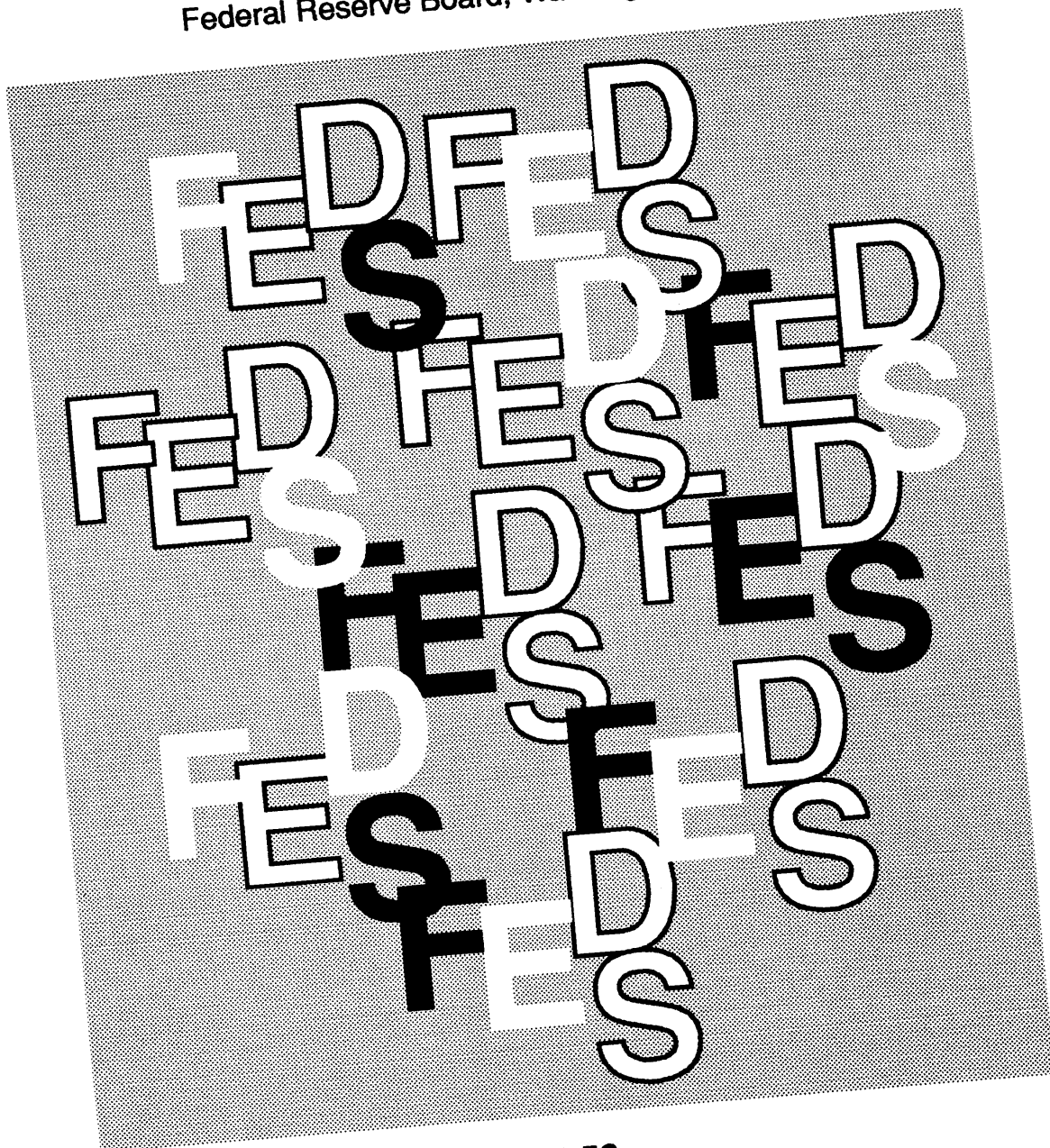


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**RECONCILING THE EVIDENCE ON EMPLOYMENT  
EFFECTS OF MINIMUM WAGES:  
A REVIEW OF OUR RESEARCH FINDINGS**

**David Neumark and William Wascher**

**December 1995**

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**September 1995**

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Abstract

In this paper, we review the evidence that we have accumulated on the employment effects of minimum wages. We point out specific areas of agreement and disagreement between our research and that of others, and where possible, offer our reconciliation of the conflicting results. Our conclusion is that the revisionist view that minimum wages do not reduce employment is not compelling, and that much of the conflicting evidence can be reconciled with the competitive view of minimum wages and low-wage labor markets.

## I. Introduction

Our research on minimum wages now covers seven papers written over a period of about five years. When we were in graduate school, the Minimum Wage Study Commission had recently completed a five-volume study of the employment effects of minimum wage laws. The authors of the papers included in the study constituted a "Who's Who" in labor economics, and the breadth of the papers suggested that there was little more to be learned about this issue; indeed, for a number of years new studies of the minimum wage were virtually nonexistent. Then, in the late 1980s, there was a series of state-level increases in minimum wages, superseding the federal minimum of \$3.35 (and later \$4.25). Given the interest in regional variation in many other areas of labor economics, and given that nearly all of the earlier work on minimum wages was based on time-series evidence, it seemed fruitful to exploit the new state-level variation in minimum wages to re-examine evidence on the employment effects of minimum wages in low-wage labor markets.

In our first paper on this topic (Neumark and Wascher, 1992), we used state-year observations from 1973 to 1989 to estimate minimum wage effects from the type of employment regressions used in the existing time-series literature. For both teenagers (aged 16-19) and young adults (aged 16-24), we concluded that most of the evidence indicated elasticities of employment with respect to minimum wages in the range -0.1 to -0.2, consistent with much of the earlier time-series evidence.<sup>1,2</sup> We also found that subminimum wages for youths moderated the disemployment effects of minimum wages.

We were, however, not the only researchers to think of exploiting the increases in state minimum wage rates in the late 1980s. David Card wrote two papers, one studying the increase in California's minimum wage in 1988 (Card, 1992b), and another--very close in spirit to our paper--that exploited the variation in changes in the minimum wage across states associated with the

increase in the federal minimum in 1990, at a time when state minimum wages varied greatly (Card, 1992a). In contrast to our findings, Card concluded that minimum wage increases caused no adverse employment effects for teenagers. Later, in perhaps the most influential of the "revisionist" minimum wage studies, Card and Alan Krueger (1994) found that employment at fast-food restaurants grew relatively faster in New Jersey than in Pennsylvania following the 1992 increase in the New Jersey minimum wage.<sup>3</sup> In addition, Card and Krueger, along with Lawrence Katz, wrote a detailed comment on our 1992 paper, claiming that "a corrected analysis of Neumark and Wascher's data shows that state-specific minimum wage increases during the 1970s and 1980s had no systematic effect on teenage employment" (Card, et al., 1994, p. 496).

Given this array of evidence against the prediction that minimum wages reduce employment, it might have been the safest course to pack up our bags (or at least our computers), and turn our attention to other issues. However, as our reply to the Card, et al., comment indicated, we were confident that we had in fact gotten most of the answers right in our 1992 paper (Neumark and Wascher, 1994a). More importantly, the "revisionist" results were quite provocative given the unanimity of the earlier research on this topic. Although the inconsistency of the revisionist minimum wage studies with the competitive model of labor demand led some researchers to consider alternative theories of the low-wage labor market,<sup>4</sup> we were of the opinion that empirical labor economists needed to conduct additional research before the new evidence on minimum wages could be embraced and the competitive model dismissed.

Consequently, our subsequent research on minimum wages has focused on the data and empirical approaches used in the recent research, to attempt to uncover the sources of divergent results, and, if possible, to arrive at a consistent picture of low-wage labor markets and the effects of minimum wages on the individuals in these markets. In our view, this research effort has met

with some success. By extending the usual methodology to account for the possibility that minimum wages may be non-binding in some markets, by accounting for the fact that the teenage group typically studied in research on minimum wages includes both higher- and lower-skilled workers, and by examining more carefully the data used in some of the existing research, we have reached the conclusion that the competitive model of low-wage labor markets, and its prediction for the employment effects of minimum wages, have not been contradicted by the revisionist evidence.<sup>5</sup>

In the following sections, we review the evidence that we have accumulated on the employment effects of minimum wages. We point out specific areas of agreement and areas of disagreement between our research and that of others, and, where possible, offer our reconciliation of the conflicting results. As a review, this paper gives short shrift to many of the issues discussed at length in the individual papers. However, we have tried to highlight the central elements of our methods and our key results, and to provide a sense of the strength of the evidence.

## II. The ILRR Symposium and Subsequent Exchange

The October 1992 issue of the Industrial and Labor Relations Review included a symposium containing three papers that used similar approaches to estimate the employment effects of minimum wages--two by Card and one by us. Our paper applied the standard model used in the time-series literature to state-level panel data on the fifty states and Washington, D.C. for the period 1973-1989. In particular, we estimated regressions of the form:

$$(1) \quad E_{it} = \alpha MW_{it} + R_{it} \beta + \epsilon_{it} ,$$

where  $E_{it}$  is the employment-to-population ratio for a particular demographic group, and  $R_{it}$  is a vector of control variables (including fixed state and year effects).  $MW_{it}$  is a coverage-adjusted

relative minimum wage, constructed as federal coverage for the state, multiplied by the higher of the federal or state minimum wage level, divided by the average wage in the state.

As long as changes in the minimum wage variable are viewed as exogenous,  $\alpha$  identifies the effect of minimum wages on employment. We estimated this equation for teenagers (16-19) and young adults (16-24), and reported results from a lengthy array of alternative specifications and estimators. Subsequent exchanges have focused on two particular specification issues: i) whether to include a school enrollment rate among the control variables  $R_{it}$  (and how to measure it); and ii) the appropriate formulation and specification of the included minimum wage variable. Panel A of Table 1 summarizes the key results from our paper, reporting fixed-effects (within-group) estimates of the elasticity of employment with respect to minimum wages.

There are three findings in Panel A that are most relevant to our subsequent research, and to our attempts to reconcile the disparate research findings. First, a significant negative effect of minimum wages on employment of teenagers only appears in specifications that include the enrollment rate. Second, the overall estimated minimum wage effects are stronger--for both teenagers and young adults--when lagged minimum wages (of one year) are added to the specification. Third, in contrast to the results for teenagers, the estimated disemployment effect for 16-24 year-olds is largely invariant to the inclusion of the enrollment rate.

This combined evidence presents a puzzle. The simple textbook theory, in which workers (at least those of the same age) are homogeneous, would predict that minimum wage effects should be stronger for the less-skilled teenagers than for the (on average) more-skilled young adults. However, this prediction is supported only when the enrollment rate is included. At the time, we interpreted this evidence as suggesting that the model excluding the enrollment rate was probably misspecified, as it seemed difficult to argue that minimum wages have the predicted effect for the

young-adult group as a whole, but not for teenagers. We therefore concluded from these results that minimum wages reduce employment, with elasticity estimates in the range -0.1 to -0.2. In some of our later research, however, we explained how the results for specifications excluding the enrollment rate--indicating net disemployment effects for young adults but not teenagers--could be reconciled with the competitive model in which some low-wage workers lose their jobs when minimum wages increase; we will return to this issue in Section III.

The failure to find evidence of a negative minimum wage effect for teenagers in specifications excluding the enrollment rate is consistent with both of Card's studies in the ILRR symposium (Card, 1992a, 1992b). Thus, we do not think there is much controversy in the recent research over whether the "reduced-form" employment effects for teenagers (i.e., those that do not control for shifts in enrollment) are small or near zero, when such employment effects are estimated from CPS data. More controversial, however, is the question of whether to put more stock in the estimates controlling for enrollment rates.

In their comment on our paper, Card, et al. (1994) criticized our inclusion of the enrollment rate--which is a supply variable--in the employment equation, which they argued should be interpreted as a labor demand function. However, as Charles Brown, Curtis Gilroy, and Andrew Kohen pointed out in their 1982 paper, if there is an uncovered sector or if some workers earn above the minimum wage, then aggregate employment is determined by both supply and demand. Card, et al., also argued that the enrollment measure we used, which counted only individuals who were in school and not employed, generated a spurious negative correlation between employment and enrollment rates that led to biased results. Although we believe that this criticism was overly strong,<sup>6</sup> we agree that it might be better to use an enrollment rate that also includes employed individuals. Therefore, in our reply to their comment (Neumark and Wascher, 1994a), we also

included an enrollment measure based on a separate question included in the CPS. Estimates using this measure are reported in Panel B of Table 1. For teenagers, the estimated minimum wage effect (-0.11) is smaller and no longer significant, while for young adults the estimated effect (-0.16) is essentially unchanged, and remains statistically significant. Thus, for teenagers the statistical strength of the conclusions is sensitive to the enrollment rate used, although the estimated elasticity is still in the range (-0.1 to -0.2) that we reported.

Of course, the potentially more important problem with the enrollment rate--as we noted in our 1992 paper--is that school enrollment is a decision that may be affected by minimum wages and employment opportunities, and so may be endogenous. This led us, in our reply to Card, et al., to compute IV estimates of equation (1) using data on school expenditures, pupil-teacher ratios, and compulsory schooling laws as instruments, to control for variation in enrollment unrelated to labor market conditions. These estimates, which are reported in Panel C of Table 1, show stronger disemployment effects, although they again are not significant for teenagers using the less restrictive enrollment definition.<sup>7</sup> These results reinforced our view that minimum wage effects are negative for both teenagers and young adults, and motivated us to investigate further the question of the determination of employment and enrollment in response to minimum wages in later research (discussed in the next section).

In addition to the question of whether the enrollment rate should be included, Card, et al. raised two other issues that, they claimed, invalidated our finding that minimum wages reduce employment. First, they criticized our use of a relative minimum wage measure (that is, a measure based on the ratio of the minimum wage level to the average wage in the state).<sup>8</sup> In particular, they claimed to show the inappropriateness of using a relative minimum wage measure by demonstrating that the correlation between the relative minimum wage measure and the average wage of teenagers

is negative, while arguing that if minimum wages increase the relative price of teen labor, the correlation should be positive. They then re-estimated our equation using the log of the nominal minimum wage level in place of the relative minimum wage variable, and found a positive minimum wage effect (significant when the enrollment rate was excluded).

However, this argument ignores the influence of shifts in the nominal level of wages relative to a fixed minimum. Clearly, parallel movements of nominal wages of teens and older workers--due to inflation, for example--will induce a negative correlation between the relative minimum wage variable and average teen wages.<sup>9</sup> The more important point is that nominal wage increases given a fixed nominal minimum wage should act in a similar manner (with the opposite sign) to increases in the nominal minimum wage, as the minimum wage becomes binding for fewer and fewer workers, and the real value of the minimum wage falls. The relative minimum wage measure captures this effect.<sup>10</sup>

Moreover, there are important theoretical reasons for using a relative minimum wage measure rather than the nominal minimum wage. First, we want a measure of the real wage, and the average wage is a good substitute for a price index, which is unavailable at the state level. Second, the relative price of teen (or young-adult) labor to that of other labor is likely to be the price to which demand for young workers is most sensitive.<sup>11</sup>

The other issue they raised with respect to the specification of the minimum wage variable was the inclusion of lagged effects, which the results in Panel A of Table 1 (and other results in our 1992 paper) suggest are important. Because Card's (1992a) paper on regional variation in minimum wages used only a one-year first-differenced version of equation (1), we were interested in whether the absence of significant minimum wage effects in his results was attributable to the failure to allow more time for minimum wages to have an effect. We first noted that if we followed

Card's procedure and estimated a one-year first-differenced equation rather than using the within-group estimator, the estimated minimum wage effect on employment was positive or zero using our data, even when the enrollment rate was included. These estimates are reported in columns (1) and (2) of Panel D of Table 1.

However, we then showed econometrically that the positive bias from omitting a lagged minimum wage effect in the one-year first-differenced equation was much more severe than in our within-group estimates. This result was also borne out empirically. When lagged minimum wages were included in either our estimated equation (reported in Panel A of Table 1), or in the one-year first-differenced equation (columns (3) and (4) of Panel D), the results consistently indicated negative employment effects, especially when the enrollment rate was included. Moreover, the estimated coefficient of the lagged minimum wage variable, itself, was often statistically significant in these specifications. Card, et al. also took issue with our suggestion that Card (1992a) did not find negative employment effects of minimum wages because he ignored lagged effects. To re-examine this question, Card, et al. reported estimates of a two-year, as opposed to one-year, first-differenced version of equation (1), and showed that these estimates, also, indicated slight positive effects of minimum wages. However, in our reply we pointed out that estimating a two-year differenced equation is not the same as including a one-year lag of the minimum wage, since the two-year differenced equation still has an omitted lagged effect, which, in our data, is negative. Thus, the evidence they offered does not really address the bias caused by omitting lagged minimum wages.

Because there is no a priori reason to rule out minimum wage effects taking a year or more to affect employment, we see no reason to be guided by anything other than the evidence on this question, which indicates that we should include lagged minimum wages in the employment

equation.<sup>12</sup> The failure of other minimum wage studies--using different experiments or different data sets--to allow a sufficiently long time for inputs to adjust may therefore influence the conclusions these studies reach.<sup>13</sup>

### III. Minimum Wage Effects on Employment and Enrollment

Our 1992 paper was intended to "check" whether the time-series elasticity estimates of -0.1 to -0.2 held up in state-level panel data. Based on the results in that paper, and those that came out of the subsequent exchange with Card, et al., we reached the following conclusions. For young adults aged 16-24, the estimated elasticities are in this range regardless of whether an enrollment rate is included. However, for teenagers the results are less robust. When enrollment is included, the estimated minimum wage effects are negative, although sometimes insignificant.<sup>14</sup> But when enrollment is excluded, the elasticity estimates are close to zero.

The fact that the estimated minimum wage coefficient for teenagers becomes more negative when the enrollment rate is included implies that the enrollment rate is negatively associated with the minimum wage variable, suggesting that minimum wage increases reduce school enrollment. Moreover, in our reply to Card, et al., we reported that the sum of the employment and enrollment rates was negatively related to the minimum wage, suggesting that "idleness" (being neither employed nor enrolled) increased in response to minimum wage increases. These findings suggested that minimum wage increases induce shifts in enrollment and labor force status of teenagers, shifts that are obscured by an exclusive focus on the net employment effects of minimum wages.

Our first paper on this topic (Neumark and Wascher, 1995a) documented more explicitly the consequences of minimum wages for employment, enrollment, and idleness, focusing on reduced-

form effects of minimum wages on each of these. We continued to use state-level data, but computed conditional logit estimates of the effects of minimum wages on the proportions of teenagers in each of four possible enrollment/employment activities. Panel A of Table 2 reports the results of this analysis.<sup>15</sup> The first row reports the elasticities, and, for ease of interpretation, the third row translates this into the minimum wage effects on the proportion in each category, using the 21 percent minimum wage increase recently proposed by the Administration.

There are two statistically significant effects of minimum wage increases: first, the proportion in school and employed declines, with an elasticity of -0.47 with respect to the minimum wage; second, the proportion idle (not in school and not employed) increases, with an elasticity of 0.64. These two elasticities are much larger than the elasticity of net employment (the proportion in school and employed plus the proportion not in school and employed) with respect to the minimum wage, which is -0.15. The larger shifts in the proportion in school and employed and in the proportion idle indicate that minimum wage increases induce employment and enrollment shifts among teenagers that are larger than is suggested by the net employment effects.

We argued that the results were consistent with a competitive model of the labor market in which teenagers have heterogeneous skills, and in which workers at different skill levels are not perfect substitutes. In particular, we speculated that a higher minimum wage, by increasing the relative price of the least-skilled teenagers, increases the relative demand for enrolled higher-skilled teenagers, bidding up their market wages and inducing some of them to leave school for employment. These higher-skilled teenagers then displace lower-skilled teenagers employed at or near the old minimum, many of whom were not enrolled. Finally--although not necessarily predicted by theory--these displaced teenagers choose not to return to school, thereby ending up idle.<sup>16</sup> Depending on the ease of substituting higher- for lower-skilled teenagers, the net effect on

employment of teenagers as a group might be quite small. In this scenario, however, even when the net employment effect is small there are still important effects of minimum wage increases on teenagers. We doubt that a minimum-wage-induced decline in enrollment of teenagers or displacement of lower-skilled teenagers already in the labor market would be viewed as desirable by most economists.

In subsequent research (Neumark and Wascher, 1995b and forthcoming), we examined these hypotheses more directly by using matched CPS data to create two-year panels in which we could observe individual teenagers' enrollment/employment activities before and after minimum wage increases. Using a multinomial logit model for enrollment/employment status that conditions on the relative minimum wage, lagged enrollment/employment status, as well as other control variables including state and year effects, we estimated the effects of minimum wages on transitions among these alternative activities. The first set of results using the individual-level data, reported in Panel B of Table 2, confirm those found in the aggregate data. Minimum wages significantly reduce the probability that teenagers are in school and employed and significantly increase the probability that they are idle.

More importantly, the results reported in the first row of Panel C confirm the substitution or displacement hypothesis. Minimum wage increases lead some teenagers to switch from in school and employed to not in school and employed, with a 21 percent minimum wage increase estimated to increase the probability of making this transition by 0.03. These workers also generally increase their hours of work. This explains why some teenagers already in the labor market are displaced. In particular, a 21 percent minimum wage hike raises the probability of moving from not in school and employed to idleness by 0.02.

If minimum wage increases cause the displacement of less-skilled teenagers from the labor

market, we should find that those teenagers who leave school to work full-time are more skilled than those who are displaced from the labor market. The last four rows of Panel C provide evidence consistent with this implication. We first disaggregate by race, conjecturing that minority teenagers are, on average, either lower-skilled or perceived as such by employers. Consistent with this conjecture and our hypothesis regarding minimum wage effects, we find that only for non-black, non-Hispanic teenagers do minimum wage increases significantly increase the probability of switching from in school and employed to not in school and employed. On the other hand, only for blacks and Hispanics do we find significant displacement from the not in school and employed category to idleness. The 0.04 effect for blacks and Hispanics represents a 29 percent increase in the probability that blacks and Hispanics become idle.

Another way to disaggregate by skill is to compute separate estimates for higher-wage and lower-wage workers.<sup>17</sup> The last two rows of Panel C report estimates disaggregated by whether an individual's lagged wage was at or above the current year's minimum wage, or below it. An individual whose lagged wage is at or above the current minimum should be more likely to have their wages bid up due to a higher current minimum wage, and therefore to leave school to work full-time. In contrast, an individual whose lagged wage is below the current minimum is more likely to be priced out of the labor market as a result of a higher current minimum wage, and therefore should be more likely to be displaced from not in school and employed to idleness (or, in principle, to full-time schooling). These are the results we obtain, although the statistical evidence is weaker, perhaps owing to the much smaller sample size. Among the higher-wage teenagers, a 21 percent minimum wage increase is estimated to increase the probability of switching from in school and employed to not in school and employed, by 0.04 (although this effect is not significant). Among the lower-wage teenagers, the same minimum wage increase significantly raises the

probability of going from not in school and employed to idleness, by 0.04. The results are similar if we focus only on those at or above the lagged minimum wage, who are more likely to be covered by minimum wage laws. Thus, the results are again consistent with minimum wage increases leading to the displacement of lower-skilled workers from the labor market.<sup>18</sup>

Our examination of minimum wage effects on employment and enrollment using microdata leads to a substantial refinement of our earlier results on the effects of minimum wages on employment of teenagers. The evidence from our 1992 paper, Card's papers in the same ILRR volume, and our subsequent exchange with Card, et al., suggested that minimum wage increases have little net employment effect on teenagers. If workers were homogeneous, and teenagers were less-skilled than young adults, then it would be surprising to find little employment effect for teenagers, yet a significant disemployment effect for young adults. However, recognizing that workers are heterogeneous, this apparent puzzle can be explained. Among teenagers, the potentially more-productive workers are more likely to be enrolled in school. In addition, for those on the margin between school and work there appears to be a sizable supply response to wage increases for more-skilled labor stemming from minimum wage increases. Thus, a higher minimum wage leads to an increase in the market wage for higher-skilled teenagers, drawing them out of school and into the labor force. These workers then displace lower-skilled teenagers who are priced out of the market, with little net employment effect (although still negative, as suggested by the point estimates).

This same perspective may also explain why, for young adults (aged 16-24) as a whole, disemployment effects are stronger in standard employment regressions. Among the older individuals in this age group, the potential for a supply response among more-skilled individuals is likely to be much lower. That is, with the initial employment rate considerably higher (and the

initial school enrollment rate lower), there is a smaller pool of higher-skilled workers who might enter the labor market in response to higher wages. Thus, the disemployment effects on lower-skilled young adults show through more clearly in the aggregate.

The results also help to explain why, for teenagers, controlling for enrollment shifts in estimates of equation (1) leads to negative employment effects. When minimum wages increase, enrollments--which are negatively correlated with employment--decline. When we control for enrollment shifts, we effectively hold enrollments constant, which reduces employment. But it is only because enrollment rates shift that employment of teenagers is left largely unchanged as a result of minimum wage increases. Thus, while the most relevant regression from the perspective of estimating an overall employment effect for teenagers may be the reduced-form regression for the employment rate, this regression masks important effects on school enrollment rates of teenagers and on employment rates of particular subgroups of teenagers.

Finally, the evidence that the disemployment effects of minimum wages fall largely on less-skilled workers implies that the conventionally-reported elasticities of employment with respect to minimum wages are misleading. An elasticity of -0.1--which may be the consensus elasticity at present--is generally interpreted to mean that a ten-percent increase in the minimum wage reduces the employment rate of teenagers by one percent. While strictly correct, there is a sense in which this compares apples to oranges. For the most part, a minimum wage increase raises the wages only of lower-wage workers; however, this same group experiences most of the employment decline. The conventional elasticity, then, is effectively the ratio of the employment decline averaged across all workers to the wage increase among lower-wage, less-skilled workers. A more relevant alternative elasticity might be the ratio of the employment decline among lower-wage workers to the wage increase among lower-wage workers (i.e., the minimum wage increase), or

similarly, the ratio of the employment decline averaged across all workers to the wage increase averaged across all workers. Either alternative would exceed the conventional elasticity, which downplays the stronger disemployment effects for those workers whom minimum wages are intended to help.

#### IV. When Do Minimum Wages Matter?

The previous section considered the implications for empirical research of going beyond the simplest textbook minimum wage model by recognizing heterogeneity of workers. Another facet of the model that is often ignored in empirical research is whether minimum wages are binding. In the textbook competitive model of low-wage labor markets, only when the minimum wage is higher than the equilibrium competitive wage should it be negatively related to employment; a legislated minimum wage below the equilibrium competitive wage should not affect employment. Thus, studies that use samples that vary with respect to whether minimum wages are binding may obtain different findings. For example, even if low-wage labor markets are competitive, some studies may reveal no effects of minimum wages on employment because they use samples in which most of the variation in minimum wages is in the range for which minimum wages are non-binding.

To estimate minimum wage effects that are not influenced by the extent to which minimum wages are binding, we have borrowed methods from the disequilibrium labor market literature to estimate these effects only from observations for which minimum wages are likely to be binding (Neumark and Wascher, 1994b). In particular, we implement an endogenous switching regression model with unknown sample separation, with the switch point defined as the intersection of the labor demand and supply curves. In this model, observations are either on the labor demand curve (the binding regime) or at the equilibrium level of employment (the non-binding regime, given by

the reduced-form employment equation). In addition to providing more reliable estimates of minimum wage effects, this approach enables us to ask whether variation in the extent to which minimum wages are binding helps to explain why we sometimes fail to find disemployment effects of minimum wages. Thus, the disequilibrium approach may provide another explanation for the absence of a negative correlation between employment rates and minimum wages in some studies, and hence prove useful in assessing the evidence from various minimum wage experiments.

In Panel A of Table 3 we report the estimated minimum wage effect and implied elasticity from the disequilibrium model, using state-level data from 1973-1989 for young adults (aged 16-24).<sup>19</sup> The estimated effect (-0.33) is negative and significant, and more than twice as large as the estimate from the corresponding "single-equation" estimate (-.14, not reported in the table).<sup>20</sup> This estimate implies that when minimum wages are binding, the elasticity of employment with respect to the minimum wage is -0.21.

The model estimates also can be used to estimate the probability that observations are on the alternative regimes, with some representative estimates shown in column (1) of Panel B. In addition, column (2) presents the average probability-weighted elasticity of employment with respect to minimum wages, computed as the average across observations of the standard elasticity multiplied by the probability that the minimum wage is binding for an observation. For the full sample, as the first row of Panel B shows, the average probability that the minimum wage is binding is 0.48, and the average probability-weighted elasticity is -0.11.

The remaining rows of Panel B provide evidence on the variation in the probability that minimum wages are binding (and variation in the elasticities) across years and regions. For example, in 1981, after the minimum wage rose to \$3.35, the estimated probability that minimum wages were binding was 0.67. But by 1989, with the federal minimum wage stagnant (although

some states had raised their minimums), this probability fell to 0.29. Similarly, there is considerable variation across regions. For example, in the low-wage South Central states the estimated probabilities that minimum wages are binding are higher than in the high-wage Mountain and Pacific states.

This variation in the probability that minimum wages are binding, along with the difference between the estimated minimum wage effect on the binding regime and the single-equation estimate of the minimum wage effect for the whole sample, suggests that standard single-equation regression estimates of minimum wage effects may be quite sensitive to the extent to which minimum wages are binding in a particular sample. The estimates reported in Panel C document this sensitivity in our data. The first two rows divide the sample based on whether the estimated probability that the minimum wage is binding is above or below 0.5. For the subset of observations with a relatively high probability that the minimum is binding, we obtain a significant negative estimated minimum wage effect. In contrast, for those observations with a low probability that minimum wages are binding, we obtain a significant positive effect.<sup>21</sup> A similar result is reflected in the next two rows of Panel C, where we report single-equation estimates for the states with the highest and lowest estimated probabilities that minimum wages are binding. For the former, the estimated minimum wage effect is significant and negative, and for the latter, the estimated minimum wage effect is significant and positive. These estimates strongly suggest that studies may conclude erroneously that minimum wages do not reduce employment if the samples that they use are heavily weighted toward observations for which minimum wages are relatively less likely to be binding.<sup>22</sup>

Panel D of the table reports the minimum wage effect from the disequilibrium model estimated for teenagers, who have been the focus of most of the recent research on minimum wages. The estimated disemployment effect when minimum wages are binding is  $-.34$ , and is

significant at the ten-percent level. The implied elasticity is -0.29, somewhat larger than the elasticity for the broader young-adult group. As the second row of the panel shows, the average estimated probability that minimum wages are binding for teenagers is 0.50, and the average probability-weighted elasticity is -0.16.

Finally, to provide some idea as to how variation in the probability that minimum wages are binding may affect results from other studies, the last two rows report the estimated probability that minimum wages were binding for teenagers in California immediately before and after the 1988 minimum wage increase studied by Card (1992b).<sup>23</sup> Before the minimum wage rose, the estimated probability that the minimum wage was binding was 0.004, with an implied elasticity of essentially zero. After the minimum wage rose, the probability rose to 0.25--one-half the average probability for the sample as a whole--with a probability-weighted elasticity of -0.07. This suggests that, even after the 1988 minimum wage hike in California, the minimum wage did not have much "bite."

In sum, the application of disequilibrium models of the labor market to estimating minimum wage effects helps to address an important shortcoming of the more traditional single-equation approach. In particular, the competitive model with homogeneous labor implies that minimum wage effects will only be evident when minimum wages are binding. This, in turn, suggests that the estimated minimum wage effects from single-equation reduced-form regressions will be sensitive to the sample used. In one sense, this is unfortunate, as it implies that studies of minimum wage effects based on relatively narrow samples may be uninformative as to general questions regarding the effects of minimum wages on employment. On the other hand, this is exactly what the theory predicts.

## V. The New Jersey-Pennsylvania Minimum Wage Study

Our research described in the preceding sections questions the basic approach of estimating minimum wage effects from simple regressions of employment on minimum wages, and shows that approaches that are better integrated with the competitive model of low-wage labor markets lead to evidence more consistent with this model. In contrast, our most recent paper (Neumark and Wascher, 1995c) raises questions about the data used, and the conclusions reached, in what is probably the most influential "revisionist" study: Card and Krueger's (1994) study of fast-food establishments in New Jersey and Pennsylvania before and after the minimum wage in New Jersey rose from \$4.25 to \$5.05. Card and Krueger's experiment is to conduct a "differences-in-differences" comparison of relative employment changes in New Jersey and Pennsylvania, to test the prediction that minimum wage increases reduce employment of affected workers. Contrary to this prediction, they find "no evidence that the rise in New Jersey's minimum wage reduced employment at fast-food restaurants in the state ..." and even that "the increase in the minimum wage increased employment ..." (p. 792). However, we have uncovered serious problems with their data, and have found that more reliable data lead to the opposite conclusion.

The most obvious problem is indicated by comparing the variability of employment change in Card and Krueger's data, which were obtained from telephone surveys, and in our data, which were obtained from administrative payroll records supplied by franchisees or parent corporations.<sup>24</sup> In particular, there are some extremely large employment changes in Card and Krueger's data. For example, in Pennsylvania the 90th centile of the employment change distribution is 11.8 workers, while the 10th centile is -17.5, for a 90th-10th centile difference of 29.3; in New Jersey the 90th-10th centile difference is 18.5 workers. This variability of employment change seems inordinately high given that the mean level of employment in Card and Krueger's survey was 21. In contrast, in

the payroll data, the 90th-10th centile differences are 7.8 in Pennsylvania, and 8.1 in New Jersey.

Our sense is that the excess variability in Card and Krueger's data is due to the imprecise nature of their survey questions eliciting the employment levels. The interviewer first verified that they were speaking with a manager or assistant manager, and then asked "How many full-time and part-time workers are employed in your restaurant, excluding managers and assistant managers?" Survey respondents were not given any time period over which to define employment, and their answers may well have ranged from employment on the shift during which the telephone survey took place, to employment over an entire payroll period. Moreover, because different managers may have been interviewed in the two waves of the survey, there is no reason to believe that the responses in the first and second waves are based on the same "definition" of employment. In contrast, the payroll data are based on data submitted by restaurant owners on the total number of hours worked by non-management employees on a weekly, bi-weekly, or monthly basis, which we converted into weekly full-time equivalent employees (FTE's) assuming a full-time workweek of 35 hours. Thus, the payroll data measure employment for a well-defined payroll period, on a consistent basis for the two survey periods.

The apparent differences in the quality of data from the two sources have substantive consequences for the differences-in-differences estimates of the effect of the New Jersey minimum wage increase. Card and Krueger's data indicate that, on average, over a period of about eight months following New Jersey's minimum wage increase, employment at fast-food restaurants in New Jersey grew by 3.39 more full-time equivalents (FTE's) than in Pennsylvania, a difference that is statistically significant and implies an elasticity of employment with respect to the minimum wage of 0.93.<sup>25</sup> In contrast, the payroll data indicate a statistically significant relative employment decline in New Jersey of 0.76 FTE's, implying an elasticity of -0.24. This latter evidence is

consistent with the findings from our other research, and with the prediction of the competitive model of low-wage labor markets.

## VI. Conclusions

In the course of presenting our research on minimum wages, it was frequently suggested to us that evidence of non-negative or even positive employment effects of minimum wages simply could not be correct, because it was inconsistent with the theory. While we certainly believe that the competitive theory of labor demand should not be discarded capriciously, we also share the perspective--with our opponents in this debate--that the ultimate test of the theory is the data, and we should stand ready to modify or replace a theory if it is convincingly rejected. We differ with them, however, in our assessment of whether the theory has been rejected. As the preceding review of our research makes clear, we do not think that the recent research on minimum wages has yielded convincing evidence that is inconsistent with the competitive model of labor markets and its prediction that--under some circumstances and for particular groups of workers--raising the minimum wage will reduce employment.

We think it is safe to say that the best-known study reporting evidence against the competitive model is Card and Krueger's New Jersey-Pennsylvania study of the fast-food industry, which suggests that raising the minimum wage resulted in higher employment. However, we have shown that a more reliable data source for this experiment indicates that relative employment fell in New Jersey following that state's minimum wage increase.

Card's studies of the California minimum wage increase in 1988 and the federal increase in 1990, and our study of federal and state minimum wage increases over a longer period, fail to find evidence of negative reduced-form effects of minimum wages on employment of teenagers,

although we find negative effects for the broader group of 16-24 year olds. However, our research has identified two potential explanations of these findings, both of which are consistent with the competitive model.

First, minimum wages are not always binding. When samples are comprised of observations in which minimum wages are relatively unlikely to be binding, we do not find disemployment effects, as theory would predict. However, for observations on states and years in which minimum wages are likely to be binding, they reduce employment of teenagers and young adults.

Second, minimum wage increases induce substitution of higher-skilled for lower-skilled teenagers. That is, such increases reduce the employment of low-skilled teenagers, but increase the supply of higher-skilled teenagers, whose wages are bid up owing to the increased price of low-skilled labor. This supply response moderates the total disemployment effect, and there is no reason to believe that the total effect should be significantly negative.

Thus, our provisional conclusion from the new minimum wage research--our own included--is that the revisionist evidence is not compelling, and that much of the conflicting evidence can be reconciled with the competitive view of minimum wages and low-wage labor markets. At the same time, we should emphasize that we do not claim to have reconciled all of the existing research findings. With the possible exception of the New Jersey-Pennsylvania study, we have not conclusively demonstrated that these studies obtained the wrong answer because of problems with either methods or data. Rather, we have offered hypotheses and supporting evidence suggesting why these studies might have failed to confirm the conventional view of minimum wages, even if that view is correct. But we also think that there is more to be learned about the economics of minimum wages and labor demand from replications of the types of studies on which the revisionist

evidence is based, and from additional testing of our hypotheses regarding the sources of variation in estimated minimum wage effects.

## Endnotes

1. This elasticity refers to the change in employment among all teenagers with respect to a change in the minimum wage, rather than the change in employment among minimum wage workers. We will discuss this difference in Section III of the paper.
2. Other papers in the recent round of research on minimum wages that find disemployment effects of minimum wages include Deere, et al. (1995), Currie and Fallick (1993), Kim and Taylor (1995), and Williams (1993).
3. This paper builds on an earlier study by Katz and Krueger (1992) studying the effect of the federal minimum wage increase in 1990 on the fast-food industry in Texas. Other papers in the recent round of research that find little or no effect of minimum wages on employment include Spriggs (1992) and Wellington (1991).
4. See Card and Krueger (1995, Chapter 11), Rebitzer and Taylor (1991), Manning (1993), and Lang and Kahn (1995).
5. This contrasts with Card and Krueger's conclusion that their findings "pose a serious challenge to the simple textbook theory that economists have used to describe the effect of the minimum wage" (1995, p. 4).
6. First, Card, et al., claimed that "schooling and work go hand in hand for most teenagers" (p. 488), which we subsequently showed to be false. Second, they claimed that the spurious negative correlation between employment and our enrollment measure arose because our enrollment rate is simply "one minus the employment rate plus some noise" (p. 488). We showed that this assertion also was strongly rejected by the data. See Neumark and Wascher (1994a).
7. These estimates are for 1978-1989. In the original paper, there is a typographical error, with the estimated coefficient on the original enrollment variable for teenagers reported as 0.25, rather than -0.25. Also, we noted in our comment that when we also correct for serially correlated and heteroskedastic errors, the estimated minimum wage effects are negative and significant (at the five- or ten-percent level) for both teenagers and young adults, using either enrollment rate.
8. We also multiplied by an estimate of the coverage rate. A similar relative minimum wage measure--usually referred to as the Kaitz index--was used in virtually all of the earlier research on minimum wages.
9. However, the relative minimum wage variable and the relative (to the overall or adult) wage of teenagers should be positively correlated. In our reply, we showed this to be the case.
10. In contrast, the minimum wage measure advocated by Card, et al., and used in Card (1992a), is the fraction of workers between the previous year's and the current year's minimum wage. We have no question that, following a minimum wage increase, this is a reasonable proxy for the importance of a minimum wage increase. But over a longer period, such a measure is likely to be less relevant, as it ignores the erosion of the real value of a fixed nominal minimum wage due to increases in the general level of wages and prices.

11. We showed in our reply that, in three of four specifications, the data did not reject the specification in which the minimum wage relative to the average adult wage is used.

12. Recent work by Baker, et al. (1994) conducts a careful study of this question, using Canadian data, and confirms that minimum wage effects are felt most strongly about a year after they increase.

13. For example, Hamermesh (1995) criticizes Card and Krueger's (1994) fast-food study for not allowing for the longer-run effects of minimum wages that would be predicted by a model of factor demands with adjustment costs.

14. Evans and Turner (1995) claim that these negative results are attributable to using the major activity in the survey week to define enrollment, rather than an independent enrollment question (available in October CPS's beginning in 1978). However, our reading of their evidence is that the differences in results stem, in large part, from mixing employment and enrollment measures from October CPS's with relative minimum wage variables from May CPS's. At any rate, it is not obvious that we want to control for enrollment not reported as the major activity as a supply shifter. Finally, in our subsequent research we have moved beyond viewing these specifications as the best way to estimate minimum wage effects, as this section of the paper explains.

15. These estimates differ slightly from those in Table 3 of our paper, because we have corrected a minor error in the programs used in that paper. However, the results are qualitatively very similar. We are grateful to William Evans and Mark Turner for pointing out this error.

16. An alternative interpretation is that minimum wage increases reduce enrollments as teens leave school and queue for jobs at the higher minimum, without any displacement of teenagers already employed. We argued that the evidence from earlier research was less consistent with this possibility, although in later work (Neumark and Wascher, 1995b and forthcoming) we report some evidence consistent with queuing.

17. We can do this for those working for a wage who are in the outgoing rotation group of the CPS, about one-ninth of the original matched sample.

18. In Neumark and Wascher (forthcoming) we show that these results are sensitive to the precise sample period used (in particular, the results are not robust when we look at smaller subperiods). However, the results are robust to using an enrollment rate definition based on an independent enrollment question, rather than the major activity in the survey week.

19. In this research, we revert to treating workers within the same state and year as homogeneous. It remains for future research to fully integrate worker heterogeneity with this disequilibrium approach.

20. These results are for specifications using the contemporaneous minimum wage variable only.

21. On the other hand, if we estimate the disequilibrium model for these two subsamples, we should still find negative effects of minimum wages in the binding regime. In Neumark and Wascher (1994b) we show that this is the case.

22. In Neumark and Wascher (1994b), we also estimate a multiple-regime model allowing for monopsony, which is one interpretation of a positive minimum wage coefficient. Alternatively, the positive effects in Panel C of Table 3 may reflect states raising minimum wages that are largely non-binding when youth labor markets are robust.

23. The minimum wage rose in July of 1988, whereas our observations are for May of each year.

24. For the samples discussed here, Card and Krueger's data set contains 385 observations, while ours contains 230 observations. In Neumark and Wascher (1995c) we provide details of the data collection procedure.

25. This estimate is based on the subsample of restaurants (distinguished by chain, ownership, and location) in the universe from which the payroll data were drawn. If we use CK's full sample, and include management employees, the estimated effect is also statistically significant, although the elasticity is slightly smaller (0.70),

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Table 1: Estimates of Elasticities of Employment with Respect to Minimum Wages

Panel A

|                               | <u>Contemporaneous<br/>minimum wage,<br/>enrollment excluded</u><br>(1) | <u>Add<br/>enrollment rate</u><br>(2) | <u>Add lagged<br/>minimum wage</u><br>(3) | <u>Add both</u><br>(4) |
|-------------------------------|-------------------------------------------------------------------------|---------------------------------------|-------------------------------------------|------------------------|
| Teenagers,<br>within-group    | 0.06                                                                    | -0.14**                               | -0.03                                     | -0.19**                |
| Young adults,<br>within-group | -0.07                                                                   | -0.10**                               | -0.18**                                   | -0.17**                |

Panel B: Alternative Enrollment Rate

|                               |     |     |     |         |
|-------------------------------|-----|-----|-----|---------|
| Teenagers,<br>within-group    | ... | ... | ... | -0.11   |
| Young adults,<br>within-group | ... | ... | ... | -0.16** |

Panel C: Instrument for Enrollment Rate

|                               | <u>Original<br/>enrollment rate</u><br>(1) | <u>Alternative<br/>enrollment rate</u><br>(2) |
|-------------------------------|--------------------------------------------|-----------------------------------------------|
| Teenagers,<br>within-group    | -.25**                                     | -.17                                          |
| Young adults,<br>within-group | -.16**                                     | -.12**                                        |

Panel D

|                                               | <u>Contemporaneous<br/>minimum wage,<br/>enrollment excluded</u><br>(1) | <u>Add<br/>enrollment rate</u><br>(2) | <u>Add lagged<br/>minimum wage</u><br>(3) | <u>Add both</u><br>(4) |
|-----------------------------------------------|-------------------------------------------------------------------------|---------------------------------------|-------------------------------------------|------------------------|
| Teenagers,<br>one-year<br>first difference    | 0.20                                                                    | 0.06                                  | -0.12                                     | -0.15                  |
| Young adults,<br>one-year<br>first difference | 0.05                                                                    | -0.01                                 | -0.17                                     | -0.19*                 |

\*\*\* denotes significant at the five-percent level. \*\* denotes significant at the ten-percent level. Elasticities are evaluated at sample means. Sources: Panels A, B, and D--Neumark and Wascher (1992). Panel C--Neumark and Wascher (1994a).

Table 2: Estimates of Minimum Wage Effects on Employment and Enrollment Activities, Teenagers

**Panel A: State-Level Data**

|                                                                          | <u>In school/<br/>not employed</u><br>(1) | <u>In school/<br/>employed</u><br>(2) | <u>Not in school/<br/>employed</u><br>(3) | <u>Not in school/<br/>not employed</u><br>(4) |
|--------------------------------------------------------------------------|-------------------------------------------|---------------------------------------|-------------------------------------------|-----------------------------------------------|
| Elasticities                                                             | -0.02                                     | -0.47**                               | 0.14                                      | 0.64**                                        |
| Mean proportion<br>in category                                           | 0.45                                      | 0.21                                  | 0.23                                      | 0.12                                          |
| Effect of 21 %<br>increase in<br>minimum on<br>proportion in<br>category | -0.006                                    | -0.02**                               | 0.01                                      | 0.02**                                        |

**Panel B: Individual-Level Data**

|                                                                          |       |         |      |        |
|--------------------------------------------------------------------------|-------|---------|------|--------|
| Effect of 21 %<br>increase in<br>minimum on<br>proportion in<br>category | -0.01 | -0.03** | 0.02 | 0.02** |
|--------------------------------------------------------------------------|-------|---------|------|--------|

**Panel C: Individual-Level Data**

|                                                                          | <u>In school/employed to<br/>not in school/employed</u><br>(1) | <u>Not in school/employed,<br/>to not in school/not employed</u><br>(2) |
|--------------------------------------------------------------------------|----------------------------------------------------------------|-------------------------------------------------------------------------|
| Effect of 21 % increase in<br>minimum on proportion<br>making transition |                                                                |                                                                         |
| All teenagers                                                            | 0.03*                                                          | 0.02**                                                                  |
| Non-blacks,<br>non-Hispanics                                             | 0.03*                                                          | 0.01                                                                    |
| Blacks and<br>Hispanics                                                  | 0.02                                                           | 0.04**                                                                  |
| Initial wage at or above<br>new minimum                                  | 0.04                                                           | 0.01                                                                    |
| Initial wage below<br>new minimum                                        | -0.01                                                          | 0.04*                                                                   |

\*\*\* denotes that effect or elasticity is based on estimates significant at the five-percent level. \*\* denotes significant at the ten-percent level. Elasticities are evaluated at sample means. Source: Panel A--Neumark and Wascher (1995a). Panels B and C--Neumark and Wascher (1995b, and forthcoming), and authors' calculations.

Table 3: Estimates from Disequilibrium Model of Employment Effects

**Panel A: Young Adults**

|                                     | Minimum wage effect<br><u>in binding regime</u><br>(1) | <u>Elasticity</u><br>(2) |
|-------------------------------------|--------------------------------------------------------|--------------------------|
| Estimates from disequilibrium model | -0.33**                                                | -0.21**                  |

**Panel B: Young Adults**

|                    | Average probability<br><u>on binding regime</u> | Average probability-<br><u>weighted elasticity</u> |
|--------------------|-------------------------------------------------|----------------------------------------------------|
| Full sample        | 0.48                                            | -0.11                                              |
| 1973               | 0.05                                            | -0.01                                              |
| 1981               | 0.67                                            | -0.16                                              |
| 1989               | 0.29                                            | -0.06                                              |
| East South Central | 0.87                                            | -0.24                                              |
| West South Central | 0.63                                            | -0.15                                              |
| Mountain           | 0.35                                            | -0.07                                              |
| Pacific            | 0.26                                            | -0.05                                              |

**Panel C: Young Adults**

|                                                     | Single-equation<br>regression estimates<br><u>of minimum wage effects</u><br>(1) |
|-----------------------------------------------------|----------------------------------------------------------------------------------|
| Prob.(binding) > 0.5                                | -0.56**                                                                          |
| Prob.(binding) ≤ 0.5                                | 0.34**                                                                           |
| East South Central and<br>West South Central States | -0.14**                                                                          |
| Pacific and Mountain States                         | 0.46**                                                                           |

**Panel D: Teenagers**

|                                     | Minimum wage effect<br><u>in binding regime</u><br>(1) | <u>Elasticity</u><br>(2)                           |
|-------------------------------------|--------------------------------------------------------|----------------------------------------------------|
| Estimates from disequilibrium model | -0.34*                                                 | -0.29*                                             |
|                                     | Average probability<br><u>on binding regime</u>        | Average probability-<br><u>weighted elasticity</u> |
| Full sample                         | 0.50                                                   | -0.16                                              |
| California, 1988 (before increase)  | 0.004                                                  | -0.001                                             |
| California, 1989 (after increase)   | 0.25                                                   | -0.07                                              |

In Panels A, C, and D, '\*' denotes that effect or elasticity is based on estimates significant at the five-percent level. '\*\*' denotes significant at the ten-percent level. Elasticities are evaluated at sample means. Source: Neumark and Wascher (1994b) and authors' calculations.