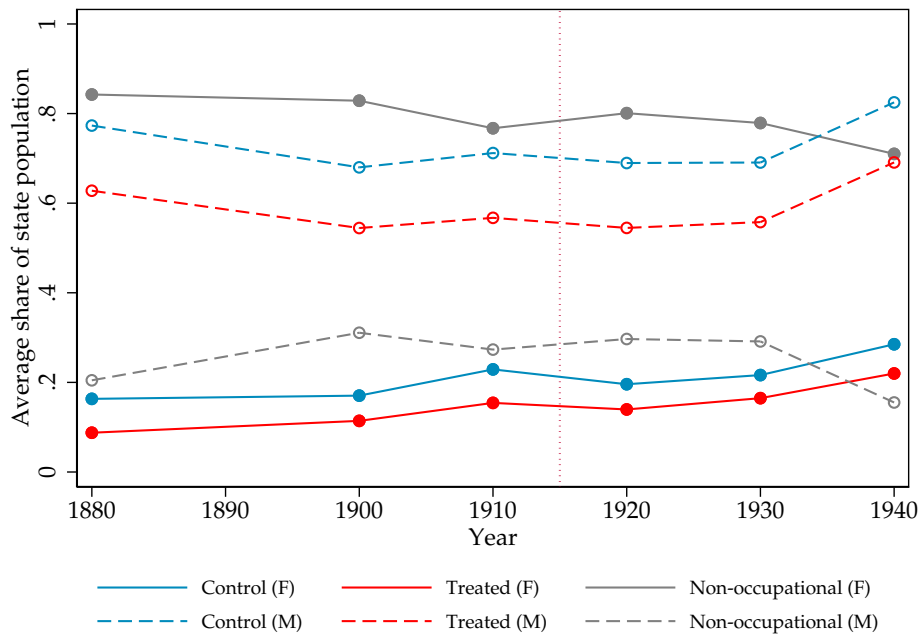


Figure Online Appendix Figure 2: Figure shows average share of state's adult population.

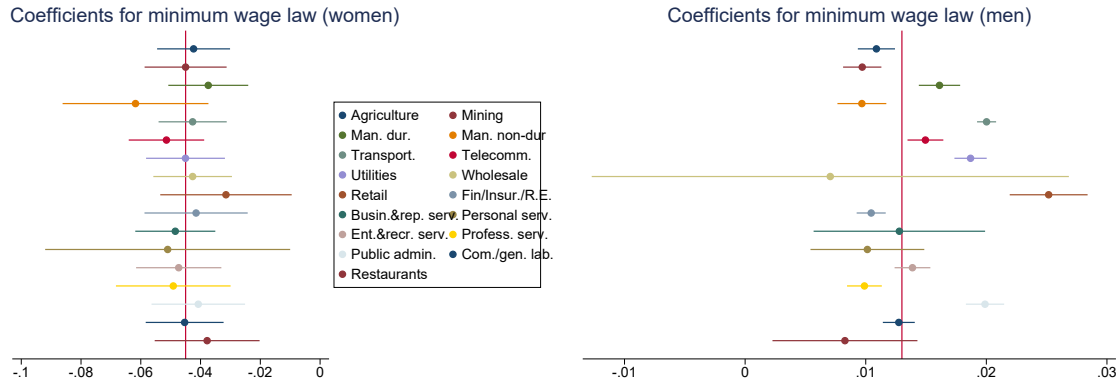


Figure Online Appendix Figure 4: Industry-Exclusion Robustness of the Results for 1 (Minimum wage) $_{ist}$ in Table 3

Notes: This figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the core specification in Column VII of Table 3, dropping one state at a time. The left figure shows results for women. The right figure shows results for men. The (red) vertical line is the baseline point estimate. The list of industries is as follows: Agriculture, mining, manufacturing of durable goods, manufacturing of non-durable goods, transportation, telecommunication, utilities, wholesale, retail, finance, insurance, and real estate, business and repair services, personal services, entertainment and recreational services, professional services, public administration, common and general labor, and restaurants. The results are sorted left-to-right and top-to-bottom, i.e., omit agricultural industry, then mining, then manufacturing of durable goods, etc.

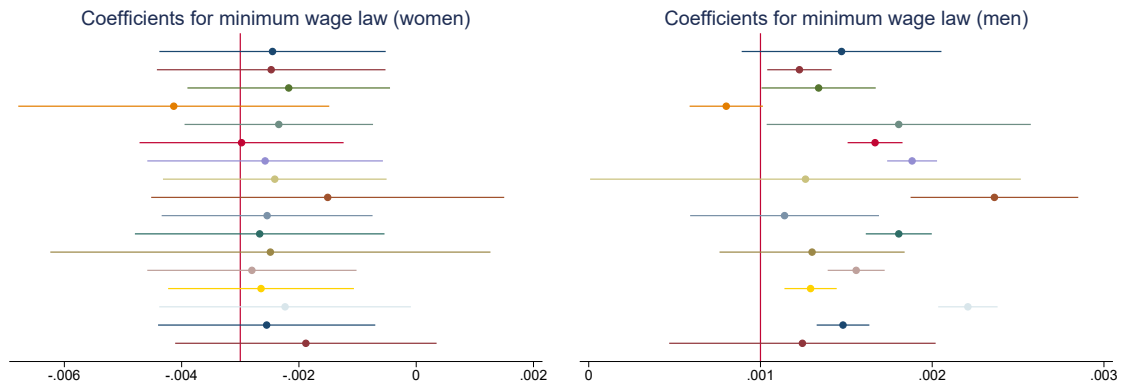


Figure Online Appendix Figure 5: Industry-Exclusion Robustness of the Results for Minimum wage, $\$_{ist}$ in Table 3

Notes: This figure reports on the point-estimate and 95th-percent confidence band that results when re-estimating the core specification in Column VII of Table 3, dropping one state at a time. The left figure shows results for women. The right figure shows results for men. The (red) vertical line is the baseline point estimate. The list of industries is as follows: Agriculture, mining, manufacturing of durable goods, manufacturing of non-durable goods, transportation, telecommunication, utilities, wholesale, retail, finance, insurance, and real estate, business and repair services, personal services, entertainment and recreational services, professional services, public administration, common and general labor, and restaurants. The results are sorted left-to-right and top-to-bottom, i.e., omit agricultural industry, then mining, then manufacturing of durable goods, etc.