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EMPLOYMENT EFFECTS OF MINIMUM AND SUBMINIMUM WAGES: REPLY TO CARD, KATZ AND KRUEGER

David Neumark
William Wascher

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ABSTRACT

In Neumark and Wascher (1992), we present findings supporting the earlier consensus that minimum wages reduce employment for teens and young adults, with elasticities in the range -0.1 to -0.2. In addition, we find that subminimum wages moderate these disemployment effects. Card, Katz and Krueger (1993) criticize numerous aspects of our analysis, and contest our conclusions. This reply presents an assessment of their arguments, as well as additional evidence related to some of the criticisms that they raise. We conclude that the issues raised by Card, et al., upon further examination, do not alter the conclusions from our original paper, and in some cases even reinforce those conclusions.

David Neumark
Department of Economics
University of Pennsylvania
3718 Locust Walk
Philadelphia, PA 19104
and NBER

William Wascher
Board of Governors of the Federal
Reserve System
20th and Constitution Avenue, NW
Washington, D.C 20551

I. Introduction and Summary

In Neumark and Wascher (1992), we present findings supporting the earlier consensus that minimum wages reduce employment for teens and young adults, with elasticities in the range -0.1 to -0.2. In addition, we find that subminimum wages moderate these disemployment effects. Although our results on minimum wage effects generally conform with earlier research, they contrast with some recent studies that find little or no effect of minimum wages on employment, or even positive effects (Wellington, 1991; Card, 1992a and 1992b; Spriggs, 1992; Katz and Krueger, 1992; Card and Krueger, 1993).[1] Card, Katz and Krueger (1993, hereafter CKK) attempt to reconcile these disparate findings by challenging our results. Because research on minimum wages is likely to influence policy decisions, we regard efforts at reconciling the conflicting evidence (including CKK, as well as Currie and Fallick (1993)) as important. Furthermore, we suspect that some of the substance of our exchange with CKK will ultimately prove useful in such efforts. However, based on our assessment of their criticisms, we do not believe that CKK's arguments or evidence alter the conclusions from our original paper.

CKK group their criticisms of our paper into five areas: the inclusion and measurement of school enrollment rates, measurement of the coverage rate, the specification of the minimum wage variable, the role of lagged minimum wage effects, and evidence on the use of subminimum wages. Below, we summarize the main points of our reply. More detailed responses and the evidence on which they are based are provided in Section II.

Enrollment Rates

CKK first raise the question of whether regression equations typically used to estimate minimum wage effects should include supply variables. We argue, as have others, that if minimum wages are not binding for all (or most) of the observations in our data set, then supply variables will help determine employment. As partial evidence that excluding the school enrollment rate from the employment equation is a misspecification, we note that disemployment effects of minimum wages should be stronger for teens than for young adults, but that this holds only when the enrollment rate is included.

Second, CKK point out that our definition of the enrollment rate precludes joint employment and enrollment, resulting in a stronger negative association between employment and enrollment than does an alternative measure that permits individuals to be counted as both employed and enrolled in school. In response, we present evidence that there is a sharper dichotomy between employment and enrollment than CKK claim. Moreover, CKK's claim that our enrollment rate is one minus the employment rate (plus measurement error) is refuted by the fact that the sum of the enrollment rate and the employment rate is related to minimum wages and other labor market conditions. More substantively, there is a sizable proportion of individuals neither in school nor employed, and this proportion is systematically negatively related to minimum wages.

Nonetheless, we do find that the estimated disemployment effects of minimum wages for teenagers, treating enrollment rates as exogenous, are in fact somewhat sensitive to the definition of the enrollment rate; an alternative definition of enrollment that allows some joint employment and enrollment suggests weaker, but still negative disemployment effects of minimum wages. We also note, however, that the estimates for young adults (aged 16-24) are not sensitive to the definition of the enrollment rate, and that estimates correcting for the potential endogeneity of enrollments indicate negative minimum wage effects for both the teenage and young-adult specifications.

The Coverage Rate

CKK criticize our coverage rate on the grounds that it mismeasures coverage of teens or young adults by federal and state minimum wage laws. They present evidence that an increase in coverage in 1985 for public sector workers did not result in employment declines relative to trend, which they interpret as evidence that our coverage measure is flawed, or that minimum wages have no effect. However, we argue that this is not a particularly good experiment, because (i) the real value of the minimum wage was declining over this period, (ii) public sector employment of youths is relatively low, and (iii) it seems inappropriate to test the competitive model of minimum wages using workers in the public sector. Moreover, regressions at the state level, controlling for other variables that affect employment, and not restricted to this particular coverage increase, indicate that higher coverage is associated with lower employment of teens and young adults.

The Kaitz Index

CKK argue that the standard coverage-adjusted relative minimum wage variable (the Kaitz index) used in most minimum wage studies is flawed because it is negatively correlated with the mean teen (or young-adult) wage. We show here, however, that a negative correlation between the relative minimum wage variable and the mean teen or young-adult wage does not contradict the suitability of the Kaitz index. For example, if the nominal minimum wage were fixed, an appropriate minimum wage variable should decline when the mean teen or young-adult wage rises, since the real value of the minimum wage falls. We then argue that a better test of the Kaitz index is whether it is positively correlated with the relative price of teen or young-adult labor, and whether this positive correlation stems from changes in minimum wage levels or coverage. We show that the Kaitz index satisfies these conditions.

CKK also assert that regressing employment rates on minimum wage levels, rather than on minimum wages relative to wages of older workers, provides better estimates of minimum wage effects, and they show that such estimates are positive or non-negative. In our view, though, the principal disemployment effect of minimum wages is likely to result from the substitution of older, higher-wage workers for younger, lower-wage workers, and the relevant price driving this substitution is the relative price of these types of labor. Moreover, using a nominal minimum wage variable does not capture changes in the "bite" of the minimum wage associated with changes in the nominal mean teen or young-adult wage, and implicitly assumes that the labor demand curve violates standard homogeneity assumptions. Because, as we show, the data are largely uninformative with respect to the correct specification of the minimum wage variable, we are inclined towards the theoretically preferred specification.

Lagged Minimum Wage Effects

CKK also address our earlier analysis of Card's (1992a) experiment that uses regional variation to study minimum wage effects. In particular, we argue that the presence of lagged minimum wage effects biases his short (one-year) first-difference estimates away from finding disemployment effects. CKK counter that using two-year differences does not overturn his results. We point out, however, that using two-year differences is not the same as introducing lags, and we show that, in our data, two-year differences still obscure lagged negative minimum wage effects.

Subminimum Wage Usage

Finally, CKK question our conclusions regarding subminimum wages. They claim that our plots of the wage distribution documenting usage of subminimum wages are misleading because five-cent ranges are used rather than exact figures. However, the evidence suggests that this procedure does not give a misleading impression, because most CPS respondents report hourly wage rates that are multiples of five cents. Also, we present evidence that contrasts with two of their claims. First, among the states with minimum wages exceeding the federal minimum, we find little or no evidence of a spike in the wage distribution at $3.50 per hour for any state except Minnesota. Second, the spikes at the subminimum wage that we find in wage distributions for teenagers do not appear in wage distributions for older workers. Both of these findings reinforce the evidence that subminimum wage laws induce spikes in the wage distribution at the subminimum.

CKK also argue that low usage of subminimum wage provisions implies that these provisions cannot have much of an impact. We show that, in fact, a relatively small proportion of workers at the subminimum is not inconsistent with our evidence on the moderating impact of subminimum wages on the disemployment effects of minimum wages. Because the disemployment effects of minimum wages are small relative to teen employment, a small fraction of employment at the subminimum is sufficient to produce the offset to the disemployment effects that we report.

II. Detailed Responses

A. Enrollment Rates

The first question CKK raise regarding the enrollment rate is whether it should be included in the employment equation. CKK argue that we are estimating an employment demand equation, and that it is "far from clear that supply-side variables, such as the enrollment rate, belong in this equation." They cite the Brown, et al. (1982) survey as evidence that researchers have not included the school enrollment rate in their preferred specifications.

However, Brown, et al. explicitly argue against the view that employment is demand-determined in the presence of minimum wages (p. 501). In fact, while it is true that in Table 1 of Brown, et al., only four of the 24 time-series studies surveyed include school enrollment as a control variable, a far higher proportion (three-fourths) of the studies include some supply variables, such as the percentage in the armed forces or the population share.

The rationale for including supply variables can be made clear by recognizing that the aggregate employment equation that we (and others) estimate is a combination of observations for which the minimum wage is binding and observations for which it is not. For the observations for which the minimum wage is binding, employment is determined solely by the demand function (ignoring lags)

(1) $E^B = \alpha MW + X^D \beta_B + \epsilon^B$,

where E^B is the employment rate, MW is the minimum wage variable, and X^D is a vector of demand shifters. In contrast, employment for the observations for which the minimum wage is non-binding is determined by the reduced form of the labor demand function and the labor supply function.[2] This reduced form is

(2) $E^N = X^D \beta_N + X^S \gamma_N + \epsilon^N$,

where X^S is a vector of supply shifters, and the "N" superscript indicates "non-binding." Because aggregate employment is $E^B + E^N$, a single equation for employment, even if it is difficult to interpret structurally, should include X^D, X^S, and MW, as in

(3) $E = \alpha MW + X^D \beta + X^S \gamma + \epsilon$.

In our paper, we further argue that the enrollment rate is a supply variable that belongs in the employment equation. One basis for this assertion relates to the expectation that if minimum wages reduce employment, they should do so more for teenagers than for young adults, because a higher proportion of teenagers are minimum-wage workers.[3] As it turns out, this result holds in our data only for specifications including the enrollment rate. Furthermore, for the broader young-adult age group (aged 16-24), we find significant negative employment effects of minimum wages whether or not we include the enrollment rate. While CKK criticize our specifications including the enrollment rate, they offer no interpretation of the puzzling difference between the results for teenagers and young adults in the specifications that exclude enrollment. In contrast, we interpret this difference as suggesting that the model excluding the enrollment rate is misspecified.

CKK also object to the enrollment variable that we use, on the grounds that it measures the proportion of persons who are both enrolled in school and not employed, and they suggest an alternative definition of the enrollment rate that would not preclude joint enrollment and employment.[4] What our variable measures, conceptually, is the proportion of individuals who are in school and, as evidenced from their behavior, have decided against seeking employment. This measure seems most likely to capture supply shifts that might affect the employment rate. By way of contrast, if we were estimating an equation for hours of work (rather than the employment rate), controlling for enrollments that occur jointly with employment might be more appropriate, since such enrollments may affect hours independently of any effects on employment. However, given our focus on employment, we see no a priori case for concluding that we have used the "wrong" enrollment rate.

CKK present two arguments suggesting that our definition of enrollment as excluding employment is inappropriate. First, they note that, based on 1988 May CPS data using a separate question on school enrollment status, 75 percent of teenagers are enrolled in school, while 65 percent of employed teenagers are also enrolled in school. They then argue that this fact implies that our enrollment rate is based on a "false dichotomy" between employment and enrollment. However, their comparison does not speak to the validity of this dichotomy; to see this, note that the enrollment rate among non-employed teenagers could, in principle, be 100 percent. To assess the validity of the dichotomy, it is necessary to compare either the enrollment rate of employed teenagers to the enrollment rate of non-employed teenagers. or alternatively, the employment rate of enrolled teenagers to the employment rate of non-enrolled teenagers. Using the 1988 May CPS (corresponding to the data set that CKK use), the first comparison shows an enrollment rate for employed teenagers of 0.67 versus an enrollment rate for non-employed teenagers of 0.84. The second comparison yields an employment rate among those enrolled in high school, or enrolled part-time or full-time in college, of 0.38, versus an employment rate among the non-enrolled of 0.60. In both cases, the relevant comparison indicates that the dichotomy between enrollment and employment is sharper than CKK suggest, although we recognize that our enrollment measure may nonetheless overstate the distinction.

CKK also conclude, based on the comparison they report, that "Far from being mutually exclusive activities, schooling and work go hand-in-hand for most teenagers." This conclusion does not follow from their comparison and is inconsistent with the facts. Again using the data for May 1988, 47.6 percent of teenagers were in school and not employed, while only 29.1 percent were in school and employed.

CKK's second argument against our definition of the enrollment rate is that it poses econometric problems. In particular, they assert that our enrollment measure is "one minus the employment rate" plus noise attributable to sampling error, in which case our results would reflect a spurious negative relationship between enrollment and employment. However, their characterization of our enrollment rate is inconsistent with the evidence. As noted above, the proportion of respondents indicating neither employment nor enrollment is substantial, and it varies widely.[5] Moreover, if CKK's characterization of our enrollment rate is correct, then when we estimate equation (3) with least squares, the estimates of α and ß should not be significantly different from zero, and the estimate of γ should not be significantly different from -1. It is true that our estimates of γ are relatively close to negative one (typically around -0.75 to -0.8). But we also find estimates of α and ß (including the coefficients of the state and year dummy variables) that are significantly different from zero, which contradicts CKK's claim.[6]

There is another way to make this point that sheds additional light on the effects of minimum wages. If CKK are correct, the sum of our enrollment rate and the employment rate is simply one plus sampling error, and should not be systematically related to any variable. In contrast, Table 1 shows that the sum of employment and enrollment rates is generally significantly negatively related to the minimum wage variable. While this result again contradicts CKK's assertion, these findings are of interest in their own right, as they show that minimum wages increase the proportion of workers neither employed nor enrolled. Minimum wage increases might have this effect, for example, if individuals leave school to queue for minimum wage jobs, or if they leave school and take jobs, displacing other lower-wage workers who remain out of school.[7]

Although CKK's characterization of our enrollment variable appears to be incorrect, we are, as in our original paper, interested in exploring the range of estimated minimum wage effects that can be obtained under plausible alternative specifications. In this sense, CKK's criticism of our enrollment rate highlights another specification issue that ought to be "thrown into the ring." Specifically, we can test the sensitivity of the estimated minimum wage effects to the definition of enrollment by using an alternative definition that is based on major activity during the survey week, and is available from the May CPS's for our entire sample period. This definition counts as enrolled any individual whose major activity is school and who is working, although it does not include those whose major activity is work and who are in school. Thus, while this enrollment definition does not fully reflect the potential overlap between school and work, it nonetheless captures most of this overlap.[8] As evidence of this, we note that CKK report that based on the independent enrollment question in the May 1988 CPS, 75 percent of teenagers are enrolled in school. Using enrollment based on major activity in the survey week, 68 percent of teenagers are measured as enrolled in school.

Columns (3) and (6) of Table 2 present estimates of minimum wage effects on employment using this alternative definition of enrollment; for comparison, the other columns provide previously reported results first excluding the enrollment rate, and then including it, but with the definition used in our paper. As might be expected, given that these estimates are based on an alternative enrollment measure that does not necessarily preclude employment, the estimated coefficients of the enrollment measure are smaller for both teenagers and young adults. The important question, however, regards the sensitivity of the estimated minimum wage effect. For young adults, the estimated minimum wage elasticity is virtually unchanged. For teens, the resulting elasticity is -0.11 instead of -0.19, near the lower end of the range of the estimated disemployment effects that we report in our paper, but still within that range.[9]

A potential problem with the estimates using either of the enrollment rates is that enrollment may be endogenous with respect to employment. We raised this warning flag in our paper, and have recently completed research (Neumark and Wascher, 1993a) that attempts to correct for the endogeneity bias, using state-specific measures of variables that can be viewed as exogenous influences on the enrollment decisions of teenagers and young adults. As reported in the last row of Table 2, the endogeneity-corrected results indicate negative effects of minimum wages on employment, using either definition of the enrollment rate. Again, though, with the alternative enrollment rate, the estimated minimum wage effect is statistically significant only for young adults, for whom the data are based on larger samples.[10]

B. The Coverage Rate

CKK criticize our coverage variable because it is based on published figures pertaining to all workers rather than just teenagers and young adults, and because it does not take account of coverage by state minimum wage laws. We would like to have been able to assess the sensitivity of our results to the mismeasurement attributable to using coverage for all workers, but there are no published coverage estimates by state for these narrower age groups. Similarly, as discussed in our original paper (p. 58), obtaining information on coverage by state laws is difficult, although some information is available for the early years of our sample. However, it is worth noting that because our models include state and year effects, problems arise from the mismeasurement of coverage only if relative coverage rates of young workers and all workers change differentially across states. This could occur, for example, if coverage expands in industries that employ relatively more young workers and that account for varying employment shares across states.[11]

CKK then attempt to demonstrate the inadequacy of our coverage variable by presenting a plot of the aggregate teen employment rate and the population-weighted average of our coverage rate. The increase in coverage in 1985, due to the extension of coverage to state and local government workers, provides, in their view, a natural experiment. The plot shows that after 1985 the teen employment rate did not increase by less than predicted by a trend and the overall employment-to-population ratio, despite the increase in coverage.

In our view, however, the graph presented by CKK does not depict a very good natural experiment. With nominal wages rising, and with the federal minimum wage and the minimum wage in a vast majority of states fixed, the effective minimum wage fell over this period, offsetting disemployment effects associated with higher coverage rates. Moreover, there may have been little overall employment response to the 1985 increase in coverage associated with the readmission of state and local government employees to the FLSA simply because relatively few teens and young adults work for state and local governments.[12] Finally, testing the competitive view of minimum wages based on an increase in coverage in the public sector seems inappropriate, since state and local governments may simply increase revenues to meet the higher wage bill entailed by expanded coverage.

A method of assessing the coverage variable that is more general than looking solely at the 1985 increase in public sector coverage, and that controls for other factors affecting employment of young workers, is to estimate our employment equation separating the coverage rate from the relative minimum wage. Such estimates are reported in Table 3.[13] The estimated elasticities of employment with respect to coverage are always negative, and are significant in all specifications but one. Thus, while the coverage data are not ideal, increases in measured coverage appear to yield the negative employment effects predicted by the standard model of minimum wages.[14]

C. The Kaitz Index

CKK's third criticism relates to our use of the Kaitz index as a minimum wage variable (and the use of this variable in most other studies of minimum wage effects; see, e.g., Brown, et al. (1982)). This variable is the product of coverage and the minimum wage level, divided by the mean wage for all workers in the state (regardless of age).[15] The motivation for the coverage adjustment is that any increase in the minimum should have a larger employment effect if the minimum covers more workers. The motivation for dividing by the mean wage is twofold: to measure how much the minimum "cuts into" the wage distribution, and to obtain a measure of the relative level of the minimum wage as compared with market-determined wages for above-minimum wage workers (Brown, et al., 1982).

The basis of CKK's criticism is their assertion that our minimum wage index ought to "have a positive association with the average wage of teenagers." The notion underlying their assertion is that minimum wages are supposed to provide a quasi-natural experiment to study the effects of exogenous wage increases on employment. Obviously, such an experiment is informative only to the extent that minimum wages actually increase the price of the affected workers. However, as CKK show, conditioning on the control variables (here the inclusion of the enrollment variable is irrelevant) and fixed state and year effects, the association is negative. CKK provide what is presumably the correct explanation of this fact: because the denominator of the minimum wage variable is the average wage for all workers, a positive partial correlation between average wage levels of teens or young adults and average wages of all workers will induce a negative partial correlation between the average teen or young-adult wage and the minimum wage index. Based on this result, CKK conclude that a literal interpretation of the negative association that we find between employment rates and the minimum wage index is that "demand curves slope up, not down."[16]

However, the basic premise underlying CKK's criticism is incorrect. In the absence of minimum wage increases we would expect a negative, rather than a positive, association between the minimum wage index and the average wage of teenagers, if the average wages of teenagers and all workers move together. In this case the minimum wage index is capturing exactly the right effect: the increase in nominal wages relative to a fixed minimum wage means that the "effective" minimum wage has fallen. Thus, we should not expect an appropriately defined minimum wage index always to have a positive association with the average wage of teenagers or young adults.

What properties should we expect of an appropriate minimum wage index, or more specifically, the Kaitz index? One requirement would seem to be that since the Kaitz index measures minimum wages relative to the mean wage for all workers, it should be positively associated with the mean teen (or young-adult) wage relative to the mean wage for all workers. Columns (1) and (3) of Table 4 show that this is indeed the case. Of course the positive estimated coefficients on the Kaitz index in these columns could reflect movements in the mean wage for all workers, which is the denominator of both the Kaitz index and the dependent variable. However, columns (2) and (4) indicate that there is also a positive partial correlation between the coverage-adjusted minimum wage level and the mean teen or young-adult wage level. This positive correlation is even stronger if we drop the coverage adjustment. These results indicate that minimum wage changes as captured by the Kaitz index do provide a valid experiment for estimating the effects of exogenous wage increases on employment.[17]

Based on their criticism of our minimum wage variable, CKK proceed to reestimate our employment equation using the log of the state minimum wage level in place of the Kaitz index, and report a positive effect of minimum wages on employment.[18] However, there are at least three reasons to question this alternative specification. First, it seems inappropriate to specify the employment equation as a function only of the nominal minimum wage. Generally, both labor supply and demand are assumed to respond to changes in real wage rates, and the use of nominal minimum wage levels would seem to violate the standard homogeneity assumption. In this sense, given the absence of regional price data, dividing the minimum wage level by an average wage measure provides a way to adjust minimum wages for nominal wage levels. Of course, the year effects included in the equation pick up aggregate changes in wage levels over time. However, neither they nor the state effects pick up variation within states, over time.

Second, in our original paper we discussed possible endogeneity arising from the fact that states might increase minimum wages when employment or employment growth is high, leading to a positive bias in the estimated minimum wage coefficient.[19] It seems likely that the minimum wage level, which CKK use, is more prone to this endogeneity bias than is a relative minimum wage variable. With a relative minimum wage variable, high labor demand might be expected to raise the average wage, offsetting in part the endogeneity bias attributable to the tendency of governments to raise minimum wages when labor markets are tight. This offset, however, is not present when the minimum wage level is used alone. This is speculative, but we regard research on the endogenous determination of minimum wages and employment as a promising area for future research.[20]

Third, going back to Marshall, there are three potential employment-reducing effects of wage increases: a scale effect, substitution away from labor and towards other inputs, and substitution away from labor that has become relatively more expensive towards labor that has become relatively cheaper. The relative minimum wage variable is intended to capture explicitly the latter effect, which we view as likely to be the principal response. In particular, a minimum wage increase raises the relative cost of less-skilled labor roughly in proportion to the minimum wage increase, leading to substitution away from less-skilled labor and towards more-skilled labor. Furthermore, a considerable portion of this more-skilled labor may consist of relatively older workers. Thus, it seems appropriate to use a relative wage measure, with the average wage in the state as a proxy for the price of more-skilled labor.[21]

We should note that CKK do acknowledge that, at least on theoretical grounds, it may be more appropriate to specify employment as depending on the minimum wage relative to the wage of other workers, and they report that when they include the log of the minimum wage and the average wage separately, the estimated coefficient of the average wage variable is insignificant. However, as reported in Table 5, in examining such evidence further, we find that the restriction implied by a relative minimum wage measure--that the coefficients of the log minimum wage and log average wage are equal (in absolute value) and opposite-signed--generally cannot be rejected for teens or young adults in specifications excluding or including the enrollment rate. Also, the estimated elasticities of employment with respect to the minimum wage variable restricted in this fashion are negative for all of these specifications (although significant or marginally so only for the specifications including enrollment). These results, coupled with the results that CKK report, suggest that the data are largely uninformative as to whether the minimum wage should be entered relative to an average wage. Thus, in the absence of evidence to the contrary, we are inclined towards what seems the theoretically preferable relative minimum wage measure.

Nonetheless, the estimates reported by CKK using the minimum wage level, together with those in Table 5 using the relative minimum wage, indicate that much of the negative effect of the relative (not coverage-adjusted) minimum wage variable comes through the mean wage for all workers, rather than the minimum wage level. Of course, as we argued above, a minimum wage index should have this property; as the general level of wages rises, a given nominal minimum wage should have less bite. But the original criticism that Freeman (1982) directed at cross-sectional estimates of standard minimum wage-employment equations--that unobserved demand variation may induce a positive correlation between employment rates and average wages, and hence a negative correlation between employment rates and relative minimum wages--may also apply here. Fixed state effects in our work, as well as Card's (1992a), are intended to capture persistent unobserved demand variation. But they will not capture year-to-year fluctuations, and hence the estimated coefficient of the minimum wage variable may be downward biased even when fixed state effects are included.

As a partial attempt to address the importance of this negative bias in the estimated coefficient of the coverage-adjusted relative minimum wage variable, we compare estimated minimum wage effects including and excluding the prime-age male unemployment rate.[22] If demand variation induces a negative correlation between the employment rate and the minimum wage variable, then the estimated minimum wage effect should be stronger (negative) when we omit an observable measure of demand conditions. Because our preferred specification includes the current and lagged minimum wage variable, we extend the basic employment equation to include the current and lagged unemployment rate, in order to fully capture any correlation between the average wage in the denominator of the minimum wage variable(s) and the unemployment rate. In general, the employment elasticities are only slightly larger (in absolute value) when the prime-age unemployment rate is excluded. For teens, the estimated elasticities including the unemployment rate are 0.01 and -0.17 (first excluding, and then including the enrollment rate), while the corresponding estimated elasticities excluding the unemployment rate are -0.01 and -0.18. For young adults the results are identical whether or not the enrollment rate is included; the estimated elasticity including the unemployment rate is -0.15, and is -0.16 when the unemployment rate is excluded. These small differences suggest that the estimated elasticities are not biased downward because of unobserved demand variation.

Finally, CKK also claim that a variable measuring the fraction affected by the minimum wage, defined as the proportion of workers between the new and the old minimum wage, is superior to the Kaitz index because "it automatically accounts for the inter-state variation in the distribution of teenage wages." Of course, the Kaitz index also accounts for inter-state variation in the wage distribution by dividing by the mean wage in the state. Moreover, the fraction affected variable has two shortcomings. First, it ignores coverage, which is an important source of variation in effective minimum wages. Second, it does not capture the erosion of the effective minimum wage due to increases in average wages; to see this, note that in the absence of an increase in the nominal minimum wage level, the fraction affected variable will be equal to zero regardless of the rate of increase in the average wage. In this sense, the fraction affected variable is subject to the same criticism as is CKK's suggestion to use the minimum wage level instead of a relative minimum wage.

D. Lagged Minimum Wage Effects

CKK question our analysis of the differences between the negative employment effects of minimum wages that we find, and Card's (1992a) findings that in one-year first differences, cross-state changes in minimum wages (using the fraction-affected variable) induced by the 1990 increase in the federal minimum wage were positively associated with employment changes. In our original paper, we show that in our data set as well, one-year first-difference estimates tend to generate positive minimum wage effects on employment. We then show that there is evidence of lagged minimum wage effects in the data, and that the omission of these lagged effects leads to substantial upward bias in short first-difference estimates.[23]

Using Card's data, CKK attempt to reexamine the issue of lagged minimum wage effects by using two-year changes instead of the one-year changes originally reported in Card's paper. In particular, they report positive coefficients on Card's minimum wage variable when the two-year change in the employment rate is used as the dependent variable. However, using two-year changes instead of one-year changes is not the same as incorporating lags, and in particular eliminates only part of the potential bias associated with omitting lagged minimum wage effects. To see this, suppose, as we assume in our paper, that the true model is

$$(4) \qquad E_{it} = \beta MW_{it} - \gamma MW_{it-1} + S_i\delta + \epsilon_{it} \ ,$$

where E is the employment rate, MW is the minimum wage variable, S is a vector of fixed state effects, i indexes states, t indexes years, $\gamma > 0$, and the other control variables have been omitted. If the model in levels omitting lags is used to obtain first differences, then the first-difference regression

$$(5) \qquad E_{it} - E_{t-1} = \beta(MW_{it} - MW_{it-1}) + (\epsilon_{it} - \epsilon_{it-1})$$

has an omitted variable (MW_{it-1} - MW_{it-2}). This omitted variable is negatively correlated with the included variable (MW_{it} - MW_{it-1}), and because the coefficient on MW_{it-1} is negative in the true model (equation (4)), the first-difference estimate of β is biased upward; in addition, the lagged effects are missing. The evidence presented in our paper is consistent with this set-up: when the first-difference model is estimated including the lagged variable (MW_{it-1} - MW_{it-2}), the estimates of β fall, and the estimates of γ are negative.

What happens when two-year differences are used? In this case the first-difference regression

$$(6) \quad (E_{it} - E_{it-2}) = \beta(MW_{it} - MW_{it-2}) + (\epsilon_{it} - \epsilon_{it-2})$$

has an omitted variable $(MW_{it-1} - MW_{it-3})$. There is less reason (if any) to believe that this omitted variable is negatively correlated with the included variable. However, there is still the problem that the model does not pick up lagged minimum wage effects. Thus, while we are less likely to get a positive estimate of β because of the omitted variable, we still do not obtain estimates of the full disemployment effects of minimum wages from the two-year first-difference model. The reason is that, given the true model, the change in the minimum wage variable between periods t-3 and t-1 affects the employment change between periods t-2 and t.

With our data, we obtain results consistent with exactly this scenario. Table 6 reports results for the two-year first-difference model, first excluding and then including $(MW_{it-1} - MW_{it-3})$. As expected, in contrast to the one-year first-difference results reported in our paper, the estimated coefficient of the contemporaneous change in the minimum wage variable does not fall once the omitted lagged variable is added. However, estimates from the two-year first-difference model (without lags) still understate the disemployment effects of minimum wages, as the estimated coefficient of the lagged change is negative, and sometimes significant. Thus, both one- and two-year differenced equations excluding the lagged minimum wage variable appear to be biased against finding a negative employment effect of minimum wages.

E. Subminimum Wage Usage

Our main evidence on the extent to which youth subminimum wages moderate the disemployment effects of minimum wages comes from our standard employment equation augmented to include an interaction between a dummy variable indicating the existence of a state subminimum wage, and the gap between the effective state minimum wage level and any state subminimum (multiplied by coverage, and divided by the average wage). The estimated coefficient of this interaction variable is positive, and generally significant. In addition to the evidence based on the employment regressions, we also report information from the outgoing rotation group files of the 1989 CPS on wage distributions for teenagers residing in states with legislated minimum wages above the federal level. CKK contest our conclusion, based on these wage distributions, that subminimum wage provisions induce spikes at the subminimum. They also contest the plausibility of our regression estimates indicating that subminimum wages moderate disemployment effects of minimum wages, based on their claim that subminimum wage provisions are used only infrequently by employers.

With respect to the question of whether subminimum wage provisions generate spikes at the subminimum, CKK note (as do we) that for eight of the 12 states we examine, there is no spike at the state subminimum; in contrast, there is some evidence of a spike for Vermont, and stronger evidence for Washington, Pennsylvania, and Minnesota. However, we point out that most of those states without spikes are high-wage states (New England states, Hawaii, Alaska, and California), with large proportions of teenagers earning above the state minimum wage. Indeed, as shown in Table 7, four of the five states with the lowest proportion of workers above the subminimum (they are ranked in increasing order of this proportion) have a spike at the subminimum. For three of these (Vermont, Pennsylvania, and Washington) the subminimum coincides with the federal minimum wage. It is true that we note the possibility that these spikes could be due to coverage by federal but not state laws (because of exemptions in the state laws), but they could also be due to state laws.

CKK suggest inferring whether these spikes at $3.35 are attributable to subminimum wages by asking whether there are similar spikes for older workers, for whom there are also likely to be a fairly large number of lower-wage workers. They claim to find a spike at $3.35 in the wage distributions for senior citizens in the same states for which we find spikes at $3.35 for teenagers. However, we could not corroborate this claim. Using the same data file as for teenagers, the last column of Table 7 reports the proportions of workers aged 60 or over at the state subminimum (actually, in a five-cent range around the subminimum, as discussed below). For two of the three states in question (Washington and Vermont) there are no older workers at the subminimum, and for the third (Pennsylvania) the proportion of older workers at the subminimum is 0.01, compared with 0.05 for teenagers.[24] Thus, it appears that the spikes in the wage distributions for teenagers at $3.35 are in fact induced by subminimum wage laws.

Minnesota is the one relatively low-wage state among the 12 we examine that has a state subminimum ($3.47) above the federal level. We report a spike at this subminimum wage level. As CKK point out, this "spike" actually covers the five-cent range ($3.47-$3.52), because we use cells extending over five-cent ranges in tabulating our distributions. Although we (regrettably) neglected to report these cell ranges in the paper, we do not think that it is inappropriate to use five-cent cells, and to include $3.47 in the five-cent range encompassing $3.50. More than 90 percent of teenagers in our 1989 sample of states with minimum wage levels above the federal level report an hourly wage that is a multiple of five cents, suggesting that many employers (or perhaps CPS respondents) round hourly wage rates to the nearest nickel or dime. Indeed, Katz and Krueger (1991) examine use of the subminimum in CPS data by reporting the proportion of workers earning within five cents (i.e., an eleven-cent range) of the $3.35 federal subminimum in 1990. Finally, employers can of course exploit subminimum wage provisions without paying workers exactly the subminimum wage, by paying a wage between the minimum and the subminimum, and thus a spike in the wage distribution slightly above the subminimum is consistent with its use.

In contrast, CKK claim that many states have a spike at $3.50 (which we suppose they would also attribute to rounding), and that our apparent spike at Minnesota's subminimum wage is thus misleading. However, Table 7 reveals little or no evidence of a spike at exactly $3.50 for any states except Minnesota, and for four of them (albeit higher-wage states), there are no observations at exactly $3.50. In addition, again using CKK's idea of examining the wage distribution for older workers to ask whether spikes for teenagers are attributable to subminimum wages, we note the absence of any older workers in Minnesota (as well as most other states) in the five-cent range including $3.50. CKK also note that the one worker in Minnesota who actually reported earning $3.47 also earned tips that boosted hourly earnings to $6.00. However, in Minnesota tips could not be used to satisfy state minimum wage laws in 1989;[25] thus, this individual was still being paid the subminimum according to law. We therefore stand by our conclusion that there is evidence of spikes in the wage distribution induced by subminimum wages, although we should point out that we do not find evidence of spikes that are much larger than those reported in the literature.[26]

Finally, low rates of usage of subminimum wage provisions are not inconsistent with moderating effects of these provisions, because the disemployment effects of minimum wages that we and others estimate are small relative to teen employment. Suppose that a youth subminimum offsets about one-third of the disemployment effect of a minimum wage, as our estimates suggest. Then, for an employment elasticity of -0.15 (roughly the midpoint of our range of estimates), a 30 percent increase in the minimum wage (paralleling that which occurred between 1989 and 1991) leads to a 4.5 percent reduction in the employment rate in a state without a subminimum wage, and a three percent reduction in a comparable state with a subminimum wage. This moderating effect of the subminimum in the latter state requires that only 1.5 percent of teenagers be employed at the subminimum, even assuming that all of the higher employment in the state with the subminimum wage occurs at the subminimum; of course, some of the additional employment could be at wages above the subminimum but below the minimum. In any case, this estimate of 1.5 percent is within the range of existing estimates of subminimum wage usage.

III. Conclusion

At present, the evidence from recent studies of minimum wages shows that there is a broad range of estimates of the employment effects of minimum wages. The wide range of estimates may be maddening from the perspective of policy-makers. But in contrast with earlier research, recent studies of minimum wages are characterized by widely-varying types of data and statistical experiments. Thus, this wide range of estimates (and the perception that it has widened) is not entirely surprising. Attempts such as CKK's comment to explain this range of estimates, and to narrow it, constitute a fruitful avenue of research, and are likely to deepen our understanding of minimum wage effects and the workings of labor markets. Nonetheless, we do not find CKK's criticism of our work to be compelling, and we do not believe that they have refuted our results.

Endnotes

1. In contrast, Taylor and Kim (1993) report large disemployment effects in retail trade, while Williams (1993) finds disemployment effects in some regions of the U.S.

2. Supply variables also determine employment for some observations if there is monopsony in low-wage labor markets (and some observations are in the region where employment is determined by the supply curve).

3. This argument is formalized in Brown, et al. (1982), pp. 493-4.

4. We measure enrollment from the "Employment Status Recode" on the CPS files, which codes an individual as employed if he or she has a job in the survey week, and only otherwise allows the possibility that the respondent is enrolled in school. As CKK point out, this variable does not correspond exactly to the label we assign it in the tables in our original paper, namely the "proportion of the age group in school."

5. For teenagers, across states, the mean proportion neither in school nor employed is 0.17, and this proportion ranges from 0.03 to 0.33. For young adults, the mean proportion is 0.20, and it ranges from 0.08 to 0.42. Other than employed and enrolled, the employment status recode classifies individuals as unemployed and looking for work, and not in the labor force because of home responsibilities, inability to work, retirement, and other reasons.

6. In addition, as we report in our paper (Table 2 and footnote 12), the partial correlations of both employment rates and enrollment rates with the minimum wage variable are negative. (Although not reported in the paper, the raw correlations are also negative.) If instead our enrollment rate variable were one minus the employment rate plus noise, then the partial correlation of the minimum wage variable with the employment rate would simply be the negative of the partial correlation of the minimum wage variable with the enrollment rate.

7. It is worth noting that evidence suggesting that the minimum wage reduces the sum of the enrollment and employment rate is not inconsistent with findings in the existing literature (including our 1992 paper) that in teenage employment equations excluding the enrollment rate, minimum wages do not reduce employment. This is because such employment equations are reduced form equations. Suppose that, as in equation (3), employment is determined by $E = \alpha MW + X^S\gamma + \epsilon$, where X^S is enrollment, $\alpha < 0$, $\gamma < 0$, and X^D has been dropped. If enrollment is determined by $X^S = \alpha' MW + \epsilon'$, where $\alpha' < 0$, then the reduced form for employment is $E = [\alpha + \alpha'\gamma]MW + \epsilon''$. In this reduced form, the expected sign of the minimum wage coefficient is ambiguous and may be close to zero even though α and α' are negative. In contrast, the reduced form for the sum $E + X^S$ is $E + X^S = [\alpha + \alpha'(1 + \gamma)]MW + \epsilon'''$. Here, the expected sign of the minimum wage coefficient is negative as long as $\gamma \geq -1$, consistent with the evidence in our paper and Table 1.

8. The alternative definitions did lead us to err in footnote 13 of our original paper, where we discuss the apparent discrepancy between the estimated coefficient of our enrollment variable and published data on the difference between the employment rates of the enrolled and non-enrolled that are based on October CPS's.

9. However, when we correct for heteroskedasticity and first-order serial correlation in the errors, the estimated elasticities are very close using the alternative enrollment measures. Using our original measure we obtain elasticities (standard errors) of -.24 (.06) for teenagers

and -.13 (.04) for young adults; the corresponding estimates using the alternative measure are -.22 (.08) and -.14 (.05). See Neumark and Wascher (1993a) for details.

10. When these estimates are also corrected for heteroskedasticity and first-order serial correlation in the errors, using our enrollment rate the elasticities (standard errors) for teenagers and young adults are -.39 (.08) and -.14 (.04); the corresponding estimates using the alternative measure are -.19 (.10) and -.15 (.05).

11. Previous time-series research using the Kaitz index sometimes used youth employment shares by industry to weight industry coverage data (Brown, et al., 1982, p. 500). However, we are unaware of any data on FLSA coverage by industry and state either. Of course, we can never expect to have the "ideal" coverage data, measuring the proportion of workers covered by minimum wage legislation ex ante (i.e., before any employment effects of minimum wages have occurred). In addition, in Table 6 of our original paper we report estimated minimum wage elasticities excluding the coverage measure, as well as incorporating our best estimates of coverage by state minimum wage laws.

12. For example, in 1990 4.7 percent of employed teenagers and 6.7 percent of employed young adults were in the state and local sector, compared with 14.1 percent of all employed workers.

13. In the table we use logs of the relative minimum wage and coverage variables so that we can test the equality of the coefficients of these variables implied by the coverage-adjusted relative minimum wage variable. The results regarding the effects of coverage are qualitatively similar using levels.

14. While there is no theoretical basis for the restriction that, say, a 10-percent increase in the minimum wage level should have the same effect as a ten-percent increase in coverage, the theoretical models of minimum wage effects surveyed in Brown, et al. (1982) suggest that there should be an interactive effect of minimum wage levels and coverage, in that an increase in the minimum wage should have a larger effect the higher is the coverage rate. In fact, as the last row of Table 3 shows, this restriction is not rejected at the five-percent significance level, although there is some evidence against the restriction for young adults at higher significance levels.

15. Technically speaking, the Kaitz index is defined as a weighted average of such a measure defined over industries. Since our minimum wage variable is in the same spirit, we use the label here as well.

16. To reach this conclusion, CKK must be reasoning as follows: When the minimum wage increases, the average teen wage increases. But their evidence suggests that the average teen wage is negatively associated with our minimum wage index, which is in turn negatively associated with employment. Therefore when the average teen wage goes up, employment goes up. (If CKK are thinking of sources of increases in average teen wages other than those stemming from minimum wage increases, then their conclusion does not follow, since we could be seeing movements along the labor supply curve.)

17. Another way to assess the validity of the Kaitz index is to ask whether increases in minimum wages are associated with increases in the Kaitz index. Regression estimates of the Kaitz index on the controls used in Table 4, as well as a dummy variable indicating whether the minimum wage increased from the previous year to the present year, indicate that the

answer to this question is yes. The estimated coefficient on the dummy variable for a minimum wage increase is 0.014, with a standard error of 0.004.

18. CKK also report a set of what they label as structural estimates of the labor demand curve, using the log of the average teen wage on the right-hand side, and instrumenting for it with the log of the minimum wage. The resulting coefficient estimates are sometimes positive and marginally significant. The reason to instrument is that a positive estimate of the coefficient on the teen wage may reflect variation along the supply curve attributable to labor demand shocks. (This same problem imparts a downward bias to estimated minimum wage effects using a minimum wage variable with an average wage in the denominator, a problem we address in our original paper and in this reply.) However, instrumenting with the minimum wage as CKK do leads to identification problems. In particular, there is no reason to expect the minimum wage to shift the labor supply curve, and hence their procedure does not identify the teen wage coefficient in the labor demand equation. For observations for which minimum wages are binding, minimum wages do trace out the labor demand curve, although not by shifting the labor supply curve; for these observations, though, there is no simultaneity problem, and thus no reason to instrument. In contrast, for observations for which minimum wages are non-binding, minimum wages are irrelevant. Thus, what is really required is to separate sample observations into those for which minimum wages are and are not binding; in Neumark and Wascher (1993b) we present one such approach.

19. In that paper, using as an instrument the average of bordering states' minimum wages (multiplied by coverage and divided by the average wage), we find no statistical evidence of endogeneity, although the estimated minimum wage effects are more negative than the OLS (within-group) estimates. We have since recomputed these estimates using the average of the higher of either federal or state minimum wages in bordering states, which is more consistent with the rest of our original paper. These estimates also are more negative than the within-group estimates, although the contrast is less sharp.

20. Cox and Oaxaca (1982) is the only paper of which we are aware that addresses the determination of minimum wages, although not in the context of joint endogeneity with employment.

21. We regard the scale effect as likely to be minor, because a minimum wage hike would have only a small influence on overall costs, except perhaps in establishments using primarily minimum-wage labor. There is virtually no minimum wage research that incorporates the relative prices of other inputs in the employment equation; one exception is Hamermesh's (1982) time-series study, which introduces a capital price measure.

22. We also addressed this question in our original paper, where we separated the coverage-adjusted minimum wage from the mean wage for all workers, and found a negative effect for the former. Here we take a different approach to the same question.

23. This is supported by the fact that the inclusion of a lagged minimum wage variable eliminates the differences between the short first-difference and within-group estimates, the latter of which are less upward biased by the omission of lagged effects.

24. These differences in the sizes of the spikes are not attributable to a larger proportion of teenagers earning wages near the minimum. For Washington and Vermont, of course, the proportion of older workers at the subminimum remains at zero even if we look only at lower-wage workers. For Pennsylvania, when we restricted attention to workers earning less

than \$5.00 per hour, the proportion of older workers at the subminimum was 0.03, compared with 0.07 for teenagers.

25. Memorandum from Minnesota Department of Labor and Industry.

26. CKK also dispute our statement that "research on subminimum wages is in its infancy, and a more definitive answer awaits further research." In their comment, CKK add a bracketed insert indicating that this statement applies to usage rates of subminimum wages. The statement actually applies to evidence on the effects of subminimum wage laws on employment. According to CKK, our statement "denies a substantial body of evidence that consistently finds extremely low usage of the subminimum wage." In fact, in our paper we cite two of the three studies that were written prior to our 1992 paper, although we neglected to cite Katz and Krueger (1991).

Of the five studies that CKK cite, four are based only on the experience with the federal subminimum implemented in 1990 (in contrast to the state subminimums that we study for the 1973-1989 period), and three of these four focus on the restaurant industry. The fifth study (Freeman, et al., 1981) looks at the federal student subminimum. It is not clear, however, that conclusions from these studies should be generalized to other forms of subminimum wages, in part because both of the federal subminimum wage programs have quite restrictive provisions. In the case of the federal student subminimum, the evidence suggests that these restrictions were constraining; Freeman, et al. report that in a survey of managers of establishments using the student subminimum, over 70% said that they would use the subminimum more frequently if these restrictions were relaxed. Finally, there are no studies of which we are aware that attempt to estimate whether subminimum wages moderate the disemployment effects of minimum wages. Thus, we contend that there is still room for additional evidence on subminimum wages.

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Table 1: Estimates of Regressions for Sum of Employment and Enrollment Rates[a]

	Teenagers (16-19)			Young Adults (16-24)		
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage-adjusted relative minimum wage	-.02 (.07)	-.20 (.08)	-.19 (.08)	-.02 (.08)	-.13 (.07)	-.12 (.06)
Coverage-adjusted relative minimum wage, lagged one year	-.20 (.07)	-.10 (.08)	-.11 (.08)	-.18 (.08)	-.12 (.07)	-.16 (.07)
Proportion of population in age group	...	...	-.13 (.17)	...	...	-.01 (.08)
Prime-age male unemployment rate	...	...	-.22 (.08)	...	...	-.46 (.06)
State and year effects	No	Yes	Yes	No	Yes	Yes
$\bar{R}^2$	.03	.45	.45	.03	.66	.68

a. Dependent variable is the sum of employment and enrollment rates. The sample consists of 700 observations covering the 50 states and Washington, D.C. For the twenty-two states identified in the CPS as early as 1973, the sample covers the years 1974-1989; for the remaining states it covers the period 1978-1989. Standard errors of estimates are reported in parentheses.

Table 2: Estimates of Minimum Wage Effects Using Alternative Definitions of the Enrollment Rate[a]

	Teenagers (16-19)			Young Adults (16-24)		
	(1)	(2)	(3)	(4)	(5)	(6)
Coverage-adjusted relative minimum wage	.10 (.11)	-.12 (.08)	.03 (.11)	-.03 (.08)	-.10 (.06)	-.05 (.07)
Coverage-adjusted relative minimum wage, lagged one year	-.14 (.12)	-.12 (.07)	-.16 (.11)	-.26 (.08)	-.18 (.06)	-.22 (.08)
Proportion of age group in school and not employed	...	-.77 (.03)	...	...	-.81 (.04)	...
Proportion of age group reporting major activity as school	...	...	-.37 (.04)	...	...	-.46 (.03)
$\bar{R}^2$	.70	.86	.74	.71	.83	.77
Elasticity	-.03 (.10)	-.19 (.07)	-.11 (.09)	-.18 (.06)	-.17 (.04)	-.16 (.05)
<u>Estimates using only lagged minimum wage variable:</u>						
Elasticity	-.08 (.09)	-.14 (.06)	-.12 (.08)	-.17 (.05)	-.14 (.04)	-.15 (.04)
<u>Estimates instrumenting for enrollment rate:</u>[b]						
Elasticity	...	-.25 (.10)	-.17 (.13)	...	-.16 (.05)	-.12 (.06)

a. Dependent variable is the employment rate. All specifications include fixed state and year effects, the proportion of population in the age group, and the prime-age male unemployment rate. See notes to Table 1 for more details.
b. Due to unavailability of instruments, sample period excludes 1974. Instruments are school expenditures per pupil, pupil-teacher ratios, and dummy variables for compulsory schooling ages. See Neumark and Wascher (1993a) for further details.

Table 3: Estimates of Minimum Wage Elasticities with the Current and Lagged Log Relative Minimum Wage and Log Coverage Rate Entered Separately[a]

	Teenagers (16-19)		Young Adults (16-24)	
	(1)	(2)	(3)	(4)
Elasticity with respect to relative minimum wage increase	-.02	-.13	-.10	-.12
	(.12)	(.08)	(.07)	(.05)
Elasticity with respect to coverage increase	-.01	-.28	-.32	-.25
	(.16)	(.11)	(.09)	(.07)
P-value for restriction: coefficient of log(coverage rate) = coefficient of log(minimum wage/ mean wage)	1.00	.38	.08	.15

a. Dependent variable is the employment rate. In all cases, elasticities are based on specifications including contemporaneous and one-year lags of relative minimum wage and coverage variables, and are evaluated at sample means. Standard errors of elasticities reported in parentheses treat coefficient estimates, but not means, as random. All specifications include fixed state and year effects, the proportion of population in the age group, and the prime-age male unemployment rate as controls. The proportion of age group in school and not employed is included as a control variable in columns (2) and (4). See notes to Table 1 for more details. P-values are based on likelihood-ratio tests.

Table 4: Estimates of Minimum Wage Effects on Mean and Relative Wages[a]

	Teenagers (16-19)		Young adults (16-24)	
	Mean teen wage/ mean wage	Mean teen wage	Mean young adult wage/mean wage	Mean young adult wage
	(1)	(2)	(3)	(4)
Coverage-adjusted relative minimum wage	.48	...	.38	...
	(.12)		(.10)	
Coverage-adjusted minimum wage	...	.34	...	.88
		(.19)		(.19)

a. Dependent variables are listed in column headings. The sample consists of 751 observations covering the 50 states and Washington, D.C., from 1973-1989 for the twenty-two states identified in the CPS as early as 1973, and from 1977-1989 for the remaining states. All specifications include fixed state and year effects, the proportion of population in the age group, and the prime-age male unemployment rate as controls. Results are virtually identical when the proportion of age group in school and not employed is included as a control variable. Minimum wage refers to the higher of the federal or state minimum wage. Mean wage refers to the mean wage for all workers.

Table 5: Tests of Relative Minimum Wage Constraint

	Teenagers (16-19)		Young Adults (16-24)	
	(1)	(2)	(3)	(4)
Specifications including contemporaneous and lagged log relative minimum wage				
Elasticity	-.02	-.11	-.08	-.11
	(.12)	(.08)	(.07)	(.05)
P-value for restrictions that minimum wage enters relative to mean wage	.57	.29	.09	.65
Specifications including contemporaneous log relative minimum wage				
P-value for restriction that minimum wage enters relative to mean wage	.42	.04	.02	.36

a. Dependent variable is the employment rate. Elasticities are based on estimated coefficients of current (and lagged) values of log(minimum wage/mean wage). The long-run elasticity evaluated at the sample means is reported. Standard errors of elasticities reported in parentheses treat coefficient estimates, but not means, as random. All specifications include fixed state and year effects, the proportion of population in the age group, and the prime-age male unemployment rate as controls. The proportion of age group in school and not employed is included as a control variable in columns (2) and (4). See notes to Tables 1 and 3 for more details. P-values are based on likelihood-ratio tests.

Table 6: Two-Year First-Difference Estimates of Minimum Wage Effects[a]

	Teenagers (16-19)				Young Adults (16-24)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coverage-adjusted relative minimum wage, t - (t-2)	.20	-.04	.26	-.02	.08	-.07	.12	-.05
	(.17)	(.12)	(.17)	(.12)	(.12)	(.10)	(.12)	(.10)
Coverage-adjusted relative minimum wage, (t-1) - (t-3)	...	...	-.35	-.13	...	...	-.26	-.15
			(.16)	(.11)			(.12)	(.09)
Proportion of age group in school and not employed, t - (t-2)	...	-.75	...	-.75	...	-.74	...	-.74
		(.04)		(.04)		(.05)		(.05)
$\bar{R}^2$	.13	.57	.14	.57	.17	.47	.18	.48

a. Dependent variable is the two-year change in the employment rate. All specifications include fixed year effects, and the two-year change (from t-2 to t) in the proportion of population in the age group and the prime-age male unemployment rate as controls. The years 1976, 1977, 1980, 1981, 1984, 1985, 1988, and 1989 are used to avoid serial correlation introduced by the differencing operation. Thus, the sample consists of 350 observations. Standard errors of estimates are reported in parentheses.

Table 7: Wage Distributions in High Minimum Wage States, 1989[a]

	Teenagers (16-19)					Older Workers (60+)
	State Minimum	State Subminimum	Proportion Above State Minimum[b]	Proportion at Subminimum[b]	Proportion at $3.50	Proportion at Subminimum[b]
	(1)	(2)	(3)	(4)	(5)	(6)
California	4.25	3.61	.69	.00	.01	.00
Washington	3.85	3.35	.69	.06	.02	.00
Minnesota	3.85	3.47	.73	.08	.06	.00
Pennsylvania	3.70	3.35	.75	.05	.02	.01
Vermont	3.65	3.35	.77	.05	.00	.00
Maine	3.75	3.35	.82	.03	.01	.00
Connecticut	4.25	3.61	.83	.00	.02	.00
Hawaii	3.85	3.35	.85	.00	.00	.01
Rhode Island	4.00	3.60	.90	.00	.02	.01
Massachusetts	3.75	3.35	.90	.01	.01	.002
Alaska	3.85	3.35	.93	.00	.00	.00
New Hampshire	3.65	3.35	.94	.00	.00	.00

a. Based on outgoing rotation group files. In states for which the state subminimum is less than or equal to $3.35, we treat the actual subminimum wage level as superseded by the federal minimum wage. An entry of .00 indicates no observations.
b. These columns refer to five-cent ranges defined as $3.33-$3.37, $3.37-3.42, etc.