# Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data

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Traditional estimates that often find minimum wage disemployment effects include controls for state unemployment rates and state- and year-fixed effects. Using CPS data on teens for the period 1990–2009, we show that such estimates fail to account for heterogeneous employment patterns that are correlated with selectivity among states with minimum wages. As a result, the estimates are often biased and not robust to the source of identifying variation. Including controls for long-term growth differences among states and for heterogeneous economic shocks renders the employment and hours elasticities indistinguishable from zero and rules out any but very small disemployment effects. Dynamic evidence further shows the nature of bias in traditional estimates, and it also rules out all but very small negative long-run effects. In addition, we do not find evidence that employment effects vary in different parts of the business cycle. We also consider predictable versus unpredictable changes in the minimum wage by looking at the effects of state indexation of the minimum wage.

## Introduction

The employment level of teens has fallen precipitously in the 2000s, coinciding with the growth of state and federal minimum wages. But are the two *causally* related? Previous research on the effects of minimum wage policies on teen employment has produced conflicting findings. One set of results—statistically significant disemployment effects with employment elasticities in the "old consensus" range of −0.1 to −0.3—is associated with studies that focus on teens and that use national-level household data (usually the Current

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Population Survey). These studies include state- and year-fixed effect controls to identify minimum wage effects. Another set of results—employment effects that are close to zero or even positive—are associated with studies that focus on low-wage sectors such as restaurants. These studies typically draw only on local comparisons and use employer-based data to identify minimum wage effects.[1]

The inconsistent findings may arise from differences in the groups being examined and/or differences in the datasets that are used. However, recent studies suggest other possibilities (Dube, Lester, and Reich 2010a,b). Lack of controls for spatial heterogeneity in employment trends generates biases toward negative employment elasticities in national minimum wage studies. Such heterogeneity also generates overstatement of the precision of local studies.

In this paper, we seek to address and resolve the conflicting findings by using CPS data on teens from 1990 to 2009 to examine heterogeneity and selectivity issues. More specifically, we consider whether the source of identifying variation in the minimum wage is coupled with sufficient controls for counterfactual employment growth. With the addition of these controls, we are able to reconcile the different findings in the literature, identify the limitations of the previous studies, and provide improved estimates.

Our central argument concerns the confounding effects of heterogeneous patterns in low-wage employment that are coupled with the selectivity of states that have implemented minimum wage increases. The presence of heterogeneity is suggested by Figure 1 and Table 1, which show that employment rates for teens vary by Census division and differentially so over time. The differences over time are not captured simply by controls for business cycles, school enrollment rates, relative wages of teens, unskilled immigration, or by the timing of federal minimum wage increases.[2]

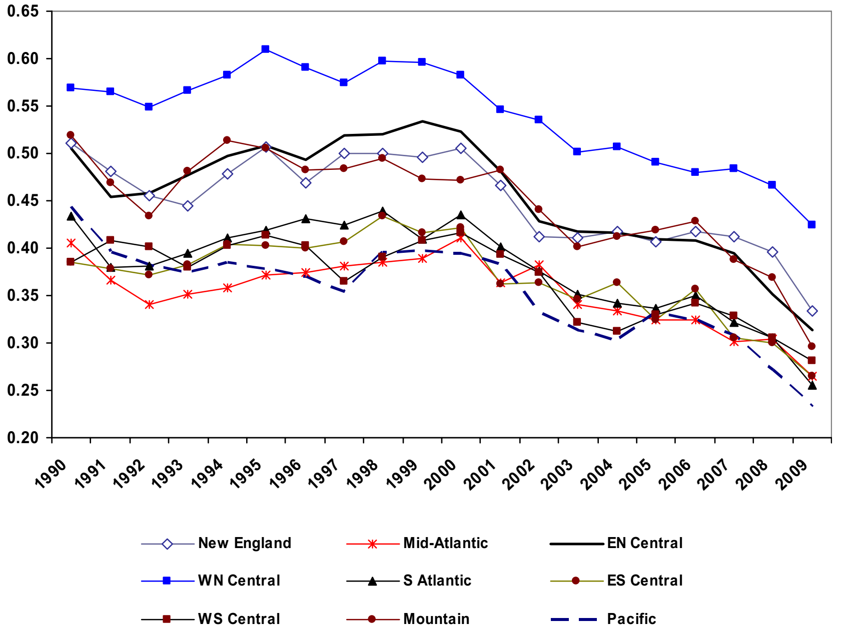
To examine the importance of spatial heterogeneity more systematically, we begin with the canonical specification of minimum wage effects. We estimate the effects on teen earnings, employment, and hours with national CPS panel data and control for state- and year fixed-effect variables. We then add two sets of controls, separately and together: (1) allowing for Census division-specific time effects, which sweeps out the variation across the nine divisions and thereby controls for spatial heterogeneity in regional economic shocks; and (2) including a state-specific linear trend that captures long-run growth differences across states. The inclusion of these geographic controls changes the estimates substantially.

[1] For recent examples of each, see Neumark and Wascher 2007a; and Dube, Naidu, and Reich 2007.

[2] For detailed analyses that arrive at these conclusions, see Aaronson, Park, and Sullivan (2006) and Congressional Budget Office (2004). Smith (2010) examines the role of technological change in increasing adult competition for low-skilled jobs.

FIGURE 1

EMPLOYMENT TO POPULATION RATIO FOR TEENS, 16–19, BY NINE CENSUS DIVISIONS, 1990–2009




NOTE: Authors' analysis of Current Population Survey data. See Table 1 for a listing of states within each Census division.

We find that adding these spatial controls changes the estimated employment elasticity from −0.118 (significant at the 5 percent level) to 0.047 (not significant). Our results highlight the importance of estimates that control for spatial heterogeneity, even at such coarse levels as the nine Census divisions. These findings suggest that previous studies are compromised by insufficient controls for heterogeneity in employment patterns coupled with selectivity of states experiencing minimum wage hikes. We also estimate a distributed lag specification to detect pre-existing trends and estimate long-run versus short-run effects. Without spatial controls, the eight quarters *prior* to the actual policy change are all associated with unusually low (and falling) teenage employment, which provides strong evidence regarding the selectivity of states and the timing of minimum wage increases. But when adequate spatial controls are included, there remains no discernible reduction in employment following the minimum wage increase. Moreover, once spatial heterogeneity is accounted for, long-term effects (of 4 years and longer) are not more negative than contemporaneous ones—in contrast to some findings in the literature.

We also examine minimum wage effects by age, gender, and race/ethnicity. Although minimum wage effects on average wages are greater for younger

TABLE 1

Employment to Population Ratios, Teens 16–19, by Census Division, Selected Years

| | 1990 | 2000 | 2009 | Change 1990–2000 | Change 2000–2009 |
|---|---|---|---|---|---|
| *United States* | 0.45 | 0.45 | 0.28 | 0.00 | −0.17 |
| *New England*<br>ME, NH, VT, MA, RI, CT | 0.51 | 0.51 | 0.33 | −0.01 | −0.17 |
| *Middle Atlantic*<br>NY, NJ, PA | 0.41 | 0.41 | 0.26 | 0.01 | −0.15 |
| *East North Central*<br>OH, IN, IL, MI, WI | 0.51 | 0.52 | 0.31 | 0.02 | −0.21 |
| *West North Central*<br>MN, IA, MO, ND, SD, NE, KS | 0.57 | 0.58 | 0.42 | 0.01 | −0.16 |
| *South Atlantic*<br>DE, MD, DC, VA, WV, NC, SC, GA, FL | 0.43 | 0.43 | 0.26 | 0.00 | −0.18 |
| *East South Central*<br>KY, TN, AL, MS | 0.39 | 0.42 | 0.26 | 0.04 | −0.16 |
| *West South Central*<br>AR, LA, OK, TX | 0.39 | 0.42 | 0.28 | 0.03 | −0.13 |
| *Mountain*<br>MT, ID, WY, CO, NM, AZ, UT, NV | 0.52 | 0.47 | 0.30 | −0.05 | −0.18 |
| *Pacific*<br>WA, OR, CA, AK, HI | 0.44 | 0.39 | 0.23 | −0.05 | −0.16 |

Note: Authors' calculations of Current Population Survey data.

teens (16–17) than for older teens (18–19), we do not detect any disemployment effect for either group. We find little difference in employment effects between male and female teens. For both white and black teens, the minimum wage has strong effects on the average wage, and spatial heterogeneity imparts a downward bias to the employment estimates, particularly so for black teens. In all cases, the employment effects are less negative (or more positive) once spatial controls are included. Including spatial controls renders the estimates for Latinos particularly imprecise and fragile, which is likely a consequence of the concentration of Latinos in a handful of Census divisions, especially in the early part of the sample.

Although the range of elasticities generated by studies in the literature may seem narrow, they contain important implications for the net benefits of a minimum wage policy for low-wage workers. Whether the net benefit is positive or negative for a group depends upon whether the sum of the estimated wage, employment, and hours elasticities is greater than or less than zero. In other words, whether the change in minimum wage increases or decreases the teen wage bill. The estimates from extant national CPS-based studies (Neumark and Wascher 2007b, 2008) often imply negative net benefits for teens; our estimates reverse this conclusion.

This paper also addresses two related topics that concern the timing of minimum wage increases—heterogeneity of minimum wage effects at different phases of the business cycle and the anticipation of minimum wage increases. Do employment effects of minimum wage increases differ between tight and slack labor markets? The recession (officially from December 2007 to June 2009) and the weak economy that continued throughout 2009 and 2010 overlapped with federal minimum wage increases in July 2008 and July 2009. We allow for differential impact of the policy in high versus low (overall) unemployment regimes. The estimated employment effect is not negative in either regime; the estimate is somewhat more positive (but not statistically significant) in periods of higher overall unemployment.

In 2001, Washington was the first state to annually index adjustments to its minimum wage. Since then, indexing has become more widespread. By 2009, ten states employed such adjustments.[3] The presence of such indexation raises the possibility that estimates using more recent U.S. data may be influenced by minimum wage increases that were anticipated. We check for this possibility by considering only non-indexed minimum wage changes. Our wage and employment results are nearly identical to our baseline estimates (although the hour effects are somewhat more negative). However, the small number of states with indexation and their geographic clustering make imprecise our estimates of the differential effects of minimum wage in indexed versus non-indexed states.

## Relation to Existing Literature

We do not attempt to review in detail the voluminous minimum wage and teen employment literature. Brown (1999) and Neumark and Wascher (2007b, 2008) provide such reviews.[4] Neumark and Wascher (2007b, 2008) summarize fifty-three studies published since 1990 that examined minimum wage effects in the U.S. Of these, seven were industry case studies, usually of restaurants; the other forty-six used national panel data, mostly on teens in the CPS, with state-fixed effects or state- and year-fixed effects. According to Neumark and Wascher, almost all of these panel studies found economically modest, but

[3] See Appendix for a summary of minimum wage indexation.

[4] As we indicate below, our interpretation of recent studies differs considerably from that of Neumark and Wascher. See also Wolfson (2010), who focuses on 18 papers that appeared between 2001 and 2010.

statistically significant, negative employment effects, *for teens only*, with elasticities that range from −0.1 to −0.3.[5]

There are reasons to question the value of counting how many of these studies produced negative employment estimates. As Wolfson (2010) finds, many of these studies probably overstate their precision due to use of conventional standard errors (not clustered by state) and may incorrectly reject the hypothesis of no employment effect. More fundamentally, however, as we show in this paper, the reliance on the state- and year-fixed effect models makes the conclusions from these papers questionable.

Two recent papers in this vein are Sabia (2009) and Neumark and Wascher (2007a). Using CPS data for 1979–2004, Sabia's main specification included controls for teen shares in the population and fixed-state effects and also year effects in a second specification (Sabia 2009: Table 4). Sabia found significant disemployment elasticities of −0.092 when year effects were excluded and −0.126 when they were included. Sabia did not, however, allow for heterogeneous trends in the places that increased minimum wages. We show here that the absence of such controls produces misleading inference.

Neumark and Wascher (2007a) used pooled national time-series cross-section CPS data on individuals and include state- and year-fixed effects in their specifications. They estimate a negative employment elasticity of −0.136 among teens, significant at the 10 percent level. As Neumark and Wascher (2007b, 2008) document, numerous studies have used the same data and specification, although many do not include year effects. We shall refer to estimation methods that employ national panels with state- and year-fixed effects as the canonical model.

Orrenius and Zavodny (2008, 2010) consider the effect of minimum wages on teen employment using the canonical model, but with an expanded set of business cycle controls beyond a single state-level unemployment rate. In that sense, this work is similar in spirit to our paper. However, instead of specific business cycle measures, we use proximity and long-term trends to control for unobserved labor market heterogeneity. Although their business cycle controls typically do not make a substantial difference to their estimated minimum wage effects, we show that our controls for spatial heterogeneity do so.

[5] Neumark and Wascher summarize their lengthy review as follows (2007b: 121): "… longer panel studies that incorporate both state and time variation in minimum wages tend, on the whole, to find negative and statistically significant employment effects from minimum wage increases, while the majority of the U.S. studies that found zero or positive effects of the minimum wage on low-skill employment were either short panel data studies or case studies of the effects of a state-specific change in the minimum wage on a particular industry."

As mentioned, minimum wage studies that use local restaurant employment data generally do not find disemployment effects.[6] A recent example is the Dube, Naidu, and Reich (2007) before–after study of the effects of a citywide San Francisco minimum wage introduced in 2004 and phased in for small firms. Similar to most other individual case studies, Dube, Naidu, and Reich were unable to address concerns about lags in disemployment effects or common spatial shocks that may have led to overstatement of the precision of their estimates. These issues were addressed by Dube, Lester, and Reich (2010a), who compared all the contiguous county pairs in the United States that straddle a state border with a policy discontinuity. This study employed county-level administrative data on restaurant employment and effectively generalized the local studies with national data.

Dube, Lester, and Reich (2010a) confirmed that existing national minimum wage studies lacked adequate controls for spatial heterogeneity in employment growth.[7] Without such controls, Dube, Lester, and Reich found significant disemployment effects within the "old consensus" range of −0.1 to −0.3. In their localized analysis, the economic and labor market conditions within the local area are sufficiently homogeneous to control for spatial heterogeneities in employment growth that are correlated with the minimum wage. Once such controls were included, Dube, Lester, and Reich found no significant disemployment effects.

The Dube, Lester, and Reich results leave unanswered the following question: Once we account for spatial heterogeneity, are findings for teen employment similar to analogous industry-based studies? Neumark and Wascher (2007b, 2008) raise this issue explicitly when they asserted that industry-based studies do not provide tests of the disemployment hypothesis of the competitive model.[8] In this paper, we provide evidence on this question by comparing our results using CPS data on teens with the Dube, Lester, and Reich results on restaurants. The CPS dataset is not large enough to consider discontinuities at state borders, but it does allow using coarser controls—Census divisions—to correct for spatial heterogeneity. Dube, Lester, and Reich (2010a) found that such controls produced results that were similar to the discontinuity-based estimates.

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[6] Card and Krueger (2000). An exception is Neumark and Wascher (2000).

[7] In a study of the effect of teen population shares on teen unemployment rates, Foote (2007) found that controlling for heterogeneous spatial trends across states generated results quite different from those using national panel data with state-fixed effects.

[8] In their conclusion, Neumark and Wascher (2007a: 165) state: "…the standard competitive model provides little guidance as to the expected sign of the employment effects of the minimum wage in the narrow industries usually considered in these studies…it is not clear to us that these studies have much to say about the adequacy of the neoclassical model or about the broader implications of changes in either the federal or state minimum wages." Yet, earlier in their paper (Neumark and Wascher 2007a: 39, note 19), they acknowledge that the significance of single-industry case studies can only be determined through evidence.

Several other papers have recently also looked at teen employment and minimum wages. A notable example is Giuliano (2007), who examined the effects of a federal minimum wage shock on employment across establishments of a single retailer in different areas of the United States. Giuliano found that overall employment and the teen share of employment increased where the minimum wage led to a greater increase in the relative wage for teenagers. While this paper offers many valuable insights into the effects of the minimum wage within a single company, it does not tell us about the broader effects on all teens.

Another strand of the literature has focused on lagged effects of the minimum wage on teen employment. Using Canadian data, Baker, Benjamin, and Stanger (1999) argue that effects associated with "high frequency" variation of minimum wages (i.e., short-term effects) on teen employment are small and that longer term effects associated with "low frequency" variation are sizeable. However, their research design does not address whether the larger negative effects associated with "low frequency" variations are driven by spatial heterogeneity across Canadian provinces—something that we find in the U.S. data.

In addition to addressing the issues of heterogeneity and selectivity, this paper expands the literature by addressing the topical issues of business cycle dynamics and indexation. The timing of minimum wage increases is often criticized, especially during recessions and periods of relatively high unemployment. Historically, increases in the minimum wage have not occurred at regular intervals. For example, the Fair Minimum Wage Act of 2007 was passed after a decade of federal inaction. The Act consisted of three consecutive 70¢ annual increases. The three phases, which were implemented in July 2007, July 2008, and July 2009, increased the minimum wage from $5.15 to $7.25 during a time of recession and increasingly higher unemployment.

Minimum wage increases are often implemented with a lag after they have been enacted. As a result, as Reich (2009) shows, they are often enacted when the economy is expanding and unemployment is low. But, by the time of implementation, the economy may be contracting and unemployment increasing, possibly leading to a spurious time series correlation between minimum wages and employment. This issue also raises the question of heterogeneous effects of the minimum wage between booms and downturns, something we address in this paper. We interact the minimum wage with the overall unemployment rate in the state to test whether minimum wage increases affect teen outcomes differentially in high versus low unemployment periods.

In the patchwork of minimum wage laws in the United States, indexation of the minimum wage to a consumer price index represents a small but growing phenomenon. These laws have been implemented only in the past decade. States that index their minimum wages, usually to a regional consumer price index, do so annually on a certain day. Supporters point to several benefits to indexation. First, it keeps real minimum wages constant instead of letting them erode over time during periods of inaction and inflation. Second, incremental and small increases over time can be anticipated by firms, who can then adjust more easily than when larger increases occur after prolonged periods of inaction.[9]

The possibility of anticipation can cause problems for estimating the effects of minimum wage increases. In a frictionless labor market, the only wage that matters is the current one. With hiring frictions and/or adjustment costs, forward-looking entrepreneurs would partly adjust their hiring practices today in anticipation of an increase in the minimum wage tomorrow. In such an environment, the coefficients associated with the contemporaneous or lagged minimum wages may underestimate the true effects, as employment may have adjusted *a priori*.[10]

Unlike in many OECD countries, in the United States most minimum wage adjustments are not automatic. Since ten states have recently implemented indexation, it is possible that recent increases have been more anticipated than earlier ones. To account for the possibility that the recent anticipated increases may be driving results using more current data, we present estimates that (1) exclude states with indexation and (2) differentiate between minimum wage impacts in indexed and non-indexed states. We also use a distributed lag model to detect anticipation effects that would be captured by employment effects associated with leading minimum wage terms.

To summarize, a fundamental issue in the minimum wage literature concerns how estimates from state panel data that are based upon state- and year-fixed effect models compare to estimates from specifications that control for spatial heterogeneity and selectivity. To address this question, we use the CPS dataset of the previous literature and incorporate additional spatial and time controls into the traditional specifications. Furthermore, we explore the timing of minimum wage increases by analyzing minimum wage effects as they relate to business cycle dynamics and indexation.

[9] Critics worry that such indexation may lead to wage-price spirals in a high inflation period—something that seems more relevant for the macro-economy of the 1970s than that of recent decades.

[10] For more on this point, see Pinoli (2008), who uses a surprising political transition in Spain to estimate differentially the effects of an unanticipated change in the policy from regular annual changes. Pinoli also posits that some of the estimated minimum wage effects are small because they represent effects from anticipated increases.

## Data

We construct an individual-level repeated cross-section sample from the CPS Outgoing Rotation Groups for the years 1990–2009. The CPS data are merged with data that capture overall labor market conditions and labor supply variation—monthly state unemployment rates and population shares for the relevant demographic groups. Additionally, each observation is merged with a quarterly minimum wage variable—the federal or state minimum, whichever is higher.

Table 2 provides descriptive statistics for the sample of teens aged 16–19 years. Non-Hispanic whites account for 65 percent of the sample, while blacks and Hispanics each account for nearly 15 percent. Hourly pay (in 2009 dollars) over the sample period averaged \$8.21, although older teens were paid more than younger teens—\$8.70 versus \$7.43. While male teens were paid more than female teens—\$8.58 versus \$7.85, pay differentials by race/ethnicity were considerably smaller.

Over the sample period, 40 percent of all teens aged 16–19 years were employed, with identical percentages for males and females. Among teens aged 16–17 years, 30 percent were employed, compared to 51 percent among teens aged 18–19 years. Among race/ethnic groups, black teens had the lowest employment rates—24 percent, followed by Hispanics—33 percent. Employed teens worked an average of 24.8 hours per week, with variation by age, gender, and race/ethnicity. Teens aged 16–17 years worked 19.1 hours per week, compared with 28.3 hours among teens aged 18–19 years. Males, blacks, and Hispanics worked somewhat more hours than females and white non-Hispanics, respectively. Finally, on average, state minimum wages were \$1.15 above federal minimum wages.

## Estimation Strategy

Our focus is to estimate the effect of minimum wage increases on wages, employment, and hours of work for teenagers. The dependent variables $y$, are respectively: the natural log of hourly earnings; a dichotomous employment measure that takes on the value one if the teen is working; and the natural log of usual hours of work. The baseline fixed-effects specification is then:

$$y_{ist} = \beta \mathrm{MW}_{st} + X_{ist}\Gamma + \lambda \cdot \mathrm{unemp}_{st} + \phi_s + \tau_t + \varepsilon_{ist} \tag{1}$$

where MW refers to the log of the minimum wage; $i$, $s$, and $t$ denote, respectively, individual, state, and time indexes; $X$ is a vector of individual

TABLE 2

DESCRIPTIVE STATISTICS, TEENS 16–19, 1990–2009

| | Mean | Std dev | N |
|---|---|---|---|
| Sample statistics | | | |
| All teens 16–19 | – | – | 447,091 |
| Teens 16–17 | 0.53 | – | 237,007 |
| Teens 18–19 | 0.47 | – | 210,084 |
| Male | 0.51 | – | 227,098 |
| White, non-Hispanic | 0.33 | – | 156,070 |
| Black | 0.07 | – | 27,329 |
| Hispanic | 0.08 | – | 28,762 |
| Female | 0.49 | – | 219,993 |
| White, non-Hispanic | 0.32 | – | 151,659 |
| Black | 0.08 | – | 28,131 |
| Hispanic | 0.07 | – | 26,968 |
| Labor market outcomes | | | |
| Hourly wage (2009$) | | | |
| All Teens | 8.21 | 8.51 | 180,161 |
| Teens 16–17 | 7.43 | 9.18 | 73,177 |
| Teens 18–19 | 8.70 | 8.02 | 106,984 |
| Male | 8.58 | 9.38 | 89,500 |
| Female | 7.85 | 7.51 | 90,661 |
| White, non-Hispanic | 8.20 | 7.73 | 149,054 |
| Black | 8.15 | 14.79 | 13,094 |
| Hispanic | 8.38 | 6.66 | 18,013 |
| Employed | | | |
| All teens | 0.40 | – | 184,796 |
| Teens 16–17 | 0.30 | – | 75,621 |
| Teens 18–19 | 0.51 | – | 109,175 |
| Male | 0.40 | – | 92,581 |
| Female | 0.40 | – | 92,215 |
| White, non-Hispanic | 0.45 | – | 153,178 |
| Black | 0.24 | – | 13,257 |
| Hispanic | 0.33 | – | 18,361 |
| Hours worked per week | | | |
| All teens | 24.77 | 12.08 | 182,730 |
| Teens 16–17 | 19.06 | 9.98 | 74,539 |
| Teens 18–19 | 28.30 | 11.90 | 108,191 |
| Male | 26.35 | 12.61 | 91,161 |
| Female | 23.17 | 11.28 | 91,569 |
| White, non-Hispanic | 24.06 | 12.09 | 151,320 |
| Black | 25.62 | 11.07 | 13,186 |
| Hispanic | 28.88 | 11.83 | 18,224 |
| Policy variables | | | |
| Minimum wage | $6.49 | 0.66 | – |
| Minimum wage (federal binding) | $6.16 | 0.42 | – |
| Minimum wage (state binding) | $7.31 | 0.57 | – |
| Unemployment rate | 5.15 | 1.86 | – |

NOTES: Sample statistics are weighted. Standard deviations reported for continuous variables. Average hourly wage is calculated for workers who reported a wage and were not self-employed or working without pay. Average hours worked is reported for workers with positive and stable usual hours of work. Race groups do not add to total because "other" is not reported. Minimum wages in 2009$.

characteristics; unemp is the quarterly (non-seasonally adjusted) unemployment rate in state $s$ at time $t;\varphi_s$ refers to the state-fixed effect; and $\tau_t$ represents time dummies incremented in quarters.[11] In this canonical specification, including state and time dummies as well as the overall unemployment rate is thought to control sufficiently for local labor market conditions facing teenage workers.

There is, however, growing evidence (Dube, Lester, and Reich 2010a,b) that these variables do not fully capture heterogeneity in underlying employment patterns in low-wage employment. To account for this heterogeneity, our second specification allows time effects to vary by Census divisions. Including division-specific time effects ($\tau_{dt}$) eliminates the between-division variation and hence better controls for spatial heterogeneity in differential employment patterns, including region-specific economic shocks:

$$y_{ist} = \beta \mathrm{MW}_{st} + X_{ist}\Gamma + \lambda \cdot \mathrm{unemp}_{st} + \phi_s + \tau_{dt} + \varepsilon_{ist}. \tag{2}$$

A state-specific linear trend variable provides a second means of controlling for heterogeneity in the underlying (long-term) growth prospects of low-wage employment and other trends in teen employment. Our third specification includes these controls:

$$y_{ist} = \beta \mathrm{MW}_{st} + X_{ist}\Gamma + \lambda \cdot \mathrm{unemp}_{st} + \phi_s + \psi_s \cdot t + \tau_t + \varepsilon_{ist} \tag{3}$$

where $\psi_s$ denotes the time trend for state $s$.

Finally, we add both the division-specific time effect and the state-specific time trend controls for our fourth specification:

$$y_{ist} = \beta \mathrm{MW}_{st} + X_{ist}\Gamma + \lambda \cdot \mathrm{unemp}_{st} + \phi_s + \psi_s \cdot t + \tau_{dt} + \varepsilon_{ist}. \tag{4}$$

The resulting estimates are less likely to be contaminated with unobservable long-term trends and region-specific economic shocks in this final (preferred) specification.

We estimate these four specifications on all teens 16–19 years of age. Wage, employment, and hours effects are also reported for sub-samples disaggregated by younger (16–17) and older teens (18–19), gender, and race/ethnicity (white-not Hispanic, black, and Hispanic) separately. We report standard errors clustered at the state level.

To detect pre-existing trends or anticipation effects, as well as the differences between long-run versus short-run effects, we also use a dynamic model. We estimate specifications 1 and 4 with distributed lags in minimum wage covering

[11] The individual characteristics include two gender categories, four race/ethnicity categories, twelve education categories, and four marital status categories.

a 25-quarter window, starting at eight quarters before the minimum wage change and continuing to sixteen quarters after the change.

$$y_{ist} = \sum_{\gamma=-2}^{4} \beta_{4\gamma} \mathrm{MW}_{s,t+4\gamma} + X_{ist}\Gamma + \lambda \cdot \mathrm{unemp}_{st} + \phi_s + \tau_t + \varepsilon_{ist} \tag{5}$$

$$y_{ist} = \sum_{\gamma=-2}^{4} \beta_{4\gamma} \mathrm{MW}_{s,t+4\gamma} + X_{ist}\Gamma + \lambda \cdot \mathrm{unemp}_{st} + \phi_s + \psi_s \cdot t + \tau_{dt} + \varepsilon_{ist} \tag{6}$$

In both cases, we can estimate the cumulative response (or time path) of the outcome $y$ from a log point increase in the minimum wage by successively summing the coefficients $\beta_{-8}$ to $\beta_{16}$.

## Results

*Wage, Employment, and Hours Effects for All Teens.* We first discuss the estimated wage, employment, and hours effects for all 16–19-year-olds for each of our four specifications. The estimated wage effects establish the presence of a "treatment"—increases in the minimum wage led to increased wages for the teen population, conditional on employment. These results are reported in Table 3. In specification 1, the canonical fixed-effects model, the treatment coefficient is 0.123 for all teens and highly significant. Adding just the division controls (specification 2) increases the magnitude of the treatment coefficient for all teens to 0.161. Adding the state-specific time trends, without division controls (specification 3) further increases the magnitude of the wage elasticity to 0.165. When state- and division-specific time trends are both included to best account for spatial heterogeneity and selectivity—our "preferred" specification 4—the treatment effect for all teens is 0.149 and remains highly significant.

These results indicate that the treatment effects of minimum wages remain significant when controls for heterogeneous spatial trends are included. Moreover, the magnitude of the estimated treatment effect is consistent with CPS earnings for teens. In a separate calculation, we found that 30.7 percent of employed teens aged 16–19 years were paid within 10 percent of the relevant state or federal minimum wage. Since not all of these teens were earning exactly the minimum wage, the estimated treatment elasticity of 0.149 is consistent with the distribution of pay at or near the minimum wage.

Figure 2, Panel A displays time paths of the wage effects of minimum wage increases. The left-hand column displays results for our specification 1, while

TABLE 3

Minimum Wage Effects on Wages, Employment, and Hours Worked, Teens 16–19, 1990–2009

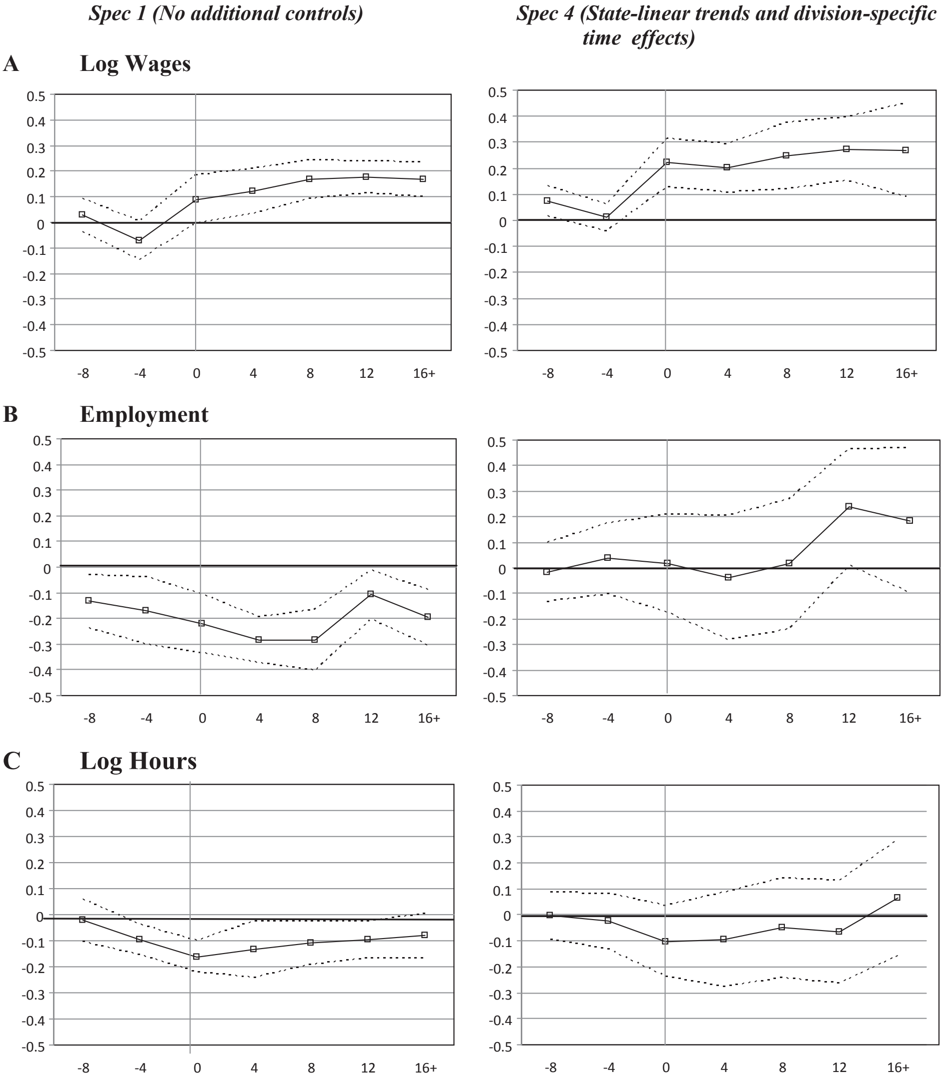
| Specification | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| A. Wage | | | | | |
| All teens | η | 0.123*** | 0.161*** | 0.165*** | 0.149*** |
| | se | (0.026) | (0.030) | (0.025) | (0.024) |
| Teens 16–17 | η | 0.197*** | 0.224*** | 0.221*** | 0.220*** |
| | se | (0.032) | (0.036) | (0.030) | (0.033) |
| Teens 18–19 | η | 0.074** | 0.115*** | 0.120*** | 0.093*** |
| | se | (0.030) | (0.037) | (0.038) | (0.033) |
| B. Employment | | | | | |
| All Teens | coeff | −0.047** | −0.015 | −0.014 | 0.019 |
| | se | (0.022) | (0.034) | (0.027) | (0.024) |
| | η | −0.118** | −0.036 | −0.034 | 0.047 |
| Teens 16–17 | coeff | −0.069** | −0.023 | −0.021 | 0.030 |
| | se | (0.028) | (0.043) | (0.032) | (0.032) |
| | η | −0.232** | −0.077 | −0.071 | 0.101 |
| Teens 18–19 | coeff | −0.027 | −0.005 | −0.010 | 0.009 |
| | se | (0.021) | (0.034) | (0.027) | (0.027) |
| | η | −0.053 | −0.010 | −0.020 | 0.018 |
| C. Hours | | | | | |
| All teens | η | −0.074** | −0.054 | −0.001 | −0.032 |
| | se | (0.035) | (0.048) | (0.040) | (0.042) |
| Teens 16–17 | η | −0.070 | 0.002 | −0.011 | 0.038 |
| | se | (0.042) | (0.074) | (0.044) | (0.073) |
| Teens 18–19 | η | −0.090** | −0.092* | −0.011 | −0.079* |
| | se | (0.042) | (0.049) | (0.050) | (0.042) |
| Division-specific time controls | | | Y | | Y |
| State-specific time trends | | | | Y | Y |

Notes: Results are reported for the coefficients on log minimum wage. η refers to the minimum wage elasticity of the outcome. For employment, the elasticity is calculated by dividing the coefficient by the relevant employment-to-population ratio. Each specification includes individual controls for gender, race (four categories), age (four categories), education (twelve categories), and marital status (four categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Wage regressions include only those who were working and paid between $1 and $100 per hour in 2009 dollars and the log hourly wage is the dependent variable. Hour regressions are restricted to those who had positive hours and the log of hours is the dependent variable. Each regression includes state-fixed effects, time-fixed effects, and additional trend controls as specified. Standard errors clustered at the state level are reported in parentheses. Significance levels are denoted as follows: ***1 percent, **5 percent, *10 percent.

the right-hand column presents results for specification 4, which includes both state-specific time trends and division-specific time effects. Both wage graphs show a clear increase right at the time of the minimum wage increase. However, the preferred specification (4) generates a sharper "treatment," which we interpret as reinforcing the validity of including additional controls.

FIGURE 2

TIME PATHS OF WAGES, EMPLOYMENT, AND HOURS IN RESPONSE TO A MINIMUM WAGE CHANGE




NOTES: Using a distributed lag specification of two leads, four lags, and the contemporaneous log minimum wage, the figures above plot the *cumulative response* of log wage, employment, and log hours to a minimum wage increase. We consider a 25 quarter window around the minimum wage increase. For employment, coefficients are divided by average teen employment-to-population ratio, so the coefficients represent employment elasticities. Specification 1 includes time- and state-fixed effects as well as the set of demographic controls: gender, race (four categories), age (four categories), education (twelve categories), and marital status (four categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Specification 4 additionally includes state-level linear trends and division-specific time effects (hence eliminating the variation among Census divisions). For all specifications, we plot the 90 percent confidence interval around the estimates in dotted lines. The confidence intervals were calculated using robust standard errors clustered at the state level.

We turn next to the employment effects, reported in Table 3, Panel B. Specification 1 shows a significant negative employment coefficient of −0.047 with a corresponding employment elasticity of −0.118, which is consistent with the literature that uses the canonical fixed-effects model.[12] In specification 2, however, allowing for division-specific time effects attenuates the elasticity to −0.036 and renders it insignificant. As specification 3 shows, the addition of a state-specific time trend to the fixed-effects model also lessens the effect of minimum wages on employment. Here, the elasticity is −0.034 and it is not statistically significant. Finally, in specification 4, the employment elasticity is 0.047 and continues to not be significant. In other words, allowing for variation in employment trends over the 1990–2009 period, we obtain minimum wage effects on employment that are indistinguishable from zero. Moreover, a 90 percent confidence interval derived using estimates from specification 4 rules out employment elasticities that are more negative than −0.052.[13]

These results indicate that estimates of minimum wage employment effects using the standard fixed-effects model of specification 1 are contaminated by heterogeneous employment patterns across states. Controlling only for within-division variation substantially reduces the estimated elasticity in magnitude. Allowing for long-term differential state trends makes the employment estimates indistinguishable from zero.[14]

The time paths for employment from our distributed lag specification are reported in Figure 2, Panel B. They provide strong evidence against the canonical model without controls for heterogeneity across states (i.e., specification 1). Specification 1 shows negative employment effects throughout the 25-quarter window, including *prior to* the minimum wage increase. The "response" of employment four quarters *prior* to the minimum wage is −0.17, which is quite similar to the contemporaneous response (−0.22) and the long-term response for sixteenth and later quarters (−0.20). There are two possible interpretations. First, it may be that these increases were anticipated, and owing to adjustment costs, firms reduced employment mostly prior to the actual implementation of the policy. Second, it may be that the measured effects prior to the policy reflect spurious pre-trends due to unobserved heterogeneity: that minimum wage changes have tended to occur at times and places of unusually low teen employment growth.

[12] The elasticity is obtained by dividing the coefficient by the employment-to-population rate of the group in question.

[13] Our 95 percent confidence intervals rule out employment elasticities more negative than −0.07. The 90 percent confidence intervals are reported in Table 8 below.

[14] In "Minimum Wage Effects by Gender, Race, and Ethnicity" we discuss our earnings and employment estimates for gender and race/ethnicity groups.

Consistent with the latter interpretation, specification 4 shows stable coefficients (close to zero) prior to the minimum wage increase, no clear effect on employment in the subsequent eight quarters, and then a small positive employment effect eight quarters after the minimum wage increase. Interestingly, there is no evidence that the long-term employment response (quarter sixteen or later) is any more negative than the contemporaneous one. For our preferred specification 4, the 90 percent confidence interval rules out any long-run employment elasticities more negative than −0.05. This result calls into question the reconciliation offered by Baker, Benjamin, and Stanger (1999) for teen employment and minimum wages—that long-run effects of minimum wage are more negative. Instead, it appears that the employment effects associated with low frequency variation in minimum wages are more negative because of spurious trends.

Overall, results from the dynamic specifications provide further evidence that failure to control for heterogeneity in employment patterns imparts a downward bias in the estimated employment response due to minimum wage changes.

Our evidence does not support disemployment effects associated with minimum wage increases, but there still may be an effect on hours. Firms may not decrease their demand for workers, but they may decrease their demand for the number of hours teens work. Alternatively, teens may have backward-bending supply schedules and may reduce the hours they offer after a minimum wage increase.

Table 3, Panel C provides estimates of the effects of the minimum wage on weekly hours worked. In specification 1, the elasticity on weekly hours is −0.074 and is significant at the 5 percent level. The effect is not as large and turns insignificant in specification 2 and more so in specification 3. In specification 4, the elasticity is −0.032, but it remains insignificant. As the time paths for hours in Figure 2, Panel C indicate, the hours effect with specification 4 becomes indistinguishable from zero within four quarters of the minimum wage increase and becomes positive in sign after twelve quarters.

We can use the evidence on hourly wages, employment, and hours together to calculate the effect on the teen wage bill. The teen wage bill elasticity equals the sum of the three elasticities: average wage, employment, and hours. If the wage bill elasticity is negative, teens as a whole are worse off from the increase in minimum wage. If it is positive, teens as a whole are better off.

In the canonical framework (specification 1), the teen wage bill elasticity is a *negative* −0.069 (= 0.123 − 0.118 − 0.074). This result indicates that an increase in the minimum wage makes teens, as a whole, worse off. In contrast, once we account for spatial heterogeneities using specification 4, we get a *positive* teen wage bill elasticity of 0.164 (= 0.149 + 0.047 − 0.032), approximately the same magnitude as the average wage elasticity. Failure to account for spatial heterogeneity thus contains important welfare implications when evaluating minimum wage changes.

*Younger Teens Versus Older Teens.* Younger teens (16–17 years) and older teens (18–19 years) differ in ways that can illuminate minimum wage effects on employment. On the one hand, younger teens tend to be less skilled and experienced than older teens and other older workers. As a result, minimum wage increases could have a greater impact on this group as employers substitute toward higher skilled groups. On the other hand, barriers to mobility, such as not having a driver's license, are likely to be greater among younger teens. Younger teens are also likely to have higher search costs because they have relatively little search experience. Hence, minimum wage increases may have greater effects on the search efforts of the younger teens, which could lead to relatively beneficial employment effects.

Our results for the younger and older teens are reported in Table 3. The results in Panel A indicate that the effect of minimum wages on earnings remains positive and significant for both age groups, and across all four specifications. The earnings elasticities are also relatively stable across the four specifications. In our preferred specification, hourly earnings increase more than twice as much among younger teens as among older ones. This is expected, since average earnings are lower for the younger teens (see Table 2) and so the minimum wage is more binding for this group.

Turning next to employment effects, Panel B shows that the disemployment effect in specification 1 is concentrated among the younger teens. This finding accords with the Neumark and Wascher claim that minimum wage increases generate the most harm for the least-skilled groups. This result is reversed, however, in specification 4, in which the employment effect becomes slightly positive for both groups. Although the point estimate is somewhat larger for the younger teens, it is not statistically significant for either group. This result is inconsistent with a purely competitive model, as we do not observe substitution toward the older teens. The result is consistent, however, with a search model, in which higher minimum wages induce greater search by both groups, especially so among the younger teens, who have less search experience.

The hour estimates for our preferred specification in Panel C indicate a positive, but not statistically significant effect among younger teens, and a modest negative effect among older teens. These results indicate that minimum wage increases do not result in employer substitution toward older teens and away from younger teens.[15]

[15] For evidence on supply effects by age and on labor market flows, see Dube, Lester, and Reich 2010b.

Our results for the two teen groups confirm the key results for teens as a whole. The canonical model is biased toward finding disemployment effects. Results from our preferred specification indicate that minimum wages increase average earnings without creating disemployment effects.

*Minimum Wage Effects and Phases of the Business Cycle.* The implementation of the two most recent federal minimum wage increases—in July 2008 and July 2009—coincided with a severe recession and increasing rates of unemployment. These two increases were enacted in the Fair Minimum Wage Act of 2007, when the economy was still in expansion. The increases in 2008 and 2009 garnered much concern because they occurred in a deteriorating economic climate.

Some observers maintained that teen unemployment would increase because of the timing of these minimum wage increases. Teen unemployment rates did indeed increase throughout 2008 and 2009. The teen unemployment rate was 16.9 percent at the start of the recession in December 2007 and increased to 20.8 percent in July 2008 and again to 24.5 percent in July 2009. Were these increases in teen unemployment a result of minimum wage increases during an especially severe economic downturn, or simply the result of harsh economic conditions?

More generally, are the disemployment effects of minimum wage for teens more pronounced (or at least present) when the labor market is slack? To the extent the measured employment effects are small for monopsonistic reasons, some firms are labor supply-constrained as opposed to labor demand-constrained. But this is less likely to be the case when the unemployment rate is high and the job vacancy rate is low. There may be other possibilities as well, including a greater consumer demand effect from an increase in minimum wages during a recession.

To test empirically for differences in the employment response in low- versus high-unemployment regimes, we estimate specifications 1–4, but add an interaction term for the log of the minimum wage and the unemployment rate—$\gamma\,(\mathrm{MW}_{st} \times \mathrm{unemp}_{st})$. Keeping in mind that MW is the log of minimum wage, the total effect of a log point increase in the minimum wage is $(\beta + \gamma \times \mathrm{unemp}_{st})$.

Table 4 presents the estimates of the joint effect of minimum wage and the unemployment rate. Results for the minimum wage, unemployment rate, and the interaction of the two are reported for each of the four specifications. Strikingly, in all of the specifications, the interaction terms are close to zero, positive in sign, and are not statistically significant.

We also estimate the joint effect $(\beta + \gamma \times \mathrm{unemp}_{st})$ for two unemployment scenarios—a low unemployment rate of 4 percent and a higher 8 percent unemployment rate. From specification 1, the employment elasticity of the

TABLE 4

MINIMUM WAGE AND UNEMPLOYMENT EFFECTS ON EMPLOYMENT, TEENS 16–19

| Specification | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| Minimum wage | coeff | −0.051 | −0.024 | −0.061 | −0.020 |
| | se | (0.044) | (0.043) | (0.049) | (0.037) |
| | η | −0.128 | −0.061 | −0.152 | −0.051 |
| MW × Unemployment rate | coeff | 0.001 | 0.002 | 0.008 | 0.008 |
| | se | (0.005) | (0.007) | (0.005) | (0.005) |
| | η | 0.002 | 0.005 | 0.020 | 0.020 |
| Unemployment rate | coeff | −0.017* | −0.017 | −0.029*** | −0.027*** |
| | se | (0.009) | (0.011) | (0.009) | (0.009) |
| | η | −0.043 | −0.044 | −0.073 | −0.067 |
| Joint minimum wage effect (4 percent unemployment) | coeff | −0.049* | −0.017 | −0.028 | 0.011 |
| | se | (0.027) | (0.033) | (0.032) | (0.026) |
| | η | −0.121* | −0.043 | −0.071 | 0.028 |
| Joint minimum wage effect (8 percent unemployment) | coeff | −0.046** | −0.010 | 0.004 | 0.043 |
| | se | (0.020) | (0.042) | (0.023) | (0.027) |
| | η | −0.114** | −0.024 | 0.010 | 0.107 |
| Division-specific time controls | | | Y | | Y |
| State-specific time trends | | | | Y | Y |

NOTES: Joint results are reported for the log of the minimum wage and the interaction between the minimum wage and overall state-level unemployment rate. Joint effects are evaluated at overall unemployment rates of 4 and 8 percent. η refers to the minimum wage elasticity of employment, which is calculated by dividing the coefficient by the relevant employment-to-population ratio. Each specification includes individual controls for gender, race (four categories), age (four categories), education (twelve categories), and marital status (four categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Each regression includes state-fixed effects, time-fixed effects, and additional trend controls as specified. Standard errors clustered at the state level are reported in parentheses. Significance levels are denoted as follows: ***1 percent, **5 percent, *10 percent.

joint effect of minimum wages and a 4 percent unemployment rate is −0.121 (−0.128 + 8 × 0.002) and significant at the 10 percent level. The effect is similar (−0.114, significant at the 5 percent level) with an imposed 8 percent unemployment rate. But using the second, third, and finally our preferred fourth specification for the two scenarios, the joint employment effects are not statistically distinguishable from zero.

Overall, the results do not indicate heterogeneous impacts of minimum wages depending on the overall rate of unemployment. Within the range of variation in the minimum wage and overall unemployment rates in our sample, the effects do not seem to vary across phases of the business cycle or across labor markets with differing labor market tightness.[16]

[16] More precisely, our specification tests for differential effects of minimum wages across times *and* places with high versus low unemployment rate. We use cross-sectional variation in the unemployment rate along with time series variation, and not just official recessions, to increase statistical power.

*Indexation of Minimum Wages and Anticipation Effects.* The dynamic evidence on employment presented above and in Figure 2 suggests that the negative leading terms for minimum wages represent spurious trends and not anticipation effects. Indeed, the leads are zero when spatial controls are included. In this section, we provide some additional evidence on the anticipation question by explicitly considering indexation. Changes in minimum wage through indexation are almost certainly anticipated.

As of 2010, ten states index the minimum wage to a (usually regional) consumer price index. The Appendix lists these states and the indexed increases in the minimum wage. All but three of these ten states are Western states, clustered in the two Census divisions that make up the Western region. As we discuss below, this clustering makes it difficult to identify precisely the differential effect of minimum wages in the presence of indexation *and* use only within-division variation in minimum wages.

Our first concern is whether the presence of indexation contaminates our baseline estimates. We begin by re-estimating specifications 1–4, but excluding all observations involving indexed minimum wages. In other words, we restrict the sample to observations from states that have never indexed their minimum wage, and observations prior to indexation in those states that have indexed. Comparing the estimates, which are in Table 5, with those in Table 3, we see that the wage and employment estimates are virtually identical. Our preferred estimate (Table 3, specification 4) suggests an employment elasticity of 0.047 in the full sample, and 0.031 in the sample excluding the observations for states when they indexed. This result suggests that the increasing use of indexation in recent years has not affected the estimated minimum wage elasticity of employment. The hours effect in the non-indexed sample reveals a somewhat more negative estimate (−0.074 versus −0.032) that is borderline significant at the 10 percent level. This evidence indicates a modest reduction in hours for teens. However, when we estimate the model with distributed lags and employ the restricted sample (results not shown), most of the negative hours effect appears to be temporary.

Additionally, we examine further the differential effect of minimum wages associated with "indexed" versus "non-indexed" increases. We estimate specifications 1–4, but now we include two additional independent variables. The first is a dichotomous variable, equal to one for the state-quarter observations in which the minimum wage was indexed, and zero otherwise ($\xi\ index_{st}$). Second, we include an interaction term for the log of the minimum wage and the dummy variable for indexation—($MW_{st} \times index_{st}$). In this specification, the minimum wage elasticity for non-indexed changes is just $\beta$ as before (or in the case of employment, $\beta$ divided by the relevant employment-to-population ratio). For indexed changes, the elasticity is $\beta + \delta$, where $\delta$ is the

TABLE 5
MINIMUM WAGE EFFECTS AND INDEXING ON WAGES, EMPLOYMENT, AND HOURS WORKED, TEENS 16–19, 1990–2009

| Specification | | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|---|
| A. Wage | | | | | | |
| Non-indexed sample | Min wage | η | 0.116*** | 0.163*** | 0.165*** | 0.152*** |
| | | se | (0.027) | (0.032) | (0.027) | (0.025) |
| All states sample | Min wage | η | 0.117*** | 0.159*** | 0.165*** | 0.146*** |
| | | se | (0.027) | (0.031) | (0.026) | (0.024) |
| | MW × Index | η | −0.023 | −0.093 | −0.010 | −0.174** |
| | | se | (0.041) | (0.056) | (0.087) | (0.076) |
| | Index | η | 0.057 | 0.181 | 0.018 | 0.333** |
| | | se | (0.082) | (0.112) | (0.165) | (0.144) |
| *Joint minimum wage effect* | | η | 0.094* | 0.066 | 0.155 | −0.027 |
| | | se | (0.050) | (0.071) | (0.100) | (0.083) |
| B. Employment | | | | | | |
| Non-indexed sample | Min wage | coeff | −0.040* | −0.011 | −0.012 | 0.012 |
| | | se | (0.022) | (0.034) | (0.028) | (0.026) |
| | | η | −0.100* | −0.027 | −0.030 | 0.031 |
| All states sample | Min wage | coeff | −0.042* | −0.013 | −0.014 | 0.018 |
| | | se | (0.021) | (0.034) | (0.029) | (0.025) |
| | | η | −0.104 | −0.032 | −0.035 | 0.045 |
| | MW × Index | coeff | −0.132*** | −0.089* | −0.069 | −0.044 |
| | | se | (0.039) | (0.052) | (0.095) | (0.077) |
| | | η | −0.330 | −0.223 | −0.171 | −0.109 |
| | Index | coeff | 0.245*** | 0.165 | 0.128 | 0.081 |
| | | se | (0.076) | (0.105) | (0.183) | (0.151) |
| | | η | 0.613 | 0.413 | 0.320 | 0.202 |
| *Joint minimum wage effect* | | coeff | −0.174*** | −0.102 | −0.083 | −0.026 |
| | | se | (0.042) | (0.063) | (0.105) | (0.084) |
| | | η | −0.435 | −0.254 | −0.206 | −0.064 |
| C. Hours | | | | | | |
| Non-indexed sample | | η | −0.088** | −0.080 | −0.016 | −0.074* |
| | | se | (0.041) | (0.051) | (0.048) | (0.041) |
| All states sample | Min wage | η | −0.083** | −0.064 | −0.013 | −0.043 |
| | | se | (0.038) | (0.050) | (0.044) | (0.041) |
| | MW × Index | η | −0.149* | −0.030 | −0.135 | −0.146 |
| | | se | (0.085) | (0.101) | (0.136) | (0.157) |
| | Index | η | 0.308* | 0.071 | 0.279 | 0.299 |
| | | se | (0.164) | (0.198) | (0.260) | (0.301) |
| *Joint minimum wage effect* | | η | −0.232** | −0.093 | −0.148 | −0.190 |
| | | se | (0.095) | (0.125) | (0.155) | (0.166) |
| Division-specific time controls | | | | Y | | Y |
| State-specific time trends | | | | | Y | Y |

NOTES: The rows labeled "Non-indexed sample" use only state-year observations that do not have indexed minimum wages; the rows labeled "All states sample" use all states and years. *Index* is a dummy variable that turns on when indexation begins and stays on thereafter. *MW* is the log of the minimum wage. *MW* × *Index* is the interaction of the log of the minimum wage and *Index*. Results are reported for the coefficients on log minimum wage, on Index, and on the interaction between the two. η refers to the elasticity of the outcome with respect to MW, Index, or the interaction. For employment, the elasticity is calculated by dividing the coefficient by the relevant employment-to-population ratio. Each specification includes individual controls for gender, race (four categories), age (four categories), education (twelve categories), and marital status (four categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Wage regressions include only those who were working and paid between $1 and $100 per hour in 2009 dollars; log hourly wage is the dependent variable. Hour regressions are restricted to those who had positive hours; log of hours is the dependent variable. Each regression includes state-fixed effects, time-fixed effects, and additional trend controls as specified. Standard errors clustered at the state level are reported in parentheses. Significance levels are denoted as follows: ***1 percent, **5 percent, *10 percent.

coefficient associated with $MW_{st} \times index_{st}$ (adjusted analogously in the case of employment).

Table 5 reports our results for these tests of the effects of indexation. The overall results here are ambiguous and imprecise. For our preferred specification 4, the coefficients all have large standard errors. The wage elasticity and employment elasticities for $\beta + \delta$ (joint effect in Table 5) are close to zero in specification 4, which suggests very little measurable effects from indexed minimum wages. However, the coefficient for indexation itself is very large and significant (0.333) in the wage regression. These results are consistent with either of two hypotheses (1) employers anticipate the changes and act prior to the changes, or (2) there is insufficient variation in minimum wages in the indexed states to estimate these elasticities robustly. Probably more consistent with (2), the hours elasticity is negative, but with large standard errors in our preferred specification, but there is an implausible large and positive effect on the introduction of indexation on hours. The imprecision and fragility of these results is likely the result of the fact that seven of the ten states with indexation are in only two divisions. Consequently, the amount of variation used to estimate these parameters is quite limited.

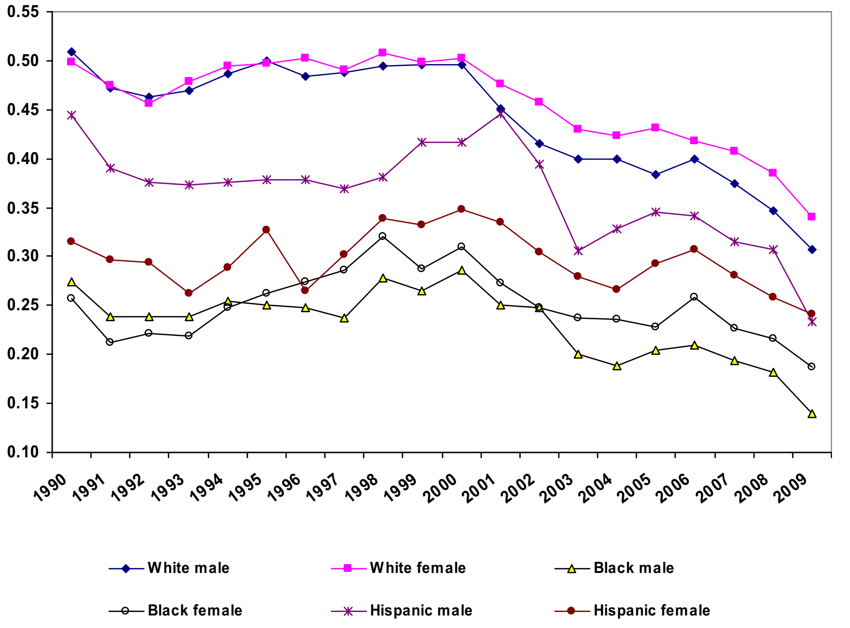
Overall, we find the baseline results robust to the restriction of the sample to non-indexed wages, which (along with our dynamic evidence) suggests that anticipation effects do not drive our baseline elasticities. However, given the limited number of states that have indexed, and their spatial clustering, we are not able to estimate precisely the differential effect of a given increase in minimum wage when it is fully anticipated versus when it is not. Unless indexation is adopted in states in other parts of the United States, additional years of data are unlikely to be of much help in identifying the differential effects of indexed versus non-indexed wage increases using our within-division identification strategy.

## Minimum Wage Effects by Gender, Race, and Ethnicity

Figure 3 displays employment rates among teens by gender, race, and ethnicity over the period 1990–2009. Three main patterns stand out, each with implications for the effects of minimum wages on specific groups. First, male teen employment rates lost ground relative to female teen employment rates in every race and ethnicity group. Second, employment rates are lower among minorities than among whites; since whites, blacks, and Hispanics are not equally distributed across states and Census divisions, estimates of minimum wage effects for each group may be affected by inclusion of controls for spatial heterogeneity. Third, employment rates for black and Latino teens seem

FIGURE 3

Employment to Population Ratio for Teens, 16–19, by Demographic Groups, 1990–2009




Notes: Authors' analysis of Current Population Survey data. White refers to non-Hispanic white.

to be more pro-cyclical than the employment rate for whites. Together, these indicate that spatial heterogeneity of business cycles coupled with selectivity of states with minimum wage increases *may* be important in estimating minimum wage effects for non-white teens.

Other factors may also be at play. A standard explanation of the lower employment rates among minority teens suggests that they are less skilled and experienced than other teens. Minimum wage increases will then have a greater impact on such groups, especially insofar as employers adjust to higher minimum wages by substituting toward higher skilled groups. The prediction is that minority teens will experience higher earning effects and greater disemployment effects, relative to all teens. An alternative view suggests that barriers to mobility are greater among minorities than among teens as a whole. Higher pay then increases the returns to worker search and overcomes existing barriers to employment that are not based on skill and experience differentials (Raphael and Stoll 2002).

To investigate these issues, we estimate our four different specifications on specific gender and race/ethnicity groups. We begin by discussing minimum wage effects for male and female teens separately. We then examine effects by race/ethnicity.

*Earnings, Employment, and Hours Effects by Gender.* Recent studies of teen wage and employment patterns report that differences between male and female teens of similar educational enrollment status have declined in recent decades and the remaining differences are small (Congressional Budget Office 2004). Figure 3 and the descriptive sample statistics in Table 2 present a similar picture. Average wages in the sample are \$8.58 for male teens and \$7.85 for female teens—a 9 percent difference—and the average employment-to-population ratio is identical for both. Figure 4A presents kernel density estimates of wages by gender. The figure suggests that the minimum wage may be more binding for females, which is consistent with the somewhat lower female wage.

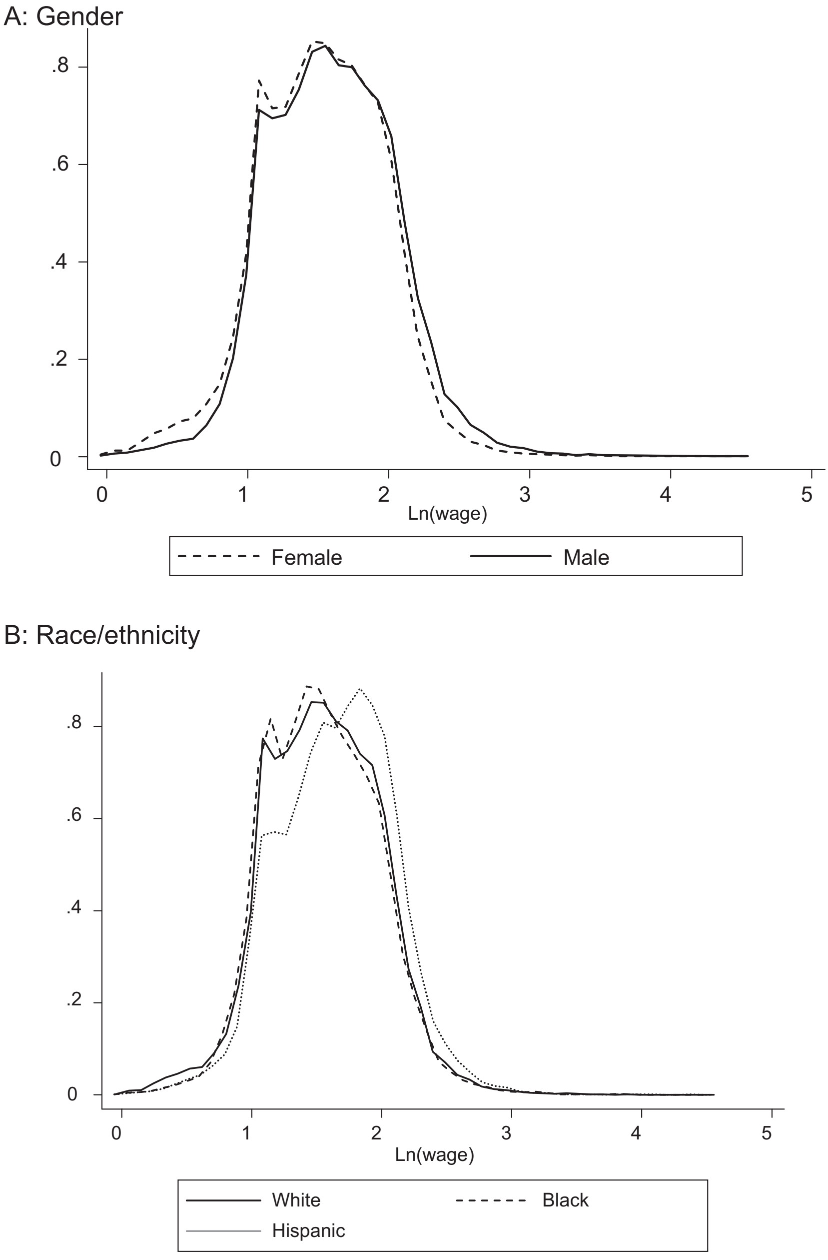
Table 6 reports our estimated wage, employment, and hours elasticities by gender. Panel A reports the presence of a large and highly significant extent of treatment for both genders. For specification 1, the wage elasticity is 0.091 for male teens and 0.147 for female teens. For specification 4, with both controls included, the estimated male teen wage elasticity is 0.099, and the female teen wage elasticity is 0.176, indicating a 75 percent greater effect for female teens. In summary, the minimum wage appears to be more binding for female teens than for male teens. This result obtains in the canonical specification (1) and even more so in our preferred specification (4). These results are consistent with a 9 percent greater average wage among male teens. Female teens are more likely to hold minimum wage jobs.

We turn next to gender patterns in the estimated employment elasticities, which are presented in Table 6, Panel B. In specification 1, the employment effects for all teens are very similar to those for male and female teens separately and are significant at the 5 or 10 percent levels. For specification 2–4, the effects are not significant, and are all smaller than the measured effects in the first specification. But while specification 1 produces significant disemployment effects for both male and female teens, specification 4 shows no significant employment effects for either male or female teens. The gap between the estimates from specification 1 and 4 is −0.175 for males and −0.159 for females. These results reinforce our previous finding that controlling for heterogeneity in employment patterns is crucial in estimating minimum wage effects. The bias arising from insufficient controls seems to affect estimates similarly for both genders.

Panel C of Table 6 provides the minimum wage effects on hours by gender. The estimate from specification 1 for females is −0.090 (significant at the 5 percent level), which is similar to the overall estimate for the total sample. The estimates from specifications 2–4 are all relatively similar to the overall

FIGURE 4

Kernel Wage Densities by Gender and by Race/Ethnicity




Notes: Densities are for the log of real wages (2009$). Analysis of CPS 1990–2009 data for teens 16–19 years of age.

TABLE 6

MINIMUM WAGE EFFECTS ON WAGES, EMPLOYMENT, AND HOURS WORKED, TEENS 16–19, BY GENDER, 1990–2009

| Specification | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| A. Wage | | | | | |
| Male | η | 0.091*** | 0.134*** | 0.123*** | 0.099** |
| | se | (0.025) | (0.031) | (0.032) | (0.026) |
| Female | η | 0.147*** | 0.172*** | 0.205*** | 0.176*** |
| | se | (0.031) | (0.039) | (0.031) | (0.034) |
| B. Employment | | | | | |
| Male | coeff | −0.045* | −0.014 | 0.002 | 0.025 |
| | se | (0.024) | (0.042) | (0.032) | (0.032) |
| | η | −0.113* | −0.036 | 0.005 | 0.062 |
| Female | coeff | −0.054** | −0.020 | −0.031 | 0.010 |
| | se | (0.025) | (0.041) | (0.028) | (0.040) |
| | η | −0.135** | −0.050 | −0.076 | 0.024 |
| C. Hours | | | | | |
| Male | η | −0.060 | −0.068 | 0.001 | −0.046 |
| | se | (0.055) | (0.065) | (0.053) | (0.060) |
| Female | η | −0.090** | −0.040 | −0.008 | −0.021 |
| | se | (0.041) | (0.055) | (0.042) | (0.048) |
| Division-specific time controls | | | Y | | Y |
| State-specific time trends | | | | Y | Y |

NOTES: Results are reported for the coefficients on log minimum wage separately by gender. η refers to the minimum wage elasticity of the outcome. For employment, the elasticity is calculated by dividing the coefficient by the relevant employment-to-population ratio. Each specification includes individual controls for gender, race (four categories), age (four categories), education (twelve categories), and marital status (four categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Wage regressions include only those who were working and paid between $1 and $100 per hour in 2009 dollars; log hourly wage is the dependent variable. Hour regressions are restricted to those who had positive hours; log of hours is the dependent variable. Each regression includes state-fixed effects, time-fixed effects, and additional trend controls as specified. Standard errors clustered at the state level are reported in parentheses. Significance levels are denoted as follows: ***1 percent, **5 percent, *10 percent.

estimates and they too are not statistically significant. The effect of minimum wages on hours for males is also not distinguishable from zero for any of the four specifications.

*Earnings, Employment, and Hours Effects by Race/Ethnicity.* We estimate minimum wage effects on teens by race/ethnicity using the same specifications as before. The geographic clustering of ethnic groups poses some challenge for our preferred specification, which only uses within-division variation in minimum wages. At the beginning of our sample period, blacks were disproportionately located in the South Atlantic division of the United States, while Hispanics were disproportionately located in California and the Southwest. Indeed, the top two divisions accounted for 52 percent of Hispanics and 49 percent of blacks, compared to 32 percent of whites. The bottom two division shares are respectively 1, 4, and 13 percent. Hence, subgroup analysis is somewhat challenging, especially for Hispanics.

Labor market outcomes for black and Hispanic teens continue to be inferior to those for white, non-Hispanic teens, although not in all respects. As Table 2 indicates, the average wage rates do not show such a disparity. Black teens have similar wages to those of white teens ($8.15 for blacks and $8.20 for whites), while Hispanic teen wages are higher ($8.38). As the kernel wage density estimates in Figure 4B show, fewer Hispanics are located in the bottom tail of the distribution near the minimum wage. In other words, Hispanic teens are less likely to be minimum wage workers.

The employment rates among these groups are quite different. During our sample period, the employment rate averaged 0.24 for black teens and 0.33 for Hispanic teens, compared to 0.45 for non-Hispanic white teens.[17] As Figure 3 shows, the employment rates of black and Hispanic teens dropped sharply since the 2001 recession. As we mentioned previously, the poorer outcomes for minority teens may reflect their more limited education or experience, relative to white teens. Moreover, if minimum wage effects lead to substitution toward more educated and experienced workers, then minimum wage policies may have more harmful effects on the employment on disadvantaged groups.

Structural studies of poorer labor market outcomes for black and Hispanic teens point to a different explanation: the spatial mismatch between urban employment and minority population distributions, as well as other disadvantages that these groups face (Raphael 1998; Raphael and Stoll 2002). In this approach, if minimum wage increases make it more worthwhile for disadvantaged teens to travel greater distances to find employment, then minimum wage increases may create relatively more beneficial employment effects for such groups. The research literature thus does not clearly predict how black or Latino teens will be affected by the policies.

Table 7, Panel A reports our estimated treatment effects on wages for separate race/ethnicity groups. For the non-Hispanic white group, the wage elasticities are substantial and significant under all four specifications. These elasticities (and their significance levels) are similar to those in Table 3 for all teens, which is not surprising since non-Hispanic whites account for 65 percent of the total teen sample. In summary, whether or not we include controls for spatial heterogeneity, we find a substantial and significant extent of treatment for whites.

The wage elasticities vary much more among black and Hispanic teens. Among black teens, the wage effect in specification 1 is positive, but not significant. From specification 2 to 4, the effect becomes economically larger and

[17] We use the term "white" and "non-Hispanic white" interchangeably. The same is true for "Hispanic" and "Latino."

TABLE 7

Minimum Wage Effects on Wages, Employment, and Hours Worked, Teens 16–19, 1990–2009, by Race/Ethnicity

| Specification | | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| A. Wage | | | | | |
| White, non-Hispanic | η | 0.129*** | 0.169*** | 0.189*** | 0.159*** |
| | se | (0.025) | (0.032) | (0.026) | (0.024) |
| Black | η | 0.090 | 0.150* | 0.179*** | 0.247*** |
| | se | (0.054) | (0.078) | (0.063) | (0.075) |
| Hispanic | η | 0.127** | −0.013 | 0.075 | −0.044 |
| | se | (0.055) | (0.057) | (0.049) | (0.074) |
| B. Employment | | | | | |
| White, non-Hispanic | coeff | −0.052 | −0.030 | −0.020 | 0.003 |
| | se | (0.031) | (0.041) | (0.030) | (0.032) |
| | η | −0.115 | −0.066 | −0.045 | 0.006 |
| Black | coeff | −0.048 | 0.050 | −0.052 | 0.060 |
| | se | (0.042) | (0.054) | (0.048) | (0.056) |
| | η | −0.200 | 0.209 | −0.218 | 0.250 |
| Hispanic | coeff | −0.010 | 0.016 | 0.019 | 0.008 |
| | se | (0.032) | (0.068) | (0.047) | (0.067) |
| | η | −0.030 | 0.048 | 0.057 | 0.025 |
| C. Hours | | | | | |
| White, non-Hispanic | η | −0.069* | −0.046 | −0.005 | −0.002 |
| | se | (0.039) | (0.053) | (0.030) | (0.035) |
| Black | η | 0.131 | 0.200 | 0.028 | −0.017 |
| | se | (0.106) | (0.146) | (0.101) | (0.160) |
| Hispanic | η | −0.154*** | −0.364*** | −0.0151 | −0.333** |
| | se | (0.046) | (0.113) | (0.087) | (0.140) |
| Division-specific time controls | | | Y | | Y |
| State-specific time trends | | | | Y | Y |

Notes: Results are reported for the coefficients on log minimum wage by race/ethnicity (non-Hispanic whites, non-Hispanic blacks, and Hispanics). η refers to the minimum wage elasticity of the outcome. For employment, the elasticity is calculated by dividing the coefficient by the relevant employment-to-population ratio. Each specification includes individual controls for gender, race (four categories), age (four categories), education (twelve categories), and marital status (four categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Wage regressions include only those who were working and paid between \$1 and \$100 per hour in 2009 dollars; log hourly wage is the dependent variable. Hour regressions are restricted to those who had positive hours; log of hours is the dependent variable. Each regression includes state-fixed effects, time-fixed effects, and additional trend controls as specified. Standard errors clustered at the state level are reported in parentheses. Significance levels are denoted as follows: ***1 percent, **5 percent, *10 percent.

highly significant. Our preferred specification 4 indicates a treatment effect of 0.247 significant at the 1 percent level.

Among Hispanic teens, the magnitude and the statistical significance of the wage elasticity fall considerably from specification 1 to specification 4 (from 0.127 and 5 percent significance to −0.044 and insignificant). In summary, we find a substantial wage effect for blacks but not for Hispanics. These results are consistent with the higher average wages earned by

Hispanic teens and the smaller numbers near the minimum wage. We remain concerned, however, by the possibility that higher Hispanic wages are interacting with higher Hispanic spatial concentration. As we discuss further below, since we cannot clearly detect a treatment for Hispanic teens once spatial controls are added, other results on hours and wages should be interpreted with caution.

We turn next to the employment elasticities by race/ethnicity, which are reported in Table 7, Panel B. Noticeably, none of the estimates is statistically significant regardless of specification. (For specification 1, lack of significance is not due to the size of the coefficients, but rather the larger standard errors.) All of the point estimates are negative in specification 1, but they are all positive in specification 4. Most of the standard errors are larger for Hispanics and blacks as compared with non-Hispanic whites, especially for the more saturated specifications. Thus, results for black and Hispanic teens reinforce the need for caution in interpreting estimates for disaggregated racial groups due to limits of the data and methodology.[18] However, it does appear that controlling for spatial heterogeneity by using within-Census division variation is particularly important when looking at African-American employment effects. The gap in the employment elasticities between specifications 4 and 1 is 0.450 for black teens, followed by 0.121 for whites, and 0.055 for Hispanics.

Panel C of Table 7 presents the results for hours. In specification 1, the hours effect is negative and significant for non-Hispanic whites and for Hispanics, but not for blacks. In specification 4, the hours effect is small and not significant for non-Hispanic whites and blacks. The growth in the standard errors for the black and Hispanic samples indicates a growing imprecision of our estimates as we add more controls for spatial heterogeneity. Moreover, among Hispanics, the hours effect is very large (−0.333) and significant in specification 4, even though the wage effect is close to zero.

As previously mentioned, the puzzling and somewhat fragile evidence for Hispanic teens may be driven by the concentration of Hispanic teens in a small number of Census divisions, on the one hand, and the small number of Hispanic teens in most states at the beginning of the sample period. These patterns reduce the ability to estimate effects for this group robustly within our methodology. The results also raise questions about the reliability of recent work on minimum wage effects on Latino teens using state- and time-fixed effect models (see Orrenius and Zavodny 2010).

In summary, we find that minimum wages for black and white teens do have strong effects on wages while not having any clear negative effect on employment or hours. The bias due to spatial heterogeneity seems particularly large

[18] For example, there is limited variation by race within divisions.

for black teens. The results for Hispanic teenagers are imprecise and fragile when we include spatial controls. As Brown (1999: 2188) also finds, there is an unfortunate but real trade-off between focusing on plausible sources of variation versus estimating impacts on these demographic teen subsamples.

## Comparisons with Restaurant Studies and Other Teen Studies

We examine next whether increases in the minimum wage have similar effects across studies and time periods that incorporate analogous controls for spatial heterogeneity. The fixed-effect models without and with controls for division-specific time controls and state-specific time trends in our study are similar to those used in Dube, Lester, and Reich (2010a), although their time frame (1990–2006) is shorter. Although the restaurant-based elasticities in the table are not exactly comparable to the teen-based elasticities, they do offer insight into the outcomes generated by using similar model specifications and controls. As already mentioned, the present study is comparable to Neumark and Wascher (2007a). They also used CPS data on teens, although for different years (1997–2005), and two specifications that are similar to ours.

Table 8 compares employment elasticities for our main results along with those of Dube, Lester, and Reich (2010a) and Neumark and Wascher (2007a). The first row repeats from Table 3 our results for 1990–2009. For comparability with the restaurant study, we present in the second row our results when we restrict the sample to 1990–2006. The third row provides the restaurant results. The fourth row provides our results when we restrict our sample to 1997–2005, the same years as Neumark and Wascher, whose results are in the fifth row. The remaining rows are included to illustrate the stability of the estimates using other panel years and the CPS teen data.

Results from specification 1 are similar across our study and the Dube, Lester, and Reich (2010a) restaurant study: they indicate large and significant negative employment effects in the typical range of a 1–3 percent from a 10 percent increase in the minimum wage. When the division control (specification 2) is added, results from the present study and from Dube, Lester, and Reich show that the economic effects are reduced substantially and that they are not statistically distinguishable from zero. Adding state-specific time trend controls without division controls (specification 3) also renders the employment outcomes in each study insignificant and smaller in absolute value. With the addition of division-specific and state-specific time controls included in specification 4, the point estimates are not significant. In Dube, Lester, and Reich, and in this paper, employment elasticities more negative than −0.05 can be ruled out at the 10 percent level.

TABLE 8

A Comparison of Minimum Wage Employment Elasticities

| | Specification | | | |
|---|---|---|---|---|
| Study | (1) | (2) | (3) | (4) |
| This study 1990–2009 | −0.118*** | −0.036 | −0.034 | 0.047 |
| | (0.055) | (0.085) | (0.068) | (0.060) |
| 90 percent CI | (−0.208, −0.028) | (−0.176, 0.104) | (−0.145, 0.077) | (−0.052, 0.146) |
| This study 1990–2006 | −0.165*** | −0.096 | −0.020 | −0.019 |
| | (0.048) | (0.104) | (0.069) | (0.089) |
| Dube et al. (2010a) QCEW | −0.207*** | −0.076 | 0.055 | 0.060 |
| Restaurants 1990–2006 | (0.063) | (0.060) | (0.042) | (0.041) |
| 90 percent CI | (−0.312, −0.102) | (−0.176, 0.023) | (−0.014, 0.124) | (−0.007, 0.127) |
| This study 1997–2005† | −0.102 | −0.196 | −0.098 | −0.100 |
| | (0.071) | (0.134) | (0.084) | (0.130) |
| Neumark and Wascher | −0.136* | | −0.178 | |
| (2007a) 1997–2005‡ | (na) | | (na) | |
| This study 1991–1998 | 0.072 | 0.025 | 0.068 | 0.057 |
| | (0.068) | (0.125) | (0.069) | (0.125) |
| This study 1999–2006 | −0.163*** | −0.143* | −0.155** | −0.037 |
| | (0.052) | (0.085) | (0.081) | (0.103) |
| This study 1987–2006 | −0.159*** | −0.085 | −0.030 | −0.028 |
| | (0.043) | (0.086) | (0.065) | (0.075) |
| Division-specific time controls | | Y | | Y |
| State-specific time trends | | | Y | Y |

Notes: Our results using CPS data for teens, except where otherwise noted. Elasticities for restaurants are not precisely comparable. They are presented to show the effects of using similar model specifications and controls. Standard errors are in parentheses. Significance levels are ***1 percent, **5 percent, *10 percent. †Our results for the same time period and specification used by Neumark and Wascher (2007a). ‡We calculated these elasticities from Neumark and Wascher (2007a), using the employment-population ratios reported in their Table 1 and the employment coefficients reported in their Table 2, specification 1. Neumark and Wascher's sample excludes all observations with imputed wages or employment, while ours includes them. Over four-fifths of the observations with imputations involve only missing wage data and hence do not need to be excluded in a regression on employment.

Since the proportions of minimum wage workers who are teens and who are restaurant workers are similar, it is perhaps not surprising that the estimated effects are also similar.[19] Differences in findings appear to be the result of different specifications and identifying assumptions, not different data sets or the groups under investigation.

While the teen and restaurant results are not exactly comparable, they both support the importance of including controls for heterogeneous trends in low-wage employment. In Dube, Lester, and Reich (2010a), inclusion of division-specific time effects and state-level linear time trends provide imperfect

[19] Of course, two groups with identical incidences of minimum wage workers need not have identical employment elasticities. This is particularly the case when one group is defined demographically and the other is defined by industry.

proxies for their local estimators, which also produce employment elasticities indistinguishable from zero. Although CPS data limitations preclude replicating the analysis at such a local level, the inclusion of these controls attenuates the disemployment effect for teens in the CPS in an analogous manner. The omission of controls for local differences in underlying local labor market conditions induces a serious bias in the teen studies as well.

The results also caution us against relying just on state linear trends to control for heterogeneity, especially when using a short panel, as in Neumark and Wascher (2007a). When census division controls are not included, the results from 1997 to 2005 look quite different from the longer 1990–2009 panel. Shorter panels with 8 years or less of data seem to be sensitive to small deviations in the sample period, but that is not the case for panels with 15–20 years of data. Generally speaking, our preferred specification 4 tends to be more stable across time periods than does specification 3 with just state linear trends. The range of coefficients for specification 3 across different sample periods is (−0.155, 0.068), a spread of 0.223. The range for specification 4 is (−0.1, 0.057) for a smaller spread of 0.157. While linear trends do a good job of eliminating long-term trend differences across states in longer panels, they are a less valuable means of controlling for spatially correlated shocks, and they are estimated poorly in shorter panels.

## Summary and Conclusions

Using the canonical fixed effects specification on the sample of teens, we estimate an employment elasticity of −0.118, similar to the −0.3 to −0.1 percent disemployment consensus of the estimates in other national CPS studies. But sweeping out the variation across Census divisions, and allowing for state-specific trends render the employment elasticities indistinguishable from zero. The employment elasticity from our preferred specification (4) is 0.047 and this result is not driven by the imprecision of the preferred estimates. Employment elasticities more negative than −0.053 can be ruled out at the 10 percent level; and those more negative than −0.072 can be ruled out at the 5 percent level.

Further evidence on the bias in the canonical fixed-effects model comes from our dynamic specifications using distributed lags. The time path of teen employment around the minimum wage change in the canonical specification indicates that teen employment was unusually low and falling substantially *prior* to the actual increase. We can rule out an anticipation effect explanation since inclusion of spatial controls renders the lead terms close to zero. The effect on hours is also close to zero once spatial controls are added. Overall, the evidence strongly points to the failure of the canonical fixed-effects specification to control for the heterogeneity and selectivity of states where minimum wages increased during this period.

The bias of the fixed-effects specification is similar for male and female teens, but particularly large for African Americans. Sweeping out variation by including spatial controls does increase the difficulty of sub-group analysis, and reduces the precision of our estimates for non-white teen groups. This imprecision is particularly true for Hispanic teens, for whom results are especially fragile.

We account for the growth in indexed (and hence likely anticipated) minimum wage increases by limiting our sample to states and time periods with non-indexed minimum wages only. Results on wage and employment are nearly identical. One exception is hours, which show a somewhat more negative effect when we focus on the non-indexed sample.

Another contribution of this paper is to test for heterogeneity in the treatment effect by business cycle phases. We do not find evidence that the effects are systematically different in periods of high versus low overall unemployment.

Since the proportion of teens and the proportions of restaurant workers who are paid at or near the minimum wage are very similar, it is of interest to compare our estimates with those in Dube, Lester, and Reich (2010a). The estimated minimum wage employment elasticities from the two studies are very close. Moreover, the results in the two studies change in similar ways with the inclusion of controls for spatial heterogeneity. These results suggest that the effects of controlling for such heterogeneity do not result from the focus on any one group, industry or dataset.

Our analysis finds that heterogeneity in employment patterns and selectivity among states constitute significant concerns for conventional minimum wage studies. Although adding division and state trend controls does not constitute a panacea, they provide important controls that mitigate the bias from unobserved heterogeneities that may be correlated with minimum wage changes. Since estimates in previous national-level studies insufficiently address this issue, they do not provide a credible guide for public policy. Interpretations of the quality and nature of the evidence in the existing minimum wage literature, such as those in Neumark and Wascher (2007b, 2008), must be revised substantially. Put simply, our findings indicate that minimum wage increases—in the range that have been implemented in the United States—do not reduce employment among teens.

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# Appendix

TABLE A1

ANNUAL MINIMUM WAGE CHANGES FOR STATES THAT INDEX

| Year | State | Indexed amount | Amount to catch federal MW | Percent due to index | Percent due to fed. MW catch-up |
|---|---|---|---|---|---|
| 2001 | Washington | 0.22 | – | 3.4 | – |
| 2002 | Washington | 0.18 | – | 2.7 | – |
| 2003 | Washington | 0.11 | – | 1.6 | – |
| 2004 | Oregon | 0.15 | – | 2.2 | – |
| | Washington | 0.15 | – | 2.1 | – |
| 2005 | Oregon | 0.20 | – | 2.8 | – |
| | Washington | 0.19 | – | 2.7 | – |
| 2006 | Florida | 0.25 | – | 4.1 | – |
| | Oregon | 0.25 | – | 3.4 | – |
| | Washington | 0.28 | – | 3.8 | – |
| 2007 | Florida | 0.27 | – | 4.2 | – |
| | Nevada | 0.18 | – | 2.9 | – |
| | Oregon | 0.30 | – | 4.0 | – |
| | Vermont | 0.28 | – | 3.9 | – |
| | Washington | 0.30 | – | 3.9 | – |
| 2008 | Arizona | 0.15 | – | 2.2 | – |
| | Colorado | 0.17 | – | 2.5 | – |
| | Florida | 0.12 | – | 1.8 | – |
| | Missouri | 0.15 | – | 2.3 | – |
| | Montana | 0.10 | 0.30 | 1.6 | 4.9 |
| | Nevada | 0.52 | – | 8.2* | – |
| | Ohio | 0.15 | – | 2.2 | – |
| | Oregon | 0.15 | – | 1.9 | – |
| | Vermont | 0.15 | – | 2.0 | – |
| | Washington | 0.14 | – | 1.8 | – |
| 2009 | Arizona | 0.35 | – | 5.1 | – |
| | Colorado | 0.26 | – | 3.7 | – |
| | Florida | 0.42 | – | 6.2 | – |
| | Missouri | 0.40 | 0.20 | 6.0 | 2.8 |
| | Montana | 0.35 | 0.30 | 4.8 | 5.3 |
| | Nevada | 0.70 | – | 10.2* | – |
| | Ohio | 0.30 | – | 4.3 | – |
| | Oregon | 0.45 | – | 5.7 | – |
| | Vermont | 0.38 | – | 4.9 | – |
| | Washington | 0.48 | – | 5.9 | – |

NOTES: Minimum wage increased twice in 2008 for Montana and 2009 for Missouri and Montana: on January 1st to index and then again on July 24th to match federal minimum wage laws.

*The large percentage increases for Nevada are not due to CPI indexing, but to federal minimum wage increases, as Nevada adjusts the wage by whichever is greater: min{CPI, 3 percent} *or* Federal Minimum +$1.