UNEMPLOYMENT INSURANCE TAKEUP RATES AND THE AFTER-TAX VALUE OF BENEFITS*

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The recent decline in the unemployment insurance (UI) takeup rate has puzzled researchers. Using administrative data with accurate information on the potential level and duration of benefits, we examine whether a separating employee receives UI. We find a strong positive effect of the benefit level on takeup, and smaller effects of the potential duration and the tax treatment of benefits. Simulations indicate that the recent inclusion of UI in the income tax base can account for most of the previously unexplained decline in UI receipt.

I. INTRODUCTION

While there is an extensive literature on takeup of programs such as AFDC, Food Stamps, and Workers' Compensation,[1] there is relatively little work on the decision to file for Unemployment Insurance (UI) benefits. This gap exists even though survey estimates indicate that takeup rates for UI are substantially below one.[2] Much of the work on takeup of UI has been motivated by the recent decline in the fraction of the unemployed receiving UI.[3] As can be seen in Figure I, abstracting from cyclical movements, this fraction, Insured Unemployment/Total Unemployment (IU/TU),[4] declined somewhat in the early 1960s, then remained fairly

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

2. Estimates in the literature for the fraction of all eligibles who receive UI range from 0.53 to 0.71. The 0.53 estimate comes from Current Population Survey supplements from 1989 and 1990 reported in Vroman [1991], while the 0.71 estimate is from Current Population Survey data for the period 1977–1987 reported in Blank and Card [1991].

3. See Burtless [1983], Burtless and Saks [1984], Kane [1988], and Corson and Nicholson [1988]. Gritz and MaCurdy [1989] and McCall [1995] examine the effect of benefit levels on recipiency, while Budd and McCall [1997] explore the role of unions.

4. The numerator is the average weekly number of insured unemployed, which includes only those claiming benefits through regular state programs, including those serving a waiting period. This number is from U. S. Department of Labor [1995]. The denominator is simply the average weekly number of unem-

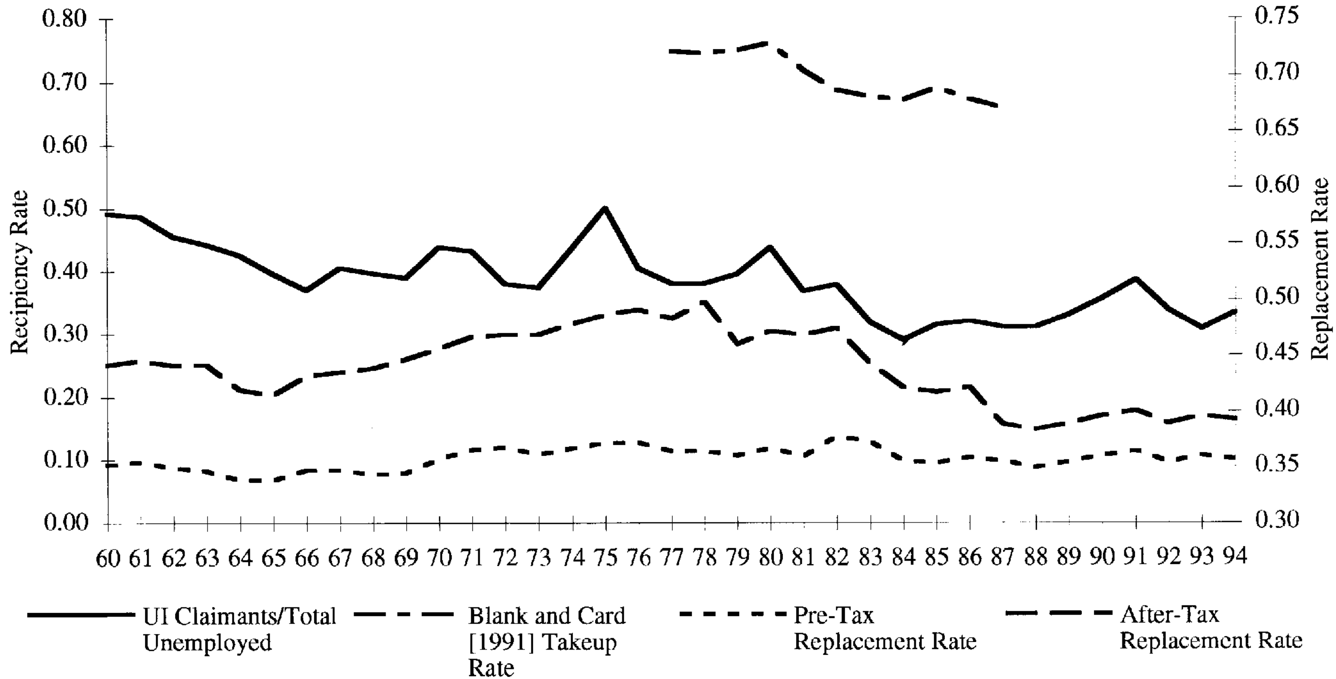

FIGURE I
UI Recipiency Rates and Replacement Rates, 1960–1994

Total unemployed from U. S. Department of Labor [Labstat Data Base]. UI claimants and pretax replacement rate from U. S. Department of Labor [1995]. After-tax replacement rate is from authors' calculations; see text for details.

stable until dropping again in the early 1980s.[5] The sources of decline in the earlier years were easily identifiable demographic and program changes,[6] but the most recent decline in the 1980s has puzzled researchers. Blank and Card [1991] conclude that no part of this most recent decline can be attributed to changes in eligibility for UI, but rather it can entirely be attributed to a decline in takeup among eligibles. They adjust IU/TU for eligibility to create a takeup series for the years 1977 to 1987, which is also plotted in Figure I. While exhibiting a slightly different cyclical pattern, the overall trend over this time period is similar to that of the IU/TU series.[7] While Blank and Card find important roles for shifting patterns of employment toward states with lower takeup rates and for declining unionization, over one-quarter of the nine-percentage-point decline between the late 1970s and the mid- to late 1980s remains unexplained.

During this same time period, however, one of the most important recent changes in the U. S. UI program occurred as UI benefits became subject to income taxes. Prior to 1979 benefits were not taxed at the federal level,[8] but beginning in 1979, benefits were taxable for single filers with income over $20,000 and married taxpayers filing jointly with income over $25,000. In September of 1982 these cutoffs were lowered to $12,000 and $18,000, respectively, effective retroactively to January 1982, and in 1987 benefits became fully taxable for all recipients. Given that previous research has usually found a positive effect of the replacement rate on takeup, it is reasonable to assume that this decline in the after-tax value of benefits could lead to a decline in takeup. The time-series evidence on this hypothesis is clearly suggestive. Figure I also plots both the pretax and after-tax replacement rates.[9] As can clearly be seen, the after-tax replace-

ployed civilians in the labor force and is from U. S. Department of Labor [Labstat Data Base].

5. A simple linear trend fit through the full series declines by about 0.4 percentage point per year. By decade, trends fit through 1960 to 1969 decline by 1.2 and through 1980 to 1989 by 1.1 percentage point per year, while a trend through 1970 to 1979 is insignificantly different from zero.

6. See Burtless and Saks [1984].

7. A trend fit through the Blank and Card [1991] series declines by 1.1 percentage point per year, as does a trend fit through the IU/TU series for the corresponding years.

8. States also differed in their treatment of UI during our sample period. In some states it was fully taxed, in some completely untaxed, while other states followed the federal treatment of UI.

9. The pretax replacement rate is not a true average replacement rate, since it is calculated as the average weekly benefit for a UI recipient divided by the average weekly wage for all covered workers (recipients and nonrecipients). It is

ment rate fell sharply over the 1980s with the inclusion of UI benefits in the income tax base. However, simply inferring a response to taxation from past estimates of the benefit elasticity may be misleading, given that individuals may respond differently to taxes than to benefits. Also, past estimates were computed using aggregate or survey data that only roughly measure benefit levels. Thus, to date the evidence is indirect and potentially biased.

In this paper we use administrative data to study the determinants of individual takeup decisions and to examine directly whether recent changes in the tax treatment of UI benefits can help explain the takeup decline. The data are well suited to the task, since our sample period surrounds one of the major tax changes and also includes a substantial part of the period of declining takeup. Additionally, the administrative data allow us to accurately assign the potential level and duration of benefits to workers separating from their employers, and then to examine whether or not they receive UI. We find that income taxation of benefits significantly reduces takeup and can account for almost all of the previously unexplained part of the decline in takeup over the early 1980s. We also find a strong positive effect of the benefit level on takeup, as well as a positive effect of the potential duration of benefits. These results confirm our economic predictions about how potential claimants respond to the generosity of benefits and their tax treatment. The estimated behavioral effects are also important for the design of UI programs since they determine the cost of program changes and their likely effect on unemployment.[10]

We begin in the next section by outlining the key features of the U. S. UI system, followed by a simple theoretical model of the

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a standard measure of system generosity, however, as reported in U. S. Department of Labor [1995]. The after-tax replacement rate is an estimate obtained in the following manner. First, for each year a marginal income tax rate (τ_y) is calculated using the employee part of the social security tax rate reported in the Committee on Ways and Means [1996] and the average marginal federal income tax rate as calculated by Barro and Sahasakul [1983] and updated by the authors. Next, a marginal UI benefit tax rate (τ_b) is calculated. Prior to the beginning of UI taxation, this tax rate is zero, while after full taxation the rate is taken to be the average marginal federal income tax rate. For the years of partial taxation, we use our data to simulate the fractions subject to UI taxation and compute their average marginal tax rates, in order to calculate $(1 - \tau_b)/(1 - \tau_y)$. The full after-tax series is then computed as pretax* $(1 - \tau_b)/(1 - \tau_y)$. In the figure (though not in the empirical work below) we ignore state taxes, but this exclusion should not greatly affect the basic pattern shown.

10. See Meyer [1995, 1996] for studies where the response of UI takeup to benefit parameters is central to evaluations of policy changes.

takeup decision in Section III. This model highlights the main economic determinants of takeup that we later analyze. Section IV then describes our data, while Section V discusses our empirical methods and main results. In Section VI we focus directly on the role of taxation in the recent decline in takeup. Section VII discusses some further implications of our results and concludes.

II. Background on the U. S. Unemployment Insurance System

While state UI systems differ in many dimensions, the eligibility and benefit rules in the six states we use in our analysis are typical of U. S. systems. First, a claimant must meet a monetary eligibility requirement; i.e., the worker must have accumulated a sufficient amount of earnings during a one-year base period prior to the separation. Our six states follow the usual rule of defining this base period to be the first four of the last five quarters completed prior to a claim. Within the base period the highest amount of earnings in any one-quarter is referred to as high quarter wages (HQW), while total earnings in the four quarters are known as base period earnings (BPE). Monetary eligibility then typically requires HQW to be above a given level and BPE to be at least 1.25 or 1.5 times HQW. In addition to satisfying monetary eligibility, a worker must also meet additional requirements, known as nonmonetary eligibility. The main nonmonetary requirement is that a worker must not have quit her last job without good cause, although there are sometimes provisions allowing UI receipt after a lengthy waiting period or an intervening period of work. In addition, claimants must search and be available for work, and must not have been fired for cause. The waiting period before eligible claimants begin receiving benefits is just one week in all our states.

For claimants satisfying all eligibility requirements, each state determines the weekly benefit amount (WBA) based on a claimant's work history in the base period. A typical benefit formula sets the WBA to be between 1/20 and 1/26 of HQW, subject to a minimum and maximum benefit level. The maximum benefit level is often reached by people with only moderately high earnings, resulting in about 45 percent of eligibles qualifying for the maximum WBA. Thus, while most formulas imply replacement rates of between 50 and 60 percent of usual wages, average replacement rates are somewhat lower. These benefits are of limited duration, however, with the potential duration (PD) of receipt proportional to the ratio of BPE to HQW. The PD is also subject to a maximum, which in our states is 26 weeks. Somewhat less than half of eligibles do not qualify for the maximum number of weeks because they have had irregular work histories (i.e., a low value of BPE/HQW). During periods of high unemployment, special programs may extend the duration of benefits beyond the regular state maximum. For parts of our sample period the Federal-state extended benefits (EB) program lengthened benefits up to 50 percent beyond the state regular duration, while the Federal Supplemental Compensation (FSC) program extended benefits up to sixteen weeks, but generally fewer weeks. The periods during which these programs were available and the length of the benefits provided depended on the state insured unemployment rate over the past thirteen weeks.

III. A Simple Model of UI Takeup

This section provides a theoretical model of the takeup decision and discusses some of its implications which we then test below. We suppose that 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 that they do not apply because they expect a short spell. As can be seen in Table I, 37 percent of those who believe they are eligible and do not apply indicate that they do not apply because they expect to get another job soon or to 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 nonapplicant and $U(y) - c$ for an applicant. For simplicity, the period is length one, the length of unemployment is λ, and the potential duration of benefits is d. The after-tax wage is w, and the after-tax unemployment benefit is b. Assume that a potential applicant takes the cumulative distribution of unemployment spell lengths that she could experience to be $F(\lambda)$. Finally, assume that the application cost varies across individuals so that $c = C + \varepsilon$, where

TABLE I
REASON FOR NOT APPLYING FOR UI BENEFITS IN CURRENT UNEMPLOYMENT SPELL, JOB LOSERS AND LEAVERS ELIGIBLE FOR UI

Reason for not applying for UI	Number in thousands	Percent
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	1117	100.00

The table is based on self-reported eligibility and is derived from Vroman [1991], Table 4. The figures represent population estimates of responses to the following question from a special CPS supplement administered in May, August, and November 1989, and February 1990: "What is the main reason . . . hasn't applied for unemployment compensation since . . . last job?" The population estimates are obtained using the CPS weights.

ε is a continuously distributed random variable, with c.d.f. L. The expected utility of an individual who does not apply is thus

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

while the expected utility of an applicant is

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

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

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

The implied probability of applying for UI is therefore

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

The effects of changes in the key individual and program characteristics can easily be determined by differentiating this probability. Higher UI benefits (or lower taxes on these benefits) raise the takeup probability, while lower application costs increase the probability of application. An increase in the potential duration of benefits increases the probability, but has a smaller effect than the same percentage change in the benefit amount unless all applicants believe they will be unemployed for at least as long as the potential duration of benefits. An increase in the wage (or a decrease in the tax rate on earnings) decreases the application probability as long as U'' is negative. One can also show that rightward shifts in the distribution of expected unemployment spell lengths will increase the application probability. We test all of these predictions (except for the last one) in Section V.

IV. The Data

Our data were collected in the late 1970s and early 1980s as part of the Continuous Wage and Benefit History (CWBH) project and are administrative records on individuals from the UI systems of six states. A longitudinal random sample was created by selecting all records for specific Social Security number ending digits. The resulting sample is thus a random sample at all points in time and allows workers to be matched over time. The states, time periods, and sampling rates are reported in Table II. Two types of records are available: quarterly wage records and UI claims records. The wage records include earnings at each employer during a quarter as well as such characteristics of the employer as firm employment and industry. Person and firm identifiers on these records allow us to create job-match histories and thus to identify separations. The UI records include weekly information on UI benefits received, which allow us to determine whether the separations result in UI receipt. A drawback to the data is that separations which are followed by a return to the same job without a full calendar quarter intervening will not be observed. However, the UI claims records allow us to detect short temporary separations that result in UI receipt. Some additional difficulties with identifying layoffs and our solutions are discussed in detail below.

IV.A. Benefit, Tax, and Previous Earnings Variables

Since these wage records contain the same earnings information used by the states, we follow state rules to determine monetary eligibility and to calculate the WBA and PD for which each individual would be eligible. To make this calculation, we first

TABLE II
SAMPLING RATES FOR CONTINUOUS WAGE AND BENEFIT HISTORY DATA AND STATE SHARES AND TIME PERIODS FOR STUDY SAMPLES

State	Sampling rate	Period used in study	Share of full sample	Share of subsample 1	Share of subsample 2
Georgia	0.10	79:2–83:4	0.287	0.313	0.319
Idaho	0.20	79:4–81:4	0.086	0.080	0.057
Louisiana	0.10	81:4–84:1	0.092	0.086	0.092
Missouri	0.05	79:2–82:4	0.245	0.245	0.251
New Mexico	0.20	81:3–83:4	0.141	0.118	0.111
South Carolina	0.20	81:4–83:4	0.148	0.158	0.170

The full sample consists of all separations. Subsample 1 removes separations likely to be spurious transitions or job-to-job quits, while subsample 2 further restricts that sample to separations from firms losing at least 5 percent of employment and five employees.

compute BPE and HQW for all employees separating from a firm, assuming that the quarter of separation is the quarter an individual files for benefits. Based on the UI claims records, we then note whether a person who was monetarily eligible and separated from her employer received UI benefits.

In addition to WBA and PD, the main variables that we use to explain whether or not someone received UI benefits are the income tax rate on earnings and on UI benefits. The model of Section III posits that workers respond to the after-tax benefit, $WBA(1 - \tau_b)$, and the after-tax wage, $W(1 - \tau_y)$, where τ_b and τ_y are the marginal tax rates on UI benefits and earned income, respectively. Since we generally use these variables in logarithms, we enter $\ln(WBA)$, $\ln(1 - \tau_b)$, and $\ln(1 - \tau_y)$ as separate explanatory variables. This specification allows workers to respond to taxes in a different way than they respond to benefits since taxes may be imperfectly perceived.[11] Recall that increases in the after-tax wage were predicted to decrease the application probability, so that 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, because they also measure other factors such as labor force attachment, we do not focus on their effect. Most of the past studies have focused on the replacement rate as the key benefit response variable, although there is no strong theoretical basis for choosing between

11. See Rosen [1976] and Solon [1985] for other papers that allow taxes to have a different effect from benefits or wages.

the replacement rate and the benefit level. Thus, we use a more flexible specification that allows the components of the after-tax replacement rate to be entered separately. This issue is discussed in more detail below.

To calculate tax rates, we approximate taxable income using BPE and apply the relevant tax schedules for a single filer with one exemption claiming the standard deduction. We use this approximation since we do not know family income or filing status. Given this assumption, we compute federal and state income taxes and OASDI.[12] The difference between the tax rates on earnings and benefits is due to benefits not being subject to OASDI, the changing federal income tax treatment of UI, and the differing and changing tax treatment of UI by the states.

As noted above, the UI weekly benefit and potential duration are determined by an individual's earnings history. Unless this history is carefully conditioned upon, it will be difficult to disentangle the effects of UI from those of past earnings.[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. While we might expect measures of past earnings to influence takeup, as they would capture commitment to the labor force as well as the degree of seasonality of a person's job, 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 piecewise linear continuous function of two variables) in *HQW* and *BPE*/*HQW*.[14] We use the earnings measures *HQW* and *BPE*/*HQW* because the *WBA* and *PD* are proportional to these two variables, subject to minima and maxima. We then choose quantiles of these variables as knot points to define our spline. The result is a set of variables that form a flexible function that controls for past earnings. Formally, let the $T - 1$ selected quantiles of $\ln(HQW)$

12. 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. In the estimates reported here, we include only the employee half of the OASDI tax in τ_y. When we include both halves, the only appreciable change in the results is a somewhat lower income tax coefficient.

13. This point has been made by Welch [1977] and has been emphasized by Meyer [1989, 1992].

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

be KH_2, KH_3, . . . , KH_T and the selected quantiles of ln(*BPE*/*HQW*) be KR_2, KR_3, . . . , KR_T . Then we enter as regressors the $2T + T^2$ variables: H_1, . . . , H_T, R_1, . . . , R_T, RH_{11}, RH_{12}, RH_{21}, . . . , RH_{TT} , where $H_1 = \ln(HQW)$, $H_i = \max(0, H_1 - KH_i)$, $i = 2$, . . . , T, $R_1 = \ln(BPE/HQW)$, $R_i = \max(0, R_1 - KR_i)$, $i = 2, \ldots, T$, and $RH_{ij} = H_i{*}R_j$, for $i = 1, \ldots, T$ and $j = 1, \ldots, T$. Other explanatory variables include indicators for firm size and industry, along with a complete set of state and time interactions.

Thus, we have several advantages over much of the past work. First, using administrative data allows us to accurately determine UI monetary eligibility. To obtain a lower bound on the misclassification of eligibility using survey data sets such as the Current Population Survey (CPS), we apply the Blank and Card [1991] methodology to our administrative data. For their sample of the unemployed in March of various years, the data limitations of the CPS force them to use the previous calendar year as the four-quarter-long base period for qualifying earnings. Unfortunately, in nearly all states the base period is at least a quarter or more earlier. We assume that this difference in timing is the only source of error in the CPS, and we make conservative assumptions about the timing of the base period.[15] Using our data for six states, we find that at least 22 percent of those eligible using CPS data are truly monetarily ineligible. The use of administrative data also allows us to estimate the impact of changes in potential duration of benefits, not just of the benefit level. Additionally, we investigate the effect of the after-tax value of benefits, rather than the pretax value used in past work. Given the change in the tax treatment of UI and the continuing puzzle of the decline in takeup over this same period, accounting for taxes is an important consideration. Finally, we are also able to carefully condition upon past earnings.

IV.B. 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 several subsamples that remove observations that are likely to

15. This calculation assumes that all of the half of the Blank and Card sample that did not begin their unemployment spell in the same calendar year, began their spells in the previous quarter. Since the error increases as the true base period moves farther away in time from the calendar year, this calculation will understate the error.

be quits or spurious transitions. First, we eliminate those cases where an individual has no reported earnings in one or two quarters, but where earnings before and after this gap are nearly equal, since these cases are likely due to a firm neglecting to report wages.[16] Additionally, we exclude separations that result in only a slight drop in usual earnings and are therefore likely to be quits resulting in a quick job-to-job move without unemployment.[17] Since these restrictions require comparing earnings before and after the separation, we also drop those cases with no subsequent earnings. Note that this procedure will also result in our screening out quits which are withdrawals from the labor force.

Starting from our full sample of 980,286 monetarily eligible separations, then, we obtain a first subsample of 505,808 separations by dropping 32,821 observations that are likely spurious transitions, 264,612 observations that are likely quits to move job to job, and 177,045 observations with no subsequent earnings that may be quits to exit the labor force. We create a second subsample of 113,088 separations by retaining only those observations from firms that experienced a decline of at least 5 percent which consisted of at least five lost employees. Such a subsample is likely to represent mainly separations due to mass layoff.[18] However, note that this subsample will also be weighted more toward separations from larger firms. Means for the key variables for the full sample and the two subsamples are presented in Table III.

V. Empirical Determinants of UI Takeup

We begin our investigation into the determinants of UI takeup by estimating linear probability models on the full sample.[19] Our main specifications are of the form,

16. The specific criterion is that real quarterly earnings before and after the gap are within 5 percent of each other.

17. Here the specific criterion is that the loss in earnings is less than 2/13 of the previous quarter earnings (i.e., equivalent to under two weeks unemployment).

18. We tried several other definitions and found qualitatively similar results.

19. Average derivatives calculated from logit models of representative specifications are virtually identical to the linear probability coefficients we present. Because of the computational problems in estimating logit or probit models on close to one million observations with up to several hundred explanatory variables, we estimate linear probability models here.

TABLE III
SAMPLE MEANS

	Full sample	Subsample 1	Subsample 2
Percent receiving UI	0.236	0.391	0.536
	(0.425)	(0.488)	(0.499)
Weekly UI benefit amount	70.410	71.450	74.390
	(24.998)	(24.077)	(23.583)
Marginal tax rate on UI benefits	0.065	0.059	0.075
	(0.139)	(0.133)	(0.144)
Marginal tax rate on income	0.274	0.279	0.295
	(0.121)	(0.116)	(0.113)
Potential duration of UI benefits	29.972	29.987	31.028
	(9.573)	(9.749)	(9.902)
High quarter earnings	2,618.700	2,624.030	2,816.370
	(2,587.720)	(2,512.890)	(2,240.950)
Base period earnings	7,664.470	7,686.820	8,393.470
	(6,990.010)	(6,626.700)	(6,264.220)
Number of observations	980,286	505,808	113,088

The full sample consists of all separations. Subsample 1 removes separations likely to be spurious transitions or job-to-job quits, while subsample 2 further restricts that sample to separations from firms losing at least 5 percent of employment and five employees. Dollar values are indexed to 1978:3. Standard deviations are in parentheses.

(1)
$$Y_{it} = \beta_1 \ln(WBA)_{it} + \beta_2 \ln(1 - \tau_b)_{it} + \beta_3 \ln(1 - \tau_y)_{it} + \beta_4 \ln(PD)_{it} + E'_{it}\beta_5 + S'_i\beta_6 + Q'_t\beta_7 + SQ'_{it}\beta_8 + I'_{it}\beta_9 + F'_{it}\beta_{10} + \varepsilon_{it},$$

where $Y_{it} = 1$ if individual i separating in quarter t receives UI, and 0 otherwise. E_{it} is a vector of flexible earnings controls, specifically variables that form a bilinear spline in ln(HQW) and ln(BPE/HQW), S_i is a vector of state dummy variables, Q_t is a vector of calendar quarter dummy variables, SQ_{it} is a vector of interactions of state and quarter dummy variables, I_{it} is a vector of industry dummy variables, and F_{it} is a vector of firm size dummy variables. All other variables were previously defined. While our main specifications are of the form (1), Table IV presents the results of several alternative specifications that evaluate the importance of state, quarter, and flexible earnings controls. Columns (1) and (2) of Table IV mimic the typical specifications seen in the literature by including only time effects or state by quarter effects, along with a simple control for BPE, rather than flexible earnings controls. Looking first at column (1), we see that while all coefficients are of the predicted sign, rather than being of similar magnitudes, the coefficients of 0.220 on $\ln(1 - \tau_b)$ and

TABLE IV
FULL SAMPLE ESTIMATES OF PROBABILITY OF UI RECEIPT AFTER A SEPARATION

	(1)	(2)	(3)	(4)	(5)
ln(weekly UI benefit amount)	0.092	0.142	0.197	0.190	0.230
	(0.001)	(0.002)	(0.005)	(0.005)	(0.013)
ln(1 − marginal tax on UI benefits)	0.220	0.167	0.126	0.105	0.134
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
ln(1 − marginal tax on income)	−0.437	−0.106	−0.245	−0.196	−0.108
	(0.007)	(0.008)	(0.011)	(0.013)	(0.012)
ln(potential duration of UI benefits)	0.028	0.093	0.062	0.066	0.105
	(0.002)	(0.002)	(0.004)	(0.004)	(0.012)
State effects	NO	YES	YES	YES	YES
Calendar quarter effects	NO	YES	YES	YES	YES
State by calendar quarter effects	NO	YES	YES	YES	YES
Earnings spline	NO	NO	24-piece	80-piece	by state
Number of observations	980,286	980,286	980,286	980,286	980,286
Percent receiving UI	0.236	0.236	0.236	0.236	0.236
Adjusted R^2	0.112	0.135	0.144	0.146	0.154

The full sample consists of all separations. The dependent variable is 1 if the separation resulted in UI receipt, 0 otherwise. All specifications include controls for industry group firm size class. If no earnings spline is included, then base period earnings is included. The earnings spline by state is a 24-piece spline interacted with state. Standard errors are in parentheses.

of -0.437 on $\ln(1 - \tau_y)$ are more than two and four times the coefficient of 0.092 on ln(*WBA*). The addition of state and state by quarter indicator variables[20] has a major effect on $\ln(1 - \tau_y)$, dropping the coefficient to -0.106. Additionally, there is a slight drop in the $\ln(1 - \tau_b)$ coefficient to 0.167 and a slight increase to 0.142 in that on ln(*WBA*). Based on the average takeup rate of 0.24 in this sample, (1) and (2) would imply benefit takeup elasticities of about 0.39 and 0.59, respectively. These benefit elasticities are close to the range of estimates obtained by others.[21]

As was noted above, though, it is important to disentangle the effects of UI from those of past earnings, something none of the past studies have done well. Column (3) more flexibly controls for past earnings by entering a 24-piece bilinear spline in ln(*HQW*) and ln(*BPE*/*HQW*), using quartiles as the knot points, and column (4) expands the bilinear spline to 80 pieces by using

20. Including just state indicator variables gives similar results to those that also include state*quarter.

21. See Corson and Nicholson [1988] and Blank and Card [1991] who use aggregate data to obtain elasticities from 0.23 to 0.56 and 0.32 to 0.58, respectively, and McCall [1994] who uses microdata to obtain elasticities from 0.26 to 0.35.

octiles as the knot points. The flexible earnings controls remove any differences in takeup propensities across people with different levels of or variability in earnings. It is assumed, for now, that any such effects of past earnings are the same across states. The identification of benefit level and potential duration effects is now purely due to differences across states in their relative treatment of individuals with different earnings histories. Identification does not come from the overall generosity of the state or effects of earnings histories that are common to all states.

While the expansion from a 24-piece to an 80-piece spline has very little effect on the estimated coefficients, suggesting that additional expansion is unnecessary, the initial move to a spline specification does affect the estimates somewhat. The coefficient of 0.190 on ln(*WBA*) indicates that there is a slightly stronger positive effect of benefits on takeup than was found previously. While somewhat less precisely estimated, the −0.196 coefficient on $\ln(1 - \tau_y)$ shows that there is an approximately equal effect of a decline in after-tax earnings. Note that if it is the replacement rate that matters to individuals, as implicitly assumed by much of the past work, then incorporation of tax considerations as well would imply this result. However, it would also imply a similar sized effect on $\ln(1 - \tau_b)$. At 0.105, though, this coefficient is just over half the size of the benefit effect. While measurement error in assigning the tax status of the claimants' benefits may be responsible for this difference, it may also be the case that individuals were less aware of the tax rate on benefits than of that on other income, and hence did not fully respond to the tax. Finally, there is a positive effect on takeup of potential duration, as indicated by the coefficient of 0.066 on ln(*PD*). As predicted, this coefficient is smaller than that on ln(*WBA*).

Model (5) presents an alternative form of earnings control by interacting the 24-piece bilinear spline with state. Such a specification is similar in spirit to a "natural experiment" approach, but is not limited to looking at only a small part of the earnings distribution or the time periods immediately before and after benefit changes. By including a possibly different flexible function of past earnings for each state as well as separate time dummies for each state, the remaining variation in benefits and taxes is due to changes in these schedules over time. More precisely, the remaining variation is the differential effect of these changes on different earnings groups in the states. This approach differs from simple differences-in-differences approaches in that it uses

TABLE V
SUBSAMPLE ESTIMATES OF PROBABILITY OF UI RECEIPT AFTER A SEPARATION

	Subsample 1		Subsample 2	
	(1)	(2)	(3)	(4)
ln(weekly UI benefit amount)	0.203	0.239	0.180	0.003
	(0.008)	(0.020)	(0.018)	(0.044)
ln(1 − marginal tax on UI benefits)	0.110	0.154	0.111	0.156
	(0.007)	(0.008)	(0.015)	(0.016)
ln(1 − marginal tax on income)	−0.230	−0.126	−0.227	−0.174
	(0.020)	(0.020)	(0.048)	(0.047)
ln(potential duration of UI benefits)	0.072	0.078	0.053	0.061
	(0.006)	(0.018)	(0.015)	(0.041)
Earnings spline by state	NO	YES	NO	YES
Number of observations	505,808	505,808	113,088	113,088
Percent receiving UI	0.391	0.391	0.536	0.536
Adjusted R^2	0.215	0.223	0.208	0.214

Subsample 1 removes those separations likely to be spurious transitions or job-to-job quits from the full sample of all separations, while subsample 2 further restricts that sample to separations from firms losing at least 5 percent of employment and 5 employees. The dependent variable is 1 if the separation resulted in UI receipt, 0 otherwise. All specifications include controls for state, calendar quarter, state by calendar quarter, industry group, and firm size class. If the earnings spline is not by state, it is an 80-piece spline, while the earnings spline by state is a 24-piece spline interacted with state. Standard errors are in parentheses.

the entire range of earnings and the entire sample period and takes the responses to be proportional to the magnitude of the changes. As can be seen in the table, the additional controls do not greatly alter our conclusions about the effects of after-tax benefits. However, the coefficient on $\ln(1 - \tau_y)$ is cut almost in half, to −0.108, making it much closer to the coefficient on $\ln(1 - \tau_b)$, but farther from the coefficient on $\ln(WBA)$. The increase in the estimated benefit effect due to the inclusion of the earnings splines suggests that past work may have underestimated the impact of benefits on takeup.[22]

Because the full sample likely includes some quits and spurious separations, Table V repeats these specifications for each of our subsamples, which have higher takeup rates of 0.39 and 0.54, respectively.[23] Looking at (1) and (2), which use the first subsam-

22. While (4) and (5) imply very large benefit elasticities of between 0.81 and 0.97, the takeup rates upon which these elasticity calculations are based are likely understated due to the possible inclusion of quits and spurious separations in the base.

23. Recall that the selection criterion for the final subsample implies that the separations are from bigger firms, on average. Since firm size is strongly positively correlated with UI takeup, the increase in the takeup rate for this sample cannot be fully attributed to screening out quits.

ple, we see coefficients very similar in magnitude to those from (4) and (5) in Table IV. In (3), which uses the second subsample, we again see that the magnitudes of all the coefficients remain stable. In (4), though, the estimated coefficient on ln(*WBA*) falls dramatically, while all others are similar to those in (2). It may be the case that in this highly restricted sample, the remaining variation in benefits is insufficient to identify an effect on takeup. By contrast, the major legislative changes in benefit taxation allow this effect to be estimated precisely across all specifications and samples.

While our data include excellent information on UI program parameters and some firm characteristics, a weakness of the data is a lack of individual characteristics. Characteristics such as age or education might affect the likelihood of filing for UI, and if correlated with UI program variables, could lead to biased estimates. We can remove these characteristics and their influence on takeup by using the Chamberlain [1980] fixed-effects logit model.[24] This model essentially differences out all characteristics of individuals that do not vary over the time period examined. Only individuals with more than one separation during our sample period contribute to the estimation of the parameters using this method. Results using this approach are reported in Table VI for each of our samples. Because of computational limitations, we use a 10 percent random sample of our data and replace the state*quarter dummy variables with state unemployment rate and insured unemployment rate controls. For the full sample shown in column (1), the marginal effects of both the benefit level, at 0.235, and the benefit tax, at 0.108, are fairly close to the numbers in columns (4) and (5) of Table IV. However, the potential duration effect of 0.031 is less than half the size of the previous estimates, while the income tax effect of 0.008 is wrong-signed, but imprecisely measured. Turning to the subsample 1 results shown in column (2) of Table VI, the estimated marginal effects are consistently a bit higher than those obtained in (1) and (2) of Table V, with the income tax effect again being wrong-signed. A similar conclusion holds for a comparison of column (3) of Table VI with (3) and (4) from Table V, but the small size of subsample

24. We use a fixed-effects logit model rather than a fixed-effects linear probability model because the introduction of fixed effects is likely to lead to many more predictions near zero or one, and in the case of the linear probability model outside the (0,1) interval. In this situation the potential biases from the linear functional form can be substantial.

TABLE VI
FIXED-EFFECT LOGIT ESTIMATES OF PROBABILITY OF UI RECEIPT AFTER A SEPARATION

	10 percent of full sample (1)	10 percent of subsample 1 (2)	10 percent of subsample 2 (3)
ln(weekly UI benefit amount)	1.294	1.291	3.352
	(0.230)	(0.339)	(1.189)
	[0.235]	[0.310]	[0.830]
ln(1 − marginal tax on UI benefits)	0.594	0.787	0.203
	(0.171)	(0.261)	(0.769)
	[0.108]	[0.189]	[0.050]
ln(1 − marginal tax on income)	0.042	0.533	−0.472
	(0.425)	(0.608)	(2.020)
	[0.008]	[0.128]	[−0.117]
ln(potential duration of UI benefits)	0.170	0.071	1.575
	(0.084)	(0.126)	(0.533)
	[0.031]	[0.170]	[0.390]
Number of observations in sample	120,461	62,722	13,709
Percent receiving UI	0.239	0.400	0.550
Number of observations used	52,491	25,239	2,316
Pseudo R^2	0.109	0.112	0.155

The full sample consists of all separations. Subsample 1 removes those separations likely to be spurious transitions or job-to-job quits from the full sample of all separations, while subsample 2 further restricts that sample to separations from firms losing at least 5 percent of employment and five employees. The results are for a random 10 percent of the individuals in each sample. The dependent variable is 1 if the separation resulted in UI receipt, 0 otherwise. Individuals always receiving or always not receiving UI are excluded. All specifications include a 24-piece earnings spline, controls for state, calendar quarter, state unemployment rate, state insured unemployment rate, industry group, and firm size class. Standard errors are in parentheses. Marginal effects calculated at the means are in brackets. The marginal effects are estimated as $\beta * (1 - p) * p$, where p is the percent receiving UI and β is the reported coefficient.

2 leads to all of the marginal effects being estimated imprecisely. In summary, the fixed-effects estimates yield a similar picture of the determinants of takeup, with the most precise estimates being quite close to, albeit generally slightly higher than, those obtained previously.

A final potential area of concern with our analyses is sample selection. Recall that short temporary layoffs that are not followed by UI receipt are not included in our sample. To evaluate whether this exclusion is likely to affect our estimates, we estimated the basic model for a sample of just permanent separations and for a sample of just temporary separations and compared the results with those of Tables IV and V. For the permanent separations the benefit effect was generally somewhat smaller than for the full sample, but still significantly different from zero, while the tax and potential duration effects were generally smaller and imprecisely estimated. For the temporary layoff sample, the benefit, benefit tax, and income tax effects were all generally somewhat larger in absolute value and strongly significant, while the potential duration effect was slightly smaller, but still significantly different from zero. It is probable, though, that differences in the results across these samples are due to individuals on temporary layoff being more familiar with their potential benefits and the tax treatment of UI, rather than a reflection of sample selection bias.[25]

A related sample selection issue is whether the labeling of a separation as a noneligible quit or as an eligible layoff is itself affected by UI. The UI system provides incentives to change either reported or actual reasons for a separation. For example, in order to qualify for UI, a worker who foresees a separation has the incentive not to quit, but to wait for a layoff, while an experience rated firm would prefer to induce a quit. Additionally, a worker has the incentive to report a separation as a layoff, while the firm has the incentive to fight a claim. While we do not condition on whether a separation is a layoff in the full sample, we eliminate most quits in creating our subsamples, leading to a potential sample selection problem. However, in early versions of the data we tried including variables for the experience rating incentives that might cause firms to contest claims and found no significant effect, although point estimates were of the expected sign. Thus, it is unlikely that these potential sources of sample selection issues have significantly biased the reported estimates.

Overall, then, the estimated coefficients are remarkably stable, implying that a 10 percent increase in the *WBA* would increase takeup by 2 to 2.5 percentage points, while a similar increase in *PD* would increase takeup by 0.5 to 1 percentage point. At the same time, a tax increase that decreased the value of after-tax benefits by 10 percent would lower takeup by 1 to 1.5 percentage point. The elasticities implied by these estimates, though, will depend upon the assumed base takeup rate. If we assume takeup to be between 0.4 and 0.6, our estimates would imply a benefit elasticity between 0.33 and 0.60.[26]

25. See Meyer and Rosenbaum [1996] for evidence on the repeat use of UI by individuals on temporary layoff.

26. Weighting the average 1977–1987 state takeup rates calculated by Blank and Card [1991] by our state sample fractions would imply a takeup rate of 0.60 in our sample. However, since several of their state takeup rates are over 1, it

VI. The Role of Benefit Taxation in the Recent Decline of UI Receipt

The above results imply that the phased-in taxation of benefits, which began in 1979 and was completed in 1987, contributed to the recent decline in takeup. We use several approaches to assess the quantitative importance of taxation in the decline. Our first approach examines the effect of taxation of benefits on trends in recipiency. Here, we estimate models similar to those above, but rather than complete time effects, we enter state-specific trends. Three quarterly unemployment rates, national, state, and state insured, are included to capture nontrending time effects.[27] We then compare the average estimated time trend with and without the inclusion of $\ln(1 - \tau_b)$. In all three samples the trend increases by 0.0007 when we account for the taxation of benefits. Thus, over the approximately twenty quarters covered by our data, UI taxation would explain a drop of 0.014, or about a 1.4 percentage-point decline in UI recipiency. A larger decline of about 2.3 percentage points would be predicted over the full 1979 to 1987 period of the phase-in.

We should also note that only in the full sample are the initial trends estimated to be negative. The relatively short time period of our data, which includes the bottom of a major recession, and the availability of only six states, makes it inappropriate for measuring broad trends. However, since the time period does cover a major change in the tax treatment of benefits, it is well suited for studying the impact of tax changes on takeup. Thus, our next two approaches take advantage of the strengths of our data. In our second method we use a "natural experiment" approach that directly estimates the impact of the tax changes on takeup in our sample.[28] In our third and final method we use the estimated benefit tax coefficient from Tables IV and V to simulate the effect over time of the phase-in.

While the tax status of individuals with incomes above \$20,000 or incomes below \$12,000 was unchanged during the

may be reasonable to consider this estimate to be an upper bound. While our first subsample likely does not completely remove all nonmonetarily ineligible separations, we feel that erring on this side is preferable to the removal of valid separations from smaller firms that occurs in the second subsample. Thus, the takeup rate of 0.39 in this sample can be considered a lower bound.

27. Note that substituting unemployment rates for time dummies in the main specifications of Tables IV and V has very little effect on the benefit and UI tax variables.

28. Our approach is similar to Classen [1979] and Meyer [1989], but is probably closest to Solon [1985].

time period covered by our data, UI benefits for the income group in the middle went from untaxed to fully taxed with the law change in the third quarter of 1982 (1982:3). Thus, this middle group can be thought of as an experimental group, while the others serve as a comparison group, allowing us to implement a "difference-in differences" or "natural experiment" specification.[29] Specifically, we estimate a linear probability model similar to that of (5) in Table IV and (1) and (3) in Table V, but without the marginal UI tax rate. Instead, we include indicators for post-1982:3, the middle income group, and the interaction of these two variables. The coefficient on the interaction term then represents the percentage-point decline in the probability of UI receipt attributable to taxation. The results for the full sample and for both subsamples are shown in Table VII. In all cases, the coefficient on the interaction is negative and significant. While point estimates for the full sample and the second subsample are very similar, at −0.019 and −0.017, that for the first subsample is much smaller, at −0.008. These coefficients indicate a substantial drop in takeup for the group whose UI benefits were newly subject to the income tax.

Finally, to carry out our simulation of the time series impact of taxation, we begin with the subsample of separations from the quarter for which we have data for all six states.[30] We first calculate the income tax rate that each individual would face in each quarter from 1979:1 to 1987:1, given the Federal tax laws in place at that time.[31] We then calculate the proportion p_i in each of three groups: those who would have earnings above \$20,000 (the first group taxed), those below \$12,000 (the last group taxed), and those in between, for each quarter. The average marginal income tax rate τ_i for each group is then computed.[32] This information allows us to simulate the effect of the UI taxation policy in place

29. The middle income group is defined so that we use the largest group possible that satisfies two criteria. First, the same real base period earnings limits must be used in each quarter, and second, the implied nominal earnings limits must only include individuals whose benefits were made newly taxable by the 1982:3 law change. The need to satisfy these two criteria excludes some observations as does the requirement that the comparison group have fixed real earnings limits to maintain comparability over time. These exclusions account for the smaller samples used in this analysis.

30. The quarter is 1981:4. Within each state, average characteristics from this quarter appear similar to those from other quarters in the sample.

31. Taxable income is approximated as before, using base period earnings, but deflated/inflated to the proper time period.

32. As a simplification, for the entire period we used the state income tax rules in place in 1981.

TABLE VII
NATURAL EXPERIMENT ESTIMATES OF THE EFFECT OF BENEFIT TAXATION ON UI RECEIPT

	Full sample (1)	Subsample 1 (2)	Subsample 2 (3)
ln(weekly UI benefit amount)	0.205	0.227	0.200
	(0.005)	(0.009)	(0.019)
After 1982:3 passage of tax increase	−0.158	0.060	0.061
	(0.005)	(0.008)	(0.018)
Middle income group	0.005	−0.002	−0.031
	(0.003)	(0.005)	(0.011)
After increase*middle income	−0.019	−0.008	−0.017
	(0.002)	(0.004)	(0.007)
ln(1 − marginal tax on income)	−0.176	−0.227	−0.225
	(0.013)	(0.021)	(0.050)
ln(potential duration of UI benefits)	0.081	0.097	0.080
	(0.004)	(0.007)	(0.016)
Number of observations	863,220	442,545	96,163
Percent receiving UI	0.228	0.379	0.520
Adjusted R^2	0.144	0.215	0.211

The full sample consists of all separations. Subsample 1 removes from the sample separations likely to be spurious transitions or job-to-job quits, while subsample 2 further restricts that sample to separations from firms losing at least 5 percent of employment and five employees. The dependent variable is 1 if the separation resulted in UI receipt, 0 otherwise. All specifications include controls for state, calendar quarter, state by calendar quarter, industry group, firm size class, and an 80-piece earnings spline. The middle income group is defined so that we use the largest group possible that satisfies two criteria. First, the same real base period earnings limits must be used in each quarter, and second, the implied nominal earnings limits must only include individuals whose benefits were made newly taxable by the 1982:3 law change. The need to satisfy these two criteria excludes some observations as does the requirement that the comparison group have fixed real earnings limits to maintain comparability over time. These exclusions account for the smaller sample sizes used in this analysis. Standard errors are in parentheses.

at the time, relative to no taxation, as $\sum 0.13*\ln(1 - \tau_i)*p_i$, where 0.13 is a representative benefit tax coefficient from Tables IV and V.

The simulation implies that in 1979:1, the immediate effect of taxing the top group was to decrease takeup by just 0.3 percentage points. As "bracket-creep" pushed more people into the taxable range, though, the size of this decline increased to 0.9 percentage points by 1981:4. The 1982:4 expansion of UI taxation then doubled this decline to 1.9. Decreases in marginal tax rates combined with bracket creep to hold the decline fairly steady until 1987:1, when completion of taxation implied a 2.6 percentage-point drop in takeup relative to no taxation. Thus, the results of the simulation are generally in accord with the previous results, so that all three methods suggest that benefit taxation is likely responsible for about a two-percentage-point decline in takeup.

Recalling that Blank and Card [1991] could not explain just over a quarter of an approximately nine-percentage-point decline,[33] or about 2.3 percentage points, over this time period, we can conclude that taxation of benefits may be fully responsible for the remaining drop.

VII. FURTHER IMPLICATIONS AND CONCLUSIONS

In this paper we investigate the determinants of UI takeup, using administrative data that allow us to accurately assign the level and duration of benefits faced by a potential claimant. Our estimates suggest that a 10 percent increase in the weekly benefit level would increase the takeup rate by between 2 and 2.5 percentage points. As might be expected, and as is predicted by our model, we find a smaller effect of the potential duration of benefits. In particular, we find that a 10 percent increase in the potential duration of benefits would increase the takeup rate by an additional 0.5 to 1 percentage point. The strength of this behavioral response has important implications for evaluating the effects of UI program parameters on program costs and outcomes. The elasticity of program costs with respect to the benefit level is the sum of the elasticities of unemployment incidence, UI takeup, and UI spell duration with respect to the benefit level. Thus, the finding of a substantial takeup elasticity means that this sum is likely much higher than previous work on duration or incidence alone would indicate.[34] In evaluating the impact of a change in available benefit levels or potential duration on program expenditures, it is important to incorporate these takeup effects. We should also note that endogenous takeup may mean past duration elasticities are biased.[35]

33. Table I in Blank and Card shows a decline in takeup from 74.9 percent in 1979 to 65.8 percent in 1987. The bulk of this decline occurs between 1980 and 1982, with takeup dropping from 76.1 to 68.7. Because the previously discussed problems with estimating monetary eligibility in the CPS imply that these takeup rates are estimated with error, and that this error increases with unemployment duration, one must be careful in interpreting these year-to-year fluctuations. In addition, different data sets and measures of takeup have a somewhat different time pattern. In general, though, the timing of the downward trend fits roughly with the timing of the major changes in the tax treatment of UI benefits.

34. Estimates of the effect of benefits on incidence are mixed, but centered around zero. See Card and Levine [1994], Anderson and Meyer [1994b], and Topel [1983, 1985]. Estimates of the duration elasticity are consistently positive, with recent estimates from Meyer [1990] and Katz and Meyer [1990] implying elasticities of about 0.7.

35. See Section 8 of Anderson and Meyer [1994a] for a formal argument and a discussion of the result.

Especially significant is our finding that individuals respond to the tax treatment of UI benefits, with our estimates implying that a tax change which lowers the after-tax value of UI benefits by 10 percent would decrease takeup by 1 to 1.5 percentage point. Based on several different approaches, we conclude that the phase-in of benefit taxation could be totally responsible for the previously unexplained portion of the decline in UI takeup over the early 1980s. Not only does this sensitivity to after-tax benefits help resolve this long-standing puzzle of the steep decline in UI recipiency, but it also has additional policy implications. For example, recently the Advisory Council on Unemployment Compensation [1996] recommended that UI benefits return to their tax-exempt status. Our results imply that the fiscal implications of such a change would include not just a decrease in tax revenue, but also an increase in UI expenditures.

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