# Systemic Disincentive Effects of the Unemployment Insurance Program

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# 1.0 Introduction

Unemployment Insurance (UI) provides short-term income support payments to eligible persons whose unemployment is caused by circumstances beyond their control. State UI Programs operate as Federal-state partnerships. States largely determine provisions affecting benefit levels and benefit duration as well as most provisions related to the UI payroll taxes paid by covered employers. The states also decide on the amounts to maintain in their trust funds at the U.S. Treasury, the source of monies to pay regular UI benefits. The Federal Government requires the states to designate a tax with an annual taxable wage base per employee of at least $7,000 and a maximum statutory tax rate of at least 5.4 percent of taxable wages. The Federal partner also imposes standards for state UI administrative performance, such as first payment promptness.

In most situations UI benefits are paid following a "clean" or nonprejudicial separation by the employer; e.g., a layoff. The support payment replaces part of the lost earnings from unemployment and is of limited duration. In the United States during nonrecessionary years, maximum potential benefit duration is 26 weeks for a given 52-week benefit year. For persons who use up (or exhaust) their full benefit entitlement, the only way additional benefits may be received is from renewed eligibility in a later benefit year obtained through additional work.

Benefit payments help support the income and living standards of the unemployed and provide automatic or built-in stability to the macro economy by automatically increasing during recessions. Payments during the strong labor markets of the late 1990s averaged about $20 billion annually. During 2002 and 2003 annual payments have exceeded $50 billion including more than $10 billion of temporary Federal benefits (Temporary Extended Unemployment Compensation or TEUC) in each year.

For some UI recipients, however, these same payments can lead to a prolonging of their time in benefit status and higher aggregate unemployment. The UI Program can adversely affect the labor market in other ways, such as increasing the volume of unemployment occurrences caused by employers. The combined effects on unemployment duration and unemployment occurrences cause the overall unemployment rate to be higher than it would otherwise be. Higher unemployment also implies, at least in the short run, lower balances in UI trust funds arising from both increased benefit payouts and reduced employment (and lower levels of payroll tax revenues). Lower balances then feed back upon employers as higher UI payroll tax rates arising through experience rating. Because most of the economic burden, or incidence, of payroll taxes ultimately rests with workers as lower wages, the higher unemployment and associated labor market distortions imply a reduction in the living standards of employed workers.

Past research has examined several facets of the effects of the UI Program on the functioning of the labor market. This report summarizes and critically assesses the relevant research conducted since the mid-1970s. Because the volume of research is extensive, the review will be selective, reflecting the author's judgement as to the most influential work. The review draws conclusions about the strength of research findings. It also identifies areas where our

understanding is too limited to draw strong conclusions. Finally, recommendations for further research are offered.

Some specific areas for further research are highlighted as particularly important. These include the following:

- Examine the effects of certain "natural experiments" reflecting large changes in benefit and tax provisions in states in recent years. Among the large changes in benefit provisions are the increase in weekly benefits in Indiana between 1991 and 1995 and the increases in California and Virginia of 2002-2003. The effects of UI tax holidays in the 1990s on employer use of temporary layoffs in Georgia, Kansas, and North Carolina should also be studied.

- Support for new research on the determinants of benefit duration in regular UI is recommended.

- Because there have been relatively few studies that estimated the effects of profiling on UI benefit duration, the analysis should be extended to other states. The extant point estimates are based on studies from nine states, with the results from Kentucky showing much larger effects than other studies. The Kentucky analysis should be replicated in other states.

More research on UI work test administration is also recommended.

## 2.0 A Taxonomy of Disincentive Effects

Paying unemployment benefits can affect labor market outcomes through several channels. Two that are the most obvious are: (1) the effects of payment levels on the duration of unemployment, and (2) the effects of potential benefit duration on actual benefit duration. Weekly payment levels are measured most commonly relative to weekly wages or as a replacement rate. Higher replacement rates reduce the reward from work and induce some claimants to prolong their spells of unemployment. In the language of labor economics, high replacement rates increase the appeal of not working (or leisure) since weeks in benefit status are compensated, albeit typically at a rate of half or less of the worker's weekly wage. As the replacement rate is higher, the reward from work (the gap between the weekly wage and the weekly benefit) decreases.

Several benefit provisions affect replacement rates. Replacement rates are higher when the minimum benefit is higher, the maximum benefit is higher, the statutory replacement rate is higher, the benefit schedule is weighted to give higher replacement to low-wage workers, and dependents' allowances are available. These differences in UI statutes have measurable effects on the inter-state variation in replacement rates.

In regular UI Programs, maximum potential benefit duration is limited to 26 weeks per benefit year. With legislation enacted during 2003 in Massachusetts and Washington, the UI Programs in all states will pay regular benefits for at most 26 weeks during nonrecessionary periods. During recessions potential benefit duration is extended under the Federal-state

Extended Benefits (EB) Program and/or under the provisions of temporary Federal benefits such as the TEUC Program of 2002-2003. Because maximum potential benefit duration has been stable in states for several years, research on the effects of increasing potential duration has frequently utilized data from periods when EB or temporary Federal benefit extensions have been in effect.

For each claimant of regular UI benefits there is a direct link between previous earnings and the maximum potential benefit amount (or MBA). Most UI Programs determine the worker's weekly benefit using earnings from the highest quarter or highest two quarters of the base period (usually the earliest four of the past five fully completed quarters prior to claiming benefits).[1] The maximum potential benefit amount (MBA) is calculated as a percentage of base period earnings (BPE) up to a maximum determined by 26 times the weekly benefit maximum. This (MBA/BPE) percentage ranges from 25 percent to 50 percent across individual states with the largest concentration of states falling into the 30-33 percent range. The ratio of the maximum potential entitlement (MBA) to the weekly benefit (or WBA) then determines the maximum potential weeks of benefits that may be collected (subject to a limit of 26 weeks). Workers with steady quarterly earnings in the base period are entitled to longer potential durations than those with irregular quarterly earnings. Also, more workers qualify for 26 weeks of potential benefits in states where the maximum entitlement is a higher percentage of base period earnings.[2]

The connection between the weekly benefit amount and maximum weeks of potential benefit duration is important to understand because an increase in weekly benefits will cause a reduction in potential duration for many workers (and vice versa). Thus, a simulation of the effects of changing the replacement rate (and the WBA) on actual benefit duration should account for an offsetting effect on potential benefit duration for many workers. Past research, however, has treated these two determinants of actual benefit duration as independent.

The replacement rate and maximum potential benefit duration are obvious factors affecting actual benefit duration. Estimates of their effects on benefit duration have been derived in numerous previous studies, many to be noted below. Several other aspects of UI Program statutes and administration also can affect benefit duration. While benefit payments are being received the claimant is to be able to work, available for work, and (in nearly all states) actively seeking work. These so called three A's (able, available, and actively seeking) define unemployment (as opposed to inactive or not in the labor force). Someone who does not meet all three criteria is not unemployed and should not receive benefits in periods when these three criteria are not satisfied.

From the perspective of UI Program administration, active monitoring of these aspects of claimant eligibility is needed. Most states have a work test to ensure that claimants are actively searching for work.[3] States typically require two contacts with potential employers per week and keeping records of such contacts (even though the agency rarely verifies the contacts). However, failure to have a record of the contact can result in suspension of benefits for the week. There may also be requirements to appear in person at a local office after a set number of weeks to review with a job counselor intervening activities to secure reemployment. These required meetings are termed eligibility review interviews or ERIs. Offers of suitable work (roughly, consistent with past work but defined differently in individual states) may not be refused (or are

limited to one or two refusals). Only limited amounts of earnings may be received without reducing weekly benefits. Active and ongoing monitoring of claimant eligibility can speed reemployment, both because useful information and services are supplied and because of sanctions when claimants do not satisfy ongoing eligibility criteria.

Past research has shown that agency monitoring of ongoing eligibility can reduce benefit duration. The monitoring function and the benefit payment functions in states are often undertaken by separate administrative entities; e.g., benefits paid by UI agency but work search monitored by the Employment Service (ES). Thus, there is a need for close inter-agency coordination to ensure that work search is monitored effectively and appropriate sanctions are applied. Past research has shown that active administration of work search requirements can reduce benefit duration in a cost-effective manner. Examples of previous literature are noted below.

Ongoing eligibility and benefit duration may also be influenced by the form that benefit payments take. Eligibility and payments are determined and administered on a weekly (or biweekly) basis. However, there have been experiments that tested the effects of paying reemployment bonuses. Bonuses were paid to claimants for some share of the remaining entitlement (say half) if a new job was secured within a set number of weeks from the date of the first benefit payment (and the new job was retained for a minimum period). The motivation for these experiments was both to speed worker reemployment and to save on UI benefit payments. The findings from the reemployment bonus experiments are reviewed below.

The policy relevance of reemployment bonuses has increased by a recent (January 2003) Bush administration proposal to establish a system of Personal Reemployment Accounts (PRAs). These accounts make $3,000 available to unemployed persons identified through profiling as likely exhaustees of UI benefits. The accounts could be used for intensive services such as training. Additionally, persons with PRAs who become reemployed during the first 13 weeks of receiving UI benefits would be eligible for half the remaining balance in their PRA. States could make the bonus payments either as a single lump sum at the time of reemployment or 60 percent at that time with the remaining 40 percent paid after 6 months of work on the new job. This "bonus" element of the proposed PRAs will be reviewed in light of the structure of payments in the earlier bonus experiments.

The preceding discussion shows that several different facets of UI benefit statutes and payments administration can affect duration in benefit status. The UI Program also can have effects on new occurrences of unemployment. Three effects can be identified:

1) Some persons knowledgeable about UI eligibility criteria may join the labor force and secure jobs with the objective of subsequently receiving UI benefits. This so-called entitlement effect of UI was identified by Hamermesh (1979), but it has received only limited attention in research.

2) UI entry requirements vary widely across states. To receive a UI first payment, the claimant must satisfy both monetary criteria and nonmonetary separation criteria. The latter refer to separations from work where there may be a question (issue) of either a voluntary quit or worker misconduct. The result of differing monetary and

nonmonetary requirements and administration is that the ratio of first payments to new initial claims for benefits routinely ranges from 0.50-0.55 in some states to 0.85-0.90 in others. These state specific ratios are typically stable from one year to the next suggesting large and systematic differences in eligibility criteria and state administrative practices. There is the further implication that because entry is so easy in some states, there are needless occurrences of first payments and perhaps even occurrences of unemployment. This aspect of inter-state variation in benefits has not been researched in any major study.

3) The experience rating of UI taxes may also affect unemployment occurrences. Experience rating is intended to assign the costs of benefit payments to those employers who cause UI costs through layoffs. Because experience rating as practiced in the states is only partial, situations frequently arise where a layoff and subsequent benefit charges are not effectively assigned to employers making the layoffs. Noncharged and ineffectively charged benefits become a shared responsibility of all UI covered employers. When benefit costs are not appropriately assigned, some employers initiate more layoffs than they otherwise would. The effect of "imperfect" experience rating has been studied by several authors, and their findings will be summarized below.

The preceding taxonomy provides an introduction to the range of possible effects of UI on workers and employers. These effects in turn can influence the functioning of the labor market and aggregate outcomes. The next section describes analytic approaches used in past research to estimate the effects on worker and employer behavior.

## 3.0 Analytic Frameworks Used to Examine UI Disincentive Effects

Individual researchers and projects have employed different approaches in estimating the disincentive effects of UI Programs. Four will be identified and briefly described even though one approach is dominant, namely single equation analysis.

### 3.1 Single Equation Analysis

By far the most common approach has been to specify a single equation where a specific UI variable; e g., duration in benefit status, is examined using multiple regressions. Measures of UI benefit availability and generosity are included in equations that also have several other control variables. The data may be time-series, cross-section, or pooled time-series and cross-section. Typically, the cross-section data are microdata for individual workers while the time series and pooled data are more aggregative.

Investigators often acknowledge potential problems of simultaneity and misspecification errors inherent in a single equation approach, but argue their estimates are reduced form estimates and valid for making inferences. Effects of the UI Program are inferred from the sign, size, and statistical significance of coefficients on UI explanatory variables like the replacement rate (weekly benefits divided by weekly wages) and potential benefit duration. Much of the research was completed prior to the 1990s.

A substantial amount of new data was generated by a series of social experiments undertaken in the 1980s and 1990s and by evaluations of temporary benefit extensions starting the temporary Federal Program of 1975-1977 (Federal Supplemental Benefits or FSB). This research was conducted with financial support mainly from the Employment and Training Administration of the U.S. Department of Labor. Experiments have explored the effects of changing the administration of work search requirements and paying cash bonuses intended to speed up the reemployment of claimants. The experiments have been conducted with control groups as well treatment groups using random assignment of persons into treatments and controls. Given the design of these studies, valid inferences as to effects of the treatments have been drawn from comparisons of means (treatment group mean less the control group mean) as well as multiple regression analysis. Since the associated regressions usually have rather low explanatory power, the comparisons of means usually have produced estimates very similar in size and statistical significance to the estimates based on multiple regressions.

### 3.2 Analysis that Decomposes the Unemployment Rate

As a matter of definition, the unemployment rate for any given period can be decomposed into frequency of occurrences and the average duration of individual spells. The basic identity is: (1) $ur = f*d$, where *ur* is the unemployment rate, *f* is the frequency of unemployment occurrences (number of occurrences per year as a ratio to the labor force) and *d* is unemployment duration (measured as a fraction of the year). One advantage of viewing the unemployment rate from an occurrences-duration perspective is that some UI claims phenomena that involve offsetting effects on *f* and *d* can be recognized and explicitly incorporated into the analysis. One study that uses this framework is Vroman (2002) where an analytic identity like (1) above is introduced and described (in report III). In report V of the same report, the offsetting effects of a high volume of employer-filed claims are documented empirically; i.e., raising the application rate for benefits but at the same time shortening average duration in benefit status because the workers have remained job-attached.

The research approach that explicitly recognizes both frequency of unemployment occurrences and unemployment duration is helpful for furthering our understanding of interstate differences in the receipt of UI benefits. Vroman (2002) documents that low recipiency is consistently observed in some states, but achieved in individual states through differing combinations of low application rates and short duration in benefit status. The framework is also helpful in pinpointing the different ways that UI Program administration affects recipiency. Separation determinations and denials are important in influencing application rates and first payment rates among applicants while nonseparation determinations and denials influence continuation rates and average benefit duration. Within a frequency-duration framework, individual administrative activities can be assigned to that aspect of unemployment which they affect most directly.

### 3.3 General Equilibrium Studies

From the perspective of the full labor market, the effects of UI benefit payments are not confined to just beneficiaries. Effects on, say, unemployment duration among beneficiaries can affect the job search intensity and job-finding success among unemployed nonbeneficiaries. To

the extent that total employment is constrained by the overall levels of real output and technology, a slower return to employment by beneficiaries will enhance the job prospects of nonbeneficiaries. While this "general equilibrium" (or displacement) effect of the UI Program has long been recognized as potentially important, e.g., Welch (1977), very little empirical work on the question has been undertaken. The analytic problem is very challenging since it requires a model of the full labor market and parameter estimates to characterize the job search and job finding behavior of the unemployed (UI recipients and nonrecipients) and the employed.

Two papers that address this "general equilibrium" problem are by Davidson and Woodbury (1993) and Levine (1993). Central to both papers is an explicit consideration of unemployed persons who are not recipients of UI benefits. In both papers, actions that change unemployment among UI recipients occur in a labor market where there are offsetting effects on nonrecipients.

Davidson and Woodbury estimate the general equilibrium effects of a reemployment bonus program using a simulation approach with parameter estimates based partly on results from the Illinois Reemployment Bonus Experiment (Woodbury and Spiegelman 1987). There are four unemployed groups with groups 1 (those eligible for the bonus) and 4 (unemployed but ineligible for UI benefits) being of greatest interest. The bonus offer increases search effort and job finding among those eligible for the bonus, but there are offsetting effects on other groups, especially UI ineligibles. The latter group experiences lower success rates in job finding, an induced reduction in search effort, and increased unemployment duration. Under a range of parameter estimates, Davidson and Woodbury find the bonus program reduces overall unemployment, but the adverse effects of the bonus on the UI ineligibles offsets some 30 to 60 percent of the employment gains realized by bonus recipients. Estimates of employment effects appear in table 2 of their paper.

Levine (1993) estimates relationships for UI recipients and nonrecipients separately where duration for each group is a function of the UI replacement rate.[4] For recipients, a higher replacement rate increases unemployment duration, but for nonrecipients it reduces duration. The net overall effect of an increase in the replacement rate is estimated to cause a small decrease in aggregate unemployment.

In sum, both papers find offsetting effects on nonrecipients. Davidson and Woodbury conclude that effects on nonrecipients offset some 30 to 60 percent of the employment gains among the reemployment bonus group. Levine finds the effects on nonrecipients from a higher replacement rate among recipients more than offsets the increase in unemployment duration among UI recipients. The range of conclusions reached by the two papers points up the need for more work on the general equilibrium implications of UI benefit payments.

While the general equilibrium aspects of UI disincentive effects can be acknowledged, these will not be treated further in this review. The effect of this decision is to make the estimated effects of the UI Program on unemployment occurrences and duration appear to be larger than would be estimated based on a full general equilibrium characterization of the labor market. The analyses of Davidson and Woodbury (1993) and of Levine (1993) are useful for providing empirical grounding for this conclusion which has strong a priori logic.

### 3.4 The UI Program and Labor Market Transitions

For the working age population, there are three labor market states: employed (E), unemployed (U), and not in labor force (N). Between any two points in time a large volume of labor market flows takes place; i.e., movement between E, U, and N. The shorthand description of the flows is given by six origin-destination descriptors: EU, EN, UE, UN, NE, and NU. The literature on the transitions between these three states (so-called "gross flows") is limited and the subset of this literature that incorporates the UI Program is even more limited.

While gross flows framework serves as the basis for at least two literature summaries (Gustman 1982 and Atkinson and Micklewright 1991) and is recognized in a third review article (Decker 1997), empirical estimation of the full set of flows incorporating effects of UI is present in just a single study (Clark and Summers 1982).

Since the majority of unemployed persons do not collect UI benefits at any time during their spell of unemployment, this framework is difficult to implement. Each of the six gross flows involves flows by UI recipients and nonrecipients. The choice of empirical strategy for the researcher is either to estimate effects of UI on flows which combine recipients with nonrecipients or to separate the two components of each flow. To date, no research has attempted such a six-way decomposition for both recipients and nonrecipients, mainly because the Current Population Survey (CPS) and other data files that might be used do not routinely have relevant data for both UI recipients and nonrecipients.

### 3.5 Analytic Approaches—Summary

While this section has introduced and described four potential approaches for estimating the effects of UI on the labor market, the single equation approach has predominated in past research and will likely continue to predominate in future work. The three alternative approaches described in the preceding paragraphs merit serious consideration. It would seem that pursuing research utilizing a general equilibrium framework should be given some support in future research. At present, we have more information about parameters that show the effects of UI estimated from single equation analyses (to be summarized in the remainder of this report) than information on how the parts of the labor market fit together to yield aggregate labor market outcomes and the net effect of UI in influencing those outcomes.

## 4.0 Past Studies of UI and Unemployment Duration

Payment of UI prolongs unemployment duration. Among the program provisions whose effects on duration have been most thoroughly studied are the weekly benefit (and the associated replacement rate) and potential benefit duration. The single equation studies of UI and unemployment duration have included one or both of these key program provisions within the set of explanatory variables. While many other aspects of UI are important in determining benefit duration—particularly aspects of program administration—consideration of these other factors is reserved mainly for section 5.0.

That UI should lengthen unemployment duration is not surprising, as it was part of the rationale for establishing UI. Those who lose jobs through no fault of their own are provided

cash support for a limited period while they search for new jobs. Better job matches can result when financial pressures on the unemployed are less pressing.

Two distinct frameworks have been utilized in assessing the impact of UI on unemployment duration. The first posits a work-leisure choice where UI payments lower the price of leisure. With static preferences and with income (derived from employment) and leisure both being normal goods, the payment of UI benefits causes labor supply to decline and reemployment to be delayed (Moffitt and Nicholson 1982). The second framework focuses on worker behavior in searching for work. Within the job search framework (Mortensen 1977), receipt of UI lowers net utility of work, raises the minimum acceptable (reservation) wage for new jobs, and reduces job search intensity. Reduced search intensity, in turn, lowers the rate of job finding and increases unemployment duration.

The two theories are difficult to distinguish empirically because they share three common predictions: (1) an increase in the level of benefits (or the replacement rate) will prolong unemployment duration, (2) an increase in potential UI benefit duration will increase actual UI benefit duration, and (3) the rate of job finding among UI recipients will increase in the weeks immediately prior to benefit exhaustion. Subsequent paragraphs will mainly emphasize these commonalities in predictions rather than findings that distinguish the two theories.

Table 1 summarizes the results from 14 studies of the determinants of UI benefit duration. As noted above, the selection is not offered as fully exhaustive of the extant literature, but rather, it reflects a judgement as to the most important and influential studies. All utilize data from the U.S. and most allow for separate effects of the level of benefits (or replacement rate) and potential benefit duration on actual benefit duration. Duration is measured as weeks of actual benefits or as hazard (exit) rates for individual weeks within unemployment spells. Because the studies are diverse, it is not always obvious how they should be summarized so that the most meaningful comparisons are possible. To the extent possible, the two right-hand columns respectively show the effects on actual duration of a 10 percent increase in the WBA (or replacement rate) and the effects of a one-week increase in potential duration.

It should also be noted that diverse measures of benefit levels (and replacement rates) were used. Newton and Rosen (1979) use the opportunity cost of unemployment measured as the gap between the weekly wage and the weekly UI benefit. UI benefits became partially taxable in late 1978 and fully taxable in late 1986. Several studies (Solon 1985; Meyer 1990) measured benefits on a “net” basis; i.e., both wages and UI benefits measured net of income and payroll taxes. These measures were based imputations whose accuracy could be questioned.

Several studies (Moffitt and Nicholson 1982; Grossman 1989; and Card and Levine 2000) estimate effects of potential duration in data where potential duration has been extended beyond the usual 26 weeks of the regular UI Program. These extensions in potential duration were temporary and occurred during recessions. Because the longer potential duration was due to a recession, caution must be exercised in assigning a causal nexus between the increase in potential duration and associated increase in actual duration. Moffitt and Nicholson (1982) estimated separate relationships for exhaustees and nonexhaustees. They found that duration for exhaustees was little influenced by the increase in potential duration.

The findings reported in table 1 display consistency in the estimated effects of benefit levels and potential duration. Both effects are consistently positive across these studies. While the point estimates for the two variables vary from study to study, a 10-percent increase in the weekly benefit (or replacement rate) increases duration an estimated 0.5 to 1.8 weeks. An increase in potential benefit duration increases actual duration by an estimated 0.1 to 1.2 weeks. When the individual point estimates for the latter are reviewed, note that all but two (Arizona in Classen 1979; Grossman 1989) fall into the range between 0.1 and 0.5 weeks. Given the differences in samples, estimation procedures, and time periods, this range is quite narrow.

Because maximum benefit entitlement formulas cause an increase in the WBA to reduce potential benefit duration for many workers (at a given level of base period earnings), the estimated effects in table 1 should be treated as gross effects on actual duration, not net effects. The offsetting relationship between the WBA and potential duration is not accounted for in any of these studies. This "offset" effect is particularly important for workers with irregular base period earnings. They are much more likely to experience reductions in potential duration if the WBA (or replacement rate) were higher.

Table 1 emphasizes average effects with detail by gender only shown for two studies (Moffitt and Nicholson 1982; Moffitt 1985b). Several studies have noted different effects of UI variables for demographic subgroups. Generally, the response of actual duration to potential duration is larger (hazard rates are lower) for women, nonwhites, older workers, and those with lower educational attainment.

One illustration of differing sensitivity among demographic groups is provided by the analysis by Corson, Needles and Nicholson (1999) of the Emergency Unemployment Compensation (EUC) Program of the early 1990s. Table III.4 of their study compares characteristics of UI-only (short term) recipients with UI and EUC (long term) recipients. Women represented 40.8 percent of UI-only recipients but 43.8 percent of UI and EUC recipients. The corresponding shares for persons aged 55–64 were 9.1 percent and 12.5 percent, respectively. For African Americans, the analogous shares were 9.8 percent and 16.9 percent, respectively. As these authors note, similar patterns were also observed during the recession of 1981–1983.

Differing sensitivity has also been documented by type of separation (permanent or temporary) and duration in benefit status. Workers on temporary layoff are less sensitive to UI program variables than those on permanent layoff. Sensitivity to UI variables (hazard rates) also declines as duration in benefit status lengthens. The combination of these two findings points up the importance of knowing the type of separation experienced by the individual. Those on temporary layoff experience much shorter average durations than those on permanent layoff,[5] but their spells are little affected by the replacement rate or potential duration.

A review of table 1 shows that 12 of the 14 studies were based on data that predate 1990. The Corson, Needles and Nicholson (1999) study was sponsored by the Office of Workforce Security (OWS) to examine the effects of the Emergency Unemployment Compensation (EUC) Program of 1991-1994. The size of the disincentive effects of both UI benefit levels and potential benefit duration estimated in this study fall within the range of estimates derived in the other

studies. Since the recent TEUC Program will pay benefits through March 2004 and followup data collection will extend for at least 1 year after its expiration, information on its labor market effects will not become available before 2005.

Note also in table 1 that about half the studies utilized Claimant Wage and Benefit History (CWBH) data. The CWBH collected administrative data on both benefits and earnings from several states, but it was terminated in the early 1980s. Thus, the data from CWBH studies have no contemporary counterparts. Only Washington has maintained its CWBH data base in more recent years.

The main benefit provisions in individual state UI Programs have exhibited strong year-to-year stability in recent years. Thus, potential duration of regular UI benefits of 26 weeks has been operative in all but two states (Massachusetts and Washington) since the mid-1980s and operative in 40 states since the late 1960s. Similarly, the provisions affecting average benefit levels (the statutory replacement rate and the maximum weekly benefit relative to the average weekly wage) have also been quite stable.

One interesting change in benefit provisions occurred in New Jersey in 1996. During the last half of the year, persons who exhausted benefits in regular UI could claim an additional 13 weeks (or half of regular benefit duration for those eligible for fewer than 26 weeks) under the New Jersey Extended Benefits (NJEB) Program. Card and Levine (2000) examined the effect of this temporary extension on actual duration. Relying on tabular data as well as regression analysis, they found that benefit duration did respond positively to the added eligibility from NJEB. The total estimated response was modest, 0.08 weeks per each week of extension, or roughly 1.0 week for 13 added weeks.

Note in table 1 that the Card and Levin estimate falls at the bottom end of the range of estimated responses of actual weeks to an increase in potential weeks. Perhaps this would be anticipated in a temporary program. Unlike national benefit extensions under FSB, FSC, EUC and TEUC, the New Jersey extension was effective for a much shorter period (just 5 months), and it occurred in a reasonably robust labor market.[6] Interestingly, Card and Levine found large numbers of exits from UI just before 26 weeks continued to occur, even though most claimants were eligible for 39 weeks under NJEB. Perhaps information about the availability of NJEB benefits was not as widespread as in other programs of extended benefits.

This was a most unusual extension of potential benefit duration. Since 1975, the only other increases in potential duration have occurred during periods when either temporary Federal benefits or Federal-state Extended Benefits (EB) were available. Because of this situation, the states do not provide a good source for natural experiments to test for effects of changes in potential duration on actual duration.

Statutory provisions related to benefit levels provide more productive ground for finding and testing the effects of state-level natural experiments. Between 1990 and 2002 there were at least three instances of large changes in benefit generosity as reflected in statewide replacement rates. The changes occurred in California, Indiana, and Virginia. Other changes were identified, but they were much smaller than in these three states.[7]

California increased the maximum weekly benefit from $230 in 2001 to $330 in 2002 (or 43 percent) with three additional $40 annual increments slated for 2003–2005. The 2001 legislation also changed (reduced) replacement rates for low-wage workers. Between 2001 and 2002 California's average replacement rate increased from 0.22 to 0.27, with smaller changes anticipated for 2003–2005. These changes follow an extended period of stability in the weekly maximum as California's replacement rate decreased throughout the 1990s. Thus, California offers a recent history of both large increases and large decreases in its replacement rate as well as changes in wage loss replacement for those at the bottom of its benefit structure.

Indiana implemented large increases in its maximum weekly benefit amount between 1991 and 1995. The cumulative change saw the maximum increase from $96 to $202 per week, or from 22 percent of weekly wages to 41 percent. As a result, the replacement rate increased from 0.26 in 1991 to 0.37 in 1995. The higher maximum has been sustained since the mid-1990s.

Following the terrorist attack of September 11, 2001, Virginia raised its weekly maximum in October from $268 to $368, or 37 percent, and then maintained the higher maximum throughout 2002. The maximum was reduced to $318 in 2003, still $50 higher than the earlier maximum of $268. In July of 2004, the maximum will increase to $326.

The changes in the maximums in all three states have exceeded 35 percent. If the elasticity of duration to the replacement rate is 0.5, effects on actual duration should be apparent. For California and Virginia, the changes are sufficiently recent that relevant microdata should be readily available from the states. Examining the effects of the recent changes in these three states should be given high research priority.

## 5.0 UI Administration and Benefit Duration

The previous section showed that level of benefit payments and potential benefit duration are two important influences on the duration of unemployment and UI benefit duration. Base period earnings patterns interact with state statutes to determine the weekly benefit, the maximum benefit amount, and potential duration for individual claimants. In modeling the effects of payment levels and potential duration, both are taken as given for individual claimants who then try to maximize utility within either a static work-leisure framework or a job search-reservation wage framework. The focus is on decision making by the claimant within an environment characterized by constraints or imperfect information.

Several aspects of UI Program administration also influence time in benefit status. However, studies of the effects of UI administrative decisions have been much less numerous. Millions of administrative decisions affecting eligibility are routinely made each year. These decisions can be grouped into three broad categories: monetary eligibility; nonmonetary separation issues (where quits and misconduct are at issue); and nonmonetary, nonseparation issues (typically where continuing eligibility is at issue). The jargon of UI administration refers to nonmonetary decisions simply as "seps" and "nonseps." For a given claimant, the monetary and separation decisions must be favorable in order to receive an initial benefit payment while nonsep decisions typically take place after benefit payments have commenced. All decisions are made within tight time frames where claimants and their former employers often disagree about

the circumstances of the job separations and/or with agency decisions about eligibility. The combined effects of a large volume of decisions, tight time tables, and frequent claimant-employer disputes yield many occasions where redress is desired by one party. Thus, UI has appeal procedures where dissatisfied parties can try to reverse earlier adverse administrative decisions.

States routinely report on their administrative activities in all three areas. For all three, the reporting shows the number of decisions and the breakdown of outcomes favorable and unfavorable to claimants. From these data, one can assess the frequency of determinations and denial rates. The data show wide inter-state variation in determination rates and in denial rates. For nonseps the issue areas are: (1) able, available, and actively seeking work, (2) disqualifying and/or deductible income, (3) refusal of suitable work, (4) reporting requirements (in-person or call-in), (5) refusal of profiling referrals, and (6) "other" which mainly relates to aliens, athletes, and school employees. Since an issue can arise in any of these areas in any week when benefits are claimed, UI agency exposure to potential issues is large. The volume of nonseps is typically measured relative to total weeks claimed (claimant contacts) during a given period (month or year).

Besides UI agency activities in the three broad aspects of administration that are subject to periodic reporting to OWS (monetary eligibility, seps, and nonseps), several other activities potentially affecting benefit duration are not part of the OWS reporting system. Examples include: the content of eligibility reviews where claimants are required to appear in person or call agency staff to report job finding activities; the number of job search contacts per week; and agency procedures to verify job search activity. These activities have elements of both carrots and sticks in trying to encourage reemployment by claimants. Agencies differ in the pursuit of these activities. A sizeable fraction add state monies to Federal administrative allocations in order to enhance the effectiveness of program administration.

Extant research on continuing eligibility and benefit duration falls into three broad areas: (1) the work test and work search verification, (2) reemployment bonus studies, and (3) profiling studies. All three have been conducted as state-level experiments, typically with randomized assignment of participants into treatment and control groups.

## 5.1 The Work Test

Nine studies of the work test are summarized in table 2A. Eight (all but Steinman 1978) utilize data from the 1980s and 1990s. It should be noted that studies using data from the mid-1980s and earlier represented a major departure from earlier research which examined grouped data. Also, the earlier studies took place within an administrative environment where claimants had greater in-person contact with local UI offices.

Typically, the studies tested more than a single treatment. Some treatments emphasized enhanced services while others emphasized increased work search requirements and/or work search verification. Thus, while the treatments usually reduced benefit duration, it often was not possible to separate the effects of carrots (enhanced services) from the effects of sticks.

Overall, the findings in table 2A are reasonably consistent. Short-run impact effects fall generally into the -0.40 to -1.00 weeks range and most treatment effects are significant.[8] The table displays 25 mean treatment effects of which 20 are negative and 17 are statistically significant at the 0.90 or 0.95 level. Of the 25 means, 16 fall into the range from -0.40 to -1.00. The estimated impacts are tightly bunched.

Only two studies present major exceptions to the preceding. The Nevada program (Steinman 1978) reported much larger treatment effects (-3.90 weeks). This study had extensive involvement of experienced staff and close monitoring of claimant job search activities. It probably had the most intensive staff input per claimant of any study identified in the table. At the opposite extreme, Ashenfelter, Ashmore, and Deschenes (1999) found that work search verification had insignificant effects on duration in all four states. In two states—Massachusetts and Virginia—the point estimate was even positive, although not significant. Because the structure of the treatments in their study was different from others, the results do not speak directly to the effects of changing work search requirements as do the other studies.

A second interesting finding relates to allowing greater claimant autonomy in search activities. Note in Johnson and Klepinger (1991) that exception reporting (Treatment 1) in Washington resulted in significantly increased benefit duration (3.34 weeks). A similar (but quantitatively smaller) finding was reported in the Maryland Work Search Demonstration where Treatment 2 did not require reporting of job contacts. Again, duration was significantly longer (0.4 weeks) than for the control group.

Increased time in benefit status, however, did not lead to reduced earnings. This treatment was the one treatment in the Maryland Demonstration where earnings increased significantly. Woodbury (2001) also reports significantly higher quarterly earnings for the exception reporting group (conditional upon employment) in a 10-year followup of the Washington Demonstration. For those reemployed, the increase in quarterly earnings was as large as for those who received intensive services ($81-$82). It seems that longer benefit duration is associated with productive wait unemployment; e.g., an increased likelihood of reestablishing earlier job matches. Greater search autonomy permits some to wait longer for former jobs, and successful reunions explain the increased earnings. Note in Woodbury (2001), however, that these effects are conditional upon securing reemployment. The unconditional effects on earnings are significantly negative (-$52). In sum, greater autonomy is productive for some but not for the majority. It would seem important to identify who benefits from increased search autonomy and who does not.

It also appears in the Maryland study that increased search intensity (Treatment 1) and increased verification (Treatment 4) both help to shorten benefit duration. The latter finding directly conflicts with the findings of Ashenfelter, Ashmore, and Deschenes. The Maryland study is also interesting in that the treatments involve the number of contacts, reporting of contacts, and verification of contacts; i.e., sticks. The larger conclusion from the Maryland demonstration is that sticks have the intended reemployment effect, but faster reemployment does not translate into significant increases in earnings.

There is also evidence in table 2A of the usefulness of carrots. Job search assistance speeded reemployment in both the District of Columbia and Florida (Decker et al. 2001), while

intensive services had positive long-run effects on both employment and earnings in Washington (Woodbury 2001).

Overall, carrots and sticks are both important tools for securing faster reemployment. Significant effects of both were found in Charleston (Corson, Long, and Nicholson 1985), and greater service intensity was productive (Treatment 3 versus Treatment 2 in Charleston).

To summarize, changing work search requirements and providing enhanced support services are both effective in reducing unemployment duration and speeding reemployment. From a benefit-cost perspective, this is a useful finding, since the job search interventions are typically inexpensive.

Improving earnings upon reemployment presents more of a challenge. The findings in table 2A indicate that longer benefit duration achieved through a relaxation of work search requirements (and reporting) is productive for some in their subsequent earnings. While this group seems to be a minority of claimants, it would be useful to try to identify them early in their spells of unemployment and then ascertain what makes their search so effective. Is it as simple as allowing some workers more time to try to return to jobs with former employers?

## 5.2 Reemployment Bonuses

During the mid-to-late-1980s, experiments were conducted in four states to test whether the payment of bonuses would speed the reemployment of UI claimants. The first was undertaken in Illinois in 1984 followed by experiments in New Jersey, Pennsylvania, and Washington over the next 5 years. All four experiments had treatment and control groups.

Except in Illinois, the payments were made directly to claimants who secured jobs early in their spells and retained the new jobs for some minimum period, e.g., 4 months. The Illinois experiment tested two treatments, payments to claimants and payments to employers. Because the effects of paying claimants were found to be much stronger, payments to employers were not included in design of the later bonus experiments. The treatments in the other three experiments tested for effects of variation in qualification periods and the level of the bonus payments. Typically, claimants were told of eligibility for the bonus after receiving their first payment.[9] The bonus payments were set as a fraction (40 to 60 percent) of the remaining entitlement. Typically, payments were made as two lump sum amounts. The first payment was made shortly after taking the new job and the second payment was made after the job had been retained for a minimum qualifying period.

Table 2B summarizes estimated effects of the four experiments. All estimated treatment effects on benefit duration were negative and roughly half (seven of 16) were statistically significant. The bulk of the point estimates (10 of 16) cluster in the range between –0.40 and –1.00 with but one estimate larger than –1.00 and the other five between –0.06 and –0.36. The largest treatment effect was found for payments to claimants in Illinois (-1.15 weeks). Overall, the bonuses reduced unemployment duration by some 0.40 to 1.00 weeks. Note that this range is the same as the range for the estimated effects of the work search demonstrations summarized in table 2A.

The bonus experiments were not too successful in identifying the marginal effects of either the level of the bonus payment or the length of bonus qualification period. Of the two experiments that tested for variation in these treatments, the Pennsylvania results show a wider range of estimated effects of high bonuses (T3, T4 and T5) and of long qualification periods (T2, T4 and T5) vis-a-vis their opposites. In Washington, the relative ranges of estimated effects over these two dimensions of treatment were even wider. The one pattern that emerged in Washington was that high bonuses had the largest treatment effects (T3 and T6). A reader can speculate whether the experimental subjects understood the eligibility rules well enough to give reliable signals on these basic elements of the experimental designs. It seems clear that more can be learned about how claimants respond to the financial incentives offered through reemployment bonuses.

Reemployment bonuses in Washington did raise claimant earnings in the long run. Note in the Woodbury (2001) analysis that all six treatment effects on quarterly earnings were positive over the 10 years following the experiments and that three estimated effects were statistically significant. When the six individual treatment effects are examined, however, there is no obvious pattern by the level of the bonus or the qualification period. Low and high bonuses were estimated to have larger effects than medium bonuses.

The treatment groups within bonus experiments claim benefits under the traditional rules of continuing eligibility but with the added financial incentive provided by the bonus payment. For members of the treatment pool, four effects on claims can be identified that are undesirable. First, some who would have had short claims irrespective of the bonus payments will receive a windfall payment without any change in behavior. Second, the bonus payments increase the attractiveness of filing for UI benefits because of the windfall represented by the bonus. With the bonus in place, it becomes much more attractive to file and collect a few weeks of benefits because of the added financial reward for securing a new job. Third, some otherwise short-term claimants will delay exiting benefit status until the final weeks of the bonus eligibility period.[10] Fourth, there are potential displacement effects as those responding positively to bonus payments secure jobs that otherwise would have been filled by other members of the claimant pool.

To the extent that the preceding behavior occurs, it raises the costs of a bonuses program. Even with delays in paying the second part of the bonus amount, some would be financially rewarded for no change in behavior, while others would alter their behavior in ways that would increase benefit payouts. Several of these considerations were raised by Meyer (1995) in his review article. He also noted that these effects would likely become larger if a permanent bonus program were to be instituted.

Interest in reemployment bonuses revived in 2003 through a proposal made by the Bush administration to establish PRAs. Persons identified through profiling as likely UI exhaustees would be granted $3,000 accounts to be used for intensive services like training. Additionally, those who become reemployed during the first 13 weeks in benefit status would receive a bonus equal to half of the remaining balance in their PRA. States could structure the payments as either a single lump sum at the time of reemployment or divided 60-40 with 60 percent paid at reemployment and 40 percent after 6 months on the new job. From the earlier research on reemployment bonuses, it would seem delayed payments should be a requirement of this

proposed program, not a state option. Making the delay a requirement is further supported by the disincentive considerations raised in the preceding discussion.

From the review of the work search and reemployment bonus experiments, observe that the average effects of both treatments are similar. Most estimates in tables 2A and 2B fall into the –0.40 to –1.00 weeks range. If the two treatments achieve roughly comparable results, it would seem that work search interventions are superior because they are less costly. Additionally, changing work search requirements presents fewer possibilities for disincentives related to added benefit payments (bonuses) as noted above.

### 5.3 Profiling

The central idea behind profiling is straightforward. The pool of eligible claimants is heterogeneous and those most likely to collect unemployment benefits for long periods can be identified on the basis of observable characteristics. In most states, the profiling algorithm was developed using data from an earlier period where claimant characteristics and the actual duration of benefits were observed. The algorithm estimates parameters in a multivariate statistical model; e.g., a regression where the dependent variable is a 0-1 variable (= 1 when the person exhausts benefits or when benefit duration exceeds a predetermined threshold). The same algorithm is then used to assign scores to current applicants.[11] Applicants are ranked and those with the highest scores are designated to receive enhanced services (where services are available). The treatments are intended to shorten benefit duration. Persons refusing to participate in the treatments may be denied benefits. Profiling is reserved for persons who are not job-attached. Persons with definite recall dates and those employed through union hiring halls are exempted from profiling.

What are the profiling interventions? Data from 1999 were as follows. The total number profiled was 6.5 million or about 70 percent of all new initial claims for UI benefits.[12] Almost 2.2 million claimants were identified as likely exhaustees, while nearly 0.8 million were actually referred for services. The profiling reporting system identifies seven service categories. Total counts in 1999 were as follows: orientation—0.44 million; assessment—0.40 million; counseling —0.20 million; job search workshops—0.25 million; placement services—0.67 million; education and training—0.14 million; and the self-employment program (present in just a few states)—0.01 million. The total number of services offered totaled 2.1 million, but the bulk were either very short-term interventions (orientation, assessment, counseling, and job search workshops, 1.29 million) or placement services (0.67 million). Education and training measures, the most significant long-term intervention, totaled less than 0.15 million. Placement services and education and training measures had completion rates of about 70 percent compared to nearly 100 percent for the quartet of very short-term (typically one-shot) interventions. Thus, the typical profiling intervention was a small-scale intervention.

Table 2C summarizes the results of four analyses of the effects of profiling, emphasizing estimated effects on unemployment duration. The table displays 16 estimates, and 15 are negative. The estimates cluster between –0.40 and –1.00 (10 of 15) with only two exceeding –1.0 and three between –0.21 and –0.29. Note that 12 of the negative estimates are significant at the

95 percent level. These results suggest profiling has a small but significant effect in reducing unemployment duration.

Of all the estimates displayed in table 2C, the estimate of –2.2 weeks from the Kentucky study (Black, Smith, Berger, and Noel 2003) is by far the largest. At least two factors seem to have been at work in Kentucky. First, Kentucky's profiling algorithm utilizes more controls than in most states. Particularly important, the local unemployment rate enters with highly significant effects on unemployment claim duration. Second, the control group was drawn from the same predicted duration interval as the treatment group in each substate area. Both factors may have combined to yield a superior control group compared to the other profiling studies.[13]

Knowledge of the impact effects of profiling on duration is still limited. The four studies summarized in table 2C span just eight states with data for three studies drawn from the mid-1990s.[14] Given the universal application of profiling in UI Programs, there is an obvious need to undertake added research to broaden our understanding of how profiling works and which interventions are most effective.

New research that provided answers to the following three questions would be especially valuable:

1) Why are the estimated impacts of the Kentucky analysis so much larger than for the other studies? Pursuit of this question should involve a comparative analysis of control group methodologies and a repetition of the approach followed in Kentucky in at least one additional state.

2) What are the effects of the carrots versus the sticks used in profiling? One aspect of this question is to note the experiences of persons sanctioned for failure to adhere to profiling requirements. A second aspect is to undertake an analysis of the comparative effects of the different profiling interventions identified above.

3) How should resources be allocated among those identified as most likely to exhaust benefits (or experience long durations)? The question (as raised by the authors of the Kentucky analysis) is: Should those with the highest scores necessarily be first in line for receiving expedited services? Those with lower scores could, on average, benefit more from interventions than those with the highest scores. An alternative would be to follow a triage approach to resource allocation. Perhaps more resources should be devoted to those who will benefit more, not necessarily those with the highest profiling scores.

The preceding questions give an idea of productive areas for added research.

## 5.4 Summary

In reviewing the studies of work search, reemployment bonuses and profiling, four findings seem especially noteworthy:

1) All three interventions reduce duration in benefit status.

2) Surprisingly, the point estimates for all three interventions cluster in the same range: –0.40 to –1.00 weeks of reduced duration.

3) Interventions that affect work search are more cost effective than reemployment bonuses. Their effects on duration are similar. However, bonuses are more expensive due to the payment of the bonuses, and they raise more issues of potential disincentive effects.

4) More research on the effects of profiling is needed, with the three areas being especially important: a better understanding of the most appropriate control group methodology; a better understanding of the relative roles of carrots versus sticks in reducing duration; and how to best allocate intervention resources among those with high profiling scores.

## 6.0 Determinants of the Number of UI Claims

The terms and conditions of UI benefit availability can affect not only unemployment duration, but also the number of claims for benefits. This number depends upon both the number of underlying spells of unemployment and the rate at which persons file for benefits. Research on the link between UI and occurrences of unemployment has primarily emphasized the effects of imperfect experience rating on employer decisions to utilize temporary layoffs, a topic to be investigated in section 7.0. The focus here is worker decisions to file or apply for UI benefits, commonly termed the UI take-up rate in the literature.

Although the UI Program has paid benefits for more than 60 years, the share of the unemployed who collect benefits is low and has declined substantially since the 1950s. National recipiency rates have fallen consistently below 0.40 in recent nonrecessionary years despite the fact that more than 95 percent of wage and salary employees work in UI-covered employment. Two other facets of UI recipiency are also noteworthy. Recipiency declined in the early 1980s and the cause(s) of the decline were investigated, most prominently by Corson and Nicholson (1988). Interestingly, there has been a noticeable increase in recipiency since the mid-1990s, but this has not yet received much attention. Recipiency varies widely by state, as noted by Vroman (2002, ch. 2) and by others. A large part of this variation is related to inter-state variation in take-up.

In general, the determinants of UI application rates have been studied much less than the determinants of benefit duration. One research approach to the application decision has been to ask questions directly of unemployed persons in the CPS. Questions about UI applications were posed in CPS supplements of 1989–1990 and again in 1993 with summaries of findings available respectively in Vroman (1991) and Wandner and Stettner (2000). Two prominent reasons for not filing given in these surveys were “quit last job” and “did not work enough.” Because more than a decade has now elapsed since the most recent of these supplements, the need to repeat this data collection seems clear.

Several approaches have been followed in examining the determinants of UI take-up. Table 3 identifies six studies undertaken since the late 1980s.[15] The studies vary in the type of

data employed (state-year averages versus microdata), the definition of take-up, and the treatment of taxes.

A common characteristic of all studies is the use of a UI benefits variable (either the level of weekly benefits or the benefit replacement rate) as a determinant of applications. Several aspects of monetary eligibility could affect applications including presence of weighted benefit schedules (which provide higher replacement for lower wage workers), dependents' allowances, and the possibility of a second benefit determination for those initially found monetarily ineligible. These factors all make replacement rates quite variable at the microlevel.

Although replacement rates are variable, it should be noted that factors besides benefit levels are of greater importance in determining take-up. Across individual states the variation in recipiency and take-up is much greater than the variation in replacement rates. For example, the coefficient of variation (the ratio of the standard deviation to the mean) for the application rate as measured by Vroman (2002) in state-level data for 1999 was 0.44 whereas for the replacement rate it was 0.15.[16] In other words, many factors besides the level of benefits enter decisions to file for UI and the collective importance of these other factors greatly exceeds the importance of the benefit level in determining take-up. In the studies noted in table 3, some of the most important of these other factors are determination rates and denial rates for both seps and nonseps, reason for unemployment, unionization, industry, occupation, method of claims filing (in-person, mail, telephone, or employer), and duration in benefit status.

Table 3 displays the key findings of these studies that pertain to the effect of benefit levels on take-up. The table gives only a small suggestion as to the number of important program variables that influence UI take-up. Note that the Corson-Nicholson (1988) study is included even though its dependent variable (the so-called IUTU ratio—Insured Unemployment/Total Unemployment) included not only elements of applications, but also benefit receipt and duration in benefit status. Besides the significant effect of the replacement rate, several aspects of nonmonetary determinations (the individual issues of quit,[17] misconduct, disqualifying income, and work test violations) all had significant effects. The decline of recipiency of the early 1980s in their analysis was found to have had several contributing factors.

Blank and Card (1991) focused on the period from 1977 to 1987. They found that benefit levels had significant effects on take-up but the decrease between 1977–1980 and 1982–1987 was fully attributed to a reduction in take-up. Thus, changes in claimant behavior rather than changes in UI Program variables carried most of the explanation for decreased application rates.

Both analyses by Anderson and Meyer (1994 and 1997) found benefits had a significant effect on take-up, but with estimated effects that depended on the sample chosen and the method of estimation. They explicitly explored the changed tax treatment of UI benefits that was modified in 1979 (to partial taxation) and again 1987 (to full taxation) and found that the change exerted an important depressing effect on UI claims.

The Vroman (2002) study is the only one of the six that did not find a positive effect of the replacement rate on applications. In certain specifications, it even found a negative coefficient for the replacement rate. Because the study did not use state-year dummy variables for controls, however, it is not persuasive on this question. Like the Corson-Nicholson study,

however, it highlighted the importance of other UI administrative variables, especially determination rates and denial rates for misconduct. This is the only one of the table 3 studies to explore the effects of the method of filing, finding significant effects. In states with a large volume of employer-filed claims, application rates were systematically higher than elsewhere.

Overall, table 3 indicates that benefit levels have a significant positive effect on take-up. As Anderson and Meyer (1997) have observed, this means that estimates of the effects of the replacement rate on UI recipiency that focus just on benefit duration overlook an important second avenue through which the total volume of weeks in benefit status is affected by the level of UI benefits.

Taxation of UI benefits reduces the attractiveness of filing for benefits and of long benefit duration. The avenue is through a reduction in the replacement rate. Consequently, larger effects of taxation on UI recipiency would be expected for claimants in higher tax brackets. Since taxation of UI benefits has been fully in place since 1987, the increases in duration and take-up rates since the mid-1990s must be attributable to other factors. Knowledge about the determinants of take-up would be enhanced with new information from another supplement to the CPS like the earlier supplements of 1989–1990 and 1993.

## 7.0 Studies of Incomplete Experience Rating

A unique feature of the UI Programs in the United States is reliance on experience rating to determine employer tax rates. Employers with higher payouts to current and former employees pay higher taxes than those with low payout. States typically operate with several tax rate schedules—higher schedules operative when trust fund balances are lower. For each schedule, however, higher payout rates are associated with higher tax rates. No other country uses experience rating to set UI tax rates.

Experience rating as practiced is only partial. There is not a one-to-one correspondence between added payouts and added taxes. Three types of benefit payouts are not effectively assigned back to individual employers. The presence of maximum and minimum tax rates mean that some payouts (termed ineffective charges) are not recovered even though the responsible employer is known. Other payments are not charged to employers, either because the employer was not responsible for the separation (termed noncharges) or because the employer has ceased operations (termed inactive account charges). In the aggregate, these three ineffectively assigned charges are large, accounting for about 40 percent of all benefit payments between 1988 and 2002 in data reported by the states to OWS.[18]

Even if an employer pays dollar-for-dollar for added benefit payouts, there is an important difference in timing between the added payouts and the added taxes. Under a 3-year benefit-ratio experience rating system with a 10 percent annual discount rate, for example, three extra dollars of payments this year followed by one added dollar of taxes in each of the next 3 years implies an effective experience rating percentage of 80 percent.[19] This present value consideration is not reflected in the empirical measures of experience rating present in the ERIs published by OWS. Thus, experience rating is partial for two reasons. First, several categories of benefits are not assigned back to individual employers. Ineffective benefit charges are typically

financed by flat rate taxes levied on all covered employers. Second, the added the benefit payments that trigger increased taxes occur in years before the years when the associated higher taxes are collected.[20]

Several phenomena can arise under partial experience rating. Three merit explicit recognition. First, some employers; e.g., those taxed at the maximum tax rate and some taxed at the minimum tax rate, may face a marginal UI tax cost of zero when they release employees. This adds to the volume of employer-initiated worker turnover, turnover that has both cyclical and seasonal elements. This literature is examined below. Second, because charged benefits may increase labor costs, some employers may be more prone to contest claims to avoid the added costs. This aspect of experience rating has not been explored extensively.[21] Third, several authors have examined the subsidies across industries and within industries that arise under partial experience rating. Industries such as agriculture and construction are consistently recipients of net subsidies while retail trade and finance are consistently subsidizer industries. This topic has been explored in the U.S. by Anderson and Meyer (1993b) and Woodbury (2003), and in Canada by Corak and Pyper (1995) and Corak and Chen (2002). Because of partial experience rating, inter-industry subsidies are smaller in the U.S. than in Canada. Cross-subsidies within industries are also of substantial size, as shown in each of the four preceding studies.

Analysis of the effects of experience rating presents a greater data assembly challenge than does research on effects of UI statutes and administrative actions on unemployment duration. To study effects on temporary layoffs using microdata, for example, one needs three types of information: (1) worker turnover data (from the employer or the worker), (2) UI tax data for the employer, and (3) information on labor market outcomes for the worker (unemployment following a temporary or permanent layoff and (perhaps) receipt of UI benefits). Because one or more of these elements may be absent from available data, researchers have had to develop data constructs that approximate the desired information. Two important consequences are: (1) the use of aggregative data, e.g., by state or industry, and (2) measurement errors which add both to noise in estimated coefficients and perhaps downward biases in parameter estimates.

Illustrative of these data issues is research using CWBH data. These data combine information on covered earnings (including employer ID number, industry, and UI tax rate) with information on benefit experiences (claimant data) in large samples from selected states during the years 1979 to 1983. Turnover and benefit experiences are inferred from quarterly earnings patterns and patterns of UI claims. While these data have much of the desired detail, they are samples (either 10 percent or 20 percent depending on the state). Thus, inferences about total employment at a given firm (an aggregation built up from all earnings records with a given employer number) will have errors—errors which are relatively larger for small firms.

Despite shortcomings of the CWBH data, they represent a major improvement over previous data which was more aggregative. The earliest studies to be cited here used industry data on turnover and tax rates. These data came from separate reporting systems; e.g., BLS establishment data and UI state-industry tax information, and presented no possibility of examining the variability of experience rating within industries.

Tables 4 and 5 respectively summarize findings from studies of the effects of experience rating on worker turnover and on seasonality. Both tables are subdivided into distinct panels to help the reader distinguish important contrasts in research strategies. In both tables, there is an evolution from analysis of data measured as state-industry averages to microdata. A second evolution in table 4 is away from a detailed set of UI tax parameters (the maximum tax rate or TMax, the minimum tax rate, the average slope between the minimum and maximum rates, the taxable wage base) to a single measure that tries to capture all aspects of the marginal tax cost (MTC) of layoffs. The MTC measures incorporate the effects of tax rate minima and maxima as well as the inter-temporal linkage of benefits (first) and (then) taxes. Measurement of MTC also evolves from state-industry averages to micro-estimates for individual firms.

Brechling was a pioneer in this research and one of his most influential pieces is identified in panel A of table 4. Note there were other dependent variables besides the two turnover measures, the layoff rate, and the rehire rate. The analysis was conducted for manufacturing industries where data were readily available. The summary in table 4 reflects his findings both for all manufacturing and for 16 two-digit industries.

In results presented for all manufacturing (his table 6.1) and two-digit industries (his table 6.2), the regression specifications include five UI tax variables as well as average wages, UI benefits, and UI coverage. Several detailed UI tax parameters were included and a number (TMax, TMin and Slope) were significantly linked to turnover measures. The tax rate on negative balance employers almost always entered with a negative effect (as expected) —a finding present in the all-manufacturing data and for the detailed industries. The use of year dummies as well as several tax variables makes it difficult to disentangle effects of individual tax variables. One suspects there is a very high degree of collinearity present in the regressions, adding to the imprecision of the parameter estimates.

Saffer (1982) utilizes a bigger data set and a much simpler characterization of UI tax rates. He found that a wider range of rates (TMax—TMin) reduced the layoff rate in regressions that also included year and industry dummies. Because of the simpler characterization of UI tax parameters, Saffer could directly estimate the elasticity of turnover to increases in the maximum tax rate, an elasticity estimate of -0.61.

Note in both studies there is no attempt to characterize the overall degree of UI experience rating and no attempt to estimate effects on turnover associated with moving to “full” experience rating. The main worth of these analyses is to show there are significant effects of UI tax parameters on turnover.

The argument about imperfect experience rating and “excess” layoffs, however, is usually directed towards employer utilization of temporary layoffs; i.e., short-term separations followed by recall within 4 weeks or less. Unlike establishment data that do not distinguish temporary layoffs from other layoffs, this distinction is made in the CPS which also identifies other reasons for unemployment; i.e., among persons who quit and labor force reentrants and new entrants.

Panel B of table 4 identifies nine studies that utilize microdata for workers, six employing the CPS. These data permit a 0-1 characterization for persons on temporary layoff and on

permanent layoff. For the first seven, however, the UI tax variable (often the marginal tax cost or MTC) is a state-by-industry construct and does not refer to the employer that initiated the layoff for the individual worker. The two Anderson and Meyer (1994, 2000) studies are unique among this group in having microdata from the worker's prelayoff employer. Thus, there is an unknown degree of measurement error in the employer tax variables used in the majority of these studies.[22] Note that two studies (Halpin 1980; Saffer 1983) reflect the earlier practice of using parameters from UI tax schedules to characterize experience rating.

The remaining studies in panel B, starting with Topel (1983), utilize the marginal tax cost (MTC) of layoffs to characterize the response of UI taxes per dollar of increased benefits (marginal increases for both benefits and taxes, each measured as present values). The downward deviation of MTC from 1.0 measures the UI tax subsidy per dollar of benefit payments. In specifications where layoffs are being explained, the MTC variable's coefficient can be used to estimate the impact of moving to "full" experience rating; i.e., the product of multiplying the downward deviation of MTC from 1.0 times the coefficient on MTC.

The six studies in panel B that use a MTC construct agree that the tax subsidy from partial experience rating has important effects on temporary layoffs. Overall, 10 to 50 percent of temporary layoff unemployment is attributed to partial experience rating. Topel estimates about half, Card and Levine some 20 to 50 percent, and Anderson and Meyer from 13 to 23 percent in six states and from 10 to 18 percent in Washington. Note that the estimates in the two Anderson and Meyer studies are the smallest even though they utilize an estimate of the MTC for individual firms.

In contrast, effects on permanent layoffs are much smaller in work by Topel and Card and Levine. Insignificant effects on "other" unemployment were found by Card and Levine. Thus, in considering the various reasons for unemployment, the impacts of incomplete experience rating are concentrated among temporary layoffs in studies that use microdata (CPS and other) where "reason" can be identified.

Panel C identifies three studies where the unit of observation is the firm. Since the firm is the entity that makes layoff decisions, these studies would have more face validity than ones where the firm and its actions are inferred from data about workers.

Burgess and Low (1993) present results of an audit of Illinois employers in 1987, a project interested in tax compliance and the identification of factors associated with noncompliance. The study is included here because it identified limitations of research where the individual firm is not the unit of observation. A substantial share (27 percent) of all layoffs (in Illinois in 1987) were "free" layoffs. The firms were experience rated, but given the job mobility of workers, the base period employer (responsible for the benefit charges) was not the separating employer. Like other ineffectively assigned benefit charges, there was no added cost to the separating employer from these layoffs. Thus, a premise of the preceding studies (that separating employers and charged employers were identical) was not met for about one layoff in four. Burgess and Low note that 1987 was a year of strong employment growth so that the free layoff percentage would be lower when measured over all phases of the business cycle. More work on this aspect of experience rating (other states, other years) is warranted.

Betcherman and Leckie (1995) studied firms with plants in both the United States and Canada to see if differences in UI tax regimes affected reliance on temporary layoffs. Results from a mail survey of 331 firms coupled with data on layoff costs (computed MTCs) indicated that UI tax rates were not an important consideration in layoff decisions. Since this is the only extant study to examine temporary layoffs that uses a cross-border perspective, it is difficult to know the appropriate weight to assign to their findings. The sample of firms was modest (331), but the approach warrants replication; e.g., with firms in auto manufacturing.

Woodbury (2003) examined temporary layoffs using large samples of longitudinal employer data from three states: Missouri, Pennsylvania, and Washington. In addition to studying measures of temporary and permanent layoffs, he also directly examined charged benefit payments associated with layoffs; e.g., benefit charges as a percent of payroll. The most important control variable was the MTC of layoffs measured at the firm level. Using three different estimation techniques (first differences, fixed effects, and instrumental variables) he found significant effects of a low MTC in increasing benefit charges. In his estimates, moving to full experience rating would lower benefit charges by 10-17 percent and reduce temporary layoffs. The estimated effects on temporary layoffs varied by state. They were largest in Missouri (10 percent), intermediate in Pennsylvania (5 percent) and essentially zero in Washington. This is a second finding (along with Anderson and Meyer 2000) of less responsiveness of temporary layoffs in Washington compared to other states.

Because the studies summarized in panel C are based on data from employers, they should be given as much (if not more) weight as those from panel B. All three studies from panel C suggest the effects on temporary layoffs are smaller than previously estimated. Two avenues for the smaller effects are suggested: the presence of free layoffs, a factor not present in other studies, as well as the smaller point estimates of the effects of MTC on temporary layoffs. At a minimum, new analyses using longitudinal employer data should be conducted to determine if the effects of imperfect experience rating are as small as suggested by these three studies in panel C of table 4.

Imperfect experience rating can also influence the seasonality of employment and unemployment for the same reasons as noted above; e.g., lowering the marginal cost of layoffs. Table 5 identifies five studies of seasonality. Three have been introduced earlier (because each study also examined the effects of UI taxes on layoffs). Table 5 displays just the findings related to seasonality.

Halpin found seasonality to be lower when the ratio of taxable to total wages is higher. This effect of the taxable wage base has not been pursued in subsequent research, but as the wage base in many states is very low relative to average wages, there is a pronounced pattern of seasonality in UI tax receipts. Several states now derive upwards of half of total annual UI taxes in payments based on first quarter accruals; i.e., receipts in April–May. The effects of this growing seasonality in collections have not been pursued in more recent analysis.

The estimated effects on employment seasonality range from measurable to zero. The largest estimate is from Anderson's 1993 study where full experience rating would reduce employment seasonality in retail trade by an estimated 14 percent. Anderson and Meyer's

estimate of reduced employment seasonality from full experience rating based on Washington State data is 11 percent and Woodbury's estimate is essentially zero. Note that the latter two estimates are both based on Washington State data. Since this state may not be representative of the U.S. (based on the studies of temporary layoffs summarized in table 4), analysis of one or more other states is probably needed before accepting the low estimates shown in table 5.

Significant unemployment seasonality related to imperfect experience rating is suggested by three studies summarized in table 5. Card and Levine, and Anderson and Meyer both find significant link to MTC, while Halpin finds a significant effect of the minimum UI tax rate. Card and Levine further find higher unemployment seasonality in construction and manufacturing than in trade and services. Since the Woodbury study had the most reliable employer data, it is unfortunate that he examined just the seasonality of employment but not unemployment.

Considering the studies summarized in table 5, it is clear that the analysis of seasonality linked to UI taxes has received much less attention than analysis of temporary layoffs. Based on the five studies, however, three inferences may be drawn: (1) imperfect experience rating has a smaller effect on seasonal than on cyclical patterns of employment and unemployment, (2) imperfect experience rating has relatively larger effects on unemployment than on employment, and (3) imperfect experience rating has smaller effects on permanent layoffs than on temporary layoffs.

What is the appropriate level of experience rating? Although the perspective of much of the research discussed here is that 100 percent (MTC = 1.0) is desirable, this overlooks several pertinent considerations: (1) noncharges and charges against inactive accounts will continue regardless of the tax treatment of layoffs, (2) new employers and those making free layoffs are not reached by UI experience rating, (3) as a practical matter, it seems likely that there will continue to be maximum tax rates even in UI Programs that operate with a high degree of experience rating, (4) recognizing that MTC = 1 implies a larger absolute amount of future taxes than the current increment in UI benefits, could this feature of a "reformed" UI tax system be sold to the employer community? It seems that partial or imperfect experience rating is destined to persist well into the future. Improvements in assigning benefit charges can and should be made, but achieving full experience rating (MTC = 1.0) is very unlikely.

The experiences of three states during the 1990s provide three "natural experiments" for examining the effects of a major change in experience rating. Total or near-total tax holidays were provided to experience rated employers in Kansas (1995–1999), North Carolina (1996), and Georgia (1999–2003).[23] If experience rating has the size of effects on layoffs suggested by earlier research, it would seem that (other things equal) large increases in temporary layoffs would have occurred during these periods. Since the Georgia tax holiday was so recent, it would seem most appropriate to undertake new research to determine the size of the increases in temporary layoffs. A project that examined layoffs in Kansas might also be feasible. Georgia and North Carolina are also relevant since they routinely have a major share of their claims arising from job-attached workers (upwards of one-half in North Carolina and upwards of one-fifth in Georgia) where the claims filing is done by the employer. Since the tax changes were large, evidence of effects might even be seen in aggregate data; e.g., turnover in manufacturing.

## 8.0 Conclusions and Recommendations for Future Research

The preceding sections have covered many topics. Several summaries and suggestions for further research have already been offered. This section draws upon the earlier summaries and suggests priorities among topics for future research.

The existing literature summarized above has amply demonstrated that UI affects the behavior of both workers and employers. At the most basic level is the question of: Which of these two major actors should receive higher priority for additional research? To this author it seems clear that added research on claimant behavior has the higher priority.

The rationale for this inference is based on the following considerations:

- Temporary layoffs are becoming much less important as a reason for unemployment. The change in temporary layoffs, especially the failure to grow as rapidly during the past two recessions as in earlier recessions, has occurred while time series indicators of the degree of experience rating (the ERI, available since 1988) have not demonstrated strong trends.

- There is a long-run upward trend in both unemployment duration and the duration of UI claims. During the past 50 years, duration has increased in a variety of indicators both from the CPS and from UI Program data. In CPS data, the increase in duration seems to have accelerated after about 1980.

- Exhaustions of UI benefits were at historic highs during 2002 and 2003—44 percent in each year—despite relatively low overall unemployment rates.

These developments, documented in report 1 of Vroman and Woodbury (2003), suggest that major changes have been occurring in the labor market. Understanding how policy related both to UI statutes and administration and to other labor market interventions can influence unemployment (particularly unemployment duration) should be where future UI research is most heavily focused.

Since claimant behavior is such a broad topic, several research areas could be considered for research emphasis. Three areas are recommended as being especially important. First, what policy interventions can be most effective in reducing the duration of benefits for the regular UI Program? This suggests added research on work search support/enforcement and more research on the effectiveness of profiling. From the earlier discussion, two areas may be especially worthy of new work:

- Different approaches to work test administration may be appropriate for different types of workers. In particular, close monitoring may be appropriate for some, while others may benefit from greater search autonomy. Trying to develop indicators of these two situations could be productive.[24]

- Because the estimated effects of profiling were so much larger in the analysis of the Kentucky data than in other studies, replication of this study in another state (or states) should be given high priority.

Second, it was noted that all but two of the studies summarized in table 1 utilized data from years prior to 1990. It would be useful to undertake a new analysis of the effects of potential duration and the replacement rate on regular UI beneficiaries. This could be done using administrative microdata or new survey data from several states. Since past studies have shown differing sensitivity to UI provisions among different demographic groups (by age, gender, and ethnicity), the analysis could be designed to yield reliable parameter estimates for detailed demographic groups. Third, large changes in benefit levels and replacement rates took place in Indiana during the mid-1990s and in both California and Virginia in the past few years. Designing a study to examine effects of these changes on take-up and benefit duration should receive support.

## Endnotes

[1] Like many UI statutory provisions there are exceptions, most commonly the use of average earnings over all weeks of the base period (six states in 2003). Also, for the eight states that have a uniform (26 week) maximum benefit duration, the link with base period earnings is less direct than in other states.

[2] These relationships are discussed in Woodbury and Rubin (1997), pp.213-220.

[3] Which activities constitute work search is to some extent a definitional issue. Someone who only looks at help wanted ads in newspapers is not considered to be actively searching in the United States, but is so considered in Canada. The U.S. labor force survey of households (or CPS) considers this to be passive job search. Active search spans activities where the person undertakes active outreach to potential employers; e.g., sending resumes, responding to help wanted ads, and making other contacts with potential employers.

[4] The published paper (Levine 1993) reports results for the effects of the replacement rate only for nonrecipients using microdata and notes that the results for recipients are generally insignificant. However, the same paper also reports results using aggregate state-year data where the replacement rate effects are as indicated in the text (positive for recipients but negative for nonrecipients).

[5] For those on temporary layoff in 2002, only 5.5 percent experienced unemployment duration of 27 weeks or longer compared to 23.5 percent for “other” job losers; e.g., those on permanent layoff.

[6] The annual unemployment rate in New Jersey in 1996 was 6.2 percent, more than 2 percentage points below the rate in 1992.

[7] Three noticeable changes in replacement rates were the temporary decrease in Maine during 1997–1998, the decrease in Ohio in 1989, and the decrease in Louisiana between 1986 and 1989. When data from 2003 become available there may be additional instances of large increases in replacement rates during 2001–2003. However, these may reflect just a compositional shift related to job losses of senior workers.

[8] Significance was tested in two ways in most studies: by comparisons of means and by regression analysis. Table 2A shows results of tests for equality of means. Because regressions typically had only modest explanatory power, the regression tests yielded quite similar results to those shown in table 2A.

[9] In New Jersey, enrollment occurred later in the claim period; i e., 5 weeks after the first payment.

[10] Meyer (1995) reports evidence of increased exit rates (job finding) in both Illinois and Pennsylvania.

[11] A few states rely just on claimant characteristics to assign scores without use of a regression equation.

[12] These statistics refer to 1999 calendar year summaries for all 53 "state" UI Programs.

[13] In each substate area, random selection into treatment and control group was done for persons with profiling scores in the same interval. Everyone in lower intervals was excluded from interventions while everyone in higher intervals was selected for interventions. The similarity of profiling scores between treatments and controls implies they are more “alike” than in the other profiling studies.

[14] The fourth, the New Jersey Reemployment Demonstration took place earlier and differed in the timing of the profiling intervention. Typically, claimants identified as likely exhaustees through profiling are contacted after

receiving a first payment. In New Jersey, the contact occurred later, after 5 weeks of benefits, and it did not have the same potential sanction since the New Jersey Demonstration preceded the implementation of profiling in the mid-1990s.

[15] The table shows two studies by Anderson and Meyer. They are presented as separate even though both are based on the same data file. Note that the number of data points in these two studies are different and that different methods of estimation were used.

[16] The application rate was measured as the ratio of (new plus additional) initial claims to new occurrences of unemployment. The latter was measured as 12 times the annual average number of unemployment spells of less than 5 weeks duration, data available from the CPS. For this application rate, the mean and standard deviation of (unweighted) state-level data were 0.496 and 0.194, whereas the respective counterparts for the replacement rate were 0.362 and 0.055. The replacement rates are much more tightly packed around their mean than are the application rates. These patterns are repeated in all years of data from that study; i.e., from 1977 to 1999.

[17] Surprisingly this variable had a positive coefficient, a finding repeated in Vroman (2002).

[18] States are to submit an annual report summarizing their experiences with benefit charging (the ETA 204 report). The report is used to develop an experience rating index (ERI) for each state. These reports were first submitted for tax rate year 1988.

[19] The present value of the future taxes for the next 3 years is calculated as $1/(1 + d) + 1/(1+ d)^2 + 1/(1 + d)^3$ for a discounted present value of 2.406 under a 10 percent discount rate (d = 0.1). The ratio of 2.406 to 3.00 yields an experience rating ratio of 0.802. To have a present value equal to the value of benefits, the employer would have to pay more in future taxes than the value of benefits paid out this year.

[20] These two categories are not exhaustive of the reasons for partial experience rating. New employers pay a flat rate tax in years before they are eligible for experience rating, effectively a zero experience rate on added layoffs. Also, some charges are not assigned to separating employers because the separating employer is not the base period employer to whom benefit charges are assigned. This latter phenomenon, termed "free layoffs," has been identified and examined by Burgess and Low (1993).

[21] Two analyses are found in Vroman (1996b) and Anderson and Meyer (2000). The latter study found evidence that movement to experience rating in Washington in 1985 was associated with a statistically significant increase in the rate of nonmonetary determinations (especially seps).

[22] Most unusual, the Feldstein (1978) study does not have a tax cost variable. It infers the effects of experience rating from the coefficient on the benefit replacement rate.

[23] In Kansas, 100 percent tax relief was provided to all positive balance employers for 5 years. In North Carolina, the relief was total in 1996 but also substantial in 1997. Rated employers in Georgia (except those at the top tax rate) were provided 99 percent relief of taxes for the 5 indicated years.

[24] Care will have to be taken to ensure that the "sorting" implicit in this suggestion is done in a nondiscriminatory manner. Since education is already used in some profiling algorithms, however, creative approaches probably can be developed.

# Appendix A: Tables

## Table 1. Studies of the Determinants of Unemployment Insurance Benefit Duration

| Study | Data | Estimation method | Effect of increase in weekly benefits on duration | Effect of a 1 week increase in potential duration |
|---|---|---|---|---|
| **Classen (1979)** | CWBH data: Arizona and Pennsylvania, 1967-1969 | Tobit duration estimates | $10 increase in WBA increases dur. 1.1 weeks | Arizona: +1.2 weeks<br>Pennsylvania: +0.7 wks |
| **Newton and Rosen (1979)** | UI beneficiaries in Georgia, 1974-1976 | Tobit duration estimates | 10% increase in WBA increases dur. 1.8 weeks | +0.4-0.5 weeks |
| **Katz and Ochs (1980)** | CPS microdata from 26 states, 1968-1977 | Maximum likelihood estimates | | +0.17-0.23 weeks |
| **Moffitt and Nicholson (1982)** | Microdata from 15 states, recipients of EB or FSB, 1975-1977 | Labor supply model, maximum likelihood estimates | 10% increase in rep. rate<br>men: +0.98 weeks,<br>women: +0.84 weeks | +0.1 weeks |
| **Moffitt (1985a)** | CWBH data: 15 states, men, 1978-1983 | Proportional hazards model | 10% increase in WBA increases duration 0.5 wks | +0.15 weeks |
| **Moffitt (1985b)** | Four micro databases, 1974-1983 | Proportional hazards models | | men: +0.17-0.45 weeks,<br>women: +0.10-0.37 weeks |
| **Solon (1985)** | CWBH: Georgia, 1978-1979 | Proportional hazards model | 10% increase in rep. rate increases duration by 0.5-1.0 weeks | +0.3 weeks |
| **Grossman (1989)** | CWBH: Three states, 1981-1984 | Proportional hazards model | 10% increase in WBA increases duration | +0.9 weeks |

## Table 1. Studies of the Determinants of Unemployment Insurance Benefit Duration (continued)

| Study | Data | Estimation method | Effect of increase in weekly benefits on duration | Effect of a 1 week increase in potential duration |
|---|---|---|---|---|
| **Meyer (1989)** | CWBH, 1979–1984 | OLS based on increases in min. and max. WBA | 10% increase in WBA increases duration 1.7 wks | -- |
| **Meyer (1990)** | CWBH: men, 1978–1983 | Proportional hazards model | 10% increase in rep. rate increases duration 1.5 wks | -- |
| **Katz and Meyer (1990)** | CWBH: 12 states, 1978–1983 | Proportional hazards model | -- | +0.16-0.20 weeks |
| **Davidson and Woodbury (1996)** | Reemployment bonus data from three states, 1984–1985 and 1988–89 | Impact estimates applied in a general equilibrium search model | -- | Illinois: +0.2 weeks, Pennsylvania and Wash-ington: +0.0-0.2 weeks |
| **Corson, Needles and Nicholson (1999)** | Survey data (16 states) and administrative data (18 states) for EUC program, 1991–1994 | Weibull, natural log of duration (weeks to first job, weeks of UI benefits) | $10 increase in WBA increases duration | +0.20-0.42 weeks |
| **Card and Levine (2000)** | New Jersey Extended Benefit Program, June – Nov. 1996 | Probit and hazard rate models | 10% age point increase in replacement rate increases duration by 1.0 week | +0.08 |

## Table 2A. Studies of the Effects of Work Search

| Study and author(s) | Design | Sample | Findings — benefit duration |
|---|---|---|---|
| **Nevada Claimant Placement Program**<br>**Steinman (1978)** | T: Increased staff attention, referrals, and eligibility checks | Eligible Nevada claimants after three UI payments,<br>Feb. 1977 to Mar. 1978<br>T = 2,371, C = 1,174 | T: -3.90** weeks |
| **Wisconsin Eligibility Review Pilot Project**<br>**Wisconsin Job Service (1984)** | T: Six hour job search workshop | Wisconsin claimants indefinitely separated from most recent job,<br>Mar. to Aug. 1983<br>T = 2,587, C = 2,277 | T: -0.62 weeks |
| **Charleston Claimant Placement and Work Test Demonstration**<br>**Corson, Long and Nicholson (1985)** | T1: Stronger work test<br>T2: T1 plus enhanced placement services<br>T3: T2 plus JSW (job search workshop)<br>C: Customary work test | Charleston, S.C.,<br>Feb. to Dec. 1983<br>T = 4,247, C = 1,428 | T1: -0.55* weeks<br>T2: -0.61 ** weeks<br>T3: -0.76 ** weeks<br>Effects larger on men and construction workers |
| **New Jersey UI Reemployment Demonstration Project**<br>**Corson, et al. (1989)**<br>**Anderson, et al. (1991)**<br>**Corson and Haimson (1996)** | T1: JSA<br>T2: JSA plus training or relocation assistance<br>T3: JSA plus cash bonus<br>C: Eligibility: first UI payment, age, tenure, indefinite layoff | New Jersey,<br>July 1986 to June 1987<br>T = 8,675, C = 2,385 | T1: -0.47** weeks<br>T2: - 0.48 **weeks<br>T3: -0.97** weeks<br>6 year T1: -0.76 weeks<br>6 year T2: -0.93 weeks<br>6 year T3: -1.72** weeks |

Key: T: treatment group, C: control group. *(**) Statistically significant at the 90 (95) percent level with a two-tailed test.

## Table 2A. Studies of the Effects of Work Search (continued)

| Study and author(s) | Design | Sample | Findings — benefit duration |
|---|---|---|---|
| **Washington Alternative Job Search Experiment**<br>**Johnson and Klepinger (1991)** | T1: Exception reporting<br>T2: New work search policy<br>T3: Intensive services<br>C: Existing work search policy | Tacoma, WA,<br>July 1986 to Aug. 1987,<br>T = 6,763, C = 2,871 | T1: +3.34** weeks<br>T2: +0.17 weeks<br>T3: -0.47* weeks<br>Exits increased preceding required service participation |
| **Maryland UI Work Search Demonstration**<br>**Klepinger, Johnson, Joesh and Benus (1998)**<br>**Klepinger, Johnson and Joesh (2002)** | T1: Report four employer contacts weekly<br>T2: Two weekly contacts required, but no reporting<br>T3: Report two contacts weekly plus a 4-day JSW<br>T4: Report two contacts weekly and both verified<br>C: Existing policy, two contacts weekly but not verified | Maryland, six local offices,<br>Jan. to Dec. 1994,<br>Combined sample 23,758 monetarily eligible new initial UI claimants | T1: -0.7 ** weeks<br>T2: +0.4* weeks<br>T3: -0.6** weeks<br>T4: -0.9** weeks<br>T1, T3, and T4: no effect on earnings<br>T2: raised earnings 4* percent,<br>Insignificant Hawthorne effect. |
| **Four-State Analysis of Work Search Enforcement**<br>**Ashenfelter, Ashmore and Deschenes (1999)** | T1: Early eligibility review and work search plan, first visit<br>T2: Review of monetary and job separation decisions, second visit<br>T3: T2 + work search verification<br>C1: Normal intake, first visit<br>C2: Verification of reason for separation, second visit | Connecticut, Massachusetts, Tennessee, and Virginia,<br>Dec. 1984 to Apr. 1985<br>T1 = 1,852, C1 = 1,806<br>T2, T3 = 1,517, C2 = 1,499 | Effect of work search verification<br>CN T3 - T2: -0.190<br>MA T3 - T2: +0.289<br>TN T3 - T2: -0.391<br>VA T3 - T2: +0.112 |

## Table 2A. Studies of the Effects of Work Search (continued)

| Study and author(s) | Design | Sample | Findings — benefit duration |
|---|---|---|---|
| **Job Search Assistance Demonstration in District of Columbia and Florida**<br>**Decker, Olsen, Freeman and Klepinger (2000)** | T1: Structured JSA<br>T2: Individualized JSA,<br>T3: T2 plus training<br>C: Profiled as likely exhaustees | District of Columbia, June 1996 to June 1996<br>D.C.: T + C = 8,071<br>Florida, Mar. 1995 to Mar. 1996<br>Florida: T + C = 12,042 | DC T1: -1.13** weeks<br>DC T2: -0.47** weeks<br>DC T3: -0.61** weeks<br>FL T1: -0.41** weeks<br>FL T2: -0.59** weeks<br>FL T3: -0.52** weeks |
| **Long-run Effects of Washington Alternative Work Search Experiment**<br>**Woodbury (2001)** | T1: Exception reporting<br>T2: New work search policy<br>T3: Intensive services<br>C: Existing work search policy | Tacoma, WA,<br>July 1986 to Aug. 1987<br>T = 6,763, C = 2,871 | Short-run effects on weeks of UI: similar to Johnson and Klepinger (1991), see above<br>10-year effects on quarterly earn.<br>T1: -$52 unconditional on emp., +$82 **conditional on emp.<br>T2: -$12 unconditional on emp., +$43 conditional on emp.<br>T3: +$137** unconditional on emp., +$81** conditional on emp. |

## Table 2B. Reemployment Bonus Experiments

| Study and author(s) | Design | Sample | Findings — benefit duration |
|---|---|---|---|
| **Illinois UI Incentive Experiments**<br>**Woodbury and Spiegelman (1987)** | Bonuses for early reemployment, about 4 times weekly benefit amount (WBA)<br>T1: $500 paid to claimants<br>T2: $500 paid to employers | Illinois,<br>July to Nov. 1984<br>T = 8,149 in two groups<br>C = 3,952 | T1: -1.15** weeks<br>T2: -0.36 weeks |
| **New Jersey UI Reemployment Demonstration Project**<br>**Corson, et al. (1989)**<br>**Anderson, et al. (1991)** | Bonus, half of remaining benefits, but declining 10% per week, avg. $1,644 or about 10* WBA<br>T1: Bonus + JSA<br>T2: JSA only<br>T3: T1 - T2 | New Jersey,<br>July 1986 to June 1987<br>T = 8,675 in six groups<br>C = 2,385 | T1: -0.90** weeks<br>T2: -0.50** weeks<br>T3: -0.40 weeks |
| **Pennsylvania Reemployment Bonus Demonstration**<br>**Corson, et al. (1992)** | Two levels of bonuses (B), two qualification periods (Q),<br>avg. about 3*WBA or $500, | Pennsylvania,<br>July 1988 to Oct. 1989<br>T = 10,694 in six groups<br>C = 3,392 | T1: Low B, Short Q: -0.41 wks<br>T2: Low B, Long Q: -0.44 wks<br>T3: High B, Short Q: -0.49 wks<br>T4: High B, Long Q: -0.92** wks<br>T5: High-decline, Long Q: -0.19 |
| **Washington Reemployment Bonus Expiriment**<br>**Spiegelman, O'Leary and Kline (1991)** | Three levels of bonuses (B), two variable qualification periods (Q), avg. bonus about $562 | Washington,<br>Feb. to Nov. 1988<br>T = 12,452 in six groups<br>C = 3,082 | T1: Low B, Short Q: -0.06 wks<br>T2: Med. B, Short Q: -0.18 wks<br>T3: High B, Short Q: -0.62*<br>T4: Low B, Long Q: -0.51* wks<br>T5: Med. B, Long Q:-0.26 wks<br>T6: High B, Long Q:-0.73** |

## Table 2B. Reemployment Bonus Experiments (continued)

| Study and author(s) | Design | Sample | Findings — benefit duration |
|---|---|---|---|
| **Long-run Effects of Washington Reemployment Bonus Experiment**<br>**Woodbury (2001)** | Three levels of bonuses (B), two variable qualification periods (Q), avg. bonus about $562 | Washington,<br>Feb. to Nov. 1988, | Short-run effects on weeks of UI: similar to Spiegelman, O'Leary and Klein (1991), see above.<br>10 year effects on quarterly earn.<br>T1: Low B, Short Q: +$67**<br>T2: Med. B, Short Q: +$28<br>T3: High B, Short Q: +$144**<br>T4: Low B, Long Q: +$61*<br>T5: Med. B, Long Q: +$3<br>T6: High B, Long Q: +$50 |

## Table 2C. Profiling Studies

| Study and author(s) | Design | Sample | Findings — benefit duration |
|---|---|---|---|
| **New Jersey UI Reemployment Demonstration Project**<br>**Corson, et al. (1989)**<br>**Anderson, et al. (1991)** | T1: JSA<br>T2: JSA plus training or relocation assistance<br>T3: JSA plus cash bonus<br>C: Eligibility: first UI payment, age, tenure, indefinite layoff | New Jersey,<br>July 1986 to June 1987<br>T = 8,675, C = 2,385 | T1: -0.47** weeks<br>T2: - 0.48 **weeks<br>T3: -0.97** weeks |
| **Six State Worker Profiling and Reemployment Services Evaluation**<br>**Dickinson, Decker, Kreutzer and West (1999)** | P: Claimants profiled and referred to early JSA<br>C: Claimants profiled but not referred to early JSA | Connecticut, Illinois, Kentucky, Maine, New Jersey, and South Carolina,<br>July 1995 to Dec. 1996,<br>P = 92,401, C = 295,920 | CT: -0.25** wks, IL: -0.41** wks<br>KY:-0.21** wks ME: -0.98** wks<br>NJ: -0.29 ** wks, SC: +0.02 wks |
| **Job Search Assistance Demonstration in District of Columbia and Florida**<br>**Decker, Olsen, Freeman and Klepinger (2000)** | T1: Structured JSA<br>T2: Individualized JSA<br>T3: T2 plus training<br>C: Profiled as likely exhaustees but no services provided | District of Columbia, June 1996 to June 1996<br>DC: T + C = 8,071<br>Florida, Mar. 1995 to Mar. 1996<br>Florida: T + C = 12,042 | DC T1: -1.13** wks, T2: -0.47** wks, T3: -0.61 weeks<br><br>FL T1: -0.41** wks, T2:-0.59** wks, T3:-0.52 weeks |
| **Kentucky Worker Profiling Evaluation**<br>**Black, Smith, Berger and Noel (2003)** | T: Claimants profiled and referred to early JSA and reemployment services<br>C: Profiled but not referred, same profiling score as treatments | Kentucky,<br>Oct. 1994 to June 1996<br>T = 1,236 C = 745 | T: -2.2 **weeks<br>T: -$143** UI benefits<br>T: +$1,054 earnings |

Key: T: treatment group, C: control group, P: participant group. *(**) Statistically significant at the 90 (95) percent level with a two-tailed test.

## Table 3. Studies of the Effect of UI Benefit Levels on Application Rates

| Study | Data | Estimation method | Effect of increase in weekly benefits on UI applications |
|---|---|---|---|
| **Corson and Nicholson (1988)** | IUTU ratios<br>Quarterly data, 11 states<br>1971–1987 | OLS | 10 % increase in replacement rate increases IUTU by 0.008 - 0.024 or 2-6 percent |
| **Blank and Card (1991)** | Beneficiary/eligible ratios<br>50 state data, 1977–1987 | Weighted OLS | 10 % increase in replacement rate increases application (take-up) rate by 5-9 percent |
| **Anderson and Meyer (1994)** | UI application (take-up) rates CWBH data, six states, 1979–-1983, small sample n = 80,331 | Logistic estimation | 10 % increase in benefit amount increases take-up rates by 4.6-7.8 percent |
| **Anderson and Meyer (1997)** | UI application (take-up) rates CWBH data, six states, 1979–1983, large sample n = 980,286 | Linear probability estimation | 10 % increase in benefit amount increases take-up rates by 2.0-2.5 percent |
| **McCall (1995)** | CPS, dislocated worker data Applications/new spells ratios, 1984–1992 | Logistic estimation | Positive effect in linear specification<br>Positive quadradic effect with maximum effect at medium replacement rates |
| **Vroman (2002)** | Applications/new spells ratios, 50 state data, 1977–1999 | OLS | Increase in replacement rate does not increase the application rate |

## Table 4. Studies of the Effects of Incomplete Experience Rating on Employee Turnover

| Study | Dependent variable(s) | Data and sample size | Main independent variable(s) | Main findings |
|---|---|---|---|---|
| **Panel A. Studies using state and industry data** | | | | |
| **Brechling (1981)** | Annual layoff rate<br>Annual rehire rate<br>Avg. weekly hours<br>Avg. unemployment duration | State-year mfg. data for reserve ratio states,<br>1962–1969<br>n = 170 for all mfg.,<br>n = 48-96 for two digit industries | Maximum, minimum and negative balance tax rates, slope of tax rate schedule, reserve ratio where maximum tax rate is reached, year dummies | Increases in TMax, TMin, and Slope increase turnover, hours and U duration. Increase in negative balance tax reduces turnover, hours and duration |
| **Saffer (1982)** | Annual layoff rate | State-year data on all-industry layoff rates (first claims/emp.)<br>1967–1975,<br>n = 468 | Maximum less minimum payroll tax rates (TMax – TMin), average weekly benefit, average weekly wage, taxable wage base, industry and year dummies | Increases in (TMax – TMin) reduces layoffs (elasticity = -0.63) |
| **Panel B. Studies using micro data for workers** | | | | |
| **Feldstein (1978)** | 0-1 dummy variable, =1 if worker on temporary layoff | Mar. 1971 CPS<br>n = 24,545 | Imputed UI replacement rate | 10% increase in replacement rate causes a 5% increase in temporary layoffs |

## Table 4. Studies of the Effects of Incomplete Experience Rating on Employee Turnover (continued)

| Study | Dependent variable(s) | Data and sample size | Main independent variable(s) | Main findings |
|---|---|---|---|---|
| **Panel B. Studies using microdata for workers** | | | | |
| **Halpin (1980)** | 0-1 dummy variable, =1 if worker on temporary layoff | 1977 Survey of Income and Education<br>n = 40,868 | Ratio of taxable to total wages, MAXGAP (gap between maximum tax rate and tax rate needed to fund benefits of negative balance firms), minimum and maximum tax rates, % of benefits noncharged | 10 percentage point increase in ratio of taxable to total wages reduces layoffs by 5 percentage points, 1 percentage point reduction in MAXGAP lowers layoffs by 15 percentage points |
| **Saffer (1983)** | Two 0-1 dummy variables, =1 if worker on temporary layoff, and =1 if worker on permanent layoff | March 1975 CPS linked to SIC two-digit data from ES202 reports<br>n = 14,899 | Proportional deviation of industry average UI tax rate from schedule midpoint, imputed UI replacement rate, state taxable wage base | Increase in estimated degree of experience rating increases probability of temporary layoffs, marginal effect on permanent layoffs |
| **Topel (1983)** | Probability of temporary layoff | March 1975 CPS linked to SIC two-digit data from ES202 reports<br>n = 8,280 | Marginal tax cost (MTC) of layoffs per dollar of benefits (MTC imputed for 551 state-2 digit cells), imputed benefit replacement rate | Increase in tax subsidy (MTC < 1.0) increases probability of temporary layoff, full experience rating would reduce temporary layoffs by 31% |

## Table 4. Studies of the Effects of Incomplete Experience Rating on Employee Turnover (continued)

| Study | Dependent variable(s) | Data and sample size | Main independent variable(s) | Main findings |
|---|---|---|---|---|
| **Panel B. Studies using microdata for workers** | | | | |
| **Topel (1984)** | Two 0-1 dummy variables, =1 if worker on temporary layoff, and =1 if worker on permanent layoff | March CPS 1973-1976 linked to SIC two-digit data from ES202 reports<br>n = 33,653 | Marginal Tax Cost (MTC) and imputed replacement rate as above in Topel (1983) | Increase in subsidy increases probability of temporary layoff, smaller effect of subsidy on permanent layoff |
| **Topel (1985)** | Two 0-1 dummy variables, =1 if worker on temporary layoff, and =1 if worker on permanent layoff (or quit) | March CPS 1977–1981 linked to SIC two-digit data from ES202 reports<br>n = 76,106 men | Marginal Tax Cost (MTC) and imputed replacement rate as above in Topel (1983) | Increase in subsidy increases probability of temporary layoff, full experience rating would reduce unemployment rate from 5.1% to 3.7% |
| **Card and Levine (1994)** | Three 0-1 dummy variables, =1 if worker on temp. layoff, =1 if worker on permanent layoff, =1 for "other" unemployment | CPS outgoing rotation groups 1979–1987<br>n = 187,598 | Marginal tax cost of layoffs (MTC) for firms in 36 states and 5 industries, personal characteristics, dummies for industry, occupation, and year | Full experience rating (MTC = 1.0) would reduce temporary layoffs by 0.28-0.52 percentage points or 20-38%, much smaller effects on permanent layoffs, no effect on "other" unemployment |

## Table 4. Studies of the Effects of Incomplete Experience Rating on Employee Turnover (continued)

| Study | Dependent variable(s) | Data and sample size | Main independent variable(s) | Main findings |
|---|---|---|---|---|
| **Anderson and Meyer (1994)** | 0-1 dummy variable, = 1 if worker laid off during the quarter | CWBH, over 300,000 quarterly wage records from six states matched with UI claims records, 1979–1983 | Marginal tax cost of layoff (MTC) measured at firm level, amount and potential duration of benefits, other controls | 13% to 23% of temporary layoffs (8% of all layoffs) due to incomplete exp. rating |
| **Anderson and Meyer (2000)** | Ratio of UI claims to employment | Washington State, all U.S states, 1982 to 1997<br>n = 51 for difference-in-difference estimates | State dummy variables interacted with post 1984 dummy variables, MTC in Washington goes from 0 in 1984 to 0.99 in 1985 | Move to full experience rating in WA would reduce claims rate by 0.31 to 0.56 percentage points or by 10-18 percent |
| **Panel C. Studies using data for individual firms** | | | | |
| **Burgess and Low (1993)** | Layoffs where employers are not charged | Illinois 1987, n = 611 firms and 84,000 workers | Experience rating charges related to layoffs | 27% of all layoffs were free layoffs with no charges to employers |
| **Betcherman and Leckie (1995)** | Firm layoff rate | 331 firms in Ontario and three U.S. states | Marginal tax cost of layoffs (MTC) | No impact of MTC on layoffs |
| **Woodbury (2003)** | UI benefit charges as a percent of payroll, layoff rate (deviation from trend), temporary layoff rate, permanent layoff rate | Longitudinal firm data from Missouri (6,812), Pennsylvania (12,792), and Washington (6,609), 1985 to 1994 | Marginal tax cost of layoffs (MTC) for individual firms | Significant effect of MTC on benefit charges. Full exp. rating reduces charges by 10-17% and temporary layoffs by 0-10% |

## Table 5. Studies of the Effects of Incomplete Experience Rating on Seasonality of Employment and Unemployment

| Study | Dependent variable(s) | Data and sample size | Main independent variable(s) | Main findings |
|---|---|---|---|---|
| **Panel A. Studies using state and industry data** | | | | |
| **Halpin (1979)** | Spectral measure of seasonal variation in unemployment | Monthly state data for three SIC 3-digit industries, 1960–1974 | Ratio of taxable to total wages, minimum and maximum tax rates (TMIN and TMAX) | Seasonal variation falls with higher ratio of taxable to total wages. Seasonal variation rises with a higher TMIN |
| **Panel B. Studies with microdata** | | | | |
| **Anderson (1993)** | Seasonal employment variability | CWBH quarterly data for retail firms in six states, 1979 to 1983<br>n = 8,278 firms<br>n = 37,749 firm-year obs | Marginal tax cost of layoff (MTC) with fixed effects for firms and years | Elasticity of seasonal variability from MTC = 0.1, full experience rating would reduce seasonal employment variability by 14% |
| **Card and Levine (1994)** | Seasonal unemployment variability. Three dummy variables in monthly data:<br>= 1 for temp. layoffs,<br>= 1 for perm. layoffs,<br>=1 for "other" unemp. | CPS outgoing rotation groups 1979-1987<br>n = 187,598 | Marginal tax cost of layoffs (MTC) for firms in 36 states and 5 industries, personal characteristics, dummies for industry, occupation and year | Large effects of MTC on layoffs in months of low seasonal demand. Large effects in construction and mfg. Small effects in trade and services. |

## Table 5. Studies of the Effects of Incomplete Experience Rating on Seasonality of Employment and Unemployment (continued)

| Study | Dependent variable(s) | Data and sample size | Main independent variable(s) | Main findings |
|---|---|---|---|---|
| **Panel B. Studies with microdata** | | | | |
| **Anderson and Meyer (2000)** | Seasonal UI claims variability and seasonal employment variability | Washington State, all U.S states, 1982 to 1997<br>n = 51 for difference-in-difference estimates | State dummy variables interacted with post-1984 dummy variables, MTC in Washington goes from 0 in 1984 to 0.99 in 1985 | 16-31 percent reduction of seasonality of claims after introduction of experience rating in 1985. Smaller reduction (11%) in the seasonality of employment |
| **Woodbury (2003)** | Seasonal employment variability | Longitudinal firm data from Missouri (6,812), Pennsylvania (12,792), and Washington (6,609), 1985 to 1994 | Marginal tax cost of layoffs (MTC) for individual firms | Very small effects of MTC on employment seasonality |