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Riccardo Marchingiglio
Analysis Group, Inc., Washington

Michael Poyker
UT Austin and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Economics of Gender-Specific Minimum Wage Legislation*

Using full count U.S. census data, we study the impact of early 20th-century state-industry-specific minimum wage laws that primarily targeted female employees. Our triple-difference estimates suggest a null impact of the minimum wage laws, potentially reflecting disemployment effects and the positive selection bias of the workers remaining in the labor force. When comparing county-industry trends between counties straddling state borders, female employment is lower by around 3.1% in affected county-industry cells. We further investigate the implications for own-wage elasticity of labor demand as a function of cross-industry concentration, the channels of substitution between men and women, and heterogeneity by marital status.

JEL Classification: J16, J23, N32

Keywords: minimum wage, labor demand, gender gap, labor markets

Corresponding author:
Michael Poyker
UT Austin
2515 Speedway
Austin, TX 78712
USA

E-mail: mikhail.poyker@austin.utexas.edu

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1 Introduction

Even before the United States enacted its first federal minimum wage law as part of the Fair Labor Standards Act (FLSA) of 1938, the employment effects of minimum wage laws were a topic of debate among economists, politicians, and policymakers (Neumark and Wascher, 2007; Fishback and Seltzer, 2021). This debate continues on today, reflecting the complexity of the issue. The literature indicates that there is no fundamental law that definitively predicts the effects of introducing or adjusting a minimum wage.[1] Instead, the impact of minimum wage laws depends on multiple factors, including the industry under study, other contemporaneous labor market regulations and institutions, and the labor market structure. Additionally, the persistent gender wage gap implies 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 examine the impact of the first U.S. minimum wage laws on women's employment, using full count census data. These initial laws, enacted in 11 states—Arizona, Arkansas, California, Kansas, Massachusetts, Minnesota, North Dakota, Oregon, Utah, Washington, Wisconsin—and the District of Columbia starting in 1912, were distinctively gender specific,[2] setting wage floors exclusively for female laborers and predominantly applying to only a subset of industries.[3] In five of these jurisdictions—California, Kansas, Massachusetts, North Dakota, and D.C.—these laws covered only women in certain industries. The legislation immediately spurred fierce debates, and in 1923, the Supreme Court struck down the minimum wage in 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 FLSA was introduced.[4]

Identifying the employment impact of minimum wage setting has posed several challenges.[5] First, the universal nature of minimum wages makes it difficult to find a suitable control group, forcing researchers to make assumptions about how strongly the price floor affects specific industries (e.g., restaurants in Leamer et al. 2019) and geographic units of interest (e.g., Seattle in Jardim et al. 2022 or cities in Dube and Lindner 2021). Second, if everyone in a certain area has the same minimum wage, substitution into illegal employment may be more likely (Bernhardt et al., 2009), which would induce non-classical measurement error into the estimates. Third, the limited availability of longitudinal data on earnings makes it difficult to disentangle

[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 in the U.S. was Oregon's universal minimum wage for women, in 1913.

[4] Section 2 provides background information on the timeline and coverage of gender-specific minimum wage laws.

[5] Note that, as explained by Manning (2021), in a setting of labor market frictions ("such that—at prevailing wages—not all workers who want a job manage to get one and that not all employers who want to hire a worker manage to find one"), the introduction or the increase of a wage floor generally leads to both an increase in the quantity of labor supplied and a decrease in the quantity of labor demanded, thereby implying that the net impact on employment will depend on which effect prevails. Like several other papers in the literature, our paper sheds light on the net impact without isolating the effect on one side of the market or the other.

the within-worker impact of minimum wage laws on earnings from a composition effect.[6]

Against this backdrop of methodological challenges, the early U.S. minimum wage regulations offer a unique opportunity for analysis. By focusing on states where minimum wage laws covered only a subset of industries, we can exploit a layer of policy variation not often available to researchers. Moreover, the fact that these laws targeted only female employees enables us to explore the legislation's differential impact on various worker groups and to investigate the channels of substitution between genders. In addition, studying the first wave of minimum wage legislation in the absence of federal regulation allows 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 versus 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 the legislation's effects on earnings. These estimates are usually difficult to obtain due to the lack of longitudinal information, which hinders the ability to differentiate between an increase in average earnings due to a composition effect (firing of low-productivity employees and hiring of new, more productive ones) and an increase stemming from a simple raise in the wage rates of pre-existing employees. We address this issue by using longitudinal data from Oregon, as documented by Obenauer and von der Nienburg (1915) from the local Bureau of Labor Statistics (BLS), which include wage information for a set of women employed both before and after the minimum wage was implemented. Within-worker analyses of these data show that the legislation led to an average increase in wages for women previously paid at below the minimum wage level, while wages for women already earning above the minimum wage threshold remained unchanged, on average. 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 estimating the employment effects on women.

In our baseline analysis, we use full count census data from 1880 to 1930 to construct a panel dataset organized by industry, occupation, gender, county, and decade. After digitizing the minimum wage laws, we link them to industries and states. We then treat the enactment of this legislation in 12 U.S. states (for simplicity, we count D.C. as a state) as a policy shock that introduces a price floor on female labor, allowing us to estimate the impact of these laws on women's employment. Since this was the first minimum wage legislation that lifted the minimum wage from zero to a positive figure (\$10 weekly, on average), we estimate our models using both a specification with a binary treatment variable and linear specifications with the dollar value of the state-industry-specific minimum wage as our main variables of interest.

We estimate our models using two separate identification strategies. The first considers the full sample of counties in the census and estimates the parameter of interest via a standard triple-differences approach. This

[6] Using data from the Current Population Survey, Clemens and Strain (2022) show that increasing the minimum wage increases the likelihood of sub-minimum wage payments, an indication of imperfect employment compliance. Jardim et al. (2018) study Seattle's minimum wage increase. Earlier examples include Linneman (1982) and Currie and Fallick (1996).

approach involves a detailed comparison: it contrasts the changes in county-industry-level female employment over time, focusing specifically on a treated industry in a county that has adopted a minimum wage for that industry against an identical industry in a county of a state where the minimum wage has not been adopted. This is done while controlling for state-specific, county-specific, industry-specific, and occupation-specific time fixed effects. However, this method may suffer from bias if local industry-specific trends in female employment are related to the implementation of minimum wage legislation. In other words, both the main independent variable of interest (the implementation of the legislation) and the main outcome of interest (female employment) could be generated by the same source (upward trends in female employment in specific local industries), thereby likely biasing a regression coefficient upwards.

To address this potential issue, we employ a second identification strategy that accounts for the local industry trends, albeit with a smaller (and potentially selected) sample. Specifically, this strategy, outlined in the specification mentioned earlier, uses a contiguous-county-pair research design. It involves comparing changes over time in county-industry-level female employment between neighboring counties in contiguous county pairs that straddle state borders. This comparison is made after controlling for 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, this strategy allows us to account for local trends in unobservable variables that may affect female employment, which in our case include changes in industry-specific local demand for female labor, gender norms, local occupational structure, and local institutions.[7]

However, our setting differs in three major ways from that of Dube, Lester and Reich (2010). First, our treatment is often industry specific, providing the flexibility to adjust for industry-specific trends and control for heterogeneity in local industry compositions.[8] Second, in our context, men are never subject to minimum wages, allowing 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.

Using our triple-difference identification strategy in the full sample, we find that, on average, the introduction of the minimum wage legislation decreased women's employment at the industry-county level.

[7]The advantages of using state borders for identification are well understood. They have also been used in other contexts, for example, manufacturing (Holmes, 1998), banking (Huang, 2008), suffrage (Naidu, 2012), and private prisons (Dippel and Poyker, 2023). There is also a growing literature that criticizes this identification strategy (see, e.g., Jha, Neumark and Rodriguez-Lopez, 2022 on Dube, Lester and Reich, 2010). As we discuss in Section 3.2, while this paper's historical and institutional context suggests that this strategy may be appropriate to study the economics of the early 20th-century minimum wage regulation, we do not take a specific stance. In fact, the strategy that exploits states' borders for identification also requires a significant reduction in the sample, restricting the sources of identifying variation and therefore inevitably leading to a much more "localized" set of estimates.

[8]This feature of our setting suggests that compared to Dube, Lester and Reich (2010), an empirical strategy that does not limit the analysis to border counties may still be valid, given that the policy's specific structure allows us to separately control for county- and industry-specific trends. Nevertheless, as discussed later, a full sample analysis may suffer from bias due to county-industry selection. Moreover, in an aggregate county-level analysis, we adopt an identification strategy that relies on identical assumptions as in Dube, Lester and Reich (2010), given that we aggregate out the industry-level variation.

However, this change is not statistically distinguishable from zero at the conventional confidence levels. We also find that the legislation had no statistically detectable effect on aggregate local female employment at the county level. However, this net null measured impact masks significant heterogeneity based on the intensity of the treatment (as measured by the share of women in treated industries) and the cross-industry concentration at the county level.

In the sample of contiguous counties straddling state borders, we find that, on average, the legislation decreased women's employment by at least 3.1% at the industry-county level.[9] This suggests the presence of two distinct margins of adjustment in response to a drop in industry-specific local labor demand: women leaving the labor force or switching industries.[10] 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 the legislation, those who worked in affected areas and industries switched to different industries or left the labor force. While we confirm that both channels are in place, we document that the decreased likelihood of employment due to being affected by the legislation is mostly driven by married women, who are 4.5 percentage points less likely to participate in the labor force in 1920.

To further investigate the labor demand impact of minimum wage at the local level, we compute the implied own-wage elasticity of employment at the county level and observe how it changes as a function of cross-industry concentration. We estimate elasticities of employment 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 by a single industry.[11]

After documenting the effect of minimum wage legislation on women's employment, we analyze labor demand for men and male minors. We find that, on average, treated industries observe a 1.2% increase in adult male employment and a 2.5% increase in male minor employment, with aggregate male labor demand not changing at the county level. These results suggest a substitution between genders at the locality-industry level. To explore the mechanism, we set up a simple labor demand framework and conclude that the women-to-men ratio decreased by 4.7% at the locality-industry level. 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 dominated by women (share of women >75%) or dominated by men (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

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

[10] In principle, they might also react by moving out of treated areas, but later in Section 4.3, we show that this is not the case.

[11] In practice, due to the lack of detailed nationwide earnings data, we compute labor demand elasticities with respect to own-wage. This is achieved by dividing the employment elasticity with respect to minimum wage, obtained from our main empirical analysis, by the earnings elasticity with respect to minimum wage, estimated using the longitudinal sample from Oregon.

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 changed how they organized their production in response to a price floor on one of their inputs.

This paper contributes to the robust literature on the effect of minimum wages (Neumark, Salas and Wascher, 2014; Dube, Lester and Reich, 2016; Cengiz et al., 2019, among others) in three main ways. 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.[12] Second, we analyze minimum wage legislation that is gender specific and often industry specific. This induced variation 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 studying 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 of Azar, Marinescu and Steinbaum (2022) and Azar et al. (2023).[13]

From a broader perspective, the paper contributes to the literature on the development of American labor institutions at the beginning of the 20th century (Fishback, 1998, 2020; Currie and Ferrie, 2000; Goldin, 2000; Allen, Fishback and Holmes, 2013; Naidu and Yuchtman, 2018; Farber et al., 2021) and to the literature on the labor outcomes of women during the same period (Landes, 1980; Goldin, 1986, 1988*a*,*b*, 1994; Gruber, 1994; Naidu, 2012; Poyker, 2019). Our study makes significant contributions by assessing the impact of one of the most debated labor institutions in this country, documenting the phenomenon of substituting female employees with male counterparts as a result of state-level economic policy interventions.[14] We argue that these policies led to a new equilibrium that increased the employment gap between genders while narrowing the earnings gap, conditional on employment.[15] This policy change presents a unique opportunity to study the interaction between gender dynamics 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.[16] The

[12]The policies we study likely include the largest relative minimum wage increase (minimum-wage-to-median-earnings ratio) in U.S. history. Using detailed earnings data from Oregon, we compute that the minimum wage was between 90% and 103% of median earnings before the regulation occurred, suggesting that in that context, it affected about 50% of working women. We also know that in 1910, women constituted 21% of the labor force, and 71% of them were in the affected industries and locations. Hence, the legislation affected around $0.21 \times 0.71 \times 0.5 = 7.5\%$ of the total labor force in the 1910 population of the 12 states. Since the 12 states constitute around a third of the female labor force, at a national level, this figure is around 2.5%. By contrast, Germany and the UK introduced minimum wages for the first time in 2015 and 1999, respectively, covering 10%–14% of the eligible workforce in Germany and 6.4% of workers in the UK. See Caliendo, Wittbrodt and Schröder (2019) for the German case and Stewart (2004) for the UK case.

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

[14]More generally, we contribute to the literature on substitution across groups (Fairris and Bujanda, 2008; Giuliano, 2013; Horton, 2017; Clemens, Kahn and Meer, 2021), where we contribute with the gender angle of labor substitution. See Clemens (2021) for a summary of the labor-labor substitution literature.

[15]As explained later in Section 8, the net earnings effect unconditional on employment is likely positive. We provide a back-of-the-envelope calculation that suggests minimum wage legislation may positively impact women. Of course, this is an average estimate, and therefore it masks the heterogeneity of the impact among women employed before the implementation.

[16]The minimum wage might also interact with inequality in the labor market through the racial gap. This topic is extensively explored in Bailey, DiNardo and Stuart (2021) and Derenoncourt and Montialoux (2021), in which the authors study the

earnings gap between genders underscores the importance of labor protection laws, such as the minimum wage, for female wage workers, who in the last decades have been disproportionately represented among low-wage earners (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, showing that marital status—which induces variation in unearned income—determines how affected female workers respond to the shock. Second, we contribute to the findings of Acemoğlu, Autor and Lyle (2004), who estimate the gender elasticity of substitution caused by the World War II (WWII) draft in the United States. Instead of a 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 similar results to Acemoğlu, Autor and Lyle (2004).

2 Background, Factual Records, and Longitudinal Evidence

2.1 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 meet their basic needs at current wage levels. For example, when Kansas Industrial Welfare Commission (1917) surveyed 5,436 women employees, they found that 31% of them earned below \$6 per week,[17] concluding that "they hardly have enough to sustain life."[18]

The inception of early minimum wage legislation occurred 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. Nevertheless, courts were inclined to favor labor protection legislation specifically aimed at women, possibly influenced by paternalistic attitudes, thereby curbing women's ability to negotiate their employment terms.[19]

The highest lower bound to wage rates was set at \$20 per week for women working in North Dakota in office occupations, while the lowest was set at \$7 per week for women working in Kansas in the laundry and dry cleaning industry.[20] The Women's Bureau, 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

contribution of minimum wage legislation on the racial earnings gap by exploiting variation generated by the extension of coverage and by the rising 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 these two papers arises from changes in the minimum wage from 0 to a positive price floor.

[17]To put this number into perspective, consider that, according to a 1910 Senate investigation into wages and prices of commodities (available at https://babel.hathitrust.org/cgi/pt?id=uc1.b3991874&view=1up&seq=9&skin=2021) in Topeka, KS, a quart of milk cost 8 cents and a dozen eggs cost 35 cents.

[18]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).

[19]We provide details on the gender bias in labor protection laws during the Lochner era in Appendix A.1.

[20]Or \$338 and \$193 per week, when translating 1915 dollars into 2022 dollars (assuming 40 hours working week—\$8.45 and \$4.83 per hour). See https://www.dollartimes.com/inflation/.

von der Nienburg, 1915; Massachusetts Minimum Wage Commission, 1916). The agency also published a report (summarized in Thies, 1990) that surveyed women and firms, concluding that the laws were efficient in raising their wages and did not result in women losing their jobs.[21] However, 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, 12 states had adopted minimum-wage-related laws.[22] Arizona, Minnesota, Oregon, Utah, Washington, and Wisconsin eventually adopted minimum wage laws covering women in all industries, while the laws implemented by Arkansas, California, D.C., Kansas, Massachusetts, and North Dakota only covered a select number of industries (Appendix Figure E.1).[23] States were empowered to punish employers who failed to comply with these laws, with sanctions ranging from fines to imprisonment (see Table D.2 for details). The enforcement apparatus varied by region, involving state and county prosecuting officers, the BLS, or industrial commissions, depending on the jurisdiction. Violations were typically revealed either through inspections or reported complaints. Importantly, employers were mandated to post information about minimum wage legislation in a visible spot for workers to know their rights. The Women's Bureau of the Department of Labor (Department of Labor, 1928, Ch. XII) provides an in-depth account of how these laws were enforced, including the penalties imposed and the approaches and outcomes of investigations.[24]

Almost immediately after the first law was implemented, manufacturers began opposing minimum wage in Oregon. 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 the state'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 5th Amendment's due process clause.[25]

Soon, minimum wage laws were abolished in Arizona (1925), Arkansas (1927), California (1925), Kansas (1925), Utah (1929), and Wisconsin (1924). However, these laws existed in Massachusetts, Minnesota, North Dakota, Oregon, and Washington until 1938, when they became obsolete.[26] For additional details, the most complete history of the pre-FLSA minimum wage legislation can be found in Fishback and Seltzer (2021).

[21]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.

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

[23]See Appendix Table D.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 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.

[24]States allowed sub-minimum wages for (i) inexperienced (less than a year of experience) female workers (generally their wages were \$1 less than minimum wages) and (ii) "slow" workers (Department of Labor, 1928, pp. 278–279.). States also 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, D.C. issued 87, and California issued at most 2,400 licenses for substandard workers (Thies, 1990, p. 740).

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

[26]Appendix Table 1 summarizes the timing of implementation and abolition of minimum wage laws.

2.2 The Impact of Minimum Wage Legislation on Wages: Longitudinal Evidence from Oregon

While the BLS studied the effects of minimum wage laws on women's outcomes since the introduction of these laws (e.g., Department of Labor, 1928), data on wages between the 19th and 20th centuries remain scarce,[27] making it difficult to estimate their impact on wage levels. To address this issue, we use unique longitudinal data from one of the first empirical minimum wage studies, conducted by Obenauer and von der Nienburg (1915).[28] Studying the impact of minimum wage legislation in Oregon, the authors collected information on wages for a sample of around 370 women across the state, with longitudinal information on their wage levels before and after the law was enacted.

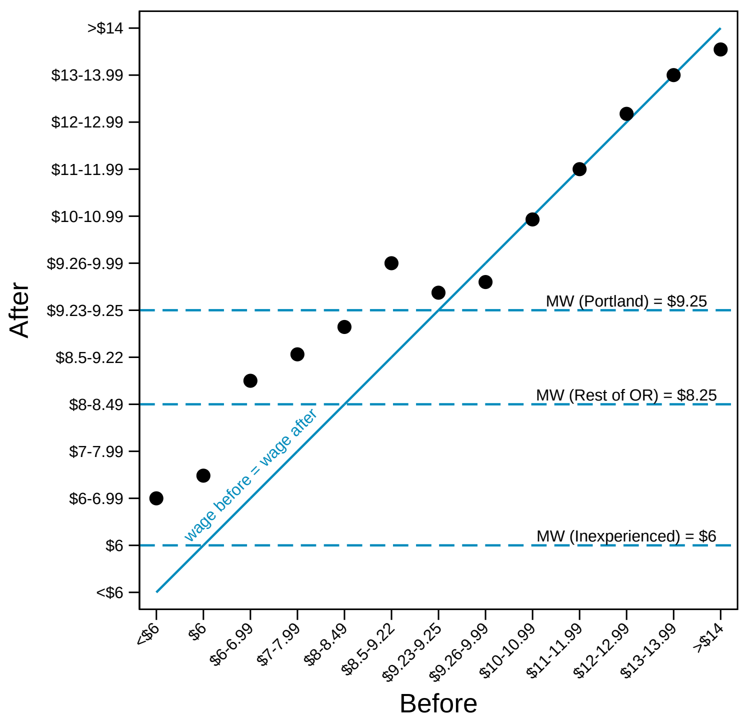

Figure 1: Changes in the Weekly Rate in Oregon before and after Minimum Wage Determination

Notes: The figure shows wage data for 374 women interviewed by the BLS (Obenauer and von der Nienburg, 1915). We do not observe whether each particular woman is located in Portland or another location in Oregon.

These data allow for a within-worker, semiparametric analysis of the impact of minimum wage laws on wage levels. Using these data, in Figure 1, we plot the weekly wage level after the minimum wage was imposed as a function of the pre-legislation wage, and then 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

[27] In Appendix A.2 we provide factual records from sources collected by local statistical bureaus, industrial commissions, and other contemporary observers at the time minimum wage laws were being put into effect.

[28] Kennan (1995) analyzes these data, concluding that in most of the observed cases, wages remained unchanged after the minimum wage laws were enacted.

rank. We find an increase in weekly earnings for all women with pre-legislation wages below the highest newly implemented minimum wage level ($9.25 weekly in Portland). However, the wage level is almost unchanged for workers with pre-legislation 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 the laws were binding.[29]

3 Data and Identification Strategy

3.1 Data

Using full count population 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.

Table 1: 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).

The data on minimum wage laws come from the U.S. Department of Labor Statistics, while laws related to women's employment come from the Women's Bureau, which we collected and coded (Department of Labor, 1924, 1927, 1928, 1937*a*, 1939). We then matched those laws to our dataset using census industry codes.[30] Table 1 and Appendix Table D.1 provide a summary of these laws.[31]

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

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

[31] We only coded 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 since we want to capture the effect of moving from a zero to a nonzero minimum wage. Because these changes

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

We use two different samples for our triple-difference specifications: a full sample and a contiguous-border county-pairs (CBCP) sample. The full sample contains all counties in all contiguous U.S. states, shown in Figure 2. The CBCP sample only includes counties in states with minimum wages and adjacent counties in states without minimum wages, and it allows conditioning on unobserved local and industry-specific trends (Holmes, 1998; Huang, 2008; Dube, Lester and Reich, 2010; Coviello, Deserranno and Persico, 2018) at the cost of a smaller sample size.[32] In our setting, using the CBCP sample enables us to control for trends in industry-specific local characteristics (such as demand for female labor, gender norms, local occupational structure, and local institutions), which may have caused a positive selection into treatment (i.e., in treated cells, it is possible that an existing upward trend in women's labor market outcomes precipitated the legislative action, rather than the legislation influencing these outcomes).

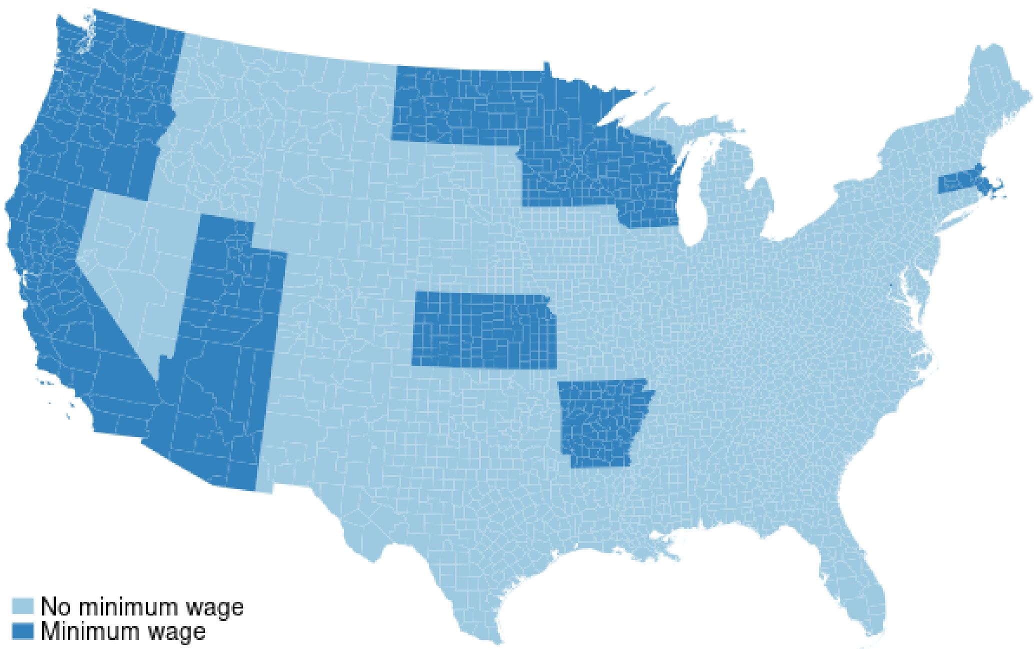

Figure 2: Counties in the Full Sample

To illustrate the value of the CBCP research design, consider comparing a (demeaned) county in Oregon to a (demeaned) county in Mississippi, as one would do in a full sample. This comparison may not produce unbiased estimates due to locality-specific unobservable trends in gender norms and occupational structure within industries. Therefore, even after conditioning on fixed effects, the selection of specific places and industries—driven by local economic conditions and gender composition in labor demand shocks—may lead to the implementation of minimum wage laws in places experiencing an increase in women's labor market

are very small, our results are almost identical if we use the minimum wages that were in effect in the 1920 and 1930 census waves or if we use wages weighted by years.

[32] 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 versus identifying the same changes in a border-county sample.

participation. This could result in a scenario where, for instance, Oregon counties exhibit faster-growing female employment in a given industry compared to the same industry in Mississippi counties. Such a disparity could suggest that the results for the full sample may be biased upwards. A better approach would then be, for example, to compare adjacent counties across the border between Arkansas and Mississippi, where gender norms and the occupational structure are expected to be more comparable than between Oregon and Mississippi.[33] Using a set of county-pair fixed effects, we can compare female employment in an industry-county cell across very similar counties with similar industry trends.

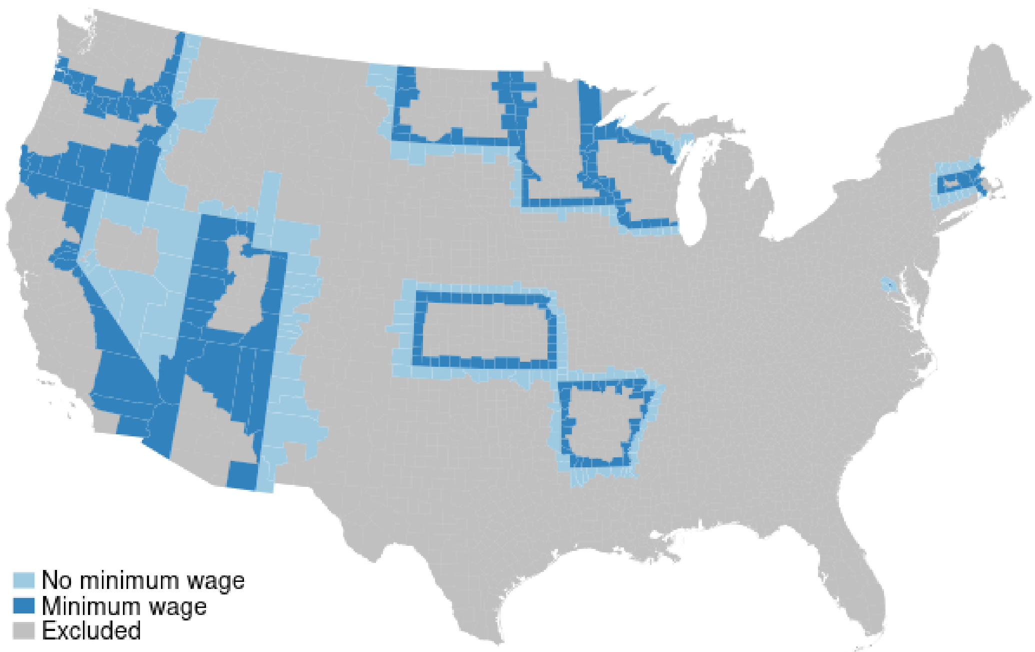

Figure 3: Contiguous-Border County-Pairs

For this identification strategy, our sample consists of only contiguous county pairs that straddle state borders. The analysis includes 12 states with gender-specific minimum wages, along with their 24 neighboring states, totaling 36 states in the study. This encompasses 701 counties across 419 distinct county-pairs. Figure 3 depicts the contiguous counties included in the analysis. Counties in minimum wage states appear in dark blue, and those in non-minimum wage states appear in light blue.

Table 2 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.[34] For example, Utah 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), and therefore when comparing Utah and Colorado (segment #34), we will use variation in all industries. Moreover, Utah shares segment #11 with Arizona, which also has a minimum wage law for women in all industries. Thus, this segment will not

[33] A specific concrete example would be that in the full sample specification, one cannot control for the county-industry-specific marriage bars, while it is likely that in two adjacent counties, people have similar views on the employment of married women.

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

Table 2: 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 E.5 does. The table also visualizes states' average minimum wages across industries with the minimum wage (or all-state minimum wages). The 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.

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 equals \$7.5 and \$10 in Arizona.

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 D.1 for details). 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 Table 1). 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.[35]

While the advantages of this strategy in terms of parameter identification (which we discuss in Section 4.1) 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 based on observable characteristics. To confirm that this is the case, Table D.3 provides summary statistics on different subsets of counties. Column I reports statistics on the socioeconomic characteristics of all counties, and column II reports the same for only counties in the CBCP sample. Reassuringly, column 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 the total population, the share of women, the ratio of employed women to employed men, or labor force participation. Column IV reports the difference between cross-border contiguous counties, showing that *within* such pairs, socioeconomic characteristics do not significantly vary between counties.

4 The Impact of Minimum Wage Legislation on Industry-Specific Local Female Employment

In this section, we report the regression analysis results for the effect of the minimum wage laws on female employment. Section 4.1 introduces our empirical specifications, and Section 4.2 reports the main employment results. Section 4.4 discusses robustness of the identification assumption and alternative explanations, and Section 4.3 contains robustness and sensitivity checks.

4.1 Empirical Specification: Employment by Industry

We first estimate the legislation's effect on the full sample of all U.S. counties. The specification is as follows:

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

[35] 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.

where the unit of observation is an industry-occupation i, in county c, nested within state s, in decade t. In this specification, all counties can contribute to the identification of β_{full}. In this section, we show and discuss results for female workers (i.e., $g = w$), and we provide results for men and male minors in Section 7.1.

Following Neumark, Salas 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 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.[36]

We use two measures of the explanatory variable of interest. The first—Minimum wage, $\$10_{ist}$—is a \$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 \$10. The second—$\mathbb{1}\,(\text{Minimum wage})_{ist}$—is an indicator variable equal to 1 if the industry-occupation i in state s at year t has a minimum wage legislation, and zero otherwise. Thus, the coefficient should be interpreted as a percentage change in employment after introducing the minimum wage.

μ_{st} are state-specific time controls, and $\Psi_{c(s)t}$ are county-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-decade fixed effects allow us to absorb location-specific trends in law enforcement, permanent migration, or labor supply shocks (e.g., WWI draft).

Φ_{is}, and Φ_{it} are industry-state and industry-decade fixed effects. The former addresses possible state-specific support to certain industries, while the latter absorbs industry-specific trends (e.g., technological progress). We also absorb industry-county fixed effects ($\Phi_{c(s)i}$). Non-industry-specific legislative trends, which are not local, will still be absorbed by state-decade fixed effects (μ_{st}) and by county-decade fixed effects ($\Psi_{c(s)t}$) in our most saturated specification.

The coefficient β_{full} essentially represents a triple-difference estimator since treatment is administrated at the state-year level, but only a subset of industries is affected. Additionally, because the treatment is on the state-industry-decade level, we double-cluster standard errors by state and industry/occupation cells.

We then estimate the effect of a minimum wage on the CBCP sample. The specification is as follows:

$$\ln\left(EmpShare_{gip(c)t}\right) = \beta_{cbcp} \cdot \text{Minimum wage}_{ist} + \mu_{st} + \Psi_{p(c)t} + \Phi_{is} + \Phi_{it} + \Phi_{p(c)i} + \epsilon_{gip(c)t}, \quad g = \{w\} \quad (2)$$

where the unit of observation is an industry-occupation i, in county-pair $p(c)$, nested within state s, in

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

decade t. Here, only contiguous county-pairs that straddle state borders can contribute to the identification of β_{cbcp}. This is reflected in the presence of county-pair-specific time fixed effects, $\Psi_{p(c)t}$.

We use the same set of fixed effects as in the full sample but use county-pairs instead of counties. Here, $\Psi_{p(c)t}$ are county-pair-decade fixed effects that allow us to absorb location-specific trends in law enforcement, permanent migration, or labor supply shocks. Industry-county-pair fixed effects ($\Phi_{ip(c)}$) address local (at the county-pair level) industry-specific time-invariant employment characteristics. Additionally, the coefficient β_{cbcp} represents a triple-difference estimator since treatment is administrated at the state-year level, but only a subset of industries is affected.

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).[37]

4.2 Impact of Minimum Wage Laws on Female Employment at the Industry Level

4.2.1 Results for the Full Sample

In this section, we estimate the effect of a minimum wage on the full sample of all U.S. counties. Table 3 contains the results of estimating Equation (1) for female employment, with each row containing coefficients from a separate regression.

The specifications become incrementally more saturated with fixed effects when moving from left to right. Column I reports results for the specification with (time-invariant) county fixed effects $\Psi_{c(s)}$, as well as state-specific industry/occupation Φ_{is} and year fixed effects μ_{st}. 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 county-specific heterogeneity in industrial policy. Columns IV–VI are analogous to the previous three columns except that we now control for county-specific year fixed effects, which allow for locality-specific employment trends.

Panel A of Table 3 reports results for the variable Minimum wage, $\$10_{ist}$. We find that in a linear setting, on average, a \$10 increase in the minimum wage is associated with a 1.3% drop in female employment as shown in column I, though this finding is not statistically significant. By the time we reach column VI, the effect of the minimum wage increase on female employment diminishes to zero. Panel B shows estimates from the same specifications, except the main right-hand-side variable is binary and equal to 1 if industry i in state s is covered by minimum wage legislation at time t. The point estimates for this panel vary, suggesting that treated industries show a reduction in female employment ranging from 3.5% to 1.4% on average, albeit not statistically significant at conventional confidence levels.

[37] All results hold if we instead double-cluster by state and border segment. Additionally, 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.

Table 3: The Impact of Minimum Wage Legislation on Women's Employment (Full Sample)

	I	II	III	IV	V	VI
	Dependent variable: Log employment share (women)					
Panel A: ~Full sample						
Minimum wage, $10	-0.013	-0.003	-0.0001	-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	1,362,571	1,362,571	1,362,571	1,362,571	1,362,571	1,362,571
Panel B: ~Full sample w dummy						
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	1,362,571	1,362,571	1,362,571	1,362,571	1,362,571	1,362,571
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 (1). Each panel contains coefficients from separate regressions with Minimum wage in dollars$_{ist}$ and $\mathbb{1}$ (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

4.2.2 Results for the CBCP Sample

Table 4 contains the results of estimating Equation (2) for female employment, using only counties in pairs that straddle state boundaries. Each row contains coefficients from a separate regression.

All the specifications in the table have an analogous set of fixed effects to those in Table 3 but use county-pair fixed effects instead of county fixed effects. 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}. 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, except we now control for county-pair-specific year fixed effects, which allow for locality-specific employment trends. Column VI is similar to the main specification of Dube, Lester and Reich (2010), except we add industry variation.

Panel A of Table 4 reports results for the variable Minimum wage, $\$10_{ist}$. We find that, on average, in a linear setting, a $10 increase in the minimum wage decreases female employment by 1.5%.[38,39] Panel B shows estimates from the same specifications, except the main right-hand-side variable is binary and equal to 1 if an industry i in state s is covered by minimum wage legislation at time t. The panel shows that treated industries experience a 3.1% reduction of female employment, on average.[40]

[38]Given that some of the minimum wage laws 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.

[39]$10 per week in 1920 is approximately $147.5 in 2022 dollars or $3.69 per hour assuming 40 hours workweek.

[40]We can also further expand our understanding of the effects of minimum wage by examining whether there are different

Table 4: The Impact of Minimum Wage Legislation on Women's Employment (CBCP Sample)

	I	II	III	IV	V	VI
	Dependent variable: Log employment share (women)					
Panel A: ~CBCP sample						
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)
Difference w full sample, p-value	0.209	0.124	0.003***	0.242	0.157	0.047**
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: ~CBCP sample w dummy						
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)
Difference w full sample, p-value	0.219	0.157	0.292	0.229	0.176	0.230
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 (2). Each panel contains coefficients from separate regressions with Minimum wage in dollars$_{ist}$ and $\mathbb{1}$ (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

4.2.3 Comparison of Results for the Full and CBCP Samples

Compared to the results for the CBCP sample in Table 4, the results for the full sample in Table 3 show coefficients that are largely negative but smaller and not significant at conventional confidence levels. While both methods yield directionally similar conclusions, there are differences in magnitude and statistical confidence between them. These discrepancies could result from the distinct selection of observations in the CBCP sample compared to the full sample. Additionally, pre-existing positive trends in local industries within treated county-industry cells might be attenuating the effects. Such trends could upwardly bias the coefficient in the full sample, suggesting a more positive or less negative impact. However, this bias is likely reduced in the estimates obtained using the county border research design.

Note that in Section 3.2, we showed that the counties in the CBCP sample represent almost a quarter (701) of all counties from 36 out of 48 (plus D.C.) states and are comparable in terms of observable characteristics to the full sample of counties.[41] This suggests that the difference between $\hat{\beta}_{full}$ and $\hat{\beta}_{cbcp}$ is consistent with the presence of an omitted variable and not with a selection bias. Pre-existing positive trends in

impacts from the introduction versus abolition of state minimum wage laws. Following the *Adkins v. Children Hospital* decision in 1923, seven states abolished their gender-specific minimum wage laws, allowing us to define a separate variable for the introduction/increase in minimum wage and a variable for the abolition/decrease in minimum wage, and estimate Equation 2. In Appendix Table D.4, we replicate the specification from column VI of Table 4, and our estimates for the introduction of a minimum wage remain negative and significant. We also see a statistically significant increase in female employment in county-industry combinations where the minimum wage was abolished. The magnitude of the coefficients for the introduction and abolition of minimum wages are similar in size but are slightly larger in absolute terms for the abolition. The differences in magnitude may stem from the fact that the locations abolishing minimum wages were originally part of the group that imposed them.

[41]See column III of Table D.3.

the employment of women in affected county-industry combinations could be the cause of gender-specific minimum wage legislation rather than a consequence. For example, McCammon (1995) discusses certain pre-existing characteristics of certain industries in certain states that might have led to labor protective legislation at the turn of the century. These include the pre-existing proportion of women in the labor force, the formation of groups of organized female labor such as the National Women's Trade Union League, the health hazards that certain industries posed to child-bearing women, and the formation of employer organizations.[42] All these factors that affected the likelihood of protective legislation for female workers are also clearly related to levels of female employment, hence potentially creating unobserved confounding trends in location- and industry-specific female employment. Therefore, the CBCP sample is particularly well-suited for analyzing the institutional context of the early 20th-century U.S.[43]

Overall, both identification strategies provide valuable information. However, in Sections 4.3 and 4.4, we focus on the robustness of the analysis performed on the CBCP sample.

4.3 Robustness of the Identification Assumption and Alternative Explanations

In this section, we explore evidence that supports the absence of residual confounding factors. First, as usual with quasi-experimental research designs based on "differencing out" endogenous variation, we provide evidence suggesting that absent the treatment, treated cells would behave in the same way as untreated cells. This is usually done in the literature by showing that *before* the treatment, units follow parallel trends. Second, we discuss and address the potential bias coming from the 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 In Appendix Table D.8, we provide several placebo tests showing that our results are not driven by pre-existing local industry and gender-specific trends. With the full set of fixed effects included, identification of our baseline estimates in Table 4 comes from within-state-industry variation. To ensure that this variation reflects the effect of minimum wage legislation changes rather than local industry trends, we adjust the treatment period by 20 years ($t-2$), relative to state- and industry-specific year fixed effects. Essentially, we apply the same treatment criteria in terms of affected industries and states but shift the timeline: the minimum wage in effect at $t = 1920$ is analyzed as if it were implemented at $t' = 1900$, and similarly, the legislation from $t = 1930$ is considered at $t' = 1910$. We exclude 1920 and 1930 from the regressions to ensure that treated states-counties are not in the regression. We find that none of the resulting estimates for female employment (Panels A and B) has a significant negative coefficient, making it unlikely that unobservable confounders are driving our results.

[42] See also, for example, Obenauer and von der Nienburg (1915) and Kansas Industrial Welfare Commission (1917).

[43] See a discussion of the validity of CBCP samples in other contexts in Dube, Lester and Reich (2010), Allegretto et al. (2013), Allegretto et al. (2017), Neumark and Wascher (2017), Cengiz et al. (2019), Schmidt, Shore-Sheppard and Watson (2019), and Jha, Neumark and Rodriguez-Lopez (2022).

In Appendix Figure E.4, we also directly test for the presence of pre-treatment trends in a fully dynamic difference-in-differences specification. The figure does not show any systematically statistically significant pre-trends, and joint tests for all the pre-trend coefficients do not reject the null hypothesis of them being zero. This suggests that the implementation timing of the minimum wage laws does not correlate with pre-existing negative trends in female employment. Additionally, the magnitude of two point estimates for periods following the enactment of the laws aligns with the point estimates of $\widehat{\beta}$s from the corresponding specifications in Table 4. Thus, the effect remains constant across decades, and our baseline specification (2) captures the entire temporal impact of the legislation.

Migration When considering potential migration across borders, it is important to contextualize our results. First, our triple-difference strategy ensures that all estimated models in this paper adjust for changes in employment rates at the county level over time. Second, permanent migration is accounted for due to our specifications all having employment rates on the left-hand side, accounting for population changes across counties over time. Third, even in the unlikely scenario that women from untreated areas became attracted by higher minimum wages and moved to treated areas without changing their residence, their movements would result in attenuated results. Finally, using a newly constructed linked census (see a full discussion in Section 6.1), we demonstrate that women in affected county-industry combinations do not differentially migrate out (see Appendix Table D.17).[44] In addition to this supply-side evidence, we also provide demand-side evidence suggesting that firms crossing over state borders to avoid policies cannot explain our results. In particular, we ensure that minimum wage laws had no impact on the total number of establishments at the county level.[45]

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 targeted maximum weekly hours for female workers (Department of Labor, 1927, 1937*b*).[46] In Appendix Table D.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 including this control. In doing this, we ensure that our main results are not driven by laws that cap working hours.[47]

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

[45] Here we use Equation 4 with log number of firms as the dependent variable. We find no effect of minimum wages (point estimate = 0.002 and s.e. = 0.025). See the full discussion of that specification in Section 5.

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

[47] We do not 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.

Confounding Factors II: WWI Draft of 1917–1918 Another confounding factor is the draft caused by the U.S.'s decision to join the Entente closer to the end of WWI. As a result, in 1917–1918, approximately 4.8 million American (mostly White) men left (~2 million of them as volunteers).[48] One might believe that such a significant shock to the labor supply could skew our results; however, we provide several reasons to demonstrate that this is not the case. First, as the 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 low-skilled occupations, it should still correlate with a specific set of treated industries in specific locations to bias our results. Third, since the drafted men were low skilled and more likely to compete for the same jobs as women (both within the industry and between low-skilled industries), this would increase the likelihood of more women being hired. Therefore, the possible correlation of the WWI draft with gender-specific minimum wage laws complicates our ability to identify a negative effect of minimum wage on employment. Finally, in Appendix Table D.9 we use county-level data from WWI veterans from the 1930 Census as a proxy for the WWI draft to show that it does not drive our results.[49] Column I controls for the log of WWI veterans, and column II shows no differential effect of the draft in locations with the minimum wage on women's employment in 1920 (columns I–III).

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

First, marriage bars were policies of individual firms and, in the case of school boards, individual localities.[50] Hence, our use of the CBCP sample is ideally suited to address such local trends; if present, they should be consistent across adjacent counties. Moreover, as marriage bars were prevalent in certain industries, our baseline specification with industry-decade and industry-state fixed effects should absorb trends in the adoption 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," which were two sectors imposed by marriage bars 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

[48] See Rockoff (2004) for additional information.

[49] Because county-pair-year fixed effects absorb any changes in male labor supply due to the WWI draft, we instead show that the draft did not affect the *aggregate* effect of minimum wage on employment, in the aggregated county-pair-decade level specification that we discuss in Section 5. Here, we assume that WWI veterans still live 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.

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

imposed extensive marriage bars and discretionary policies related to married women.[51] As a result, we first show that our results hold when excluding $t = 1930$ (see Table D.10) to omit the decade when marriage bars became more widespread (we discuss subsample analysis in greater detail in Section 4.4). Additionally, we show that our results are not driven by any particular industry. Figure E.6 shows the robustness of our results when various industries are excluded, including educational services (part of entertainment and recreation services) and clerical work (e.g., public administration or business services). The point estimates and significance of the effect of minimum wage remains almost unchanged.

Finally, the impact of marriage bars could confound our results due to the reintegration of WWI veterans into the workforce. Following WWI, veterans (predominantly low skilled) returning home would eventually compete with women in job market. Starting from the 1920s, marriage bars were introduced to exclude married women from employment, thereby giving preference to favor men. Thus, the wartime increase in female employment, coupled with the increased cost of female labor brought on by the minimum wage legislation and the subsequent reintegration of male veterans into the workforce, would have incentivized firms to adopt marriage bar policies. To address this concern, we interact the number of WWI veterans with minimum wage in 1930 in column III of Table D.9 to show that locations with minimum wages that experienced a larger return of veterans indeed experienced an even larger decline in female employment afterward (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 have exacerbated the effect of minimum wage laws, but they do not explain away our results.[52]

Confounding Factors IV: Unions and Suffrage Finally, we briefly discuss the role of unions and women's economic and political standing. 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), AFL President Samuel Gompers was openly opposed to any labor regulation that legislated a minimum wage (Amsterdam, 1982). While we are unaware 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 1920, with the 19th Amendment. However, 15 states granted the right to vote to women before then, and of these 15, 6 had also implemented minimum wages for women. This means that, not surprisingly, a correlation exists between women's suffrage and minimum wage legislation targeting women (states with pre-amendment female suffrage were about three times as likely to pass gender-specific minimum wage laws). However, all our specifications partial out state-year and state-industry fixed effects, taking care of any state-level institutional change.

[51] Twelve percent 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.

[52] We believe this is a very interesting topic deserving further investigation.

In a scenario in which progressive ideas regarding female employment were spreading at a local level and in certain industries (e.g., favored by specific technologies and specific gender ratios), minimum wage legislation and female employment could be jointly determined by the same source, thereby biasing our results.[53] However, the CBCP identification approach described earlier allows us to partial out common local industry trends and to address this source of the omitted variable.

4.4 Additional Robustness and Sensitivity Checks

In this section, we provide additional robustness and sensitivity checks. We consider robustness to (i) excluding any industry or any minimum wage state (and its adjacent border segments), (ii) including non-occupational groups or excluding 1880 or 1930 census years, and (iii) alternative specifications.

To show that our results are not driven by any specific state, Figure E.5 reports on the robustness of the estimate in column VI to dropping one state at a time. The estimated coefficient always remains significantly different from zero. For example, dropping Kansas, which shares border segments with four states (segment #16, 17, 18, 19 in Table 2), decreases the coefficient the most, from -0.031 to -0.037. Dropping D.C., which shares border segments with two states (segment #14, 15), increases the coefficient the most, from -0.031 to -0.028.[54] Similarly, in Appendix Figure E.6, we show the robustness of the estimate in column VI to dropping one industry at a time.[55] The estimated coefficient always remains significantly different from zero. Dropping manufacturing of non-durable goods decreases the coefficient the most, from -0.031 to -0.051, while dropping retail increases the coefficient the most, from -0.031 to -0.017.[56] Our results align with the recent findings by Harasztosi and Lindner (2019) that disemployment effects are larger in tradable sectors than in non-tradable ones; however, this pattern appears to be less pronounced in the case of historical minimum wage laws for women due to the cross-gender substitution margin. In addition, our results hold if we keep non-occupational industries (i.e., "keeping house") instead of setting them to missing. We repeat our baseline results for women in Appendix Table D.5.

In Appendix Table D.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 their minimum wage regulation, and they might have done so as a reaction to the effects of the regulation

[53]For example, Department of Labor (1928) suggests that local union representatives were invited to the wage boards at the stage of determining minimum wage levels. In principle, these representatives could have initiated campaigns in favor of female employment before legislation was enacted.

[54]Dropping Massachusetts, which shares border segments with five states (segments # 20, 21, 22, 23, 24), has almost no effect on the coefficient, but the 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%).

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

[56]Dropping personal services increases standard errors the most, as omitting this industry reduces the sample size the most, by 25%.

itself.[57] Excluding 1930 also helps us address the potential concern that the onset of the Great Depression disadvantaged female workers. Since the Great Depression might have amplified the legislation's impact, causing upward bias in our estimates, it is important to show that our results also hold when excluding 1930. Overall, our main results are robust to any of these qualitative and quantitative exclusions.

Finally, in Appendix Table D.6, we introduce a different specification that adds industry-occupation-year and industry-occupation-state fixed effects.[58] Comparing the baseline coefficient of Minimum wage, $\$10_{ist}$ in the first row of column VI of Table 4 to the coefficient in column I of Table D.6, we see that including industry-occupation-year fixed effects increase R^2 from 0.797 to 0.902. However, the coefficient does not change much and remains significant. The results hold when we add industry-occupation-state fixed effects. Furthermore, we obtain similar results in a specification with non-occupational 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.

5 The Impact of Minimum Wage Legislation on Aggregate Local Female Employment

In the previous section, we demonstrated that the legislation decreased women's employment in affected industries and locations. However, using the triple-differences specification in Equation 2, we cannot disentangle the two channels that might give rise to a drop in labor demand. In particular, we are interested in determining the extent to which the legislation's impact on women's employment is due to an aggregate decrease in employment versus a shift of employment across different industries. In this section, we estimate the former channel, while Section 6 focuses on the latter.

To estimate the legislation's aggregate impact at the local level, we start by presenting results on the full sample of U.S. counties and aggregate our data up to the county-year level. Our differences-in-differences specification is as follows:

$$\ln\left(EmpShare_{gc(s)t}\right) = \beta^1_{full} \cdot \text{Min. wage}_{st} + \beta^2_{full} \cdot \text{Min. wage}_{st} \times \text{Share affected workers}_{c(s),1910} + \mu_t + \Psi_{c(s)} + t\lambda_s + \mathbb{X}_{c(s)t} + \varepsilon_{gc(s)t}, \quad (3)$$

where $EmpShare_{gc(s)t} = \log\left(\frac{Total\ Employment_{gc(s)t}}{Total\ Working\ Age_{gc(s)t}}\right)$ in county c nested in state s, gender $g = w$, and year t. Share affected $\text{workers}_{c(s),1910}$ is the share of female workers employed in industries affected by minimum wage laws in 1910 (i.e., before the treatment). This variable equals 0 if state s has never adopted a minimum wage and equals 1 if the state adopted a universal minimum wage for women. $\Psi_{c(s)}$ and μ_t are county and

[57] 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 unconcerned with data quality issues that are consistent across locations and industries.

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

decade fixed effects, respectively. $t\lambda_s$ are state-specific linear trends, and $\mathbb{X}_{p(c)t}$ is the matrix of county-year level controls. Here, we only employ the most parsimonious set of controls: log of population, the share of women, the share of the rural population, and the share of the literate population.[59] We cluster standard errors by state. We include a measure of treatment intensity as we expect the impact to vary as a function of the share of women targeted by the legislation.

Table 5: The Impact of Minimum Wage Legislation on Women's Aggregate Employment at the County Level (Full Sample)

	I	II	III
	Dependent variable: Log employment share (women)		
Panel A: ~Full sample			
Average minimum wage, $	0.001	0.027**	-0.111***
(mean av. min. wage $6)	(0.007)	(0.013)	(0.026)
Average minimum wage, $ x Share women in treated industries in 1910		-0.031*	
		(0.016)	
Average minimum wage, $ x HHI in 1910			0.190***
			(0.047)
Mean of the interacted variable	-	0.69	0.58
R-squared	0.778	0.779	0.780
Observations	14,135	14,135	14,135
Panel B: ~Full sample w dummy			
1(Minimum wage)	0.010	0.121***	-0.523***
	(0.042)	(0.037)	(0.078)
1(Minimum wage) x Share women in treated industries in 1910		-0.169***	
		(0.014)	
1(Minimum wage) x HHI in 1910			0.898***
			(0.126)
Mean of the interacted variable	-	0.69	0.58
R-squared	0.778	0.781	0.779
Observations	14,135	14,135	14,135

Notes: This table reports the results from estimating Equation (3). Each observation is a gender-specific county-decade, and each regression includes county, state, and year fixed effects. The following variables are used as controls: log of total population, share of women, share of rural population, and share of literate population. Standard errors, clustered at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5 reports the results of the estimation. Panel A shows the results using the dollar value of minimum wage as the main treatment variable, and Panel B shows the results using a dummy variable. Column I presents the results without the interaction term, indicating no negative effect of minimum wages on aggregate employment. Column II shows the estimates derived from Equation (3), incorporating an interaction with the pre-treatment share of workers in affected industries. β^2_{full} appears to be negative, suggesting that counties with a larger share of affected women experienced a larger decline in women's labor force participation.

We then replicate Table 5 using the CBCP identification strategy and compare it with the results for the full sample. Our differences-in-differences specification is as follows:

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

$$\ln\left(EmpShare_{gp(c)t}\right) = \beta^1_{cbcp} \cdot \text{Min. wage}_{st} + \beta^2_{cbcp} \cdot \text{Min. wage}_{st} \times \text{Share affected workers}_{p(c),1910} + \mu_t + \Psi_{p(c)} + \Phi_s + t\lambda_s + \mathbb{X}_{p(c)t} + \varepsilon_{gp(c)t}, \quad (4)$$

where our specification is the same as in Equation 3, but the observation is a county-pair $p(c)$ instead of county c. $\Psi_{p(c)}$, Φ_s, and μ_t are county-pair, state, and decade fixed effects, respectively. We double-cluster the standard errors by state and border segment.

Table 6: The Impact of Minimum Wage Legislation on Women's Aggregate Employment at the County Level (CBCP Sample)

	I	II	III
	Dependent variable: Log employment share (women)		
Panel A: ~CBCP sample			
Average minimum wage, $	0.006	0.040**	-0.116***
(mean av. min. wage $6)	(0.009)	(0.015)	(0.030)
Average minimum wage, $		-0.038**	
x Share women in treated industries in 1910		(0.017)	
Average minimum wage, $			0.207***
x HHI in 1910			(0.056)
Mean of the interacted variable	-	0.71	0.59
Difference w full sample specification, p-value	0.662	0.553	0.898
~difference for the interaction, p-value		0.764	0.808
R-squared	0.770	0.772	0.775
Observations	3,020	3,020	3,020
Panel B: ~CBCP sample w dummy			
1(Minimum wage)	0.037	0.189**	-0.489***
	(0.056)	(0.071)	(0.099)
1(Minimum wage)		-0.237***	
x Share women in treated industries in 1910		(0.079)	
1(Minimum wage)			0.903***
x HHI in 1910			(0.173)
Mean of the interacted variable	-	0.71	0.59
Difference w full sample specification, p-value	0.689	0.783	0.798
~difference for the interaction, p-value		0.869	0.982
R-squared	0.768	0.773	0.773
Observations	3,020	3,020	3,020

Notes: This table reports the results from estimating Equation (4). Each observation is a gender-specific county-decade, and 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, and share of literate population. Standard errors, double-clustered at the state and border segment level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6 reports the results of the estimation. Panel A shows the results using the dollar value of minimum wage as the main treatment variable, and Panel B presents the results using a dummy variable. We observe the same pattern that we find in Section 4.2: all coefficients have the same signs as in the full sample, and the difference between the two specifications is not statistically significant at conventional confidence levels. Column I also indicates no negative effect of minimum wages on aggregate employment. Column II presents the estimates derived from Equation (4) by adding the interaction with the pre-treatment share of workers in affected industries. β^2_{cbcp} is negative, suggesting that counties with a larger share of affected women experienced a larger decline in women's labor force participation. In Section 5.1, we focus on the results in column III.

Tables D.11 and D.12 show that our results are robust to adding state-specific linear time trends. This adjustment is important as it accounts for the possibility that states may follow different trends regarding attitudes toward female employment, offering a closer comparison to the specification in Equations 1 and 2 that include state-year fixed effects. The results in Table D.11 for the full sample remain very similar to those in Table 5. However, the measured impact of the legislation in Table D.12 is higher in magnitude than corresponding specifications in Table 6.

Similar to the analysis at the county-industry level, the aggregate county-level analysis shows that both identification strategies provide valuable information. Moreover, the county-level results are very consistent across the two specifications.

5.1 Minimum Wage and Cross-Industry Local Labor Market Concentration

In an alternative specification, appearing in column III of Tables 5 and 6, 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 legislation's impact is related to the structure of the local labor market.[60] 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.[61]

The intuition centers on the concentration of firms within industries. In markets without a price floor with only 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 the firms to move to a higher-wage, higher-employment equilibrium. While we do not observe within-industry cross-firm concentration at the county level, we build a within-county concentration index 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 in both the full sample and CBCP sample specifications.[62]

[60] 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 county-level U.S. manufacturing census 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.

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

[62] The results for men are shown in Table D.18, and as expected, they mimic the results for women but with a flipped sign and a smaller magnitude. We discuss the results on the sample of men in greater detail in Section 7.1.

The results in column III show that the introduction of minimum wage laws in a market where each industry controls a very small share of overall employment before the legislation (e.g., cross-industry HHI = 0) would shrink employment by 37.6% (Panel B). Conversely, in a labor market dominated by only one industry, the estimated impact of the minimum wage would be an *increase* of female employment by 60.6%. In the average marketplace, where the pre-existing HHI is 0.59, the impact is approximately –1.8%.

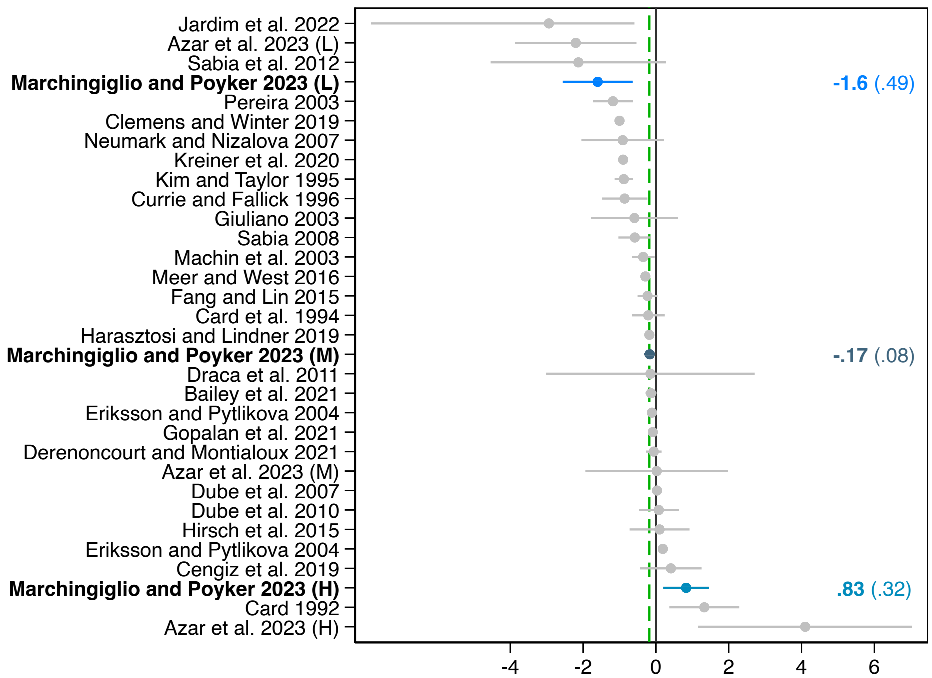

Figure 4: Own-Wage Employment Elasticity with Respect to Wage, Compared to the Previous Literature

Notes: Estimates from the previous literature are primarily from Harasztosi and Lindner (2019), with the addition of results from Meer and West (2016), Cengiz et al. (2019), Clemens and Wither (2019), Kreiner, Reck and Skov (2020), Bailey, DiNardo and Stuart (2021), Derenoncourt and Montialoux (2021), Gopalan et al. (2021), Jardim et al. (2022), and Azar et al. (2023). Note that the confidence intervals in Jardim et al. (2022) are asymmetric because they are based on permutation inference. 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), and 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.

Next, we compute the implied own-wage elasticities of labor demand. To do this, in Table D.13 we re-estimate Table 6, but instead of using the dollar value or a dummy for minimum wage, we use the inverse hyperbolic sine of the dollar value of the minimum wage in industry-occupation i in state s at year t.[63] Thus, the coefficient should be interpreted as an elasticity. We use the results from Table D.13 and the elasticity of earnings with respect to minimum-wage levels derived from the earnings data from Oregon.

[63] 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.

The pre-legislation lower bound on weekly earnings in Oregon was \$6, while the statewide minimum wage level imposed in 1914 was \$8.25.[64] To calculate the elasticity of earnings with respect to the minimum wage, we assume that the minimum wage increased by $\frac{8.25-6}{6} = 37.5\%$, which translates into an increase in post-legislation earnings of 6.8%. 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 Table D.13 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 on the impact of the minimum wage as a function of labor market concentration, as nicely summarized by Azar et al. (2023). Following their approach, in Figure 4 we plot the values of our implied elasticities for different levels of cross-industry concentration and compare them to the literature.

6 The Impact of Minimum Wage Legislation on Female Labor Market Outcomes: Evidence from Individual-Level Data and Linked Census Waves

To complete our understanding of the impact of minimum wage legislation on female employment, 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 exit the labor force or switch industries due to the legislation. This specification also allows us to directly test whether individuals migrated out of affected markets, thereby corroborating the previously shown evidence in the CBCP analysis.

6.1 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 link around 30% of the starting records.[65] For this analysis, we further restrict the sample to women aged 16 to 65 in 1920 who were working in 1910. To avoid incurring matching bias due to a change in last name after marriage, we only keep women who are either never or always married.[66]

[64] This rate was applied to all of Oregon except Portland, which had a minimum wage of \$9.25.

[65] To do this, we use restricted full count census data from 1910 and 1920 available on the NBER server, which include full strings or recorded names and last names. Appendix B contains more details on the linked sample's construction.

[66] 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 men (e.g., Bailey et al., 2020; Abramitzky, Boustan and Eriksson, 2012; Long and Ferrie, 2013). To provide more context, in 1910, by age 30, 90% of the women in the full count census were married. For this reason, we risk losing mostly young unmarried women aged 16 to 30, who account for 12% of the total female population in the 1910 Census. Studies in this literature do not drop matched observations based on consistent marital status across census waves because men do not change their last names after marriage. We are unaware 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 the authors must match as many women as possible from one industry, they innovatively combine full count census and FamilySearch data. As a result, they can obtain a linkage of all women in the telephone operator industry at the trade-off of a lower matching rate due to the additional step of connecting

We estimate the following equation:

$$y_{i,1920} = \beta \cdot \mathbb{1}\text{Minimum wage}_{js(1910)} + \delta_{c(1910)} + \gamma_{j(1910)} + \eta X_i + \varepsilon_{ict}, \quad (5)$$

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 (5), we set up a model of the impact of minimum wage legislation, as measured by β, on outcomes for individual i in 1920, conditional on county of residence c in 1910 ($\delta_{c(1910)}$), industry of employment j in 1910 ($\gamma_{j(1910)}$), and individual controls (X_i) for birthplace, age-bins, literacy, and race. The variable $\mathbb{1}(\text{Minimum wage})_{js(1910)}$ equals 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.

6.2 Estimates from the Linked Sample

Table 7 shows the results of estimating Equation (5).[67] Column I indicates that the minimum wage legislation correlates with a drop in the probability of remaining employed in the same industry in 1920 by over 4 percentage points. Column II shows that this decline is partly attributed to a reduction in labor force participation by 3.2 percentage points. Column III reveals that, after conditioning on labor force participation, the likelihood of being employed in the same industry drops by almost 6 percentage points. Since the share of women in the sample who were in the labor force in 1920 is about 40%, the contribution of the latter channel to the overall reduction in the probability of being employed 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.

In Panel B, we again estimate columns I–III with an interaction that allows us to distinguish the legislation's impact for married and never-married women. Column I shows that the overall effect is equally driven by both groups, while columns II and III indicate that the drop in labor force participation is exclusively driven by married women.[68]

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, with marital status playing a key role in differentiating these two channels. In particular, it appears that while both married and unmarried women are driving the overall result in column I, the decline in labor force participation is predominantly attributed to married women. We interpret this as the result of two potential channels working in the same direction, which is in line with the findings of Goldin (1988*a*). First, on the supply side, some married women may have seen their work as temporary, so a decrease in the demand for their demand could have merely expedited their departure from the labor force. Second, on the demand side, Goldin (1988*a*) explains that employers

FamilySearch data with the census. Similarly, Withrow (2021) constructs a linked census of women using FamilySearch to study rural-to-urban migration in the 1920–1940s.

[67] Appendix Table D.14 reports the coefficients from an analogous estimation using different right-hand-side variables.

[68] These results are robust to alternative ways of matching individuals. Tables D.15 and D.16 show that our results hold when we use more lenient or stricter restrictions on race, age, and first and last name spelling similarities when matching individuals across censuses.

were reluctant to retain some married women who, potentially due to a higher reservation wage, were less "docile" or "less willing to remain in the same dead-end positions all were assigned."

Now that we have painted a comprehensive picture of the impact of gender-specific minimum wage on women, we will go back to the industry-specific setting and the substitution between genders.

Table 7: The Impact 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 the results from estimating Equation (5). Each observation is an individual, and each regression includes county, state, birthplace, individual's industry in 1910, and age-bin fixed effects. Dummies for literacy in 1910 and race are used as controls. Here we use the sample of always- and never-married women who were between 16 and 65 years old in 1920. See details on the linked sample construction in Appendix B. Results for Minimum wage in dollars$_{ist}$ are reported in Table D.14. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

7 The Impact of Minimum Wage Legislation on Men's Employment and the Gender Elasticity of Substitution

So far, we have focused on how the minimum wage laws impacted women. This section shifts the focus to the legislation's impact on male labor demand. Additionally, we explore the broader implications of gender substitution in shaping the effects of these laws on labor markets.

7.1 Impact on Men's Employment

We estimate Equation (2) for men and find that the minimum wage legislation increased male labor demand by as much as 1.2% (columns I and II of Table 8).[69] This result corresponds to an effect at the county-industry level and, taken together with the results presented in Table 4, suggests a substitution between genders, on average.[70]

We also find similar results when estimating the laws' effect on men's aggregate employment. Table D.18 replicates Table 6 but with log employment share of men as the dependent variable. We find an increase in men's aggregate employment in column I (though smaller in magnitude than for women and insignificant), large employment gains in areas where a larger share of women were employed in treated industries in 1910 in column II, and smaller employment gains in areas with higher local market concentration in column III.[71] Additionally, Table D.19 reports county-level results for the full sample of counties.

Table 8: The Effect of Minimum Wage Legislation on Men's Employment

	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***	0.029	0.034**	-0.061**	-0.054**
(mean min. wage \$10.2)	(0.001)	(0.0014)	(0.018)	(0.015)	(0.025)	(0.0233)
R-squared	0.654	0.716	0.823	0.833	0.814	0.824
Observations	801,897	801,897	129,401	129,401	63,877	63,877
Panel B:						
1(Minimum wage)	0.013***	0.006***	0.030***	0.035***	-0.056	-0.060*
	(0.001)	(0.0010)	(0.007)	(0.001)	(0.034)	(0.032)
R-squared	0.654	0.716	0.823	0.833	0.814	0.824
Observations	801,897	801,897	129,401	129,401	63,877	63,877
County-pair-year FEs	✓	✓	✓	✓	✓	✓
Ind.-county-pair & occup.-county-pair FEs.		✓		✓		✓

Notes: This table reports the results from estimating Equation (2) for men (adults and minors) and female minors for the two specifications with the most fixed effects (the equivalent of columns V and VI in Table 4). 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

[69] 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 the legislation targeted female minors only in all minimum wage states except California and Minnesota. These results relate to the literature on sub-minimum wage (Neumark and Wascher, 1992) and contemporaneous discussions regarding age-specific minimum wages (Taylor, 2020). Our results also align with the recent findings of the effect of age-specific minimum wage on youth employment in Denmark (Kreiner, Reck and Skov, 2020).

[70] Using estimates for the aggregate effects from column II of Panel B, Tables 6 and D.18, we can estimate that women experienced a decrease in employment by around 36,000 in the 12 treated states and men by 108,000. Based on the aggregate effects shown in Table 6, we estimate that roughly one-third of the women from the linked sample left employment. Hence, an additional 54,000 women lost their jobs in the affected industries and switched to other jobs. The maximum net positive impact on employment would be approximately 72,000.

[71] To provide additional insights about the economic magnitude of the impact of minimum wage laws on male and female employment, we examine 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 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%, while male employment would have decreased even more by $(4.35\% + 1.2\%)$=5.5%

7.2 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 genders 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:

$$Y_i = AK_i^{\alpha_i} L_i^{1-\alpha_i}, \tag{6}$$

$$L_i = \left[(\theta_{w_i} W_i)^{\frac{\sigma_i - 1}{\sigma_i}} + (\theta_{m_i} M_i)^{\frac{\sigma_i - 1}{\sigma_i}} \right]^{\frac{\sigma_i}{\sigma_i - 1}}. \tag{7}$$

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 $\sigma_i < 1$. They are also 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:

$$\log\left(\frac{W_i}{M_i}\right) = (1 - \sigma_i) \log\left(\frac{\theta_{m_i}}{\theta_{w_i}}\right) - \sigma_i \log\left(\frac{\omega_{w_i}}{\omega_{m_i}}\right). \tag{8}$$

We initially treat the whole economy as only one industry, so we drop the subscript i. As previously mentioned, 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 and women. However, the main difficulty with estimating σ in Equation (8) 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 8 using the regression specification from column VI of Table 4 and $\log\left(\frac{W}{M}\right)$ as the dependent variable:

$$\log\left(\frac{\#EmployedWomen_{ic(s)t}}{\#EmployedMen_{ic(s)t}}\right) = \beta \cdot \mathbb{1}\text{Minimum wage}_{ist} + \mu_{st} + \Psi_{p(c)t} + \Phi_{is} + \Phi_{it} + \epsilon_{ip(c)t}. \tag{9}$$

The resulting estimate yields $\hat{\beta} = -0.046$ (see Table 9, column I). This result is consistent with our findings in Tables 4 and 8, suggesting the presence of substitution of women and men in treated industries.

Table 9: 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)	-0.046**	-0.074**	-0.035**
	(0.017)	(0.032)	(0.014)
Δ		-0.039**	
s.e.		(0.018)	
R-squared	0.762	0.531	0.812
Observations	167,747	56,889	109,634

Notes: This table estimates the same specifications as Table 4 (column VI) except the dependent variable is defined as $\log\left(\frac{\#EmployedWomen_{ic(s)t}}{\#EmployedMen_{ic(s)t}}\right)$. It reports the baseline results from estimating Equation (2). The number of observations is smaller than the number of women or men separately in the CBCP sample in Table 4 because not all observations (defined at the county, industry-occupation, and decade levels) had both employed men and employed women. The 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 by 6.8 percentage points, on average. We also expect wages for men to grow given the increase in demand. While we do not observe male wages, in Appendix Table D.21, 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 sub-groups 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), implying that $\hat{\sigma} \in \left[\frac{-0.046}{0.068-0.023}, \frac{-0.046}{0.068-0.061}\right] = [1.02, 6.57]$.

Second, we examine 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% for those in printing (row 8), which implies an elasticity of substitution of $\hat{\sigma} \in \left[\frac{-0.046}{0.068-0.040}, \frac{-0.046}{0.068-0.060}\right] = [1.64, 5.75]$. 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 yield $\sigma > 1$, suggesting that female and male laborers are gross substitutes. These findings are remarkably similar to those in Acemoğlu, Autor and Lyle (2004), where the authors estimate the elasticity of substitution between genders using a surge in female employment in the United States during WWII to be $\hat{\sigma}_{AAL2004} \in [1.64, 5.08]$.[72]

We further explore the extent to which the elasticity of substitution between genders is related to the share of women in a given industry. This analysis is presented in columns II and III of Table 9, where we estimate $\hat{\beta}$ for two sets of industries. In particular, we re-estimate the same model as in column I but computed on a subset of industries where the female share ranges from 25% to 75% (in column II) and on

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

a subset of industries with either a low (< 25%) or high (> 75%) share of women (column III). We find that the estimated coefficient for less gender-segregated industries is twice as large as that 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 gender 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, complements, or both. The highest degree of substitutability is observed in non-segregated industries.

7.3 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 pre-treatment occupational ranking. Our aim here is to see whether, by relaxing the assumption of homogeneous labor that we imposed in the previous section, the types of jobs lost by women due to the legislation are comparable to those acquired by men for the same reason.

To do this, we estimate Equation (2) but add an interaction with a measure of occupational ranking. In Panel A of Appendix Table D.20, 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). 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 genders, 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.[73] Our results also support contemporaneous sources noting that the most-affected women were predominantly the lowest skilled or the most inexperienced (Stecker, 1927, p. 140).[74] At the same time, the employment of skilled women was not affected to the same degree (Stecker, 1927, pp. 174–184).[75]

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, can substitute away from minimum-wage-covered workers.

[73] This might be due to mechanization or to a different organization of the production function that increased the ratio of higher-skilled over lower-skilled occupations.

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

[75] Because Sundstrom (1988) presents evidence that in the 19th century, U.S. firms relied on internal promotion to fill skilled positions—suggesting 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, in Appendix C we provide suggestive evidence on the long-run effect of sex-specific minimum wage laws on female labor force participation.

7.4 Qualitative Evidence from New York and Ohio

We turn to qualitative empirical evidence confirming some of the results presented earlier. Specifically, we examine evidence from Ohio and New York, which enacted minimum wage laws in the laundry industry in 1933 and 1935, respectively. While our sample does not include these two states due to limitations of the decadal census, which prevent us from isolating the impact of these laws from the impact of the 1938 minimum wage provision in the FLSA, we have access to valuable qualitative empirical evidence through interview data. These data were made available by the work conducted by the U.S. Department of Labor (1938).[76]

In Ohio, a survey by the U.S. Department of Labor (1938) estimated that 2.5% of the women employed before the enactment had lost their jobs due to the legislation. Of these women, some remained unemployed in the long term, some gained employment in another industry, and some managed to be hired back in the same industry. In New York, 1 out of 10 surveyed firms stated that they had implemented some changes to their shift from women to men due to the minimum wage law. In particular, some managers found paying men a higher rate more profitable.

The survey also found that in New York, following the legislation's enactment, there was not only gender substitution within the same occupations but also a disproportionate increase in male hires. This trend was due to the introduction of new machinery that largely (i) replaced the work done by women and (ii) required operation by men. While this can be specific to the laundry industry, this evidence is qualitatively consistent with the results presented in this paper.

8 Welfare and Unconditional Earning Effect

In this paper we largely focus on the measurable effects of gender-specific minimum wage legislation. In this short section, we use the estimates described above to provide a back-of-the-envelope quantitative estimate of the earnings effect unconditional on employment. With the caveat of a partial equilibrium framework, this calculated figure aids in understanding the broader impact of the policies that we study.

First, using the estimates of the employment effect, we identify the minimum average wage increase among women affected by the policy such that the total earning effect (i.e., after accounting for the negative impact on employment) would be positive. In our sample, the employment rate of women is 12%, and 7 out of 10 employed women in 1910 worked in county-industry combinations affected by the policy. From the analysis of the linked data shown in Table 7, we also know that out of 3 women no longer employed in affected industries, 1 leaves the labor force and 2 switch industries (likely moving to unaffected county-industry combinations). We use the estimate of a negative employment effect of 3%, roughly equivalent to 0.3 percentage points of all women. Based on this estimate and the linked analysis, we deduce that roughly 0.1 percentage points of women exit the labor force entirely, reducing their wages to 0, while 0.2 percentage

[76] See Logan (2015) for an example of valuable qualitative empirical evidence used in economic history.

points switch industries. Considering that this law impacts 7 out of 100 women (12% employment $\times$ 70% affected), we make the following assumptions: women who switch industries maintain their pre-minimum wage earnings, 0.1% lose their wages completely, 0.2% keep the same earnings, and the rest experience wage increases up to the new minimum wage level. Assuming uniform effects across this group, and taken as one homogeneous group, the affected women will be *on average* better off as long as the average earning impact is 1.4% (0.1% / 7% = 1.4%).[77]

Second, we compare the minimum earnings increase to our estimates of the average earnings increase to establish that the policies studied in this paper had, in fact, increased unconditional earnings. When using the estimates from the Oregon case study, the total earning effect unconditional on employment equals $\frac{0.1}{7} \times (-100\%) + \frac{(7-0.3)}{7} \times 6.8\% = 5.1\%$.

9 Discussion and Conclusion

[Include a brief restatement of the paper's objectives.] We present three main findings. First, pre-FLSA minimum wage legislation effectively increased wages for treated women with lower pre-minimum wage earnings. Second, this legislation led to a decline in female employment in treated industry-county cells, coupled with a marginal increase in male employment. In particular, the gender-specificity of these laws highlights that men and women were, on average, considered gross substitutes in the American labor market at the beginning of the 20th century. Third, while net female employment at the county level decreased, the aggregate impact depends on the degree of pre-legislation cross-industry concentration in employment, resulting in implied employment elasticities with respect to wages that range from –1.6 (in low concentration areas) to 0.8 (in high concentration areas).

Further analysis provides suggestive evidence that prolonged exposure to minimum wage laws discouraged women from entering the labor force. Using the full count 1940 Census—the first census collected after a federal minimum wage law was implemented—we find in Appendix C that the number of years a state maintained an active gender-specific minimum wage decree is negatively correlated with the level of female labor force participation in 1940. In contrast, no similar long-run correlation is observed concerning men's labor force participation. Hence, gender-specific minimum-wage laws not only adversely affected female employment but may have also discouraged women from working, even when the minimum wage equalized across genders. This result is in line with that of Sorkin (2015) and Harasztosi and Lindner (2019), who find a long-run effect of the minimum wage on employment elasticities.

In summary, our findings indicate that while minimum wages negatively impacted employment levels, the legislation had a positive impact on earnings conditional on employment (likely resulting in a positive net impact) and may have had non-monetary positive effects as well.[78] Grounded in historical data, our estimated parameters can be used in a comparative perspective to inform contemporary policy debates, such

[77] Note that if the 0.2% of women switching industries took a pay cut of 50%, the average earning increase for affected women who stay employed that is required to affect the negative employment effect would be 1.4% + 0.2/7 $\times$ 50% = 2.8%.

[78] For example, Dow et al. (2020) show that an increase in minimum wages has reduced suicide among low-wage workers.

as the Raise the Wage Act of 2019 (HR 582), which proposes a gradual increase in the federal minimum wage gradually to $15 by 2024. Research by Godøy and Reich (2019) suggest that such an increase would have no negative effect on employment, even in states with historically low wages like Alabama and Mississippi, where the relative minimum wage would rise to 0.77 and 0.85, respectively.

This paper lays the groundwork for future research. Expanding our analysis to include Canadian census data is a promising next step, given Canada's implementation of gender-specific minimum wage laws in the early 20th century (Department of Labor, 1925*a*; Russell, 1991). Moreover, collecting additional wage data from states beyond Oregon could allow a deeper investigation of the earnings effects of these laws. The ultimate goal is to deepen our understanding of the political economy surrounding the introduction of minimum wages in a more institutionally simple environment. Finally, our institutional context, coupled with the analysis of linked census data, opens avenues to explore how minimum wages impact family and educational decisions, as well as the differential employment effects on minority women.

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Online Appendix

to

The Economics of Gender-Specific Minimum Wage Legislation

A Additional Background Information

A.1 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[79], present in the Fifth and the Fourteenth Amendments to the U.S. Constitution.[80] 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*.[81]

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).[82]

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.[83] 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."[84] 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.

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

[80] 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.

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

[82] 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."

[83] 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.

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

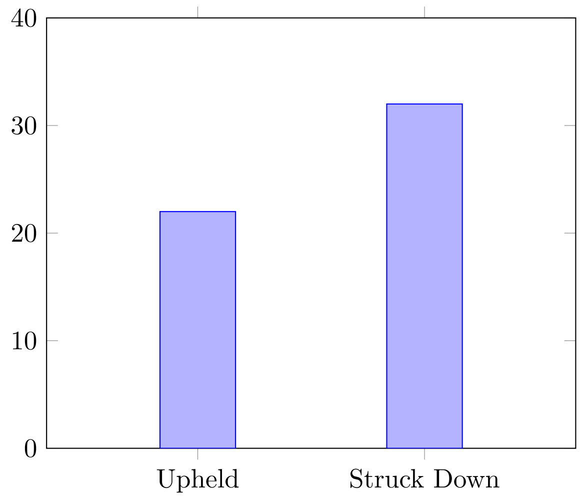

Figure A.1: Decisions in state cases involving general protective labor legislation (excluding cases involving children), 1873–1937. (Novkov, 2001, Table 4, p.31)

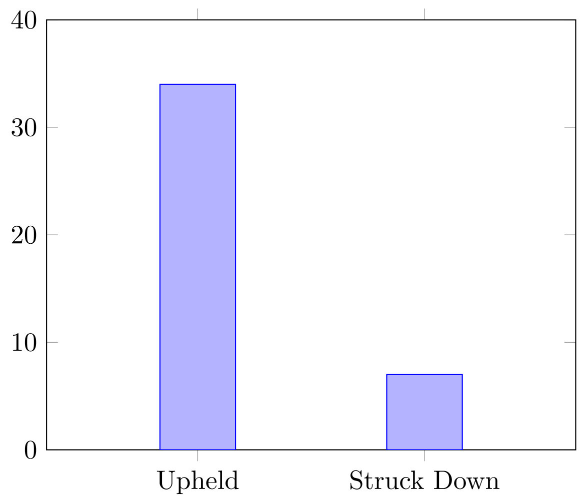

Figure A.2: Decisions in state cases involving protective labor legislation limited to women (excluding cases involving children), 1873–1937. (Novkov, 2001, Table 4, p.31)

A.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 E.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."[85] 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."[86] Among those who supported the minimum wage, Wolman (1924) highlights the need to support nonunionized workers in a position of weak bargaining power.[87]

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."

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).

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

B Construction of the Linked Sample

We construct a linked sample of women using individual data from Census waves in 1910 and 1920. To do this we use Restricted Full Count Census data from 1910 and 1920 available on the NBER server, which includes full strings or recorded names and last names. 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. (2021) shows that coefficient estimates (in analyses of intergenerational mobility) are similar when using linked samples based on various automated methods. To address the possibility that different restrictions may affect the results (see, e.g., Bailey et al., 2020), we run the specifications using samples in which we (1) we do not restrict for race code, (2) we restrict to age errors within plus or minus 1 year, (3) we impose stricter restrictions for string matching (i.e., Jaro-Winkler score to be 0.95 or 0.99 or higher), and we find quantitatively and qualitatively consistent results. Results are at Tables D.15 and D.16.

C 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:

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

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.[88] 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 C.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 C.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

[88] 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 C.2, we reestimate equation (10) 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 C.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 (10) from Table C.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

D Tables

Table D.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 D.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 D.2: Court Actions in Cases of Violation of Minimum Wage Laws, by State

#	State	Fine		Imprisonment		Fine and	Notes
		Minimum	Maximum	Minimum	Maximum	imprisonment	
1	Arizona	$50	$100	10 days	60 days	May be both	Each offense adds up cumulatively
2	Arkansas	$25	$100	-	-	-	Each day of non-complience is a separate offense
3	California	$50	-	30 days	-	May be both	
4	District of Columbia	$25	$100	-	3 months	May be both	
5	Kansas	$25	$100	-	-	-	Each offense adds up cumulatively
6	Massachusetts	$5	$50	-	-	-	Each offense adds up cumulatively
7	Minnesota	$10	$50	10 days	60 days	-	
8	North Dakota	$25	$100	10 days	3 months	May be both	
9	Oregon	$25	$100	10 days	3 months	May be both	
10	Utah	*	*			-	Fine was individually determined by local prosecuting officer
11	Washington	$25	$100	-	-	-	
12	Wisconsin	$10	$100	-	-	-	Each day of non-complience is a separate offense

Notes: *Source:* Department of Labor (1928).

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

	I	II
	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)
R-squared	0.797	0.797
Observations	273,883	273,883

Notes: This table estimates the same specifications (the most conservative, in column VI) as the baseline Table 4 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 D.5: Effect of Minimum-Wage Legislation on Employment of Women (No Missing Occupations)

~Baseline, no missing non. occupational	I	II	III	IV
	Dependent variable: Log employment share (women)			
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)
R-squared	0.751	0.795	0.751	0.795
Observations	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 4 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 D.6: Effect of Minimum-Wage Legislation on Employment of Women (Alternative Fixed Effects)

	I	II	III	IV	V	VI
	Dependent variable: Log employment share (women)					
non-occupational industries	without		with		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)
Ind.-occup.-year FEs.	✓	✓	✓	✓	✓	✓
Ind.-occup.-state FEs.		✓		✓	✓	✓
R-squared	0.902	0.916	0.910	0.923	0.916	0.923
Observations	273,883	273,883	322,740	322,740	273,883	322,740

Notes: This table estimates the most conservative specification (column VI) from the baseline Table 4 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 D.7: Effect of Minimum-Wage Legislation on Employment, by Gender, with Maximum-Working-Hours Controls

	I	II	III	IV
	Dependent variable: Log employment share			
	Women		Men	
Minimum wage, \$10	-0.024**		0.016***	
(mean min wage \$10.2)	(0.010)		(0.004)	
1(Minimum wage)		-0.044***		0.014**
		(0.008)		(0.006)
1(Max. working hours law)	0.005	0.004	0.012	0.012
x 1(State ever had minimum wage)	(0.013)	(0.013)	(0.008)	(0.009)
R-squared	0.797	0.797	0.751	0.751
Observations	272,397	272,397	801,903	801,903

Notes: This table estimates the baseline specification from column VI of Table 4 for women and column II of Table 8 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 D.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)					
Sample of women						
Panel A:						
Minimum wage, $10	-0.056	0.006***	0.010	-0.042	0.015***	0.021
(mean min. wage $10.2)	(0.036)	(0.001)	(0.015)	(0.036)	(0.0010)	(0.015)
R-squared	0.759	0.779	0.846	0.765	0.784	0.850
Observations	93,947	93,947	93,947	93,947	93,947	93,947
Panel B:						
1(Minimum wage)	-0.060	-0.048	0.004	-0.053	0.000	0.012
	(0.037)	(0.039)	(0.027)	(0.038)	(0.0020)	(0.029)
R-squared	0.759	0.779	0.846	0.765	0.784	0.850
Observations	93,947	93,947	93,947	93,947	93,947	93,947
	Dependent variable: Log employment share (men)					
Sample of men						
Panel C:						
Minimum wage, $10	-0.022	-0.022	-0.021	-0.019	-0.015	-0.015
(mean min. wage $10.2)	(0.016)	(0.029)	(0.023)	(0.015)	(0.0252)	(0.019)
R-squared	0.630	0.650	0.729	0.635	0.656	0.733
Observations	335,623	335,623	335,623	335,623	335,623	335,623
Panel D:						
1(Minimum wage)	-0.029*	-0.037	-0.037	-0.030*	-0.031	-0.035
	(0.016)	(0.035)	(0.027)	(0.016)	(0.0307)	(0.022)
R-squared	0.630	0.650	0.729	0.635	0.656	0.733
Observations	335,623	335,623	335,623	335,623	335,623	335,623
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 estimates all specifications from Table 4 for women (Panels A and B) and for men (Panels C and D) 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. The dependent variable in Panels A and B is log employment share of women. The dependent variable in Panels C and D is log employment share of men. 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 D.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 6 but with additional controls for the log of WWI veterans (computed using the 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 II but WWI veterans 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 D.10: Effect of Minimum-Wage Legislation on Employment of Subsample

Panel A~w/o 1880	I	II	III	IV
	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)
R-squared	0.737	0.737	0.649	0.649
Observations	259,164	259,164	731,331	731,331
Panel B~w/o 1930	I	II	III	IV
	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)
R-squared	0.756	0.756	0.649	0.649
Observations	168,736	168,736	531,778	531,778
Panel C~w/o 1880 and 1930	I	II	III	IV
	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)
R-squared	0.753	0.753	0.642	0.642
Observations	155,513	155,513	461,189	461,189

Notes: This table estimates the baseline specification from column VI of Table 4 for women and column II of Table 8 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 D.11: Robustness for Table 5: Results With State-Specific Linear Time Trends

	I	II	III
	Dependent variable: Log employment share (women)		
Panel A: ~Full sample			
Average minimum wage, $	-0.002	0.024	-0.106***
(mean av. min. wage $6)	(0.010)	(0.021)	(0.030)
Average minimum wage, $		-0.028	
x Share women in treated industries in 1910		(0.020)	
Average minimum wage, $			0.176***
x HHI in 1910			(0.050)
Mean of the interacted variable	-	0.69	0.58
R-squared	0.787	0.787	0.788
Observations	14,135	14,135	14,135
Panel B: ~Full sample w dummy			
1(Minimum wage)	0.020	0.122**	-0.465***
	(0.049)	(0.051)	(0.094)
1(Minimum wage)		-0.164***	
x Share women in treated industries in 1910		(0.017)	
1(Minimum wage)			0.801***
x HHI in 1910			(0.157)
Mean of the interacted variable	-	0.69	0.58
R-squared	0.787	0.787	0.788
Observations	14,135	14,135	14,135

Notes: This table reports the results from estimating equation (3). Each observation is a gender-specific county-decade. Each regression includes county, 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, clustered at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.12: Robustness for Table 6: Results With State-Specific Linear Time Trends

	I	II	III
	Dependent variable: Log employment share (women)		
Panel A: ~CBCP sample			
Average minimum wage, $	-0.017**	0.015	-0.125***
(mean av. min. wage $6)	(0.006)	(0.017)	(0.044)
Average minimum wage, $		-0.035**	
x Share women in treated industries in 1910		(0.017)	
Average minimum wage, $			0.184**
x HHI in 1910			(0.071)
Mean of the interacted variable	-	0.71	0.59
Difference w full sample specification, p-value	0.18	0.74	0.71
~difference for the interaction, p-value		0.78	0.92
R-squared	0.797	0.798	0.800
Observations	3,020	3,020	3,020
Panel B: ~CBCP sample w dummy			
1(Minimum wage)	-0.019	0.132*	-0.376**
	(0.038)	(0.066)	(0.153)
1(Minimum wage)		-0.266***	
x Share women in treated industries in 1910		(0.077)	
1(Minimum wage)			0.606**
x HHI in 1910			(0.225)
Mean of the interacted variable	-	0.71	0.59
Difference w full sample specification, p-value	0.51	0.78	0.62
~difference for the interaction, p-value		0.33	0.48
R-squared	0.797	0.798	0.800
Observations	3,020	3,020	3,020

Notes: This table reports the results from estimating equation (4). 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 D.13: The Effect of Minimum-Wage Legislation on Aggregate Employment of Women at the County Level (Elasticities)

	I	II	III
	Dependent variable: Log employment share (women)		
~ elasticity			
log (Minimum wage)	-0.033**	0.059*	-0.291***
	(0.014)	(0.034)	(0.088)
log (Minimum wage) x Share women in treated industries in 1910		-0.108***	
		(0.032)	
log (Minimum wage) x HHI in 1910			0.437***
			(0.142)
Mean of the interacted variable	-	0.71	0.59
R-squared	0.797	0.798	0.800
Observations	3,020	3,020	3,020

Notes: This table reports the results from the estimating equation (4) with ihs of minimum wage as the explanatory variable. 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 D.14: 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:			
Minimum wage, \$10 (mean min. wage \$10.2)	-0.041**	-0.028	-0.064**
	(0.019)	(0.018)	(0.026)
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
FEs: County in 1910	✓	✓	✓
FEs: Industry in 1910	✓	✓	✓
Individual controls	✓	✓	✓

Notes: This table presents results of the estimation of (5) 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 B. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.15: Robustness for Panel A of Table 7: Alternative Matching Restrictions

	I	II	III
	Dependent variable:		
	1(Same industry)	1(LFP)	1(Same industry)
Sample:	All	All	In the LF
Panel A: ~baseline			
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: baseline + w/o same race			
1(Minimum wage)	-0.041**	-0.034*	-0.054**
	(0.019)	(0.019)	(0.025)
Sample size change relative to baseline	16%	16%	9%
R-squared	0.160	0.279	0.301
Observations	63,916	63,916	24,115
Panel C: baseline + age error 1 year			
1(Minimum wage)	-0.048**	-0.041**	-0.058**
	(0.021)	(0.019)	(0.025)
Sample size change relative to baseline	-26%	-26%	-27%
R-squared	0.194	0.302	0.331
Observations	41,088	41,088	16,217
Panel D: baseline + Jaro Winkler score >0.95			
1(Minimum wage)	-0.039*	-0.026	-0.054
	(0.024)	(0.022)	(0.033)
Sample size change relative to baseline	-39%	-39%	-38%
R-squared	0.205	0.304	0.342
Observations	33,490	33,490	13,641
Panel E: baseline + age error 1 year + Jaro Winkler score >0.95			
1(Minimum wage)	-0.045*	-0.035	-0.056
	(0.026)	(0.021)	(0.034)
Sample size change relative to baseline	-49%	-49%	-50%
R-squared	0.207	0.312	0.346
Observations	28,203	28,203	10,932
Panel F: baseline + Jaro Winkler score >0.99			
1(Minimum wage)	-0.043*	-0.035	-0.056*
	(0.024)	(0.024)	(0.033)
Sample size change relative to baseline	-39%	-39%	-41%
R-squared	0.197	0.299	0.339
Observations	33,537	33,537	13,097
FEs: County in 1910	✓	✓	✓
FEs: Industry in 1910	✓	✓	✓
Individual controls	✓	✓	✓

Notes: This table replicates Panel A of Table 7 but uses alternative restrictions during the census linkage. Panel A shows the baseline results from Panel A of Table 7 for reference. Panel B relaxes the condition that the linked individual has to name the same race code in both censuses. Panel C only allows +/- 1-year error in age in comparison to +/- 2 years in the baseline. Panel D set a higher cut-off for for Jaro-Winkler score — $\geq$ 0.95 for both first and last names instead of $\geq$ 0.90 in the baseline. Panel E combines both +/- 1-year error in age and Jaro-Winkler score $\geq$ 0.95 in addition to the baseline. Panel F set a higher cut-off for Jaro-Winkler score — $\geq$ 0.99 for both first and last names instead of $\geq$ 0.90 in the baseline. 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 B. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.16: Robustness for Panel B of Table 7: Alternative Matching Restrictions

	I	II	III
	Dependent variable:		
	1(Same industry)	1(LFP)	1(Same industry)
Sample:	All	All	In the LF
Panel A: ~baseline			
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
Panel B: baseline + w/o same race			
1(Minimum wage) x Married	-0.031*	-0.056***	-0.045
	(0.016)	(0.020)	(0.059)
1(Minimum wage) x Never married	-0.032*	-0.002	-0.055**
	(0.020)	(0.014)	(0.025)
Sample size change relative to baseline	16%	16%	9%
R-squared	0.197	0.398	0.301
Observations	63,916	63,916	24,115
Panel C: baseline + age error 1 year			
1(Minimum wage) x Married	-0.034*	-0.048**	-0.074
	(0.018)	(0.020)	(0.057)
1(Minimum wage) x Never married	-0.040*	-0.011	-0.057**
	(0.021)	(0.015)	(0.024)
Sample size change relative to baseline	-26%	-26%	-27%
R-squared	0.231	0.429	0.331
Observations	41,088	41,088	16,217
Panel D: baseline + Jaro Winkler score >0.95			
1(Minimum wage) x Married	-0.033	-0.050**	-0.048
	(0.022)	(0.021)	(0.075)
1(Minimum wage) x Never married	-0.032	0.001	-0.054*
	(0.021)	(0.015)	(0.032)
Sample size change relative to baseline	-39%	-39%	-38%
R-squared	0.240	0.423	0.342
Observations	33,490	33,490	13,641
Panel E: baseline + age error 1 year + Jaro Winkler score >0.95			
1(Minimum wage) x Married	-0.038*	-0.055***	-0.06
	(0.023)	(0.019)	(0.082)
1(Minimum wage) x Never married	-0.034	-0.001	-0.055*
	(0.025)	(0.015)	(0.031)
Sample size change relative to baseline	-49%	-49%	-50%
R-squared	0.243	0.427	0.346
Observations	28,203	28,203	10,932
Panel F: baseline + Jaro Winkler score >0.99			
1(Minimum wage) x Married	-0.045*	-0.072***	-0.068
	(0.025)	(0.020)	(0.088)
1(Minimum wage) x Never married	-0.032	-0.003	-0.055*
	(0.022)	(0.017)	(0.031)
Sample size change relative to baseline	-39%	-39%	-41%
R-squared	0.232	0.411	0.339
Observations	33,537	33,537	13,097
FEs: County in 1910	✓	✓	✓
FEs: Industry in 1910	✓	✓	✓
Individual controls	✓	✓	✓

Notes: This table replicates Panel B of Table 7 but uses alternative restrictions during the census linkage. Panel A shows the baseline results from Panel A of Table 7 for reference. Panel B relaxes the condition that the linked individual has to name the same race code in both censuses. Panel C only allows +/- 1-year error in the age in comparison to +/- 2 years in the baseline. Panel D set a higher cut-off for for Jaro-Winkler score — ≥ 0.95 for both first and last names instead of ≥ 0.90 in the baseline. Panel E combines both +/- 1-year error in age and Jaro-Winkler score ≥ 0.95 in addition to the baseline. Panel F set a higher cut-off for Jaro-Winkler score — ≥ 0.99 for both first and last names instead of ≥ 0.90 in the baseline. 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 B. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.17: The Effect of Minimum-Wage Legislation on Individual Migration Choices of 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
FEs: County in 1910	✓	✓	✓	✓
FEs: Industry in 1910	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓

Notes: This table presents results of the estimation of (5) but uses a different outcome variable — a dummy of whether a woman is residing in 1920 in the same state or county as she was in 1910. 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 B. Standard errors, double-clustered at the county-industry level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.18: The Effect of Minimum-Wage Legislation on Aggregate Employment of Men at the County Level (CBCP Sample)

	I	II	III
	Dependent variable: Log employment share (men)		
Panel A:			
Average minimum wage, $	0.001	-0.006	0.009***
(mean av. min. wage $6)	(0.004)	(0.006)	(0.003)
Average minimum wage, $		0.019**	
x Share women in treated industries in 1910		(0.007)	
Average minimum wage, $			-0.070***
x HHI in 1910			(0.021)
Mean of the interacted variable	-	0.71	0.28
R-squared	0.822	0.822	0.824
Observations	3,042	3,042	3,042
Panel B: ~w dummy			
1(Minimum wage)	-0.010	-0.049**	0.042*
	(0.027)	(0.023)	(0.023)
1(Minimum wage)		0.159***	
x Share women in treated industries in 1910		(0.056)	
1(Minimum wage)			-0.323***
x HHI in 1910			(0.073)
Mean of the interacted variable	-	0.71	0.28
R-squared	0.822	0.822	0.824
Observations	3,042	3,042	3,042
Panel C: ~ elasticity			
log (Minimum wage)	0.001	-0.010	0.020***
	(0.009)	(0.009)	(0.006)
log (Minimum wage)		0.033**	
x Share women in treated industries in 1910		(0.015)	
log (Minimum wage)			-0.147***
x HHI in 1910			(0.036)
Mean of the interacted variable	-	0.71	0.28
R-squared	0.822	0.822	0.824
Observations	3,042	3,042	3,042

Notes: This table reports the results from the estimating equation (4). 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 D.19: The Effect of Minimum-Wage Legislation on Aggregate Employment of Men at the County Level (Full Sample)

	I	II	III
	Dependent variable: Log employment share (men)		
Full Sample			
Panel A:			
Average minimum wage, $	0.003	-0.010***	0.008***
(mean av. min. wage $6)	(0.003)	(0.003)	(0.003)
Average minimum wage, $		0.032***	
x Share women in treated industries in 1910		(0.006)	
Average minimum wage, $			-0.041***
x HHI in 1910			(0.014)
Mean of the interacted variable	-	0.69	0.15
R-squared	0.694	0.695	0.695
Observations	14,213	14,213	14,213
Panel B: ~w dummy			
1(Minimum wage)	0.004	-0.018	0.035
	(0.023)	(0.013)	(0.032)
1(Minimum wage)		0.083	
x Share women in treated industries in 1910		(0.100)	
1(Minimum wage)			-0.210**
x HHI in 1910			(0.095)
Mean of the interacted variable	-	0.69	0.15
R-squared	0.694	0.695	0.695
Observations	14,213	14,213	14,213
Panel C: ~ elasticity			
log (Minimum wage)	0.005	-0.018***	0.018**
	(0.008)	(0.005)	(0.008)
log (Minimum wage)		0.063***	
x Share women in treated industries in 1910		(0.019)	
log (Minimum wage)			-0.104***
x HHI in 1910			(0.033)
Mean of the interacted variable	-	0.69	0.15
R-squared	0.694	0.695	0.695
Observations	14,213	14,213	14,213

Notes: This table replicates Table D.18 but uses the full sample of counties instead of the CBCP sample. Standard errors, clustered at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.20: The Effect of Minimum-Wage Legislation on Employment of Men and Women, across Occupational Income Score

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

Notes: This table estimates the same specifications as the baseline Table 4 (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 D.21: 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*).

E Figures

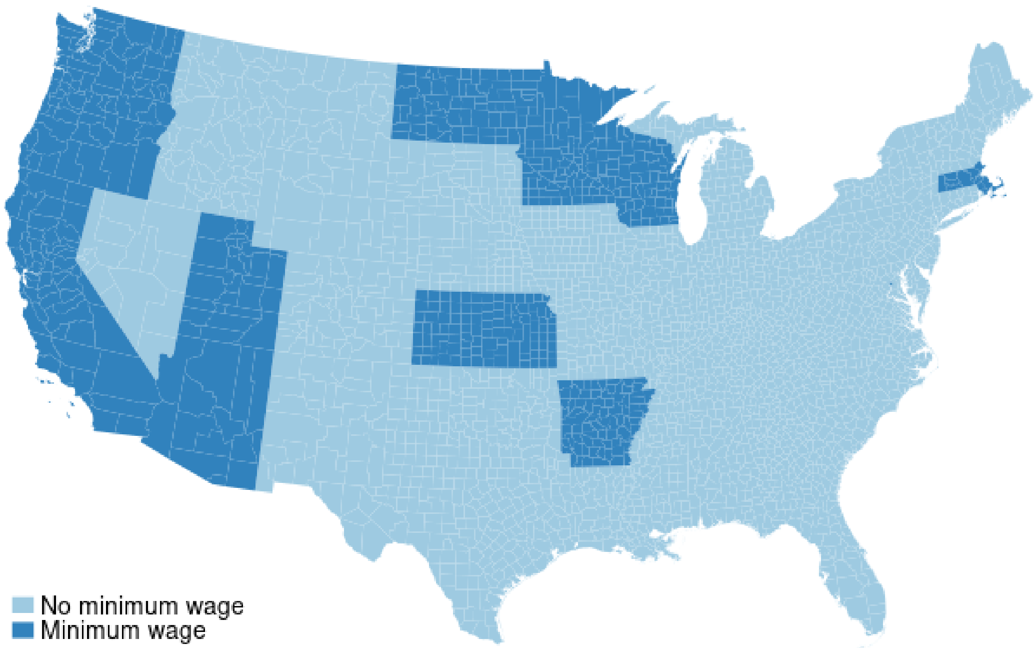

Figure E.1: States with female-specific minimum-wage laws. Dark blue indicates states with minimum-wage laws: Arizona, Arkansas, California, Kansas, Massachusetts, Minnesota, North Dakota, Oregon, Utah, Washington, Wisconsin, and the District of Columbia. Light blue indicates states that don't have minimum-wage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

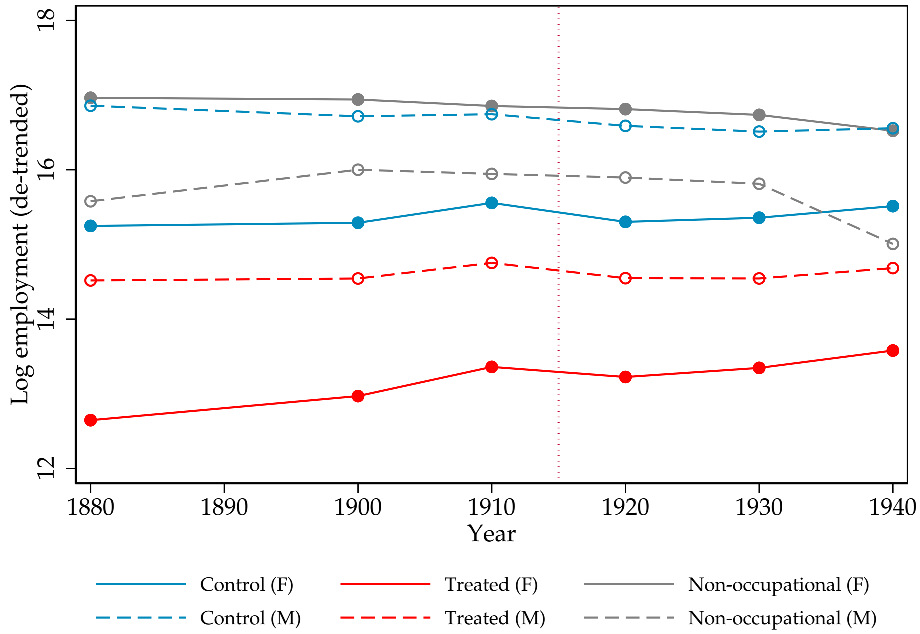

Figure E.2: De-Trended Log Employment.

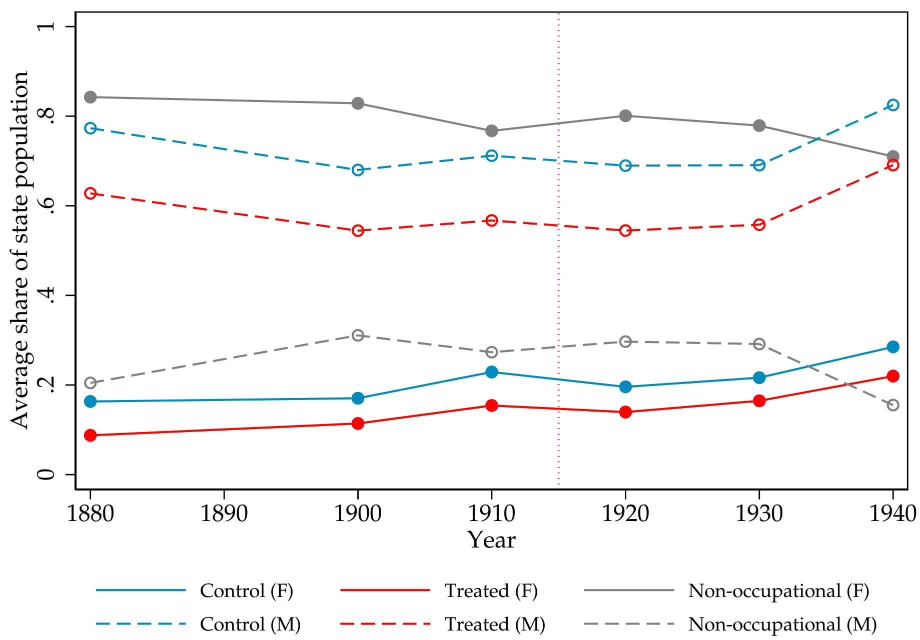

Figure E.3: Average Share of State's Adult Population.

Figure E.4: Fully dynamic difference-in-differences specification, estimated according to the equation (2), except that the treatment (dummy for minimum wage) 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. The figure shows results for our preferred specification in column VI of Table 4.

Figure E.5: State-Exclusion Robustness of the Results for $\mathbb{1}$ (Minimum wage)$_{ist}$ in Table 4

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 4, dropping one state at a time. One dropped state may imply dropping several state-border-segments (see Table 2). 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.

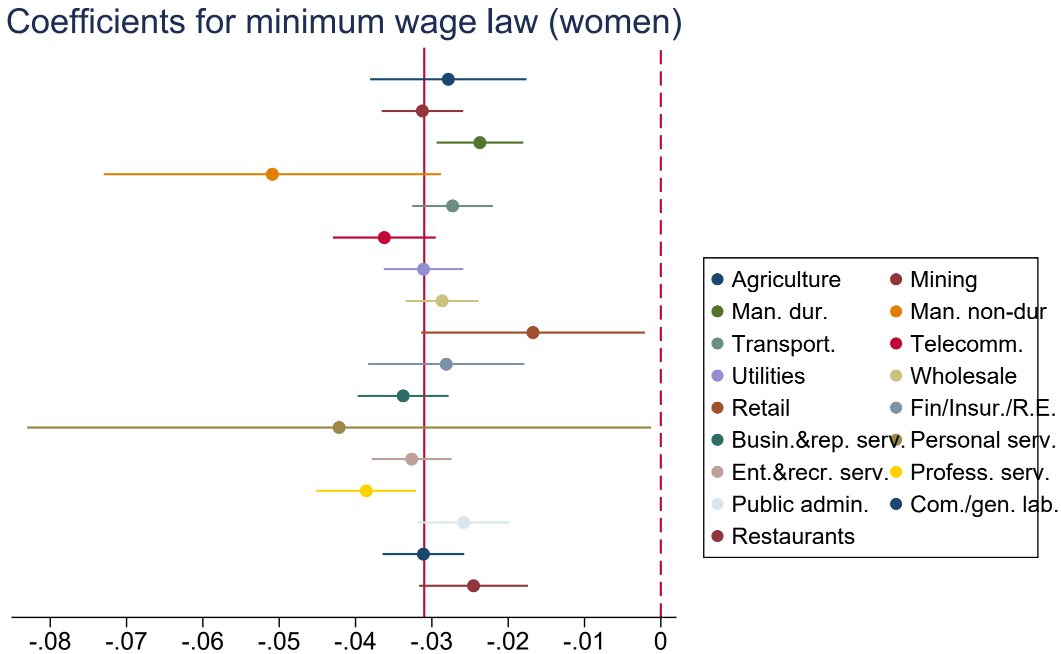

Figure E.6: Industry-Exclusion Robustness of the Results for $\mathbb{1}\,(\text{Minimum wage})_{ist}$ in Table 4

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 4, 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.