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Abstract

The principal justification for minimum wage legislation resides in improving the economic condition of low-wage workers. Most previous analyses of the distributional effects of minimum wages have been confined to simulation exercises employing rather restrictive assumptions that guarantee the conclusion that an increase in the minimum wage reduces poverty. In contrast, we adopt a more flexible "reduced-form" approach that links increases in both federal and state minima to contemporaneous changes in poverty rates. For the period 1983-96, we find indication of a poverty-reducing effect of minimum wages among older junior-high dropouts and among teenagers.

1 Introduction

Supporters of minimum wage laws generally argue that minimum wages improve the economic lot of low-wage workers. However, if increases in the minimum wage lead to reduced employment among this group, the effects on poverty and other aspects of the distribution are ambiguous. Economists who have examined these effects have primarily relied on simulation exercises that hinge importantly on a number of simplified and even contentious assumptions, such as those having to do with disemployment effects and family labor supply responses. We follow an alternative, reduced-form approach that in correlating changes in federal and state minima with corresponding movements in (several measures of) the family income distribution avoids these problems.

Our analysis uses state-level data for the years 1983 to 1996, for three groups most likely to be affected by minimum wage mandates: teenagers, young adults, and junior high school dropouts. We focus primarily on the poverty rates of these groups, but also offer a parallel analysis of the association between minimum wages and average earnings, and between minimum wages and weeks worked.

2 Theoretical Considerations and Related Empirical Work

In the standard competitive textbook model, a *ceteris paribus* increase in an effective minimum wage shrinks the employment of unskilled labor.[1] This disemployment effect implies that the impact of minimum wage increases on the distribution of earnings for low-wage workers is uncertain. Some workers may gain and others lose, implying that the benefits of the minimum wage increase are not distributed evenly across low-wage workers. Moreover, it is not clear that low-wage workers would gain in an expected earnings sense because the impact on average earnings depends on the scale of the displacement effect, as well as labor supply elasticities in the uncovered sector (see Gramlich, 1976). Only in the absence of disemployment effects are the implications more transparent, since every low-wage worker should gain in this case.

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[1] There may also be wage and employment consequences for other factors of production according to whether they are gross complements or substitutes for low-wage labor.

It is the joint presence of losers and gainers from minimum wage increases that contributes to a similar theoretical ambiguity regarding the consequences of minimum wages for the family income distribution. Moreover, in discussing family effects, there are a number of other complications that do not arise when the focus is simply on earnings. First, changes in the minimum wage may affect the labor supply of other members of the families of workers directly affected by the minimum wage. Since such effects involve both income and cross-substitution effects, these responses become difficult to predict. Second, the effects of minimum wage increases will also depend heavily on the initial family income position of workers whose earnings are increased by the change, since such workers are not necessarily concentrated at the low-end of the family income distribution. Third, there is little guidance from theory to help us identify the position in the family income distribution of those workers who might lose jobs when the minimum wage is increased. In particular, an important question in determining the distributional effects is whether job losses are concentrated among families at either the low or high end of the distribution. And, finally, both losers and winners from the minimum wage increase may change their living arrangements as a result of their reduced, or increased, earnings. Even if there were no disemployment effects, these complications lead to ambiguous theoretical predictions for the effects of minimum wage increases when attention shifts from earnings to the family income distribution.

Given the lack of clear theoretical predictions as to the consequences of a rise in the minimum wage for the distribution of family incomes, the extant empirical work has attempted to provide answers by simulating the effect of actual or hypothetical mandates on an existing distribution. The outcome measures used in these exercises have included poverty rates (or gaps), the income-to-needs ratio, and quintile or decile shares.

The simulation exercises generally allow for both disemployment effects and incomplete coverage. Disemployment effects are calculated using a range of elasticities suggested by estimates from time-series studies. One problem with this procedure is that these time-series studies typically report elasticities that indicate how minimum wages affect the employment of all teenagers, or young adults, and not just those workers with wages directly affected by the minimum wage increase. Applying these same elasticities directly to a group of minimum wage workers thus inevitably understates the implied disemployment effect from the time-series estimate. Furthermore, the simulation studies by Horrigan and Mincy (1993), Johnson and Browning (1983), and Mincy (1990) appear to assume that the disemployment effect is a proportion of initial hours worked for each affected worker, so that all workers share in the disemployment effect.[2] Because the elasticities used in these studies never exceed unity, this assumption ensures that *every* low-wage worker will gain from the simulated minimum increase.[3]

Simulation studies must also make some assumption about minimum wage effects on workers initially working below the current minimum. Most studies first assume that these workers would be raised to the new minimum and then, for the partial coverage case, repeat the exercise assuming that they are unaffected by the change (see Burkhauser and Finegan, 1989; Mincy, 1990; Horrigan and Mincy, 1993). The exception is Johnson and Browning (1983), who increase the wages of workers currently below the minimum by the same proportion as the increase in the minimum wage. As for workers earning between the old and new minima, the simulations assume that their wages are increased to the new minimum.

Previous studies of the effects of minimum wages on poverty find that minimum wages reduce poverty, with simulated effects that appear to be somewhat strong. Thus, Burkhauser and Finegan (1989), who do not allow for disemployment effects, find that a 24 percent increase in the minimum wage in 1984 would reduce the share of covered low-wage workers in poverty by 21 percent (reducing poverty from 14 percent to 11 percent for these workers). That said, they also report that less than 12 percent of the increase in incomes stemming from the hike in the minimum wage would accrue to poor families, while 39 percent would go to families with incomes at least three times the poverty line. Mincy (1990), who simulates the impact of a 27 percent increase in the minimum wage applied to the 1987 income distribution, obtains what appears to be a bigger poverty-reducing effect. Specifically, he finds that the number of poor families - among all families, not just those with a low-wage worker - should fall by 6 percent in the partial coverage case and 9 percent in

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[2] The proportion is a function of the size of each worker's wage change resulting from the minimum wage increase.

[3] Johnson and Browning (1983) provide one set of results where the elasticity of demand is set equal to one, Also, the poverty simulations in Mincy do allow some families who are nonpoor before the minimum wage increase to fall below the poverty line because of a loss of employment; at the same time, however, he also assumes that every *ex ante* poor family will experience an income increase if one of its members is affected by the minimum wage increase.

the full coverage case.[4] These magnitudes are, however, quite sensitive to the choice for the elasticity of demand for adult minimum-wage workers.

Simulations that consider the impact of minimum wages on the inequality of family incomes point to very modest effects. In a study that is notable for its attempt to accommodate accompanying changes in tax and transfer payments, Johnson and Browning (1983) find that a hypothetical 22 percent increase in the minimum wage in 1976 would have had /essentially no effect on the Gini coefficient for family incomes. Similarly, while Horrigan and Mincy's (1992) simulations based on a $4.71 minimum wage in 1987 point to a modest decline in *earnings* inequality, the percentage of family income held by each quintile of families is virtually unaffected.

The consensus view of prior simulation studies would appear to be that minimum wages reduce poverty, but primarily by increasing, however slightly, incomes throughout the distribution. The explanation for this result is that low-wage workers are distributed rather evenly across the family income distribution. Nevertheless, there are several simplifying assumptions implicit in these simulations that may be very important to their outcomes. The findings may be the product of the particular assumptions regarding the magnitude and incidence of displacement effects. There are also complications arising from assumptions about the coverage of the minimum increase for workers initially below the current minimum. These studies also ignore issues raised by possible changes in labor supply of other family members, and by possible changes in the family unit.

The only direct empirical study of minimum wage effects on the poverty rate is that contained in Card and Krueger's *Myth and Measurement: The New Economics of the Minimum Wage* (1995). They estimate regressions in which the change in a state's poverty rate from 1989 to 1991 is regressed on the fraction of the state's labor force (in 1989) that should have been affected by the federal minimum wage increases in 1990 and 1991, that is, the fraction in 1989 below the level of the 1991 minimum wage. Their results vary across specifications but always provide a negative coefficient estimate, suggesting that when more workers are affected, poverty rates are more likely to fall.

[4] These are average effects from five simulations using published estimates of teenager and adult disemployment effects. On Burkhauser and Finegan's definition, low-wage workers represent approximately 15 percent of the labor force. If a similar percentage of families have low-wage workers, then Mincy's findings suggest that more families will be removed from poverty by a minimum wage increase than do Burkhauser and Finegan's results.

However, their results for the overall poverty rate are small and statistically insignificant. They also estimate separate regressions in which the poverty rate for workers only is the dependent variable, and find somewhat stronger effects, but this approach fails to capture the impact of any disemployment effects of minimum wages. Despite the weak nature of their statistical evidence, Card and Krueger (1995, p. 307) conclude that their analysis "points to a modest poverty-reducing effect of the minimum wage."

Unlike simulation treatments, our approach relaxes the need to build a restrictive and perhaps unrealistic model in assessing minimum-wage effects on poverty. We instead correlate actual changes in poverty (and other distributional measures) with actual changes in the minimum wage. Taking a cue from the literature on employment effects of minimum wages, we estimate our models by focusing on particular groups of workers considered most likely to be affected by minimum-wage laws. Our approach differs from the regression analysis of Card and Krueger (1995) in two main ways: first, we focus on the effects among families with low-wage workers, rather than for all families (where the effect, if any, is likely to be very small); and, second, we consider a much longer time-frame than their two-year interval.

3 Empirical Models and Data

Studies that focus on the effects of minimum wages on the *employment* of teenagers and young adults typically estimate equations of the form:

$$E_t = \hat{a}_1 f(M_t) + \hat{a}_2 X_t + \mathring{a}_t \ ,$$

where E is the employment (or unemployment) rate of the affected group, $f(M_t)$ is some function of the prevailing minimum wage, and X is a vector of variables intended to capture other influences on the demand for low-wage workers (see the survey in Brown, Gilroy and Kohen, 1982). Most of these studies have employed monthly time-series data on the labor force activity of teenagers and young adults, though a recent study by Neumark and Wascher (1992) uses cross-section, time-series data comprising state-level observations for the 1973-89 time period.

As we are interested in the effects of minimum wages on the income distribution, we could also estimate time-series models in which the dependent variable is some characteristic of the income distribution for low-wage workers. Data on the family income distribution are available only on an annual basis, however, leaving a fairly small sample to use for such estimations. Therefore we chose instead to create a panel of state-level observations for the 1983-1996 period.[5] These panel data include measures of poverty and upper-income status for particular groups of individuals in each state (plus the District of Columbia). The base sources for our measures are the March Current Population Surveys (CPS) for 1984-1997, which contain information on individual and family income from the previous calendar year.[6] The CPS data also contain the poverty line for each family, which we use in constructing our key measure: the percentage of individuals whose families have income below the family's poverty line. Inevitably, there is some arbitrariness in the definition of the official poverty lines, and so we also estimate models in which the poverty rates are calculated using new poverty lines that are 1.25 times the official poverty cutoffs.

We calculated poverty rates for three groups whose income position we consider most likely to be affected by changes in the minimum wage. The first two groups are defined on the basis of age, with one group containing all individuals between the ages of 16 and 19 ("teenagers"), and the second group comprising individuals aged 20 to 24 ("young adults"). Haugen and Mellor (1990) find that roughly 60 percent of minimum-wage workers fall in these two categories. We also identify a group of individuals we call "junior-high dropouts" - those above the age of 24 but with nine or fewer years of completed schooling - because these workers are also likely to be involved in low-wage labor markets.[7] Poverty rates were also calculated for the group of prime-age individuals (those aged between 25 and 54 years).

For each of our three groups, we estimate several models in which the group's poverty rate is the dependent variable. Denoting this poverty rate by P_{st} for state s in year t, our models are of the form

$$P_{st} = \text{á}_s + \text{ã}_t + \text{â}\log(M_{st}) + \text{ë}X_{st} + \text{å}_t ,$$

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[5] See Neumark and Wascher (1992), who discuss other advantages of using state-level data in analyzing the effects of minimum wages.

[6] The CPS measure of income includes earnings, government transfers, pensions, and interest and dividend income.

[7] Addison and Blackburn (1993) find that, in 1992, 44 percent of teenagers, 19 percent of young adults, and 24 percent of junior-high dropouts were paid at or below the minimum wage (among those with jobs). For all individuals, only 12 percent were paid at or below the minimum.

where á and ã represent state-specific and year-specific intercepts. These equations are estimated as fixed-effects models. The year effects should pick up any nationwide recessionary effects on the distributional measure (their inclusion also frees us from having to choose a price index to express the minimum wage variable in constant dollars), while the state effects should control for any tendency for some states to provide better labor-market opportunities for low-wage workers than other states, as well as differences between states in the generosity of AFDC benefits, UI replacement rates, *etc.*[8] The vector X consists of characteristics of the dependent-variable group (specifically, average age, percent white non-hispanic, and percent hispanic).

Given that there may be differential business cycle effects across states, we also estimate specifications that include state- and year-specific independent variables that measure state-specific cycle effects. In these specifications, one cyclical control that we use is the poverty rate of prime-age individuals. Our reasoning for this approach rests on the argument that the prime-age part of the population is unlikely to be directly affected to any great degree by changes in the minimum wage, so that the prime-age measure should largely reflect how the state of the cycle in that state and that year affects the outcome indicator. Additionally, we will also use the prime-age unemployment rate as an alternative cyclical control.

If the only minimum wage law were federal legislation, our suggested model could not be estimated, because the minimum wage variable would be a linear function of the year dummies. Minimum wages are not the same across states, however, as some states set wage floors that are higher than the federal standard. It is this variation in the minimum wage variable that allows us to identify a minimum-wage effect. Specifically, the identification of the minimum-wage effect is achieved through a combination of increases in the minimum wage for higher-income states as their state minima are raised, and increases for lower-income states as the federal minimum was subsequently increased. Following Card, Katz, and Krueger (1994), we enter the "prevailing" minimum wage - the state or federal minimum, whichever is higher - in logarithmic form, rather than use the coverage-adjusted relative minimum wage variable (the "Kaitz index“) used in studies of minimum wage

[8] The state effects also control for any tendency for state price levels to differ on average from the national price level.

effects on employment.[9] Because there were only three states with prevailing minimum wages above the federal minimum wage in the 1978-1984 period, we began our analysis of poverty rates in 1983.[10]

A final measurement issue has also to be addressed. Minimum wages often change within a calendar year, implying that our family income data (from a given calendar year) may correspond to two different periods with different minima. For example, the federal minimum wage increases that occurred in 1990 and 1991 went into effect on April 1 in each of those years, so that the effective minimum wage in most states was different in the first three months and the last nine months. Our solution to this problem is to use a weighted average of the minimum wages when the minimum changed within the calendar year, with the weights set equal to the proportion of the year in which the particular minimum was in effect.[11] A list of the minimum wages in states with an above-federal minimum is provided in Table 1.

The dependent variables are estimates of the true population quantities, and the variability of these estimates is likely to be higher when the underlying sample size is smaller.[12] This naturally leads to heteroskedasticity in the error terms in our regression, and so we use weighted least squares. The traditional solution is to assume that the error variance is inversely proportional to the sample size, but this approach implicitly assumes that the model would have a perfect fit if the population quantities were known. Accordingly, we use weights equal to the inverse of the predicted values from regressions of squared

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[9] Card, Katz, and Krueger (1994) criticize the relative minimum wage variable because it tends to be negatively correlated with the average wage of teenagers. They find virtually no evidence of disemployment effects when the logarithm of the minimum wage is used in place of the relative minimum in the employment equations estimated by Neumark and Wascher (1992). The absolute minimum wage may also be more relevant than the relative minimum wage when analyzing absolute measures of poverty.

[10] There were some changes in prevailing minimum wages in the 1973-1977 period. Unfortunately, all of the states cannot be separately identified in the March CPS public use samples before 1977.

[11] Information on the value of state minima and the date at which they changed was obtained from the review of state labor law changes for the previous year provided in each year's January issue of the *Monthly Labor Review.*

[12] In particular, our dependent variable is a weighted sample mean of the form p=Ó$w_i x_i$, where x_i is the individual poverty indicator and w_i is the corresponding CPS weight for that individual (normalized to sum to one). This leads to a dependent variable with unconditional variance (due to sampling variability) of ð(1-ð)Ów_i^2, where ð is the true poverty rate.

OLS residuals on a constant and the inverse of the weighted sample size for the particular state/year observation (see Blackburn, 1997).[13]

4 Findings

4.1 Effects on Poverty

Averages of the state-level poverty rates over the 1983-96 period are provided in the first row of Table 2. Poverty rates among the groups most likely to be affected by increases in the minimum wage are at least double those of prime-age individuals. A similar result holds when poverty rates are calculated with poverty lines inflated by 25 percent. We also calculated the percentage of individuals in families above three times the poverty line, and above five times the poverty line, and report these results in Table 2. The difference between prime-age individuals and teenagers or young adults (but not junior high dropouts) in the upper-income percentages is somewhat smaller, reflecting a relatively high degree of income inequality among our low-wage groups. Not surprisingly, average weekly earnings appear to be considerably smaller for teenagers and junior high dropouts (but less so for young adults) than for prime-age individuals. Table 2 also reports the average cell size for calculating the state/year statistics; these are all reasonably large, although there is naturally a great deal of variation in these cell sizes. Fortunately, the smallest cell size for any of our state/year observations is 34, so that none of our dependent variables are calculated using an extremely limited sample of individuals.[14]

Fixed-effects estimates of the impact of the minimum wage on the probability of being in poverty are reported in Table 3.[15] In specification (1), we include the logarithm of the minimum wage without a state-specific control

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[13] The skedastic functions we estimate are of the form $\sigma^2 = a + b\Sigma w_i^2$. In instances in which the constant is not statistically significant (at the 5 percent level), we assume the variance of the error term is proportional to the sum of squared weights.

[14] We also estimated models in which the sample was restricted to state/year observations in which at least 60 individuals were used in constructing the poverty rate used as the dependent variable. Results under this sample restriction were qualitatively similar to those obtained using the complete sample of state/year observations.

[15] We also calculated F-statistics for the presence of state and year effects. We uniformly rejected the null hypothesis that state effects are absent in Table 3. The analogous test for year effects often did not reject the null; however, we decided to include the year effects in all regressions to obviate the need to choose a specific price deflator.

for the business cycle. For each group, we find a negative estimated coefficient for the minimum wage variable, implying that higher minimum wages reduce poverty. This estimate is statistically significant (at conventional levels) for both teenagers and junior-high dropouts. The implied elasticity for both of these groups suggests that a 10 percent increase in the minimum wage would lower the dropout poverty rate by 5 percent (representing around a full percentage point drop for both of these groups). Among the group-specific controls, the percent of the group that is white and non-hispanic always shows a strong and statistically significant decreasing effect on poverty that is quite consistent across groups. The coefficient estimate for the average age for the group is always negative, as expected, but is only statistically significant for junior-high droputs (for whom there is much greater variation in this variable than for teenagers and young adults).[16]

In specification (2) of Table 3, we include the prime-age poverty rate as an additional independent variable. The coefficient estimate for the prime-age poverty rate is always positive and statistically significant. Its inclusion reduces slighty the elasticity of the poverty rate with respect to the minimum, but the coefficient estimate remains statistically significant for junior-high dropouts, and at least marginally significant for teenagers. Specification (3) substitutes the level of the minimum wage in place of its logarithm, with little effect on the statistical inference or implied elasticities.[17]

One problem with using the prime-age poverty rate as a control, however, is that it is not exogenously determined with respect to the poverty rate of other groups. Indeed, to the extent that many members of our affected groups reside with prime-age individuals, the poverty rates of the two groups are clearly simultaneously determined. This problem is compounded by the fact that we use estimates of poverty rates, with many of the same families being used to form the poverty rates of the affected groups and the prime-age

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[16] We also estimated specifications that included the percentage of the group that was female as an independent variable. This variable was statistically significant in many specifications (with the expected positive sign), but had essentially no effect on the estimated minimum wage effect coefficients. Additionally, estimates using only the female part of the junior-high dropout sample provided results that were similar to the estimates using the complete junior-high dropout sample.

[17] Because we are here using the level of the minimum wage rather than its logarithm, we now index for inflation using the CPI-U price index.

individuals.[18] As an alternative control, therefore, we use the unemployment rate for prime-age individuals in specification (4) of Table 3. With this control, estimated minimum wage effects are virtually identical to those obtained without cyclical controls. The final specification in Table 3 includes both the prime-age poverty rate and the male unemployment, with minimum wage results the same as in specification (2).

As a way of summarizing the estimated effects of minimum wages on poverty, we performed a back-of-the-envelope calculation that combines the point estimates (for specification 5 of Table 3) for all three of our low-wage groups. The result suggests that a 25 percent increase in the minimum wage should lower the 1996 poverty rate for these three groups combined by 9 percent (a drop of 1.9 percentage points, from a poverty rate of 22.4% to one of 20.5%). The 95 percent confidence interval ranges from a decline in the poverty rate by 14 percent to a decline of 3 percent.[19] By comparison, Burkhauser and Finegan (1989) found that a similar increase in the minimum wage in 1984 would have decreased poverty among "low-wage" workers by 21 percent. Although comparisons are difficult because of differences in the definition of low-wage workers, our results do seem to suggest a similar poverty-reducing impact to that suggested by Burkhauser and Finegan.

We next explored the sensitivity of our results to the estimation technique and the equation specification -- see Table 4. First, we estimated our equations by OLS rather than weighted least squares, using standard errors that are asymptotically robust to heteroskedasticity. This is an alternative manner of handling the heteroskedasticity problem that naturally arises from the difference in cell sizes across states and years. Unfortunately, these results

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[18] We also estimated equations in which the prime-age poverty rate is the dependent variable, using the prime-age unemployment rate as a cyclical control. The estimated coefficient (standard error) for the log of the minimum wage was -.034 (.014), suggesting that minimum wages do lower prime-age poverty. Although statistically significant, this coefficient estimate is considerably smaller in absolute value than those reported in Table 3 for either teenagers or dropouts. The poverty-reducing effect for prime-age poverty could largely result from the fact that many prime-age individuals are in families that also include teenagers and junior-high dropouts. Indeed, when we use the poverty rate for prime-age individuals with more than 12 years of schooling and who live alone as the dependent variable, we obtain a positive coefficient estimate (standard error) on the minimum wage variable of .042 (.034).

[19] The calculation of the confidence interval assumes that the coefficient estimates for the three groups are uncorrelated, and that the prime-age poverty rate is unaffected by minimum wage changes.

(presented in the first column of results in the table) suggest that our findings are somewhat sensitive to the estimation technique. The absolute value of the minimum wage coefficient estimate for teenagers increases, while the young adult and dropout coefficient estimates decrease in absolute value. The general tenor of the results is similar -- a decreasing effect on poverty -- but the particular group that is most affected does change with these results. Given the considerable variation in sample sizes (and therefore in error variances), we prefer the GLS estimates of Table 3 as they should be efficient. Nonetheless, either manner of handling heteroskedasticity leads to the conclusion that there is considerably stronger evidence of poverty-reducing effects for teenagers and dropouts than for young adults.

A second sensitivity consideration stems from concerns about the arbitrariness of the poverty-line threshold. We used an alternative poverty rate as the dependent variable, where this poverty measure is based on poverty lines that are set 25 percent higher than the official lines. These results are provided in the second column of results in Table 4. There is now considerably less evidence of a poverty-reducing effect for teenagers (and the estimated effect for young adults remains statistically insignificant), but the effect for dropouts remains statistically significant and of a similar magnitude to that found using the official poverty lines. It would appear that any decrease in poverty arising from minimum wage increases for teenagers is associated with movements of initially poor individuals to income levels that are just above the official poverty lines.

Neumark and Wascher (1992) found evidence of a lagged response of employment to changes in the minimum wage. We considered a similar lagged response on poverty by estimating models that also included the minimum wage lagged one year as an independent variable. The results are presented in the third column of Table 4. The coefficient estimate for the lagged minimum wage is never individually statistically significant. It is also the case that the sum of the coefficients on the current and lagged minimum wage are always negative. However, this sum is only statistically significant for junior-high dropouts (for whom the estimated total impact is exactly the same as in Table 3). For teenagers, the sum is of a similar magnitude as without the lag, but the statistical quality of the support is weakened. There is considerably less evidence of an effect for young adults in this specification.

In an earlier version of this paper, we found very little evidence of a poverty-reducing effect for junior-high dropouts. Two major differences between our current specifications and those of the earlier version are that we were considering a shorter time period, and that we failed to include important group-specific characteristics. To illustrate the importance of the choice of sample period to the estimated minimum wage effects, the penultimate column of Table 4 reports results when the sample is restricted to observations from the 1983-89 period. Excluding information from the 1990s, we see very little evidence of a minimum wage effect (the coefficient estimate for teenagers is actually positive). Admittedly, the standard errors are increased due to the reduction in the sample size, but the failure to find a statistically significant estimate in the limited time period is due to the change in the coefficient estimates and not merely the result of an increase in their standard errors. It appears the case that the nature of the minimum wage effect on poverty changed in an important manner between the 1980s and the 1990s.

The fixed-effects procedure assumes that the state effects are stable over the sample period for which the equation is estimated. If this restriction is inappropriate, the changes in the minimum wage could be partly picking up differential trends in the demand conditions for low-wage workers within states. This could be behind the different results found when the sample period is restricted to 1983-89, as the state effects are more likely to be essentially stable over a shorter period. One way to relax the constraint of stable state effects is to include additional controls that allow for separate trend effects for each state. Estimates with these additional controls are presented in the final column of Table 4. The support for a poverty-reducing effect of minimum wages for teenagers is greatly weakened by these additional controls (compared to the results in the final column of Table 3), but they have no effect on the estimated coefficient for dropouts.[20]

In summary, there is considerable evidence of a poverty-reducing effect of minimum wages for junior-high dropouts, with this evidence primarily coming from reactions to minimum wages that were increased in the 1990s. There is evidence of a poverty-reducing effect for teenagers, but this support is much more sensitive to issues of specification.

4.2 Effects on Earnings and Employment

We also estimate equations in which the dependent variable is average earnings of individuals in our low-wage groups. Our intention is to explore the

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[20] As a group, the state/trend interactions are never statistically significant at conventional levels.

connection between changes in the minimum wage and these average earnings amounts. The first two columns of Table 5 present results for specifications (with cyclical and group-specific controls) in which we use either the log of annual earnings or the log of weekly earnings as the dependent variable. These results suggest an increasing impact of higher minimum wages on both annual earnings and weekly earnings for all three low-wage groups. The impact appears strongest for teenagers, while for young adults and junior-high dropouts the estimated coefficients suggest elasticities from changes in the minimum wage of 40 percent or less for both annual earnings and weekly earnings. Although the effect on average earnings appears strongest for teenagers, Table 3 indicated stronger poverty-reducing effects of minimum wages among junior-high dropouts. This dissonance may partly be the result of a more direct connection between earnings and family income for junior-high dropouts than for teenagers. Thus, for example, in 1996, the average earnings of a junior-high dropout represented 22 percent of the associated average family income. The average teenager's earnings at that time were 11 percent of its family income.

Although not the primary focus of the paper, we also estimate equations that allow us to explore the impact of minimum wages on the employment of workers in our low-wage groups. The March CPS contains information on weeks worked over the calendar year, which we averaged across individuals in each of our groups to provide a measure of average weeks worked over the year in each state. The logarithm of this variable was then used as the dependent variable in equations that included the log of the minimum wage as an independent variable. We also included the log of average weeks worked for prime-age individuals as a control, as well as the unemployment rate and the log of average weekly earnings for prime-age individuals. Given the definition of the dependent variable, these equations are comparable to equations elsewhere in the literature that estimate disemployment effects by regressing the employment rate on a function of the minimum wage. The primary difference is that we are implicitly using the employment rate averaged over the 52 weeks of the year as the dependent variable, rather than the employment rate in a given week.[21]

The fitted equations for the full sample period are presented in the third column of results in Table 5, and the results are frankly unexpected. Previous

[21] Another difference is that we estimate our regressions in logarithmic form, although results of a similar nature are found if the level of weeks worked is used.

work would suggest that we should expect a negative coefficient estimate for the minimum wage variable, but in our results the point estimate is positive for all three groups. And although statistically insignificant for young adults, the minimum wage coefficient estimate is statistically significant for junior-high dropouts and (at least marginally) for teenagers. The estimates can be interpreted as elasticities, and suggest a particularly large positive elasticity of 40 percent for junior-high dropouts.

The minimum wage effect reported for teenagers is not typical of research findings in this area (we are not aware of any parallel research results for junior-high dropouts). The usual result is that minimum wages tend to lower teenage employment rates. Thus, for example, in a similar analysis of employment effects, Neumark and Wascher (1992) report an elasticity of teenage employment with respect to the minimum wage of roughly -0.1. One obvious difference between our studies is the time period examined, Neumark and Wascher's sample period ending in 1989. The difference in time periods again seems to be a major factor in the difference in our results; if we confine our sample period to 1983-1989, we also obtain a negative coefficient estimate that indicates a small (but statistically insignificant) elasticity of -0.2 (see the last column of Table 6).[22] Results for dropouts also provide much less evidence of an employment-increasing effect from raising minimum wages when estimated using only the data from the 1980s. In fact, our employment estimates are more consistent with the line of research advocated by Card and Krueger (1995), including the studies by Card (1992a), Card (1992b), Katz and Krueger (1992), Card, Katz and Krueger (1994), and Card and Krueger (1994).

As noted earlier, the evidence of a poverty-reducing effect from raising minimum wages largely comes from the experience in the 1990s. This pattern of results seems to coincide with a change in the employment impacts of minimum wages that also appears to have occurred after 1990. Given the lack of evidence of an employment-reducing effect from higher minimums in this period (and the possibility that employment might have even been increased), it is not surprising that there is also evidence that poverty is reduced because of these post-1990 increases. It is also informative that our coefficient estimates suggest a poverty-reducing effect of a similar magnitude to that suggested by Burkhauser and Finegan (1989); they assumed no disemployment effects in

[22] There are several other differences in the two studies; for example, the specification of the minimum wage variable differs (Neumark and Wascher use the Kaitz index), and we do not control for school enrollment behavior.

performing their simulations, and our evidence suggests that this may have not been a misleading assumption.

5 Summary and Discussion

Studies of minimum wage effects on the *employment* of low-wage workers typically use a reduced-form approach to examine the association between changes in the minimum wage and employment rates of teenagers or young adults. We employ a similar strategy in investigating the relationship between minimum wage changes and the family-income position of low-wage workers, using state-level panel data. Most previous studies of minimum-wage effects on the income distribution have relied on simulation exercises that potentially ignore many of the important consequences of minimum wages. Our analysis does not make any assumptions about these consequences, thereby allowing an improved and more direct method for estimating the impact of minimum wages on the distribution of family income.

Our results provide evidence that increases in minimum wages in the 1990s *have* served to reduce poverty. This evidence is strongest when we examine the poverty rates of junior-high dropouts. There is also evidence that minimum wages reduce poverty for teenagers, but support for this result is somewhat more sensitive to specification changes (in particular, to allowing state effects to follow separate trends over time). In contrast, our results suggest that minimum wage increases in the 1980s did not appear to reduce poverty for the groups we examine. A supplementary analysis of employment for our low-wage groups suggests that this difference may be associated with a complete lack of any disemployment effect from higher wage minima in the 1990s. Indeed, our results suggest that there may even be an employment-increasing effect from higher minimum wages during this period (at least for junior-high dropouts and for teenagers).

Why underlies this apparent change in the impact of the minimum wages on both employment and poverty? We are not sure. Both the 1983-1989 period and the 1990-96 period began with the economy in a downturn, and ended with sustained growth. It is therefore difficult to argue that there was more room for the minimum wage to improve the economic conditions of low-wage workers in the later period compared to the earlier one. It is true that the labor market situation of low-skilled workers deteriorated rapidly during the 1980s, and that this deterioration did not continue during the early 1990s (though it had not begun any reversion before 1995). Yet for this change to account for our pattern of our results, it would have to be the case that the relative demand shifts against low-wage workers varied across states in a manner that was correlated with minimum-wage changes in those states, and it is again difficult to readily see how this might be the case.

We did examine the possibility that our employment and poverty results were the result of a differential change in the relative supplies of low-skilled to high-skilled labor in minimum-wage increasing states compared to states without minimum increases. Our approach was to include the ratio of the population of the low-wage group to the population of prime-age individuals as an additional regressor in our employment equations. This variable was generally not statistically significant, however, and often had an unexpected positive sign (we expected that a reduced supply of a low-wage group would increase employment of that group). It also tended to leave the minimum wage coefficient estimate unaffected.

We also considered whether or not the general economic conditions in states that increased minimum wages on their own accord differed greatly from those in states that chose not to do so, and, in particular, whether these differences might have themselves been greater for states increasing minima in the 1990s than in the 1980s. Conceivably, the labor-market conditions confronting junior-high dropouts might have been particularly severe (relative to the nation as a whole) in states that increased minimum wages in the 1990s, as opposed to the 1980s, implying that there was more potential improvement in those conditions in the 1990s as the overall economy improved. To explore these comparisons, we separated states into those which did and those which did not raise minimum wages over the 1983-89 period, and computed some average characteristics at the start of that period (1983) for these two groups of states. These averages are presented in the first two columns of results in Table 6. It is clear that economic conditions were initially better in those states that did increase wage minima, and that this difference was particularly large in the poverty rate for junior-high dropouts. But if a similar type of comparison is drawn for states increasing minimum wages in the 1989-96 interval (measuring conditions at the start of that period), much the same pattern is observed -- see the final two columns of Table 6. This should not be too surprising, given that it is largely the same group of states increasing minimums in both periods.[23]

[23] As can be seen from Table 1, only New Jersey and Iowa increased their minimum wage in the 1989-96 period but not in the earlier period. Only Wisconsin increased its minimum in the 1983-89 period but not in the later period. The comparison in Table 6 are still useful,

We are not sure of an explanation for why minimum wage impacts appear to have had such different effects in the 1990s than in the 1980s. It is intriguing to us that these differences show up in both employment and poverty analyses, and suggests that it may be useful to apply research strategies from previous papers that discovered disemployment effects to data from the 1990s to see if these results continue to hold. It is also interesting that the paper most often cited for suggesting a positive employment effect from minimum wages (Card and Krueger, 1994) studied a minimum-wage change that occurred in the 1990s. It may be that we are seeing a continuation of a trend pointed out by Wellington (1991), in which any deleterious effects of minimum wages on the labor market of low-skilled workers appear to have gradually dissipated over time.

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however, as the economic conditions in those states relative to the national as a whole could have changed between the two periods.

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Tables

Table 1: State Minimum Wages for States Above the Federal Minimum Wage[a]

State	Year													
	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
Alaska	3.85	3.85	3.85	3.85	3.85	3.85	3.85	4.19	4.64	4.75	4.75	4.75	4.75	4.88
California						3.80	4.25	4.25	4.25					
Connecticut	3.37	3.37	3.37	3.37	3.47	3.88	4.25	4.25	4.27	4.27	4.27	4.27	4.27	4.40
Delaware														4.59
Dist. Of Col.[b]	3.82	3.83	3.85	3.99	4.16	4.33	4.33	4.37	4.50	4.50	4.69	5.25	5.25	5.25
Hawaii						3.85	3.85	3.85	4.15	4.65	5.25	5.25	5.25	5.25
Iowa								3.85	4.25	4.65	4.65	4.65	4.65	4.68
Mass.				3.45	3.60	3.70	3.75	3.79						4.75
Maine			3.45	3.55	3.65	3.65	3.75	3.85	4.15					
Minnesota						3.55	3.85	3.95	4.25					
New Hamp.					3.45	3.55	3.65	3.76						
New Jersey										4.85	5.05	5.05	5.05	5.05

Table 1 (continued): State Minimum Wages for States Above the Federal Minimum Wage[a]

State	Year													
	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
Oregon							3.53	4.25	4.75	4.75	4.75	4.75	4.75	4.75
Pennsylvania							3.67	3.78						
Rhode Island				3.45	3.60	3.83	4.10	4.25	4.40	4.45	4.45	4.45	4.45	4.55
Vermont				3.40	3.50	3.60	3.70	3.80	4.15				4.50	4.75
Washington							3.85	4.25	4.25			4.90	4.90	4.90
Wisconsin							3.50							
Federal Minimum	3.35	3.35	3.35	3.35	3.35	3.35	3.35	3.69	4.14	4.25	4.25	4.25	4.25	4.38

[a]If the minimum wage changed in the middle of the year, the minimum wage was calculated as a weighted average of the old and new minima, with weights equal to the percentage of the year in which the relevant minimum was in effect. Blank entries imply that the prevailing minimum in that state and year was the same as the federal minimum. Data on state minima were obtained from January issues of the *Monthly Labor Review*.

[b]Until 1993, the District of Columbia (D.C.) maintained different minimum wages for different occupations. For this period, our minimum wage measures for D.C. were averages of these various minimum wages.

Table 2: Means and Standard Deviations for the State-Level Sample, 1983-96[a]

Distribution measure	Group			
	Prime-age individuals	Teenagers	Young adults	Junior-high dropouts
Poverty rate	.09 (.03)	.18 (.07)	.18 (.06)	.26 (.08)
Percent below 1.25 times the poverty line	.12 (.04)	.23 (.08)	.23 (.07)	.36 (.09)
Percent above 3 times the poverty line	.57 (.08)	.45 (.10)	.41 (.11)	.21 (.08)
Percent above 5 times the poverty line	.26 (.08)	.18 (.07)	.16 (.08)	.06 (.04)
Weekly earnings[b]	511 (69)	95 (27)	254 (46)	138 (52)
Number of individuals in state/year cell	1142 (953)	171 (153)	213 (199)	271 (290)

[a]These are unweighted means (and standard deviations) across the 714 state-year combinations. [b]The weekly earnings variable was calculated including a zero for workers with no earnings; it is expressed in 1996 dollars.

Table 3: GLS Fixed-Effects Estimates of Poverty Rate Equations[a]

Group/ Independent variable	Specification (1)	(2)	(3)	(4)	(5)
Teenagers					
Log of minimum wage	-.09 (.04)	-.07 (.04)		-.09 (.04)	-.07 (.04)
Level of minimum wage[b]			-.15 (.08)		
Prime-age poverty rate[c]		.88 (.10)	.88 (.10)		.82 (.11)
Prime-age male unemployment rate				.63 (.12)	.20 (.13)
Average age[d]	-.12 (.15)	-.10 (.14)	-.10 (.14)	-.15 (.15)	-.11 (.14)
Percent white, non-hispanic	-.23 (.04)	-.19 (.04)	-.19 (.04)	-.22 (.04)	-.19 (.04)
Percent hispanic	.16 (.06)	.08 (.06)	.08 (.06)	.11 (.06)	.07 (.06)
Elasticity[e]	-.50	-.39	-.40	-.50	-.39
Young adults					
Log of minimum wage	-.05 (.04)	-.04 (.04)		-.06 (.04)	-.04 (.04)
Level of minimum wage			-.08 (.09)		
Prime-age poverty rate		.59 (.10)	.59 (.10)		.48 (.11)
Prime-age male unemployment rate				.57 (.13)	.32 (.14)
Average age	-.08 (.11)	-.12 (.11)	-.12 (.11)	-.10 (.11)	-.13 (.11)
Percent white, non-hispanic	-.19 (.04)	-.19 (.04)	-.19 (.04)	-.19 (.04)	-.19 (.04)

Table 3 (continued): GLS Fixed-Effects Estimates of Poverty Rate Equations

Group/ Independent variable	Specification				
	(1)	(2)	(3)	(4)	(5)
Percent hispanic	.10 (.07)	.04 (.06)	.04 (.06)	.05 (.07)	.02 (.06)
Elasticity	-.28	-.22	-.21	-.33	-.22
Junior-high dropouts					
Log of minimum wage	-.13 (.04)	-.12 (.04)		-.13 (.04)	-.12 (.04)
Level of minimum wage			-.26 (.09)		
Prime-age poverty rate		.33 (.11)	.33 (.11)		.22 (.11)
Prime-age male unemployment rate				.44 (.13)	.34 (.14)
Average age[d]	-.05 (.01)	-.05 (.01)	-.05 (.01)	-.05 (.01)	-.05 (.01)
Percent white, non-hispanic	-.18 (.05)	-.17 (.05)	-.17 (.05)	-.17 (.05)	-.17 (.05)
Percent hispanic	-.05 (.06)	-.07 (.06)	-.07 (.06)	-.09 (.06)	-.09 (.06)
Elasticity	-.50	-.46	-.48	-.50	-.46

[a]Weights were constructed from a regression of squared OLS residuals on a constant and a factor proportional to the sampling error in the dependent variable. In situations where the constant in this regression was insignificant at the 5 percent level, it was dropped in obtaining the GLS estimates. The sample size for each regression is 714 (51 states observed in 14 years). The numbers in parentheses are standard errors.

[b] The minimum wage variable is expressed in tens of 1996 dollars (using the CPI-U price index)

[c] The prime-age poverty rate is for all individuals aged between 25 and 54 years, except in specification (5) where it refers to prime-age individuals living alone with an educational level higher than a high-school degree.

[d]This is average age in the targeted group, divided by 10.

[e]The elasticity is calculated at the average poverty rate for the specific group.

Table 4: Alternative Estimation Strategies for Poverty Rate Equations[a]

Group/ Independent variable	OLS Estimates (Robust Standard Errors)	Alternative Poverty Rate[b]	Lagged Minimum Effects	Sample Period: 1983-89	State/Year Trends Included
Teenagers					
Log of minimum wage	-.11 (.05)	-.03 (.04)	-.03 (.06)	.04 (.07)	-.03 (.05)
Lagged log of minimum wage[c]			-.03 (.06) [.15]		
Prime-age poverty rate	.75 (.12)	.85 (.09)	.80 (.11)	.86 (.17)	.79 (.12)
Young adults					
Log of minimum wage	-.02 (.05)	-.06 (.05)	.02 (.07)	-.01 (.05)	-.05 (.04)
Lagged log of minimum wage			-.03 (.07) [.87]		
Prime-age poverty rate	.48 (.11)	-.06 (.05)	.55 (.12)	.54 (.15)	.53 (.12)
Junior-high dropouts					
Log of minimum wage	-.08 (.05)	-.14 (.05)	-.17 (.06)	-.01 (.05)	-.12 (.04)
Lagged log of minimum wage			.05 (.06) [.01]		
Prime-age poverty rate	.32 (.13)	.29 (.11)	.22 (.12)	.28 (.15)	.23 (.13)

[a]Unless otherwise noted, all estimations use the same specification as in the fifth specification of Table 3, and use the same GLS correction as noted in that table. Standard errors are in parentheses.
[b]The alternative poverty rate is measured using poverty lines equal to 1.25 times the official cutoffs. The alternative poverty rate for prime-age individuals is used as the prime-age poverty measure in this specification.
[c]The number in brackets is the p-value for a test that the sum of the current and lagged coefficients is equal to zero.

Table 5: GLS Fixed-Effects Estimates of Earnings and Weeks Worked Equations[a]

Group/ Independent variable	**Dependent variable**[b]			
	Annual Earnings	Weekly Earnings	Weeks Worked	
			Full Sample	1983-89 Only
Teenagers				
Log of minimum wage	.65 (.20)	.69 (.20)	.23 (.12)	-.24 (.21)
Log of prime-age earnings[c]	.68 (.17)	.64 (.14)	.33 (.09)	.38 (.15)
Log of prime-age weeks			.40 (.25)	.02 (.41)
Young Adults				
Log of minimum wage	.21 (.10)	.23 (.11)	.03 (.06)	-.01 (.10)
Log of prime-age earnings	.60 (.09)	.47 (.08)	.02 (.05)	.05 (.08)
Log of prime-age weeks			.52 (.13)	.27 (.22)
Junior-High Dropouts				
Log of minimum wage	.40 (.22)	.36 (.28)	.38 (.14)	.21 (.16)
Log of prime-age earnings	.22 (.17)	.15 (.17)	.03 (.10)	.43 (.13)
Log of prime-age weeks			.11 (.25)	-.41 (.36)

[a]All specifications include (as controls) the male unemployment rate, average age, percent white-non-hispanic, percent hispanic, and state and year dummies. The estimation uses the same GLS procedure as used for the Table 3 estimates. The average earnings variables are calculated including zeros for those individuals with no earnings over the course of the year. Standard errors are in parentheses.
[b]All dependent variables are in logs.
[c]This is annual earnings in the annual earnings equations, and weekly earnings in the other equations.

Table 6: Average Characteristics of States with and without State Minimum Wage Increases[a]

Characteristic	1983-89		1989-96	
	With Increase	Without Increase	With Increase	Without Increase
Prime-Age Poverty Rate	.08	.11	.06	.09
Prime-Age Weekly Earnings [b]	$501	$459	$582	$501
Prime-Age Percent White, Non-Hispanic	.84	.85	.83	.83
Prime-Age Male Unemployment Rate	.065	.069	.033	.036
Teenager Poverty Rate	.19	.21	.13	.17
Young Adult Poverty Rate	.15	.20	.13	.18
Dropout Poverty Rate	.21	.28	.21	.26

[a]The characteristics are for the first year of the specified period. They are simple averages across states.
[b]The weekly earnings variable is in current dollars.