

## REVIEW SYMPOSIUM

### *Myth and Measurement: The New Economics of the Minimum Wage\**

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#### Editor's Introduction by Ronald G. Ehrenberg<sup>†</sup>

*Myth and Measurement: The New Economics of the Minimum Wage* (Princeton University Press, 1995), by David Card and Alan B. Krueger, is an extraordinarily important book. Announced to the world in stories in the *New York Times*, the *Washington Post*, and the *Chronicle of Higher Education*, the book provoked swift reaction. On the one hand, it was widely cited by proponents of minimum wage increases; on the other, vituperative denunciations by critics of its findings appeared in numerous editorial and op-ed articles in a wide range of publications, including the *Wall Street Journal* and *Business Week*.

Why has *Myth and Measurement* engendered so much controversy? In part, because it deals with the minimum wage. The minimum wage was the first piece of protective labor legislation adopted at the national level, and proposals to increase the minimum wage invariably lead to heated debate between labor and business interests. When a book co-authored by the then chief economist in the Clinton Labor Department purports to show that, contrary to

received wisdom, minimum wage increases do not appear to have any adverse effects on employment, it is predictable that conservative critics will attack its findings.

The controversy also stems, however, from the fact that *Myth and Measurement* is much more than an analysis of the minimum wage. If the authors' analyses are correct, they have, perhaps unintentionally, presented a devastating critique both of economic theory and of empirical research methods in economics. Taken at face value, their findings suggest that simple competitive demand and supply models do not provide an adequate description of low-wage labor markets, the very markets in which one might expect these models to "work the best." Taken at face value, their findings also cast considerable doubt on the empirical research methods used by generations of labor economists. Labor economists have prided themselves on the care they have taken in conducting empirical research; if the empirical basis of their findings is so weak, what about the methods used by the rest of the economics profession?

These issues are so profound that, right or wrong, *Myth and Measurement* may well be the most important labor economics monograph of the 1990s. Such a book's findings deserve to be evaluated in one place by economists with a wide variety of perspectives. When I suggested the idea of a review symposium to the editors of the *ILR Review*,

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\*David Card and Alan B. Krueger, *Myth and Measurement: The New Economics of the Minimum Wage* (Princeton: Princeton University Press, 1995). x, 422 pp. ISBN 0-691-04390, \$29.95 (cloth).

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they quickly agreed, arranged for pre-publication copies of *Myth and Measurement* to be sent to Charles Brown (Michigan), Richard Freeman (Harvard), Daniel Hamermesh (Texas A&M), Paul Osterman

(MIT), and Finis Welch (Texas A&M), and asked each to provide us with five- or six-page reactions in time for this issue of the *Review*. All agreed, and their reviews follow.

### Comment by Charles Brown\*

Economists' function in society is to point out unintended consequences. Our habitual refrain is that simple policy fixes may have more fizz than fix and may do unanticipated collateral damage. Our cheerful side—the “invisible hand” demonstration that greed has unintended benefits—goes unappreciated, and so our dismal side dominates public perception.

The minimum wage debate, in which we have warned that attempts to raise poorly paid workers' wages will cost some of them their jobs, is a good example of our dismal side. Moreover, that theoretical argument has been supported by respectable empirical evidence. The most often cited evidence comes from time series studies of teenage employment. My own reading of that evidence is that 10% increases in the minimum wage reduced teenage employment by about 1%.

Over the past two decades, the evidence has gradually gotten more tenuous. In particular, extending the time series studies has produced weaker evidence of non-zero minimum wage effects.

David Card and Alan Krueger's (CK's) *Myth and Measurement: The New Economics of the Minimum Wage* provides both a comprehensive look at earlier work and important new contributions. Their bottom line is that there is little evidence in either their own work or their analysis of others' that minimum wage increases reduce employment. Moreover, they argue that this lack of support for a central prediction of the

textbook model should lead us to actively consider alternative models, and they present several alternatives that base monopsony power on informational imperfections (rather than a lack of competition among employers, as in the “company town” rendition). They also argue that the minimum wage has had a significant equalizing effect on the wage distribution and a modest equalizing effect on the distribution of income, and find some evidence that increases in the minimum wage reduce the stock-market value of low-wage firms. I will focus on their analyses of the effect on employment in the United States.

In Chapters 2–4, CK focus on a variety of cross-sectional comparisons. In New Jersey, where the state minimum wage was increased in 1992, fast-food employment rose slightly, while in neighboring Pennsylvania, where there was no minimum-wage increase, it fell slightly. In New Jersey, employment rose in the restaurants required by the law to increase their wages while it fell in those that initially paid more than the new minimum. Teenage employment rose in California after that state increased its minimum wage in 1988, while teen employment in comparison states with no minimum wage increase was flat; restaurant employment rose a bit less in California than elsewhere. Finally, teen employment and restaurant employment increased faster in states where the 1990 and 1991 federal minimum wage increases had the largest effects on the wage distribution. One can find individual coefficients consistent with the traditional view that the minimum wage reduces employment. But unless (and, perhaps, even if) one focuses on the negative boundary of each confidence

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interval, the evidence of negative effects is for small ones—smaller than the effects found by the traditional time series estimates, and if anything smaller than those found by the more recent time series analyses.

The before-after nature of these comparisons is designed to ensure that observed employment changes are due to changes in the minimum wage, and not to other factors. The authors devote a fair amount of cleverness and effort to convincing the reader that unmeasured differences in pre-existing trends or changes in other omitted variables are not responsible for the lack of negative employment effects where the minimum wage increase should have been more important. The “before” is shortly before the relevant minimum wage increase, and the “after” is often less than a year after the minimum wage increase.<sup>1</sup> CK argue that measurable effects can occur over the short period thus defined, because of the very high turnover rates in these labor markets. Neither quasi-fixed hiring costs nor discharge costs (for example, liability for unemployment insurance benefits) are relevant when the desired employment adjustment can be achieved in a few months simply by not replacing workers who quit. So while the data are too short-run to capture much of the potential longer-run substitution of capital or other inputs for unskilled labor, they provide a very useful alternative to the short-run effects previously measured in time-series work.

This brief summary cannot do full justice to the relevant chapters, but I hope it does fairly convey the “collage” strategy that the authors employ. One might worry that employers had begun adjusting to the minimum wage increase prior to CK’s baseline, or that sampling error in CPS-based measures of the proportion of workers initially earning less than the new minimum biases

the estimates toward zero, or that a future study will show that teenage employment responds to the declining real minimum wage in the 1980s if one takes account of technologically driven declines in the demand for low-wage workers over the period. But taken together, the various pieces do support the authors’ reading of their evidence—short-run effects are smaller than we thought.

CK have much less to say about longer-run effects. It is possible, as they concede, that the minimum wage has essentially no effect in the short run, while the capital stock is fixed, but has a more significant longer-run effect as capital (or other inputs) can be substituted for low-wage labor. What clues they can find about long-run effects do not suggest they are important, but my impression is that there is very little evidence either way to change whatever prior one brings to the discussion of long-run effects. I would still expect employment effects of a minimum wage increase to be more negative in the long run than in the short run, but this is a matter that demands further investigation.<sup>2</sup>

In Chapter 11, CK argue that the absence of short-run effects is inconsistent with the textbook competitive model of the labor market, and so calls for a consideration of alternative models. The main contenders here are monopsony-like models in which workers face less than perfectly elastic labor supply because of informational imperfections. A typical firm cannot have any desired level of employment at the going wage, at least in the short run. Workers search for higher-wage jobs but do not know the wage currently offered by all em-

<sup>1</sup>The “before” period in the New Jersey–Pennsylvania study is uncomfortably close to the minimum wage increase (1–2 months, in an environment where there was real question about whether the increase would take effect), and the baseline for the Texas study is between the 1990 and 1991 increases in the federal minimum.

<sup>2</sup>Changes in the level of the minimum wage have a large transitory component due to the pattern of periodic large increases offset by gradual erosion, but coverage increases are more permanent. One troubling aspect of the time series studies is that when separate level and coverage effects are allowed, negative effects of coverage are particularly hard to find. In any case, the relationship between legislated changes in either level or coverage and a typical employer’s forecast of the wage she or he will be required to pay in the long run is subtle enough to greatly complicate attempts to measure long-run effects.

ployers; consequently, raising the offered wage will increase the fraction of applicants who accept an offer, and reduce the fraction of current workers who quit, but neither response is infinitely elastic.

Does the evidence strongly favor a monopsony-like model, based on informational imperfections rather than a shortage of competing employers? In my view, it does not. If firms do not reduce employment following a minimum wage increase, one would expect output to be constant, too. Output will increase if employers can extract a bit more effort from their better-paid workers, or if employment actually increases in response to a minimum wage increase, as Chapter 2's fast-food results suggest. For output to rise, price must fall. CK devote a lot of effort to measuring this largely neglected effect of minimum wages, but the gods do not smile: the estimates, though imprecise, tend to point toward price increases.

Another aspect of minimum wage labor markets that might have provided support for a monopsony model is the bonus that is sometimes paid to a worker who recruits another worker to the firm. Twenty to thirty percent of the fast-food restaurants in the New Jersey–Pennsylvania study paid such bonuses. If one takes the bonus as an indication of the gap between the wage and the marginal product of labor, one might have expected bonuses to become less common in New Jersey (relative to Pennsylvania) after New Jersey increased its minimum wage. In fact, the bonuses declined

more in Pennsylvania, though the difference is not significant.

Even if the minimum wage reduces marginal labor cost, it raises average cost per worker, reduces profitability, and should lead to less entry into and faster exit from such industries.<sup>3</sup> The policy concern that employment effects are more unfavorable in the long run would therefore remain.

Throughout the book, CK are careful to show that changes in the legally required minimum wage do change the wages that employers pay: the spike at the minimum wage moves along with the law. Why this does not trigger more visible reductions in employment of affected workers, at least in the short run, remains puzzling. Resolving this puzzle and estimating longer-run effects are related but distinct tasks. I believe that many aspects of CK's approach—pooling quite different data sets and different approximations to an ideal experiment, focusing on cross-sectional as well as time-series variation, emphasizing related outcomes (price changes, wage distributions) along with employment effects, and eagerly bringing new data to an old controversy—will characterize future work on both sides of the minimum wage debate.

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<sup>3</sup>In models in which firms have some latitude in choosing the wage they will pay, the first order effect of a change in the wage is zero. But this lack of effect occurs only when one holds constant the wages offered by all other firms. In contrast, the minimum wage raises the wages of low-wage employers as a group. Hence one would not expect a minimum wage increase to leave costs unaffected.

### Comment by Richard B. Freeman\*

#### *What Will a 10%...50%...100% Increase in the Minimum Wage Do?*

Economists, like every “little boy and every gal” in Gilbert and Sullivan's *Iolanthe*

(“that's born...either a little Liberal or a little Conservative”), are divided into two basic groups. On one side are those who believe that responses to price incentives are usually large—Big Responders (BRs). On the other side are those who believe that responses to price incentives are generally small—Small Responders (SRs).

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Present a BR with an exogenous change in price or wage—for instance, a mandated increase in the minimum—and his prior is that there will be a large response in quantities. BRs feel comfortable with perfect competition, Heckscher-Ohlin trade models, factor price equalization, large responses in effort and hours to marginal taxes, welfare traps, arbitrage of financial opportunities across national lines, and large employment losses to administered wages. Forced to choose between a first-approximation economic model with an infinite elasticity of response and one with zero elasticity, the BR economist opts for infinity: “In the long run, there are many substitutes, new competitors, suppliers, and so on.”

Present an SR with a change in price or wage, and his prior is that quantities will not change much. SRs feel comfortable with input-output analysis, imperfect competition, factor content analyses of the effects of trade on employment, the correlation of investment and savings across countries, backward-bending supply curves, and the persistence of economic rents. Forced to choose between a first-approximation model with infinite elasticity of response and one with zero elasticity, the SR economist opts for zero: “In the real world, costs of adjustment are large, uncertainty slows responses, habits change gradually, and so on.”

What does the BR-SR divide have to do with the Card and Krueger volume? A lot. Along with its other valuable contributions, the book provides the most important evidence in recent years on the SR side of this recurrent debate. It does so through an exemplary empirical analysis of the effects of minimum wages, a subject on which most economists tend to be BRs by inclination.

It is important to recognize at the outset that there is little in economic analysis to help us decide whether quantity responses to price incentives are likely to be large or small. Many economists expect larger responses in the long run, or when budget constraints are important. Logic tells us that massive changes in prices (say, tripling our wages) that turn balance sheets from

the black to the red will have large effects on quantities (the Deans might close down our departments). But *whether the BR or SR perspective applies to minimum wages in the range observed in the United States is a purely empirical question.* It does not have to do with acceptance or rejection of neoclassical economic theory. There is no theoretical or *a priori* reason for assuming that minimum wages that average 40–50% of hourly earnings in manufacturing have large or small effects on employment, or even that minimum wages that average 70% or 80% of average earnings will necessarily have large effects. Indeed, economic theory is so “rich” that it offers us monopsony models that predict increases in employment in response to minimum wages. Only careful empirical analysis can determine the magnitude of employment responses to the minimum wage. The BR-SR divide is an empirical one.

This book offers the most careful and wide-ranging analysis of the empirical evidence on minimum wages in the United States that any social scientist could ask for. On many issues, the evidentiary base is sufficiently diffuse to allow economists with different priors to reach different conclusions—to make a lawyers’ case, as it were, for their preferred BR or SR world. (Labor economists think the same is true of virtually all macro-economic studies, in which conclusions seemingly hinge on assumptions—called structural models—that determine what one sees in limited time series data, because the evidence does not “speak for itself.”) There is less scope for disparate interpretations, however, on the subject of the short- or medium-run effects of the minimum wage on employment. Because minimum wage changes occur in discrete exogenous jumps over time and across areas, they constitute “natural experiments.” Card and Krueger exploit this variation admirably. Indeed, their analysis is a model of how to do empirical economics. The authors examine not one, not two, but many such experiments to judge the employment effects of minimum wage increases. Using both CPS data and new data that they and their colleagues gathered, Card and

Krueger closely examine conditions before and after the minimum wage changed in several states. Where possible, they include control groups. They look at evidence on the effects of the minimum wage on the distribution of earnings and, using event studies, the effects of announced increases on the value of firms and on prices. Forget for the moment the results or possible data problems in particular parts of the work. If you want to find out what the world says about an issue, rather than force the world into your preconceived structural model, Card and Krueger show how to do so. Richard Lester, to whom the book is dedicated, should be well satisfied to be recognized by the authors of such a careful and searching analysis.

The only thing I would like to have seen added to this study is some detailed case investigations of low-wage employers' response to the minimum, or better yet, some ethnographic experience. Such analysis would have pleased Lester too, if I am not mistaken. A few Ronald Reagan-style anecdotes would go a long way toward defusing some of the criticisms the book has received from employer groups.

In my view the weakest chapter, partly for want of information at the time of the book's writing, is that on foreign experiences. The book refers to recent British work on the abolition of wage councils that yields SR results consistent with those in this book: no adverse change in employment (Machin and Manning 1994). In addition, Gregory and Duncan's 1991 analysis of the employment consequences of comparable worth in Australia found that large mandated increases in women's pay had, at most, small employment effects on those women. As those two studies suggest, there is more empirical support for the SR view than most economists have recognized. In addition, there is no evidence that minimum wages in developing countries have contributed to their employment problems during periods of structural adjustment, in part because minima have proven to be sawdust rather than hardwood floors in times of economic crisis.

Methodology aside, Card and

Krueger's empirical results suggest strong and, in some cases, surprising conclusions: negligible employment effects in most cases, and positive employment effects in some well-designed natural experiments. Their summary table (Table 12.1) shows one zero effect and six positive employment effects, of which two are significant.

Are the results believable? They are generated by well-designed analyses, but even the best natural experiment does not have the power of a controlled laboratory experiment, so we (and the authors) must apply some prior judgment. It could be, for instance, that employment effects predicted by BRs simply take longer to take effect, or (as the authors suggest in some cases) that the minimum wage is not truly exogenous.

To economists with an SR orientation, the results will fit with priors and thus surely pass any believability test. To those who work with empirical data, the quality of the analysis makes the results believable, largely because the design of most of the studies is such that "ceteris paribus" is much more than a theorists' fable. I read Table 12.1 as rejecting negative effects rather than showing that minimum wages raise employment, and I would be surprised if further work found much additional evidence that minimum wage increases were associated with employment gains. But empirical analysis is full of surprises.

Economists with a BR orientation who give more weight to priors than to evidence will find the results troubling. But they cannot simply denounce the findings. One virtue of Card and Krueger's book is that it has shifted the burden of proof about the employment effects of the minimum wage. Even a cursory look at data from the last decade provides support for their position as opposed to a BR view of the U.S. minimum wage. During the 1980s, the Reagan Administration maintained the minimum wage at its nominal level, which reduced its real value, with no apparent benefits in job creation for low-paid workers. Employment of the group most affected, teenagers, fell relative to that of adults. Teenage

unemployment rates improved at most a whisker relative to that of adults. The time worked by workers at the bottom of the wage distribution fell, absolutely and relative to workers higher in the wage distribution.

At the end of the decade and in the early 1990s, the federal minimum was increased and several states raised their minimum. If you think these changes should have had noticeable adverse effects on employment, Card and Krueger show otherwise. To answer a slightly paraphrased version of Charles Brown's 1988 question, "Yes, Virginia, the employment effects of the minimum wage were overrated."

Maybe there is a case for large employment changes in response to the minimum over some long time horizon. This book does not reject the possibility that ten years after, say, New Jersey raised its minimum (and assuming that it maintains the level of the minimum wage relative to other wages), employment in low-wage sectors will be lower than it would have been if the minimum had remained unchanged. To test this possibility will, however, be exceedingly difficult, because economies change so much over a decade (Card and Krueger mention inflation; I would stress structural changes) that even the most creative economist will find it hard to achieve a believable *ceteris paribus*. For this reason, I would bet the family house that no future empirical study showing large employment effects of the minimum wage over longer periods—if there ever is such a study—will match the authority of Card and Krueger's rejection of employment responses in the time periods they explore. Empirical analysis of quantitative responses to price incentives is most convincing over periods during which the structure of the economy, technology, and so on can safely be viewed as fixed.

In an era with rising inequality, it is natural to look anew at increases in the minimum wage as a way to improve the earnings of low-paid workers. The policy implications of this major book on the minimum wage therefore clearly demand attention.

The authors suggest that their results should lead to "a reorientation of policy discussions away from the efficiency aspects of the minimum wage and toward distributional issues" (p. 393). I hope this is the case. In recent years the BR view has dominated much public discussion. Supply-side economics typically posits large responses to prices, taxes, regulations, and so on. Labor market regulations, including administered wages and other mandatory labor costs, are often said to explain European unemployment problems. Textbook discussion of minimum wages has the same flavor, stressing job losses, not wage gains. Increases in the minimum wage thus seem exceedingly dangerous, and supporters of such increases are often viewed as populists with little knowledge of economics. This book goes a long way toward redressing this imbalance. Perhaps it will be part of an overall resurgence of SR economics. At the least, the book should make it respectable to discuss the minimum wage as a policy option, with benefits and costs, in an era of rising inequality.

What factors ought we to consider as part of a "reorientation" of public discussion? There are, in my view, five issues in assessing the policy of using the minimum wage to help low-paid workers.

1. *Does the minimum wage redistribute income to low-wage workers?* It will do so if the estimated elasticity is below one. The book shows that modest increases in the minimum are likely to have no effect on employment. While there are researchers, notably Neumark and Wascher, who argue the opposite, the debate is over whether modest minimum wage increases have "no" employment effect, modest positive effects, or small negative effects. It is *not* about whether or not there are large negative effects.

2. *Does the minimum wage divide the work force into insiders, employed permanently at the minimum, and outsiders who suffer long-term joblessness because of the minimum?* In the U.S. labor market, with its high turnover, particularly in low-wage jobs, it is hard to make the case for such a division; even

analysts who believe in segmented labor markets do not argue for the European insiders (employed)–outsiders (unemployed) division. The studies by Topel (1993) and Juhn, Murphy, and Topel (1991) of employment at the lower end of the wage structure make it clear that low and falling wages, not excessively high minimum wages or other administered wages, have reduced employment at the bottom tier of the wage distribution.

3. *Are low-wage workers low-income workers?*

No one who advocates the minimum wage wants to raise the pay of teenagers in upper-income families at the expense of lower-income consumers. If the result of an effective minimum wage is that Harvey Poor pays more for his hamburger so Melissa or Roderick Well-to-Do can earn more pocket money, the minimum will be redistributive, but in a regressive direction. In Chapter 9 of the book, Card and Krueger show that currently many workers paid around the minimum are, in fact, from low-income families: one-third of workers whose wages were affected by the 1990 and 1991 increases in the minimum came from the bottom 10% of the earnings distribution. The widening dispersion of wages has meant that more low-skill young adults earn “teenage” wages. Card and Krueger do not explore the distribution of consumer products that use minimum wage-type labor. I would expect lower-income families to purchase more such products, but I would also expect the differences in consumer spending patterns to be modest.

4. *How does the minimum wage fit with other economic policies?* We all know that, despite the valiant efforts of the Invisible Hand, at best we live in a second-best world. The effects of the minimum wage or of any other policy must be judged in the context of numerous other policies and institutions. Many on both sides of the aisle in Congress favor Earned Income Tax Credits as a way to improve the economic well-being of low-wage workers. EITCs subsidize low-wage employers. Minimum wages “tax” those

employers. The two policies would seem to complement one another. While no one has analyzed the quantitative interactions between these (and other) policies, my guess is that the minimum wage looks better in the second-best world in which we live than it does in most textbooks.

Finally, the bottom line question is:

5. *At what level should we fix the minimum wage if we are to redistribute income without risking sizable job loss?*

*Myth and Measurement—The New Economics of the Minimum Wage* makes a convincing case that we have overestimated the dangers of job losses and that the level of the minimum wage that does more good than harm is probably much higher than many economists have previously thought. The book shows as well as empirical economics can that 10%–20% increases in the current U.S. federal minimum will do little if any harm to employment. Card and Krueger are properly cautious in extrapolating analysis of the minimum in the range found in the United States to much higher minima. At some point, every SR economist becomes a BR economist. A 100% increase in the minimum? A 200% increase? I know (sadly) that if you raise my pay to rock star levels, my employers will disemploy me, tenure and my singing talents notwithstanding. Still, if, within certain ranges, the minimum has little effect on employment, per Card and Krueger; if low wages reduce employment through supply responses, per Juhn, Murphy, and Topel; and if earnings inequality has become a major national problem, as we all recognize, policy debate should concentrate on the question, “What level of the minimum can redistribute income to low-paid workers without serious job loss?” We do not have too many weapons in the policy arsenal to raise the pay of low-wage workers. This book has caused me to revise upward the level of the minimum wage at which I believe income can be redistributed without causing job losses. I predict it will do the same for you.



Comment by Daniel S. Hamermesh\*

*What a Wonderful World This Would Be<sup>1</sup>*

The major socioeconomic problem facing the United States in the past 15 years has been the widening of earnings differentials, which has been especially severe at the lower end of the distribution. It would be wonderful to ameliorate this problem at a stroke by raising the minimum wage and increasing wage rates of low-wage workers without reducing their employment. The published articles that comprise most of this book form the intellectual basis for President Clinton's announcement in January 1995 that he believes this most desirable result would ensue.

Though they are far less important, the biggest research problems facing labor economists are the difficulty of modeling the processes that generate economic outcomes and the lack of data to estimate those models. It would be wonderful to solve these problems too at a stroke. Based on the arguments of this book (especially Chapters 2–4, which the authors believe are their chief contribution to the literature), we can do so if we can find natural experiments describing the shocks we wish to study, in this case the effects of higher minimum wages. I wish both of Card and Krueger's (CK's) wonderful results were correct. They are not.

How Natural Were Their Experiments?

Our research lives would be easier if we could perform true socioeconomic experiments; and despite their expense, some have been possible (for example, Woodbury and Spiegelman 1987). They allow us to measure the effect of a treatment on an outcome  $Y$  by calculating the difference

$Y_T - Y_C$ , where  $T$  denotes a treatment group and  $C$  denotes a control group (usually of individuals, but perhaps of firms, industries, or geographical areas). Since the members of the two groups are chosen randomly, they are presumably statistically identical in all respects that might affect  $Y$ . Thus any difference we observe between the groups after the treatment can be attributed to it.

Usually, however, we do not have the funding or the access to randomized subjects that would allow us to evaluate the effect of a (policy) treatment by means of laboratory-style experiments. An alternative to such experiments, and the mainstay (CK's term) of the work presented in this book, is a series of what CK refer to as "natural experiments" (hereafter NEs). To make use of NEs, the authors propose calculating the following "difference-in-differences" between groups  $T$  and  $C$  before (Time 1) and after (Time 2) the treatment is administered at Time  $P$  (the time of policy intervention or the occurrence of some other event):

$$D^2 = [Y_{T2} - Y_{C2}] - [Y_{T1} - Y_{C1}].$$

Two assumptions are implicit throughout the evaluation of the NE: (1)  $D^2$  would be zero if the treatment had not occurred, so a nonzero  $D^2$  indicates the effect of the treatment (that is, nothing else could have caused the difference in the outcomes to change), and (2) Time  $P$  follows Time 1 (the intervention occurs after we measure the initial outcomes in the two groups).

A large variety of issues with this approach should be considered. I limit myself to those that do not seem to have been raised before and that appear central to evaluating CK's claims of a nonnegative employment effect of a higher minimum wage. Three conditions are particularly relevant in interpreting CK's work: (1) Time 1 must be sufficiently before Time  $P$  that group  $T$  did not adjust to the treatment before Time 1—otherwise  $[Y_{T1} - Y_{C1}]$  will

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<sup>1</sup>Sam Cooke, 1960.

reflect the effect of the treatment; (2) Time 2 must be sufficiently after Time  $P$  to allow the treatment's effects to be fully felt; and (3) We must be sure that the same difference [ $Y_{T1} - Y_{C1}$ ] would have been observed at Time 2 if the treatment had not been imposed, that is,  $C$  must be such a good control that there is no need to adjust the differences for factors other than the treatment that might have caused them to change.

Condition 1 is especially important in studying the effect of legislation. Laws do not just happen, especially in our presidential system. They are discussed at great length prior to their enactment, and they are often preprogrammed years in advance of their effective dates. Most interested observers know the likelihood of the change long before the date of enactment (and even longer before its effective date). In their study of the fast-food industry in New Jersey ( $T$ ) and Pennsylvania ( $C$ ), CK examine the effect of an increase in the state minimum wage in New Jersey in April 1992 (Time  $P$ ). Time 1 is February 1992, Time 2 is November 1992. But the minimum was enacted in 1990, long before Time 1; and Time 2 is only 7 months after the effective date. One can justifiably argue that the policy intervention really occurred long before Time 1.

Conditions 1 and 2 are clearly not met in this "experiment" unless one can argue that employers will not preadjust to the policy change and will adjust very quickly at the time of the treatment. This is CK's claim, which they base (p. 67 and elsewhere) on the high quit rates of teenage and other low-wage labor. If labor were the only productive input their claim would be valid, since adjustment of employment demand would be rapid. Yet, as they note elsewhere (p. 367), "Over the short run...nonlabor inputs may be costly to adjust or may be 'sunk' (an example is the physical structure of a fast-food restaurant)." We do know (Hamermesh 1993) that firms adjust capital slowly. We also believe that labor and machinery are dynamic p-complements—if one input is adjusted slowly, the adjustment of the other is slowed. The full effect

on employment of a rise in the minimum wage will not be felt as quickly as is necessary for Time 2 to be sufficiently after Time  $P$  in this NE. This difficulty also means that for evaluating firms' eventual employment responses Time 1 is not sufficiently before the treatment.

In CK's second NE study, an examination of fast-food outlets in Texas, Time 1 is December 1990 and Time 2 is July–August 1991. CK view Time  $P$  for this increase in the federal minimum wage as April 1, 1991, but the change was enacted well before Time 1 (in 1989). Their third study in this genre, of the increase in the California state minimum in January 1988, compares California to the rest of the United States (or selected comparison areas) in 1987 (Time 1) and 1989 (Time 2). Yet the treatment had been recommended by the state Labor and Employment Commission in May 1987. It was in the air during much of Time 1, and was thus hardly a policy surprise at Time  $P$  (January 1988). Like the New Jersey–Pennsylvania study, these two NE studies are plagued by the problems (a) that Time  $P$  precedes Time 1 and (b) that Time 2 follows much too closely upon even CK's dating of the treatment to allow a measurement of treatment effects.

The overlap of Times  $P$  and 1 also vitiates the event studies that comprise CK's efforts to infer the effect of the minimum wage on stockholders' wealth (Chapter 10). These studies examine whether "news" about the minimum wage alters the paths of returns to holding a particular company's shares. The events they focus on are so minuscule (for example, the news [p. 340] that on September 22, 1987, a "move in Congress to boost minimum wage revives perennial debate") that no one could reasonably expect any effect on streams of returns to shares in particular companies relative to market returns. That CK find none for most individual events is unsurprising. Even though a piece of legislation may have major effects, in a large representative democracy individual actors' roles are so small that their statements can have only tiny effects on the market valuation of shares.

Even if CK had no difficulties with Con-

ditions 1 and 2, their failure to satisfy Condition 3 would cast grave doubt on their approach. The propinquity of New Jersey and Pennsylvania and their similar  $Y_1$  are not reasons to expect that their  $Y_2$  would have been similar absent the treatment. To make such a claim is to argue that *any* two economic outcomes that are similar at one time will be similar at some other. That is nonsense on its face, and it is what requires us to model the determinants of  $Y_{T1} - Y_{C1}$ . If  $Y$  represents employment and we are interested in the effect of shocks to wages in unit  $T$ , this difficulty becomes especially important. We know (for example, Freeman 1977) that the variance in employment that we observe over time is predominantly caused by demand shocks (perhaps measured by shocks to product demand). Changes in employment engendered by supply shocks (movements along the labor demand curve) appear to account for a much smaller fraction of this variance. Unless CK are certain that relative demand shocks are the same at Times 1 and 2 between groups  $T$  and  $C$ , any changes in the relative shocks will swamp the effect of a higher minimum wage that moves employers up their demand curves for low-skilled labor in group  $T$ .

An NE is not a panacea for research, though under certain conditions (including my Conditions 1 and 2) it is a useful tool for evaluation. It is more powerful when substantial effort is made to control for the changing determinants of the outcome (Condition 3). Two better uses of this approach, Card's (1990) study of the Mariel boatlift, a true policy shock to the Miami labor market, and Angrist's (1990) examination of the effect of the Vietnam draft lottery on earnings, meet the first two conditions but not the third. CK's research on minimum wages meets none of them. Their cases are *neither natural nor experiments*.

#### Time-Series, Panel, and International Results

CK's Chapters 6–8 are designed “to probe the robustness of past estimates” (p. 236). Their probe convinces them that the previ-

ous results are “surprisingly fragile” (pp. 240, 242, 271, and 355). Their general conclusion is that this evidence “is consistent with...if anything...a small, positive effect on employment” (p. 236). This is an astounding conclusion based on their evidence, especially for the U.S. time series. *Every* estimate that they cite or generate is negative, though not all are significantly so. No unbiased reader could conclude from Chapter 6 anything other than that the effect is small and negative and thus inconsistent with results from CK's NEs.

CK reproduce earlier results showing that employment effects in the United States are weaker once one includes the 1980s in the data; and they provide the interesting finding that a similar attenuation appears in Canadian data. Are these changes the outcome of a declining demand elasticity for low-skilled labor? Do they stem from the smaller mass of the wage distribution around the minimum in the 1980s (so that a given percentage increase in the minimum relative to the average wage affected a smaller percentage of low-skilled workers)? Why these changes occur is unclear, and it would have been good to examine them in more detail.

#### Theoretical Explanations

The simplest theoretical rationalization for CK's results (especially their central NE results) is that they are observing very short-run responses. No one would expect the minimum wage to reduce low-skilled workers' employment immediately; and immediately (or several months) is the difference between Times  $P$  and 2 in their NEs. Even ignoring the other severe problems with their results, those results are perfectly consistent with standard economic analysis in the presence of adjustment costs in factor demand.

CK offer two theoretical justifications for their findings. The first, to which much of Chapter 11 is devoted, is the concept of monopsony dressed up in dynamic clothing. Their argument is that firms must pay higher wages to attract new workers to replace the flow of quits. Of course this is

true (though probably very minor in the fast-food industry, given the evidence that hiring costs are very low for low-skilled workers); but it speaks only to short-run monopsony, not to static monopsony. Thus their subtitled "new economics" explains the possible short-run absence of negative employment effects of higher minima, but so does the standard theory of dynamic factor demand. Even CK, however, would not argue that this can be a long-run effect, especially in low-wage and densely populated labor markets.

The only argument CK adduce in support of their apparent belief in the long-run positive effects of minimum wage increases is the equally hoary idea often referred to as shock theory, presented here in the discussion of some early results for Puerto Rico (p. 247): "turnover and absenteeism declined, the screening of job applicants improved and 'managerial effort' improved." If shock theory were valid, we would expect CK's event studies (Chapter 10) to demonstrate that news about the possible enactment of a higher minimum wage raises share prices, because the theory implies that the shock will raise the affected firms' profits. Their results show no such effect. In reality, however, no one should expect any shock effect resulting from alterations to our nearly 60-year-old minimum wage policy, as an application of the following argument *mutatis mutandis* should make clear:

Shock theory is most plausible as applied to the unionization of a previously nonunion enterprise.... It is much harder to imagine repeated waves of successful innovation in response to annual wage increases negotiated with an established union. (Rees 1973:83)

### Conclusion

CK's arguments on the employment ef-

fects of the minimum wage are in the same vein as those of the losing side in the old antimarginalist controversy. (See Lester 1946, to whom CK dedicate their work; and Machlup 1946, for the arguments that eventually prevailed.) The authors challenge economic notions that make logical sense with new evidence; but they never offer a convincing theoretical explanation for why the old logic fails. Lacking that, readers should examine their evidence very carefully. That examination yields the inescapable conclusion that, even on its own grounds, CK's strongest evidence is fatally flawed. A fair interpretation is that they have shown that employers plan in part for minimum wage increases and that the part they do not plan for leads them to adjust slowly (and us to fail initially to observe reduced employment).

One can reasonably believe it is wrong for a society to allow jobs to pay as little as \$4.25 per hour, and that we should be willing to aid the increased number of people who are not employed when the minimum is raised above that level. Aid would include well-funded training subsidies and direct training programs, as well as efforts to ease school-to-job transitions. A wonderful world of reduced inequality through higher wage minima with no loss of jobs is regrettably not an option. Similarly, without true experiments there are no easy research strategies that might allow us to avoid the modeling necessary to control for changes in other variables that determine the outcomes of interest to us. In the end, the book's less important contribution will be to stimulate additional careful work on the specific issue of the minimum wage. Its bigger contribution is to make us realize how difficult it is to measure the effects of labor market policies.

**Comment by Paul Osterman\***

David Card and Alan B. Krueger (CK) have written a book that represents a phenomenal amount of careful and honest research and that will be a classic in the minimum wage literature and also in the broader field of empirical labor economics. At the same time, this book is a damning indictment of how labor economics has been practiced over the past three decades, an indictment whose force may not be fully recognized even by the authors.

CK argue that the standard prediction regarding the effect of minimum wage increases on employment is wrong. Although they never present a single "best" estimate, their essential conclusion is that the employment impact is near zero, with some evidence even supporting a positive effect. Although the focus of the book, and the headlines it has garnered, center on this finding, CK also examine related issues such as the effect of the minimum wage on the wage structure of the firm, on the wages of workers who are paid above the minimum, on the earnings distribution, and on firm performance. In many respects these issues are more interesting than the employment effect.

CK's methods will extend the book's influence beyond its topic. First, they work with a remarkable range of data. These include studies of the fast-food industries in New Jersey, Pennsylvania, and Texas; comparisons of California with a set of neighboring states after the California minimum wage increase of 1988; national cross-state and time series analysis; international evidence on Canada, Puerto Rico, and England; and stock market event studies. As a result, their findings do not rise or fall on any specific data set or specification.

More fundamentally, CK make a powerful case that what they term "natural experiments" are a more appropriate way to conduct policy analysis than cruder research

based on time series or broad cross-sections. Comparing the growth of teenage fast-food employment in New Jersey, which raised its minimum wage, with that in neighboring Pennsylvania, which did not, is an example. "Experiments" of this kind cannot replace traditional research, in part because they are not always available and in part because they suffer from their own weaknesses. However, it seems a sure bet that a growing fraction of empirical work will take this approach.

No researcher will be able to write about the minimum wage without referring to this book, but the book will not be the last word. There are a number of issues left dangling that provide grounds for a counter-attack. It is possible, for example, that firms reduce fringe benefits and training investments to offset the minimum wage rise, and CK's evidence on this subject is not as impressive as their evidence on employment. CK also find that in both America and Canada the employment effect of the minimum wage has weakened in recent years, yet they provide no explanation for that result.

These issues of technique and result will doubtlessly dominate the debate engendered by the book. I want, however, to turn to another question: what the book tells us about the practice of labor economics. The answer is troubling with respect to the quality, and objectivity, of empirical work, and it is certainly damning with regard to how much progress we have made in understanding the labor market.

That an increase in the minimum wage should reduce employment is one of labor economists' core beliefs, driven by the basic model of downward-sloping demand curves and firms as price takers. There is a library of research to support this prediction. CK systematically examine this research and their findings are not pretty. Although they are too polite to say so, in effect they charge that some investigators pushed the limits of acceptable practice to produce results consistent with theory.

CK report a meta-analysis of time series

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studies suggesting that journals and authors are systematically biased toward publishing findings that accord with their priors. This also implies that they avoid publishing papers presenting results with which they disagree. This pattern is disturbing, but things get worse when CK dissect specific studies. These studies, they find, committed many and varied sins. Among these sins were including endogenous school enrollment rates on the right-hand side of an employment model, studying effects of the minimum wage on individual employment transitions using a population just at the point of retirement, making strong and unsupported assumptions regarding the shape of the earnings distribution in the absence of the minimum wage, failing to use weighted least squares when observations were industries of sharply varying sizes and data noise, and simply ignoring findings that were inconsistent with the point the authors wanted to make. These errors are fairly simple, and CK's meta-analysis suggests they were overlooked not because of some recent technical econometric advances that only now have brought them to light, but rather because they produced the "right" results.

There is an even deeper way in which this book damns labor economics as it has been practiced. It demonstrates how shallow is our understanding of how labor markets work and how little progress has been made since the research of the institutionalists of the 1940s and 1950s.

In addition to their analysis of the employment effect, CK provide evidence on a number of other characteristics of low-wage labor markets that seem inconsistent with received wisdom. These include widespread failure of employers to utilize the sub-minimum wage, the finding that many firms that are legally exempt from the minimum pay it anyway, the double fact of wage variation for seemingly identical employees and wage uniformity for seemingly heterogeneous workers, and the effort by some firms to maintain their internal wage structure after increases in the minimum.

At various points CK offer ad hoc explanations of these findings. These include

the use of the wage as a tool to reduce turnover and enhance loyalty and motivation (p. 154), the idea that some employers simply believe in a "fair" or "just" wage and this wage becomes identified with the minimum (p. 159), the existence of a shock effect that enables firms to gain new efficiencies after the imposition of a minimum (p. 247), and employee and employer concern about the fairness of traditional relative wage structures and hence avoidance of the sub-minimum (p. 168). These are all reasonable ideas and they can all be found developed in virtually the same degree of detail, but supported by more compelling interviews with employers, in Chapter 6 of Lloyd Reynolds's *The Structure of Labor Markets* (1951).

In a brief penultimate chapter, CK emphasize two other models. In the first, firms can choose between two ways of organizing themselves: a low-wage/high-turnover/high-vacancy model, and a higher-wage/lower-turnover/higher-stock-of-employment model. Given a tradeoff between wage levels and recruiting costs, both approaches are potentially efficient. A minimum wage increase forces some firms from the former to the latter strategy and, as a result, can increase the stock of employment.

In the event the reader dislikes this story, CK offer up another idea based on a model developed by Rebitzer and Taylor: in the spirit of efficiency wages, they suggest that firms face a tradeoff between high wages and more intensive monitoring to gain employee effort. Because monitoring demands considerable managerial time, there is a limit to the number of employees who can be supervised, and each firm reaches a supervision/wage optimum for obtaining effort. If firms are forced to raise their wage, they can hire more workers for the same amount of direct supervision, since the higher wage makes shirking more costly.

The first model is attractive because it implicitly speaks to a broader function of the minimum wage that is not discussed in the book but that is clear from the historical record. The minimum wage was intended to outlaw certain types of employ-

ment, typically called sweatshops. It was intended to force firms from one cluster of human resource practices to another. Indeed, the same objective holds for other forms of labor market regulation, and the model provides a rationale for broader public interventions in the labor market than just the minimum wage. The implication is that firms can be forced to offer higher-quality jobs without a consequent loss of employment.

The difficulty is that all the stories described in the foregoing several paragraphs are just speculation; little direct evidence is provided for any of them. For example, despite the central role the fast-food industry plays in the book's argument and the great care that went into the research, no evidence is presented for that industry that the affected restaurants in New Jersey reduced their employment vacancies or in other ways moved to the high-wage/low-recruitment-cost method of operation. Nor is any evidence presented regarding how actual practices in terms of supervision and dismissal threats changed after minimum wages rose. This lack of grounding makes these models, and all of the others, appear simply as clever post-hoc rationalizations. Referring to our understanding of internal labor markets, George Baker and Bengt Holmstrom (1995) recently wrote, "At this point there is hardly any feature . . . that cannot be given some logical explanation using the right combination of uncertainty, asymmetric information, and opportunism." Evidently, the same can be said of low-wage labor markets.

Equally troubling, the perspective in the models that are presented, as well as in CK's suggestions for future research, is truncated. The models and the future research suggestions are based on an individualistic view of the labor market, that is, on ideas of firms optimizing vis-à-vis workers, who in turn behave as isolated individuals. There is no sense in any of the models of the group or social aspects of the labor market, even though several of the authors' findings—such as the idea of a "just" wage and the importance of maintaining the relative wage structure—point in this direction. The

earlier institutional work, research that CK's evidence shows was both correct and as convincing and high in quality as any that followed, strongly supported the idea that the labor market is in part a social institution. CK embrace part of this tradition, the idea that firms have wage policies, but they shy away from the more challenging implications of the older research and their own findings.

CK have accomplished a great deal in this book, and it is not reasonable to expect them also to construct a convincing understanding of labor markets when none is to be found in the literature. But it is notable that when they turn to the professional corpus to explain their findings all they find are parables, and that, despite the mathematical sophistication with which some of those parables are presented, they are virtually identical to the ideas provided us by Reynolds, Lester, Dunlop, Kerr, and other researchers 40 years ago. The deep question posed by this book is why so little progress has been made.

As everyone knows, economists are the most arrogant of social scientists. They are eager to claim that they do not do "journalism" (by which they mean mere story-telling) or "sociology" (their code for study of irrational or otherwise inexplicable behavior). Instead, they explain, they have a core or canonical model that captures essential features of the world, and in their daily work they develop clear and useful elaborations of that model and then rigorously test these ideas. Yet the lesson of this book is that an economist, when asked about the effect of the minimum wage, must first of all report that most past research has been seriously flawed in a somewhat unseemly haste to assure that the results conform to the theory. Beyond this admission, the economist must reply that we have no generally applicable model of the labor market. The best that can be said is that different firms will respond in different ways. Some will move up a demand curve and cut employment, others will experience a shock effect and maintain or expand employment, others will shift to a low turnover/low vacancy regime, hence maintaining employ-

ment, and still other firms will continue doing business as before but simply pay the new minimum because it seems fair. We have no systematic explanation of why some firms choose one course over another, we cannot explain the relative proportions, and hence we cannot really make reliable predictions.

It is not easy to prescribe how the profession should change its orientation to produce a richer body of knowledge. Certainly, we should not give up the sophisticated techniques that CK use so well, nor should we pronounce all model-building a waste of time. However, it does seem appropriate to emphasize other research

styles, such as field work, to value inductive reasoning based on direct observation, and to search for models whose implications can be directly tested instead of relying on indirect support. The profession needs to be more catholic in terms of the kind of research it values and the career incentive structures it offers young scholars.

Although this book raises very sharp questions about the practice of labor economics, the book itself is terrific. CK's creative, careful, and above-the-board empirical work is a model of how to do good believable research, and this book will be influential for a long time.

### Comment by Finis Welch\*

The book will interest everyone involved in the minimum wage debates, and it will cause economists to question seriously the models they use and how they do empirical research.

Ronald G. Ehrenberg, Cornell University  
(Princeton University Press flier announcing the book)

Ron is right. I question David Card and Alan Krueger's models and how they do empirical research. Although the notoriety surrounding *Myth* suggests important conclusions that challenge economists' fundamental assumptions, I am convinced that the book's long-run impact will instead be to spur, by negative example, a much-needed consideration of standards we should institute for the collection, analysis, and release of primary data.

With very few exceptions, labor economists have been content either to rely on introspection and logic alone to draw their conclusions, or to analyze publicly available secondary data for which collection methods are well documented. Although the collection and analysis of primary data is the sum and substance of economic his-

tory and experimental economics, it is new to us. Two of the studies in this book derive from the authors' own surveys. As a profession, we and especially our peer-reviewed journals must develop standards for the description and release of such data, and it might even be useful to establish guidelines for the collection of surveys. Especially rigorous standards may be needed when one finds results that contradict widely held opinions. It may be good short-run debate strategy to announce one's results loudly and then attack critics and the existing literature as inept or inconsistent, but the research would have a more durable impact if the time and energy devoted to defending it were devoted instead to examining alternative interpretations. *Myth's* primary argument is that increases in minimum wages do not reduce employment. If this result is robust, the authors can only gain from critical review.

*Myth* is built around four articles. Three of them—two authored by Card alone, and one co-authored by Larry Katz and Alan

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Krueger—were published in the October 1992 issue of this journal. The fourth, concerning fast-food restaurants in New Jersey and Pennsylvania, was published in the *American Economic Review* (*AER*). It was co-authored by David Card and Alan Krueger. The Katz/Krueger paper about fast-food restaurants in Texas and the Card/Krueger paper rely exclusively on telephone surveys collected by or under the direction of the authors. Both papers provide only a brief description of the survey methodology. Copies of the analytical data file have been made available along with a copy of the questionnaire for the New Jersey/Pennsylvania survey. To my knowledge, there has been no public release of any part of the Texas Survey.<sup>1</sup>

My review concentrates on the New Jersey/Pennsylvania paper. There is little to say about the Texas survey paper. I have been unable to get the data, and the authors' analysis is inconclusive. I discuss David Card's two studies only briefly.

### The New Jersey Study

The 1989 Amendment to the Fair Labor Standards Act was debated in Congress in 1988 and signed into law in November 1989. It called for an April 1, 1990, increase in the federal minimum from \$3.35 to \$3.80 per

hour, to be followed by an April 1, 1991, increase to \$4.25. In February 1990, before the first of the two federal increases took effect, the New Jersey legislature passed a bill calling for an April 1, 1992, increase in the state minimum wage to \$5.05. The Card/Krueger study uses Pennsylvania, where the minimum remained at the federal level of \$4.25, as a control for measuring employment effects of the New Jersey increase in the minimum wage.

David Card's two earlier papers rely on the Outgoing Rotations File of the Current Population Survey (CPS) to infer effects on teen employment of other increases in the minimum wage, so it is instructive to compare Pennsylvania and New Jersey using the same CPS data before we examine the alternative data used by the authors. In 1988, when the federal increases were just beginning to be discussed and before the New Jersey debates, the teenage employment rate in Pennsylvania was 45.8%; it was 44.9% in New Jersey.<sup>2</sup> In the year following New Jersey's minimum wage increase, teenage employment in Pennsylvania was 43.6%, within sampling error of the 1988 level.<sup>3</sup> In contrast, the teenage employment rate in New Jersey had fallen to 34.5%—seventh lowest among U.S. states.<sup>4</sup>

The author's study is based on a telephone survey of fast-food restaurants from four chains: Burger King, KFC, Roy Rogers, and Wendy's. The first wave of the survey in late February and early March 1992 was immediately before New Jersey increased its minimum to \$5.05—although the increase had been on the books for over two years. The second wave was conducted 7–8

<sup>1</sup>In preparing for this review, I sent letters to each author of each paper (so that Alan Krueger got two) requesting copies of the questionnaire, interviewer instructions, coder instructions, full machine-readable data file of responses, and analysis file (that is, the edited file) that was used for the paper. Later, I telephoned both Card and Krueger and asked for the same material plus anything they had on pre-test results for the survey instrument.

David Card responded that he had sent me the questionnaire and analysis file (which he had) and that he did not have the other materials. Regarding the New Jersey/Pennsylvania study, Alan Krueger said he believed they had sent me all that was required. Regarding the Texas survey for the paper with Larry Katz, Krueger protested that he would get me everything I requested when he got the chance. When I indicated my deadline for this review, he said that would be too soon. Evidently, Larry Katz did not receive my letter. The Katz/Krueger paper indicates in text and a footnote that the data and questionnaires are available on request.

<sup>2</sup>As with Card's earlier studies, teenagers include men and women ages 16–19. The employment rate calculations use the CPS Earner Study sample weights. Since each of the wage increases occurs on April 1, years are defined as the 12 months beginning on that date.

<sup>3</sup>The change in Pennsylvania's employment rate is –2.2% and the standard error of the change is 2.3%; the 1988 and 1992 levels are less than one standard deviation apart.

<sup>4</sup>The states with lower teen employment in 1992 are Washington, D.C., Mississippi, West Virginia, New York, Louisiana, and Arizona.

months after the increase in November and December of 1992.

The baseline survey covered 410 establishments with completed interviews (331 in New Jersey and 79 in Pennsylvania), and the authors report an 87% completion rate on the first wave. The second wave had a reported 90% completion by telephone, leaving 39 nonresponding establishments, which were subsequently contacted directly. The direct contact showed that 10 of the nonrespondents were closed (some temporarily, for repair and renovation, and others permanently) and of the 29 remaining, 28 consented to personal interviews.

The survey was recently challenged by the Employment Policies Institute (EPI) in an editorial by Richard Berman in the *Wall Street Journal* and a supporting background paper, "The Crippling Flaws in the New Jersey Fast Food Study." The first part of the challenge (citing employment levels at each of the two survey waves for a number of stores) is that on the surface the data are incredible. The second part refers to an attempt to match the survey data with payroll records. In a companion paper by David Neumark and William Wascher, the survey and its analytical results are challenged by comparing the payroll records with the Card/Krueger employment tallies. After reviewing the survey instrument and the analytical data file from the Card/Krueger Survey, I agree with the EPI criticism. The numbers are incredible.

It is not clear that the interview process was formalized. There has been no response to my requests for anything regarding interviewer instructions and training, coder instructions, or pre-test results. The two key questions, concerning wages and employment, invite inaccurate responses.

Immediately after the introduction the employment quiz begins:

Q1. How many full-time and part-time workers are employed in your restaurant, excluding managers and assistant managers?

Q2. And how many managers and assistant managers?

The interview form has two spaces for

the answer to the first question (full-time and part-time) and one space for the second. Question 1 makes the assumption that assistant managers cannot be part-time; it fails to define part- and full-time; and its compound nature probably confused respondents.<sup>5</sup> The wage questions are no better:

Q4. What is the average starting wage for a nonmanagement employee at your restaurant today?

Q5. Is it the same starting rate for full-time and part-time workers?

Question 4 has two blanks in which an interviewer would write a response. The first blank is for a numeric response and the second is for "minimum wage" so that the coder can fill in the applicable value. Question 5 has two spaces, one for part-time and the other for full-time. Although the second wage question appears to have been intended to draw two responses, many might believe a simple "yes" or "no" would suffice. In any case, reviewers cannot examine the coded responses to Questions 4 and 5 for evidence of the confusion one expects them to elicit, because the "analysis" file that is distributed contains only one starting wage for each survey wave with nothing to indicate whether it is taken from Question 4 or Question 5.

Among the 79 survey records shown for Pennsylvania, 68 have valid wage records in each of the two waves. At the baseline interview, 22 of these 68 are coded as having a starting wage of \$4.25 (the applicable minimum), while the remaining 46 had starting wages ranging from \$4.35 to \$5.50 per hour. By the time of the second wave, 13 of the 22 (59%) that initially paid \$4.25 had increased their starting wage. It may not be surprising that starting wages increased, but would it be surprising if they fell? Among the 46 who initially paid more than the minimum, who therefore could reduce wages without violating federal law, 27 (also 59%) are coded as having done so

<sup>5</sup>The questionnaire has no interviewer check items nor any directions regarding responses to questions by store representatives.

by the time of the second interview! Among the 331 records for New Jersey, 302 have valid starting wage observations for each survey wave and 23 of them had first wave starting wages above \$5.05 per hour. They are the only ones that could lower the starting wage without violating New Jersey law. The analysis files show that 19 of the 23 (83%) lowered their starting wage and 18 of the 19 lowered the starting wage to the second wave minimum of \$5.05 per hour!

It is tempting to argue that the fact of falling wages is itself proof that the increased minimum reduced employment opportunities among firms that would otherwise pay wages below the minimum. The "proof" is that those who would otherwise pay more can, with the increased minimum, find an adequate supply of labor at a reduced wage. Of course, the alternative to this argument is that there is so much random noise in the data that they should be dismissed altogether. The employment data support the second view.

Before examining patterns of changes in employment, it is worth pointing out that the coded levels of employment are anomalous. In particular, in the coded responses to the baseline survey, there are 28 records that report fractional full-time employees (the fraction is always one-half), 29 records that report fractional numbers of part-time employees, and one record that reports a fractional number of managers.<sup>6</sup> One can imagine a case where part-time employees work 20 hours per week such that someone who works 10 hours is one-half of a part-timer from an hours worked perspective. Even in this case, it is hard to imagine what is meant by one-half of a full-time employee. Perhaps someone who works 60 hours is a

full-timer and a half. These alternative interpretations suggest that full-time and part-time are defined and there is no indication that they are.

Recall that the employment question begins "How many full-time and part-time...", making it unclear whether the interviewer is asking for the combined or separate count of part- and full-time workers. Among the 378 stores that were open and have valid responses to the employment questions in both survey waves, there are 67 coded as having no full-time employees (other than managers) in the first wave. Thus 18% of the stores have no full-time employees at the time of the baseline interview. Strikingly, 46 of the 311 (15%) that initially had full-time employees reportedly had none by the second interview, 7–8 months later. Conversely, 47 of the 67 (70%) that initially had no full-time employees are coded as having added them by the time of the second interview. The magnitude of these swings is not trivial. Employment averaged a little over 21 workers in both interviews.<sup>7</sup> Among stores that lost all full-time workers, the average loss was 10.7. Among stores that initially had none and are coded as having added them by the second interview, the average gain was 10.4. Is the technology so flexible and in such rapid flux that these numbers are unremarkable, or are the coded responses dominated by error?

Among the 378 stores with valid non-zero employment in both survey waves, average first wave employment is 21.14 and average second wave employment is 21.27. The trivial difference in averages between survey waves masks astonishing changes within stores. The largest gain is 34 employees, the largest loss is 41.5, and the standard deviation of the change is 9.0 employees! In examining changes in employment between the two survey waves it is important to recall that noise-dominated data regress toward means.

When the first wave data are arrayed by

<sup>6</sup>Noting this anomaly, I asked Alan Krueger (over the phone) how it could be explained. He seemed surprised that such observations exist and suggested that in cases where the respondent was unsure whether particular employees were part-time or full-time they (meaning him, his coauthor, or assistants) may have divided them 50/50 between part- and full-time. Of the 48 records with fractional employees, when summed over types, only 10 sum to integer numbers of employees as they would if some number of employees was divided between part- and full-time.

<sup>7</sup>Following Card and Krueger, employment is full-time equivalence with part-time workers counted as one-half.

full-time equivalent employment, we discover an amazing fact. The 21 restaurants with the smallest employment averaged only 7.5 employees at the time of the baseline survey. In the interval between the first and second interviews, employment increased for *every one* of these restaurants, with an average gain of 5.4 employees—a 71% increase in 7–8 months. At the opposite extreme, the 22 largest restaurants averaged 47.4 employees at the baseline and employment fell for *every one* of them in the following 7–8 months, with an average loss of 19.8 employees—a 42% decrease. As with the changes in wages, we could invent an esoteric theory to explain the great equalization in firm size that apparently occurred in New Jersey and Pennsylvania in a very brief period.<sup>8</sup> Or we could simply note that independently of state and survey wave effects, the correlation between first and second interview employment is only 0.552—an astonishingly low value for two surveys administered in the same year.

Are these data of the kind that you would recommend as the basis for national policy?

### The Texas Study

This is the study that relies on the survey of 100 fast-food restaurants in Texas. I have been unable to secure the data on which it is based and, therefore, have little to say about it.<sup>9</sup> Perhaps the most interesting feature is that in regressions attempting to explain employment changes one cannot reject the hypothesis that all estimated coefficients are jointly zero. In other words, the study is uninformative. This raises a fundamental point that is stressed in every introductory statistics course ad nauseam,

<sup>8</sup>Certainly if the phenomenal wage and employment equalization is real, the story for fans of industrial organization is much more exciting than the null result for minimum wage effects.

<sup>9</sup>In comparison to the reported response rates for the New Jersey/Pennsylvania survey (87% first wave; 90% second wave before personal interview), the response rates for the Texas survey appear unusually low (57% on the first wave and a 66% second wave response). It appears that the completed two-wave survey included less than 4 in 10 of those initially contacted.

a point that needs to be borne in mind in evaluating all of the Card/Krueger studies summarized in *Myth*: an inconclusive result does not prove there is no effect.

### The Two Card Papers

David Card's first paper examines teenage employment responses to the 1988 increase in California's minimum wage. Using a control or comparison group of "comparable" states—Georgia, Florida, New Mexico, Arizona, and Dallas/Ft. Worth—where the minimum did not change, he concludes that the wage hike in California did not reduce teenage employment. Georgia? New Mexico? Dallas?

When the minimum wage was increased, California's teenage employment fell only briefly and then rebounded above previous levels. The problem with attributing the employment increase to the wage hike is that California's economy was expanding while the comparison areas were stagnant.

Each of the four cornerstone papers in *Myth* calculates the cost of an increase in the minimum wage by calculating what it would cost to raise the wages observed before the increase to the level of the new minimum. California raised its minimum from \$3.35 to \$4.25 per hour on July 1, 1988. Using the same CPS data as Card and California teenage wages in January-March of 1988, I find that in order to increase every wage in the \$3.35–\$4.24 range to \$4.25, the average wage would have increased by 4.33%. When the exercise is repeated for April-June the increase is 5.68%.

Since wages between \$3.35 and \$4.25 continue to be reported after July 1, I repeated the exercise for the last two quarters of 1988. In July-September the average would have increased 1.21%, and for the final quarter, the average wage would have increased 1.04% by raising wages in the \$3.35–\$4.24 range to \$4.25. I do not know whether the below-minimum wages that are reported result from non-compliance or from measurement error, but I do know that the after-the-fact calculations estimate the exaggeration in the before-the-fact cal-

culations. Subtracting the cost increase calculations for July–December from the calculations for January–June results in an estimate that the increase in the minimum wage in California raised the average cost of hiring teenagers by 3–4.5%. In fact, we see far larger quarter-to-quarter changes in average wages when the minimum is held constant. For example, the average teen wage fell 6.5% between the first and second quarters of 1988 when the minimum was \$3.35 per hour, and it fell 9.3% between the third and fourth quarters when the minimum was \$4.25 per hour. Two points are relevant. First, in an expanding economy the employment response to an exogenous increase in costs of 3–4.5% may be hard to detect from a relatively small sample of the population. Second, labor markets are dynamic and wages fluctuate in response to many factors, especially for teenagers, who are predominantly students. There are seasonal shifts in supply and demand; there is trend and cycle; there is composition—today’s working teens are not the same as tomorrow’s, who may be either more or less productive; there is sampling error since we observe a small fraction of workers; and, finally, there is measurement error. The quarterly fluctuations in average wages indicate that the background noise in wages may dominate the signal of the higher minimum.

The simple before-and-after comparisons of the California study are interesting but their reliability is at best conjectural.

The second Card study examined differences across states in changes in teenage employment surrounding the 1990 increase in the federal minimum wage. According to Card, if increases in the minimum reduce employment, it follows that the reductions should be greatest in the states with the lowest wages, because compliance with the increased minimum is more expensive in those states. Donald Deere, Kevin Murphy, and I (1995) criticized this idea by pointing out that the low-wage states are in the South and Southwest, where relative employment growth was most rapid. The 1990 increase in the federal minimum was only half as large as the 1988 increase in

California, and the employment responses to it were probably proportionately smaller. Is it surprising that the increase did not reverse the regional patterns of employment growth that have become so familiar in the past few decades?

### Concluding Comments

My review has concentrated on what I see as the primary weaknesses of the studies that form the foundation of *Myth*. The twin issues are data quality and the “experiments” being reviewed. I have avoided the purely economic issues, but they ought to be mentioned.

First, two of the four studies are restricted to the fast-food industry. Nothing, repeat nothing, in ordinary competitive theory predicts that employment in any given industry or in any given restaurant will decline in response to an increase in the minimum wage. The questions revolve around factor intensities. Suppose that Chinese and Mexican restaurants are low-wage labor-intensive relative to fast-food chains, like those of the Katz/Krueger and Card/Krueger surveys, that specialize in fried chicken and hamburgers. Suppose also that Chinese and Mexican food are consumer substitutes for hamburgers and fried chicken. In this case, an exogenous increase in the cost of low-wage labor will raise the cost of Chinese and Mexican food relative to the cost of hamburgers and fried chicken. If the consumer substitution between restaurant foods swamps factor substitution within fast-food restaurants, the demand for low-wage labor in fast-food restaurants will increase in response to an increased wage.

The same point, in somewhat different clothes, holds for firms within an industry. Assume that the technology of fast-food production is such that larger firms are less intensive (that is, have smaller expenditure shares) in low-wage labor than are smaller firms. In this case an exogenous increase in the cost of low-wage labor increases the size of the most efficient firms. If all firms are identical, the output of every firm increases. Numbers of firms will fall, but numbers can

fall because some existing firms close or because some that would otherwise open do not. It is hard for even well-designed surveys to identify those firms that otherwise would have opened. Finally, there is the “dynamic monopsony” theory offered by the authors. It, frankly, does not pass the straight-faced test. I live in a college town that has several dozen fast-food restaurants, but only one Wendy’s. Do Messrs. Card and Krueger really believe that if Wendy’s increased its employment, say, by adding another two or three part-time students, that would raise the wage that I have to pay to have my lawn mowed? I direct their attention to the “mono” in monopsony. Just how many fast-food chains are there? And what is their share of total low-wage employment, singly and en masse?

Returning to the four cornerstones of *Myth*: the Texas study can be dismissed. There are no conclusions to examine. The New Jersey study is a monument to poor survey methodology. The two studies by David Card use control groups that are questionable—and, I think, misleading—and the “experiments” have little “bite.” David Card’s two papers and the Katz/Krueger paper appear in this journal (October 1992) as part of a symposium organized by Ronald Ehrenberg and Alan Krueger. The New Jersey/Pennsylvania

Survey is published in the *American Economic Review* as testimony to the vagaries of the review process.

As I indicated in the introduction, the importance of *Myth* lies in the issues it raises. There is really no question about surveys; they ought to be planned carefully and conducted systematically. Questions should be clear and concise. Interviewers should be trained and armed with check item questions to clarify ambiguous responses. Contingencies should be considered and reactions to them should be standardized and committed to writing. Finally, methods and procedures should be distributed for critical review. There is, however, a question as to the use of primary data. I have always thought that our reliance on generally available secondary data is an important safeguard. It is clearly dangerous to the science if we each have our own “pocket” surveys that reportedly contain earth-shattering results.

Finally and most important, there is the confusion between an inconclusive result in a statistical experiment and “proof” that the answer is indeed inconclusive. When we return empty-handed from a long search, it is tempting to announce that there is no treasure in “them thar hills,” but the only proof that we have to offer is that we did not find it.

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