On Layoffs and Unemployment Insurance

By Robert H. Topel*

The problem studied in this paper is how current systems of providing and financing unemployment insurance (UI) affect the private decisions that generate unemployment. It is widely recognized that the provision of UI affects the search strategies of jobless individuals by raising reservation wages, thus increasing the average duration of unemployment spells.¹ In various forms, almost all empirical work on the impact of UI has studied this incentive. More recently, the focus of theoretical research has shifted to the role of UI in affecting the joint decisions of workers and firms that generate transitions to unemployment. Here the emphasis is on current methods of financing benefits—via partially “experience rated” payroll taxes on individual employers—and the incentives that these methods provide toward increasing the incidence of temporary layoff unemployment.² In terms of what is known about magnitudes of effects, this role of experience rating is probably the most important unresolved empirical issue in UI research. It is the main concern in this paper.

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²The empirical literature on duration effects of UI is large and continues to grow. For useful surveys of empirical estimates, see Finis Welch (1977) or Daniel Hamermesh (1979). Relatively recent contributions are Nicholas Kiefer and George Neumann (1979) and Steven Nickell (1979).

³For example, Feldstein (1978) measures UI incentives by the ratio of available benefits to disposable weekly earnings. Nothing can be inferred from this variable about the role of experience rating and UI subsidization in affecting unemployment. Only Frank Brechling (1981) has attempted to tie experience rating attributes of states to measures of labor turnover.

That current methods of financing unemployment insurance subsidize unemployment is undeniable; as it turns out, experience rating in determining employer taxes is normally incomplete, so that in almost all cases, the value of benefits received by unemployed workers exceeds their incremental cost to firms. The empirical magnitude of this wedge, which often equals or even exceeds the total money value of benefits paid out, can provide a powerful incentive toward increased layoffs. Despite this fact, however, previous research has been severely limited by the absence of any reliable measure of the experience rating subsidy to unemployment that is both relevant for individual workers, and that can be used in empirical analysis.

My point of departure in this paper is in measuring the extent of subsidization that is implied by the structures of UI financing systems in the United States. Differences in these structures across states provide the empirical leverage needed to identify the incentive effects of the UI subsidy. The estimates reported below, based on a large sample of individuals from the 1975 Annual Demographic File of the Current Population Survey (CPS), indicate that incomplete experience rating may account for as much as 30 percent of all spells of temporary layoff unemployment. Additionally, most of the impact of UI on layoffs is accounted for by this subsidy; nonsubsidized benefits are found to have an insignificant impact on layoff decisions.

Appealing to the theory of job search, the duration of unemployment spells has played a prominent role in previous empirical research on the effects of UI. The incentives

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that operate in the present context are quite different, however, and theory indicates that UI will affect both the probabilities that individuals enter unemployment (the frequency of spells, via layoff decisions) and that they leave unemployment (the duration of spells, via search and rehire decisions). The empirical methodology developed below estimates the effects of UI, and other variables, on each of these transitions. Consistent with theory, the estimates indicate that the UI subsidy increases unemployment by changing both the frequency and the expected duration of layoff spells. It is important to note that this methodology adjusts for the fact that UI may induce a greater number of short spells, which could actually reduce the mean duration of unemployment in any sample.

The paper is organized as follows. The first section develops the empirical foundation for the problem analyzed here, focusing on the structure of UI systems and methods of financing in the United States, and on the incentives implied for the joint employment decisions of workers and firms. Section II describes the data and the empirical strategy used to estimate UI effects on unemployment, and Section III reports the econometric evidence.

I. The Institutional Setting

Among unemployed individuals who have separated from their previous jobs (i.e., excluding entrants to the labor force), temporary layoffs and discharges consistently dominate quits, accounting for nearly 90 percent of this category in an average year. Since quits are the major reason for UI eligibility under state laws, it is clear that the large majority of passages to unemployment are, in principle, compensable by the UI system. In fact, tabulations of a special questionnaire administered with the May 1976 CPS indicate that 75 percent of all temporary layoffs and 70 percent of discharges were either receiving UI benefits or had an application to receive them pending. In short, most "involuntary" transitions into unemployment involve the receipt of UI benefits for at least some portion of time spent unemployed.

Despite this important connection, not all individuals who may experience unemployment are equally "insured" by the UI system. Subject to several qualifying restrictions, a typical state program may pay weekly benefits to an unemployed worker that are equal to half of foregone, before-tax weekly earnings, up to a maximum for a stipulated period. Variations on this basic algorithm are numerous, however, and the proportion of spendable earnings that are replaced by UI benefits—the so-called "replacement ratio"—can differ across individuals both within and between state programs. To illustrate the range of these differences, Table 1 shows calculated values of the replacement ratio for hypothetical individuals in several state programs in 1975. Thus, for example, a married worker in Michigan with two dependents and a weekly wage of $100 would have experienced only a 4 percent decline in disposable income had he become unemployed,5 while a layoff would have reduced the same worker's income by more than 40 percent in California or Florida. Similar differences can be generated within state programs: a tripling of the weekly wage to $300 in Texas implies a 34 percentage point decline in the replacement ratio, while the same experiment in Wisconsin—with a more liberal benefit ceiling and progressive state income taxes—would reduce the replacement ratio only slightly.

With experience-rated UI financing, the effects of these differences on the incidence of unemployment, and especially on layoff decisions, are theoretically ambiguous.6 Con-

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5 These points are developed formally in the expanded version of this paper, which examines the demand for insurance by workers and the feedback effects of workers' search strategies on the layoff and rehire decisions of firms. See my (1982a) article.

6 Though not shown in the table, a worker in Michigan with three or more dependents would experience a significant increase in disposable income during an unemployment spell.
TABLE 1—Benefit Replacement Ratios for Qualified Workers in Selected State Unemployment Insurance Programs, 1975

<table>
<thead>
<tr>
<th>State</th>
<th>Single</th>
<th>Married, 2 Dependents</th>
</tr>
</thead>
<tbody>
<tr>
<td>California</td>
<td>.63</td>
<td>.50</td>
</tr>
<tr>
<td>Florida</td>
<td>.62</td>
<td>.50</td>
</tr>
<tr>
<td>Massachusetts</td>
<td>.70</td>
<td>.45</td>
</tr>
<tr>
<td>Michigan</td>
<td>.76</td>
<td>.45</td>
</tr>
<tr>
<td>New York</td>
<td>.65</td>
<td>.46</td>
</tr>
<tr>
<td>Ohio</td>
<td>.62</td>
<td>.46</td>
</tr>
<tr>
<td>Texas</td>
<td>.64</td>
<td>.40</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>.67</td>
<td>.45</td>
</tr>
</tbody>
</table>

Note: Replacement ratio is calculated as potential benefit amount under state qualifying provisions as a proportion of after-tax weekly earnings. Marginal tax rates are based on standard deductions under federal and state income tax laws for the indicated family structure. Benefit amounts reflect dependency allowances where applicable.

Consider, for example, the case where wages and benefits do not substitute perfectly in worker preferences (workers are risk averse), so that there is a private demand for UI as insurance for workers. In the absence of a UI system, optimal employment agreements between workers and firms would provide for some form of private unemployment compensation that mitigates workers' income risks. In this context, the incentive effects of publicly administered UI depend on 1) who pays for the benefits that workers receive, and 2) the level of required benefit payments relative to what would have existed privately. In general, if UI is a binding constraint on employment agreements, so firms must pay benefits above the privately optimal level, then higher benefits will increase the cost of laying off workers and the incidence of unemployment will decline. In contrast, if firms are not liable for any increase in benefits, then UI subsidizes unemployment and the incidence of layoffs will increase. Our current systems of financing benefit payments result in a diverse mixture of these offsetting extremes, with firms usually being only partially liable for the benefits their workers receive. Thus, the size and even the sign of UI's effect on unemployment depend on the degree of subsidization that is relevant in any particular case.

The size of this subsidy can be quite important. In the United States, as in no other country, UI benefits are financed by taxes on employer payrolls that are related to their individual histories of generating unemployment. As already noted, this system of experience rating is highly imperfect. Yet some employers are more heavily subsidized by imperfect rating than others. To focus on these differences, and to highlight the main empirical issues, Table 2 reports some characteristics of the distribution of insured unemployment and tax liabilities for six major state systems in 1967 and 1978.

An important feature of state programs is that assigned employer tax rates are bounded, so that firms with relatively high average unemployment, who pay the maximum rate, may consistently accumulate deficits of tax liabilities relative to benefit payments. For these employers and their workers, the marginal cost of benefits is zero since an increase in insured unemployment can cause no incremental taxes. The empirical importance of this fact is illustrated in row A of the table, which shows for each state the estimated proportion of total employment that occurred in firms whose accumulated past tax contributions were smaller than benefit withdrawals. Especially in 1967, this proportion is fairly small (averaging about 10 percent of covered employment), yet row B shows that roughly half of all UI benefits were received by employees of these firms. Evidently, firms that entered these years with negative balances due to high past unemployment had higher than normal unemployment during 1967 and 1978 as well. In fact, rows C and D show that estimated unemployment rates for deficit employers in 1967

The same argument implies that firms would have a greater incentive to rehire a worker on layoff in order to avoid the costs of continued benefits. Thus, the duration of spells would be shorter as well. Of course, for workers who are discharged from their previous jobs, so there is no possibility of recall, only search incentives are relevant and average duration of spells would increase.

8The data used in these calculations were collected and compiled by Joseph Becker (1972, 1981). Complete data for other states and years are not available.
Table 2—Summary Statistics for Selected State Programs: The Distribution of Insured Unemployment for Positive and Negative Balance Firms, 1967 and 1978

<table>
<thead>
<tr>
<th></th>
<th>California</th>
<th>Massachusetts</th>
<th>Michigan</th>
<th>New York</th>
<th>Ohio</th>
<th>Wisconsin</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. Proportion of Total Taxable Wages Paid by Negative Balance Firms&lt;sup&gt;a&lt;/sup&gt;</td>
<td>14.2</td>
<td>14.3</td>
<td>11.8</td>
<td>13.1</td>
<td>3.6</td>
<td>28.0</td>
</tr>
<tr>
<td>B. Proportion of Total Charged Benefits Charged to Negative Balance Firms&lt;sup&gt;a&lt;/sup&gt;</td>
<td>51.8</td>
<td>52.5</td>
<td>55.3</td>
<td>46.0</td>
<td>61.6</td>
<td>61.6</td>
</tr>
<tr>
<td>C. State Insured Unemployment Rate&lt;sup&gt;b&lt;/sup&gt;</td>
<td>3.9</td>
<td>3.3</td>
<td>2.9</td>
<td>3.1</td>
<td>2.6</td>
<td>3.6</td>
</tr>
<tr>
<td>D. Estimated Insured Unemployment Rate for Negative Balance Firms&lt;sup&gt;c&lt;/sup&gt;</td>
<td>14.2</td>
<td>12.1</td>
<td>13.6</td>
<td>10.9</td>
<td>25.1</td>
<td>8.7</td>
</tr>
</tbody>
</table>

<sup>a</sup>Source: Becker (1972, p. 112; 1981, p. 83).
<sup>b</sup>Average weekly insured unemployment. Source: U.S. Department of Labor, Handbook of Unemployment Insurance Financial Data (1978). Data for 1978 are unpublished, and were obtained directly from the Labor Department.
<sup>c</sup>(Row B - Row A) × Row C.

Averaged about five times the implied means for positive balance employers, but this difference sharply narrowed as greater numbers of firms achieved deficits by 1978. Thus, the data indicate extensive cross subsidization within self-financing state systems. And since most firms with negative balances pay the maximum allowable tax rates, it follows that the marginal cost of benefits that is relevant for a major portion of insured unemployment is also heavily subsidized.

While these facts document an important degree of cross subsidization in the current UI system, nothing is implied by them about the empirical role of UI financing in affecting unemployment, since workers in high unemployment sectors are, by construction, subject to larger subsidies. For example, the insured unemployment rate for workers in the construction industry is typically about 10 percent, which is sufficiently high to guarantee that employers in this industry pay the maximum rate and face a full marginal subsidy on benefits. It is clearly not legitimate to infer from this fact that large subsidies cause above-average unemployment in this industry. This type of reverse causality presents a serious inference problem in isolating independent effects of the subsidy, and methodology for dealing with it is developed below. In addition, while these data indicate that a significant portion of unemployment is subject to zero experience rating, they say nothing about the costs and incentives implied for the majority of firms who pay tax rates between the state minima and maxima, and who are experience rated to some degree. This degree of rating, which is implied by the structure of state systems, plays an important role in the empirical analysis. Here, I outline the essential features of measuring the degree of experience rating, and relegate technical details to the Appendix. More detailed descriptions of particular experience rating methods appear in my article with Welch.

<sup>9</sup>Ceteris paribus, this shift implies that a larger proportion of covered employment is subject to zero experience rating. For details and causes of these trends, see my article with Welch (1980).

<sup>10</sup>For the states listed in Table 2, about 75 percent of employment fell in this category. For further evidence, see Becker (1981, Table 24, pp. 164–67).
A. The Degree of Experience Rating in Current Financing Systems

Under federal legislation, states may design their own schemes of benefit financing and payroll taxation so long as assigned tax rates can be justified on the basis of an employer's unemployment history. As with Social Security, these taxes are paid on a taxable wage base per employee that may be chosen by the states. Federal requirements on the form of state systems are quite loose, and as a result there is considerable heterogeneity both in the type of system chosen and, for a given system, in the particular parameters that affect incentives. In this paper, I focus on two general methods of experience rating accounting, the reserve-ratio and benefit-ratio methods, because they are the most common and because their incentives can be parsimoniously summarized for empirical research. Together, these methods apply to more than 80 percent of insured employment in the United States.

I define the degree of experience rating $e$ as the value of marginal tax liabilities paid by employers per dollar of benefits received by workers. Thus, an employer faces incomplete experience rating ($e < 1$) if an additional layoff spell generates benefits that have a greater present value than the associated increase in taxes. Empirically, there are two common features of financing systems that assure incomplete rating for most employers. First, as noted above, assigned tax rates are bounded above and below so that taxes are completely insensitive to changes in layoff behavior in some sectors. In these cases, $e = 0$. Second, even for rated firms whose tax rates fall between the minimum and maximum in a state, where taxes are sensitive to layoffs, incremental tax liabilities are spread through time. Interest is not charged on these liabilities, so the degree of rating depends on the time profile of tax changes. Thus, even in cases where the full nominal value of benefits is eventually repaid, which turns out to be common, the timing of taxes yields an interest-free loan for which $0 < e < 1$.

The derivations required to completely characterize these dimensions of experience rating are contained in the Appendix. The results of this analysis may be summarized as follows. In a steady state, any employer's history of generating UI payments to workers is completely determined by his long-run equilibrium rate of insured unemployment, which I denote by $\mu$. If the employer's tax contributions per worker are to balance the flow of benefits generated by $\mu$, there must exist a tax rate, $\tau$, such that $\tau W = \mu B$, where $W$ is the taxable wage base per employee and $B$ is UI benefits expressed as an annual rate. Therefore, in order that $\mu$ be sustainable in this sense, $\tau$ must lie between the minimum and maximum rates charged in the employer's state. Letting $\rho = W/B$, this requirement can be expressed in terms of a pair of bounds on the equilibrium unemployment rate $\mu$:

$$\rho \tau_{\text{min}} \leq \mu \leq \rho \tau_{\text{max}}.$$  

If $\mu$ lies outside the range given in (1), the employer pays $\tau_{\text{min}}$ or $\tau_{\text{max}}$ independently of his layoff behavior, and so $e = 0$. Therefore, the bounds defined by (1) determine the range of equilibrium unemployment rates that are subject to experience rating within state systems. The relation of these bounds to the data in Table 2 on negative balance firms is obvious.

If $\mu$ lies within the range given in (1), tax dynamics in response to changes in an employer's layoff behavior determine the degree of experience rating. These dynamics are nontrivial, and they differ substantially across state systems, but their implications can be summarized by the shape of the cost function shown in Figure 1. In the illustrated case, which depicts experience rating in a typical state in 1975, rated firms paid about 80 cents in future taxes for each incremental dollar of benefits received by their workers. Nonrated firms with unemployment rates greater than $\mu_{\text{max}}$ or smaller than $\mu_{\text{min}}$ paid

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11 For a summary of these laws, see Becker (1972) and U.S. Department of Labor (1979).
12 The wage base chosen may not fall below a federally mandated level, which now stands at $6,000.
13 Equation (1) applies exactly in reserve-ratio systems, but it requires slight modification for benefit-ratio accounting. See the Appendix.
nothing. In some states, depending on the speed with which taxes are adjusted, this marginal cost to rated firms may actually exceed \$1 (\epsilon > 1) and, as shown, \epsilon need not be constant throughout the rated range.

Estimates of the bounds in (1) that are relevant for different state systems may be calculated from information on maximum and minimum tax rates, and on the levels of benefits and taxable wages in each state. Estimates of the degree of experience rating in each state can be similarly derived from the formulae used by each state to calculate taxes.\textsuperscript{14} Some values for both margins of experience rating are shown in Table 3 for illustrative state programs. These data show that both the range of unemployment over which experience rating is relevant and the degree of rating within that range differ substantially across state systems. Thus, for example, in 1975, experience rating for firms and their workers in Michigan was truncated above at an insured unemployment rate of 7 percent, while this upper bound occurred at only 3.8 percent in California. This means that for employers with equilibrium rates of, say, 6 percent, \textit{UI} would increase the value of unemployment relative to employment in California by the full amount of available benefits, while 86 percent of \textit{UI} would be repaid in Michigan. In terms of magnitudes of incentives, at a typical 1975 weekly benefit amount of \$90, that is a difference in costs of \$77 per week.\textsuperscript{15} Even within the range where the marginal cost of benefits is positive, a California firm would repay only 58 cents of each dollar received by an additional unemployed worker. At the other extreme, an identically situated firm in Texas would repay more than the value of benefits that workers receive, so that layoffs are actually taxed for rated employers.

By any standard, these differences in layoff incentives offered to otherwise identical firms are large. It is this type of variation in the two relevant margins of experience rating—its range and degree—that may be exploited in the cross section to estimate the impact of these incentives on layoff decisions.\textsuperscript{16} I turn to the empirical methodology for identifying these effects.

II. The Data and an Empirical Strategy

A. The CPS Data and the Basic Model

To estimate the impact of these differences in incentives on layoff unemployment, I utilize a sample of more than 8,000 individuals selected from the March 1975 file of the

\textsuperscript{14} Described in the Appendix.

\textsuperscript{15} These calculations ignore the additional effect on the subsidy caused by the tax-exempt nature of most UI payments. This omission is corrected in the econometric analysis.

\textsuperscript{16} For purposes of empirical analysis, I treat these differences in costs as exogenous and do not speculate on the political forces that may support them.
Simply stated, the empirical strategy links to each observation in the CPS information on the level of available UI benefits and the characteristics of experience rating that apply in an individual's state and industry. Using straightforward econometric techniques, it is then possible to decompose the effect of these variables on the probability that an individual is on layoff into component probabilities of transitioning to and from layoff unemployment. The particular virtue of the March CPS file for this purpose is its wealth of information on the personal characteristics and employment status of individuals, which includes retrospective information on earnings and employment during the calendar year that preceded the survey. This retrospective data is essential, since it is used to establish the level of UI benefits for which each individual qualifies. The relative incidence of temporary layoff unemployment was fairly high in 1975, which facilitates an analysis of the factors that influence layoff and rehire decisions. Hence my choice of that year.

To minimize the influence of vagaries in individual labor force participation decisions on the results, the sample includes only persons who were between the ages of 20 and 65, and who were full-time, full-year labor force participants during the previous year. In addition, to focus on persons who were subjective to layoff risks, I selected privately employed, nonprofessional workers who were not self-employed and who did not work in the agricultural sector. A final selection criterion was imposed by the data: the CPS reports "state" of residence for each observation, but smaller states are aggregated into larger geographic units to protect the confidentiality of responses. Since the identification of UI effects depends crucially on differences among states in methods of financing and liberality of benefits, I consider only individuals for whom state of residence could be exactly identified. The resulting sample of 8,280 individuals represented 19 reserve-ratio and benefit-ratio financing systems and 29 two-digit (SIC) industry classifications. Of these, 555 individuals (6.7 percent of the sample) were on temporary layoff. Since the economywide layoff rate was slightly above 3 percent in this year, it is clear that this sample faces greater layoff risk than the general population. No attempt is made here to extend the results to other, more heterogeneous populations or to those with weaker labor force attachment.

The estimation strategy uses two distinct pieces of information on an individual's labor market status to estimate the effects of UI, and other variables, on transitions to and from layoff unemployment: current status (for example, on temporary layoff) and, for unemployed workers, the duration of the current spell in progress. To use this information, certain assumptions on the processes that generate transitions are required. I assume that the underlying processes are stationary, and that the per period probabilities of entering and leaving layoff unemployment are constant, independent of time spent employed or unemployed. Denote these constant "hazards" by $\lambda_l^e$ and $\lambda_e^e$ for transitions from employment to layoff, and from layoff to employment, respectively. With this structure, the Appendix establishes that in a sample of individuals who are employed or on layoff, the contribution to the sample likelihood by individual $j$ who is on layoff at the survey date is

$$L_j = \frac{\lambda_l^e \lambda_e^e \exp[-T_j \lambda_l^e]}{\lambda_l^e + \lambda_e^e},$$

17The CPS is a monthly survey from which the government's employment and unemployment statistics are compiled. The survey is slightly different in each month, and only the March file contains information on employment and earnings during the previous year.
18A complete listing of selection criteria and sample characteristics may be obtained from the author.
19These included all manufacturing industries, plus construction, mining, wholesale trade, retail trade, and transportation industries.
20The stationarity and constant hazards assumptions imply that transitions are first-order Markov. Thus, the probability that a person is on layoff at the sample date is simply the proportion of time that an infinitely lived person in such an environment would spend unemployed. Formally, the hazard is the conditional probability of passage given duration $T$. Constancy implies that this probability is independent of $T$, and so the distribution of completed spell lengths is negative exponential. Further details are appended.
where $T_i$ is the duration of the layoff spell in progress. Equation (2) is simply the joint probability of observing an individual on layoff and in the $T$th week of his spell. In contrast, if the individual is employed then duration in employment is not observed, and so the contribution to the likelihood is simply

$$L_j = \lambda_j^e / \left[ \lambda_j^e + \lambda_j^u \right].$$

The transition rates $\lambda^e$ and $\lambda^u$ are the basic elements that we seek to identify in the estimation procedure. Intuitively, under the stated assumptions the probabilities that a randomly selected individual is employed or on layoff depend on both these transition rates, as in (3). The single piece of information given by the empirical frequency of layoff spells in the sample would then be inadequate to identify each of $\lambda^e$ and $\lambda^u$. However, the information given by the empirical distribution of durations, $T$, among unemployed workers allows us to estimate $\lambda^u$—the rate at which individuals leave unemployment—separately. This estimate may then be used, via (3), to identify $\lambda^e$. Estimating the two transition rates jointly from the full sample of individuals who are both employed and on layoff enhances efficiency.

To close the empirical model, I specify the dependence of $\lambda^u$ on a vector of explanatory variables, $X_j$, for the $j$th individual as

$$\lambda_j^u = \exp (X_j \beta^u),$$

and $\lambda^e$, the transition rate from employment, is written conformably. Equation (4) is non-negative, as required, for all values of the vectors $\beta$, which are to be estimated. The number of individual parameters to be estimated is twice the number of regressors in $X$. Thus, for example, $\beta^e$ measures the impact of exogenous variables on the probability of entering unemployment, while (3) implies that the total impact of $X$ on the probability of unemployment (the unemployment rate) is measured by the difference $\beta^e - \beta^u$. The likelihood defined by equations (2), (3), and (4) is globally concave, and so standard techniques are effective in maximizing it. Summary statistics for the variables included in $X$ are reported in Appendix Table A1, and are discussed below.

B. Specifying and Identifying the Effects of UI

In estimating this model, I focus on two UI variables as determinants of transitions, and thus of employment status at a point in time. These are the benefit-replacement ratio and the amount of benefits that are subsidized by methods of financing. The former variable is common in empirical work on the effects of UI, and in this specification it captures incentive effects that do not depend on imperfections in experience rating or the UI subsidy. Potential benefits were imputed for each observation from available personal information and from the qualifying provisions of each state's UI law, while the relevant marginal tax rates were calculated from state and federal tax tables based on family structure and on earnings during the previous calendar year. As Table A1 indicates, for the typical individual in this sample, available UI benefits would have replaced 56 percent of spendable weekly earnings during a spell of unemployment. Sources of variance in this variable both within and between state programs were illustrated in Table 1 above.

Imputation of the UI subsidy that is relevant for each individual is based directly on the cost functions described above. The subsidy to unemployment occurs both because of imperfect experience rating and because UI benefits are not normally subject to in-

21With knowledge of only the relative frequency of spells, the ratio $\lambda_j^u/\lambda_j^e$ could be identified from (3).

22Equations (3) and (4) imply that the unemployment probability is determined by a logistic distribution. Thus $\beta = \beta^e - \beta^u$ is the coefficient on $x$ in a standard logit model of unemployment incidence.

23The required information included weeks worked during the previous year, weekly earnings, and family structure (to calculate dependent's allowances). Given heterogeneity in state laws, the computer program for this imputation is fairly complex. It is available on request from the author.
income taxation. Thus, even with full experience rating \( e(\mu) = 1 \), unemployment would be subsidized by the different tax treatments of earnings and benefits. Combining these two incentives, the change in the value of unemployment induced by a weekly benefit amount of $b$ is simply

\[
(5) \quad b\left((1/1 - t) - e(\mu)\right),
\]

where \( t \) is the individual's marginal tax rate on earned income.\(^{24}\) The crucial element of (5) is \( e(\mu) \), so that precise imputation of the subsidy requires information on \( \mu \) for each individual's employer. Such detailed information on unemployment histories is clearly unavailable. In its place, I utilize time-series data on insured unemployment rates at the two-digit SIC level of aggregation,\(^ {25}\) and corresponding CPS information on the industry classification of each observation's most recent employer. I assume that in each industry the equilibrium insured unemployment rate is equal to the actual insured rate over the period 1971–74. Calling this rate in industry \( i \), \( \bar{\mu}_i \), the simplest imputation method would assign \( e_s(\bar{\mu}_i) \) for individuals in state \( s \). With slight modification to account for within-industry variation in equilibrium rates among employers, this method was used to impute the degree of experience rating in the 551 state-industry cells.

The modification simply recognizes that, within any industry, equilibrium rates will be distributed about the industry mean. Consequently, even in industries where \( \bar{\mu}_i \) lies outside a state's experience rated range defined in (1), experience rating may affect layoff decisions for some firms and conversely for industries where \( \bar{\mu}_i \) is within the rated interval. There is no information on the form of these distributions, and so any allowance for this type of effect must be somewhat crude. Thus, since the potential for within-industry variance is larger in industries with high average unemployment, I assume an industry density for \( \mu, f_i(\mu) \), that is triangular with the range of variation in \( \mu \) proportional to the mean.\(^ {26}\) The factor of proportionality \( \gamma \) is the same in all industries, and so the assumed range of \( \mu \) is equal to \( \bar{\mu}_i \pm \gamma \bar{\mu}_i \) in the \( i \)th industry. The expected degree of experience rating for person \( j \) in industry \( i \) may then be imputed using the density \( f_i(\mu) \) and the cost function \( e(\mu) \) in each state program. The parameter \( \gamma \) may be chosen on the basis of the model's overall fit, and a value of \( \gamma > 0 \) indicates effects of experience rating even where \( \bar{\mu}_i > \mu_{\text{max}} \). Of course, because of the form of \( f(\mu) \), this effect is constrained to die out the farther is \( \bar{\mu}_i \) from the experience rated range.

The fact remains that industries with extremely high or low average unemployment will have less imputed experience rating—since a larger proportion of \( f_i(\mu) \) would lie outside the range defined in (1)—and this may seriously bias the results in favor of powerful experience rating effects.\(^ {27}\) This source of bias may be purged by estimating the model with vectors of fixed industry effects in the specifications of both \( \lambda^{el} \) and \( \lambda^{le} \). That is, I assume

\[
(6) \quad x_j\beta^{el} = \alpha^{el}_i + Z_j\theta^{el},
\]

\[
\text{and} \quad x_j\beta^{le} = \alpha^{le}_i + Z_j\theta^{le},
\]

\(^{26}\)The density is of the form

\[
f_i(\mu) = \begin{cases} 
(1 + (\mu - \bar{\mu}_i))/(\gamma \bar{\mu}_i)^2 & \mu < \bar{\mu}_i; \\
(1 - (\mu - \bar{\mu}_i))/(\gamma \bar{\mu}_i)^2 & \mu > \bar{\mu}_i.
\end{cases}
\]

\(^{27}\)Table 5, below, makes this especially clear. Industries such as Apparel and Miscellaneous Manufacturing have relatively high values of \( \bar{\mu}_i \), high sample layoff rates, and large imputed subsidies. Even if the subsidy did not affect layoffs, this relationship would almost surely yield a positive estimated effect of the subsidy on unemployment. The strategy indicated in the text simply subtracts these cross-industry effects, and concentrates solely on variation in the subsidy within industries.
where the $\alpha_i$ represent industry-specific shifters of the transition rates. In this specification, differences in levels of exogenous variables across industries are captured by the $\alpha_i$, so only within-industry variation in $Z_i$ is used in estimating $\theta^e$ and $\theta^{le}$.

Since $\overline{\mu}_j$ is the same for all persons in industry $i$, within-industry variation in the degree of experience rating is caused only by differences between states in the location and height of the cost function, $e(\mu)$. Therefore, any estimated impact of the UI subsidy reflects only the within-industry experiment of changing the function $e(\mu)$, while differences between industries in average levels of the subsidy and unemployment do not affect the results.

III. The Evidence

All of the specifications that I have estimated show strong effects of unemployment insurance on layoff unemployment. More importantly, the evidence is that most of UI's impact on layoffs is caused by current methods of experience rating. In this sample, the layoff unemployment rate would have been about 30 percent lower if the subsidy to unemployment caused by the current UI system had been eliminated. In contrast, simple changes in the level of UI benefits that leave the subsidy constant would have only minor effects on layoffs.

Table 4 summarizes the main results for the determinants of temporary layoff unemployment. In the table, I report two specifications of the model, the first of which ignores any information on experience rating. Thus, all UI effects in this specification are captured by the replacement ratio, and the impact of this variable will reflect the average degree of UI subsidization in addition to any independent effects of changing the level of available UI. The estimates in the first specification show a strong positive impact of UI on unemployment, and the dominant share of this effect is due to a statistically significant increase in the probability of experiencing a layoff, rather than to increased duration of spells. To fix ideas on the magnitudes of these effects, evaluating the model at sample means yields an estimated monthly probability of entering unemployment of .029. At means, the point estimate of the ratio's impact on this transition implies that the per period layoff probability is unit elastic with respect to benefits $(1.82 \times .56 = 1.02)$, so a 10 percent across-the-board reduction in the level of $UI$ (about $8 per week) would reduce the monthly probability of entering layoff by about 0.3 percentage points. At the other margin, however, there is no significant effect of the replacement ratio on the probability of leaving layoff.

Overall, the point estimate of $UI$'s total effect in the third column (1.70) implies that a 10 percent reduction in the level of benefits would eliminate about 8.8 percent of all layoff spells in this sample. I have experimented with various nonlinear forms for the effects of the ratio (for example, splines and quadratics), but these findings were not affected by such changes in specification. In short, the estimates in the first specification indicate that the incidence of temporary layoff unemployment is quite sensitive to the availability of unemployment insurance, and that transitions into layoff unemployment play a prominent role in explaining this relationship.

The effects of other exogenous variables in model A are largely self-explanatory. Recall that the specification controls for differences caused by the characteristics of particular industries, so the estimates reflect within-industry changes in exogenous variables. For example, since the usual statistical determinants of earnings are controlled for in the

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28 Since there are 29 industries represented in the data, the model estimates 58 industry coefficients in addition to the other parameters of the model. In practice, this brought to 78 the number of parameters in the estimated models. The procedure for estimating the fixed effects in a nonlinear model like this one is considerably more complicated than simply subtracting out industry means and it is expensive to compute.

29 This total impact of $UI$ is similar to that of Feldstein (1978), who regressed layoff unemployment on the replacement ratio. Feldstein's data were for a different year (1971) and a more heterogeneous sample, which makes comparisons difficult. Note that Feldstein's OLS method of estimating the probability of a layoff is not capable of identifying separate effects on transitions.
Table 4—Unemployment Insurance and Layoff Unemployment

<table>
<thead>
<tr>
<th>Exogenous Variables: Z_j</th>
<th>Specification A</th>
<th>Specification B</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Entering Layoff^a</td>
<td>Leaving Layoff^b</td>
</tr>
<tr>
<td>Subsidy</td>
<td>.683 (1.35)</td>
<td>- .402 (1.05)</td>
</tr>
<tr>
<td>Replace Ratio</td>
<td>1.82 (2.66)</td>
<td>.117 (2.41)</td>
</tr>
<tr>
<td>Weekly Wage x 100</td>
<td>.047 (4.04)</td>
<td>.065 (7.94)</td>
</tr>
<tr>
<td>Age</td>
<td>- .058 (-1.52)</td>
<td>.034 (1.27)</td>
</tr>
<tr>
<td>Age^2 / 100</td>
<td>.036 (.761)</td>
<td>- .047 (1.39)</td>
</tr>
<tr>
<td>Age = 25^e</td>
<td>- .040 (-2.67)</td>
<td>.011 (1.03)</td>
</tr>
<tr>
<td>Age = 40^e</td>
<td>- .029 (-5.25)</td>
<td>- .003 (-.829)</td>
</tr>
<tr>
<td>Education</td>
<td>.050 (-.428)</td>
<td>- .068 (-.890)</td>
</tr>
<tr>
<td>Education^2 / 100</td>
<td>- .066 (-.119)</td>
<td>.045 (1.08)</td>
</tr>
<tr>
<td>Education = 12^e</td>
<td>- .066 (-1.96)</td>
<td>.027 (1.15)</td>
</tr>
<tr>
<td>Race (White)</td>
<td>- .092 (-4.91)</td>
<td>.044 (3.88)</td>
</tr>
<tr>
<td>Sex (Male)</td>
<td>.177 (1.00)</td>
<td>.224 (1.79)</td>
</tr>
<tr>
<td>Industry Effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>ln 2</td>
<td>-3636.5</td>
<td></td>
</tr>
<tr>
<td>Proportion Unemployed</td>
<td>.071</td>
<td></td>
</tr>
</tbody>
</table>

Note: Asymptotic normal statistics in parentheses.

^a Estimate of \( \theta^{e1} \).

^b Estimate of \( \theta^{e2} \).

^c Estimate of \( \theta^{e1} - \theta^{e2} \).

^d Derivative of unemployment probability at sample mean.

^e Age and education quadratics evaluated at the indicated level.

specification, the coefficient on the weekly wage might be interpreted as the effect of unobserved components of individual or job-specific productivity. There is no evidence in these data that unemployed workers are materially different in this respect than their employed counterparts, nor are the individual transition rates significantly affected by earnings capacity. This is not too surprising, since most workers on layoff will end up returning to their old jobs. The effects of age and education, however, are strong. On average, a 25-year old is about 50 percent more likely to be unemployed than a 40-year old (wage constant), and an extra year of schooling reduces unemployment by over half a point. The age profile of unemployment probabilities is nonlinear, becoming flatter at older ages. As with the impact of the replacement ratio, these effects are
concentrated on the probability of entering layoff. Finally, there are no significant effects of race or sex on layoff unemployment rates, although men have slightly shorter spells than women. These results are not sensitive to changes in the specification of UI effects, and so they will not be referred to further below.

The imputed UI subsidy to unemployment is controlled for in specification B. It turned out that the dispersion parameter \( \gamma \) was quite costly to estimate directly in the maximum likelihood procedure, though in experiments with the imputation the data preferred a value of \( \gamma = 0.25 \). This value implies a within-industry standard deviation in \( \mu \) of only about 15 percent of the mean, and the results are not statistically different from those obtained when \( \gamma = 0 \). Thus, there is little evidence that within-industry dispersion of \( \mu \) is an important consideration in evaluating the incentive effects of experience rating. The direct impact of imputed experience rating on layoff unemployment, however, is both statistically and numerically significant.

In the specification, I have entered the subsidy as a proportion of weekly earnings so that the imputed degree of rating, \( e(\mu) \), is the only source of independent variation in the subsidy relative to the replacement ratio. In light of the powerful effect of the replacement ratio estimated in the first model, this restriction offers a strong test of the role of experience rating in affecting layoff decisions, and serves to divide the total impact of UI between its subsidized and nonsubsidized components. Thus, note that when the subsidy is controlled for the estimated impact of the replacement ratio falls by over 60 percent relative to model A, and it is no longer significant by standard criteria. At means, the new point estimate of .603 implies that a 10 percent change in the benefit level that holds the subsidy constant would change the layoff probability by twenths of a percentage point. In contrast, the UI subsidy has a powerful and statistically significant effect on the layoff unemployment rate. Since the average subsidy is equal to 31 percent of the weekly wage, the point estimate in the last column implies that the subsidy accounts for (approximately) .072 \times .31 / .071 = 31 percent of the sample's layoffs. In other words, if the incentives toward increased unemployment offered by current methods of experience rating and income taxation were entirely eliminated, the average layoff unemployment rate in this sample would decline by more than one fourth.

The data and discussion of Section I imply that current methods of financing UI will have a larger impact on layoffs in high unemployment industries, where the subsidy to unemployment is larger and experience rating less relevant to layoff decisions. Table 5 illustrates this distributional effect, showing the predicted amount of the layoff unemployment rate that is accounted for by the subsidy in selected industries. Thus, for example, in the high unemployment Apparel industry (\( \bar{\mu} = 7.65 \) percent), the subsidy is equal to about 56 percent of weekly earnings, on average, and so it accounts for over 6.7 points of the industry layoff rate in this year. In contrast, in Primary Metals, average un-

\[ 30 \] This finding is consistent with that of Robert Hall (1972) and others, who found that differences in the duration of unemployment across groups are of minor significance in explaining differences in unemployment rates. That this also holds for temporary layoffs is somewhat more surprising, given the symmetry of layoff and rehire decisions.

\[ 31 \] Note that the worker's union status is not an explanatory variable. This is because union membership is not reported in the March file (except in 1971). However, any differences across industries in unionization rates are captured by the industry effects, \( \alpha_i \). It is therefore highly doubtful that the omission of union status could strongly affect the results.

\[ 32 \] Since the replacement ratio is \( b/w(1 - r) \), the subsidy as a proportion of weekly earnings multiplies the ratio by the quantity \( 1 - e(\mu)(1 - r) \). This is an interaction term, and the results imply that most of the effect of UI comes through this interaction with experience rating.

\[ 33 \] The point estimate of 1.01 in the transition to layoff is still numerically large, but it is imprecisely estimated and is partially offset by a positive and insignificant estimated effect on transitions from layoff. Note that at the mean of the subsidy (.31), the effect of the same 10 percent change in benefits is also about .2 points (.072 \times .31 / .1 = .0022), so that the total effect is almost evenly divided. When local market conditions are controlled for in Table 6, however, the share of this effect shifts strongly to the subsidy.
employment is much lower ($\bar{\mu} = 2.43$ percent) and experience rating more extensive, so the mean imputed subsidy accounts for only 1.7 points of total layoff unemployment.

The decomposition of the total impact of the subsidy into its component effects on probabilities of entering and leaving layoff also shows the expected signs, though the point estimates are only moderately larger than their standard errors. Using the point estimates, 62 percent of the total effect on unemployment is due to increased transitions from employment to layoff (the layoff probability is 20 percent higher), while the point estimate of $-0.402$ in layoff transitions implies that the mean level of the subsidy increases the average duration of layoffs by about 1.3 weeks. Thus, though the estimates from the decomposition are imprecise, there is evidence in these data that the significant impact of the subsidy on layoff unemployment is due to both more frequent and longer layoff spells. And as in Table 5, these effects will be nonneutral across industries, being larger in poorly experience rated sectors.

Changes in the reported specification that excluded various combinations of demographic characteristics did not materially affect the results, nor did restrictions of the sample to men only, or to white men only. However, one change in the basic specification does affect the estimated impact of unemployment insurance. Recall that the results shown in Table 4 rely heavily on between-state differences in the liberality of benefits, and the extent of experience rating to estimate incentive effects of UI. Thus, to the extent that there are important differences across states in “local” market conditions affecting layoff, rehire, and search decisions, these results may be biased.34 For example, rapidly growing states such as Texas and Florida are reputed to be “strong” local markets, and the data of Tables 1 and 3 reveal relatively low benefit levels and strong experience rating in those states. To control for these long-term differences among areas, I have reestimated the model in Table 5 including as an explanatory variable the imputed growth rates of private, nonagricultural employment in each individual’s state. These rates were estimated from quadratic trend regressions for each state, and the re-

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34 I am indebted to Bob King for raising this issue.
TABLE 6—ESTIMATED EFFECTS OF UNEMPLOYMENT INSURANCE AND STATE GROWTH OF LAYOFF UNEMPLOYMENT

<table>
<thead>
<tr>
<th>Exogenous Variables: Z_j</th>
<th>Entering Layoff^a</th>
<th>Leaving Layoff^b</th>
<th>Total Effect^c</th>
<th>Entering Layoff^a</th>
<th>Leaving Layoff^b</th>
<th>Total Effect^c</th>
</tr>
</thead>
<tbody>
<tr>
<td>Subsidy</td>
<td>.612</td>
<td>-.396</td>
<td>1.007</td>
<td>.874</td>
<td>-.219</td>
<td>1.093</td>
</tr>
<tr>
<td></td>
<td>(1.21)</td>
<td>(-1.04)</td>
<td>(3.01)</td>
<td>(1.98)</td>
<td>(.649)</td>
<td>(3.82)</td>
</tr>
<tr>
<td>Replacement Ratio</td>
<td>.861</td>
<td>.559</td>
<td>.302</td>
<td>-.519</td>
<td>.141</td>
<td>-.660</td>
</tr>
<tr>
<td></td>
<td>(1.01)</td>
<td>(.949)</td>
<td>(.491)</td>
<td>(-1.50)</td>
<td>(0.32)</td>
<td>(-2.45)</td>
</tr>
<tr>
<td>Growth Rate</td>
<td>-.426</td>
<td>.205</td>
<td>-.631</td>
<td>(1.18)</td>
<td>(.874)</td>
<td>(-2.28)</td>
</tr>
<tr>
<td></td>
<td>(-1.18)</td>
<td>(.874)</td>
<td>(-2.28)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ln c</td>
<td>-3926.2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Asymptotic normal statistics in parentheses. Other explanatory variables are as in Table 4.

^a Estimate of \( \theta^{el} \).
^b Estimate of \( \theta^{le} \).
^c Estimate of \( \theta^{el} - \theta^{le} \).

Results of including them are summarized in Table 6.35

Even after controlling for industry composition, rapidly growing states have fewer layoffs, and the majority of this effect is again attributable to the probability of entering unemployment. At means, a one percentage point increase in long-term growth (less than a standard deviation) reduces the layoff rate by about .5 points. Most importantly, however, controlling for local growth reduces the impact of the replacement ratio by about 50 percent from the estimate in Table 4. In fact, the impact of the ratio is now negligible: a 10 percent change in the benefit level that leaves the subsidy constant would change the layoff unemployment rate by only one-tenth of a point, though the standard error on this estimate is large.36 The effects of the unemployment insurance subsidy, however, are not materially different than in Table 4, and its total effect on unemployment remains highly significant. For completeness, I also report in the table the estimated effect of the UI subsidy when the replacement ratio is excluded. Again, the results are not highly sensitive to this exclusion, though the impact on transitions into unemployment is somewhat larger in this case. I conclude from this evidence that the impact of UI is overstated when local market conditions are ignored, but this bias falls most on the replacement ratio. Methods of financing UI, in contrast, continue to have strong and stable effects on layoffs that are consistent with theory.

IV. Summary and Concluding Remarks

The vast majority of employer-initiated spells of unemployment involve the payment of unemployment insurance benefits. This empirical connection between layoff decisions and the UI system may affect observed unemployment rates for two reasons. First, the legislated level of benefits that must be paid to laid off workers can change the relative costs of unemployment to both workers and their employers, and this alone may influence layoff, rehire, and search decisions. Second—and this has been the main focus of this paper—current systems of UI financing clearly subsidize the occurrence of unemployment, since most employers are only partially liable for the benefits that their
workers receive. Largely for lack of suitable measures of the incentives offered to firms, however, these effects of unemployment insurance on the decisions that generate unemployment have been previously untested.

The empirical evidence presented in this paper indicates that both the extent and the effect of UI subsidization are important. Examination of the structures of state financing laws shows that even experience-rated firms often face significant unemployment subsidies, and an important portion of aggregate employment is not subject to experience rating at all. The econometric analysis showed that the impact of the unemployment insurance subsidy on layoff unemployment is powerful—the imputed subsidy accounts for more than a quarter of all layoffs in the data—and the effects on the probabilities of entering and leaving unemployment are in accord with theory. For workers who are subject to temporary layoff, UI appears to have a more important effect on transitions into unemployment than on the more commonly studied duration of spells. Taken together, these findings offer strong support for the hypothesis, originally proposed by Feldstein (1973), that methods of financing unemployment insurance can have important effects on the incidence of unemployment.

These results imply that, without changing benefit levels available to unemployed workers, a significant reduction in layoff unemployment could be achieved by changing the incentives offered by current UI laws. It is tempting to conclude from these findings that subsidies to unemployment should be eliminated via complete experience rating of UI taxes (full employer liability) and the symmetric tax treatment of benefits and earned income. My analysis does not justify that conclusion, however, since very little is known about the optimal structure of UI financing systems. For example, under circumstances where labor turnover may be viewed as a public good (see Hall, 1979, and Peter Diamond, 1981), firms (and workers) will undervalue separations and so complete experience rating of UI may inefficiently discourage permanent layoffs. These arguments are far less applicable for temporary layoffs, however, and they cannot be used to justify the current structure of experience rating where many layoff-prone employers face no marginal cost of benefits at all. On these grounds, the empirical effects identified in this paper can be viewed as costs of the current organization of unemployment insurance financing in the United States.

**APPENDIX**

**A. Calculating the Degree of Experience Rating**

1. **Benefit-Ratio Systems.** In benefit-ratio systems, an employer’s tax rate depends on the ratio of total benefits charged to the employer’s account over the past $T$ years to total taxable wages for the same period. This is the benefit ratio:

$$BR = \frac{\sum_{j=1}^{T} B_{t-j}N_{t-j}}{\sum_{j=1}^{T} WN_{t-j}},$$

or

$$BR = \rho \sum_{j=1}^{T} n_{t-j} \mu_{t-j},$$

where $\rho = B/W$ and $n_{t-j} = N_{t-j}/\sum N_{t-k}$ is the share of year $t-j$ employment in total employment over the past $T$ years. Thus, the benefit ratio is just a share-weighted average of past unemployment rates times the “charge rate,” $\rho$. In some benefit-ratio states, the firm’s tax rate is just $\tau_i = BR_i$, but, in others, $BR_i$ is multiplied by a factor of proportionality, $\lambda$, equal to the ratio of total state benefit payments to those which are charged to firm accounts. Thus $\tau_i = \lambda BR_i$, with $\lambda \geq 1$. The present value of taxes caused by a transitory change in $\mu$ relative to benefits received may then be calculated to be

$$e = \lambda \left(1-(1+i)^{-T}\right)/T_i,$$

where $i$ is the rate of interest. This marginal cost of benefits is less than unity (for $\lambda = 1$) and is declining in $T$ because the implicit interest-free loan is repaid over a longer period when $T$ rises. In most states, $T = 3$ years, while the currently operating system in Michigan sets $T = 5$ years. Thus, with $T = 3$ and $i = .1$, $e = .828\lambda$ while with $T = 5e = .758\lambda$. 

...
2. Reserve-Ratio Systems. Under this method of accounting, each employer's tax rate, \( \tau \), depends on the ratio of total funds in its account to its total taxable payroll—the reserve ratio. If \( R_t \) is total reserves credited to the employer's account in year \( t \), \( W \) the taxable wage base per employee, and \( N \) the total number of employees, then the reserve ratio is \( r_t = R_t / WN \). Assuming \( WN \) is approximately constant (most state programs use weighted averages over several years), \( r_t \) follows

\[
r_{t+1} = r_t - \rho \mu_r + \tau_t - \rho \mu_r,
\]

where \( \rho = B/W \).

Between the maximum and minimum rates, \( \tau_t \) is defined as a step function of \( r_t \). When these steps are small, the function may be treated as approximately linear: \( \tau = \eta_0 - \eta_1 r \). Then taxes follow the difference equation

\[
\tau_{t+1} W = \tilde{r}_{t+1} = (1 - \eta_1) \tilde{r}_t + \eta_1 B \mu_r.
\]

Using this equation, a current increment to \( \mu_r \) generates future taxes worth \( B \eta_1/(\eta_1 + i) \) where \( i \) is the rate of interest. Thus, dividing by the value of benefits received, \( e = \eta_1/(\eta_1 + i) \). In a typical system, \( \eta_1 = .3 \) so with \( i = .1, e = .75 \).

For a large step in the tax function at ratio \( \hat{r} \), the linear approximation is less appropriate. In general, the step may be characterized as \( \tau = \tau_1 \) for \( r < \hat{r} \) and \( \tau = \tau_0 \) for \( r > \hat{r} \). The tax rate that would support a steady-state value of \( \mu \) is \( \tau^* = \mu \tau_0 + \phi (\tau_1 - \tau_0) \) where \( 0 < \phi < 1 \). Thus, when \( \tau = \tau_0 \), reserves decline at rate

\[
r_t - r_{t-1} = \tau_0 - \rho \mu_r = -\phi (\tau_1 - \tau_0) = -\phi \Delta \tau.
\]

Conversely, if \( \tau = \tau_1 \), reserves accumulate at rate

\[
r_t - r_{t-1} = \tau_1 - \rho \mu_r = (1 - \phi)\Delta \tau.
\]

Now the tax authority sets \( \tau \) on the basis of \( r \) at annual evaluations. A firm with \( \tau_0 < \tau^* < \tau_1 \) will find that its taxes alternate between \( \tau_0 \) and \( \tau_1 \) as its reserve ratio is above or below \( \hat{r} \) at the evaluation dates. Define the beginning of such a cycle, \( i \), as the first instant where \( r_i \) crosses \( \hat{r} \) from above, and let \( q \) be the proportion of a year remaining from \( i \) to an evaluation. Assuming \( \phi < .5 \) (derivations for \( \phi > .5 \) are symmetrical), reserves decline to \( \hat{r} - \Delta \tau \phi \alpha \) at the first evaluation, they rise for one year to \( \hat{r} + \Delta \tau (1 - \phi (1 + \alpha) \), and then decline to \( \hat{r} \) after \( \phi^{-1} - (1 + \alpha) \) periods. Thus, the length of a cycle is \( \hat{\phi}^{-1} \), of which \( \phi^{-1} - 1 \) periods have a tax of \( \tau_0 \) and one has a tax of \( \tau_1 \). As of the first evaluation, the present value of the surcharge \( \Delta \tau \) for one period is \( \Delta \tau (1 - e^{-i})/i \). Assuming that \( \alpha \) is uniformly distributed on the unit interval, the expected present value of the tax surcharge as of the period \( i \) is

\[
\int_0^1 \Delta \tau (1 - e^{-i}) e^{-i \alpha} d\alpha = \Delta \tau \left( \frac{1 - e^{-i}}{i} \right)^2.
\]

Since this surcharge occurs every \( \hat{\phi}^{-1} \) periods, the present value of the firm's future tax rate is

\[
\frac{\tau_0 + \Delta \tau (1 - e^{-i})^2 + \Delta \tau \left( \frac{1 - e^{-i}}{i} \right) e^{-i/\phi}}{1 - e^{-i/\phi}}.
\]

Now consider a change in unemployment that lasts exactly one period and generates change in reserves \( dr = -\rho d\mu \). This change implies that the end of the cycle occurs \( dr/\phi \Delta \tau \) periods sooner, and so the change in the present value of taxes as measured from the point where the cycle would have ended is

\[
\beta = \Delta \tau \left( \frac{1 - e^{-i}}{i} \right)^2 \left( \frac{\exp (i dr/\phi \Delta \tau) - 1}{1 - e^{i \phi^{-1}}} \right).
\]

Discounting this value to the period \( t_0 \), where the shock commences, and assuming that \( t_0 \) is uniformly distributed on \( (0, \phi^{-1}) \), that is, that layoffs can begin at any time, yields an expected present value of the tax increment of

\[
(2) \quad \frac{\beta}{i} = \left( \frac{1 - e^{-i}}{i} \right)^2 \left( \frac{\exp (i dr/\phi \Delta \tau) - 1}{1 - e^{i \phi^{-1}}} \right) \phi \Delta \tau.
\]

The change in benefits received per unit of taxable payroll is just \( dr \), so dividing (2) by \( dr \) and letting \( x = (\phi \Delta \tau / i dr)^{-1} \), we have an expected marginal cost of benefits of \( ((1 - e^{-i/i})^2/(e^x - 1)/x \). Now \( x \to 0 \) as \( dr \to 0 \), and so as increments to unemployment become small this marginal cost approaches
Therefore, the reserve-ratio system generates approximately a one-year interest-free loan for UI payments made by firms located in large steps of the tax function. Consequently, the marginal cost of benefits in reserve-ratio states jumps from \( \eta_1/(\eta_1 + i) \) to \( 1/(1 + i) \) for equilibrium unemployment rates in the range \( (\tau_0/\rho, \tau_1/\rho) \).

**B. The Likelihood Function**

In a sample of full-time full-year participants, individuals may be categorized among three labor force "states" as of the sample period, \( t \). These are employment (\( e \)), on temporary layoff (\( l \)), and unemployed without prospect of rehire (\( d \)). Over time, individuals move among these states, and I assume that in a stationary environment the cumulative distribution of completed spell lengths \( \tau \) in state \( i \) is \( F^i(\tau) \). The conditional density of incomplete spell lengths among workers in state \( i \) at the survey date is then

\[
(A2) \quad h^i(T) = \frac{\nu^i(t - T)}{V^i(t)}(1 - F^i(T)),
\]

where \( \nu^i(t - T) \) is the probability that a spell commenced at \( t - T \) and \( V^i(t) \) is the probability of observing an individual in \( i \). The probability of observing the \( T \)th week of a spell of type \( i \) is then simply \( h^i(T) V^i(t) \).

Now if transitions are first-order Markov, a well known result from renewal theory (D. R. Cox, 1962) states that the distribution functions \( F^i(\tau) \) must be exponential. (For a more general analysis, see Chris Flinn and James Heckman, 1980.) The conditional exit-time densities given elapsed time in the state—the hazard functions—are constants given by \( f^i(\tau)/(1 - F^i(\tau)) \). Denote these transition rates by \( \lambda^e, \lambda^d, \lambda^l, \lambda^e, \lambda^d \). I assume that \( \lambda^d = x^d = 0 \), so that permanent layoffs cannot become temporary and conversely.

Straightforward calculation shows that the steady-state probability of observing randomly selected individual \( a \) in state \( e \), employment, is simply

\[
(A3) \quad \phi^e(t) = \frac{\lambda^e}{\lambda^e + \lambda^d + \lambda^l \phi^l(t) / \lambda^d},
\]

which is independent of \( t \). Using this, we have, for example, \( \nu^i(t - \tau) = \lambda^e \phi^e(t) \). Therefore, the probabilities of observing the

| Table A1 — Variable Definitions and Summary Statistics, March 1975 CPS Sample |
|-----------------------------|-----------------------------|-----------------------------|
| Variable                   | Definition                                | Mean | Standard Deviation |
| Subsidy                    | Total UI subsidy as proportion of weekly earnings (imputed) | .31  | .19               |
| Replacement Ratio          | Potential UI benefits as proportion of weekly after-tax earnings (imputed) | .56  | .15               |
| Growth Rate<sup>a</sup>    | Predicted rate of growth of state, private, non-agricultural employment from auxiliary regressions | 1.27 | 1.86              |
| Weekly Wage                | Annual earnings ÷ weeks worked last year  | 202.49 | 98.02         |
| Age                        | Age in years                             | 39.50 | 12.60            |
| Education                  | Years of completed schooling             | 12.20 | 2.50             |
| Sex                        | = 1 if individual is male, = 0 otherwise | .73  | .45               |
| Race                       | = 1 if individual is white, = 0 otherwise | .90  | .31               |
| Layoffs<sup>a</sup>        | Duration of a temporary layoff spell in progress (weeks) | 10.50 | 7.50             |
| Layoffs<sup>a</sup>        | = 1 if worker on temporary layoff, = 0 otherwise | 7.11  | 25.70            |
| Observations               |                                         | 7,806 |                  |

Note: Original sample included 8,280 observations. The estimation procedure conditions on the sample of workers who are on layoff or employed, so discharges (435) and quits (39) are deleted from the final sample.

<sup>a</sup>Growth Rate and Layoffs are shown in percent.
The 5th week of a layoff and discharge spell are, respectively,

\[ \phi(t, T) = \phi(t) \lambda^e \exp(-T \lambda^e), \]

(A4) \[ \phi^d(t, T) = \phi(t) \lambda^d \exp(-T \lambda^d). \]

(A5) These depend on all four hazards, and so estimation of the full model involves a number of parameters equal to four times the number of regressors. We may economize to analyze passages to and from temporary layoff, however, by conditioning the sample on those individuals who are not in state $d$. Using (A3) and (A4) this reduces the number of unknowns to $\lambda^e$ and $\lambda^e$, and results in equations (2) and (3) of the text. This sample restriction reduced the sample size to 7,806 from 8,280.

Summary statistics for these data used in estimating this model are shown in Table A1.

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