Chapter 32

MINIMUM WAGES, EMPLOYMENT, AND THE DISTRIBUTION OF INCOME

CHARLES BROWN*

University of Michigan and NBER

Contents

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Abstract

After nearly a decade of relative quiet, the increases in the US minimum wage that began in 1990 have coincided with a renewed interest in its effects. Recent work suggests that a relative consensus on the effects of the minimum wage on employment came undone; on balance, however, the recent estimates seem if anything smaller than those suggested by the earlier literature, and the puzzle of why they are relatively small remains. Effects of the minimum wage on the wage distribution became clearer with the declining real minimum wage in the 1980s; nevertheless, the ability of minimum wages to equalize the distribution of family incomes remains quite limited.

JEL codes: J38; J23; D31; D33

1. Introduction

The effects of the minimum wage on employment and the distribution of income have been hotly debated policy questions for over 50 years. By the early 1980s, research on the effects of the minimum wage in the US began to show signs of consensus (Eccles and Freeman, 1982) – relatively modest effects of the minimum wage on employment (of teenagers who were most likely to be directly affected), and on the distribution of income (because many minimum wage workers were members of middle-income families). It was tempting to conclude, to borrow Henry Kissinger's analysis of academic politics, that the minimum wage debate was so spirited because the stakes were so low. Recent research has suggested the employment effects might be larger, or non-existent, at least for increases over the observed range. Other research has asked whether the growing inequality in the distribution of adult wages has strengthened the link between minimum wages and distributional objectives. The purpose of this chapter is to evaluate the evidence, old and especially new, on these topics. The main focus is on the US experience; minimum wages elsewhere are often intertwined with other institutions, such as unemployment transfers and collective bargaining (Dolado et al., 1997) and this complicates both the analysis of such laws and a proper evaluation of those analyses.

The next section reviews the theory that links minimum wage increases to employment; Section 3 describes historical patterns in the level of the minimum wage and of expanding coverage; the next five sections discuss empirical research on the effects of the minimum wage on employment and other employment-related outcomes. Next, we turn to the literature on the minimum wage and the distribution of wages and of income. Finally, we offer some tentative conclusions and attempt to identify themes for future work.

2. Theory

2.1. Basics

The simplest model of the effects of the minimum wage is one with complete coverage, homogeneous labor, and a competitive labor market. Instead of the familiar equilibrium where the demand for labor $D(w)$ is equal to the supply of labor $S(w)$ at equilibrium wage w^* and employment E^*, a binding minimum wage ($w_m > w^*$) leads to demand-determined employment $E_m = D(w_m)$ and an excess supply of labor $S(w_m) - D(w_m)$ (Fig. 1). Since we are simply moving back along the demand curve, the employment loss $\ln(E_m) - \ln(E^*)$ depends only on the elasticity of demand for labor and the gap between the minimum wage and the competitive wage, $\ln(w_m) - \ln(w^*)$.

Whether this excess supply of workers is counted as unemployed or as "discouraged" workers depends on whether they report searching (unsuccessfully) for work, so one needs further assumptions about labor force participation (in the presence of unemployment) to say much about the effects on unemployment. One plausible assumption is that workers decide whether to participate in the labor force based on the probability of being employed ($D(w_m)/S(w_m)$) and the wage if successful (w_m), perhaps on their product, the expected wage.[1]

The increase in measured unemployment seems a poor indicator of the costs of the minimum wage; the effect on unemployment will be small if workers are easily discouraged and withdraw from the labor force. In fact, Mincer (1976) and Wessels (1980) model labor force participation as a function of the expected reward from participating; declining labor force participation (which would minimize "unemployment effects") signals that the minimum wage has made participation less attractive.

Fig. 1 serves as a general guide to both the short- and longterm effects of a minimum wage, but the presumption is that demand is more elastic in the long run, as substitution of other factors for the more expensive labor becomes possible.

[1] Both Gramlich (1976) and Mincer (1976) make this sort of assumption, although in the context of more complicated two-sector models.

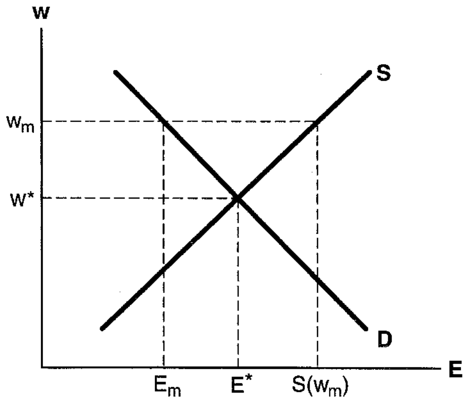

Fig. 1. Minimum wage with complete coverage.

2.2. Two-sector models

Historically, minimum wage laws in the US have not applied to all employers, with exemptions based on industry and size. As discussed in more detail in Section 3, coverage of the law has expanded gradually. Compliance with the law is not perfect; Ashenfelter and Smith (1979) argue non-compliance is important, and this increases the de facto size of the uncovered sector. Given that time series analyses have used data from periods with different levels of coverage, it is helpful to ask how our conclusions change under partial coverage. It will turn out that an uncovered sector may dilute but not eliminate the negative effects of the minimum wage on employment.

Demand for labor in the covered sector $D^c(w_m)$ depends on the minimum wage; demand for labor in the uncovered sector $D^u(w_u)$ depends on the (market-determined) wage in that sector. In the absence of a minimum wage, workers earn $w*$ in both sectors, and

$$S(w*) = D^c(w*) + D^u(w*).$$

For simplicity, normalize employment so that $E* = 1$, and wages so that $w* = 1$. Then $D^c(w*)$ is equal to c, the fraction of the market employed by covered employers prior to the minimum wage, and $D^u(w*) = 1 - c$.

Modeling supply is more difficult once the minimum wage is introduced, however, since there are two different wages that might influence supply, and not all those willing to work at the higher of these wages will be able to find work.

Welch (1976) assumes that the $D^c(w_m)$ available positions in the covered sector are allocated randomly among the $S(w_m)$ workers willing to work at the minimum wage; $f = D^c(w_m)/S(w_m)$ is the probability that each will succeed. Because $w_m > w*$, $f < c$. The uncovered-sector wage w_u then equates the supply of workers willing to work at that wage who have not already been hired in the covered sector with uncovered-sector demand; i.e.,

$$(1 - f)S(w_u) = D^u(w_u).$$

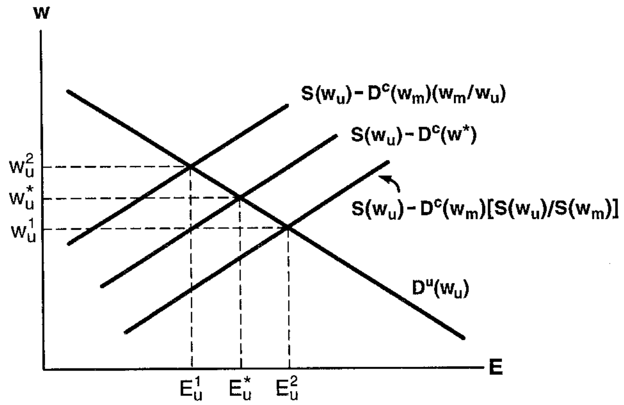

Fig. 2. Minimum wage with an uncovered sector.

This can be rewritten as

$$D^u(w_u) = S(w_u) - D^c(W_m)[S(w_u)/S(w_m)].$$

Notice (Fig. 2) that at $w_u = w^*$, there is excess supply (since $D^c(w_m) < D^c(w^*)$ and $S(w^*)/S(w_m) < 1$), and so the wage in the uncovered sector must fall (to w_u^1). Total employment is less than employment in the absence of the minimum wage: the increase in uncovered-sector employment only partially offsets the loss in the covered sector.[2]

Gramlich (1976) and Mincer (1976) assume that workers *choose* one sector or the other, and in equilibrium expected wages must be the same in both. In the simplest versions of their models[3], this means that $w_u = Pw_m$, where P, the probability of finding work in the covered sector, is $D^c(w_m)/[D^c(w_m) + U]$, and U is the number of unemployed. Since returns to participation in each sector are the same, supply is a function of just w_u (or, equivalently, of Pw_m). It is then easy to show that

$$U = D^c(w_m)[(w_m/w_u) - 1].$$

The uncovered wage must then clear the market:

$$D^u(w_u) = S(w_u) - D^c(w_m) - U = S(w_u) - D^c(w_m)[w_m/w_u].$$

In this model, the wage in the uncovered sector may either rise or fall (although if it falls, it does so by less than on Welch's assumptions, because the term that multiplies D^c is less

[2] To see this, note that the horizontal distance between the two supply curves at w^* is less than the loss of employment in the covered sector (since some have reservation wages above w^*), and the increase in employment in the uncovered sector is less than the horizontal distance between the two supply curves at w^*.

[3] Gramlich allows those who choose the covered sector but do not find a job to receive unemployment benefits; Mincer considers the possibility that new entrants to the covered sector are less likely to be employed next period than those already employed (so that job-finding chances depend on turnover). In the simple version of the model discussed here, unemployment benefits are ignored and there is complete turnover of jobs each period.

than one for Welch, greater than one for Gramlich–Mincer). In Fig. 2, w_u rises to w_u^2. Total employment falls in either case, and by more than in Welch's model.[4]

The Welch model assumes workers can work in the uncovered sector if they search unsuccessfully for work at w_m, while the Mincer and Gramlich models assume the worker chooses one sector or the other. The idea that workers much choose one sector or the other seems less plausible in the US than in a developing country (where the covered sector is urban, and the uncovered sector rural, as in Todaro (1969)). Brown, Gilroy and Kohen (BGK) (Brown et al., 1982, p. 492) suggest a modification of the Gramlich–Mincer model that allows those working in the uncovered sector to search for covered employment, but with lower probability of finding covered employment than those who search for such work full time. As the relative efficiency of search while employed in the uncovered sector increases, both the employment loss and the increase in unemployment due to the minimum wage are reduced.

The preceding analysis assumes that the wage in the uncovered sector is flexible, and so free to adjust to a minimum wage in the covered sector. If, on the other hand, w_m is the federal minimum wage in a state with its own lower minimum wage for small employers not covered by the federal law, it might be more appropriate to think of the "uncovered" sector as those employers subject to the state minimum. In this case, w_u would not adjust to the imbalance between demand and supply in the uncovered sector.

The Welch and Gramlich–Mincer models present uncluttered analyses of the uncovered sector; they abstract from capital reallocation across sectors and changes in relative prices of covered- and uncovered-sector output. With uncovered-sector employment held fixed, the proportional change in employment due to a change in the minimum wage is simple and intuitive, $c\eta\Delta\ln(w_m)$. But once changes in uncovered-sector employment are taken into account, neither model leads to particularly tractable functional forms for the change in total employment (Brown et al., 1982, pp. 491–492). As a result, the empirical literature is only loosely related to these formal models (for an exception, see Abowd and Killingsworth, 1981).

2.3. *Heterogeneous labor*

We expect minimum wages to affect the employment of relatively unskilled workers, and potentially to have indirect effects on those who are better paid. But even if we are not interested in the better-paid group directly, there is no observable skill indicator that neatly divides workers into those whose wage depends directly on the minimum wage and those

[4] If $w_u > w^*$, employment falls because wages in both covered and uncovered sectors have increased, and so less labor is demanded in each. If $w_u < w^*$, the labor force is smaller ($S(w_u) < S(w^*)$) than before the minimum wage, and some workers are unemployed, so that employment $S(w_u) - U$ is less than in the absence of the minimum wage $S(w^*)$.

[5] We cannot use the worker's wage directly, of course, because that wage may change when the minimum wage does. Even without a change in w_m, wages of those paid the minimum wage in one year may be very different one year later (Smith and Vavrichek, 1992).

who earn more.[5] Hence in any "low-wage" group such as teenagers, high school dropouts, or fast-food workers, there will be a mixture of directly affected and better-paid workers. In a sense, the better-paid workers are an uncovered sector, but those displaced by the minimum wage do not have the opportunity of moving there.

An increase in the minimum wage raises the price of relatively unskilled workers, and makes inputs that are good substitutes for such workers more attractive. Workers in low-wage groups who earn a bit more than the minimum wage often do the same tasks as their less-skilled co-workers, and are likely to be very good substitutes for minimum wage workers. Changes in employment for the group as a whole reflect the balance of these losses and gains. As long as less-skilled labor is also a substitute for the composite non-labor input, total employment will fall in response to an increase in the minimum wage.[6] But small overall employment impacts may reflect an unattractive balancing of gains by relatively advantaged workers and losses by those directly affected (Abowd and Killingsworth, 1981, p. 144; Deere et al., 1996, p. 35; Freeman, 1996, p. 642).

As long as the minimum wage is set low enough that it affects only a small share of employment, the effect of the minimum wage on total employment is likely to be small and in any case swamped by other factors. Thus, it makes sense to focus on the analysis of low-wage groups, where the proportion directly affected is larger and so the anticipated effect on group employment is likely to be larger. This explains the dominance of teenagers as the group most studied in the empirical work. The same line of argument leads us to expect larger (proportionate) effects on teenagers than on young adults, and larger proportionate effects on employment of black and female teenagers than on employment of white male teens.

While recognizing that not all workers are directly affected by the minimum wage is a step in the right direction, a more satisfactory model would allow for a continuous distribution of worker skill. The simplest model of this type has one type of worker skill, and each worker's wage is equal to the price of skill times the worker's endowment of skill. Thus, in the absence of the minimum wage, the wage distribution reflects the distribution of skill. Once a minimum wage is introduced, those whose value of marginal product is less than w_m are no longer employed (Kosters and Welch, 1972). As fewer workers are employed the price of skill rises, and those whose wage was just below w_m are once again employable. As we shall see in Section 8, however, observed wage distributions are not simply truncated at the minimum wage; while relatively few workers are paid less than w_m, there is a pronounced spike in the wage distribution at w_m. Heckman and Sedlacek (1981) and Pettengill (1981, 1984) provide more detailed models with continuous distributions of worker ability that take account of the effect of reduction in low-skill employment on the rest of the wage distribution. Grossman (1983) suggests that relative-wage comparisons by workers may also lead employers to raise wages of workers already paid more than the minimum.

[6] Even if more- and less-skilled workers are perfect substitutes, overall employment falls since it takes less than one skilled worker to replace each minimum-wage worker.

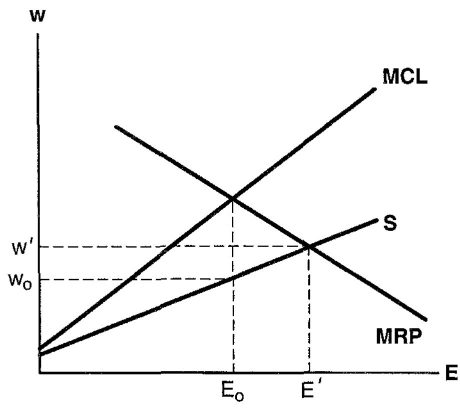

Fig. 3. Minimum wage under monopsony.

2.4. Monopsony

Although they are not, in the end, intended to believe it, undergraduate students are exposed to the possibility that a "skillfully set" minimum wage increases employment under monopsony.

The monopsonist faces an upward-sloping supply curve for labor, and so seeks to maximize π, the difference between revenue R and labor cost:

$$\pi = R(L) - w(L)L.$$

Choosing the profit maximizing level of employment yields

$$R'(L) - w(L) - w'(L)L = 0,$$

which implies the marginal revenue product of labor, R', is equal to $w(1 + 1/\varepsilon)$, where ε is the elasticity of labor supply.

A minimum wage makes the supply of labor perfectly elastic up to $S(w_{\mathrm{m}})$, and as long as $w_{\mathrm{o}} < w_{\mathrm{m}} < w'$, raising the minimum wage moves the equilibrium rightward along the supply curve, increasing employment (Fig. 3). Further increases in w_{m} beyond w' move the equilibrium along the marginal revenue product curve. Note, however, that even a clumsily set minimum wage can leave employment higher than in the monopsonistic equilibrium, as long as $(w_{\mathrm{m}}/w_{\mathrm{o}}) < 1 + (1/\varepsilon)$.

How much the wage can be raised under monopsony before employment starts to fall thus depends on the elasticity of labor supply. The consensus view has been that the typical minimum-wage employer is not a mining company in an isolated company town but a retail trade or service employer in a labor market with many such employers. The elasticity of labor supply to any one such employer should therefore be "close to" infinite, and the opening for skillfully set minimum wage negligible.[7] Moreover, as Stigler (1946) argued, the fact that w' varies among employers while w_m is uniform makes it less likely that most employers affected by the law will be in the employment-enhancing range.

2.5. Search models

Card and Krueger (1995, pp. 373–379) suggest another interpretation of the monopsony model to re-establish its relevance for actual minimum-wage markets. They present a model that focuses on turnover behavior, implicitly linked to search behavior by workers and firms. In any relatively short period, the quit rate q depends on the wage, as does the number of workers who apply to and are hired by the firm H. Equilibrium requires that quits ($= q(w)L$) per period equal new hires, $H(w)$. This means that equilibrium employment is equal to $L = H(w)/q(w)$; since $H' > 0$ and $q' < 0$, if the firm wishes to increase employment it must raise the wage. In effect, $H(w)/q(w)$ is the labor supply function facing the firm. The elasticity of labor supply is then $\theta_H - \theta_q$, where θ_H and θ_q the elasticities of H and q with respect to w. Empirically plausible values of these θs yields an elasticity of labor supply of 5–10, which suggests the range of wages over which minimum wage increases could increase employment is not negligible. I see two problems with this way of rescuing the monopsony model.

First, H is surely a function of L as well as of w; a large retail outlet must get more applicants at any given wage than a mom and pop store in the same area. If we assume new hires are equal to $h(w)L$, equilibrium requires that $h(w) = q(w)$, and the firm can have any level of employment it wants at this wage. If $H = L^{\lambda}h(w)$, the elasticity of labor supply to the firm is now $(\theta_h - \theta_q)/(1 - \lambda)$.

Second, H (or h) and q depend on alternative wages as well as the wage offered by the firm. The elasticity derived in the previous paragraphs shows how supply changes if the firm increases its wage, alternative wages constant. An increase in the minimum wage, however, increases wages elsewhere. With complete coverage, an increase in the minimum wage increases wages at a covered firm and elsewhere (= other covered firms) in the same proportion, and so does little or nothing to increase hires or reduce quits.

Burdett and Mortensen (1998) offer a more formal search model in which search frictions generate a monopsony-like equilibrium, and a minimum wage can increase employment. In their model, employment at any one firm depends explicitly on the wage distribution as well as the wage offered by that firm. However, if many employers are paying w_m, an individual employer has an incentive to pay a slightly higher wage (profit per worker is slightly lower but equilibrium employment significantly higher). Hence, the spike in the observed wage distribution we observe at the minimum wage (Section 8) is not

[7] Rebitzer and Taylor (1995) present an efficiency-wage model in which the wage each firm must pay to deter shirking is an increasing function of firm size. This creates an upward-sloping wage–employment relationship that functions like the upward sloping marginal labor cost function of a traditional monopsony model, but "works" with a large number of employers.

consistent with the model.[8] And, with heterogeneous workers and employers, Stigler's doubts about the ability of a uniform minimum wage to raise employment carry over to search models as well.[9]

2.6. *Offsets*

Thus far, we have implicitly assumed that if the minimum wage increases by 10%, both compensation per hour to minimum-wage workers and cost per hour of minimum-wage labor to the employer increase by 10% as well. However, this need not be the case. Just as mandated improvements in non-wage aspects of a job (health insurance, safety, layoff notification) may lead to lower wages, mandated improvements in the wage give employers an incentive to cut other aspects of the job package. A number of such margins have been suggested–fringe benefits, employer-provided training, and required levels of effort (Wessels, 1980; Mincer, 1984).

To fix ideas, imagine that employers pay \$5 per hour and provide "free" food that costs \$0.50 (per hour worked) to provide and is valued by workers at \$0.50 per hour as well. Then a \$5.50 minimum wage would lead employers to end the free meals, leaving their cost of labor, the compensation received by workers, and employment unaffected. Alternatively, if the \$0.50 of food is valued by workers at \$1.00, eliminating the free food would reduce compensation, and so make it impossible for the employer to maintain the old level of employment; in this case, the free food would be curtailed but not eliminated. With higher labor costs, employers would employ fewer workers; with compensation as seen by workers reduced, less labor would be supplied.

From this perspective, the availability of offsets reduces the attractiveness of minimum wage increases to the workers who are directly affected, but limits the employment loss as well. If, however, employers respond by raising the effort standard they require on the job, employment effects may be *magnified* rather than mitigated. Suppose, for example, a 10% increase in the minimum wage is offset by a 10% increase in enforced effort. Then employment in efficiency units is not changed, but employment in bodies or in hours worked would be reduced by 10%.

More generally, the algebra of effort is discouraging. Suppose that we measure labor in efficiency units, defined as number of workers (or hours) L times effort e. Demand for such efficiency units will depend on the cost per unit of effort; a constant-elasticity relationship would be

[8] Joseph Altonji has noted that the ability of a tiny wage increase to lead to a large increase in employment depends on all employers being equally attractive to workers. If workers care about some non-wage attribute that differs for each worker-employer pair (e.g., commuting costs), tiny wage increases would not bring large increases in employment, and so would not undo the mass point at the minimum wage. I have not found a paper that explicitly models this intuition.

[9] Koning et al. (1995) model both the wage distribution and unemployment durations in an explicit equilibrium search framework. They find small reductions in search unemployment but large increases in structural unemployment due to minimum wage increases for Dutch youth. They do not discuss the spike at the minimum wage, although it appears from their wage histograms that it is not very important in their data.

$$\ln L + \ln e = \eta(\ln w_{\mathrm{m}} - \ln e),$$

which implies

$$\ln L = \eta(\ln w_{\mathrm{m}}) - (\eta + 1)(\ln e).$$

If the elasticity of e with respect to w is α, then

$$\mathrm{d}\ln L/\mathrm{d}\ln w_{\mathrm{m}} = \eta - (\eta + 1)\alpha = (1 - \alpha)\eta - \alpha.$$

Larger values of α make the employment response larger unless demand is elastic; if demand is elastic, the minimum-wage elasticity of employment is less than 1 in absolute value only if α is sufficiently larger than 1.[10]

3. Evolution of minimum wage legislation in the US

In 1938, the Fair Labor Standards Act mandated a minimum wage of 25 cents per hour, or about 40% of the average hourly earnings of production workers in manufacturing. Only about half of production workers were covered, and low-wage sectors (agriculture, retail trade, and services) were largely excluded.

Since then, the nominal minimum wage has been increased at irregular intervals. When a new minimum wage becomes effective, it is typically equal to roughly 50% of average hourly earnings of private workers (closer to 55% in the 1950s and 1960s, 40% in the 1990s) (see Table 1). Moreover, since 1961 the increases have been staggered, with about half of the increase in the year the law was changed, and half in the following year. Between increases in the minimum wage, inflation and real-wage growth increase average hourly earnings by as much as 30–40%, and so reduce the ratio of the (fixed) minimum wage to (rising) average hourly earnings. As a result, the relative minimum wage follows a saw-toothed pattern (Fig. 4).

Coverage expansions have been more discrete, and usually permanent. Coverage remained essentially unchanged from 1938 until extended in 1961, 1967, and 1974 primarily in agriculture, retail trade, and services. Not only was the fraction of workers covered expanded, but the expansions were in relatively low-wage sectors where the law was likely to be a binding constraint. Within industries, coverage was extended based on firm or establishment sales, with each extension sweeping in smaller and therefore lower-wage employers in these industries. For example, at the time the $2.00 minimum wage became effective in May 1974, only 3.7% of workers covered prior to the 1966 amendments were earning less than $2.00; 13.4% of those first covered in 1967 and 18.0% of those first

[10] In Rebitzer and Taylor's (1995) efficiency wage model, workers either shirk or they do not, and in equilibrium none shirk. In a version of their model with continuously variable effort, one might expect effort to increase in response to the minimum wage.

Table 1
Minimum wage levels and coverage[a]

Effective date	New w_m ($)	w_m/ahe	Since last increase		Fraction covered	
			$\Delta\ln w_m$	$\Delta\ln(\text{ahe})$	Private	Government
Oct. 1938	0.25	0.37			~0.50	0
Oct. 1939	0.30	0.43	0.18	0.03	~0.55	0
Oct. 1945	0.40	0.36	0.29	0.48	~0.55	0
Jan. 1950	0.75	0.57	0.63	0.45	~0.55	0
Mar. 1956	1.00	0.56	0.29	0.30	0.55	0
Sept. 1961	1.15	0.53	0.14	0.20	0.63	0
Sept. 1963	1.25	0.54	0.08	0.06	0.63	0
Feb. 1967	1.40	0.53	0.11	0.13	0.77	0.40
Feb. 1968	1.60	0.58	0.13	0.06	0.77	0.40
May 1974	2.00	0.48	0.22	0.41	0.83	1.00
Jan. 1975	2.10	0.48	0.05	0.05	0.83	1.00
Jan. 1976	2.30	0.49	0.09	0.07	0.84	0.28
Jan. 1978	2.65	0.48	0.14	0.15	0.85	0.27
Jan. 1979	2.90	0.49	0.09	0.09	0.86	0.27
Jan. 1980	3.10	0.48	0.07	0.07	0.86	0.27
Jan. 1981	3.35	0.48	0.08	0.09	0.86	0.27
Apr. 1990	3.80	0.39	0.13	0.34	0.87	1.00
Apr. 1991	4.25	0.42	0.11	0.04	0.86	1.00
Oct. 1996	4.75	0.41	0.11	0.14		
Sept. 1997	5.15	0.43	0.08	0.03		

[a] Notes: w_m/ahe, ahe is average hourly earnings, private economy. For years prior to 1947, average hourly earnings were available only for manufacturing. Private economy ahe is estimated as 0.93 times manufacturing ahe, based on the relationship between the two series in 1947–1956. October data interpolated from annual averages. Coverage of private workers: first available coverage ratios are for 1953; 1956, 1961, and 1963 ratios are from 1957, 1962, and 1964 respectively; 1967 and 1968 ratios reflect minor coverage expansion in 1969 as well. Coverage of government workers was reduced by a Supreme Court decision in 1976, which was later reversed.

covered in 1974 were earning less than the new minimum (US Department of Labor, 1975, Table 1).[11]

Coverage of government-sector workers was introduced by the 1966 Amendments and increased to complete coverage in 1975. Coverage fell in 1976, and rebounded in 1985, due to changing Supreme Court decisions.

These patterns have a number of important implications. First, while the minimum wage relative to average hourly earnings varies significantly over the period, the saw-toothed pattern suggests that such variation is short-lived, and a rational forecast of the minimum

[11] Because the minimum wage for most of the workers first covered in 1966 or 1974 was initially set at $1.90 rather than $2.00, this calculation slightly understates the extent to which recently and newly covered workers were the most affected. For similar evidence in other years, see Peterson (1981, Tables 16 and 17).

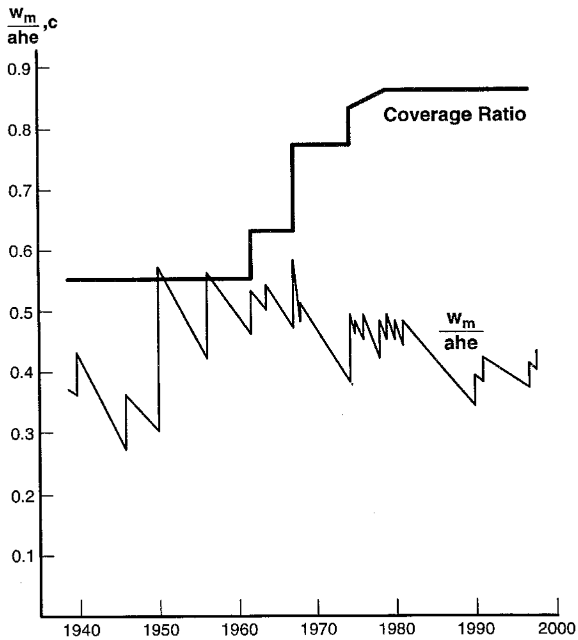

Fig. 4. Minimum wage relative to average hourly earnings and private-sector coverage ratio.

wage over a 5- or 10-year horizon would have much less variation. Second, newly covered establishments face a near-permanent change (with the only escape being to shrink below the coverage threshold).

4. Time series evidence

4.1. Overview

Given that federal law imposes the same minimum wage on high- and low-wage states, and that state minimum wage laws have historically been relatively unimportant, it is not surprising that time series variation in minimum wages and employment have been an important source of evidence on the employment effects of the minimum wage. Perhaps more surprising is that while the general trend in labor economics has been away from time-series data to cross-sectional or panel-data studies (Stafford, 1986), the time series evidence has, until quite recently, retained its primacy in the minimum wage debates.

The basic statistical model in the time series literature is

$$E_t = \alpha X_t + \beta MW_t + \varepsilon_t,$$

where E is the employment/population ratio, X is a cyclical indicator, often a time trend, plus other relevant control variables, and MW is the level of the minimum wage, usually relative to average wage (usually multiplied by the fraction of employment covered by the minimum, the so-called Kaitz index, following Kaitz, 1970).[12]

Most studies focus on teenagers because a sizeable minority of teenagers' wages are directly affected by the minimum wage; for older groups plausible variation in employment due to the minimum wage is swamped by other factors. Given this focus on young workers, the "other" control variables have tended to have a youth-oriented focus as well: the relative share of teenagers in the labor-force age population, the fraction of teenagers in the armed forces (and so unavailable for civilian employment, the traditional employment measure), the fraction of teenagers (16–19 year olds) who are 16–17, etc.

E and MW are often replaced by their logarithms, in which case β is an elasticity. But it is not a "demand elasticity" of the usual sort. With a double-log specification, we have

$$\beta = (\Delta \ln E)/(\Delta \ln w_{\mathrm{m}}).$$

If we define E^* as the employment of those directly affected by the minimum wage increase and w^* as the average wage of those directly affected, then a natural measure of the elasticity of demand for low-wage labor would be

$$\eta = (\Delta \ln E^*)/(\Delta \ln w^*).$$

As noted above, only a subset of teenagers (or members of any other low-wage group) are directly affected; if employment of those not directly affected does not change (or increases, because they are substitutes for those in E^*), $\Delta \ln E$ will be significantly smaller (in absolute value) than $\Delta \ln E^*$ (Gramlich, 1976, p. 260).

Moreover, when the minimum wage is increased by 10%, many teenagers receive no increase at all. Card and Krueger (1995, p. 117) report that in 1989 two-thirds of all employed teenagers were already earning more than $3.80 (the level to which the minimum wage was raised in April of 1990), and half were already earning more than $4.25, the 1991 minimum. Some of those already earning more than the new minimum received small increases, but some of those below the minimum wage work in uncovered jobs (or for non-compliant employers). On balance, between 1989 and 1992 (when the minimum wage increased by 27%), the average wage of teenagers increased only 9% (Card and Krueger, 1995, p. 121); Deere et al. (1996, p. 31) estimate that in March 1990 the increase required to bring teenagers up to the $4.25 minimum of April 1991 was only 4%.[13] Thus, $\Delta \ln w^*$ is significantly smaller in absolute value than $\Delta \ln w_{\mathrm{m}}$.

[12] Often when coverage was extended, the minimum wage for newly covered employers was lower than the "regular" minimum wage, and Kaitz's index took account of that difference. His index was equal to $\sum_i c_i(w_{\mathrm{m}}/w_i) + c_i'(w'_{\mathrm{m}}/w_i))$, where c_i is the fraction of employment in industry i covered previously, c'_i is the fraction of employment that is newly covered, w_i is the average wage in industry i, and w_{m} and w'_{m} are the minimum wage applicable to previously and newly covered employers.

[13] Difference in base period and Card and Krueger's inclusion of 1992 wage growth account for part of the difference. I suspect most of the rest is due to "spillovers" – wage increases to teens already earning more than $4.25 are included in Card and Krueger's measure, but not in Deere et al.'s.

Because the numerator of β is smaller (in absolute value) than the numerator of η, while the denominator of β is larger, $|\eta| > |\beta|$. Neumark and Wascher (1997) estimate that among those 16–24 in 1995, 21.3% earned at least the \$4.25 minimum wage in force at the time but less than the September 1997 minimum wage of \$5.15; because many of them were already earning more than \$4.25, w^* increased by only 10.8%, even though w_m was increasing by 21.2%. If only the employment of those initially earning between \$4.25 and \$5.15 was affected by the 1996–1997 increases that brought w_m to \$5.15, we have

$$\eta = \beta(0.212/0.108)/0.213 = 9.2\beta.$$

Implicitly, Neumark and Wascher take the 4.3% of youth whose reported wage was below \$4.25 as unaffected by the law. Given that their wage data come from CPS data reports which have some random reporting error and appear to have many responses rounded to even-dollar amounts, it is not clear that someone reported to earn \$4.00 is unaffected by the law. Even if they really represent employment at establishments that are uncovered by or not compliant with the law, their wages may be affected.[14]

Most studies of young workers focus on teenagers. For them, the share directly affected is larger, and the fraction of those directly affected who were at or below the old minimum (and so receiving the full increase in the minimum) is probably larger as well. A rough calculation based on Card and Krueger's tabulations of teenage wages surrounding the 1990–1991 increase suggests – assuming those below the old minimum wage are unaffected – that $\eta \approx 5\beta$.[15] The time series evidence is mostly drawn from the 1960s and 1970s, when the minimum wage had more bite on the wage distribution, so the appropriate multiplier for time series studies of teenagers is probably less than 5.

Estimates of β re-scaled as the proportional change in employment from a 10% increase in the minimum wage (coverage constant) are presented in Table 2.

Brown et al. (1982) summarized the studies available at that time, either published or in draft. We noted that the estimated reductions in teen employment from a 10% minimum wage increase ranged from 1 to 3%, and the estimates were generally "significant" statistically.[16] We did not have much luck in finding one or two key choices that would explain why some studies' estimates were higher than others. Studies which included "more recent" (i.e., 1970s) data, included more control variables (some early studies

[14] See Section 8 for evidence that uncovered-sector employers often pay exactly the minimum wage. To gauge the importance of those below \$4.25 for the calculation, assume that they get the same 21.2% increase as those initially earning \$4.25. Then $\eta = \beta(0.212/0.126)/0.266 = 6.3\beta$.

[15] The minimum wage increased from \$3.35 to \$4.25, a 27% increase. Based on Card and Krueger's Fig. 4.2, roughly 40% of teens earned between \$3.35 and \$4.24 prior to the increase, and average wages in this interval were about \$3.75, so the average wage increase of those directly affected was about half of the minimum wage increase.

[16] Many of the studies reported separate regressions by race and/or sex and the estimates in the table are weighted averages of those dis-aggregated results.

Table 2
Estimated effect of a 10% increase in the minimum wage on teenage employment and unemployment: time-series studies[a]

Study	Percent change in teenage employment	Change in teen unemployment rate (in percentage points)
Kaitz (1970)	−0.98	−0.01
Adie (1971)		2.53
Moore (1971)		3.65
Kosters and Welch (1972)	−2.96	
Lovell (1972)		−0.00
Adie (1973)		0.52
Lovell (1973)		−0.25
Kelly (1975)	−1.20	
Gramlich (1976)	−0.94	
Kelly (1976)	−0.66	
Hashimoto and Mincer (1970); Mincer (1976)	−2.31	0.45
Welch (1976)	−1.78	
Ragan (1977)	−0.65	0.75
Mattila (1978)	−0.84	0.10
Iden (1980)	−2.26	
Abowd and Killingsworth (1981)	−2.13	
Betsey and Dunson (1981)	−1.39	
Boschen and Grossman (1981)	−1.50	
Hamermesh (1981)	−1.21	
Ragan (1981)	−0.52	
Freeman (1982)	−2.46	0.00
Wachter and Kim (1982)	−2.52	0.51
Brown et al. (1983)	−1.14	0.01
Solon (1990)	−0.99	
Wellington (1991)	−0.63	
Klerman (1992)	−0.52	
Card and Krueger (1995)	−0.72	

[a] Source: Brown et al. (1982), updated by author.

did not even include time trends), and included coverage in the minimum-wage variable tended toward the low end of that range. These emerged as our preferred estimates.

More recent studies (the last three in Table 2) find point estimates of the loss of teen employment from a 10% minimum wage increase that were uniformly smaller than 1%, and in some cases not statistically significant at conventional levels. Because these studies replicated earlier specifications taking advantage of additional years of data, their clear message is that including the 1980s reduces the estimated effect of the minimum wage on employment.

While the time series literature began with a focus on teen unemployment, over time fewer studies even reported unemployment effects. The available estimates varied quite a lot, although most suggested a 10% increase in the minimum wage would raise the teen unemployment rate by less than 0.75 percentage point. Labor force participation is negatively related to the minimum wage, which helps account for (or is implied by, depending where one starts) the relatively small unemployment effects.

In principle, the effect of the minimum wage on young adults (age 20–24) is ambiguous: raising the wages of those who would otherwise earn less should reduce employment, but raising the wages of teenagers (who may be good substitutes for young adults in many jobs) should raise young-adult employment. Since a smaller proportion of young adults is directly affected, any negative effect is likely to be much smaller for young adults than for teenagers when that impact is expressed as a proportionate change in employment of all young adults. While relatively few studies even consider young adult employment, those that do tend to produce smaller estimated minimum-wage impacts (Brown et al., 1982, Table 6; Wellington, 1991, Table 3).

4.2. Hours versus bodies

Based more on data availability than unconstrained preference, the time series literature has measured employment by numbers employed, and neglected variation in hours per worker. The few studies that have addressed this issue relied on the relative short time series of published information on weekly hours. This limited evidence suggests that the minimum wage reduces hours worked by employed teen-aged workers, so that "full-time equivalent" employment falls more than number employed (Gramlich, 1976; Brown et al., 1983).[17]

At first glance, this makes sense; the reduction in employment is spread across both of the available margins. However, we know that full-time workers are paid more per hour than apparently similar part-time workers. This suggests that, over the relevant range of work-weeks, those working more hours per week produce more per hour. If so, we should expect employers to lengthen work-weeks in response to a minimum wage increase (Barzel, 1973). Perhaps firms are raising average output per hour by limiting break time (Oi, 1997, p. 9).

4.3. Differences by race and sex

The effect of the minimum wage on teenage employment is a combination of effects by

[17] The FTE reduction is perhaps 40% larger than the more widely estimated employment loss, although this difference is estimated with lamentable imprecision.

[18] Card and Krueger (1995, Table 4.1) show that 53% of those teenagers earning $3.35–$4.24 in 1989 (and so likely to be affected by the 1990–1991 increases in the minimum wage) were female, compared to 48% of all employed teenagers. For black teenagers, the corresponding proportions were 14 and 12%. These differentials would be larger, on average, in the period covered by the time-series studies.

race and sex that might be expected to differ. Given the lower market wages of blacks and women, we expect more workers in these groups to be directly affected, i.e., their wage increased by law, and their employment prospects reduced, and fewer at higher wage levels where substitution would increase employment.[18] This formalizes a longstanding policy concern that the negative effects of the minimum wage on employment may be particularly hard on black teens.

Empirically it turns out there is very little to learn about these demographic differences from the time series studies. Differences between groups are estimated with limited precision (particularly for blacks, who are not over-sampled in the CPS data used in all the studies in Table 2), and there is no pattern as one looks across studies. Card and Krueger (1995, Table 6.9) confirm that this imprecision persists when the data are extended into the 1990s. Their point estimates suggest somewhat larger effects for blacks than whites, but larger effects for males than females; none of the differences is statistically significant.[19]

4.4. Coverage

If the minimum wage had no effect on employment in the uncovered sector, the proportional change in total employment would equal the proportional change in covered-sector employment times the fraction of employment that is covered; i.e., $\mathrm{dln}(E) = c\mathrm{dln}(E_c) = c\eta\mathrm{dln}(w_m)$. Formal two-sector models show that employment in the covered sector is likely to change, and the reduced-form employment equations that emerge from these models combine coverage and the relative level of the minimum wage in a much more complicated expression.

The dominant empirical response to this problem has been to use the Kaitz index, which is a coverage-weighted sum of the ratio of the minimum wage to the average wage in each industry.[20] Other studies try to estimate separate "level" and coverage effects. In this specification, the effect of the level of the minimum wage tends to be larger (e.g., a 2% rather than a 1% reduction in teen employment from a 10% increase in w_m) but coverage

[19] The typical time-series study that explores differences by race or sex simply estimates separate equations for different groups. But the variance of the difference between, say, coefficients for blacks and whites is not the sum of the two variances, because there is likely a common component between the disturbances in the black and white employment equation. Hence one cannot tell from the published tables whether the black-white or male-female difference in coefficients is estimated with reasonable precision. Calculations reported by Brown et al. (1983, p. 22) suggest that, at least in their sample, the common error component is not large enough to significantly reduce the standard error of the black-white difference.

[20] While, as noted in Section 2, the functional form suggested by formal two-sector models is too complicated to be useful, one might at least prefer a form that "makes sense" in the absence of employment responses in the uncovered sector. That would lead to the level of coverage multiplying the logarithm of the minimum wage. In principle, w_m should be normalized by w_o; in practice, the average wage is used instead. But once we normalize the minimum wage by an average wage measure (which is greater than the minimum wage for all candidate average wage measures), the logarithm of the ratio of w_m to the average wage is negative, and so this form would force coverage and level effects to be of opposite sign.

effects are weaker or non-existent (Brown et al., 1983, Table 3). However, while we could not reject the hypothesis that coverage effects were zero, we also could not reject the "Kaitz" restriction than $\ln w_{\mathrm{m}}$ and $\ln c$ have equal effects. Wellington (1991, Table 1) and Card and Krueger (1995, Table 6.8) report similarly weak coverage effects including more recent data; Wellington finds that whether one can reject the Kaitz restriction is sensitive to specification.

4.5. Leads and lags

With very few exceptions, the time series studies of the minimum wage relate employment at time t to the minimum wage at time t. This stands in contrast to most other studies of employment demand, which find that lagged adjustment is important. Two justifications for this contemporaneous-response assumption have been offered.

First, voluntary turnover rates in low-wage labor markets are very high, so that a desired reduction in employment can be achieved quickly just by not replacing those who quit. So there is no "firing cost" or increase in unemployment insurance taxes to worry about, as there might be in reducing the numbers of more skilled workers. There are relatively few hiring costs, either; because expected tenure is brief, it does not make sense to make large investments in training or even screening minimum-wage workers. Hamermesh (1995, p. 836) notes however that lagged adjustment of other inputs such as capital will delay the adjustment of labor, even if there are no direct costs of adjusting the latter.

Second, changes in minimum wage laws become effective several months after proposed increases have become law; indeed, when a phased increase is enacted the forewarning of the second increase is over a year. For example, the increases to \$4.75 in October 1996 and to \$5.15 in September 1997 were both enacted in August 1996.

In any case, early studies tended to allow lagged responses to the minimum wage, and for these Table 2 reports the sum of these responses. More recent studies usually assume contemporaneous response. Hamermesh (1981) and Brown et al. (1983) report the estimates both ways and find that lags (and, in BGK, leads) do not matter much.

This does not mean that the short- and longterm effects of the minimum wage are the same. The data are not rich enough to identify longterm responses if, indeed, they are different. But it does mean that shortterm estimates are not very sensitive to allowing the relatively short lags that have been considered.

4.6. What happened?

Earlier I noted that, especially among studies with sample periods including the late 1970s, there was reasonable consensus about the effects of the minimum wage on employment. Studies that include the 1980s all report estimates below this consensus range, and increasingly we cannot reject the hypothesis that the true effect is zero.

What happened? At a mechanical level, the answer is simple: between 1981 and 1990, the nominal minimum wage remained constant, and its value relative to average wages fell accordingly. While teenage employment increased, so did employment generally, and teenage employment did not increase as fast as declining adult unemployment (and a declining minimum wage variable) would have predicted.[21]

One hypothesis is that the minimum wage had declined relative to other wages by so much that its further gradual erosion had little effect (Hamermesh, 1995, p. 837). While it is true that the mid-1980s was a period of relatively low minimum wages, its impact on teenagers was probably not very different than it had been in the early 1970s.[22]

One might instead look to the data; does a more flexible functional form (e.g., quadratic) allow one to predict how much the decline in the relative minimum wage or in the Kaitz index should have reduced the marginal impact of the minimum wage? Attempts along these lines have not been successful; more complicated functions have not been estimated with any useable precision (Wellington, 1991, p. 35; Card and Krueger, 1995, p. 203).[23] Given the difficulties of estimating even first-order effects, this should not be terribly surprising.

Another important change in the 1980s was the increase in wage inequality in general, and the declining position of relatively unskilled workers in particular. This increase in the dispersion of the distribution of hourly wages has several implications. First, the minimum wage relative to the equilibrium wage for teenagers would decline less than the minimum relative to an average wage (Deere et al., 1996, pp. 37–38). This means that the number of teenagers whose wage is directly affected by the minimum would be declining less rapidly than a relative-minimum-wage variable would predict. Second, for teenagers not directly affected by the minimum wage because they earn more, increasing wage inequality could either increase or reduce average wages (relative to trend) and lead to supply responses (relative to trend). Whether the technological or other changes that dominated the 1980s can account for the relatively slow growth of teen employment in that decade remains an open question.

Kennan (1995, p. 1955) notes that the predicted change in teenage employment from the earlier "consensus" is small relative to month to month fluctuations in teen employment from all causes. "In short, we are looking for a needle in a haystack." Given that the previous studies used "different but closely related datasets", the likelihood of important omitted variables, and other problems common to all the time series studies, the "consensus" estimate was none too reliable in the first place. Kennan supports his argument by

[21] Deere et al. (1996, Fig. 3-6) show significant variation in the proportion of teenagers employed relative to 20–24 year olds. This ratio was rising in 1979, declined in 1980 (when the minimum wage was increased), increased from 1983–1990, and fell in 1990–1992, before recovering. They find the ratio is closely related to the relative wages of the two groups, and interpret the increase in the 1980s as consistent with a declining relative level of the minimum wage over this period. But there is no control for general business conditions in this part of their analysis.

[22] The fraction of teenagers earning the minimum wage or less in 1987 (the year after Wellington's sample ended) was 28.7% (US Census Bureau, 1989, Table 675), while in 1973 (following 5 years of rapidly rising average wages but constant minimum wage) it was 26.3% (Gilroy, 1981, Table 22). Moreover, coverage of low-wage industries was expanded in 1974, so the fraction of those at or below the minimum who were directly affected was likely to be higher in the mid-1980s than in the early 1970s.

[23] Both add the square of the Kaitz index rather than the square of the relative minimum wage.

showing wide variation in coefficients in a set of time series estimates; but none of his look much like any of those in the literature.[24] I believe he overstates the point, but it is valid nonetheless. Even within the narrow boundaries of the traditional literature, one can see the sense of his comment–the most recent estimates which have precipitated the crisis are all within the confidence intervals of the typical early 1980s estimate.[25]

Whatever the cause, the more agnostic message of the more recent time series estimates has stimulated a revived interest in other approaches, which make greater use of cross-sectional variation.

5. Cross-state comparisons

The basic idea behind use of cross-state comparisons is straightforward and appealing: minimum wage laws will have a larger effect on employment in low-wage than high-wage states, because the minimum wage will be a binding constraint for more workers in low-wage states. More recent studies have included much more careful attempts to control for other differences between states that would otherwise bias our estimates.

5.1. Early cross-state studies

As cross-sectional data became more widely available – and more widely used in other branches of labor economics – several studies used 1970 Census data to estimate cross-sectional versions of the employment equation used in the time series studies. Replacing the time subscript with an i subscript for state (or metropolitan area), we have

$$E_i = \alpha X_i + \beta MW_i + \varepsilon_i.$$

Despite the apparent similarity to the time-series version, there was an important difference. In the time-series context, the minimum wage index varies because of variation in coverage and the periodic re-adjustment of the level of the minimum; variation in average wages is essentially trend and (with trend separately accounted for in the typical study) does not identify β. While cross-section studies also used a Kaitz-like minimum wage index, the source of variation was different. The federal minimum wage was constant across observations, state laws mattered relatively little because federal coverage had been

[24] Kennan presents time series regressions using employment of young teens (16–17) with minimum wage elasticities from -0.003 to -0.037. His minimum variable is deflated by the CPI and does not include coverage. He includes two lags of the dependent variable whose coefficients are, predictably, not inconsequential (they sum to 0.92–0.96), and complicate the interpretation of the minimum wage coefficient. In some specifications, the dependent variable in the logarithm of the employment/population ratio, in others it is ln(employment); in the latter, ln(population) and its lag are included (with nearly offsetting coefficients), but not adult population. There is no discussion of why these specifications are preferable to those used in other time-series papers, or why the variation among them represents variation among a reasonable set of specifications.

[25] Wolfson (1998) makes a point similar to Kennan's that changing the specification to account for possible unit roots weakens the estimated minimum wage effect and increases its standard error.

extended to most workers (and state laws specified minimums no higher than the federal law), and federal coverage varied relatively little across states. Thus, most of the variation was due to variation in average wages across states (Welch and Cunningham, 1978, p. 144).

Some of these studies estimated minimum wage effects at the upper end of the 1–3% range of the time series studies, but others found negligible effects. In general, studies that controlled for more other factors estimated smaller effects of the minimum wage. But because the crucial variation was coming from average wages rather than variation in the minimum wage itself, this approach provided "at best a weak test of the effect of the minimum" (Freeman, 1982, p. 120).

5.2. Panel-data studies

Minimum wage studies using state-level data more or less vanished in the early 1980s, but have reappeared recently in a much more interesting form. Two unrelated developments appear to be responsible for this resurgence. First, the availability of Current Population Survey files with wage-rate data allowed researchers to tabulate their own panels of state observations over time. This not only allowed researchers to introduce state-level fixed effects in the analysis, but permitted examination of the effects of the minimum wage on wages, and on enrollment as well as employment (and on the interaction between the two). Second, as the federal minimum remained constant in nominal terms in the 1980s, states began to raise their own minimum wages *above* the federal minimum. Alaska and the District of Columbia have traditionally set their minimum wage above the federal minimum; but by 1989 13 states had done so, including California, Massachusetts, and Pennsylvania (Neumark and Wascher, 1992, Table 1).

A representative estimating equation in the literature using state data over time is

$$E_{it} = \alpha X_{it} + \beta MW_{it} + \gamma_i + \delta_t + \varepsilon_{it},$$

where γ_i and δ_t are fixed effects for state and time, respectively. The state fixed effects provide protection against the danger that the minimum wage coefficient will pick up largely regional variation (since average wages are lower in the South, MW tends to be higher there).

Neumark and Wascher (1992) provide the most detailed attempt to date to combine federal and state minimum wage laws into a single "minimum wage" variable. To simplify matters somewhat, in years when a state's minimum wage is less than the federal minimum m_f, the state minimum is irrelevant to those covered by the federal law, and so "the" minimum wage is m_f for c_f of the state's employment, and m_s for c_s of its workers. (There are exemptions from state coverage, too, which make $c_\mathrm{s} + c_\mathrm{f} < 1$.) In years when the state minimum is higher, it applies to both federal- and state-covered workers. Thus, in the spirit of the "Kaitz" index from the time series literature, the minimum wage variable would be

$$MW^* = [c_\mathrm{f}\max(m_\mathrm{f}, m_\mathrm{s}) + c_\mathrm{s}m_\mathrm{s}]/w_\mathrm{s}.$$

However, data on workers covered by state laws is available for only 3 years toward the beginning of their 1973–1989 sample period. After experimenting with a patched-together measure of state coverage, they opt instead for

$$MW = c_{\mathrm{f}} \max(m_{\mathrm{f}}, m_{\mathrm{s}})/w_{\mathrm{s}}.$$

Based on annual data for 1973–1989[26], their estimates of β are essentially zero for teenagers if enrollment rates are not included among the control variables, but in line with the time series findings when enrollment is held constant (Table 3). Neumark and Wascher also find somewhat larger effects when both MW_{it} and $MW_{i,t-1}$ are included. Estimated effects of the minimum wage on employment of those 16–24 are much less affected by controlling for enrollment; but the implied elasticities for 20–24 year olds are (implausibly, in my view) large relative to those for teenagers alone.[27] Finally, they identify states with separate "sub-minimum" wage provisions for students or youth, and find the latter somewhat moderate the effect of the minimum wage on youth employment.

Neumark and Wascher's conclusions were challenged by Card et al. (1994). A number of issues emerge from this interchange (Neumark and Wascher, 1994, 1996a; Card and Krueger, 1995).

First, as noted above, Neumark and Wascher do not have the data on state coverage rates that are needed to construct a strict analogue to the Kaitz index. Since the difference between available and true minimum wage variables amounts to $c_{\mathrm{s}}m_{\mathrm{s}}/w_{\mathrm{s}}$, we can write

$$E_{it} = \alpha X_{it} + \beta[c_{ft}\max(m_{ft}, m_{sit})/w_{it}] + \beta[c_{sit}m_{sit}/w_{it}] + \gamma_i + \delta_t + \varepsilon_{it},$$

where the first term in brackets is the Neumark and Wascher minimum wage variable and the second term in brackets is in effect an omitted variable. Bias on this count seems more likely to overstate β (in absolute value), although this is at best an educated guess.[28] Card et al. find that, if "the" minimum wage is defined simply as the higher of the state or

[26] For 1973–1976, they have data for only 22 states, because in these years the Current Population Survey public use files did not separately identify small states.

[27] Using the enrollment variable in Neumark and Wascher (1992), the elasticities are −0.19 for teens and −0.17 for 16–24 year olds, and so nearly as large for young adults as teenagers despite a far smaller fraction being directly affected. Using an alternative enrollment variable that is less mechanically linked to employment status, the elasticity is larger for 16–24 year olds than for teens, and statistically significant only for the former. See Neumark and Wascher (1994, Table 2).

[28] To think about the likely bias this could create in a model with fixed effects for state and year, we need to focus on the variation in the two variables in brackets after state and year effects in these variables have been swept out. In states with no minimum wage, or one that is never increased above the federal level, the first term in brackets will be very well predicted by year and state dummies, and the omitted variable probably has little independent variation as well. In states that raised their minimum wage above the federal level in the late 1980s, both terms will likely be above the level otherwise predicted from state and year effects. Card et al. (1994, p. 492) also note that Neumark and Wascher's coverage variable refers to all workers, not teenagers, and that measured federal coverage jumps by nine percentage points in 1985, as a result of a Supreme Court decision on the applicability of the federal minimum wage to state and local government employees (few of whom are low-wage teenagers). They do not, however, argue that these measurement issues are likely to be related to fluctuations in teenage employment.

Table 3
Estimated effect of a 10% increase in the minimum wage on teenage and young adult employment: studies using states over time

Source/table	Data	Minimum wage variable	Control variables[a]	Percent change in employment due to 10% minimum wage increase		Notes
				Age 16–19	Age 16–24	
Neumark and Wascher (1992, Table 2)	May CPS, 1973–1989, $N = 751$	$\max(m_f,m_s)*c_f/w$	UR, pop share, state, year	0.6 (0.9)	−0.7 (0.4)	
			Same + ESR enrollment	−1.4 (0.6)	−1.0 (0.4)	
Neumark and Wascher (1992, Table 5)		$\max(m_f,m_s)*c_f/w$ current + 1 lag	UR, pop share, state, year	−0.3 (1.0)	−1.8 (0.6)	
			Same + ESR enrollment	−1.9 (0.7)	−1.7 (0.4)	
Card et al. (1994, Table 1)		$\ln \max(m_f,m_s)$	UR, pop share, state, year	3.7 (1.9)		b
		Same	Same + ESR enrollment	0.9 (1.2)		b
Neumark and Wascher (1994, Table 2)		$\max(m_f,m_s)*c_f/w$ current + 1 lag	UR, pop share, state, year, MA enrollment	−1.1 (0.9)	−1.6 (0.5)	
Neumark and Wascher (1994, fn. 9)		$\max(m_f,m_s)*c_f/w$ current + 1 lag	Same	−2.2 (0.8)	−1.4 (0.5)	c
Burkhauser et al. (1997, Table 1)	SIPP monthly, Jan. 1990–May 1992, $N = 1218$	$\ln \max(m_f,m_s)$	UR, pop share, ln w-all, state, month	−8.7 (1.1)	−3.6 (0.5)	d
Burkhauser et al. (1997, Table 3)	CPS monthly, Jan. 1990–May 1992, $N = 1479$			−4.9 (0.9)	−1.9 (0.5)	d

Burkhauser et al. (1997, Table 5)	CPS monthly, Jan. 1979–Dec. 1992, $N = 8568$			−3.7 (0.5)	−1.9 (0.2)	d
Burkhauser et al. (1997, Table 1A)	CPS monthly, Jan. 1979–Dec. 1992, $N = 8568$		Same + year	0.2 (0.9)		d
Card (1992a, Table 3)	CPS Apr.–Dec. 1989–1990, $N = 51$	Fraction of teens with $\$3.35 < w < \$3.79$	E/P	−0.4 (1.1)		e
			UR	0.4 (1.4)		e
Card and Krueger (1995, Table 4.4)	CPS annual 1989–1992, $N = 51$	Fraction of teens with $\$3.35 < w < \$4.24$	E/P	0.5 (1.5)		e
Card and Krueger (1995, Table 4.5)		Same	Same + lagged E/P	1.0 (1.5)		e
Deere et al. (1995, Table 4)	CPS annual 1985–1992, $N = 408$	Dummy vars for 1990, 1991–1992	ln E/P, state, trend	−3.5 (0.5−0.6)		f

[a] Definitions of control variables: UR, prime age male unemployment rate, unemployment rate for all workers in Card (1992); pop share, teen (or young adult) population as fraction of labor-force age population; enrollment, proportion of teen (or young adult) population (ESR is based on employment status recode, MA is based on major activity); E/P, employment/population ratio, for all workers in Card (1992) and Card and Krueger (1995), for males in Deere et al. (1995); ln w-all, ln(average hourly earnings for all workers).

[b] Adding ln(adult wage) as control does not change estimated minimum wage effect.

[c] GLS estimates that allow for first-order serial correlation and heteroskedasticity.

[d] Standard errors corrected for heteroskedasticity.

[e] Dependent variable and independent variables (other than minimum wage variable) are 1989–1990 or 1989–1992 changes, so in effect there are fixed state effects.

[f] Separate estimates for males and females. Only female equation has time trend. My standard error calculation assumes correlation between male and female estimates is not negative.

federal minimum (without coverage adjustment), the minimum-wage coefficient is positive (although not significant when enrollment is held constant). Neumark and Wascher (1994, p. 504) suggest that *not* adjusting for coverage produces a stronger relationship between the minimum wage and teen *wages*.

Second, Neumark and Wascher do not include the state average wage as a separate independent variable, and so any effect of average state wages (or the factors that determine it) on teenage employment may lead estimates of β to be too negative. Neumark and Wascher (1994) report regressions (with current and lagged minimum wage variables[29]) that include state average wages as a separate control variable. The estimates are negative but generally not significant; however, the restriction that it is the *ratio* of the minimum wage to the average wage which affects teen employment is usually not rejected.

Third, the evidence of negative effects on employment appears to depend on controlling for enrollment. There is a strong negative relationship between enrollment rates and the minimum wage in Neumark and Wascher's data, contrary to Mattila's (1978) time-series results. If enrollment and minimum wages happen to be negatively correlated, it is important to take account of this chance correlation; in much the same spirit that, e.g., cyclical variables are typically held constant. If minimum wages reduce employment and enrollment, reduced-form and enrollment-constant employment equations have very different interpretations, and it is not clear that the latter are to be preferred. (If the minimum wage reduces school enrollment (Neumark and Wascher, 1996b), this is important in its own right, perhaps more important than the employment loss.)

Suppose there happens to be a correlation between minimum wages and enrollment. It seems unlikely that the effect of a one-point reduction in enrollment is larger than 0.01 times the raw difference in employment rates for enrolled and non-enrolled teens. Neumark and Wascher's results for teenagers are so sensitive to enrollment because the estimated effect of enrollment on employment is implausibly large; if one constrains the effect of enrollment on employment to be no larger than the raw difference in employment probabilities, minimum-wage effects for teens are small.[30]

Burkhauser et al. (1997) also use pooled data by state over time, and rely in part on differences in state minimum wage laws relative to the federal minimum to identify the

[29] The relative minimum wage variable is apparently not coverage adjusted, in response to Card et al.'s reservations about the use of federal-only coverage. I cannot determine from the regressions that are reported how important the different treatment of coverage might be.

[30] The difference in employment rates between teenagers who are enrolled and those who are not is -0.22 (Neumark and Wascher, 1994, p. 499). Imposing this estimate on equations that allow for lagged minimum wage effects leads to estimated effects of a 10% increase in the minimum wage on teen employment of 0.5 and.7% depending on the enrollment variable (based on Neumark and Wascher, 1994, Table 2, where the OLS effects of enrollment on employment are -0.77 and -0.37). Neumark and Wascher also present IV estimates, but the minimum wage estimates are even larger than the OLS effects. Alternatively, one can control for exogenous determinants of school enrollment (Neumark and Wascher, 1995, Table 3). This produces an employment elasticity of -0.05 in one specification and 0.05 in another – "essentially zero" (Neumark and Wascher, 1995, p. 202).

effect of minimum wage laws on employment. They use monthly data from both the Survey of Income and Program Participation and the CPS. In response to the Card et al. critique of Neumark and Wascher's work, they define "the" minimum wage as the greater of the federal and state minimum, with *no* adjustment for either federal or state coverage. They also control separately for the log of the average adult wage in the state, along with the prime age male unemployment rate, the proportion of the working age population accounted for by teenagers, and fixed effects for state and month. Here, "month" is a seasonal variable that distinguishes January from February, but not January of one year from January of the next.

They find higher minimum wages significantly reduce teenage employment, although the estimates prove quite sensitive to the sample used for the estimation. SIPP data for January 1990 to May 1992 suggest a 10% increase in the minimum wage reduces teenage employment by 8.7%; using CPS data for the same months lead to a smaller 5.9% reduction; extending the CPS sample to (include 1979–1992) reduces it still further, to 3.7%. *T*-ratios for the minimum wage variables range from 5 to 8. Using SIPP data, they estimate a 3.6% reduction for 16–24 year olds as a group, which implies a tiny positive effect on those 20–24. Among those 16–24, effects are larger for blacks (−5.1%) than others (−3.2%) although the difference does not appear statistically significant.

Burkhauser et al. show that most of the SIPP-CPS difference is due to SIPP not separately identifying (and so they excluded) small states. As between the CPS estimates based on shorter and longer samples, there is no obvious reason to prefer the shorter sample.[31]

This leaves the sizeable difference between their smallest estimate and those of Card–Katz–Krueger using the same data. As the last line from Burkhauser et al. in Table 3 shows, the key difference is that Card–Katz–Krueger and Neumark–Wascher include year dummies, while Burkhauser et al. do not. Thus, if one uses cross-sectional variation to identify the minimum wage effect, it is negligible. If one uses variation over time as well, the estimated minimum wage effects are substantial.

However, relying primarily on time-series variation when using panels of state data over time raises the question of whether the state × year design is preferable to a simple time-series approach. The state × year design uses different patterns in the control variables in different states over time to better identify these effects, but the simple time series approach can use published data for more years than are available for state × year cells built up from public-use CPS files. The wide variation across time periods in Burkhauser et al.'s estimates is discouraging.

Card (1992a,b) and Card and Krueger (1995) offer a different strategy for taking advantage of cross-state differences in minimum wage impacts while avoiding the problems posed by lack of good data on the coverage of state minimum wage laws. They focused on

[31] Burkhauser et al. (1999) report that if one corrects for both heteroskedasicity and serial correlation, or allows for a 1-year lag in the effect of the minimum wage, a 10% increase in the minimum wage is estimated to reduce teenage employment by about 2% (in specifications that include controls for year effects). But the estimated effects are much weaker when the sample is extended through 1997.

increases which raised the minimum from $3.35 in 1989 to $3.80 in 1990 and to $4.25 in 1991. Based on 1989 CPS data, they calculated the fraction of teenagers whose wages were above $3.35 but below $4.25; i.e., those whose wage would have to be increased to comply with the new law. While the overall increase in teen wages needed to comply with the new law was fairly small, there is considerable state-to-state variation in the fraction of teenagers between $3.35 and 4.25, in part because some states had raised their own minimums (Card and Krueger, 1995, p. 122).

They then regressed the change in the mean ln(wage) of teens and their employment/population ratio in each state between 1989 and 1992 on this fraction. As expected, teen wages rise more in states with a larger fraction of teens directly affected by the new law[32]; each percentage point of teenagers directly affected raising wages by 0.28%. Employment, however, grew faster in states where the minimum wage impact was greater (an extra percentage point of teens between $3.35 and $4.25 increasing the teenage employment/population ratio by 0.13 point.) Controlling for the growth of overall employment reduced the coefficient of the minimum wage variable in the wage equation to 0.22, and in the employment equation to zero (0.01, with a standard error of 0.03, to be precise!).

Because Card and Krueger's "fraction affected" is different from the minimum wage variables used in other studies, it is worthwhile to recalibrate our expectations for what this coefficient should be. If the minimum wage law simply led employers to raise those between $3.35 and $4.25 up to $4.25, the coefficient in the wage equation would be 0.15. So the coefficient of 0.22 reflects spillovers – some of those being paid $4.25 getting raises, too – or, more worrying, economies in high impact states being healthy in ways not accounted for by the increase in overall employment to population ratios. If the 27% increase in the minimum wage had reduced teenage employment by 2.7% (as might have been predicted from the time series literature) the coefficient of the minimum wage variable would be -0.03.[33] Thus, while the point estimate suggests no employment loss, the confidence interval stretches to (barely) include the traditional estimate.

The change when controlling for overall employment growth reflects the fact that states most affected by the minimum wage increase were those least affected by the recession.

[32] States which had raised their own minimums above the federal level by 1989 are partially accounted for by this procedure. A state like California that had already raised its minimum to $4.25 had few workers below $4.25, and so low "impact"; presumably this impact is reflected in 1989 employment. For states that had made smaller increases, and so had spikes at $3.65, for example, the procedure would not show a reduced "proportion affected". This is related to the fact that Card's measure counts how many are below the new minimum, but not how far below they happen to be.

[33] Simple "topping up" by employers would raise the average wage of affected teens by 15% (since average wages of those in this range increase from the actual mean of $3.68 to $4.25). The average wage would then increase by 0.15 times the proportion whose wage was increased. If the 27% increase in the minimum wage had reduced teenage employment by 2.7% (i.e., 1.35 percentage points on a base of 49%), the coefficient of the minimum wage variable would be $-0.0135/0.414 = -0.03$. (0.414 is the fraction of teenagers with wages between $3.35 and $4.25 initially – the mean of the "minimum wage" variable.)

Capturing as much as possible of this – and any pre-existing trends in growth of different states – is therefore important. While Card cannot add observations by going back to earlier years (since, in periods when the nominal minimum wage is constant and its real value is declining, his minimum wage variable is hard to define), adding lagged employment/population ratios (for adults and teenagers) in each state is feasible. Their addition make no difference to the results (if anything, the minimum wage coefficient increases).

Card (1992b) reports that teen employment grew faster in California than in neighboring states following the 1988 increase in its minimum wage. He also checks for effects on hours worked per week, but finds none.

Many of those whose wages increase in response to minimum wage increases are not teenagers, and many teenagers earn more than the minimum. With these facts in mind, Card and Krueger repeated the analysis for those whose demographic characteristics predict they would be low-wage workers.[34] The relationship between proportion actually in the \$3.35–4.25 range in each state and employment/population ratios is very similar to that found for teenagers.

Deere et al. (1995) take a seemingly similar approach and obtain quite different results. Using CPS data by state from 1985 to 1992, they estimate the equation

$$\ln(E)_{it} = \alpha_{it}\ln(E')_{it} + \beta_{90} + \beta_{91-92} + \gamma_i + \varepsilon_{it},$$

where E is the teenage employment/population ratio, E' is the employment/population ratio of all men 15–64, β_{90} and β_{91-92} are year-specific dummies to capture the effect of the minimum wage increases in 1990 and 1991, and γ_i is a state fixed effect.[35] Their estimates suggest teenage employment was 7% (males) and 11% (females) lower in 1991–1992 than it would have been had the minimum wage not been increased. For blacks, their estimate is 10%, marginally larger than the average of males and females of all races. They find similar, although smaller, differences for adult drop-outs.

Probably the most important difference[36] between the Card–Krueger and Deere–Murphy–Welch results is that the Deere–Murphy–Welch minimum wage variable does not vary according to the expected impact of the minimum wage on the state's labor market. Thus, the regressions present evidence that employment of groups likely to be affected by minimum wage increases were lower than would be forecast based on the experience of the late 1980s, but the inference that these are "minimum wage effects" is indirect. Curiously, dummy variables for earlier years are not statistically significant; the

[34] Using a linear probability model that predicts (among those employed) the probability of earning \$3.35 to \$4.25 in 1989, they identify the 10% of CPS respondents (whether employed or not) with the highest predicted probability of being in this interval.

[35] The model in the text is for males. They also estimate regressions for females and for blacks (both sexes pooled). For females, they include a time trend; for blacks, a dummy variable distinguishing males and females.

[36] Other differences beyond Deere et al.'s longer sample period are the different cyclical indicator (men 15–64 rather than population of both sexes), using the logarithm rather than the level of the employment/population ratios, and including 15 year olds in the dependent variable.

gradual erosion of the minimum wage prior to 1990 seems to have left no evidence of the expected improvement in teen employment.

On balance, studies that use states over time as the unit of observation and rely on cross-state variation in the minimum wage variable find minimum wage effects that are not consistently different from zero. Those that rely primarily on time-series variation in the minimum wage (i.e., do not control separately for year or trend) tend to be much more negative than the "pure" time series studies, but the variation associated with sample period and specification is troubling.

From a methodological viewpoint, the return of "degree of impact" measures that focus on proportion of workers directly affected or wage increases needed to comply with a new minimum, rather than the "relative minimum wage" variable that dominates the time-series literature, is significant. The degree of impact measures are conceptually cleaner, and remind us that an increase in the minimum wage does not raise average wages of teenagers as a group by anything close to the legislated increase. But these measures are not well-suited for studying periods when the minimum wage is constant, and so its impact should be declining. While there is more to be learned from a year in which the minimum wage increases by 10 or 15% more than average wages than from a year of modest decline, the periods between increases should together contain about as much information as the periods of increase.

6. Studies of low-wage industries

6.1. A traditional method of studying minimum wages

Observing changes in employment in low-wage industries following an increase in the minimum wage or extension of its coverage to a new industry has a very long history in the study of minimum laws in the US.

Kennan (1995, pp. 1952–1954) noted that as early as 1915, a Bureau of Labor Statistics study by Obenauer and Nienburg compared employment before and after a minimum wage for women was introduced in Oregon retail stores. They found that women's employment fell absolutely and relative to men's, but attributed much of the decline to a recession that occurred about the same time. Later studies compared employment in power laundries in New York (which also adopted a minimum wage for women) to employment in Pennsylvania (which did not), and of dry cleaners in Ohio to those of Indiana.

Peterson (1957) and Lester (1960) studied changes in employment in low-wage manufacturing industries as the minimum wage was increased from 40 to 75 cents per hour in 1950. They compared plants already paying 75 cents an hour to plants initially paying less, but reached different conclusions. Kennan (1995, p. 1954) notes that both recognized that the growth of high- and low-wage plants could have been affected by factors other than the minimum wage; e.g., in hosiery the high wage plants were further along in deploying new technology.

Studies of the impact of the 1959 increase in the minimum wage on low-wage manufacturing were undertaken by the US Labor Department. Establishments were classified by degree of "impact"; i.e., the proportional increase in average wages needed to bring all those below the new minimum wage up to that standard. There was general agreement in this instance that employment at high impact plants declined relative to low-impact ones, although the results were somewhat sensitive to the period over which the impact was measured. The tabular data presented by the Labor Department includes employment before and after the increase in both high- and low-impact parts of each industry; pooling these data we found each 10% increase in average wages needed to meet the requirements of the new law was associated with a 2–3% loss of employment (Brown et al., 1982, p. 521).

Similar studies were done when coverage of the minimum wage was extended to some employers in retail trade in the early 1960s. Here several comparisons are possible; different lines of business within retail trade differed in their degree of impact, and data on uncovered stores was also collected. Analysts at the time reached different conclusions as to whether the extension reduced employment (Brown et al., 1982, p. 517). Similar analyses were done in newly covered service establishments. Overall, our reanalysis of the published data finds negative but quite imprecisely estimated effects.

As this brief summary[37] indicates, these early studies implicitly identified at least four different ways of defining "treatment" and "control" groups, so that differences in employment change between treatments and controls could be calculated. The early Oregon retail trade study includes only covered establishments, but allows a comparison between adult women (whose wages were raised by the law) and adult men. This in effect identifies the elasticity of substitution between men and women, rather than the elasticity of labor demand. The early studies of power laundries and dry cleaning use states which did not implement minimum wage coverage as controls. In the later studies of the Federal minimum wage in manufacturing, high-impact establishments are the treatment group, and low-impact (i.e., high wage) units are the controls. In retail trade, the uncovered sector serves as the control for the newly covered treatments.

Reviewing this literature 30 or more years later, one is struck both by the ingenuity used in finding "control" groups and by the absence of persuasive argument in favor of the validity of the control group chosen or consideration of whether differences reported could be due to chance alone.

6.2. Methodological issues

From these variously defined treatment and control groups, an estimate of the treatment is obtained. If Y is employment, T and C stand for treatment and controls, and 1 and 2 stand

[37] I have emphasized studies that have the most in common with the more modern studies discussed below. A more complete survey is presented in Brown et al. (1982, pp. 514–522).

for the period before and after treatment, then the simple "difference in difference" estimate of the impact of treatment is

$$(Y_{T2} - Y_{C2}) - (Y_{T1} - Y_{C1}),$$

or, equivalently,

$$(Y_{T2} - Y_{T1}) - (Y_{C2} - Y_{C1}).$$

Given that "assignment" to treatment or controls is not random, there is always concern about the validity of the implicit assumption that the observed change for the controls, $(Y_{C2} - Y_{C1})$, tells us what would have happened to the establishments faced with raising wages to comply with the law, had the minimum wage change not taken place.

The control groups in early studies of the minimum wage are vulnerable on this account. The Oregon retail trade minimum wage was implemented in a recession, which might be expected to influence men's and women's employment differently. Dry cleaning employment in Indiana may have been growing at a different rate than that in Ohio prior to the Ohio minimum wage; or Ohio may have been subject to a different idiosyncratic shock in that year. High-wage hosiery plants were recognized to be using more advanced technology. High-wage plants may be located in different areas than low-wage plants, a particular concern if the product is sold in regional rather than national markets. The Labor Department was, however, careful to select both treatments and controls from the same broad region, typically the South, or report data separately by region. Newly covered retail trade businesses were larger than their uncovered neighbors. Assuming that small and large retail firms would have grown at the same rate is risky. And, if the minimum wage leads covered stores to raise prices and lose business, uncovered stores are a likely beneficiary.

In my reading of these early studies and later critiques of them, I find less discussion of two other concerns that have been raised about more recent studies (Hamermesh, 1995, p. 835): Y_{T1} must not be affected by the treatment, and period 2 must be sufficiently after the minimum wage becomes effective for the impact to be felt. This is, in effect, the same lags and leads issue that came up in the discussion of the time series results. But the issue of Y_{T1} being contaminated is likely to apply with greater force here. In a time series context, one has many quarters or months prior to the minimum wage increase, and the regression in effect averages these. In the studies under review here employment is typically measured only once prior to the change. If one or two quarters' employment is somewhat contaminated by anticipatory employment reductions (or increases, if the employer tries to beat the price hike) the effect will be more severe where averaging with earlier uncontaminated periods is absent.

Concern that period 2 is "long enough" after the minimum wage increase is more a matter of interpretation than a condition for unbiased estimates of the treatment effect. If T2 is a few months after the increase, the data will give us an estimate of shortterm effects; if the gap is several years we will get longer-run estimates.

In general, economists tend to be more interested in longer-run effects. However, the longer the interval between the periods 1 and 2 the greater the likely error induced by any non-comparability of the control group (e.g., if treatment and control groups are subject to different underlying trend growth rates in employment, the bias is proportional to the difference between T1 and T2).

6.3. Recent studies of a low-wage industry: retail trade

In an analysis of the effects of the 1988 California minimum-wage increase on wages and employment in retail trade, Card (1992b) finds that wages grew about 5% faster in California retail trade than in the retail trade industries of a group of neighboring states without minimum-wage increases. Depending on the choice of base year and comparison group, employment increased one percentage point faster or slower than elsewhere following the increase. Given the small effect of the California minimum-wage increase on average wages in retail trade (roughly half the size of the effect on teenagers' wages) it is not surprising that employment effects are difficult to detect.[38]

Kim and Taylor (1995) use within-state variation to re-analyze Card's conclusions. They compare wage and employment growth in different sub-industries in retail trade, and in different counties, using County Business Pattern (CBP) data. They find little consistent relationship between wage growth and employment growth in the years prior to the 1988 increase (as might be expected given that both demand and supply shifts are at work), but a significant negative relationship (with estimated demand elasticities of -0.9 based on industry and -0.7 based on county contrasts) in 1988–1989, the year following the minimum-wage increase. By itself, a 5% wage increase and a demand elasticity of -0.8 would produce a 4% employment decline; Kim and Taylor argue that robust demand in 1988–1989 obscured this loss.

Ordinary least squares estimates suffer from two problems. First, the CBP "wage" is quarterly payroll divided by employment in one specified pay period. Random fluctuations in employment in this pay period relative to the quarter as a whole (or simple measurement error in reporting employment) will induce a negative correlation between measured wages and employment. Second, there is the standard simultaneous-equations bias. Kim and Taylor's instrumental variable estimates are nearly identical to those using OLS for 1988–1989, and essentially patternless in earlier years.[39]

Kim and Taylor use the lagged value of the average wage (low average wages mean a larger wage increase in response to the minimum) and average firm size (which they argue is positively related to the wage increase because of greater compliance by large firms). They note that both are significantly related to wage growth, and easily pass an over-identification test. Card and Krueger (1995) note, however, that the demand elasticities

[38] Machin and Manning (1994) find significant effects of minimum wages on wages, but if anything positive effects on employment, using data from industries in the UK that are covered by Wages Councils.

[39] The bias that arises from the calculation of the wage measure would bias the estimate toward -1. The simultaneous equation bias would bias it toward zero. The latter might be smaller in 1988–1989, when more of the wage variation is presumably coming from the exogenous minimum wage increase. Weak instruments would bias the IV estimates toward the OLS estimate.

one gets from using the two instruments separately are substantively different, and much rides on the use of firm size as an instrument. They also note that the results for 1989–1990 (which, without a minimum wage increase, should have reverted to the patternless of the earlier years) are in fact quite similar to those for 1988–1989; evidence of a negative demand elasticity vanishes if one looks at 1987–1989 changes; i.e., using 1987 as the base year.[40] Thus, there are uncomfortable concerns surrounding Kim and Taylor's otherwise robust elasticity estimate.

6.4. *Recent studies of a low-wage industry: fast food*

Several recent studies have used the difference-in-difference methodology to study the impact of minimum wage increases on employment in the fast food industry. Fast food restaurants are an important employer of minimum-wage workers, and the larger chains have sometimes taken public positions against minimum wage increases. Absence of tips make it relatively easy to measure the hourly wage and hence who is a minimum wage worker.

Katz and Krueger (1992) studied employment responses of major fast-food chains in Texas (a relatively low-wage state, and hence a sensible place to look for minimum-wage impacts) between December 1990 and August 1991, a period which brackets the April 1991 increase in the Federal minimum wage from \$3.80 to \$4.25. On average, restaurants in their sample needed to raise wages 8% above 1990 levels to reach \$4.25, although of course this ranged from zero to 12%.

Katz and Krueger divided their sample according to the starting wage paid in December of 1990. Those paying \$3.80 would face the largest wage increases the following April, while those already paying \$4.25 would not need to raise wages in order to comply with the law. Restaurants with starting wages between \$3.80 and \$4.25 made up a third, medium-impact group.

A simple summary of their findings is that restaurants initially paying \$3.80 increased the log of full-time employment by 0.168, while those initially paying \$4.25 or more reduced mean log of employment by 0.168. Employment at restaurants whose wage initially fell between these extremes was essentially unchanged. A regression of the change in log employment on the proportional wage increase needed to comply with the new law (equal to max(0, ln(\$4.25/1990 starting wage)), and so equal to zero, by definition, for the high-wage group), yields a coefficient of 1.85 (SE = 1.00) for employment measured in bodies and 2.64 (SE = 1.06) for employment in full-time equivalents (FTEs).[41] The least absolute deviation estimate of the latter coefficient was 1.16 (0.55).

[40] Given the tangled history of the California minimum-wage increase (it was announced by a state commission in December 1987, effective July 1988, after a May 1987 legislative attempt at repeal was vetoed by the Governor), I would have thought the 1988 data might be contaminated by the impending increase and so using 1987 as a base would produce a larger estimate.

[41] These regressions included dummy variables for company ownership, chain to which the restaurant belonged, and the logarithm of city population. None of these mattered.

There is no sign that restaurants forced to raise their wages to meet the new minimum wage requirement reduced employment.

Card and Krueger (1994) surveyed fast-food restaurants in Pennsylvania and New Jersey in February–March and again in November–December 1992, before and after an April 1992 increase in the New Jersey minimum wage from \$4.25 to 5.05. Compared to the Texas study, the analysis of the New Jersey law had four advantages. First, it allows two ways of defining treatment and control groups (New Jersey versus Pennsylvania, and, within New Jersey, the high- versus low-impact contrast used in Texas). Second, the minimum wage increase in question was larger, and the fraction initially in the high-wage, no-impact group (>\$5.05) was larger. Third, interviewers were more persistent in getting interviews, and in-person visits determined whether non-responding restaurants had closed. Fourth, the sample was more than three times as large (357 versus 104).

Card and Krueger found that full-time equivalent employment increased faster in New Jersey than in Pennsylvania (by 2.75 FTEs, SE = 1.34), and faster in the New Jersey restaurants that made the largest increases in starting pay in order to comply with the law than in those already paying \$5.05 or more (by 3.36 FTEs, SE = 1.30), on a base of roughly 21 FTEs per restaurant.

Regressing the proportional change in FTE employment on the required proportional increase in starting wage (= 0 for Pennsylvania and high-wage New Jersey units) gave elasticities of about 0.34 (SE = 0.26), with small variations depending on control variables. These become statistically significant if the data are weighted by initial employment (elasticity = 0.81, SE = 0.26).

Card and Krueger note that the unemployment rate increased faster in New Jersey than in Pennsylvania during 1992. This makes it unlikely that the employment gains in New Jersey were due business-cycle differences. It is harder to rule out this explanation for the within-New Jersey results, although experiments with geographically defined dummy variables make relatively little difference for the estimates.

Having two natural comparisons – New Jersey versus Pennsylvania, and high- versus low-wage restaurants in New Jersey – provides opportunities to address the control group issue. Neither high-wage New Jersey units nor those in Pennsylvania were required to raise wages in response to the New Jersey increase; if both are valid control groups for the New Jersey restaurants who were forced to raise wages, the employment changes of the two control groups should be the same. In fact, the change in mean FTE in Pennsylvania was −2.16 (1.25) while among high-wage New Jersey restaurants it was −2.04 (1.14). So if there are problems with the control groups, they must have problems with similar impacts on both.

The unexpected results of these two studies have, not unexpectedly, generated considerable controversy. Hamermesh (1995) is particularly critical of the timing of the surveys relative to the minimum wage increases, arguing that the "before" interviews occurred after employers in Texas and New Jersey knew that minimum-wage increases were coming. (In New Jersey, there was a serious movement to stretch out the increase over 2 years, which failed just before the increase took place, so what employers knew is not clear.) Similarly, he argues that the second interview took place before serious adjustment could occur. Card and Krueger (1995) rely on the traditional argument that adjustments are likely to occur with neither leads nor lags, given high turnover rates in the industry.

One disadvantage of special-purpose surveys like those considered here is that one is unlikely to be "in the field" quickly enough that T1 precedes any possible adjustment to the increase; that would require an ongoing survey program or prescience denied those who are in the industry. While these timing issues might lead to underestimating an employment reduction by affected restaurants, it is hard to see how it could change the sign of the estimated effect.

Welch (1995) raises the possibility that the employment gains reported by Card and Krueger came at the expense of non-chain restaurants, for which the minimum wage may have been an even larger burden. Card and Krueger note that this hypothesis is less plausible as an explanation for the within-New Jersey results, since presumably the demise of mom and pop restaurants benefited high- as well as low-wage chain restaurants. Without detailed information on the location of the two groups of New Jersey restaurants, it is difficult to evaluate their reply.

Welch also criticizes the survey methods of the New Jersey study, and points to implausible employment and wage changes at individual restaurants. In particular, a majority of those with wages initially above the new (federal in Pennsylvania, state in New Jersey) minimum reduced nominal starting wages at T2; virtually all of those with the lowest (highest) employment at T1 increased (reduced) employment by T2. This sort of mean reversion would be expected if employment is measured with considerable error (uncorrelated across surveys).

Both the Texas and New Jersey studies have limited assessments of the accuracy of the survey reports in the form of correlations between the original response and a re-interview. For employment reports in Texas, this is 0.76, in New Jersey 0.70. If we assume that the reporting errors are uncorrelated both with true values and with each other, these reliabilities are equal to the ratio of the variance in true employment to the variance in measured employment. This in turn would imply that the fraction of total variance in the *change* in employment that appears to be measurement error is just under two-thirds in Texas and nearly half in New Jersey.[42] Details of the calculation below:

[42] Let E and E' represent the original and re-interview reports, and E^* be the true value of employment. Then $\text{corr}(E, E') = \text{cov}(E, E')/[\text{var}(E)\text{var}(E')]^{0.5} = \text{var}(E^*)/\text{var}(E)$ and $\text{var}(e)/\text{var}(E) = 1 - \text{corr}(E, E')$. So on this assumption – which is the benchmark assumption in the re-interview literature – 76 (70)% of the variance in reported employment in Texas (New Jersey and Pennsylvania) would be "true", and the remainder error. For changes in employment assume that the errors at T1 and at T2 are uncorrelated with true values and each other. Then $\text{var}(E_{T2} - E_{T1}) = \text{var}(E_{T2}^* - E_{T1}^*) + 2\text{var}(e)$.

	TX			NJ + PA		
	T1		T2	T1		T2
SE of mean employment	0.49		0.46	0.65		0.63
Sample size	398		396	104		104
Variance of employment	95.6		83.8	43.9		41.3
Reliability	0.70		0.70	0.76		76
Variance of error	28.7		25.1	10.6		9.9
SE of mean Δemployment		0.46			0.65	
Sample size		384			104	
Variance of Δemployment		81.3			43.9	
Variance of error		53.8			20.5	
Variance of true change		27.5			23.4	

It is not clear, however, that these difficulties can account for Card and Krueger's results. Unless the errors in measuring employment were other than random, they would inflate standard errors but would not bias coefficients. Errors in measuring T1 starting wages would presumably lead to some misclassification of New Jersey restaurants' "degree of impact" groups; this should bias the "wage increase required" coefficient toward (but not through) zero for within-New Jersey comparisons but have no effect on the New Jersey–Pennsylvania comparison.

Neumark and Wascher (1998) collected similar data for Pennsylvania and New Jersey fast-food restaurants. Because they were concerned about measurement error in the Card–Krueger data, they relied on payroll data collected from the restaurants or their headquarters. The data differ from Card and Krueger's in a number of ways, although Neumark and Wascher go to considerable effort to show that most of these differences would not account for the differences in results between their data and Card and Krueger's.[43]

They find similar mean changes in employment, but much smaller standard deviations (9.6 versus 3.2 FTEs). The difference-in-difference estimate of the effect of the New Jersey minimum wage increase is -1.0 (SE $=$ 0.43), versus 4.0 (2.2) in the most nearly comparable estimates from the Card and Krueger data. Various refinements do not change either Neumark and Wascher's finding or the message of the corresponding reworking of Card and Krueger's.

Neumark and Wascher do offer one clue to the explanation: of the 5-FTE difference in the

[43] Neumark and Wascher's data are from payroll records supplied by the fast-food outlets or their headquarters, and employment is measured in hours rather than in bodies. They convert to full-time-equivalents dividing hours by 35. Their data refer to non-management employees, but show that in Card and Krueger's data this makes little difference. Finally, their sampling is from the Chain Operators Guide, while Card and Krueger used the Yellow Pages. Neumark and Wascher sampled from the same zip codes as Card and Krueger, but in some zip codes their sample is larger and in others smaller.

two estimates, four-fifths is due to different estimates of mean employment changes in Pennsylvania −3.0 (2.14) for Card and Krueger and 1.0 (0.34) for Neumark and Wascher.[44]

Both Neumark–Wascher and Card–Krueger appeal to data collected by the BLS to resolve the controversy. Neumark and Wascher find that employment in Eating and Drinking establishments (not necessarily "fast food") increased more slowly in New Jersey than in Pennsylvania (by 0.3 percentage points), although this is reversed if one limits the sample to the "border" counties originally sampled by Card and Krueger. They note that in either case, the New Jersey–Pennsylvania difference was less in New Jersey's favor in 1992 than it was in either of the surrounding years.

In response, Card and Krueger (1998) analyze data on employment at fast-food chains from BLS "ES-202" data. Results from a longitudinal file (following the same establishments over time, including closings) show that employment grew insignificantly faster (0.2–0.5 [SE = 1.0] workers per establishment, or less than 1% [SE = 3%] with a proportional change specification) in New Jersey between February–March and November–December 1992. Comparing employment across all establishments in February and November (and so including units that opened in between) showed employment growing about 4 percentage points faster in New Jersey, but a time-series plot of the two states' employment shows that this result is quite sensitive to choice of end month.[45]

In principle, fast-food restaurants (or eating and drinking places more generally) are a promising place to look for minimum-wage effects. The fraction of these workers who are directly affected by the minimum wage is considerably higher than the comparable fraction for teenagers,[46] and so the difference between the elasticity of employment with respect to the minimum wage and the elasticity of demand for low-wage labor should be much smaller.

Nevertheless, my reading of the evidence on employment changes following the Texas and New Jersey increases is that it is very hard to reject the hypothesis of no effect. The Texas and New Jersey papers' reliance on special-purpose surveys meant that questions about pre-existing trends in the two states must be handled indirectly (there is no evidence on whether employment was growing faster in the treatment or control group prior to the

[44] One other aspect of Neumark and Wascher's data has proven controversial. The initial round of data collection was conducted by the Employment Policies Institute which "has a stake in the outcome of the minimum wage debate"; Neumark and Wascher then undertook a second round to produce a combined sample as representative as possible of the zip code areas initially considered by Card and Krueger. Evidence of negative minimum wage effects is stronger in the EPI sample than in the sample collected directly by Neumark and Wascher. Neumark and Wascher went to considerable lengths to verify the accuracy of the EPI-collected data. The possibility of non-random response (particularly in the original EPI sample) remains. Since the Card–Krueger and Neumark–Wascher data essentially agree about New Jersey, the response bias would have to be in Pennsylvania. As it happens, one franchise owner supplied all of the original EPI observations in Pennsylvania.

[45] Following the 1996 increase in the Federal minimum wage, which brought the minimum in Pennsylvania closer to the New Jersey state minimum, fast-food employment grew substantially faster in Pennsylvania, although Card and Krueger acknowledge that other factors besides the minimum wage are probably at work here.

[46] Gilroy (1981) reports that 44% of teenagers earned w_m or less. Among non-supervisory workers in eating and drinking places, the corresponding fraction was 58%.

change). In principle, ES-202-based analyses could circumvent this problem, and allow a more intensive analysis of how estimated effects depend on the choice of "before" and "after" time periods.[47] From a broader perspective, it is important to remember that whatever happened in Texas and New Jersey are just two data points, and (again, in principle) ES-202 data could be exploited to pool several such state-based experiments.

7. Comparisons of low- and high-wage workers

With the availability of longitudinal data on individual workers, it became possible to compare the employment experience of individual workers who were directly affected by a minimum wage increase with those who were not. These studies can also be understood as applications of the difference-in-difference methodology.

The first such study (Egge et al., 1970) used data from the National Longitudinal Survey of Young Men (age 14–24 in 1966) to study employment transitions surrounding the 1967 increase in the minimum wage from \$1.00 to \$1.40. They compared those paid between \$1.00 and \$1.40 on their current or last job as of the 1966 survey (treatment group) to those paid more than \$1.40 (controls). Their exact results depended on how employment is defined (employment at survey date or weeks worked in previous year), and the age-enrollment group considered. Overall, they concluded there is little evidence of an adverse effect on employment.

Using higher-wage workers as controls raises three issues. First, Egge et al. note that low-wage workers are less likely to be employed, and more likely to leave employment, even in the absence of the minimum wage. Second, there is little reason to believe that business-cycle effects between year 0 and year 1 will be the same for high- and low-wage workers. Third, by construction each individual in the sample ages by one year, and age effects on employment may well differ for low- and high-wage workers.

Linneman (1982) focuses on adult employment changes due to the 1974–1975 minimum wage increases. He begins with the individual's 1973 wage (if s/he worked in 1973) or a predicted 1973 wage for non-workers. This wage is adjusted upward for inflation (and for changes in experience, etc.) to form a predicted 1974 wage, $\hat{w}$. Individuals are then classified as above- or below-minimum wage depending on whether their predicted 1974 wage $\hat{w}$ is above or below the 1974 minimum w_{m}; for those with $\hat{w} < w_{\mathrm{m}}$, he defines $\mathrm{GAP} = w_{\mathrm{m}} - \hat{w}$ (if $\hat{w} > w_{\mathrm{m}}$, $\mathrm{GAP} = 0$).

Among those whose predicted 1974 wage is below w_{m}, the proportion working at all during the year falls from 0.64 to 0.51, while the proportion working remains constant at 0.72 for those above the minimum wage. Similarly, among those working, annual hours

[47] Neumark and Wascher present regressions that relate the proportional change in employment (in New Jersey and in Pennsylvania) to the proportional change in the nominal minimum wage, controlling for changes in the unemployment rate. They find elasticities of −0.11 to −0.16, some of which are close to statistically significant. However, their minimum wage measure does not reflect the usual presumption that the effect of the minimum wage is eroded by increases in nominal wages in each state as a whole.

fall by 237 (14%) for those (predicted to be) below the minimum wage, and unchanged for those above (his Table 4). Given that the logarithm of the real minimum wage increased by only 0.12 between 1973 and 1974 – less if one takes account of the fact that the minimum increased in mid-year – these are very large employment changes.

Linneman then estimates probit equations for the probability of working in 1974 or 1975, and OLS hours of work equations, as functions of GAP and the standard set of control variables. Evaluating the importance of the minimum wage once other variables are held constant is difficult, but it appears that the effect of the minimum-wage increase implied by his estimates is much smaller.[48] Among those whose predicted wage would be above the minimum wage, probability of employment falls, and hours worked if employed rise, even for those earning three times the minimum wage.

Linneman concludes by combining wage, employment, and hours-worked effects. He finds that, on average, earnings increase, with those directly affected losing about $78 per year, while those above the minimum wage gain an average of $69. Since the (predicted) wage is not held constant in any of these calculations, it is hard to know how much of these effects should be attributed to being affected by the minimum wage and how much to being a low-wage worker.

Ashenfelter and Card (1981, reported in Card and Krueger, 1995) also study the 1974–1975 minimum wage increase, but they divide their sample of those employed in 1973 into four groups according to their 1973 wage (greater or less than the 1975 minimum wage of $2.10) and coverage status (based on reported industry of employment). The motivation here is the recognition that higher-wage workers are likely to have quite different employment probabilities than low-wage workers, and so they exploit differences in coverage to construct a better control group. They find 1995 employment is lower for those initially earning less than $2.10, but that this difference is the same whether or not they were directly affected by the law.

Using coverage status to define the control group has its own problems. First, coverage status depends on industry and employer size, so that assignment based on (worker-reported industry) inevitably misclassifies workers. Second, for whatever reason, wage distributions for uncovered-sector employers also show a spike at the minimum wage (see Section 8), so the control group is, to some extent, affected by the treatment. Third, in markets with high turnover rates, it is not clear that any negative effect of the minimum wage on employment would be concentrated on those *initially* employed in the covered sector.

[48] Consider first a worker paid the minimum wage ($1.60) in 1973, for whom GAP in 1974 would equal 0.118 (ln 2.00 − ln (1.60 + inflation factor)). Linneman's employment probit for 1974 is $-0.3433\text{GAP} + 0.0588(\text{GAP}^2)$ + other variables. Evaluated at GAP = 0.118, the first two terms sum to −0.04. But the change in the probability of employment is this −0.04 multiplied by the value of the standard normal density, which is always less than 0.4. So the implied change in probability is −0.016, not negligible but far smaller than the 0.13 decline in probability of working among the workers below the minimum wage. Based on a similar calculation, the change in hours worked (for those employed at all) is −60 (versus a gross change of −237). For 1975, the corresponding change in employment probability is −0.004, and in hours worked is −20.

Currie and Fallick (1996) use the National Longitudinal Study of Youth (age 14–21 in 1979) to measure the effects of the 1980 and 1981 minimum wage increases, using panel data for 1979–1987. They define a dummy variable ("BOUND") equal to one if the individual is employed in year $t-1$ at a wage $w_{\mathrm{m},t-1} < w_{t-1} < w_{\mathrm{m},t}$, and zero otherwise. In most specifications, they also require that employment in year $t-1$ be in a "covered" industry, although they acknowledge that their ability to infer coverage (based on industry) is limited. They also construct a "WAGEGAP" variable, equal to $w_{\mathrm{m},t} - w_{t-1}$ when BOUND $=$ 1, and WAGEGAP $=$ 0 otherwise. In 1982 and later years, BOUND and WAGEGAP $=$ 0 for all observations.

They then estimate an employment equation

$$E_{i,t} = \alpha \mathrm{WAGEGAP}_{it} + X_{it}\beta + \gamma_i + \delta_t + \varepsilon_{it},$$

where $E_{i,t} = 1$ if i was employed in year $t-1$ and again in year t, and $E_{it} = 0$ if i was employed in year $t-1$ but not in t. They find that α is negative and statistically significant, as is BOUND when it is used instead of WAGEGAP. The probability of remaining employed is reduced by about 0.03 for the fifth or so of the sample that is directly affected by the increases in 1980 or 1981.[49]

Currie and Fallick recognize that the control group issue is fundamental to their study. They note that their control group is in fact composed of three sub-groups: those who earn less than the old minimum wage in $t-1$, those who earn more than the new minimum wage in $t-1$, and those who are working in uncovered jobs in $t-1$. Separating these groups reveals that those bound by the new minimum wage increase suffer employment reductions relative to those who initially earned more than the minimum wage, marginally relative to those who earned less than the old minimum wage, and in fact do much better than those in uncovered jobs. In effect better-paid workers are the heart of the control group.

Are better-paid workers an appropriate control group? Because we expect low-wage workers will have lower employment-retention probabilities in general, one starts with serious doubts on this score. Currie and Fallick do, however, include fixed effects; so in effect their regressions tell us that employment of those who are bound by the minimum wage increases in 1980–1981 was lower, relative to higher-wage workers, in 1980–1981 than in other years. Card and Krueger (1995, p. 228) question whether the fixed effects are really fixed–employer information about productivity, which presumably drives the re-employment probabilities, evolves rapidly for young workers. It is also possible that the 1981 recession was harder on low-wage workers than others, which would contribute to Currie and Fallick's negative coefficient.

[49] Abowd et al. (1997) find that French men age 25–30 who earn the minimum wage are much less likely to remain employed following an increase in the minimum, compared to men paid just above the minimum. Results are weaker for younger males (which they attribute to employment promotion programs for younger workers) and for teenage and young-adult women.

Currie and Fallick respond to these concerns by defining another dummy variable NEARMIN, equal to 1 if w_{t-1} is within 0–15 cents greater than $w_{m,t-1}$, to capture the effect of having low wages apart from being BOUND. When added to the regression it is insignificant (both practically and statistically), and the coefficient of the minimum-wage variable does not change appreciably. This is a reasonably reassuring response to some of the control-group concerns,[50] although the possibility that the recession was harder on low-wage youth is harder to reject.

While the Currie–Fallick specification appears at first glance to apply the same methodology to individual data that Card and Krueger used in analyzing state-level data, there is a subtle but perhaps important difference. Unlike Card and Krueger's analysis of the 1990–1991 minimum wage increases, Currie and Fallick include years when the nominal minimum wage was constant, and so the real minimum wage was falling. One might have expected the employment-retention probabilities of low-wage workers to *increase* in these years, as the minimum wage became less binding.[51] Nothing in Currie and Fallick's specification captures this response directly, and it is possible that the coefficient of their dummy variable for low-wage workers is biased (upward) on this account.

While Currie and Fallick devote a lot more effort to reducing any bias due to control-group problems than did Egge et al., these efforts do not change the results very much. The difference between their results and Egge et al.'s is striking. Since the fixed effects and NEARMIN do not account for the difference, what does? Low-wage workers faring well in expansions and poorly in recessions is a tempting conjecture in the absence of obvious alternatives.

8. Impacts of minimum wages on other outcomes

Thus far, we have reviewed studies that focus directly on the link between the minimum wage and employment. In this section, our focus shifts to the effect of the minimum wage on related outcomes. While a range of such outcomes have been studied, the emphasis here is on outcomes that may provide indirect clues about the effect of the minimum wage on employment.

8.1. Wage distribution spike at the minimum wage

Among those who are employed, the distribution of ln(wage) tends to look bell-shaped, with occasional spikes at round-dollar amounts (particularly if the data come from house-

[50] In fact, their NEARMIN variable is close to Card and Krueger's (1985, p. 239) suggestion for testing the validity of their control group.

[51] Abowd et al. (1997) find that young workers in the US who are employed at the minimum wage are much less likely to have been employed the previous year and so a falling real minimum wage facilitates their entry into employment. An alternative reading of this evidence is simply that those entering employment often do so at the minimum wage.

Table 4
Frequency of minimum-wage and near-minimum-wage employment[a]

Month	w_m (\$)	w_m/ahe	Type of survey[b]	Sample[c]	Interval (\$)	Frequency (%)	Interval (\$)	Frequency (%)	Interval (\$)	Frequency
Oct.–Dec. 1996	4.75	0.40	H	WSH	<4.75	4.3	4.75	2.8		
				WSH 16–19		14.8		11.9		
Jan.–Dec. 1992	4.25	0.40	H	WSH	<4.25	3.0	4.25	4.8		
				WSH 16–19		9.3		19.8		
Jan.–Dec. 1989	3.35	0.35	H	WSH	<3.35	2.2	3.35	2.9		
				WSH 16–19		6.1		11.7		
Jan.–Dec. 1988	3.35	0.36	H	WSH	<3.35	2.2	3.35	4.3		
				WSH 16–19		6.2		16.9		
				WS	≤3.35	5.3				
				WS 16–19		23.6				
May 1978	2.65	0.47	H	PNSWS	<2.65	11.3	2.65–2.99	11.2		
			E	PNSWS		4.3		18.2		
May 1973	1.60	0.41	H	WS	≤1.60	7.9				
				WS 16–19		26.3				
Apr. 1970	1.60	0.50	E	PNSWS	<1.60	6.8	1.60–1.64	4.0	1.75–1.79	2.8
				PNSWS-OC		0.6		2.9		2.0
	1.45			PNSWS-NC	<1.45	8.3	1.45–1.49	6.5	1.60–1.64	6.9
	NA			PNSWS-U	<1.60	30.4	1.60–1.64	7.2	1.75–1.79	5.4

[a] Sources: 1996, 1992: US Census Bureau (1997, Table 675; 1993, Table 676); 1988: Haugen and Mellor (1990, Tables 2 and 1); 1978: Gilroy (1981, Table A-3); 1973: Gilroy (1981, Table 22); 1970: Peterson (1981, Table 18).

[b] H, household; E, establishment.

[c] WS, wage and salary; H, paid by the hour; NS, non-supervisory; P, private (non-agricultural); OC, old coverage (covered prior to 1967); NC, new coverage (covered by 1967 Amendments); U, largely uncovered.

hold surveys rather than employer reports). Often there is another spike, at the minimum wage, even when the minimum is not a round-dollar amount. Spikes at the minimum wage are stronger when the minimum wage is more binding; e.g., in wage distributions for teenagers rather than for all workers, and in years when the minimum wage has been raised rather than after several years of a constant nominal and eroding real minimum wage.

The first two lines of Table 4 show that, in 1996, 26.7% of teenagers paid by the hour reported wages at or below the minimum wage, including nearly 12% who were at the minimum, while for adults the mass point at the minimum is much less pronounced.[52] A similar pattern is present in other years, with the fraction at the minimum increasing between 1989 and 1992, reflecting the 1990–1991 increases in the minimum wage. Indeed, Card and Krueger (1995, pp. 156–157) show that the fraction of teenagers receiving $3.80 per hour was much larger in 1990 (when the minimum wage was $3.80) than in 1989 (when it was $3.35), and the fraction receiving $4.25 was much larger in 1991 (when the minimum wage was $4.25) than in earlier years.

Data for 1988 show that including workers not paid by the hour (calculating their wage as usual weekly earnings divided by usual weekly hours) has relatively little effect on the fraction at or below the minimum wage, because low-wage workers tend to be paid by the hour.

For 1978, data are available from an establishment survey as well as the CPS. Employers and workers tend to report similar fractions of workers paid less than $3.00 per hour, although the fraction at or slightly above the minimum is much higher in the employer reports. Whether this represents employers hiding non-compliance or workers reporting their wages with less precision than we would prefer is unclear (Gilroy, 1981).

The data for 1970 reveal several interesting patterns. First, even among non-supervisory workers as a group, there is a noticeable jump in the wage distribution at the minimum wage that is, for example, more pronounced than at $1.75 (or, not shown, $2.00). Second, employers first covered by the 1967 Amendments, for whom the legal minimum was $1.45, often paid the basic minimum. Third, even the wage distribution of the *un*covered sector exhibits a spike at the minimum.

Wage distributions for European countries also exhibit spikes at the minimum wage, often more pronounced than those in the US (Dolado et al., 1997, Table 1). Machin and Manning (1997, p. 735) attribute this to the minimum wage being higher, relative to the average wage, in Europe than in the US.

These spikes pose a puzzle for nearly all of the models presented in Section 2. Suppose a firm employs two groups of workers, one at $5.00 and one at $4.50, in the absence of the minimum wage. Now a $5.00 minimum wage is imposed. What the data show is that at least some of the $4.50 workers are now employed at $5.00. While one might imagine that employment of low-skill workers had declined enough to raise the marginal product of those who remained by 11%, enabling the some of the $4.50 workers to remain employed,

[52] DiNardo et al. (1996) show that minimum wage spikes are occasionally evident in the male wage distribution, and stronger in the distribution of female wages, particularly in the late 1970s when the minimum wage was high relative to average wages.

employers should now compete actively for those initially paid \$5.00, bidding up their wages and unpiling the spike at \$5.00.

8.2. Offsets

In the absence of a minimum wage, non-wage job characteristics are determined by a comparison of the cost of improving such characteristics against workers' willingness to pay for improvements by accepting lower wages. Other things equal, an increase in the wage (due to minimum wage legislation) increases the number of workers willing to work while reducing employers' demand for workers. This imbalance gives employers an incentive to look for cost-saving changes in non-wage job characteristics; if successful, such offsets could reduce the predicted impact of the minimum wage on employment. Moreover, offsets can potentially account for the spike at the minimum wage; among workers receiving the minimum wage, reductions in non-wage job characteristics would be most severe for those whose wage was raised most by the law.

In an influential paper on unemployment, especially among youth, Feldstein (1973) suggested that the minimum wage discouraged employers from providing training. Ordinarily, workers pay for training (all of general training, part of specific training) by accepting lower wages than they could earn in a job without training; this wage reduction offsets the cost the employer would otherwise bear in providing the training. The minimum wage interferes with this process because reducing the wage below the required minimum is illegal (even if compensation, including value of training, is above the minimum).

It is clear that, to the extent that the minimum wage reduces employment, on-the-job training and the general human capital that one obtains just by establishing a work record are reduced. It is less clear that, among those who are employed, human-capital acquisition is impaired to any significant degree; the training content of such jobs, and the scope for reducing training (without rendering the worker useless) may be limited.

Available evidence suggests that training for low-wage workers is significant enough to be a potential offset to minimum wage increases, but only for some of these workers. In a 1980 survey of employers, "Almost half of the jobs held by low-wage workers involve formal training," averaging 12.3 days. About 70% of the jobs held by low-wage workers involve "on-the-job" training, defined as jobs in which it took 5 or more days for a worker to reach company standards. On average, 24.6 days were required to reach that standard (Converse et al., 1981, p. 260). However, employers did not report cutting training in response to the minimum wage; in fact about an eighth of all establishments with minimum wage workers said they had increased the responsibilities of low-wage workers in order to offset the minimum wage increase, and half of these reported increased training along with the increased responsibility (Converse et al., 1981, p. 280).

Several studies (Lazear and Miller, 1981; Leighton and Mincer, 1981; Hashimoto, 1982) tried to determine whether minimum wage increases were associated with flatter age-earnings profiles. They obtained mixed results. In general, there are two difficulties with such approaches. First, it is likely that on-the-job training is a complement to other forms of human capital such as schooling, so that low-wage workers would get less training even in the absence of a minimum wage. Second, an increase in the minimum wage has the immediate effect of increasing the wages of directly affected workers. If the increase has no effect on such workers' wages several years later (when they would, in any case, have earned more than the new minimum), the age-earnings profile will be "flatter", but this is an artifact of the fact that the minimum wage is less likely to be binding for older workers, not (necessarily) a sign that less training is being produced (Card and Krueger, 1995, p. 171).

On balance, there does not seem to be much evidence that reductions in training are a significant offset to the increase in labor costs due to the minimum wage.

Perhaps the simplest way of offsetting the effects of a minimum-wage increase is to reduce the fringe benefits offered to workers. However, relatively few minimum-wage workers receive health insurance or participate in pension plans, so scope for such reductions is limited. Wessels (1980) reports that, in 1972, among low-wage firms (average hourly wage of $2.00 or less), only 3% of non-office workers had pension plans and 27% had health insurance; among employers offering such benefits, each accounted for only 2% of payroll. Paid leave was more common (58% of workers) but again accounted for only 2% of payroll. Not surprisingly, therefore, only 2% of establishments surveyed said they responded to a minimum wage (in retail trade in New York state) by reducing such fringes.

Fast-food restaurants provide a more promising although atypical opportunity to search for offsetting changes in fringe benefits, because such employers often provide free or reduced-price meals to their employees. Katz and Krueger (1992) asked their sample of fast-food managers in Texas whether fringe benefits had been reduced in response to the increased minimum wage. While 91% of their sample provided some fringe benefit(s) to workers, only 4% reduced fringes following the minimum wage increase, and this proportion was no higher in those restaurants forced to raise wages in order to reach the new minimum wage. Card and Krueger (1994) similarly find no significant difference in free or reduced-price meals when comparing New Jersey and Pennsylvania outlets, or high- versus low-impact outlets within New Jersey.

Another fringe benefit reported by Card and Krueger is the bonus paid to employees who help recruit new workers–just under a quarter of the restaurants surveyed had such bonuses. While one might expect a higher minimum wage would make recruiting easier and so make such bonuses less necessary, their use declined slightly faster in Pennsylvania than in New Jersey.

Overall, then, there is little evidence that minimum wage increases are offset by cutting fringe benefits.

While many low-wage employers provide little training and few fringe benefits – and so cannot reduce these in response to a minimum wage – all employers have standards for punctuality, cooperation, and effort. If employers are required to pay above-market wages, expecting more effort, etc. from workers is a natural and potentially universal response.

Given difficulties of measuring effort, evidence on changes in required levels of effort is, predictably, thin and indirect. Wessels (1980) reports that one-sixth of the establishments in his sample of New York retailers reduced hiring of extras, and another 3% reduced meal and rest periods. Above we noted employers' claiming to increase worker responsibility, which might imply additional effort as well. With effort less directly measurable than fringe benefits or even training, such hints are all the evidence we have that effort standards might be raised in response to the minimum wage.

Unlike reductions in fringe benefits, offsetting minimum wage increases with increased effort standards is unlikely to blunt the expected negative effect on employment. A 10% increase in the minimum wage offset by a 10% increase in effort and therefore labor services per worker would lead to a 10% reduction in workers (or worker hours) employed.

8.3. Spillovers

As noted in the discussions of heterogeneous workers and of the possible unpiling of the spike at the minimum wage, it is reasonable to expect that an increase in the minimum wage from $4.50 to $5.00 will make workers initially skilled enough to earn slightly more than $5.00 more attractive to employers. When the minimum wage is $4.50, employers are indifferent between such workers and workers earning $5.40 who are 20% more productive. If (some of) these $4.50 workers now must be paid $5.00, those making $5.40 are now a significantly better bargain. Surprisingly, there is relatively little evidence on the effect of the minimum wage on the wages of those higher in the wage distribution.

Gramlich (1976) indirectly estimated the importance of spillovers by comparing his estimate of the effect of the minimum wage on average hourly earnings to the effect one gets by assuming the only effect of the minimum wage is on those whose wage initially fell between the old (w_{m}) and new (w'_{m}) minimum wages (i.e., $w_{\mathrm{m}} \leq w < w'_{\mathrm{m}}$). This direct effect accounted for about half of the effect on average earnings. Gramlich noted that the remaining "emulation" may include effects on those initially earning less than w_{m} (due to incomplete coverage or noncompliance) as well as wage increases for those initially earning more than the new minimum.[53]

Grossman (1983) estimated spillover effects for workers in nine low-wage (but not minimum-wage) occupations in non-manufacturing industries. While there is some evidence of spillover effects, they are estimated with relatively large standard errors.

[53] Gramlich's calculation of the effect on average wages is based on a time-series analysis of data from 1954–1975, whereas the data for directly affected workers is based on analysis of CPS data surrounding the 1974 increase. While CPS wage distributions that would let one study earlier minimum wage increases are not available, it appears from BLS calculations of "wage-bill impacts" which assume wages increase only for directly affected workers (Peterson, 1981, Table 17) that the 1974 increase had a relatively small direct effect on wages in covered establishments. On the other hand, coverage had been expanded so that covered establishments accounted for a larger fraction of total employment and wages. If the "typical" increase in Gramlich's sample period had a larger direct effect than did the 1974 increase, his estimated emulation effects would be too high. Cox and Oaxaca (1981) estimate a significantly larger effect of the minimum wage on average wages than did Gramlich. However, they assume that minimum wages increases lead to a proportionate increase in the wages of all low-wage workers (including those initially somewhat above the minimum).

Given the details of her sample, her results to not allow us to say much about the importance of spillovers of those "just above" the minimum wage, where such impacts are likely to be most important.[54]

Converse et al. (1981) asked employers whether, in response to the January 1979 increase in the minimum wage to $2.90, they increased wages of workers who had previously been earning more than the new minimum. The authors tried to verify that these increases would not have been given had the minimum wage not increased; i.e., that they were not due to generally increasing nominal wage levels. By this standard, 17% of establishments employing minimum-wage workers reported a "ripple" effect on wages. For 64% of these establishments, the increases stopped at $4.00 per hour or less. Given that the 1979 increase in the minimum wage essentially matched increases in average hourly earnings in the economy as a whole, the study was forced by timing to focus on a minimum wage "increase" that was quite small by historical standards.

Katz and Krueger (1992) provide detailed information on ripple responses following the 1990 and 1991 increases in the minimum wage. Among restaurants whose starting wage was equal to the old minimum wage, a significant minority increased the wages of those paid between the old and new minimum to a level above the new minimum (for outlets that paid the old minimum wage originally, 41% raised wages of those between w_m and w'_m to a new level above w'_m in 1990, only 16% did so in 1991). But only 9% of these restaurants increased the wage of those earning $4.50 per hour before the 1991 increase. These results are consistent with the idea that spillovers are limited to a minority of workers above the minimum wage, and die out fairly quickly as one moves up the wage distribution. However, they also find that almost no employers delay or reduce the first raise that new hires receive if they remain with the firm. Card and Krueger (1994) find no significant changes in time to first raise or amount of wage in response to New Jersey's state minimum wage increase, again suggesting a spillover from the minimum wage of starting workers to the wage of those who remain long enough to progress above that level. However, since this first raise averages 5% after 19 weeks on the job, there is again not much evidence of spillovers extending very far up the wage distribution.

Card and Krueger (1995, pp. 163–166) analyze changes in the teenage wage distribution following the 1990 and 1991 increases. They find that the fraction of teenagers earning less than $4.50 declined more rapidly in states most affected by the increase to $4.25 by 1991, but there was little difference in the fractions earning less than $5.00.[55] They conclude the

[54] Because she wanted to be sure she was not capturing the direct effect of the minimum wage on those earning less than the new minimum, she eliminated occupations with any minimum-wage workers. Among the set of occupations included in the Area Wage Survey, this left nine occupations that were low-wage but not directly affected. Five of the nine were office occupations. Moreover, her sample was dominated by relatively large and relatively Northern cities, and the Area Wage Survey includes only establishments with at least 50 workers (100 in some cities). The focus on occupations not directly affected, plus the sampling of cities and establishment sizes, led to a sample where spillovers would have to go fairly high up the wage distribution in order to be detectable. In the median occupation, average hourly earnings were about 80% above the minimum wage.

[55] Of course, this might just be a reduction in employment in the lower tail of the distribution, but recall that they do not find that employment fell faster in high-impact states.

data "provide some support for the existence of spillover effects up to $4.50 per hour, but little evidence of spillovers beyond $4.50."

The evidence on spillovers is very limited, but it suggests that increases in minimum wages lead to increases for those above the minimum as well, although these spillovers do not extend very far up the wage distribution. From one perspective, such spillovers seem broadly consistent with the goals of minimum wage legislation; if raising wages of those earning the minimum wage is a good thing, increasing the wages of those who were earning a bit more than the minimum would also be viewed favorably. From another perspective, these spillovers may be cause for concern. Recall that, in discussing the effects of the minimum wage on employment of a heterogeneous group such as teenagers, "small" overall effects might result from losses to low-wage teens partially offset by increasing employment of better-paid teens. The spillovers can be read as evidence that demand for (and therefore employment of) better-paid teens (and others) increased. However, the tentative evidence that these spillovers do not extend very far up the wage distribution[56] would suggest that the gainers and losers are not *very* different in their skills or other attributes.

8.4. Prices

The standard model of labor demand predicts that employment of unskilled workers falls in response to a minimum wage because other inputs are substituted for unskilled labor and because the increased minimum wage increases the cost of and so reduces the demand for products that use such labor intensively. While there is a sizeable literature on the effects of the minimum wages on prices in general, the effects on relative prices of industries that use low-skill labor have been studied less intensively.

Wessels (1980, pp. 67–69) summarizes Department of Labor studies comparing price increases by Southern and non-Southern firms in low-wage industries following the 1961 and 1967 increases in the minimum wage. Since wages were lower in the South, it was the "high-impact" region and one might expect prices in affected industries to increase faster there. In manufacturing, Wessels finds little consistent pattern in price increases by region; but in services relative prices do increase faster in the South.

Katz and Krueger (1992) and Card and Krueger (1995) compared the price changes by fast food restaurants that were affected to different degrees by the minimum wage. Katz and Krueger found that Texas restaurants that experienced larger increases in starting wage (due to the increased federal minimum wage) increased their prices less rapidly; this relationship was not statistically significant.[57] Card and Krueger (1995, Table 2.8)

[56] A recent paper by Acemoglu (1997) finds positive effects of minimum wages on the number of "good" jobs (occupations with wages significantly higher than the observable characteristics of their occupants would predict), which would suggest spillovers higher up the wage distribution.

[57] The elasticity of price with respect to starting wage was estimated as -0.089 (0.133). So one could reject the hypothesis that the elasticity was equal to low-wage labor's share of total costs, as long as (as seems likely) that cost share is greater than 0.2.

report a positive, although statistically insignificant relationship in their New Jersey–Pennsylvania sample, and a positive and sometimes significant relationship in national samples (their Table 4.10).

The limited available evidence thus suggests that minimum wage increases often lead to increases in prices by the directly affected firms, although how they compare to the simplest competitive prediction is not clear.

9. The minimum wage and the wage and income distributions

Much of the justification for minimum wage laws lies in a desire to help those at the bottom of the economic ladder. Despite warnings by George Stigler over 50 years ago (Stigler, 1946) that those who work for the lowest wages are not necessarily members of the poorest families, public discussions tended to assume that those at the bottom of the wage distribution were also likely to be at the bottom of the income distribution. The first careful studies of the effect of the minimum wage on the distribution of income suggested that the link between low wage and low income was disappointingly weak (Gramlich, 1976; Kelly, 1976). More recent work has to some extent reassessed that conclusion, but has also focused more directly on the distribution of wages as well as the distribution of income.

9.1. *Effects on the wage distribution*

Combining the insights of the theoretical models and the evidence in the preceding sections suggests that the minimum wage may affect the wage distribution in many ways. First, some of those who would otherwise earn less than the minimum wage may be less likely to be employed, or employed for fewer hours. The loss of low-wage jobs would tend to make the measured wage distribution more equal, although this is not the sort of equalization that proponents of the minimum wage have in mind. Reduced hours have a similar effect, if the wage distribution is hours- rather than worker-weighted. Second, some of those who would otherwise earn less are boosted up to w_m, producing the spike in the wage distribution at the minimum wage. Third, wages of low-wage workers not covered by the minimum wage may be increased or reduced. Fourth, the increase in the wages of directly affected workers will make substitutes for these workers more attractive, and this is likely to raise demand for workers just above the minimum. Wages of such workers should increase, and more may be pulled into the labor force. Fifth, minimum wages may indirectly affect those further up the wage distribution, although most work on the subject implicitly or explicitly assumes the effects on well-paid workers are small.

Meyer and Wise (1983a,b) were the first to focus on the spike at the minimum wage and the apparent thinning of the wage distribution at lower wages. Their plotted empirical wage distributions strongly suggested that both thinning below w_m and a piling up at w_m

were empirically important. They fit a wage distribution that allowed for both thinning and piling up, but otherwise looked like a standard wage distribution, with ln(wage) depending on the usual schooling, experience, etc. variables and a normally distributed error. They then assumed that this distribution would hold below w_{m}, too, in the absence of a minimum wage. The difference between the thinned distribution below w_{m} plus the spike at the minimum, and the fitted distribution at or below w_{m}, was taken as a measure of the employment loss at the minimum wage. Meyer and Wise find that their estimate of the effect of a given minimum wage on employment was essentially the same using 1978, when the actual minimum was low (and so the estimate was based largely on the observed wage distribution) as using 1973 when the actual minimum wage was high (and so the estimate relied more heavily on projecting the wage distribution below the actual minimum).[58]

This approach was criticized by Dickens et al. (1994), who noted that spillover effects (which thicken the distribution just above w_{m}, and were assumed to be negligible by Meyer and Wise) would lead them to over-predict the wage density below w_{m} in the absence of the minimum wage, and so over-estimate the employment loss. Using British data, they find that their estimates are sensitive to the details of fitting the wage distribution above w_{m} and then extrapolating it back to lower wage levels.[59]

For thinking about the wage distribution, the important point is that Meyer and Wise assumed a parametric wage distribution and then estimated the effect of the minimum wage on the distribution at or below w_{m}. In contrast, papers that focus on the effect of the minimum wage on the distribution of wages or income typically do the reverse; they begin with the empirical distribution of wages or income, assume that an increase in the minimum wage boosts the wages of those between the old and new minimum, make some assumption about the extent of employment loss and the effect (if any) on those initially earning sub-minimum wages, and usually assume that spillovers above the minimum wage are unimportant.

DiNardo et al. (1996) analyze the effects of changes in the minimum wage on wage inequality, focusing in particular on the 1979–1988 period when inequality increased significantly for both men and women. Their baseline estimates assume no employment loss and no spillovers; they assume that, had the real minimum wage in 1988 remained at its 1979 level, the shape of the wage distribution (conditional on schooling, experience, etc.) below the minimum would been the same as it was in 1979. Between 1979 and 1988, the logarithm of the ratio of the minimum wage to average wages fall by 0.27 for men, the standard deviation of ln(wage) increased by 0.072, and the falling real minimum wage can

[58] Meyer and Wise tended to emphasize the employment gain from eliminating the minimum wage, which makes comparison with other studies difficult. However, they report that, over the 4 years 1973, 1976, 1977, and 1978, keeping the minimum wage at \$1.60 (a 30% cut, on average, over these years) would have increased employment by 5%. This implies an elasticity of -0.16.

[59] The Meyer–Wise approach to estimating minimum wage effects has not been used in the more recent US literature, but has been used more with European data. For a brief survey and critique, see Dolado et al. (1997, p. 332).

account for 0.018 of the increase. For women, the decline in the relative minimum wage was 0.36, the increase in the standard deviation of ln(wage) 0.090, and the increase due to the falling minimum wage 0.027.

While these calculations are based on fairly strong assumptions rather than fitting a minimum wage variable to the actual changes in inequality, DiNardo et al. report the effects of various alternatives as well. Assuming that a higher minimum wage increase in 1988 would have no effect on those below the actual 1988 minimum, or allowing a disemployment elasticity of 0.15,[60] has little effect on the estimates. The baseline simulations take anyone earning less than \$3.00 in \$1979 (rather than \$2.90) as being directly affected by the minimum wage, and this matters a lot: the spike at \$3.00 is important. As one would expect, variations in the minimum wage matter more for inequality measured by the standard deviation of $\ln(w)$ or the 90–10 differential, and less for the Gini, since the latter places less weight on the low end of the wage distribution.

Changes in the standard deviation of $\ln(w)$ of 0.018 (men) or 0.027 (women) are large if measured against the increases in inequality over the period (0.072 and 0.090, respectively), and obviously smaller if compared to the initial level of inequality (0.501 and 0.429). But given the policy interest in the 1979–1988 changes, the calculated contribution of the minimum wage is too large to be ignored. These changes are larger if spillovers are important.[61]

Card and Krueger (1995) compare changes in the wage distribution between 1989 and 1992 in states according to the fraction of their workers who were directly affected by the 1990 and 1991 minimum wage increases. They confirm the prediction of DiNardo et al. that such increases measurably reduce wage inequality. Machin and Manning (1994) use data for British industries subject to different minimum wages and also find that higher relative minimum wages reduce wage dispersion.

9.2. *Effects on the distribution of income*

Moving from the distribution of wages to the distribution of income is complicated by several considerations. Many families have several earners, so that a minimum-wage worker can be part of a relatively affluent family. In contrast, the poorest families in the US have little or no labor earnings, and the minimum wage is powerless to improve their status. Several simple statistics have been used to characterize the relationship between having wages at or near the minimum ($w < w^*$, where w^* is often set at w_m) and being part of a low-income family or household ($Y < Y^*$).

[60] Since a 32 log-point increase in the minimum wage is being discussed, this presumably means a 5% loss of employment for those at or below the new minimum. This is a different elasticity notion than is found in the literature on teenagers, where the proportionate change in employment of all teens (including those above the minimum wage) is considered.

[61] The authors note that the wage distribution for women (but not for men) was much denser just above the 1979 minimum than above the same point in the real wage distribution in 1988, which suggests – but does not prove – that spillovers might be important for women.

First, among low-wage workers, what fraction are poor (i.e., what is Prob($Y < Y^* \mid w < w^*$)? Using 1973 data, Gramlich (1976) chose $w^* = \$2.00$ (versus $w_m = \$1.60$) and $Y^* = \$4000$ (roughly, the poverty line for a family of four). He found that 23% of adult low-wage workers were "poor", as compared to 6% of all adult workers. In contrast, only 6.6% of low-wage teenage workers were in poor families, compared with 8.2% of teenagers at all wages. Since there were more than twice as many low-wage adults as low-wage teens, overall about 18% of low-wage workers were poor. Raising wages of those in the vicinity of the minimum was not a particularly target-efficient strategy for raising low incomes. A number of other studies have found the fraction of low-wage workers who are "poor" to be about 20% (Kohen and Gilroy, 1981, Table 4; Johnson and Browning, 1983, Table 1; Smith and Vavrichek, 1987, Table 2; Card and Krueger, 1995, Table 9.1; Burkhauser et al., 1996, Table 4).[62] This fraction is roughly doubled if one sets Y^* at 1.5 times the poverty line.

Burkhauser and Finegan (1989, Table 2) show that the fraction of low-wage workers who are poor fell from 42% in 1959 to 18% in 1984, reflecting both a decline in the unconditional probability of being poor and the probability that a low-wage worker would be a family "head" whose earnings would determine its economic status. Card and Krueger (1995) argue that other forces (declining relative incomes for families with children, so that now minimum-wage teens have lower family incomes than other teens, reversing the pattern in Gramlich's data, increased wage inequality among adults) has increased the fraction of minimum wage-workers who are poor.

A second summary statistic is Prob($w < w^* \mid Y < Y^*$): among workers who are in low-income families, what fraction earn low wages? Because the proportion of *workers* who are low-wage tends to be higher than the proportion who are poor, changing the conditioning event in this way raises the conditional probability, typical values being 0.3–0.4.

Tabulations based on the fractions of workers who are low-wage and/or poor can reveal only part of the story, however. Many families are poor because they have no workers, or because those who are employed work few hours (Kelly, 1976), others because they work full time but have large families (Bell, 1981, p. 451). What is particularly striking is that 25.7% of poor families in 1989 had no workers, and only 12.6% had a "minimum wage" worker[63] (Burkhauser et al., 1996, Table 3).

Given this relatively loose link between being a low-wage worker and being a member of a low-income family, we should perhaps expect that simulations of the effect of raising the minimum wage show relatively modest effects on poverty. Most of these simulations have begun by making assumptions that minimize indirect effects – no employment loss, no offsets to increased earnings due to increased prices or reduced transfers – and no spillovers. Earnings gains per household are roughly equal across deciles of the income

[62] Dolado et al. (1997) report a stronger relationship in European countries.

[63] Here "minimum wage" workers are those earning \$3.35–4.25 per hour, and so include those whose wages were raised by the 1990 and 1991 minimum wage increases.

distribution (Johnson and Browning, 1983, Table 2) or the distribution of family-size-adjusted income (Burkhauser et al., 1996, Tables 5 and 1A). Of course, even an equally distributed gain improves an initially unequal distribution, and one gets modest simulated reductions in poverty (Mincy (1990) estimates that, in 1987, raising the minimum wage from \$3.35 to \$4.25 would, on these assumptions, have reduced the number of poor families by 6.9%). Taking account of possible dis-employment and losses of means-tested transfers reduce these impacts (Johnson and Browning, 1983; Mincy, 1990; Horrigan and Mincy, 1993).

Given uncertainty about the size of any employment losses, particularly for adults – and about whether "disemployment" is best modeled as a proportionate reduction in annual hours of all low-wage workers or a "lightening-strikes" reduction of annual hours to zero for an unlucky subset – it is natural to ask whether recent changes in the minimum wage can be linked to observable changes in the distribution of family income or the poverty rate. Card and Krueger (1995, Table 9.7) report relatively small and statistically insignificant differences in the change in poverty rates across states with differing impacts of the 1990–1991 minimum wage increase. Neumark and Wascher (1997) relate year-to-year changes in poverty status to changes in minimum wage rates and find that higher minimum wages increase poverty, although again the effect is small and statistically insignificant. Together, these studies underline the difficulty of identifying small impacts in available data.

10. Conclusions and future directions

My reading of the new and old evidence suggests that the shortterm effect of the minimum wage on teenage employment is small. Time-series estimates that centered on an elasticity of -0.10 moved closer to zero in samples that included the 1980s. Studies that relate changes in employment/population ratios by state, as a function of the "impact" of the minimum wage on the state (measured either by the minimum wage relative to the average wage, or the fraction of the workforce whose wage must be raised to comply with a new increase) show much more varied results. A tentative pattern is that studies that control for year as well as state find much smaller minimum-wage impacts than those which do not control separately for year effects, and so treat aggregate shortfalls in teen employment in years of minimum wage increases as minimum wage impacts. It is not clear why year effects matter so much; since all studies of this genre hold constant the adult or all-age employment/population ratio in the state/year cell, "year" effects associated with business cycles should already be taken care of. Given the substantial variability in estimated minimum wage effects that do not control for year separately, I would put more weight on those that do, and this tends to reinforce the message of the time-series studies that the minimum-wage effect is small (and zero is often hard to reject). The recent studies of the fast-food industry (which estimate a quite different response) and studies that follow individual employment transitions also seem broadly consistent with this conclusion.

Even an elasticity of -0.1 would likely seem small to anyone who had not been conditioned by the evolution of the minimum wage literature to expect such a small response. As emphasized in Sections 2 and 4, this is *not* a elasticity of demand for low-wage labor; but a rough correction for the fact that most teenagers are not directly affected by the minimum wage[64] would multiply the minimum wage elasticity by about 5 to get the implied elasticity of demand. As a demand elasticity, less than 0.5 in absolute value seems surprisingly small.

10.1. Accounting for "small" employment effects

Suppose one accepts this reading of the literature. How can we account for the small response?

One possible explanation is that minimum wage coverage is incomplete, and compliance among covered employers may be imperfect. Five-sixths of all non-supervisory workers are covered by the minimum wage, but the proportion of low-wage non-supervisory workers covered is lower. While we do not have a good count, we know that establishments not required to pay the minimum wage tend to be small firms in retail trade and services, who tend to be low-wage employers. But the rough correction described above counts those who report hourly wages below the minimum as not affected by the law, and this probably overstates the importance of the uncovered or non-compliant sector. Moreover, for a relatively small uncovered sector to absorb most of the workers displaced from the covered sector by a minimum wage increase, the decline in the uncovered sector wage would have to be quite large (or demand in the uncovered sector would have to be much more elastic). We have little hard evidence on what happens to uncovered-sector wages, but declines in the uncovered-sector wage of the same order of magnitude as the minimum wage increase seem implausible. Popular press stories extolling the benefits of the minimum wage for uncovered-sector employers are not a prominent feature of the minimum wage debate! More hard evidence on the behavior of wages in the uncovered-sector is needed.

A second possibility is that small effects on teenage employment mask a perverse substitution of more- for less-skilled teenagers (Neumark and Wascher, 1996). The limited evidence of limited spillovers reported in Section 7 is consistent with such substitution; the lack of strong evidence that minimum wages reduce black teenagers' employment more than whites' is less consistent. Closer analysis of wage distribution data may provide some further clues here. Moreover, studies that look at individuals' labor force transitions following minimum wage increases (as in Section 7) have not focused on those earning a bit more than the new minimum wage, whose employment prospects should be helped if such substitution is important.

[64] And many of those whose wage is increased when the minimum wage rises are already earning more than the old minimum, and so their wage increase is smaller than the legislated minimum wage hike.

The limited time-series literature on minimum wages and the work weeks of those who are employed suggests that hours per week fall when the minimum wage increases, so the effect on hours worked is more pronounced than the effect on bodies employed. Surprisingly, this line of attack has not been prominent in the recent research, on either side of the debate. In principle, there is more to be learned here from extending the time series than for the traditional employment regressions, since the available time series data on hours of teenagers go back only to the mid-1960s. And since all of the studies that focus on states over time use data from years when the hours data are available, a parallel focus on hours in these studies would lead to no loss of sample at all.[65]

Another possibility is that the labor market is closer to the monopsony model than to the competitive market that nearly all studies assume. This has probably been the most controversial part of the debate stimulated by Card and Krueger's *Myth and Measurement*. Card's (1992b) study of California's increase in its state minimum wage suggested teenage employment grew faster there than in comparison states, although the difference was smaller and not significant when the set of comparison states was extended. The Katz–Krueger and Card–Krueger studies of fast food also reported faster employment growth that was often statistically significant for restaurants at which the minimum wage's impact on wages was larger; but in Card and Krueger's more recent analysis of ES202 data the difference is small and statistically insignificant. Fast-food employers often pay bonuses to employees who recruit friends as new workers, suggesting a less than infinitely elastic supply of labor at the going wage. But such bonuses did not fall faster in New Jersey than in Pennsylvania following the New Jersey minimum-wage hike. Finally, the limited price data suggest that, if anything, prices rise after a minimum wage increase. If employment is expanding, so presumably is output, and prices should fall. Admittedly, the price data are limited, and it would be very useful to know whether, in sectors most affected by the minimum wage, prices rise at roughly the rate predicted by the increases in the minimum wage and the share of minimum-wage workers in the cost structure. Based on the available evidence, the monopsony model will not replace the competitive diagram in the souls of labor economists.

A more sympathetic reading of *Myth and Measurement* would view the monopsony models – including models emphasizing search by workers and employers – as being more appropriate some of the time, and so contributing to rather small effects in the aggregate. Progress in testing this possibility will depend on far better understanding of minimum wages and prices – are price increases considerably smaller than predicted by the competitive model, too? – or perhaps on specifying contexts in which the traditional model is least appropriate.

The possibility that minimum wage increases are offset by changes in other elements of the job package is unlikely to account for relatively small employment elasticities. Fringe

[65] Hungerford (1997) uses panel data on states over time to investigate questions about part-time work. He finds that minimum wage increases increase involuntary part-time employment.

benefit cuts that we can observe are not nearly large or widespread enough to make a large difference for employment. And, while I find it hard to believe that employers do not respond to minimum wage increases by raising standards of effort, punctuality, etc., these are likely to lead to *larger* (more negative) effects of minimum wages on numbers or hours employed. Evidence on the scale of such adjustments is sadly lacking; but if they are important, they are likely to intensify rather than resolve puzzle of the small employment elasticities.

A final possibility is that the demand for low-wage labor is just not terribly elastic in the short term. The last four words of the preceding sentence highlight the largest and most important gap in the minimum wage literature. There is simply a stunning absence of credible evidence – indeed, of credible attempts – on the longterm effects. As noted in Section 3, minimum wages are adjusted periodically, so that there is a modest amount of variation in the minimum wage, relative to other wages, over time. But this variation is not permanent, since an increase will be followed by several years of erosion, and then at some point another increase; and it provides few clues about the longterm responses (Mincer, 1984, p. 322). Baker et al. (1999) argue that regressions in the typical time-series study estimate a mixture of short- and longterm effects. In Canada, they find the shortterm responses negligible and the longterm responses substantial.[66]

Changes in coverage have been more nearly permanent. The gradual extension of coverage to ever-smaller retail trade and service firms has not been repealed. Time series studies have been consistently unable to find coverage effects in the minority of studies that try to separate coverage from the level of the minimum wage (Brown, 1996). Looking specifically at retail trade and service employment, given substantial time series both before and after the extensions, may have some ability to detect longterm changes in the structure of these industries due to the minimum wage.[67]

10.2. Effects on the distributions of wages and of incomes

While the effects of the minimum wage on employment remain somewhat controversial, and accounting for the relatively small observed responses remain a puzzle, the effects of the minimum wage on the distribution of wages and incomes seem to be more nearly settled. The minimum wage does have a visible effect on the wage distribution, particularly for teenagers although also for adults in years when it was high relative to average wages. How much of this effect is due to low-wage workers being less represented in the wage distribution (i.e., not employed or working fewer hours) is less clear, but it clear from

[66] Baker et al. (1999) argue that Neumark and Wascher's (1992, 1994) findings are consistent with theirs; to my eye, this pattern is weaker when Neumark and Wascher exclude enrollment.

[67] Belman and Wolfson (1997) study a range of low-wage industries including several in retail trade. Evidence that the minimum wage raises wages is weaker than one might expect, although they argue there is little evidence of employment effects in the subset of industries with significant wage effects. Their focus, however, is on shortterm impacts and does not make direct use of changes in coverage of the minimum wage law.

the spike at the minimum wage that a significant fraction of those affected do receive wage increases up to the minimum (at least in the short term).

It is much less clear how the minimum wage affects the distribution of wages measured over several years rather than in one year or at one point in time. Suppose that, at any one point in time, the minimum wage leads to $x\%$ of those affected to not be employed, and $100 - x\%$ to have their wages boosted to w_m. One extreme possibility is that $x\%$ permanently lose their jobs, never to work again. The other extreme is a daily game of musical chairs, so that over any reasonably period the gains and losses are shared equally by those directly affected. My sense is that opponents of the minimum wage tend to see gainers and losers as different people, with those who would otherwise have earned low wages earning no wages; supporters of the minimum wage see a much more nearly equal sharing of gains and losses.[68] There is not, however, much guidance in the literature for resolving this difference.

When one moves from the distribution of wages to the distribution of income, the equalizing potential of the minimum wage is greatly diluted.

10.3. The future of research on the minimum wage

A careful reader has by now noticed that under cover of summarizing what we know about the minimum wage, I have focused instead on areas where our understanding comes up short. Filling in these gaps is not easy; if it were, they would not have remained as gaps. In some but not all cases, use of the CPS micro data public use files – a useful innovation of the most recent round of minimum wage research – provides opportunities for disaggregation and focusing on wage distributions that time series studies which are captive of published CPS tabulations could not address. Progress in filling these gaps will also improve our general knowledge of how labor markets – or at least low-wage labor markets – work, and that may well be the largest payoff to the effort.

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