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ABSTRACT

Despite clear theoretical predictions of UI effects on takeup there is little work on the link between program generosity and the propensity to file for benefits. Administrative data allow us to assign the potential level and duration of benefits accurately for a sample of workers separating from their employers, whether or not UI was ever actually received. We then use these values along with marginal tax rates as our main explanatory variables in logit equation estimates of the probability that a separating employee receives UI. We find a strong positive effect of the benefit level on takeup, but little effect of the potential duration of benefits. The estimates imply elasticities of the takeup rate with respect to benefits of about 0.46 to 0.78. Our estimates also show that potential claimants respond to the tax treatment of benefits. Simulations of the effects of taxing UI benefits indicate that recent tax changes can account for most of the decline in UI receipt in the 1980's.

In addition, we find theoretical and empirical support for the proposition that those with short unemployment spells are less likely to file. We show that if the decision to file for UI is affected by benefit levels and the expected duration of unemployment, it will bias estimates of the effects of UI on unemployment duration.

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1. Introduction

There is an extensive empirical literature on the effect of unemployment insurance (UI) on the duration of unemployment. However, there is very little work on the effect of UI program generosity on the propensity to file for benefits given a job separation. We believe that despite clear theoretical predictions of UI effects on this takeup decision and a policy interest in their magnitude, the question has not been thoroughly examined due to a previous lack of appropriate data. This paper seeks to fill a gap in the literature by estimating the effects of UI benefits on the probability of receipt, given a separation from a former employer.

Theoretical arguments suggest that the generosity of benefits should affect the takeup rate. More generous UI benefits increase the value of applying for UI relative to its costs. Theoretical arguments also suggest that the generosity of UI should affect search on the job and thus affect the probability of finding a new job following notice of a job termination, but prior to an actual separation. Despite predictions such as these, though, there has been little examination of the effect of benefits on transitions to unemployment or on takeup rates. This is true despite extensive work on takeup of other social insurance programs such as AFDC, Food Stamps and Workers' Compensation. Additionally, survey estimates indicate that takeup rates for UI are substantially below one, with the range of estimates in the literature for the fraction of eligibles receiving UI ranging from 0.55 to 0.83.[1] One of the main difficulties in examining UI takeup, however, is the difficulty of determining eligibility in most data sets.

The degree of empirical strength behind the theoretical predictions on unemployment and takeup effects also has important implications for the design of unemployment insurance, since

[1]The 0.55 estimate comes from a special Current Population Survey supplement reported in Vroman (1991), while the 0.83 estimate is for Panel Study of Income Dynamics household heads reported in Blank and Card (1991).

the disincentive effect of UI may not be limited to its effect on durations of unemployment. There may be both a takeup effect of higher benefits on the currently unemployed if more unemployed now register for UI, and an unemployment effect if more people enter unemployment following a separation.[2] Understanding the magnitude of these effects is crucial when evaluating how changes in the UI system will affect program costs.

The effect of benefits on transitions to unemployment and on takeup is important for other reasons as well. Recently, there has been much discussion of the decline in the fraction of the unemployed receiving UI, with several authors attributing this decline primarily to a decline in takeup.[3] Thus, estimating the determinants of takeup is essential to more fully understand this apparent decline. Furthermore, during the period of declining claims the largest cut ever in the value of UI benefits took place as the taxation of benefits was phased in. Estimating the effects of benefits and their taxation on takeup is thus an important element in understanding recent trends.

The effect of benefits on transitions to unemployment and on takeup is also important for the interpretation of the recent UI reemployment bonus experiments.[4] One aspect of these programs was to increase the financial reward to initially filing for UI and then finding a job quickly. Thus, knowledge of the sensitivity of the number of claims to an increase in the rewards to filing is an essential element in evaluating these proposals.

Lastly, if we believe that the decision to file for UI benefits is affected by UI benefit levels and the expected duration of unemployment, it is likely that all current estimates of the effects of

[2]There may also be a further unemployment effect if UI benefits increase the duration of unemployment for those now induced to apply.

[3]In particular, see Blank and Card (1991) and Corson and Nicholson (1988), as well as a discussion of earlier trends in Burtless (1983).

[4]See Meyer (1994) for a summary of the UI bonus experiments.

UI on unemployment duration are biased. The intuition for this result is that once takeup is endogenous it leads to a correlation between UI generosity and expected spell duration in the sample of recipients. The last section of the paper provides a simple model which illustrates this result. Note that it is also likely that this result is applicable to other programs such as AFDC and workers' compensation.

In this paper, we make use of a unique data set to estimate the effects of UI program generosity on the takeup of benefits, conditional on a separation. Administrative data allow us to assign the potential level and duration of benefits quite accurately for a sample of workers separating from their employers, whether or not UI was ever actually received. We then use these values along with marginal tax rates as our main explanatory variables in logit equation estimates of the probability that the separating employee actually receives UI. We find a strong positive effect of the benefit level on takeup, but little effect of the potential duration of benefits. We also find that our estimated tax rate effects closely fit simple economic predictions. The paper proceeds then with Section 2, in which we present a model of the takeup decision which gives some clear predictions of the effects of UI generosity and other variables on the filing decision. Section 3 outlines the main institutional features of UI, while Section 4 relates this paper to the previous literature on UI incentive effects. Section 5 describes the data used, while Sections 6 and 7 follow with our empirical methods and main results. Section 8 presents our simple model of duration estimate biases caused by endogenous takeup. Section 9 provides estimates of the overall effect of UI benefits on weeks of receipt. Section 10 then offers some conclusions.

2. A Model of UI Takeup

This section introduces a simple static model of the takeup decision and briefly discusses an alternative search model. In the static model a potential applicant maximizes expected utility, which is taken to be a function of income and the stigma or transaction costs of applying for UI. The worker weighs these costs of applying against the benefits, which are determined primarily by the level and duration of benefits and the distribution of possible spell lengths that the worker believes she faces. This emphasis on expected spell length is motivated by the large fraction of nonapplicants who indicate the they do not apply because they expect a short spell. As can be seen in Table 1, 37 percent of those that believe they are eligible and did not apply indicate that they did not apply because they expected to get another job soon or be recalled. The next most common reasons (besides "other" and "don't know") are "too much work/hassle to apply" at under 7 percent and "too much like charity/welfare" at under 6 percent.

Formally, let the utility of income y be U(y) for a non-applicant and U(y)-c for an applicant. For simplicity, let the length of the period be 1 unit, let the length of unemployment be λ, and the potential duration of benefits be d. Also let the after-tax wage be w and the after-tax unemployment benefit be b. Assume that a potential applicant takes the cumulative distribution of unemployment spell lengths that she could experience to be $F(\lambda)$. Lastly we assume that the application cost varies across individuals so that $c=C+\epsilon$, where ϵ is a continuously distributed random variable unobserved by us, with c.d.f. L, and L'> 0 everywhere.

The expected utility of an individual who does not apply is

$$\int_0^1 U(w(1-\lambda))dF(\lambda) \ ,$$

while the expected utility of an applicant is

$$\int_0^d U(w(1-\lambda)+\lambda b)dF(\lambda) + \int_d^1 U(w(1-\lambda)+db)dF(\lambda) - C - e$$
$$= \int_0^1 U(w(1-\lambda)+b\min\{d,\lambda\})dF(\lambda) - C - e .$$

An individual decides to apply if the benefits exceed the costs, i.e. if

$$\int_0^1 \left[U(w(1-\lambda)+b\min\{d,\lambda\})-U(w(1-\lambda))\right]dF(\lambda) > C+e .$$

The implied probability of applying for UI is thus

$$P = L\left(\int_0^1 \left[U(w(1-\lambda)+b\min\{d,\lambda\})-U(w(1-\lambda))\right]dF(\lambda) - C\right) .$$

The effect of changes in various parameters of the model are now easily calculated.

$$\frac{\partial P}{\partial C} = -L' < 0 ,$$

$$\frac{\partial P}{\partial b} = L'\int_0^1 U'\min\{d,\lambda\}dF(\lambda) > 0 ,$$

$$\frac{\partial P}{\partial d} = L'\int_d^1 U'b\,dF(\lambda) \geq 0 , > 0 \text{ if } F(d) < 1 , \text{ and}$$

$$\frac{\partial P}{\partial w} = L'\int_d^1 \left[(1-\lambda)U'(w(1-\lambda)+bmin\{d,\lambda\})-U'(w(1-\lambda))\right]dF(\lambda) \leq 0, < 0 \text{ if } U''< 0 .$$

Thus, higher benefits and lower application costs increase the probability of application. A marginal increase in the potential duration of benefits increases the probability, but only if the potential applicant believes she may be unemployed at least as long as the potential duration. An increase in the wage decreases the application probability of a risk averse individual.

We might also want to consider the effect of changes in the assumed distribution for λ. If $G(\lambda) \leq F(\lambda)\ \forall\ \lambda$ and $G(\lambda) \neq F(\lambda)$ for some $\lambda \leq d$, then the probability of applying is higher under the distribution G than under F. If the individual is risk averse so that $U'' < 0$, then the probability is higher even without the restriction $\lambda \leq d$. In other words, rightward shifts in the distribution will increase the application probability. The exception to this rule is that risk neutral workers will not change their application probability in response to shifts in the distribution after the benefit exhaustion point.

There are several simplifications in this model. Leisure does not enter a person's utility and the cost (or stigma) of applying does not depend on b. This second restriction means that there is no variable stigma in the terminology of Moffitt (1983). We also assume that the potential applicant decides whether to apply at the beginning of her unemployment spell and cannot make a sequential decision as her spell progresses. Such a sequential model would probably give similar predictions about the effects of variables on ever applying, but it would allow us to model the decision about when to apply. We might expect that UI affects not only receipt conditional on unemployment, but also whether a laid off worker actually experiences unemployment. The intensity of search on the job will affect the probability that an individual given a layoff notice will find a job before becoming unemployed. A search model with search on the job such as Mortensen (1990) would lead to such a result if the model were augmented to allow search intensity to be endogenous. Thus, UI benefits should affect the probability that a worker becomes unemployed in the first place.

Applying our model in a structural way to data would involve several additional issues. For example, the model ignores other income sources which would affect the utility of a given stream of income from work and UI. One would also expect that the subjective duration distribution and the cost of applying would differ across individuals and depend on such characteristics as industry. In order to identify such a model, it would be useful to have certain variables that affect the cost of applying only, or only the duration distribution. The current paper does not implement this type of structural model, but rather estimates a reduced form model suggested by this structure.

3. The Structure of the U.S. Unemployment Insurance System

While state UI systems differ in many dimensions, each system shares several key characteristics. First, each state has a schedule relating the weekly benefit amount (WBA) to a claimant's work history in the base period, subject to a minimum and maximum benefit level. The base period is generally defined as the first 4 of the last 5 quarters completed prior to a claim. Within that base period, the highest amount of earnings in any one quarter is designated high quarter wages (HQW), while total earnings are designated base period earnings (BPE). A typical benefit formula will then set the WBA to be between 1/20 and 1/26 of HQW, with monetary eligibility for the program dependent upon BPE being at least 1.25 or 1.5 times HQW. The maximum benefit level is often reached by people with only moderately high earnings, resulting in about 35 percent of claimants qualifying for the maximum WBA. Thus, while most claimants have replacement rates of between 50 and 60 percent of usual wages, average replacement rates are somewhat lower. In addition to satisfying monetary eligibility, a worker must also meet nonmonetary eligibility requirements. Most notably, the claimant must search and be available for work, and must not have been fired for cause. Most states also exclude those workers who quit their last job, although there are sometimes provisions for UI receipt after a lengthy waiting period or an intervening period of work. The standard waiting period for eligible claimants is just one week.

The potential duration of benefit receipt is usually related to past work history, in a manner similar to the benefit level. The duration of benefits is generally proportional to the ratio of BPE to HQW, and often subject to a 26 week maximum. Somewhat less than half of recipients do not qualify for the maximum number of weeks because they have had irregular work histories. There are also provisions during times of high unemployment for the extension of benefits to workers who remain unemployed beyond the duration of their state benefits. These extensions are based upon the initial state potential durations, though, so for example the main extended benefits program extends benefits 50 percent longer than the state duration, up to a maximum of 13 weeks. Thus, a worker who originally qualified for less than 26 weeks of benefits would receive a shorter extension than would a worker who originally qualified for the maximum. Finally, each state relies on an experience-rated payroll tax to finance benefits. That is, a firm's tax rate is determined by its past use of the UI system, subject to a minimum and maximum rate. In essence, then, most firms can expect to repay through higher taxes some fraction of the benefits generated by an extra layoff. A more detailed discussion of the history and main provisions of the U.S. system can be found in Anderson and Meyer (1993a).

4. Relationship to Previous Work on UI Incentive Effects

Most of the previous literature on the incentives inherent in UI has focused on the effects of UI on the duration of unemployment. There is also a much smaller body or research on the effect of UI on transitions from employment to unemployment.[5] Nearly all of this literature, however, assumes that the generosity of benefits does not also affect the decision to become a UI

[5]There are many good surveys of the UI literature, including Atkinson and Micklewright (1991), Burtless (1990), Gustman (1982), Welch (1977), and Hamermesh (1977).

recipient. Despite the importance of this issue, there has been almost no past work on benefit takeup rates. This situation is somewhat surprising, given that analyses of takeup are a key part of the work on other major social insurance programs, such as Food Stamps, Aid to Families with Dependent Children (AFDC) and worker's compensation (WC).[6]

A few empirical papers use individual level data to model the probability of UI receipt. Gritz and MaCurdy (1989) find substantial effects of the level and duration of benefits on the probability of UI receipt conditional on unemployment and estimated eligibility in the Youth Cohort of the National Longitudinal Survey. Meyer (1992a) examines a 36 percent increase in unemployment benefits in New York State and finds disproportionate increases in the number of claims for the classes of workers which received the increase. In an earlier examination of 17 benefit increases in 6 states, he found no significant effect of benefits on the number of claims (Meyer (1989)). Using data from the Displaced Worker Surveys of the 1984-1992 Current Population Survey, McCall (1994) finds a significantly positive effect of the UI replacement rate on the probability of UI takeup. Additionally, he concludes that there is some evidence that the effect is smaller at higher replacement rates. While the estimated elasticities vary somewhat over samples and specifications, the average tends toward 0.3 for white-collar workers and 0.2 for blue-collar workers.

Two mainly aggregate data studies were motivated by a desire to understand the declining UI claims rate in the 1980's. Corson and Nicholson (1988) examine state by year data on the fraction of the unemployed that receive UI. They call this variable the claims ratio and they regress it on the UI replacement rate and a battery of other variables. The replacement rate is measured as the average weekly benefit of UI recipients divided by the average weekly wage of

[6]See Moffitt (1983, 1992) for references to the literature on AFDC and Food Stamps and Ehrenberg (1988) for references on workers' compensation.

the employed. They obtain elasticity estimates for the replacement rate with respect to the claims rate of between 0.23 and 0.56. Corson and Nicholson find that their variable measuring the taxation of UI benefits can explain all of the recent decline in UI receipt, but they discount this result since the tax variable is highly correlated with the variables they use to capture the time trend in the claims ratio. Corson and Nicholson also estimate takeup rates using individual data from the Panel Study of Income Dynamics (PSID). In the PSID data they again find a large effect of their benefit taxation variable, but they do not include a replacement rate variable.

Blank and Card (1991) examine aggregate data similar to that of Corson and Nicholson but they adjust the unemployment numbers by UI eligibility rates estimated using the March Current Population Survey (CPS). The resulting ratio of weeks of UI claims to weeks of eligible unemployment is used as a takeup rate. Blank and Card's estimates imply a benefit replacement rate takeup elasticity of 0.32 to 0.58. One should note that the dependent variable in these last two studies is a measure of weeks of UI received divided by weeks of unemployment (adjusted for eligibility in the case of Blank and Card). Therefore, these estimates of the replacement rate elasticities may reflect benefit effects on the duration of UI claims rather than on the incidence of claims. Blank and Card find that their adjustment for eligibility cannot explain any of the decline in the insured unemployment rate, and they conclude that the decline is due to a decline in the takeup rate. Blank and Card also analyze PSID data but they do not account for the partial taxation of benefits. They do include the same average state replacement ratio which they used in the CPS study. In this case, though, the replacement rate is always insignificant and generally the opposite sign from predictions and the CPS results.

5. The Matched Employment and UI History Data

The data we use were collected in the late 1970's and early 1980's as part of the Continuous Wage and Benefit History (CWBH) project and consist of two types of administrative records from the UI systems of 6 states.[7] One type of data is quarterly wage records for a large sample of the state's covered workers. Since the main category of noncovered workers is the self-employed, the sample effectively covers all employees in each state. A person and firm identifier on these records allow us to create job-match histories. If a specific job match last appears in a quarter other than the final quarter of data collection, we identify a separation to have occurred at that time. A drawback to this method is that separations which are followed by a return to the same job without a full calendar quarter intervening will be missed. However, a second type of data consist of UI claims records, so by matching these to the wage records we can identify those short temporary separations that result in UI receipt. A detailed description of this matching process and other characteristics of the data are contained in Anderson and Meyer (1992).

Since these wage records contain the same earnings information used by the states to determine the generosity of UI benefits, we have very good information to compute monetary eligibility and the level and duration of benefits. We first calculate base period earnings and the high quarter wage for all employees separating from their job match, assuming that the quarter of separation is the quarter of the initial claim. These values are then used in combination with the state laws to estimate the WBA and initial potential duration (PD) for which the worker would be eligible. All of the dollar values are then indexed using state average weekly earnings. Unlike survey data, these administrative records provide very good information on monetary eligibility and the generosity of the program, but unlike most administrative data sources, this information is

[7]The states are Georgia, Idaho, Louisiana, Missouri, New Mexico, and South Carolina.

available regardless of actual UI receipt. Having matched the employee claim records to the employer wage records, though, we can identify those separations which do actually result in UI receipt, and we define a binary variable to reflect this. The final data set, by providing a measure of receipt conditional only on separation, allows us to explore the incentive effects of UI in a new way. The main disadvantage of the data is that we do not have individual demographic variables. However, we do try fixed effects models below which difference out any such variables.

6. Empirical Methodology

BENEFIT, TAX AND PREVIOUS EARNINGS VARIABLES

With the data described above, we are able to test our simple model of the effects of benefit generosity on takeup. The most important UI variables are the weekly benefit amount and the potential duration of benefits. In addition, we test several economic predictions about the effect of taxes. A general property of economic models is that workers should respond to the after-tax weekly benefit amount, $WBA(1-\tau_b)$, where τ_b is the marginal tax rate on UI benefits. Taking logarithms, we obtain $\ln(WBA)$ and $\ln(1-\tau_b)$ which we enter as separate explanatory variables. We then test the equality of the coefficients on these two variables, as would be implied by a wide class of models.[8] Note that in our model of Section 2 the partial derivative of the application probability with respect to $\ln(WBA)$ and $\ln(1-\tau_b)$ are equal. We also examine if workers respond to the tax rate on earnings as predicted for risk averse workers in Section 2. Increases in the after-tax wage were predicted to decrease the application probability, so that

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[8]Rosen (1976) and Solon (1985) are other papers which employ a similar test of tax effects. They estimate a "coefficient of tax perception" using a different parameterization.

increases in $(1-\tau_y)$ should also decrease the application probability. Since we believe that earnings should have an independent effect on the claim probability since they measure factors such as labor force attachment, we do not focus on their effect. This issue is discussed more below.

To implement this formulation, we first approximate the marginal tax rate on earned income using the single filer schedules[9] and define adjusted gross income (AGI) to be BPE minus the exemption amount. When UI benefits are not taxable, we set the marginal tax rate on UI benefits to zero, otherwise we use the value for earned income. Benefits were not taxed at the federal level prior to 1979. After that, we base our determination of taxation on the single filer cutoff, which prior to 1982 was AGI > $20,000, and after that was AGI > $12,000.[10] Similarly, state marginal tax rates are estimated using the state tax schedules.[11] Additionally, we assign a marginal OASDI payroll tax rate that is zero for those with earnings above the statutory maximum and is equal to the employee rate for those below.[12] Then, the total marginal tax rate on UI benefits (τ_b) is the sum of applicable federal and state taxes, while that on income (τ_y) is the sum of the federal, state and OASDI rates.

As noted above, UI benefit levels and potential durations are determined from formulas based on past work history. Unless this past history is carefully conditioned upon, it will be

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[9]We could impute marital status, but then we would also need to impute spouses income.

[10]States also differ in their treatment of UI. In some states UI is fully taxed, in some completely untaxed, while in others they follow the federal treatment of UI.

[11]We obtained the State tax schedules and the information on the treatment of UI from Commerce Clearing House's State Tax Handbook supplemented and checked against State and Federal tax returns. We thank Tom Downes and Dan Feenberg for making available State tax returns for various years.

[12]Our results did not appear to be very sensitive to this incidence assumption.

difficult to disentangle the effects of UI from those of past work history.[13] To see this problem in its most extreme form consider a single state at a point in time. Both the benefit level and potential duration would be simple functions of past earnings, so it would be impossible to identify the effects of UI without assuming a particular functional form for the effects of earnings on takeup. We might expect measures of past earnings to influence takeup as they might be expected to capture commitment to the labor force as well as the degree of seasonality of a person's job. However, we have little reason to know the particular form this relationship takes. Thus, we flexibly condition on past earnings by using a bilinear spline (a piece-wise linear continuous function of two variables) in BPE and BPE/HQW.[14] We use the earnings measures HQW and BPE/HQW because the WBA and potential duration are proportional to these two variables, subject to minima and maxima. We then use the quartiles of these variables as knot points to define our spline. The result is a set of 24 variables which form a flexible and continuous two dimensional function which controls for past earnings. Formally, let the 25th, 50th, and 75th percentiles of ln(HQW) be KH_2, KH_3, and KH_4 and the corresponding percentiles of ln(BPE/HQW) be KR_2, KR_3, and KR_4. Then we enter as regressors the 24 variables: $H_1,\ldots,H_4$, $R_1,\ldots,R_4$, RH_{11}, RH_{12}, $RH_{21},\ldots$, RH_{44}, where $H_1=\ln(HQW)$, $H_i=\max(0,H_1-KH_i)$, $i=2,\ldots,4$, $R_1=\ln(BPE/HQW)$, $R_i=\max(0,R_1-KR_i)$, $i=2,\ldots,4$, and $RH_{ij}=H_i{*}R_j$, for $i=1,\ldots,4$ and $j=1,\ldots,4$.

Other explanatory variables include firm size, industry, state, year, and quarter dummy variables. The national unemployment rate for the last month of the previous quarter is also included in the logit equations. Additionally, the average state insured unemployment rate for the three months ending with the last month of the previous quarter is used in some specifications.

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[13]This point has been made by Welch (1977) and has been emphasized by Meyer (1988, 1992a).

[14]Poirier (1976) provides a full discussion of the use of bilinear splines.

SAMPLES AND SUBSAMPLES

While we have very good information on monetary eligibility, we have no information on whether a worker is nonmonetarily eligible for UI benefits. Thus, besides the full sample, we create two subsamples of separations that are more likely to be layoffs rather than quits, in order to assess the importance of this omission. Since the wage records contain average firm employment over the quarter, we can identify shrinking firms (provided that the employer appears in the data for more than one quarter) and calculate the extent of the employment decline. For each of the separations in our sample, we return to the main CWBH data files and, if possible, attach prior quarter employment at that firm. From this sample we create the first subsample which includes only those separations from firms where employment declined over the past quarter. We also create variables for the absolute value of that decline and for the percentage decline. To focus on separations that are likely to be the result of mass layoffs, we define subsamples based on various combinations of percentage and absolute declines. For the empirical work below we characterize a "mass layoff" to have occurred if the firm both declined at least 5 percent and lost at least 15 workers.[15]

7. Empirical Results

DESCRIPTIVE STATISTICS FOR THE SAMPLES AND SUBSAMPLES

Means for the key variables for the full sample and the declining employment and mass layoff subsamples are presented in Table 2. Each observation is a separation of a worker who is

[15]Various other definitions were experimented with and found to produce qualitatively similar results.

monetarily eligible for UI.[16] There is a decrease in the number of separations and an increase in the fraction of the separations that result in a UI claim as we move from the full sample to the declining employment sample and the mass layoff sample. We begin with 80,331 separations, 22 percent of which result in a UI claim. The declining employment and mass layoff samples have 29,947 and 11,382 separations and UI claim rates of 0.31 and 0.41 respectively. The receipt rate is higher in the subsamples both because the separations are more likely to be layoffs rather than quits and also because an unemployment spell is more likely following a separation as the subsamples more heavily weight sectors where employment is declining. The industries with the highest representation are manufacturing, services, retail trade and construction, while the years 1981-3 are the most common among the 6 represented. In the mass layoff sample there is a higher representation of manufacturing and the high layoff year of 1982 becomes more prominent. The separations are spread across our 6 states, and 5 firm sizes, with the importance of the smallest firms falling dramatically in the mass layoff sample.

LOGIT ESTIMATES OF TAKEUP ON THE MAIN SAMPLES

To assess the determinants of UI claims we estimate a series of logit equations to see which of the monetarily eligible separations result in UI receipt. Table 3 reports a series of specifications for our full sample, while Tables 4 and 5 repeat these specifications for our two subsamples. All specifications include benefit and tax variables, the national unemployment rate, year, industry, firm size and quarter of separation dummies. Specifications (1), (2), (3), (5) and (6) also include the 24 variable earnings spline described above, while specification (4) enters only

[16]The sample data cover the following time periods in each state: Georgia 1979:2-1983:4; Idaho 1979:4-1981:4; Louisiana 1981:4-1984:1; Missouri 1979:2-1982:4; New Mexico 1981:3-1983:4; and South Carolina 1981:4-1983:4. However, for the subsamples we must look back one additional quarter, so each state's time frame begins one quarter later.

ln(HQW) and ln(BPE/HQW), rather than the spline.[17] In specification (2), we also include state dummy variables, and in specifications (3), (4), (5) and (6) state-year interactions are also added.

Specification (5) is simply an alternative version of specification (3). In it, we use a restricted definition of potential duration which excludes the estimate of any additional weeks of extended or supplemental benefits for which the worker may be eligible. We try this alternative specification because the expanded measure of potential duration is imperfect, since we do not have the exact date of separation only the quarter. Rather, we assign the appropriate number of extra weeks of benefits for a worker if the extended or supplemental benefit program was in effect for at least half of the quarter of separation. Specification (6) repeats specification (3), but with the sample restricted to exclude temporary layoffs. This specification is tried because of a concern over the fact that some temporary layoffs are only observed because of UI receipt.

BENEFIT LEVEL AND TAX VARIABLES

The main specifications in the three tables give fairly similar results for the coefficient on the logarithm of the weekly benefit amount and, to a lesser extent, the tax variables. The weekly benefit is found to have a large and highly significant effect on the probability of a UI claim. This result appears in every specification and sample used. The elasticity of the claim probability with respect to the benefit amount in the base model of specification (3) is 0.46 for the mass layoff sample and 0.62 for the full sample. While the coefficient in specification (6) is somewhat lower than the others, due to the lower probability of UI receipt the elasticity is actually a bit higher, at

[17] In the subsamples it was necessary to reduce the flexibility of the spline slightly by dropping the variable RH_{44} in order to obtain convergence.

0.78 for the full sample and 0.63 for the mass layoff sample. Thus, these estimates indicate large effects of UI on the probability of beginning a claim, conditional on a UI eligible separation.

The coefficients on the tax variables accord quite well with the predictions of economic theory. The coefficients on ln(WBA) and $\ln(1-\tau_b)$ are always of the same sign and generally are of a similar magnitude. However, equality of the coefficients can only be accepted in the mass layoff sample, given p-values for the likelihood ratio test statistics for the equality of the two coefficients of 0.001, 0.02 and 0.06 in the three samples for our base specification in column (3). The coefficient on $\ln(1-\tau_b)$ is generally smaller than the benefit coefficient. This result is consistent with either incomplete perception of the tax rate or measurement error in our assignment of the rate. Additionally, the coefficient on $\ln(1-\tau_y)$ is always negative as predicted, though it is usually not significantly different from zero. Overall, the results accord well with the predictions of economic theory, especially given that we must impute marginal tax rates without knowledge of deductions and filing status.

Simulations of the effect of the taxation of UI benefits, which began in 1979 and was completed in 1987, suggest that this tax change can explain a large part of the decline in takeup over this period. Blank and Card estimate a 12 percent decline over this period using their CPS data and estimated eligibility. We estimate declines of 11.4, 12.1 and 8.9 percent in our three samples by changing the tax on benefits from zero to the regular income tax rate for each individual. Our estimated decline would be larger if we used the benefit coefficient rather than the tax coefficient and smaller if we used the sample which excludes temporary layoffs. There are several reasons, though, why our estimate is likely to be an overestimate of the expected decline in the takeup rate as measured by Blank and Card. First, our estimate of the effect of after-tax benefits on claims is partly an effect of benefits on the probability of entering unemployment.

Thus, we expect that higher benefits would increase both the numerator and denominator of the Blank and Card takeup rate and thus not increase their measure as much as ours. Second, we use states with below average takeup, and benefits and taxes are likely to have a larger marginal effect when takeup is lower.

POTENTIAL DURATION AND THE SENSITIVITY OF ESTIMATES TO MODELING

While the estimated effect of the level of benefits is similar in all specifications and samples, the coefficient on potential duration is very sensitive to the methodology used. Specification (1) in each of the samples does not include state fixed effects or state-year interaction dummies. The coefficient on potential duration is negative and at least marginally significant in all three. This result strongly suggests that there are important omitted state differences in takeup rates that are correlated with potential duration. These state differences could be characteristics like unionization rates or differences in state UI programs like disqualification rates, the number and staffing of UI offices and the amount of paperwork required for filing. Once state fixed effects are included, the coefficient on potential duration is always positive as expected, though rarely significantly so. Likelihood ratio test statistics indicate that the state dummies are extremely significant in all samples.

Another important lesson in methodology comes from comparisons of specifications (3) and (4) in Tables 2 through 4. In specification (4), a substantial component of the identification of the effect of potential duration comes from restricting the logarithm of BPE and HQW to enter the argument of the logit function linearly. This is similar to the manner in which many past UI studies have controlled for past earnings, though most have not had available the exact values of the appropriate earnings measures. Recall from Section 3 that the potential duration of benefits within a state at a point in time is a simple monotonic function of the ratio of these two earnings variables. While the restricted model indicates a large and always statistically significant positive effect of potential duration on the claim probability, the specification with flexible controls indicates very little evidence of an effect of potential duration on claims. Likelihood ratio tests strongly support the presence of the additional earnings variables that make up the bilinear spline, calling into question a large effect of potential duration. This result is not implausible given our theoretical model where the potential duration only matters at all for those who think they may exhaust benefits.

EFFECTS OF OTHER VARIABLES

Besides the variables just discussed, we include in the specifications of Tables 2 through 4 the national unemployment rate in the last month before the quarter of separation and a number of variables for which we do not report coefficient estimates. These variables include industry dummies and firm size dummies. There are several reasons why these variables can affect the claim rate. They may affect the likelihood that a separation is a quit and thus nonmonetarily ineligible for UI, or they may affect the expected duration of unemployment, and thus the reward to filing for UI. Also, they may affect the probability that a layoff is a temporary one. If an individual expects to be laid off temporarily, she is less likely to search intensively, and more likely to wait to be recalled.[18] Thus, temporary layoffs are more likely to involve unemployment and the possibility of UI receipt, rather than an immediate change of employers. Some of these variables may also affect the costs of filing a claim. In large firms in highly unionized industries, for example, the worker is more likely to be made aware of the procedure for filing, and a claim may even be filed for the worker. As reported in the Tables, the coefficient on the logarithm of

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[18]See Feldstein (1975), Katz (1986), Katz and Meyer (1990) and Anderson (1992) for models of this phenomenon and empirical support for this argument.

the U.S. unemployment rate is positive and significant in most specifications. The estimates are large, suggesting that a one percentage point increase in the unemployment rate would increase takeup by about one percentage point. The explanations for this result include the procyclicality of quits and the countercyclicality of temporary layoffs. If in high unemployment times there are few quits and more temporary layoffs, a higher fraction of separations will be nonmonetarily eligible and fewer people will take new jobs before being unemployed. Note, though, that the importance of the unemployment rate persists in the mass layoff subsample where quits should be much less common, so they cannot be the entire story. In a search model, however, the offer arrival rate should also be procyclical. Thus, the unemployment rate may be positively related to the probability that a job will not be found quickly after notice of termination, even for those permanently laid off. Finally, the state insured unemployment rate in specification (5) is never significant and barely affects the results.

While not shown in the tables, each of the other sets of explanatory variables tends to be significant. The dummy variables for major industry groups are highly significant, and indicate that UI receipt is higher in construction, mining, and especially manufacturing. One reason for this result may be that these industries have high levels of temporary layoffs, so industry may proxy for nonmonetary eligibility or a tendency to become unemployed and wait for recall rather than move to a new job. Again, the industry effects remain in the mass layoff sample, suggesting that nonmonetary eligibility does not explain most of these patterns. In addition, manufacturing, mining and construction tend to be highly unionized industries and union status has been found to be strongly related to UI receipt.[19] Finally, the probability of UI receipt rises with firm

[19]See Corson and Nicholson (1988) and Blank and Card (1991). Although the reasons for this relationship are not clear, one possibility is that union representatives are available to provide information and generally reduce the costs of filing for UI.

size, though in the mass layoff sample only the largest category of firms (employment at least 2000) is significantly different from the smallest (employment under 20).

ADDITIONAL ISSUES

In some instances, nonmonetary eligibility can be thought of as being affected by UI. The UI system provides incentives to change either reported or actual reasons for separation. For example, in order to qualify for UI, workers who foresee a separation have the incentive not to quit, but to wait for a layoff, while an experience-rated firm would prefer to induce a quit. Additionally, workers have the incentive to report a separation as a layoff, while firms have the incentive to fight the claim. Imperfect experience rating implies that the incentive to fight a claim will vary across firms.

We can directly address the issue of firm incentives to fight claims. Since our data include the firm's exact UI tax rate and all of our states use the reserve ratio method of experience rating, we can calculate the tax cost of a layoff in the manner of Topel (1983). As described in Anderson and Meyer (1992), we can also estimate the firm's current reserve ratio based on the state tax schedule. By including this reserve ratio as a control (albeit a somewhat imperfect one) for the firm's past layoff history, we can interpret the tax cost variable as reflecting the firm's degree of experience rating, and hence as a measure of its incentive to contest claims. While the coefficient on tax cost is negative as expected, it is small and insignificant in all three samples, suggesting that any effects from firm incentives to contest claims are small.

We also estimate conditional logit models using the approach of Chamberlain (1980). The results suggest that our estimates have not been badly biased by the omission of individual characteristics like age, sex, race, and education that are not available in our data. Such characteristics are differenced out of the estimation equation. In addition, this method controls for differences across firms in their inherent proclivity to layoff workers and to contest claims. We use pairs of observations from the same worker-firm match. While the point estimates are supportive of the previous results, however, only in the largest sample is the coefficient on ln(WBA) significant, since the layoff samples become quite small and standard errors rise. A similar problem arises in our second alternative approach. In order to exploit just the variation due to changes in state laws, we try a "natural experiment" approach, using only data from before and after increases in UI benefits. These specifications give coefficients of similar magnitude, but again the drastically reduced sample sizes produce generally insignificant coefficients. A final approach involves including quadratic and cubic terms in the benefit and tax variables. Here the quadratic term in benefits is often significant, but the specifications yield similar mean derivatives of the takeup probability with respect to benefits.

THE PLAUSIBILITY OF OUR ESTIMATED TAKEUP RATES

To examine the plausibility of our estimated takeup rates which are conditional on a separation, we compare them to estimates from other sources. One should note that past estimates differ greatly among themselves, in part because each paper uses different measures of takeup. Vroman (1991) provides numbers which allow takeup measures to be calculated based on self-reported eligibility. Blank and Card (1991) are able to roughly impute eligibility using a measure of individual earnings and the reason for unemployment in the CPS. Their approach gives a fraction of eligible weeks of unemployment that are UI compensated rather than a fraction of spells. On the other hand, their estimates of takeup from the PSID are measures of the fraction of eligible spells in which UI is received. In the comparisons below we rely on Blank and Card's CPS takeup estimates as they are in the middle of the range of takeup estimates.

Our estimate of takeup in our full sample needs to be adjusted for the fraction of separations which are not layoffs and thus generally ineligible for UI. If we divide our takeup rate for monetarily eligible separations by the fraction of separations which are layoffs we obtain an estimate of the fraction of monetarily and nonmonetarily eligible separations that result in UI receipt. Since estimates of the fraction of separations that are layoffs range from 0.3 to 0.5, depending on the business cycle,[20] the implied takeup rate for our data are .44 to .73. This can be compared to the takeup rate of 0.58 estimated for our 6 states in Blank and Card (1991).[21] Alternatively, we can assume that the mass layoff sample includes a very small fraction of nonmonetarily ineligible claims. Then we would expect that dividing our mass layoff claims rate by the fraction of layoffs that result in unemployment would yield a takeup rate similar to other estimates. Estimates of the fraction of layoffs that result in unemployment range from under 0.70 to 0.86 in three sources.[22] Thus, the implied takeup rate for those who experience unemployment is between 0.48 and 0.59. Thus in both samples, it appears that our takeup rate conditional on a separation is reasonable. The near doubling of the claims rate as we go from the full sample to the mass layoff sample also suggests that we have excluded most quits from the latter sample. We do however suspect that our takeup rates are biased downward due to spurious separations where a firm neglects to send in wage records for a quarter or two. Anderson and Meyer (1994) discuss the prevalence of such errors and argue that while they clearly occur they only account for a small part of measured separations.

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[20]These estimates are from the PSID and are reported in McLaughlin (1990) and Altonji and Williams (1992). The layoff fraction is generally over 0.4 in the years we examine.

[21]We weight the estimated takeup rates for the 6 states by their sample fractions.

[22]Mincer (1991) gives the under 0.70 figure, Gottschalk and Maloney (1985) give 0.76 for involuntary separations which includes fires, and Gibbons and Katz (1991) give 0.86 for displaced workers.

8. Endogenous Takeup and Biases in Duration Estimates

This section shows that estimates of the effect of UI benefits on the duration of unemployment are likely biased if an individual's decision to apply for benefits depends on her level of benefits and expected duration of unemployment. Recall that in accordance with the model of section 2, the regression results presented above indicate that the level of benefits does have a significantly positive effect on the probability of UI receipt. An additional prediction of the model is that the application probability will increase with an increase in expected spell length. In order to investigate whether this relationship holds empirically, we would like to calculate UI takeup rates by completed spell length, implicitly assuming that completed spell length is a good indicator of expected spell length. Unfortunately, weeks worked and/or weeks unemployed are not available in the data. However, we are able to estimate the spell length based on changes in quarterly earnings.

In order to obtain an estimate of the number of weeks unemployed, we first define normal quarterly earnings to be earnings in the quarter prior to the separation. Normal weekly earnings are normal quarterly earnings divided by 13. The number of weeks unemployed in the quarter of separation (or reemployment) is then estimated as normal quarterly earnings minus earnings in the quarter of separation (or reemployment), divided by normal weekly earnings. We then truncate each of these measures to be an integer between 0 and 13, and add them together with 13 weeks for each full quarter of missing wage records to obtain our estimate of the completed spell length. As can be seen in Table 6, the UI takeup rate increases steadily as estimated spell length increases from under 2 weeks up to 5 months, and remains at this high level for longer spell lengths. This is true both for the full sample and for the mass layoff sample. The very low rate of UI receipt in these samples is in large part due to our exclusion of temporary layoffs. Including these separations would spuriously suggest that short spells are associated with high takeup. In the full sample and mass layoff sample, such separations account for 66 percent and 73 percent of UI receipt respectively. We should note that a longer time period to file could be partly responsible for this increase, but in nearly all cases we only count UI receipt if it occurred within the quarter of separation. Thus, empirically it appears that the probability of UI receipt depends not only on the level of benefits, but also on the expected duration of unemployment.

To lay out our example, we first need to make several assumptions. For simplicity, let the probability of applying be described by the following linear probability model. Let P denote the probability of applying where $P = \alpha + \beta\bar{\lambda} + \gamma b + \delta\bar{\lambda}b$, and $\bar{\lambda}$ is expected duration of unemployment and b is the UI benefit amount. One can think of this model for P as a first order approximation to a more sophisticated model, noting that the model of Section 2 had an interaction between $\bar{\lambda}$ and b, albeit not a linear one. Now, let the duration of unemployment be determined by the simple linear equation $\lambda = \mu + \pi\bar{\lambda} + \rho b + e$. Then the probability limit of $\hat{\rho}$, the OLS estimate of ρ from a regression of λ on b and a constant is given by

$$\text{plim}\ \hat{\rho} = \rho + \pi\frac{COV(b,\bar{\lambda})}{VAR(b)} .$$

We are interested in this probability limit because typically we have only extremely rough proxies for expected duration of unemployment, with the most reasonable first approximation being that the expected duration is not observed by the researcher. It should be clear that the direction of the bias in ρ is determined by the COV(b, $\bar{\lambda}$), where this is the covariance in the sample of recipients. Even if b and $\bar{\lambda}$ are independent in the sample of potential applicants, they are likely to have a nonzero covariance in the sample of recipients. Consider the case where there are two equally likely values of b and $\bar{\lambda}$. Let those values be 0 and 1 without loss of generality, and assume that the distributions of the two variables are independent. One should note that in this case our linear model for P with the interaction is completely general, since the four parameters allow all possible values for the probabilities of applying for the four different combinations of the two variables.

In this case some algebra shows that

$$COV(b,\bar{\lambda}) = \frac{\alpha\delta - \beta\gamma}{(4\alpha + 2\beta + 2\gamma + \delta)^2}$$

which can be positive or negative. Thus, when expected spell duration and benefit generosity affect takeup, duration estimates for the resulting sample of recipients are likely to be biased, though the direction for the bias is unclear. It seems intuitively plausible that the marginal UI applicant would have a shorter than average duration, but the result above makes it clear that such intuition is not necessarily correct. It is also likely that this same argument can be applied to other programs such as AFDC and workers' compensation where spell durations are of interest and we expect the generosity of benefits and expected duration to affect participation.

9. The Overall Effect of Benefits on UI Receipt

It is useful to know how our estimated effect of the UI benefit amount on the probability of takeup combines with other effects to determine an overall effect on the number of weeks of UI receipt. Let W be the number of weeks of UI receipt (per person) and note that $W=LP\lambda$, where L is the probability of a layoff, P is the takeup probability as above, and λ is now the length of UI receipt. Then

$$\ln(W) = \ln(L) + \ln(P) + \ln(\lambda).$$

The elasticity of W with respect to the UI benefit b is given by

$$\frac{dln(W)}{dln(b)} = \frac{\partial \ln(L)}{\partial \ln(b)} + \frac{\partial \ln(P)}{\partial \ln(b)} + \frac{\partial \ln(\lambda)}{\partial \ln(b)}, \text{ or}$$

$$e_W = e_L + e_P + e_\lambda,$$

where e_j denotes the elasticity of j with respect to b. Thus, the total effect of benefits on weeks of UI receipt is the sum of the layoff, takeup and duration elasticities.

We can obtain estimates of the layoff and duration elasticities from other sources to obtain this total effect. Much of the work on layoffs has focused on the substantial effects of experience rating which we do not examine here. Card and Levine (1994) assume that the benefit elasticity for layoffs is negative, while Anderson and Meyer (1993b) find a range of estimates centered on zero, and Topel finds positive effects (insignificant in Topel (1983) and significant in Topel (1984)). For illustrative purposes we will set e_L to zero. There are many estimates of the duration elasticity, with recent estimates from Meyer (1990) and Katz and Meyer (1990) implying elasticities of about .7. Given our estimate of the takeup elasticity of approximately .6, this calculation suggests that the overall elasticity of weeks of UI with respect to the level of benefits may be more than one. Note that this calculation implies that the total effect of UI benefits on weeks of receipt is nearly doubled by taking into account takeup. Additionally, if we think layoffs rise with the level of UI benefits, the total effect of the benefit level would be larger although the fraction attributable to takeup lower. Obviously, the converse is true if we believe that higher benefits reduce layoffs.

One might wonder, however, if it is appropriate to simply sum the elasticities above. For example, we have seen that the probability of receipt is likely to depend upon the expected length of receipt. In the theoretical example above, the empirical estimate of ϵ_λ would be biased due to entry effects. While the likely direction of this bias is downward, we saw that theoretically it could be in either direction. In calculating the total effect then, it is really a question of whether we think the parameter estimates in various empirical studies have estimated the appropriate derivatives. Nevertheless, the main lesson is that one must consider the additional effect of benefits on takeup probabilities in order to fully gauge the impact on weeks of UI receipt. We should note, however, that the above calculation of the overall effect of UI benefits on weeks of UI receipt is also not complete, since it ignores displacement effects and possible entitlement effects of the UI benefit level.

10. Conclusions

Using a unique data set with very good information on potential UI duration and benefit levels we examine the probability of UI receipt conditional on a job separation. We find large effects of benefit levels on the incidence of claims, but almost no effect of the potential duration of benefits. The estimates imply elasticities of the takeup rate with respect to benefits of about 0.46 to 0.78. The benefit level estimates are very similar in several alternative sets of estimates. On the other hand, the potential duration estimates are changed dramatically by the omission of state fixed effects or by the imposition of strong assumptions on the way previous earnings affect the claim probability. We argue that this shows the importance of controls for permanent differences across states and of flexible earnings controls. We also find strong support for some simple predictions about the effects of the tax rate on benefits and earnings. Our estimates of benefit level effects are similar in magnitude to those found by some past researchers and somewhat larger than those of some others. Stronger comparisons are difficult to make given that each paper uses a different concept of takeup.

Our results also have several important implications for UI program design. Our simulations of the effects of taxing UI benefits indicate that recent tax changes can account for most of the decline in UI receipt in the 1980's. Additionally, as it appears that the effect of benefits on the size of the claimant population is large, this effect needs to be accounted for when determining appropriate benefit amounts and funding requirements. Furthermore, it is likely that past estimates of the effects of benefits on duration are biased. Our results also suggest that a large increase in the incentive to file claims, as would occur under a reemployment bonus for example, would likely cause a large increase in the number of claims.

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Table 1

Reason for not Applying for UI Benefits in Current Unemployment Spell, Job Losers and Leavers Believing Themselves Eligible for UI[a]

Reason for Not Applying for UI	Number (1000's)[b]	Percent of Total Group
Plan to file soon	57	5.10%
Don't know about UI/how to apply	63	5.64%
Expected to get another job soon/be recalled	414	37.06%
Too much work/hassle to apply	76	6.80%
Too much like charity/welfare	64	5.73%
Previously used up UI	43	3.85%
Other	213	19.07%
Don't know	187	16.74%
Total[c]	1117	100.00%

[a]The table is derived from Vroman (1991) Table 4 and represents responses to the following question from a special CPS supplement:

What is the main reason . . . hasn't applied for unemployment compensation since . . . last job?

[b]Population estimates obtained by weighting.

[c]Responses of "Don't think eligible" represented the population equivalent of an additional 1095 thousand people.

Table 2

Sample Statistics

	Full Sample	Declining Employment Sample	"Mass Layoff" Sample
Variable Mean (Standard Deviation)			
Received UI	0.221	0.307	0.405
	(0.415)	(0.461)	(0.491)
Weekly Benefit Amount (1982$)	114.81	117.42	122.51
	(42.16)	(40.79)	(40.89)
Potential Duration of Benefits (including EB and FSC)	31.02	31.36	31.72
	(9.94)	(10.10)	(10.10)
Potential Duration of Benefits (excluding EB and FSC)	22.89	23.25	23.46
	(4.86)	(4.59)	(4.53)
Base Period Earnings (1982$)	11872.47	12474.25	13320.69
	(10600.22)	(9965.36)	(9652.75)
High Quarter Wages (1982$)	4107.34	4220.03	4506.01
	(3954.63)	(3576.80)	(3438.97)
Marginal Tax Rate on UI Benefits	0.072	0.081	0.096
	(0.143)	(0.147)	(0.154)
Marginal Tax Rate on Income	0.274	0.283	0.295
	(0.117)	(0.113)	(0.113)
U.S. Unemployment Rate	8.398	8.441	8.467
	(1.514)	(1.555)	(1.537)
State Insured Unemployment Rate	3.57	3.55	3.60
	(1.17)	(1.17)	(1.20)
Industry Distribution			
Agriculture	0.015	0.011	0.012
Mining	0.031	0.039	0.053
Construction	0.121	0.108	0.111
Manufacturing	0.230	0.302	0.388
Transportation and Communication	0.055	0.052	0.049
Wholesale Trade	0.057	0.052	0.033
Retail Trade	0.192	0.169	0.122
FIRE	0.040	0.032	0.011
Services	0.228	0.203	0.186
Public Sector	0.031	0.031	0.035
Sample Size	80331	29947	11382

Table 3

Logit Estimates using Full Sample

	Coefficient (Standard Error) Average Derivative					
	(1)	(2)	(3)	(4)	(5)	(6)
Ln (Weekly Benefit Amount)	0.882	1.109	1.030	0.963	1.051	0.932
	(0.073)	(0.089)	(0.090)	(0.067)	(0.095)	(0.121)
	0.117	0.146	0.136	0.128	0.138	0.080
Ln (1 - Marginal Tax Rate on UI Benefits)	0.692	0.619	0.579	1.008	0.578	0.305
	(0.102)	(0.104)	(0.105)	(0.089)	(0.105)	(0.140)
	0.092	0.082	0.076	0.133	0.076	0.026
Ln (1 - Marginal Tax Rate on Income)	-1.739	-0.285	-0.330	0.230	-0.314	-0.140
	(0.275)	(0.295)	(0.297)	(0.269)	(0.297)	(0.378)
	-0.231	-0.038	-0.043	0.031	-0.041	-0.012
Ln (Potential Duration) [Includes EB and FSC]	-0.365	0.102	0.109	0.371	---	0.030
	(0.047)	(0.058)	(0.068)	(0.064)	---	(0.087)
	-0.049	0.014	0.014	0.049	---	0.003
Ln (Potential Duration) [Excludes EB and FSC]	---	---	---	---	0.156	---
	---	---	---	---	(0.097)	---
	---	---	---	---	0.021	---
Ln (US Unemployment Rate)	0.426	0.256	0.795	0.420	1.053	1.469
	(0.195)	(0.209)	(0.227)	(0.223)	(0.175)	(0.289)
	0.057	0.034	0.105	0.056	0.139	0.127
Ln (State Insured UR)	1.055	0.487	0.053	0.001	---	-0.156
	(0.040)	(0.060)	(0.079)	(0.078)	---	(0.100)
	0.140	0.064	0.007	0.000	---	-0.014
24 Variable Earnings Spline	YES	YES	YES	NO	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
State Effects	NO	YES	YES	YES	YES	YES
State by Year Effects	NO	NO	YES	YES	YES	YES
Excludes Temporary Layoffs	NO	NO	NO	NO	NO	YES
-2 Log Likelihood	67218.0	66820.9	66629.6	67205.8	66630.5	42109.9
Number of Observations	80331	80331	80331	80331	80331	69754
Percent of Sample with UI	0.221	0.221	0.221	0.221	0.221	0.103

Note: All Specifications also include controls for major industry group, firm size class and quarter of separation.

Table 4

Logit Estimates using Declining Employment Sample

	Coefficient (Standard Error) Average Derivative					
	(1)	(2)	(3)	(4)	(5)	(6)
Ln (Weekly Benefit Amount)	0.777	1.170	1.139	1.111	1.143	0.899
	(0.109)	(0.133)	(0.136)	(0.100)	(0.143)	(0.179)
	0.121	0.181	0.176	0.174	0.176	0.100
Ln (1 - Marginal Tax Rate on UI Benefits)	0.818	0.752	0.663	1.084	0.668	0.358
	(0.152)	(0.155)	(0.156)	(0.134)	(0.156)	(0.207)
	0.127	0.116	0.102	0.169	0.103	0.040
Ln (1 - Marginal Tax Rate on Income)	-2.189	-0.937	-0.885	-0.316	-0.867	-1.126
	(0.431)	(0.465)	(0.467)	(0.423)	(0.467)	(0.595)
	-0.341	-0.145	-0.136	-0.049	-0.134	-0.125
Ln (Potential Duration) [Includes EB and FSC]	-0.428	0.047	0.130	0.396	---	-0.203
	(0.072)	(0.087)	(0.106)	(0.098)	---	(0.133)
	-0.067	0.007	0.020	0.062	---	-0.023
Ln (Potential Duration) [Excludes EB and FSC]	---	---	---	---	0.131	---
	---	---	---	---	(0.151)	---
	---	---	---	---	0.020	---
Ln (US Unemployment Rate)	0.492	0.476	0.649	0.292	1.009	2.001
	(0.291)	(0.092)	(0.340)	(0.333)	(0.262)	(0.428)
	0.077	0.048	0.100	0.046	0.156	0.222
Ln (State Insured UR)	0.997	0.476	0.103	0.049	---	-0.079
	(0.062)	(0.092)	(0.119)	(0.117)	---	(0.149)
	0.155	0.074	0.016	0.008	---	-0.009
23 Variable Earnings Spline	YES	YES	YES	NO	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
State Effects	NO	YES	YES	YES	YES	YES
State by Year Effects	NO	NO	YES	YES	YES	YES
Excludes Temporary Layoffs	NO	NO	NO	NO	NO	YES
-2 Log Likelihood	28550.9	28376.5	28306.5	28614.2	28308.5	17740.0
Number of Observations	29947	29947	29947	29947	29947	24208
Percent of Sample with UI	0.307	0.307	0.307	0.307	0.307	0.143

Note: All Specifications also include controls for major industry group, firm size class and quarter of separation.

Table 5

Logit Estimates using "Mass Layoff" Sample

	Coefficient (Standard Error) Average Derivative					
	(1)	(2)	(3)	(4)	(5)	(6)
Ln (Weekly Benefit Amount)	0.556	1.111	1.121	1.005	1.145	0.899
	(0.166)	(0.208)	(0.215)	(0.158)	(0.226)	(0.283)
	0.093	0.186	0.186	0.169	0.190	0.122
Ln (1 - Marginal Tax Rate on UI Benefits)	0.761	0.637	0.523	0.892	0.522	0.031
	(0.234)	(0.237)	(0.242)	(0.207)	(0.242)	(0.319)
	0.128	0.106	0.087	0.150	0.086	0.004
Ln (1 - Marginal Tax Rate on Income)	-1.393	-1.080	-1.242	-0.869	-1.253	-1.034
	(0.667)	(0.718)	(0.724)	(0.649)	(0.724)	(0.916)
	-0.234	-0.180	-0.206	-0.147	-0.208	-0.140
Ln (Potential Duration) [Includes EB and FSC]	-0.322	0.103	0.128	0.444	---	-0.157
	(0.112)	(0.134)	(0.166)	(0.153)	---	(0.203)
	-0.054	0.017	0.021	0.075	---	-0.021
Ln (Potential Duration) [Excludes EB and FSC]	---	---	---	---	0.188	---
	---	---	---	---	(0.241)	---
	---	---	---	---	0.031	---
Ln (US Unemployment Rate)	0.503	0.451	1.019	0.664	1.111	2.072
	(0.469)	(0.498)	(0.540)	(0.528)	(0.424)	(0.666)
	0.084	0.075	0.169	0.112	0.184	0.281
Ln (State Insured UR)	1.024	0.457	-0.120	-0.205	---	-0.444
	(0.096)	(0.147)	(0.185)	(0.182)	---	(0.230)
	0.172	0.076	-0.020	-0.035	---	-0.060
23 Variable Earnings Spline	YES	YES	YES	NO	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
State Effects	NO	YES	YES	YES	YES	YES
State by Year Effects	NO	NO	YES	YES	YES	YES
Excludes Temporary Layoffs	NO	NO	NO	NO	NO	YES
-2 Log Likelihood	11560.6	11506.8	11430.5	11592.3	11430.8	7135.7
Number of Observations	11382	11382	11382	11382	11382	8411
Percent of Sample with UI	0.405	0.405	0.405	0.405	0.405	0.195

Note: All Specifications also include controls for major industry group, firm size class and quarter of separation.

Table 6

UI Receipt by Length of Completed Spell

	Full Sample		"Mass Layoff" Sample	
Estimated Time Spent Unemployed	Number of Permanent Separations	Percent Receving UI	Number of Permanent Separations	Percent Receving UI
Less than 2 weeks	20956	4.61	1947	6.62
2 to 4 weeks	4367	10.10	388	19.00
1 to 2 months	4071	14.42	368	23.33
2 to 3 months	2708	15.40	249	26.33
3 to 4 months	1744	18.99	137	34.76
4 to 5 months	999	26.73	73	45.93
5 to 6 months	914	25.16	87	40.00
6 to 7 months	894	20.13	64	38.46
7 to 8 months	479	26.72	44	50.00
8 to 9 months	507	27.22	53	40.45
9 to 10 months	595	21.51	49	39.51
10 to 11 months	307	28.34	28	48.15
11 to 12 months	287	23.34	27	40.00
Over 1 year	2147	19.00	211	30.59
Total	40975	10.68	3725	19.67

Note: Temporary separations and separations not followed by employment are excluded. See text for a complete description of how spell length was estimated.