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New Estimates of Minimum Wage Effects After Correcting for Spatial Heterogeneity and Selectivity

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ABSTRACT

Traditional estimates of minimum wage effects on employment include controls for state unemployment rates and state and year fixed-effects. Using Current Population Survey data, we show that such estimates often are biased and that the estimates vary with the source of identifying variation. Without sufficient controls for heterogeneous employment patterns that would occur without minimum wage policies, traditional estimates vary substantially both in sign and magnitude depending on time period and hence with the selectivity of states with minimum wage hikes. Estimates without sufficient controls also vary across demographic groups in a counterintuitive manner that suggests misspecification problems. To account for heterogeneous employment patterns and selectivity among states with minimum wages, we include controls for long-term growth differences among states by using a state-specific linear trend, and controls for heterogeneous responses to economic shocks by including Census division-specific time effects. In the 1990 to 2006 period, including these controls reduces the magnitude of the estimated employment elasticity from -0.168 (significant at the 1 percent level) to -0.024 (not significant). Although the division and state trend controls do not constitute a panacea, they do provide important tools to mitigate the bias that results from unobserved spatial heterogeneities in employment patterns that are correlated with the minimum wage. Since estimates in most previous national-level studies insufficiently address this issue, economists' estimates of minimum wage effects must be revised accordingly.

1. Introduction

Despite a steady stream of studies, research on the effects of minimum wage policies on wages and employment continues to arrive at conflicting findings. One set of results—statistically significant disemployment effects with employment elasticities in a "consensus" range of -0.1 to -0.3—are associated with studies that focus on less-skilled groups such as teens, that use national-level household data (usually from 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]

In an attempt to reconcile these conflicting studies, Dube, Lester and Reich (2007) find that unobserved spatial heterogeneities in employment trends can generate biases toward negative employment elasticities in national studies as well as overstated precision in local studies. They use instead a local estimator that samples all the contiguous border counties in the U.S. that straddle a state minimum wage gradient and they do not find significant disemployment effects. In a recent national study that includes some controls for heterogeneity in teen employment trends, Neumark and Wascher (2007) still obtain a disemployment effect in the canonical range, although the effect is no longer significant at conventional levels. However, Neumark and Wascher consider teens over the period 1997-2005 and Dube, Lester and Reich focus on restaurant employment over the period 1990-2006. The conflicting findings thus may still arise from differences in the groups being examined, differences in the datasets that are used, and whether the source of identifying variation in the minimum wage is coupled with sufficient controls for counterfactual employment growth.

[1] For recent contrasting reviews, see Neumark and Wascher (2006) and Dube, Lester and Reich (2007).

We contribute further to this research by estimating the importance of each of these potential sources of difference. Like most of the research to date, we use CPS data and focus on teens. We also deploy similar methods as Neumark and Wascher (2007), but add more controls for heterogeneity among states, and we also consider a longer time period--1990 to 2006.

Our most important result concerns the confounding effects of heterogeneous patterns in low-wage employment that are coupled with the selectivity of states experiencing minimum wage hikes. The presence of such patterns 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 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] We find that national estimates that include state and time fixed- effects and state-level unemployment rates do not control adequately for such heterogeneous employment trends.

To examine more systematically the importance of spatial and temporal heterogeneity, we begin with the canonical specification of minimum wage effects on teen earnings and employment with national CPS panel data and state and year fixed-effect variables. We then add two controls, separately and together: a) allowing for Census division-specific time effects, which eliminates the variation among 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 among states. With these geographic controls, the estimates change substantially.

We find that adding division-specific time controls and state-specific time trends as controls reduces the estimated employment elasticity from -0.168 (significant at the 1 percent level) to -0.024 (not significant). Our results highlight the importance of estimators that control for geographical proximity as well as other controls that are related to underlying growth prospects. We also test for

[2] For a detailed analysis that arrives at these conclusions, see Congressional Budget Office (2004).

short and long lags in minimum wage effects and we find the same results as before. 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 present further evidence on this point by analyzing the time-path of residual teenage employment around changes in minimum wage, using distributed lags covering 19 quarters around the minimum wage change. We find that when we do not include division-specific time controls and state-specific time trends, the nine quarters *prior* to the actual policy change are all associated with unusually low (and falling) teenage employment. Moreover, inclusion of these controls suggests that there is no visible reduction in employment following the minimum wage hike.

Following Baker et al. (1999), we also examine whether minimum wage effects differ by high versus low frequency variation in the data, which can be thought of as short versus long term effects. Unlike Baker et al., we find that the traditional fixed-effects specification produces negative elasticities at both high and low frequency variation, although the higher frequency component is substantially larger in magnitude. However, the long-term effect becomes positive and insignificant when we include controls for state-level trends and division-specific time effects. Once heterogeneity and selectivity are accounted for, we find no clear evidence of negative employment effects in our data—either short term or long term.

Another of our findings indicates that it is critical to consider the source of minimum wage variation. States affected by federal minimum wages tend to have higher counterfactual growth in low-wage employment, while those affected by state-level increases tend to have lower growth. Between 1990 and 1998, most of the variation in minimum wage changes resulted from federal minimum wage increases. But from 1998 to 2006, all of the variation in minimum wage changes occurred at the state level. Our estimated minimum wage effects differ markedly between these two periods when we do not include controls for state-specific and division-specific time effects. This

result constitutes strong evidence for the presence of heterogeneity in employment patterns and selectivity of states with minimum wage increases. Moreover, considering the late 1990s and 2000s only can be misleading because of the varying impact of the 2001 recession across states.

We show that timing issues also rationalize the Neumark and Wascher findings-- that while including state-level trends leads the employment elasticity to becomes more negative (-0.136 to -0.178), the standard error also increases substantially and hence the coefficient is no longer significant. By focusing just on the 1997-2005 period, Neumark and Wascher's variation is particularly sensitive to selectivity among states. Given the heterogeneous response of teen employment to the 2001 recession, coarse variables such as state-level trends are likely to be relatively poor controls in such short panels. Consistent with this interpretation, we show that their results are very sensitive to shifting their sample by just one year (to 1998-2006). In contrast, when we include state-level trends in the full period (1990-2006), the point estimates go from about -0.168 to close to zero, mirroring the finding in Dube, Lester and Reich.

We also find similar patterns in the analysis of minimum wage effects upon detailed gender, race and ethnicity groups. Using the conventional national estimator with state and time fixed-effects, we obtain significantly positive earnings effects for most groups, but significant disemployment effects only for some, and in a pattern that does not accord well with the relative earnings of the different groups. When our estimators include state-specific linear trends and division-specific time controls, we find that estimated earnings effects are more similar across groups and that employment effects for detailed subgroups among teens are centered again on no effect. However, increases in standard errors in the CPS data suggest that these results are not definitive.

To examine further the robustness of our estimated employment effects on teens using the CPS, we compare our results to the Dube, Lester and Reich (2007 estimates for restaurant workers

using the Quarterly Census of Employment and Wages (QCEW). Since nearly identical proportions of teen workers and restaurant workers are paid within a dollar of the minimum wage, their respective minimum wage elasticities may also be quite similar, provided that relevant product market structure and product demand elasticities are similar as well.

Our estimates for teens using household data indeed are similar to the Dube, Lester and Reich estimates for restaurants using administrative establishment data. This finding holds for the minimum wage elasticity estimates with respect to earnings and employment, and it does not depend on whether we include the state-specific trends and division-specific time controls in our estimates. The similarity of these results suggest that Dube, Lester and Reich's findings of no significant disemployment effects do not result simply from using establishment data or from focusing on a particular industry.

In the next sections of the paper we discuss in turn the related literature, the data, our estimation strategy, our main results, our results for specific gender and race/ethnicity groups, and our comparisons with our study of restaurant employment.

2. Related Literature

For the most part, minimum wage studies using national CPS panel data with state and year fixed-effects find modest but statistically significant negative employment effects on teens, with elasticities that range from -0.1 to -0.3. Sabia (2006) and Neumark and Wascher (2007) are two recent papers in this vein. Sabia (2006) uses grouped CPS data for 1979 to 2004 to study the retail industry only. For teens in retail, Sabia's main specification includes controls for teen shares in the population and fixed state effects; Sabia also adds year effects in a second specification (Sabia 2006, Table 3, columns 4 and 6). This approach is similar to that of Burkhauser et al (2000). Sabia finds significant disemployment elasticities of -0.217 when year effects are excluded and -0.298 when they

are included. Sabia does not, however, control for state-specific time trends. As we shall show below, the robustness of his findings consequently remains open.

Neumark and Wascher (2007) use pooled time-series cross-section individual CPS data for 1997 to 2005. Unlike in their previous minimum wage studies, in this paper Neumark and Wascher include state-specific time trends in one of their specifications and cluster their standard errors at the state level.[3] They find that the existence of state-specific time trends cannot be rejected. Their standard model, without state-specific time trends, estimates a negative employment elasticity of -0.136 among teens, significant at the 10 percent level. When they include state-specific time trends the estimated employment elasticity for teens is somewhat larger (-0.178), but is no longer significant. Neumark and Wascher also estimate specifications with lagged minimum wages included as an explanatory variable and specifications for eleven teen subgroups, disaggregated by gender, race and ethnicity. We discuss these results further in the context of our findings later in the paper.

Neumark and Wascher motivate 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. But a better approach would include the entire period of state minimum wage variation over a longer period using a consistent methodology and test whether minimum wage effects differ in the more recent period. As we report below, we find that their results are driven in large part by the source of the identifying variation in the time period they include.

As mentioned previously, Dube, Lester and Reich (2007) show that previous national minimum wage studies lack adequate controls for spatial heterogeneity in employment growth.[4]

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[3] Neumark (private correspondence) reports that clustering has only small effects on the standard errors, with some increasing and some decreasing.

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

Without such controls, Dube, Lester and Reich find significant disemployment effects within the -0.1 to -0.3 consensus estimates. But once they add such controls, Dube, Lester and Reich find no significant disemployment effects. 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. An important question, which we consider below, is whether Dube, Lester and Reich's results can be replicated beyond the restaurant industry and the QCEW data.

To summarize, a fundamental issue in the minimum wage literature concerns whether localized specifications are superior to national specifications with fixed effects. To address this question, we use the CPS dataset of the previous literature and we incorporate additional spatial and time controls into the national panel specifications.

3. Data

We construct an individual-level repeated cross-section sample from the CPS Outgoing Rotation Groups for the years 1990 to 2006.[5] The CPS data are merged with data that capture aggregate demand and supply variation—monthly state unemployment rates and population shares for demographically conditioned regressions. Additionally, each observation is merged with a quarterly minimum wage variable—whichever is higher of the federal or state minimum.

Table 2 provides descriptive statistics for the sample of teens aged 16 to 19. Non-Hispanic whites account for nearly 71 percent of the sample, while blacks and Hispanics each account for nearly 15 percent. Hourly pay over the sample period averages $6.34. Although male teens are paid more than female teens—$6.61 versus $6.07, pay differentials by race/ethnicity are considerably

[5] We use a start year of 1990 to make the clearest comparison with Dube, Lester and Reich's results with QCEW restaurant data, which are available on a consistent basis beginning in 1990. We have also included earlier years in our CPS sample; the results are not affected. See Section 5.3 below.

smaller. Employment patterns do not correspond to these wage patterns. Over the sample period, 42 percent of all teens 16-19 were employed, with identical percentages for males and females. Black teens had the lowest employment rates—25 percent, followed by Hispanics—34 percent. Finally, state minimum wages averaged about $1 over the federal minimum wage.

4. Estimation Strategy

Our focus is to estimate the effect of minimum wage increases upon wages and employment of teenagers. The dependent variable, *y*, is either the natural log of hourly earnings, or a dichotomous employment measure that takes on the value one if the person is working. 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 (nonseasonally 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.[6] In this canonical 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

[6] The individual characteristics include 2 gender categories, 4 race/ethnicity categories, 12 education categories and 4 marital status categories. These are the same categories used in Neumark and Wascher (2007).

and hence better controls for spatial heterogeneity in differential employment patterns, including region-specific economic shocks:

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

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. Indeed, both Neumark and Wascher (2007) and Dube, Lester and Reich (2007) find that the inclusion of state-specific time trends cannot be statistically rejected. Our third specification includes these controls:

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

where ψ_s denotes the time trend for each state.

Finally, we add both the division-specific time effect and the state-specific time trend controls for our fourth specification. The resulting estimates are less likely to be contaminated with unobservable long term trends and region-specific economic shocks

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

We estimate these four specifications on all teens 16-19 as well as on sub-samples disaggregated by gender, and race/ethnicity (white (non-Hispanic), black or Hispanic, black separately, Hispanic separately). We cluster standard errors at the state level to account for the lack of independence among observations within a state at any given time, which might be caused by serial correlation in employment rates within states over time.

5. Main Results

We discuss first our estimated wage and employment effects for all 16-19 year-olds with each of our four specifications. We then check our results by examining the time path of teenage

employment around the time of minimum wage changes, the possibility of lagged effects, and by disaggregating our results by different time periods.

5.1 Wage and employment effects for all teens

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. But the magnitudes vary among the specifications. In Specification 1, the fixed-effects model, the treatment coefficient is 0.139 for all teens. Adding just the division controls (Specification 2) modestly increases the magnitude of the treatment coefficient for all teens to 0.166, similar to that in Specification 1. Adding the state-specific time trends, without division controls (Specification 3) increases the magnitude of the wage elasticity to 0.240. When both state- and division-specific time trends are included (Specification 4), the treatment effect for all teens is 0.168 and the effect 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 from the 2006 CPS, we found that 26.4 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 0.168 is consistent with the distribution of pay at or near the minimum wage.

We turn next to the employment effects, reported in Table 3, Panel B. Specification 1 shows a significant negative employment coefficient of -0.070. The corresponding employment elasticity is

-0.168, which is consistent with the literature that uses the canonical fixed-effects model.[7] In Specification 2, however, allowing for division-specific time effects reduces the elasticity to -0.103 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 -0.025 and it is not significant. Finally, in Specification 4, the employment elasticity is -0.010 and remains insignificant. In other words, allowing for variation in employment trends over the 1990 to 2006 period produces minimum wage effects on employment that are indistinguishable from zero.

These results indicate that estimates of minimum wage employment effects using the basic 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 substantially. Allowing for long-term differential state trends makes the employment estimates indistinguishable from zero.[8]

5.2 *Lagged effects and high and low frequency components of minimum wage variation*

Although Neumark and Wascher (2007) allow for and sometimes find one-year lagged employment effects of minimum wages, other researchers (Dube, Naidu and Reich, 2007; Dube, Lester and Reich, 2007) do not detect lagged effects. Since the median employment duration of most minimum wage jobs is less than six months, it is not clear why substantial employer adjustments would be spread over a year or more. Nonetheless, we check whether our results hold when we allow minimum wages to affect employment with a lag. To do so, we substitute in each of the four specifications a one-year lagged minimum wage for the minimum wage variable.

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

[8] We discuss in Section 6 below our earnings and employment estimates for gender and race/ethnicity groups.

The results are displayed in Table 4, for elasticities only. The lagged minimum wage employment elasticity is negative (-0.098) and significant in Specification 1, but it becomes much smaller (-0.029) and insignificant when the division-specific time controls are incorporated in Specification 2. When we include state-specific time trends (Specification 3) the employment elasticity becomes positive, but it is not significant. This finding does not change when we also add divisional controls (Specification 4).

When we include both contemporary and lagged minimum wage variables in Specification 1, the contemporaneous effect is large (-0.243) and highly significant, while the lagged effect is of opposite sign (0.107) and not significant. With the addition of division-specific trend controls (Specification 2), both effects become smaller in absolute value and both are now statistically insignificant. When we include the lagged and the contemporary minimum wage variables in Specification 3 (with state-specific time trends), the elasticities have opposite signs (-0.114 and 0.131, respectively) and both are significant; the sum of the two elasticities, however, is not significant.[9] When we also include division-specific time controls (Specification 4)—each coefficient becomes smaller in absolute value and both are insignificant. In summary, we do not find that minimum wages create disemployment effects either contemporaneously or with a one year lag.

To explore the presence of lags further, Figure 2 displays time paths of our estimated wage and employment effects. The left-hand column displays results for our specification 1, while the right-hand column present results for Specification 4. The wage graphs both show a clear increase around the time of the change. But including the controls (Specification 4) generates a cleaner "treatment" effect, which we interpret as reinforcing the validity of including additional controls.

The employment effects also differ between the two columns. Specification 1 (without the state-specific-linear-trends and division-time effects) shows negative employment effects throughout

[9] As determined by a joint significance test.

the 19 quarter window, including prior to the minimum wage increase. In contrast, Specification 4 (with both added controls) show relatively more stable coefficients (closer to zero) prior to the increase, and no clear effect on employment subsequently.

To examine the possibility of effects with even longer lags, we also estimate effects for low-frequency (or long-term) and high-frequency (or short-term) components of minimum wage variation. In their study of minimum wage effects in Canadian provinces over the period 1975-93, Baker et al (1999) find the high-frequency component is indistinguishable from zero, while the high-frequency component is negative and significant. Using the same method for U.S. states and counties for 1990-2006, Dube, Lester and Reich find that minimum wage effects are not associated with any frequency component of minimum wage variation.

As Baker et al (1999) point out, and as is discussed in detail in Dube, Lester and Reich, the inclusion of both contemporaneous and one-year lagged minimum wage allows one to estimate the effect of high and low-frequency component of minimum wage changes. The impact of a minimum wage change at the low-frequency component is recovered by summing the contemporary and one-year lagged effects; it captures the effect of minimum wage variation at cycles of four years or longer. In contrast, the high-frequency component is equivalent to the difference between the contemporary and the one-year lagged effects. In their study of minimum wage effects in Canadian provinces over the period 1975-93, Baker et al (1999) find that the high-frequency component is positive or indistinguishable from zero, while the high-frequency component is negative and significant. Using the same method for U.S. states and counties for 1990-2006, Dube, Lester and Reich find that minimum wage effects for restaurant workers are not associated with any frequency

component of minimum wage variation, and both components tend to zero when controls for local heterogeneity are included.[10]

Panel C of Table 4 reports the effects for low-frequency (or long-term) and high-frequency (or short-term) components of minimum wage variation. Specification 1 suggests negative elasticities for both the high and low frequency components, with a substantially larger effect at the higher frequency. Taken at face value, this result suggests that a large disemployment effect occurs in the short run, and smaller (but still substantial) negative effects occur over the long run. (This is quite different from Baker et al., who find the high frequency component to be positive or zero.) Alternatively, this result suggests that the apparent disemployment effect associated with the high-frequency component results from the correlation of minimum wage changes and local economic shocks. And the (smaller) negative effect associated with the low-frequency component may reflect long-term differences among states with different minimum wages.

Consistent with this interpretation, we find that adding division-specific controls (Specification 2) results in smaller and insignificant effects at both high and low frequency components. This result is consistent with the presence of local shocks, which are partly controlled for by focusing only on within-division variation. In Specification 3, adding only the state-specific linear trend eliminates the low-frequency component, indicating there is no measurable long-term effect on employment once long-term trend differences between states are accounted for. In Specification 4, after adding both the division-specific time controls and the state-specific linear trend controls, the magnitude of the high-frequency component is much reduced in magnitude and it

[10] In an analysis that is similar in spirit to ours, MacDonald and Myatt (2004) call into question whether the Baker et al findings are robust to controls for heterogeneity across Canadian provinces. Using seven-year rolling sample periods, they show that the disemployment coefficients are correlated with cyclical unemployment. They also conduct province-by-province regressions and show that the employment effects vary from positive to negative.

is no longer significant. Now, however, the long-term effect (associated with the low-frequency component) is essentially zero. In summary, the long term effects are quite similar to those that estimate just the contemporaneous effect, and once we control for heterogeneity, these effects are indistinguishable from zero.

To explore further the timing of minimum wage effects in our data, we estimate a distributed lag model with leads and lags in minimum wage covering a 19 quarter window. Specifically, we include leads and lags in minimum wages starting at 9 quarters before the minimum wage change and continuing to 9 quarters after the change, where each lead and lag is a centered average of 3 quarters.[11] Figure 2 displays time paths of our estimated wage and employment effects. 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. The wage graphs both show a clear increase around the time of the change. But including the controls (Specification 4) generates a cleaner "treatment" effect, which we interpret as reinforcing the validity of including additional controls.

The time paths for employment provide strong evidence against the canonical model without controls for heterogeneity across states (i.e., specification 1). Specification 1 shows negative (and falling) employment effects throughout the 19 quarter window, including prior to the minimum wage increase. This results provides strong evidence that minimum wage changes are occurring at times of unusually low teen employment. Consistent with this interpretation, Specification 4 (with both added controls) shows relatively more stable coefficients (closer to zero) prior to the minimum wage increase and no clear effect on employment subsequently. Overall, the time paths provide further

[11] For example, "lead 9" is the average minimum wage during the 10^{th}, 9^{th} and 8^{th} quarter *after* the minimum wage increase. The coefficient associated with "lead 9" then reflects the average residual teen employment during the 10^{th}, 9^{th} and 8^{th} quarter *before* the minimum wage increase. We employ the 3 quarter smoothing to reduce the substantial quarter-to-quarter variation in the data. Unsmoothed estimates produce similar findings, but the pattern is somewhat obscured by the quarter to quarter variation.

evidence that, in the period under consideration, failure to control for heterogeneity in employment patterns leads to a negative bias in the estimated employment response from minimum wage changes.

5.3 Differences by time period

Our evidence thus far suggests that selectivity of the states with minimum wage increases and heterogeneity of employment patterns across states produce biased estimates for specifications that do not account for such heterogeneity. In this section, we provide further evidence on this issue by looking at different sub-periods in the data—specifically, the 1990-1998 period versus the 1998-2006 period. The two periods constitute different sources of identifying variation. While most of the changes in the first period occurred through federal changes, in the entire latter period minimum wage increases occurred only at the state level. To the extent that states enacting local minimum wage increases are systematically different from those that do not, the two periods should provide different estimates when controls for such heterogeneity are not included. Considering different periods also permits a direct comparison with the sample years in Neumark and Wascher (2007), who looks at the 1997-2005 period. We can then account more clearly for the source of the difference between their findings and ours.

Table 5 reproduces from Table 3 our estimates for our four specifications for our sample period of 1990-2006, and reports as well our estimates first for a longer sample period, 1987-2006, and then for two sub-periods, 1990-1998 and 1998-2006. The table also includes our results and those of Neumark and Wascher (2007) for their sample years, 1997 to 2005.

We report results from 1987 on because state minimum wages were first implemented then in three states (Alaska, Connecticut and Massachusetts). By 1989 fifteen states and the District of Columbia had increased their minimum wage standards above the federal level. Comparing the first

two rows in Table 5 shows that starting the sample in 1987 provides estimates that are essentially identical as starting the sample in 1990.

We next examine the effect of splitting our sample into two time periods of equal duration. For the earlier period (1990-1998), the employment effect is not significant in any of the specifications. For the later period (1998- 2006), specifications without both state trends and division-specific time effects (i.e., Specifications 1-3) suggest disemployment effects. These findings are similar to the findings in Neumark and Wascher (2007). However, the estimates using Specification 4 (with the full set of controls) suggest small and statistically insignificant employment effects.

As we mentioned above, the source of identifying variation is quite different in the two periods. Most minimum wage changes in the earlier period were federal increases, while all of the increases in the later period occurred at the state level. Moreover, the states enacting these policies were not randomly distributed among all states, as they were usually coastal states and slower-growing states.[12]

Equally important, as Figure 1 and Table 1 indicate, the teenage employment rate experienced a large negative shock with the 2001 recession. In every one of the nine Census divisions, teen employment-population ratios showed no long-run trend in the period from 1990 to 1998. The ratios declined subsequently, beginning in 1998 in some divisions and in 2000 in others. Moreover, the magnitude of the decline differed among the divisions and teen employment rates did not rebound in most of the U.S. through the course of the subsequent recovery.

Putting these two facts together suggests that focusing only on the latter period, and doing so *without* sufficient controls for heterogeneity in employment trends, is likely to produce very

[12] This pattern began to change by 2006, when a larger set of states implemented minimum wage laws. Dube, Lester and Reich (2007) provide a detailed discussion of the pattern of state minimum wage increases from 1987 to 2007.

misleading results. Consistent with this evidence, we find that the results from our fixed-effects specification (Specification 1) are very sensitive to the period under consideration, while the specification with the richest controls (Specification 4) is quite stable, with an employment elasticity close to zero. Moreover, as we have already seen, for the full 1990-2006 period, including either state trends (Specification 2) or focusing on within-division variation (Specification 3) produced a small elasticity. In the 1998-2006 period, as Figure 1 suggests, spatial and temporal heterogeneity were much more complex.

The lack of sufficient controls for this heterogeneity consequently is particularly severe if we limit our attention to the 1997 to 2005 time period, as do Neumark and Wascher (2007). As Table 5 shows, even a one-year change in the start and ending dates, to 1998 to 2006, generates somewhat different results.[13]

6. Minimum wage effects by gender, race and ethnicity

Figure 3 displays employment rates among teens by gender, race and ethnicity over the period 1990-2006. 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, indicating that minimum wage policies may have had less negative effects among female teens than among male teens. 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

[13] In Table 5 our results for 1997 to 2005 show somewhat smaller employments effects compared to Neumark and Wascher's estimates for the same years and the same specification. The individual control variables are also the same, but the samples are slightly different. Neumark and Wascher exclude all observations with imputed values for wages or earnings or employment, while we include such observations. Although the proportion of observations with missing values on one of these variables has risen to more than 30 percent, over four-fifths of these missing values are for wages or earnings. They do not need to be excluded from an employment regression.

group may be affected by inclusion of controls for spatial heterogeneity. Third, minority employment rates are much more affected than are whites by the business cycle, in both prosperity and recession, indicating that spatial heterogeneity of business cycles coupled with selectivity of states with minimum wage increases may be important in estimating minimum wage effects for these minority groups.

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, then, 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. In this perspective, higher pay 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 and gender. We close this section with a summary of our findings for eleven demographic subgroups.

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 4 and the descriptive sample statistics in Table 2 above present a similar picture. Average wages in the sample are $6.61 for male teens and $6.07 for female teens and the average employment to population ratio is identical

for both. These patterns suggest that minimum wage changes should have similar effects on male and female teens.

Table 3 reports our estimated wage and employment elasticities by gender. In Specification 1 the wage elasticity is 0.109 for male teens and 0.160 for female teens, indicating a much greater (about 54 percent larger) treatment effect among female teens. In Specification 2, with division-specific time controls included, the male teen wage elasticity increases (to 0.153) and it is much closer to the female wage elasticity (0.167). In Specification 3, with just the state-specific trend control included, the estimated male teen wage elasticity increases again (to 0.208) and the female teen wage elasticity is 0.271, implying a greater effect on female teens-- about 30 percent greater. Finally, for Specification 4, with both controls included, the estimated male teen wage elasticity is 0.172 and the female teen wage elasticity is 0.148.

In summary, without controls (Specification 1), the gender differences are much greater than one would expect. When both sets of controls are added (Specification 4) to account for spatial heterogeneity, the treatment effect on earnings remains substantial and significant for both male and female teens. Moreover, with the full set of controls the gender differences are somewhat smaller.

We turn next to gender patterns in the estimated employment elasticities. In each of the four specifications the employment effects for all teens are indistinguishable from those for male and female teens separately.[14] But while Specification 1 estimates significant disemployment effects for both male and female teens, Specification 4 shows no significant employment effects for either male or female teens. These results support the view that gender is not an important distinction in the teen labor market and they reinforce our previous finding that controlling for heterogeneity in employment patterns is crucial in estimating minimum wage effects.

6.2 Earnings and employment effects by race/ethnicity and gender

[14] As we indicate below, however, the gender patterns are not similar for specific race and ethnicity groups.

In contrast to the declining significance of gender in the teen labor market, labor market outcomes for black and Hispanic teens continue to be inferior to those for white, non-Hispanic teens. As Table 1 indicates, during our sample period the employment rate averages .25 for black teens and .34 for Hispanic teens, compared to .47 for white non-Hispanic teens.[15] Moreover, as Figure 4 shows, the employment rates of black and Hispanic teens have dropped sharply since the 2001 recession. As we mentioned previously, the poorer outcomes for minority teens may reflect their more limited skills or experience, relative to white non-Hispanic teens. If, moreover, minimum wage effects lead to substitution toward more skilled and experienced workers, then minimum wage policies may have more harmful effects on the employment on disadvantaged groups.

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 teens will be affected by the policies.

Table 6 reports our estimated treatment effects on wages for separate race/ethnicity and gender groups. For the white, non-Hispanic 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 the white, non-Hispanic group accounts for about 71 percent of the total teen sample. This same pattern appears for males and females in this group. Comparing across the specifications, the wage effects are somewhat higher with controls than

[15] Wage rates do not show such a disparity, however. Black teens have the same wage as non-Hispanic white teens ($6.32), while Hispanic teen wages are higher ($6.54).

without, although more so for males than for females. Including the controls makes the treatment effects essentially the same for males and female teens in this group.

In summary, whether or not we include controls for spatial heterogeneity, we find a substantial and significant treatment effect for white non-Hispanics. However, the treatment effects become much more similar by gender with the controls than without them. Since the underlying gender-based labor market differences are small, this result provides some support for the use of the controls.

The treatment effect on wages varies much more among black and Hispanic teens. Among all black teens, in Specification 1 the wage effect is positive, but the separate estimates by gender indicate that significant wage effects are concentrated only among black males. Controlling for division-specific trends (Specification 2) reduces the treatment effect among black teens and renders it insignificant. Controlling for state-specific time trends (Specification 3), the treatment effect for black teens becomes large and significant and similar in magnitude to that for all teens. The treatment effect on black teens in Specification 4 is significant and it is higher than among all teens (0.232 versus 0.168) and it is almost twice as high in Specification 4 as in Specification 1 (0.232 versus 0.124).

Substantial gender differences among black teens also appear in Specification 4. For black males the minimum wage elasticity is 0.148, similar to that for all teens, but the standard errors are now four times as high as for the white, non-Hispanic estimates. For black females the wage elasticity is 0.291 and significant (at the 10 percent level). Here too the standard errors for black female teens are four times higher than for all teens, suggesting caution in making inferences. Nonetheless, the results suggest that adding controls increases the magnitude and significance of the treatment effect on black teens.

Among Hispanic teens, the magnitude and the statistical significance of the wage elasticity fall considerably from Specification 1 to Specification 4 (from 0.148 and 1 percent significance to 0.110 and ten percent significance). As with black teens, gender differences among Hispanic teens are present in three of the four specifications. In Specification 1, Hispanic female teens show a large and significant treatment effect (0.234 and 1 percent significance level), but Hispanic male teens do not (0.084 and not significant). In Specification 4, the treatment effect for Hispanic females is still positive (0.132) but it is not significant; the treatment effect for Hispanic males is also indistinguishable from zero. Again, however, the large increase in the standard errors means we cannot make any strong inferences.

We turn next to the employment elasticities by race/ethnicity and gender, which are reported in Table 7. In the white, non-Hispanic group of teens, the employment effect is negative and significant under Specification 1, but it is not significantly different from zero in Specifications 2 to 4. These results mirror those for all teens.

Among black teens, the employment elasticity changes from negative but not significant in Specification 1 to *positive* (0.288) and significant at the ten percent level in Specification 4. While the estimates are similar for black males and females in Specification 1, in Specification 4 the gender differences become very strong. Among black males, the estimated employment effect is not significant under any of the four specifications. For black females, in contrast, the employment effect is not significant under Specification 1 but becomes positive, significant and vary large—the estimated elasticity is 0.591—in Specification 4. This result is surprising, given that the treatment effect on earnings in Specification 1 in Table 6 is significant for black males but not for black females.[16]

[16] In Table 7, the standard errors among black teens do not increase, as they do in the corresponding cells of Table 6.

Among all Hispanic teens, and among Hispanic female teens, the employment effect is not significant in any of the four specifications. But among Hispanic males, a significant negative employment elasticity (-0.253) appears in Specification 1, even though the corresponding treatment effect was insignificant for this group. However, when additional controls are included, the employment effect for Hispanic male teens becomes insignificant.

To summarize, without controls beyond those in the traditional specifications, the estimates indicate negative and significant employment effects for white, non-Hispanic and for Hispanic males, but not for blacks or for Hispanic females. Yet the estimated wage elasticities without controls (Specification 1) reported in Table 6 were positive among black males but not black females, and positive for Hispanic females but not Hispanic males. The estimated employment effects thus do not accord with the corresponding Specification 1 estimated wage elasticities for these groups.

With spatial trend controls added, we find that employment effects are insignificant for most of these demographic groups, except for black females. We do find a significant and *positive* employment effect for black female teens. Equally important, we find no clear association between the extent of wage gains by different groups and the measured employment effects. However, for some of these detailed groups, the standard errors are substantially higher than among all teens, suggesting caution for interpreting the results.

It is therefore instructive to compare our results for detailed demographic groups to those in Neumark and Wascher (2007). Neumark and Wascher estimate specifications for each of 11 teen demographic groups.[17] As is the case for all teens, they estimate four different specifications. Their Specification 1 includes fixed effect controls for state and time (each observation is associated with a state, year and month). Their Specification 2 adds state-specific time trends. Their Specification 3 is

[17] These comprise 5 teen groups for males and females combined, and another 6 groups separately by gender—3 for males and 3 for females.

identical to 2, except that the minimum wage is lagged one year. Their preferred model is Specification 4, with both contemporaneous and lagged minimum wages, although they do not actually find much in the way of lagged employment effects.

We display in Table 8 Neumark and Wascher's results for each of the 11 groups of teens. Among the 11 teen demographic groups, the effect on wages is positive for 8 groups and negative for 3; the estimated elasticities range considerably, from -0.369 to 0.386. Of the 8 wage elasticities that are positive, 6 are statistically significant. Turning to their employment effects among the 11 groups, 9 of Neumark and Wascher's estimated employment elasticities are negative and 2 are positive, but only one of the 11 employment elasticities is statistically significant: -0.664, for the black or Hispanic group. For this group, however, the wage elasticity is negative (-0.093), but not significant, indicating that they find disemployment effects only for a group for which they do not observe a minimum wage treatment effect.

Table 8 also includes results for our Specification 4 for the same 11 teen groups. In our results the wage elasticities are all positive and 10 out of 11 are significant; moreover, with the controls added the wage elasticity point estimates exhibit a narrower range, from 0.110 to 0.232. Our estimated employment effects are insignificant for 9 of the groups and positive and significant for only two—black females and black or Hispanic females—for which we also observe a positive and significant treatment effect.

To summarize the findings in both studies, Figure 4 provides a scatter plot of the data in Table 8 as well as the unweighted trend line for each set of findings. In the Neumark and Wascher results, a larger wage elasticity is associated with a smaller disemployment effect, contrary to the expected relationship. This anomaly casts further doubt on whether the Neumark and Wascher (2007) estimates pick up a true disemployment effect. For our results, the scatter plot shows that employment effects are bunched near zero, whatever the wage elasticity.

7. Comparisons with restaurant studies

We examine in this section 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- and state-specific time trends in our study are similar to those used in Dube, Lester and Reich (2007), but as already mentioned the data and the group that is studied are different. Table 9 provides results from two similar specifications across the two studies. The Specification 1 results are similar—elasticities in both studies are in the typical range of a 1 to 3 percent disemployment effect from a 10 percent increase in the minimum wage, and both sets of estimates are significant at the 1 percent level. But when we include the division control (Specification 2), in both studies the disemployment effect is reduced substantially and it is 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). The same is true with division-specific and state-specific time controls included in Specification 4.

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 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 very similar, it is not surprising that the estimated effects are also similar. Differences in findings are 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 -0.17, similar to the -0.3 to -0.1 percent disemployment consensus of the estimates in other national CPS studies. But including a Census division-specific time control to the specification reduces this effect to -0.10 and it is no longer distinguishable from zero. Adding a state-specific time control also reduces the employment effect and renders it insignificant. The inclusion of division and state-specific time controls thus greatly affects the economic and statistical significance of the estimates. Our plot of the time path of teen employment around the minimum wage change using only the canonical time and state controls indicates that teen employment was unusually low and falling substantially *prior* to the actual increase. 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.

We also find that traditional fixed-effects estimates are sensitive to the demographic group under study and to the sample time frame, but they become much more similar when richer controls are included in the specifications. Our separate examination of the period from 1998 to 2006--dates similar to those in Neumark and Wascher (2007)-- raises serious concerns regarding the source of identifying variation over this period, especially in light of the selectivity of the states and the heterogeneous impacts of the 2001 recession. Absent a rich set of controls for counterfactual

employment, we find that such identifying variation can produce misleading results.

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 effects are very close in both studies. 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 national-level 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 must be revised accordingly.

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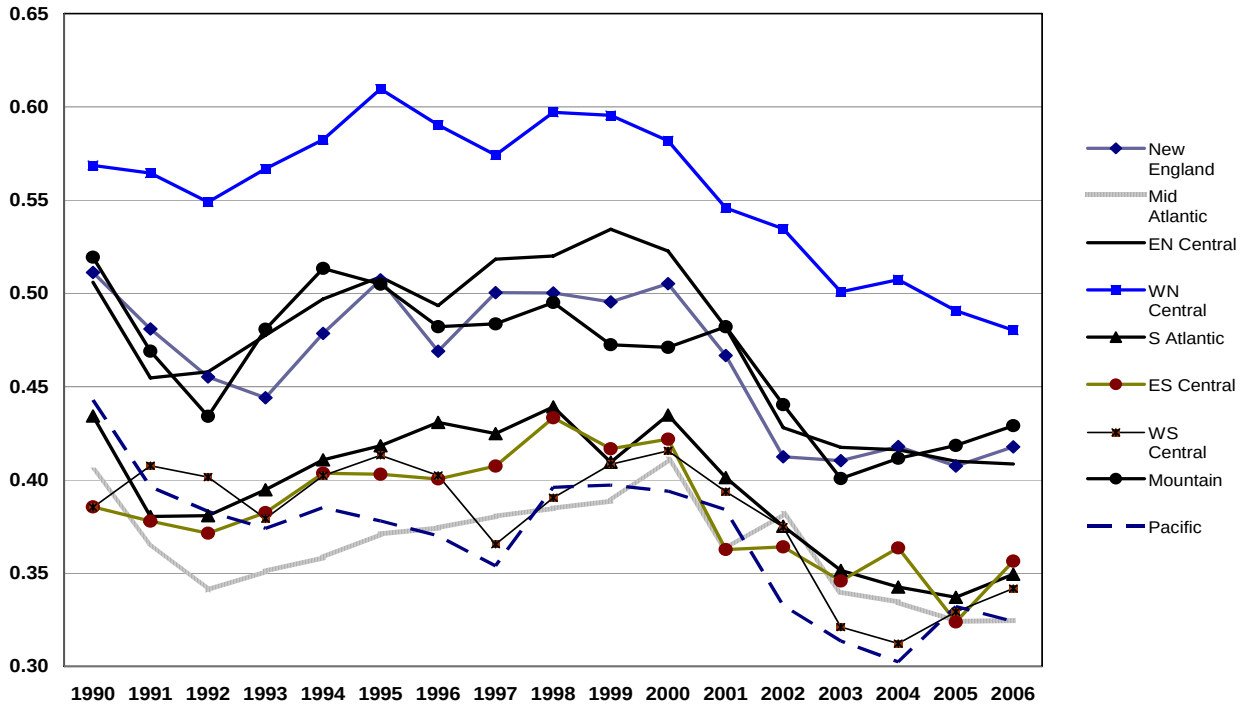
Figure 1 Employment to population ratio for teens, 16-19, by nine Census divisions, 1990-2006

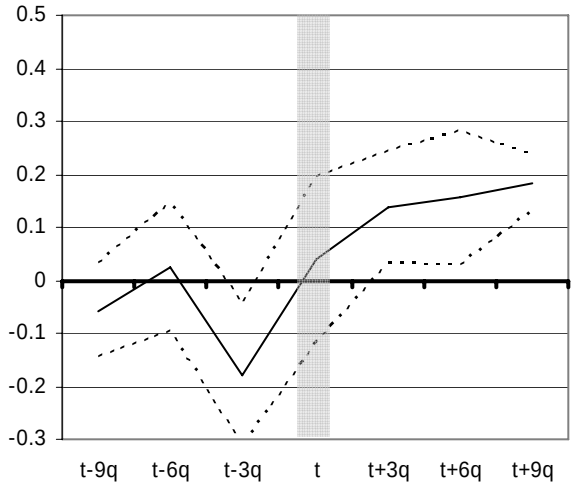


Note: Authors' analysis of Current Population Survey data.

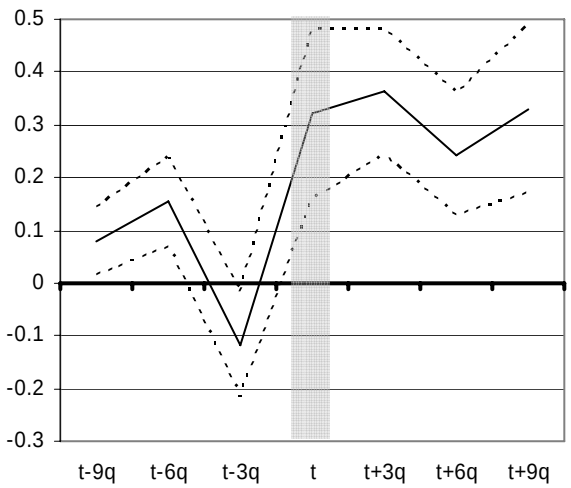
Figure 2 Time paths of wage and employment to minimum wage change

A Log Wage

Spec 1 (No additional controls)

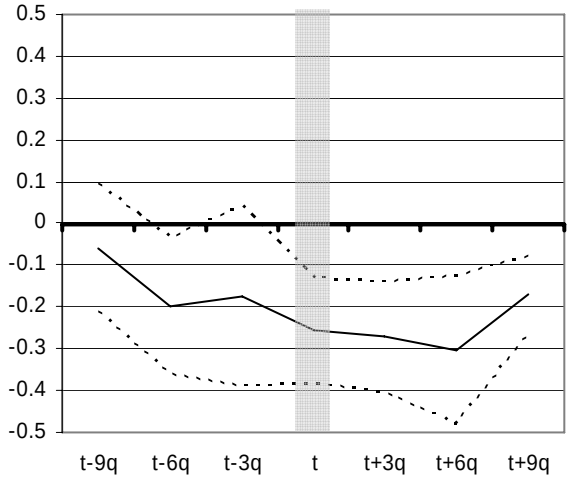


Spec 4 (State-linear trends and division-specific time effects)

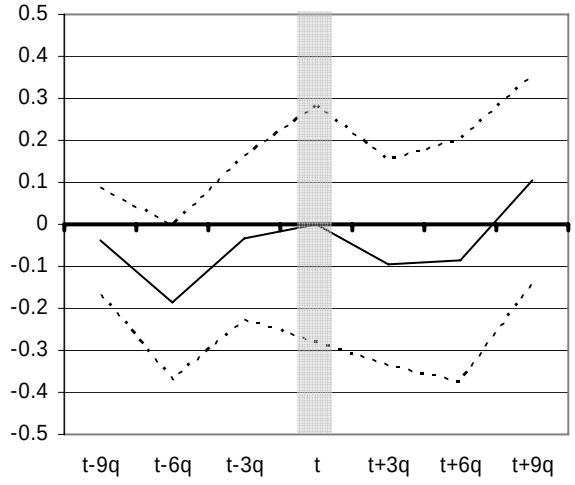


B Employment

Spec 1 (No additional controls)

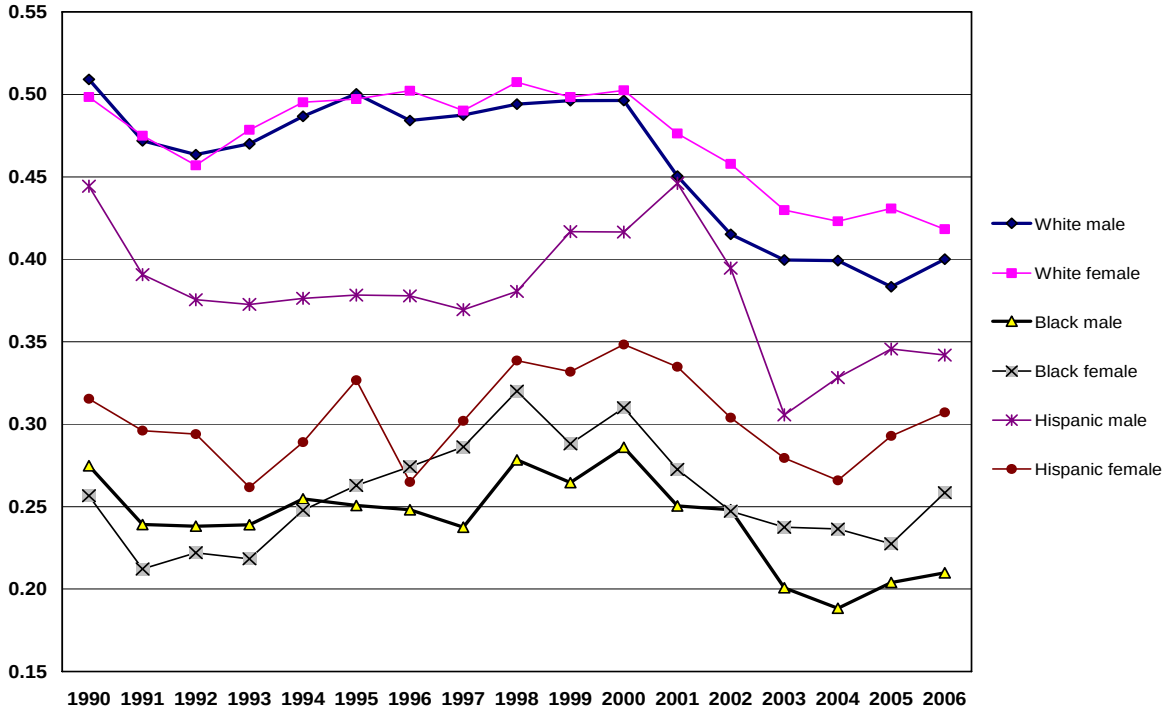


Spec 4 (State-linear trends and division-specific time effects)



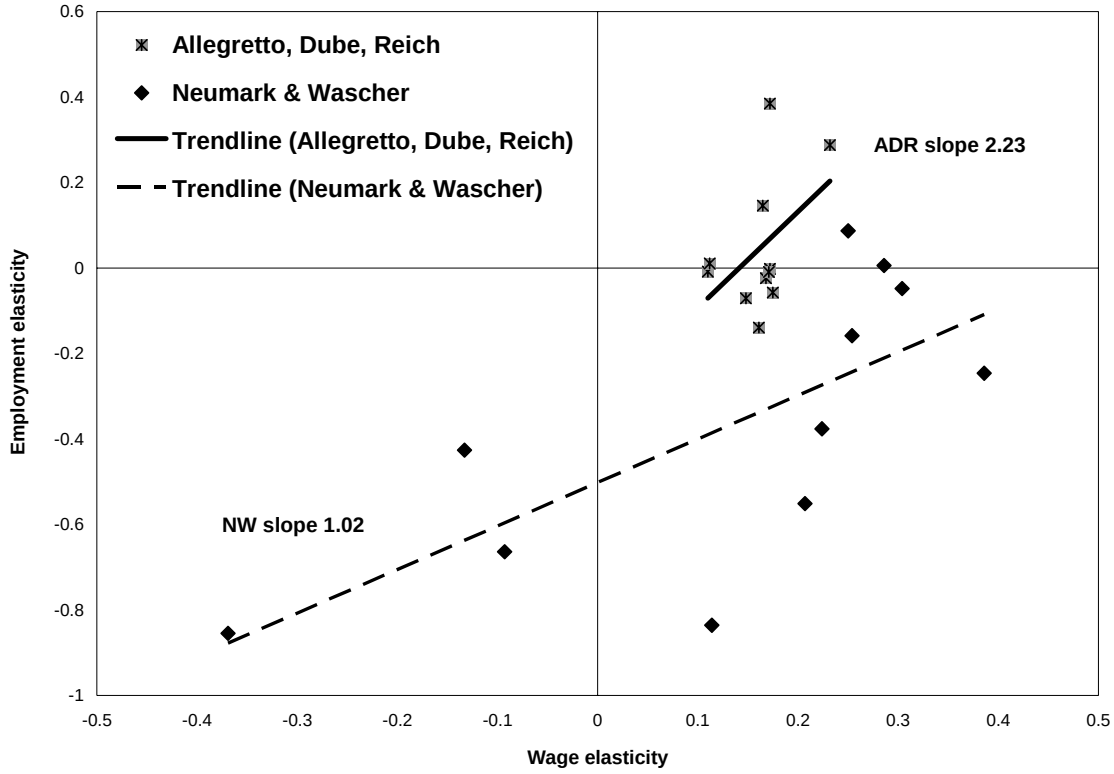
Notes: Using a distributed lag specification of three leads and three lags, the figures above plot the *cumulative response* of employment and wage to a minimum wage increase. The time paths should be read as the cumulative response of log wage or employment to a one-time one-log-point increase in the minimum wage at time t. Here time is incremented in 3 quarter intervals, so t is a 3 quarter period centered around the quarter of the minimum wage increase; t-3q is the last period before the implementation period t; t+3q is the period between the 2nd and 4th quarter after increase. The effect at time t is the sum of all the coefficients in distributed lag structure up to time t. 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, 1990-2006



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

Figure 4 Scatter plot of wage and employment elasticities, this study and Neumark and Wascher 2007



Note: Allegretto, Dube, Reich results are for teens, 16-19 for the years 1990-2006. Neumark and Wascher (2007) elasticities are from Table 5, for the years 1997-2005. Data for this scatter plot are in Table 8.

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

	1990	1998	2006	Change 1990 to 1998	Change 1998 to 2006
United States	0.45	0.45	0.40	0.00	-0.08
New England Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut	0.51	0.50	0.42	-0.01	-0.08
Middle Atlantic New York, New Jersey, Pennsylvania	0.41	0.39	0.33	-0.02	-0.06
East North Central Ohio, Indiana, Illinois, Michigan, Wisconsin	0.51	0.52	0.41	0.01	-0.11
West North Central Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas	0.57	0.60	0.48	0.03	-0.12
South Atlantic Delaware, Maryland, DC, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida	0.43	0.44	0.35	0.01	-0.09
East South Central Kentucky, Tennessee, Alabama, Mississippi	0.39	0.43	0.36	0.05	-0.08
West South Central Arkansas, Louisiana, Oklahoma, Texas	0.39	0.39	0.34	0.01	-0.05
Mountain Montana, Idaho, Wyoming, Colorado, New Mexico, Arizona, Utah, Nevada	0.52	0.50	0.43	-0.02	-0.07
Pacific Washington, Oregon, California, Alaska, Hawaii	0.44	0.40	0.32	-0.05	-0.07

Source: Authors' calculations from Current Population Survey data.

Table 2 Descriptive statistics, teens 16-19, 1990-2006

	Mean	Std dev	N
Sample statistics			378,862
Male	0.51	--	191,649
White, non-Hispanic	*0.36*	--	*145,344*
Black	*0.07*	--	*23,215*
Hispanic	*0.08*	--	*23,090*
Female	0.49	--	187,213
White, non-Hispanic	*0.35*		*141,689*
Black	*0.08*		*23,871*
Hispanic	*0.07*		*21,653*
Labor market outcomes			
Employed	0.42	--	161,253
Male	0.42	--	81,038
Female	0.42	--	80,215
White, non-Hispanic	0.47	--	134,478
Black	0.25	--	11,587
Hispanic	0.34	--	15,188
Hourly wage	6.34	6.59	157,107
Male	6.61	7.29	78,279
Female	6.07	5.79	78,828
White, non-Hispanic	6.32	6.02	130,770
Black	6.32	11.19	11,441
Hispanic	6.54	5.39	14,896
Policy variables			
Minimum wage	4.96	0.77	--
Minimum wage (state above federal)	5.99	0.89	--
Unemployment rate	5.47	1.46	--

Source: CPS data. White includes Asian-American. Weighted statistics. 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.

Table 3 Minimum wage effects on wages and employment, teens 16-19, 1990-2006

Specification		(1)	(2)	(3)	(4)
A Wages					
All	η	0.139***	0.166***	0.240***	0.168***
	se	(0.027)	(0.035)	(0.040)	(0.029)
Males	η	0.109***	0.153***	0.208***	0.172***
	se	(0.027)	(0.033)	(0.048)	(0.035)
Females	η	0.160***	0.167***	0.271***	0.148***
	se	(0.033)	(0.044)	(0.047)	(0.038)
B Employment					
All	coeff	-0.070***	-0.043	-0.010	-0.010
	se	(0.020)	(0.043)	(0.029)	(0.038)
	η	-0.168***	-0.103	-0.025	-0.024
Males	coeff	-0.066***	-0.030	-0.003	-0.001
	se	(0.023)	(0.053)	(0.043)	(0.056)
	η	-0.158***	-0.071	-0.008	-0.002
Females	coeff	-0.082***	-0.067	-0.024	-0.030
	se	(0.024)	(0.047)	(0.025)	(0.047)
	η	-0.198***	-0.161	-0.058	-0.071
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 of the minimum wage. Each specification includes individual controls for gender, race (4 categories), age (4 categories), education (12 categories), and marital status (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 were working and paid between $1 and $100 per hour in 1990 dollars. Standard errors are clustered at the state level.

Table 4 Lagged effects of minimum wage effects on employment, teens 16-19, 1990-2006

Specification		(1)	(2)	(3)	(4)
A Lagged minimum wage included only					
Lag MW	η	-0.098*	-0.029	0.057	0.038
	se	(0.055)	(0.081)	(0.055)	(0.076)
B Minimum wage and lagged minimum wage included					
MW	η	-0.243***	-0.148	-0.114*	-0.064
	se	(0.071)	(0.110)	(0.064)	(0.086)
Lag MW	η_{-1}	0.107	0.083	0.131***	0.079
	se	(0.076)	(0.057)	(0.048)	(0.062)
C Frequency components					
High frequency	$\eta - \eta_{-1}$	-0.345**	-0.231	-0.247***	-0.142
	se	(0.140)	(0.140)	(0.090)	(0.112)
Low frequency	$\eta + \eta_{-1}$	-0.133**	-0.066	-0.018	0.015
	se	(0.053)	(0.103)	(0.069)	(0.098)
Division-specific time controls			Y		Y
State-specific time trends				Y	Y

Notes: η denotes the employment elasticity associated with the contemporaneous minimum wage, and η_{-1} denotes the elasticity associated with the minimum wage with a one year lag. Results are reported for the log of the minimum wage. Significance levels are ***1%, **5%, *10%. Each specification includes individual controls for gender, race (4 categories), age (4 categories), education (12 categories), and marital status (4 categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Standard errors are clustered at the state level.

Table 5 Minimum wage effects on employment, teens 16-19, by time period

Specification	(1)	(2)	(3)	(4)
	Employment elasticities and standard errors			
1990 to 2006	-0.168***	-0.103	-0.025	-0.024
	(0.048)	(0.105)	(0.071)	(0.091)
1987 to 2006	-0.161***	-0.092	-0.033	-0.033
	(0.044)	(0.087)	(0.067)	(0.077)
1990 to 1998	0.072	0.025	0.067	0.057
	(0.068)	(0.125)	(0.069)	(0.125)
1998 to 2006	-0.169***	-0.152*	-0.164**	-0.042
	(0.053)	(0.086)	(0.082)	(0.105)
1997 to 2005 †	-0.104	-0.199	-0.102	-0.102
	(0.070)	(0.133)	(0.084)	(0.130)
1997 to 2005NW ‡	-0.136*	n/a	-0.176	n/a
	n/a		n/a	
Division-specific time controls		Y		Y
State-specific time trends			Y	Y

Notes: Significance levels are ***1%, **5%, *10%. Results are reported for the log of the minimum wage. Each specification includes individual controls for gender, race (4 categories), age (4 categories), education (12 categories), and marital status (4 categories), as well as controls for the non-seasonally adjusted unemployment rate, and the relevant population share for each demographic group. Standard errors are clustered at the state level.

† Our results for the time period used by Neumark and Wascher (2007). ‡ These results are from Neumark and Wascher (2007), Table 2. We use the same specification as Neumark and Wascher, including the same controls, but our samples are slightly different. 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. It is not clear that they should be excluded in a regression on employment.

Table 6 Minimum wage effects on wages, teens 16-19, by race/ethnicity and gender, 1990-2006

Specification		(1)	(2)	(3)	(4)
Race/ethnicity	Gender	Wage elasticities and standard errors			
White, non-Hispanic	All	0.142*** (0.027)	0.188*** (0.037)	0.254*** (0.044)	0.175*** (0.031)
	Male	0.110*** (0.031)	0.162*** (0.036)	0.227*** (0.055)	0.171*** (0.037)
	Female	0.163*** (0.034)	0.198*** (0.046)	0.277*** (0.049)	0.161*** (0.042)
Black	All	0.124* (0.063)	0.074 (0.094)	0.267** (0.111)	0.232** (0.101)
	Male	0.180* (0.093)	0.082 (0.147)	0.322** (0.145)	0.148 (0.169)
	Female	0.085 (0.075)	0.088 (0.156)	0.214* (0.118)	0.291* (0.165)
Hispanic	All	0.148*** (0.054)	0.017 (0.061)	0.148** (0.060)	0.110* (0.062)
	Male	0.084 (0.057)	-0.053 (0.090)	0.097 (0.070)	-0.024 (0.136)
	Female	0.234*** (0.061)	-0.003 (0.124)	0.279*** (0.106)	0.132 (0.130)
Black or Hispanic	All	0.113*** (0.037)	0.064 (0.049)	0.172*** (0.050)	0.165*** (0.051)
	Male	0.093** (0.039)	0.085 (0.089)	0.136*** (0.050)	0.112 (0.105)
	Female	0.109*** (0.027)	0.153*** (0.033)	0.208*** (0.048)	0.172*** (0.035)
Division-specific time controls			Y		Y
State-specific time trends				Y	Y

Note: See note to Table 3. White includes Asian-American. Significance levels are ***1%, **5%, *10%.

Table 7 Minimum wage effects on employment, teens 16-19, by race/ethnicity and gender, 1990-2006

Specification		(1)	(2)	(3)	(4)
Race/ethnicity	Gender	Employment elasticities and standard errors			
White, non-Hispanic	All	-0.167***	-0.139	-0.046	-0.057
		(0.046)	(0.096)	(0.062)	(0.084)
	Male	-0.132**	-0.106	0.000	-0.009
		(0.058)	(0.124)	(0.103)	(0.128)
	Female	-0.221***	-0.213*	-0.103*	-0.139
		(0.054)	(0.111)	(0.056)	(0.120)
Black	All	0.093	0.244	0.062	0.288*
		(0.170)	(0.238)	(0.203)	(0.155)
	Male	0.003	0.304	-0.195	0.142
		(0.233)	(0.306)	(0.332)	(0.289)
	Female	-0.147	0.332	0.302	0.591**
		(0.281)	(0.286)	(0.290)	(0.269)
Hispanic	All	-0.123	-0.159	-0.013	-0.008
		(0.104)	(0.230)	(0.134)	(0.252)
	Male	-0.253**	-0.132	-0.059	0.124
		(0.118)	(0.234)	(0.173)	(0.317)
	Female	-0.004	-0.144	-0.045	0.052
		(0.135)	(0.404)	(0.141)	(0.472)
Black or Hispanic	All	-0.138	0.002	0.073	0.146
		(0.104)	(0.218)	(0.142)	(0.152)
	Male	-0.250**	-0.039	-0.095	0.011
		(0.108)	(0.249)	(0.165)	(0.213)
	Female	-0.046	0.085	0.206	0.385*
		(0.139)	(0.253)	(0.163)	(0.222)
Division-specific time trends			Y		Y
State-specific time trends				Y	Y

Note: For details see note to Table 3. White includes Asian-American. Significance levels are ***1%, **5%, *10%. η refers to the employment elasticity.

Table 8 A comparison of wage and employment elasticities for teens, 16-19, by demographic group

	Allegretto, Dube and Reich		Neumark and Wascher	
	Wage	Employment	Wage	Employment
All	0.168***	-0.024	0.254***	-0.158
White, non-Hispanic	0.175***	-0.057	0.304***	-0.048
Black or Hispanic	0.165***	0.146	-0.093	-0.664**
Black	0.232**	0.288*	0.114	-0.836
Hispanic	0.110*	-0.008	-0.133	-0.426
Males	0.172***	-0.002	0.224***	-0.376
White, non-Hispanic	0.171***	-0.009	0.386***	-0.246
Black or Hispanic	0.112	0.011	-0.369**	-0.855
Females	0.148***	-0.071	0.286***	0.006
White, non-Hispanic	0.161***	-0.139	0.250**	0.087
Black or Hispanic	0.172***	0.385*	0.207	-0.551

Note: Significance levels are ***1%, **5%, *10%. Allegretto, Dube and Reich results are for 1990-2006 and are estimated with our full set of controls (Specification 4). Neumark and Wascher (2007) results are for 1997-2005 and are taken from their Table 5.

Table 9 A comparison of minimum wage employment elasticities across three studies

Study	Specification			
	(1)	(2)	(3)	(4)
This study CPS, teens 1990-2006	-0.168*** (0.020)	-0.103 (0.043)	-0.025 (0.029)	-0.024 (0.038)
90% CI	(-0.247,-0.089)	(-0.274, 0.069)	(-0.140, 0.091)	(-0.173, 0.124)
Dube et al (2007) QCEW, restaurants 1990-2006	-0.207*** (0.063)	-0.076 (0.060)	0.055 (0.042)	0.060 (0.041)
90% CI	(-0.312, -0.102)	(-0.176, 0.023)	(-0.014, 0.124)	(-0.007, 0.127)
Neumark and Wascher (2007) CPS, teens 1997-2005	-0.136* na		-0.178 na	
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. Significance levels: ***1%, **5%, *10%.