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The Elusive Effects of Minimum Wages*

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The economy is a miserable experimental design.
Robert E. Lucas, Jr.

THE U.S. MINIMUM WAGE is now $4.25 per hour, and Congress is talking about increasing it. If the minimum were to be increased to $25 per hour many workers would lose their jobs. Cause and effect would be obvious, even to the most jaundiced eye. If the minimum is raised instead to just $5.15 per hour (as the President has proposed) the effect will not be obvious, and much research effort will be devoted to uncovering it. If only Congress could be persuaded to randomize the timing of the increase (perhaps by giving each employer or each local labor market a lottery number with lump sum compensation), we might learn something about the employment effects of minimum wages (although there would still be the problem of predicting the market effect of a general change from the effects of local changes). Alas, legislators tend to view their job as being complicated enough already, and past minimum wage legislation has not made any attempt to facilitate policy evaluation. David Card and Alan Krueger's book, *Myth and Measurement: The New Economics of the Minimum Wage* attempts to disentangle the evidence. The main theme of the book is that legislatures have at times inadvertently generated useful "quasi-experimental" data, and the data give no support to the view that minimum wage increases have reduced employment.

Economists like to complain about their undeserved reputation for disagreeing with each other. After all, most of us agree that rent controls and tariffs and minimum wages have undesirable side effects, and we like to think that our differences are less substantial than the public perception. On the other hand, some of us take a more perverse view. The title of Card and Krueger's book suggests that our agreements are based on theoretical myths, to be dispelled by some new economics to be found inside, although neither the myths nor the new economics are ever explicitly identified in the book itself. Perhaps we deserve our reputation after all.

The dominant theme in *Myth and Measurement* is that economists have given too much credence to theoretical predictions about the effects of minimum wage laws, with too little attention to the data. The bulk of the evidence in the book concerns the effects of the two most recent increases in the federal minimum wage, and of two recent in-

* *Myth and Measurement: The New Economics of the Minimum Wage*. By DAVID EDWARD CARD AND ALAN B. KRUEGER Princeton: Princeton University Press, 1995. Pp. x, 422. $29.95. ISBN 0–691–04390.

creases in state minima. The most important chapter considers data for the fast food industry, in relation to the New Jersey minimum wage increase in 1992, and in relation to the impact of the 1991 federal increase on a low-wage state (Texas). Another major chapter examines the increase in the California minimum wage in July 1988, comparing teenage employment and retail trade employment in California with the experience of other states before and after 1988. Without these two chapters there would probably not have been a book, and it seems fair to evaluate the book mainly on the strength of the empirical studies of New Jersey and California and Texas. A sharper characterization is that the book is about increases in employment caused by increases in the minimum wage. This is unfair in that the inside of the book generally presents detailed and careful analyses of the evidence, acknowledging that minimum wages sometimes seem to reduce employment, and often seem to have no discernible effect. Yet this material is surrounded by suggestions that moderate increases in the minimum wage can be granted without fear of negative consequences, and with a reasonable hope that employment will actually be increased.

At the same time it should be said that *Myth and Measurement* contains much informative material that is not limited to the employment effects of minimum wages. There is a chapter on how changes in the minimum shift the distribution of wages, for example raising pay for workers who were already above the minimum. Time-series studies of teenage employment rates are reviewed, and to some extent debunked. The conclusions of these studies are regarded by Card and Krueger as the conventional wisdom, in the usual pejorative sense—more on this below. Cross-section and panel data studies based on variations in minimum wages across states and over time are then examined, although hardly from an objective point of view:

> We expect that, as traditional time-series regressions fail to produce statistically significant disemployment effects, interest in cross-section methods in this area will increase further. (p. 208)

A chapter reviewing the international evidence focuses mainly on Puerto Rico, where the minimum wage "really bites," with some discussion of Canada. By this time the reader knows what to expect, and, sure enough, "we find that Puerto Rico's experiences provide surprisingly fragile support for the textbook model" (p. 240). Additional chapters address the question of whether the minimum wage reduces poverty or inequality, and whether increases in the minimum wage are noticed by the stock market. Finally, there is a chapter dealing with the possibility of constructing theoretical models that allow minimum wages to increase employment. By this point the evidence has already been thoroughly analyzed without much apparent need for theory, so this last chapter seems like having an architect work out plans for a building that is already standing by itself.

In a book of this size (over 400 pages) one would like to see a chapter tracing the history of economists' attempts to measure the effects of minimum wages. Instead, the book contains only casual and sometimes misleading references. For example,

> The idea of using natural experiments is hardly new in economics. Indeed, the earliest research, by Richard Lester (1946) and others, used that approach. Nevertheless, it is controversial—perhaps because studies based on the natural experiment approach often seem to overturn the "conventional wisdom." (p. 21)

This begs the question of how the wisdom became conventional in the first place. Moreover, the contribution of Lester (to whom the book is dedicated) is overstated. A more representative list of natural experiment studies would include work by Marie L. Obenauer and Bertha von der Nienburg (1915), John F. Maloney (1942), and John M. Peterson (1957, 1959), and these studies did not overturn the conventional wisdom (perhaps that is why Card and Krueger did not mention them).

What Did We Know Before?

The theoretical prediction that minimum wages reduce employment is simple and general. For concreteness, think of a profit-maximizing fast food place employing 20 workers

TABLE 1
EMPLOYMENT CHANGES IN PORTLAND RETAIL STORES, 1913–1914

	Men	Girls (Age 16–18)	Ratio (Girls/Men)	Women (Age > 18)	Ratio (Women/Men)	Women Age Unknown
Before (Mar/Ap 1913)	940	138	.1468	1543	1.641	152
After Mar/Ap 1914)	868	160	.1843	1327	1.529	59
Change	–72	22	.0375	–216	–0.113	–93
% Change	–7.7%	15.9%	23.6%	–14%	–6.3%	–61.2%

Source: Obenauer and Nienburg (1915, Table 3, pages 14–15).

at the minimum wage of $4.25 per hour, making (maximal) profits at the rate of $30 per hour. Suppose the minimum wage rises to $5.05 per hour (with no effect on other prices, or on the demand for fast food). If this increase is granted, and nothing else changes, profits must fall to $14 per hour. Let L^* be the number of workers employed after all profit-maximizing adjustments have been made, and note that the new profit rate must be at least $14. Now if L^* is above 20, the profit from hiring L^* at the old $4.25 wage would be above $30. So L^* can't be above 20, and that's that. This argument has nothing to do with what the production function is, or whether the firm has some monopoly power, or any other details of the firm's operations. The only assumption is that the availability of L^* workers at the higher wage implies the availability of L^* workers at the lower wage (so the argument breaks down if the firm has some monopsony power).

There is a long history of empirical studies attempting to pin down the effects of minimum wages, with limited success. The emphasis in this literature has shifted from detailed case studies to regression methods applied to time-series and cross-section data. Card and Krueger eschew the complicated econometric procedures used in recent work in favor of the simple comparisons used in the earliest studies. Where one state legislates higher wages than its peers, for example, one can compare employment before and after the change in the affected state, with unaffected states serving as a "control group." Similarly, changes relative to a control group can be analyzed when a minimum wage law applies only to a particular demographic group, or when some workers are already paid more than the new statutory minimum, so that they are not directly affected.

One of the earliest empirical studies of minimum wages was a remarkably detailed Bureau of Labor Statistics report by Obenauer and Nienburg (1915)[1], using payroll and interview data for women and men in Oregon retail stores, before and after a minimum wage for women in Oregon became effective in 1913 and 1914. Under an act of 1913 creating an Industrial Welfare Commission "to protect the lives and health and morals of women and minor workers," a minimum wage of $9.25 a week was established for women in Portland with more than a year of experience in their current job. For inexperienced women, and for girls aged 16–18, the minimum was set at $6 per week. At the same time a maximum of 50 hours per week was established for all women (and the minimum wage for those working less than 50 hours was apparently pro-rated, so that in practice there was an hourly minimum)[2]. Obenauer and Nienburg obtained payroll data directly from the books of 33 retail stores in Portland (and seven more in Salem, where the minimum wage for experienced

[1] Prior to this there were 13 BLS bulletins and reports on minimum wages published between 1896 and 1915, mainly concerned with minimum wages in other countries, especially Australia and New Zealand. See BLS Bulletin 174, September 1915.

[2] The Consumer Price Index can be used to make a rough comparison: an hourly wage of 18.5 cents in 1914 means about $2.71 at 1995 prices.

women was $8.25 per week). The Portland data covered 1546 women and 868 men, with information on wage rates and hours worked for women, and total earnings for men. These data were supplemented by interviewing 443 women individually, in order to obtain detailed information on changes in employment status that might have been attributable to the minimum wage orders.

The main employment effects in the Oregon data are listed in Table 1. This table is a good introduction to Card and Krueger's main empirical results, because the study was based on the idea of a "natural experiment," and because the minimum wage apparently caused employment to rise in some cases. The employment counts for men and women are not commensurate, so instead of using the difference in differences for employment levels, the change in employment ratios can be used to measure the minimum wage effects. By this measure the minimum wage orders reduced adult female employment by 6.3 percent, and increased teenage female employment by 23.6 percent. Obenauer and Nienburg pointed out that the overall decline in employment was due to a general recession: for example, total sales in these Portland stores fell by 8.6 percent over this period. Moreover, they noted that the jobs held by men were less vulnerable to this decline than women, so that the difference in differences estimate does not capture a pure minimum wage effect. They concluded that "Little, if any, of the loss of employment among women as a group can be related to the minimum-wage determinations" (p. 12). Nevertheless there was unmistakable evidence, confirmed in the interviews, that experienced women in the least-skilled positions (such as errand girls) lost their jobs in favor of girls and "apprentices" (women with less than one year's experience) who could be paid $6 instead of $9.25 per week. This effect was explained as follows:

> department-store men do not consider an ordinary bundle wrapper or a stock girl, whatever her experience, to be worth $9.25. To earn $9.25, in the judgment of the employer, she must be put at work requiring more skill.

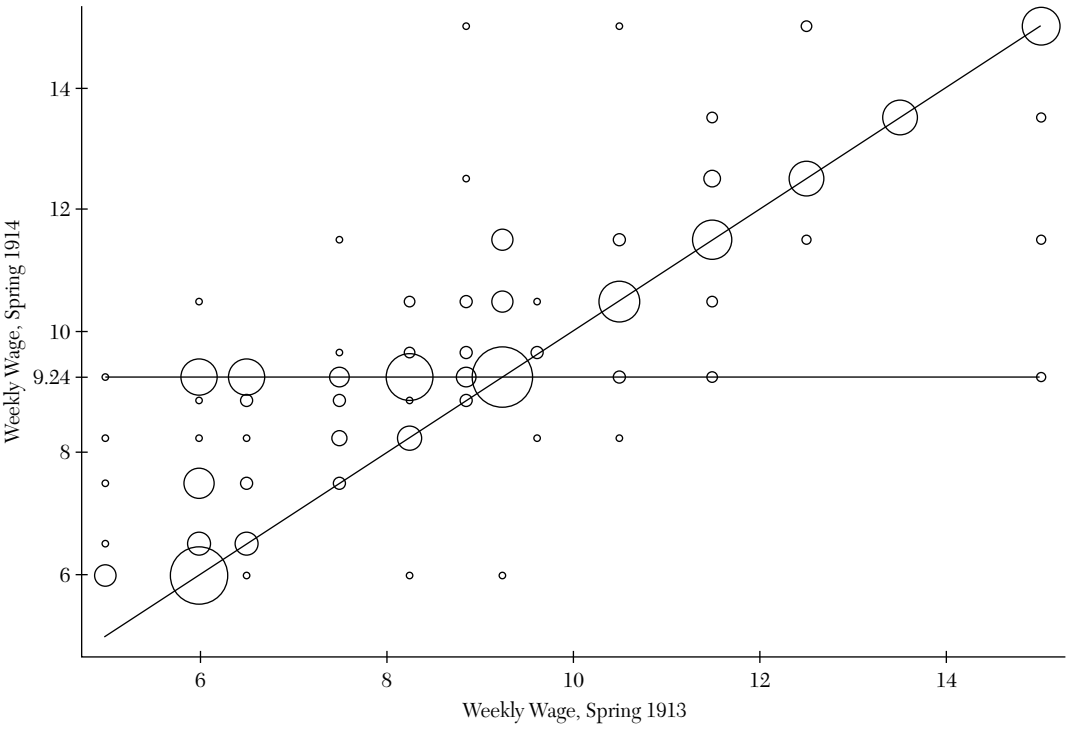

Figure 1. Wage Changes in Oregon, women, 1913–14

> Twenty-three of the women making changes in occupation had . . . gone into better positions. . . . There are some women, however, who have an aversion for certain occupations and others who can not perform more skilled duties. . . . Under the present conditions they will not be retained more than their [one-year] apprenticeship period. (p. 72)

Another interesting feature of the Obenauer and Nienburg study is the detailed tabulation of the joint distribution of (grouped) wage rates before and after the minimum wage orders, for the 374 women who were interviewed and who had been employed in Oregon before 1914. (See Obenauer and Nienburg 1915, Table 32, p. 75.) The "bubble-plot" of this distribution[3] in Figure 1 shows that in most cases (204 of 374) there was no wage change between 1913 and 1914. There were many women who did get wage increases: 73 to the new minimum wage (shown as $9.24), 82 to other levels. The wage fell in only 15 cases.

Subsequent U.S. Department of Labor studies compared power laundries in New York State, where a minimum wage for women became effective in 1934, with a "control group" from Pennsylvania, and compared dry cleaners in Ohio with a control group from Indiana. Early studies of the impact of the Fair Labor Standards Act establishing the federal minimum wage in 1938, and the subsequent amendments raising the minimum to 30 cents in 1939, 75 cents in 1950, and $1 in 1956, were based on comparisons of covered and uncovered workers, or high-wage and low-wage workers.

These studies (as well as the work of Obenauer and Nienburg) were reviewed by Peterson (1959), who acknowledged a "generally accepted conclusion" that minimum wage laws had no appreciable effect on employment, but complained that the conclusion was due to "a tendency to interpret the facts incorrectly." In his view, "a closer reading of the data . . . reveals . . . the results expected on the assumption of a negatively sloped demand curve for labor."

Peterson made a reasonable case for negative employment effects, but admitted that other reasonable interpretations are possible, basically because the data do not come from a controlled experiment. Lester (1960), considering the same body of evidence, concluded that almost anything could happen to employment following a moderate increase in the minimum wage. In other words, the data are not decisive, and strong prior beliefs are needed in order to reach a conclusion. For instance, data for the seamless hosiery industry showed a regular pattern in relation to the large increase in the federal minimum wage in 1950 (from 40 cents to 75 cents per hour). Comparing the last three months of 1949 with the period from February to June 1950, after the increase, it was found that man-hours fell more in plants where the wage was initially below 75 cents than in plants paying above 75 cents. On the other hand, Peterson and Lester both pointed out that there was a trend toward mechanization in the industry around this time, and that the high-wage plants were at a more advanced stage in this process.

Time-Series Studies

The case-study or "natural experiment" method used in the early minimum wage studies was later abandoned in favor of regression models using time-series data on (mostly teenage) employment. A widely quoted review reached the following conclusion:

> In summary, our survey indicates a reduction of between one and three percent in teenage employment as a result of a 10 percent increase in the federal minimum wage. We regard the lower part of this range as most plausible because this is what most studies, which include the experience of the 1970s and deal carefully with minimum-wage coverage, tend to find. (Charles Brown, Curtis Gilroy, and Andrew Kohen 1982, p. 508)

[3] This uses the grouped data. The groups mostly use $1 ranges, and the plot uses the midpoint of the range. The minimum wage group is given as $9.23–$9.25, because some jobs paying $9.23 were judged to be in compliance with the law. Finer details of the distribution are available in the original: there is an itemization for each of 440 women interviewed, showing wages in 1913 and 1914 for each woman, together with age, experience, job description, hours worked, reason for any change in employment, and some other information.

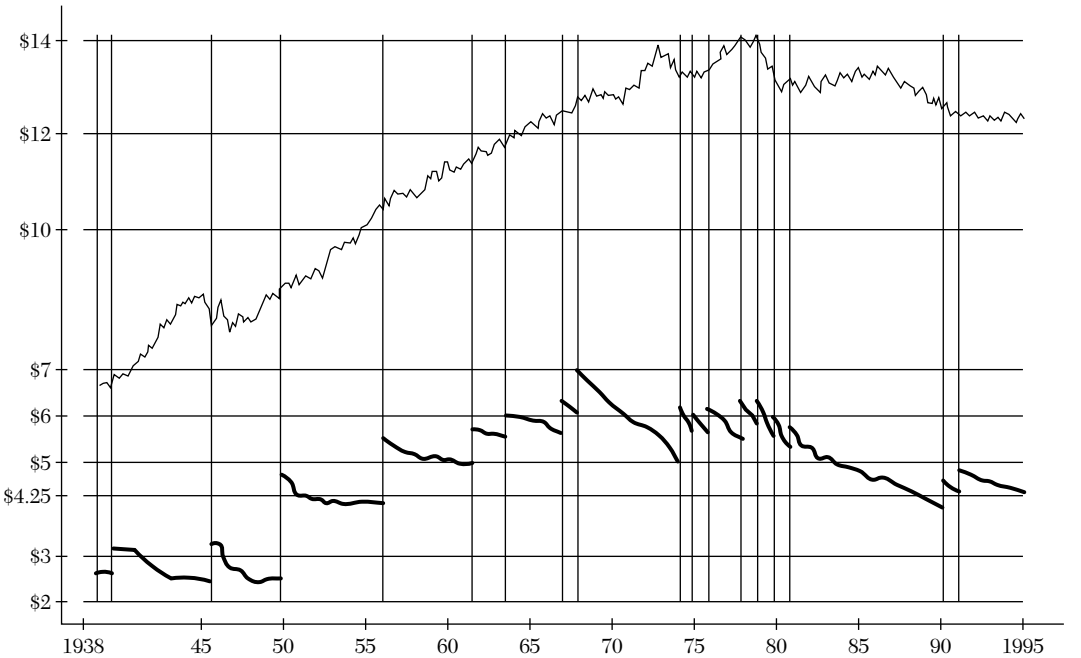

Figure 2. U.S. Real Minimum Wages and Average Wages of Manufacturing Production Workers, $1995

This statement, which is treated as the "conventional wisdom" in *Myth and Measurement*, is based on a tabulation of many overlapping studies, by various authors, using various specifications, on different but closely related data sets. The summary conclusion is unconvincing. There seems to be an implicit belief that an average of the estimates from many such studies must mean something. But in fact if there is one impeccable study in the set, and if the results of this study are inconclusive, what is gained by tossing in the results of the other studies and taking an average? What if all of the studies are impeccable, and they are all inconclusive?

The data underlying the time-series estimates are illustrated in Figures 2 and 3. Figure 2 shows the monthly history of the real (CPI-deflated) minimum wage, in relation to real average hourly earnings of production workers in manufacturing. The vertical lines show the dates on which increases in the nominal minimum were implemented, and the downward drift between each pair of lines shows the declining value of a dollar. From the point of view of experimental design, the picture looks promising, particularly in the earlier years. The usual problem in analyzing the effects of one economic time series on another is that everything changes too gradually, but here we have substantial discrete jumps in the explanatory variable.

Figure 3 is more sobering. The seasonally adjusted employment rate for teenagers (both sexes, 16 or 17 years old) displays large cyclical swings and a high degree of serial correlation. Among the 17 minimum wage changes shown in Figure 2, the median increase was 12 percent, which would produce at most a three percent reduction in the teenage employment rate, if the conventional summary is accepted. That means we are looking for employment rate changes of about one percentage point, and such changes happen all the time, even from one month to the next. In short, we are looking for a needle in a haystack.

Yet the claim in the literature is that, with enough effort, the needle can be found. Some illustrative time-series regressions are

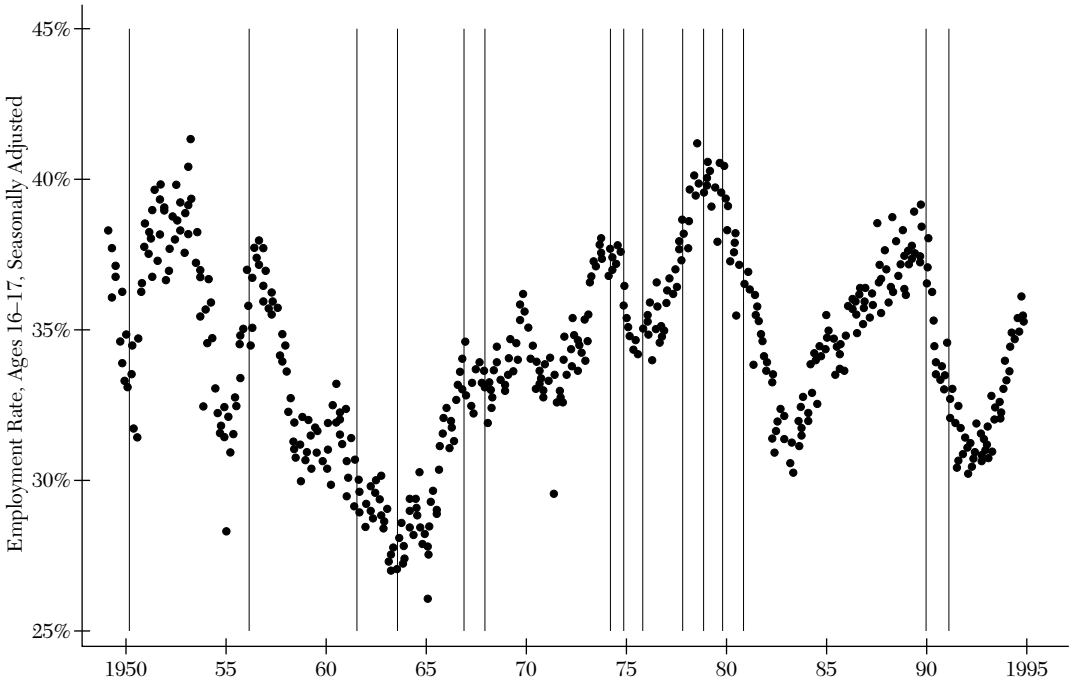

Figure 3. U.S. Teenage Employment Rate (ages 16 and 17), with Dates of Minimum Wage Changes

shown in Table 2. These indicate that the minimum wage has a tiny negative effect on teenage employment, which can be made to seem precise by jiggling the specification and the sample period. But this kind of estimate is surely not reliable—it rests on heroic aggregation assumptions, some potentially important explanatory variables are left out, and the serial correlation in the employment series is a mystery.

New Jersey and Pennsylvania

When the most recent amendment to the Fair Labor Standards Act was passed in 1989, the New Jersey legislature obligingly designed a natural experiment. On April 1, 1992, the minimum wage in New Jersey increased to $5.05, while other states, including Pennsylvania, accepted the federal minimum of $4.25. Around the beginning of March, Card and Krueger had research assistants telephone about 300 burger and fried chicken joints in New Jersey, and about 100 in eastern Pennsylvania, asking a manager of each restaurant about 25 questions relating to employment, wages, and prices. Then in November and December most of the same restaurants were asked the same questions again. Card and Krueger argue that because the labor turnover rate in the fast food business is high, there was plenty of time for employers to adjust employment levels before the second wave of interviews (and no need to anticipate the new law at the time of the first wave). The 39 missing responses in the second wave of the telephone survey were carefully tracked down, because the lack of response might mean that the establishment had gone out of business. Only six had in fact closed permanently, and these were recorded as having zero employment in the second wave (another four were temporarily closed, and these were treated as having missing employment data). There was no attempt to gather information about hours worked, which is unfortunate, because labor demand theory says only that the number of people working at any given time should fall when the wage rises, and this could easily be con-

TABLE 2
LOGLINEAR TIME SERIES ESTIMATES, AGES 16 AND 17, MONTHLY DATA

	Employment Rate		Employment			
	1949–94	1954–93	1949–94	1949–94	1954–93	1954–93
Real Minimum Wage	–0.0037	–0.0151	–0.0225	–0.0169	–0.0371	–0.0322
SE	*0.0076*	*0.0111*	*0.0108*	*0.0108*	*0.0135*	*0.0136*
Unemployment Rate, men, 35–44	–0.0088	–0.0107	–0.0147	–0.0599	–0.0166	–0.0480
SE	*0.0033*	0.0038	0.0039	0.0128	0.0041	0.0144
Unemployment Rate Lagged				0.0479		0.0332
SE				0.0130		*0.0147*
Population, Ages 16 and 17			1.0181	0.9954	0.8510	0.8490
SE			0.2932	*0.2900*	*0.3572*	*0.3557*
Population lagged			–0.9294	–0.9192	–0.7481	–0.7550
SE			0.2969	0.2936	0.3599	0.3584
Dependent Variable, Lag One	0.5524	0.5998	0.5384	0.5439	0.5749	0.5757
SE	0.0391	0.0428	0.0391	0.0387	0.0431	0.0429
Dependent Variable, Lag Two	0.4002	0.3574	0.3907	0.3948	0.3467	0.3525
SE	0.0389	0.0426	0.0384	0.0380	0.0420	0.0419
Constant	–0.0343	–0.0082	–0.1774	–0.1538	–0.2183	–0.2023
SE	0.0170	0.0235	0.0572	0.0569	0.0700	0.0701
R^2	0.9071	0.9141	0.9891	0.9893	0.9887	0.9889
Durbin-Watson	2.05	2.08	2.05	2.07	2.08	2.10
Sample Size	550	480	550	550	480	480
Root MSE	0.02778	0.02651	0.02762	0.02731	0.02623	0.02612

Employment: Employed Both Sexes 16 to 17 Years, seasonally adjusted
Unemployment: Unemployment Rate Men 35 to 44 Years
Population Civilian Noninstitutional Population Both Sexes 16 to 17 Years
These data are from the Bureau of Labor Statistics Historical Data Diskette (Major Labor Force Series), which is available by ftp from hopi.bls.gov

sistent with an increase in the number of names on the weekly payroll.[4] At the same time, it should be said, first, that the standard time-series analysis refers to teenage employment rates without regard to hours, and, second, that it might be more difficult to obtain good data on hours than on employment in a telephone survey.[5] The final data set contains 331 observations for New Jersey, and 79 for Pennsylvania (these data are available by anonymous ftp from irs.princeton.edu)[6].

The book should contain a copy of the survey, but it does not.[7] Moreover, it would be

[4] Suppose for example that the wage increase induces restaurant managers to link variations in the number of people working over the course of each day more closely to variations in demand, so that hours are reduced during the lull between breakfast and lunch, and between lunch and dinner. This might involve having different people working during each mealtime, if workers do not like holes in their work schedules.

[5] The Bureau of Labor Statistics compiles a quarterly census of all employers covered by the UI system, commonly referred to by its form number, 202. This contains employment and payroll, but not wages or hours. A monthly establishment survey (form 790) collects information on employment, wages and hours, but this is not a census, and it presumably does not contain enough fast food places to support the kind of detailed analysis carried out by Card and Krueger.

[6] The book claims (p. 18) that the "key data sets" used in the analysis of the fast food industry, and in the cross-state and time-series analyses, are available by ftp. So far, only the New Jersey and Pennsylvania data have been put in the archive.

[7] A copy of the survey was kindly provided by David Card. It begins as follows: "Hello, may I please speak to the manager or assistant manager? I'm conducting a survey for economists at Princeton University on the effects of the Minimum Wage in the restaurant industry. The survey will

TABLE 3
EMPLOYMENT BEFORE AND AFTER NEW JERSEY WAGE INCREASE (AVERAGE NUMBER OF FTE WORKERS PER RESTAURANT[a])

	New Jersey	Pennsylvania	Difference
Before	20.4	23.3	−2.9
(SE)	*(0.51)*	*(1.35)*	*(1.44)*
After	21	21.2	−0.2
Difference	0.6	−2.1	2.7
(SE)	*(0.54)*	*(1.25)*	*(1.36)*

[a] FTE means that each part-time worker was counted as equivalent to .5 full-time workers.

valuable to have an explicit analysis of practical lessons learned from the survey. The most admirable feature of the Card-Krueger work is the collection of new data that forces us to rethink old answers. If this method is to gain acceptance, the practitioners must pay more attention to survey design.

Most people who have read an economics book (and many who have not) know what to expect in this experiment: employment should fall in New Jersey, and not in Pennsylvania, other things being equal. But what if other things are not equal? After all, we are comparing early spring with early winter, and a lot can happen in nine months. Then one must hope that changes in these other things affecting the fast food business in New Jersey are matched by similar changes in Pennsylvania. If so, the effect of the minimum wage increase will show up as the difference between the employment change in New Jersey and the change in Pennsylvania. This "difference in differences" estimate is summarized in Table 3.

The difference in differences estimate in Table 3 says that the minimum wage increase caused a significant *increase* in employment. Employment did not change in New Jersey, while employment fell in Pennsylvania. Should we conclude that the same fall in employment would have been seen in New Jersey if the minimum wage had not been increased? The answer depends on conjectures about what caused the employment changes in Pennsylvania. My own reaction is that the same difference in differences would have been more persuasive if employment had risen by 2.7 in New Jersey, with no change in Pennsylvania. The result in Table 3, on the other hand, is like having a drug trial in which the drug has no effect but the placebo makes people sick.

Another useful way to look at the Card-Krueger data is simply to plot employment before and after the minimum wage increase, for New Jersey and Pennsylvania. Each point in Figures 4a and 4b represents employment before and after for a single restaurant. The finding of a significant positive employment effect is well hidden in these plots. The median lines intersect above the 45° line for New Jersey, and below the 45° line for Pennsylvania, indicating that the median employment level rose in New Jersey and fell in Pennsylvania, but the magnitude of these changes is trivial in relation to the many large employment changes, in both directions, recorded for individual restaurants in both states.

One can also compare restaurants in New Jersey that were already paying starting wages above $5.05 with restaurants paying less, on the theory that the high-wage employers were not directly affected by the new law.[8] Card and Krueger link this comparison with a previous survey of Texas fast food places by Krueger and Lawrence Katz, taken before and after the U.S. minimum wage increased from $3.80 to $4.25 in April 1991.

only take a few minutes and your answers will be kept strictly confidential. Would you mind answering a few questions about your restaurant?

1. How many full-time and part-time workers are employed in your restaurant, excluding managers and assistant managers?

Number full-time:

Number part-time:

2. And how many managers and assistant managers?"

[8] The wage data were obtained in response to the following question:

"4. What is the average starting wage rate for a nonmanagement employee at your restaurant today?

——— ($ per hour), ———the minimum wage"

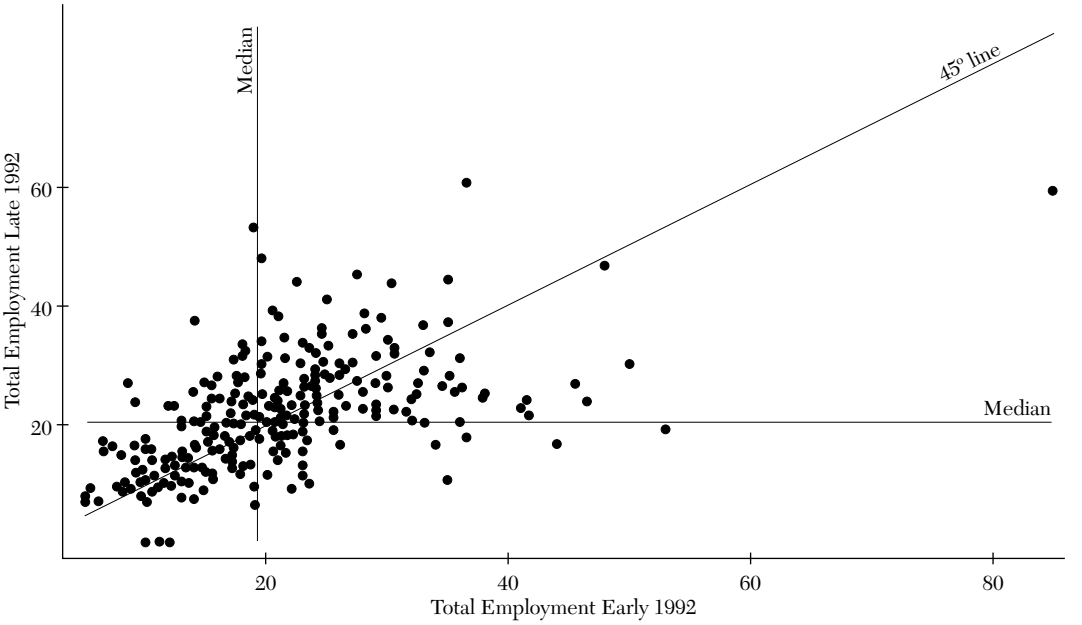

Figure 4a. Employment Changes in New Jersey Restaurants

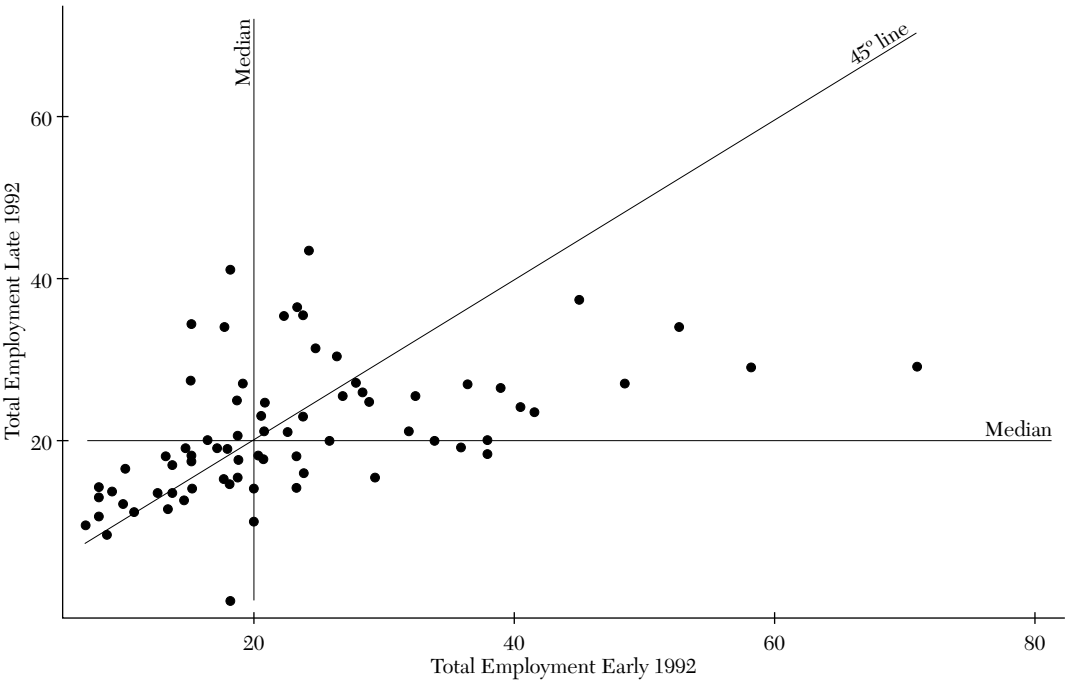

Figure 4b. Employment Changes in Pennsylvania Restaurants

TABLE 4
WAGE DISTRIBUTIONS

	New Wages							
	New Jersey			Pennsylvania				
Old Wage	$5.05	$5.06–$5.75	Total	$4.25	$4.26–$4.99	$5.00	$5.05–$6.25	Total
$4.25	91	4	95	13	8	0	1	22
$4.26–$4.49	14	0	14	1	0	0	0	1
$4.50	43	5	48	3	2	2	1	8
$4.51–$4.74	17	5	22	0	0	0	0	0
$4.75	33	4	37	1	7	1	0	9
$4.76–$4.99	11	3	14	0	1	1	0	2
$5.00	38	6	44	2	6	6	1	15
$5.05	3	1	4	0	0	0	0	0
$5.06–$5.75	18	5	23	0	2	2	0	4
Total	268	33	301	20	26	12	3	61

Explanation: Wages in early 1992 are listed on the left, and wages in late 1992 are listed across the top. Each cell counts the number of observations matching the row and column wages. Observations where either wage is missing are left out.

A breakdown of the wage changes in New Jersey and Pennsylvania is given in Table 4. The survey clearly captured the effect of the New Jersey minimum wage increase on New Jersey wages. There were 95 observations at the old minimum in New Jersey, and 91 of these are found at the new minimum, together with 159 cases that had previously been found between the old and the new minimum. There is a clear tendency for wages to cluster at multiples of a quarter. Starting wage rates in both states were mostly already above $4.25 (the federal minimum) in the first round of the survey. Wage decreases are fairly common in these data: note especially that of 23 New Jersey observations that were above $5.05 initially, 18 fell to exactly $5.05 in the second round. A good guess is that this is mostly due to measurement error. If so, the high-wage subsample is not a valid control group. This point is obscured by Card and Krueger: without listing the wage data, they include 45 observations with initial wages of exactly $5 in the control group of 73 New Jersey restaurants that were supposed to be unaffected by the minimum wage increase. It would be interesting to see whether there are similar problems in the data for Texas, but these data are not yet in the data archive.

Table 5 shows employment changes in New Jersey and Texas, classified by wage levels recorded before the minimum wage increases. There is a clear pattern that would be impressive if the wage classification could be trusted. Employment fell in places that were already paying more than the new minimum, rose in places that were paying the old minimum, and stayed about even in between. These are not movements along a labor demand curve, as Card and Krueger emphasize. Whether the data are consistent with some alternative theory is a question that Card and Krueger are content to leave open. The effects are suspiciously big. For New Jersey, comparing the low and high wage numbers, the difference in differences estimate is that a 19 percent increase in the minimum wage increased employment in low-wage restaurants by 17 percent; the corresponding estimate for Texas is that a 12 percent increase in the minimum increased employment by 33 percent. The high-wage data are suspect, however. In addition to the problems identified in Table 4 with respect to New Jersey, there are only eleven Texas observations

TABLE 5
COMPARISON OF EMPLOYMENT IN LOW-WAGE AND HIGH-WAGE FAST-FOOD PLACES

Previous Wage	<$4.25	$4.25–$5.00	>$5.00	$0.80	Change[a] 19%
New Jersey, 1992	Low	Middle	High		
Sample Size: N = 314	N = 101	N = 140	N = 73	Low-High	
FTE Employment: Before	19.6	20.1	22.3	–2.7	
SE	*0.8*	*0.8*	*1.1*	*1.36*	
After	20.9	21.0	20.2	0.7	
SE	*1.0*	*0.8*	*1.0*	*1.42*	
Change	1.3	0.9	–2.0	**3.4**	17%
SE	*0.95*	*0.71*	*1.14*	*1.3*	
Previous Wage	<$3.80	$3.80–$4.25	>$4.25	$0.45	12%
Texas, 1991	Low	Middle	High		
Sample Size: N = 100	N = 40	N = 53	N = 11	Low-High	
Before	14.65	16.21	16.5	–1.9	
SE	*1.03*	*0.88*	*2.34*	*2.56*	
After	16.9	16.34	13.87	3.0	
SE	*0.97*	*0.88*	*2.09*	*2.30*	
Change	2.25	0.13	–2.63	**4.9**	33%
SE	*1.1*	*0.9*	*2.1*	*2.3*	

[a]The changes in the last column show the minimum wage increases as a percentage of the previous levels, and the employment increases as percentages of the initial employment levels in the first column.

where the wage was initially above the new minimum.

There is no doubt that measurement error is a serious issue in the fast food data (as in all surveys). This is evident in Figure 3, and in Table 4. On the other hand removing the outliers from these data seems unlikely to change the conclusion that there is no sharp decrease in employment in New Jersey resulting from the sudden upward shift in the wage distribution.[9] Positive employment effects are another matter.

[9] The recent hysterical outburst by Paul Craig Roberts (*Business Week*, 4/24/95), drawing on payroll data collected by the Employment Policies Institute, is not worth serious attention because there is nothing to check. David Neumark and William Wascher (1995) have used these data to attack the credibility of data supplied to them by Card and Krueger, while refusing to release their own data. This kind of hit-and-run scholarship will not get us very far.

The Hungry Teenager Theory

A common reaction to the finding that fast-food employment apparently increased is that teenagers like cheeseburgers. In New Jersey, according to Card and Krueger's CPS tabulation, about one teenage worker in three was earning less than $5.05 before the minimum wage increase, and there may have been spillover effects increasing wages for those already above $5.05, to preserve differentials. So earnings for teenagers might easily have risen by ten percent, and they might spend ten percent more on fast food, which would be a noticeable increase, if teenagers account for a large fraction of the demand for fast food. It is odd that Card and Krueger give no thought to this. The idea is that the distribution of income matters: the minimum wage takes money from people who would buy yachts, and gives it to people who buy cheeseburgers (with a bonus effect if yacht-

building workers are laid off, and buy more fast food to save money). If this explanation is right, there must be a reason for the strong opposition of the fast food industry to minimum wage increases: in other words, while wages, product demand, and employment go up, profits must go down.

California, 1988

In July, 1988 the California minimum wage increased from $3.35 to $4.25. About 50 percent of California teenagers were earning less than $4.25 just before the increase. About half of all workers who had been earning wages between $3.35 and $4.25 were employed in retail trade, and about a third of these were in eating and drinking places. Card and Krueger devote a chapter to a series of empirical questions arising from this episode. Card (1992) originally compared the employment rates and weekly hours of California teenagers, before and after the increase, with teenage employment rates in other states; he also made similar comparisons for retail trade workers, with emphasis on restaurant workers. Taeil Kim and Lowell Taylor (1995) followed this up by comparing employment counts within detailed retail trade industries (such as department stores, gas stations, and boat dealers) within California, asking whether employment changed more in those industries that had paid relatively low wages before the minimum wage increase. Kim and Taylor made similar comparisons for retail trade employment across counties within California. Card and Krueger also re-examined Kim and Taylor's results, adding some new data in the process.

Given such detailed and relevant data, carefully studied, one might hope for sharp results on the employment effects of the minimum wage. Instead, the authors of these studies apparently still disagree about the conclusions to be drawn from the data. The employment rate of California teenagers rose, relative to a set of comparison states, between 1987 and 1989. The economy as a whole was improving over this period, and it is difficult to say whether the improvement in California was due to minimum wages or to something else.

Kim and Taylor introduced two additional ways of looking at the California data. The first uses data on employment in 50 detailed retail trade industries in California compared with the aggregate of other states, measured in March 1988 and March 1989, in an attempt to capture the effects of the California minimum wage increase in July 1988. The second uses aggregate retail trade employment for 57 counties in California, again for March 1988 and March 1989.

In both cases, there is a measurement problem: the County Business Patterns data do not have direct measures of wages, so the wage must be obtained by dividing payroll by employment. The data refer to March 31 of each year (although it is not clear what the payroll period is). OLS results for the detailed industries show a strong negative effect when comparing 1988 and 1989, and this stands out when checked against three other comparisons (1985 to 86, 86 to 87, and 87 to 88). The question is whether the negative effect is due to division bias. One answer is that it would show up in the other comparisons, and it did not. Another answer is that instrumental variables can be used to obtain consistent estimates of the minimum wage effect, despite the division bias. The industry wage for 1987 can be used as an instrument for the wage change from 1988 to 1989, on the theory that low-wage industries would have to make big changes to comply with the minimum wage increase. Card and Krueger show that this instrument, by itself, gives an inconclusive result. Another instrument is average establishment size across industries, on the theory that small establishments find it easier to cheat on the minimum wage law. When this is used (with or without the lagged wage) there is a strong negative effect for 1988/89, but not for 1987/89.

Kim and Taylor's estimates using county data are ignored by Card and Krueger, and indeed these estimates are of little value, because they are based on a misspecified regression. Sales tax data are used to measure output in retail sales establishments, by county, and the effects of output growth are netted out of the employment regression. The theoretical relationship is specified as

$$log(E) = a_0 + \sigma \log(W) + a_1 \log(Y)$$

where E is employment, W is the wage, Y is output and σ is the elasticity of substitution between labor and some other factor of production, arbitrarily called capital. There are two ways to interpret this equation. Kim and Taylor treat it as a conditional labor demand function representing the cost-minimizing choice of employment for a given level of output. This obviously can't be right: the cost-minimizing employment choice can't be independent of the price of capital.[10]

The other interpretation is that the equation combines the loglinear relationship between the marginal and average products of labor, implied by the CES technology, with an assumption that labor is paid its marginal product. In that case W stands for the real (product) wage, and Y and E are chosen together, to maximize profit. This interpretation is fine, except that it makes no sense to treat Y as exogenous. An increase in the minimum wage causes the profit-maximizing levels of both output and employment to fall, with employment falling by more than output (assuming constant returns).

Card and Krueger's overall conclusion from the California data is more cautious than one would have expected, based on the introductory and concluding chapters of the book:

> On balance, we believe that the evidence from California shows that the increase in the state minimum wage had a significant impact on wages but no large or systematic effects on employment. (p. 110)

Publication Bias

In the course of a useful review of the weaknesses of conventional time-series estimates of minimum wage effects, Card and Krueger present a novel set of "meta-analysis" results, which they construe as evidence of publication bias.

> Researchers have much discretion over the explanatory variables that they include, the functional form that they impose, the age group on which they focus, the sample that they analyze, and the estimation technique that they use. Researchers may be induced to choose among specifications in part by whether the specifications produce negative and statistically significant employment effects, and reviewers and editors may be induced to publish these studies more often than those containing specifications that produce insignificant effects. (p. 186)

True enough, but what can be done about it? Card and Krueger give a surprising answer: plot the (unsigned) t-statistic of the minimum wage variable in each study against the square root of the sample size, and infer publication bias if the plot does not look like a 45° line (which it does not).

The idea behind this is not spelled out, but it may be explained as follows. The t-statistic in a regression with n observations on k explanatory variables (including the constant) satisfies the equation $t^2(n-k) = r^2/(1-r^2)$ where t is the t-statistic for one of the regressors, and r is the partial correlation coefficient between this regressor and the dependent variable. As the sample size increases, r converges to its population value, assuming stationarity of the partial correlation, so t must grow at the same rate as the square root of $n - k$.

The link between this and publication bias is that the people who did the early minimum wage studies stand accused of playing around with the specification until they found one that had a high sample t-statistic. "Because the statistical significance of the minimum wage effect was overstated in the early studies, however, the later studies discovered weaker effects of the minimum wage." This might explain why the t-statistic does not increase as much as expected, when more data come in. But how are we to decide that the pattern found in the 15 available studies is unlikely to be a chance occurrence? (And if this meta-analysis had turned out the other way, would it still have been included in the book?)

Conclusion

Myth and Measurement is a serious, well-written book, well worth reading (despite its

[10] Errors die hard: this one can be traced to Daniel Hamermesh (1986), where the derivative of the CES cost function is incorrectly stated. The correct derivative unfortunately gives a less simple conditional labor demand function.

misleading title). It is at its best when using standard methods to look at new data, and at its worst when pretending to use new economics to explode myths. A high standard is maintained in the formal argument, but there is an ambivalence about interpretation that is disconcerting. Sometimes the empirical results are cautiously stated: "The weight of this evidence makes it very unlikely that the minimum wage has a large, negative employment effect" (p. 390). Few would disagree with this, but it is hardly worth 400 pages. Elsewhere, a more controversial tone appears:

> Under close scrutiny, the bulk of the empirical evidence on the employment effects of the minimum wage is shown to be consistent with our findings in chapter 2–4, which suggest that increases in the minimum wage have had, if anything, a small, positive effect on employment, rather than an adverse effect. In our opinion, the conventional view that increases in the minimum wage necessarily have an adverse effect on employment has very weak empirical foundations. At a minimum, we believe that our reanalysis of the literature should encourage economists to keep an open mind about the effect of a minimum wage. (p. 236)

Here the authors seem intrigued by the novelty of a positive employment effect, and can not quite resist it even though the result is surely as fragile as the competing results that they criticize. Is the purpose of the book to lay claim to a revolutionary new finding, just in case it turns out to be right in the end? This might explain the argumentative style that runs through the book, distracting attention from the results. The findings are tilted toward the view that the minimum wage does not reduce employment: results that are favorable to this view are accepted at face value, but unfavorable results are exposed to "close scrutiny" and found wanting.

The finding of "no large or systematic effects on employment" in California was quoted above. That quote was taken from the summary at the end of the chapter on California. The summary at the beginning of the same chapter says the same thing, more or less:

> Nevertheless, we find no indication that these wage gains led to employment losses for teenagers or other low-wage workers. To the contrary, we find the rise in the minimum wage actually may have increased both wages *and* employment rates of teenagers in the state. Even in the retail trade industry, we find little evidence that the increase led to significant employment losses. (p. 79)

Here the congenial results for teenagers are highlighted, and the uncongenial retail trade results are minimized. A more balanced assessment is that the minimum wage may have a small effect on employment, but it is hard to detect in noisy data. Why not just say that?

Myth and Measurement's lasting contribution may well be to show that we just don't know how many jobs would be lost if the minimum wage were increased to $5.15, and that we are unlikely to find out by using more sophisticated methods of inference on the existing body of data. What is needed is more sophisticated data. The fast food data for Texas and New Jersey show the potential benefits, but these data were collected with minimal resources. Given the resources available for data collection in government statistical agencies, much more could be done, as was shown 80 years ago by Obenauer and Nienburg (1915).

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