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A Re-examination of Time Series Evidence of the Effect of the Minimum Wage on Youth Employment and Unemployment

Paul Wolfson

The most widely quoted range for the minimum wage elasticity of employment, based on time series analysis from the 1970s and early 1980s, is -0.1 to -0.3. This note re-examines the same teenage employment data, taking into account unit roots in the data and correcting more completely for the seasonal fluctuations. The result is a substantial reduction in the magnitude of both the point estimate and the t-statistic of this elasticity. The time series data provide no evidence that increases in the Federal minimum wage from 1954-1979 had a negative effect on teenage employment during this period.

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A Re-examination of Time Series Evidence of the Effect of the Minimum Wage on Youth Employment and Unemployment

> The most frequently studied group in the empirical literature is teenage. Time series studies typically find that a 10% increase in the minimum wage reduces teenage employment by one to three percent. This range includes estimates based on a wide spectrum of specifications and on different sample periods, but all used the same basic data source, the CPS. We believe that the lower half of that range is to be preferred; to the extent that differences in results can be attributed to differences in the specification chosen, the better choices seem to produce estimates at the lower end of the range. There may well be problems common to all the studies that lead to understating this impact, but that possibility remains to be shown. Cross-section studies of the effect on teenage employment produce a wider range of estimated impacts, which are roughly centered on the range found in the time-series research. (Brown, Gilroy and Kohen (1982), p. 524)

This statement defined what became the most widely held belief in recent years about the employment effect of the minimum wage. Using meta-analysis, a technique that is rare in economics, Card and Krueger (1995a, b) raise doubts about the reliability of the time-series studies behind this belief. Deere, Murphy and Welch (1996) are also skeptical, arguing that the response of employment to the minimum wage depends on the response of *employment costs* to the minimum wage, and this will vary for many reasons not captured in the specifications that are estimated. Two further reasons for doubting this conclusion exist, reasons that are due to problems with the statistical techniques on which they are based; these concern unit roots and the seasonal behavior of the data.

Solon (1985) considers virtually the same issues, serial correlation and seasonality. At that time, the problems related to unit roots were not yet particularly well known, so it is not surprising that he does not consider them. In addition, as we shall see, his correction for seasonality is initially promising but has some problems. Because Brown, Gilroy and Kohen (1983), designed their analysis to be comparable to earlier time-series studies and provide their data in an appendix to an earlier version (Brown, Gilroy and Kohen (1981), pp. 122-125), I will focus on them.

Unit Roots

Unlike most of their predecessors, Brown, Gilroy and Kohen (1983) recognize and address the serial correlation present in their data.[1] If the problem were only conventional serial correlation, the resulting parameter estimates would be inefficient, but only the statistics derived from the residuals would be inconsistent. The neglect of unit roots that are present makes the matter more serious, typically leading to inconsistent parameter estimates. This was not widely recognized at the time, the explosion of interest among economists in unit roots and cointegration for the most part coming after this research was completed. As elsewhere in economics, understanding of unit roots undermines much earlier work.

Which variables contain unit roots? Of the variables used in their basic specification, four, including the two most important ones, give strong evidence of being integrated. They are the teenage employment ratio, EM/P, and the Kaitz index, YK. The third other two variables are the unemployment rate for prime-age adult males, UPR and the ratio of enrollment in Federal training & employment programs of those age 16-21 to civilian population age 16-21, TR/P.[2] In fact, the low Durbin-Watson statistic that they interpreted as conventional serial correlation is now recognized as an indicator of unit roots in the dependent variable.

Variation in Seasonal Effects

The second issue is the treatment of seasonality. For their quarterly data, Brown, Gilroy and Kohen use seasonal dummies. Because the seasonals of EM/P vary over time, and the pattern of variation differs across seasonals, this use of dummies amounts to mis-specification. If sufficiently serious, this mis-specification may either mask the employment effect of the minimum wage or give rise to a spurious relationship as this effect is conflated with the true seasonals.

Table 1 provides some evidence on the behavior of seasonals in EM/P. In each regression (column), the dependent variable is the first difference of the logarithm of EM/P. The first column shows the results with conventional quarterly dummies. The important points of this regression are the very high R^2 and adjusted-R^2 values and the Durbin-Watson statistic. The seasonal dummies "explain" more than 95% of the variance in the log-difference of EM/P, and the p-value of the Durbin-Watson statistic is 0.33, so (first-order) serial correlation is not a concern.[3]

The second column includes separate linear trends for each quarter. The Durbin-Watson statistic drops considerably, but its p-value, 0.15, is still high enough for comfort. Although the overall time trend and the trend for the first quarter are each statistically insignificant, those for the second and third quarters are both statistically sigificant. Also worth noting is that these last two are negative while the first two are both positive. The R^2 and adjusted-R^2 statistics have both risen, to 0.9687 and 0.9661, respectively. Displaying them to the fourth digit suggests that the differences between regressions are necessarily small; in large part, because these R^2 statistics for the first regression are so near one, any improvements would have to be small. Seen from the other direction, however, we can say that the second regression leaves 26.5% less of the variation unexplained than does the first one with only seasonal dummies. An F statistic for the hypothesis that the three seasonal time trends are jointly equal to zero has a p-value of 0.00. According to this regression, the seasonals have declined in severity from the 1950s to 1970s, especially the negative winter and positive summer seasonals.

This treatment of the seasonals has some serious shortcomings. First, extrapolation suggests that eventually the seasonals will switch signs. Second, examinatifon of each seasonal separately indicates that a linear trend is appropriate only for the first quarter. Spring appears to have a concave quadratic trend similar to that of Summer but flatter, while the Fall trend is a convex quadratic: figure 1 displays the data for each season with fitted trends.[4]

Solon (1985) presents one solution to this, a specification that includes quadratic seasonal trends (column 3).[5] The R^2 and adjusted R^2 statistics are a great deal higher than those for the previous two regressions, reducing the unexplained variation by 48% compared to the equation with only seasonal dummies and by 29% relative to the one with seasonal trends. F-statistics examining the hypotheses that either the linear or the quadratic seasonal terms are jointly equal to zero each have p-values of zero. Test statistics (t-tests) for the differences between the second and third quarter linear terms, and between the second and third quarter quadratic terms both have p-values greater than 0.5, indicating no difference in how the seasonals have changed over time (the dummies for these two quarters are not the same).

A simpler specification, without the difficulties of the two specifications without quadratic seasonal terms, includes only the seasonal dummies and a seasonal lag, i.e., a fourth lagged term (last column).[6] Compared to the first regression, the unexplained variation is down by 28%, and despite the presence of fewer regressors it is down by 2% from the regression with linear seasonal time trends. While the adjusted-R^2 statistic and Schwarz's Bayes Information Criterion both indicate that this is better than either of the first two methods for treating seasonality, it is apparently dominated by the specification with quadratic seasonal terms.[7]

Evaluating Previous Research

Let us now turn to the equation that Brown, Gilroy and Kohen (1983) used to examine the minimum wage and employment. Table 2 displays results of each method for treating seasonality, both with and without the unit root correction: the BGK results are in the first row, along with some additional statistics.[8] The logarithmic version of their basic equation (with the variables defined below) is:

$$\mathrm{Log}(EM/P) = b_0 + b_1\mathrm{Log}(YK) + b_2\mathrm{Log}(UPR) + b_3\mathrm{Log}(POP) + b_4SY + b_5(AF/P) + b_6(TR/P) \qquad (1)$$

$$+ b_7Q_1 + b_8Q_2 + b_9Q_3 + b_{10}T + b_{11}T^2$$

EM/P: the ratio of civilian employment to civilian population for teenagers aged 16-19 years:

YK: the "Kaitz" index:

UPR: the ratio of civilian unemployment to civilian labor force, for males ages 25-54:

POP: the ratio of the teenage civilian population to the total civilian population:

SY: the fraction of those aged 16-19 who are aged 16-17:

AF/P: the ratio of teenagers who are in the armed forces to the total teenage population"

TR/P: the ratio of enrollments in federal training and employment programs for those aged 16-21, to those in the civilian population aged 16-21:

Q_j: dummies for quarter j, j = 1, 2, 3:

T: a linear time trend.

Due to the logarithmic specification of the equation, the minimum wage elasticity of employment corresponds to b_1 in the equation above.

The OLS DW statistic indicates serious problems of (first order) serial correlation. The statistic for the GLS estimate with the correction for serial correlation suggests that it has addressed the problem satisfactorily (notice that although we have "found" a unit root, the estimated serial correlation is only about 0.65). A regression of the residuals on a constant and four lags suggests otherwise.[9] An insignificant F-statistic from this regression would indicate that there is no serial correlation of less than fourth order. The F-statistic has a p-value of 0.003, due entirely to the fourth or seasonal lag; serial correlation remains a problem.

The second row presents results of an equation which adds the terms $b_{12}T_1 + b_{13}T_2 + b_{14}T_3$, where $T_j = T \times Q_j$, for j = 1, 2, and 3, to equation (1). The low value of the DW coefficient, 0.97, indicates that first order serial correlation remains a problem in the OLS estimates. The p-value of the F-statistic already mentioned rises to 0.09, so we could, if we chose, not reject the null of no low order serial correlation by setting our rejection criterion to 0.05. However from what we have seen about seasonality in EM/P, this is unlikely to be a good idea. It is interesting to note that the elasticity estimates are similar for all the specifications considered so far, roughly -0.1.

Row three, labeled Solon, presents results of estimating an equation which adds both linear and quadratic seasonal components to equation (1): $b_{12}T_1 + b_{13}T_2 + b_{14}T_3 + b_{14}T_1^2 + b_{15}T_2^2 + b_{14}T_3^2$. For the OLS estimates, the Durbin-Watson statistic is 1.65. While not as low as in the previous specification, it has a p-value of 0.01, indicating serial correlation. It appears that GLS takes care of the serial correlation, with a Durbin-Watson statistic of 2.05, but the F-statistic of the regression of residuals is highly significant, suggesting otherwise.[10]

The fourth row presents results of estimating

$$\mathrm{Log}(EM/P) = b_0 + b_1\mathrm{Log}(YK) + b_2\mathrm{Log}(UPR) + b_3\mathrm{Log}(POP) + b_4SY + b_5(AF/P) + b_6(TR/P) + b_7Q_1 + b_8Q_2 + b_9Q_3 + b_{18}\,\mathrm{Log}(EM/P_{-4}). \quad (2)$$

Because of the lagged dependent variable, the DW is inappropriate for examining serial correlation; the p-value of Durbin's alternative h statistic is instead shown. At a value of 0.00 for the OLS estimate, it strongly rejects the hypothesis of no serial correlation. The F-statistic for examining serial correlation in the GLS residuals has a very high p-value of 0.54, so it appears that this correction has solved the problem of serial correlation. There are two other problems however, no doubt related. The first, which is evident from the table, is that the point estimate of the elasticity is both small and not at all statistically significant; the t-statistic is 0.54. The second problem is that we know that unit roots are present in EM/P, YK and UPR, and their presence makes this regression suspect.

This brings us to equations with corrections for the unit roots. The first of these is a modification of Brown, Gilroy and Kohen's Basic specification, equation (1) above:

$$\Delta \log(EM/P) = b_0 + b_1 \Delta Log(YK) + b_2 \Delta Log(UPR) + b_3 Log(POP) + b_4 SY + b_5(AF/P) + b_6(TR/P) + b_7 Q_1 + b_8 Q_2 + b_9 Q_3 + b_{10} T + b_{11} T^2 \qquad (3)$$

where the Δ indicates a first difference. For this specification (row 5) and the two that allow for varying seaonals, T_3 (rows 6 & 7), the OLS DW statistics suggest problems with negative serial correlation, and the F-statistics on the regression of GLS residuals indicate the presence of serial correlation there.

The last row shows results from an equation with a unit root correction and the seasonal AR term, similar to equation (2) but with first differences included as in (3):

$$\Delta \log(EM/P) = b_0 + b_1 \Delta Log(YK) + b_2 \Delta Log(UPR) + b_3 Log(POP) + b_4 SY + b_5(AF/P) + b_6(TR/P) + b_7 Q_1 + b_8 Q_2 + b_9 Q_3 + b_{15} \Delta Log(EM/P_{-4}). \qquad (4)$$

The OLS and GLS estimates are virtually identical, not surprising since Durbin's alternative h statistic, with a p-value of 0.23, indicates no serial correlation, and the F-statistic from the GLS residuals' regression has an even larger p-value. The elasticity estimates are about -0.03 with t-statistics of -0.7. The point estimate is less than one-third of the one that Brown, Gilroy and Kohen (1983) report from their OLS estimates, and less than one-half the figure that they report based on the AR correction. This is a far cry from the -0.1 to -0.3 range that Brown, Gilroy and Kohen believed contained the elasticity. With a standard error of 0.046, the true value of the elasticity may lie in the lower end of that range, but the probability is considerably greater that it is out of the range altogether; in fact the probability that the true value is positive is greater than the probability that it lies in that range.

Unit roots and the problems they present were unknown at the time of this research. The result of not correcting for their presence in these data, along with the inadequate treatment of seasonality, is inconsistent estimates of the minimum wage elasticity of employment. Once these problems are corrected, the point estimates and t-statistics decline substantially in magnitude, and there is no longer any evidence in these data of a negative employment effect associated with increases in the minimum wage.

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Table 1
Results of Techniques for Removing Seasonality

Regressors	Quarterly Dummies	Quarterly Trends	Quarterly Trends à la Solon	4^{th} Lag of ΔLog(EM/P)
Constant	-0.20 (-17.7)	-0.22 (-16.6)	-0.16 (-10.6)	-0.08 (-4.26)
Q_1 1^{st} Quarter Dummy	0.10 (11.6)	0.08 (5.2)	0.02 (0.99)	0.04 (3.64)
Q_2 2^{nd} Quarter Dummy	0.32 (36.8)	0.36 (22.8)	0.28 (13.8)	0.13 (4.30)
Q_3 3^{rd} Quarter Dummy	0.34 (38.9)	0.40 (25.4)	0.31 (16.4)	0.14 (4.33)
T Linear Trend	0.00025 (0.58)	0.00064 (1.52)	-0.0024 (-3.61)	
T^2 Quadratic Trend	-0.0000013 (-0.33)	-0.0000018 (-0.50)	0.000026 (4.42)	
$T_1 = T \times Q_1$ 1^{st} Qtr Linear Trend		0.00037 (1.41)	0.0037 (4.30)	
$T_2 = T \times Q_2$ 2^{nd} Qtr Linear Trend		-0.00059 (-2.33)	0.0036 (4.08)	
$T_3 = T \times Q_3$ 3^{rd} Qtr Linear Trend		-0.00103 (-4.04)	0.0036 (4.32)	
$T_1^2 = T^2 \times Q_1$ 1^{st} Qtr Quadratic Trend			-0.000031 (-3.93)	
$T_2^2 = T^2 \times Q_2$ 2^{nd} Qtr Quadratic Trend			-0.000039 (-4.87)	
$T_3^2 = T^2 \times Q_3$ 3^{rd} Qtr Quadratic Trend			-0.000043 (-5.73)	
ΔLn(EM/P)$_{-4}$ Fourth Lag				0.58 (6.30)
# of Observations	103	103	103	98
Durbin-Watson	1.93	1.79	1.95	
R^2	0.9574	0.9687	0.9778	0.9693
Adjusted R^2	0.9553	0.9661	0.9751	0.9680
Schwarz's Bayes Information Criterion	-3.85	-4.03	-4.24	-4.16

Note: t-statistics in parentheses.

Table 2
Estimated Minimum Wage Elasticity of Employment

	OLS			GLS (AR1)			
Specification	Elasticity[1]	DW	DH alt[2] (p-value)	Elasticity[3]	DW	ρ	F[4] (p-value)
BGK Basic[5]	-0.106 (-3.25)	0.91		-0.089 (-1.92)	2.02	0.65 (8.6)	0.003
BGK + linear seasonal trends[6]	-0.107 (-3.52)	0.97		-0.105 (-2.45)	2.00	0.66 (8.8)	0.09
Solon:[7]	-0.101 (-3.03)	1.65		-0.103 (-2.68)	2.05	0.097 (1.77)	0.00
BGK + seasonal AR[8]	-0.015 (0.55)		0.00	0.02 (0.54)		0.73 (9.6)	0.54
Accounting for Unit Roots							
1st Difference	-0.055 (-1.03)	2.37		-0.055 (-1.13)	2.14	-0.20 (-2.08)	0.001
1st Difference + seasonal trends	-0.079 (-1.60)	2.22		-0.073 (-1.59)	2.12	-0.14 (-1.41)	0.02
Solon + 1st Difference	-0.040 (-0.49)	2.48		-0.057 (-0.81)	2.15	-0.33 (-3.52)	0.000
1st Difference +	-0.034		0.230	-0.033		-0.14	0.356

[1]The elasticity is the estimated coefficient on log(YK) in an OLS regression with log(EM/P) as the dependent variable: EM/P is the ratio of civilian employment to civilian population for teenagers aged 16-19 years. For equations labelled 1st Difference, the presence of unit roots is taken into account by substituting first differences of log(EM/P), log(YK), and log(UPR) for the corresponding undifferenced variables. The numbers in parentheses below the estimates are t-statistics.

[2]This is Durbin's alternative h statistic for examining serial correlation when one of the regressors is a lag of the dependent variable. It is normally distributed under the null hypothesis; the table displays p-values.

[3]This estimate is from a regression with a Cochrane-Orcutt correction for first order serial correlation.

[4]This is the regression F statistic from $Res = a_0 + a_1Res_{-1} + a_2Res_{-2} + a_3Res_{-3} + a_4Res_{-4}$, where Res are the GLS residuals. A statistically significant result indicates serial correlation in the residuals. The table displays p-values.

[5]The RHS variables are log(YK), log(UPR), log(POP), SY, AF/P, TR/P, dummies for the first 3 quarters, Q1-Q3, a linear time trend, T, and a quadratic time trend, T^2, where the variables are defined as:

YK: the "Kaitz" index:
UPR: the ratio of civilian unemployment to civilian labor force, for males ages 25-54:
POP: the ratio of the teenage civilian population to the total civilian population:
SY: the fratction of those aged 16-19 who are aged 16-17:
AF/P: the ratio of teenagers who are in the armed forces to the total teenage population:
TR/P: the ratio of enrollments in federal training and employment programs for those aged 16-21, to those in the civilian population aged 16-21.

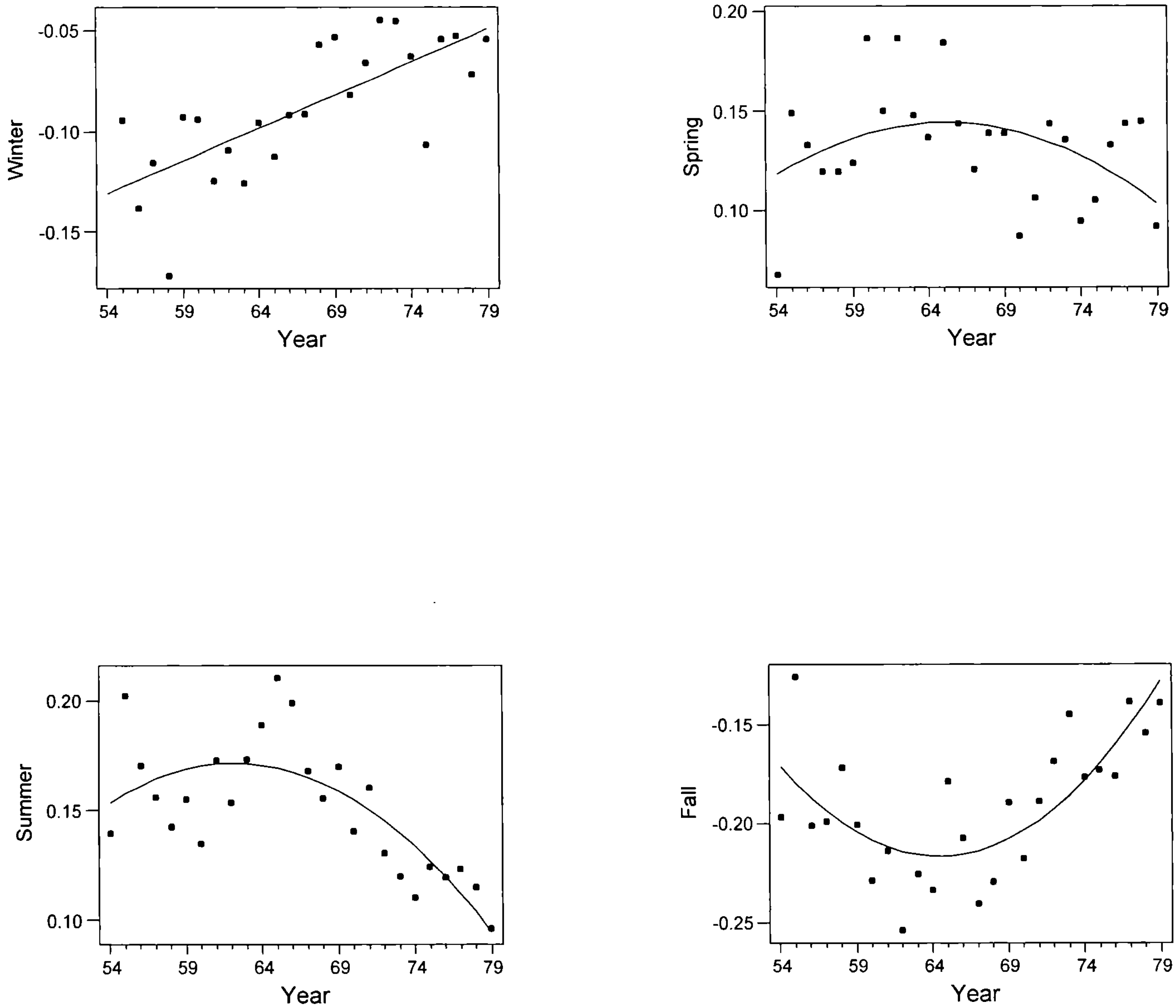
[6]This adds linear seasonal terms, $T_j = T \times Q_j$, j = 1, 2, 3, to the BGK Basic specification.

[7]This adds linear seasonal terms, $T_j = T \times Q_j$, j = 1, 2, 3, and quadratic seasonal terms, $T_j^2 = T^2 \times Q_j$, j = 1, 2, 3, to the BGK Basic specification

[8]This specification includes no time trends, linear or quadratic, seasonal or not, and includes instead a seasonal lag of the dependent variable: $\log(EM/P)_{-4}$ and $\Delta\log(EM/P)_{-4}$ as appropriate..

FIGURE 1

TRENDS IN THE SEASONALS OF ΔLOG(EM/P)



Notes

[1]See their footnote 11.

[2]The p-values for the Dickey-Fuller statistics are 0.20, 0.55, 0.36 and 0.98 for EM/P, YK, UPR, and TR/P. Thus, the test does not reject the null hypothesis of a unit root at conventional significance levels for any of these variables. The regressions on which these are based have seasonal dummies, and linear and quadratic seasonal trends. They have 6, 3, 3, and 10 lags of the dependent variable, respectively. When the unit root tests are based on logarithms of the first three variables, the p-values are 0.41, 0.46 and 0.45, respectively, with the same lag structure, and same set of seasonal variables. Cointegration tests do not indicate any cointegration among these variables.

[3]Removing the time trends reduces the R^2 by 0.0005.

[4]Furthermore, all these graphs suggest serial correlation around the fitted trend lines, making even this detrending technique problematic.

[5]This equation is estimated with a serial correlation correction. The DW statistic from OLS is 1.60, which has a p-value of 0.03. The R^2 and adjusted-R^2 statistics are for the untransformed data for comparability with the other regressions.

[6]This equation is also estimated with a serial correlation correction. The DW statistic is biased because of the lagged dependent variable, but the p-value of the Durbin h alternative from an OLS estimate, is 0.078. While this suggests that at a 5% level we need not concern ourselves with serial correlation, to be on the safe side, to be more confident in the validity of the hypothesis tests that follow, the results with the correction are reported. The R^2 and adjusted-R^2 statistics are for the untransformed data for comparability with the other regressions.

[7]Separate coefficients for each quarter on the seasonal lag are another possibility, and this specification does have the highest R^2 among these specifications: 0.9814. The Wald statistic that the terms on the seasonal lags are jointly equal to zero is asymptotically distributed $\chi^2(3)$ under the null, and has the value 2.8, and p-value of 0.42. Furthermore, the adjusted-R^2 (0.9679) is smaller and Schwarz's Bayes Information Criterion (-4.05) is larger for this specification than for the one with a single seasonal lagged term.

[8]These are from table 2, Brown, Gilroy and Kohen (1983), first without, then with, a Cochrane-Orcutt correction for serial correlation.

[9]This is the regression F-statistic from the regression $res = a_0 + a_1 res_{-1} + a_2 res_{-2} + a_3 res_{-3} + a_4 res_{-4}$, where res indicates the residuals from the GLS regression.

[10]For estimates with an AR1 correction for a linear, rather than log-linear specification, Solon reports several test results for serial correlation, all statistically insignificant at a 0.05 level: the Durbin-Watson test for first order autocorellation, the Wallis test for fourth-order autocorrelation and the Box-Pierce test for all eight auto-correlations. I estimated this model and could not repeat these results. The Durbin-Watson statistic at 2.01 is quite encouraging; but the auto-correlations at lags 2 and 4 are statistically significant at 0.262 (t-statistic: 2.6) and 0.395 (t-statistic: 2.7) respectively; and the Box-Ljung Q statistics for and 8 lags are 24.2 and 31.2, respectively, both statistically significant at virtually any level. For the GLS estimates of the log-linear specification, the Durbin-Watson is of course 2.05; the auto-correlations at lags 2 and 4 are 0.321 (t-statistic: 3.15) and 0.345 (t-statistic: 3.11) respectively; and the Box-Ljung Q-statistics at 4 and 8 lags are 24.3 and 30.4, respectively.

USING QUANTILE AVERAGES IN MATCHED OBSERVATIONAL STUDIES

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Summary

In two observational studies, one of the effects of minimum wage laws on employment and the other of the effects of lead exposures, an estimated treatment effect's sensitivity to hidden bias is examined. The estimate uses the combined quantile averages that were introduced in 1981 by B. M. Brown as simple, efficient, robust estimates of location admitting both exact and approximate confidence intervals and significance tests. Closely related to Gastwirth's estimate and Tukey's trimean, the combined quantile average has asymptotic efficiency for Normal data that is comparable to a 15% trimmed mean, higher efficiency than the trimean, but it has resistance or breakdown comparable to the trimean and better than the 15% trimmed mean. Combined quantile averages provide consistent estimates of an additive treatment effect in a matched randomized experiment. This paper discusses sensitivity analyses for combined quantile averages when used in a matched observational study in which treatments are not randomly assigned. In a sensitivity analysis in an observational study, subjects are assumed to differ with respect to an unobserved covariate that was not adequately controlled by the matching, so that treatments are assigned within pairs with probabilities that are unequal and unknown. The sensitivity analysis proposed here uses significance levels, point estimates and confidence intervals based on combined quantile averages, and examines how these inferences change under a range of assumptions about biases due to an unobserved covariate. The procedures are applied in the studies of minimum wage laws and lead exposures. The first example is also used to illustrate sensitivity analysis with an instrumental variable (IV).

Key words: Combined quantile average, Gastwirth's estimate, instrumental variable, matched pairs, nonrandomized experiment, permutation inference, randomization inference, observational study, robust estimate, sensitivity analysis, trimean.

1. TWO OBSERVATIONAL STUDIES WITH DIFFERING SENSITIVITY TO BIAS

1.1 *Introduction*

In an observational study of treatment effects, after adjusting for observed pretreatment differences, perhaps by matching on observed covariates, there is typically concern that treated and control groups may not have been comparable prior to treatment in ways that were not observed and recorded. The possible impact of such hidden biases on inferences about treatment effects is explored using sensitivity analysis; see §2.3.

In two examples with differing sensitivity to hidden bias, sensitivity analyses are developed and illustrated for a robust estimate of treatment effect introduced by B. M. Brown (1981). One example permits estimation by an instrumental variable in the sense of Angrist, Imbens and Rubin (1996) and, in this example, the sensitivity analysis for instrumental variable estimation discussed by Rosenbaum (1996,§4) is illustrated.

1.2 *Example 1: Minimum Wage and Employment in the Fast-Food Industry*

Card and Krueger (1994) examined changes in employment in four fast food chains in New Jersey before and after an increase in the minimum wage from $4.25 to $5.05 per hour. Stores in New Jersey were compared to similar stores in neighboring eastern Pennsylvania where the minimum wage had not been increased. While economic theory predicts a substantial decline in the use or consumption of labor when its price is substantially increased, Card and Krueger (1994,p772) found "no indication that the rise in the minimum wage reduced employment".

From the public data file of survey results in Card and Krueger (1995,p18), Table 1 describes 66 pairs of restaurants, one from New Jersey and one from Pennsylvania. The table contains full time equivalent employees (FTE) and starting wages before and after New Jersey's increase in the minimum wage. The stores were matched for restaurant chain and starting wages before the minimum wage increase. For instance, pair #56 describes two Wendy's restaurants, #226 and #59, both of which had a starting wage of $4.25 prior to New Jersey's increase in the minimum wage. In the New Jersey store, the starting wage rose to $5.30 while in the Pennsylvania store the starting wage remained at $4.25. Employment rose at the New Jersey store from 13 to 15 full time equivalent workers and it rose from 10.5 to 12 at the Pennsylvania store. Notice that the starting wage at most New Jersey stores rose substantially to equal exactly the new minimum wage.

Typically, survey data contains sporadic inaccuracies, and this may be true in Table 1. For instance, in pair 8, the claimed starting wage of $6.25 in the Pennsylvania store may be correct but is certainly an outlier. Similarly, in pair 29, the Pennsylvania Burger King reported a decline in employment from 70.5 to 29 full time equivalents, which again may be correct, but is a dramatic change. Suffice it to say that, in this context, one would prefer to use robust statistical methods that reflect stable trends found in most stores, that is, methods that are not strongly influenced by extreme results in one or two stores.

Figure 1 presents boxplots of the employment counts in New Jersey and Pennsylvania before and after New Jersey's increase in the minimum wage. Figure 2 presents five number summaries of employment and starting wages. If the 19% increase in the minimum wage had depressed employment at fast food restaurants in New Jersey, then the "NJ-After" boxplot would have been expected to be lower than the others, but instead the four boxplots appear similar. Again, there are several outliers.

The absence of a decline in employment following New Jersey's increase in the minimum wage was viewed skeptically by several authors (Deere, Murphy and Welch 1995, Sowell 1995). Typically sensitivity analyses ask about the magnitude of the biases that would need to be present to explain an ostensible treatment effect. In §4, a sensitivity analysis will ask about the magnitude of the hidden biases that would need to be present to explain the apparent absence of effect in Figures 1 and 2.

Card and Krueger's (1994) study is both interesting and careful in certain methodological respects, and also quite controversial in its economic conclusions. The current analysis is intended to illustrate statistical methods, and it makes no attempt to fully examine Card and Krueger's findings or the objections of their several critics.

1.3 *Example 2: Lead in Children of Workers in a Lead Related Industry*

Morton, et. al. (1982) paired 33 exposed children whose fathers worked in a battery plant where lead was used in the production of batteries to control children of the same age and neighborhood whose parents worked elsewhere. Concerned that exposed children might ingest lead brought home by fathers in their clothes and hair, Morton, et. al. obtained blood samples from the matched children, recording blood lead levels in μgrams of lead per decaliter of blood. The 33 matched pair differences are given in Table 2 and plotted in Figure 3. This example is discussed with using the Hodges-Lehmann estimate in Rosenbaum (1993,1995). Together, the two examples will illustrate different levels of sensitivity to hidden bias.

2. NOTATION AND REVIEW

2.1 *Notation: Matched Pairs with an Additive Treatment Effect*

In a matched observational study, subjects are paired on the basis of observed pretreatment variables or covariates, forming n pairs. One subject in each pair is assigned to treatment and the other to control. Within pair i, $i=1,\ldots,n$, write $Z_i=1$ if the first subject received the treatment and $Z_i=0$ otherwise. The model of an additive treatment effect asserts that the jth subject in pair i would exhibit response r_{ij} if assigned to control and response $r_{ij}+\tau$ if assigned to treatment. The task is to draw inferences about τ. The treated-minus-control difference in responses in pair i is then $D_i = (2Z_i-1)(r_{i1}-r_{i2}) + \tau$.

In a randomized experiment, treatments are assigned independently in distinct pairs and $\text{prob}(Z_i=1) = \frac{1}{2}$ for $i=1,\ldots,n$. In randomization inference, the r_{ij} are viewed as fixed features of the finite population of 2n subjects, and only quantities that depend on Z_i, such as D_i, are random variables (Fisher 1935). In this case, D_i takes values $(r_{i1}-r_{i2}) + \tau$ and $(r_{i2}-r_{i1}) + \tau$ each with probability $\frac{1}{2}$, so D_i is symmetrically distributed about τ and $E(D_i)=\tau$. Also, notice that $|D_i-\tau| =$

$|r_{i1}-r_{i2}|$ is a constant, not varying with Z_i. Write $D_{(1)} \le D_{(2)} \le \ldots \le D_{(n)}$ for the order statistics of the D_i.

2.2 *Quantile Averages in Randomized Experiments*

Several robust estimators are based on averages of a few order statistics, for instance, Mosteller's (1946) estimate, Gastwirth's (1966) estimate, and Tukey's (1977) trimean. For instance, the trimean is twice the median plus the quartiles divided by four. Brown (1981) proposed a class of closely related estimators with good efficiency and robustness properties, permitting exact and approximate inference linked to binomial distributions. Brown defines the class of estimators in several different ways, but the definition that is most useful for the current discussion is that the estimator of τ is the solution $\hat{\tau}$ to a certain equation, which will now be given. Brown assumes there are no ties, but the development that follows permits a substantial number of ties, as much as 1/3 of the pairs. For clarity, it is useful to distinguish the true τ, the estimate $\hat{\tau}$ of τ which solves the equation, a possibly incorrect but hypothesized value τ_0 of τ, and a free variable $\tilde{\tau}$ that appears in the defining equation.

If A is a finite set, write $|A|$ for the number of elements of A. If c is an integer, $0 \le c \le n$, define the set $A(c,\tilde{\tau})$ to be the subset of $\{1,2,\ldots,n\}$ containing the indices for the c largest values of $|D_i-\tilde{\tau}|$. For instance, the elements of $A(11,0)$ and $A(22,0)$ are identified by 1's in Table 2. More precisely, if there are ties among the $|D_i-\tilde{\tau}|$'s, then define $A(c,\tilde{\tau})$ to be the indices of c of the largest $|D_i-\tilde{\tau}|$ together with all indices of $|D_i-\tilde{\tau}|$ tied with some of these c largest $|D_i-\tilde{\tau}|$, so that $|A(c,\tilde{\tau})| \ge c$. Write $c(\tilde{\tau}) = |A(c,\tilde{\tau})|$. When ties are present, $c(\tilde{\tau})$ will vary slightly with $\tilde{\tau}$. Then $\hat{\tau}$ is defined to be the solution of the equation

$$0 = \frac{1}{a(\tilde{\tau}) + b(\tilde{\tau})} \left\{ \sum_{i \in A(a,\tilde{\tau})} \operatorname{sgn}(D_i - \tilde{\tau}) + \sum_{i \in A(b,\tilde{\tau})} \operatorname{sgn}(D_i - \tilde{\tau}) \right\} \tag{1}$$

where a<b, and

$$\mathrm{sgn}(x) = \begin{cases} 1 & \text{if } x>0 \\ 0 & \text{if } x=0 \\ -1 & \text{if } x<0 \end{cases}$$

Here, if $c=0$, then $A(c,\tilde{\tau})$ is empty, and a sum over $i \in A(c,\tilde{\tau})$ equals zero. As Brown observed, with $a=0$ and $b=n$, the solution $\hat{\tau}$ to (1) is the sample median, with $a=0$ and $b=2k-1$, the solution $\hat{\tau}$ to (1) is $\{D_{(k)}+D_{(n-k+1)}\}/2$, and, in general, the solution $\hat{\tau}$ to (1) is a close relative of estimates like Gastwirth's estimate and Tukey's trimean; see Brown (1981) for details. Let $T(\tilde{\tau})$ denote the right hand side of the equation (1). Brown (1981, Table 1) demonstrated that with $a=n/3$ and $b=2n/3$, the estimate $\hat{\tau}$ is both efficient and robust. Specifically, with $a=n/3$ and $b=2n/3$, Brown shows $\hat{\tau}$ has a high efficiency of 90% for Normal data, similar to a 15% trimmed mean, which is better than the trimean. Roughly speaking, the breakdown is the fraction of the distribution that may be moved to ∞ without moving the estimator to ∞. With $a=n/3$ and $b=2n/3$, Brown shows that $\hat{\tau}$ has a 25% breakdown point, comparable to the trimean and better than the 15% breakdown point of the 15% trimmed mean. See also Bickel and Lehmann (1975,§6) for a comparatively favorable evaluation of location measures based on quantile averages.

Very informally, one might say for motivation only that $T(\tilde{\tau})$ ignores $\mathrm{sgn}(D_i-\tilde{\tau})$ for the $n-b$ smallest $|D_i-\tilde{\tau}|$, counts once the middle $b-a$ of the $|D_i-\tilde{\tau}|$, and counts twice the a largest $|D_i-\tilde{\tau}|$. Equating $T(\tilde{\tau})$ to zero yields an estimate $\hat{\tau}$ that balances positive and negative $\mathrm{sgn}(D_i-\hat{\tau})$'s, giving somewhat greater weight to larger $|D_i-\hat{\tau}|$'s.

Using the following observation, Brown (1981,§5) obtains both exact and approximate confidence intervals and hypothesis tests for τ. Say that there are "few ties" if at least $b(\tau)$ pairs are not tied, that is if $|D_i-\tau| = |r_{i1}-r_{i2}| > 0$ for at least $b(\tau)$ pairs. For instance, with Brown's suggestion of $b=2n/3$, there are few ties if $|D_i-\tau|=0$ for less than 1/3 of the sample. When, in this sense, there are few ties, the ties will not affect the distribution of $T(\tau)$. Specifically, if there are few ties, then the tied observations do not contribute $\mathrm{sgn}(\cdot)$ terms to $T(\tau)$ in (1) so $T(\tau)$ is the mean of $a(\tau)+b(\tau)$ terms each of which is 1 or -1. In a randomized experiment, at the true τ, if $|D_i-\tau|>0$, then $\mathrm{sgn}(D_i-\tau)$ equals 1 or -1 each with probability 1/2. If there are few ties in a randomized experiment, then at the true value τ, the statistic $T(\tau)$ is

$$T(\tau) = \frac{4G + 2H}{a(\tau) + b(\tau)} - 1$$

where G and H are independent binomial random variables with sample sizes $a(\tau)$ and $b(\tau)-a(\tau)$ and probabilities of success $\frac{1}{2}$. This expression for $T(\tau)$ is obtained by algebraic rearrangement of (1), where a B=1 or B=0 Bernoulli trial becomes a 1 or -1 signed trial as $2B-1$. As $E\{T(\tau)\}=0$, because 4G has expectation $2\cdot a(\tau)$ and 2H has expectation $b(\tau)-a(\tau)$, it follows that the estimate $\hat{\tau}$ is found by equating $T(\tilde{\tau})$ to its expectation at the true τ and solving.

2.3 *Brief Review of Sensitivity Analysis in Observational Studies*

In an observational study, treatments are not assigned to subjects at random, and there may be little reason to believe that $\mathrm{prob}(Z_i=1)=\frac{1}{2}$ because matched subjects may differ with respect to unobserved covariates. A sensitivity analysis asks: How large must the departure from random assignment be before the qualitative conclusions of the study are altered? For instance, Cornfield, et. al. (1959,p193) found that, if an unobserved characteristic were to explain the observed association between smoking and lung cancer, the characteristic would have to be a near perfect predictor of lung cancer and 9 times more common among smokers than among nonsmokers — a high degree of insensitivity to hidden bias. Methods of sensitivity analysis are discussed by Bross (1966), Cornfield, et. al. (1959,p193), Gastwirth (1992), Greenhouse (1982), Rosenbaum and Rubin (1983), Rosenbaum (1987, 1993, 1995a,b), and Schlesselmann (1978).

One approach to sensitivity analysis proceeds as follows. Treatment assignments are initially assumed to be independent of one another and governed by unknown probabilities that are functions of observed covariates x and unobserved covariates u. It is then assumed that two subjects with the same observed covariates x will differ in their odds of receiving the treatment by at most a factor of $\Gamma \geq 1$ because they may differ in terms of the unobserved covariates u. For instance, if $\Gamma=2$, then two subjects who appear the same in terms of the observed covariate x may, in fact, differ in their odds of receiving the treatment to the extent that one is twice as likely as the other to receive the treatment. The value of Γ will be varied to examine the sensitivity of the inference to departures from random assignment of treatment. If treated and control subjects are matched using the observed covariates x alone, then matching eliminates the contribution to bias from observed covariates x, leaving a potential bias due from the unobserved covariates. Conditioning on the presence of one treated and one control subject within a matched pair yields:

$$\frac{1}{\Gamma} \leq \frac{\text{prob}(Z_i=1)}{1-\text{prob}(Z_i=1)} \leq \Gamma \quad \text{for } i=1,\ldots,n; \tag{2}$$

see Rosenbaum (1987,1995b,§4.2) for details. The model (2) says that paired subjects may differ in their probabilities of exposure to the treatment by at most a factor of $\Gamma \geq 1$. Write $p_i = \text{prob}(Z_i=1)$ and $\mathbf{p} = (p_1,\ldots,p_n)^T$, so (2) is the same as saying that $\mathbf{p}$ lies in the n–dimensional cube $\left[\frac{1}{1+\Gamma}, \frac{\Gamma}{1+\Gamma}\right]^n$. For $\Gamma=1$, the cube consists of the single point $\left(\frac{1}{2},\ldots,\frac{1}{2}\right)^T$ for a randomized experiment, but as Γ increases, the treatment assignment probabilities $\mathbf{p}$ may be progressively further from a randomized experiment. As Γ increases, there is a range of inferences, i.e., a range of point estimates, confidence intervals, significance levels, that reflect uncertainty about $\mathbf{p}$. A sensitivity analysis computes this range of inferences for several values of Γ, thereby showing how conclusions might change with increasing departures from a randomized experiment. An observational study is sensitive to hidden bias if small departures from random assignment could alter the study's conclusions, and it is insensitive if large departures would be required to alter the conclusions. Several examples are discussed in Rosenbaum (1995b,§4) showing that observational studies vary markedly in their degree of sensitivity to bias. The current paper proposes sensitivity analysis for combined quantile averages. Specifically, subject to (2), bounds are obtained on the distribution of $T(\tau)$, and hence also bounds on point estimates, significance levels, and confidence intervals based on $T(\tau)$. Related work about individual quantiles and Hodges-Lehmann estimates is discussed in Rosenbaum (1995a,1993).

3. SENSITIVITY ANALYSIS FOR QUANTILE AVERAGES

3.1 *Some Mechanical Preliminaries*

In a randomized experiment the treatment assignment probabilities are constant, $\text{prob}(Z_i=1) = \frac{1}{2}$ for all i, and this yields certain simplifications that no longer apply when $\text{prob}(Z_i=1)$ may vary with i. This section makes very slight changes in the mechanical details of the method of §2.2 so that generalization is possible.

The following argument shows that $T(\tilde{\tau})$ is monotone decreasing in $\tilde{\tau}$. As $\tilde{\tau}$ increases, each individual $\text{sgn}(D_i-\tilde{\tau})$ is monotone decreasing. Also, as $\tilde{\tau}$ increases, the set $A(c,\tilde{\tau})$ changes, but in a predictable way. If $\text{sgn}(D_i-\tilde{\tau})=1$, then $|D_i-\tilde{\tau}| = D_i-\tilde{\tau}$ decreases as $\tilde{\tau}$ increases until $\text{sgn}(D_i-\tilde{\tau})$ is no longer 1, so that while $\text{sgn}(D_i-\tilde{\tau})=1$, index i may leave $A(c,\tilde{\tau})$ but will not enter $A(c,\tilde{\tau})$. Conversely, if $\text{sgn}(D_i-\tilde{\tau})=-1$, then $|D_i-\tilde{\tau}| = \tilde{\tau}-D_i$ increases as $\tilde{\tau}$ increases, so i may enter $A(c,\tilde{\tau})$ but will not leave it. In short, as $\tilde{\tau}$ increases, the individual $\text{sgn}(D_i-\tilde{\tau})$'s are all monotone decreasing, and the sets, $A(a,\tilde{\tau})$ and $A(b,\tilde{\tau})$, tend more and more to select negative $\text{sgn}(D_i-\tilde{\tau})$'s for inclusion in the averages in (1); hence $T(\tilde{\tau})$ is a monotone decreasing function of $\tilde{\tau}$. As a result, if κ is a known constant, the solution to the equation $\kappa=T(\tilde{\tau})$ may be found by a one-dimensional line search, although an explicit solution of $0=T(\tilde{\tau})$ is also available for randomized experiments without ties (Brown 1981,§4).

In general, there may be either an interval of solutions to $\kappa=T(\tilde{\tau})$ or no exact solution. In both cases, the "solution" of $\kappa=T(\tilde{\tau})$ is defined as the average of the $\inf\{\tilde{\tau}:T(\tilde{\tau})\leq\kappa\}$ and $\sup\{\tilde{\tau}:T(\tilde{\tau})\geq\kappa\}$, whereupon the estimate is well defined.

3.2 *The Behavior of $T(\tau)$ in an Observational Study*

In an observational study, the treatment assignment probabilities, $\mathbf{p}$, are unknown. This section determines the behavior of $T(\tau)$ at the true τ under the model (2) for $\mathbf{p}$.

At the true value of τ, $D_i-\tau = (2Z_i-1)(r_{i1}-r_{i2})$, from which it follows that $|D_i-\tau| = |r_{i1}-r_{i2}|$ is fixed, not varying with Z_i, so $A(a,\tau)$ and $A(b,\tau)$ are fixed sets of indices not varying with Z_i. If $|D_i-\tau|=0$, then $\text{sgn}(D_i-\tau)=0$ with certainty, and otherwise $\text{sgn}(D_i-\tau)$ is 1 or -1. Specifically, $\text{prob}\{\text{sign}(D_i-\tau)=1\} = p_i$ if $r_{i1}-r_{i2}>0$, $\text{prob}\{\text{sign}(D_i-\tau)=0\} = 1$ if $r_{i1}-r_{i2}=0$, and $\text{prob}\{\text{sign}(D_i-\tau)=1\} = 1-p_i$ if $r_{i1}-r_{i2}<0$. For any specified set of treatment assignment probabilities, $\mathbf{p}$, the distribution of $T(\tau)$ is the distribution of fixed linear function of independent Bernoulli trials governed by these p_i, so $E\{T(\tau)\}$ and $\text{prob}\{T(\tau)\geq k\}$ could be determined in a straightforward way yielding point estimates, significance levels and confidence intervals for τ. The difficulty is that in an observational study, $\mathbf{p}$ is unknown, so $E\{T(\tau)\}$ and $\text{prob}\{T(\tau)\geq k\}$ cannot be calculated.

Under the model (2), the treatment assignment probabilities are bounded by $1/(1+\Gamma) \leq p_i \leq \Gamma/(1+\Gamma)$. If $|D_i-\tau|=0$, then $\text{prob}\{\text{sgn}(D_i-\tau)=0\}$ no matter what value is taken by p_i. If $|D_i-\tau|\neq 0$, then $\text{sgn}(D_i-\tau)$ is determined by Z_i, so that $1/(1+\Gamma) \leq \text{prob}\{\text{sgn}(D_i-\tau)=1\} \leq \Gamma/(1+\Gamma)$ and $1/(1+\Gamma) \leq \text{prob}\{\text{sgn}(D_i-\tau)=-1\} \leq \Gamma/(1+\Gamma)$.

Although $\mathbf{p}$ is unknown, the distribution of $T(\tau)$ is bounded by the distributions of two known random variables. Define

$$T_+ = \frac{4G_+ + 2H_+}{a(\tau) + b(\tau)} - 1$$

where G_+ and H_+ are independent binomial random variables with sample sizes $a(\tau)$ and $b(\tau)-a(\tau)$ and probabilities of success $\Gamma/(1+\Gamma)$, and define T_- in the same way with probability $1/(1+\Gamma)$ in place of $\Gamma/(1+\Gamma)$. Then for every k, $\text{prob}(T_- \geq k) \leq \text{prob}\{T(\tau) \geq k\} \leq \text{prob}(T_+ \geq k)$. In other words, the tail area of $T(\tau)$ is unknown because treatments were not randomly assigned, but for each fixed $\Gamma \geq 1$, the tail area may be bounded by two known distributions derived using binomial probabilities. It follows that:

$$E(T_+) = \frac{\Gamma-1}{1+\Gamma}, \quad E(T_-) = \frac{1-\Gamma}{1+\Gamma}, \quad var(T_+) = var(T_-) = \frac{\Gamma\{12a(\tau)+4b(\tau)\}}{\left[(1+\Gamma)\cdot\{a(\tau)+b(\tau)\}\right]^2}. \tag{3}$$

Note that $var(T_+)$ and $var(T_-)$ may change slightly with τ since $a(\tau)$ and $b(\tau)$ may change slightly with τ; however, the notation does not explicitly indicate this.

3.3 *Inference About τ in an Observational Study: Sensitivity Analysis*

The bound, $prob(T_- \geq k) \leq prob\{T(\tau) \geq k\} \leq prob(T_+ \geq k)$, which depends on Γ in (2), forms the basis for inference about τ including estimates, tests, and confidence intervals. These inferences are computed for several values of Γ thereby displaying the sensitivity of the study's conclusions to departures of various magnitudes from random assignment of treatments.

For fixed Γ, the bounds, $prob(T_- \geq k)$ and $prob(T_+ \geq k)$, may be calculated exactly using nonstandard but straightforward manipulations involving a weighted sum of binomials, or, more easily they may be approximated using the moments (3) and the Normal distribution. This yields bounds on significance levels for testing hypotheses about τ. Specifically, to test $H_0{:}\tau=\tau_0$, one calculates $\{T(\tau_0)-E(T_+)\}/\sqrt{var\{T_+\}}$ and $\{T(\tau_0)-E(T_-)\}/\sqrt{var\{T_-\}}$, and refers these deviates to the standard Normal distribution to obtain the approximate upper and lower bounds on the tail area of $T(\tau_0)$ under the hypothesis $H_0{:}\tau=\tau_0$. For $\Gamma=1$, the two bounds are equal to each other and also equal to the significance level for a randomized experiment as discussed in Brown (1981).

For each fixed Γ, as is usual, confidence intervals are obtaining by inverting the test. There is an approximate $1-\alpha$ confidence interval for each of the possible value of **p**; the shortest interval containing all of these $1-\alpha$ confidence intervals is the sensitivity interval:

$$\left[\inf\left\{\tilde{\tau}: \frac{T(\tilde{\tau}) - E(T_+)}{\sqrt{var(T_+)}} \leq z_{\alpha/2}\right\}, \sup\left\{\tilde{\tau}: \frac{T(\tilde{\tau}) - E(T_-)}{\sqrt{var(T_-)}} \geq -z_{\alpha/2}\right\}\right], \tag{4}$$

where $z_{\alpha/2}$ is the upper $\alpha/2$ percentile of the standard Normal distribution. This interval starts at the randomization interval when $\Gamma=1$, but it becomes progressively longer as Γ increases.

The range of point estimates of τ is obtained as follows. For each **p** there is an expectation for $T(\tau)$ and an estimate of τ obtained by setting $T(\tilde{\tau})$ equal to its expectation and solving. The maximum and minimum expectations are given in (3), and since $T(\cdot)$ is monotone decreasing, the minimum and maximum estimates of τ for **p** subject to (2) are obtained by solving $T(\tilde{\tau}) = E(T_+)$ and $T(\tilde{\tau}) = E(T_-)$. Again, this yields the randomization estimate when $\Gamma=1$ and an increasingly long range of estimates as Γ increases.

4. SENSITIVITY ANALYSIS IN THE EXAMPLES

4.1 *Lead in Children Whose Fathers Work in a Lead Related Industry*

For the matched pair differences in lead levels in Table 2, the median $D_{(17)}$ of the 33 is 15 μg/dl. The median 15 is the unique solution to (1) when a=0 and b=33, and for these values of a and b, inferences based on T are essentially the same as inferences based on the sign test. The quartiles of the 33 differences are the $k=9^{th}$ observation from the top and bottom, $D_{(9)} = 4$ and $D_{(25)} = 25$, with average $\{D_{(9)}+D_{(25)}\}/2 = (4+25)/2 = 14.5$. This quantile average is the unique solution to (1) with a=0 and b = 2k−1 = 2x9−1 = 17. Using $a \doteq n/3$ and $b \doteq 2n/3$ or a = 11 and b=22, as suggested by Brown (1981) based on robustness and efficiency calculations, $T(\tilde{\tau})$ is zero from $\tilde{\tau}=15$ to $\tilde{\tau}=15.5$, so $\hat{\tau} = 15.25$ μg/dl.

Table 3 displays the sensitivity analysis for point and interval estimates of τ in the lead exposure data. With $\Gamma=1$, the two subjects in a matched pair do not differ in their chances of receiving the treatment, yielding randomization inferences. In particular, the point estimate of 15.25 μg/dl and the 95% confidence interval of [9, 19.5] are those obtained by Brown's method. With $\Gamma=2$, two children matched for age and neighborhood may differ in their odds of exposure by a factor of 2 because they differ in terms of an unobserved covariate not controlled by the matching. In this case, there is a range of point estimates, from 10.75 to 19.5 μg/dl, reflecting uncertainty about the impact of this unobserved covariate, and a longer 95% confidence interval, [7, 26]. For Γ=4, a large hidden bias associated with a four fold increase in the odds of exposure, the range of point estimates is 8 to 24 μg/dl and the confidence [−1,41] barely includes the null hypothesis of no treatment effect. This example is insensitive to moderately large biases, in agreement with the analysis in Rosenbaum (1993) using the Hodges-Lehmann estimate.

4.2 *Minimum Wage Increases: The Model of a Constant Multiplicative Effect*

Table 4 shows a parallel sensitivity analysis for the minimum wage data. If increasing the minimum wage decreased employment, a multiplicative effect would be more plausible than an additive effect, in the sense that a two-employee reduction means more in a small store than in a large one. Since there is little indication of an effect, there is no empirical basis for selecting either an additive or a multiplicative effect, but a multiplicative effect is more logical and more consistent with tradition in economic models. For this reason, the analysis is applied changes on the log scale, that is to the matched pair difference in pair i of changes in employment counts after taking natural logs, D_i = {log(NJ:After)−log(NJ:Before)}−{log(PA:After)−log(PA:Before)}. As seen in Table 4, in the absence of hidden bias, Γ=1, the quantile average estimate of τ is .0375. Since exp(x) is approximately 1+x for x near zero, this value is approximately an estimated 3.75% greater *increase* in employment in the New Jersey store than in the matched Pennsylvania store, where exp(.0375)−1 = 3.82% which is close to 3.75%. In the absence of hidden bias, consistent with Card and Krueger's (1994) findings, rather than a substantial decrease in employment, the point estimate suggests a slight increase, and the confidence interval suggests that a small decrease in employment or a large increase in employment are both plausible.

In Table 4, the sensitivity analyses for point estimates and confidence intervals both indicate greater sensitivity to bias here than in the lead exposure data in Table 3. A point estimate of a substantial decline in employment is reached only for Γ=3, a moderately large bias. On the other hand, the lower endpoint of the confidence interval is not inconsistent with a substantial decline in employment for Γ=1.5, a much smaller bias. It is, then, fair to say, as Card and

Krueger said, that the data provide "no indication that the rise in the minimum wage reduced employment," but also fair to say that someone who strongly expected a substantial decline in employment is not forced by the data to abandon that expectation if a rather moderate bias of $\Gamma=1.5$ is deemed plausible. Still, as far as the data alone indicate, such a bias, operating in the opposite direction, is also not inconsistent with a substantial increase in employment. In contrast, in the lead exposure data, the higher levels of lead in the blood of children of lead workers can only be explained away by biases that are much larger.

(In Table 4, certain numbers appear several times because $T(\tilde{\tau})$ is a step function, and the location of its steps determine the values of estimates and the endpoints of confidence intervals.)

5 SENSITIVITY ANALYSIS WITH AN INSTRUMENTAL VARIABLE

5.1 *Legislation as an Instrument Acting Through Wage Changes in the Minimum Wage Study*

Arguably, the legislation increasing New Jersey's minimum wage has an economic effect only to the extent that it changes wages paid to employees. For instance, in pair 26 in Table 1, the New Jersey Burger King restaurant reported paying $5.00 per hour as a starting wage prior to the increase in the minimum wage and the required $5.05 after the increase, so the starting wage changed only slightly in this store, and one might therefore expect a negligible impact on employment in this store. On the other hand, many New Jersey stores increased the starting wage from the old minimum of $4.25 to the new minimum of $5.05, a 19% increase, and a large impact might be expected in such a store.

In other words, both the employment count and the starting wage actually paid to employees are outcomes possibly affected by the treatment, that is, by the law increasing New Jersey's minimum wage. However, one might wish to consider a model for the effect of the treatment on employment that says the effect of the minimum wage increase is not a constant multiplier, but rather is proportional to the impact of the change in starting wages, and that the legislation has no impact beyond the impact on actual wages. This is close to the model used by Angrist, Imbens and Rubin (1996) in their reformulation of the traditional instrumental variables

(IV) model. This section illustrates the sensitivity analysis with an instrumental variable proposed in Rosenbaum (1996), using combined quantile averages as the estimator. A robust estimator is needed here because of the outliers in the wage changes noted in §1.2.

5.2 *Instrumental Variables in Randomization Inference and Sensitivity Analysis*

In general, the jth unit in pair i has two potential values, w_{Tij} and w_{Cij}, of a secondary outcome, where w_{Tij} is observed if this unit receives the treatment and w_{Cij} is observed if this unit receives the control (Rubin 1974). Recall that, in pair i, $Z_i=1$ indicates that unit j=1 received the treatment and unit j=2 received the control, and $Z_i=0$ indicates the opposite assignment of treatments. Let W_{ij} be the observed value of this secondary outcome, so $W_{i1}=w_{Ti1}$ if $Z_i=1$, $W_{i1}=w_{Ci1}$ if $Z_i=0$, $W_{i2}=w_{Ti2}$ if $Z_i=0$, and $W_{i2}=w_{Ci2}$ if $Z_i=1$. The IV model used here says that the primary outcome for the jth unit in pair i is observed to be $r_{ij} + \beta W_{ij}$. If $W_{ij} = Z_i$, then this becomes the model of an additive treatment effect in §2.1. The effect of the treatment on the jth unit in pair i is the difference between the responses this unit would exhibit under treatment, and control, namely $(r_{ij}+\beta w_{Tij})-(r_{ij}+\beta w_{Cij}) = \beta(w_{Tij}-w_{Cij})$. This model asserts that the primary outcome is impacted by the treatment exclusively through the impact of the treatment on the secondary outcome, an assertion that Angrist, Imbens and Rubin (1996,p447) call the exclusion restriction. Unlike Angrist, Imbens and Rubin, it will not be assumed that the treatment Z_i is randomly assigned, and instead a sensitivity analysis will be conducted.

In pair i, write V_i for the observed treated-minus-control difference in values of the secondary response, $V_i = Z_i(w_{Ti1}-w_{Ci2})+(1-Z_i)(w_{Ti2}-w_{Ci1}) = (2Z_i-1)(W_{i1}-W_{i2})$. In matched pair i, the treated-minus-control difference in responses is $D_i = (2Z_i-1)(r_{i1}-r_{i2}) + \beta V_i$.

The hypothesis $H_0{:}\beta=\beta_0$ is tested by calculating the observed quantity $D_i - \beta_0 V_i$, which equals $(2Z_i-1)(r_{i1}-r_{i2})$ under the null hypothesis, and using $D_i - \beta_0 V_i$ in place of $D_i-\tau_0$ in quantile average statistic on the right hand side of (1). Write $\tilde{T}^*(\beta)$ for this statistic when evaluated at $D_i - \tilde{\beta} V_i$. If treatments were randomly assigned, then as before the randomization distribution of $(2Z_i-1)(r_{i1}-r_{i2})$ creates the null distribution of $T^*(\beta_0)$. The test is inverted to obtain a confidence set for β, namely the set of β_0's not rejected by the test. The sensitivity analysis also proceeds as before, with the sensitivity model placing bounds on the distribution of $(2Z_i-1)(r_{i1}-r_{i2})$, and hence bounds on the null distribution of $T^*(\beta_0)$.

When IV inference is based on means or other least squares calculations, various assumptions are needed to ensure the existence of the estimate, even asymptotically; see assumptions 4 and 5 and proposition 1 in Angrist, Imbens and Rubin (1996,p447). In many ways, the situation is simpler both theoretically and practically for permutation tests and confidence sets. The tests and confidence sets are valid without additional assumptions beyond the model for treatment assignment and the model above for the form of the treatment effect. However, both tests and confidence sets may exhibit recognizably peculiar behavior in circumstances similar to those which lead to instability or nonexistence of the estimate based on means. Specifically, in both randomization inference and sensitivity analysis with an instrumental variable, the confidence set for β, though valid as a set estimate, need not be an interval, and if it is an interval, it may be empty or infinite in length. As an unlikely but clear example, if $w_{Ti1}=w_{Ci2}$ and $w_{Ti2}=w_{Ci1}$ for each i, then $V_i=0$ for each i, so changes in $\tilde{\beta}$ produce no change in the value of $T^*(\tilde{\beta})$, and the confidence set is either the entire real line or the empty set. An empty confidence set indicates that $T^*(\tilde{\beta})$ is in the tail of its null distribution for all values of $\tilde{\beta}$ so that the adjusted differences $D_i - \tilde{\beta} V_i$ do not appear to satisfy the null hypothesis of no effect for any value of $\tilde{\beta}$. Of course, such an empty or infinite confidence set suggests that the the treatment effect is not appropriately modelled as a function of V_i. A sufficient condition for the set estimate to be an interval is that $V_i>0$ for all i, which says that in every pair the treated unit is observed to have a higher secondary outcome than the control unit. In this case, the argument at the beginning of §3.1 continues to apply, so $T^*(\cdot)$ is monotone decreasing, and the confidence set is an interval. When some V_i are positive and others are negative, increasing $\tilde{\beta}$ causes some $D_i - \tilde{\beta} V_i$ to increase and others to decrease, so even though the quantile average statistic is a monotone increasing function of the n–dimensional vector of adjusted differences $(D_1-\tilde{\beta}V_1,\ldots, D_n-\tilde{\beta}V_n)$, the differences themselves are not monotone decreasing in $\tilde{\beta}$, and therefore $T^*(\tilde{\beta})$ is not guaranteed to be monotone decreasing. As a practical matter, $T^*(\tilde{\beta})$ may be plotted as a function of $\tilde{\beta}$, and the confidence interval may be taken to be the shortest interval that includes the entire confidence set. Empty, infinite or extremely long confidence intervals suggest reconsideration of the model which asserts that treatment effects are proportional to V_i.

5.3 *Instrumental Variable Estimate in the Minimum Wage Example*

In the minimum wage example, define the secondary response to be the change in the log of starting wages, so V_i is the difference of these changes for the New Jersey and Pennsylvania restaurant in pair i. For instance, from Table 1, $V_1 = \{\log(5.05)-\log(4.25)\}-\{\log(4.40)-\log(4.25)\} = .138$, so in this pair of stores, starting wages rose about 14.8% = exp(.138)–1 faster in the New Jersey than in the Pennsylvania store. All but three of the V_i's are positive, so in 63 of the 66 pairs, there was a greater increase in starting wages at the New Jersey store. Since employment and wage changes are both measured on the log scale, β is an elasticity.

In the absence of bias, $\Gamma=1$, Figure 4 plots the standardized deviate based on $T^*(\tilde{\beta})$ against $\tilde{\beta}$ for the minimum wage data. Notice that the curve tends to decline with $\tilde{\beta}$ but is not quite monotone decreasing, the slight steps upwards being due to the three stores with positive V_i's. Horizontal lines at 0 and ±1.96 also appear in the graph. The curve crosses the horizontal zero line at $\hat{\beta}=.602$, which is the point estimate, and it crosses 1.96 at –.513 and –1.96 at 1.926 so the approximate 95% confidence interval for β is [–.513,1.926] when $\Gamma=1$. In the absence of hidden bias, the positive point estimate would suggest increasing the minimum wage increases rather than decreases employment, but the confidence interval includes both positive and negative elasticities.

The sensitivity analysis proceeds in the same way except that the moments in (3) for the bounds on the null distribution are used. The results appear in Table 5. As in Table 4, the unexpected positive point estimates are found for $\Gamma=1$ and $\Gamma=1.5$, but the confidence interval suggests positive and negative elasticities are both plausible. In this particular instance, similar conclusions are reached whether the effect of the minimum wage is modelled as a single multiplicative effect or as an effect that varies with the actual change in starting wages.

5. CONCLUSION

The assumptions upon which an inference depends are of two kinds. First, there are implementation assumptions, such as an assumption of Normal errors, which aid in deriving or evaluating a statistical method, but which are not typically of scientific interest. Inferences are sought that are robust to implementation assumptions. Second, there are pivotal assumptions, such as an assumed absence of hidden bias in an observational study, which are unavoidably central to the scientific interpretation of the study. Pivotal assumptions are usefully examined using sensitivity analysis.

Brown's (1981) combined quantile averages possess ease of calculation, high efficiency for Normal data, high breakdown in the face of extreme observations, and exact and approximate significance levels and confidence intervals linked to the binomial distribution. This paper proposed sensitivity analyses for combined quantile averages when used in observational studies, including sensitivity analysis for the estimate, and for the associated significance level and confidence interval. The use of the estimate with an instrumental variable was also illustrated.

The two examples illustrate several points. The studies differ in their sensitivity to hidden bias, the lead exposure example being quite insensitive to small and moderate biases, and the minimum wage study being fairly sensitive to moderate biases. Usually, a sensitivity analysis asks about the magnitude of the biases that would need to present to explain an ostensible treatment effect, but the minimum wage study posed the opposite question. Economic theory anticipated a large negative impact on employment from an increase in the minimum wage, but the data gave no indication of this. The sensitivity analysis asked, in part, about the magnitude of the bias that would need to be present for this absence of apparent effect to be consistent with an economic theory anticipating a large negative effect. In principle, one might anticipate that wage legislation affects employment through changes in actual wages, and therefore one might anticipate better estimates from an instrumental variable (IV) model using wages as the predictor and legislation as the instrument. In this example, however, the IV estimate was slightly more sensitive to bias than the conventional estimate using legislation alone.

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Table 1: Matched pairs of New Jersey (NJ) and Pennsylvania (PA) stores, before (B) and after (A) New Jersey's increase in the minimum wage

	Chain	FTE employment				Starting wages				Store ID's	
		NJ-B	NJ-A	PA-B	PA-A	NJ-B	NJ-A	PA-B	PA-A	NJ	PA
1	BK	15.50	26.50	15.50	14.00	4.25	5.05	4.25	4.40	230	521
2	BK	18.00	32.00	18.00	19.00	4.25	5.05	4.25	4.25	258	45
3	BK	19.00	20.00	19.00	25.00	4.25	5.25	4.25	4.38	168	48
4	BK	19.50	47.50	19.50	27.00	4.25	5.05	4.25	4.25	267	41
5	BK	23.00	19.00	22.75	21.00	4.25	5.05	4.25	4.25	91	435
6	BK	24.00	26.50	24.00	23.00	4.25	5.05	4.25	4.75	105	476
7	BK	24.00	25.50	24.50	43.50	4.25	5.05	4.25	4.75	340	501
8	BK	26.00	25.00	26.00	20.00	4.25	5.05	4.25	6.25	385	477
9	BK	27.00	35.00	27.00	25.50	4.25	5.05	4.25	4.25	66	40
10	BK	30.00	26.00	32.50	25.50	4.25	5.05	4.25	4.25	38	37
11	BK	33.00	29.00	36.50	27.00	4.25	5.05	4.25	4.25	200	430
12	BK	38.00	24.50	39.00	26.50	4.25	5.05	4.25	4.25	68	471
13	BK	41.00	22.50	41.50	23.50	4.25	5.05	4.25	4.90	202	472
14	BK	41.50	21.50	45.00	37.50	4.25	5.05	4.25	4.25	418	434
15	BK	13.00	19.00	21.00	17.50	4.37	5.05	4.35	4.25	249	522
16	BK	24.00	32.00	24.00	16.00	4.50	5.05	4.50	4.25	84	42
17	BK	30.50	22.50	32.00	21.00	4.50	5.05	4.50	4.50	64	475
18	BK	50.00	30.00	48.50	27.00	4.50	5.05	4.50	4.35	70	478
19	BK	18.50	24.75	52.50	34.00	4.55	5.05	4.50	5.05	213	432
20	BK	16.00	15.50	15.50	34.50	4.75	5.25	4.75	4.75	2	450
21	BK	17.00	21.50	18.50	41.25	4.75	5.05	4.75	4.50	172	503
22	BK	20.50	39.00	21.00	24.50	4.75	5.05	4.75	4.50	71	448
23	BK	22.50	44.00	22.50	35.50	4.75	5.25	4.75	4.50	114	473
24	BK	18.50	13.00	15.50	27.50	4.87	5.05	4.87	5.00	156	449
25	BK	16.00	24.50	13.50	18.00	5.00	5.50	5.00	4.50	89	474
26	BK	23.00	23.25	23.50	36.50	5.00	5.05	5.00	5.00	268	451
27	BK	27.50	27.00	26.50	30.50	5.00	5.25	5.00	4.50	371	469
28	BK	35.00	10.50	58.00	29.00	5.00	5.05	5.00	5.00	409	468
29	BK	46.50	23.75	70.50	29.00	5.12	5.05	5.00	4.75	152	445
30	BK	20.00	20.50	28.50	26.00	5.50	5.05	5.50	4.75	85	470
31	KF	8.00	8.50	7.50	9.50	4.25	5.05	4.25	4.25	278	438
32	KF	10.50	14.00	10.50	16.50	4.25	5.05	4.25	4.25	216	51
33	KF	9.50	8.00	8.50	14.00	4.50	5.05	4.50	5.00	185	454
34	KF	8.50	9.00	8.50	13.00	4.50	5.05	4.50	4.50	158	485
35	KF	14.00	16.50	14.00	17.00	4.50	5.25	4.50	4.75	159	50
36	KF	11.50	14.00	11.00	11.00	4.75	5.05	4.75	4.25	318	407
37	KF	9.00	14.00	9.00	8.50	5.00	5.05	5.00	5.00	299	458
38	KF	10.00	7.50	9.50	13.50	5.00	5.05	5.00	4.75	30	483
39	KF	15.00	18.50	11.00	11.00	5.25	5.05	5.25	5.00	31	455
40	KF	9.50	9.50	16.75	20.00	5.50	5.05	5.25	5.00	274	481
41	RO	18.75	24.00	18.00	15.00	4.25	5.05	4.25	4.35	351	459
42	RO	13.50	14.50	13.00	13.50	4.50	5.05	4.50	4.50	366	492
43	RO	15.50	11.50	18.00	34.00	4.50	5.05	4.50	4.25	325	514
44	RO	15.00	19.00	15.00	12.50	4.75	5.05	4.75	4.75	101	511
45	RO	19.50	28.50	20.25	10.00	4.75	5.05	4.75	4.91	225	516
46	RO	5.00	8.00	14.00	13.50	5.00	5.05	5.00	4.25	6	509

47	RO	15.50	17.50	15.00	19.00	4.95	5.05	5.00	4.75	34	487
48	RO	21.00	18.00	15.50	17.50	5.00	5.05	5.00	4.75	78	462
49	RO	21.00	14.00	18.50	14.50	5.00	5.05	5.00	4.25	349	489
50	RO	23.00	15.50	19.00	17.50	5.00	5.05	5.00	4.75	164	515
51	RO	24.00	21.00	20.25	14.00	5.00	5.05	5.00	5.00	163	490
52	RO	24.50	36.00	20.50	18.00	5.00	5.05	5.00	4.75	161	496
53	RO	26.00	16.50	21.00	21.00	5.00	5.05	5.00	5.00	33	495
54	RO	30.00	26.00	29.50	15.50	5.00	5.05	5.00	4.75	190	488
55	RO	35.00	37.00	36.00	19.00	5.00	5.05	5.00	4.90	77	493
56	WE	13.00	15.00	10.50	12.00	4.25	5.30	4.25	4.25	226	59
57	WE	18.00	23.50	17.50	19.00	4.25	5.05	4.25	4.35	195	443
58	WE	18.00	17.50	25.00	25.00	4.25	5.05	4.25	4.25	310	60
59	WE	24.00	24.00	25.00	31.50	4.50	5.05	4.25	4.50	247	444
60	WE	36.50	60.50	38.00	18.00	4.25	5.05	4.25	4.25	142	441
61	WE	53.00	19.00	38.00	20.00	4.62	5.05	4.67	4.50	104	57
62	WE	22.00	23.00	23.50	18.00	4.75	5.05	4.75	4.50	248	58
63	WE	29.00	21.50	28.00	26.75	4.75	5.05	4.75	5.00	166	498
64	WE	18.25	33.00	19.00	20.50	5.00	5.05	5.00	5.00	82	499
65	WE	33.50	32.00	34.00	20.00	5.00	5.50	5.00	5.25	406	56
66	WE	18.00	33.50	24.00	35.50	5.50	5.05	5.50	4.75	36	61

Notes: BK = Burger King, KF = Kentucky Fried Chicken, RO = Roy Rogers, WE = Wendy's. NJ = New Jersey, PA = Pennsylvania, B = Before and A = After NJ's increase in the minimum wage. Store ID's refer to the variable SHEET in Card and Krueger's (1995,p18) data file.

Table 2: Lead in children of lead workers

Pair	Difference	A(11,0)	A(22,0)
1	22	0	1
2	5	0	0
3	23	1	1
4	-6	0	0
5	18	0	1
6	25	1	1
7	13	0	1
8	47	1	1
9	15	0	1
10	16	0	1
11	6	0	0
12	1	0	0
13	2	0	0
14	7	0	0
15	0	0	0
16	4	0	0
17	-9	0	1
18	-3	0	0
19	36	1	1
20	25	1	1
21	1	0	0
22	16	0	1
23	42	1	1
24	30	1	1
25	25	1	1
26	23	1	1
27	32	1	1
28	17	0	1
29	9	0	1
30	-3	0	0
31	60	1	1
32	14	0	1
33	14	0	1

TABLE 3: Sensitivity of point estimates and confidence intervals for the lead exposure data

Value of Γ	Minimum	Maximum	95% Confidence interval
1	15.25	15.25	[9, 19.5]
2	10.75	19.5	[7, 26]
3	8.5	22.5	[1.5, 33]
4	8	24	[−1, 41]

TABLE 4: Sensitivity of point estimates and confidence intervals for the minimum wage data

Value of Γ	Minimum	Maximum	95% Confidence interval
1	.0375	.0375	[−.0234, .1997]
1.5	.0118	.1321	[−.1254, .2663]
2	−.0250	.2004	[−.1899, .3157]
3	−.1254	.2663	[−.2984, .4112]

TABLE 5: Sensitivity of point estimates and confidence intervals using an instrumental variable for the minimum wage data

Value of Γ	Minimum	Maximum	95% Confidence interval
1	.60	.60	[−.51, 1.93]
1.5	.09	1.41	[−1.45, 2.56]
2	−.59	1.95	[−2.36, 3.55]

Figure 1

Employment in New Jersey and Pennsylvania

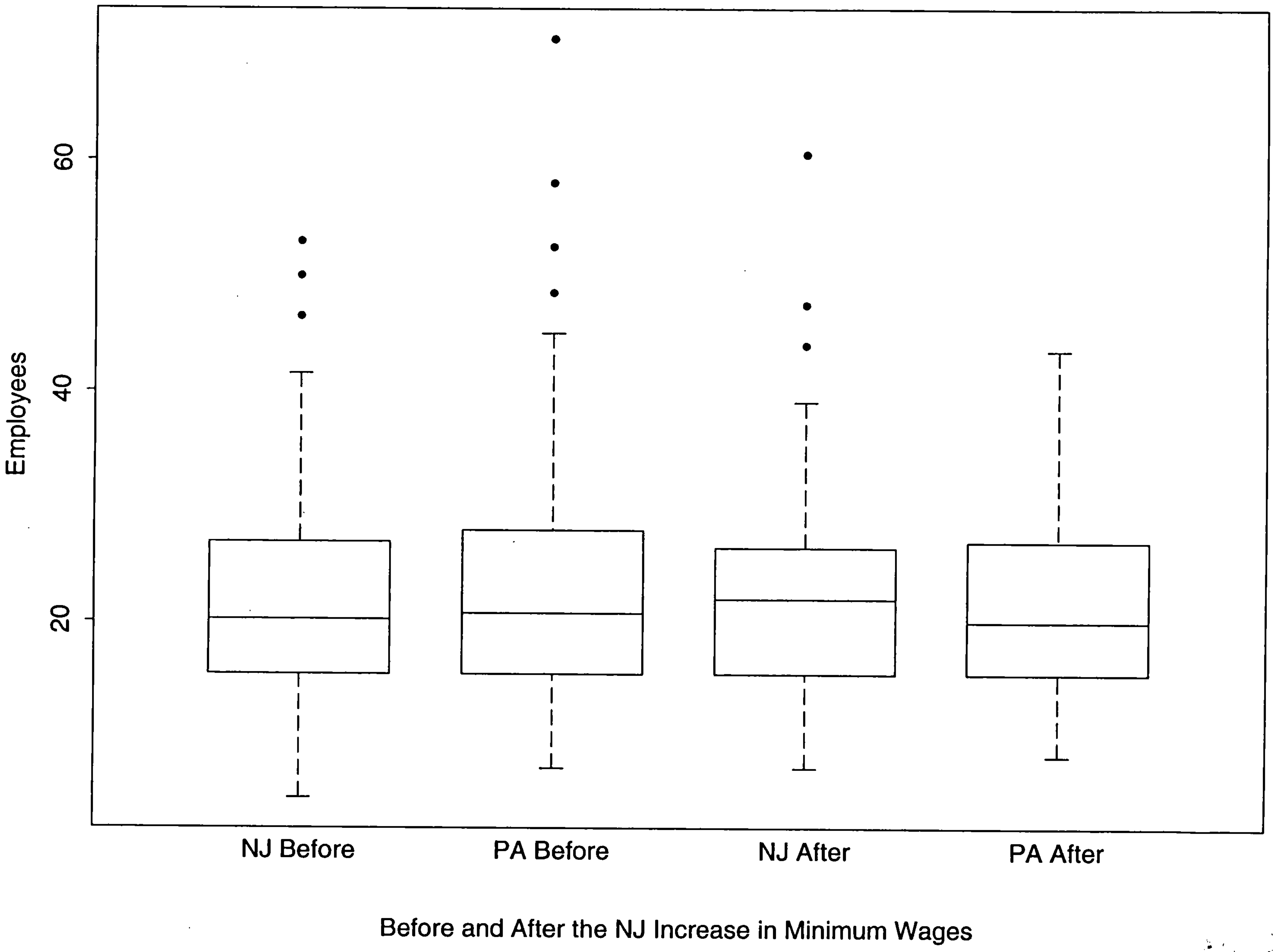


FIGURE 2: Five number summaries of employment and starting wages

Employment in matched stores

	New Jersey		Pennsylvania	
Before	20.25		20.75	
	15.5	26.75	15.5	27.75
	5	53	7.5	70.5
After	22		20	
	15.75	26.5	15.62	26.94
	7.5	60.5	8.5	43.5

Starting wages in matched stores

	New Jersey		Pennsylvania	
Before	4.68		4.71	
	4.25	5.00	4.25	5.00
	4.25	5.50	4.25	5.50
After	5.05		4.50	
	5.05	5.05	4.25	4.86
	5.05	5.50	4.25	6.25

Median	
Lower Quartile	Upper Quartile
Minimum	Maximum

Figure 3

Lead in Children of Lead Workers

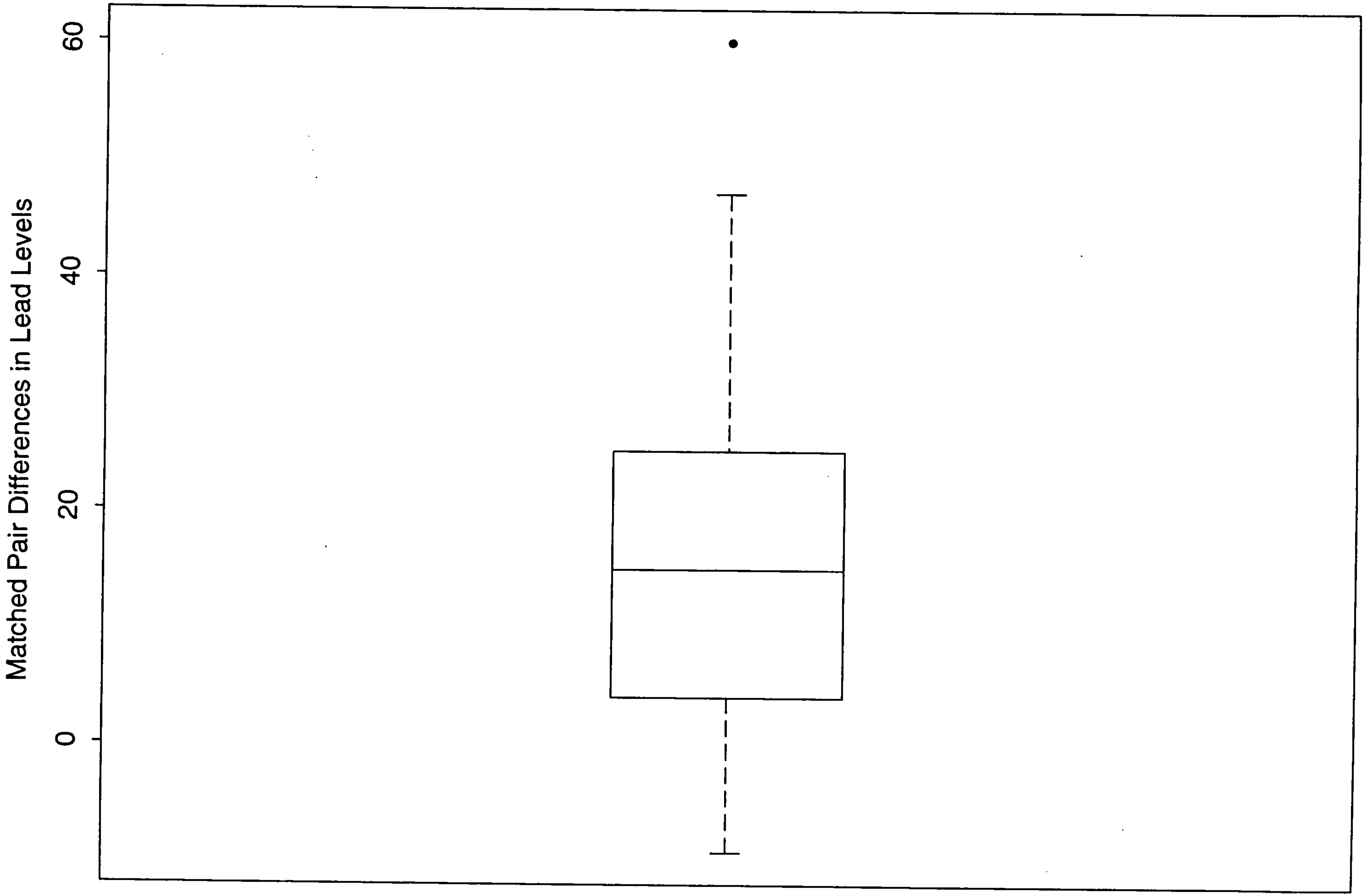
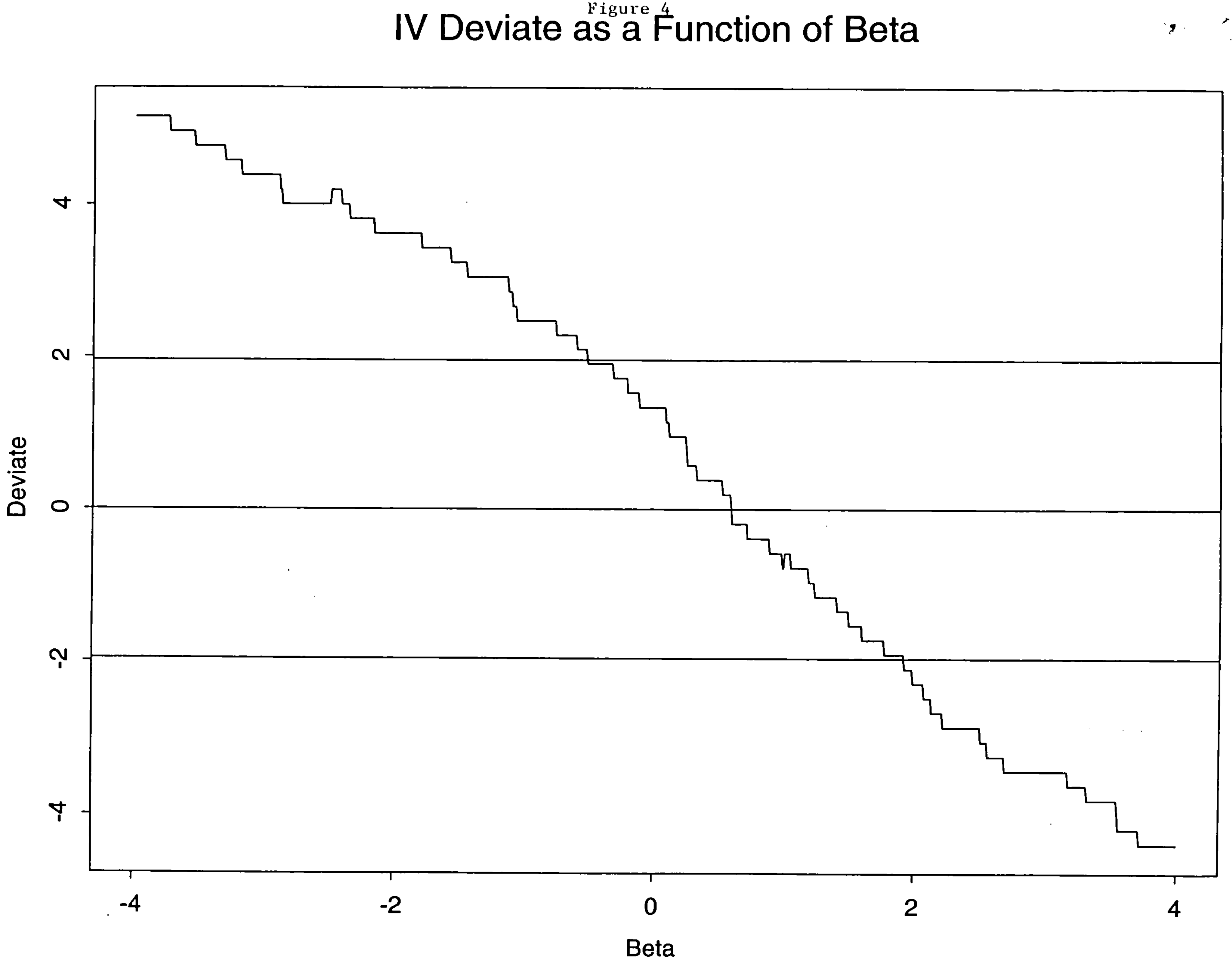


Figure 4



CHOICE AS AN ALTERNATIVE TO CONTROL IN OBSERVATIONAL STUDIES

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ABSTRACT

In a randomized experiment, the investigator creates a clear and relatively unambiguous comparison of treatment groups by exerting tight control over the assignment of treatments to experimental subjects, ensuring that comparable subjects receive alternative treatments. In an observational study, the investigator lacks control of treatment assignments and must seek a clear comparison in other ways. Care in the choice of circumstances in which the study is conducted can greatly influence the quality of the evidence about treatment effects. This is illustrated in detail using three well-conducted observational studies, one each from economics, clinical psychology and epidemiology. Other studies are discussed more briefly to illustrate specific points. The design choices include: (i) the choice of research hypothesis, (ii) the choice of treated and control groups, (iii) the explicit use of competing theories, rather than merely null and alternative hypotheses, (iv) the use of internal replication in the form of multiple manipulations of a single dose of treatment, (v) the use of undelivered doses in control groups, (vi) design choices to minimize the need for stability analyses, (vii) the duration of treatment, and (viii) the use of natural blocks.

Key words: Causal effects, control groups, internal replication, observational studies, sensitivity analysis, stability analysis, treatment effects, undelivered doses.

1. INTRODUCTION AND SOME GROUND RULES

1.1 *Active Observation: Control and Choice*

Many observational studies do not succeed in providing tangible enduring, and convincing evidence about the effects caused by treatments, and those that do succeed often exhibit great care in their design. Particularly at the early stages of design, this care consists of choices that determine the circumstances of the study and the data to be collected. A convincing observational study is the result of active observation, an active search for those rare circumstances in which tangible evidence may be obtained to distinguish treatment effects from the most plausible biases. In an experiment, treatment effects are seen clearly because the environment is tightly controlled, whereas in a compelling observational study, control is, to a large extent, replaced by choice — the environment is careful chosen.

Studies of samples that are representative of populations may be quite useful in describing those populations, but may be ill suited to inferences about treatment effects. For instance, describing early work in public program evaluation, Donald Campbell (1988,p324) wrote:

> There was gross overvaluing of, and financial investment in, external validity, in the sense of representative samples at the nationwide level. In contrast, the physical sciences are so provincial that they have established major discoveries like the hydrolysis of water ... by a single water sample ...

Passive observation of a natural population followed by regression analysis is often unsuccessful as an approach to inference about treatment effects; see, in particular, Box (1966) and Freedman (1997). A recent report of the National Academy of Sciences (Meyer and Fienberg 1992, p106) concerning evaluation studies of bilingual education makes a similar point:

> In comparative studies, comparability of students in different programs is more important than having students who are representative of the nation as a whole. Elaborate analytic methods will not salvage poor design or implementation of a study.

Here, several principles are discussed which guide the choices made in the early planning and design of an observational study. The principles are illustrated using three well-conducted observational studies from economics, clinical psychology and epidemiology. The studies are briefly described in §2 and the principles in §3. Section 1.2 mentions certain ground rules in discussions of the quality of evidence.

1.2 *The Quality of Evidence: Some Ground Rules*

Most empirical disciplines make use of some form of mathematical reasoning. To discuss the quality of evidence provided by an empirical study one must first recognize that evidence is not proof, can never be proof, and is in no way inferior to proof. It is never reasonable to object that an empirical study has failed to prove its conclusions, though it may be reasonable, perhaps necessary, to raise doubts about the quality of its evidence. As expressed by Sir Karl Popper (1968,p50): "If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are."

One expects that a proof will be correct and that evidence will be candidly presented, despite occasional disappointments. One is often concerned about the relevance of a proof and the quality of evidence. If it is proved that a certain bridge can withstand certain forces, and yet the bridge collapses, it not the correctness but the relevance of the proof that is likely to be called into question. The distinction between the mathematical correctness and relevance of a proof is familiar in most empirical sciences that use mathematical methods, for instance, economics, where Samuelson in his 1947 technical treatise, *The Foundations of Economic Analysis,* wrote:

> ... our theory is meaningless in the operational sense unless it does imply some restrictions upon empirically observable quantities, by which it could conceivably be refuted. (p7) ... By a *meaningful theorem* I mean simply a hypothesis about empirical data which could be refuted, if only under ideal conditions. A meaningful theorem may be false. (p4)

Evidence may refute a theorem, not the theorem's logic, but its relevance. A related point is made by Quine (1951).

Evidence, unlike proof, is both a matter of degree and multifaceted. Useful evidence may resolve or shed light on certain issues while leaving other equally important issues entirely unresolved. This is but one of many ways that evidence differs from proof.

Evidence, even extensive evidence, does not compel belief. Rather than being forced to a conclusion by evidence, a scientist is responsible and answerable for conclusions reached in light of evidence, responsible to his conscience and answerable to the community of scientists. Of this, Michael Polanyi (1964) writes:

> ...our decision what to accept as firmly established cannot be wholly derived from any explicit rules but must be taken in the light of our own personal judgment of the evidence.
>
> Nor am I saying that there are no rules to guide verification, but only that there are none which can be relied on in the last resort...
>
> We may conclude that just as there is no proof of a proposition in natural science which cannot conceivably turn out to be incomplete, so also there is no refutation which cannot conceivably turn out to have been unfounded. There is a residue of personal judgment required in deciding — as the scientist eventually must — what weight to attach to any particular set of evidence in regard to the validity of a particular proposition. (p30-31)
>
> The scientist takes complete responsibility for every one of these actions... (p40) ... for the process as a whole — he will assume full responsibility before his own conscience. (p46)

Evidence may be convincing beyond reasonable doubt, yet being convinced or remaining skeptical is a judgment for which a scientist is responsible. The desire to be compelled rather than convinced by evidence is the desire to evade responsibility for judging the evidence.

Aesthetics matter in proof, but also in evidence. A beautiful proof is simple, illuminating its conclusion and rendering it obvious. The same aesthetic applies to evidence. In both cases, the simple and the obvious are the product of care, effort and skill.

2. THE THREE OBSERVATIONAL STUDIES

2.1 *Basis for Selecting These Studies, and a Limited Warranty*

The following several issues guided the choice of studies.

(i) The studies employ interesting, sound and instructive methodology in their design and, at the very least, competent methodology in their analysis.

(ii) They come from three fields, economics, clinical psychology, and epidemiology, which conduct many observational studies, some of which are of excellent design and execution.

(iii) The treatments and outcomes under investigation in these studies are immediately intelligible and interesting to individuals outside these three particular fields.

A limited warrantee and a few caveats are in order. In selecting these studies, I am not asserting that they have reached true conclusions about the effects of the treatments they study. Such an assertion is never warranted based on the results of a single observational study. Indeed, the economics example is currently the focus of fierce debate — that is part of the charm of this particular example. However, because of their careful design, these three studies have provided evidence that must be reckoned with by anyone who wishes to hold views contrary to their conclusions. That is to say, to hold views contrary to their conclusions, one must explain, in tangible terms, why their findings may be set aside, and moreover, one's explanation for setting findings aside may itself be challenged as inadequate. This is, I think, what the author of an observational study aspires to do, namely to collect evidence that must be adequately reckoned with and not dismissed in forming a judgment about the effects of a treatment.

The studies have various implications for public policy, but it is their methodologies and not their policy implications that are under examination. This deserves emphasis, for it is easily misunderstood. Commonly, when evidence from varied studies is weighed, the goal is to examine the conclusions of those studies. That common goal is not the goal here. Although studies of the minimum wage, bereavement and occupational hazards will be discussed, no conclusions are reached about these subjects. This paper concerns the relationship between choices in research design and the quality of the resulting evidence about effects caused by treatments.

My discussion of these studies is based on their published reports, so issues of data quality are not explored. Inspection of the data file for the minimum wage study hints that interviewers and respondents may, on occasion, have misunderstood one another, as happens in many if not all surveys. Issues of this sort may or may not be relevant to the conclusions of the three studies, but they are not relevant to the methodological issues under discussion here.

These three studies are useful for teaching about observational studies, where issues not addressed in this paper would also be discussed. In particular, adjustments for overt biases and sensitivity analyses for hidden biases are not discussed in the current paper because they are discussed in detail elsewhere; however, these three studies can also be used to illustrate these techniques, which are central to the later, analytical parts of an observational study. Two of the studies are matched, and two use regression adjustments. The original published account of the epidemiology example includes its small data set together with a nonparametric analysis. The data for the larger economics example is publicly available electronically by FTP or File Transfer Protocol; see Card and Krueger (1995,p18). A sensitivity analysis for the epidemiology example is given in Rosenbaum (1993, see also 1995,§4) and requires only elementary calculations. A related analysis is given in Rosenbaum (1991). For various methods of sensitivity analysis, see: Angrist, Imbens and Rubin (1996), Copas and Li (1997), Cornfield, et. al. (1959), Gastwirth (1993), Greenhouse (1982), Manski (1990), Rosenbaum and Rubin (1983b), and Rosenbaum (1986, 1987, 1995,§4).

2.2 *The Economics Example: The Effects of Increasing the Minimum Wage*

The most familiar of all arguments of economic analysis are those of comparative statics, as discussed, for example, in the early chapters of Samuelson's (1947) *Foundations of Economic Analysis*. Arguments of this form (c.f. Samuelson's "Illustrative Market Case" on page 17) suggest that forcibly, sharply increasing the price of a commodity will decrease the demand for that commodity, other things "being equal".

Viewing labor as a commodity and applying such considerations leads many economists to expect that an increase in the minimum wage will cause a decline in employment among workers receiving the minimum wage. For this reason, many economists argue that increasing the minimum wage hurts the very individuals it is intended to benefit. Inspection of the details of such a theorem suggest that, as theoretical argument removed from data, this expectation seems logical.

Writing in the *American Economic Review* in 1994, David Card and Alan Krueger (CK) attempted to estimate the effects of an increase in New Jersey's minimum wage that occurred on April 1, 1992. CK looked at employment in the fast food industry (Burger King, Kentucky Fried Chicken, Wendy's and Roy Rogers) in New Jersey before and after the increase in the minimum wage and compared this to a control group consisting of fast food restaurants in adjacent, eastern Pennsylvania. They found "no evidence that the rise in New Jersey's minimum wage reduced employment at fast-food restaurants in the state."

Their study is highly controversial in its conclusions and somewhat unusual in its design. To shed light on its design, it is useful to describe a related study conducted in the more traditional manner, which reached very different conclusions. The traditional study used here for comparison actually came in response to Card and Krueger, and was conducted by Deere, Murphy and Welch (1995), or DMW. The DMW study attempted to estimate the effects of the increases in the Federal minimum wage that took place from $3.35 to $3.80 on April 1, 1990 and from $3.80 to $4.25 on April 1, 1991 by applying regression to national, monthly data from the Current Population Survey from 1985 to 1993. DMW conclude: "The regression estimates have no surprises. When the cost of employing low-wage laborers is increased, fewer low-wage laborers are employed." Without taking sides on the substantive conclusion, §3 will at several places compare the designs of these two studies.

Before leaving this introduction to the studies by CK and DMW, one should note that their conclusions do not, strictly speaking, contradict one another. First, CK discuss a particular change in a state minimum wage while DMW discuss a particular change in the Federal minimum wage, and it is possible in principle that these different interventions had different effects. Second, DMW examine changes in employment in certain demographic groups with varied percentages of minimum wage earners, while CK discuss employment by particular fast food restaurants. Note, however, that the simple comparative statics argument above would suggest that these distinctions should not matter and the same general pattern of effects should be seen in both cases.

2.3 *The Psychology Example: The Effects of Loss of a Spouse or Child in a Car Crash*

Lehman, Wortman and Williams (1987) (LWW) attempted to estimate the long term psychological effects of a sudden and unexpected death of a spouse or a child in a motor vehicle crash. The study identified 39 individuals who had lost a spouse and 41 individuals who had lost a child in a motor vehicle crash four to seven years prior to the study. This group of exposed subjects was the result of a selection process that applied various criteria and sampling to a record of motor vehicle fatalities in Michigan between 1976 and 1979, with some nonresponse discussed in the paper.

Noting that "many previous studies on the impact of bereavement have not included control or comparison groups...", so that these studies were "difficult to interpret", LWW constructed a control group in the following way. From a reservoir of 7,581 individuals who came to renew their licenses, one control was matched to each exposed subject based on gender, age, family income in 1976 (i.e., before the crash), education level, number and ages of children. The outcomes included measures of depression and various psychiatric symptoms. Bereaved spouses and parents both were depressed substantially and significantly more than matched controls between 4 and 7 years after the loss, and bereaved spouses exhibited higher rates of several other psychiatric symptoms.

LWW concluded:

> The results presented here suggest that the current theoretical approaches to bereavement may need to be reexamined ... [The discussion goes on to contrast the study's results with the views of Bowlby and Freud, among others.] (p228)
>
> From 4 to 7 years after the sudden loss of a spouse or child, bereaved respondents showed significantly greater distress than

> did matched controls, suggesting little evidence of timely resolution. Contrary to what some early writers have suggested about the duration of the major symptoms of bereavement ... both spouses and parents in our study showed clear evidence of depression and lack of resolution at the time of the interview, which was 4 to 7 years after the loss occurred... The present study suggests that exposure to stress can trigger enduring changes in mental health and functioning. (p229)

2.4 *The Epidemiology Example: Lead in Children of Workers Subject to Occupational Exposures to Lead*

Morton, Saah, Silberg, Owens, Roberts and Saah (1982) (MSSORS) examined lead levels in the blood of children whose parents worked in a factory that used lead in the manufacture of batteries. They suspected that parents brought lead home in their clothes and hair, thereby exposing their children. Thirty-three children from different families with a parent at the battery factory were matched to thirty-three unexposed control children, the matching being based on age, neighborhood and exposure to traffic.

They found that exposed children had substantially higher levels of lead in their blood than did matched control children. The exposed children were also classified in two ways, namely parental exposure to lead on the job (low, medium or high), and parental hygiene upon leaving the factory (good, moderately good, poor). They found lower levels of lead in the blood of exposed children whose parents had lower exposures to lead on the job, and lower levels of lead among children whose parents had better hygiene. MSSORS (1982,p555) concluded: "...the data presented justify more stringent enforcement of lead containment practices..."

3. **CHOICE IN THE DESIGN OF OBSERVATIONAL STUDIES**

3.1 *The Choice of Research Hypothesis: Narrow, Focused, Controlled Examination of a Broad Theory*

Consider, first, a laboratory experiment in the physical or bio-chemical sciences. It begins with a broad theory that makes assertions about the effects of a treatment. Specifically, such a broad theory makes innumerable predictions about would be observed in the innumerable circumstances in which the particular treatment might be applied, now and in the future, in different locations, etc. A laboratory experiment makes no effort to draw up a frame comprising all circumstances, locations and times when the treatment might be applied and to draw a representative sample of such circumstances. Rather, the laboratory experiment examines the theory under highly unrepresentative circumstances, namely circumstances in which sensitive, calibrated measuring instruments are used in an environment carefully freed of forces that might intrude on the experiment, in which the treatment is delivered at doses sufficient to produce dramatic effects if the theory is correct or to produce an equally dramatic absence of effect if the theory is incorrect. In a way, it is part of the essence of a laboratory experiment that its circumstances are unrepresentative. In a well-conducted laboratory experiment, one of the rarest of things happens: the effects caused by treatments are seen with clarity.

Observational studies of the effects of treatments on human populations lack this level of control, but the goal is the same. Broad theories are examined in narrow, focused, controlled circumstances.

Broad theories are desired because they predict more, and in consequence are both more useful and can be more thoroughly scrutinized. Popper (1968), again:

> ... those theories should be given preference which can be most severely tested (p121) ... [which] explains *why simplicity is so highly desirable...* [Simple theories] are to be prized more highly than less simple one *because they tell us more; because their empirical content is greater; and because they are better testable.* (p142) ... Theories are not verifiable, but they can be 'corroborated'. ...we should try to assess what tests, what trials, [the theory] has withstood (p251) ... it is not so much the number of corroborating instances which determines the degree of corroboration as *the severity of the various tests* to which the hypothesis can be, and has been, subjected. But the severity of the tests, in its turn, depends upon the *degree of testability,* and thus upon the simplicity of the hypothesis: the hypothesis which is fasifiable in a higher degree, or the simpler hypothesis, is also the one which is corroborable in a higher degree. (p267)

Broad theories permit close scrutiny in numerous particular cases, and a research hypothesis is intended to focus attention on one such case in which close scrutiny — a severe test — is possible. Passing such a severe test corroborates but does not prove the theory. Platt (1964) makes a similar point.

In the economics example, broad arguments from comparative statics predict that increases in the cost of labor will diminish employment. If true, this theory operates in innumerable instances in the US economy on a daily basis; however, in almost all such instances, it operates amid numerous other forces that obscure its effects. For instance, if wages rise faster in one company than in a second company making a different brand of a similar product, then the theory may be true even though employment does not decline in the first company, because it was precisely increasing demand for its brand of product that led the first company to raise wages in an effort to increase output by expanding its workforce. Even if the theory is correct, it rarely operates in isolation. Card and Krueger attempted to find one of those rare instances in which the theory might be hoped to operate in relative isolation. Specific aspects of this isolation are discussed later, but the point here concerns their choice of research hypothesis. The effect of the increase in the New Jersey minimum wage on fast food chains in 1992 is, in itself, at most a very minor footnote to economic history. However, as an opportunity to closely scrutinize and thereby possibly corroborate or refute the broad theory that forcible increases in wages cause declines in employment, the increase in the 1992 increase in New Jersey's minimum wage becomes much more important. In observational studies, one chooses a research hypothesis that permits a broad theory to operate dramatically and in relative isolation.

Some accounts in the popular press have emphasized, perhaps a bit more than emphasized, the breadth of the theory challenged by Card and Krueger's results. Writing in *Forbes*, the Stanford economist Thomas Sowell (1995) said:

> ...if true, these results challenge the very foundations economics. If rises in the price of labor do not reduce employment, why should we expect that a rise in the price of anything else affects the quantity purchased? This is to economics what disproving the law of gravity would be to physics.

Whether or not this is what CK did, whether or not they were really operating on this scale, nonetheless, the spirit of Sowell's remark is right: one seeks broad and consequential theories exposed to decisive challenges in focused, clear circumstances.

In the psychology example, the belief that bereavement should have short lived effects on mental functioning stems from a much broader theory, the dominant but not universally accepted theory in clinical psychology, which holds that the structure of mental functioning is largely shaped by a mixture of biology and experiences as a young child in relation to caretakers, usually parents. In particular, that theory suggests that bereavement should not greatly alter mental functioning over long periods of time. As in the economics example, if the broad theory is correct, then it is in constant operation in innumerable lives, but in almost all cases its operation is obscured by limited knowledge of the biology of mental functioning, by the difficulty in accurately measuring the experiences of early childhood, and by the difficulty in distinguishing what is cause and what is consequence in the mental life and experiences of an adult. Sudden deaths from car crashes are a situation in which the broad theory operates or fails to operate with clarity.

In short, the choice of a research hypothesis focuses a broad theory on a narrow instance in which the theory's operation may be viewed clearly. Often, this setting permits the theory to operate in relative isolation from other forces, or to operate on a dramatic scale due to concentrated exposures, or else the treatment is imposed suddenly, at a discrete moment, in a manner not influenced by the individuals under study.

3.2 *A Control Group*

In designing an observational study, a key step is to choose a situation in which a control group can be constructed. A control group consists of subjects or units which did not receive the treatment. The control groups used in the three examples were described in §2.

Some observational studies do not have a control group. For instance, in the study by DMW, everyone covered by the Current Population Survey was in a region affected by the increase in the

Federal minimum wage, so there is no control group — all subjects received the treatment. In contrast, the CK study of New Jersey's minimum wage had a control group consisting of branches of the same fast food chains across the Delaware river in eastern Pennsylvania where the minimum wage had not been increased.

Lacking a control group, DMW estimate the effect of the increase in the Federal minimum wage using regression in the following way. The Federal minimum wage increases went into effect on April 1, 1990 and April 1, 1991, so DMW define years which begin on April 1 and end on March 31. They focus on two groups of individuals, namely teenagers aged 15 to 19 and adult high school dropouts aged 20-54, reasoning that these groups contain a disproportionate number of individuals earning the minimum wage, so they should be most affected by changes in the minimum wage. These two groups are examined in parallel but separate analyses, but for brevity, only the analysis for teenagers is described here. Within these groups, men, women, and blacks are examined in similar but not exactly parallel analyses, so to permit a brief discussion here, the focus will be on the analysis for male teenagers. For each year from 1985 to 1992, for each state, there is an outcome measure, namely the log of the fraction of male teenagers in the US who were employed, as estimated by the current population survey. DMW regress this outcome on the following predictors: (i) the log of the fraction of 15-64 year old men who were employed in the same state and year, (ii) state indicator variables, and (iii) indicator variables identifying the level of the Federal minimum wage. Concerning male teenagers, they write: "Compared to the employment level projected from the movement in aggregate employment with the $3.35 minimum wage, teenage employment was 4.8 percent ... lower in 1990 ... and 7.3 percent ... lower in 1991-1992." In other words, employment among male teenagers in 1990 and 1991-2 fell more sharply than employment among all 15-64 year old men in the same period, adjusting for state-to-state differences that are constant through time. Qualitatively similar though numerically different results were obtained for females, blacks, and adult high school dropouts. Their conclusion from these regressions was quoted in §2.2.

Conclusions reached in the absence of a control group are not necessarily wrong, but they are typically open to plausible objections and legitimate skepticism of types that are inapplicable with a control group. The assumption implicit in DMW's method of estimation is that changes in the employment fraction for male teenagers would have been proportional to changes in the employment fraction among all 15-64 year old men if the minimum wage had not been increased, and any departure from proportionality is an effect caused by the change in the minimum wage. One might wonder, for instance, whether, when employment is declining, employment among teenagers and high school dropouts might fall disproportionately more than employment in the general workforce even without an increase in the minimum wage. Alternatively, a rise in the minimum wage might increase employment in certain demographic groups and decrease it in others because employers find that they can pull into the workforce better educated or more experienced workers who, at lower wages, would not work or who would work fewer hours. For instance, an anecdotal account in the popular press of changes in employment practices in response to the most recent increase in the Federal minimum wage describes an employer as introducing special hiring practices to avoid "wasting the extra 50 cents on unreliable help" (Duff 1996). Even a bit of nonlinearity on the log scale might be mistaken for an effect of changing the minimum wage. Objections of this sort may well be incorrect and unfounded, but what is important here is that they can reasonably be raised for a study which lacks a control group.

If raising the minimum wage decreases employment then, DMW reasoned, the larger decreases should occur in groups with more minimum wage earners. Although DMW focused on comparisons involving teenagers and adult dropouts, other groups with a disproportionate number of minimum wage earners were also examined briefly. For instance, low, medium and high wage states were compared, the first group being thought to be most impacted by increases in the minimum wage. Men and women were compared, where women as a group receive lower wages. These comparisons pointed in the opposite direction from the comparisons discussed in earlier paragraphs; see their Table 3. Employment in low wage states declined less than employment in high wage states. Employment among women declined less than employment among men following the increases in the minimum wage. Women in low wage states experienced no decline in employment. If increasing the minimum wage decreased employment, the reasoning DMW applied to teenagers and dropouts would have predicted larger employment declines in low wage states and among women. DMW (1995,p234) write: "The latter fact is easily dismissed based on long-standing trends." Evidently, certain interactions do exist in which different demographic groups experience larger or smaller declines in employment, although DMW believe they can distinguish which effects are caused by the minimum wage and which are irrelevant demographic trends, and perhaps others will agree with them. Nonetheless, looking at the methodology, inconsistencies of this sort can arise in studies in which treated and control groups are replaced by groups with higher and lower exposure to the treatment.

An interesting paper by Holland and Rubin (1983) looks at various "paradoxes" that arise when there is no control group and a model or a calculation is used to estimate the treatment effect. They interpret the paradoxes as consequences of diverging, uncheckable and unarticulated assumptions about what the control group would look like.

If a study lacks a control group, a minimal requirement is the articulation of the assumptions about what the control group would look like together with a discussion of the tangible evidence in support of those assumptions and the sensitivity of conclusions to violations of the assumptions. Articulation and evaluation of assumptions about the absent control group aid in judging the roles evidence and assumptions play in the study's conclusions.

3.3 *Defining Treated and Control Groups: Sharply Distinct Treatments that Could Happen to Anyone*

In a randomized experiment, treated and control groups are defined by, first, defining the treatments themselves and, second, defining and implementing a mechanism for random assignment of treatments. Typically, in prolonged experiments with human populations, two or at most a few quite distinct treatments are compared (Peto, et. al. 1976).

The situation in observational studies is different. The investigator does not control the assignment of treatments to subjects, and so must define a treated and a control group using available subjects who have already received treatment or control. The goal in defining the treatment groups should be to produce a situation that resembles, to the extent possible, the situation in a randomized experiment: markedly distinct treatments that could happen to anyone. Consider the two parts separately.

In discussing the design of controlled trials, Peto, Pike, Armitage, Breslow, Cox, Howard, Mantel, McPherson, Peto and Smith (1976,p590) say:

> A positive result is more likely, and a null result is more informative, if the main comparison is of only 2 treatments, these being as different as possible. ...it is the mark of good trial design that a null result, if it occurs, will be of interest.

Treatment groups that are distinct in concept but not in actual implementation may not differ in their outcomes even if the conceived but unimplemented distinction would have an important effect on outcomes. For instance, this was a concern in a National Academy of Sciences report on studies of bilingual education which found that: "...Immersion and Early-exit Programs were in some instances indistinguishable from one another" (Meyer and Fienberg 1992, p102).

To say that the distinct treatments "could happen to anyone" is shorthand. One wishes to define the treated and control groups in such a way the treatment could easily have happened to the controls, and the treated subjects could easily have been spared the treatment, the actual assignment of subjects to treatments being determined by haphazard or ostensibly irrelevant circumstances. Haphazard is not random, and haphazard treatment assignments can produce severe, consequential and undetected biases that would not be present with random assignment of treatments. Still, haphazard or ostensibly irrelevant assignments are to be preferred to assignments which are known to be biased in ways that cannot be measured and removed analytically.

A careful example of defining treated and control groups comes from the study by LWW of the effects of a traumatic loss of a spouse or child. First, the treated and control conditions are markedly distinct. The loss is confined to a spouse or a child less than 18 years of age living at home, and the loss was produced suddenly by a car crash. One could study the effects of a loss of other relatives, such as an adult's parent or adult's adult sibling, or gradual losses due to chronic disease, but these might have smaller psychological effects. As in experiments, effects should be demonstrated for markedly distinct treatments before refined studies of smaller effects are undertaken.

Second, LWW took care to define the treated group so it "could happen to anyone". In particular, they used published, relatively objective criteria to appraise probable responsibility or fault in the car crashes, and then insisted that the treated group consist of individuals from cars that were not at fault. For instance, if one car crossed a center dividing line and collided with an oncoming car, the occupants of the first car would be ineligible for the study while the occupants of the second car would be eligible. Their reasoning was that fault in car crashes is related to alcohol and drug use and to certain forms of pychopathology, all of which would be studied later as outcomes for survivors. In contrast, a car crash for which one is not responsible could happen to any driver. No matter what the particulars, car crashes are a far cry from random numbers, but car crashes for which one is not responsible are plausibly a limited but meaningful step closer to random.

(Expressed in the formal notation in Rosenbaum and Rubin 1983a, the goal of this section would be: Define treated T and control C conditions so that responses (r_T, r_C) differ markedly under T and C, that is, $r_T - r_C$ is large, but treatment assignment Z is conditionally independent of responses (r_T, r_C) given covariates X, or in Dawid's (1979) notation $(r_T, r_C) \perp\!\!\!\perp Z \mid X$, and moreover that prob(Z=T|X) is far from 0 or 1 for all X. A formal development of the reasons this condition is desirable is given in Rosenbaum and Rubin 1983a where it is described as ignorable assignment. Briefly, if these conditions held, then adjustment for X would suffice to estimate treatment effects. Moreover, when the treatment effects $r_T - r_C$ are large and departures from ignorability are not large, then inferences about the treatment will be judged insensitive to plausible bias; see Rosenbaum (1987,1995,§4).)

3.4 *Competing Theories, Not Just Null and Alternative Hypotheses*

In his essay, "How to be a good empiricist -- a plea for tolerance in matters epistemological", Paul Feyerabend (1968) writes:

> You can be a good empiricist only if you are prepared to work with many alternative theories rather than with a single point of view and 'experience'. This plurality of theories must not be regarded as a preliminary stage of knowledge which will at some time in the future be replaced by the One True Theory. Theoretical pluralism is assumed to be an *essential feature* of all knowledge that claims to be objective...
> The *function* of such concrete alternatives is, however, this: They provide means of criticizing the accepted theory in a manner which goes *beyond* the criticism provided by a comparison of that theory 'with the facts'... This, then, is the methodological justification of a plurality of *theories:* Such a plurality allows for a much sharper criticism of accepted ideas than does the comparison with a domain of 'facts' which are supposed to sit there independently of theoretical considerations. (p14-15)

Elsewhere, Feyerabend (1975) argues that alternative theories are needed to unearth new facts, advising one to:

> ...introduce and elaborate hypotheses which are inconsistent with well-established theories and/or well-established facts. ... the evidence that might refute a theory can often be unearthed only with the help of an incompatible alternative (p29) ... many facts become available only with the help of alternatives, [so] the refusal to consider [alternative theories] will result in the elimination of refuting facts as well. (p42)

In designing observational studies, one seeks a design that will permit a comparison of several different coherent theories that could explain the process under study, not a single comparison of one anticipated effect with one null hypothesis of no effect. When distinguishing between competing theories, one looks for situations in which the theories make different predictions. As Feyerabend indicates, this permits a sharper, more focused, critique of a proposed theory. Ideally, competing theories both make numerous predictions about observable quantities, yielding many opportunities to corroborate either theory or to refute both theories.

Card and Krueger's minimum wage study provides an example. Indeed, they quote the following remark of Paul Samuelson in support of the principle of competing theories: "In economics it takes a theory to kill a theory; facts can only dent a theorist's hide" (Card and Krueger 1995, p355).

An increase in the minimum wage in New Jersey might bring about decreased employment among minimum wage earners at fast food restaurants; that is a first theory discussed in §2.2. It is fair, I think, to say that this first theory anticipates reduced business at fast food restaurants which now must pay higher prices for labor. An alternative theory is that employers would make the minimal changes required to comply with the law, raising wages for minimum wage earners, raising prices to offset increased costs, and hoping that customers would make only minor changes in behavior in response to price increases. In particular, most customers had the choice of paying higher prices or of buying less, but did not practically have the alternative of buying at lower prices at a restaurant unaffected by the minimum wage increase. Card and Krueger (1995,p359) write: "For purposes of modeling the effect of an industry-wide wage increase ... the relevant product-demand elasticity is one that takes into consideration simultaneous price adjustments at all firms." A moment's arithmetic suggests that an increase from $4.25 to $5.05 an hour in the minimum wage, a 19% increase, might be covered by a much smaller percentage increase in prices, since many workers already earn more than the minimum $4.25, since wages are only a part of labor costs, and labor costs are only a part of total costs. In other words, one theory anticipates higher costs leading to reduced business, while the other theory anticipates higher costs leading to higher prices without reduced business. Neither theory is a null hypothesis of no change. Both theories explain how increased costs are paid. Both theories make numerous predictions about observables, so the data could contradict both theories in a manner suggesting some hidden bias. In fact, Card and Krueger (1994,§3E and §5) found no association between increases in the minimum wage and (i) the number of hours a restaurant is open on a weekday, (ii) the number of cash registers, (iii) the number of cash registers in operation at 11:00am, but they did find some evidence of, perhaps, 4% faster increases in prices in NJ than Pennsylvania during this period. During the period of the minimum wage increases, CK (1984, Table 2) found that the average price of a specifically defined "full meal" changed from $3.04 to $3.03 in the Pennsylvania restaurants and from $3.35 to $3.41 in the New Jersey restaurants. These comparisons are not inconsistent with wage increases being offset by higher prices.

In short, scrutiny of a theory is aided by a competing theory. The competition between theories suggests circumstances in which the theories make sharply different predictions about particular observable quantities.

3.5 *Internal Replication: Multiple Treatment Assignment Mechanisms*

Replication is important in observational studies, as it is in experiments. Susser (1987) writes:

> The epidemiologist's alternative to exact replication is the consistency of a result in a variety of repeated tests... Consistency is present if the result is not dislodged in the face of diversity in times, places, circumstances, and people, as well as of research design. (p88)

In part, biases that are peculiar to the circumstances of one study may not replicate, whereas an effective treatment is expected to produce similar results in studies of varied circumstance and design.

Some observational studies incorporate a form of internal replication. These studies replicate the treatment assignment mechanism, so that essentially the same treatment is assigned to subjects by more than one process. For instance, this is true of two of the studies in §2. If two treatment assignment mechanisms produce a pattern of associations consistent with an actual treatment effect, then to explain the pattern as a hidden bias, one must attribute a bias to both assignment mechanisms, and moreover a bias yielding the pattern anticipated from an actual effect. In Popper's terms, each mechanism provides a check on the theory that the treatment is the cause of its ostensible effects, so if several assignment mechanisms produce compatible estimates of effect, then this theory receives greater corroboration.

In the MSSORS study, children were exposed to low levels of lead in three different ways. First, the parents of control children did not work in the battery factory. Second, among exposed children whose parents did work in the battery factory, some were believed to have received lower doses of lead because their parents worked in jobs in the factory that provided little exposure to lead. Third, some parents exposed to high levels of lead practiced good hygiene. In fact, no matter which device assigned a child to low exposures to lead, the results were similar — children with lower exposures tended to have less lead in their blood. This pattern could, conceivably, be the result of hidden bias, but it is not easy to imagine biases that would produce all three associations.

In the CK study of the increase in the New Jersey minimum wage, a fast food restaurant could escape a legal requirement to increase wages in either of two ways. Restaurants in Pennsylvania were not required to increase wages. Restaurants in New Jersey whose lowest wage was above the new minimum wage were not required to increase wages. In connection with their Table 3, CK (1994,p778) write: "Within New Jersey, employment expanded at the low-wage stores ... and contracted at the high-wage stores... Indeed, the average change in employment at the high-wage stores... is almost identical to the change among Pennsylvania stores..." In other words, restaurants placed under no new legal requirement by the minimum wage saw similar employment changes, whether they were Pennsylvania restaurants or high-wage New Jersey restaurants.

In short, the central concern in an observational study is that treatments may be assigned to subjects in a biased manner. The choice of a setting in which essentially the same treatment is assigned to subjects by several very different processes provides a partial check of this central concern. More precisely, the theory that the treatment is the cause of its ostensible effects predicts similar effects no matter which mechanism delivered the treatment, and this prediction offers an opportunity to refute or corroborate the theory.

3.6 *Nondose Nonresponse: An Absent Association*

A causal theory not only predicts the presence of certain associations, but also the absence of certain others. In some studies, it is possible to determine the dose of treatment that a control would have received had this control received the treatment. Call this the potential dose. While it is often reasonable to expect higher responses from treated subjects who received higher doses, the same pattern is not expected among controls — higher potential doses that were never received should not predict higher responses if higher responses are being caused by the treatment.

The minimum wage study contains an example. Card and Krueger (1994) define a measure, GAP_i, of the impact of the minimum wage legislation on restaurant i, as the percent increase in the starting wage in restaurant i needed to achieve the new New Jersey minimum wage. A restaurant that already paid more that the new minimum would have $GAP_i = 0$. A restaurant that paid the old minimum wage would have $GAP_i = 18.8\%$. This variable GAP_i resembles a dose of treatment in that the law has greater impact on the starting wage when GAP_i is larger. A concern, however, is that GAP_i is not only a dose of treatment, but also a variable describing local labor market conditions. Presumably, some restaurants must pay higher starting wages than others to attract employees, so GAP_i is confounded with labor market conditions. For instance, the labor market in the poor city of Camden is different from the labor market in the relatively affluent suburb of Princeton. Still, this confounding should operate in a similar way in New Jersey and Pennsylvania. For the control restaurants in Pennsylvania, the undelivered potential dose of treatment is the percent change in the starting wage needed to achieve New Jersey's new minimum wage. If these undelivered potential doses predicted employment changes in Pennsylvania, than that could not be an effect of New Jersey's minimum wage legislation, and would strongly suggest confounding. Card and Krueger (1994, p784) write:

> ...we exclude stores in New Jersey and (incorrectly) define the GAP_i variable for Pennsylvania stores as the proportional increase in wages necessary to raise the wage to \$5.05 per hour. In principle the size of the wage gap for stores in Pennsylvania should have no systematic relation with employment growth. In practice, this is the case. There is no indication that the wage gap is spuriously related to employment growth.

In short, there is often the concern that not only the assignment to treatment or control but also the dose of treatment is confounded with unobserved covariates. When potential but undelivered doses are known for controls, the theory that the treatment is the cause of its ostensible effects predicts that undelivered doses should be unrelated to responses, offering a check of this theory.

3.7 *Stability Analyses, and Minimizing the Need for Stability Analyses*

A complex analysis involves numerous implementation decisions. The audience for such an analysis typically wishes to be assured that conclusions are not artifacts of such decisions, but rather are stable over analyses that differ in apparently innocuous ways. All three examples in §2 include stability analyses.

A sensitivity analysis asks how plausible changes in assumptions change conclusions. In contrast, a stability analysis asks how ostensibly innocuous changes in analytical decisions change conclusions. A sensitivity analysis typically examines a continuous family of departures from a critical assumption, in which the magnitude of the departure and the magnitude of the change in conclusions are the focus of attention. In contrast, a stability analysis typically examines a discrete decision, and the hope and expectation is that the conclusions are largely unaltered by changing this decision. Stability analyses are necessary in most complex analyses; however, the extent to which they are needed varies markedly from study to study.

The MSSORS study of lead exposures contains an example about lead related hobbies. MSSORS (1982,p552-3) write:

> ...11 pairs were found in which the study children had potential for lead exposure other than that due to the father's occupation, while their matched controls had no such exposures. Exogenous sources of lead found in the children's environment were automobile body painting, casting of lead and playing with spend gun shell casings in the home. Six children in the study group had fathers whose hobby was casting lead into fish sinkers; none of the control children's fathers did this. It was speculated that those who work with lead on the job are more accustomed to handling lead, thereby promoting its use in the home environment ... [A]ny study/control matched pair in which one of these hobbies was present for the study child and not present for the control was eliminated from the analysis ...

> When these 11 children and their controls were eliminated, and the remaining 22 pairs were analyzed, the study and control groups continued to be statistically different (p<.001).

MSSORS go on to show that the estimates of lead levels do not change much when the 11 pairs are excluded. Notice that a discrete decision -- whether or not to include the 11 pairs -- is investigated by carrying out the analysis both ways and comparing the results, a process that is very different from sensitivity analysis.

Similarly, in the LWW study of the effects of the death of a spouse or child in a car crash, there was concern that the death of a spouse might alter family income and, possibly, that it this sustained loss of income and not the death itself which has psychological effects. This was investigated by adjusting for income by regression, concluding that most of the findings remained stable (LWW 1987,p226). The minimum wage study included several stability analyses, for instance, various ways of handling temporarily closed restaurants; see CK (1984, Table 5).

The examples mentioned above have a common feature which arises frequently in observational studies. It is clear that one wishes to compare treated and control subjects who were comparable prior to treatment, so it is clear that visible pretreatment differences should be removed by adjustments of one kind or another. It may happen, however, that treated and control groups are first noted to differ after the start of treatment. In this case, the posttreatment difference may reflect unobserved pretreatment differences or effects caused by the treatment or the effects of other treatments occurring at the same time. Lead hobbies are another treatment coexisting with occupational lead exposure. Income loss is an outcome affected by the loss of a spouse. The closing of restaurants may be related to business conditions, the level of the minimum wage being one such condition. Adjustments for posttreatment differences may remove part of the actual treatment effect, and they may either remove bias or introduce bias into comparisons (Rosenbaum 1984). Stability analyses are common in this setting, and they seek to demonstrate that results are stable whether or not adjustments are made for a posttreatment difference. An alternative approach is explicit about the effect of the treatment on the posttreatment variable, replacing the discrete choice of a stability analysis by the continuous variation in assumptions of a sensitivity analysis; see Rosenbaum (1984,§4.3, §4.4).

As another example with a different result, the minimum wage debate between Neumark and Wascher (1992) and Card, Katz and Krueger turns in part on a stability analysis. Neumark and Wascher had conducted a panel study of changes in state minimum wages between 1973 and 1989 in relation to unemployment among teenagers and young adults, reaching the conclusion that increases in minimum wages depress employment. Card, Katz and Krueger then commented that the results were unstable in the following sense. Newmark and Wascher had made adjustments for the "proportion of the age group enrolled in school". Card, Katz and Krueger argued that, first, the conclusions about the minimum wage change dramatically if adjustments are not made for this variable and, second, that the definition of this variable is such that it is "mechanically" related to the response variable, namely employment, because anyone working even part time was counted as not enrolled in school. In their response, Neumark and Wascher disagreed.

The purpose here is to examine methodology and not the minimum wage debate. From a methodological view, the study by Newmark and Wascher (1992) relies heavily on analytical models in comparisons, whereas the study by Card and Krueger (1994) relies more on the design of the study and the choice of circumstances in which fairly comparable units, here the restaurants, are being compared. A study which relies heavily on analytical models and adjustments will make many more implementation decisions in analysis, and so will need to conduct more extensive stability analyses to be convincing. This is an argument in favor of simple study designs that compare ostensibly comparable units under alternative treatments.

3.8 *The Role of Time: Abrupt, Short-Lived Treatments*

The sense that a treatment must precede its effects impacts the study of treatments in many ways, with similar issues arising in experiments and observational studies. A treatment may be delivered over a prolonged period of time, so the distinction between what precedes treatment and what follows it may not be sharp. For instance, this is true in the MSSORS study of occupational exposures to lead.

Subjects may switch from one treatment to another, possibly in part in response to an earlier failure of the first treatment. Other extraneous treatments may intervene, and these interventions may themselves be, in part, effects stimulated by the treatments under study. Subjects may know about the treatment before they receive it, and part of the ultimate effect of the treatment may begin to materialize before the treatment is delivered. For instance, the increases in New Jersey's minimum wage were public legislation well before the increases actually took place. For discussion of various aspects of the role of time in intervention studies, see: Campbell and Stanley (1963,p5-6), Holland (1993), Joffe, et. al. (1997), Peto, et. al. (1976), Robins (1992), Rosenbaum (1984), Rubin (1991,§5.2), Schafer (1996), Sobel (1995), and Susser (1987,p86).

In research design, given the choice, one would prefer a single, abrupt, unexpected, short-lived treatment of dramatic proportions. With such a treatment, the line between what precedes treatment and what follows it is sharply drawn. This is true of only one of the three studies in §2, namely the LWW study of the psychological effects of the death of a spouse or a child in a car crash.

Another example of an abrupt, short-lived treatment is found in a study of the effects of immigration on labor markets. This is generally a difficult topic because immigration occurs gradually and immigrants may favor labor markets where jobs are available and attractive. It is usually difficult to disentangle the effects of immigration on labor markets from the effects of labor markets on immigration. Card (1990) exploited a rare exception, in which immigration was abrupt, short-lived, unexpected and of dramatic proportions. In the Mariel Boatlift, about 125,000 Cubans immigrated to Miami between May and September 1980, increasing Miami's labor force by seven percent. Card compared changes in Miami's labor market following the Mariel Boatlift to the concurrent changes in four unaffected cities, Atlanta, Houston, Los Angeles and Tampa-St. Petersberg.

3.9 *Natural Blocks*

A final opportunity to use choice in place of control in the design of an observational study involves natural blocks. Whereas matching is used to pair unrelated individuals with similar values of measured covariates, natural blocks form pairs or groups of individuals who are related in ways judged to be important but difficult to measure explicitly. Twins, siblings, neighbors, and schools are familiar examples of natural blocks.

Twins, for example, resemble one another in terms of genetics and childhood environment in many ways that cannot practically be described in measured covariates. The LWW study of the psychological effects of the loss of a spouse or a child could not adjust for genetic differences between exposed subjects and controls. In related work, Lichtenstein, Gatz, Pedersen, Berg and McClearn (1996) examined the psychological effects of widowhood by comparing twins, one bereaved and one still married. In partial corroboration of the LWW study, they too found long term psychological effects of the loss of a spouse, suggesting that genetic differences are not a likely explanation of the psychological outcomes. Behrman, Rosenzweig and Taubman (1996) use twins in an economic observational study of the college quality on subsequent earnings.

It is possible to combine matching for covariates and pairing using natural blocks. In the MSSORS study of lead exposure, exposed and control children were compared to a neighbor's child of about the same age. Here, age is a covariate whereas neighborhood is a block. Similarly, in Rosenbaum (1986), high school dropouts were matched to students with similar grades, test scores, and behavior who remained in the same high school. Here, the high school is the block. In both cases, matching controlled for blocks and began the adjustment for covariates.

4. SUMMARY

In many well-conducted observational studies, clarity about treatment effects is enhanced by the choice of circumstances in which the study is conducted. Several principles guiding choice have been suggested. The use of choice in place of control has been illustrated with examples from economics, clinical psychology and epidemiology. Because similar principles apply in diverse disciplines, the principles are properly viewed as part of statistics.

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The Minimum Wage: the Bark is Worse than the Bite

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Abstract

This study examines the effect of changes in the federal minimum, between 1967 and 1991, on wages and employment in 32 industries selected for their presumed sensitivity to the minimum wage. Using current time series techniques most commonly associated with macroeconomics and finance, we test first for a wage response; only where one is found do we test for an employment response. 25% of the industry/minimum-wage-increase pairs show evidence of an appropriate wage response. 8 of these 54 show a statistically significant negative employment response, while 6 show significant, positive employment responses. Positive effects may be due to either a high variance distribution centered on zero, or markets with 'lemons' problems concerning worker quality. Limiting analysis to industries in which the minimum wage binds provides no evidence of a consistent negative relationship between the historical minimum wage and employment.

The Minimum Wage: the Bark is Worse than the Bite

No participant in the recent debate over the minimum wage has claimed that the minimum wage can never reduce employment, nor has anyone claimed that the minimum wage is always and everywhere an employment reducing phenomenon.[1] In simplest form, the neoclassical model of the minimum wage makes a precise prediction; if the minimum wage is set above the market clearing wage, competitive, wage-taking employers slide up their labor demand schedule and employment declines.[2] More sophisticated statements recognize that the equilibrium wage distribution is not degenerate and that the wage is only one part of labor costs, but the prediction is little affected; if the minimum wage raises wages, employment will be lower than otherwise.[3]

An issue confronting researchers is to identify situations that satisfy the first condition, a minimum wage which is binding, prior to determining the effect on employment. The common practice has been to identify markets believed *a priori* to be directly affected by the minimum wage and go straight to examination of the employment effect: thus the numerous studies of teenagers and of racial minorities. A shortcoming of this approach is that it is not possible to know if the lack of an empirical relationship between the minimum wage and employment accurately reflects the effect of the minimum wage or occurs because the minimum was not binding (at that time) on the particular market being analyzed. This allows negative findings to be dismissed, perhaps too readily

A more satisfactory approach, pioneered by David Card (Card, 1992a & 1992b), is to test for both wage and employment effects. A positive and significant wage effect assures that the minimum wage to be binding and that the necessary condition for meaningful employment tests is attained. We take advantage of monthly data on wages and employment by two and three digit SIC industries available from Employment, Hours and Earnings to extend this methodology. We examine the effect of the 11 minimum wage increases between 1967 and 1991 on average wages and employment in 32 low wage and youth industries.[4] A Box-Jenkins forecasting equation is applied to each of the

212 industry/minimum wage increase pairs -- hereafter industry/wage pairs -- and these are tested for a positive and significant relationship between increases in the minimum wage and the average wage. The effect of the minimum wage on employment is then tested on the subset of pairs which pass this *wage filter*, pairs for which there was positive wage effect. This procedure may be too strict in excluding industries (type I error), because the average wage is a relatively weak measure of the wage consequences of the minimum wage. However, it ensures that the employment tests are limited to a set of industries in which the effect of a minimum wage increase is robust.

The estimated effects of minimum wage increases on the average wage are relatively weak. Only one quarter of our industry/wage pairs, 54 in all, display a positive relationship between the minimum wage and the average wage. Furthermore, despite limiting the employment estimates to the set of industries in which we are assured the minimum wage is binding, there is little evidence of a strong negative employment effect. Only eight of the employment estimates are negative and significant while an additional six are positive and significant. The mean employment elasticity is negative, in the conventionally accepted range, but this result is an artifact of several large negative outliers. The median is positive, as is the modal interval. Trimming the largest positive and negative outliers results in a mean which is negative but too close to zero to be economically significant. The positive employment effect may be a product of problems in the association of wages and employee quality similar to that described by Akerlof in his *Market for Lemons.*

These results are in keeping with contemporary research which finds little evidence of consistently negative employment effects. We conclude that there are many markets in which minimum wage increases are not binding. Adjusting for this by limiting tests to those markets in which there is a measurable wage effect, however, does not reveal a strong relationship between increases in the minimum wage and employment loss.

1. Data: Sources and Preliminary Analysis

Our research focuses on employment and wages in two groups of industries that should be sensitive to the minimum wage: industries with average wages close to the minimum and industries which employ large numbers of teenager and young adults. The first group, which we refer to as 'Low Wage' industries, consists of 27 three digit SIC industries in which the average hourly wage of production and non-supervisory employees in January 1990 was less than 210% of the new minimum wage of $3.80 (less than $8).[5] At that time, they had 13,915,000 employees, representing 15.5% of total private employment and included industries such as wooden containers (SIC 244: 43,100 employees in January 1990), hotels and motels (701: 1,520,100), department stores (531: 2,221,200), and personal leather goods (317: 15,200). The second group, designated 'Youth' industries, consists of the 5 industries in which at least 30% of the labor force is under age 25 in 1989.[6] These industries are leather tanning and finishing (SIC 311: 15,000 employees in January 1990), automotive rentals (751: 171,900), auto repair services (753: 522,200), grocery stores (541: 2,868,800), dairy product stores (545: 22,300), retail apparel stores (560: 1,222,200), and eating and drinking establishments (580: 8,395,000).[7]

Monthly observations on employment and average wages for our industries were taken from the Employment, Earnings and Hours (EEH) series of the United States Bureau of Labor Statistics (BLS) and this data is used throughout the time series analysis. Changes in the definition of industries and the introduction of new industries limited the length of some data series. Nevertheless we were able to examine the increases in the federal minimum wage of 1991, 1990 and 1981 for all of our industries, and were able to go back to the 1967 increase for nearly half of our industries. In all, there are 212 interventions in the Low Wage and Youth industries, where an intervention is an industry/wage pair. Data on the minimum wage are from Ehrenberg and Smith (1987) and Kaufman (1991).[8]

According to standard theory, the individuals who are at risk from an increase in the minimum wage are those earning less than the new, higher minimum when it is increased. If these individuals constitute a large fraction of an industry's labor force, the employment consequences should be readily detected; if very small, detection may be difficult. The information needed to calculate this fraction is not available from the EEH and so we turn to an alternative data source, the Current Population Survey (CPS)for this estimate. Table 1 presents these for (most of) the industries that we examine in the year prior to the increase.[9]

Two patterns are apparent. First, a substantial fraction of employees in these industries earned less than the 'new' minimum wage in the year before the increase. In 1981, for example, this fraction ranged from 8.1% (SIC 239, miscellaneous fabricated textile products) to 78.4% (SIC 56, eating and drinking places); most fall between 9% and 27%. Only 7.3% of the labor force as a whole were bound by the minimum wage in that year. Other years' data follows a similar pattern. Second, there is a decline in the fraction of employees below the new minimum over time. In the 1970s and early 1980s, eating and drinking establishments had between 68% and 78% of employees below the new minimum. This fraction had fallen to 35% by the 1990 increase and 25% by the 1991 increase. The decline in the fraction of employees below the new minimum is consistent with the declining real value of the minimum wage during the 1980s.[10] Despite this decline, most of our industries have a sufficient number of employees 'bound' by the minimum wage to produce a detectable effect even for the 1991 most recent increase in the minimum wage.

The CPS data can also be used to show that the wage filter methodology provides a conservative test of the conventional theory. The methodology, uses the *mean* wage as the dependent variable. The relationship between increases in the minimum wage and change in the *mean* wage is inherently weaker and more difficult to detect than the relationship between the minimum wage and change in total employment, the latter being a *marginal* effect. The wage filter limits the tests to industries with large wage effects and it is those very industries in which the largest employment effects are expected.

Consider the effect of the 1990 increase in the minimum wage on the average wage in hardware stores. The average wage for hardware store employees two months before the increase in the wage was $6.65 and Table I indicates that 11.7% of the employees in this industry were 'bound' by the increase. Suppose that the average wage for 'bound' employees was the mid-point between the old and new minimum, $3.58; this would imply an average wage for unbound employees of $7.06. What then happens to the mean wage when the minimum rises? If employees earning less than the new minimum are all 'sacked', the mean wage will increase by 6% to $7.06, half the magnitude of the reduction in employment. If instead, the only consequence of the increase is to raise the wages of the bound employees to the new minimum, the mean wage will increase by three cents, to $6.68 per hour.

Similar results are obtained for other industry/wage pairs. Again, assuming that the average wage of employees bound by the minimum wage is the midpoint between the old and new minimum, we can calculate the effect of raising the minimum on the mean wage under the assumption that all and that none of the bound employees are sacked. When all employees are sacked a natural measure of the relative wage and employment effect is the ratio of the elasticity of the mean wage with respect to the minimum wage to the elasticity if employment with respect to the minimum wage. A ratio with an absolute value less than one is indicative of a *mean* wage effect which is weaker than the *marginal* employment effect. The top graph in figure 1 illustrates this using a box and whiskers plot. The median value of the graph, the middle line in the box, is slightly below one half; the 25^{th} percentile (the lower horizontal line) lies at about .35; while the 75^{th} percentile is at 0.6. The top and bottom of the whiskers, the extreme values which lie no more than one and one half times the inter-quartile range from the near end of the box, extend up to 0.78 and down to 0.24. The outside values, designated by asterisks, result from the treatment of eating and drinking establishments under the minimum wage laws.

We cannot use this ratio to assess the case in which no employees are sacked, so we evaluate the change in the mean wage associated with all bound employees wages being increased to the new minimum wage. The bottom box and whiskers diagram indicates just how small this effect is. The median effect is less than 1%; there would be less than a 1.5% increase in the mean wage for 75% of the industry/wage pairs; even in the most extreme instance the increase in the mean is less than 5%.

Both scenarios suggest that the mean wage provides a conservative standard of the wage effect increases in the minimum wage. The magnitude of the response of the mean wage will depend on the fraction of the labor force bound by the minimum wage, the gap between the minimum wage and the wages of those earning above the minimum, and whether increases in the minimum wage affects the employment or earnings of the bound labor force. However, the change in the mean wage is virtually always considerably smaller than the employment effect, and the increase is very small if there is no employment effect. Although the shift in the mean wage should not be undetectable, our *wage filter* criterion provides a standard which assures that only industries with large wage effects are tested for employment effects. This should incline our second stage results toward detecting any employment result.

2. Econometric Framework

Our analysis uses a forecasting approach to estimating the effect of minimum wage 'interventions'. This has several advantages: the application of forecasting equations to estimate interventions is well established; use of relatively short time periods avoids the assumption of structural stability common to time series research on this topic; issues of structural modeling, which have proven controversial in this literature, are also sidestepped.[11]

The Box-Jenkins methodology presumes that both employment and the average wage are serially correlated within each industry, and exploits this in constructing a forecasting models. Hence the lagged wages are included in the wage equation, lagged employment in the employment equation. Many prior studies of employment suggest that macroeconomic conditions (the unemployment and inflation rates, and a dummy for whether the economy is in recession) contain additional information that improves the forecasts and these are included in the model (see Brown et al.) We use relatively short sample periods, nine years, but monthly data provide an ample number of observations, at least 100 per estimate.

When the wage is not integrated, the estimated equation has the following form:[12]

$$1a)\quad W_t = \sum_{s=1}^{12} a_{1s} W_{t-s} + \sum_{s=1}^{12} b_{1s} \Delta CPI_{t-s} + \sum_{s=1}^{12} c_{1s} \Delta Un_{t-s} + d_1 \Delta MWx_t + e_1 \Delta MWy_t + f_1 BC + g_1 t + C + \varepsilon_t$$

where W_t is the average wage in a SIC industry; CPI is the consumer price index; and UN is the unemployment rate; MWy and MWx are two minimum wage variables; all of these variables are in logs, and subscripts indicate lags in months.[13] In addition, BC is a dummy variable equal to one during months that the NBER has identified as periods of recession; t is a linear time trend, C is a constant term, and ε is an error term. The delta, Δ, indicates first differences to remove unit roots. When the wage is integrated but not cointegrated with any of the other variables, the equation is:

$$1b)\quad \Delta W_t = \sum_{s=1}^{12} a_{1s} \Delta W_{t-s} + \sum_{s=1}^{12} b_{1s} \Delta CPI_{t-s} + \sum_{s=1}^{12} c_{1s} \Delta Un_{t-s} + d_1 \Delta MWx_t + e_1 \Delta MWy_t + f_1 BC + C + \varepsilon_t$$

Finally, when it is cointegrated with other variables in the regression, it becomes:

$$1c)\quad \Delta W_t = \sum_{s=1}^{12} a_{1s} \Delta W_{t-s} + \sum_{s=1}^{12} b_{1s} \Delta CPI_{t-s} + \sum_{s=1}^{12} c_{1s} \Delta Un_{t-s} + d_1 \Delta MWx_t + e_1 \Delta MWy_t + f_1 BC + h_1 ECM_{Wt-1} + C +$$

where ECM_{Wt-1} is the lagged error correction mechanism, the residual from the cointegrating equation of the Wage on the RHS variables with which it is cointegrated. The corresponding equations for employment are:

$$2a)\quad N_t = \sum_{s=1}^{12} a_{2s} N_{t-s} + \sum_{s=1}^{12} b_{2s}\,\Delta CPI_{t-s} + \sum_{s=1}^{12} c_{2s}\,\Delta Un_{t-s} + \sum_{s=0}^{11} d_{2s}\,\Delta MWx_{t-s} + \sum_{s=0}^{11} e_{2s}\,\Delta MWy_{t-s} + f_2 BC + g_2 t + \varepsilon_t$$

$$2b)\quad \Delta N_t = \sum_{s=1}^{12} a_{2s}\,\Delta N_{t-s} + \sum_{s=1}^{12} b_{2s}\,\Delta CPI_{t-s} + \sum_{s=1}^{12} c_{2s}\,\Delta Un_{t-s} + \sum_{s=0}^{11} d_{2s}\,\Delta MWx_{t-s} + \sum_{s=0}^{11} e_{2s}\,\Delta MWy_{t-s} + f_2 BC + \varepsilon_t$$

and

$$2c)\quad \Delta N_t = \sum_{s=1}^{12} a_{2s}\,\Delta N_{t-s} + \sum_{s=1}^{12} b_{2s}\,\Delta CPI_{t-s} + \sum_{s=1}^{12} c_{2s}\,\Delta Un_{t-s} + \sum_{s=0}^{11} d_{2s}\,\Delta MWx_{t-s} + \sum_{s=0}^{11} e_{2s}\,\Delta MWy_{t-s} + f_2 BC + h_2 ECM_{Nt} + \varepsilon_t$$

where N is employment in an industry. The sample period is 8 years prior and one year after "the current increase," the specific increase in the minimum wage under study.

Following our testing sequence, we turn first to equation 1, the wage equation. We are interested in measuring the effect of the increase in minimum wage, the minimum wage intervention, which always occurs one year prior to the end of our sample period. Measurement is complicated because until the 1991 increase, more than one increase had occurred in each nine year period, and there is no reason to expect their effects to be comparable. We resolve this problem by having rolling samples, each ending a year after a minimum wage increase, and including two minimum wage measures. MWy is the nominal minimum wage for the full sample period, while MWx is the nominal minimum for the first 8 years. MWx does not incorporate the last minimum wage increase in the sample, remaining at the last value of the minimum wage prior to the intervention being studied.[14] Conditional on inclusion of MWx, the coefficient on MWy measures only the effect of the last increase in the minimum wage.[15]

Most variables are entered with twelve month lags, but this is not the case for the minimum wage variables, MWx and MWy. Rather, these variables are contemporaneous with the industry average wage. This reflects the mandatory nature of increases in the minimum wage and the scheduling of the increase, which has occurred during the week prior to the collection of data for the EEH since 1957 as well as the nature of the industries and labor markets under study. [16] As firms are required to pay the minimum wage, the industry average wage will immediately incorporate the increase in the minimum wage and the changes will be contemporaneous in monthly data. The casual nature of the employment relationship, the low level of capitalization and utilization of technology and the low training requirements in these industries also suggest that wage consequences of an increase in the minimum wage will be entirely contemporaneous. There is little point in raising the wage prior to the mandatory increase in labor markets with high turnover and low skill requirements. Similarly, the limited capitalization, the off-the-shelf nature of the technologies used in these industries, and the brief training required to master the jobs also suggest that little or no time would be required to adjust to the new minimum wage.

Finally, the nominal minimum wage is used as a proxy for its real value for three reasons. One, the appropriate deflator would be an industry specific index of production prices, but these are seldom available before the 1980s. Two, the combination of the logarithms of the nominal wage and the CPI approximates the real minimum wage. Three, during the period under study, the real minimum wage rises only when the nominal minimum rises, and this specification captures such increases.[17]

Our measure of the response of average wages to increases in the minimum wage is **e**, the coefficient on MWy. The wage filter consists of a 1-sided t-test, at a 5% significance level, that **e** is positive. We use industry/wage pairs that pass this test to identify instances where the minimum wage did indeed bite.

The next step is to estimate the employment equation, equation (2), on this subset of industries and periods. This equation differs from the wage equation in that lags on the minimum wage variables are also included to allow for convex adjustment costs of employment within firms. Again the timing of the increases and the reference period for the data suggest the use of lags zero through eleven rather than one through 12, as with the other variables.[18]

Our interest in the wage equation (equation 1) is limited to whether it is possible to detect a positive and significant effect of the minimum wage on the average wage. Equation (2) allows us to examine the employment effect of the minimum wage in two different ways. A pure time series approach with no explicit economic content is to test for Granger causality running from MWy to employment. This involves an F-test of the $\{e_i\}$ coefficients and the error correction term, where included, and is essentially a test of whether information about this minimum wage increase improves employment forecasts.

A significant Granger test can result from either a positive or negative employment response to the minimum wage. According to the neo-classical model, the employment effect is strictly negative. We calculate the elasticity of employment with respect to the minimum wage for each industry and minimum wage pair as the sum of the e coefficients, $\sum_{s=0}^{-11} e_s$, and use an asymptotic t-test to determine its sign and significance.

3. The Wage-Effect Filter

We turn first to the wage estimates. Results for the 212 wage tests are reported in Table II; Figures 2 & 3 summarize information about the p-values and distributions of elasticities from the wage test. We turn first to the figures. Figure 2 displays distributions of p-values for the test H_o: $e = 0$ vs. H_1: $e > 0$: where e is the coefficient on the contemporaneous minimum wage variable in equation 1. On the right of the upper figure is the Null Distribution, the large sample distribution of p-values when the null hypothesis is correct. The 'boxes' for both the Low Wage and Youth estimates are clearly shifted downward, in a direction consistent with rejecting the null of no effect. The median values for these industries both fall at or below a p-value of .25, when they should be at a p-value near 0.5 to be consistent with the null. For both, the first quartile is quite low, at or below 0.05. The third quartiles are less than the median of the null distribution for both sets of industries.

The distribution graph below plots this same information with the 45 degree line playing the role of the Null Distribution in the box plot. Again, the frequency of low p-values, those which allows us to reject the null, is substantially above that which would be expected if the null hypothesis held. Further, the results are not the outcome of a few outliers, which would produce spikes above the 45 degree line. Rather, it appears that the population as a whole is shifted in a direction consistent with the alternative hypothesis.

Estimates of the elasticity of the mean wage with respect to the minimum wage are also of interest and these are provided in Figure 3.[19] The two diagrams on the left of the page display the full distribution for Low Wage (top) and Youth (bottom) industries; the two on the right separate the estimates according to their statistical significance. The mean elasticity for Low Wage Industries is 0.113 (SE: 0.020) with a median elasticity of 0.0969. For Youth Industries the mean and median are 0.0666 (SE: 0.0182) and 0.0637 respectively.

As the graphs illustrate and the mean and median confirm, the weight of these distributions lies above zero. It is useful to separate the statistically significant and non-significant elasticities, provided in the right hand graphs in the figure. It is no surprise to find that in each set of industries, the statistically significant estimates tend to be larger than the insignificant ones. However, the considerable overlap between statistically significant and insignificant estimates in the box-plots make clear that it is not merely the largest point estimates, the upper tail, which are statistically significant. The overall distribution of the Youth industries is clearly bimodal; i.e., there are two distinct groups of events here. The distribution in the Low Wage industries is less obvious. Nevertheless, when the statistically significant and insignificant estimates are merged together, the distribution displays a prominent positive bulge, due primarily to the statistically significant estimates.

Turning to our wage filter, we assess the estimated wage elasticity against a null of a zero or negative effect using a 10% significance test. Although the analysis in Figures 2 & 3 suggests a considerable wage effect in most industries, only 54 out of 212 tests, one in four, meet the conventional criteria. As suggested at the start of this section, such criteria may be too rigorous a standard for an average wage measure, but it has the advantage of limiting the employment estimates to markets in which there is no question about the wage effect. We now turn to tests of the employment effect when the minimum wage clearly bites.

3. The Employment Impact When the Minimum Wage Bites

Estimates of our 54 employment elasticities may be found in Table 3 and these are depicted in the bottom of Figure 4. The typical estimated elasticity is not significantly different from zero in either a one or two tailed test but estimates are widely dispersed, ranging from -14.3 to 6.75. Weighted averages for each year, with statistically insignificant estimates set to zero, are at the bottom of the table. They range from -2.37 to 2.14; 5 years have positive averages, and 6 have negative ones. There are a number of industry/wage pairs which are large and negative and several which are large and positive. The result, depicted in the lower right of Figure 4, is a distribution which is centered on zero with a large variance and a negative skew. This skewness is reflected in the measures of central tendency. The mean estimate is -0.29, within the range that Brown, Gilroy and Cohen proposed; this is the only support for the standard hypothesis. Measures of central tendency can be sensitive to large outliers. That this is the case here is clear from both the median, 0.05 and the 10% trimmed mean employment elasticity, -0.009, both well above the commonly accepted range. The modal effect falls between 0 and 0.7, also substantially above the range proposed by Brown, Gilroy and Cohen.[20]

The significance of the employment effect for these 54 events are judged by two tests: first, a Granger test of whether addition of the contemporaneous minimum wage variable improves the fit of the forecasting equation; and second, a one sided t-test of the sum of the minimum wage elasticities to determine if they are, as expected, negative. The Granger tests provide little evidence that the minimum wage affects employment. As shown in the box plot and p-value frequency distribution in the top of Figure 4, the Granger test is virtually identical to what would be expected if the null hypothesis of no effect were true. The Box plot for the Granger lies slightly above that for the Null Distribution, the p-value line for the Granger is somewhat above the 45 degree line in the lower corner of the distribution graph, but then follows that line very closely.

Results from the one sided Sum of Lags test are similar. Few of the tests are negative and statistically significant at a 5% level, eight out of 54. As indicated by the box plot, the distribution of p values is more dispersed than the null but otherwise similar. The distribution of one sided p-values has a heavy left hand tail, but then follows and falls below the 45 degree line. The dispersion of the estimates is more apparent in two tailed tests of the significance of the Sum of Lags. The number of negative and significant tests remains the same, eight, but there are six positive and significant tests (in a 10%, 2-sided test). The box plot of the p-values lies below that for a null distribution. Similarly, the plot of the distribution of p-values lies above the 45 degree line. Both are consistent with a distribution which is centered on zero but which is more widely dispersed than the null distribution.

The dispersion of the estimates is not entirely surprising. Disaggregated data are likely to show greater variance and these industry data are considerably more disaggregated than the data on teens and young workers typically used in time series analysis. The results suggest that there are instances in which increases in the minimum wage can have a large negative effect on employment, as well as instances in which it appears to have large positive effects. The relatively small number of significant results, and the apparent randomness and dispersion of significant results does not point toward any simple rule for when increases in the minimum might be expected to have a negative effect on employment.

To summarize, the estimated effect of the minimum wage on employment is notably weak. This sample is defined by the presence of a positive and statistically significant minimum wage bite. Despite this filtering, fewer than three-tenths of the estimated employment effects are statistically significant, and barely one-seventh are negative and significant (at a 10% level). Given the nature of the sample, these results are not sufficiently strong to support the hypothesis that increases in the minimum wage will typically cause employment to decline.

6. Conclusions

A necessary condition for the minimum wage to influence employment is that it be binding on at least some employees in an industry. Most research on the minimum wage has selected groups subject to the minimum wage using *a priori* criteria.

This research uses an approach that allows us to test the theory more precisely. Looking at minimum wage interventions in 34 industries between 1967 and 1991, we first test to determine whether increases in the minimum wage have resulted in a detectable effect on the average wage of an industry. We then limit our study of employment effects to those industries in which there was a statistically significant and positive effect on the minimum wage. Although a test of the average wage may be too rigorous, and may exclude some instances in which minimum wage increases were binding on employees in the industry, it assures that there is a substantial wage effect in those industries which 'pass' the wage filter.

The wage filter equations indicate that minimum wage increases are often not binding. The industries selected for this study, industries at the bottom of the industry wage distribution or industries with large numbers of young employees, should have been quite sensitive to the effects of the minimum wage. Despite this, only 54 out of 212 industries demonstrated significant positive relationships between the minimum wage and the industry average wage. This suggests that the lack of a relationship between the minimum wage and employment found in many contemporary studies might be a result of a minimum wage which was not binding. Perhaps the lack of results was because researchers were looking at the wrong markets, or mixing markets in which the minimum wage mattered with many others in which it did not.

Results from our employment equations do not support this view. A majority of our employment estimates are not statistically significant. Those which are, are fairly evenly distributed between positive and negative effects, a description that also fits the set of average elasticities for each increase, weighted by employment in the industries that pass the wage filter.[21] Although the median employment elasticity fits the conventional view, and the mean, if anything, goes beyond that, the figure is an artifact of several unusually large and suspect estimates. Elimination of these extreme estimates resulted in a mean very close to zero. The dispersion of estimates, whether by year or by industry, does not support any simple reconciliation of the results with the standard view of the effect of the minimum wage on employment. Scatterplots of the wage and employment elasticities provided in figure 5, reveal no prominent patterns in the estimates.[22]

The large and statistically significant positive employment effects are puzzling, especially for industries in which we have already found similar wage effects. In response to similar findings that Card and Krueger reported, Finis Welch skeptically suggested that they were trying to repeal the law of demand.[23] An explanation of this finding would increase its credibility and perhaps lower the level of acrimony surrounding this issue.

The law of demand contains an important qualification, one that is quite common in economic analysis: to wit, *ceteris paribus*. What sorts of things might not be equal? And why? Begin with the second question. It is plausible that Akerlof's (1970) model of the *Market for Lemons* aptly characterizes some labor markets, especially low wage and youth labor markets. When the average wage is low, potential suppliers of high quality labor choose not to work. Increases in the minimum wage raise the average wage, and by attracting higher quality labor into the market, make it profitable for firms to increase employment.

Why would firms wait for an increase in the minimum wage to hire high quality labor? Why not try to attract it through higher wages? That is the strategy that banks follow in models of credit rationing.[24] Instead of raising interest rates to clear the market, banks are thought to keep them low to attract higher quality borrowers. Of course, they attract more borrowers of all quality, and have to screen them carefully; but at the lower interest rates, the average quality is higher, and the screening costs can be amortized over what is hoped to be a long term relationship.

Certainly in many labor markets, higher compensation is used to attract higher quality candidates, and screening (and the associated costs) are important, as are long term relationships. This is not the case in low wage and youth industries, where turnover is typically high. In a high turnover industry, raising wages is not an efficient way to improve average quality of potential employees because the screening costs would have to be repeated too frequently and could not be amortized over a long period.

Furthermore, information networks among workers already within an industry are likely to be more efficient than among those who are outside of it or entirely outside of the labor force. If a single firm were to raise its wage offer, most of those who would hear about it would be those already in the industry, presumably the suppliers of lower quality labor that the firm hopes to avoid. In contrast, increases in the minimum wage are well publicized, and if they draw higher quality workers into the labor force, would be much more effective toward this goal.

It is not our claim that increases in the minimum wage could not have an adverse impact on employment. We are limited to an analysis of the historical experience with the minimum wage and this experience has been one of moderate increases. Movement outside of this experience, a doubling of the minimum wage, might well have an employment effect. However, our research supports the view that the moderate increases in the minimum wage which have been acceptable to the political processes of this country do not affect employment in a consistent and systematic fashion. Judging by the acrimony of recent debates about the minimum wage, it strikes us that the minimum wage's bark has been worse than its bite.

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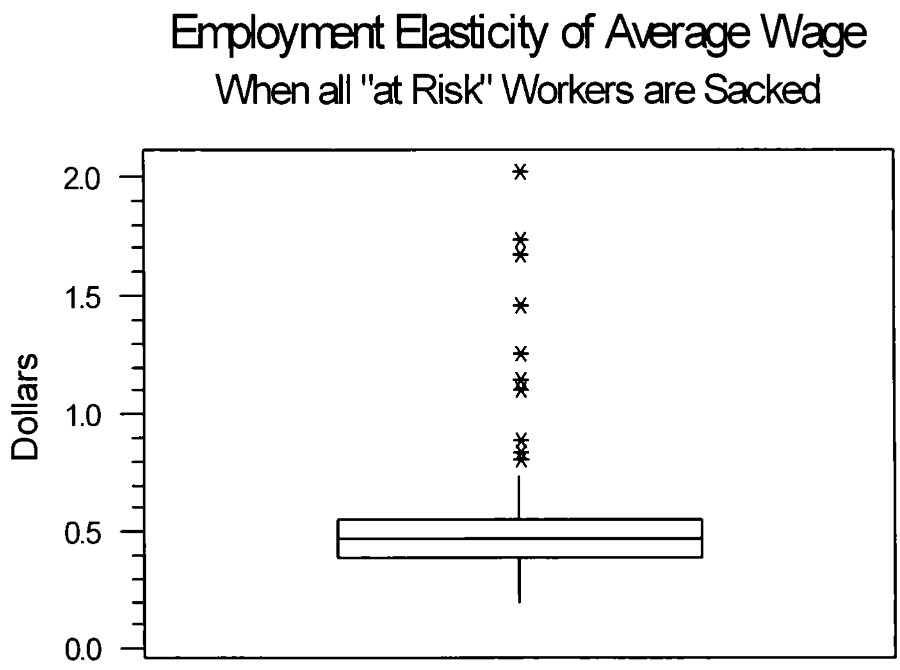
Figure 1


Employment Elasticity of Average Wage
When all "at Risk" Workers are Sacked
Dollars
2.0
1.5
1.0
0.5
0.0

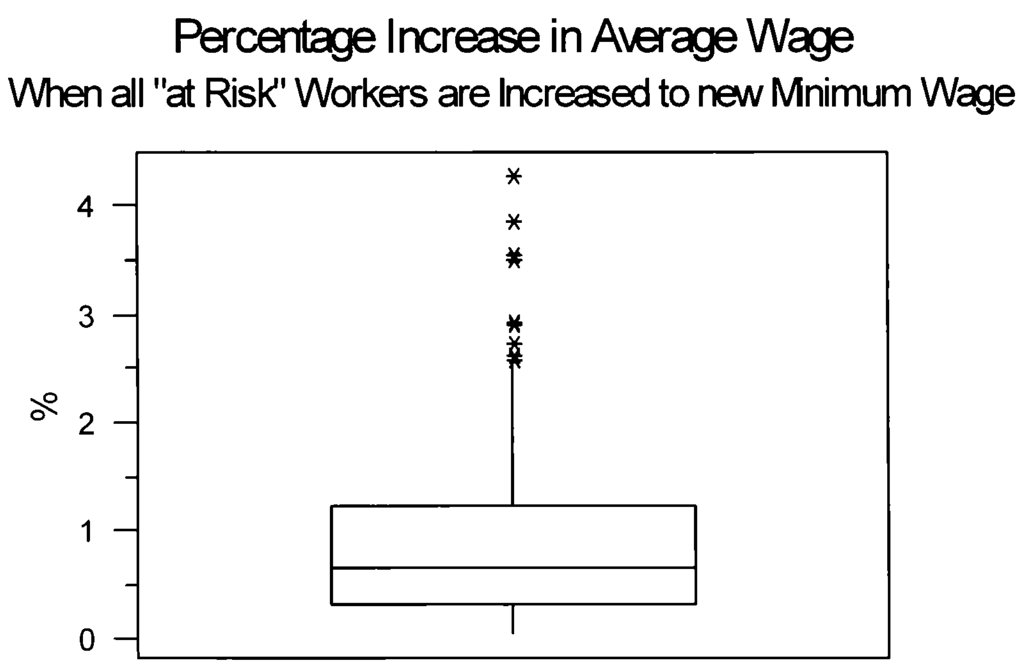
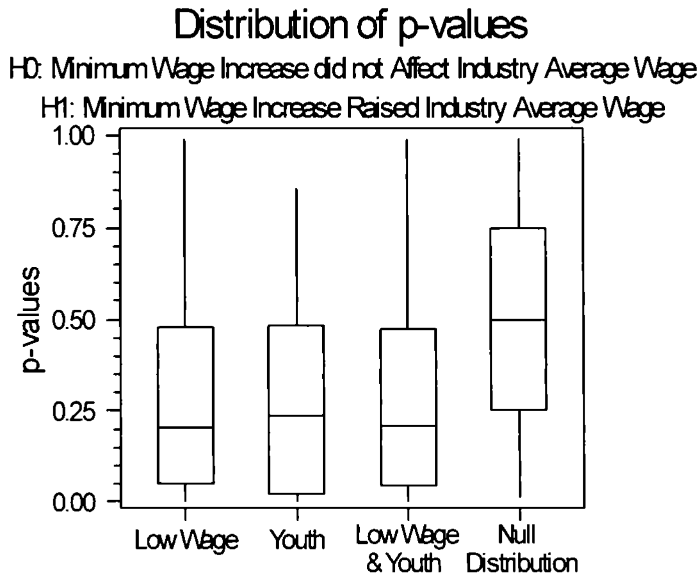

Percentage Increase in Average Wage
When all "at Risk" Workers are Increased to new Minimum Wage
%
4
3
2
1
0

Figure 2
The Wage-Effect Filter



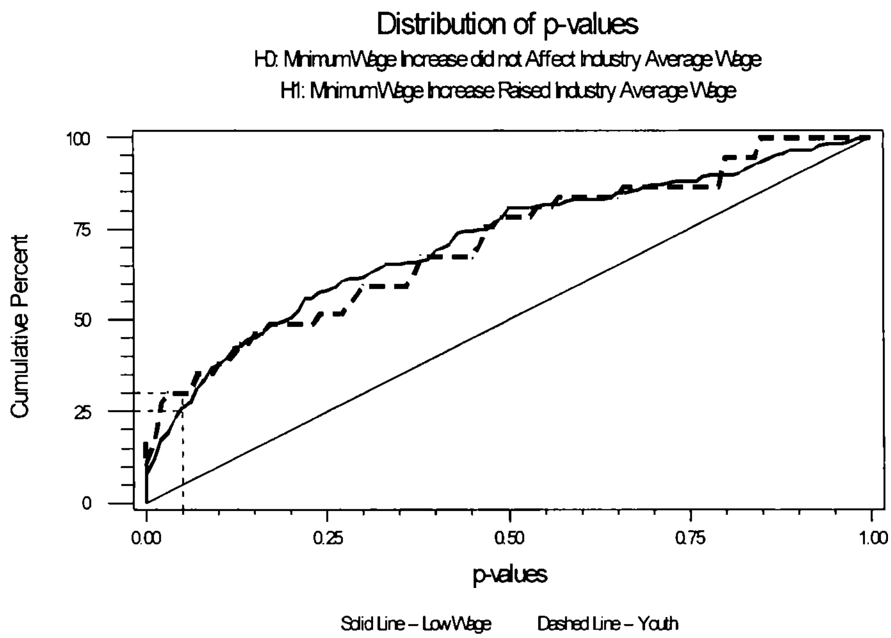


Figure 3

Distribution of Estimated Minimum Wage Elasticities of Average Wage

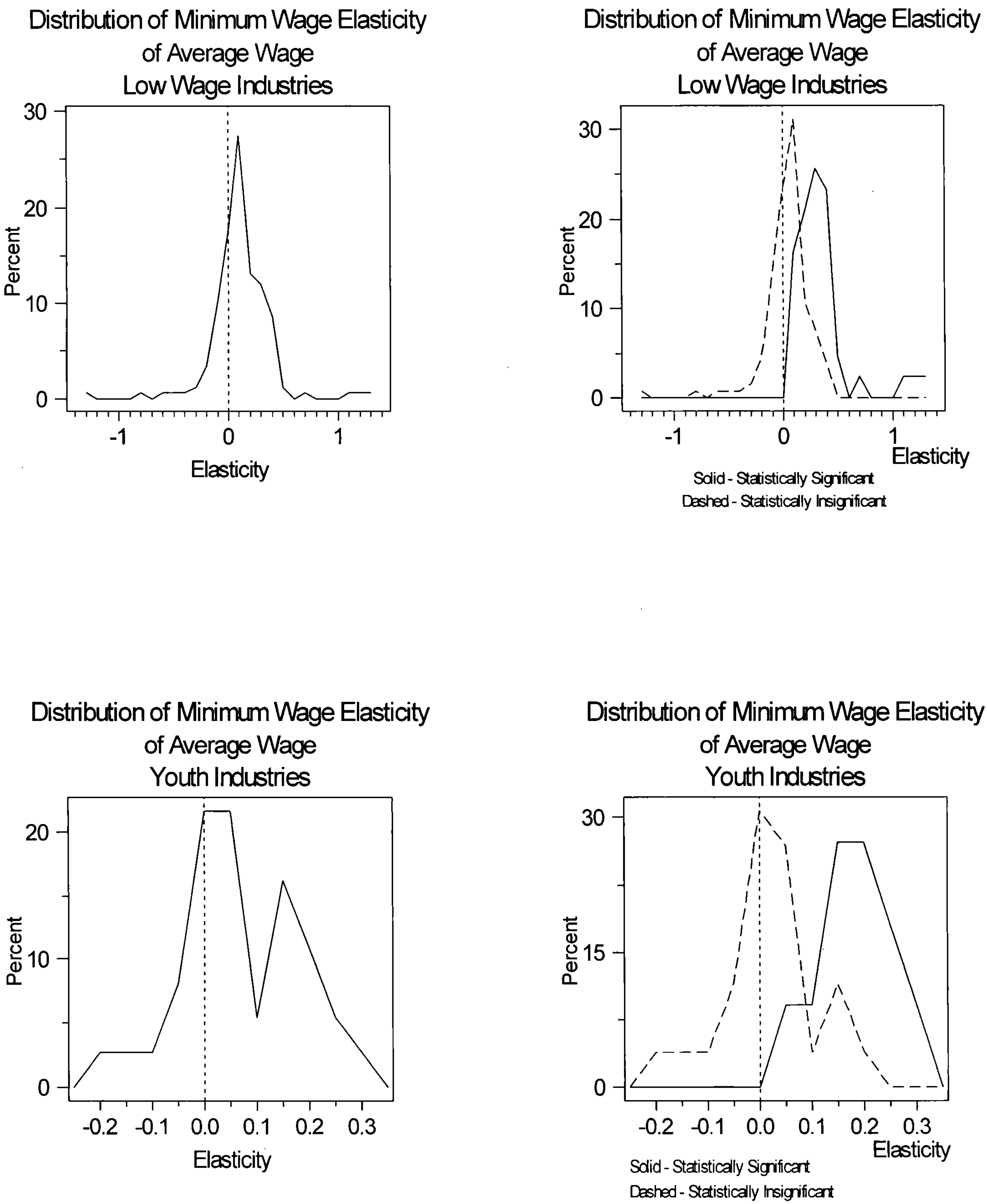
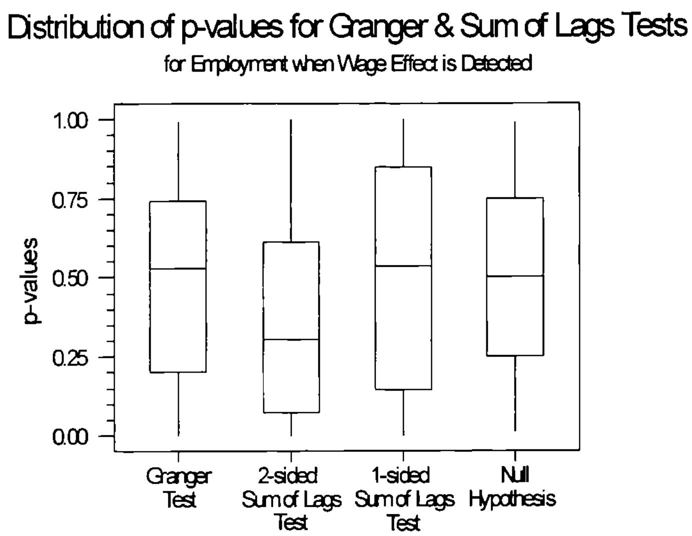
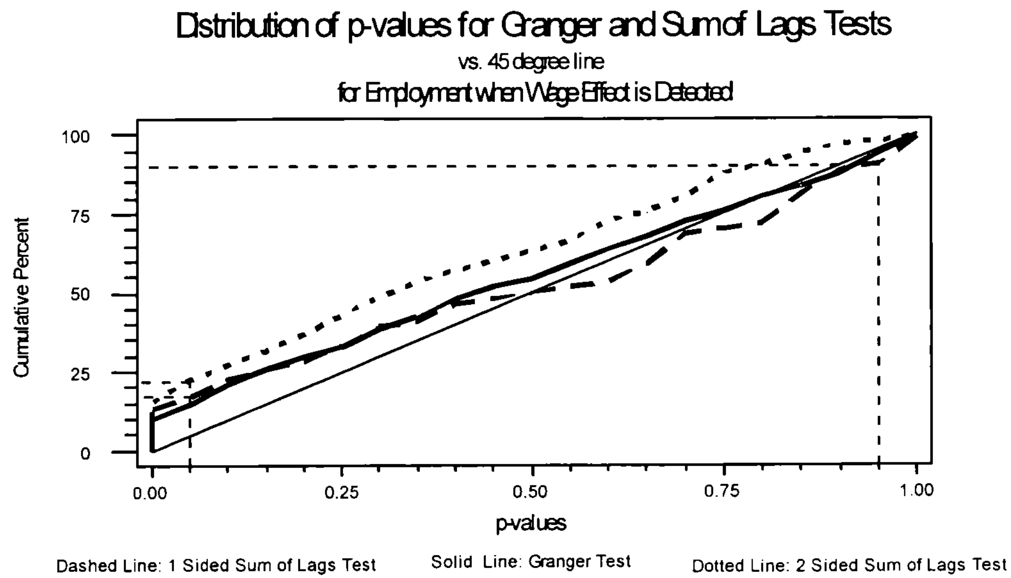
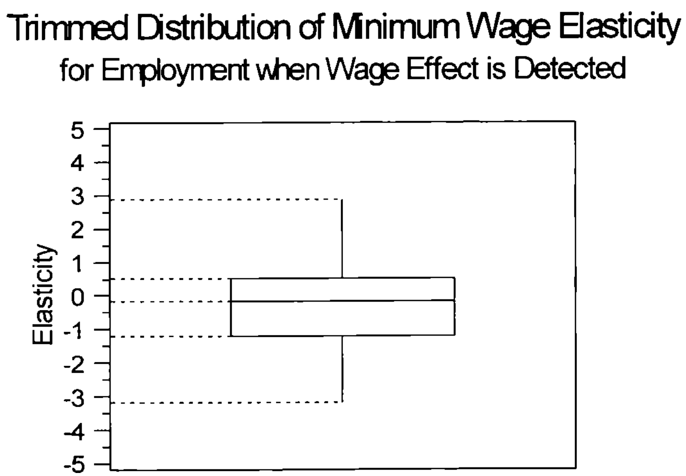


Figure 4
Employment Effects When Wage Effect is Detected







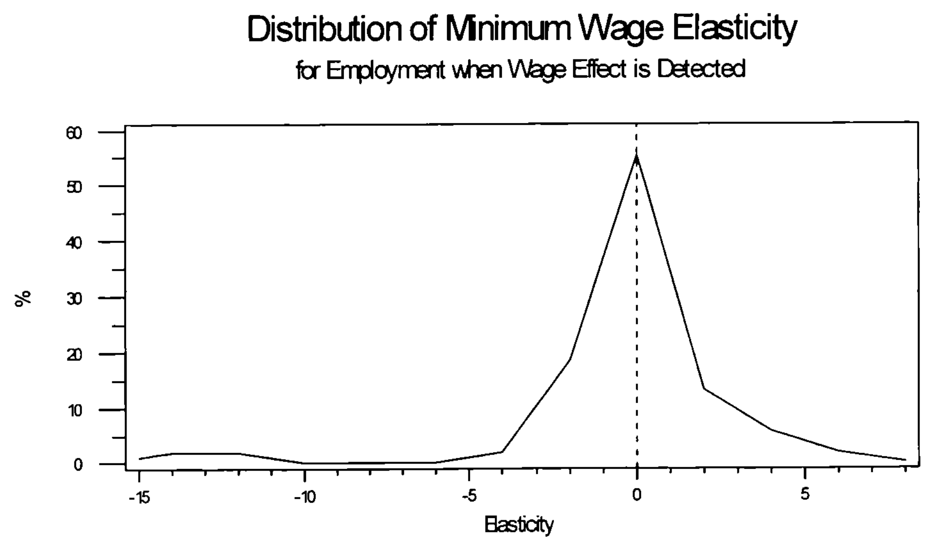
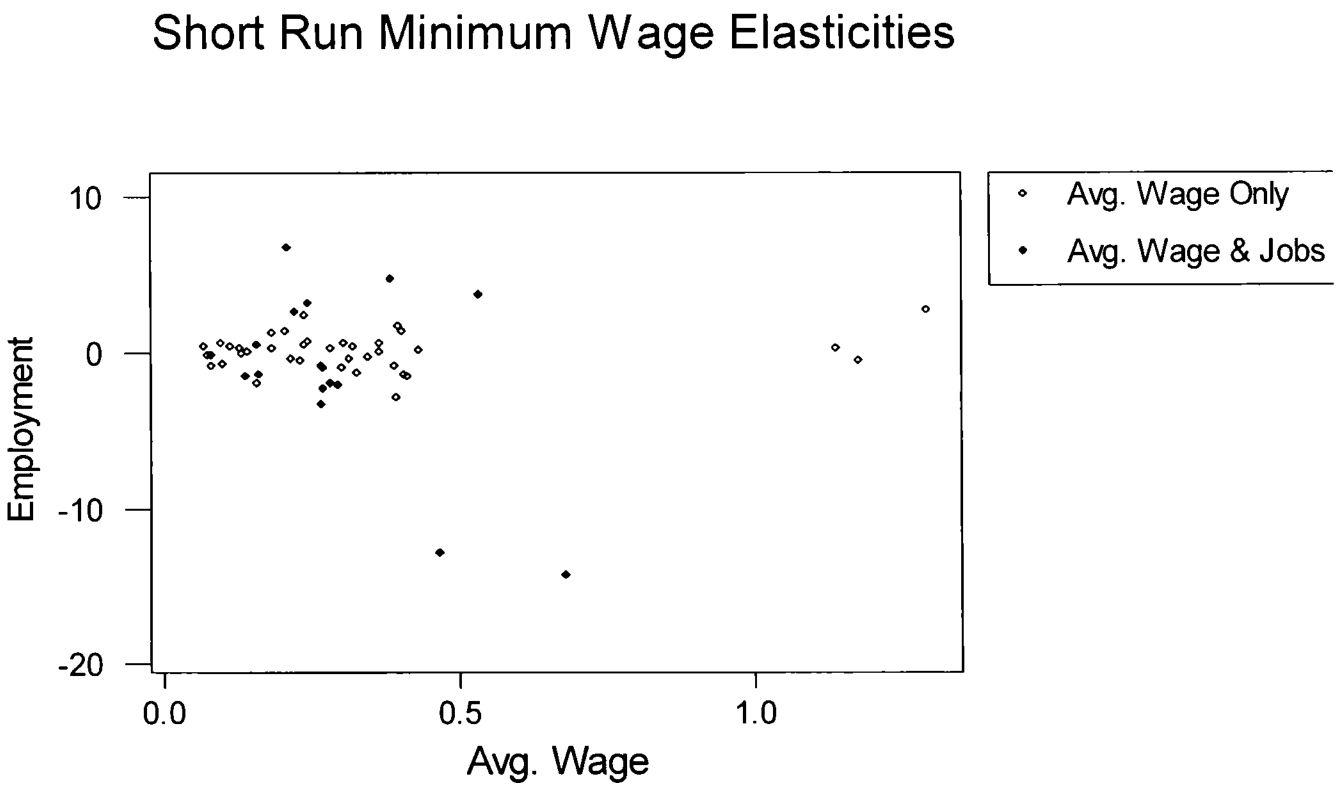


Figure 5


Short Run Minimum Wage Elasticities
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0
-10
-20
0.0
0.5
1.0
Employment
Avg. Wage
Avg. Wage Only
Avg. Wage & Jobs

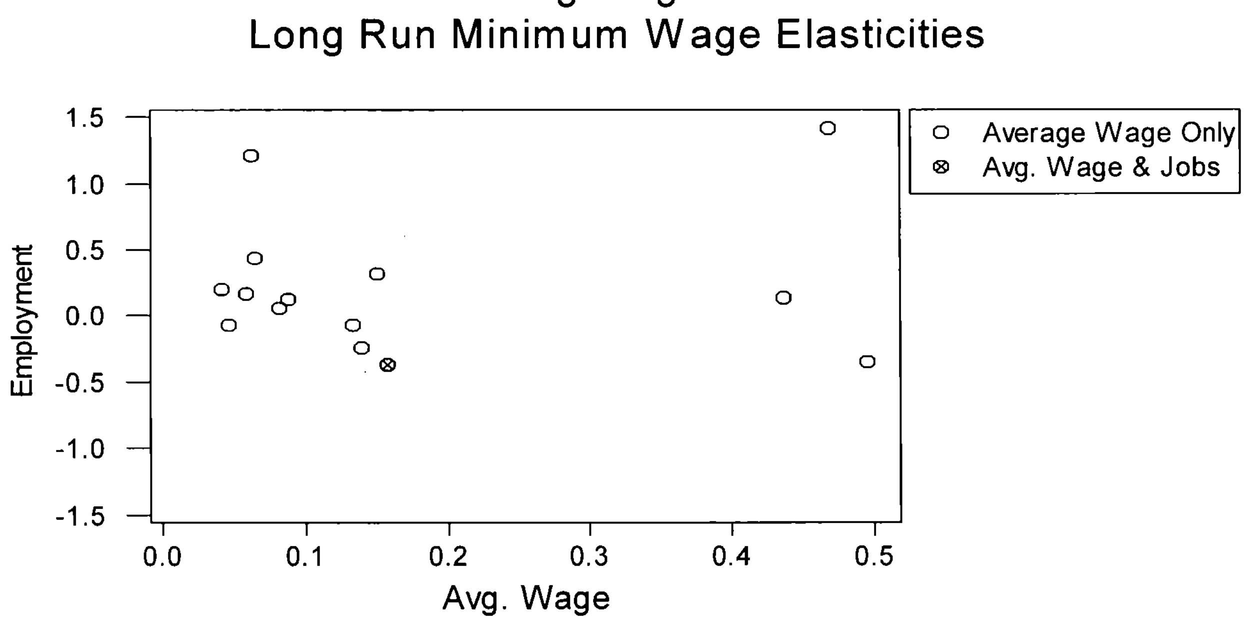
Long Run Minimum Wage Elasticities
1.5
1.0
0.5
0.0
-0.5
-1.0
-1.5
0.0
0.1
0.2
0.3
0.4
0.5
Employment
Avg. Wage
Average Wage Only
Avg. Wage & Jobs

TABLE 1

Fraction of Employees Earning Below the New Minimum Wage

SIC	Industry	91	90	81	80	79	78	76	75	74
Minimum Wage After Raise		$4.25	$3.80	$3.35	$3.10	$2.90	$2.65	$2.30	$2.10	$2.00
LOW WAGE INDUSTRIES:										
533	Variety Stores	9.1	27.0	16.2	16.2	54.5	59.8	21.6	14.6	50.1
562	Women's Clothing	m	m	27.2	27.1	40.2	46.8	26.5	22.8	33.5
835	Child Day Care	8.7	20.6	na	na	na	na	na	na	
566	Shoe Stores	5.5	13.4	11.0	9.9	47.6	25.8	10.3	9.2	32.6
565	Family Clothing	m	m	27.2	27.1	40.2	46.8	26.5	22.8	33.5
554	Gas Service Stn	10.2	15.7	21.0	21.3	49.9	46.6	23.0	19.1	42.1
546	Retail Bakery	7.5	20.0	12.4	11.6	s	61.0	s	s	s
238	Misc Apparel Str	8.9	15.0	32.1	32.2	38.9	42.3	33.6	31.7	33.1
525	Hardware Str	4.0	11.7	12.4	11.7	19.4	28.2	13.1	11.6	16.2
591	Drug Stores	4.8	12.4	21.8	20.5	38.0	45.0	21.9	18.9	32.8
721	Laundries	6.0	12.5	18.6	18.8	43.2	42.2	22.2	22.6	36.9
723	Beauty Shops	13.9	19.7	22.0	19.3	38.9	42.6	22.2	15.3	32.9
701	Hotels & Motels	8.5	14.2	45.6	35.4	49.1	45.9	29.3	32.6	48.7
734	Building Services	6.7	11.9	14.4	14.4	32.4	31.5	12.0	10.0	13.6
805	Nursing Facility	4.2	9.3	37.9	33.0	50.3	48.4	37.0	34.5	43.9
531	Department Str	4.2	9.7	39.3	37.5	33.5	33.3	36.2	35.6	28.4
225	Knitting Mills	3.7	9.2	9.1	7.4	26.9	25.4	10.7	10.3	18.6
599	Retail Stores	6.0	12.0	27.1	23.5	37.7	34.6	20.4	18.4	30.5
239	Misc Fabr Textile	2.3	3.9	8.1	9.8	29.7	28.0	8.8	9.1	22.7
561	Mens Clothing	m	m	27.2	27.1	40.2	46.8	26.5	22.8	33.5
553	Auto/Home Supply	1.5	4.7	10.8*	12.7*	25.2	21.0	9.7*	7.6*	9.7
YOUTH INDUSTRIES: by SIC										
58	Eating/Drinking Pl	25.4	35.2	78.4	74.2	61.6	63.9	73.4	68.0	53.6
541	Grocery Stores	6.4	13.2	34.1	29.2	23.3	21.4	26.2	28.8	18.1
311	Leather Tanning	-	7.4	s	s	s	s	s	s	s

m - missing
* - change in definition

na - not a distinct category in earlier CPS system
s - very small sample size

TABLE 2

Statistically Significant Elasticities

	91	90	81	80	79	78	76	75	74	68	67
Low Wage Industries by SIC											
533	0.23	.	.	**1.29**	**1.17**	**1.14**	0.53	.	.	.	.
562	0.23	.	.	.	.	**0.28**	0.38	.	.	.	.
317	.	0.27	0.40	.	.	**0.30**	0.41	.	.	.	.
399	.	.	.								
835	.	0.24	.								
566	.	.	.	.	.	.	.	.	0.32	.	.
565	.	.	**0.68**	0.47	0.39	.	.	.	.	.	.
554	.	.	.								
546	.	.	.								
302	.	.	.								
238	.	.	.								
594	.	.	.								
525	.	0.10	.								
244	.	.	.								
591	.	.	0.29	**0.40**	**0.39**	**0.27**	**0.24**	.	.	.	.
721	0.08	.	0.14								
316	.	.	.	.	.	**0.36**	.	.	.		
723	.	.	.								
701	.	.	.	.	0.22	**0.18**	.	0.31	**0.16**	.	.
734	.	.	.								
805	.	0.07	**0.36**								
531	.	.	.	0.32	**0.34**	**0.43**	**0.30**	.	.	0.16	.
225	.	.	.	**0.20**	0.13	**0.11**	.	.	.	0.09	.
599	.	.	.								
239	.	.	.	.	.	.	.	.	.	.	.
561	.	.	0.26	.	.	.	**0.40**	.	.	.	.
553	.	.	.								
Youth Industries by SIC											
56	.	.	**0.26**	.	.	0.13	0.21	.	.	**0.18**	0.16
541	.	.	.								
58	**0.14**	**0.08**	.	0.23	**0.28**	**0.21**	.	.	0.07		
311	.	.	.	.	.	.	.	.	.	.	.
753	.	.	.								

Estimates of e_1 in equation 1 (1a, 1b or 1c), when significant at a 5% level:

H_0: Minimum Wage Increase had no effect on the Industry Average Wage
H_1: Minimum Wage Increased the Industry Average Wage

Legend: **bold** - significant at 1% level
. - not significant at 5% level

	# events	a	b	Total	
Low Wage	175	20	23	43	(25%)
Youth	37	6	5	11	(30%)
Total	212	26	28	54	(25%)

Table 3

Employment Elasticity of the Minimum Wage When a Positive Wage Effect is Detected

	91	90	81	80	79	78	76	75	74	68	67
Low Wage Industries											
533	2.48			2.81	-0.46	0.28	**3.76[a]**				
562	0.55					0.31	**4.73[a]**				
317		-0.88	1.79			0.61	-1.51				
835		**3.20[a]**									
566									-1.27		
565			**-14.3[a]**	**-12.8[a]**	-2.81						
525		-0.71									
591			**-1.96[b]**	-1.32	-0.80	**-2.20[a]**	0.74				
721	-0.80		**-1.43[a]**								
316						0.61					
701					**2.61[a]**	0.36		-0.38	**0.54[c]**		
805		0.45	0.09								
531				0.48	-0.23	0.26	-0.91			**-1.33[c]**	
225				1.47	0.33	0.44				0.61	
561			-0.75				1.49				
Youth Industries											
56			**-3.19[a]**			0.00	**6.75[a]**			1.34	-1.86
58	0.11	-0.09		-0.47	**-1.86[a]**	-0.29			-0.12		
Average: (weighted)	0.22	0.51	-2.37	-0.68	0.26	-0.19	2.14	-0.38	0.29	-0.79	-1.86

a: significant at 0.01 level (2 sided)
b: significant at 0.05 level (2 sided)
c: significant at 0.10 level (2 sided)

	# events	total -	a-	b-	c-	total +	a+	c+
Low Wage	43	6	4	1	1	5	4	1
Youth	11	2	2			1	1	
Total	54	8	6	1	1	6	5	1

Notes

[1]In a series of studies, Card, Katz and Krueger have concluded that several recent changes in the minimum wage had no noticeable adverse effect on employment (Card 1992a, 1992b; Card and Krueger 1994, 1995; Katz and Krueger 1992; Krueger 1995). Neumark and Wascher (1992, 1995a and 1995b), Currie and Fallick (1993), Kim and Taylor (1995), and Deere, Murphy and Welch (1995) present contrary evidence. Many of these writers have sharply criticized each others' analyses (Card, Katz and Krueger 1994, Ehrenberg 1992, Deere, Murphy and Welch (1995), and Neumark and Wascher 1994, 1995c).

[2]This takes compliance and enforcement largely for granted: see Ashenfelter and Smith (1979).

[3]Presumably, employers determine levels of wages, fringe benefits and other expenses that minimize overall labor costs. When minimum wage legislation raises wages, partial offsets may well be possible by reductions in other costs but the possibility of complete offsets would be a surprise. That would suggest behavior that is not cost-minimizing, a more serious contradiction of the neo-classical model than the minimum wage literature typically contemplates: but see Reynolds (1965).

[4]All of the industries have sufficiently long time series for examination of the minimum wage increases of 1981, 1990 and 1991. Only fifteen are long enough to study increases back through 1974, and only thirteen extend far enough back for analysis of the 1967 and 1968 increases.

[5]Our data source do not provide industry wage distributions; instead we use average hourly wages to proxy the proportion of employees at or near the minimum. We consider this issue further in our discussion of Table I.

[6]The 1989 ORG files, a compilation of earnings records of the 1989 CPS, have 650,000 observations and include wage data for all employed participants.

[7]We based our selection of Youth industries on age distributions for 3 digit industries, constructed from the 1989 Out-Going- Rotation file from the BLS. Several industries that satisfy these criteria are not among those analyzed because of insufficiently long time series. For example, automotive rentals (SIC 751) and dairy product stores (SIC 545) both satisfy the youth criterion, but wage data are available only from the mid 1980s.

[8]The Consumer Price Index (CPI) and Unemployment Rate (UN) are from the BLS.

[9]The May CPS is used for increases prior to 1980, the larger ORG files are used from 1980 on. CPS industry classifications are broader than the SIC system used in the EEH data and some CPS industries aggregate several SIC industries. Table I is limited to industries in which CPS and SIC industries are closely matched. Although the CPS includes all employees, the EEH is limited to production and non-supervisory employees, about 80% of total employment. The fraction of EEH 'employees' below the minimum wage will be larger than that found in the CPS.

[10]Until 1981, Congress raised the minimum wage when it fell below 40% of the average manufacturing wage. By the time of the March 1990 increase, the minimum wage stood at 31.4% of the manufacturing average; in April 1991 it was 33.6% of that average. The long period in which the minimum wage remained unchanged, combined with market pressures on wages over the intervening nine years, greatly reduced the fraction of employees below the new minimum.

[11]While a structural model would use available information more efficiently, part of the current controversy revolves around the specification and measurement of the variables in structural equations (Card, Katz and Kreuger, 1994). Much as reduced form equations, a forecasting model avoids such

disputes.

[12]In keeping with contemporary time series techniques, we conduct tests for trend stationarity, unit roots and cointegration in each of our 212 samples. The logarithms of the CPI, the unemployment rate and the minimum wage all display unit roots, so we test for cointegration between these variables and the dependent variable when it has a unit root.

A Granger causal relation runs from the Consumer Price Index (CPI) to the minimum wage, but not from the minimum wage to the CPI, for both the full sample period and for all but two of the nine year subsamples. There is no evidence of Granger causation in either direction between the Unemployment Rate (UN) and the minimum wage in the full sample or in any sub samples. The inclusion of inflation in the equation avoids confusing the impact of inflation with that of the minimum wage, without masking minimum wage effects.

[13]For the average wage, the number of degrees of freedom varies from 66 to 73 degrees of freedom. Five of the average wage series are cointegrated with the RHS variables: SIC 554, Gasoline Service Stations; SIC 302, Rubber and Plastic Footwear; SIC 594, Miscellaneous Shopping Goods; SIC 805, Nursing Facilities, and SIC 553, Auto and Home Supply Stores.

For employment, the number of degrees of freedom for the unrestricted regression varies from 45 to 59, depending on the sample period, the industry, and the results of the AWS and cointegration tests. Three of the employment series are cointegrated with the CPI and nominal minimum wage: SIC 244, Wood Containers; SIC 316, Hotels and Motels; and SIC 239, Miscellaneous Fabric and Textile Manufactures. The restricted regressions have 9 to 13 more degrees of freedom.

1990 has a large number of degrees of freedom because no increases in the Federal minimum wage occurred during the preceding 8 years, so the d coefficients are set to zero. 1981 has a slightly smaller number of degrees of freedom for industries that have a unit root and/or begin in 1972. One more degree of freedom is lost whenever either a trend term is needed to stabilize any variable, or employment is cointegrated with any of the other variables.

[14]MWx is not included inthe equation that studies the 1990 increase since no change occurred in the previous eight years.

[15]Splitting the minimum wage variable controls for increases in the minimum occurring prior to the increase under examination, and allows us to interpret the sum of the coefficients e_1 as the minimum wage elasticity of employment (in this industry at the time of the change in the minimum wage).

[16]Minimum wage increases have taken place on the first or third of the month since March 1956: before then, the last week of the month was the practice.

[17]This may not be the case were industry specific prices used.

[18]The twelve month lag used in this research is consistent with the literature. Most studies find that employment adjustments are completed within four quarters (Hammermesh, 1993), Neumark and Wascher (1992) also use a one year adjustment period.

[19]Outside values, points more than one and half times the inter-quartile range away from the first and third quartile, are not shown to allow larger scale.

[20]Finally, there is little evidence of a pattern of effects by industry or year; there is some clustering of negative and significant outcomes in 1981 and in the family clothing (SIC 565) and drug stores (SIC 591), but such clustering is sparse and likely due to chance.

[21]Statistically insignificant point estimates of the employment elasticity are set to zero for this calculation.

[22] The scatterplots in figure 5 make this especially clear. For observations with a statistically significant and positive elasticity of the average wage with respect to the minimum wage, both graphs show this elasticity and the corresponding employment elasticity. The upper plot shows short run elasticities for both variables; observations with a statistically significant employment elasticity, with an "x" in the middle of the circle, appear darker. Except for eight observations, all form a shapeless nebula, with no discernable internal pattern. Of those eight, 3 have positive and statistically significant employment elasticities, and lie largely above the nebula. Two have statistically significant and unusually large, negative employment elasticities. These two, and one of the previously mentioned three observations have 3 of the six largest wage elasticities. But the three observations with far and away the largest wage elasticities do not have statistically significant employment elasticities.

The second scatterplot is the same graph, except using long run rather than short run elasticities. It should not be surprising that far fewer industry/wage pairs show evidence of statistically significant wage effects, only 14, nor that both types of elasticity are much smaller (compare the scales of the axes in the two graphs), nor that only one of the observations that passes this verison of the wage filter has a corresponding employment elasticity that is statistically significant. During this period, inflation reduces the real minimum wage over time. The average wage elasticity for the observations with statistically insignificant employment elasticities exceeds that of the one observation with a statistically significant employment elasticity, due to the three observations with the largest wage elasticities.

[23]Comments at the AEI symposium on the minimum wage, June 1, 1995.

[24]See Stiglitz and Weiss (1981), and Jaffee and Russell (1976).

A Time Series Analysis of Employment, Wages and the Minimum Wage

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Recent empirical research indicates that increases in the minimum wage have no significant impact on employment. This study tests for the employment effects of minimum wage increases by applying a Box-Jenkins forecasting equation to time series employment data in thirty-four Low Wage and Youth industries for the eleven increases in the federal minimum wage between 1967 and 1991. Testing for Granger causality from the minimum wage to employment and for the sign of the effects, we find little evidence that increases in the minimum wage systematically reduce employment. Increasing the power of the tests to better distinguish between small and no employment effects does not alter these outcomes. The results are consistent with the core findings of the 'new minimum wage' research and with older theories of the minimum wage associated with Lester and Reynolds.

A Time Series Analysis of Employment, Wages and the Minimum Wage

The recent round of research on the employment effect of the minimum wage has produced conflicting results. Findings have ranged from elasticities in the 'consensus' range, -0.1 to -0.3, to positive employment impacts.[1] This difference has generated controversy, much of it centering on methodology. Indeed, methodological innovation is a distinctive feature of this work. All research has examined populations likely to be sensitive to increases in the minimum wage. To this end, different analyses have studied longitudinal data on low wage individuals, applied panel data techniques to annual teenage employment rates by state, and measured employment shifts in fast food establishments across minimum wage changes. Still, as Card and Krueger (1995, ch. 6) note, "The main evidence usually cited to support the claim of adverse employment effects of the minimum wage is based on time-series analysis ..."

Developments in time series analysis in the last decade and a half — notably the recognition of unit roots and cointegration -- suggest that a new look at time series data may have large returns. To apply these techniques, we estimate Box-Jenkins forecasting models for employment and wages in 34 industries that should be particularly sensitive to the minimum wage: 27 industries with low average wages and 7 industries in which the fraction of young workers is quite high. Doing this separately for each minimum wage increase between 1974 and 1991, we have a total of 234 industry/minimum wage 'events'. The model incorporates controls for concurrent macro-economic events and further controls for mis-specification by benchmarking the estimates for the 34 'experimental' industries against those from 6 industries which should be insensitive to minimum wage increases.

Our initial estimates do not indicate a consistent relationship between increases in the minimum wage and employment. We examine the robustness of our analysis by examining two additional issues: whether the tests are sufficiently powerful to discern the very small employment effects suggested in some research and whether the lack of an employment effect is attributable to the minimum wage having no detectable impact on wages in the industries under study. Although the power of the initial tests is low, recasting the tests to conduct a "fair horse race" between the null of no effect and the alternative of the consensus effect, an elasticity of -0.3, does not alter our results. Estimates of the wage effect find a consistent and positive response in the target industries, especially the low wage ones, and all but none in the high-wage control group. Again, the lack of consistent employment effects can be attributed neither to the insensitivity of the techniques employed nor to the insensitivity of relevant wages to the minimum wage.

Our conclusion, in keeping with much of the new minimum wage research, is that there is little evidence that minimum wage increases of the magnitude likely to be legislated will affect employment. This is consistent with the work of Reynolds (1960) and Lester (1963), which suggest that the tenuous link between the minimum wage and employment may be attributed to information and transaction costs - particularly monitoring and adjustment costs, employee heterogeneity, and the dominant effects of larger economic trends.[2]

II. Data: Sources and Preliminary Analysis

Our research focuses on employment in two groups of industries that should be sensitive to the minimum wage: industries with average wages close to the minimum and industries which employ large numbers of teenager and young adults.[3] The first group of industries, which we call the 'Low Wage' industries, consists of 27 three digit SIC industries whose January 1990 average hourly wage of production and non-supervisory employees was less than 210% of the new minimum wage of $3.80 (less than $7.98).[4] They have 13,915,000 employees and represent 15.5% of total private employment. Among these industries are wooden containers (SIC 244: 43,100 employees in January 1990), hotels and motels (701: 1,520,100), janitorial building services (734: 805,200), department stores (531: 2,221,200), and personal leather goods (317: 15,200).

The second group, designated 'Youth' industries, consists of the 7 industries, five 3 digit and two 2 digit SIC industries, in which at least 30%

of the labor force is under age 25 in 1989. These industries are leather tanning and finishing (SIC 311: 15,000 employees in January 1990), automotive rentals (751: 171,900), auto repair services (753: 522,200), grocery stores (541: 2,868,800), dairy product stores (545: 22,300), retail apparel stores (560: 1,222,200), and eating and drinking establishments (580: 8,395,000).

We perform the same analyses on 6 benchmark industries which, because of their high wages, should be insensitive to changes in the minimum wage. These are Electrical Work (SIC 173: 543,600 employees), Blast Furnaces (331: 276,300), Automotive Vehicles (371: 727,700), Advertising (731: 236,100), Radio and Television Broadcasting (483: 234,700), and Engineering and Architectural Services(871: 780,500). In January 1990, average wages in these industries were three to four times the minimum wage.

Observations on employment come from the Employment, Earnings and Hours (EEH) series of the United States Bureau of Labor Statistics (BLS). These series, based on a survey covering 40% of payroll employment, provide monthly data on industry employment by three digit SIC code. The longest series start in the 1920s and many begin in the 1950s, but shifts in industry definitions and the introduction of new industries make some quite short.[5] We were able to examine the increases in the federal minimum wage of 1991, 1990 and 1981 for all of our industries and were able to examine minimum wage increases as early as 1967 for nearly half our sample. There are 234 interventions in the Low Wage and Youth industries, where an intervention is an industry-minimum wage change pair. Data on the minimum wage are taken from Kaufman (1991).[6] We based our selection of Youth industries on age distributions for 3 digit industries constructed from the 1989 Out-Going-Rotation file from the BLS.

IIa. Exploratory Analysis of the Data

According to standard theory, the individuals who are at risk from an increase in the minimum wage are those earning less than the new minimum. If these individuals constitute a large fraction of an industry's labor force, the employment consequences should be readily detected; if very small, detection may be difficult. Table I presents estimates of this fraction for (most of) the industries that we examine in the year prior to the increase.[7]

Two patterns emerge. First, a substantial fraction of employees in Low Wage and Youth industries earned less than the 'new' minimum wage in the year preceding the increase. For example, in 1981 this fraction ranged from 8.1% (SIC 239, miscellaneous fabricated textile products) to 78.4% (SIC 56, eating and drinking places); most fall between 9% and 27%. Only 7.3% of the labor force as a whole were bound by the minimum wage in that year. For other years, particularly prior to 1981, similarly large fractions of the labor force earned less than the new minimum wage.

The second pattern is the decline in the fraction of employees below the new minimum over time. In the 1970s and early 1980s eating and drinking establishments had between 68% and 78% of employees below the new minimum. This fraction had fallen to 35% by the 1990 increase and 25% by the 1991 increase. The decline in the fraction of employees below the new minimum is consistent with the declining real value of the minimum wage during the 1980s.[8] Although several of the industries under study had a very small fraction of employees below the 1991 minimum wage, the figure for most of our industries was between 4% and 10%.

A threshold issue for this research is whether our data indicates that changes in the minimum wage influenced employment and wages. We use a one sided prediction t-test to determine whether employment was substantially lower after the minimum wage than in the preceding months for each of the 234 minimum wage/industry interventions in our data set.[9] The critical region for the test is in the left tail.[10] The top row of Figure 1 displays distributions of p-levels for each industry group. The first of these graphs is a box-plot for each of the three industry groups as well as a 'null' distribution of p-values constructed to represent no employment effect.[11] The box plots are arranged so that the midpoint line in the box is the median, the lines at the top and bottom of the box are the boundaries of the second and third quartile, the ends of the lines emerging from the box are the boundaries of the first and fourth quartile. As is readily seen, the p-values for the Low Wage and Youth industries lie below that of the null distribution. This is consistent with employment declining after the change in the minimum wage. However, the High Wage industries box plot is also below that of the null distribution suggesting that employment may also have declined for reasons other than the increase in the minimum wage.

The graphs next to the box plot display cumulative distributions of p-levels for each sample alongside a 45 degree line. If the minimum wage had no effect, we would expect the p-values to be uniformly distributed and lie along the 45 degree line. If the minimum wage reduces employment, the distribution should be above the 45 degree line starting near to the left hand side of the graph. If there were a positive employment effect, the p-values would lie below the 45 degree line. For the Low Wage and Youth industries, the left tail of the distribution is clearly heavier than one would expect if employment did not fall following increases. The High Wage industries show a different pattern; the distribution of p-values follows the 45 degree line quite closely for roughly the first third of the distribution, but thereafter are too highly concentrated around (what should be) the median. Although there is some evidence of a negative employment shift in the High Wage industries, it is weaker than that in the Low Wage or Youth industries.

The bottom row of Figure 1 duplicates this exercise for the average wage. The analog to an employment decline in figure 1 is a wage increase following an increase in the minimum wage. The box-plot in the upper left shows a very pronounced response in the Low Wage industries, with the third quartile below where we would expect to find the median if wages did not rise following a minimum wage increase. The response of Youth industry wages is less certain. While the median and third quartile are shifted down, the left tail, which is where the critical value lies, is smaller than would be expected if there were no response. The distribution of the High Wage industries is higher than that of the null distribution, indicative of no discernable wage effect. We have found an increase in the wage and a decline in employment in industries where theory suggests that these responses are most likely, and not elsewhere (i.e., the High Wage control group). This suggests that the EEH data is sufficiently accurate to detect the effects of interest. This analysis does not, of itself, constitute an adequate test of minimum wage effects as we have had to assume that the data is *iid,* that wages and employment are not serially correlated, that the minimum wage changes were similar in magnitude (which they are not), and that employment and wages are not subject to seasonal fluctuations. We now turn to regression analysis to rectify these problems.

III. Econometric Framework

The Box-Jenkins framework that underlies our analysis begins with the presumption that industry employment is serially correlated, and that this can be exploited in forecasting. Much previous research on employment suggests that macroeconomic conditions (the unemployment and inflation rates, and a dummy for whether the economy is in recession) contain additional information that improves the forecasts (see Brown et al.)[12] The current minimum wage is required to test whether it improves forecasts, while past values of the minimum are needed to control for the effects of prior increases. The resulting equation is (with all variables in logs):

$$1)\qquad N_t = \sum_{i=1}^{12} a_i N_{t-i} + \sum_{i=1}^{12} b_i CPI_{t-i} + \sum_{i=1}^{12} c_i UN_{t-i} + \sum_{i=0}^{11} d_i MWx_{t-i} + \sum_{i=0}^{11} e_i MWy_{t-i} + X_t$$

where N is employment in a 3 digit SIC industry, CPI is the consumer price index and UN is the unemployment rate. The sample period for each equation is 8 years prior and one year after each minimum wage increase. MWx is the nominal minimum wage for the eight years before the specific change under study; for the twelve months following this event, it remains at the last value of the minimum wage prior to the event. MWy, the measure of the minimum wage change 'event', is the value of the minimum wage throughout the entire nine year period.[13] Because of the timing of the minimum wage increase relative to the sampling period of the survey, we use the contemporaneous minimum; the inclusion of eleven lags is based on the belief that changing the amount of labor involves convex adjustment costs for the firm. Consequently, the minimum wage variables have lags 0 through 11, rather than 1 through 12.[14] **X** is a vector consisting of a constant, a dummy variable equal to one during months that the NBER has identified as periods of recession, and up to two additional variables (described below).

We use these equations to determine whether information about the Federal minimum wage improves forecasts of employment, once all the other information is included. Although a structural model would make more efficient use of available information, part of the current controversy revolves around the specification and measurement of the variables in structural equations (Card, Katz and Kreuger, 1994). Much as reduced form equations, a forecasting model avoids such disputes. A wage variable would presumably improve employment forecasts but is omitted from this model. The average wage is affected by the minimum wage; including it would duplicate and potentially mask the employment effect of the minimum wage. This specification may result in an over estimate of the employment effect of the minimum wage but, in doing so, assures that minimum wage effects have not been attributed to other variables.

Most studies of the minimum wage have used relatively long time series. An implicit, typically untested, assumption of these studies is the stability of the underlying structure of industries over the period. Skepticism on this score leads us to restrict our sample periods to no more than nine years. The monthly employment series from Employment, Earnings and Hours provide ample observations for the sample period.

Why use the nominal minimum wage when the real minimum wage is the relevant variable? There are three answers. First, the most desirable deflator would be an industry specific index of production prices but these are seldom available before the 1980s. Second, the combination of the logarithms of the nominal wage and the CPI approximates the real minimum wage. Third, in our sample period the real minimum wage rises only when the nominal minimum rises, and this specification captures such increases.[15]

Before turning to substantive analysis, it is necessary to test whether the variables exhibit any trends during the sample period.[16] If any of the variables is trend stationary, X includes a linear time trend. First differences replace levels of any variable displaying a unit root. The CPI, and minimum wage both have unit roots, but not the unemployment rate; when industry employment also contains a unit root, we test for cointegration with these other two. Where we find it, we include one lag of the residual from the cointegrating regression in X (the error correction mechanism).[17]

Equation (1) allows us to examine the employment effect of the minimum wage in two different ways. First, we test for Granger causality for the entire period (eight years before and one year after the change) with an F-test of the $\{e_i\}$ coefficients and, where included, the error correction term.[18] This is essentially a test of whether information about the minimum wage improves employment forecasts.

A significant Granger test can result from either a positive or negative employment response to the minimum wage. According to conventional analysis however, the employment effect is strictly negative. We calculate the elasticity of employment with respect to the minimum wage for each industry and minimum wage pair as the sum of the e coefficients, $\sum e$, and use an asymptotic t-test to determine its sign and significance.

A limitation of the Box-Jenkins methodology is that the minimum wage variable may proxy coincident events that affect employment but are not included in the equation. Inclusion of the macroeconomic variables is one step toward correcting this problem. A second is to benchmark the Low Wage and Youth results against those from industries which have few workers at or near the minimum wage, on High Wage, or control group. Bench marking is particularly important as a control for seasonal employment effects. Most minimum wage changes coincide with well established seasonal shifts in employment.[19] One solution would be to include seasonal controls but this could mask minimum wage effects. Inclusion of the High Wage industries allows us to filter out false positives associated with normal seasonal fluctuations.

Another issue is whether these tests, which have limited degrees of freedom, are sufficiently powerful to distinguish between the consensus employment effect and a null of no effect. We return to this question after reviewing some initial results.

IV. Results

How well do these equations capture movements in the data? When the equation is constrained so that each $d_i = 0$, the median adjusted R^2 statistics is 0.73, the mean is 0.64 and the standard deviation is 0.31.[20] The fit for the Low Wage and Youth industries is somewhat better, that for the High Wage industries somewhat less good.[21] R^2 is not a statistical test, but it is encouraging that these results are similar to those found in research using contemporary time series techniques.

Results are presented in Tables II - IV. Table II summarizes the estimated employment elasticities and test results. Tables III and IV report results by year and industry for the Granger and Sum of Lags tests, respectively. In the latter tables industries are sorted within group by 1990 average wage and presented in ascending order within each category. Our discussion begins with the employment elasticities (Figure 2 and Table IIa), and proceeds through the remaining material in the order of the tables while referring to Table IIb to track the distribution of results for each test. Except where noted, the discussion pertains to Low Wage and Youth industries.

A. Employment Elasticities (Table IIa)

The employment weighted median elasticities ($\sum e_i$) shown in Table IIa are within the consensus range, -0.1 - -0.3. The median for the Low Wage and Youth industries is -0.15 with an inter-quartile range of 2.06. Two of the three largest (negative) effects occur in recession years, 1975 and 1980, but the largest occurs in 1967, and three years show positive effects. Both the annual medians and especially the inter-quartile ranges strongly indicate considerable variability in the effects of the minimum wage. The elasticity of the High Wage industries is very close to zero, 0.0006 (1.69).

The left hand side of Figure 2 shows the distributions of the minimum wage elasticities of employment for our three industry groups. The first box-plot, for employment, shows that the range of estimates is quite large, from about -15 to 10 for the Low Wage and Youth groups. The range for the High Wage industries is, surprisingly, more skewed negative. The second box-plot, in which the outside values are trimmed, shows that nearly all the estimates for the Low Wage and Youth groups lie between -5 and 4, and -3 and 2 for the High Wage group. In both cases, the median is small and negative (-0.16 and -0.14 for the Low Wage and Youth, and for the High Wage, respectively). Beyond the difference in (trimmed) range and a very slight shift upwards for the High Wage group, there is very little to distinguish the 3 groups from each other. This is further borne out in the frequency distribution, in which the combined Low Wage and Youth elasticities almost perfectly overlay the High Wage elasticities.

B. Granger Test (Table III)

The Granger test examines whether the minimum wage contains information over and above that of other RHS variables that improves forecasts, without regard to the direction of the impact. The summary in Table II shows that 18% of the trials produce statistically significant test statistics.[22] In the Low Wage industries, 15% of the 189 statistics reject the null of no Granger causality at a 10% level, 10% do so at a 5% level, and 2% at a 4% level. Of the 45 Youth industry test statistics, 31% are significant at a 10% level, 16% at a 5% level, and 4% at a 1% level.

The top row of Figure 3 shows the distribution of p-values for the Granger test. The box-plot indicates very little difference between either the Low Wage or Youth industries and the null distribution of no effect. The Low Wage inter-quartile range is slightly larger than it "should" be, but is nearly symmetric around 0.55, and the first quartile of the Youth industries is compressed. The distribution of High Wage p-values is shifted up, relative to the null distribution. The distribution plot to the right shows that the Low Wage industries follow the 45 degree line very closely, with somewhat more values in the lower and upper ranges, and slightly fewer in the central range than might be expected. The Youth industries tell a different story, with many more small p-values than if there were no effect. The evidence suggests that taking account of the increase in the minimum wage has been useful in forecasting Youth employment.

Table 3 gives more detailed information about results that are statistically significant at the 10% level. The significant tests do not form any readily detectable pattern by industry, by year, or by the magnitude of the increase in the minimum wage. Of the Low Wage industries, only Knitting Mills (SIC 225), shows particular sensitivity to the minimum wage, with more than half, 8 of 11 tests, statistically significant. In contrast, fifteen industries have no significant tests and an additional four have only one significant test. The year in which the largest fraction of Low Wage industries show a statistically significant response is 1976: 36%. The next largest number of significant responses occurs in 1974, when 4 out of 14 tests, 29%, are statistically significant. Three years, 1967, 1968, and 1979 have only one significant response.

There is also no simple correspondence between either the size of the minimum wage increase or the fraction of employees below the new minimum wage and the proportion of significant tests. The year with the largest increase in the minimum wage, 1974 at 25%, has the second largest fraction of statistically significant responses; but 1968, with the second largest increase (14%) has only one (8%) significant response. The next largest increases were 13% (1990), and 12% (1967 and 1991). Fifteen percent of trials are significant in 1990, 7% in 1991 and 8% in 1967. The fraction of employees below the minimum wage is also not closely linked to the number of significant tests. The proportion of employees below the new minimum wage is smallest in 1991 and the number of significant tests is also small (7%). But the year with the next lowest fraction below the minimum wage, 1990, has an average percentage of statistically significant tests (15%).

The pattern is similar for the Youth industries. Leather Tanning (SIC 311) is relatively sensitive (6 out of 11 trials), as is Eating and Drinking Establishments (SIC 58) with 5 of 11 significant trials, but the others are not.

In the High Wage control group, 6% of the trials are significant at the 1% level, 9% at the 5% level, but 12% at the 10% level. These all suggest that the controls for coincident factors have not been entirely successful - not surprising given the problematic nature of seasonality - and that part of the measured response of employment in the Low Wage and Youth industries is likely due to such coincident factors. Adjusting for the control group, the increase in the minimum wage is affecting employment in about 6% of the trials of the Low Wage and Youth industries, rather than 18% (using a 10% criterion for significance). Given the *a priori* reasons for selecting these industries, this is a weak result.

C. The Sum of Lags (Table IV)

Granger tests indicate whether or not a relationship exists between the minimum wage and employment but do not indicate its sign. A more stringent test, one in keeping with accepted theory, is whether the sum of the minimum wage coefficients is negative. The Sum of Lags differs significantly from zero in 29% of the interventions, but the expected negative effect is found in only 19% of the interventions; 10% are positive.

The second row of graphs in Figure 3 displays results for this test. The box-plot indicates that the p-value of all 3 groups are shifted down relative to the null distribution, with Youth being shifted the most and High Wage the least. Both the box-plots and the distribution graph on the right make clear that the difference between the High Wage and the other groups is small in the lower tail.

Patterns of significance by industry and year are less pronounced than in the Granger tests and the signs of the Sum of Lags are notably mixed. About two-thirds of the industries in the sample (31 out of 34) have at least one significantly negative test, and in only one industry are more than half the trials significant and negative (SIC 235, Miscellaneous Apparel Stores). Twelve of the industries with negative tests also have positive tests and in four industries the only significant statistics are positive.

The only year that has both more than two significant sums of lags and no significant positive sums is 1967. There is also no evidence of a relationship between either the size of the increase in the minimum wage or the proportion of employees below the new minimum, and negative employment effects. The year in which the proportional change in the minimum wage was largest, 1974, has both positive and negative significant sums. The weakness of the relationship between the magnitude of the employment elasticity and the proportion bound is confirmed by the lack of significance of the regression coefficient of the former on the latter.[23]

The proportion of statistically significant results for the control group of High Wage industries is slightly less than that of Low Wage and Youth industries. 26% of the High Wage sums are significantly different from zero (against 29% for the other industries), but more of these are positive than negative: 15% against 11%, respectively, compared to 10% and 19% for the Low Wage and Youth industries. After benchmarking these results against the High

Wage results, we find a statistically significant negative employment effects in only 8% of the trials. This point is illustrated in the graph at the bottom left hand side of Figure 2, the distribution of estimated elasticities for the Low Wage and Youth industries – the solid line - and the High Wage industries, the dashed line. The two distributions lie virtually on top of one another with the weight of the Low Wage and Youth distribution lying only slightly to the left of the High Wage distribution. Their similarity suggests that much of the apparent effect of the minimum wage in the 'experimental' industries is in fact concurrent economy wide events.

V. Some Confounding Factors: Wage Effects and the Power of the Tests

To this point, there is only weak evidence that increases in the minimum wage systematically reduce, or even affect, employment in Low Wage or Youth industries. The fraction of significant Granger test statistics, and Sum of Lags statistics that are negative and significant is reduced once benchmarked to the High Wage industries. Such results may not be seen as decisive. It may be that employment effects are proving elusive not because the minimum wage does not influence employment, but because it either does not affect the wage or the wage effects are not detectable in this data. It is also possible that our tests are not powerful enough to distinguish an elasticity in the -0.1 to -0.3 range from zero. We address both of these issues in turn.

Va. Average Wages and the Minimum Wage (Table VII)

It may be that employment effects are proving elusive either because the minimum wage is not affecting wages, or if it is, because the data does not capture these changes. The declining fraction of the labor force affected by the minimum wage, documented in Table I, suggests that the minimum wage may be becoming both of decreasing importance in wage determination and increasingly difficult to detect.

We have already seen preliminary evidence of wage effects from the prediction t-tests in Figure 1. Do these disappear when we apply the more sophisticated Box-Jenkins apparatus? Using an analogous forecasting equation where W is the industry average wage, we examined the elasticity of the average wage with respect to the minimum wage:

$$(2) \qquad W_t = \sum_{i=1}^{12} a_i W_{t-i} + \sum_{i=1}^{12} b_i CPI_{t-i} + \sum_{i=1}^{12} c_i UN_{t-i} + dMWx_t + eMWy_t + X_t$$

The wage equation differs from the employment equation in two respects. Because minimum wage legislation is takes effect on a particular date, we anticipate an immediately response in the average wage and include no lags of the minimum wage variables. Also, in keeping with the logic of Box-Jenkins estimation, the lagged values of the average wage replace those of employment.

A relationship between increases in the minimum wage and the average wage is inherently weaker, and more difficult to detect, than the relationship between the minimum wage and employment. The mean wage of production employees, the wage available in the EEH, is an inert measure of the effect of increases in the minimum wage on employee wages. The magnitude of the response will depend on the fraction of the labor force bound by the minimum wage, the gap between the minimum wage and the wages of those earning above the minimum, and whether increases in the minimum wage affect the employment or earnings of the bound labor force.[24] Although not inherently undetectable, the response of mean wages will be smaller even where there is an employment affect. We would therefore expect our wage estimates to produce fewer significant results than were obtained from the employment equations. If the Box-Jenkins technique can find wage results, it should be capable of finding employment results where they exist.

The estimated wage elasticities and their statistical significance indicate a strong relationship between increases in the minimum wage and increases in the average wage. The median elasticity of the average wage with respect to the minimum is 0.11 for Low Wage industries, 0.03 for Youth industries. In contrast, it is slightly negative (-0.0128) in the High Wage control. The bottom graphs in Figure 2 bring out most clearly the differences between the distributions of elasticities for employment and for the average wage. Although there is overlap between the High Wage estimates and the Low Wage and Youth estimates, the latter are strongly skewed toward the positive side of graph, while the distribution for the High Wage industries is symmetrically distributed around zero. This contrasts with distributions of the employment elasticities, immediately to the left in Figure 2, where the distributions lie virtually on top of one another.

Estimates of the sign and significance of the wage elasticities are in Table V. The difference in responses between the Low Wage and the Youth industries on the one hand and the High Wage industries on the other is striking. 27% of the Low Wage and Youth trials are significant in a 10% test; this is true of only 8% of the High Wage trials (2 negative, 2 positive). The significant responses are almost all positive for the Low Wage and Youth trials: of the 27% only 3, less than 2% of the total, are negative and 54 are positive. This compares favorably with the Sum of Lag results for employment in which, of 234 Low Wage and Youth trials, 23 were positive and only 44 ("only" in comparison to the 54 above) were negative, as predicted by theory.[25] Figure 4 illustrates these differences. The box-plot shows that for Low Wage industries, the median p-value is less than 0.25, where the first quartile should be if there were no response, while the third quartile is less than 0.50. The response of the Youth industries is only slightly less dramatic. In contrast, and as expected, the High Wage industries show no response at all. The distribution of p-values reinforces this point. While the Low-Wage and Youth results lie considerably above the line indicating no wage effect, the p-values for the High Wage industries lie virtually on the 45 degree line.

The strength of the wage results suggest that the slight employment effects cannot be attributed to increases in the minimum wage not affecting wages nor to data which is not responsive to changes in the minimum wage. Given the weakness of the mathematical relationship between increases in the minimum wage and mean wages, the strength of the wage response suggests that the minimum wage may be pushing up the wage structure of Low Wage industries.

Vb. Issues of Power

One dimension of the current debate is whether the tests used in the new minimum wage research are sufficiently powerful to distinguish between an employment elasticity of zero and the small elasticities typically estimated. We examine the power of our tests with Monte Carlo simulations. This requires that we assume a particular value for the elasticity of employment with respect to the minimum wage. Although an elasticity consistent with the data in Table I would range from -0.6 to -2.0, we apply a more stringent standard by testing against an elasticity of -0.3, the upper end of the range Brown et al. found most plausible for young workers.[26] For each industry-year combination, we estimated equation 1 with the restriction that $\Sigma_0^{11} ei = -0.3$, and used the resulting equation to generate values for employment that have a minimum wage elasticity of -0.3. To this deterministic series we added a sequence of random numbers drawn from a normal distribution mean zero and standard deviation equal to the estimated standard error of equation 1, and analyzed the resulting series just as we did the actual employment series. We repeat this process five hundred times. Performing this routine for each of the 234 industry-year events allows us to examine the power of each test reported in our tables.[27] The set of marginal critical values for the Granger and Sum of Lags test for each industry-year pair is used to estimate the tests' power against the alternative hypothesis, that the employment elasticity is not zero but -0.3, for any size test.

Our results find that neither test is particularly powerful at distinguishing between small and zero employment elasticities. For a Granger test of size 0.10, where the null hypothesis is that the employment elasticity with respect to the minimum wage is 0, and the alternative hypothesis is that the elasticity is -0.3, the average power is 0.36.[28] Simulation results for the Sum of Lags test indicate the tests are quite weak: 76% of these tests have power less than 0.10.

What can be done to improve the power of these tests? Classical statistical theory suggests that the size (significance) of a hypothesis test should be derived from the minimization of a loss function which takes type I and type II errors as arguments. As few research projects have well specified alternative hypothesis or loss functions, most fall back on a second best solution, using the arbitrary but familiar 10%, 5% and 1% significance tests. In this instance, however, a well specified alternative exists, and the obvious loss function is one that treats both types of error as equally costly. That is the test size for each of the 234 events should be set so that the probability of committing a type I error, with the null of zero, is the same as the probability of committing a type II error, where the alternative hypothesis is that the elasticity is -0.3. This places the alternative on an equal footing with the null, and provides a neutral test of the conventional hypothesis. We use the simulation results to resize our tests.[29] The average test sizes are then 0.33 and 0.68 for the Granger and Sum of Lags respectively.

What is the effect of this rule? Not surprisingly, it increases the number of Granger and Sum of Lags tests in which it is possible to reject the hypothesis of no employment effect. Table VI shows the effect of this rule on the Granger results. It is not possible to reject a hypothesis of an employment elasticity of -0.3 in 35% of the tests on Wage and Youth industries and there are now patterns by year and by industry. Employment is more sensitive to increases in the minimum wage in manufacturing industries than in service or retail; 45% of the tests in manufacturing are statistically significant while only 31% of the service and retail tests are significant.[30] There is also evidence that the proportion of significant tests is related to the fraction of the labor force earning less than the minimum wage, at least where these differences are large. As noted, there is a clear decline in the fraction of the labor force earnings less than the minimum wage following the 1981 increase. Paralleling the decline in the fraction of the labor force bound by the new minimum wage after 1981, 40% of the tests on Low Wage and Youth industries are significant in tests before 1990, but only 22% of tests are significant in 1990 and 1991.[31]

Resizing of the tests has a parallel effect on the Sum of Lags tests (Table VI). Here 91 (39%) of the tests for Low Wage and Youth industries are now negative and significant. Some years, notably 1980 and 1981, have a large numbers of negative tests; negative and significant results are also common in Youth industries. There is a parallel increase in the number of significant, positive tests, to 19% of the total. Significant positive outcomes display few clear patterns, except in being more common in Youth than Low Wage industries. 42% of the tests for these groups remain insignificant.

Despite the change in the proportion of significant tests, less than forty percent of the results are consistent with the predictions of standard theory, a weak outcome given the certainty of the theory. Even this outcome dissipates once the tests are benchmarked to the High Wage industries. The purpose of the benchmark tests is to provide a standard against which the Low Wage and Youth results can be compared. It does not make sense to reset the size the benchmark tests to equalize the probability of type I and II error, we do not believe that the elasticity of employment with respect to the minimum wage is other than zero for these industries. Comparability does require that the test size of the High Wage Industries be set to the average of that for the Low Wage and Youth industries. Under this standard, 20% of the revised Granger tests are statistically significant. Twenty of the Sum of Lags tests, 30% of the tests, are negative and significant; 16 (24%) are positive and significant, and 54% are non-significant. There is a 15 percentage point difference in the number of significant Granger tests between the Low Wage and Youth industries and the High Wage industries. For the Sum of Lags, the difference in negative and significant tests is less than 9 percentage points.

In summary, adjusting the significance tests for the Low Wage and Youth industries for the power of the tests does substantially increase the proportion of results which are consistent with the minimum wage reducing employment. However, once these results are appropriately benchmarked to the High Wage controls, this relationship disappears and there is little evidence that the results in Tables III and IV are attributable to low power.

VII. Interpretation and Implications

What are we to make of these results? The large number of tests suggests that we might treat the outcomes of our tests as distributions of test statistics and apply conventional hypothesis tests to the results. If we followed this mechanistically, we would find that at least 10% of the trials for the Granger and Sum of Lags tests were significant at a 10% level. Given such results, the hypothesis that the minimum wage does not affect employment could be rejected.

Such a mechanistic approach is inappropriate. The test statistics are not independent of one another as required by conventional hypothesis testing. Complex relations among the industries and years, along with common seasonal movements in employment, render it difficult to assure independence even with controls for common macro-economic phenomena.[32] We have accounted for coincident factors by establishing a control group of six High Wage industries unlikely to be influenced by changes in the minimum wage. Although statistically significant results are always more common for the experimental than the control group, the difference is not great. This is particularly true of the most direct test of the conventional hypothesis, the Sum of Lags test, where the difference in the proportion of significant tests between the Low Wage and Youth industries and the High Wage group is between seven and eight percentage points.

VIIa. Consistency with Convention

A more fruitful approach is to assess whether these results are consistent with conventional views of the minimum wage. How many significant tests are required to confirm the received analysis? This is a matter of judgement, but it is useful to set out plain criteria. In its typical form, accepted theory holds that increases in the minimum wage *will* cause reductions in employment unless no employees earn less than the new minimum. A more subtle version recognizes that, in some instances, the elasticity of demand for labor will be sufficiently small to produce statistically undetectable effects. In our view, the results of this study are not consistent with either criterion.

First, the fraction of statistically significant trials is never sufficiently great to support even the modified position. The test most favorable to the conventional hypothesis, the Sum of Lags adjusted for power, indicates a negative and significant effect in forty percent of the cases; benchmarking reduces the fraction to less than one in ten cases. Even without benchmarking, this result does not support the view that there is a dominant negative elasticity with an occasional lapse for the industry with particularly inelastic demand. With benchmarking, negative and significant results are too sparse to support either version of the conventional hypothesis; the same holds for the results from the Granger, the adjusted Granger, and the Sum of Lags.

Second, the adjusted-for-power Sum of Lags produces a substantial number of results which are at odds with convention. The positive outcomes, which have been found in other 'new minimum wage research', are too frequent to be considered a statistical anomaly. The Sum of Lags tests have less the appearance of a consistent negative effect than the results from an underlying distribution of elasticities with a median close to conventional estimate but which is dominated by its large variance.[33]

Third, there is no reason to believe that the employment effects in the industries under study are too small to be detectable. The elasticity of labor demand with respect to the wage is sufficiently large, -0.93 for non-manufacturing industries (26 of the 34 Low Wage and Youth industries included in this study), and -0.49 for manufacturing, to produce observable effects.[34] An alternative metric of the potential employment effect is the fraction of employees below the new, higher minimum wage. Although the fraction of the labor force below the minimum can be small in specific year/industry pairs, it seldom constitutes less than 4% any industry's labor force. In most instances, 5% to 30% of the labor force of industries in our sample should be at risk when the minimum wage is increased. Our position, that small elasticities of employment demand has not been a problem, is further substantiated by the results of the resized Granger and Sum of Lags tests, which addressed this problem directly.

Finally, our estimates of the effect of increases in the minimum wage on mean wages suggests that weak employment effects cannot be attributed to a lack of wage effects. The effect of increases in the minimum wage on the mean wage is considerably stronger, and more in keeping with our expectations, than the employment effect.

VIII. Conclusion

Our findings are consistent with recent studies which find that legislated increases in the minimum wage have not influenced employment. The fragility of measured negative employment effects and the frequency of positive relationships between the minimum wage and employment parallel the 'new minimum wage research'. The similarity in results across studies using very different data and techniques indicates that current findings are not the product of particular methodologies and reinforces the conclusion that historic minimum wage increases have not consistently reduced employment.

These results do not imply that increases in the minimum wage never affect employment but that the effect is neither large nor consistent. The arguments of Lester and Reynolds, that the possibility of increasing employee productivity allows for moderate increases in the minimum wage without a loss of employment, provide a reasonable explanation of our findings. We cannot know the effect of an overnight doubling of the minimum wage, but such an increase is beside the point in the historic or current American context.[35] Taking the past as a guide, foreseeable increases in the nominal minimum are likely to cause neither large nor systematic decreases in employment.

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Figure 1
Distribution of p-values for 1-sided Prediction t-statistic:
Employment & Average Wage

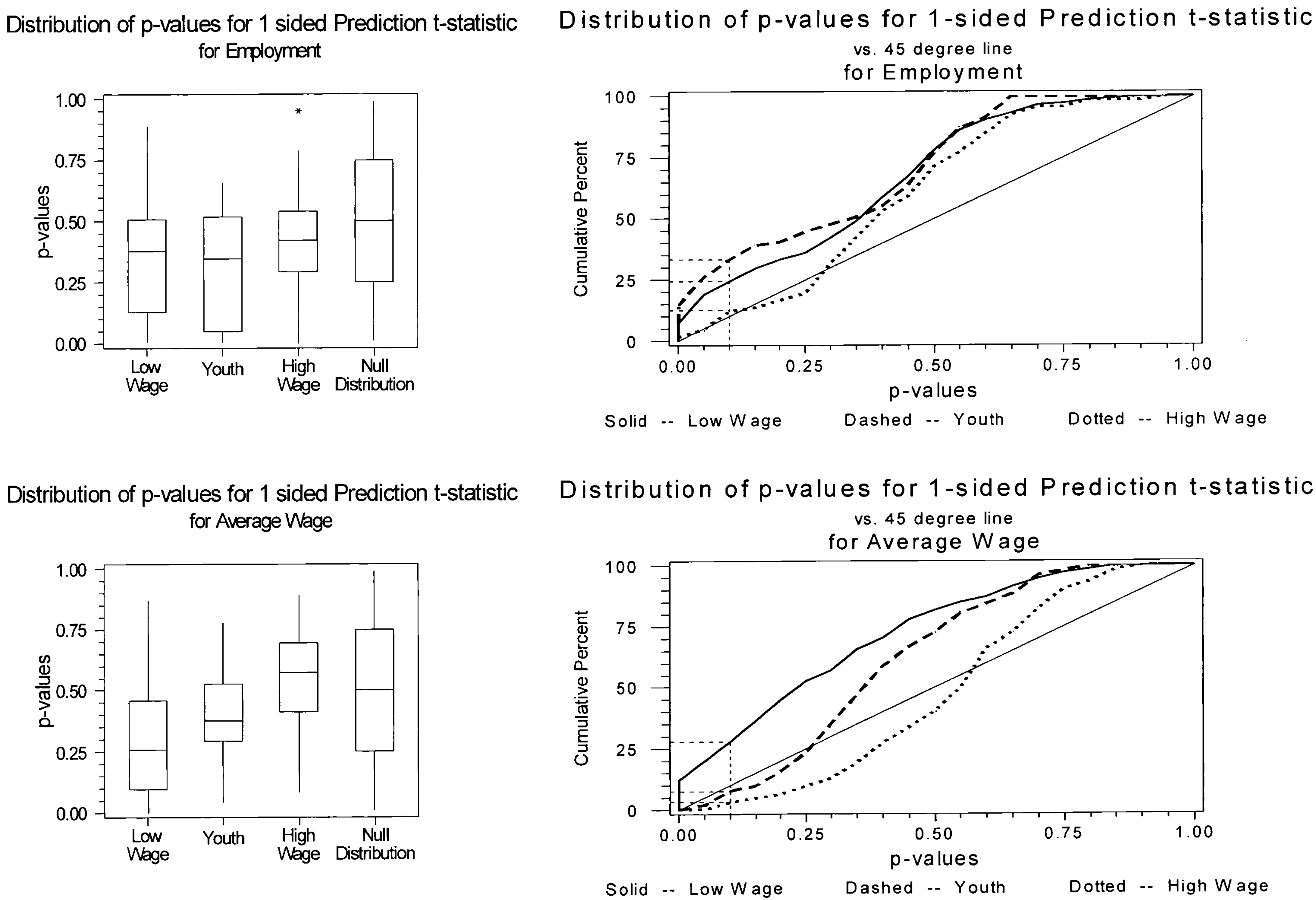


Figure 2

Distribution of Estimated Elasticities (Sums of Lags)

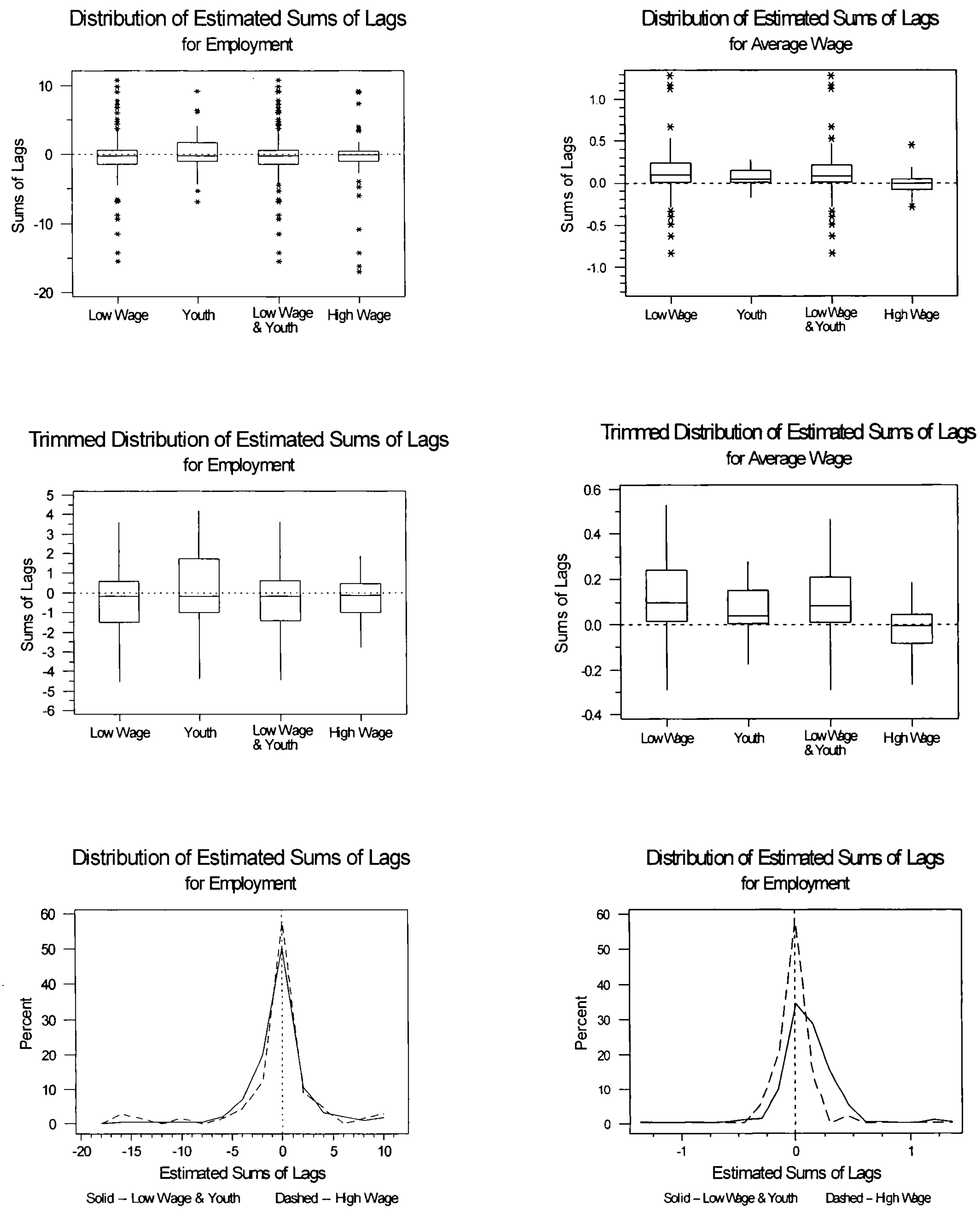


Figure 3
Distribution of p-values for Granger and Sum of Lags Tests
for Employment

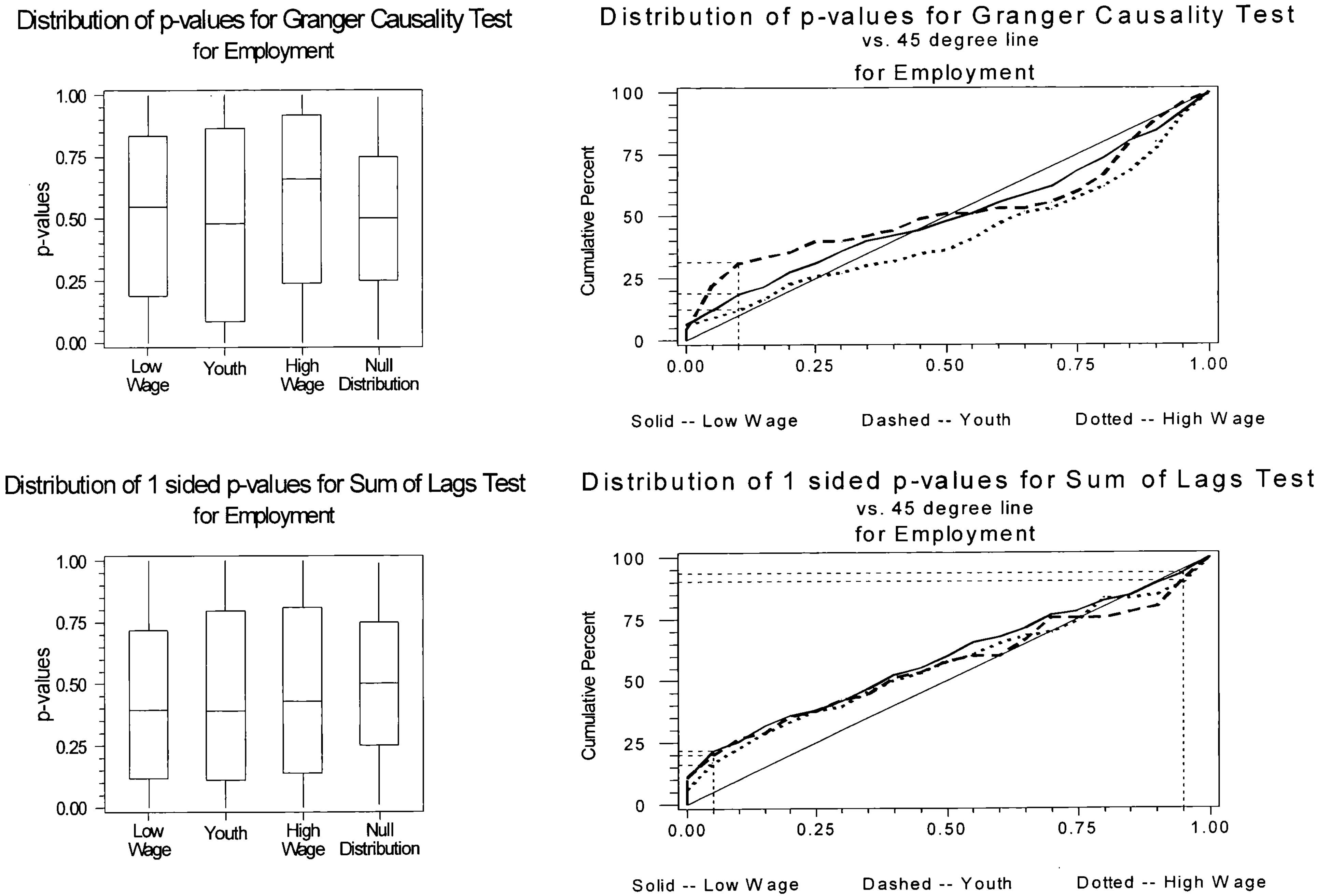
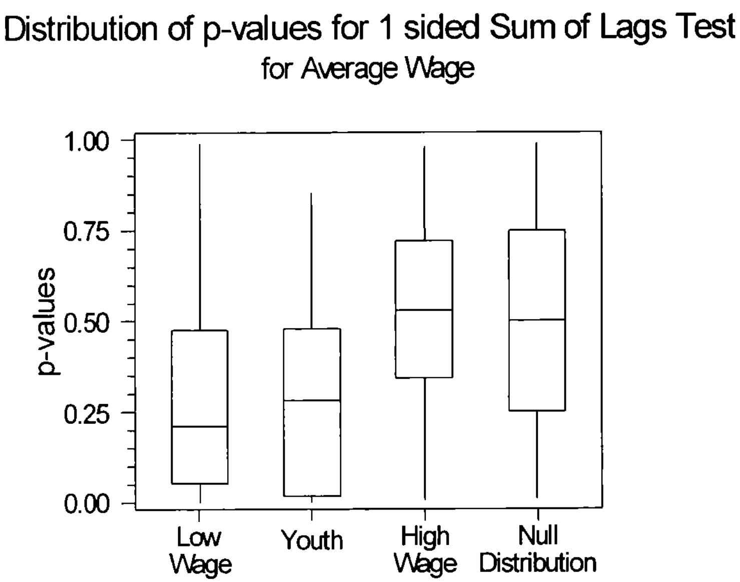


Figure 4
Distribution of p-values for Sum of Lags Test
for Average Wage



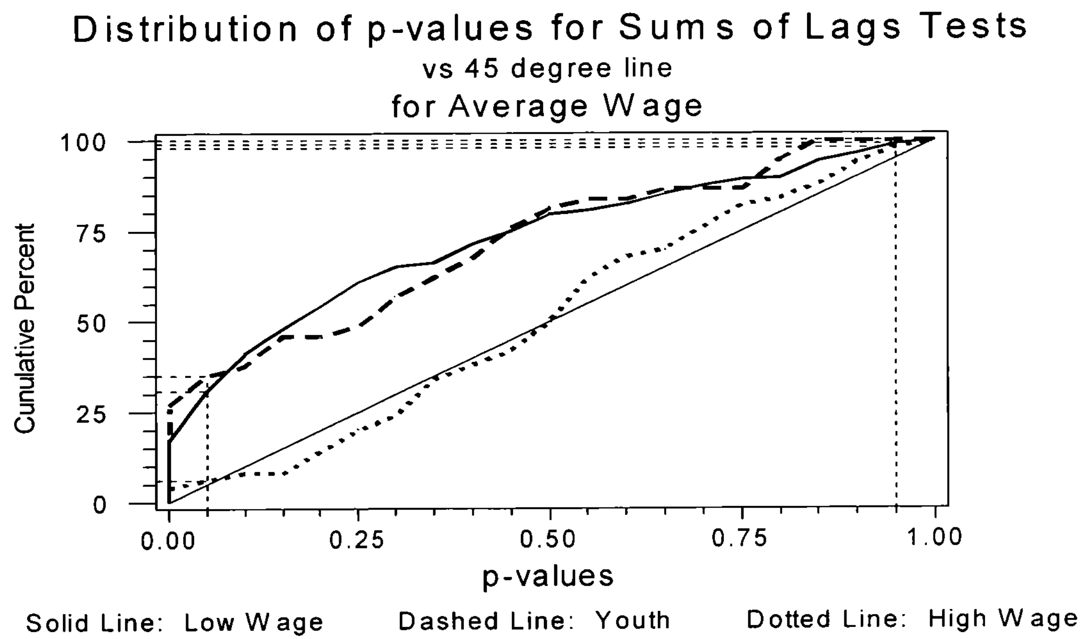


TABLE I
Fraction of Employees Earning Below the New Minimum Wage

SIC	Industry	91	90	81	80	79	78	76	75	74
LOW WAGE INDUSTRIES:										
533	Variety Stores	9.1	27.0	16.2	16.2	54.5	59.8	21.6	14.6	50.1
562	Women's Clothing	m	m	27.2	27.1	40.2	46.8	26.5	22.8	33.5
835	Child Day Care	8.7	20.6	na	na	na	na	na	na	
566	Shoe Stores	5.5	13.4	11.0	9.9	47.6	25.8	10.3	9.2	32.6
565	Family Clothing	m	m	27.2	27.1	40.2	46.8	26.5	22.8	33.5
554	Gas Service Stn	10.2	15.7	21.0	21.3	49.9	46.6	23.0	19.1	42.1
546	Retail Bakery	7.5	20.0	12.4	11.6	s	61.0	s	s	s
238	Misc Apparel Str	8.9	15.0	32.1	32.2	38.9	42.3	33.6	31.7	33.1
525	Hardware Str	4.0	11.7	12.4	11.7	19.4	28.2	13.1	11.6	16.2
591	Drug Stores	4.8	12.4	21.8	20.5	38.0	45.0	21.9	18.9	32.8
721	Laundries	6.0	12.5	18.6	18.8	43.2	42.2	22.2	22.6	36.9
723	Beauty Shops	13.9	19.7	22.0	19.3	38.9	42.6	22.2	15.3	32.9
701	Hotels & Motels	8.5	14.2	45.6	35.4	49.1	45.9	29.3	32.6	48.7
734	Building Services	6.7	11.9	14.4	14.4	32.4	31.5	12.0	10.0	13.6
805	Nursing Facility	4.2	9.3	37.9	33.0	50.3	48.4	37.0	34.5	43.9
531	Department Stores	4.2	9.7	39.3	37.5	33.5	33.3	36.2	35.6	28.4
225	Knitting Mills	3.7	9.2	9.1	7.4	26.9	25.4	10.7	10.3	18.6
599	Retail Stores	6.0	12.0	27.1	23.5	37.7	34.6	20.4	18.4	30.5
239	Misc Fabr Textile	2.3	3.9	8.1	9.8	29.7	28.0	8.8	9.1	22.7
561	Mens Clothing	m	m	27.2	27.1	40.2	46.8	26.5	22.8	33.5
553	Auto/Home Supply	1.5	4.7	10.8*	12.7*	25.2	21.0	9.7*	7.6*	9.7
YOUTH INDUSTRIES: by SIC										
58	Eating/Drinking Pl	25.4	35.2	78.4	74.2	61.6	63.9	73.4	68.0	53.6
541	Grocery Stores	6.4	13.2	34.1	29.2	23.3	21.4	26.2	28.8	18.1
311	Leather Tanning	-	7.4	s	s	s	s	s	s	s
545	Dairy Prod Stores	14.6	29.1	9.8	s	s	s	s	s	s
753	Auto Rental	3.7	5.2	9.9	11.0	14.1	15.0	10.0	7.0	9.4
HIGH WAGE INDUSTRIES: by SIC										
731	Advertising	1.2	0.9	4.0	5.4	10.9	2.3	2.8	s	1.9
371	Auto Vehicle	0.4	0.4	6.1	5.7	0.6	0.7	2.8	1.7	0.4
331	Blast Furnaces	0.0	0.9	3.6	3.9	0	0	0.0	0.0	0.6
871	Eng/Arch Services	0.5	0.5	5.6	4.0	2.6	4.4	5.1	4.8	1.1
483	Radio/TV Brdcst	1.7	3.1	7.4	7.4	9.8	4.9	5.9	4.3	3.9

m - missing
* - change in definition

na - not a distinct category in earlier CPS system
s - very small sample size

Table II
Summary Statistics

(a) Estimates of Employment Elasticities
Annual Medians & Inter-quartile Ranges
(*Inter-quartile Ranges in Italics*)

Total	91	90	81	80	79	78	76	75	74	68	67
Low Wage & Youth											
-0.15	-0.025	-0.40	-0.75	-0.63	0.094	0.25	0.77	-0.59	-0.10	-0.30	-1.46
2.06	*2.63*	*2.14*	*2.02*	*3.75*	*1.05*	*0.74*	*6.20*	*9.67*	*1.69*	*1.52*	*4.05*
Low Wage Industries											
-0.16	-0.16	-0.42	-0.87	-0.58	0.29	0.37	0.26	0.25	-0.096	-0.50	-1.82
2.06	*1.89*	*2.07*	*1.92*	*3.49*	*0.89*	*0.70*	*4.57*	*10.50*	*1.81*	*1.07*	*4.29*
Youth Industries											
-0.15	1.92	-0.067	-0.44	-0.73	-0.62	0.0004	6.09	-2.63	-0.32	0.44	-0.57
2.70	*2.89*	*3.95*	*3.58*	*5.11*	*2.11*	*1.62*	*6.18*	*6.29*	*1.65*	*2.19*	*5.34*
High Wage Industries											
0.0006	-0.047	0.68	1.35	0.0006	0.095	-0.51	-0.28	0.013	0.21	-0.16	-16.15
1.69	*6.84*	*6.71*	*8.65*	*3.46*	*5.30*	*4.86*	*8.10*	*15.61*	*3.08*	*1.63*	*17.01*

(b) Distributions of Test Statistics

GRANGER:	a	b	c	Total
Low	7	11	11	29 (15%)
Youth	2	5	7	14 (31%)
Total	9	16	18	43 (18%)
High	4	2	2	8 (12%)

SUM OF LAGS:	Positive				Negative			
	a+	b+	c+	Total	a-	b-	c-	Total
Low Wage	7	5	5	17 (9%)	10	10	16	36 (19%)
Youth	3	1	2	6 (13%)	1	4	3	8 (18%)
Total	10	6	7	23 (10%)	11	14	19	44 (19%)
High Wage	5	2	3	10 (15%)	1	2	3	7 (11%)

Table III
GRANGER TEST OF MINIMUM WAGE vs. TOTAL EMPLOYMENT

	91	90	81	80	79	78	76	75	74	68	67
LOW WAGE INDUSTRIES: by SIC											
553	.	b	.	.	.	.	.	a	.	.	.
562	.	c	.	.	.	.	.	.	.	.	.
317	.	.	.	.	.	.	.	.	.	.	.
399	.	.	c								
835	.	.	.								
566	.	.	.	.	.	.	.	.	.	.	.
565	.	.	.	.	.	.	.	.	.	.	.
554	.	.	.	.	.	.	c	.	a	.	.
546	.	.	.								
302	b	.	a								
238	.	.	.								
594	.	.	.								
525	.	.	.								
244	.	.	.								
591	.	.	.	.	.	a	.	.	.	c	b
721	.	.	.								
316	.	.	.	.	.	c	.	.	.		
723	.	.	.								
701	.	.	.	.	b	.	c	.	.	.	.
734	.	.	c	.	.	.	c	.	c		
805	.	.	.								
531	.	.	.	.	.	.	.	.	.	.	.
225	b	a	b	c	.	a	a	b	b	.	.
599	.	.	.								
239	.	.	.	b	.	.	c	.	b	.	.
561	.	b	.	.	.	.	.	.	.	.	.
553	.	.	.								
YOUTH INDUSTRIES: by SIC											
56	.	a	.	.	.	.	c	.	.	.	.
541	.	.	.								
58	.	.	b	.	b	c	.	c	b	.	.
311	b	a	c	c	.	b	c	.	.	.	.
545	.	.	.								
751	.	c	.								
753	.	.	.								
HIGH WAGE INDUSTRIES: by SIC											
731	.	.	.	.	a	.	.	.	.	.	.
371	a	b	.	c	.	.	.	.	.	.	.
331	.	.	.	.	.	.	.	.	.	.	.
871	.	.	.	.	.	.	.	.	.	.	c
173	b	.	.	.	.	.	.	.	.	.	.
483	.	.	.	.	.	.	a	.	.	a	.

a: 0.01 ≥ marginal significance level
b: 0.05 ≥ marginal significance level > 0.01
c: 0.10 ≥ marginal significance level > 0.05
.: marginal significance level > 0.10

Table IV
SUM OF LAGS

	91	90	81	80	79	78	76	75	74	68	67
Low Wage Industries: by SIC											
533	b+	a+	.	.	.	.	b+	a-	a-	.	c-
562	.	.	.	.	.	.	a+	b-	.	.	c-
317	.	.	.	.	.	.	.	b+	c-	.	.
399	.	.	.								
835	.	a+	.								
566	.	b-	.	.	.	.	a+	c-	.	.	b-
565	.	.	.	a-	c-	.	.	c-	c-	.	c-
554	.	.	.	.	.	.	b-	.	a+	.	.
546	.	.	b-								
302	.	.	.								
238	.	b-	c-								
594	.	.	.								
525	b+	.	c-								
244	.	.	.								
591	.	.	.	.	.	a-	.	c+	.	c-	a-
721	.	.	a-								
316	.	.	.	.	c+	.	.	.	.		
723	.	.	.								
701	.	.	.	c+	a+	.	a-	.	c+	.	.
734	.	.	.	c-	.	.	a-	.	.		
805	.	.	.								
531	.	b-	.	.	.	.	c-	.	.	c-	.
225	b-	.	b-	.	.	.	.	b+	.	.	.
599	.	.	.								
239	.	a-	c-	a+	.	.	.	c+	a-	.	.
561	.	.	.	b-	.	.	.	.	.	.	.
553	.	c-	.								
Youth Industries: by SIC											
56	.	a-	.	b-	.	.	a+	c-	b-	.	c-
541	.	.	.								
58	c+	.	b-	.	.	.	.	.	c-	.	.
311	.	b-	.	.	.	.	a+	.	.	.	.
545	.	.	c+								
751	b+	a+	.								
753	.	.	.								
High Wage Industries: by SIC											
731	.	.	.	.	.	.	.	.	.	.	.
371	a+	a+	b+	.	.	.	.	.	.	.	c-
331	.	.	a+	.	.	.	.	.	.	.	c-
871	.	.	b-	.	.	a+	.	.	.	.	.
173	a+	c+	a-	.	c+	c+	a-	.	.	.	.
483	.	.	c-	.	.	.	b+	.	.	.	b-

a: 0.01 ≥ significance level
b: 0.05 ≥ significance level > 0.01
c: 0.10 ≥ significance level > 0.05
.: significance level > 0.10

+: sum is positive and significant
-: sum is negative and significant

Table V
SIGN and SIGNIFICANCE of MINIMUM WAGE EFFECT on AVERAGE WAGE

	91	90	81	80	79	78	76	75	74	68	67
Low Wage Industries: by SIC											
533	.	.	.	a+	a+	a+	b+	.	.	.	b-
562	.	.	.	.	.	b+	b+	.	.	.	.
317	.	c+	c+	.	.	b+	c+	.	.	.	.
399	.	c-	.								
835	.	c+	.								
566	.	.	.	.	.	.	.	b-	b+	.	.
565	.	.	a+	c+	b+	.	.	.	.	.	.
554	.	.	.								
546	.	.	.								
302	.	.	.								
238	.	.	.								
594	.	.	.								
525	.	b+	.								
244	.	.	.								
591	.	.	b+	b+	a+	a+	b+	.	.	.	.
721	b+	.	c+								
316	.	.	.	.	.	a+	.	.	.		
723	.	.	.								
701	.	.	.	.	b+	a+	.	c+	a+	.	.
734	.	.	.								
805	c+	c+	a+								
531	.	.	.	c+	b+	a+	a+	.	.	c+	.
225	.	.	.	b+	b+	a+	.	.	.	b+	.
599	.	.	.								
239	.	.	.	.	.	.	.	.	.	.	.
561	.	.	c+	.	.	.	a+	.	.	.	.
553	.	.	.								
Youth Industries: by SIC											
56	.	.	b+	.	.	b+	b+	.	.	a+	b+
541	.	.	.								
58	a+	b+	.	c+	a+	a+	.	.	b+		
311	c+	.	.	.	.	.	.	.	.	.	.
545											
751											
753	.	.	.								
High Wage Industries: by SIC											
731	.	.	.								
371	.	.	.	.	.	.	.	.	.	.	.
331	.	.	.	.	.	.	.	.	b+	.	.
871	.	.	.								
173	.	c-	.	.	.	.	.	b+	.	.	.
483	.	b-	.	.	.	.	.	.	.	.	.

	Trials	total+	a+	b+	c+	total-	a-	b-	c-
Low Wage	175	42	14	16	12	3		2	1
Youth	39	12	4	6	2	0			
LW & Youth	214	54	18	22	14	3		2	1
High Wage	50	2		2		2		1	1

TABLE VI

GRANGER TESTS OF MINIMUM WAGE vs. EMPLOYMENT
when POWER = 1 - ALPHA
Prob(Type II error) = Prob(Type I error)

	91	90	81	80	79	78	76	75	74	68	67
LOW WAGE INDUSTRIES: by SIC											
533	.	*	*	.	.	.	.	*	*	.	.
562	.	*	.	.	.	*	*	*	*	.	.
317	.	.	*	.	.	.	.	*	*	.	.
399	*	.	*								
835	.	.	.								
566	*	.	.	.	.	.	*	.	.	.	.
565	.	.	.	*	.	*	*	.	*	.	.
554	.	*	.	.	.	.	*	.	*	.	.
546	*	.	.								
302	*	.	*								
238	*	.	.								
594	.	.	.								
525	.	.	.								
244	*	.	.								
591	.	.	.	.	.	*	.	*	*	*	.
721	.	.	.								
316	.	.	*	.	.	*	*	.	.		
723	*	.	*								
701	.	.	.	*	*	.	.	.	.	.	.
734	.	.	.	.	.	.	.	.	.	*	.
805	.	.	.								
531	.	.	*	.	.	.	*	.	.	*	*
225	.	*	*	*	.	.	*	*	*	*	.
599	.	.	.								
239	.	.	*	*	.	*	.	.	*	*	.
561	.	*	.	.	.	*	*	.	.	.	.
553	.	.	.								
YOUTH INDUSTRIES: by SIC											
56	.	*	.	.	.	.	*	*	*	.	.
541	.	.	.								
58	.	.	*	.	.	.	*	.	*	.	.
311	.	*	*	*	*	*	*	.	.	.	.
545	.	.	.								
751	.	.	.								
753	.	.	.								
HIGH WAGE INDUSTRIES: by SIC											
731	.	.	.	.	*	*	.	.	.	.	*
371	*	*	.	*	*	.	.	.	.	.	.
331	.	*	*	.	.	.	.	.	.	*	.
871	.	.	.	.	.	.	.	.	*	.	*
173	.	.	*	.	.	*	.	.	.	.	.
483	.	.	.	.	.	.	*	.	.	*	*

*: reject H0
.: do not reject H0

TABLE VII

SIGNIFICANT SUM OF LAG TESTS
when POWER = 1 - ALPHA
Prob(Type II error) = Prob(Type I error)

LOW WAGE INDUSTRIES: by SIC	91	90	81	80	79	78	76	75	74	68	67
533	+	+	.	.	-	-	+	-	-	-	+
562	-	-	.	.	-	+	+	-	-	-	-
317	.	-	+	.	.	.	.	+	-	-	.
399	-	-	-								
835	.	+	-								
566	.	-	-	.	+	+	+	-	-	.	+
565	+	-	-	-	-	-	+	-	-	-	.
554	.	-	.	+	+	+	-	.	+	-	+
546	+	+	-								
302	.	-	-								
238	-	-	-								
594	+	-	.								
525	+	+	-								
244	-	+	.								
591	.	.	-	-	-	-	-	+	.	-	.
721	.	+	-								
316	-	-	.	-	.	-	-	.	.		
723	+	+	-								
701	.	-	.	+	+	+	-	.	+	-	+
734	-	-	+	-	+	.	-	.	-		
805	+	+	.								
531	.	-	-	.	.	.	-	-	+	-	-
225	-	-	-	+	.	+	.	+	.	+	.
599	.	+	-								
239	.	-	-	+	-	.	-	+	-	-	+
561	.	-	-	-	.	+	+	.	-	.	.
553	+	.	+								
YOUTH INDUSTRIES: by SIC											
56	-	-	-	-	.	-	-	-	-	.	.
541	.	-	-								
58	-	-	-	-	.	.	.	.	-	-	.
311	.	-	-	.	-	+	+	.	.	+	+
545	-	-	-								
751	+	+	-								
753	.	-	-								
HIGH WAGE INDUSTRIES: by SIC											
731	-	.	-	.	.	.	.	-	.	.	-
371	-	-	-	-	-	-	.	-	-	.	-
331	-	-	-	.	.	.	.	.	.	.	-
871	-	.	-	.	-	.	-	-	.	-	-
173	-	-	-	.	-	-	-	-	-	-	-
483	.	-	-	-	-	.	-	-	-	-	-

+ Statistically significant and positive
. Not statistically significant
- Statistically significant and negative

Notes

[1]Card, Katz and Krueger have contributed a series of studies which have concluded that changes in the minimum wage have no noticeable adverse effect on employment (Card 1992a, 1992b; Card and Krueger 1994, 1995; Katz and Krueger 1992; Krueger 1994). Findings that disagree with this result are to be found in Neumark and Wascher (1992, 1995a and 1995b), Currie and Fallick (1993), Kim and Taylor (1995), and Deere, Murphy and Welch (1995). Many of these writers have been sharply critical of each others' analyses (Card, Katz and Kreuger 1994, Ehrenberg 1992, and Neumark and Wascher 1994 and 1995c, and Deere, Murphy and Welch 1996).

[2]Reynolds (1963) and Lester (1960) argued that employment may not respond to moderate changes in the minimum wage due to frictions and macroeconomic fluctuations. Employee heterogeneity, and costs of monitoring and adjustment can make accurate assessment of individual productivity costly and keep employers below production frontiers. The optimal response to a higher minimum wage, in these circumstances, is often not to reduce employment but to improve management practices, to increase employees' skills through training, and to use the higher wage to improve the mix of employees. Clark (1980) finds union organization has such an effect on management practices and work organization in the concrete industry. Finally, the timing of the minimum wage increase is important. Business cycles and secular growth, effects which time series research finds have many times the employment effects of the minimum wage, may dominate firm employment decisions.

[3]Inclusion of a number of industries is yet another advantage, since it gives a broader picture than that afforded by studies of specific industries.

[4]Our data source does not provide industry wage distributions; instead we use average hourly wages to proxy the proportion of employees at or near the minimum. We consider this issue further in our discussion of Table I.

[5]Several industries that satisfy the criteria for Low Wage or Youth industries are not in the sample because data are not available for a sufficient period.

[6]The Consumer Price Index (CPI) and Unemployment Rate (UN) are from the BLS.

[7]The May CPS is used for increases prior to 1980, the larger ORG files are used from 1980 on. CPS industry classifications are broader than the SIC system used in the EEH data and some CPS industries aggregate several SIC industries. Table I is limited to industries in which CPS and SIC industries are closely matched. Although the CPS includes all employees, the EEH is limited to production and non-supervisory employees, about 80% of total employment. The fraction of EEH 'employees' below the minimum wage will be larger than that found in the CPS.

[8]Until 1981, Congress raised the minimum wage when it fell below 40% of the average manufacturing wage. By the time of the March 1990 increase, the minimum wage stood at 31.4% of the manufacturing average; in April 1991 it was 33.6% of that average. The long period in which the minimum wage remained unchanged, combined with market pressures on wages over the intervening nine years, greatly reduced the fraction of employees below the new minimum.

[9]See Siegel (1997), p. 339, for details.

[10]EEH data are collected for the pay period that includes the twelfth of the month. Since 1956 minimum wage increases have occurred earlier in the month. Many of the minimum wage increases occurred at intervals of one year, so in most cases we compared the month of the increase to the 10 months prior to it. Thus if the increase occurred on 1/1/76, we compared the employment change for January 1976 to the mean change over the period from March 1975 through December 1975. As the 1975 increase occurred 8 months after the 1974 increase, we used a 6 month period there.

[11] Asterisks indicate *outside values*, values that are more than 1.5 times the inter-quartile range below the first quartile or above the third quartile.

[12]A Granger causal relation runs from the Consumer Price Index (CPI) to the minimum wage, but not from the minimum wage to the CPI, for both the full sample period and for all but two of the nine year subsamples. There is no evidence of Granger causation in either direction between the Unemployment Rate (UN) and the minimum wage in the full sample or in any sub samples. Including inflation in the equation will avoid confusing the impact of inflation with that of the minimum wage without masking minimum wage effects.

[13]Splitting the minimum wage variable controls for increases in the minimum occurring prior to the increase under examination, and allows us to interpret the sum of the coefficients e_i as the minimum wage elasticity of employment (in this industry at the time of the change in the minimum wage).

[14]The twelve month lag used in this research is consistent with the literature. Most studies find that employment adjustments are completed within four quarters (Hammermesh, 1993), Neumark and Wascher (1992) also use a one year adjustment period.

[15]This may not be the case were industry specific prices used.

[16]Augmented Weighted Symmetric (AWS) tests are used to detect unit roots.

[17]The number of degrees of freedom for the unrestricted regression varies from 45 to 59, depending on the sample period, the industry, and the results of the AWS and cointegration tests. Six of the employment series are cointegrated with the CPI, and nominal minimum wage: SIC 525, Hardware Stores; SIC 316, Luggage; SIC 56, Retail Apparel Stores; SIC 751, Automotive Rental; SIC 871, Engineering and Architecture Services; and SIC 173, Electrical Work. The error correction mechanism, one lag of the residual from the cointegrating equation, is included in the X matrix to account for this in the Granger Causality tests. The restricted regressions have 9 to 13 more degrees of freedom. While the degrees of freedom may seem small to those accustomed to dealing with large cross section data sets, it is typical of time series research.

[18]1991 has a large number of degrees of freedom because there are only 9 lagged terms. 1990 has a large number because no increases in the Federal minimum wage occurred during the preceding 8 years, so the d coefficients are set to zero. 1981 has a slightly smaller number of degrees of freedom for industries that have a unit root and/or begin in 1972. One more degree of freedom is lost whenever either a trend term is needed to stabilize any variable, or employment is cointegrated with any of the other variables.

[19]Of the 11 interventions we examine, 6 occur on January 1st (all those in the period 1975-1981), 2 in February (1967 and 1968), 2 in April (1990 and 1991) and 1 in May (1974). The inclusion of monthly dummies could easily mask the effect of the minimum wage increases. This is especially true for those that took place in the period 1975-1981, since employment typically declines in January even without any change in the minimum wage.

[20]We report goodness of fit statistics for the constrained equation because of its nature as a forecasting equation, and the irrelevance of goodness of fit measures for the unconstrained equation in this context.

[21]For the Low Wage and Youth industries, the mean R^2 is slightly larger, 0.69 and the standard deviation slightly smaller, 0.29. The median and mean of the High Wage industries are both lower, 0.55 and 0.46 (0.32) respectively.

[22]When discussing the Granger tests, the phrase "statistically significant" refers to an F-test with a 10% size. For the Sum of Lags tests, it refers to a 10% 2-sided asymptotic t-test.

[23]The regression coefficient was 0.936 with a standard error of 1.41. There were 145 observations and the regression included an intercept. R^2 was 0.003.

[24]Consider the effect of the 1990 increase in the minimum wage on the average wage in hardware stores. The average wage for hardware store employees two months before the increase in the wage was $6.65 and Table I indicates that 11.7% of the employees in this industry were 'bound' by the increase. Assume that the average wage for 'bound' employees was the mid-point between the old and new minimum, $4.03 and, consequently, the average wage of the unbound employees was $7.05. What then happens to the mean wage when the minimum rises? If employees earning less than the new minimum are all 'sacked', the new mean wage will increase by 6% to $7.05. This is half the magnitude of the reduction in employment. If instead, the only consequence of the increase is to raise the wages of the bound employees to the new minimum, the mean wage will increase by two cents, to $6.67 per hour.

[25]The 4 statistically significant, negative results for the Average Wage occur in 1975 (1 of 2 statistically significant results that year), 1980 (2 of 6) and 1990 (1 of 3). 1978 has the largest proportion of statistical significant, positive results at 75%, followed by 1968 (55%) and 1976 (50%). Over this period, the average wage was most sensitive in the three industries with the lowest average wage, Variety Stores (SIC 533), Women's Clothing Stores (SIC 562) and Personal Leather Goods Manufacturing (SIC 317), and in Drug Stores (SIC 591). In each, 6 of 11 trials are positive and significant.

[26]Elasticities consistent with Table I are calculated as the ratio of the percent of employees below the new minimum wage (From Table I) to the percent change in the minimum wage. We assume that individuals with wages below the new minimum would be laid off when the wage rises above their marginal product.

[27]All programming and results for the Monte Carlo tests, along with all other programming and data used for this article, are available from the authors.

[28]The distribution of tests indicates that the power is at least .50 in 34% of the industry-minimum wage pairs. Low power seems to be particularly common to tests in 1968, and to a lesser extent in 1967 and 1979-1981; in each of these years, mean power is less than 0.3. Years in which this test has greatest power are 1991, 1990 and 1974; mean power in each exceeds 0.4.

[29]The marginal level of significance at which the null hypothesis, of no effect for the Granger test, or that the sum of lagged coefficients equals zero is retained for each Monte Carlo iteration. The 500 iterations for each industry-minimum wage increase is then used to adjust the test size, β, so that Power = 1 - β.

[30]This recalls the conclusions of a series of Department of Labor studies of the effect of increases in various industries. See Brown et. al., pp. 519-522, for a summary.

[31]Even here there is contrary evidence as the fraction of workers below the new minimum drops between 1990 and 1991, but the fraction of significant tests remains constant.

[32]This is due to the likely existence of time components in the error term which are shared across industries, and industry components which are shared across years (within an industry). The latter is particularly likely as trial periods overlap. One possible approach would be to recast the research as panel data with controls for industry and year. The gains from this approach, better control for industry and time factors, would be purchased at the cost of imposing a common structure on many shared coefficients in the model.

[33]As an asymptotic rather than an exact distribution, the Sum of Lags test may be unlikely to reject the null of no effect. The large number of significant, positive sums, between two-thirds and four-fifths the number of significant,

negative sums, offsets this drawback and reinforces our interpretation.

[34]Heckman and Sedlacek as reported in Hammermesh (1993) table 3.2.

[35]Evidence that large increases in the minimum wage may be associated with adverse employment effects can be found in Katz, Loweman and Blanchflower (1995)

The Minimum Wage: the Bark is Worse than the Bite

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Abstract

The theory of the minimum wage indicates that increases in the minimum will only influence employment when the minimum is binding. Despite this, most research on this topic assumes that certain markets will be affected by the minimum wage and tests for an employment effect without determining if any employees are being affected by the increase. This research takes advantage of a extensive monthly data set for employment and wages by industry to implement a more rigorous testing procedure.

Starting with thirty-two industries selected for their presumed sensitivity to the minimum wage, we first test for a statistically significant positive response of the industry average wage to increases in the minimum wage. Only in industry/minimum-wage-increase pairs where we find one do we go on to test for an employment effect. The average industry wage responds in this fashion in only 25% (54) of the 212 industry/minimum-wage-increase pairs studied does. Only eight of these show a statistically significant negative impact on employment; indeed six display a significant and positive employment impact, which we explain by an appeal to Akerlof's *Market for Lemons*. Thus, despite limiting our research to industries in which the minimum wage is binding, we find little evidence of a negative relationship between minimum wage increases and employment.

The Minimum Wage: the Bark is Worse than the Bite

No participant in the recent debate over the minimum wage has claimed that the minimum wage can never reduce employment, nor has anyone claimed that the minimum wage is always and everywhere an employment reducing phenomenon.[1] In simplest form, the neoclassical model of the minimum wage makes a precise prediction; if the minimum wage is set above the market clearing wage, competitive, wage-taking employers slide up their labor demand schedule and employment declines.[2] More sophisticated statements recognize that the equilibrium wage distribution is not degenerate and that the wage is only one part of labor costs, but the prediction is little affected; if the minimum wage raises wages, employment will be lower than otherwise.[3]

An issue confronting researchers is to identify situations that satisfy the first condition, a minimum wage which is binding, prior to determining the effect on employment. The common practice has been to identify markets believed *a priori* to be directly affected by the minimum wage and go straight to examination of the employment effect: thus the numerous studies of teenagers and of racial minorities. A shortcoming of this approach is that it is not possible to know if the lack of an empirical relationship between the minimum wage and employment accurately reflects the effect of the minimum wage or occurs because the minimum was not binding (at that time) on the particular market being analyzed. This allows negative findings to be dismissed, perhaps somewhat too readily

A more satisfactory approach, pioneered by David Card (Card, 1992a & 1992b), is to test for both wage and employment effects. A positive and significant wage effect assures that the employment test meets the necessary condition for the minimum wage to be binding. We take advantage of monthly data on wages and employment by two and three digit SIC industries available from Employment, Hours and Earnings to extend this methodology. We examine the effect of the 11 minimum wage increases between 1967 and 1991 on average wages and employment in 32 low wage and youth industries.[4] A Box-Jenkins forecasting equation is applied to each of the 212 industry/minimum wage increase pairs -- hereafter industry/wage pairs -- and these are tested for a positive and significant relationship between increases in the minimum wage and the average wage. The effect of the minimum wage on employment is then tested on the subset of pairs in which we found a positive wage effect. This procedure may be too rigorous in excluding industries (type I error), because the average wage is a relatively inert measure of the wage consequences of the minimum wage. However, it assures that the employment tests are limited to a set of industries in which the effect of a minimum wage increase is robust.

The estimated effects of minimum wage increases on the average wage are relatively weak. Only one quarter of our industry/wage pairs, 54 in all, display a positive relationship between the minimum wage and the average wage. Furthermore, despite limiting the employment estimates to the set of industries in which we are assured the minimum wage is binding, there is little evidence of a strong negative employment effect. Only eight of the employment estimates are negative and significant and an additional six are positive and significant. The mean employment elasticity is negative and below the conventionally accepted range, but this result is an artifact of several large negative outliers. The median is in the conventionally accepted range, and the modal interval is positive. Trimming the largest positive and negative outliers results in a mean which is negative but too close to zero to be economically significant. To explain the positive employment responses in situations where the minimum wage clearly binds, we suggest that Akerlof's model of the *Market for Lemons* is applicable to some low wage labor markets.

These results are in keeping with contemporary research which finds little evidence of consistently negative employment effects. We conclude that there are many markets in which minimum wage increases are not binding. Adjusting for this by limiting tests to those markets in which there is a measurable wage effect, however, does not reveal a strong relationship between increases in the minimum wage and employment loss.

1. Data: Sources and Preliminary Analysis

Our research focuses on employment and wages in two groups of industries that should be sensitive to the minimum wage: industries with average wages close to the minimum and industries which employ large numbers of teenager and young adults. The first group, which we refer to as 'Low Wage' industries, consists of 27 three digit SIC industries in which the average hourly wage of production and non-supervisory employees in January 1990 was less than 210% of the new minimum wage of $3.80 (less than $8).[5] At that time, they had 13,915,000 employees, representing 15.5% of total private employment and included industries such as wooden containers (SIC 244: 43,100 employees in January 1990), hotels and motels (701: 1,520,100), department stores (531: 2,221,200), and personal leather goods (317: 15,200). The second group, designated 'Youth' industries, consists of the 5 industries in which at least 30% of the labor force is under age 25 in 1989.[6] These industries are leather tanning and finishing (SIC 311: 15,000 employees in January 1990), automotive rentals (751: 171,900), auto repair services (753: 522,200), grocery stores (541: 2,868,800), dairy product stores (545: 22,300), retail apparel stores (560: 1,222,200), and eating and drinking establishments (580: 8,395,000).[7]

Monthly observations on employment and average wages for our industries were taken from the Employment, Earnings and Hours (EEH) series of the United States Bureau of Labor Statistics (BLS). Changes in the definition of industries and the introduction of new industries limited the length of some data series. Nevertheless we were able to examine the increases in the federal minimum wage of 1991, 1990 and 1981 for all of our industries, and were able to go back to the 1967 increase for nearly half of our industries. In all, there are 212 interventions in the Low Wage and Youth industries, where an intervention is an industry/wage pair. Data on the minimum wage are from Ehrenberg and Smith (1987) and Kaufman (1991).[8]

According to standard theory, the individuals who are at risk from an increase in the minimum wage are those earning less than the new minimum. If these individuals constitute a large fraction of an industry's labor force, the employment consequences should be readily detected; if very small, detection may be difficult. Table 1 presents estimates of this fraction for (most of) the industries that we examine in the year prior to the increase.[9]

Two patterns are apparent. First, a substantial fraction of employees in these industries earned less than the 'new' minimum wage in the year before the increase. In 1981, for example, this fraction ranged from 8.1% (SIC 239, miscellaneous fabricated textile products) to 78.4% (SIC 56, eating and drinking places); most fall between 9% and 27%. Only 7.3% of the labor force as a whole were bound by the minimum wage in that year. Other years' data follows a similar pattern. Second, there is a decline in the fraction of employees below the new minimum over time. In the 1970s and early 1980s, eating and drinking establishments had between 68% and 78% of employees below the new minimum. This fraction had fallen to 35% by the 1990 increase and 25% by the 1991 increase. The decline in the fraction of employees below the new minimum is consistent with the declining real value of the minimum wage during the 1980s.[10] Despite this decline, most of our industries have a sufficient number of employees 'bound' by the minimum wage to produce a detectable effect even for the 1991 most recent increase in the minimum wage.

2. Econometric Framework

Our analysis uses a forecasting approach to estimating the effect of minimum wage 'interventions'. This has several advantages: the application of forecasting equations to estimate interventions is well established; use of relatively short time periods avoids the assumption of structural stability common to time series research on this topic; issues of structural modeling, which have proven controversial in this literature, are also sidestepped.[11]

The Box-Jenkins methodology presumes that both employment and the average wage are serially correlated within each industry, and exploits this in constructing a forecasting models. Hence the lagged wages are included in the wage equation, lagged employment in the employment equation. Many prior studies of employment suggest that macroeconomic conditions (the unemployment and inflation rates, and a dummy for whether the economy is in recession) contain additional information that improves the forecasts and these are included in the model (see Brown et al.) We use relatively short sample periods, nine years, but monthly data provide an ample number of observations, at least 100 per estimate.

The equations for wages and employment are

$$1)\quad W_t = \sum_{s=1}^{12} a_{1s} W_{t-s} + \sum_{s=1}^{12} b_{1s} CPI_{t-s} + \sum_{s=1}^{12} c_{1s} Un_{t-s} + d_1 MWx_t + e_1 MWy_t + f_1 X$$

$$2)\quad N_t = \sum_{s=1}^{12} a_{2s} N_{t-s} + \sum_{s=1}^{12} b_{2s} CPI_{t-s} + \sum_{s=1}^{12} c_{2s} Un_{t-s} + \sum_{s=0}^{11} d_{2s} MWx_{t-s} + \sum_{s=0}^{11} e_{2s} MWy_{t-s} + f_2 X$$

where W_t is the average wage in a SIC industry; N is employment in an industry; CPI is the consumer price index; and UN is the unemployment rate. MWy and MWx are two minimum wage variables. **X** is a vector consisting of a constant, a dummy variable equal to one during months that the NBER has identified as periods of recession, and additional variables needed to adjust for trend stationarity, unit roots and cointegration.[12] All variables are in logs, and subscripts indicate lags in months.[13] The sample period is 8 years prior and one year after "the current increase," the specific increase in the minimum wage under study.

In line with our testing sequence, we turn first to equation 1, the wage equation. We are interested in measuring the effect of the increase in minimum wage, the minimum wage intervention, which always occurs one year prior to the end of our sample period. Measurement is complicated because until the 1991 increase, more than one increase had occurred in each nine year period, and there is no reason to expect their effects to be comparable. We resolve this problem by having rolling samples, each ending a year after a minimum wage increase, and including two minimum wage measures. MWy is the nominal minimum wage for the full sample period, while MWx is the nominal minimum for the first 8 years. MWx does not incorporate the last minimum wage increase in the sample, remaining at the last value of the minimum wage prior to the intervention being studied.[14] Conditional on inclusion of MWx, the coefficient on MWy measures only the effect of the last increase in the minimum wage.[15]

Most variables are entered with twelve month lags, but this is not the case for the minimum wage variables, MWx and MWy. Rather, these variables are contemporaneous with the industry average wage. This reflects the mandatory nature of increases in the minimum wage and the scheduling of the increase, which has occurred during the week prior to the collection of data for the EEH since 1957.[16] As firms are required to pay the minimum wage, the industry average wage will immediately incorporate the increase in the minimum wage and the changes will be contemporaneous in monthly data.

Finally, the nominal minimum wage is used as a proxy for its real value for three reasons. One, the appropriate deflator would be an industry specific index of production prices, but these are seldom available before the 1980s. Two, the combination of the logarithms of the nominal wage and the CPI approximates the real minimum wage. Three, during the period under study, the real minimum wage rises only when the nominal minimum rises, and this specification captures such increases.[17]

Our measure of the response of average wages to increases in the minimum wage is •, the coefficient on MWy. The wage filter consists of a 1-sided t-test, at a 5% significance level, that • is positive. We use industry/wage pairs that pass this test to identify instances where the minimum wage did indeed bite.

The next step is to estimate the employment equation, equation (2), on this subset of industries and periods. This equation differs from the wage equation in that lags on the minimum wage variables are also included to allow for convex adjustment costs of employment within firms. Again the timing of the increases and the reference period for the data suggest the use of lags zero through eleven rather than one through 12, as with the other variables.[18]

Our interest in the wage equation (equation 1) is limited to whether it is possible to detect a positive and significant effect of the minimum wage on the average wage. Equation (2) allows us to examine the employment effect of the minimum wage in two different ways. A pure time series approach with no explicit economic content is to test for Granger causality running from MWy to employment. This involves an F-test of the $\{e_i\}$ coefficients and the error correction term, where included, and is essentially a test of whether information about this minimum wage increase improves employment forecasts.

A significant Granger test can result from either a positive or negative employment response to the minimum wage. According to the neo-classical model, the employment effect is strictly negative. We calculate the elasticity of employment with respect to the minimum wage for each industry and minimum wage pair as the sum of the e coefficients, $\sum_{s=0}^{-11} e_s$, and use an asymptotic t-test to determine its sign and significance.

3. The Wage-Effect Filter

We turn first to the wage estimates. Use of the *mean* wage as the dependent variable provides a rigorous test of the wage effect. The relationship between increases in the minimum wage and change in the *average* wage is inherently weaker and more difficult to detect than the relationship between the minimum wage and change in total employment, the latter being a *marginal* effect.

Consider the effect of the 1990 increase in the minimum wage on the average wage in hardware stores. The average wage for hardware store employees two months before the increase in the wage was $6.65 and Table I indicates that 11.7% of the employees in this industry were 'bound' by the increase. Suppose that the average wage for 'bound' employees was the mid-point between the old and new minimum, $3.68; this would imply an average wage for unbound employees of $7.04. What then happens to the mean wage when the minimum rises? If employees earning less than the new minimum are all 'sacked', the new mean wage will increase by 6% to $7.04, half the magnitude of the reduction in employment. If instead, the only consequence of the increase is to raise the wages of the bound employees to the new minimum, the mean wage will increase by one cent, to $6.66 per hour.

The magnitude of the response of the mean wage will depend on the fraction of the labor force bound by the minimum wage, the gap between the minimum wage and the wages of those earning above the minimum, and whether increases in the minimum wage affects the employment or earnings of the bound labor force. Although the shift in the mean wage should not be undetectable, our *wage filter* criteria assures that only industries with large wage effects are tested for employment effects. This should further incline our second stage results toward detecting an employment result.

Results for the 212 wage tests are reported in Table II; Figures 1 & 2 summarize information about the p-values and distributions of elasticities from the wage test. We turn first to the figures. Figure 1 displays distributions of p-values for the test H_o: e = 0 vs. H_1: e > 0: where e is the coefficient on the contemporaneous minimum wage variable in equation 1. The upper figure is a box plot in which the center line is the median, the outer lines of the box are the 25th and 75th percentiles, and the "whiskers" above and below extend to the extreme values. On the right of this figure is the Null Distribution, the large sample distribution of p-values when the null hypothesis is correct. The 'boxes' for both the Low Wage and Youth estimates are clearly shifted downward, in a direction consistent with rejecting the null of no effect. The median values for these industries both fall at or below a p-value of .25, when they should be at a p-value near 0.5 to be consistent with the null. For both, the first quartile is quite low, at or below 0.05. The third quartiles are less than the median of the null distribution for both sets of industries.

The distribution graph below plots this same information with the 45 degree line playing the role of the Null Distribution in the box plot. Again, the frequency of low p-values, those which allows us to reject the null, is substantially above that which would be expected if the null hypothesis held. Further, the results are not the outcome of a few outliers, which would produce spikes above the 45 degree line. Rather, it appears that the population as a whole is shifted in a direction consistent with the alternative hypothesis.

Estimates of the elasticity of the mean wage with respect to the minimum wage are also of interest and these are provided in Figure 2.[19] The two diagrams on the left of the page display the full distribution for Low Wage (top) and Youth (bottom) industries; the two on the right separate the estimates according to their statistical significance. The mean elasticity for Low Wage Industries is 0.113 (SE: 0.020) with a median elasticity of 0.0969. For Youth Industries the mean and median are 0.0666 (SE: 0.0182) and 0.0637 respectively.

As the graphs illustrate and the mean and median confirm, the weight of these distributions lies above zero. It is useful to separate the statistically significant and non-significant elasticities, provided in the right hand graphs in the figure. It is no surprise to find that in each set of industries, the statistically significant estimates tend to be larger than the insignificant ones. However, the considerable overlap between statistically significant and insignificant estimates in the box-plots make clear that it is not merely the largest point estimates, the upper tail, which are statistically significant. The overall distribution of the Youth industries is clearly bimodal; i.e., there are two distinct groups of events here. The distribution in the Low Wage industries is less obvious. Nevertheless, when the statistically significant and insignificant estimates are merged together, the distribution displays a prominent positive bulge, due primarily to the statistically significant estimates.

Turning to our wage filter, we assess the estimated wage elasticity against a null of a zero or negative effect using a 10% significance test. Although the analysis in Figures 1 & 2 suggests a considerable wage effect in most industries, only 54 out of 212 tests, one in four, meet the conventional criteria. As suggested at the start of this section, such criteria may be too rigorous a standard for an average wage measure, but it has the advantage of limiting the employment estimates to markets in which there is no question about the wage effect. We now turn to tests of the employment effect when the minimum wage clearly bites.

4. The Employment Impact When the Minimum Wage Bites

Estimates of our 54 employment elasticities may be found in Table 3 and these are depicted in the bottom of Figure 3. The typical estimated elasticity is not significantly different from zero in either a one or two tailed test but estimates are widely dispersed, ranging from -14.3 to 3.20. There are a number of industry/wage pairs which are large and negative and several which are large and positive. The result, depicted in the lower right of Figure 3, is a distribution which is centered on zero with a large variance and a negative skew. This skewness is reflected in the measures of central tendency. The mean estimate is -0.6, substantially below the range proposed by Brown, Gilroy and Cohen. Measures of central tendency can be sensitive to large outliers. That this is the case here is clear from both the median, -.15 and the 10% trimmed mean employment elasticity, -0.23. These figures, well within the range proposed by Brown, Gilroy and Cohen, provide the strongest support for the standard hypothesis. The modal effect falls between 0 and 0.6, substantially above the range proposed by Brown, Gilroy and Cohen.[20]

The significance of the employment effect for these 54 events are judged by two tests: first, a Granger test of whether addition of the contemporaneous minimum wage variable improves the fit of the forecasting equation; and second, a one sided t-test of the sum of the minimum wage elasticities to determine if they are, as expected, negative. The Granger tests provide little evidence that the minimum wage affects employment. As shown in the box plot and p-value frequency distribution in the top of Figure 3, the Granger test is virtually identical to the what would be expected if the null hypothesis of no effect were true. The Box plot for the Granger lies slightly above that for the Null Distribution, the p-value line for the Granger is somewhat above the 45 degree line in the lower corner of the distribution graph, but then follows that line very closely.

Results from the one sided Sum of Lags test are similar. Few of the tests are negative and statistically significant at a 5% level, eight out of 54. As indicated by the box plot, the distribution of p values is more dispersed than the null but otherwise similar. The distribution of one sided p-values has a heavy left hand tail, but then follows and falls below the 45 degree line. The dispersion of the estimates is more apparent in two tailed tests of the significance of the Sum of Lags. The number of negative and significant tests remains the same, eight, but there are six positive and significant tests (in a 10%, 2-sided test). The box plot of the p-values lies below that for a null distribution. Similarly, the plot of the distribution of p-values lies above the 45 degree line. Both are consistent with a distribution which is centered on zero but which is more widely dispersed than the null distribution.

The dispersion of the estimates is not entirely surprising. Disaggregated data are likely to show greater variance and these industry data are considerably more disaggregated than the data on teens and young workers typically used in time series analysis. The results suggest that there are instances in which increases in the minimum wage can have a large negative effect on employment, as well as instances in which it appears to have large positive effects. The relatively small number of significant results, and the apparent randomness and dispersion of significant results does not point toward any simple rule for when increases in the minimum might be expected to have a negative effect on employment.

To summarize, the estimated effect of the minimum wage on employment is notably weak. This sample is defined by the presence of a positive and statistically significant minimum wage bite. Despite this filtering, fewer than three-tenths of the estimated employment effects are statistically significant, and barely one-seventh are negative and significant (at a 10% level). Given the nature of the sample, these results are not sufficiently strong to support the hypothesis that increases in the minimum wage will typically cause employment to decline.

6. Conclusions

A necessary condition for the minimum wage to influence employment is that it be binding on at least some employees in an industry. Most research on the minimum wage has selected groups subject to the minimum wage using *a priori* criteria.

This research uses an approach that allows us to test the theory more precisely. Looking at minimum wage interventions in 34 industries between 1967 and 1991, we first test to determine whether increases in the minimum wage have resulted in a detectable effect on the average wage of an industry. We then limit our study of employment effects to those industries in which there was a statistically significant and positive effect on the minimum wage. Although a test of the average wage may be too rigorous, and may exclude some instances in which minimum wage increases were binding on employees in the industry, it assures that there is a substantial wage effect in those industries which 'pass' the wage filter.

The wage filter equations indicate that minimum wage increases are often not binding. The industries selected for this study, industries at the bottom of the industry wage distribution or industries with large numbers of young employees, should have been quite sensitive to the effects of the minimum wage. Despite this, only 54 out of 212 industries demonstrated significant positive relationships between the minimum wage and the industry average wage. This suggests that the lack of a relationship between the minimum wage and employment found in many contemporary studies might be a result of a minimum wage which was not binding. Perhaps the lack of results was because researchers were looking at the wrong markets, or mixing markets in which the minimum wage mattered with many others in which it did not.

Results from our employment equations do not support this view. A majority of our employment estimates are not statistically significant. Those which are, are fairly evenly distributed between positive and negative effects. Although the median employment elasticity fits the conventional view, and the mean, if anything, goes beyond that, the figure is an artifact of several unusually large and suspect estimates. Elimination of these extreme estimates resulted in a mean very close to zero. The dispersion of estimates, whether by year or by industry, does not support any simple reconciliation of the results with the standard view of the effect of the minimum wage on employment.

The large and statistically significant positive employment effects are puzzling, especially for industries in which we have already found similar wage effects. In response to similar findings that Card and Krueger reported, Finis Welch skeptically suggested that they were trying to repeal the law of demand.[21] An explanation of this finding would increase its credibility and perhaps lower the level of acrimony surrounding this issue.

The law of demand contains an important qualification, one that is quite common in economic analysis: to wit, *ceteris paribus*. What sorts of things might not be equal? And why? Begin with the second question. It is plausible that Akerlof's (1970) model of the *Market for Lemons* aptly characterizes some labor markets, especially low wage and youth labor markets. When the average wage is low, potential suppliers of high quality labor leave the labor market rather than work. Increases in the minimum wage raise the average wage, and by attracting higher quality labor into the market, make it profitable for firms to increase employment.

Why would firms wait for an increase in the minimum wage to hire high quality labor? Why not attract high quality labor by offering higher wages? That is certainly what banks do in models of credit rationing.[22] Instead of raising interest rates to clear the market, banks are thought to keep them low to attract higher quality borrowers. Of course, they attract more borrowers of all quality, and have to screen them carefully; but at the lower interest rates, the average quality is higher, and the screening costs can be amortized over what is hoped to be a long term relationship.

Certainly in many labor markets, higher wages and salaries are used to attract higher quality candidates, and screening (and the associated costs) are important, as are long term relationships. This is not the case in low wage and youth industries, where turnover is typically high. In a high turnover industry, raising wages is not an efficient way to improve average quality of potential employees because the screening costs would have to be repeated too frequently and could not be amortized over a long period.

Furthermore, information networks among workers already within an industry are likely to be more efficient than among those who are outside of it or entirely outside of the labor force. If a single firm were to raise its wage offer, most of those who would hear about it would be those already in the industry, presumably the suppliers of lower quality labor that the firm hopes to avoid. In contrast, increases in the minimum wage are well publicized, and if they draw higher quality workers into the labor force, would be much more effective toward this goal.

It is not our claim that increases in the minimum wage could not have an adverse impact on employment. The historic experience with the minimum wage has been one of moderate increases and it is this experience which we analyze. Movement outside of this experience, a doubling of the minimum wage, might well have an employment effect. However, our research supports the view that moderate increases in the minimum wage do not affect employment in a consistent and systematic fashion. Judging by the acrimony of recent debates about the minimum wage, it strikes us that even where it does bite, the bark has been worse.

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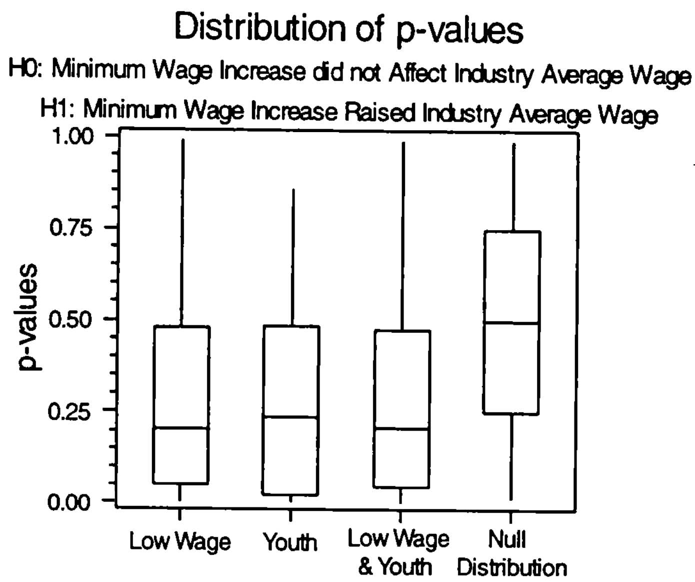
Figure 1
The Wage-Effect Filter



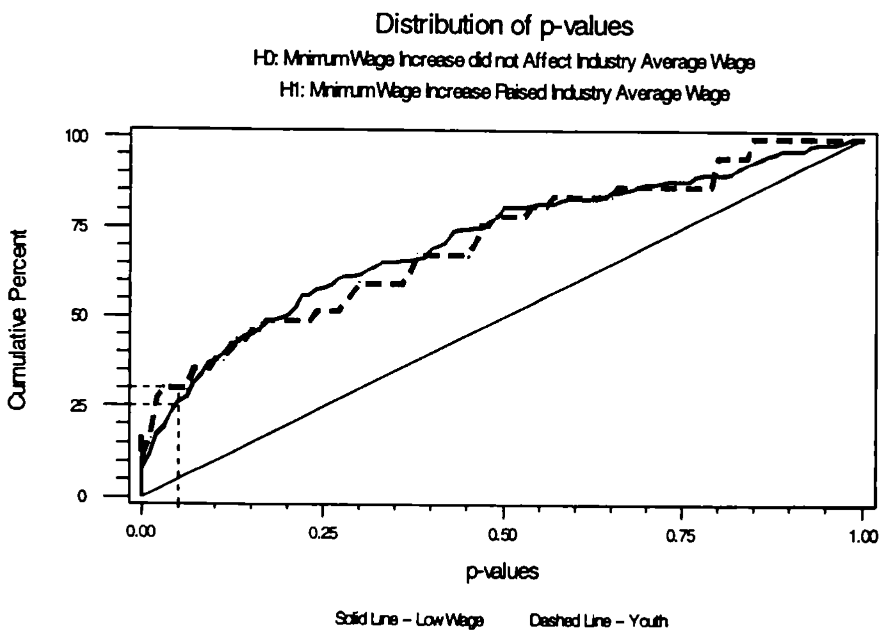
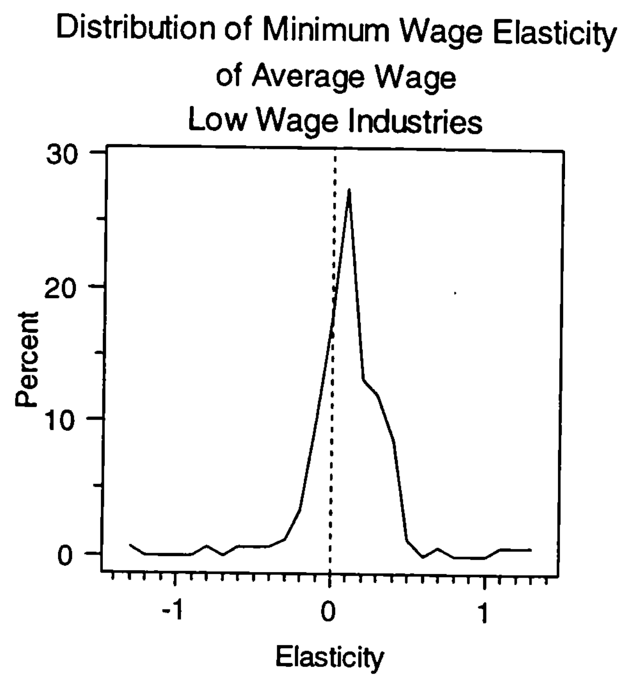
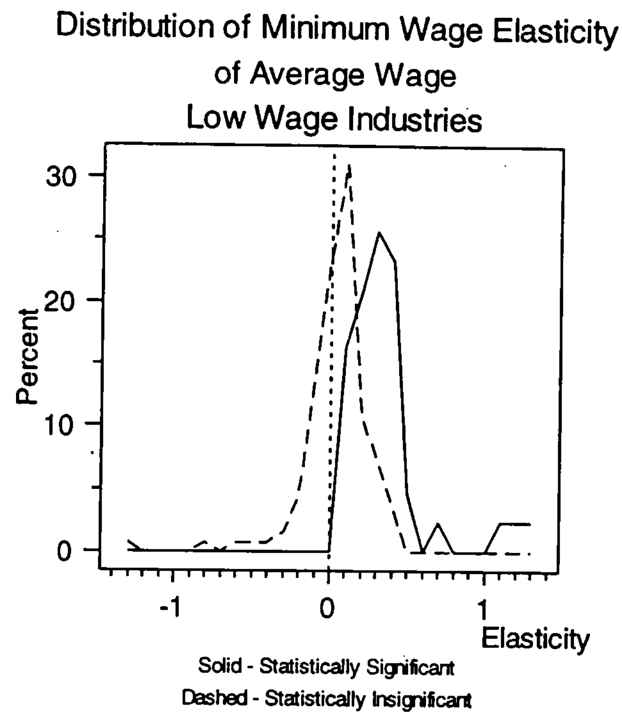
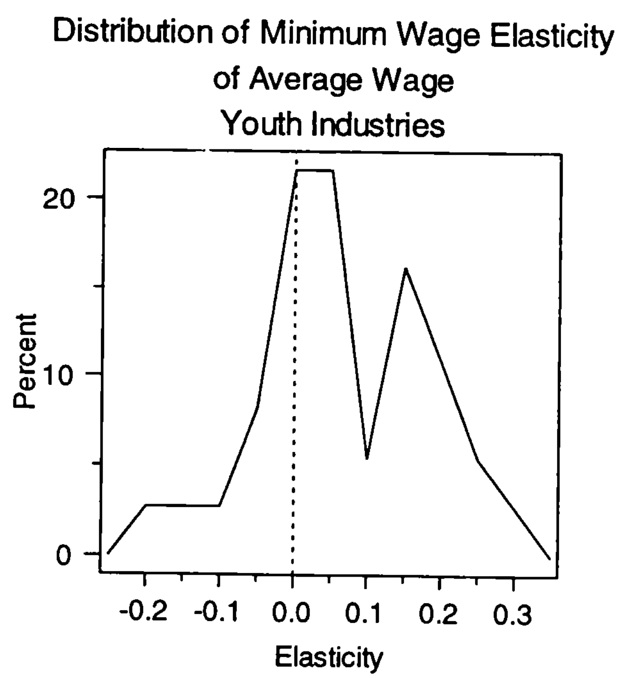


Figure 2
Distribution of Estimated Minimum Wage Elasticities of Average Wage







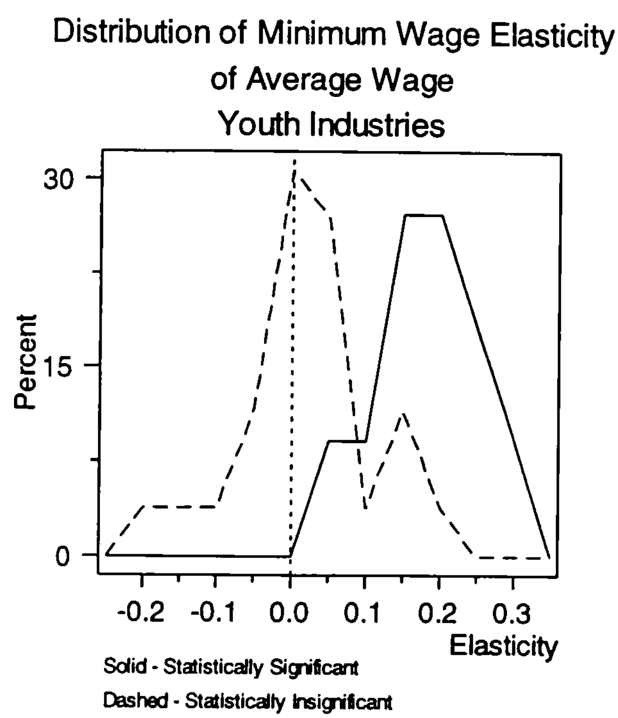


Figure 3

Employment Effects When Wage Effect is Detected

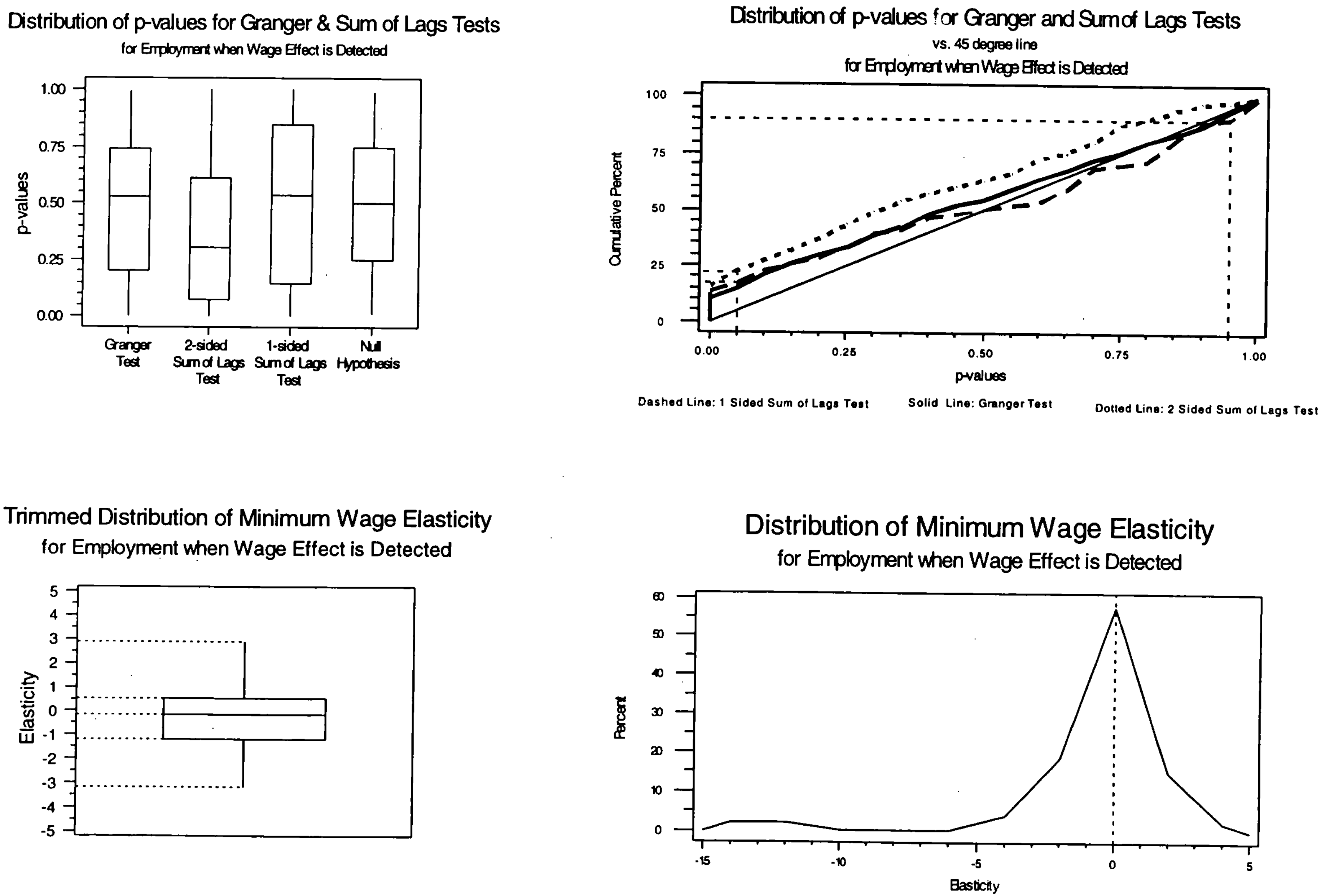


TABLE 1

Fraction of Employees Earning Below the New Minimum Wage

SIC	Industry	91	90	81	80	79	78	76	75	74
LOW WAGE INDUSTRIES:										
533	Variety Stores	9.1	27.0	16.2	16.2	54.5	59.8	21.6	14.6	50.1
562	Women's Clothing	m	m	27.2	27.1	40.2	46.8	26.5	22.8	33.5
835	Child Day Care	8.7	20.6	na	na	na	na	na	22.8	33.5
566	Shoe Stores	5.5	13.4	11.0	9.9	47.6	25.8	na	na	
565	Family Clothing	m	m	27.2	27.1	40.2	46.8	10.3	9.2	32.6
554	Gas Service Stn	10.2	15.7	21.0	21.3	40.2	46.8	26.5	22.8	33.5
546	Retail Bakery	7.5	20.0	12.4	11.6	49.9	46.6	23.0	19.1	42.1
238	Misc Apparel Str	8.9	15.0	32.1	32.2	s	61.0	s	s	s
525	Hardware Str	4.0	11.7	12.4	11.7	38.9	42.3	33.6	31.7	33.1
591	Drug Stores	4.8	12.4	21.8	20.5	19.4	28.2	13.1	11.6	16.2
721	Laundries	6.0	12.5	18.6	18.8	38.0	45.0	21.9	18.9	32.8
723	Beauty Shops	13.9	19.7	22.0	19.3	43.2	42.2	22.2	22.6	36.9
701	Hotels & Motels	8.5	14.2	45.6	35.4	38.9	42.6	22.2	15.3	32.9
734	Building Services	6.7	11.9	14.4	14.4	49.1	45.9	29.3	32.6	48.7
805	Nursing Facility	4.2	9.3	37.9	33.0	32.4	31.5	12.0	10.0	13.6
531	Department Str	4.2	9.7	39.3	37.5	50.3	48.4	37.0	34.5	43.9
225	Knitting Mills	3.7	9.2	9.1	7.4	33.5	33.3	36.2	35.6	28.4
599	Retail Stores	6.0	12.0	27.1	23.5	26.9	25.4	10.7	10.3	18.6
239	Misc Fabr Textile	2.3	3.9	8.1	9.8	37.7	34.6	20.4	18.4	30.5
561	Mens Clothing	m	m	27.2	27.1	29.7	28.0	8.8	9.1	22.7
553	Auto/Home Supply	1.5	4.7	10.8*	12.7*	40.2	46.8	26.5	22.8	33.5
						25.2	21.0	9.7*	7.6*	9.7
YOUTH INDUSTRIES: by SIC										
58	Eating/Drinking Pl	25.4	35.2	78.4	74.2	61.6	63.9	73.4	68.0	53.6
541	Grocery Stores	6.4	13.2	34.1	29.2	23.3	21.4	26.2	28.8	18.1
311	Leather Tanning	-	7.4	s	s	s	s	s	s	s

m - missing
* - change in definition

na - not a distinct category in earlier CPS system
s - very small sample size

TABLE 2

Significant test Results

H_0: Minimum Wage Increase had no effect on the Industry Average Wage
H_1: Minimum Wage Increased the Industry Average Wage

	91	90	81	80	79	78	76	75	74	68	67
Low Wage Industries by SIC											
533	b	.	.	a	a	a	b	.	.	.	.
562	b	.	.	.	.	a	b	.	.	.	.
317	.	b	b	.	.	a	b	.	.	.	.
399	.	.	.								
835	.	b	.								
566	.	.	.	.	.	.	.	.	b	.	.
565	.	.	a	b	b	.	.	.	.	.	.
554	.	.	.								
546	.	.	.								
302	.	.	.								
238	.	.	.								
594	.	.	.								
525	.	b	.								
244	.	.	.								
591	.	.	b	a	a	a	a	.	.	.	.
721	b	.	b								
316	.	.	.	.	.	a	.	.	.		
723	.	.	.								
701	.	.	.	.	b	a	.	b	a	.	.
734	.	.	.								
805	.	b	a								
531	.	.	.	b	a	a	a	.	.	b	.
225	.	.	.	a	b	a	.	.	.	b	.
599	.	.	.								
239	.	.	.	.	.	.	.	.	.	.	.
561	.	.	b	.	.	.	a	.	.	.	.
553	.	.	.								
Youth Industries by SIC											
56	.	.	a	.	.	b	b	.	.	a	b
541	.	.	.								
58	a	a	.	b	a	a	.	.	b		
311	.	.	.	.	.	.	.	.	.	.	.
753	.	.	.								

Legend: a - significant at 0.01 level
b - significant at 0.05 level
. - not significant at 0.05 level

	# events	a	b	Total	
Low Wage	175	20	23	43	(25%)
Youth	37	6	5	11	(30%)
Total	212	26	28	54	(25%)

Table 3
Employment Elasticity of the Minimum Wage When a Positive Wage Effect is Detected

	91	90	81	80	79	78	76	75	74	68	67
Low Wage Industries											
533	2.48			2.81	-0.46	0.28	**3.76**[a]				
562		0.55				0.31	**4.73**[a]				
317		-0.88	1.79								
835		**3.20**[a]									
566									-1.27		
565			**-14.3**[a]	**-12.8**[a]	-2.81						
525		-0.71									
591			**-1.96**[b]	-1.32	-0.80	**-2.20**[a]	0.74				
721	-0.80		**-1.43**[a]								
316						0.61					
701					**2.61**[a]	0.36		-0.38	**0.54**[c]		
805		0.45	0.09								
531				0.48	-0.23	0.26	-0.91			**-1.33**[c]	
225				1.47	0.33	0.44				0.61	
561			-0.75				1.49				
Youth Industries											
56			**-3.19**[a]			0.00	**1.34**[a]			-4.20	-1.86
58	0.11	-0.09		-0.47	**-1.86**[a]	-0.29			-0.12		

a: significant at 0.01 level (2 sided)
b: significant at 0.05 level (2 sided)
c: significant at 0.10 level (2 sided)

	# events	total -	a-	b-	c-	total +	a+	c+
Low Wage	43	6	4	1	1	5	4	1
Youth	11	2	2			1	1	
Total	54	8	6	1	1	6	5	1

Notes

[1]In a series of studies, Card, Katz and Krueger have concluded that several recent changes in the minimum wage had no noticeable adverse effect on employment (Card 1992a, 1992b; Card and Krueger 1994, 1995; Katz and Krueger 1992; Krueger 1995). Neumark and Wascher (1992, 1995a and 1995b), Currie and Fallick (1993), Kim and Taylor (1995), and Deere, Murphy and Welch (1995) present contrary evidence. Many of these writers have sharply criticized each others' analyses (Card, Katz and Krueger 1994, Ehrenberg 1992, Deere, Murphy and Welch (1995), and Neumark and Wascher 1994, 1995c).

[2]This takes compliance and enforcement largely for granted: see Ashenfelter and Smith (1979).

[3]Presumably, employers determine levels of wages, fringe benefits and other expenses that minimize overall labor costs. When minimum wage legislation raises wages, partial offsets may well be possible by reductions in other costs but the possibility of complete offsets would be a surprise. That would suggest behavior that is not cost-minimizing, a more serious contradiction of the neo-classical model than the minimum wage literature typically contemplates: but see Reynolds (1965).

[4]All of the industries have sufficiently long time series for examination of the minimum wage increases of 1981, 1990 and 1991. Only fifteen are long enough to study increases back through 1974, and only thirteen extend far enough back for analysis of the 1967 and 1968 increases.

[5]Our data source do not provide industry wage distributions; instead we use average hourly wages to proxy the proportion of employees at or near the minimum. We consider this issue further in our discussion of Table I.

[6]The 1989 ORG files, a compilation of earnings records of the 1989 CPS, have 650,000 observations and include wage data for all employed participants.

[7]We based our selection of Youth industries on age distributions for 3 digit industries, constructed from the 1989 Out-Going- Rotation file from the BLS. Several industries that satisfy these criteria are not among those analyzed because of insufficiently long time series. For example, automotive rentals (SIC 751) and dairy product stores (SIC 545) both satisfy the youth criteria, but wage data are available only from the mid 1980s.

[8]The Consumer Price Index (CPI) and Unemployment Rate (UN) are from the BLS.

[9]The May CPS is used for increases prior to 1980, the larger ORG files are used from 1980 on. CPS industry classifications are broader than the SIC system used in the EEH data and some CPS industries aggregate several SIC industries. Table I is limited to industries in which CPS and SIC industries are closely matched. Although the CPS includes all employees, the EEH is limited to production and non-supervisory employees, about 80% of total employment. The fraction of EEH 'employees' below the minimum wage will be larger than that found in the CPS.

[10]Until 1981, Congress raised the minimum wage when it fell below 40% of the average manufacturing wage. By the time of the March 1990 increase, the minimum wage stood at 31.4% of the manufacturing average; in April 1991 it was 33.6% of that average. The long period in which the minimum wage remained unchanged, combined with market pressures on wages over the intervening nine years, greatly reduced the fraction of employees below the new minimum.

[11] While a structural model would use available information more efficiently, part of the current controversy revolves around the specification and measurement of the variables in structural equations (Card, Katz and Kreuger, 1994). Much as reduced form equations, a forecasting model avoids such

disputes.

[12]In keeping with contemporary time series techniques, we conduct tests for trend stationarity, unit roots and cointegration in each of our 212 samples. If we find trend stationarity, X includes a linear time trend. First differences replace levels of any variable displaying a unit root. The logarithms of the CPI and minimum wage both display unit roots, so we test for cointegration between these variables and the industry average wage or industry employment. Where we find it, we include one lag of the residual from the cointegrating regression in X (the error correction mechanism).

A Granger causal relation runs from the Consumer Price Index (CPI) to the minimum wage, but not from the minimum wage to the CPI, for both the full sample period and for all but two of the nine year subsamples. There is no evidence of Granger causation in either direction between the Unemployment Rate (UN) and the minimum wage in the full sample or in any sub samples. Including inflation in the equation will avoid confusing the impact of inflation with that of the minimum wage without masking minimum wage effects

[13]For the average wage, the number of degrees of freedom varies from 66 to 73 degrees of freedom. Five of the average wage series are cointegrated with the RHS variables: SIC 554, Gasoline Service Stations; SIC 302, Rubber and Plastic Footwear; SIC 594, Miscellaneous Shopping Goods; SIC 805, Nursing Facilities, and SIC 553, Auto and Home Supply Stores. The error correction mechanism, one lag of the residual from the cointegrating equation, is included in the X matrix to account for this in the Granger Causality tests.

For employment, the number of degrees of freedom for the unrestricted regression varies from 45 to 59, depending on the sample period, the industry, and the results of the AWS and cointegration tests. Three of the employment series are cointegrated with the CPI and nominal minimum wage: SIC 244, Wood Containers; SIC 316, Hotels and Motels; and SIC 239, Miscellaneous Fabric and Textile Manufactures. The restricted regressions have 9 to 13 more degrees of freedom. While the degrees of freedom may seem small to those accustomed to dealing with large cross section data sets, it is common in time series research.

1990 has a large number of degrees of freedom because no increases in the Federal minimum wage occurred during the preceding 8 years, so the d coefficients are set to zero. 1981 has a slightly smaller number of degrees of freedom for industries that have a unit root and/or begin in 1972. One more degree of freedom is lost whenever either a trend term is needed to stabilize any variable, or employment is cointegrated with any of the other variables.

[14]MWx is not included inthe equation that studies the 1990 increase since no change occurred in the previous eight years.

[15]Splitting the minimum wage variable controls for increases in the minimum occurring prior to the increase under examination, and allows us to interpret the sum of the coefficients e_i as the minimum wage elasticity of employment (in this industry at the time of the change in the minimum wage).

[16]Minimum wage increases have taken place on the first or third of the month since March 1956: before then, the last week of the month was the practice.

[17]This may not be the case were industry specific prices used.

[18]The twelve month lag used in this research is consistent with the literature. Most studies find that employment adjustments are completed within four quarters (Hammermesh, 1993), Neumark and Wascher (1992) also use a one year adjustment period.

[19]Outside values, points more than one and half times the inter-quartile range away from the first and third quartile, are not shown to allow larger scale.

[20]Finally, there is little evidence of a pattern of effects by industry or year; there is some clustering of negative and significant outcomes in 1981 and in the family clothing (SIC 565) and drug stores (SIC 591), but such clustering is sparse and likely due to chance.

[21]Comments at the AEI symposium on the minimum wage, June 1, 1995.

[22]See Stiglitz and Weiss (1981), and Jaffee and Russell (1976).

Joyce —
Pls. mail a copy
of this to Jon
Orszag & return to me.
Thx.
Alan

Minimum Wage Legislation and the Working Poor

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May 1995
Revised version: December 1997

ABSTRACT

Despite the fact that a primary goal of minimum wage legislation is to raise the incomes of the working poor, the effect of minimum wages on the earnings of working families with incomes below the official poverty level has been consistently neglected in the literature. Using data from the Current Population Survey, we estimate a family earnings equation for the years immediately following the 1990 and 1991 increases in the federal minimum wage and use this equation to estimate the post-increase earnings of working families who were poor or nearly poor in 1989. In order to determine whether this change can be attributed to the increase in the minimum wage, we take advantage of the natural experiment suggested by Card (1992) in which states are categorized by the fraction of workers in the state who will receive a wage increase as a result of the new legislation. We then use a difference-in-difference approach which compares the earnings changes of working poor families in "high-impact" states (those with a large fraction of workers affected by the increase) and "low-impact" states. We find that the earnings growth of poor and near-poor families in high-impact states was approximately ten percentage points higher than those of similar families in low-impact states. These results are robust for a variety of definitions of the working poor and over several time periods covering the 1990 and 1991 federal minimum wage increases.

JEL Codes: D31, I30, J18, J38

I. INTRODUCTION

Studies examining the effect of minimum wages on employment abound. Several recent empirical papers on the subject have received widespread attention. However, very little research addresses the equally relevant question of how minimum wages affect the group of people that they are intended to help—the working poor. This paper examines the impact of federally mandated wage floors on a number of labor market characteristics of families in which at least one member works. We compare the effects for families in different income categories, including those whose total family income falls below the official poverty threshold and those with incomes slightly above the poverty line. Our most noteworthy finding is that the earnings of both poor and nearly poor working families rose substantially as a result of the 1990 and 1991 increases in the federal minimum wage. Those who lived in states where the impact of the wage increase was large earned between 7.8 and 10.6 percentage points more than otherwise similar families living in states where the impact was small. Interestingly, even high income families earned more if they lived in states where the minimum wage had a strong effect, although the effect was not as strong as for lower income families.

Our focus on the working poor is not meant to imply that the effect of the legislation on non-poor families is unimportant. However, much of the rhetoric surrounding the minimum wage reflects a conviction that people who are willing and able to work should be able to earn enough to support themselves and their families. Before asking what consequences minimum wages have for non-poor families, we should be sure that they have the intended effect on those they are meant to help. The importance of understanding the distributional effects of minimum wage legislation was recognized by Stigler as long ago as 1946, but the first rigorous estimation of the distributional impacts was not undertaken until Gramlich did so in 1976. Despite this, the effect of minimum wages on employment has continued to dominate the literature. Most previous studies do recognize that certain demographic groups, such as teenagers, women, and minorities, are more likely to be affected by wage floors because of their disproportionate representation in the ranks of low-wage workers. While these are important aspects of the distributional issue, they still ignore a fundamental piece of the puzzle; the distinction among workers in different income classes. See Card and Krueger (1995, chapter 9) and Horrigan and Mincy (1993) for recent analyses that directly consider the impact on low income families. Careful reading of the literature that does focus on distributional effects reveals that the authors have investigated questions that are slightly different than the one we are asking; for example, they have studied the impact on the overall income and wage distributions or on the *overall* incidence of poverty (e.g., Gramlich 1976, Horrigan and Mincy 1993). These papers show that the minimum wage does decrease earnings inequality slightly, but it has little effect on the distribution of family income or on the poverty rate in general. This is not surprising since, as a group, minimum wage earners do not comprise a large percentage of the poor, nor are they heavily concentrated at the bottom of the income distribution.

Wage floors are a blunt instrument (at best) for alleviating poverty and we agree that there are other policies that are better suited to the general goal of helping the poor. However, the minimum wage remains an important topic for study because of its perceived role as an aid to the *working* poor. Throughout the 1960s and 1970s, a full-time, year-round worker earning the federal minimum wage was able to support a family of three at or above the poverty level. However, this has not been the case since 1979, despite increases in the federal wage floor in

1990, 1991, and 1997 (Levitan, Gallo, and Shapiro 1993).[1] This is particularly striking given the prominent role of work requirements in welfare reform efforts at both the state and federal levels, most of which are based on the expectation that former welfare recipients will be self-sufficient once they are working. Minimum wages are often criticized because they may have adverse employment effects, but even if this is true they may serve a useful role by affirming our society's emphasis on the value of work.[2] In addition, minimum wages enhance the effectiveness of the Earned Income Tax Credit (Blank 1994).

A secondary reason for continuing to focus attention on the minimum wage is the fact that federal and state legislators persistently do so. The federal minimum wage increased from $4.25 to $4.75 per hour on October 1, 1996 and to $5.15 per hour on September 1, 1997. Some states have increased their minimum wages by an even greater amount. For example, Oregon's minimum wage was increased to $5.50 per hour on January 1, 1997 and will be raised an additional $.50 per year for the next two years, resulting in a wage floor of $6.50 per hour starting January 1, 1999.

In addition to our focus on the distributional effects and the impact on the working poor,

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[1]A person who works full-time year-round at the most recently mandated federal minimum wage of $5.15 per hour will earn $10,300 per year. The 1996 poverty threshold for a family of three was $12,516 (U.S. Census Bureau 1997).

[2]In fact, a recent series of studies has contradicted the traditional belief among economists that increases in minimum wages necessarily cause decreases in employment (Card and Krueger 1995). Wellington (1991) also found little evidence of an employment effect for the 1980s. On the other hand, Neumark and Wascher (1995) and Currie and Fallick (1996) are recent papers that support the traditional view. A review of earlier research by Brown, Gilroy, and Kohen (1982) reports that a 10 percent increase in the minimum wage results in a decrease in employment of from 1 to 3 percent for teenagers, with the lower end of the range being more likely. These very inelastic estimates imply that even if there is an employment effect, the aggregate earnings of minimum wage workers should increase.

the inclusion of adults in our sample distinguishes our research from previous literature. Because teenagers are disproportionately represented among those earning the minimum wage or less, there is also a disproportionate number of studies focusing on teens in the minimum wage literature. A few studies have also addressed the effects of minimum wage laws on young adults (aged 20-24), but older workers have been ignored in most analyses. This is an important omission, especially considering that over seventy percent of workers earning between $3.35 and $4.25 per hour in March 1990 were aged 20 or older (Card and Krueger 1995, p.282). In their influential 1982 review of minimum wage research, Brown, Gilroy, and Kohen noted that "Uncertainty about the effects [of the minimum wage] on adults is a serious gap in the literature, since half of all minimum wage workers...are adults." (p.524). Thirteen years later, the fraction of minimum wage workers that are adults had grown, but the gap in the literature remains.

Our analysis follows the standard outline. We discuss our methodology in section II, present results in section III, and give our conclusions in the final section.

II. METHODOLOGY

Much of the minimum wage research over the past two and a half decades has relied on the analysis of time series data. The lack of a clear counterfactual is the primary criticism of the time series approach. Although time series analysis allows one to examine changes in relevant variables across a period in which the minimum wage is increased, it provides only minimal indication of what might have happened in the absence of the new legislation. Early minimum wage studies (e.g., Lester 1964) took advantage of the natural experiment created by the cross-state variation in the impact of a nationwide wage floor. This methodology, now frequently called the "difference-in-difference" approach, also relies on a before-and-after comparison *within* each state, but in addition, it allows *cross* state comparison, which provides an easily identifiable control group. Card (1992) revived the natural experiment methodology, measuring the impact of the 1990 and 1991 minimum wage increases by the fraction of workers initially earning less than the new minimum in that state.

A. Definitions of "the working poor"

A family is considered to be poor if and only if total family income falls below the federal poverty threshold. However, definitions of "the *working* poor" vary, depending on the amount of time a person is required to work in order to be counted as working. Our empirical analysis examines five different definitions. A family with income below the poverty line and at least one member who either works or looks for work for at least 27 weeks in a given year is defined as working and poor by the U.S. Department of Labor (definition 1). This criteria is fairly strict because it requires a family to participate in the labor force for at least half of the year. On the other hand, it counts people who are unemployed as well as those who are actually working. The most stringent definition (definition 2) counts a family as working only if at least one family member works full-time (at least 35 hours per week), year-round (at least 50 weeks per year). Because many poor families have a member who works only part-time or does not work for the entire year, some researchers consider families in which at least one person works at least part-time or part of the year to be working and poor (definition 3) . Lastly we consider the separate criteria of definition 2: families with at least one full-time (not necessarily full-year) worker (definition 4) and those with at least one full-year (not necessarily full-time) worker (definition 5).

Many working families have very low incomes, but not low enough to be officially counted among the poor. Thus, we also consider the impact on "near-poor" families, which we define as those who earn between 100 and 125 percent of the poverty line. In addition, we compare the effects on low income families with those of their higher income counterparts.

B. Measuring the impact across states

Variation in the fraction of workers affected exists both because some states had pre-established wage floors greater than the new federal standard and because state wage distributions reflect differences in the cost of living, industrial mix and inflation conditions in each state. In theory, the impact of an increase in the federal minimum wage in a particular state can be measured by the fraction of workers in that state who are earning less than the new minimum when it takes effect. However, some workers are employed in firms that are exempt from the legislation, and others can be paid below-minimum wages by virtue of their age or school enrollment status. Finally, some firms may pay below-minimum wages in violation of the law. For all of these reasons, the fraction of workers initially earning less than the new minimum wage is an imperfect proxy for the impact of the legislation.

Ideally, one would use only those workers who are covered by the new law in the calculation of the fraction of workers affected. In practice it is very difficult to determine exactly who is covered and who is not. The size of the uncovered sector is also difficult to estimate due to the number and complexity of the exemptions to the federal law, as well as to the existence of state laws that supplement the federal law. Schiller (1994, p.132) reports that "the U.S. Department of Labor estimates that only thirty percent of all employers and approximately seventy percent of the workforce is directly covered by Fair Labor Standards Act...statutes." Levitan, Gallo, and Shapiro (1993, p.50) claim that "by 1990 the law covered 87 percent of all private nonsupervisory employees."

While measurement error of this type may affect estimates of employment effects, it is less important in the analysis of distributional effects. When we ask whether or not increasing the minimum wage increases the earnings of working poor families, we do not necessarily care whether or not those families have moved into the uncovered sector. If they are earning more, then it doesn't matter whether or not they are covered.[3]

A somewhat crude, but fairly easy way to alleviate the problem is to use the fraction of workers earning *at least* the old minimum, but less than the new minimum. The underlying assumption is that anyone who was not earning at least the original minimum must work in the uncovered sector. If the new law does not expand coverage to these individuals, then they will not be affected by it. Although this does not eliminate everyone who may be in the uncovered sector, it does reduce the measurement error. Card (1992) and Card and Krueger (1995, chapter 4) use this measure to study the impact of the 1990 and 1991 legislation on the employment of teenagers.

Using this measure, we calculate the fraction of workers affected by the April 1, 1990 increase in the federal minimum wage from \$3.35 to \$3.80 per hour in each state and by the subsequent increase to \$4.25 per hour that took place on April 1, 1991 (table 1). As table 1 indicates, there is substantial variation across states in the fraction of workers affected by the 1990

[3]Of course, we may want to know how the effects of the wage increase are distributed *among* the working poor as well. This is an important question, but beyond the scope of the present paper.

change: 0.4 percent in Alaska, 5.3 percent in Michigan (the median state), and 12.9 percent in Mississippi.[4] There is similar variation across states in the fraction affected by the 1991 change: from a minimum of 0.7 in Alaska to a maximum of 13.2 in Mississippi (column 4 of table 1). The final column of table 1 shows the number of workers earning between \$3.35 and \$4.25 per hour *after* the second change in the federal minimum. The numbers remain positive due to noncompliance, misreporting, and exemptions as discussed above. However, the fraction below the new minimum is substantially smaller than in the earlier period (column 4) and is uniformly distributed across states. For use in the difference-in-difference analysis, we classify states as "high impact" if the fraction of workers affected is above the median (5.3%) and "low impact" otherwise.

C. Family-level Analysis

Card and Krueger (1995, chapter 9) examine the effect of minimum wages on poverty rates of the working poor. They use data from the March 1990 and 1991 Current Population Surveys (CPS) to estimate a regression equation with the change in the poverty rate among all workers in a state as the dependent variable and the fraction of workers affected in each state as the key independent variable. Their estimates of the coefficient on the fraction affected variable are uniformly negative, implying either greater decreases or slower growth in the poverty rate among

[4]These estimates are based on self reported wage rates for hourly workers and the ratio of weekly earnings to usual hours for salaried workers. The question of whether to include or exclude salaried workers has been a point of contention in the literature (see Klein 1992). In our data the correlation in the fraction affected estimated with and without salaried workers exceeds 0.99. Thus the distinction is irrelevant for our purposes. We use the broader definition in order to maximize the sample size.

workers in high-impact states. This is encouraging for proponents of the minimum wage. However, these estimates are generally statistically insignificant. This problem may be due to the fact that the data are aggregated to the state level. Because the number of people earning less than the new minimum wage and the number of poor workers are both relatively small, some of the states in the sample have very few observations. Therefore, the means are measured imprecisely for those states. The noisiness of the data has been one of the main sources of criticism of their work.[5]

Another obvious criticism is the paucity of regressors. Although Card and Krueger do implicitly control for time-invariant state level characteristics by using the *change* in the fraction of workers who are poor as the dependent variable, it is possible that the fraction of workers affected by the wage increase is picking up other factors affecting the workers' poverty rate. For example, the differential effect across states of the recession that occurred during the period in question likely affected both the fraction of workers earning less than $3.35 per hour and the poverty rate among workers.

One final criticism entails the definition of the dependent variable itself. The change in the poverty rate among workers captures only those people who work both before *and after* the increase in the minimum wage. Of course, Card and Krueger spend most of their book presenting evidence disputing the presence of an employment effect, so this may not be a major concern. Nonetheless, it would be more appropriate to consider only people who were working and poor before the wage increase, and try to determine how that specific group was affected. This cannot

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[5]Given this problem, it is perhaps surprising that if one were to use the slightly weaker criterion of a .20 significance level (or use a one tailed test at the .10 level), most of their estimates would be significant.

be done directly with the CPS (at least, not for a very long time period) because individuals are only interviewed for two four-month periods, one year apart.

In view of the shortcomings of using data aggregated to the state-level, we use family-level data instead. Unfortunately, this does not change the fact that the CPS is not a true longitudinal data set.[6] To work around this limitation we identify a sample of families at a point in time and use year specific regressions equations to synthetically trace their earnings profile following a change in the minimum wage. Assuming there is no systematic difference in the way the sample is chosen each year, there should be no significant difference in the earnings of families with a given set of characteristics unless the relationship itself changed.

Of course, if the relationship did change, we need some way to determine whether the change was due to the minimum wage or to some other structural shift. In fact, many other macroeconomic changes did take place during this period. Most notably, late 1991 marked the end of an economic downturn, and overall employment began increasing in 1992. We use the difference-in-difference approach to identify the portion of the change that can be attributed to the minimum wage legislation by comparing the difference between predicted and actual earnings across high and low impact states. We also make comparisons across income categories in an attempt to further control for unobserved differences in states.

In order to determine specifically how the working poor are affected, we categorize

[6]Despite this drawback, we wish to use the CPS for two reasons. First, we want to be sure that any differences in the results from the two methodologies are not due to systematic differences in the data set used. More importantly, we need a data set the size of the CPS in order to get a reasonable number of observations on the working poor, which are a relatively small fraction of the entire population of workers. As mentioned in the text this can be difficult, even with the CPS, but it is less of a problem than with other available data sets.

families by their income level in the starting period into one of four groups: those with incomes below the officially defined poverty line (these are the families we have referred to as "poor families"), those with incomes above the poverty line but less than 125 percent of that line (which we call "near-poor families"), those with incomes of at least 125 percent but less than 150 percent of the poverty line, and those earning 150 percent or more of the poverty level for families of their size.[7] We calculate the difference-in-difference separately for each income category.

Our empirical strategy involves three steps. First we estimate a model of log family earnings in a base year, prior to the minimum wage change of interest, controlling for gender, veteran status, race, age, and education of the householder, including quadratic terms in age and education, place of residence (urban, central city, etc.), the number of children aged 18 or less, the number of adults (male and female), and marital status. We also include state dummy variables to control for differences in economic conditions and other state-level characteristics. A tobit specification is used to account for the truncation of log earnings at zero. The regression produces a set of coefficient estimates (β_1), an estimate of the first period error variance (σ_1^2), and a vector of residuals ($\hat{e}_1$).[8] A similar regression analysis based on data from the second period, subsequent to the change in the minimum wage, provides estimates of β_2 and σ_2^2 .

The second step in the analysis uses the two sets of regression results to compute the

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[7]In much of the literature on poverty, the two middle categories defined here are collapsed into one. That is, families with incomes above the poverty line but below 150% of that line are often referred to as "the near-poor."

[8]The value of $\hat{e}_1$ is the difference between actual and predicted log earnings for families with positive earnings in period 1; for families with zero log earnings we use the expected value of the error, conditional on the truncation. See Greene (1993, chapter 22) or Maddala (1983) for details.

expected second period log earnings for each family observed in the first period. Formally, the predicted earnings are:

$$\widehat{LNEARN_2} = \max(0,\ X_1\hat{\beta}_2 + \frac{\hat{\sigma}_2}{\hat{\sigma}_1}\hat{\varepsilon}_1)\ .$$

The first period residual includes all of the unobserved family characteristics omitted from the regression. To take advantage of this information, we predict the second period residual by rescaling the first period residual by the relative standard deviations. The max function transforms the estimate of the latent variable implied by the tobit specification into its observed counterpart.

Lastly, we compute the difference between the predicted second period log earnings and the observed first period log earnings for each family. The difference approximates the growth of earnings over the period. To determine the effect of the minimum wage legislation, we group the observations by the fraction of workers affected in the state in which the family lives, and then average the predicted changes in log earnings separately for each of the four income categories in each group of states. We use the CPS sample family weights to compute averages within income categories.

III. Results

Table 2 shows the results of the family level analysis for the change in earnings between

1989 and 1990.[9] The predicted earnings growth is shown separately for high and low impact states, and for each of several definitions of working poor families. The results for families that had at least one member who worked at any time during the year are shown first. Earnings for poor families in this category grew by 2.9 percentage points on average in high impact states (row 1, column 1) compared to an earnings loss of 7.7 points in low impact states (column 2). The decline in earnings is not surprising as 1990 was a recession period. The difference between high and low impact states was 10.6 percentage points (column 3). For near-poor families with at least one worker, the difference in earnings growth across high and low impact states was slightly lower at 10.0 percentage points (row 2, column 3). Families between 125 and 150 percent of the poverty level also had higher earnings in high impact states compared to low impact states (row 3, column 3). The effect for higher income families was smaller; a difference between high and low states of 8 percentage points.[10]

Minimum wages are often criticized because many of the benefits go to individuals who are not poor. Our results show that despite this fact, a minimum wage increase does benefit poor and near-poor families with at least one worker and the effect is stronger for these families than for those with higher incomes. Column 4 shows the difference in the impact between the specified income grouping and the below poverty group. The frequency of negative numbers in this column

[9]Parameter estimates from the earnings equations are available upon request. It is important to note that in all pairs of years considered we found statistically significant differences in the parameter and variance estimates across years.

[10]Perhaps surprisingly, earnings grew more quickly for higher income families living in high impact states as well (row 4, column 3). This may be the result of minimum wage workers being spread throughout the income distribution, which is why minimum wages are a blunt instrument for alleviating poverty, or it may be indicative of some other systematic difference between states. This latter possibility is considered below.

reveals that the effect is largest for the lowest income group: the exact group targeted by the minimum wage.

These findings are reinforced by the results based on the U.S. Department of Labor definition of the working poor (families with at least one member in the labor force for twenty-seven weeks or more, shown in rows 5 through 8) and families with a full-time worker (rows 9 through 12), or a full-year worker (rows 13 through 16). The pattern for families with a member working full-time and full-year (rows 17 through 20) is similar. The last set of rows in table 2 examine the predicted earnings growth of all families, working or not. Families in high impact states continue to have higher earnings growth, or slower earnings declines, than families in low impact states. However, the difference is smaller due to the fact that families without labor market attachment are included in this group.

The federal minimum wage increased again in April of 1991. Table 3 examines the 1989-1991 earnings growth of the same families in order to assess the extent to which the second change in the federal minimum continued to aid the working poor. It measures the two year impact of both changes in the wage floor on working families observed in 1989. Although the National Bureau of Economic Research officially dates the end of the recession as third quarter 1991, employment markets did not pick up for several quarters after that. Table 3 reflects this since earnings growth was lower and more frequently negative here than in table 2 for families with incomes below 150 percent of the poverty threshold. That said, earnings growth in the high impact states continues to exceed that of low impact states in every case. While the difference is smaller for most of the income categories, it is larger for families with incomes between 100-124 percent of the poverty level. In fact, this group benefits even more than the lowest income group (except for families with one or more full-time or full-time/full-year worker). In sum, the aggregate impact of two-stage minimum wage disproportionately increases the incomes of the poor and the near-poor.

One criticism of many minimum wage studies has been that they examine only the short-run effects so they may miss the adverse employment effect, which may not be felt immediately. Table 4 extends the ending period for our analysis to 1992 in order to fully assess the effect of the changes. Despite another year of economic recovery, lower income families still had small and often negative earnings growth in this period. Nonetheless, earnings growth was much stronger in high impact states for families in every category and once again, the impact is greatest for poor and near-poor families.

In both tables 2 and 3, earnings growth in high impact states for families in the highest income group seems particularly strong. This raises concerns that the high/low distinction is actually measuring a phenomenon other than the effect of minimum wages. To explore this possibility further, we examine family earnings growth over the 1988 to 1989 period, when the federal minimum wage was held constant. We continue to use the fraction of workers affected by the 1990 minimum wage increase to define high and low impact states. If the high impact designation actually corresponds to something unrelated to minimum wages, then we should observe above average earnings growth in high impact states during this period despite the fact that there was no change in the minimum wage.[11] As table 5 indicates, the high impact states had *lower* earnings growth than the low impact states between 1989 and 1990 in almost every case.

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[11]In conducting this experiment we are implicitly assuming that the unspecified characteristic of high impact states is constant across time.

The exception is for families in the 124-150 percent range, but even for this group, the difference in income growth is less than 2 percent — much lower than the differences found in the post-increase periods. This finding supports the assumption that the differences observed in the later periods are due to the changes in the minimum wage. Hence, we conclude that increasing the minimum wage in 1990 and 1991 raised the average incomes of the working poor.

IV. Conclusion

Minimum wage legislation is an ongoing topic of debate amongst economists and policy makers. There are many relevant questions. However, we intentionally focus our empirical analysis on the relatively narrow question of how the 1990 and 1991 federal minimum wage changes affected the working poor. While this group is a small portion of the population affected by the regulatory change, it is the target of the legislation; hence the effect on this group is an important criterion in assessing the benefits of increasing the minimum wage.

We use the state level variation in the fraction of hourly workers affected by the federal wage increases to assess the impact of minimum wage changes on the earnings of poor (1989 incomes below the poverty line) and near-poor (1989 incomes of 100 to 124 percent of poverty level) families who were working before the legislation took effect. Previous studies found minimum wage legislation is relatively ineffective at decreasing overall poverty rates (e.g., Gramlich 1976, Horrigan and Mincy 1993) and consequently minimum wage legislation is not a solution to the poverty problem in the United States. However, we argue that the minimum wage is not intended as a panacea for poverty, but that its purpose is to support the incomes of the *working* poor. The policy does have a noticeable impact on this target group. Our results are robust using a variety of definitions of the working poor as well as several time periods covering the 1990 and 1991 minimum wage increases. The earnings growth of working poor families living in high impact states — states with at least 5.3 percent (the median) of the hourly earnings distribution between \$3.35 and \$3.80 per hour immediately prior to the 1990 increase in the federal minimum wage — was approximately 10 percentage points higher than that of similar families living in low impact states. Families with incomes between 100 and 125 percent of the poverty threshold saw similar differences in earnings growth. In summary, the 1990 and 1991 increases in the federal minimum wage appear to have benefitted the group they were specifically meant to help; the working poor.

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Table 1
Fraction of Workers Affected by the 1990 and 1991 Increases in the Federal Minimum Wage

State	Percent of 04/89-03/90 wages between $3.35-$3.80	04/89-03/90 ranking from low to high	04/89-03/90 sample size	Percent of 04/90-03/91 wages between $3.80-$4.25	Percent of 04/91-03/92 wages between $3.35-$4.25
Alaska	0.4%	1	2469	0.7%	0.6%
California	0.9%	2	10789	0.8%	0.9%
Rhode Island	1.0%	3	1655	1.6%	1.1%
Connecticut	1.1%	4	1745	1.2%	1.0%
Massachusetts	1.1%	5	6940	1.3%	1.3%
New Hampshire	1.1%	6	1586	1.5%	1.6%
Washington	1.2%	7	2098	1.3%	1.1%
District of Columbia	1.3%	8	1600	1.5%	1.7%
Hawaii	1.5%	9	1637	2.7%	1.7%
Vermont	1.5%	10	1546	2.5%	1.3%
Maine	1.6%	11	1893	2.8%	1.6%
New Jersey	1.7%	12	6844	1.6%	1.5%
Maryland	2.3%	13	1884	2.8%	1.5%
Minnesota	2.5%	14	2042	4.3%	1.7%
Delaware	2.5%	15	1790	2.9%	1.6%
New York	3.0%	16	9467	3.3%	2.0%
Oregon	3.1%	17	1736	0.8%	1.0%
Nevada	3.4%	18	1942	4.2%	1.2%
Pennsylvania	3.7%	19	7301	4.4%	1.9%
Virginia	4.0%	20	2782	4.8%	2.0%
Florida	4.5%	21	7711	5.5%	2.2%
Illinois	4.7%	22	7359	4.8%	2.0%
Wisconsin	5.2%	23	2638	5.7%	2.2%
Indiana	5.3%	24	2202	6.2%	2.5%
North Carolina	5.3%	25	7711	6.0%	2.7%
Michigan	5.3%	26	7202	5.5%	2.1%
Ohio	5.5%	27	7741	6.5%	1.9%
Arizona	5.5%	28	1765	6.7%	1.6%
Iowa	6.0%	29	2174	6.3%	2.5%
Colorado	6.0%	30	1845	5.9%	1.8%
Georgia	6.2%	31	2018	6.8%	1.9%
Utah	6.4%	32	1978	7.4%	2.2%
Missouri	6.6%	33	1907	8.6%	2.7%
Kansas	6.6%	34	2129	6.8%	2.1%
Texas	7.4%	35	8023	8.8%	2.2%
Oklahoma	7.5%	36	1831	9.4%	2.2%
Nebraska	7.6%	37	2193	7.7%	2.1%
Wyoming	7.6%	38	1480	8.0%	3.0%
Alabama	7.7%	39	1918	10.1%	3.4%
Tennessee	8.0%	40	2071	7.8%	2.0%
Idaho	8.0%	41	2040	8.0%	3.5%
South Carolina	8.0%	42	2433	6.8%	3.0%
North Dakota	8.4%	43	2180	8.6%	3.9%
Kentucky	8.6%	44	1828	9.6%	2.3%
New Mexico	9.5%	45	1895	8.5%	2.7%
Montana	9.7%	46	2109	10.9%	2.5%
South Dakota	10.5%	47	2338	9.9%	2.2%
Louisiana	11.4%	48	1487	10.5%	3.3%
Arkansas	12.1%	49	2074	11.5%	2.8%
West Virginia	12.8%	50	1762	12.5%	3.6%
Mississippi	12.9%	51	2001	13.2%	3.5%

Weighted estimates from the outgoing rotations of the Current Population Survey. Based on reported wage rates for hourly workers and estimated wages (weekly earnings/usual hours) for salaried workers.

Table 2
Family earnings growth of working poor, March 1990 - March 1991 CPS
Compares 1989 and 1990 earnings

Family characteristic	Row	Ratio of family income to low-income	Growth in high impact states (1)	Growth in low impact states (2)	Difference in growth rate (high-low) (3)	Difference compared with below poverty (4)
One or more worker	1	Below low-income level	2.9%	(7.7%)	10.6%	
	2	100-124 percent	4.6%	(5.4%)	10.0%	(0.6%)
	3	125-149 percent	5.6%	(4.6%)	10.2%	(0.4%)
	4	150+ percent	6.9%	(1.1%)	8.0%	(2.6%)
One or more person in LF 27 weeks	5	Below low-income level	3.5%	(7.1%)	10.6%	
	6	100-124 percent	4.0%	(5.0%)	9.0%	(1.6%)
	7	125-149 percent	5.4%	(4.7%)	10.1%	(0.5%)
	8	150+ percent	6.9%	(1.1%)	8.0%	(2.6%)
One or more full-time worker	9	Below low-income level	3.7%	(6.7%)	10.5%	
	10	100-124 percent	3.3%	(5.8%)	9.1%	(1.4%)
	11	125-149 percent	4.8%	(5.0%)	9.8%	(0.7%)
	12	150+ percent	6.8%	(1.1%)	7.9%	(2.6%)
One or more full-year worker	13	Below low-income level	4.8%	(5.6%)	10.4%	
	14	100-124 percent	4.5%	(4.1%)	8.5%	(1.9%)
	15	125-149 percent	5.8%	(4.6%)	10.4%	(0.0%)
	16	150+ percent	7.1%	(0.8%)	7.9%	(2.5%)
One or more full-time/full year worker	17	Below low-income level	4.8%	(4.0%)	8.8%	
	18	100-124 percent	3.3%	(4.5%)	7.8%	(0.9%)
	19	125-149 percent	5.5%	(5.1%)	10.6%	1.8%
	20	150+ percent	7.0%	(0.9%)	7.8%	(0.9%)
All (working or not)	21	Below low-income level	2.5%	(5.9%)	8.5%	
	22	100-124 percent	3.5%	(3.8%)	7.3%	(1.2%)
	23	125-149 percent	4.8%	(3.0%)	7.8%	(0.7%)
	24	150+ percent	6.6%	(1.0%)	7.6%	(0.9%)

NOTES: Numbers shown in parentheses are negative.
High-impact states are those with more than 5.3% of the 1990 workforce earning between \$3.35 and \$3.80 per hour. Low-impact states are those with less than 5.3% of the 1990 workforce earning between \$3.35 and \$3.80 per hour.

Table 3
Family earnings growth of working poor, March 1990 - March 1992 CPS
Compares 1989 and 1991 earnings

Family characteristic	Row	Ratio of family income to low-income	Growth in high impact states (1)	Growth in low impact states (2)	Difference in growth rate (high-low) (3)	Difference compared with below poverty (4)
One or more worker	1	Below low-income level	(2.9%)	(11.8%)	8.9%	
	2	100-124 percent	3.2%	(7.8%)	11.0%	2.1%
	3	125-149 percent	4.5%	(2.9%)	7.4%	(1.5%)
	4	150+ percent	13.9%	6.3%	7.6%	(1.3%)
One or more person in LF 27 weeks	5	Below low-income level	(2.2%)	(11.2%)	9.1%	
	6	100-124 percent	2.4%	(8.0%)	10.4%	1.3%
	7	125-149 percent	3.7%	(3.3%)	6.9%	(2.1%)
	8	150+ percent	14.1%	6.4%	7.6%	(1.4%)
One or more full-time worker	9	Below low-income level	(3.5%)	(13.1%)	9.6%	
	10	100-124 percent	0.7%	(9.6%)	10.3%	0.7%
	11	125-149 percent	2.0%	(4.1%)	6.1%	(3.4%)
	12	150+ percent	13.8%	6.4%	7.5%	(2.1%)
One or more full-year worker	13	Below low-income level	(0.2%)	(9.9%)	9.7%	
	14	100-124 percent	1.3%	(7.1%)	8.4%	(1.3%)
	15	125-149 percent	3.4%	(4.5%)	7.9%	(1.8%)
	16	150+ percent	14.3%	6.7%	7.7%	(2.0%)
One or more full-time/full year worker	17	Below low-income level	(2.0%)	(12.4%)	10.3%	
	18	100-124 percent	(0.9%)	(9.7%)	8.8%	(1.5%)
	19	125-149 percent	1.6%	(5.9%)	7.5%	(2.8%)
	20	150+ percent	14.2%	6.6%	7.6%	(2.7%)
All (working or not)	21	Below low-income level	(1.9%)	(9.1%)	7.2%	
	22	100-124 percent	2.5%	(5.4%)	7.8%	0.7%
	23	125-149 percent	3.9%	(1.8%)	5.7%	(1.4%)
	24	150+ percent	13.3%	6.1%	7.2%	0.0%

NOTES: Numbers shown in parentheses are negative.
High-impact states are those with more than 5.3% of the 1990 workforce earning between $3.35 and $3.80 per hour. Low-impact states are those with less than 5.3% of the 1990 workforce earning between $3.35 and $3.80 per hour.

Table 4
Family earnings growth of working poor, March 1990 - March 1993 CPS
Compares 1989 and 1992 earnings

Family characteristic	Row	Ratio of family income to low-income	Growth in high impact states (1)	Growth in low impact states (2)	Difference in growth rate (high-low) (3)	Difference compared with below poverty (4)
One or more worker	1	Below low-income level	(5.9%)	(19.5%)	13.5%	
	2	100-124 percent	1.1%	(13.8%)	15.0%	1.4%
	3	125-149 percent	2.2%	(8.8%)	11.1%	(2.5%)
	4	150+ percent	17.3%	4.1%	13.2%	(0.3%)
One or more person in LF 27 weeks	5	Below low-income level	(4.5%)	(18.0%)	13.5%	
	6	100-124 percent	0.8%	(13.7%)	14.5%	1.0%
	7	125-149 percent	2.1%	(9.2%)	11.3%	(2.2%)
	8	150+ percent	17.6%	4.3%	13.3%	(0.2%)
One or more full-time worker	9	Below low-income level	(5.7%)	(19.8%)	14.1%	
	10	100-124 percent	0.4%	(14.8%)	15.2%	1.1%
	11	125-149 percent	0.8%	(9.6%)	10.4%	(3.7%)
	12	150+ percent	17.5%	4.3%	13.2%	(0.9%)
One or more full-year worker	13	Below low-income level	(0.9%)	(14.6%)	13.7%	
	14	100-124 percent	0.9%	(13.0%)	13.9%	0.2%
	15	125-149 percent	1.3%	(9.6%)	10.9%	(2.8%)
	16	150+ percent	18.1%	4.8%	13.3%	(0.3%)
One or more full-time/full year worker	17	Below low-income level	(2.0%)	(16.0%)	14.1%	
	18	100-124 percent	(0.2%)	(14.4%)	14.2%	0.2%
	19	125-149 percent	0.4%	(10.6%)	10.9%	(3.1%)
	20	150+ percent	18.2%	4.9%	13.3%	(0.7%)
All (working or not)	21	Below low-income level	(4.2%)	(15.1%)	10.9%	
	22	100-124 percent	1.0%	(9.7%)	10.7%	(0.1%)
	23	125-149 percent	2.1%	(6.2%)	8.3%	(2.6%)
	24	150+ percent	16.5%	3.9%	12.6%	1.8%

NOTES: Numbers shown in parentheses are negative.
High-impact states are those with more than 5.3% of the 1990 workforce earning between $3.35 and $3.80 per hour. Low-impact states are those with less than 5.3% of the 1990 workforce earning between $3.35 and $3.80 per hour.

Table 5
Family earnings growth of working poor, March 1989-March 1990 CPS
Compares 1988 and 1989 earnings

Family characteristic	Row	Ratio of family income to low-income	Growth in high impact states (1)	Growth in low impact states (2)	Difference in growth rate (high-low) (3)	Difference compared with below poverty (4)
One or more worker	1	Below low-income level	6.9%	7.2%	(0.3%)	
	2	100-124 percent	6.1%	7.8%	(1.7%)	(1.4%)
	3	125-149 percent	5.4%	4.8%	0.6%	1.0%
	4	150+ percent	5.3%	5.7%	(0.5%)	(0.1%)
One or more person in LF 27 weeks	5	Below low-income level	6.6%	7.1%	(0.5%)	
	6	100-124 percent	6.4%	7.8%	(1.4%)	(0.9%)
	7	125-149 percent	5.7%	4.8%	1.0%	1.5%
	8	150+ percent	5.2%	5.7%	(0.5%)	0.1%
One or more full-time worker	9	Below low-income level	6.8%	7.8%	(1.0%)	
	10	100-124 percent	6.4%	8.5%	(2.1%)	(1.1%)
	11	125-149 percent	6.2%	5.2%	1.1%	2.1%
	12	150+ percent	5.3%	5.7%	(0.4%)	0.6%
One or more full-year worker	13	Below low-income level	5.0%	6.5%	(1.5%)	
	14	100-124 percent	6.1%	7.0%	(0.9%)	0.6%
	15	125-149 percent	5.8%	4.3%	1.5%	3.0%
	16	150+ percent	5.2%	5.6%	(0.4%)	1.1%
One or more full-time/full year worker	17	Below low-income level	5.4%	6.6%	(1.2%)	
	18	100-124 percent	6.3%	8.1%	(1.7%)	(0.6%)
	19	125-149 percent	6.4%	4.5%	1.9%	3.1%
	20	150+ percent	5.3%	5.6%	(0.4%)	0.8%
All (working or not)	21	Below low-income level	5.1%	5.5%	(0.4%)	
	22	100-124 percent	4.5%	5.5%	(1.0%)	(0.6%)
	23	125-149 percent	4.2%	3.5%	0.7%	1.2%
	24	150+ percent	5.0%	5.4%	(0.4%)	(0.0%)

NOTES: Numbers shown in parentheses are negative.
High-impact states are those with more than 5.3% of the 1990 workforce earning between $3.35 and $3.80 per hour. Low-impact states are those with less than 5.3% of the 1990 workforce earning between $3.35 and $3.80 per hour.

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