Employment Effects of Three Rounds of Federal Minimum Wage Hikes

Kenneth J. McLaughlin*
Hunter College and the Graduate Center of the City University of New York

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Abstract

This paper presents event-study estimates of the effects of the 1990–1991, 1996–1997, and 2007–2009 rounds of federal minimum wage hikes on the employment of teens and high school dropouts in states without super-federal minimum wages. In state-year panel data from the Current Population Survey, a control group of people ages 25–59 with at least a high school education generates counterfactual series that track teen and dropout employment rates quite well (outside the periods of minimum wage hikes). Deviations from the counterfactual series in the post-hike period identify the employment effects of the minimum wage hikes.

For the 1990–1991 and 2007–2009 rounds, the employment effects for teens and dropouts are negative, statistically significant, economically large, and robust to the treatment of trends and year effects. Differences by sex and race are small compared to the difference by age: disemployment effects for younger teens (ages 15–17) are twice the size of the effects for older teens (ages 18–19). Welfare reform contaminates analysis of the 1996–1997 round, but monthly estimates of the employment effects in that round resemble monthly estimates in the 1990–1991 round until welfare reform rolled out in the second half of 1997.

1. Introduction

The 1990–1991 and 2007–2009 recessions hit U.S. teens particularly hard. The teen employment rate plunged 15 percent (from 35.6 percent to 30.4 percent) over the three years between January 1989 and January 1992. From the end of 2006 to the end of 2010, the teen employment rate plummeted nearly 30 percent (from 29.6 percent to 20.9 percent). For comparison, the employment rate of the working-age population in the United States fell by about 3 percent (1.8 percentage points) and about 8 percent (5.1 percentage points) over the same periods.

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These recessions coincided with increases in the federal minimum wage. The 1989 amendments to the Fair Labor Standards Act raised the minimum wage to $3.80 on April 1, 1990 and to $4.25 on April 1, 1991. Although the NBER's business cycle dating marks the cyclical peak four months after the new minimum wage took effect, the overall employment rate peaked the month before the April 1990 hike and continued through December 1991. The Fair Minimum Wage Act of 2007 amended the Fair Labor Standards Act to raise the federal minimum wage from $5.15 to $5.85 on July 24, 2007, to $6.55 on July 24, 2008, and to $7.25 on July 24, 2009. Although economic activity peaked five months after the $5.85 minimum wage took effect, the overall employment rate peaked eight months before the August 2007 hike and declined for more than a year after the August 2009 hike.

My purpose is to identify the effect of federal minimum wage hikes on the employment rates of teens and high school dropouts. How much of the blame for the dramatic drops in the employment rates of teens and dropouts belongs with the minimum wage hikes? I estimate the effects of these two rounds of federal minimum wage hikes on the employment rates of teens and dropouts in states without super-federal state minimum wages; that is, I exclude a state if its state minimum wage exceeds the federal minimum wage. I also analyze employment effects in the 1996–1997 round of minimum wage hikes and explore how the employment effects of the minimum wage vary by sex, race, and age.

My method is simple and direct. I treat each round of federal minimum wage hikes as a compound event and compare the change in the employment rates of teens and older high school dropouts in the wake of the event with the change in the employment rate in a control group. My control group is prime-age people with at least a high school education. The method handles the excess sensitivity of teen employment (relative to control employment) to aggregate fluctuations and allows the teen trend in employment to differ from the employment trend of the control group.

Deere, Murphy, and Welch's (1995) analysis of the federal minimum wage hikes in 1990 and 1991 is my point of departure. They compare employment rates before and after the April–1990 and April–1991 minimum wage hikes by age, education, sex, race, ethnicity, marital status, and state-average wage. Their takeaway is that the biggest declines in employment following the 1990–1991 hikes were for low-wage groups.

Deere, Murphy, and Welch also compare the employment changes of low-wage workers to workers who are unlikely to be bound by the minimum wage. For instance, employment losses of teens dwarfed the employment losses of men ages 25–64. That finding surely surprises no one since employment rates declined in the 1990–1991 recession, and employment of teens and other low-wage workers is quite sensitive to aggregate fluctuations. To address the excess sensitivity of teen employment, Deere, Murphy, and Welch regress the employment rates of teens and dropouts on the employment rate of men ages 15–64, as well as two dummy variables that indicate the periods with the $3.80 (April 1990 – March 1991) and $4.25 (April 1991 – March 1993) minimum wages. The employment rate of men ages 15–64 controls for aggregate economic activity, and the coefficient on this variable handles the differential sensitivity of teen employment to macro fluctuations. Estimates from these state-year regressions reveal large and statistically significant disemployment effects of the minimum wage (Deere, Murphy, and Welch 1995, Table 4).

Deere, Murphy, and Welch's method delivers difference-in-difference (DD) estimates with men ages 15–64 as the control group. But their method departs from canonical DD in two ways. First, an estimated regression coefficient scales the employment rate of the control group to produce the counterfactual. Second, the analysis omits year effects: the scaled employment rate of men ages 15–64 replaces the year effects.

In this paper, I apply the event method to two extra rounds (1996–1997 and 2007–2009) of federal minimum wage hikes and improve on Deere, Murphy, and Welch's analysis of the 1990–1991 round in several ways. For the 1990–1991 round, I have bigger samples each year and three extra post-event years. I exclude states that had state minimum wages that exceeded the federal minimum wage. I also explore robustness to trends—including state-specific trends, which Deere, Murphy, and Welch ignore—and year effects. And I improve the composition of the control group. My control group consists of prime-age high-school graduates. In addition, my standard errors reflect clustering at the state level.

My estimates of the effects of the 1990 and 1991 minimum wage hikes in section 3 reinforce Deere, Murphy, and Welch's evidence. I precisely estimate economically large disemployment effects in this round. Teen job losses in the wake of this round of minimum wage hikes are too big to be consistent with the comovements of teen and prime-age high-school graduate employment rates. The estimates are robust to the treatment of trends, and sensible differences emerge across groups. My results sharply contrast with Card's (1992b) difference-in-difference estimates based on differential responses between low-wage and high-wage states.

Estimating employment effects of the 2007–2009 round is more challenging since a differential trend in the teen employment rate pushes in the same direction as the recession and the minimum wage hikes in depressing teen employment. Nevertheless, in section 4, the estimates on state-year panel data point to statistically significant and economically large disemployment effects. Differences across groups are not as sharp. My results differ from Hoffman (2014) who finds no significant effect of the 2009 hike on teen employment. Indeed, 2009 is the year when the disemployment effects of the 2007–2009 increases in the minimum wage clearly appear. My results reinforce Clemens and Wither's (2019) evidence: by the second year following the July–2009 increase in the minimum wage to \$7.25, the employment rate of low-wage workers in the Survey of Income and Program Participation fell 6.6 percentage points in states where the federal wage was binding relative to states with super-federal state minimum wages.

In section 5, I address a deficiency of the event method—the inability to distinguish minimum wage effects from year effects. A two-step estimator solves the problem by scaling the control group's employment rate to satisfy the common-trend requirement. The first step fits the relationship between the employment rates of teens and the control group. The first-step estimate of the slope coefficient β then transforms the control group's employment rate. This amounts to creating a new control group, one with an employment rate that is more volatile than that of prime-age high-school graduates. The second step departs from canonical difference-in-differences in only one way: the transformed employment rate replaces the employment rate of the control group. The *scaled-control* difference-in-difference estimates demonstrate that my main results are robust to the treatment of year effects.

Section 6 contains several extensions. First, welfare reform in 1997 confounds my estimation of the employment effects of the 1996 and 1997 minimum wage hikes. But monthly analysis from the September 1996 hike until welfare reform rolled out in the second half of 1997 suggests that teen employment was unusually low for the months until welfare reform took hold. Second, the 1990–1991 and 2007–2009 round of minimum wage hikes reduced the employment rates of high school dropouts (ages 25–54), although the magnitudes are about half the size of the disemployment effects on teens. Third, I use quarterly data to analyze how quickly employment responds to increasing the minimum wage. And fourth, I use data on the pre-hike distributions of wages to convert the estimates into labor demand elasticities. My evidence of highly elastic demands for teens and dropouts suggests that the 1990–1991 and 2007–2009 rounds of federal minimum wage hikes reduced the total earnings of teens and dropouts.

2. Methods and Data

The goal is to estimate the effect of federal minimum wage events on the employment of low-wage workers. My primary evidence comes from estimates of teen employment-rate regressions on state-year averages from the Current Population Survey (CPS). Variation in the employment rate of a control group, as well as trends, generate a counterfactual series, and systematic deviations from the counterfactual series in the period after the minimum wage event identify the employment effects of the minimum wage. So these are difference-in-difference estimates.

I also present event plots of teen employment rates in aggregate data. Simple regressions on 11–14 aggregate data points generate the counterfactual series, and the plots reveal the patterns that drive the regression estimates in the state-year panels.

Following Deere, Murphy, and Welch's (1995) lead, I organize monthly survey responses from the CPS into minimum-wage years. In the analysis of the 1990–1991 minimum wage hikes, observations from April 1990 through March 1990 form minimum-wage year 1990. In the 2007–2009 round, data from the August 2007 through July 2008 surveys are in minimum-wage year 2007. The data subsection below describes the underlying data.

Event Study

The event study departs from canonical DD estimation because an employment-rate regression scales variation in the control group's employment rate. That is, a regression generates the counterfactual series by estimating a slope coefficient. The method simplifies to the canonical model if the slope coefficient is one.

On state-year averages, I regress the log of the teen employment rate $\ln e_{st}^{T}$ in state s in minimum-wage year t on the log of the employment rate in a control group $\ln e_{st}^{C}$, time t, and a dummy variable D_{st} that indicates the post-event period. That is,

$$\ln e_{st}^{T} = \alpha_s + \beta \ln e_{st}^{C} + \gamma_s t + \delta D_{st} + \epsilon_{st} \tag{1}$$

for $s = 1, \ldots, S$ and $t = 1, \ldots, N$. This specification includes state fixed effects α_s and state-specific trends γ_s, and the estimator weights the observations by teen population. Since each round of federal minimum wage hikes involves two or three events, the estimates in sections 3 and 4 include two or three post-event dummies.

If $\beta = 1$ fits the relationship between the treatment and control variables, the control group satisfies the common-trend condition of canonical DD estimation. Otherwise, the estimated value of β scales the employment rate of the control group (in logs) to generate the counterfactual series. That is, the counterfactual series is

$$\widehat{\ln e_{st}^T} = \hat{\alpha_s} + \hat{\beta} \ln e_{st}^C + \hat{\gamma}_s t \tag{2}$$

for each state. (With a cubic aggregate trend, we add $\hat{\gamma} t^2$ and $\hat{\delta} t^3$.) The coefficient on the control variable scales that variable to fit comovements of the treatment and control variables aside from the event.

The quality of the estimates depends on the quality of the counterfactual series. The minimum wage should not bind on the control group, but variation in the control group's employment should match variation in teen employment aside from the effects of the minimum wage. Popular identifying comparisons between treatment and control groups include (1) low-wage workers in the covered and uncovered sectors (Ashenfelter and Card 1981), (2) low-wage and high-wage establishments (e.g., Katz and Krueger 1992; Card and Krueger 1994; Hoffman and Trace 2009) or states (e.g., Card 1992b), and (3) affected and unaffected states (e.g., Card 1992a; Card and Krueger 1994; Hoffman and Trace 2009; Powers 2009; Clemens and Wither 2019) or adjacent counties (e.g., Dube, Lester, and Reich 2010).

A group makes for a satisfactory control if a linear (or possibly nonlinear) transformation of the group's employment rate satisfies the common-trend condition. That is, the event method produces a counterfactual series on the basis of a data-driven estimate of the relationship between the treatment and control groups' employment rates. Transforming an outcome variable to produce a counterfactual series for difference-in-difference estimation is not new. Indeed, the synthetic control method generates a counterfactual series on the basis of a data-driven weighted average of the outcome variables among unaffected states, countries, or other groups (e.g., Abadie, Diamond, and Hainmueller 2010). As β transforms the control group's employment rate to match movements of the teen employment rate, synthetic control transforms the outcome variables in unaffected groups to match movements in the treatment group's outcome variable.

Control Group. For the control group, I target people who tend to be high-wage workers, so the minimum wage would directly affect few of them. Employment-rate fluctuations in this group might be mild compared to those of teens and other low-wage workers, but the estimation strategy handles that concern.

Deere, Murphy, and Welch (1995) use the employment rate of men ages 15–64 to control for movements in the macroeconomy. That variable includes teens, who are the treatment group, as well as other low-wage people. So men ages 15–64 is not a legitimate control group.[1] To address this problem, I limit the control's age range to 25–59 and exclude high school dropouts. Prime-age high-school graduates are my control group.

Few workers in this control group earned wages between the old and new minimum

[1] Card (1992b) also uses the overall employment rate (including teens) to control for movements in the macroeconomy in his analysis of the effect of the 1990 hike in the federal minimum wage on teen employment. His fitted regression on annual data from 1975 to 1989 is $\widehat{e_t^T} = \text{constant} + 2.17 e_t - .86 t$, where e_t is the overall employment rate. (Card does not report the standard errors.) He notes that the "prediction equation tracks the actual teenage employment rate up to 1989 remarkably well" (p. 26).

Table 1: Workers with Wages Between the Old and New Minimum Wages

	1990–1991 Round		*2007–2009 Round*	
Period	Ages 15–19	HS+ Ages 25–59	Ages 15–19	HS+ Ages 25–59
1–12 months pre-first hike	40.1	3.7	40.6	4.0
1–12 months post-last hike	8.3	1.0	10.3	1.2
13–24 months post-last hike	5.0	0.8	7.5	1.0

Notes: Entries are percentages of wage and salary workers with hourly earnings greater than or equal to the old minimum wage ($3.35 or $5.15) and less than the new minimum wage ($4.25 or $7.25). Data are from the Current Population Survey's outgoing rotation group files, April 1989–March 1993 and August 2006–July 2011, in all 50 states and the District of Columbia.

wages in the two rounds of minimum wage hikes. Table 1 presents tabulations from the CPS's outgoing rotation group files. Less than 4 percent of prime-age high-school graduates earned between the old ($3.35) and new ($4.25) minimum wages before the first hike in April 1990; that percentage fell to 1 percent in months 1–12 and 13–24 after the April–1991 hike. In the 2007–2009 round of hikes, 4 percent of prime-age high-school graduates earned between the old ($5.15) and new ($7.25) minimum wages; that percentage fell to about 1 percent in the first and second years after the final hike in July 2009.

Many teens, however, earned between the old and new minimum wages before the initial hike, and the percentage in this treatment zone dropped rapidly in the months after the last hike. In the 12-month period before the April–1990 minimum wage hike, 40 percent of the working teens earned a wage between the old ($3.35) and new minimum wage ($4.25). See Table 1. That percentage dropped to 8 percent in the 12 months following the April 1991 hike and to 5 percent in months 13–24. In the 2007–2009 round of hikes, 41 percent of working teens earned between the old ($5.15) and new ($7.25) minimum wages in the 12 months before the July 2007 hike; that percentage fell to 10 percent in months 1–12 and 8 percent in months 12–24 after the final hike.

So the wage evidence confirms that many teens were treated with the higher minimum wage. A small part of the control group is, from the perspective of estimation, contaminated by the treatment. To the extent this is meaningful (e.g., is not due to measurement error in wage reports), difference-in-difference calculations tend to understate the effect of the minimum wage on the employment of teens.

Parsing the Event from Fluctuations and Trend

The 1990–1991 and 2007–2009 rounds of minimum wage hikes coincided with the 1990–1991 and 2007–2009 recessions. The challenge is to estimate how much of the sizable drop in teen employment in each round was caused by bumping up the minimum wage. How much of the decline in teen employment is due to the recession, and how much is left for the minimum wage to explain? To the extent that changes in a control group's employment rate deliver good predictions for changes in the teen employment rate in the absence of the new minimum wages, parsing the effects of the minimum-wage event from recession-driven job loss is easy enough.

Differential trends, however, add to the identification challenge. For instance, around the 2007–2009 hikes, the teen employment rate trended down relative to the employment rate of prime-age high-school graduates. Should we attribute the prodigious drop in the teen employment rate in the wake of the jumps in the minimum wage to (1) higher minimum wages, (2) the recession, or (3) the downward trend in teen employment. At least two of these three factors push toward lower teen employment. And separate identification of the three influences could be difficult in a short period. On the other hand, teen employment was trending up relative to employment of prime-age high-school graduates from the mid-1980s through the mid-1990s.

Fitting Teen Employment When the Minimum Wage Does Not Change

Readers of Deere, Murphy, and Welch (1995) might suspect that fluctuations in the aggregate employment rate of men are not up to the task of predicting fluctuations in the teen employment rate. Perhaps the resulting counterfactual series does not fall enough in the recession, so the estimator attributes too much of the gap between teen and control-group employment rates in the post-treatment period to the minimum wage hikes.

Identification hinges on the employment rates of teens and this group of older, educated people moving closely together. Why would employment of teens and prime-age high-school graduates move together? Perhaps a *common* factor drives fluctuations in labor demand across age and education groups in the same direction. The event method then scales the employment rate of older, educated people to match the magnitude of fluctuations in the teen employment rate. But if shifts in *relative* demands across age- and education-specific groups drive fluctuations in group employment rates, the method fails. For instance, if the main factor driving employment fluctuations were technological innovations that shift relative labor demands between younger and older workers, then the employment rates of teens and prime-age high-school graduates would move in opposite directions.

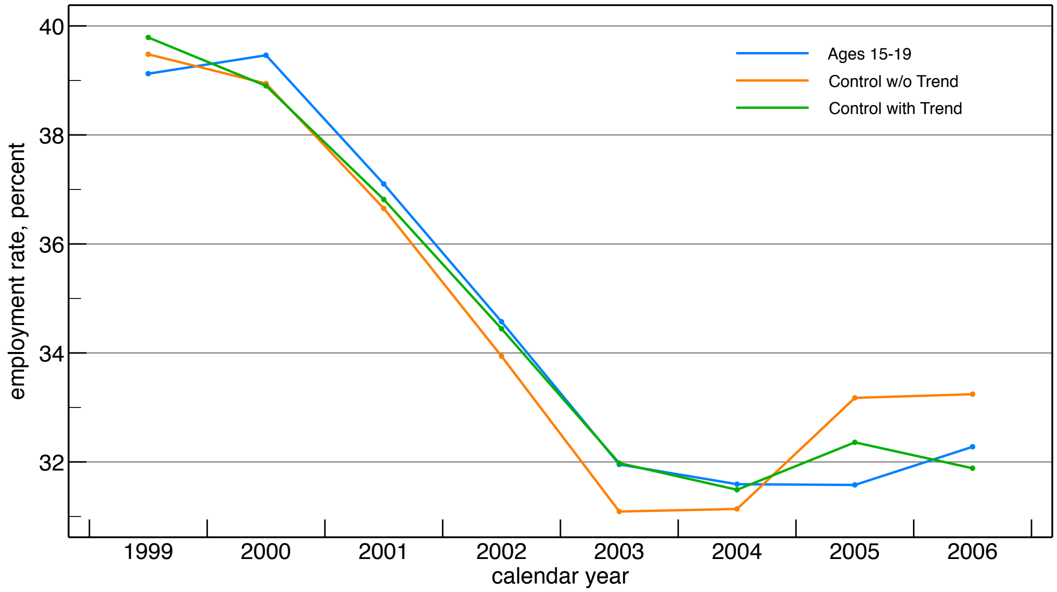
How well does the employment rate of prime-age high-school graduates predict the teen employment rate? To answer this question, I estimate the relationship in a period with a recession but without a minimum wage hike. The eight calendar years from 1999 through 2006 fit the bill. (The period starts 16 months after the September 1997 minimum wage hike and ends seven months before the July 2007 hike.) I exclude the 20 states with state minimum wages that bind anytime over the eight years. Figure 1 displays the sharp drop in the teen employment rate beginning in the 2001 recession. The figure also plots predicted values from a regression of the teen employment rate on the control group's employment rate and time.

$$\widehat{e_t^T} = \underset{(19.7)}{-116.8} + \underset{(0.27)}{2.14} e_t^C - \underset{(0.15)}{0.52} t \tag{3}$$

with $R^2 = .980$. (Both employment rates are expressed as percentages.) The teen employment rate doubles fluctuations in the control group's employment rate and trends down by about a half a percentage point per year relative to the control group. The figure also displays fitted values from a regression that omits the time trend.

With or without a time trend, fitted values capture the 8-percentage-point drop in the teen employment rate. And this result carries over across states, too. Column 1 of Table A1 in the appendix reports estimates of a regression on 248 state-year averages (with state fixed effects) and a quadratic aggregate trend. The estimate of β is 1.70 (with standard error

Figure 1: Fitting the Teen Employment Rate, 1999–2006



Notes: Sample draws from the 31 states with state minimum wages that never bind over the eight years of analysis. Equation 3 generates the "Control with Trend" series. Fitted values from $\widehat{e_t^T} = \underset{(18.8)}{-190.8} + \underset{(0.26)}{3.13} e_t^C$ $(R^2 = .933)$ produce the "Control w/o Trend" series. There is no evidence of first-order serial correlation of the residuals from this regression or equation 3. In addition, estimating equation 3 in first differences has a negligible effect on the estimates.

0.62), and the R^2 is .932. On average across states, the regression's fitted values for the teen employment rate drop 8.4 percentage points from 1999 to 2004. In addition, the scatter plot in Appendix Figure A1 shows that peak-to-trough changes in the teen employment rates are strongly correlated across states with changes in the fitted values of the regression ($\rho = .76$). Indeed, changes in state employment rates of prime-age high-school graduates, with the help of a quadratic time trend, predict changes in state employment rates of teens remarkably well.

The main takeaway from this exercise is that, in the context of the event method, prime-age high-school graduates work well as a control group. With the help of trends, including nonlinear trends in the state-level regressions, the employment rate of prime-age high-school graduates accurately tracks the teen employment rate.

Data

I draw the employment data from the Current Population Survey's basic monthly files. Employment is for pay in the private or public sectors; I exclude the self-employed and people in the armed forces. Teens are ages 15–19, and the control group includes people ages 25–59 who have completed at least 12 grades of school.[2]

[2]Many 15-year-olds worked in the late 1980s. In 1989, the calendar year before the 1990 minimum wage hike, the employment rate of 15-year-olds was 15 percent, which matched the employment rate of 67-year-olds. In the calendar year before the 2007 hike, the minimum wage of 15-years-olds was 7 percent, almost as high as the employment rate of 76-year-olds.

Table 2: Samples Sizes by Event

			Group	
Event	Period[a]	States (no.)	Ages 15–19	HS+, Ages 25–59
1990–1991 Hikes	1985–1995	All	1,352,295	7,568,763
		Binding Federal (33)	895,589	4,800,031
2007–2009 Hikes	2000–2013	All	1,573,694	9,854,336
		Binding Federal (18)	466,464	2,765,899

Notes: [a]Minimum-wage years; e.g., for the 1990–91 minimum wage hikes, 1985 refers to April 1985–March 1986, and 1995 refers to April 1995–March 1996.

Limiting the analysis to people in states without super-federal minimum wages requires a monthly panel of state minimum wages. I construct the monthly panel from Autor, Manning, and Smith's (2016) panel, Sutch's (2011) panel, January issues of the *Monthly Labor Review* ("State Labor Legislation Enacted in" the prior year), states' Department of Labor web sites, and news reports. Table A2 in the appendix indicates the states that make it into each round of analysis.

Table 2 lists the sample sizes over the two periods. For the 1990–1991 hikes, the period of analysis spans the 11 minimum-wage years from April 1985 through March 1996. In states with nonbinding state minimum wages throughout the 11-year period, I compute the employment rates of teens from nearly 900,000 responses; for the control group of prime-age high-school graduates, 4.8 million observations work their way into the employment-rate calculations. (Observations in these states comprise about 65 percent of the observations across all states.) For the 2007–2009 hikes, the analysis spans the 14 minimum-wage years from August 2000 through July 2014. In the 18 states where the federal minimum wage binds throughout, roughly 466,000 observations of teens and 2.8 million observations of prime-age high-school graduates comprise the sample, and that is nearly 30 percent of the sample across all states.

3. 1990–1991 Minimum Wage Hikes

Late in 1989, President George H.W. Bush signed the 1989 amendments to the Fair Labor Standards Act into law. The amendments raised the minimum wage to $3.80 on April 1, 1990 and to $4.25 on April 1, 1991. The amendments also established an 85-percent subminimum wage for teens during their first 90 days at work and eliminated special treatment of retail firms.

When the $3.80 minimum wage took effect on April 1, 1990, 11 states and the District of Columbia had state minimum wages higher than the new federal minimum wage. Six other states (four before and two after the first hike) had super-federal minimum wages sometime during the analysis period. So the federal minimum wage was binding in 33 states throughout the 11 years. Estimation focuses on these states.

The 1990–1991 recession nearly coincided with the federal minimum wage hikes. The NBER's business cycle dating marks the cyclical peak at July 1990, the fourth month with the new minimum wage, and the cyclical trough at March 1991, the month before the minimum wage rose to $4.25. The employment rate fell from 63.2 percent in January 1990

Table 3: Employment Before and After the 1990–91 Events[a]

	Rate		*Index*	
Minimum Wage Year[b]	Ages 15–19	HS+ Ages 25–59	Ages 15–19	HS+ Ages 25–59
1985	36.0	69.1	100.0	100.0
1986	36.5	69.7	101.3	100.9
1987	37.2	70.6	103.2	102.2
1988	38.6	70.8	107.2	102.5
1989	39.8	71.5	110.5	103.5
1990	36.7	71.3	101.7	103.3
1991	33.6	70.6	93.4	102.3
1992	33.5	70.8	92.9	102.5
1993	34.4	71.1	95.5	102.9
1994	35.3	71.6	97.9	103.6
1995	35.6	72.1	98.7	104.4

Notes: [a]States with state minimum wages that never bind during the analysis period. [b]Minimum-wage years begin in April of the indicated calendar year and end in March of the next year; e.g., 1990 refers to April 1990–March 1991.

to 61.2 percent in December 1991, so the period of declining employment contains the two minimum wage hikes. In addition, the unemployment rate peaked at 7.8 percent in June 1992, 15 months into the recovery.

Aggregate Patterns

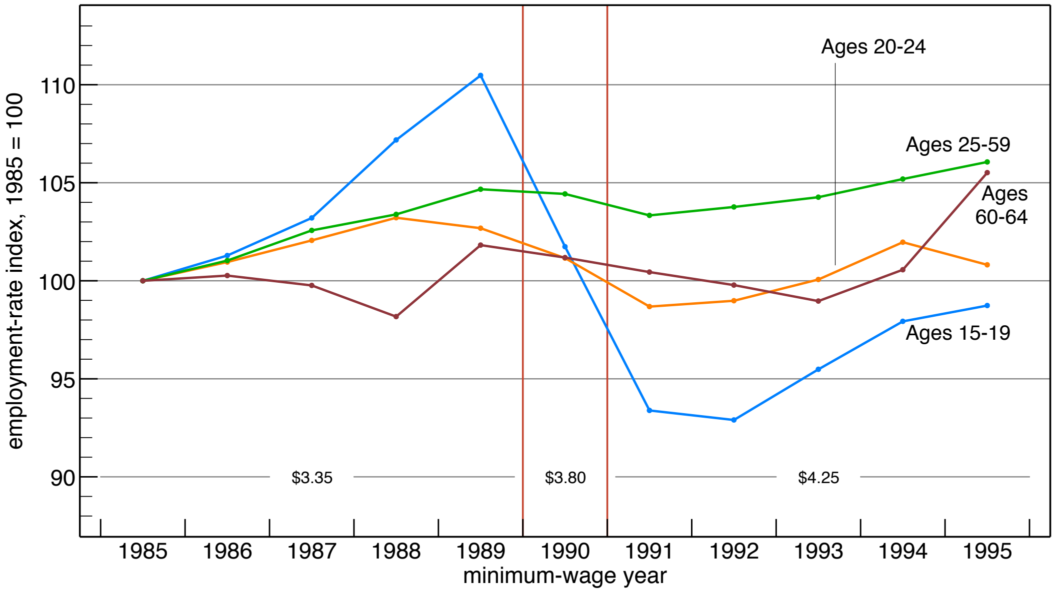
The teen employment rate trended up from 36 percent in minimum-wage year 1985 to nearly 40 percent in the 12 months before the first hike, but it dropped 3.1 percentage points in minimum-wage year 1990 and another 3.1 percentage points in minimum-wage year 1991. See Table 3. Although economic recovery began in minimum-wage year 1991, the teen employment rate in minimum-wage year 1995 remained 4.2 percentage points below its value in the year before the hike to $3.80 in April 1990.

The employment rate of prime-age high-school graduates also fell in minimum-wage years 1990 and 1991. But that drop was only 0.9 percentage points over two years. And the control group's employment rate more than fully recovered by minimum-wage year 1995.

Figure 2 expresses these patterns as indices for several age groups. The base year is 1985. (The last two columns of Table 3 also list the indices for teens and prime-age high-school graduates.) After growing 10.5 percent over the five years before the minimum wage hike, the teen employment-rate index fell 17.6 percent over the next three years. The before-and-after comparisons for older age groups are muted in comparison. Indeed, in Table 3, the employment-rate index for prime-age high-school graduates fell only 1 percent over those three years. Teen employment is quite sensitive to aggregate fluctuations, but the magnitudes in this table and figure suggest that something in addition to the recession drove teen job loss.

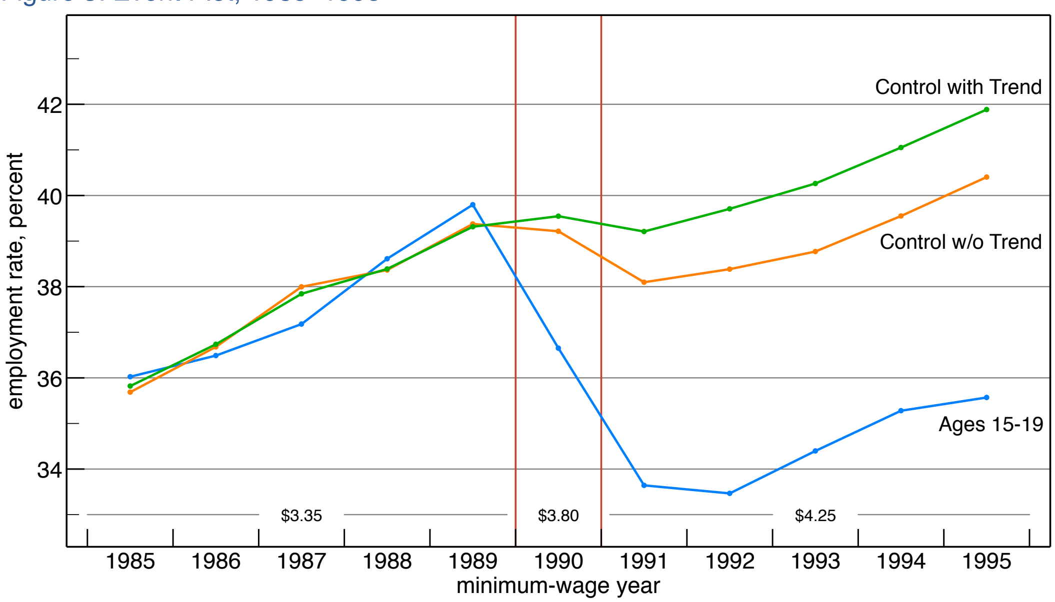
The patterns in Figure 2 and Table 3 might not inspire confidence in generating a

Figure 2: Employment-Rate Indices by Age, 1985–1995



counterfactual series on the basis of employment rates of older (and educated) people. As Figure 3 illustrates, however, fitted values from the regression of the teen employment rate e_t^T on the employment rate of prime-age high-school graduates e_t^C track the teen employment rate remarkably well before the first minimum wage hike. The fitted series, "Control w/o Trend" in the figure, is also close to parallel with the teen employment rate after

Figure 3: Event Plot, 1985–1995



Notes: Equation 4 (excluding the effects of the minimum-wage indicators) generates the "Control w/o Trend" series. Fitted values from $\widehat{e_t^T} = \underset{(59.24)}{-27.53} + \underset{(0.86)}{0.91} e_t^C + \underset{(0.43)}{0.33} t - \underset{(0.49)}{2.90} D_t^{90} - \underset{(1.80)}{5.95} D_t^{91-}$ $\left(R^2 = .968\right)$ form the "Control with Trend" series. There is no evidence of serially correlated residuals from this regression or equation 4, and estimates from a first-differenced specification are similar.

minimum-wage year 1991.

I regress the teen employment rate e_t^T on the employment rate of prime-age high-school graduates e_t^C and include dummy variables to indicate whether an observation is from 1990 (D_t^{90}) or from 1991 or after (D_t^{91-}). On the 11 aggregate data points, the estimated regression equation is

$$\widehat{e_t^T} = \underset{(13.19)}{-70.57} + \underset{(0.19)}{1.54}\, e_t^C - \underset{(0.37)}{2.56}\, D_t^{90} - \underset{(0.38)}{4.57}\, D_t^{91-} \tag{4}$$

with $R^2 = .964$. (Robust standard errors are in parentheses.) A 1 percentage point increase in the employment rate of the control group increases the employment rate of teens 1.5 percentage points. This relationship shifted down 2.6 percentage points in minimum-wage year 1990 and 4.6 percentage points in subsequent years. That is, on the basis of this simple regression, increasing the minimum wage to \$3.80 in April 1990 reduced the employment rate of teens 2.6 percentage points in minimum-wage year 1990, and the increase to \$4.25 a year later cut teen employment 4.6 percentage points thereafter. Indeed, the negative effect of the minimum wage hike on teen employment persisted well beyond minimum-wage year 1992, which is when Deere, Murphy, and Welch's analysis ends.

Adding an aggregate trend only boosts estimates of the disemployment effect. A second regression on aggregate data adds a linear time trend and generates the second counterfactual series in Figure 3. (See the regression estimates in the figure's notes.) The time trend, however, is not statistically significant.

The estimates also are not sensitive to including states with state minimum wages that bind sometime or throughout the analysis period. Deere, Murphy, and Welch note that only two states and DC had minimum wages above \$4.25 on April 1, 1990. So they do not exclude any states (or DC). But 18 "states" (including the district) had super-federal minimum wages at some time during the 11-year period of analysis. My estimates are from the sample of 33 other states, but the results are not sensitive to inclusion or exclusion of states with super-federal state minimum wages. And this is not surprising since many states increased their state minimum wages around the time of the federal increases. Overall, ignoring that the federal minimum wage did not bind in many states does not taint Deere, Murphy, and Welch's analysis.

The aggregate patterns indicate that recession-driven job loss of prime-age high-school graduates does not explain the sharp drop in teen employment beginning in minimum-wage year 1990 even though the control group's employment rate fits the teen employment rate at other times. And employment responses to the minimum wage hikes persisted well beyond the period that Deere, Murphy, and Welch analyze.

State-Level Regressions

The evidence from regressions on state-year averages fortifies the evidence from annual averages. Table 4 presents estimates from regressions of the teen employment rate (in logs) on the employment rate of prime-age high-school graduates (in logs) and three indicators of minimum-wage years: 1990, 1991, and 1992–1995. All specifications also include state fixed effects. I limit the sample to the 33 states with state minimum wages that never bind

Table 4: Teen Employment-Rate Regressions on State-Year Averages, 1985–1995[a]

		States with State Minimum Wages That Never Bind								
	All States (1)	Full Sample (2)	Full Sample (3)	Full Sample (4)	Women (5)	Men (6)	Blacks (7)	Whites (8)	Ages 15–17 (9)	Ages 18–19 (10)
Constant	0.11	−0.31	−0.45	−0.69	−0.70	−0.22	−0.12	−0.50	−0.66	−0.24
	(0.08)	(0.15)	(0.19)	(0.21)	(0.24)	(0.18)	(0.35)	(0.19)	(0.23)	(0.16)
ln(Employment Rate),	2.34	2.33	2.01	1.23	1.44	2.55	5.42	1.56	2.40	1.64
Ages 25–59, HS+	(0.21)	(0.38)	(0.46)	(0.55)	(0.60)	(0.44)	(0.87)	(0.47)	(0.57)	(0.38)
Year Effects$\times$100										
1990	−6.74	−6.54	−8.58	−9.19	−9.84	−7.49	−15.66	−6.77	−15.16	−5.52
	(1.15)	(1.53)	(1.47)	(1.55)	(1.71)	(1.92)	(4.09)	(0.47)	(2.20)	(1.41)
1991	−12.53	−12.73	−15.95	−17.97	−18.81	−13.30	−23.57	−14.13	−24.18	−9.61
	(1.39)	(2.00)	(2.75)	(3.78)	(3.82)	(2.43)	(7.52)	(2.96)	(3.27)	(2.23)
1992–1995	−13.78	−12.40	−17.47	−20.28	−21.44	−13.89	−22.59	−15.88	−24.15	−9.07
	(1.22)	(1.85)	(3.01)	(4.51)	(3.93)	(3.24)	(6.89)	(3.43)	(3.76)	(2.55)
Year			0.009		0.017	0.001	0.010	0.009	0.012	0.003
			(0.004)		(0.005)	(0.005)	(0.009)	(0.005)	(0.006)	(0.003)
State Trends[b]				3.77						
				[0.000]						
N	561	363	363	363	363	363	342	363	363	363
R^2	.918	.904	.906	.928	.871	.856	.745	.882	.902	.837
Teen Employment Rate, 1989	40.7	39.8	39.8	39.8	39.3	40.3	24.8	43.7	28.8	54.9

Notes: [a] The dependent variable is the natural log of the employment rate of teens, ages 15–19. The teenage population weights the least squares estimates on state (51 or 33) $\times$ year (11) observations from the basic monthly CPS files, April 1985 to March 1996. Minimum-wage years begin in April of the indicated calendar year and end in March of the next year; e.g., 1990 refers to April 1990–March 1991. Each regression includes state fixed effects. Cluster-robust standard errors are in parentheses. [b] F-statistic $F(32, 293)$, which is calculated using robust estimates of the variance-covariance matrix, tests of the null hypothesis that all state trends are equal. p-value is in brackets.

over the analysis period, although I include estimates from all states (column 1) for comparison. The various specifications in the table explore the influences of trends, as well as differences by sex, race, and age.

For the full sample (columns 2–4), there is clear evidence of large and statistically significant disemployment effects of the minimum wage hikes. In each specification, teen employment is strongly linked to employment in the control group. Including the linear aggregate trend (column 3), which is positive and statistically significant, magnifies the disemployment effects. This confirms the pattern in Figure 3. These estimates of the effects of the minimum wage hikes on the log of the teen employment rate are -8.6 percent for 1990, -16.0 percent for 1991, and -17.5 percent for 1992–1995.

The estimated employment effects in this round of minimum wage hikes are robust to including state-specific trends. Indeed, point estimates of eventual effect of the \$4.25 federal minimum wage are -17.5 percent with a linear aggregate trend (column 3) and -20.3 percent with linear state-specific trends (column 4). Estimates of the three minimum wage effects with state trends are not significantly different from the estimates with a linear trend in column 3 (p–value $=$ .29). Including state-specific trends, however, reduces the estimate of β, shifting some of the explanation for fluctuations in teen employment from the comovements with prime-age high-school graduates to state trends.

So the teen employment-rate regressions on state-year averages reveal a large disemployment effect for teens in the 12 months immediately following the first event, and the disemployment effect roughly doubles in subsequent years.

To express the eventual effect—the 17.5 percent fall in the teen employment rate—as an elasticity, divide by 26.9 percent, the percentage increase in the minimum wage from \$3.35 to \$4.25. The ratio is $-.65$, so a 10 percent increase in the minimum wage reduces teen employment by 6.5 percent. To express the eventual effect in terms of the teen employment rate, multiply -17.5 by 39.8 percent, the teen employment rate in the year before the first minimum wage hike. The bump to a \$4.25 minimum wage eventually cut the teen employment rate by 7.0 percentage points.

By Groups. Without exception, the disemployment effects of these minimum wage hikes are large and statistically significant for teenage women and men, teenage blacks and whites, and younger teens and older teens. Columns 5–10 of Table 4 report the results.

Teenage women suffered the lion's share of minimum-wage-driven job loss. In columns 5 and 6, the estimated employment effects of the \$4.25 minimum wage in minimum-wage years 1992–1995 convert to elasticities of $-.80$ for teenage women and $-.52$ for teenage men, and the difference is statistically significant (p–value $= .04$). In addition, increasing the minimum wage in 1990 and 1991 reduced the employment rate of teenage women 8.4 percentage points (from 39.3 to 30.9 percents) and of teenage men 5.6 percentage points (from 40.3 to 34.7 percent).

On the basis of the point estimates in columns 7–8 of Table 4, the minimum wage hikes reduced employment of teenage blacks more than teenage whites in *percentage* terms but a bit less in absolute terms. The substantially lower employment rate of black teens before the 1990 minimum wage hike explains the difference. Although estimates of the minimum wage effects for black teens are not precise, they are significantly larger than the estimates for white teens (p–value $= .0003$). And the implied elasticities are $-.84$ for black teens and $-.59$ for white teens. The hikes reduced the employment rate of black teens 5.6 percentage points (from 24.8 to 19.2 percent); for white teens, these minimum wage hikes reduced the employment rate 6.9 percentage points (from 43.7 to 36.8 percent).

The effect of the 1990–1991 round of minimum wage hikes is most striking for younger teens. Estimates of the minimum wage effects for younger teens are much larger than the estimates for older teens (p–value $= .0000$). The elasticity is $-.90$ for teens ages 15–17, which is substantially larger than $-.34$, the elasticity for teens ages 18–19. On the basis of the estimates in column 9, increasing the minimum wage from \$3.35 to \$4.25 sliced the employment rate of younger teens 7 percentage points (from 28.8 percent to 21.8 percent). This round of minimum wage hikes reduced the employment rate of older teens 5 percentage points (from 54.9 to 49.9 percent).

4. 2007–2009 Minimum Wage Hikes

President George W. Bush signed the Fair Minimum Wage Act of 2007 into law on May 25, 2007. The act amended the Fair Labor Standards Act to raise the federal minimum wage from \$5.15 to \$5.85 on July 24, 2007, to \$6.55 on July 24, 2008, and to \$7.25 on July 24, 2009.

When the law took effect in 2007, 30 states and the District of Columbia had super-federal state minimum wages. By July 2009, only 13 states and DC had super-federal state minimum wages. The federal minimum wage binds throughout the period I analyze (i.e., August 2000 through July 2014) in 18 states. Another seven states simply "scooped" the federal hike. For instance, West Virginia matched the federal hikes 12 months in advance; Iowa raised its minimum wage to $6.20 in April 2007 and to $7.25 in January 2008; and Pennsylvania scooped the federal minimum wage hikes by raising its minimum wage to $6.25 in January 2007 and to $7.15 in July 2007. None of these seven states had a super-federal state minimum wage when the federal minimum wage reached $7.25 in July 2009.

Like the 1990–1991 round of federal minimum wage hikes, this round coincided with a recession. According to the NBER's business cycle dating, economic activity peaked in December 2007, the fifth month with the new minimum wage. The U.S. Department of the Treasury sent out tax rebate checks in the summer of 2008, and the federal government extended eligibility for unemployment insurance benefits from 26 weeks to 39 weeks in most states in November 2008 and to 99 weeks in some states in April 2010. The economy reached a cyclical trough in June 2009, a month before the minimum wage rose to $7.25. Although the unemployment rate peaked at 10.0 percent in October 2009, four months into the recovery, the employment rate fell for four and a half years from 63.4 percent in December 2006 to 58.2 percent in July 2011. So the period of declining employment contains all three minimum wage hikes.

Aggregate Patterns

The teen employment rate trended down from almost 36 percent in minimum-wage year 2000 to 30 percent in the 12 months before the first hike, but it dropped 1.4 percentage points in minimum-wage year 2007 and another 6.5 percentage points in minimum-wage years 2008 and 2009. See Table 5. Although economic recovery began toward the end of minimum-wage year 2008, the teen employment rate in minimum-wage year 2013 remained 7.2 percentage points below its value in the year before the July–2007 hike to $5.85.

The employment rate of prime-age high-school graduates did not fall in minimum-wage year 2007. The control group's employment rate fell 3.6 percentage points from minimum-wage year 2007 to 2009 and recovered from there. Although this is small in comparison to the drop in teen's employment rate, it's more than triple the magnitude of the drop in the employment rate of prime-age high-school graduates around the 1990–1991 round of minimum wage hikes. Furthermore, the control group's employment had not fully recovered by minimum-wage year 2013.

Figure 4 expresses these patterns as base-year-2000 indices for several age groups. (The last two columns of Table 5 also list the indices for teens and prime-age high-school graduates.) After slipping 15.6 percent over the six years before the minimum wage hike, the teen employment-rate index fell 25.2 percent over the next four years. The before-and-after comparisons for older age groups pale in comparison. Indeed, in Table 5, the employment-rate index for prime-age high-school graduates fell 5 percent over two years, which is huge except in comparison to the fall in the teen's index.

Figure 5 reveals the importance of differential trends in this round of minimum wage hikes. Fitted values from the regression of the teen employment rate e_t^T on the employment rate of prime-age high-school graduates e_t^C without a trend variable understate the fall in the teen employment rate before the minimum wage hike in 2007. Adding a trend variable improves the fit over the first seven years. The fitted series, "Control with Trend" in the figure, is also close to parallel with the teen employment rate between minimum-wage years 2010 and 2013. So I focus on the counterfactual that is fitted with a trend.

I regress the teen employment rate e_t^T on the employment rate of prime-age high-school graduates e_t^C, a quadratic time trend, and dummy variables to indicate whether an observation is from minimum-wage years 2007–2009 $\left(D_t^{07-09}\right)$ or 2010–2013 $\left(D_t^{10-}\right)$. On the 14 aggregate data points, the estimated regression equation is

$$\widehat{e_t^T} = \underset{(7.2)}{-81.5} + \underset{(0.10)}{1.61}\, e_t^C - \underset{(0.14)}{0.74}t + \underset{(0.009)}{0.025}t^2 - \underset{(0.42)}{1.18}\, D_t^{07-09} - \underset{(0.61)}{3.41}\, D_t^{10-} \tag{5}$$

with $R^2 = .995$. (Robust standard errors are in parentheses.) A 1 percentage point increase in the employment rate of the control group increased the employment rate of teens 1.6 percentage points. The teen employment rate trended down (relative to the employment rate in the control group) at a decreasing rate. This relationship shifted down 1.2 percentage points in minimum-wage years 2007–2009 and 3.4 percentage points beginning in minimum-wage year 2010. Including the trend terms trims the disemployment effect of the three-part rise in the minimum wage from 5.5 percentage points to 3.4 percentage points.

An unfortunate feature of this round is that there are only 18 states with state minimum

Table 5: Employment Before and After the 2007–09 Events

	Rate		Index	
Minimum Wage Year[a]	Ages 15–19	HS+ Ages 25–59	Ages 15–19	HS+ Ages 25–59
2000	35.6	73.0	100.0	100.0
2001	33.3	71.7	93.7	98.2
2002	30.9	71.0	86.8	97.3
2003	29.5	70.5	82.8	96.6
2004	29.6	71.1	83.3	97.4
2005	29.8	71.1	83.9	97.4
2006	30.0	71.3	84.4	97.7
2007	28.6	71.7	80.4	98.3
2008	25.0	69.4	70.5	95.1
2009	22.1	68.1	62.1	93.3
2010	21.0	68.7	59.2	94.1
2011	21.9	69.3	61.7	95.0
2012	21.8	69.9	61.4	95.8
2013	22.8	69.9	64.0	95.8

Notes: [a]Minimum-wage years begin in August of the indicated calendar year and end in July of the subsequent year; e.g., 2007 refers to August 2007–July 2008. States with state minimum wages that never bind during the analysis period.

Figure 4: Employment-Rate Indices by Age, 2000–2013

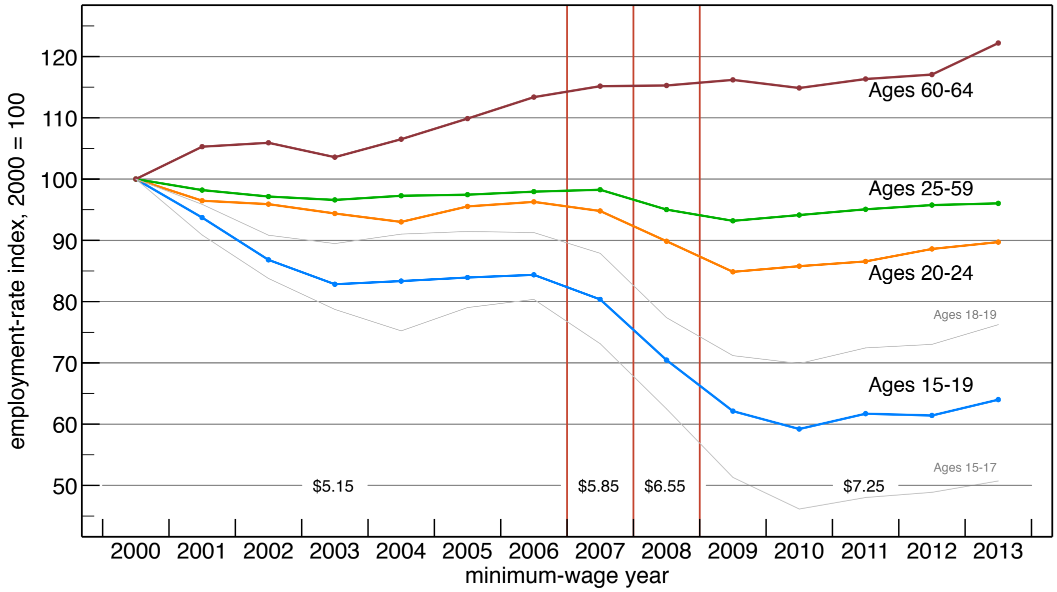
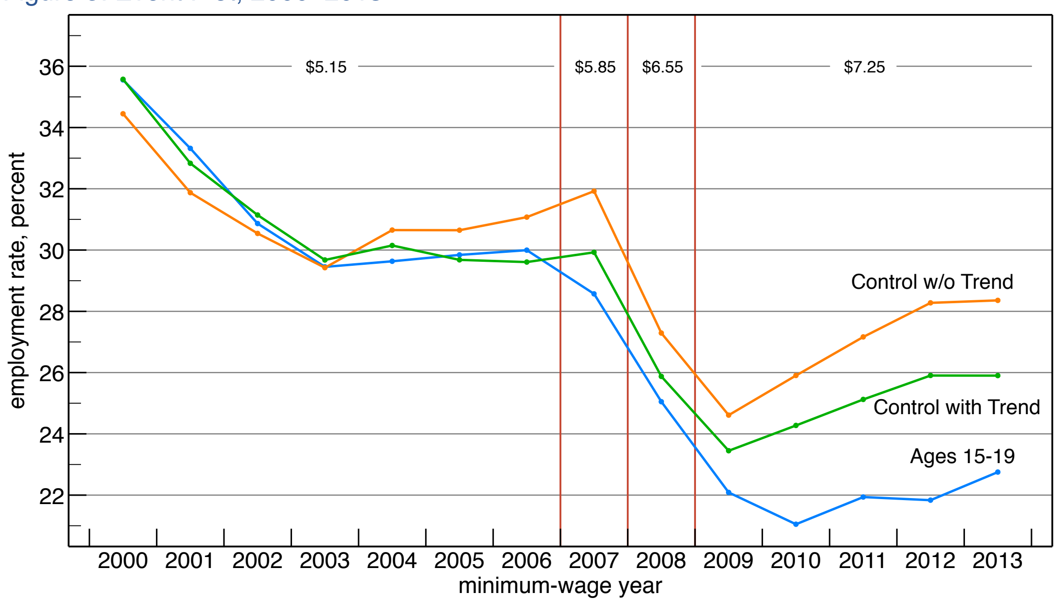


Figure 5: Event Plot, 2000–2013



Notes: Equation 5 (excluding the effects of the minimum-wage indicators) generates the "Control with Trend" series. The regression $\widehat{e_t^T} = -\underset{(17.2)}{111.6} + \underset{(0.24)}{2.00}\, e_t^C - \underset{(0.65)}{2.71}\, D_t^{07\text{–}09} - \underset{(0.62)}{5.53}\, D_t^{10\text{–}} \left(R^2 = .972\right)$ gives the fitted values for the "Control w/o Trend" series. The residuals from this regression, unlike equation 5, are serially correlated. Correcting for first-order serial correlation has a negligible effect on the estimates. In addition, estimating equation 5 in first differences has little effect on the estimates.

wages that do not bind at any time during the period of analysis. Although the patterns are a little different for the states with always-binding state minimum wages, the patterns for all states are similar to the patterns for the 18 states. One reason for this is that 22 of the 30 states (and DC) with super-federal state minimum wages in July 2007 increased their minimum wages over the next 24 months. In the state-level regressions, I also check the

Table 6: Teen Employment-Rate Regressions on State-Year Averages, 2000–2013[a]

		States with State Minimum Wages That Never Bind								
	All States (1)	Full Sample (2)	Full Sample (3)	Full Sample[b] (4)	Women (5)	Men (6)	Blacks (7)	Whites (8)	Ages 15–17 (9)	Ages 18–19 (10)
Constant	−0.08	−0.35	−0.38	−0.53	−0.45	−0.32	−0.64	−0.33	−0.57	−0.18
	(0.16)	(0.21)	(0.19)	(0.19)	(0.21)	(0.22)	(0.47)	(0.15)	(0.23)	(0.17)
ln(Employment Rate), Ages 25–59, HS+	2.23	2.50	1.87	1.11	1.50	2.25	2.58	1.85	2.23	1.50
	(0.43)	(0.66)	(0.59)	(0.64)	(0.64)	(0.65)	(1.59)	(0.43)	(0.70)	(0.55)
Year Effects × 100										
2007	−12.64	−9.42	−4.81	−5.19	−4.83	−4.50	−6.38	−4.34	−8.58	−4.67
	(1.27)	(2.17)	(2.48)	(2.42)	(3.11)	(3.21)	(7.77)	(1.96)	(3.63)	(2.67)
2008	−17.80	−15.03	−12.92	−16.59	−9.97	−16.25	−4.88	−12.14	−19.10	−13.74
	(1.59)	(2.67)	(3.82)	(3.82)	(5.29)	(5.43)	(10.84)	(4.04)	(6.07)	(3.57)
2009–2013	−31.10	−27.39	−26.10	−31.91	−25.06	−27.28	−23.18	−23.03	−43.34	−22.09
	(2.23)	(2.20)	(4.16)	(3.78)	(6.30)	(5.87)	(11.85)	(4.34)	(6.92)	(4.83)
Cubic Trend[b]			✓	✓	✓	✓	✓	✓	✓	✓
State Trends[c]				5.29						
				[0.000]						
N	714	252	252	252	252	252	231	252	252	252
R^2	0.914	0.915	0.930	0.944	0.888	0.878	0.598	0.922	0.919	0.873
Teen Employment Rate, 2006	30.0	30.0	30.0	30.8	30.4	29.6	20.9	32.6	19.7	48.0

Notes: [a]The dependent variable is the natural log of the employment rate of teens, ages 15–19. The teenage population weights the least squares estimates on state (51 or 18) × year (14) observations from the basic monthly CPS files, August 2000 to July 2014. Minimum-wage years begin in August of the indicated calendar year and end in July of the next year; e.g., 2007 refers to August 2007–July 2008. Each regression includes state fixed effects. Cluster-robust standard errors are in parentheses. [b]The regression with state-specific trends (column 4) includes the squared and cubed terms but omits the linear term. [c]F-statistic $F(17, 210)$, which is calculated using robust estimates of the variance-covariance matrix, tests the null hypothesis that all state trends are equal. p-value is in brackets.

sensitivity of the estimates to including the seven states that scooped the federal minimum wage hikes.

State-Level Regressions

The evidence from regressions on state-year averages strengthens the evidence from annual averages. Table 6 presents estimates from regressions of the teen employment rate (in logs) on the employment rate of prime-age high-school graduates (in logs) and three indicators of minimum-wage years: 2007, 2008, and 2009–2013. All specifications also include state fixed effects. I limit the sample to the 18 states with state minimum wages that never bind over the analysis period, although I also report estimates from all states (column 1) for comparison. And the sample period is 14 years. The various specifications in the table explore the influence of trends, sensitivity to including states that scoop the federal hike, and differences by sex, race, and age.

For the regressions with or without a nonlinear aggregate trend (columns 2–3), teen employment is strongly linked to employment in the control group, and the employment effects of the minimum wage hikes are large and statistically significant after 2007. Unlike the aggregate pattern in Figure 5, the point estimates are insensitive to including a trend. The aggregate trend in column 3 is cubic, and that explains the difference. Imposing a linear aggregate trend in a regression on state-year averages generates much smaller estimates of the employment effects (in absolute value) than those in columns 2–3. With a linear aggregate trend, the large drop in the teen employment rate from minimum-wage years 2000 to 2003 produces a sharp downward trend, inflates the coefficient on the control variable, and shrinks the disemployment effects of the minimum wage.[3] For this round of minimum wage hikes, estimates of the disemployment effects on data that exclude minimum-wage years 2000 and 2001 and without any trend match the estimates from all years with a cubic trend (column 3). (See column 2 of Table A1 in the appendix.) So the data call for a nonlinear trend because of the undue influence the 2001 recession has on the estimate of a linear trend.

The estimates are also robust to state-specific trends. The regression in column 4 includes state trends as well as squared and cubed terms for the aggregate trend. The estimated disemployment effects are statistically significant and large. Estimates of the three minimum wage effects with state trends in column 4 are not significantly different from the estimates with a cubic aggregate trend in column 3 (p–value $=$ .58). Including the state trends, however, depresses the estimate of β, as it did in the 1990–1991 round.

For the specification with a cubic aggregate trend (column 3), estimates of the effects of the minimum wage hikes on the log of the teen employment rate are -4.8 percent for 2007, -12.9 percent for 2008, and -26.1 percent for 2009–2013. The estimates for 2008 and 2009–2013 are statistically significant.

To express the eventual effect—the 26 percent fall in the teen employment rate—as an elasticity, divide by 40.8 percent, the percentage increase in the minimum wage from \$5.15 to \$7.25. The ratio is $-.64$, so a 10 percent increase in the minimum wage cuts teen employment 6.4 percent. To express the eventual effect in terms of the teen employment rate, multiply -26.1 by 30.0 percent, the teen employment rate in the year before the 2007 minimum wage hike. Hiking the minimum wage to from \$5.15 to \$7.25 cut the teen employment rate by 7.8 percentage points. Recall that the teen employment rate fell 9 percentage points between 2006 and 2010.

Since seven states scooped the federal minimum wage hikes by simply raising their state minimum wages in advance of the federal hikes (but ending up without a binding state minimum wage in July 2009), I can expand the sample of states to 25 without departing appreciably from the event design. Table A1 in the appendix reports estimates from regressions without (column 3) and with (column 4) state-specific trends on data that include these seven states. The estimated disemployment effects shrink a bit. For instance, in the specification with a cubic aggregate trend, including the seven scooping states depresses the eventual effect of the minimum wage hikes from -26.1 to -23.2. But the estimated disemployment effects in the broader sample do not differ significantly from the baseline estimates in column 3 of Table 6; indeed, the estimates are robust to including the seven scooping states.

By Groups. The disemployment effects of these minimum wage hikes are large and statistically significant for teenage women and men, teenage whites and blacks, and younger

[3] Neumark, Salas, and Wascher (2014, p. 616) argue that estimates of employment trends are sensitive to recessions near the beginning or end of a panel, and biased trend estimates bias estimated minimum wage effects. To deal with this *end-point bias*, they suggest including nonlinear trends and varying the sample period.

teens and older teens. Except for the comparison by age, significant disemployment effects by group do not consistently emerge until 2009. Columns 5–10 of Table 6 report the results.

Teenage women and men shared the brunt of the job loss in this round of minimum wage hikes. Indeed, estimates of the employment effects for women in column 5 do not differ significantly from the estimates for men in column 6. The estimates of the eventual effects in columns 5 and 6 convert to elasticities of $-.61$ for teenage women and $-.67$ for teenage men. In addition, increasing the minimum wages in 2007, 2008, and 2009 reduced the employment rate of teenage women 7.6 percentage points (from 30.4 to 22.8 percents) and of teenage men 8.1 percentage points (from 29.6 to 21.5 percent). Recall that teenage women suffered most of the job loss from the 1990–1991 minimum wage hikes.

In this round of federal minimum wage hikes, there is no evidence that the new minimum wages reduced employment of black teens more than white teens in *percentage* terms. Indeed, the elasticities are $-.57$ for black teens and $-.56$ for white teens for the hike to the $7.25 minimum wage. The estimated effect of the $7.25 minimum wage on the employment rate of black teens is large (-23.2) but imprecisely estimated ($t = 1.96$). Since the base employment rate was lower for black teens, the drop in the employment rate was smaller for black teens (4.8 percentage points from 20.9 to 16.1 percent) than for white teens (7.5 percentage points from 32.6 to 25.1 percent).

The estimated employment effects of the 2007–2009 round of minimum wage hikes are significantly larger for teens ages 15–17 (p–value $= .002$), which translates into more-elastic employment responses from these younger teens. The elasticity is -1.06 for younger teens, which is much larger (in absolute value) than $-.54$, the elasticity for teens ages 18–19. On the basis of the estimates in column 9, increasing the minimum wage from $5.15 to $7.25 cut the employment rate of younger teens 8.5 percentage points (from 19.7 percent to 11.2 percent). This round of minimum wage hikes reduced the employment rate of older teens 10.6 percentage points (from 48.0 to 37.4 percent). Despite younger teens' more-elastic response to the minimum wage hikes, the employment rate of younger teens fell less than the employment rate of older teens; the substantially lower employment rate of younger teens explains the difference.

Comparison with the 1990–1991 Round. To compare the magnitudes of the employment effects across the two events, I focus on the eventual effects for the specifications with aggregate trends but without state-specific trends. Those estimated effects are -17.5 percent in column 3 of Table 4 and -26.1 percent in column 3 of Table 6. Although the disemployment effect in the 2007–2009 round is larger, the increase in the minimum wage was also larger in the recent round (40.1 percent) than in the earlier round (26.9 percent). In fact, the two elasticities are essentially equal: $-.65$ in the 1990–1991 round and $-.64$ in the 2007–2009 round. But there is nothing profound about this equality. Indeed, in section 6's analysis of labor demand elasticities, I show that the demand for teen labor in the latest round of minimum wage hikes must have been more elastic than in the earlier round to produce the larger disemployment effects. The wage increases alone in the latest round were not big enough.

In columns 3–10 of Table 6, with few exceptions, statistically significant employment effects do not appear until minimum-wage years 2008 or even 2009. Unlike the 1990 minimum wage hike, the 2007 hike had at best a marginally significant effect on teen em-

Table 7: Wages in the Treatment Zone, Minimum-Wage Years 1989 and 2006

	Minimum Wage Hike				
	1990	1991	2007	2008	2009
Teens					
Percentage in the Treatment Zone[a]	32.6	50.4	14.3	34.7	49.8
Average Percentage Increase in the Wage[b]	3.3	9.1	1.4	4.5	9.6
High School Dropouts					
Percentage in the Treatment Zone[a]	9.6	17.8	2.7	10.2	18.5
Average Percentage Increase in the Wage[b]	0.9	2.7	0.2	1.0	2.6

Notes: Data are from the Current Population Survey's outgoing rotation group files, April 1989–March 1990 and August 2006–July 2007, for states with state minimum wages that never bind during the analysis period. [a]Entries are percentages of hourly wage workers with hourly wages greater than or equal to the pre-event minimum wage and less than the new minimum wage for the indicated year. For example, 50.4% of teens who were paid by the hour reported wages in minimum-wage year 1989 in the range [\$3.35, \$4.25), the 1989 and April–1991 values of the minimum wage. [b]Entries are the average percentage increases in the pre-event wage to reach the minimum wage in the indicated minimum-wage year; wage increases of workers with wages above the new minimum wage are set to zero.

ployment. In fact, the teen employment elasticities with respect to the minimum wage were $-.64$ in minimum-wage year 1990 and $-.31$ in minimum-wage year 2007.

One possibility is that the 2007 hike to \$5.85 did not directly affect many teens (Hoffman 2014). Indeed, the 1990 minimum wage directly affected more teens than the 2007 minimum wage, and the difference is quite large. Table 7 displays how the percentage of teen workers with wages in the treatment zone in the 12 months before the first minimum wage hike varied with each hike. In minimum-wage year 1989, the wages of 32.6 percent of hourly paid teens were in the treatment zone between the existing \$3.35 minimum wage and the \$3.80 minimum wage coming in April 1990. In minimum-wage year 2006, only 14.3 percent of hourly paid teens earned at least the \$5.15 minimum wage at the time but less than the \$5.85 wage coming in July 2007. So fewer—and not many—teens were treated with the \$5.85 wage in minimum-wage year 2007. [4]

5. Year Effects and Scaled-Control DD Estimates

The event specification in equation 1, as well as the estimates in Tables 4 and 6, excludes year effects. Since identification of the minimum wage effects is off the coefficients on the year dummies, the event method does not separately identify the effects of the minimum wage and the time periods on employment. For instance, the 17.5 percent drop in teen employment in minimum-wage years 1992–1995 could have been a -17.5 percent year effect in each of those four years. Deere, Murphy, and Welch (1995, p. 235–36), however, favor the minimum wage interpretation on parsimony grounds. They show that replacing the employment rate of the control group and the minimum wage dummies with year dummies

[4]Table 7 also presents the average percentage increase in the pre-hike wage to reach each of the five new minimum wages. For the typical teen, the bump to reach the new minimum wage was smaller in 2007 than in 1990. Although the statutory increase in the minimum wage from 1989 to 1990 was 13.4 percent, teen wages in minimum-wage year 1989 had to grow by only 3.3 percent to reach the 1990 minimum wage (\$3.80). The statutory increase in the minimum wage from 2006 to 2007 was 13.6 percent, but the data from minimum-wage year 2006 reveal that teen wages had to rise by merely 1.4 percent to reach the 2007 minimum wage. Although the two statutory increases were essentially equal in percentage terms, the average distance (from below) to the new minimum wage was much shorter in 2007 because far fewer teens were in the treatment zone.

does not improve the fit, which suggests that year effects are not important. Nevertheless, the estimated minimum wage effects could be nothing but year effects.

Canonical DD estimation does separately identify minimum wage and year effects. Fluctuations that are common to the treatment and control groups identify the year effects. Differential effects for the treatment group after the treatment identify the treatment effect, the effect of minimum wage hikes in the current context. Applying this method with a control group of prime-age high-school graduates does not work, however, because the employment rate of teens moves more than one-for-one with variation in the employment rate of prime-age high-school graduates. This violates the common-trend requirement. So my strategy is to transform the employment rate of the control group to satisfy the requirements of canonical DD estimation. The result is a two-step estimator that separately identifies minimum wage effects and year effects.

A first-step regression (in logs) of the teen employment rate on the employment rate of prime-age high-school graduates, as well as trends and state effects, fits the comovements of the two series by estimating the value of the scale parameter β. In fact, Tables 4 and 6 present first-step estimates of β. With $\hat{\beta}$ in hand, a mean-preserving spread transforms the employment rate of the control group. This amounts to generating a new control group that resembles the prime-age high-school graduates but with larger fluctuations in employment.

On pooled teen and scaled-control data, the second-step regresses the employment rate (in logs) on teen, state, and year dummy variables, a linear differential trend for teens, and interactions of the teen dummy with the minimum-wage dummies. Aside from scaling the employment rate of the control group, this is canonical difference-in-difference estimation. But since an estimate of β transforms the dependent variable, I apply a nonparametric bootstrap with 1000 replications to the two-step procedure to generate the standard errors. These standard errors reflect sampling variability in β, as well as the usual sampling variability in the second-step regression. The standard errors also reflect clustering by state.

I report two varieties of scaled-control DD estimates in Table 8. The first variety estimates the first and second steps on the same sample period. For instance, for the 1990–1991 round, I estimate β in the first step on data from the same 11-year period as I estimate the employment effects in the second step. For this one-sample variety, the estimates are $\hat{\beta} = 2.01$ for the 1990–1992 round (Table 4, column 3) and $\hat{\beta} = 1.87$ for the 2007–2009 round (Table 6, column 3). Alternatively, I estimate β on a period without the complication of a minimum wage hike. Column 1 of Table A1 presents a regression with $\hat{\beta} = 1.70$ on the placebo period, calendar years 1999 to 2006. This second method generates two-sample scaled-control DD estimates of the employment effects of the federal minimum wage.

Table 8 displays scaled-control DD estimates of the effects of the 1990–1991 and 2007–2009 rounds of minimum wage hikes on teen employment rates. These estimates resemble the event-method estimates in Table 4, and differences between the one-sample and two-sample estimates are minor. Statistically significant disemployment effects emerge by minimum-wage year 1991 in the 1990–1991 round. For instance, the eventual effect of the 1991 minimum wage hike is -17.1 percent in the event estimates from Table 4, -11.5 percent in the one-sample estimates in Table 8, and -18.2 percent in the two-sample estimates. In the latest round of hikes, the eventual effect of raising the minimum wage to \$7.25 in 2009 is -26.1 percent in the event estimates from Table 6, -24.2 percent in

Table 8: Teen Employment-Rate Regressions on State-Year Averages: Scaled-Control DD Estimates[a]

	1985–1995		2000–2013	
	1 Sample (1)	2 Sample (2)	1 Sample (3)	2 Sample (4)
Constant	−0.48	−0.46	−0.14	−0.24
	(0.04)	(0.05)	(0.08)	(0.06)
Teens	−0.64	−0.67	−0.87	−0.78
	(0.01)	(0.04)	(0.02)	(0.03)
Teens × Year(s)[b]				
Post–1	−5.74	−8.50	−10.15	−2.61
	(3.48)	(3.85)	(5.92)	(4.96)
Post–2	−12.28	−16.47	−12.01	−9.19
	(3.69)	(5.11)	(9.62)	(8.15)
Post–3+	−11.52	−18.19	−24.23	−16.87
	(2.55)	(6.30)	(7.10)	(8.48)
β	2.01	1.70	1.87	1.70
	(0.28)	(0.53)	(0.41)	(0.53)
N	726	726	504	504
R^2	0.948	0.949	0.954	0.965

Notes: [a]Each sample pools state-year averages of the treatment (i.e., teens) and control (i.e., prime-age high-school graduates) groups from states with state minimum wages that never bind over the analysis period. Each regression includes year and state fixed effects, as well as a linear differential trend for teens. The dependent variable is the natural log of the employment rate of the group in state s in minimum-wage year t. A mean-preserving spread—using the first-step estimate of β—transforms the employment rate of the control group. Standard errors are bootstrapped with 1000 repetitions and clustering by state. [b]Each teens×post variable interacts the teens dummy variable with a dummy variable indicating the first, second, or later years after the first minimum wage hike. The teens×post variables' coefficients and standard errors are multiplied by 100.

the one-sample estimates in Table 8, and −16.9 percent in the two-sample estimates. One difference here is that statistically significant disemployment effects of the latest round of minimum wage hikes do not appear until 2009, and that effect is only marginally significant.

These estimates reinforce the principal finding that the 1990–1991 and 2007–2009 rounds of minimum wage hikes reduced the employment of teens. Estimates of the disemployment effects of the minimum wages from the scaled-control DD method are similar to the event-method estimates, so omitted year effects are not the source of the large disemployment effects in the event-method regressions in Tables 4 and 6.

6. Extensions

In this section, I extend the analysis in four directions. First up is the 1996–1997 round of federal minimum wage hikes, which occurred in a period of rapid growth. Second, I apply the event method to high school dropouts. Third, I repeat the 1990–1991 and 2007–2009 event studies on quarterly data to expose the speed of adjustment. Fourth, I translate the estimates to elasticities of demand for teens and dropouts. These estimates of labor demand elasticities are what I need to determine whether increasing the minimum wage increases or decreases total earnings in these low-wage groups.[5]

1996–1997 Minimum Wage Hikes and Welfare Reform

In August 1996, President Clinton signed the 1996 amendments to the Fair Labor Standards Act, which increased the minimum wage to $4.75 on October 1, 1996 and to $5.15 on September 1, 1997. The amendments also established a $4.25 sub-minimum wage for teens during their first 90 days on a job.

These two minimum wage hikes went into effect in a period of rapid growth. In the labor market, the employment rate grew from 62.7 percent in January 1996 to 64.7 percent in April 2000. The unemployment rate trended down from 5.6 percent early in 1996 to 3.9 percent late in 2000.

There is not much time between the April–1991 and October–1996 events to identify the relationship between the treatment and control groups. But there is plenty of time after the October–1997 hike to estimate the relationship. The analysis period begins in October 1993, 30 months after the April–1991 hike, and ends in October 2006, nine years after the September–1997 hike. Each minimum-wage year begins in October of that year and ends in September of next year.[6] (This means that most of minimum-wage year 1997, for example, is calendar-year 1998.) The analysis focuses on the 37 states that did not have super-federal state minimum wages in any month during the 12 minimum-wage years.

The employment rate of teens rose from 37.8 percent in the 12 months before the October–1996 minimum wage hike for four years to 40.0 percent in 1999. See Figure 6. Of course, meaningful disemployment effects could be hiding in the expanding labor market. Perhaps the teen employment rate would have grown to 41 or 42 percent without the minimum wage hikes.

Two counterfactual series in Figure 6 address that possibility. On the 12 aggregate data points, the estimated regression equation is

$$\widehat{e_t^T} = -\underset{(16.9)}{113.7} + \underset{(0.23)}{2.11}\, e_t^C + \underset{(0.14)}{0.61}\, t + \underset{(0.62)}{0.94}\, D_t^{96} + \underset{(0.77)}{2.24}\, D_t^{97,98} + \underset{(1.02)}{3.15}\, D_t^{99-} \tag{6}$$

with $R^2 = .988$. These estimates generate the "Control with Trend" series in the figure, which also displays a counterfactual series from a regression that excludes the trend term. Although the "Control w/o Trend" counterfactual might hint of a negative effect of the minimum wage on teen employment, the counterfactual with a trend points to this round of minimum wage hikes *increasing* the teen employment rate by about 3 percentage points.[7]

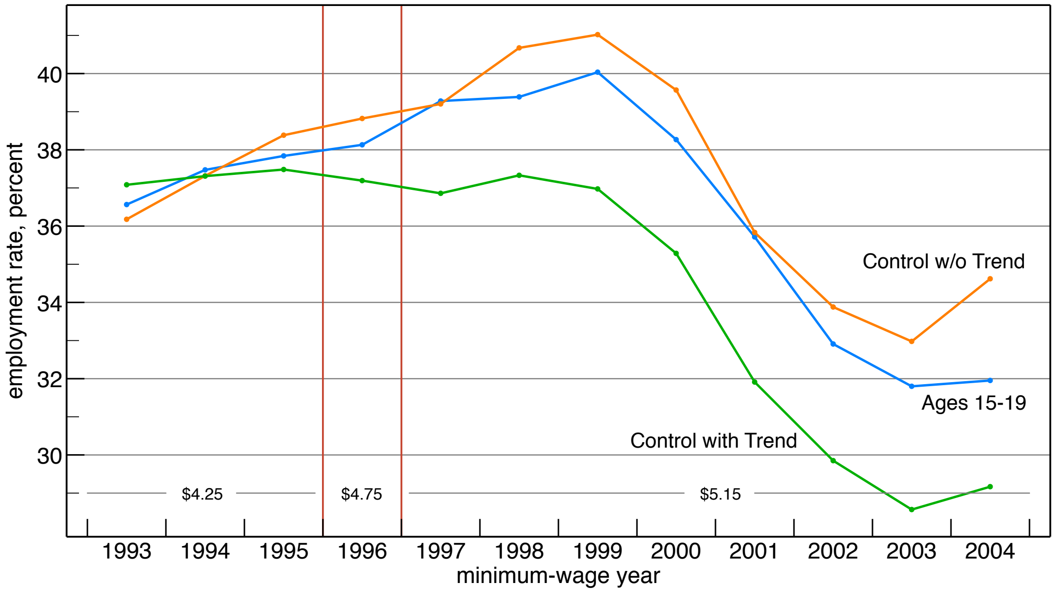
A second major legislative event also dominated this period. On August 22, 1996, President Clinton signed into law the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, and the law took effect on July 1, 1997. Welfare reform replaced Aid to Families with Dependent Children (AFDC) with TANF, Temporary Assistance for

[5] In another extension, I exclude people with only a high school education from the control group. Estimated disemployment effects tend to be a bit larger in specifications with the more-educated control group.

[6] There is one exception. Since the first minimum wage hike was October 1, 1996, I assign September 1996 to minimum-wage year 1995, which has 13 months. This leaves 11 months (i.e., October 1996–August 1997) for minimum-wage year 1997.

[7] Column 5 of Appendix Table A1 presents estimates on state-year averages with a cubic aggregate trend.

Figure 6: Event Plot, 1993–2004



Notes: Equation 6 (excluding the effects of the minimum-wage indicators) generates the "Control with Trend" series. Fitted values from $\widehat{e_t^T} = -\underset{(22.8)}{168.7} + \underset{(0.32)}{2.85}\, e_t^C - \underset{(0.97)}{0.69}\, D_t^{96} - \underset{(0.81)}{0.60}\, D_t^{97,98} - \underset{(0.60)}{1.20}\, D_t^{99-} \left(R^2 = .948\right)$ form the "Control w/o Trend" series.

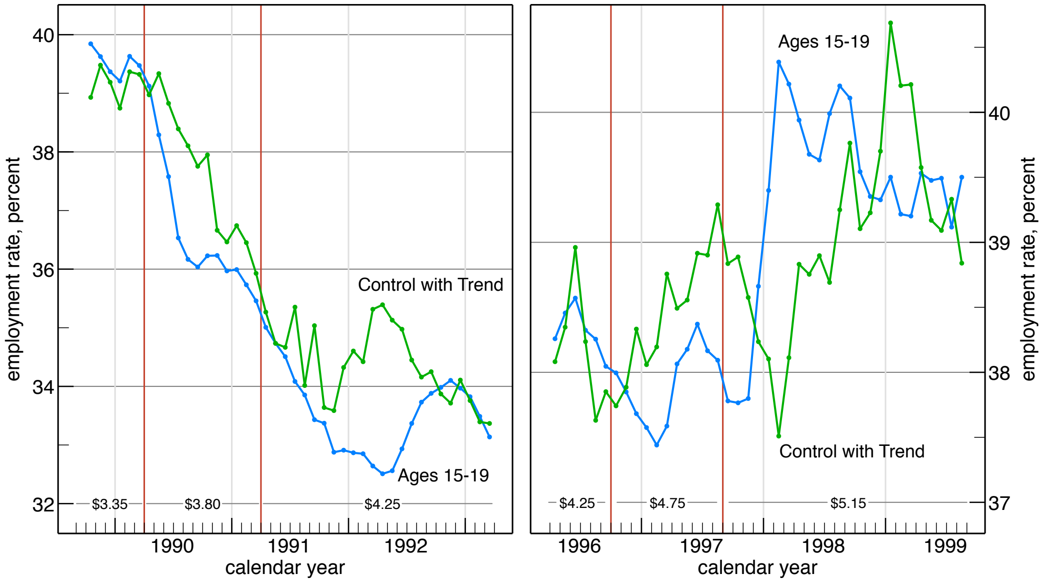
Needy Families, in the form of block grants to states. Key features of the reform include work requirements (or job search), a lifetime limit of five years for benefits from federal funds, limits on food stamps, and federal funding of child care subsidies and federal income tax credits for child care in low-income families (Blank 2002).

Welfare reform was a major event with important consequences for the labor market, including the employment of teens. Indeed, one purpose of welfare reform was to break the cycle of poverty by dealing with unmarried teen moms. Many of the program's features encouraged work. To the extent welfare reform hit teens more than older educated people, the event plot in Figure 6 mixes the effects of the minimum wage hikes with the effects of welfare reform.

The first minimum wage hike, however, preceded welfare reform by nine months. Monthly analysis of this nine-month might identify the short-term employment effects of the minimum wage hikes before welfare reform complicates matters.

Figure 7 displays event plots on seasonally-adjusted monthly data from the 1990–1991 round in the left panel and the 1996–1997 round in the right panel. The plots begin six months before the first minimum-wage hike and end 24 months after the second hike. Each plot depicts the counterfactual (with a trend), as well as the teen employment rate. In the right panel, teen employment quickly drops below the counterfactual series in the months after the October–1996 increase in the minimum wage. Indeed, the teen employment rate lies below the counterfactual series for 12 consecutive months from December 1996 through November 1997. A dramatic reversal, however, begins in December 1997 when the teen employment rate jumps up. The teen employment rate remained well above the counterfactual series for the next 12 months. If welfare reform caused the teen employment rate to

Figure 7: Minimum Wage Hikes and Welfare Reform: Seasonally-Adjusted Monthly Data



Notes: The X11 method seasonally adjusts each series.

jump late in 1997 (several months after the law took effect), then the experience over the first year of the higher minimum wage points to a small (about 1 percentage point) disemployment effect. For comparison, the figure's left panel illustrates how the teen employment rate fell below the counterfactual for almost 2.5 years after the increase in the minimum wage on April 1, 1990.

The event method clearly does not apply cleanly to the minimum wage hikes in 1996 and 1997. Ignoring confounding influences from welfare reform delivers significant positive estimates of the effect of the higher minimum wages on teen employment. But monthly data on the period between the first minimum wage hike and when welfare reform took effect suggest that increasing the minimum wage to $4.75 on October 1, 1996 reduced teen employment a small amount.

High School Dropouts

The 1990–1991 and 2007–2009 rounds of minimum wage hikes also depressed employment of a second group of low-wage workers: high school dropouts. My sample of high school dropouts are people ages 25–54 who have not completed grade 12. Employment of dropouts, unlike teens, is not highly sensitive to fluctuations in economic activity.

Although the wages of most high school dropouts are well above the minimum wage, some dropouts reported wages between the old and new minimum wages in each round of minimum wage hikes: 17.8 percent of high school dropouts earned between the old and new minimum wages ($3.35 and $4.25) in the 12 months before the federal government lifted the minimum wage to $3.80 on April 1, 1990. (See Table 7.) In the year before the July–2007 hike to $5.85, 18.5 percent of the high school dropouts worked for wages between the old minimum wage ($5.15) and the $7.25 minimum wage that took effect in July 2009. Both numbers are substantially lower than the comparable percentages for teens in Table 7, but they are also much higher than the percentages for the control group of prime-age high-

Table 9: Employment-Rate Regressions on State-Year Averages, High School Dropouts[a]

	1990–1991 Round				*2007–2009 Round*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	−0.40	−0.53	−0.49	−0.42	−0.33	−0.32	−0.45	−0.27
	(0.09)	(0.11)	(0.15)	(0.01)	(0.09)	(0.10)	(0.08)	(0.003)
ln(Employment Rate, Ages 25–59, HS+)	0.94	0.65	0.70		0.93	0.92	0.40	
	(0.25)	(0.29)	(0.38)		(0.31)	(0.31)	(0.26)	
Dropouts				−0.29				−0.25
				(0.02)				(0.04)
Year Effects $\times 100$[b]								
1990	−0.60	−2.40	−2.32	−2.02				
	(1.15)	(1.38)	(1.47)	(1.37)				
1991	−3.20	−6.02	−5.85	−4.90				
	(1.54)	(2.06)	(2.42)	(1.87)				
1992–1995	−6.00	−10.43	−10.28	−8.75				
	(1.36)	(2.27)	(2.63)	(1.80)				
2007					−2.44	−1.82	−1.24	−2.32
					(0.87)	(0.83)	(1.12)	(0.97)
2008					−5.77	−5.00	−6.38	−5.53
					(2.29)	(2.66)	(2.55)	(1.99)
2009–2013					−6.66	−5.43	−8.27	−6.12
					(2.68)	(3.46)	(2.95)	(1.93)
Year		0.007				−0.002		
		(0.003)				(0.001)		
Year Effects				✓				✓
State Trends			✓				✓	
N	363	363	363	726	252	252	252	504
R^2	0.890	0.894	0.927	0.904	0.861	0.862	0.886	0.871

Notes: [a]The dependent variable is the natural log of the employment rate of high school dropouts, ages 25–54. The dropout population weights the least squares estimates on state-year data from the basic monthly CPS files, April 1995–March 1996 and August 2000–July 2014. Minimum-wage years begin in April (August) of the indicated calendar year and end in March (July) of the next year in the 1990–1991 (2007–2009) round. Each regression includes state fixed effects, and column (7) includes a quadratic aggregate trend. Cluster-robust standard errors are in parentheses. [b]Estimates in columns (4) and (7) are the coefficients on the interaction terms—the dropout dummy variable interacted with each year (or years) dummy variable.

school graduates.

Table 9 reports estimates of dropout employment-rate regressions (in logs) on state-year averages. Regression estimates for the 1990–1991 round of hikes (in columns 1–3) identify a positive trend in the employment of dropouts (relative to the control group) and are entirely robust to the presence of state-specific trends. These patterns echo the findings for teens. But for dropouts, statistically significant employment effects do not appear until minimum-wage year 1991. In the regression with a linear aggregate trend (column 2), the disemployment effects grow from 2.4 percent in 1990, which is not statistically significant, to 6.0 percent in 1991 and to 10.4 percent after that.

Estimates of the dropout employment-rate regressions from the 2007–2009 round of minimum wage hikes are sensitive to the treatment of trends, and how we specify the trends matters. The estimates are in columns 4–6 of Table 9. Without any trend, the employment effects of the minimum wages are negative and statistically significant in minimum-wage years 2007, 2008, and thereafter. Those estimates are mostly robust to including state-specific trends, which are jointly significant. In the regression with state trends (column 7), significant disemployment effects appear beginning in minimum-wage year 2008. (This pattern resembles the estimates for the 1990–1991 round.) The eventual effect of the 2009 minimum wage on employment of high school dropouts is -8.3 percent. With a linear aggregate trend, however, the effect falls to a statistically insignificant -5.4 percent, but the trend is not statistically significant either.

The estimated effects of the minimum wage hikes on the employment rate of high school dropouts are robust to the treatment of year effects. Columns (4) and (8) of the table present canonical DD estimates, which separately identify minimum wage effects and year effects on employment of dropouts. (Since the comovements of dropout and prime-age high-school educated employment rates are close to one for one, I present simple DD estimates (i.e., $\beta = 1$) rather than two-step scaled-control estimates.) In column (4), the effects of the 1990 and 1991 minimum wage hikes on the employment rate of dropouts match the estimates from the event regressions in columns (2) and (3). And for the 2007–2009 round in column (8), the estimates closely resemble the event estimates in columns (6) and (7). Allowing for year effects in this way shrinks the estimated effects of the minimum wage slightly, but the estimates confirm an important pattern for dropouts: significant disemployment effects emerge only after the second minimum wage hike in each round.

The estimated disemployment effects of minimum wages for high school dropouts are also about half the size of the disemployment effects in the teen employment-rate regressions. This pattern should not be surprising since most high school dropouts earn well above the minimum wage.

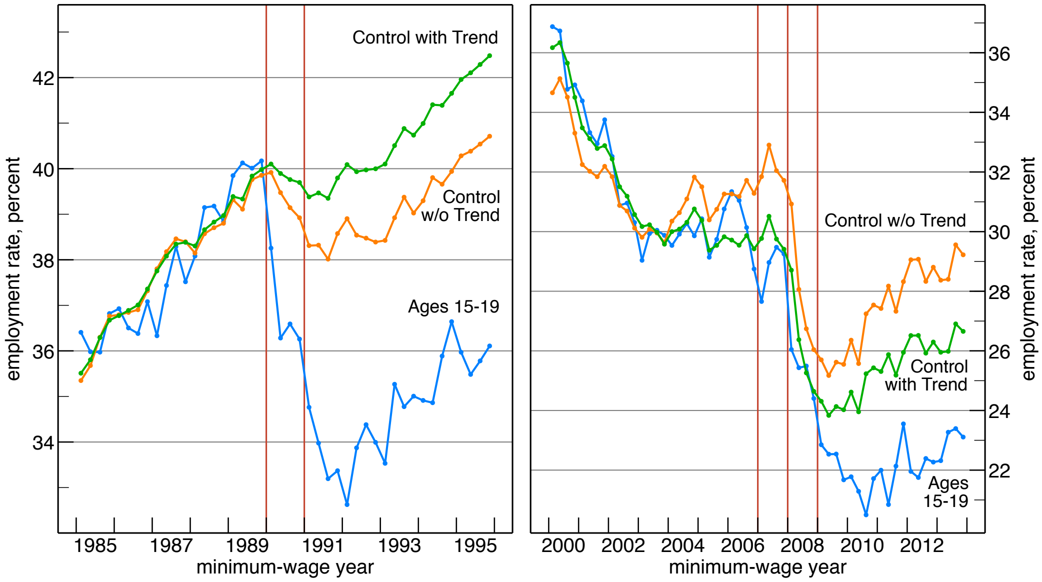
Quarterly Data

Analysis at the quarterly frequency reinforces the annual analysis and exposes the speed of the employment response. Figure 8 displays the event plots using seasonally adjusted quarterly series.

In the left panel, the teen employment rate fell nearly 2 percentage points relative to the counterfactual series in the first quarter with the \$3.80 minimum wage (i.e., April through June). The relative drop in the next quarter was almost another 2 percentage points. And the teen employment rate fell (relative to the counterfactual series) in four of the first five quarters following the new \$4.25 minimum wage. In this case, it took several quarters for the full disemployment effect to emerge.

The right panel of Figure 8 plots the teen employment response to the 2007–2009 events in quarterly data. In each of the first six quarters following the July–2007 minimum wage hike, the teen employment rate hovered below the counterfactual series. But the difference was meaningful in only three of those quarters. Indeed, persistent minimum-wage-driven job loss of teens did not emerge until minimum-wage year 2009. For this round of hikes, the quarterly evidence simply confirms the patterns already apparent in the annual data in Figure 5.

Figure 8: Event Plots, Quarterly, 1985–1995 and 2000–2013



Notes: The X11 method seasonally adjusts each series.

Regression estimates on state-year-quarter averages highlight the transition period. The regressions in columns 6 and 7 of Appendix Table A1 include quarter effects, state effects, and state-specific trends. The regressions also include a dummy variable that identifies the first quarter of the first minimum wage hike (e.g., April, May, and June of 1990) and another dummy variable that identifies the first quarter of the second hike (e.g., April, May, and June of 1991).

In the 1990–1991 round of minimum wage hikes, the transition quarters are positive and statistically significant, and the disemployment effects are substantially larger than the estimates that ignore the transition (e.g., column 4 of Table 4). In particular, the point estimates imply a disemployment effect of −5.4 percent in quarter 1 of minimum-wage year 1990, −11.3 in quarters 3–4 of minimum-wage year 1990, −15.7 percent in quarter 1 of minimum-wage year 1991, −21.0 in quarters 3–4 of minimum-wage year 1991, and −22.2 percent thereafter.

These quarterly estimates from the 1990–1991 round suggest coherent dynamics; estimates from the 2007–2009 do not. The estimates in column 6 of Appendix Table A1 indicate that the only disemployment effect in minimum-wage year 2007 was in the first quarter (i.e., August, September, and October of 2007). Increasing the minimum wage to $5.85 in July 2007 had no discernible effect on teen employment for the rest of minimum-wage year 2007. For the hike to $6.55 in July 2008, the estimated first-quarter effect is not statistically significant, so the evidence points to a common disemployment effect of 9.5 percent that applied throughout minimum-wage year 2008.

Labor Demand Elasticities

My estimates are larger (in absolute value) than most estimates of disemployment effects of minimum wages in the literature (see Neumark and Wascher 2008, chapter 3). My preferred estimates with a linear aggregate trend for 1990–1991 and a cubic aggregate trend for 2007–

2009 are 17.5 and 26.1 percent reductions in the employment rates of teens, respectively. These translate into elasticities of −.65 and −.64, so a 10 percent increase in the federal minimum wage cuts teen employment by 6.5 (or 6.4) percent in these data.[8] The point estimates for high school dropouts imply that a 10 percent increase in the minimum wage cuts the employment of dropouts by 3.9 percent based on the estimate from the 1990–1991 round and by 2.0 percent based on the 2007–2009 estimate.

These elasticities are not estimates of the elasticities of demand for teenage and dropout labor (Deere, Murphy, and Welch 1996; Neumark and Wascher 2008, p. 83–86). For instance, although the minimum wage rose 27 percent in the 1990–1991 hikes, most teens earned above the new $4.25 minimum wage before the first hike in 1990; that is, a minority of teens were paid a wage in the treatment zone between the old minimum and the new minimum. Furthermore, many of the teens treated with the new minimum wage were paid more than the old minimum wage before the increase; that is, their wages increases were smaller than the bump in the minimum wage.

What elasticity of demand for a low-wage group directly affected by the minimum wage would generate the estimated disemployment effect for that group (e.g., 17.5 percent for teens in the 1990-1991 round)? To answer this question for teens, divide the disemployment effects by the average percentage change in teen wages—that is, the percentage increase in the cost of teen labor. For workers who earned the pre-hike minimum wage, the percentage change in the wage is simply 27 percent in the 1990–1991 round and 41 percent in the 2007–2009 round. For teens who earned at least the new minimum wage before the increase, the percentage change is zero. And for each teen with a pre-hike wage between the old and new minimum wages, we compute the percentage increase up to the new minimum wage. Averaging these three factors produces the average percentage increase in the wages of teens, which is the percentage increase in the cost of teen labor (assuming no change in employment). Table 7's calculations from the CPS's distributions of teen wages in minimum-wage years 1989 and 2006 reveal that the average percentage changes in teen wages were much smaller than the 27 and 41 percent minimum-wage hikes.

Table 10 compares statutory and cost-based employment elasticities for teens and high school dropouts in the 1990–1991 and 2007–2009 rounds of minimum wage hikes.

Teens. For the 1990–1991 event, the average percentage bump in the teen wage was 9.1 percent—16.8 percent for workers in the treatment zone and 0 for workers with wages greater than $4.25. For the 2007–2009 event, raising the minimum wage to $7.25 would bump the average teen wage 9.6 percent—18.2 percent for teens treated with the $7.25 minimum wage and 0 for higher-wage teens. Although the 2007–2009 round had a much larger increase in the minimum wage, it had essentially the same effect on the average increase in teen wages because few teens worked for the minimum wage in 2006.

Dividing the two estimates of the disemployment effect (i.e., 17.5 percent and 26.1 percent) by these average percentage increases in teen wages delivers the following demand elasticities associated with teens: −1.9 for 1990–1991 and −2.7 for 2007–2009. Given the increases in the minimum wages and the pre-hike distributions of teen wages, these two labor demand elasticities would generate the estimated job losses of teens in these two rounds of

[8]The 95-percent confidence intervals are [−.88, −.42] for 1990–1991 and [−.86, −.42] for 2007–2009.

Table 10: Employment Elasticities

	Teens	Dropouts
1990–1991		
Minimum Wage Effect (%)	−17.5	−10.4
Statutory Increase in the Minimum Wage (%)	26.9	26.9
Employment Elasticity, Statutory	−0.65	−0.39
Average Increase in the Wage (%)	9.1	2.7
Employment Elasticity, Cost-Based	−1.9	−3.9
2007–2009		
Minimum Wage Effect (%)	−26.1	−8.3
Statutory Increase in the Minimum Wage (%)	40.8	40.8
Employment Elasticity, Statutory	−0.64	−0.20
Average Increase in the Wage (%)	9.6	2.6
Employment Elasticity, Cost-Based	−2.7	−3.2

Notes: Minimum wage effects are from Tables 4, 6, and 9. Each statutory employment elasticity is the minimum wage effect divided by the percentage increase in the minimum wage. From Table 7, the average increases in the wage measure in percentage terms the distance from below to reach the $4.25 or $7.25 new minimum wage; wage increases of workers with wages above the new minimum wage are set to zero. Data to compute distances to the new minimum wages are from the Current Population Survey's outgoing rotation group files, April 1989–March 1990 and August 2006–July 2007, for states with state minimum wages that do not bind during the analysis period. Each cost-based employment elasticity is the minimum wage effect divided by the average increase in the wage.

minimum wage hikes.

High School Dropouts. Table 10 also compares employment elasticities for high school dropouts. To reach the new minimum wages, the average percentage bumps in dropout wages were tiny compared to the 27 and 41 percent minimum-wage hikes. For the 1990–1991 event, the average percentage bump in dropout wages was 2.7 percent (14.7 percent for workers in the treatment zone and 0 for dropouts with wages greater than $4.25). For the 2007–2009 event, the average distance (from below) to the $7.25 minimum was 2.6 percent for dropouts (13.8 percent for dropouts treated with the $7.25 minimum wage and 0 for higher-wage dropouts). Although the 2007–2009 round had a much larger increase in the minimum wage, it had the same effect on the average increase in dropout wages because very few high school dropouts worked for the minimum wage in 2006.

Dividing the two estimates of the disemployment effect for high school dropouts (i.e., 10.4 percent and 8.3 percent) by these average percentage increases in dropout wages produces the following cost-based elasticities of demand for dropouts: −3.9 for 1990–1991 and −3.2 for 2007–2009. [9] These two labor demand elasticities generate the estimated job losses of dropouts in these two rounds of minimum wage hikes, given the increases in the minimum wages and the pre-hike distributions of dropout wages.

[9] Although my estimates are larger than most estimates of the disemployment effects, these labor demand elasticities are in line with estimates of labor demand elasticities for low-skill workers (Hamermesh 1993, chapter 3, section IV). From his survey of the evidence, Hamermesh concludes own-wage demand elasticities decline with skill; that is, labor demands are more elastic for younger workers, less-educated workers, and blue collar workers. Some of the estimates for young (Table 3.9), less-educated (Table 3.8), and blue collar (Table 3.7) workers are quite large. Furthermore, the elasticities that Hamermesh reports exclude scale effects, so they likely understate the relevant elasticities of demand for low-skill workers.

How does the federal minimum wage affect the total earnings of teens and high school dropouts? Since these estimates of the labor demand elasticities are much greater than one in absolute value, I predict that increasing the federal minimum wage cut the total earnings of teens and dropouts dramatically.[10]

7. Summary and Conclusion

Using state-year panel data from the Current Population Survey, I estimate the effects of federal minimum wage hikes on employment of teens and high school dropouts. The estimates point to economically large and statistically significant employment losses. Indeed, teen and dropout job losses in the 1990–1991 and 2007–2009 recessions were much larger than we would predict on the basis of variation in the employment rates of prime-age high-school graduates. The evidence is robust to the treatment of trends, including nonlinear and state-specific trends, and year effects.

For teens in both the 1990–1991 and 2007–2009 rounds, I estimate that a 10 percent increase in the minimum wage would eventually reduce the employment rate by between 4.2 percent and 8.7 percent. That this range is insensitive to the period of analysis hides a more-elastic employment response to the latest hike in the minimum wage. The disemployment effects are also much larger for younger teens.

Although the 2007–2009 minimum wage hikes were much larger than the 1990–1991 hikes, the latest round of minimum wage hikes bumped teen wages up essentially the same amount as the 1990–1991 round. But by the time the 2009 minimum wage took full effect, teen job loss was much bigger. So the demand for teen labor must have been more elastic in the latest round. Indeed, my estimates of the elasticities of demand for teen labor are -1.9 in the 1990–1991 round and -2.7 in the 2007–2009 round.

I also estimate that a 10 percent increase in the minimum wage would eventually reduce the employment rate of high school dropouts by 2.8 percent (based on the 1990–1991 estimate) or by 1.5 percent (based on the 2007–2009 estimate). These are smaller than the effects for teens because far fewer dropouts had wages in the treatment zone before the rise in the minimum wage. In fact, the implied labor demand elasticities are larger for dropouts.

A distinctive feature of the latest round of federal minimum wage hikes is that few teens had a wage between the old minimum wage and the 2007 minimum wage, which explains why meaningful disemployment effects did not emerge until 2008 or even 2009.

Employment rates of teens and dropouts dropped dramatically in the 1990–1991 and the 2007–2009 recessions. Evidence from the aggregate patterns suggests that minimum wage hikes caused much of the teen and dropout job loss in these recessions. Regression estimates from the state-year panels place even more blame on the federal minimum wage.

[10]Using my elasticities to predict the effect on teen earnings (1) ignores the effects of the minimum wage on the workweek and (2) assumes that job loss is random among the treatment group. Recognizing that the lowest wage workers in the treatment zone would suffer most of the job loss, Deere, Murphy, and Welch (1996, pp. 34–35) argue that elasticities like mine understate the lost earnings of teens.

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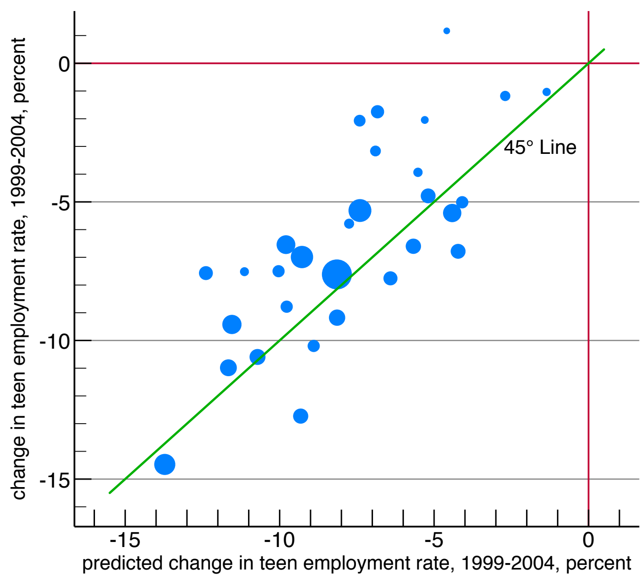
Appendix

Figure A1: Changes in Actual and Fitted Teen Employment Rates by State, 1999 to 2004



Notes: For the 31 states with state minimum wages that do not bind over the 8 year period, the size of a state's dot reflects its teen population. Fitted values are from a regression on state-year averages with state fixed effects and nonlinear trends. See column 1 of Appendix Table A1.

Table A1: Supplementary Teen Employment-Rate Regressions on State-Year Averages[a]

			With 7 Scooping States			*Quarterly Data*	
	1999–2006 (1)	2002–2013 (2)	2000–2013 (3)	2000–2013 (4)	1993–2004 (5)	1985–1995 (6)	2000–2013 (7)
Constant	−0.70	−0.63	−0.10	−0.29	−0.13	−0.77	−0.51
	(0.17)	(0.16)	(0.14)	(0.16)	(0.13)	(0.13)	(0.11)
ln(Employment Rate), Ages 25–59, HS+	1.70	1.70	2.01	1.26	2.19	1.22	1.10
	(0.62)	(0.52)	(0.46)	(0.52)	(0.38)	(0.31)	(0.29)
Year Effects $\times 100$							
1990						−10.68	
						(1.75)	
1991						−19.38	
						(3.25)	
1992–1995						−20.10	
						(3.27)	
1996					0.87		
					(1.50)		
1997					4.45		
					(1.98)		
1998–2004					9.00		
					(2.70)		
2007		−5.93	−4.79	−5.11			−2.90
		(2.43)	(2.14)	(2.13)			(2.18)
2008		−14.20	−9.99	−13.26			−15.84
		(3.37)	(3.18)	(3.36)			(3.38)
2009–2013		−26.75	−23.19	−28.60			−32.08
		(3.00)	(4.09)	(3.96)			(3.01)
Quarter-Year Effects $\times 100$							
Quarter $1 \times$ Post 1						5.32	−8.33
						(2.01)	(1.72)
Quarter $1 \times$ Post 2						4.96	−1.58
						(2.27)	(3.38)
Trend[b]	quadratic	none	cubic	cubic	cubic	linear	cubic
State Trends[c]	8.63			4.32			
	[0.000]			[0.000]			
N	248	216	350	350	444	1,452	1,008
R^2	0.932	0.930	0.927	0.942	0.925	0.817	0.833

Notes: [a]The dependent variable is the natural log of the employment rate of teens, ages 15–19. The sample includes states with state minimum wages that never bind over the indicated sample period. Teenage population weights the least squares estimates on state-year averages from the basic monthly CPS files. In column headers 2–7, each year refers to a minimum-wage year; see the text. Each regression includes state fixed effects. The regressions in columns 6 and 7 include quarter effects. Cluster-robust standard errors are in parentheses. [b]The regression with state-specific trends (column 4) includes the squared and cubed aggregate terms but omits the linear term. [c]F-statistic, which is calculated using robust estimates of the variance-covariance matrix, tests of the null hypothesis that all state trends are equal. p-value is in brackets.

Table A2: States Without Binding State Minimum Wages

	Period of Analysis			
State	1985–1995	1993–2004	1999–2006	2000–2013
Maine				
New Hampshire		✓	✓	✓+
Vermont				
Massachusetts				
Rhode Island				
Connecticut				
New York	✓	✓		
New Jersey				
Pennsylvania		✓	✓	✓+
Ohio	✓	✓	✓	
Indiana	✓	✓	✓	✓
Illinois	✓	✓		
Michigan	✓	✓		
Wisconsin		✓		
Minnesota		✓		
Iowa			✓	✓+
Missouri	✓	✓	✓	✓+
North Dakota		✓	✓	✓
South Dakota	✓	✓	✓	✓
Nebraska	✓	✓	✓	✓
Kansas	✓	✓	✓	✓
Delaware	✓			
Maryland	✓	✓		
District of Columbia				
Virginia	✓	✓	✓	✓
West Virginia	✓	✓		✓+
North Carolina	✓	✓	✓	✓+
South Carolina	✓	✓	✓	✓
Georgia	✓	✓	✓	✓
Florida	✓	✓		
Kentucky	✓	✓	✓	✓
Tennessee	✓	✓	✓	✓
Alabama	✓	✓	✓	✓
Mississippi	✓	✓	✓	✓
Arkansas	✓	✓		✓+
Louisiana	✓	✓	✓	✓
Oklahoma	✓	✓	✓	✓
Texas	✓	✓	✓	✓
Montana	✓	✓	✓	
Idaho	✓	✓	✓	✓
Wyoming	✓	✓	✓	✓
Colorado	✓	✓	✓	
New Mexico	✓	✓	✓	
Arizona	✓	✓	✓	
Utah	✓	✓	✓	✓
Nevada	✓	✓		
Washington				
Oregon				
California				
Alaska				
Hawaii				

Notes: A ✓ indicates that the period of analysis includes the specified state. A ✓+ indicates that the specified state is one of the seven states that "scooped" the federal minimum wage hike.