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Causal Impact of State-Level Minimum Wage Increases Between 2015 and 2024 on Employment Rates for Teenagers Aged 16–19 in the United States

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ABSTRACT

This study estimates the causal effect of state-level minimum wage increases on the teen employment rate from 2015 to 2024. Using approximately 671,000 person-month CPS observations, I applied a difference-in-differences (DiD) design that compared states which raised their minimum wage above the federal floor value of $7.25 as opposed to states that did not.

Several specifications were used in this study: a simple DiD model, a two-way fixed-effects DiD model, a fixed-effects model which accounted for inflation adjusted minimum wage, and a never-treated control group. An event study is also used to see how the effect of a minimum wage increase evolves year by year, as opposed to one averaged number.

The two-way fixed-effects model explains around 91.5% of the variation in teen employment and produces a statistically significant pooled value of +1.17%. However, the event study reveals that this positive average hides underlying effects. The effect is near zero in the first couple of years, and then shifts to slightly negative three or more years after the minimum wage increase. The pooled coefficient is better understood not as evidence of a positive employment effect, but as evidence that a minimum-wage increase did not produce a very large disemployment effect immediately.

The results line up with past works on minimum wage. Even so, a partial parallel-trends violation at event time -2 weakens the strength of the causal interpretation, since the event study detects a pre-treatment deviation that that pooled linear trend test does not.

KEYWORDS

Teenage employment, Difference-in-differences, Two-way fixed effects, Minimum wage, Event study, Naive difference-in-differences, State-month observations, Disemployment.

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INTRODUCTION

At $7.25 an hour, the federal minimum wage in 2026 is worth far less than it was when it was last raised in 2009: the growing inflation rate over the last decade has eroded the value of the American dollar. In the absence of federal action, individual states have become the main driving factors behind wage policy in the United States. In fact, thirty states and the District of Columbia have set minimum wages above the federal floor, while twenty states still match it. This discrepancy among states has created an environment where neighboring states with similar economies have taken contrasting paths regarding policy. This paper examines the variation among states to estimate the causal impact of state-level minimum wage increases between 2015 and 2024 on employment rates for teenagers aged 16 to 19, compared to states that did not raise their minimum wage during the same period.

The minimum wage debate is at the intersection of two opposing ideas in economics: raising the minimum wage will decrease employment, and raising the minimum wage will have little to no disemployment effects. The latter view was popularized by Card and Krueger (1994) and was reinforced by Cengiz et al. (2019). They found near-zero employment effects, but their designs primarily focus on low-wage workers through shorter effect windows. Neumark and Sorkin argue that the reliance on short-term windows hides the harm that affects the least experienced and most substitutable workers. Sorkin specifically revealed the short-run / long-run distinction between the previous studies on minimum wage analysis: motivating the time period of the event study in this paper. Do these reassuring near-zero results remain when the most inexperienced group (teens) is isolated and the effect is traced over a longer period of time, rather than pooling everyone into a single static coefficient result?

Results remain very sensitive to time period, geography, and age group. Many other studies focus on the adult workforce, but not specifically adolescents and the teenage demographic. Some of these adult focused works include Olusegun (2024), the Congressional Budget Office (2021), and work from the Federal Reserve Bank of Boston. Compared to adults, teenagers are disproportionately employed in minimum-wage jobs, lack the experience to compete when hiring conditions are worse, and are often excluded from early labor-market opportunities (Sorkin 2015, Neumark, LISER). Specifically, focusing on 16 to 19 year olds was a primary decision of this study because of these young adults' exposure to the job market. There are millions of high-school students applying to jobs in a variety of markets, who are often overlooked due to their inexperience working in a business environment.

Furthermore, the period 2015 to 2024 acts as a perfect "natural experiment" for the effects of the minimum wage to be observed. Federal inaction during this period has frozen the federal floor at $7.25 since 2009, indicating that nearly all minimum wage variation during this period is state driven. Additionally, this 10 year window brackets the COVID-19 shock, and allows the study to account for the influence of the pandemic. Using difference-in-differences (DiD), a method that compares how outcomes change in states affected by policy against those that do not employ policy changes, this study bridges a key gap: no prior work analyzes the 2015-2024 period for the 16-19 age group utilizing multifactored state-level DiD regression models.

This study analyzes around 671,000 person-month CPS observations aggregated into a 510-observation state-year panel. The paper utilizes several difference-in-differences specifications and supplements them with an event study to see how minimum wage effects evolve over time.

Using this design, I found that the pooled two-way fixed effects estimate is small, positive, and statistically significant (roughly a 1.17% increase in the teen employment rate). However, the event study reveals this finding masks a near-zero effect on impact that becomes slightly negative 3 or more years after a minimum wage increase. Altogether, the results suggest that minimum-wage increases observed between 2015 and 2024 did not produce a large disemployment effect. Instead, they revealed that employers adjusted gradually instead of cutting teen jobs outright.

METHODOLOGY

This study uses a difference-in-differences (DiD) design to identify the causal impact of state-level minimum wage increases on teen employment. In this study, teens are defined as people aged 16 to 19 years old. States that increased their minimum wage were the "treatment group". Contrastingly, states that did not change their minimum wage values were the "control group". The term between treatment status and the post treatment period captures the causal effect of interest: the change in teen employment that can be attributed to the increase in minimum wage.

Furthermore, Goodman-Bacon (2021) and Biewen et al. (2022) revealed how study design can affect conclusions. Their work brought to light that many older state-level comparisons contain methodological flaws. Card, Kluve, and Weber (2010) strengthen this argument through their meta-analysis about short-term policy evaluations and how they miss long-run effects entirely. My study draws on the design flaws of previous models to better its own structure.

This analysis uses microdata from the Current Population Survey (CPS), obtained through IPUMS (Flood et al. 2025), covering all 50 states and the District of Columbia. The dataset consists of person-month observations while also including key variables. These variables are year, month, state, survey weight, age, and employment status. The dataset is then merged with a panel of state-level minimum wages by year, capturing policy variation across states. Some notable details include minimum wage increases in states such as California, New York, and New Jersey. The baseline minimum wage was set to $7.25 and was larger for states that raised the minimum wage above the federal baseline.

$$\text{Employment}_{st} = \beta_0 + \beta_1\text{Treated}_s + \beta_2\text{Post}_t + \beta_3(\text{Treated}_s \times \text{Post}_t)$$

- Employment_{st} is the weighted teen employment rate in state, s, during year, t.

- β_0 is the intercept, or the baseline employment rate for control states in the period before treatment.
- β_1 captures the average difference between treated and control states.
- $Treated_s$ is equal to 1 if state, s ,ever raised its minimum wage during the sample period, and is 0 otherwise.
- β_2 captures the average change over time common to all 51 units.
- $Post_t$ is equal to 1 in the post treatment period, and is equal to 0 for pre-treatment years.
- β_3 is the coefficient of interest, and captures the DiD treatment effect of the minimum wage increase.

Analyzing the results from past papers, variation in Goodman-Bacon's work (2021) for the minimum wage provided them the perfect setting for a difference-in-differences analysis. This is because the treated and control states in Goodman-Bacon's work have very similar employment rates in the pre-period. This same concept can be applied to the study in this paper. In this case, the treatment is the increase in the minimum wage and the response is the subsequent effect on the teenage employment rate. Having similar trends in the pre-period is extremely important, so that the post-treatment difference can be credited to the minimum wage increase.

The check for these similar conditions is done through analyzing the parallel trends of both the treated and control states. Looking through previous work, this parallel trend check is standard in minimum-wage papers that utilize a difference-in-differences procedure. Prime examples include Goodman-Bacon (2021) and Card and Krueger (1994).

Parallel-Trends Diagnostic

The validity of the DiD estimate depends on the parallel-trends assumption: absent a wage increase, treated and controlled states would have followed the same employment trajectory. To test this, I regressed teen employment on the interaction of treatment status and a straightforward (linear) time trend. This was done while being restricted to pre-treatment observations. The estimated interaction coefficient value is 0.0009 (standard error equals 0.003, p-value equals 0.751), making the finding statistically indistinguishable from 0. The pre-treatment trends in teen employment were thus statistically parallel between the treated and control states, supporting the assumption my DiD design is based upon.

The sample is restricted to individuals aged 16-19, with around 671,000 observations that have no missing value. Employment status is defined as using CPS codes, which indicate a youth's active employment. The data is then aggregated to a state-year panel consisting of 510 observations (50 states plus the District of Columbia over 10 years), with survey weights applied to find representative employment rates. On average, each state-year cell includes an estimated 1,300 teen respondents. Descriptive statistics indicate an average teen employment rate of 34.6%[1]: a nationally representative sample. However, there is a large amount of variation between states and years through the regression. The average minimum wage was $7.99, with a median of $7.25.

Two main regression models are estimated here, and additional robustness specifications appear in the Results section. The first model in this section includes a baseline that covers the treatment, post, and interactive terms. The second model incorporates state and year fixed effects, with standard errors clustered at the state level to account for within-state correlation. The inclusion of fixed effects controls for factors that can affect the regression due to its time span. This includes the COVID-19 pandemic in 2020, which saw an abnormally low teenage employment rate across both models due to global events. The treatment variable is defined as an indicator to check if a state's minimum wage increases over the course of a year. However, due to data limitations changes occurring in 2015 are not captured. A pre-period baseline is required from each prior year in this design, and there is no year prior to 2015 for this study. All data used in this study is publicly available and was utilized in accordance with standard IPUMS research agreements. No human subjects were directly involved, and the study utilizes a desk methodology, relying on secondary data sources.

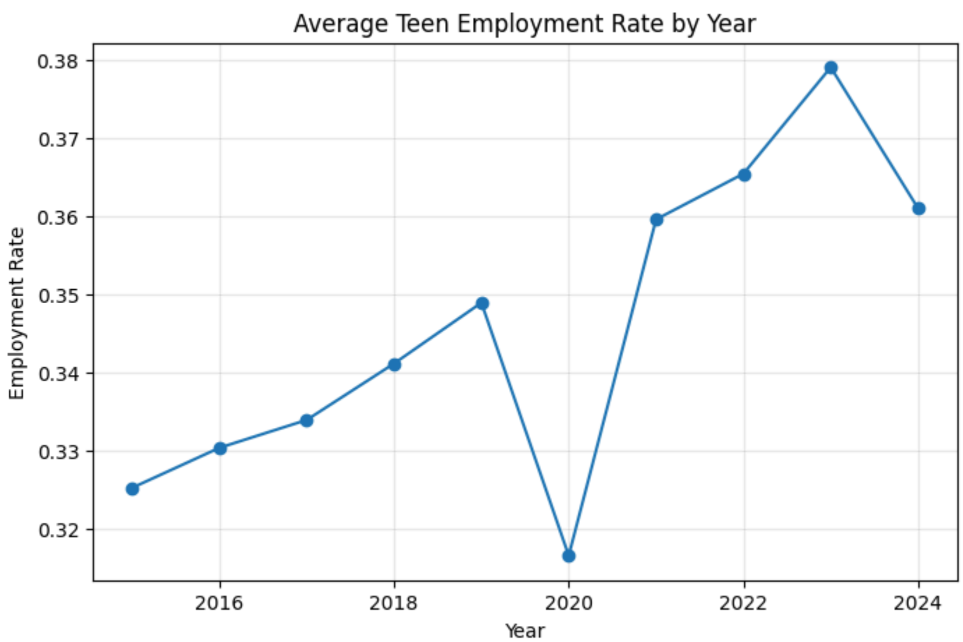

Figure 1: Average Teen Employment Rate by Year from 2015 to 2024

RESULTS

Descriptive Statistics

The sample for this regression is made up of 510 state-year observations spanning the 50 states plus the District of Columbia, over the 10-year period from 2015 to 2024. The CPS microdata was restricted to teenagers aged 16 to 19 years old, and any observations which were missing values were dropped.

Approximately 671,000 person-month records were combined into the state-year panel using IPUMS final person weights as a variable (WTFINL). On average, each state-year data entry summarizes the employment status of approximately 1,300 teenage respondents.

The weighted teen employment rate averaged 34.6 %[2] across the panel, with great variation across states and over time. This variation ranged from 12.6 to 60.8 %[2]. The average state-year minimum wage was $7.99, with the median being at the federal floor value of $7.25. Wage variation was focused on smaller groups of states, including California, New York, Washington, Massachusetts, Oregon, Colorado, Illinois, New Jersey, and Florida. These states increased their minimum wage over the 10-year observational period.

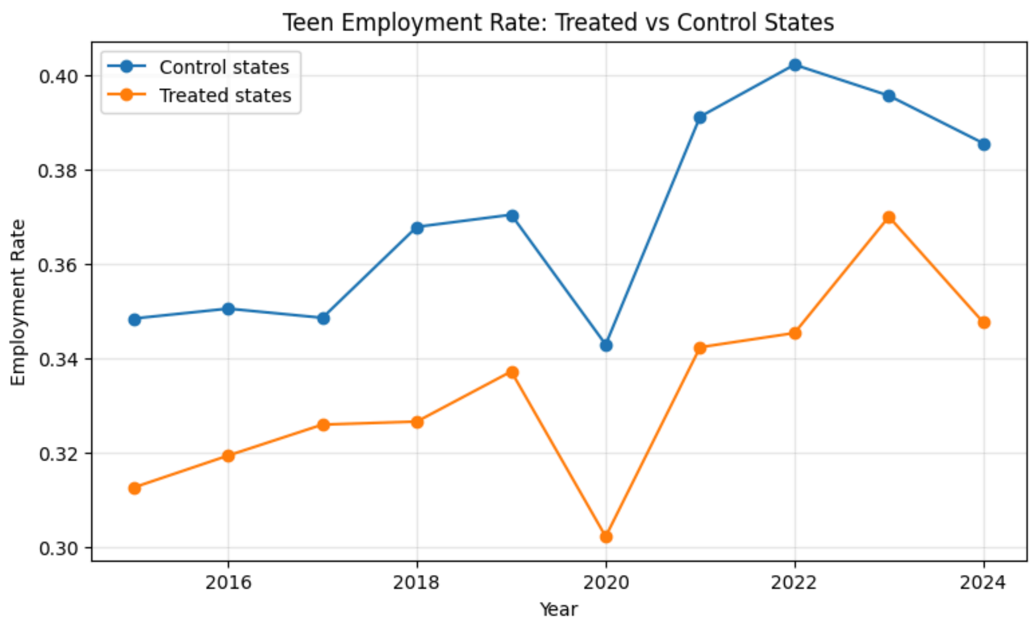

Figure 2: Employment Rate Comparison between Treated and Control States from 2015 to 2024

Base Model: Naive Difference-in-Differences

The first DiD model estimates the standard 2 x 2 difference-in-differences equation without including fixed effects. The model does not fit the data well, as only 8.6 % of the variation in the state-year teen employment rate is explained by the change in minimum wage with an r^2 equalling 0.086. The estimated treatment effect is β_3 equals 0.0099, standard error equalling 0.012, and a p-value of 0.392, making the estimate statistically insignificant. The treated main effect, however, is negative, large, and significant. This can be displayed with the β_1 value of -0.0564, a standard error of 0.024, and a p-value of 0.017. This indicates that states which raised their minimum wage had teen employment rates around 5.6 % lower than control states on average.

$\text{Employment}_{st} = \beta_0 + \beta_1\text{Treated}_s + \beta_2\text{Post}_t + \beta_3(\text{Treated}_s \text{ x } \text{Post}_t) + \varepsilon_{st}$

- Employment_{st} is the weighted teen employment rate in state, s ,during year, t.
- β_0 is the intercept, or the baseline employment rate for control states in the period before treatment.
- β_1 captures the average difference between treated and control states.
- Treated_s is equal to 1 if state, s ,ever raised its minimum wage during the sample period, and is 0 otherwise.
- β_2 captures the average change over time common to all 51 units.
- Post_t is equal to 1 in the post treatment period, and is equal to 0 for pre-treatment years.
- β_3 is the coefficient of interest, and captures the DiD treatment effect of the minimum wage increase.
- ε_{st} is the error term.

Preferred Specification: Two-Way Fixed Effects DiD

The preferred specification changes the baseline with state and year fixed effects and clusters standard errors at the state level. The specification explains 91.5 % of the variation in teen employment rates, which is much more substantial than the previous model. This reflects the importance of including long-term cross state differences and shared national time shocks, like the COVID-19 pandemic in 2020, which influenced teenage employment.

The estimated treatment effect is β_3 equals 0.0117, standard error equivalent to 0.005, and a p-value of 0.026. Additionally, a 95% confidence interval ranges from (0.001, 0.022). These findings are statistically significant at the α equals 0.05 level, and the coefficient implies that states which raised their minimum wage experienced an increase in their teen employment rate by approximately 1.17 %. This finding is related to control states, after accounting for time-invariant state characteristics and year-specific shocks. Relative to the baseline mean teen employment rate of 34.6 %, this corresponds to roughly a 3 % proportional increase in teenage employment.

The contrast between models 1 and 2 is very important to observe. The naive DiD model results in a statistically insignificant effect because it attributes the policy treatment (minimum wage increase) to the ongoing negative gap between treated and control states. Once the state fixed effects absorb that difference, the within-state, post-treatment variation in teen employment emerges clearly and is precisely estimated.

$\text{Employment}_{st} = \beta_3(\text{Treated}_s \text{ x } \text{Post}_t) + \alpha_s + \gamma_t + \varepsilon_{st}$

- Employment_{st} is the weighted teen employment rate in state, s ,during year t.

- β_3 is the coefficient of interest, and captures the DiD treatment effect of the minimum wage increase.
- $Treated_s$ is equal to 1 if state, s ,ever raised its minimum wage during the sample period, and is 0 otherwise.
- $Post_t$ is equal to 1 in the post treatment period, and is equal to 0 for pre-treatment years.
- α_s is the vector of state fixed effects, and it absorbs all of the time-invariant differences across states.
- γ_t is the vector of fixed year effects, and it absorbs national time shocks that all states will experience.
- ε_{st} is the error term.

Dynamic Treatment Effects: Event Study

While the linear pre-trend test supports parallel trends on average, an event study provides a more detailed look into the dynamics of the treatment itself. I estimated separate coefficients for each year relative to a state's first minimum-wage increase, with event time = -1 removed as the reference period. The resulting plot reveals a more nuanced view than the static DiD coefficient suggests.

In the pre-period, coefficients at event times -4 and -3 are not statistically distinguishable from 0 (point estimates of -0.0068 and -0.0194 respectively, with confidence intervals that cross 0). However, the coefficient at event time -2 is -0.0317, with a 95% confidence interval ranging from (-0.056, -0.007), which shows that the treated states had a much lower employment percentage two years before treatment as opposed to the period immediately before treatment. Based on this finding, some strict parallel pre-trends are partially violated, but the linear-trend test from the section above does not detect a differential trend. This linear trend test pools all the pre-treatment years.

Post treatment effects are short-run, small, and not statistically significant. The coefficients at times 0, +1, and +2 range from -0.005 to -0.003. All of the confidence intervals for these times contain zero. The coefficients turn more negative at event time +3 (-0.0205, 95% confidence interval from (-0.03, -0.011)) and stay negative at +4 as well. At event time +4, the coefficient is -0.0112 (95% CI: -0.024, 0.002). This reveals that the finding is not statistically significant at event time +4.

$$Employment_{st} = \sum_{\substack{k=-4 \\ k \neq -1}}^{+4} \beta_k D^k_{st} + \alpha_s + \gamma_t + \varepsilon_{st}$$

- k is event time, the number of years relative to a state's first minimum-wage increase (so k=0 is the year of treatment, k < 0 is pre-treatment, and k > 0 is post treatment).
- β_k is the set of coefficients of interest, each capturing the treatment effect at event time k relative to the reference period.

- D^k_{st} is an indicator equal to 1 when state s is k years away from its first minimum-wage increase, and is 0 otherwise.
- α_s is the vector of state fixed effects, and it absorbs all of the time-invariant differences across states.
- γ_t is the vector of fixed year effects, and it absorbs national time shocks that all states will experience.
- ε_{st} is the error term.

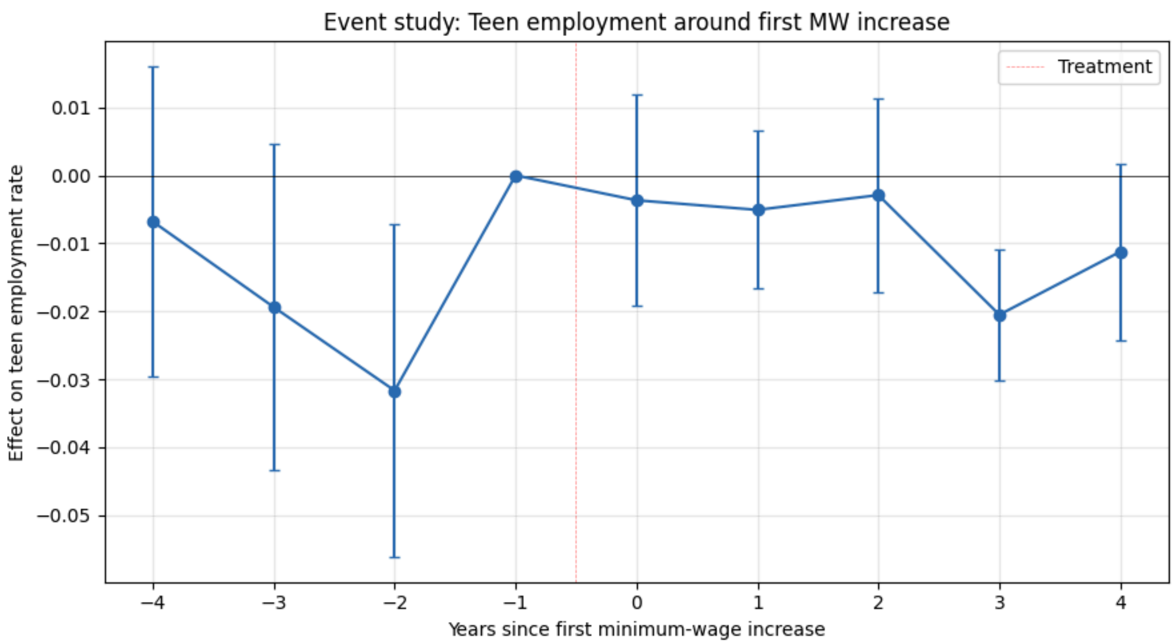

Figure 3: Graph of Confidence Intervals Estimating Minimum Wage Effect on Teen Employment Rate

Event time -1 is the baseline year[3]. This means that it is considered the year before the minimum wage change, which is why it has no confidence interval.

Robustness Checks

I ran three additional models to check how sensitive the headline DiD estimate is to specific choices. These choices include continuous real-wage specification, a never-treated comparison, and a continuous-dose specification.

Continuous real-wage specification.

Including the inflation-adjusted real minimum wage (in 2024 dollars) to the FE DiD produces a larger DiD coefficient of β_3 equals 0.0148 (standard error is 0.006, and the p-value is 0.02). The real-wage level itself is small and not statistically significant, with β equals-0.0019, standard error equals 0.002, and p-value being 0.303. This suggests that the employment response is noticeable at around the same time as the discrete policy change, rather than depending on the size of the real wage.

Never-treated comparison.

One main concern with a two-way FE DiD under staggered treatment is that already-treated states get used as controls for states that will be treated later. This can bias the estimate when treatment effects are already so different across state boundaries. To fix this issue, I re-ran the FE DiD using only never-treated states as controls. The result was β_3 equivalent to 0.0117 (standard error being 0.005, and a p-value of 0.026.) The 95% confidence interval for this comparison spans from (0.001, 0.022). These values are identical to the headline estimate. The fact that the two-way FE estimator and the never-treated comparison agree tells me that the staggered-timing bias is not influencing the result, despite the heterogeneous dynamics that the study detects.

Continuous-dose specification

To check even further, I replaced the binary treatment indicator with the value of the real-minimum wage itself. This treated the wage level as a continuous dose as opposed to a discrete policy event. The specification also asks whether larger wage increases produce larger employment effects. The estimated coefficient is small, negative, and statistically insignificant, as seen by β equals -0.0015, standard error equals 0.002, and p equalling 0.39. The coefficient's insignificance indicates that larger wage increases were not associated with larger employment changes.

DISCUSSION

The estimates reveal a small positive average effect of state minimum-wage increases on teen employment (around 1.17% to 1.48%), which is significant at the 5 % level. However, these positive coefficient results are only applicable in a short frame following the immediate increase of the minimum wage. An important factor to consider for this finding is that longer-horizon estimates have a near-zero result. The result stays strong when controlling for the real wage level and when limiting the controls to only never-treated states. The positive sign goes against the standard competitive prediction that minimum wage increases lower teenage employment. However, the finding aligns with Card and Krueger (1994) and Cengiz, Dube, Lindner, and Zipperer (2019). The previous works found small or non-negative employment effects from medium level minimum-wage increases. The Federal Reserve Bank of Boston also finds that minimum wage increases slowly raise prices and support consumer spending. Possible explanations include reduced employer monopsony, a lower turnover rate, more labor supply present at higher wages, and demand-side effects that are created from higher earning households.

The event study provides a different view than the pooled DiD coefficient. The +1.17 % pooled estimate averages over many different factors. This includes near-zero effects in the first two post-treatment years and slightly negative effects, three plus years out. These findings support the idea that employers adjust to minimum wages slowly through automation, slower hiring, or greater reliance on older, more experienced workers. Employers consider these alternatives rather than cutting the job position entirely. Neumark (LISER) also found that minimum wage increases make the job market more rigid, or selective, for new and inexperienced workers. Sorkin (2015) reveals that short-run employment studies underestimate the true negative effects on newer workers, as many companies today are fully automating entry level positions. This means that the total number of entry-level jobs is reduced, and the competition for the ones that are left is even higher for teenage workers. Grigsby and Zorzi (2023) also support this concern, showing that workers in automated industries are facing long term earning losses.

The pooled coefficient is a weighted average that hides heterogeneity, which is a known issue with two-way fixed-effects estimators when treatment effects are not similar (Goodman-Bacon 2021; de Chaisemartin and D'Haultfœuille 2020). The positive pooled effect differs from the common pattern where immediate effects are near zero and longer-run effects (3+ years) turn negative. Due to this, the headline coefficient should not be interpreted as evidence of a positive employment effect. Instead, it is better read as evidence that minimum-wage increases of the size observed in this panel did not result in a large negative employment effect on impact. The positive pooled coefficient reflects the near-zero effects in the first post-treatment years rather than a genuine positive response, while the event study shows the effect drifting negative at longer horizons. The strongest reading is that moderate minimum-wage increases from 2015 to 2024 had minimal employment effects that depended on the year and region.

The continuous-dose specification revealed a close-to-zero result. The close-to-zero result is consistent with the binary specification: both point towards very small employment effects in actual practice. Looking at this finding alongside the binary specification, it suggests that the employment response is tied to the act of raising the wage floor rather than the strength of the increase. This outcome is more consistent with an employer adjustment of non-wage margins rather than a smooth labor-demand response.

The 5.6% employment gap between treated and control states reflects pre-existing geographical differences between the two groups, rather than a causal effect of policy. States that chose to raise their minimum wages are much different than those who did not in regard to industry mix, demographics, urbanization, and political environment. These are pre-existing differences, which are older than the minimum wage increases, that the state fixed effects absorb. This pattern motivates the inclusion of state fixed effects in the preferred specification.

CONCLUSION

This paper analyzed whether state-level minimum wage increases between 2015 and 2024 positively or negatively affected employment for teenagers aged 16 to 19 years old. Using a difference-in-differences design on a CPS state-year panel of 510 observations, the answer is: near-zero on impact, but slightly negative over time. The analysis aggregated teen employment to the state-year level and accounted for inflation across different states.

This allowed the analysis to be flexible for varying minimum wage values across states. The two-way fixed-effects estimate puts the immediate effect of minimum wage increases at around + 1.17% on teen employment rate. This finding is statistically significant at the 5% significance level. The event study, however, shows that this positive average hides variation within the states over time. The concurrent effect, in the year the wage increase takes effect, is very close to 0. Furthermore, three or more years after the increase, the coefficients turn negative.

The near-zero pooled result aligns with past works, such as Card and Krueger (1994) and Cengiz et al. (2019). The negative longer-run finding is consistent with Sorkin's (2015) argument that firms adjust to minimum wage changes gradually.

Limitations of the Estimates

The event study coefficient at event time -2 is meaningfully negative. Despite this violation, two features support the confidence in the post-treatment estimates: the deviation is limited to a single pre-period year rather than a sustained long-term trend. Additionally, the never-treated comparison produces an estimate which is identical to the headline result. This is a partial violation of the parallel-trends assumption in the study. Some of what the regression model calls a treatment effect can be a pre-existing difference. Treatment is also coded as binary, which can overlook the effect of pre-existing differences between firms' employment responses across state lines. The panel is annual and state-level, so any effect that examines hours worked, specific industries, or sub-state local labor markets is not visible. Standard errors are clustered at the state-level, but the justification for this result is not very strong because only 51 units are used in this study. The COVID period is included in the panel, and year fixed effect can only partially account for the shocks that affected treated states and control states. Finally, the dependent variable bundles the 16- to 19-year-olds without separating those that are in school and those that are out of school. This does not account for the out-of-school teens, who are most affected by the policy.

Additionally, the paper builds on the previous work from Card and Krueger (1994), which is the basis paper for difference-in-differences analysis for the minimum wage, and Cengiz et al. (2019) and Sorkin (2015). It draws on the weaknesses of these previous works to influence its own study design, addressing the staggered-treatment problem, confounding from the selection process, and the verification of a parallel-trends assumption. By controlling for these different factors, the analysis can do a better job of isolating the effect of the minimum wage on teen employment from other influences.

This paper contributes to the debate over minimum wage policy by focusing on an overlooked group: teenagers aged 16 to 19 years old, who are disproportionately employed in minimum-wage jobs and most exposed to changes to the wage floor. The findings suggest that moderate state-level increases observed between 2015 and 2024 did not cause large teen job losses on impact. This is relevant for policymakers who are considering further increases to the minimum wage. At the same time, the small negative shift three plus years after the policy is enacted is a caution that can influence employment over time. This is a common pattern that warrants closer study as more states raise their minimum wages and more post-treatment data becomes available.

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