The Economics of Gender-Specific Minimum-Wage Legislation*

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Abstract

During the 1910s, twelve U.S. states passed and implemented the country's first minimum-wage laws. They covered only female employees, often in a subset of industries. We study the impact of this regulation using full-count Census data. Our identification strategy compares county-industry trends in county-pairs that straddle state borders. We find that female employment decreased by at least 3.1% at the county-industry level. Across counties, we find that the own-wage elasticity of labor demand varies from around –1.6 to 0.8 as a function of the local cross-industry concentration. Affected female workers switch industries or drop out of the labor force. The latter channel is driven exclusively by married women. We document a rise in male labor demand, and we investigate the channels of substitution between men and women. While on average men and women are gross substitutes, we find evidence that the margin of substitution is driven by the replacement of women in low-rank occupations with men in middle- or high-rank occupations.

Keywords: Minimum Wage, Labor Demand, Gender Gap, Labor Markets.

JEL codes: J16, J23, N32.

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Even before the United States enacted its first federal minimum wage law—as part of the Fair Labor Standards Act (FLSA) of 1938—economists, politicians, and policy makers were debating the employment effects of minimum-wage laws (Neumark and Wascher, 2007; Fishback and Seltzer, 2021). This debate rages on today. The abundant literature in economics has shown that no indisputable, infallible fundamental law can be defined that unambiguously predicts the effects of the introduction or a perturbation of a minimum wage.[1] Instead, the way minimum-wage laws affect employment depends on multiple factors, including the industry considered in the study, other contemporaneous labor-market regulation and institutions, and the structure of the labor market. At the same time, gender gaps in earnings imply that female wage workers are often over-represented among those earning at or below minimum-wage levels (e.g., Autor, Manning and Smith, 2016).

In this paper, we use the implementation of the first U.S. minimum-wage laws to estimate the impact of the introduction of a price floor on labor. These laws were gender-specific:[2] they imposed a lower bound only on women's earnings, they were passed only in certain states, and often they covered only a subset of industries. In our preferred specification, we identify the impact of minimum-wage laws by employing a triple-difference estimation strategy that relies on comparing county-industry specific trends between counties that share a state border, in a contiguous-county research design. The gender-specific nature of this legislation allows us to explore the substitution between genders in American labor markets. Using a newly constructed linked sample of women, we also explore the impact of minimum-wage legislation on labor supply.

Starting in 1912, eleven 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 five of these jurisdictions—California, Kansas, Massachusetts, North Dakota, and the District of Columbia—these decrees covered only women in certain industries. These laws immediately spurred fierce debates, and in 1923 the Supreme Court struck down the minimum wage in Washington, D.C., as unconstitutional. While that ruling slowed further adoption of the laws in other states, most gender-specific minimum-wage regulations continued to exist until the introduction of the universal federal minimum wage.[4]

Identifying the employment impact of minimum-wage setting has posed several challenges. First, the universal nature of minimum wages makes it difficult to find a suitable control group, forcing researchers to make assumptions related to the extent the price floor is binding in particular industries (e.g., restaurants in Leamer et al. 2019) and geographic units of interest (e.g., Seattle in Jardim et al. 2017). Second, if all

[1] The most recent wave of empirical evidence started amassing at the beginning of the 1990s—with contributions from Holzer, Katz and Krueger (1991), Card (1992), Neumark and Wascher (1992), Card and Krueger (1994), and Card, Katz and Krueger (1994), among others—and it continues to grow (e.g., Aaronson et al., 2018; Clemens and Wither, 2019; Luca and Luca, 2019; Okudaira, Takizawa and Yamanouchi, 2019).

[2] While we fully acknowledge the difference between sex, a biological trait, and gender, a social identity, following our own reading of the economics literature that studies difference in outcomes between men and women (e.g., Blau and Kahn, 2017), we refer to gender as a synonym for sex throughout the paper.

[3] Massachusetts was the first of these states to pass a minimum-wage law, but it was not put into effect until 1914. According to the Department of Labor's *Bulletin of the Women's Bureau* no. 40, printed in 1924, the first minimum-wage law enacted was Oregon's universal minimum wage for women, in 1913.

[4] Background information on the timeline and coverage of gender-specific minimum-wage laws appears in Section 1.

people in a certain area have the same minimum wage, substitution into illegal employment might be more likely (Bernhardt et al., 2009), which would induce nonclassical measurement error in the estimates. Third, 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.[5]

The first U.S. minimum-wage regulations are intriguing for two reasons. First, in the states where minimum-wage decrees covered only a subset of industries, we are able to exploit a layer of policy variation not often available to researchers. Second, in all instances, the laws covered only female employees, enabling us to explore the differential impact of minimum-wage legislation on covered versus uncovered workers and investigate the channels of substitution between genders. In addition, studying the first wave of minimum-wage legislation in the absence of federal regulation enables us to understand the treatment effect of minimum wages compared to a counterfactual scenario of an absence of a price floor on labor, rather than relying only on variation in treatment intensity (i.e., high vs. low minimum wage). We present extensive analyses of the many dimensions of minimum-wage legislation as it pertains to American labor markets, including its effects on earnings, the channels of response adopted by affected workers, the role played by the local labor-market structure, and the impact on the occupational ranking mix.

We start by showing a minimum wage's effects on earnings. These are usually difficult to estimate: 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 preexisting employees. We isolate the latter channel by using longitudinal data from Oregon, where the local Bureau of Labor (Obenauer and von der Nienburg, 1915) collected wage data on a set of women employed both before and after the minimum wage was implemented. Within-worker analyses based on those data show that minimum-wage legislation led to an average increase in wage for women previously employed at below-minimum-wage levels, and to no average changes in the wages of women already earning more than minimum levels required by the new law. In particular, the 25th percentile of weekly earnings increased from \$6 to \$8–8.49, while the 75th percentile remained unchanged at a range of \$10–10.99. This motivating evidence gives way to the estimation of the employment effects on women.

In our baseline analysis, using full-count Census data from 1880 to 1930, we first 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 twelve U.S. states (for simplicity, we count the District of Columbia as a state) as a policy shock that introduces a price floor on female labor, and we estimate the impact of these laws on the employment of women. Since this was the first minimum-wage legislation that lifted the minimum wage from zero to a positive level (\$10 weekly, on average), we estimate our model using both a specification with a binary treatment variable and linear specifications with the dollar value of the state-industry-specific minimum wage (or its logarithm) as

[5]Using data from the Current Population Survey, Clemens and Strain (2019) show that increasing the minimum wage increases the likelihood of sub-minimum-wage payments, an indication of imperfect employment compliance.

our main variables of interest.

In our preferred specification, which uses a contiguous-county-pair research design, the identification strategy relies on comparing changes over time in county-industry-level female employment between neighboring counties in contiguous county pairs that straddle state borders, after partialling out state-specific, pair-specific, industry-specific, and occupation-specific time fixed effects. In the vein of Dube, Lester and Reich (2010), by focusing only on pairs of counties that straddle state borders, we are able to account for local trends in unobservables, which in our case include changes in local demand for female labor, gender discrimination, and local institutions.[6] However, our setting differs from the one in Dube, Lester and Reich (2010) in three major ways. First, our treatment is often industry-specific, so the additional level of variation allows us to flexibly account for industry-specific trends, and control for heterogeneity in local industry-mix.[7] Second, in our context, men are never subject to minimum wages, and this allows us to estimate the impact of the regulation separately for covered and uncovered workers based on gender. 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 can estimate both the effect of minimum wages compared to the counterfactual of the absence of such regulation, and the impact of a higher minimum wage along an intensive margin.

We find that, on average, the adoption of minimum-wage legislation decreased employment of women by at least 3.1% at the industry-county level, while aggregate local female employment decreased by 1.9% at the county level.[8] This suggests the presence of two distinct margins of adjustment in response to a drop in industry-specific local labor demand. In particular, women might exit employment, or switch industries.[9] To investigate this further, we construct a new linked dataset of women observed in the labor force in 1910 and quantify the extent to which, in 1920, after the onset of minimum-wage legislation, those who worked in affected areas and industries move to different industries or drop out of the labor force. While we confirm that both channels are in place, we document that the decreased likelihood of employment as a result of being affected by minimum-wage legislation is mostly driven by married women, who are 4.5 percentage points less likely to supply their labor to markets in 1920.

We further investigate the labor demand impact of minimum wage at the local level by computing the implied own-wage elasticity of labor demand at the county level, and observe how it changes as a function of cross-industry concentration. We estimate elasticities of labor demand with respect to own-wage ranging from around –1.6 in a context of low cross-industry concentration (as measured by a county-level Herfindahl-Hirschman Index (HHI) across industry codes) to a +0.8 elasticity in the context of local markets dominated

[6] The advantages of using state borders for identification are well understood. State borders have also been used in other contexts, e.g., manufacturing (Holmes, 1998), banking (Huang, 2008), suffrage (Naidu, 2012), and private prisons (Dippel and Poyker, 2019).

[7] In an aggregate county-level analysis, we instead adopt an identification strategy that relies on identical assumptions, as in Dube, Lester and Reich (2010).

[8] We show evidence that our results cannot be explained by pre-trends, specific states or industries, moving across borders, or concurring contemporaneous labor protection legislation (i.e., maximum hours) and processes (i.e., World War I, marriage bars, suffrage, unionization, or onset of the Great Depression).

[9] In principle they might also react by moving out of treated areas, but later in the paper we show that this is not the case.

by a single industry.[10]

After documenting the effect of minimum-wage legislation on women, we analyze labor demand for men. We document that, on average, treated industries observe a 1.2% increase in adult male employment and a 2.5% increase in minor male employment. At the same time, at the county level, aggregate male labor demand did not move. These results suggest that at the locality-industry level, there was substitution between genders. To explore the mechanism, we set up a simple labor-demand framework and conclude that at the locality-industry level, the women-to-men ratio decreased by 4.7%. This impact is larger (–7.5%) in industries in which the share of women is similar to the share of men (25-75%), and smaller (–3.7%) in industries that are either women-dominated (share of women >75%) or men dominated (share of women <25%). After calibrating the change in relative labor cost, we find that, with a constant elasticity of substitution (CES) aggregator of gender-specific labor inputs, the elasticity of substitution is greater than 1, implying that genders were gross substitutes. To conclude the discussion on substitution, we also provide evidence that the margin of substitution is driven by a replacement of women in low-rank occupations with men in middle- or high-rank occupations, suggesting that firms might have altered the way the organized their production in response to a price floor on one of their inputs.

This paper makes three main contributions to the robust literature on the effect of minimum wages (Neumark, Ian and Wascher, 2014; Dube, Lester and Reich, 2016; Cengiz et al., 2019, among others). First, by studying the first U.S. minimum-wage laws, we estimate the effect of introducing a price floor on labor, rather than simply estimating the effect of an incremental change in minimum-wage levels.[11] Second, we analyze minimum-wage legislation that is gender-specific and often industry-specific. The variation induced by these decrees allows us to study the minimum wage in a uniquely transparent environment, and to explore the mechanisms of substitution between workers as a response to the imposition of an input price floor. We take this opportunity to explore the dynamics of the substitution away from factors of production subject to price floors. Third, we contribute to the literature that studies the impact of minimum-wage legislation across markets with different levels of concentration. We measure cross-industry concentration at the county level and compute implied own-wage elasticities of labor demand that are largely in line with the findings in Azar et al. (2019).[12]

From a broader perspective, this paper contributes to the literature on the development of American labor institutions at the beginning of the 20th century,[13] and to the literature on the labor outcomes of women during the same period.[14] First, we estimate the impact of one of the most debated and widely

[10] In practice, in absence of nation-wide detailed earnings data, we compute labor demand elasticities with respect to own-wage by dividing the employment elasticity with respect to minimum wage obtained in the main empirical analysis by the earnings elasticity with respect to minimum wage estimated using the longitudinal sample from Oregon.

[11] The policies we study likely include the largest relative minimum-wage increase (minimum-wage-to-median-earnings ratio) in U.S. history. Using detailed earning data from Oregon, we compute that the minimum wage was between 90% and 103% of median earnings before the regulation was put into effect.

[12] However, in our context, the source of identification comes from differences across counties in county-pairs that straddle state borders, and includes data on all industries.

[13] Among the others, Fishback (1998, 2020); Currie and Ferrie (2000); Goldin (2000); Allen, Fishback and Holmes (2013); Naidu and Yuchtman (2018); Farber et al. (2021).

[14] E.g., Landes (1980); Goldin (1986, 1988*a*,*b*, 1994); Naidu (2012); Poyker (2019).

implemented labor institutions in this country. Second, we document the existence of substitution of women employees by male employees due to 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 earnings gap, conditional on employment. We see this policy change as a unique opportunity to study the interaction between gender roles in the labor market and the evolution of labor relations.

Our paper also contributes to the rapidly growing literature on the gender gap in the labor market.[15] Gender gaps in earnings make labor-protection legislation, such as the minimum wage, more salient to female wage workers, who in the last decades have been consistently over-represented among those earning at or below minimum-wage levels (e.g., Autor, Manning and Smith, 2016). In this regard, our contribution is twofold. First, we explore the individual response of women to a negative shock to labor demand. In particular, we show that marital status—which induces variation in unearned income—determines how affected female workers respond to the shock. Second, we contribute to the findings in Acemoğlu, Autor and Lyle (2004) who estimated gender elasticity of substitution caused by draft during the World War II in the United States. Here, instead of gender-specific labor supply shock, we exploit a demand shock that is asymmetric across genders. We present the estimates of the elasticity of substitution between genders and find them extremely similar to those estimated by Acemoğlu, Autor and Lyle (2004).

1 Background, Factual Records, and Longitudinal Evidence

1.1 The First Minimum Wage in the United States

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 many women could not satisfy their basic needs at current wage levels. For example, when the Kansas Industrial Welfare Commission (1917) surveyed 5,436 women employees, it found that 31% of them earned below $6 per week, concluding that "they hardly have enough to sustain life."[16]

Early minimum-wage legislation came during the *Lochner* era, a period in which American jurisprudence was characterized by a peculiar aversion to any legislation that could be seen as limiting economic liberty. The general view was that introducing a minimum wage would deprive workers and employers of their liberty to negotiate the terms of the employment relationship. Courts were, however, inclined to favor labor-protection legislation that covered only women. Perhaps moved by a paternalistic motive, they would limit womens' liberty of negotiating their employment contracts. We provide details on the gender-bias in labor protection

[15] The minimum wage might also interact with inequality in the labor market through the racial gap. This topic is extensively explored in Derenoncourt and Montialoux (2020), in which the authors study the contribution of minimum-wage legislation on racial earnings gap by exploiting variation generated by the extension of coverage and by the raising rates introduced by the FLSA of 1966. Due to the introduction of coverage in previously FLSA-exempted industries (e.g., agriculture, retail), part of the variation exploited by Derenoncourt and Montialoux (2020) arises from changes in minimum wage from 0 to a positive price floor.

[16] The percentages of employed women who 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).

laws during the Lochner Era in Appendix A.

The highest lower bound to wage rates was set in North Dakota at $20 per week for women working in office occupations; the lowest minimum wage was set at $7 per week for women working in Kansas in the laundry and dry cleaning industry. The Women's Bureau of Labor, which monitored the effect of minimum-wage laws on earnings, reported that these laws were effective in raising the pay of low-skilled women (e.g., Obenauer and von der Nienburg, 1915; Massachusetts Minimum Wage Commission, 1916). 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.[17] Reports from nongovernmental 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.

By 1920, twelve states had adopted minimum-wage-related laws.[18] Arizona, Minnesota, Oregon, Utah, Washington, and Wisconsin eventually adopted minimum-wage laws covering women in all industries, while Arkansas, California, the District of Columbia, Kansas, Massachusetts, and North Dakota implemented minimum-wage laws covering only selected industries (Appendix Figure F.1).[19] States were empowered to punish employers who failed to comply with these laws. The penalty was either a fine or imprisonment. The Women's Bureau of the (Department of Labor, 1928, Ch. XII) describes the enforcement of these laws, the penalties, and the methods and results of investigations.[20]

Almost immediately after the the first law was implemented, in Oregon, manufacturers started to oppose minimum wage. The ensuing legal disputes escalated to the Supreme Court in 1917, in *Settler v. O'Hara.* In a 4-4 tie,the Supreme Court upheld Oregon's minimum wage (McKenna and Zannoni, 2011). Undeterred, opponents continued in their crusade, culminating in another Supreme Court case, in 1923, *Adkins v. Children Hospital.* This time, in a 5-to-3 vote, the Supreme Court struck down the D.C. minimum-wage law, deeming it unconstitutional under the Fifth Amendment's due process clause.[21]

Soon, 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 1938, when they became obsolete.[22] For additional details, the most complete guide to the pre-FLSA minimum-wage legislation can found in Fishback and Seltzer (2021).

[17] For 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.

[18] Decrees were passed later in California (1922) and Massachusetts (1924, 1925, and 1927).

[19] See Appendix Table E.1 for the complete list of minimum-wage laws by industry and year of adoption. Colorado, Nebraska, South Dakota, and Texas also imposed minimum-wage legislation, but they never enforced them, thus 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.

[20] States allowed subminimum 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. 278–279.). States required that employers receive an official license stating that a particular worker was not productive enough ("slow"). Few of these licenses were issued: Washington issued 50, DC issued 87, and California issued at most 2,400 licenses for substandard workers (Thies, 1990, p. 740).

[21] See McKenna and Zannoni (2011) for additional legal details.

[22] Appendix Table E.2 summarizes the timing of implementation and abolition of minimum-wage laws.

1.2 What Contemporary Observers Said

Here, we provide factual records from sources collected by local statistical bureaus and industrial commissions at the time these laws were being put into effect.

Mary Elizabeth Pidgeon, a research economist for the U.S. Department of Labor Women's Bureau said:

> 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, 1937*a*, pp. 8–9)

Appendix Figure F.2 shows that female employment in affected industries in treated states appears to have grown at a comparatively faster pace before the decrees were implemented. Thus, the statement above may be entirely explained by preenactment trends in the data.

The economics literature at the time was (not surprisingly) split among those who, while perhaps agreeing with the legislation's intent, were doubtful about its effects, and those who enthusiastically approved of it. Among the former, Taussig (1916) stated:

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

Similarly, another economist of the marginalist tradition, John Bates Clark (1913), who was also an observer of earlier policies that took place in New Zealand (1894), Australia (1896), and Great Britain (1909), maintained that "we can be sure, without further testing, that raising the prices of goods will, in the absence of counteracting influences, reduce sales; and that raising the rates of wages will, of itself and in the absence of any new demand for labor, lessen the number of workers employed."[23] Clark's view on the minimum wage was elaborate. While he recognized the negative pressure on labor demand as a result of the introduction on a price floor, he advocated for mandatory arbitration and minimum-wage legislation with "emergency employment."[24] Among those who supported the minimum wage, Wolman (1924) highlights the need to support nonunionized workers in a position of weak bargaining power.[25]

Nongovernmental industrial commissions documented the negative effect of minimum-wage laws on women's labor demand. Merchants and Manufacturers Massachusetts (1916), for instance, describes the following case:

> [*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."

[23] Clark (1913), p. 290.
[24] Clark (1913), p. 294.
[25] See Prasch (2000) for a comprehensive review of American economists' views on minimum-wage legislation during the Progressive Era.

The position of labor organizations was not uniform. In fact, the introduction of a minimum wage was one of the legislation recommendations of the National Women's Trade Union League in 1911 (Beyer and Smith, 1929, p. 56). However, the American Federation of Labor, the most widely present (and overwhelmingly male-dominated) labor association in the United States at the time, was strictly against any state intervention in industrial relations that would limit the freedom of bargaining between organized workers and employers (McCammon, 1995).

1.3 Effects of Minimum-Wage Legislation on Wages: Longitudinal Evidence from Oregon

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), but data on wages between the 19th and 20th centuries 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 one of the first empirical minimum-wage studies, by Obenauer and von der Nienburg (1915) in Oregon.[26]

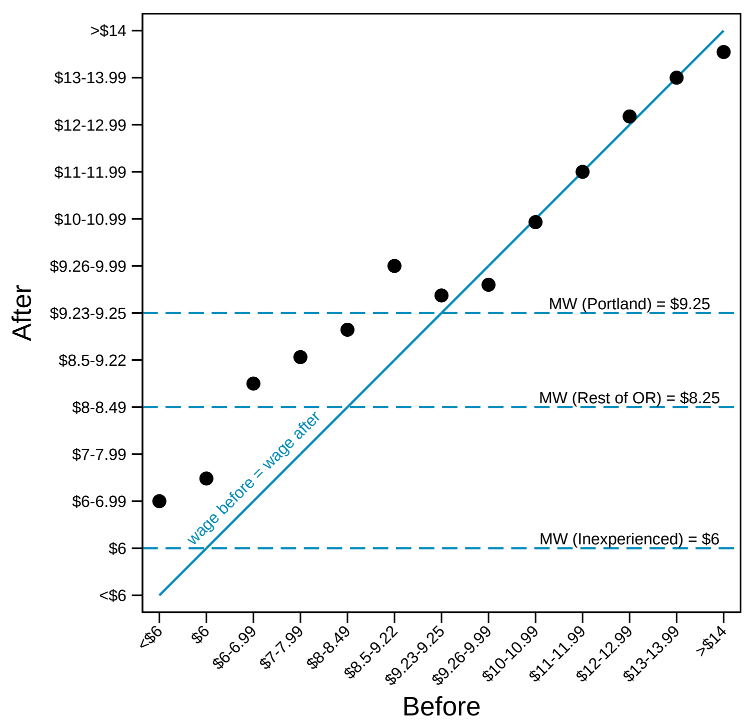

Figure 1: Changes in the Weekly Rate in Oregon Before and After Minimum-Wage Determination for 374 women interviewed by the Bureau of Labor Statistics (Obenauer and von der Nienburg, 1915). We do not observe whether each particular woman is located in Portland or another Oregon location.

In 1915, the Bureau of Labor Statistics published a report featuring data collected to study the impact of minimum-wage legislation in Oregon. Among the data and statistics, they collected information on wages

[26] Kennan (1995) analyzed these data. He concluded that in most of the cases observed, wages remained unchanged after the minimum-wage laws were put into effect.

for a sample of around 370 women across the state with longitudinal information about their wage levels before and after the law was enacted.

These data allow for a within-worker, semiparametric analysis of the impact of minimum-wage laws on wage levels. We plot the weekly wage level after the minimum wage was imposed as a function of the wage before, then we compare the resulting curve with a 45-degree line, which represents the locus where the empirical curve would lie if wages were constant for each wage rank. What we observe in Figure 1 is that all women with prelegislation wages below the highest newly implemented minimum-wage level ($9.25 weekly, in Portland) show an increase increase in weekly earnings, while the wage level is almost unchanged for workers with prelegislation earnings above the highest minimum wage. The 25th percentile of weekly earnings increased from $6 to $8–8.49, while the 75th percentile remained unchanged at a range of $10–10.99. 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.[27]

2 Data and Identification Strategy

2.1 Data

We start with 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. After counting the number of observations in each gender-county-decade cell, we use the ratio of employed adults in each gender-industry-occupation-county-decade cell over the total number of observations in each cell as the primary left-hand-side variable of interest.

The data on minimum-wage laws come from the U.S. Department of Labor Statistics. The Women's Bureau published a list of laws related to employment of women that we collected and coded (Department of Labor, 1924, 1927, 1928, 1937*a*, 1939). Then we matched those laws to our dataset using Census industry codes.[28] We summarized these laws in Appendix Table E.1 and Appendix Table E.2.[29]

2.2 Sample Construction and Identification in the Border-County-Pair Setting

Contiguous-border county-pairs (CBCP) form the best treatment-control comparison, because they allow conditioning on unobserved local and industry-specific trends (Holmes, 1998; Huang, 2008; Dube, Lester and Reich, 2010; Coviello, Deserranno and Persico, 2018).[30] In our setting, this is particularly important because

[27]Other examples (albeit nonlongitudinal) of the effectiveness of minimum-wage laws on raising female earnings can be found in (Thies, 1990, pp. 727–735), who analyzes the case of wage increases in the brush industry in 1911–1914 in Massachusetts.

[28]We use the variables `IND1950` and `OCC1950` containing approximately 150 and 250 categories respectively.

[29]We codified only laws enacted up to 1930. In a few cases, the dollar value of the minimum wage changed several times between Census waves. When we compute the 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 a zero to a nonzero minimum wage. Because these changes are very small, our results are all almost identical if we use minimum wages in play at the time of the 1920 and 1930 Census waves, or if we use wages weighted by years.

[30]See Dube, Lester and Reich (2010) for a taxonomy of the differences between identifying the effect of state-level policy changes in a "full sample" of all counties vs identifying the same changes in a border-county sample.

the CBCP sample allows controlling for trends in gender discrimination in the labor market, labor-force participation, and growth in female-intensive industries.

Our preferred identification sample consists of only contiguous county pairs that straddle state borders. The twelve states with gender-specific minimum wages (we count the District of Columbia as a state) have 24 discrete adjacent states—36 states are thus included in the analysis. The analysis covers 701 counties in 419 distinct county-pairs. Figure 2 depicts the contiguous counties included in the analysis. Counties located in minimum-wage states appear in dark blue, and those located in non-minimum-wage states appear in light blue.

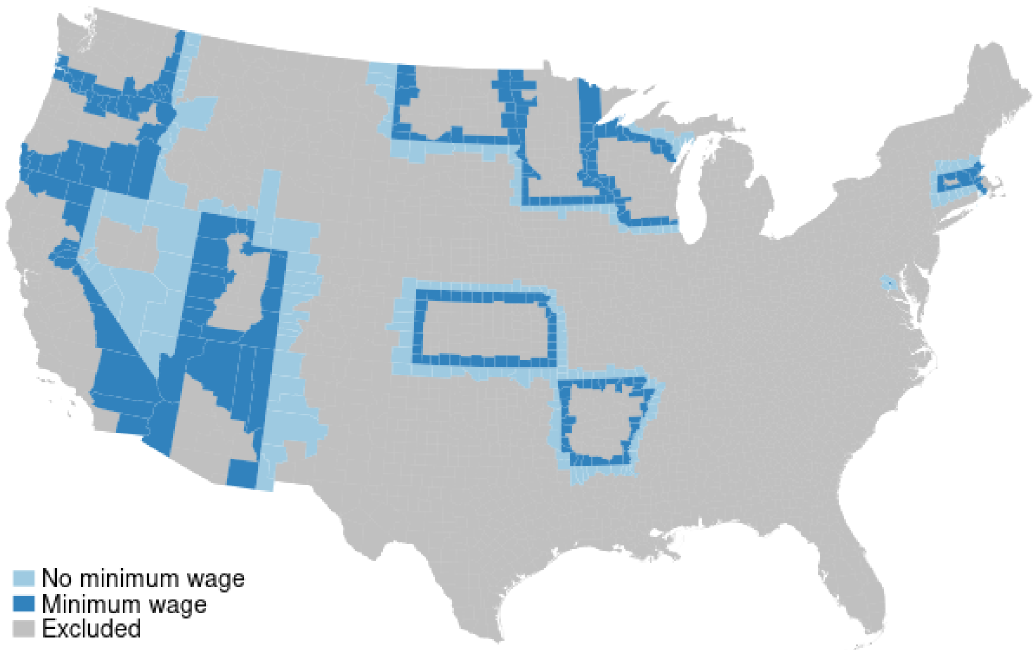

Figure 2: Contiguous-Border County-Pairs in Our Sample

Table 1 provides a detailed breakdown of the number of pairs on each of the 42 state border-segments and clarifies how many segments are linked to each state.[31] 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 the weekly minimum wage in Utah is equal to \$7.5 and in Arizona equal to \$10.

Similarly, segment #13, shared by California and Oregon, will provide identifying variation since, while Oregon has a universal minimum-wage law for women, California's minimum-wage laws cover only a subset of industries (see Appendix Table E.1 for details).

[31]We define a border-segment as the set of all counties on both sides of a border between two states.

Table 1: Contiguous-Border County-Pairs

	Pairs		# counties			Types of min.wage laws		Avg. weekly min. wage, $		# periods when laws are active	
Segment	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 3 will do. The table also visualize states' average minimum wages across industries with the minimum wage (or all-state minimum wages). Number of periods when laws are active indicates whether laws were active only in 1920 or in 1920 and 1930. In segment #33, OR–WA, we set Multnomah County (containing the city of Portland) to have a different minimum wage than the rest of the counties in Oregon.

In segment #28, both Minnesota and Wisconsin have minimum-wage laws that cover women in all industries; however, Wisconsin abolished its law in 1924 (see Appendix Table E.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.[32]

While the advantages of this strategy in terms of parameter identification (which we discuss in Section 3.1) do not 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 that this is the case, Table E.3 provides summary statistics on different subsets of counties. Column-set I reports statistics on socioeconomic characteristics of all counties. Column-set II reports the same for only counties in the CBCP sample. 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. In particular, our CBCP sample is not statistically different from the full sample in terms of total population, share of women, ratio of employed women to employed men, or labor-force participation. Column-set IV reports the difference between cross-border contiguous counties. It shows that *within* such pairs, socioeconomic characteristics do not significantly vary between counties.

3 Effects on Industry-Specific Local Female Employment

In this section, we report the results of the regression analysis for the effect of gender-specific minimum-wage laws on female employment. Section 3.1 introduces our empirical specification. Section 3.2 reports the main employment results. Section 3.3 contains robustness and sensitivity checks.

3.1 Empirical Specification: Employment by Industry

First, we estimate the effect of a minimum wage on the CBCP sample. The specification is as follows:

$$\ln\left(EmpShare_{gip(c)t}\right) = \beta \cdot \text{Minimum wage}_{ist} + \mu_{st} + \Psi_{p(c)t} + \Phi_{is} + \Phi_{it} + \Phi_{p(c)i} + \epsilon_{gip(c)t}, \quad g = \{w\} \quad (1)$$

where the unit of observation is an industry-occupation i, in county-pair $p(c)$, nested within state s, in decade t. Here, only contiguous county-pairs that straddle state borders can contribute to the identification of β. This is reflected in the presence of county-pair-specific time fixed effects, $\Psi_{p(c)t}$. Here, we show results only for female workers (i.e., $g = w$); we provide results for men and minors in Section 6.1.

Following Neumark, Ian and Wascher (2014) and others, 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

[32] All segments generate variation for the dollar value and log specifications; two segments (#11 and #33) do not contribute to the identification for the binary-variable specification. In other words, if both states have adopted minimum-wage laws for all industries, these border-segments are, essentially, dropped.

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.[33]

We use three measures of the explanatory variable of interest. The first—Minimum wage, $\$10_{ist}$—is a ten-dollar (\$10) 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 the minimum wage by ten dollars. The second—$\mathbb{1}\left(\text{Minimum wage}\right)_{ist}$—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 the minimum wage. The third—$\log\left(\text{Minimum wage}\right)_{ist}$—is an inverse hyperbolic sine of the dollar value of the minimum wage in industry-occupation i in state s at year t. It can be interpreted in the same way as a standard logarithmic variable but without needing to adjust for zero values (Burbidge, Magee and Robb, 1988; Card and DellaVigna, 2017) if there is no minimum-wage legislation. Thus the coefficient should be interpreted as elasticity.

μ_{st} are state-specific time controls, $\Psi_{p(c)t}$ are county-pair-decade fixed effects. Violations of minimum-wage laws were not unusual, and heterogeneity likely existed in law enforcement and penalties across states (Women's Bureau of the Department of Labor, 1928). State- and county-pair-decade fixed effects allow us to absorb location-specific trends in law enforcement, permanent migration, or labor supply shocks (e.g., World War I draft).

Φ_{is}, and Φ_{it} are industry-state and industry-decade fixed effects. The former address possible state-specific support to certain industries; while the latter absorbs industry-specific trends (e.g., technological progress). We also absorb industry-county-pair fixed effects ($\Phi_{ip(c)}$) that address local, county-pair-specific support for certain industries or introduction of marriage bars.

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 a full sample specification, we would be able to control for a variety of location and industry trends, but the identifying variation still would rely on comparing (after absorbing fixed effects) a county-industry cell in, for example, North Dakota with a county-industry cell in Pennsylvania. Dube, Lester and Reich (2010) provide 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 labor, gender discrimination, and

[33] The results do not change if we use $\ln\left(\#employed_{gic(s)t}\right)$ as a dependent variable and control for the logarithm of the gender-specific population level on the right-hand side. Also, results are qualitatively unchanged (and statistically significant) if we use the raw employment share as the dependent variable.

the evolution of local labor institutions. At the same time, non-industry-specific legislative trends, which are not local, will still be absorbed by state-decade fixed effects (μ_{st}) and by county-pair-decade fixed effects ($\Psi_{p(c)t}$) in our preferred specification. As our 12 states are more likely to impose labor protective regulation for women we also expect them to have positive trends in female employment thus biasing the coefficient β upward in the full sample. In Appendix B.1, we introduce a specification and discuss results for the effects of minimum wages on the full sample of all U.S. counties. We see that, while the coefficients are negative, they are smaller in absolute terms and not significant, reinforcing our concerns of local positive trends in female employment.[34]

The presence of a single county in multiple pairs along a border-segment induces a mechanical correlation across county-pairs, 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 levels (Cameron, Gelbach and Miller, 2011).[35]

3.2 Minimum-Wage Laws Decreased Female Employment at the Industry-Level

Table 2 contains results of the estimation of Equation (1) for female employment, using only counties in pairs that straddle state boundaries. Each row contains coefficients from a separate regression.

Specifications get incrementally more demanding from left to right: Column I reports results for the specification with (time-invariant) county-pair fixed effects $\Psi_{p(c)}$, as well as state-specific industry/occupation Φ_{is} and year fixed effects μ_{st}.[36] Column II adds industry/occupation-specific year fixed effects Φ_{it}, absorbing all national-level occupation-specific technological changes over time. Column III includes county-pair-specific industry/occupation fixed effects, that control for locality-specific heterogeneity in industrial policy. Columns IV–VI are analogous to the previous three columns, with the exception that we now control for county-pair-specific year fixed effects, which allow for locality-specific employment trends. Column VI, our preferred baseline specification, is analogous to the most conservative specification in Dube, Lester and Reich (2010), with the exception that we add industry variation.

In the first row of Table 2, we report results for the variable Minimum wage, $\$10_{ist}$. We find that, on average, in a linear setting, increasing minimum wage by \$10 corresponds to a 1.5% drop in female employment.[37] The second row contains estimates from the same specifications, with the exception that the main right-hand-side variable is binary, and equal to 1 if an industry i in state s is covered by minimum-

[34] If one is concerned that our results with CBCP are not generalized to the full sample, later, in Section 5 we show significant negative effect of minimum wage laws on employment and labor force participation using the novel census linkage of always married and never married women on the *full* sample. That specification uses within women variation, thus addressing the local trends in female employment and allowing us to look at all counties at the expense of excluding women who got married between censuses in order to ensure correct linkage.

[35] All results hold if, instead, we double-cluster by state and border-segment. Our results are robust to the way we define end-counties in the border-segments (i.e., those that may belong to more than one border-segment). Our results also hold if we cluster by the number of pairs that each county has.

[36] Appendix Figure F.3 contains the raw data results (without fixed effects).

[37] Given that some of the minimum wage decrees we study in this paper were imposing a floor on weekly earnings, a working hours adjustment would be an additional margin along which employers could respond to new legislation. In this case, the likely response would have been an increase in weekly hours, which would imply that the estimates we obtain are a lower bound on the actual effect of minimum wage on employment.

wage legislation at time t. This panel shows that treated industries have on average 3.1% lower employment of women. Finally, the last panel estimates an elasticity of employment with respect to minimum wage. The estimate shows that a 100% increase in the minimum-wage level (i.e., roughly corresponding to the ratio between the newly introduced minimum wage and the lowest earning level before the Oregon law) corresponds to a 0.8% decrease in female employment. Since the identifying variation in minimum-wage implementation is due to changes from no minimum wage (i.e., minimum wage equals 0) to some positive value (i.e., around \$10 on average), the first two panels in Table 2 are more easily interpretable, because the elasticity parameter in Panel C does not capture well the effect of changes from 0 to positive values.[38]

Table 2: The Effect of Minimum-Wage Legislation on Employment of Women

	I	II	III	IV	V	VI
	Dependent variable: Log employment share (women)					
Panel A:						
Minimum wage, \$10	-0.056**	-0.032**	-0.025***	-0.053*	-0.025**	-0.015***
(mean min. wage \$10.2)	(0.027)	(0.013)	(0.008)	(0.027)	(0.011)	(0.0044)
R-squared	0.713	0.734	0.792	0.719	0.740	0.797
Observations	273,883	273,883	273,883	273,883	273,883	273,883
Panel B:						
1(Minimum wage)	-0.075***	-0.050***	-0.041***	-0.075***	-0.045***	-0.031***
	(0.024)	(0.009)	(0.006)	(0.024)	(0.008)	(0.0032)
R-squared	0.713	0.734	0.792	0.719	0.740	0.797
Observations	273,883	273,883	273,883	273,883	273,883	273,883
Panel C:						
log (Minimum wage)	-0.023**	-0.013***	-0.011***	-0.023**	-0.011***	-0.008***
inverse hyperbolic sin	(0.009)	(0.003)	(0.001)	(0.010)	(0.003)	(0.0006)
R-squared	0.713	0.734	0.792	0.719	0.740	0.797
Observations	273,883	273,883	273,883	273,883	273,883	273,883
County-pair & year FEs	✓	✓	✓			
County-pair-year FEs				✓	✓	✓
Industry-state & occupation-state FEs	✓	✓	✓	✓	✓	✓
State-year FEs	✓	✓	✓	✓	✓	✓
Industry-year & occup.-year FEs		✓	✓		✓	✓
Ind.-county-pair & occup.-county-pair FEs			✓			✓

Notes: This table reports the results from estimating equation (1). Each panel contains coefficients from separate regressions with Minimum wage in dollars$_{ist}$, $\mathbb{1}$ (Minimum wage), and log(Minimum wage) as explanatory variables. Each observation is a gender-specific industry-occupation-county-decade. Standard errors, triple-clustered at the state (36), industry-occupation (4,714), and border-segment (42) levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

3.3 Robustness of the Identification Assumption and Alternative Explanations

We provide more 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,

[38]We can also further expand our understanding of the effects of minimum wage by examining whether there are different impacts from the introduction versus abolition of state minimum wage laws. As after *Adkins v. Children Hospital* in 1923, seven states abolished their gender-specific minimum wage laws, we can separately define variable for introduction/increase in minimum wage and variable for abolishment/decrease in minimum wage and estimate our equation 1. For this, in Appendix Table E.4 we replicate our most conservative specification from Column VI of Table 2. Our estimates for introduction of minimum wage remain negative and significant. At the same time we see statistically significant increase in female employment in location-industries where minimum wage was abolished. The magnitude of the coefficients for introduction and abolishment of minimum wages are similar in sizes; although, they are slightly larger in absolute terms for the abolishment. It is hard to interpret it as the set of states/locations that abolished minimum wages is different (a subsample) from the initial set of states that imposed it.

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.[39] Second, we discuss and address the potential bias coming from internal migration of either individual workers (supply-side) or establishments (demand-side), induced by the regulation. Third, we show that potentially confounding factors, such as contemporary labor legislation, are not driving the results.

Parallel trends To tackle the issue of the "parallel trends," in Appendix Table E.8, we provide several placebo tests that demonstrate that our results are not driven by preexisting 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 identifies 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 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 the minimum wage of $t = 1930$ is set at $t' = 1910$. We exclude 1920 and 1930 from the regressions to make sure that treated states-counties are not in the regression. None of the resulting estimates 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.

Migration When accounting for the possibility of migration across borders, it is worth mentioning the interpretation of our results. First, all estimated models in this paper, by applying a triple-difference strategy, account for county-level changes in employment rates over time. Second, permanent migration is accounted for because our specifications all have employment rates on the left-hand side, thereby accounting for changes in population across counties over time. Third, even in the unlikely scenario 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 attenuated results. Finally, using a newly constructed linked census (see full discussion in Section 5.1) we demonstrate that women in affected counties-industries do not differentially migrate out (see Appendix Table E.12).[40] In addition to this supply-side evidence, we also provide demand-side evidence suggesting that our results cannot be explained by firms crossing over state borders to avoid policies. In particular, we check that minimum wage laws had no impact on the total number of establishments at the county level.[41]

Confounding Factors I: Maximum Working Hours Legislation To address potentially confounding factors we directly control for another piece of labor legislation passed at around the same time that covers

[39] We also test for the presence of pretreatment trends in a fully-dynamic difference-in-differences specification. We plot the corresponding graph in Appendix Figure F.4.

[40] While we are not able to measure commuting movements across counties, these too would have the impact of attenuating our estimates. Given the state of public transportation and the scarce availability of personal means of transportation—especially to women earning minimum wage—, we believe that our results are not biased by this particular channel.

[41] Here we use equation 2 with log number of firms as the dependent variable. We find, essentially, zero effect of minimum wages (point-estimate= 0.002 and s.e.=0.025). See full discussion of that specification in Section 4.

maximum weekly hours for female workers (Department of Labor, 1927, 1937*b*).[42] In Appendix Table E.7, we add to our main specification an interaction term between a binary variable indicating that a county-industry-decade cell has working-hours regulation and a binary variable equal to 1 if the state has ever had minimum-wage legislation. 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 that cap working hours.[43]

Confounding Factors II: World War I Draft of 1917–1918 Another confounding factor that may affect our results is the draft caused by U.S. decision to join Entente closer to the end of the World War I (WWI). As a result, in 1917–1918 approximately 4.8 millions of the American (mostly White) men were drafted ($\sim$2 millions of them as volunteers).[44] One may think that such a significant shock for the labor supply can bias our results; hence, here we provide several arguments explaining that it does not. First, as WWI draft is a male labor supply shock, similar to our discussion of migration, our triple-difference strategy accounts for county-level changes in employment rates over time with county-pair-year fixed effects. Second, our treatment is industry-specific and even if the drafted men were more likely to be from a low-skilled occupations it should still correlate with a specific set of treated industries in a specific locations to bias our results. Third, since the drafted men were low-skilled, and were more likely to compete for the same jobs with women (both within industry and between low-skilled industries) this would actually make women more likely to be hired. Thus possible correlation of WWI draft with gender-specific minimum wage laws is working against us finding negative effect of minimum wage on employment. Finally, we use county-level data from WWI veterans from the 1930 Census as a proxy for WWI draft to show that it does not drive our results.[45] Column I of Appendix Table E.9 controls for the log of WWI veterans and column II shows no differential effect of draft in locations with minimum wage on the employment of women in 1920 (Columns I–III).

Confounding Factors III: Marriage Bars Another possible confounding factor is related to the marriage bars, popular in the first half of the 20th century in the U.S. Marriage bars were policies adopted by firms and local school boards, with the intention to fire single women when they married and not to hire married women. Here we provide evidence that marriage bars do not explain our result.

First, marriage bars were policies of individual firms and, in the case of school boards, individual local-

[42] The best reference on this topic may be Goldin (1988*b*).

[43] We don't intend to causally estimate the effect of maximum (weekly) working hours of women on female (and male) employment in this paper. We coded only working-hours legislation in minimum-wage states that can be confounded to our treatment. 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.

[44] See Rockoff (2004) for the additional information.

[45] Because county-pair-year fixed effects absorb any changes in male labor supply due to WWI draft, we, instead, show that WWI draft did not affect *aggregate* effect of minimum wage on employment, in the aggregated county-pair-decade level specification that we discuss later in Section 4. Here we make an assumption that WWI veterans are still living in the same county in which they were drafted. We do exclude immigrants, non-citizens, and those whose birthplace is not the same as their current place of residence. Overall, our results do not depend on how we define WWI veterans.

ities.[46] Hence, our identification with CBCP sample is ideally suited to address such local trends; if they exist, they should be similar in adjacent counties. Moreover, as marriage bars were concentrated in certain industries our baseline specification with industry-decade and industry-state fixed effects should absorb trends in introduction of marriage bars.

Second, marriage bars were not widespread in 1920 (our first decade of treatment) and became more widespread by 1930 (our second decade of treatment for some states). Goldin (1988*a*, p.6) writes that "in 1920 just 11% of all married women in the labor force were teachers and clerical workers," two sectors where marriage bans were imposed at that time. However, by 1928, "61% of all school boards would not hire a married woman teacher and 52% would not retain any who married while on contract." By 1931, in addition to schools, office occupations such as insurance offices, publishing firms, banks, and public utilities imposed extensive marriage bars and discretionary policies related to married women.[47] As a result, we first show that our results hold without $t = 1930$ to exclude the decade when marriage bans become more widespread (we discuss subsample analysis in grater details in the next Section 3.4). And second, we show that our results are not driven by a particular industry. Figure F.5 shows robustness of our results to omission of various industries, including educational services (part of Entertainment and Recreation Services) and clerical work (e,g., public administration or business services). The point estimates or significance of the effect of minimum wage remains almost unchanged.

Finally, marriage bars could confound our results through the return of WWI veterans. After the end of the WWI, (predominantly low-skilled) veterans that returned home would eventually compete with women. And starting from the 1920s, the introduction of marriage bars sought to exclude married women from the labor market to favor men. Thus, the wartime increase in the female workforce and the rise in the cost of female labor via the minimum wage legislation would make it more profitable for firms to introduce marriage bar regulations. To address this concern, we interact WWI veterans with minimum wage in 1930 in column III of Table E.9 to show that locations with minimum wages that experienced larger return of veterans indeed experienced even larger decline in female employment afterwards (although marginally insignificant, p-value=0.122). However, the interaction does not explain our results; the coefficient for minimum wage remains negative and significant. Overall, these findings suggest that marriage bars may exacerbated the effect of minimum wage laws but they do not completely explain away our results.[48]

Confounding Factors IV: Unions and Suffrage Finally, we briefly discuss the role of unions and of women's political power. First, the National Women's Trade Union League in 1911 called for legislation guaranteeing a minimum wage (Beyer and Smith, 1929, p. 56). During the same period, despite increasing collaboration between the League and the American Federation of Labor, AFL President Samuel Gompers was openly opposed to any labor regulation that legislated a minimum wage (Amsterdam, 1982). While we

[46] The best reference on this topic may be Goldin (1988*a*).

[47] 12% of all firms in Goldin's data had marriage bars and 23% of all female employees were in firms having such policies. See Table 1 in Goldin (1988*a*) for details.

[48] We believe that this is a very interesting topic deserving a separate further investigation.

are not aware of direct evidence that this was the case, one of the amplifying channels behind the reduction in female employment might have been the waning support of male-dominated trade unions. Second, women's suffrage came about in the United States in 1920, with the 19th Amendment; however, fifteen states granted the right to vote to women before 1920. Of these fifteen, six had also implemented minimum-wage for women. This means that, not surprisingly, a correlation exists between women's suffrage and minimum-wage legislation covering women (states with preamendment female suffrage were about three times as likely to pass gender-specific minimum-wage laws). However, all our specifications partial out state-year fixed effects, taking care of any state-level institutional change.

3.4 Additional Robustness and Sensitivity Checks

Here, we provide additional robustness and sensitivity checks. We consider robustness to (i) exclusion of any industry or any minimum-wage state (and its adjacent border-segments); (ii) inclusion of nonoccupational groups or exclusion of 1880 or 1930 Census years; and (iii) alternative, more conservative specifications.

To demonstrate that our results are not driven by any specific state, Figure 3 reports on the robustness of our preferred estimate in column VI to dropping one state at a time. The estimated coefficient always remains significantly different from zero. Dropping Kansas, which shares border-segments with four states (segments # 16, 17, 18, 19 in Table 1), decreases the coefficient the most, from -0.031 to -0.037. Dropping the District of Columbia, which shares border-segments with two states (segments # 14, 15), increases the coefficient the most, from -0.031 to -0.028.[49] Similarly, in Appendix Figure F.5, we show the robustness of our preferred estimate in column VI to dropping one industry at a time.[50] The estimated coefficient always remains significantly different from zero. Dropping manufacturing of nondurable goods decreases the coefficient the most, from -0.031 to -0.051. Dropping retail increases the coefficient the most, from -0.031 to -0.017.[51] Our results are inline with the recent findings by Harasztosi and Lindner (2019) that disemployment effects are larger in tradable sectors as opposed to non-tradable ones. However, this pattern appears to be less pronounced in the case of historical minimum wage laws for women because of the cross-gender substitution margin.

Our results hold if we keep nonoccupational industries instead of setting them to missing. We repeat our baseline results for women in the Appendix Table E.5.

In Appendix Table E.10, we show the robustness of our main results to dropping 1880 and 1930, both one at a time and together. Panel A shows the results after dropping observations in 1880, Panel B does the same with 1930, and Panel C shows the results after dropping both 1880 and 1930. We are particularly interested in the robustness to excluding 1930 observations, because by that year some states had repealed

[49]Dropping Massachusetts, which shares border-segments with five states (segments # 20, 21, 22, 23, 24), has almost no effect on the coefficient; however, standard errors increase. We hypothesize that this happens because dropping these five border-segments (and thus six states) decreases the sample size the most (by 31%).

[50]This exercise also helps us to address possible confounding factors that may be concentrating in a specific industry. E.g., marriage bars among teachers (Goldin, 1988*a*) or competition with prison-made goods in apparel industry (Poyker, 2019).

[51]Dropping personal services increases standard errors the most. This is because omitting this industry reduces the sample size the most, by 25%.

their minimum-wage regulation, and they might have done so as a reaction to the effects of the regulation itself.[52] Omission of 1930 also helps us to address possible concern that the onset of the Great Depression disadvantaged female workers. As the Great Depression might have amplified the impact of the minimum wage legislation causing upward bias in our estimates, it is important to demonstrate that our results also hold without 1930. Overall, our main results are remarkably robust to any of these exclusions, both qualitatively and quantitatively.

Figure 3: State-Exclusion Robustness of the Results for $\mathbb{1}$ (Minimum wage)$_{ist}$ in Table 2

Notes: This figure reports on the point-estimate and 90th-percentile confidence band that results when re-estimating the core specification in Column VI of Table 2, dropping one state at a time. 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., AR is omitted first, then AZ, then CA, etc.

Finally, in Appendix Table E.6, we introduce an even more conservative specification than the one in (1), by adding industry-occupation-year and industry-occupation-state fixed effects.[53] Comparing the baseline coefficient of Minimum wage, \$10$_{ist}$ in the first row of column VI of Table 2 to the coefficient in column I of Table E.6, we see that including industry-occupation-year fixed effects increased R^2 from 0.797 to 0.902.

[52] While we also decided to show robustness to excluding 1880 observations after an anonymous referee raised the question of data quality for that year, we are not particularly worried about data quality that is not different across locations and industries.

[53] We can do so because our observation is on county-pair, industry-occupation, and year levels, and because previously we were including industry- and occupation-interacted fixed effects separately.

However, the coefficient did not change much and remains significant. Results hold when we add industry-occupation-state fixed effects. We obtain similar results in a specification with nonoccupational groups (columns III–IV) and other measures of minimum-wage treatment (columns V–VIII). While this specification yields significant estimates of comparable magnitude, it is restrictive—up to 15% of the observations are singletons absorbed by fixed effects. Nevertheless, we consider these results important in showing that there's not enough room for unobservables to explain away our results.

4 Effects on Aggregate Local Female Employment

In the previous section, we demonstrated that the introduction of a minimum wage decreased employment of women in affected industries and locations. However, using the triple-differences specification in Equation 1, we are not able to disentangle the two channels that might give rise to a drop in labor demand. In particular, we are now interested in understanding to what extent the impact of minimum-wage legislation on employment is driven by an aggregate decrease in employment or by a movement across industries. In this section, we estimate the former channel; in Section 5, we focus on the latter channel.

To estimate the aggregate impact of minimum-wage legislation at the local level, we use the same CBCP identification strategy outlined above, except this time we aggregate our data up to the county-year level. Our differences-in-differences specification is as follows:

$$\begin{aligned}\ln\left(\mathit{EmpShare}_{gp(c)t}\right) &= \beta_1 \cdot \text{Min. wage}_{st} + \beta_2 \cdot \text{Min. wage}_{st} \times \text{Share affected workers}_{p(c),1910} \\ &\quad + \mu_t + \Psi_{p(c)} + \Phi_s + t\lambda_s + \mathbb{X}_{p(c)t} + \varepsilon_{gp(c)t}, \qquad (2)\end{aligned}$$

where $\mathit{EmpShare}_{gp(c)t} = \log\left(\frac{\mathit{Total\ Employment}_{gp(c)t}}{\mathit{Total\ Working\ Age}_{gp(c)t}}\right)$ in county-pair $p(c)$, gender $g = w$, and year t. Share affected workers$_{p(c),1910}$ is the share of female workers employed in industries affected by minimum-wage laws in 1910 (i.e., before the treatment). This variable is equal to 0 if state s has never adopted a minimum wage and is equal to 1 if state adopted a universal minimum wage for women. $\Psi_{p(c)}$, Φ_s, and μ_t are county-pair, state, and decade fixed effects, respectively. $t\lambda_s$ are state-specific linear trends. $\mathbb{X}_{p(c)t}$ is the matrix of county-year level controls. Here we only employ the most parsimonious set of controls: log of population, share of women, share of rural population, and share of literate population.[54] We double-cluster standard errors by state and border-segment. We include a measure of treatment intensity because we expect the impact to vary as a function of the share of women covered by the legislation.

[54]Our results remain virtually unchanged if we additionally control for share of Black or other available variables from the census.

Table 3: The Effect of Minimum-Wage Legislation at the County Level

	I	II	III	IV	V	VI
	Dependent variable: Log employment share					
Sample	Women			Men		
Panel A:						
Average minimum wage, $	-0.017**	0.015	-0.125***	0.001	-0.006	0.009***
(mean av. min. wage $6)	(0.006)	(0.017)	(0.044)	(0.004)	(0.006)	(0.003)
Average minimum wage, $		-0.035**			0.019**	
x Share women in treated industries in 1910		(0.017)			(0.007)	
Average minimum wage, $			0.184**			-0.070***
x HHI in 1910			(0.071)			(0.021)
Mean of the interacted variable	-	0.71	0.59	-	0.71	0.28
R-squared	0.797	0.798	0.800	0.822	0.822	0.824
Observations	3,020	3,020	3,020	3,042	3,042	3,042
Panel B: ~w dummy						
1(Minimum wage)	-0.019	0.132*	-0.376**	-0.010	-0.049**	0.042*
	(0.038)	(0.066)	(0.153)	(0.027)	(0.023)	(0.023)
1(Minimum wage)		-0.266***			0.159***	
x Share women in treated industries in 1910		(0.077)			(0.056)	
1(Minimum wage)			0.606**			-0.323***
x HHI in 1910			(0.225)			(0.073)
Mean of the interacted variable	-	0.71	0.59	-	0.71	0.28
R-squared	0.797	0.798	0.800	0.822	0.822	0.824
Observations	3,020	3,020	3,020	3,042	3,042	3,042
Panel C: ~ elasticity						
log (Minimum wage)	-0.033**	0.059*	-0.291***	0.001	-0.010	0.020***
	(0.014)	(0.034)	(0.088)	(0.009)	(0.009)	(0.006)
log (Minimum wage)		-0.108***			0.033**	
x Share women in treated industries in 1910		(0.032)			(0.015)	
log (Minimum wage)			0.437***			-0.147***
x HHI in 1910			(0.142)			(0.036)
Mean of the interacted variable	-	0.71	0.59	-	0.71	0.28
R-squared	0.797	0.798	0.800	0.822	0.822	0.824
Observations	3,020	3,020	3,020	3,042	3,042	3,042

Notes: This table reports the results from estimating equation (2). Each observation is a gender-specific county-decade. Each regression includes county-pair, state, and year fixed effects. The following variables are used as controls: log of total population, share of women, share of rural population, share of literate population, and state-specific linear time trends. Standard errors, double-clustered at the state–border-segment level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3 reports the results of the estimation. Panel A shows the results with the dollar value of minimum wage as the main treatment variable, Panel B shows the results using a dummy variable, while Panel C shows the results with the inverse hyperbolic sine of the minimum-wage level, which can be interpreted as an elasticity of employment with respect to the minimum wage. Column I shows the results without the interaction, and suggests that the female employment-to-population ratio decreased by 1.9% (Panel B). Column II shows the estimates coming directly from equation (2) by adding an interaction with the pretreatment share of workers in affected industries. β_2 appears to be negative and significant, suggesting that counties with a larger share of affected women experienced a larger decline in labor-force participation.

β_1, which corresponds to the impact of the minimum wage when the share of treated workers is 0, is small and not statistically distinguishable from 0. In Section 4.1, we focus on Column III.

4.1 Minimum Wage and Cross-Industry Local Labor-Market Concentration

In an alternative specification, appearing in column III of Table 3, we interact the minimum-wage treatment with a measure of county-level concentration across industries in the pre-period—a cross-industry HHI computed in 1910—to understand whether the impact of minimum-wage legislation is related to the local labor-market structure.[55] We build on the theoretical prediction that market concentration (e.g., oligopsony or monopsonistic competition) can be associated with positive effects of the minimum wage on employment (e.g., Stigler, 1946; Bhaskar, Manning and To, 2002) and interact the policy variable with a cross-industry HHI at the county level in 1910.[56] The intuition is built on cross-firm within-industry concentration. In a market without a price floor with a few employers, firms can keep the wage and employment levels down, below perfect-competition levels. However, the introduction of a price floor on labor might force firms to move to a higher-wage, higher-employment equilibrium. We do not observe within-industry cross-firm concentration at the county level, and instead we build a within-county index of concentration across industries to capture this channel. Consistent with the theoretical prediction, we find that the higher the market concentration (i.e., few industries employ all the active labor force), the smaller the negative impact of the price floor on female labor demand.[57]

The results in column III show that, while the introduction of minimum-wage legislation in a market in which each industry controls a very small share of the prelegislation overall employment (e.g., cross-industry HHI = 0) would shrink employment by 12.5%, in a labor market dominated by only one industry the estimated impact of the minimum wage would be to *increase* female employment by 6.9%. Next, we compute the implied own-wage elasticities of labor demand using the results from Panel C and the elasticity of earnings with respect to minimum-wage levels derived from the earnings data from Oregon. The prelegislation lower bound on weekly earnings in Oregon was \$6, while the statewide minimum-wage level imposed in 1914 was \$8.25.[58] To calculate the elasticity of earnings with respect to the minimum wage, we assume that the increase in minimum wage was by $\frac{8.25-6}{6} = 37.5\%$, which translated into an increase in

[55] This measure is computed as follows:

$$HHI_{c,1910} = \sum_{i \in I_{c,1910}} s_{ic}^2,$$

where i is an index for an industry belonging to the set $I_{c,1910}$ of industries that employed one or more people in county c in 1910. Unfortunately, data limitations prevent us from seeing an index of market concentration based on employment levels at the firm or establishment level; however, using data from Haines (2010), we verified that our measure of cross-industry concentration is positively correlated with the inverse of the number of establishments in a given county (both measured in 1900, given that establishment data are not available in 1910). This is reassuring because 1/(N of establishments) in a given county is the lower bound on local cross-establishment concentration.

[56] Our results hold if, instead, we use only industries covered by minimum wage laws to construct HHI. However, it is more difficult to interpret such measure as some counties in border states are not treated. In such situations we have to use the set of treated industries of their respective treated pairs to construct HHI. These results are available upon request.

[57] The results for men are shown in columns IV–VI and, as expected, they mimic the results for women, but with a flipped sign and smaller magnitude. We discuss the results on the sample of men in greater detail in Section 6.1.

[58] This rate was applied in Oregon with the exception of the city of Portland, which had a minimum wage of \$9.25.

postlegislation earnings of 6.8%. Taken together, these numbers imply an elasticity of $\frac{0.068}{0.375} = 0.18$. Given the elasticity of earnings with respect to the minimum wage, we use the estimates from Panel C to compute the implied own-wage elasticity by dividing the estimates by 0.18. We find that the own-wage elasticity to the minimum wage ranges from −1.6 (HHI = 0) to 0.8 (HHI = 1). These values are in line with those found in the previous literature related to the impact of the minimum wage as a function of labor-market concentration, as nicely summarized by Azar et al. (2019). Following their approach, we plot the values of our implied elasticities for different levels of cross-industry concentration and compare them to the literature in Figure 4.

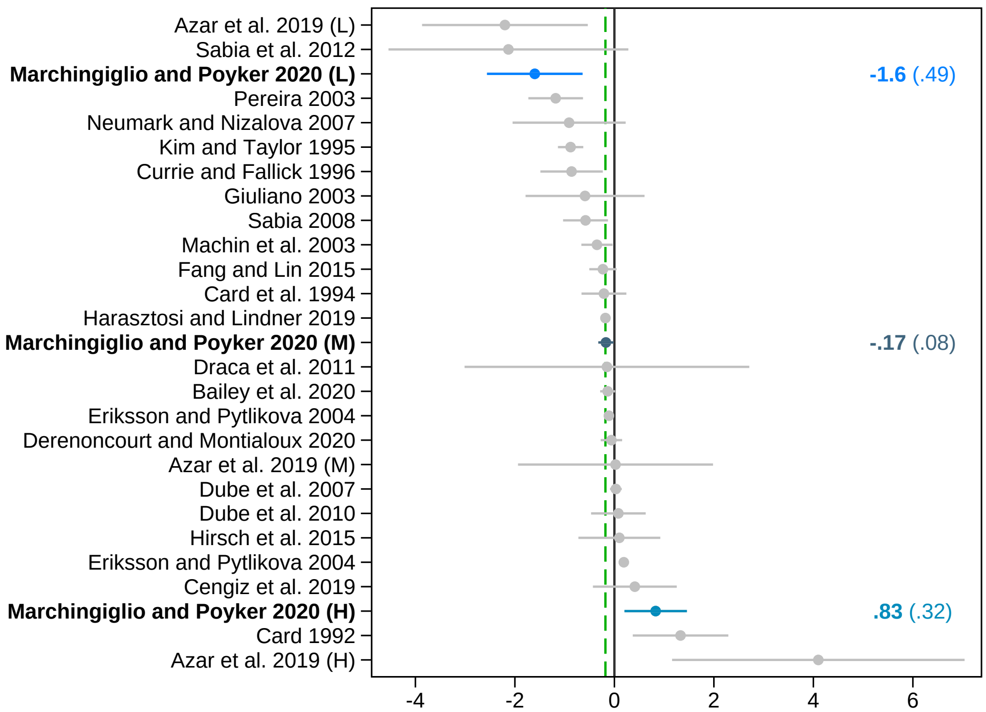

Figure 4: Own-Wage Employment Elasticity with Respect to Wage Compared to the Previous Literature

Notes: Estimates from the previous literature are primarily from the collection by Harasztosi and Lindner (2019), with the addition of results from Azar et al. (2019), Cengiz et al. (2019), Bailey, DiNardo and Stuart (2020), and Derenoncourt and Montialoux (2020). We compute three elasticity levels, corresponding to a cross-industry concentration index equal to 0, 0.6 (the median value), and 1—Low (L), Medium (M), High (H). As a reference, the vertical dashed line corresponds to the average implied elasticity, equal to -0.18. See the text for more details on the computation of the elasticities. Standard errors are computed assuming no uncertainty on the estimates of the elasticity of earnings with respect to minimum wages.

5 Effect on Female Labor-Market Outcomes: Evidence from Individual-Level Data and Linked Census Waves

To complete our understanding of the response of female workers to minimum-wage legislation, we now turn to an individual-level, longitudinal analysis. In this Section, we use the full sample of counties, and identify the effect from within-individual variation. This analysis allows us to estimate to what extent affected women chose to drop out of the labor force or switched industry of employment because of minimum-wage legislation. This specification also allows to directly test whether individuals migrated out of affected markets, thereby corroborating the previously shown evidence in the CBCP analysis.

5.1 Linked-Sample Construction and Empirical Specification

We construct a new sample of linked records of women between 1910 and 1920. We start with a 10% random sample in 1910 that we link to the entire population of women in 1920. Using age, racial/ethnic group, place of birth, and string similarity of names, we manage to link around 30% of the starting records.[59] For this analysis, we further restrict the sample to women aged 16 to 65 in 1920 who were working in 1910. To avoid incurring in matching bias due to a change in last name after marriage, we only keep women who are either never married or always married.[60] We estimate the following equation:

$$y_{i,1920} = \beta \cdot \mathbb{1}\text{Minimum wage}_{js(1910)} + \delta_{c(1910)} + \gamma_{j(1910)} + \eta X_i + \varepsilon_{ict}, \tag{3}$$

where $y_{i,1920}$ is the dependent variable (labor-force participation, employment in the same industry) measuring the change in individual employment status in 1920, after holding the labor-market outcome in 1910 fixed. In Equation (3), we set up a model of the impact of minimum-wage legislation, as measured by β, on outcomes for the individual i in 1920, conditional on county of residence c in 1910, industry of employment j in 1910, and individual controls. The variable $\mathbb{1}(\text{Minimum wage})_{js(1910)}$ is equal to 1 if the industry and state where person i was working in 1910 is newly covered by a minimum-wage law between 1910 and 1920.

[59] Appendix C contains details on the construction of the linked sample.

[60] With these conservative restrictions, we achieve a match rate of around 10%. This is smaller than previous match rates (20-30%) in the literature linking males (e.g., Abramitzky, Boustan and Eriksson, 2012; Long and Ferrie, 2013). To provide more context, in 1910, by age 30, 90% of women in the full count Census are married. For this reason, we are at risk of losing mostly young unmarried women between 16 and 30, who account for 12% of the total female population in the 1910 Census. Papers in this literature do not drop matched observations based on consistent marital status across Census waves, because men do not change last names after marriage. We are not aware of other papers constructing and analyzing a linked sample of married or never-married women. The closest to our paper is Feigenbaum and Gross (2020) who construct a linked sample of *all* women employed as telephone operators. Because they have to match as many women as possible from one industry they innovatively combine full-count census and FamilySearch data. As a result they are able to get a linkage of all women in the telephone operator industry at the trade-off of smaller matching rate due to the additional step of connecting FamilySearch data with the census. Similarly, Withrow (2021) constructed linked census of women using FamilySearch to study rural-to-urban migration in 1920–1940s.

5.2 Estimates from the Linked Sample

Table 4 shows the results of the estimation of (3).[61] In column I, we show that minimum-wage legislation is associated with a drop in the probability of working in the same industry in 1920 by more than 4 percentage points. Column II shows that at least part of this is due to a decline in labor-force participation by 3.2 percentage points. Column III shows that, after conditioning on labor-force participation, the likelihood of working in the same industry drops by almost 6 percentage points. Since the share of women in the sample who are in the labor force in 1920 is about 40%, the contribution of the latter channel to the overall reduction in the probability of working in the same industry is around $\frac{0.4\times -0.058}{-0.043} = 54\%$, while the remaining 46% can be explained by reduced overall labor-force participation.

Table 4: The Effect of Minimum-Wage Legislation on Individual Women—Evidence from a Linked Sample

	I	II	III
	Dependent variable:		
	1(Same industry)	1(LFP)	1(Same industry)
Sample:	All	All	In the LF
Panel A:			
1(Minimum wage)	-0.043**	-0.032*	-0.058**
	(0.020)	(0.019)	(0.025)
R-squared	0.178	0.285	0.318
Observations	55,190	55,190	22,064
Panel B:			
1(Minimum wage) x Married	-0.029*	-0.045**	-0.045
	(0.016)	(0.022)	(0.054)
1(Minimum wage) x Never married	-0.037*	-0.005	-0.059**
	(0.019)	(0.014)	(0.025)
R-squared	0.215	0.412	0.318
Observations	55,190	55,190	22,064
FEs: County in 1910	✓	✓	✓
FEs: Industry in 1910	✓	✓	✓
Individual controls	✓	✓	✓

Notes: This table presents results of the estimation of (3). Each observation is an individual. Each regression includes county, state, birthplace, individual's industry in 1910, and age-bin fixed effects. The following variables are used as controls: dummies for literacy in 1910 and race. Here we use the sample of always-married and never-married women only who were between 16 and 65 years old in 1920. See details on linked sample construction in Appendix C. Results for Minimum wage in dollars$_{ist}$ and log(Minimum wage) are reported in Table E.11. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

[61] Appendix Table E.11 reports the coefficients from an analogous estimation using different right-hand-side variables.

In Panel B, we estimate again columns I–III with an interaction that allows us to distinguish the impact of minimum-wage legislation for married and never-married women. Column I shows that the overall effect is equally driven by both groups, but columns II and III show that the drop in labor-force participation is exclusively driven by married women.

The results in this section show that the adjustment due to the decline in labor demand led to both a reshuffling across industries and a drop in labor-force participation; the distinction between these two channels is a function of marital status. Now that we've painted a comprehensive picture of the impact of gender-specific minimum-wage on women, we will now go back to the industry-specific setting and the substitution between genders.

6 Effects of Sex-Specific Minimum Wages on the Employment of Men, and the Gender Elasticity of Substitution

So far, we have focused on the impact of minimum-wage legislation on the treated gender. In this section, we study the impact of a female-labor price floor on male labor demand, and, in a more general framework, how substitution between genders played a role in determining the impact of this legislation on labor markets.

6.1 Sex-Specific Minimum Wages Increased Employment of Men

Table 5: The Effect of Minimum-Wage Legislation on Employment of Men

	I	II	III	IV	V	VI
	Dependent variable: Log employment share					
Sample	Adult men		Minor men		Minor women	
Panel A:						
Minimum wage, \$10	0.015***	0.011***	0.034**	0.029***	-0.061**	-0.054**
(mean min. wage \$10.2)	(0.001)	(0.0011)	(0.015)	(0.0039)	(0.025)	(0.0233)
R-squared	0.696	0.751	0.833	0.878	0.824	0.880
Observations	802,535	802,535	129,359	129,359	63,785	63,785
Panel B:						
1(Minimum wage)	0.018***	0.012***	0.035***	0.025***	-0.060*	-0.038
	(0.001)	(0.0010)	(0.001)	(0.0012)	(0.032)	(0.0302)
R-squared	0.696	0.751	0.833	0.878	0.824	0.880
Observations	802,535	802,535	129,359	129,359	63,785	63,785
Panel C:						
log (Minimum wage)	0.005***	0.003***	0.013***	0.010***	-0.021**	-0.015
inverse hyperbolic sin	(0.000)	(0.0004)	(0.003)	(0.0007)	(0.009)	(0.0092)
R-squared	0.696	0.751	0.833	0.878	0.824	0.880
Observations	802,535	802,535	129,359	129,359	63,785	63,785
County-pair-year FEs	✓	✓	✓	✓	✓	✓
Ind.-county-pair & occup.-county-pair FEs.		✓		✓		✓

Notes: This table reports the results from estimating equation (1) for men (adults and minors) and minor women for the two most conservative specifications (equivalent of Columns V and VI in Table 2). Each observation is a gender-specific county-decade. Standard errors, triple-clustered at the state (36), industry-occupation (4,714), and border-segment (42) levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

We estimate equation (1) for men. We show that the onset of minimum-wage legislation for women was responsible for an increase in male labor demand by as much as 1.2% (columns I and II of Table 5).[62] This result corresponds to an effect at the locality-industry level, and, taken together with the results presented in Table 2 suggests that on average there was substitution between genders. Columns IV trough VI in Table 3 present estimates at the aggregate local level; they show a positive net aggregate impact on male employment only in areas where a large number of industries are treated.[63]

To investigate this mechanism further, we next create a labor-demand setting in a simple model in which male and female labor is demanded by a profit-maximizing firm.

6.2 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.

$$Y_i = AK_i^{\alpha_i} L_i^{1-\alpha_i}, \tag{4}$$

$$L_i = \left[(\theta_{w_i} W_i)^{\frac{\sigma_i - 1}{\sigma_i}} + (\theta_{m_i} M_i)^{\frac{\sigma_i - 1}{\sigma_i}} \right]^{\frac{\sigma_i}{\sigma_i - 1}}. \tag{5}$$

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:

$$\log\left(\frac{W_i}{M_i}\right) = (1 - \sigma_i) \log\left(\frac{\theta_{m_i}}{\theta_{w_i}}\right) - \sigma_i \log\left(\frac{\omega_{w_i}}{\omega_{m_i}}\right). \tag{6}$$

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 locality-industry 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

[62] We also present analogous results for female and male minors, showing that the results by gender mirror those found for adults (columns III–VI). This makes sense, because in all minimum-wage states except California and Minnesota the legislation covered female minors only. These results are related to the literature on subminimum wage (Neumark and Wascher, 1992) and contemporaneous discussions regarding age-specific minimum wages (Taylor, 2020). Our results are also in-line with the recent findings of the effect of age-specific minimum wage on the employment of youth in Denmark (Kreiner, Reck and Skov, 2020).

[63] To provide additional insights about the economic magnitude of the impact of minimum-wage laws on male and female 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, the 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 of employment in absolute terms, our estimates show that, absent a minimum wage, female employment would have increased by $(-0.5\% + 3.1\%)$=2.6%. At the same time, male employment would have decreased even more, by $(4.35\% + 1.2\%)$=5.55%

and women. However, the main difficulty with estimating σ in equation (6) is that we do not observe wages directly. Our approach 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 6 using our most conservative regression specification from column VI of Table 2 and $\log\left(\frac{W}{M}\right)$ as the dependent variable.

$$\log\left(\frac{\#EmployedWomen_{ic(s)t}}{\#EmployedMen_{ic(s)t}}\right) = \beta \cdot \mathbb{1}\text{Minimum wage}_{ist} + \mu_{st} + \Psi_{p(c)t} + \Phi_{is} + \Phi_{it} + \epsilon_{ip(c)t}. \qquad (7)$$

The resulting estimate yields $\hat{\beta} = -0.047$ (see Table 6, column I). This result is consistent with our findings in Tables 2 and 5 suggesting the presence of substitution of female laborers with male laborers in treated industries.

Table 6: 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***	-0.075**	-0.037**
	(0.017)	(0.031)	(0.014)
Δ		-0.038**	
s.e.		(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 2 (Column VI) except that 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 (1). The number of observations is smaller than the number of women or men separately in the CBCP Sample in Table 2 because not all observations (defined on the county, industry-occupation, and decade levels) 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, triple-clustered at the state, industry-occupation, and border-segment levels, are in parentheses. *** 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 Appendix Table E.14 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, 1925*b*), 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 $\hat{\sigma} \in \left[\frac{-0.047}{0.068-0.023}, \frac{-0.047}{0.068-0.061}\right] = [1.05, 6.71]$. 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 $\hat{\sigma} \in \left[\frac{-0.047}{0.068-0.040}, \frac{-0.047}{0.068-0.060}\right] = [1.68, 5.87]$. 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. These findings are remarkably similar to those in Acemoğlu, Autor and Lyle (2004) where they estimated elasticity of substitution between genders using surge in female employment in United States during the World War II to be $\hat{\sigma}_{AAL2004} \in [1.64, 5.08]$.[64]

We further explore the extent to which 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 6, we estimate $\hat{\beta}$ for two sets of industries. In particular, we reestimate 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 a low ($< 25\%$) 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 male and female employees is larger in industries where neither sex predominates. Indeed, in male-dominated industries, women are more likely to be complements to men, while in female-dominated industries, women are either much more valuable than men, or complements, or both. The highest degree of substitutability is observed in nonsegregated industries.

6.3 Occupation and Substitution

In the previous sections, we have documented a within-industry substitution between men and women as a response to a shock to the price of female labor. We investigate this channel further by differentiating across pretreatment occupational ranking. Our aim here is to see whether, by relaxing the assumption of homogeneous labor that we imposed in the previous section, we can find that the types of jobs lost by women because of minimum-wage legislation are comparable to those acquired by men for the same reason.

To do this, we estimate equation (1) with the exception that we add an interaction with a measure of occupational ranking. In Panel A of Appendix Table E.13, we interact the treatment with a binary variable equal to 1 for occupation-industry combinations with an occupational score less than or equal to 25, (the median, conditional on positive), while in Panel B, we interact the treatment with an index of occupational score quartiles (1 for the bottom, 4, for the top). The results, presented for both men and women, show that while women lost jobs predominantly in the lowest part of the occupational score distribution, men tended to benefit from higher demand in higher-score occupations. These results suggest that firms might have reorganized their production by switching to higher-skilled male labor.[65] Our results also support

[64] See Table 10 of Acemoğlu, Autor and Lyle (2004).

[65] This might be due to mechanization or a different organization of the production function that increased the ratio of

contemporaneous sources that noted that the most-affected women were predominantly the lowest-skilled or the most inexperienced (Stecker, 1927, p. 140).[66] At the same time, employment of skilled women was not affected to the same degree (Stecker, 1927, pp. 174–184).[67]

In the case of a universal minimum wage that applies unconditionally to the entire workforce, one additional margin of adjustment could be the investment in training provided by firms to existing workers in an effort to increase their marginal product (e.g., De Fraja, 1999). While we do not measure this margin, we believe that under conditional minimum wage, this adjustment is less likely to be adopted by employers, who, as we have shown, are able to substitute away from minimum-wage-covered workers.

7 Discussion and Conclusion

This paper shows three main results. First, pre-FLSA minimum-wage legislation was effective in increasing wages for treated women with lower pre-minimum-wage earnings. Second, minimum-wage legislation led to a decline in female employment in treated industry-county cells and a (smaller in magnitude) increase in male employment. In particular, the gender-specificity of these laws allows us to show that men and women were, on average, gross substitutes in the American labor market at the beginning of the 20th century. Third, at the county level, net female employment decreased, but the aggregate impact depends on the degree of prelegislation cross-industry concentration in employment, resulting in implied elasticities of employment with respect to wage that range from −1.6 (in low concentration areas) to 0.8 (in high concentration areas).

We provide suggestive evidence that 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—in Appendix D, 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. At the same time, we find no evidence of any long-run correlation between the legacy of minimum-wage legislation and the labor-force participation of men. Hence, 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 the minimum wage on employment elasticities.

This paper shows that minimum wages had a negative impact on employment levels, but minimum-wage legislation had a *positive* impact on earnings, and may have had nonmonetary positive effects as well.[68]

While we have produced our evidence using historical data, our estimated parameters can be used in

higher-skilled over lower-skilled occupations.

[66] See also Massachusetts Minimum Wage Commission (1916); Peterson (1959); Peterson and Stewart (1969).

[67] Because Sundstrom (1988) presents evidence that in the 19th century U.S. firms 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 sex-specific minimum-wage laws on female labor force participation in Appendix D.

[68] For example, Dow et al. (2019) show that an increase in minimum wages has reduced suicide among low-wage workers.

a comparative perspective to feed contemporary policy debates.[69] The 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 an increase will have no negative effect on employment. Even in Alabama and Mississippi, two of the lowest-wage states, the relative minimum wage—the ratio of minimum wage to the state's median wage—would rise to 0.77 and 0.85, respectively. In this paper, we analyze an unprecedented increase in minimum wages (the ratio of minimum wage to median wage in the Oregon data is around 90%) that may be comparable in size.

[69] This paper opens a lane for a follow-up work, as one could 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, 1925*a*; Russell, 1991). Also, 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 introducing minimum wages in a more institutionally simple environment. Finally, our institutional context, coupled with the analysis of linked Census data, affords a look at the effect of minimum wages on family choices (e.g., marriage, number of children) and educational choices, as well as to study differential employment effects on minority women.

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Online Appendix

to

The Economics of Gender-Specific Minimum-Wage Legislation

A Labor Protection Laws and the Lochner Era

Early U.S. minimum-wage legislation came during a period known as the *Lochner* era, in which American jurisprudence was characterized by a peculiar aversion to any legislation that could be seen as infringing on economic liberty.

In 1895, the State of New York passed the Bakeshop Act, which stated: "No employee shall be required, permitted or suffered to work in a biscuit, bread or cake bakery or confectionery establishment more than sixty hours in one week." Joseph Lochner—a bakery owner who was indicted for violating the act—appealed to the Supreme Court, which in 1905 ruled 5-to-4 (in *Lochner v. New York*) that limiting working hours was unconstitutional. The argument supporting this view was based on the due process clause[70], present in the Fifth and the Fourteenth Amendments to the U.S. Constitution.[71] The clause says that no one shall be "deprived of life, liberty or property without due process of law." The interpretation at the time (especially regarding the word "liberty") was that the government could not interfere with the freedom to contract and negotiate an employment relationship. The previously mentioned *Adkins v. Children Hospital* is considered in legal scholarship as one of many products of the legacy of *Lochner v. New York*.[72]

It is quite possible that, moved by a paternalistic view, coupled with evidence that women were earning too-low wages, state legislators implementing minimum-wage laws somehow thought that the due process clause would not have been appealed to in the case of female employees. Indeed, Novkov (2001) shows that, between 1873 and 1937, while U.S. courts were almost 50% more likely to strike down than uphold "general" protective labor legislation, the odds of upholding protective labor legislation limited to women were about 5 to 1 (see Figures A.1 and A.2).[73]

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 evidence on the first minimum-wage experiments—on both the state the federal levels—was discussed.[74] On 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."[75] On the other hand, Peterson (1959) went against what he perceived as common wisdom among policy analysts and labor economists at the time, saying that minimum wages had essentially no employment effects, and, using three pre-FLSA case studies of minimum-wage laws for women, he claimed that minimum-wage laws did decrease women's employment. Overall, however, pre-FLSA minimum-wage laws have never been empirically analyzed beyond the case-study approach adopted by Lester (1941) and Peterson (1959), partly because relevant data is extremely scarce.

[70] Based on the primary holding annotation to the Supreme Court decision accessed here. Last accessed July 2020.

[71] It is commonly understood (e.g., Leek, 1945) that the due process clause in the Fifth Amendment and the one reported in the Fourteenth Amendment are intended to have the same meaning, the former applying to the federal government, the latter applying explicitly to the states.

[72] See Powell (1924) for a comprehensive legal analysis on the judiciality of minimum-wage legislation during the Lochner era.

[73] Novkov (2001) (p. 29) specifies that "general legislation was written in gender-neutral terms but largely applied to occupations that were dominated by male laborers."

[74] What started as primarily a theoretical debate about which assumptions were most suitable for predicting and understanding 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.

[75] Lester (1941), p. 334. Accessed here. Last access July 2020.

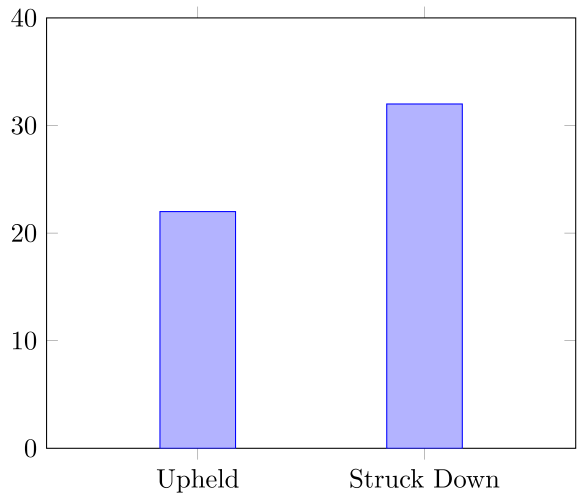

Figure A.1: Decisions in state cases involving general protective labor legislation (excluding cases involving children), 1873–1937. (Novkov, 2001, Table 4, p.31)

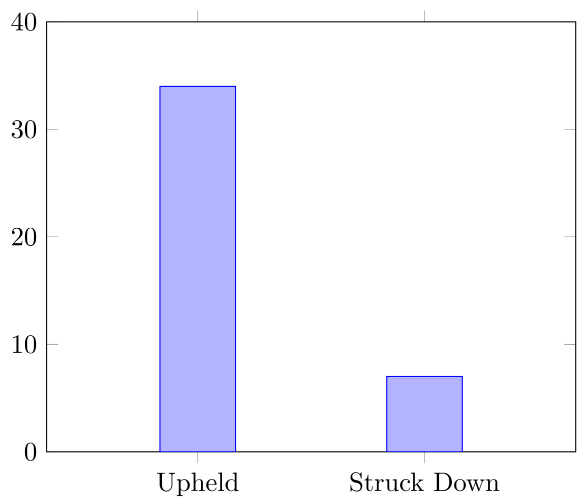

Figure A.2: Decisions in state cases involving protective labor legislation limited to women (excluding cases involving children), 1873–1937. (Novkov, 2001, Table 4, p.31)

B Additional Results

B.1 Employment Results: Full Sample

In this section, we estimate the effect of a minimum wage on the full sample of all U.S. counties. The specification is as follows:

$$\ln\left(EmpShare_{gic(s)t}\right) = \beta \cdot \mathbb{1}\text{Minimum wage}_{ist} + \mu_{st} + \Psi_{ct} + \Phi_{is} + \Phi_{it} + \epsilon_{gic(s)t}, \quad g = \{m, w\}, \tag{8}$$

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.

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 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 addresses 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.

We double-cluster standard errors by state and industry/occupation (Cameron, Gelbach and Miller, 2011).

Table B.1 contains results for female employment.

Table B.1: Effect of Minimum-Wage Legislation on Employment of Women (Full Sample)

	I	II	III	IV	V	VI
	Dependent variable: Log employment share (women)					
Panel A:						
Minimum wage, \$10	-0.013	-0.003	-0.001	-0.013	-0.003	0.004
(mean min. wage \$10.2)	(0.021)	(0.011)	(0.010)	(0.021)	(0.011)	(0.0081)
R-squared	0.673	0.689	0.750	0.681	0.697	0.756
Observations	1,363,979	1,363,979	1,363,979	1,363,979	1,363,979	1,363,979
Panel B:						
1(Minimum wage)	-0.035	-0.024	-0.021	-0.035	-0.022	-0.014
	(0.022)	(0.016)	(0.018)	(0.023)	(0.015)	(0.0138)
R-squared	0.673	0.689	0.750	0.681	0.697	0.756
Observations	1,363,979	1,363,979	1,363,979	1,363,979	1,363,979	1,363,979
Panel C:						
log (Minimum wage)	-0.009	-0.005	-0.004	-0.009	-0.004	-0.002
inverse hyperbolic sin	(0.008)	(0.005)	(0.005)	(0.009)	(0.005)	(0.0041)
R-squared	0.673	0.689	0.750	0.681	0.697	0.756
Observations	1,363,979	1,363,979	1,363,979	1,363,979	1,363,979	1,363,979
County FEs	✓	✓	✓			
County-year FEs				✓	✓	✓
Industry-state & occupation-state FEs	✓	✓	✓	✓	✓	✓
State-year FEs	✓	✓	✓	✓	✓	✓
Industry-year & occup.-year FEs		✓	✓		✓	✓
Ind.-county & occup.-county FEs			✓			✓

Notes: This table reports the results from estimating equation (8). Each panel contains coefficients from separate regressions with Minimum wage in dollars$_{ist}$, $\mathbb{1}$ (Minimum wage), and log(Minimum wage) as explanatory variables. Standard errors, double-clustered at the state and industry-occupation levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

C Construction of the Linked Sample

We construct a linked sample of women using individual data from Census waves in 1910 and 1920. We start by selecting women aged 15 or older who are not observed in group quarters. We then select a 10% random sample of women from 1910, to be potentially linked to the entire set of women observed in 1920. We join the two samples of women in 1910 and 1920 using the phonetic measure Soundex applied to both first and last name. We then drop most common names (i.e., those that have more than 1,000 matches), matches with an age inconsistency of more than 3 years, and matches with different birth place. We then select matches with high last-name-string similarity (i.e., a Jaro-Winkler score of 0.9 or more). Finally, for each 1910 individual observation that still shows more than one match, we select the one with minimum age error, then we drop all the individuals who, even after this last restriction, have more than one linked observation, and keep matches with the same race code. For this analysis, we further restrict the sample to women aged 16 to 65 in 1920 who were working in 1910. To avoid incurring in matching bias due to a change in last name after marriage, we only keep women who are either never married or always married.

This strategy makes use of similar parameters used by the previous literature. A recent paper by Abramitzky et al. (2019) shows that coefficient estimates (in analyses of intergenerational mobility) are similar when using linked samples based on various automated methods.

D Long-Run Effect of Minimum-Wage Legislation

Here, we ask whether the fact that gender-specific minimum-wage laws, which existed for up to 26 years before the 1938 FLSA was passed, 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:

$$LFP_{c(s),1940} = \alpha + \beta \cdot MinWageLegacy_s + LFP_{c(s),1910} + \Delta LFP_{c(s),1900-10} + \epsilon_{cs}, \tag{9}$$

where $LFP_{c(s),1940}$ is 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 a state had gender-specific minimum-wage laws for at least ten years.[76] Second, we use log of number of years that minimum-wage laws were active. 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, pretreatment labor-force participation $LFP_{c(s),1910}$, and pretreatment trend in the dependent variable $\Delta LFP_{c(s),1900-10}$.

Panel A of Table D.1 reports the result for the more-than-10-years-minimum-wage-history dummy. Having minimum-wage laws for at least ten 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 pretreatment dependent variable (columns III–IV) or pretreatment trend in the dependent variable in columns V and VI.

Table D.1: Long-Run Effect of Minimum-Wage Laws

	I	II	III	IV	V	VI
Panel A	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**	-0.001	-0.020*	-0.001	-0.020*	-0.001
	(0.010)	(0.003)	(0.010)	(0.003)	(0.010)	(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
	I	II	III	IV	V	VI
Panel B	Dependent variable: Labor-force participation in 1940					
	Women	Men	Women	Men	Women	Men
Log # years under min. wage. laws	-0.006*	-0.000	-0.005*	-0.000	-0.006*	-0.000
	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)	(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: Each observation is a county. The explanatory variable in Panel A is an indicator variable equal to unity if a state had minimum-wage laws for at least ten 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 the inverse hyperbolic sine of the number of years that a state had minimum-wage laws. 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 pretreatment changes in 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 parentheses. *** p<0.01, ** p<0.05, * p<0.1

[76] We exclude Arizona and the District of Columbia, because they only had minimum-wage laws for five and eight years, respectively.

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 further substantiate the hypothesis, we explore individual-level Census data. In columns I and II of Appendix Table D.2, we reestimate equation (9) 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-wage 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 twelve treated states we have only 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 in 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.

Table D.2: Long-Run Effect of Minimum-Wage Laws: Placebo with Migrants from Non-Minimum-Wage States

	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***		-0.010	
	(0.007)		(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 the baseline specification (9) from Table D.1 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 include only those people who five years before 1940 chose a state of residence that did not have minimum-wage laws. Here, we control for marital status and Census region. Standard errors, clustered at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

E Tables

Table E.1: List of 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
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 (including printing)
California	1922	1922	Needle trades
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
Kansas	1918	1918	Mercantile
Kansas	1918	1918	Laundry and dry cleaning
Kansas	1918	1918	Telephone operators
Kansas	1919	1919	Manufacturing
Massachusetts	1914	1914	Brush
Massachusetts	1915	1915	Laundry and dry cleaning
Massachusetts	1915	1916	Retail
Massachusetts	1916	1917	Women's clothing
Massachusetts	1919	1919	Office and other building cleaners
Massachusetts	1919	1920	Candy making
Massachusetts	1919	1919	Fruit and vegetable canning
Massachusetts	1920	1920	Paper boxes
Massachusetts	1923	1924	Druggists
Massachusetts	1925	1925	Bread and other bakery products
Massachusetts	1927	1927	Toys, games, and sporting goods
Minnesota	1914	1914	Mercantile
Minnesota	1914	1914	Manufacturing
Minnesota	1918	1918	ALL
North Dakota	1920	1920	Public housekeeping
North Dakota	1920	1920	Personal service
North Dakota	1920	1920	Office occupations
North Dakota	1920	1920	Manufacturing
North Dakota	1920	1920	Laundry and dry cleaning
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, 1937*a*, 1939).

Table E.1 (cont.): List of 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
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, 1937*a*, 1939).

Table E.2: Timing of Imposing and Abolishing Minimum-Wage Legislation

#	State	Year when first law is imposed	Year when abolished	# of years active before FLSA	Comments
1	Arizona	1917	1925	8	Overturned by Supreme Court in Murphy v. Sardell.
2	Arkansas	1915	1927	12	Overturned by Supreme Court in Donham v. West Nelson Manuf. Go.
3	California	1913	1925	12	Withdrawn by state in Gainer v. A.B.C. Dorhram.
4	District of Columbia	1918	1923	5	Overturned by Supreme Court on a 5-3 vote in Adkins v. Children's Hospital.
5	Kansas	1915	1925	10	Overturned by Kansas Supreme Court in Topeka Laundry Co. v. Court of Industrial Relations.
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	Overturned by federal district court following Adkins.

Notes: *Sources:* Levitan (1979) and Thies (1990).

Table E.3: Border-County Balance Table

	I		II		III		IV	
	All-County Sample		Contiguous Border County-Pair Sample		Differences (Between Full and CBCP Sample)		Differences (Between Counties in Pair)	
	Mean	s.d.	Mean	s.d.	Mean	P-value	Mean	P-value
County Controls (1920):								
Population	118,437	(300,947)	139,626	(393,022)	21,189	[0.613]	1,792	[0.679]
# prime age adults	70,930	(187,088)	84,282	(243,564)	13,352	[0.606]	598	[0.822]
Ratio of employed women to employed men	1.052	(7.631)	1.088	(8.138)	0.036	[0.375]	-0.004	[0.463]
Share Black	0.018	(0.009)	0.019	(0.010)	0.001	[0.160]	-0.001	[0.339]
Share literate	0.733	(0.076)	0.744	(0.066)	0.011	[0.175]	-0.001	[0.741]
Share rural	0.604	(0.317)	0.589	(0.328)	-0.015	[0.645]	0.009	[0.389]
Share women	0.006	(0.003)	0.006	(0.003)	0.000	[0.304]	-0.000	[0.232]
Labor-force participation	27.4	(447.5)	29.0	(554.8)	1.55	[0.688]	0.243	[0.170]
# 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. In column-sets I–III, an observation is a county(-pair) industry-occupation in 1920. In column-set IV, an observation is a county in 1920.

Table E.4: Effect of Minimum-Wage Legislation on Employment: Introduction vs. Abolishment of the Minimum Wages

	I	II	III
	Dependent variable: Log employment share (women)		
Introduction: Minimum wage, \$10 (mean min. wage \$10.2)	-0.012*** (0.0026)		
Abolishment: Minimum wage, \$10 (mean min. wage \$10.2)	0.019** (0.0083)		
Introduction: 1(Minimum wage)		-0.029*** (0.0032)	
Abolishment: 1(Minimum wage)		0.036*** (0.0129)	
Introduction: log (Minimum wage) inverse hyperbolic sin			-0.007*** (0.0004)
Abolishment: log (Minimum wage) inverse hyperbolic sin			0.010*** (0.0019)
R-squared	0.797	0.797	0.797
Observations	273,883	273,883	273,883

Notes: This table estimates the same specifications (the most conservative, in column VI) as the baseline Table 2 but with negative changes in minimum wages defined as a separate variable. Here, abolishment of minimum wage always happens only in $t = 1930$. Introduction of minimum wages includes initial introduction of the minimum wages both, in $t = 1920$ and in $t = 1930$ for some industries. Standard errors, triple-clustered at the state, industry-occupation, and border segment levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.5: Effect of Minimum-Wage Legislation on Employment of Women (No Missing Occupations)

~Baseline, no missing non. occupational	I	II	III	IV	V	VI
	Dependent variable: Log employment share					
Minimum wage, \$10	-0.043***	-0.043***				
(mean min. wage \$10.2)	(0.015)	(0.0146)				
1(Minimum wage)			-0.051***	-0.050***		
			(0.013)	(0.0138)		
log (Minimum wage)					-0.016***	-0.016***
inverse hyperbolic sin					(0.005)	-0.0046
R-squared	0.751	0.795	0.751	0.795	0.751	0.795
Observations	322,740	322,740	322,740	322,740	322,740	322,740
County-pair-year FEs	✓	✓	✓	✓	✓	✓
Ind.-county-pair & occup.-county-pair FEs.		✓		✓		✓

Notes: This table estimates the same specifications (the two most conservative, in columns V and VI) as the baseline Table 2 but without dropping nonoccupational industries. Standard errors, triple-clustered at the state, industry-occupation, and border segment levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.6: Effect of Minimum-Wage Legislation on Employment of Women

	I	II	III	IV	V	VI	VII	VIII
	Dependent variable: Log employment share (women)							
non-occupational industries	without		with		w/o	w	w/o	w
Minimum wage, \$10	-0.021**	-0.013**	-0.041*	-0.045*				
(mean min wage \$10.2)	(0.0097)	(0.0057)	(0.0213)	(0.0250)				
1(Minimum wage)					-0.027**	-0.051*		
					(0.013)	(0.026)		
log (Minimum wage)							-0.007***	-0.016*
inverse hyperbolic sin							(0.002)	(0.009)
Ind.-occup.-year FEs.	✓	✓	✓	✓	✓	✓	✓	✓
Ind.-occup.-state FEs.		✓		✓	✓	✓	✓	✓
R-squared	0.902	0.916	0.910	0.923	0.916	0.923	0.916	0.923
Observations	273,883	273,883	322,740	322,740	273,883	322,740	273,883	322,740

Notes: This table estimates the most conservative specification (column VI) from the baseline Table 2 but adds an additional set of fixed effects. All results are dubbed for the specification without dropping nonoccupational industries. Standard errors, triple-clustered at the state, industry-occupation, and border-segment levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.7: 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***			0.022**		
	(0.006)			(0.011)		
Minimum wage, $		-0.003**			0.002***	
(mean min wage $10.2)		(0.001)			(0.000)	
log (Minimum wage)			-0.012***			0.004*
inverse hyperbolic sin			(0.003)			(0.002)
1(Max. working hours law)	0.001	0.003	0.003	0.015*	0.014	0.014
x 1(State ever had minimum wage)	(0.015)	(0.015)	(0.015)	(0.008)	(0.009)	(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 the baseline specification from column VI of Table 2 for women and column II of Table 5 but adding the interaction of the main explanatory variable with a state-industry-year-specific dummy for maximum-working-hours legislation. Standard errors, triple-clustered at the state, industry-occupation, and border-segment levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.8: Placebo Estimates with 1900–1910 Treatment Instead of 1920–1930 Treatment

	I	II	III	IV	V	VI
1900-1910 placebo treatment	Dependent variable: Log employment share					
	Women			Men		
Minimum wage, \$10	0.021			-0.015		
(mean min wage \$10.2)	(0.015)			(0.019)		
1(Minimum wage)		0.012			-0.035	
		(0.029)			(0.022)	
log (Minimum wage)			0.005			-0.009
inverse hyperbolic sin			(0.008)			(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 the baseline specification from column VI of Table 2 for women and column II of Table 5 but uses a lagged outcome variable: 1900 log employment share is treated by 1920 minimum-wage legislation, and 1910 log employment share is treated by 1930 minimum-wage legislation. Standard errors, triple-clustered at the state, industry-occupation, and border-segment levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.9: Effect of Minimum-Wage Legislation on Employment: WWI Draft and the Effect of Returning Veterans (via Marriage Bars)

	I	II	III
	Dependent variable: Log employment share (women)		
Average minimum wage, $	-0.017***	-0.028**	-0.018***
(mean av. min. wage $6)	(0.006)	(0.011)	(0.007)
Log WWI veterans x 1920	0.011	0.008	
inverse hyperbolic sin	(0.010)	(0.010)	
Average minimum wage, $ x		0.001	
Log WWI veterans x 1920		(0.001)	
Log WWI veterans x 1930			0.029*
inverse hyperbolic sin			(0.014)
Average minimum wage, $ x			-0.001
Log WWI veterans x 1930			(0.001)
R-squared	0.797	0.797	0.798
Observations	3,020	3,020	3,020

Notes: This table estimates the same specifications as in column I of Panel A Table 3 but with additional controls for the log of WWI veterans (computed using 1930 Census). In column I we add a log of veterans as a control; the variable is interacted with the 1920 dummy. In column II we add its interaction with the minimum wage. Column III is the same as column III but WWI veterans are are interacted with 1930 dummy. Standard errors, double-clustered at the state and border segment levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.10: Effect of Minimum-Wage Legislation on Employment of Subsample

Panel A~w/o 1880	I	II	III	IV	V	VI
	Dependent variable: Log employment share					
	Women			Men		
Minimum wage, \$10	-0.024*			0.018***		
(mean min wage \$10.2)	(0.013)			(0.005)		
1(Minimum wage)		-0.043***			0.022***	
		(0.010)			(0.001)	
log (Minimum wage)			-0.011***			0.006***
inverse hyperbolic sin			(0.004)			(0.000)
R-squared	0.737	0.737	0.737	0.649	0.649	0.649
Observations	259,164	259,164	259,164	731,331	731,331	731,331
Panel B~w/o 1930	I	II	III	IV	V	VI
	Dependent variable: Log employment share					
	Women			Men		
Minimum wage, \$10	-0.033***			0.032***		
(mean min wage \$10.2)	(0.011)			(0.001)		
1(Minimum wage)		-0.039***			0.032***	
		(0.000)			(0.001)	
log (Minimum wage)			-0.012***			0.011***
inverse hyperbolic sin			(0.000)			(0.000)
R-squared	0.756	0.756	0.756	0.649	0.649	0.649
Observations	168,736	168,736	168,736	531,778	531,778	531,778
Panel C~w/o 1880 and 1930	I	II	III	IV	V	VI
	Dependent variable: Log employment share					
	Women			Men		
Minimum wage, \$10	-0.034**			0.037***		
(mean min wage \$10.2)	(0.013)			(0.001)		
1(Minimum wage)		-0.038***			0.043***	
		(0.001)			(0.001)	
log (Minimum wage)			-0.011***			0.014***
inverse hyperbolic sin			(0.001)			(0.000)
R-squared	0.753	0.753	0.753	0.642	0.642	0.642
Observations	155,513	155,513	155,513	461,189	461,189	461,189

Notes: This table estimates the baseline specification from column VI of Table 2 for women and column II of Table 5 but with a restricted sample. Panel A omits the 1880 census. Panel B omits the 1930 census. Panel C omits both. Standard errors, triple-clustered at the state, industry-occupation, and border-segment levels, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.11: Effect of Minimum-Wage Legislation on Individual Women—Evidence from a Linked Sample

	I	II	III
	Dependent variable:		
	1(Same industry)	1(LFP)	1(Same industry)
Sample:	All	All	In the LF
Panel A:	-0.041**	-0.028	-0.064**
Minimum wage, \$10	(0.019)	(0.018)	(0.026)
(mean min. wage \$10.2)			
R-squared	0.178	0.285	0.318
Observations	55,190	55,190	22,064
Panel B:			
Minimum wage, \$10 x Married	-0.025*	-0.038*	-0.057
	(0.015)	(0.021)	(0.049)
Minimum wage, \$10 x Never married	-0.038**	-0.004	-0.065**
	(0.019)	(0.014)	(0.026)
R-squared	0.215	0.412	0.319
Observations	55,190	55,190	22,064
Panel C:			
log (Minimum wage)	-0.015**	-0.011*	-0.020**
inverse hyperbolic sin	(0.007)	(0.006)	(0.009)
R-squared	0.178	0.285	0.318
Observations	55,190	55,190	22,064
Panel D:			
log (Minimum wage) x Married	-0.010*	-0.015**	-0.017
	(0.005)	(0.007)	(0.018)
log (Minimum wage) x Never married	-0.013**	-0.002	-0.021**
	(0.007)	(0.005)	(0.009)
R-squared	0.215	0.412	0.318
Observations	55,190	55,190	22,064
FEs: County in 1910	✓	✓	✓
FEs: Industry in 1910	✓	✓	✓
Individual controls	✓	✓	✓

Notes: This table presents results of the estimation of (3) for Minimum wage in dollars$_{ist}$ and log(Minimum wage). Each observation is an individual. Each regression includes county, state, birthplace, individual's industry in 1910, and age-bin fixed effects. The following variables are used as controls: dummies for literacy in 1910 and race. Here, we use the sample of always-married and never-married women only who were between the ages of 16 and 65 in 1920. See details on linked sample construction in Appendix C. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.12: The Effect of Minimum-Wage Legislation on Individual Women—Evidence from a Linked Sample

	I	II	III	IV
	Dependent variable:			
	1(Same state)	1(Same county)	1(Same state)	1(Same county)
Sample:	All	All	CBCP	CBCP
Panel A	0.006	0.003	0.000	-0.002
Minimum wage, $10	(0.010)	(0.012)	(0.024)	(0.027)
(mean min wage $10.2)				
R-squared	0.146	0.141	0.177	0.163
Observations	55,190	55,190	12,835	12,835
Panel B				
1(Minimum wage)	0.008	0.006	0.001	-0.004
	(0.010)	(0.012)	(0.024)	(0.028)
R-squared	0.146	0.141	0.177	0.163
Observations	55,190	55,190	12,835	12,835
Panel C				
log (Minimum wage)	0.002	0.002	-0.0001	-0.001
inverse hyperbolic sin	(0.003)	(0.004)	(0.008)	(0.009)
R-squared	0.146	0.141	0.177	0.163
Observations	55,190	55,190	12,835	12,835
FEs: County in 1910	✓	✓	✓	✓
FEs: Industry in 1910	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓

Notes: This table presents results of the estimation of (3). Each observation is an individual. Each panel contains coefficients from separate regressions with Minimum wage in dollars$_{ist}$, $\mathbb{1}$ (Minimum wage), and log(Minimum wage) as explanatory variables. Each regression includes county, state, birthplace, individual's industry in 1910, and age-bin fixed effects. The following variables are used as controls: dummies for literacy in 1910 and race. Here we use the sample of always-married and never-married women only who were between 16 and 65 years old in 1920. See details on linked sample construction in Appendix C. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.13: The Effect of Minimum-Wage Legislation on Employment of Men and Women, across Occupational Income Score

	I	II	III	IV	V	VI
	Dependent variable: Log employment share					
Sample	Women			Men		
Panel A:						
Minimum wage, \$10	-0.045*			-0.052		
x occupational score≤25	(0.0247)			(0.0473)		
Minimum wage, \$10	-0.006			0.020***		
x occupational score>25	(0.0197)			(0.0019)		
1(Minimum wage)		-0.059*			-0.063	
x occupational score≤25		(0.0296)			(0.0561)	
1(Minimum wage)		-0.025			0.015***	
x occupational score>25		(0.0159)			(0.0016)	
log (Minimum wage)			-0.019*			-0.023
x occupational score≤25			(0.0096)			(0.0182)
log (Minimum wage)			-0.005			0.005***
x occupational score>25			(0.0056)			(0.0007)
R-squared	0.803	0.803	0.803	0.722	0.722	0.722
Observations	232,681	232,681	232,681	736,331	736,331	736,331
Panel B:						
Minimum wage, \$10	-0.237*			-0.211		
	(0.1318)			(0.1381)		
Minimum wage, \$10	0.080			0.068		
x occupational score quartile	(0.0487)			(0.0411)		
1(Minimum wage)		-0.223*			-0.241*	
		(0.1239)			(0.1364)	
1(Minimum wage)		0.069			0.075*	
x occupational score quartile		(0.0440)			(0.0409)	
log (Minimum wage)			-0.088**			-0.090*
			(0.0409)			(0.0445)
log (Minimum wage)			0.029*			0.028**
x occupational score quartile			(0.0147)			(0.0133)
R-squared	0.804	0.804	0.804	0.723	0.723	0.723
Observations	232,681	232,681	232,681	736,331	736,331	736,331

Notes: This table estimates the same specifications as the baseline Table 2 (Column VI), with the exception that the main right-hand-side variable is interacted with a measure of prelegislation occupational ranking. Each observation is a gender-specific industry-occupation-county-decade. Standard errors are triple-clustered at the state (36), industry-occupation (4,714), and border-segment (42) levels. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table E.14: 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 stitchingt)	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, 1925*b*).

F Figures

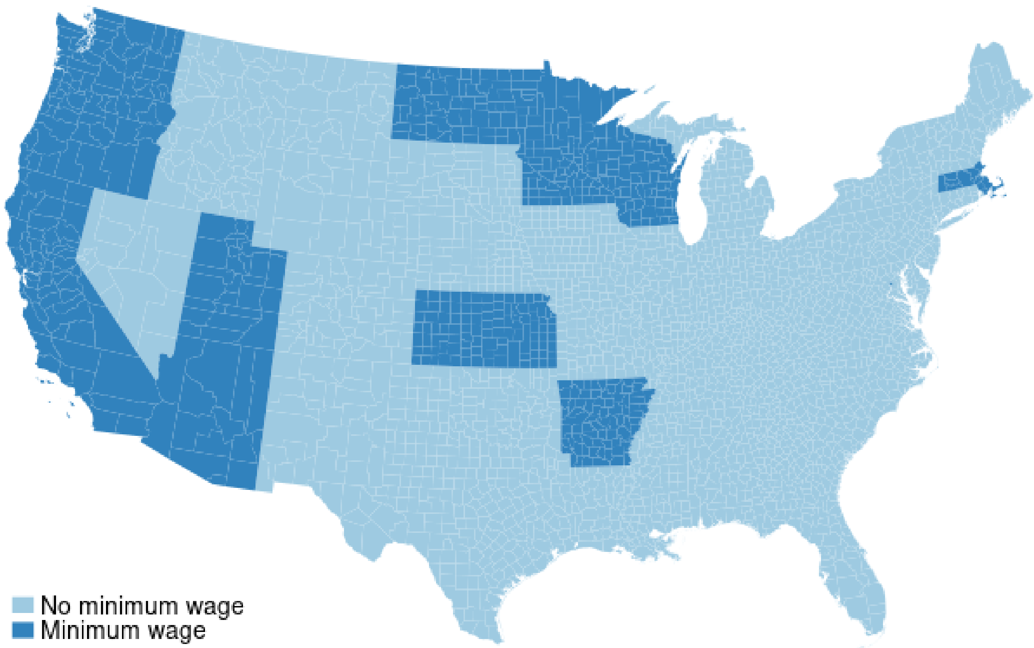

Figure F.1: States with female-specific minimum-wage laws. Dark blue indicates states with minimum-wage laws: Arizona, Arkansas, California, Kansas, Massachusetts, Minnesota, North Dakota, Oregon, Utah, Washington, Wisconsin, and the District of Columbia. Light blue indicates states that don't have minimum-wage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

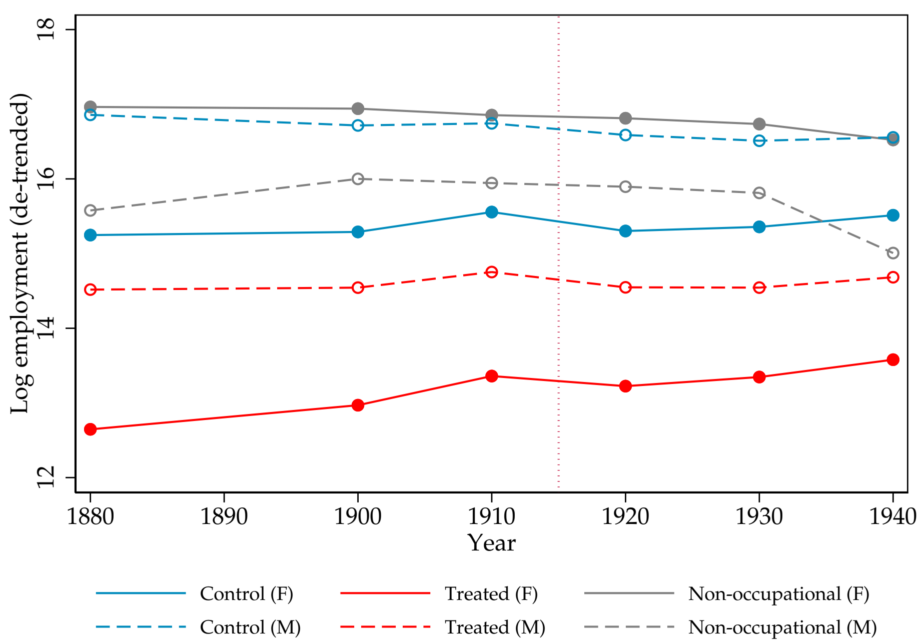

Figure F.2: De-Trended Log Employment.

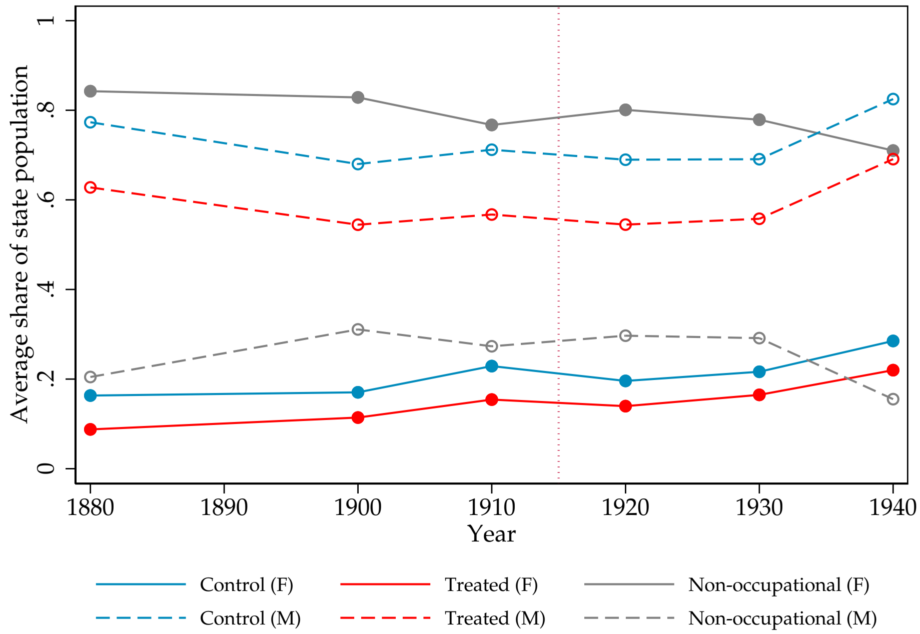

Figure F.3: Average Share of State's Adult Population.

Figure F.4: Fully dynamic difference-in-differences specification, estimated according to the equation (1), except that the treatment is interacted with decade fixed effects. The outcome variable is the log of employment share of women. This figure reports on the point-estimates with 90th-percent confidence bands. Each decade-specific coefficient is plotted in the graph, using 1910 as the baseline omitted decade.

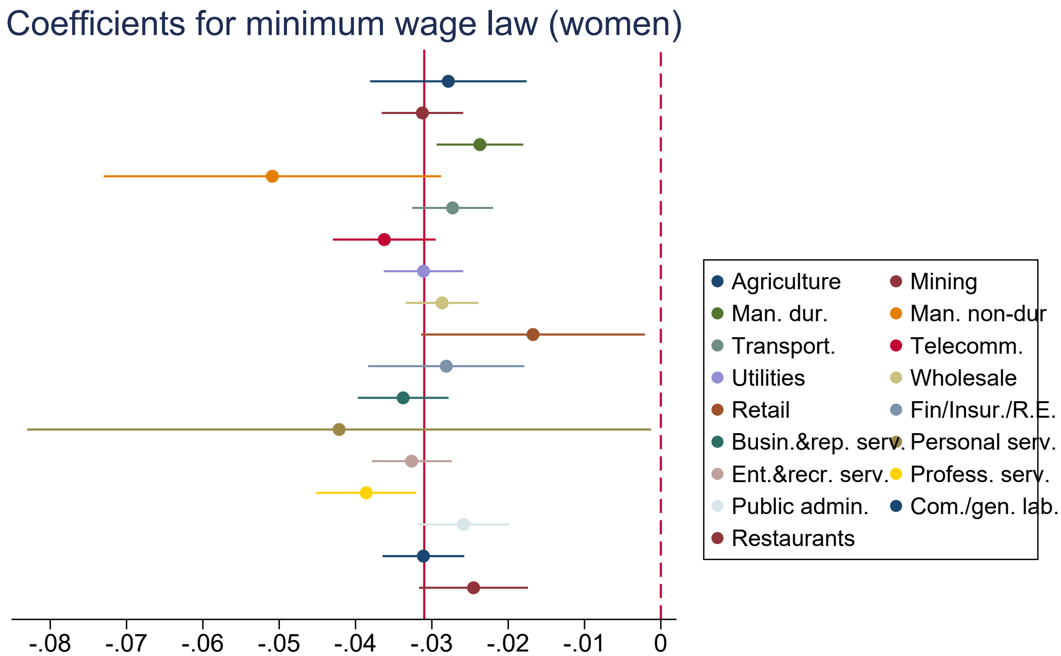

Figure F.5: Industry-Exclusion Robustness of the Results for $\mathbb{1}$ (Minimum wage)$_{ist}$ in Table 2

Notes: This figure reports on the point-estimate and 90th-percentile confidence band that results when re-estimating the core specification in Column VI of Table 2, dropping one state at a time. The (red) vertical line is the baseline point estimate. The list of industries is as follows: agriculture, mining, manufacturing of durable goods, manufacturing of nondurable 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., the agricultural industry is omitted first, then mining, then manufacturing of durable goods, etc.