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Contributions

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Employment Effects of the 2009 Minimum Wage Increase: New Evidence from State-Based Comparisons of Workers by Skill Level

Abstract: In July, 2009, when the US Federal minimum wage was increased from \$6.55 to \$7.25, individuals in nearly one-third of all states were unaffected, since the state minimum wage already exceeded \$7.25. We use this variation to make comparisons of the employment of low-skill workers with their peers across states and with workers within states who were arguably unaffected by the increase, using DID and DIDID methods. Our data come from the 2009 Current Population Survey, 4 and 5 months before and after the increase. We find little evidence of negative employment effects for teens or less-educated adults. Further control for demographic characteristics and state fixed effects have relatively small effects on the size and significance of estimated effects.

Keywords: minimum wage, low-wage workers, low-wage workers

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1 Introduction

While the employment effects of minimum wage legislation are straightforward in simple models of the labor market,[1] the research literature has been somewhat less settled. The controversial analyses of the New Jersey–Pennsylvania state minimum wage difference (Card and Krueger 1994) reported a positive

1 A frequently-cited 1979 survey of professional and business economists found that 90% "generally agreed" or "agreed with provisions" that "a minimum wage increases unemployment among young and unskilled workers" (Kearl et al. 1979). This survey precedes the less-settled empirical literature of the past two decades.

employment effect of an increase on employment in fast food restaurants, a finding that was cited by President Clinton in support of legislation to increase the minimum wage in 1996 and 1997. Subsequent research by Neumark and Wascher (2000) and by Card and Krueger (2000) substantially weakened the evidence for that positive effect. But other similar findings appear in industry- and/or location-focused studies of Card (1992), Katz and Krueger (1992), Dube, Naidu, and Reich (2007), Dube, Lester, and Reich (2010), Allegretto, Dube, and Reich (2011), and Addison, Blackburn, and Cotti (2012), among others. Other research does support the more conventional negative employment effect, including Deere, Murphy, and Welch (1995), Neumark and Wascher (2007, 2008), Hoffman and Trace (2009), and Sabia, Burkhauser, and Hansen (2012), among others.

In this paper, we examine the impact of the July, 2009 increase in the minimum wage from $6.55 to $7.25 by making a set of comparisons across states and labor market groups. This increase, which was the third of a set of annual increases beginning in 2007 that boosted the minimum by a total of 40%, provides an interesting test case of the employment effects of the minimum wage. In 2006, prior to the first of the increases, just 2.2% of hourly workers were employed at wages equal to or below the Federal minimum, and three-quarters of them were almost certainly tipped employees who are legally paid an hourly wage less than the minimum. The minimum had been fixed for so long – nearly a decade – that it was largely irrelevant in the sense that equilibrium wage rates in low-skill markets were already likely above the wage floor. But by 2009, it was, as we show below, at a level at which its impact was binding over a larger population share and where, consequently, its employment effects might be more visible.

In examining the impact of the 2009 increase, we take advantage of the fact that some states already had, as of early 2009, a state minimum wage that exceeded $7.25 per hour and, in addition, had no further increase in that calendar year. As a result, the July 2009 minimum wage increase affected the wages of low-wage workers in some states, but not in others. Our analysis, which draws on and combines the case study and demographic group approaches of the existing literature, focuses initially on two kinds of comparisons: employment changes for workers likely to be affected by the minimum wage between the two groups of states and the employment change within the states with a minimum wage increase for those same workers with the change for a group of workers who were unaffected by the increase. Both of these approaches are implemented using standard difference-in-difference techniques. We also use a difference-in-difference-in-difference comparison that combines the two approaches and that provides important control for unmeasured factors that might have affected relative labor demand by skill group or by state group. In the context of the deteriorating US economy in 2009, this is a particularly important adjustment. Finally, we use the corresponding regression specifications of these difference models to incorporate further control for individual traits and for state fixed effects.

We find clear evidence that the minimum wage did affect wage rates for low-wage workers, but, like some other recent studies, we find weak evidence of negative employment effects for two groups that we show were affected by the increase. The employment rate of not-in-college teens in states with an increase fell by a bit more than teens in the other states and also fell by more than individuals in the same states who were essentially unaffected by the minimum wage increase. But the difference is relatively small and in no instances close to statistical significance. The DIDID estimate and regression estimates that control for covariates are even smaller. Employment of adult workers with less than a high school degree is essentially unaffected in all comparisons; all of our estimates are very small and some are actually positive. We also look at hours of work changes and find effects that are consistent with the employment rate changes.

Our results do not imply that minimum wages do not ever have negative effects on employment or that labor demand curves are not downward sloping. We have sufficient evidence to the contrary on both counts. Our results are specific to the time, place, and increase in the minimum wage examined.

The next section briefly reviews the empirical literature on the new economics of the minimum wage. Our data and methods are presented in Section III. Results are presented in Section IV. A summary and discussion is presented in Section V.

2 Employment effects of the minimum wage

The extensive economic literature on the employment effects of the minimum wage includes a very wide variety of approaches and findings. Through the 1980s and early 1990s, most of the literature used an aggregate time-series analysis of a single national employment rate for plausibly affected at-risk groups such as teens and less-educated workers (see, e.g. Brown, Gilroy, and Kohen 1983; Wellington 1991).[2] This approach typically estimated an average

2 We use "at-risk" here to refer to demographic groups that have a relatively high proportion of workers earning wages between the old and new minimum wage.

minimum wage impact on employment over a relatively long time period, using a measure of the minimum wage relative to average hourly earnings, adjusted for coverage, as a measure of the level of the minimum wage.

In the early 1990s, alternative research approaches, now often referred to collectively as the "new economics of the minimum wage," emerged that estimated minimum wage effects by focusing on employment impacts across geographic units, industries, and/or demographic groups, rather than over time. This approach is typically more disaggregate than the earlier literature and often focuses on a single minimum wage increase or a far shorter time series than in the earlier time-series approach. Well-known early contributions include Katz and Krueger (1992), who examined the effect of the 1991 minimum wage increase on employment in fast food restaurants in Texas; Card (1992), who took advantage of state variation in average wage levels to examine the impact of the 1990 increase; Neumark and Wascher (1992), who exploited differences in state minimum wages to identify the minimum wage effect (see also Burkhauser, Couch, and Wittenburg 2000); the NJ–PA natural experiment analyzed by Card and Krueger (1994, 2000) and Neumark and Wascher (2000); and Deere, Murphy, and Welch (1995), who examine differences in employment changes across demographic groups with different exposure to the minimum wage following the 1990 and 1991 minimum wage increase. More recent contributions include Hoffman and Trace (2009), who examine employment changes in New Jersey and Pennsylvania following the 1996 and 1997 minimum wage increases that eliminated the previous minimum wage differential in these states, and Sabia, Burkhauser, and Hansen (2012), who present a similar, but more extensive state-based analysis of employment of less-skilled workers in New York relative to its surrounding states following an increase in the NY minimum in 2004–2006. See Neumark and Wascher (2007, 2008) for a comprehensive survey of this literature as of the mid-2000s and Neumark, Salas, and Wascher (2013) for a recent critical review of the most recent research contributions.

As most observers of this literature appreciate, the simple textbook prediction – that a downward-sloping demand curve for labor implies a negative impact of a minimum wage on the employment of workers whose equilibrium wage is less than the new minimum – has sometimes been difficult to confirm in practice. The time-series approach typically finds a small negative effect with employment elasticity about −0.1 to −0.3. State-based panel data analyses of Neumark and Wascher (1992) and Burkhauser, Couch, and Wittenburg (2000) similarly find negative impacts, although Card's state-based analysis does not. Negative employment effects for at-risk groups are reported in Deere, Murphy, and Welch (1995), Hoffman and Trace (2009), and Sabia, Burkhauser, and Hansen (2012). In contrast, contrary findings are more common in the industry-focused studies, especially those involving employer surveys such as Katz and Krueger (1992) and Card and Krueger (1994). More recent industry-focused studies that also do not find a negative effect include Dube, Naidu, and Reich (2007), Dube, Lester, and Reich (2010), and Addison, Blackburn, and Cotti (2012). In a recent contribution using a 1999–2009 time series for teens, Allegretto, Dube, and Reich (2011) find no negative effects once employment trends within regions are controlled. Neumark, Salas, and Wascher argue that the findings of Allegretto, Dube, and Reich are highly sensitive to model specification and suggest that the new spatial heterogeneity literature with its finding of no disemployment effects is not definitive.

Like other research areas in labor economics, some studies of minimum wage effects have adopted the language and methodology of natural experiments, looking for situations in which minimum wages were increased exogenously in some labor market but not in another reasonably similar one or that differentially affected groups of prospective workers. Card's (1992) analysis of impacts across states was the first to explicitly use the language of natural experiments.[3] Other early examples include Card and Krueger's famous NJ–PA analysis and Deere, Murphy, and Welch's comparison of employment changes by group relative to exposure to the new minimum wage.[4] Recent contributions in this style include Dube, Naidu, and Reich (2007), Hoffman and Trace (2009), and Dube, Lester, and Reich (2010), all of whom exploit policy changes that raised the minimum wage in one geographic area, but not in some adjacent area that serves as a control group.

We are not aware of any analyses of the 2009 minimum wage increase as a case study, other than a brief report by Mulligan (2010) in the *NY Times Economix* blog. Mulligan noted that the minimum wage did not increase in some states, using a classification of states similar to, though not identical to, the one we use below (see Footnote 5 for details). To identify the impacts of the 2009 increase, he focused on the ratio of part-time to full-time employment and how that ratio changed before and after the increase in the two groups of states. He finds that the ratio increased in both groups of states after the minimum wage increase, but that the increase is smaller in the states where the minimum wage increased than in the other states in 4 of the 5 months following the

3 Card writes "The imposition of a national minimum wage standard provides a natural experiment in which the 'treatment effect' varies across states depending on the fraction of workers initially earning less than the new minimum" (Card 1992, 22).

4 Deere, Murphy, and Welch adopt the general approach but not the vocabulary or full method in their analysis.

increase. He concludes that the evidence is "consistent with the hypothesis that the federal increase [in July 2009] caused part-time job losses in the affected states, but not in the others." The difference does not appear to be large, and no indication of statistical significance is presented.

3 Data and methods

To evaluate the employment effects of the 2009 minimum wage increase, we compare employment in February and March 2009 with employment in November and December 2009 as a function of the change in the minimum wage over this time period at the state level. Prior to the July 24, 2009 increase, 33 states had a minimum wage less than $7.25 and thus were directly affected by the increase. Illinois and the District of Columbia already had, as of 2008, a minimum wage above the mandated 2009 level, but further increased their minimum on July 1, 2009, just prior to the federal increase. The remaining 16 states had a minimum wage above $7.25 as of early 2009 and, unlike Illinois and DC, had no further increase. Workers in these 16 states were, thus, unaffected by the July increase in the Federal minimum and experienced no increase in the February–December time period.[5] In most of these states, the state minimum reflects application of a statutory formula that updates the state minimum annually based on the CPI or other wage index.[6] As a result, the state minimums are reasonably regarded as exogenous, rather than as a response to specific (e.g. favorable or tight) labor market conditions.

We use a difference-in-difference approach to examine the employment impact of the minimum wage increase, with and without adjustment for covariates. We make three comparisons involving two time periods and several demographic groups. Our first measure is a between-state comparison, $\mathrm{DID}_B = (E^T_{j2} - E^T_{j1}) - (E^C_{j2} - E^C_{j1})$, where E_{jt} is the employment rate in time t of

5 The coding here differs slightly from that used in an EPI study by Even and Macpherson (2010), which finds that 19 states were unaffected by the July 24, 2009 increase. The EPI classification includes DC, IL, and KY, all of whom increased their minimum wage July 1, as states unaffected by the increase in the Federal minimum. Since our coding focuses on changes in the effective minimum wage between February and November, 2009, rather than its level on July 24, these three states are appropriately included among states with an increase. For more details, see Appendix Table 1.

6 Typical examples include Vermont, Washington, and Montana, all of whom by statute increase the state minimum wage at the same rate as the increase in the CPI from August to August for the preceding 12 months. Florida's increase is based on the percentage change in the CPI for urban wage earners and clerical workers in the South Region for the 12-month period prior to September 1.

some group j whose employment is likely to be affected by the minimum wage increase and T and C identify residence in a state with a minimum wage increase (T) or with no increase (C) between time 1 and time 2. The between-state estimate measures the minimum wage employment effect as the difference in employment changes of at-risk workers in states with and without increases in the minimum wage.

Our second measure is a within-state comparison, $\mathrm{DID}_W = (E^T_{j2} - E^T_{j1}) - (E^T_{k2} - E^T_{k1})$, where k is some other group of workers whose employment is likely to be unaffected by the minimum wage increase. The within-state comparison measures the difference between the employment change of at-risk workers in states with a minimum wage increase with the corresponding change for workers whose equilibrium wage is sufficiently high that they are essentially unaffected by the minimum wage increase.[7] This kind of comparison is similar to what Deere, Murphy, and Welch did by comparing employment changes for groups of workers with differing levels of exposure to the minimum wage and also to Card and Krueger's analysis of impacts on restaurant employment within New Jersey by the level of their average wage prior to the legislated increase.

Lastly, we estimate a difference-in-difference-in-difference comparison, $\mathrm{DIDID} = \left[\left(\Delta E^T_J - \Delta E^T_k\right) - \left(\Delta E^C_J - \Delta E^C_k\right)\right]$ or, equivalently by re-arranging terms, $\left[\left(\Delta E^T_J - \Delta E^C_J\right) - \left(\Delta E^T_k - \Delta E^C_k\right)\right]$, where Δ is the change in the associated employment rate. In the first DIDID expression, the first term is exactly the within-state difference (DID_W) for groups j and k discussed previously and the second term is the corresponding measure for workers in states where the minimum wage did not increase. The second DIDID expression is the difference in the between-state difference for affected group j and unaffected group k; now, the first term is the DID_B estimate for group j and the second is the corresponding term measured for some group of workers unaffected by the minimum wage. The DIDID comparison thus examines whether employment changes by minimum wage exposure within states differed across the treatment and control states or, equivalently, whether the between-state impact differs for affected and unaffected groups. As explained below, the DIDID measure is particularly useful as a way to adjust for omitted variable bias in the other two measures.

The two DID estimates have a natural interpretation in the context of a regression as the estimated coefficient on the interaction of time period 2 and a treatment status dummy variable that denotes either residence in a state where

7 If groups j and k are related factors of production, the demand for k could be affected when the wage for j changes. Given the definition of groups we use and the magnitudes of impacts that we find, this impact is likely to be negligible.

the minimum increased (for the between-state estimates) or membership in a group likely to be affected by an increase in the minimum (for the within-state estimates). If some variable is omitted that is correlated with minimum wage treatment status and affects employment for one of the groups, then standard omitted variable bias is present. For example, the estimated within-state treatment effect would be biased if some variable Z is omitted that affects the employment of group j relative to group k in the treatment states in period 2, thus confounding the estimation of the minimum wage effect with a non-constant time period effect. Similarly, the between-state estimates will be biased if some omitted factor affects the employment of group j differently in the two groups of states in period 2. These are certainly plausible effects to expect, especially in the context of the changing 2009 economic conditions.

One way to assess the extent of this bias and to correct for it is to estimate "pseudo" within-state and between-state treatment effects parallel to DID_W and DID_B. The pseudo within-state DID_W is estimated for j and k workers in the control states, where neither group is affected by a minimum wage increase, while the pseudo between-state DID_B is estimated for group k, a group unaffected by the minimum wage increase in both sets of states. In both cases, the true minimum wage employment effect is zero, so evidence of a non-zero effect is an indication of some omitted variable effect. For example, a non-zero pseudo-DID_B estimated for group k might indicate that labor market conditions were more or less favorable in one set of states, while a non-zero pseudo-DID_W in the control group states might indicate that labor market conditions were more or less favorable for group j relative to k independent of the minimum wage change. Again, these are realistic concerns in light of the changing macroeconomy in 2009.

The DIDID estimator provides a straightforward way to adjust the DID estimates for exactly this kind of potential bias, because the additional difference term in the DIDID estimator uses the pseudo effects to net out the bias. Consider the first DIDID specification shown above: $\mathrm{DIDID} = \left[\left(\Delta E_J^T - \Delta E_k^T\right) - \left(\Delta E_J^C - \Delta E_k^C\right)\right]$. The first term is the within-in state estimate (DID_W^T), while the second term is $\mathrm{DID}_W{}^C$, which is simply the pseudo within-state treatment effect. Thus, in this version, the DIDID uses evidence on changing employment patterns for j and k workers in unaffected states to adjust the within-state estimate for the same groups in the states where the minimum increased. Similarly, consider the second specification of the DIDID: $\mathrm{DIDID} = \left[\left(\Delta E_J^T - \Delta E_J^C\right) - \left(\Delta E_k^T - \Delta E_k^C\right)\right]$. Here, the first term is $\mathrm{DID}_B{}^T$, while the second term is $\mathrm{DID}_B{}^C$, the pseudo between-state treatment effect for unaffected group k. In this case, we are adjusting the between-state estimate for a group affected by the minimum wage for potential differential state effects estimated for a group unaffected by the increase. Thus, we view the value of the DIDID estimates as a way to control for otherwise unmeasured effects correlated with treatment status, especially those related to the changing macroeconomic environment. This is a very important strength of this approach.

We also estimate two regression models of the treatment effect to allow for the impact of covariates. The first specification adds basic individual demographic characteristics such as age, race, Hispanic ethnicity, and gender, which could be important if race and ethnicity differ across the two groups of states. The regression equivalent of the between-state and within-state DID model[8] is:

$$E_{ist} = \beta_0 + \beta_1 TR_{ist} + \beta_2 T2_{ist} + \delta TR_{ist} \times T2_{ist} + \mathbf{Z}_{ist}\boldsymbol{\theta} + \mu_{ist} \quad [1]$$

In eq. [1], E_{ist} is an employment indicator for individual i in state s in time period t, TR is a dummy for residence in a treatment state (or membership in a treatment group in the within-state analyses), $T2$ is a dummy for the second time period, $\mathbf{Z}$ is a vector of individual characteristics, and μ is an error term. The interaction term $TR_{ist} \times T2_{ist}$ identifies the minimum wage treatment effect δ for the DID model. In a model without covariates, it is exactly equivalent to the treatment effect from the two DID estimators discussed above.

In the second regression specification, we substitute a full set of state fixed effects for the uniform effect in eq. [1] of residence in a state in which the minimum was subsequently increased. This controls for otherwise unobserved permanent state effects that may affect employment levels.

Our data come from the Current Population Survey (CPS) for February, March, November, and December 2009, 4 and 5 months before and 4 and 5 months after the July 24, 2009 federal minimum wage increase. We treat February and March as the period one sample and November and December as the period 2 sample. We restrict our sample to individuals between ages 16 and 59. Each pair of months includes about 80,000 persons meeting the sample inclusion criteria.

The minimum wage can only have employment effects if it is binding for at least some workers, that is, if the new minimum is above what would otherwise be the prevailing market equilibrium for some group of workers. In our implementation, we also need to identify a group for which the minimum wage is almost certainly not binding. We do this using age and education to classify workers. As potentially at-risk workers, we use workers age 16–19 who are not in college[9] and less-educated adult workers, here defined as persons age 20–59

8 The DIDID model includes additional interaction terms to capture the treatment effect.

9 This education restriction eliminates some 18- and 19-year olds who are college students and thus results in a less-skilled and younger group of teen workers that are more likely to be affected by a minimum wage increase.

with less than 12 years of education. We use males age 30–49 with some post-secondary education as a control group that is largely unaffected by an increase in the minimum wage. We present evidence on the validity of these group definitions in the next section.

When the Federal minimum wage increased from $6.55 to $7.25 per hour, some states already had a minimum wage in place in early 2009 between these two wage levels and had no further increase in their state minimum wage. As a result, nine states had atypical increases in the minimum wage over this time period, including $.0.04 in Florida and $0.10 in Alaska, Delaware, New Jersey, New York, and Pennsylvania, $0.20 in Missouri, $0.25 in Illinois, and $0.35 in Montana. In order to facilitate a clearer comparison of wage and employment changes, we exclude individuals in these nine states from most of our analyses. As we show below, the sub-sample of states with the full $0.70 increase is very similar in observed characteristics to the larger sample of states with any increase in the minimum.

In light of the economic downturn in 2009, it is natural to wonder whether the change in economic conditions was reasonably similar in the two groups of states. It appears that it was. We computed a weighted-average unemployment rate in the two groups of states, using the February–March average civilian labor force as weights. The February/March average unemployment rate was higher in the control group states than in the treatment states, but the increase over the February/March–November/December time period is very similar – 1.2 percentage points in the states with no increase in the minimum wage and 1.0 percentage point in the states where the minimum wage increased. The two biggest states – California (control) and Texas (treatment) – had almost identical unemployment rate increases of 1.5 and 1.55 percentage points, respectively. Note also that the DIDID analysis provides additional control for changes in economic conditions across the two groups of states.

4 Findings

All estimates are based on microdata on individual employment computed from the 2009 CPS. In computing means and in our regressions, we use sample weights that enable us to reproduce exactly the official Bureau of Labor Statistics (BLS) reported employment rates for the civilian labor force for the 4 months analyzed.[10] This

10 About 0.5% of potential sample members are excluded because they are in the Armed Forces, including about 1.0–1.5% of the sample at ages 22–31.

Table 1: Sample means (weighted), individuals age 16–59, February/March 2009, by subsequent minimum wage increase

	Control states (no increase in min. wage)	Treatment states (all with minimum wage increase)	Treatment states (full increase only)
Age	37.43	37.48	37.25
Age 16–19	0.095	0.093	0.094
Black	0.071	0.157	0.164
Hispanic	0.200	0.136	0.132
Male	0.498	0.494	0.493
Not HS graduate	0.168	0.164	0.178
College graduate	0.272	0.259	0.243
Employment rate	0.688	0.695	0.696
Number of observations	57,170	102,707	67,526

Source: CPS, February and March, 2009.

means that our estimates of employment rates for subgroups are exactly what the BLS would report for those groups if it presented data for them.

Weighted means for the period 1 samples are shown in Table 1. The first column shows the population characteristics of persons in the 16 states with no 2009 increase in the minimum wage, while the other two columns show the corresponding information for all states with an increase in the minimum wage and in just those states with the full $0.70 increase. A comparison of columns 2 and 3 indicates that the exclusion from the analysis of workers in states with a small minimum wage increase does not change the measured characteristics of the sample. The two groups appear to be very similar on almost all measures, differing at most by about 1.5 percentage points for two educational attainment measures. The characteristics of individuals in states with the full minimum wage increase (column 3) and those in states with no increase (column 1) are also quite similar on most dimensions. Age, educational attainment, and the proportion male are virtually identical. Race and ethnicity differ more: the control group states, which include Arizona, California, Colorado, and New Mexico, have a higher representation of Hispanic workers and a lower proportion of blacks than the other group.[11] This suggests that control for these demographic characteristics via regression may be important. The employment rates for individuals in the two groups of states are virtually identical.

11 Arizona, California, and New Mexico were, respectively, 30.1%, 36.6%, and 44.9% Hispanic and 4.2%, 6.7%, and 3.0% black in 2009. The national average is 15.1% and 12.3%.

Table 2: Distribution of employment by industry, control, and treatment states, March 2009

Industry	Control states (%)	Treatment states (%)
Agricultural, forestry, fishing, and hunting	1.5	1.8
Mining	0.3	1.1
Construction	7.6	8.2
Durable goods mfg.	7.9	6.7
Nondurable goods mfg.	3.5	4.2
Wholesale trade	2.6	2.6
Retail trade	11.5	12.0
Trans. and warehousing	3.6	4.5
Utilities	0.9	0.8
Information	2.5	2.0
Finance and insurance	4.4	4.3
Real estate and rental and leasing	2.1	2.0
Prof. and technical services	7.1	5.6
Management, admin., and waste management services	4.7	4.3
Educational services	8.9	9.5
Health care and social assistance	12.6	11.7
Arts, entertainment, and recreation	2.2	1.9
Accommodation and food services	7.2	7.2
Private households	0.6	0.4
Other services, except private households	4.2	4.4
Public administration	4.1	4.9

Source: CPS, March, 2009.

The distribution of employment across industries is also very similar in the two groups of states. Table 2 shows employment shares as of March, 2009 for a two-digit industrial classification. As can be seen in the table, the employment distributions differ very little, usually by 0.5% or less. An overall index of dissimilarity, ranging from 0 for no difference to 100 for complete non-overlap, is 5.3.

4.1 Wage effects

Before turning to the employment analysis, we examine three issues that underlie the comparisons we make. The first is whether the federal minimum was effective in raising wages of the two at-risk groups and thereby could potentially have had an impact on their employment. The second is whether the higher state minimums were effective in the sense that the wage distribution differed across the two groups of states prior to the July increase. The third is whether the control group of older more-educated males was, in fact, unaffected by the change in the minimum. To examine these issues, we use the 2009 CPS Merged Outgoing Rotation Group files (CPS-MORG), because only these files contain wage information for workers paid by the hour. The CPS-MORG files are based on the one-quarter of the regular CPS file that rotates out of the sample in its fourth and eighth months. The annual merged file contains these files for the entire calendar year; the total sample is three times as large as any single monthly file, although for any single month it is only one-quarter as large.[12]

This file is the basis for the annual tabulations the BLS provides on characteristics of minimum wage workers. Using the wage variable in the file and the appropriate sample weight, we were able to exactly replicate the 2009 case counts and percentages for workers at and below the federal minimum wage ($6.55 for January through July, $7.25 for August through December) reported in Table 1 of the BLS report for all adults and for teens (Bureau of Labor Statistics 2010). This means that the percentages we report below are exactly those the BLS would report if they organized the data as we do.

Table 3 shows how the wage distribution changed for our two at-risk groups in the states where the minimum wage increased. Because monthly samples are relatively small, especially when divided between the two groups of states, we pool the data for January–April and also for October–December, that is, 2–6 months before and after the July increase. The table shows the distribution below $6.55, a group presumably reflecting primarily the tipped employee subminimum[13]; exactly at $6.55; between $6.55 and $7.24; and exactly at $7.25. If the minimum wage increase had the expected effect on wages in these states, we should see relatively little change in the proportion below $6.55, because the legal minimum for those workers was unchanged.[14] The proportions of workers at $6.55 and between $6.55 and $7.24 would both decrease and the proportion at $7.25 would increase.

By and large, that is exactly what is seen in Table 3. In the states where the Federal minimum wage increased in July, 2009, nearly 16% of employed teens not in college earned less than the minimum in January through April and

12 That is why we do not use this for our employment analysis.

13 The tipped employee minimum was $2.13 in both 2008 and 2009, in spite of the increase in the Federal minimum.

14 There may be some exceptions to this depending on the extent of the tip credit in a state. In a state with a small tip credit, the proportion earning less than $6.55 might decline when the minimum wage increased to $7.25.

Table 3: Wage distribution for low-wage workers paid by hour, January–April and September–December, 2009 in states where minimum wage increased

Wage level	Teens, not in college		Adults, 20–59, not high school graduate	
	Jan–Apr	Sept–Dec	Jan–Apr	Sept–Dec
$W <$ \$6.55	0.1583	0.0899	0.0611	0.0227
$W =$ \$6.55	0.0676	0.0071	0.0305	0.0004
\$6.56 $< W <$ \$7.24	0.2112	0.0918	0.0802	0.0379
$W =$ \$7.25	0.0451	0.2041	0.0116	0.0526
Number of observations	755	614	855	917

Source: Author calculations from 2009 CPS-MORG files.
Note: Federal minimum wage equals $6.55 in February and $7.25 in November.

nearly another 7% earned exactly the minimum. After the minimum wage increase, the percentage earning exactly the old minimum falls to less than 1%, while the fraction earning between the old and new minimums falls in half and the fraction earning exactly the new minimum more than quadruples. The proportion of teens earning less than the old minimum wage also decreases, but more modestly than the other changes.

The same pattern appears in the wage distribution for the less-educated adults in columns 3 and 4. The percentage earning exactly the old minimum wage falls from 3% to almost 0 and the corresponding percentage earning exactly the new minimum increases from about 1% to more than 5%. The fraction earning less than $6.55 falls to about 40% of its original level. It appears that many of the workers earning between the two minimum wage levels in the first part of the year are earning the new minimum in the latter part.

Looking more carefully at the timing of the wage changes confirms that the impact is basically coincident with the imposition of the new minimum. Sample sizes by single month are much smaller, so some caution is appropriate in interpreting month-to-month movements. For teens, the fraction earning exactly $6.55 drops from about 6.5% in April and May to 3% in July and August and then to 1% or less for the rest of the year, while the fraction earning exactly $7.25 more than triples between July and August. The same pattern is exhibited by the less-educated older workers. For them, the fraction earning exactly $6.55 falls from 2% in June and July to 0.5% in August and zero thereafter. The proportion earning $7.25 jumps 250% from July to August. This suggests that the wage changes are not acted on in advance, so that a comparison of employment before and after the wage change is not tainted by anticipatory responses.

We also find substantial differences between the wage distribution for the two groups of workers in the treatment and control states in the January–April time period. As shown in Table 3, 22.6% of teens and 9.2% of less-educated adults in the treatment states earned either exactly the federal minimum or less than it. In the states with a higher minimum, the corresponding fractions are 6.0% for teens and 2.2% for the adults (these results not shown in Table 3). This is a difference of about 3.8:1 or more in both cases, which certainly indicates that the state minimums did affect the wage distribution of less-skilled workers.

Finally, the CPS-MORG files confirm that males age 30–49 with at least some college were essentially unaffected by the increase in the minimum wage. Most such workers are, in fact, not paid by the hour and are thus exempt from the minimum wage provisions. In the states where the federal minimum did increase, only 36% of these workers were paid by the hour. Of this group, 1.5% earned less than the $6.55 minimum in the January–April period, 0.16% earned exactly the minimum, 0.87% earned between $6.55 and $7.24, and 0.08% earned exactly $7.25. Recomputed as a fraction of all such workers in the group, whether paid by the hour or not, the total at $7.25 or below is less than 1%, so they are clearly a valid control group.

4.2 Employment effects

We begin our analysis of employment effects in Table 4 with the between-state estimates (DID_B). The top two panels show the before and after employment rates for workers in states with a full increase in the minimum wage and the corresponding groups in the states with no increase. The bottom panel shows the difference-in-difference estimates. The *t*-statistics shown are for tests of no difference in the change in the relevant employment rates.

The employment rate for teens not in college fell by 2.43 percentage points between February/March and November/December in the states with the full minimum wage increase and by 1.81 percentage points in the states with no increase. The between-state DID estimate is thus −0.62 percentage points, approximately a 2.4% decline in employment and implying an employment elasticity of −0.226. The difference is not close to statistical significance (*t*-statistic = 0.51). Note that the baseline employment of this group is lower in the control group states than in the treatment states (difference equals 2.3 percentage points, *t*-statistic = 2.70). This is consistent with the higher baseline unemployment rate in these states.

In the next two columns, we examine employment changes for all adults age 20–59 with less than a high school degree and then just for the corresponding

Table 4: Between-state DID estimates of impact of 2009 minimum wage increase on employment of at-risk groups

	Age 16–19 (not in college)	Not HS grad. (Age 20–59)	Not HS grad. (Age 20–59, male)
Treatment states			
Before			
Mean	0.2575	0.5448	0.6459
Std. dev.	0.0059	0.0061	0.0081
N	5,515	6,587	3,508
After			
Mean	0.2332	0.5413	0.6390
Std. dev.	0.0059	0.0061	0.0081
N	5,176	6,591	3,534
Difference	−0.0243	−0.0035	−0.0069
t-statistic	2.89	0.41	0.60
Control states			
Before			
Mean	0.2342	0.5441	0.6520
Std. dev.	0.0063	0.0069	0.0092
N	4,516	5,145	2,663
After			
Mean	0.2161	0.5368	0.6312
Std. dev.	0.0062	0.0070	0.0094
N	4,401	5,035	2,652
Difference	−0.0181	−0.0073	−0.0208
t-statistic	2.05	0.74	1.58
DID_B (*T*–*C*)	−0.0062	0.0038	0.0139
t-statistic	0.51	0.29	0.80
Elasticity	−0.226	0.065	0.202

Source: CPS, February/March and November/December 2009. Sample includes individuals in states with full increase in minimum wage or with no increase in minimum wage.

group of males. Employment for these groups barely fell in the states with an increased minimum wage (–0.4 and –0.7 percentage points, respectively) and actually fell more in the states with no increase. As a result, the between-state DID estimates are actually both slightly positive (0.38 percentage points and 1.39 percentage points, respectively), but with very low statistical significance. Thus, between-state comparisons do not show evidence of a meaningful negative minimum wage employment effect of the July, 2009 increase. Teens in states with a rising minimum wage fared just slightly worse relative to their peers in states where the minimum wage did not increase, while for less-educated adult workers, there is no negative impact whatsoever.

Table 5: Within-state DID and DIDID estimates of impact of 2009 minimum wage increase on employment rate of at-risk groups

	Males, age 30–49, at least some college	Age 16–19 (no college)	Not HS grad. (age 20–59)
A. Treatment states			
Before	0.8950	0.2575	0.5448
After	0.8853	0.2332	0.5412
Difference (*t*-statistic)	−0.0097	−0.0243	−0.0035
	(2.00)	(2.93)	(0.46)
DID_W (*t*-statistic)		−0.0146	0.0062
		(1.52)	(0.62)
Elasticity		−0.531	0.106
B. Control states			
Before	0.8744	0.2342	0.5441
After	0.8750	0.2161	0.5368
Difference (*t*-statistic)	0.0006	−0.0181	−0.0073
	(0.12)	(2.05)	(0.74)
Pseudo-DID_W		−0.0188	−0.0079
(*t*-statistic)		(1.81)	(0.70)
C. DIDID estimates			
DID_B (T–C)	−0.0103	−0.0062	0.0038
DIDID (T–C)		0.0041	0.0141
(*t*-statistic)		(0.29)	(0.94)
Elasticity		0.149	0.242

Source: CPS, February/March and November/December 2009. Sample includes individuals in states with full increase in minimum wage or with no increase in minimum wage.

Within-state estimates (DID_W) are shown in Panel A of Table 5. First, we compare employment rate changes for teens and for less-educated adults[15] in states with a minimum wage increase with those for males, age 30–49 with at least some college in the same states. As can be seen in the first column in the top panel of the table, the employment rate for the 30- to 49-year-old males fell by 0.97 percentage points in these states (a 1.1% drop), presumably for reasons having little or nothing to do with the increase in the minimum. We already saw in Table 4 that the employment rate for teens in those states fell by 2.43 percentage points (repeated here in column 2, row 3), generating a within-state DID estimate for teens of −1.46 percentage points. The DID estimate is still not statistically

15 For ease of presentation, we do not present results for males, age 20–59 without a high school degree. Results are very similar to those shown for all age 20–59 without a high school degree.

significant at conventional levels, but it is closer to that level and the implied elasticity is now −0.531, more than twice the previous estimate. As in the previous analysis, we find no evidence of a minimum wage effect for less-educated adults. The decline in their employment rate (column 3, row 3) is less than that of the comparison group of prime-age males, so the within-state DID estimate is positive, just like the between-state estimate. The effect is very small and not close to statistical significance.

The larger within-state estimates are not surprising, since employment of less-skilled lower-wage workers is typically more cyclically sensitive. The DIDID estimate adjusts for this using information on the same relative employment shift in the states with no minimum wage increase. The second panel of the table shows these changes. In these states, the employment rate for prime-age educated males was essentially unchanged between early and late 2009, indicating the possible presence of more favorable overall labor market conditions than in the states with a minimum wage increase where the corresponding employment rate fell by nearly 1 percentage point. Employment rates fell in these states by 1.81 and 0.73 percentage points, respectively, for the teens and the less-educated adults; presumably, these declines do not reflect a minimum wage effect, but rather shifts in relative factor demands. For both the teens and the less-educated adults, the corresponding within-state DID estimates are actually more negative than for the states where the minimum increased. For example, teens fared worse in terms of employment relative to prime-age more-educated adults in states where the minimum wage did not increase (–1.88) than in the states where the minimum did increase (–1.46). The same pattern holds for less-educated adults (–0.0079 vs 0.0062).

The resulting DIDID estimates are shown in the bottom section of Table 5. As discussed earlier, both sets of DID estimates could be biased: the between-state estimate will be biased if economic conditions differ between the two groups of states and the within-state estimates will be biased if relative demands by skill group changed in states where the minimum increased coincident with the increase, something that is quite plausible in an economic down turn. The DIDID estimates enable us to address this concerns using the between-state DID estimate for groups not affected by the minimum wage to correct the between-state estimate for the at-risk groups and using the within-state DID estimate from the control states for two groups unaffected by the minimum wage increase to correct the within-state estimate in the treatment states.

The DIDID can be computed either as the difference in the between-state DIDs from Table 4 and the pseudo-DID$_B$ estimates from Table 5 or as the difference between the within-state DIDs shown in the top panel of Table 5, and the pseudo-DID$_W$ from the second panel. For convenience, we therefore repeat the DID_B estimates from Table 4, including also the corresponding term for the more-educated prime-age males (Panel C, column 1, row 1). This pseudo-DID_B term for this group is negative (–0.0103) and suggests that the labor market worsened over time for workers unaffected by the minimum wage in the states where the minimum increased. If we interpret this result as applying more generally to workers in the minimum wage increase states, it implies that the earlier between-state DID estimates are too negative because they do not account for this differential labor market effect. In effect, some of the apparent negative minimum wage effect captured in the between-state estimate reflects broader labor market trends that differed across the two sets of states.

The within-state estimates are also too negative, because they do not account for the worsening position of the at-risk groups relative to prime-age adults in states with no minimum wage increase. This implies the presence of a more general shift in labor demand that negatively impacted at-risk groups relative to more-skilled groups. Reasoning in the same way as for the between-state DID, it follows that some portion of the within-state estimate reflects not the impact of the minimum wage increase, but rather the relative decline in demand for less-skilled at-risk workers.

For teens, the estimated DIDID impact of the minimum wage increase on employment is slightly positive (0.41 percentage points, but not nearly statistically significant), rather than slightly negative (the between-state estimate) or more negative (the within-state estimate).[16] Just as for the other two estimators, we find a small, positive, but statistically insignificant employment effect on less-educated adults.

4.3 Multivariate analysis

As already noted, the two samples differed in terms of race and Hispanic ethnicity. We can account for those factors by estimating the regression equivalents of our various estimators, adding race (black = 1), gender (male = 1), and ethnicity (Hispanic = 1) as covariates. Table 6 presents our regression estimates.[17] The table includes results from three specifications for each of the difference estimators: a baseline model with no covariates, a model that includes the covariates, and a model that adds state fixed effects. For ease of

16 This DIDID effect (0.0041) is, equivalently, the difference in the between-state effects for teens and prime-age males (= –0.0062–(–0.0103)) or the difference in the within-state effects (–0.0146–(–0.0188)). All other DIDID results can be calculated in the same way.

17 Standard errors are clustered at the state level.

Table 6: Regression estimates of impact of 2009 minimum wage increase on employment (standard errors in parentheses) revised October 17, 2012 to include adults not HS graduates

Model (control variables)[a]	Age 16–19, no college			Not high school grad., age 20–59		
	Between-state estimate	Within-state estimate[b]	DIDID[b]	Between-state estimate	Within-state estimate[b]	DIDID[b]
1. No covariates	−0.0062	−0.0146	0.0041	0.0038	0.0062	0.0141
	(0.0122)	(0.0090)	(0.0133)	(0.0131)	(0.0096)	(0.0142)
2. Demog. traits	−0.0033	−0.0170*	−0.0003	0.0026	0.0037	0.0115
	(0.0117)	(0.0090)	(0.0133)	(0.0126)	(0.0093)	(0.0138)
3. Demog. traits and state fixed effects	0.0005	−0.0166*	0.0008	0.0029	0.0033	0.0116
	(0.0116)	(0.0089)	(0.0132)	(0.0126)	(0.0093)	(0.0138)
Number of observations	19,608	27,254	50,988	23,358	29,741	54,738

Notes: * = statistically significant at 10% level. [a]Model 1 also includes constant term, dummy variables for residence in state with full minimum wage increase and time period 2, and an interaction term (time × min wage state). Minimum wage effect is measured by interaction term. Model 2 adds dummy variables for gender, race, and Hispanic ethnicity. Model 3 adds state fixed effects. [b]Sample also includes males age 30–49 with at least some college, residing in states with full minimum wage increase. DIDID model also includes the corresponding sample in states with no increase in minimum wage.

presentation, the table presents just the minimum wage employment effect estimate.[18] Note that the estimates in the first row are identical to those presented in Tables 4 and 5, exactly as they should be.

While we do not present the coefficients for the covariates, those estimates are consistent with other analyses of how race, gender, and ethnicity affect employment rates. For teens, males are slightly less likely to be employed than females, while blacks and Hispanics are considerably less likely to be employed than whites. The Hispanic effect varies across the estimation method more than the other two; it is more negative between states than either within states or in the DIDID model.[19]

For teens, control for individual characteristics (row 2) makes the small, negative between-state estimate of the impact of the minimum wage smaller in

18 The full results are available upon request.

19 The Hispanic coefficient in the between-state model measures the impact across all states for the sampled group (e.g. teens), while the corresponding within-state estimate measures the impact for a different age and geographic sample. Thus, the estimates could well be different.

absolute value, increases the within-state estimate enough to make it statistically significant at the 10% level, and leaves the DIDID estimates very small and with very low statistical significance. The further control for state fixed effects has almost no effect on the estimates. The between-state estimate changes from very small and negative to very small and positive, while the within-state estimate barely changes. The DIDID estimate, which we regard as the most reliable, is positive, very small, and has very low statistical significance.

The results are quite similar for the sample of adults with low education. All the estimated effects in Table 6 are small, positive, and statistically insignificant. We find absolutely no evidence that the 2009 minimum wage increase affected the employment of this group.

Finally, we re-estimated the regression models in Table 6, adding control for the average unemployment rate in February and March, 2009 (results not shown in Table 6). The unemployment rate measure is always negative and statistically significant, but it barely affects the DID estimates. For example, the between-state estimate for teens in model 2 drops from −0.0033 with no control for the unemployment rate to −0.0015 with control; the within-state estimate changes from −0.0170 to −0.0171, and the DIDID estimate changes from −0.0003 to 0.0007. Statistical significance does not change for any estimates. The estimated coefficient also decreases in absolute value for adults with less than a high school degree. No conclusions are altered by the inclusion of this measure of the unemployment rate.

4.4 Hours of work effects

It is possible that the negative employment effects of the minimum wage could be manifested in terms of hours of work for employed workers rather than employment rates. That possibility is addressed in Table 7. Panel A shows the average hours of work for the same demographic groups considered above, while Panel B derives the various DID and DIDID estimators. The row labeled DID_W (C) is what we have previously called the pseudo within-state DID estimate. We include it to show the calculation of the DIDID estimates.

For both groups of prime-age educated males, hours of work were essentially unchanged, so the between-state DID effect shown in the first row of Panel B is less than one-tenth of an hour. This result is quite consistent with what we saw in Table 4, where employment rates were also little changed. Both groups of teens had a modest increase in average work hours, with a slightly smaller increase in the states where the minimum wage increased. Thus, the between-state DID estimate shown in the first row of Panel B is −0.30 hours, but the within-state

Table 7: DID and DIDID estimates of impact of 2009 minimum wage increase on work hours of selected groups of workers

	Male, age 30–49, at least some college	Age 16–19 (no college)	Not HS grad. (age 20–59)
A. Mean hours worked			
Treatment states			
Before	42.71	21.36	36.49
After	42.52	21.90	35.68
Control states			
Before	42.00	19.11	34.99
After	41.90	19.95	35.03
B. Estimates			
DID_B	−0.09	−0.30	−0.84**
	(0.37)	(0.44)	(2.05)
DID_W (*T*)	x	0.74	−0.62*
		(1.51)	(1.93)
DID_W (*C*)	x	0.94**	0.13
		(2.15)	(0.47)
DIDID (*T*–*C*)	x	−0.21	−0.75
		(0.29)	(1.56)
Elasticity	x	−0.09	−0.19

Source: CPS, February/March and November/December 2009.
Notes: Sample includes individuals in states with full increase in minimum wage or with no increase in minimum wage. *t*-Statistics shown in parentheses. ** = statistically significant at 5% level; * = statistically significant at 10% level.

estimate in the second row (comparing hours of work changes for teens in states where the minimum wage increased to those for prime-age educated adults in the same states) is 0.74 hours. Because hours of work for teens in states with no increase in the minimum wage increased by more relative to prime-age males than it did in the states with an increase (0.94 v 0.74–see rows 2 and 3 of Panel B), the DIDID estimate of the impact of the minimum wage on the work hours of teens is −0.21 (Panel B, row 4). Equivalently, hours of work for teens in states with an increase in the minimum wage fell by more relative to teens in states with no increase than did hours of work for prime-age educated males relative to their peers. The negative hours of work effect for teens is small – elasticity of −0.09–and not statistically significant. This result is largely consistent with the findings in Tables 4–6.

Finally, for adults with less than a high school education, we do find some evidence of negative effects not found in our analysis of employment rates.

Average hours of work fell by about 0.80 hours in states with an increase and were essentially unchanged in the states with no increase. Both the between-state and the within-state DID estimates are negative and statistically significant at about a half to three-quarters of an hour. The DIDID estimate is −0.75, which is nearly statistically significant at the 10% level. This estimate implies an hours of work elasticity of −0.17.

5 Summary and discussion

In this paper, we took advantage of the existence of state laws that in many cases established a legal minimum that exceeded the new mandated 2009 federal minimum wage of $7.25. We paid particular attention to the timing of increases in state minimums to identify states with no increase in the effective minimum between early and late 2009. We examined employment before and after the increase for relatively low-skilled groups residing in states where the minimum wage increased, comparing them to other workers plausibly unaffected by the change in the minimum either across states or within states. We computed both between-state and within-state DID estimates of the impact of the minimum wage increase and then combined the two kinds of comparisons to compute DIDID estimates. This latter estimate provides additional control for differential labor market conditions across states and/or across the demographic groups. Finally, we used regression models to extend the difference models and incorporate the effects of covariates and state fixed effects.

Our estimates are based on a single case study, namely the 2009 increase in the Federal minimum. By 2009, the Federal minimum wage had been increased enough that it covered a more substantial portion of the low-skilled labor market and thus could well have had an impact. We find clear evidence that the minimum wage increase affected the low-skill wage distribution in the states where the minimum increased and that this effect was far greater than in the states that already had a higher state minimum. Using workers in virtually all states, we have the advantage of sample sizes much larger than that are common in some studies, even when we focus on relatively narrow groups defined by both education and age. Our comparison is quite sharply defined: we focus simply on whether the effective minimum wage for workers in a state increased or remained constant over an 8- to 10-month period. As such, our estimates show relatively short-run impacts.

For not-in-college teens, we find mostly small negative effects on employment rates in all approaches. None are statistically significant at conventional levels, although the within-state estimate is close. For less-educated adult workers, we find no evidence of any negative effects on employment; most of our estimates are slightly positive, quite small, and not close to statistical significance. In the DIDID estimations, which we regard as the most reliable, the estimated effects are consistently small and are as likely to be positive and negative. None are statistically significant, despite the large sample sizes.

Our study results are not outliers in the recent research literature; findings of no effects are about as common as findings of effects in the recent literature. We believe that the simplicity of our study design is an asset in interpretation: it is absolutely clear that employment changes did not differ meaningfully between the set of states where the federal minimum increased and those where it did not.

We emphasize that our results are based on a single episode of a minimum wage increase. As a result, our findings do not imply that minimum wages can be raised in general without negative impacts or even that labor demand is not, in general, negatively related to wage rates. We recognize that in other settings and with other legislated increases in the minimum, employment effects might be different.

Appendix

Table 1: Effective state minimum wage rates 2008 and 2009

	7/24/2008	1/1/2009	7/24/2009
Alabama	$6.55	$6.55	$7.25
Alaska	$7.15	$7.15	$7.25
Arizona	$6.90	$7.25	$7.25
Arkansas	$6.55	$6.55	$7.25
California	$8.00	$8.00	$8.00
Colorado	$7.02	$7.28	$7.28
Connecticut	$7.65	$8.00	$8.00
DC	$7.55	$7.55	$8.25
Delaware	$7.15	$7.15	$7.25
Florida	$6.79	$6.79	$7.25
Georgia	$6.55	$6.55	$7.25
Hawaii	$7.25	$7.25	$7.25
Idaho	$6.55	$6.55	$7.25

(*continued*)

Table 1: (*Continued*)

	7/24/2008	1/1/2009	7/24/2009
Illinois	$7.75	$7.75	$8.00
Indiana	$6.55	$6.55	$7.25
Iowa	$7.25	$7.25	$7.25
Kansas	$6.55	$6.55	$7.25
Kentucky	$6.55	$6.55	$7.25
Louisiana	$6.55	$6.55	$7.25
Maine	$7.25	$7.25	$7.25
Maryland	$6.55	$6.55	$7.25
Massachusetts	$8.00	$8.00	$8.00
Michigan	$7.40	$7.40	$7.40
Minnesota	$6.55	$6.55	$7.25
Mississippi	$6.55	$6.55	$7.25
Missouri	$6.65	$6.65	$7.25
Montana	$6.55	$6.55	$7.25
Nebraska	$6.55	$6.55	$7.25
Nevada	$6.85	$6.85	$7.55
New Hampshire	$7.25	$7.25	$7.25
New Jersey	$7.15	$7.15	$7.25
New Mexico	$6.55	$7.50	$7.50
New York	$7.15	$7.15	$7.25
North Carolina	$6.55	$6.55	$7.25
North Dakota	$6.55	$6.55	$7.25
Ohio	$7.00	$7.30	$7.30
Oklahoma	$6.55	$6.55	$7.25
Oregon	$7.95	$8.40	$8.40
Pennsylvania	$7.15	$7.15	$7.25
Rhode Island	$7.40	$7.40	$7.40
South Carolina	$6.55	$6.55	$7.25
South Dakota	$6.55	$6.55	$7.25
Tennessee	$6.55	$6.55	$7.25
Texas	$6.55	$6.55	$7.25
Utah	$6.55	$6.55	$7.25
Vermont	$7.68	$8.06	$8.06
Virginia	$6.55	$6.55	$7.25
Washington	$8.07	$8.55	$8.55
West Virginia	$6.55	$6.55	$7.25
Wisconsin	$6.65	$6.65	$7.25
Wyoming	$6.55	$6.55	$7.25

Source: Labor Law Center.com, http://www.laborlawcenter.com/t-State-Minimum-Wage-Rates.aspx and state web sites.

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