THE WHITE HOUSE

WASHINGTON

January 20, 1995

MEMORANDUM TO THE PRESIDENT

THROUGH: Leon Panetta, Robert Rubin

FROM: Gene Sperling

On the minimum wage, there are three decisions that must be made: 1) whether to support a minimum wage increase; 2) if so, how much to increase it; and, 3) when to announce any decision. After several meetings and significant consultation with the Hill, the NEC has come to a single recommendation on the first two issues, while timing still represents a fairly open issue that must be resolved.

I. BRIEF BACKGROUND: The Federal minimum wage is currently $4.25 an hour. The minimum wage had been frozen at a nominal level of $3.35 per hour from 1981 to 1990. In 1990, Congress voted to raise the minimum wage by 90 cents -- in two 45 cent stages -- from $3.35 to $3.80 in 1990, and from $3.80 to $4.25 in 1991. The real value of the minimum wage is now above its value from 1988 to 1990, but is below its value for every other year going back to 1955.

The minimum wage has two important purposes. First, the minimum wage raises the bargaining power for low-wage workers who are not represented by a union and have low skills. The minimum wage may also have a "ripple" effect, whereby wages of workers earning somewhat more than the minimum wage are increased to maintain internal wage standards. As a result, the minimum wage helps many poor and moderate-income families. The second purpose of the minimum wage is that it induces firms to train their workers and take a high-value added/low turnover strategy.

II. EMPLOYMENT AND DISTRIBUTIONAL EFFECTS: The potential effect of a minimum wage increase on employment has been the principal argument raised in opposition to such an increase. While the potential employment effects of a minimum wage increase surely need to be considered, the weight of the empirical evidence suggests that the effect of a moderate raise from its current level is likely to be negligible. Laura Tyson points out that while there is no respectable case for the large job loss figures used by some special interests, there is respectable economic literature that would show a small, marginal job loss. However, Laura and others found most compelling the variety of recent studies examining the effects of raises in the federal minimum wage both across the nation and in particular states, as well as the effects of raising state minimum wages above the federal level.

For example, studies of the federal minimum wage examined the effects of a 90 cent increase in two increments over a two-year period. One of the state studies examined the effects of New Jersey recently raising its state minimum wage by 80 cents, from $4.25 an hour to $5.05 an hour; another examined the effects of California increasing its minimum wage 90 cents from $3.35 an hour in 1987 to $4.25 an hour in 1988 (which would equal $5.20 in 1993 dollars). All of these studies found that the minimum wage increases did not reduce employment opportunities. (These studies were conducted by David Card, Alan Krueger, Larry Katz.)

An important point to note about the minimum wage is that while there may be some who are teens or just providing a supplement to a family living comfortably, most are people who are either sole earners for their families or are providing vital support to help maintain a middle income existence. Indeed, 36% of minimum wage workers are the sole earner in their family, while the average minimum wage worker provides nearly 50% of the income in their household.

While increasing the minimum wage does help many people at the bottom of the economic ladder it is also, as mentioned above, important for families where a second income is keeping them barely in the middle class. For example, 43% of minimum wage earners are in the bottom 20% of income, 45% are in the middle 60% -- and only 12% are in the top 20%. Only about one-third of minimum wage earners are teens while two-thirds are adults. Some argue against the minimum wage because it is not as targeted as the EITC toward working poor families. Others find it to be a positive point that an increase in the minimum wage would have such a positive effect on both working poor and middle income households.

III. RECOMMENDATION

NEC Recommendation: The general recommendation from your economic advisors as well as Leon Panetta, George Stephanopoulos and Pat Griffin was to call for a 75 cent increase over two years -- 50 cents the first year and 25 cents the second year -- bringing the minimum wage to a total of $5.00. The rationale for the two-stage increase to 75 cents is based on both our best estimate of what is sound economically, as well as representing a reasonable political starting point for a minimum wage proposal. From a political perspective, the most important single issue is the degree of support we can gather from Congressional Democrats for our proposal. Several people believe that the best chance for passage is a single 50 cent increase. Pat Griffin believes that we should just go with a single 50 cent increase. Some, such as Gene Sperling, believe that even if we want to end up at a 50 cent increase proposal, we should start with the 75 cent increase so that we have room to bargain and still pass a 50 cent increase. Leon believes a single 50 cent increase has the best chance for passage, but is open to the notion of starting higher to give us bargaining room.

The only other difference is that George Stephanopoulos would prefer to add indexing to the 75 cent increase. Secretary Reich is generally supportive of indexing, but not sure he supports it if is done from only a 75 cent increase.

a) **Economic Rationale**: As mentioned above, the economic literature demonstrates that there is good evidence that a minimum wage increase under 90 cents does not have negative employment consequences. While a $1 increase may still be in the ball park, our feeling is that we would be on stronger footing if our increase was within the levels where the recent economic studies by Katz and Krueger showed no negative employment impact. Furthermore, at $5.00, the national minimum wage would still be below the highest state minimum wage levels -- New Jersey at $5.05 and Hawaii at $5.25.

There was also agreement among most of the economists on the NEC that 75 cents in one year might be too steep and that we were on sounder ground dividing it between 50 cents and 25 cents. Furthermore, a 50 cent increase immediately puts the minimum wage level back up to where it was when the last minimum wage increase passed in 1991. The additional 25 cents increase puts the number up to an even $5.00. While some may have found a 75 cent minimum wage increase to be small by historical standards, the minimum wage increase you call for must also be considered in the context of the dramatic increase you proposed and signed for the EITC.

b) **Political Rationale:** As mentioned above, you must consider your recommendation both in terms of the statement it makes and the degree your proposal increases the chance of passage. A higher minimum wage increase is likely to be more satisfactory to those who most support the minimum wage increase -- organized labor and some progressive Democrats. The AFL-CIO calls for a $1.50 increase -- with three 50 cent increases plus indexing. Sabo and seven Democratic co-sponsors called last year for an increase in the minimum wage to $6.50, while denying employers a tax deduction for excessive compensation (more than 25 times the lowest compensation paid any other employee). Kennedy also supports a minimum wage increase close to the AFL-CIO recommendation. On the other hand, several Democrats in the Senate have expressed some reservations about proposing a minimum wage increase, and would feel better if we kept the increase to a lower level.

Advocates who argue for a higher number increase state, that since the chance of passage of the minimum wage with the present Congress is considered small by some, the proposal should at least accomplish the political goal of being satisfactory to labor. An argument for going for a lower number -- 50 cents -- is that it would be small enough to lessen intense opposition from some business groups, while responding to the concerns of some Democrats who feel that an increase in the minimum wage goes against the "new Democrat" message. Furthermore, Pat Griffin and others felt that a 50 cent proposal had the best chance of passage.

We felt that a two-stage 75 cent increase without indexing made the most sense from a political perspective as well as an economic perspective. First, while a 75 cent increase would be unlikely to satisfy organized labor and the most ardent supporters of the minimum wage, it would meet your campaign promise of making up the lost value due to inflation since the last minimum wage increase while adding 25 cents to spare, and it seems to be a level that could hold most Democrats in Congress together. Finally, to the degree that a one-time 50 cent increase represents the proposal with the best chance of passage, a 75 cent proposal gives us some negotiating room.

IV. INDEXING: The proposal that was considered for indexing would tie the minimum wage to the wages of the median worker or the 25th percentile worker. These standards are preferable to indexing to the CPI because they would avoid contributing to an inflationary cycle that could result if the CPI was used. The arguments for indexing are that it would protect the value of the minimum wage without making Congress continually have to seek new legislation. Politically, Republicans who oppose indexing would be put in the position of having to support indexing the capital gains tax cut for well-off Americans while opposing indexing for wages for workers at the lowest wage levels.

Nonetheless, the NEC and White House staff felt that a 75 cent increase was adequate without indexing. The arguments against it were the low chance of passage, the risk that it would be perceived as inflationary (even with our attempts to index to a measure other than the CPI), and the concern that the perceived benefit to workers would not be large enough to overcome the political downsides. Furthermore, even strong advocates of a stronger minimum wage increase might not support an indexing proposal because they would fear that indexing after only a 50 cent or 75 cent increase would lock them into too low of a level for the future. Bob Rubin also stressed that while business groups would oppose us on a minimum wage increase, if indexing were added it would add significant intensity to that opposition that would work against us on this as well as other business-related matters.

V. SUB-MINIMUM WAGE:

The NEC also discussed the issue of adding a sub-minimum wage for teenagers. There was a three-year trial subminimum wage in the 1989 minimum wage increase, (15% less for teens for up to 6 months), but the results were that it was hardly used for several reasons -- including recordkeeping requirements. Because the impact was so minimal, some might claim that we should go ahead and do this. Yet, the decision of the NEC was that in light of that de minimis impact, there was no economic rationale to justify the problems a subminimum wage proposal would create with those likely to support passage of a minimum wage increase.

VI. ARGUMENTS AGAINST NEC RECOMMENDATION: Despite the fact that the NEC was virtually unanimous it its recommendation, we wanted to give you "cons" -- the opposing arguments you are likely to hear for either not increasing the minimum wage or, on the other hand, calling for a larger increase.

- **Hurts New Democrat Message:** Some Congressional Democrats believe that this works against the "new Democrat" message -- as an increase in the minimum wage is perceived as a traditional Democratic/labor agenda item and is likely to be vigorously opposed by small business. Arguing against this concern is the fact that while there will be intense small business opposition, polls consistently show that 70% of Americans support an increase in minimum wage. Other Democrats concerned about the minimum wage have argued that if we do propose a minimum wage that we package it together with other policies designed to increase wages for working Americans.

- **Will Create Opposition without Creating Strong Support**: A 75 cent increase is likely to run the risk of stirring strong small business opposition without generating strong support from organized labor which is likely to feel it is far too low. Arguing against this is the degree that we can work with Democrats to work together to draw a line on the side of working Americans, against Congressman Armey and others who strongly oppose this popular issue.

- **Unfunded Mandates:** An increase in the minimum wage does affect some state and local governments who do pay the minimum wage for some jobs and therefore could claim that this is an unfunded mandate.

VII: TIMING: The main timing question is whether or not to announce your minimum wage decision in the State of the Union.

Pro:

- The State of the Union allows you to directly make your case over the heads of the special interests to the general public on an issue where the general public is with us.

- The State of the Union allows you to blend the minimum wage in with an overall strategy to raise incomes for working people.

- There has been considerable speculation about how we are going to decide this issue -- waiting past the State of the Union continues that process -- going earlier may distract from the build up to the State of the Union.

Con:

- A minimum wage increase in the State of the Union might be treated as a major news item that could distract from the basic messages in the State of the Union.

- There is still time to announce it prior to the State of the Union (though it would have to be almost immediately) or after it as a way of keeping our economic story going or building up to rewarding work for the welfare working session.

- There is some evidence of increasing anxiety about doing this among Congressional Democrats.

- There was general agreement in the last NEC meeting that this announcement should be in the State of the Union -- though there exists today significant disagreement on this issue within the White House.

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Introduction

Replication and reanalysis are important endeavors in economics, especially when new findings run counter to conventional wisdom. In their comment on our 1994 American Economic Review article, Neumark and Wascher (1997) challenge our conclusion that the April 1992 increase in the New Jersey minimum wage led to no loss of employment in the fast-food industry. Using data on a non-randomly selected set of restaurants initially collected for Richard Berman of the Employment Policies Institute (EPI) and later supplemented by their own data collection efforts, Neumark and Wascher (hereafter NW) conclude that "... the New Jersey minimum wage increase led to a relative decline in fast-food employment in New Jersey" compared to Pennsylvania.[1] They attribute the discrepancies between their findings and ours to problems in our fast-food restaurant data set. Specifically, they argue that our use of employment data derived from telephone surveys, rather than from payroll records, led us to draw faulty inferences about the effect of the New Jersey minimum wage.

In this paper we attempt to reconcile the contrasting findings by analyzing administrative employment data from a new *representative* sample of fast-food employers in New Jersey and Pennsylvania, and by re-analyzing NW's data. Most importantly, we use the Bureau of Labor Statistics's (BLS's) ES-202 data file to examine employment growth of fast-food restaurants in a set of major chains in New Jersey and nearby counties of Pennsylvania.[2] We draw two samples from the ES-202 files: a longitudinal file that tracks establishments between 1992 and

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[1]In the March 1995 version of their paper, NW relied exclusively on 71 observations collected by EPI. Subsequent versions have also included information from their supplemental data collection.

[2]The ES-202 data are also known as the Business Establishment List. Persons employed by an eligible organization may apply to BLS for restricted access to ES-202 data for statistical research purposes.

1993, and a series of repeated cross-sections from the end of 1991 through 1996. Because the BLS data are derived from Unemployment Insurance payroll records, the employment measures are free of the kinds of survey errors that may have affected our earlier results. In addition, because the ES-202 data include information for every covered employer, there is no reason to doubt the representativeness of the BLS sample.

A comparison of fast-food employment growth in New Jersey and Pennsylvania over the period of our original study confirms the main findings in our 1994 paper, and calls into question the representativeness of the sample assembled by Berman, Neumark, and Wascher. Consistent with our original sample, the BLS fast-food data set indicates slightly faster employment growth in New Jersey than in the Pennsylvania border counties over the time period that we initially examined, although in most specifications the difference between New Jersey and Pennsylvania is small and statistically insignificant. We use the BLS data to examine longer-run effects of the New Jersey minimum wage increase, and to conduct a preliminary analysis of the effect of the 1996 increase in the federal minimum wage, which was binding in Pennsylvania but not in New Jersey (where the state minimum wage already exceeded the new federal standard). Our analysis of this new policy intervention is consistent with the conclusion that modest changes in the minimum wage have little systematic effect on employment.

In light of these findings we go on to re-examine the Berman-Neumark-Wascher (BNW) sample and evaluate NW's contention that the rise in the New Jersey minimum wage caused employment to fall in the state's fast-food industry. Our re-analysis leads to three main conclusions. First, the pattern of employment growth in the BNW sample of fast-food restaurants across chains and geographic areas *within* New Jersey is quite consistent with our original survey

data. The differences between the BNW sample and ours are attributable to differences in the sample of Pennsylvania restaurants in their data set, which unlike our original sample and the BLS data, show a *rise* in fast-food employment in the state. Second, the anomalous behavior of the BNW Pennsylvania sample is driven by restaurants owned by one Burger King franchisee, who comprised the entire EPI sample of Pennsylvania franchises. Third, the patterns of employment changes (or more precisely, scaled hours changes) across different restaurants in the BNW sample vary systematically with the period over which the data were reported. In particular, establishments that reported on a bi-weekly basis had faster growth than those that reported on a monthly or weekly basis. The reporting periods may matter because of different seasonal factors in the two waves of the BNW sample (e.g., holidays or weather) or because of imperfect scaling of hours. In any case, a disproportionately high fraction of Pennsylvania restaurants reported their data in bi-weekly intervals, leading to an overstatement of relative employment growth in that state. Once this peculiar feature of the sample is taken into account, the BNW data indicate virtually identical hours growth in New Jersey and eastern Pennsylvania.

Based on all the evidence now available, including the BLS sample, our earlier sample, and the BNW sample, we conclude that the increase in the New Jersey minimum wage in April 1992 had little or no systematic effect on total fast-food employment in the state, although there may have been individual restaurants where employment rose or fell in response to the higher minimum wage.

I. Analysis of Representative BLS Fast-Food Restaurant Sample

A. Description of BLS ES-202 Data

On April 1, 1992, the New Jersey state minimum wage increased from $4.25 to $5.05 per hour, while the minimum wage in Pennsylvania remained at $4.25. To examine the effect of the New Jersey minimum wage increase using representative payroll data, we applied to the BLS for permission to analyze their ES-202 data. The ES-202 database consists of quarterly employment records reported by employers to their state Employment Security Agencies. The first question on the New Jersey tax form requests the "Number of covered workers employed during the pay period which includes the 12th day of each month."[3] The BLS maintains these data as part of the Covered Employment and Wages Program. We analyze two types of samples from the ES-202 file: a longitudinal file and a series of repeated cross-sections.

The longitudinal sample consists of restaurants belonging to a set of the largest fast-food chains.[4] Restaurants in the sampled chains employed 13 percent of all employees in the eating and drinking industry in New Jersey and eastern Pennsylvania in 1992. There is considerable overlap between the restaurants in the BLS sample and those in our original sample. Restaurants in the sampled chains were identified by a name search of the appropriate fields of the ES-202 file. This search was conducted on all establishments in the eating and drinking industry (SIC 5812) in New Jersey and eastern Pennsylvania in the first quarter of 1991, first quarter of 1994, and fourth quarter of 1996. The observations from these quarters were inspected to eliminate

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[3]The first question on the Pennsylvania form requests the "Total covered employees in pay period incl. 12th of month." Employers are asked to report employment for each month of the quarter. A copy of these forms is available from the authors on request.

[4]For confidentiality reasons, BLS has requested that we not reveal the identity or number of these chains. We can report, however, that there are fewer than 10 chains in the sample.

establishments that were not restaurants in the relevant chains, or to add restaurants that were missed by the algorithm used in the computerized name search. If a restaurant in one of the fast-food chains was discovered that was not identified by the name search, the computer algorithm was amended to include that name.

The original Card-Krueger (CK) sample contained data on restaurants in 7 counties of Pennsylvania (Bucks, Chester, Lackawanna, Lehigh, Luzerne, Montgomery, and Northampton). Because this is a somewhat idiosyncratic group -- with some counties located right on the New Jersey border and others off the border -- we decided to expand the sample to include 7 additional counties: Berks, Carbon, Delaware, Monroe, Philadelphia, Pike, and Wayne. In the results that follow, we present estimates for both our original 7 counties and for the larger set of 14 counties.

Once restaurants in the relevant chains and counties were identified, we merged data on the restaurants for selected months between the first quarter of 1992 and the fourth quarter of 1994.[5] To mirror the CK sample, only establishments with non-zero employment in February or March of 1992 -- the months covered by Wave 1 of our survey -- were included in the longitudinal analysis file.[6] The final data set contains 687 establishments. A total of 16 (2.3 percent) of these establishments had zero or missing employment in November or December of 1992, the months covered by Wave 2 of our survey. These establishments either closed or could

[5]The ES-202 file also contains data on total quarterly payroll. These data are problematic for our purposes, however, because any income drawn by the franchise owner should be included in this field for incorporated restaurants. Thus, if an increase in workers' earnings is offset by a decline in the income the owner draws, the quarterly payroll data will not reflect the increase in workers' pay.

[6]Additionally, to ensure that the sample consisted exclusively of restaurants (as opposed to, e.g., headquarters or monitoring posts), the authors restricted the sample to establishments with an average of 5 or more employees in February and March 1994, and average monthly payroll per employee below $3,000 in 1992:Q1 and 1992:Q4. These restrictions eliminated 17 observations from the original sample of 704 observations.

not be tracked because their reporting information changed.

A potential limitation of the BLS longitudinal sample for the present paper should be noted. The ES-202 data pertain to "reporting units" that may be single establishment units or multi-establishment units. The BLS encouraged employers to report their data at the county level or below in the early 1990s. Some employers were in the process of switching to a county-level reporting basis during our sample period. Consequently, some restaurants that remained open were difficult to track because they changed their reporting identifiers. Fortunately, most of the restaurants that were in this situation could be tracked by searching addresses and other characteristics of the stores. All of the restaurants that were not linked to subsequent months' data were assumed closed and assigned zero employment for these months, even though some of these restaurants may not have closed. This is probably a more common occurrence for New Jersey than Pennsylvania: 0.4 percent of the Pennsylvania restaurants had zero or missing employment at the end of 1992, as compared to 3.4 percent of New Jersey restaurants. In our original survey, 1.3 percent of Pennsylvania restaurants and 2.7 percent of New Jersey restaurants were temporarily or permanently closed at the end of 1992.[7]

Also note that because firms are allowed to report on more than one unit in a county in the BLS data, some of the records reflect an aggregation of data for multiple establishments. We address both of these issues in the analysis below. Perhaps more importantly, these problems do not affect the cross-sectional files that we also analyze.

To draw the repeated cross-section file, the same name search algorithm was applied each

[7]An interviewer visited all of the non-responding stores in both states to determine if they were closed in our original survey.

quarter between 1991:Q4 and 1996:Q4.[8] This time period spans the months covered in our original survey. Again, data were selected for the same chains in New Jersey and the 14 counties in eastern Pennsylvania. Every month's data from the sampled quarters was selected. The cross-sectional sample probably provides the cleanest estimates of the effect of the minimum wage increase because it incorporates *births* as well as deaths of restaurants, and because possible problems caused by changes in reporting units over time are minimized.

B. Summary Statistics and Differences in Differences

Table 1 reports basic employment summary statistics for New Jersey and for the Pennsylvania counties, before and after the April 1992 increase in New Jersey's minimum wage. Panel A is based on the BLS sample of fast-food restaurants. In the first row, the "before" period pertains to *average* employment in February and March of 1992, and the "after" pertains to *average* employment in November and December of 1992.[9] The second row reports employment figures for February and November, which were the most common survey months in our telephone survey. The third row uses data spanning the 12-month period from March 1992 to March 1993. Finally, for comparison, Panel B of Table 1 reports the corresponding employment statistics calculated from the CK survey.[10]

Several conclusions are apparent from the means in Table 1. First, the BLS data indicate

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[8]Because of a relatively high frequency of imputed employment values prior to 1991:Q4, we do not use data from earlier months.

[9]In one case, employment was zero in March 1992, so the February figure was used.

[10]For comparability with BLS data, we calculate total employment in our survey data by adding together the number of full-time, part-time and managerial workers. This approach differs from Card and Krueger (1994), which weights part-time workers by 0.5 to derive full-time equivalent employment. The original CK data are available via anonymous ftp from the MINIMUM directory of IRS.PRINCETON.EDU.

a slight rise in employment in New Jersey's fast-food restaurants over the period we studied, and a slight decline in employment in Pennsylvania's restaurants over the same period. Our telephone survey data indicate a net gain in New Jersey relative to Pennsylvania of 2.4 workers per restaurant, whereas the BLS data in row 2 indicate a smaller net gain of 1.1 workers between February and November of 1992. Second, between March 1992 and March 1993, the BLS data indicate that both New Jersey and Pennsylvania experienced a decline in average employment, with the decline being larger in Pennsylvania. Third, the average employment level in the BLS data is somewhat greater than the average level in our data, probably because some of the observations in the BLS data pertain to multiple establishments. Fourth, our data and the BLS data indicate that average restaurant size was initially larger in Pennsylvania than in New Jersey. By contrast, the BNW data set indicates that "full-time equivalent employment" was initially greater in New Jersey than in Pennsylvania (see Section III below). Finally, the BLS data indicate that the results for the 7 Pennsylvania counties that we used in our initial sample and the wider set of 14 counties are generally similar.

Neumark and Wascher (1997) and others have emphasized the fact that the dispersion in full-time employment changes in our data set is greater than the dispersion in changes in total hours worked in the BNW data. Interestingly, the BLS data display roughly the same standard deviation of employment changes as was found in our original sample. For example, in New Jersey the standard deviation of the change in employment across reporting units between February and November of 1992 was 10.12 according to the BLS data, which slightly exceeds the standard deviation calculated from our survey data (9.82) over approximately the same months. One problem with this comparison is that some of the BLS reporting units combine

multi-establishment restaurants that may have broken out over time, whereas our unit of observation was consistently the individual restaurant. To address this issue, we restricted the BLS sample to reporting units that initially had fewer than 40 employees; reporting units this size are almost certainly individual restaurants. The standard deviation of employment changes for this truncated BLS sample is 9.0 for New Jersey and 6.8 for Pennsylvania; these figures compare to 8.0 and 8.8, respectively, if we likewise truncate our survey data.

More generally, the criticism that our telephone survey was flawed because the employment changes show a high degree of dispersion strikes us as off the mark for three other reasons. First, reporting errors in employment data collected from a telephone survey are not terribly surprising. Dispersion in our data is not out of line from that in other establishment-level employment surveys (e.g., Davis, Haltiwanger, and Schuh, 1996). Second, as long as measurement error in the dependent variable has the same mean in New Jersey and Pennsylvania, estimates based on our data will be unbiased. We know of no reason to suspect that the New Jersey and Pennsylvania managers who responded to our survey would misreport employment data in a systematically different way. Third, the standard deviation of full-time equivalent employment changes is potentially sensitive to the way data on hours, or combinations of part-time and full-time employees, are scaled. NW convert weekly payroll hours data into a measure of employment by dividing by 35, but a smaller divisor would lead to larger dispersion of employment in their data. The standard deviation of *proportionate* changes in employment is invariant to scaling and is fairly similar in all three data sets: 0.29 in the BLS data, 0.35 in BNW's data, and 0.39 in our earlier survey data.

C. Regression-Adjusted Models

Panels A and B of Table 2 present basic regression estimates using the BLS ES-202 sample of fast-food restaurants. The models presented in this table essentially parallel the main specifications in Card and Krueger (1994). The dependent variable in the first two columns is the change in the number of employees, and the dependent variable in the last two columns is the proportionate change in the number of employees.[11] Employment changes are measured between February-March 1992 and November-December 1992. Columns 1 and 3 include as the only regressor a dummy variable indicating whether the restaurant is located in New Jersey or eastern Pennsylvania. These estimates correspond to the difference-in-differences estimates that can be derived from row 1 of Table 1. The models in columns 2 and 4 add a set of additional control variables: dummy variable for the identity of the restaurant chain; and a dummy variable indicating whether the reporting unit was a sub-unit of a multi-unit employer.[12]

The regression results in Panel A of Table 2, which are based on the employment changes for restaurants in same geographic region surveyed in our earlier work, indicate small, positive coefficients on the New Jersey dummy variable.[13] Each of the estimates is individually statistically insignificant, however. We interpret these estimates as indicating that New Jersey's employment growth in the fast-food industry over this period was essentially the same as it was for the same set of restaurant chains in the seven Pennsylvania counties.

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[11]Following Card and Krueger (1994), the denominator of the proportionate change is the average between first and second period employment.

[12]Observations that are not reported as sub-units of multi-unit establishments are either part of a multi-unit reporting firm or the only restaurant owned by a particular reporting unit.

[13]Because, in principle, the BLS sample contains the population of fast-food restaurants in the designated chains, an argument could be made that the OLS standard errors understate the precision of the estimates. Nonetheless, throughout the paper we rely on conventional tests of statistical significance.

In Panel B, regression results are presented using the wider set of 14 Pennsylvania counties as the comparison group. These results also indicate somewhat faster employment growth in New Jersey following the increase in the state's minimum wage. Only in the proportionate change specifications without covariates, however, is the difference in growth rates between New Jersey and Pennsylvania restaurants close to being statistically significant.

For comparison, Panel C contains the corresponding estimates from our original sample. These estimates differ (slightly) from those reported in our original paper because we now measure employment as the unweighted sum of full-time workers, part-time workers, and managerial workers. The estimates based on our sample are qualitatively similar to those based on the BLS data, with positive coefficients on the New Jersey dummy variable. In addition, in both data sets the inclusion of additional explanatory variables does not add very much to the explanatory power of the model.

D. Specification Tests

The BLS data analyzed in Tables 1 and 2 suggest that the New Jersey minimum wage increase had either no effect, or a small positive effect, on fast-food industry employment in New Jersey *vis-a-vis* eastern Pennsylvania. To probe this finding further, in Table 3 we examine a variety of other specifications and samples. Panel A of the table presents results using our original 7 Pennsylvania counties, and Panel B uses the wider set of 14 counties. In all of the models, we include a full set of chain dummy variables and the sub-unit dummy variable. In addition, we report results for both the change in employment specification (column 1) and the proportionate employment growth specification (column 2).

For reference, the first row replicates the basic specifications from Table 2. Rows 2 and 3 examine the sensitivity of our results to alternative choices for handling stores with missing employment data in November-December 1992. In the base specification these stores are assigned 0 employment in the second period, which is equivalent to assuming that they all closed. Recall that some of these stores may have actually remained open but changed reporting identifiers. In row 2, we delete from the sample all stores with missing employment data in the second period; this is equivalent to assuming that all of these stores remained open but were randomly missing employment data. Finally, in row 3, we use the imputation codes in the ES-202 database to attempt to distinguish between closed stores (with an imputation code of 9) and those that had missing data for other reasons. In particular, we add back to the sample any restaurant with missing employment data (or those with 0 reported employment) if they were assigned an imputation code indicating a closure. In our opinion, this is the most appropriate sample for measuring the effect of the minimum wage on the set of stores that were in business just before the rise in the minimum. A comparison of the results in rows 2 and 3 with the base specifications indicates that eliminating stores with missing or zero second-period employment, or including such stores only if the imputation code indicated the store was closed, tends to increase the coefficient on the New Jersey dummy variable.

Two of the observations in the sample had employment increases about twice the mean Wave 1 size; the next largest increase was less than the mean size.[14] These large employment changes may have occurred because one franchisee acquired another outlet, or for other reasons. To probe the impact of these two outliers, they are dropped from the sample in row 4. The

[14]Large negative employment changes are more likely because of restaurant closings.

estimates are not very much affected by these observations, however.

In Card and Krueger (1994) we calculated the proportionate change in employment with average employment over the two periods in the denominator. (This procedure is used by other analysts of employment change data, including Davis, Haltiwanger, and Schuh, 1996). This specification was selected because we thought it would reduce the impact of measurement error in the employment data. We have continued to use that specification so far in this paper. The specification in row 5, however, measures the proportionate change in employment with the first period employment in the denominator. With this specification, New Jersey's employment growth is slightly lower than that in the 7 county Pennsylvania sample, although employment growth in New Jersey continues to be greater than in Pennsylvania when the 14 county sample is used.

In row 6 we eliminate from the sample restaurants that are located in counties on the New Jersey shore. These counties may have different seasonal demand patterns than the rest of the sample. The results are not very different in this truncated sample, however. Another way to hold seasonal effects constant is to compare employment over the same months. In row 8 we measure employment changes from March 1992 to March 1993. This 12-month change has the added advantage of allowing employers more time to adjust employment. In levels, the faster employment growth in New Jersey is notably larger when 12-month changes are used.

Finally, in row 8 we measure employment changes from February 1992 to November 1992. As mentioned, these are the months when the preponderance of data in our survey was collected. It is probably not surprising that these results are quite similar to the base specification estimates, which use the average February-March 1992 and average November-December 1992

employment data.

On the whole, we interpret the BLS longitudinal data as indicating that fast-food industry employment growth in New Jersey was about the same, or slightly stronger, than that in Pennsylvania following the increase in New Jersey's minimum wage. It is possible to choose samples and/or specifications in which employment growth was slightly weaker in New Jersey than in Pennsylvania. This is what one would expect if the true difference in employment growth was very close to zero. We doubt, however, that a representative sample of fast-food restaurants would show robust evidence that employment growth was significantly weaker in New Jersey than in eastern Pennsylvania in the months we have studied.

II. Repeated Cross-Sections from the BLS ES-202 Data

As described above, we used the quarterly BLS ES-202 data to draw repeated cross-sectional data sets for the fast-food chains each month from the end of 1991 through 1996. With these data, we calculated total employment for New Jersey, for the 7 counties of Pennsylvania used in our original study, and for the broader set of 14 eastern-Pennsylvania counties each month. Figure 1 summarizes the time-series patterns of aggregate employment from these files. For each of the three geographic regions, the figure shows aggregate monthly employment in the fast-food industry relative to their respective February 1992 levels.

The figure reveals a pattern that is consistent with the longitudinal estimates. In particular, between February and November of 1992 -- the main months our survey was conducted -- fast-food employment grew by 3 percent in New Jersey, while it fell by 1 percent in the 7 Pennsylvania counties and fell by 3 percent in the 14 Pennsylvania counties. Although

it is possible to find some pairs of months surrounding the minimum wage increase over which employment growth in Pennsylvania exceeded that in New Jersey, on whole the figure provides little evidence that Pennsylvania's employment growth exceeded New Jersey's in the few years following the minimum wage increase.

A. The Effect of the 1996 Federal Minimum Wage Increase

On October 1, 1996 the federal minimum wage increased from $4.25 per hour to $4.75 per hour. This increase was binding in Pennsylvania, but not in New Jersey because New Jersey's $5.05 state minimum already exceeded the new federal minimum. Consequently, the same comparison can be conducted in reverse, with New Jersey now serving as a "control group" for Pennsylvania's experience. The results in Figure 1 clearly indicate greater employment growth in Pennsylvania than in New Jersey following the 1996 minimum wage increase. Between December 1995 and December 1996, for example, employment grew by 11 percent in the 14 Pennsylvania counties and 2 percent in New Jersey. In the 7 county Pennsylvania sample employment grew by 16 percent. It is possible that the superior growth in Pennsylvania relative to New Jersey reflects a delayed reaction to the 1992 increase in New Jersey's minimum wage, but we doubt that employment would take so long to adjust in this high-turnover industry. We also doubt that Pennsylvania's strong employment growth was caused by the 1996 increase in the federal minimum wage, but there is certainly no evidence in these data to suggest that the hike in the federal minimum wage caused Pennsylvania restaurants to lower their employment relative to what it otherwise would have been in the absence of the minimum wage increase.

One caveat, however, is that these data might understate New Jersey's employment growth

because one chain in the sample discontinued reporting on 37 of its New Jersey establishments between the second and third quarter of 1996. These restaurants might have been closed, or they might have been spun off to another enterprise that is not among the fast-food chains in our sample. To gauge the importance of this phenomenon, we can perform the following hypothetical calculation. Suppose these 37 establishments had continued in the sample, with their employment equal to the average size of establishments in this chain as of 1996:Q1. In this counterfactual scenario, New Jersey's employment would have grown by 6.9% between December 1995 and December 1996 -- still well below Pennsylvania's employment growth rate. Thus, the BLS data provide little support for the view that the 1996 increase in the federal minimum wage adversely impacted employment in fast-food restaurants in Pennsylvania *vis-a-vis* New Jersey, at least in this short period following the federal minimum wage increase.

III. A Reanalysis of the Berman-Neumark-Wascher (BNW) Data Set

A. Genesis of the BNW sample

The conclusion we draw from the BLS data and our original survey data is qualitatively different from the conclusion NW draw from the data they collected in conjunction with Berman and the EPI. This discrepancy led us to re-analyze the BNW sample, devoting particular attention to the possible non-representative nature of the data.[15] We suspect that problems in the BNW sample arise because a scientific sampling method was *not* used in the EPI data collection effort, and because the data were collected three years after the minimum wage increase.

The BNW sample was assembled in several phases. First, Berman (1995) released a

[15]The BNW data that we analyze were downloaded from www.econ.msu.edu in November 1997.

report based on 41 restaurants.[16] This report, which included analysis conducted by David Neumark, made no mention of the methods used to identify the restaurants in the sample. A companion paper by Neumark and Wascher (1995a) reported that the EPI sample contained 71 restaurants and provided some information on the origins of the sample.[17] Evidently, informal industry contacts were heavily relied on to select these 71 restaurants. We refer to this sample, augmented with information for one New Jersey store that closed during 1992, as the "original Berman sample." The sample contains data for 10 franchisees in the Burger King and Wendy's chains, although *all* of the Pennsylvania restaurants belong to one Burger King franchisee who apparently supplied EPI with data on his Pennsylvania outlets but not his New Jersey outlets.

Around the time that Berman launched the EPI study, the Wall Street Journal (January 31, 1995, p. 1) reported:

> "Lobbyists for small businesses, retailers and big companies huddled Friday at the Washington office of the National Restaurant Association to plot strategy against the Clinton effort to boost the $4.25-per-hour minimum wage. ... The group plans to poke holes in a study widely used by proponents to show employment rose in New Jersey fast-food restaurants when the minimum wage was raised there."

In this environment, concerns were raised about the representative character of the data assembled by Berman's organization (e.g., Schmitt, 1995). In their August 1995 paper Neumark and

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[16]The actual sample size was not reported. We estimate the sample size from Figures 1 and 2 of the report, which display data for a total of 41 restaurants. Berman also referred to these data in a Wall Street Journal op-ed on March 29, 1995.

[17]The description of the data set in the earliest Neumark and Wascher (1995a) paper states, "Contact was made with franchisees identified in the Chain Operator's Guide"; no mention was made of informal contacts. The most recent version of the paper (Neumark and Wascher 1997, Appendix A) states that "... the original EPI sample was drawn partly from franchisees in the Chain Operator's Guide ..., partly from restaurants owned by other franchisees ... that were identified informally, and partly from parent corporations...." A letter explaining the origins of the sample from the Chief Economist of EPI dated October 21, 1995 made no mention of the Chain Operator's Guide. Information in Neumark and Wascher (1997; Table 7) implies that at least half of the franchised restaurants in the Berman/EPI sample were identified through informal contacts.

Washer wrote that "... to avoid conflicts of interest we subsequently took over the data collection effort from EPI, so that the remaining data came from the franchisees or corporations directly to us." During the period from March to August 1995 they added information for 18 additional restaurants owned by franchisees in the original Berman sample, along with information from 7 additional franchisees and one chain. We refer to this sample of 154 restaurants as the Neumark-Wascher (NW) sample. Data for 9 other restaurants were apparently collected by EPI after NW took over data collection (see Neumark and Washer 1995b, footnote 9).[18] We include these 9 restaurants in the pooled BNW sample, but exclude them from the original Berman subsample and from the NW subsample.

Although NW attempted to draw a complete sample of those restaurants not included in the original Berman sample, there are reasons to doubt the representative nature of their data. First, at least one-third of the franchise owners in the NW supplementary sample had previously supplied some data to EPI. Second, and probably more important, NW's letter to franchisees stated that they were collecting data to "re-examine the New Jersey-Pennsylvania minimum wage study" and emphasized that they were working "in conjunction with ... a restaurant-supported lobbying" organization. This lead-in may have affected response patterns for restaurants with different employment trends in New Jersey and Pennsylvania, accounting for the low response rate in their survey.

For whatever reasons the combined BNW sample represents only a fraction of fast-food restaurants in New Jersey and eastern Pennsylvania belonging to the four chains in the sample.

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[18]The most recent version of NW's data set includes an indicator variable for restaurants collected by EPI. This variable shows a total of 81 restaurants in the EPI sample, representing the 72 restaurants in the original Berman sample and the 9 restaurants which were provided directly to EPI after March 1995.

While we do not have a precise count of the total number of restaurants in the potential sample universe, we can obtain a lower bound estimate from the number of working telephone listings we found in January 1992 for our own fast-food sample. In New Jersey, where the geographic boundaries of the sample frame are unambiguous, we found 364 valid phone numbers, whereas the BNW sample contains 163 restaurants (see Card and Krueger, 1995, Table A.2.1). In eastern Pennsylvania, we found 109 working phone numbers in the 7 counties we surveyed, whereas the BNW sample contains 72 restaurants in as many as 19 counties.[19] These comparisons suggest that the BNW sample includes fewer than half of the potential universe of restaurants. If the BNW sample were *random* this would not be a problem. As explained below, however, many features of the sample suggest otherwise. In particular, the Pennsylvania restaurants in the original Berman sample appear to be substantially different from other restaurants in the data set. If restaurants in the original Berman sample are non-representative, their presence biases results based on the combined BNW sample even if the NW subsample is representative.[20] As it happens, any down-weighting of the original Berman restaurants in the pooled BNW sample substantially weakens the conclusion that employment fell in New Jersey relative to Pennsylvania.

B. Basic Results

Table 4 shows the basic patterns of fast-food employment in the pooled BNW sample and

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[19]BNW's sample universe covers a broader region of eastern Pennsylvania than ours because BNW define their geographic area based on our three-digit zip codes. These zip codes cover 19 counties, although our sample universe only included restaurants in 7 counties.

[20]Neumark and Wascher note that there are potential concerns with the Berman sample and attempt to address these concerns by analyzing a subsample of restaurants that were listed in the Chain Operator's Guide. This does not adequately address the issue, however, because even within the potential universe of restaurants in the Guide, some restaurants may have been brought into the BNW sample through informal contacts, while others are not included in the sample (possibly because of a lack of informal contacts).

in various subsamples. The first three rows of the table report data on NW's main employment measure, which is based on average payroll hours reported for each restaurant in February and November of 1992. Franchise owners reported their data to EPI and Neumark-Wascher in different time intervals -- weekly, bi-weekly, or monthly -- for up to three "payroll periods" before and after the rise in the minimum wage. NW converted the data (for the maximum number of payroll periods available for each franchisee) into average weekly payroll hours divided by 35. As shown in row 1 of Table 4, this measure of full-time employment for the pooled BNW sample shows that stores were initially smaller in Pennsylvania than New Jersey, and that during 1992 stores in Pennsylvania expanded while stores in New Jersey contracted slightly. The "difference in differences" of employment growth is shown in the right-most column of the table, and indicates that relative employment fell by 0.85 full-time equivalents in New Jersey from the period just before the rise in the minimum wage to the period 6 months later.

In rows 2 and 3 we compare these relative trends for restaurants in the original Berman sample and in NW's sample. The difference in relative employment growth in the pooled sample is driven by data from the original Berman sample, which shows positive employment growth in both states, but especially strong growth in Pennsylvania. We emphasize that *all the Pennsylvania restaurants* in the original Berman sample belong to a single Burger King franchisee.[21] Thus, any conclusion about the growth of average payroll hours in the fast-food industry in New Jersey relative to Pennsylvania hinges on the representativeness of the data for

[21]An earlier draft of our paper reported that this franchisee "apparently provided the earliest data to EPI." David Neumark subsequently informed us that no record was kept of the date or order in which the data were provided. Restaurants belonging to this franchisee are the first observations listed in NW's data file, and are identified as belonging to owner number 3.

this single franchisee.

The final row of Table 4 reports relative trends in an alternative measure of employment available for a subset of restaurants in the pooled BNW sample -- the total number of non-management employees. In contrast to the pattern for total payroll hours, non-management employment rose *faster* in New Jersey than Pennsylvania.[22] Taken at face value, these findings suggest that the rise in the New Jersey minimum wage was associated with an increase in employment, and a small decline in hours per worker.[23] Unfortunately, although one-half of restaurants in the original Berman sample supplied non-management employment data, only 10 percent of restaurants in the NW subsample reported it. Thus, the sample available for studying relative trends in employment is relatively sparse.

C. Regression Adjusted Models

The simple comparisons of relative employment growth in Table 4 make no allowances for other sources of variation in employment growth. The effects of controlling for some of these alternative factors are illustrated in Table 5. Each column of the table corresponds to a different regression model fit to the changes in employment observed for restaurants in the pooled BNW sample.

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[22]Among the subset of stores that reported non-management employment, the difference-in-differences in average payroll hours / 35 is -0.43, with a standard error of 0.55. Thus, there is no strong difference between relative payroll hours trends in the pooled BNW sample and among the subset of restaurants that reported non-management employment.

[23]To compare relative changes in hours and employees it is convenient to work with logarithms, so scaling is not an issue. For the sample of 55 observations that reported both numbers of employees and hours, the difference-in-differences of log payroll hours is -0.018; the difference-in-differences of log management employees is 0.066; and the difference-in-differences of log employees minus log hours is 0.084 (t-ratio = 2.28). Thus, the apparent opposite movement in hours and employees is statistically significant for this small sample.

Column 1 presents a model with only a New Jersey dummy: the estimated coefficient corresponds to the simple difference-of-differences reported in row 1 of Table 4. Column 2 reports a model with only an indicator for observations in the NW subsample. This variable is highly significant (t-ratio over 8) and negative, implying that restaurants in the NW subsample had systematically slower employment growth than those in the original Berman sample. The model in column 3 explores the effect of chain and company-ownership controls. These are jointly significant and show considerable differences in average growth rates across chains, with slower growth among Roy Rogers and KFC restaurants than Wendy's or Burger King outlets.

Finally, the model in column 4 includes indicators for whether the restaurant's employment data were derived from bi-weekly or monthly intervals (with weekly data the omitted category). These variables are also highly significant, and suggest that the period over which the data were reported affects measured employment trends. Relative to restaurants that provided weekly data (25 percent of the sample), restaurants that provided bi-weekly data experienced faster hours growth between the two waves of the survey, while restaurants that provided monthly data had slower hours growth.

An important lesson from columns 1-4 of Table 5 is that a wide variety of factors affect measured employment growth in the BNW sample. Many of these factors are also highly correlated with the New Jersey dummy. For example, a disproportionately high fraction of New Jersey stores in the pooled sample were obtained by NW. Since the NW subsample has slower growth overall, this correlation might be expected to influence the estimate of relative employment trends in New Jersey. Additionally, the Pennsylvania sample contains none of the slow-growing KFC outlets. Thus, it may be important to control for these other factors when

attempting to measure the relative trend in New Jersey employment growth.

The models in columns 5-7 include the indicator for New Jersey outlets and various subsets of the other covariates. Notice that the addition of any subset of controls lowers the magnitude of the New Jersey coefficient by 20-90 percent, and also improves the precision of the estimated coefficient by 10-15 percent. None of the estimated New Jersey coefficients are statistically significant at conventional levels once the other controls are included in the model. Simply controlling for an intercept shift between restaurants in the NW subsample and the balance of the pooled data set reduces the size of the estimated New Jersey coefficient by over 50 percent.

The addition of controls indicating the time interval over which the hours data were reported has a particularly strong impact on scaled hours. Even controlling for chain and ownership characteristics, the bi-weekly payroll indicator is highly significant (t=3.19). In the appendix we present further estimation results and a figure illustrating the differences in Wave 1 hours, Wave 2 hours, and hours growth for restaurants that provided data over different time intervals in the BNW sample.[24] These results suggest that the differences by reporting interval (particularly between restaurants that reported bi-weekly data and other restaurants) are generic, and not an artifact of specific functional form assumptions or outliers. We are unsure of the reasons for the highly significant differences in measured growth rates between restaurants that reported data over different payroll intervals, although it is possible that they reflect differential seasonal factors or mis-scaling of hours. For example, many restaurants are closed on

[24]In Appendix Table 1 we also report employment growth models for the subset of Burger King restaurants only. Among these restaurants the situation is similar: the bi-weekly payroll indicator is positive and highly significant (t=2.61); and controlling for types of payroll data the New Jersey effect is small and statistically insignificant.

Thanksgiving, which may have been more likely to be covered by some November payroll intervals than others; no adjustment to work hours was made to allow for whether stores were closed during holidays. Most importantly, Table 5 indicates that controlling for the type of payroll data has a large effect on the estimated New Jersey relative employment effect, because a much lower fraction of New Jersey restaurants supplied bi-weekly data. Once these differences are taken into account, the employment growth differential between New Jersey and Pennsylvania all but disappear, even in the pooled BNW sample.

D. Alternative Specifications and Samples

The results in Tables 4 and 5 suggest that the measured effects of the New Jersey minimum wage differ between the original Berman sample and the subsequent NW sample. Table 6 reports the estimated coefficient on the New Jersey dummy from a variety of alternative models fit to the pooled BNW sample, the NW subsample, and the original Berman sample. Each row of the table corresponds to a different model specification or alternative measure of the dependent variable; each column refers to one of the three indicated samples. For example, the first row reports the estimated New Jersey effect from models that include no other controls: these correspond to the differences-in-differences reported in Table 4.

Row 2 of the table illustrates the influence of the data from the Burger King franchisee who supplied the Pennsylvania observations in the original Berman sample. When the 26 restaurants owned by this franchisee are excluded, the estimated New Jersey effect in the pooled BNW sample becomes positive.[25] Without this owner's data it is impossible to estimate the

[25]This franchisee supplied data on 23 restaurants (all in Pennsylvania) to the original Berman/EPI data collection effort, and on three additional restaurants (all in New Jersey) to NW's later sample.

New Jersey effect in the original Berman sample. In the NW sample, however, the exclusion has a negligible effect.

Rows 3 and 4 of Table 6 show the estimated New Jersey coefficients from specifications that control for chain and company ownership, and these variables plus indicators for the type of payroll data supplied to BNW (bi-weekly, weekly or monthly). As noted, once controls for the payroll period are included, the New Jersey effect falls to essentially zero in the pooled sample. In the original Berman sample, the New Jersey effect switches sign.

In most of their analysis NW utilize an employment measure based on the average scaled hours data taken over varying numbers of payroll periods across restaurants in their sample. (Data are recorded for up to three payroll periods per restaurant in each wave). To check the sensitivity of the results to this choice, we constructed a measure using only the *first* payroll period for restaurants that reported more than one period. In principle, one would expect this alternative measure to show the same patterns as the averaged data. As illustrated in Panel B (rows 5 and 6) of Table 6, the use of the alternative employment measure leads to results that are qualitatively similar to the averaged data, but uniformly less supportive of NW's conclusion of a negative employment effect in New Jersey. Even in the original Berman sample the use of the simpler hours measure leads to estimates that are well below conventional significance levels.

Finally, Panel C of Table 6 reports estimates from models that use the proportional change in average payroll hours at each restaurant -- rather than the change in the level of average hours -- as the dependent variable. The latter specifications are more appropriate if employment responses to external factors (such as a rise in the minimum wage) are roughly proportional to the scale of each restaurant. Inspection of these results suggests that the signs of the New Jersey

effects are generally the same as in the corresponding models for the levels of employment, although the coefficients in the proportional change models are relatively less precise.

Our conclusion from the estimates in Table 6 is that most (but not all) of the alternatives show a negative relative employment trend in New Jersey, although the magnitudes of the estimated effects are generally much smaller than the naive difference-in-differences estimate from the pooled BNW sample. *The estimated New Jersey effect is most negative in the original Berman sample, although even in this sample the estimated New Jersey effect reverses sign once controls are introduced for the payroll period of the data.* In the NW sample or in the pooled sample that excludes data for the Pennsylvania franchisee who supplied Berman's data, the relative employment effects are small in magnitude and uniformly statistically insignificant (t-ratios of 0.7 or less). These patterns highlight the crucial role of the original Berman data in drawing inferences from the BNW sample. Without these informally-collected data (or more precisely, without the observations from the single Burger King franchisee who provided the initial Pennsylvania data) the BNW sample provides little indication that the rise in the New Jersey minimum wage lowered fast-food employment.

E. Consistency of the BNW Sample with the Card-Krueger Sample

While we have shown that the evidence of a decline in relative New Jersey employment from the BNW sample is not robust, it is nevertheless interesting to ask how the employment patterns in the two data sets compare. The public-use versions of both data sets include only the first three digits of the zip code of each restaurant, rather than full addresses. This limitation necessitates comparisons of employment trends by restaurant chain and "three-digit zip code

area".[26]

A useful summary of the degree of consistency between the two samples is provided by a bivariate regression of the average employment changes (by chain and zip code area) from one sample on the corresponding changes from the other. In particular, if the employment changes in the BNW sample are taken to be the "true" change for the cell, then one would expect an intercept of 0 and a slope coefficient of 1 from a regression of the observed employment changes in our data on the changes for the same zip code area and chain in the BNW data.[27] This prediction has to be modified slightly if the employment changes in the BNW sample are "true" but scaled differently than in our survey. In particular, if the ratio of the mean employment level in our survey to the mean employment level in the BNW sample is k, then the expected slope coefficient is k.

Table 7 presents estimation results from regressing employment growth rates by chain and zip-code area from our fast-food sample on the corresponding data from the BNW sample. Although 98 chain-by-zip-code cells are available in our data set, only 48 cells are present in the pooled BNW sample. Column 1 shows results for these cells, while columns 2 and 3 present results separately for chain-by-zip-code areas in New Jersey and Pennsylvania. The data underlying the analysis are also plotted in Figure 2.

Inspection of Figure 2 and the regression results in Table 7 suggests that there is a

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[26]The first three digits of the postal zip code do not correspond to any conventional geographic entity.

[27]The situation is more complex if the BNW data are treated as a noisy measure of the truth, e.g., because of sampling or nonsampling errors. In particular, let λ_j represent the reliability of the observed employment changes (by zip-code and chain) in survey j (j=1,2). In this case, if the measurement errors in the two surveys are uncorrelated, the probability limit of the regression coefficient from a linear regression of the employment change in survey 1 on the change in survey 2 is λ_2 (the reliability of the second survey), and the probability limit of the R-squared is $\lambda_1 \cdot \lambda_2$ (the product of the reliability ratios).

reasonably high degree of consistency between the two data sets: the correlation coefficient is 0.47. The two largest negative outliers are in zip codes containing a high proportion of restaurants from EPI's Pennsylvania sample. In light of this finding, and the concerns raised in Table 6 about the influence of the data from the franchisee who supplied these data, we show a parallel set of models in Panel B of Table 7 that excludes this owner's data from the average changes in the BNW sample.

Looking first at the top panel of the table, the regression coefficient relating the employment changes in the two data sets is 0.78. An F-test for the joint hypothesis that this coefficient is 1 and that the intercept of the regression is 0 has a prob-value of 0.47. Comparisons of the separate results for New Jersey and Pennsylvania suggest that within New Jersey the two data sets are in closer agreement. Across the relatively small number of Pennsylvania cells the samples are less consistent, although we can only marginally reject the hypothesis of a zero intercept and unit slope. Because we used the same survey methods and interviewer to collect data from New Jersey and Pennsylvania, there is no reason to suspect different measurement error properties in the two states in our sample. A comparison of the results in the bottom panel of the table shows that the exclusion of data from the franchisee who provided EPI's Pennsylvania sample *improves* the consistency of the two data sets, particularly in Pennsylvania. While not decisive, this comparison suggests that the key differences between the BNW sample and our sample are driven by the data from the single franchisee who supplied the Pennsylvania data for the Berman sample. Together with the other evidence in Tables 4-6, this finding leads us to question the representativeness of the EPI's sample.

F. Patterns of Employment Changes Within New Jersey

The main inference we draw from Table 7 is that the employment changes in the BNW and Card-Krueger data sets are reasonably highly correlated, especially within New Jersey. Bigger discrepancies arise between the relatively small subsamples of Pennsylvania restaurants. The consistency of the BNW and CK New Jersey samples is worth emphasizing, since in our original paper we found that comparisons of employment growth within New Jersey (i.e., between restaurants that were initially paying higher and lower wages) led to the same conclusion about the effect of the minimum wage as comparisons between New Jersey and Pennsylvania. As a further check on this conclusion we merged the average "proportionate gap" between the new minimum wage and the Wave 1 starting wage for each of the chain-by-zip-code areas in New Jersey derived from our survey data with the average employment changes for the corresponding cells from the BNW sample. We then regressed the average changes in employment (ΔE) on the average gap measure (GAP) for the 37 overlapping cells.[28] The estimated regression equation, with standard errors in parentheses, is:

$$\Delta E = -2.00 + 17.98\ GAP \qquad R^2 = .09.$$
$$\qquad\quad (1.11)\quad (9.75)$$

The coefficient on the GAP variable in BNW's data is similar in magnitude to the estimate we obtained using the New Jersey micro data from our survey (13.1 with a standard error of 6.6). Furthermore, if we estimate another cell-level model with BNW's employment data and add dummy variables indicating the restaurant chain, we obtain:

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[28]At the restaurant level this proportionate gap is defined as the difference between the new minimum wage and the restaurant's starting wage in Wave 1 divided by the starting wage in Wave 1. (The gap is set to zero if the starting wage is above the new minimum wage.) The GAP measure in our regressions is the weighted average of the restaurant-level proportionate gaps, where the weights are the number of restaurants in the cell.

$$\Delta E = \underset{(0.78)}{0.77} + \underset{(5.76)}{12.00}\ GAP + Chain\ Dummmies \qquad R^2 = .78.$$

In this model, the coefficient on the GAP variable is slightly smaller than in the bivariate regression, but it has a higher t-ratio. The positive estimated coefficients on the GAP variable indicate that employment rose faster at New Jersey restaurants located in areas that were required to raise their entry wage the most when the minimum wage increased. Thus, the pattern of employment growth rates within BNW's sample of New Jersey restaurants confirms our original finding that the rise in the minimum wage had no adverse effect on employment growth at lower-wage relative to higher-wage restaurants in the state.

IV. Conclusion

After analyzing BNW's data, NW's data, our original survey data, and most importantly, the BLS ES-202 fast-food establishment data, we reach the following conclusion: *The increase in New Jersey's minimum wage probably had no effect on total employment in New Jersey's fast-food industry, and possibly had a small positive effect.* We have previously written that, because of frictions in the labor market, a minimum wage increase can be expected to cause some firms to reduce employment and others to raise employment, with these two effects potentially cancelling out if the rise in the minimum wage is modest (Card and Krueger, 1995, esp. pp. 13-14). If this view is correct, then it would not be surprising to find some specifications and data definitions that yield a negative impact of the minimum wage on employment. But we doubt that a representative survey of fast-food restaurants in New Jersey and eastern Pennsylvania would show a significant adverse impact of the minimum wage on total employment.

The only data set that indicates a significant decline in employment in New Jersey relative

to Pennsylvania is the small, non-random group of restaurants collected by Berman and EPI. Results of this data set stand in contrast to our survey data, to the BLS's payroll data, and to the supplemental data collected by Neumark and Wascher. The difference between the New Jersey-Pennsylvania comparison in our original survey and BNW's data cannot be reconciled by inherent differences between a telephone survey and administrative payroll records because the BLS ES-202 data are based on administrative payroll records. Instead, we suspect the common denominator is that representative surveys indicate statistically insignificant and small differences in employment growth between New Jersey and eastern Pennsylvania, while the nonrepresentative data informally collected for Berman produce anomalous results.

An alternative interpretation of the full spectrum of results is that the New Jersey minimum wage increase did not reduce total employment, but it did reduce the average number of hours worked per employee. Neumark and Wascher (1997) reject this interpretation. Although we are less quick to rule out this possibility, we are agnostic about any conclusion concerning average hours worked per employee that relies so heavily on the informally-collected Berman/EPI sample, and the exclusion of controls for the length of the reporting interval. Moreover, within New Jersey the BNW data indicate that hours grew *more* at restaurants in the lowest wage areas of the state, where the minimum wage increase was more likely to be a binding constraint. This finding runs counter to the view that total hours declined in response to the New Jersey minimum wage increase.

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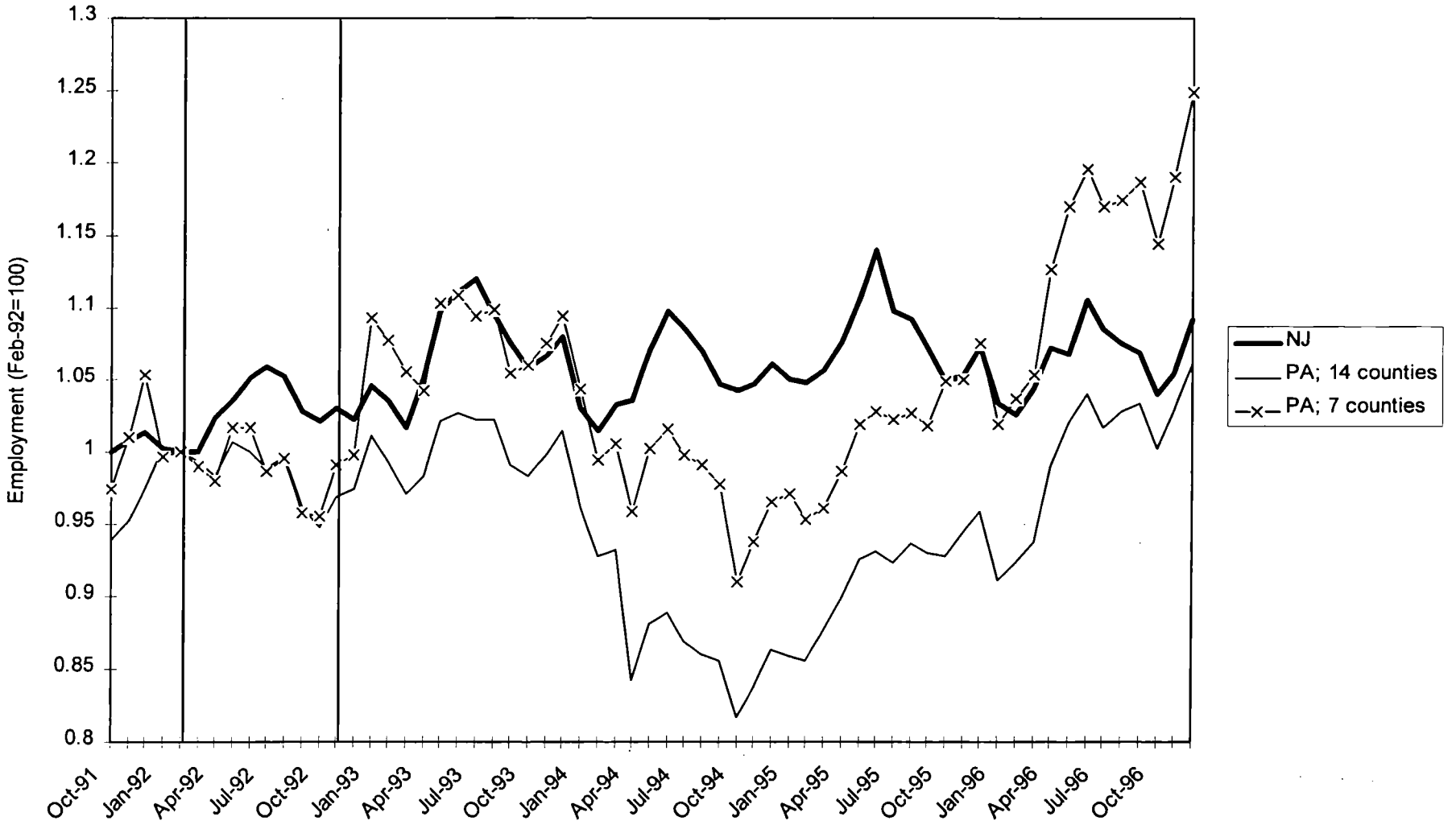
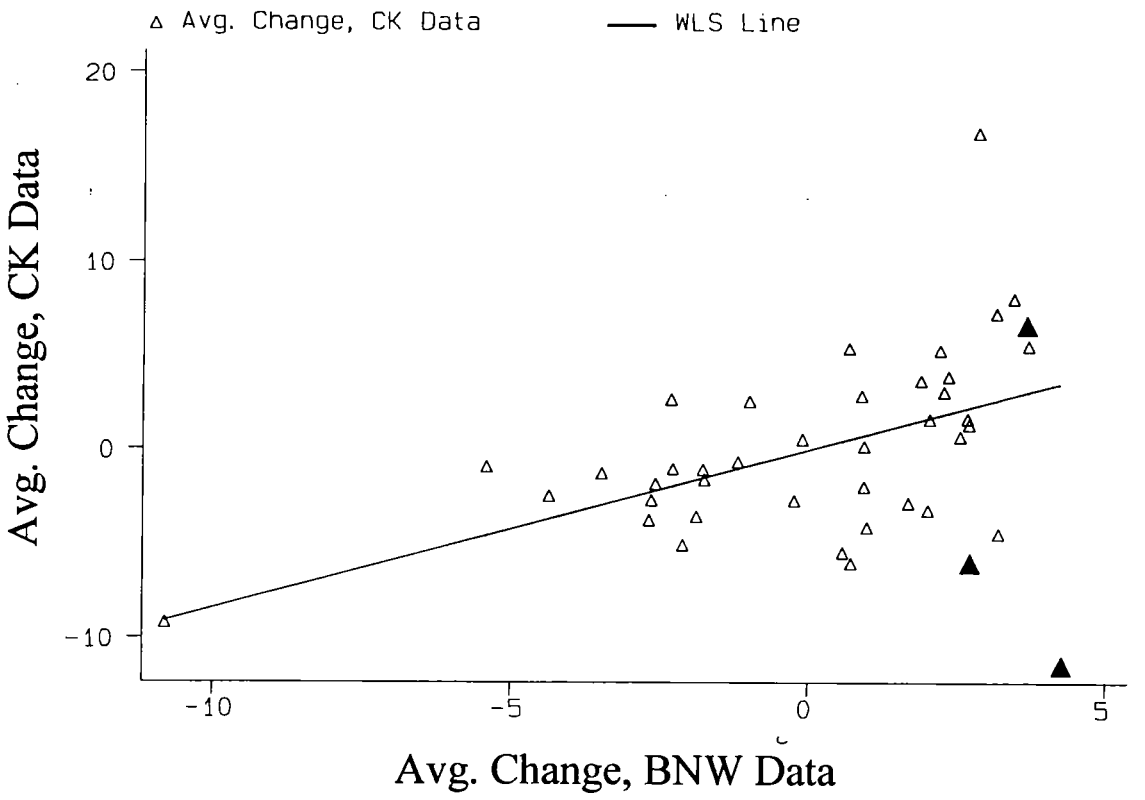
Figure 1
Employment in NJ and PA Fast Food Restaurants, Oct. 1991 to Dec. 1996



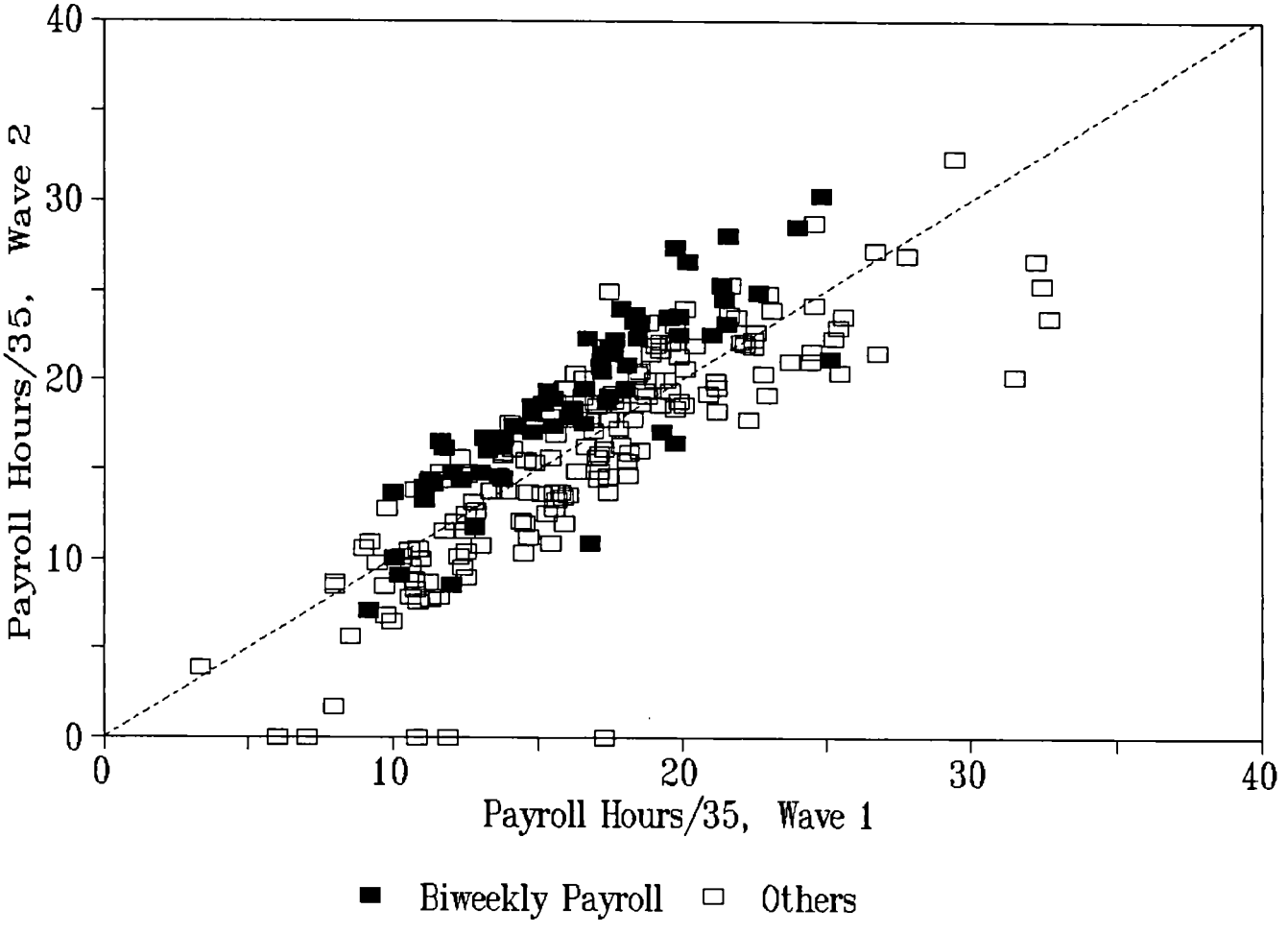
Source: Authors' calculations based on BLS ES-202 data.

Note: Vertical lines indicate dates of original Card-Krueger survey.

Figure 2: Graph of CK Data vs. BNW Data at the 3-Digit Zip Code by Chain Level



Notes: Shaded triangles indicate Berman/EPI Pennsylvania sample. The line shown on the graph is the WLS regression fit using the subsample collected by NW. Weights are the number of restaurants in the cell based on CK data.



Appendix Figure: Hours in Wave 2 versus Wave 1, by Type of BNW Payroll Data

Table 1: Descriptive Statistics for Fast-Food Restaurants Drawn from BLS ES-202 Data and Card-Krueger Survey

Means with Standard Deviations in Parentheses:

	New Jersey			7 Pennsylvania Counties			14 Pennsylvania Counties		
	Before	After	Change	Before	After	Change	Before	After	Change
A. BLS ES-202 Data									
Feb-Mar to Nov-Dec 92	37.2 (19.9)	37.6 (21.0)	0.41 (9.82)	42.5 (23.2)	42.4 (23.5)	-0.12 (10.94)	44.8 (53.7)	44.3 (59.9)	-0.53 (12.32)
Feb-92 to Nov-92	37.2 (19.9)	37.8 (20.9)	0.57 (10.12)	42.7 (23.8)	42.2 (23.2)	-0.54 (12.82)	44.9 (53.6)	44.4 (60.4)	-0.58 (13.83)
Mar-92 to Mar-93	37.2 (20.1)	34.8 (20.0)	-2.48 (13.99)	42.3 (22.8)	37.5 (18.6)	-4.80 (22.74)	44.7 (54.0)	40.7 (54.5)	-4.0 (18.1)
B. Card-Krueger Survey Data									
Feb-92 to Nov-92	29.8 (12.5)	30.0 (13.0)	0.19 (9.82)	33.1 (14.7)	30.9 (10.6)	-2.23 (11.98)	NA	NA	NA

Notes: Sample sizes for the first two rows are 437 for NJ, 127 for PA 7 counties, and 250 for PA 14 counties; sample sizes for third row are 436, 127, and 250, respectively; sample sizes for the last row are 309 for NJ and 75 for PA. The 7 PA counties used in the middle columns are the same counties used in Card and Krueger (1994); these 7 counties are a subset of the 14 counties in the last three columns (see text). The unit of observation for the BLS data is the "reporting unit," which in some cases includes multiple establishments. The unit of observation in the Card-Krueger data is the individual restaurant.

Table 2: Basic Regression Results; BLS ES-202 Fast-Food Data and Card-Krueger Survey Data

Explanatory Variables	Dependent Variable: Change in Levels (1)	(2)	Proportionate Change (3)	(4)
Panel A: All of NJ and 7 Pennsylvania Counties, BLS Data				
NJ Indicator	0.536 (1.017)	0.225 (1.029)	0.007 (0.029)	0.009 (0.029)
Chain Dummies and Sub-unit Dummy Var.	No	Yes	No	Yes
Reg. Std. Error	10.09	9.99	0.286	0.281
R-Squared	0.001	0.029	0.000	0.046
Panel B: All of NJ and 14 Pennsylvania Counties, BLS Data				
NJ Indicator	0.946 (0.856)	0.272 (0.859)	0.045 (0.024)	0.032 (0.024)
Chain Dummies and Sub-unit Dummy Var.	No	Yes	No	Yes
Reg. Std. Error	10.80	10.63	0.303	0.294
R-Squared	0.002	0.042	0.005	0.071
Panel C: Original Card-Krueger Survey Data				
NJ Indicator	2.411 (1.323)	2.488 (1.323)	0.029 (0.050)	0.030 (0.049)
Chain and Co. Ownership Dummies	No	Yes	No	Yes
Reg. Std. Error	10.28	10.25	0.385	0.382
R-Squared	0.009	0.025	0.001	0.024

Notes: Each regression also includes a constant. Sample size is 564 for panel A, 687 for panel B, and 384 for panel C. Sub-unit dummy variable equals one if the reporting unit is a sub-unit of a multi-unit employer. For comparability with the BLS data, employment in the CK sample is measured by the total number of full- and part-time employees. Standard errors in parentheses.

Table 3
Sensitivity of New Jersey Employment Growth Differential to Specification Changes; BLS ES-202 Fast-Food Restaurant Sample

Specification and Sample	Change in Levels (1)	Proportionate Change (2)	Sample Size
	Panel A: NJ and 7 PA Counties		
1. Basic Specification	0.225 (1.029)	0.009 (0.029)	564
2. Excluding Closed Stores	0.909 (0.950)	0.031 (0.024)	549
3. Excluding Closed Stores Unless Imputation Code=9	0.640 (0.973)	0.022 (0.025)	553
4. Drop Large Outliers	0.251 (0.970)	0.009 (0.028)	563
5. Proportionate Change with Initial Employment In Base	---	-0.001 (0.032)	564
6. Excluding New Jersey Shore	0.032 (1.092)	0.008 (0.030)	480
7. March 1992 to March 1993 Employment Change	2.345 (1.678)	0.007 (0.035)	563
8. February 1992 to November 1992 Employment Change	1.048 (1.104)	0.013 (0.032)	564

- Continued -

Table 3: Continued

Specification and Sample	Change in Levels (1)	Proportionate Change (2)	Sample Size
	Panel B: NJ and 14 PA Counties		
1. Basic Specification	0.272 (1.029)	0.032 (0.024)	687
2. Excluding Closed Stores	0.639 (0.776)	0.055 (0.021)	671
3. Excluding Closed Stores Unless Imputation Code=9	0.338 (0.787)	0.044 (0.021)	675
4. Drop Large Outliers	0.722 (0.784)	0.032 (0.023)	685
5. Proportionate Change with Initial Employment In Base	---	0.020 (0.024)	687
6. Excluding New Jersey Shore	0.069 (0.924)	0.030 (0.025)	603
7. March 1992 to March 1993 Employment Change	1.196 (1.258)	0.009 (0.027)	686
8. February 1992 to November 1992 Employment Change	0.624 (0.927)	0.027 (0.024)	687

Notes: The table reports the coefficient (with standard error in parentheses) for the New Jersey dummy variable from a regression of the change in employment (column 1) or proportionate change in employment (column 2) on a New Jersey dummy variable, chain dummy variables, a dummy variable indicating whether the restaurant is reported as a sub-unit of a multi-establishment employer, and a constant.

Table 4: Descriptive Statistics for Levels and Changes in Employment by State, BNW Data

	Means With Standard Deviations in Parentheses:						Difference in Differences NJ-PA (std. error)
	New Jersey			Pennsylvania			
	Before	After	Change	Before	After	Change	
Total Payroll Hours/35:							
1. Pooled BNW Sample	17.5 (5.5)	17.5 (5.9)	-0.1 (3.4)	15.1 (4.0)	15.9 (5.9)	0.8 (3.5)	-0.85 (0.49)
2. NW Subsample	17.7 (6.1)	16.7 (6.3)	-1.0 (3.3)	13.4 (3.8)	12.4 (4.9)	-1.0 (3.5)	-0.05 (0.61)
3. Original Berman Subsample	17.1 (3.5)	19.3 (4.3)	2.1 (2.7)	16.9 (3.4)	20.4 (4.3)	3.4 (2.1)	-1.28 (0.63)
Non-Management Employment:							
4. Pooled BNW Sample	24.8 (6.0)	28.4 (6.8)	3.6 (3.0)	29.0 (5.5)	31.3 (6.8)	2.2 (4.7)	1.39 (1.20)

Notes: See text for description of employment variables and samples. Sample sizes are as follows. Row 1: New Jersey 163; Pennsylvania 72. Row 2: New Jersey 114; Pennsylvania 40. Row 3: New Jersey 49; Pennsylvania 23. Row 4: New Jersey 19; Pennsylvania 33.

Table 5: Estimated Regression Models for Change in Average Payroll Hours/35, BNW Data

	Specification:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New Jersey	-0.85 (0.49)	--	--	--	-0.36 (0.44)	-0.66 (0.41)	-0.09 (0.42)
NW Subample (1=yes)	--	-3.49 (0.42)	--	--	-3.44 (0.43)	--	--
Chain Dummies:							
Roy Rogers	--	--	-3.56 (0.81)	--	--	-3.14 (0.85)	-1.98 (0.89)
Wendy's	--	--	-0.85 (0.67)	--	--	-0.71 (0.67)	-1.35 (0.70)
KFC	--	--	-6.51 (0.90)	--	--	-6.30 (0.90)	-6.56 (0.89)
Company-Owned	--	--	-0.89 (0.76)	--	--	-1.31 (0.81)	-0.72 (0.95)
Payroll Data Type:							
Bi-weekly	--	--	--	1.73 (0.52)	--	--	1.65 (0.52)
Monthly	--	--	--	-2.60 (0.48)	--	--	-1.06 (0.89)
R-squared	0.01	0.23	0.41	0.30	0.23	0.10	0.45
Standard Error of Regression	3.47	3.07	2.70	2.95	3.08	3.32	2.62

Notes: Standard errors in parentheses. Sample consists of 235 stores. Dependent variable in all models is the change in average weekly payroll hours divided by 35 between wave 1 and wave 2.

Table 6: Estimated Relative Employment Effects in New Jersey for Alternative Samples and Specifications, BNW Data

	Pooled BNW Sample	Neumark-Wascher Sample	Original Berman Sample
A. Change in Average Payroll Hours/35			
1. No Controls	-0.85 (0.49)	-0.05 (0.61)	-1.28 (0.63)
2. Exclude First PA Franchisee, No Controls	0.37 (0.56)	-0.11 (0.62)	--
3. Controls for Chain and Ownership	-0.66 (0.41)	-0.27 (0.53)	-0.95 (0.64)
4. Controls for Chain, Ownership and Payroll Period	-0.09 (0.42)	-0.22 (0.54)	0.58 (0.72)
B. Change in Payroll Hours/35 Using First Pay Period per Restaurant			
5. No Controls	-0.55 (0.50)	0.18 (0.63)	-0.85 (0.67)
6. Controls for Chain, Ownership and Payroll Period	0.07 (0.43)	-0.15 (0.52)	0.96 (0.78)
C. Proportional Change in Average Payroll Hours/35			
7. No Controls	-0.06 (0.05)	-0.00 (0.07)	-0.09 (0.07)
8. Controls for Chain and Ownership	-0.05 (0.05)	-0.03 (0.07)	-0.04 (0.07)
9. Controls for Chain, Ownership and Payroll Period	-0.01 (0.05)	-0.03 (0.07)	-0.08 (0.08)

Notes: Pooled BNW sample has 235 observations; NW sample has 154 observations; original Berman sample has 71 observations. In row 2, data for 26 restaurants owned by one franchisee are excluded. In this row only, pooled BNW sample has 209 observations; and NW sample has 151 observations. Dependent variable in panel A is the change in average payroll hours between the first and second waves, divided by 35. Dependent variable in panel B is the change in payroll hours for the first payroll period reported by each store between the first and second waves, divided by 35. Dependent variable in panel C is the change in average payroll hours between the first and second waves, divided by average payroll hours in the first and second waves. Standard errors in parentheses.

Table 7: Comparisons of Employment Growth by Chain and Zip-code Area, Card-Krueger data versus Berman-Neumark-Wascher Data

	NJ and PA	NJ Only	PA Only
A. Using Combined BNW Sample			
Intercept	-0.32 (0.56)	0.41 (0.50)	-3.91 (1.77)
Change in Employment in BNW Sample	0.78 (0.22)	0.90 (0.19)	0.65 (0.68)
R-squared	0.22	0.38	0.09
Standard Error	8.97	7.35	10.76
P-value: intercept=0, slope=1	0.47	0.65	0.07
Number of Observations (Chain x Zip-code Cells)	48	37	11
B. Using Combined BNW Sample Excluding Data from One Franchisee			
Intercept	-0.26 (0.54)	0.36 (0.51)	-3.52 (1.67)
Change in Employment in BNW Sample	0.87 (0.21)	0.91 (0.20)	0.93 (0.73)
R-squared	0.28	0.39	0.17
Standard Error	8.56	7.40	10.27
P-value: intercept=0, slope=1	0.71	0.72	0.14
Number of Observations (Chain x Zip-code Cells)	46	36	10

Notes: Dependent variable in all models is the mean change in full-time employment for fast-food restaurants of a specific chain in in a specific 3-digit zip code area, taken from the Card-Krueger data set. Independent variable is the mean change in payroll hours divided by 35 for fast-food restaurants (in the same chain and zip code area) taken from the BNW data set. In panel B, restaurants in the BNW sample obtained from the franchisee who provided Berman's Pennsylvania data are deleted prior to forming average employment changes by chain and zip code area. All models are fit by weighted least squares using as a weight the number of observations in the chain-by-zip-code cell in the Card-Krueger data set. Standard errors in parentheses.

Appendix Table 1: Effects of Payroll Period on Payroll Hours in Waves 1 and 2 and on Changes in Payroll Hours, BNW Data

	Hours/35, Wave 1			Hours/35, Wave 2			Change in Hours/35			Burger King Franchisees Change in Hours/35		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Biweekly Payroll Data	-3.25	--	-2.65	-1.57	--	-1.00	1.68	--	1.65	1.11	--	1.03
	(0.94)		(0.98)	(0.99)		(1.03)	(0.49)		(0.52)	(0.34)		0.40
Monthly Payroll Data	-2.66	--	-3.11	-3.74	--	-4.17	-1.09	--	-1.06	-0.91	--	-0.89
	(1.68)		(1.68)	(1.77)		(1.77)	(0.88)		(0.89)	(0.65)		(0.66)
New Jersey	--	2.11	1.67	--	1.44	1.58	--	-0.66	-0.09	--	-0.84	-0.17
		(0.76)	(0.80)		(0.79)	(0.84)		(0.41)	(0.42)		(0.37)	(0.41)
R-Squared	0.11	0.09	0.12	0.24	0.23	0.25	0.45	0.42	0.45	0.15	0.04	0.15
Standard Error	4.98	5.02	4.94	5.24	5.25	5.21	2.61	2.69	2.62	1.59	1.67	1.60
P-value for Excluding Payroll Type Variables[a/]	0.002	--	0.013	0.065	--	0.059	0.000	--	0.001	0.001	--	0.001

Notes: Models in columns 1-9 are estimated on sample of 235 restaurants. Models in columns 10-12 are estimated on sample of 94 Burger King restaurants only. Models in columns 1-9 include controls for chain and company ownership. Standard errors in parentheses.

[a/]Probability value of F-test for exclusion of two indicators for type of payroll data.

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Estimating the Employment Impact of the 1996 Minimum Wage Increase Using Deere, Murphy, and Welch's Approach

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I. Introduction

Deere, Murphy, and Welch (1995) (hereafter, DMW) examined the impact of the 1990 and 1991 increases in the federal minimum wage on teenagers and adults with less than a high school education. After controlling for general labor market conditions, they concluded that the 1990 and 1991 hikes significantly reduced employment of these generally low-wage groups (see Appendix Table 1, which reproduces their principal findings).

This paper applies their method for estimating the employment effects of the minimum wage to the October 1996 increase. In contrast to the DMW results, we find that the most recent increase in the minimum wage generally did not affect the employment opportunities of teenagers and adults with less than a high school education.

II. Description of Data

DMW analyzed data from the Current Population Survey's (CPS) Outgoing Rotation Group (ORG) for 12 month periods beginning in April of each year from 1985-92. (Both the 1990 and 1991 increases took place on April 1.) We use data from both the CPS ORG and the full monthly CPS for six-month periods beginning in October of each year from 1991 through 1996. The first five six-month periods correspond to a federal minimum wage of $4.25. The last six-month period, which runs from October 1996 through March 1997, corresponds to the first six months of the new $4.75 minimum wage.

The full monthly CPS, which includes as a subset all members of the ORG, has a sample size four times that of the ORG. Our analysis therefore relies most heavily on the full monthly CPS. The structure of the CPS, however, poses a potential problem to analysis based on the full monthly CPS. Respondents to the full monthly CPS appear in the survey for four consecutive months, leave the survey for eight months, and then enter the survey again for four more months. This means that month-to-month samples of the full monthly CPS are not independent from one another. (The ORG, which is made up of individuals in either their first or second four-month appearance in the survey, in contrast, is independent from month to month.) The nonrandom nature of month-to-month changes in the composition of the survey could affect our estimates in ways that are difficult to model. We therefore also report our key results using the smaller, independent ORG sample.

In 1994, the CPS underwent significant design changes that had an important impact on the measurement of employment rates of younger workers and many women (see Polivka (1996)). We adopt several strategies for coping with this change. Our preferred approach is to limit the analysis to the new survey format (from 1994 to the present). We also report results for the full period since the 1991 increase with and without dummy variable controls for the old survey format.

Following DMW, we separately study the impact on male, female, and black teenagers (ages 15-19), and on male, female, and black adults (ages 20-54) with less than a high school education by constructing a panel data set with the 51 states (including the District of Columbia) as the unit of analysis.

III. Framework and Results

DMW regress the natural log of the employment-to-population rate for each of the six groups in each state in each year (beginning in April) against the natural log of the employment-to-population rate for males ages 15-64 (as a control for the business cycle), and separate dummy variables for the years 1990 , when the federal minimum wage rose to $3.80, and 1991 and 1992, when the federal minimum wage rose again to $4.25. To control for the long-term rise in employment rates of women, DMW include a trend term in the women's regressions. Given the small sample sizes for blacks in each CPS, they also combined data for male and female blacks and included a dummy variable for state-year observations corresponding to men.

Table 1 shows the outcome of a similar analysis for October 1, 1996 increase using data from the full monthly CPS for the three six-month periods October-March 1994-95, 1995-96, and 1996-97. We find a statistically significant decline in employment for teenage males, but no negative effects on the other five groups. The coefficient estimates for female and black teens, and less-than-high-school educated adult men, women, and blacks are all positive and statistically significant in the case of the adult men.

Table 2 reports results from similar regressions on just the ORG data. The basic pattern changes little. The employment rate of teen males appear to fall, though the result is now statistically significant at only the 10% level. The employment rate of adult men with less than a high school degree still grows significantly. Women's employment changes are very close to zero and not statistically significant. Black employment also shows no statistically significant change. (Note that the root mean squared error is uniformly higher using the smaller ORG sample and that the number of valid state-year observations for blacks is smaller than with the full monthly CPS.)

The results in Tables 1 and 2 refer to a shorter time frame (3 "years") than the one employed by DMW (8 years). The 1994 change in the CPS, however, makes it difficult for us to confidently incorporate earlier data. Table 3 illustrates the magnitude of the problems with comparability. Columns 1991 and 1992 report employment rates using the old survey; columns 1994, 1995, and 1996 report employment rates using the new survey. Column 1993 includes three months of data from October, November, and December 1993, gathered using the old survey, and three months of data from January, February, and March 1994, gathered using the new survey. The discontinuity for teenagers and adult women is apparent. For teenagers, employment rates using the new survey are generally about twice those obtained with the earlier survey. For adult women, measured employment rates appear to be 4-5 percentage points higher with the new survey. For both groups, the "1993" data, which mixes both surveys, produces employment rates that lie about halfway between the old and the new survey estimates.

Table 4 reports results from regressions using all six "years" of data with dummy variables for the old survey (1991 and 1992) and partial use of the old survey (1993). The results do not differ substantially from those in Table 1. Teen employment falls, but none of the declines is statistically significant. Employment rates for all adults increase, though only the increase for men is statistically significant. (Appendix Table 2 displays results for the same regressions without the survey controls. The results are similar to those in Table 4, except for teenage women, where the regressions implies that the 12% increase in the minimum wage in October led to an implausible 29% decline in female teen employment.)

Another potential objection to the results in Table 1 is that the estimated elasticities of teenage and less-than-high-school-educated adult employment with respect to the overall male employment rate are implausibly low. According to the estimates in Table 1, for example, teen female employment grows only 0.88% when overall male employment rises 1%; black teen employment actually falls 1.47% when overall male employment increases 1%. Table 5 reestimates the employment effects, imposing a uniform elasticity of 2.0 on the employment rates for all six groups. (Card and Krueger (1995) use this elasticity to adjust changes in teen employment rates for changes in overall employment.) The results differ little in qualitative terms from those in Table 1. (Appendix Table 3 reports identical regressions using the higher elasticities estimated originally by DMW. The results are similar to those in Tables 1 and 5.)

We thank Danielle Gao for research assistance.

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Appendix Table 1

TABLE 4—REGRESSION ESTIMATES OF EMPLOYMENT LOSSES FROM THE 1990–1991 INCREASE IN THE FEDERAL MINIMUM WAGE

Variable	Men	Women	Blacks
Teenagers, Aged 15–19:			
Employment rate, men aged 15–64	3.29 (18.37)	2.27 (8.19)	4.76 (6.41)
Year effects			
Minimum = $3.80	−4.78 (4.97)	−6.63 (3.73)	−7.47 (2.02)
Minimum = $4.25	−7.29 (9.04)	−11.37 (4.80)	−10.00 (3.16)
Test: other year effects, Pr > *F*	0.87	0.32	0.88
High-School Dropouts, Adults Aged 20–54:			
Employment rate, men aged 15–64	1.19 (12.18)	1.66 (7.42)	1.32 (3.50)
Year effects			
Minimum = $3.80	−1.49 (2.93)	−2.54 (1.80)	−4.43 (2.33)
Minimum = $4.25	−3.13 (7.14)	−5.17 (2.72)	−6.66 (4.10)
Test: other year effects, Pr > *F*	0.19	0.924	0.39

Notes: Observations are aggregates by state and year (1985–1992). Years begin April 1. The dependent variable is the natural log of the employment / population ratio for the group indicated. The aggregate employment variable refers to men only and is also a natural logarithm. The minimum-wage variables are indicators for 1990 (when $3.80 applied) and 1991 and 1992 (when $4.25 applied). This specification has one indicator that identifies the 1990 observations and another that identifies the 1991 and 1992 observations without distinguishing between them. The coefficients on the minimum-wage indicators have been multiplied by 100. The rows labeled "Test: other year effects, Pr > *F*" refer to the alternative hypothesis that each of the eight years has an independent effect. The regressions that are restricted to women include a trend term; the regressions for blacks include pooled observations for men and women with an indicator that distinguishes between them. All regressions also include state effects. Numbers in parentheses are *t* statistics.

Source: Reproduced from Deere, Murphy, and Welch (1995).

APPENDIX TABLE 2
Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage, No Survey Controls
CPS ORG, 1991-97

	Men		Women		Blacks	
(a) Teenagers 15-19						
Employment Rate of Men 15-64	9.57	**	3.92	**	8.88	**
	(0.38)		(0.58)		(0.88)	
Year Effect, Minimum Wage = $4.75	-1.01		-28.55	**	6.46	
	(4.02)		(5.00)		(8.29)	
Root MSE	0.236		0.232		0.549	
Sample Size	306		305		381	
(b) Less than High School Adults 20-54						
Employment Rate of Men 15-64	0.57	**	1.20	**	1.13	
	(0.12)		(0.30)		(0.44)	
Year Effect Minimum Wage = $4.75	2.27	*	3.55		5.82	
	(1.14)		(2.43)		(4.17)	
Root MSE	0.068		0.113		0.283	
Sample Size	306		306		410	

Source: CPS Outgoing Rotation Group, October-March 1991-97.

Notes:
(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.
(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.
(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.
(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.
(5) Regressions exclude controls for the pre-1994 version of the CPS survey.

APPENDIX TABLE 3

Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage, Fixing Coefficient on Adult Male Employment Using Estimates from Deere, Murphy, and Welch (1995)

Full Monthly CPS, 1994-97

	Men		Women	Blacks
(a) Teenagers 15-19				
Employment Rate of Men 15-64	3.29 (--)		2.27 (--)	4.76 (--)
Year Effect, Minimum Wage = $4.75	-3.11 (1.42)	*	0.19 (3.93)	3.45 (5.33)
(b) Less than High School Adults 20-54				
Employment Rate of Men 15-64	1.19 (--)		1.66 (--)	1.32 (--)
Year Effect Minimum Wage = $4.75	2.67 (0.87)	**	-1.95 (1.45)	1.44 (3.11)

Source: Full monthly CPS, October-March 1994-97.

Notes:

(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.

(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.

(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.

(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.

(5) Coefficient on adult male employment fixed to estimate for corresponding regressions Deere, Murphy, and Welch (1995), Table 4.

TABLE 1
Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage
Full Monthly CPS, 1994-97

	Men		Women		Blacks	
(a) Teenagers 15-19						
Employment Rate of Men 15-64	2.52	**	0.88		-1.47	
	(0.55)		(0.73)		(1.88)	
Year Effect, Minimum Wage = $4.75	-2.93	*	2.10		4.96	
	(1.41)		(4.01)		(5.21)	
Root MSE	0.082		0.113		0.365	
Sample Size	153		153		228	
(b) Less than High School Adults 20-54						
Employment Rate of Men 15-64	1.53	**	1.29	**	2.12	#
	(0.31)		(0.49)		(1.09)	
Year Effect Minimum Wage = $4.75	2.55	**	0.24		1.33	
	(0.87)		(2.69)		(3.12)	
Root MSE	0.050		0.075		0.221	
Sample Size	153		153		238	

Source: Full monthly Current Population Survey, October-March 1994-97.

Notes:
(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.
(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.
(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.
(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.

TABLE 2
Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage
CPS ORG, 1994-97

	Men		Women		Blacks	
(a) Teenagers 15-19						
Employment Rate of Men 15-64	3.18	**	0.35		1.08	
	(0.61)		(0.74)		(2.19)	
Year Effect, Minimum Wage = $4.75	-3.69	#	0.02		-1.58	
	(1.93)		(4.68)		(7.43)	
Root MSE	0.111		0.133		0.485	
Sample Size	153		153		196	
(b) Less than High School Adults 20-54						
Employment Rate of Men 15-64	1.72	**	1.60	**	2.76	*
	(0.37)		(0.56)		(1.14)	
Year Effect Minimum Wage = $4.75	2.71	*	-0.06		5.55	
	(1.18)		(3.42)		(4.08)	
Root MSE	0.067		0.096		0.273	
Sample Size	153		153		213	

Source: CPS Outgoing Rotation Group, October-March 1994-97.

Notes:
(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.
(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.
(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.
(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.

TABLE 3
Employment-to-Population Rates, Full CPS and ORG, 1991-97

	Six Month Period Beginning October					
	1991	1992	1993	1994	1995	1996
(a) Teenagers, 15-19						
Males, Full CPS	--	--	--	0.349	0.335	0.337
Males, ORG	0.157	0.157	0.254	0.346	0.337	0.335
Females, Full CPS	--	--	--	0.351	0.342	0.352
Females, ORG	0.147	0.150	0.249	0.348	0.340	0.352
Black Males, Full CPS	--	--	--	0.199	0.181	0.177
Black Males, ORG	0.094	0.089	0.137	0.197	0.180	0.172
Black Females, Full CPS	--	--	--	0.179	0.220	0.225
Black Females, ORG	0.101	0.101	0.148	0.187	0.209	0.228
(b) Less than High School Adults 20-54						
Males, Full CPS	--	--	--	0.710	0.712	0.733
Males, ORG	0.693	0.696	0.698	0.718	0.711	0.735
Females, Full CPS	--	--	--	0.440	0.436	0.449
Females, ORG	0.398	0.385	0.405	0.433	0.431	0.450
Black Males, Full CPS	--	--	--	0.538	0.526	0.526
Black Males, ORG	0.544	0.523	0.535	0.545	0.517	0.531
Black Females, Full CPS	--	--	--	0.403	0.379	0.385
Black Females, ORG	0.360	0.334	0.349	0.408	0.358	0.405

Source: Full monthly CPS and ORG.

TABLE 4
Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage
CPS ORG, 1991-97

	Men		Women		Blacks	
(a) Teenagers 15-19						
Employment Rate of Men 15-64	5.11	**	2.20	**	3.10	*
	(0.56)		(0.61)		(1.56)	
Year Effect, Minimum Wage = $4.75	-4.69		-3.07		-1.59	
	(3.46)		(5.89)		(8.39)	
Root MSE	0.197		0.213		0.535	
Sample Size	306		305		381	
(b) Less than High School Adults 20-54						
Employment Rate of Men 15-64	1.14	**	1.09	**	1.25	
	(0.20)		(0.35)		(0.79)	
Year Effect Minimum Wage = $4.75	2.85	*	4.54		5.91	
	(1.15)		(3.18)		(4.33)	
Root MSE	0.066		0.113		0.283	
Sample Size	306		306		410	

Source: CPS Outgoing Rotation Group, October-March 1991-97.

Notes:
(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.
(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.
(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.
(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.
(5) All regressions include controls for the pre-1994 version of the CPS survey.

TABLE 5

Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage, Fixing Coefficient on Adult Male Employment

Full Monthly CPS, 1994-97

	Men		Women	Blacks
(a) Teenagers 15-19				
Employment Rate of Men 15-64	2.00 (--)		2.00 (--)	2.00 (--)
Year Effect, Minimum Wage = $4.75	-2.81 (1.41)	*	0.56 (3.90)	4.13 (5.23)
(b) Less than High School Adults 20-54				
Employment Rate of Men 15-64	2.00 (--)		2.00 (--)	2.00 (--)
Year Effect Minimum Wage = $4.75	2.39 (0.87)	**	-0.95 (2.58)	1.34 (3.11)

Source: Full monthly CPS, October-March 1994-97.

Notes:

(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.

(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.

(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.

(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.

Table 20. Employed wage and salary workers paid hourly rates with earnings at and around the prevailing Federal minimum wage, by State, fourth quarter 1997 averages, not seasonally adjusted

Number of workers (in thousands) — Continued

	$5.15		$5.16 to $5.64		$5.65 to $6.14		$6.15 to $6.64		$6.65 to $7.14		$7.15 or more	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Total, 16 years and over	2,115	3.0	4,411	6.2	5,471	7.7	3,361	4.7	4,783	6.7	46,818	65.9
Alabama	61	4.8	80	6.3	103	8.2	78	6.2	101	8.1	755	60.0
Alaska	2	.9	4	2.3	12	6.5	3	1.6	7	4.0	150	83.3
Arizona	41	3.4	71	5.9	113	9.4	52	4.3	95	7.9	744	62.1
Arkansas	48	7.6	56	9.0	48	7.7	39	6.2	51	8.2	341	54.4
California	219	2.7	576	7.2	718	9.0	390	4.9	564	7.0	5,135	64.2
Colorado	15	1.3	56	4.9	58	5.1	70	6.1	91	7.9	814	70.8
Connecticut	6	.7	51	5.4	49	5.1	15	1.6	72	7.6	692	73.0
Delaware	1	.7	3	1.8	18	9.6	9	4.6	11	5.9	135	70.2
District of Columbia	–	–	4	3.5	11	9.9	2	2.1	8	7.3	80	71.3
Florida	111	3.0	278	7.4	419	11.2	170	4.6	284	7.6	2,237	59.9
Georgia	79	4.3	107	5.8	140	7.5	90	4.8	99	5.3	1,220	65.7
Hawaii	–	–	23	7.7	11	3.7	20	6.8	19	6.4	213	72.8
Idaho	14	3.7	24	6.4	29	7.7	23	6.0	31	8.0	241	63.5
Illinois	71	2.4	145	4.8	235	7.8	141	4.7	246	8.2	1,975	65.8
Indiana	49	2.6	117	6.2	137	7.3	69	3.7	84	4.5	1,331	71.1
Iowa	28	3.2	51	6.0	42	4.9	30	3.5	78	9.2	587	69.1
Kansas	21	3.1	68	9.8	71	10.4	29	4.3	49	7.1	425	61.7
Kentucky	28	2.8	59	5.7	66	6.4	71	6.9	69	6.7	667	65.2
Louisiana	61	5.6	67	6.1	115	10.5	54	5.0	72	6.6	661	60.5
Maine	5	1.4	24	6.5	23	6.3	20	5.4	20	5.6	256	70.7
Maryland	6	.4	107	7.9	49	3.6	34	2.5	106	7.8	981	72.1
Massachusetts	17	1.0	55	3.4	107	6.6	42	2.6	91	5.6	1,248	76.7
Michigan	44	1.5	143	4.8	205	6.8	101	3.3	155	5.1	2,182	72.6
Minnesota	44	3.0	86	5.8	107	7.3	41	2.8	102	6.9	1,022	69.6
Mississippi	63	9.0	56	8.0	58	8.2	39	5.6	63	9.0	350	49.9
Missouri	22	1.3	117	7.3	101	6.2	113	7.0	66	4.1	1,129	70.0
Montana	16	7.5	25	11.6	22	10.2	17	7.9	12	5.4	119	54.7
Nebraska	18	3.5	29	5.9	52	10.4	23	4.6	55	11.1	290	58.3
Nevada	15	2.8	18	3.4	33	6.3	14	2.7	36	6.9	367	70.8
New Hampshire	2	.7	13	3.7	21	6.4	15	4.6	26	7.6	243	72.4
New Jersey	28	1.4	109	5.5	121	6.2	84	4.3	112	5.7	1,393	70.6
New Mexico	15	3.3	35	7.8	36	8.0	14	3.1	36	8.1	285	64.0
New York	81	2.1	179	4.6	322	8.3	152	3.9	255	6.6	2,582	66.6
North Carolina	72	3.6	133	6.7	172	8.6	107	5.4	150	7.5	1,252	62.7
North Dakota	7	4.3	17	9.7	18	10.5	11	6.2	16	10.4	89	52.2

See footnotes at end of table.

Table 20. Employed wage and salary workers paid hourly rates with earnings at and around the prevailing Federal minimum wage, by State, fourth quarter 1997 averages, not seasonally adjusted

Number of workers (in thousands) — Continued

	Total employed	Total paid hourly rates		Less than $4.25		$4.25		$4.26 to $5.14		$4.75		$5.00	
		Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Ohio	3,260	3,260	100.0	96	2.9	–	–	123	3.8	3	.1	89	2.7
Oklahoma	744	744	100.0	17	2.3	3	.4	39	5.2	–	–	36	4.9
Oregon	922	922	100.0	–	–	–	–	16	1.8	–	–	12	1.3
Pennsylvania	3,245	3,245	100.0	55	1.7	4	.1	117	3.6	7	.2	84	2.6
Rhode Island	294	294	100.0	8	2.9	1	.4	6	2.1	–	–	4	1.2
South Carolina	967	967	100.0	12	1.3	6	.7	56	5.8	3	.3	46	4.8
South Dakota	217	217	100.0	3	1.2	1	.5	8	3.6	–	–	7	3.0
Tennessee	1,413	1,413	100.0	19	1.3	–	–	58	4.1	7	.5	34	2.4
Texas	4,993	4,993	100.0	97	2.0	9	.2	261	5.2	31	.6	187	3.7
Utah	579	579	100.0	6	1.0	–	–	29	5.0	–	–	27	4.7
Vermont	178	178	100.0	3	1.5	1	.4	10	5.4	–	–	7	4.1
Virginia	1,673	1,673	100.0	32	1.9	–	–	64	3.8	–	–	64	3.8
Washington	1,635	1,635	100.0	4	.3	4	.3	55	3.4	–	–	42	2.6
West Virginia	453	453	100.0	11	2.4	–	–	16	3.5	–	–	10	2.3
Wisconsin	1,764	1,764	100.0	34	1.9	8	.5	43	2.4	7	.4	28	1.6
Wyoming	123	123	100.0	3	2.5	–	–	3	2.5	1	.7	2	1.8

See footnotes at end of table.

Table 20. Employed wage and salary workers paid hourly rates with earnings at and around the prevailing Federal minimum wage, by State, fourth quarter 1997 averages, not seasonally adjusted

Number of workers (in thousands) — Continued

	$5.15		$5.16 to $5.64		$5.65 to $6.14		$6.15 to $6.64		$6.65 to $7.14		$7.15 or more	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Ohio	99	3.0	184	5.6	212	6.5	190	5.8	176	5.4	2,181	66.9
Oklahoma	32	4.3	58	7.7	77	10.3	44	5.9	59	7.9	417	56.0
Oregon	3	.3	60	6.5	77	8.4	51	5.6	69	7.5	645	70.0
Pennsylvania	154	4.8	213	6.6	150	4.6	170	5.3	158	4.9	2,224	68.5
Rhode Island	7	2.2	18	6.1	22	7.4	21	7.3	21	7.1	190	64.5
South Carolina	51	5.2	48	5.0	58	6.0	48	5.0	48	5.0	639	66.1
South Dakota	5	2.1	17	7.8	24	10.9	10	4.7	25	11.4	125	57.7
Tennessee	8	.5	57	4.0	107	7.6	95	6.7	116	8.2	954	67.5
Texas	279	5.6	395	7.9	489	9.8	243	4.9	364	7.3	2,856	57.2
Utah	8	1.4	28	4.8	62	10.7	31	5.4	59	10.3	356	61.5
Vermont	2	1.1	9	5.2	15	8.1	4	2.1	19	10.5	117	65.7
Virginia	62	3.7	62	3.7	100	6.0	74	4.4	107	6.4	1,173	70.1
Washington	38	2.3	79	4.8	100	6.1	92	5.6	88	5.4	1,175	71.9
West Virginia	22	4.9	55	12.1	37	8.1	17	3.9	29	6.4	266	58.7
Wisconsin	32	1.8	124	7.0	92	5.2	83	4.7	103	5.8	1,245	70.6
Wyoming	5	4.4	18	15.0	10	8.4	8	6.4	9	7.4	66	53.4

>0 Value too small to display.
– Data not available.

SOURCE: U.S. Department of Labor, Bureau of Labor Statistics, unpublished tabulations from the Current Population Survey, 1997.

NOTE: Data exclude the incorporated self employed. Detail for the above race and Hispanic-origin groups will not sum to totals because data for the "other races" group are not are not presented and Hispanics are included in both the white and black population groups. Also note that the distinction between full and part-time workers is based on hours usually worked. These data will not sum to totals because full or part-time status on the principal job is not identifiable for a small number of multiple jobholders. Users are cautioned that these data are based on a sample and are therefore subject to sampling error; the degree of error may be quite large for less populous States. It should be noted that it is not possible to determine whether workers surveyed in the CPS are actually covered by the Fair Labor Standards Act or by individual State minimum wage laws. Thus, some workers being reported as earning an hourly wage of $5.15 may not, in fact, be covered by Federal or State minimum wage laws; at the same time, the presence of a sizable number of workers with wages below the prevailing Federal minimum wage does not necessarily indicate violations of the Fair Labor Standards Act or applicable State laws, because there are numerous exemptions to these mimimum wage statutes.

Bureau of Labor Statistics
Office of the Commissioner
Facsimile Cover Sheet

Room 4040, Postal Square Building
2 Massachusetts Avenue, NE
Washington, DC 20212

To: Mr. Jon Orszag

Organization: NEC
Phone: 456-5367
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From: Bill Parks
Title: Special Asst. to the Commissioner
Office Phone: 606-7807
Office Fax: 606-7797

Date: 3/18/ 98
Pages including this cover page: 11

Comments: I'm sending this on the "Super fine" resolution (!) on our new fax machine. Hopefully it will be legible.

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Briefing Paper

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THE SKY HASN'T FALLEN

An Evaluation of the Minimum-Wage Increase

By Jared Bernstein and John Schmitt

In the summer of 1996, Congress voted to increase the federal minimum wage from $4.25 to $5.15 per hour. On August 20, 1996, President Clinton signed the legislation, which implemented the increase in two stages: a $0.50 per hour increase on October 1, 1996, followed by a $0.40 increase on September 1, 1997. Supporters argued that the bill would raise the earnings of millions of low-wage workers and primarily benefit the poorest working families. Opponents claimed that the increase would lead to widespread job loss among the very workers the legislation was intended to help.

The first stage of the increase has been in effect for over six months, and it is now possible to investigate these claims. This paper examines data from the U.S. government's monthly Current Population Survey for the six months from October 1996 through March 1997 (and for the same period a year earlier) to assess the impact of the minimum-wage increase on the distribution of wages, the distribution of family income, and the employment opportunities of low-wage workers. While the analysis deals only with the first six months at the new wage level, these early results suggest clearly that:

- the higher minimum wage has substantially boosted the earnings of low-wage workers;
- the benefits of the minimum-wage increase went primarily to low-income working families; and
- employment opportunities for teenagers and young adults — groups that opponents of the minimum wage singled out as particularly vulnerable — were not adversely affected by the increase.

The following sections look at these three findings in detail.

TABLE 3
Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage
CPS ORG, 1991-97

	Men	Women	Blacks
(a) Teenagers 15-19			
Employment Rate of Men 15-64	3.842**	1.552**	3.179*
	(0.414)	(0.450)	(1.420)
Year Effect, Minimum Wage = $4.75	-3.914#	0.805	-2.349
	(2.000)	(3.381)	(6.933)
Root MSE	0.113	0.123	0.462
Sample Size	306	306	424
(b) Less than High School Adults 20-54			
Employment Rate of Men 15-64	1.302**	1.241**	1.725*
	(0.191)	(0.320)	(0.713)
Year Effect Minimum Wage = $4.75	2.913**	8.251**	5.574
	(0.878)	(2.327)	(3.624)
Root MSE	0.050	0.083	0.244
Sample Size	306	306	438

Source: CPS Outgoing Rotation Group, October-August, 1991-97.

Notes:
(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.
(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.
(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.
(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.
(5) All regressions include controls for the pre-1994 version of the CPS survey.

Results using 11 months of data per year

Replaces Table 4 (which is based on 6 months of data per year

Table 18. Distribution of wage and salary workers paid hourly rates, fourth quarter 1997 averages, not seasonally adjusted

(Numbers in thousands) — Continued

Total

HOURLY RATE	
$7.51 - $7.54	23
$7.55 - $7.59	101
$7.60 - $7.64	106
$7.65 - $7.69	98
$7.70 - $7.74	117
$7.75 - $7.99	711
$7.75	209
$7.76 - $7.79	41
$7.80 - $7.84	144
$7.85 - $7.89	88
$7.90 - $7.94	127
$7.95 - $7.99	102
$8.00 - $8.99	7,235
$9.00 - $9.99	5,343
$10.00 or more	30,289

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SOURCE: U.S. Department of Labor, Bureau of Labor Statistics, unpublished tabulations from the Current Population Survey, 1997.

NOTE: Data exclude the incorporated self employed. Detail for the above race and Hispanic-origin groups will not sum to totals because data for the "other races" group are not are not presented and Hispanics are included in both the white and black population groups. Also note that the distinction between full and part-time workers is based on hours usually worked. These data will not sum to totals because full or part-time status on the principal job is not identifiable for a small number of multiple jobholders.

The Minimum Wage, Welfare Reform and Jobs for Low-skilled Workers

During the fourth week of August 1996, President Clinton signed into law two bills with serious implications for the ability of low-skilled workers to find jobs. On the 20th, he signed a bill that would increase the federal minimum wage from $4.25 to $4.75 effective October 1, 1996, and from $4.75 to $5.15 on September 1, 1997. On the 22nd, he signed a bill that would "end welfare as we know it," in part by requiring adult welfare recipients to find work within two years or lose their benefits. Lost in the fanfare over the signing of these two bills were the pernicious interactions that the new laws would have in the entry-level job market. Just as the welfare reform bill would be forcing hundreds of thousands of low-skilled adults into the job market, the higher minimum wage would be destroying the very jobs that could have offered these welfare recipients a first step on the ladder of economic success. Now, just as the clock is running out on those welfare recipients, organized labor and its supporters are pushing for yet another round of increases in the minimum wage.

The effect of such increases can only serve to further undermine the potential for welfare reform to succeed. Even in a booming economy, one key fact about welfare recipients cannot be ignored: most of them simply do not possess the skills needed to fill existing job vacancies. The classified ads are thick with job listings each week, but many welfare recipients lack the education and work skills required by employers. For example, two-thirds of welfare recipients scored in the bottom quartile of the Armed Forces Qualifying Test, with one-third in the bottom *decile*.[1] They might qualify for entry-level jobs if the wage rates reflected the skill levels of these applicants. Sadly, with each upward increment in the minimum wage, hundreds of thousands of entry-level job opportunities are destroyed.

Recently, in championing further increases in the minimum wage, Senator Ted Kennedy (D-MA) relied upon two erroneous claims: (1) "employment does not go down because the minimum wage goes up";[2] and (2) benefits from minimum wage increases go primarily to low-income families. In this article we outline the solid economic analysis that refutes these two claims. First, we calculate from official government employment data that the October 1, 1996, 50-cent minimum wage increase destroyed approximately 128,000 teen jobs. In other words, employment *does* go down when the minimum wage goes up, and it went down after the 1996 increase despite strong performance in the economy as a whole.[3] The more recent September 1, 1997, minimum wage increase has undoubtedly destroyed even more jobs, but it is still too early to accurately measure these additional losses. Second, we calculate from the same data that the average family income for minimum wage workers is more than $35,000, hardly what most Americans would classify as "low-income." Thus, the minimum wage increase does not target low-income families.

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Did the 1996 Increase in the Minimum Wage Destroy Jobs?

Supporters of the minimum wage legislation claimed that it would benefit millions of low-wage workers without causing significant job loss. Opponents challenged this free-lunch notion, citing overwhelming evidence to the contrary from academic studies that examined the employment effects of previous increases in the federal minimum wage (see Deere, Murphy and Welch, *American Economic Review* 85:2, 1995, pp. 232-237). We add to this existing body of evidence by providing the first comprehensive study of the employment effects associated with the October 1, 1996, minimum wage increase.

Both supporters and critics of the minimum wage agree that any negative employment effects associated with minimum wage increases are most easily detected by observing teenaged workers, who account for approximately one-third of all minimum wage workers. Based upon an analysis of changes in teen employment following the 1996 increase in the federal minimum wage, we estimate that 128,000 teen jobs were destroyed by that increase. To the extent that teenaged workers are representative of all minimum wage workers, these findings imply that approximately 380,000 entry-level jobs were destroyed by the 1996 minimum wage increase.

Change in Employment-to-Population Ratios Following the 1996 Minimum Wage Increase

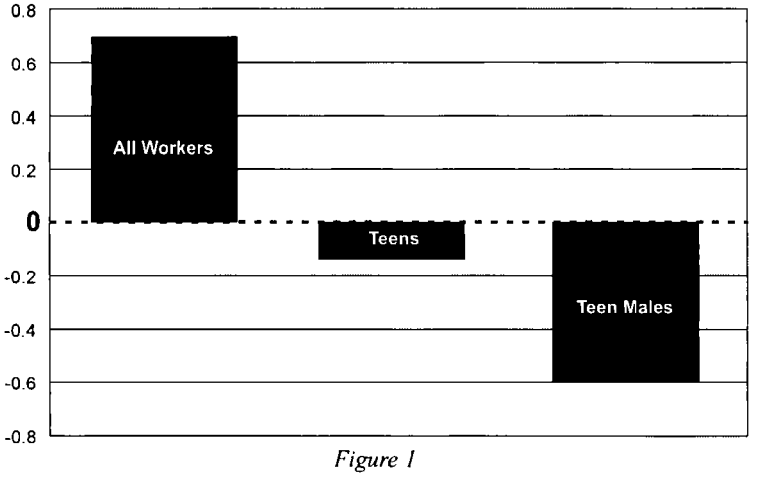

Figure 1

We used employment data from the U.S. Department of Labor in a straightforward test of whether or not the 1996 increase in the federal minimum wage reduced employment from what it would have been in the absence of the increase.[4] As shown in Panel A of Table 1, we found that the employment rate for all workers increased by 0.70 percentage points, from 62.89 percent to 63.59 percent. For teen workers age 16-19, however, the employment rate *declined* by 0.14 percentage points, from 43.27 percent to 43.13 percent.[5] For teen males, the decline was especially severe, falling 0.60 percentage points from 43.34 percent to 42.74 percent.[6]

To put these teen employment changes into perspective, it is useful to translate them into job losses. In order to do this, however, we need a benchmark to control for the effect on employment of changes in economic conditions across the two measurement periods. From October 1995 through August 1997, strong economic growth led to the creation of more than 4.5 million jobs. The most logical benchmark for measuring changes in teen employment is the change in the employment rate for all workers, i.e., the change in employment that would have occurred if the teen employment rate had increased by 0.70—the average employment-rate change for all workers from the period before to the period after the 1996 minimum wage

increase. Previous research has shown that teen employment closely tracks aggregate employment (again, see Deere, Murphy and Welch, 1995). Therefore, we calculate employment changes due to the minimum wage increase as the actual change in employment relative to the change in employment projected using this benchmark.

As shown in Panel B of Table 1, we calculate that teen employment would have increased by 107,000 jobs had the teen employment rate increased by 0.70 percentage points—the employment-rate change for all workers and the expected employment change for teens had there not been an increase in the minimum wage. Actual teen employment, however, declined by 21,000 jobs. Hence, for all teens, the number of lost jobs due to the minimum wage increase is the combined projected (unrealized) increase and actual decrease, or 128,000 (107,000 and -21,000). Because teen workers account for only one in three minimum wage workers, this analysis suggests that approximately 380,000 jobs were destroyed by the 1996 minimum wage increase from $4.25 to $4.75. Moreover, there are at least twice as many workers who were affected by the yet-unexamined September 1, 1997, minimum wage increase from $4.75 to $5.15. The number of jobs destroyed by that increase is likely to be much larger, especially coming so soon (11 months) after the prior increase.

Teen Job Loss Following 1996 Minimum Wage Increase

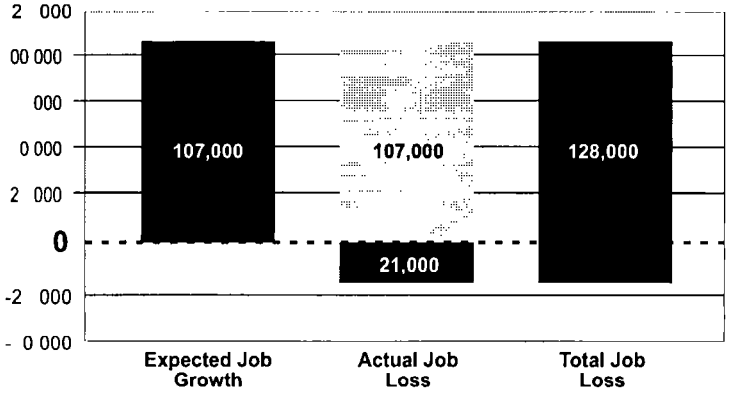

Figure 2

The 1996 wage hike (50 cents) was the first mandated increase to follow several years of strong economic growth. (Prior to 1996, the minimum wage last increased in 1991.) Thus, the effects of the 1996 wage hike were, to some extent, muted by the fact that market wages had risen with the strong economy, making the initial 50-cent increase less "binding" in the workforce. The second wage hike (40 cents, in 1997) may have had a larger impact because the new mandated pay floor was much closer to — if not above — the market rate of pay for entry-level employees nationwide. However, we won't be able to accurately measure that number until the increase has been in effect long enough to trigger all adjustments. Empirical research suggests about one year of data are needed to accurately measure such employment effects (see Deere, Murphy and Welch, 1995).

Table 1 also breaks out teen job losses due to the minimum wage increase by gender. This breakout reveals that the pain of the minimum wage increase was borne disproportionately by teen male workers. Of the 128,000 teen jobs that were lost, teen males lost 102,000, accounting for almost 80 percent of the total teen job loss. This asymmetry of job loss shows how important it is to consider the effects of minimum wage legislation on demographic subgroups as well as the aggregate effects.

Table 2 breaks out teen job losses due to the minimum wage increase by race and ethnicity as well as gender.[7] Because of the small sample sizes in the Bureau of Labor Statistics survey database, the results for Black and Hispanic teens are less reliable (statistically) than the overall teen numbers or the results for White teens. Nonetheless, the change in employment for Black and Hispanic teens is instructive. The figures in Panel A of Table 2 reveal that

teen Black, non-Hispanic males suffered the most from the minimum wage increase. For this demographic group, the employment rate declined a full percentage point, from 25.21 percent to 24.21 percent at the same time the overall employment rate was increasing by 0.7 percentage points. As shown in Panel B of Table 2, this suggests that job losses among teen Black, non-Hispanic males total to 20,000, or more than 7 percent of the approximately 280,000 workers in this demographic group. Yet these are the types of workers minimum wage advocates would have us believe are the beneficiaries of the minimum wage increases. The data in Table 2 also suggest that teen Hispanic males suffered as a result of the 1996 minimum wage increase, with job losses at 8,000 or 2 percent of the approximately 400,000 workers in this demographic group.

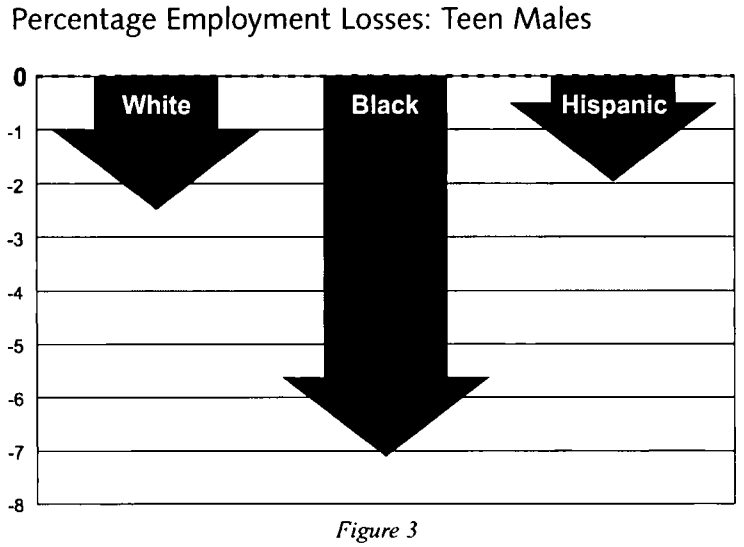

Figure 3

In summary, the data are now in and the evidence is clear. The October 1996 increase in the minimum wage from $4.25 per hour to $4.75 per hour was not so painless as its proponents would profess, despite a strong job market. A straightforward review of government employment data demonstrates that this increase hit minimum wage workers with job losses, and hit them hard. The analysis here suggests that as many as 380,000 jobs were lost, with one-third of these losses concentrated among teenagers aged 16-19. Especially vulnerable were Black males aged 16-19, as well as teen males in general. These are the faces of the "losers" from the 1996 minimum wage increase. And the pain does not stop here. During 1998, as the full effects of the second step increase in the minimum wage from $4.75 to $5.15 come into play, hundreds of thousands of additional entry-level jobs will be at risk.

Did the Minimum Wage Increase Go to Low-Income Families?

The most often cited justification for raising the minimum wage is that such increases benefit low-income families. To quote Sen. Kennedy, "No one who works for a living should have to live in poverty."[8] However, this raises a fundamental question: Do increases in the minimum wage primarily benefit low-income families? If the answer is no, then policymakers should pursue proven, effective anti-poverty tools such as the earned income tax credit, and abandon efforts to increase the minimum wage.

We used official government data from the U.S. Department of Labor to calculate who benefited from the 1996 minimum wage increase.[9] As shown in Table 3 (and Figure 4), more than 70 percent of the minimum wage gains went to families and individuals in the upper four income quintiles (those earning more than $15,777 per year), and *more than half* of the minimum wage gains went to those in the upper three income quintiles (earning more than $29,424 per year). We find that less than 18 percent of the minimum wage gains went to families and individuals living below the federally defined level of poverty, which in 1996 was $12,516 for a family of three.

We are not the first to show that the benefits of minimum wage increases go in large part to upper income families. In an analysis of the 1989 law that raised the minimum wage from $3.35 to $4.25, researchers found that the increases "yielded more income for low-wage workers living in upper income households than it did for low-wage workers who lived in poor households."[10]

When we restrict the analysis to traditional families (i.e, exclude single persons living alone), the trend is even more instructive. Almost 80% of the gains went to families in the upper four income quintiles, and *every family* in those quintiles has an income that far exceeds the poverty level for a family of four. From these numbers, it is clear that increases in the minimum wage *do not* go predominantly to low-income families, despite the rhetoric to the contrary. Even former Labor Secretary Robert Reich, a strong and vocal advocate for a higher minimum wage, has conceded that "most minimum wage workers are not poor."[11]

Distribution of 1996 Minimum Wage Gains Across All Individuals and Families, by Income Level

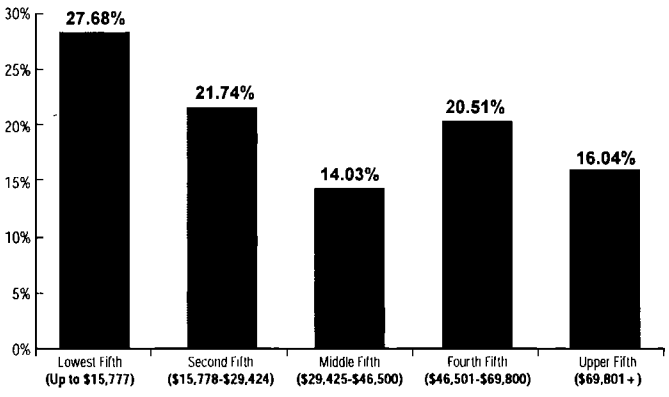

Figure 4

Conclusion

In this article, we answer the two most important questions that should be asked about increases in the minimum wage. Do increases cause job loss? Yes, and those losses traditionally have fallen most heavily on Black teens. Do most of the gains from increases go to low-income families? No. Less than one dollar in five goes to low-income families living in poverty, while more than half of the gains go to families and individuals with annual incomes greater than $29,000. Taken together, these answers imply that the minimum wage does nothing to combat poverty. Other researchers have come to a similar conclusion. For example, Neumark and Wascher ("Do Minimum Wages Fight Poverty?" NBER Working Paper No. 6127, August 1996) conclude, "On average, minimum wages appear to slightly increase the proportion of families that are poor." In other words, the number of families that fall into poverty because of job loss is greater than the number of families that rise out of poverty because of the higher minimum wage.

When 1998 draws to a close, the hundreds of thousands of low-skilled adult welfare recipients forced to seek work by the "Personal Responsibility and Work Opportunity Act of 1996" will face even fewer opportunities for work than they could have imagined. Do we really want to exacerbate this already bleak situation by increasing the minimum wage yet again, and destroying even more jobs? Or should we be looking for alternative policy prescriptions to help the working poor, such as exempting them from payroll taxes, or expanding the earned income tax credit? These are the questions we should be asking legislators as they face these important policy decisions.

Table 1: Employment Effects of the October 1, 1996, Increase in the Federal Minimum Wage from $4.25 to $4.75: Workers Age 16-19

Panel A: Average Monthly Populations and Employment-to-Population Ratios: Workers Age 16-19

	Average Monthly Population (in thousands)		Average Monthly Employment-to-Population Ratio		
	Oct. 1995-Aug. 1996	Oct. 1996-Aug. 1997	Oct. 1995-Aug. 1996	Oct. 1996-Aug. 1997	Change in Employment Rate
All Workers Age 16-64	199,974	202,417	62.89	63.59	0.70
Workers Age 16-19	14,806	15,285	43.27	43.13	-0.14[5]
Male Workers Age 16-19	7,529	7,786	43.34	42.74	-0.60[6]
Female Workers Age 16-19	7,278	7,499	43.19	43.54	0.35

Panel B: Employment Changes due to the 1996 Increase in the Federal Minimum Wage: Workers Age 16 to 19

	Projected Employment Change*	Actual Employment Change	Employment Change due to Minimum Wage Increase
All Teen Workers	107,000	-21,000	-128,000
Male Teen Workers	55,000	-47,000	-102,000
Female Teen Workers	52,000	26,000	-26,000

* The Projected Employment Change is calculated as the 0.70 percentage-point change in the employment-to-population ratio for all workers multiplied by the 1997 population of teen workers age 16-19.

Table 2: Employment Effects of the October 1, 1996, Increase in the Federal Minimum Wage from $4.25 to $4.75: Workers Age 16-19 By Race, Ethnicity and Gender

Panel A: Average Monthly Populations and Employment-to-Population Ratios: Workers Age 16 to 19

	Average Monthly Population (in thousands)		Average Monthly Employment-to-Population Ratio		
	Oct. 1995-Aug. 1996	Oct. 1996-Aug. 1997	Oct. 1995-Aug. 1996	Oct. 1996-Aug. 1997	Change in Employment Rate[7]
White, Non-Hispanic	9,940	10,136	50.20	49.95	-0.25
White, Non-Hispanic Male	5,081	5,176	49.80	49.25	-0.55
White, Non-Hispanic Female	4,859	4,961	50.62	50.68	0.06
Black, Non-Hispanic	2,293	2,349	26.70	26.64	-0.06
Black, Non-Hispanic Male	1,133	1,157	25.21	24.21	-1.00
Black, Non-Hispanic Female	1,160	1,192	28.17	29.00	0.83
Hispanic	1,925	2,074	32.09	32.98	0.89
Hispanic Male	981	1,084	36.49	36.46	-0.03
Hispanic Female	944	990	27.52	29.18	1.66

Panel B: Employment Changes due to the 1996 Increase in the Federal Minimum Wage: Workers Age 16 to 19

	Projected Employment Change	Actual Employment Change	Employment Change due to Minimum Wage Increase
White, Non-Hispanic	71,000	-25,000	-96,000
White, Non-Hispanic Male	36,000	-28,000	-64,000
White, Non-Hispanic Female	35,000	3,000	-32,000
Black, Non-Hispanic	16,000	-1,000	-17,000
Black, Non-Hispanic Male	8,000	-12,000	-20,000
Black, Non-Hispanic Female	8,000	10,000	2,000
Hispanic	15,000	18,000	3,000
Hispanic Male	8,000	0	-8,000
Hispanic Female	7,000	16,000	9,000

Data Source: Monthly Statistics from the Bureau of Labor Statistics *Current Population Survey*.

Table 3: 1996 Minimum Wage Gains Across All Individuals and Families, by Income Level

	Individuals and Families			Families		
	Minimum Wage Share	Average Family Size	Upper Income Limit of Quintile	Minimum Wage Share	Average Family Size	Upper Income Limit of Quintile
Lowest Fifth	27.68%	2.56	$15,777	21.63%	3.51	$19,200
Second Fifth	21.74%	2.93	$29,424	21.37%	3.43	$33,815
Middle Fifth	14.03%	3.24	$46,500	17.79%	3.58	$50,000
Fourth Fifth	20.51%	3.62	$69,800	22.46%	3.80	$73,139
Upper Fifth	16.04%	3.84	*	16.76%	3.91	*

*This quintile includes the richest American families and has no upper income limit.

Data Source: Monthly Statistics from the Bureau of Labor Statistics *Current Population Survey*.

Endnotes

[1] See Krista Olson and LaDonna Pavetti, *Personal and Family Challenges to the Successful Transition from Welfare to Work*, The Urban Institute, 1996.

[2] Statement of Senator Edward M. Kennedy, *U.S. Congressional Record*, July 11, 1997, Vol. 143, No. 98 p.S7303.

[3] This is generally accepted as fact by economists. A recent survey of labor economists co-authored by Alan Krueger (who believes higher minimum wages may *increase* employment) reports that "the median labor economist reported that a 10 percent increase in the minimum wage would be associated with a 1 percent decrease in teenage employment." See p. 6 of Victor Fuchs, Alan Krueger and James Poterba, "Why do Economists Disagree about Policy," NBER Working Paper, August 1997.

[4] Specifically, we used data from the Bureau of Labor Statistics *Current Population Survey* to compare the average monthly employment-to-population ratio ("employment rate") during the full 11 months for which the $4.75-per-hour minimum wage was in effect (October 1, 1996-August 31, 1997) with the average monthly employment rate during the same 11 months one year earlier (October 1, 1995-August 31, 1996). This type of comparison avoids problems of seasonality that arise when comparing different time periods within the calendar year. As we are looking at 11-month averages, this approach also minimizes the effects of monthly fluctuations in the data. We look at 11 months of data because the minimum wage increased from $4.75 to $5.15 on September 1, 1997, so that the $4.75 minimum wage was in effect for only 11 months.

[5] The -0.14 decline in teen employment is statistically different from the +0.70 increase in overall employment at the 10% level using a two-tail test, and is significantly less than the +0.70 increase at the 5% level using a one-tail test.

[6] The -0.60 decline in teen male employment is statistically different from the +0.70 increase in overall employment at the 10% level using a two-tail test, and is significantly less than the +0.70 increase at the 5% level using a one-tail test.

[7] Because of the small sample sizes for Black and Hispanic teens in the available data, the measured changes in their employment-to-population ratios cannot be reliably distinguished from the employment ratio change for all workers, using standard statistical techniques.

[8] Senator Edward M. Kennedy, speech to the National Press Club luncheon, Washington, D.C., December 12, 1997.

[9] Specifically, we used data from the Bureau of Labor Statistics March 1996 *Current Population Survey* to examine how the income gains from the minimum wage are distributed across income quintiles. We looked at workers whose hourly wages were in the range that would be affected ($4.25-$4.74) by the 1996 increase and then calculated their hourly increase based on the new minimum of $4.75. This value was multiplied by their annual hours worked in 1995 to arrive at the annual gains from the minimum wage hike. Within each quintile we maintained equal numbers of persons rather than equal numbers of families to avoid the equal families-unequal persons (and hence earners) problem. The minimum wage gain shares were then easily calculated by quintile.

[10] See p. 91 of Richard Burkhauser, Kenneth Couch and Andrew Glenn, 1996, "Public Policies for the Working Poor: The Earned Income Tax Credit versus Minimum Wage Legislation," *Research in Labor Economics* 15, 65-10.

[11] Robert B. Reich, Memorandum to the President of the United States, July 20, 1993.

Recent Publications

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The Impact of a Health Insurance Mandate on Labor Costs and Employment, by June E. O'Neill and Dave M. O'Neill, Baruch College, City University of New York, September 1993.

Table 14. Distribution of wage and salary workers paid hourly rates, by selected characteristics, fourth quarter 1997 averages, not seasonally adjusted

(Numbers in thousands)

Characteristic	Total paid hourly rates	Less than $4.25	$4.25	$4.26 to $5.14	$4.75	$5.00	$5.15	$5.16 to $5.64	$5.65 to $6.14	$6.15 to $6.64	$6.65 to $7.14	$7.15 or more
SEX AND AGE												
Total, 16 years and over	71,081	1,072	145	2,905	233	2,251	2,115	4,411	5,471	3,361	4,783	46,818
16 to 24 years	15,793	463	55	1,562	150	1,160	1,145	2,415	2,307	1,092	1,533	5,221
16 to 19 years	5,973	183	23	977	98	735	654	1,399	1,055	434	428	820
20 to 24 years	9,821	280	31	585	52	425	491	1,017	1,253	658	1,106	4,401
25 years and over	55,287	609	90	1,343	83	1,090	970	1,995	3,163	2,269	3,249	41,597
25 to 54 years	48,105	530	84	1,087	70	867	819	1,641	2,574	1,916	2,704	36,750
25 to 34 years	17,722	303	33	454	45	341	361	752	1,147	795	1,206	12,671
35 to 44 years	18,194	168	26	368	7	295	284	577	958	653	906	14,256
45 to 54 years	12,189	59	26	265	19	231	174	312	469	469	593	9,823
55 years and over	7,182	79	6	256	13	223	151	355	589	353	545	4,847
55 to 64 years	5,609	26	3	148	8	129	74	190	380	279	391	4,118
65 years and over	1,573	53	3	108	4	94	77	165	210	74	155	729
Men, 16 years and over	35,326	312	86	1,237	101	931	755	1,659	2,295	1,351	2,016	25,616
16 to 24 years	8,107	144	36	750	66	554	520	1,071	1,114	532	818	3,122
16 to 19 years	3,021	68	17	476	46	358	334	650	490	220	225	541
20 to 24 years	5,086	76	19	274	20	196	187	421	623	312	593	2,581
25 years and over	27,219	168	50	487	35	377	235	588	1,181	819	1,198	22,494
Women, 16 years and over	35,755	761	59	1,668	133	1,319	1,359	2,752	3,176	2,011	2,767	21,202
16 to 24 years	7,687	319	19	812	84	606	624	1,344	1,194	560	715	2,099
16 to 19 years	2,952	115	6	501	51	377	320	749	564	214	203	279
20 to 24 years	4,735	204	13	311	33	229	304	596	629	346	513	1,820
25 years and over	28,068	442	40	856	49	713	735	1,408	1,982	1,451	2,051	19,103
RACE AND HISPANIC ORIGIN												
White												
Total, 16 years and over	58,395	946	82	2,203	184	1,727	1,632	3,571	4,393	2,718	3,841	39,010
Men	29,425	254	42	995	85	771	631	1,357	1,862	1,093	1,603	21,586
Women	28,971	692	40	1,208	99	956	1,001	2,214	2,531	1,624	2,238	17,424
Black												
Total, 16 years and over	9,562	79	58	557	41	401	423	630	850	503	694	5,769
Men	4,357	33	44	179	11	105	106	217	344	204	296	2,935
Women	5,205	45	15	378	30	296	318	412	506	299	398	2,834

See footnotes at end of table.

Table 14. Distribution of wage and salary workers paid hourly rates, by selected characteristics, fourth quarter 1997 averages, not seasonally adjusted

(Numbers in thousands) — Continued

Characteristic	Total paid hourly rates	Less than $4.25	$4.25	$4.26 to $5.14	$4.75	$5.00	$5.15	$5.16 to $5.64	$5.65 to $6.14	$6.15 to $6.64	$6.65 to $7.14	$7.15 or more
Hispanic origin												
Total, 16 years and over	8,765	77	27	492	58	387	388	744	1,034	590	744	4,669
Men	5,262	26	17	242	28	195	158	350	551	360	424	3,134
Women	3,503	51	10	250	30	191	231	394	483	230	319	1,535
FULL- AND PART-TIME STATUS AND SEX												
Full-time workers												
Total, 16 years and over	53,422	442	70	1,157	108	900	841	1,837	3,063	2,307	3,480	40,225
Men	29,800	151	42	567	52	408	275	774	1,455	1,008	1,599	23,929
Women	23,622	291	28	590	56	492	567	1,063	1,608	1,299	1,880	16,296
Part-time workers												
Total, 16 years and over	17,529	629	75	1,738	125	1,340	1,272	2,567	2,402	1,043	1,303	6,500
Men	5,458	159	44	668	49	521	479	879	835	342	417	1,635
Women	12,071	470	31	1,070	76	820	793	1,688	1,567	701	886	4,864
FAMILY RELATIONSHIP												
Husbands	17,570	67	12	246	19	181	147	279	551	510	679	15,080
Wives	17,173	237	13	465	28	395	364	838	1,143	920	1,319	11,873
Women who maintain families	5,210	129	11	237	14	196	240	323	473	331	400	3,067
Men who maintain families	1,800	13	15	25	>0	19	16	103	106	61	130	1,333
Other persons in families:												
Men	8,226	126	44	709	62	541	444	1,018	1,068	475	695	3,647
Women	6,786	228	27	707	70	521	508	1,136	1,015	427	507	2,233
All other men [1]	7,730	106	15	257	19	190	149	259	571	305	512	5,556
All other women [1]	6,586	167	8	259	21	207	247	456	546	333	541	4,029

[1] The majority of these persons are living alone or with a non-relative

>0 Value too small to display.

SOURCE: U.S. Department of Labor, Bureau of Labor Statistics, unpublished tabulations from the Current Population Survey, 1997.

NOTE: Data exclude the incorporated self employed. Detail for the above race and Hispanic-origin groups will not sum to totals because data for the "other races" group are not are not presented and Hispanics are included in both the white and black population groups. Also note that the distinction between full and part-time workers is based on hours usually worked. These data will not sum to totals because full or part-time status on the principal job is not identifiable for a small number of multiple jobholders.

Table 18. Distribution of wage and salary workers paid hourly rates, fourth quarter 1997 averages, not seasonally adjusted

(Numbers in thousands)

Total

HOURLY RATE	
Total	71,061
Under $3.35	842
Under $2.50	547
$2.13	158
$2.14 - $2.49	157
$2.50 - $2.99	147
$3.00 - $3.34	148
$3.00 - $3.04	88
$3.05 - $3.09	15
$3.10 - $3.19	15
$3.20 - $3.29	29
$3.35	3
$3.36 - $3.49	4
$3.36 - $3.39	4
$3.40 - $3.44	1
$3.50 - $3.79	48
$3.50 - $3.54	36
$3.65 - $3.69	3
$3.75 - $3.79	10
$3.80	>0
$3.81 - $3.99	3
$3.85 - $3.89	2
$3.95 - $3.99	1
$4.00	146
$4.01 - $4.24	24
$4.05 - $4.09	1
$4.15 - $4.19	9
$4.20 - $4.24	15
$4.25	145
$4.26 - $4.49	45
$4.30 - $4.34	4
$4.35 - $4.39	33
$4.40 - $4.44	7
$4.45 - $4.49	3
$4.50 - $4.99	547
$4.50 - $4.54	146
$4.55 - $4.59	38
$4.60 - $4.64	13
$4.65 - $4.69	36

See footnotes at end of table.

Table 18. Distribution of wage and salary workers paid hourly rates, fourth quarter 1997 averages, not seasonally adjusted

(Numbers in thousands) — Continued

Total

HOURLY RATE	
$4.70 - $4.74	5
$4.75	233
$4.80 - $4.84	21
$4.85 - $4.89	17
$4.90 - $4.94	29
$4.95 - $4.99	8
$5.00 - $5.49	6,728
$5.00	2,251
$5.05 - $5.09	25
$5.10 - $5.14	38
$5.15	2,115
$5.16 - $5.19	18
$5.20 - $5.24	87
$5.25 - $5.29	1,455
$5.30 - $5.34	131
$5.35 - $5.39	216
$5.40 - $5.44	227
$5.45 - $5.49	166
$5.50 - $5.99	3,248
$5.50 - $5.74	2,273
$5.50	1,897
$5.51 - $5.54	34
$5.55 - $5.59	59
$5.60 - $5.64	120
$5.65 - $5.69	112
$5.70 - $5.74	50
$5.75 - $5.99	975
$5.75	610
$5.76 - $5.79	27
$5.80 - $5.84	81
$5.85 - $5.89	109
$5.90 - $5.94	78
$5.95 - $5.99	70
$6.00 - $6.49	5,665
$6.00	4,176
$6.01 - $6.04	28
$6.05 - $6.09	41
$6.10 - $6.14	89
$6.15	69

See footnotes at end of table.

Table 18. Distribution of wage and salary workers paid hourly rates, fourth quarter 1997 averages, not seasonally adjusted

(Numbers in thousands) — Continued

Total

HOURLY RATE	
$6.16 - $6.19	36
$6.20 - $6.24	80
$6.25 - $6.29	678
$6.30 - $6.34	88
$6.35 - $6.39	121
$6.40 - $6.44	194
$6.45 - $6.49	65
$6.50 - $6.99	2,970
$6.50 - $6.74	2,218
$6.50	1,773
$6.51 - $6.54	32
$6.55 - $6.59	113
$6.60 - $6.64	111
$6.65 - $6.69	126
$6.70 - $6.74	62
$6.75 - $6.99	752
$6.75	458
$6.76 - $6.79	35
$6.80 - $6.84	74
$6.85 - $6.89	84
$6.90 - $6.94	38
$6.95 - $6.99	63
$7.00 - $7.99	7,794
$7.00 - $7.49	5,085
$7.00	3,633
$7.01 - $7.04	76
$7.05 - $7.09	43
$7.10 - $7.14	90
$7.15	40
$7.16 - $7.19	40
$7.20 - $7.24	179
$7.25 - $7.29	515
$7.30 - $7.34	113
$7.35 - $7.39	96
$7.40 - $7.44	129
$7.45 - $7.49	130
$7.50 - $7.99	2,709
$7.50 - $7.74	1,998
$7.50	1,552

See footnotes at end of table.

TABLE 1
Determinants of Changes in State Averages of Teenage Wage and Employment Rates
(Full Monthly Current Population Survey)

	Change in Mean Log Wage		Change in Employment Rate	
	(1)	(2)	(3)	(4)
(a) 1995-96				
Fraction of Working Teens	0.116#	0.119*	-0.101*	-0.103*
Earning $4.25-4.74 in 1995	(0.059)	(0.059)	(0.040)	(0.040)
Change in Adult	--	-0.520	--	0.250
Employment Rate		(0.610)		(0.417)
R-squared	0.074	0.110	0.116	0.123
(b) 1994-96				
Fraction of Working Teens	0.129**	0.129**	-0.059	-0.060
Earning $4.25-4.74 in 1994	(0.044)	(0.044)	(0.047)	(0.043)
Change in Adult	--	-0.460	--	1.116
Employment Rate		(0.351)		(0.343)
R-squared	0.147	0.177	0.031	0.206
(c) 1994-95				
Fraction of Working Teens	0.036	0.033	0.021	0.021
Earning $4.25-4.74 in 1994	(0.055)	(0.056)	(0.039)	(0.040)
Change in Adult	--	0.461	--	-0.010
Employment Rate		(0.687)		(0.487)
R-squared	0.009	0.018	0.006	0.006

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 signicant 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.

Results using full monthly CPS to calculate state employment rates

Replaces Table 4 (which uses CPS ORG data to calculate state employment rates)

1-20-98

TJ -

Here are 2 recent papers on the minimum wage. Both are in draft form, but we feel fairly comfortable about results so far. We have a lot of updating to do as new data becomes available. Please call if you have any questions.

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Three Tests of the Employment Impact of the October 1996 Increase in the U.S. Minimum Wage

John Schmitt and Jared Bernstein

DRAFT -- COMMENTS WELCOME

December 5, 1997

I. Introduction

In the summer of 1996, the United States Congress passed, and President Bill Clinton signed into law, a bill that increased the federal minimum wage in two stages. The first stage, which went into effect on October 1, 1996, raised the federal minimum wage from $4.25 per hour to $4.75. The second stage, implemented 11 months later on September 1, 1997, further increased the minimum wage to $5.15 per hour. In the year before the increase took place, about 9.9 million workers or 8.9% of employees earned wages in the range affected by the full increase.

The legislative battle around the 1996-97 increases helped to generate two, related debates. The first concerned the potential employment impact of the increases. From the 1960s through the end of the 1980s, most research indicated that increases in the minimum wage reduced employment opportunities of low-wage workers, especially teenagers. Typical estimates suggested that a 10% increase in the minimum wage would reduce teen employment 1-3% (Brown, Gilroy, and Kohen (1982)). Beginning at the end of the 1980s, a growing body of research questioned this earlier consensus (see, for example, Wellington (1991), Card (1992a, 1992b), Katz and Krueger (1992), Card and Krueger (1994, 1995), Spriggs and Klein (1994), Belman and Wolfson (1996), and Wolfson (1997)). Most of these new studies found employment effects that were statistically or economically indistinguishable from zero. Intriguingly, some found positive employment responses to minimum wage increases.

The second, related, debate was confined to labor economists and concerned the most appropriate economic model of the low-wage labor market. The traditional estimates of employment losses pointed to a simple competitive model. The newer findings, especially those indicating positive employment effects, leant support to search-based or other friction-induced, dynamic monopsony models of the low-wage labor market (see, for example, Dickens, Machin,

and Manning (1993), Manning (1994), Palley (1995), Bhaskar and To (1996), and Acemoglu (1997)).

This paper examines data on the impact of the first stage (October 1, 1996) increase in the minimum wage on the employment of teenagers and adults without high school diplomas, groups that critics of the minimum wage argued would be most adversely affected by the increase in the minimum wage. We measure the employment effect using three different tests. The first is a simple difference-in-difference estimator of changes in employment rates of the two groups before and after the 1996 increase (see Bernstein and Schmitt (1997) for more a more detailed discussion). The second is a state-level-panel-data based test originally conducted by Deere, Murphy, and Welch (1995) to measure the impact of the 1990 and 1991 increases in the US national minimum wage. The third test exploits variation in the "bite" of the minimum-wage across states, following the procedure used by Card (1992a) and Card and Krueger (1995) to evaluate the employment impacts of the 1990 and 1991 increases.[1]

Section II briefly reviews the wage impact of the first round of the 1996 increase. Section III discusses the results of each of the tests. Section IV summarizes the findings and comments on their implications for the public policy debate on the employment effects of the minimum wage and the narrower question of modeling the low-wage labor market.

II. Wage Effects

About 3.6% of all employees --roughly 4 million workers-- received a pay raise as a result of the October 1, 1996 increase.[2] The impact of the wage increase, however, was not evenly distributed across demographic groups. Table 1A shows the distribution of wages for all workers, teens age 16-19, and adults (age 20-54) with less than high school education. The source of the underlying data is the merged Outgoing Rotation Group (ORG) of the Current Population Survey for the months of October 1995 through August 1996.[3] While only about 3.6% of all workers earned between $4.25 and $4.74 per hour prior to the first increase , 22.4% of teen workers and 6.7% of less-than-high-school-educated adults fell in this range. Black and Hispanic teens and less-educated adults were even more likely to earn low wages. About 32.7% of black teenagers and 9.1% of less-educated black adults, for example, made between $4.25 and 4.74; among Hispanic female teens, the rate was 32.4%; and for less-educated female Hispanic adults, 12.8%.

Tables 1B and 1C demonstrate that after the October 1, 1996 increase, the share of workers in the affected ranged dropped sharply. Table 1B summarizes the wage distribution from comparable CPS data over the 11 months immediately after the first-stage increase. Table 1C displays the change in the share of workers in each range between the first and the second 11-month periods. The share of all workers in the $4.25-4.74 range fell 2.1 percentage points to 1.5%. Among teens, the share dropped 15.3 percentage points to 7.1% of the total. Both black and Hispanic teens saw decreases of about 20-percentage-points, with declines especially large for black males (25.4 percentage points) and Hispanic females (27.1 percentage points). Among less-than-high-school-educated adults, the share in the affected range fell by more than half, to 2.8%,

with the largest drops among black (-6.2) and Hispanic (-8.2) females.

Some of the workers in Tables 1A and 1B have hourly earnings below the federal minimum wage. Several factors account for these subminimum wages. First, the Fair Labor Standards Act (FLSA) allows for exemptions for a small number of workers including, for example, family members employed in family-owned businesses and employees of businesses with annual sales below the applicable FLSA threshold in each period. The 1996 amendments to the FLSA, which implemented the 1996 and 1997 minimum-wage hikes, also permits employers to pay teenage workers no less than $4.25 per hour during the first 90 calendar days of their employment.[4] Second, some employers may be violating the law, though noncompliance has generally not been considered to be a widespread.[5] Finally, reporting errors involving hourly wage rates or --in the case of salaried workers, weekly earnings and weekly hours-- could also lead to subminimum wages.

Two other features of the wage distributions deserve attention. First, after the minimum-wage increase, the wage distributions for almost all of the demographic groups show "spikes" at $4.75, the new level of the minimum wage. Prior to the increase only 3.0% of teenagers made exactly $4.75 per hour; after the increase, the share at exactly $4.75 more than tripled to 9.7%. Less-than-high-school-educated adults experienced a proportional increase, from 1.0% to 3.0% at $4.75; the effect was particularly strong for black females, where the share earning $4.75 rose from 1.9% to 6.9%. Second, the data suggest strong "spillover" effects, with the share of workers earning $5.15 or more --a level above that mandated by the first stage of the increase-- growing substantially. This phenomenon holds especially among less-than-high-school-educated adults, where, after the first increase, the share of workers earning $4.76-5.14 generally fell

slightly even as the share at $5.15 or more rose.

The existence of spikes and strong spillover effects bears on the debate over modeling the low-wage labor market. Simple competitive labor market models cannot easily explain the existence of spikes and strong spillovers. Standard competitive theory would predict that after the wage hike, the new distribution should be truncated at the new minimum wage (except for that share of the distribution that falls below the minimum wage for one of the reasons mentioned above). At the same time, while the density of workers at each point of the new distribution should rise --because only workers with wages above the new distribution remain-- the increases in density at any point should be uniform and, assuming that the minimum-wage increase did not bite too far up the distribution, relatively small.

III. Employment Effects

The data in the preceding section indicate that while the first stage of the minimum-wage increase had only a limited impact on the overall wage distribution, its effects on teenagers and, to a lesser degree, on less-than-high-school-educated adults were significant. This section uses wage and employment data from the CPS to implement three tests of the employment impact of the October 1, 1996 increase. We have developed the first test elsewhere (Bernstein and Schmitt (1997)) and here extend it to less-than-high-school-educated adults. We borrow the second two tests from separate research conducted by Deere, Murphy, and Welch (1995) and Card (1992a) on the 1990 and 1991 increases in the minimum wage.

A. Difference-in-Differences Estimates

The first test examines changes in employment rates for demographic subgroups of teenagers and less-than-high-school-educated adults (ages 20 to 54) by race or ethnicity and sex. We derive the employment estimates from the full sample of the CPS, rather than the CPS Outgoing Rotation Group, which is also known as the "Quarter Sample," since only one-fourth of the individuals in each month's CPS sample provide information on their earnings. Thus, the use of the full CPS effectively increases the sample size by four and allows a much more disaggregated analysis of employment rates.[6]

Table 2 implements the difference-in-difference test. The example of teens in the first row of panel (b) illustrates the basic logic. The number in column 4 indicates that the employment rate for all teens was 46.6% in the six-month period immediately before the October 1, 1996 increase in the minimum wage. The next column shows that the teen employment rate fell to 40.8% in the six-months immediately after the increase went into effect. At first blush, it appears

that the 11.8% increase in the minimum wage may have led to a 5.8 percentage-point decline in teen employment. The six-month period before the October hike, however, includes the summer months when teen employment is generally high; the six-month period after October includes most of the school year, when many teens leave the labor market. In the first three columns of the table, we therefore examine the employment change over the same period a year earlier.[7] For all teens, we see that the employment rate fell 6.9 percentage points between April-September and October-March in the earlier period. We calculate the "raw" minimum-wage effect, in column 7 as the difference between the employment change in the post-minimum-wage period and the change in the earlier base period.

In column 8, we attempt to control for differences in national economic growth over the four subperiods. We do this by adjusting the raw difference-in-difference estimate for each subgroup to reflect the difference-in-difference in national employment rates for the corresponding adult population. Specifically, we regress the monthly employment rate for each teen group on the monthly employment rate for adults of the same sex from January 1989 through March 1995 (the month immediately before the first of the four subperiods begins). We use this estimated relationship, together with actual changes in the corresponding adult employment rates, to adjust (generally to lower) the estimated employment change attributable to the minimum wage. In the case of teens, for example, the 1.1 percentage-point increase in teen employment after the minimum-wage hike, for example, falls to 0.5 percentage points, reflecting the faster growth in overall employment rates in 1996-97 compared to 1995-96.

Column 8 shows the results of one final adjustment to the difference-in-difference estimates. Since workers in the demographic subgroups are not uniformly distributed throughout

the country, we must also control for differences in the estimated employment rates that are due only to a correlation between the state distribution of members of the demographic subgroup and state differences in economic growth rates. We do this by estimating employment rates, purged of state effects, for each of the subgroups in each of the four subperiods. Specifically, we regress a variable that takes the value one if the individual is employed and zero otherwise on the four time periods and a set of state controls. This provides a "raw" difference-in-difference estimate purged of state differences in employment growth, which we then adjusted for national economic growth as in Column 7.[8]

The final column reports the standard error for the raw difference-in-difference estimate in Column 7. Since the full CPS sample is not independent from month to month (see endnote number 6), we had to adjust the conventionally-calculated standard errors to reflect this nonindependence. The net result of the adjustments was generally to lower the standard errors relative to those calculated using the incorrect, standard definition. We have not attempted to calculate separate standard errors for the growth- and growth-and-state-adjusted difference-in-differences estimates.

Twelve of the 24 estimates are positive or zero; 12 are negative. Two of the positive estimates (for Hispanic teens and Hispanic less-educated adults) are statistically significant at the 10%-level; one (for Hispanic female teens) is significant at the 5% level; none of the negative results is significant at conventional levels.

B. Time Dummy Estimates (Deere, Murphy, and Welch (1996))

The second test of employment effects borrows from Deere, Murphy, and Welch (1995) (hereafter, DMW) , who examined the impact of the 1990 and 1991 increases in the federal

minimum wage on teenagers and adults with less than a high school education. They analyzed data from the CPS ORG for 12-month periods beginning in April of each year from 1985-92. (Both the 1990 and 1991 increases took place on April 1.) DMW regress the natural log of the employment-to-population rate for each of the six groups in each state in each year against the natural log of the employment-to-population rate for males ages 15-64 (as a control for the business cycle), and separate dummy variables for the years 1990, when the federal minimum wage rose to $3.80, and 1991 and 1992, when the federal minimum wage rose again to $4.25. To control for the long-term rise in employment rates of women, DMW also include a trend term in the women's regressions. Given the small sample sizes for blacks in each CPS ORG, they combined data for male and female blacks and included a dummy variable for state-year observations corresponding to men. Appendix Table 1 is a copy of the principal results of their study. They found that both minimum wage hikes reduced employment for male, female, and black teens and adults with less than a high school diploma.

Table 3 applies the DMW test to ORG data for the 11-month periods, beginning in October of each year, from 1991 through 1996. The coefficient on the minimum wage effect variable for male teenagers suggests that the 1996 wage-hike reduced male teen employment by 3.9 percentage points. The result is statistically significant at the 10% level. Female teens experienced a small, statistically insignificant rise in employment. Black teens saw employment drop about 2.3 percentage points, but the result was not statistically significant. The minimum-wage increase apparently led to a 2.9 percentage-point rise in employment among less-educated men and a 8.3 percentage-point increase for less-educated women, with both results statistically significant at the 1% level. Employment also rose 5.6 percentage points mong less-educated

black workers, but the result was not statistically significant.

C. State Variation (Card (1992a) and Card and Krueger (1995))

The final test exploits variation in wage distribution across US states to identify the employment impact of the minimum wage. CPS ORG data show substantial differences across states in the share of teenage workers earning $4.25-4.74 in the period before the 1996 increase -- from less than 10% in New Jersey, Hawaii, Washington, Oregon, and Alaska to half or more in Oklahoma, North Dakota, Louisianna, and West Virginia. If the minimum wage leads to employment losses, we would expect those losses to be greatest in states with the largest share of workers in the affected wage range. Card (1992a), therefore, proposed regressing the change in employment rates for teens in each state before and after the minimum wage increased against the initial share of teenagers in each state with wages in the affected range. Appendix Table 2 is a copy of the main results from Card and Krueger's (1995) implementation of this test on data corresponding to the 1990 and 1991 increases in the minimum wage. Column 4 reports the preferred results (the regression includes a control for overall employment growth). The estimates of changes in employment share are close to zero and not statistically significant across any of the three time periods they studied.

Table 4 presents our results using this third test with CPS ORG data corresponding to the 1996 increase. The regression in panel (b), column 4 presents the results closest in spirit to Card and Krueger's preferred specification. The regression, which includes controls for the changes in the adult employment rate, suggests that the minimum-wage increase lowered the employment of teens. The estimated effect is large –the value translates to an elasticity of about -1.0-- and statistically significant at the 5% level.[9] Figure 1 graphs the change in state teen employment rates

between 1994 and 1996 and the share of teens in each state earning in the affected range in 1994. Table 5 applies the same test to adults ages 20-54 with less than a high school diploma.[10] The specification in panel (b), column 4 suggests that the minimum wage had an economically small and statistically insignificant positive impact on less-than-high-school-educated employment.

IV. Summary and Conclusions

Table 6 displays the employment elasticities implicit in the three preceding tests for the full 1990-91 and 1996 increases. In general, the effects are not statistically well-defined. The first test finds statistically significant results in only three of 24 (not independent) tests -- all cases where the estimated elasticity is positive. The Card and Krueger test finds significant employment changes in only one of three cases (large and negative). The Deere, Murphy, and Welch test yields statistically significant results in nine of 12 cases (seven negative and two positive), but this may be related to this test's reliance on a year indicator-variable as a measure of the minimum-wage effect in the context of a state-panel with several hundred observations.

The first test shows employment elasticities in 1996 that are close to zero for all teens, white teens, and all less-educated adults and white less-educated adults.[11] The same test finds employment elasticities around -0.5 for black teens and less-educated adults, but these results are not statistically well-defined. This procedure also suggests positive and sometimes statistically signficant elasticities, ranging from 0.2 to 1.8 for Hispanic teens and less-educated adults.

The second and third tests give very different results depending on the increase that is being tested. For 1990-91, the Deere, Murphy, and Welch test finds large statistically significant, negative employment effects (around -1.0) for teens and around -0.4 for less-educated adults. In 1996, the same test produces a negative elasticity for male teens and black teens (both -0.7, statistically significant at the 10% level for male teens), but positive elasticities of 0.2 to 1.6 for female teens and less-educated results (statistically significant at the 1% level for male and female less-educated adults). The Card and Krueger test found an elasticity of 0.0 for teens in 1990-91, but suggests a large, statistically significant negative elasticity of -1.0 in 1996. The same test finds

no employment response for less-than-high-school-educated adults in 1996.

Overall, the employment results suggest poorly defined effects that vary substantially from labor market to labor market, from year to year. Most of the estimates are close to zero, suggesting that it may be difficult to find the employment impact because they are small relative to unmodeled factors that affect low-wage employment. The lack of consistent, well-defined negative employment effects, in combination with evidence of spikes in the wage distribution and wage spillovers, argue against a simple competitive model for the labor market, but provide fewer clear-cut clues about exactly what form an alternative model should take.

Endnotes

*We thank David Webster and, especially, Danielle Gao for research assistance. We also thank Dean Baker, Lawrence Mishel, and participants at an Economic Policy Institute seminar for helpful comments.

1. In subsequent versions of this paper, we hope to incorporate two additional tests.

2. Unless otherwise stated, all data refer to employees age 16 and older; they exclude the self-employed.

3. The choice of the 11-month period flows from the 11-month period between the first-stage increase on October 1, 1996 and the second-stage increase on September 1, 1997.

4. We have not seen any analysis of the use of the teen subminimum. The 1989 amendments to the FLSA also established a teen subminimum (which subsequently lapsed). The evidence suggests that employers made little use of the earlier subminimum.

5. Recent anecdotal evidence suggests that noncompliance may disproportionately affect undocumented immigrant workers, who probably make up only a small share of CPS respondents in Table 1.

6. The use of the full CPS also introduces a complication. While the ORG sample is independent from one month to the next, individuals are in the full CPS for four consecutive months (then out for eight months and in again for four -- the Outgoing Rotation Group sample is made up of the "outgoing" group of individuals in their fourth and sixteenth months in the full sample). The standard errors of the differences and differences-in-differences that we will calculate must take this nonindependence into account. For details on the methods used to calculate the standard errors, see Bernstein and Schmitt (1997).

7. The need to compare with an earlier period limits us to a six-month analysis--otherwise, the months analyzed in the later "experimental" period would partially overlap with the months covered in the earlier "control" period. We also experimented with earlier base periods, which produced qualitatively similar results.

8. We have not yet calculated state-adjusted estimates for less-than-high-school-educated adults.

9. The estimated elasticity is the ratio of the coefficients on the share of affected teens in column 4 (the employment change) and column 2 (the wage change).

10. Card and Krueger did not estimate the employment impact on less-than-high-school-educated adults.

11. We are still acquiring the full monthly CPS data necessary to apply these tests to the 1990-91 increases.

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Appendix Table 1

TABLE 4—REGRESSION ESTIMATES OF EMPLOYMENT LOSSES FROM THE 1990–1991 INCREASE IN THE FEDERAL MINIMUM WAGE

Variable	Men	Women	Blacks
Teenagers, Aged 15–19:			
Employment rate, men aged 15–64	3.29 (18.37)	2.27 (8.19)	4.76 (6.41)
Year effects			
Minimum = $3.80	−4.78 (4.97)	−6.63 (3.73)	−7.47 (2.02)
Minimum = $4.25	−7.29 (9.04)	−11.37 (4.80)	−10.00 (3.16)
Test: other year effects, Pr > *F*	0.87	0.32	0.88
High-School Dropouts, Adults Aged 20–54:			
Employment rate, men aged 15–64	1.19 (12.18)	1.66 (7.42)	1.32 (3.50)
Year effects			
Minimum = $3.80	−1.49 (2.93)	−2.54 (1.80)	−4.43 (2.33)
Minimum = $4.25	−3.13 (7.14)	−5.17 (2.72)	−6.66 (4.10)
Test: other year effects, Pr > *F*	0.19	0.924	0.39

Notes: Observations are aggregates by state and year (1985–1992). Years begin April 1. The dependent variable is the natural log of the employment / population ratio for the group indicated. The aggregate employment variable refers to men only and is also a natural logarithm. The minimum-wage variables are indicators for 1990 (when $3.80 applied) and 1991 and 1992 (when $4.25 applied). This specification has one indicator that identifies the 1990 observations and another that identifies the 1991 and 1992 observations without distinguishing between them. The coefficients on the minimum-wage indicators have been multiplied by 100. The rows labeled "Test: other year effects, Pr > *F*" refer to the alternative hypothesis that each of the eight years has an independent effect. The regressions that are restricted to women include a trend term; the regressions for blacks include pooled observations for men and women with an indicator that distinguishes between them. All regressions also include state effects. Numbers in parentheses are *t* statistics.

Source: Reproduced from Deere, Murphy, and Welch (1995).

Appendix Table 2

TABLE 4.4
Estimated Regression Models for Changes in State Averages of Teenage Wages and Teenage Employment–Population Rates

	Models for Change in Mean Log Wage		*Models for Change in Employment Rate*	
	(1)	(2)	(3)	(4)
A. Changes, April–December 1989 to April–December 1990				
1. Fraction of Affected Teenagers[a]	0.15	0.14	0.02	−0.01
	(0.03)	(0.04)	(0.03)	(0.03)
2. Change in Overall Employment Rate[b]	—	0.46	—	1.24
		(0.60)		(0.60)
3. *R*-Squared	0.30	0.31	0.01	0.09
B. Changes, April–December 1989 to April–December 1991				
4. Fraction of Affected Teenagers[a]	0.29	0.24	0.13	0.04
	(0.04)	(0.04)	(0.04)	(0.04)
5. Change in Overall Employment Rate[b]	—	1.03	—	1.69
		(0.41)		(0.44)
6. *R*-Squared	0.57	0.62	0.15	0.35
C. Changes, January–December 1989 to January–December 1992				
7. Fraction of Affected Teenagers[a]	0.28	0.22	0.13	0.01
	(0.04)	(0.05)	(0.03)	(0.03)
8. Change in Overall Employment Rate[b]	—	1.05	—	1.94
		(0.51)		(0.31)
9. *R*-Squared	0.58	0.62	0.31	0.62

Note: Standard errors are shown in parentheses. The models in panels A and B are estimated on 51 state-level observations (including the District of Columbia), using data derived from monthly Current Population Survey (CPS) samples for 1989–1991. The models in panel C are estimated on 50 state-level observations (excluding the District of Columbia), using wage data derived from CPS files for 1989 and 1992 and teenage employment rates taken from U.S. Department of Labor, *Geographic Profiles of Unemployment and Employment*. All models are estimated by weighted least squares, using the number of teenagers in the the state in the 1989 CPS file as a weight.

[a]Fraction of teenagers earning $3.35–3.79 per hour in the state (panel A) or $3.35–4.24 per hour (panels B and C) in 1989. In panels A and B, the fraction affected is estimated using data for April–December only. In panel C, the fraction affected is estimated using data for all 12 months of 1989.

[b]The change in the overall employment–population rate for the state, taken from *Geographic Profiles of Unemployment and Employment*.

Source: Reproduced from Card and Krueger (1995).

APPENDIX TABLE 3

Alternative Regression Models for the Change in State Teen Employment Rates, 1994-96

	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Working Teens Earning $4.25-4.74 in 1995	-0.130** (0.047)	-0.124* (0.054)	-0.138* (0.056)	-0.125** (0.047)	-0.112* (0.045)	-0.136* (0.050)
Change in Adult Employment Rate	0.953** (0.342)	0.849* (0.378)	--	0.896* (0.355)	1.061** (0.356)	2.000 -.-
Change in Log Adult Wage	0.190 (0.208)	--	--	--	--	--
Adult Employment Level						
1996	--	--	0.877 (0.529)	--	--	--
1995	--	--	0.374 (0.728)	--	--	--
1994	--	--	-0.989 (0.667)	--	--	--
1993	--	--	-0.143 (0.545)	--	--	--
1992	--	--	-0.193 (0.413)	--	--	--
Teenage Employment Level						
1994	--	--	--	-0.038 (0.060)	-0.386* (0.156)	--
1993	--	--	--	--	0.468* (0.217)	--
1992	--	--	--	--	0.102 (0.145)	--
Region Dummies	No	Yes	No	No	No	No
R-squared	0.233	0.198	0.130	0.177	0.316	0.132

Notes: See text Table 4.

APPENDIX TABLE 4

Alternative Regression Models for the Change in State LTHS Adult Employment Rates, 1994-96

	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Working LTHS Earning $4.25-4.74 in 1995	0.005 (0.084)	-0.097 (0.094)	0.012 (0.092)	-0.007 (0.085)	-0.007 (0.083)	-0.007 (0.097)
Change in Adult Employment Rate	1.064** (0.232)	1.273** (0.228)	--	1.229** (0.259)	1.097** (0.264)	2.000 -.-
Change in Log Adult Wage	-0.234 (0.145)	--	--	--	--	--
Adult Employment Level						
1996	--	--	0.870* (0.342)	--	--	--
1995	--	--	0.866 (0.476)	--	--	--
1994	--	--	-1.448** (0.449)	--	--	--
1993	--	--	0.076 (0.388)	--	--	--
1992	--	--	-0.336 (0.271)	--	--	--
LTHS Employment Level						
1994	--	--	--	0.100 (0.068)	-0.221 (0.166)	--
1993	--	--	--	--	0.275 (0.191)	--
1992	--	--	--	--	0.084 (0.168)	--
Region Dummies	No	Yes	No	No	No	No
R-squared	0.337	0.515	0.404	0.331	0.391	0.000

Notes: See text Table 5.

TABLE 1A
Distribution of Wages by Demographic Characteristics,
October 1995 -- August 1996

	Share of Employees (%) Earning						
	$0.50 -- $4.24	$4.25	$4.26 -- $4.74	$4.75	$4.76 -- $5.14	$5.15	Number in Sample
(a) All	2.1	1.5	2.1	0.5	4.7	89.1	138,842
(b) Teens	7.3	10.2	12.2	3.0	20.2	47.1	7,902
Male	6.4	9.4	12.3	2.4	19.3	50.2	3,913
Female	8.2	11.0	12.2	3.6	21.0	44.0	3,989
White	7.2	9.3	12.0	3.1	20.4	48.0	6,350
Male	6.3	8.4	11.9	2.7	19.4	51.2	3,129
Female	8.1	10.1	12.0	3.5	21.3	45.0	3,221
Black	9.4	15.2	17.5	3.8	20.1	34.0	627
Male	9.5	16.6	19.8	1.1	17.7	35.3	283
Female	9.3	14.0	15.7	6.1	22.1	32.8	344
Hispanic	5.6	14.6	12.4	1.9	19.7	45.9	639
Male	4.6	11.3	11.6	0.8	20.8	50.9	371
Female	7.1	19.0	13.4	3.4	18.3	38.8	268
(c) LTHS Adults	4.6	2.9	3.8	1.0	9.7	77.9	10,313
Male	3.4	1.9	2.9	0.6	7.4	83.7	6,264
Female	6.4	4.3	5.4	1.7	13.3	68.9	4,049
White	3.9	1.6	3.3	0.8	7.9	82.6	5,198
Male	2.7	1.0	2.1	0.3	5.3	88.7	3,145
Female	5.7	2.5	5.1	1.5	12.0	73.2	2,053
Black	3.7	3.5	5.6	2.1	11.7	73.3	1,187
Male	3.0	2.3	4.1	2.3	8.8	79.5	604
Female	4.5	4.8	7.2	1.9	14.8	66.9	583
Hispanic	5.8	4.9	4.1	1.2	11.8	72.2	3,373
Male	4.7	3.4	3.7	0.7	10.2	77.3	2,232
Female	7.9	8.0	4.8	2.3	14.9	62.1	1,141

Source: Authors' analysis of CPS ORG data.

TABLE 1B
Distribution of Wages by Demographic Characteristics
October 1996 -- August 1997

	Share of Employees (%) Earning						
	$0.50 -- $4.24	$4.25	$4.26 -- $4.74	$4.75	$4.76 -- $5.14	$5.15	Number in Sample
(a) All	1.9	0.4	1.1	1.6	4.4	90.6	140,699
(b) Teens	5.1	2.6	4.5	9.7	22.5	55.7	7,993
Male	4.2	2.8	4.0	9.2	22.0	57.8	3,939
Female	5.9	2.4	5.0	10.1	22.9	53.7	4,054
White	4.8	2.4	4.6	9.8	21.9	56.6	6,379
Male	3.9	2.4	4.3	9.4	21.2	58.8	3,144
Female	5.7	2.3	4.9	10.1	22.5	54.5	3,235
Black	7.1	4.7	7.3	9.1	27.1	44.6	634
Male	6.4	5.7	5.3	7.8	29.4	45.4	282
Female	7.7	4.0	8.8	10.2	25.3	44.0	352
Hispanic	6.4	2.8	2.1	11.0	24.7	53.0	675
Male	5.9	3.2	1.3	9.9	23.1	56.6	373
Female	7.0	2.3	3.0	12.3	26.8	48.7	302
(c) LTHS Adults	3.6	0.6	2.2	3.0	9.0	81.6	10,509
Male	2.7	0.3	1.7	1.8	6.9	86.5	6,403
Female	5.0	1.1	2.9	4.8	12.3	73.8	4,106
White	3.3	0.4	1.8	1.8	6.6	86.1	5,075
Male	2.0	0.1	1.3	0.6	4.1	91.9	3,079
Female	5.1	0.8	2.5	3.7	10.6	77.3	1,996
Black	4.1	1.1	3.0	4.5	11.4	75.9	1,256
Male	3.8	0.5	2.0	2.0	10.1	81.6	635
Female	4.5	1.8	4.0	6.9	12.7	70.0	621
Hispanic	3.8	0.9	2.5	4.1	11.2	77.5	3,630
Male	3.1	0.7	2.1	3.4	9.4	81.4	2,413
Female	5.2	1.4	3.2	5.7	14.9	69.7	1,217

Source: Authors' analysis of CPS ORG data.

TABLE 1C

Change in Distribution of Wages by Demographic Characteristics, October 1995 -- August 1996 to October 1996 -- August 1997

	Share of Employees (%) Earning					
	$0.50 -- $4.24	$4.25	$4.26 -- $4.74	$4.75	$4.76 -- $5.14	$5.15
(a) All	-0.2	-1.1	-1.0	1.1	-0.3	1.5
(b) Teens	-2.2	-7.6	-7.7	6.7	2.3	8.6
Male	-2.2	-6.6	-8.3	6.8	2.7	7.6
Female	-2.3	-8.6	-7.2	6.5	1.9	9.7
White	-2.4	-6.9	-7.4	6.7	1.5	8.6
Male	-2.4	-6.0	-7.6	6.7	1.8	7.6
Female	-2.4	-7.8	-7.1	6.6	1.2	9.5
Black	-2.3	-10.5	-10.2	5.3	7.0	10.6
Male	-3.1	-10.9	-14.5	6.7	11.7	10.1
Female	-1.6	-10.0	-6.9	4.1	3.2	11.2
Hispanic	0.8	-11.8	-10.3	9.1	5.0	7.1
Male	1.3	-8.1	-10.3	9.1	2.3	5.7
Female	-0.1	-16.7	-10.4	8.9	8.5	9.9
(c) LTHS Adults	-1.0	-2.3	-1.6	2.0	-0.7	3.7
Male	-0.7	-1.6	-1.2	1.2	-0.5	2.8
Female	-1.4	-3.2	-2.5	3.1	-1.0	4.9
White	-0.6	-1.2	-1.5	1.0	-1.3	3.5
Male	-0.7	-0.9	-0.8	0.3	-1.2	3.2
Female	-0.6	-1.7	-2.6	2.2	-1.4	4.1
Black	0.4	-2.4	-2.6	2.4	-0.3	2.6
Male	0.8	-1.8	-2.1	-0.3	1.3	2.1
Female	0.0	-3.0	-3.2	5.0	-2.1	3.1
Hispanic	-2.0	-4.0	-1.6	2.9	-0.6	5.3
Male	-1.6	-2.7	-1.6	2.7	-0.8	4.1
Female	-2.7	-6.6	-1.6	3.4	0.0	7.6

Source: Authors' analysis of CPS ORG data.

TABLE 2
Estimated Change in Employment Rates (%) Associated
With the Minimum Wage Increase in October 1996
Difference-in-Difference Estimates

	1995-96			1996-97			Difference-in-Difference			
	Apr-Sep (1)	Oct-Mar (2)	Difference (3)	Apr-Sep (4)	Oct-Mar (5)	Difference (6)	Raw (7)	Growth-Adjusted (8)	Growth- & State-Adjusted (9)	Standard Error (10)
(a) All	63.2	62.6	-0.6	63.6	63.2	-0.4	0.2	n.a.	n.a.	0.2
(b) Teens	47.6	40.7	-6.9	46.6	40.8	-5.8	1.1	0.5	0.4	0.7
Male	48.8	40.3	-8.5	47.5	39.8	-7.7	0.8	0.4	0.4	1.0
Female	46.3	41.0	-5.3	45.7	41.7	-4.0	1.3	0.8	0.6	1.0
White	54.8	47.0	-7.8	54.3	47.1	-7.2	0.6	0.1	0.0	0.9
Male	55.8	46.1	-9.7	54.5	45.7	-8.8	0.9	0.5	0.4	1.2
Female	53.7	47.9	-5.8	54.1	48.6	-5.5	0.3	0.0	0.0	1.3
Black	27.6	25.0	-2.6	28.6	24.4	-4.2	-1.6	-2.1	-1.8	1.8
Male	27.3	22.9	-4.4	28.3	21.3	-7.0	-2.6	-2.9	-2.3	2.6
Female	27.9	27.1	-0.8	28.9	27.4	-1.5	-0.7	-1.4	-1.4	2.5
Hispanic#	37.3	31.4	-5.9	34.1	32.5	-1.6	4.3	4.1	3.8	2.1
Male	40.8	36.6	-4.2	39.9	36.1	-3.8	0.4	0.5	0.7	3.1
Female*	33.6	26.0	-7.6	28.0	28.6	0.6	8.2	7.0	6.5	2.9
(c) LTHS Adults	57.7	57.6	-0.1	59.3	59.6	0.3	0.4	-0.2	--	0.7
Male	71.9	71.2	-0.7	74.2	73.3	-0.9	-0.2	-0.5	--	0.9
Female	43.0	43.6	0.6	43.6	44.9	1.3	0.7	0.1	--	1.0
White*	60.6	60.4	-0.2	62.6	61.6	-1.0	-0.8	-1.1	--	1.0
Male*	71.5	71.3	-0.2	74.6	73.2	-1.4	-1.2	-1.6	--	1.2
Female	48.5	48.5	0.0	48.8	48.8	0.0	0.0	-0.1	--	1.4
Black#	44.3	44.6	0.3	47.1	45.3	-1.8	-2.1	-2.8	--	1.9
Male	53.2	52.6	-0.6	56.2	52.6	-3.6	-3.0	-3.4	--	2.9
Female	37.0	37.9	0.9	38.9	38.5	-0.4	-1.3	-2.3	--	2.4
Hispanic	60.7	60.3	-0.4	61.3	64.0	2.7	3.1	2.5	--	1.3
Male	81.1	79.2	-1.9	82.0	82.5	0.5	2.4	2.1	--	1.5
Female	38.8	39.7	0.9	38.9	42.8	3.9	3.0	2.2	--	1.8

Note: Rows marked with * are significant at the 5% level; #, at the 10% level.

Source: Authors' analysis of full monthly CPS.

TABLE 3
Regression Estimates of Employment Changes from the 1996 Increase in the Federal Minimum Wage
CPS ORG, 1991-97

	Men	Women	Blacks
(a) Teenagers 15-19			
Employment Rate of Men 15-64	3.842**	1.552**	3.179*
	(0.414)	(0.450)	(1.420)
Year Effect, Minimum Wage = $4.75	-3.914#	0.805	-2.349
	(2.000)	(3.381)	(6.933)
Root MSE	0.113	0.123	0.462
Sample Size	306	306	424
(b) Less than High School Adults 20-54			
Employment Rate of Men 15-64	1.302**	1.241**	1.725*
	(0.191)	(0.320)	(0.713)
Year Effect Minimum Wage = $4.75	2.913**	8.251**	5.574
	(0.878)	(2.327)	(3.624)
Root MSE	0.050	0.083	0.244
Sample Size	306	306	438

Source: CPS Outgoing Rotation Group, October-August, 1991-97.

Notes:
(1) Regressions weighted by number of individuals in sample in state and year corresponding to each observation.
(2) Standard errors in parentheses. #: statistically significant, 10% level; *: statistically significant, 5% level; **, statistically significant, 1% level.
(3) Coefficient on Year Effect when Minimum Wage=$4.75 is multiplied by 100.
(4) Regressions for women include an annual time trend. Regressions for blacks use pooled data for men and women with an indicator variable for men.
(5) All regressions include controls for the pre-1994 version of the CPS survey.

TABLE 4

Determinants of Changes in State Averages of Teenage Wage and Employment Rates

	Change in Mean Log Wage		Change in Employment Rate	
	(1)	(2)	(3)	(4)
(a) 1995-96				
Fraction of Working Teens Earning $4.25-4.74 in 1995	0.109*	0.121*	-0.143**	-0.150**
	(0.054)	(0.054)	(0.045)	(0.045)
Change in Adult Employment Rate	--	-0.611	--	0.358
		(0.463)		(0.387)
R-squared	0.077	0.109	0.172	0.187
(b) 1994-96				
Fraction of Working Teens Earning $4.25-4.74 in 1994	0.121**	0.129**	-0.111*	-0.123*
	(0.042)	(0.041)	(0.049)	(0.046)
Change in Adult Employment Rate	--	-0.595#	--	0.951
		(0.301)		(0.342)
R-squared	0.147	0.211	0.094	0.220
(c) 1994-95				
Fraction of Working Teens Earning $4.25-4.74 in 1994	0.025	0.027	-0.008	-0.006
	(0.052)	(0.053)	(0.048)	(0.049)
Change in Adult Employment Rate	--	-0.218	--	-0.247
		(0.629)		(0.581)
R-squared	0.005	0.007	0.001	0.004

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. Standard errors are in parentheses. ** indicates signicant at the 1%-level; *, 5%-level; #, 10%-level.

(2) Coefficients from a weighted least squares regression, using the number of teenagers in the state in the earlier year's CPS ORG sample.

TABLE 5

Determinants of Changes in State Averages of Less Than High School Adults Wage and Employment Rates

	Change in Mean Log Wage		Change in Employment Rate	
	(1)	(2)	(3)	(4)
(a) 1995-96				
Fraction of Working LTHS Earning $4.25-4.74 in 1995	0.429**	0.398*	0.205#	0.155
	(0.152)	(0.152)	(0.104)	(0.094)
Change in Adult Employment Rate	--	0.618	--	0.980
		(0.442)		(0.275)
R-squared	0.140	0.174	0.073	0.268
(b) 1994-96				
Fraction of Working LTHS Earning $4.25-4.74 in 1994	0.420**	0.421**	0.034	0.012
	(0.141)	(0.143)	(0.100)	(0.085)
Change in Adult Employment Rate	--	-0.037	--	1.066**
		(0.397)		(0.236)
R-squared	0.153	0.153	0.002	0.300
(c) 1994-95				
Fraction of Working LTHS Earning $4.25-4.74 in 1994	0.189	0.187	-0.042	-0.049
	(0.147)	(0.148)	(0.084)	(0.075)
Change in Adult Employment Rate	--	0.355	--	1.232**
		(0.642)		(0.323)
R-squared	0.033	0.039	0.005	0.237

Notes:

(1) The dependent variable is the change in the less-than-high-school-educated, adult employment-to-population rate in each of the 50 states and the District of Columbia. Standard errors are in parentheses. ** indicates signicant at the 1%-level; *, 5%-level; #, 10%-level.

(2) Coefficients from a weighted least squares regression, using the number of less-than-high-school-educated adults in the state in the earlier year's CPS ORG sample.

TABLE 6

Summary of Estimated Employment Elasticities of Minimum Wage Increases

	Teens		LTHS Adults	
	1990/91	1996	1990/91	1996
(a) Bernstein and Schmitt				
All	--	0.1	--	-0.0
Male	--	0.1	--	-0.1
Female	--	0.1	--	0.0
White	--	0.0	--	-0.2
Male	--	0.1	--	-0.2
Female	--	0.0	--	-0.0
Black	--	-0.6	--	-0.5
Male	--	-0.8	--	-0.5
Female	--	-0.4	--	-0.5
Hispanic	--	0.9#	--	0.4#
Male	--	0.2	--	0.2
Female	--	1.8*	--	0.5
(b) Deere, Murphy, and Welch				
Male	-0.6**	-0.7#	-0.2**	0.3**
Female	-1.0**	0.2	-0.4**	1.6**
Black	-1.4**	-0.7	-0.6**	1.1
(c) Card and Krueger				
All	0.0	-1.0*	--	0.0

Notes:

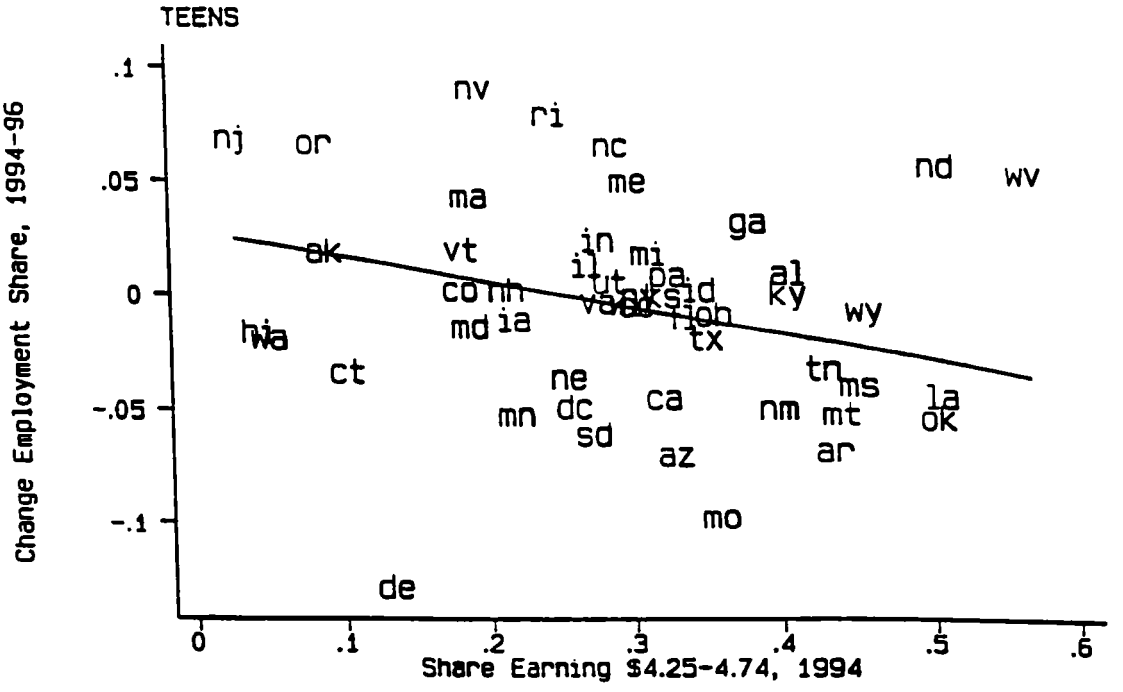
(1) 1990/91 estimates based on full, two stage increase from $3.35 to $4.25. 1996 estimates based on increase from $4.25 to $4.75.

(2) Estimates in panel (a) use growth- and state-adjusted employment changes.

(3) Estimates in panels (a) and (b) are employment elasticities with respect to an increase in the minimum wage, evaluated at the average employment rate; estimates in panel (c) are estimated elasticities with respect to wage changes induced by the minimum wage increase.

(4) Estimates with **, indicate based on underlying coefficient significant at the 10%, level; *, 5% level; **, 1% level.

Figure 1


TEENS
Change Employment Share, 1994-96
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Share Earning $4.25-4.74, 1994

Job Loss in a Booming Economy

The Employment Policies Institute
JANUARY 1998

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

Type of Employees	Percent of Employees Earning						Number in Sample
	$0.50 – $4.24	$4.25	$4.26 – $4.74	$4.75	$4.76 – $5.14	$5.15 +	
All	2.1	1.5	2.1	0.5	4.7	89.1	75,732
Teenagers (16-19)	7.2	9.8	10.2	2.9	20.5	49.4	4,482
Male	6.2	9.1	10.3	2.5	19.9	52.0	2,270
Female	8.3	10.5	10.2	3.3	21.1	46.6	2,212
White	7.1	9.1	9.6	2.8	20.3	51.1	3,608
Male	6.2	8.0	9.6	2.6	19.4	54.2	1,817
Female	8.1	10.2	9.7	3.0	21.2	47.9	1,791
Black	8.9	14.2	17.0	3.9	20.1	35.9	359
Male	7.1	18.3	21.3	1.8	17.2	34.3	169
Female	10.5	10.5	13.2	5.8	22.6	37.4	190
Hispanic	5.5	12.8	12.0	2.5	23.5	43.7	366
Male	5.0	10.9	9.5	1.8	25.0	47.7	220
Female	6.2	15.8	15.8	3.4	21.2	37.7	146
Young Adults (20-24)	3.1	3.0	3.7	1.1	9.1	80.1	7,368
Male	2.4	2.7	3.2	0.7	8.3	82.5	3,705
Female	3.7	3.2	4.1	1.4	9.9	77.7	3,663
White	2.8	2.3	3.2	1.0	8.3	82.4	5,470
Male	1.8	1.9	2.7	0.7	7.5	85.5	2,732
Female	3.8	2.8	3.7	1.4	9.1	79.4	2,738
Black	3.4	5.2	6.4	2.3	13.5	69.2	653
Male	3.8	6.9	4.8	2.8	13.5	68.2	289
Female	3.0	3.8	7.7	1.9	13.5	70.1	364
Hispanic	4.6	6.1	4.4	0.7	12.1	72.1	856
Male	4.8	5.4	5.2	0.2	10.9	73.4	497
Female	4.2	7.0	3.3	1.4	13.9	70.2	359

Source: EPI analysis of CPS ORG.

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

Type of Employees	Percent of Employees Earning						Number in Sample
	$0.50 – $4.24	$4.25	$4.26 – $4.74	$4.75	$4.76 – $5.14	$5.15 +	
All	1.9	0.4	1.1	1.6	4.4	90.6	76,745
Teenagers (16-19)	5.2	2.7	5.5	11.5	23.6	51.5	4,094
Male	4.2	2.4	5.1	11.1	23.7	53.4	2,034
Female	6.1	3.0	6.0	11.8	23.5	49.7	2,060
White	5.0	2.4	5.7	11.1	23.6	52.2	3,267
Male	4.2	2.0	5.5	10.6	23.6	54.2	1,637
Female	5.8	2.9	5.9	11.7	23.6	50.1	1,630
Black	6.8	5.4	8.9	11.9	27.1	39.9	336
Male	5.1	5.8	6.6	10.9	29.9	41.6	137
Female	8.0	5.0	10.6	12.6	25.1	38.7	199
Hispanic	5.5	2.9	2.3	14.8	24.3	50.1	345
Male	3.6	3.6	1.5	15.2	23.9	52.3	197
Female	8.1	2.0	3.4	14.2	25.0	47.3	148
Young Adults (20-24)	2.6	1.0	2.0	3.2	8.8	82.4	7,285
Male	2.1	1.1	1.9	2.6	7.5	84.8	3,694
Female	3.1	0.9	2.1	3.7	10.2	79.9	3,591
White	2.5	0.9	1.6	2.7	8.0	84.3	5,318
Male	2.1	0.9	1.5	1.9	6.6	86.9	2,672
Female	3.0	0.9	1.7	3.5	9.4	81.6	2,646
Black	4.4	1.9	3.6	3.3	11.7	75.1	676
Male	4.2	2.8	2.8	2.4	11.5	76.2	286
Female	4.6	1.3	4.1	3.8	11.8	74.4	390
Hispanic	2.5	1.3	2.8	6.5	11.8	75.1	906
Male	1.9	1.5	3.0	6.8	10.1	76.7	533
Female	3.5	1.1	2.4	6.2	14.2	72.7	373

Source: EPI analysis of CPS ORG.

TABLE 1C
Change of Distribution of Wages by Demographic Characteristics, April-September 1996 to October 1996-March 1997

	Change in 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 +
All	-0.2	-1.1	-1.0	1.1	-0.3	1.5
Teenagers (16-19)	-2.0	-7.1	-4.7	8.6	3.1	2.1
Male	-2.0	-6.7	-5.2	8.6	3.8	1.4
Female	-2.2	-7.5	-4.2	8.5	2.4	3.1
White	-2.1	-6.7	-3.9	8.3	3.3	1.1
Male	-2.0	-6.0	-4.1	8.0	4.2	0.0
Female	-2.3	-7.3	-3.8	8.7	2.4	2.2
Black	-2.1	-8.8	-8.1	8.0	7.0	4.0
Male	-2.0	-12.5	-14.7	9.1	12.7	7.3
Female	-2.5	-5.5	-2.6	6.8	2.5	1.3
Hispanic	0.0	-9.9	-9.7	12.3	0.8	6.4
Male	-1.4	-7.3	-8.0	13.4	-1.1	4.6
Female	1.9	-13.8	-12.4	10.8	3.8	9.6
Young Adults (20-24)	-0.5	-2.0	-1.7	2.1	-0.3	2.3
Male	-0.3	-1.6	-1.3	1.9	-0.8	2.3
Female	-0.6	-2.3	-2.0	2.3	0.3	2.2
White	-0.3	-1.4	-1.6	1.7	-0.3	1.9
Male	0.3	-1.0	-1.2	1.2	-0.9	1.4
Female	-0.8	-1.9	-2.0	2.1	0.3	2.2
Black	1.0	-3.3	-2.8	1.0	-1.8	5.9
Male	0.4	-4.1	-2.0	-0.4	-2.0	8.0
Female	1.6	-2.5	-3.6	1.9	-1.7	4.3
Hispanic	-2.1	-4.8	-1.6	5.8	-0.3	3.0
Male	-2.9	-3.9	-2.2	6.6	-0.8	3.3
Female	-0.7	-5.9	-0.9	4.8	0.3	2.5

Source: EPI analysis of CPS ORG.

Who Benefited From the Increase?

While it appears to be true that the minimum-wage increase raised the pay of low-wage workers, is it also true that these higher wages went to low-income families? **Figure 1** illustrates how the policy predominantly benefited low-income working families. (See the appendix for a discussion of the methods used in this figure.) In the case of all working families, including those with only one member, 55.6% of the gains from the increase went to families in the bottom 40%, and 71.2% of the gains went to families in the bottom 60%. Note that families in the bottom two-fifths received only 16.0% of all income, while families in the bottom three-fifths received 32.2% of all income. Families in the top fifth, with 45.0% of all income, received the smallest share of gains from the increase (13.5%). The patterns are similar for families of two or more individuals (bottom figure). In the end, most of the added earnings generated by the minimum-wage increase went to the low-income families who needed it most.

Did the Increase Cause Job Loss?

The potential for job loss is the most controversial aspect of the minimum-wage increase. Opponents of the increase have consistently contended that it lowers employment opportunities for teenagers (and to a lesser extent, young adults). Supporters of the increase have disagreed, citing much empirical research showing little or no "disemployment" effects.[6]

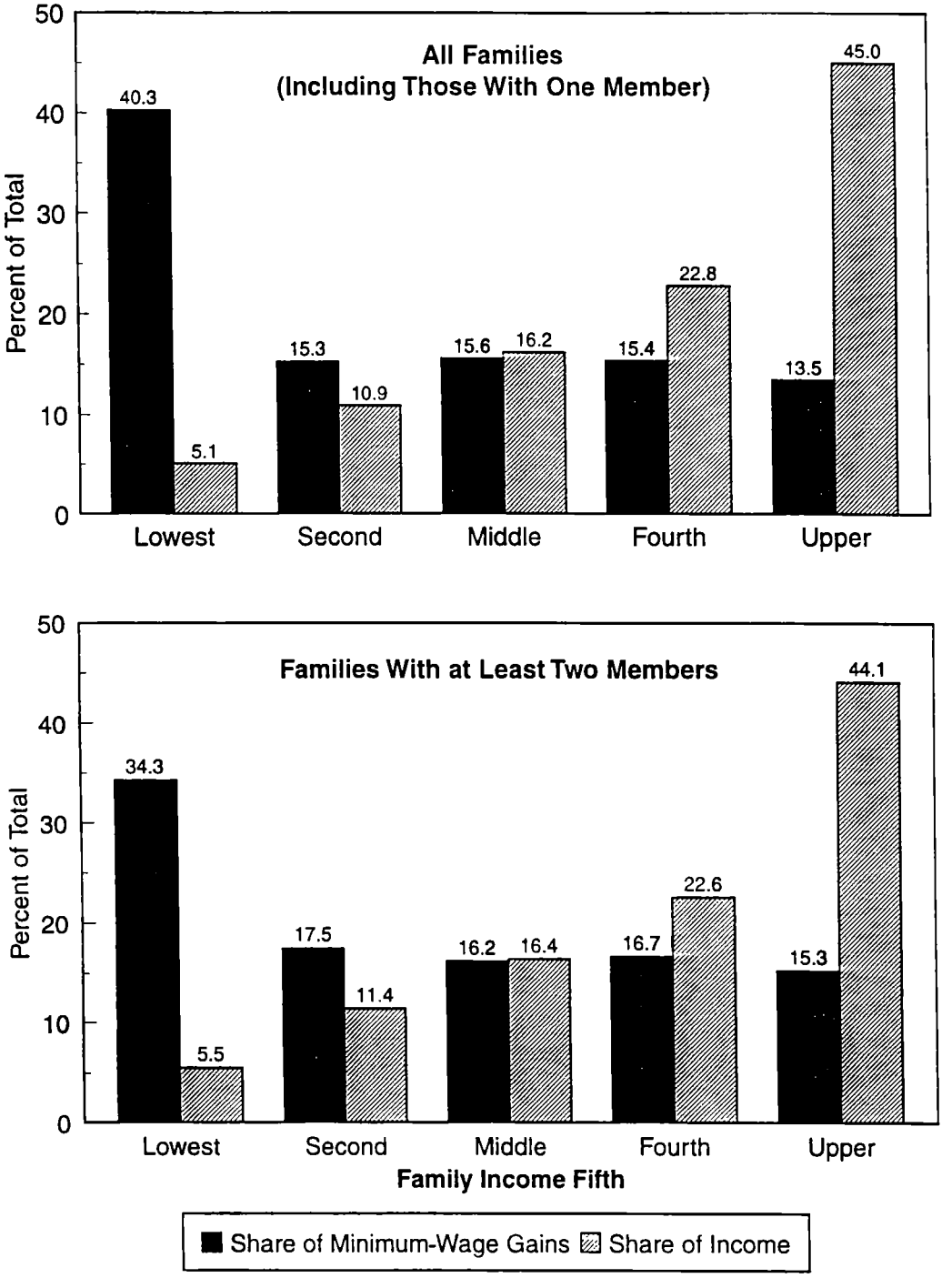
To determine if the minimum-wage increase led to declining job opportunities, we examined the trends in the employment rates (employment to population ratios) of teenage and young adult workers. Our examination takes into account three factors that can have an impact on the results: seasonal variations, economic growth, and regional trends. We find that *the minimum-wage increase had no significant effect on the employment of teens or young adults, one way or the other.*

Table 2 summarizes our analysis of the minimum-wage increase's impact on employment. Column 4 indicates the percentage of persons in each age group employed during the six-month period immediately before the October 1 minimum-wage increase. Among teenagers, for example, the employment rate was 46.6% in April-September 1996. Column 5 lists the percentage employed 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 six-month period fell to 40.8%, a drop of 5.8 percentage points when compared to the preceding six months. But this analysis would fail to take into consideration the seasonal variations inherent in teenage employment trends, especially the temporary rise in teenage employment associated with summer vacation.

To control for the seasonal employment effect, we compare the change in employment between April-September 1996 and October 1996-March 1997 with the change between the same two periods a year earlier — when the minimum wage did not change. Columns 1 and 2, which show the employment rates for these earlier periods, indicate that, among all teenagers, the employment rate fell from 47.6% in the April to September period to 40.7% in the October to March period — 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.

FIGURE 1

Distribution of Minimum-Wage Gains and Income Shares by Fifth, 1996



Note: Families have at least one earner.

Source: EPI analysis of March CPS data (see data appendix).

TABLE 2
Estimated Change in Employment Associated With the Minimum-Wage Increase in October 1996, All, Teenagers, and Young Adults

	1995-96			1996-97			Difference-in-Difference		
	Apr-Sep (1)	Oct-Mar (2)	Difference (3)	Apr-Sep (4)	Oct-Mar (5)	Difference (6)	Raw (7)	Growth-Adjusted (8)	State & Growth Adjusted (9)
All	63.2	62.6	-0.7	63.6	63.2	-0.3	0.3	n.a.	n.a.
Teenagers (16-19)	47.6	40.7	-6.9	46.6	40.8	-5.8	1.0	0.5	0.4
Young Adults (20-24)	70.6	68.1	-2.5	70.9	69.4	-1.5	0.9	0.6	0.7

Note: This method of estimating the employment effect of the minimum-wage increase is explained in the text. None of the estimated employment changes in columns 7, 8, and 9 is statistically significant at standard levels.

Source: Economic Policy Institute analysis of CPS.

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. We observe a similar trend for young adults.

We must also account for the impact of economic growth, both nationally and within regions. The data in columns 8 and 9 adjust for these two factors.[7] The "growth-adjusted" estimates in column 8 are less positive than the "raw" estimates derived above, indicating that economic growth was more rapid in the year that the minimum wage increased than it had been in the preceding year. 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. However, as with the employment gains found above, none of these results are statistically significant.

Table 3, which focuses only on the differences between the year-to-year periods (the complete table is in the appendix), builds on this analysis by examining the employment effects of the wage increase on groups organized by sex and race. White male teenagers saw employment rates increase after the minimum wage went into effect, even after controlling for overall economic growth and state differences in

TABLE 3
Differences-in-Differences in Employment Rates, All Groups

	Difference-in-Difference		
	Raw	Growth Adjusted	State & Growth Adjusted
All	0.3	n.a.	n.a.
Teenagers (16-19)	1.0	0.5	0.4
Male	0.7	0.4	0.4
Female	1.4	0.8	0.6
White	0.6	0.1	0.0
Male	0.8	0.5	0.4
Female	0.3	-0.0	-0.0
Black	-1.6	-2.1	-1.8
Male	-2.6	-2.9	-2.3
Female	-0.6	-1.4	-1.4
Hispanic	4.3	4.1	3.8 #
Male	0.5	0.7	0.7
Female	8.1	7.0	6.5 *
Young Adults (20-24)	0.9	0.6	0.7
Male	1.2	0.9	1.1
Female	0.6	0.5	0.7
White	0.5	0.2	0.4
Male	1.4	1.1	1.2
Female	-0.5	-0.5	-0.3
Black	-2.4	-2.7	-2.5
Male	-5.5	-5.8	-5.6 #
Female	0.2	0.4	0.5
Hispanic	5.6	5.0	5.1 *
Male	4.4	3.9	4.0 #
Female	6.1	6.4	6.4 *

#: statistically significant at the 0.10 level

*: statistically significant at the 0.05 level

Note: This method of estimating the employment effect of the minimum-wage increase is explained in the text.

Source: Economic Policy Institute analysis of CPS.

employment growth. This increase, however, was not statistically significant.[8] Among black teenagers, employment rates appeared to fall, but these declines were also not statistically significant. Hispanic teenagers were the only group that experienced a statistically significant employment increase, even after adjusting for overall economic growth and state effects. This employment increase suggests that raising the minimum wage may have been responsible for this group's increased employment.[9] Hispanic females experienced the largest employment jump.

Changes in the employment rates for male and female young adults and for white young adults were also positive but statistically insignificant. Employment rates for young adult black males were negative, but statistically significant only at a marginal (0.10) level. Shifts in the employment rates of Hispanic young adults were positive and significant (at the 0.05 level). As with teenagers, this result for Hispanics appears largely to reflect rising employment rates among young women.

We advise against drawing strong conclusions from the results for minority subgroups since the employment data for these subgroups are volatile and produce substantial variations in both the size of the estimated employment effects and their statistical significance. For example, estimates of the size of the minimum wage's employment effect on black men, age 20-24, vary by a factor of two over the six-month period and are not consistently statistically significant.[10] The high variance for this particular subgroup, and other minority subgroups, reflects both an underlying employment volatility and the relatively small sample sizes.

The findings in this report raise a number of questions:

(1) Why use employment instead of unemployment rates?

Employment rates are typically used in the analysis of minimum-wage increases because they directly address the question of whether the increase has led to job loss. Unemployment rates are less reliable in this regard because they can rise due to an increase of job seekers entering the labor force. An increase in the minimum wage, for example, can draw additional workers into the labor force who are initially unemployed as they begin their job search. If this inflow causes the labor force to grow more quickly than job opportunities available to absorb the new workers, unemployment rates will rise faster than employment rates, even though no jobs have been lost.

(2) Is six months long enough for the disemployment effects to materialize?

Job turnover rates in the U.S. economy are high, especially for low-wage workers. Six months is a reasonable amount of time to observe at least *some* impact. Those who argue otherwise should specify a time period after which their claims can be put to an empirical test.

(3) Isn't it likely that the disemployment effects will surface only after the full increase is in place?

The opposition based its arguments on the traditional textbook model which maintains that increasing the cost of low-wage labor will lead to disemployment. The staggered implementation of the increases does not alter this prediction. As this study shows, significant proportions of young workers' wages were raised by the first increase, and, according to the textbook theory of

disemployment, many of these workers should have lost their jobs. As with past increases in the minimum wage, this prediction proved incorrect.

Conclusion

This examination of the increase in the minimum wage shows that, so far, the policy has had the desired impact: it has raised the earnings of low-wage workers, lifted the incomes of their families, and, despite opponents' claims, has not led to job loss. This last finding is particularly important for future debates over raising the minimum wage. Given the statistically and economically insignificant (and mostly positive) employment effects of the change, it might be more useful if the next debate spends less time focusing on the cost of the increase and more time on the benefits to low-income families.

July 1997

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DATA APPENDIX

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, below). Our sample includes all employees 16 years of age or older who earned between $0.50 and $100.00 per hour (in 1989 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.

Figure 1: The Distribution of Family Income and Wage Gains From the Increase

These data are derived from the March 1996 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, as described above, 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 1995.

For workers whose earnings were in the affected range ($4.25-$4.74) in March 1996, we first calculated their hourly increase as a result of setting the minimum at $4.75. We then multiplied this value by their annual hours worked in 1995. This product represents their annual gains from the increase (note the assumption of no hours reduction). We then divided families in quintiles based on their annual income, with an equal number of persons (not families) in each quintile. Finally, we calculated the share of income and wage gains that accrued to each quintile.

Table 2: Employment Rates

The data for this table come from the basic monthly files of the 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 2 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 were 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 American, 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, and the extent of this error in Tables 2 and 3 is represented by asterisks (and given in column 9 of Appendix Table 2).

Given the nonrandom sampling framework of the CPS, the calculation of standard errors is not straightforward.[11] 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 in Table 2.

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 0.619.

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 in Table 2. 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 years old and over (by sex), 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+ 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 2. 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."

APPENDIX TABLE 1
Distribution of Wages by Demographic Characteristics, Differences-in-Differences, 1995/96 to 1996/97

	Change in 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 +
All	0.08	-0.4	-0.2	0.6	-0.1	0.0
Teenagers (16-19)	-1.9	-6.4	-4.1	8.4	3.6	0.4
Male	-2.3	-6.0	-6.1	8.9	4.7	0.6
Female	-1.6	-6.6	-2.1	7.6	2.7	0.0
White	-2.1	-5.4	-3.4	7.8	3.6	-0.5
Male	-2.5	-5.3	-4.9	7.9	4.9	0.1
Female	-1.7	-5.3	-2.0	7.8	2.5	-1.4
Black	-4.2	-5.1	-7.0	6.6	5.4	4.2
Male	-3.6	-7.0	-16.2	9.5	13.5	3.7
Female	-5.1	-3.0	0.7	4.3	-1.1	4.1
Hispanic	4.4	-16.9	-7.7	13.8	5.4	0.8
Male	3.9	-9.3	-8.9	16.9	-0.2	-2.3
Female	5.2	-27.1	-6.8	9.7	13.2	5.7
Young Adults (20-24)	0.0	-2.5	-0.7	1.8	-0.3	1.6
Male	-0.1	-2.4	-0.7	1.8	-1.0	2.6
Female	0.3	-2.6	-0.7	1.9	0.5	0.5
White	0.0	-1.7	-0.8	1.4	-0.1	1.2
Male	0.3	-1.6	-0.4	1	-0.9	1.3
Female	-0.3	-1.9	-1.2	1.7	0.7	0.9
Black	1.4	-3.1	0.1	0.2	-0.6	2.1
Male	-1.0	-2.6	-0.9	-1.3	0.1	5.7
Female	3.5	-3.3	0.8	1.0	-1.6	-0.4
Hispanic	0.0	-7.3	0.0	5.6	-1.8	3.5
Male	-0.7	-6.5	-1.9	6.8	-3.8	6.3
Female	1.2	-8.2	2.7	4.1	0.9	-0.6

Source: EPI analysis of CPS ORG.

APPENDIX TABLE 2
Estimated Change in Employment Associated With the Minimum-Wage Increase in October 1996

	1995-96			1996-97			Difference-in-Difference			
	Apr-Sep (1)	Oct-Mar (2)	Difference (3)	Apr-Sep (4)	Oct-Mar (5)	Difference (6)	Raw (7)	Growth-Adjusted (8)	State-Adjusted (9)	Standard Error (10)
All	63.2	62.6	-0.7	63.6	63.2	-0.3	0.3	n.a.	n.a.	0.2
Teenagers	47.6	40.7	-6.9	46.6	40.8	-5.8	1.0	0.5	0.4	0.7
Male	48.8	40.3	-8.5	47.5	39.8	-7.7	0.7	0.4	0.4	1.0
Female	46.3	41.0	-5.3	45.7	41.7	-4.0	1.4	0.8	0.6	1.0
White	54.8	47.0	-7.8	54.3	47.1	-7.2	0.6	0.1	0.0	0.9
Male	55.8	46.1	-9.7	54.5	45.7	-8.8	0.8	0.5	0.4	1.2
Female	53.7	47.9	-5.8	54.1	48.6	-5.5	0.3	-0.0	-0.0	1.3
Black	27.6	25.0	-2.6	28.6	24.4	-4.2	-1.6	-2.1	-1.8	1.8
Male	27.3	22.9	-4.4	28.3	21.3	-7.0	-2.6	-2.9	-2.3	2.6
Female	27.9	27.1	-0.8	28.9	27.4	-1.5	-0.6	-1.4	-1.4	2.5
Hispanic	37.3	31.4	-5.9	34.1	32.5	-1.6	4.3	4.1	3.8	2.1
Male	40.8	36.6	-4.2	39.9	36.1	-3.8	0.5	0.7	0.7	3.1
Female	33.6	26.0	-7.6	28.0	28.6	0.6	8.1	7.0	6.5	2.9
Young Adults	70.6	68.1	-2.5	70.9	69.4	-1.5	0.9	0.6	0.7	0.7
Male	77.1	72.8	-4.3	76.4	73.4	-3.0	1.2	0.9	1.1	0.9
Female	64.2	63.5	-0.7	65.6	65.5	-0.1	0.6	0.5	0.7	0.9
White	75.5	72.9	-2.6	76.0	73.8	-2.2	0.5	0.2	0.4	0.7
Male	80.7	75.9	-4.8	80.2	76.8	-3.4	1.4	1.1	1.2	1.0
Female	70.4	69.9	-0.5	71.8	70.8	-1.0	-0.5	-0.5	-0.3	1.1
Black	55.8	56.0	0.2	57.7	55.4	-2.3	-2.4	-2.7	-2.5	2.1
Male	60.4	60.2	-0.2	61.4	55.6	-5.8	-5.5	-5.8	-5.6	3.1
Female	52.0	52.5	0.5	54.5	55.2	0.7	0.2	0.4	0.5	2.7
Hispanic	64.8	61.5	-3.3	65.0	67.3	2.3	5.6	5.0	5.1	1.9
Male	78.6	74.5	-4.1	77.5	77.8	0.3	4.4	3.9	4.0	2.4
Female	49.3	47.1	-2.2	51.1	55.0	3.9	6.1	6.4	6.4	2.8

Source: EPI analysis of CPS.

Endnotes

1. See, for example, Brown et al. 1982 and Card and Krueger 1995.

2. Note that some workers in Tables 1A and 1B earn less than the prevailing minimum wage ($4.25 for Table 1A; $4.75 for Table 1B). These workers (1) may not be covered by the federal minimum wage (family members working in a family-owned business, for example); (2) may work for employers who are not complying with the law; (3) may have inaccurately reported their hourly wage, usual hourly earnings, or usual weekly hours; or (4) may have been assigned an erroneous hourly wage as a result of the hours-imputation process used for workers who do not work a usual number of hours per week.

3. We use official Census terminology to refer to racial and ethnic minorities. Our categories, however, differ from the Census in that our white and black groups exclude Hispanics.

4. Among black teenagers, the wage effects were largest for males; among Hispanic teens, females received the largest boost.

5. The shift in the distribution could reflect ongoing nominal wage growth, but it seems more likely that most of the movement in the teen and young adult wage distribution responds to the minimum-wage increase. First, the overall wage distribution, which is both less responsive to the minimum wage and more responsive to average wage growth, changes little across the two six-month periods. Second, the pattern of changes in the distribution is consistent with the idea that it is the minimum wage, not nominal wage growth, that is behind the shift in the distribution. After the increase, many workers are "bunched" at exactly $4.75.

6. For an excellent review of the previous literature on the minimum wage, see Card and Krueger (1996), chapters 6-9.

7. See the data appendix for an explanation of the methods used to adjust the raw employment rate changes.

8. White female teenagers appeared to experience a small (0.3 percentage point) increase in employment, but the rise disappears after controlling for economic growth.

9. Those differences marked with the # sign are statistically significant at the 0.10 level, that is, if we were to repeat the estimation procedure 100 times with a different random sample from the same population, we would expect that the confidence interval we construct would include the "true" employment change 90 times. Results marked with an asterisk are significant at the 0.05 level, that is, if we were to repeat the estimation procedure 100 times we would expect that the confidence interval we construct would include true value 95 times. In this sense, results marked * are more reliable than those marked #.

10. For instance, when we repeat the analysis using seven-month periods, the impact of the raw and growth-adjusted employment effect for black men, age 20-24, diminishes and is no longer statistically significant. The seven-month results for the other groups are not qualitatively different from those using the six-month periods in Table 2.

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

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Economics With an Agenda

Wednesday, March 18, 1998; Page A20

In light of the fact that James K. Glassman chose to begin his Feb. 24 op-ed column, "Don't Raise the Minimum Wage," with the statement "Here we go again," it is only fair that supporters of an increase be given an opportunity to respond by saying -- to paraphrase Ronald Reagan -- "There he goes again."

During the 1996 debate, The Post published at least three columns by Mr. Glassman in opposition to increasing the minimum wage [March 4, March 30, May 28]. In the first, he relied on arguments in the 1979 edition of an economics textbook, even though the 1994 edition had incorporated findings that "the undesirable effects of the minimum wage laws are far less serious than previously was widely believed."

This time, Mr. Glassman has outdone himself in seeking to resurrect the reasoning of a Supreme Court decision from the 1920s that was reversed in 1937. That alone should tell Post readers something about the mind-set of opponents, some of whom hope to return to the laissez-faire economy of the 19th century.

Of far greater concern is Mr. Glassman's citation in his latest column of an Employment Policies Institute allegation that 128,000 jobs for teenagers were lost as a result of the 1996 legislation. The institute developed the estimate based on a ratio of employed youth to the total teenage labor force following the first increase in October 1996 -- but ignored the fact that the ratio for the entire labor force nonetheless rose. Moreover, calculations for the period following the second increase in September of 1997 show an increase in the teenage ratio six times that of the ratio for all workers. In fact, from August 1996 to January 1998 the youth ratio rose from 41 percent to 45.6 percent, a gain of 4.1 points overall.

Mr. Glassman also cited an unnamed "survey" to claim that "90 percent" of economists believe that, based on potential job losses, the case against increasing the minimum wage is 'open and shut.' " If that is the case, then how is it that 101 economists, including three Nobel Prize laureates, declared in 1995 that "the overall effects on the labor market, affected workers, and the economy, would be positive"?

Perhaps Mr. Glassman also should consider that even with the 1996 increase, the real purchasing power of the minimum wage is below that of 30 years ago, and only 42.2 percent of the nation's average wage, compared with 56.1 percent in 1968 and 46.5 percent in 1980. At a time when Americans still are recovering from the effects of the Reagan era and the gap between rich and poor is greater than at any

time since before World War II, further increases in the minimum wage are needed.

AMY ISAACS

National Director

Americans for Democratic Action Inc.

Washington

DPC

Special Report

March 19, 1998
Publication: SR-45-Employment

Keeping Pace With Prosperity: Americans Deserve a Fair Minimum Wage

DPC Staff Contact: Chuck Cooper (202-224-3232)

Democratic Policy Committee
United States Senate
Washington, D.C., 20510-7050

Tom Daschle, Chairman
Harry Reid, Co-Chairman

DPC

What do you think about these #s going into our general sheets? Do they "jive" with our current set of numbers?

Robin

Keeping Pace With Prosperity: Americans Deserve a Fair Minimum Wage

By nearly every measure, we have the strongest economy in a generation. A disappearing deficit, rising worker productivity, and increased business investment have given us robust economic growth, the lowest unemployment rate in 24 years, and the lowest core inflation rate in three decades.

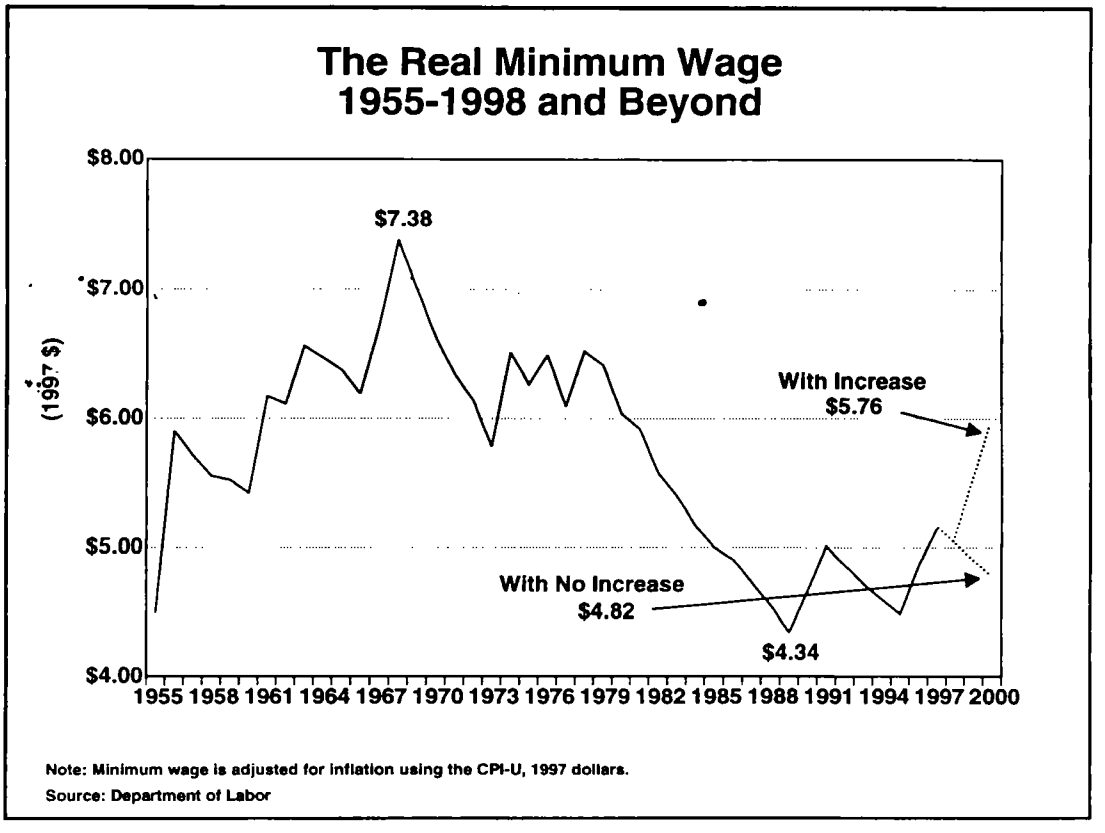
But the benefits of prosperity have not flowed to lower wage workers. A full-time minimum wage worker earns $10,712 per year—$2,600 below the poverty level for a family of three. And while the wealthiest five percent saw their investments increase an average of $51,000 in 1996 alone, the real wages of the bottom 60 percent of American workers are less than they were a decade ago (Bureau of the Census).

Democrats believe that honoring work means paying workers a fair wage. The Democratic legislative proposal, the *Fair Minimum Wage Act of 1998*, would provide a 50 cent increase to the minimum wage on January 1, 1999, and a second increase on January 1, 2000. This modest increase, which would raise the minimum wage to $6.15 per hour, will help millions of lower-income Americans and their families.

Do American Workers Need a Minimum Wage Increase?

While the recent increase in the minimum wage helped millions of low-wage Americans, more needs to be done. The minimum wage increase passed in 1996 prevented the minimum wage from falling to its lowest inflation-adjusted level in 40 years.

The proposed increase to $6.15 would get the minimum wage back to where it was in the early 1980s. If we do nothing, the real value of the minimum wage in the year 2000 will be just $4.82 in 1997 dollars—nearly as low as it was before the 1996 minimum wage increase (Department of Labor).



Across the country, soup kitchens, food pantries and homeless shelters are increasingly serving the working poor—not just the unemployed. According to a recent survey by the U.S. Conference of Mayors, 38 percent of those seeking emergency food aid were employed, compared to 23 percent in 1994. Local government officials now cite low-paying jobs as the most common cause of hunger in their communities (U.S. Conference of Mayors).

Will an Increase in the Minimum Wage Lead to Job Losses?

Republicans claim an increase in the minimum wage will result in a significant loss of jobs. Critics of the minimum wage contend that employment losses would be particularly hard on women, minorities and teenagers, which are the very groups that Democrats believe would be helped by a minimum wage hike.

In fact, there were increases in employment for women (2.7 percent), African Americans (14 percent), Hispanics (11 percent), and teenagers (5.1 percent) between September 1996 and January 1998 (Department of Labor).

Since 1982, nearly two-dozen studies have concluded that modest minimum-wage increases have no detectable effect on employment. One of the most comprehensive studies, conducted by Princeton economists David Card and Alan Krueger, found that employment in fast-food restaurants grew at least as quickly, and possibly slightly faster, in New Jersey as in neighboring counties in Pennsylvania after New Jersey increased its State minimum wage (David Card and Alan B. Krueger, "Unemployment Chimera," *The Washington Post,* March 6, 1998).

On February 2, 1998, a group of leading economists, including a Nobel Prize winner, a former president of the American Economics Association, and a former Secretary of Labor, wrote to the President stating their opinion that the recent increases did not have a negative effect on employment. (A copy of the letter is attached to this report as Appendix A).

Who Will Benefit From an Increase in the Minimum Wage?

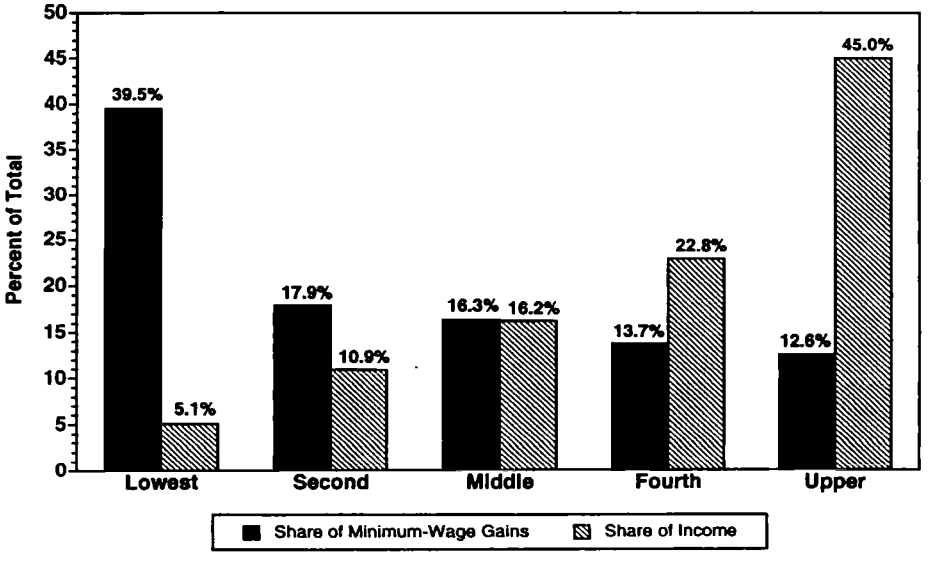
Republicans claim that increases in the minimum wage are poorly targeted because they mostly go to teenagers in high-income families. An analysis of Department of Labor data reveals:

- 74 percent of the workers who will benefit from the increase are adults;
- 57 percent of the gains from the increase will go to working families in the bottom 40 percent of the income scale;

- 50 percent of the affected workers work full time;
- 40 percent of the workers are the sole breadwinners in their families;
- 60 percent of the workers are female;
- 15 percent of the workers are African-American; and,
- 14 percent of the workers are Hispanic.

Twelve million workers will benefit from the increase—more than ten percent of the workforce in 18 States. (A State-by-State breakdown of the number of workers affected by the new minimum wage proposal is attached as Appendix B.) The distributional data reveal that the modest increase in the minimum wage proposed by Democrats will go overwhelmingly to the families that need it most: hard-working adult workers in lower-income families.

Distribution of Minimum-Wage Gains and Income Shares by Fifth, 1996
All Working Families



Source: EPI analysis of 1996 March CPS data.

Will Raising the Minimum Wage Really Make a Difference?

The proposed increase in the minimum wage will improve the quality of life for millions of American workers and their families. Under the proposed increase, a full-time minimum wage worker will earn an additional $2,000 by the end of the year 2000. Lower-wage families could use this extra money to pay for:

- groceries for six months, or;
- rent for 10 months, or;
- utility bills for 12 months, or;
- health care for 13 months, including insurance premiums, prescription drugs, and other health-related out-of-pocket expenses, or;
- a computer, clothing, and school supplies for a child in school.

Hours of Work Needed Per Week to Stay out of Poverty

1979

40 hours at minimum wage

1998

52 hours at minimum wage

Source: Democratic Policy Committee

Do Americans Support an Increase in the Minimum Wage?

Recent polls show that Americans strongly support increasing the minimum wage.

- A January 1998 *Washington Post*-ABC poll found that 76 percent of Americans support an increase in the minimum wage.
- A January 1998 Peter Hart Research poll found that 77 percent of Americans support raising the minimum wage.
- A February 1998 *Los Angeles Times* poll found 78 percent support an increase in the minimum wage.
- A February 1998 Pew poll found 80 percent support the Democratic proposal to increase the minimum wage.

Democrats Believe Low-Wage Americans Deserve a Raise

The *Fair Minimum Wage Act of 1998* would raise the minimum hourly wage by $1 over the next two years. In an era of economic growth in which the gap between rich and poor has widened, raising the minimum wage is a matter of fundamental fairness. Numerous studies have shown that modest increases in the minimum wage, especially during times of low unemployment, have not had a negative impact on employment. Analyses of Department of Labor data also reveal that benefits from an increase in the minimum wage will go to the adult, low-income workers who need it the most. As the sixtieth anniversary of the minimum wage approaches, Democrats will continue to fight to ensure that American workers earn a livable wage.

Appendix A

February 2, 1998

The Honorable William J. Clinton
President
The White House
1600 Pennsylvania Ave., N.W.
Washington, D.C. 20500

Dear Mr. President:

As economists who are concerned about the erosion in the living standards of households dependent on the earnings of low-wage workers, we applaud your endorsement of an increase in the Federal minimum wage.

We believe that the 1996 and 1997 increases had a beneficial effect not only on those whose earnings were increased by 90 cents an hour but also on the economy as a whole. Billions in added consumer demand helped fuel our expanding economy in those years.

We have been particularly impressed by the fact that the 1996 and 1997 increases did not have, as is often predicted during minimum wage debates, a negative effect on either prices or employment. While those increases were going into workers' pockets, total employment levels rose by 2.7 percent, teen employment levels rose 2.2 percent, and the employment of women workers rose 2.7 percent. And, at the same time, the CPI remained virtually stable.

Given the nation's unemployment rate and strong economy without inflation, now is the time to deepen our public commitment to a decent minimum wage. We strongly support the Kennedy-Bonior bill. That legislation would increase the minimum wage by 50 cents in each of the next three years, bringing the minimum wage to $6.65 per hour on September 1, 2000. Thereafter, the minimum wage would be indexed to the rate of inflation. We believe that the bill will help low-wage workers and their families without significantly jepoardizing employment opportunities or causing other harmful economic effects.

Thank you for your leadership in this important area.

Sincerely,

Eileen Appelbaum
Economic Policy Institute*

Barbara R. Bergmann
American University
University of Maryland

Peter Berg
Economic Policy Institute*

Jared Bernstein
Economic Policy Institute*

Barry Bluestone
University of Massachusetts/Boston

Sheldon Danziger
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of Technology

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Harvard University

* For Identification Purposes Only

Appendix B

Workers Affected by the New Minimum Wage Proposal, by State

	Percent of Workforce	Number
Northeast	6.8%	1,505,725
New England	5.4%	324,469
Maine	7.9%	41,970
New Hampshire	3.7%	19,855
Vermont	7.5%	19,368
Massachusetts	4.8%	133,879
Rhode Island	8.3%	35,740
Connecticut	5.0%	73,656
Mid-Atlantic	7.4%	1,181,256
New York	7.1%	523,458
New Jersey	6.0%	216,710
Pennsylvania	8.7%	441,088
Midwest	7.8%	2,159,836
East North Central	7.6%	1,474,846
Ohio	8.9%	440,152
Indiana	7.3%	198,858
Illinois	7.2%	374,273
Michigan	7.3%	304,995
Wisconsin	6.3%	156,568
West North Central	8.2%	684,990
Minnesota	6.7%	147,231
Iowa	9.8%	127,608
Missouri	6.9%	169,210
North Dakota	11.7%	32,387
South Dakota	10.3%	31,396
Nebraska	8.8%	64,927
Kansas	10.2%	112,231
South	9.7%	3,851,376
South Atlantic	8.5%	1,751,579
Delaware	6.6%	22,054
Maryland	5.5%	129,352
District of Columbia	4.2%	9,262
Virginia	6.6%	194,415
West Virginia	13.7%	92,379

	Percent of Workforce	Number
South		
South Atlantic (cont.)		
North Carolina	8.5%	283,832
South Carolina	10.4%	171,742
Georgia	8.1%	271,569
Florida	9.8%	576,973
East South Central	10.1%	674,056
Kentucky	8.4%	132,340
Tennessee	8.7%	192,818
Alabama	11.7%	211,217
Mississippi	12.9%	137,681
West South Central	11.6%	1,425,740
Arkansas	13.3%	132,059
Louisiana	12.7%	213,663
Oklahoma	11.9%	154,486
Texas	11.1%	925,533
West	8.5%	2,080,955
Mountain	8.3%	586,134
Montana	11.6%	39,348
Idaho	9.7%	49,433
Wyoming	14.6%	29,266
Colorado	6.0%	109,088
New Mexico	11.4%	75,100
Arizona	9.5%	178,798
Utah	7.3%	64,497
Nevada	5.3%	40,605
Pacific	8.6%	1,494,822
Washington	6.9%	170,445
Oregon	7.8%	106,258
California	9.2%	1,179,533
Alaska	3.2%	8,074
Hawaii	6.4%	30,511
U.S.	8.4%	9,597,892

Note: Affected workers are those who earned between the current minimum and $5.72 in 1997. $5.72 is the proposed 2000 level ($6.15) in 1997 dollars.

Source: EPI analysis of CPS ORG data.

Appendix C

Increases in the Minimum Wage (1955-1996)

Date of Legislation	Date Increase Took Effect	Minimum Wage	Amount of Increase	Percent Increase	Unemployment	Inflation
Aug. 12, 1955	Mar. 1, 1956	$1.00	.25	33%	4.1%	3.0%
		(was .75)				
May 5, 1961	Sep. 3, 1961	1.15	.15	15%	6.7%	0.7%
	Sep. 3, 1963	1.25	.10	9%	5.7%	1.6%
Sep. 23, 1966	Feb. 1, 1967	1.40	.15	12%	3.8%	3.0%
	Feb. 1, 1968	1.60	.20	14%	3.6%	4.7%
Apr. 8, 1974	May 1, 1974	2.00	.40	25%	5.6%	12.9%
	Jan. 1, 1975	2.10	.10	5%	8.5%	6.9%
	Jan. 1, 1976	2.30	.20	10%	7.7%	4.9%
	Jan. 1, 1977	n/a	n/a	n/a	7.1%	6.7%
Nov. 1, 1977	Jan. 1, 1978	2.65	.35	15%	6.1%	9.0%
	Jan. 1, 1979	2.90	.25	9%	5.8%	13.3%
	Jan. 1, 1980	3.10	.20	7%	7.1%	12.5%
	Jan. 1, 1981	3.35	.25	8%	7.6%	8.9%
	Jan. 1, 1982	n/a	n /a	n/a	9.7%	3.8%
Nov. 17, 1989	Apr. I, 1990	3.80	.45	13%	5.6%	6.1%
	Apr. 1, 1991	4.25	.45	12%	6.8%	3.1%
Aug. 20, 1996	Oct. 1, 1996	4.75	.50	12%	5.4%	3.3%
	Sep. 1, 1997	5.15	.40	8%	4.9%	1.7%
Proposal	Jan. 1, 1999	5.65	.50	10%		
	Jan. 1, 2000	6.15	.50	9%		

Appendix D

What Democratic Presidents Have Said About the Minimum Wage

"Our Nation, so richly endowed with natural resources and with a capable and industrious population, should be able to devise ways and means of insuring to all able-bodied working men and women a fair day's pay for a fair day's work."

—President Franklin D. Roosevelt

"They [Republicans] say they are in favor of good wages —but they argue that the minimum wage should be held down to starvation level. They are making exactly the same speeches now against the 75 cent minimum wage that they made back in the thirties gains a 40-cent minimum wage."

—President Harry S. Truman

"[An increase would] improve the level of living, morale, and efficiency of many of our lowest paid workers, and provide incentives for their more productive utilization. This can actually increase productivity and hold down unit costs with no adverse affects."

—President John F. Kennedy

"We must provide all possible assistance to those who seek work and decent living conditions for those who do work."

—President Lyndon B. Johnson

"This bill (FLSA) said that Americans who had to work with their hands, the laborers of our Nation, those from low-income families, should be treated fairly. They should be given the right to an income which would at least buy the necessities of life."

—President Jimmy Carter

"In an economy that honors opportunity, all Americans must be able to reap the rewards of prosperity. Because these times are good, we can afford to take one simple, sensible step to help millions of workers struggling to provide for their families: We should raise the minimum wage."

—President Bill Clinton

This place is like a morgue — where is everyone?

JB

GOP Heaven: No Agenda, No Problem!

This week Republicans got blitzed in a special House election they could have won. So maybe it's time to rethink their current four-corner stall, run-out-the-clock political strategy.

Sure, and President Clinton really wants to tell us the Monica Lewinsky story. Rethinking is in fact heresy to GOP leaders determined to spend most of this year admiring their own poll numbers and wishing upon a Kenneth Starr. The latest internal party survey has Congress at 63% approval, higher than anyone can remember.

Republicans have begun to believe this is due to their own charm and good looks, not just the booming economy. With their balanced budget in the bank, they figure they can coast to re-election in November.

House GOP leader Dick Armey spoke at a private dinner this week about the year's agenda to reform car insurance and bankruptcy laws, and to provide school choice for Washington, D.C. Brings chills of anticipation, doesn't it? But at least Mr. Armey is still trying. Without his even limited agenda, the only GOP ideas would be Mr. Clinton's.

This is clear from the budget that Senate Republicans will unveil next week. They've given up on anything but token tax cuts, despite the growing budget surplus.

They're petrified that cutting taxes will invite Mr. Clinton to demagogue them for not "saving" any surplus for Social Security. (Never mind that Mr. Clinton's alternative is to spend it.) So the same Republicans who wouldn't cut taxes because the government had too little money now say they can't cut taxes because the government has too much.

House Republicans are at least willing to cut some taxes to offset any cigarette tax increase. But the Senate GOP lacks the votes even for that. So leader Trent Lott is leaning toward a play-it-safe strategy that would devote all tobacco taxes to Medicare. The thinking is that at least this will block Democrats from spending the windfall on social pork barrel.

This has to be more clever than it sounds. "It's saying let's spend new money to replace the money we just saved on Medicare in last year's budget agreement," says Indiana Rep. David McIntosh, in head-scratching consternation. It may work as defensive politics inside the Senate, but it doesn't help the GOP set any agenda in the country.

Potomac Watch

By Paul A. Gigot

Republicans are overflowing with reasons for doing nothing. Repealing racial preferences would only inflame the Democratic base. Tax reform is too complicated, and Republicans can't agree. Reforming Social Security to allow individual accounts risks angering the elderly. And cutting federal spending . . . fill in the blank.

The political risk is that come summer Mr. Clinton will be calling them "do-nothing." Democrats will demand votes to raise the minimum wage (again), mandate family leave for PTA meetings, and regulate health care in ways that (stealthily) raise insurance costs. Republicans will have nothing to say to change the subject.

In a nonpresidential year, "most voters don't turn out. That means the Republican and Democratic bases decide the election. And we're doing none of the things that attracted the people who elected us in 1994," says Mr. McIntosh.

This week's special-election rout in California helps his case. Republicans have held the Santa Barbara seat as recently as 1994 and even Bob Dole carried it in 1996. The media have blamed intra-GOP fighting for the loss, but that's more symptom than disease. Party factions always emerge when there's no broader cause to rally behind.

Newt Gingrich

The defeat also showed that Democrats can match GOP spending when it matters. Republicans have been counting on cash to substitute for ideas. But in the 50 or so races that will really be competitive this November, Democrats will have more than enough money to play rough.

"If we were going to have a great year, we would have won that race," says Mr. McIntosh. On Election Day, Newt Gingrich, a.k.a. Speaker Pangloss, told fellow GOP leaders that their candidate might win, and couldn't lose by more than the 10,000 votes the GOP incumbent lost by in 1996. The actual margin was more than 12,000.

Given the six-year Clinton itch, it's hard to see Republicans losing control of Congress in November. But the 20- to 40-seat gain that Mr. Gingrich has been advertising now looks like fantasy. Unless Mr. Clinton drowns in Monica Lewinsky's arms, the better odds are for a GOP gain of just 10 or so House seats. A possible Senate pickup of four or five could slip to one or two.

Liberal Republicans would continue to hold the balance of power in Congress—which means nothing much good will get done next year either. And it keeps Democrats well within striking range of taking back the House in 2000.

Faster than anyone expected, Republicans are demonstrating the limits of congressional power. Lacking a national leader, they've splintered into factions. Hoping above all else to hold onto their jobs, they've lost their zeal for reform. And bamboozled by a popular (if unbelievable) president, they'll take no policy risks.

But, hey, how about those approval ratings?

THE WALL STREET JOURNAL FRIDAY, MARCH 13, 1998

Minimum Wage: Who Pays?

By HARRIET F. CANE

President Clinton and his allies in Congress are calling for another increase in the minimum wage. But they should consider the experience of small-business owners like me, who struggled through the last increase. I own and manage a small cafe. I have had as many as 16 employees; I now have nine. Most of them are teenagers; the rest, working mothers.

Before the last increase I wrote letters to the president and my congressmen. I explained that the mandated wage increase was only the tip of the iceberg. To maintain the wage increment for senior employees, I would have to raise their wages above the new minimum. My monthly payroll would increase by $570—and that didn't include the payroll taxes for Social Security, Medicare, unemployment insurance and workman's compensation. For my efforts I received nicely worded form letters about the benefits of the wage increase.

When the increase passed, I had to reduce staffing hours. Result: I am working harder to earn my money. I already worked six days a week, every week. The staffing cutbacks increased my workload by 15 hours a week. I also cut back on outside services, so I am now mopping my own floors two weeks each month and doing all my own accounting, the weekly laundry and as many of the repairs as I can.

When Mr. Clinton signed the wage increase into law, he had by his side a minimum-wage worker who stated that now she did not have to choose between paying her electric bill or her gas bill. The same evening, our local news interviewed a woman who said she would now be able to buy her daughter a compact disk player for graduation. I do not begrudge either of these women their good fortune. But business owners work hard too, and we also have to make tough choices. I suffer from several chronic illnesses, and the wage increase has forced me to cut back on medical care.

Money for minimum wage increases has to come from somewhere. Mr. Clinton's proposed increase would raise my annual payroll by $7,200, forcing me to close my doors. To the politicians I say this: You have the power to destroy the American Dream for thousands of small-business owners. If you pass another increase in the minimum wage, you can tell the teenagers and working mothers I employ why they no longer have jobs. Then try asking for their votes.

Ms. Cane owns the Sweet Life restaurant in Marietta, Ga.

THE WALL STREET JOURNAL FRIDAY, MARCH 13, 1998

Hamas pins death of agent on Israel

Vows 'earthquake' to avenge killing

By Abraham Rabinovich
SPECIAL TO THE WASHINGTON TIMES

JERUSALEM — Israel braced for a new round of terror yesterday after Palestinians accused it of responsibility for the mysterious death of a master Hamas bomb maker.

The body of Mohia-din Sharif was found Sunday night in the West Bank city of Ramallah alongside a car that had been blown up by more than 100 pounds of explosives.

Palestinian authorities, within whose jurisdiction the blast occurred, initially described it as a "work accident" by a terrorist rigging a large car bomb, apparently for use against an Israeli target.

This appraisal changed dramatically yesterday when Palestinian pathologists identified the body as that of Mr. Sharif, who has been near the top of Israel's wanted list for two years.

The pathologists said he had been killed by gunfire three hours before the explosion. They reported that he had been shot twice in the chest and once in a leg.

The findings led many Palestinians, including security officials, to conclude that Mr. Sharif was killed by agents of the Shin Bet, the Israeli security service, and that the explosion was deliberately set off by them afterward.

Israeli officials went to unusual lengths to deny any connection with the incident.

"I can say categorically that Israel had nothing to do with it," said Prime Minister Benjamin Netanyahu at a news conference. Defense Minister Yitzhak Mordechai issued a similar denial.

"Sharif was a terrorist with blood on his hands, and it is a good thing that he has gone to the next world," said Mr. Mordechai, "but I

see HAMAS, *page A18*

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can declare that Israel and its security services have nothing to do with this event."

In everyone's mind is the memory of Hamas' reaction to the killing of Mr. Sharif's predecessor, Yehiye Ayyash, the bomb maker known as "the engineer."

He was killed two years ago in the Gaza Strip when a booby-trapped cellular telephone that had been given to him by a supposed friend was detonated by an electronic impulse as he talked to his father.

Israel denied involvement at the time, but with a wink. In the following two months Hamas blew up three Israeli buses with suicide bombers, killing dozens.

The series of bombings contributed to Mr. Netanyahu's surprise election victory a few months later on a hard-line, anti-terrorist platform.

Hamas warned of attacking Zionists "all over the world."

Hamas wasted little time in warning of similar retaliation this time, not only in Israel, but also abroad.

"The Zionists must prepare for a strong, violent, earthquake reaction," said a Hamas leaflet, adding that retribution would come "sooner than many can imagine."

It also warned of attacks against Zionist targets "all over the world."

The tension comes just as the peace process, stalled for more than a year, shows signs of possible movement.

Although U.S. negotiator Dennis Ross flew home from the Middle East this week without the hoped-for breakthrough, there is the feeling in Jerusalem that Mr. Netanyahu is laying the groundwork for a further Israeli pullback on the West Bank that would be less than Washington is demanding but more than the hard-liners in his Cabinet have been prepared to accept.

This is signaled not just by Mr. Netanyahu's statements, but by the remarks and political body language of far-right leaders, who appear to be reconciled to a further West Bank pullback. Even Palestinian leader Yasser Arafat hinted in this direction yesterday.

"I can't say that Ross' visit was a failure," he said. "His efforts are continuing."

Should large-scale terrorism resume, it could well place the peace process back in the deep freeze.

A senior Palestinian official, Nabil Shaath, said that if Israeli involvement is proven it would mean that "state-sanctioned assassinations continue to be the policy of Israel."

However, Mr. Arafat, clearly thinking of the ramifications of renewed terrorism, declined to blame Israel. "The matter is now under investigation," he said.

The Washington Times
THURSDAY, APRIL 2, 1998

Democrats doubt value of minimum-wage war

By Nancy E. Roman
THE WASHINGTON TIMES

A1

Democrats in Congress, who scored one of their biggest political victories in pushing through an increase in the minimum wage two years ago, find themselves surprisingly divided as they gear up to do it again.

With the majority Republicans in the House and Senate united against another increase, Democrats are split over the political and economic payoffs of the issue, complicating prospects for action this year as the tight legislative calendar winds down.

"It's a low priority on its merits, and a low priority in terms of political benefit," said Rep. Gary A. Condit, California Democrat, who chairs a group of conservative Democrats known informally as the "Blue Dogs."

The Blue Dogs were asked by House Minority Leader Richard A. Gephardt of Missouri to rank the top issues facing Congress. Increasing the minimum wage was not on that list.

"I don't see what we get out of it," Mr. Condit said.

Rep. Barbara B. Kennelly, Connecticut Democrat and a supporter of another increase, noted

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there is little time to bring people together.

"Every day that passes makes it seem less likely," she said.

Sen. Edward M. Kennedy, Massachusetts Democrat, is leading the effort to raise the minimum wage from the current $5.15 to $6.15 per hour by 2000. Mr. Kennedy spearheaded the drive in 1996 — over GOP leadership opposition — that resulted in a 90-cent increase in the minimum wage over two years.

"It could happen again," he said.

On Tuesday, Mr. Kennedy advised a group of proponents of an increase in the minimum wage that its passage was "inevitable," said Jim Manley, a spokesman for Mr. Kennedy.

Mr. Kennedy is working closely with Rep. David E. Bonior, Michigan Democrat, and several other House Democrats — mostly from urban areas.

"A bunch of men making $136,000 a year [saying] some guy shouldn't get $6 an hour is a national joke," said Rep. David R. Obey, Wisconsin Democrat.

Mr. Bonior said yesterday he still expects an increase to pass.

"If it came up for a vote, we would get a good total — including Blue Dogs," he said.

Mr. Kennedy had planned to use the same strategy he used in 1996, relying on Democratic and liberal, pro-labor Republican votes in the face of staunch GOP leadership opposition.

But Mr. Kennedy, Mr. Bonior and Mr. Obey have run into strong opposition within their own party. Conservative Democrats echo Mr. Condit in arguing that another increase in the minimum wage is unnecessary.

That House division caused Mr. Kennedy to rethink his strategy. Sources said he wants as strong a vote as possible in the House.

On Friday, Mr. Kennedy decided not to force a Senate vote on raising the minimum wage.

Sources close to Mr. Kennedy said he backed off in part because of fierce opposition from Senate Majority Leader Trent Lott. The Mississippi Republican said he was tired of getting "beat up" on the issue.

Mr. Lott noted that the proposed increase would result in a 40 percent increase in the minimum wage since 1996.

"The majority of small businesses have been milked and shot at long enough," said a clearly annoyed Mr. Lott, speaking at a press conference featuring small-business owners who told of having to lay off workers.

Democrats have been quick to remind the GOP that those were the same arguments they heard in 1996 from GOP leaders who vowed to block a minimum wage increase.

Later that day, Mr. Kennedy distributed a statement from the chief executive officer of Burrito Brothers Inc., telling how his fast-food chain grew and prospered in spite of a higher minimum wage.

"Since the last minimum wage passed, my company has doubled in size," said Eric Sklar, who founded the Mexican food chain in 1989.

Georgia Rep. John Linder, chairman of the National Republican Campaign Committee, said GOP leaders warned business groups they had better get to work if they wanted to avoid a repeat of 1996's vote.

"Message received," said Ralph Hellmann, director of federal government relations with the National Federation of Independent Business.

He said anti-increase forces will launch a massive mailing to small businesses over Congress' Easter break.

Rep. Jennifer Dunn, Washington Republican, warned that she and fellow GOP members only went along with the minimum wage increase last time in exchange for tax breaks for small business.

"I would never have voted for it otherwise," she said.

Rep. Charles T. Canady, Florida Republican, said, "There is only so much you can do for entry-level jobs.

"I voted for it last time, but I'm opposed to doing it again this soon," he said.

Mr. Linder noted that even pro-labor Republicans have been quiet.

"We're not hearing from them," he said.

Mr. Obey said that if Democrats lose this battle, it will be because too many members circulate among the "social elite" in their districts and not their minimum-wage constituents.

"The average congressman is talking to a bunch of guys who wear suits," he said, adding that he once attended a fund-raiser at an art museum where "men in tuxedoes drinking champagne with strawberries lectured me on why they can't afford to pay the minimum wage."

"If it wouldn't have been impolite, I would have heaved in the champagne," he said.

Rep. Elijah E. Cummings, Maryland Democrat, said he wants to increase the minimum wage, but he conceded it is an uphill battle.

"It's going to be difficult," he said. "It's not one of those issues like child care. . . . It's not one of those issues that a lot of people come out for."

The Washington Times
THURSDAY, APRIL 2, 1998

James K. Glassman

Don't Raise the Minimum Wage

Here we go again. To the cheers of Democrats on Capitol Hill, President Clinton has announced he wants to raise the minimum wage by another dollar. It's been increased from $4.25 to $5.15 over the past two years. Next stop: $6.15.

The minimum wage can't be boosted without GOP help, and the last time, 160 Republicans in the House and 27 in the Senate voted for the hike. They wanted to look "compassionate"—even though the minimum wage hurts the very people it's supposed to help. That capitulation, of course, encouraged the latest move. This time, they had better muster some courage, stand and fight.

It won't be easy, with Democratic hucksters selling the press and the public a shiny new perpetual motion machine. Using a dubious study, they contend they can raise the minimum wage without putting people out of work—something economic theory and an avalanche of data dispute.

"Every time we have raised the minimum wage in my lifetime," said President Clinton on Feb. 13, "there have been those that say, 'If you do this, it will cost jobs.' The last time we did it, it didn't cost jobs. We continued to create jobs at a very brisk pace."

This is nonsense, and Clinton is smart enough to know it. Yes, job growth has been brisk since the October 1996 increase. The question is whether it would have been brisker without it.

Economists aren't certain about many things, but on the minimum wage, nearly all of them (90 percent, according to one survey) believe that the case is open and shut. All else being equal, if you raise the price of something (for instance, labor), then the demand for it (for instance, by employers) will decline. That's not just a theory; it's a law.

The effects have been measured dozens of times. Charles Brown of the University of Michigan, for example, cites 19 studies between 1970 and 1991 and concludes, "a 10 percent increase in the minimum wage reduced teen employment by 1 to 3 percent." Using Labor Department data, the Employment Policies Institute, an industry group, says that is precisely what has happened since the last increase: It "resulted in 128,000 fewer teen jobs."

Raising the minimum wage hurts people whose work is worth $5.15 rather than $6.15, depriving them of a chance to learn on the job and move up. What the government says is, "Too bad, Mr. Entry-Level Worker. We won't let you work for $5.15 anymore. If no one is willing to pay you $6.15, you're out of a job."

Unlike low-skilled workers, businesses have choices. They can replace workers with machines or with fewer but more skilled employees (whose wages aren't set by government decree), or they can buy their labor abroad.

Don't believe that it's low-wage people who are hurt? Consider history. In 1923, the District of Columbia passed one of the earliest minimum-wage laws, which "had the effect of depriving Ms. Willie Lyons of her job as the operator of an elevator in the Congress Hotel," writes legal scholar Hadley Arkes in his book, "The Return of George Sutherland."

After being fired, Lyons sued to overturn the law. It was *she* who was damaged, not her employer. And the Supreme Court, with an eloquent opinion by Sutherland, ruled in her favor. (It reversed itself in 1937.)

The minimum wage was then increased regularly until Ronald Reagan took office (and 18 million jobs were created in eight years). Now, wage hikes are back. Democrats are armed with a study by David Card and Alan Krueger of Princeton, which purports to show that a 1992 increase in the New Jersey state minimum wage actually led to increased employment in fast-food spots.

That study has been disputed by other economists, notably David Neumark of Michigan State and William Wascher of the Federal Reserve, who themselves have published seven works on minimum wages. They contend the New Jersey hike "led to a relative decline" in employment.

Card and Krueger say they've just reconfirmed their study, but ask yourself: If a 90-cent increase has a small positive effect, why shouldn't a $9 increase have a large positive effect?

Maybe Card and Krueger are right, but more likely, they haven't looked hard enough for other explanations—which is why they have so few supporters among labor economists.

Still, the question at hand is whether an *additional* $1 hike will be benign. Not even Krueger thinks so.

"I guess I'd be less confident that another increase in the minimum wage, so quickly after the previous one, would have no effect on employment," he said two weeks ago on National Public Radio. "I would think it's probably more likely that it could have an adverse effect." Hmmm.

There's a bigger issue at stake here than economics: whether the state should interpose itself between two people freely entering into a contract.

Arkes writes that in the 1923 minimum-wage cases, Sutherland "simply sought to judge whether the formulas in the legislation contained real principles: When the law imposed restraints on personal freedom, did it in fact proscribe things that were in principle wrong . . .?"

Sutherland thought that, by paying $35 a month to Willie Adkins, the employer "has neither caused nor contributed to her poverty. On the contrary, to the extent of what he pays, he has relieved it."

But there may be a case for boosting the pay of low earners. Instead of hiking the minimum wage, why not end the payroll tax for everyone making, say, less than $7 an hour? That would raise take-home pay while actually boosting demand for labor.

Such a tax cut is urgent with so many Americans leaving welfare for work. With a higher minimum wage, they may not find it.

The writer is a fellow at the American Enterprise Institute.

The Washington Post
TUESDAY, FEBRUARY 24, 1998

Susan Low Bloch

Laws Made For a President's Foes

The current presidential crisis is embarrassing, dangerous and avoidable—at least in the future—if we recognize and correct the unfortunate legal structure we have created. The combination of the Supreme Court's decision allowing Paula Jones's suit to proceed while the president is still in office and the independent counsel statute that authorizes single-minded and wholly unrestrained pursuit of the president has emerged as a deadly combination that can seriously damage the office of the presidency.

Had the suit by Paula Jones against President Clinton been deferred until the president was out of office, as many constitutional law professors urged, the present circus would not be taking place. It was the relentless discovery pursued by Paula Jones's attorneys that triggered the present frenzy.

This is not to say President Clinton might not bear some responsibility for allowing this feeding frenzy to find fodder for its sustenance. I do not know the facts and urge everyone to avoid a rush to judgment. My principal concern is that we recognize the dangerous legal structure we have created in which it is relatively easy for political enemies of the president to attack and undermine the office.

It is too late to avoid the present crisis. We have to hope we survive it without unduly eroding the majesty and power of the office of the presidency. But we should try to restructure our system so that it minimizes, rather than increases, the likelihood of such crises. The Supreme Court is unlikely to reconsider its decision to allow civil damage actions to proceed against sitting presidents. But Congress can and should act to protect the presidency in the future. Indeed, the Supreme Court in the *Clinton v. Jones* case explicitly invited Congress to do so.

Specifically, Congress should enact a law providing that presidents may defer civil damage actions until they are out of office. The events of the past few weeks make it clear that one cannot sue a sitting president for civil damages and expect the case to proceed in the same way it would against an ordinary citizen. That is especially true if the cause of action is a salacious accusation. The level of scrutiny, the ability to get attention and the leverage to make exorbitant settlement demands are much greater when the president is the target. In addition, given that discovery in civil sexual harassment suits is notoriously wide-ranging and unconstrained, there is likely to be a broad fishing expedition that can do the kind of substantial damage we have witnessed these past few weeks. Without the discovery permitted in Paula Jones's suit, there would have been no subpoena demanding Monica Lewinsky's testimony at this time. Moreover, these "he says, she says" suits are extraordinarily difficult to dismiss without a trial.

Adding to this volatile mix is the institution of the independent counsel. Congress created this office, after Watergate, to try to de-politicize inquiries into serious accusations against high executive officials. But coupling this institution with the decision to allow suits such as Paula Jones's to proceed against a sitting president throws the worst nightmare of politics and talk-show tripe right back into the legal morass. The independent counsel statute gives the appointed counsel awesome power, unlimited money and unconstrained time with which to scrutinize the activities of one individual, including the president of the United States. These features, as Justice Scalia astutely observed in 1988, are dangerous. The convergence of these two bodies of law—the extravagant breadth of the independent counsel statute and the unfortunate decision to permit civil damage actions against a sitting president to proceed—provides powerful tools with which political antagonists can undermine the president. The combination is clearly challenging this president and, left unmodified, threatens all future presidents as well.

Whatever the outcome of the current crisis, I hope we learn from this dreadful experience. We should repair our legal system to protect the office of the presidency for ourselves and our children.

The writer is a constitutional law professor at the Georgetown University Law Center. She helped to write the amicus briefs in 'Paula Jones v. Clinton.'

The Washington Post
TUESDAY, FEBRUARY 24, 1998

THE WALL STREET JOURNAL
MONDAY, FEBRUARY 23, 1998

Will the House Hear From Starr?

Rule of Law

By Terry Eastland

As a result of his Lewinsky investigation, Kenneth Starr could become the first independent counsel to advise Congress on impeachment. If Mr. Starr does that, he will be acting under the authority of Section 595(c), a never-before-used provision of the 1978 independent counsel statute.

The wording of Section 595(c) is clear. It orders an independent counsel to "advise the House of Representatives of any substantial and credible information . . . that may constitute grounds for impeachment."

The provision has its origins in Watergate. At that time, of course, there was no independent counsel law, and special prosecutor Leon Jaworksi, like his predecessor, Archibald Cox (who was fired by President Nixon), was appointed by the attorney general and worked under legal authority defined by the Justice Department.

In the course of his investigation, Jaworski grew convinced that the president had committed obstruction of justice. "The grand jury wanted to indict him," Jaworski wrote in his memoir. But the prosecutor had doubts about whether the Constitution permits the prosecution of a sitting president, and he anticipated that Nixon would contest any indictment in the courts, with the resulting and awful spectacle of an indicted president waiting months for the Supreme Court to decide the issue.

So Jaworski took a different course. At his request, the grand jury provided the court with a report (which his aides drafted) detailing Nixon's role in the coverup. And then, also at Jaworski's request, the presiding judge, John Sirica, authorized transmission of the report to the House Judiciary Committee. Under the Constitution, impeachment is a task assigned to the House of Representatives.

In the aftermath of Watergate, Section 595(c) of the independent counsel law contemplated the kind of situation that Jaworski faced and Mr. Starr may soon face. Mr. Clinton is effectively, if not officially, a "target" of Mr. Starr's newest probe. The allegations against the president include perjury, suborning perjury and conspiring to obstruct justice. If Mr. Starr is or becomes persuaded of Mr. Clinton's culpability, he will have to decide whether to press forward with an indictment.

Mr. Starr has not indicated his views on this matter. Yet even if he believes the Constitution permits the indictment of a sitting president (the Supreme Court has never ruled on the issue), the same prudential considerations that persuaded Jaworski not to indict are powerful, especially given that the Clinton defense strategy consistently has relied on measures designed to delay. There can be little doubt that Mr. Clinton would battle an indictment in the courts and that the country would have to suffer through a long period of uncertainty.

Whatever Mr. Starr might conclude about indictment, his obligation under Section 595(c) is unquestionable: If he has evidence that the president may have committed an impeachable offense, he must inform the House. The question before Mr. Starr would be only the terms in which he should do this.

Section 595(c) requires him to give the House "substantial and credible information . . . that may constitute grounds for an impeachment." In other words, Mr. Starr would have to have reasonable grounds for believing that the president had committed an impeachable offense.

Which leads to the question: What is an impeachable offense? In Watergate, the House Judiciary Committee staff adopted the view of most scholars—that an impeachable offense need not be a criminal offense but may be "substantial misconduct." That is, it's not necessary for a president actually to have broken the law for him to be impeached.

Section 595(c) thus invites an independent counsel to define impeachable offenses broadly. But what Mr. Starr *may* do is not necessarily what he *should* do. It would be wiser for Mr. Starr to focus any impeachment report more narrowly, sticking to indictable offenses.

If he has evidence that the president may have committed an impeachable offense, he must inform the House.

While it's true that an independent counsel is not an ordinary prosecutor and that he has the authority to issue a final report that discusses more than the outcomes of actual prosecutions, the most credible aspect of a counsel's work concerns his core responsibility—criminal investigation. Over the life of the independent counsel statute, counsels who have made comments about the ethics or fitness for office of those they have investigated have aroused controversy and undermined their central mission. The same would hold true for Mr. Starr.

One can expect the president's lawyers to try to stop Mr. Starr from making any report to the House. But it is hard to see how they could succeed or even force a delay for very long. Two Nixon aides—H.R. Haldeman and Gordon Strachan—unsuccessfully contested the transmission of Jaworski's report. (Nixon himself raised no objection.)

Mr. Clinton's lawyers could try a separation-of-powers challenge, arguing that Section 595(c) is unconstitutional because it confers on an executive officer (the independent counsel) a portion of the impeachment responsibility that the Constitution gives to the House. But this would be an uphill battle, given the Supreme Court's seven-to-one majority sustaining the independent counsel act in the 1988 *Morrison* case. And it would be a disingenuous one at that, since Mr. Clinton did not object in any respect to the 1994 reauthorization of the independent counsel statute; on the contrary, he signed it enthusiastically.

Once Mr. Starr's impeachment report was received by the House, the question then would become whether it would lead to an impeachment inquiry. A report narrowly focused on indictable offenses would be difficult for members of either party to dismiss, since the president would stand accused of serious crimes. Whether a House inquiry, if undertaken, would then lead to impeachment, much less whether the Senate would convict (a two-thirds majority is required), are other matters entirely, best left for assessment when they ripen, if they ever do.

For now, it is best to continue following the saga of the Clinton scandals one step at a time—with perhaps the next major step being an impeachment report by Mr. Starr.

Mr. Eastland is publisher of The American Spectator and author of "Ethics, Politics and the Independent Counsel."

Workers Need Tax Cuts, Not a Minimum Wage Hike

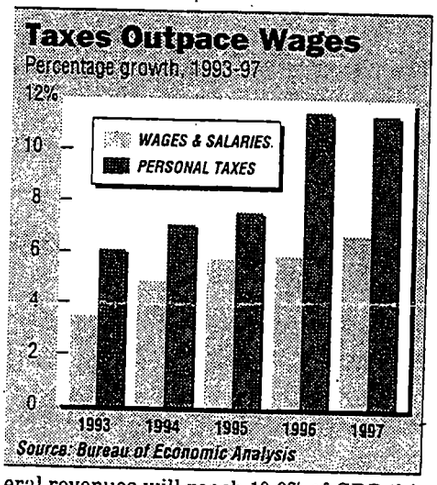
By J.T. YOUNG

President Clinton and Sen. Ted Kennedy (D., Mass.) are pushing for another increase in the minimum wage, which has already gone up twice in the past two years. But while it's true that Americans have been financially squeezed, especially during the Clinton years, the real culprit is not employers' parsimony but the government's voracious tax appetite. If Messrs. Clinton and Kennedy are serious about easing the burden on American workers, they should be pushing for tax cuts.

According to the most recent data from the Department of Commerce's Bureau of Economic Analysis, total wages and salaries (for both government and private-sector workers) rose 6.7% in 1997, well ahead of the 1.7% inflation rate. Now for the bad news: The bureau reports that total federal, state and local tax payments by individuals rose by 11.4% in 1997. As the chart nearby shows, wage growth has lagged behind tax growth during every year of Mr. Clinton's term in office.

The bureau has been collecting such data since 1960, and the record is striking. Before 1993, taxes had not grown faster than wages since 1989, the first year of the Bush administration. The last time taxes outpaced wages two years in a row was 1980-81. On the other hand, on Jimmy Carter's watch, as on Mr. Clinton's, tax growth exceeded wage growth every single year. In fact, during the 17 years in which Democrats have occupied the White House (1961-68, 1977-80 and 1993-97), only once—in 1964—did wages grow faster than taxes.

This record of excessive tax growth shows every sign of continuing. Last year, federal tax revenue as a percentage of gross domestic product reached its highest level since World War II—19.8%. Total federal revenues will reach 19.9% of GDP this year and, If Mr. Clinton's budget is enacted, 20.1% next year—a post-World War II record. Add in state and local taxes, and the total approaches one-third of GDP.



These are not just abstract figures, as Senate Majority Leader Trent Lott pointed out in his response to the president's State of the Union address. They have a real impact on America's families. The typical American family's total tax burden is 38% of income. No wonder Americans feel financially squeezed.

But liberals like Messrs. Clinton and Kennedy are all too happy to raise taxes. Their solution to the financial pressure their policies have imposed on American families? More federal interference. In short, they seek to make Main Street pay Pennsylvania Avenue's bill. Do they really want Americans to prosper? If they do, they should start by cutting taxes.

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