Assessing the Employment Effects of State-Level Minimum Wage Increases in U.S. Low-Wage Industries, 2010–2018

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Presented to the Department of Economics

in partial fulfillment of the requirements

for a Bachelor of Arts degree

May 2025

HAVERFORD COLLEGE

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Abstract

The federal minimum wage in the United States has remained static since its last change in 2009. Due to stagnation at the federal level, states have moved to implement state-level wage increases to keep up with rising living costs and labor market conditions. This thesis investigates the short-run labor market effects of these increases in the United States from 2010 to 2018, focusing on low-wage industries. Through a staggered difference-in-differences (DiD) framework, the analysis shows how minimum wage hikes of at least $0.50 affected the employment-population ratio, labor force participation rate, and unemployment rate. This study adds to the growing literature suggesting that minimum wage increases during the 2010s had limited negative effects on employment outcomes.

Acknowledgments

I'd like to begin by thanking Professor Ball, whose time, insight, and guidance were instrumental in the completion of my thesis. Also, his superior knowledge of the band Little Feat proved unexpectedly invaluable and will stick with me for foreseeable future.

Additionally, I'd like to thank my parents for 21 years of unwavering support and inspiring me to push harder and farther not only in my education, but in every aspect of my life.

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I. Introduction

This thesis is motivated by the argument surrounding increasing the minimum wage and its effect on employment. Those who support wage increases argue that they will increase earnings for low-wage workers without significantly affecting employment rates. However, opponents fear such policies will lead to job losses, especially in low-skilled sectors. The prior empirical research is divided because various industries, periods, and methodological approaches have produced conflicting findings on the topic. The research question for this study aims to look deeper into the effects of employment when low-wage industries raised their minimum wage from 2010 through 2018.

The question takes on greater importance after multiple years of federal inaction. The federal minimum wage has remained static since 2009, which has led states to take charge of wage-setting, producing a natural policy variation to study. Different states have implemented varying minimum wage policies through significant hikes or no changes, enabling research into regional and temporal variations. The decentralized policy environment provides ideal conditions to naturally study labor market effects in industries where minimum wage regulations become binding.

This thesis will examine three employment indicators: the employment-to-population ratio, labor force participation, and unemployment rate. The three metrics were chosen because they provide generally separate views of labor market responses to help understand whether employees lose their positions, choose to exit work, or experience extended unemployment after wage increases.

The final analysis data is built using panel data from the Current Population Survey (CPS) from 2010 to 2018, covering employed individuals in leisure and hospitality, food service,

and retail, the three sectors where minimum wage changes are most likely to affect significantly. This dataset is merged with state-level minimum wage data, unemployment rates from the BLS, and per capita GDP from the BEA to control for economic conditions across states and over time.

To analyze and estimate the causal effects of state minimum wage increases, this study applies a staggered difference-in-differences framework with event study variation to correct for bias in traditional models when treatment timing varies across units and provide a more precise identification of post-treatment effects. The treatment threshold for the analysis is defined as a minimum wage increase of $0.50 or more; a cutoff that was calculated through an analysis of wage adjustment frequencies suggests this level best captures material increases while excluding smaller jumps, for example, inflation-indexed changes.

II. Literature Review

Minimum wage is one of the most contested issues in labor economics, and as stated earlier, it stems from conflicting findings on the topic. According to supply and demand-based classical economic theory, when a wage floor is established above the market equilibrium, it lowers worker demand and increases labor costs (Stigler, 1946). This viewpoint was cemented as fact for a significant period and influenced popular economic theory while stoking early opposition to minimum wage legislation. Since the 1990s, however, many empirical studies have challenged this view, following Card and Krueger's (1994) seminal work. The studies described and discussed in this literature review help paint the picture of the minimum wage debate in the United States.

A. Minimum Wage and Employment Meta-Analyses

As the debate has continued researchers have increasingly turned to meta-analyses to interpret these distinct findings. Meta-analyses are thorough assessments that combine the findings of multiple individual studies into a larger conglomerate. Instead of gathering new data or conducting brand new experiments, these analyses analyze patterns in the literature to determine which conclusions are most reliable and under what circumstances specific effects are more likely to occur. Some notable contributions that will be discussed include Belman and Wolfson's (2014) review of over 200 studies and Neumark and Wascher's (2008) synthesis of more than 90, which help map out broader empirical trends, methodological shortcomings, and sectoral or geographic nuances. This section will examine several of these reviews, focusing on their data, techniques, and broader implications for interpreting the labor market consequences of minimum wage policies.

As noted above, Neumark and Wascher (2008) looked at more than 90 empirical studies in one of the most thorough analyses of the minimum wage literature to find the usual consequences of minimum wage hikes on employment. Their aim was not only to summarize results but also to evaluate the credibility of several different methodological approaches. Classification of studies by their empirical design was central to their analysis; they highlighted that experimental techniques, especially difference-in-differences (DiD) models, as well as longitudinal tracking and instrumental variable (IV) strategies, produced more reliable findings than simpler before-and-after or cross-sectional comparisons, which often failed to control for confounding factors like geography or sector.

The studies examined drew on several data sources in their analysis, with the Current Population Survey (CPS), the National Longitudinal Survey of Youth (NLSY), and state-level business establishment data being the most prevalent. Observation units varied across studies

from individual employees to firm- and state-level aggregates. Many studies concentrated on particularly vulnerable groups like teenagers and young adults or on low-wage sectors, which often take the biggest hit regarding minimum wage policy effects.

This section will review highlights from three of the studies they looked at to demonstrate the range of approaches and outcomes. First, Currie and Fallick (1996) used CPS panel data and a before-and-after design, discovering that raising the minimum wage decreased the retention of younger and less skilled workers. Next, in a longitudinal study spanning France and the U.S., Abowd, Kramarz, and Margolis (1999) examined entry and exit rates using matched employer-employee data. They discovered modest but consistent employment losses, particularly for new labor market entrants. Lastly, using a DiD approach with CPS and state-level data, Burkhauser, Couch, and Wittenburg (2000) reported considerable wage gains for low-wage workers and few job losses. These differences highlighted Neumark and Wascher's central idea that methodological design significantly influences reported results.

Their findings in terms of their central idea were that research using the most demanding statistical methods discovered that raising the minimum wage resulted in fewer jobs, particularly for vulnerable groups like younger or less experienced workers. They argued that increases in the minimum wage frequently had the effect of technological substitution, in which capital replaced labor, or substitution effects, in which employers shifted toward higher-skilled labor. They also found that the adverse effects were concentrated in labor-intensive industries with narrow profit margins, like retail and hospitality, and small or rural businesses that have little ability to absorb higher costs and price changes.

The review by Neumark and Wascher is vital because it addresses differences between subgroups. They argued that the effects of the minimum wage are inconsistent when you begin

breaking down the results by age, skill level, and industry. For example, rural businesses may be more constrained and more likely to reduce hours or postpone hiring new employees. In contrast, some urban companies may be able to offset higher costs through efficiency gains or modest price increases. By indicating that the very workers that minimum wage policies are intended to assist, young, inexperienced, or marginally engaged in the labor force, may be the ones who suffer the most when wage floors are raised, their findings ultimately supported a more cautious approach in terms pro-increase minimum wage policy.

On the other side of the meta-analyses' spectrum, Belman and Wolfson (2014) offer a more optimistic reading of the minimum wage literature than Neumark and Wascher's cautious approach. In another of the most thorough reviews to date, they combined more than 200 empirical studies to evaluate how firms adjust to wage floors through adjustments to operations, workforce management, and firm behavior, in addition to determining whether raising the minimum wage impacts employment. In contrast to meta-analyses that only consider employment counts, Belman and Wolfson focused on a broader range of outcomes, such as employee turnover, productivity, price adjustments, and the long-term dynamics of labor market adaptation.

Their review covered both cross-sectional and longitudinal studies from a methodological standpoint, but they gave weight to research designs that could monitor employee or company behavior over time. In the highlighted studies, large administrative datasets, including the Quarterly Census of Employment and Wages (QCEW), the Current Population Survey (CPS), and matched employer-employee records, were once again used for analysis. The credibility and design of these sources made it possible to conduct analyses at several levels of observation, such as industry-wide labor trends, firm-level hiring and exit patterns, and individual

employment trajectories. They could describe the direction and heterogeneity of minimum wage effects by comparing results across different settings, such as urban versus rural areas, other periods, and sectors with varying labor intensity.

Based on their findings, Belman and Wolfson argued that while increasing the minimum wage might result in job losses, especially for the youngest or least skilled workers, these impacts are usually minor and frequently outweighed by employer adaptations. They further discussed how reducing employee departures is an essential mechanism because higher wages create satisfied employees who maintain high morale and eliminate the need for new employee recruitment and training. Going even, their study showed that the benefits of this approach become most significant in low-wage industries that experience frequent employee departures, and productivity improvement functions as a central adjustment channel.

Lastly, argued that small price increases are a typical and frequently successful tactic in customer-facing service industries. The higher labor costs can be transferred to customers without losing market share because demand in these sectors is relatively inelastic, especially in sectors like dining and hospitality.

Belman and Wolfson's study shifted the focus from merely cutting headcount to assessing how companies manage rising wage costs. Their findings main finding was that employment trade-offs should be balanced against gains in job quality, stability, and firm performance. Their review established a foundation for future studies that acknowledge the effects of the minimum wage as dynamic and multifaceted rather than consistent and instantaneous.

In the final meta-analysis discussed in this section John Schmitt (2013) reevaluated the minimum wage literature by pointing out methodological errors in studies that reported negative impacts on employment. His analysis was published by the Center for Economic and Policy

Research. He stressed that most studies do not find a significant negative impact on employment when appropriately rigorous econometric methods are used, such as fixed effects, difference-in-differences models, and controls for regional variation.

Like Belman and Wolfson, Schmitt pointed out that businesses usually adjust to wage increases by increasing efficiency, lowering turnover, and raising prices only slightly rather than eliminating jobs. Schmitt's main methodological critique is what makes his contribution important. He contested that many studies that find disemployment effects tend to overstate the risks associated with minimum wage policy and fail to account for confounding trends occurring at the time of the study. He also underlined that when derived from studies using robust designs, null results are significant and deserving of consideration in policy discussions.

Schmitt's work contributed to the growing consensus that when moderate and well-designed, minimum wage increases do not result in significant job losses. He stressed once again the necessity of a meticulous empirical approach, which forms the basis of a large portion of the more recent literature.

These three meta-analyses highlight how methodology and context greatly influence how raising the minimum wage affects employment. More rigorous designs, particularly those that control for regional trends, typically find null or modest effects versus studies that use basic comparisons and frequently report job losses. The targeted empirical studies discussed in the following section both ease and result from this tension, which has forced economists toward more sector-specific, localized, and causally credible analyses.

B. Industry and Region-Specific Studies

While the previous section of literature and studies combined general assessments of minimum wage policy results across several contexts and methodological approaches, this one

examines studies concentrating on sectors and geographic areas, especially those most sensitive to wage policy, such as fast food, hospitality, and retail. Looking deeper into industries defined by low average salaries and high labor intensity is particularly important for minimum wage studies. Furthermore, many popular studies in this category have relied on natural policy variation across state or municipal borders to help isolate causal. This specific topic of the minimum wage debate delivers precise data about the effects on low-wage labor markets, company responses, and employment statistics, which helps validate the methods used later in this thesis.

Starting off with possibly the most influential piece in all of labor economics, the paper by David Card and Alan Krueger (1994), *Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania*, established itself as a landmark study within the minimum wage literature. Their study presented an experimental approach to test the common economic belief that increasing the minimum wage will lead to reduced employment opportunities. The paper used a difference-in-differences (DiD) method to analyze fast-food employment shifts by comparing New Jersey to Pennsylvania since New Jersey increased its minimum wage, but Pennsylvania kept its existing rate.

To collect new data for the study, Card and Krueger conducted a telephone survey of 410 fast-food restaurants, made up of household names such as Burger King, Wendy's, KFC, and Roy Rogers.

The fast-food industry was selected because of its large reliance on low-wage labor, but also because of its operational standardization, which reduced confusing business-model variation. They collected data in two waves; the first occurred in February 1992, before the increase in the minimum wage in New Jersey, and the second occurred in November 1992,

following the implementation of the new wage floor in the state. This allowed Card and Kruger to track changes in employment over time by using this pre-post structure, highlighting firm adjustments made in direct response to policy changes.

To guarantee that labor input was comparable across establishments, regardless of the mix of full-time and part-time workers, Card and Krueger converted their employment data into full-time equivalent (FTE) positions. To determine whether companies were lowering hours rather than eliminating jobs by recording individual wage levels, hours worked, and headcount. They also asked restaurants to collect pricing data for their popular menu items, including burgers, fries, and soft drinks, to see if they had transferred their labor cost increases to customers through small price hikes.

When designing the analysis framework, seasonal variations, macroeconomic conditions, and more general industry trends that would impact both states were all considered. By differentiating the change in employment levels between states, the analysis was able to isolate the policy's effect with a high degree of validity.

Going against conventional economic belief, Card and Krueger found no evidence of job losses after the minimum wage hike in New Jersey. Their data actually indicated a slight but favorable increase in employment in New Jersey compared to Pennsylvania. According to their findings, fast-food companies could absorb higher labor costs without cutting staffing levels through a combination of small price increases passed onto customers and operational changes like improved efficiency or a different staffing arrangement. This directly contradicted the traditional theory and sparked several follow-up studies that reexamined the labor demand elasticity in low-wage industries.

There is no debate that the work of Card and Krueger significantly affected future research and policy. First, it questioned the belief that rising minimum pay is always bad for low-wage jobs. Second, it aided in the popularization of the DiD method as a useful and valid empirical tool in economics, particularly under clear treatment and control groups generated by policy changes.

Next, refining the geographic identification strategy used to measure minimum wage effects, Dube, Lester, and Reich (2010), in their study, *Minimum Wage Effects Across State Borders,* advanced the literature by building on Card and Krueger's work. They designed a panel dataset of employment and wage trends across contiguous county pairs situated on either side of state borders with varying minimum wage laws for use in their analysis. When compared to earlier state-level studies, which might have been muddled by more general economic or demographic variations across states, this border-county design enabled them to account for local labor market conditions with much more clarity.

The border-county data used in their study came from the Quarterly Census of Employment and Wages (QCEW). They analyzed employment patterns at the county/month level, which provided detailed observations of labor market changes across space and time. The authors concentrated on low-wage sectors, such as retail trade and food services, where minimum wage policies were anticipated to have the most significant impacts.

The study's primary methodological innovation as several others in this category was using difference-in-differences (DiD) estimation this time across matched county pairs. The authors compared the relative change in employment and wages after the policy change for each pair of neighboring counties, one in a state that raised the minimum wage and the other in a state that did not. This tactic increased the strength of their causal claims by accounting for

unobserved elements that both countries shared, such as consumer demand and regional economic trends.

According to their findings, Dube, Lester, and Reich stated no statistically significant proof that raising the minimum wage decreased employment in low-wage industries. They frequently saw modest wage increases without a corresponding slowdown in job expansion. These results contradicted previous research that used aggregate state-level comparisons and national data to find adverse effects; the authors contended that omitted variables biased these studies and did not consider local labor market integration. One of the main contributions was the study's focus on geographic heterogeneity, the notion that labor markets are frequently more localized than state-level borders imply.

Another pivotal study on the topic was written by Meer and West (2016) aiming to advance minimum wage literature by focusing on the long-term effects of minimum wage increases on job growth rather than the short-term employment outcomes. In their paper, *Effects of the Minimum Wage on Employment Dynamics*, they made the case that traditional studies might overlook significant lag responses. Businesses may respond to rising labor costs by slowing the rate of new job creation rather than eliminating employment levels over a longer period.

State-level panel data from the QCEW and the Business Dynamics Statistics (BDS) covering private-sector employment in the United States from 1977 to 2011 were used in their analysis. Their models adjusted for macroeconomic variables, state and year fixed effects, and the observation unit was the state-year. Since they believed that net job creation and gross hiring rates were more sensitive to employer behavior, they concentrated on these metrics rather than overall employment levels.

The main discovery of Meer and West was that raising the minimum wage had a statistically significant detrimental impact on employment growth over time. The slowdown was especially noticeable in industries with a high concentration of low-wage workers. According to their findings, employment may seem stable in the short term, but long-term growth may be slowed down, a dynamic that studies looking at only contemporaneous outcomes may overlook.

The last paper discussed in this section is by Allegretto, Dube, and Reich (2011) who also used a panel dataset of U.S. states, with their scope ranging from 1990 to 2006. Their study, *Do Minimum Wages Reduce Teen Employment?*, directly reacted to earlier research that used national time-series data to show detrimental effects. They argued that many of those findings were skewed by omitted variable bias, specifically the failure to account for regional economic trends that influence employment independently of minimum wage laws.

They addressed this by comparing neighboring counties with varying minimum wage laws across state lines using a difference-in-differences (DiD) approach. The analysis was able to determine constant economic conditions by matching counties within the same local labor markets, such as unemployment trends, industrial composition, and demographic structure. A county in a specific quarter was the observation unit, with the Current Population Survey (CPS) acting as their primary data source.

The study concentrated on teenagers in low-wage industries Their framework didn't only control for region-specific trends, but also time-fixed effects and county-pair effects. Compared to earlier national panel studies, they could more effectively isolate the causal effect of minimum wage increases.

Their research revealed no statistically significant proof that raising the minimum wage hurts the employment of teenage low-wage workers. Expectedly, they discovered that research that did not account for regional variations exaggerated the impact of unemployment.

Given its methodological rigor and applicability to contemporary state-level analyses, this study has established itself as a foundation in the empirical minimum wage literature.

The literature covered in this section about industries and regions indicates that the impacts of minimum wage policies are inconsistent and differ depending on the sector, location, and period. Retail, hospitality, and fast-food studies indicate that these industries might absorb wage increases without significant job losses, particularly if businesses can adjust through productivity or pricing. These findings reaffirm the significance of industry focus and strong identification strategies, principles that direct the empirical design of this thesis.

C. Adjustment Mechanisms and Labor Market Dynamics

Deeper research into the labor market and business responses to rising wage floors extends past the question of job loss following minimum wage increases. This section reviews research about adjustments made at both firm and market levels to develop a more advanced understanding of minimum wage laws' impact on the labor market. The studies discussed below demonstrate the fundamental mechanisms which explain employment effects variations across different contexts and sectors and time periods by shifting attention from simple headcounts to labor dynamics.

The first paper in the section written by Hirsch, Kaufman, and Zelenska (2015) offers one of the more thorough analyses of how companies adjust to increases in the minimum wage without necessarily laying off workers. The study surveyed more than 1,200 fast-food establishments in the U.S. to see firsthand how businesses reacted to wage increases. Their

method offered unique insight into the internal decision-making and operational strategies businesses use to deal with rising labor costs by gathering firsthand information from managers rather than depending on aggregated labor market data.

Several different methods were being taken by businesses to fight rising labor costs; most frequently, the researchers found that modest price increases, modifications to scheduling procedures, and labor hour reductions were being used. Some businesses reported increased productivity and task redistribution, but these reactions were typically small-scale rather than significant. Most importantly, the study discovered that a common reaction to a minimum wage increase was wage compression, in which higher-paid workers experience slower wage growth.

The study is highlighted for its focus on multi-channel adjustments. The authors emphasized how businesses can avoid layoffs while meeting cost pressures rather than treating job loss as the only outcome of interest.

The other paper that is important to discuss regarding market dynamics is by Aaronson, French, and Sorkin (2018), who used a dynamic "putty-clay" production model to examine how businesses react to increases in the minimum wage over time. They argue in their paper, *Firm Dynamics and the Minimum Wage: A Putty-Clay Approach*, that although labor and capital inputs are fixed in the short term, firms gradually adapt through organizational restructuring, technology adoption, and investment choices.

According to the findings of the study, raising the minimum wage tends to slow job growth, decrease hiring, and increase exit rates among low-productivity firms rather than immediately causing mass layoffs. At the same time, surviving businesses frequently make capital investments, boost output, or gradually change the makeup of their workforce. These

responses result in some small job losses, but more significantly, they reallocate jobs to more productive companies, changing the structure of the low-wage labor market.

D. Implications for This Thesis

The three sections of literature review above provide essential background information about minimum wage effects and the debate surrounding the topic. The results of studies in this field show inconsistent findings, yet most discrepancies stem from differences between study methods, time spans, and location-based and industry-specific aspects.

The papers and articles discussed demonstrate that reliable identification techniques represent the primary lesson in minimum wage research. The comparison of the fast-food industry between New Jersey and Pennsylvania led Card and Krueger (1994) to establish natural experiments as a research method. Dube, Lester, and Reich (2010), along with Allegretto, Dube, and Reich (2011), advanced this method by using geographically matched comparisons to control for regional economic trends. This thesis implements a staggered difference-in-differences (DiD) method, which follows experimental research principles despite not using border-county data. The research design uses state-level minimum wage timing differences to study treatment effects by controlling for unobserved state characteristics and national time trends.

There is a clear emphasis on industry-level specificity as a key element. Research studies before this work focused on analyzing specific subgroups, such as teenagers and employment data. Giotis and Mylonas (2022) and Belman and Wolfson (2014) explain how minimum wage laws produce different industry outcomes. This research examines low-wage service sectors because businesses in these industries must overcome specific challenges while minimum-wage

workers represent a significant workforce population. The sectoral focus of this research follows the overall direction of policy and how it affects diversity in the literature.

This thesis contains multiple dependent variables which include unemployment rate and labor force participation rate and employment-to-population ratio that are typically analyzed separately in labor market outcome studies. The research of Meer and West (2016) supports the use of time-based employment metrics which include job creation and workforce attachment. The previous study by Neumark and Wascher (2008) analyzed only headcount employment. The research assesses wage increases in labor market participation through various outcome variables during both short-term and long-term periods.

Finally, Aaronson, French, and Sorkin (2018) and Hirsch, Kaufman, and Zelenska (2015) demonstrate that minimum wage increases may not always result in detectable changes in employment statistics. Businesses in service-oriented industries typically respond to wage pressures by changing compensation structures, reallocating tasks, adjusting hours, and increasing productivity. These adjustment channels serve as essential factors for interpreting results, particularly when employment does not change despite wage pressures.

III. Background

The United States minimum wage policy has experienced deliberate federal stagnation while showing active local experimentation throughout history. The Fair Labor Standards Act (FLSA) created the first federal minimum wage standard through the New Deal labor laws when it became effective in 1938. The minimum wage achieved its peak value during 1968 because the government raised it multiple times to fight inflation and changing cost-of-living expenses. The federal government has reduced its minimum wage adjustment frequency, so the wage standard

falls behind price inflation. The federal minimum wage reached $7.25 per hour during 2009 and has maintained this rate until present day (Congressional Research Service, 2022).

The federal wage policy underwent a fundamental shift because of state and local minimum wage increases, which resulted from congressional deadlock between 2010 and 2018. State governments and grassroots organizations independently raised minimum wages above federal standards because the actual purchasing power of the federal wage kept declining. The federal minimum wage was higher than state minimum wages in 29 states and Washington D.C. in 2018 (U.S. Department of Labor, 2018). The "Fight for $15" movement began with New York City fast-food worker walkouts in 2012 through ballot initiatives, which evolved into a national movement for wage increases and union rights (Rolf, 2016).

Multiple states including Massachusetts and New York passed laws which established wage increases toward $12 or $15 per hour during this time. California passed a 2016 law that raised minimum wages for large employers to $15 by 2022 but smaller businesses received the wage increase in 2023. The federal minimum wage did not change in Georgia and Mississippi and Texas. The wage policy underwent its natural experiment. Different states raised their minimum wages at different times and levels, enabling researchers to study these policies' effects on the labor market.

The political discourse at the time was split. Supporters of wage increases advocated for higher earnings because they believed low-wage employees needed more money to survive in an economy with increasing living costs and growing income disparities. They cited studies that found employment suffered only slightly from the wage increase, but service sector employment remained unaffected (Dube, Lester, & Reich, 2010; Allegretto, Dube, & Reich, 2011). Their opponents expressed concern that increased wage regulations would reduce employment

opportunities for young workers and those who hold low-skilled or part-time positions. The potential side effects of reduced work hours automation and business shutdowns fueled opponents who cited these risks (Neumark & Wascher, 2008). Economists and advocacy organizations produced opposing reports to back each side of this dispute, which was contested in state legislatures, election campaigns, and media outlets, leading to ineffective efforts at the federal level.

The discussion centered on low-wage sectors, including retail, food service, personal care, and hospitality, which employ most minimum-wage workers in the United States (Bureau of Labor Statistics, 2018). According to opponents, these industries would not be able to absorb the cost increases without laying off workers or reducing hours. Supporters argued that higher wages would increase stability, lower turnover, and boost consumer spending.

The remainder of this thesis investigates the effects of state-level minimum wage increases while considering the shifting policy environment. The research investigates a critical period in minimum wage policy discussions by examining state-level policy adoption, which became the standard national approach during this rising decentralization and experimental policymaking. The analysis and findings contribute to empirical discussions through its staggered difference-in-differences design, which examines industries under direct observation of minimum wage impacts while studying variations in timing and magnitude of wage increases.

IV. Data

To investigate the labor market impacts of state minimum wage changes from 2010 to 2018, this thesis builds a state-by-year panel dataset with a particular focus on low-wage industries. Wage-specific variables from the Outgoing Rotation Group (ORG) samples are added to the core dataset, which is constructed from individual-level responses in the Current

Population Survey (CPS) and accessed through IPUMS-CPS. The CPS is conducted by both the U.S. Census Bureau and the Bureau of Labor. It is essentially a monthly labor force survey that offers holistic data on demographics, industry, occupation, hours worked, earnings, and employment status.

A specific subset of variables was chosen when the data was extracted to support the empirical objectives of the study and speed up the data-cleaning procedure. Identification fields (year, month, and state), demographics (age, sex, race, and education), labor force status indicators, industry codes, and wage-related variables (hourly pay status and weekly earnings) were among the variables that were taken from the Basic Monthly CPS and the ORG supplement. Table 1 displays a complete list of the variables that were extracted.

After downloading the data for all months from January 2010 through December 2018, the raw sample included over 13.7 million person-level observations. Then, filters were applied to narrow the sample to the relevant working population. The sample was first limited to individuals aged 16 to 64 to focus on the working-age population. Next, only those who were currently employed (EMPSTAT == 10 or 12) were retained. To isolate low-wage industries, individuals were kept only if they reported working in sectors historically associated with minimum wage employment. This included retail trade (IND codes 5700–5790), leisure and hospitality (8660), and food services (8680). Lastly, the sample was restricted to hourly workers (PAIDHOUR == 1), which excludes salaried and tipped workers, whose earnings are less reliably captured in the data.

Several cleaning steps were taken to improve data quality. Hourly wage reports below $3 or above $999.99 were removed to eliminate implausible responses. Responses indicating that usual hours worked per week “varied” (coded 997) were treated as missing and then set to zero.

Observations missing valid values for either hours worked, or hourly wage were excluded. Derived variables were then constructed, including a log-transformed hourly wage, a full-time indicator (35+ hours/week), and weekly earnings, calculated as the product of hourly wage and usual weekly hours worked. Weekly earnings were winsorized at the 99th percentile, setting values above $1,215 to missing to reduce the influence of outliers. All monetary variables, including wages, earnings, and minimum wage values, were converted to constant 2018 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). After all filters and cleaning steps, the final cleaned individual-level CPS dataset included 85,423 observations.

To link individual outcomes to wage policy, annual minimum wage data were obtained from the Federal Reserve Economic Data (FRED) platform. The minimum wage series for each state were downloaded, reshaped from wide to long format, and cleaned to remove malformed entries and non-state regions such as, Puerto Rico, Guam, and the Virgin Islands. The result was a panel dataset of 459 state-year observations from 2010 to 2018, which was then merged with the CPS data by state and year. All statutory minimum wage values were adjusted to real 2018 dollars using the CPI-U to ensure comparability across time.

Two additional datasets were merged to control for broader economic conditions. First, annual state unemployment rates were collected from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS). Monthly unemployment figures for each state were averaged to produce annual values. Second, state-level GDP per capita was obtained from the Bureau of Economic Analysis (BEA) using the SAINC1 and SAINC51 data series. This variable was used to account for variations in state-level economic development and growth. All datasets were merged using consistent state and year identifiers.

The cleaned individual-level data were then aggregated to the state-by-year level, producing a balanced panel of 459 observations (50 states plus D.C. across 9 years). Aggregation was done using CPS person-level sampling weights (WTSUPP) to ensure representativeness. For each state year, the following labor market outcomes were calculated for individuals in the filtered low-wage industries: the employment-to-population ratio, the labor force participation rate, and the unemployment rate. These three outcomes capture different dimensions of labor market engagement and collectively provide a broad view of state-level employment dynamics within low-wage sectors.

A summary of the variables used in the final dataset, including sources, construction details, and coverage, is presented in Table 2. Descriptive statistics for the key variables in the final state-year panel are shown in Table 3.

The purpose of the final dataset is to support the policy analysis carried out in the subsequent empirical section by establishing a connection between state minimum wage laws and important employment outcomes within low-wage sectors. The inherent variance in minimum wage timing and scope during state-level policy experimentation allows for meaningful comparisons between states and across time periods. The annual, state-level format allows for the use of panel econometric techniques, including staggered difference-in-differences and event study designs, to identify dynamic effects. This foundation positions this thesis to contribute to ongoing academic and policy debates about the effects of raising the minimum wage on the labor market.

V. Methodology

A. Hypothesis

The framework of this study is built to evaluate the impact on unemployment rates, employment-to-population ratios, and disposable income to investigate the effects of state-level minimum wage increases on low-wage sectors. Classical economic theory predicts job losses from higher labor costs, yet a large amount of recent of empirical research shows that these effects tend to be either insignificant or negligible, especially in low-wage sectors in the short run.

The null hypothesis (H_0) states that minimum wage increases in states do not lead to meaningful changes in employment or unemployment within low-wage industries. The alternative hypothesis (H_1) states that these policies generate quantifiable changes in labor market results, which vary based on state conditions and policy magnitude and timing.

This methodology attempts to bring precision to a heavily divided policy debate by focusing on variation across treatment timing and intensity between 2010 and 2018.

B. Staggered Differences in Differences Framework

The analysis used to obtain results discussed later implements a staggered difference-in-differences (DiD) design based on Callaway and Sant'Anna (2021) specific design to evaluate minimum wage impacts on employment within low-wage industries. With the design employing event study variation through the examination of U.S. state minimum wage increases which occurred at different times and in different states between 2010 and 2018. The framework benefits from a natural experimental environment because it allows the study of policy effects through variations in treatment timing.

In the regression, every state that increases its minimum wage is considered a member of a cohort, which is determined by the year the policy goes into effect. Results before and after the

minimum wage increase are compared for each treated state. The analysis includes a control group made up of states that maintained their minimum wage at pre-reform levels or had not implemented any wage increases at that point. This approach maintains proper controls while preventing the comparison of treated states with other treated states at incorrect time points.

The Callaway and Sant'Anna estimator enables the estimation of treatment effect heterogeneity across cohorts and over time by organizing the analysis to estimate effects specific to each treatment group and period. It also enables aggregation of these effects in a manner that reflects the actual implementation of the policy across time and space. This is especially important in the context of the minimum wage, as states made independent decisions regarding the scope and timing of their changes rather than implementing a unified national reform.

The estimation follows the regression model depicted below:

$$Y_{st} = \alpha + \sum_{k \neq -1} \beta_k * EventTime_{st}^{(k)} + \delta_s + \gamma_t + X'_{st}\theta + \varepsilon_{st}$$

Where:

- Y_{st} = outcome of interest (e.g., employment-to-population ratio) in state s and year t
- $EventTime_{st}^{(k)}$ = dummy variables indicating k before or after a state's minimum wage increase (with k = -1 as the omitted baseline)
- δ_s = state fixed effects that absorb time-invariant differences between states
- γ_t= year fixed effects that capture national shocks and time trends
- X_{st} = vector of time-varying state-level controls
- ε_{st} = error term

This specification allows the model to trace out the effect of minimum wage increases over time. The event time dummies ($EventTime_{st}^{(k)}$) indicate how many years before or after a state adopted a minimum wage increase. The coefficients β_k estimate the difference in outcomes between treated and not-yet-treated states k years relative to treatment, with $k = -1$ (the year just before treatment) serving as the reference period. This enables the estimation of dynamic treatment effects over multiple years as well as a visual inspection of pre-trends.

The inclusion of state fixed effects (δ_s) accounts for time-invariant differences across states, such as geography, baseline labor market structures, or political leanings. Year fixed effects (γ_t) absorb national shocks or macroeconomic events, such as federal policy changes or nationwide recessions, that could affect all states simultaneously. Finally, time-varying state-level covariates (X_{st}) such as unemployment rates and GDP per capita are included to control for local economic conditions that could influence employment outcomes independently of minimum wage policy.

To identify true policy-driven treatment events, an analysis was conducted to find the best threshold to be considered a "material" wage increase. The analysis revealed that the vast majority of state-year observations (244 out of 459) featured no change in the minimum wage, while many small fluctuations, such as $0.05 or $0.10 increases, appeared to result from automatic inflation indexing rather than legislative action. In contrast, discrete jumps of $0.50, $0.75, or $1.00 were far more likely to reflect deliberate policy reforms. Based on this distribution, The research establishes treatment status through a $0.50 minimum threshold for yearly wage changes while excluding minor adjustments to minimize noise and focus on significant policy impacts. The selection meets both statistical power requirements and

meaningful intervention detection standards and follows best practices in empirical labor economics.

The empirical design reliability was assessed through various robustness checks performed during the analysis. The analysis evaluates multiple treatment definitions, including 25-cent, 50-cent, and one-dollar minimum wage increases and different treatment timing and subsamples. The analysis performed these checks to verify that the results are not dependent on specific threshold choices or sample configurations. The robustness section presents additional details about these tests together with their findings. The structure enables a credible and flexible estimation of minimum wage effects in a setting where policy changes occurred at different times with different intensities and under different local conditions.

By estimating treatment effects relative to each state's policy adoption year and comparing only to states that had not yet implemented changes, the model avoids the biases that can arise from comparing already-treated states to newly treated ones. The event study framework provides a clear picture of the timing, duration, and direction of any effects, which makes it well-suited for evaluating labor market dynamics in response to staggered policy adoption.

In the context of this study, the staggered difference-in-differences design is necessary to generate reliable estimates, not merely a methodological preference. Conventional DiD models assume that all treated units receive treatment simultaneously and that control units do not receive treatment during the analysis period. Neither of these presumptions applies to the minimum wage policy in the United States over the time period.

Between 2010 and 2018, states raised their minimum wages at different times, usually through multi-year phase-ins or repeated increases. As a result, the implementation of policies is

very state-specific and irregular. The basic logic of a control group would be violated if a standard two-way fixed effects DiD model were applied in this case, as it would treat this variation as noise and frequently compare treated units to units that have already received treatment. In fact, recent research has shown that these models may produce erroneous or even reversible indications of the treatment effect when effects vary over time or between cohorts (Goodman-Bacon, 2021).

For instance, even though State A already feels the effects of its treatment, a traditional DiD model might use State A as a control for State B in 2015 if State A raised its minimum wage in 2012 and State B followed suit in 2015. Negative weighting, in which the influence of some treatment effects is incorrectly subtracted from others in the aggregate estimate, can result from this type of tainted comparison (Goodman-Bacon, 2021; Sun & Abraham, 2021). This risk is increased in a setting with numerous treatment cohorts, such as this one.

By carefully arranging comparisons so that treated states are only compared to legitimate control units at the time of treatment, a staggered DiD approach solve this problem. This maintains DiD's counterfactual reasoning: what would have happened to the treated unit in the event that the policy hadn't been put into place? In a situation where minimum wage increases differ in size and timing across various state labor markets, it also enables the analysis to remain sensitive to treatment effect heterogeneity (Callaway & Sant'Anna, 2021).

In summary, staggered DiD is the only suitable approach due to the variability of the policy environment by state, which includes different magnitudes, staggered adoption, and diverse economic backgrounds.

C. Traditional Difference-in-Differences Model Comparison

To understand the framework, it is helpful to compare the staggered difference-in-differences design with a conventional DiD, which assumes that all treated units receive the policy simultaneously in order to demonstrate the need for it. In this scenario, a single binary variable that changes from 0 to 1 in the year the policy is implemented and stays at 1 after that would be used to model treatment.

The estimation would follow the regression model depicted below:

$$Y_{st} = \alpha + \beta * Post_t * Treated_s + \delta_s + \gamma_t + X'_{st}\theta + \varepsilon_{st}$$

Where:

- Y_{st} = outcome of interest in state s and year t
- $Treated_s$ = dummy variable equal to 1 for treated states
- $Post_t$ = is a dummy equal to 1 in all years after treatment begins
- δ_s = state fixed effects
- γ_t= year fixed effects
- X_{st} = vector of time-varying state-level controls
- ε_{st} = error term

By contrasting the change in outcomes in treated states before and after treatment with the change in untreated states during the same time period, this model calculates a single average treatment effect. This arrangement, however, is predicated on the idea that treatment is administered consistently throughout all states simultaneously and that its effects are consistent across time and units.

This model would not be appropriate in the context of this study, where states raised their minimum wages to varying degrees and at varying times between 2010 and 2018. There are serious bias risks when using a single treatment indicator in a staggered setting. The DiD

identification strategy might be broken, for instance, if treated units that were given the policy earlier are mistakenly used as controls for units that were treated later. Furthermore, the estimate from this model would blend effects from various cohorts, creating a misleading average that might not accurately reflect the experience of any state if the policy's effect changes over time.

D. Comparing Staggered Difference-in-Differences Methods

While evaluating policies implemented at different times across units requires a staggered difference-in-differences (DiD) design, not all staggered DiD frameworks are equally reliable or appropriate. The traditional approach uses a two-way fixed effects (TWFE) model, which includes unit and time-fixed effects and estimates a single treatment effect by pooling treated and untreated units across time. This model gained popularity for its simplicity and computational efficiency, especially in panel data settings. However, when treatment effects vary over time or across treated units, as is often the case in policy settings like minimum wage increases, the TWFE approach can yield misleading results (Goodman-Bacon, 2021; Sun & Abraham, 2021).

This issue is based on how TWFE constructs its average treatment effect estimate, it combines all possible comparisons between treated and untreated units, including problematic ones where already-treated units are used as controls for newly treated ones. The so-called “forbidden comparison” introduces contamination because previously treated units are no longer valid counterfactuals. Furthermore, if treatment effects differ by timing or by state characteristics, TWFE can assign negative weights to some comparisons, meaning those estimates subtract from, rather than contribute to, the overall average. These distortions are particularly severe in settings like this one, where treatment timing is staggered, policy intensity varies, for example, $0.25 vs. $1.00 increases, and economic conditions differ across states.

The Callaway and Sant'Anna (2021) estimator serves as the solution to these problems because it specifically addresses staggered adoption with treatment effect heterogeneity. The method developed by Callaway and Sant'Anna constructs group-time average treatment effects (ATTs) that compare newly treated units in a given period only to units that are untreated or not yet treated to prevent the use of invalid counterfactuals. The group-specific effects receive a weighting system that maintains causal interpretation while avoiding TWFE's pooled method. The estimator allows users to create event studies that help both visualize treatment effect dynamics and check the parallel trends assumption.

The framework provides transparent and flexible analysis that matches well with the minimum wage policy well because states implement changes at different times and magnitudes under different labor market conditions. The Callaway and Sant'Anna approach produces more credible and interpretable policy impact estimates because it avoids TWFE model biases and allows treatment effects to vary.

E. Limitations and Assumptions

The validity of the staggered difference-in-differences approach in this thesis depends on multiple basic assumptions. The first assumption is the parallel trends assumption which states that labor market outcomes in treated and untreated states would have followed similar paths if policy changes had not occurred. The event study design, enables the estimation of pre-treatment coefficients for each cohort which enables a visual assessment of that assumption. The coefficients would be near zero and statistically insignificant before policy adoption if there were no discernible difference between the treatment and control groups (Callaway & Sant'Anna, 2021; Sun & Abraham, 2021). Although these diagnostic tools provide insight into the parallel trends, the assumption remains essential for determining causal effects.

The absence of anticipation is another presumption, which states that people and businesses do not change their behavior when they are aware of upcoming increases in the minimum wage. In practice, employers may start modifying wages, hours, or staffing levels before the official policy is put into effect, especially in states where increases are announced or gradually implemented. According to Dube, Lester, and Reich (2010), these anticipatory effects have the potential to skew pre-treatment trends and add bias to post-treatment estimates.

The model also assumes that there are no cross-state spillover effects. This means that the labor market conditions in neighboring or economically connected states should not be affected by a minimum wage increase in one state. Nonetheless, research indicates that employees and businesses can and do react to wage disparities across borders, particularly in interstate metropolitan areas (Dube et al., 2010). Results may be skewed if migration or cross-border commuting happens as a result of policy differences.

Lastly, the analysis assumes that treatment effect heterogeneity receives appropriate consideration. The Callaway and Sant'Anna (2021) framework enables researchers to calculate treatment effects for specific group-time cohorts while allowing effects to differ based on state-specific policy implementation contexts and years. The model reduces bias from conventional two-way fixed effects models because it accounts for time-dependent effect changes (Goodman-Bacon, 2021). These estimates require sufficient variation together with adequate sample size across treatment cohorts to maintain their robustness.

Finally, the quality of the underlying data limits the analysis. Although the Current Population Survey (CPS) is a popular and high-quality labor force dataset, non-sampling errors can occur because it is based on self-reported responses. Measurement errors may be introduced by inaccurate wage reporting or incorrect industry classification. Furthermore, the timing and

scope of statutory minimum wage laws vary from state to state, as does their structure; some have provisions for small businesses, tipped workers, and youth labor (Autor, Manning, & Smith, 2016). The state-level minimum wage variable used in this analysis may not adequately account for these subtleties, which could limit its precision.

VI. Results

A. Employment-to-Population Ratio

This section begins with the employment-to-population ratio, the primary outcome of interest in this study for assessing how state-level minimum wage increases influence labor force engagement in low-wage sectors. Figure 1 displays the results of the staggered event study, plotting the estimated treatment effects for each event time relative to the baseline year prior to policy implementation ($k = -1$). Table 4 presents the corresponding regression coefficients and standard errors.

The pre-treatment coefficients, which span four years before treatment, are all close to zero and statistically insignificant, which supports the assumption of parallel trends. This increases the credibility of the identification strategy, indicating that treated and control states followed similar trends before the policy intervention. Post-treatment effects remain modest and statistically insignificant throughout the window. The largest positive coefficient appears in the second year after treatment ($k = +2$), where the employment-to-population ratio rises by approximately 0.17 percentage points. However, the standard error for this estimate is large enough that it includes both small positive and negative values, indicating substantial uncertainty. No post-treatment coefficient is statistically different from zero at conventional significance levels.

The average treatment effect on the treated (ATT) from the Callaway and Sant'Anna estimator confirms these findings. As shown in Table 5, the ATT estimate for employment is 0.09 percentage points with a robust standard error of 0.12, again suggesting a null effect. These results align with the hypothesis that minimum wage increases have limited or negligible effects on overall employment levels in low-wage industries.

Several factors may explain these muted effects. Prior literature suggests that modest increases in minimum wages may be absorbed through channels other than headcount reductions, including reductions in turnover, improvements in labor productivity, and modest price adjustments (Schmitt, 2013; Dube, Lester & Reich, 2010; Hirsch, Kaufman & Zelenska, 2015). Moreover, employer responses may vary depending on local demand conditions and competitive pressures, which the pooled design may not fully capture.

It is also possible that the effects are heterogeneous across states or subgroups, such as teens or part-time workers, but the pooled estimates average across all treated units and may, therefore, mask underlying variation. While this section focuses on overall population-level effects, subsequent robustness checks, and alternate specifications explore potential subgroup responses and sensitivity to treatment timing and intensity.

B. Labor Force Participation Rate

The labor force participation rate (LFPR), which measures the percentage of working-age people who work or seek work, provides a more comprehensive view of labor market involvement than employment statistics alone. The LFPR provides insights into how minimum wage changes impact both employment status and job-seeking activities among workers who might be entering or leaving the labor market.

The results presented in Figure 2, the event study plot, show that changes in labor force participation surrounding state-level minimum wage increases are minimal. Pre-treatment coefficients are flat and statistically indistinguishable from zero, satisfying the parallel trends assumption. Following treatment, the estimated effects remain small in magnitude and mostly negative. No post-treatment year displays a statistically significant shift in participation.

Table 6 presents the full set of event-time coefficients for LFPR across multiple years before and after treatment. Consistent with the plot, the estimates reveal no evidence of anticipatory effects nor any delayed responses. Most post-treatment coefficients remain near zero, fluctuating narrowly and showing no consistent upward or downward trend.

Meanwhile, Table 7, which reports the average treatment effects on the treated (ATTs) by year, echoes these findings. The ATT in the first year post-treatment is slightly positive (0.087 percentage points), while years two through five show small negative estimates, none of which are statistically significant. These results reinforce the overall pattern: State minimum wage increases do not significantly alter participation behavior in low-wage sectors.

One potential explanation is that competing forces offset each other. On the one hand, higher minimum wages may draw discouraged or inactive workers back into the labor force by improving the expected returns to job search. On the other hand, if employers reduce vacancies or tighten hiring standards in response to higher labor costs, those incentives may be counterbalanced. The net effect, as shown here, appears close to zero.

The analysis provides little evidence that raising the minimum wage materially changes the rate at which individuals participate in the labor force. This aligns with the broader hypothesis of muted overall effects: although minimum wage increases may influence wages and

working conditions, their impact on participation, like employment, is limited in scope and statistically indistinct from zero over the period studied.

C. Unemployment Rate

The impact of state-level minimum wage increases on unemployment within low-wage industries was also examined using the staggered DiD framework. Results from the event study plot (Figure 3), event time coefficients (Table 8), and the average ATT summary (Table 9) indicate no significant or systematic changes in the unemployment rate following a minimum wage hike of $0.50 or more.

The visualization in Figure 3 shows the predicted treatment impacts on unemployment rates throughout the event time period. The coefficients stay close to zero throughout the post-treatment period without any noticeable rising or falling pattern. The confidence intervals stay broad throughout all periods and completely overlap with zero, which supports the finding that there is no statistical significance. There is no evidence of differential pre-trends prior to treatment, further supporting the validity of the identification strategy.

Table 8 presents the underlying coefficient estimates from the event study regression. None of the post-treatment coefficients are statistically significant at conventional levels. Most hover within a ±0.2 percentage point range, suggesting minimal employment displacement or labor market friction resulting from minimum wage increases. In particular, coefficients for years 1, 2, and 3 post-treatment, which might be expected to capture the bulk of short-run policy response, are small in magnitude and statistically indistinguishable from zero.

Finally, the average treatment effect across all treated states and years is reported in Table 9. The mean ATT is economically negligible and not statistically significant, which further supports the conclusion that state-level minimum wage increases did not significantly increase

unemployment in the sectors studied. These findings confirm that wage increases of $0.50 or more did not significantly reduce employment levels throughout the 2010–2018 in low-wage industries

D. Robustness Checks

Additionally, multiple robustness checks were conducted to validate the primary research results in the analysis. The tests evaluated how minimum wage effects on employment in low-wage industries changed based on modeling choices such as control variables and treatment threshold definitions. The first robustness check examined the results' sensitivity when state-level control variables such as unemployment rate and GDP per capita were excluded. The results showed no significant change when the regression model did not include state-level control variables. The estimated treatment effects for employment-to-population ratio, labor force participation, and unemployment rate showed similar timing and direction when compared to the baseline model as presented in Tables 10-12 and Figures 4–6. The coefficients showed some variations in magnitude and precision during the later post-treatment periods but, the overall policy response patterns remained consistent. The analysis of internal validity receives support from this consistency because it demonstrates that results are not dependent on time-varying local economic conditions.

The second set of robustness checks tested alternative thresholds for defining treatment. In the baseline specification, states were considered treated in the year they increased their minimum wage by at least $0.50. To evaluate the impact of this threshold choice, treatment was redefined using more permissive ($0.25) and more stringent ($0.75 and $1.00) thresholds. The results presented in Figures 7-9 showed weaker and less consistent treatment effects under these alternative definitions especially under the $0.75 and $1.00 specifications. The higher thresholds

resulted in fewer treated units which produced larger standard errors and less precise estimates. The estimated effects sometimes showed different directions than the baseline pattern because of the reduced sample size. The $0.25 threshold introduced significant noise which probably picked up small inflation-indexed adjustments rather than real policy changes. These findings validate the use of the $0.50 threshold as a balanced and empirically grounded definition of treatment that captures substantive policy action while retaining adequate sample size and statistical power.

Together, the robustness checks demonstrate that the core results are not artifacts of a particular specification. The estimated effects remain directionally consistent without controls and are most stable when the $0.50 threshold is used to define policy interventions. These checks strengthen confidence in the main findings and illustrate the importance of thoughtful modeling decisions in difference-in-differences research.

VII. Discussion

The research analyzed employment changes from state-level minimum wage hikes in low-wage industries throughout the United States from 2010 through 2018 through a staggered difference-in-differences framework. The analysis shows that minimum wage increases have produced small statistically insignificant effects on employment-to-population ratio and labor force participation rate and unemployment rate. The estimated directions show possible disemployment effects, but the results fail to reach statistical significance and do not demonstrate conclusive negative employment impacts in these specific sectors.

Importantly, the robustness checks reinforce the credibility of these findings. When the model is estimated without state-level controls, or when the treatment definition is tightened to only include wage increases of $0.75 or $1.00, the general pattern of null or modest effects

remains consistent. This indicates that the results are not an artifact of model specification or arbitrary thresholds and suggests a degree of robustness to empirical choices.

The results support current empirical studies that dispute the traditional prediction of extensive employment reductions following minimum wage increases (Card & Krueger, 1994; Dube, Lester, & Reich, 2010; Allegretto et al., 2018). The study contradicts theoretical predictions by showing that low-wage industries with high employee turnover, low-skill labor, and limited profit margins do not experience significant employment changes from small wage increases.

From a policy perspective the findings support existing studies which demonstrate that small minimum wage increases have a minimal impact on employment rates in low-wage industries. The observed effects could stem from multiple factors including employee retention rates and businesses' ability to raise prices and boost worker efficiency which enables them to maintain profitability (Hirsch, Kaufman, & Zelenska, 2015; Cengiz et al., 2019).

The findings provide evidence about minimum wage effects on employment duration in low-wage industries, yet they do not rule out additional effects on work hours or wage patterns and vulnerable groups including teenagers and part-time workers (Neumark & Wascher, 2008; Meer & West, 2016).

VIII. Conclusion

This thesis set out to examine the impact of state-level minimum wage elevations on employment within low-wage industries throughout the United States from 2010 to 2018. The research employed a staggered difference-in-differences design to examine three essential labor market outcomes which included employment-to-population ratio and labor force participation

rate and unemployment rate. The findings demonstrated that minimum wage increases produced tiny effects which were not statistically meaningful for all three studied measures.

The study supports ongoing empirical research by showing that retail, food services and hospitality sectors do not experience significant disemployment effects when minimum wage increases moderately. The results demonstrate robustness through alternative specifications and treatment definitions.

The findings from this research fail to demonstrate substantial employment effects but this discovery holds significant value. The data indicates that wage floors can increase step by step without causing major employment problems in vulnerable industries. The analysis focuses on employment data but does not evaluate other possible adjustments such as work hours changes and wage compression effects and firm responses.

Future research could expand this work by looking at distributional effects across demographic subgroups, or by looking at long run dynamics, or by using firm level data to see how businesses have adjusted operationally. Nonetheless, this analysis provides a good foundation for understanding how recent state-level wage hikes have affected the labor market and provides timely evidence for policymakers who are trying to decide whether or not to raise the minimum wage.

IX.Data Sources

- Federal Reserve Bank of St. Louis. (2025). *State minimum wage rates*. FRED. https://fred.stlouisfed.org/
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- U.S. Department of Labor. (2018). *Minimum wage laws in the states*. https://www.dol.gov/agencies/whd/minimum-wage/state

X. Data Appendix

Variable Name: state

Missing Observations: 0/459

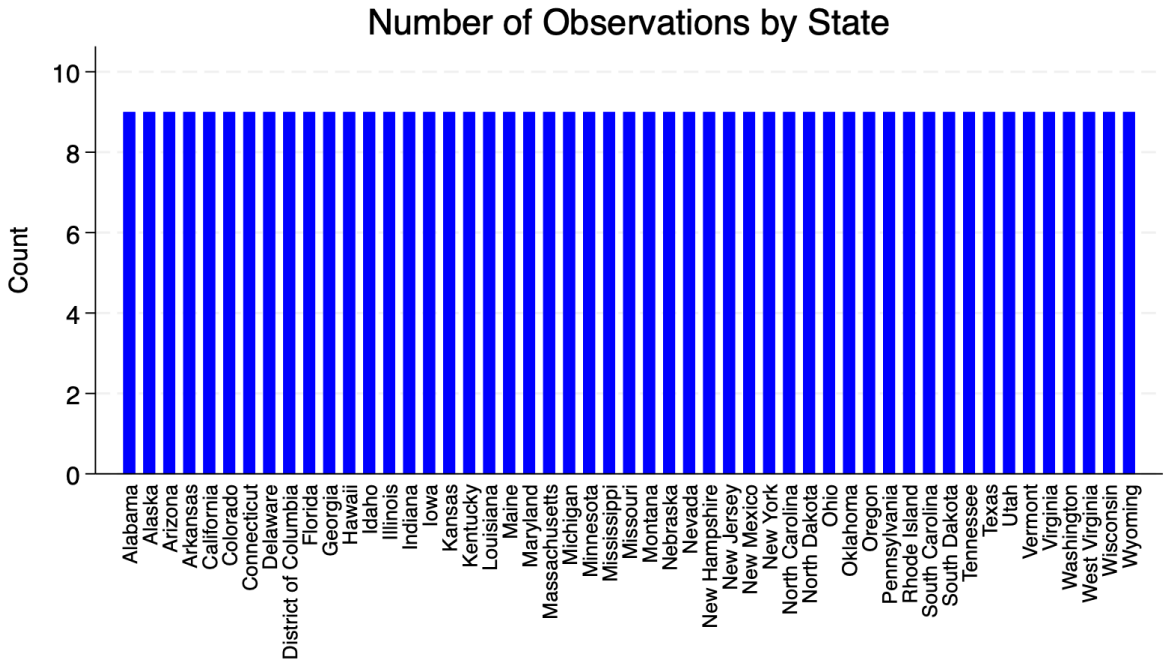
Definition: State where the observation was recorded. Each observation represents one U.S. state in a given year.

Possible Values: Name of U.S. States as well as District of Columbia (e.g., Alabama, Alaska, …, Wyoming)

Frequency Table:

state	Freq.	Percent	Cum.
Alabama	9	1.96	1.96
Alaska	9	1.96	3.92
Arizona	9	1.96	5.88
Arkansas	9	1.96	7.84
California	9	1.96	9.80
Colorado	9	1.96	11.76
Connecticut	9	1.96	13.73
Delaware	9	1.96	15.69
District of Columbia	9	1.96	17.65
Florida	9	1.96	19.61
Georgia	9	1.96	21.57
Hawaii	9	1.96	23.53
Idaho	9	1.96	25.49
Illinois	9	1.96	27.45
Indiana	9	1.96	29.41
Iowa	9	1.96	31.37
Kansas	9	1.96	33.33
Kentucky	9	1.96	35.29
Louisiana	9	1.96	37.25
Maine	9	1.96	39.22
Maryland	9	1.96	41.18
Massachusetts	9	1.96	43.14
Michigan	9	1.96	45.10
Minnesota	9	1.96	47.06
Mississippi	9	1.96	49.02
Missouri	9	1.96	50.98
Montana	9	1.96	52.94
Nebraska	9	1.96	54.90
Nevada	9	1.96	56.86
New Hampshire	9	1.96	58.82
New Jersey	9	1.96	60.78
New Mexico	9	1.96	62.75
New York	9	1.96	64.71
North Carolina	9	1.96	66.67
North Dakota	9	1.96	68.63
Ohio	9	1.96	70.59
Oklahoma	9	1.96	72.55
Oregon	9	1.96	74.51
Pennsylvania	9	1.96	76.47
Rhode Island	9	1.96	78.43
South Carolina	9	1.96	80.39
South Dakota	9	1.96	82.35
Tennessee	9	1.96	84.31
Texas	9	1.96	86.27
Utah	9	1.96	88.24
Vermont	9	1.96	90.20
Virginia	9	1.96	92.16
Washington	9	1.96	94.12
West Virginia	9	1.96	96.08
Wisconsin	9	1.96	98.04
Wyoming	9	1.96	100.00
Total	459	100.00	

Bar Graph showing percent frequency distribution:



Variable Name: year

Missing Observations: 0/459

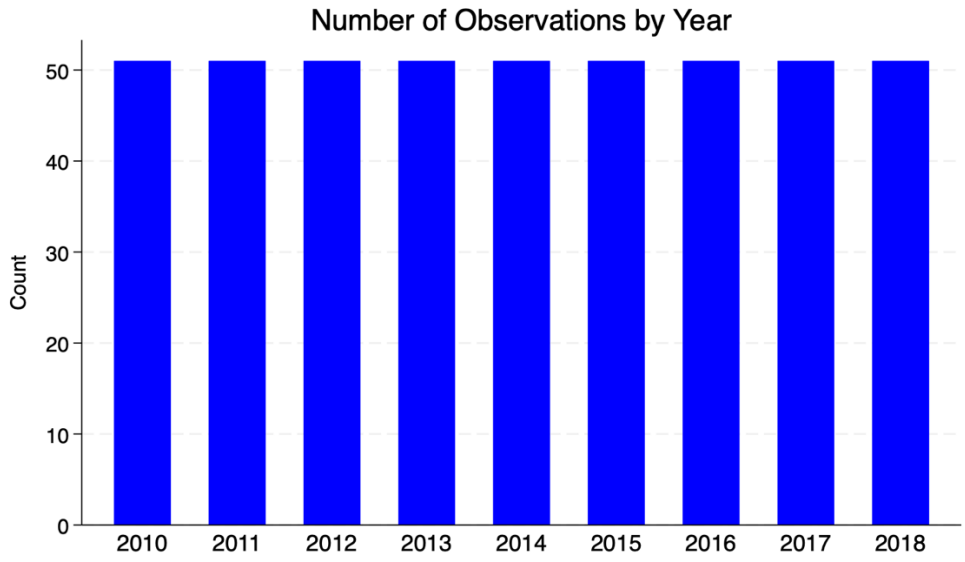
Definition: Calendar year of the observation, spanning from 2010 to 2018.

Possible Values: 2010, 2011, 2012, 2013, 2014, 2015, 2016, 2017, 2018

Frequency Table:

year	Freq.	Percent	Cum.
2010	51	11.11	11.11
2011	51	11.11	22.22
2012	51	11.11	33.33
2013	51	11.11	44.44
2014	51	11.11	55.56
2015	51	11.11	66.67
2016	51	11.11	77.78
2017	51	11.11	88.89
2018	51	11.11	100.00
Total	459	100.00	

Bar Graph showing percent frequency distribution:



Variable Name: state_min_wage

Missing Observations: 0/459

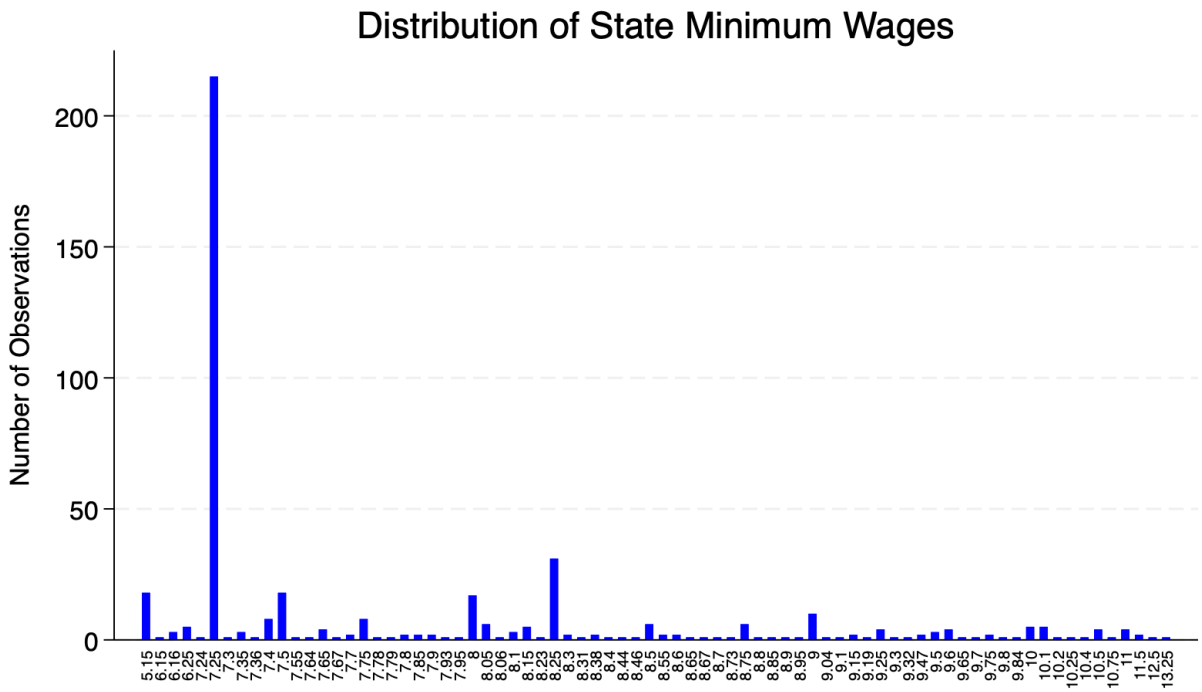
Definition: Statutory minimum wage (in dollars) applicable to nonexempt employees in each state and year.

Possible Values: Continuous values, mostly ranging between $5.15 and $13.25

Frequency Table:

state_min_wage	Freq.	Percent	Cum.
7.25	215	46.84	46.84
8.25	31	6.75	53.59
5.15	18	3.92	57.52
7.5	18	3.92	61.44
8	17	3.70	65.14
9	10	2.18	67.32
7.4	8	1.74	69.06
7.75	8	1.74	70.81
8.05	6	1.31	72.11
8.5	6	1.31	73.42
8.75	6	1.31	74.73
6.25	5	1.09	75.82
8.15	5	1.09	76.91
10	5	1.09	78.00
10.1	5	1.09	79.08
7.65	4	0.87	79.96
9.25	4	0.87	80.83
9.6	4	0.87	81.70
10.5	4	0.87	82.57
11	4	0.87	83.44
6.16	3	0.65	84.10
7.35	3	0.65	84.75
8.1	3	0.65	85.40
9.5	3	0.65	86.06
7.7	2	0.44	86.49
7.8	2	0.44	86.93
7.85	2	0.44	87.36
7.9	2	0.44	87.80
8.3	2	0.44	88.24
8.38	2	0.44	88.67
8.55	2	0.44	89.11
8.6	2	0.44	89.54
9.15	2	0.44	89.98
9.47	2	0.44	90.41
9.75	2	0.44	90.85
11.5	2	0.44	91.29
6.15	1	0.22	91.50
7.24	1	0.22	91.72
7.3	1	0.22	91.94
7.36	1	0.22	92.16
7.55	1	0.22	92.37
7.64	1	0.22	92.59
7.67	1	0.22	92.81
7.78	1	0.22	93.03
7.79	1	0.22	93.25
7.93	1	0.22	93.46
7.95	1	0.22	93.68
8.06	1	0.22	93.90
8.23	1	0.22	94.12
8.31	1	0.22	94.34
8.4	1	0.22	94.55
8.44	1	0.22	94.77
8.46	1	0.22	94.99
8.65	1	0.22	95.21
8.67	1	0.22	95.42
8.7	1	0.22	95.64
8.73	1	0.22	95.86
8.8	1	0.22	96.08
8.85	1	0.22	96.30
8.9	1	0.22	96.51
8.95	1	0.22	96.73
9.04	1	0.22	96.95
9.1	1	0.22	97.17
9.19	1	0.22	97.39
9.3	1	0.22	97.60
9.32	1	0.22	97.82
9.65	1	0.22	98.04
9.7	1	0.22	98.26
9.8	1	0.22	98.47
9.84	1	0.22	98.69
10.2	1	0.22	98.91
10.25	1	0.22	99.13
10.4	1	0.22	99.35
10.75	1	0.22	99.56
12.5	1	0.22	99.78
13.25	1	0.22	100.00
Total	459	100.00	

Bar Graph showing percent frequency distribution:



Variable Name: employment_population_ratio

Missing Observations: 0/459

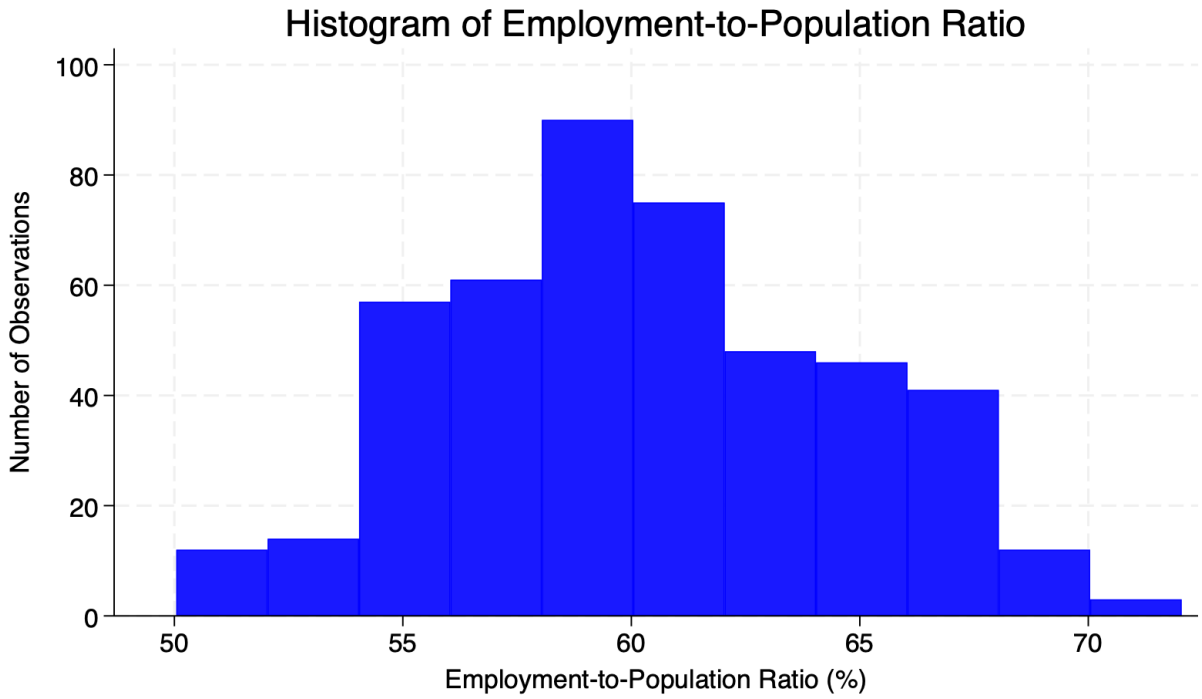
Definition: Percentage of employed individuals in low-wage industries relative to the total working-age population (ages 16-64) in each state and year.

Possible Values: Continuous variable; typically between 40% and 70%

Frequency Table:

Variable	Obs	Mean	Std. dev.	Min	Max
employment~o	459	60.30103	4.341982	50.04167	70.65

Histogram showing percent frequency distribution:



Variable Name: labor_force_participation_rate

Missing Observations: 0/459

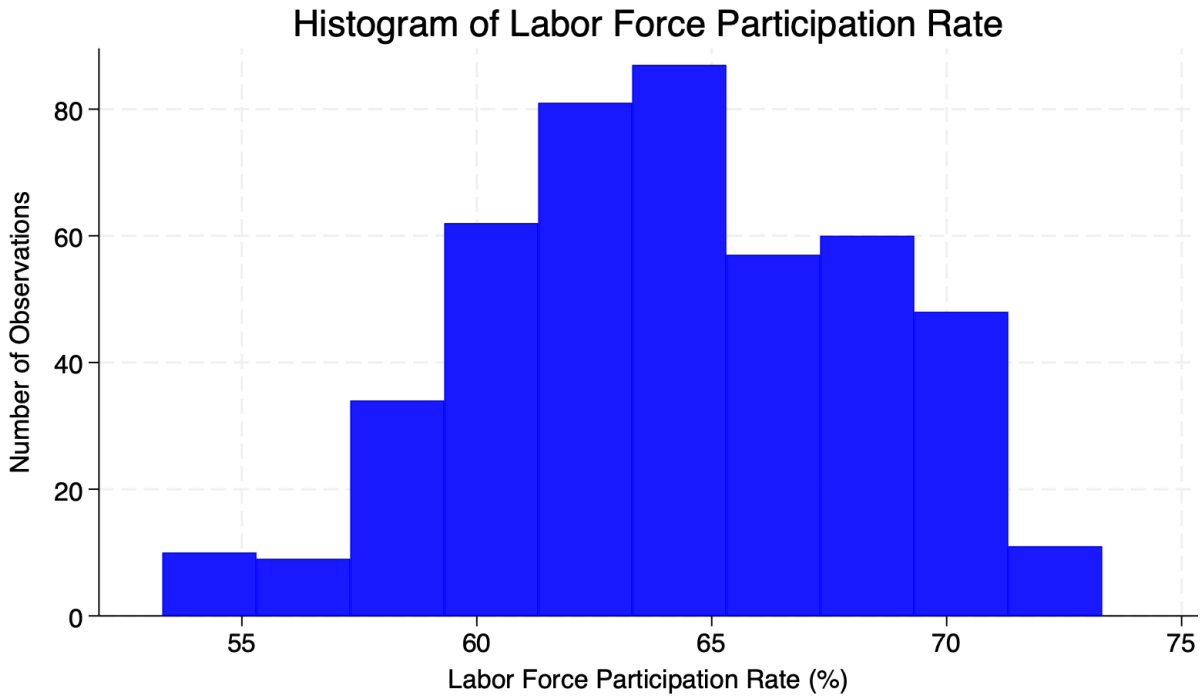
Definition: Percentage of the working-age population either employed or actively seeking employment in low-wage industries in each state and year.

Possible Values: Continuous variable; typically between 50% and 75%

Frequency Table:

Variable	Obs	Mean	Std. dev.	Min	Max
labor_forc~e	459	64.15203	4.062383	53.30833	72.975

Histogram showing percent frequency distribution:



Variable Name: unemployment_rate

Missing Observations: 0/459

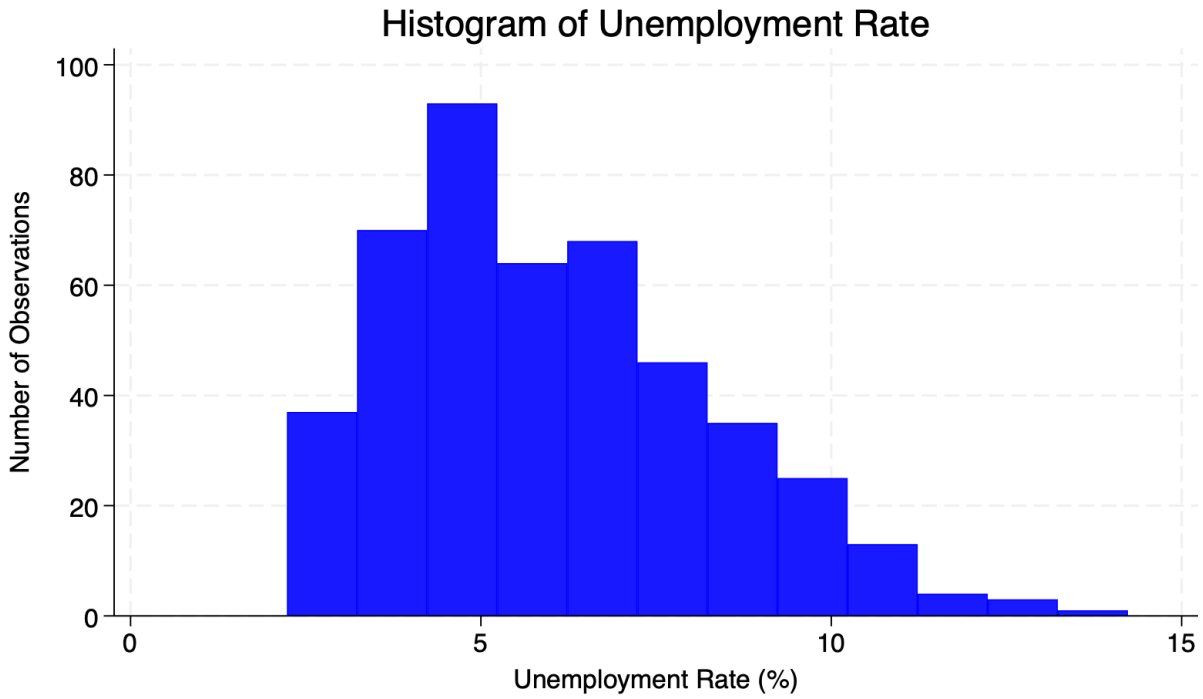
Definition: Percentage of the labor force in low-wage industries that is unemployed and actively seeking work, in each state and year.

Possible Values: Continuous variable; typically between 40% and 70%

Frequency Table:

Variable	Obs	Mean	Std. dev.	Min	Max
unemployme~e	459	6.029158	2.220622	2.233333	13.25

Histogram showing percent frequency distribution:



XI. Tables and Figures

A. Tables

Table (1) - Selected CPS Variables Extracted from IPUMS (2010–2018)

Category	Variable	Description
Identification	YEAR	Survey Year
Identification	MONTH	Survey Month
Identification	STATEFIP	State of residence (FIPS code)
Demographics	AGE	Age of respondent
Demographics	SEX	Gender of respondent
Demographics	RACE	Race / ethnicity
Demographics	EDUC	Educational attainment

Employment & Labor Force	EMPSTAT	Employment status
Employment & Labor Force	CLASSWKR	Class of worker
Employment & Labor Force	OCC	Occupation code
Employment & Labor Force	IND	Industry code
Employment & Labor Force	UHRSWORKT	Usual hours worked per week
Employment & Labor Force	WKSTAT	Full vs part-time employment
Wage & Earnings	HOURWAGE	Reported hourly wage
Wage & Earnings	PAIDHOUR	Indicator for hourly pay status
Wage & Earnings	EARNWEEK	Reported weekly earnings
Wage & Earnings	INCWAGE	Total wage/salary income
Wage & Earnings	EARNWT	Earnings weight for wage analysis

Table (2) - Key Variables in Final State-Year Panel Dataset and Data Sources

Variable	Description	Source
state	State name	CPS / IPUMS
year	Survey year	CPS / IPUMS
state_min_wage	Real (inflation-adjusted) statutory minimum wages in dollars	FRED
employment_population_ratio	Share of working-age population (16–64) employed in low-wage industries	CPS (Weighted Aggregate)
labor_force_participation_rate	Share of working-age population (16–64) in the labor force in low-wage industries	CPS (Weighted Aggregate)
unemployment_rate	Share of the labor force unemployed in low-wage industries	CPS (Weighted Aggregate)
state_unemployment_rate	Annual average unemployment rate	BLS LAUS
state_gdp_per_capita	State GDP per capita	BEA

Table (3) - Summary Statistics for Final State-Year Panel Dataset

Variable	Mean	SD	Min	Max
year	2014	2.585	2010	2018
income_	292,199.846	356,813.876	25,988.7	2,411,055.1
disposable_income_	258,057.958	310,032.082	23,828.5	2,057,748.6
state_min_wage	7.764	1.107	5.15	13.25
unemployment_rate	6.029	2.221	2.233	13.25
labor_force_participation_rate	64.152	4.062	53.308	72.975
employment_population_ratio	60.301	4.342	50.042	70.65

Table (4) – Event Study Coefficients for Employment-to-Population Ratio

Years Relative to Treatment	ATT Estimate	Standard Error
-4.0	0.053	0.327
-3.0	0.156	0.211
-2.0	0.201	0.172
-1.0	0.299	0.189
0.0	0.027	0.226
1.0	0.25	0.396
2.0	-0.769	0.88

Table (5) – ATT Summary Table for Employment-to-Population Ratio

	(1)
	b/se
g2011	
t_2010_2011	0.000
	(.)
t_2010_2012	0.000
	(.)
t_2010_2013	0.000
	(.)
t_2010_2014	0.000
	(.)
t_2010_2015	0.000
	(.)
t_2010_2016	0.000
	(.)
t_2010_2017	0.000
	(.)
t_2010_2018	0.000
	(.)
g2014	
t_2010_2011	0.233
	(0.415)
t_2011_2012	0.361
	(0.367)
t_2012_2013	0.541
	(0.300)
t_2013_2014	-0.250
	(0.453)
t_2013_2015	-0.433
	(0.811)
t_2013_2016	-0.769
	(0.907)
t_2013_2017	0.000
	(.)
t_2013_2018	0.000
	(.)
g2015	
t_2010_2011	0.183
	(0.332)
t_2011_2012	0.096
	(0.175)
t_2012_2013	0.058
	(0.179)
t_2013_2014	0.089
	(0.190)
t_2014_2015	0.179
	(0.205)
t_2014_2016	-0.108

	(0.292)
t_2014_2017	0.000
	(.)
t_2014_2018	0.000
	(.)
---	
g2016	
t_2010_2011	0.000
	(.)
t_2011_2012	-1.114
	(0.046)
t_2012_2013	0.000
	(.)
t_2013_2014	0.358
	(0.083)
t_2014_2015	0.492
	(0.095)
t_2015_2016	0.593
	(0.023)
t_2015_2017	0.000
	(.)
t_2015_2018	0.000
	(.)
---	
g2017	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2016_2018	0.000
	(.)
---	
g2018	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)

t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2017_2018	0.000
	(.)
---	
wgt	
w2011_2011	0.000
	(.)
w2011_2012	0.000
	(.)
w2011_2013	0.000
	(.)
w2011_2014	0.000
	(.)
w2011_2015	0.000
	(.)
w2011_2016	0.000
	(.)
w2011_2017	0.000
	(.)
w2011_2018	0.000
	(.)
w2014_2011	0.070
	(0.022)
w2014_2012	0.070
	(0.022)
w2014_2013	0.070
	(0.022)
w2014_2014	0.070
	(0.022)
w2014_2015	0.070
	(0.022)
w2014_2016	0.070
	(0.022)
w2014_2017	0.000
	(.)
w2014_2018	0.000
	(.)
w2015_2011	0.090
	(0.026)
w2015_2012	0.090
	(0.026)
w2015_2013	0.090
	(0.026)
w2015_2014	0.090
	(0.026)
w2015_2015	0.090
	(0.026)
w2015_2016	0.090
	(0.026)
w2015_2017	0.000

	(.)
w2015_2018	0.000
	(.)
w2016_2011	0.000
	(.)
w2016_2012	0.010
	(0.015)
w2016_2013	0.000
	(.)
w2016_2014	0.010
	(0.015)
w2016_2015	0.010
	(0.015)
w2016_2016	0.010
	(0.015)
w2016_2017	0.000
	(.)
w2016_2018	0.000
	(.)
w2017_2011	0.000
	(.)
w2017_2012	0.000
	(.)
w2017_2013	0.000
	(.)
w2017_2014	0.000
	(.)
w2017_2015	0.000
	(.)
w2017_2016	0.000
	(.)
w2017_2017	0.000
	(.)
w2017_2018	0.000
	(.)
w2018_2011	0.000
	(.)
w2018_2012	0.000
	(.)
w2018_2013	0.000
	(.)
w2018_2014	0.000
	(.)
w2018_2015	0.000
	(.)
w2018_2016	0.000
	(.)
w2018_2017	0.000
	(.)
w2018_2018	0.000
	(.)

Table (6) – Event Study Coefficients for Labor Force Participation Rate

Years Relative to Treatment	ATT Estimate	Standard Error	Z	P-Value
-4.0	0.265	0.322	0.822	0.411
-3.0	0.275	0.189	1.457	0.145
-2.0	0.386	0.171	2.258	0.024
-1.0	0.498	0.263	1.894	0.058
0.0	0.106	0.232	0.459	0.646
1.0	-0.328	0.521	-0.63	0.528
2.0	-1.407	1.408	-1.0	0.317

Table (7) – ATT Summary Table for Labor Force Participation Rate

	(1)
g2011	
t_2010_2011	0.000
	(.)
t_2010_2012	0.000
	(.)
t_2010_2013	0.000
	(.)
t_2010_2014	0.000
	(.)
t_2010_2015	0.000
	(.)
t_2010_2016	0.000
	(.)
t_2010_2017	0.000
	(.)
t_2010_2018	0.000
	(.)
g2014	

t_2010_2011	0.353
	(0.359)
t_2011_2012	0.501
	(0.394)
t_2012_2013	0.819
	(0.517)
t_2013_2014	-0.211
	(0.463)
t_2013_2015	-0.827
	(1.276)
t_2013_2016	-1.407
	(1.539)
t_2013_2017	0.000
	(.)
t_2013_2018	0.000
	(.)
---	---
g2015	
t_2010_2011	0.419
	(0.298)
t_2011_2012	0.214
	(0.224)
t_2012_2013	0.310
	(0.209)
t_2013_2014	0.260
	(0.225)
t_2014_2015	0.299
	(0.233)
t_2014_2016	0.060
	(0.257)
t_2014_2017	0.000
	(.)
t_2014_2018	0.000
	(.)
---	---
g2016	
t_2010_2011	0.000
	(.)

t_2011_2012	-1.123***
	(0.091)
t_2012_2013	0.000
	(.)
t_2013_2014	0.272***
	(0.105)
t_2014_2015	0.399*
	(0.217)
t_2015_2016	0.602***
	(0.035)
t_2015_2017	0.000
	(.)
t_2015_2018	0.000
	(.)
---	---
g2017	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2016_2018	0.000
	(.)
---	---
g2018	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)

t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2017_2018	0.000
	(.)
---	
wgt	
w2011_2011	0.000
	(.)
w2011_2012	0.000
	(.)
w2011_2013	0.000
	(.)
w2011_2014	0.000
	(.)
w2011_2015	0.000
	(.)
w2011_2016	0.000
	(.)
w2011_2017	0.000
	(.)
w2011_2018	0.000
	(.)
w2014_2011	0.070***
	(0.022)
w2014_2012	0.070***
	(0.022)
w2014_2013	0.070***
	(0.022)
w2014_2014	0.070***

	(0.022)
w2014_2015	0.070***
	(0.022)
w2014_2016	0.070***
	(0.022)
w2014_2017	0.000
	(.)
w2014_2018	0.000
	(.)
w2015_2011	0.090***
	(0.023)
w2015_2012	0.090***
	(0.023)
w2015_2013	0.090***
	(0.023)
w2015_2014	0.090***
	(0.023)
w2015_2015	0.090***
	(0.023)
w2015_2016	0.090***
	(0.023)
w2015_2017	0.000
	(.)
w2015_2018	0.000
	(.)
w2016_2011	0.000
	(.)
w2016_2012	0.010
	(0.013)
w2016_2013	0.000
	(.)
w2016_2014	0.010
	(0.013)
w2016_2015	0.010
	(0.013)

w2016_2016	0.010
	(0.013)
w2016_2017	0.000
	(.)
w2016_2018	0.000
	(.)
w2017_2011	0.000
	(.)
w2017_2012	0.000
	(.)
w2017_2013	0.000
	(.)
w2017_2014	0.000
	(.)
w2017_2015	0.000
	(.)
w2017_2016	0.000
	(.)
w2017_2017	0.000
	(.)
w2017_2018	0.000
	(.)
w2018_2011	0.000
	(.)
w2018_2012	0.000
	(.)
w2018_2013	0.000
	(.)
w2018_2014	0.000
	(.)
w2018_2015	0.000
	(.)
w2018_2016	0.000
	(.)
w2018_2017	0.000
	(.)

w2018_2018 0.000
(.)

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Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table (8) – Event Study Coefficients for Unemployment Rate

Years Relative to Treatment	ATT Estimate	Standard Error
-4.0	-0.441	0.065
-3.0	0.324	0.111
-2.0	0.131	0.177
-1.0	0.23	0.14
0.0	0.239	0.302
1.0	0.127	0.228
2.0	-0.02	0.517

Table (9) – ATT Summary Table for Unemployment Rate

---
(1)

b/se
---
g2011
t_2010_2011 0.235
(0.097)
t_2010_2012 -0.540
(0.088)
t_2010_2013 -1.547
(0.118)
t_2010_2014 -2.529
(0.233)
t_2010_2015 -2.441
(0.265)
t_2010_2016 -3.152
(0.334)
t_2010_2017 0.000
(.)
t_2010_2018 0.000
(.)
---

g2014
t_2010_2011 0.163
(0.425)
t_2011_2012 0.160
(0.309)
t_2012_2013 0.291
(0.580)
t_2013_2014 0.077
(0.412)
t_2013_2015 -0.213
(1.164)
t_2013_2016 -0.272
(1.340)
t_2013_2017 0.000
(.)
t_2013_2018 0.000
(.)
---
g2015
t_2010_2011 0.360
(0.134)
t_2011_2012 0.137
(0.105)
t_2012_2013 0.324
(0.221)
t_2013_2014 0.267
(0.217)
t_2014_2015 0.194
(0.242)
t_2014_2016 0.187
(0.380)
t_2014_2017 0.000
(.)
t_2014_2018 0.000
(.)
---
g2016
t_2010_2011 -0.441
(0.084)
t_2011_2012 0.009
(0.038)
t_2012_2013 -0.150
(0.080)
t_2013_2014 -0.121
(0.238)
t_2014_2015 -0.369
(0.198)
t_2015_2016 -0.226
(0.064)
t_2015_2017 0.000
(.)
t_2015_2018 0.000
(.)

g2017	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2016_2018	0.000
	(.)
g2018	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2017_2018	0.000
	(.)
wgt	
w2011_2011	0.009
	(0.014)
w2011_2012	0.009
	(0.014)
w2011_2013	0.009
	(0.014)
w2011_2014	0.009
	(0.014)
w2011_2015	0.009
	(0.014)
w2011_2016	0.009
	(0.014)
w2011_2017	0.000
	(.)
w2011_2018	0.000

	(.)
w2014_2011	0.065
	(0.023)
w2014_2012	0.065
	(0.023)
w2014_2013	0.065
	(0.023)
w2014_2014	0.065
	(0.023)
w2014_2015	0.065
	(0.023)
w2014_2016	0.065
	(0.023)
w2014_2017	0.000
	(.)
w2014_2018	0.000
	(.)
w2015_2011	0.083
	(0.021)
w2015_2012	0.083
	(0.021)
w2015_2013	0.083
	(0.021)
w2015_2014	0.083
	(0.021)
w2015_2015	0.083
	(0.021)
w2015_2016	0.083
	(0.021)
w2015_2017	0.000
	(.)
w2015_2018	0.000
	(.)
w2016_2011	0.009
	(0.013)
w2016_2012	0.009
	(0.013)
w2016_2013	0.009
	(0.013)
w2016_2014	0.009
	(0.013)
w2016_2015	0.009
	(0.013)
w2016_2016	0.009
	(0.013)
w2016_2017	0.000
	(.)
w2016_2018	0.000
	(.)
w2017_2011	0.000
	(.)
w2017_2012	0.000
	(.)

w2017_2013	0.000
	(.)
w2017_2014	0.000
	(.)
w2017_2015	0.000
	(.)
w2017_2016	0.000
	(.)
w2017_2017	0.000
	(.)
w2017_2018	0.000
	(.)
w2018_2011	0.000
	(.)
w2018_2012	0.000
	(.)
w2018_2013	0.000
	(.)
w2018_2014	0.000
	(.)
w2018_2015	0.000
	(.)
w2018_2016	0.000
	(.)
w2018_2017	0.000
	(.)
w2018_2018	0.000
	(.)

Table (10) – ATT Summary Table for Employment-to-Population Ratio (No Controls)

	(1)
g2011	
t_2010_2011	0.000
	(.)
t_2010_2012	0.000
	(.)
t_2010_2013	0.000
	(.)
t_2010_2014	0.000
	(.)
t_2010_2015	0.000
	(.)
t_2010_2016	0.000

	(.)
t_2010_2017	0.000
	(.)
t_2010_2018	0.000
	(.)
---	---
g2014	
t_2010_2011	0.233
	(0.460)
t_2011_2012	0.361
	(0.366)
t_2012_2013	0.541*
	(0.314)
t_2013_2014	-0.250
	(0.421)
t_2013_2015	-0.433
	(0.877)
t_2013_2016	-0.769
	(0.922)
t_2013_2017	0.000
	(.)
t_2013_2018	0.000
	(.)
---	---
g2015	
t_2010_2011	0.183
	(0.326)
t_2011_2012	0.096
	(0.172)
t_2012_2013	0.058
	(0.195)
t_2013_2014	0.089
	(0.197)
t_2014_2015	0.179
	(0.216)
t_2014_2016	-0.108
	(0.284)
t_2014_2017	0.000

	(.)
t_2014_2018	0.000
	(.)
---	---
g2016	
t_2010_2011	0.000
	(.)
t_2011_2012	-1.114***
	(0.056)
t_2012_2013	0.000
	(.)
t_2013_2014	0.358***
	(0.083)
t_2014_2015	0.492***
	(0.099)
t_2015_2016	0.593***
	(0.023)
t_2015_2017	0.000
	(.)
t_2015_2018	0.000
	(.)
---	---
g2017	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2016_2018	0.000

	(.)
g2018	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2017_2018	0.000
	(.)
wgt	
w2011_2011	0.000
	(.)
w2011_2012	0.000
	(.)
w2011_2013	0.000
	(.)
w2011_2014	0.000
	(.)
w2011_2015	0.000
	(.)
w2011_2016	0.000
	(.)
w2011_2017	0.000
	(.)
w2011_2018	0.000
	(.)
w2014_2011	0.070***

	(0.022)
w2014_2012	0.070***
	(0.022)
w2014_2013	0.070***
	(0.022)
w2014_2014	0.070***
	(0.022)
w2014_2015	0.070***
	(0.022)
w2014_2016	0.070***
	(0.022)
w2014_2017	0.000
	(.)
w2014_2018	0.000
	(.)
w2015_2011	0.090***
	(0.023)
w2015_2012	0.090***
	(0.023)
w2015_2013	0.090***
	(0.023)
w2015_2014	0.090***
	(0.023)
w2015_2015	0.090***
	(0.023)
w2015_2016	0.090***
	(0.023)
w2015_2017	0.000
	(.)
w2015_2018	0.000
	(.)
w2016_2011	0.000
	(.)
w2016_2012	0.010
	(0.014)

w2016_2013	0.000
	(.)
w2016_2014	0.010
	(0.014)
w2016_2015	0.010
	(0.014)
w2016_2016	0.010
	(0.014)
w2016_2017	0.000
	(.)
w2016_2018	0.000
	(.)
w2017_2011	0.000
	(.)
w2017_2012	0.000
	(.)
w2017_2013	0.000
	(.)
w2017_2014	0.000
	(.)
w2017_2015	0.000
	(.)
w2017_2016	0.000
	(.)
w2017_2017	0.000
	(.)
w2017_2018	0.000
	(.)
w2018_2011	0.000
	(.)
w2018_2012	0.000
	(.)
w2018_2013	0.000
	(.)
w2018_2014	0.000
	(.)

w2018_2015	0.000
	(.)
w2018_2016	0.000
	(.)
w2018_2017	0.000
	(.)
w2018_2018	0.000
	(.)
Observations	154

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table (11) – ATT Summary Table for Labor Force Participation Rate (No Controls)

	(1)
g2011	
t_2010_2011	-0.048
	(0.100)
t_2010_2012	-0.127
	(0.162)
t_2010_2013	-0.330
	(0.236)
t_2010_2014	-0.433
	(0.266)
t_2010_2015	0.233
	(0.295)
t_2010_2016	-0.560*
	(0.318)
t_2010_2017	0.450
	(.)
t_2010_2018	0.000
	(.)
g2014	
t_2010_2011	-0.101
	(0.220)

t_2011_2012	0.603**
	(0.246)
t_2012_2013	0.377
	(0.244)
t_2013_2014	-0.033
	(0.212)
t_2013_2015	0.538
	(0.368)
t_2013_2016	0.611
	(0.424)
t_2013_2017	1.204***
	(0.420)
t_2013_2018	0.000
	(.)
---	---
g2015	
t_2010_2011	0.335
	(0.225)
t_2011_2012	0.307
	(0.247)
t_2012_2013	0.380*
	(0.230)
t_2013_2014	0.182
	(0.203)
t_2014_2015	0.192
	(0.204)
t_2014_2016	-0.061
	(0.304)
t_2014_2017	0.456
	(0.283)
t_2014_2018	0.000
	(.)
---	---
g2016	
t_2010_2011	0.375
	(0.246)
t_2011_2012	-0.913***
	(0.167)

t_2012_2013 -0.987***
(0.088)

t_2013_2014 0.432***
(0.083)

t_2014_2015 0.373
(0.270)

t_2015_2016 0.510***
(0.080)

t_2015_2017 0.967
(.)

t_2015_2018 0.000
(.)

---

g2017

t_2010_2011 -0.073
(0.332)

t_2011_2012 0.110
(0.227)

t_2012_2013 0.171
(0.116)

t_2013_2014 0.044
(0.108)

t_2014_2015 0.148
(0.387)

t_2015_2016 0.435***
(0.061)

t_2016_2017 0.379***
(0.086)

t_2016_2018 0.000
(.)

---

g2018

t_2010_2011 0.000
(.)

t_2011_2012 0.000
(.)

t_2012_2013 0.000
(.)

t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)
t_2017_2018	0.000
	(.)
wgt	
w2011_2011	0.006
	(0.009)
w2011_2012	0.006
	(0.009)
w2011_2013	0.006
	(0.009)
w2011_2014	0.006
	(0.009)
w2011_2015	0.006
	(0.009)
w2011_2016	0.006
	(0.009)
w2011_2017	0.006
	(0.009)
w2011_2018	0.000
	(.)
w2014_2011	0.045***
	(0.014)
w2014_2012	0.045***
	(0.014)
w2014_2013	0.045***
	(0.014)
w2014_2014	0.045***
	(0.014)
w2014_2015	0.045***

	(0.014)
w2014_2016	0.045***
	(0.014)
w2014_2017	0.045***
	(0.014)
w2014_2018	0.000
	(.)
w2015_2011	0.058***
	(0.015)
w2015_2012	0.058***
	(0.015)
w2015_2013	0.058***
	(0.015)
w2015_2014	0.058***
	(0.015)
w2015_2015	0.058***
	(0.015)
w2015_2016	0.058***
	(0.015)
w2015_2017	0.058***
	(0.015)
w2015_2018	0.000
	(.)
w2016_2011	0.006
	(0.009)
w2016_2012	0.006
	(0.009)
w2016_2013	0.006
	(0.009)
w2016_2014	0.006
	(0.009)
w2016_2015	0.006
	(0.009)
w2016_2016	0.006
	(0.009)

w2016_2017	0.006
	(0.009)
w2016_2018	0.000
	(.)
w2017_2011	0.026**
	(0.012)
w2017_2012	0.026**
	(0.012)
w2017_2013	0.026**
	(0.012)
w2017_2014	0.026**
	(0.012)
w2017_2015	0.026**
	(0.012)
w2017_2016	0.026**
	(0.012)
w2017_2017	0.026**
	(0.012)
w2017_2018	0.000
	(.)
w2018_2011	0.000
	(.)
w2018_2012	0.000
	(.)
w2018_2013	0.000
	(.)
w2018_2014	0.000
	(.)
w2018_2015	0.000
	(.)
w2018_2016	0.000
	(.)
w2018_2017	0.000
	(.)
w2018_2018	0.000
	(.)

Observations 184

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table (12) – ATT Summary Table for Unemployment Rate (No Controls)

---
(1)

---
g2011
t_2010_2011 0.323***
(0.107)

t_2010_2012 -0.464***
(0.154)

t_2010_2013 -1.380***
(0.192)

t_2010_2014 -2.415***
(0.264)

t_2010_2015 -2.044***
(0.315)

t_2010_2016 -2.752***
(0.144)

t_2010_2017 -4.983
(.)

t_2010_2018 0.000
(.)
---
g2014
t_2010_2011 -0.219
(0.280)

t_2011_2012 0.010
(0.223)

t_2012_2013 -0.125
(0.160)

t_2013_2014 -0.397**
(0.192)

t_2013_2015 -0.373
(0.394)

t_2013_2016	-0.406
	(0.517)
t_2013_2017	-1.599***
	(0.400)
t_2013_2018	0.000
	(.)
---	
g2015	
t_2010_2011	0.341***
	(0.131)
t_2011_2012	0.206**
	(0.097)
t_2012_2013	0.505**
	(0.211)
t_2013_2014	0.331
	(0.228)
t_2014_2015	0.194
	(0.211)
t_2014_2016	0.184
	(0.350)
t_2014_2017	-0.490
	(0.316)
t_2014_2018	0.000
	(.)
---	
g2016	
t_2010_2011	-0.450***
	(0.069)
t_2011_2012	0.047
	(0.124)
t_2012_2013	-0.053
	(0.244)
t_2013_2014	-0.087
	(0.190)
t_2014_2015	-0.370*
	(0.198)
t_2015_2016	-0.230***
	(0.076)

t_2015_2017	-0.867
	(.)
t_2015_2018	0.000
	(.)
---	
g2017	
t_2010_2011	0.198***
	(0.058)
t_2011_2012	-0.225*
	(0.121)
t_2012_2013	-0.673**
	(0.297)
t_2013_2014	-0.642***
	(0.121)
t_2014_2015	-0.475**
	(0.197)
t_2015_2016	-0.160***
	(0.041)
t_2016_2017	-0.325***
	(0.017)
t_2016_2018	0.000
	(.)
---	
g2018	
t_2010_2011	0.000
	(.)
t_2011_2012	0.000
	(.)
t_2012_2013	0.000
	(.)
t_2013_2014	0.000
	(.)
t_2014_2015	0.000
	(.)
t_2015_2016	0.000
	(.)
t_2016_2017	0.000
	(.)

t_2017_2018	0.000
	(.)
---	---
wgt	
w2011_2011	0.006
	(0.009)
w2011_2012	0.006
	(0.009)
w2011_2013	0.006
	(0.009)
w2011_2014	0.006
	(0.009)
w2011_2015	0.006
	(0.009)
w2011_2016	0.006
	(0.009)
w2011_2017	0.006
	(0.009)
w2011_2018	0.000
	(.)
w2014_2011	0.045***
	(0.015)
w2014_2012	0.045***
	(0.015)
w2014_2013	0.045***
	(0.015)
w2014_2014	0.045***
	(0.015)
w2014_2015	0.045***
	(0.015)
w2014_2016	0.045***
	(0.015)
w2014_2017	0.045***
	(0.015)
w2014_2018	0.000
	(.)
w2015_2011	0.058***

	(0.015)
w2015_2012	0.058***
	(0.015)
w2015_2013	0.058***
	(0.015)
w2015_2014	0.058***
	(0.015)
w2015_2015	0.058***
	(0.015)
w2015_2016	0.058***
	(0.015)
w2015_2017	0.058***
	(0.015)
w2015_2018	0.000
	(.)
w2016_2011	0.006
	(0.009)
w2016_2012	0.006
	(0.009)
w2016_2013	0.006
	(0.009)
w2016_2014	0.006
	(0.009)
w2016_2015	0.006
	(0.009)
w2016_2016	0.006
	(0.009)
w2016_2017	0.006
	(0.009)
w2016_2018	0.000
	(.)
w2017_2011	0.026**
	(0.013)
w2017_2012	0.026**
	(0.013)

w2017_2013	0.026**
	(0.013)
w2017_2014	0.026**
	(0.013)
w2017_2015	0.026**
	(0.013)
w2017_2016	0.026**
	(0.013)
w2017_2017	0.026**
	(0.013)
w2017_2018	0.000
	(.)
w2018_2011	0.000
	(.)
w2018_2012	0.000
	(.)
w2018_2013	0.000
	(.)
w2018_2014	0.000
	(.)
w2018_2015	0.000
	(.)
w2018_2016	0.000
	(.)
w2018_2017	0.000
	(.)
w2018_2018	0.000
	(.)
Observations	184

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

B. Figures

Figure (1) – Event Study Plot for Employment-to-Population Ratio

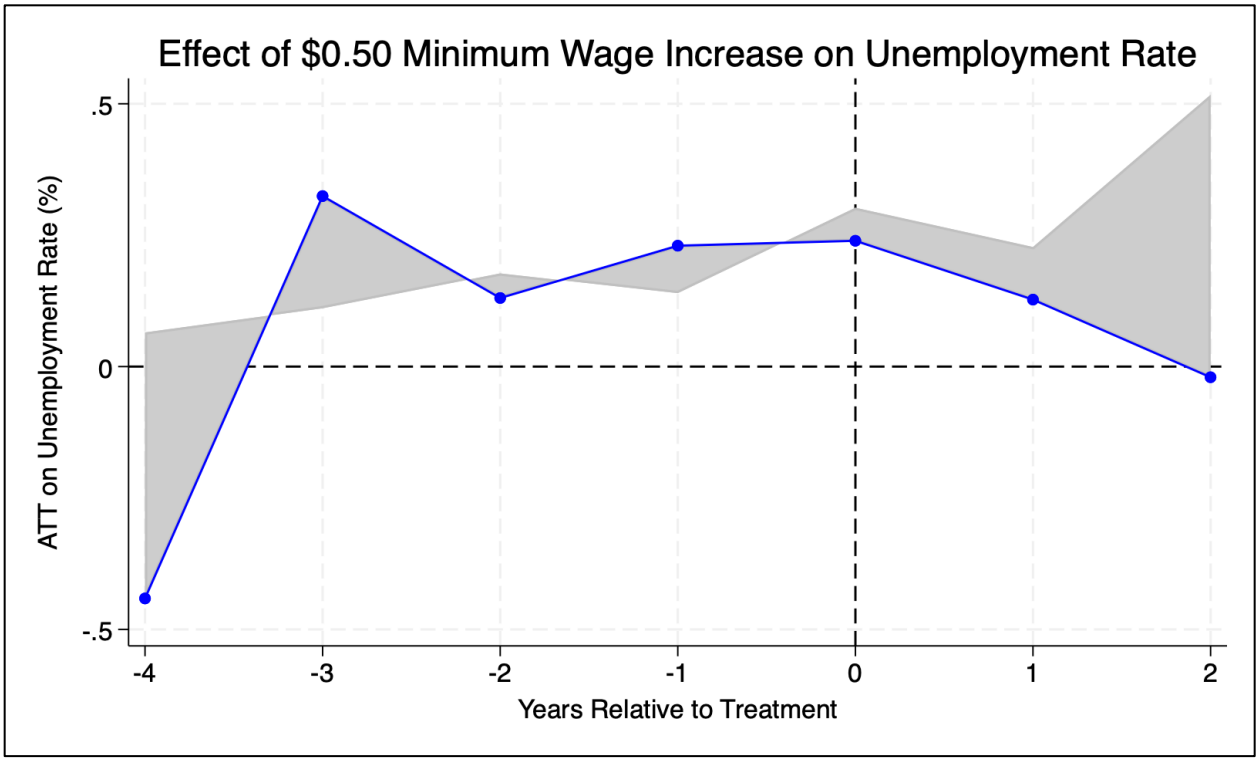


Figure (2) – Event Study Plot for Labor Force Participation Rate

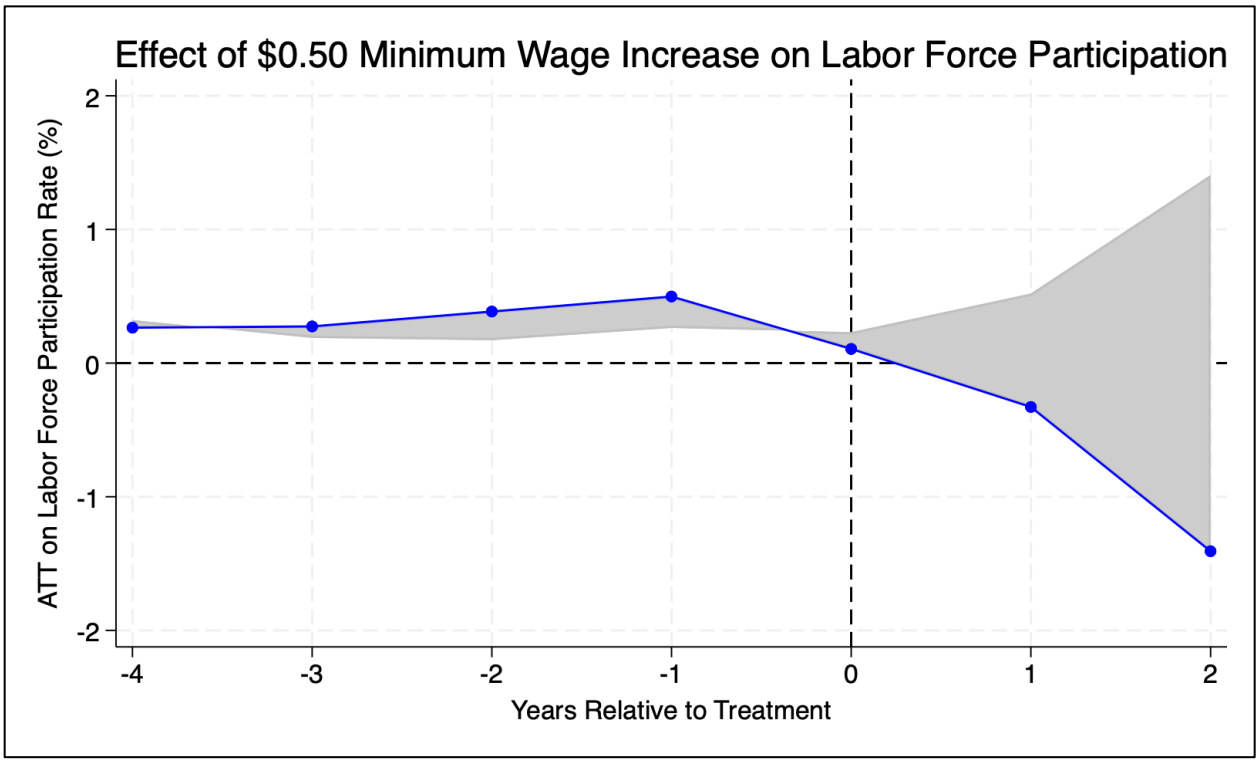


Figure (3) – Event Study Plot for Unemployment Rate

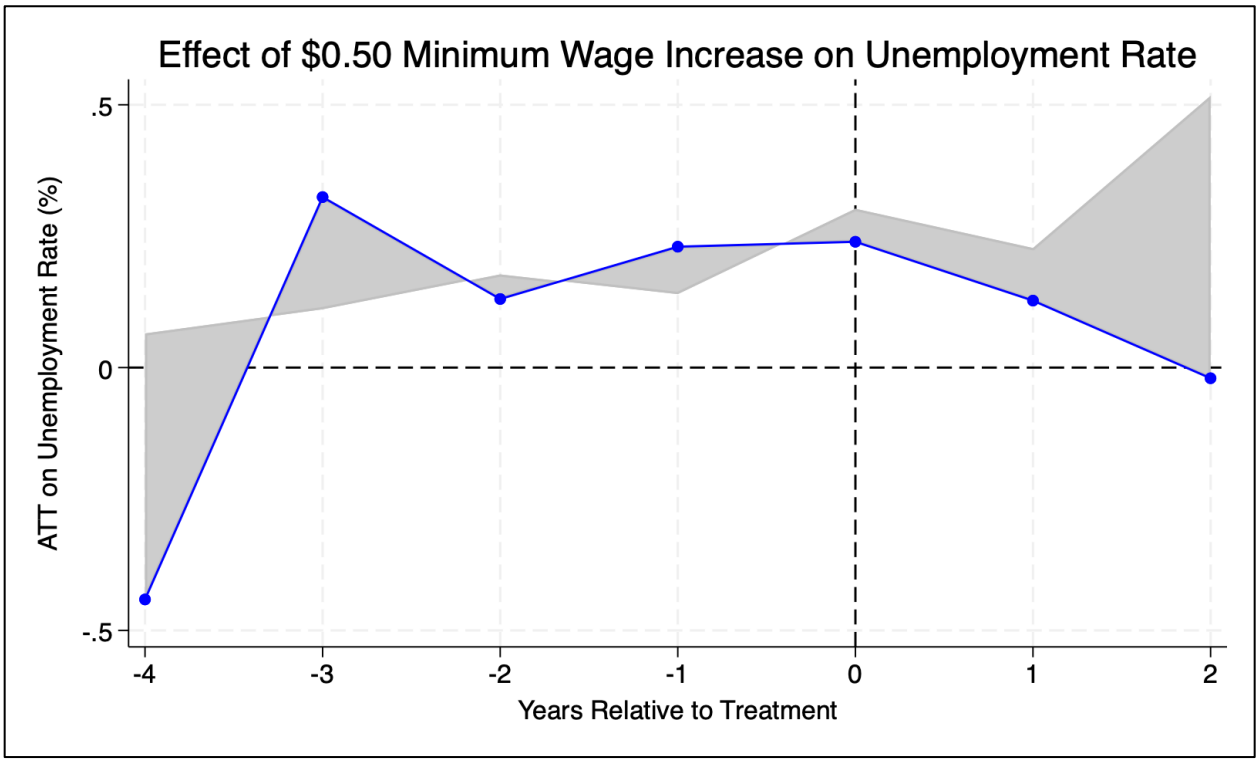


Figure (4) – Event Study Plot for Employment-to-Population Ratio (No Controls)

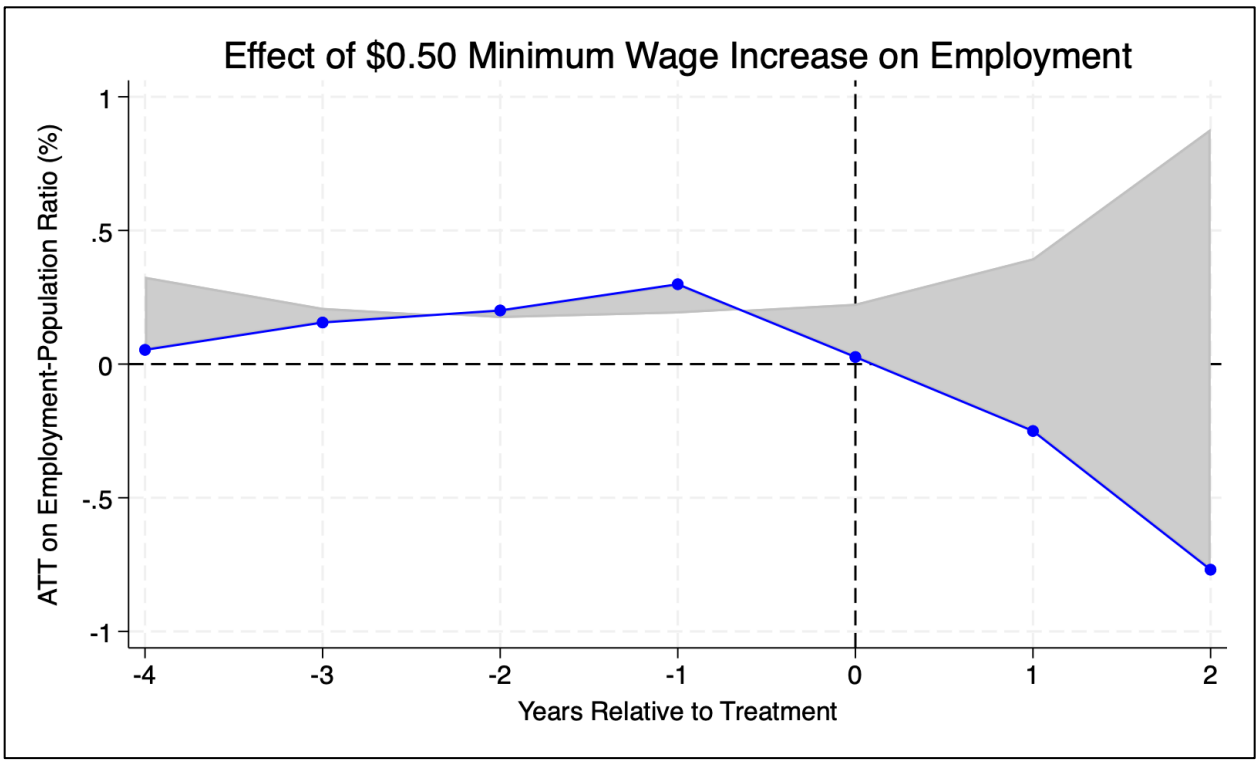


Figure (5) – Event Study Plot for Labor Force Participation Rate (No Controls)

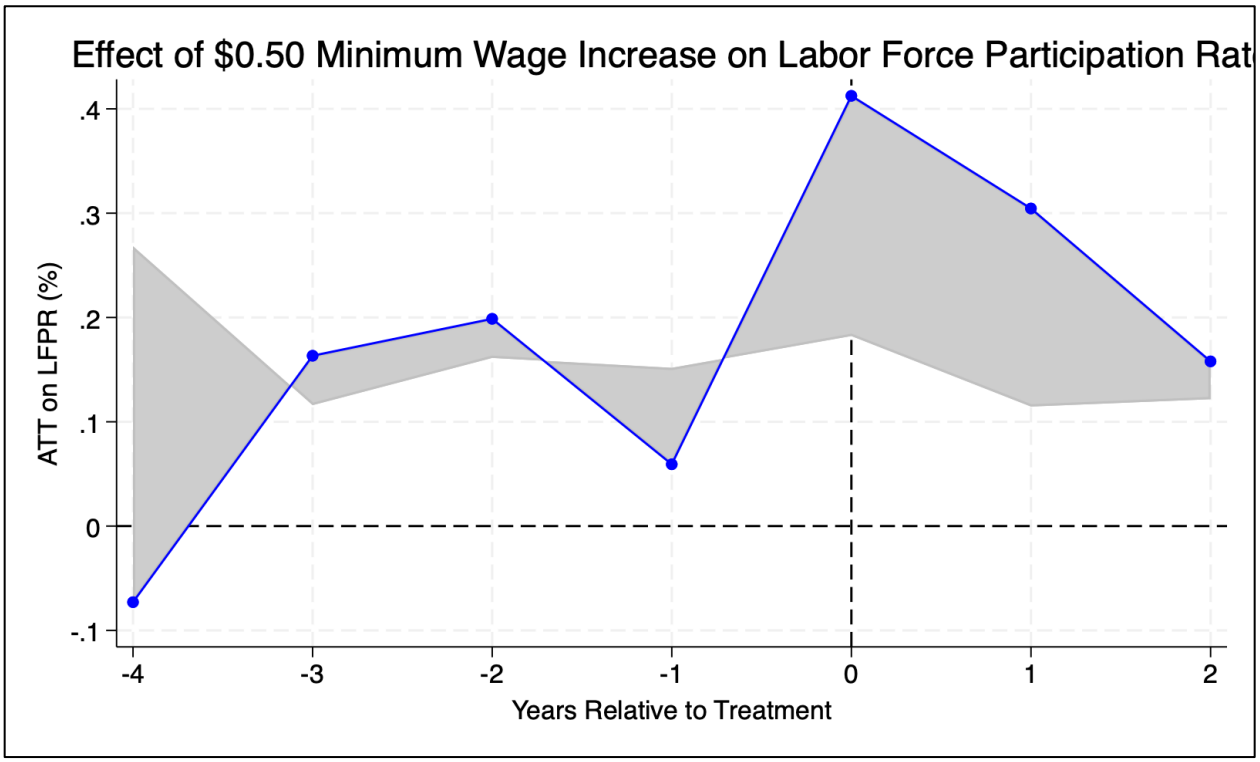


Figure (6) – Event Study Plot for Unemployment Rate (No Controls)

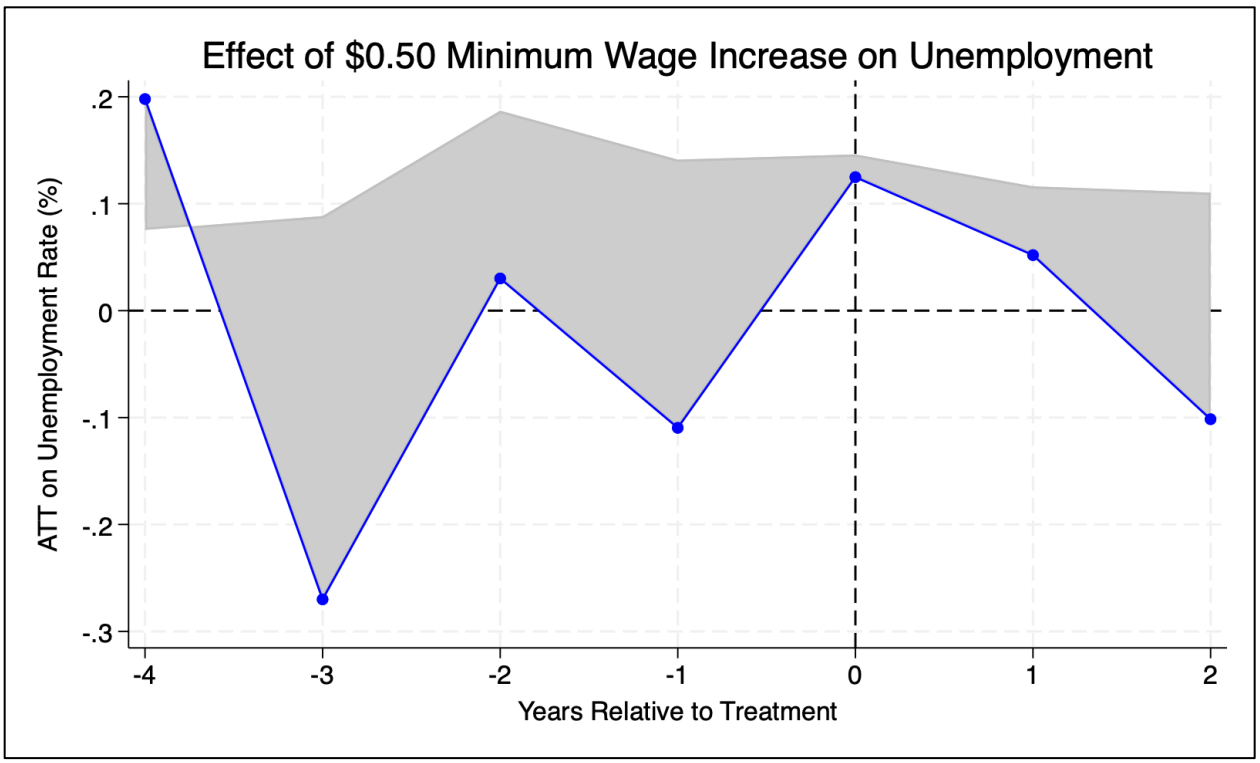


Figure (7) – Event Study Plot for .25 Cent Threshold

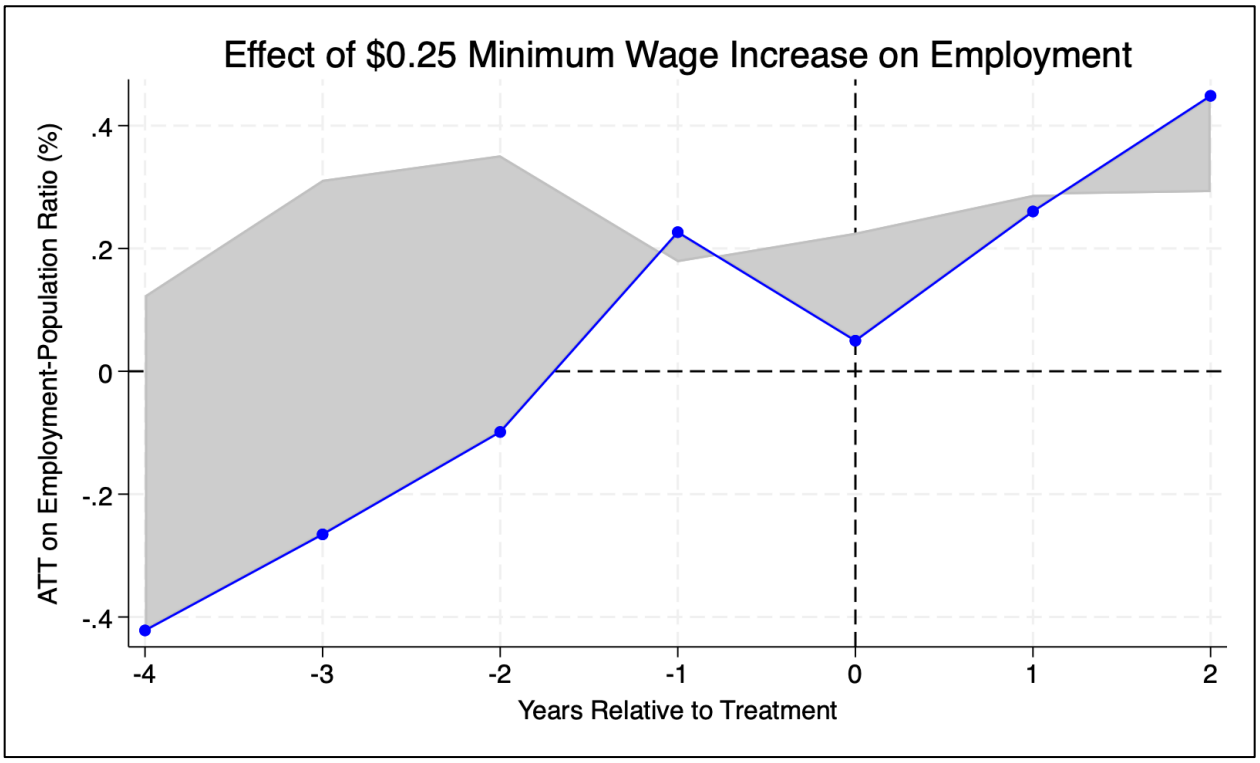


Figure (8) – Event Study Plot for .75 Cent Threshold

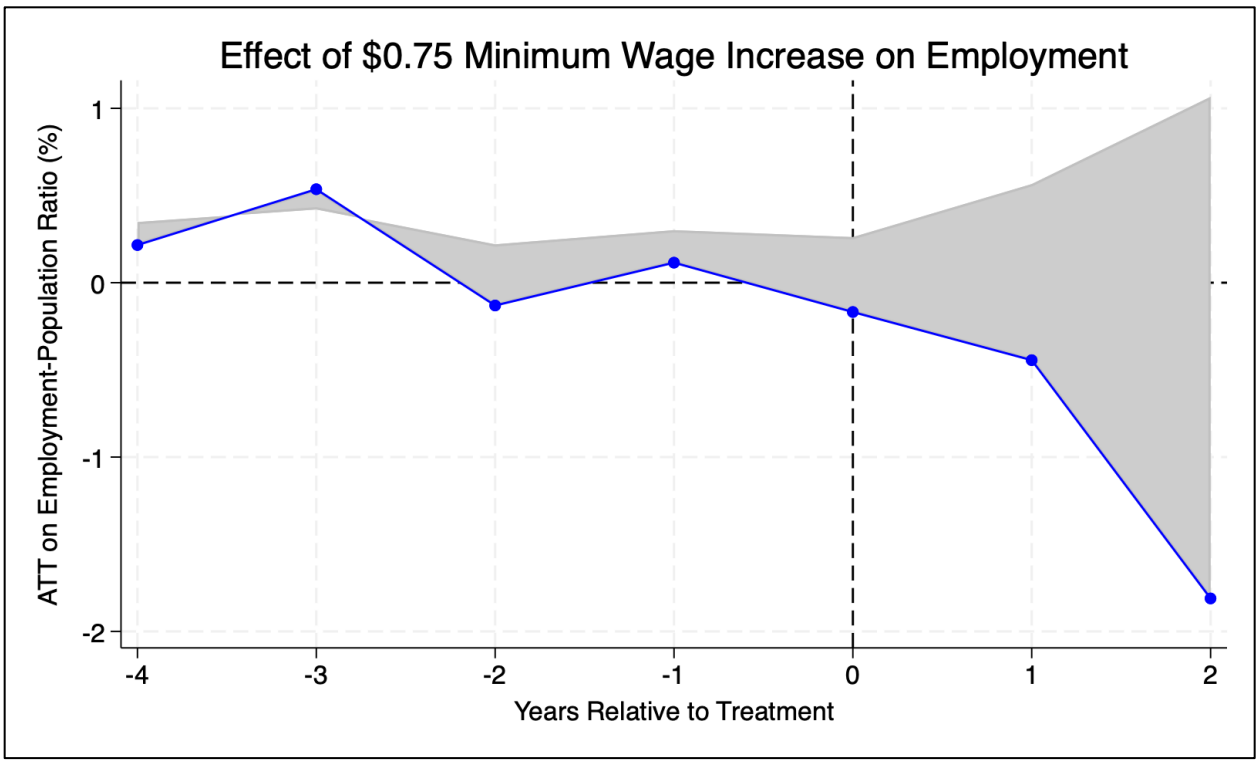
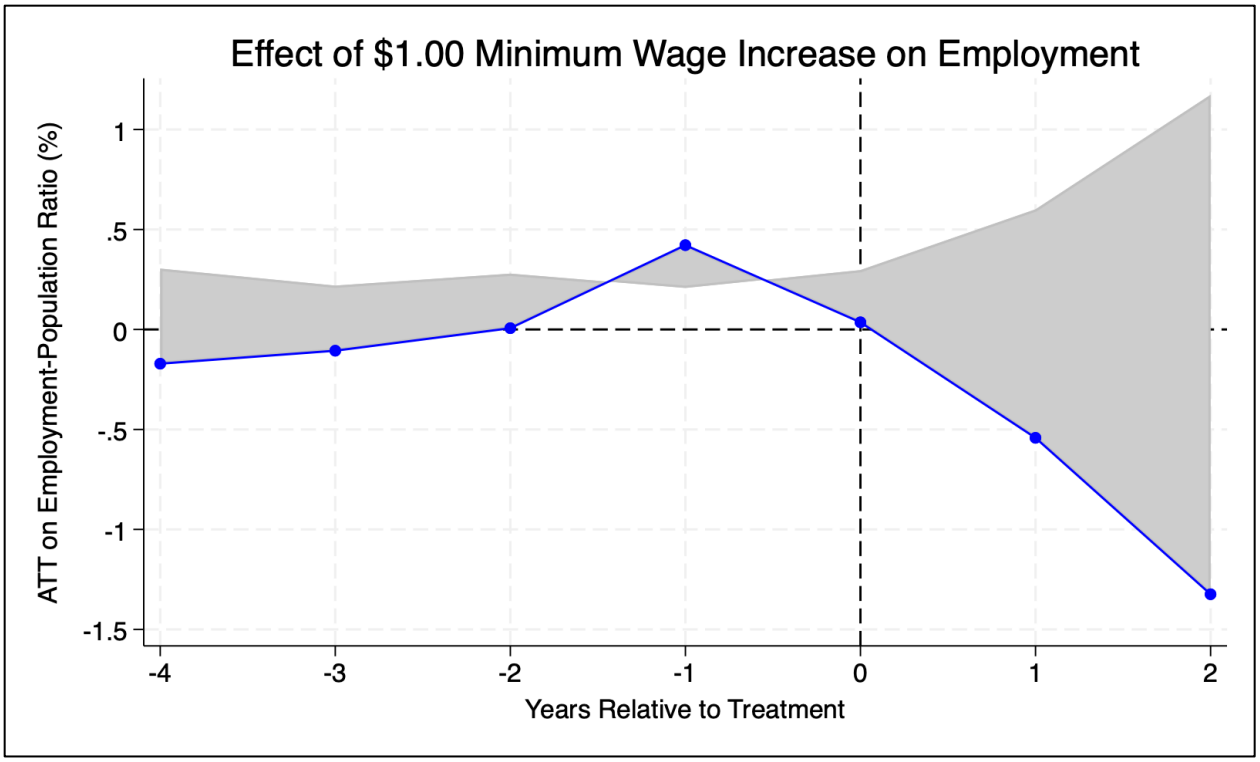


Figure (9) – Event Study Plot for 1 Dollar Threshold



XII. Citations

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