Making Work Pay

The Impact of the 1996-97 Minimum Wage Increase

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EXECUTIVE SUMMARY

Federal legislation increased the minimum wage from $4.25 to $4.75 on October 1, 1996 and to $5.15 on September 1, 1997. This study examines the impact of these increases on the employment opportunities, wages, and incomes of low-wage workers and their households.

The principal findings are that:

- The 1996 and 1997 minimum wage increases raised the wages of almost 10 million workers. About 71% of these workers were adults and 58% were women. Just under half (46%) worked full time and another third worked 20 to 34 hours per week.
- The average minimum wage worker is responsible for providing more than half (54%) of his or her family's weekly earnings.
- The two-stage increase disproportionately benefited low-income working households. Although households in the bottom 20% of the income distribution (whose average income is $15,728) receive only 5% of total family income, they received 35% of the benefits from the minimum wage increase.
- Four different tests of the two increases' employment impact — applied to a large number of demographic groups whose wages are sensitive to the minimum wage — fail to find any systematic, significant job loss associated with the 1996-97 increases. Not only are the estimated employment effects generally economically small and statistically insignificant, they are also almost as likely to be positive as negative.

The 1996-97 increase in the minimum wage has proven to be an effective tool for raising the earnings of low-wage workers without lowering their employment opportunities.

These empirical results, particularly those showing strong wage gains and no negative impact on job opportunities, are at odds with traditional economic theory, which argues that a rise in the minimum wage must cost jobs. Over the last decade, however, new economic models designed to reconsider low-wage labor markets may help explain the increasingly weak link between the minimum wage and low-wage employment opportunities. These more recent models, often referred to as "dynamic monopsony" models, incorporate the costs of recruiting, training, and motivating low-wage workers, variables neglected by more traditional models. Not only do these new models more realistically reflect the character of the low-wage labor market, but they also offer a better explanation of our central finding: the 1996-97 increase in the minimum wage has proven to be an effective tool for raising the earnings of low-wage workers without lowering their employment opportunities.

INTRODUCTION

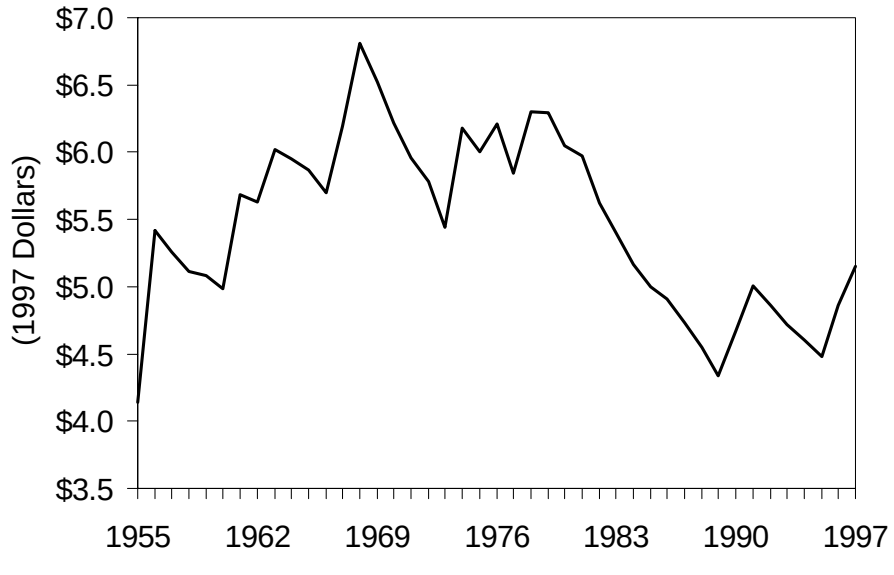
One of the most troubling economic phenomena of the past two decades has been the decline in the inflation-adjusted wages of low-wage workers. The real hourly wage of workers at the 10th percentile of the wage scale clearly reflects this trend — their wages fell 15% between 1979 and 1997.[1] Since many low-wage workers are breadwinners or major contributors to their households' incomes, this decline has played an important role in lowering the living standards of those with modest incomes.[2]

The real hourly wage of workers at the 10th percentile of the wage scale fell 15% between 1979 and 1997.

Over most of this same 1979-97 period, the inflation-adjusted value of the minimum wage fell precipitously (see **Figure 1**). The connection between the declining real value of the minimum wage and falling incomes among low-income households — particularly during a period when overall economic growth was positive — led policy makers to consider lifting the wage floor.[3] Thus, after a nine-year period in the 1980s in which the minimum wage was ignored at the federal level, Congress passed two separate $0.90 increases in the minimum wage. The first took place over the period 1990-91 and raised the minimum from $3.35 to $4.25; the second increase — from $4.25 to $5.15 — was implemented in 1996-97. This report focuses on the impact of this most recent increase.[4]

FIGURE 1

Real Minimum Wage in 1997 Dollars, 1955-97



Of course, raising the minimum wage always generates heated debate. Opponents of the policy generally claim that by making low-wage workers more expensive to their employers, increasing the minimum wage is more likely to cost workers their jobs than to raise their earnings. Supporters of the policy, on the other hand, argue that the minimum wage has historically been key to raising the earnings of low-wage workers and claim that little real-world evidence exists that the minimum wage lowers employment opportunities.

The first two sections of this study focus on the beneficiaries of the recent minimum wage increase. Our main findings from these sections are that:

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- Almost 10 million low-wage workers benefited from the increase.
- Most of these workers (71%) were adults and 58% were female.
- Close to half (46%) of the beneficiaries of the increase worked full time and another third worked 20-34 hours per week.
- The average minimum wage worker brings home more than half (54%) of his or her family's weekly earnings.
- The benefits of the increase disproportionately help those working households at the bottom of the income scale. Although households in the bottom 20% (whose average income was $15,728 in 1996) received only 5% of total national income, 35% of the benefits from the minimum wage increase went to these workers. In this regard, the increase had the intended effect of raising the earnings and incomes of low-wage workers and their households.

The employment effects of minimum wage increases do, of course, remain an important area of focus in evaluating the policy. To this end, economists have developed a wide battery of tests to measure the impact of such increases. While many of these tests have been applied to earlier increases in the minimum wage, there has been little analysis of the 1996-97 increase. In the third section of this study, we apply these same tests to the most recent increase.

Given that the second step of the increase was implemented on September 1, 1997, it is reasonable to ask whether enough time has elapsed to reliably test for employment effects. All told, we have data for 17 months after the first increase, and six months after the second. We compare employment changes over these months to at least another 18 months before the increases. If employers truly adjust their workforce in response to minimum wage increases, we can think of no reason why they would wait longer than a year and five months to implement at least some

changes. Thus, while our tests should generally be considered an examination of "short-run effects," the short run can be quite long, especially relative to the typical job tenure in this high-turnover market. In addition, we have structured our employment tests quite flexibly, testing each step of the increase separately as well as over the full increase.

Our main findings regarding the increase's disemployment effects are that:

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- None of our various tests show systematic job loss resulting from the 1996-97 minimum wage increase.
- The effect on employment is generally economically small and statistically insignificant; any impact is almost as likely to be positive as negative, varying unpredictably across demographic groups.
- One of the tests of the employment effects, initially proposed by economists Deere, Murphy, and Welch (1995), shows the net employment of affected groups rising after the minimum wage increase.
- Our update of the traditional method for estimating the minimum wage's employment effects (time-series models) reveals a statistically insignificant result that is less than half the magnitude of even the lowest of earlier estimates.

After presenting these results, this study concludes with a discussion of an alternative model of the economy within which our employment results are better understood. A Data Appendix at the end of the study provides details of our methods and data sources.

SECTION 1 — WHO BENEFITS FROM THE MINIMUM WAGE INCREASES?

Historically, debates over whether to increase the minimum wage have focused almost solely on the possibility of job loss and ignored the potential benefits. Any reasonable policy analysis needs to examine both costs and benefits, and in this section we focus on the beneficiaries of the most recent minimum wage increase.

When focusing on the beneficiaries of minimum wage increases, opponents of the policy have suggested that most minimum wage workers are teenagers living in high-income households. Such is not the case. The data presented below contradict this characterization, showing that most workers affected by minimum wage increases are adults, and most reside in lower-income households. At the same time, the results show that the policy is far from perfectly targeted. For example, about one-quarter of the benefits from the increase went to working households in the top 40% of the income scale. Even so, taken as a whole, the results from this section suggest that minimum wage increases are still accomplishing the goal of raising the earnings of low-wage workers in lower-income households.

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Table 1 examines the characteristics of workers in the affected wage range in the year before the increase (see the **Data Appendix** for a description of data sources and methods). Column 1 shows that the increase reached close to 10 million workers, or 8.9% of the labor force. Most workers in the affected range were adults (71.4%, or 100% minus the teenager share of 28.6%), and 58.2% were female. Looking at the age distribution by gender reveals that 43.2% of affected workers were adult females and 28.2% were adult men. Thus, according to these variables, the single largest group affected by the increase is adult women. While 46% of affected workers work full time in a typical week (at least 35 hours per week), another 33.3% work between 20 and 34 weekly hours.

Comparing the workers directly affected by the new minimum wage (Column 1) to all workers (the last column) shows the extent to which affected workers are concentrated in certain categories. For example, female and minority workers are over-represented in the affected range, while males and white workers are under-represented. Similarly, affected workers are much more likely to be in retail trade industries (where they tend to be service providers such as clerks or waiters) than are higher-wage workers. Teenagers, who represent a minority of minimum wage workers, are, because of their typically low-wage rates, over-represented in the affected range. Finally, minimum wage workers are disproportionately non-union, reflecting the union wage premium to higher-wage workers.

An addendum to Table 1 shows the average and median share of weekly

TABLE 1
Characteristics of Minimum Wage and Other Workers, October 1995-September 1996

Characteristic	Workers Directly Affected by New Minimum ($4.25-$5.14)	Other Low-Wage Workers ($5.15-$6.14)	Higher-Wage Workers ($6.15+)	All Workers
Average Wage	$4.73	$5.72	$14.64	$12.73
Employment	9,886,158	9,610,926	89,079,931	110,999,085
Share of Total	8.9%	8.7%	80.3%	100.0%
Demographics				
Male	41.8%	41.9%	54.9%	52.3%
16-19	13.7	8.1	1.0	2.9
20+	28.2	33.8	53.9	49.4
Female	58.2	58.1	45.1	47.7
16-19	14.9	7.9	0.7	2.8
20+	43.2	50.2	44.4	44.9
White	62.8	67.7	77.9	75.4
Male	24.6	26.2	42.8	39.4
Female	38.2	41.5	35.1	36.0
Black	16.1	13.8	10.4	11.3
Male	6.4	5.5	5.1	5.3
Female	9.8	8.3	5.3	6.0
Hispanic	17.5	14.8	7.9	9.5
Male	9.3	8.6	4.9	5.7
Female	8.2	6.2	3.0	3.8
Teens(16-19)	28.6%	16.0%	1.7%	5.6%
Work Hours				
Full Time (35+)	46.0%	62.7%	87.7%	81.1%
Part Time				
20-34 hours	33.3%	25.4%	9.0%	13.0%
1-19 hours	20.7	11.9	3.3	5.9
Industry				
Manufacturing	8.8%	12.7%	19.7%	17.8%
Retail Trade	42.6	35.8	12.2	17.3
Union*				
Union	4.4%	6.3%	19.1%	16.4%
Nonunion	95.6	93.7	80.9	83.6

Addendum: The Share of Weekly Earnings Contributed by Minimum Wage Workers, 1997**

	Average	Median
All Families With an Affected Worker	54%	41%
Excluding One-Person Families	44	27

* Includes both union members and nonmembers covered by union contracts.
** See Data Appendix for data source and methods.

earnings that affected workers contribute to their households. Imagine, for example, a family with two workers, one of whom is in the affected range and one of whom earns an hourly wage above that range.[5] If the minimum wage worker earns $100 per week and the higher-wage earner makes $300 per week, the minimum wage earner's contribution is 25% (100/400). The addendum shows that the average worker in the affected range brings home more than half (54%) of the household's weekly earnings; the median share is 41% (the divergence between the average and the median is due to the fact that this distribution is skewed toward those with share values close to one). Since one-person households in these calculations obviously contribute all of their earnings, we also examined the share of weekly earnings contributed by minimum wage workers, excluding one-person households. The average share for these households comes to 44%, while the median share is 27%. Based on these findings, the earnings of minimum wage workers are quite important to their households' well being.

Most of the benefits of the increase in the minimum wage go to lower-income working households.

Nevertheless, a stated policy goal of minimum wage increases is to raise not just the *earnings* of low-wage workers, but also the *incomes* of low-income households. In this regard, it makes sense to examine where minimum wage workers lie in the household income distribution. **Table 2** addresses this issue.

We first focus on working households (including those with just one person), headed by a prime-age (25-54) earner (see the Data Appendix for a discussion of the methods used in these figures). These households serve as a useful benchmark for the targeting question because they exclude both households with no earners, who clearly do not benefit from the policy, and households headed by younger or older persons, who tend to be less connected to the labor force. (The Data Appendix presents the same statistics for all households, regardless of the age of the household head.)

Table 2 reveals that most, though not all, of the benefits of the increase in the minimum wage go to lower-income working households. Among working households headed by a prime-age individual, 35.3% of the benefits of the increase go to households in the bottom fifth of the income distribution. These households, with an average income of $15,728 in 1996, received 5.4% of the total national income that year. Shifting to the top of the income distribution, note that the smallest share of the benefits from the increase, 12.2%, goes to households in the top fifth, whose total national income share was 45.3%. Combining the bottom two groups in the table shows that 58.1% of the benefits from the increase go to working, prime-age households in the bottom 40%, who receive 16.4% of total national income.

Table 2 also shows how these figures change when the total includes prime-age households with no workers, and therefore no beneficiaries of the wage in-

TABLE 2
Distribution of Minimum Wage Gains and Income Shares by Fifth for Various Household Types

Income Quintile	Share of Gain From Increase	Share of Income	Average Income
Prime-Age Working Households,* 1997			
1 (Bottom)	35.3%	5.4%	$15,728
2	22.8	11.0	32,547
3	15.2	15.9	47,699
4	14.5	22.3	66,104
5 (Top)	12.2	45.3	134,128
All Prime-Age Households (Including Nonworking), 1997			
1 (Bottom)	28.0%	3.8%	$10,518
2	22.8	9.8	26,965
3	20.2	15.6	42,848
4	15.8	22.7	62,502
5 (Top)	13.3	48.0	131,991

* Prime-age households are headed by a person age 25-54. One person households are included. The top panel excludes households with no earners. See Data Appendix for data source and methods.

crease. Their inclusion leads to a somewhat more uniform distribution of the benefits from the increase among the bottom 60%, although the bottom fifth continues to receive the largest share of the benefits (28.0%). Note also that the inclusion of nonworking households lowers both the average income and income share going to the bottom group relative to the working household distribution shown above. Here again, at least half of the benefits from raising the minimum wage go to households in the bottom 40% of the distribution.

Table 2 shows that, while the minimum wage is well targeted toward prime-age households, there is some "leakage." In both of the cases examined above, more than one-quarter of the benefits of the increase go to households in the top two-fifths, whose average incomes put them well above the low-income range. Nevertheless, the majority of the benefits go to those who need them: low-wage workers in low-income households.

Table 3, which shows the number and percentage of workers affected by both stages of the increase by state and region, makes a final important point about the increases' beneficiaries. Nationally, close to 10 million workers, or 8.9% of the workforce, received a pay increase due to the $0.90 increase in the minimum wage.

TABLE 3
Workers Affected by the 1996-97 Minimum Wage Increase by State

State	Share	Number
Northeast	6.8%	1,451,104
New England	5.4	314,970
Maine	7.9	40,631
New Hampshire	5.1	26,369
Vermont	7.0	17,854
Massachusetts	5.0	133,840
Rhode Island	7.3	30,747
Connecticut	4.6	65,527
Mid-Atlantic	7.3%	1,136,135
New York	7.6	542,091
New Jersey	4.6	156,725
Pennsylvania	8.9	437,319
Midwest	7.9%	2,171,201
East North Central	7.7	1,474,021
Ohio	8.7	419,729
Indiana	9.3	247,721
Illinois	6.8	354,033
Michigan	7.2	289,417
Wisconsin	6.6	163,120
West North Central	8.5%	697,180
Minnesota	6.0	127,595
Iowa	9.0	117,129
Missouri	8.1	194,198
North Dakota	13.6	36,307
South Dakota	10.2	31,095
Nebraska	8.8	63,554
Kansas	11.8	127,303
South	10.8%	4,191,171
South Atlantic	9.2	1,863,740
Delaware	6.2	20,189
Maryland	5.7	134,276
District of Columbia	5.5	12,601
Virgina	8.4	250,109
West Virginia	14.8	99,772
North Carolina	9.2	297,471
South Carolina	11.3	176,471
Georgia	9.4	294,580
Florida	10.1	578,272
East South Central	11.8%	779,788
Kentucky	10.4	160,092
Tennessee	8.7	196,412
Alabama	14.0	246,162
Mississippi	16.5	177,123
West South Central	13.0%	1,547,643
Arkansas	14.8	155,325
Louisiana	15.3	250,269
Oklahoma	14.6	189,111
Texas	12.0	952,938

(cont.)

TABLE 3 *(cont.)*
Workers Affected by the 1996-97 Minimum Wage Increase by State

State	Share	Number
West	8.8%	2,072,682
Mountain	8.8	594,236
Montana	11.9	38,865
Idaho	10.7	51,078
Wyoming	12.5	25,555
Colorado	6.1	106,209
New Mexico	12.3	76,434
Arizona	9.9	180,343
Utah	8.6	71,857
Nevada	6.2	43,894
Pacific	8.8%	1,478,446
Washington	5.4	126,229
Oregon	7.3	98,980
California	10.0	1,230,381
Alaska	3.6	8,590
Hawaii	3.0	14,266
U.S.	8.9%	9,886,158

Note: Affected workers are those who earned between $4.25 and $5.14 in the year prior to the minimum wage increase.

Source: EPI analysis of CPS ORG data.

In every region except the Northeast, a number of states had more than 10% of their workforce affected by the full sweep.

In terms of percent of the workforce affected, the increase has the largest impact in the South, where hourly wages tend to be lower than in other parts of the nation. In Mississippi, for example, 16.5% of the workforce was affected by the full increase. States in the Western region also had relatively high shares of workers in the affected range — Wyoming and New Mexico both had shares above 12%. While the higher wage rates in the Northeast meant that fewer workers were affected in that part of the country, the increase still raised the wages of close to 1.5 million workers in that region.

SECTION 2 — HOW DO MINIMUM WAGE INCREASES AFFECT WAGE TRENDS?

As we noted in the introduction, the long fall in the minimum wage over the 1980s (see Figure 1) partly accounts for the decline in the inflation-adjusted hourly pay of low-wage workers over that period. If this is true and all else were equal, then increases in the minimum wage should be identifiable in the wage trends of low-wage workers throughout the 1990s, when the two increases were legislated (the 1990-91 increase raised the minimum from $3.35 to $4.25; the most recent increase took the minimum to $5.15 in 1996-97). This, in fact, seems to hold true, with the timing of the increases corresponding quite closely to the real wage increases of low-wage workers.

The hourly pay of low-wage female workers is closely tied to the minimum wage.

Figures 2A-2C show low-wage trends for all low-wage workers combined as well as those specifically for male and female low-wage workers. The trends in these figures are for the 10th and 20th percentile wage (i.e., the worker at the 10th percentile earns more than 10% of the workforce but less than the remaining 90%), adjusted for inflation and set equal to 100 in 1989. Both Figures 2A (All) and 2C (Females) show evidence of a minimum wage effect, particularly at the 10th percentile. The pattern for all workers most clearly follows the increases in the minimum wage, with increases for both the 10th and 20th percentiles in 1990-91 and 1996-97 (in this last period, only the 20th percentile increased in both years).

For females at the 10th percentile, real wages increased in 1990-92 (the minimum wage was increased in April of 1990 and 1991), fell through 1996, and increased again in 1997. The indexed graphs in these figures do not reveal the actual wage levels, but it is notable that the 1997 10th percentile hourly wage for women was $5.15, exactly the same as the minimum wage at the end of that year.[6] Clearly, the hourly pay of low-wage female workers is closely tied to the minimum wage.

The case for males is less clear cut, as might be expected since their wage levels place them above the minimum, even at the 10th percentile (the 10th percentile male wage in 1997 was $5.92). Neither the 10th nor 20th percentile male hourly wage shows any reaction to the 1990-91 increase, and the 10th percentile male wage actually began to climb in 1994 before the most recent set of increases went into effect. But the real wages for men at the 20th percentile do increase in 1997; this wage — $7.36 in 1997 — is, however, significantly higher than the minimum, and we are hesitant to attribute its rise directly to the minimum wage increase.

FIGURE 2A

Real Hourly Wage Trends,
All Low-Wage Workers, 1989-97

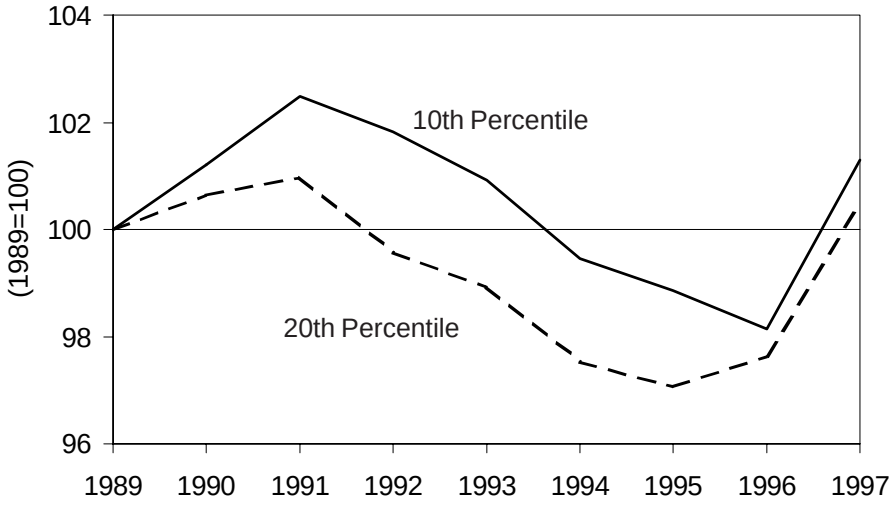


FIGURE 2B

Real Hourly Wage Trends,
Male Low-Wage Workers, 1989-97

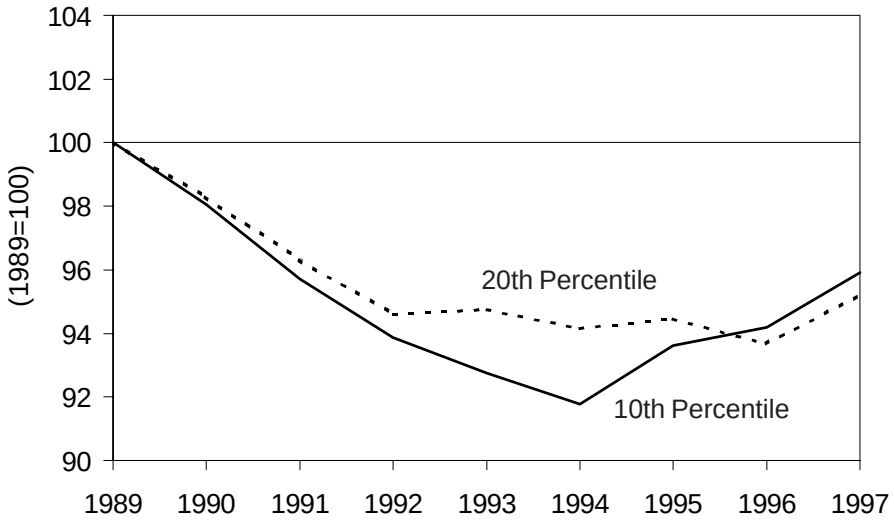
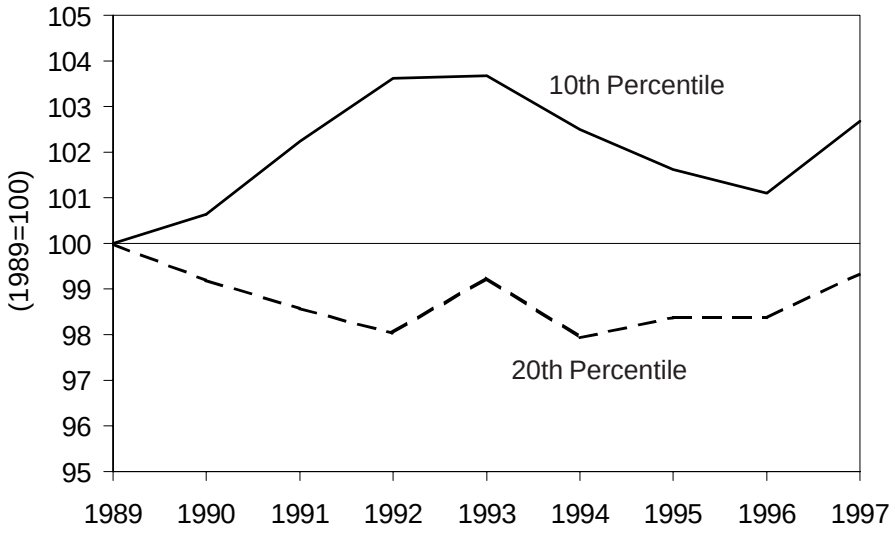


FIGURE 2C

Real Hourly Wage Trends,
Female Low-Wage Workers, 1989-97



SECTION 3 — DID THE MOST RECENT INCREASE IN THE MINIMUM WAGE LEAD TO JOB LOSSES FOR LOW-WAGE WORKERS?

In this section, we examine the results from four tests performed to determine the impact of recent minimum wage increases on the employment levels of low-wage workers. The first test — difference-in-differences — simply examines the changes in national employment rates of various types of low-wage workers, controlling for various factors, like seasonality, overall economic growth, and differences between states. The second test — introduced by economists Deere, Murphy, and Welch (1995) — uses state-level data to examine changes in employment rates for a more limited group of low-wage workers in response to changes in the minimum wage. The third test — proposed by economist David Card (1992) — also exploits state variation in the impact of minimum wage changes, but includes a measure of how deeply the new minimum wage "bites" or cuts into the state wage scale. Card's model was developed around the hypothesis that, since wages vary widely across states, any impact of the minimum wage on jobs should be most obvious in states with low wages. The final test we perform in this study updates the time-series analysis that has traditionally been used to measure the impact of minimum wages on teenage employment rates.

Employment rates are commonly used in the analysis of minimum wage increases because they speak directly to the question of whether the increase led to job loss.

Prior to presenting these results, however, there are a number of characteristics common to each of the tests that need to be elucidated. First, the dependent variable in all the tests is the employment rates (employment divided by population) of various types of low-wage workers. Employment rates are commonly used in the analysis of minimum wage increases because they speak directly to the question of whether the increase led to job loss. Unemployment rates are less reliable in this regard because they can rise in response to an increase in voluntary job seekers entering the labor force, who are initially unemployed as they begin their job search. If this influx causes the labor force to grow more quickly than the growth in job opportunities needed to absorb the new workers, unemployment rates will rise faster than employment rates even though no jobs were lost.

Second, like the majority of the economics literature on the minimum wage, our employment analysis looks at two groups of workers: teenagers and those adults between the ages 20 and 54 with less than a high school education. Opponents of minimum wage increases argue that the policy is most damaging to young persons, such as teenagers, just entering the labor force, as well as to other workers whose low skill levels tend to place them in the low-wage segment of the labor market. As

shown in **Appendix Tables 1A-1C**, both groups have relatively large shares (though not a majority) of workers in wage ranges affected by the increase. In this regard, these are the groups of workers most appropriate for tests of the employment hypotheses.

Test 1: Difference-in-Differences

One of the simplest means of testing the impact of the minimum wage increases is to use the "difference-in-differences," or DD test. The rationale behind DD is that if the minimum wage increase led to job losses for low-wage workers, then we would expect such workers' employment rates to fall after an increase.[7] However, simply comparing employment rates before and after is insufficient; we must also account for various factors that can potentially confound the comparison. Teenage employment rates, for example, are highly seasonal, and consequently these seasonal effects must be extracted. The overall economy may also be growing when the increase goes into effect. If, for example, the overall economy grew more rapidly in the year in which the minimum wage rose than in the comparison year (and this was the case during the last increase), then the raw difference-in-differences estimate might be biased against finding job losses, since economic growth would be boosting employment despite the minimum wage.[8] A final potential bias can be found in the lack of uniformity in economic growth between states. If low-wage employment is not evenly distributed across the states, then the DD estimate might confuse regional variations in economic growth with positive or negative employment effects of the minimum wage. As explained in the Data Appendix, each of these factors is accounted for in the following results.

Simply comparing employment rates before and after is insufficient; we must also account for various factors that can potentially confound the comparison.

Table 4 shows the results from a series of DD tests for teenagers and less-educated adults by gender, with each panel representing the test results over a different time period, marked T1-T4 on the table. We test three time periods: one for the first increase in the minimum wage (October 1, 1996); one for the second increase (September 1, 1997); and one over the full period covering both increases (see the table note for a further description).

The first panel in Table 4 examines the first increase. The column headed "T3" in this panel shows the share of persons in each group employed during the six-month period immediately before the October 1 minimum wage increase. Among teenagers, the employment rate was 46.6% in April 1996-September 1996. The next column, headed "T4," shows the employment share for the six-month period immediately after the increase went into effect (October 1996-March 1997). For teenagers, the employment rate in this most recent period fell to 40.8%, a drop of 5.9 percentage points when compared to the preceding six months. A simplistic

TABLE 4
Difference-in-Differences: The Employment Impact of the 1996-97 Minimum Wage Increase on Teenagers and Adults (Age 20-54) with Less Than a High School Education

							Difference-in-Differences				
	T1	T2	Difference	T3	T4	Difference	Raw	Growth Adjusted	State & Growth Adjusted	Std Err	T-Stat
First Increase*											
All Teenagers	47.6%	40.7%	-6.9	46.6%	40.8%	-5.9	1.0	0.5	0.4	0.9	0.46
Males	48.8	40.3	-8.4	47.5	39.8	-7.7	0.7	-0.2	-0.3	1.3	-0.23
Females	46.3	41.0	-5.3	45.7	41.7	-4.0	1.4	1.4	1.2	1.3	0.92
All LTHS Adults	57.7	57.6	-0.1	59.3	59.6	0.2	0.3	-0.1	-0.0	0.9	-0.03
Males	71.9	71.2	-0.7	74.2	73.3	-0.9	-0.2	-1.2	-1.1	1.1	-0.96
Females	43.0	43.6	0.7	43.6	44.9	1.3	0.6	0.6	0.7	1.3	0.57
Second Increase*											
All Teenagers	46.3%	41.1%	-5.2	46.1%	41.9%	-4.2	1.0	1.4	1.4	0.9	1.48
Males	47.1	40.4	-6.8	46.3	41.6	-4.7	2.0	2.2	2.3	1.3	1.74
Females	45.4	41.9	-3.5	46.0	42.3	-3.6	-0.1	0.3	0.2	1.3	0.17
All LTHS Adults	58.8	59.7	0.9	61.7	61.9	0.2	-0.7	-0.3	-0.3	0.9	-0.36
Males	73.7	73.5	-0.2	75.3	74.5	-0.8	-0.7	-0.4	-0.4	1.1	-0.37
Females	43.1	45.1	2.0	46.4	47.8	1.4	-0.6	-0.3	-0.5	1.3	-0.35
Full Increase*											
All Teenagers	47.5%	41.0%	-6.5	46.3%	41.9%	-4.3	2.2	0.4	0.3	0.9	0.35
Males	48.5	40.9	-7.6	47.1	41.6	-5.5	2.0	0.0	0.0	1.3	0.01
Females	46.5	41.1	-5.4	45.4	42.3	-3.1	2.4	1.6	1.4	1.3	1.10
All LTHS Adults	57.5%	57.8%	0.3	58.8%	61.9%	3.0	2.7	1.2	1.1	0.9	1.26
Males	71.4	71.4	0.0	73.7	74.5	0.8	0.8	-1.3	-1.4	1.1	-1.21
Females	43.1	43.8	0.7	43.1	47.8	4.6	3.9	3.3	3.2	1.3	2.55

* Each panel represents a difference-in-differences test for six-month periods before and after a minimum wage increase. In the top panel (First Increase), T1 is April 1995-September 1995, T2 is October 1995-March 1996, T3 is April 1996-September 1996, and T4 is October 1996-March 1996. In the middle panel (Second Increase), T1 is March 1996-August 1996, T2 is September 1996-February 1997, T3 is March 1997-August 1997, and T4 is September 1997-February 1998. In the bottom panel (Full Increase), T1 is March 1995-August 1995, T2 is September 1995-February 1996, T3 is March 1996-August 1996, and T4 is September 1997-February 1998.

"before-and-after" analysis, then, would suggest that the minimum wage led teenage employment to fall by 5.9 percentage points (see column 6, headed "Difference").

The six-month period before the minimum wage increase, however, includes the summer months when teenage employment rises. The six-month period after the increase is made up entirely of months when most teenagers are in school and employment rates are lower.

To control for this seasonal effect, we compare the change in employment between April 1996-September 1996 and October 1996-March 1997 with the change across the same two periods a year earlier when the minimum wage did not change.

After controlling for the seasonal employment effect, the data suggest that teenage employment actually increased after the minimum wage rose.

Columns 1 and 2 show the employment rates for these earlier periods. Among all teenagers, the employment rate fell from 47.6% in the April-to-September period (T1) to 40.7% in the October-to-March period (T2) — a 6.9 percentage-point drop (see column 3). The decline in teenage employment after the minimum wage increase went into effect, then, was 1.0 percentage point *lower* than a year earlier when the minimum wage remained unchanged. So, after controlling for the seasonal employment effect, the data suggest that teenage employment actually *increased* after the minimum wage rose (though this increase was not statistically significant, as we shall see below). For less-educated adult workers, the DD in column 7 (Raw) for all persons in this category is also positive (0.3%).

These "raw" (i.e., unadjusted) difference-in-differences estimates, however, may still provide biased estimates of the actual employment effects for the two reasons noted above. To address this possibility, the estimates of the employment changes in columns 8 and 9 adjust the "raw" estimate to correct for both overall economic growth and state differences.[9] The "growth-adjusted" estimates in column 8 indicate that economic growth was more rapid in the year that the minimum wage increased than it had been in the preceding year. Controlling for overall economic growth rates generally reduces the minimum wage's positive employment effects and makes any negative employment effects more negative. The growth in teen employment, for example, falls from a 1.0 percentage-point increase to a smaller 0.5 percentage-point increase after controlling for overall growth. Further controlling for the different levels of economic growth within states (column 9) subtracts another 0.1 percentage point from the positive employment effect for teenagers.

All of these considerations, though, largely ignore the issue of whether the measured changes are statistically significant, that is, whether observed differences reflect "real" changes in economic circumstances or are merely the result of random economic fluctuations. The final two columns correct this omission by presenting standard errors and "t-statistics." These reveal the statistical significance

of the growth- and state-adjusted estimates in column 9. Only a t-statistic with an absolute value greater the 1.96 is generally considered significant in this type of research. Since none of the estimates for teenagers meets this criterion, the employment effects, though positive for all workers and females and negative for males, should be considered indistinguishable from zero (no effect).

Looking at the first panel in Table 4 for the less-educated adult group, the raw DD is 0.3 percentage points (i.e., ignoring state and growth effects as well as statistical significance, employment rates grew slightly for this group). Accounting for growth and state effects lowers the estimate to zero overall, and to -1.1 for males and 0.7 for females. The t-statistics, however, reveal that none of these changes are significant.

While some of the effects are relatively large, for the most part they reflect the same lack of significant employment rate changes.

The second (*Second Increase*) and third (*Full Increase*) panels show the results for other time periods. The second panel reveals that the employment impact of the second increase — from $4.75 to $5.15 on September 1, 1997 — had much the same impact, or lack thereof, as the first. State- and growth-adjusted estimates were positive for teenagers and negative for less-educated adults, but again none of the results are significantly distinguishable from zero.

The third panel presents the DD estimates over the full time period, from a few months before the first increase to six months after the second increase. This overall test of the full impact of the increase again reveals no significant negative effects for these groups. For all teenagers, the full impact led to an insignificant growth of 0.3 percentage points, driven by the (also insignificant) growth in the employment rates for female teenagers. The pattern is similar for the less-educated adult group, but in this case, the growth in employment rates for females is statistically significant.

Table 5 shows the adjusted DDs with their standard errors and t-statistics for subgroups of teenagers and less-educated adult workers by race and gender. While some of the effects are relatively large (compared to the overall results from the previous table), for the most part they reflect the same lack of significant employment rate changes as the previous table (their t-statistics are still well below 1.96 in absolute value). This is because most of the estimates for these subgroups depend on smaller underlying samples that produce less reliable estimates of the true impact of the policy change. For teenagers, the only result to reach statistical significance (t-statistic greater than 1.96) is the large and positive effect for Hispanic females. While the employment effects for the other groups fluctuate around zero, they are too small to be confidently distinguished from zero (i.e., no change at all).

For less-educated adult workers, none of the results from the first or second steps of the increase are statistically significant. Here again, some of the changes

TABLE 5
Difference-in-Differences: The Employment Impact of the 1996-97 Minimum Wage Increase on Teenagers and Adults (Age 20-54) With Less Than a High School Education, by Gender and Race

	Teenagers			Adults, LTHS, 20-54		
	State & Growth Adjusted	Std Err	T-Stat	State & Growth Adjusted	Std Err	T-Stat
First Increase*						
All	0.4%	0.9	0.46	-0.0%	0.9	-0.03
Males	-0.3	1.3	-0.23	-1.1	1.1	-0.96
Females	1.2	1.3	0.92	0.7	1.3	0.57
White	-0.0	0.9	-0.04	-1.2	1.2	-0.95
Males	-0.4	1.6	-0.26	-2.1	1.6	-1.36
Females	0.3	1.6	0.18	-0.3	1.8	-0.14
Black	-1.8	1.8	-1.00	-1.9	2.4	-0.81
Males	-3.8	3.2	-1.17	-3.7	3.7	-1.00
Females	-0.7	3.2	-0.21	-0.7	3.1	-0.22
Hispanic	3.9	2.1	1.84	2.9	1.6	1.75
Males	1.5	3.9	0.39	1.6	1.9	0.85
Females	8.1	3.7	2.21	3.4	2.3	1.47
Second Increase*						
All	1.4	0.9	1.48	-0.3	0.9	-0.36
Males	2.3	1.3	1.74	-0.4	1.1	-0.37
Females	0.2	1.3	0.17	-0.5	1.3	-0.35
White	1.6	1.1	1.39	0.9	1.3	0.67
Males	2.3	1.6	1.45	0.4	1.6	0.23
Females	0.4	1.6	0.24	0.8	1.9	0.43
Black	2.1	2.3	0.90	1.1	2.5	0.44
Males	3.2	3.2	1.00	-0.4	3.7	-0.11
Females	0.3	3.3	0.08	2.9	3.3	0.89
Hispanic	0.3	2.7	0.12	-2.1	1.6	-1.31
Males	2.0	3.9	0.51	-1.3	1.8	-0.72
Females	-1.9	3.6	-0.54	-3.0	2.3	-1.28
Full Increase*						
All	0.3	0.9	0.35	1.1	0.9	1.26
Males	0.0	1.3	0.01	-1.4	1.1	-1.21
Females	1.4	1.3	1.10	3.2	1.3	2.55
White	0.2	1.1	0.21	0.1	1.2	0.09
Males	0.3	1.6	0.20	-2.2	1.6	-1.38
Females	1.2	1.6	0.73	2.1	1.9	1.15
Black	-2.8	2.3	-1.21	2.3	2.4	0.93
Males	-4.8	3.3	-1.47	-3.7	3.7	-1.00
Females	-0.6	3.2	-0.19	7.0	3.1	2.25
Hispanic	4.1	2.7	1.51	2.7	1.6	1.67
Males	3.2	3.9	0.84	1.0	1.9	0.56
Females	7.8	3.7	2.13	3.3	2.3	1.43

* See previous table note for a description of the time periods covered by each panel.

for the subgroups are relatively large but remain statistically insignificant due to the wide error margins. Over the full period, there was significant employment growth for black females, driven by their fast rate of employment growth in the six months following the second increase.

Thus, the DD tests reveal no significant employment losses from the 1996-97 increase in the minimum wage, either for teenagers or the less-educated adult group. In fact, the only statistically significant results were positive employment effects for Hispanic female teenagers and for less-educated female adults over the full increase (t=2.55).

The DD tests reveal no significant employment losses from the 1996-97 increase in the minimum wage, either for teenagers or the less-educated adult group.

How reliable are these findings? Relative to some of the other employment tests in this study, we consider the DD test to be fairly weak. Despite our best efforts, we still question whether this test effectively controls for economic growth, a drawback this test shares with others in the literature. However, even if the test is not powerful enough to separate the minimum wage effect on, say, teenage employment from the effect of macroeconomic growth on such employment, this would underscore the negligible impact of the most recent wage increase on job growth. If the recent increase generated the economic upheaval that its opponents predicted, this would be reflected in the DD test. In this regard, the fact that the test does not uncover such dislocation speaks both to the weakness of the test and the small impact of the recent increase in low-wage workers' employment rates.

With these caveats in mind, we would summarize the DD test results as revealing no change in the employment rates of low-wage workers over the period of the recent minimum wage increase.

Test 2: Deere, Murphy, and Welch

Donald Deere, Kevin Murphy, and Finis Welch (1995) have developed a separate test of the minimum wage's impact on employment. In their 1995 paper published in the *American Economic Association Papers and Proceedings*, they found large job losses associated with the 1990 and 1991 increases in the federal minimum wage. In this section, we use the Deere, Murphy, Welch (DMW) approach to test the employment effects of the 1996 and 1997 increases. The results that we obtain here for the most recent increases differ markedly from those obtained by the DMW test in connection with the 1990 and 1991 increases. If anything, the DMW test indicates that the 1996-97 hikes in the minimum wage actually *increased* employment among groups most affected by the minimum wage.

The DMW test's framework is simple. It analyzes movements in the employment rates of teenagers (ages 15-19) and less-than-high-school-educated adults (ages 20-54) in the 50 states and the District of Columbia over several years. It uses

movements in each state's adult male employment rate to control for business-cycle effects on teen or less-than-high-school-educated adult employment. It then judges the effect of the minimum wage by comparing state employment levels across years before and after the minimum wage hike.[10]

We use this basic approach to estimate the employment impact of the 1996-97 increases in the minimum wage using data from the basic monthly Current Population Survey, the same data analyzed in the preceding difference-in-differences test. Following the DMW test, we examine the employment impacts on six demographic groups: male, female, and black teenagers (ages 15-19) and male, female, and black less-than-high-school-educated adults (ages 20-54). We present two sets of estimates for each group.

On net, the results indicate that employment among all the affected groups increased.

Table 6 presents the first set of employment estimates for the six demographic groups. The data cover six separate 11-month periods running from October 1991-August 1992 through October 1996-September 1997. During the first five of these six periods, the minimum wage was $4.25 per hour, but during the last 11-month period the minimum wage was $4.75.[11] The results provide little support for the assertion that the first minimum wage hike reduced employment opportunities for low-wage workers. The minimum wage increase appears to have *raised* employment of less-educated adult male workers (up 2.6%) and less-educated black workers (up 5.2%). In fact, these increases were economically meaningful and statistically significant. The 1996 increase had no apparent impact on the employment opportunities of female teens, black teens, or less-educated adult women. Employment rose in two of these cases and fell in one, but in none was the change statistically significant. The minimum wage appeared only to reduce employment opportunities for male teens (down 3.2%). On net, the results indicate that employment among all the affected groups increased.

Table 7 presents the second set of employment estimates for the same groups. These data cover seven consecutive six-month periods running from September 1991-February 1992 through September 1997-February 1998. This arrangement of the data allows us to examine the employment impact of both the October 1996 and September 1997 increases. The results provide even less support than those in Table 6 for the view that minimum wage hikes cost jobs. Both minimum wage increases appear to have *raised* less-educated adult male employment (up 2.5% after the minimum wage went to $4.75, and then up 2.9% after the minimum wage went to $5.15). Employment among less-educated black adults also increased dramatically after the second increase in the minimum wage (up 9.9%). The employment effects of the minimum wage were also, on net, positive for less-educated adult women, female teens, and black teens, though none of these changes was statistically sig-

TABLE 6
Regression Estimates of Employment Changes From the 1996 Increase in the Minimum Wage: DMW Test
(Estimates based on 11-month periods from October through August)

	Male	Female	Blacks
Teenagers 15-19			
Employment Rate of Men 20 and older	1.36** (0.34)	0.27 (0.34)	0.68 (0.84)
Year Effect, Minimum Wage = $4.75	-3.15** (1.32)	1.67 (2.06)	-0.55 (3.67)
Root MSE	0.074	0.074	0.265
Sample Size	306	306	514
Less Than High School Adults 20-54			
Employment Rate of Men 20 and Older Excluding LTHS	1.59** (0.23)	1.05* (0.43)	0.84 (0.72)
Year Effect, Minimum Wage = $4.75	2.61** (0.70)	3.13 (2.63)	5.23* (2.66)
Root MSE	0.039	0.073	0.187
Sample Size	255	255	424

Notes:
(1) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **: statistically significant, 1% level.
(2) Regressions weighted by number of individuals in sample in state and year corresponding to each observation; 51 state effects included, but not shown.
(3) Coefficient on year effect when minimum wage=$4.75 is multiplied by 100.
(4) All regressions include two dummy variables for pre-1994 version of CPS. The first takes value 1 in 1991 and 1992, 0 otherwise; the second takes the value 1 in 1993, 0 otherwise. Three of 11 month-years beginning in October 1993 are using old survey; eight months use the new survey.
(5) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.
(6) Regressions for less-than-high-school-educated adults are for 1992-97 only to avoid problems associated with change in education coding in 1992.

nificant. Employment opportunities appeared to decline for male teens, but, again, the change was not statistically significant.[12] Overall, these second DMW employment results suggest that the minimum wage increased employment among teens and less-educated adults.

In strong contrast to earlier findings using the DMW test, the results obtained here in connection with the 1996 and 1997 minimum wage increases indicate that, if anything, the back-to-back wage hikes *raised* the employment levels of affected workers, with less-educated adults being the major beneficiaries.

TABLE 7
Regression Estimates of Employment Changes From the 1996 and 1997 Increases in the Minimum Wage: DMW Test
(Estimates based on 6-month periods from September through February)

	Male	Female	Blacks
Teenagers 15-19			
Employment Rate of Men 20 and older	0.75* (0.33)	0.19 (0.35)	0.76 (0.88)
Year Effect, Minimum Wage = $4.75	-2.55 (1.77)	2.98 (2.92)	1.53 (4.99)
Year Effect, Minimum Wage = $5.15	-1.69 (1.79)	4.79 (4.18)	1.16 (5.08)
Root MSE	0.101	0.106	0.354
Sample Size	357	357	559
Less Than High School Adults 20-54			
Employment Rate of Men 20 and Older Excluding LTHS	1.51** (0.21)	0.84* (0.39)	1.13 (0.77)
Year Effect, Minimum Wage = $4.75	2.45** (0.89)	-0.53 (3.11)	3.00 (2.66)
Year Effect, Minimum Wage = $5.15	2.87** (0.91)	1.33 (4.75)	9.87** (3.42)
Root MSE	0.050	0.088	0.232
Sample Size	306	306	476

Notes:
(1) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **: statistically significant, 1% level.
(2) Regressions weighted by number of individuals in sample in state and year corresponding to each observation; 51 state effects included, but not shown.
(3) Coefficient on year effects are multiplied by 100.
(4) All regressions include two dummy variables for pre-1994 version of CPS. The first takes value 1 in 1991 and 1992, 0 otherwise; the second takes value 1 in 1993, 0 otherwise. Three of 11 month-years beginning in October 1993 are using old survey; eight months use the new survey.
(5) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.
(6) Regressions for less-than-high-school-educated adults are for 1992-97 only to avoid problems associated with change in education coding in 1992.

Test 3: Card

David Card (1992) has developed a third test to determine the employment effects of the minimum wage.[13] He observed that there is wide variation across states in wage levels and, therefore, an increase in the federal minimum wage does not affect all states equally. Card reasoned that a higher federal minimum wage should have a bigger impact on wages and employment in low-wage states and a smaller impact on wages and employment in higher-wage states. As a result, Card proposed examining state-level employment data gathered before and after a minimum-wage increase to see whether employment patterns differed systematically between low-wage and higher-wage states. If the traditional view that minimum wages reduce employment were true, we would expect that the imposition of a higher federal minimum wage would lower employment in low-wage states relative to higher-wage states. Card applied his test to the 1990 and 1991 increases in the federal minimum wage and found no systematic relationship between changes in teen employment rates across states and the share of a state's teens that were affected by the two federal increases.

The two most recent increases in the federal minimum wage had little systematic impact on the employment opportunities of affected workers.

In this section, we apply Card's test to the 1996 and 1997 increases. Our results generally confirm Card's earlier findings. The two most recent increases in the federal minimum wage had little systematic impact on the employment opportunities of affected workers.[14]

In this approach to estimating the employment impact of the 1996-97 increases in the minimum wage we use employment data from the basic monthly Current Population Survey (CPS) and wage data from the Outgoing Rotation Group (ORG) of the CPS.[15] We use the larger, basic CPS to provide the most accurate measure of employment rates. Since the basic CPS does not include wage data for all respondents, we use the smaller ORG to calculate the relevant wage measures. As with the DMW test, we implement the Card test on two samples. The first is a sample of three 11-month periods from October 1994-August 1995 through October 1996-August 1997 for the 50 states plus the District of Columbia. The first two 11-month periods predate the October 1, 1996 increase; the second 11-month period immediately follows that increase. The second sample consists of four six-month periods from September 1994-February 1995 through September 1997-February 1998, which allows us to look at the first six months of employment effects of the September 1, 1997 increase as well as the combined effects of the first and second increases over a 17-month period from October 1996 to February 1998.

Table 8A presents the first set of 11-month results for teenagers (ages 16-19). The regressions in columns 1 and 2 analyze the effect of the October 1, 1996 increase on teen wages; columns 3 and 4 examine the impact on teen employment.

TABLE 8A
Regression Estimates of Teen Wage and Employment Changes From the 1996 Increase in the Minimum Wage: Card Test
(Estimates based on 11-month periods from October through August)

	Change in Mean Log Wage		Change in Employment Rate	
	(1)	(2)	(3)	(4)
1995-96				
Fraction of Working Teens	0.143*	0.145*	-0.109**	-0.110**
Earning $4.25-$4.74 in 1995	(0.058)	(0.057)	(0.041)	(0.041)
Change in Adult	—	-0.699	—	0.229
Employment Rate		(0.579)		(0.414)
R-squared	0.112	0.138	0.128	0.133
1994-96				
Fraction of Working Teens	0.164**	0.166**	-0.056	-0.060
Earning $4.25-$4.74 in 1994	(0.051)	(0.050)	(0.050)	(0.045)
Change in Adult	—	-0.423	—	1.124**
Employment Rate		(0.382)		(0.344)
R-squared	0.177	0.198	0.025	0.203
1994-95				
Fraction of Working Teens	0.051	0.047	0.030	0.026
Earning $4.25-$4.74 in 1994	(0.054)	(0.055)	(0.041)	(0.042)
Change in Adult	—	0.481	—	-0.018
Employment Rate		(0.641)		(0.487)
R-squared	0.018	0.029	0.008	0.008

Notes:
(1) The dependent variable is the change in the teen (16-19) employment-to-population rate in each of the 50 states and the District of Columbia (from the basic CPS).
(2) Standard errors are in parentheses. ** indicates significance at the 1% level; *: 5% level; #: 10% level.
(3) Coefficients from a weighted least squares regression, using the number of teenagers in the state in the earlier year's CPS ORG sample.

Although columns 1 and 3 do not control for overall economic activity in each state, columns 2 and 4 use changes in the adult employment rate to attempt to control for economic growth. The first panel (1995-96) compares changes in teen wages and employment between October 1995-August 1996 (when the minimum wage was $4.25) and October 1996-August 1997 (when the minimum wage was $4.75) across states with different initial shares of low-wage teens. This first set of estimates suggests that the 1996 minimum wage increase led to a statistically significant increase in the average teen wage, but also reduced teen employment by a statistically significant amount.[16] This estimate, however, is sensitive to the choice

of the pre-increase period. In the second panel (1994-96), which compares changes between October 1994-August 1995 (two years before the minimum wage hike) and October 1996-August 1997 (the 11-month period immediately after the first increase), the minimum wage has an even larger effect on teen wages, but the employment effect is smaller and no longer statistically significant. The third panel (1994-95) illustrates how the Card test performs when there is no increase in the federal minimum wage. The minimum wage remained constant at $4.25 between October 1994-August 1995 and October 1995-August 1996. As we would expect, under these circumstances, the share of low-wage teens in each state has no impact on the change in teen wages or teen employment. Overall, this first set of results indicates that the October 1, 1996 increase had a statistically significant impact on average teen wages, but it had no clear cut impact on teen employment.

Overall, this first set of results indicates that the October 1, 1996 increase had a statistically significant impact on average teen wages, but it had no clear cut impact on teen employment.

Table 8B displays corresponding estimates for less-than-high-school-educated adults, ages 20 to 54. As with teens, the 1996 increase led to a statistically significant increase in the average wage of less-educated adults. The 1996 increase was also associated with a *rise* (significant at the 10% level) in less-educated adult employment as shown in the table's first panel (1995-96).[17] As before, however, this employment estimate is sensitive to the choice of year to which it is compared. When we compare the less-educated employment change between October 1994-August 1995 (two years before the first increase) with October 1996-August 1997 (the 11-months after the first increase), the employment increase is smaller and is no longer statistically significant (see the 1994-96 panel).

Table 9A presents similar results for teenagers across both the 1996 and the 1997 increases. The table's first panel (1996-97) reports the impact of the 1997 increase on teen wages and employment rates. The 1997 increase raised teen wages by a statistically significant margin, but had only a small, statistically insignificant positive effect on teen employment. The second panel (1995-97) shows the results from a test across both the 1996 and 1997 increases. The wage increase is large and statistically significant, while the employment change is small and statistically insignificant. The third panel (1994-97) also tests the wage and employment effects of the combined increases, using a year earlier as the base period. The results are almost identical to those in the second panel — large, statistically significant, positive wage effects and small, statistically insignificant employment effects.

The fourth, fifth, and sixth panels repeat the tests of the first increase performed in the first three panels of Table 8A. The main purpose of these last three tests is to gauge how much the use of data for six-month versus 11-month periods may be affecting our conclusions. We find that the six-month periods in Table 9A give results that are qualitatively identical to those of the 11-month periods in Table

TABLE 8B
Regression Estimates of LTHS Adult Wage and Employment Changes From the 1996 Increase in the Minimum Wage: Card Test
(Estimates based on 11-month periods from October through August)

	Change in Mean Log Wage		Change in Employment Rate	
	(1)	(2)	(3)	(4)
1995-96				
Fraction of Working LTHS Earning $4.25-$4.74 in 1995	0.365* (0.155)	0.368* (0.157)	0.213# (0.110)	0.197# (0.104)
Change in Adult HS+ Employment Rate	—	-0.200 (0.511)	—	0.913** (0.339)
R-squared	0.101	0.104	0.071	0.193
1994-96				
Fraction of Working LTHS Earning $4.25-$4.74 in 1994	0.375* (0.145)	0.378* (0.147)	0.098 (0.106)	0.119 (0.097)
Change in Adult HS+ Employment Rate	—	0.117 (0.422)	—	0.937** (0.277)
R-squared	0.120	0.121	0.017	0.206
1994-95				
Fraction of Working LTHS Earning $4.25-$4.74 in 1994	0.198 (0.150)	0.198 (0.152)	-0.009 (0.082)	-0.011 (0.073)
Change in Adult HS+ Employment Rate	—	0.173 (0.597)	—	1.070** (0.289)
R-squared	0.034	0.036	0.000	0.223

Notes:
(1) The dependent variable is the change in the LTHS (20-54) employment-to-population rate in each of the 50 states and the District of Columbia (from the basic CPS).
(2) Standard errors are in parentheses. ** indicates significant at the 1% level; *: 5% level; #: 10% level.
(3) Coefficients from a weighted least squares regression, using the number of teenagers in the state in the earlier year's CPS ORG sample.

8A. As expected, however, the statistical precision of the six-month results is not as high as with the 11-month periods. These last three panels suggest, though, that the use of six-month periods does not substantially reduce the accuracy of the tests relative to the use of 11-month periods.

Table 9B displays the corresponding six-month results for less-educated adults. The test of the 1997 increase in the table's first panel (1996-97) is the first that shows no significant impact of the minimum wage hike on average wages. At the same time, the test indicates that the hike was associated with a fall in employment

TABLE 9A
Regression Estimates of Teen Wage and Employment Changes From the 1996 and 1997 Increases in the Minimum Wage: Card Test
(Estimates based on 6-month periods from September through February)

	Change in Mean Log Wage		Change in Employment Rate	
	(1)	(2)	(3)	(4)
1996-97				
Fraction of Working Teens Earning $4.75-$5.14 in 1996	0.129* (0.062)	0.137* (0.062)	0.020 (0.058)	0.025 (0.059)
Change in Adult Employment Rate	—	0.565 (0.477)	—	0.371 (0.455)
R-squared	0.082	0.108	0.002	0.016
1995-97				
Fraction of Working Teens Earning $4.25-$5.14 in 1995	0.200** (0.057)	0.204** (0.058)	-0.041 (0.048)	-0.043 (0.049)
Change in Adult Employment Rate	—	0.154 (0.473)	—	-0.102 (0.401)
R-squared	0.202	0.204	0.014	0.016
1994-97				
Fraction of Working Teens Earning $4.25-$5.14 in 1994	0.233** (0.062)	0.231** (0.060)	-0.023 (0.049)	-0.024 (0.048)
Change in Adult Employment Rate	—	0.773# (0.387)	—	0.498 (0.307)
R-squared	0.223	0.283	0.005	0.056
1995-96				
Fraction of Working Teens Earning $4.25-$4.74 in 1995	0.255** (0.059)	0.245** (0.058)	-0.081# (0.042)	-0.081# (0.042)
Change in Adult Employment Rate	—	-0.923 (0.573)	—	-0.014 (0.413)
R-squared	0.274	0.311	0.072	0.072
1994-96				
Fraction of Working Teens Earning $4.25-$4.74 in 1994	0.228** (0.064)	0.227** (0.064)	-0.022 (0.051)	-0.023 (0.050)
Change in Adult Employment Rate	—	0.456 (0.426)	—	0.516 (0.337)
R-squared	0.206	0.225	0.004	0.050
1994-95				
Fraction of Working Teens Earning $4.25-$5.14 in 1994	0.079 (0.066)	0.071 (0.067)	0.031 (0.048)	0.034 (0.049)
Change in Adult Employment Rate	—	0.594 (0.720)	—	-0.193 (0.533)
R-squared	0.029	0.042	0.009	0.011

Notes:
(1) The dependent variable is the change in the teen (16-19) employment-to-population rate in each of the 50 states and the District of Columbia (from the basic CPS).
(2) Standard errors are in parentheses. ** indicates significant at the 1% level; *: 5% level; #: 10% level.
(3) Coefficients from a weighted least squares regression, using the number of teenagers in the state in the earlier year's CPS ORG sample.

TABLE 9B
Regression Estimates of LTHS Adult Wage and Employment Changes From the 1996 and 1997 Increases in the Minimum Wage: Card Test
(Estimates based on 6-month periods from September through February)

	Change in Mean Log Wage		Change in Employment Rate	
	(1)	(2)	(3)	(4)
1996-97				
Fraction of Working LTHS Earning $4.75-$5.14 in 1996	0.291 (0.179)	0.277 (0.182)	-0.238# (0.131)	-0.234# (0.134)
Change in Adult HS+ Employment Rate	—	0.338 (0.621)	—	-0.075 (0.456)
R-squared	0.051	0.057	0.063	0.064
1995-97				
Fraction of Working LTHS Earning $4.25-$5.14 in 1995	0.335** (0.109)	0.337** (0.110)	-0.053 (0.078)	-0.057 (0.078)
Change in Adult HS+ Employment Rate	—	-0.144 (0.462)	—	0.374 (0.326)
R-squared	0.161	0.163	0.009	0.036
1994-97				
Fraction of Working LTHS Earning $4.25-$5.14 in 1994	0.255* (0.121)	0.271* (0.129)	-0.050 (0.091)	-0.101 (0.094)
Change in Adult HS+ Employment Rate	—	-0.168 (0.424)	—	0.545# (0.310)
R-squared	0.083	0.086	0.036	0.066
1995-96				
Fraction of Working LTHS Earning $4.25-$4.74 in 1995	0.465* (0.200)	0.449* (0.199)	0.161 (0.108)	0.151 (0.107)
Change in Adult HS+ Employment Rate	—	0.852 (0.686)	—	0.538 (0.370)
R-squared	0.100	0.128	0.043	0.083
1994-96				
Fraction of Working LTHS Earning $4.25-$4.74 in 1994	0.335# (0.170)	0.316# (0.170)	0.152 (0.138)	0.129 (0.137)
Change in Adult HS+ Employment Rate	—	0.519 (0.472)	—	0.612 (0.378)
R-squared	0.074	0.096	0.024	0.075
1994-95				
Fraction of Working LTHS Earning $4.25-$5.14 in 1994	0.166 (0.117)	0.154 (0.120)	0.068 (0.068)	0.045 (0.068)
Change in Adult HS+ Employment Rate	—	0.343 (0.629)	—	0.691# (0.356)
R-squared	0.040	0.045	0.020	0.091

Notes:
(1) The dependent variable is the change in the LTHS (20-54) employment-to-population rate in each of the 50 states and the District of Columbia (from the basic CPS).
(2) Standard errors are in parentheses. ** indicates significant at the 1% level; *: 5% level; #: 10% level.
(3) Coefficients from a weighted least squares regression, using the number of teenagers in the state in the earlier year's CPS ORG sample.

for less-educated workers (significant at the 10% level). Across both increases in the minimum wage (see the second and third panels), however, wages rose substantially, with no apparent impact on the employment opportunities of less-educated adults. The fourth, fifth, and sixth panels demonstrate that these six-month results are qualitatively similar to those obtained using the longer 11-month periods.

The Card test consistently finds that the 1996 and 1997 minimum wage increases had a strong impact on the wages of teens and less-than-high-school-educated adults. Results from the same test, however, suggest that the minimum wage has no consistent, measurable impact on the employment opportunities of these same two groups.

Correcting the traditional methodology to incorporate recent improvements in statistical methods reveals disemployment effects that are consistently economically small and statistically insignificant.

Test 4: Time-Series Estimates

Traditionally, economists have examined the employment effects of minimum wage increases using statistical analyses of time-series data. In this approach, analysts examined the history of teenage employment rates attempting to discern, when all else is equal, the extent to which changes in the minimum wage affect teen employment rates. In this section, we briefly review the findings from previous time-series analyses and provide an update that includes the most recent increase. We also introduce some important modifications to the earlier methodology as suggested by economists such as Paul Wolfson (1997). We find in our updated analysis that, as revealed by the conventional time-series methodology, the disemployment effect has both fallen and become less statistically significant over time. Moreover, correcting the traditional methodology to incorporate recent improvements in statistical methods (again, see Wolfson 1997) reveals disemployment effects that are consistently economically small and statistically insignificant.

Previous Findings: A 1982 literature review by Brown, Gilroy, and Kohen neatly summarizes the pre-1990s conventional wisdom on the employment effects of the minimum wage. Their review examined time-series models of the changes in the employment rates of low-wage workers (mostly teenagers), given an increase in the minimum wage and controlling for other factors that are thought to influence the supply of and demand for teenage employment.[18] After an extensive review, they concluded:

> Time-series studies typically find that a 10% increase in the minimum wage reduces teenage employment by one to three percent.…We believe that the lower half of that range is to be preferred....The effect of the minimum wage on young adult (20-24) employment is negative and smaller than that for teenagers.

This finding seemed to corroborate economists' intuition that increases in the minimum wage should lead to lower levels of employment among concentrated groups of low-wage workers. The magnitude of the effect, however, was small in the sense that even at the high end of this estimate, many more workers would benefit from the increase than would be disemployed.[19]

A few years after the Brown, Gilroy, and Kohen study, Wellington (1991) published an update of their work, with both more years of data (the original study's analysis stops in the late 1970s, while Wellington's goes up to 1986) and greater attention to the seasonal variation in the data (as we have noted throughout, teenage employment rates are highly seasonal). These additions led her to conclude that "a 10% increase in the minimum wage is estimated to reduce teen employment by less than 1% — the lower end of the range of previous estimates."

Our addition of the time period covering the most recent increase leads to a further lowering of the disemployment estimate.

Most recently, Card and Krueger (1995) updated Wellington's work by taking the analysis up to 1993, thus capturing the 1990-91 increases in the minimum wage. Interestingly, these updates revealed a consistent decline in the negative employment effect of the minimum wage, suggesting that estimates of job losses resulting from minimum wage increases have been falling over time.

In order to further test this development, we have updated the traditional time-series model and examined the minimum wage effect on teenage employment in three different time periods.[20] Each period has the same starting point of 1954, but we have added extra years in each model, thus tracking the change in the employment effect as more information on the relationship between teenage employment and minimum wages becomes available.

Table 10 shows the results for the different time periods. Each column represents the employment effect of the minimum wage over the respective time span. The analysis is constructed so that the coefficients in the first row of each panel represent teenage employment elasticities with respect to the minimum wage. Thus, the first row of the top panel suggests that, between 1954 and 1979, a 10% increase in the minimum wage would lead to a 0.95% decrease in teenage employment,[21] an effect considered statistically significant by conventional social science standards.[22] This effect falls to -0.75% when 10 more years of data are added, and again to -0.66% when we include the most recent data (which incorporates the 1996-97 increases in the minimum). Thus, like Card and Krueger's update, our addition of the time period covering the most recent increase leads to a further lowering of the disemployment estimate. Note also that the most recent two estimates (the second and third columns in the table) are not statistically significant at the generally accepted level of t greater than 1.96 (in absolute value).

TABLE 10
Times Series Models of the Minimum Wage Impact on Teenage Employment*

	1954-79	1954-89	1954-97
Basic Model			
Kaitz Index**	-0.950	-0.750	-0.660
t-statistic	-2.389	-1.677	-1.714
Wolfson Model			
Kaitz Index**	-0.500	-0.350	-0.290
t-statistic	-1.010	-0.865	-0.778

* See Data Appendix for full regression output, data sources, and methods.
** This variable measure the impact of a 10% increase in the minimum wage on teenage employment rates (see text for explanation).

New Time-Series Research: Since models of the type described above were introduced, there have been some important advances in the approach to time-series analysis. New work in this area pays more careful attention to two shortcomings of the earlier research. First, the early time-series models inadequately adjust the data for seasonality and, second, they pay inadequate attention to "stationarity" conditions that alter the statistical properties of tests of the minimum wage.[23]

Economist Paul Wolfson (1997) takes into account both of these problems in a recent re-analysis of the same time-series used above.[24] To account for the non-stationarity problem, Wolfson differences some of the key variables (see Data Appendix); to account for the seasonality of teenage employment rates (the dependent variable), he uses lagged seasonal differences as a regressor.

The results from our update of these regressions are shown in the bottom panel (Wolfson Model) of Table 10.[25] There are three points to consider from this panel. First, the coefficients are about half the magnitude of those from the top panel (Basic Model), which fails to account for the problems raised by Wolfson. Second, note that here, too, the coefficients decline in magnitude as the model is updated. Finally, each of the t-statistics is well below (in absolute value) conventional significance levels. Thus, according to this time-series model, the disemployment effect on teenage workers, while negative, is statistically indistinguishable from zero.

In sum, the time-series results provide further evidence that the job loss effects from minimum wage increases have been overemphasized in the debate over the policy.

SECTION 4 — HOW CAN WE EXPLAIN OUR RESULTS?

The preceding section presented a range of estimates regarding the minimum wage's employment effects from four different statistical tests applied separately to teenagers and less-educated adult workers across two increases in the federal minimum wage. As the employment elasticities in **Table 11** demonstrate, the data from these four tests provide little support for the view that minimum wage increases systematically reduce employment opportunities for teenagers or less-educated adults. Not only are the estimated employment effects generally small and statistically insignificant, but they are also almost as likely to be positive as negative, varying unpredictably across demographic groups.

The data from these four tests provide little support for the view that minimum wage increases systematically reduce employment opportunities for teenagers or less-educated adults.

In this section, we attempt to explain what it is about the low-wage labor market that might account for these results. We first try to make sense of our findings in the context of the standard "competitive" model. We then examine how new models based on the idea of "dynamic monopsony" might offer a better explanation.

The Competitive Model

To organize their thinking about low-wage labor markets and the minimum wage, economists have traditionally made certain assumptions about labor markets:

- that many small employers and many individual workers participate;
- that all employers and all workers can, without cost, enter or leave the market;
- that all employers can, without cost, hire or fire workers and all workers can find or leave employment without incurring loss;
- that all employers and all workers have perfect, instantaneous knowledge of the technologies, tastes, and abilities of all market participants;
- that each worker's productivity is identical and known to each employer before the worker is hired; and
- that all workers work to their full potential without the need for guidance or supervision.

These and a few other technical assumptions describe the basic world envisioned in the "competitive" model of the low-wage labor market.[26]

TABLE 11
Estimated Employment Elasticities From Four Tests of the 1996-97 Increases in the Minimum Wage

	Teens			LTHS Adults		
	1996	1997	1996+97	1996	1997	1996+97
(a) **Difference-in-Differences**						
All	0.08	0.38	0.03	0.00	-0.06	0.09
Male	-0.06	0.61	0.00	-0.13	-0.07	-0.09
Female	0.23	0.05	0.15	0.14	-0.14	0.35**
White	0.00	0.37	0.02	-0.17	0.18	0.01
Male	-0.07	0.54	0.03	-0.25	0.07	-0.15
Female	0.05	0.09	0.11	-0.05	0.20	0.20
Black	-0.58	0.95	-0.50	-0.36	0.29	0.24
Male	-1.28	1.52	-0.90	-0.59	0.09	-0.33
Female	-0.22	0.13	-0.10	-0.16	0.92	0.88*
Hispanic	0.96	0.10	0.56	0.41	-0.41	0.21
Male	0.33	0.62	0.39	0.17	-0.19	0.06
Female	2.30*	-0.76	1.23*	0.73	-0.91	0.40
(b) **Deere, Murphy, Welch**						
Male	-0.72**	-0.54	n.a.	0.31**	0.47**	n.a.
Female	0.39	1.55	n.a.	0.61	0.36	n.a.
Black	-0.21	0.62	n.a.	0.97	2.57	n.a.
(c) **Card**						
All	-0.56	0.18	-0.16	0.43	-0.84	-0.27
(d) **Time Series**						
All	n.a.	n.a.	-0.29	n.a.	n.a.	n.a.

Notes:
(1) Estimates with # are based on underlying coefficients significant at the 10% level; *: 5% level; **: 1% level.
(2) Estimates in panel (a) are employment elasticities with respect to a change in the minimum wage, evaluated at the average employment rate in 1995, using growth- and state-adjusted employment changes.
(3) Estimates in panels (b) are employment elasticities with respect to a change in the minimum wage, evaluated at the average employment rate in 1995.
(4) Estimates in panel (c) are employment elasticities with respect to a change in the average wage corresponding to a given change in the minimum wage.
(5) Estimate in panel (d) is the employment elasticity with respect to a 10% increase in the Kaitz index over the period 1954-97.

Economists interpret the assumption that there are many small employers and many individual workers as a guarantee that neither employers (through employers' organizations) nor workers (through unions) can collude to set wages. In practice, the assumption that these economic actors are small relative to the size of the market also means that no one individual or firm can control or even influence the market price or wage. In more technical terms, all actors are "price-takers," rather than "price-setters."

The logic behind the conclusion that individual actors are powerless "price-takers" provides some insight into the way economists use the simple competitive model to draw economic conclusions and to evaluate policies such as the minimum wage. Imagine that a small firm thought that it could increase its profit by paying its workers a lower wage. If the firm were to lower its wage by 10%, then the competitive model predicts that the employer would immediately lose all its workers and be forced to close its doors. According to this model's assumptions, workers have perfect, instantaneous knowledge of the market. They can switch jobs instantly and without cost. Firms also have perfect, instantaneous knowledge of the market and can hire new workers instantly and without cost. In this "competitive" market, the individual firm's decision to lower its wages by 10% immediately triggers a chain of events: the offending firm's workers quit; competitors poach the firm's workers by offering the going wage; and the competitors gobble up the now bankrupt firm's market share.[27]

According to this model's assumptions, workers have perfect, instantaneous knowledge of the market.

Imagine, on the other hand, that a different small employer, motivated by a desire to improve the living standards of its workers, decided to give its staff a 10% pay increase. As with the preceding example of a pay cut, the logic of the competitive model predicts imminent bankruptcy for the well-intentioned firm. If the firm attempts to pass on the 10% increase in wage costs to its customers in the form of higher prices, then competitors paying the going wage — and thus who have not experienced an increase in costs — will continue to sell the product at the old price and capture the more expensive firms' share of the market.[28] If, instead, the firm decides to pay higher wages but continues to sell its product at the going rate, the firm's lower return will drive it out of business.

In such a market, the introduction of a minimum wage unambiguously lowers employment. Again, the logic is simple. Before the minimum wage goes into effect, individual employers can hire as many workers as they would like at the going wage;[29] similarly, all workers can effortlessly find employment at the going wage. In such a world, firms operate without vacancies (or they are posted and filled instantaneously); workers never experience involuntary unemployment (they move instantaneously and without loss from school to work or from one job to the

next). After the imposition of a binding minimum wage, however, firms would reduce employment until the point where the productivity of the worker next in line to be fired just equaled the new minimum wage.[30] At the same time, workers who previously worked for less than the minimum wage would still like to work at the new minimum wage, but can't, and would now become involuntarily unemployed.

While we have several quarrels with this view of low-wage labor markets (which we will address below), we believe that it is important to attempt to explain our employment results in the context of this model. Given the unambiguous predictions of the simple, competitive model, how is it that the employment results generally appear to be economically small and statistically insignificant? How is it that the employment results appear almost as likely to suggest employment gains as employment losses after the two minimum wage increases studied here went into effect? We examine three possible "competitive" model explanations.

The data we presented above examine employment losses over periods ranging from six to 17 months, which should be more than enough time to observe any significant job losses.

Explanation One: Short-Run Effects Versus Long-Run Effects

The first possibility is that we have not waited long enough to observe the employment declines precipitated by the minimum's increase. For a variety of reasons, firms may not be able to adjust their employment levels quickly. In the short run, before firms can change their investment in plants and machinery or reorganize the way work is done, they may not be able to alter their employment patterns significantly. Trucks, for example, require one driver each. In the short-run, a low-wage trucking firm would face little choice but to absorb the higher wage costs. In the long run, however, the firm could buy larger trucks that would allow it to ship the same amount of material in fewer trucks with fewer drivers.

We are skeptical, though, that this short-run versus long-run issue is important here. The data we presented above examine employment losses over periods ranging from six to 17 months, which should be more than enough time to observe any significant job losses. First, as Table 1 indicates, most of the jobs affected by the minimum wage are in the service sector, where the capacity for quickly adapting production processes is probably much greater than in more capital-intensive manufacturing industries.

Second, the level of job turnover among low-wage workers is high — probably on the order of 50% per year.[31] This high turnover rate means that a firm with 100 jobs will, in the course of a year, have 150 different employees. For simplicity's sake, imagine that these workers leave in equal shares during each month of the year (about 4% per month). Even if the employment losses are at the high end of those

predicted by the harshest critics of the minimum wage — that a 10% increase in the minimum wage reduces employment 3% — firms could adjust employment fully in only a few months. For example, the 50-cent increase in the minimum wage on October 1, 1996 represented an 11.8% increase. Using the extreme estimates for job loss, employment would have fallen about 3.5%, which amounts to less than one month of natural employment turnover. Even if job turnover were implausibly low — say, just 10% per year — the necessary employment adjustment would take only four to five months.[32] This analysis suggests that we can reasonably assume that if the recent minimum wage increases did lower employment, the 17 months of data analyzed here would be more than sufficient to observe these reductions.

Even if job turnover were implausibly low — say, just 10% per year — the necessary employment adjustment would take only four to five months.

Explanation Two: Adjusting Hours, Not Jobs

Even if the competitive model adequately captures the workings of the low-wage labor market, we still might observe little change in employment after the minimum wage goes up because firms may choose to reduce the hours worked by their existing employees rather than reduce their total number of employees.[33] At one level, this makes perfect sense in the context of the competitive model. After all, it is the cost of an hour of work, not the cost of hiring an individual worker, that has risen. Firms should seek to reduce their use of the input whose price has risen (hours of work), not their use of an input (keeping an employee on the roster) whose price, in and of itself, has not changed.

If firms adjust hours instead of employment levels, then the preceding tests for job impact would not capture the true economic impact of the minimum wage increase. It is also true, however, that under such circumstances the increases in the minimum wage would have had an unambiguously positive impact on the affected workers. To illustrate, imagine again that the "job losses" were at the high end of those proposed by opponents of the minimum wage. A 10% increase in the minimum wage would reduce hours (not employment) by about 3%. After a 20% increase in the minimum wage, each minimum wage worker would have hours cut about 6% (this represents about one hour per week on a 20-hour week). Meanwhile, each minimum wage worker would receive 20% more pay for each remaining hour worked (in this example, 20% more for the remaining 19 hours worked). On a weekly basis, workers would receive 14% more in income for 6% fewer hours on the job, a development that few workers would oppose.[34]

The usual objection to the idea that firms would reduce hours rather than employees is that firms face "fixed costs" for keeping workers. These fixed costs make it more economical to concentrate the reduction in hours worked on a limited number of employees. This allows firms to reduce the total number of hours to the

newly desired level while, at the same time, reducing the fixed costs associated with keeping an employee on the roster (administration, hiring, training, benefits, etc).

The fixed-cost argument is intriguing and raises several subtle issues. First, fixed costs are inconsistent with the simple competitive model, which assumes that firms can hire any number of hours of work as long as they pay the going wage. With fixed costs, firms must pay the going rate for hours worked, plus a cost for each additional worker. To the extent that these fixed costs are low, firms will be more likely to respond to increases in the minimum wage with reductions in hours rather than workers, with the resulting unambiguous improvement for workers.[35] To the extent that fixed costs are high, the competitive model may not adequately reflect the workings of the low-wage labor market, a possibility that we will discuss below when we examine an alternative model of the low-wage labor market.

To the extent that these fixed costs are low, firms will be more likely to respond to increases in the minimum wage with reductions in hours rather than workers, with the resulting unambiguous improvement for workers.

Second, the fixed-cost argument may overstate the net costs of keeping a worker on the payroll because the argument ignores the potential *benefits* to low-wage employers of keeping a large roster of employees, particularly in service-sector industries with large fluctuations in demand. Given that low-wage workers may be less committed to their jobs than workers at higher wages, keeping extra low-wage workers on the roster (at fewer hours than they would like to work) reduces staffing crunches when a current worker calls in sick or quits. A large roster in a setting where many workers would like to work more hours at the going wage may also allow employers to motivate "good" workers by providing them with extra hours or to punish "bad" workers by withholding those extra hours. If some low-wage employers use these or similar strategies, the net fixed costs of keeping employees on the roster may be small. As before, this suggests that firms may be more likely to respond to minimum wage increases by reducing hours rather than by reducing employment, a response that would leave workers unambiguously better off because, over plausible ranges, workers' weekly incomes would rise even as their hours worked fell. In any event, the possibility that firms could employ these kinds of strategies implies a substantial departure from the assumptions guiding the competitive model.

Explanation Three: Small Effects

Even in the context of a competitive model, we might notice no systematic decline in employment for a third reason: the employment effects may be too small to be reliably detected by the available data, or at least small relative to the other factors that determine low-wage employment. These other determinants of low-wage employment include overall demand in the economy (whether the economy is boom-

ing or in a recession), specific demand for low-wage workers (which depends on, among other things, the changing nature of technology and the organization of work), changes in the supply of low-wage workers (in response, for example, to changes in welfare policies or immigration levels), and other factors relevant to particular labor markets.

All the tests we presented in the previous section attempt to control for what is probably the most important determinant of low-wage employment — the level of overall demand in the economy. That the results seem to vary substantially across recessions and booms suggests, however, that these controls might not be adequate.[36] The tests generally do not control explicitly for other factors such as changes in the relative demand for, or supply of, low-wage workers.[37] The assumption behind all the tests is that these other factors changed little over the six- to 17-month period under study and therefore should not contaminate the results. While other economic research suggests that this widely used assumption is reasonable, the volatile nature of the employment estimates raises some doubts.

If the minimum wage effects are too small to measure reliably or are small relative to other determinants of low-wage employment, then we probably should base conclusions about the employment effects of the minimum wage on a wide range of tests implemented in a variety of economic conditions. In fact, in addition to the four tests presented here in connection with the 1996-97 increases, a large body of research over the last decade has consistently found no significant connection between the minimum wage and job loss.[38] Without abandoning the competitive model of the labor market, Nobel-prize-winning economist Robert Solow has concluded that "the main thing about this research is that the evidence of job loss is weak. And the fact that the evidence is weak suggests that the impact on jobs is small."[39]

The competitive model, which assumes that all employers and workers participate in the labor market on an equal footing and with perfect information about the nature of the market, is obviously unrealistic.

A New Model: Dynamic Monopsony

The competitive model, which assumes that all employers and workers participate in the labor market on an equal footing and with perfect information about the nature of the market, is obviously unrealistic. That the model is unrealistic, however, is not, in and of itself, a problem. In fact, it is precisely the unrealistic nature of models that makes them useful. A road map of Virginia, for example, is extremely unrealistic: it is completely flat and measures only a few square feet, but these are also exactly the features that make it a useful tool. A map the size of the entire state, complete with the Blue Ridge mountains, would not fit in your glove compartment. As with a road map, any model should be judged not on how superficially "realistic" it is but, rather, on how well it helps you to find your way.

Our findings on the employment effects of the 1996-97 increases in the minimum wage, which are consistent with a large body of recent research on the topic, argue that the competitive model has not been particularly helpful in predicting the job impact of the minimum wage.[40] Research over the last decade suggests that one particular assumption of the competitive model may play a central role in producing the less-than-satisfactory results: that firms paying less than the going wage can retain or attract no workers, while firms paying the going wage can hire all the workers they desire and therefore never see any need to pay above the going wage.[41]

Over the last decade, many economists have incorporated into otherwise standard economic models the assumption that small, identical firms have some discretion in the wages that they pay.

This assumption about the supply of labor to individual firms, like many others made by the competitive model, is unrealistic. As economist Alan Manning has noted, a firm that lowers its hourly wage by one cent per hour is very unlikely to lose its entire workforce instantly.[42] Workers cannot easily find identical, alternative employment at the going wage and, even if they could, they would not be able to make the switch without incurring some personal or economic cost. Likewise, a firm that pays above the going wage is not doomed to bankruptcy. The higher wage may facilitate recruitment, thereby lowering associated costs of keeping vacancies open, or it may reduce turnover, thereby lowering training costs. The higher wage could also motivate workers, resulting in increased productivity. The competitive model also fails to take into account that firms paying the going wage often are not able to hire all the workers they would like at the going wage.[43]

This more realistic view of the labor supply available to individual firms suggests that firms have some discretion in setting their wages. Among otherwise identical firms, some may choose to pay low wages and incur higher recruitment, training, or supervising costs, while others might choose to pay higher wages and enjoy lower indirect labor costs. In the context of the minimum wage debate, there are two key questions: how does altering the assumption about firms' discretion in wage setting change conclusions about the economic impact of the minimum wage, and how important are any of these theoretical effects in the real world?

Over the last decade, many economists have incorporated into otherwise standard economic models the assumption that small, identical firms have some discretion in the wages that they pay.[44] Almost all of these models, which have come to be known as "dynamic monopsony" models, make predictions about the employment impact of the minimum wage that differ significantly from the simple competitive model.[45] In these new models, an increase in the minimum wage over certain ranges can increase both the wages and employment of low-wage workers. Once the minimum wage moves above a certain level, however, these new models, like the simple competitive model, also predict that the employment levels of low-wage workers will fall.[46]

The rationale behind these predictions is fairly straightforward. Imagine the case of a firm that currently pays below the proposed new minimum wage. That firm will have a higher vacancy rate, higher quit rate, higher training costs, and possibly lower average productivity than a competitor already paying the new minimum wage rate. The proposed increase in the minimum wage would lower the vacancy rate, lower the quit rate, reduce training costs, and possibly increase average productivity at the low-wage firm. Direct labor costs will be higher, but indirect labor costs related to recruitment, training, and supervising workers will be lower because vacancies will be easier to fill, turnover will be lower, and work effort higher at the new wage. To illustrate this idea, take the simple case of a small, low-wage firm with 20 employees that generally operates with two vacancies. After a minimum wage increase, the firm may find it easier to recruit new workers and to retain existing ones. After the minimum wage increase, the firm may now operate with 21 employees and just one vacancy.

Our empirical findings suggest that the low-wage labor market is still below the critical level where moderate increases in the minimum wage would result in significant job losses.

Of course, if the minimum wage is set too high, then employment would fall even in a labor market characterized by dynamic monopsony. Beyond a critical value of the minimum wage, the increases in the direct labor cost exceed the reductions in indirect labor costs and employment falls. Unfortunately, economic theory gives us little practical guidance about this critical level of the minimum wage, and we must therefore fall back on the empirical analysis of the effect that past minimum wage increases have had on employment. Our empirical findings with respect to the 1996-97 increases, and research by many others looking at the 1990-91 and earlier increases, suggest that the low-wage labor market in the United States is still below the critical level where moderate increases in the minimum wage would result in significant job losses.

Yet one question remains. If firms can lower costs by raising wages, then why don't they raise wages on their own, without a government-mandated increase in the minimum wage? In labor markets characterized by dynamic monopsony, firms are effectively free to choose their own business strategy. Firms have no such latitude in a perfectly competitive market, as illustrated in the earlier examples of competitive firms seeking to lower or raise their workers' pay. Monopsonistic firms can choose the "low road" (with low wages and high indirect costs) or the "high road" (with high wages and low indirect costs).[47] The formal modeling process reveals that, thanks to the offsetting nature of the direct and indirect labor costs, both the low and the high road are equally profitable to firms.[48] Under monopsonistic conditions, the unfettered market allows some firms to choose business strategies that pay wages and set employment at levels below what is socially optimal and economically efficient. Under monopsonistic conditions, however, not all

firms choose low-wage business strategies. In this context, a minimum wage increase simply compels firms to choose the "high road" rather than the "low road."

The dynamic monopsony model, which captures the direct and indirect costs facing employers, is theoretically more appealing than the cruder competitive model. More importantly, the dynamic monopsony model makes predictions about the effects of the minimum wage that are more consistent with the decidedly mixed empirical results summarized in Table 11 and reported in the large body of recent research. Competitive models predict unequivocally that minimum wage increases will reduce the employment opportunities for low-wage workers. Dynamic monopsony models allow for the possibility that moderate increases in the minimum could raise employment, leave it unchanged, or, in the case of a large enough increase, lower employment.

The dynamic monopsony model, which captures the direct and indirect costs facing employers, is theoretically more appealing than the cruder competitive model.

The competitive model predicts that, *when all else is equal*, raising the price of labor will reduce the demand for labor. The dynamic monopsony model realistically incorporates the possibility that all else is *not* held equal, especially when raising the minimum wage can reduce employer costs in other dimensions. Economic theory indicates that incorporating these small doses of reality into formal economic models can lead to radical changes in the minimum wage's impact on employment opportunities in the low-wage labor market. The empirical evidence of small, even occasionally positive, employment effects of the minimum wage suggests that new models based on dynamic monopsony are more consistent with reality than traditional models based on competitive assumptions. Taken together, the new theoretical models and the weight of empirical evidence over the last decade argue for a serious re-evaluation of the way we think about the low-wage labor market and the effects of moderate increases in the minimum wage.

CONCLUSION

Even though our economy as a whole has grown over the past few decades, low-wage workers have fallen far behind. Over the 1980s, this problem was in large part generated by the nine-year decline in the real value of the minimum wage. Congress has acted to reverse this decline by raising the minimum wage twice in the 1990s, restoring the wage floor to 82% of its 1979 value.[49] These increases have predictably led to heated debates over whether they would achieve the intended goal of lifting the wages and incomes of low-wage workers and their households. In this review of the impact of the 1996-97 increase, we find that the policy is working — the increase has raised the earnings of low-wage workers without leading to significant job losses.

This review of the impact finds that the policy is working — the increase has raised the earnings of low-wage workers without leading to significant job losses.

Those who benefited from the increase were mainly low-wage adult workers, most of whom were female. Close to half (46%) worked full time, while another third worked at least 20 hours per week. The average minimum wage worker brought home over half (54%) of his or her family's weekly earnings, and most of the benefits from the increase were concentrated among low-income working households.

We ran a wide battery of tests to determine the employment effect of the increase, focusing on the employment rates of two groups of low-wage workers: teenagers and adults with less than a high school degree. Our findings were for the most part statistically insignificant, suggesting that the increase had no effect on the job prospects of these two groups. Not only are the estimates from these employment tests generally small and statistically insignificant, but they are also almost as likely to be positive as negative and vary unpredictably across demographic groups. One of the tests of the employment effects, initially proposed by minimum wage opponents Deere, Murphy, and Welch, shows the net employment of affected groups *increasing* after the minimum wage was raised. Our update of the traditional method for estimating minimum wage employment effects (time-series models) reveals a statistically insignificant effect, and one that is less than half the magnitude of even the earlier low-end estimates suggested.

How do we interpret these varied results from the employment tests? Our findings certainly fail to support the traditional economic model that unequivocally predicts job loss from a minimum wage increase. With this in mind, we offer a different and, in our estimation, more realistic view of how low-wage labor markets respond to moderate minimum wage increases. This view stresses the dynamic nature of low-wage sectors, incorporating various inefficiencies, such as high turnover and vacancies, into the model. In this model, minimum wage increases of the magnitude of the 1996-97 increase have the potential to reduce such inefficiencies, thus increasing employee pay without lowering employment.

DATA APPENDIX

Table 1: Characteristics of Minimum Wage and Other Workers

Except for the addendum, the data from this table come from the CPS ORG as described in a section below (see *Description of Wage Data*). The period of observation, October 1995-September 1996, was chosen since it is the year before the first step of the minimum wage increase. Thus, the affected group includes those workers who earned between the old minimum of $4.25 and the new minimum of $5.15. As in the rest of this report, and unlike the BLS's classification system, Hispanics are an exclusive category.

For the addendum (which shows the share of weekly earnings contributed by minimum wage workers) we use the 1997 March CPS as described in the next section. The March database is used here because it offers a more reliable set of family identifiers than the ORG, enabling us to sum family weekly earnings among households with an affected worker. Note that since the first step of the increase had been implemented by March of 1997, the affected range at that time was $4.75-$5.14. We use usual weekly earnings in March of 1997 from the outgoing rotation group in the March data for the calculations in this part of the table.

Table 2: Distribution of Gains From the Increase

These data are derived from the March 1997 Current Population Survey (CPS). The March CPS includes a retrospective supplement wherein respondents are asked to provide information on the prior year's income. We took hourly wage data, however, from the March ORG, since we judged these wage data to more accurately reflect the wage structure over the period of interest. Information on annual income, however, is from 1996.

At the time these data were collected in March 1997, the federal minimum wage was set at $4.75. Thus, the affected range was defined as $4.75-$5.14. We first calculated the hourly increase as a result of raising the minimum to $5.15, as would occur on September 1, 1997. We then multiplied this value by their annual hours worked in 1996. This product represents the annual gains from the increase (note the assumption of no hours reduction). We then divided households into quintiles based on the annual income, with an equal number of persons (not households) in each quintile. Finally, we calculated the share of income and wage gains that accrued to each quintile.

As noted in the text, the table focuses on households headed by a prime-age (25-54) person. In order to test the sensitivity of this restriction, we examined the distribution of all working households (again, including one-person units). These results are shown in Appendix Table 2. The gains of the increase are slightly more concentrated at the bottom of the income scale, as might be expected since we have now included households headed by younger persons, who typically have lower earnings and incomes than prime-age households. Nevertheless, the distributional results are quite similar, with the gains from the increase concentrated among working households in the bottom 40% of the income distribution.

Tables 3 and 4: Difference-in-Differences

The data for this table come from the basic monthly files of the Current Population Survey (CPS), the primary source of labor market microdata on employment, unemployment, wage rates, and demographics. Each month the Census Bureau, under contract with the Bureau of Labor Statistics (BLS), collects nationally representative data from approximately 50,000 households. Sample weights, which represent the inverse of the probability that a person with specific demographic and geographic characteristics will be sampled, are provided so the data can be "weighted up" to national levels. These data provide the basis for the monthly unemployment rate, released by BLS on the first Friday of each month.

The employment rates in Table 4 are calculated from the weighted sample over the time periods shown. For example, the 47.6% employment rate for teenagers in column 1 represents the average weighted employment levels from April to September 1995 divided by the average weighted population level over that same period. One advantage to using the basic monthly files is that their sample sizes are large enough to allow for fairly reliable disaggregation by demographic group. For example, there was an average of around 7,000 unweighted cases per month of 16-19-year-olds, with about 5,000 white, slightly less than 1,000 African Americans, and 750 Hispanic teens. (Note that our racial/ethnic classifications, unlike those of BLS, are exclusive. In BLS data, Hispanics can be in other racial categories; in our data, they are in an exclusive category.)

Standard Errors: Large sample sizes are considered advantageous in quantitative analysis because statistical reliability rises with the size of the underlying sample. Nevertheless, any sampled data is prone to sampling error,

represented by standard errors in the second to last column in Tables 3 and 4.

Given the nonrandom sampling framework of the CPS, the calculation of standard errors is not straightforward.[50] The calculation of the most reliable standard errors would call for the use of special sampling weights that are not included on the public use file. BLS publishes such standard errors for many of its estimates, but these rarely correspond to either our time frame or demographic groups. Therefore, we needed to derive an efficient and reliable method for estimating standard errors for the many categories of low-wage workers that we examine.

We began by using a slightly altered version of the general formula for the standard error of a rate:

$$\sqrt{(p_{wt}*(1-p_{wt}))/n_{unwt}}$$

p in this case is the weighted rate, while *n* is the unweighted average of the population in question over the time period. Averages, instead of sums, are appropriate because 75% of the cases in the monthly CPS are in the sample one month to the next, due to the sample rotation procedure. We adjust this value by a factor provided by BLS for data over longer time periods. This factor, from various tables in the appendix to the monthly publication *Employment and Earnings*, deflates the standard error slightly due to the independence of 25% of the monthly cases. By calculating rates and standard errors for some of the same groups of persons published in *Employment and Earnings*, we determined that this method was acceptable for levels.

The same publication allows us to calculate BLS's standard errors for changes, and these revealed that our approach tended to overestimate standard errors of differences and thus introduce the possibility of incorrectly rejecting significant findings. In order to avoid this possibility, we further adjusted the standard errors of the differences by the ratio of the BLS standard errors of differences to our previously adjusted standard errors. Including both the deflation factor noted above (which lowers the standard error due to the inclusion of more independent cases) and the correction for the overestimate just noted, we derived an adjustment factor of 0.781.[51]

Adjusting for Overall Growth and State Effects: As discussed in the text, we attempt to extract the effect of overall economic growth on our estimate of the impact of the minimum-wage increase on employment rates. The adjusted value appears in column 8 of Table 4. We produced this adjustment by regressing the time series of employment rates (prior to the increase) for the various groups in the table on the employment rates of workers 25-54 years old (by gender), a trend variable, and dummies for the summer months (since the employment data were not seasonally adjusted). We then multiplied the coefficient on the 25-54 variable by the change in those adults' employment rates over the periods in question, and subtracted this value from the raw estimate in column 7 of Table 4. Since the growth in the employment rates of adults is a good proxy for overall labor market growth, this procedure should remove the effect of such growth.

In order to adjust for state effects (the different impact of state economies on the employment rates of teens and young adults), we estimated a weighted linear probability model (1 = employed, 0 = not employed) for the CPS microdata with dummy variables for each demographic group in each time period and for 50 of the 51 states including the District of Columbia. Without a constant term, the coefficients on the dummies for each demographic group in each time period represent the employment rate of that group in that time period, controlling for "state effects."

Time-Series Estimates

The models in this section regress the teenage employment rate on the logged Kaitz index and a set of controls, using quarterly, non-seasonally adjusted data from 1954-97. For 1954-93, we use the data set used by Card and Krueger (1995, Chapter 6); for 1994-97, we updated the available variables (as noted below, all variables in Card and Krueger Table 6.5.A were updated, except the share of teenagers in the armed forces, which is not available after 1993).

The Kaitz index, as described in the text, is a coverage-weighted estimate of the nominal minimum wage relative to the average production-worker wage. The index is also weighted by teenage employment density by industry, as well as by industry coverage rates (see Wellington 1991). While the Department of Labor recently released minimum wage coverage rates through 1996, the methodology used for these recent estimates is somewhat different than that of the earlier coverage estimates (see U.S. DOL 1998, Appendix C). The new methodology leads to a significantly lower level of the Kaitz index in the one year (1990) for which both methodologies exist. We adjusted the level between 1993 and 1994 so as to account for this difference.[52]

The control variables in the model include the (logged) adult male unemployment rate, the fraction of teenagers aged 16-17, and the log of the teenage share of the population. We omit the share of teenagers in the armed forces as

a control variable, because such data are unavailable post-1993. Since this variable is insignificant (and has little impact on the Kaitz coefficient) in models prior to 1993, we are not concerned about its omission. The conventional model (Figure 2) uses the same trend and seasonal controls as in Wellington (1991), i.e., seasonal dummies, trend, trend squared, both of which are interacted with the seasonal dummies.[53] These models include an AR(1) term, estimated with a nonlinear least squares algorithm using the Marquardt method.[54]

As noted in the text, Wolfson (1997) raises a number of concerns regarding this specification. First, as he and other have shown (see also Williamson and Mills 1997), some of the variables in the regression are nonstationary. As contemporary time-series literature emphasizes, not accounting for this problem leads to spurious regression results (see, for example, Enders 1995). A key result of Wolfson (who examines the series for 1954-79 only) is that the dependent variable contains a unit root (i.e., is nonstationary).[55] Williamson and Mills, however, strongly reject this hypothesis. We ran various specifications of the Augmented Dickey-Fuller unit test for the full period and found that some specifications rejected the null hypothesis of a unit root while others failed to reject.

Since the ADF test is not a particularly powerful test, we hesitate to accept the null that teenage employment rates are nonstationary (the time plot confirms our ambivalence). However, a number of factors support the Wolfson specification. If we assume that logged teenage employment rates are I(0), that is, stationary in levels, and run the model with the dependent variable in levels, the residuals are clearly serially correlated. Finally, note that in these other specifications, the coefficient on the differenced logged Kaitz index was similar to those in Table 10 (i.e., close to 0 and insignificant).

Unlike the teenage employment rate, ADF tests on the logged Kaitz, the adult male unemployment rate, and the log of the teenage share of the population are unable to reject the null of a unit root, and thus these variables are entered as first differences. The other variables in this alternative model are the fraction of teenagers age 16-17, and, as an additional seasonal control, the fourth lag of the dependent variable. Residuals from these models were serially uncorrelated as confirmed by both inspections of their correlograms and Breusch-Godfrey tests (which, for each time period, failed to reject the null hypothesis of no serial correlation).

Tables 1A-C: Description of Wage Data

This study uses wage data from the Outgoing Rotation Group (ORG) of the Current Population Survey (CPS). The ORG sample consists of one-fourth of the people responding to the full monthly sample of the CPS (the data source for our examination of employment changes; see the notes for Table 2). Our sample includes all employees 16 years of age or older who earned between $0.50 and $100.00 per hour (in nominal dollars).

Hourly earnings include tips, overtime, bonuses, and commissions for each worker. Calculation of hourly earnings involves several steps designed to cope with problems of (1) variable hours, (2) topcoding, and (3) tips, overtime, bonuses, and commissions. A description of the full procedure follows.

All non-topcoded, hourly paid workers who say that they do not receive tips, overtime, bonuses, or commissions are given the usual hourly wage from their survey response. All non-topcoded workers who are not paid by the hour are assigned an hourly wage calculated as their usual weekly earnings including tips, overtime, bonuses, and commissions divided by their usual number of hours worked per week.

Usual Hours: Beginning in 1994, the CPS allowed respondents to indicate that they did not have a "usual number of hours worked per week." Since the calculation in this study of hourly earnings for salaried employees and for hourly paid employees who receive tips, overtime, bonuses, and commissions depends on dividing usual weekly earnings by usual hours worked, we have been forced to estimate usual hours worked per week for about 7% of the sample who report that their hours vary. When necessary, we predict usual weekly hours for those reporting variable hours by using the results from a regression equation on the sample (by year, by sex) that reports usual and actual hours worked in the previous week. Specifically, we regress usual hours worked per week on actual hours worked in the reference week and full-time status, a dummy variable for married, five dummy variables for educational attainment, a quartic in age, three dummy variables for race and ethnicity, and 10 dummy variables for broad occupation (all interacted with full-time status). We then use the coefficients from this equation to estimate usual weekly hours for those workers who say that their hours vary.

Tips: About 20% of hourly paid workers received tips, overtime, bonuses, or commissions as part of their regular earnings. Since many workers who may have been affected by the minimum wage worked in industries where tipping is common, we have been careful to include these earnings in our calculation of usual hourly rates. The CPS survey first asks hourly paid workers to report their hourly wage excluding tips, overtime, bonuses, and commissions. Later,

it asks if hourly paid workers receive tips, overtime, bonuses, or commissions. If these workers answer yes, then the survey asks for the worker's usual weekly earnings including tips, overtime, bonuses, and commissions. In these cases, we use the usual weekly earnings figure, divided by the usual hours worked per week (reported or estimated as above) to estimate the usual hourly earnings including tips, overtime, bonuses, and commissions.

APPENDIX TABLE 1A
Distribution of Wages by Demographic Characteristics, April 1996-September 1996

	Percent of Employees Earning						
Type of Employees	$0.50-$4.24	$4.25	$4.26-$4.74	$4.75	$4.76-$5.14	$5.15+	Number in Sample
All	2.1%	1.5%	2.1%	0.5%	4.7%	89.1%	75,732
Teenagers (16-19)	6.8	10.1	10.5	2.7	20.2	49.7	4,482
Male	5.4	9.1	10.5	2.5	19.7	52.6	2,270
Female	8.3	11.1	10.4	2.9	20.7	46.6	2,212
White	6.6	9.2	9.4	2.6	20.1	52.1	3,608
Male	5.3	7.8	9.2	2.7	19.8	55.2	1,817
Female	7.8	10.7	9.7	2.5	20.5	48.8	1,791
Black	8.6	14.4	16.8	4.3	18.6	37.3	359
Male	5.9	18.6	22.1	2.4	14.5	36.5	169
Female	11.2	10.4	11.7	6.1	22.6	38.1	190
Hispanic	5.6	12.4	14.2	2.3	22.2	43.3	366
Male	4.8	10.0	10.8	1.6	22.9	49.9	220
Female	6.9	15.9	19.2	3.3	21.2	33.5	146
Less Than High School (20-54)	4.4	3.3	4.1	1.2	8.9	78.2	5,639
Male	3.4	2.4	3.0	1.0	7.4	82.8	3,515
Female	6.2	4.7	5.9	1.6	11.4	70.1	2,124
White	3.5	1.8	3.2	0.7	6.4	84.3	2,818
Male	2.3	1.3	2.1	0.5	4.0	89.8	1,745
Female	5.6	2.7	4.9	1.2	10.5	75.1	1,073
Black	3.5	4.0	5.3	2.4	11.9	72.9	669
Male	2.8	3.2	2.9	2.5	9.6	79.0	351
Female	4.4	5.2	8.5	2.1	15.0	64.7	318
Hispanic	5.3	4.9	4.7	1.4	10.4	73.2	1,850
Male	4.6	3.7	4.2	1.1	9.9	76.5	1,260
Female	7.1	7.8	5.9	2.1	11.6	65.6	590

Source: EPI analysis of CPS ORG.

APPENDIX TABLE 1B
Distribution of Wages by Demographic Characteristics, October 1996 to March 1997

	Percent of Employees Earning						
Type of Employees	$0.50-$4.24	$4.25	$4.26-$4.74	$4.75	$4.76-$5.14	$5.15+	Number in Sample
All	1.9%	0.4%	1.1%	1.6%	4.4%	90.6%	76,745
Teenagers (16-19)	5.1	2.9	5.4	10.5	23.1	53.0	4,094
Male	4.2	2.7	4.7	10.3	22.7	55.4	2,034
Female	6.1	3.2	6.0	10.6	23.5	50.6	2,060
White	4.8	2.6	5.6	9.8	22.9	54.3	3,267
Male	3.9	2.0	5.2	9.6	22.6	56.7	1,637
Female	5.6	3.2	6.0	10.1	23.2	51.9	1,630
Black	7.1	5.2	8.1	11.5	27.2	40.9	336
Male	5.9	5.9	6.1	10.7	28.8	42.6	137
Female	8.1	4.7	9.6	12.0	25.9	39.6	199
Hispanic	6.3	3.3	2.3	13.9	23.3	51.1	345
Male	4.3	4.2	1.3	14.3	22.4	53.4	197
Female	9.0	1.9	3.6	13.4	24.4	47.7	148
Less Than High School (20-54)	3.6	0.8	2.7	3.5	9.0	80.3	5,655
Male	2.6	0.4	2.1	2.0	7.0	85.9	3,393
Female	5.3	1.5	3.9	6.1	12.3	71.0	2,262
White	3.3	0.5	2.0	2.3	7.3	84.7	2,775
Male	2.1	0.1	1.4	0.6	4.8	91.0	1,642
Female	5.1	1.1	2.9	4.8	11.2	75.0	1,133
Black	4.6	1.1	3.5	4.6	9.4	76.8	684
Male	4.1	0.7	2.3	1.4	8.6	82.9	336
Female	5.3	1.6	4.8	8.2	10.2	69.9	348
Hispanic	3.4	1.1	3.5	4.8	10.5	76.7	1,914
Male	2.6	0.7	2.8	3.7	8.6	81.6	1,268
Female	5.2	1.8	5.0	7.1	14.7	66.1	646

Source: EPI analysis of CPS ORG.

APPENDIX TABLE 1C
Distribution of Wages by Demographic Characteristics, September 1997 to February 1998

	Percent of Employees Earning						
Type of Employees	$0.50-$4.24	$4.25	$4.26-$4.74	$4.75	$4.76-$5.14	$5.15+	Number in Sample
All	1.5%	0.1%	0.6%	0.2%	2.3%	95.3%	74,267
Teenagers (16-19)	3.8	0.5	1.5	1.5	10.6	82.1	4,223
Male	3.8	0.6	1.5	1.2	10.4	82.4	2,122
Female	3.9	0.5	1.5	1.7	10.8	81.7	2,101
White	3.7	0.3	1.5	1.3	10.2	83.0	3,364
Male	3.7	0.5	1.3	0.9	10.2	83.3	1,703
Female	3.7	0.2	1.6	1.6	10.1	82.8	1,661
Black	3.1	2.4	1.8	1.3	15.1	76.4	336
Male	2.3	2.1	4.0	1.8	16.5	73.2	153
Female	3.6	2.7	0.0	0.9	13.9	78.9	183
Hispanic	4.5	0.5	1.4	1.8	8.7	83.1	380
Male	3.0	0.6	1.2	1.1	7.5	86.6	204
Female	6.6	0.4	1.7	2.9	10.4	78.1	176
Less Than High School (20-54)	2.8	0.3	1.1	0.5	5.4	89.8	5,715
Male	2.0	0.2	1.2	0.3	4.8	91.5	3,449
Female	4.2	0.5	1.1	0.8	6.4	87.1	2,266
White	3.0	0.2	0.9	0.3	3.2	92.3	2,674
Male	2.1	0.0	1.1	0.2	2.8	93.7	1,632
Female	4.5	0.5	0.6	0.3	3.9	90.1	1,042
Black	3.3	0.5	2.2	0.3	6.5	87.4	679
Male	3.0	0.1	2.3	0.0	6.4	88.2	322
Female	3.5	0.9	2.1	0.5	6.6	86.5	357
Hispanic	2.2	0.3	1.0	0.9	7.2	88.4	2,041
Male	1.6	0.4	1.0	0.6	6.4	90.0	1,338
Female	3.6	0.1	1.1	1.6	8.7	84.8	703

Source: EPI analysis of CPS ORG.

APPENDIX TABLE 2
Distribution of Minimum-Wage Gains and Income Shares by Fifth, All Working Households

Income Quintile	Share of Gain From Increase	Share of Income	Average Income
1	40.9%	5.0%	$14,133
2	19.8	10.7	30,451
3	17.5	16.0	45,605
4	10.9	22.6	64,468
5	10.8	45.7	130,518

Source: 1997 March CPS

APPENDIX TABLE 3
Times Series Regressions Basic Models: Dependent Variable Is Log Teenage Employment Rates

	1954-79		1954-89		1954-97	
	Coefficient	t-Statistic	Coefficient	t-Statistic	Coefficient	t-Statistic
Log Kaitz Index	-0.095	-2.389	-0.075	-1.677	-0.066	-1.714
Log Adult Unemployment Rate	-0.108	-6.197	-0.100	-5.120	-0.092	-5.269
Fraction of Teens 16-17 Years Old	-1.147	-3.000	-0.873	-1.862	-0.951	-2.214
Log Teen Share of Population	-0.601	-2.211	-0.454	-1.315	-0.068	-0.419
AR(1)	0.730	9.639	0.903	26.294	0.901	27.569
Adjusted R-squared	0.976		0.966		0.966	
Durbin-Watson	1.97		2.18		2.20	

All models include quarterly seasonal dummies, trend, trend squared, both of which are interacted with the seasonal terms.

Wolfson Models: Dependent variable is first differenced log teenage employment rates

	1954-79		1954-89		1954-97	
	Coefficient	t-Statistic	Coefficient	t-Statistic	Coefficient	t-Statistic
Log Kaitz Index (first difference)	-0.050	-1.010	-0.035	-0.865	-0.029	-0.778
Log Adult Unemployment Rate (first difference)	-0.113	-4.655	-0.103	-5.935	-0.106	-6.362
Fraction of Teens 16-17 Years Old	0.055	0.267	0.030	0.187	0.025	0.208
Log Teen Share of Population (first difference)	-0.353	-0.642	-0.125	-0.275	0.036	0.118
Teenage Employment Rates (first difference, fourth lag)	0.509	5.894	0.512	7.969	0.554	9.629
Adjusted R-squared	0.972		0.976		0.975	

All models include quarterly seasonal dummies.

ENDNOTES

1. Workers at the 10th percentile earn more than 10% and less than 90% of the workforce (authors' calculation from CPS earnings files, as described in Webster 1997).

2. Census Bureau data on family income show that the average income of households in the bottom 20% of the income distribution fell 10.5% between 1979 and 1996 (these data are available on the EPI Datazone at www.epinet.org).

3. Since the minimum wage is not indexed to inflation, if Congress does not mandate an increase, then the buying power of the minimum falls at the rate of inflation.

4. Our earlier EPI Briefing Paper, "The Sky Hasn't Fallen," evaluates the first step of this increase.

5. These figures come from the March CPS, which allows us to aggregate households more accurately than the ORG files. Since the data are for March of 1997 (after the first but before the second increase), the affected range is $4.75-$5.15. See the Data Appendix for more details on these calculations.

6. These wage series are described in Webster (1997).

7. If not actually lower, we would expect their growth to decelerate.

8. The implication is that employment growth would have been even greater in the absence of a minimum wage increase.

9. See the Data Appendix for an explanation of the methods used to adjust the raw employment rate changes.

10. More formally, they estimate weighted least squares regressions on the following model:

$$\mathbf{e}_{it} = \alpha_i + \beta \mathbf{c}_{it} + \gamma_1 \mathbf{MW}_{1t} + \gamma_2 \mathbf{MW}_{2t} + \varepsilon_{it}$$

where:

- $\mathbf{e}_{it}$ is the natural logarithm of the employment-to-population rate of teenagers or less-than-high-school-educated adults in state i in year t;
- α_i is a state "fixed effect";
- $\mathbf{c}_{it}$ is the natural logarithm of the state employment-to-population rate for adult males;
- $\mathbf{MW}_{1t}$ is an indicator variable that takes the value 1 in years after the first increase and before the second and 0 otherwise;
- $\mathbf{MW}_{2t}$ is an indicator variable that takes the value 1 in years after the second increase and 0 otherwise; and
- ε_{it} is a well-behaved disturbance term.

In regards to $\mathbf{c}_{it}$, DMW use the state employment-to-population rate for 15-64-year-olds. We use the state employment rate for males 20 and older in regressions explaining teen employment rates and the employment rate for males 20 and older with a high school degree or more in regressions explaining less-than-high-school-educated adult (20-54) employment rates. Our approach avoids inducing a mechanical correlation between the state employment control variable and the dependent variable.

As for the rest of the equation, the coefficient β provides an estimate of the responsiveness of low-wage employment to changes in overall male employment. If β equals 1.0, for example, then a 1% increase in the employment rate for adult men would raise teen or less-than-high-school-educated employment 1%. The coefficients γ_1 and γ_2 are, in the DMW framework, estimates of the employment impact of the minimum wage. If γ_1 equals -1.0, for example, that would suggest that the first increase in the minimum wage reduced employment 1%. A similar interpretation would hold for γ_2. (Here, and in subsequent tables, we multiply the coefficient by 100.)

11. The federal minimum wage increased from $4.25 to $4.75 on October 1, 1996 and from $4.75 to $5.15 on September 1, 1997.

12. We also tested for joint significance of the two minimum wage variables for male, female, and black teens and less-educated female adults. None of the pairs was jointly significant at the 10% level.

13. His research originally appeared in the *Industrial and Labor Relations Review*. It subsequently appeared in Card's book with Alan Krueger, *Myth and Measurement: The New Economics of the Minimum Wage*. We follow the Card and Krueger treatment.

14. Card (1992) did not examine the impact on less-than-high-school-educated adult workers. Card's test takes the following form:

$$\Delta e_{it} = \alpha + \beta \Delta c_{it} + \gamma s_{it-1} + \varepsilon_{it}$$

where:

- Δe_{it} is the change in the employment-to-population rate of teenagers or less-than-high-school-educated adults in state i before and after the minimum-wage increase;
- α is a constant;
- Δc_{it} is the change in the employment-to-population rate of adults before and after the minimum wage increase (assumed to be independent of the minimum wage increase and, therefore, to control for state demand conditions);
- s_{it-1} is the share of working teenagers or less-educated adults in state i who earned between the old minimum wage and the new minimum wage in the period before the minimum wage increase (this is the share of workers in each state affected by the increase); and
- ε_{it} is a well-behaved disturbance term.

As with the DMW test, the coefficient β provides an estimate of the responsiveness of low-wage employment to changes in the overall employment rate. (DMW use the male, 15-64-year-old employment rate; Card uses the employment rate for all workers 16-64. We use the employment rate for all adults 20 years and older in the regressions for teens and the employment rate for all adults 20 years and older with a high school degree or more in regressions for less-educated adults.) The coefficient γ is an estimate of the employment impact of the minimum wage. If γ is negative, then the minimum wage increase is associated with reduced employment opportunities since it means that employment fell in states with a high share of workers affected by the minimum wage relative to employment in states with fewer affected workers. Card also uses the same model to estimate the change in wages across states. (The conventional employment elasticity is the ratio of β in the employment equation to β in the analogous wage equation.)

15. DMW (1995) used data from the smaller Outgoing Rotation Group, which is a one-quarter sample of the basic Current Population Survey.

16. Using the estimates from columns 2 and 4, the estimated employment elasticity is -0.110/0.145, or approximately -0.8.

17. The estimated employment elasticity is 0.197/0.368, or about 0.5.

18. Specifically, the minimum wage is measured using the Kaitz index. This index is a measure of the nominal minimum wage relative to average production, nonsupervisory wage, measured at the industry level and weighted by both industry minimum wage coverage and teenage industry employment. See Data Appendix for complete description of both this index and the control variables in the model.

19. See Richard Freeman (1996) for an elaboration of this point.

20. See Data Appendix for a discussion of data sources and model specification.

21. Technically, the coefficient represents the change in the teenage unemployment rate given a 1% change in the Kaitz index (see Data Appendix).

22. A t-statistic with an absolute value greater than 1.96 is considered statistically significant.

23. A stationary series is one in which mean and variance remain constant over time. Time-series regressions using nonstationary variables are likely to yield spurious results.

24. Wolfson's analysis uses data only up to 1979. We update the model through 1997, using a slightly different specification (see Data Appendix), since not all of the control variables are available through the full period.

25. The results are fully reported in the Data Appendix.

26. The model is called "competitive" because firms and workers are atomistic agents that eschew collusion (such as cartels or employers' associations) or cooperation (such as trade unions) choosing instead always to compete head-to-head.

27. Strictly speaking, the events play out instantaneously and simultaneously.

28. This underscores the importance of the assumption that neither employers nor workers can collude.

29. The supply of available labor is unlimited from the perspective of the individual firm, which is small compared to the total market. The industry as a whole must still increase the going wage to attract more workers to the industry.

30. Even though all jobs and workers are alike, the productivity of each newly created job is lower than the next most-recently created job because of diminishing marginal returns, with the idea being that each additional worker contributes marginally less than the previous worker. As employers reduce the number of workers after the minimum wage rises, the productivity of the marginal job therefore rises. A slightly more sophisticated model would allow for workers to have different levels of productivity. In this context, the minimum wage would displace the lowest-productivity workers first. Firms would cut back until the productivity of the next worker to go was equal to the minimum wage.

31. Card and Krueger (1995, 167), citing data from the Bureau of National Affairs (1985) estimate that turnover in fast-food restaurants may be as high as 300% per year.

32. Some critics of the minimum wage have argued that recent tests have failed to find employment losses because firms begin to make employment adjustments as soon as they believe that the minimum wage rise is inevitable (sometimes described as when the bill is signed, when the bill passes Congress, or the point when it is clear that the bill will pass and be signed into law). Given that the cost of adjusting low-wage employment is probably low, we see few advantages for firms to begin the process early, especially since during the adjustment period they will be operating with a staff that is below that which is profitable at the going wage.

33. Some evidence exists that firms may reduce employees' hours, rather than the number of employees (see Neumark and Wascher 1996, 12). For theoretical discussions of substituting hours for workers, see Michl (1996) and Palley (1995).

34. This calculation ignores "efficiency" losses resulting from reductions in output of goods produced using minimum wage workers. Economists generally consider these losses to be small relative to the distributional changes.

35. For large increases in the minimum wage, employers may cut hours more than they must raise wages. Under these circumstances, individual workers would be made worse off. The statement in the text holds over the range of increases in the minimum wage for which the elasticity of demand for low-wage workers is less than one in absolute value.

36. The Deere, Murphy, Welch (1995) test shows substantial employment losses in connection with the 1990-91 minimum wage hikes, which also correspond with an economic recession; the same test generally shows a neutral or positive employment response to the 1996-97 increases, which took place in the context of a booming economy. The Card (1992) test of state variation in the impact of the minimum wage shows no employment impact of the minimum wage in 1990-91and a negative impact in 1996-97. The Card test results run counter to the expected bias from a failure to control for the business cycle and argue that other, unmodeled factors played a role in one or both of the tests.

37. The time-series tests, which do include some supply factors, are an exception.

38. See, for example, Belman and Wolfson (1997, 1998); Card (1992a, 1992b); Card and Krueger (1994, 1995); Dickens, Machin, and Manning (1995); Evans and Turner (1996); Katz and Krueger (1992); Machin and Manning (1995); Neumark and Wascher (1995); Spriggs and Klein (1994); Wellington (1991); Wessels (1997); Wolfson (1998); and Zavodny (1996).

39. Louis Uchitelle, "Minimum Wage and Jobs," *The New York Times*, January 12, 1995, p. D1.

40. Belman and Wolfson (1997) have framed the evaluation of the competitive model in this way. For a more general, theoretical analysis see Card and Krueger (1995, Chapter 11) and Manning (1994, n.d.).

41. This assumption is closely related to the conclusion that firms have no discretion over the wages they pay their workers.

42. See Manning (n.d.). The discussion here draws on this introduction to dynamic monopsony.

43. Anecdotal accounts abound, for example, that fast-food restaurants in tight labor markets offer current employees bonuses for recruiting new workers.

44. Most of the papers don't assume firms can choose the wage, but this characteristic flows from assumptions about frictions, usually related to information flows, in the labor market. For example, unemployed workers are not instantly aware of which firms currently have vacancies and instead must search. In this context, firms that offer higher wages fill vacancies more quickly than those offering lower wages. The following is an incomplete list of papers describing dynamic monopsonistic models: Acemoglu (1997); Bhaskar and To (1996); Burdett and Mortensen (1989); Manning (1994); Palley (1996); Swinnerton (1996); and Zavodny (1995).

45. The term "dynamic monopsony" refers to the connection between these models and older, static models of "monopsony" in labor markets, the most familiar of which is the case of the one-company town. In the one-company town, the employer faces an upward sloping supply of labor — in order to increase its workforce, the firm must increase the wage it pays new employees (and usually the wage it pays its existing employees as well). A well-known result in labor economics establishes that under such circumstances, within limits, the introduction of a minimum wage can raise workers wages and increase total employment. Most textbooks dismiss monopsony as a theoretical curiosity since there are few company towns. The recent monopsony models share important features with this earlier model, but have been described as "dynamic" because it is the dynamics of the multi-employer labor market (usually driven by information) that give a degree of monopsonistic power to many, small employers.

46. The "tipping-point" is when the minimum wage reaches the wage that would be set in a perfectly competitive market. In a dynamic monopsonistic setting, the market wage is below the competitive equilibrium wage.

47. Or some combination of the two extremes.

48. This is true in equilibrium. Not all models of this type imply multiple equilibria. In some models, a firm's profits fall as employment and output rise.

49. The 1979 minimum wage, in 1997 dollars, was $6.29.

50. These issues are discussed in the BLS's monthly *Employment and Earnings*. See, for example, pp. 225-42 in the January 1997 issue.

51. Note that this factor is slightly larger than that used in our earlier briefing paper (Bernstein and Schmitt 1997). This is due to our use of updated BLS parameters.

52. We multiplied the post-1993 Kaitz index values by the ratio of 1993:4 value of the index using the old methodology to the value of the 1993:4 index calculated under the new method. Prior to the release of the new coverage rates, we constructed the Kaitz index using 1990 coverage rates (the most recent year available to us). The post-1994 correlation between this series and the series using the new coverage rates is 0.99. Thus, since minimum wage coverage rates change little over this time period, the update has had no effect on the employment-effect estimates.

53. Earlier time-series models included a variable for the share of teenagers in federal training and employment programs. Wolfson (1997) includes this variable. Card and Krueger (1995, 195) point out that the variable is not readily available after 1986, and its inclusion has little impact on the Kaitz coefficient. The variable is also omitted by Williams and Mills (1997; 1998).

54. The AR(1) term is estimated using the Marquart method. As Card and Krueger (1995) show in Table 6.6, the coefficient on the Kaitz index varies with the use of different AR(1) algorithms. Relative to other techniques, the Kaitz coefficient is slightly larger (in absolute value) using the Marquart correction, with standard errors on the low-end of those in Card and Krueger's table. In this regard, this method leads to a larger (negative) minimum wage effect relative to other AR(1) correction algorithms.

55. Wolfson tries a wide variety of alternative specifications for this test and is not able to reject the null in any case. Recall, however, that his data set runs from 1954 to 1979, while ours goes through 1997.

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