American Economic Association

Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Comment
Author(s): David Neumark and William Wascher
Source: *The American Economic Review*, Vol. 90, No. 5 (Dec., 2000), pp. 1362-1396
Published by: American Economic Association
Stable URL: http://www.jstor.org/stable/2677855
Accessed: 02/05/2014 10:01

Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Comment

By DAVID NEUMARK AND WILLIAM WASCHER*

In a past article in this *Review,* David Card and Alan B. Krueger (1994) reported evidence that called into question the conventional view of the labor-market effects of minimum-wage laws. In particular, Card and Krueger (hereafter, CK) surveyed fast-food establishments in New Jersey and Pennsylvania in 1992 before and after the minimum wage in New Jersey rose from \$4.25 to \$5.05. By comparing the changes in employment in these two states, they constructed a simple "difference-in-differences" test of the prediction that minimum-wage increases reduce employment of affected workers. Contrary to this prediction, CK find "no evidence that the rise in New Jersey's minimum wage reduced employment at fast-food restaurants in the state ..." and in fact that "the increase in the minimum wage increased employment" (p. 792).[1] Given the prominence

that this study has received, both in support of proposals to increase minimum wages in the United States and elsewhere, and as evidence against the competitive labor-market model, we believe that a careful reevaluation of its results is of interest.

In this paper, we present new evidence based on administrative payroll records obtained from a sample of Burger King (BK), Wendy's, Roy Rogers (RR), and Kentucky Fried Chicken (KFC) restaurants in New Jersey and Pennsylvania, drawn from the same geographic areas and the same chains from which CK's sample of restaurants was drawn, and hence most likely overlapping substantially with the restaurants in their sample. We compare results using these payroll data to those using CK's data, which were collected by telephone surveys. We have two main findings to report.

First, the employment data collected by CK indicate substantially more variability over the period between their surveys than do the payroll data. The standard deviation of employment change in CK's entire sample exceeds that in the payroll data by a factor of nearly three, and the 90th–10th centile difference exceeds that in the payroll data by a factor of 2.6. At a more disaggregated level (by restaurant chain, ownership, and state), the ratio of the standard deviation in CK's data to that in the payroll data is often above four. In our view, the much higher variability of employment change in the data collected by CK, coupled with other evidence of severe measurement error in this survey, provides motivation for a reevaluation of this "experiment" using an alternative data source.

Second, estimates of the employment effect of the New Jersey minimum-wage increase from the payroll data generally lead to the opposite conclu-

[1] Similarly, summarizing this study on the first page of their widely cited book *Myth and Measurement* (1995), CK state that "Relative to restaurants in Pennsylvania, where the minimum wage remained unchanged, we find that employment in New Jersey actually *expanded* with the increase in the minimum wage" (1995 p. 1, italics theirs).

sion from that reached by CK. For the subset of restaurant types represented in the payroll data, CK's data imply that the New Jersey minimum-wage increase (of 18.8 percent) resulted in an *increase* in fast-food employment of between 11 percent and 16.8 percent relative to the Pennsylvania control group, with elasticities ranging from 0.54 to 0.89; their full sample also points to a large positive employment effect. In contrast, a simple replication of CK's difference-in-differences estimation using the payroll data indicates that the New Jersey minimum-wage increase led to a 3.9-percent to 4.0-percent *decrease* in fast-food employment in New Jersey relative to the Pennsylvania control group, with elasticities in the range of −0.21 to −0.22. Sensitivity analyses of the estimates yield qualitatively similar results, although the range of elasticities using the payroll data expands to roughly −0.1 to −0.25. The estimated disemployment effects in the payroll data are often statistically significant at the 5- or 10-percent level, although there are some estimators and subsamples that yield insignificant—although almost always negative—estimates. Similarly, the positive estimated employment effects in CK's data hold up in additional sensitivity analyses and are often statistically significant. Thus, in contrast to CK's evidence suggesting that the results of the New Jersey-Pennsylvania minimum-wage experiment are "difficult to explain with the standard competitive model" (1994 p. 792), the payroll data are generally consistent with the prediction that raising the minimum wage reduces the demand for low-wage workers.

I. Card and Krueger's Results

On April 1, 1991, the federal minimum wage rose to $4.25 per hour. At that time, the federal minimum wage was the prevailing minimum in both Pennsylvania and New Jersey. On April 1, 1992, New Jersey's minimum wage was increased to $5.05, while Pennsylvania's minimum wage did not change. CK surveyed restaurants in four fast-food chains in New Jersey and eastern Pennsylvania in two periods: February 15–March 4, 1992, before the New Jersey increase, and November 5–December 31, 1992, eight to nine months after the increase. As explained below, the employment data they collected are as of the interview date, but do not cover a precisely defined time interval.

Panel A of Table 1 reports descriptive statistics and the main result from CK's data: on average, over the eight to nine months following New Jersey's minimum-wage increase, employment at the fast-food restaurants in New Jersey grew by relatively more full-time equivalents (FTE's) than in Pennsylvania.[2] As shown in column (1), this difference arises from a small employment increase in New Jersey (0.5 FTE's) and a large employment decline in Pennsylvania (2.3 FTE's). Column (6) shows the difference-in-differences regression estimate with controls for chain and ownership (franchise vs. company-owned). The estimate indicates that employment in New Jersey grew by a statistically significant 2.78 FTE's relative to Pennsylvania, suggesting that minimum wages increase employment. The implied elasticity of employment with respect to the minimum wage is 0.70.

The same rows also illustrate that there are some extremely large employment changes in CK's data. The largest employment decline is 41.5 FTE's (a 64.4-percent decline, from 67.5 to 24 FTE's), the largest increase is 34 FTE's (a 233-percent increase, from 15 to 50 FTE's), and the standard deviations of employment change are 8.5 in New Jersey and 10.9 in Pennsylvania.[3] The variability appears high, given that the mean level of employment was about 21 FTE's in each wave of the survey, although there is no discussion of this issue in CK's paper.[4]

II. The Payroll Data

A. *Data Collection*

We set out to obtain payroll records in each zip-code area represented in CK's data, for each

---

[2] As in CK, full-time equivalents are defined as the number of full-time nonmanagement employees, plus one-half the number of part-time nonmanagement employees, plus the number of management employees. The results shown in panel A differ slightly from those shown in Tables 3 and 4 in CK's paper because CK exclude restaurants missing starting wages; if we do the same, we replicate their results exactly.

[3] The largest relative employment decline for a restaurant that remained open is 77.3 percent, and the largest relative increase is 336 percent.

[4] CK do, however, report that the standard deviation of employment change for the entire sample is 8.8 FTE's, in a footnote to their Table 4. Surprisingly, the 13 largest employment declines in CK's data are for restaurants that remained open.

TABLE 1—EMPLOYMENT CHANGES IN THE CARD/KRUEGER DATA

	Descriptive statistics by state					Regression estimates	
	Mean (1)	Standard deviation (2)	Maximum/ minimum (3)	90th/10th centile (4)	Observations (5)	New Jersey-Pennsylvania employment change (6)	Elasticity (7)
A. *Card/Krueger Full Sample*							
Change in total FTE's							
Pennsylvania	−2.3	10.9	22.8/−41.5	11.7/−18	75	2.78	0.70
New Jersey	0.5	8.5	34/−34	10.5/−9.5	309	(1.16)	
B. *Card/Krueger Full Sample, Nonmanagement FTE's*							
Change in nonmanagement FTE's							
Pennsylvania	−2.5	10.8	23.8/−43.5	10.2/−17.7	76	2.97	0.89
New Jersey	0.5	8.2	35/−34	10/−8.1	315	(1.12)	
C. *Card/Krueger Restaurants in Sampled Universe*							
Change in nonmanagement FTE's							
Pennsylvania	−3.0	13.2	23.8/−43.5	15.1/−19.3	38	3.07	0.85
New Jersey	0.6	8.6	35/−25.5	10.7/−8.1	176	(1.80)	
D. *Card/Krueger Restaurants in Nonsampled Universe*							
Change in nonmanagement FTE's							
Pennsylvania	−2.0	7.8	16/−20	5.6/−16.5	38	1.80	0.61
New Jersey	0.2	7.7	21.5/−34	8.3/−8.5	139	(1.45)	

Notes: "FTE's" refers to full-time equivalent employees as of the interview date. The "sampled universe" is Burger King and KFC franchises, and Wendy's and Roy Rogers franchises and company-owned restaurants, in the zip-code areas listed in Table 3. Estimates in column (6) are from a regression of employment change on a dummy variable for New Jersey, and dummy variables for chains and company-owned units. Standard errors are reported in parentheses. Elasticities in column (7) are evaluated at sample means for the corresponding sample.

type of restaurant (classified by chain and ownership) in that zip-code area. We were able to obtain payroll records for Burger King and KFC franchises, and Wendy's and Roy Rogers franchises and company-owned restaurants, in over 80 percent (25 out of 31) of the zip-code areas in which CK's restaurants were located. We collected some of the data ourselves, while some were collected by the Employment Policies Institute (EPI) and then supplied to us. In Appendix A, we provide more details regarding the collection of the data; in that Appendix and below we also present evidence documenting the accuracy of the data collected by the EPI, and discuss other concerns regarding the data. "Zip-code areas" refer to the first three digits of the zip code, which is the information available in the data provided by CK. Both our data-collection efforts and those of the EPI were restricted to zip-code areas and restaurants—distinguished by chain and ownership—represented in CK's data; for example, information was requested on company-owned

Roy Rogers restaurants in all zip-code areas in which CK had such restaurants. Most of the observations either come from franchisees that were listed in the *Chain Operators Guide* for 1995 (the year in which we began this research), had restaurants in any of these chains, and were headquartered in Pennsylvania, New Jersey, or New York, or from the parent corporations (which are also listed in the *Guide*).[5] Both we and the EPI requested that these franchisees or parent corporations provide information from their payroll records on the total number of hours worked by nonmanagement employees in the pay periods spanning the dates for each wave of CK's survey, separately for each restaurant in the zip-code areas represented in CK's data. To make the data more comparable

[5] In principle, a franchisee headquartered in any state could have outlets in New Jersey or Pennsylvania. We assumed that we would cover most outlets by surveying these three states. Appendix A provides details on the *Chain Operators Guide*.

to CK's data, the hours data were then converted into full-time equivalent employees (FTE's) assuming a full-time workweek of 35 hours. Overall, the two data-collection efforts resulted in 45 franchisees contacted, of which 25 responded, with 17 supplying data and eight indicating they had no outlets in the zip-code areas in which we were interested. In the case of the parent corporations, two supplied data and two refused to do so.

CK's data set does not include a unique restaurant identifier (such as an address), so we were unable to match up individual units. Their data set includes the first three digits of the zip code (the zip-code sectional area) in which each surveyed restaurant is located, however, and so we could match units by approximate location, although some zip-code areas are rather broad. Moreover, we were able to identify some zip-code areas in which we are quite confident that we have collected data from all franchised restaurants (or company-owned restaurants) in a chain, so that we could distinguish between zip-code areas in which all of the restaurants included in CK's data set also appear in our data set, and zip-code areas in which we had only partial data so that restaurants included in CK's data set might not be represented in our data set. In the final analysis, we were able to obtain data on either all franchised restaurants or all company-owned restaurants in a chain for 24 zip-code/chain/ownership combinations (7 in Pennsylvania and 17 in New Jersey) and data on some restaurants for an additional 25 zip-code/chain/ownership combinations.

B. Limitations and Representativeness of the Payroll Data

Table 2 provides a comparison of restaurants in CK's data to those in the payroll data. Panel A shows the number of each type of restaurant in CK's full sample and the number of zip-code areas covered. Panel B shows similar numbers for the subset of CK's restaurants (distinguished by chain, ownership, and zip-code area) that are represented in the payroll data; we refer to this subset as the "sampled universe." Finally, panel C reports the same information for the payroll data. Note that while the payroll-data sample is smaller than CK's full sample, it is a bit larger than the "sampled universe" (235 vs. 214) be-

cause we have obtained data on some restaurants that either were not surveyed by CK or did not respond to their survey; this difference is particularly pronounced for the company-owned restaurants, where we have data on all units owned by the corporations that responded. Nonetheless, we obviously do not have a completely representative sample of the restaurants in CK's data, in terms of chain, ownership, and zip-code area.[6] Below, we consider the sensitivity of estimated employment effects to differences between the restaurants represented in the two data sources. In addition to these differences, we were able to obtain data only on nonmanagement employees, whereas CK also had data on managerial employment (which is a relatively small percentage of total employment).

To examine the extent to which these limitations of the payroll data might influence the results, we compare descriptive statistics and results in CK's full sample to those obtained from CK's sample when we limit the employment definition to nonmanagement FTE's, and when we restrict attention to the zip-code areas and restaurant types for which payroll observations were available. This evidence is presented in panels B–D of Table 1. In panel B, we restrict attention to nonmanagement employment as measured in CK's data. The variability of employment change and the regression estimate of the minimum-wage effect on employment are very similar to what is reported in panel A,

---

[6] There are a couple of apparent discrepancies in Table 2. First, CK appear to have Roy Rogers company-owned restaurants in three more New Jersey zip-code areas than do we (16 vs. 13). For three of the zip-code areas in which CK report such restaurants, however, Roy Rogers indicated to us that they had no company-owned restaurants in the relevant time period. Second, although there are four Wendy's restaurants in New Jersey classified as company-owned in CK's data, Wendy's reported no company-owned restaurants in New Jersey when providing us with their payroll data. Because CK interviewed a manager or assistant manager, it is possible that they obtained an incorrect classification of the ownership status of some restaurants from their telephone survey. We did obtain data on two Wendy's franchises in one zip-code area in which CK report two company-owned restaurants; we included these observations in the payroll data set, but did not revise CK's data. This is reflected in column (5), which reports Wendy's franchises in three zip-code areas in the sampled universe of CK's data, and four in the payroll data, and correspondingly in the totals in column (9).

TABLE 2—SAMPLE CHARACTERISTICS

	BK franchise (1)	BK company (2)	RR franchise (3)	RR company (4)	Wendy's franchise (5)	Wendy's company (6)	KFC franchise (7)	KFC company (8)	Total (9)
A. *Card/Krueger Full Sample*									
Restaurants in Card/ Krueger data									
Pennsylvania	34	0	5	12	7	6	4	8	76
New Jersey	104	25	25	53	36	4	40	28	315
Zip-code areas in Card/Krueger data									
Pennsylvania	10	0	2	3	4	3	3	3	28
New Jersey	20	10	10	16	13	3	15	11	98
B. *Card/Krueger Sampled Universe*									
Restaurants in Card/ Krueger data									
Pennsylvania	20	0	0	12	0	6	0	0	38
New Jersey	93	0	6	50	14	0	13	0	176
Total									214
Zip-code areas in Card/Krueger data									
Pennsylvania	5	0	0	3	0	3	0	0	11
New Jersey	15	0	2	13	3	0	4	0	37
C. *Payroll Data*									
Restaurants in payroll data									
Pennsylvania	31	0	0	32	0	9	0	0	72
New Jersey	63	0	7	67	16	0	10	0	163
Total									235
Zip-code areas in payroll data									
Pennsylvania	5	0	0	3	0	3	0	0	11
New Jersey	15	0	2	13	4	0	4	0	38

Notes: "Zip-code areas" refers to first three digits of zip code. The numbers in column (9) for total observations on zip-code areas are the numbers of zip-code/chain/ownership combinations. CK's full sample includes 31 distinct zip-code areas, whereas our sample includes 25.

using total employment. Thus, the restriction to nonmanagement employment seems unimportant.

In panel C, we report results using the sampled universe of CK's observations. For comparison, in panel D we also report results using the subset of CK's observations not represented in the payroll data. With respect to the variability of employment change, the restaurants in the sampled universe in Pennsylvania exhibit considerably greater variability than the restaurants in the nonsampled universe, while those in New Jersey exhibit similar (if slightly higher) variability. This suggests that a simple comparison of the variability of employment change in the payroll data with that in the corresponding sampled universe in CK's data will overstate the variability of employment change in CK's data. However, this overstatement is considerably less than indicated by the differences between the Pennsylvania figures in panels C and D alone, because there are many more observations in New Jersey in CK's data. If we instead

combine the Pennsylvania and New Jersey restaurants in CK's data, the standard deviation of employment change is 9.7 FTE's in the sampled universe and 7.7 in the nonsampled universe, while the 90th–10th centile range is 21.2 in the sampled universe and 16.4 in the nonsampled universe.

Turning to the influence of the sampling limitations associated with the payroll data on the estimated minimum-wage effects, the figures in column (1) of panels C and D indicate that, according to CK's data, we have sampled from Pennsylvania cells in which employment declines were larger (3.0 vs. 2.0), and from New Jersey cells in which employment growth was higher (0.6 vs. 0.2). This is also reflected in the regression estimates of relative employment growth in New Jersey using CK's data, which, as reported in column (6), are 3.07 in the sampled universe and 1.80 in the nonsampled universe.

These results indicate that we have, if anything, sampled from a set of cells in which the restaurants in CK's data had *larger* employment increases resulting from the higher minimum wage. Thus, although it may be possible, in principle, to pick a sample from CK's data that would overturn their results, it is clear that our data set was not generated in this manner. That is, differences in the results generated by the payroll data are due to differences in the data, rather than to the selection of a subsample in which CK's results are reversed. Nonetheless, it is still most instructive to compare results obtained from the payroll data to results obtained from the sampled universe of CK's data set— specifically, Burger King and KFC franchises, and Wendy's and Roy Rogers franchises and company-owned restaurants, in the zip-code areas represented in the payroll data.

III. Comparisons of the Variability of Employment Change in the Payroll and Telephone Survey Data

A. *Evidence on the Measurement of Employment Change*

Table 3 provides a comparison of FTE employment changes in CK's data and the payroll data, broken down by restaurant chain, ownership status, and the state in which the restaurant is located.[7] Panel A reports on zip-code areas for which we have complete data—that is, data from all franchises or company-owned restaurants in a chain. Thus, the observations in CK's data ought to appear in the payroll data for these zip-code/restaurant combinations, although the reverse need not be true. As indicated in the last two rows of panel A, the payroll data show substantially less variation in FTE employment over time than do CK's data; the standard deviations of employment change in CK's data are 8.0 for Pennsylvania and 6.9 for New Jersey, versus 2.8 and 3.0 in the payroll data. For both states, an F-test rejects the hypothesis of equal standard deviations in the two data sources, with a p-value below 0.01.[8] The 90th–10th centile differences are also considerably smaller in the payroll data. Moreover, when disaggregated by type of restaurant and state, the measures of dispersion are always considerably higher in CK's data than in the payroll data. Note that this is also true for the two sets of company-owned restaurants; because the parent corporations provided payroll data on all restaurants in the relevant zip-code areas for company-owned restaurants, there is no question that we actually have a "census" of restaurants to compare with CK's data in this case.

As shown in panel B, the variability of FTE employment change also is much higher in

---

[7] For restaurants from which we obtained data for more than one payroll period within the time frame of CK's surveys, we randomly selected data for one period, because averaging over multiple pay periods would tend to decrease the variability of employment change in the payroll data, compared with CK's data. Later, when we examine the implied minimum-wage effects using the payroll data, we obtain more accurate estimates by averaging over data for multiple payroll periods to estimate employment levels. In Table 3 (and Figures 1–4 discussed in this section), we also exclude restaurants in either data set that had closed by the second wave of the survey. This precludes the possibility *that the differences in estimates of employment variability* in the two data sources arise because of differences in the number of closed restaurants. However, the qualitative conclusions are similar even if the closed restaurants are included.

[8] This F-test is based on the ratio of two independent sums of squared normal random variables. In this context, the sums may not be independent because some restaurants overlap in the two samples, although we cannot identify these. The positive covariance between the estimates from the two samples that presumably results from this overlap would imply that we, if anything, overstate the p-value.

TABLE 3—CARD/KRUEGER DATA VS. PAYROLL DATA, CHANGES IN NONMANAGEMENT EMPLOYMENT

	Card/Krueger data				Payroll data				Equal standard deviations (*p*-value) (9)
	Mean change in FTE's (1)	Standard deviation (2)	90th/10th centile (3)	Observations (4)	Mean change in FTE's (5)	Standard deviation (6)	90th/10th centile (7)	Observations (8)	
A. *Zip-Code Areas with Payroll Data on All Units*									
Burger King franchises									
Pennsylvania (189)	−11.3	14.5	−1/−21.5	2	5.0	2.9	9.0/2.5	4	
New Jersey (088, 086, 072)	4.3	10.1	19.6/−12.7	12	3.0	1.9	5.5/−0.0	15	
Wendy's franchises									
New Jersey (088)	4.6	6.7	13.5/−2.5	4	2.2	2.5	4.6/−3.0	8	
Wendy's company-owned									
Pennsylvania (194, 181, 180)	−4.5	10.4	10.5/−19	6	1.8	2.1	3.8/−3.3	9	
Roy Rogers company-owned									
Pennsylvania (194, 190, 189)	−3.9	5.9	2.6/−15.4	12	−1.3	1.9	1.6/−3.5	31	
New Jersey (088, 085, 084, 081, 080, 079, 078, 077, 076, 074, 073, 071, 070)	−1.1	5.4	8.2/−6.7	46	−1.8	2.3	1.0/−4.6	64	
Total									
Pennsylvania	−4.8	8.0	2.9/−18.9	20	−0.1	2.8	3.3/−3.4	44	0.00
New Jersey	0.3	6.9	11/−6.5	62	−0.6	3.0	3.6/−3.8	87	0.00
B. *Zip-Code Areas with Payroll Data on Some Units*									
Burger King franchises									
Pennsylvania (194, 190, 186, 182)	−1.1	17.3	19.7/−31.4	18	2.9	1.9	5.5/0.6	27	
New Jersey (087, 085, 083, 082, 080, 079, 078, 076, 074, 073, 071, 070)	0.9	9.5	10.5/−13.9	80	2.0	1.9	4.7/−0.3	48	
Wendy's franchises									
New Jersey (076, 072, 070)	6.2	13.2	33.8/−6.5	10	0.9	5.8	7.1/−10.5	7	
Roy Rogers franchises									
New Jersey (086, 085)	3.0	4.9	11.5/−1.5	6	−0.3	3.6	6.1/−4.7	7	
KFC franchises									
New Jersey (086, 085, 080, 077)	−1.4	3.7	5/−6.1	13	−4.0	4.3	0.7/−11.3	10	
Total									
Pennsylvania	−1.1	17.3	19.5/−31.4	18	2.9	1.9	5.5/0.6	27	0.00
New Jersey	1.0	9.4	10.5/−11.6	111	0.8	3.6	4.7/−3.5	72	0.00
C. *All Sampled Zip-Code Areas*									
Pennsylvania	−3.0	13.2	15.1/−19.3	38	1.0	2.9	4.8/−3.0	71	0.00
New Jersey	0.8	8.6	10.8/−7.6	173	0.1	3.3	4.4/−3.7	159	0.00
Pennsylvania and New Jersey	0.1	9.7	11/−11.2	211	0.4	3.3	4.4/−3.5	230	0.00

Notes: Only restaurants that remained open are included in this table. Because we cannot match establishments across the two data sources, and because CK's data set does not include interview dates for the first wave, we obtained records for as many payroll periods as possible within the spans February 15–March 4, 1992, and November 5–December 31, 1992 (the dates covered by CK's survey). For the payroll data, we have data on one to four payroll periods within each of these spans. We randomly selected one payroll period. Payroll data are reported either weekly, biweekly, or monthly. The monthly reports refer to February or November. We divided hours reported by 2 for biweekly reports. For the Burger King franchisees supplying monthly reports, the figures refer to total hours worked in the month. Therefore, to arrive at a consistent weekly hours number, we divide monthly hours by 29/7 for February (1992 was a leap year), and by 30/7 for November. The Roy Rogers company data were also reported as monthly, but actually refer to hours worked in a four-week period. Thus, for these restaurants we simply divide by 4. Data on two Roy Rogers company-owned restaurants in zip-code areas in which there appear to be no such restaurants are omitted from the disaggregated estimates using CK's data in panel B.
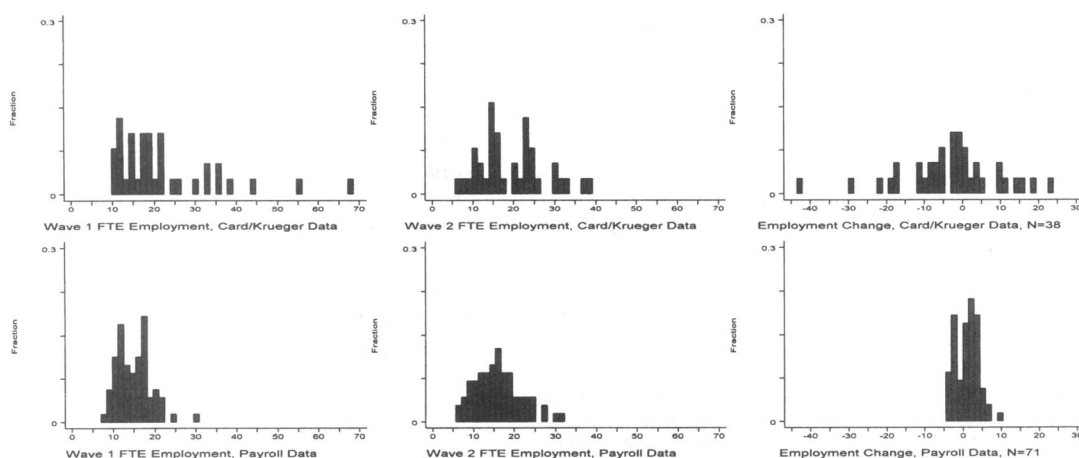
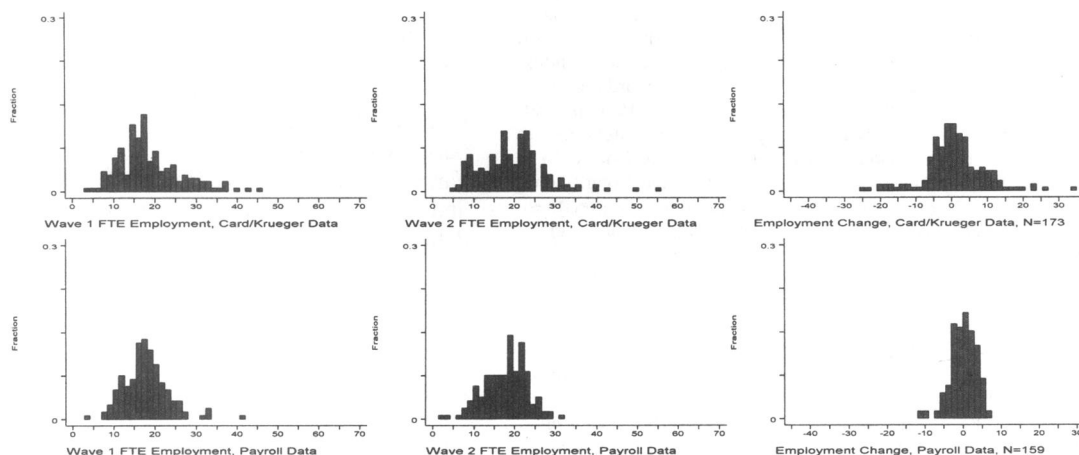FIGURE 1. COMPARING THE TWO DATA SETS, PENNSYLVANIA

FIGURE 2. COMPARING THE TWO DATA SETS, NEW JERSEY

CK's data than in the payroll data for the zip-code areas in which we have data on only some franchises, with the difference particularly pronounced for Pennsylvania. Finally, in panel C we report similar statistics for all of the observations in panels A and B combined, which provides the simplest comparison of variability in the two data sources. The standard deviation of employment change is 9.7 in CK's data, versus 3.3 in the payroll data, while the 90th–10th centile difference is 22.2 in CK's data, versus 7.9 in the payroll data. Again, the standard deviations are significantly different.

The greater variability in CK's data is not driven by one or two outliers. Figures 1 and 2 show histograms for the initial employment levels and employment changes in the two data sources, using the payroll data and the sampled universe of CK's data. In Figure 1, the upper left-hand panel shows the distribution of initial FTE employment levels in CK's data for Pennsylvania, while the lower left-hand panel shows the same distribution in the payroll data. The initial employment levels in the payroll data are much more clustered in the 10 to 20 range and exhibit fewer extremely large values. The middle panels show the same histograms for FTE employment levels at the time of the second

survey. Again, CK's data exhibit more employment variability than the payroll data. Finally, the two right-hand panels show the histograms for employment changes. Here the contrast is most striking, with the employment changes in CK's data much more dispersed than in the payroll data. Figure 2 displays similar histograms for New Jersey. Again, employment changes are much more dispersed in CK's data than in the payroll data, although less so than for Pennsylvania.[9,10]

---

[9] At the suggestion of one of the referees, we also compared the variability in CK's data and the payroll data with estimates reported in the gross flows literature. Generally, these other estimates of employment growth variability fall in between the variability indicated in CK's data and the payroll data. For example, the standard deviation of the log difference in employment is 0.21 in the payroll data and 0.44 in CK's data; this compares with an estimate of about 0.35 from Jonathan S. Leonard (1987), who calculated changes in firm size for all industries using unemployment-insurance tax records for Wisconsin. Similarly, Steven J. Davis et al. (1996 p. 100) report standard deviations of annual employment growth rates in U.S. manufacturing plants ranging from 0.36 in 1978 to 0.41 in 1982 using data from the Longitudinal Research Database. We are reluctant to interpret these comparisons too literally, however. In particular, given that the New Jersey-Pennsylvania experiment was conducted for a narrow industry in a relatively small geographic area, and that franchised outlets are often subject to restrictive franchise agreements regarding staffing, we would argue that these other estimates should be expected to show greater variability of employment change.

[10] In their Reply (Card and Krueger, 2000), CK report standard deviations of employment change in the ES-202 data that are, surprisingly, of similar magnitudes to those from their telephone survey. We think longitudinal analyses of the ES-202 data (not to mention these results) are intriguing and bear further exploration. However, we are reluctant at this juncture to accept the ES-202 estimates at face value. The longitudinal data in this period suffer from the potential problem of changes in the level at which data are reported, because the Bureau of Labor Statistics (BLS) was in the process of encouraging multiestablishment reporters to provide their employment estimates at the establishment or county level rather than grouping all of their establishments together (the BEL breakouts project); CK also acknowledge this problem in their Reply. In addition, we know little about the implications of data imputation in the ES-202, which occurs at about a 7-percent rate nationally but is apparently significantly higher for small businesses. Finally, the figures provided by CK raise some questions about the accuracy of these data. For example, Figure 2 of CK's Reply, which reports what they regard as more reliable cross-sectional data for a select group of fast-food chains, indicates employment declines of about 5 percent in three out of four months in the first half of 1994, and increases of

B. Explaining the Difference in the Variability of Employment Change

One potential explanation of the greater variability of employment change in CK's data relative to the payroll data is that the payroll data measure hours (which we convert to FTE's), whereas CK's data measure number of employees. If there are fixed costs to hiring and firing workers, we might expect hours to be more variable than employment, and thus measured employment changes in the payroll data to be more variable. On the other hand, if fast-food restaurants require a relatively constant number of hours of work, but there are fluctuations from week to week in the number of individuals working part-time or full-time, there may be more variation in the number of employees than in the number of hours worked. Although, in principle, the variability of employment change measured in these two ways could differ, we are skeptical that this explanation could account for much of the higher variability in CK's data. In particular, CK made an effort to capture hours variation by soliciting data on both part-time and full-time employment and converting these data to a full-time-equivalent employment concept. In addition, we are able to obtain a direct comparison of the variability of employment change—with employment measured as the number of employees—for a subset of observations in the payroll data for which respondents happened to supply data on nonmanagement employment (even though we did not ask for these data). We can then extract the subsample of CK's data, distinguished by chain, ownership, and zip-code area, from which these observations in the payroll data are drawn, and compare the variability of the change in number of employees (i.e., without distinguishing full-

---

similar magnitudes in the first half of 1996, in the 7 Pennsylvania counties represented in their original data set. Based on discussions with BLS staff, we understand that in this type of narrowly defined cross-sectional data set, units may move in or out of the sample over time because of errant coding of the franchisee name, which would affect measured changes over time. We emphasize that these concerns are only speculative; further research with the newly accessible disaggregated ES-202 data may ultimately prove informative about minimum-wage effects and many other questions.
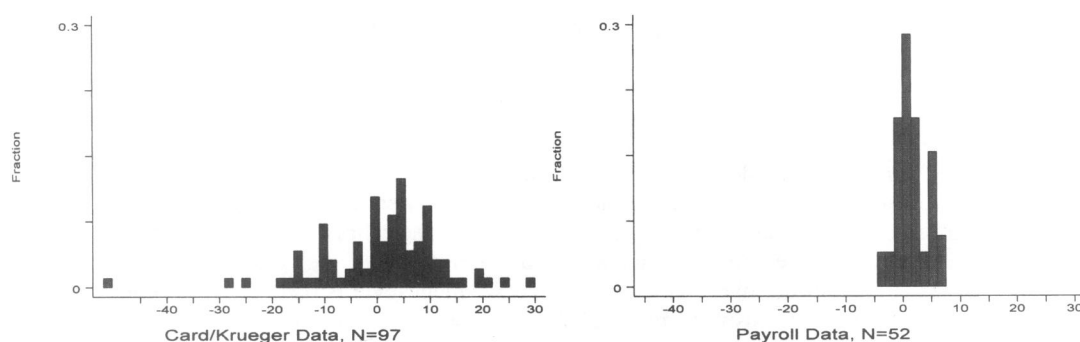
Card/Krueger Data, N=97

Payroll Data, N=52

FIGURE 3. VARIABILITY IN THE CHANGE IN NUMBER OF EMPLOYEES

time from part-time) in the two data sources.[11] For the 52 restaurants in the payroll data that supplied information on employment levels, the standard deviation of the change in the number of workers employed is 2.9. For the 97 restaurants in the corresponding sampled universe of CK's data, the standard deviation is 11.5.[12] Figure 3 shows the histograms for employment change measured this way. As in the previous figures, it is apparent that CK's data indicate much more dispersion than do the payroll data, although this evidence is based on a very small sample.[13]

Alternatively, one could argue that our decision to divide hours by 35 to obtain FTE employment artificially reduces the standard deviation of employment change in our data, if 35 overstates full-time hours. We made this adjustment because CK attempt to measure FTE employment, and because the standard definition of full-time employment (in the CPS, for

example) is 35 hours a week or more. Of course, we could have defined the "average worker" as one who works 20 hours per week, but then we would also want to blow up the number of workers in CK's data in the same way, to get "part-time equivalent" workers, rather than full-time equivalent workers, which would lead to a similar qualitative comparison of variability in the two data sources.[14]

---

[11] As in Table 3, when we have data from more than one payroll period, we randomly select one period.

[12] The same qualitative conclusion holds up if we analyze the employment levels by state. The standard deviations of employment change based on CK's data are 16.1 for Pennsylvania ($N = 23$) and 9.5 for New Jersey ($N = 74$). The comparable figures in the payroll data are 2.5 for Pennsylvania ($N = 33$) and 3.1 for New Jersey ($N = 19$). Thus, again, the variability of employment change is higher by a factor of three or more in CK's data. In all cases, the p-values for the test of equality of standard deviations are below 0.01.

[13] In the payroll data, it appears that the hours data are less variable than the data on number of employees. In this subset of 52 restaurants, the standard deviation of the change in FTE employment using the hours data is 2.2 (vs. 2.9 using number of employees).

[14] CK nonetheless report, in their Reply (Card and Krueger, 2000), evidence suggesting that scaling by 35 hours in the payroll data artificially reduces the dispersion of employment change in our data. Specifically, they report a standard deviation of proportional employment change of 0.35 in our data and 0.39 in their data. Given the depiction of the data in Figures 1 and 2, with roughly equal means and much higher dispersion in levels in CK's data, this seems surprising. Indeed, if we stick to our original procedures used in Figures 1 and 2 and in Table 3, but simply use unscaled hours in our data set (the point on which CK focus) and adopt CK's definition of proportional change relative to average employment in the two periods, we obtain a standard deviation of proportional employment change of 0.21 in our data and 0.42 in their data. In other words, using the standard deviation to measure the variability of employment change, the ratio of this variability in CK's data relative to ours is 2.0. One can obtain different estimates by dividing the change by first-period employment rather than the average, or by retaining the closed restaurants—although, as explained in the text, we argue against doing the latter in comparing employment variability in the two data sets. If we use first-period employment, the resulting standard deviations are 0.19 in our data and 0.53 in theirs, for a ratio of 2.8. If the closed restaurants are included in both samples, we obtain standard deviations of 0.24 in our data and 0.54 in theirs (a ratio of 2.25) if we divide by first-period employment, and 0.35 in our data vs. 0.48 in theirs (with a ratio of 1.37) if we divide by the average. Thus, the first three ways of doing this calculation show smaller dispersion in our data (standard deviations of 0.21, 0.19, or 0.24, vs. 0.35 for the calculation CK report). The 0.39 standard deviation

Instead, we suspect that the high variability of employment change in CK's data is due to the imprecision of their survey questions eliciting employment levels. After verifying that they were speaking with a manager or assistant manager, CK's interviewer asked "How many full-time and part-time workers are employed in your restaurant, excluding managers and assistant managers?" Survey respondents were not given any time period over which to define employment, and their answers may well have ranged from employment on the shift during which the telephone survey took place to employment over an entire payroll period, which could be as long as one month. Moreover, because there appears to have been no effort to interview the same manager in the two waves of the survey, there is no reason to believe that the responses in the first and second waves are based on the same "definition" of employment; this absence of persistence in the measurement error may explain the much higher variability of employment change.[15] In contrast, the payroll data contain total hours worked for a well-defined payroll period (which is specified as either weekly, biweekly, or monthly), on a consistent basis for the two survey periods, and

should therefore provide a more reliable measure of FTE employment change by restaurant.

In fact, the evidence is consistent with rather severe classical measurement error in the employment changes as measured by CK's data.. To see this, consider the subset of 11 restaurants that were accidentally interviewed twice by CK in their first wave. For these restaurants, CK report that the reliability ratio of the employment level is 0.70. If we assume classical measurement error in each wave, with the measurement error uncorrelated across waves, then the reliability ratio of the employment change is considerably lower. For example, if the correlation of the true employment levels across the two waves were 0.5, the reliability ratio for CK's employment changes would be 0.54. In fact, the estimate of the correlation in the levels from the payroll data is 0.81. Assuming that the payroll data have no measurement error would imply that CK's measure of employment change has a reliability ratio of 0.31.[16]

Figure 4 provides some additional perspective on the extent of measurement error in CK's data relative to the payroll data, and on the correlation of the measurement error over time. Given that there likely is a fair amount of persistence in the size of fast-food establishments, we would expect there to be a strong positive correlation between employment levels measured about nine months apart. However, this correlation is likely to be weaker when there is substantial measurement error, as long as the measurement error is not constant across time. The upper-left panel of Figure 4 shows a plot of wave 2 vs. wave 1 employment for CK's data, while the upper-right panel shows the same plot for the payroll data (using a randomly selected payroll period when more than one was reported). The correlation is considerably higher in the payroll data. This can also be seen by plotting the change in employment against the initial employment level of each restaurant. If employment changes are largely unpredictable

---

for the CK data cited in CK's Reply (Card and Krueger, 2000) is obtained by adding back in managers and dropping the procedure of weighting part-time workers at one-half the weight given to full-time workers. [Defining employment in their data this way, the relative variability of employment in CK's data when the closed restaurants are included and the change is divided by the average is 1.09 (0.385/0.354). Higher estimates—1.43, 1.68, or 1.42—are obtained from each of the alternative ways of doing this calculation.] But this definition is inappropriate for comparing CK's data to ours; following the lead set by their original paper, our data are intended to measure full-time equivalent employment, and given the data we collected, the comparison has to be based on nonmanagerial employment (we assume that managerial employment would lower the variability of employment change in our data, as it does in theirs).

[15] Even if CK had collected data defined the same way in the two waves of their survey, a higher variance might be expected in their data if they measured employment at a point in time, while we measure it for a payroll period, which entails some averaging. However, because of the inherent ambiguity in the way CK's survey elicited employment information, we regard it as more likely that changes in the "measurement period" between the two waves of their survey are responsible for the higher variance. Either way, we maintain that our measure is more reliable. For additional criticism of CK's survey instrument, see Finis Welch (1995).

[16] If there is uncorrelated measurement error in the payroll data as well, then 0.81 understates the correlation of true employment, in which case the reliability ratio in CK's data could be even lower. On the other hand, if the measurement error is positively correlated across waves, then the reliability ratio of the change in employment would be higher.
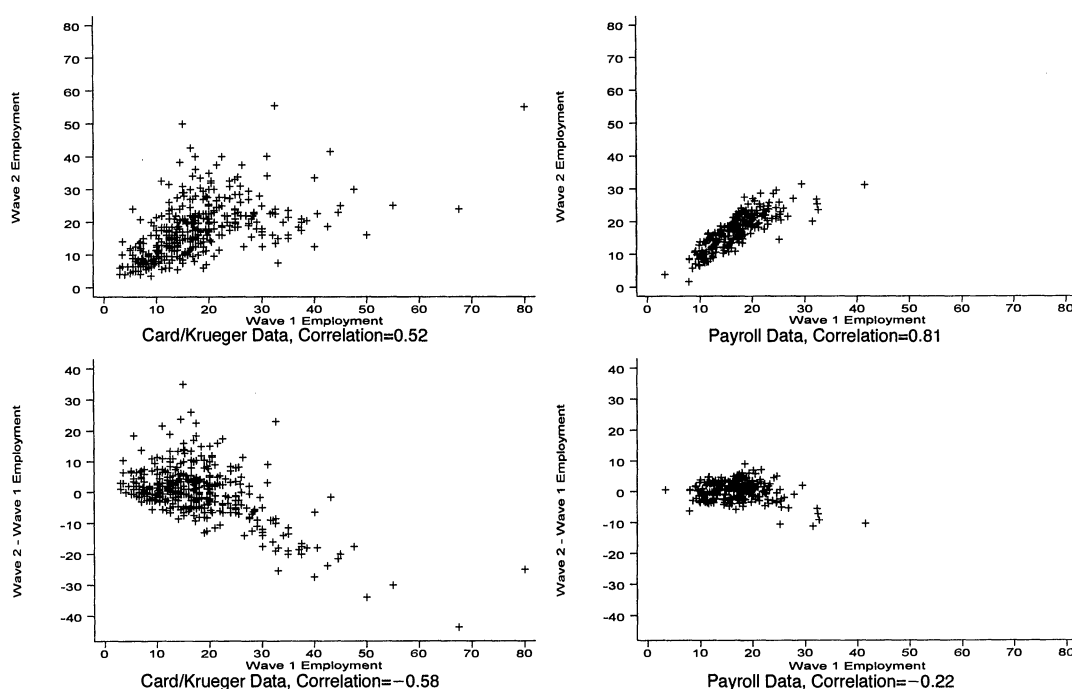
FIGURE 4. ASSESSING MEASUREMENT ERROR IN THE CHANGE IN EMPLOYMENT

based on initial employment, then the correlation between the change in employment and the initial level should be near zero. With classical measurement error that is uncorrelated over time, this correlation should be negative (although we might also expect some regression to the mean in true employment, given the possibility that there are other random influences on employment).[17] The bottom panels show the corresponding plots for the two data sets. In the payroll data, the correlation is relatively small (−0.22), but in CK's data the relationship is strongly negative, with a correlation of −0.58. Thus, these plots suggest that there is considerable classical measurement error in CK's data that is uncorrelated over time.[18]

----

[17] John Kennan (1995) and Welch (1995) also discuss this point.

[18] This evidence suggesting considerable classical measurement error in CK's data does not rule out a systematic component to the measurement error as well, biasing upward employment growth in one state relative to the other.

IV. Comparisons of Estimated Employment Effects in the Payroll and Telephone Survey Data

A. Preliminary Comparisons

The comparisons in Table 3 also indicate that the average employment changes by state differ in the two data sources. For example, CK's data in panel A indicate an average employment decline of 4.8 FTE's in Pennsylvania, while the payroll data show an average employment decline of 0.1 FTE's. For New Jersey, CK's data indicate average employment growth of 0.3 FTE's, while the payroll data indicate an average employment decline of 0.6 FTE's. Because CK's statistical experiment identifies the effect of minimum wages on employment from the difference between employment growth in New Jersey and Pennsylvania, these differences in average employment growth in the two data sources—in particular, the large difference for Pennsylvania—suggest that the payroll data will show much different employment effects of

TABLE 4—REPLICATION RESULTS FOR REDUCED-FORM MODELS FOR CHANGE IN EMPLOYMENT, DIFFERENCE-IN-DIFFERENCES ESTIMATES OF MINIMUM-WAGE EFFECTS

A. *Specifications for Change in Employment* [*CK Table 4, Columns (i) and (ii)*]

	Card/Krueger data				Payroll data	
	CK results (1)	Add observations missing starting wages or data on managers (2)	Drop observations on BK and KFC company-owned restaurants (3)	Sampled universe (4)	Single payroll period (5)	Averages of payroll data (6)
New Jersey dummy, no	2.37	2.94	3.24	3.64	−0.93	−0.85
controls	(1.16)	(1.12)	(1.24)	(1.71)	(0.51)	(0.49)
New Jersey dummy,	2.34	2.97	3.24	3.07	−0.68	−0.66
controls for chain and ownership	(1.17)	(1.12)	(1.26)	(1.80)	(0.43)	(0.41)
Implied elasticity	0.70	0.89	0.93	0.85	−0.22	−0.21
Percentage effect	13.3	16.8	17.4	16.0	−4.0	−3.9
Number of observations	357	391	330	214	235	235
Adjusted R^2	0.009	0.019	0.019	0.038	0.371	0.407

B. *Specifications for Proportional Change in Employment* [*CK Table 5, Columns (i) and (iii)*]

	Card/Krueger data				Payroll data	
New Jersey dummy,	0.054	0.092	0.120	0.097	−0.040	−0.039
controls for chain and ownership	(0.058)	(0.055)	(0.058)	(0.078)	(0.032)	(0.031)
Implied elasticity	0.29	0.49	0.64	0.51	−0.21	−0.21
Adjusted R^2	0.007	0.016	0.023	0.058	0.302	0.318

Notes: Standard errors are reported in parentheses. The estimates are for nonmanagement employment. The elasticities and percentage effects in panel A are computed at the sample means for the change-in-levels specifications, using the estimates including controls. In panel B, the results for row 1 of CK's Table 5 are replicated.

the New Jersey minimum-wage increase than those estimated by CK.

B. *Replication Analysis of the New Jersey-Pennsylvania Experiment*

Table 4 reports results from replicating CK's analysis of the New Jersey-Pennsylvania minimum-wage experiment using the payroll data. In this replication analysis, we stick as closely as possible to CK's original analysis, so that differences in the results stem only from differences in the data. Below, we present some additional analyses of the two data sets to probe the robustness of the results. We have clearly distinguished the replication analysis from these additional analyses, so that the reader can assess the evidence from the replication analysis in isolation from the sensitivity analysis, which potentially reflects our choices of specifications to report.

Panel A of Table 4 reports difference-in-differences estimates using the change in the level of employment. Paralleling CK's Table 4, we first report regression estimates with the New Jersey dummy as the only independent variable, and then adding controls for chain and ownership. Column (1) reports CK's results, while column (2) reports results obtained from their data using a somewhat larger sample that includes observations missing data on starting wages or number of managers, neither of which we use in these specifications. With or without the controls, CK's data indicate positive and statistically significant effects of the minimum wage on employment, with elasticities at or above 0.7.[19] These estimates correspond very

[19] If some of the extreme outliers apparent in Figures 1 and 2 are dropped, the estimated minimum-wage effect in CK's data remains positive, but not statistically significant.

closely to those in columns (i) and (ii) in CK's Table 4; they differ slightly because we limit our definition of employment to nonmanagement employees, to best match the payroll data. We next restrict the sample to exclude Burger King and KFC company-owned restaurants [column (3)], because there are no observations on such restaurants in the payroll data. The results are little changed, with a positive and significant minimum-wage effect, and an elasticity of 0.93. The fact that this sample restriction increases the estimated positive minimum-wage effect in CK's data again suggests that the opposite results we obtain with the payroll data, reported below, do not stem from the exclusion of certain types of restaurants. Finally, in column (4) we report results using only the sampled universe of CK's data, which leads to similar conclusions.[20]

Column (5) reports results for the same specification, replacing CK's data with the payroll data, and using the same single payroll reading from each wave that we used in Table 3 and Figures 1–4. We now include the closed restaurants in the sample, as they do. In contrast to CK's results, the payroll data indicate that relative employment *decreased* in New Jersey. The estimate from the regression with no controls shows a relative employment decline of 0.93 FTE's in New Jersey, and the estimate with controls indicates a decline of 0.68 FTEs; the first estimate is significant at the 10-percent level, and the second is nearly so. The

implied elasticity of employment with respect to minimum wages (based on the second regression) is −0.22, toward the upper end of the range of disemployment effects found in other minimum-wage studies (e.g., Charles Brown et al. [1982] and Neumark and Wascher [1992]).[21] Of course, it is not clear what elasticity we ought to expect for fast-food employment, as estimates from existing research generally refer to overall employment effects for teenagers or young adults.[22]

To this point, we have used the payroll data set in which we randomly selected one payroll record within the periods in which CK's surveys were conducted. However, we can obtain more precise estimates of employment in each period by averaging across all possible payroll records within CK's survey periods, when we have more than one such record (which we do for 37 percent of the observations). Estimates using these averages are reported in column (6) of Table 4, and in our view provide the best estimates from the payroll data. In this case, the estimated disemployment effects of the New Jersey minimum-wage increase are similar: −0.85 without controls (significant at the 10-percent level) and −0.66 with controls, with both standard errors slightly lower. The latter estimate implies an elasticity of employment with respect to the minimum wage of −0.21, and an employment loss of 3.9 percent in New Jersey relative to Pennsylvania.[23]

Panel B reports specifications defining the dependent variable in terms of proportional

---

Dropping observations with employment changes of 30 or more FTE's (in absolute value), the estimated coefficient corresponding to column (1) is 1.42, with a standard error of 1.10 and an implied elasticity of 0.44. Using instead a cutoff of changes of plus or minus 20 FTE's, the estimated coefficient is 1.27, with a standard error of 0.97 and an implied elasticity of 0.40.

[20] The high estimated elasticities of employment with respect to the minimum wage suggest that something is awry with CK's data if we interpret them as providing estimates of the elasticity of labor demand, as is standard in the literature. However, as Stephen Machin and Alan Manning (1994) point out, if labor markets are characterized by dynamic monopsony then we may be identifying the slope of the supply of labor curve to individual firms. In this case, an elasticity of 0.85 might be interpreted as too low, if anything. Machin and Manning are also careful to point out that micro-level evidence of a positive relationship between wage variation induced by minimum wages and employment at the establishment level does not imply that in the aggregate economy, raising the minimum wage would raise employment.

[21] Note also that the adjusted R^2 jumps from 0.04 to 0.37, apparently reflecting the considerable sampling error in CK's data, and the fact that differences in employment growth across chains are strongly significant in the regressions using the payroll data, but not in CK's data.

[22] CK argue that if the conventional labor-demand model is correct, the existing estimates should provide a lower bound for minimum-wage effects on fast-food employment, presumably because a relatively large proportion of fast-food workers are paid at or near the minimum wage. However, the minimum-wage effect will also depend on the elasticity of substitution between low-skill labor and other inputs, the share of low-wage labor in total costs (overall and relative to other employers), the elasticity of supply of other factors, and the elasticity of demand for fast-food products. We are not aware of estimates of these magnitudes in this industry.

[23] We also tried weighting by the number of payroll records used in constructing these averages. This had little influence on the estimates.

changes, results that CK do not highlight as much but report in their table of specification checks. We follow CK in defining the dependent variable as the change in employment divided by average employment in the two waves (setting this to -1 for restaurants that closed), although we think it would be preferable to use initial employment in the denominator.[24] The results reported in columns (1)–(4) again use CK's data. The estimated minimum-wage effects are positive (although not always significant), and the estimated elasticities range from 0.29 to 0.64. As in panel A, the payroll data indicate opposite-signed employment effects. In this case, although the estimates are not statistically significant, the estimated elasticity using the payroll data is -0.21, similar to what we obtained using the change-in-levels specification in panel A.

Thus, the replication analysis using the payroll data yields qualitatively different answers compared with those obtained from CK's data. Whereas CK's data indicate minimum-wage effects on employment that are positive and sometimes significant, the payroll data indicate effects that are negative and sometimes significant. One can argue that some of the differences in results are not statistically significant, but many of them are. Indeed, given that there are overlapping observations in the two data sets, the standard error of the difference between the estimates from the two data sets could be considerably smaller than indicated by the standard errors of the separate estimates. Regardless, we doubt that any researcher would use the results from the payroll data to conclude that minimum wages either have no effect on employment or increase employment.

C. *Wage-Gap Regressions: Further Replication Analysis*

CK also present results in which they regress employment changes on the percentage difference between the minimum wage and the start-

ing wage paid by the restaurant in the first wave of the survey, with this "wage-gap" variable set to zero for Pennsylvania restaurants. This experiment continues to identify minimum-wage effects off of the difference in employment growth between New Jersey and Pennsylvania, but adds information on the extent to which the minimum-wage increase would have raised starting wages in New Jersey. Again, in contrast to the conventional prediction, CK find a positive effect of this wage-gap measure.

We did not collect data that would allow us to reliably reevaluate CK's results from the wage-gap specifications because we felt that recovering estimates of starting wages could be rather complicated for the respondents and might discourage them from supplying the more important hours data. Moreover, the New Jersey-Pennsylvania comparison was widely regarded as the key methodological contribution of CK and therefore provided the most compelling evidence that they had to offer; we presume that this is why CK went beyond the earlier paper by Lawrence F. Katz and Krueger (1992)—which used within-state variation in initial wages in Texas to study minimum-wage effects—and instead identified the minimum-wage effect from the difference in employment change between New Jersey and Pennsylvania.[25]

In any event, although we do not have information on the starting wages paid at the restaurants for which we have payroll data, we can obtain a relatively crude comparison of results for the wage-gap regressions by estimating the average starting wage by restaurant type (distinguished by chain and ownership) and zip-code area using CK's data, matching this average to corresponding establishments in the payroll data, and estimating CK's specification. We caution, however, that our evidence on this point is not as credible as the direct FTE com-

---

[24] Dividing by average employment results in a small number of restaurants that remained open, but that show proportional changes of between -1 and -2, which implies a more than 100-percent decline in employment. While setting these to -1 made virtually no difference, in this subsection of the paper we follow CK's procedures.

[25] We also regard the New Jersey-Pennsylvania experiment as providing more compelling evidence than the within-New Jersey variation in the wage gap, as it would not be at all surprising for variation in starting wages at the time of the first survey—and hence variation in the "treatment" in the wage-gap specification—to be related to variation in economic conditions across restaurants. For example, if restaurants paying higher wages are those that recently expanded employment, we might expect lower subsequent employment growth at the higher-wage restaurants, which could explain CK's results.
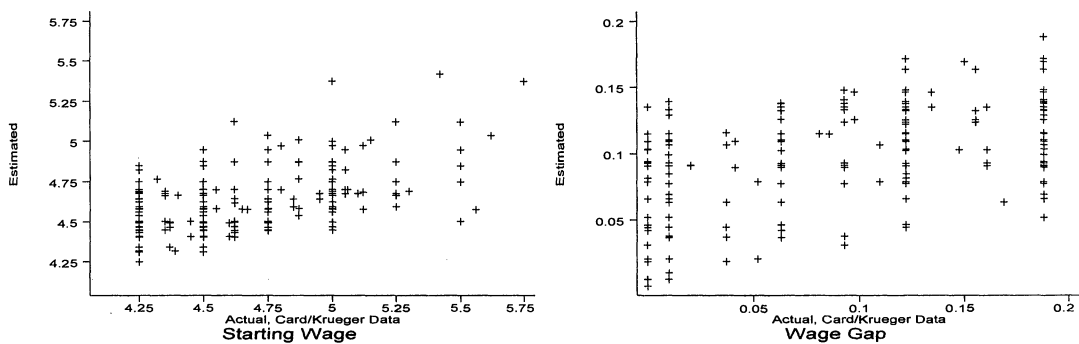
FIGURE 5. ESTIMATING STARTING WAGES AND WAGE GAPS

TABLE 5—REPLICATION RESULTS FOR REDUCED-FORM MODELS FOR CHANGE IN EMPLOYMENT,
WAGE-GAP ESTIMATES OF MINIMUM-WAGE EFFECTS

A. *Specifications for Change in Employment [CK Table 4, Columns (iii) and (iv)]*

	Card/Krueger data			Payroll data
	CK results (1)	Sampled universe (2)	Same as column (2), estimated wage gap (3)	(4)
Wage-gap variable, controls for	14.7	22.6	28.4	−1.8
chain and ownership	(6.1)	(8.9)	(12.7)	(3.2)
Implied elasticity	0.48	0.71	0.89	−0.06
Number of observations	357	204	204	231

B. *Specifications for Proportional Change in Employment [CK Table 5, Columns (ii) and (iv)]*

	Card/Krueger data			Payroll data
Wage-gap variable, controls for	0.40	0.76	1.25	−0.02
chain and ownership	(0.30)	(0.39)	(0.55)	(0.24)
Implied elasticity	0.22	0.45	0.73	−0.01

Notes: See notes to Tables 1 and 4. Standard errors are reported in parentheses. The estimates are for nonmanagement employment. The estimated wage gap used in columns (3) and (4) is calculated from the Card/Krueger data by zip-code area using the appropriate sample (defined by chain and ownership). The elasticity is evaluated at the sample mean of the wage-gap measure for New Jersey, since that mean multiplied by the estimated minimum-wage coefficient estimates the reduction in employment from closing the wage gap. Correspondingly, the mean employment level for New Jersey is used.

parisons in Table 4. As shown in Figure 5, a given actual starting wage in CK's data can be mapped into a wide range of possible estimated starting wages, and thus the wage-gap measure we use likely suffers from considerable measurement error. Nonetheless, it is of interest to check whether the same reversal of the sign of the minimum-wage effect occurs in the wage-gap specification. We can also obtain a more comparable set of results with CK's data by estimating the wage-gap regression with their sample, but using the same average wage mapping to construct the wage-gap variable that we are forced to use with the payroll data.

The results of this exercise are reported in Table 5.[26] We begin by reporting CK's results for the change in employment levels, in column (1) of panel A. The estimated coefficient on the wage gap is 14.7, and the implied elasticity with respect to the minimum wage is 0.48. In column (2) we restrict the sample to the sampled universe, while continuing to use the actual starting wage. The estimated effect rises substantially,

[26] From this point on, we show results for the averaged payroll data; the results were very similar using the randomly selected payroll observations.

and the elasticity climbs to 0.71. Column (3) shows the effect of using the mean starting wage in the zip-code area (for the corresponding type of restaurant) to construct the wage-gap measure—as we have to do with the payroll data—instead of using the actual reported starting wage at the restaurant. The point estimate is larger and, as would be expected, the standard error increases.

Finally, column (4) reports results from the payroll data. In contrast to the results obtained with CK's data, the estimated coefficient on the wage-gap variable is negative, although it is small and insignificant, with an elasticity of only -0.06. The larger positive estimates in columns (2) and (3) than in column (1), using CK's data, indicate that the differences in the results from using the payroll data are not attributable to the restriction to a subset of zip-code areas and restaurant types or to the use of a mean starting wage to construct the wage-gap measure. Panel B reports similar results for the specification using the proportional change in employment as the dependent variable. The qualitative results are similar: CK's data indicate positive effects of the minimum wage on employment that are significant for the sampled universe, while the payroll data show small and insignificant negative effects.

Despite the same sign reversal in the payroll data that we saw for the New Jersey-Pennsylvania experiment, the evidence from the two data sources appears less different in the wage-gap specifications, although the absence of data on actual starting wages in the payroll data makes this comparison tenuous. The smaller differences for the wage-gap specification are even more apparent if we drop the Pennsylvania observations (rather than defining the wage gap for these specifications to be zero, as CK do), so that the wage-gap coefficient is identified solely off of the within-New Jersey variation. In this case, the estimated coefficients of the wage-gap variable using the payroll data turn positive but remain insignificant. For example, using the change-in-levels specification, the estimate (standard error) is 7.80 (6.01), with an elasticity of 0.26. (Although CK did not report estimates for this specification and subsample, the estimates for this subsample with their data are very similar to those in Table 5.) Thus, the sharp differences between our

results and CK's appear to stem mainly from the differences in employment growth between New Jersey and Pennsylvania, rather than differences across restaurants within New Jersey.[27]

D. Sensitivity Analyses for the New Jersey-Pennsylvania Experiment

In order to highlight the differences in the results generated by the two data sets, we have thus far focused almost exclusively on the specifications reported by CK. In Table 6, we move beyond this pure replication analysis and report some sensitivity analyses of the effect of the minimum-wage increase on employment growth in New Jersey relative to Pennsylvania, using both CK's data and the payroll data. Our intent in this section is to explore whether the differences in results are specific to the main set of specifications reported by CK, or rather extend to a wide range of reasonable alternative specifications and estimators, some of which were not reported by CK; the latter turns out to be the case.

We first report results with the closed restaurants excluded from both data sources, rather than including them with second-period employment set to zero. Standard competitive theory suggests that a minimum-wage increase will result in some combination of employment falling at establishments that remain in business and other establishments being driven out of business. But because closing is a rare occurrence (in the payroll data sample five restaurants closed, and in CK's data six closed) and may generate extreme values for the change in employment, random differences between the two data sets in the sample distribution of closings across states could have an undue influence on

---

[27] Of course, given that we do not have the actual wage data, we cannot rule out the possibility that the results are driven by problems with CK's wage data, either because of measurement error or a lack of correspondence between the wage-gap measure and the increase in unit labor costs imposed by a minimum-wage increase. For example, Welch (1995) suggests that there may be considerable measurement error in CK's data, based on the fact that a large majority of restaurants whose starting wages at the time of the first interview were above the minimum prevailing at the time of the second interview lowered their wages as of the second interview, consistent with the regression to the mean that measurement error generates.

TABLE 6—SENSITIVITY ANALYSIS OF ESTIMATED MINIMUM-WAGE EFFECTS

	Card/Krueger data		Payroll data	
	Change in employment (1)	Proportional change in employment (2)	Change in employment (3)	Proportional change in employment (4)
A. Exclude closed restaurants	3.24	0.120	−0.72	−0.026
	(1.81)	(0.075)	(0.34)	(0.022)
Implied elasticity	0.89	0.64	−0.23	−0.14
B. Include only closed restaurants appearing in both data sources	3.12	0.106	−0.79	−0.040
	(1.80)	(0.076)	(0.34)	(0.025)
Implied elasticity	0.86	0.56	−0.25	−0.21
C. Divide by initial employment	—	0.053	—	−0.042
		(0.092)		(0.029)
Implied elasticity	—	0.28	—	−0.22
D. Weighted to represent restaurants in sampled universe of CK's data	—	—	−0.79	−0.046
			(0.46)	(0.032)
Implied elasticity	—	—	−0.24	−0.25
E. Random-effects estimates	—	—	−0.50	−0.030
			(0.46)	(0.033)
Implied elasticity	—	—	−0.16	−0.16
LM test for random effects (*p*-value)	—	—	0.76	0.45
F. Robust (Huber) regressions	1.14	0.117	−0.62	−0.027
	(1.42)	(0.081)	(0.31)	(0.020)
Implied elasticity	0.31	0.62	−0.19	−0.14
G. Quantile regressions				
25th centile	4.25	0.217	−0.74	−0.025
	(3.44)	(0.131)	(0.34)	(0.027)
Implied elasticity	1.18/1.65	1.15	−0.23/−0.30	−0.13
Median	3.00	0.124	−0.39	−0.050
	(2.42)	(0.138)	(0.35)	(0.027)
Implied elasticity	0.83/0.91	0.66	−0.12/−0.12	−0.27
75th centile	0.25	−0.007	−0.42	−0.034
	(2.45)	(0.115)	(0.49)	(0.017)
Implied elasticity	*0.07/0.06*	*−0.04*	*−0.13/−0.11*	*−0.18*

Notes: Estimated coefficients from the regressions with controls included are reported, with standard errors in parentheses. The estimates are for nonmanagement employment. All closed restaurants are included in panels C–G. In all panels except C, the proportional change is calculated relative to average employment in the two waves (as in CK), but reset to −1 if the change is between −1 and −2. All elasticities for the change-in-levels specifications are computed at sample means, except the second entries in panel G, which are computed at the corresponding centile. Standard errors for the quantile regressions are based on bootstrapped standard errors using 1,000 replications.

the results. As panel A of Table 6 shows, however, the qualitative results are largely unchanged. The estimated minimum-wage effects using CK's data are similar to those obtained when the closed restaurants are included.[28] In the payroll data, the evidence of a disemployment effect of the minimum wage

is slightly stronger when these restaurants are excluded, with the estimate now significant at the 5-percent level in the change-in-levels specification.

In panel B, we add back into the samples two closed restaurants that appear to be common to both data sources (i.e., they appear in the same zip-code area for the same type of restaurant distinguished by chain and ownership). Of the estimates presented thus far, we regard these as providing the most reliable comparison between the two data sources, because there are not

[28] Throughout this table, we show results using CK's data for the sampled universe only; as in Table 4, the results were qualitatively similar using CK's full sample, with somewhat smaller elasticities.

identifiable inconsistencies regarding restaurant closings. Again, the estimates using CK's data are little changed. For the payroll data, this more restrictive rule for including closed restaurants yields stronger negative estimated minimum-wage effects on employment, significant at the 5-percent level in the specification for the change in levels, and nearly significant at the 10-percent level in the specification for the proportional change in employment. The estimated elasticity is -0.25 for the first specification, and -0.21 for the second.

In panel C, we reestimate the proportional-change specification defining the dependent variable as the change in employment divided by the initial level of employment (whereas CK divided by the average of employment in the two waves of the survey). We view this specification as preferable to CK's, because conditional on the initial employment level, dividing by the average attenuates employment increases and amplifies employment decreases (and because dividing by initial employment avoids the potential for declines in employment to exceed 100 percent). With CK's data, the evidence of a positive employment effect is weaker using this specification. In contrast, with the payroll data the estimate (standard error) of the minimum-wage effect rises to -0.042 (0.029), implying an elasticity of -0.22. [If the closed restaurants are dropped for this specification, the estimate is -0.031 (0.021), with an elasticity of -0.17.] Thus, although in general the evidence from the payroll data is weaker in the proportional-change specifications, there are some reasonable proportional-change specifications that yield marginally significant negative employment effects, and the estimated elasticities are generally similar to those from the change-in-levels specifications.

In panel D, we explore the influence of the different representation of restaurant types in the payroll data and in CK's data. In particular, we weight the observations in the payroll data to correspond to the restaurant types in the sampled universe of CK's data set, calculating separate weights by chain, ownership, and state. For example, Table 2 indicated that Burger King franchises in Pennsylvania are overrepresented in the payroll data by a factor of 31/20, and so we apply a weight of 20/31 to observations on these units. The weighted estimate of

the minimum-wage effect for the specification for the change in levels is negative and significant at the 10-percent level (with an elasticity of -0.24), while the estimated effect for the proportional-change specification is stronger than before, although still insignificant (with an elasticity of -0.25).

Because we obtained data on multiple restaurants owned by franchisees (or corporations), we also estimated random-effects models that allow for nonindependent errors among restaurants with the same owner, to see whether we are overstating the statistical significance of our results.[29] These estimates, reported in panel E of Table 6, indicate a smaller negative minimum-wage effect on employment (e.g., -0.50 in the specification for changes in levels), and, as we would expect, a larger standard error. However, LM tests indicate that we cannot reject the hypothesis of no common error across restaurants with the same owner, suggesting that the OLS estimates are preferable (i.e., consistent and more efficient) to the random-effects estimates.

Panel F reports results from robust (Huber) regressions, which downweight outliers. Perhaps not surprisingly, given the wide dispersion of employment change in CK's data, the employment effect with CK's data falls to 1.14 and becomes statistically insignificant, although this reduction occurs only for the change-in-levels specification. In contrast, the results using the payroll data are little different from the linear regression estimates, indicating a significant (at the 5-percent level) negative effect of the minimum wage in the specification for changes in levels, and a negative but insignificant effect in the proportional-change specification.

An alternative robust regression technique is quantile regression. In the difference-in-differences context, the use of medians (or other quantiles) is probably of limited interest, since estimating the minimum-wage "effect" using the difference of the median level in each period is not particularly meaningful. However, quantile regression is a valid robust technique for estimating the regression model in terms of employment changes, and thus we computed

---

[29] Because CK's sample was drawn from telephone books, they could not have identified common owners (except for their company-owned restaurants).

these estimates at the 25th, 50th, and 75th cen-
tiles to gauge the sensitivity of the results. The
estimates are reported in panel G. In CK's data,
the estimates are relatively large and positive
for the 25th and 50th centiles, but the estimated
minimum-wage effects are much smaller at the
75th centile, and actually negative (although
insignificant) for the proportional-change spec-
ification. Given that the outliers in CK's data
appear to be concentrated in the lower tail for
Pennsylvania (see the upper right-hand panel of
Figure 1), the quite different estimate at a higher
centile in these data is not surprising. In the
payroll data, the quantile regression estimates
also move around somewhat, but are uniformly
negative and sometimes statistically significant.
Interestingly, at the 50th and 75th centiles the
estimated coefficients from the proportional-
change specification are significant at the 5- or
10-percent level, and in general the evidence of
disemployment effects from the proportional-
change specification appears as strong as that
from the change-in-levels specification.

Overall, the estimates reported in Table 6 and
discussed in this section reinforce our general
conclusion that CK's data point to positive ef-
fects of New Jersey's minimum-wage increase
on relative employment growth in that state,
while the payroll data point in the opposite
direction. The pattern of estimated signs is gen-
erally consistent with this, and many of the
positive estimates using CK's data and many of
the negative estimates using the payroll data are
significantly different from zero.

E. Explaining the Different Results

An obvious question that arises from these
results is whether there are identifiable concep-
tual differences between the payroll data and
CK's data that might lead to opposite-signed
estimates of the effect of New Jersey's
minimum-wage increase on relative employ-
ment growth in that state. In this subsection, we
address several potential explanations for the
disparity in results generated by the two data
sources.

Employment vs. Hours.—One possibility is
that the different estimated employment effects
in the two data sets arise because CK survey
numbers of employees, while we collected data

on total hours worked. Of course, both CK and
we converted the raw employment/hours data to
a full-time equivalent basis, so that this expla-
nation must imply discrepancies or errors in the
conversion to FTE's, rather than a conceptual
difference in the focus of the two studies. Nev-
ertheless, given the relatively inexact nature of
the full-time equivalent measure developed by
CK, it seems worth exploring whether this
might be a source of the disparity in results
generated by the two data sources.

This explanation requires that the relative
number of employees in New Jersey rose fol-
lowing that state's minimum-wage increase
(consistent with CK's results), while the relative
number of hours worked fell (consistent with
the payroll data).[30] This could happen, for ex-
ample, if the higher minimum wage led to a
shift from full-time to part-time employment in
New Jersey fast-food restaurants. In their arti-
cle, however, CK argue that the conventional
model would predict the opposite result: em-
ployers facing a higher minimum wage would
be likely to substitute toward full-time workers
who "in fast-food restaurants are typically older
and may well possess higher skills than part-
time workers" (p. 784). Moreover, CK's data do
not support an explanation based on a shift
toward part-time employment. Repeating the
regression shown in panel A of Table 1 sepa-
rately for full-time and part-time employees
(treating managers as full-time, as CK do), we
find that the number of full-time workers in
New Jersey rose by 3.16 (with a standard error
of 1.32) relative to Pennsylvania, while the
number of part-time workers fell by 0.60 (with
a standard error of 1.31).[31] The signs of these
coefficients are the opposite of what would be
required to argue that New Jersey's minimum-
wage increase led to a substitution of part-time
for full-time workers. Thus, the patterns of em-
ployment change in CK's data suggest that the
different results in the two data sets are not due

[30] This assumes that hours of full-time and part-time
workers remain unchanged. Alternatively, a change in the
hours of one of these groups could also cause a divergence
of results in the two data sets.

[31] The results are very similar if we exclude managers, as
in panel B of Table 1, or if we look only at the sampled
universe of CK's data, as in panel C of Table 1. Katz and
Krueger (1992) report a similar result.

to differing effects of minimum wages on employment and hours.

In contrast, the payroll data provide some limited evidence that the minimum-wage increase led to substitution towards part-time workers. Using the subset of our data (52 observations) for which we can compare number of workers and hours or FTE's, the difference-in-differences regression estimate (standard error) is 0.96 (0.70) for the number of workers, and -0.87 (0.57) for FTE's. Of course, this does not explain the difference between the results from the payroll data and CK's data (which indicate a shift *away* from part-time workers). It does, however, suggest that our evidence of negative minimum-wage effects for FTE's does not necessarily carry over to the number of workers employed, although we would caution that the evidence on number of workers is based on a very small sample.[32]

Nonetheless, aside from the pure replication aspect of this paper, we regard the hours effects as most pertinent to the study of low-wage labor markets. Because the minimum wage is defined in hourly terms, it seems most appropriate to test for its effects by looking at changes in hours worked. Moreover, a principle goal of minimum-wage research has been to test the competitive model of labor demand, and the direct implications of this model hold only for the number of hours worked. Thus, if our results for hours are accepted, the conclusion is that raising the minimum wage reduces the amount of labor employed, which is the prediction of the textbook model. We recognize, however, that policy makers may have an independent interest in minimum-wage effects on employment, because of the political focus on the potential job loss associated with a minimum-wage increase.

Sampling Issues.—Some participants in the recent minimum-wage debate have questioned the representativeness of our sample, suggesting that the difference in results from the two data sources may reflect a flaw in our sampling procedure—in particular, the fact that some of the observations were provided to us by the Employment Policies Institute. As explained in detail in Appendix A, the original EPI sample was drawn partly from franchisees in the *Chain Operators Guide* that were headquartered in New York, New Jersey, or Pennsylvania, partly from restaurants owned by other franchisees (headquartered elsewhere) that were identified informally, and partly from parent corporations (Wendy's in particular). In all cases, the restaurants surveyed by the EPI were drawn from zip-code areas in which one or two franchisees (or a corporation) owned all of the restaurants in a chain, in order to obtain a set of zip-code areas with complete data that would provide a more direct comparison of the variability of employment change between the payroll data and CK's data. Subsequent to EPI's initial data collection, we attempted to survey all of the remaining franchisees in the *Chain Operators Guide* that were headquartered in New York, New Jersey, or Pennsylvania, as well as the remaining parent corporations. Given the differences that we have highlighted thus far between the full payroll sample and CK's data, two issues regarding the sampling arise. First, did the multistage fashion in which the data were collected, and the EPI data collection in particular, yield a final sample that is nonrepresentative and biased in a direction generating results most at odds with

---

[32] It has also been suggested to us that the disparity in results might be driven by a difference in the way in which the two data sources treat undocumented workers. In particular, if CK's telephone survey picked up undocumented workers that were not included on our payroll records, then the payroll data would overstate the disemployment effect of the higher minimum wage to the extent that New Jersey employers substituted undocumented for documented workers in response to the minimum-wage increase. Although we are unaware of any direct evidence on the use of undocumented workers in the fast-food industry, we are skeptical of this explanation for two reasons. First, the relative employment increase in New Jersey indicated in CK's data would require that—in relative terms—employers in New Jersey substituted more than three undocumented workers for each documented worker in response to the minimum-wage increase. (The payroll data indicate a decline of about one FTE in response to the minimum-wage increase, while CK's data indicate an increase of at least two FTE's, for a gross gain of three or more undocumented workers.) Second, the biggest difference between the two data sources is the greater employment decline in Pennsylvania in CK's data, rather than the greater employment increase in New Jersey that would be expected if New Jersey's minimum-wage increase led employers there to substitute undocumented for documented workers. Finally, note that even if this were the explanation for the different results, substitution of uncovered for covered workers in response to a minimum-wage increase would be consistent with the standard competitive model of low-wage labor markets.

TABLE 7—ESTIMATES AND CHARACTERISTICS OF DATA FROM ALTERNATIVE SOURCES

| | Data collected by us | | Data collected by EPI | | | Both sources, consistent universe |
	All restaurants (1)	Restaurants comparable to data from EPI (2)	All restaurants (3)	Franchisees not in Chain Operators Guide (4)	Franchisees in Chain Operators Guide and parent corporations (5)	All franchisees drawn from Chain Operators Guide and parent corporations (6)
Pennsylvania observations	40	8	32	0	32	72
New Jersey observations	114	30	49	32	17	131
Mean (standard deviation) of employment change						
Pennsylvania	−1.0	2.3	3.0	—	3.0	0.8
	(3.5)	(1.1)	(2.2)		(2.2)	(3.5)
New Jersey	−1.0	1.9	2.1	2.1	2.2	−0.6
	(3.3)	(2.1)	(2.7)	(3.1)	(1.7)	(3.3)
Regression estimates of relative employment growth, change in employment in New Jersey	−0.27	−0.33	−0.95	—	−1.63	−0.74
	(0.53)	(0.80)	(0.63)		(0.67)	(0.42)
Implied elasticity	−0.09	−0.10	−0.29	—	−0.49	−0.23
Regression estimates of relative employment growth, proportional change in employment in New Jersey	−0.018	−0.055	−0.039	—	−0.083	−0.043
	(0.041)	(0.040)	(0.041)		(0.033)	(0.030)
Implied elasticity	−0.10	−0.29	−0.21	—	−0.44	−0.23

Notes: Estimated coefficients from the regressions with controls included are reported, with standard errors in parentheses. Standard deviations are based on random selection of one payroll period when multiple payroll periods were available. Column (2) includes Burger King franchises in New Jersey and Pennsylvania and Wendy's franchises in New Jersey, which are the types of restaurants in the data collected by the EPI that also appear in the data we collected.

CK's findings? Second, given the circumstances, what is the proper way to handle the subsamples?

Columns (1) and (3) of Table 7 provide a comparison of the set of observations that we collected with the data collected by the EPI and supplied to us. Two differences are apparent. First, the standard deviations of employment change in both Pennsylvania and New Jersey are somewhat higher in the data we collected. Second, the difference-in-differences estimates of the disemployment effect of the minimum wage are smaller in the data collected by us (with elasticities of −0.09 to −0.1) than in the data collected by the EPI (with elasticities ranging from −0.21 to −0.29). Even in the data we collected, however, the standard deviation is well below that in CK's data, and the point estimates of the minimum-wage effect are negative. In addition, as can be seen in column (2), if we restrict attention to the

subset of the observations we collected that are represented in the data collected by the EPI (by chain and ownership status), the differences in standard deviations are reversed, and the difference in disemployment effects is reversed in the proportional-change specification.

Nonetheless, the comparison between columns (1) and (3) might be read as consistent with the EPI having somehow selected a set of observations in which the results were most discordant with CK's results. Should we then, as some have suggested, discard the EPI data and focus solely on the data we collected? In that case, the remainder of the sample (the data collected by us) would be biased in the other direction, as the estimates in column (1) might suggest. Note, in particular, that the Pennsylvania observations are roughly divided between columns (1) and (3), so that the bias in the sample not included in the EPI data could be as

large as that in the data collected by the EPI.[33] Indeed, because we supplemented the EPI data by attempting to collect data from the remainder of the franchisees listed in the *Chain Operators Guide* and the parent corporations, only the full sample is representative of the universe of fast-food restaurants in the appropriate chains in the relevant geographic areas. Thus, the proper response to concerns over bias associated with the nonrepresentativeness of data collected by the EPI is not to discard the EPI data, but rather to base the analysis on the full payroll data sample—including those observations collected by the EPI. This, of course, is the sample we have analyzed throughout the paper.[34]

The only legitimate objection to the validity of the combined sample is that some observations added by the EPI were not drawn from the *Chain Operators Guide,* but rather were for

franchisees identified informally. In columns (4) and (5) of Table 7, we present the results separately for these two subsets of observations. There are three things to note. First, the restaurants in Pennsylvania were all owned by a franchisee who was drawn from the *Guide.* Given that the main difference between the payroll data and CK's data was in the Pennsylvania sample, this fact casts doubt on the importance of non-*Guide* franchisees in explaining the different results. Second, the mean employment change in the New Jersey stores from franchisees identified through other means was virtually identical to the mean employment change for those identified through the *Guide* (although the standard error was higher). Third, the estimated disemployment effect of the minimum wage in the subset identified through the *Guide* is actually larger than that estimated using the EPI subsample as a whole, although this result is based on a small sample.

Nonetheless, in column (6) we report results combining the observations we collected with the subset of observations collected by the EPI that either were from franchisees listed in the *Chain Operators Guide* or were obtained from a parent corporation (Wendy's); this is a sample drawn from a well-defined, valid universe of restaurants in the relevant geographic area. These estimates reveal disemployment effects of the minimum wage very similar to those reported in earlier tables, with a negative employment effect that is significant at the 10-percent level in the change-in-levels specification, and a negative but insignificant effect in the proportional-change specification. In both cases the implied elasticity is -0.23.

Response Bias.—The previous discussion addressed the potential nonrepresentativeness stemming from the sampling procedures. A second issue related to representativeness of the sample is whether response bias led to an overrepresentation in the full payroll sample of restaurants for which actual employment changes deviated most from CK's results (e.g., the subset of restaurants in New Jersey that had the sharpest employment declines); this could occur, for example, if the owners of these restaurants were more motivated to participate by supplying data. By comparing the results from the payroll data with those from the sampled

---

[33] This is especially true if the EPI, in its initial data-collection efforts, encountered roughly the same nonresponse rate as we did.

[34] Note also that the issue of the larger standard errors in the subsample we collected, which has been raised in a polemical essay by John Schmitt (1996), is essentially a "red herring." Given that the EPI sample was drawn from areas where one or two franchisees (or a corporation) owned all or most of the restaurants (and thus was nonrandom), the greater uniformity of employment changes in the EPI sample should not be surprising, and the statistical tests performed by Schmitt—regarding whether the two subsamples are random samples of the same population—are uninformative. This point is reinforced by the fact that the representation of chains in the two samples is very different, because the data on all company-owned restaurants in a chain arrived together. Thus, for example, the data collected by the EPI include no Roy Rogers company-owned restaurants, whereas these constitute 99 of the 154 restaurants in the data we collected.

The other issue Schmitt discusses is the evolution of estimates through the early drafts of this study. This also seems to us to be a nonstarter. Although the earliest draft received a lot of attention because of the minimum-wage debate at the time, it was a preliminary version (and so labeled), and the estimates have changed because of corrections and additions to the data, changes in the definition of variables, and more generally responses to comments and advice from colleagues.

Given the possible nonrandom nature of the two separate sets of data, we also estimated the basic models including an EPI dummy. In the change-in-levels specification, we get an estimated coefficient on the New Jersey dummy of -0.62, with a standard error of 0.41; the coefficient (standard error) on the EPI dummy is 0.55 (0.54). In the proportional-change specification, the estimated coefficient of the New Jersey dummy is -0.035 (0.030), and that of the EPI dummy is 0.029 (0.039).

TABLE 8—CARD/KRUEGER DATA VS. PAYROLL DATA, DIFFERENCE-IN-DIFFERENCES ESTIMATES OF MINIMUM-WAGE EFFECTS
ON NONMANAGEMENT EMPLOYMENT, SUBSAMPLE WITH COMPLETE DATA IN THE PAYROLL DATA

	Estimate of minimum-wage effect on relative employment in New Jersey:		Implied elasticity (3)	Number of observations (4)	Adjusted R^2 (5)
	New Jersey dummy, no controls (1)	New Jersey dummy, controls for chain and ownership (2)			
Card/Krueger data:	4.81	4.43	1.23	84	0.085
	(1.86)	(2.13)			
Payroll data:	−0.58	−0.72	−0.23	133	0.433
	(0.54)	(0.45)			

Notes: The elasticities are computed at the sample means, using the regression estimates in column (2). Only the closed restaurants common to both samples are included in this table.

universe of CK's data, we have eliminated any biases associated with differential response patterns across zip-code areas, chains, or ownership type. Indeed, as noted in Section II, we have sampled from the subset of restaurants in CK's data for which the estimates show larger employment gains resulting from New Jersey's minimum-wage increase than in their total sample.

However, it is still possible that there is nonrandom representation of respondents within zip-code/chain/ownership groups. To address this concern, we first compare results from the payroll data and CK's data for the cells in which the payroll data include information on all restaurants in the cell, so that the restaurants surveyed by CK are also included in the payroll data. For these zip-code/chain/ownership groups, we can think of CK's data as representing a random sample of restaurants, and our data as providing data on the complete universe. Thus, any deviations in these subsamples of the two data sources cannot be attributed to response bias in the payroll data.[35]

The results are reported in Table 8. They indicate that in the CK data represented in the complete data subsample of the payroll data, the estimated minimum-wage effect for New Jersey remains positive and statistically significant; the regression estimate of relative employment growth in New Jersey (with controls) is 4.43. Turning to the payroll data, the regression estimate with controls is −0.72. This evidence indicates that nonrepresentativeness of respondents in our sample within zip-code/chain/ownership groups is not the source of the differences between our results and those obtained by CK.[36]

Another way to address the nonresponse issue is to ask how different the employment changes would have to be in the restaurants for which there are no payroll data in order to generate CK's result. There is no precise way to answer this question, because our data are not a strict subset of CK's sample. However, as we pointed out in Table 1, there is no indication from CK's data that we sampled from zip-code/chain/ownership cells in which relative employment growth in New Jersey was lower. In fact, according to CK's data, employment growth in the nonsampled universe was actually lower in New Jersey and higher in Pennsylvania than in the sampled universe, suggesting that, if anything, the sample we drew understates the relative employment losses in New Jersey.

[35] This subset of our data may still include restaurants that were most motivated to participate, but because the same restaurants are represented in the corresponding universe from which CK's subset is drawn, the differences between the estimates using our data and those using CK's data cannot be attributed to this nonrandomness.

[36] The payroll sample in Table 8 excludes two restaurants that closed but that did not appear in CK's data, in order to make the two samples as consistent with each other as possible for this comparison. However, the same qualitative conclusion holds if we include all closed restaurants, although the estimated minimum-wage effect for the payroll sample is then smaller.

With respect to concerns about response bias within zip-code/chain/ownership cells, the best we can do is to show how large the nonresponse bias would have to be under a given set of assumptions to generate CK's results. For example, if we assume that the sample frame listed in Table 1 in CK's article is the appropriate universe for the payroll data, we can compute this potential nonresponse bias under the further assumption that there was no response bias in CK's survey.[37] Using this method implies that nonrespondents to the payroll survey in New Jersey would have had to report an employment change of 0.8 FTE's to make the payroll data for that state match CK's data;[38] this size response bias seems possible and is well within the standard deviation of employment changes in the payroll survey. In contrast, restaurants in Pennsylvania that were not included in the payroll data would have had to report an employment decline of 9.0 FTE's in order to make the average employment change in the payroll data match CK's data.[39] Putting these together indicates that, in order for the minimum-wage effect in the payroll data to match CK's results, the restaurants not included in the payroll data would have had to have shown a relative employment increase in New Jersey of nearly 10 FTE's in response to the minimum-wage increase. Such a large overall response bias, in our view, is so implausible that the claim that nonrepresentativeness of our data—which CK emphasize in their Reply (Card and Krueger, 2000) to this Comment— explains the differences between the results from the payroll data and their telephone survey seems quite dubious to us, although it could account for a nonnegligible share of the considerably smaller differences between the relative employment growth estimates in the payroll

data and in the ES-202 data that CK analyze in their Reply.[40,41]

Summary.—Summing up, we cannot point to any compelling reason to expect systematically different estimates of employment growth in

---

[37] CK's table puts the sample frame size at 364 restaurants in New Jersey and 109 in eastern Pennsylvania.

[38] This is the solution to $0.5 = 0.1 \cdot (159/364) + g \cdot (205/364)$, where g is employment change in the New Jersey restaurants not included in the payroll data, and 0.1 is the mean employment change in New Jersey in the payroll data. (See Table 3.)

[39] This is the solution to $-2.5 = 1.0 \cdot (71/109) + h \cdot (38/109)$, where h is the mean employment change in the Pennsylvania restaurants not included in the payroll data, and 1.0 is the mean employment change in Pennsylvania in the payroll data. (See Table 3.)

[40] Of course, these latter differences could also be due to the representation of different chains in CK's ES-202 data set used in their Reply (Card and Krueger, 2000). CK note that for confidentiality reasons they are unable to confirm that they used exactly the four chains from which the data were collected in their original study, and their text suggests that they have included additional chains.

[41] In their Reply (Card and Krueger, 2000), CK also argue that our sample is not representative because it has a disproportionately high fraction of Pennsylvania restaurants that reported their data in biweekly periods, "leading to a faster measured employment growth in that state" (p. 1398). However, there is no factual basis of which we are aware for asserting that this fraction is disproportionate relative to the sampled universe (and hence no basis for the claim that the higher fraction in Pennsylvania than in New Jersey leads to "overstatement" of employment growth in Pennsylvania); in particular, CK have no information on the payroll period used by the restaurants in the sampled universe in either their telephone survey or the ES-202 data (which refer only to the payroll period that includes the 12th of the month). Thus, although CK have succeeded in identifying a restaurant-level characteristic that reduces the estimated disemployment effect (like many of the other sensitivity analyses we report), they have not provided any evidence that this characteristic helps to explain the differences between our payroll data and their telephone survey data.

In addition, CK report in their Reply (p. 1411) that the negative estimate of relative employment growth in New Jersey becomes positive if one particular franchisee (with 26 restaurants) is dropped from the sample. They argue that "any conclusion about the growth of average payroll hours in the fast-food industry in New Jersey relative to Pennsylvania hinges on the experiences of this one restaurant operator" (p. 1409). We fail to see why one would expect the data from each individual franchisee (or any other narrow subset of the data) to be "representative." Moreover, they have inexplicably based this conclusion on a regression excluding the controls for chain and ownership. When these controls are included, the contrast is much less sharp, with the estimate changing from -0.66 in the full sample to -0.37. Finally, one can just as easily identify franchisees or owners that when excluded increase the estimated disemployment effect. For example, excluding one particular Burger King franchisee with 13 restaurants in New Jersey from the sample raises the estimated effect (standard error) to -0.81 (0.42), while dropping all of the Roy Rogers company-owned restaurants raises it to -0.84 (0.55); when both are dropped the point estimate rises to -1.19 (0.59). But without a compelling a priori reason to drop any of these subsets of observations, emphasizing the results with a particular subset of observations dropped is just a data-mining exercise.

New Jersey and Pennsylvania in the two data sources. Rather, we think that there is simply considerable measurement error in CK's data, attributable to the design of their survey. In contrast, our data should accurately reflect the actual quantity of labor employed at each restaurant in each survey period. Of course, if the measurement error in CK's data were random, it would not bias the estimated employment effect, as the measurement error is in the dependent variable. There are two possibilities. One is that the measurement error in CK's data is random, but it is so severe that this large-sample result simply does not hold in the relatively small samples considered. Because the Pennsylvania sample in both data sources is rather small, and much of the difference between the two data sources arises for Pennsylvania, it is entirely possible that random measurement error in CK's data is the culprit.[42]

The second possibility is that the measurement error is not random. Although we have no explanation for systematic bias in CK's data, the histograms in Figures 1 and 2 are consistent with both of these possibilities: the variance of employment change in CK's data is considerably greater than that in the payroll data (and other properties of their data are consistent with relatively severe classical measurement error); and the central tendencies of employment change in the two data sources are different, with a particularly strong indication that CK's survey results overstate the employment decline in Pennsylvania.

V. Alternative Evidence

Given the substantive differences between the two data sources in the estimates of mean employment growth in New Jersey and Pennsylvania over the February-to-November 1992 period, it would be useful to compare these estimates with either sample-based estimates or administrative records from standard government data-collection programs. The most obvious source of data in this regard is the BLS

series on establishment employment. Unfortunately, however, the BLS does not produce employment counts specifically for fast-food restaurants, but instead provides numbers on employment in all eating and drinking places (SIC 58); these data are available at the national, state, and county level.[43]

The BLS data on employment growth in eating and drinking establishments from February–March to November–December 1992 (in each case averaging over the two months) are shown in Table 9, along with similar figures for the preceding and following years and the comparison figures on the percentage changes in employment from CK's data and the payroll data.[44] The first set of columns in panel A presents state-level data separately for New Jersey and Pennsylvania from the BLS-790 program. The second set of columns shows the same data from the ES-202 program, while the third set of columns includes only those Pennsylvania counties covered by CK's data.[45] Technically, both the BLS-790 data and the ES-202 data are based on unemployment-insurance tax records through the first quarter of 1995. However, there are differences between the two data sources that reflect technical adjustments to the BLS-790 data to make them more comparable over time. In this respect, the BLS does not consider the ES-202 data to provide valid time-series data, so the BLS-790 data on employment

---

[42] Of course, if the measurement error in CK's data is random, then the evidence of positive employment effects in their data would be even stronger in the absence of measurement error, because the reported standard errors are inflated because of the measurement error.

[43] Although monthly data exist for each of these geographic levels of disaggregation, the BLS only publishes employment data on eating and drinking places at the national level. State data from the BLS-790 program are available via the Internet, while state and county data from the ES-202 program are available upon request from the BLS.

[44] The results in this section are qualitatively similar using the change from February to November, the months from which most of CK's data and the payroll data come.

[45] Because CK's data set did not indicate the county in which each restaurant was located, we could not use that information to define the sample frame for the payroll data. More recently (and in their Reply [Card and Krueger, 2000] to our paper), they indicated that the Pennsylvania restaurants in their sample came from 7 counties: Bucks, Chester, Lackawanna, Lehigh, Luzerne, Montgomery, and Northhampton. However, in their Reply CK also report results adding 7 additional Pennsylvania counties, including the city of Philadelphia, to their sample. We would note that in the regression estimates with the establishment-level ES-202 data that they report in Tables 2 and 3 of their Reply, it is only when these 7 additional counties are added that the few marginally significant (and 2 more significant) positive employment effects arise.

TABLE 9—ALTERNATIVE ESTIMATES OF EMPLOYMENT GROWTH IN EATING AND DRINKING PLACES, PERCENTAGE CHANGES

A. *BLS Data*

Year	BLS-790, February–March to November–December, total state			ES-202, February–March to November–December, total state			ES-202, February–March to November–December, total New Jersey, and 7 Pennsylvania counties in Card/Krueger data		
	New Jersey	Pennsylvania	Difference	New Jersey	Pennsylvania	Difference	New Jersey	Pennsylvania	Difference
1991	5.2	2.7	2.5	3.8	3.0	0.8	3.8	2.7	1.1
1992	6.0	4.9	1.1	5.1	5.1	0.0	5.1	4.8	0.3
1993	6.6	4.3	2.3	6.8	4.6	2.2	6.8	6.1	0.7

B. *Card/Krueger Data*

	Card/Krueger data, total employment			Card/Krueger data, nonmanagement employment			Card/Krueger data, nonmanagement employment, sampled universe		
	New Jersey	Pennsylvania	Difference	New Jersey	Pennsylvania	Difference	New Jersey	Pennsylvania	Difference
1992 FTE's	2.3	−9.8	12.1	2.7	−12.4	15.1	3.3	−13.4	16.7
1992 employees	0.6	−6.7	7.3	0.8	−7.5	8.3	0.6	−8.1	8.7

C. *Payroll Data, Using Averages*

	Payroll data		
	New Jersey	Pennsylvania	Difference
1992 FTE's	−0.5	5.1	−5.6

Note: Percentage employment growth across all establishments is reported.

growth might be regarded as more reliable for this analysis.

Focusing on 1992, there are two things of note. First, the difference between employment growth in New Jersey and Pennsylvania is quite small over the February–March to November–December period, and in two of the three data sources, New Jersey shows slightly faster growth than Pennsylvania—a fact that, at first glance, might be taken to be more consistent with CK's data than with the payroll data.[46] However, the second point to note is that in neither state are the employment growth figures in the BLS data close to those in CK's data (reported in panel B), or in the payroll data

(reported in panel C). Most strikingly, the BLS data do not show the sharp employment decline in Pennsylvania that is exhibited in CK's data; this decline in Pennsylvania is the most noticeable difference between CK's data and the payroll data. These differences between the BLS data and the fast-food data suggest that movements (which may be seasonal) in the non-fast-food component of the eating and drinking SIC dominate those in the fast-food component.[47] This, in turn, suggests that the data on eating and drinking places are not particularly informative with regard to the reliability of either CK's data or the payroll data.[48]

---

[46] If we instead use data on December-to-December employment changes, which would eliminate seasonal differences between the fast-food and non-fast-food components of the restaurant industry, the New Jersey-Pennsylvania employment growth differences over 1992 are −0.7 for the BLS-790 data, 0.1 for the ES-202 total state data, and 0.9 for the ES-202 data restricting the Pennsylvania data to the counties covered by CK's data. Thus, in this case the more reliable BLS-790 data indicate slower employment growth in New Jersey.

[47] According to the 1992 Census of Retail Trade, establishments classified as refreshment places where customers order and pay at the counter but with inside seating (of which fast-food restaurants are only a part) comprise about 20 percent of all eating and drinking establishments in New Jersey and Pennsylvania.

[48] At an early stage of this research, we explored the possibility of matching the establishment-level ES-202 data with CK's establishments to compare employment levels and changes (the ES-202 data have no information on hours). However, our request for access to these data for this

Nonetheless, the BLS employment data for eating and drinking places are of independent interest because they provide an alternative estimate of minimum-wage effects for an industry where such effects might be sizable. A better test than a simple comparison of employment growth for one year is one that allows for state-specific differences in employment growth. CK's statistical experiment, by focusing on employment growth over a single period, precludes an analysis of this type, a point emphasized by Daniel S. Hamermesh (1995). A simple version of this test comes from comparing the difference in employment growth between New Jersey and Pennsylvania in 1992 with the differences in the surrounding years. In particular, for each of the three data series in panel A of Table 9, the differential between New Jersey and Pennsylvania employment growth was lower in 1992 than in either 1991 or 1993, suggesting that New Jersey's increase in the minimum wage slowed relative employment growth in this sector.[49] The more basic point is that because employment growth in New Jersey was higher than in Pennsylvania in 1991 (and in 1993), CK's experiment of comparing employment growth in the two states in 1992 may not be a particularly good one, since the treatment group (New Jersey) and control group (Pennsylvania) obviously have other differences apart from the imposition of the minimum-wage increase in the treatment group.

We also subjected the BLS data to a more formal test by regressing the annual percent change in employment in each state from 1982 to 1996 on the percent change in the minimum-wage level in each state/year. These results are shown in Table 10. In the first two columns of the top panel, we use annual observations on the February–March to November–December employment change in each state as measured by the BLS-790 data. In both cases, the estimated coefficient on the minimum-wage variable indicates a negative ef-

fect, and the coefficient is statistically significant at the 5-percent level in the specification in which the change in the state unemployment rate is not included as a control variable. In the third and fourth columns, we use December-to-December changes, thereby removing the seasonal variation in the data. In this case, the estimated coefficient on the minimum-wage variable is negative and statistically significant at the 5- or 10-percent level in both specifications, with estimated elasticities close to what we obtain from the fast-food employment data.

In the bottom panel, we repeat the analysis with the ES-202 data, with the results using all of Pennsylvania shown in columns (1) and (2) and the results using the 7 Pennsylvania counties covered by CK's survey shown in columns (3) and (4).[50] The coefficient estimates using total employment changes for each state are quite similar to those in the upper panel, with the coefficient on the minimum-wage variable negative and statistically significant at the 5-percent level in the specification excluding the unemployment rate, and negative and statistically significant at the 10-percent level in the specification including it. The estimated coefficient on the minimum-wage variable is also negative when the Pennsylvania sample is restricted to the 7 counties covered by CK's sample, although in this case, the minimum-wage effect is smaller and is not statistically significant when the unemployment rate control is included in the regression.

Taken as a whole, it is our view that the BLS data on employment at eating and drinking places neither confirm nor reject our findings from the payroll data that the New Jersey minimum-wage increase appears to have reduced fast-food employment in that state. The BLS data do, however, provide complementary

---

purpose was not granted. We understand that new guidelines have been developed under which the establishment-level ES-202 data will be made available to researchers, but that matching on an establishment basis would still not be permitted.

[49] We also find this result when we expand the number of Pennsylvania counties to include all 19 counties for which at least part of the county was in one of the zip-code areas included in the original CK data set and thus in which a restaurant in the payroll data could have been located.

[50] This analysis can be problematic with the ES-202 data, owing to a break in the series in January 1991. In particular, a change in reporting methods by payroll processing firms led to a large downward correction to the universe employment counts for establishments using such firms. Historical adjustments were made to the BLS-790 data to account for this change; however, no historical revisions were made to the ES-202 data (American Statistical Association, 1993). This has no effect on the February–March to November–December changes, but will affect the change from December 1990 to December 1991. For this reason, we present only estimates for the February–March to November–December changes.

TABLE 10—REGRESSION ESTIMATES OF MINIMUM-WAGE EFFECTS ON EMPLOYMENT GROWTH
RATE, EATING AND DRINKING PLACES IN NEW JERSEY AND PENNSYLVANIA

A. *Using BLS-790 Data*

Variable	Percent change February–March to November–December		Percent change December to December	
	(1)	(2)	(3)	(4)
Minimum wage (percent	−0.18	−0.15	−0.21	−0.15
change)	(0.09)	(0.10)	(0.07)	(0.08)
New Jersey	−0.08	−0.05	−0.08	−0.04
	(0.94)	(0.95)	(0.79)	(0.77)
Change in unemployment rate	—	−0.32	—	−0.55
		(0.47)		(0.37)
Constant	5.77	5.62	2.58	2.33
	(0.70)	(0.74)	(0.59)	(0.60)
R^2	0.13	0.15	0.23	0.29

B. *Using ES-202 Data*

	Percent change February–March to November–December, total state		Percent change February–March to November–December, New Jersey and 7 Pennsylvania counties	
Minimum wage (percent	−0.20	−0.16	−0.16	−0.11
change)	(0.08)	(0.09)	(0.09)	(0.10)
New Jersey	0.01	0.04	0.21	0.25
	(0.81)	(0.81)	(0.96)	(0.96)
Change in unemployment rate	—	−0.43	—	−0.49
		(0.39)		(0.46)
Constant	5.90	5.70	5.57	5.34
	(0.60)	(0.62)	(0.71)	(0.74)
R^2	0.21	0.24	0.10	0.15

Notes: The sample period is 1982–1996. The results were insensitive to correcting for
first-order serial correlation. Standard errors are in parentheses.

evidence that minimum-wage increases reduce
employment in the restaurant industry.

VI. Conclusions

This paper describes our reevaluation of the
findings from Card and Krueger's New Jersey-
Pennsylvania minimum-wage study. Using data
from administrative payroll records for a sample
of fast-food restaurants that most likely overlaps
extensively with CK's sample, our analysis re-
veals two findings. First, the data collected by
CK show much greater variability in employ-
ment change than is observed in the payroll
data, with the patterns of differences consistent
with rather severe measurement error in CK's
data. Second, whereas CK's data imply that the

New Jersey minimum-wage increase led to an
increase in full-time-equivalent employment at
fast-food restaurants in New Jersey relative to
the Pennsylvania control group, the payroll data
indicate that the minimum-wage increase led to
a *decline* in fast-food FTE employment in New
Jersey relative to the Pennsylvania control
group. The direct replication analysis with the
payroll data yields an elasticity of −0.21 to
−0.22, while additional sensitivity analyses
widen the range of estimated elasticities to lie
generally between −0.1 and −0.25.

How should this evidence be interpreted, es-
pecially in light of Card and Krueger's previous
work? Their results using the telephone survey
data always indicate positive effects of mini-
mum wages on labor demand. These estimated

effects are significant for many specifications, although not for all of them.[51] Our reevaluation of the New Jersey-Pennsylvania experiment using the payroll data generally indicates negative effects of minimum wages on labor demand. Again, the estimated effects are significant for some specifications and subsamples, but by no means for all of them. Is it possible to conclude that the two alternative data sets lead to the same conclusion? In our view, this is an untenable position.

The fundamental question, although one not easily subjected to statistical analysis, is whether the qualitative conclusions that Card and Krueger drew from the New Jersey-Pennsylvania minimum-wage experiment would have been much different had their analysis been based on the payroll data, rather than on the telephone survey data. They stated that they found "no evidence that the rise in New Jersey's minimum wage reduced employment at fast-food restaurants in the state," that "the increase in the minimum wage increased employment," and that their findings "are difficult to explain with the standard competitive model" (p. 792). We regard the payroll data as most consistent with the three opposite conclusions.

Having argued that the payroll data provide a qualitatively different answer from CK's survey data, the question that remains on the table is what conclusion we should draw regarding what actually happened to employment in the CK-surveyed fast-food chains as a result of New Jersey's minimum-wage increase. We have not focused on CK's methods of analysis, which have drawn negative reactions from others (e.g., Hamermesh [1995] and Welch [1995]), although our impression of the data presented in CK's Reply (Card and Krueger, 2000) is that they raise some questions about the validity of the assumptions needed to interpret the difference-in-differences estimates as a natural experiment. Nonetheless, even under the premise that the geographic proximity of the samples renders all other things equal, we believe that, in the final analysis, the payroll data

raise serious doubts about the conclusions CK drew from their data, and provide a reasonable basis for concluding that New Jersey's minimum-wage increase reduced fast-food employment in these chains in New Jersey relative to the Pennsylvania control group. Combined with the new evidence from the ES-202 data that CK present in their Reply, we think we can be more decisive in concluding that New Jersey's minimum-wage increase did not *raise* fast-food employment in that state.

APPENDIX A: THE PAYROLL DATA

This Appendix provides a detailed discussion of the payroll data collected from fast-food restaurants in New Jersey and Pennsylvania. The data were provided by franchise owners and corporate administrators in zip codes and restaurants—distinguished by chain and ownership—represented in CK's data. A subset of the data collected at the beginning of this project was gathered by the EPI, and then forwarded to us. Specifically, the EPI collected data on 72 Burger King and Wendy's franchises, and nine company-owned Wendy's restaurants, in CK's zip-code areas.[52] Subsequently, we took over the data-collection process and gathered all of the remaining data, on an additional 154 restaurants, for a total sample of 235.

The sample drawn by the EPI was from franchisees listed in the *Chain Operators Guide,* as well as from other franchisees or corporations operating in New Jersey or the relevant areas of Pennsylvania for which the EPI was able to gather information. The EPI drew its sample with the simple goal of comparing the payroll data supplied by the franchisees with CK's data. According to the EPI's Executive Director, Richard Berman, the motivation for doing so came from inspection of data provided to them by CK and curiosity regarding the sometimes large employment changes apparent for restaurants in CK's data along with the vagueness of the employment questions in CK's survey; importantly, the impetus for the study did not come from any restaurant owner either offering data or encouraging the EPI

---

[51] In their paper, 39 percent of the estimates are significantly greater than zero at the 5-percent significance level, and an additional 22 percent are significantly greater than zero at significance levels between 5 and 10 percent.

[52] We originally analyzed the earliest data collected by the EPI (which did not yet include the company-owned Wendy's restaurants) in a draft written in March 1995 (Neumark and Wascher, 1995).

to solicit data.[53] The EPI first collected data from franchises in New Jersey, and subsequently from franchises in Pennsylvania. They requested payroll data on hours worked by nonmanagement employees for the pay periods spanning the dates for each wave of CK's survey; these data were supplied on a weekly, biweekly, or monthly basis depending on the employer, and in the former two cases sometimes for more than one payroll period between the relevant dates. CK's data set does not include a unique restaurant identifier (such as an address), and so the EPI was not able to match up and compare data for individual restaurants. However, the CK data set does include the first three digits of the zip code (the zip-code sectional area) in which each surveyed restaurant is located, enabling some matching of units by location. The EPI attempted to identify some zip-code areas in which data could be obtained from all franchised restaurants in a chain, in order to obtain a set of zip-code areas in which all of the restaurants included in CK's data set also appeared in the payroll data set. This was based on information supplied by franchisees, sometimes relating to franchise agreements that might specify, for example, that one or two franchisees had all franchises in the region.[54] The "complete data" subsample analyzed separately in some parts of the paper consists of data on franchised outlets in zip-code/chain pairs in which we have all restaurants; these data, which were all collected by the EPI, cover Burger King franchises in both states, and Wendy's franchises in New Jersey. This subsample also includes data on Wendy's and Roy Rogers company-owned restaurants—the former collected by the EPI, and the latter by us.

In order to collect data on all of the types of restaurants represented in CK's data, even when we could not get all restaurants in a chain/ownership/zip-code cell, we conducted a supplementary data-collection effort relying solely on the *Chain Operators Guide*. In particular, we

identified franchisees that had restaurants in any of the four chains covered in CK's data and that were headquartered in Pennsylvania, New Jersey, or New York; while in principle a franchisee located in any state could have outlets in' New Jersey or Pennsylvania, it was impractical to survey franchisees in all other states, and we assumed that we would cover most outlets by surveying these three states. We initially faxed a letter to each franchisee. Following the questions the EPI used initially, we requested payroll data for the pay periods spanning the dates for each wave of CK's survey.[55] The letter mentioned the EPI as well as CK's study, since some preliminary conversations with restaurant owners indicated that most knew about both the EPI and the "New Jersey-Pennsylvania study" or the "Princeton study." We felt that mentioning the EPI would maximize the likelihood that franchisees would participate, although we recognize that it has raised the potential problem of response bias, an issue we address in the main body of the paper. Since many owners seemed familiar with this debate, however, there was no way to request the data in a vacuum. Finally, of course, we wanted to maintain comparability with the original data collected by the EPI. Nonetheless, we acknowledge that data collected from an external source would be desirable, assuming that such data were of sufficient quality. A sample of the initial letter, which came from the first author on Michigan State stationery, is attached as Appendix B.

We used a strategy—chosen at the outset—of making at least four attempts to elicit data from each franchisee, including a telephone follow-up to each fax, and additional telephone calls and faxed letters, unless the franchisee clearly indicated that they would not supply the data or informed us that they had no outlets in the zip-code areas (in Pennsylvania) covered in CK's data. In the initial letter, we asked the respondents to supply us with data for as many payroll periods as possible in February and November of 1992, especially including the

---

[53] Personal communication, December 2, 1997.

[54] When we collected the additional data, we also checked that no other franchisee reported restaurants in a chain and zip-code area for which we had been told we had complete coverage of restaurants. In only two cases did a contradiction arise (for Burger King franchises in two Pennsylvania zip-code areas), and we removed these two zip-code/chain pairs from the subsample classified as having complete data.

[55] The data-collection effort focused on current franchisees in the zip-code area, who were asked to supply data on all restaurants in operation at the time of CK's study (as were the corporations). Below, we discuss information on franchisees that may have been operating at the time of CK's study but subsequently closed.

last payroll period in each month, if the payroll period was other than monthly. (February covers most of the period spanned by the first wave of CK's survey, and most of their data in the second wave come from November.) Because we cannot uniquely identify the restaurants in CK's data set, this is the closest we can come to matching up the time frame covered in the two data sources. The hours data were converted to a weekly basis, and divided by 35 to obtain a measure of full-time-equivalent (FTE) employees. This parallels CK's procedure of obtaining data on numbers of full-time and part-time employees, and weighting part-time workers by one-half to estimate FTE employment. We requested data on hours rather than number of employees because initial explorations with franchise owners revealed that hours data were easier to obtain.[56] In the main body of the paper, we discuss issues arising from the possible noncomparability of the two measures of employment.

Overall, the two data-collection efforts resulted in 45 franchisees, of which 25 responded, with 17 supplying data and eight indicating they had no outlets in the zip-code areas in which we were interested. Of the remainder, nine declined to supply data, and 11 did not return repeated messages or respond to faxes.[57] The payroll administrator at each of the parent corporations was also contacted, to attempt to obtain data on company-owned restaurants. Roy Rogers and Wendy's supplied data, while Burger King and KFC declined to do so.

In the final analysis, the two data-collection efforts yielded data on either all franchised restaurants or all company-owned restaurants in a chain for 24 zip-code/chain/ownership combinations (7 in Pennsylvania and 17 in New Jersey) and data on some restaurants for an additional 25 zip-code/chain/ownership combinations, for a total of 235 observations.[58] Ex-

cept for the Roy Rogers company-owned restaurants, the data in the complete data subsample were collected by the EPI; almost all of the rest of the data were collected by us. With the exception of the observations collected by the EPI on franchisees not in the *Chain Operators Guide,* this sample is one drawn from the universe of company-owned restaurants and franchisees that appear in the *Guide.* This raises a few issues.

The first concerns the universe of franchisees included in the *Chain Operators Guide.* Information obtained from the publisher of this guide indicates that while it may not attain universal coverage of all chain restaurants, coverage is likely very high, although franchisees must have at least three units to be included.[59] In addition, while listings in restaurant associations are one source of information for the *Guide,* inclusion in the guide is not specifically related to membership in a lobbying organization, payment of any fees, etc. Based on this information, we see no reason to believe that the sample drawn from the *Chain Operators Guide* introduces any particularly important biases, especially as regards differences between restaurants in New Jersey and Pennsylvania.

The second issue is the influence on our re-

---

[56] Conversations with franchise owners and controllers indicated numerous reasons to maintain hours records, including: the ability to document hours worked by individuals to resolve pay disputes; tracking overtime; documenting compliance with hours restrictions on minors; and monitoring eligibility for pensions.

[57] The reason offered for refusals was almost always that retrieving the data was too much work.

[58] The individuals who supplied the data on company-owned restaurants indicated that we received data on all

units from the corresponding chains. This is consistent with evidence reported in the paper that we have data on more company-owned restaurants in these chains than do CK.

[59] We provide the following quotations from a letter from the Associate Editor of the *Guide*, dated January 10, 1997.

> Our ultimate goal is 100% coverage of the chain restaurant industry ... . Prospective companies are identified from a variety of sources, including restaurant association membership lists, wholesaler customer lists, industry trade reading, and Internet resources. After determining that a company meets our inclusion criteria, namely operating 3 or more units, we gather as much third-party information as is available, and a questionnaire is generated for our research staff. The questionnaire probes for information specific to the company's operation (square footage, projected expansion, menu types, services offered). Our researchers, via phone or mail, then contact the company to verify and augment the information on file. Once we have a complete profile of the company, the information is released into our database, where it becomes a listing in our directory. Companies are then contacted annually to update their listing.

TABLE A1—RESPONSE OF FRANCHISEES TO THE SURVEY

A. *Response Rates*

	Number of franchisees surveyed	Number responded (response rate)	Number provided data	Number with no units in zip-code area	Number refused	Number did not respond
New Jersey	19	12 (0.63)	12	—	2	5
Pennsylvania	15	9 (0.60)	4	5	3	3
New York	11	4 (0.36)	1	3	4	3

B. *Characteristics of Respondents and Nonrespondents*

					Pennsylvania and New Jersey franchisees:	
	Burger King	Wendy's	Roy Rogers	KFC	Units in single state	Units in multiple states
Respondents supplying data	15	4	1	1	9	7
Nonrespondents/refusals	9	3	3	2	8	5

Notes: Note that some New Jersey franchisees own Pennsylvania restaurants, and vice versa. Here they are classified by the location of the corporate office. Nonrespondents were those who did not return or respond to at least four phone calls or faxes. Some franchisees own restaurants in other states, although we have no information on the distribution of restaurants by state. We therefore report these numbers only for franchisees with corporate offices in New Jersey and Pennsylvania, because we assume that those with offices in New York have most of their restaurants in New York. Numbers in the first four columns of panel B add up to one more than the number supplying data plus the number of refusals/nonresponses, because one franchisee owned restaurants in two chains.

sults of including the franchisees (representing 32 restaurants) in New Jersey that were not listed in the *Chain Operators Guide* as headquartered in Pennsylvania, New Jersey, or New York, but were identified by the EPI based on other information (to obtain zip-code areas with complete data with which to compare CK's data). Because we do not know the sampling frame for these 32 restaurants, there are valid scientific reasons to exclude them. We explore the consequences of doing so in the main text; briefly, it has essentially no effect on the results.

Third, there is substantial nonresponse from the universe of franchisees listed in the *Chain Operators Guide*. This nonresponse raises potential issues of sample selection bias. Of course, we have no payroll data from the nonrespondents, but in Appendix Table A1 we compare respondents and nonrespondents along a couple of dimensions on which information is available in the *Chain Operators Guide*. Panel A shows that response rates were much higher for those franchisees headquartered in New Jersey and Pennsylvania. Panel B provides information on chains, and whether the franchisee owned units in multiple states (the New York franchisees had to own units in New Jersey or Pennsylvania). Nonresponse was less frequent for Burger King and Wendy's franchisees, while

respondents differed little from nonrespondents in terms of whether they owned restaurants in multiple states. In the main body of the paper, we provide additional information on potential response bias by comparing CK's data for the chain/ownership/zip-code area cells for which we did and did not receive payroll data. We also present analyses indicating that response bias is very unlikely to be driving our results.

It is possible that some of the nonresponding franchisees had gone out of business by the time of our data-collection effort, which could affect our results. To examine this possibility, we obtained the 1992 and 1993 versions of the *Chain Operators Guide*. For Pennsylvania, only one franchisee disappeared between 1993 and 1995, and this franchisee almost certainly did not have restaurants in the zip-code areas covered in CK's survey.[60] (A second franchisee changed names.) For New Jersey, three franchisees disappeared between 1993 and 1995. This might suggest that, if anything, nonresponse attributable to franchisees going out of business between 1993 and 1995 biases the results against

---

[60] This was a small franchisee based in the center of the state.

finding an employment decline in fast-food res-
taurants in New Jersey. However, this would
require that minimum-wage effects on closings
(of entire franchisees) occur with a lag, as no
franchisees in either state disappeared between
1992 and 1993. In other work, we have found
evidence of lags of about a year in standard
disemployment effects of minimum wages
(Neumark and Wascher, 1992).

In addition to these sampling issues, because
the EPI has a stake in the outcome of the
minimum-wage debate, we felt it important to
assess the validity of the data they collected and
supplied to us. We took three steps to verify the
accuracy of these data. First, we spoke with
each franchise owner who supplied data to the
EPI and confirmed that they provided numbers
from their administrative payroll records. Most
of the franchise owners indicated that their pay-
roll records are computerized and maintained by
ADP, a payroll processing firm, and that they
simply provided information supplied by ADP.
Second, we requested and received signed state-
ments from each franchisee that supplied data to
the EPI (and from Wendy's corporate headquar-
ters) attesting to the veracity of the data that the
EPI supplied to us. These statements included a
transcription of the actual payroll data; in every
case, the numbers exactly matched the data we
received from the EPI. Third, we verified that—
for the zip-code/chain/ownership combinations
for which we have data on all restaurants in the
zip-code area—whenever CK's data indicated a
restaurant closing, the payroll data also indi-
cated a restaurant closing. This was confirmed
for two such closings in CK's data. (CK were
careful to document restaurant closings, so this
is a dimension on which we can use their data to
assess the reliability of our data.) Thus, we do
not have reservations regarding the validity of
the data.

APPENDIX B: SAMPLE LETTER

Dear _____ (*franchise owner*):

I am writing to request data for research I am
conducting in conjunction with the Employ-
ment Policies Institute, a restaurant-supported
lobbying and research organization. In particu-
lar, we are collecting employment data from
fast-food restaurants to reexamine the New
Jersey-Pennsylvania minimum-wage study. If

you can help provide the needed data, I would
be most appreciative.

What I need, precisely, is the number of
nonmanagement payroll hours worked in each
of your _____ (*Pennsylvania, New Jersey, or
both*) restaurants in February and November of
1992. I can use the data in whatever form you
record them—monthly, biweekly, or weekly.
However, if the data are biweekly or weekly, I
need data from the end of each month. I also
need the first three digits of the zip code in
which each restaurant is located.

I am willing to abide by any confidentiality
requirements that you may have. Please call me
if you have any questions.

(*The following paragraph was included for
franchisees with restaurants only in Pennsylva-
nia, and otherwise excluded. The final para-
graph was included in all letters.*)

You may be able to dispense with this request
quite quickly, because I am interested in restau-
rants only in zip codes beginning with _____ (*list
of zip-code areas for corresponding restaurants
in CK's data*). If you have no restaurants in
these zip codes, please let me know, and that
will be all I need.

Finally, if you ultimately determine that you
cannot provide these data, please inform me. An
indication as to the reason you cannot provide
them would be helpful. Again, thank you for
your assistance.

Sincerely,

David Neumark
Professor of Economics

REFERENCES

American Statistical Association. "Report from
ASA Panel on the Bureau of Labor Statistics
Current Employment Survey," 1993.

**Brown, Charles; Gilroy, Curtis and Kohen, An-
drew.** "The Effect of the Minimum Wage on
Employment and Unemployment." *Journal
of Economic Literature*, June 1982, *20*(2), pp.
487–528.

Card, David and Krueger, Alan B. "Minimum
Wages and Employment: A Case Study of the
Fast-Food Industry in New Jersey and Penn-
sylvania." *American Economic Review*, Sep-
tember 1994, *84*(4), pp. 772–93.

_____ . Myth and measurement: The new economics of the minimum wage. Princeton, NJ: Princeton University Press, 1995.

_____ . "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Reply." American Economic Review, December 2000, 90(5), pp. 1397–420.

Chain Operators Guide. Various issues.

Davis, Steven J.; Haltiwanger, John C. and Schuh, Scott. Job creation and destruction. Cambridge, MA: MIT Press, 1996.

Hamermesh, Daniel S. "Myth and Measurement: The New Economics of the Minimum Wage: Review Symposium: Comment." Industrial and Labor Relations Review, July 1995, 48(4), pp. 835–38.

Katz, Lawrence F. and Krueger, Alan B. "The Effect of the Minimum Wage on the Fast-Food Industry." Industrial and Labor Relations Review, October 1992, 46(1), pp. 6–21.

Kennan, John. "The Elusive Effects of Minimum Wages." Journal of Economic Literature, December 1995, 33(4), pp. 1950–65.

Leonard, Jonathan S. "In the Wrong Place at the Wrong Time: The Extent of Frictional and Structural Unemployment," in Kevin Lang and Jonathan S. Leonard, eds., Unemployment and the structure of labor markets. New York: Blackwell, 1987, pp. 141–63.

Machin, Stephen and Manning, Alan. "The Effects of Minimum Wages on Wage Dispersion and Employment: Evidence from the U.K. Wages Councils." Industrial and Labor Relations Review, January 1994, 47(2), pp. 319–29.

Neumark, David and Wascher, William. "Employment Effects of Minimum and Subminimum Wages: Panel Data on State Minimum Wage Laws." Industrial and Labor Relations Review, October 1992, 46(1), pp. 55–81.

_____ . "The Effects of New Jersey's Minimum Wage Increase on Fast-Food Employment: A Re-Evaluation Using Payroll Records." Unpublished manuscript, Michigan State University, March 1995.

Schmitt, John. "Cooked to Order." American Prospect, May–June 1996, pp. 82–85.

Welch, Finis. "Myth and Measurement: The New Economics of the Minimum Wage: Review Symposium: Comment." Industrial and Labor Relations Review, July 1995, 48(4), pp. 842–49.