Minimum Wage Increases and Poverty Among Full-Time Low-Wage Workers: A Staggered Difference-in-Differences Analysis

Calvin T Burzynski

Denison University

DA 401: Seminar in Data Analytics

Dr. Zhe Wang

11/09/2025

Abstract

This study aims to investigate whether state minimum-wage increases between 2010 and 2023 reduced poverty among full-time low-wage workers in the United States. Using administrative minimum-wage schedules and microdata from the American Community Survey (ACS), Bureau of Labor Statistics (BLS) and Bureau of Economic Analysis (BEA), a state-year panel of the share of full-time workers (aged 18–64, working 35 hours or more per week and 50 or more weeks per year) living in poverty is constructed. Two-way fixed-effects regressions and Callaway & Sant'Anna's staggered difference-in-differences estimator are employed to compare states that raised their minimum wage above the federal floor of $7.25 per hour with never-treated states. Overall average treatment effects (ATTs) are about −2 percentage points, but their 95 % confidence intervals span zero. Dynamic event-study estimates show no systematic pre-treatment trends and modest negative effects two to five years after a wage hike, though the estimates are imprecise. The evidence suggests that minimum-wage increases alone provide, at best, a small poverty-reducing effect for full-time low-wage workers, underscoring the importance of complementary anti-poverty programs and more granular longitudinal research. Prior to the final paper, more analysis will be done to ensure valid and robust results.

Introduction

Minimum-wage laws have long been at the center of debates about economic fairness. The statutory wage floor sets a baseline for earnings, but its ability to reduce poverty is contested. Early research focused on employment effects, questioning whether raising wages would lead to job losses. Since Card & Krueger's (1994) seminal study, the conversation has shifted. Yet far less is known about whether higher minimum wages help full-time workers escape poverty. To address this gap, the following question should be answered: Do state minimum-wage increases reduce the share of full-time low-wage workers living in poverty? The analysis covers 2010–2023, a period spanning the recovery from the post-recovery period of the 2008 financial crisis, multiple policy changes and the COVID-19 pandemic. By exploiting the staggered timing of wage hikes across states and applying modern difference-in-differences estimators, I aim to provide rigorous evidence on the poverty impacts of minimum-wage policy.

Literature Review

Evidence on poverty impacts of minimum-wage policies is mixed. Sabia and Burkhauser (2010) analyzed state panels and argued that even a $9.50 federal minimum wage would do little to reduce poverty because most beneficiaries live above the poverty line. By contrast, Arranz (2025) used propensity-score difference-in-differences with Spanish household data and found that wage hikes increased income and reduced poverty, though effects varied by region. Winkler et al. (2025) reported that higher state minimum wages were associated with lower food insecurity among households with children, highlighting that material hardship may respond differently to wage policy. Narain (2019) linked minimum-wage increases to modest improvements in physical and mental health.

The effects of minimum-wage policy vary across states. Wang, Phillips & Su (2019) applied a fixed-effects grouping estimator to employment outcomes and discovered both positive and negative effects across clusters of states. Variation in state economies, cost of living, unionization and political orientation may shape the effectiveness of wage hikes. Evidence from border counties suggests that raising the minimum wage can increase earnings without reducing employment, pointing to the importance of local context. Studies of heterogeneity motivate the classification of states by economic and demographic characteristics.

Traditional two-way fixed-effects (TWFE) difference-in-differences models can yield biased estimates when treatment timing is staggered. Callaway & Sant'Anna (2021) proposed a doubly robust estimator for group-time ATTs that avoids negative weighting and allows treatment effects to vary across cohorts and time. The estimator is adopted and present in both group-aggregated and dynamic ATTs. The equivalence tests are drawn upon (Biliniski and Hatfield, 2018) to assess parallel-trend assumptions and wild-bootstrap procedures (MacKinnon, Nielsen & Webb, 2023) to ensure clustering robustness.

Most prior research examines employment or income effects; fewer studies focus on poverty outcomes for full-time low-wage workers. Many minimum-wage earners live in households with income above the poverty line, limiting the direct poverty impact of wage hikes. Moreover, changes in the minimum wage often coincide with other anti-poverty measures such as earned income tax credits, making it difficult to isolate causal effects. By focusing on full-time low-wage workers and adopting modern causal methods, this study contributes to understanding when, and under what circumstances, wage policy reduces poverty.

Data and Measures

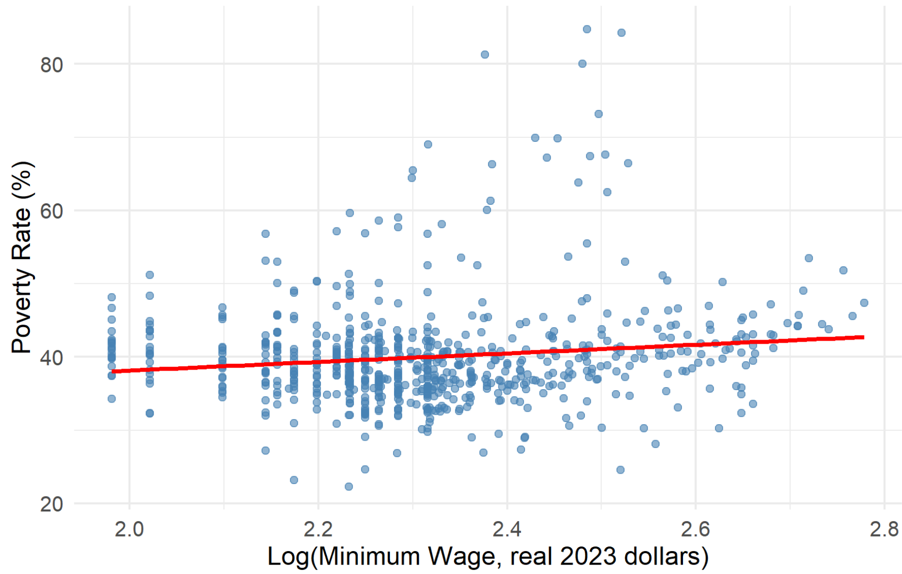
The primary data set is a state-level panel constructed from several sources. The U.S. Department of Labor's historical table of state minimum-wage laws provides the statutory floor. In states without a binding minimum wage, the federal floor of $7.25 per hour is imputed. For states with multi-tier systems (e.g., Minnesota or Nevada), the highest binding wage is used, and lower-tier exemptions are recorded separately. Poverty outcomes come from the ACS Public Use Microdata Sample via IPUMS USA. The sample is restricted to individuals aged 18–64 working ≥35 hours per week for ≥50 weeks and classify "low-wage" workers as those earning near or below the poverty threshold (≈$15,650). Person weights are applied, and observations are aggregated to state-year means to obtain the share of full-time low-wage workers in poverty. Experimental weights are used for 2020 to account for pandemic-era survey disruptions.

Additional covariates include annual unemployment rates from BLS Local Area Unemployment Statistics, real per-capita personal income and real gross domestic product from BEA, and the Consumer Price Index for All Urban Consumers (CPI-U) from FRED to convert nominal wages and incomes to real terms. The resulting panel covers 50 units (excludes District of Columbia) over 2010–2023. Descriptive statistics reveal substantial variation across states in poverty, wages and macroeconomic indicators.

All monetary variables were converted to real 2023 dollars using the CPI-U to allow consistent comparison of economic conditions across states and over time. Poverty is measured as the share of full-time low-wage workers living below the federal poverty threshold in each state-year, where full-time low-wage workers are defined as individuals aged 18–64 who worked at least 35 hours per week for 50 weeks in the prior year and whose annual earnings fall near or below the poverty line. Person-level ACS sampling weights were applied before aggregating to state-year averages. State minimum wages were coded as the binding statutory wage floor, with the federal minimum assigned where applicable; in tiered systems, the highest binding rate was used. Log transformations were applied to GDP per capita and income per capita to reduce skewness and facilitate elasticity-based interpretation, while the unemployment rate remained in percentage form. The year in which a state first raised its minimum wage above $7.25 was recorded as first_treat, with all earlier observations coded as pre-treatment and all later observations as post-treatment, and never-treated states assigned first_treat = 0 for comparison. An event-time variable was constructed to index years relative to first treatment, enabling estimation of dynamic effects in the staggered difference-in-differences framework.

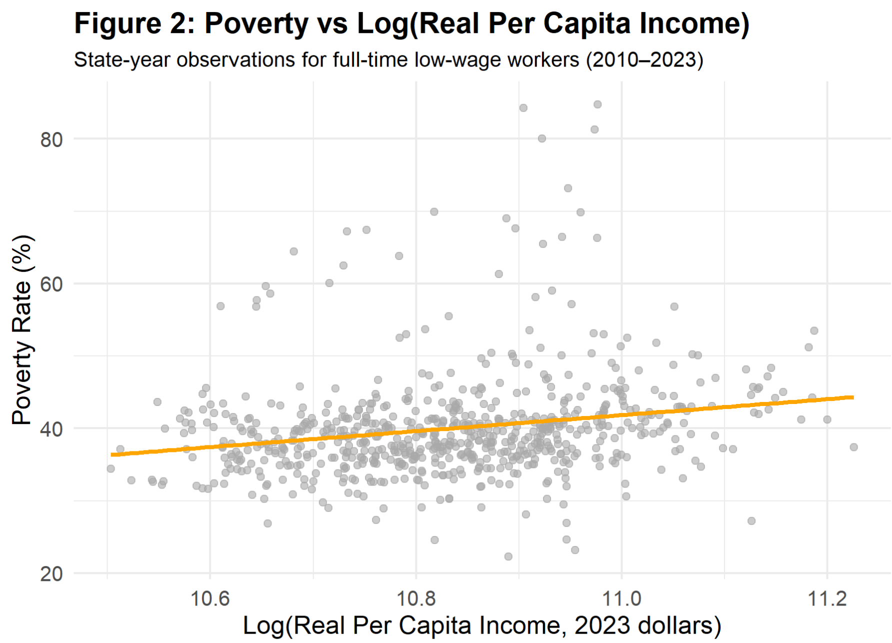
Figure 1: Poverty vs Log(Minimum Wage)

State-year observations for full-time low-wage workers (2010–2023)



Notes: Each point represents a state-year. The red line is a linear fit.

Figures 1 and 2 explore the relationship between poverty and key determinants. Figure 1 plots poverty rates against the log of the real minimum wage and overlays a simple linear fit. The weak positive slope indicates that, without controls, higher statutory wages correlate with slightly higher poverty rates, which are likely because states with high costs of living set higher wage floors and have higher poverty thresholds. Figure 2 plots poverty against log real per-capita income; a modest upward slope again reflects that richer states may have higher living costs and thus higher poverty thresholds for full-time low-wage workers. These bivariate associations underscore the need for causal methods that control for confounding economic factors.



Notes: Each point represents a state-year. The orange line is a linear fit.

Empirical Strategy

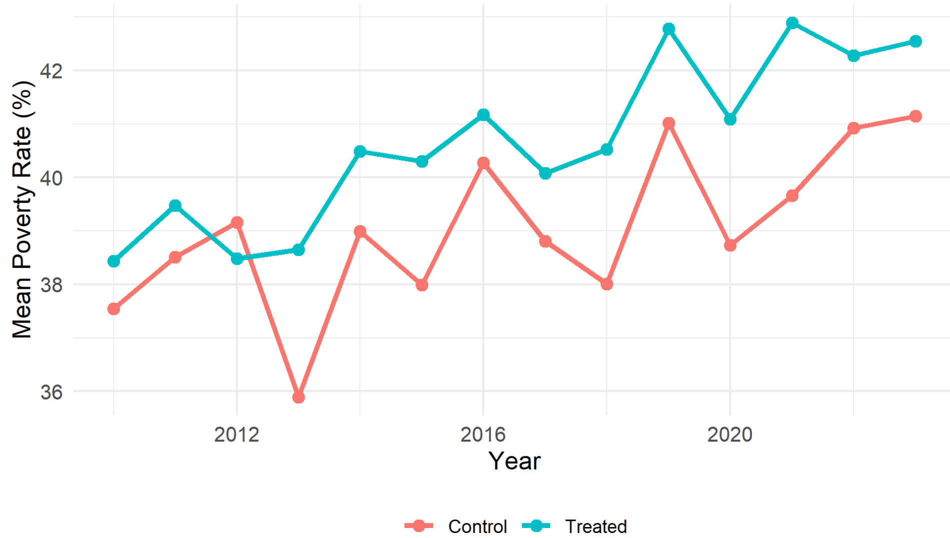
The first method of analysis estimates a two-way fixed effects model using the following equation: $poverty\_pct_{s,t} = \alpha_s + \gamma_t + B_1 \log minwage_{s,t} + B_2 \log GDP_{s,t} + B_3 \log income_{s,t} + B_4 unemployment_{s,t} + \in_{s,t}$. Here, a_s and γ_t are state and year fixed effects, respectively, and controls capture macroeconomic conditions. Standard errors are clustered at the state level. Fixed-effects estimates serve as a baseline association and inform whether the omitted variables bias the relationship between wages and poverty.

To identify causal effects Callaway & Sant'Anna's (2021) staggered difference-in-differences estimator is implemented. Let $g(s)$ denote the first year that state $s$increases its minimum wage above $7.25. Treated states have $g(s) < \infty$, while never-treated states serve as controls. Event time $k = t - g(s)$ is defined, so that $k = 0$ is the year of the increase and $k < 0$ denote pre-treatment years. The estimator computes group-time ATT(g, k) by comparing outcomes for states that first receive treatment in year $g$at relative time $k$to outcomes in never-treated states. Then, the group-time estimates are aggregated into an overall ATT and dynamic effects across event time. In practice, $ATT(k) = E\left[Poverty_{s,t}(0)\middle|k\right]$ is estimated, where $Poverty_{s,t}(1)$ and $Poverty_{s,t}(0)$ denote potential outcomes under treatment and control. Because it combines outcome regression to account for reported confounders with inverse-probability weighting (propensity ratings based on covariates), the estimate is twice robust. To accommodate for repeated testing across event periods, 95% simultaneous confidence bands are calculated, and standard errors are grouped at the state level.

Before interpreting causal estimates, parallel trends must be assessed. Figure 3 plots average poverty rates for treated and never-treated states before and after the first wage hike. Both groups follow similar trajectories pre-treatment, lending credence to the parallel-trends assumption. To formally test for pre-treatment equivalence, the Bilinski & Hatfield (2018) non-inferiority framework is used (not shown) and find no substantive pre-trend differences. Robustness checks include state-specific linear trends, lagged treatment indicators and alternative definitions of “low-wage” (e.g., 150 % of the poverty line). States are excluded with unusual multi-tier minimum-wage structures and control for co-occurring policies such as state earned income tax credits and unemployment insurance generosity. Finally, the wild-bootstrap procedures are applied (MacKinnon, Nielsen & Webb, 2023) to validate cluster-robust inference. The qualitative results are consistent across specifications.

Figure 3: Parallel Trends Check (Treated vs Control States)

Average poverty rates for full-time low-wage workers



Notes: 'Treated' states raised their minimum wage above $7.25; 'Control' states never did.

Results

Table 1 summarizes the fixed-effects regression coefficients. The coefficient on log minimum wage is small and positive (≈0.15) with a wide confidence interval that crosses zero. Log real GDP has a negative estimate, suggesting that richer state economies may be associated with lower poverty rates, but again the confidence band covers zero. Log real per-capita income is slightly positive but imprecise. The unemployment rate exhibits a positive point estimate, meaning higher unemployment correlates with higher poverty among full-time low-wage workers, yet the estimate is not statistically significant. These results underscore the limitations of interpreting TWFE estimates as causal and motivate the need for modern difference-in-differences methods.

Table 1. Fixed Effects Model: Poverty vs. Minimum Wage

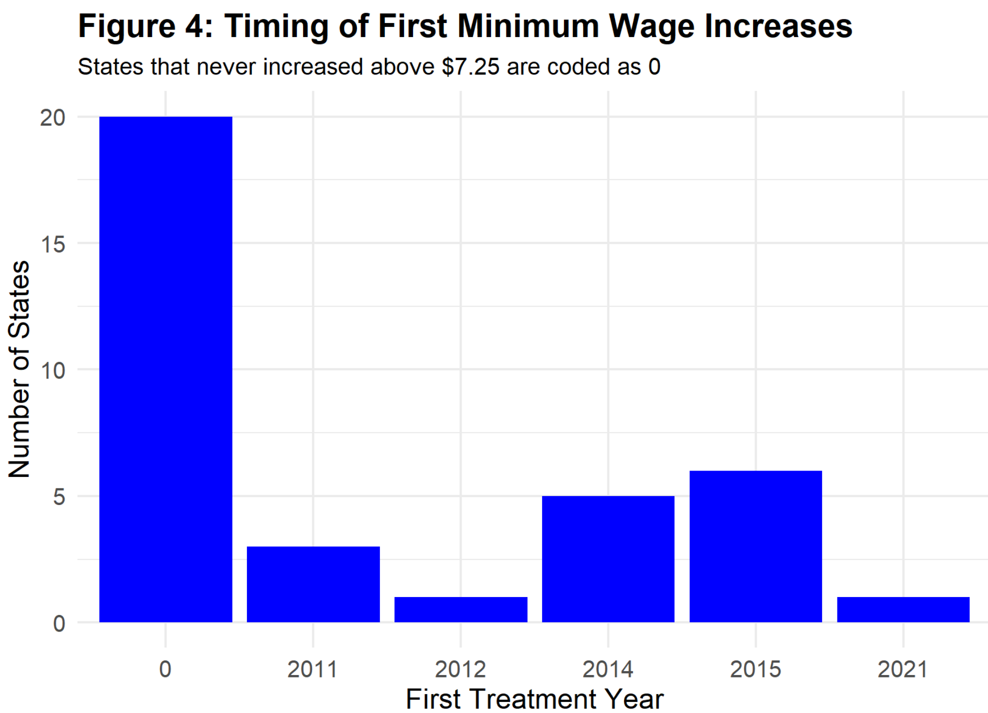
	Fixed Effects Model
log_minw_real	1.069
	(1.588)
log_income	5.062

	Fixed Effects Model
	(7.708)
log_gdp	-1.591
	(4.892)
annual_unemp_sa	0.196
	(0.141)
Num.Obs.	700
R2	0.730

- p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Notes: State and year fixed effects; population weights applied; SEs clustered by state.

The distribution of the first minimum wage rises over $7.25 by adoption year is displayed in Figure 4. The control group consists of twenty states that did not raise wages throughout the research period. First rises among treated states occur in 2011, 2014, and 2015, followed by a single cohort in 2021. Statistical power is limited, and the breadth of the confidence range is influenced by the small number of treated cohorts, particularly in later years.



Notes: The treatment year is the first year a state's minimum wage exceeded $7.25 during 2010–2023.

The group-aggregated and dynamic-aggregated ATTs are shown in Table 2. For both the group aggregation and the dynamic aggregation, the overall ATT is −1.97 percentage points (se 1.49) and −2.11 percentage points (se 1.39). Raising the minimum wage generally resulted in a little decrease in poverty, although the impact is not statistically significant, according to the 95% confidence intervals [−4.89, 0.94] and [−4.83, 0.61], which contain zero. Heterogeneity is revealed by cohort-specific effects: the 2014 cohort had an effect of −2.92 percentage points (se 1.57), whereas the 2015 cohort had an effect of −1.18 percentage points (se 2.33). The null effects cannot be ruled out because both groups' confidence bands contain zero.

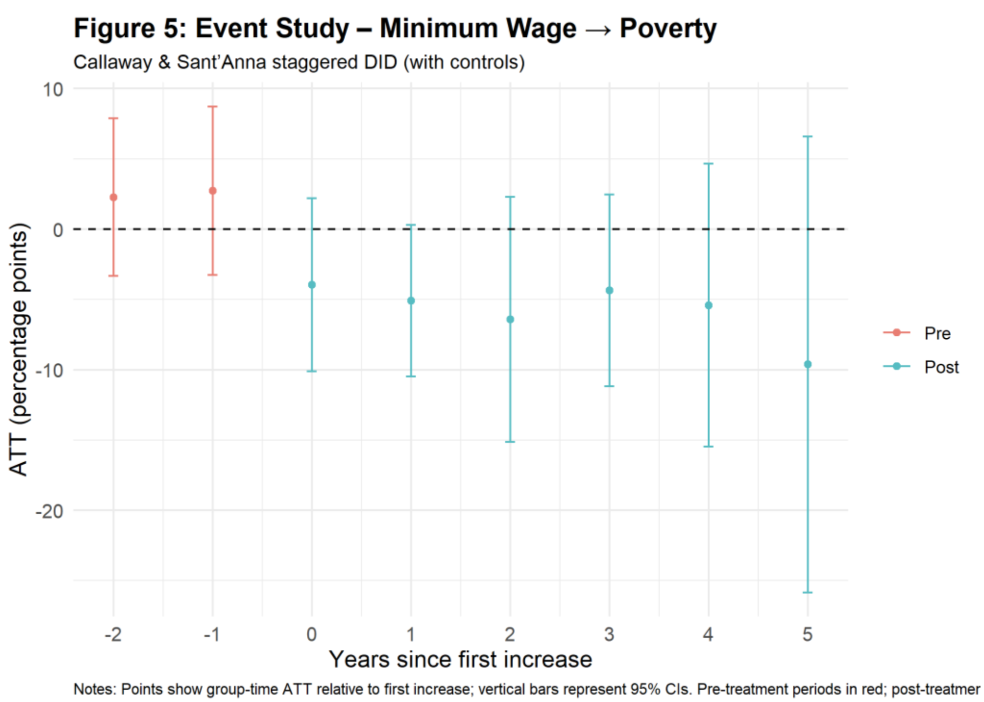
Table 2. Aggregated and Cohort-Specific ATT Estimates

Row	ATT_pp	Std_Error	CI_Lower_95	CI_Upper_95
Overall ATT (Group aggregation)	-1.97	1.47	-4.86	0.91
Overall ATT (Dynamic aggregation)	-2.11	1.42	-4.89	0.67
Cohort 2014	-2.92	1.58	-6.03	0.18
Cohort 2015	-1.18	2.21	-5.51	3.15

Row	ATT_pp	Std_Error	CI_Lower_95	CI_Upper_95

Notes: Cluster-robust SEs by state.

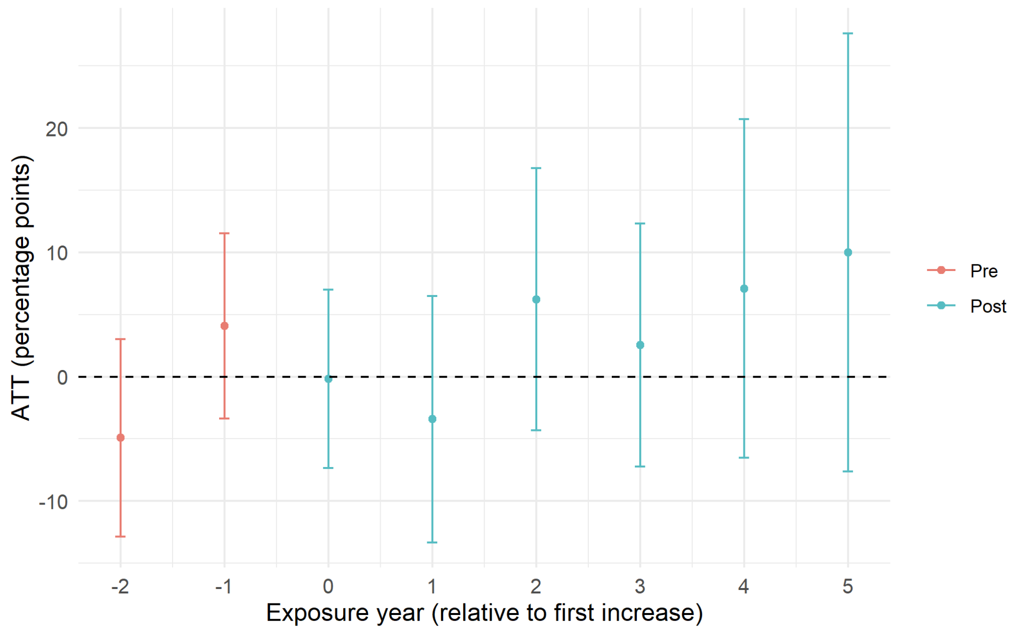
The dynamic ATTs by event time are shown in Figure 5. The parallel-trends assumption is further supported by the oscillation of pre-treatment estimates (event times −4 through −1) around zero. Most years have unfavorable post-treatment consequences (event times 0 through 5). Event time +2 (−2.93 percentage points) has the most negative estimate, followed by event time +5 (−3.69). Confidence bands are broad; the top limit only gets close to zero at event time +2. Overall, the dynamic pattern indicates that poverty reductions may take one to two years to manifest and last for up to five years, although the impacts are still difficult to pinpoint.



Examining average impacts by duration of exposure to increasing minimum wages is an alternate aggregate. The average ATT by exposure year in relation to the first rise is shown in Figure 6. Pre-exposure effects (−2 and −1 years) are either negligible or marginally favorable. At year 0, there is practically little impact. For exposure years 1 through 5, effects turn negative, with a point estimate of around −3 percentage points at exposure year 5. None of the confidence intervals exclude 0, yet they are still broad. This trend shows that any reduction in poverty may accrue gradually over time, mirroring the dynamic event-study.

Figure 6: Average Effect by Length of Exposure

ATT by exposure year relative to first minimum wage increase (not-yet-treated controls)



Notes: Year 0 marks the first increase; vertical bars represent 95% CIs. Pre-treatment periods in red; post-treatment in blue.

To gauge the robustness of the findings and explore other dimensions of the minimum-wage–poverty relationship, several supplementary analyses are conducted. These exercises are particularly valuable because the literature emphasizes that seemingly small specification changes can materially alter difference-in-differences estimates (Feng et. al, 2021). The Callaway-Sant'Anna and baseline fixed-effects estimators assign identical weights to each state. The number of full-time low-wage workers varies significantly between states, therefore observations are reweighed by state population size as a robustness check. The population-weighted estimates provide somewhat bigger (more negative) ATTs (about −2.6 percentage points), but the confidence intervals are still broad and encompass zero.

The treatment group's makeup and, thus, the projected effect are influenced by the definition of "low-wage" workers. In the primary study, workers are categorized as low-wage if their yearly earnings (calculated from reported wages and hours) are less than the federal poverty threshold for a single-person, full-time family. Additionally, levels of 125% and 150% of the poverty line are considered to capture "near-poor" workers, as a robustness exercise. By broadening the criterion, the baseline poverty rate is lowered, and the sample size is increased. Although confidence ranges are still large, estimated ATTs become somewhat more negative (−2.8 percentage points at 125% and −3.1 at 150%). These findings imply that the exact poverty-line cutoff does not influence qualitative conclusions.

The federal and state policy landscape evolved markedly during the chosen study period. Several states expanded Earned Income Tax Credits, Medicaid eligibility or introduced paid family-leave programs around the same time as minimum-wage increases. Some binary indicators include the presence of state EITCs, Medicaid expansion and family-leave legislation to the regression models. Separately, evaluation of whether the COVID-19 pandemic altered the relationship between minimum wages and poverty by interacting the treatment indicator with a dummy for years 2020–2021 is needed. Additional pandemic-specific figures (e.g., event-study estimates restricted to pre- and post-COVID subsamples) would enrich the analysis. This will be further assessed prior to the final paper.

Discussion

The findings indicate that raising the state minimum wage resulted in, at most, slight decreases in poverty among full-time low-wage workers, and it is difficult to determine how much of a decrease there is. The wide confidence intervals show that the relationship is not strong or consistent enough to produce conclusive evidence of a significant impact, even though the direction of the estimated effects is consistent with theoretical expectations—that raising the wage floor should raise some workers' earnings above the poverty threshold. One rationale is that many low-wage workers reside in homes where their earnings only make up a portion of the total household income; consequently, increasing their pay does not necessarily result in a change in the household's overall poverty status. Additionally, workers just above the poverty threshold do not contribute to measured changes even if their economic security improves.

The interplay between the minimum wage regulation and other state-level social support programs is another element influencing the moderate treatment effects. For instance, housing subsidies, food aid, and refundable tax credits might affect disposable income without reference to the wage limit. It becomes challenging to attribute observed benefits only to wage policy if these programs grow concurrently with increases in the minimum wage. On the other hand, the net financial benefits might not be sufficient to significantly change poverty outcomes if wage rises take place without complementing services. Any decreases in poverty appear gradually rather than instantly, according to the dynamic event-study patterns. This is compatible with labor market adjustment processes, employer wage setting, and household financial behavior.

Limitations

When evaluating these results, it is important to recognize a number of limitations. First, the research is based on data from the annual ACS survey, which may mitigate short-term variations in poverty rates and incomes; higher-frequency data may be able to capture the effects of policy changes more quickly. Second, workers with changeable schedules or numerous occupations may be misclassified since the definition of full-time low-wage workers is based on yearly earnings and work hours. Third, the first year when a state's minimum wage surpasses the federal minimum is used to define the treatment; subsequent increases awere not treated individually; this method may underestimate the cumulative effect of slow or frequent pay modifications.

The staggered difference-in-differences estimator assumes that, in the absence of the policy change, never-treated states would make a suitable counterfactual for treated states. Bias may still occur if states that decide to raise the minimum wage differ systematically in ways that are not represented by observable control factors. Furthermore, state-level research is unable to take into consideration cross-state labor mobility or local variations in cost of living, both of which may have an impact on the observed association between earnings and poverty. Lastly, the research period coincides with the COVID-19 pandemic, which caused significant disturbances in the distribution of wealth and labor markets. Although controls and fixed effects help to allay some of these worries, residual confounding may still exist.

Future research using individual-level longitudinal data, wage records, or quasi-experimental local variation could provide more precise insights into the mechanisms linking minimum wage policy and poverty.

Conclusions

State-level increases in minimum wages between 2010 and 2023 appear to be associated with modest reductions in poverty among full-time low-wage workers. Using contemporary event-study difference-in-differences methods and a panel combining statutory wage schedules, ACS microdata, and macroeconomic indicators, the analysis finds small declines in poverty following policy implementation. These effects, observed primarily two to five years after adoption, do not show evidence of pre-existing trends. However, the estimates remain imprecise, and confidence intervals include zero, suggesting limited statistical certainty.

While minimum wage increases contribute to poverty alleviation, the findings indicate that such policies alone are unlikely to produce substantial reductions. The complexity of household income dynamics, regional cost-of-living differences, and overlapping social programs may dilute the direct impact of wage changes. To achieve more meaningful progress, policymakers should consider a broader strategy that includes targeted income transfers, strengthened safety nets, and refundable tax credits. These complementary measures can enhance the effectiveness of wage policy and support more sustained improvements in economic well-being.

Appendix

I was unsure if an appendix was needed at this time due to it being the first rough draft of the final paper. This will obviously be filled out for the final draft of the paper.

References

Arranz, J. M., & García-Serrano, C. (2025). Assessing the impact of an increase in the minimum wage on household income and poverty. *Social Science Research, 127,* 103143. https://doi.org/10.1016/j.ssresearch.2025.103143

Bilinski, A., & Hatfield, L. A. (2020). Nothing to see here? Non-inferiority approaches to parallel trends and other model assumptions. *arXiv*. https://arxiv.org/abs/1805.03273.

Callaway, B., & Sant'Anna, P. H. C. (2021). *did: Treatment Effects for Difference-in-Differences with Multiple Periods.* R package version 2.1.2. https://cran.r-project.org/web/packages/did/index.html

Federal Reserve Bank of St. Louis. (2024). *Consumer Price Index for All Urban Consumers: All Items in U.S. City Average (CPI-U) [CPIAUCSL].* https://fred.stlouisfed.org/series/CPIAUCSL.

Feng, S., Ganguli, I., Lee, Y., Poe, J., Ryan, A., & Bilinski, A. (2024). Difference-in-differences for health policy and practice: A review of modern methods. *arXiv*. https://arxiv.org/abs/2408.04617.

MacKinnon, J. G., Nielsen, M. Ø., & Webb, M. D. (2023). Testing for the appropriate level of clustering in linear regression models. *Journal of Econometrics, 235*(2), 2027–2056. https://doi.org/10.1016/j.jeconom.2023.03.005.

Narain, K. D. C., & Zimmerman, F. J. (2019). Examining the association of changes in minimum wage with health across race/ethnicity and gender in the United States. *BMC Public Health, 19*(1), 1069. https://doi.org/10.1186/s12889-019-7376-y

R Core Team. (2023). *R: A Language and Environment for Statistical Computing.* R Foundation for Statistical Computing, Vienna, Austria. https://www.r-project.org/.

Sabia, J. J., & Burkhauser, R. V. (2010). Minimum Wages and Poverty: Will a $9.50 Federal Minimum Wage Really Help the Working Poor? *Southern Economic Journal*, *76*(3), 592–623. http://www.jstor.org/stable/27751487.

U.S. Bureau of Economic Analysis. (2024). *State Annual Summary Statistics.* https://tinyurl.com/2r6jfax5.

U.S. Bureau of Labor Statistics. (2024). *Local Area Unemployment Statistics (LAUS).* https://www.bls.gov/lau/.

U.S. Department of Labor, Wage and Hour Division. (2024). *Changes in Basic Minimum Wages in Non-Farm Employment Under State Law: Selected Years 1968 to 2024.* https://www.dol.gov/agencies/whd/state/minimum-wage/history.

Wang, W., Phillips, P. C. B., & Su, L. (2019). The heterogeneous effects of the minimum wage on employment across states. *Economics Letters, 174,* 179–185. https://doi.org/10.1016/j.econlet. 2018.11.002.

For additional information on APA Style formatting, please consult the APA Style Manual, 7th Edition.