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

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ABSTRACT

Traditional estimates of minimum wage 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 vary with 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 small disemployment effects. Dynamic evidence further shows the nature of bias in traditional estimates, and it also rules out more negative long run effects. We do not find evidence of heterogeneous employment effects in different parts of the business cycle. We also consider predictable versus unpredictable changes in the minimum wage by looking at indexation of the minimum wage in some states.

1. 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—are associated with many studies that focus on teens, that use national-level household data (usually the Current Population Survey), and that 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, that use employer-based data, and that use only local comparisons 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 evidence suggests other possibilities (Dube, Lester and Reich, forthcoming). Unobserved spatial heterogeneities in employment trends can generate biases toward negative employment elasticities in national minimum wage studies as well as overstate the precision of local studies.

In this paper, we seek to address and resolve the conflicting findings by using CPS data on teens over the 1990 to 2009 period and providing a detailed examination of heterogeneity and selectivity issues. More specifically, we consider whether the source of identifying variation in the minimum wage is coupled with sufficient controls for

[1] Card and Krueger (2000); Neumark and Wascher (2007); Dube, Naidu and Reich (2007).

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

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

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

results highlight the importance of estimates that control for spatial heterogeneity, even at as coarse a level as 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 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 we include these controls, there is no visible 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 gender and by race/ethnicity. Overall, we find little difference in the employment or hours effects on male and female teens. For both white and black teens the minimum wage has strong effects on the average wage; and in all cases 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 benefits are positive or negative for a group depends upon whether the sum of the estimated wage, employment and hours elasticities is greater than or less 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 (e.g., Neumark and Wascher 2007) 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. The recession that began at the end of 2007 and continued through 2009 overlapped with federal minimum wage increases in July 2008 and July 2009. Do employment effects of minimum wage increases differ between tight and slack labor markets? 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.

Automatically indexed adjustments to state minimum wages began in Washington State in 2001. Since then indexing has become more wide spread; 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

[3] See Appendix Table A-1 for a summary of minimum wage indexation.

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 hours effects are somewhat more negative). However, given the small number of states with indexation – and their geographic clustering – our estimates of the differential effects of minimum wage in indexed versus non-indexed states are imprecise.

2. Relation to Existing Literature

We do not attempt to review the minimum wage and teen employment literature here. For two such reviews, see Brown (1999) and Neumark and Wascher (2008); our interpretation of recent studies differs considerably from Neumark and Wascher. Instead, here we will relate the topics explored in this paper to the most relevant recent papers in the literature.

For the most part, minimum wage studies using national CPS panel data with state and year fixed-effects find economically modest but statistically significant negative employment effects on teens, with elasticities that range from -0.1 to -0.3. Sabia (2009) and Neumark and Wascher (2007) are two recent papers in this vein. Using CPS data for 1979 to 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 (2007) used pooled time-series cross-section individual CPS data for 1997 to 2005. Neumark and Wascher motivated their selection of the period since 1997 by arguing that welfare reform and expansions of the EITC may have changed the dynamics of the low-wage labor market. They estimate a negative employment elasticity of -0.136 among teens, significant at the 10 percent level.

As we already mentioned, minimum wage studies that use local restaurant employment data generally do not find disemployment effects.[4] 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 lead to overstatement of the precision of their estimates. These issues were addressed by Dube, Lester and Reich (forthcoming), who compared all the contiguous counties in the U.S. that share a state border. This method employed county-level administrative data on restaurant employment and effectively generalized the local studies with national data. As previously mentioned, Dube, Lester and Reich confirmed that existing national minimum wage studies lacked adequate controls for spatial heterogeneity in employment growth.[5] Without such controls, Dube, Lester and Reich found significant disemployment effects, within the "old consensus"

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

[5] 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.

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.

An important question, which we consider below, is whether the Dube, Lester and Reich results about spatial heterogeneity apply to teen employment. While the CPS data do not allow us to consider discontinuities around state borders, we use coarser controls for spatial heterogeneity, which in Dube, Lester and Reich produced similar results as the discontinuity-based estimates.

Several other papers have recently also looked at teen employment and minimum wages. A notable example is Giuliano (2007), who looks at the effects of a federal minimum wage shock on employment across establishments of a single retailer in different areas of the U.S. Giuliano found that both overall employment and teen employment did not fall. While examining the effects within a single company is instructive for many reasons, the study does not tell us about the 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.

Orrenius and Zavodny (2008) considered the effect of minimum wages on teen employment and expanded the set of business cycle controls beyond a single state-level unemployment rate. In that sense, it 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.

Keeping in mind 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 (2010) 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 wages increases affect teen outcomes differentially in high versus low unemployment periods.

In the patchwork of minimum wage laws in the U. S., 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.[6]

The possibility of anticipation can cause problems for estimating the effects of minimum wage. 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*.[7]

Unlike in many OECD countries, in the U.S. most minimum wage adjustments are not automatic. Since ten states have recently implemented indexation, it is possible that

[6] 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.

[7] For more on this point, see Pinoli (2008), who uses a surprising political transition in Spain to differentially estimate 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.

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 a) exclude states with indexation and b) 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 leaded 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 effects 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.

3. Data

We construct an individual-level repeated cross-section sample from the CPS Outgoing Rotation Groups for the years 1990 to 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 to 19. 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 $5.41. Although male teens were paid more than female teens—$5.62 versus $5.20—pay differentials by race/ethnicity were considerably smaller.

Over the sample period, 40 percent of all teens 16-19 were employed, with identical percentages for males and females. 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 somewhat higher average hours among males, blacks and Hispanics. Finally, on average, state minimum wages were $1.26 above federal minimum wages.

4. 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 the natural log of hourly earnings, a dichotomous employment measure that takes on the value one if the person is working, or the natural log of usual hours of work. The baseline fixed-effects specification is then:

$$y_{ist} = \beta MW_{st} + X_{ist}\Gamma + \lambda \cdot unemp_{st} + \phi_s + \tau_t + \varepsilon_{ist} \qquad (1)$$

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 characteristics, *unemp* is the quarterly (non-seasonally adjusted) unemployment rate in state *s* at time *t,* ϕ_s refers to the state fixed effect and τ_t represents time dummies incremented in quarters.[8] In this canonical

[8] The individual characteristics include 2 gender categories, 4 race/ethnicity categories, 12 education categories and 4 marital status categories.

specification, including state and time dummies as well as the overall unemployment rate is thought to sufficiently control for local labor market conditions facing teenage workers.

There is, however, growing evidence (Dube, Lester and Reich 2007) 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 (τ_{dt}) eliminates the between-division variation and hence better controls for spatial heterogeneity in differential employment patterns, including region-specific economic shocks:

$$y_{ist} = \beta MW_{st} + X_{ist}\Gamma + \lambda \cdot unemp_{st} + \phi_s + \tau_{dt} + \varepsilon_{ist} \quad (2)$$

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:

$$y_{ist} = \beta MW_{st} + X_{ist}\Gamma + \lambda \cdot unemp_{st} + \phi_s + \psi_s \cdot t + \tau_t + \varepsilon_{ist} \quad (3)$$

where ψ_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:

$$y_{ist} = \beta MW_{st} + X_{ist}\Gamma + \lambda \cdot unemp_{st} + \phi_s + \psi_s \cdot t + \tau_{dt} + \varepsilon_{ist} \quad (4)$$

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 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 a 25-quarter window, starting at 8 quarters before the minimum wage change and continuing to 16 quarters after the change.

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

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

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 adding the coefficients β_{-8} through β_{16}.

5. Results

5.1 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"—that increases in the minimum wage led to increased wages for the teenage population, conditional on employment. Table 3, Panel A presents the estimated effects on wages for all teens, for male teens, and for female teens. The coefficient, which is also the wage elasticity, is positive and significant at the 1 percent level in all the specifications except the last specification for males, which is significant at the 5 percent level. The magnitudes vary somewhat among the specifications. In specification 1, the fixed-effects model, the treatment coefficient is 0.123 for all teens. 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 (specification 4), the treatment effect for all teens is 0.149 and remains highly significant.

These results indicate that the treatment effect of minimum wages remains 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 16-19 were paid within ten 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 the right-hand column present results for specification 4, which includes both state-specific time trends and division-specific time effects. Both wage graphs show a clear increase just at the time of the change. However, the preferred specification (4) generates a sharper "treatment," which we interpret as reinforcing the validity of including additional controls.

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.[9] 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 significant. Finally, in specification 4, the employment elasticity is 0.047 and remains insignificant. In other words, allowing for variation in employment trends over the 1990 to 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 employment elasticities more negative than -0.052.[10]

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.[11]

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

[9] The elasticity is obtained by dividing the coefficient by the employment-to-population rate of the group in question.
[10] Confidence intervals are reported in Table 8 below.
[11] In Section 6 we discuss our earnings and employment estimates for gender and race/ethnicity groups.

employment effects throughout the 25 quarter window, including prior to the minimum wage increase. The "response" of employment 4 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 16th and later quarters (-0.2). 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.

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 8 quarters and then a small positive employment effect 8 quarters after the minimum wage increase. Interestingly, there is no evidence that the long term employment response (quarter 16 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 -0.1. This result calls into question the reconciliation offered by Benjamin, Baker and Stanger 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, the dynamic evidence 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.

Given that our evidence does not support disemployment effects associated with minimum wage increases it may be the case that there is 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. Hence, 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 12 quarters.

We can use the evidence on hourly wages and hours together to calculate the effect on the teen wage bill. The wage bill elasticity will be negative if the labor (headcount plus hours) demand elasticity is less than -1; in this case, teenagers as a whole are worse off from the increase in minimum wage. The teen wage bill elasticity is just the sum of the three elasticities: average wage, hours, and employment. In the canonical framework (specification 1), the wage bill elasticity is *negative* (-0.069 = 0.123 - 0.118 - 0.074). This indicates that an increase in the minimum wage makes teens, as a whole, worse off. In contrast, once we account for spatial heterogeneities in specification 4, we get a *positive* wage bill elasticity of 0.164 (0.149 + 0.047 -0.032). Failure to account for spatial heterogeneity therefore has important welfare implications when it comes to evaluating minimum wage changes.

5.2 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 high rates of unemployment. These two increases were enacted in the Fair Minimum Wage Act of 2007, at a time when the economy was still in expansion. The increases in 2008 and 2009 garnered much attention 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. But was this increase in teen unemployment a result of minimum wage increases during an especially severe economic downturn?

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

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 ($\beta + \gamma$*$unemp_{st}$) 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 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.[12]

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[12]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.

5.2 Indexation of Minimum Wage and Anticipation Effects

Our dynamic evidence presented above suggests that the negative lead terms for minimum wages represent spurious trends and not anticipation, since 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. Appendix A1 lists these states and the indexed increases in the minimum wage. All but 3 of these 10 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 precisely identify the differential effect of minimum wages in the presence of indexation *and* use only within-division variation in minimum wages.

Our primary concern is whether the presence of indexation contaminates our baseline estimates. We begin by re-estimating specifications 1 through 4 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 in Table 5 with those in Table 3, we see that the wage and employment estimates are virtually identical. Our preferred estimate (specification 4) suggests an employment elasticity of 0.019 in the full sample, and 0.012 in the sample excluding indexation. This result suggests that the increasing use of indexation in recent years has not affected the estimated minimum wage elasticity of employment. For hours, when we exclude indexed observations, we do find a somewhat more negative estimate (-0.074 versus -0.032) that is borderline significant at the 10% level. When we use the most credible source of variation, we obtain evidence of 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 seems temporary.

Additionally, we estimate the differential effect of minimum wages associated with "indexed" versus "non-indexed" increases. We estimate specifications 1 through 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— ($MW_{st}*index_{st}$). In this specification, the minimum wage elasticity for non-indexed changes is just β as before. For indexed changes, the elasticity is $\beta + \delta$, where δ is the coefficient associated with $MW_{st}*index_{st}$.

Table 6 reports our results for tests of the effects of indexation. The overall results here are ambiguous and imprecise. For our preferred specification 4, all the coefficients are measured with considerable error. The wage elasticity and employment elasticities for $\beta + \delta$ are close to zero, suggesting 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 precisely estimate 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 U.S., 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.

6. 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 to be more pro-cyclical than are 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 earnings 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.

6.1 Earnings and employment 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 \$5.62 for male teens and \$5.20 for female teens—an 8 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, consistent with the somewhat lower female wage.

Table 3 reports our estimated wage, employment and hours elasticities by gender. Panel A reports the presence of a 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, indicating a 60 percent larger treatment effect among 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. In summary, the minimum wage appears to be more binding for female teens than for male teens. This result obtain in the canonical specification (1) and even more so in our preferred specification (4). These results are consistent with an 8 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 3, 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 through 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. And the bias arising from insufficient controls seems to affect estimates similarly for both genders.

Panel C of Table 3 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 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.

6.2 Earnings, employment and hours effects by race/ethnicity

We estimate minimum wage effects on teens by race/ethnicity using the same specifications as before. A complication here is spatial heterogeneity: at the beginning of our sample period blacks were disproportionately located in the South Atlantic division of the U.S., 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. 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 the same wage as white teens ($5.34), while Hispanic teen wages are higher ($5.78). 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 0.46 for non-Hispanic white teens.[13] 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

[13] We use the term “white” and “non-Hispanic white” interchangeably. The same is true for “Hispanic” and “Latino”.

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 whites accounts 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 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 are 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 to 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.[14] 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 0.448 for black teens, followed by 0.121 for whites, and 0.070 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 robustly estimate effects for this group within our methodology.

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

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 for black teens. The results for Hispanic teenagers are imprecise and fragile when we include spatial controls. There is an unfortunate but real tradeoff between focusing on plausible sources of variation versus estimating impacts on particular subsamples of teens.

7. Comparisons with restaurant studies

We examine next whether increases in the minimum wage have similar effects across studies that incorporate analogous controls for spatial heterogeneity. The fixed-effects 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 (DLR, 2007), but as already mentioned the data and the group studied were similar to Neumark and Wascher (2007)—they used CPS data (although different years: 1997-2005) and two specifications similar to ours. Table 8 provides employment effects for our main results along with those of DLR and Neumark and Wascher. Although the elasticities in the table are not directly comparable they do offer insight into the outcomes generated by using similar model specifications and controls.

Results from specification 1 are similar across the three studies—which indicate large and significant negative employment effects in the typical range of a 1 to 3 percent from a 10 percent increase in the minimum wage. When the division control (specification 2) is added (this specification was not included in Neumark and Wascher) results from this study and from DLR show the economic effect is reduced substantially and 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—except in Neumark and Wascher (more on this below). With the addition of division-specific and state-specific time controls included in specification 4 both of the point estimates are positive and 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.

While these results are not directly comparable, they support two conclusions. The first concerns the importance of including controls for heterogeneous trends in low-wage employment. In Dube, Lester and Reich, inclusion of division-specific time effects and state-level linear time trends provide imperfect 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 (2007). The results from 1997-2005 look quite different from the longer 1990-2009 panel when census division controls are not included. (Although not shown here, results from specifications 2 and 4 are much more stable across sample periods.) Shorter panels of 5 to 10 years seem to be sensitive to small deviations in the sample period, but that is not the case for panels with 15 to 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.

The second conclusion concerns the similar coefficients for each specification across the two studies. 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.[15] Differences in findings appear to be the result of different specifications and identifying assumptions, not different data sets or the groups under investigation.

8. 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 renders the employment elasticities indistinguishable from zero. The employment elasticity from our preferred

[15] Of course, it does not necessarily follow that if two groups have similar incidence of minimum wage workers that the employment elasticities would necessarily be the same. This is particularly true when one group is defined demographically while the other by industry.

specification (4) is 0.047. 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 over this period.

The bias of the fixed-effects specification is similar for male and female teenagers, 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 is particularly true for Hispanic teens, for which 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 to those in Dube, Lester and Reich (2007). 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 of states constitutes a significant concern for conventional minimum wage studies. Although adding division and state trend controls do not constitute a panacea, they do provide important controls that mitigate the bias that results from unobserved heterogeneities that may be correlated with minimum wage changes. Since estimates in previous national-level studies insufficiently address this issue, the interpretation of the evidence in the existing minimum wage literature (such as those reviewed by Neumark and Wascher (2008) must be revised accordingly.

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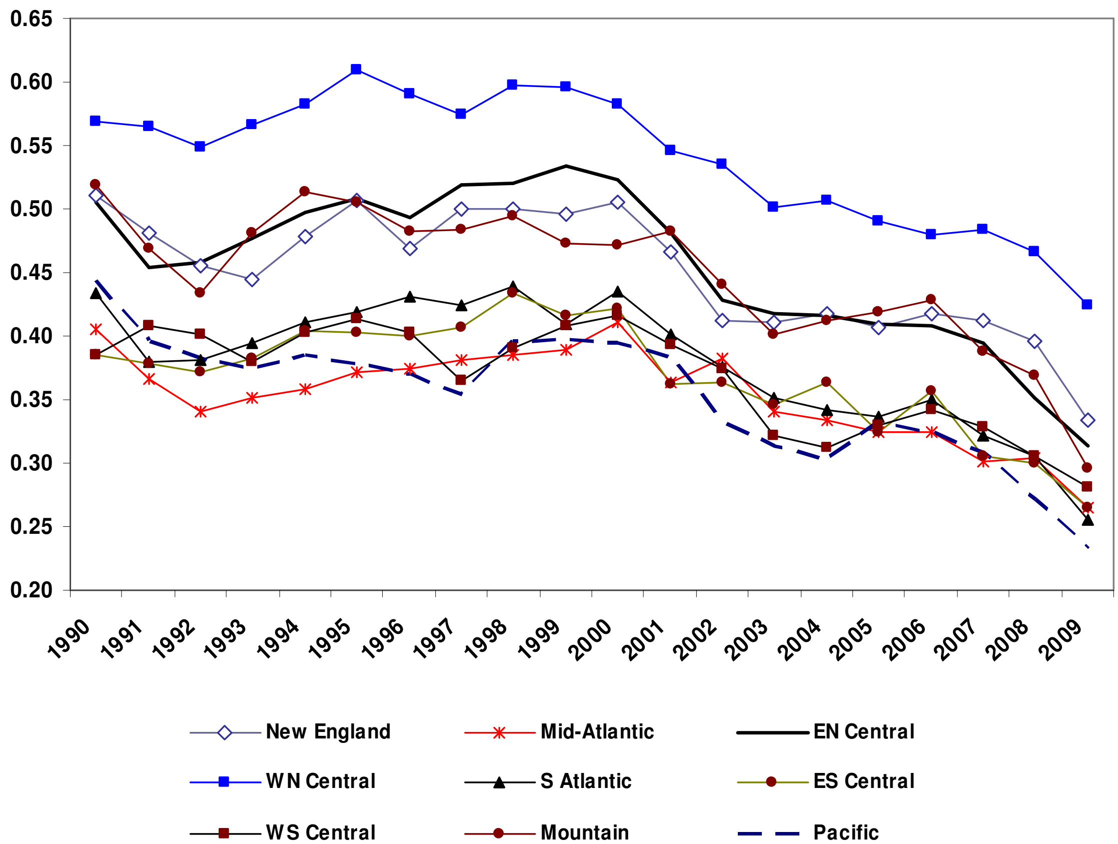
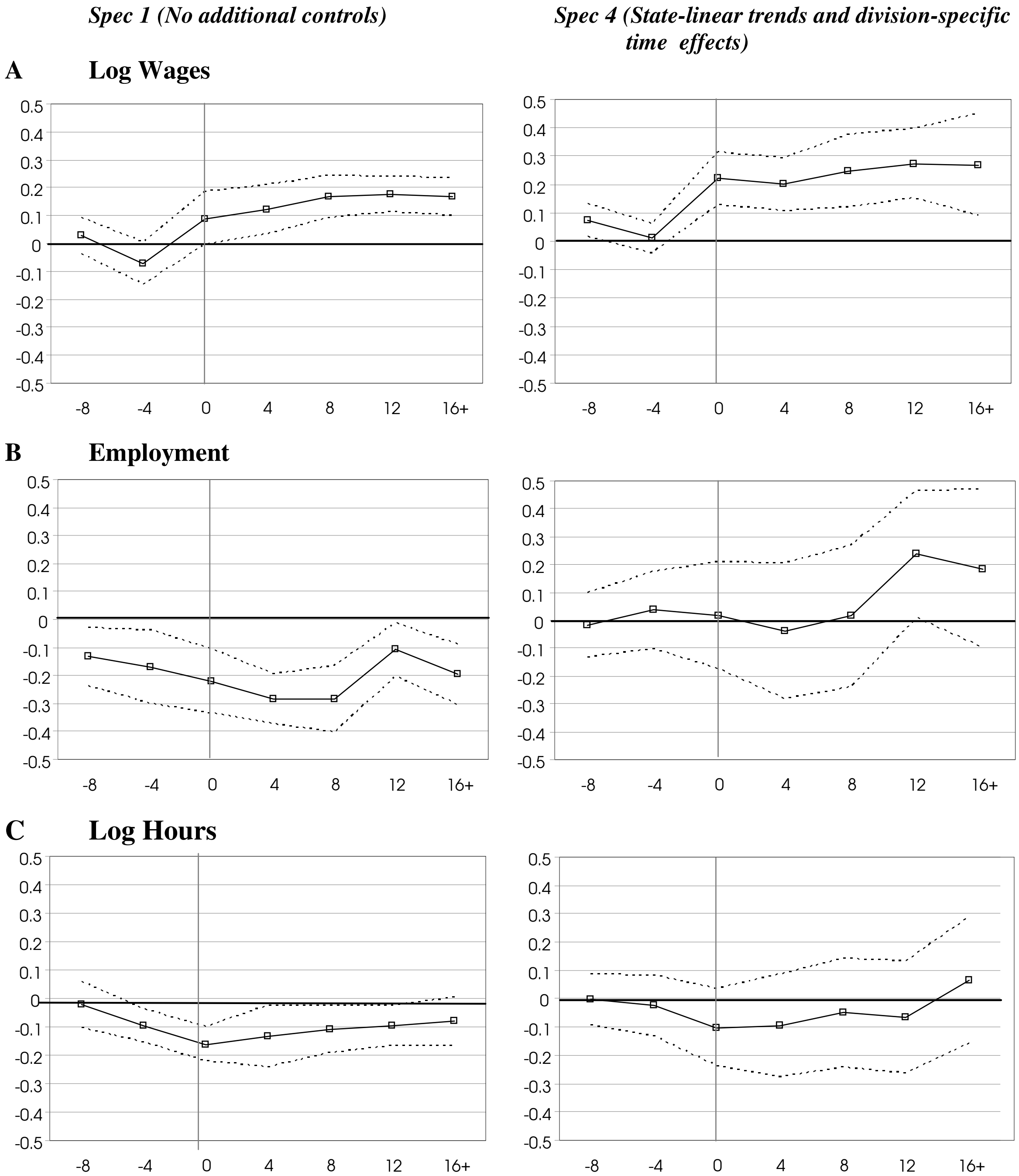
Figure 1 Employment to population ratio for teens, 16-19, by nine Census divisions, 1990-2009



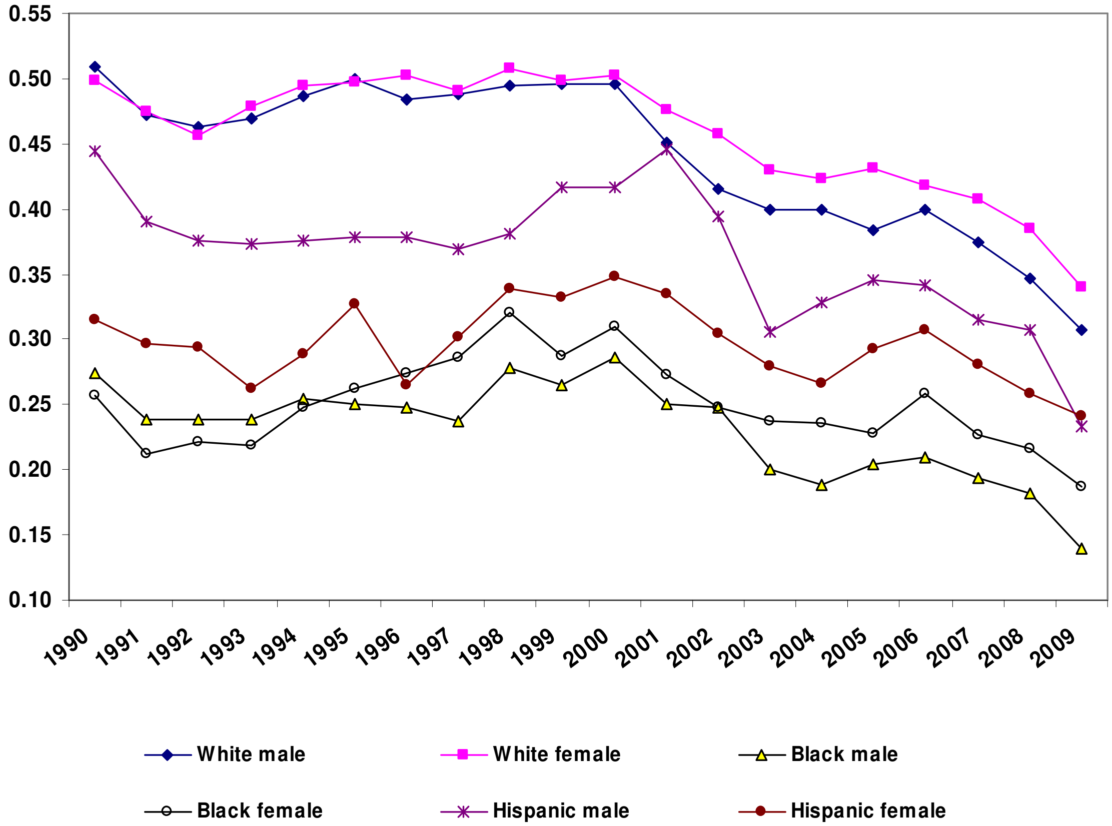
Notes: Authors' analysis of Current Population Survey data. See Table 1 for a listing of states within each Census division.

Figure 2 Time paths of wages, employment and hours in response to a minimum wage change



Notes: Using a distributed lag specification of 2 leads, 4 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 reported in the text. 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% confidence interval around the estimates in dotted lines. The confidence intervals were calculated using robust standard errors clustered at the state level.

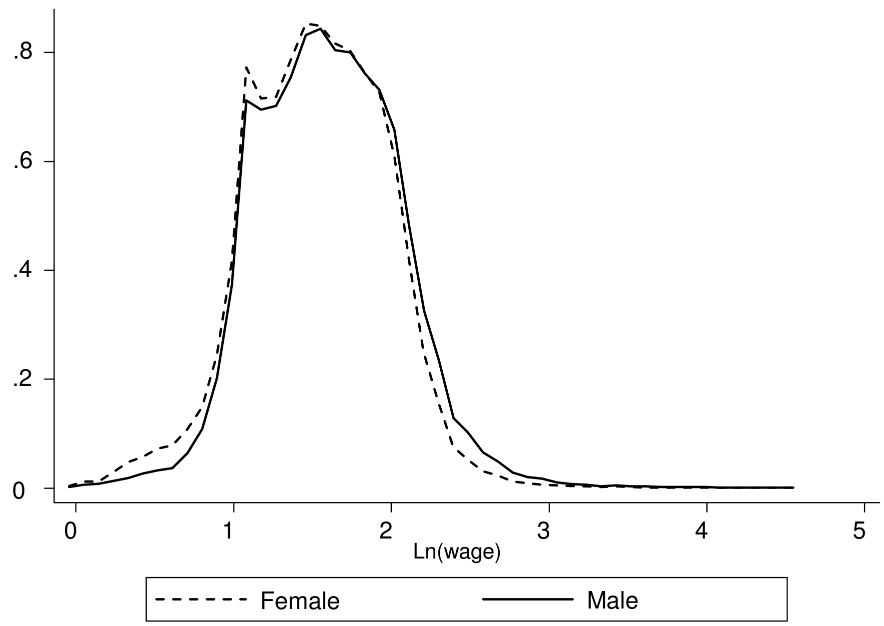
Figure 3 Employment to population ratio for teens, 16-19, by demographic groups, 1990-2009



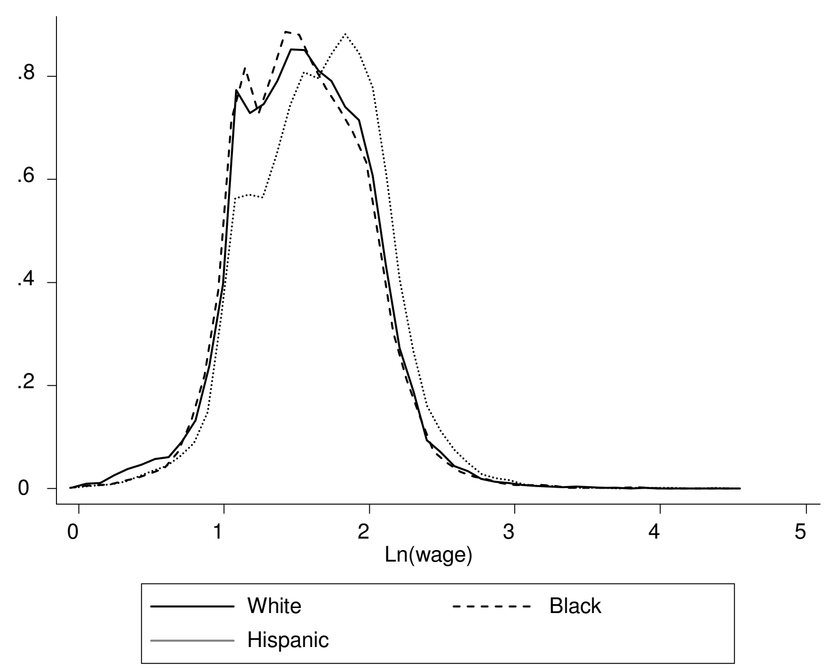
Notes: Authors' analysis of Current Population Survey data. White refers to not Hispanic white.

Figure 4 Kernel wage densities by gender and by race/ethnicity

A: Gender



B: Race/ethnicity



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

Table 1 Employment to population ratios, teens 16-19, by Census division, for selected years

	1990	2000	2009	Change 1990 to 2000	Change 2000 to 2009
United States	0.45	0.45	0.28	0.00	-0.17
New England Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut	0.51	0.51	0.33	-0.01	-0.17
Middle Atlantic New York, New Jersey, Pennsylvania	0.41	0.41	0.26	0.01	-0.15
East North Central Ohio, Indiana, Illinois, Michigan, Wisconsin	0.51	0.52	0.31	0.02	-0.21
West North Central Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas	0.57	0.58	0.42	0.01	-0.16
South Atlantic Delaware, Maryland, DC, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida	0.43	0.43	0.26	0.00	-0.18
East South Central Kentucky, Tennessee, Alabama, Mississippi	0.39	0.42	0.26	0.04	-0.16
West South Central Arkansas, Louisiana, Oklahoma, Texas	0.39	0.42	0.28	0.03	-0.13
Mountain Montana, Idaho, Wyoming, Colorado, New Mexico, Arizona, Utah, Nevada	0.52	0.47	0.30	-0.05	-0.18
Pacific Washington, Oregon, California, Alaska, Hawaii	0.44	0.39	0.23	-0.05	-0.16

Note: Authors' calculations of Current Population Survey data.

Table 2 Descriptive statistics, teens 16-19, 1990-2009 CPS data

	Mean	Std dev	N
Sample Statistics			**447,091**
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			
Employed	0.40	--	178,627
Male	0.40	--	90,284
Female	0.40	--	88,343
White, non-Hispanic	0.46	--	140,636
Black	0.24	--	16,076
Hispanic	0.33	--	21,915
Hourly wage	$5.41	$5.06	180,161
Male	$5.62	$5.60	89,500
Female	$5.20	$4.45	90,661
White, non-Hispanic	$5.36	$4.72	149,054
Black	$5.34	$7.67	13,094
Hispanic	$5.78	$4.71	18,013
Hours worked per week	24.77	12.08	182,730
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	$5.21	1.00	--
Minimum wage (federal binding)	$4.88	0.70	--
Minimum wage (state binding)	$6.14	1.12	--
Unemployment rate	5.15	1.86	--

Notes: Current Population Survey data. Notes: Race groups do not add to total because “other” is not reported. Sample statistics are weighted. Standard deviations reported for continuous variables. Average hourly wage (2009$) 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 usual hours of work.

Table 3 Minimum wage effects on wages, employment and hours worked, teens 16-19, 1990-2009

Specification		(1)	(2)	(3)	(4)
A. Wages					
All	η	0.123***	0.161***	0.165***	0.149***
	se	(0.026)	(0.030)	(0.025)	(0.024)
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					
All	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
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					
All	η	-0.074**	-0.054	-0.001	-0.032
	se	(0.035)	(0.048)	(0.040)	(0.042)
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: η refers to elasticity. Significance levels are ***1%, **5%, *10%. Results are reported for the log minimum wage. Each specification includes individual controls for gender, race (4 categories), age (4 categories), education (12 categories) and marital stats (4 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. Standard errors are clustered at the state level.

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	coeff	-0.049*	-0.017	-0.028	0.011
(4% unemployment)	se	(0.027)	(0.033)	(0.032)	(0.026)
	η	-0.121*	-0.043	-0.071	0.028
Joint minimum wage effect	coeff	-0.046**	-0.010	0.004	0.043
(8% unemployment)	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: Significance levels are ***1%, **5%, *10%. Joint results are reported for the log of the minimum wage and the interaction between the minimum wage and unemployment. Joint effects are evaluated at unemployment rates of 4% and 8%. η refers to the elasticity. Also, see notes to Table 3.

Table 5 Non-indexed minimum wage effects on wage, employment and hours worked, teens 16-19, 1990-2009

Specification		(1)	(2)	(3)	(4)
A. Wages					
All	η	0.116***	0.163***	0.165***	0.152***
	se	(0.027)	(0.032)	(0.027)	(0.025)
Male	η	0.083***	0.129***	0.126***	0.112***
	se	(0.024)	(0.032)	(0.032)	(0.026)
Female	η	0.140***	0.178***	0.200***	0.167***
	se	(0.034)	(0.041)	(0.032)	(0.035)
B. Employment					
All	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
Male	coeff	-0.034	-0.008	0.005	0.017
	se	(0.026)	(0.040)	(0.033)	(0.032)
	η	-0.084	-0.020	0.012	0.042
Female	coeff	-0.051**	-0.018	-0.031	0.002
	se	(0.025)	(0.044)	(0.029)	(0.044)
	η	-0.128**	-0.046	-0.078	0.006
C. Hours					
All	η	-0.088**	-0.080	-0.016	-0.074*
	se	(0.041)	(0.051)	(0.048)	(0.041)
Male	η	-0.064	-0.096	-0.015	-0.098
	se	(0.062)	(0.068)	(0.062)	(0.063)
Female	η	-0.113***	-0.071	-0.022	-0.054
	se	(0.042)	(0.055)	(0.047)	(0.047)
Division-specific time controls			Y		Y
State-specific time trends				Y	Y

Note: see notes to Table 3. Additionally, only observations with non-indexed minimum wages are used in this analysis.

Table 6 Minimum wage effects and indexing on wages, employment, and hours worked, teens 16-19, 1990-2009

Specification			(1)	(2)	(3)	(4)
A. Wage						
All	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						
All	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		coef	-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						
All	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: see Table 3 notes. Additionally, *Index* is a dummy variable that is turned on when indexation begins and it stays on thereafter. *MW*Index* is the interaction of the log of the minimum wage and *Index*.

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. Wages					
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: See Table 3 notes.

Table 8 A comparison of minimum wage employment elasticities

Study	Specification			
	(1)	(2)	(3)	(4)
This study	-0.118***	-0.036	-0.034	0.047
CPS, teens	(0.055)	(0.085)	(0.068)	(0.060)
1990-2009				
90% CI	(-0.208, -0.028)	(-0.176, 0.104)	(-0.145, 0.077)	(-0.052, 0.146)
Dube et al. (2007)	-.207***	-0.076	0.055	0.060
QCEW, restaurants	(0.063)	(0.060)	(0.042)	(0.041)
1990-2006				
90% CI	(-0.312, -0.102)	(-0.176, 0.023)	(-0.014, 0.124)	(-0.007, 0.127)
Neumark & Wascher (2007)	-0.136*		-0.178	
CPS, teens	(na)		(na)	
1997-2005				
Division-specific time controls		Y		Y
State-specific time trends			Y	Y

Notes: Elasticities are not directly comparable. They are presented to show the effects of using similar model specifications and controls. Standard errors in parentheses. Significance levels are ***1%, **5%, *10%.

Appendix Table A-1 Annual minimum wage changes for states that index

Year	State	Indexed amount	Amount to catch Federal MW	% due to index	% 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 in order to match Federal minimum wage laws. *The large percentage increases for Nevada are not due to CPI indexing but due to Federal minimum wage increases as Nevada adjusts the wage by which ever is greater: min{CPI, 3%} *or* Federal Min + $1.