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The Effect of the Minimum Wage on Employment and Unemployment

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Introduction

ALTHOUGH ARGUMENTS for and against the minimum wage are the same today as when the Fair Labor Standards Act was passed forty years ago, they are now accompanied by more sophisticated approaches to the measurement of the law's impact. Moreover, the increase in minimum wage coverage makes the issue more important. The employment/unemployment effect of the minimum wage continues to be a pivotal issue around which present-day debate centers (Robert Goldfarb, 1974; Steven Zell, 1978; and Sar Levitan and Richard Belous, 1979), and will be the focus of our attention.

Despite abundant studies of the employment and unemployment effects of the minimum wage in the U.S., there is no comprehensive review of their findings (although E. G. West and Michael McKee, 1980a, have undertaken a broad assessment of some of the Canadian and American literature). The purpose of this article is to determine what generalizations this literature supports and to diagnose causes of the most important disagreements. This should help economists to identify the directions for further research and policy makers to interpret the many results.

Section I of this paper discusses the theoretical framework in which the minimum wage has been analyzed. Sections II and III contain analyses of time-series and cross-section studies, respectively, of the effects of the minimum wage on teenagers, while Section IV takes up the impact on adults. Section V describes the effects on low-wage industries and labor markets. Conclusions are in Section VI.

I. *Theory*

Most textbook treatments of the employment effects of the minimum wage rely on the simple supply-and-demand model of price floors, and the outcome is often contrasted with that which occurs under monopsony. In recent years, the analysis of the effects of a minimum wage in competitive labor markets has been significantly extended to include formal treatment of a minimum wage which applies to one sector of a two-sector economy, or which has no direct effect on some workers because they earn more than the minimum.

The first three parts of this section deal briefly with the traditional analysis, while the next four deal with more recent additions to the literature. A theme that runs through our treatment of these additions is how the employment and unemployment effects of the minimum are related to the parameters which each model introduces. The final part of this section deals with the implications of these models for the effect of the minimum wage on the efficiency of the labor market.

A. *Simple Supply-Demand Model*

The most basic model of minimum wage effects on employment and unemployment focuses on a single competitive labor market with homogeneous workers whose wage W_0 would otherwise fall below the legally set minimum wage W_m. Employers minimize costs both before and after the minimum wage law, workers' skills and level of effort are identical and given exogenously, and all workers in the market are covered by the minimum wage. Adjustment to the new equilibrium is not considered. In this model, initial employment E_0 is determined by supply and demand; once the minimum wage is introduced, employment falls to E_m, the level demanded at wage W_m (Figure 1).

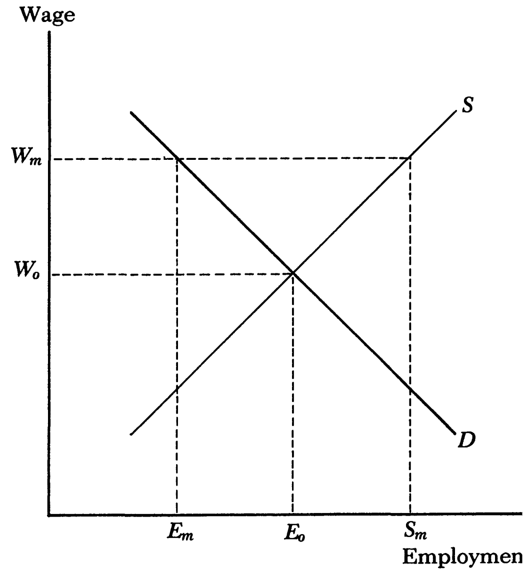

Figure 1.

The proportional reduction in employment ($ln\ E_m$-$ln\ E_0$) is equal to the proportional wage increase ($ln\ W_m$-$ln\ W_0$) times the elasticity of demand.

If employment would otherwise increase, the "reduction" in employment predicted by the model may take the form of a lower rate of employment growth rather than an actual decline in the number employed. If employment actually declines, it may take the form of not replacing workers who quit rather than discharging workers.[1]

While the model determines an excess supply of labor at the new minimum wage, S_m-E_m, this excess supply does not correspond to the official measure of unemployment (Finis Welch, 1976, p. 8), or even to the increase in such unemployment above some "frictional" level. S_m represents the number (or work-hours) of those persons willing to work at W_m, but some of the S_m-E_m who are not employed may

[1] Muriel Converse, et al. (1981, p. 282) based on interviews with employers, report that only 12 percent of the disemployment due to the 1980 increase in the minimum wage took the form of discharges.

decide that prospects of finding work are too dim to make actively searching for work worthwhile. Those not actively looking for work are not included in the official unemployment count.

B. *Monopsony*

A well-known exception to the conclusion that the minimum wage reduces employment is the monopsony case (George Stigler, 1946). Without a minimum wage, the monopsonistic employer's marginal cost of labor everywhere exceeds the supply price; labor is hired until marginal cost and demand are equal (Figure 2). A minimum wage makes the employer a price-taker, up to the level of employment $S(W_m)$. Thus, a minimum wage between the original monopsony wage W_o and the competitive wage W_1 will increase employment (S. Charles Maurice, 1974); choosing $W_m = W_1$ brings employment to its competitive level, E_1. Once W_m equals W_1, further increases would reduce employment below the competitive level. The monopsony model has not motivated much recent work, perhaps because there

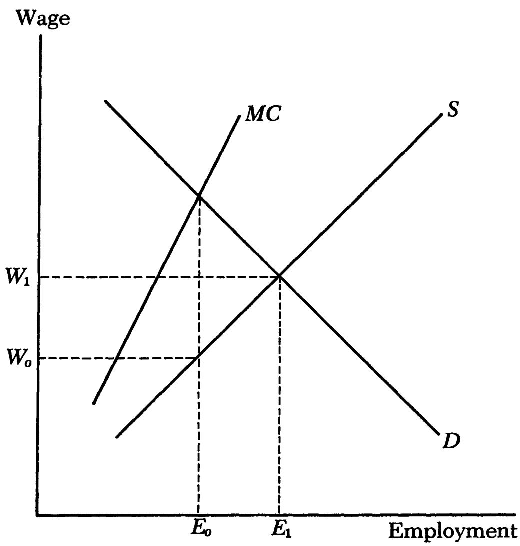

Figure 2.

is little evidence that it is important in modern-day low-wage labor markets (West and McKee, 1980b).[2]

C. *"Shock" Effects*

If employers do not minimize costs, there is the possibility that they will respond to a minimum wage increase by raising the productivity of their operation to offset the increase (Lloyd Reynolds and Peter Gregory, 1965, p. 193). This possibility is often labeled a "shock" effect—the minimum "shocks" employers into greater productivity.

Such a shock effect might reduce the disemployment from a minimum wage (increase) but is unlikely to eliminate it (West and McKee, 1980b). First, while some firms may be in a position to take advantage of previously unrealized economies, other firms may not be so fortunate. Surveys of employers find reports of such responses from some but not all firms (U.S. Department of Labor, 1959b). Second, firms may have failed to minimize costs by using *too much* labor at the previous wage W_o; cost-cutting would then take the form of discharging (or not replacing) the extra workers.

Presumably, the scenario most favorable to the shock argument is one in which the employer is able to call forth greater levels of effort in response to the minimum. A formal model consistent with cost-minimizing employer behavior along these lines has been developed by John Pettengill (1981). Just as rent controls are thought to induce landlords to lower apartment quality in response to excess demand, competitive employers may

[2] One "test" of the monopsony model is to determine whether it is common for a small number of employers to employ a majority of the workers in a labor market. Robert Bunting's 1962 study of 1,774 labor markets (most "labor markets" being counties) found that the four largest employers employed at least half of the semi- and unskilled workers in less than 3.7 percent of the labor markets (p. 101).

raise the required level of effort in response to minimum-wage-induced excess supply. Higher effort levels can offset the effective increase in the minimum wage, depending on a parameter that expresses the amount by which workers will increase effort at W_m rather than not work at all.[3] For what appear to be plausible values of this parameter, effort reductions can offset much of the disemployment effect which would otherwise occur.[4]

D. *Two-Sector Model*

Coverage under the minimum wage provisions of the Fair Labor Standards Act has increased gradually, but even today it is not complete. Since the 1977 Amendments to the Act, roughly 84 percent of all private nonfarm nonsupervisory wage and salary workers have been subject to the minimum wage, compared with 53 percent in 1950 (Welch, 1978, p. 3). Roughly 80 percent of low-wage workers (those with wages at or below the minimum) work in establishments subject to the minimum wage (Gilroy, 1981a). Thus it makes sense to consider a model in which coverage is incomplete, particularly in studying effects of the minimum wage in earlier periods when coverage was less extensive than it is today.

In Welch's 1974 model of a partial-coverage minimum wage, the covered sector reacts to the minimum as it would if coverage were universal. Workers displaced by the minimum wage "migrate" to the uncovered sector, shifting supply there outward. As a result, wages fall and employment increases in the uncovered sector.

Those displaced from the covered sector do not automatically become employed in the uncovered sector. As wages in the uncovered sector fall, some of those displaced by the minimum wage (as well as some of those originally employed in the uncovered sector) decide not to work in the uncovered sector because the wage there is less than their reservation wage. Therefore, the effect of the minimum wage on total employment depends on the elasticity of labor supply and the reservation wages of those who do not obtain covered sector work, as well as more obvious factors such as the size of the covered sector and the elasticity of labor demand.

Let S and D denote supply and demand; let the subscripts c and u refer to covered and uncovered industries, and let c be the proportion of employment before the minimum wage which is in industries about to become subject to it, i.e.:

$$c = \frac{D_c(W_o)}{D_c(W_o) + D_u(W_o)} \tag{1}$$

Before the minimum wage is introduced, wages in the two sectors are equal, and the supply of labor in the uncovered sector, $(1-c)S(W_o)$, equals demand in the uncovered sector, $D_u(W_o)$.

Welch assumes that, after the minimum wage is introduced, each of the $S(W_m)$ workers willing to work at the minimum wage has the *same* probability of obtaining one of the $D_c(W_m)$ covered sector jobs. Therefore, this probability equals

$$f = \frac{D_c(W_m)}{S(W_m)} \tag{2}$$

If wages are measured so that $W_o = 1$ and $ln(W_o) = 0$, the proportional increase in the wage in the covered sector is $ln(W_m)$. The uncovered wage W_u must now equate the new uncovered sector

[3] Increased effort is just one potential offset to a minimum wage. Other working conditions or fringe benefits, especially opportunities for on-the-job training, have been considered by Martin Feldstein (1973), Wilson Mixon (1975), David Luskin (1979), Walter Wessels (1980), Masanori Hashimoto (1981), Jacob Mincer and Linda Leighton (1981), and Edward Lazear and Frederick Miller (1981).

[4] It is even possible that employers would gain from a minimum wage, though Pettengill does not emphasize that possibility. In effect, the minimum wage would lead employers to confront workers with a level-of-effort requirement that a competitive market would not otherwise permit.

supply, $S_u'(W_u) = S(W_u)(1-f)$, with demand, $D_u(W_u)$.

If η is the elasticity of demand for labor (assumed to be the same in both sectors) and ϵ is the elasticity of labor supply, the uncovered-sector wage W_u will be a function of η, ϵ, c, and W_m. Given W_m and W_u, we can find employment in each sector as well as total (covered plus uncovered) employment. If we measure total employment so that $E_o = 1$, the minimum wage elasticity of employment $\eta_m = ln(E_m)/ln(W_m)$ is equal to $c\eta\epsilon ln(W_m)/[1 - c + \epsilon ln(W_m)]$.[4a] Note that while η_m is proportional to the demand elasticity η, it is not likely to be close to η. If $\epsilon = 0$, $\eta_m = 0$: covered-sector employment losses are offset exactly by uncovered sector gains. As ϵ increases, so does η_m, approaching $c\eta$ as ϵ approaches infinity. For "reasonable" values of the parameters, η_m can be much smaller than η, e.g.: if $c = .7$, $ln(W_m) = .6$, $\epsilon = .3$, and $\eta = -1.0$, $\eta_m = -.26$.

A more convenient but perhaps less plausible assumption is that those with the *lowest* reservation wages find covered-sector employment. In this case, $S_u'(W_u) = S(W_u) - D_c(W_m)$, and the employment elasticity η_m equals $c\eta\epsilon/[\epsilon - (1-c)\eta]$. Thus, η_m no longer varies with the proportional wage increase for covered-sector workers $ln(W_m)$. It remains true that $-\eta_m < -c\eta$, approaching $-c\eta$ as ϵ becomes larger. For the "reasonable" values used earlier, η_m equals only $-.35$. As one would expect, the disemployment effect is larger as coverage c is increased.

[4a] The proportional change in covered-sector employment is demand determined, and equals $\eta\ ln\ W_m$. The condition that supply equal demand in the uncovered sector can be solved for $ln\ W_u$ (see Welch, 1976, p. 22, eq. 6):

$$ln\ W_u = -c\ ln\ W_m/[1 - c + \epsilon ln W_m]$$

The proportional change in uncovered employment is then equal to $\eta\ ln\ W_u$. The proportional change in total employment (which, given the normalization $E_o = 1$, equals the level of total employment) is equal to $c\eta ln\ W_m + (1-c)\eta ln\ W_u$, which, after substituting the above expression for $ln\ W_u$ and simplifying, yields the expression in the text.

E. *Two-Sector Model with Queueing for Covered-Sector Jobs*

Neither the simplest supply-demand model nor Welch's two-sector extension relate the minimum wage to *un*employment. Mincer (1976) and Edward Gramlich (1976) provide such a link, by considering a fourth labor market status, remaining unemployed while searching for covered-sector employment, in addition to the three statuses identified by Welch (covered and uncovered employment, and labor force nonparticipation). They assume that workers choose the sector which offers the highest expected wage. Those workers who choose the covered sector receive W_m if they are employed; if P is the probability of being employed, the expected wage in the covered sector is PW_m. (Gramlich, 1976, allows transfers of rW_m to the unemployed, so the expected wage becomes $[P + r(1-P)]W_m$.) P depends on the number of unemployed looking for covered-sector jobs, U, relative to covered employment:

$$P = \frac{1}{1 + \dfrac{\alpha U}{D_c(W_m)}} \tag{3}$$

The parameter α depends on the assumptions made about labor-market turnover. If there is complete turnover (i.e., each covered-sector job is filled anew in any period), each of the D_c workers employed in the covered sector and the U unemployed workers looking for such jobs have identical probabilities of being employed in the covered sector in any subsequent period. That probability equals $D_c/(U + D_c)$, which simplifies to equation (3) with $\alpha = 1$. This was Gramlich's assumption. Mincer argued that, with incomplete turnover each period, α should be greater than one. Because the model includes no barriers to uncovered-sector employment, expected wages in the uncovered sector are equal to W_u. In equilibrium, expected wages in the two sectors must be equal:[5]

$$PW_m = W_u \tag{4}$$

The supply of labor, which is equal to the number of labor force participants, depends on expected wages of labor market participants; by equation (4) this just equals W_u. By definition, this supply of participants is either employed in one of the sectors or unemployed:

$$S(W_u) = D_c(W_m) + D_u(W_u) + U. \tag{5}$$

The three equations (3) — (5) can be solved for the three endogenous variables W_u, P, and U, as functions of W_m and, implicitly, c. With no minimum wage, $U = 0$ and labor force and employment are equal. Once again, measure employment so that this initial level of employment is unity. If one assumes that demand elasticities in the two sectors are equal, the resulting expression for the logarithm of total employment is:

$$ln\ E = \frac{c\left(\epsilon + \frac{1}{\alpha}\right)\eta}{\epsilon + \frac{c}{\alpha} - (1-c)\eta}\ ln\ W_m \tag{6}$$

The minimum wage elasticity of employment η_m is again less than η in absolute value; for $\alpha = 1$ and the parameter values used earlier, η_m equals $-.7$ when $\eta = -1$. Not surprisingly, more complete coverage intensifies disemployment effects.

The model can also be solved for the level or rate of unemployment. The unemployment rate is the ratio of unemployed to labor force participants, the latter given by equation (5):

$$\frac{U}{S(W_u)} = \frac{c(\epsilon - \eta)}{\epsilon\alpha + c - \alpha(1-c)\eta}\ ln\ W_m \tag{7}$$

Thus, the measured unemployment rate is an increasing function of the minimum wage and (as can be seen by differentiating Equation 7 with respect to c) an increasing function of c.

One can show that the uncovered-sector wage rises if $\eta + 1/\alpha$ is positive and falls otherwise. Since this is the expected wage in both sectors, it is a convenient measure of the effect on those who remain employed. If W_u rises (because workers leaving uncovered jobs to queue in the covered sector dominate the influx of workers from the covered sector), additional workers enter the labor market. If W_u falls, workers leave the labor force and measured unemployment is less than the employment reduction due to the minimum wage. If W_u does rise, it rises by a smaller proportion than W_m.

While the Mincer-Gramlich approach adds unemployment—interpreted as queueing for covered-sector jobs—to the two-sector model, it makes the overly strong assumption that one cannot search for covered-sector jobs while employed in the uncovered sector. If the two sectors are geographically separate, as might be true in developing countries (Michael Todaro, 1969), this assumption would be realistic. In the U.S., where coverage depends on industry and firm or establishment size, covered and uncovered establishments may be next door to each other.

The simplest generalization of the Mincer-Gramlich model would allow those in the labor force two strategies. One strategy is to search for covered employment if not employed in the covered sector; the other is to work in the uncovered sector (perhaps searching for covered-sector work) if not employed in the covered sector. The first strategy gives a probability P of being employed in the covered sector, and a probability $1 - P$

[5] The statement in the text ignores worker risk aversion. Allan King (1974) and Gramlich (1976) relax this assumption, though in a one-sector model context.

of being unemployed. The second strategy gives a lower probability of covered-sector work, BP ($B < 1$), and thus a probability of working in the uncovered sector equal to $1 - BP$.

In general, this extension produces few unexpected conclusions. Larger values of B reduce the effect of the minimum wage on both employment and unemployment. This is because, as B increases, uncovered employment becomes more attractive compared with full-time search for a covered-sector job (unemployment). Perhaps the most surprising result is that, while $B = 0$ corresponds to the Mincer-Gramlich model, there does not appear to be a special case corresponding to the Welch model. This is because, regardless of the parameters, the attractiveness of the two strategies is equalized in the Mincer-Gramlich model, whereas covered jobs are rationed (by an unspecified mechanism) in Welch's model.

While the idea of queueing unemployment certainly corresponds more closely to the official concept of unemployment than the supply-demand gap in the simplest model, the distinction between nonparticipation and unemployment is much sharper in the queueing model than in the real world. Kim Clark and Lawrence Summers, for example, argue that many young people are not actively searching for work but are willing to work if an opportunity is presented (1979, p. 9).

F. *Heterogeneous Workers*

While the theory outlined so far captures important aspects of the relationship between the minimum wage and employment, its applicability to empirical work is limited by the focus on a homogeneous group of workers earning the minimum wage. Given available data, empirical work has focused on groups of workers (usually demographic groups such as teenagers) in which a significant fraction earns more than the minimum wage—and therefore is not "directly" affected by it. Models of labor markets with heterogeneous workers have been a subject of much recent work among labor economists in general, and the minimum wage literature is starting to reflect that development.

The simplest heterogeneous-worker model allows for two types of workers, one of whom initially earns less than the new minimum wage. Let the subscripts 1 and 2 denote the directly affected low-wage and higher-wage workers, respectively, and let h be the proportion of workers who are initially in Group 1. Group 1 workers receive W_m while Group 2 workers receive W_2. For simplicity, the effect of W_m on both W_2 and output produced is neglected. This is a reasonable simplification where minimum wage workers are a fairly small proportion of the workforce. Finally, assume that Group 1 workers are substitutes for both Group 2 workers and the composite nonlabor input.

The key question is the relationship between η_{1+2}, the elasticity of $E_1 + E_2$ with respect to W_m (which is what is typically estimated), and η_1, the elasticity of E_1 with respect to W_m (which corresponds to the conventional own-price elasticity of demand for Group 1 workers). Clearly, η_{1+2} will be less in absolute value than η_1, because Group 2 workers' employment is increased by the minimum wage. The assumptions made above are sufficient to prove a more interesting bound on η_{1+2}:[6]

[6] In the conventional theory of labor demand in competitive markets, the (constant-output) elasticities of demand are

$$\eta_j = \partial\, ln(E_j)/\partial\, ln(W_m) = k_1\, \sigma_{1j}$$

where k_j is group j's share of total costs and σ_{ij} is the elasticity of substitution of inputs i and j. The elasticity of total employment with respect to W_m is

$$\eta_{1+2} = h\eta_1 + (1-h)\eta_2$$

Let $j = 3$ index a composite nonlabor input. The substitutability assumptions in the text are that σ_{12}

$$h\eta_1 < \eta_{1+2} < h\left(1 - \frac{W_m}{W_2}\right)\eta_1 \qquad (8)$$

This simple model suggests that, in comparing effects of minimum wages on employment of different demographic groups, those with a larger share of minimum wage workers (larger h) will face more severe disemployment. Thus, it is frequently predicted that minimum wage laws will have larger negative effects on black teenagers than white teenagers, because a larger proportion of the black teenagers would be directly affected by the minimum wage.

John Abowd and Mark Killingsworth (1981) generalize the two-skill model by allowing for covered and uncovered sectors, with low-wage workers faced with the same choices as in the Mincer-Gramlich model, and by allowing W_2 to change in response to the minimum wage. They provide approximate reduced-form expressions for changes in employment of the two types of workers separately, but even these approximations prove quite cumbersome.

An alternative model assumes a distribution of wages which mirrors the distribution of worker skill in the absence of the minimum wage. In the presence of the minimum wage, those with value of marginal product below that minimum are not employed, and the distribution of wages is truncated at the minimum (Marvin Kosters and Welch, 1972).

Two recent models of the minimum wage with heterogeneous workers (James Heckman and Guilherme Sedlacek, 1981; and Pettengill, 1981) allow for continuous distributions of worker skill, and hence relate the minimum wage to the wage distribution as well as to the level of employment.

Heckman and Sedlacek (1981) assume workers have two kinds of skill, each of which is useful in only one sector. These two skills may be positively or negatively correlated across workers. A worker is offered a wage in the covered sector W_c equal to the price of skill in the covered sector, times the number of units of covered-sector skill possessed by the worker; wages offered in the uncovered sector W_u are determined analogously. Each worker chooses the sector where the wage he or she is offered is highest, so aggregate supplies of skill to each sector depend on the relative prices of the two skills. Employers hire skill to the point where the value of the marginal product of skill is equal to its price.

A minimum wage would, if skill prices were fixed, lead covered-sector employers to discharge all workers earning less than the minimum, $W_c < W_m$. However, this is equivalent to a reduced supply of skill in the covered sector, which leads to an increase in its price. The increased price of covered-sector skill raises the wage offered to some of those not employable with a minimum wage at the old skill price up to or above the minimum (i.e., they are "re-employed") and attracts some of those initially employed in the uncovered sector (those for whom W_c was "slightly" below W_u at the initial skill prices). The price of skill in the uncovered sector may rise or fall, but must fall relative to the price of covered sector skill. This is analogous to the result for covered and uncovered wages with homogeneous workers.

In order to limit the complexity of the model, Heckman and Sedlacek assume that each industry uses only one skill and that workers with different levels of this skill are perfect substitutes in that industry's production. This imposes a very strong conclusion: wages of all workers

and σ_{13} are positive. Since σ_{12} and thus η_2 is positive, $\eta_{1+2} > h\eta_1$. Using the fact that

$$\sum_j k_j\sigma_{1j} = 0,$$

$\sigma_{13} > 0$ implies

$$\sigma_{12} < -\sigma_{11}(k_1/k_2) = -\sigma_{11} W_m h/[W_2(1-h)]$$

Substituting $k_1\sigma_{1j}$ for each η_j in the expression for η_{1+2}, and then substituting the above inequality for σ_{12} yields

$$\eta_{1+2} < k_1 h\sigma_{11}(1 - W_m/W_2) = h(1 - W_m/W_2)\eta_1$$

who remain in an industry increase (or decrease) by the same proportion in response to the minimum. This contradicts the conventional wisdom on the subject, which holds that such wage changes are largest for those initially just above the minimum.[7]

Pettengill's 1981 model also focuses on the continuous distribution of worker quality. As in the Heckman-Sedlacek model, worker quality is taken as predetermined, and workers seek the employment opportunity offering the highest wage. However, each worker has a unique quality (skill) ranking q, rather than a set of (possibly negatively correlated) skills. The demand side of the market is also quite different. Industries are identified with a continuous distribution of production "tasks" that differ in their sensitivity to worker quality. While a higher-rated worker is assumed to be more productive than a lower-rated one on all tasks, the relative productivity of the higher-rated worker will be largest on the tasks with the greatest quality sensitivity. Competition ensures that the highest quality workers are employed to perform the highest quality tasks. Substitutability between different types of labor is not explicitly modelled, but is implicit in the notion of tasks arrayed according to their quality sensitivity. The minimum wage is seen as eliminating the lowest-quality labor from the market, which potentially alters the entire wage distribution. The resulting level of employment and distribution of wages depend on the elasticity of substitution between labor and capital on each task and the elasticity of demand for output on each task (both of these elasticities are assumed constant across tasks), the elasticity of supply of each quality of labor, labor's share of total income, and the rate at which quality sensitivity varies across tasks.

Effects of the minimum wage must be calculated numerically, since no reduced-form employment equation can be derived. For a full-coverage minimum wage set at 55 percent of the median wage, the results are surprisingly insensitive to the wide range of parameters chosen. Total employment declines by 6 to 10 percent, and the wage of the lowest quality worker who remains employed rises 7 to 20 percent above its pre-minimum level. This wage increase is analogous to Heckman and Sedlacek's increased price of covered-sector skill, but wage increases diminish as one considers successively higher-quality workers. Pettengill also considers a partial-coverage minimum wage and endogenous worker effort; as noted earlier, these greatly reduce the calculated disemployment.

With worker effort endogenous, different quality workers can receive the same (minimum) wage, because greater effort is required of the lower-quality workers. Thus, this version of the model predicts a spike in the wage distribution at the minimum wage. That spike is a striking feature of observed wage distributions[8] and is not explained by most competing models including those which emphasize truncation at the minimum wage (Kosters and Welch, 1972; Heckman and Sedlacek, 1981).[9]

G. *Lagged Adjustment*

While lagged adjustment is often assumed in empirical studies of the effect

[7] Based on employer interviews, Converse, et al. (1981, p. 299) report that "Of establishments giving a wage increase to maintain differentials (after the 1980 increase in the minimum wage to $3.10), approximately 47 percent indicated that the differential increases stopped at a wage of $4.00 per hour or less."

[8] Gilroy (1981a, p. 162) reports that roughly half of all those receiving the minimum wage or less received a wage equal to the minimum wage. "Equal to" the minimum wage was defined as within a 10 cent interval centered on the minimum, but the overwhelming majority of these workers reported receiving exactly the minimum.

[9] Robert Meyer and David Wise (1981) suggest another explanation—workweeks are adjusted until the ratio of marginal product to hours worked is equal to the minimum wage.

of the minimum wage, the theory underlying this assumption is virtually undiscussed. Lagged adjustments to minimum wage increases are probably less plausible than in most other contexts where such lags are routinely assumed.

One important consideration is the fact that plausible adjustments in employment of minimum wage workers can be accomplished simply by reducing the rate at which replacements for normal turnover are hired. Employers report that separation rates among minimum wage workers averaged 13 percent per month (Converse, et al., 1981).

Of course, inability to adjust *other* inputs instantaneously would create lagged responses in employment of even a perfectly flexible input (M. Ishaq Nadiri and Sherwin Rosen, 1969, p. 462). It is not clear, however, that the required adjustments of other inputs are large enough to generate appreciable lags. Let subscript 1 denote minimum wage labor and 2 refer to the composite of other inputs; and let k_1, σ_{12} and g be input 1's share of costs, the elasticity of substitution, and the elasticity of demand for output, respectively. Then the proportional change in demand for input 2 equals $k_1(\sigma_{12} - g)$ times the proportional change in the minimum wage. Since k_1 is quite small,[10] and the demand elasticity g offsets at least part of the substitution toward input 2, the indicated change in other inputs is likely to be small.

A final consideration is the fact that minimum wage increases are enacted months or even years before they take effect.[11] Thus, "leads" are as plausible as "lags," and the lag may be very short.

H. *Welfare Effects*

The effect of the minimum wage in the simplest competitive market is straightforward: employment is reduced, and the efficiency of the labor market is impaired, because some individuals whose marginal product exceeds their reservation wage are unable to work. Under monopsony, a minimum wage could increase employment and enhance the efficiency of the labor market.

The remaining models often identify factors that could reduce the disemployment effects of a minimum wage—improved managerial efficiency (or additional worker effort), movement from the covered to the uncovered sector, or (partially) offsetting increases in employment of better-paid workers. Each of these mitigating factors, however, has a welfare cost of its own. For example, workers displaced into the uncovered sector end up working in jobs where their marginal product is less than it was in the covered sector. Thus, a zero employment loss would not imply that welfare costs were negligible. The welfare economics of these more complicated models has not received much formal development. As we shall see, the more refined models of minimum wage effects have served to interpret the empirical results, but the estimating equations have rarely served to identify the refinements (e.g., the supply and demand elasticities embedded in Equation 6 are not separately identified). Thus, even if formal welfare treatments were available, key parameter estimates would be largely conjectural.

[10] For the economy as a whole, minimum wage workers (those earning the minimum wage or less) account for only about four percent of labor costs, and therefore an even smaller fraction of total costs (Brown, 1981).

[11] The increase that took effect in March 1956 was approved in August 1955; those that took effect in September 1961 and September 1963 were approved in May 1961; those that became effective in February 1967 and 1968 were approved late in September 1966; the May 1974 and January 1975 increases were approved in April 1974; and the most recent increases, which became effective between January 1978–81 were enacted in November 1977. Thus, the first increase mandated by each amendment to the Fair Labor Standards Act was approved an average of three months in advance of its effective date, while remaining increases were announced more than a year before they became effective.

II. *Time-series Studies of Teenagers and Youth*

Most of the time-series studies present estimates of minimum wage effects only for youth and some only for teenagers. These groups are most often disaggregated by age (16–17, 18–19, and 20–24 years), sex, and race. Peter Mattila (1978 and 1981) and James Ragan (1977 and 1981) further disaggregate by school enrollment status. Gramlich (1976) estimates effects on full- and part-time workers separately while Welch (1976),[12] Daniel Hamermesh (1981), and Robert Cotterman (1981) consider the distribution of employment of teenagers by industry. However, limitations of time-series data have precluded disaggregation by region and detailed industry.

A. *Basic Equations Estimated*

Time-series studies that attempt to estimate the effect of minimum wages on the labor force status of youth have relied upon single equation models of the type

$$Y = f(MW, D, X_1 \ldots X_n)$$

where the dependent variable Y is a measure of labor force status. Independent variables include MW as a measure of the minimum wage, D as an aggregate demand (business cycle) variable to account for changes in the level of economic activity, and $X_1 \ldots X_n$ representing a host of other exogenous explanatory variables to control for labor supply, school enrollment, participation in the armed forces, and the like (Table 1).

To measure the "employment effect" of the minimum wage, the ratio of employment to population is used most often as the dependent variable. "Unemployment effects" are usually measured as the effect of the minimum wage on the proportion of the labor force (or of the population) unemployed. Unemployment equations were a characteristic of the earlier studies; recent research has estimated the effects of the minimum wage on the employment-population and labor force-population ratios, and has derived the unemployment effects from these. Several of the most recent studies focus on employment effects to the exclusion of any unemployment considerations (Abowd and Killingsworth, 1981; Charles Betsey and Bruce Dunson, 1980; John Boschen and Herschel Grossman, 1981; and Hamermesh, 1981). The shift in emphasis from "unemployment" to "employment" effects is important. In our view, it is a positive development, for four reasons.

First, the "employment" effects more nearly measure the extent of harm if the minimum wage does restrict job opportunities. Suppose that an increase in the minimum wage were known to have reduced employment by 10 jobs, compared with what employment would otherwise have been. Some of those who would otherwise have been employed may give up looking for jobs, and hence not be counted as "unemployed." But the harm done is not reduced on this account. Furthermore, if additional individuals enter the labor force to search for the now-more-attractive jobs (leading unemployment to increase by more than 10), the harm of the job-loss is not increased. Consequently, the "employment effects" more nearly measure the "cost" of the minimum wage in terms of job opportunities.

Second, the concept of unemployment is not precise, simply because the job search process is necessarily nebulous. While the official classification of individuals as employed is quite straightforward, the classification of persons as unemployed depends upon their having made some active effort (however serious) to seek work within the past four weeks. In other words, the line between *unemployment* and *not-in-the labor force* is not well

[12] Welch (1976) contains modifications to Welch (1974) as suggested by Siskind (1977). See also Welch (1977).

TABLE 1: TIME-SERIES STUDIES

	Kaitz (1970)	Moore (1971)	Adie (1971)	Kosters and Welch (1972)	Lovell (1972, 1973)	Adie (1973)	Kelly (1975)	Kelly (1976)	Gramlich (1976)	Hashimoto and Mincer (1970) Mincer (1976)
Period	1954–68[1]	1954–68	1954–70	1954–68	1954–68	1954–65	1954–68	1954–74	1948–75	1954–69
Data	Q	M	M	Q	Q	M	Q	Q	Q	Q
Disaggregation										
Age	X	X			X		X	X	X[8]	X
Sex	X	X	X	X	X	X	X	X	X[8]	X
Race	X	X	X	X	X	X	X			X
Other									X[8]	
Dependent Variable										
L/P	X									X
E/P	X						X	X		X
U/P	X				X					
U/L		X	X			X				
Other									X[9]	
Independent Variables										
Minimum Wage	a	b	d	a	a[4]	c	a	a	c[10]	a
Business Cycle	X	X	X	X	X	X	X	X	X	X
Population Share	X	X[3]			X		X			
Armed Forces	X						X	X[7]	X[11]	X
School Enrollment	X						X	X		
Employment Programs	X						X		X[11]	
Time									X	X
Other	X[2]						X[5]	X[5]	X[12]	
Functional Form	Linear	Linear	Log	Log	Linear	Log	Linear	Linear	Log	Linear
Lag Structure		AL	AL			AL	AL	AL	AL	AL

Notes:
L/P = labor force participation rate
E/P = employment-population ratio
U/P = (1 − E/P)
U/L = unemployment rate
a = BLS measure defined as ratio of minimum wage to average hourly earnings weighted by coverage
b = coverage separated out as another variable
c = real minimum wage
d = ratio of minimum wage to average wage
AL = Almon lag
X−1 = minimum wage lagged one period
Log = double logarithmic

[1] Estimates over several different time periods.
[2] Agricultural employment variable to reflect rural-urban population shift.
[3] For nonwhite regression only.
[4] Coverage separated out from BLS minimum wage variable in Lovell (1973).
[5] Welfare (in Kelly, female equations only).
[6] Other minimum wage variables also used.
[7] Male equations only.
[8] Teenagers, adult males, and adult females by full- and part-time status.

drawn. Hyman Kaitz (1970) and Alan Fisher (1973) make this point explicitly.

Third, focusing on employment status allows one to distinguish between full-time and part-time employment. However, the impact of the minimum wage on length of workweek has received relatively little attention in the literature.[13]

Finally, the changes in methods for measuring labor force status introduced to the Current Population Survey (CPS) in 1967 affected the count of the unemployed significantly more than that of the

[13] Gramlich (1976) finds that among teenagers (and to some extent among adult men) there is a rise in part-time employment and a decline in full-time employment due to increases in the minimum wage. Mattila's results are consistent with those of Gramlich for 18–19 year olds but not for 14–17 year olds (1981, p. 77). In studies of low-wage manufacturing industries (not limited to young workers), Albert Zucker (1973) finds relatively small reductions in weekly hours worked by production workers due to the minimum wage, while Mixon (1975) finds some evidence that the minimum increased regular hours of work (overtime effects were weak and inconsistent among industries).

BY MAJOR CHARACTERISTICS

Welch (1976)	Ragan (1977)	Mattila (1978)	Al-Salam, Quester and Welch (1981)	Freeman (1979)	Mattila (1981)	Ragan (1981)	Wachter and Kim (1979)	Iden (1980)	Abowd and Killingsworth (1981)	Betsey and Dunson (1981)	Boschen and Grossman (1981)	Brown, Gilroy and Kohen (1981)	Hamermesh (1981)
1954–68	1963–72	1947–76	1954–78	1948–77	1947–77	1963–78	1962–78	1954–79	1954–79	1954–79[1]	1948–79	1954–79[1]	1954–78
Q	Q	A	M	A	A	Q	Q	Q	Q	Q	A	Q	Q
	X	X		X	X	X	X					X	
	X	X			X	X	X	X				X	
	X					X	X	X		X		X	
X[13]	X[15]	X[15]	X[18]		X[15]	X[15]							
	X	X		X	X							X	
	X	X	X	X	X	X	X	X		X		X	
							X						
				X	X							X	
X[14]									X[21]		X[22]		X[9]
a[6]	a	a	b	d	a	a	d	a	c	a	b	a	a
X	X	X	X	X	X	X	X	X	X	X	X	X	X
	X		X	X		X	X[20]	X	X			X	
		X	X		X		X[7]	X		X		X	
			X										
	X		X			X		X				X	
X	X[16]	X	X	X		X[16]	X[16]	X	X	X	X	X	X
		X[17]			X[20]					X[5]		X	
Log	Log	Linear	Linear	Semi-log	Logit	Log	Log	Linear	Log	Linear	Log	Log	Log
	X−1	AL			AL	X−1		AL		AL	[23]		

[9] Teenage nonfarm employment.
[10] Three dummy variables to account for coverage changes.
[11] Teenagers equations only.
[12] Number of children 1–5 years in female regressions only.
[13] Also proportion of teenagers to total employed by industry (14–19 years).
[14] Teenage employment ÷ adult employment.
[15] Enrollment status by labor force status ÷ population.
[16] Female equations only.
[17] Benefits from GI bill.
[18] Teenage employment/population ratios: total and ratio of black-to-white.
[19] Rate of return to college, overall shares of service and agricultural employment, proportion of women 18–19 with children (female equations only).
[20] Youth population ÷ total population instead of subgroup ÷ total population.
[21] Teenage employment.
[22] Teenage employment ÷ total employment.
[23] Minimum wage in current period together with next year's value.

employed (Robert Stein, 1967).[14] This discontinuity in the unemployment series renders the unemployment effect estimates less reliable than the estimates of employment effects.

[14] Comparisons of *employment* estimates from the CPS and a special test sample utilizing the more restrictive but objective procedures were well within the expected sampling error; *unemployment* estimates under the new definition, however, were about 100,000 lower in 1966 than the official CPS figure. Unemployment among teenagers averaged 65,000 or one full percentage point less than the CPS estimate.

The key variable, minimum wage, has generally been measured by the ratio of the nominal legal minimum wage to average hourly earnings weighted by coverage, as devised by Kaitz (1970). Ratios of minimum wage rates to average hourly earnings are calculated for each industry, weighted by the proportion of workers covered. These are combined into an index in which the weight for each industry ratio is the number of persons employed in the industry as a proportion of total employment (Thomas

Gavett, 1970). Specifically, the index takes the form

$$\Sigma(E_i/E_t)\{(MW_i/AHE_i)(C_i) + (MW_i^*/AHE_i)(C_i^*)\}$$

where

E = nonagricultural employment
MW = basic minimum wage rate
AHE = average hourly earnings of nonsupervisory workers
C = proportion of nonsupervisory workers covered by the basic minimum wage rate
MW^* = minimum wage rate for newly covered workers
C^* = proportion of nonsupervisory employees covered by the minimum wage applicable to newly covered workers
i = major industry division
t = total private nonagricultural economy

Most studies that use this index use *teenage* employment ratios as weights.

The Kaitz index has the advantage of summarizing a great deal of information about the minimum wage law in a single variable. Consistent with the models discussed in Section I, it includes information about both the relative level of the minimum wage compared with market-determined wages and the degree of coverage; it also reflects the existence of lower minimums in newly covered industries. Thus it seems superior to three alternatives which have appeared in the literature—dummy variables for changes in the level or coverage of the minimum wage (Hugh Folk, 1968; James Easley and Robert Fearn, 1969; Peter Barth, 1969; and Yale Brozen, 1969),[15] the "real" minimum wage (Douglas Adie and Gene Chapin, 1971, Adie, 1973; Gramlich, 1976; and Abowd and Killingsworth, 1981), or the ratio of the minimum wage to average hourly earnings ignoring coverage (Arthur Burns, 1966; Lester Thurow, 1969; and Adie, 1971). As a result, most studies have used the Kaitz index, or some variant of it, to represent the provisions of minimum wage laws.[16]

Hamermesh (1981) departs from the standard Kaitz index in two quite different ways. First, he uses an estimate of average hourly earnings of *teenagers* instead of an economy-wide average in the relative minimum wage portion of the index. (He then includes the teen/adult average wage ratio as a separate variable.)[17] Second, he corrects hourly wage data to better reflect hourly *compensation* by including costs such as Social Security taxes, pension contributions, vacation pay, training, and corrects the minimum wage for the first two factors.

An alternative strategy is to include separate measures of the level and coverage of the minimum wage. As Gramlich (1976) has observed, the Kaitz variable assumes that a 10 percent increase in the level of the minimum wage has the same effect as a 10 percent increase in coverage—an assumption that has no theoretical justification. Fisher (1973) also argues against using a variable that makes these separate effects indistinguishable. As a statistical matter, however, the tendency for minimum wage increases and coverage extensions to occur simultaneously makes separate estimation of level and coverage effects difficult.

[15] Brozen reports changes in unemployment rates in months spanning minimum wage changes. This is formally equivalent to a dummy-variable approach.

[16] Terrence Kelly (1976) uses two other specifications: one that weights the variable by the industrial distribution of adults, and another that assumes the equilibrium (market-clearing) wage of teenagers has risen one-half as fast as average hourly earnings in manufacturing.

[17] Wachter and Choongsoo Kim also use teen earnings in their relative wage term but, unlike Hamermesh, exclude coverage and do not include the teenage/adult wage ratio. This leads to the debatable restriction that doubling both average teenage wages and the minimum wage leaves teenage employment unaffected.

The business cycle variable common to all studies is a measure of the overall demand for labor, although many proxies are used: adult unemployment or prime-age male unemployment rates, the Federal Reserve Board's index of industrial production, and the gap between actual and potential GNP. There is wide variation in the choice of other control variables in these studies. Nearly three-quarters of the studies use a time trend variable. Half of the studies incorporate a variable to control for participation in the armed forces as well as an overall potential labor supply variable, most often measured by the ratio of a particular group's population to the total working-age population. About one-third of the studies control for school enrollment and/or participation in employment and training programs (Table 1).

The most extensive discussion has focused on the inclusion of the youth population share variable. Adie and Lowell Galloway (1973) and Fisher (1973) have argued that this variable should not be included in either employment or unemployment equations estimating minimum wage effects. Because the simple supply-demand model suggests that employment is demand-determined in the presence of the minimum wage, excess labor supply is irrelevant; as a result, supply side variables (such as the population share) do not belong in the employment equation. Furthermore, because supply and demand would equilibrate in the absence of the minimum wage, increases in the supply of teenagers which increase teenage unemployment are really "minimum wage" effects as well, and are mistakenly attributed to the impact of supply-side variables.

Once the overly restrictive assumptions of the simple model are relaxed, this view loses much of its attractiveness. For example, the view that employment of teenagers is demand-determined may be correct for the half of teenagers who earn the minimum wage, but is difficult to accept for the remaining half who earn more than the minimum. Their employment must depend on the relative supply as well as the demand for teenage labor. Moreover, even if the demand-determination argument were correct, including truly exogenous supply-side variables would not bias the minimum wage coefficient in the employment equation, although the precision with which it can be estimated may be reduced to some degree.[18]

Excluding supply-determining variables from equations explaining teenage *un*employment also seems incorrect. Contrary to the apparent message of the simplest supply-demand model, some teenagers would still be counted as unemployed in the absence of the minimum wage, as is obvious from the unemployment statistics of teenagers who ordinarily earn more than the minimum (Michael Lovell, 1973; pp. 531–32; Goldfarb, 1974; pp. 264–65). Hence, the extent of unemployment *not* caused by the minimum wage must be held constant, and including variables that reflect relative supplies is necessary. Perhaps this does introduce some ambiguity into estimates of the effect of the minimum wage on teenage unemployment—how much teenage unemployment would be reduced if the minimum wage were repealed. However, the relevant policy issue is the effect of *marginal* changes in the minimum wage,

[18] Note that virtually all of the studies discussed above estimate employment equations whose dependent variable is the employment-*to-population* ratio. Thus, even studies that appear not to introduce supply side variables in the list of independent variables have effectively included such factors in the dependent variable. If employment of teenagers is really demand determined, the proper dependent variable would be employment, not the employment-to-population ratio.
Using the employment-to-population ratio as a dependent variable, conceivably, could be justified as controlling for trend influences on the demand for labor. But since teenage population has not grown at a uniform rate, this seems clearly inferior to adding a time trend as an explicit control variable.

and holding the relative supply of teenagers constant is certainly necessary to make that evaluation.[19]

Most studies use quarterly observations. This permits the capture of short-term cyclical fluctuations in aggregate demand (considerably more difficult with annual data) and mitigates the adverse effects of severe short-term variations in the values of variables, particularly sampling variation for small age-sex-race cells (a characteristic of monthly data). Linear and double-log specifications (in which the logarithm of the dependent variable depends on the logarithm of the minimum wage variable) are about equally common. About one-half employ some form of lag structure in their analyses.

All studies use labor force data from the Current Population Survey. As a result, one could argue that there really are not 25 independent studies. Since the earlier studies include about 15 years of data and the later studies about 25, the later ones can be thought of as replications of the earlier ones. However, subtle differences exist in the variables included, the form they take, the functional form of the equation, and the lag or lead structure utilized.

While most studies present estimates for several subgroups (necessitating the aggregation discussed below), most present only one specification. Where more than one specification was presented, we have tried to include the one that seemed most preferred by the author, or for which conversion to the 10-percent-increase format used below was most straightforward.[20]

B. *Results*

Only the findings of those studies that attempt to measure the employment and/or unemployment effects of a minimum wage are reported here. In order to enhance the comparability of these studies, their results are displayed in terms of *elasticities* for employment and *percentage point increases* for the unemployment rate. To measure employment effects, Tables 2 and 3 present the percent change in employment due to a 10-percent change in the minimum wage, i.e., 10 times the employment elasticity of the minimum wage $\eta(E)$. For studies that regress the logarithm of an employment measure (the employment-population ratio (E/P), for example) on the logarithm of the minimum wage (W_m), the coefficient of the minimum wage variable is simply $\eta(E)$. For studies that use a linear rather than a double-logarithmic specification, $\eta(E)$ equals the regression coefficient times $\overline{W_m} / (\overline{E/P})$, where the bar indicates the mean value over the sample period.

To further enhance comparability of results, several types of aggregation are necessary, particularly in calculating impacts for all teenagers in Table 3: combining (1) separate estimates for 16–17 and 18–19 year olds when estimates for the 16–19 group are not presented; (2) estimates

[19] The above argument might suggest an interaction of the minimum wage with relative teenage population in determining teenage unemployment. Given the difficulty in estimating even first-order effects precisely, the interactive approach has not been pursued. Note, however, equations that use the logarithm of the unemployment rate as the dependent variable implicitly impose a multiplicative interaction between relative supply and the minimum wage.

[20] The only case where estimates are dramatically affected by such a choice is Abowd and Killingsworth (1981). They present one equation based on a constrained nonlinear estimation, and another approximate, but still constrained, estimate. We include the former in the table; the approximation produces a larger estimated effect, −4.28 (1.99). The constraints depend on the identification of teenagers with minimum wage workers and adults with above-minimum wage workers. Since about half of all teenagers earn the minimum wage, and less than half of all minimum wage workers are teenagers, we find the constraints quite debatable.

TABLE 2
ESTIMATED IMPACT OF A 10 PERCENT CHANGE IN THE MINIMUM WAGE, BY SEX, AGE, AND RACE

	Percent Change in Employment (10 × Elasticity)								Change in Unemployment Rate (in percentage points)							
	White Males		Nonwhite Males		White Females		Nonwhite Females		White Males		Nonwhite Males		White Females		Nonwhite Females	
	16–17	18–19	16–17	18–19	16–17	18–19	16–17	18–19	16–17	18–19	16–17	18–19	16–17	18–19	16–17	18–19
Kaitz (1970)	−1.700*	−.814*	−.075	1.988	−2.072*	.077	1.236	.002	.344[a]	.073[a]	−1.445[a]	−1.529[a]	.181[a]	−.226[a]	.609[a]	.457[a]
Lovell (1972)[b]	—	—	—	—	—	—	—	—	−.043	−.087	−.191	−.224	.049	.019	.165	1.147*
Lovell (1973)[b]	—	—	—	—	—	—	—	—	−.381	−.555*	−.824	1.152	−.839	.225	1.567	.701
Kelly (1975)	−2.185*	−1.164*	−3.957*	−.326	−1.548*	−.174	−.149	.069	—	—	—	—	—	—	—	—
Kelly (1976)[c]	.07	−.76*	—	—	−1.33*	−.59*	—	—	—	—	—	—	—	—	—	—
Ragan (1977)	−.71[a]	−.89[a]	−5.14[a]	−2.47[a]	−.13[a]	−.07[a]	−2.57[a]	1.15[a]	1.19[a]	.68[a]	.98[a]	.73[a]	.36[a]	.79[a]	.84[a]	−.31[a]
Mattila (1978)[c]	−.73[a]	−.71[a]	—	—	−1.17[a]	−.90[a]	—	—	−.06[a]	.04[a]	—	—	.32[a]	.17[a]	—	—
Wachter and Kim (1979)	−2.777*	−1.192	−4.458	−2.566	−3.578*	−2.166	−13.39*	−4.784	.611[a]	.304[a]	−.901[a]	1.159[a]	.628[a]	.099[a]	3.171[a]	1.153[a]
Mattila (1981)[c]	—	−1.23[a]	—	—	—	−.22[a]	—	—	—	.14[a]	—	—	—	−.02[a]	—	—
Ragan (1981)	.06[a]	−.78[a]	−4.40[a]	−2.97[a]	−.42[a]	−.30[a]	−.32[a]	.92[a]	—	—	—	—	—	—	—	—

Notes:
* = Statistically significant.
a = No significance tests available because reported coefficients were calculated from disaggregated data or (for unemployment effects) from employment and labor-force effects.
b = Equations are based on the unemployment/population ratio; while they have been converted to unemployment rate impacts, they are not strictly comparable.
c = Estimates are not disaggregated by race; impacts shown in "white" columns are for all members of that column's age-sex group.

TABLE 3
ESTIMATED IMPACT OF A 10 PERCENT CHANGE IN THE MINIMUM WAGE ON TEENAGERS 16–19 YEARS, BY SEX AND RACE

	Percent Change in Employment (10 × Elasticity)					Change in Unemployment Rate (in percentage points)				
	White Males	White Females	Nonwhite Males	Nonwhite Females	All Workers	White Males	White Females	Nonwhite Males	Nonwhite Females	All Workers
Kaitz (1970)	−1.210[a]	−.746[a]	1.165[a]	.438[a]	−.98*	.190[a]	−.034[a]	−1.556[a]	.519[a]	−.006[a]
Adie (1971)	—	—	—	—	—	.731[a]	3.256[a]	5.793[a]	12.761[a]	2.525[a]
Moore (1971)	—	—	—	—	—	2.960*		8.901*		3.649[a]
Kosters and Welch (1972)	−3.31*	−2.41*	−3.56*	−3.01*	−2.96[a]	—	—	—	—	—
Lovell (1972)[b]	—	—	—	—	—	−.067[a]	.030[a]	−.210[a]	.793[a]	−.001[a]
Adie (1973)	—	—	—	—	—	.160	.700*	1.925*	2.787*	.518*
Lovell (1973)[b]	—	—	—	—	—	−.475[a]	−.181[a]	.494[a]	.505[a]	−.249[a]
Kelly (1975)	−1.620[a]	−.700[a]	−1.775[a]	−.080[a]	−1.204[a]	—	—	—	—	—
Kelly (1976)[c]	−.35[a]	−.96[a]	—	—	−.66[a]	—	—	—	—	—
Gramlich (1976)	—	—	—	—	−.94[a]	—	—	—	—	—
Hashimoto and Mincer (1970) & Mincer (1976)	−2.05*		−4.65		−2.31[a]	.412[a]		.693[a]		.445[a]
Welch (1976)	—	—	—	—	−1.78*	—	—	—	—	—
Ragan (1977)	−.81[a]	−.09[a]	−.35[a]	−.10[a]	−.65[a]	.91[a]	.62[a]	.83[a]	.10[a]	.75[a]
Mattila (1978)[c]	−.72[a]	−1.00[a]	—	—	−.84[a]	.00[a]	.23[a]	—	—	.10[a]
Freeman (1979)	—	—	—	—	−2.46[a]	—	—	—	—	.00[a]
Wachter and Kim (1979)	−1.883[a]	−2.722[a]	−3.290[a]	−7.710[a]	−2.519[a]	.431[a]	.306[a]	.265[a]	1.814[a]	.512[a]
Iden (1980)	−2.07*		−4.04*		2.26[a]					
	−2.31*		−3.81*							
Abowd and Killingsworth (1981)	—	—	—	—	−2.13	—	—	—	—	—
Al-Salam, Quester & Welch (1981)[c]	−1.19[a]	—	—	—	—	—	—	—	—	—
Betsey and Dunson (1981)	−1.50*		−.33		−1.39[a]					
Boschen and Grossman (1981)	—	—	—	—	−1.50	—	—	—	—	—
Brown, Gilroy and Kohen (1981)	−1.08*		.16		−.96	.12		−.74		.02
Hamermesh (1981)	—	—	—	—	−1.21	—	—	—	—	—
Ragan (1981)	−.41[a]	−.35[a]	−3.51[a]	.51[a]	−.52[a]	—	—	—	—	—

Notes: See notes, Table 2.

for different race-sex groups when results for teenagers as a whole are not reported; and (3) separate estimates for enrolled and non-enrolled individuals. For any two groups, elasticities are aggregated according to the formula:

$$\eta(E_1 + E_2) = \bar{S}\,\eta(E_1) + (1 - \bar{S})\,\eta(E_2)$$

where

$$\bar{S} = E_1/(\,E_1 + E_2)$$

The unemployment effects in Tables 2 and 3 represent the change in the unemployment rate due to a 10-percent change in the minimum wage. For example, .500 would indicate that a minimum wage increase of 10 percent is estimated to raise the unemployment rate from, say, 6.0 to 6.5 percent. For the studies that estimate separate employment and labor force equations in logarithmic form using the employment-population ratio (E/P) and labor force participation rate (L/P) as dependent variables, the minimum wage coefficients are the employment and labor force elasticities $\eta(E)$ and $\eta(L)$. Where the equations are linear, the regression coefficients must be multiplied by $\overline{W_m}/(\overline{E/P})$ and $W_m/(\overline{L/P})$, respectively, to derive $\eta(E)$ and $\eta(L)$. The impact, x, of a change in the minimum wage on the unemployment rate can then be derived as follows:

$$\begin{aligned}
u &= 1 - (E/L) = \text{the unemployment rate} \\
\Delta u &= (L\Delta E - E\Delta L)/L^2 = E/L(\Delta L/L - \Delta E/E) = (1-u)(\Delta L/L - \Delta E/E) \\
x &= \Delta u/(\Delta W_m / W_m) \\
&= (1-u)(\eta(L) - \eta(E)) \\
&= \text{the impact on the unemployment rate (in percentage points) of a 1 percent change in the minimum wage}
\end{aligned}$$

Thus, if the minimum wage increases by 10 percent $(\Delta W_m/W_m = .10)$, Δu expressed as a decimal is $.10x$, and the change in the unemployment rate in percentage points is $10x$. For studies in which the dependent variable is the unemployment rate expressed in percentage points, x is calculated as the regression coefficient for the minimum wage multiplied by $\overline{W_m}$. Just as the employment elasticities were aggregated as described earlier, so the labor force elasticities were similarly weighted using labor force shares.

On balance, a 10 percent increase in the minimum wage is estimated to result in about a 1–3 percent reduction in total teenage employment (Table 3). All studies find a negative employment effect for all teenagers together and the signs are almost exclusively negative for the various age-sex-race subgroups. Since it is necessary to compute many of the overall "effects" from the disaggregated equations, it is not possible to conduct tests to determine whether they are statistically significant. The coefficients from these disaggregated equations are mostly negative, with about half being statistically significant.

Although the research is consistent in finding some employment reduction associated with minimum wage increases, the estimated effects on unemployment appear to be considerably more varied. Of particular note are the large positive unemployment effects estimated by Adie (1971) and Thomas Moore (1971) on the one hand, and the negative unemployment effects estimated by Lovell (1973) on the other in response to a 10 percent increase in the minimum wage.

Yet, excluding these studies, the unemployment effects for all teenagers of the remaining nine studies reported in Table 3 are within a relatively narrow band—ranging from very small negative effects (virtually zero) to 0.75 percentage point. Implicitly or explicitly, studies finding disemployment effects but little or no unemployment impacts are finding labor-force withdrawal in response to minimum wage increases.

"Wrong-signed" coefficients are somewhat more common among the demographic subgroups in Tables 2 and 3 for the unemployment effects than was true for the employment effects. Because many of the unemployment effects are calculated from employment and labor force equations, their statistical significance could not be determined.

It is extraordinarily difficult to determine a few critical specification choices that explain the range of results. The overwhelming majority of the studies in Tables 2 and 3 contain no sensitivity analyses whatsoever. Moreover, the limited evidence available suggests that the effects of various choices are not necessarily additive—how the results are affected by one choice may depend on how another choice has been resolved.

The sample period chosen seems to have relatively minor effects on the estimated employment impacts. Both Hamermesh (1981) and Brown, Gilroy, and Kohen (1981) report that the estimates are not appreciably affected by extending the sample period from 1954–69 (roughly the sample period of the eight earliest studies in Table 1) to more recent years. However, Betsey and Dunson (1980) find considerably smaller effects over the full sample period than in the earlier period alone. There is a tendency for unemployment effects to be smaller in studies using data that includes the experience of the 1970s, although the differences between the three largest estimates (Moore, 1971; Adie, 1971 and 1973) and the others in Table 3 are probably due to other differences as well (see below).

The treatment of coverage has led to some interesting, if disturbing, results. Of those studies which allow for separate estimates of the effects of changing the level of the minimum and the proportion of workers covered, the general tendency is for coverage effects to be weaker, both in statistical significance and magnitude (Moore, 1971; Gramlich, 1976; Boschen and Grossman, 1981; Brown, Gilroy and Kohen, 1981; an exception is Nabeel Al-Salam, Aline Quester, and Welch, 1981). The imprecision of the estimates does not allow for confident rejection of the Kaitz restriction, or of the hypothesis that coverage effects are zero. Studies which ignore coverage altogether in creating the minimum wage variable (Richard B. Freeman, 1979; Wachter and Kim, 1979; and Abowd and Killingsworth, 1981) tend to report larger estimated employment effects, but we can see no justification for this omission.[21] Hamermesh (1981) concludes that his refinements of the Kaitz index have little impact on the estimated effect of the minimum wage.

Given the wide variation in control variables which reflect the supply or composition of teenage labor, relatively few confident judgments can be made about the impact of these supply-side control variables on the estimated effects of the minimum wage on employment. Betsey and Dunson (1980) report that controlling for welfare benefits reduces estimated minimum wage impacts, although the resulting estimates are not very stable across sample periods.[22] Al-Salam, Quester, and Welch (1981) note that the estimated minimum wage effects are higher (by about −0.5 in the measure in Table 3) when three "potentially endogenous" factors (fraction of teenagers in training programs, in school, or in the armed forces) are not held constant. Abowd and Killingsworth (1981) find little impact of including or excluding training enrollments, while

[21] Wachter and Kim (1979) are quite cautious about the interpretation of their "minimum wage" coefficients in light of the failure of coverage to contribute to the equation. They argue that their coefficients can be seen as reflecting changes in government social welfare expenditures during the 1960s, as well as the minimum wage.

[22] Kelly (1975 and 1976) also includes a welfare variable in equations explaining female labor force status. However, his "residualization" of this variable effectively guarantees that the estimated minimum wage impact will not be appreciably affected by the welfare variable's inclusion.

Brown, Gilroy, and Kohen (1981) find that the estimates are insignificantly affected by adding or deleting these or similar variables, at least with a double-log functional form.[23] Ragan (1977 and 1981) and Mattila (1978 and 1981) control for school enrollment, either directly or with exogenous variables thought to affect the enrollment decision, and their estimates are among the smaller ones in the literature.

Control variables appear to be more of a factor in the unemployment equations. Lovell (1973) reports that nearly the entire difference between his estimates and those of Moore (1971, pp. 534–35) is due to his inclusion of the teen population share as a control variable. In general, the results of others confirm this conclusion. Four of the five largest unemployment estimates appear in studies which exclude the population share (Adie, 1971 and 1973; Moore, 1971, who includes the share variable only in the nonwhite equation; Hashimoto and Mincer, 1970) while the five smallest estimates are all found in studies that include it (Kaitz, 1970; Lovell, 1972 and 1973; Freeman, 1979; and Brown, Gilroy, and Kohen, 1981). However, the results appear less sensitive to this specification choice as the sample period is extended.[24]

Table 3 also reveals few differences between those studies which assume that the effect of the minimum wage is instantaneous and those which assume a lagged response (usually of the Almon lag form).[25] Boschen and Grossman argue that responses to the minimum wage should depend on future values of the minimum wage and coverage, both increases which are announced in advance and the expected values of the minimum when increases have not been announced. Empirically, they assume that next year's value of the minimum is known, and beyond one year, the ratio of the minimum wage to average wages is expected to equal the average value of this ratio over the sample period. The combined effect of a change in current and next-year values can be calculated from their coefficients; it is shown in Table 3, and is not very different from the median value in the table. The "long-run" impact of a "permanent" change is not calculated.

Because it is difficult to explain the range of estimates in the literature by a few critical specification choices, it is not easy to produce a "best" estimate of the employment and unemployment effects. We are inclined to assign greater weight to papers that include a significant portion of the experience of the 1970s in the sample, and include coverage (either separately or in the Kaitz form) as well as the level of the minimum wage and control for exogenous factors governing the relative supply of teenagers. The impact of that preference is to concentrate the "preferred" estimates at the lower end of the range found in the literature, for both employment and unemployment effects.

The theory suggests that the disemploy-

[23] We find that the two specification choices with the largest impact are including a measure of welfare benefits or the young adult (20–24) population share. Both tend to increase the estimated impact of the minimum wage by about one-half of a percentage point, compared with the Table 3 value. However, in each case the added variable has a significant but wrong-signed (positive) effect on teenage employment.

[24] Brown, Gilroy, and Kohen (1981) find no significant effect of the decision on whether to include the teenage population share in an equation that runs the entire 1954–79 sample period, and which includes most of the previously mentioned control variables.

[25] The only papers that compare lagged and unlagged forms of the same equation show relatively small differences. Hamermesh (1981) reports slightly larger disemployment effects with lagged responses, but prefers the unlagged estimates because the a priori case for lags is weak. Brown, Gilroy, and Kohen (1981) add an Almon lag to an equation that includes the current-quarter value but cannot reject the hypothesis that there is no lagged response. For a discussion of the difficulty in estimating distributed lag models in this context, see Wachter (1976).

ment effects would be larger for those whose wages would otherwise be the lowest—blacks, women, and young teenagers. Tables 2 and 3 show some tendency for disemployment and unemployment effects to be more serious for 16–17 year olds than older teenagers; unemployment effects are more often larger for females than males, but disemployment effects vary the opposite way.

The most often discussed differences among teenagers are the black-white comparisons. A narrow majority of the comparisons in Table 3 show larger employment and unemployment effects for blacks. But the pattern is reversed among studies that include the 1970s: Wachter and Kim (1979) and George Iden (1980) find larger minimum wage effects among blacks than whites; Ragan (1977), Betsey and Dunson (1980) and Brown, Gilroy and Kohen (1981) find the opposite. Iden's black and white equations are not strictly comparable since the time trend variable (generally significant in minimum wage studies) is not the same in both equations. These mixed results erode much of the confidence we place in a black-white or even male-female comparison of minimum wage effects.

There may also be a problem with the reliability of some of these estimates because of the relatively small sample size of the population and labor force estimates of nonwhites (Welch, 1976, p. 13). More generally, many of the disaggregated effects cannot be calculated precisely, and the differences in such effects are likely to be estimated with even less precision.

Since the size of the CPS sample has grown over time, weighting the observations by the estimated number of nonwhite teenagers *actually surveyed* seemed desirable. This would place greater weight on the more recent observations, which are presumably subject to smaller sampling errors. Having done this, we found the resulting estimates to be only slightly closer to the white teenage results (Brown, Gilroy and Kohen, 1981).

While it is often asserted that blacks are more adversely affected than whites by the minimum wage, previous studies provide conflicting evidence on the issue. In any case, while these studies do not *disprove* the claim that nonwhites are more adversely affected, we conclude from the body of literature that such an assertion must rest on theoretical rather than empirical grounds, at least insofar as the time-series evidence is concerned.

In summary, our survey indicates a reduction of between one and three percent in teenage employment as a result of a 10 percent increase in the federal minimum wage. We regard the lower part of this range as most plausible because this is what most studies, which include the experience of the 1970s and deal carefully with minimum-wage coverage, tend to find. The other consistent finding is a notable withdrawal from the labor force by teenagers in response to an increased minimum, to the extent that unemployment effects of the higher minimum are considerably weaker than the disemployment effects.

III. *Cross-section Studies of Teenagers*

The studies reviewed thus far have relied on differences over time to estimate minimum wage effects—how did employment of teenagers change when the minimum wage changed? An alternative approach is to rely on cross-section data, usually by making comparisons between states or metropolitan areas which differ in the importance of the minimum wage.

A basic question that must be confronted with the cross-section approach is how one can identify differences in degree of importance of the minimum wage when, at one point in time, a single Federal minimum wage law applies to all states? If all the observations have the same value for the "minimum wage variable," one cannot estimate the minimum wage's effect. Several answers to this question have been provided in the literature on youth. Early studies, using 1960 Census data, asked whether *state* minimum wage laws lowered teenage employment. With the extension of Federal minimum wage coverage in retail trade and services in the 1960s, the importance of state laws was reduced, and later studies relied on the argument that the *impact* of the Federal minimum depends on average wage levels in the area (high-wage areas being less affected) and on the extent to which the area's industries are subject to the Federal law.[26]

Studies that focus on differences in state laws generally determine the impact of these laws on (average) wages of teenagers, and the impact of higher wages on teenage employment. The latter impact is of greater interest for studying effects of Federal minimum wage increases.

Generally speaking, the three studies surveyed (Edward Kalachek, 1969; Arnold Katz, 1973; Paul Osterman, 1979) revealed that higher wages reduced teenage employment. A 10 percent increase in average wages of (all) white teenagers (not just those at the minimum wage) reduced teenage employment by a few percent; there is some evidence that employment of black teenagers is more responsive to changes in their average wage.

Cross-section studies of the effect of the *Federal* minimum wage are a recent addition to the literature (Table 4). As in the time-series studies, youth employment is assumed to depend on the minimum wage, the demand for labor (as reflected in the area unemployment rate) and other factors. As can be seen from Table 4, there are significant differences between studies in the extent of attempts to control for these other factors. Studies which distinguish between student and nonstudent employment, or part-time and full-time employment (Ronald Ehrenberg and Alan Marcus, 1979, James Cunningham, 1981) include a more extensive list of control variables.[27]

Estimates of the employment effects of a 10 percent change in the minimum wage based on these cross-section studies are presented in Table 5. These estimates vary much more widely than the time-series results in Tables 2 and 3.

Unfortunately, it is difficult to determine which differences among studies are responsible for the different results. As was true of the time-series analyses reviewed in Section II, the studies rarely report how their estimates of minimum wage effects are altered by modifying the list of control variables, or other changes. There is one generalization apparently supported by Table 5: studies which attempt to control for many other determinants of youth employment (Ehrenberg and Marcus, 1979; and Cunningham, 1981) find smaller minimum wage effects than those with few controls (Welch and Cunningham, 1978; Freeman, 1979) for all teenagers (or all white teenagers). However, this relationship does not hold for individual race-sex groups, and there are no indications as to *which* control variables are primarily responsible for these changes.[28]

[26] Differences in average wage level prove far more important than differences in Federal coverage (Welch and James Cunningham, 1978, p. 144).

[27] Unfortunately, adding additional control variables need not bring the minimum wage effect closer to its "true" value. For example, measurement error in the minimum wage variable would tend to reduce the absolute value of its coefficient; adding variables correlated with it (i.e., correlated with average wage levels) would further depress the estimated minimum wage effect.

[28] In the early section of his paper, Freeman also includes a broader set of control variables, but the minimum wage coefficients for these equations are not reported.

TABLE 4
CROSS-SECTION STUDIES OF THE FEDERAL MINIMUM WAGE BY MAJOR CHARACTERISTICS

	Welch and Cunningham (1978)	Ehrenberg and Marcus (1979)	Ehrenberg and Marcus (1979)	Cunningham (1981)	Freeman (1979)	Cogan (1981)
Year	1970	1970	1966	1970	1970	1960–70
Unit of Observation	State	State	Indiv.	State	SMSA	State
Disaggregation: Sex		X	X	X		
Race		X	X	X		
Age	X	X	X	X	X	
Dependent Variable	E/P	E/P[1]	E_i[1]	E/P[2]	E/P[7]	E/P
Minimum Wage Variable						
Adjusted for Federal and State Coverage Variations	X	X	X	X		X
Other Independent Variables						
Youth/Adult Population	X	X	X		X	X
State or Area Unemployment Rate	X	X	X	X	X	
Urban/Total Population	X	X		X		
Family Income		X	X	X		
State Youth Minimum Wage Differential	X[3]	X	X			
Compulsory Schooling Laws		X	X			
Other		X[4]	X[5]	X[6]		X[8]

Notes:
E/P = employment/population ratio
E_i = dummy variable for individual employment status
[1] Four employment by enrollment statuses distinguished.
[2] Full-time/part-time and covered/uncovered employment distinguished.
[3] Included in construction of minimum wage variable; not included as a separate variable.
[4] School expenditures per pupil; farm/total population; "female-headed"/total families with children; nonwhite/total population; adult female education.
[5] Proportion of teenagers enrolled in federal training programs; individual's urban residence (yes/no), schooling, armed forces, family size, etc.
[6] Unionization; median adult schooling; school expenditures per pupil; p*, an estimate of the dependent variable based on its 1960 value, adjusted for (non-minimum wage) trends. All independent variables except p* expressed as proportional 1960 to 1970 changes.
[7] Also labor force participation and unemployment rates.
[8] Demand for agricultural workers; retail sales (as proxy for nonfarm labor demand); proportion of nonwhite teenagers in school; dummy variable for Southern states; E/P from previous Census, constrained to have coefficient of one.

Because most of the variation in the "minimum wage" variable (usually the fraction of workers covered times the ratio of the minimum wage to average wages) comes from variation in wage levels across states or areas, one is usually not certain whether the estimated effects are "minimum wage" effects or "state average wage effects." As a result, Freeman (1979, p. 8) concludes that the cross-section approach provides "at most a weak test of the effect of the minimum." Cunningham

TABLE 5
IMPACT OF A 10 PERCENT CHANGE IN THE MINIMUM WAGE ON TEENAGE EMPLOYMENT

	Percent Change in Employment (10 × Elasticity)				
	16–19 Year Olds				
	White Males	White Females	Nonwhite Males	Nonwhite Females	All Workers
Welch and Cunningham (1978)	—	—	—	—	−4.82[a]
Ehrenberg and Marcus (1979)	—	—	—	—	—
Census Data	.15[a]	.30[a]	—	—	—
NLS Data	−6.5[a]	—	—[b]	—	—
Freeman (1979)	—	—	—	—	−3.22[a]
Cunningham (1981)	—	—	—	—	—
Employment	−.07	−.44*	−.54	.00	−.24[a]
Full-time Equivalent	—	—	—	—	—
Employment	.41[a]	−.02[a]	−.96[a]	.07[a]	.15[a]
Cogan (1981)	—	—	−5.1*	—	—

Notes:
* Statistically significant.
[a] Computed from disaggregated estimates; no significance tests available
[b] Not reported. From reported coefficients, an estimate of 5 to 8 percent (positive) can be inferred.
[c] Full-time equivalent calculated as (Full-time) + ½ (Part-time)

(1981) and John Cogan (1981) provide more reassurance on this score than do the other studies, because they include the value of the dependent variable from the previous Census as a control variable.[29]

Two cross-section studies of teenage employment are not included in Table 4 because they used quite different approaches to the problem of estimating the effects of the minimum wage. Karl Egge et al. (1970) used cross-section data to analyze changes in the employment status of young men before and after the 1967 increase in the Federal minimum wage. They compared those whose 1966 wage was below the mandated minimum with other young men who were, presumably, not affected by the minimum. Their hypothesis was that if the minimum wage reduced employment opportunities, those earning less than the new minimum should have less favorable changes in employment status. They did not consistently find such a pattern.

Robert Meyer and David Wise (1981) use a quite different approach to estimating employment effects of the minimum wage, inferring them from the distribution of wages at one point in time. They assume that, in the absence of the minimum wage, the wage distribution for out-of-school teenagers would be given by

$$ln(w) = BX + e,$$

where X is a vector of worker characteristics and e is a normally distributed error term. Assuming that P_1 of those who would have wages less than the minimum remain in subminimum wage jobs while P_2 are raised to the minimum and

[29] Cogan's results may be distorted by the form chosen for several of the control variables. For example, in explaining the change in the *proportion* of nonwhite male teenagers employed in each state, his variable controlling for changing agricultural demand is the *numerical* change in such labor demand (e.g., −20,000 workers) rather than the change in (agricultural demand/population).

$(1 - P_1 - P_2)$ are disemployed, they estimate P_1, P_2, and B using maximum likelihood methods. They find that a 10 percent increase in the minimum wage would reduce employment of nonenrolled teenagers by 3.6 percent.

This estimate depends on the assumed functional form relating the wage to the personal characteristics and on the assumed distribution of the error term. Perhaps the main concern is that even if the Meyer and Wise model correctly specified the "uncensored" wage distribution, censoring of low-wage workers for reasons unrelated to the minimum wage might distort the distribution in a way that looked (to the eye and, presumably, to a maximum-likelihood algorithm) like a minimum-wage induced thinning of the lower tail. Teenagers who receive the lowest offered wages and who decide not to work would potentially have this effect.

It is more difficult to summarize neatly the principal findings of the cross-section studies than those of the time-series studies. The range of estimates is wider, and the number of studies smaller. On the basis of the cross-section studies alone, one is able to say little with confidence. The broader range of estimated employment effects does, however, roughly center on the 1–3 percent range which we found in the time-series studies. In that sense, one can fairly say that the cross-section evidence is not inconsistent with the time-series estimates.

IV. *The Minimum Wage and Adult Employment*

When we turn from teenagers to other population groups, we find a dramatic reduction in the number of studies of minimum wage effects on employment and unemployment. Those which provide estimates of the effect of the minimum wage on young adults (aged 20–24) show fairly consistent negative employment effects and positive unemployment impacts (Table 6). They tend to find smaller effects than those estimated for teenagers (e.g., generally less than a 1 percent reduction in their employment in response to a 10 percent increase in the minimum) although the effects vary somewhat across sex-race groups. Mincer (1976) and Wachter and Kim (1979) find larger effects for black than white males, but Wachter and Kim find large positive effects for black females.

The three available cross-sectional studies of young adults (Freeman, 1979; Cunningham, 1981; Meyer and Wise, 1981) also find smaller disemployment effects for young adults than they find for teenagers. However, the range is once again somewhat wider (from 0.2 to 2.2 percent) than in the time-series studies.

As noted in the discussion of the theory of the minimum wage, one expects to be able to detect effects of the minimum wage most readily if the group studied contains a relatively large fraction of workers who would have earned less than the mandated wage in the absence of minimum wage legislation. While teenagers and, to a lesser extent, young adults fit this description, adults generally do not. As a result, the minimum wage could increase or reduce adult employment and in either case, the effect may be so small compared to total adult employment that it will not be detected with precision.

Time-series studies on the subject produce quite mixed results. Mincer (1976) reports statistically significant employment reductions among white males over age 65 and white female adults but not for other age, sex, and race combinations. Gramlich (1976, pp. 438–43) finds statistically insignificant reductions for adult males and no effect for adult females. Hamermesh's (1981) results imply a small and statistically insignificant increase in adult employment because the minimum

TABLE 6
ESTIMATED IMPACT OF A 10 PERCENT CHANGE IN THE MINIMUM WAGE ON YOUTH 20–24 YEARS, BY SEX AND RACE

	Percent Change in Employment (10 × Elasticity)					Change in Unemployment Rate (in percentage points)				
	White Males	Nonwhite Males	White Females	Nonwhite Females	All Workers	White Males	Nonwhite Males	White Females	Nonwhite Females	All Workers
Moore (1971)	—	—	—	—	—	.271		—	—	—
Hashimoto and Mincer (1970) & Mincer (1976)	−1.84*	−3.54*	—	—	—	.613[a]	1.118[a]	—	—	—
Mattila (1978)[c]	−1.46[a]		.24[a]		−.74[a]	.23[a]		.00[a]		.13[a]
Freeman (1979)	—	—	—	—	1.0*	—	—	—	—	−.2*
Wachter and Kim (1979)	−.443	−2.109	−1.333*	2.973*	−.719[a]	.558[a]	1.178[a]	−.080[a]	−.016[a]	.345[a]
Brown, Gilroy and Kohen (1981)	—	—	—	—	−.26	—	—	—	—	.23*

Notes: See notes, Table 2.

wage raises the wages of competing teenagers. Boschen and Grossman (1981) find that employment of adult women is significantly increased as the level (but not coverage) of the minimum wage is raised. The only conclusion emerging from these studies is that it is difficult to estimate the effect of the minimum wage on adult employment with any precision from time-series data.

A cross-section study by Peter Linneman (1980) adopts a quite different approach to estimating adult disemployment effects. Given data on wages and other characteristics such as age and education of workers in 1973, he estimates the wage such workers would have earned in 1974, had the minimum wage not been increased. He argues that those directly affected by the minimum wage are those whose predicted wages would have been less than the new 1974 minimum and that the negative employment effects should be greatest for those whose predicted wage was furthest below the minimum. Linneman finds that this was indeed the case. While he does not estimate the overall reduction in adult employment due to the minimum wage increase, his results permit the inference that it is substantial.[30] However, Linneman also finds that those with wages well above the minimum suffered lower employment than they would have with a constant minimum wage, while most theoretical predictions would have yielded the opposite result. This raises the possibility that his results reflect the fact that low-wage workers are less likely to be employed without convincingly implicating the minimum wage as a cause of this problem.[31]

V. *Evidence from Low-Wage Industries and Areas*

In contrast to the studies reviewed thus far, which focus on the effect of the minimum wage on subgroups of the population classified by individual or demographic characteristics, a smaller set of studies focuses on the effect of the minimum wage on different industries or areas. In line with the observation that such effects will be most reliably detected when a significant fraction of workers in the sample studied are directly affected by increases in the minimum wage, these studies focus on low-wage industries or low-wage areas.

Most studies isolate the impact of the minimum wage by comparing changes in employment, over a period which brackets an increase in the minimum, between units of observation which differ in the extent to which wages initially fell below the new minimum. Implicitly, this assumes that, in the absence of the minimum wage increase, observations with high concentrations of workers initially paid less than the new minimum would have experienced roughly the same employment growth as observations with fewer low-paid workers. As noted below, this assumption is often open to challenge. Compared to the studies reviewed earlier, the studies in this section tend to have fewer explicit control variables to capture

[30] Linneman reports that earnings of adults, who would otherwise earn less than the minimum wage, are reduced by the minimum wage increase when wage gains and employment reductions are both taken into account. This finding would imply at least a 1 percent reduction in employment of these adults in response to a 1 percent increase in the minimum.

[31] If workers with predicted wages slightly above the minimum are the only above-minimum workers to experience employment reductions, the result could be easily rationalized. If W is the offered wage for a worker at the lower minimum wage, W^* is the predicted wage, and W_m the minimum wage, then $W < W_m$, the condition that the worker would be displaced by the minimum, would still occur with nonzero probability even if W^* exceeded W_m. In fact, Linneman's actual disemployment estimates run too far up the predicted wage distribution for this to be a likely explanation.

the effect of factors besides the minimum wage, and so lean more heavily on pre-increase employment to implicitly control for these differences.

A. *Employment Effects in Newly Covered Sectors*

Most of the six amendments to the Fair Labor Standards Act (FLSA) have included changes in coverage of minimum wage workers. The 1961 and 1966 amendments, in particular, resulted in coverage increases for retail trade and services, while the 1966 amendments provided for significant increases among agricultural workers. Studies that have focused on these low-wage industrial sectors are reviewed below.

Agriculture. The statutory minimum wage for covered farm workers has risen in seven steps from $1.00 per hour in February 1967 to eventual parity with other covered workers at $2.65 in 1978. The studies that measure the impact of minimum wages on employment in the agricultural sector build upon earlier econometric analyses of the farm labor market (G. Edward Schuh, 1962; and Edward Trychniewicz and Schuh, 1969) and tend to support the competitive hypothesis that increases in the minimum wage result in adverse employment effects.[32]

In an aggregate time-series study of U.S. agriculture over the 1946–78 period, Bruce Gardner (1981) finds significant disemployment effects, with the minimum wage reducing the number of hired farm workers by 60,000 (about 5 percent of its 1979 level). He also reports that disaggregated regional estimates, although not statistically significant, exhibit some adverse employment effects. Unfortunately, the individual regional estimates are not reported, making it impossible to compare their relative sizes.

Earlier time-series analyses are based on fewer years experience with the minimum wage in agriculture, during a period when that minimum was lower relative to other wages. These studies find larger reductions in employment due to the minimum than Gardner's five percent estimate. For example, Gardner (1972), using annual data over the 1947–70 period, estimates that the 1966 FLSA-extended minimum wage coverage reduced hired farm employment by about 18 percent from what it would otherwise have been in the 1967–70 period. Theodore Lianos (1972), studying twelve southern states forming three regions over the 1950–69 period, finds that both total and hired farm employment decreased with the imposition of a Federal minimum wage on the agricultural sector. Over the years 1967 to 1969, the reduction in employment under various assumptions is estimated to have been between 24 and 51 percent. H. F. Gallasch (1975), using pooled cross-section data, also finds significant disemployment effects associated with the imposition of a minimum wage, i.e., that over the 1951–71 period, a 10 percent increase in the agricultural minimum wage resulted in a 6 percent decrease in the employment of hired farm workers.

In a more specialized study, John Trapani and J. R. Moroney (1981) estimate the effect of the 1966 FLSA amendments on employment of seasonal cotton workers as the difference between actual employment and that predicted (based on 1960–66 data) to have occurred in the absence of the 1966 introduction of a minimum wage. Using pooled cross-section data on 14 cotton-producing states, they find that extended minimum wage coverage accounts for 65 percent of the decline in peak-month cotton-farming jobs between 1967 and 1969. With employment

[32] An example of earlier agricultural research that is consistent with this position is Frank Maier (1961). For a survey of the literature see Gilroy (1981b).

on cotton farms plummeting from 193,000 to 47,000 between these years, the authors' estimates indicate that the minimum-wage-induced employment decrease would be about 50 percent. As might be expected, the greatest effects are found in regions where wages, on average, are lower—the south central and southeastern states.

Although the results of these studies are consistent in finding significant disemployment effects, they must be interpreted with care. First, the effects of the agricultural minimum wage are made difficult to interpret by the heterogeneity of the farm labor force, which includes low-skill manual laborers and high-skill managers, children and retired persons, full-time workers and seasonal/part-time laborers.[33] Additional problems arise with both the measurement and classification of agricultural employment. Although nearly all studies use the number of hired farm workers as the dependent variable, there is evidence that a number of family farm workers (for whom data are also collected in the agricultural survey) should be included as hired labor. This exclusion could lead to an overestimate of the proportionate minimum wage effects. The distinction between self-employed and hired labor is also sometimes difficult to make. Sharecroppers, for example, are counted as self-employed, but may work for wages at certain times, and many farm owner-operators work for wages on other farms.

Second, the minimum wage effects are difficult to interpret because of the exempt status of many employers and employees, serious doubts about the degree of FLSA enforcement, and the questionable knowledge of the legal requirements among both farmers and farm workers. For example, immediate family members of a farm operator are exempt, but more distant relatives often do farm work on the "family" farm. Formally, they are subject to the provisions of the FLSA if they are paid a wage, but there is doubtless great temptation to forego the formality in such cases. There is also reason to believe that the formality is ignored in the case of nonrelative neighbors with whom there are long-standing work relationships.

Third, although all studies include one or more trended variables (e.g., "time" or nonfarm wages) which should yield more confidence in the interpretation of the effects of the minimum wage variable, the specification of the minimum wage variable itself is open to question. Trapani and Moroney (1981) do not include an explicit minimum wage variable in their study, Lianos (1972) uses a crude dummy-variable proxy, and Gardner (1972) employs the nominal value of the minimum wage. Gallasch (1975) and Gardner (1981) deflate the nominal minimum by economy-wide (not agricultural) price indices, although the reason for this specification is unclear. Although coverage data are not rich, none of the studies attempts to account for changes in coverage, nor is there any mention in the studies of its potential impact. This omission may have relatively minor consequences, however, because coverage has not varied greatly since agriculture was first covered in 1967.

Finally, apart from Gardner's 1981 paper, no study includes more than five observations in the post-1966 period. Pooling cross-section data is one way to circumvent this problem (Gallasch, 1975).

On balance, these problems do not lead one to conclude that the estimates are biased in a known direction. Rather, they raise questions about the reliability of the estimates in that the problems could lead to either over- or under-estimates of the "true" minimum-wage effect.

In a descriptive study of the extension

[33] See Gardner (1981) for an extended discussion of this.

of coverage to certain hired farm workers, the U.S. Department of Labor also acknowledges a sharp drop in agricultural employment on covered farms after the introduction of the $1.00 minimum in 1967 (U.S. Department of Labor, 1972, p. 23). However, the relative drop was smaller among covered than among uncovered farms. The analysis is weakened by the failure to disentangle the comparison of covered and uncovered farms from that of large and small farms. Thus, if employment on larger farms would have grown more rapidly or fallen less rapidly than on others in the absence of minimum wage coverage, the comparisons between covered and uncovered farms will *understate* any negative employment impact of the minimum wage.

Retail Trade. There are several published studies of the impact of extending (partial) minimum wage coverage to the heterogeneous, low-wage retail trade sector. According to a U.S. Department of Labor analysis (U.S. Department of Labor, 1963, p. 40), employment in covered establishments in the South (where the impact of a $1.00 minimum imposed in September 1961 was greatest) fell by 10.6 percent between June 1961 and June 1962, while employment in uncovered establishments rose by 4.8 percent. Analogously, another Department of Labor study (U.S. Department of Labor, 1966b, p. 49) reports that nationally uncovered employment grew slightly faster than covered employment during this period. However, the same study indicates that the covered sector grew more rapidly in the succeeding three years, during which there were two increases in the minimum applicable to retail trade.

In a later study of a large segment of the retail trade industry—eating and drinking establishments—the degree of impact of the 1966 extension of minimum wage coverage is measured by the increase in average wages necessary to bring all workers in an establishment up to the minimum (U.S. Department of Labor, 1968a). Establishments are categorized as either "high-," "low-," or "no-impact." In this case, the Department of Labor finds no clear correlation between the degree of impact and the employment changes that followed the 1966 extension of coverage.

Subsequent studies of the retail trade industry have reached conflicting conclusions. William Shkurti and Belton Fleisher (1968) conclude that while the adverse effect of the minimum wage on the economy as a whole was probably small, some segments have experienced substantially less employment growth than would otherwise have occurred. Analyzing employment changes from 1961 to 1965 (a period that includes the effective dates for the $1.00 and $1.15 minima in retail trade), they find that employment grew most slowly in those "lines of business" (e.g., variety stores) in which the wage impact of the minimum was the largest. Moreover, within some lines of business, the rate of increase of employment was smaller in the South (where the impact of the minimum wage was the largest) than elsewhere. However, Jack Karlin's (1967) analysis of the 1961–1966 employment changes (which included the September 1965 increase to $1.25) reaches very different conclusions. Specifically, he finds that covered retail trade employment rose more rapidly in the South than elsewhere and that the larger increases in employment occurred in those lines of retail trade where the wages of a considerable fraction of the work-force would have to have been raised to the level of $1.25 per hour. These divergent conclusions appear to reflect differences in judging whether two columns of numbers (minimum wage impact and employment growth) are or are not related, as well as differences in the time periods studied and other differences in the data utilized.

Neither of these studies presents formal measures of statistical association to support the qualitative inferences; neither controls for pre-existing trends.

In partial response to the shortcomings and inconsistencies of these studies of retail trade employment, we have applied conventional regression techniques to the same data. Two alternative dependent variables (percentage change in hours and persons employed) were regressed on a minimum wage variable (the percent of workers in covered establishments in 1961 who were earning less than the "new" minimum wage) for three alternate time periods (1961–62, 1961–65, and 1961–66). In addition, alternate specifications of the equation contain differing combinations of the following control variables: region of the country, line of business, and percentage change in employment in the uncovered sector (coefficient constrained to 1.0). Because the number of observations is limited to 26 (seven lines of business times four regions, minus two cells too small to report), none of the coefficients can be estimated with much precision and none of the minimum wage effects would be judged statistically significant by conventional standards. However, in all versions of the equation containing the full complement of control variables, the minimum wage variable's coefficient carries a negative sign. Nonetheless, as noted in assessing the studies of agriculture, the value of even careful statistical analysis is weakened to the extent that the covered-uncovered comparison really reflects an underlying "large-small" comparison. If larger retail trade establishments would have grown more rapidly than others in the absence of minimum wage coverage, then comparisons between covered and uncovered firms will understate any negative impact of the minimum wage, unless size of establishment is held constant.

Based on annual data covering the period 1948–79, Boschen and Grossman (1981) use time-series regressions to estimate a significant disemployment effect of the minimum wage in the retail trade sector. Although their results do not present an estimate of disemployment due directly to the 1961 imposition of the minimum, their methodology does produce an estimated (short-run) elasticity of employment with respect to the minimum of −.03. For comparison purposes, this is about one-fifth as large as their estimated elasticity of employment of all teenagers.

Instead of focusing on the effects of the minimum wage on the level of employment, Janice Madden and Joyce Cooper (1981) ask whether the minimum wage affected states' share of output and employment in wholesale and retail trade. To the extent that firms' decisions on where to locate are based on labor costs, increases in the minimum wage should make states with larger concentrations of low-wage workers or a larger fraction of workers subject to minimum wage laws less attractive locations. Low-wage states may be growing because their wages are low, and this would increase their share of wholesale and retail trade employment. Madden and Cooper deal with this to some extent by controlling for the growth of state population and income. They report no consistent evidence of the hypothesized effects in either industry. They note, however, that limitations of the state-by-industry data base they constructed back to 1958 may have obscured such effects.

In a study of the age composition of retail trade employment, Philip Cotterill and Walter Wadycki (1976) use 1967 Survey of Economic Opportunity cross-section data to analyze the effect of minimum wage coverage on the substitution between teenage and adult labor. Although they conclude that there is no evidence that employers substituted for teenage labor, the study clearly suffers from the inability to measure what would have been the utilization rate of the two groups in the absence of the minimum wage. In addition, their conclusion runs counter to the findings in David Kaun's 1965 study of substitution in low-wage manufacturing industries. He is able to show that as a result of a change in relative factor costs, due to a minimum wage increase, firms alter relative factor inputs, with the greatest change taking place where the minimum wage requires the greatest upward wage adjustment (most notably among small producers).

In probably the most thorough statistical study of a specific industry, Fleisher (1981) concludes that employment in retail trade has been significantly curtailed as a result of the 1961 imposition and the subsequent increases in the Federal minimum wage. Using a mixture of time-series regressions, forecast relative wages in retail trade, and estimates of consumer demand equations for retail trade services, he infers that during the 1960s retail trade employment was about 5 percent lower than it would otherwise have been for each 5 percent that the average hourly labor cost in retail trade was raised by increases in the minimum wage. Further, he finds that employment measured by hours of work was reduced in greater proportion than was employment measured by persons working. It should be noted that this implied "elasticity" with a value approximately equal to one, is not comparable to the economy-wide minimum wage elasticities of employment discussed in preceding sections of this paper. In fact, Fleisher opts for a minimum wage variable different from any of those utilized in other time-series studies; namely, "the proportionate increase in the forecast wage needed to bring all workers at least to the minimum wage" (p. 85).

Despite the general confirmation of significant disemployment in retail trade resulting from the imposition of the legal wage floor, Fleisher finds notable variation within the industry. Specifically, his disaggregated results point to a negligible (nonsignificant) effect on department store employment and particularly strong effects on variety stores and food stores although, in the latter case, the impact on hours of work is much weaker than on number of persons working. This nonuniformity of findings and their consequently limited generalizability is compounded by the omission from the multivariate analyses of eating and drinking places, many of which are major employers of minimum wage workers.

Service Industries. Analogous to its study of eating and drinking establishments in the retail trade sector, the U.S. Department of Labor has issued reports on several service industries in which minimum wage coverage was extended by the 1966 FLSA Amendments; namely hospitals, hotels and motels, and laundries and cleaning establishments. In none of these three cases does the report find clear evidence of a correlation between the degree of impact of extending minimum wage coverage (i.e., the increase in average wages necessary to bring all workers in an establishment up to the minimum) and the employment changes following the extension (U.S. Department of Labor, 1970, p. 27; U.S. Department of Labor, 1968b, p. 18; U.S. Department of Labor, 1969, p. 18). The conclusion in the laundry and cleaning services study, however, is incorrect.[34] It is noteworthy that these

[34] In the other two studies, as well as in the study of eating and drinking places, the conclusion is based on the average percent change in employment in "high-," "medium-," "low-," and "no-impact" establishments. In the study of laundry and cleaning services, however, the conclusion rests on a cross-tabulation of the degree of impact and the *direction* of change in employment (no change, increase, decrease). Using Appendix Table 35 of the report, we calculated the average employment change by impact group, the measure used in the other studies. There is a clear negative relationship between degree of impact and employment change:

Impact	None	Low	Medium	High
Percent Change in Employment	+1.5	−1.0	−2.0	−4.6

studies are focused only on determining *whether* high-impact establishments had smaller employment increases (or larger employment declines) than low-impact establishments. There are additional important, if subtle, questions that might be addressed with these data: (1) Is it likely that any observed relationship between the degree of impact and employment changes could be due to chance alone? (2) How large is the relationship (if any) between degree of impact and employment change?

Both questions can be answered by combining the data from these studies for various industries and computing the average "elasticity" of employment with respect to minimum wage impacts. The basic assumption underlying this procedure is, apart from the minimum wage increase, employment in high-, medium-, and low-impact establishments in a particular industry would have grown or declined by approximately the same proportion. We allow growth rates in different industries to differ. Because we are computing an *average* elasticity, differences in degree of responsiveness among establishments are ignored.

Our preferred estimates of the employment elasticity are in the −.05 to −.12 range, implying that a minimum wage increase that had a "direct" wage impact of 10 percent would reduce employment by about 1 percent.[35] However, these estimates are not very precise, owing to the small number of observations (four industries times four impact groups) and would not pass conventional tests of statistical significance, i.e., estimates of this size could arise due to chance alone when the "true" elasticity was zero. On the other hand, it is well to bear in mind that these estimates are probably biased downward because the low-wage high-impact establishments were concentrated in the South, where employment would have grown more rapidly in the absence of extended minimum wage coverage.

In a descriptive study, Kenneth Gordon (1981) focuses on the private household service sector's response to the 1974 minimum wage coverage extension by comparing the rate of change in employment of private household workers (defined to exclude employees of *firms* offering cleaning or similar services) before and after 1974. He finds that since 1974 the long-term decline in the absolute number of household workers has slowed dramatically, precisely the opposite of what one would expect to observe if the minimum wage were having an adverse effect on employment in this sector.[36] Gordon does find that black women in this industry experienced large employment losses over the 1974–78 period, although he concludes that this is probably not related to the extension of minimum wage coverage since wages for blacks are considerably higher than those for whites. Nevertheless, he points to other ways in which a disemployment effect of the extension of coverage has been manifested. There is some evidence that hours of work have been slightly reduced and that the amount of involuntary part-time work has increased. Gordon concludes that one prin-

[35] The choice of weights makes a considerable difference to the estimates. If the less plausible establishment weights are used, the employment effects would be considerably larger and "significant" statistically. As noted in footnote 34, there is a consistent negative relationship between impact and employment growth in laundry and dry cleaning (an industry with many small establishments) but not in other industries, so that the establishment weighting gives greater weight to the industry with the strongest negative relationship.

[36] This is in accord with the findings by Brozen (1962), but for a very different reason, under different circumstances. Brozen found that when the minimum wage rose the number of persons employed as household workers actually rose. Apparently, some of the persons who lost jobs in the covered sectors as well as those who would normally have entered and failed to find work, took jobs in the then-noncovered household sector.

cipal reason for the modest observed effects in this sector is the even more modest levels of compliance with and enforcement of the law.

B. *Employment Effects in Low-Wage Manufacturing*

In connection with the 1956 increase in the minimum wage, the U.S. Department of Labor studied several manufacturing industries in which it could reasonably be expected that employment effects would be discernible. The analyses are based on establishment-level employment data from before and after the date of increase in the minimum wage, in the manner previously described in the discussion of newly covered service industries. Twelve low-wage industries have been studied, and in some cases the industries are further subdivided according to geographic region. In general, the studies focus on the Southern portion of low-wage industries, because the greatest impacts were expected to occur there. In each industry, establishments are classified into "high-," "medium-," and "low-impact" groups according to the increase in average wages needed to bring all workers in the establishment up to the minimum, relative to other establishments in that industry. In general, the percentage change in employment is found to be more positive (or less negative) in the low-impact than in the high-impact establishments.[37] On average, the increase in employment in high-impact firms is 5 percent lower than that in low-impact firms (U.S. Department of Labor, Office of the Secretary, 1959, p. 9).

Once again, the failure to exploit fully the available data prompts us to reanalyze them in search of somewhat more precise answers to the question of the employment impact of the change in the minimum wage. Unlike the case of the service industries, analysis of the low-wage manufacturing sector is complicated by the fact that employment was measured in different months in different industries. On the other hand, the larger number of observations in the manufacturing data increases the precision of our estimates in comparison to the service industries. Our preferred estimate[38] is −0.24, suggesting that a minimum wage increase with a direct "impact" of 10 percent would reduce employment by 2.4 percent. The alternate specifications of the equation suggest employment losses that are larger than the preferred estimates, but not dramatically so (the median estimate is −0.36). The preferred estimate is statistically significant.

In the broader context of estimating labor demand equations, Zucker (1973) uses quarterly time-series data to analyze the impact of minimum wage changes on employment in seven low-wage, nondurable-goods manufacturing industries during the period 1947–66. By and large, the results are in conformity with the theoretical expectation that increases in the minimum wage (relative to the actual average wage) lead to reductions in employment. This disemployment impact is found to prevail for both number of workers and number of hours worked, and the results imply that the latter were adjusted both

[37] Depending on how "industry" is defined, and the time after the increase when the increase was measured, this pattern was observed in eight industries out of twelve (U.S. Department of Labor, Office of the Secretary, 1959, p. 9), ten out of eleven (George Macesich and Charles Stewart, 1960, p. 286), nine out of fourteen (John Peterson and Stewart, 1969, p. 78), or thirteen out of fourteen (Peterson and Stewart, 1969, p. 79). See also H. M. Douty (1960).

[38] The "preferred" estimate uses all of the available data and weights the observations by initial employment. The number of industry dummy variables included and the inclusion or exclusion of average establishment size make almost no difference, given the choice of dependent variable and weighting.

more rapidly and to a larger extent than was the former.[39]

Mixon (1975) also uses time-series data to investigate the impact of minimum wage changes on employment in twenty (three-digit) low-wage manufacturing industries during the period 1958–69.[40] Using the length of the average (regular) workweek as the measure of employment, the minimum wage is found to have the expected effect in but six of the 20 industries. Moreover, in only two of the 20 is there evidence that increasing the minimum resulted in a significant decrease in the average amount of overtime worked per week.

Similarly mixed but somewhat stronger results are reported in Boschen and Grossman's 1981 study of eight low-wage manufacturing industries based on annual data for the period 1948–79. The composite minimum wage effect on employment is found to be negative in six of the eight industries and, in half of these cases, the coefficient is statistically significant. On average, the results imply that a 10 percent increase in the minimum wage would diminish employment by just less than one percent.

C. *Evidence from Low-Wage Areas*

In pursuit of the impact of the increase in the minimum wage to $1.00, effective March 1956, the U.S. Department of Labor also collected data on employment before and after the increase in seven low-wage areas. Comparisons of the change in covered employment with the "degree of impact" of the increased minimum show (1) larger employment gains in high-impact areas when comparing February 1956 with April 1956 and (2) no relationship when comparing April 1956 with April 1957 (U.S. Department of Labor, Office of the Secretary, 1959, pp. 250 and 254).[41] A later analysis compares the growth of covered employment relative to uncovered employment. Covered employment is found to have grown *faster*, although the reverse is found when the analysis is restricted to those establishments included in both the pre- and post-increase surveys (U.S. Department of Labor, Office of the Secretary, 1959, p. 11).

Similar data have been collected to study the effects of the 1961 and 1963 increases in Southern metropolitan, Southern nonmetropolitan, and North Central nonmetropolitan areas. An early analysis (U.S. Department of Labor, 1965, p. 14) of the Southern nonmetropolitan areas uses the high- versus low-impact comparison and finds no employment effects.

A later report that analyzes data for all three types of areas places much less reliance on the degree-of-impact comparisons (U.S. Department of Labor, Office of the Secretary, 1966, pp. 64, 97, 130–31). While some problems are noted in newly covered retail trade establishments (U.S. Department of Labor, Office of the Secretary, 1966, pp. 66–67, 98, 131), the general conclusion is that there were no harmful employment effects. But this conclusion

[39] Zucker's estimates of the elasticity of employment with respect to the minimum wage (relative to the one-period lag average wage) is −0.91 for hours of work and −0.79 for number of workers (p. 275).

[40] This study actually attempts to focus attention on other economic effects of the minimum wage using such dependent variables as the average amount of overtime work per week, the layoff rate and the quit rate. All in all, the empirical results for those measures are no more regular than those for the length of the regular workweek. Mixon enters the real minimum and average wages as separate variables, so his estimate of the effect of the minimum wage would not be affected by changes in employment due to the average wage level per se.

[41] The latter comparison is somewhat strange, since the "base period" of the comparison is one month after the minimum became effective. Presumably, the intention is to determine whether there are any "extra" effects that occur after the first month at the new minimum.

rests largely on the (virtually irrelevant) fact that covered employment generally rose after the minimum wage increases.

None of these studies controlled for prior employment trends. Thus, if low wage areas tend to grow more rapidly in the absence of the minimum wage, the impact of the minimum would be underestimated by these studies.

Marshall Colberg (1960) analyzes the growth of manufacturing employment in Florida from January to April 1956 by studying a matched sample of plants, but the data are aggregated so that the county is the unit of observation. He finds a negative relationship that is marginally significant statistically between the rate of increase in hourly wages and employment growth.[42] Generally, there are, however, some hints that high-wage counties would have grown more rapidly even in the absence of the minimum wage (p. 114).

In a time-series study over the 1970–77 period, Charlie Carter (1978) finds that increases in the minimum wage have adverse effects on unemployment rates, with the degree of impact greater in low-wage regions like the Southeast. Specifically, his equation implies that a 10 percent increase in the minimum wage (Kaitz) variable would raise the jobless rate in the eight Southeastern states together by half a percentage point.

The methodologically most sophisticated study of the effect of the minimum wage in low-wage industries and areas is that of Heckman and Sedlacek (1981). They apply their model (discussed in Section I) to manufacturing employment in South Carolina from 1948–71. They estimate the employment effects of the minimum wage separately for the four race-sex groups, but do not report estimates for black females.[43] They find that the "direct" effect of a 20 percent increase in the minimum wage would be to reduce employment by 22, 36 and 34 percent for white males, white females, and black males, respectively. The "indirect" effects of rising skill prices on employment are positive, but small (no more than 3 percentage points for any group).

As noted in Section I, one assumption of the model is questionable: namely, that all those who remain employed in the covered sector experience the same proportional wage increase. Moreover, the wage distribution predicted by the model does not have the spike at the minimum wage we observe in real world data. These issues are worrisome because Heckman and Sedlacek (unlike nearly all other papers on this subject) *use* these theoretical models in *deriving* rather than in *interpreting* the results. The highly nonlinear model makes it impossible, for us at least, to trace through the consequences of these specification choices for the estimates.

VI. *Conclusions*

Our survey has focused on the effects of the minimum wage on employment and unemployment. These effects are relevant to, but do not uniquely determine, its efficiency and distributional consequences. Thus, one cannot easily infer the deadweight loss due to the minimum wage from its effects on labor force

[42] Each one percent increase in average wages is associated with a .12 percent reduction in employment when all counties are included. Among low-wage counties, the estimated relationship is much larger—.92 percent versus .12 percent—but the estimate is less significant statistically—.15 versus .10 level (Colberg, 1960, p. 113).

[43] They found it impossible to obtain reasonable estimates of the parameters of the skill distribution for black females. They attribute this to the enormous increase in black female employment in manufacturing (a 791 percent increase from 1960 to 1971), presumably due to factors not captured by the model. Since their model is overidentified, the exclusion of a demographic group has no effect on the identification of the remaining parameters of their model.

status.[44] Moreover, the effect of the minimum wage on the distribution of income depends on its impact on the wage distribution and the position of low-wage workers in the income distribution, as well as on the employment effects we have surveyed. The impact on wages includes both the relatively straightforward raising of the wages of some workers up to the minimum and the effect on wages above the minimum (which is presumably positive for those just above the minimum, who are good substitutes for minimum-wage workers). The impact on wages might itself be the subject of a separate (though shorter) survey. The relatively weak correlation between low wages and membership in low-income households (Gramlich, 1976; Kelly, 1976) weakens the impact of the minimum wage on the distribution of household income, whatever its effect on the distribution of earnings.[45]

Theoretical analysis of the relationship between the minimum wage and employment and unemployment has been extended considerably in the last decade. A major development has been the formal treatment of a minimum wage with partial coverage, and of workers' decisions to search for covered employment rather than work in the uncovered sector. Extending that theory to deal with continuously variable labor quality is a rather recent addition to the literature and has many applications beyond the minimum wage. Thus far, at least, theoretical models that take account of continuously variable labor quality are relatively complex, and that complexity is a decided drawback in empirical work based on these models. An unanswered question is whether this complexity *can* be reduced without losing much of the realism of the models.

The most frequently studied group in the empirical literature is teenage. Time-series studies typically find that a 10 percent increase in the minimum wage reduces teenage employment by one to three percent. This range includes estimates based on a wide spectrum of specifications and on different sample periods, but all used the same basic data source, the CPS. We believe that the lower half of that range is to be preferred; to the extent that differences in results can be attributed to differences in the specification chosen, the better choices seem to produce estimates at the lower end of the range. There may well be problems common to all the studies that lead to understating this impact, but that possibility remains to be shown. Cross-section studies of the effect on teenage employment produce a wider range of estimated impacts, which are roughly centered on the range found in the time-series research. Estimates of the minimum wage effect of a 10 percent increase on teenage unemployment rates range from zero to over three percent, but estimates from 0 to .75 percentage points are most plausible.

The effect of the minimum wage on young adult (20–24 years) employment is negative and smaller than that for teenagers. This conclusion rests on much less evidence than is available for those 16–19 years. The direction of the effect on adult employment is uncertain in the empirical work, as it is in the theory. While some adults are undoubtedly displaced by the minimum wage, others may be employed because the minimum wage protects them from teenage competition. Uncertainty about the effects on adults is a serious gap in the literature, since half of all minimum wage workers (and, of course, a larger fraction of all workers) are adults.

Less can be said with confidence about

[44] Effects on labor force status depend on but often do not identify the underlying supply and demand elasticities. Moreover, the "offsets" mentioned in Footnote 3 would greatly complicate the measurement of deadweight loss.

[45] For recent studies on the effects on the wage and income distributions, see *Report of the Minimum Wage Study Commission* (1981, Vols. VI and VII).

the effect of the minimum wage on employment in low-wage industries and areas. In part, this reflects a smaller number of studies and the fact that there is less recent work (and therefore less work with now-common statistical tools) to survey. Negative employment effects are a consistent feature of the studies of low-wage manufacturing and agriculture, but findings are quite mixed elsewhere. In several studies, minimum wage effects are reported as the ratio of the percentage change in covered employment to the percentage increase in average wages due to the minimum wage. This elasticity of covered-sector labor demand is about -1.0 in some cases, and less than one in absolute value in others.

With few exceptions, the theoretical developments of the last decade have had relatively little effect on the estimation of minimum wage effects. It is difficult to distinguish a 1970 paper from a 1980 paper from the empirical work alone. While the theory is useful in interpreting the results, its integration into the empirical work is incomplete at this point.

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