

FOIA MARKER

This is not a textual record. This is used as an administrative marker by the William J. Clinton Presidential Library Staff.

Collection/Record Group: Clinton Presidential Records
Subgroup/Office of Origin: National Economic Council
Series/Staff Member: Julia Chamovitz
Subseries:

OA/ID Number: 5308
FolderID:

Folder Title:
[Minimum Wage] [3]

Stack:	Row:	Section:	Shelf:	Position:
S	18	3	4	1

Raising the minimum wage: effects on family poverty

*Increases in the minimum wage
reduce poverty among families
more than previously estimated,
a study of household data shows*

Ronald B. Mincy

Disemployment effects and weak links among low wages, low income, and poverty have been at the center of the debate about the poverty-reducing effect of a higher minimum wage. Recent studies do not settle the debate. Charles Brown, Andrew Kohen, and Curtis L. Gilroy concluded that disemployment effects are small.¹ But Edward M. Gramlich and Terrence Kelly showed that most low-wage workers are not poor.² Several recent studies, using a variety of methods and data sources, confirm this result.³ Most economists analyzing the subject conclude from this evidence that a higher minimum wage is a poor tool for fighting poverty. But economists who specialize in studies of the poor concentrate on changes that have occurred while the minimum wage remained at its 1981 level. For example, between 1981 and 1986, the poverty line for a family of three increased from \$7,250 to \$8,737 (20.5 percent) and the number of poor workers increased from 8.6 to 8.9 million (2.7 percent).⁴ Anxious for ways to reduce working poverty that do not affect the Federal budget, poverty specialists still favor a higher minimum wage.⁵

Analysts on both sides of this debate neglect important barriers to reducing poverty through raising the minimum wage. These barriers are related to (1) the provisions of the minimum wage law, (2) the characteristics of low-wage workers, and (3) the characteristics of poor families with low-wage workers.

Previous studies suggest how some of these barriers affect poverty reduction through a higher minimum wage. But with new data and recent empirical evidence, researchers can integrate these factors much better today. This article uses the new data and evidence to provide more reliable and up-to-date estimates of the effect of a higher minimum wage on the poverty gap—the difference between the actual income of poor families and the income needed to bring these families up to the official poverty level—and on the number of poor families.

The study is organized as follows. The first section reviews and updates information showing how coverage, compliance, and the characteristics of the working poor affect poverty reduction through a higher minimum wage.

The next section develops a model—incorporating disemployment effects—to simulate the poverty-reducing effect of a higher minimum wage. The following section discusses the Current Population Survey (CPS) data on wages, hours, employment, and poverty status used for the simulations. The fourth section simulates the effect of a higher minimum wage on the poverty gap for poor and near-poor families with at least one member paid less than \$4.25 in 1987. The simulations use different assumptions about disemployment effects, coverage and compliance, and the level of the minimum wage. The final section summarizes the results and suggests policy conclusions.

Ronald B. Mincy is a research associate at the Urban Institute. Laura Wheaton, Susan Weiner, and Jonathan Parker provided computational and research assistance.

↑ Barriers to success of fighting poverty through a min. wage

Our model shows that a higher minimum wage would have a larger poverty-reducing effect than previous studies suggest. With full coverage, full compliance, and a \$4.25 minimum wage, the poverty gap among families with at least one low-wage worker would fall by \$881 million (11.1 percent) and the number of such families would fall by 193,000 (8.7 percent). These declines in poverty would be 3 to 4 percentage points lower if coverage and compliance were at 1987 levels. Changing assumptions about disemployment effects makes little difference because disemployment effects fall heavily on teenagers, whose contribution to family income is small.

Barriers to poverty reduction

The most often cited barriers to reducing poverty through a higher minimum wage are that: (1) few low-wage workers are in poor families and (2) few poor low-wage workers have full-time and full-year jobs.⁶ To look at more recent results, define low-wage workers to be workers earning less than \$4.25 per hour, the minimum wage effective April 1991. Define the low-wage working poor to be low-wage workers in poor families. In 1986, only 10.09 percent of low-wage teenagers and 18.04 percent of low-wage adults were in poor families. This weak link creates an important targeting problem. For example, Robert V. Burkhauser and T. Aldrich Finegan show that 40.0 percent of the higher earnings resulting from a higher minimum wage would go to workers in families with incomes at least three times the poverty level.⁷

Table 1 shows why the age, gender, employment status, and family characteristics of the low-wage working poor also create barriers to poverty reduction. Teenagers and women represent 77.7 percent of the low-wage working poor. These demographic characteristics help us to understand the close link between low wages and limited work hours. Only 14.9 percent of the low-wage working poor were full-time and full-year workers. Many teenagers and women want part-time or part-year work to avoid conflicts with other activities—such as schooling and child care. So a higher minimum wage may have small effects on poverty even with strong overall demand for low-paid labor. Further, 87.0 percent of the low-wage working poor had no other low-wage worker in their family. So the contribution of a higher minimum wage to the reduction of family poverty is generally through boosting the earnings of a single worker.

Exemptions to and noncompliance with the law also create barriers. Many of the low-wage working poor cannot benefit from a higher min-

Table 1. Selected characteristics of low-wage working poor paid up to \$4.24 per hour, 1987

Worker characteristics	Number of workers (thousands)	Percent
Total	1,922	100.0
Age group and gender: ¹		
Adults:		
Female	1,135	59.1
Male	430	22.4
Teenagers (16 to 19 years):		
Female	165	8.6
Male	193	10.0
Employment status:		
Full-time, full-year	286	14.9
Other	1,635	85.1
Number of low-wage family members:		
One member	1,672	87.0
Two members or more	250	13.0

¹ Percentages may not add to total because of rounding.

SOURCE: Unpublished tabulations by The Urban Institute based on Current Population Survey, March 1987.

imum wage because they are *not* covered by the minimum wage law. Small retail and service establishments employ the largest share of workers that are not covered. We have no data on the size of establishments employing low-wage workers. But table 2 shows that retail trade and service establishments employ 76.1 percent of the low-wage working poor. Without more coverage of retail trade and service establishments, many working poor families will be unaffected by a higher minimum wage. Other victims of minimum wage violations.

Without a survey including matched data on worker and family characteristics, it was difficult to study the poverty-reducing effects of a higher minimum wage. Kelly matched May 1974 Current Population Survey (CPS) data on wages and hours to March 1974 data on family income and poverty status.⁸ He used the result to study the effect of raising the minimum wage from \$2 to \$2.65, a 32-percent jump. This increase reduced the poverty gap and the number of poor families by 5.8 percent, which Kelly thought was "amazingly small." Unfortunately, reporting errors in each survey and errors occurring while matching records from the two surveys could have occurred. In addition to reporting and matching errors, the matched sample did not accurately represent the U.S. population because weights for the matched sample were not prepared in advance.⁹

Analysts neglect key barriers to reducing poverty.

This study uses better data than were available to Kelly and incorporates the disemployment effect. The March CPS now includes wage and hours data for employees, along with income and poverty status data for their families. The March CPS also has weights for respondents and their families, so researchers can estimate population distribution from sample distributions. Kelly's simulations ignored the disemployment effect because economists had reached no consensus on its size. However, Brown, Kohen, and Gilroy have provided widely accepted estimates for the disemployment effect.¹⁰ The following section develops a method for incorporating these estimates into simulations of the effect of a higher minimum wage on poverty.

Incorporating disemployment effects

A higher minimum wage leads some firms to reduce employment, thereby penalizing some workers who were paid less than the new minimum wage. The poverty gap narrows if workers employed after the new minimum wage takes effect (hereafter, ex-post) have higher earnings than workers employed before (hereafter, ex-ante). This will occur if employment is relatively inelastic with respect to a higher minimum wage.¹¹

To incorporate this disemployment effect, some notation must be introduced. M is the number of poor families ex-ante and N is the number of poor families ex-post. If the number of poor families falls, $M > N$. P_i is the poverty line for the i th poor family and F_i is income

accruing to that family ex-ante. N_i is the number of working members of the i th family who earn less than the new minimum wage ex-ante. $E(dL_j)$ is the expected change in the j th worker's earnings, including the disemployment effect.

The model ignores changes in hours and prices following a higher minimum wage. A few studies estimate the effect of a higher minimum wage on hours worked.¹² But these studies are based on different samples, use different methods, and yield different conclusions. Studies of the effects of a higher minimum wage on prices also lack consensus. Specific estimates of changes in prices and hours worked could be used to adjust the simulations. For now, the model incorporates only the widely accepted estimates of the disemployment effect. This means that the simulations overestimate the poverty-reducing effect of a higher minimum wage.

The expected change in earnings for the j th worker is:

$$(1) E(dL_j) = H_j [\lambda_j dw_j - (1-\lambda_j) w_j],$$

where j is the probability that the j th worker is employed ex-ante; w_j is the j th worker's wage ex-ante and dw_j is the change in the j th worker's wage; and H_j is annual hours worked by the j th worker.

If a higher minimum wage affects ex-ante poor families only, the ex-post poverty gap is:

$$(2) G = \sum_i^N [(P_i - F_i) - \sum_j^{N_i} E(dL_j)]$$

The ex-post poverty gap is smaller than the ex-ante poverty gap $[\sum_i^M (P_i - F_i)]$ if the change in earnings $(\sum_i^N \sum_j^{N_i} E(dL_j))$ is positive.

Higher minimum wages increase poverty if fewer workers from near-poor families find jobs. To account for this possibility, add $P_k - [F_k - \sum_j^{N_k} (1 - \lambda_j) H_j w_j]$ (> 0) to equation 2. This term represents the expected poverty gap that occurs when the k th near-poor family becomes poor after one member or more is unable to find a job. If there are n such families with at least one worker earning less than the new minimum wage, the ex-post poverty gap is:

$$(3) G = \sum_i^N [(P_i - F_i) - \sum_j^{N_i} E(dL_j)] + \sum_k^n P_k - [F_k - \sum_j^{N_k} (1 - \lambda_j) H_j w_j]$$

To complete the model, expected changes in earnings must be estimated. This requires information about the ex-post employment probability. A crude estimate of this probability can be derived from estimates of the elasticity of employment with respect to minimum wage changes (hereafter, the employment elasticity).

Table 2. Workers earning up to \$4.24 per hour and covered by the minimum wage, by industrial sector, 1987

Industry	Number of workers (thousands)	Workers as a percent of total	Workers covered by the minimum wage (percentage)
Total	12,215	100.0	71.2
Agriculture, fishing, and forestry	238	1.9	36.5
Mining	32	.3	86.3
Construction	213	1.7	89.5
Manufacturing	1,243	10.2	85.9
Transportation, communication, and utilities	151	1.2	88.0
Wholesale trade	242	2.0	68.4
Retail trade	5,750	47.1	76.0
Finance, insurance, and real estate	239	2.0	64.8
Private household services	427	3.5	68.1
All services except private household services	3,537	29.0	59.1
Public administration	123	1.0	60.3
Industry not reported	20	.2	-

SOURCES: See table 1 and Minimum Wage and Maximum Hours Standards Under the Fair Labor Standards Act (U.S. Department

of Labor, 1986), table 8.

NOTE: Dash indicates data not available

Table 3. How changes in the minimum wage, with full compliance and coverage, affect family poverty under varying employment elasticities, 1987

Wage and employment elasticity levels	Poverty gap ¹		Poor families		Poverty gap per family	
	In millions of dollars	Percent reduction	Number, in millions	Percent reduction	Family average	Percent reduction
\$3.35 ²	\$7,940	—	2.218	—	\$3,581	—
\$3.80:						
Zero employment elasticities	7,451	6.2	2,083	6.1	3,577	.1
Published employment elasticities ³	7,502	5.5	2,104	5.1	3,565	.4
High employment elasticities ⁴	7,664	3.5	2,183	1.6	3,511	2.0
\$4.25:						
Zero employment elasticities	6,960	12.3	1,995	10.0	3,488	2.6
Published employment elasticities ³	7,059	11.1	2,025	8.7	3,486	2.7
High employment elasticities ⁴	7,374	7.1	2,177	1.8	3,387	5.4

¹ The poverty gap is the difference between family income and the official poverty line, summed over all poor families.

² The current (1987) level of minimum wage, poverty gap, number of poor families, and poverty gap per family.

³ The results in this row are averages of five simulations assuming the following published estimates of employment elasticities for young adults (ϵ_a) and teenagers (ϵ_t): (1) $\epsilon_a =$

$-.026$ and $\epsilon_t = -.10$; (2) $\epsilon_a = -.074$ and $\epsilon_t = -.1$; (3) $\epsilon_a = -.026$ and $\epsilon_t = -.30$; (4) $\epsilon_a = -.074$ and $\epsilon_t = -.3$; and (5) $\epsilon_a = -.10$ and $\epsilon_t = -.30$.

⁴ This row assumes $\epsilon_a = -.30$; and $\epsilon_t = -.30$.

Source: Unpublished tabulations by The Urban Institute based on the Current Population Survey, March 1987.

If all workers were identical, the ex-post employment probability would be related to the employment elasticity as follows:

$$(4) \lambda = j^1/j^0 = 1 + (dw/w)\epsilon'$$

where J^0 is ex-ante employment; J^1 is ex-post employment; w is the wage and dw is wage change, which are identical for all workers; and $\epsilon'(\leq 0)$ is the employment elasticity.

The following approximation keeps a simple link between the ex-post employment probability and the employment elasticity but drops the assumption that workers are identical:

$$(5) \lambda_j = 1 + (dw_j/w_j)\epsilon$$

Substituting equation (5) into equation (2) and rearranging terms gives:

$$(6) E(dL_j) = H_j dw_j [1 + (1 + (dw_j/w_j))\epsilon']$$

Then, substituting equations (5) and (6) into equation (3) gives:

$$(7) G = \sum_i^N [(P_i - F_i) - \sum_j^N N_i H_j dw_j / [1 + (1 + (dw_j/w_j))\epsilon']] + \sum_k^N P_k - F_k + \sum_j^N H_j dw_j \epsilon'$$

The approximation is a compromise reached by substituting time series estimates of average employment elasticities for individual employment probabilities. The model uses separate elasticity estimates for teenagers ($\epsilon = \epsilon_t$) and adults ($\epsilon = \epsilon_a$). Then, the model uses individual wage data (dw_j and w_j) as weights. Brown, Kohen, and Gilroy provide low and high esti-

mates of employment elasticities for teenagers (16 to 19 years old) and young adults (between 20 and 24 years old). The low estimates of the teenager and young adult elasticities are -0.10 and -0.026 . The high estimates are -0.30 and -0.074 .

The best proxy for ϵ' is an estimate of the employment elasticity for low-wage workers. Published estimates of the employment elasticity of teenagers are good proxies for ϵ_t because, in 1986, more than one-third of all teenagers earned below the current minimum wage and almost three-quarters of all teenagers earned below \$4.25. Good proxies for ϵ_a are hard to find. Most estimates of the employment elasticity of adults are small or statistically insignificant. Even so, these estimates are lower than the employment elasticity of low-wage adults.¹³

The model uses several employment elasticities to substitute for the employment elasticity of low-wage adults. The basic simulation uses employment elasticities of young adults. The range of estimates for young adults was -0.026 to -0.074 , so this simulation may overstate the poverty-reducing effect of a higher minimum wage. Two other simulations substitute low and high values of the employment elasticity of teenagers for the employment elasticity of low-wage adults. None of these assumptions is best on theoretical grounds, but the true effect of a higher minimum wage on poverty should fall within the range of the simulation results.

Besides simulations using published estimates of employment elasticities, a simulation

Among the poor, many who work are not covered by the legislation.

was made assuming no disemployment effect for teenagers or adults ($\epsilon_a = \epsilon_t = 0.00$). The results are compared with Kelly's simulations.¹⁴

A final methodological issue is the way the model treats workers now paid below the minimum wage; that is, workers exempt under the law and victims of minimum wage violations. The first set of simulations assumes all workers receive a higher wage. This assumption allows a higher minimum wage and expanded coverage to occur at the same time, which is consistent with the historical pattern of gradual expansion toward universal coverage. A second set of simulations assumes that workers now paid below the minimum wage, whatever the reason, do not benefit from a higher minimum wage.

Evaluating the CPS data

The simulations use data from the March 1987 Current Population Survey (CPS). One-quarter of the respondents in this household survey were asked if family members who worked were paid by the hour and if so, what was the hourly wage in 1987. The March CPS also includes 1986 data on weekly hours and poverty status. Because the CPS now combines earnings and poverty status data in the same survey, the simulations can avoid the matching errors that may have adversely affected Kelly's results.¹⁵

The simulations avoid two other errors by relying on the CPS. First, as indicated, the CPS is a household survey, not an establishment survey. So deliberate misreporting of illegal wage

payments does not affect the simulations.¹⁶ Second, simulated changes in expected earnings rely on reported wage rates from the CPS, not on the quotient of usual weekly earnings and usual weekly hours. Respondents can make one error when reporting a wage rate, but two errors when reporting usual weekly earnings and usual weekly hours.

But the CPS has two faults. First, the wage and industry status data are for 1987, but all other data are for 1986. As a result, the simulations implicitly assume that wages did not change between the two years. If low-wage employers index their wages to the legal minimum, which did not change after 1981, this assumption is likely to be true for many workers. Second, the sample should exclude exempt workers; however, the CPS lacks the necessary data for doing so.¹⁷ The following results partially adapt to this fault with two sets of simulations. The first set assumes complete coverage and compliance, so this set overestimates the poverty-reducing effect. The second set assumes compliance and coverage at 1987 levels, and so underestimates the poverty-reducing effect.

Impact on poverty

Table 3 suggests a larger poverty-reducing effect of a higher minimum wage, accompanied by full coverage and compliance, than other studies. Using the \$3.80 minimum wage and published estimates of teenager and adult disemployment effects,¹⁸ we find that the poverty

Table 4. How changes in the minimum wage, with compliance and coverage unchanged, affect family poverty under varying employment elasticities, 1987

Wage and employment elasticity levels	Poverty gap ¹		Poor families		Poverty gap per family	
	In millions of dollars	Percent reduction	Number in millions	Percent reduction	Family average	Percent reduction
\$3.35 ²	\$7,940	—	2,218	—	\$3,581	—
\$3.80:						
Zero employment elasticities	7,686	3.2	2,153	2.9	3,565	.4
Published employment elasticities ³	7,707	2.9	2,155	2.8	3,577	.1
High employment elasticities ⁴	7,770	2.1	2,180	1.7	3,570	.3
\$4.25:						
Zero employment elasticities	7,244	8.8	2,065	6.9	3,454	3.5
Published employment elasticities ³	7,305	8.0	2,077	6.3	3,517	1.8
High employment elasticities ⁴	7,489	5.7	2,168	2.2	3,508	2.0

¹ The poverty gap is the difference between family income and the official poverty line, summed over all poor families.

² The current (1987) level of minimum wage, poverty gap, number of poor families, and poverty gap per family.

³ The results in this row are averages of five simulations assuming the following published estimates of employment elasticities for young adults (ϵ_a) and teenagers (ϵ_t). (1) $\epsilon_a =$

$-.026$ and $\epsilon_t = -.10$; (2) $\epsilon_a = -.074$ and $\epsilon_t = -.10$; (3) $\epsilon_a = -.026$ and $\epsilon_t = -.30$; (4) $\epsilon_a = -.074$ and $\epsilon_t = -.30$; and (5) $\epsilon_a = -.10$ and $\epsilon_t = -.30$.

⁴ This row assumes $\epsilon_a = -.30$ and $\epsilon_t = -.30$.

SOURCE: Unpublished tabulations by The Urban Institute based on the Current Population Survey, March 1987.

The effects of disemployment make little difference in narrowing the poverty gap.

gap narrowed from \$7.94 billion to \$7.50 billion.¹⁹ A subsequent increase in the minimum wage to \$4.25 narrows the poverty gap further to \$6.96 billion. The combined effect of the two increases is an 11.1-percent reduction in the poverty gap. If there were no disemployment effect, the poverty gap would decrease by 12.3 percent.

Disemployment effects make little difference because these effects are small for adults, but adults make the largest contributions to the income of poor families. Adults contribute more to family income than teenagers, in part because adults work more hours during the week and more weeks during the year. There also are more adults than teenagers among the low-wage working poor. As a result, in the non-disemployment-effect simulation, for example, adults account for 87 percent of the additional earnings.

The poverty-reducing effect in table 3 is much larger than Kelly's estimate. The percentage decline in the poverty gap, assuming no disemployment effect, was more than five times Kelly's simulation, even though the minimum wage change considered here is just 5 percentage points larger.²⁰ Besides the superior data now available, substantive reasons account for a larger poverty-reducing effect in 1986. First, between 1973 and 1986, the number of poor workers grew from 6.2 to 8.9 million, and between 1976—the earliest year for which data are available—and 1986, average family size fell from 3.39 to 3.21 persons.²¹ Similar percentage changes in wages would have caused a greater change in the ratio of earnings to needs in 1986 low-income families as compared with 1973 low-income families. This, in turn, would have caused the 1986 poverty gap to decrease by a larger amount.

Simulations assuming high disemployment effects for adults and teenagers show small declines in poverty. If low-wage adults and teenagers had the assumed upper bound elasticity for teenagers (-0.30), the ex-post poverty gap would have been \$7.4 billion. This gap is just 7.1-percent smaller than the ex-ante poverty gap. A previous observation explains why the poverty gap decreases so little. Low-wage adults contribute more to family income. If the employment probabilities of low-wage adults fall substantially after a minimum wage change, a small decline in the poverty gap would result.

Besides the decrease in the aggregate poverty gap, the decline in the number of poor families and the increase in income per family also are important. Again, assuming a \$4.25 minimum wage and published estimates of minimum wage employment elasticities, almost 200,000

fewer families with a low-wage worker would be poor. The poverty gap for each of these families would be \$95 smaller, on average. These results represent an 8.7-percent decline in the number of working poor families and a 2.7-percent decline in the poverty gap per family. Thus, in percentage terms, the aggregate poverty gap would decrease by more than either the number of working poor families or the poverty gap per family. This measurement anomaly comes from removing many families with small poverty gaps from the sample of poor families.²²

Changing assumptions

Using different assumptions about minimum wage employment elasticities has little effect on the simulations. As we have already seen, simulations differ substantially only when they assume that the adult employment elasticity is equal to the upper bound for teenagers, rather than one of the published values for young adults.

Changing assumptions about coverage and compliance has large effects on the simulations. Table 4 shows the poverty-reducing effect of a higher minimum wage with coverage and compliance at 1987 levels. That is, workers benefit from minimum wage increases only if their 1987 wage was at least \$4.25. The resulting declines in poverty are 3 to 4 percentage points smaller than declines in poverty assuming full coverage and compliance. This poverty gap falls by \$635 million (8.0 percent) and the number of poor families falls by 138,000 (6.3 percent). Thus, 28 percent of the declines in the poverty gap, assuming full coverage and compliance, is due to more coverage and compliance.

Summary and conclusions

This article develops a model to simulate the effect of a higher minimum wage on poverty using wage, hours, employment, and poverty status data from the March 1987 Current Population Survey. Researchers can now match wage and hours data for workers to poverty status data for families. Such matches enable researchers to distinguish the working poor from other workers earning the minimum wage or less, so that researchers can distinguish the effects of a higher minimum wage on poverty from the effects of a higher minimum wage on the distribution of income. Besides these data, the model uses time series estimates of the disemployment effect to adjust for lower employment probabilities. Next, we compute a poverty gap for poor and near-poor families with at least one member

If full coverage and compliance are assumed, the effects are significant.

paid less than \$4.25 in 1987. Finally, we simulate the aggregate poverty gap, using alternative assumptions about disemployment effects, coverage and compliance, and the value of the minimum wage.

If full coverage and compliance accompany an increase of the minimum wage to \$4.25, the poverty gap among families with at least one low-wage worker falls by \$881 million (11.1 percent) and the number of such families falls by 193,000 (8.7 percent). These declines in poverty would be 3 to 4 percentage points higher if the new minimum wage were \$4.65 and 3 to 4 percentage points lower if coverage and compliance were at 1987 levels. Debates about the poverty-reducing effect of a higher minimum wage often focus on disemployment effects, but alternative assumptions about the size of these effects make little difference in our analysis. The reason is simple: disemployment effects fall heavily on teenagers, who make small contribu-

tions to family income.

These results appear to favor a higher minimum wage, but caution is necessary. Despite its inefficiency in reaching the poor, a higher minimum wage would significantly reduce poverty among working families. Further, the decline in poverty is sensitive to changes that policymakers can control—such as coverage and the value of the new minimum wage. But this possibility supports neither complete coverage nor a much higher minimum wage. Either move would require higher percentage wage changes than ever before, and the effects of such changes are uncertain. In particular, economists can only guess about the size of the disemployment effect for low-wage adult workers, who make the largest contributions to family income. If disemployment effects for low-wage adults are similar to disemployment effects for teenagers, complete coverage or a much higher minimum would have small poverty-reducing effects. □

Footnotes

ACKNOWLEDGMENT: The author thanks Isabel Sawhill for many helpful suggestions during the course of this research and John Palmer, Joseph Minarik, Irwin Garfinkel, Marvin Kosters, Sar Levitan, Charles Link, Ralph Smith, Roald Euler, and Saul Hoffman for comments on a earlier draft.

¹ Charles Brown, Curtis L. Gilroy, and Andrew Kohen, "The Effects of the Minimum Wage on Employment and Unemployment," *Journal of Economic Literature*, June 1982, pp. 487–528.

² Edward M. Gramlich, "Impact of Minimum Wages on Other Wages, Employment and Family Income," *Brookings Papers on Economic Activity* 2, 1976, pp. 409–51; and Terrence Kelly, *Two Policy Questions Regarding the Minimum Wage* (Washington, The Urban Institute, 1976).

³ Carolyn S. Bell, "Minimum Wages and Personal Income," paper presented at the American Enterprise Institute Conference on Legal Minimum Wages (Washington, 1979); Robert V. Burkhauser and T. Aldrich Finegan, *The Minimum Wage and the Poor: The End of a Relationship*, Working Paper, 87–221 (Nashville, TN, Vanderbilt University, 1987); William R. Johnson and Edgar K. Browning, "The Distributional and Efficiency Effects of Increasing the Minimum Wage: A Simulation," *American Economic Review*, March 1983, pp. 204–11; Andrew Kohen and Curtis L. Gilroy, "The Minimum Wage, Income Distribution and Poverty," *Report of the Minimum Wage Study Commission*, March 1981, pp. 1–30; and Donald O. Parsons, *Poverty and the Minimum Wage* (Washington, 1980).

⁴ U.S. Bureau of the Census, *Current Population Reports*, Series P–60, Nos. 138 and 160.

⁵ David T. Elwood, *Poor Support: Poverty in the American Family* (New York, Basic Books, 1988); Sar A. Levitan and Issac Shapiro, *Working But Poor: America's Contradiction* (Baltimore, The Johns Hopkins University Press, 1987); Isabel V. Sawhill, "Poverty and the Underclass," in *Challenge to Leadership: Economic and Social Issues for the Next Decade* (Washington, 1988).

⁶ In "The Minimum Wage," Kohen and Gilroy summa-

rize these and other studies of the effects of minimum wages on the distribution of income.

⁷ See Burkhauser and Finegan, "The Minimum Wage and the Poor."

⁸ See Kelly, "Two Policy Questions."

⁹ The May 1974 CPS data on low wages are suspect because respondents reported wages for the month and year that a new minimum wage took effect. In response to a question about their "normal" wage, respondents could have reported the wage they received before or after the minimum wage change.

¹⁰ See Brown, Gilroy, and Kohen, "The Effect of the Minimum Wage."

¹¹ Estimates of the elasticity of employment with respect to minimum wage changes are between –0.10 and –0.30 for teenagers, and less than –0.10 for young adults. See Brown, Kohen, and Gilroy, "The Effect of the Minimum Wage."

¹² See Gramlich, "Impact of Minimum Wages," and Peter Linneman, "The Economic Impacts of Minimum Wage: A New Look at an Old Question," *Journal of Political Economy*, June 1982, pp. 443–69; Brigitte Sellekaerts, "Impact of Minimum Wage Legislation on Wage and Price Inflation," *Report of the Minimum Wage Study Commission*, 1981, pp. 1–17; and Albert Zurker, "Minimum Wages and the Long Run Elasticity of Demand for Low Wage Labor," *Quarterly Journal of Economics*, May 1973, pp. 267–77.

¹³ There are two reasons. First, few adults make low wages. Second, a higher minimum wage may cause employers to substitute higher quality adult labor for lower quality adult labor. Wages are positively correlated with labor quality, so the percentage decline in low-wage, adult employment exceeds the percentage decline in total adult employment. In "The Effect of the Minimum Wage," Brown, Gilroy, and Kohen review studies of the employment elasticity of adults. Two studies report that—for some adults—employment rises after a higher minimum takes effect. See Daniel S. Hamermesh, "Minimum Wages and the Demand for Labor," *Economic Inquiry*, July 1982, pp. 365–80; and John F. Boschen and Hershel I. Grossman, "The Federal

Minimum Wage, Employment and Inflation." *Report of the Minimum Wage Study Commission*, 1981, pp. 19-43.

¹⁴ Estimates of differential unemployment effects for blacks and whites or men and women yield mixed results. Therefore, the literature does not seem to justify further demographic disaggregation of the ex-post employment probability.

¹⁵ Before proceeding with the simulation, our sampling procedure was validated using estimates by the U.S. Congressional Budget Office. The sampling procedure estimated that there were 53.2 million workers paid by the hour in 1985. This estimate appears in Ralph E. Smith and Bruce Vavrichek, "The minimum wage: its relation to incomes and poverty," *Monthly Labor Review*, June 1987, p. 24-30.

¹⁶ Larry Cartensen and Henry Woltman, "Comparing Earnings Data from the CPS and Employer's Records," in *Papers Presented at the Annual Meetings of the American Statistical Association* (Washington, American Statistical Association, 1979).

¹⁷ Besides industry and occupation, an establishment's annual sales volume is a criterion for determining if the establishment's employees are covered by the minimum wage. The annual sales volume exemptions present a serious problem because they affect retail and service establishments, which employed 76 percent of the workers earning less than \$4.35. Because the CPS lacks sales volume information, it is impossible to exclude all exempt workers from our analysis.

¹⁸ See Brown, Gilroy, and Kohen, "The Effect of the

Minimum Wage."

¹⁹ The text and tables present results of simulations after taking an average of five simulations involving a specific value of the new minimum wage and published estimates of teenager and adult unemployment effects. A table showing results of the five simulations separately is available from the author on request.

²⁰ Kelly also simulated the ex-post poverty gap using a number of new minimum wages. One of his simulations used a 25-percent jump in the minimum wage (\$1.60 to \$2) and produced a 2.5-percent decline in the poverty gap.

The present simulation uses a 27-percent jump in the minimum wage (\$3.35 to \$4.25) and produces a 12.3-percent decline in the poverty gap.

²¹ U.S. Bureau of the Census, "Household and Family Characteristics: March 1986," *Current Population Reports*, Series P-20, No. 419, 1987; "Household and Family Characteristics: March 1986," *Current Population Reports*, Series P-20, No. 311, 1977; "Characteristics of the Low Income Population: 1973," *Current Population Reports*, Series P-60, No. 98, 1975; "Characteristics of the Poverty Population 1983," *Current Population Reports*, Series P-60, No. 160, 1988 (Washington, U.S. Government Printing Office.)

²² To measure the total benefit, one should count the additional income accruing to all poor and potentially poor families, those that would escape poverty and those that would not. The author plans to extend the model to make this calculation as well.

A note on communications

The *Monthly Labor Review* welcomes communications that supplement, challenge, or expand on research published in its pages. To be considered for publication, communications should be factual and analytical, not polemical in tone. Communications should be addressed to the Editor-in-Chief, *Monthly Labor Review*, Bureau of Labor Statistics, U.S. Department of Labor, Washington, D.C. 20212.



DRAFT

DEPARTMENT OF THE TREASURY
WASHINGTON

August 18, 1993 9:20 AM

MEMORANDUM FOR DEPUTY ASSISTANT TO THE PRESIDENT BOWMAN CUTTER

Through: Alicia Munnell, Asst. Secretary of the Treasury for Economic Policy

From: Brad De Long, Dep. Asst. Secretary of the Treasury for Financial Analysis

Subject: **ASSESSING GLOBALIZATION AS A CAUSE OF BLUE-COLLAR WAGE DECLINES**

"Globalization" is shorthand for the telecommunications, trade, and transport revolutions that allow enterprises to efficiently organize value chains that span continents. Henry Ford managed his automobile assembly line from Detroit, and so had to employ blue-collar workers in Detroit: his early twentieth-century span of control extended no further—and blue-collar workers in Detroit earned high wages because Ford could not play them off against potential workers in Florida, let alone workers in Thailand.

Today firms are increasingly able to spin their webs of enterprise across continents, and to move almost any component of the value chain that is labor-intensive to places where labor is cheap without losing efficiency. M.I.T. Press typesets its books in Hong Kong. Apple produces computers in Singapore (and Ireland). Swissair has announced a move of its accounting department to Bombay. But conventional wisdom is that the largest shift in relative demand for workers is a shift away from demand for high-priced U.S. blue-collar workers—that the collapse in relative blue-collar wages seen in the 1980s is the result of "globalization," and that the collapse is likely to continue in the 1990s as globalization accelerates.

Successful management of the ongoing process of "globalization" is essential if America is to fulfill its economic potential. But there is a tendency to overstate the impact that globalization has had to date—especially a tendency to incorrectly attribute a large share of the fall in the relative wages and relative numbers of blue-collar workers. I believe that *to date* the decline in blue-collar wages and numbers has other causes—technology and demand. It is not due to trade or globalization, or at least a smaller component of the past decline is due to trade and globalization than the conventional wisdom thinks.

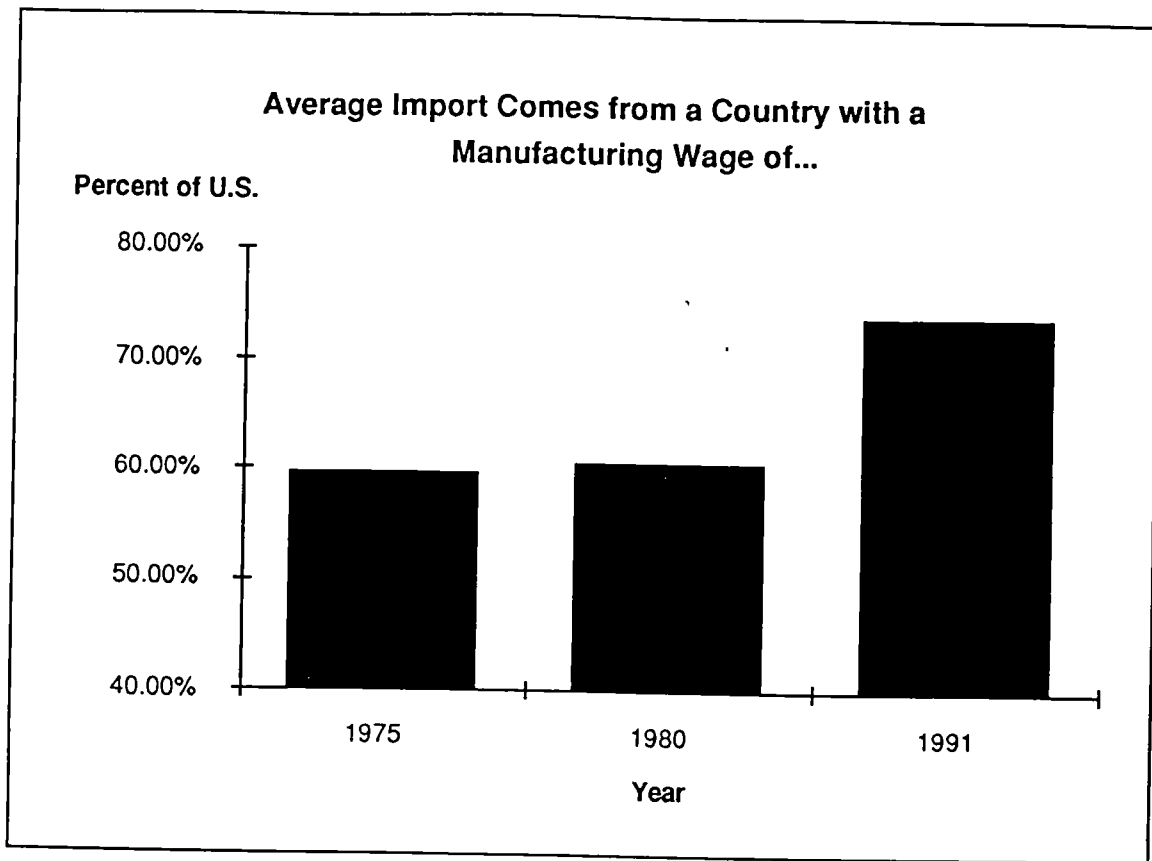
Note that I do not want to make any claims about how important “globalization” will be in the future. I am not yet confident enough that I have the pulse on long-run trends to want to commit to a position on future trends. This memo merely makes an argument that globalization—which is likely to be more important in the future—has not been very important in the past as a source of significant downward pressure on the wages and numbers of America’s blue-collar workers.

Here are seven reasons why I believe that globalization has been overrated:

- The U.S. is not facing increasing competition from low-wage countries. Existing U.S. trading partners have become richer—and seen their manufacturing wages increase—faster than newly-industrializing low-wage economies have been able to begin exporting to the United States. The wage level of the average import into the United States has grown from 59 percent of the U.S. level in 1975 to 73 percent of the U.S. level today.
- Imports are not the principal cause of falling blue-collar wages, or falling demand for blue-collar workers. The most rapid growth in the relative share of imports occurs in the 1970s, during which blue-collar wages keep pace with productivity and with the wages of white-collar workers.
- Why hasn’t trade been a factor placing significant downward pressure on U.S. blue-collar wages and jobs? Because U.S. international transactions are of three types, only one of which has the potential to put significant downward pressure on the wages of blue-collar workers.
 - Trades of U.S. goods for foreign goods: the decrease in blue-collar job demand because we do not produce what we import is roughly offset by the increase in blue-collar job demand by export-producing firms.
 - The trade deficit component—trades in which the U.S. imports foreign goods and foreigners take the dollars from their sales and invest them in America. The decrease in blue-collar demand in industries that produce the goods we import is roughly offset. Blue-collar job demand in construction, machine tools, and other investment-goods industries rises because foreign capital finances more investment in America than would take place otherwise.
 - Trades in which we import goods and export services: this is the only component that could put significant downward pressure on American blue-collar wages and jobs. Instead of managers, professionals, and technicians in

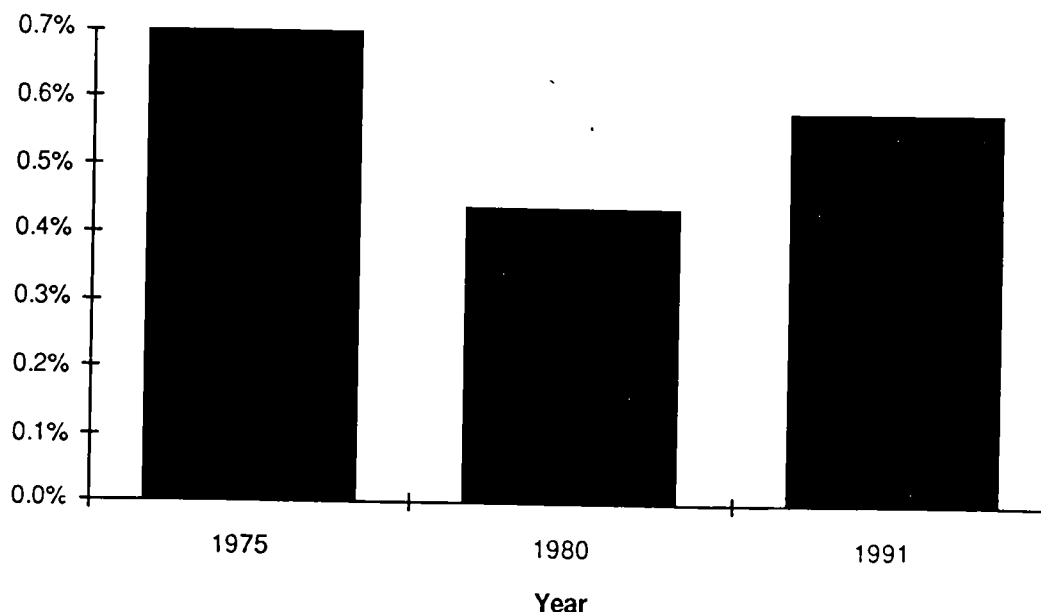
Chicago employing blue-collar workers in Gary, managers, professionals, and technicians in Chicago employ blue-collar workers in Thailand. We trade the services of what Labor Secretary Reich calls “symbolic analysts” for goods produced by their blue-collar workers. *But to date this component of trade is relatively small.*

- While in the future U.S. demand for blue-collar workers may be depressed if trade of our service exports for their manufactured products continues to grow rapidly, to date net service exports are still only a small share of national product. If we shifted the workers and resources that produce all service exports into producing goods, we would see an increase in blue-collar jobs (and a decrease in service sector jobs) of perhaps 600,000.
- In 1962 36% of the workforce were blue-collar “production workers”; in 1992 26% of the labor force falls into the blue-collar category. If today we had the same occupational distribution of employment that we had in 1962, there would be 12,000,000 more blue-collar jobs. Thus “Globalization”—trading our services for their goods—is responsible for only five percent of the relative decline in the share of blue-collar jobs in our economy over the past three decades.
- Moreover, most of our service-sector exports are not the products of high-paid “symbolic analysts.” The largest service-export categories are travel and transportation: someone making the bed of a busload of Japanese tourists on their way to Yellowstone. Travel and transportation provide a strong demand for low-wage workers.
- The shift to a post-industrial economy—the decline in blue-collar workers as a share of the non-agricultural labor force—has been underway for most of this century. It is not a new trend that has emerged in the last ten, or even the last twenty years. It is hard to blame a phenomenon that is seen only in the past decade or two—falling blue-collar wages—on trends that have been underway for the better part of a century.



- The manufacturing wage in the country providing the average import to the United States has not fallen but risen over time.
- In 1975 the average import to the United States came from a country where manufacturing compensation (at then-current exchange rates) was 59.6% of the United States. In 1991 the average import came from a country where manufacturing compensation was 73.9% of the United States level.
- Existing U.S. trading partners have become richer—and seen their manufacturing wages increase—faster than newly-industrializing low-wage economies have been able to begin exporting to the United States.

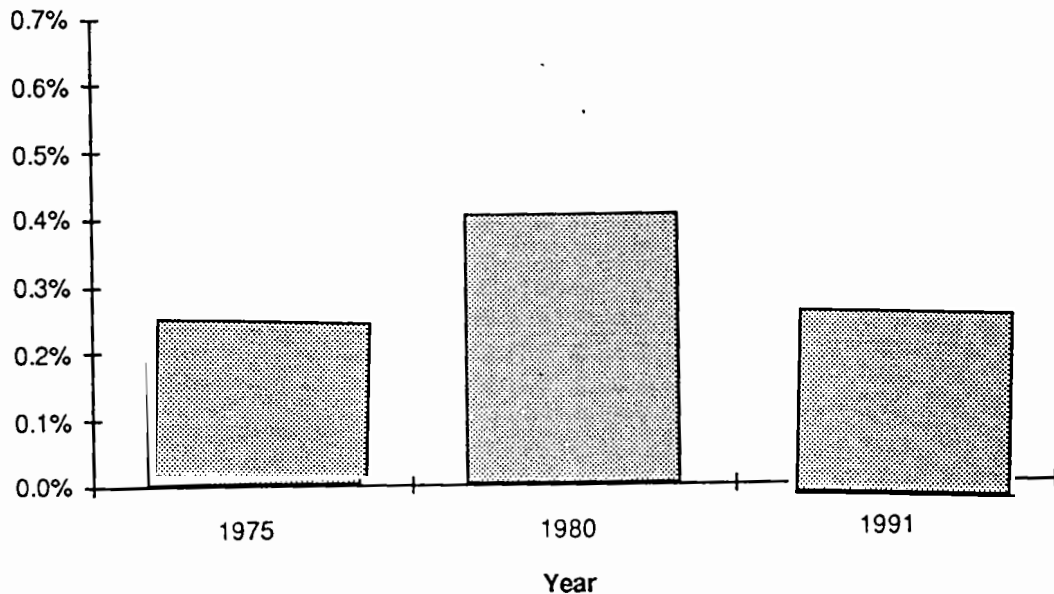
Non-Oil Imports from Low-Wage Countries,* as a Share of GDP



*Countries with manufacturing wage levels less than half the U.S.

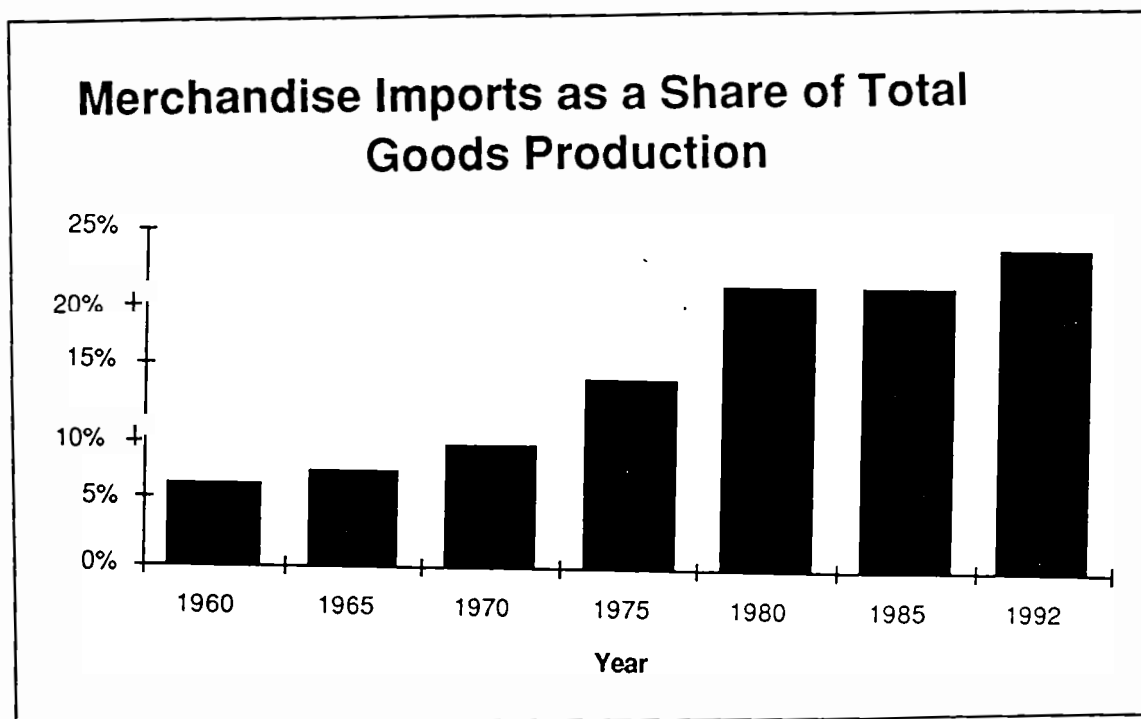
- Non-oil imports from low-wage countries are *lower* relative to U.S. national product than they were fifteen years ago.
- Japan was our second-largest trading partner in 1975, and is our second-largest trading partner today. But in 1975 Japan was a low-wage country—with manufacturing compensation 47 percent of the U.S.—and so imports from Japan did put downward pressure on U.S. wages in import-competing industries. But in 1991 Japanese manufacturing compensation was 93 percent of the U.S., and today it is higher than U.S. levels: imports from Japan no longer count as imports from places where labor is cheap.
- Imports from low-wage countries as a share of GDP have shrunk because those low-wage countries today that have become export platforms to the U.S. are less important trading partners than those countries that were low-wage producers in 1975 but are high-wage producers today.

Non-Oil Imports from Very Low-Wage Countries,* as a Share of GDP

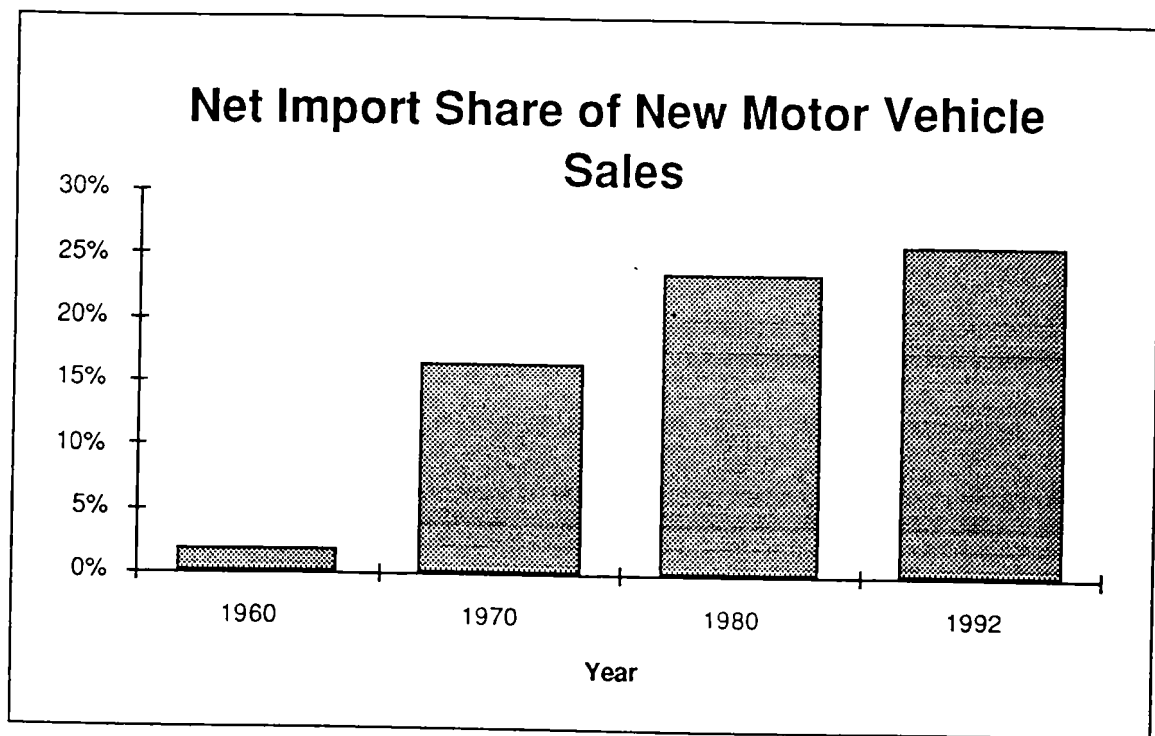


*Countries with manufacturing wage levels less than a quarter of the U.S.

- Non-oil imports from very low-wage countries are *lower* relative to U.S. national product than they were fifteen years ago.
- Taiwan, Singapore, and Korea were very low-wage economies in 1975, with wage levels one-twentieth that of the United States. Today their manufacturing wage levels are about a third of American levels.
- Thus a good deal of what would have been downward pressure on American wages had real incomes in Korea, Singapore, and Taiwan remains constant has disappeared: they are now middle-income economies, not poor economies. So U.S. firms actually face less relative competition from workers in very poor economies than they did two decades ago.



- Merchandise imports as a share of total goods output rose from six percent in 1960 to around fifteen percent in 1975—a period during which blue-collar wages and average productivity grew relatively rapidly.
- Since 1975 merchandise imports have grown no more rapidly than in the previous period—from fifteen to twenty-five percent of total goods output. Yet blue-collar wages, which rose rapidly before 1975, have stagnated since.

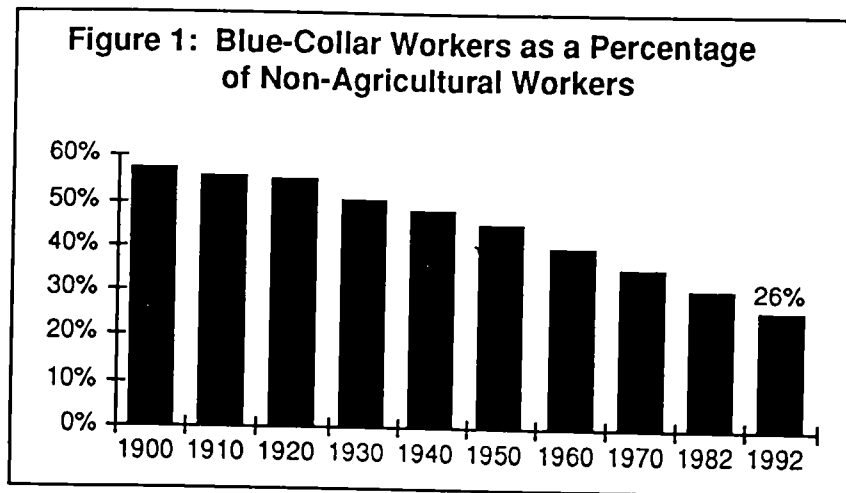


- The biggest leap in the share of motor vehicles that are imported came from 1960 to 1970—from 2 to 16 percent—a decade that saw blue-collar worker wages rise rapidly.
- The period since 1980, that has seen the most rapid relative decline in blue-collar wages, has seen next to no change in the import share of motor vehicle sales.

Service Trade In 1991 In Billions of Dollars

	<u>Exports</u>	<u>Imports</u>	<u>Balance</u>
Travel	\$53.86	\$39.87	\$13.99
Passenger Fares	\$17.35	\$10.94	\$6.41
Other Transportation	\$22.77	\$23.45	-\$0.68
Affiliated Services	\$33.73	\$13.83	\$19.90
Unaffiliated Bus., Prof. & Tech. Serv.	\$12.68	\$3.59	\$9.09
Unaffiliated Royalties and Licenses	\$4.13	\$1.75	\$2.38
Unaffiliated Education	\$6.14	\$0.80	\$5.34
Unaffiliated Financial Services	\$5.44	\$3.46	\$1.98
Unaffiliated Telecommunications	\$3.30	\$6.52	-\$3.22
Other Unaffiliated Services	\$8.42	\$3.02	\$5.40
Total	\$167.82	\$107.23	\$60.59

- The U.S. trade surplus in services in 1991 was only \$60 billion—one percent of GDP.
- If we had shifted the workers and resources that produce these service exports into producing goods, we would have seen an increase in blue-collar jobs (and a decrease in service sector jobs) of perhaps 600,000.
- In 1962 36% of the workforce were blue-collar “production workers”; in 1992 26% of the labor force falls into the blue-collar category. If today we had the same occupational distribution of employment that we had in 1962, there would be 12,000,000 more blue-collar jobs.
- “Globalization”—trading our services for their goods—is responsible for only five percent of the relative decline in the share of blue-collar jobs in our economy over the past three decades.
- Moreover, most of our service-sector exports are not the products of high-paid “symbolic analysts.” The largest service-export category (\$53 billion) in the table above is travel: someone making the bed of a busload of Japanese tourists on their way to Yellowstone.
- Travel and transportation make up 56% of service exports, and travel and transportation have a strong demand for low-wage workers. Even expanded service exports place only limited downward pressure on employer demand for less-skilled workers.



- The shift to a post-industrial economy—the decline in blue-collar workers as a share of the non-agricultural labor force—has been underway for most of this century. It is not a new trend that has emerged in the last ten, or even the last twenty years.
- Fully three-fifths of non-agricultural workers were blue-collar workers in 1900; only a quarter are in this category today. But the decline in the blue-collar job share has been gradual and steady: its pace has not significantly accelerated since the 1920s.
- The popular image of the non-agricultural workforce—of assembly-line workers, white-collar managers, and bosses, of large mass-production firms as the representative places in which Americans work—was never completely true. Even at the beginning of the 1930s blue-collar industrial workers made up only half of America's non-agricultural workforce, and blue-collar industrial workers in manufacturing and construction made up only 28 percent.

**Change in Employment Shares by
Industry and Occupation, 1962-1992**

	<u>Agriculture</u>	<u>Manufacturing Construction</u>	<u>Trans., Util., Dist. Businesses</u>	<u>Service- Delivery Industries</u>	<u>Other Non- Agricultural Industries</u>	<u>Total</u>
Farmers	-4.86%	0.09%	0.07%	0.37%	0.09%	-4.23%
Production workers	-0.19%	-7.53%	-1.48%	-0.12%	-0.87%	-10.20%
Service delivery workers	0.00%	-0.20%	1.32%	-0.64%	0.24%	0.72%
Sales, Cler., Admin Support	0.05%	-1.52%	3.89%	3.49%	0.46%	6.38%
Mgrs., Prof., & Tech. Wkrs.	0.07%	-0.15%	-2.02%	7.96%	1.46%	7.34%
TOTAL	-4.92%	-9.30%	1.78%	11.06%	1.38%	0.00%

**Employment by Industry and
Occupation, 1992**

	<u>Agriculture</u>	<u>Mfg. & Construction</u>	<u>Trans., Util., Dist. Business</u>	<u>Service- Delivery Industries</u>	<u>Other Non- Agricultural Industries</u>	<u>Total</u>
Farmers	2.32%	0.09%	0.07%	0.37%	0.09%	2.94%
Production workers	0.10%	14.51%	6.92%	3.22%	0.83%	25.58%
Service delivery workers	0.01%	0.31%	4.15%	7.77%	1.44%	13.69%
Sales, Cler., Admin Support	0.12%	2.99%	12.69%	6.44%	5.44%	27.68%
Mgrs., Prof., & Tech. Wkrs.	0.18%	5.04%	3.89%	16.85%	4.14%	30.11%
Total	2.73%	22.95%	27.72%	34.66%	11.95%	100.00%

EMPLOYMENT EFFECTS OF MINIMUM AND SUBMINIMUM WAGES: PANEL DATA ON STATE MINIMUM WAGE LAWS

DAVID NEUMARK and WILLIAM WASCHER*

Using panel data on state minimum wage laws and economic conditions for the years 1973–89, the authors reevaluate existing evidence on the effects of a minimum wage on employment. Their estimates indicate that a 10% increase in the minimum wage causes a decline of 1–2% in employment among teenagers and a decline of 1.5–2% in employment for young adults, similar to the ranges suggested by earlier time-series studies. The authors also find evidence that youth subminimum wage provisions enacted by state legislatures moderate the disemployment effects of minimum wages on teenagers.

A federal minimum wage was first implemented in the United States with the passage of the Fair Labor Standards Act of 1938, which now covers more than 90% of all workers. Since its enactment, there has been widespread debate about the merits of minimum wage laws, along with numerous efforts to evaluate their economic effects. By the early 1980s, the

amassed body of theoretical and empirical research by economists, including that of the Minimum Wage Study Commission (1981), suggested that the imposition of minimum wages decreases employment opportunities for workers with wages at or near the minimum wage. More explicitly, a 10% increase in the minimum wage apparently reduced teenage employment by 1% to 3%, with proportionately smaller effects for 20–24-year-olds, reflecting their smaller representation in the minimum wage population (Brown et al. 1982; Brown 1988). *EO's
Consensus*

* David Neumark is Assistant Professor of Economics at the University of Pennsylvania and a Faculty Research Fellow of the National Bureau of Economic Research. William Wascher is a Senior Economist at the Board of Governors of the Federal Reserve System. The authors gratefully acknowledge helpful comments from Orley Ashenfelter, Charles Brown, Andrew Foster, Daniel Hamermesh, George Jakubson, Alan Krueger, Paul Taubman, Wayne Vroman, Ron Warren, and seminar participants at the NBER Labor Studies Summer Institute, the Federal Reserve Board, the University of Pennsylvania, and the Cornell University "New Minimum Wage Research" conference, and research assistance from Donna Boswell and Richard Johnson. They also thank Wayne Vroman for providing minimum wage data for Washington, D.C.

The data set and programs used in this paper are available on request from David Neumark, Department of Economics, 518 McNeil Building, University of Pennsylvania, Philadelphia, PA 19104.

An important shortcoming of that body of research, however, is that the results were based almost exclusively on time-series data on the federal minimum wage, on workers covered by the federal minimum, and on the aggregate labor market. The authors of these time-series minimum wage studies often recognized the inadequacy of their data for empirical analysis of minimum wage effects (for example, Wachter and Kim 1978). Yet, out of the vast number of U.S. minimum wage studies, only about ten do not resort to time-series data. *Short-
comings*

In this study, we augment the national time-series data with information on geographic differences in the minimum wage. Individual states have frequently legislated changes in the level or coverage of the minimum wage, and although states cannot impose a minimum wage lower than the federal minimum (for workers covered by the federal minimum), many states have periodically raised their minimum wage above the federal level or extended coverage to workers excluded from federal legislation. In addition, states with minimum wages above the federal level have sometimes implemented exemptions from the higher state level for specific subgroups of the labor force, such as teenagers or students. Using this information, we construct a panel data set on minimum wage laws and labor market conditions at the state level for the years 1977-89 (or, for some states, 1973-89) to reevaluate the existing evidence on the consequences of minimum wages for the youth labor market. In our view, this integration of state data into the analysis of minimum wage effects makes three contributions to the literature on minimum wages.

Integration of state data into the analysis rather than just federal

(First), studies using the federal minimum wage in time-series analyses face a number of potential problems. The minimum wage variable used in these studies exhibits relatively little variation over time, and what variation exists is correlated with changes in government social welfare and training programs, and with the effects of the draft; this problem makes it difficult to isolate the effects of minimum wages. Moreover, state minimums are sometimes set above the federal level. A few states nearly always set minimum wage levels above the federal level; and in the latter part of the 1980s, as many as 25% of states had minimum wage levels above the federal level. The federal minimum wage variable used in past time-series studies therefore measures the effective minimum wage with error; this may be particularly severe in the latter part of the 1980s. In contrast, the data on state minimum wage laws and economic conditions permit a pooled time-series cross-

section analysis of minimum wage effects, exploiting the greater independent variation in relative minimum wages at the state level, and avoiding the measurement error caused by using a uniform federal minimum wage.

(Second), the use of panel data addresses an important criticism leveled at the small number of previous studies that used single cross-sections (in part to address some of the problems with time-series analyses of minimum wage effects). Most of these studies included either a dummy variable for the existence of a minimum wage law (Katz 1973; Freeman 1982) or data on state levels and coverage (Cotterill and Wadycki 1976; Welch and Cunningham 1978). As a result, much of the variation in the included minimum wage variable (typically the minimum wage level, multiplied by coverage, divided by an average wage) arose from variation in average wage levels across states, leading critics to argue that the estimated wage effects largely reflected state "average wage" effects rather than minimum wage differences (Brown et al. 1982). In contrast, the use of panel data permits explicit estimation of state (or year) effects as distinct from the effects of changes in the minimum wage variable, and so permits a cleaner evaluation of minimum wage effects from the cross-sectional (and time-series) variation in the data. In addition, considerable variation in the minimum wage variable comes from changes in legislated minimum wages at the state level during the latter part of the 1980s.

(Third), the use of state data permits a direct evaluation of the effects of lower minimum wage levels for subgroups of the population. The latest federal minimum wage legislation—effective in April 1990—attempted to alleviate the adverse effects of the minimum wage on the employment of youths by introducing, for the first time at the federal level, a "subminimum" wage for newly employed workers. Because a broad federal youth subminimum had never before been implemented, however, aggregate time-series data for the U.S. economy cannot be used to evaluate the potential for a subminimum to mitigate the disemploy-

ment effects of the minimum wage.¹ In contrast, numerous states have implemented subminimums for young workers or students in years past, so data on state legislation can be used to examine the effects of subminimum wage provisions.

Some recent research has challenged the finding of the earlier time-series studies that increases in minimum wages reduce employment of teenagers and young adults, with elasticities in the range of -0.1 to -0.3 . Wellington (1991) argues that the addition to the sample period of the 1980s (through 1986), during which the minimum wage fell sharply in real terms, has the effect of reducing the estimated disemployment effects. For a hypothesized 10% rise in the minimum wage, she reports disemployment effects of less than 1% for teenagers and essentially zero for young adults. Card, in "Do Minimum Wages Reduce Employment?" (in this issue), compares the employment experiences of Californian workers during the late 1980s, when the state minimum wage level rose sharply, with the experiences of similar workers in labor markets with no changes in minimum wage laws. He finds no evidence of a disemployment effect from the higher minimum wage. In his other paper in this issue ("Using Regional Variation in Wages . . ."), Card draws a similar conclusion from a short panel data set on all states, for the period 1989–90. Finally, Katz and Krueger, in a survey of fast-food restaurants in Texas (also in this issue), find no evidence of declines in employment in response to the 1990

increase in the federal minimum wage, at least in establishments that remained in operation following the minimum wage increase. In contrast, our estimates corroborate the range of estimates from existing time-series studies (Brown et al. 1981), with elasticities concentrated from -0.1 to -0.2 . We conduct numerous sensitivity analyses to attempt to reconcile the apparently conflicting findings.

The State Minimum Wage Panel Data Set

The panel data set of minimum wages, minimum wage coverage, and local economic conditions consists of annual observations covering the 50 states and the District of Columbia for the years 1977 through 1989, and extending back to 1973 for a subset of 22 larger states for which the CPS identified state of residence from 1973 through 1976. We obtained from each state labor department a chronology of applicable minimum wage legislation dating back to the early 1970s. These data were cross-checked against information available from the Bureau of National Affairs' Compensation Primer and the chronologies constructed by Questor (1981).²

In most cases, this procedure yielded a single time series of the minimum wage level, from which we extracted the value in effect during May of each year (for consistency with the data on state labor market conditions described below). For a few states, however, the state minimum wage level differed by occupation or labor force group, and additional steps were necessary to obtain a single value that best captured the effective state law. Where the varying levels clearly were subminimums for youths, students, newly covered workers, or very low-skilled occupations, we used the highest value in each year. In two

¹ Past legislation sometimes permitted some classes of employers (primarily colleges and universities) to pay subminimum wages to full-time students, but had never before been generalized to all young or new workers. In a study for the Minimum Wage Study Commission, Freeman et al. (1981) analyzed this specific provision using a data set constructed from records of private sector employers certified to hire full-time students. Their main focus, however, was on the micro-level variables associated with the employment of workers under this subminimum wage. Brown et al. (1983) found that adjusting the Kaitz index of the minimum wage for coverage exemptions for students had no discernible impact on time-series estimates of minimum wage effects.

² Our principal focus is on legislated minimum wage levels, rather than state laws extending minimum wage coverage. As explained below, estimates of coverage by state laws are hard to come by. In addition, in the few time-series studies that consider the effects of coverage separately from the effects of (relative) minimum wage levels, coverage effects tend to be weaker (Brown et al. 1982).

other cases, the existence of multiple minimum wage levels was not automatically suggestive of a subminimum. In the District of Columbia there are nine separate minimum wage levels for different industries and occupations, as well as differing minimum wage levels for youths, students, and JTPA workers. In this case, we used the weighted average of the minimum wage levels for adults across the nine categories, weighted by estimated employment in each category in each year. In Minnesota in recent years, the state minimum wage level for workers covered by the federal law (FLSA) differed from the level for those covered only by state law; in this case, we used the state minimum for workers covered by the FLSA.³

Table 1, column (1) shows the states with legislated minimum wage levels above the federal level for each year in our sample, along with the legislated levels. Throughout the 1970s and much of the 1980s, only a few states set a minimum wage above the federal level.⁴ By 1989, however, the number of states with higher minimums had risen to 13.⁵ Columns (2) and (3) report the federal minimum wage level for each year and the (unweighted) average percentage difference between the state and federal minimum wage levels for states with legislated minimum wage levels exceeding the federal level. Perhaps not surpris-

ingly—given that states tended to raise their minimum wages when the federal minimum wage was stagnant—the average percentage differential is greatest when the number of states with minimums exceeding the federal minimum is largest, rising to a peak of 16% in 1989.

In addition to minimum wage levels, for each state we collected information on the existence of subminimum wage provisions that permit employers to pay a lower wage to specific subgroups of the labor force. Generally, subminimum wage provisions enacted by state legislatures in the past have taken two forms: a subminimum (or exemption) based on age; or one based on student or apprentice (learner) classification. As shown in Table 1, most states with minimum wages above the federal level had subminimum wage provisions in their minimum wage legislation. Columns (6) and (7) of Table 1 also show that over the sample period, about half of all states have had youth subminimums, and two-thirds of all states have had student/apprentice subminimums.

Comprehensive time-series information on state minimum wage coverage was more difficult to assemble. For the federal law, the Department of Labor has published estimates of the number of wage and salary workers in each state by their coverage status under the minimum wage provisions of the FLSA for most years in our sample. For coverage by state laws (above and beyond FLSA coverage), data are available from the Department of Labor only for the years 1974, 1975, and 1977; as a result, we use the FLSA coverage estimates for each state for the available years. For years with no official estimates (1979–81), we assumed that federal coverage on a state-by-state basis changed in proportion to the change in coverage for the United States as a whole. For the years after 1986 (the latest data available), we assumed that coverage rates held steady at their 1986 level. Because FLSA coverage differs across states, this approach seems preferable to using just data on minimum wage levels, and should help to reduce measurement error in the effective minimum wage.

³ This difference is relevant in Minnesota because the state minimum wage levels for both groups were higher than the federal minimum wage in 1988 and 1989. New legislation effective in 1991 eliminated this two-tier schedule, although the new structure sets a higher minimum wage for large employers than for small employers (who are less likely to be covered by the FLSA).

⁴ Legislation in both Connecticut (beginning in 1974) and Alaska (beginning in 1977) automatically keeps the state minimum wage above the federal level. In Alaska, a constant differential of 50 cents per hour is maintained. In Connecticut, the law through 1987 set the state minimum .5% above the federal level, resulting in a differential of just a few cents.

⁵ With the increase in the federal minimum to \$3.80 per hour in 1990 and \$4.25 in 1991, the number of states with higher minimum levels has again dropped, to five in 1991.

Table 1. State Minimum Wage Levels, 1973-89.

States with Minimum Wages Exceeding Federal Minimum ^a (1)	Federal Minimum (2)	Average % Difference Between State and Federal Minimum (3)	Average Coverage-Adjusted Relative Minimum Wages		Proportion (Number of Changers) with Subminimums: Student/ Apprentice Youth	
			State Minimum > Federal Minimum (4)	State Minimum = Federal Minimum (5)	Student/ Apprentice (6)	Youth (7)
1973 CA ^y CT ^y DC ^y MA ^s NJ ^y NY ^y 1.65 1.85 2.16 1.85 1.75 1.85	1.60	15.7	.30	.31	.55	.64
1974 CT ^y DC ^y 2.01 2.19	2.00	5.0	.29	.36	.59 (1)	.59 (1)
1975 CT ^y DC ^y NJ ^y 2.11 2.45 2.20	2.10	7.3	.30	.35	.59 (0)	.55 (1)
1976 CT ^y DC ^y HI ^s 2.31 2.55 2.40	2.30	5.2	.31	.36	.59 (0)	.55 (0)
1977 AK ^y CA ^y CT ^y DC ^y HI ^s NJ ^y 2.80 2.50 2.31 2.76 2.40 2.50	2.30	10.7	.33	.35	.63 (2)	.51 (1)
1978 AK ^y CT ^y DC ^y 3.15 2.66 2.79	2.65	8.1	.33	.38	.65 (1)	.53 (1)
1979 AK ^y CT ^y DC ^y 3.40 2.91 2.95	2.90	6.4	.33	.38	.63 (1)	.55 (1)
1980 AK ^y CT ^y DC ^y 3.60 3.12 3.14	3.10	6.0	.32	.38	.63 (0)	.55 (0)
1981 AK ^y CT ^y DC ^y 3.85 3.37 3.48	3.35	6.5	.34	.38	.63 (0)	.55 (0)
1982 AK ^y CT ^y DC ^y 3.85 3.37 3.62	3.35	7.9	.30	.35	.59 (2)	.53 (1)
1983 AK ^y CT ^y DC ^y 3.85 3.37 3.82	3.35	9.9	.32	.34	.59 (0)	.53 (0)
1984 AK ^y CT ^y DC ^y 3.85 3.37 3.82	3.35	9.9	.32	.33	.59 (0)	.53 (0)
1985 AK ^y CT ^y DC ^y ME ^s 3.85 3.37 3.85 3.45	3.35	8.4	.35	.35	.59 (0)	.53 (0)
1986 AK ^y CT ^y DC ^y ME ^s 3.85 3.37 3.86 3.55	3.35	9.2	.33	.34	.67 (6)	.53 (0)
1987 AK ^y CT ^y DC ^y MA ^y ME ^s NH ^y 3.85 3.37 4.16 3.55 3.65 3.45 RI ^y VT ^y 3.55 3.45	3.35	8.3	.34	.34	.67 (0)	.53 (0)
1988 AK ^y CT ^y DC ^y HI ^s MA ^y ME ^s 3.85 3.75 4.33 3.85 3.65 3.65 MN ^y NH ^y RI ^y VT ^y 3.55 3.55 3.65 3.55	3.35	11.6	.33	.33	.67 (0)	.53 (0)
1989 AK ^y CA ^y CT ^y DC ^y HI ^s MA ^y 3.85 4.25 4.25 4.33 3.85 3.75 ME ^s MN ^y NH ^y PA ^s RI ^y 3.75 3.85 3.65 3.70 4.00 VT ^y WA ^y 3.65 3.85	3.35	16.5	.33	.32	.67 (0)	.55 (1)

1991 only 5 states above Fed.

States with postal codes underlined had relative coverage-adjusted minimum wages in the upper quartile. High-wage states with student/apprentice subminimum wage provisions are denoted with an "s" superscript, and those with youth subminimum wage provisions are denoted with a "y" superscript.

Sources: State labor departments; U.S. Department of Labor, *Minimum Wages and Maximum Hours Standards*, various issues; and authors' calculations from Current Population Survey microdata tapes.

For each state-year observation, a coverage-adjusted minimum wage is computed as the product of the greater of the federal or state minimum wage, and federal coverage for the state. We then computed, for each state, the ratio of the coverage-adjusted minimum wage prevailing in May of each year to the state average hourly wage during the same month; this is the variable used in the analysis.⁶ Columns (4) and (5) of Table 1 provide more information on the role of state minimum wage laws in influencing effective minimum wages. These columns report the average coverage-adjusted relative minimum wage variable separately for states with minimum wage levels exceeding the federal level and states in which the federal minimum wage level is binding.

A comparison of columns (4) and (5) reveals that minimum wage levels higher than the federal minimum generally did not result in higher relative minimum wages; indeed, throughout most of the sample period, relative minimum wages were higher in states *without* minimum wage levels exceeding the federal level, reflecting the lower average market wage in those states. This pattern is also revealed by the small number of states with minimum wages exceeding the federal minimum that were in the upper quartile of the distribution of

the coverage-adjusted relative minimum wage in each year; these states are underlined in the first column of Table 1. Over the 1980s, however, the average relative minimum wage in states in which the federal minimum wage was binding declined, and by the end of the sample period, the average relative minimum wage was roughly the same for both sets of states. Thus, when evaluated in terms of changes, the rising incidence of state minimum wage laws did boost relative minimum wages during the 1980s.

For data on state labor market conditions over the same period, we also used the May files of the Current Population Survey (CPS). Variables estimated from the CPS include employment rates for teenagers (aged 16–19) and young adults (16–24); unemployment rates for prime-age (25–64) men; proportions of the population aged 16–19 or 16–24; and the proportions of individuals aged 16–19 or 16–24 enrolled in school. In all cases, the variables are calculated from the individual survey responses, aggregated to the state level using the CPS demographic weights.⁷

Empirical Evidence on Minimum Wage Effects

Reconsidering the Existing Evidence

As noted above, nearly all past minimum wage studies have focused on time-series data at the national level. In particular, the typical time-series study estimates a regression equation of the form

$$(1) \quad E_t = \alpha_0 + \alpha_1 MW_t + X_t \beta + \epsilon_t$$

E is the employment-to-population ratio for the age group under study;⁸ MW is a cov-

⁶ Because state-specific wage rates are not published outside of manufacturing, the average state wages used are estimated as the mean usual hourly wage from the May Current Population Surveys (CPS). We chose the May CPS because, prior to 1983, the questions pertaining to usual weekly hours and earnings were only asked in May. For all data computed from the CPS, we deleted persons under age 16, self-employed workers, unpaid family workers, and persons indicating agricultural production or agricultural services as their current or most recent industry.

The ratio of the coverage-adjusted minimum wage to the average wage for the age group studied may better indicate how much the minimum wage cuts into the wage distribution. For many states, however, the cell sizes from which we can compute mean wages for teenagers and young adults are quite small (especially after 1982, when wage information was elicited from only one-fourth of the sample). In addition, the average teen wage is heavily influenced by the minimum wage.

⁷ Madden (1991) reports that the January 1985 reweighting of the CPS, when data from the 1980 U.S. Census were incorporated, resulted in shifts toward more prosperous households (presumably reflecting the shift of the population from urban to suburban areas), with possibly different employment rates. The individual-year dummy variables that we include should capture the effects of this reweighting.

⁸ We focus on the employment-to-population

erage-adjusted minimum wage variable; X is a set of variables to capture aggregate business-cycle effects, the changing age structure of the population, and, in some specifications, school enrollment rates; and the "t" subscript indicates the year or quarter that the data describe. Although employment and wages are determined by the interaction of labor supply and labor demand, this equation is assumed to represent a reduced form capturing the effects of exogenous variables on equilibrium employment.⁹ Existing evidence on minimum wage effects estimated from specifications like equation (1) indicates elasticities in the range of -0.1 to -0.3 for teenagers aged 16–19, and somewhat smaller elasticities for 20–24-year-olds (Brown et al. 1982; and Brown 1988).

In contrast, the data on state minimum wage laws and state-level economic conditions permit a pooled time-series cross-section analysis of minimum wage effects, exploiting the greater variation in relative minimum wages at the state level, and avoiding the measurement error caused by using a uniform federal minimum wage. Moreover, because the variation in minimum wage levels across states should be more independent of the growth of social welfare and training programs than are changes in the federal minimum, this analysis should yield more reliable estimates of the effects of minimum wages on employment and unemployment of teenagers and young adults.

Specifically, our panel data set permits us to estimate an equation of the form

$$(2) \quad E_{it} = \alpha_0 + \alpha_1 MW_{it} + X_{it}\beta + Y_t\gamma + S_i\delta + \epsilon_{it},$$

where i indexes states and t indexes years.

ratio, rather than the unemployment rate, because, as pointed out by Mincer (1976), the effects of minimum wages on unemployment rates are ambiguous.

⁹ As Brown et al. (1982) point out, a simple supply and demand model with homogeneous workers implies that in the presence of a (binding) minimum wage, employment is demand-determined. Supply variables become important because many workers, including teenagers, earn more than the minimum wage. Thus, overall employment will depend on supply as well as demand variables.

Y_t is a set of fixed year effects, and S_i is a set of fixed state effects. Throughout, we assume that ϵ_{it} is serially uncorrelated and orthogonal to the regressors, but that Y_t and S_i may be correlated with the other regressors. For the analysis, 751 observations are available (data for 50 states and Washington, D.C. multiplied by the 13 years for which complete data are available, plus an additional four years of data for the 22 larger states identified in the CPS from 1973 through 1976).

The inclusion of state effects addresses a major criticism of studies that use purely cross-sectional data: that unmeasured economic conditions of state economies might give rise to persistently tight labor markets and high wages in particular states (Freeman 1982). In equation (2), such a relationship would generate a negative correlation between MW_{it} and E_{it} , and hence an estimate of the disemployment effect that is too large. Panel data can resolve this problem, while still exploiting geographic variation. We can also control for year effects in the data, corresponding perhaps to business-cycle or cohort-size effects that are not captured in the variables usually included in minimum wage studies; this problem is insurmountable in a time-series study.¹⁰

An important specification issue with

¹⁰ Only three studies for the United States and one for Canada use repeated observations on states (or regions) to remove the influence of state (or region) effects on cross-sectional estimates. For the United States, Cunningham (1981) and Cogan (1981) use states as the unit of observation, and attempt to identify minimum wage effects from changes in employment rates across decennial Censuses; neither of these studies uses information on youth or student subminimums, and only the Cunningham paper uses data on state minimum wage levels. Lester (1946) provides a similar experiment, comparing employment growth in particular industries in the North and the South following the implementation of the FLSA, which had a larger impact in the lower-wage South. Swidinsky (1980), studying minimum wage effects in Canada, takes an approach most similar to ours, using data for five regions over a 20-year period. He reports an employment elasticity of -0.17 for Canadian teenagers. Swidinsky also notes that youth and student subminimum wages vary across regions, but does not carry out a direct analysis of the moderating effects of subminimum wages.

respect to equation (2) is whether to include the proportion of the age group enrolled in school as an independent variable. The reason for including school enrollment is to capture exogenous variation in schooling patterns, due, for example, to changes in the costs of or returns to education or in mandatory schooling laws. Omitting the enrollment rate can lead to specification error, because enrollment rates and minimum wages may be (partially) correlated. *A priori*, the sign of this correlation is ambiguous. High minimum wages may lead young persons to stay in school, either because of worsened employment opportunities or because schooling increases the probability of employment in the covered sector (Leighton and Mincer 1981). Alternatively, if being in school makes job search more difficult, high minimum wages may reduce enrollment as young persons leave school to queue for minimum wage jobs (Mincer 1976).

On the other hand, including the school enrollment rate (without instrumenting) may lead to endogeneity bias. Because school and work represent alternative opportunities for many young persons, exogenous changes in employment rates may affect enrollment rates as well, imparting a negative bias to the coefficient of the school enrollment variable (if factors associated with high employment rates lead to low enrollment rates). The bias transmitted to the coefficient of the minimum wage variable again is ambiguous *a priori*, depending on the (partial) correlation between the enrollment rate and the minimum wage variable; a negative correlation will lead to an overly large estimated minimum wage effect.

The bias from the specification error and the endogeneity bias, however, will be of opposite signs, suggesting that estimates from specifications alternately including and excluding the enrollment rate will bracket the true minimum wage effect. One potential solution, of course, is to include the school enrollment rate and instrument for it. Unfortunately, we do not believe that there are valid exclusion

restrictions on the basis of which to instrument for the enrollment variable. Instead, throughout the paper we report least squares results both excluding and including the enrollment rate, and present additional evidence on the likelihood of bias in each specification.

Table 2 reports results from alternative specifications of the pooled time-series cross-section standard minimum wage model. Panel A reports specifications with different combinations of fixed state and year effects for teenagers (aged 16–19), and B repeats that analysis for young adults (16–24). Unless otherwise noted, the standard within-group estimator is used to estimate the fixed-effects models.

Columns (1) and (2) report estimates from specifications using fixed state and year effects. In the specifications excluding the school enrollment rate, the estimated minimum wage effect is slightly positive for teenagers and is negative for young adults, and is marginally significant only for young adults. In the specifications including the school enrollment rate, the disemployment effects are negative and statistically significant for both teenagers and young adults, with a larger disemployment effect for teenagers. The corresponding elasticities of the employment-to-population ratio with respect to the minimum wage variable, evaluated at the sample means, are reported in the last row of each table.

The remaining columns of Table 2 report estimates from specifications excluding year effects, excluding state effects, and excluding both. The estimated minimum wage effects vary considerably as different fixed effects are excluded. The elasticities are more positive when year effects are dropped (as compared to columns (1) and (2)) and more negative when state effects are dropped. The differences between the estimated elasticities with and without the fixed state effects confirm the suspicions of critics of earlier cross-section studies. Omitting state effects imparts a negative bias to the minimum wage estimates, suggesting that unmeasured local economic conditions can com-

Table 2. Within-Group and OLS Estimates of Minimum Wage Effects on the Employment-to-Population Ratio, Teenagers (16-19) and Young Adults (16-24).^a
(Standard Errors in Parentheses)

Independent Variable	Specification							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Teenagers								
Minimum Wage	.07 (.10)	-.17 (.07)	.21 (.07)	-.11 (.05)	-.33 (.08)	-.25 (.04)	-.18 (.07)	-.23 (.04)
Proportion of Population in Age Group	-.19 (.22)	-.11 (.15)	-.24 (.15)	-.13 (.10)	.35 (.28)	-.05 (.15)	.02 (.21)	-.16 (.11)
Prime-Age Male Unemployment Rate	-.54 (.11)	-.31 (.07)	-.86 (.08)	-.63 (.06)	-1.53 (.13)	-.76 (.07)	-1.39 (.11)	-.80 (.06)
Proportion of Age Group in School	—	-.75 (.03)	—	-.75 (.03)	—	-.95 (.02)	—	-.96 (.02)
Year Effects	Y	Y	N	N	Y	Y	N	N
State Effects	Y	Y	Y	Y	N	N	N	N
p-value for Restricted Model ^b	—	—	.00	.00	.00	.00	.00	.00
R ²	.69	.86	.68	.85	.20	.78	.18	.78
Elasticity ^c	.06	-.14	.17	-.09	-.27	-.20	-.14	-.18
B. Young Adults^d								
Minimum Wage	-.11 (.07)	-.16 (.06)	.02 (.06)	-.12 (.04)	-.21 (.06)	-.23 (.04)	-.14 (.05)	-.21 (.04)
Proportion of Population in Age Group	.42 (.10)	.08 (.07)	-.19 (.06)	-.24 (.05)	.28 (.11)	-.16 (.08)	-.11 (.08)	-.27 (.05)
Prime-Age Male Unemployment Rate	-.53 (.08)	-.47 (.06)	-.70 (.06)	-.69 (.05)	-1.36 (.09)	-1.13 (.07)	-1.17 (.08)	-1.06 (.05)
Proportion of Age Group in School	—	-.80 (.04)	—	-.82 (.04)	—	-.99 (.03)	—	-1.02 (.03)
p-value for Restricted Model ^b	—	—	.00	.00	.00	.00	.00	.00
R ²	.70	.82	.66	.80	.28	.66	.23	.65
Elasticity ^c	-.07	-.10	.01	-.07	-.13	-.14	-.09	-.13

^a The sample consists of 751 observations covering the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1973-89; for the remaining states it covers the period 1977-89. The minimum wage variable is the greater of the state or federal minimum wage level, multiplied by federal minimum wage coverage for the state, divided by the average wage in the state. The prime-age male unemployment rate is for men aged 25-64.

^b Likelihood-ratio test.

^c Evaluated at sample means.

^d The inclusion/exclusion of state and year effects for young adults parallels that for teenagers.

plicate the estimation of minimum wage effects in such studies. The other coefficients also change when fixed effects are excluded, most noticeably the estimated coefficient on the prime-age male unemployment rate. These changes in coefficients, along with the rejection of the restrictions imposed by dropping either the state or year effects, lead us to select as

the best specifications those with fixed state and year effects.¹¹

¹¹ In results not reported in the tables, we checked the validity of the linear specification of our fixed year- and state-effects specification. We set up a grid search over a specification of the model with a Box-Cox transformation of the dependent and

More generally, minimum wage elasticities from our fixed-effects models that include the school enrollment variable are broadly consistent with the evidence presented by Brown et al. (1982), if perhaps toward the lower end of their consensus range. However, the positive estimated minimum wage elasticity for teenagers from the fixed-effects model that excludes the school enrollment rate is unusual.¹² A critical question, then, is whether the change in the minimum wage effect for teenagers in these alternative specifications reflects specification error in the equation excluding this rate, or instead reflects bias in the column (2) estimates stemming from endogeneity of the enrollment rate. For two reasons, we believe that the specification error that results from omitting the school enrollment rate is more severe than the endogeneity bias that results from including the enrollment rate.

First, one might expect minimum wage effects, if they are present, to be stronger for teenagers than for young adults, since more teenagers earn wages near the

independent variables, with the same transformation applied to all variables; the Box-Cox parameter ranged from 0 to 1 in increments of 0.1. For all four alternatives (teenagers and young adults, with and without the school enrollment rate), the likelihood was monotonically increasing as the Box-Cox parameter increased. This result validates the linear specification, and in particular rules out a double-log specification. (A double-log specification seems dubious on *a priori* grounds, since it implies that an increase in the minimum wage from 50 cents to one dollar has the same percentage effect on employment as an increase from two dollars to four dollars.)

¹² The increase in the estimated disemployment effects of minimum wages for teenagers, when school enrollment rates are added to the equation, implies that the partial correlation between the school enrollment rate and the minimum wage variable is negative for this group. In specifications identical to those in column (1) of Table 2 except that the enrollment rate is the dependent variable, the estimated coefficient (standard error) of the minimum wage variable was -0.32 (0.10) for teenagers and -0.07 (0.06) for young adults. In "Do Minimum Wages Reduce Employment?" (this issue), Card finds a similar result for California: the school enrollment rate of teenagers fell with the rise in the minimum wage. In contrast, Mattila (1978) finds that, in time-series data, minimum wages are positively associated with enrollment rates.

minimum. This expectation is confirmed only in the specifications including the enrollment rate; in the specifications excluding the enrollment rate, the estimated employment effects are negative only for young adults. The same general result appears in later tables; in specifications excluding the school enrollment rate, minimum wage effects are weaker (although often negative) for teenagers than for young adults, whereas the reverse holds for specifications including the enrollment rate.

Second, the potential endogeneity bias arises from an auxiliary equation in which the enrollment rate is a function of, among other variables, the employment rate. Factors that increase the employment rate through ϵ_{it} in equation (2) then reduce the enrollment rate in the auxiliary equation, leading to an overly strong negative estimated correlation between employment and enrollment rates in equation (2). Presumably, one important factor that shifts the employment rate is the level of economic activity. If so, then we would expect the endogeneity bias in the coefficient of the enrollment rate, in equation (2), to be much more severe if the prime-age male unemployment rate is excluded from the equation. But when the unemployment rate is excluded from the specifications shown in Table 2, the estimated coefficient of the enrollment rate is virtually the same as in the specifications including the unemployment rate; the coefficient differs by at most 0.02 for the specifications including year and state effects. Because the inclusion of the unemployment rate should remove a significant portion of the endogeneity bias, these results suggest that endogeneity bias is relatively unimportant. This conclusion leads us to prefer the specifications including the school enrollment rate as providing more nearly unbiased estimates of minimum wage effects.¹³

¹³ We should point out, however, that the implied estimate of the effect of the enrollment rate on the employment rate (in the range -0.75 to -0.82) is large compared with the mean difference in employ-

Other potential biases. Another possible source of bias arises from the potential endogeneity of state minimum wage levels. If state legislators choose the timing of minimum wage increases to correspond to periods in which minimum wage increases will have the smallest (or perhaps least noticed) disemployment effects, the estimated minimum wage effects in Table 2 will be biased upward, as long as economic conditions are not fully captured in the prime-age male unemployment rate. To test for this type of bias, we consider instrumental-variables estimation of the fixed-effects specifications from Table 2.

Instrumental-variables estimation is straightforward in the presence of fixed effects; the state and year effects simply have to be included in the first-stage regression of the endogenous variable on the exogenous variables and the instruments. Our instrumental variable is the mean of the minimum wage level in all geographically bordering states (adjusted for coverage in the state by the FLSA, and divided by the mean wage in the state). This instrument will be valid if higher minimum wage levels in neighboring states influence the likelihood of enacting a higher minimum wage in a state, but neither affect nor are affected by labor market conditions in the state.¹⁴ On the other hand, minimum wage levels in bordering states may influence a state's labor market conditions through the mo-

bility of firms, in which case the minimum wage in bordering states may belong in the employment equation.

Two-stage least squares estimates of the fixed-effects specification are reported in panel A of Table 3. For both teenagers and young adults, the instrumental-variables estimates of the employment elasticities are more strongly negative, consistent with endogeneity bias generating a positive correlation between employment rates and minimum wages. The standard errors of the estimates, however, are large enough that the coefficient estimates are not significantly different from those that ignored endogeneity, and hence provide no statistical evidence of endogeneity bias. In view of these results, along with concerns about the validity of the instrument, we ignore the endogeneity of minimum wages in the rest of this paper, although this is a potentially promising avenue for future research.

A second potential problem with estimating minimum wage effects from equation (2) is that the denominator of the relative minimum wage variable, which is the average level of wages in the state, may reflect the interaction of labor demand and supply. For example, exogenous labor demand shifts will induce a positive correlation between employment rates and average wages, possibly causing a spurious negative correlation between the minimum wage variable and employment rates, because the average wage appears in the denominator of the minimum wage variable. Of course, because year effects are included in the models estimated, only state-specific variation in labor demand shifts would create this bias. Unfortunately, there do not appear to be valid identifying assumptions to address this problem by instrumenting for the average wage with an exogenous variable from the labor demand function; equation (2) is intended to be the reduced form of the labor demand and supply equations (aside from this problem) and therefore already includes all exogenous variables from these equations.

Nonetheless, we would like to verify that the minimum wage effects estimated in

ment rates between enrolled and non-enrolled youth: for example, for 16-24 year-olds in 1978 (roughly the middle of our sample), the employment rates were 42.5% for the enrolled and 73.3% for the non-enrolled. These differences might be suggestive of downward simultaneity bias in the estimated coefficient of the enrollment rate. Of course, the raw difference in means could easily understate (or overstate) the "causal" effect of the enrollment rate by ignoring the effects of other variables, or by ignoring heterogeneity bias captured by the fixed state effects in Table 2. Nonetheless, these differences point to the value of efforts to address directly the endogeneity of enrollment rates in minimum wage equations, through the construction of valid instruments.

¹⁴ In Maine, for example, the state minimum wage law mandates that the minimum wage cannot exceed the average minimum wage in the other five New England states.

Table 3. Examination of Potential Biases in Within-Group Estimates of Minimum Wage Effects.^a
(Standard Errors in Parentheses)

Independent Variable	Teenagers (16-19)		Young Adults (16-24)	
	(1)	(2)	(3)	(4)
A. Instrumental-Variables Estimates				
Minimum Wage	-.02 (.20)	-.28 (.14)	-.33 (.15)	-.32 (.11)
Proportion of Population in Age Group	-.18 (.22)	-.08 (.15)	.43 (.10)	.09 (.08)
Prime-Age Male Unemployment Rate	-.53 (.11)	-.31 (.07)	-.52 (.08)	-.46 (.06)
Proportion of Age Group in School	—	-.76 (.03)	—	-.80 (.04)
Elasticity ^b	-.01	-.22	-.21	-.20
B. Separating Mean Wage from Coverage-Adjusted Minimum Wage				
Coverage-Adjusted Minimum Wage Level	.01 (.02)	-.03 (.01)	-.02 (.02)	-.03 (.01)
Mean Wage	-.01 (.01)	.004 (.004)	.003 (.004)	.01 (.003)
Proportion of Population in Age Group	-.24 (.22)	-.12 (.15)	.42 (.10)	.09 (.07)
Prime-Age Male Unemployment Rate	-.55 (.11)	-.33 (.07)	-.53 (.08)	-.47 (.06)
Proportion of Age Group in School	—	-.75 (.03)	—	-.80 (.04)
R ²	.69	.86	.69	.82
p-value for Restricted Model in Log-Log Specification ^c	.09	.08	.71	.70

^a The sample consists of 751 observations covering the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1973-89; for the remaining states it covers the period 1977-89. All specifications include year effects. The instrumental variable in Panel A is the average of the minimum wage level in bordering states, each multiplied by federal minimum wage coverage for the state and divided by the average wage in the state. For Hawaii and Alaska the state's value of the minimum wage level is substituted for the average of the bordering states.

^b Evaluated at sample means.

^c This is the p-value from a likelihood ratio test of the log-log specification of the model. In the unrestricted model the coverage-adjusted minimum wage level and the mean wage are entered separately. In the restricted model the coefficient of the coverage-adjusted minimum wage level is restricted to be the same in absolute value as the coefficient of the mean wage, but with the opposite sign.

Table 2 indeed reflect the negative influence of minimum wages on employment. As a suggestive exercise, in panel B of Table 3 we report results from specifications in which we separate the numerator and denominator of the minimum wage variable. The estimates reveal a negative effect of the coverage-adjusted minimum wage level on employment, for the three fixed year and state effects specifications in which there was a negative effect in

Table 2. In contrast, the average wage has a positive effect in the same three specifications. The positive effect of the average wage does not necessarily reflect labor demand shifts, since an increase in average wages should otherwise reduce the disemployment effects of minimum wages. But assuming that whatever bias exists in the coefficient of the mean wage is not transmitted to the coefficient of the coverage-adjusted minimum wage level,

these results suggest a true negative effect of minimum wages.

Reconciling Conflicting Evidence in Recent Research on Minimum Wage Effects

Our finding of significant negative effects of minimum wages on the employment of teenagers and young adults contrasts with conclusions drawn by Card in his study of the rise in California's minimum wage between 1987 and 1989 and in his study of the increase in the federal minimum wage in 1990 (papers included in this issue). Specifically, Card reports a positive contemporaneous correlation between changes (that is, short first differences) in the minimum wage and changes in the employment of teenagers.¹⁵ To determine whether Card's results hold more generally, we examine first-difference estimates of our four specifications. We use pairwise first differences, forming differences over a year and including the data only for the odd-numbered years, to avoid the serial correlation in the errors that would be induced by using the data for all years.¹⁶ The results are presented in panel A of Table 4.

The estimated minimum wage effects are strikingly different from the fixed-state-effects estimates reported in Table 2, and are similar to the results reported by Card. In particular, in three of the four specifications there is a positive (although statistically insignificant) effect of minimum wages on the employment-to-population ratio, and there is clearly no

statistically significant evidence of negative effects of minimum wages on employment. Thus, a principal source of the differences between Card's results and those reported here is the method of estimation of equation (2)—specifically, its estimation using the within-group (least squares dummy variable) estimator, as in Table 2, or instead using a short first-difference estimator, as in Card's papers and in panel A of Table 4. Because both estimators of equation (2) should be consistent, the differences in the results suggest that the equation is misspecified. In this section, we consider two sources of model misspecification that could underlie these differences.

Model misspecification: the fixed-effects assumption. The estimates in Table 2 suggest that omitting fixed state effects leads to overstated disemployment effects of minimum wages; that is, the coefficients of the minimum wage variable are biased downward. One possible source of misspecification in equation (2) is an omitted variable related to state economic conditions that is not completely fixed, but rather is serially correlated. If the omitted state effect is positively serially correlated, then the short first-difference estimator should net out the state effect more completely than the within-group estimator, which uses longer differences, on average. In this case, the within-group estimator would still be prone to downward bias from the omitted state effect; in contrast, the short first-difference estimator would yield more nearly consistent estimates of the minimum wage effects.

To explore this possibility, we conduct a specification test for the fixed-effects assumption (Heckman and Hotz 1989). The test entails computing a first-difference estimate, including in the specification the level of the dependent variable from a period prior to that over which the first difference is computed. If the earlier level of the dependent variable enters the first-differenced equation with a statistically significant coefficient, an omitted state effect likely remains in the differ-

¹⁵ The samples used in Card's papers differ from ours: one focuses on a comparison of California's labor market with a few other labor markets over the period 1987–89; the other looks at the 50 states and Washington, D.C., as does our paper, but only over the period 1989–90.

¹⁶ Correcting for serial correlation in fixed-effects models is complicated because of bias in the standard quasi-difference estimator of the correlation parameter (see, for example, Kiefer 1980). The results that follow are unchanged if even-numbered years are used. Odd-numbered years were chosen in order to utilize the data for 1989, when the maximum number of states had minimum wages exceeding the federal level.

enced data, and the fixed-effects assumption becomes questionable.¹⁷

To examine this question, we computed first-difference estimates of equation (2) over an eight-year interval, for all of the years in our data set for which this computation is possible. This is the length of the longest difference used in the within-group estimator (although in the within-group estimator, these differences are defined relative to the state-specific mean); if the fixed-effects assumption fails, it should be most apparent in the long difference. Panel B of Table 4 reports these estimates. Consistent with the possibility of a downward bias associated with time-varying state effects, the estimated minimum wage coefficients in the long first difference are strongly negative.

Results from the specification tests, however, reported in panel C of the table, indicate that the long-difference specification does not violate the fixed-effects assumption particularly strongly. The estimated coefficient from the employment-to-population ratio lagged nine years, when it is added to the eight-year first-difference specification, is statistically significant in two of the four specifications. But evidence against the fixed-effects assumption is stronger for the short first difference. When the employment-to-population ratio lagged two years is added to the one-year first-difference specification for which estimates were reported in Panel A of Table 4, the coefficient of the lagged dependent variable is statistically significant and negative in three of the

four specifications.¹⁸ Based on this evidence, we conclude that an inappropriate fixed-effects assumption in the long-difference or within-group estimates does not underlie the differences between these estimates and the short first-difference estimates.

Model misspecification: dynamic effects. An alternative explanation of the differences between the within-group and short first-difference estimates is that the basic model is misspecified because it ignores lags in the effects of minimum wage changes.¹⁹ If equation (2) is correctly specified, then both the within-group and short (or long) first-difference estimators are consistent. If the equation is misspecified by omitting lags, however, then the omitted variable bias is likely far more severe in the short first-difference estimator.

Suppose that the true model is not equation (2), but rather

$$(3) E_{it} = \beta MW_{it} - \gamma MW_{it-1} + S_i \delta + \epsilon_{it},$$

where $\gamma > 0$, and other regressors from equation (2) have been suppressed for notational convenience. Suppose that the estimated model is

$$(4) E_{it} = \beta' MW_{it} + S_i \delta' + \epsilon_{it}.$$

That is, the lagged minimum wage variable is omitted. What is the effect of the omitted variable bias on the estimate of

¹⁷ Suppose the fixed state effect, S , is AR1,

$$S_{it} = \rho S_{it-1} + \epsilon_{it},$$

where ϵ is i.i.d., and uncorrelated with S and the other regressors, and ρ is less than one in absolute value. Then in a first difference computed over τ years, the state effect that remains is $(\rho^\tau - 1)S_{it-\tau}$. For $\rho > 0$, this state effect is likely to be closer to zero the shorter is τ , and to become a larger negative number as τ grows. Thus, in this example, the short first-difference estimator will be less prone to bias from the omitted state effect. Since $S_{it-\tau}$ is positively correlated with $E_{it-\tau-1}$, including this latter variable will detect failure of the fixed-effects assumption. Finally, we might expect the coefficient on $E_{it-\tau-1}$ to be negative, since $S_{it-\tau}$ enters the differenced equation with a negative coefficient.

¹⁸ We also computed the specification test for four- and six-year first differences, to see if there was anything idiosyncratic about the eight-year first difference. For the six-year first difference, the fixed-effects assumption is rejected for two specifications, and for the four-year first difference, it is rejected only once.

¹⁹ Brown et al. (1982) discuss the arguments for and against the likely existence of significant lags in minimum wage effects. Lagged effects may arise for the standard reasons, either because of hiring and training costs, or because of an inability to adjust other inputs quickly. But strong lags in minimum wage effects are sometimes considered less likely because of high turnover among low-wage workers, and because minimum wage changes are typically enacted some time before they actually take effect. Card, in "Using Regional Variation in Wages . . ." (this issue), reports empirical evidence consistent with a lag between legislated increases in minimum wages and upward wage adjustments by employers.

Table 4. First-Difference Estimates of Minimum Wage Effects on the Employment-to-Population Ratio, and First-Difference Specification Tests.^a
(Standard Errors in Parentheses)

Independent Variable	Teenagers (16-19)		Young Adults (16-24)	
	(1)	(2)	(3)	(4)
A. Short (One-Year) First Difference				
Minimum Wage	.25 (.17)	.07 (.12)	.08 (.12)	-.02 (.10)
Proportion of Population in Age Group	.45 (.34)	-.20 (.26)	.12 (.15)	-.11 (.13)
Prime-Age Male Unemployment Rate	-.17 (.17)	-.15 (.12)	-.15 (.12)	-.20 (.10)
Proportion of Age Group in School	—	-.77 (.05)	—	-.73 (.06)
R ²	.01	.46	.01	.28
Elasticity ^b	.20	.06	.05	-.01
B. Long (Eight-Year) First Difference				
Minimum Wage	-.01 (.14)	-.31 (.09)	-.20 (.10)	-.23 (.08)
Proportion of Population in Age Group	-.05 (.34)	.31 (.22)	.51 (.14)	.13 (.10)
Prime-Age Male Unemployment Rate	-.78 (.15)	-.45 (.10)	-.57 (.11)	-.50 (.08)
Proportion of Age Group in School	—	-.81 (.04)	—	-.88 (.06)
R ²	.11	.62	.17	.56
Elasticity ^b	-.01	-.24	-.12	-.14
C. Specification Tests				
Employment-to-Population Ratio:				
Nine-Year Lag, in Panel B Specification	-.06 (.03)	-.04 (.02)	.02 (.02)	.01 (.02)
Two-Year Lag, in Panel A Specification	-.05 (.03)	-.04 (.02)	-.05 (.02)	-.04 (.02)

^a In Panel A, the sample consists of 350 observations covering the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974-89; for the remaining states it covers the period 1978-89. Only odd-numbered years were included, to avoid serial correlation introduced by first differencing.

In Panel B, the sample consists of 292 observations. For the 22 early CPS states, the sample covers the years 1982-89; for the remaining states it covers the period 1986-89. Because there were no overlapping differences, using all observations in these periods does not induce serial correlation in the errors. All specifications include year effects.

^b Evaluated at sample means.

β' ? In the within-group estimation, we have

$$(5) \quad E(b') = \beta - \gamma\beta_x^{wg},$$

where b' is the OLS estimate of β' , and β_x^{wg} is the coefficient from the auxiliary regression

$$(6) \quad MW_{it-1} = \beta_x^{wg} MW_{it} + S_i \delta + \eta_{it}.$$

In the first-difference estimation, we have

$$(7) \quad E(b') = \beta - \gamma\beta_x^{FD},$$

where β_x^{FD} is the coefficient from the auxiliary regression

$$(8) \quad \Delta MW_{it-1} = \beta_x^{FD} \Delta MW_{it} + \Delta \eta_{it},$$

which is just the first-difference form of equation (6).

Equation (6) is not a structural equation, since it seems likely that MW_{it} is correlated

with the residual (assuming that there is some serial correlation in MW_{it}). All we are interested in, however, is comparing the relative magnitudes of the additional bias from the alternative methods of treating the fixed state effects, so we can interpret η_{it} in equation (6) as the part of the residual that is uncorrelated with MW_{it} . The only reason for the bias to differ between the fixed-effects and first-difference estimators is that β_x^{WG} and β_x^{FD} differ. Again, though, we are simply comparing the coefficients from a within-group and first-difference estimator of the same regression, so why should these coefficients differ?

The reason is that in the first-difference estimator, the construction of the differences induces a strong negative correlation between the omitted lagged minimum wage variable and the included contemporaneous minimum wage variable. To develop this argument explicitly, note that equations (6) and (8) suffer from the same problem as the standard dynamic model with fixed effects (Kiefer 1980), namely, the problem that estimation induces a correlation between the residuals and the regressors. This is easiest to see in equation (8). The right-hand-side variable in equation (8) is $(MW_{it} - MW_{it-1})$, and the residual is $(\eta_{it} - \eta_{it-1})$. But η_{it} and MW_{it-1} are obviously correlated; the covariance between the regressor and the residual is $-\text{Cov}(MW_{it-1}, \eta_{it}) = -\text{Var}(\eta_{it})$. If this negative bias is large enough to bring about a negative β_x^{FD} in equation (8), then equation (7) implies that the first-difference estimator of β' (the coefficient on MW_{it}) in equation (4) is upward biased.

When the within-group estimator is used, however, the bias is less by a factor of T (the number of time periods). The within-group estimation is carried out by subtracting from each variable its state-specific mean and using OLS on the transformed data. Thus, β_x^{WG} in equation (6) is estimated from the regression

$$(9) \quad (MW_{it-1} - T^{-1} \sum MW_{it-1}) \\ = \beta_x^{WG} (MW_{it} - T^{-1} \sum MW_{it}) \\ + (\eta_{it} - T^{-1} \sum \eta_{it}).$$

There is still a correlation between the error and the regressor, since η_{it} and MW_{it-1} still appear in them, but the covariance between the error and regressor is now $-[(T+1)/T^2]\text{Var}(\eta_{it})$,²⁰ which converges to $-T^{-1}\text{Var}(\eta_{it})$ as T gets large.²¹ Thus, the within-group estimator of the coefficient β' in equation (4), given that equation (3) is the true model, is less biased by a factor of T than the first-difference estimator. If this bias is small enough so that the estimate of β_x^{WG} in equation (6) remains positive, then equation (5) implies that the within-group estimator of β' in equation (4) is downward biased, in contrast to the first-difference estimator.

Thus, the existence of lagged negative minimum wage effects could in principle account for the difference between the within-group and first-difference estimates. To examine this possibility, in Table 5 we report estimates of dynamic specifications of minimum wage effects using both within-group and first-difference estimators. The specifications for which coefficient estimates are reported include the contemporaneous minimum wage variable as well as the variable lagged once. The estimates of the minimum wage coefficients reveal evidence of significant lags in these effects. In all eight columns, the coefficient on the lagged minimum wage variable is more negative than that on the contemporaneous minimum wage variable, and in the young-adult specifications the lagged coefficient is statistically significant. In addition, the long-run elasticities from the fixed-effects and first-difference estimates are considerably closer. The marked changes in the first-difference estimates of the employment elasticities, coupled with

²⁰ This expression is true for all but the first or last observations, for which the covariance is:

$$-[1/T^2]\text{Var}(\eta_{it}).$$

²¹ This follows because for any t ,

$$\begin{aligned} \text{Cov}(MW_{it} - T^{-1} \sum MW_{it}, \eta_{it} - T^{-1} \sum \eta_{it}) \\ = -T^{-1} \text{Cov}(MW_{it}, \eta_{it+1}) - T^{-1} \text{Cov}(MW_{it-1}, \eta_{it}) \\ + T^{-2} \sum_{i=1}^{T-1} \text{Cov}(MW_{it-1}, \eta_{it}), \\ = -[(T+1)/T^2]\text{Var}(\eta_{it}). \end{aligned}$$

Table 5. Within-Group and First-Difference Estimates of Dynamic Specifications of Minimum Wage Effects on the Employment-to-Population Ratio.^a
(Standard Errors in Parentheses)

Independent Variable	Teenagers (16-19)				Young Adults (16-24)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Minimum Wage	.10 (.11)	-.12 (.08)	.15 (.17)	.01 (.13)	-.03 (.08)	-.10 (.06)	-.01 (.13)	-.09 (.11)
Minimum Wage, Lagged One Year	-.14 (.12)	-.12 (.07)	-.30 (.18)	-.19 (.13)	-.26 (.08)	-.18 (.06)	-.26 (.13)	-.22 (.11)
Proportion of Population in Age Group	-.11 (.23)	-.13 (.16)	.46 (.34)	-.19 (.26)	.42 (.10)	.08 (.08)	.14 (.15)	-.09 (.13)
Prime-Age Male Unemployment Rate	-.53 (.11)	-.29 (.08)	-.10 (.17)	-.11 (.13)	-.52 (.09)	-.47 (.06)	-.09 (.12)	-.15 (.10)
Proportion of Age Group in School	—	-.77 (.03)	—	-.77 (.05)	—	-.81 (.04)	—	-.73 (.06)
Estimator ^b	WG	WG	FD	FD	WG	WG	FD	FD
p-values: ^c								
0-1 Lags vs. 0 Lags	.20	.12	.09	.14	.00	.00	.04	.04
R ²	.70	.86	.01	.46	.71	.83	.02	.29
Elasticity ^d	-.03 (.10)	-.19 (.07)	-.12 (.24)	-.15 (.17)	-.18 (.06)	-.17 (.04)	-.17 (.13)	-.19 (.11)
<i>Elasticities from Alternative Specifications:</i>								
0 Lags	.04	-.14	.20	.06	-.08	-.11	.05	-.01
1 Lag Only	-.08	-.14	-.28	-.16	-.17	-.14	-.16	-.12
0-1 Lags of All Variables in Equation	.01	-.17	-.06	-.17	-.15	-.15	-.15	-.22

^a The sample covers the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974-89 for the fixed-effects estimates, and 1975-89 for the first-difference estimates; for the remaining states the corresponding periods are 1978-89 and 1979-89. For the first-difference estimates, the sample includes only odd-numbered years, to avoid serial correlation introduced by first differencing.

^b All specifications include year effects.

^c Likelihood-ratio test.

^d Long-run, evaluated at sample means. Standard errors treat coefficient estimates, but not means, as random.

evidence of significant lagged minimum wage effects, are consistent with greater upward bias in the first-difference estimates than in the within-group estimates in specifications omitting the lags.²²

In addition, we computed the auxiliary regressions (6) and (8), and verified that downward bias dominates the first-difference estimates (equation (8)), but not the within-group estimates (equation (6)). For the four alternative specifications the

coefficients for equation (8) were between -0.26 and -0.27. In contrast, the coefficients were between 0.42 and 0.43 for equation (6). These coefficients are consistent with an upward bias in the first-difference estimates of minimum wage effects in specifications omitting a lagged minimum wage variable and a downward bias in the within-group estimates of minimum wage effects in similar specifications; a comparison of the estimates in Tables 2 and 4 with those in Table 5 reveals exactly these biases.²³ The conclu-

²² In results reported in Table 6, the same qualitative conclusions were drawn from specifications including two lags of the minimum wage variable.

²³ For example, in Table 2, column (1), panel A,

sion that the omission of lagged minimum wage effects biases the short first-difference estimates upward is buttressed by the finding that the long (eight-year) first-difference estimates in Table 4 indicate strong disemployment effects of minimum wages. These long-difference estimates are not prone to the same bias from omitting lagged minimum wage effects, since the omitted variable ($MW_{it-1} - MW_{it-9}$) is not necessarily negatively correlated with the included variable ($MW_{it} - MW_{it-8}$).²⁴

The table also reports results of specification tests and robustness checks for these dynamic specifications. The p-values from likelihood-ratio tests indicate that the data reject the specification with the contemporaneous minimum wage variable alone for all of the young-adult models. For teenagers, the p-values range from 0.09 to 0.20, a result that does not indicate rejection. But in results not reported in the table, we compared the likelihoods for the non-nested models including only the contemporaneous minimum wage variable, or, alternatively, only the lagged variable. The likelihood was higher for the model with lags in the three cases in which the contemporaneous specification indicated a positive elasticity (the fixed-effects specification excluding school enrollment, and the two first-difference specifications). At the bottom of the table we report elasticities for the specification including only the contemporaneous minimum wage variable and, alternatively, only the lagged minimum wage variable.²⁵

the coefficient of the contemporaneous minimum wage variable is 0.07, which rises to 0.10 in Table 5, column (1). In contrast, in Table 4, column (1), this coefficient is 0.25, which falls to 0.15 in Table 5, column (3). The same pattern holds for all of the specifications.

²⁴ As a further test, we also computed elasticities for first differences of other lengths, using the same sample used for the eight-year long difference reported in Table 4. The elasticities were negative for six- and four-year first differences (except for the specification for teenagers excluding the enrollment rate), and were positive for two- and one-year first differences.

²⁵ The elasticities from the contemporaneous specification differ slightly from those reported in

These elasticities tell much the same story: once lagged effects are allowed, negative elasticities result for all specifications. In the last row, we report the elasticities including one lag of each of the control variables, to examine whether the lagged minimum wage variable is simply picking up lagged effects of these other variables. The results are largely unchanged.

Finally, the estimates of the elasticities are larger (more negative) than in any of the previous fixed-effects (or first-difference) estimates. The elasticities are bunched in a range from -0.15 to -0.2; the single exception is the specification for teenagers excluding the school enrollment rate. These estimates are close to the midrange of the consensus of past time-series studies.

Robustness checks. In Table 6 we report the elasticities from within-group estimates of the dynamic specification of the model (with one lag of the minimum wage variable), exploring the robustness of the results to variations in data construction, model specification, and sample definition. In panel A we report estimates using no coverage adjustment; the minimum wage variable is simply the minimum wage level divided by the mean wage in the state. The estimated elasticities are generally slightly smaller in absolute value without the coverage adjustment, compared to the elasticities in Table 5 (columns (1), (2), (5), and (6)), probably because a one-unit increase in the coverage-adjusted minimum wage variable corresponds to a larger increase in the minimum wage level than does a one-unit increase in the unadjusted minimum wage variable.

In panel B, we attempt to adjust for coverage by using state minimum wage laws along with the FLSA provisions.²⁶ In

Table 2 because here they are computed from the same sample for which the lagged model can be estimated.

²⁶ We used the 1974 estimates from the Department of Labor for 1973, and interpolated the 1975 and 1977 estimates for 1976. Estimates beyond 1977 were extrapolated as constant percentages equal to the value in 1977 for subsequent years. Prior to 1976, most state and local government employees were covered by the FLSA as a result of the 1966 and 1974

this case, evidence of negative employment elasticities is weaker. As discussed above, however, we have very little hard evidence on coverage by state minimum wage laws, and therefore discount these estimates.

In panel C, we substitute the federal minimum wage level for the state level, to examine the extent to which our use of state minimum wage levels, in contrast to the federal minimum wage level used in time-series studies, influences the results. The estimated elasticities are slightly larger than those using the state minimum wage levels. In panel D we re-estimate the same models using state population estimates from the CPS to weight the observations.²⁷ Again, there is relatively little change in the estimates, although there is some widening of the range of elasticities.

amendments to the Act. In 1976, however, the Supreme Court ruled that the minimum wage and maximum hours provisions of the FLSA could not be applied to state and local government activities that are an integral part of traditional government functions, a ruling that was reversed in 1985. As a result, federal coverage was depressed by the absence of state and local workers from 1977 to 1984, but picked up these workers again in 1985. Consistent with the treatment of state and local government workers in the federal coverage estimates, for states that explicitly covered these workers we assumed that state coverage was equal to its 1977 value from 1978 through 1984, and its 1976 value (interpolated from 1975 and 1977 Department of Labor estimates) from 1985 through 1989. For other states, we simply used the 1977 value throughout the sample period. Colorado and Kansas did not enact a state minimum wage until 1978, after the last available estimate of state coverage; we use state coverage estimates of zero for these states. In cases for which the sum of the federal coverage estimate and the state coverage estimate exceeded unity, we adjusted the state coverage estimate downward to constrain the two to sum to unity.

Given our coverage estimates, the numerator of the relative minimum wage variable is defined as follows: for observations with minimum wage levels greater than or equal to the federal level, the state level multiplied by the sum of federal and state coverage; for observations with a minimum wage below the federal level, the federal minimum wage multiplied by federal coverage, plus the state minimum wage multiplied by state coverage.

²⁷ One reason not to weight the estimates throughout the paper is that a larger state does not necessarily provide more accurate estimates for a single labor market, because a larger state is more likely to encompass many labor markets.

In panels E and F we report the elasticities that result from including a second lag of the minimum wage variable, and then also introducing one and two lags of each of the control variables. Again, the estimates are little changed.

Finally, in panels G-J we report estimates first splitting the sample into separate time periods, and then splitting the sample between states for which the minimum wage never exceeded the federal level and states that had higher state minimums at some point in the sample period. The estimates reveal stronger elasticities in the latter half of the sample period, and for the states with higher state minimums; these results are mutually consistent, because it was in the latter part of the sample period that relatively more states raised their minimum wages above the federal level.

Evidence on Student and Youth Subminimum Wage Provisions

The final issue we explore in this paper is the potential for youth or student subminimum wage provisions to reduce the adverse disemployment effects of minimum wages. Katz and Krueger (this issue) attempt to assess the likely impact of the new federal subminimum through a small survey of employers in the period immediately following the implementation of the new federal legislation, studying in particular the extent of usage of the federal subminimum in the Texas fast food industry. Because many states have had such subminimums in the past, however, our state-level data provide a complementary means of estimating the impact of these subminimums.

The empirical question is whether—controlling for the state minimum wage level as well as state labor market condition—states with subminimum wages exhibit higher employment rates. The simplest approach to this question is to augment equation (2) to be

$$(10) \quad E_{it} = \alpha_0 + \alpha_1 MW_{it} + \alpha_2 SUB_{it} \cdot (MW_{it} - SMW_{it}) + X_{it}\beta + S_t\delta + \epsilon_{it}$$

where SUB is a dummy variable for the

Table 6. Sensitivity of Within-Group Estimates of Dynamic Specifications of Minimum Wage Effects on the Employment-to-Population Ratio: Long-run Elasticities Evaluated at Sample Means.^a
(Standard Errors in Parentheses)

Change in Specification	Teenagers (16-19)		Young Adults (16-24)	
	Excluding Enrollment (1)	Including Enrollment (2)	Excluding Enrollment (3)	Including Enrollment (4)
A. No Coverage Adjustment (N = 700)	-.06 (.11)	-.13 (.08)	-.09 (.06)	-.11 (.05)
B. Federal and State Coverage Adjustment (N = 700)	.11 (.09)	-.02 (.06)	-.07 (.05)	-.09 (.04)
C. Federal Minimum Wage Level in Place of State Minimum Wage Level (N = 700)	-.07 (.10)	-.20 (.07)	-.20 (.06)	-.17 (.04)
D. Weighting by State Population (N = 700) ^b	.06 (.11)	-.18 (.07)	-.07 (.06)	-.12 (.04)
E. 0-2 Lags of Minimum Wage Variable (N = 649)	.02 (.12)	-.21 (.08)	-.22 (.07)	-.21 (.05)
F. 0-2 Lags of All Variables (N = 649)	.06 (.12)	-.21 (.08)	-.18 (.06)	-.19 (.05)
G. Sample Restricted to 1974-81 (N = 321)	-.18 (.15)	-.10 (.10)	-.20 (.09)	-.09 (.07)
H. Sample Restricted to 1982-89 (N = 408)	-.03 (.15)	-.11 (.10)	-.15 (.08)	-.17 (.06)
I. Sample Restricted to States with Minimum Wage Level Equal to Federal Minimum Wage in All Years (N = 488)	-.01 (.14)	-.16 (.10)	-.09 (.08)	-.11 (.06)
J. Sample Restricted to States with Minimum Wage Level Above Federal Minimum Wage in at Least One Year (N = 212)	-.01 (.15)	-.27 (.09)	-.18 (.08)	-.22 (.06)

^a The sample covers the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974-89, and for the remaining states it covers the period 1978-89, for rows A-D, and for rows G-J, with the restrictions noted. For rows E and F, 1974 (1978) is dropped. All specifications include state and year effects, and the contemporaneous and lagged minimum wage variable, unless otherwise noted. Standard errors treat coefficient estimates, but not means, as random.

^b Elasticities are evaluated at weighted mean.

existence of either a youth or student subminimum wage, and SMW_{it} is the subminimum wage level (we call the interactive variable the "subminimum wage gap"). Estimates of α_2 greater than zero would support the hypothesis that youth or student subminimums reduce the adverse effects of minimum wages on employment rates for young workers.

We construct the estimate of SMW_{it} in two steps. First, for workers covered by a state minimum wage law, but not the FLSA, a subminimum reduces the wage

paid from the level of the state minimum for all workers down to the minimum allowable wage for youths or students. The limited information we have on student and youth subminimum wage provisions suggests that, on average, these provisions permit wage payments equal to about 75% of the minimum wage for other workers. Consequently, for each observation we construct a variable equal to 25% of the state minimum wage level, multiplied by the effective state coverage rate (that is, excluding workers covered by federal law), and divided by the mean

wage in the state.²⁸ Second, in states with a minimum wage level above the federal level, a subminimum wage provision would reduce the wage paid to workers covered by the FLSA from the state minimum wage level to the greater of the federal level or 75% of the state level. For these states we add a second term that is the smaller of 25% of the state level and the difference between the state and federal levels, all multiplied by federal coverage and divided by the mean wage in the state.

Results for teenagers, to whom these subminimum wage provisions are most likely to apply, are reported in Table 7. We report results separately for student/apprentice subminimums, for youth subminimums, and for the existence of any form of subminimum. Consistent with the previous estimates of the minimum wage equation, we report results using contemporaneous and lagged values of the minimum wage and subminimum wage variables. Because this may be asking too much of the data, however, we also report results for more parsimonious specifications that include only lagged values of each variable.²⁹

None of the specifications provide statistically significant evidence that student subminimum wage provisions moderate the disemployment effects of minimum wages (columns (1)–(4)). When information on youth subminimums is incorporated into the specifications (columns (5)–(8)), however, there is statistically significant evidence that state youth subminimum wage provisions moderate the disemployment effects for teenagers.³⁰ In

the specifications with the contemporaneous and lagged minimum wage and subminimum wage variables, the positive effects of subminimum wages are significant at or near the 10% level, and in the parsimonious specifications, the effects are significant at the 5% level in three of four cases.³¹

To interpret the coefficients, consider as an example a state with its minimum wage level set above the federal level in 1989 (say, at \$4.00 per hour), with full federal coverage, but without a subminimum wage provision. In this case, the full disemployment effect from the state minimum wage is $(\alpha_1 \times 4.00)$; using the coefficients in column (8), the employment rate for teenagers is 0.84 percentage point below what it would be in the absence of a minimum wage. If the state enacts a youth subminimum wage provision that lowers the youth minimum to the federal level (\$3.35 in 1989), the disemployment effect will be reduced to $(\alpha_1 \times 4.00) + (\alpha_2 \times 0.65)$, or 0.55 percentage point. Thus, in this example, the introduc-

for student subminimums could be that these subminimum wage provisions vary widely across states in terms of the students to whom they apply, with distinctions based on full-time or part-time student status, time of the year (whether school is in session), and so on. In contrast, youth subminimum wage provisions are more uniform. In addition, student subminimums may apply to some young adults, reducing the ability to substitute teenagers exempted from the minimum wage, or might lead to substitution between students and nonstudents within the teenage group, which would be masked in our employment rates for all teenagers.

³¹ In contrast to our earlier analyses, in the construction of the subminimum wage gap we utilized information on state minimum wage coverage. To ignore state coverage would entail treating states with minimum wage levels at or below the federal minimum wage level identically, whether or not they had subminimum wage provisions. That is, the subminimum wage effect would be identified solely from the high minimum wage states. This model misspecification seems worth avoiding, despite the measurement error in state coverage. For purposes of comparison, we recomputed the specifications in Table 7 using only federal coverage. This recomputation resulted in insignificant coefficients for the subminimum wage variables; p-values for the significance of the youth or "any" subminimum wage variables were concentrated in the range from 0.2 to 0.4.

²⁸ There were two cases in which states with minimum wage levels below the federal level had a subminimum wage provision on the books, but our estimate of state coverage was zero. In these cases we treated the observation as if there was no subminimum.

²⁹ For the youth subminimum or "any" subminimum specifications, which reveal some statistical evidence of subminimum wage effects, the restriction dropping the contemporaneous variables is never rejected at the 10% significance level, whereas the restriction dropping the lagged variables (instead) is rejected in two out of four cases.

³⁰ One reason for the absence of significant effects

Table 7. Within-Group Estimates of Effects of Subminimum Wage Provisions on the Employment-to-Population Ratio of Teenagers.^a
(Standard Errors in Parentheses)

Independent Variable	Student/Apprentice Subminimums				Youth Subminimums				Any Subminimums			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Minimum Wage	.09 (.12)	-.12 (.08)	—	—	.05 (.12)	-.11 (.08)	—	—	.08 (.12)	-.12 (.08)	—	—
Minimum Wage, Lagged One Year	-.13 (.12)	-.12 (.08)	-.09 (.10)	-.17 (.07)	-.15 (.12)	-.16 (.08)	-.14 (.11)	-.21 (.07)	-.17 (.12)	-.15 (.08)	-.14 (.11)	-.20 (.07)
Subminimum × Subminimum Wage Gap ^b	.04 (.30)	.02 (.20)	—	—	.39 (.32)	-.00 (.21)	—	—	.24 (.30)	.06 (.20)	—	—
Subminimum × Subminimum Wage Gap, Lagged One Year	-.15 (.37)	.06 (.25)	-.16 (.30)	.12 (.20)	.27 (.37)	.43 (.25)	.48 (.32)	.44 (.21)	.44 (.35)	.39 (.23)	.56 (.29)	.44 (.20)
Proportion of Population in Age Group	-.11 (.24)	-.12 (.16)	-.10 (.23)	-.14 (.16)	-.10 (.23)	-.13 (.16)	-.09 (.23)	-.14 (.16)	-.09 (.23)	-.11 (.16)	-.08 (.23)	-.13 (.16)
Prime-Age Male Unemployment Rate	-.53 (.11)	-.29 (.08)	-.53 (.11)	-.30 (.08)	-.54 (.11)	-.31 (.08)	-.55 (.11)	-.31 (.08)	-.54 (.11)	-.30 (.08)	-.54 (.11)	-.31 (.08)
Proportion of Age Group in School	—	-.77 (.03)	—	-.77 (.03)	—	-.77 (.03)	—	-.77 (.03)	—	-.77 (.03)	—	-.76 (.03)
Significance of Subminimum Wage Variables ^c	.91	.92	.59	.53	.11	.11	.11	.03	.07	.07	.04	.02
R ²	.70	.86	.70	.86	.70	.86	.70	.86	.70	.86	.70	.86

^a The sample covers the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974–89; for the remaining states it covers the period 1978–89. The minimum wage variable is the greater of the state or federal minimum wage level, multiplied by federal minimum wage coverage for the state, divided by the average wage in the state. The prime-age male unemployment rate is for men aged 25–64. All specifications include state and year effects.

^b The minimum wage gap is defined as follows: for all observations, it includes 25% of the state minimum wage level, multiplied by state coverage (i.e., the proportion of workers covered by the state minimum wage, but not the FLSA), divided by the mean wage; for observations with minimum wage levels greater than the federal level, this is added to the smaller of either 25% of the state minimum wage level or the difference between the state and federal minimum wage level, multiplied by federal coverage, divided by the mean wage.

^c p-value for likelihood-ratio test.

tion of the subminimum provision reduces the disemployment effect of the minimum wage by about 35%.

Given the evidence against the use of subminimum wage provisions in one labor market reported by Katz and Krueger (this issue), we considered more carefully the possibility that the results reported in Table 7 represent a spurious correlation. For example, subminimum wage provisions could simply coincide with relatively high employment in particular states, perhaps because states with subminimum wage provisions also have relatively lax enforcement of state minimum wage laws. To examine this question, in Table 8 we report estimates of the same specifications as in Table 7, but only for individuals aged 20-24. Because youth subminimums typically apply to individuals aged 18 or less, if the youth subminimums reduce the ad-

verse effects of the minimum wage on the employment of teenagers, they should have little effect on, and perhaps magnify, the employment losses of those aged 20-24. On the other hand, if the findings reflect a spurious correlation between youth subminimums and employment rates, we might expect to find a similar positive association for these older youths.

In Table 8, the point estimates of the effects of subminimum wages on the employment rates of 20-24-year-olds are generally negative and, at the least, do not replicate the patterns found for teenagers. This result suggests that youth subminimums do moderate the disemployment effects of minimum wages on teenagers. On the other hand, the results for young adults suggest partial substitution away from young adults toward teenagers, which would imply that the moderation of

Table 8. Within-Group Estimates of Effects of Subminimum Wage Provisions on the Employment-to-Population Ratio of 20-24-Year-Olds.^a
(Standard Errors in Parentheses)

Independent Variable	Youth Subminimums			
	(1)	(2)	(3)	(4)
Minimum Wage	-.04 (.10)	-.07 (.09)	—	—
Minimum Wage, Lagged One Year	-.22 (.11)	-.18 (.09)	-.24 (.09)	-.21 (.08)
Youth Subminimum $\times$ Subminimum Wage Gap ^b	-.02 (.27)	-.07 (.24)	—	—
Youth Subminimum $\times$ Subminimum Wage Gap, Lagged One Year	.08 (.31)	-.20 (.27)	.07 (.27)	-.23 (.24)
Proportion of Population in Age Group	.51 (.14)	.35 (.12)	.50 (.14)	.34 (.12)
Prime-Age Male Unemployment Rate	-.59 (.09)	-.62 (.08)	-.59 (.10)	-.62 (.08)
Proportion of Age Group in School	—	-.77 (.05)	—	-.77 (.05)
Joint Significance of Subminimum Wage Variables ^c	.97	.55	.79	.32
R ²	.56	.67	.56	.67

^a The sample covers the 50 states and Washington, D.C. For the 22 states identified in the CPS as early as 1973, the sample covers the years 1974-89; for the remaining states it covers the period 1978-89. The minimum wage variable is the greater of the state or federal minimum wage level, multiplied by federal minimum wage coverage for the state, divided by the average wage in the state. The prime-age male unemployment rate is for men aged 25-64. All specifications include state and year effects.

^b The minimum wage gap is defined as follows: for all observations, it includes 25% of the state minimum wage level, multiplied by state coverage (i.e., the proportion of workers covered by the state minimum wage, but not the FLSA), divided by the mean wage; for observations with minimum wage levels greater than the federal level, this is added to the smaller of either 25% of the state minimum wage level or the difference between the state and federal minimum wage level, multiplied by federal coverage, divided by the mean wage.

^c p-value for likelihood-ratio test.

overall disemployment effects is less than that for teenagers alone.

As a second means of studying the validity of the subminimum wage results, we examined wage distributions for teenagers, to see if there are spikes at the subminimum wage, below the minimum wage level. We extracted information on wages from monthly CPS files for 1989, restricting attention to those states with legislated minimum wages above the federal minimum wage; in other states, only workers exempted from federal coverage would be expected to be found at the subminimum.

For the six New England states, there is no evidence of spikes at the subminimum, with the possible exception of Vermont. However, these are high-wage states. In the New England states excluding Vermont and Maine, over 80% of teenagers earn wages higher than four dollars per hour. In Alaska, Hawaii, and California, also high-wage states, there are no spikes at the subminimum.

The lowest-wage states are Pennsylvania, Minnesota, and Washington, in which fewer than 60% of teenagers earn wages exceeding four dollars. As Figure 1 shows, in all three of these states, there are spikes at the subminimum wage. The spike for Minnesota is particularly noteworthy. Minnesota's subminimum (\$3.47) exceeds the federal minimum (\$3.35), and the spike appears at \$3.47, suggesting that the spikes we find at the subminimums for some states do not simply reflect workers covered by federal but not state legislation, or some fraction of employers ignoring state, but not federal, minimum wage laws. Overall, we interpret these data on wage distributions as evidence that the subminimum wage regressions reflect true effects; for some states—in particular, those with relatively low wages—subminimum wage provisions do induce spikes at the subminimum, suggesting that these provisions have a real impact.

Conclusion

Using a specially constructed panel data set on state minimum wage laws and labor

market conditions, we have presented new evidence on the effects of minimum wage on the employment of teenagers and young adults, and assessed the extent to which youth or student subminimum wages reduce the adverse disemployment effects of minimum wages. Our reexamination of the existing evidence provides a range of estimated elasticities of employment-to-population ratios with respect to minimum wages. For teenager (aged 16–19), we find elasticities between -0.1 and -0.2 , using correctly specified models (Table 5); that is, a 10% increase in the minimum wage appears to be associated with a 1% to 2% decrease in employment of teenagers. In specifications taking account of school enrollment rates, the results suggest an elasticity closer to -0.2 than -0.1 . For young adults (aged 16–24), our best estimate of the range is from -0.15 to -0.2 . In general, our results support the consensus of negative effects suggested by the earlier time-series evidence surveyed by Brown et al. (1982), although, as in that survey, it is possible to point to some evidence to the contrary.

In contrast, the results Card reports in his two papers published in this issue appear to conflict directly with our results since they are based on similar statistical experiments for similar units of observation. In fact, our results do not differ significantly for the one specification that appears in both Card's papers and this paper: teenage employment specification that do not control for school enrollment rates. We argue, however, that this model is misspecified by the exclusion of the school enrollment rate. Moreover, we show that the failure to consider lagged effects of minimum wages, especially in the short first-difference estimators used by Card, results in substantial upward bias in the estimated effects of minimum wage on employment, leading to elasticities that are too close to zero, and frequently positive. For the other specifications we

problems
w/ Card's research

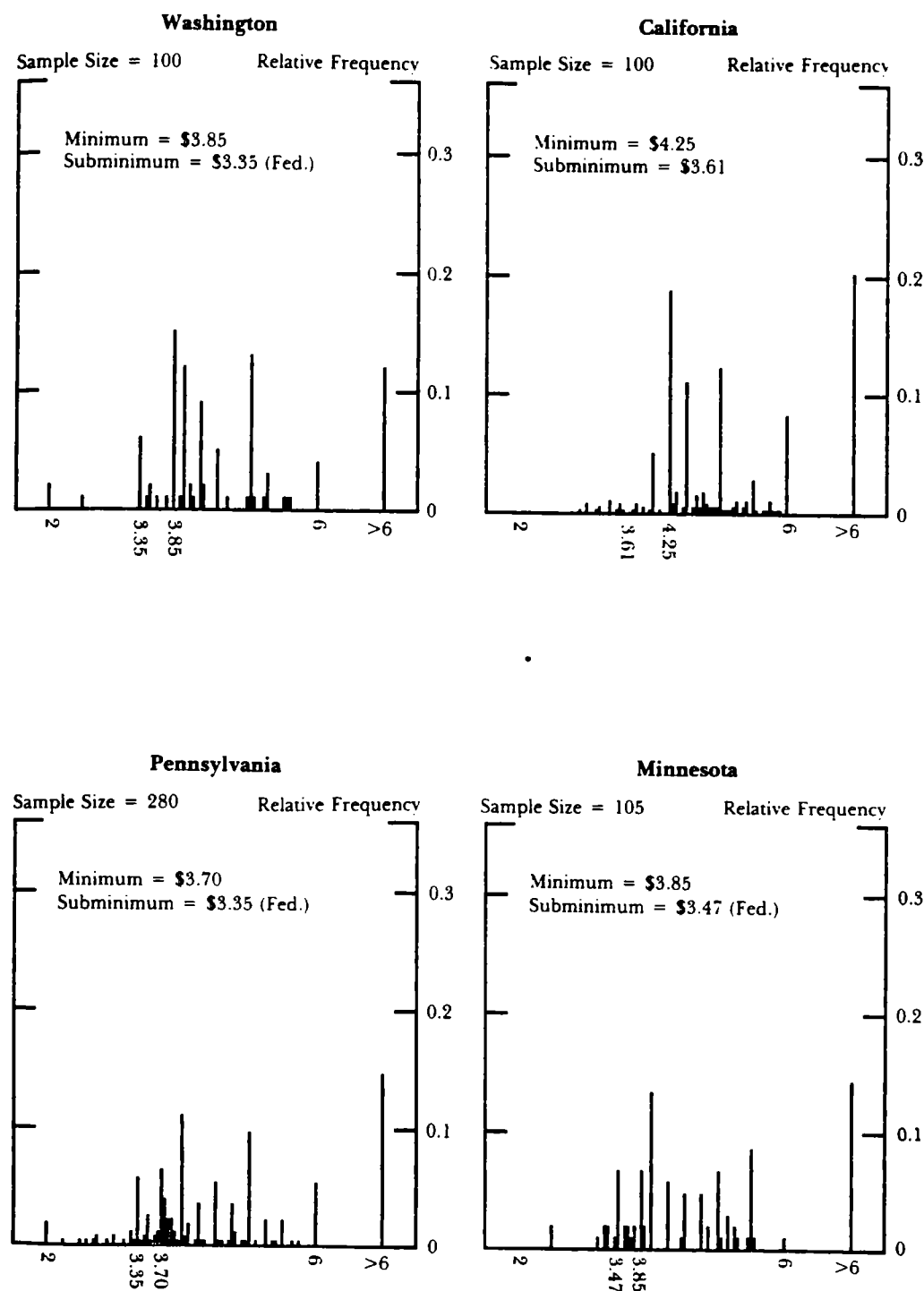


Figure 1. Wage Distributions for Teenagers (Ages 16-19) in High Minimum Wage States, 1989.

estimate (a specification that includes school enrollment rates in the equation for teenagers, and both specifications for young adults), the evidence from correctly specified models points to consistently negative effects of minimum wages on employment, with negative long-run elasticities in the ranges reported above.

The apparently anomalous result for teenagers in specifications that do not control for school enrollment rates suggests that further research that controls for school enrollment rates, while attempting to correct for their potential endogeneity, will be fruitful. In addition, it would be informative to explore the reasons for the lag between the enactment of minimum wage hikes and the employment changes that result; an interesting project for such research might exploit cross-state variation in the length of periods between

enactment of minimum wage legislation and the actual increases, paralleling time-series experiments in Brown et al. (1983).

Based on our preferred specifications, we also provide estimates of the role of subminimum wages in reducing the adverse effects of the minimum wage. Our results indicate that youth subminimums, but not student subminimums, moderate the disemployment effects of minimum wages on teenagers. We present additional evidence that, in our data, the effects of subminimum wage provisions are not spurious. This result stands in contrast to the puzzling finding, reported in Katz and Krueger (in this issue), that fast-food restaurants in Texas did not utilize the federal subminimum wage introduced in 1990. But research on subminimum wages is in its infancy, and a more definitive answer awaits further research.

REFERENCES

- Brown, Charles. 1988. "Minimum Wage Laws: Are They Overrated?" *Journal of Economic Perspectives*, Vol. 2, No. 3 (Summer), pp. 133-46.
- Brown, Charles, Curtis Gilroy, and Andrew Kohen. 1983. "Time-Series Evidence of the Effect of the Minimum Wage on Youth Employment and Unemployment." *Journal of Human Resources*, Vol. 18, No. 1 (Winter), pp. 3-31.
- . 1982. "The Effect of the Minimum Wage on Employment and Unemployment." *Journal of Economic Literature*, Vol. 20 (June), pp. 487-528.
- Cogan, John. 1981. "The Decline in Black Teenage Employment: 1950-1970." NBER Working Paper No. 683.
- Cotterill, Philip G., and Walter J. Wadycki. 1976. "Teenagers and the Minimum Wage in Retail Trade." *Journal of Human Resources*, Vol. 11, No. 1 (Winter), pp. 69-85.
- Cunningham, James. 1981. "The Impact of Minimum Wages on Youth Employment, Hours of Work, and School Attendance: Cross-sectional Evidence from the 1960 and 1970 Censuses." In Simon Rottenberg, ed., *The Economics of Legal Minimum Wages*. Washington, D.C.: American Enterprise Institute, pp. 88-123.
- Freeman, Richard B. 1982. "Economic Determinants of Geographic and Individual Variation in the Labor Market Position of Young Persons." In Richard B. Freeman and David A. Wise, eds., *The Youth Labor Market Problem: Its Nature, Causes, and Consequences*. Chicago: University of Chicago Press, pp. 115-54.
- Freeman, Richard B., Wayne Gray, and Casey E. Ichniowski. 1981. "Low Cost Student Labor: The Use and Effects of the Subminimum Wage Provisions for Full-Time Students." In Minimum Wage Study Commission, Vol. 5, pp. 305-35.
- Gramlich, Edward M. 1976. "Impact of Minimum Wages on Other Wages, Employment and Family Incomes." *Brookings Papers on Economic Activity*, Vol. 2, pp. 409-51.
- Heckman, James J., and V. Joseph Hotz. 1989. "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training." *Journal of the American Statistical Association*, Vol. 84, No. 408 (December), pp. 862-74.
- Katz, Arnold. 1973. "Teenage Employment Effects of State Minimum Wages." *Journal of Human Resources*, Vol. 8, No. 2 (Spring), pp. 250-56.
- Kiefer, Nicholas M. 1980. "Estimation of Fixed Effects Models for Time Series of Cross-Sections with Arbitrary Intertemporal Covariance." *Journal of Econometrics*, Vol. 14, No. 2 (October), pp. 195-202.
- Leighton, Linda, and Jacob Mincer. 1981. "The Effects of the Minimum Wage on Human Capital Formation." In Simon Rottenberg, ed., *The Economics of Legal Minimum Wages*. Washington, D.C.: American Enterprise Institute, pp. 155-73.
- Lester, Richard A. 1946. "Shortcomings of Marginal Analysis for Wage-Employment Problems." *American Economic Review*, Vol. 36, No. 1 (March), pp. 63-82.
- Madden, Janice F. 1991. "Changes in the Distribution of Income Within the Philadelphia Metropoli-

- itan Area." Unpublished paper, University of Pennsylvania.
- Mattila, J. Peter. 1978. "Youth Labor Markets, Enrollments, and Minimum Wages." *Proceedings of the 31st Annual Meetings of the Industrial Relations Research Association*. Madison, Wis.: Industrial Relations Research Association, pp. 134-40.
- Mincer, Jacob. 1976. "Unemployment Effects of Minimum Wages." *Journal of Political Economy*, Vol. 84, No. 4 (August, Pt. 2), pp. S87-S105.
- Minimum Wage Study Commission. 1981. *Report of the Minimum Wage Study Commission*. Washington, D.C.: GPO.
- Questor, Aline O. 1981. "State Minimum Wage Laws, 1950-1980." In *Minimum Wage Study Commission*, Vol. 2, pp. 23-152.
- Swidinsky, Robert. 1980. "Minimum Wages and Teenage Unemployment." *Canadian Journal of Economics*, Vol. 13, No. 1 (February), pp. 158-71.
- Wachter, Michael L., and Choongsoo Kim. 1982. "Time Series Changes in Youth Joblessness." In Richard B. Freeman and David A. Wise, eds., *The Youth Labor Market Problem: Its Nature, Causes, and Consequences*. Chicago: University of Chicago Press, pp. 155-85.
- Welch, Finis, and James Cunningham. 1978. "Effects of Minimum Wages on the Level and Age Composition of Youth Employment." *Review of Economics and Statistics*, Vol. 60, No. 1 (February), pp. 140-45.
- Wellington, Allison J. 1991. "Effects of the Minimum Wage on the Employment Status of Youths: An Update." *Journal of Human Resources*, Vol. 26, No. 1 (Winter), pp. 27-46.

USING REGIONAL VARIATION IN WAGES TO MEASURE THE EFFECTS OF THE FEDERAL MINIMUM WAGE

DAVID CARD*

The imposition of a national minimum wage standard provides a natural experiment in which the "treatment effect" varies across states depending on the fraction of workers initially earning less than the new minimum. The author exploits this fact to evaluate the effect of the April 1990 increase in the federal minimum wage on teenagers' wages, employment, and school enrollment. Comparisons of grouped and individual state data confirm that the rise in the minimum wage increased teenagers' wages. There is no evidence of corresponding losses in teenage employment or changes in teenage school enrollment.

ONE of the traditional criticisms of a federal minimum wage policy is that it imposes a higher relative wage floor in regions with lower average wages (see Stigler 1946:360-61). An appropriate minimum wage for New Jersey, for example, may have devastating labor market consequences in Mississippi.¹ From an evaluation perspective, however, a uniform minimum wage is an under-appreciated asset. A rise in the federal minimum wage will typically affect a larger fraction of workers in some states than in others. This variation provides a simple natural experiment for measuring the effect of legislated wage floors, with a "treatment effect" that varies across states depending on the fraction of workers initially earning less than the new minimum.

* The author, who is Professor of Economics at Princeton University, thanks Christopher Burris for research assistance and Charles Brown, Gary Fields, Larry Katz, and Alan Krueger for comments. Copies of the computer programs used in the preparation of this paper are available on request from David Card at the Department of Economics, Princeton University, Princeton, NJ 08544.

¹ A classic example of this reasoning is the effect of the federal minimum wage in Puerto Rico. See Reynolds (1965).

This paper examines the experiences following the April 1990 rise in the federal minimum wage to evaluate the effects of minimum wages on the teenage labor market. In 1989, one-quarter of all 16-19-year-olds earned between \$3.35 per hour (the existing federal minimum rate) and \$3.80 per hour (the new minimum). Across states, however, this fraction varied from under 10% in New England and California to over 50% in many southern states. Much of this variation is attributable to the presence of state-specific wage floors above the federal rate. In the late 1980s many states responded to the decade-long freeze in the federal minimum wage by raising their own minimum rates above \$3.35 per hour. These state-specific wage floors created remarkable geographic dispersion in teenage wage rates, setting the stage for the empirical analysis reported here.

Minimum Wage Statutes in 1989-90

The federal minimum wage increased to \$3.35 per hour in January 1981 and remained frozen throughout the 1980s.

By the close of the decade, cumulative inflation had eroded the purchasing power of the minimum wage to its lowest level since January 1950.² The decline in the real value of the federal minimum wage prompted state legislatures and wage boards to respond with state-specific minimum rates above the federal standard. The first of these higher minimums arose in the New England states—Maine (\$3.45 effective January 1985), Massachusetts and Rhode Island (both \$3.55 effective July 1986), New Hampshire (\$3.45 effective January 1987), and Connecticut (\$3.75 effective October 1987). By 1989 a total of 16 states and the District of Columbia had wage floors above \$3.35.³

Political pressure for an increase in the federal minimum wage culminated in March 1989 with passage of a House resolution to raise the minimum to \$4.55 over three years. A similar bill passed the Senate but was vetoed by the President. A bill providing for smaller wage increases and a liberalized youth subminimum was introduced in November 1989 and passed into law with Presidential support. This bill raised the minimum wage in two steps—to \$3.80 on April 1, 1990, and to \$4.25 on April 1, 1991—and set a training minimum equal to 85% of the regular minimum wage for employees aged 16–19.

Other provisions of the federal minimum wage were modified only slightly by the April 1990 law. The tip credit, which allows employees to credit a portion of their tips toward the minimum, was raised from 40% to 45%. Consequently, the federal minimum wage for tipped employees rose from \$2.01 to \$2.09 per hour. Exemptions for smaller businesses were

also expanded and simplified. Previously, retail and service enterprises with an annual sales volume of less than \$250,000 were exempt from coverage. This threshold was raised to \$500,000 and extended to all industries.⁴

The Effect on Teenagers: An Overview

Because teenagers are typically at the bottom of the earnings distribution, and because a large fraction of low-paid workers are teenagers, the minimum wage literature has concentrated on the youth labor market (see the chapters of the Minimum Wage Study Commission [1981] and the review article by Brown, Gilroy, and Kohen [1982]). Simple models of the teenage labor market predict varying responses to the rise in the federal minimum wage, depending on the fraction of workers initially earning below the new rate. (See Welch [1976] for a thorough overview.) Examination of the interstate patterns of wage and employment growth for teenagers between 1989 and 1990 provides a credible test of the proposition that changes in teenage labor market outcomes reflect changes in the minimum wage, rather than other factors that coincided with the law.⁵

Table 1 presents some descriptive information on teenagers taken from the monthly files of the Current Population Survey (CPS) in 1989 and 1990. Each month, individuals in the two "outgoing rotation groups" of the survey are asked to provide supplementary information on earnings and hours on their main job (if they have one). The data in Table 1 and throughout this paper are based on the responses for this 1/4 sample of the CPS. To facilitate a comparison of the periods

² Using the Consumer Price Index for all items, the real federal minimum wage in January 1950 was \$4.08 (in 1990 dollars). It ranged between \$3.64 (in 1954) and \$6.00 (in 1968). Its value in 1989 was \$3.53.

³ The widespread setting of state minimum wages above the federal rate was unprecedented. For example, Cullen (1960) observed that the federal minimum wage had served as a *ceiling* for state-specific minimum rates during the period from 1940 to 1960.

⁴ See Bureau of National Affairs (undated, 91: 1415–22).

⁵ A similar evaluation methodology figured prominently in many early studies of minimum wage laws (see especially Lester 1965:518–23), but that approach has been largely supplanted in the recent literature by aggregate time-series studies (for example, Welch 1976; Brown, Gilroy, and Kohen 1982, 1983; Wellington 1991).

Table 1. Characteristics of Teenagers and Teenage Workers, 1989 and 1990.

Description	April-December 1989					April-December 1990				
	All	All Workers	Workers with Wage:			All	All Workers	Workers with Wage:		
			<\$3.35	\$3.35-3.79	≥\$3.80			<\$3.35	\$3.35-3.79	≥\$3.80
1. Percent of All	—	49.0	3.5	11.9	31.1	—	46.4	2.6	3.4	38.2
2. Percent of Workers	—	100.0	7.1	24.4	63.6	—	100.0	5.6	7.4	82.3
3. Female (%)	49.7	48.3	61.0	53.4	45.6	49.7	48.3	62.1	51.7	47.8
4. Nonwhite (%)	19.0	11.9	10.7	15.1	10.8	19.7	11.8	8.3	17.5	11.4
5. Hispanic (%)	9.9	8.1	5.5	6.9	8.8	10.4	8.7	7.9	5.9	9.2
6. Educ < 12 (%)	62.8	53.0	65.2	68.1	45.8	64.4	53.1	68.6	73.4	49.9
7. Age 16-17 (%)	48.2	38.9	52.4	54.3	31.6	48.0	37.6	50.1	54.2	35.0
8. Enrolled in School	56.5	45.6	51.3	55.8	41.2	57.4	46.3	48.3	61.3	45.3
9. Hours/Week	—	26.6	22.0	22.5	28.8	—	26.4	22.8	20.1	27.1
10. Avg. Wage (\$/hr.)	—	4.61	2.46	3.49	5.28	—	4.84	2.46	3.54	5.12
<i>Including Tips and Commissions:</i>										
11. Av. Wage (\$/hr.)	—	4.77	3.06	3.61	5.41	—	4.99	3.04	3.67	5.24
12. Weekly Wage (\$/week)	—	134.3	69.5	82.2	161.0	—	137.8	70.7	79.7	147.2
13. Percent Reporting Tips > 0	—	11.0	24.5	12.2	9.8	—	11.5	28.2	13.3	10.9
<i>Industry Distribution:</i>										
14. Agriculture	—	4.2	6.0	2.2	3.4	—	4.4	8.9	2.5	3.3
15. Retail Trade	—	50.1	49.5	68.4	45.2	—	50.0	48.0	63.5	50.1
16. Service	—	26.2	35.8	22.6	25.9	—	27.0	37.9	27.2	26.1
17. Sample Size	18,511	9,205	674	2,326	5,735	18,549	8,625	499	653	7,049

Notes: Data are taken from 1989 and 1990 monthly Current Population Survey files (outgoing rotation groups for April-December of each year). "All Workers" include unpaid and self-employed workers. Workers in specified wage ranges exclude self-employed workers and those with allocated hourly or weekly earnings. The wage measure in row 10 is based on straight-time wages of hourly rated workers. Wage in row 11 includes pro-rated tips and commissions for hourly rated workers.

before and after the rise in the minimum wage, the samples include only the April-December surveys of each year.

The first and sixth columns of Table 1 present data for all teenagers, and the remaining columns pertain to employed teenagers and those with hourly wages in specified intervals.⁶ The U.S. teenage population includes a high fraction of nonwhites (20%) and Hispanics (10%); the respective proportions of the working population are lower. Employed teenagers also tend to be older and have more years of completed education than nonworking

teenagers. A majority of teenagers (56.5%) report that they are "attending or enrolled in high school, college, or university." A slightly lower fraction (48%) report that their main activity during the survey week was "in school." These fractions must be interpreted carefully, since school attendance rates vary over the year. During 1989 the average fraction of teenagers enrolled in school varied from 77% in April to 14% in July and August.

The CPS collects hourly wage information for individuals who are paid by the hour (93% of teenagers) and usual weekly earnings for other workers. The wage measure presented in row 10 of Table 1

⁶ The construction of the wage variable is explained below.

and used to define the columns of the table is the reported wage for hourly rated workers and the ratio of usual weekly earnings to usual weekly hours for other workers. By this "straight-time" wage measure, teenage workers earned an average of \$4.61 per hour in 1989, compared to an average of \$10.10 for all workers in the United States. Seven percent of teenagers earned less than the federal minimum wage of \$3.35 per hour, 24% earned from \$3.35 to \$4.24 per hour, and 64% earned \$3.80 per hour or more. Another 5% either were self-employed, worked without pay, or failed to report earnings information.⁷

One difficulty with the wage measure in row 10 is that some workers who report being paid by the hour also receive tips or commissions. This practice is especially widespread in retail trade, where over one-half of the teenagers are employed (see row 15 of the table). For hourly rated workers the CPS also collects usual weekly earnings *including* regular tips and commissions. This information can be used to construct an estimate of average weekly tips and an alternative measure of hourly wages. The average level of wages including pro-rated tips (in row 11 of Table 1) is 3% higher than the average based on straight-time earnings, reflecting the addition of tips and commissions for just over 10% of teenage workers.⁸

The characteristics of teenagers with "straight-time" earnings less than the minimum wage are presented in the third column of Table 1. There are various explanations for subminimum pay, including noncoverage (for tipped employees in

retail trade and full-time students under the student subminimum),⁹ employer noncompliance, and measurement error. Examination of the wage distribution of teenagers earning less than \$3.35 shows a substantial spike (21% of workers) near the tipped minimum of \$2.01 per hour, suggesting that many subminimum-wage workers are exempt from the \$3.35 standard. This is further confirmed by the higher incidence of tip income among subminimum wage teenagers: 25% of subminimum wage earners report strictly positive tip income, versus 11% overall. When hourly wages are calculated including tip income, 19% of workers with straight-time pay less than \$3.35 have effective wages above the minimum wage. Even including usual tip income, however, a substantial number of teenagers reported subminimum wages in 1989.

Employer noncompliance may partly explain this finding. Compared with other teenagers, subminimum wage workers are more likely to work in agriculture and household services, where noncompliance may be higher. Another factor is the relatively high fraction of subminimum wage workers who report being paid by the week or month, rather than by the hour (25%, versus 7% of all working teenagers). Some salaried workers are legally exempt from the minimum wage, and others may have over-reported their usual weekly hours, leading to a downward bias in their imputed hourly wage.

The next column of Table 1 presents the characteristics of teenagers reporting hourly wages of \$3.35 to \$3.79 in 1989. For simplicity, I refer to this group as "affected workers," since a rise in the minimum wage is most likely to affect employees of complying firms in the covered sector who previously earned less than the new rate. Affected teenagers are

⁷ The Census Bureau allocates responses for individuals who do not answer the earnings questions in the CPS (about 3% of teenage workers). To avoid measurement error, I do not use the earnings data for these individuals.

⁸ To avoid problems posed by measurement error, I set the wage including tips equal to the reported straight-time hourly wage *unless* the difference between average weekly earnings including tips and the product of the straight-time wage and usual weekly hours is positive. The average wage measures in Table 1 also exclude individuals with reported or imputed wages less than \$1 per hour or greater than \$20 per hour.

⁹ Under the pre-1989 law, employers in retail trade, agriculture, and higher education were permitted to pay full-time students a subminimum wage 15% below the regular rate. The available evidence suggests that use of this exemption was relatively modest. Freeman, Gray, and Ichniowski (1981) estimate that only 3% of student hours in the late 1970s (when the minimum was relatively high) were worked under the subminimum provisions.

more likely to be enrolled in school than those with either higher or lower wages, and they are also more likely to be employed in retail trade. Some 40% of affected workers report an hourly wage *exactly* equal to the 1989 minimum wage. Their wage distribution shows additional spikes at \$3.50 and \$3.75, with an average of \$3.49 per hour.

The five right-hand columns of Table 1 present corresponding information for 1990. Teenagers as a whole reported slightly higher enrollment rates during April–December of 1990 than in the same months of 1989. The teenage employment rate, on the other hand, fell by 2.5 percentage points.¹⁰ For comparison, the annual average teenage employment rates for 1989 and 1990 (published by the Bureau of Labor Statistics) were 47.5% and 45.4%. Thus, the employment data for April to December reflect a slightly larger downturn than the annual averages.

The teenage wage distribution also shifted between 1989 and 1990, with a sharp reduction in the fraction of workers earning \$3.35–\$3.79 per hour (from 24.4% to 7.4%) and a mean increase of 5%. A comparison of the 1989 and 1990 distributions shows the elimination of the previous spike at \$3.35 per hour and the emergence of a new spike at \$3.80. Interestingly, there was only a slight reduction in the fraction of teenagers reporting wages (exclusive of tips) under \$3.35.

Although these patterns are suggestive of the effect of the new minimum wage law, even stronger evidence of its impact is provided in Figure 1, which shows quarterly averages of the fractions of teenagers earning less than \$3.35, exactly \$3.35, and \$3.36–\$3.79 per hour from 1989-I to 1990-IV. The figure indicates an abrupt drop in the fraction earning less than \$3.80 per hour in the second quarter of

1990 (that is, after April 1). Most of this drop reflects a reduction in the fraction earning \$3.35–\$3.79, with little evidence of an effect on the fraction earning less than \$3.35. The effect of the minimum wage law was mainly concentrated on workers who previously earned at least the old minimum wage but less than the new rate.

Two other aspects of Figure 1 also deserve comment. First, there is only a slight dip in the fraction earning less than \$3.80 per hour in the first quarter of 1990, even though the new minimum wage was signed into law in November 1989. Most employers evidently waited until the effective date of the law to increase the wages of their teenage employees. Second, the fraction of workers earning exactly \$3.35 shows a continuing decline after 1990-II, suggesting some lag in the adjustment of wages (or in the reporting process).

Before turning to a regional analysis of the effects of the increased federal minimum wage, it is worthwhile to analyze the aggregate change in teenage employment between 1989 and 1990. Much of the existing literature has used the correlation between minimum wages and aggregate teenage employment to infer the effect of the law. As noted in Table 1, teenage employment fell between 1989 and 1990. Part of this decline is clearly attributable to the 1990 recession, which began in mid-year. The youth labor market is highly cyclical, and the onset of a recession would be expected to lower teenage employment by several percentage points. This historical relationship is illustrated in Figure 2, which graphs annual average teenage employment rates for 1975–90 along with the predicted rates from a linear regression on a trend and the overall employment-population ratio.¹¹ The prediction equation tracks the actual teenage employment rate up to 1989 remarkably well; the

¹⁰ The standard errors of the 1989 and 1990 employment rates for all teenagers in the top row of Table 1 are both 0.4%. The standard error for the change in employment rates between 1989 and 1990 is 0.5%.

¹¹ The regression is estimated with data for 1975–89. The fitted equation is
 Teen Employment = Constant – 0.86 · Trend
 + 2.17 · Overall Employment Rate,
 with an R^2 of 0.99.

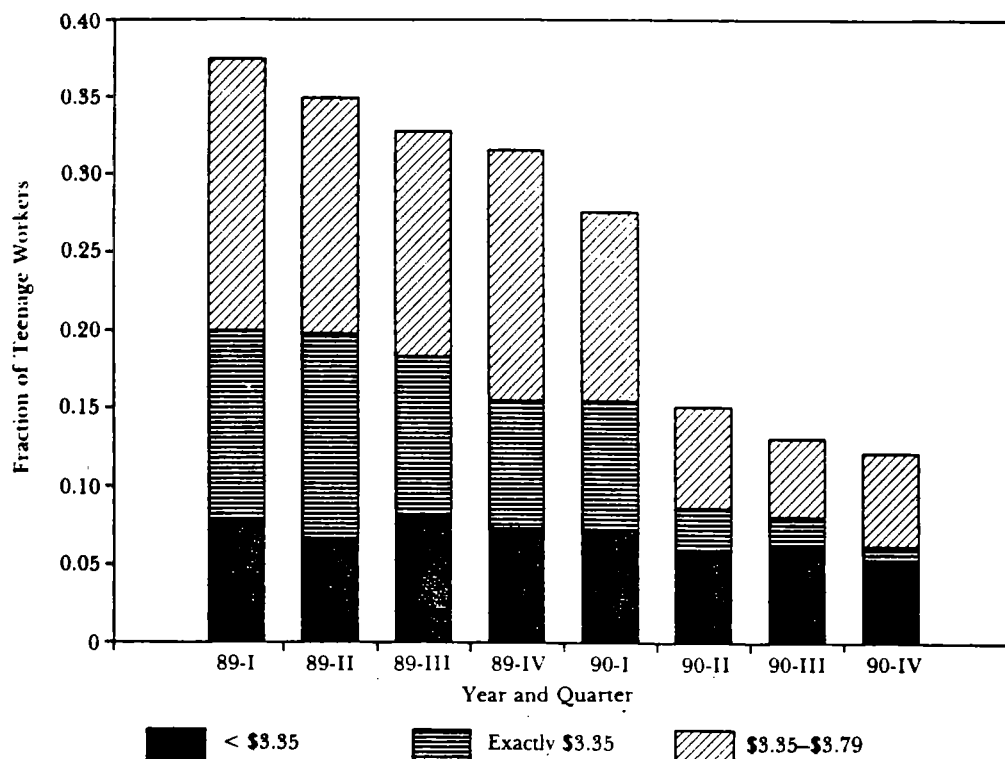


Figure 1. Share of Teenagers Earning Less Than \$3.80 per Hour, 1989–1990.

1990 rate, however, was about 0.6% lower than expected. Although it may be tempting to attribute this discrepancy to the effect of the increased minimum wage, it should be noted that the real minimum wage was relatively high in 1976, 1979, and 1981, and then trended down throughout the late 1980s with little apparent effect on employment.¹²

A Grouped Analysis

The nationwide data in Table 1 and Figure 1 conceal considerable interstate variation in the distribution of teenage wages prior to the rise in the federal minimum wage. This wide variation suggests two com-

plementary approaches to analyzing the effect of the 1990 increase in the minimum wage. The first is to aggregate states into groups with similar fractions of affected workers in 1989. This approach generates relatively large sample sizes in each group, permitting a quarterly analysis along the lines of Figure 1. A second approach is to use all the states and pool the months before and after April 1990 for each state. I first present the grouped analysis, then turn to a state-by-state analysis.

Figure 3 plots the fraction of workers earning \$3.35–\$3.79 by quarter for three groups of states: states with under 20% of teenage workers earning \$3.35–\$3.79 in 1989 (“high-wage states”); states with over 40% of teenage workers earning \$3.35–\$3.79 in 1989 (“low-wage states”); and all other states (“medium-wage states”). The high-wage group contains 16 states, most of which had passed state-specific minimum wages above \$3.35 per hour (all of New England, New York, New Jersey, Minnesota, Delaware, Maryland, District of Columbia,

¹² If the prediction equation is re-estimated including the logarithm of the real value of the federal minimum wage (deflated by the Consumer Price Index), the estimated minimum wage coefficient is -2.5 , with a standard error of 1.7 . This coefficient implies that a 10% increase in the minimum wage will reduce teenage employment by 0.25%—a smaller effect than is usually estimated in the literature. See Wellington (1991) for some recent estimates.

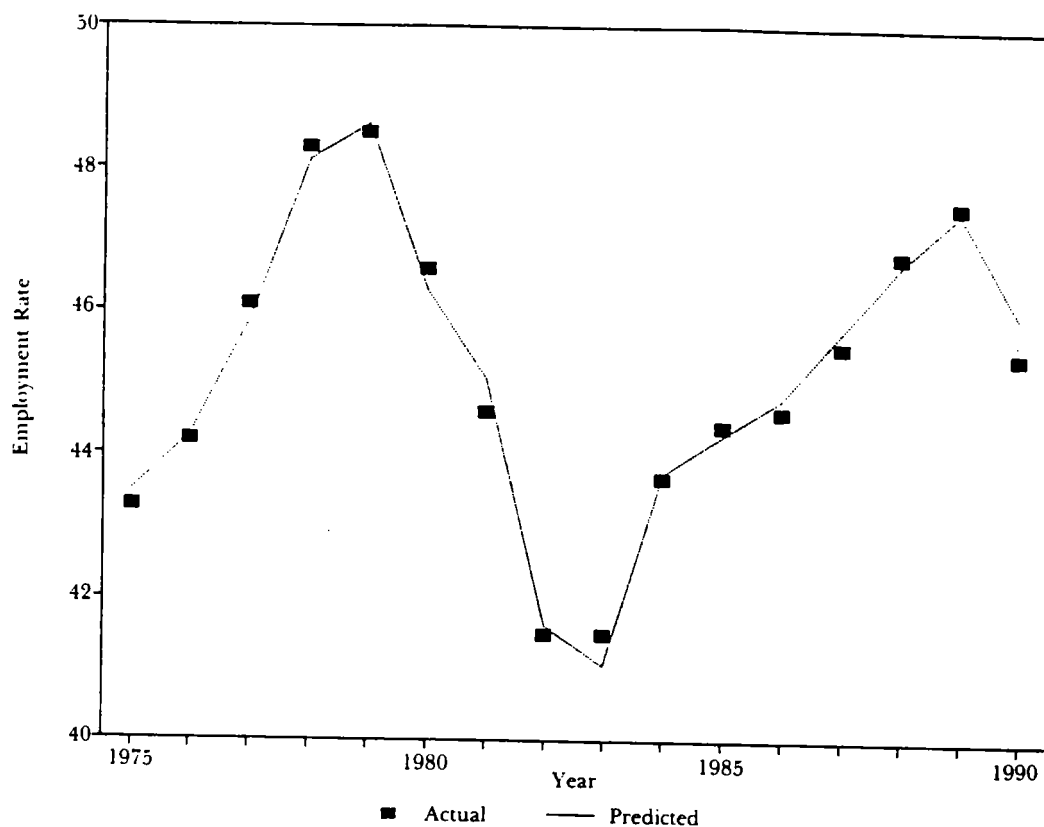


Figure 2. Actual and Predicted Teenage Employment-Population Rate, 1975-1990.

Nevada, Washington, California, Alaska, and Hawaii). The low-wage group contains 11 southern and mountain states (West Virginia, South Carolina, Kentucky, Tennessee, Mississippi, Arkansas, Louisiana, Oklahoma, Montana, Wyoming, New Mexico) plus North Dakota and South Dakota. The medium-wage group includes the remaining 22 states.

As expected, the impact of the 1990 minimum wage law is concentrated among the low- and medium-wage states. Both state groups show a sharp decline in the fraction of teenagers earning \$3.35-\$3.79 per hour after April 1, 1990. By the end of 1990, the fractions of teenagers earning \$3.35 to \$3.79 per hour were remarkably similar across states.

Table 2 presents quarterly averages of teenage wages and employment rates by state group, along with their sampling errors and the differences in the outcomes between corresponding quarters of 1989 and 1990. Assuming that underlying labor market trends

were the same in the three groups of states, one way to estimate the effect of the federal minimum wage is to compare outcomes in 1990 to outcomes for the same quarter in 1989, and then to compare these differences across the three groups of states. To facilitate this comparison, the bottom row of the table gives the average differences between the second, third, and fourth quarters of 1989 and 1990.

Looking first at earnings, the high-wage states show an average 4% wage gain between 1989 and 1990, with no evidence of an accelerated trend after 1990-I (that is, after the increase in the minimum wage). Average wages in the low- and medium-wage states, on the other hand, show a noticeable upsurge in 1990-II. Comparing the last three quarters of 1989 and 1990 across the three groups, the data in Table 2 suggest that the rise in the federal minimum wage increased average teenage wages by 2% in the medium-wage states and by 6% in the low-wage states.

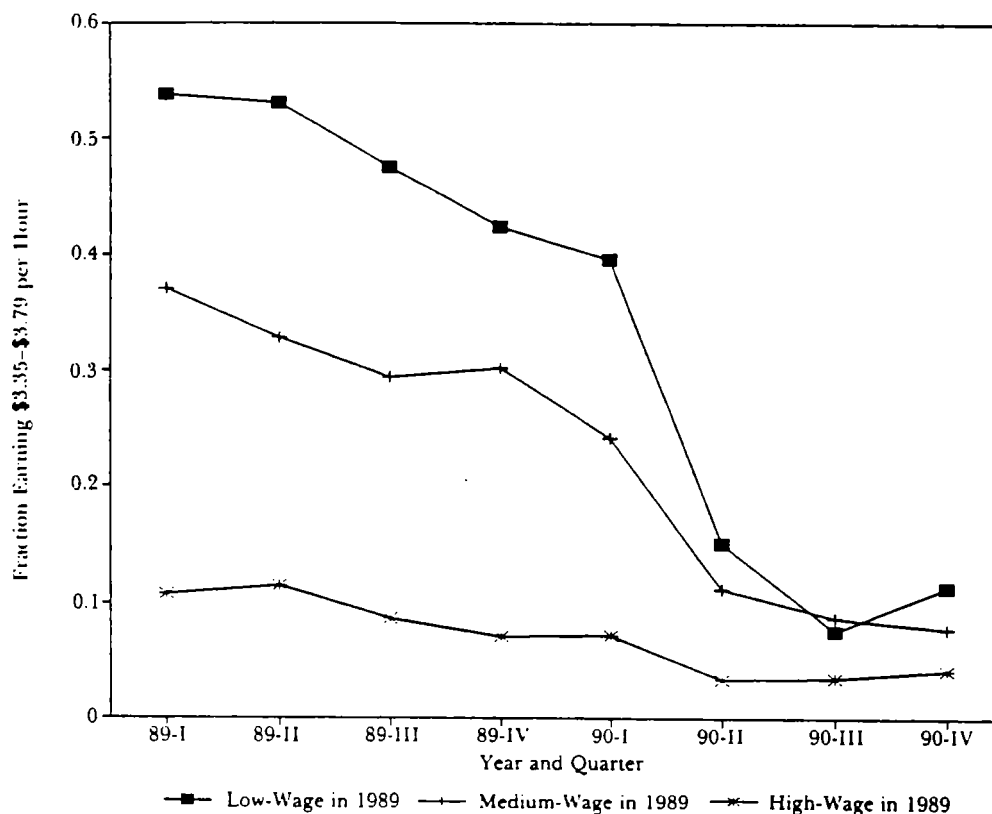


Figure 3. Fraction of Teenage Workers Earning \$3.35-\$3.79 per hour: Three Groups of States, 1989-1990.

As a benchmark, it is useful to compare these estimated effects to the wage gains implied by a naive model in which the only effect of the minimum wage is to raise the earnings of affected workers up to the new minimum. Such a model will tend to understate the wage gains if there are significant disemployment effects of the rise in the minimum wage, or if the increase in the minimum wage "spills over" to higher-wage workers.¹³ In low-wage states the fraction of affected workers fell from over 50% in 1989 to 10% in 1990-IV. Ignoring any disemployment or spillover effects, the predicted effect of the increased federal minimum on average wages in the low-wage states is then 0.40 times the average percentage increase for a wage earner who moves from the affected wage range to the new minimum wage. As shown in Table 1, the average wage

of affected workers was \$3.49 per hour. An increase to \$3.80 is therefore equivalent to a 9% wage increase. Thus, if the only effect of the minimum wage is to increase the earnings of workers in the \$3.35-3.79 range up to \$3.80, the predicted wage impact in the low-wage states is 3.6%. A similar calculation for the medium-wage states implies a 2.1% wage impact. These benchmarks provide a close approximation to the observed wage gain in the medium-wage states but significantly under-predict the wage gain in low-wage states.

The right-hand columns of Table 2 present teenage employment-population rates by state group and quarter. One obvious aspect of these data is the seasonal pattern of employment, which shows a peak in the third quarter and a trough in the first. It is also interesting to note that teenage employment increased in all three groups of states between 1990-I and 1990-II, although employment rates were

¹³ See Grossman (1983) for an earlier analysis of his spillover hypothesis.

Table 2. Mean Log Wages and Employment Rates in Three Groups of States.
(Standard Errors in Parentheses)

State Group	Mean Log Wages			Employment Rates		
	Low-Wage	Medium-Wage	High-Wage	Low-Wage	Medium-Wage	High-Wage
<i>1989</i>						
I	1.33 (0.01)	1.41 (0.01)	1.56 (0.01)	33.9 (1.4)	44.4 (0.9)	44.3 (1.1)
II	1.33 (0.01)	1.42 (0.01)	1.58 (0.01)	37.9 (1.5)	50.8 (0.9)	45.4 (1.2)
III	1.35 (0.01)	1.43 (0.01)	1.61 (0.01)	45.4 (1.5)	55.7 (0.9)	52.0 (1.1)
IV	1.37 (0.02)	1.44 (0.01)	1.58 (0.01)	41.7 (1.5)	47.5 (0.9)	46.6 (1.1)
<i>1990</i>						
I	1.38 (0.02)	1.44 (0.01)	1.61 (0.01)	37.8 (1.5)	45.5 (0.9)	42.0 (1.1)
II	1.45 (0.01)	1.48 (0.01)	1.62 (0.01)	44.1 (1.5)	50.0 (0.9)	45.8 (1.1)
III	1.43 (0.02)	1.48 (0.01)	1.66 (0.01)	42.6 (1.5)	50.8 (0.9)	49.0 (1.1)
IV	1.47 (0.02)	1.50 (0.01)	1.61 (0.01)	34.7 (1.4)	45.1 (0.9)	41.0 (1.1)
<i>Change from 1989 to Same Quarter of 1990</i>						
I	0.05 (0.02)	0.03 (0.01)	0.05 (0.02)	3.9 (2.0)	1.1 (1.3)	-2.3 (1.5)
II	0.12 (0.02)	0.06 (0.01)	0.04 (0.01)	6.2 (2.1)	-0.8 (1.3)	0.4 (1.6)
III	0.08 (0.02)	0.05 (0.01)	0.05 (0.01)	-2.8 (2.1)	-4.9 (1.3)	-3.0 (1.5)
IV	0.10 (0.02)	0.06 (0.01)	0.03 (0.02)	-7.0 (2.1)	-2.4 (1.3)	-5.6 (1.5)
Average for II, III, IV	0.10 (0.01)	0.06 (0.01)	0.04 (0.01)	-1.2 (1.2)	-2.7 (0.8)	-2.7 (0.9)

Note: The low-wage group includes states with more than 40% of teens earning \$3.35-3.79 per hour in 1989; the high-wage group includes states with less than 20% of teens earning \$3.35-3.79 per hour in 1989; and the medium-wage group includes all other states.

uniformly lower in 1990 than in 1989. The quarterly differences in the lower panel indicate that teenage employment fell by more in the high-wage states than in the low-wage states. Averaged over the last three quarters of each year, teenage employment growth was 1.5% higher in the low-wage states than in the high-wage states (standard error = 1.5%), with no difference between the medium-wage and high-wage states.

Ignoring other sources of relative teenage employment growth, the data in Table 2 suggest that the rise in the federal minimum wage increased teenage employment in the low-wage states, with no measurable ef-

fect in the medium-wage states. The effect in low-wage states is the opposite of the prediction from conventional models of the teenage labor market. One explanation for this finding is interstate variation in the timing and severity of the 1990 downturn. In fact, there is some evidence of a stronger downturn in the initially high-wage states and a more moderate recession in the low-wage states. Between the last three quarters of 1989 and 1990 the employment-population ratio for all workers grew by 0.45 percentage points in the low-wage states, fell by 0.01 points in the medium-wage states, and fell by 0.23 points in the high-wage states. These differences can potentially explain at

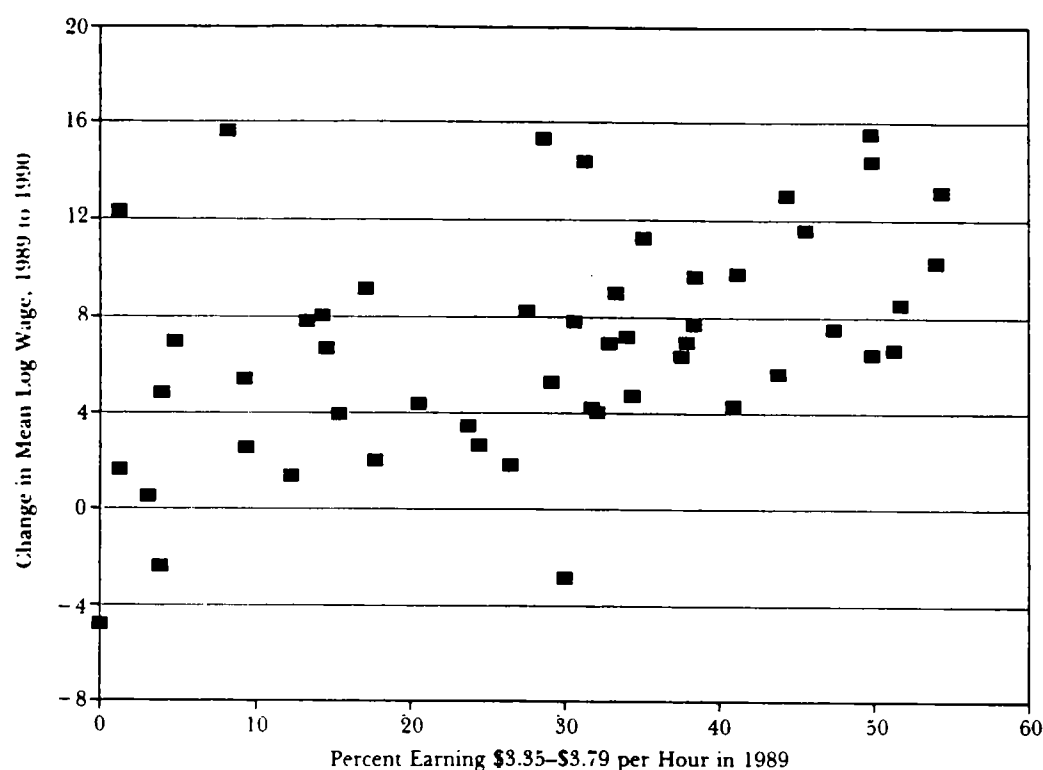


Figure 4. Change in Mean Log Wage of Teenage Workers Versus Percent Earning \$3.35-\$3.79 per Hour in 1989.

least some of the differences in teenage employment growth among the three state groups.

To investigate this question more formally, I fit a regression model to the quarterly teenage employment rates in the three state groups, including group-specific intercepts, quarterly dummies, the overall employment rate for the state-group and quarter, and group-specific dummies measuring the change in teenage employment after 1990-II (that is, after the increase in the minimum wage).¹⁴ The estimated employment effects in the post-increase period are -2.5% for the low-wage states, -2.7% for the medium-wage states, and -2.6% for the high-wage states. These estimates suggest that differences in the strength of the aggregate labor market can potentially explain almost all of the intergroup variation in teenage

employment growth between the last three quarters of 1989 and 1990. Accounting for aggregate factors, however, there is no indication of an adverse employment effect in the low-wage states, where the increase in the federal minimum wage raised teenage wages by 6%.

An Analysis by State

An alternative to the grouping strategy used in Table 2 and Figure 3 is to treat each state as a separate observation, and to correlate changes in employment, wages, and other outcomes with the fraction of affected workers in the state. Owing to the relatively small numbers of observations for many states, I have not analyzed quarterly data by state. Rather, I have aggregated data for the last three quarters of 1989 and 1990 for each state. Comparisons between 1989 and 1990 allow a "pre/post" comparison of the effect of the increase in the federal minimum wage on April 1, 1990. The data for

¹⁴ The model is estimated on data for 1989 and 1990. The implied elasticity of the teenage employment rate with respect to the overall employment rate is 1.70 (with a standard error of 0.83).

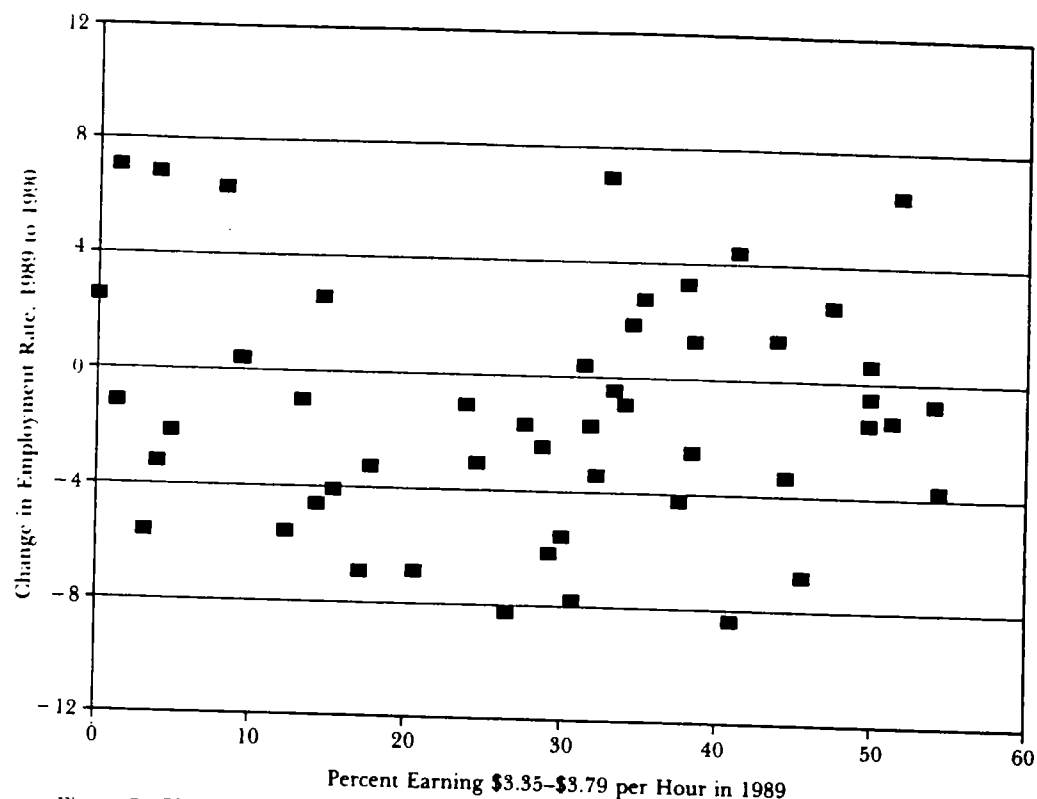


Figure 5. Change in Teenage Employment Rates Versus Percent Earning \$3.35-\$3.79 per Hour in 1989.

the two years are drawn from the same months and therefore are unaffected by any systematic seasonal effects.

Figure 4 illustrates the interstate correlation between the fraction of teenagers earning \$3.35-\$3.79 per hour in 1989 and the increase in mean log wages between 1989 and 1990. The estimated regression model corresponding to the figure is presented in the first column of Table 3.¹⁵ The estimated slope is 0.15, somewhat higher than the benchmark effect (.088) predicted by assuming that the rise in the minimum wage simply raised the wages of those in the affected wage range to \$3.80 per hour. As suggested by the figure, the estimated regression coefficient is fairly precise: variation in the fraction of affected workers in 1989 explains a respect-

able 30% of the interstate variation in wage growth between 1989 and 1990.

Columns (2) and (3) of Table 3 introduce two alternative "macro-level" labor market indicators into the wage change equation. These are the change in the overall employment-population rate in the state between 1989 and 1990, and the corresponding change in the overall unemployment rate. Both variables are based on state-level averages published in the Bureau of Labor Statistics' "Geographic Profiles of Employment and Unemployment." Changes in overall employment or unemployment rates help to control for any state-specific labor demand shocks that may be correlated with the fraction of affected workers. As it happens, neither of these variables is very highly correlated with the growth rate of teenage wages, and their inclusion hardly affects the model.

Figure 5 plots state-level observations on the change in the teenage employment pop-

¹⁵ I have weighted the regression model by the average CPS extract size for each state. Unweighted results are very similar.

Table 3. Estimated Regression Equations for State-Average Changes in Wages and Employment Rates of Teenagers, 1989-1990.
(Estimated Standard Errors in Parentheses)

Explanatory Variable	Equations for Change in Mean Log Wage:			Equations for Change in Teen Employment-Population Ratio:				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. Fraction of Affected Teens	0.15 (0.03)	0.14 (0.04)	0.15 (0.04)	0.02 (0.03)	-0.01 (0.03)	0.01 (0.04)	-	-
2. Change in Overall Emp./Pop. Ratio	-	0.46 (0.60)	-	-	1.24 (0.60)	-	-	1.27 (0.66)
3. Change in Overall Unemployment Rate	-	-	-0.24 (0.92)	-	-	-0.16 (0.95)	-	-
4. Change in Mean Log Teenage Wage*	-	-	-	-	-	-	0.12 (0.22)	-0.06 (0.24)
5. R-squared	0.30	0.31	0.30	0.01	0.09	0.01	0.01	0.09

Notes: Estimated on a sample of 51 state observations. Regressions are weighted by average CPS extract sizes for teenage workers in each state. All regressions include an unrestricted constant. The mean and standard deviation of the dependent variable in columns 1-3 are 0.0571 and 0.0417; the mean and standard deviation of the dependent variable in columns 4-9 are -0.0225 and 0.0361.

* In columns 7-9, the change in mean log is instrumented by the fraction of teenage workers earning \$3.35-3.79 in 1989.

ulation rate between 1989 and 1990 against the fraction of affected wage earners in 1989. Unlike the corresponding plot for wage changes, this figure suggests no strong relation between the fraction of affected wage earners and the change in employment rates. The estimated regression models in columns 4-6 of Table 3 confirm this visual impression. Whether or not overall labor market indicators are included as additional controls, the fraction of affected teenagers in 1989 has virtually no effect on the change in employment rates.¹⁶

The estimated wage change models in columns 1-3 and the estimated employment change models in columns 4-6 can be interpreted as "reduced-form" equations from a very simple structural model that explains the wage increase between 1989 and 1990 in state i (ΔW_i) as a function of the fraction of teenagers in the affected wage range in the state in 1989

(F89 _{i}) and other variables (X_i), and the employment change in state i (ΔE_i) as a movement along the teenage employment demand function:

$$(1) \quad \Delta W_i = a + bF89_i + cX_i + e_i$$

$$(2) \quad \Delta E_i = \alpha + \beta \Delta W_i + \gamma X_i + \epsilon_i$$

Here the coefficient β is a conventional labor demand elasticity, and e_i and ϵ_i are residual components of wage growth and employment demand. The reduced-form employment change equation is

$$(3) \quad \Delta E_i = \alpha + b\beta F89_i + (\gamma + c\beta) X_i + \beta e_i + \epsilon_i$$

Comparison of (1) and (3) shows that the elasticity of demand for teenage labor can be obtained by taking the ratio of the "Fraction Affected" coefficient in the employment growth equation to the corresponding coefficient in the wage growth equation. Alternatively, the same numerical estimate of the demand elasticity can be recovered by estimating the employment change equation (2) by two-stage least squares, using the fraction of teenagers in the affected wage range as an instrumental variable for the change in teenage wages. Such estimates are presented in columns 7-9 of Table 3.

The implied employment demand elasticities are uniformly small. When the

¹⁶ One potential issue in the estimation of standard errors for the models in Table 4 is the presence of systematic correlation between the residuals of nearby states. This "spatial correlation" will tend to lead to understated standard errors. As a rough check, I computed the Durbin-Watson (DW) statistics for the residuals. If the states are sorted by region, this computation provides a test for spatial correlation. The DW statistics for both the wage and employment models are very close to 2, giving no evidence of spatial correlation.

overall employment-population ratio is included as a control variable (column 8), the estimated elasticity is negative but close to 0. Without controlling for overall labor market conditions (column 7) or using the overall unemployment rate as a control (column 9), the estimated elasticity is positive but close to 0. As suggested by the grouped analysis in Table 2, there is no evidence of a significant disemployment effect of the federal minimum wage.

The analysis in Table 3 can be extended in several directions. One extension is to model the dynamic structure of employment growth. Another is to consider more general measures of the impact of the federal minimum wage on state-specific wage changes. Both issues are addressed by the estimates in Table 4.

The first 4 columns of Table 4 report reduced-form employment growth regressions that include lagged values of the dependent variable.¹⁷ For simplicity, I have only reported models that include the overall employment-population ratio; models that include the aggregate unemployment rate as an alternative control variable yield similar conclusions. The estimates in column 1 suggest that the lagged employment growth exerts a significant negative effect on current growth. This pattern is consistent with an underlying second-order autoregressive model of teenage employment at the state level.¹⁸ In column 2 I include the lagged value of the overall employment change. Controlling for the contemporaneous aggregate employment change and the lagged dependent variable, this variable has a small and statistically insignificant coefficient. In both specifications the coefficient of the fraction of affected teenage wage earners (in row 1) is small and insignificantly different from 0.

One potential difficulty with the esti-

mated models in columns 1 and 2 is the presence of measurement error in the lagged dependent variable. Random sampling errors in the state-specific teenage employment rate will tend to create a negative bias in the estimated coefficient of the lagged teenage employment growth rate. To check for the magnitude of this bias, column 3 presents a model in which the lagged dependent variable is instrumented by the lagged change in the overall employment-population ratio. The results of this exercise suggest the bias is small enough to be safely ignored.

Although labor demand shocks affecting teenage employment in a state are likely to be captured by the overall employment rate in the state, it is possible that other regional shocks may also play a role. To test this hypothesis, in column 4 of Table 4 I present a model that includes the change in the overall employment rate for nine different regions of the country. The addition of this variable lowers the coefficient on the state-specific employment rate, although the regional employment change is not itself statistically significant. The coefficient of the fraction of affected wage earners also falls slightly (to -0.003).

Columns 5–9 of Table 4 present instrumental variables estimates of the state-specific teenage employment demand equation, allowing for an effect of the lagged dependent variable. These models differ by the choice of variable(s) used as instruments for the change in teenage wages. Following the specifications of Table 3, columns 5 and 6 present models that use the fraction of teenagers earning \$3.35–\$3.79 per hour in 1989 to instrument the wage change. In columns 7–9 I use three alternative measures of the wage impact of the federal minimum. The model in column 7 uses the fraction of teenagers earning exactly \$3.35 per hour in 1989. The model in column 8 uses both the fraction of teenagers at the old minimum wage and the fraction in the affected wage range. Finally, the model in column 9 uses the fraction of teenagers earning less than \$3.35 and the fraction earning \$3.35–\$3.79 per hour in 1989.

¹⁷ The dependent variable in Table 5 is the state-specific change in the teenage employment rate between the last three quarters of 1989 and the last three quarters of 1990. The lagged change in the teenage employment-population ratio is based on data for all four quarters of 1988 and 1989.

¹⁸ A similar pattern for overall state-level employment is suggested by the results in Topel (1986).

Table 4. Estimated Regression Equations for State-Average Changes in the Employment Rate of Teenagers, 1989–1990.
(Estimated Standard Errors in Parentheses)

Explanatory Variable	Reduced-Form Employment Equations				Structural Employment Demand Equations ^b				
	OLS (1)	OLS (2)	IV ^a (3)	OLS (4)	(5)	(6)	(7)	(8)	(9)
1. Fraction of Affected Teens	0.02 (0.04)	0.02 (0.04)	0.02 (0.06)	-0.00 (0.04)	—	—	—	—	—
2. Change in Overall Emp./Pop. Ratio	1.05 (0.58)	1.05 (0.59)	1.04 (0.65)	0.81 (0.62)	0.96 (0.65)	0.82 (0.67)	0.92 (0.64)	0.94 (0.64)	1.10 (0.62)
3. Lagged Change in Teen Emp./Pop. Rate	-0.41 (0.18)	-0.41 (0.20)	-0.44 (0.63)	-0.44 (0.19)	-0.44 (0.20)	-0.44 (0.21)	-0.45 (0.20)	-0.45 (0.20)	-0.40 (0.20)
4. Lagged Change in Overall Emp./Pop. Rate	—	-0.03 (0.64)	—	—	—	—	—	—	—
5. Change in Regional Emp./Pop. Rate	—	—	—	1.61 (1.49)	—	1.63 (1.60)	—	—	—
6. Change in Mean Log Teenage Wage	—	—	—	—	0.16 (0.28)	-0.02 (0.36)	0.19 (0.27)	0.18 (0.27)	0.06 (0.23)
7. R-squared	0.18	0.18	0.11	0.20	0.18	0.19	0.18	0.18	0.17

Note: See note to Table 3. In all columns the dependent variable is the change in the state-average teenage employment rate from 1989 to 1990 (April–December only).

^a In column 3 the lagged change in the teenage employment-population ratio is instrumented by the lagged change in the overall employment-population ratio.

^b The change in mean log teenage wage is endogenous. In columns 5 and 6 the instrument is the fraction of teenagers in the state earning \$3.35–\$3.79 per hour in 1989; in column 7 the instrument is the fraction earning \$3.35 per hour in 1989; in column 8 the instruments are the fractions earning exactly \$3.35 and \$3.35–\$3.79 per hour in 1989; and in column 9 the instruments are the fractions earning less than \$3.35 per hour and \$3.35–\$3.79 per hour in 1989.

Regardless of specification, the models suggest negligible wage elasticities, although the estimated standard errors are large enough that one cannot rule out a small negative employment demand elasticity.

The results in Table 3 and 4 suggest that interstate differences in teenage employment growth between 1989 and 1990 were unrelated to the state-specific wage impact of the federal minimum wage increase. Another closely monitored outcome for teenagers is the fraction enrolled in school. A standard hypothesis in the literature is that increases in the minimum wage will increase school enrollment. (See, for example, Ehrenberg and Marcus 1980.) This prediction, however, is based on the assumption that increases in the minimum wage reduce teenage employment opportunities. In light of the results in Tables 3 and 4, it is interesting to correlate interstate changes in enrollment with differences in the wage effect of the federal minimum wage.

To abstract from the seasonal pattern of

school enrollment, I used CPS data for September–December of 1989 and 1990 to construct state-specific estimates of the change in the fraction of teenagers enrolled in school (either full- or part-time). In the United States as a whole, the fraction of teenagers enrolled in school during September–December rose from 73.7% in 1989 to 74.6% in 1990. Across states, changes in enrollment are negatively correlated with changes in employment rates (the correlation is -0.19, with a probability value of 0.18). I then fit a simple regression model for the change in enrollment as a function of the change in the overall employment rate in the state and the fraction of teenagers in the affected wage range in 1989. The coefficient of the overall employment change variable is -0.46 (with a standard error of 0.77), suggesting that enrollment growth was faster (although not significantly so) in states that experienced bigger employment reductions between 1989 and 1990. The coefficient of the fraction affected variable is -0.003 (with a standard error

of 0.05), implying that changes in enrollment were essentially unrelated to the potential wage impact of the rise in the federal minimum wage. As with the employment results, there is no evidence of a connection between teenage school enrollment and the minimum wage.

Conclusion

I have used the experiences generated by the April 1990 rise in the federal minimum wage to measure the effects of the minimum wage on teenagers. The imposition of a national wage standard sets up a very useful natural experiment in which the "treatment effect" in any particular state depends on the fraction of workers initially earning less than the new minimum. By the end of the 1980s, interstate dispersion in teenage wages was remarkable. Many states had already passed state-specific minimum wages above the new federal standard. The fraction of teenagers potentially affected by the rise in the minimum wage ranged from under 5% in some New England and West Coast states to over 50% in some southern states.

The 1990 law raised the minimum wage by 13%. Estimates in the previous literature (Brown, Gilroy, and Kohen 1982)

suggest that this increase would lower aggregate teenage employment by 1 to 4 percentage points. More important, however, these employment losses should have been concentrated in low-wage states, providing a test of the hypothesis that the changes are attributable to the minimum wage.

Comparisons of grouped and individual state data confirm that the rise in the minimum wage raised average teenage wages. The wage gains were as big as or slightly bigger than the increases predicted by assuming that individuals earning less than the new minimum rate had their wages "topped up" to the new standard. On the other hand, there is no evidence that the rise in the minimum wage significantly lowered teenage employment rates or altered school enrollment patterns. These findings, although at odds with conventional predictions, are consistent with the earlier "case study" literature (Lester 1960) and with the findings of two recent studies using a similar methodology: my study of the 1988 California minimum wage law, and Katz and Krueger's study of the effects of the recent federal minimum wage increases on the fast-food industry in Texas (both in this issue).

REFERENCES

- Brown, Charles, Curtis Gilroy, and Andrew Kohen. 1982. "The Effect of the Minimum Wage on Employment and Unemployment." *Journal of Economic Literature*, Vol. 20, No. 2 (June), pp. 487-528.
- . 1983. "Time Series Evidence on the Effect of the Minimum Wage on Youth Employment and Unemployment." *Journal of Human Resources*, Vol. 18, No. 2 (Winter), pp. 3-31.
- Bureau of National Affairs. Undated. *Labor Relations Reporter Wages and Hours Manual*. Washington, D.C.
- Card, David. 1991. "Do Minimum Wages Reduce Employment? A Case Study of California, 1987-89." In this issue.
- Cullen, Donald E. 1961. *Minimum Wage Laws*. Bulletin 43. New York State School of Industrial and Labor Relations, Cornell University.
- Ehrenberg, Ronald, and Alan J. Marcus. 1980. "Minimum Wage Legislation and the Educational Outcomes of Youth." In Ronald Ehrenberg, ed., *Research in Labor Economics*, Vol. 3. Greenwich, Conn.: JAI Press, pp. 61-93.
- Freeman, Richard, Wayne Gray, and Casey E. Ichniowski. 1981. "Low Cost Student Labor: The Use and Effects of the Youth Subminimum Provisions for Full-Time Students." *Report of the Minimum Wage Study Commission*, Vol. 5. Washington, D.C.: GPO, pp. 305-35.
- Grossman, Jean B. 1983. "The Impact of the Minimum Wage on Other Wages." *Journal of Human Resources*, Vol. 18, No. 3 (Summer), pp. 359-78.
- Katz, Lawrence F., and Alan B. Krueger. 1992. "The Effects of the Minimum Wage on the Fast Food Industry." In this issue.
- Lester, Richard A. 1960. "Employment Effects of Minimum Wages." *Industrial and Labor Relations Review*, Vol. 13, No. 2 (January), pp. 254-64.

- . 1964. *Economics of Labor*, 2nd Edition. New York: Macmillan.
- Minimum Wage Study Commission. 1981. *Report of the Minimum Wage Study Commission*. Washington, D.C.: GPO.
- Reynolds, Lloyd G. 1965. "Wages and Employment in the Labor Surplus Economy." *American Economic Review*, Vol. 55, No. 1 (March), pp. 19-39.
- Stigler, George. 1946. "The Economics of Minimum Wage Legislation." *American Economic Review*, Vol. 36, No. 3 (June), pp. 358-65.
- Topel, Robert H. 1986. "Local Labor Markets." *Journal of Political Economy*, Vol. 94, No. 3, Part 2 (June), pp. S111-S143.
- U.S. Department of Labor, Bureau of Labor Statistics. Various years. "Geographic Profiles of Employment and Unemployment." Bulletin Nos. 2327, 2361, 2381. Washington, D.C.: GPO.
- Welch, Finis. 1976. "Minimum Wage Legislation in the United States." In Orley Ashenfelter and James Blum, eds., *Evaluating the Labor Market Effects of Social Programs*. Princeton, N.J.: Princeton University Industrial Relations Section.
- Wellington, Alison J. 1991. "Effects of the Minimum Wage on the Employment Status of Youths: An Update." *Journal of Human Resources*, Vol. 26, No. 1 (Winter), pp. 27-46.

41
Hours of work per week manufacturing
Durée du travail par semaine : industries manufacturières

Actual hours of work per worker
Heures de travail effectuées par ouvrier

Country or area Pays ou zone	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Argentina ^{1,2} Argentine ^{1,2}	186.7	180.4	181.3	177.9	178.4	179.7	188.4	...	...	...
Australia ^{3,4} Australie ^{3,4}	37.4	37.4	36.1	35.5	# 36.2	36.9	37.0	37.3	37.9	37.7
Male Homme	38.6	38.6	37.2	36.5	# 37.4	38.4	38.6	38.8	39.6	39.4
Female Femme	33.8	33.9	32.4	32.3	# 32.7	32.5	32.7	32.9	33.1	32.8
Austria ^{5,6} Autriche ^{5,6}	146.0	144.7	145.5	144.3	145.2	144.9	142.1	139.9	141.0	139.6
Belgium ^{7,8} Belgique ^{7,8}	33.4	33.8	33.6	33.5	33.7	33.1	33.0	33.0	33.4	33.6
Bermuda ⁹ Bermudes ⁹	...	...	...	37.4	37.1	35.5	37.3	38.3	...	...
Bolivia ³ Bolivie ³	46.8	46.3	...	...	...	44.9	...	...	...	46.5
Brunei Darussalam ¹⁰ Brunei Darussalam ¹⁰	47.1	47.8	48.1	46.0	46.9	46.4	...	...	...	...
Canada ¹¹ Canada ¹¹	38.5	38.5	37.7	# 38.4	38.5	38.8	38.7	# 38.8	38.8	38.6
Chile ^{4,9} Chili ^{4,9}	...	...	43.3	43.2	43.6	43.1	44.3	44.1	44.0	43.2
Male Homme	...	...	45.6	45.3	45.7	45.2	46.0	45.9	6.1	45.1
Female Femme	...	...	37.1	37.7	37.8	38.2	37.3	38.8	37.8	37.9
Costa Rica ^{5,9,10} Costa Rica ^{5,9,10}	49.8	48.5	48.0	49.3	42.5	43.0	...	...	...	...
Cuba ^{11,12} Cuba ^{11,12}	42.4	42.2	41.8	43.1	43.2	43.4	# 43.4	43.0	# 42.6	...
Cyprus ^{5,8,13} Chypre ^{5,8,13}	41.0	41.0	41.0	41.0	41.0	41.0	41.0	41.0	41.0	41.0
Male Homme	43.0	42.0	42.0	42.0	42.0	42.0	42.0	42.0	42.0	43.0
Female Femme	40.0	40.0	40.0	40.0	40.0	40.0	40.0	40.0	40.0	40.0
Czechoslovakia ¹¹ Tchécoslovaquie ¹¹	43.5	43.3	43.1	43.1	43.0	43.1	43.1	43.1	43.0	40.7
Denmark Danemark	32.6	32.2	32.5	33.0	33.0	32.1	32.8	32.1	31.8	32.0
Ecuador Equateur	45.0	45.0	45.0	44.0	44.0	...	...	...	...	...
Finland Finlande	33.2	32.8	32.5	32.4	32.3	32.3	33.4	32.1	32.1	31.8
France ³ France ³	40.6	40.3	39.4	38.9	38.7	# 38.6	38.6	38.6	38.7	38.7
Germany† · Allemagne† F. R. Germany ¹ R. f. Allemagne ¹	41.6	41.1	40.7	40.5	41.0	40.7	40.4	40.1	40.0	39.9
Male Homme	42.2	41.6	41.1	40.8	41.4	41.1	40.8	40.5	40.4	40.2
Female Femme	40.0	39.4	39.1	39.1	39.7	39.5	39.2	39.0	38.9	38.7

Hours of work per week: manufacturing

Actual hours of work per worker (cont.)

Durée du travail par semaine : industries manufacturières

Heures de travail effectuées par ouvrier [suite]

Country or area Pays ou zone	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Guernsey ¹ Guernsey	45.8	46.9	47.7	47.6	50.4	48.7	47.3	43.9	47.1	42.4
Male	46.5	48.1	49.0	49.2	51.9	48.8	48.2	44.6	48.6	42.7
Female	42.2	41.1	41.4	40.7	42.0	41.2	42.5	39.7	39.2	38.6
Greece ¹ Grèce	40.7	39.5	38.6	# 38.5	38.2	# 39.3	39.1	39.2	41.1	41.1
Male	41.9	40.9	39.6	# 39.2	39.0	# 40.0	39.8	39.8	41.6	41.5
Female	39.1	37.7	37.2	# 37.7	37.2	# 38.3	38.4	38.5	40.5	40.7
Guam ¹ Guam	37.7	38.1	37.5	39.7	39.4	38.5	37.6	39.9	...	...
Guatemala ¹ Guatemala	45.1	46.1	45.7	46.5	48.7	48.6	...	...	...	...
Hong Kong ¹ Hong-kong	49.1	46.2	45.2	45.5	44.8	44.8	45.2	44.5	45.9	44.8
Male	...	...	...	...	...	47.7	48.1	47.3	48.6	47.0
Female	...	...	...	...	...	41.4	41.7	41.1	42.6	41.8
Hungary ¹ Hongrie	160.8	160.7	153.9	152.4	147.1	145.2	144.5	147.7	147.3	142.6
Ireland ¹ Irlande	41.1	41.6	40.5	40.8	41.1	# 41.1	41.4	41.1	41.9	41.7
Male	42.9	43.3	42.0	42.4	42.7	# 42.8	42.9	42.6	43.3	43.5
Female	36.9	37.7	37.0	37.1	37.2	# 37.6	38.1	38.2	39.1	38.3
Israel ¹ Israël	38.3	39.0	38.1	38.0	38.5	# 38.6	38.8	38.2	38.1	39.2
Male	39.2	40.1	39.0	38.9	39.3	# 39.7	40.0	39.4	39.3	40.5
Female	35.1	35.1	34.9	35.0	35.4	# 35.3	35.0	34.5	34.3	35.6
Italy ¹ Italie	7.7	7.8	7.7	7.7	7.8	...	...	...	...	...
Japan ¹ Japon	41.2	41.0	# 40.9	41.1	41.7	# 41.5	41.1	41.3	41.8	41.4
Male	42.4	42.1	# 42.1	42.3	43.0	# 42.9	42.4	42.6	43.4	43.0
Female	38.4	38.3	# 38.3	38.6	38.8	# 38.4	38.4	38.7	38.6	37.9
Kenya ¹ Kenya	...	...	...	...	43.0	42.0	43.0	41.0	43.0	42.0
Korea, Republic of ¹ Corée, République de	53.1	53.7	53.7	54.4	54.3	53.8	54.7	54.0	52.6	50.7
Male	52.8	53.4	53.6	53.9	54.0	53.5	54.4	54.0	52.8	52.9
Female	53.5	53.9	54.0	55.0	54.8	54.2	55.2	54.1	52.4	50.3
Luxembourg ¹ Luxembourg	40.0	41.2	41.2	41.0	39.7	40.2	40.5	40.8	41.2	40.3

41

Hours of work per week: manufacturing

Actual hours of work per worker (cont.)

Durée du travail par semaine : industries manufacturières

Heures de travail effectuées par ouvrier (suite)

Country or area Pays ou zone	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Male										
Homme	40.2	41.2	41.2	41.0	39.6	40.3	40.7	40.8	41.2	40.5
Female										
Femme	38.4	40.6	40.6	40.9	40.2	39.4	39.3	41.0	41.1	38.9
Mexico ³										
Mexique ³	47.7	47.6	46.5	46.8	47.3	47.7	47.3	47.1	...	...
Myanmar ^{15 16}										
Myanmar ^{15 16}	7.4	7.5	7.6	7.6	7.6	7.6	7.7	7.8	8.0	7.7
Netherlands ^{5 4}										
Pays-Bas ^{5 4}	41.0	40.8	40.8	40.7	40.5	40.4	40.2	40.0	39.9	39.9
Male										
Homme ¹³	41.1	40.9	40.8	40.8	40.5	40.5	40.3	40.1	40.0	40.0
Female										
Femme ¹³	40.2	40.1	40.1	40.1	39.8	39.8	39.5	39.5	39.3	39.3
Netherlands Antilles ^{1 4}										
Antilles néerlandaises ^{1 4}	40.0	...	42.6	43.5	...	42.2	43.4	...	...	...
Male										
Homme	...	...	...	43.5	...	42.1	43.4	...	...	...
Female										
Femme	...	...	...	...	...	39.8	43.3	...	...	...
New Zealand ^{1 12}										
Nouvelle-Zélande ^{1 12}	39.6	39.6	# 39.6	39.5	40.4	40.7	39.6	40.2	39.9	40.5
Male										
Homme	40.5	40.2	# 40.4	40.2	41.3	41.6	40.5	41.1	40.8	41.3
Female										
Femme	37.4	37.5	# 37.3	37.6	38.2	38.4	37.4	38.0	37.8	38.3
Norway Norvège										
Male										
Homme	31.0	30.8	30.7	30.6	31.0	30.6	30.6	29.7	29.8	...
Female										
Femme	25.8	25.3	24.9	24.9	25.3	25.2	24.8	24.1	24.4	...
Panama										
Panama	...	45.5	45.8	45.9	45.4	44.8	...	...	...	...
Peru ^{6 18}										
Pérou ^{6 18}	45.6	45.0	45.4	47.8	45.9	44.8	48.5	47.4	48.5	46.7
Philippines ⁹										
Philippines ⁹	46.2	47.6	48.9	# 46.5	45.9	47.0	48.1	49.4	...	...
Poland ^{2 14}										
Pologne ^{2 14}	159.0	147.0	149.0	152.0	152.0	153.0	151.0	151.0	154.0	150.0
Portugal										
Portugal	39.0	38.8	38.8	38.6	* 38.6	38.8	38.9	39.1	...	...
Puerto Rico ¹										
Porto Rico ¹	38.0	38.2	37.5	38.7	38.7	38.5	39.0	39.0	39.4	39.8
San Marino ³										
Saint-Marin ³	36.6	35.1	35.7	...	34.9	34.5	34.4	31.7	...	36.7
Singapore ⁵										
Singapour ⁵	48.6	48.7	48.3	48.9	48.6	47.0	48.5	49.2	...	...
South Africa ¹²										
Afrique du Sud ¹²	48.4	48.2	47.1	46.2	46.5	46.2	46.6	46.8	47.3	46.9
Spain ²⁰										
Espagne ²⁰	...	37.8	38.0	37.4	36.5	36.3	36.7	35.4	35.8	36.8
Male										
Homme	...	38.3	38.5	38.1	36.9	36.8	37.0	35.8	36.3	37.3

41

Hours of work per week: manufacturing

Actual hours of work per worker [cont.]

Durée du travail par semaine: industries manufacturières

Heures de travail effectuées par ouvrier [suite]

Country or area Pays ou zone	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Female Femme	...	36.2	36.0	35.4	35.0	34.5	35.4	33.9	34.3	35.0
Sri Lanka ¹ Sri Lanka ¹¹	45.3	52.6	46.5	48.9	48.6	47.4	47.3	51.5	49.1	50.5
Sweden ¹² Suède ¹²	37.6 ¹	37.4	37.6	37.7	38.1	38.3	# 38.3	38.4	38.5	38.5
Male Homme	39.2 ¹	39.1	39.3	39.4	39.8	39.9	# 39.9	39.9	40.0	40.0
Female Femme	32.8 ¹	32.3	32.5	32.6	33.2	33.5	# 33.5	34.1	34.4	34.4
Switzerland ¹³ Suisse ¹³	...	43.8	43.7	43.2	43.1	43.0	42.6	42.4	41.9	41.7
Thailand ¹⁴ Thaïlande ¹⁴	...	...	...	189.5	195.5	194.8	195.4	...	...	...
USSR ¹⁵ URSS ¹⁵	40.5	40.7	40.4	40.3	...	...	...	...	...	...
Byelorussian SSR ¹⁶ RSS de Byélorussie ¹⁶	(40.4)	(40.4)	(40.4)	(40.4)	...	...	...	...	...	...
United Kingdom ¹⁷ Royaume-Uni ¹⁷	...	...	41.0	# 41.5	41.7	41.8	41.6	42.2	42.4	42.2
Male Homme	41.9	42.0	42.0	# 42.5	42.8	43.0	42.7	43.5	43.6	43.4
Female Femme	37.3	37.5	37.8	# 38.1	38.1	38.1	38.1	38.4	38.7	38.6
United States ¹⁸ Etats-Unis ¹⁸	39.7	39.8	38.9	40.1	40.7	40.5	40.7	41.0	41.1	41.0
United States Virgin Is. ¹⁹ Iles Vierges américaines ¹⁹	41.1	42.3	42.3	41.4	42.7	41.7	41.9	42.2	42.6	41.7
Uruguay ²⁰ Uruguay ²⁰	...	...	...	...	...	...	43.4	44.2	...	...
Venezuela ²¹ Venezuela ²¹	44.1	46.7	...	43.3	40.1	40.6	40.7	40.2	...	...
Yugoslavia ²² Yougoslavie ²²	...	184.0	...	181.0	...	184.0	...	...	184.0	...

Source:

International Labour Office (Geneva).

Source:

Bureau international du Travail (Genève).

* All data shown which pertain to Germany prior to 3 October 1990 are indicated separately for the Federal Republic of Germany and the former German Democratic Republic based on their respective territories at the time indicated. For detailed explanatory notes on data pertaining to Germany, see Annex I - Country Nomenclature.

1 Average of less than twelve months (Canada: 1983).

2 Per month.

3 Including salaried employees.

4 Labour force sample survey.

5 Including mining and quarrying.

6 One month of each year.

7 Excluding primary iron and steel.

8 Hours paid for.

9 Persons aged 15 years and over.

10 Civilian labour force employed.

11 State industry.

12 Prior to 1986, including water. Prior to 1988, including fishing, mining, and quarrying and gas.

13 Adults only.

† Toutes les données se rapportant à l'Allemagne avant le 3 octobre 1990 figurent dans deux rubriques séparées basées sur les territoires respectifs de la République fédérale d'Allemagne et l'ancienne République démocratique allemande selon la période indiquée. Pour les notes explicatives en détail sur les données concernant l'Allemagne, voir Annexe I - Nomenclature des pays.

1 Moyenne de moins de douze mois (Canada: 1983).

2 Par mois.

3 Y compris les employés salariés.

4 L'enquête par sondage de la main-d'oeuvre

5 Y compris les industries extractives.

6 Un mois de chaque année.

7 Non compris la sidérurgie.

8 Heures rémunérées.

9 Personnes âgées de 15 ans et plus.

10 Main-d'oeuvre civile occupée.

11 Industrie d'Etat.

12 Avant 1986, y compris l'eau. Avant 1988, y compris la pêche, les industries extractives et le gaz.

13 Adultes seulement.

41

Hours of work per week: manufacturing
Actual hours of work per worker [cont.]

Durée du travail par semaine : industries manufacturières
Heures de travail effectuées par ouvrier [suite]

- 14 Socialised sector.
- 15 Per day.
- 16 Workers engaged for less than 30 days (excluding casual workers).
- 17 Prior to 1982, full-time employees and half of part-time employees.
- 18 Lima only.
- 19 Excluding Transkei, Bophuthatswana, Venda, Ciskei.
- 20 Persons aged 16 years and over.
- 21 Accident insurance statistics, excl. overtime.
- 22 Workers on adult rates of pay.
- 23 Including quarrying.

- 14 Secteur socialisé.
- 15 Par jour.
- 16 Travailleurs engagés pour moins de 30 jours (non compris les travailleurs occasionnels).
- 17 Avant 1982, salariés à plein temps, y compris la moitié des salariés à temps partiel.
- 18 Lima seulement.
- 19 Non compris Transkei, Bophuthatswana, Venda, Ciskei.
- 20 Personnes âgées de 16 ans et plus.
- 21 Statistiques d'assurance-accidents, non compris les heures supplémentaires.
- 22 Travailleurs rémunérés sur la base de taux de salaire pour adultes.
- 23 Y compris les carrières.

Technical notes, tables 39-41

Detailed data on labour force and related topics are published in the *Year Book of Labour Statistics*. [7] The series shown in the *Statistical Yearbook* give an overall picture of the availability and disposition of labour resources and, in conjunction with other macro-economic indicators, can be useful for an overall assessment of economic performance. The *Year Book of Labour Statistics* provides a comprehensive description of the methodology underlying the labour series. Brief definitions of the major categories of labour statistics are given below.

"Employment" is defined to include persons above a specified age who, during a specified period of time, were in one of the following categories:

(a) "Paid employment", comprising persons who perform some work for pay or profit during the reference period or persons with a job but not at work due to temporary absence, such as vacation, strike, education leave;

(b) "Self-employment", comprising employers, non-account workers, members of producers' cooperatives, persons engaged in production of goods and services for own consumption, unpaid family workers and apprentices;

(c) Members of the armed forces. Students, homemakers and others mainly engaged in non-economic activities during the reference period who, at the same time, were in paid employment or self-employment are considered as employed on the same basis as other categories.

"Unemployment" is defined to include persons above a certain age and who during a specified period of time were in one of the following categories:

(a) "Without work", i.e. were not in paid employment or self-employment;

(b) "Currently available for work", i.e. were available for paid employment or self-employment during the reference period; or

(c) "Seeking work", i.e. had taken specific steps in a specified period to find paid employment or self-employment.

The following categories of persons are not considered to be unemployed:

(a) Persons intending to establish their own business or farm, but who had not yet arranged to do so and who were not seeking work for pay or profit;

(b) Former unpaid family workers not at work and not seeking work for pay or profit.

For various reasons, national definitions of employment and unemployment often differ from the recommended international standard definitions and thereby limit international comparability. Intercountry comparisons are also complicated by a variety of types of data collection systems used to obtain information on employed and unemployed persons.

Table 39 presents absolute figures on the distribution of employed persons by major divisions of economic activity. Data are arranged as far as possible according to the major divisions of economic activity of the *International Standard Industrial Classification of All Economic Activities*. [44] The column for total employment includes economic activities not adequately defined.

Table 40: Figures are presented in absolute numbers and in percentages. Data are normally annual averages of monthly, quarterly or semi-annual data.

The series generally represent the total number of persons wholly unemployed or temporarily laid off. Percentage figures, where given, are calculated by comparing the number of unemployed to the total members of that group of the labour force on which the unemployment data are based.

Table 41 shows hours of work in manufacturing industries as a whole. Where in a few cases other industries are also included, this is indicated in a footnote. The series generally represent the average hours actually worked by wage-earners. In a few cases, the data relate to hours paid for rather than actual hours worked. "Hours paid for" comprises in addition to hours actually worked paid annual vacation, public holidays, sick leave and the like. The time of year to which the figures refer is not the same for all countries.

2134
194
1973

Department of Economic and
Social Development
Statistical Office

Département du développement
économique et social
Bureau de statistique

Statistical Yearbook

Thirty-seventh issue

1988/89

Data available as of
1 September 1991

Annuaire statistique

Trente-septième édition

Données disponibles
au 1^{er} septembre 1991



United Nations / Nations Unies New York, 1992



BD

Minimum Wage

Bill Dickens
Darryl Wills &
- see what group
has written

- compare ours to other countries - OECD
- nominal v. virtual minimum wage
the real cost per hour / emp.
(health care etc).
- effects - arguments for and against
(raising it / indexing it. Bringing it
up to comparable 60's / 70's levels)

Darryl Wills - sources

Get:

2.11.93

Hearing of the Jt. Econ. Cte.

Chaired by Rep. Oberly

2359 KIOB

Search:

minimum pre/1 wage
~~base~~ w/ 15 ~~raise~~ rais!
w/ 15 ~~index~~ index
and compare!

Lebrun Wold (Yorio)
Jill - Econ

UDZOCWA

Authors
Dickens
Blinder
Tyson
Wills
Katz
Reich
Stiglitz

all
Johns

Conversions

• Belgium

$$\frac{35.5}{.879} = 40.38$$

$$\frac{36.9}{40.38} = \$.91$$

• France

$$\frac{5.88}{.879} = 6.69$$

$$\frac{31.9}{6.69} = \$ 4.77$$

✓ • Greece

$$\frac{123}{.879} = 139.93$$

$$\frac{2452}{139.93}$$

• DENMARK

$$\frac{8.72}{.879} = 9.92$$

$$\frac{59}{9.92} = \$ 5.95$$

Formula

Private Final
Consumption
Expenditure

$$\div .879$$

~~Take that~~

Divide
the min. wage
by the
above #

✓ marked
this was
given in
per day
or month.
(
Not hour